

Under the Cover of Darkness: How Ambient Light Influences Criminal Activity

Jennifer L. Doleac* Nicholas J. Sanders †

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Abstract

We exploit Daylight Saving Time (DST) as an exogenous shock to daylight, using both the discontinuous nature of the policy and the 2007 extension of DST, to consider how light impacts criminal activity. Regression discontinuity estimates show a 7% decrease in robberies following the shift to DST. As expected, effects are largest during the hours directly affected by the shift in daylight. We discuss our findings within the context of criminal decision-making and labor supply, and estimate that the 2007 DST extension resulted in \$59 million in annual social cost savings from avoided robberies.

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*Frank Batten School of Leadership and Public Policy, University of Virginia, Charlottesville, VA 22904. Email: jdoleac@virginia.edu.

†Department of Economics, College of William & Mary, Williamsburg, VA, 23185. Email: njsanders@wm.edu.

“Only the government would believe you could cut a foot off the top of a blanket, sew it to the bottom, and have a longer blanket.” — Unknown

1 Introduction

Social organization around a common understanding of time demonstrates the importance of the clock in daily life. Social norms assign the time one should wake up, attend work or school, eat lunch, return home, and sleep. Time coordination plays a major role in social interaction; Hamermesh, Myers, and Pocock (2008) show even something as simple as television viewing schedules can influence time coordination among individuals. Though advancements in recordable television relaxed this particular restriction of time, the clock in many ways still dictates daily time use. Regardless of whether it is light or dark outside, or personal desires for different schedules, most follow the default instructions provided by the clock. This suggests we should pay attention to whether default schedules — or, equivalently, the clock itself — are set optimally.

One important question is whether clocks sync optimally with ambient daylight. Ambient light can impact human behavior in a number of ways, such as quality of sleep and alertness during the day. For example, Wong (2012) and Carrell, Maghakian, and West (2011) show the impact of school schedules on student outcomes, including school day start and end times on academic performance. Could ambient light also affect individual safety? If criminals are less likely to offend in broad daylight, and schedules relative to clock time are mostly fixed (as for those with “9 to 5” jobs), the amount of ambient light at key hours could affect public safety, which suggests society could reduce the overall social costs of crime by simply shifting the clock.

Criminal response to something as basic as outdoor environment is not without precedent. Research finds the, in general, calendar affects criminal behavior, but researchers know less

about the impact of ambient light on outdoor crime.¹ By increasing the likelihood of capture and the expected cost of criminal activity, light lowers the net expected wage from crime and could deter criminal behavior. Policy-makers and law enforcement have long presumed this effect. Alternatively, increasing light might increase street crime if individuals stay out later, increasing the probability of interacting with a criminal and decreasing criminal search costs. The additional foot traffic could increase the “demand” for crime even as we expect the “supply” to decrease. The net effect is most relevant to policy-makers, but difficult to obtain without random assignment of ambient light. The exogenous shift of daylight caused by Daylight Saving Time (DST) provides an opportunity to consider the role of light in street crime.

DST shifts an hour of available daylight from the morning to the evening each day in the spring, and back to the morning in the fall. The United States Congress has extended the length of DST a number of times with the intent of decreasing energy consumption, but occasionally cites an additional benefit of a decrease in criminal activity. Most street crime occurs in the evening around common commuting hours of 5 to 8 PM, and more ambient light during typical high-crime hours makes it easier for victims and passers-by to see potential threats and later identify wrongdoers (Calandrillo and Buehler, 2008). If people adhere to default schedules, shifting DST could have a meaningful effect on crime. But humans adapt, so it is not obvious that shifting daylight from one time of day to another would change the total amount of any activity. Criminals might adjust behavior to follow the darkness (or daylight)². It is ultimately an empirical question whether DST make a difference in this context, and we are the first to rigorously analyze the impact of DST on crime rates. Such analysis is important — because the start and end dates of DST are arbitrary, there is often debate whether timing is optimal. The social cost of violent crime is high, so even a

¹See Heaton (2012) for evidence that liberalizing bans on Sunday liquor sales increased minor crime and alcohol-involved serious crime, and Jacob and Lefgren (2003) for evidence that juvenile delinquency increases when students are on summer vacation.

²Such behavioral adjustment seems to be the case for energy consumption, as we discuss in Section 2.

small drop in crime rates due to an increase in evening daylight could make extending DST cost-effective.

We use both a regression discontinuity (RD) design and a difference-in-difference (DID) approach to test the impact of a change in ambient light on street crime, using the DST variation in sunset times as an exogenous shock to light. We focus on two different sources of variation for identification, as DST varies the amount of ambient light during high-crime hours of the day in two ways. First, under DST, in the spring (fall) of each year, the sun discontinuously rises and sets an hour later (earlier). Second, due to a legislated extension of DST, during a three-week (one-week) period in the spring (fall), the sun rises and sets an hour later during the same period in 2007 and 2008 than it did in 2005 and 2006. The legislation extending DST in 2007 provides an opportunity to directly control for time-of-year effects, which would otherwise be a concern since DST occurs simultaneously across 48 states (Arizona and Hawaii do not observe DST) and at approximately the same time each year.³

The RD model exploits the amount of daylight in key hours changing discontinuously from one day to the next, while other factors that affect crime outcomes are “smoothly changing” over the year. Our DID approach uses the three-week policy change the 2007 DST extension caused, combined with the within-day variation of the impact of DST on light. We hypothesize DST has the strongest impact during the hours of light transition (sunrise and sunset) — all other hours of the day remain either light or dark as before. We compare the shift in criminal activity during the two hours just after the pre-DST sunset time to the shift in criminal activity for all other hours.

RD results show daily cases of robbery, a violent and socially costly street crime, decrease

³An additional interesting case is that of Indiana, where observance of DST varied across counties for a period of time. Kotchen and Grant (2011) use this variation, and the eventual shift to common-state observance, as a quasi-experiment to help identify the impacts of DST of energy use. Despite the intended purpose of DST as a source of energy savings, they find DST may have increased residential electricity demand.

by approximately 7% in the weeks after DST begins, with a 19% drop in the probability of any robbery occurring. A 27% decrease in the robbery rate during the sunset hours drives much of this result. Our finding is highly robust to various RD specifications, and we find no such effects when rerunning the analyses using “placebo” dates to further test for general time trends. DID results similarly suggest a 20% decrease in the robbery rate during sunset hours. We also consider other violent crimes: rape, aggravated assault, and murder. We find no consistent impacts for aggravated assault, but suggestive evidence of impacts for rape and murder, though results are more sensitive to time-of-year controls than robbery. Using the social cost of crime, we estimate the benefit of the 2007 shift of DST was a national decrease of \$246 million in social crime costs per year, a nationwide social savings of \$12 million per hour of additional ambient light during high-crime hours⁴.

As an additional consideration, we examine our results as a potential indication of criminal labor supply. By increasing the within-hour probability of capture, and thus the within-hour expected cost of crime, all else held constant DST lowers the hourly net wage for robbery. Our hour-specific results suggest criminals are not reallocating robbery activity to alternate hours during the day, which accompanied by the total drop in robberies suggests criminals decrease their activity when the net wage decreases, at least in the short run. We further provide the first large-scale demonstration of how ambient light impacts crime rates in the United States, and evidence on the optimal timing of daylight with respect to public safety.⁵

The remainder of this paper proceeds as follows: Section 2 provides background on DST policy and the relevant changes used for identification. Section 3 describes a model for what

⁴This assumes criminals do not shift avoided robberies to other times of year. We argue consumption smoothing across more and less lucrative times of year is unlikely for this population, which typically does not have the financial resources (i.e., savings) or ability (i.e., bank accounts, discount rates) to go without income for long periods of time. Intertemporal shifts across hours are more likely than intertemporal shifts across months, and consider the former in our analysis. However, this is ultimately a general equilibrium question that our empirical strategy cannot directly address.

⁵Van Koppen and Jansen (1999) tackle a similar topic using data from the Netherlands between 1988 and 1994, though their variation comes from daylight hours in summer vs. winter (given the large differences in darkness in the Netherlands across seasons).

factors might influence crime and how they relate to our analysis. Section 4 describes the data. Section 5 details our empirical strategies. Section 6 considers the results, and explores the robustness of our findings. Section 7 provides discussion of possible mechanisms and policy implications, including avoided social costs of crime.

2 Daylight Saving Time

DST shifts the relationship between clock time and sunset. At 2 AM on the first day of DST, clocks shift ahead one hour, removing a clock-recorded hour from that day and reallocating daylight from the early morning to the evening hours by pushing sunrise and sunset back one hour. Later in the year, at the end of DST, clocks shift from 3 AM back to 2 AM, adding a clock-recorded hour to that day and reallocating daylight from the evening back to the morning. Anecdotal history suggests DST was first posed by Benjamin Franklin as a means to save money on candles by moving daylight from a time when few were working in the morning to a later, more work-intensive time — despite the move from a wax-based lighting infrastructure, policy-makers still cite DST as a means of energy conservation (Prerau, 2005). In reality, history credits George Vernon Hudson with the development of the more modern version of DST.

