Summary and Evaluation of Smith (2016)

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Introduction

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Originally introduced as a wartime measure (e.g. German Empire, Austria-Hungary 1916; USA 1918), the idea of Daylight Saving Time (DST) is simple: Aligning sunlight with wakeful hours saves energy, in particular used for lighting. To implement this, when days are getting longer at some point clocks are fast-forwarded by one hour such that sunrise and sunset occur one hour later than before (spring transition from Standard Time (ST) to DST), and when days are getting shorter again at some point clocks are turned back (fall transition) to reverse this effect. Today over 1.5 billion people are impacted by some form of DST. Places that do not observe DST include Africa and Asia, and equatorial places where little variation in sunrise and sunset times do not justify it.

Unfortunately, recent studies challenge the economic foundation of DST. In a quasi-experimental design, Kellogg and Wolff (2008) use that DST was extended in parts of Australia due to the 2000 Sydney Olympics. They show that while evening energy consumption decreased as expected, this effect was at least offset by higher morning consumption. Kotchen and Grant (2011) explore the issue of DST and energy savings based on the state of Indiana. With the exception of a couple of counties DST was not practiced in Indiana until the policy was introduced on state-level in 2006. Their results suggest that DST increases residential electricity demand by 1% (significant at 1% level). Notably, the assumption DST is grounded on, namely that it reduces energy for lighting, holds. However, Kotchen and Grant (2011) find that increased energy demand for heating and cooling more than offset energy savings related to lighting.

This recent evidence that DST fails on its main objective opens up a fundamental question: Is DST a good practice from a social welfare point of view or not? To tackle this question it is necessary to have a look at side effects of DST, of which there are, at least potentially, a whole bunch of. Some side effects are immediately apparent: Exemplarily, the transitions between DST and ST cause a disruption to meetings and travel, among others. Obviously, this is the reason for transition days being Sundays, because that keeps disruptions to weekday schedules as small as possible. Other side effects are more subtle. Doleac and Sanders (2015) show that because there is more ambient light in the evenings DST reduces crime in the US. They estimate that the immediate impact of the spring transition is a 7% decrease in robberies. Using data from Finland, Sipilä et al. (2016) find that ischemic stroke occurrences are increased in the first two days following a transition between ST and DST, presumably due to circadian rhythm disruption, which is likely to affect other health outcomes as well. In the end, side effects are manifold and difficult to compare, and effects are hard to be balanced out against each other.

[1]All figures are taken from the original paper. All regression tables are excerpts or compilations or both from figures in the original paper.

One additional major side effect of DST is its impact on fatal vehicle crashes. Sauber-Schatz et al. (2016) reports that each year more than 32,000 deaths and 2 million nonfatal injuries occur on US roads, making it the number one cause of accidental death in the US. The literature proposes two mechanisms that DST might affect crash counts through. Firstly, DST shifts the set of (clock) hours which fall under daylight and influences the lighting conditions under which we are on the streets. As fatal vehicle crashes happen to be more prevalent in the evening hours (c.f. Smith (2016), online appendix, figure A-1), shifting one hour of light from the morning hours to the higher risk evening hours might yield a net reduction of fatal vehicle crashes during DST. Secondly, because the transition days have 23 (spring) and 25 (fall) hours, respectively, the transitions disrupt sleep patterns and might influence the quantity of sleep. Barnes and Wagner (2009) use the American Time Use Survey to show that on average Americans sleep (significant) 40 minutes less on the night of the spring transition, but only (insignificant) 12 minutes more on the night of the fall transition. This finding indicates that the missing hour in the spring is largely made up for by cutting on hours devoted to sleep, whereas the additional hour in the fall is not or at least not in the first place used for sleep. In the literature, the terminology "ambient light mechanism" and "sleep mechanism" are used to refer to these potential mechanisms. Now even if it did so only slightly, if DST influenced crash risk it might prevent or cause sizeable numbers of crashes, and ultimately save or end the lives of quite a few individuals. Hence, there is a number of studies that try to estimate the impact of DST on fatal vehicle crashes.

However, the results of those studies did not create consensus. One set of studies is concerned with the sleep mechanism exclusively. At large, those studies compare crash counts for the Monday immediately after a transition to crash counts for other Mondays in that period of the year. Using Canadian data for 1991 and 1992, Coren (1996) finds that Mondays following the spring transition are on average associated with an 8% increase in traffic accidents and Mondays after the fall transition are associated with a decrease of roughly the same magnitude. Vincent (1998) uses Canadian data for 1984-1993, but fails to replicate Coren's results as he does not find any significant effects. Varughese and Allen (2001) find a significant increase in fatal crashes for the Monday following the spring transition for US data.[2] Lahti et al. (2010) do not find significant changes due to transitions between ST and DST in Finnish data for 1981-2006. At the end of the day, this set of studies suggests that DST increases crash counts or has no effect.

