Testing for Robustness in the Relationship between Fatal Automobile Crashes and Daylight Saving Time

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This paper considers an article by Sood & Ghosh (2007) concerning the effects of Daylight Saving Time on fatal automobile crashes in the United States. They analyze distinct short and long run effects of DST on crash frequency between 1976 and 2003. The impetus for reconsidering their analysis is a federally mandated 2007 change in Daylight Saving Time, expanding its duration by "springing forward" earlier and "falling back" later. This natural experiment, similar to one in 1987 looked at by Sood & Ghosh (2007), allows for control over day of the month and seasonal effects in examining 2004–2010 crash data. After replicating the findings for the 1976–2003 period covered in the original study, the analysis of the new sample overcomes contamination problems to cautiously support the long term fatal crash-saving effects of DST, particularly in the fall, as well as supporting the lack of statistically significant evidence of short term DST fatal crash-causing.

1. INTRODUCTION

Daylight Saving Time (DST) is a practice used in many countries whereby clocks are set an hour forward during summer months and then reset an hour backward during the winter months. The goal is to better align hours of human activity with hours of daylight, an effort aimed to conserve energy through the transfer of daylight from mornings to evenings. While the true energy yields of DST are still questioned (for example, see Kandel & Sheridan (2007)), the side effects of the convention are numerous, and in particular recent policy changes in the United States have made the situation more interesting.

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Table 1: Beginning and ending dates of DST in the US for states without an exemption statute[2]

Year	DST Begins (2:00 am)	DST Ends (2:00 am)
Begins last Sunday in April, ends last Sunday in October		
1976	04/25	10/31
1977	04/24	10/30
1978	04/30	10/29
1979	04/29	10/28
1980	04/27	10/26
1981	04/26	10/25
1982	04/25	10/31
1983	04/24	10/30
1984	04/29	10/28
1985	04/28	10/27
1986	04/27	10/26
Begins first Sunday in April, ends last Sunday in October		
1987	04/05	10/25
1988	04/03	10/30
1989	04/02	10/29
1990	04/01	10/28
1991	04/07	10/27
1992	04/05	10/25
1993	04/04	10/31
1994	04/03	10/30
1995	04/02	10/29
1996	04/07	10/27
1997	04/06	10/26
1998	04/05	10/25
1999	04/04	10/31
2000	04/02	10/29
2001	04/01	10/28
2002	04/07	10/27
2003	04/06	10/26
2004	04/04	10/31
2005	04/03	10/30
2006	04/02	10/29
Begins second Sunday in March, ends first Sunday in November		
2007	03/11	11/04
2008	03/09	11/02
2009	03/08	11/01
2010	03/14	11/07

DST in the United States was first organized at the federal level with the passing of the Uniform Time Act of 1966, mandating that DST should begin on the last Sunday of April and should end on the last Sunday of October. However, any community that wishes to be exempt from the practice can simply pass an ordinance to do so: as a result, Arizona, Hawaii, and all US territories (and Indiana prior to 2006) have refrained from implementing DST. On the other hand, if a state or territory wishes to follow DST, it must start and stop on the dates set down by the government. After over a year of extended DST surrounding the 1973 Arab Oil Embargo, from 1976 to 1986 the United States began DST on the last Sunday in April and ended DST on the last Sunday in October. In 1986, DST was changed to start on the first Sunday in April, while the Energy Policy Act of 2005 further changed DST to begin on the second Sunday in March and end on the first Sunday in November, taking effect starting in 2007 (Aldrich, 2005).

[2]Source: timeanddate.com.

These shifts in the timing of DST (see Table 1) are the principal means of studying its effects on automotive accidents in this paper.

There is a growing body of literature regarding the effects of DST on the frequency of automobile crashes. A majority of the literature reports beneficial (meaning that DST decreases the overall number of automotive accidents) long run effects, while there is considerable controversy over the short run effects of DST (Huang, 2010). In the long run, it is thought that moving an hour of daylight from the morning, when fewer people are driving, to the evening, when more people are driving, increases visibility and therefore should decrease accident rates (Ferguson, 1995). Huang (2010) supports these findings, along with numerous other articles demonstrating how many accidents year-long DST could save (for example, Broughton et al. (1999) claim that year-round DST could have prevented up to 973 fatal automotive accidents between 1987 and 1991).

In the short run, however, the situation gets more complicated. Although the benefits of increased visibility still hold, as in the long run, disruptions in the circadian rhythm associated with the loss of an hour of sleep in the spring increase drowsiness, a major cause of traffic accidents (Ledger, 1994). Coren (1996a) (1996b) connects the dots, showing that the small sleep loss associated with DST in the spring can result in an increased likelihood of fatal automotive accidents. Furthermore, Hicks et al. (1998) demonstrate that increased sleepiness interacts with alcohol, increasing the percentage of fatal alcohol-related accidents in the two weeks following the DST change in both the spring and the fall.

Sood & Ghosh (2007) analyze the above two effects in further detail by exploiting a 1987 US change in DST activation date as a natural experiment. As DST implementation before 1987 was scattered, they use the change in DST activation as an opportunity to let the years after the change act as a treatment group and the years before the change act as a control group to test the effects of DST. They use 28 years of United States fatal crash data in the analysis, and further assess a shorter seven year interval within the larger period to verify robustness of findings.

In this paper, I further develop these tests of robustness. I use the same data source and methodology, beginning by replicating the major findings of Sood & Ghosh (2007) to show the viability of the statistical framework. An additional seven year period, from 2004 to 2010, is then examined, in an effort to determine if this new interval exhibits the same general results as the replicated one.