Energy savings has been the expressed goal of every recent change to DST policy. A Congressional experiment in 1974 extended DST to last for a full year (clocks were not returned to their baseline time in the fall), with the goal of reducing energy consumption during a foreign oil embargo. In 1986, Congress permanently extended DST by one month to begin earlier in the spring (April), and in 2005, voted to permanently extend DST (effective in 2007), citing the events of September 11th, 2001, and ongoing wars in the Middle East as driving popular interest in reducing America’s dependence on foreign oil. This most recent change moved the start of DST from the first Sunday in April to the second Sunday of March, and pushed the end back from the last Sunday of October to the first Sunday of

November.⁶ We focus on the impact of the beginning (spring shift) of DST, as the 2007 policy produced a larger change in the spring than in the fall (three weeks vs. one week), and we are concerned fall timing associated with Halloween is a confounder. We do, however, show fall results largely agree with our spring findings.

Despite the intent of reducing energy and fuel use, empirical evidence suggests changes in DST did no such thing. Using variation in DST policy across the state of Indiana, Kotchen and Grant (2011) show DST resulted in an increase in energy consumption. Using changes in DST policy in Australia prompted by hosting the Olympics, Kellogg and Wolff (2008) find no energy savings. DST does, however, appear to have an impact on daily activity — Wolff and Makino (2012) find the larger blocks of evening daylight produced by DST induce people to spend more time outdoors, with the positive health effect of burning an average of 10% more calories per day.

While no recorded changes in DST explicitly target criminal activity, an observational study of the 1974 year-long DST experiment suggested violent crime fell 10-13% in Washington, D.C. during the impacted time of year (Calandrillo and Buehler, 2008). While small in scope and isolated to a comparison of across-year crime rates, discussion of DST as a crime-reducing policy often cites this result. Our paper tests for this effect across the country, using richer, more recent data and a cleaner natural experiment. Prior to considering these effects, however, we consider how DST might impact criminal behavior in a theoretical framework. We first pose the choice to engage in criminal behavior as a function of, among other things, ambient light and the probability of capture. We then consider how criminal labor supply might shift in response to the increased cost of criminal behavior associated with higher probability of capture.

⁶The week in the fall was reportedly due to lobbying by candy manufacturers to include Halloween (NPR, 2007).

3 Factors in Criminal Deterrence

The classic Becker (1968) model of crime predicts a rational criminal will break the law if the expected benefit exceeds the expected cost. The expected cost of crime is a function increasing in the probability someone will catch the criminal and the discounted punishment he would receive. Thus the number of crimes criminals commit should fall if society does any of the following: (1) incarcerates more likely offenders, (2) increases the probability of apprehending offenders who commit new crimes, and (3) makes punishments more severe.

Changes in crime come in two forms: an incapacitation effect, and a deterrent effect. Incarcerating offenders has an incapacitation effect where individuals are physically prevented from committing crimes. But incarceration is extremely expensive, and the experience of prison could have negative long-term effects on the inmates and their families. Increasing punishment has a deterrent effect, in that it increases the expected cost of crime, making criminal activity less appealing to potential offenders and influencing the marginal criminal in their decision. But it is an open question whether or not potential criminals can be meaningfully deterred from offending by increasing the expected cost of crime.⁷ Lengthy sentences have little to no deterrent effect, possibly because offenders highly discount the future (Lee and McCrary, 2005), and individuals who are impatient are unlikely to base today's decisions on a change that they only feel years from now.

It is a top policy priority to find more cost-effective ways to decrease crime, and focusing on how offenders respond to changes in the other parameter of the expected cost function — the likelihood of getting caught — might lead policy-makers toward more promising interventions.⁸ Indeed, all else held constant, the social planner prefers policies that increase the deterrence factor, as they have a lower overall cost to society: the crime never occurs (saving victims) and incarceration is unnecessary.⁹ However, legislators must be careful

⁷See Abrams (2012) for a review of the literature on the deterrent effect of longer sentences.

⁸See for example Cook and Ludwig (2011), Doleac (2012), Kilmer, Nicosia, Heaton, and Midgette (2013).

⁹Increasing law enforcement employment is one way to deter criminal behavior via probability of capture.

that policies are cost-effective and do not have unintended consequences that mitigate any deterrent effect.¹⁰

3.1 Ambient Light and its Effect on Crime

We conduct our analysis in the framework of a simple model of criminal behavior, where criminals attempt a crime if the expected benefits are greater than the expected costs. More light means witness as more likely to spot criminals committing crimes and more likely to recognize and identify criminals apprehended at a later time. Let the expected cost of crime be a function of the (discounted) length of sentence if captured (T) and probability of capture (P), which is a function of ambient light (L) as well as a large number of other factors (F) such as number of police, etc. We treat criminal behavior as a labor decision, thus we also include a disutility from labor factor (D), which includes search costs for potential victims, and thus depends on ambient light (L). An individual will commit a crime if:

$$E[Benefit_{crime}] > E[Cost(T, P(L, F), D(L))_{crime}]. \quad (1)$$

In partial equilibrium, we expect $\partial P/\partial L$ and $\partial C/\partial P$ to be positive; greater amounts of light increase the probability of capture, which increases the cost of crime and decreases the propensity to commit crime. In general equilibrium, the effect of additional light is ambiguous. If, for example, more light means individuals are more likely to remain outdoors longer, as is suggested by Wolff and Makino (2012), this increases the number of potential victims for criminals, decreasing search costs ($\partial D/\partial L < 0$), which in turn decreases the expected cost of crime ($\partial C/\partial D > 0$). We are unable to directly separate between these two

Prior evidence suggests this is effective, though police do more than simply arrest suspects, so the precise treatment is unclear (Levitt, 2004). Similarly, databases and registries that make it easier to identify suspects increase the probability of catching repeat offenders (Doleac, 2012). For instance, adding offenders to DNA databases appears to decrease crime rates due to a combination of deterrent and incapacitation effects.

¹⁰For instance, Prescott and Rockoff (2011) and Agan (2011) find no beneficial impact of sex offender registries on crime or recidivism.

effects; we enterpret our results as the net effect of an increase in ambient light from DST.

Our analysis allows us to superficially consider the role of both the incapacitation and deterrence effects. We separately consider changes in total daily crime and crime within hours where DST directly affects light. Even with increased light, some criminals will still choose to offend, and will face a higher probability of capture and incarceration. Once off the streets, they will be unable to commit additional crimes during any hour of the day. The incapacitation effect of DST on crime will be evident at all hours of the day, but any deterrent effect should be operative during the evening hours that were formerly dark but are now light¹¹.

3.2 Investigating Daily Criminal Labor Supply

Labor supply models provide a framework to model criminal behavior. Without information on how victims adjust behavior as a product of DST, we are unable to consider whether criminal search costs increase or decrease. However, we can begin to address the issue of daily labor supply for criminals. Camerer, Babcock, Loewenstein, and Thaler (1997) consider a similar question when they investigate how taxi drivers adjust daily labor supply when hourly wages vary with the effort required to find patrons, while Jacob, Lefgren, and Moretti (2007) consider criminal substitution across longer time periods when weather displaces criminal activity. Like cab drivers, criminals are “self-employed” and have the ability to choose the number of hours in which they engage in criminal activity. Our further analog here is one of criminals searching for “patrons” — do criminals adjust their daily labor supply when the net hourly wage changes? We restrict our discussion here to robbery, the crime where discussion of a net wage is most comparable.

In a classic labor model, individuals work more hours when net wages are higher, and

¹¹DST shifts the hour of sunrise as well. We focus on sunset, as most street crime occurs in the evenings. In prior versions of this paper we specifically considered the hour of sunrise as well, and saw no DST-related shift in behavior in the morning. Hourly results shown in the Appendix address this issue as well.

conversely work fewer hours when net wages are lower (in favor of substituting away to leisure). We consider the net hourly wage of criminal behavior as the expected benefits of criminal activity minus the expected costs. The expected benefit for robbery is the financial return, while the expected costs are an increasing function of the probability of capture. DST should result in a lower net wage, and the classic model predicts fewer crimes, which would mean not just a reduction in crime during the hour of daylight shift but also for the day overall. This parallels the standard model of criminal deterrence. A behavioral model would suggest lower net wages result in increased criminal hours in an attempt to obtain some set level of criminal income, and may result in a net daily impact of zero. We cannot observe the number of hours “worked” by criminals, but we do observe the number of crimes reported. We use this as a measure of the volume of criminal activity.