A second set of studies is concerned with the ambient light mechanism alone (e.g. Ferguson et al. (1995), Broughton et al. (1999) and Coate and Markowitz (2004)) and suggest that DST causes a net-reduction in fatal crashes. At large, those studies construct a measure of light quantity (e.g. as a function of minutes under daylight, twilight and darkness) for each hour and relate the number of fatal crash counts in a certain hour to the respective light level. Furthermore, some of them consider (simulated) crash counts under counterfactual light conditions, imposing DST light levels on the ST part of the year. For US and British data, those studies suggest that DST reduces road fatalities and in particular decreases crash risk for pedestrians. Furthermore, they propose to extend DST or practice it whole year round, based on their simulation-based predictions. However, those results and especially the predictions rely on potentially heavy assumptions, e.g. that there are no indirect effects of light levels on traffic levels and no systematic changes to driver behavior under counterfactual hours of light.

There are few studies that seek to unite both strands of literature and consider the sleep and the ambient light mechanism together. Sood and Ghosh (2007) note that results of earlier DST/crash studies might depend on whether one considers short term (immediately after the transition) or long term (over the whole time span under DST) impacts. They argue that the sleep mechanism might potentially account for short term increases in fatal vehicle crashes, and that improved visibility in the evenings through the ambient light mechanism might create long run beneficial effects. Using US data, Sood and Ghosh (2007) find no significant effects immediate to the spring transition and show that week-of-the-month fixed effects might have confounded earlier studies. In addition, they find a significant reduction in fatal crashes due to DST

[2] In addition, they detect that the fall transition day itself is associated with a significant increase, and argue that behavioral responses account for that. That is, people might anticipate that the subsequent day has 25 hours on Saturdays immediately preceding the fall transition. As a consequence, they stay up longer or consume more alcohol, which expresses itself in increased traffic fatalities on early Sunday mornings.

(6-11%) during the first nine weeks of DST. Notably, the authors mention that one limitation of their results is that they rely on the assumption that there were no changes to the seasonal crash profile over the years.

Using the same data set as Sood and Ghosh (2007) but considering more recent data (2002-2011), Smith (2016) aims to shed more light on the relation between DST and fatal vehicle crashes, and tries to address the concern that changes to seasonal crash profiles might have confounded earlier studies. The author makes use of a 2007 policy change[3] that extended DST both in the spring and in the fall.[4] Firstly he assesses the overall impact of DST, immediate as well as long term, by means of a regression discontinuity (RD) approach and a day-of-year fixed effects (FE) approach. Secondly, the two approaches and different identifying assumptions allow him to take a shot at disentangling the two mechanisms and assessing their quantitative impact. At the end of the day, Smith (2016) extends his analysis to earlier data[5], to reconcile his findings with the literature. The author finds a significant increase in fatal crash counts (5-6.5%) immediate to the spring transition, but no impact due to the fall transition. Beyond that, he argues that the sleep effect seems to be driving the results. Because he is able to do this along several ways employing independent assumptions, and because he gives a possible explanation for why his results differ from those of Sood and Ghosh (2007), he provides good reasons to actually believe his findings and furtherly disputes the practice of DST.

The remainder of this summary/discussion will be structured as follows: I will start with a review of the paper by Smith (2016). That is, I will briefly introduce his data and methodology, present overall results, explore the different ways the author assesses the quantitative impact of each of the two potential mechanisms, and outline how his results fit into the literature. Subsequently, I will discuss the paper and express some points of criticism, and finally conclude.

Review of Smith (2016)

Data

To tackle the question of how DST impacts the number of fatal crashes, the author employs the Fatality Analysis Reporting System (FARS) data compiled by the US National Highway Traffic and Safety Administration. FARS records both time and location of every fatal crash on public streets in the US since 1975. Precisely, a crash is considered to be fatal if it involves the death of a crash affected person within 30 days of the crash. For the main analysis the author considers data from 2002-2011, such that there are five years of data on each side of the 2007 extension, and for reconciliation with earlier studies the author extends his analysis to the older sample from 1976-2001. Because they constitute outliers the author excludes FARS designated holidays such as Independence Day and Christmas Day. Furthermore, since the transition days are not 24 hours long, respective crash counts are adjusted by counting the nearest-to-missing hour (3am-4am) twice in the spring and the hour that occurred twice (1am-2am) half in the fall. However, results are robust to multiplying spring transition crash counts by 24/23 and fall transition crash counts by 24/25, or just dropping transition days from the sample.

Empirical Strategy

The aim is to identify the (causal) impact of DST on the number of fatal crash counts. Dependent variable is the natural logarithm of the fatal crash count on day d in year y. Hence, estimates will be read as approximate percentage changes in fatal crash counts. All regression tables have robust standard errors reported in parentheses.

[3]This refers to the US Energy Policy Act from 2005, in which the spring transition date was altered from the first Sunday of April to the second Sunday of March, and the fall transition date from the last Sunday of October to the first Sunday of November. Effectively the extension is in force since 2007.

