Juvenile Crime and the Four-Day School Week*

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Abstract

We leverage the adoption of a four-day school week across schools within the jurisdiction of rural law enforcement agencies in Colorado to examine the causal link between school attendance and youth crime. Those affected by the policy attend school for the same number of hours each week as students on a typical five-day week; however, treated students do not attend school on Friday. This policy allows us to learn about two aspects of the school-crime relationship that have previously been unstudied: one, the effects of a frequent and permanent schedule change on short-term crime, and two, the impact that school attendance has on youth crime in rural areas. Our difference-in-difference estimates show that following policy adoption, agencies containing students on a four-day week experience about a 20% increase in juvenile criminal offenses, where the strongest effect is observed for property crime.

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Highlights

1. This paper documents the short-term causal effect of school attendance on youth crime.

2. To estimate this relationship, we exploit the adoption of the four-day school week policy – a long-lasting schedule change – across schools within the jurisdiction of rural law enforcement agencies in Colorado over the period 1997-2014.

3. As a result of policy adoption, on average juvenile crimes increase. The findings are particularly strong for property crime.

4. Juvenile crime appears to increase in non-school locations and decline at school on Friday, the day treated students are off from school, suggesting incapacitation as a mechanism.

1 Introduction

The Federal Office of Juvenile Justice and Delinquency Prevention reports that the majority of juvenile crimes are committed during non-school hours, peaking between 3 p.m. and 6 p.m. (Snyder and Sickmund, 2006). As such, a common belief among parents, policymakers, and school officials is that lengthening the time students are in school or expanding youth programs will keep youth out of trouble. While the intuition behind this belief is reasonable, little is known regarding the extent to which school or youth program participation changes youth criminal behavior in the short-term, if at all, and even less is known about the school-crime relationship in rural areas (Weisheit et al., 1994).

Establishing the causal link between school attendance and youth crime is challenging because often the unobservable characteristics of an individual that determine school attendance – e.g., patience, risk aversion, or motivation – also influence criminal behavior. One way to isolate the contemporaneous relationship between school attendance and crime is to exploit variation in school schedules. In this paper we leverage the adoption of the four-day school week policy across schools within the jurisdiction of rural law enforcement agencies in Colorado over the period 1997-2014. Those affected by the policy spend the same number of hours in school each year as students on a standard five-day week; however, treated students typically do not attend school on Friday. Since many school districts throughout the US have started to experiment with alternative schedules (e.g., year-round school or four-day weeks), understanding the impact of theses changes on criminal behavior has important policy implications.

This study builds upon an area of empirical research examining the relationship between education and criminal behavior. A majority of this work focuses on the longer-run effects of educational attainment on crime.[1] Among the studies that estimate a causal relationship, the general finding is that more education reduces subsequent criminal behavior (Lochner, 2004; Lochner and Moretti, 2004; Berthelon and Kruger, 2011). These results, however, provide little insight into the contemporaneous effect of school attendance on youth crime because adult crime is temporally distinct

[1] See Lochner (2011) for a thorough discussion of the education and crime literature.

from school attendance.

To this point, our study contributes to a growing body of literature aimed at understanding the short-term effects of school attendance on youth crime. Studies of this nature typically rely on exogenous variation in day-to-day school attendance.[2] In the foundational paper, Jacob and Lefgren (2003) use teacher in-service days to estimate a causal relationship between school attendance and crime in urban settings. They find that juvenile property crime declines by 14% on days when school is in session but violent crime for this same group increases by 28% on school days.

In a follow-on study, Luallen (2006) exploits school attendance variation caused by teacher strikes that resulted in canceled school days. He finds that juvenile property crimes increase on days with strikes but violent crimes decline, and that the results are solely driven by urban areas. Akee et al. (2014) estimate the school-crime relationship based on public school teacher furlough days in Hawaii and find that time off from school is associated with significantly fewer juvenile crimes.

