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Neeraj Sood* Arkadipta Ghosh†

*RAND Corporation, sood@rand.org
†Pardee RAND Graduate School, aghosh@mathematica-mpr.com

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The Short and Long Run Effects of Daylight Saving Time on Fatal Automobile Crashes*

Neeraj Sood and Arkadipta Ghosh

Abstract

Prior literature suggests that Daylight Saving Time (DST) can both increase the risk of automobile crashes in the short run and decrease the risk of automobile crashes in the long run. We use 28 years (1976-2003) of automobile crash data from the United States, and exploit a natural experiment arising from a 1986 federal law that changed the time when states switched to DST to identify the short run and long run effects of DST on automobile crashes. Our findings suggest that (1) DST has no significant detrimental effect on automobile crashes in the short run; (2) DST significantly reduces automobile crashes in the long run with a 8-11% fall in crashes involving pedestrians, and a 6-10% fall in crashes for vehicular occupants in the weeks after the spring shift to DST.

KEYWORDS: daylight saving time, automobile crashes, fatal, accidents

*Neeraj Sood (Corresponding Author) RAND Corporation, 1776 Main Street, Santa Monica, CA 90407. Arkadipta Ghosh, Pardee RAND Graduate School, 1776 Main Street, Santa Monica, CA 90407.

1. Introduction

Many countries around the world follow the convention of Daylight Saving Time (DST) in order to conserve energy and make more efficient use of daylight. The basic mechanics of DST are succinctly summarized in the phrase "spring forward, fall back", i.e., clocks are moved forward by an hour during the summer months to transfer daylight from the mornings to the evenings, and they are again switched back by an hour during the winter months to have more daylight in the otherwise cold and dark winter mornings.

While the energy-saving benefits of DST are well known (Aldrich, 2005), and have even prompted a month's extension in the duration of DST from 2007 onwards (as per the Energy Policy Act of 2005), there is some ambiguity surrounding the short and long run effects of DST on automobile crashes. One strand of the literature posits that DST is likely to increase crashes in the short run due to disruptions in the sleep cycle associated with the one hour "loss" of time immediately after the switch to DST. In contrast, a separate strand of the literature emphasizes the long run beneficial effect (i.e., fewer crashes) of DST arising from an extended period of better visibility. Bernstein (2005) provides a brief overview of this controversy.

In this paper, we bring together these two separate pieces of the literature under one comprehensive analysis by analyzing data on motor vehicle crashes in the U.S. over a 28-year period (1976 – 2003). In particular, our methods rely on a natural experiment arising from a 1986 federal law that changed the time when states switched to DST. This law mandated that starting in 1987 all states switch to DST on the first Sunday of April.[1] Prior to the implementation of this law, implementation of DST was sporadic, and if implemented, states switched to DST on the last Sunday in April.[2] We use this natural experiment to estimate both the long and short-run effects of DST on fatal automobile crashes.[3]

1.1 Short run versus long run effects of DST on automobile crashes

When states switch to DST on the 1st Sunday of April, a typical motorist experiences two opposing effects on driving conditions in the morning and

[1] The only states in the continental US that do not observe DST are Indiana (the Eastern Time Zone portion) and Arizona (almost entirely except the Navajo Indian Reserve).

[2] Prerau (2005) provides a history of DST. A brief overview is also available at http://webexhibits.org/daylightsaving/e.html. Access date: December 22, 2005.

[3] We cannot study the fall change from DST as it occurred at the same time in both treatment and control years.

evening. As clocks are moved one hour forward, light condition deteriorates in the early hours of the morning and improves in the evening. Given that there is an elevated risk of crashes in darkness than in daylight (Fridstrom and Ingebrigsten, 1991; Fridstrom et al., 1995; Green, 1980), the shift to DST increases the likelihood of automobile crashes in the morning and simultaneously reduces the likelihood of crashes in the evening. There is some evidence that the benefit (fewer crashes) of improved light condition in the evening outweighs the negative impact of poor visibility in the mornings leading to an overall saving in crashes during DST.

For instance, Fergusson et al. (1995) use data from USA for the years 1987-1991 to estimate the effect of light conditions on traffic safety. They find that a change from daylight to twilight led to a 300% increase in crashes involving pedestrians, and a smaller 15% increase in crashes for vehicle occupants. They use these estimates to simulate the effects of retaining DST throughout the year and find that such a policy would on average save about 180 fatal collisions every year.[4] Several British studies (Road Research Laboratory, 1970; Broughton and Sedman, 1989; Broughton and Stone, 1998; Broughton, Hazelton and Stone, 1999) have used a similar strategy and find significant collision reduction associated with Daylight Saving Time (or, British Summer Time). These long run crash saving benefits of DST due to better visibility in the evenings has prompted several of these authors to conclude that retaining DST throughout the year would "save additional lives".