[4]Figure 6 in the appendix shows the impact of how DST influences sunrise and sunset times, and illustrates the change caused by the 2007 extension.

[5]That "earlier data" is roughly the set of data used in the study by Sood et al. as cited previously.

Regression Discontinuity (RD) Approach

Every year on the first Sunday of April (before the 2007 extension) or the second Sunday of March (after the 2007 extension), respectively, there is a discrete transition from ST to DST. All days (within a time frame that excludes previous or subsequent fall transition dates) that occur before the transition date come within ST, whereas all days that occur after come within DST. Transition in fall follows an analogous protocol. This discrete transition between ST and DST is exploited in a sharp RD design, which yields estimates of the immediate effect of a transition on fatal crash counts.

To eliminate day-of-week fixed effects (e.g. crash counts are higher on weekends than weekdays) and long term trends, the author demeans logged crash counts by weekday and year by regressing the former on day-of-week and year dummies. From this regression he obtains ten residuals for each day relative to the spring and fall transition, as he regards ten years of data. For both transitions, he averages the residuals for each day relative to the transition. With the averaged residuals, Smith (2016) follows the strategy outlined in Imbens and Lemieux (2008). That is, he runs local linear regressions[6] on both sides of the cutoff separately, using a uniform kernel[7] and the optimal bandwidth selector of Calonico et al. (2014).[8] The difference between the values from both regressions right at the cutoff informs the estimate of the immediate causal impact of the transition from ST to DST and vice versa, respectively, on fatal crash counts.

For the estimate to be consistent, it is required that the distributions of (demeaned) potential outcomes conditional on days relative to the transition, i.e. both the distribution of demeaned fatal crash counts that would realize under ST and DST are continuous in days relative to the transition (the forcing variable) around the transition date. In other words, consistency requires that all other factors besides ST/DST assignment which have an impact on fatal crash risk are continuous at the transition date. One should keep in mind that for each day relative to the transition, potential outcomes for either DST or ST is counterfactual, i.e. never realizes and hence always remains unobserved. This implies that in principle the required condition for consistency is untestable. However, only if the above holds true, we can compare observed demeaned fatal crash counts immediately before the transition to fatal crash counts immediately after the transition and attribute any discontinuities to the transition between ST and DST. Ultimately, Smith (2016) employs a placebo test that will be outlined later to back up the credibility of this assumption.

Day-of-Year Fixed Effects (FE) Approach

As the RD approach only yields estimates of immediate effects, there is obviously a demand for a strategy that allows to make a statement about longer lasting effects of the transition between ST and DST, firstly to reinforce RD results and secondly to enable a more thorough examination of the underlying mechanisms. Smith (2016) employs a Day-of-Year Fixed Effects Model which makes use of variation in DST assignment in the data.

Apparently, the 2007 extension created three weeks of variation in the spring and one week of variation in the fall. Because the DST assignment rule (transition on Sundays) creates an additional week of variation[9], there are roughly four weeks of variation in the spring and two weeks of variation in the fall. Hence, the sample includes switching days, i.e. days which are ST in some of the ten years of observation, but DST in others. After taking out any fixed effects related to the day of the year, the weekday and the year, comparing

[6]Note that one is in particular interested in the boundary values from the regressions. For this reason, local linear regression has serious advantages over local constant regression. E.g. for local constant regression, just as the function to be estimated is nonconstant around the cutoff (abstracting from the potential jump, for means of exposition think of a smooth function), the estimate for the value left of the cutoff will in practice tend to be too low if the function is increasing and too high if the function is decreasing. Contrarily, as long as the function to be estimated is not too convex or concave around the cutoff, (again abstracting form the jump,) local linear regression will provide a workable approximation.

[7]As Imbens and Lemieux as cited above argue, if different choices of kernel do make a difference, this is primarily indicative of sensitivity to bandwidth choice.

[8]I will focus only on this particular specification, but results are robust to different choices regarding kernel and bandwidth selector.

[9]Suppose the transition date is always the first Sunday of April, then this day varies from the 1st of April to the 7th of April.

demeaned crash counts of days falling under DST as opposed to falling under ST informs an estimate of the average effect of DST over switching dates.

Relative to a differences-in-differences approach (e.g. Sood and Ghosh (2007)), this approach is more robust to changes in the seasonal crash profile across years, as it not only considers differences between crash counts before and after the 2007 extension, but also makes use of variation due to the DST assignment rule. The estimation equation follows.

$$\ln(Fatals_{dy}) = \beta_0 + \beta_1 SpDST_{dy} + \beta_2 FaDST_{dy} + DayofYear_d + DayofWeek_{dy} + Year_y + \epsilon_{dy} \quad (1)$$

The logged number of fatal crashes of day d in year y is regressed on a constant, dummies for the day of the year, the weekday and the year to pull out fixed effects, and $SpDST_{dy}$ and $FaDST_{dy}$ are dummies for day d in year y falling under DST and occurring in spring (SpDST), i.e. before June 30th, or occurring in Fall (FaDST), i.e. after July 1st, respectively. In contrast to the parameter of interest in the RD approach, β_1 captures the average effect of a day in spring falling under DST as opposed to ST, and is identified by all spring switching dates. Analogously, β_2 is identified by fall switching dates and captures the effect for the fall.