The examination consists of a series of OLS and Poisson regressions to determine the respective long and short term effects of DST on crash incidence. The results obtained generally support the findings of Sood & Ghosh (2007). In the long term, the spring 2004–2010 results suffer from contamination problems, but the fall 2004–2010 data does exhibit a 4% increase in fatal accidents due to the removal of DST. In the short term, none of the findings for the 2004–2010 period point to statistically significant evidence (at the 10% level) of an increase in fatal accidents following the implementation of DST.

The rest of this paper proceeds in the following manner: Section 2 introduces the FARS data set, Section 3 describes the statistical methodology, and Section 4 displays the regression results. Section 5 provides concluding remarks.

2. DATA

The data come from the Fatality Analysis Reporting System (FARS), a database containing 36 years of fatal accident data from 1975–2010. This data set is provided by the National Highway Traffic Safety Administration, established by the Highway Safety Act of 1970. FARS documents "all qualifying fatalities that occurred within the 50 States, the District of Columbia, and Puerto Rico," where qualifying means that the crash must involve a vehicle on public routes and a death within 30 days of the accident (U.S. Department of Transportation, 2010). As the goal of this paper is to test the results of Sood & Ghosh (2007) for an extended time interval, I exclude the year 1975 from the analysis (DST was retained during this period due to the oil embargo mentioned in the introduction) along with data for Arizona, Hawaii, Indiana, and Puerto Rico (at least parts of which didn't use DST for the majority of the time period), as they have. Although Sood & Ghosh (2007) include Alaska in their data set, as it does observe DST, I considered excluding as during the summer Alaska has nights without significant darkness (Alaska.com), a problem when studying DST's effect on driver visibility. I return to this issue later, during the regression analysis.

Following the example of Sood & Ghosh (2007), I use the "accident" files from the FARS database, containing "information about crash characteristics and environment conditions at the time of the crash." Importantly, the file contains exactly one record per crash, making it simple to determine the number of fatal accidents per day (or week). As our interest is the effect of DST on the number of accidents, not on their severity, the number of fatalities caused by each respective crash is ignored. A small number of incidents (about 0.1% in the 1976-2003 data, according to Sood & Ghosh (2007)) have missing dating information and therefore must be excluded.

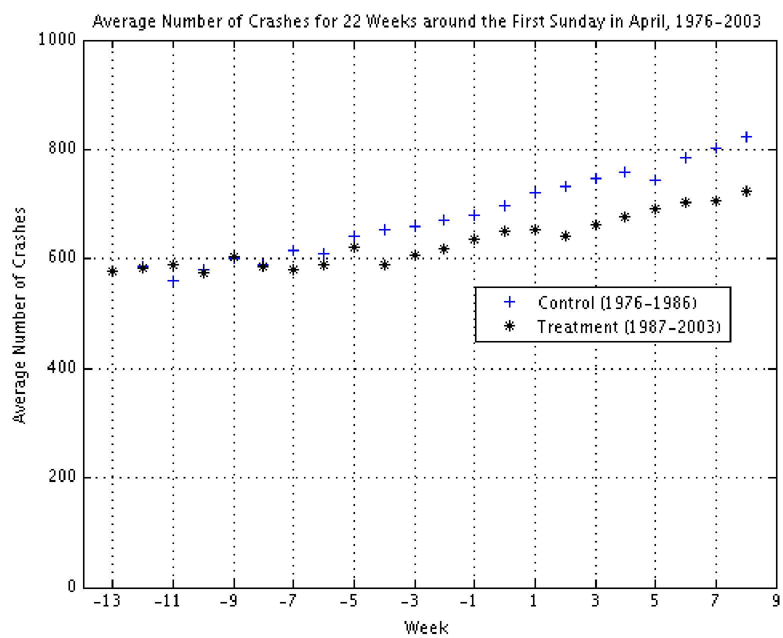
3. METHODOLOGY

Separate approaches are used to evaluate the effects of DST in the long and short run; we describe each procedure independently.

3.1. Techniques for measuring the long run effects of DST

The 1987 required change in DST start date from the last Sunday of April to the first Sunday of April provides the framework for the statistical analysis in the replication case. Sood & Ghosh (2007) use 13 weeks of data before the time change occurs in treatment years (discussed below) and 9 weeks of data afterwards, and for our replication regressions we follow suit. The average crash count for each of these weeks is reported in Figure 1. Following Sood & Ghosh (2007), our strategy is to compare counts from the weeks after the change in DST to the weeks before, and then further compare results from the treatment years (1987-2003) to the control years (1976-1986), where the "control years" act as controls due to DST starting later (or not at all in the case of many states between 1976 and 1986) in that period. To be clear, since DST starts on the first Sunday of April in the treatment years, we regard the "9 weeks after" in both the control and treatment years to be weeks after the first Sunday of April, despite DST starting later in the control years. The result is that we estimate (via OLS) the differences-in-differences estimator (see, for example, Stock & Watson (2006)) detailed below.