However, the imposition of DST could increase automobile crashes, at least in the short run, through sleep deprivation or disruption in the circadian rhythm. The reasoning is that a person is at risk of losing an hour's sleep on the morning after DST comes into effect as clocks are moved forward by an hour during the night. So, short-term sleep deprivation or disruptions in the circadian rhythm are quite likely in the first few days after the switch to DST. Monk and Folkard (1976) and Monk and Aplin (1980) studied individual subjects both before and after the time changes to find that the shift to DST brought about significant disruptions in the sleep cycle, mood, and performance efficiency. A possible effect of such disruptions is fatigue while driving and thereby a higher propensity for collisions.[5] Finally, the shift to DST might increase automobile crashes by increasing speeding and alcohol related crashes in the short run. The

[4] In an earlier study, Meyerhoff (1978) finds that there was a net reduction of 0.7% in automobile crashes during the DST period - March and April 1974, compared to the corresponding non-DST period in 1973.

[5] In fact, Coren (1996(a)) found that *all* accidental deaths increase by 6.5% in the first four workdays immediately following the spring change, while there was no significant change in accidental deaths during the fall change.

rationale is that collisions due to speeding might increase if people rush to work on the mornings after DST starts, as people find themselves rising late (Grekin, 1996). Similarly, prior research suggests that the interaction between sleepiness and alcohol might also increase alcohol related crashes (Hicks et al., 1998).

The evidence on the immediate or short run impact of the shift to DST on the risk of motor vehicle crashes is mixed. Most studies compare the DST Monday crash count with the crash counts on the prior Monday or both the prior and subsequent Mondays. While several U.S. studies (Hicks, Lindseth, and Hawkins, 1983; Coren, 1996b&c; (1998a&b); Varughese and Allen, 2001), and one British study (Monk, 1980) report a significant increase in crash counts immediately following the shift to DST, other studies from Canada (Vincent, 1998) and Sweden (Lambe and Cummings, 2000) have found no significant immediate impact of time changes on automobile crashes.

2. Data & methods

We use data from the Fatality Analysis Reporting System (FARS) database of the National Highway Traffic Safety Administration (NHTSA). FARS documents all qualifying fatal crashes over 1975-2003 that occurred within the 50 states, District of Columbia, and Puerto Rico. It includes all crashes that involved "a motor vehicle traveling on a traffic way customarily open to the public, and resulted in the death of a person (occupant or nonmotorist) within 30 days of the crash" (Tessmer, 2002). We exclude the year 1975 from this analysis as DST was retained for an extended period during 1974 (10 months) and 1975 (8 months), following the oil crisis of 1973, and use 28 years of crash data from 1976 to 2003 for the continental United States. We use national data, instead of data disaggregated by state for the following two reasons. First, we do not have data on which states observed DST before 1987, and are therefore unable to use any additional identifying information. Second, there are only two control states – Hawaii and Puerto Rico – that did not observe DST during 1987-2003, and neither of these lie in the continental United States, which generates the bulk of our data. Hence, we dropped these two states from our analysis, along with Arizona and Indiana, parts of which also did not observe DST.

We use the "accident files" from the FARS database to estimate the number of crashes occurring on specific days prior and subsequent to the spring switch to DST, i.e., we count the number of fatal crashes or collisions (not fatalities) occurring on a particular day or week. We exclude less than 0.1% of the records from our analysis as they have missing information on the date of the crash. Additionally, we merge the "accident files" with the "vehicle files" to analyze collisions involving speeding and drunk driving. A particular crash was coded as involving speeding, if the given travel speed (an estimated or derived variable in FARS) for any of the vehicles in the crash exceeded the speed limit. However, about 50% of the records have missing information for travel speed, and speed limit is missing from 5% of the records. Hence, our analysis of speeding-related crashes suffers from a substantial missing data problem. Crashes are identified as alcohol related if there is "sufficient information" to conclude that the driver was drinking, i.e., the driver had a measurable or estimated blood alcohol concentration (BAC) of 0.01 g/dl or above, or if the investigating officer reported alcohol involvement. However, note that often there is insufficient information to discern whether the driver was drinking (Tessmer, 2002). For example, in the FARS dataset the rate of BAC testing ranged from 33% in 1977 to roughly 50% in the 1990's (Yi et al., 2005).

2.1 Empirical strategy for identifying possible long run effects of DST

Our empirical strategy relies on a natural experiment arising from a 1986 federal law that changed the time when states switched to DST. This law mandated that starting in 1987 all states switch to DST on the first Sunday of April. Prior to the implementation of this law, implementation of DST was sporadic and if implemented states switched to DST on the last Sunday in April. As in previous studies, we examine weekly crash incidence for 13 weeks before and 9 weeks after the change to DST (Fergusson et al., 1995). However, unlike prior research the empirical strategy is to compare automobile crash counts in the 13 weeks before and 9 weeks after the first Sunday of April in years where the shift to DST occurred on the first Sunday of April (1987-2003, Treatment Years) and also in years where there was no shift to DST (1976-1986, Control Years) on the first Sunday of April. Note that this is the conventional difference-of-difference (DD) estimator, since we are differencing collision trends both within years and across treatment and control years.

