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

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A 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 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.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.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,

$$\begin{aligned} crime = \alpha + \beta_1 day + \beta_2 DST + \beta_3 DST * day + \beta_4 weekendXDST + \\ \omega W + \lambda_{jurisdictionXyear} + \gamma_{dow} \end{aligned} \tag{1}$$

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.

B 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	0.043	-0.038	-0.086	0.028
	(0.022)	(0.022)*	(0.021)*	(0.029)***	(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	No Population	Only Larger Population	
	Basic	2 Weeks (Linear)	8 Weeks (Cubic)	Year Fixed Effects	Weights	> Median	> 75th Perc.
Panel A: Daily Results							
DST	-0.215*	-0.071	-0.298**	-0.203	-0.175	-0.199	-0.315*
	(0.122)	(0.145)	(0.135)	(0.313)	(0.173)	(0.137)	(0.164)
Panel B: Sunset Hours							
DST	-0.120***	-0.090*	-0.115**	-0.188	-0.151**	-0.116**	-0.171**
	(0.042)	(0.052)	(0.048)	(0.127)	(0.06)	(0.05)	(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 **??**. 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 of the 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	0.044	-0.065	0.000
	(0.113)	(0.069)	(0.191)	(0.026)
LPM: 6 Weeks Prior to DST				
DST	0.010	0.005	0.003	-0.001
	(0.008)	(0.006)	(0.007)	(0.002)
Crimes per 1,000,000: 6 Weeks After DST				
DST	0.094	-0.032	-0.083	-0.051**
	(0.114)	(0.068)	(0.212)	(0.022)
LPM: 6 Weeks After DST				
DST	-0.002	0.006	-0.004	0.006
	(0.007)	(0.007)	(0.007)	(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 Daylight Saving Time 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	0.007	-0.035	-0.043
	(0.142)	(0.066)	(0.233)	(0.028)
Panel B: Probability of Any Crime Occurring				
DST	-0.002	0.001	-0.006	-0.001
	(0.004)	(0.002)	(0.005)	(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).

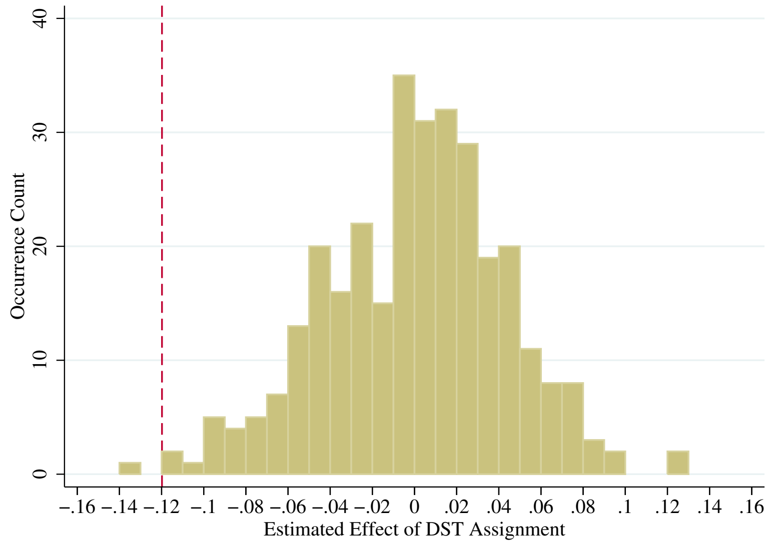
Table A-6: Regression Discontinuity Estimate of Impact of Daylight Saving Time on Daily Crime Rate and Probability of at Least One Crime Occurring

	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)

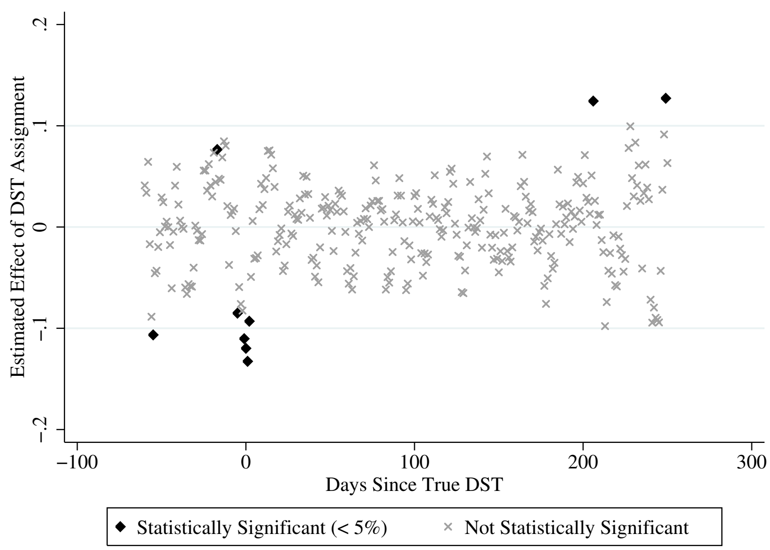
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Daily total is calculated by summing hourly data across all hours within the day. Standard errors, clustered at the jurisdiction level, are reported in parenthesis. Panel A uses regressions with crimes per million population as the main variable of 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 Impact for Hours Since Sunset (0 and 1) Using "False" Daylight Saving Time 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.

References

Jacob, B., L. Lefgren, and E. Moretti (2007): "The Dynamics of Criminal Behavior Evidence from Weather Shocks," *Journal of Human Resources*, 42(3), 489–527.