American Economic Journal: Applied Economics 2016, 8(2): 65–91
http://dx.doi.org/10.1257/app.20140100

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

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Daylight Saving Time (DST) impacts over 1.5 billion people, yet many of its impacts on practicing populations remain uncertain. Exploiting the discrete nature of DST transitions and a 2007 policy change, I estimate the impact of DST on fatal automobile crashes. My results imply that from 2002–2011 the transition into DST caused over 30 deaths at a social cost of $275 million annually. Employing four tests to decompose the aggregate effect into an ambient light or sleep mechanism, I find that shifting ambient light only reallocates fatalities within a day, while sleep deprivation caused by the spring transition increases risk. (*JEL* I12, Q48, R41)

Daylight Saving Time (DST) in the United States 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 four 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 AM to 3 AM. The process is then reversed for the fall transition with clocks “falling back” from 2 AM to 1 AM. This alters the relationship between clock time and solar time by an hour, moving sunlight from the morning to the evening (see Figure 1). Although the general concept of DST was first suggested by Benjamin Franklin in a satirical essay in 1784 (Aldridge 1956), it was over a

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[†] Go to http://dx.doi.org/10.1257/app.20140100 to visit the article page for additional materials and author disclosure statement or to comment in the online discussion forum.

century later when the formal procedure was proposed by George Vernon Hudson, an entomologist who wanted more light in the evenings to pursue his passion of collecting insects (Hudson 1895). The policy was first used during World Wars I and II, but 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. With an average of over 40,000 annual fatalities from 2002–2011, motor vehicle crashes are the number one cause of accidental death in the United States (Center for Disease Control and Prevention (CDC) 2015). Given the large base level of fatalities, even a small change in fatal crash risk is a potentially large killer. DST impacts practicing populations through two primary mechanisms. First, it creates a short-term disruption in sleeping patterns following the spring transition. Using the American Time Use Survey, Barnes and Wagner (2009) find that Americans sleep 40 minutes less on the night of the spring transition, but they do not sleep a significant amount more on the night of the fall transition despite the extra hour. Second, DST creates darker mornings and lighter evenings than would be observed under Standard Time. Even this one hour shift in light can have major consequences; Doleac and Sanders (forthcoming) find that increased ambient light in evenings reduces crime while Wolff and Makino (2013) suggest that it increases time devoted to exercise.[2]

Existing evidence of the impact of DST on fatal vehicle crashes has generated little consensus.[3] One early set of studies focuses exclusively on a sleep effect, generally comparing crash counts on the first Monday of DST with the prior and subsequent Mondays. These studies suggest either no impact or an increase in crashes due to DST.[4] A second set of studies focuses on the ambient light mechanism using an indirect two-step approach. First they estimate the impact of ambient light (Ferguson et al. 1995; Broughton, Hazelton, and Stone 1999) or sunrise and sunset times (Coate and Markowitz 2004) on fatal crashes. Then they use these estimates to simulate the impact of imposing DST light levels on the rest of the year. While these studies suggest a net reduction in fatal crashes through this mechanism, these calculations require potentially strong assumptions about driver behavior under counterfactual hours of light.[5]

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[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).

[2] Since fatal crashes are more prevalent in the evening (online Appendix Figure A-1), 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.

[3] When conducting a review of this literature, Aries and Newsham (2008) comment that "the results of prior investigations differ and are sometimes contradictory" (p. 1863).

[4] Using data from Canada covering 1991–1992, Coren (1996) finds an increase in crashes on the first Monday of DST relative to the previous and subsequent Mondays. However, this estimate has been criticized for considering only two years of data and a subsequent extension by Vincent (1998) using Canadian data from 1984–1993 found no significant effect. Using similar methods with US data from 1975–1995, Varughese and Allen (2001) find an increase in crashes on the Monday following the spring transition. Finally, Lahti et al. (2010) examine crashes spanning 1981–2006 in Finland and find no significant difference between the week before and week after the spring transition.

[5] For instance, 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.

The first paper that seeks to unite this literature is Sood and Ghosh (2007), who made use of a natural experiment arising from a 1987 spring extension to DST. Using US data from 1976–2003 and a difference-in-differences (DD) framework, they found that crashes fell by 6–11 percent during the first nine weeks of DST. They attribute this effect to the ambient light mechanism, as any sleep effect would dissipate quickly. However, in their full sample the same specification suggests that a three-week placebo period directly before DST experienced a smaller, but statistically significant reduction in fatal crashes, raising concerns about the underlying assumptions.[6] To test for a sleep effect, they consider the first Monday of DST as the treated period. Using the same DD framework, they find no evidence of a sleep effect. Despite a negative point estimate, the test's low power prevents them from being able to rule out even an 11 percent increase in fatal crashes.

Building on the previous literature, my contribution is three-fold. First, following Sood and Ghosh's use of a natural experiment, I exploit two sources of quasi-experimental policy variation to identify the overall effect of DST on fatal vehicle crashes. The use of these identification strategies in tandem, one using within-year variation in DST coverage and the other using across-year variation exploiting a policy change, addresses the concern raised by Sood and Ghosh that their results could be partially driven by changes to the seasonal crash profile across years. Second, I exploit differential timing in when each mechanism is active to decompose the overall impact into an ambient light and sleep component. Motivated by recent literature on sleep disruptions, this includes the use of a higher powered test that allows for a sleep-treatment period of about one week.[7] Finally, I extend my analysis to the historical sample and discuss how the impact of DST has changed over time, reconciling the findings using this novel approach with the previous literature.

To identify the overall impact of DST on fatal vehicle crashes, I use detailed records of every fatal crash occurring in the United States from 2002–2011 and two identification strategies. First, I exploit the discrete changes between Standard Time and DST within a year, using a regression discontinuity (RD) design. Then using variation created primarily by a 2007 policy change, 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.[8] In both specifications I find a 5–6.5 percent increase

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[6] Further, they find significant reductions in crashes in many of the weeks beyond the 3–4 week extension period. They consider this to be a period that went from control-group to treatment-group, stating, "Prior to the implementation of this law [the 1987 DST extension], implementation of DST was sporadic…" (p. 4). Given their sample choice and time period of 1976–2003, this hedging was unneccessary. Since the Emergency Daylight Time Act ended in 1975, all states in the contiguous United States except Indiana and Arizona (omitted from their sample) have practiced DST every year in accordance with the federally mandated transition dates (Gurevitz 2005). Hence, this estimated reduction in crashes during a time period that was DST in all years suggests that at least part of their estimated effect could be due to a changing of the seasonal crash profile across time. Sood and Ghosh (2007) recognize this issue, stating "One limitation of our approach is that it assumes that no change occurred in seasonal trends or week of the month effects across treatment and control years."

[7] 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" (p. 291).