Figure 1[3]



$$[\log(\text{treat\_avg\_s87}) - \log(\text{ctrl\_avg\_s76})]_i = \alpha + \beta_1 \cdot \text{wa\_dummy}_i + u_i \tag{1}$$

To produce the above regression, weekly fatal crashes in the treatment and control periods for each of the 22 weeks in our sample are averaged, yielding a single weekly

[3]Generated using MATLAB. Note that crashes refer to fatal traffic accidents, and the abscissa is measured in weeks surrounding the first week after the time change (week zero here).

time series. An observation i consists of the log of average crashes in treatment years for a given week minus the the log of average crashes in control years for the same week (relative to the start of DST). The "log of the means" is used as opposed to the "mean of the logs" (which would yield the generalized f-mean) for simplicity and consistency: both means would work to produce the log-linear regression (with slightly different interpretations), but the "log of the means" fulfills the purpose nicely and is consistent with the methods used by Sood & Ghosh (2007), helpful for the replication part of this paper. Note that "s87" and "s76" in our regression simply denote the larger period from which the statistics are drawn (so "s87" statistics are drawn from the 1987–2003 period). The regressor is a simple "week after" (WA) dummy, which is one for any week after the first Sunday in April (representing a week after the start of DST in the treatment years). β_1 represents the causal effect of DST on the prevalence of fatal accidents "under the identifying assumption that in the absence of DST, treatment and control years would experience a similar change in crash counts in the weeks before and after the first Sunday of April" (Sood & Ghosh, 2007).

Again following the example of Sood & Ghosh (2007), we further implement a second "individual week" (IW) regression, utilizing the same regressand but different regressors:

$$[\log(\text{treat\_avg\_s87}) - \log(\text{ctrl\_avg\_s76})]_i = \alpha + \sum_{k=1}^{9} \beta_k \text{wa}_k\text{\_dummy}_i + u_i \qquad (2)$$

Here, the regressors are kth "week after" dummies for $k = 1, \ldots, 9$, with a coefficient β_k estimating the impact of DST in the kth week after the time change. All of the regressions are implemented using the GRETL statistical package, and all of the long run regressions use HAC (bandwidth 2, Bartlett kernel) standard errors.

Following the above replication experiments, I then produced similar regressions for the 2004–2010 time period. The 2007 change in DST start and end dates motivates the analysis: since changes were made in both the spring (from the first Sunday in April to the second sunday in March) and fall (from the last Sunday in October to the first Sunday in November), both time changes can be studied.

For the spring change, due to the backwards shift in the timing of DST to the second week in March (and therefore less days in the first part of the year), we only use 10 weeks before the time change in our sample. Here, again, we compare the weekly crash averages from the 2007–2010 treatment years to the 2004–2006 control years; these averages are shown graphically in Figure 2. The "9 weeks after" refers to weeks after the second Sunday in March. The regressions are as before:

$$[\log(\text{treat\_avg\_s07}) - \log(\text{ctrl\_avg\_s04})]_i = \alpha + \beta_1 \cdot \text{wa\_dummy}_i + u_i \tag{3}$$

$$[\log(\text{treat\_avg\_s07}) - \log(\text{ctrl\_avg\_s04})]_i = \alpha + \sum_{k=1}^{9} \beta_k \text{wa}_k\text{\_dummy}_i + u_i \tag{4}$$

For the fall change, things are slightly more complicated. The fall change extends the DST end date forwards from the last week in October to the first week in November, so we must use 2004–2006 as the treatment period and 2007–2010 as the control period. This is because during the first week of November, when DST timings are different between the years, 2004–2006 lacks DST while 2007–2010 retains it, and so in fact these regressions test the opposite impact of DST that the spring regressions test. Therefore, we use the week following the last Sunday in October as our first week after DST, in both periods. Due to date issues we can only use 8 weeks after the time change in our sample (no data in 2011 to add on to the end of 2010), but we use 16 weeks before to improve the power of our tests. The weekly crash averages in both the treatment and control years (which, recall, are reversed in the fall relative to the spring tests) are shown in Figure 3. It is interesting to note that between 2004 and 2010 (and to some extent between 1976 and 2003), the later years' weekly fatal accident averages are invariably lower than in the previous years. This trend has no bearing on the impact of DST, and is controlled in the calculation of the differences-in-differences estimator.

Figure 2[4]