We implement our empirical model by first transforming our data into a single weekly time series (22 weeks). Each observation in this time series represents the log of average crashes in treatment years (after the law change, i.e., 1987-2003) for a given week minus the log of average crashes in the control years (before the law change, i.e., 1976-86) for the same week. In other words, this time series measures the crash counts in treatment years relative to crash counts in control years for each of the 22 weeks in our analysis. We then regress this difference in log crash counts on a "week after" dummy variable set equal to one for the 9 weeks after the first Sunday of April. We estimate Newey-West standard errors to account for autocorrelation in errors (Newey and West, 1987).[6] The

[6] We are thankful to an anonymous referee and the editor of this journal for suggesting this strategy.

coefficient on the "week after" dummy is an estimate of the causal effect of DST on the risk of automobile crashes 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.[7] However, this assumption might not hold if there are changes in the weekly pattern of crashes between treatment and control years (our data span 28 years). Therefore, to test the robustness of our results, we repeat our analysis for a narrower 7-year window (1984-1990) that spans both treatment and control years where such changes in the weekly pattern of crashes are less likely. Additionally, we include a dummy variable for "3 weeks before the start of DST" in our regressions, which is equal to one for the 3 weeks immediately before the start of DST. The coefficient on this dummy variable provides a test as to whether the DST effect picked up by the "week after" dummy is truly arising due to DST, or is in fact a continuation of a downward trend in crashes in treatment years even before the start of DST.

In addition, some states observed DST from the last Sunday of April in the control years. Thus, there is a problem of contamination in the control years of our data in the weeks after the last Sunday of April. In order to test for bias due to this contamination, we distinguished between the effects of DST in the first 3 weeks versus in the entire 9-week period following the shift to DST. This addresses the contamination problem in the control years, since no states observed DST in the first 3 weeks after the first Sunday of April during 1976-86, even if some states did so for the next 6 weeks after the last Sunday of April.[8]

Finally, since prior research has found greater crash saving benefits for pedestrians (Fergusson et al., 1995; Broughton, Hazelton and Stone, 1999), we estimate the above models separately for crashes involving pedestrians and those involving vehicles only. For the purpose of this analysis, any crash between pedestrians or pedal cyclists and motor vehicles is referred to as a pedestrian crash, and any crash involving motor vehicles only is referred to as a vehicular crash.

[7] Our empirical approach could also be implemented as a Poisson regression model in which crash counts (in given week and year) are regressed on year fixed effects, week fixed effects and an interaction term between treatment years (year > 1986) and weeks after dummy. The coefficient on that interaction term is identical to the coefficient we report in the paper.

[8] It is interesting to note here that the last Sunday of April is typically either 3 or 4 weeks after the first Sunday, and for 3 of the 11 control years in our data (1976-1986), the last Sunday was 4 weeks after the first Sunday of April, while for the remaining 8 years it was 3 weeks after the first Sunday. Hence, we preferred to separately look at the first 3 weeks, rather than the first 4.

2.2 Empirical strategy for identifying possible short run effects of DST

As in the previous analysis, we use 28 years of data and a difference in difference estimation strategy to estimate the causal effect of DST on automobile crashes immediately after the switch to DST. In contrast, most prior studies on the short term effects of DST have used a few years of data and a simple method to estimate the effect of DST: compare crash counts on the Monday immediately following the DST shift with the crash counts on both the prior and subsequent Monday, i.e., they bracket the counts of crashes on the Monday immediately following the shift to DST using the two Mondays on either side. This simple method of accounting for trends over time has the potential advantage of adjusting for the trend in crashes within each study year. However, it is possible that automobile crashes on the first Monday (i.e., the Monday immediately following the first Sunday of April), during the years 1987-2003 might be different than crashes on prior and subsequent Mondays not only due to the shift to DST but also due to week of the month effects or other seasonal patterns in automobile crashes. For example, if there are higher crashes during the first week of a month compared to other weeks then the change in crashes on the first Monday (which typically lies in the first week) might reflect "first week" effects rather than the effect of DST on automobile crashes. To isolate these seasonal and week of the month effects, we compare automobile crashes on the first Monday to crashes on the prior Monday, and on both the prior and subsequent Mondays – for the years 1976 to 1986 as well. The rationale is that during the years 1976-1986, the switch to DST did not occur on the first Sunday of April, and therefore the change in crashes between the first Monday and the prior Monday, or between the first Monday and the average count of crashes on the prior and subsequent Mondays during these years will reflect only week of the month or other seasonal trends in automobile crashes. Finally, we compare the change in automobile crashes on the first Monday for treatment and control years to estimate the short run effect of DST on automobile crashes.

Thus, we first report the average crash counts in treatment and control years on: (1) The first Monday of April, (2) the Monday prior to the first Monday of April, and (3) the Mondays prior and subsequent to the first Monday of April. We then compute the ratio of crashes for treatment and control years separately between: (1) the first Monday of April and the prior Monday, and (2) the first Monday of April and the prior or subsequent Monday. Finally, we compute the ratio of ratios for each of the above analyses by dividing the ratio for treatment years by the ratio for control years. These ratio of ratios measure the immediate or short run impact of DST on fatal automobile crash incidence.[9]

As in the previous analysis, we test the robustness of our results, by repeating our analysis for a narrower 7-year window (1984-1990). Prior research suggests that the shift to DST might also increase speeding and alcohol related automobile crashes in the short run. Therefore, we use data on travel speeds and posted speed limits for every crash, and data on whether a collision involved a drunk driver to test for the possible rise in speeding or alcohol related crashes immediately after the shift to DST.