[8] The 2007 policy change led to both a spring and fall extension to DST, allowing me to consider the impact of DST in both seasons. Further relative to a DD approach, this model uses additional variation in DST coverage created within a particular transition rule.

in fatal crashes immediately following the spring transition. Conversely, I find no impact following the fall transition when no significant shock to sleep quantity occurs. 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.

To determine what portion of the increase in fatal crashes is due to sleep loss versus reallocating ambient light, I employ four tests. First, I isolate the light mechanism by examining only the fall transition. Then, I look at the difference between aggregate estimates in the fall (light mechanism) and spring (light and sleep mechanism) to determine the net impact of the 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 spring DST. Finally, I examine crash factors as reported by the investigating officer. While the reallocation of ambient light leads to additional morning crashes and fewer evening crashes during DST, these impacts offset and all four tests suggest that sleep deprivation is driving the increase in fatal crashes.

My preferred specification reveals a 5.6 percent increase in fatal crashes, persisting for six days following the spring transition. This suggests that the spring transition into DST is responsible for over 30 deaths annually at a social cost of $120 to $300 million.[9] Additional back-of-the-envelope calculations imply that a 1-hour decrease in sleep duration increases the prevalence of fatigue related fatal crashes by 46 percent, underscoring the huge costs of even minor disruptions to sleep schedules given the current sleep-deprived culture in the United States.[10] The total costs of DST due to sleep deprivation could be orders of magnitude larger when worker productivity is considered (Kamstra, Kramer, and Levi 2000; Wagner et al. 2012; and Gibson and Shrader 2014).[11]

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 a 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

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

[10] According to a National Center for Health Statistics survey, nearly 30 percent of American adults reported sleeping less than six hours per day in 2005–2007 (Schoenborn and Adams 2010).

[11] 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 percent of American workers (Winston 2013) and the increase in fatal crashes suggests that sleep loss likely reduces productivity.

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 United States and details the mechanisms through which DST influences crash risk. Section II introduces the data, highlighting the visual discontinuity in raw crash counts at the spring transition. Section III describes the RD and FE identification strategies, outlining the requirements for causal estimates. Section IV presents results, including those that differentiate between the sleep and light mechanisms. Section V extends the sample to cover the historical 1976–2001 period, reconciling the results from the FE and RD models with the previous literature. Section VI concludes with a brief summary and further remarks about the implications for DST as a policy.

I. Daylight Saving Time in the United States

Daylight Saving Time has been a consistent feature in most US states since the Uniform Time Act of 1966.[12] 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. In 1986, an amendment to the Uniform Time Act moved the spring transition from the final Sunday in April to the first Sunday in April, effective starting in 1987. More recently, both transition dates were altered 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 three to four week extension in the spring and a one week extension in the fall.

Recall that DST alters the risk of a fatal crash in two ways: reallocating ambient light from the morning to the evening; and disrupting sleep schedules. Despite strong evidence suggesting the importance of ambient light in fatal crash risk (Fridstrom et al. 1995, Sullivan and Flannagan 2002), the implication for net crashes due to DST remains unclear. DST does not alter the amount of light in a day, it simply reallocates it between the morning and the evening. 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 AM 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. This reallocation of light within a day creates riskier morning driving conditions and less risky evening driving conditions during DST.[13] Since fatal crashes are more prevalent in the evening (online Appendix Figure A-1), 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.

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

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

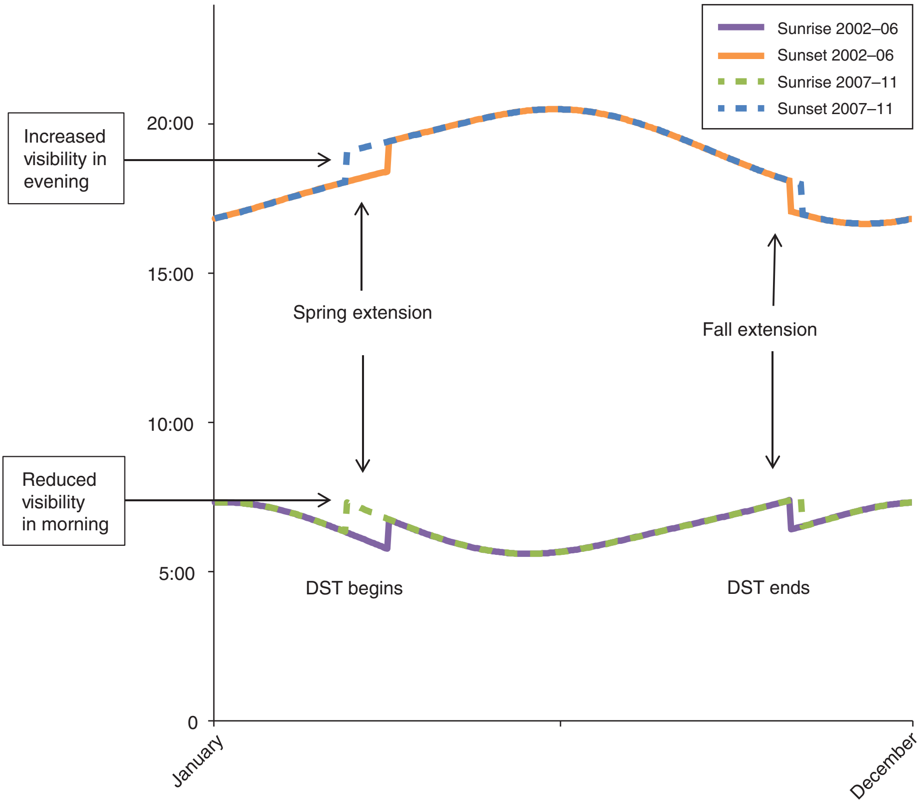

Figure 1. The Influence of Daylight Saving Time on Ambient Light

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

Source: Authors' calculations

The implications for the sleep mechanism are more certain. Since sleep is a key factor in alertness and control (Smith, McEvoy, and Gevins 2002), sleep deprivation likely reduces driving safety. In a study of 400 US Army soldiers, Legree et al. (2003) find a correlation of 0.20 between driver at fault accidents and self-reported insufficient sleep. The sleep mechanism is triggered by the transition into DST, when clocks jump forward from 2 AM to 3 AM 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).[14]

Figure 1 is also useful for thinking about when each mechanism is active. The light mechanism is active for the entire duration of DST, illustrated by the later sunrise and sunset times during this period. Further, this impact should be felt primarily during the hours directly surrounding sunrise and sunset because the amount of ambient light is significantly altered. In contrast, the sleep mechanism should only be felt for a relatively short period following the spring transition, and it likely impacts all hours as a drowsy driver would presumably be more at risk throughout the entire day. I will make use of these differences to identify the dominant mechanism.