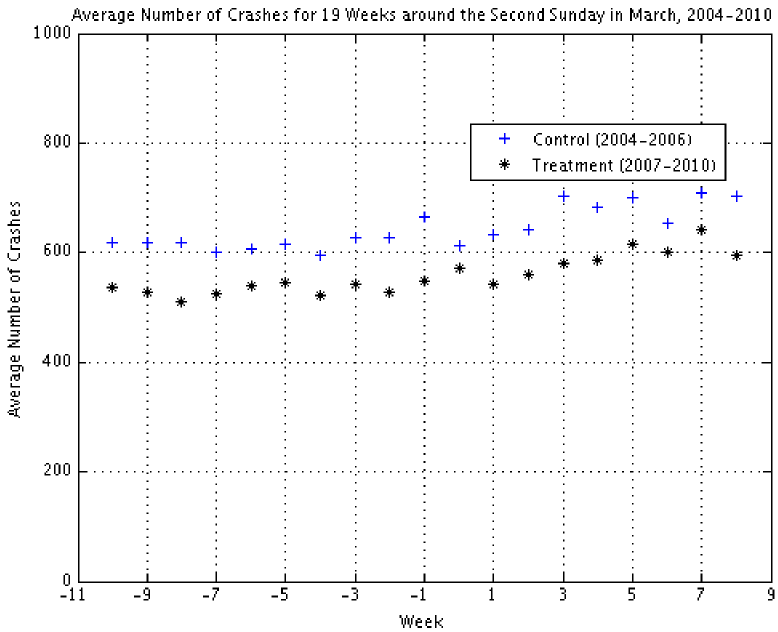
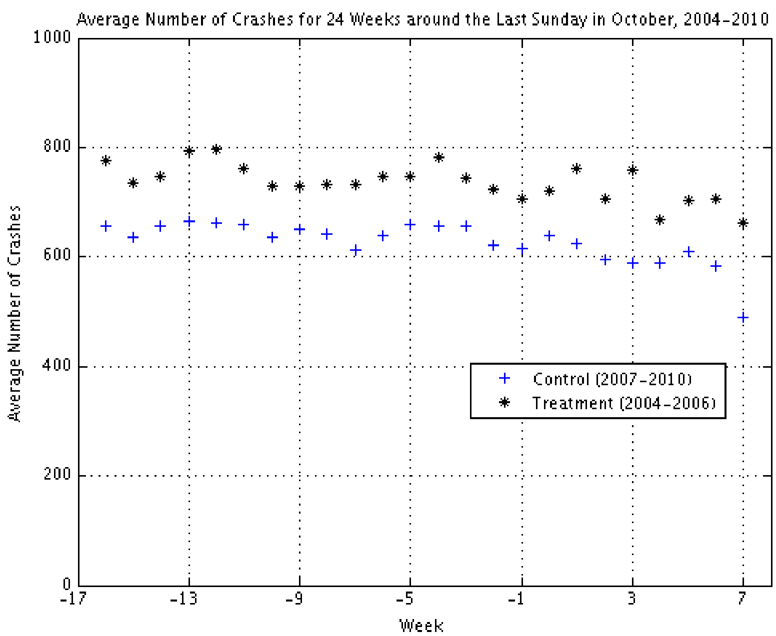


Figure 3[5]



[4]Generated using MATLAB. Note that crashes refer to fatal traffic accidents, and the abscissa is measured in weeks surrounding the first week after the time change (week zero here).

[5]Generated using MATLAB. Note that crashes refer to fatal traffic accidents, and the abscissa is measured in weeks surrounding the first week after the time change (week zero here). Further note the reversal of treatment and control years relative to the other experiments.

The fall regressions are shown below:

$$[\log(\text{treat\_avg\_f04}) - \log(\text{ctrl\_avg\_f07})]_i = \alpha + \beta_1 \cdot \text{wa\_dummy}_i + u_i \tag{5}$$

$$[\log(\text{treat\_avg\_f04}) - \log(\text{ctrl\_avg\_f07})]_i = \alpha + \sum_{k=1}^{9} \beta_k \text{wa}_k\text{\_dummy}_i + u_i \tag{6}$$

There are a few other important factors considered by Sood & Ghosh (2007) that haven't yet been taken into account. In all of the long run regressions, the control years "catch up" to the treatment years after a few weeks, in the sense that DST does start in the control years at some point. This contamination was addressed in the original treatment by Sood & Ghosh (2007) by distinguishing between the first three weeks after the time change and the rest of the weeks (for the first regression). Replicating this correction in the new time period regressions didn't provide any significant aid to the regressions. Sood & Ghosh (2007) also split up their regressions into "vehicular" and "pedestrian" fatal accidents; the focus here is on overall trends, so I ignored this component of their examination.

3.2. **Techniques for measuring the short run effects of DST**

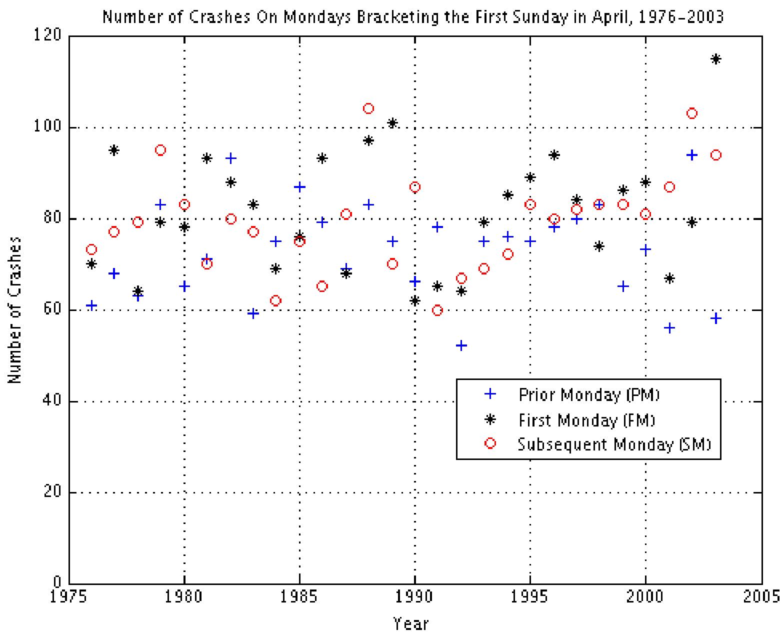
The same data and framework used to study the long run effects will also be used to measure the short run effects of DST, building again on the changes in the time that DST takes effect in 1987 and 2007. Sood & Ghosh (2007) take three pieces of data from each year in the sample: crash counts on the Mondays immediately following the (treatment year) Sundays on which DST changes (which I will call "first mondays" or FMs), and crash counts on both the prior (PMs) and subsequent Mondays (SMs). So for the 1976–1987 data set, data is taken from the Monday immediately following the first Sunday in April and both the prior and subsequent Mondays. The following approach they follow is the intuitive one, and identical to the regressions used in this paper. Sood & Ghosh (2007) average these Mondays by treatment or control period, where for the 1976–2003 data, the treatment period is 1987-2003 (as in the long run scenario). Following this, they create the ratios FM/PM and FM/(PM or SM) (where the second ratio is the first Monday averages over the averages of the prior and subsequent Mondays) to control for seasonal/week of the month effects, whereby a specific week within a month might simply have higher average crash rates than other weeks. Finally, Sood & Ghosh (2007) estimate the short run effect of DST on fatal accidents by taking two final ratios, that of the treatment ratio over the control ratio (a "ratio of ratios") for each of the FM/PM and FM/(PM or SM) ratios. This, similar to the long run strategy, is a differences-in-differences technique for estimating the impact of DST. I generate and report these ratios alongside the regression statistics in the results section of the paper.