Basic RD Results

According to the protocol outlined in the part on the RD approach in the previous section, figure 1 plots average residuals on days relative to the transition for the spring, the fall and respective placebos transitions, and solid lines result from nonparametric regression. Besides the seasonal trend that can be largely attributed to seasonal changes in vehicle miles traveled, Panel A displays a clear discontinuity in demeaned logged fatal vehicle crashes right at the spring transition. Contrarily, Panel C does not at all exhibit such a discontinuity for the fall transition.

The placebo tests deal with the concern that (in particular) the discontinuity at the spring transition might be caused by a change at the cutoff of the forcing variable (i.e. the transition) other than the treatment (i.e. DST as opposed to ST). Suppose for example that every year on the first Sunday of April (pre 2007 spring transition date) the Superbowl and on the second Sunday of March (post 2007 spring transition date) the finale of American Idol takes place, and that a large fraction of Americans go to work sleep deprived and with a hangover on the next day. This might in fact result in a discontinuity at the transition date as apparent in Panel A, which would have nothing to do *per se* with the transition from ST to DST, and confound our estimate of the parameter of interest. To test for such a possibility, the author assigns the post 2007 transition date to the pre 2007 data and vice versa. If the discontinuity in Panel A was indeed caused by changes at the cutoff such as the two contrived events above, Panel B should display a similar discontinuity as Panel A. That this is not the case backs up the assumption for consistency.

Figure 2 provides the according estimates. The immediate impact of the spring transition is an increase in fatal vehicle crashes by approximately 6.5% (significant at the 1% level), and the placebo transition yields a near zero point estimate. Splitting the sample into pre 2007 and post 2007 subsamples shows that the larger part of this aggregate effect is due to the impact of the spring transition in the subsample of data from 2002-2006, whereas the effect is insignificant for the 2007-2011 data. While cutting the sample into two halves obviously reduces precision, the author reads the subsample results as "point estimates remain[ing] positive and within a few percantage points of the combined sample" (c.f. Smith (2016), p. 76). However, the difference in point estimates between both subsamples of roughly two standard errors are somehow striking. The estimates in the lower part suggest that the fall transition has no immediate effect. The estimate for the placebo transition is weakly significant, but due to the negative result for the true transition this is probably of minor interest and as the author argues significance goes away for different bandwidths.