II. Data

For fatal vehicle crash data, I use the Fatality Analysis Reporting System (FARS), compiled by the National Highway Traffic and Safety Administration. These data contain a record of every fatal crash occurring in the United States since 1975. To qualify for inclusion in the dataset, the crash must involve a motor vehicle traveling on a trafficway that is open to the public and it must result in the death of either a motorist or non-motorist (e.g., pedestrian) within 30 days of the crash.[15] Importantly for parsing out mechanisms, each record includes the exact time and location of the accident.

I focus on crashes in the most recent ten-year period, from 2002–2011, allowing for five years on either side of the 2007 DST extension.[16] Consistent with other DST literature, my sample is the continental United States excluding Arizona and Indiana because at least part of those states did not practice DST consistently over the entire sample time frame.[17] 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 AM hour twice, using it as a proxy for the missing 2–3 AM hour. For the 25-hour fall transition date, I divide the fatalities occurring from 1–2 AM by two, because this hour occurred twice.[18] Finally, because fatal crash risk is substantially different on holidays than on non-holidays, I omit the FARS designated holidays from the analysis.[19]

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

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

[15] Motor vehicles include cars, passenger and commercial trucks, buses, motorcycles, etc.

[16] In addition to allowing me to exploit the 2007 natural experiment, this recent time frame provides an up-to-date measure of the impact of sleep loss given the current sleep patterns in the United States (according to the National Sleep Foundation, the percentage of Americans averaging less than six hours of sleep increased by 8 percentage points from 1998 to 2009). In Section V, I consider the earlier period from 1976–2001.

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

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

[19] FARS designated holidays are New Years, Memorial Day, Fourth of July, Labor Day, Thanksgiving, Christmas, and some of the surrounding days.

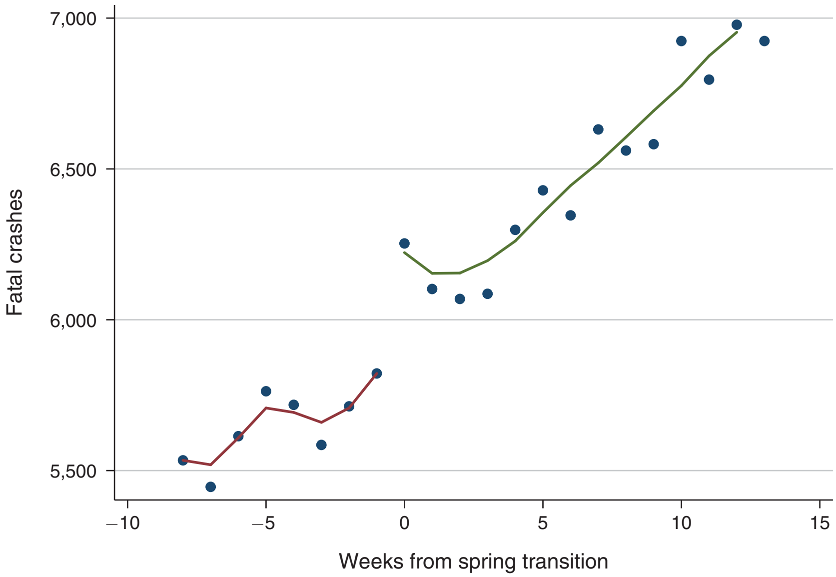

Figure 2. Fatal Crashes around the Spring Transition

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

Source: Authors' calculations

crashes per day across the entire United States and the mode for daily crashes at the state level is zero. Aggregating smooths out potential confounders such as weather which could drive results in some states or even regions, but likely not the entire United States.

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.[20] This provides suggestive evidence that the spring transition is associated with a short-term increase in fatal crashes.[21] 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.

To supplement the FARS data, in some specifications I incorporate additional data. 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, in some specifications I use weekly gasoline prices from the US Energy Information Administration to

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

[21] Further evidence of this discontinuity in the raw data can be seen in online Appendix Table A-1.

help control for driving patterns, because fuel prices exert a strong influence on VMT (Gillingham 2014). For a robustness check, I use VMT data from Caltrans Performance Measurement System, which tracks driving patterns on a subset of California roadways.

III. Empirical Strategy

A. *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 transition 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 policy's 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).[22] 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 per Imbens and Lemieux (2008) with the demeaned crash data. The estimation equation is seen below:

$$\begin{aligned} \ln Fatals_{dy} &= \beta_0 + \beta_1 DST_{dy} + f(DaysToTran) \\ &\quad + f(DST_{dy} \times DaysToTran_{dy}) + \varepsilon_{dy}. \end{aligned} \tag{1}$$

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 transition date.[23]

My baseline specification uses Calonico, Cattaneo, and Titiunik's (2014a) optimal 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 the date around the transition. Stated differently, if all other factors

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[22] Results using a global polynomial are qualitatively identical and are available in online Appendix Table B-3.

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

affecting fatal crash risk, besides DST, are continuous at the transition date, the RD design will provide consistent estimates of the effect of DST. In the online Appendix, 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.

B. *Day-of-Year Fixed Effects* (*FE*)

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.[24] Figure 3 shows days of the year that fall under both DST and Standard 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:

$$\text{(2)} \qquad \ln \mathit{Fatals}_{dy} = \beta_0 + \beta_1 \mathit{SpDST}_{dy} + \beta_2 \mathit{FaDST}_{dy} + \mathit{DayofYear}_d + \mathit{DayofWeek}_{dy} + \mathit{Year}_y + \varepsilon_{dy}.$$

$\mathit{DayofYear}_d$ is a separate dummy for each day of the year, flexibly controlling for the impact of seasonality on fatal crashes.[25] $\mathit{DayofWeek}_{dy}$ and Year_y are day-of-week and year dummies, respectively. SpDST_{dy} is an indicator equal to one if the day falls under DST and occurs before July first. It is identified by the switching dates seen in Figure 3. FaDST_{dy} is an indicator equal to one if the day falls under DST and occurs after June 30th—identified by the analogous fall switching dates. Note, that β_1 here is a different parameter from what is found using the RD design.

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[24] For example, March 11th is Standard Time in 2002–2006, 2010, and 2011, but is DST in the years 2007–2009.

[25] I create dummies for each month/day combination (e.g., an August 25th dummy). This is slightly different than creating a dummy for the one hundredth 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 because it addresses any persistent date effects (e.g., a first day of the month effect) and it generates more conservative estimates.