3. Results

3.1 Long run results

Figures 1 and 2 plot the average crash incidence for each of the 22 weeks around the first Sunday of April, by treatment and control years, for pedestrian and vehicular crashes respectively. In both figures, some visible differences in trends can be seen in the weeks immediately after the first Sunday of April, especially in the first 2 to 3 weeks. In particular, both figure 1 and figure 2 show some evidence of a decline in crashes in the post 1987 period, immediately after the first Sunday of April. We posit that this decline is crashes observed in the post 1987 period (but not in the pre 1987 period) might be due to the implementation of DST in the post 1987 period. Additionally, there is some indication – at least in figure 1 – that there is a downward trend for the treatment observations compared to the control observations in the weeks immediately before the first Sunday of April. As mentioned before, we use a dummy variable for "3 weeks before the start of DST" to investigate whether this downward trend is significant. A negative and significant coefficient on this variable would indicate that there was significant crash saving even in the weeks before the start of DST, and hence, any evidence for a DST-induced saving in crashes (from the "week after" dummy) must be interpreted with caution.

[9] Our approach is identical to a Poisson regression in which the crash count for a particular Monday in any year is regressed on a 1st week dummy (set equal to one for the Monday immediately after the first Sunday of April, and zero for the Mondays before and after), a treatment dummy (one for treatment years: 1987-2003), and an interaction between these two dummies, along with year fixed effects. The coefficient on the interaction term is therefore, the DD estimate of the short-run or immediate impact of DST on automobile crashes. To account for correlation in observations across adjacent weeks, we implement these regressions with standard errors clustered at the year level, and thus obtain robust estimates.

Figure 1
Average Pedestrian Crash Counts for 13 Weeks Before and 9 Weeks after the Spring Change: By Treatment (1987-2003) and Control (1976-86) Years

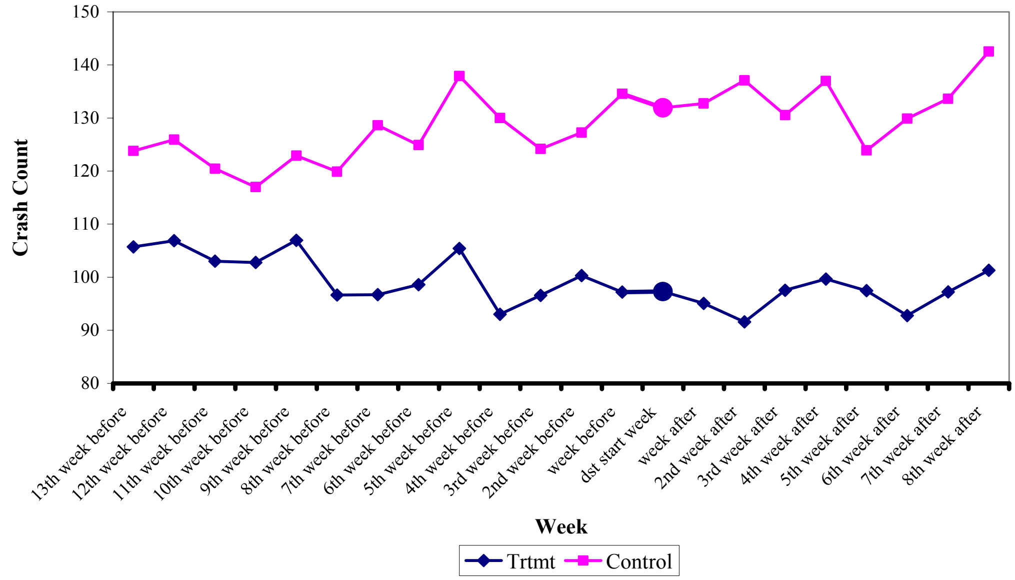

Figure 2
Average Vehicular Crash Counts for 13 Weeks Before and 9 Weeks after the Spring Change: By Treatment (1987-2003) and Control (1976-86) Years