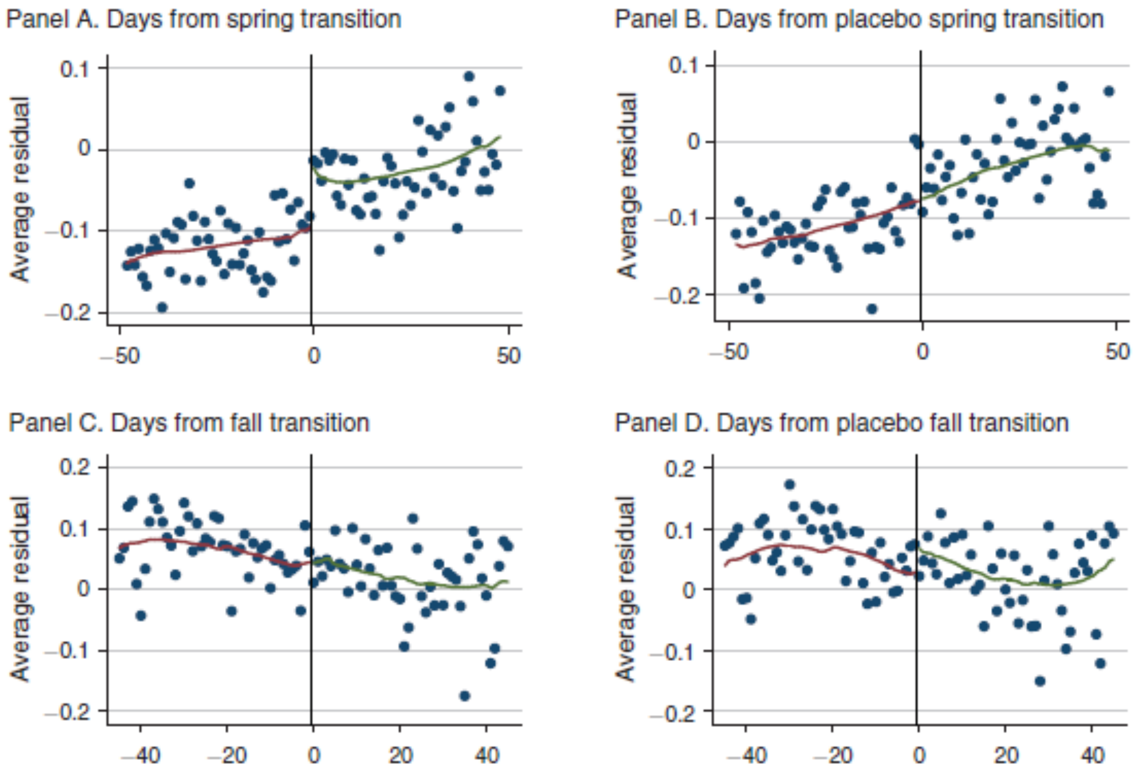

Figure 1: Residual Plots - Spring, Fall and Placebo Transitions.

Basic RD Estimates (Spring)

		Placebo	2002-2006	2007-2011
	(1)	(2)	(3)	(4)
DST	0.0649***	0.000536	0.0941***	0.0375
	(0.0231)	(0.0225)	(0.0302)	(0.0361)
Observations	550	550	235	265

Basic RD Estimates (Fall)

		Placebo	2002-2006	2007-2011
	(1)	(2)	(3)	(4)
Leaving DST	0.00114	0.0361*	0.0274	−0.00260
	(0.0236)	(0.0218)	(0.0265)	(0.0327)
Observations	381	381	215	225

Figure 2: Basic RD Estimates.

The different results for the spring and fall transitions could be explained in several ways.[10] The author

[10]For instance, the strong increase at the spring transition might be caused by sleep deprivation having per se even stronger effects but being mitigated by beneficial effects from more light in the evenings, and the effects from additional sleep and less light in the evenings might offset each other in the fall. Alternatively, there might just be no effect due to the ambient light

employs different identification assumptions to disentangle the effects of both mechanisms.

Investigation of the Two Mechanisms

The most straightforward and most assumption-heavy way to identify the effects of both mechanisms alone would be a simple spring-fall comparison. Recall that whereas the spring transition experiences a significant decrease in sleep on average, the average increase at the fall transition is insignificant (Barnes and Wagner, 2009). Hence, assuming that the fall transition is subject to the ambient light but not the sleep mechanism, and that both are active in the spring, the separate effects could be differentiated out. However, this obviously required a huge leap of faith. Firstly, despite being insignificant, the increase in sleep in the fall might have an effect. Secondly, the disruption due to a transition might affect not only sleep quantity, but also quality. Thirdly, sunrise and -set times in the spring and the angle of the sun differ between the spring and the fall transition dates, even in the absence of DST, and hence ambient light effects might not be perfectly comparable.

RD Subsampling

One possibility to explore both mechanisms in a more reliable way is to consider RD estimates for different subsamples. The idea is that hours distant from sunrise and sunset would be light/dark no matter whether the day falls under ST or DST, and should therefore not be affected by the ambient light mechanism. Apparently, the identifying assumption is that for this subset the ambient light mechanism is muted. Note that the transition alters the angle of the sun also for the subset of hours distant from sunrise and -set ("least-light-impacted hours"), so to believe the identifying assumption one has to be willing to assume that this has no effect on crash counts. For a comparison of effects on different subsets of hours to be helpful, the additional assumption that the sleep effect is constant over the day is required.

If the sleep mechanism is driving the overall spring results, a similar effect should be observed for the least-light-impacted hours, but if the ambient light mechanism drives the overall results, the effect on this subsample should at most be marginal. There are more hypotheses that can be derived for the subsampling results. The spring transition should experience more fatal crashes in the morning hours (less light) and a reduction of fatal crashes during the evening hours (more light).[11] Qualitatively, the results should reverse for the fall transition.

The subsequent figure 3 shows the estimates for the three subsamples at the fall transition. Column 1 shows the familiar aggregate effect. In line with the hypotheses, columns (2) and (3) show that crash risk is (with significance at the 1% level for both subsamples) shifted from the morning to the evening hours, with the effect on the evening being stronger. The near zero estimate in column (4) mirrors the aggregate result and suggests that there is no sizeable sleep effect.

Figure 4 presents the RD subsampling results for the spring transition. Column (2) shows that the immediate effect of the spring transition on the morning hours is an approximate increase in fatal crashes by 20%. Strikingly, there is only an insignificant decrease of approximately 3% for the evening hours (column (3)). This can be explained with the help of the estimate for the least-light-impacted hours in column (4), which exhibits a similar approximate increase in fatal crashes as the aggregate sample. As outlined above, this suggests that the sleep mechanism causes a significant increase in fatal crashes. Accordingly, the estimates for the morning and evening hours include both the ambient light effect and the sleep effect: The increase in fatal crashes in the morning hours due to a reduction of ambient light is reinforced by an additional increase

mechanism, and the insignificant additional sleep might not influence crash counts in the fall whereas the sleep mechanism might cause the increase at the spring transition.