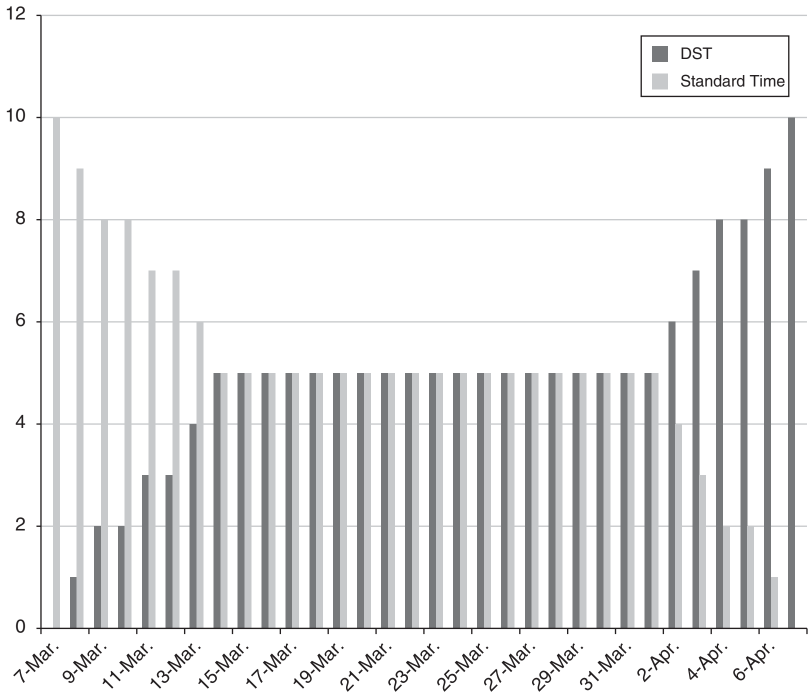

Figure 3. Variation in DST Coverage—Spring

Note: Each bar represents the number of times that date falls under that time regime from 2002–2011.
Source: Authors' calculations

Regression discontinuity estimates the effect of DST right at the spring transition, whereas this FE specification measures the average effect of DST over all dates that are sometimes DST and sometimes Standard Time during the spring. 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.

IV. Results

A. *Main RD Results*

Figure 4 illustrates the RD 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 relevant transition date. If DST has an impact on fatal crashes, this should be evident in a trend break right at the transition date. Focusing first on panel A, the spring transition, there is a clear jump in fatal crashes occuring right at the transition.[26]

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[26] The complete seasonal trend in fatal crashes is illustrated in online Appendix Figure B-1. The plot follows a gradual arc demonstrating the seasonal pattern in fatal crashes, where crashes rise from winter lows, peaking in

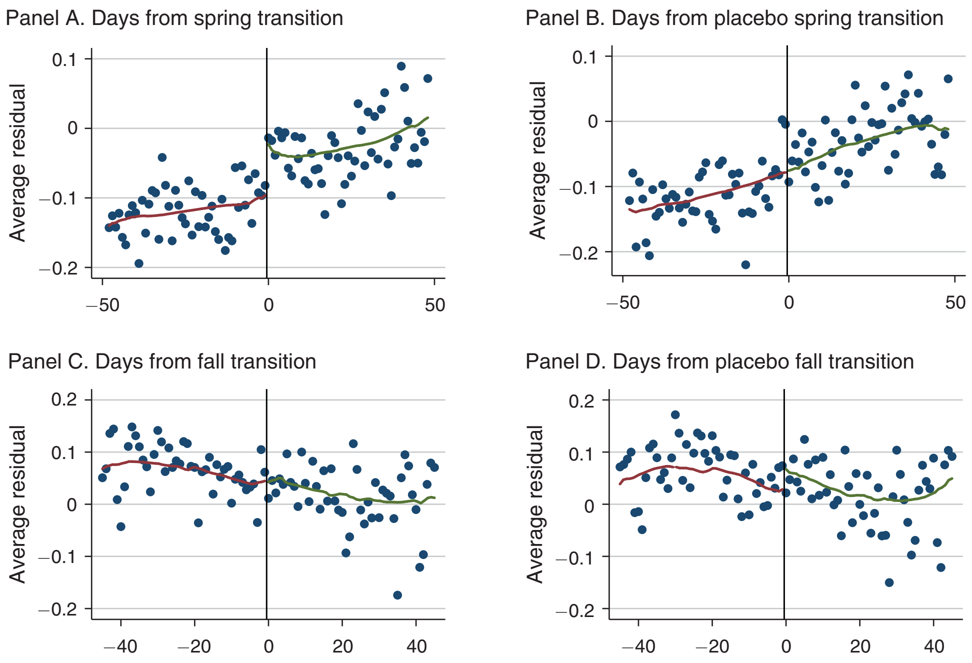

FIGURE 4. RESIDUAL PLOTS—SPRING, FALL, AND PLACEBO TRANSITIONS

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 true or placebo transition. Placebo is as defined in the text. Fitted lines are results of locally weighted regressions.

Source: Authors' calculations

Table 1 shows the corresponding regression estimates. The spring transition into DST is associated with a 6.5 percent increase in fatal crashes.[27] 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 investigate whether the increase is due to one particular transition rule, I split the data into an early subsample (2002–2006) 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, the point estimates for both time periods remain positive and within a few percentage points of the combined sample.

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

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late summer before dropping again through the fall. This longer view of the data makes it more apparent that the residuals quickly resume the seasonal trajectory after a short disruption.

[27] Due to the possibility that crash risk is correlated across time, I explore several alternative standard error calculations in online Appendix Table B-1. Across alternative clustering schemes, Newey West standard errors with a variety of lag structures, and the robust bias corrected confidence intervals of Calonico, Cattaneo, and Titiunik (2014b), the base specification remains significant at the 5 percent level.

TABLE 1—RD ESTIMATES OF THE IMPACT OF ENTERING DST ON FATAL CRASHES (*Spring*)

	(1)	(2)	(3)	2002–2006 (4)	2007–2011 (5)	Placebo (6)
DST	0.0649*** (0.0231)	0.0499*** (0.0176)	0.0626*** (0.0215)	0.0941*** (0.0302)	0.0375 (0.0361)	0.000536 (0.0225)
Bandwidth selector	CCT	IK	CV	CCT	CCT	CCT
Observations	550	966	670	235	265	550

Notes: Dependent variable is the log fatal crashes demeaned by day-of-week and year. All specifications use 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 (2014a); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Authors' calculations

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. This test is illustrated in panel B of Figure 4. The lack of a visual discontinuity and the corresponding near-zero estimate in column 6 suggest that the increase in crashes is not simply due to the transition dates, but due to the actual policy.

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 crash count on the transition date by 24/23 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 online Appendix Table B-2). The remainder of online Appendix Table B-2 shows that results are robust to alternative kernel choice, while online Appendix Table B-3 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.

Given the relatively small sample size, and in order to put the magnitude of this effect in context, I conduct a permutation test. I estimate the baseline specification, local linear regression with a uniform kernel, and the bandwidth selector of Calonico, Cattaneo, and Titiunik (2014a), for every date relative to the spring transition (e.g., once assuming a transition occurred 50 days from the true transition, once assuming the transition occurred 51 days from the true transition, etc.).[28] The distribution of coefficient estimates from this permutation test is shown in Figure 5. The true effect, indicated by the vertical line, is clearly an outlier and has an implied

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[28] In order to keep the true treatment effect from influencing these estimates, I do not include estimates for the first two weeks of DST (potentially sleep treated) or any dates that would include this time frame within a 27-day bandwidth (the length used in the main specification).