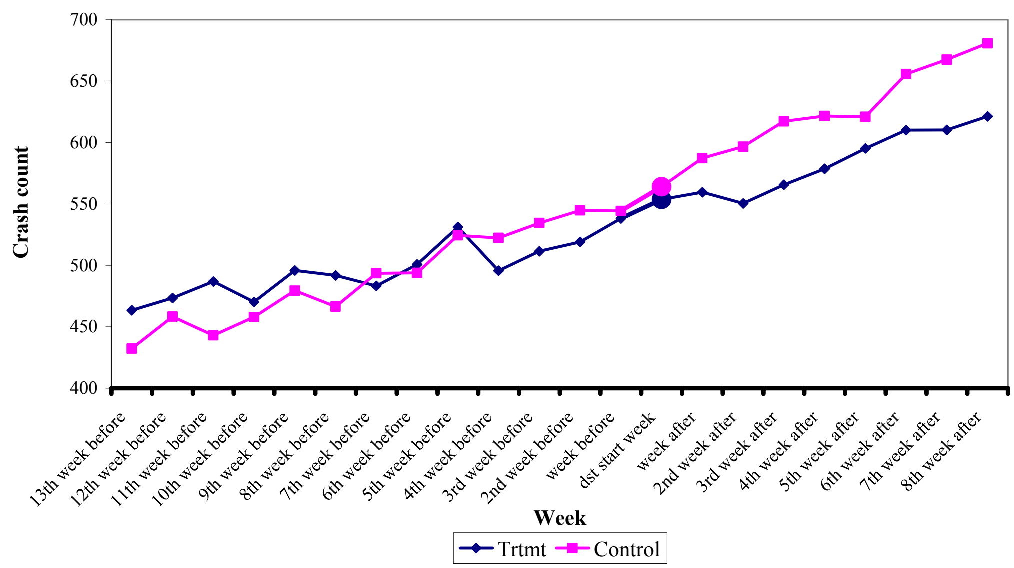

Table 1 presents the estimates from OLS regressions (with Newey-West standard errors). The coefficient on the "weeks after 1st Sunday of April" dummy variable measures the percent decline in crash incidence due to the implementation of DST in treatment years, and the coefficient on the "3 weeks before the start of DST" dummy captures any possible crash saving that might arise even before the start of DST in treatment years. In specification (2) the coefficient on the "1st 3 weeks after 1st Sunday of April" dummy measures whether this DST effect is different in the first 3 weeks.

Panel A of table 1 reports results from regressions that use all 28 years of data from 1976 to 2003. The estimates and associated confidence intervals in column 1 suggest that there is a marked decline in both pedestrian and vehicular crashes in the weeks following the 1st Sunday of April for treatment years. Specifically, pedestrian crashes decline by 11% as a consequence of DST, while vehicular crashes fall by 9-10%. Moreover, the estimates in column 2 clearly show that these effects are not significantly different for the first 3 weeks after the 1st Sunday of April – weeks that are not contaminated by possible implementation of DST in some states during the control years.

However, recall that this analysis provides causal estimates of the effect of DST on the risk of automobile crashes only under the 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. However, this assumption might not hold if there are changes in the weekly pattern of crashes in the 28-year span of our data. Panel B reports results from regressions that use only 7 years of data (1984-1990) spanning both treatment and control years where such changes in the weekly pattern of crashes are less likely. We find that our results are robust to restricting the analysis to 7 years of data (panel B) – estimates in column 1 suggest that pedestrian and vehicular crashes decline by about 8 and 6% following the imposition of DST, and these estimates are not significantly different from corresponding estimates in panel A. Moreover, these effects are not different for the first 3 weeks after the start of DST (column 2 estimates of the "1st 3 weeks" dummy in panel B), confirming once again that contamination in control years is not a major problem in our data.

Finally, the coefficients on the "3 weeks before the start of DST" dummy indicate that there was no significant decline in pedestrian crashes before the start of DST in treatment years - either in the longer 28-year window (1976-2003), or the shorter 7-year window (1984-1990). However, there is a significant 6% decline in vehicular crashes even in the 3 weeks immediately before the start of DST – at least in the 28-year window (1976-2003). This suggests that the crash-saving benefit of DST for vehicular crashes needs to be interpreted with caution. Note however, that the point estimate on this "3 weeks before the start of DST" dummy is smaller than that for the "weeks after" dummy, and more importantly, this effect is smaller and not significant in the shorter 7-year window (1984-1990). We believe that the results from the shorter window (panel B) are more reliable because changes in seasonal patterns of crash incidence are less likely over this shorter 7-year period, and hence, overall, the results do provide some evidence of the crash saving benefit from DST even for vehicular crashes.

Table 1
Long-run effect of DST on automobile crashes

Panel A: 1976-2003	Pedestrian Crashes		Vehicular Crashes	
	(1)	(2)	(1)	(2)
Weeks after 1st Sunday of April	-0.11***	-0.10***	-0.09***	-0.10***
	(-0.18, -0.04)	(-0.17, -0.03)	(-0.13, -0.06)	(-0.14, -0.06)
Last 3 weeks before 1st Sunday of April	-0.06	-0.06	-0.06***	-0.06***
	(-0.14, 0.02)	(-0.15, 0.02)	(-0.09, -0.03)	(-0.09, -0.03)
1st 3 weeks after 1st Sunday of April		-0.04		0.03*
		(-0.09, 0.02)		(-0.00, 0.05)
Observations	22	22	22	22
Panel B: 1984-1990				
	(1)	(2)	(1)	(2)
Weeks after 1st Sunday of April	-0.08**	-0.06	-0.06***	-0.07***
	(-0.16, -0.00)	(-0.14, 0.02)	(-0.10, -0.03)	(-0.10, -0.04)
Last 3 weeks before 1st Sunday of April	-0.02	-0.02	-0.04*	-0.04
	(-0.10, 0.05)	(-0.10, 0.06)	(-0.09, 0.01)	(-0.09, 0.01)
1st 3 weeks after 1st Sunday of April		-0.05		0.03
		(-0.13, 0.02)		(-0.03, 0.09)
Observations	22	22	22	22

Note: Estimates are from OLS regressions on a single weekly time series of the differences in log of average weekly crash counts between treatment and control years on a "week after 1st Sunday of April" dummy and a "last 3 weeks before the 1st Sunday of April" dummy; also a "1st 3 weeks after 1st Sunday of April" dummy, in column 2. Confidence intervals are based on Newey-West standard errors.