4 Data

We obtain crime data from the National Incident-Based Reporting System (NIBRS) for years 2005-2008. NIBRS data include detailed information on each reported crime, including the hour of occurrence, the type of committed offense, and whether or not there was an arrest. NIBRS classifies reporting areas as jurisdictions, which vary in size and geographic makeup. For example, a jurisdiction could be a county, a city government, or a combination of similar institutions. Though NIBRS reporting has gradually expanded over time, the geographic scope remains limited. As of 2007, jurisdictions reporting to NIBRS covered approximately 25% of the population and 25% of crimes reported in the Uniform Crime Reporting (UCR) system, and while some larger cities report, the data are disproportionately from smaller population centers. For example, though Texas reports data to NIBRS, reporting jurisdictions only cover around 20% of the state population, and only one reporting jurisdiction has a population over one million. How criminals make timing decisions might vary between

highly urban areas and more rural zones, and we interpret our results with this in mind.¹² For our primary analysis, we restrict attention to jurisdictions that consistently reported for two years prior to the 2007 DST extension and two years after.¹³ In the end, we have 558 jurisdictions covering a total population from 22 to 24 million persons, depending on the year. Data are predominantly in the eastern portion of the country. Figure 1 maps reporting regions, separated by time zone.

Our primary focus is on the crime of felony robbery. This is often a street crime in which the victim does not know the offender (muggings, for instance, would be classified as robberies), and thus should be particularly affected by ambient light. It also is one of the few financially motivated violent crimes, and thus responsive to changes in net wage.¹⁴ We also consider additional violent crimes that might represent robberies gone wrong: rape, aggravated assault, and murder. However, NIBRS data show victims are much more likely to know their offenders for these crimes, so we expect a substantially more muted impact.

If the classic labor model holds, then: (1) the largest effects should occur during the hours directly impacted by DST (those just around sunset), where the net wage for robbery has decreased the most, and (2) total criminal behavior should decrease. If ambient light is the relevant mechanism and criminals are not operating in a behavioral model, DST should not increase crime at 3 PM, which is light both directly before and after DST, or 10 PM, which is dark both directly before and after DST. If offenders are making up for lost time, however, criminals should increase activity in different hours.

To better measure the direct timing of the effect, we match reporting regions to sunset

¹²For a detailed listing of which regions report by state and population coverage, see http://www.jrsa.org/ibrcc/background-status/nibrs_states.shtml.

¹³In prior version of this paper, we found our general results were robust to using a non-balanced panel (available upon request).

¹⁴In earlier versions of this paper, we expanded our analysis to possible placebo crimes, such as forgery and swindling, that should be unaffected by darkness, and other property crimes (Doleac and Sanders, 2012). However, such crimes face the complication that the reported time of the crime is very noisy. For example, individuals discover a burglary upon returning home, or a stolen car on the following morning, but have no idea what time during the day the burglary occurred. Robbery remains our main focus, as the time of occurrence is likely well known.

records. Using latitude and longitude data from NIBRS and daily sunrise and sunset times from the National Oceanic and Atmospheric Administration, we calculate the specific daily hour of sunset for each jurisdiction. Figure 2 is a frequency histogram of sunset times used in our analysis, by year, using the recorded sunset time for the day directly before the beginning of DST in the spring. Times are earlier in 2007 and 2008, as sunset gradually occurs later as the year progresses and DST begins three weeks earlier in those years. We define the DST treatment variable of interest as a binary indicator that takes a value of one during DST and zero at all other times. DST is “off” in the beginning of the year, “on” beginning April 3, 2005; April 2, 2006; March 11, 2007; and March 9, 2008, and “off” again beginning October 30, 2005; October 29, 2006; November 4, 2007; and November 2, 2008. Crime rates trend differently throughout the year, and RD estimates are most valid in the area of the discontinuity. We restrict the majority of our analysis within 3 weeks of the DST cutoff in each year, though in robustness checks we expand our bandwidth to 8 weeks on either side of the DST transition and allow for flexible time trends. We also investigate other times of year where we expect no shock to daylight as placebo tests.

Table 1 shows the raw, non-trend adjusted average crime rate per million persons for all crimes in our analysis, for the 3 weeks before and after the spring transition of DST. The first column shows averages across all weeks and all years. Columns 2 and 3 split the sample into pre- and post-DST, but still show daily totals. Columns 4 and 5 focus on the same six week framework, but focus on crime in only the hours around sunset. The second panel shows the population, in millions, covered by these reports each year, as well as the number of reporting used jurisdictions (which is constant across years).

5 Empirical Strategy

We first consider the effect of DST on daily crime rates. This is the relevant policy question in determining the cost-effectiveness of DST. It also speaks to the question of criminal labor

supply, in that it addresses whether or not criminals reallocate activity across hours in the day to maintain a constant daily total, or whether the relationship between daylight and clock time matters. Next, we consider impacts by hour of the day. If ambient light is important in the criminal activity decision, changes in daily crime rates will be strongest during the hours of light transition that, prior to DST, were dark, but are now light.¹⁵ This is the time that has the greatest relative increase in ambient light making it the “treated” period.¹⁶

5.1 Regression Discontinuity

We begin with a regression discontinuity (RD) design, where the running variable is days before and after DST, scaled such that the running variable is equal to 0 at the first day of DST. This is not directly equivalent to using day-of-year as our running variable, as DST is determined not by a specific date but by a specific Sunday in the month independent of calendar date. We control for the running variable using a linear model with a varied slope on either side of the cutoff.

Despite the discontinuous nature of DST, the use of time as the running variable means some assumptions of RD may fail. DST always begins on a Sunday, which has different crime patterns than other days. As a potential adjustment, we include day of week fixed effects. Given prior findings that weather can impact criminal behavior (Jacob, Lefgren, and Moretti, 2007), we also control for daily county level average temperature and rainfall¹⁷. Finally, we include jurisdiction-by-year fixed effects to allow for baseline differences in crime

¹⁵We therefore expect that the criminal response should be largest during the “time since sunset” hours of 0 and 1, the periods covering sunset and dusk. Dusk is the time at which it becomes completely dark. It occurs, on average, about 30 minutes after sunset.

¹⁶We include more information on how we calculate time since sunset in the replication files. In prior versions, we conducted the same analysis using specific hour of day rather than hour relative to sunset. Results were similar and present only in the hours most frequently impacted by shifting sunset (6 and 7 PM). We demonstrate these results in the Appendix.

¹⁷Weather data are from Schlenker and Roberts (2009).

rates across reporting jurisdictions and years.

$$crime = \alpha + \beta_1 day + \beta_2 DST + \beta_3 DST * day + \omega W + \lambda_{jurisdiction \times year} + \gamma_{dow}, \quad (2)$$

where W is a vector of weather variables and λ and γ are the noted fixed effects. We use two different outcomes of interest: (1) crimes per million population, a continuous variable, and (2) an indicator function for whether or not a crime occurred in a given jurisdiction/time cell, which we estimate using a linear probability model. We do not control for population, as jurisdiction-by-year fixed effects indirectly contain this information. However, we do weight regressions by the jurisdiction population. We cluster all standard errors by jurisdiction to allow for common variation in crime rates. Our analysis is similar for both individual hours and daily results, where we sum all crimes to daily totals using the outcome of crimes per million.

5.2 Difference-in-Difference

Our DID model uses both the variation in the timing of DST across years and the variation in the impact of DST across hours of the day. For this specification, we limit analysis to the time period that is standard time before the 2007 policy change, but classified as DST from 2007 onward. The earlier beginning of DST is March 9th (2008), and the latest is April 3rd (2006), so our analysis includes 25 days per year. We again use crimes per million and probability of any crime occurring as our outcomes of interest, and collapse all data to the day-by-sunset level: the hour of sunset (hour 0) and just following sunset (hour 1) comprise one group, while all other hours of the day comprise the other. The relevant regression is,

$$crime = \alpha + \beta_1 Post2007 + \beta_2 sunset + \beta_3 sunset \times Post2007 \quad (3)$$

Given the use of hours within the same day as a control group, we can omit all variables that do not vary by hour. We omit day of week and jurisdiction-by-year fixed effects, as they provide no additional identification for β_3 , the coefficient of interest. As with RD estimates, we weight all regressions by population.

6 Results

6.1 Regression Discontinuity

Figure 3 shows a visual illustration of our local linear estimates for robbery, rape, aggravated assault, and murder rates before and after DST. We use a bandwidth of 21 days to estimate the shape of changes in crime rates over time to match our range choice in our regressions, and weight all by population using the following estimation (we omit subscripts are omitted for simplicity),

$$crime = \alpha + \beta_1 day + \beta_2 DST + \beta_3 DST * day, \quad (4)$$

We use this regression to generate a predicted value for each day, which we then graph as a solid line. Scatter points are average true observed crime rates, collapsed to the daily level, though we omit weekends, which have much higher crime rates, for a more readable axis (note weekends are included in all regressions below). The robbery figure shows a clear, large change in the pattern of daily total crimes. Graphs for other crimes are less suggestive, with little deviation from trend and no persistent effects.