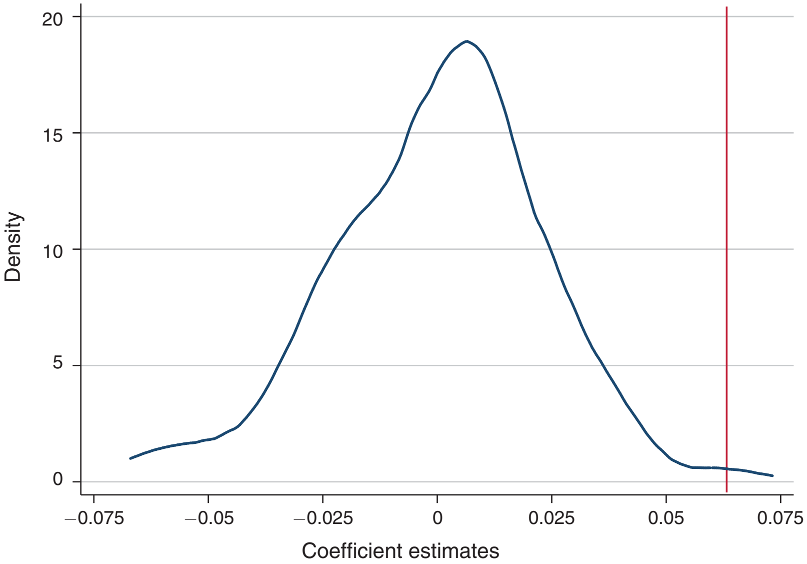

Figure 5. Distribution of Coefficient Estimates from Permutation Test

Notes: The kernel density function uses an Epanechnikov kernel and shows the distribution of coefficient estimates from the permutation test described in the text. The vertical line at 0.0649 is the true effect found for the spring transition. The p-value implied from this permutation test is 0.007.

Source: Authors' calculations

p-value of 0.007. This suggests that this 6.5 percent increase is quite large relative to typical variation seen in the data.

Now I turn to the fall transition to test whether there is an analogous reduction in crashes when leaving DST. Panel C of Figure 4 illustrates the RD 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 dates (columns 4–5). Using an analogous placebo test to that used in the spring does reveal a marginally significant effect. However, the visual evidence is not particularly compelling and the marginal significance level disappears when using alternative bandwidths (see online Appendix Table B-5).[29] 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 to the mechanisms through which DST could impact crash risk to explain this asymmetric effect.

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[29] If these dates are truly associated with a small increase in crash risk, then the absence of this increase at the fall DST transition could be interpreted as evidence of a slight drop in crash risk when leaving DST. This would be consistent with the small (and statisically insignificant) increase in sleep associated with the fall transition date.

TABLE 2—RD ESTIMATES OF THE IMPACT OF LEAVING DST ON FATAL CRASHES (*Fall*)

	(1)	(2)	(3)	2002–2006 (4)	2007–2011 (5)	Placebo (6)
Leaving DST	0.00114 (0.0236)	−0.000182 (0.0153)	0.00630 (0.0242)	0.0274 (0.0265)	−0.00260 (0.0327)	0.0361* (0.0218)
Bandwidth selector	CCT	IK	CV	CCT	CCT	CCT
Observations	381	850	347	215	225	381

Notes: Dependent variable: log fatal crashes demeaned by day-of-week and year. All specifications use 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 (2014a); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Authors' calculations

B. *Mechanisms*

The spring transition is subject to both the light and sleep mechanisms. Hence, the 5–6.5 percent 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 a significant change in sleep quantity, this mechanism is muted, leaving the light mechanism as the primary factor.[30] The aggregate effect of zero when leaving DST in the fall suggests no net impact of DST through the light mechanism. Online Appendix Table B-6 estimates the spring and fall discontinuities simultaneously, allowing for a direct test of equal and opposite effects. Across all three bandwidths this null is rejected at the 5 percent level. However, given differences in sunrise and sunset times across these transitions and the possibility that the sleep mechanism still plays a limited role in the fall, I caution against putting too much weight on a direct spring versus fall comparison. To further disentangle the mechanisms, I use the initial RD framework with subsamples of hours selected to isolate the impact of one mechanism or the other.

Light.—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 (+/− two hours from the average sunrise time around the transition date in each location) and evening hours (+/− two hours

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[30] There still could be some impact through the sleep channel in the fall, as Barnes and Wagner (2009) did find a 12-minute (though statistically insignificant) increase in sleep. Further, adjusting to a modified sleep schedule could cause a reduction in sleep quality, even as quantity increases (Lahti et al. 2006).

Table 3—RD Estimates of the Influence of Ambient Light on Fatal Crashes When Leaving DST (*Fall*)

	Morning			Evening			Least light impacted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Leaving DST	−0.115**	−0.180***	−0.128**	0.187***	0.147***	0.180***	−0.0134
	(0.0501)	(0.0386)	(0.0569)	(0.0457)	(0.0321)	(0.0380)	(0.0275)
Bandwidth selector	CCT	IK	CV	CCT	IK	CV	CCT
Observations	580	989	482	467	886	616	415

Notes: Dependent variable is the log fatal crashes demeaned by day-of-week and year. All specifications use 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" is defined as +/− two hours from the average sunrise time in that location around the fall transition; "Evening" is defined as +/− two hours from the average sunset time in that location around the fall transition. Least light impacted are the remaining hours. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2014a); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Authors' calculations

from the average sunset time around the transition date in each location).[31] Then I estimate the RD model from equation (1) on these subsamples for the fall transition. 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) and a significant increase during the evening (less ambient light). These results suggest that light still plays an important role in fatal crash risk. Further, the near zero point estimate for the remaining, least light-impacted hours, suggests that the sleep mechanism is playing at most a limited role in the fall. Thus, the zero aggregate effect in the fall suggests that the morning and evening light impacts balance out and light has no net impact through DST. Crashes are simply redistributed between the morning and the evening.[32] This redistribution can be seen more clearly in the kernel density function in Figure 6.

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 near zero. Taking a closer look at the spring residual plot in Figure 7

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[31] Morning and evening hours are determined as follows: sunrise and sunset times are calculated for the day before and day of the average transition date in the sample (March 23rd in the spring and November 1st in the fall) for each crash location. The calculation is based on the longitude and latitude of the crash site using the algorithm of Meeus (1991) as implemented in Stata by Gibson and Shrader (2014). If longitude and latitude are missing, average latitude and longitude for the county are used, and if county is also missing then state averages are used. The average of the sunrise (sunset) time on the day before and day of the transition, provide a center-point for the time period treated by a change to ambient light (e.g., sunrise in Louisville, Kentucky the day before the spring transition is 6:44 AM and the day of the spring transition is 7:42 AM, with a center-point of 7:13 AM). To account for the fact that light levels are still affected to a degree beyond the gap in sunrise time, I add two hours on either side of the center-point. Hence, for the Louisville example the morning hours in the spring would be 5:13 AM–9:13 AM.