In contrast to the three existing studies listed above, the day-to-day variation in school schedule used in this paper – the adoption of the four-day school week – allows one to learn about the short-term effects of a more permanent and intentional schedule change on youth crime. This schedule change is distinct because (1) families are made aware of the change in advance and, in principle, have more time to plan compared to changes brought on by strikes or furloughs which occur more spontaneously, and (2) this change occurs each week throughout the school year rather than affecting only a handful of weeks. Furthermore, because four-day school weeks are primarily adopted in rural areas, we are able to shed light on the short-term effects of school attendance on crime for areas that are relatively understudied.

There are several ways in which a four-day school week could affect juvenile crime patterns. On the one hand, crime may decrease as a result of the policy. Because students on a four-day week

[2] An exception is Anderson (2014) which uses the minimum drop-out age and finds a negative relationship between education and youth arrests. Additionally, a related body of literature relies on experimental interventions in after-school programs to determine the impact of school attendance on youth criminal activity. Insight from these studies is limited due to selective participation; programs of this nature are typically not mandatory and those most at risk may avoid them (Cross et al., 2009; Rodríguez-Planas, 2012).

attend school longer on the days school is in session and because it is well documented that juvenile crime peaks between 3 p.m. and 6 p.m., it is possible that this schedule reduces juvenile crime. Assuming that parents work a standard 9 a.m. to 5 p.m. schedule, with the extended school day the amount of unsupervised time during peak crime committing hours is reduced Monday through Thursday. On the other hand, switching students to a four-day week schedule may increase juvenile crime. Students now have a full weekday off per week and are likely unsupervised, particularly if their parents work. They may also treat Thursday night as the new Friday night, essentially gaining another weekend night.

Using data on reported crimes and aggregating to the law enforcement agency-year level, we show that on average crimes in treated agencies increase as a result of the policy.[3] In particular, property crimes increase substantially when four-day school weeks are adopted. Alternatively, we find no evidence that juvenile violent crime is affected. Our results are in line with a recent report from the US Department of Justice that shows larceny – one component of property crime – is the most common juvenile crime, especially in rural areas (Puzzanchera, 2013).[4]

The paper proceeds as follows. In Section 2 we provide details on the policy setting. Section 3 and Section 4 describe the data and empirical framework, respectively. The results are presented in Section 5. This section includes results from the agency-year analysis, several sensitivity tests, and the day-of-week analysis. Section 6 concludes.

[3]We define an agency as "treated" if it contains at least one high school on a four-day week in a given year. For more detail on the coding of the treatment see Subsection 3.2.

[4]Property crime is defined as the unlawfully taking of property from the possession of another without the use of force, threat or fraud and comprises several types of theft including larceny, burglary, motor vehicle theft and occasionally arson. Larceny is the most common type of property crime committed by juveniles and includes shoplifting, pick-pocketing, bicycle theft, theft from a vehicle including vehicle parts, or theft from a building or structure where no break-in was involved.

2 The Four-Day School Week Policy

As of 2016, 21 states have a portion of their public schools on a four-day week schedule.[5] The primary motivation for states to implement this policy is to reduce transportation costs, which are particularly large for rural school districts. The four-day school week became popular during the energy crisis in the 1970s, at which time many states began changing laws regarding the number of days spent in school. Over the following decades there was a slow shift towards the four-day week schedule in remote, rural districts.

During this period the Colorado legislature changed their law from a mandatory number of school days to a mandatory number of hours, enabling districts in the state to adopt a four-day week. To compensate for one fewer day of instruction, those on the four-day week schedule attend school for 7.5 hours per day for 144 days instead of the standard 6 hours per day for 180 days. Between 1980 and the present, Colorado school districts have continually switched to this schedule. In 1980, three districts adopted the policy but by 1992 thirty-seven districts had a least some schools on a four-day week. As of 2017, of the 178 school districts in Colorado, 98 operate on a four-day week schedule, though in some of these districts only a subset of the schools follow this schedule.[6]

Given that cost considerations are central to the decision to switch, research on four-day school weeks has primarily focused on financial savings. Grau and Shaughnessy (1987), using data from ten school districts in New Mexico, document that districts operating on a four-day week experience a 10%-25% savings on fuel, electricity and transportation costs. Griffith (2011) examines six school districts that are either on the four-day week or in transition to that schedule and finds

[5]States that have adopted this schedule, with the first adopters dating back to the 1930s, include: Arizona, California, Colorado, Georgia, Idaho, Kansas, Kentucky, Louisiana, Michigan, Minnesota, Missouri, Montana, Nebraska, New Mexico, Nevada, Oklahoma, Oregon, South Dakota, Utah, Wisconsin and Wyoming. However, many of these programs are limited with only a subset of the state's schools following this schedule. Arkansas, Delaware, Pennsylvania, New Hampshire,Virginia and Washington also have approved a four-day week policy but have not yet implemented it. See http://www.ncsl.org/research/education/school-calendar-four-day-school-week-overview.aspx for background on specific state legislation regarding four-day schools.