The first two columns of Table 2 show RD results from equation (2) using total daily crime rates for robbery, rape, aggravated assault, and murder as outcomes. Column 1 shows results using crimes per million. Aside from the addition of weather controls and time fixed effects, these regressions are the analog of Figure 3, and show a similar pattern. We find an economically significant reduction in robbery, where DST results in a 7% drop in incidences

per million, though the result is only significant at 10%. We also see effects for rape, which has a decrease of 11% and is again significant at 10%. No statistically significant results exist for aggravated assault or murder.

Column 2 repeats the analysis using a linear probability model (LPM) with the binary outcome of “did any incident of crime X occur in this jurisdiction on this day.” This has the benefit of being less sensitive to outliers, such as an unusually large number of robberies on a single day.¹⁸ Results are similar to the crimes per million outcomes. DST results in a 1.5 percentage point drop in the probability of any robbery occurring on a given day, a decrease of approximately 19%. We do not find statistically significant effects for any other crime, suggesting some outlier days may be responsible for the rape findings using crimes per million.

We next consider crimes reported in specific hours. Hourly data can suffer from issues such as flawed recording, incorrect victim recall, and any other such sources of measurement error, and we approach the following analysis with that in mind. However, in almost all cases hourly analysis strongly supports that (1) criminals engaging in robbery alter their behavior most drastically in the hours most affected by the DST policy, and (2) criminals engaging in robbery do not shift their behavior to other hours of the day in a consistent manner. We focus on the former point, and leave the latter for the Online Appendix.

Columns 3 and 4 of Table 2 mirror those of Columns 1 and 2, but focus on the hours most affected by daylight change (0 and 1 hours from calculated sunset as discussed above). All regressions include weather controls as well as day of week and jurisdiction-by-year fixed effects. DST correlates with 0.12 fewer robberies per million during the hours following sunset (a decrease of 27% from pre-DST means, significant at the 1% level), or a decrease of 0.7 percentage points in the probability of any robbery occurring (a 10% decrease, significant at the 10% level). DST correlates with 0.35 fewer rapes per million during hours following

¹⁸For computational simplicity when using a large number of fixed effects, we prefer the LPM. We repeat the analysis using a logit, and find similar results (available upon request).

sunset (a decrease of 38%, significant at the 10% level). Again, we find no statistically significant effects for any other crime.

6.2 RD Robustness checks

In the Appendix, we discuss and present results of a wide variety of robustness checks, including using different bandwidths and polynomials, additional controls, and restricted samples. We also present results of placebo tests using fake DST dates, and show results for all hours to test for reallocation of criminal activity across the day. Finally, we test for effects in the fall, and compare effects on weekdays (when commuters are more prevalent in the evening hours) with those on weekends. All Appendix tests support our main findings.

6.3 Difference-in-Difference results

Despite the discontinuous nature of Daylight Saving Time, the use of time as the running variable can complicate the RD design. One identifying assumption of the RD model is the continuity of unobservable factors that determine outcomes (crime rates) with respect to the running variable (time). Given DST always occurs on a Sunday, our data may violate this assumption. Controlling for day of week fixed effects can help reduce that particular issue, but there remain other time factors, such as the timing of holidays, that may further complicate identification. The 2007 policy change helps control for this concern, as DST occurs at a different time of year for 2 years of our analysis. Additionally, the test for effects by hour is a check for such complications — there is no reason potential confounders would systematically only impact the hours that are most sensitive to Daylight Saving Time with regards to light shift. As an additional check for non-policy related background trends, we repeat our analysis using a difference-in-difference model that does not depend on the same assumptions as the RD.

Our difference-in-difference results take advantage of the period in March that is standard

time during 2005 and 2006 but DST during 2007 and 2008, along with the fact that the light impacts of DST only appears to matter during the hours of sunset. We thus collapse our crime rates to two observations per day: one during the hours of sunset, and the other for all other hours. Columns 5 and 6 of Table 2 show difference-in-difference results for all four crimes. As with RD, in the difference-in-difference model only robbery shows a consistent, statistically significant decrease in crime. The DID estimate shows a drop of 0.21 robberies per million population, equivalent to a 20% decrease. This result is very similar to the RD estimate described above. Using the LPM, the DID interaction suggests a 2.7 percentage point drop in the probability of a robbery.

Figure 4 illustrates our robbery result graphically. We run the following regression,

$$crime = \beta_1 + \tau_{hours} + \beta_2 post2007 + \pi_{hours \times post2007} + \lambda_{jurisdiction}. \quad (5)$$

The coefficients from the vector π represent the difference in crime rates, by hour, for the same time of year between the years 2005/2006, when the month of March was not DST, and 2007/2008, when it was. Figure 4 plots those coefficients, along with the 95% confidence interval, for each hour of the day. The hours of sunset are the only ones that see a systematic decrease in robbery after 2007.

7 Discussion and Conclusion

We present the first rigorous empirical estimates of the effect of ambient light on violent crime. We find DST lowers robbery rates by 7%, with the largest results occurring during the hours most affected by the shift in daylight. This effect is large but not unreasonable relative to other interventions that operate primarily by increasing the probability of capture. For instance, Ayres and Levitt (1998) find that the availability of LoJack anti-theft technology reduces auto theft by 10%; and Kilmer, Nicosia, Heaton, and Midgette (2013) find that

requiring frequent breathalyzer tests as a condition of community release or probation reduces DUI arrests by 12% and domestic violence arrests by 9%.

The impact of DST on robbery rates is the net effect of several factors, particularly if the “prime time” for crime is when most people are on their way home after work: (1) Daylight itself could discourage offenders from committing crime because they are more visible and easier to identify; (2) DST might increase foot traffic at key times due to the later sunset, which might increase the number of potential witnesses in addition to increasing visibility, though this could also increase the number of potential victims; (3) Changes in offenders’ schedules due to the later sunset (later family dinners or sports practices, substitution for their own leisure, etc.) might make them unavailable to commit crime until after most potential victims have gone home. The first two explanations imply DST has a deterrent effect on crime, while the third explanation implies an incapacitation effect that does not rely on incarceration. Regardless of the mechanism, it is clear the relationship between daylight and clock time matters when it comes to crime.

One must compare the benefits of avoided crimes, along with the potential health benefits found in Wolff and Makino (2012), with cost increases associated with DST. In addition to potentially increasing energy consumption, DST appears to have several other negative consequences. A 2012 poll by Rasmussen Reports found only 45% of Americans think DST is “worth the hassle,” and remembering to change one’s clocks—and occasionally being early or late for appointments—is inconvenient (Rasmussen, 2012). Groups consistently lobbying against DST extensions include the national Parent Teacher Association (PTA), which expressed concern children are at risk of being kidnapped while waiting in the dark for a schoolbus, and the airline industry, because changing flight schedules is costly¹⁹.

The growing literature on the negative effect of early school start times on academic per-

¹⁹We find no evidence that ambient light affects kidnapping, but statistical power is low (results available upon request.) The Air Transport Association estimated that the 2007 extension would cost airlines \$147 million (Koch, 2005).

formance suggests extending DST could negatively affect students by making classes earlier relative to sunrise (Wong, 2012).²⁰ Medical research on circadian rhythms suggests shifts in the sleep cycle can have negative impacts on response time and cognition, and on the Monday following DST there is higher observed rate of traffic accidents, workplace injuries, and heart attacks (Coren, 1996; Varughese and Allen, 2001; Barnes and Wagner, 2009). Janszky and Ljung (2008) note that changing one's clocks "can disrupt chronobiologic rhythms and influence the duration and quality of sleep" for several days, and also hypothesize negative physical effects as a result of the policy. However, most of these costs are due to the switch from Standard Time to DST rather than the impact of a later sunset per se, and are likely small in comparison to the benefits of the substantial drop in violent crime.

There remains the specific valuation of the social benefits of the decreased crime seen as a result of DST. McCollister, French, and Fang (2010) estimate the social cost of a robbery at \$42,310.²¹ A back-of-the-envelope calculation implies the three-week extension of DST avoids \$59.2 million nationally each year in avoided robberies.²² If we include the suggested impacts on rape (with an estimated social costs per crime of \$240,776), the total social cost savings come to \$246 million. These savings are from the three-week period of DST extension. General equilibrium effects are likely to vary substantially across different seasons and geographic regions, so one should do out-of-sample prediction with caution, but assuming a linear effects in other months, the implied social savings from a permanent,

²⁰While Carrell, Maghakian, and West (2011) also consider how early classes impact school performance, their effect is independent of sunrise and thus should not be a long-term effect of DST. However, the deprivation of sleep schedules in the initial time shift may have its own effects.