[32] Online Appendix Table B-7 shows the analagous table for the spring transition.

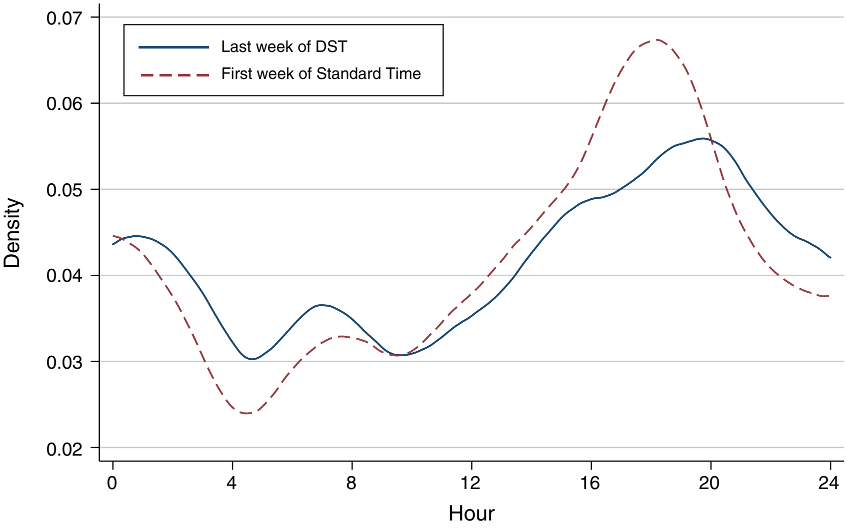

Figure 6. Reallocation of Fatal Crashes (*Fall transition*)

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

Source: Authors' calculations

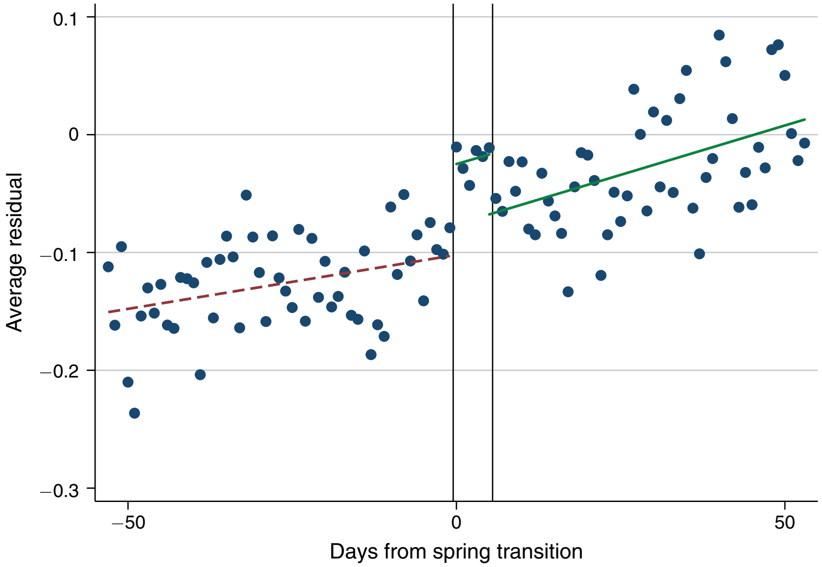

Figure 7. Spring Residual Plot—Six-Day Sleep Impact

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 trend on residuals.

Source: Author's calculations

Table 4—RD Estimates of the Influence of Sleep Loss on Fatal Crashes (*Spring*)

	All hours	Least light impacted hours		
	(1)	(2)	(3)	(4)
DST	0.0649*** (0.0231)	0.0751*** (0.0266)	0.0540** (0.0219)	0.0683*** (0.0238)
Bandwidth selector	CCT	CCT	IK	CV
Observations	550	530	810	670

Notes: Dependent variable is the log fatal crashes demeaned by day-of-week and year. All specifications use 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 those more than two hours away from the average sunrise and sunset time at the spring transition in that location. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2014a); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Authors' calculations

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 phenomenon seen here.

To pry further at the sleep mechanism, I focus on a subsample of hours furthest away from sunset and sunrise to mitigate the light impact.[33] Online Appendix Figure B-2 illustrates the discontinuity while Table 4 provides the regression results. The point estimates are quite similar to the full day impacts and are significant at conventional levels. 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.

C. *Fixed Effects Model*

Table 5 presents the results from the FE model. While the initial columns examine the spring DST period as a whole, columns 3–7 break spring DST down into three components: the first six days of DST, where the sleep effect should be felt

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[33] 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.

Table 5—FE Estimates of the Impact of DST on Fatal Crashes—Decomposing Spring DST

	All hours				Least light impacted	Morning	Evening
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spring DST	0.0319* (0.0165)	0.0307* (0.0165)					
First six days of DST			0.0565** (0.0231)	0.0559** (0.0230)	0.0574** (0.0272)	0.205*** (0.0514)	−0.0265 (0.0453)
Next eight days of DST			0.0254 (0.0201)	0.0240 (0.0201)	0.0289 (0.0234)	0.130** (0.0603)	−0.0812* (0.0450)
Remainder of spring DST			0.0142 (0.0197)	0.0123 (0.0197)	0.00907 (0.0230)	0.126** (0.0553)	−0.0588 (0.0429)
Fall DST	0.0228 (0.0249)	0.0221 (0.0247)	0.0218 (0.0250)	0.0211 (0.0248)	0.0446 (0.0303)	0.259*** (0.0709)	−0.159*** (0.0482)
ln(gas price)		−0.0457* (0.0246)		−0.0469* (0.0247)	−0.0449 (0.0289)	−0.101* (0.0557)	−0.0307 (0.0484)
Observations	3,341	3,341	3,341	3,341	3,341	3,341	3,341
Adjusted R^2	0.734	0.735	0.735	0.735	0.753	0.184	0.319

Notes: Dependent variable is the log fatal crashes; all specifications use day-of-year, day-of-week, and year dummies. Remainder of spring DST is an indicator variable equal to one if the day occurs after the first two weeks of DST and before July 1st. Fall DST is an indicator variable equal to one if the day falls under DST and occurs after June 30th. "Morning" is defined as +/− two hours from the average sunrise time in that location around both transitions; "Evening" is defined as +/− two hours from the average sunset time in that location around both transitions; least light impacted are the remaining hours. Robust standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Authors' calculations

most strongly;[34] the next eight days of DST, the longest any sleep study suggests a sleep impact could persist; and the remainder of spring DST, 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 3.2 percent increase in fatal crashes over the roughly one month of switching dates. The fall estimate is not significantly different from zero, again suggesting little impact of DST in the fall.[35] 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 ln(gasoline prices) to help control for driving patterns. Results are stable across columns and continue to suggest that DST causes a significant increase in crashes during the spring and has little 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 5.7 percent increase in fatal crashes, quite similar to the 5–6.5 percent increase found in the RD design. The

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[34] 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.