95% confidence intervals in parentheses; *** Significant at 1%; ** Significant at 5%; * Significant at 10%

To further break down the effect of DST in each of the 9 weeks following the start of DST, Table 2 presents coefficient estimates of dummies for each of the 9 weeks from similar Newey-West regressions – for both the longer (1976 – 2003) and shorter (1984 – 1990) panels. The estimates in panel A suggest that there are significant crash savings for both pedestrian and vehicular crashes in almost all the weeks following the start of DST, with the saving in pedestrian crashes reaching its peak in the 3rd week after the start of DST, while the saving in vehicular crashes reaches its peak in the 4th week (also 8th and 9th weeks) after the start of DST for the entire 1976-2003 period. Panel B estimates also show significant crash saving for almost all the 9 weeks after the start of DST.

Table 2
Effect of DST in each of the 9 weeks after the start of DST

	Panel A: 1976-2003		***Panel B: 1984-1990***	
	Pedestrian Crashes	Vehicular Crashes	Pedestrian Crashes	Vehicular Crashes
	(1)	(2)	(1)	(2)
1st week after DST	-0.08**	-0.03	-0.02	-0.03*
	(-0.16 - -0.00)	(-0.08 – 0.02)	(-0.10 - 0.05)	(-0.07 - 0.00)
2nd week after DST	-0.11***	-0.06**	-0.17***	0.06***
	(-0.19 - -0.03)	(-0.11 - -0.01)	(-0.24 - -0.09)	(0.02 - 0.09)
3rd week after DST	-0.18***	-0.09***	-0.14***	-0.12***
	(-0.26 - -0.10)	(-0.14 - -0.04)	(-0.21 - -0.06)	(-0.16 - -0.09)
4th week after DST	-0.07*	-0.10***	0.03	-0.08***
	(-0.15 - 0.00)	(-0.15 - -0.05)	(-0.05 - 0.10)	(-0.11 - -0.04)
5th week after DST	-0.09**	-0.08***	-0.18***	-0.03*
	(-0.17 - -0.02)	(-0.13 - -0.03)	(-0.26 - -0.11)	(-0.06 - 0.01)
6th week after DST	-0.01	-0.06**	0.01	-0.05**
	(-0.09 - 0.06)	(-0.11 - -0.01)	(-0.07 - 0.08)	(-0.08 - -0.01)
7th week after DST	-0.12***	-0.08***	-0.03	-0.10***
	(-0.19 - -0.04)	(-0.13 - -0.03)	(-0.10 - 0.05)	(-0.14 - -0.07)
8th week after DST	-0.10**	-0.10***	-0.09**	-0.07***
	(-0.17 - -0.02)	(-0.15 - -0.05)	(-0.17 - -0.02)	(-0.11 - -0.04)
9th week after DST	-0.12***	-0.10***	-0.08**	-0.06***
	(-0.19 - -0.04)	(-0.15 - -0.05)	(-0.15 - -0.00)	(-0.09 - -0.02)
Constant	-0.22***	0.01	0.02	0.07***
	(-0.30 - -0.15)	(-0.04 – 0.06)	(-0.05 - 0.10)	(0.04 - 0.11)
Observations	22	22	22	22

Note: Estimates are from OLS regressions on a single weekly time series of the differences in log of average weekly crash counts between treatment and control years on dummies for each of the 9 weeks after the 1st Sunday of April

95% confidence intervals in parentheses; *** Significant at 1%; ** Significant at 5%; * Significant at 10%

3.2 Short run results

Tables 3A and 3B report mean automobile crash counts on the first Monday of April (i.e. the Monday immediately following the first Sunday of April), mean automobile crash counts on the prior Monday, and the combined mean from the prior and subsequent Mondays for both treatment) and control years. They also report the ratios, and the ratio-of-ratios with standard errors adjusted for clustering within each year. The results in table 3A are derived from data on all 28 years of crashes – 1976-2003, whereas, those in table 3B are from crash data during 1984-1990.

The results for all crashes in table 3A show that during the treatment years (1987-2003), mean automobile crashes on the first Monday (82) were 13% higher than mean automobile crashes on the Monday of the prior week (73). We find a similar pattern of crash incidence in the control years -- mean automobile crashes on the first Monday (81) were 10% higher than mean automobile crashes on the Monday of the prior week (73). Consequently, the ratio-of-ratio estimate (treatment ratio / control ratio) is 1.02, and is not statistically different from 1. Thus, these results suggest no significant increase in crashes in the short run due to DST. We reach a similar conclusion by using both the prior and subsequent Mondays as controls – the ratio of ratios estimate is 0.98, and once again, not statistically different from 1. Therefore, these simple means of automobile crashes by week type, along with the corresponding ratio-of-ratios show no evidence that the implementation of DST in treatment years led to an immediate increase in crashes on the first Monday of April.