[6]For more information on the four-day week policy in Colorado see Penn (2017).

that the policy yields a maximum of about 5.5% savings.[7] Despite their growing prevalence, little work has been done to understand the impact of this policy on students. To the best of our knowledge, the only published study is Anderson and Walker (2014). Their analysis focuses on the state of Colorado and they find a modest, but statistically significant, positive relationship between the policy and elementary school students' math and reading test scores. Their findings suggest that switching to a four-day week does not compromise student achievement, and may even improve it.[8]

3 Data

We combine several data sources to obtain the final analysis sample which is comprised of 63 Colorado law enforcement agencies, both sheriff and police, from 1997-2014. The outcome, crime counts, comes from the National Incident Based Reporting System (NIBRS). A complete list of law enforcement agencies is from the US Department of Justice (DOJ), the universe of schools and school-level characteristics are from the Common Core of Data (CCD) – a database maintained by the National Center for Education Statistics, and information on school schedules and timing of the four-day week adoption come from the Colorado Department of Education.[9] Additionally, we contacted all schools classified as operating on a four-day week schedule by the Colorado Department of Education to verify the year of policy adoption and to determine if any schools had switched back to a five-day week.[10] We also obtain county-level unemployment rates, which we map to agencies, from the Local Area Unemployment Statistics database maintained by the Bureau

[7] Four-day school weeks have been of interest in popular media as well and journalists have gone to some effort to examine specific cases of the policy change. A TIME Magazine article (Kingsbury, 2008) reports that some rural school districts experienced large savings on transportation, utility, and insurance costs as a result of the policy and a Wall Street Journal article (Herring, 2010) sheds light on the savings that the policy has brought to a rural district in Georgia.

[8] Alternatively, in an unpublished manuscript, Thompson (2017) examines the effect of the four-day school week on student achievement in Oregon and finds that switching to this schedule leads to a decline in test scores.

[9] We thank Mark Anderson (Montana State University) and Jhon Penn (Colorado Department of Education) for helping us obtain school calendar data.

[10] Montezuma-Cortez School District, Hayden School District RE-1, Arriba-Flagler School District C-20, and Archuleta County High School switch to a four-day week and back within our sample period.

of Labor Statistics. Although schools began switching to a four-day week in 1980, Colorado did not start fully participating in the NIBRS program until 1997 and the most recent year of the CCD is 2014. Consequently, the analysis sample is constrained to the years 1997 to 2014.

Summary statistics for the analysis sample are presented in Table 1 which includes an unbalanced panel of 836 agency-years. Because four-day schedules are primarily undertaken in rural areas, we restrict the analysis to agencies in rural counties. Using the US Census Bureau's definition of rural, an agency is rural if it is in a county that is not part of a metropolitan statistical area (MSA).[11] For context, as reported in Panel A, 34% of all rural agencies have at least one school that operates on a four-day week which translates to about 44% of the rural high school population. A majority of the treated agencies contain schools with Friday off, 28% compared to the 6% with Monday off. Panel B shows that, on average, there are approximately 20 crimes per 1,000 individuals age 14-17. Property crime is the most prevalent with, on average, 8.5 offenses per 1,000 individuals age 14-17.

3.1 NIBRS Data

NIBRS provides detailed information on reported crimes – both arrests and citations that do not necessarily result in an arrest – at the incident level and include the date and location of the crime, offender demographics, and criminal reporting agency. The date allows us to match reported crimes with the treatment variable by academic year as well as examine changes in crime by day of the week. Demographic characteristics of the offender allow us to precisely identify juveniles, specifically high school age students, and the agency identifier allows for the aggregation of offenses to the agency level.