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

point estimate shrinks to an insignificant 2.5 percent during the next eight days and diminishes further to 1.4 percent 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 fuel prices in column 4 leaves results qualitatively identical.

Columns 5–7 explore these impacts across different times of day, reinforcing previous findings regarding both mechanisms. Column 5 uses just the subsample of hours least impacted by the light mechanism. Thus, the 5.7 percent increase in crashes during the first six days of DST provides a measure of the impact of just the sleep 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.

Ambient light is the primary mechanism during the remainder of spring DST and during the fall. In both of these time periods there is an increase in morning crashes (less light) and a decrease in evening crashes (more light). While it is anomalous that the next eight days experience a bigger reduction in evening crashes than the remainder of spring, these estimates are not significantly different. The larger magnitudes in the morning relative to the evening may indicate that ambient light is more important in the morning, perhaps through some interaction with type of trip or driver alertness that vary across the day. Coate and Markowitz (2004) similarly find a larger ambient light effect in the morning relative to the evening.

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. To test for this mechanism in a more direct manner, I turn to crash factors as reported by the investigating officer.

D. *Crash Factors*

While the precise cause of a fatal vehicle crash is often unknown, the investigating officer does file a report documenting crash factors. Such factors include poor weather conditions, driving under the influence, and drowsiness. Of these, drowsiness is the most likely to be measured with great error and underreporting. While alcohol in the bloodstream can be tested postmortem, drowsiness cannot. Imagine a single vehicle, single occupant, fatal crash in a remote area. Perhaps the driver fell asleep at the wheel, but without any direct evidence this cannot be reported. Only 2.61 percent of fatal crashes in my sample have drowsiness reported as a crash factor.

The problem with accurately measuring drowsiness as a crash factor has been recognized by the literature. To generate a true measure of the influence of fatigue on vehicle crashes, the Virginia Tech Transportation Institute, sponsored by the National Highway Traffic and Safety Administration, conducted the 100-Car Naturalistic Driving Study. This study tracked all driving occurring in 100 cars for over a one-year period. Video documentation of driver behavior for over 2,000,000 vehicle miles traveled provided unprecedented information regarding driver crash

Table 6—FE Estimates of the Impact of DST on Fatal Crash Causes

	Drowsiness (1)	Drunk driving (2)	Bad weather (3)
First six days of DST	1.310**	1.032	0.956
	(0.140)	(0.0362)	(0.108)
Next eight days of DST	1.003	1.042	0.944
	(0.108)	(0.0290)	(0.0996)
Remainder of spring DST	1.031	1.028	1.095
	(0.103)	(0.0285)	(0.109)
Fall DST	1.085	1.065*	0.895
	(0.134)	(0.0401)	(0.131)
Crash-factor prevalence	0.0261	0.336	0.109
Observations	3,341	3,341	3,341

Notes: Dependent variable is crash counts by reported crash factor; all specifications use day-of-year, day-of-week, and year dummies. Remainder of spring DST is an indicator variable equal to one if the day occurs after the first two weeks of DST and before July 1st. Fall DST is an indicator variable equal to one if the day falls under DST and occurs after June 30th. Reported estimates are incidence-rate ratios from a negative binomial model with tolerance of 0.0001 for convergence. Crash-factor prevalence is the share of all crashes that have that particular crash factor indicated by the reporting officer. Robust standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Authors' calculations

factors. They found that driving while drowsy was a factor in 22–24 percent of all crashes and near-crashes (Klauer et al. 2006). Using this measure, the best available on the importance of fatigue as a crash factor, suggests that drowsiness is reported in only one-ninth of its true incidence in the FARS data.[36]

Despite the likelihood that drowsiness is drastically underreported as a fatal vehicle crash factor, there is no reason to suspect drowsiness to be underreported any differently under DST relative to Standard Time. Therefore, to test as directly as possible for crash mechanisms, I use the FE model to analyze crashes attributable to drowsiness, as well as the competing crash factors of drunk driving and bad weather. Due to the presence of zeros in these data subsamples, I estimate the FE model using negative binomial regression and report the incident rate ratios.[37]

Table 6 presents the regression results. In column 1, the dependent variable is the number of crashes where drowsiness is a factor. The coefficient estimate of 1.31 implies that there is a 31 percent increase in crashes attributable to drowsiness during the first six days of DST. While this estimate is quite noisy (95 percent confidence interval of 1.06–1.61), it is still useful to back out how much of the total increase in fatal crashes this channel could explain. Accounting for the underreporting in fatal vehicle crashes by using the estimate from Klauer et al. (2006), a back-of-the-envelope calculation implies that this channel could be responsible for

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[36] This statement assumes that drowsiness would also be a factor in 22–24 percent of all *fatal* crashes. This study considers all crashes as the sample of just 100 vehicles does not allow for the separate consideration of fatal crashes.

[37] A comparable version of Table 5 using negative binomial regression is shown in online Appendix Table B-8 for completeness.

a 7 percent increase in overall fatal crashes, approximately the entire magnitude of the spring DST effect.[38]

Columns 2 and 3 present the results for drunk driving and bad weather related crashes, respectively. In both cases, there is no significant change in the incident rate of crashes related to these factors during the spring, suggesting that these causes are not driving the overall increase. However, the point estimates for drunk driving are always positive and the fall impact is marginally significant. This could be anamolous, or could indicate a small increase in drunk driving associated with DST—perhaps through the ambient light channel if people are more likely to drink when it is light in the evening.

This analysis complements the indirect tests for each mechanism and contributes to the balance of evidence that points strongly towards DST increasing fatal crash risk through the mechanism of sleep deprivation. In the next section, I explore whether this effect has changed over time.

V. Evidence from Earlier Years

The existing literature has reached conclusions regarding the impact of DST on fatal vehicle crashes that occasionally conflict with my findings and each other. Notably, many previous studies have found a net reduction in crashes due to the ambient light mechanism (Ferguson et al. 1995; Broughton, Hazelton, and Stone 1999; Coate and Markowitz 2004; and Sood and Ghosh 2007), something I do not find evidence of during the 2002–2011 sample period. Further, existing studies reach contradictory conclusions about whether sleep deprivation increases crash risk—something I find strong evidence of.[39] In an effort to reconcile my results with the existing literature, I consider the rest of the FARS data series, which spans 1976–2001 and covers the time frames used in previous studies.[40]

Over the 1976–2001 period, there was one key change to the practice of DST. During the first portion of the sample (1976–1986), DST began on the final Sunday of April. In 1986, the Uniform Time Act was amended to extend DST such that the transition would occur on the first Sunday in April, effective starting in 1987. This extension is quite similar to the 2007 policy change, in that it added three to four weeks of DST in the spring. However, in this instance there was no change to the fall transition date. Using this natural experiment covering the spring transition, I conduct the same day-of-year fixed effects analysis from equation (2) using the historical 1976–2001 sample.