[11] Rougly, for each location the author defines morning hours as +/- two hours from average sunrise time around the transition date for that location, and evening hours accordingly for sunset. All remaining hours are aggregated to a subsample of "least-light-impacted hours" that are distant to sunrise and sunset.

Fall RD Estimates by Time of Day

	Aggregate (1)	Morning (2)	Evening (3)	Least Light Impacted (4)
Leaving DST	0.00114	−0.115***	0.187***	−0.0134
	(0.0236)	(0.0501)	(0.0457)	(0.0275)
Observations	381	580	467	415

Figure 3: Fall RD Estimates by Time of Day.

due to sleep deprivation, whereas the beneficial effect on evening hours due to more ambient light is largely offset by the effect of sleep deprivation.

Spring RD Estimates by Time of Day

	Aggregate (1)	Morning (2)	Evening (3)	Least Light Impacted (4)
DST	0.0649***	0.198***	−0.0335	0.0751**
	(0.0231)	(0.0485)	(0.0388)	(0.0266)
Observations	550	810	790	530

Figure 4: Spring RD Estimates by Time of Day.

At large, the results from the RD subsampling approach indicate that the sleep mechanism drives the significant aggregate effect at the spring transition, whereas this mechanism is not active at the fall transition.[12] With regard to the ambient light mechanism, the results suggest that the beneficial effect on evening hours due to DST are largely offset by the negative effect on morning hours, and vice versa for ST.

Decomposing Spring DST in the FE Model

A different way to disentangle the effects that are due to ambient light and sleep is to decompose spring DST. If the overall increase at the spring transition is indeed caused by sleep deprivation, the estimate should decrease as we consider average effects over days further away from the transition. Because there are roughly four weeks of variation in DST assignment over the ten years of observation for the spring, the average effect of DST over switching days closely after the transition can be compared to the average effect over switching days that are some time after the transition.

Why should the interval after the fall transition not be decomposed accordingly? Recall that average effects are identified by the switching dates only, and that there are only roughly two weeks of switching dates in the fall. If one was interested in the average effect of a day occurring two weeks after the fall transition and falling under ST again as opposed to still being part of DST, there are just no switching dates to identify this effect. Even if one considers a year in which the transition date falls on the first day of the two week interval in fall, there are no switching days in our sample which are two weeks after this transition.

Figure 5 shows the resulting estimates. Column (1) presents the estimates for the coefficients β_1 and β_2 from the baseline FE model as outlined earlier. The average effect of DST over spring switching dates is a weakly significant approximate 3% increase in fatal crashes, and the average effect of DST over fall switching

[12] Of course, this is to a large extent an identification assumption.

FE Estimates - Decomposing Spring DST

	All Hours		Least light impacted	Morning	Evening
	(1)	(2)	(3)	(4)	(5)
Spring DST	0.0319*				
	(0.0165)				
First six days of DST		0.0565**	0.0574**	0.205***	−0.0265
		(0.0231)	(0.0272)	(0.0514)	(0.0453)
Next eight days of DST		0.0245	0.0289	0.130**	−0.0812*
		(0.0201)	(0.0234)	(0.0603)	(0.0450)
Remainder of spring DST		0.0142	0.00907	0.126**	−0.0588
		(0.0197)	(0.0230)	(0.0553)	(0.0429)
Fall DST	0.0228	0.0218	0.0446	0.259***	−0.159***
	(0.0249)	(0.0250)	(0.0303)	(0.0709)	(0.0482)
Observations	3,341	3,341	3,341	3,341	3,341
Adjusted R^2	0.734	0.735	0.753	0.184	0.319

Figure 5: Decomposing Spring DST in the FE Model.

dates[13] is an insignificant approximate 2% increase. Column (2) decomposes spring DST into the first six days, the next eight days, and remaining (switching) days at least two weeks after the transition. Column (3) decomposes spring DST and is concerned only with the subset of least light impacted hours. Analogously, columns (4) and (5) regard the subsamples of morning and evening hours.

Observe that the point estimates with exception of column (5) decrease as one goes further away from the spring transition, which is consistent with a diminishing sleep effect. Next, note that for all coefficients the estimates for the subsample of least-light-impacted hours (3) are really close to the aggregate estimates (1), what reinforces the hypothesis that the sleep mechanism drives overall results, whereas effects due to the ambient light mechanism balance out. Lastly, consider the estimate on the remainder of spring for the subsample of least-light-impacted hours: Two weeks after the transition the sleep effect should have completely dissipated (and Smith (2016) refers to sleep studies here, but obviously this is also an identification assumption), and in the subsample at hand the light mechanism is muted. Therefore, the near zero point estimate indicates that the author is not missing a third important mechanism through which DST impacts fatal crashes. Overall, the results from this approach are in line with the results from RD subsampling.