Table 3A also reports results for crashes involving speeding and drunk driving. The results are similar to those for all crashes: for both treatment and control years, there is a similar increase in either type of crashes in going from the prior Monday to the first Monday, or in the comparison of first Monday with both prior and subsequent Mondays. The only exception is for alcohol-related crashes where going from the prior Monday to the first Monday, there is a statistically significant increase in crashes for treatment years. But once again, the ratio-of-ratio estimate is statistically insignificant. These results therefore, suggest no significant increase in either speeding or alcohol related crashes following the spring shift to DST.

As discussed before, the identifying assumption of our approach is that in the absence of DST the pattern of crashes by week type would be similar in the treatment and control years. However, since the treatment and control years span several years it is possible that the seasonal or week of the month pattern of collisions changed over these years. Therefore, to check the sensitivity of our results we computed our estimates with only 7 years of data (1984-1990), and results from this shorter panel appears in table 3B. Once again, we failed to detect any significant immediate effect of DST on crash incidence on the DST Monday relative to the Monday before, or the combined Mondays before and after[10].

The analysis above rules out several potential explanations for a rise in crash incidence immediately following DST including (1) disruption in sleep due to one hour time "loss" associated with DST, (2) speeding to make up for delayed awakening on the mornings after DST, and (3) aggravation of the impairment effect of alcohol due to sleep disruption associated with DST.

Table 3A
Averages and ratios of crash counts on Mondays around the switch to DST: by treatment and control years for 1976 - 2003

	Treatment Years	Control Years
All Crashes		
First Monday	82.12	80.73
Monday of prior week	72.71	73.09
Ratio: First Monday/Prior Monday	1.13** (1.02 - 1.25)	1.10* (1.00 - 1.23)
Treatment ratio/Control ratio	1.02 (0.89 - 1.18)	
Monday of prior or subsequent week	77.09	74.55
Ratio: First Monday/Prior or subsequent Monday	1.07 (0.98 - 1.16)	1.08* (0.99 - 1.19)
Treatment ratio/Control ratio	0.98 (0.87 - 1.11)	
Speeding Related Crashes		
First Monday	22.41	21.00
Monday of prior week	19.23	18.45
Ratio: First Monday/Prior Monday	1.17* (1.02 - 1.32)	1.14 (0.91 - 1.43)
Treatment ratio/Control ratio	1.02 (0.79 - 1.32)	
Monday of prior or subsequent week	21.94	19.32
Ratio: First Monday/Prior or subsequent Monday	1.02 (0.90 - 1.15)	1.09 (0.93 - 1.27)
Treatment ratio/Control ratio	0.94 (0.78 - 1.14)	
Alcohol Related Crashes		
First Monday	20.47	20.18
Monday of prior week	17.29	19.18
Ratio: First Monday/Prior Monday	1.18*** (1.04 - 1.35)	1.05 (0.88 - 1.26)
Treatment ratio/Control ratio	1.13 (0.91 - 1.40)	
Monday of prior or subsequent week	18.73	19
Ratio: First Monday/Prior or subsequent Monday	1.09 (0.98 - 1.22)	1.06 (0.90 - 1.25)
Treatment ratio/Control ratio	1.03 (0.85 - 1.25)	

95% confidence intervals in parentheses; *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

[10] There is a problem with lack of power due to far fewer observations for the shorter panel, and consequently, the standard errors and confidence intervals for the estimates in table 3B are wider than those for the estimates in table 3A.

Table 3B
Averages and ratios of crash counts on Mondays around the switch to DST: by treatment and control years for 1984 - 1990

	Treatment Years		Control Years	
All Crashes				
First Monday	82		79.33	
Monday of prior week	73.25		80.33	
Ratio: First Monday/Prior Monday	1.12	(0.95 - 1.31)	0.99	(0.82 - 1.19)
Treatment ratio/Control ratio	1.13	(0.91 - 1.42)		
Monday of prior or subsequent week	79.38		73.83	
Ratio: First Monday/Prior or subsequent Monday	1.03	(0.83 - 1.29)	1.07	(0.88 - 1.31)
Treatment ratio/Control ratio	0.96	(0.73 - 1.26)		
Speeding Related Crashes				
First Monday	25.75		21.33	
Monday of prior week	20.75		21.67	
Ratio: First Monday/Prior Monday	1.24*	(0.99 - 1.56)	0.98	(0.48 - 2.02)
Treatment ratio/Control ratio	1.26	(0.65 - 2.46)		
Monday of prior or subsequent week	23.13		20.67	
Ratio: First Monday/Prior or subsequent Monday	1.11	(0.88 - 1.40)	1.03	(0.59 - 1.80)
Treatment ratio/Control ratio	1.08	(0.63 - 1.84)		
Alcohol Related Crashes				
First Monday	21.25		19.00	
Monday of prior week	19.75		22.00	
Ratio: First Monday/Prior Monday	1.08	(0.82 - 1.42)	0.86*	(0.74 - 1.01)
Treatment ratio/Control ratio	1.25	(0.93 - 1.67)		
Monday of prior or subsequent week	20.63		20.17	
Ratio: First Monday/Prior or subsequent Monday	1.03	(0.83 - 1.27)	0.94	(0.70 - 1.27)
Treatment ratio/Control ratio	1.09	(0.79 - 1.52)		