Table 7 shows the results of this analysis juxtaposed with the results for the baseline sample of 2002–2011.[41] Recall that the remainder of spring DST provides a measure of the light mechanism, as any impact through the sleep channel should

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[38] Back-of-the-envelope calculation assumes that drowsiness is truly a factor in 23 percent of all fatal crashes and that the first 6 days of DST increase the volume of these crashes by 31 percent.

[39] Coren (1996) and Varughese and Allen (2001) find an increase in crashes on the Monday following the spring transition into DST, while Vincent (1998), Sood and Ghosh (2007), and Lahti et al. (2010) suggest no effect.

[40] To remain consistent with Sood and Ghosh (2007), I omit the first year of FARS data (1975) because it was subject to alternative DST cutoffs following the 1973 oil crisis.

[41] Gasoline prices are unavailable before 1990. For the sake of comparability, I do not use gasoline prices as a control in either sample.

Table 7—FE Estimates of the Impact of DST on Fatal Crashes across Time

	2002–2011		1976–2001	
	All hours (1)	Least light impacted (2)	All hours (3)	Least light impacted (4)
First six days of DST	0.0565**	0.0580**	0.0164	0.0511***
	(0.0231)	(0.0273)	(0.0146)	(0.0162)
Next eight days of DST	0.0254	0.0302	−0.0125	0.0202
	(0.0201)	(0.0234)	(0.0131)	(0.0146)
Remainder of spring DST	0.0142	0.0109	−0.0388***	−0.00433
	(0.0197)	(0.0229)	(0.0131)	(0.0146)
Observations	3,341	3,341	8,691	8,691
Adjusted R^2	0.735	0.753	0.772	0.789
p-value for test of no difference between first 6 days and remainder of spring DST	0.0698	0.108	0.0004	0.001

Notes: Dependent variable is log fatal crashes; all specifications use day-of-year, day-of-week, and year dummies. Remainder of spring DST is an indicator variable equal to one if the day occurs after the first two weeks of DST and before July 1st. Columns 1 and 2 include indicator for fall DST (no fall extension occurred during 1976–2001). Least light impacted hours exclude +/− two hours from the average sunrise and sunset times around both transitions for that sample period. Robust standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Authors' calculations

have dissipated after two weeks. In the baseline sample, there is no significant impact through the light channel. However, in the 1976–2001 sample there is a statistically significant reduction in crashes of 3.9 percent, suggesting a net effect through the light mechanism consistent with the previous literature. This notion is reinforced by examining column 4, the least light-impacted hours. During the hours furthest from sunrise and sunset, there is no significant impact during the remainder of spring DST indicating that this 3.9 percent increase is driven solely by the hours closest to sunset and sunrise, which are treated by the light mechanism. The reason this net effect has dissipated over time is likely due to changes in the daily crash profile. During the 1976–2001 period, crashes were much more frequent in the evening than the morning. While crashes remain more frequent in the evening, the morning-evening crash differential has shrunk significantly over time, mitigating the net benefit of this channel (see online Appendix Figure B-4).

Next, consider the sleep mechanism, which should be most strongly present during the first six days of DST. The estimate in column 3 is a statistically insignificant 1.6 percent increase. However, during the first six days of DST, the light channel is also active. To back out an estimate of the sleep mechanism, I take the difference in the effect during the first six days of DST (sleep and light active) and the remainder of spring DST (light active). This difference implies a 5.5 percent increase in crashes through the sleep channel, with an F-test for equal effects during the first six days of DST and the remainder of spring DST rejected at the 1 percent level. Column 4 is also useful for considering the sleep mechanism, as it effectively isolates hours that are untreated by the light mechanism. Here, the coefficient on

TABLE 8—RD ESTIMATES OF THE IMPACT OF ENTERING DST ON FATAL CRASHES ACROSS TIME (*Spring*)

	2002–2011			1976–2001		
	(1)	(2)	(3)	(4)	(5)	(6)
DST	0.0649*** (0.0231)	0.0499*** (0.0176)	0.0626*** (0.0215)	0.0398** (0.0176)	0.00264 (0.0116)	0.0420** (0.0180)
Bandwidth selector	CCT	IK	CV	CCT	IK	CV
Observations	550	966	670	1,014	2,155	962

Notes: Dependent variable is log fatal crashes demeaned by day-of-week and year. All specifications use 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. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2014a); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Authors' calculations

the first six days of DST is a statistically significant 5.1 percent, providing further evidence that the sleep mechanism was present, and of similar magnitude during the historical sample.

Table 8 shows the results of estimating the RD model for the baseline and historical samples. The RD estimates are consistent with the finding from the FE model that the net impact of entering DST in the spring is smaller during the historical sample. The opposing nature of the sleep and light effects in the historical sample likely contributed to the mixed results regarding the sleep mechanism seen in the existing literature. Estimates of the sleep mechanism that assume the absence of an impact from the light mechanism are biased downwards.

VI. 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 5–6.5 percent.

I employ four 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.[42] All four tests suggest that the sleep deprivation

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[42] Ambient light likely contributed to a net reduction in fatal crashes historically, consistent with the findings of Ferguson et al. (1995); Broughton, Hazelton, and Stone (1999); Coate and Markowitz (2004); and Sood and Ghosh (2007). However, this net effect seems to have disappeared, at least partly due to changes to the daily profile of fatal crashes across time.

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 the spring transition into DST caused over 30 deaths annually at a social cost of $275 million.[43]

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 forthcoming) and encouraging exercise (Wolff and Makino 2013).[44] 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; and Gibson and Shrader 2014). Although fatal vehicle crashes are an extreme measure of productivity, driving is an activity that over 90 percent of American workers 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. The results imply that a one hour sleep loss increases the probability of being in a drowsiness-related fatal crash by 46 percent.[45] 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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[43] Social cost is calculated as follows: multiplying the 5.6 percent 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 yields an extra 30.2 deaths per. Applying the Department of Transportation's $9.1 million value of a statistical life, this a $275 million social cost per year.

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

[45] This calculation uses the 31 percent increase in drowsiness-related fatal crashes during the first six days of DST, scaled up from the 40-minute sleep loss associated with the spring transition date to a 1-hour sleep loss by multiplying by 1.5.

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