²¹The social costs of crime include estimated tangible and intangible costs. McCollister, French, and Fang (2010) divide these into four categories: (1) direct economic losses suffered by the crime victim, including medical care costs, lost earnings, and property loss/damage; (2) local, state, and federal government funds spent on police protection, legal and adjudication services, and corrections programs, including incarceration; (3) opportunity costs associated with the criminals choice to engage in illegal rather than legal and productive activities; and (4) indirect losses suffered by crime victims, including pain and suffering, decreased quality of life, and psychological distress.

²²We base these calculations on an estimated reduction in crimes per 1,000,000 residents per day, 21 days of DST, and a US population of approximately 310 million. The number of robberies prevented each year is: $0.215 * 21 * (310,000,000 / 1,000,000) = 1,400$.

year-long change in ambient light would be almost 20 times higher.

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Table 1: Average Crimes per Million Population For the Three Weeks Prior and Three Weeks Post Daylight Saving Time

Crime Rate per Million	Total	All Day		Sunset Hour	
		Pre-DST	Post-DST	Pre-DST	Post-DST
Robbery	3.286 (8.816)	3.192 (8.696)	3.381 (8.933)	0.448 (2.838)	0.341 (2.498)
Rape	1.046 (5.222)	1.036 (5.251)	1.056 (5.192)	0.093 (1.478)	0.081 (2.776)
Aggravated Assault	8.747 (16.996)	8.193 (16.254)	9.300 (17.69)	0.950 (5.059)	1.143 (5.44)
Murder	0.141 (1.631)	0.142 (1.634)	0.140 (1.628)	0.016 (0.648)	0.011 (0.451)
Year		2005	2006	2007	2008
Total Population (1,000,000)	-	22.998	23.194	23.449	23.651
Total Reporting Jurisdictions					558

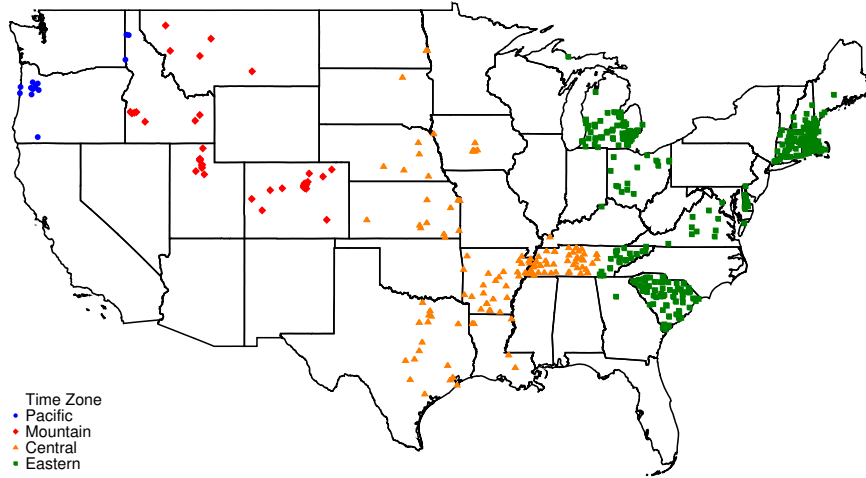
Notes: Daily total is the average of total daily crimes, calculated by summing hourly data across all hours within the day. Sunset hour data is the average of total crimes occurring in the hour of sunset and the hour directly following sunset (dusk). Standard deviations are reported in parentheses. Population and crime data come from the National Incident-Based Reporting System (NIBRS). Jurisdiction refers to region used for collecting crime data, and generally refers to a county, city, or similar municipality. Means are weighted by population.

Table 2: Effects of DST on crime

	Regression Discontinuity: Daily Totals		Regression Discontinuity Sunset Hour		Diff-in-Diff: Sunset vs. Other Hours	
	Crimes Per 1,000,000	Prob. Of Crime Occurring	Crimes Per 1,000,000	Prob. Of Crime Occurring	Crimes Per 1,000,000	Prob. Of Crime Occurring
Robbery	-0.215* (0.122)	-0.015** (0.008)	-0.120** (0.041)	-0.007* (0.004)	-0.214*** (0.081)	-0.027*** (0.008)
Share of Pre-DST Mean	-0.07	-0.19	-0.27	-0.10	-0.20	-0.22
Rape	-0.119* (0.069)	-0.003 (0.007)	-0.35* (0.019)	-0.004 (0.003)	0.058 (0.052)	0.006 (0.008)
Share of Pre-DST Mean	-0.11	-0.06	-0.38	-0.32	0.17	0.14
Agg. Assault	0.350 (0.213)	0.000 (0.008)	0.041 (0.070)	-0.008 (0.006)	-0.012 (0.212)	-0.011 (0.007)
Share of Pre-DST Mean	0.04	0.00	0.04	-0.08	-0.00	-0.06
Murder	-0.010 (0.035)	0.005 (0.010)	-0.002 (0.007)	-0.002 (0.002)	-0.018 (0.015)	-0.007 (0.007)
Share of Pre-DST Mean	-0.07	0.88	-0.89	-0.67	-0.37	-0.65

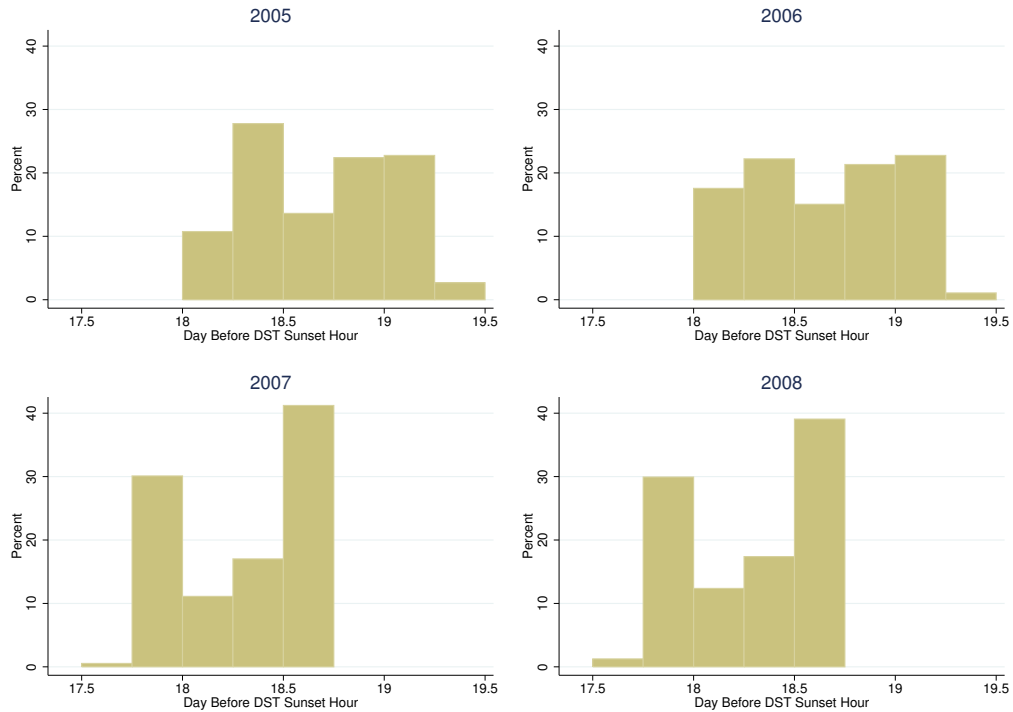
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the jurisdiction level. The outcome variable is either (a) crimes per million population or (b) the probability of at least one of the crimes occurring, as the column headers describe. Population-weighted coefficients show the change in the outcome variable due to the transition to Daylight Saving Time (DST). We calculate hours since sunset using data on the hour of sunset for each jurisdiction on the day prior to the beginning of DST. "Sunset hours" refer to the hour of and just following sunset. All regression discontinuity models include day of week fixed effects, jurisdiction-by-year fixed effects, controls for weather (county average daily temperature and rainfall), and a running variable control of days since the beginning of DST, where we allow the slope of the running variable to vary before and after DST. Difference-in-difference regressions include data from March 9th through April 3rd in all four years of the analysis. First difference is whether or not the included weeks classified as DST, which varies by year (not classified as DST in 2005/2006, classified as DST in 2007/2008). Second difference is whether or not the crime occurred in a hour classified as impacted by sunset (hours 0 and 1, as calculated in Section 5). Regressions use 558 jurisdictions, with a total of 94,744 day-by-hour-by-jurisdiction observations for the three weeks prior to and the three weeks following the beginning of DST (for the RD regressions) and 116,064 hour-group-by-day-by-jurisdiction observations (for the DID regressions). Population and crime data come from the National Incident-Based Reporting System (NIBRS).