95% confidence intervals in parentheses; *** Significant at 1%; ** Significant at 5%; * Significant at 10%

4. Discussion and conclusions

This paper analyzed the long run and immediate effects of DST on automobile crashes. We find that the transition to DST does not have any significant immediate impact on crash incidence, after controlling for collision trends within and across years. In a separate analysis on the long-run effect of DST on crashes, we find that DST does have a significant crash saving effect. Specifically, there is a 8-11% decline in pedestrian crashes and a 6-10% decline in vehicular crashes in the weeks following the switch to DST.

Our results suggest that short-term sleep disruption associated with DST does not cause a spurt in automobile crashes - a result that mostly contradicts findings in the existing literature. Our results might differ from the prior literature for 2 reasons. First, unlike most previous studies that used 3 to 4 years of data, we use a larger dataset spanning 28 years (1976-2003) of crash data. Second our empirical strategy not only controls for trends in crashes within each year, but also accounts for the pattern of collisions in the absence of DST by including a set of control years in our analysis. Thus, previous research might have attributed seasonal or week of the month changes in crash incidence to sleep disruption effects of DST. For example, in a separate analysis with the same data, we find that crashes generally tend to be higher towards the beginning of a month than towards the end – for all months and years in the data. In particular, we compare crashes on the first Monday of a month to the prior and subsequent Mondays and find a 7% ($p<0.01$) increase in the incidence of crashes on the first Monday. These results hold irrespective of whether we include or exclude the month of April (the month of the shift to DST) in the estimation sample. These results suggest that the significant sleep deprivation effect of DST found in some of the earlier work could be an artifact of such week of the month changes in crash incidence that are unrelated to DST.

Our results on the long run effects of DST are consistent with the prior literature, which also finds a significant reduction in fatal automobile crashes associated with DST. These results suggest that DST might not only conserve energy but also save lives by reducing traffic fatalities. We did find a somewhat stronger crash-saving effect of DST for pedestrian crashes, which is also consistent with findings in the previous literature. Moreover, results for vehicular crashes from the longer 28-year window (1976-2003) suggested that there was a decline in vehicular crashes even in the weeks before the start of DST for treatment years, making the evidence on the crash saving benefit of DST for vehicular crashes somewhat weaker. However, we did not find any such significant decline in vehicular crashes in the weeks preceding the start of DST for the treatment years, in the shorter 7-year window (1984-1990), the results from which are somewhat more reliable due to a smaller chance of there being any changes in the seasonal pattern of crash incidence over the shorter 7-year period. Hence, overall, we do find that DST saves lives by reducing the incidence of motor vehicle crashes – for both pedestrians and vehicular occupants.

The results of this study should be viewed in light of its limitations. 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. However, this concern is mitigated by the fact that our results are robust to restricting our analysis to a narrower 7-year window (1984-1990) when such changes in the pattern of crashes are less likely. Another limitation of our analysis is that our data are limited to fatal crashes only. Therefore, these data cannot capture changes in non-fatal crashes induced by the switch to DST. Further, due to significant missing data on speeding and alcohol related crashes, the analysis of these two types of crashes should be viewed with caution. We also note that our finding that short-term sleep deprivation due to DST does not increase automobile crashes does not imply that *severe* sleep deprivation is not a serious concern. Prior studies have established that long hours without sleep, extended work shifts, and being seriously fatigued increase the risk of traffic crashes (Itoi et al., 1993; Knipling and Wang, 1994; Horne and Reyner, 1995; Pack et al., 1995; McCartt et al., 1996; Dement, 1997; Lyznicki et al., 1998; Sagberg, 1999; Stutts, Wilkins, and Vaughn, 1999; McCartt et al., 2000; Cummings et al., 2001; Connor et al., 2002; Barger et al., 2005; See Connor et al., 2001 for a review of epidemiological studies).

The policy implications of this analysis are quite straightforward. We show that DST does save lives through reduced automobile crashes, and does not impose any short term costs through a higher risk of crashes due to sleep deprivation. Hence, from this perspective alone, the extension of DST in the U.S. by another 4 weeks from 2007 onwards (as per the Energy Policy Act, 2005) will save additional lives and also possibly reduce energy costs. However, for a full policy evaluation of DST, one also needs to consider the possible costs associated with retaining DST for an extended period of time. Such costs arise through the inconvenience of changing clocks twice a year, desynchronosis leading to lost productivity and risk of financial loss (Kamstra et al., 2000), and the inconvenience caused to farmers whose hours are set by the sun and not by clocks. Such an evaluation of all possible costs and benefits associated with DST is beyond the scope of this research but would be a fruitful avenue for future research.

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