Smith (2016) presents further evidence from an examination of reported crash factors, which are left out for brevity and rely on some assumptions and a bunch of back-of-the-envelope calculations. Essentially it is argued that immediately after the spring transition there is an increased frequency of driver drowsiness being reported as a crash factor, complementing the evidence for a significant sleep effect.

Reconciliation With Earlier Studies

As outlined in the introduction, earlier studies occasionally find a net reduction in fatal crashes due to DST or do not find that the sleep mechanism has an impact. In an attempt to reconcile his findings with the literature, Smith (2016) extends his analysis to a historical sample of the FARS data (1976-2001). Importantly, in 1986 there was a policy intervention that extended DST by three weeks in the spring. This

[13] Note the higher standard errors due to less switching dates being available.

spring DST extension, quite similar to the 2007 extension of spring DST, is used to run the FE model analysis on the historical sample.

The author's findings are twofold (c.f. figure 7 in the appendix). One the one hand the earlier sample exhibits a significant and dissipating sleep effect, similar in size to the recent sample's effect, that can be observed with regard to the subsample of least-light-impacted hours. On the other hand, there is a significant (1% level) reduction of fatal crashes of approximately 4% for the remainder of spring in the historical sample, where there should not be an effect due to the sleep mechanism anymore. However, the average effect for the remainder of spring in the subsample of least-light-impacted hours is close to zero. This indicates that there was a net reduction of fatal crashes during DST in the historical sample due to the ambient light mechanism, caused solely by its impact on hours close to sunrise and -set.

The author concludes that earlier studies found mixed results because both mechanisms had opposing effects at the spring transition. Furthermore, he attributes the dissipation of net effects due to ambient light over the years to changes in the daily crash profile. Although crashes in the more recent sample are still more frequent in the evening than in the morning, this differential has decreased from the historical sample to the recent one (c.f. Smith (2016), online appendix figure B-4).

Discussion

Given that recent studies challenge the idea that DST saves energy, it is unclear whether it is a good policy from a social welfare point of view, and side effects should play a role in its evaluation. Resolving the dissent on the impact of DST on fatal vehicle crashes which is arguably one of the major side effects is a relevant contribution to the evaluation of the policy. Smith (2016) shows that DST increases fatal crash counts for a couple of days after the spring transition due to sleep deprivation. In particular the synergy of both approaches (RD, FE) and the use of different and partially independent identification assumptions (no effect through ambient light mechanism on least-light-impacted hours, dissipating sleep effect/no sleep effect two weeks after a transition, sleep effect constant over the day) contributes to the persuasiveness of his study. Also the exhibition of how his and earlier studies fit together under consideration of the recent and the historical sample adds to the persuasive power of his results.

However, there are some points to be criticized in the paper under scrutiny. Not thematized in this review, in his main FE specification the author controls for vehicle miles traveled using gas price as an instrument to preclude that seasonal changes to driving patterns confound estimates. It is observed that the estimates for the parameters of interest actually do not change at all. Note though that day-of-year fixed effects take up any seasonal within year variation, hence also variation in vehicle miles traveled, so there should really be no substantial point in instrumenting for vehicle miles traveled, although it also does not really hurt.

Another point regards the results from decomposing spring in the FE Model shown in figure 5 (Table 5 in the paper). The author complains about the anomaly in column (5), where the estimate for "Next eight days of DST" is smaller than for "Remainder of spring DST". As the 2007 extension shifted the spring transition by three weeks, the "Remainder of spring DST" estimate is basically constructed by comparison of demeaned fatal crash counts on the last days of the 4 week variation interval before 2007 (given the pre 2007 transition date, they sometimes fall under ST but never occur two weeks after the transition) and after 2007 (given the post 2007 transition date, they never fall under ST but some of them occur two weeks after the transition). In other words, for this parameter the years before 2007 are "control" and the years after 2007 are "treated", and the author is in some sense effectively back in a difference-in-difference framework. It would be interesting to see whether the anomaly survived if this problem was accounted for, and the author should have noted that the potential confounder of seasonal changes in crash risk across years he claims to address with the FE approach persists for this parameter.

One thing that caused me a lot of grief personally was that the estimation equations the author reports are not identified without further constraints (c.f. Smith (2016), pp. 73f). I reckon that the constraint imposed in the RD equation is f(0)=0 and that the constraints in the FE equation are that all three fixed effects are zero on average. However, I am not sure about the extent to which the respective constraints are common convention.[14]

Upon evaluation of the study, it is to be mentioned that a bunch of ideas the author implements are not new. For instance, the idea to use the discontinuous transition between ST and DST together with an extension of DST to identify effects can be found in Doleac and Sanders (2015), and the idea of subsampling the least light impacted hours to examine the effects of manipulation of hours hit with daylight is used in Kellogg and Wolff (2008). Nevertheless, the study is well done and the use of the two different estimation strategies and several approaches regarding identification is definitely an achievement worth mentioning.