The crime outcomes studied include counts of any crime, property crime, drug violations, and violent crime among high school age students (ages 14-17).[12] We exclude 18 year-olds from our

[11] The MSAs omitted are Denver, Boulder, Greeley, Colorado Springs, Fort Collins, Pueblo, and Grand Junction. Non-metropolitan counties include micropolitan (micro) areas, which are non-metro labor market areas centered on urban clusters of 10,000-49,999 persons, and all counties that are not classified as metropolitan or micropolitan.

[12] According to a report put out by the National Institute of Justice, ages 15-19 are the peak years for juvenile offending (National Institute of Justice, 2014)

definition because, depending on their birthdate, they may no longer be in school. Property crime is a broad category and includes burglary, larceny and motor vehicle theft. Violent crimes include homicide, sexual assault, robbery, and assault.

While NIBRS data cover most of Colorado, there are some agencies that do not participate in the NIBRS program at all or in some years. In order to obtain unbiased estimates of the treatment, it is important that zero crimes reported reflect no crime in the agency-year rather than underreporting or non-participation in the program. To begin with, there are 118 rural sheriff and police agencies. We drop 46 of these agencies because there is at least one year, after the initial year of program participation, where there are no crimes observed. We interpret these as non-participating agency-years, rather than zero crime years, and as such drop the entire agency in an attempt to create the cleanest possible dataset. Nine other agencies are dropped because there are less than ten crimes, on average, reported per year. We again view these agencies as underreporting.[13] Correctly coding zero crimes becomes even more important in the day-of-week analysis, especially when the data are disaggregated by location and day of week. We discuss this in more detail in Subsection 5.3.

Some incidents reported in the NIBRS dataset involve more than one offense (e.g., breaking and entering while in possession of illegal drugs). In these cases we count the incident in both categories. While this results in some double counting of incidents, the alternatives are less palatable. Dropping all of these incidents results in a substantial loss of data. Some systems use a hierarchy such that an incident is categorized as its most "severe" offense type; however, these distinctions are often arbitrary and can result in under-counting of some kinds of crimes, especially drug offenses.

3.2 Construction of Analysis Sample

To construct a list of agencies matched to schools, we begin with a complete list of all agencies. The universe of crime reporting agencies from the DOJ include the agency type (sheriff or police) and the agency address which we convert to latitude and longitude using Texas A&M's geocoding

[13] We obtain similar results when all 118 agencies are included in the analysis, but the point estimates are noisier.

service.[14] Next we construct a comprehensive list of Colorado high schools by appending the CCD school files from 1997 to 2014.[15] This dataset contains the latitude and longitude of each school. Schools are then matched to corresponding police agencies. We define a school to be in a police agency boundary if it is within a 10 mile radius of the police agency.[16] Note, although uncommon, it is possible for schools to match to more than one police agency and agencies can contain multiple schools. Next we map all schools to sheriff agencies. Since sheriff agencies often have jurisdiction over a county, we define a school as part of a particular sheriff agency if it is located in the same county.[17] Multiple schools may map to a single sheriff agency.

The agency-school file is merged, by school, with the CCD file for each year (1997-2014) creating an agency by year panel that includes time-varying school level characteristics (percent white, the percent on free or reduced lunch and the student/teacher ratio). Next we merge in the list of schools that have adopted a four-day schedule each year from 1997 to 2014. Finally, we merge in the 18 years of NIBRS data by agency-year and collapse the entire dataset to the agency-year level of observation.

4 Empirical Framework

In the first part of the analysis, we estimate the following difference-in-difference model

$$Y_{at} = \beta_0 + \beta_1 T_{at} + \alpha X_{at} + \gamma_a + \delta_t + \varepsilon_{at} \tag{1}$$

where Y_{at} is a count of crimes in agency a and in year t. X_{at} is a vector of time-varying agency-level characteristics (unemployment rate, percent of students eligible for free lunch, percent white, and student/teacher ratio). γ_a is an agency-level fixed effect which controls for any time-invariant characteristics of the agency, δ_t is a year fixed effect to account for temporal changes in crime over the 18 year time horizon, and ε_{at} is the usual error term. The treatment variable T_{at} is equal to one

[14] The geocoding service is located at the following site: http://geoservices.tamu.edu/Services/Geocode/.