The regression technique used is also outlined by Sood & Ghosh (2007). For the replication of their results, I take panel data of the "FMs", "PMs", and "SM"s from 1976–2003, and then set up two separate panels: one including only FMs and PMs, and one including all three sets of Monday averages. The total fatal crashes for each Monday by year are included in Figure 4. An observation it consists of the number of crashes on a PM ($t = 1$), FM ($t = 2$), or SM ($t = 3$; included in regression (8) but not in (7), seen below) in year i. Note that "s76" in our regression simply denotes the larger period from which the statistics are drawn (so "s76" statistics here are drawn from the 1976–2003 period). The regression formulas are shown below in equations (7) and (8).

$$\begin{aligned}\text{fm\_pm\_s76}_{it} = \alpha + \beta_1 \text{fm\_dummy}_{it} + \beta_2 \text{treat\_dummy}_{it} + \\ + \beta_3 (\text{fm}\times\text{treat})\text{\_dummy}_{it} + \sum_{k=1}^{28} \gamma_k \text{year}_k\text{\_dummy}_{it} + u_i \qquad (7)\end{aligned}$$

$$\begin{aligned}\text{fm\_pm\_or\_sm\_s76}_{it} = \alpha + \beta_1 \text{fm\_dummy}_{it} + \beta_2 \text{treat\_dummy}_{it} + \\ + \beta_3 (\text{fm}\times\text{treat})\text{\_dummy}_{it} + \sum_{k=1}^{28} \gamma_k \text{year}_k\text{\_dummy}_{it} + u_i \qquad (8)\end{aligned}$$

Figure 4[6]



Note that we perform Poisson regressions in the short run situation, suitable to the accident count data used. The regressors are a first Monday dummy (one if the observation is a FM), a treatment dummy (one if the observation is in the treatment, in this case 1987–2003), an interaction dummy between the two (one if the previous two conditions are met), and year fixed effects. Note that we let GRETL sort out the collinearity issues, removing excess dummies included above for the sake of clarity. The important coefficient here is β_3, which measures the short run impact of DST on the prevalence of fatal accidents (where we note that to get the intuitive ratio of ratios mentioned initially, we simply add β_3 to unity). All of the short run regressions use QML standard errors.

As in the long run situation, I accompany the replication of the 1786–2003 experiments by Sood & Ghosh (2007) with an original analysis of the spring and fall 2004–2010 situation. I use the same regression techniques, merely adjusted for the new

[6]Generated using MATLAB. Note that crashes refer to fatal traffic accidents.

Figure 5[7]

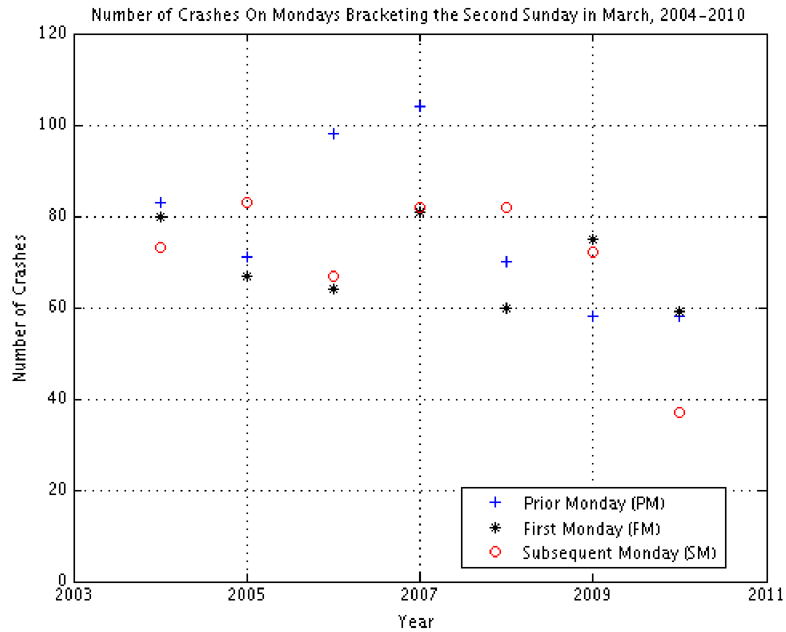
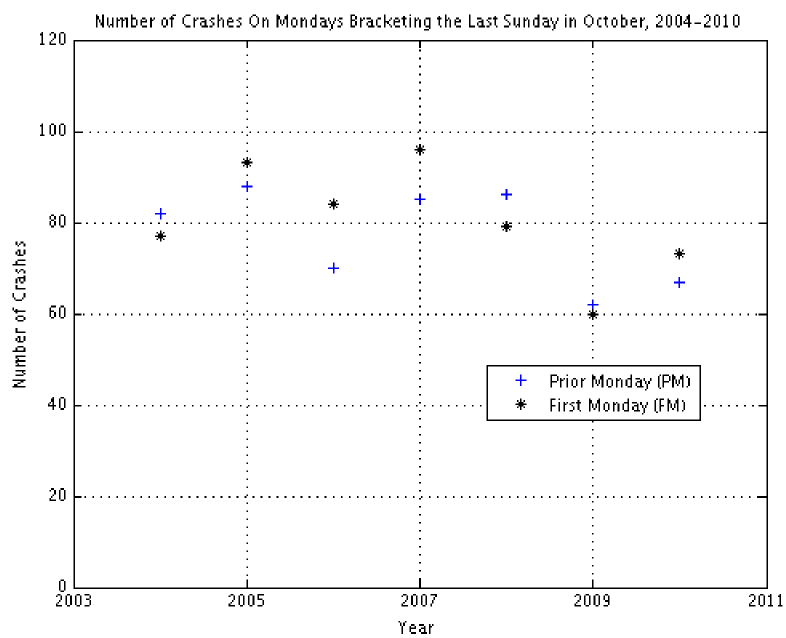