Figure 1: Reporting Regions Used in Primary Analysis



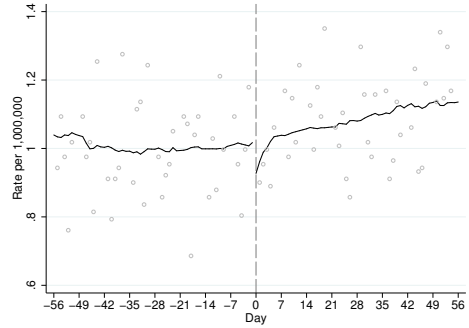
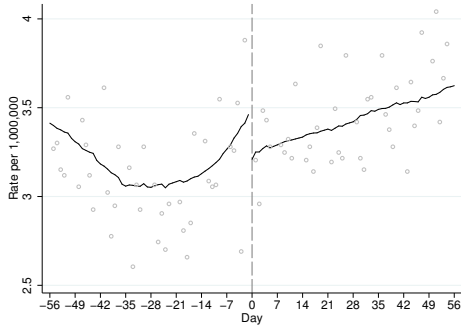
Notes: Latitude and longitude information are taken from the 2005 Law Enforcement Identifiers Crosswalk. Each point is one of the 558 reporting jurisdictions included in the main analysis, as described in Section 4.

Figure 2: Distribution of Sunset Times in the Day Before DST

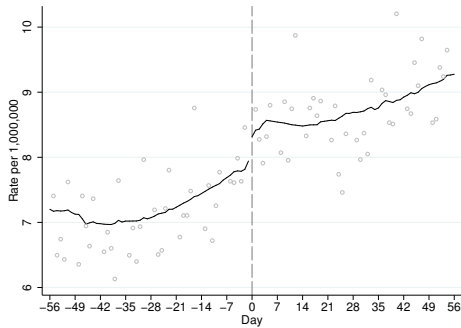


Notes: Sunset times are taken from www.timeanddate.com/worldclock/sunrise.html and are calculated as described in the Replication Files. Vertical axis represents the number of different sunset times used, where jurisdiction sunset time is determined by latitude and longitude. Horizontal axis shows time of day using 24-hour time.

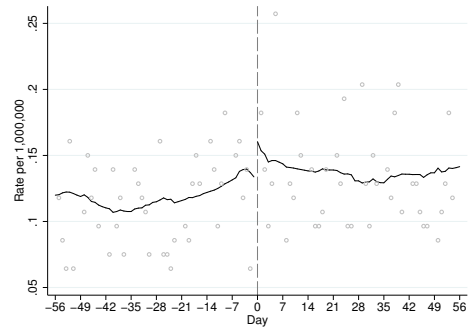
Figure 3: Daily Estimates of Local Linear Regression RD Impact
 Robbery Rate per Million
 Rape Rate per Million



Agg. Assault Rate per Million

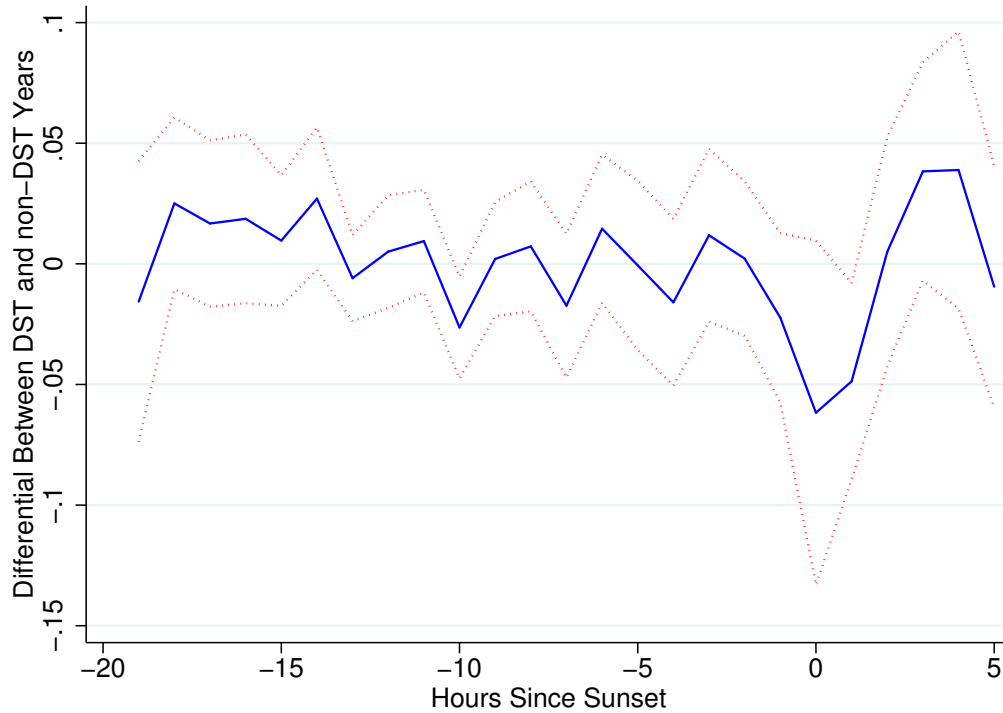


Murder Rate per Million



Notes: Solid lines in all graphs are predicted outcome values based on a local linear regression as specified by equation 4 in Section 6.1. Outcome variable is crimes per million population, where individual figure titles indicate specific crimes. Horizontal axis variable “Day” refers to days since the beginning of Daylight Saving Time, which varies by year. Scatter plot is collapsed outcomes, by days since DST. Graphs do not include weekend data for display simplicity.

Figure 4: Difference in Robberies per Million Population Across 2007 Policy Change by Hours Since Sunset



Notes: Solid line indicates coefficients on an interaction between hours since sunset and whether or not the day was classified as Daylight Saving Time, which varies by year. Estimates are done using equation (5) in Section 6.3. Data include only days in March and April which were classified as standard time in 2005/2006 but as DST in 2007/2008. Dotted line shows the 95% confidence interval for the estimates, where standard errors are clustered by jurisdiction.

A Online Appendix

A.1 Robustness Checks

In this section, we examine how robust our daily and hourly results are to a variety of RD specifications. We focus on robbery, given it is the only crime with consistent significant effects in our main estimates. Table A-2 shows our results. Panel A shows results using daily totals. Panel B focuses on the hours of sunset. In Column 1, we replicate our main result a point of reference. In Column 2, we use a more restrictive bandwidth of 2 weeks, while Column 3 expands our bandwidth to 8 weeks with a cubic polynomial in the running variable (with varied slope on either side of the cutoff). Column 4 includes week of year fixed effects. Column 5 repeats the main analysis without weighting by population. Columns 6 and 7 focus on jurisdictions with the larger populations, where outliers are less likely influence our results: Column 6 uses only jurisdictions in the upper 50th percentile of the sample population, while Column 7 further restricts to only the upper 25th percentile.

Daily results are largely stable. In all cases but the bandwidth of 2 weeks, point estimates are within a standard error of the baseline specification, including inclusion of week of year effects. However, only the larger bandwidth results are significant at 5%, as many of our robustness checks are taxing on the data. Results follow a similar pattern for hours of sunset, but are more consistently significant at 5%. In all cases estimates are within a standard error of the primary model. The persistence using the longer bandwidth provides an interesting contrast to Jacob, Lefgren, and Moretti (2007), who find short term shifts in crime due to weather tend to become muted in the long run. One fundamental difference is that DST effects are less transitory than weather, and in the spring transition daylight continues to increase during the year for the evening hours. While we prefer the continuity of the “hours since sunset” model (as sunset times change when the DST policy shifted earlier in the year), as an alternate specification, we show robbery rates by hour of day rather than hours since

daylight. Table A-1 shows results by hour for 4-8 pm. Results are negative and largest for the hours of 6 and 7 pm, though only 7 pm is statistically significant at conventional levels (at 1%).

As a whole, results support a DST caused a change in the behavior of criminals engaging in robbery. A remaining concern is whether RD results are an indication of criminal response or a product of background effects for which our model cannot sufficiently control. If, for example, crime is shifting during the year in a manner for which time controls do not adequately account, we might incorrectly attribute this to a discontinuous impact of DST. Table A-3 tests for such effects by assigning a “false” DST either 6 weeks prior to the true treatment or 6 weeks post and repeating our daily results. In each case, we repeat the “Daily Totals regressions from Table 2 now using the false DST as treatment. Panel A shows results for the earlier period, and Panel B shows results for the later period. In both cases the LPM finds no statistically significant effects for any of the four crimes considered. When considering crimes per million population, 7 of the 8 estimates are statistically insignificant. We only find statistically significant results for murder, and in this case only for the “late” DST, though murder is a sufficiently rare crime such that results can be singular outliers rather than true shifts — the lack of any result using the LPM, which is less sensitive to such issues, is more indicative of true crime patterns.