To conclude with, in his comment on the policy of DST Smith (2016) focuses a lot on the immediate impact of the spring transition, referring to effects lasting one or two weeks, e.g. in his back-of-the-envelope calculation on deaths caused by DST. However, he disregards the fact that effects that last over the whole period of DST/ST, even if they are considerably smaller, might in the end have a bigger overall impact. For this reason, I would be cautious to deny net effects due to the ambient light mechanism, but rather note that it requires a precise quantification of longer-lasting effects to make a reliable statement.

Conclusion

Smith (2016) documents that the spring transition from ST to DST (temporarily) increases crash risk by well over 5%, while there is no effect on the number of fatal crashes from the fall transition. He shows that the sleep mechanism is driving overall effects, and that the ambient light effect has a huge impact on crash risk during hours close to sunrise/-set but merely reallocates crash risk within a day without having a net effect. The author calculates that at the end of the day the practice of DST through the sleep loss due to the spring transition causes roughly 30 deaths related to vehicle crashes per year. Furthermore, the evidence for the sleep effect suggests that the practice of DST impacts the economy besides its effect on energy consumption. For instance, employees are more prone to errors and in general less productive if sleep deprived (e.g. Lockley et al. (2007), Wagner et al. (2012)), what they are likely to be in the days subsequent to the spring DST transition. The study shows that side effects of DST are relevant and should be taken into account in an evaluation of the policy, and as recent studies challenge that DST decreases energy consumption, there is an obvious demand for a thorough and general reevaluation of the policy.

Appendix

The Influence of Daylight Saving Time on Ambient Light for St. Louis, Missouri.

Figure 6 depicts sunrise and sunset times for St. Louis, Missouri, before the 2007 DST extension (solid lines) and after (dashed). The wavelike alteration of sunrise and sunset times is disrupted by the spring (fall) transition, where clocks are fast forwarded (turned back) by one hour. As a consequence, the sun rises one hour later (earlier), but also sets one hour later (earlier), that is one hour of light is shifted from the morning to the evening (vice versa).

FE Estimates of the Impact of DST Across Time.

Figure 7 shows the FE estimates for the recent (2002-2011, columns (1) and (2)) and the historical (1976-2001, columns (3) and (4)) sample. Spring DST is decomposed into the first six days, the next eight days and the remainder of spring. Whereas the estimates for the least-light-impacted hours do not differ between

[14] For the sake of completeness, the author is missing subscripts at one place in the RD estimation equation and reports a negative standard error on one estimate in online appendix table B-7.

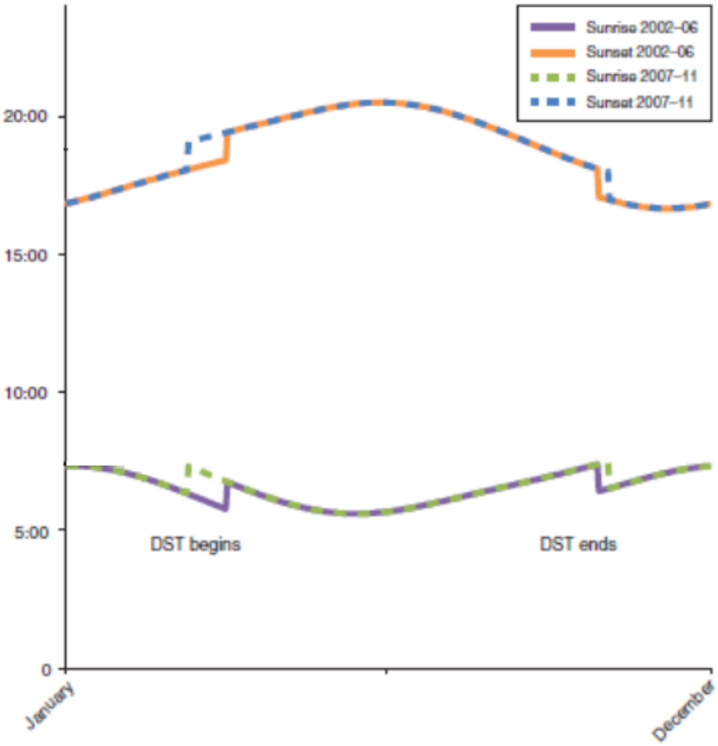

Figure 6: The Influence of Daylight Saving Time on Ambient Light for St. Louis, Missouri.

the recent and the historical sample, they do differ for the aggregate of all hours. This indicates that on the one hand there was an effect due to the sleep mechanism in the historical sample similar to the effect in the recent one, and on the other hand the ambient light mechanism had a different impact on fatal crash counts in the recent sample than it had in the historical one. Because there was no extension to DST in the fall during 1976-2001, the author focuses only on the spring.

	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

Figure 7: FE Estimates of the Impact of DST Across Time.

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