Figure 6[8]



[7]Generated using MATLAB. Note that crashes refer to fatal traffic accidents.
[8]Generated using MATLAB. Note that crashes refer to fatal traffic accidents.

data and time changes. The analysis covers both spring and fall time changes, as DST expanded both backward in the spring and forward in the fall.

For the spring DST activation change between 2004 and 2010, data is collected for the Mondays immediately following the second Sundays in March as well as for both the prior and subsequent Mondays. The fatal crash totals for each Monday in this interval are included in Figure 5. The treatment period is 2007–2010, while the control period is 2004–2006, as in the long run. The regressions are as before:

$$\begin{aligned} \text{fm\_pm\_s04}_{it} = \alpha + \beta_1 \text{fm\_dummy}_{it} + \beta_2 \text{treat\_dummy}_{it} + \\ + \beta_3 (\text{fm}{\times}\text{treat})\text{\_dummy}_{it} + \sum_{k=1}^{28} \gamma_k \text{year}_k\text{\_dummy}_{it} + u_i \end{aligned} \tag{9}$$

$$\begin{aligned} \text{fm\_pm\_or\_sm\_s04}_{it} = \alpha + \beta_1 \text{fm\_dummy}_{it} + \beta_2 \text{treat\_dummy}_{it} + \\ + \beta_3 (\text{fm}{\times}\text{treat})\text{\_dummy}_{it} + \sum_{k=1}^{28} \gamma_k \text{year}_k\text{\_dummy}_{it} + u_i \end{aligned} \tag{10}$$

For the fall DST activation change between 2004 and 2010, we note that as in the long run situation the treatment and control periods must be "reversed" relative to the other analyses, so in fact the treatment period is 2004–2006 and the control period is 2007–2010. Data is collected for the Mondays immediately following the last Sundays in October as well as for the prior Mondays. Note that no data is collected on the subsequent Mondays, as DST was only extended by one week in the fall, and so there would be a serious problem of contamination if one were to run the same secondary regression with FM/(PM or SM) as in the spring cases. The fatal crash totals for each Monday in this interval are included in Figure 6. Note that, as before, due to the "reversed" nature of the fall test relative to the spring test, the fall coefficients measure the opposite impact of DST relative to the spring coefficients ("without DST" treatment compared to "with DST" control in the fall, "with DST" treatment compared to “without DST” control in the spring). The regression is shown below:

$$\begin{aligned}\text{fm\_pm\_f04}_{it} &= \alpha + \beta_1 \text{fm\_dummy}_{it} + \beta_2 \text{treat\_dummy}_{it} + \\ &\quad + \beta_3 (\text{fm}\times\text{treat})\text{\_dummy}_{it} + \sum_{k=1}^{28} \gamma_k \text{year}_k\text{\_dummy}_{it} + u_i \end{aligned} \tag{11}$$

The other important point with the short run data regards the unfinished question of whether or not to include Alaska in the overall data set, mentioned earlier. Due to its periods of 24 hour daylight in the summer, it seemed somewhat unreasonable to include it with the rest of the continental US, despite its inclusion by Sood & Ghosh (2007). However, I tested the spring and fall short run analyses with and without data from Alaska and it made little difference in the regressions, likely due to the relatively small number of fatal crashes in the state. As such I chose to include the state in the overall data set for the sake of consistency with the original paper.

4. RESULTS

The discussion is divided into two parts, analyzing the long term and short term effects of DST on fatal accident prevalence individually.

4.1. Results on the long term effects of DST

The results of the OLS regressions on the long term effects of DST are presented in Table 2. The coefficients of the “WA” regressors measure percentage increases in crash prevalence due to DST activation in the treatment period (or due to DST removal in the fall tests); a negative coefficient represents a decline in crash incidence due to DST (or due to DST removal in the fall).