As an additional demonstration of the unique nature of our effects by time of year, we repeat our main RD estimates assigning a false DST for each day of the year, ranging from 60 days before to 260 days following the true beginning of DST. Panel A of Appendix Figure A-1 shows the distribution of all estimated effects, where the vertical dashed line indicates our main result estimates. Panel B of Appendix Figure A-1 plots the estimated effects by days since the true beginning of DST. We plot estimates that are statistically significant at 5 percent with diamonds, with all statistically insignificant estimates with an x. Combined, the two graphs show (1) the estimates during the true time of DST are the most negative

of all estimates, and (2) almost every other estimate is statistically insignificant. Indeed, of the 310 regressions we run, only 10 of the estimated effects are statistically significant at 5 percent, and 5 of those are clustered around the true DST period.

A.1.1 Reallocation to other hours

As part of our investigation into the criminal model of labor supply, we consider whether or not criminals actively reallocate robbery to different hours. Jacob, Lefgren, and Moretti (2007) find suggestive evidence of temporal reallocation of criminal activity on a longer time scale, but little is known about how criminals respond in the short run. The use of daily results will address this to a degree, as reallocation across hours should result in a zero net outcome for the day. As an additional check Table A-4 includes all 2-hour groupings ranging from 18 hours before to 6 hours following sunset. We use 2-hour groupings to better avoid problems of across-hour measurement error or any one hour being influenced by singular outliers, and to better match our reported results in Table 2. We see statistically significant impacts only during the evening hours of sunset. This strongly supports the classic labor supply model, where criminals work fewer overall hours when the net wage of robbery decreases.

A.1.2 Fall vs. spring

We focus on results for the spring DST transition, because the fall DST transition occurs around Halloween. Our concern is that trick-or-treating and any associated activity will confound our results, along with potentially unusual criminal activity. Even so, for completeness we run our analysis using the timing of the fall time change. Table A-5 mirrors the analysis in Table 2 but uses the fall transition — here, DST is “on” for the first part of the sample and “off” at the year progresses. No results are statistically significant. Robbery maintains the same sign as before, but is significantly smaller in magnitude (though due to very large

standard errors, we cannot reject an effect similar to the spring transition).

A.1.3 Weekdays vs. weekends

Availability of potential crime victims is constantly changing throughout the day. When discussing the impacts by hour of day, and why criminals may or may not reallocate behavior across different hours, it is important to note prime commuting times (5-8 pm) are also the hours most impacted by changes in daylight as a product of DST. One reason criminals may not actively shift behavior to later hours (which remain dark even after DST) is that the supply of potential victims is much lower outside commuting hours, so committing crime outside prime commuting hours is less desirable.

There is no direct test as to the supply of victims, but if commute time is a major factor the impact on crime rates should occur primarily on weekdays, when people most often commute from work. Table A-6 shows results where we include an indicator for weekends and interact it with the indicator for DST. The regression model now becomes,

$$crime = \alpha + \beta_1 day + \beta_2 DST + \beta_3 DST * day + \beta_4 weekendXDST + \omega W + \lambda_{jurisdictionXyear} + \gamma_{dow} \quad (6)$$

Panel A shows daily results using crimes per million, while Panel B shows the probability of any crime occurring using the LPM. In both cases, all robbery results occur during the weekdays. This further supports that DST has large impacts when commuting hours line up with hours impacted by DST.

Separating results this way also suggests some impacts for rape and murder during the weekend, though neither holds for both the crimes per million model and LPM. This need not contradict the commuter time model, as we expect criminal behavior for rape and murder to differ from robbery. The NIBRS data suggest most rape victims know their assailants, and commuter traffic is not a likely optimal scenario for seeking rape victims. However, we

have no *a priori* reasoning why effects would be larger on weekends.

A.2 Tables and Figures

Table A-1: Regression Discontinuity Estimates of the Impact of Daylight Saving Time on Robbery Rate by Clock Hour

	4 pm	5 pm	6 pm	7 pm	8 pm
DST	-0.022 (0.022)	0.043 (0.022)*	-0.038 (0.021)*	-0.086 (0.029)***	0.028 (0.031)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the jurisdiction level, are reported in parentheses. Regressions use crimes per million population as the main variable of interest. Regressions are weighted by population and coefficients should be interpreted as the change in the number of crimes per million population that occurs within the relevant hour with the transition to Daylight Saving Time (DST). All regressions include day of week fixed effects, jurisdiction-by-year fixed effects, controls for weather (county average daily temperature and rainfall), and a running variable control of days since the beginning of DST, where the slope of the running variable is allowed to vary before and after DST. Regressions use 558 jurisdictions, with a total 94,744 day-by-hour-by-jurisdiction observations for the three weeks prior to and the three weeks following the beginning of DST. Population and crime data come from the National Incident-Based Reporting System (NIBRS).

Table A-2: Variations on the Regression Discontinuity Specification Using Daily and Hourly Robbery Rates

	Bandwidth of			Include Week of Year Fixed Effects	No Population Weights	Only Larger Population > Median	> 75th Perc.
	Basic	2 Weeks (Linear)	8 Weeks (Cubic)				
Panel A: Daily Results							
DST	-0.215* (0.122)	-0.071 (0.145)	-0.298** (0.135)	-0.203 (0.313)	-0.175 (0.173)	-0.199 (0.137)	-0.315* (0.164)
Panel B: Sunset Hours							
DST	-0.120*** (0.042)	-0.090* (0.052)	-0.115** (0.048)	-0.188 (0.127)	-0.151** (0.06)	-0.116** (0.05)	-0.171** (0.07)
Observations	93,744	62,496	241,056	93,744	93,744	46,872	23,520

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the jurisdiction level, are reported in parentheses. Regressions use robberies per million population as the main variable of interest. Panel A: Results are for the daily robbery rate. Panel B: Results are for the combined hour of sunset and hour directly following sunset. Hours since sunset are calculated using data on the hour of sunset for each jurisdiction on the day prior to the beginning of Daylight Saving Time (DST). Unless otherwise indicated, regressions are weighted by population and coefficients should be interpreted as the change in the number of crimes per million population that occurs within the relevant day or hours with the transition to Daylight Saving Time (DST). All regressions include day of week fixed effects, jurisdiction-by-year fixed effects, controls for weather (county average daily temperature and rainfall), and a running variable control of days since the beginning of DST, where the slope of the running variable is allowed to vary before and after DST. Population and crime data come from the National Incident-Based Reporting System (NIBRS). Column 1 is the baseline result and the equivalent of the combined results in Column 1 of Table 2. Column 2 uses a bandwidth of 2 weeks. Column 3 uses a bandwidth of 8 weeks and allows for cubic functions of either side of the cutoff. Column 4 includes week-of-year fixed effects. Column 5 does not weight by population. Column 6 omits any jurisdictions below the median population in the sample. Column 7 omits any jurisdictions below the 75th percentile of population in the sample.

Table A-3: Regression Discontinuity Estimates: Impact of “False” Daylight Saving Time on Crime Rates using Daily Totals

	Robbery	Rape	Agg. Assault	Murder
Crimes per 1,000,000: 6 Weeks Prior to DST				
DST	0.123 (0.113)	0.044 (0.069)	-0.065 (0.191)	0.000 (0.026)
LPM: 6 Weeks Prior to DST				
DST	0.010 (0.008)	0.005 (0.006)	0.003 (0.007)	-0.001 (0.002)
Crimes per 1,000,000: 6 Weeks After DST				
DST	0.094 (0.114)	-0.032 (0.068)	-0.083 (0.212)	-0.051** (0.022)
LPM: 6 Weeks After DST				
DST	-0.002 (0.007)	0.006 (0.007)	-0.004 (0.007)	0.006 (0.008)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Daily total is calculated by summing hourly data across all hours within the day. Standard errors, clustered at the jurisdiction level, are reported in parentheses. Crimes per million uses regressions with crimes per million population as the main variable of interest. Regressions are weighted by population and coefficients should be interpreted as the change in the number of crimes per million population that occurs with the transition across the Sunday 6 weeks prior to DST (Panel A) and the Sunday 6 weeks after DST (Panel B). Linear probability model (LPM) uses regressions with a 0/1 binary indicator as the main variable of interest. Coefficients should be interpreted as the change in the probability of at least one incident of the relevant crime occurring with the transition across the Sunday 6 weeks prior to DST (Panel A) and the Sunday 6 weeks after DST (Panel B). All regressions include day of week fixed effects, jurisdiction-by-year fixed effects, controls for weather (county average daily temperature and rainfall), and a running variable control of days since the beginning of DST, where the slope of the running variable is allowed to vary before and after DST. Regressions use 558 jurisdictions, with a total 94,744 day-by-jurisdiction observations for the three weeks prior to and the three weeks following the beginning of DST. Population and crime data come from the National Incident-Based Reporting System (NIBRS).