In the spring 1976–2003 regressions, we compare our findings to those of Sood & Ghosh (2007). Although they focus on differences between pedestrian and vehicular crashes, the principal finding of a significant decline in fatal accidents in the weeks following the activation of DST in the treatment years is consistent with the results of

Table 2: OLS regressions of log average differences in weekly crash counts on various dummies[9]

	1976–2003: Spring		2004–2010: Spring		2004–2010: Fall	
	(1)	(2)	(3)	(4)	(5)	(6)
Regressor/Test Statistic	WA	IW	WA	IW	WA	IW
Constant	-0.0322241^{*}	-0.0322241^{*}	-0.151490^{***}	-0.151490^{***}	0.148545^{***}	0.148545^{***}
	(0.0168153)	(0.0168153)	(0.00960868)	(0.00960868)	(0.00442720)	(0.00442720)
WA Dummy	-0.0750621^{***}		0.0194947		0.0393924^{**}	
	(0.0191450)		(0.0174202)		(0.0185429)	
1st WA		-0.0341642^{*}		0.0858606^{***}		-0.0291262^{***}
		(0.0168153)		(0.00960868)		(0.00442720)
2nd WA		-0.0630207^{***}		-0.00470199		0.0502646^{***}
		(0.0168153)		(0.00960868)		(0.00442720)
3rd WA		-0.101109^{***}		0.0121566		0.0260781^{***}
		(0.0168153)		(0.00960868)		(0.00442720)
4th WA		-0.0886932^{***}		-0.0419143^{***}		0.103697^{***}
		(0.0168153)		(0.00960868)		(0.00442720)
5th WA		-0.0797506^{***}		-0.000770135		-0.0263770^{***}
		(0.0168153)		(0.00960868)		(0.00442720)
6th WA		-0.0401778^{**}		0.0258978^{**}		-0.00699201
		(0.0168153)		(0.00960868)		(0.00442720)
7th WA		-0.0786620^{***}		0.0643862^{***}		0.0437843^{***}
		(0.0168153)		(0.00960868)		(0.00442720)
8th WA		-0.0920015^{***}		0.0515244^{***}		0.153810^{***}
		(0.0168153)		(0.00960868)		(0.00442720)
9th WA		-0.0979797^{***}		-0.0169873		
		(0.0168153)		(0.00960868)		
$\overline{R}^2$	0.500348	0.312525	0.026980	0.389968	0.150061	0.748301
Log-likelihood	42.43305	44.54189	37.98174	48.45912	43.98046	63.17956
Observations	22	22	19	19	24	24

the regressions shown here. Every coefficient in the first two regressions is negative, and most are highly significant at the 1% level. Fatal accidents decline by about 7.5% overall in the long run after DST goes into effect, peaking within the 9 week period around the fourth and ninth weeks at about 9% and 10%, respectively (mirroring the results of Sood & Ghosh (2007) closely).

[9]OLS regressions generated in GRETL. Number above each regression name corresponds to the regression's equation in Section 3. "WA" stands for a "week after" regression, while "IW" stands for a "individual week" regression. Standard errors included in parentheses under the values of the coefficients. *** = significant at 1%; ** = significant at 5%; * = significant at 10% (generated using a two-tailed p-test).

In the spring 2004–2010 regressions, the outcome is markedly different. The overall coefficient is slightly positive (though not significant at the 10% level) in regression (3), denoting a crash increase after the activation of DST in the treatment group. At the individual week level, there are several significant positive coefficients, most markedly in the first week after the start of DST (8.6% increase in incidence, significant at the 1% level). Although seemingly odd, these statistics are not entirely unexplainable. First, the first regression was over 22 weeks, averaging using 28 years of data; these regressions are over only 19 weeks, averaging using merely 7 years of data. If we consider the 7 year weekly regressions of Sood & Ghosh (2007), we find similar highly positive constants (for example, a 6% increase in vehicular crashes at the 1% significance level) seemingly out of place. However, their results overall are more reasonable than those of the spring 2004–2010 regressions. The difference is the result of contamination; during the 1987–2003 treatment years, significantly more states relative to the 1976–1986 control years implemented DST. During the 2007–2010 treatment years, on the other hand, a similar number of states relative to the 2004-2006 control years implemented DST. The result is that attempting a regression searching for long run effects 9 weeks after the start of DST given only a 3 week (fairly short run) window of actual change is a difficult undertaking. A similar regression was attempted with the number of "after DST weeks" lowered to two, but the small sample size crippled the regression and gave little significant information.

A similar issue should be expected to plague the fall 2004–2010 regressions. Although the overall "week after" statistic is somewhat as expected (roughly a 4% increase in crashes due to the removal of DST in the fall, in treatment (2004–2006) years), the "individual week" data again seems to suffer from contamination, as several of the statistics are significantly negative. Whereas the spring change in DST start time was 3 weeks, the fall change in end time was only a single week, potentially increasing the contamination problem. Seemingly strangely, however, the fall $\overline{R}^2$ is the highest by far of all the OLS regressions: this is accounted for by the increased sample size in the fall data set (it is fairly easy to add more "prior to DST" weeks in the fall regression). Furthermore, although the above indicates that one must be careful in interpreting the fall results, the majority of the "individual week" data is significant in the expected direction: for example, there are 10% and 15% increases in crashes due to the removal of DST in the 4th and 9th weeks, respectively, after the time reset, the same benchmark weeks as in the spring 1976–2003 OLS results.

4.2. **Results on the short term effects of DST**

The results of the Poisson regressions on the short term effects of DST are presented in Table 3. The coefficient of primary importance is the interaction dummy (resulting in the derived "Treatment/Control" ratio): the interpretation is analogous to the long run case, measuring percentage increase in crash prevalence due to DST activation in the treatment period (or due to removal in the fall tests). A positive coefficient represents an increase in crash incidence due to DST (or due to DST removal in the fall).

In the spring 1976–2003 regressions, we again compare our findings to those of Sood & Ghosh (2007). Our results are essentially identical: although the treatment ratios are greater than one (and significantly so in the case of the "FM/PM" ratio), so are the control ratios (indicating a likely week of the month effect). The result is statistically insignificant short term effects due to DST (specifically 2% and -2% effects, not significant at the 10% level, for the "FM/PM" and "FM/(PM or SM)" regressions respectively).

In the spring 2004–2010 regressions, the outcome is fairly similar. In these regressions, the treatment ratios are actually less than one (although not significantly, likely due to far fewer observations), but the control ratios are as well, and the result is again statistically insignificant short term effects due to DST (specifically 12% and 9% effects, not significant at the 10% level, for the "FM/PM" and "FM/(PM or SM)"

Table 3: Poisson regressions of Monday crash counts on various dummies[10]

	1976–2003: Spring		2004–2010: Spring		2004–2010: Fall
	(7)	(8)	(9)	(10)	(11)
Regressor/Test Statistic	FM/PM	FM/(PM or SM)	FM/PM	FM/(PM or SM)	FM/PM
FM Dummy	0.0993725***	0.0796674**	−0.177571**	−0.118309*	0.0263173
	(0.0357297)	(0.0361142)	(0.0747278)	(0.0712135)	(0.0319578)
Treatment Dummy	−0.00637913	0.124825	−0.384069***	−0.426945**	0.0798060
	(0.193733)	(0.154782)	(0.0995602)	(0.168619)	(0.0556489)
Interaction Dummy	0.0230742	−0.0161305	0.124461	0.0949481	0.0303780
	(0.0506292)	(0.0501080)	(0.103248)	(0.104951)	(0.0502650)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes
$\overline{R}^2$	0.029993	0.018361	0.072669	0.096894	0.003128
Log-likelihood	−192.2941	−300.6388	−47.41185	−76.71578	−44.53575
Overdispersion Test	3.25347	0.0157727	6.51031	0.844374	13.6054
Observations	56	84	14	21	14
Treatment FM Average	82.1765	82.1765	68.7500	68.7500	84.6667
Control FM Average	80.7273	80.7273	70.3333	70.3333	77.0000
Treatment PM Average	72.7059		72.5000		80.0000
Control PM Average	73.0909		84.0000		75.0000
Treatment (PM or SM) Average		77.1176		70.3750	
Control (PM or SM) Average		74.5455		79.1667	
Treatment FM/PM	1.1303**		0.9483		1.0583
	(0.0590)		(0.1103)		(0.0711)
Control FM/PM	1.1045*		0.8373		1.0267
	(0.0585)		(0.1080)		(0.0536)
Treatment FM/(PM or SM)		1.0656		0.9769	
		(0.0468)		(0.0997)	
Control FM/(PM or SM)		1.0829*		0.8884	
		(0.0502)		(0.0735)	
Treatment/Control	1.0230742	0.9838695	1.124461	1.0949481	1.0303780
	(0.0506292)	(0.0501080)	(0.103248)	(0.104951)	(0.0502650)

regressions respectively). It is interesting to note that the $\overline{R}^2$ statistics are actually higher for these regressions, though still abysmally low.

[10]Poisson regressions generated in GRETL. Number above each regression name corresponds to the regression's equation in Section 3. "FM/PM" stands for a regression including only "first Monday" and "prior Monday" data, while "FM/(PM or SM)" stands for a regression including "first Monday", "prior Monday", and "subsequent Monday" data. Standard errors included in parentheses under the values of the coefficients. *** = significant at 1%; ** = significant at 5%; * = significant at 10% (generated using a two-tailed p-test).

In the fall 2004–2010 regression, the effect being considered is slightly different: a positive interaction dummy indicates increased short run crash prevalence due to the removal of DST in the fall. Similarly to the other short term regressions, the "FM/PM" interaction coefficient (representing a 3% short term crash increase due to the removal of DST in the 2004–2006 treatment years) is not significantly different from zero at the 10% level. In fact, none of the values in the fall regression were significant at even the 10% level, meaning that any inferences taken from the regression must be used with caution.

5. Conclusions

The purpose of this paper was to test the robustness of the findings of Sood & Ghosh (2007) in analyzing the long and short term effects of DST, utilizing a similar natural experiment provided by the Energy Policy Act of 2005. I find that, despite contamination and power issues, the data generally supports the analysis by Sood & Ghosh (2007). DST appears to have significant fatal crash-saving effects in the long run; Sood & Ghosh (2007) show this via a spring test, this paper cautiously supports this finding using a fall test (the spring test in this paper gave little evidence either way). In the short run, the data shows little evidence of DST causing fatal accidents, although limited power hampers this result somewhat.

The policy implications of this analysis are clear: extending DST should save lives due to automobile accidents in the long run due to increased evening visibility, while in the short run any sleep disruption caused by DST shouldn't cost lives in accidents. If fatal accidents are a major policy issue, then extending DST seems like a worthwhile experiment to perform, although the problematic issue of sleep loss extends not only to traffic accident incidence and could cause other issues.

In researching the effects of DST, the FARS database is an excellent resource for analyzing the effects of DST on traffic fatalities. If such data could be collected from other countries exhibiting similar time-altering systems (for example, British Summer Time is the equivalent to Daylight Saving Time in the United Kingdom), perhaps further research in this direction could provide more conclusive results.

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