Table A-4: Regression Discontinuity Estimates of the Impact of Daylight Saving Time on Robbery Rate by Two-Hour Time Blocks Since Sunset Hours

	Hours Before Sunset										Sunset			Hours Since Sunset	
	18/17	16/15	14/13	12/11	10/9	8/7	6/5	4/3	2/1	0/1	2/3	4/5			
DST	0.013	0.004	-0.012	0.011	-0.033	-0.031	0.009	-0.041	0.043	-0.120***	0.002	-0.066			
	0.035	0.030	0.021	0.027	0.024	0.031	0.026	0.033	0.030	0.041	0.046	0.046			

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the jurisdiction level, are reported in parentheses. Regressions use crimes per million population as the main variable of interest. Hours since sunset are calculated using data on the hour of sunset for each jurisdiction on the day prior to the beginning of Daylight Saving Time (DST). Regressions are grouped in two hour periods, indicated by column titles. Regressions are weighted by population and coefficients should be interpreted as the change in the number of crimes per million population that occurs within the relevant hours with the transition to Daylight Saving Time (DST). All regressions include day of week fixed effects, jurisdiction-by-year fixed effects, controls for weather (county average daily temperature and rainfall), and a running variable control of days since the beginning of DST, where the slope of the running variable is allowed to vary before and after DST. Regressions use 558 jurisdictions, with a total 94,744 day-by-hour-by-jurisdiction observations for the three weeks prior to and the three weeks following the beginning of DST. Population and crime data come from the National Incident-Based Reporting System (NIBRS).

Table A-5: Regression Discontinuity Estimate of Impact of Fall DST Transition on Daily Crime Rate and Probability of at Least One Crime Occurring

	Robbery	Rape	Agg. Assault	Murder
Panel A: Crimes per 1,000,000				
DST	-0.058 (0.142)	0.007 (0.066)	-0.035 (0.233)	-0.043 (0.028)
Panel B: Probability of Any Crime Occurring				
DST	-0.002 (0.004)	0.001 (0.002)	-0.006 (0.005)	-0.001 (0.001)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Daily total is calculated by summing hourly data across all hours within the day. Standard errors, clustered at the jurisdiction level, are reported in parentheses. Panel A uses regressions with crimes per million population as the main variable of interest. Regressions are weighted by population and coefficients should be interpreted as the change in the number of crimes per million population that occurs with the transition from Daylight Saving Time (DST). Panel B uses regressions with a 0/1 binary indicator as the main variable of interest, done using a linear probability model. Coefficients should be interpreted as the change in the probability of at least one incident of the relevant crime occurring with the transition from DST. All regressions include day of week fixed effects, jurisdiction-by-year fixed effects, controls for weather (county average daily temperature and rainfall), and a running variable control of days since the end of DST, where the slope of the running variable is allowed to vary before and after DST. Regressions use 558 jurisdictions, with a total 94,744 day-by-jurisdiction observations for the three weeks prior to and the three weeks following the end of DST. Population and crime data come from the National Incident-Based Reporting System (NIBRS).

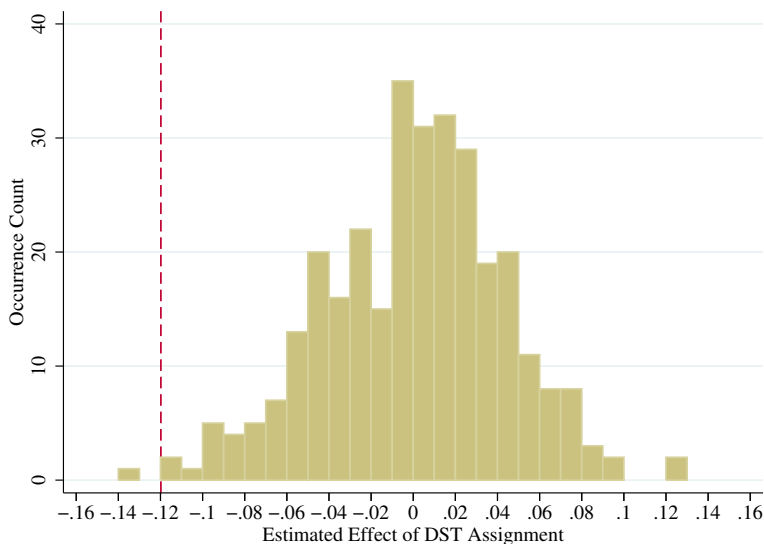
Table A-6: Regression Discontinuity Estimate of Impact of Daylight Saving Time on Daily Crime Rate and Probability of at Least One Crime Occurring

	Robbery	Rape	Agg. Assault	Murder
Panel A: Crimes per 1,000,000				
DST	-0.239*	-0.064	0.275	0.001
	(0.126)	(0.07)	(0.221)	(0.035)
DST X Weekend	0.083	-0.188**	0.255	-0.036
	(0.106)	(0.073)	(0.235)	(0.026)
Panel B: Probability of Any Crime Occurring				
DST	-0.016*	0.000	-0.002	0.007
	(0.008)	(0.006)	(0.007)	(0.01)
DST X Weekend	0.001	-0.007	0.006	-0.008**
	(0.009)	(0.009)	(0.008)	(0.004)

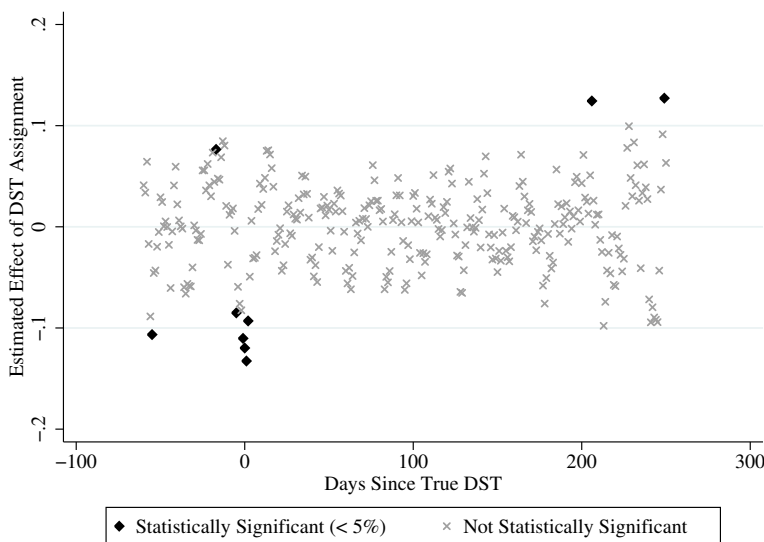
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Daily total is calculated by summing hourly data across all hours within the day. Standard errors, clustered at the jurisdiction level, are reported in parenthesis. Panel A uses regressions with crimes per million population as the main variable of interest. Regressions are weighted by population and coefficients should be interpreted as the change in the number of crimes per million population that occurs with the transition to Daylight Saving Time (DST). Panel B uses regressions with a 0/1 binary indicator as the main variable of interest, done using a linear probability model. Coefficients should be interpreted as the change in the probability of at least one incident of the relevant crime occurring with the transition to DST. All regressions include day of week fixed effects, jurisdiction-by-year fixed effects, controls for weather (county average daily temperature and rainfall), and a running variable control of days since the beginning of DST, where the slope of the running variable is allowed to vary before and after DST. Regressions also include an interaction between an indicator for weekend (Saturday or Sunday) and DST. Regressions use 558 jurisdictions, with a total 94,744 day-by-jurisdiction observations for the three weeks prior to and the three weeks following the beginning of DST. Population and crime data come from the National Incident-Based Reporting System (NIBRS).

Figure A-1: Local Linear Regression RD Impact for Hours Since Sunset (0 and 1) Using “False” DST Assignment

Panel A: Histogram of Estimated DST Effect by Day



Panel B: Scatter Plot of Estimated DST Effect by Days Since True DST



Notes: For both graphs, estimates are from assigning the beginning of DST to different days in the year, as expressed in days since the “true” beginning of spring DST, for hours since sunset of 0 and 1. Day 0 indicates correct timing of DST by year. Panel A shows the histogram of all estimated effects of DST, from 60 days before to 250 days after spring DST. Dashed line indicates our main estimated effect. Panel B shows the scatter plot of all estimated effects of DST by day. We mark any effect that is statistically significant at a minimum of 5% with a diamond, and effects that are not statistically significant with an x.