[15] Appending all of the years rather than using the most recent year captures school openings and closures.

[16] Results are robust to using a 5 mile radius around the agency, see Table 3. All distances are calculated "as the crow flies".

[17] We obtain similar results when we limit the analysis to police agencies only, see Table 3.

if an agency has at least one high school operating on a four-day week in a given year and zero otherwise. We exclude summer months because no agencies are treated during this time.

Given that crime is reported as a count, estimating the above model with a standard Ordinary Least Squares approach is not appropriate as there is a large share of agency-year observations with a zero-count on juvenile crimes. That is, crime is positively skewed.[18] We instead estimate the model using Fixed-Effects Poisson Quasi-Maximum Likelihood (QMLE); a model often used to accommodate count data with an excess number of zeros (Hausman et al., 1984; Osgood, 2000).[19] To account for the fact that each agency has a different student population, and therefore a different potential for crime, we include total student population as the exposure variable. Finally, we report cluster-robust standard errors to account for overdispersion and within agency correlation of the dependent variable (Wooldridge, 1999).[20]

The identifying assumption for estimating the effect of a four-day week is that the adoption of the school schedule is uncorrelated with other unobserved time-varying determinants of juvenile crime. The inclusion of agency and year fixed effects controls for time-invariant agency-level variables and overall time trends that might affect crime. Further, we also include several agency-level time-varying controls to reduce concerns that unobserved characteristics that also explain juvenile crime are correlated with the adoption of a four-day week.

Although the identifying assumption is not directly testable there are several indirect tests to support its plausibility. We begin with an event study. Suppose schools enact policies such as a calendar change, aimed at reducing juvenile delinquency, in response to a high crime rate in the area. A pre-existing trend of this nature would undermine the causal interpretation of the treatment, as it will be impossible to distinguish the effect of the four-day school week from the pre-treatment trend. An event study presented in Figure 1 reveals no evidence of differential trends in high school

[18] Aggregating to the agency-year-day of week level of crime, as we do in the second part of the analysis, only further exacerbates the issue of the high incidence of zero-count observations.

[19] In a robustness check, we also estimate a linear probability model where the outcome takes on a value of one if any crime is observed in the given agency-year, and zero otherwise. Results for this analysis are reported in the appendix.

[20] The QML estimator only restricts the within-agency mean and variance to be equal, so a majority of the overdispersion present in the juvenile crime data is accounted for without robust standard errors.

crime prior to the implementation of the policy. In the periods following adoption, as expected, crime increases in the treated agencies.

Anecdotally, there is also little reason to believe that the adoption of the policy is a response to crime patterns or correlated with unobservable time-varying agency characteristics that also explain juvenile crime. In Colorado, the schools that have adopted a four-day week schedule most often cite financial savings as the reason (Grau and Shaughnessy, 1987; Donis-Keller and Silvernail, 2009; Anderson and Walker, 2014). The Colorado Department of Education states that the schedule is almost entirely adopted by schools in rural districts that serve a dispersed group of students because they can save on transportation (Penn, 2017). Administrators of these rural schools also cite that Friday is commonly the day students have off in an attempt to reduced school-related absences. Because rural schools in Colorado are geographically isolated, students often travel substantial distances for sports competitions and other school-related extracurricular activities. As such, adopters aim to schedule the majority of sports competitions on Fridays.[21] Other reasons schools have decided to switch to a four-day week include parent support and increased academic performance.

Another potential violation of the identifying assumption is if other types of policies that also affect juvenile crime are implemented simultaneously with the four-day week schedule. If this is the case, it would be difficult to distinguish the effect of the four-day week from these other policies. However, after talking with school officials and representatives from the Colorado Department of Education, we have found little reason to believe this to be true. It is the case that some schools that operate Monday-Thursday have implemented Friday School which is typically a two-hour drop in study hall one to four Friday mornings per month. This program was established for students who have attendance issues to make up missed school time. Because students violating attendance laws are, on average, also a group that is more likely to misbehave, the implementation of Friday School should only attenuate any positive effect of the policy. Finally, in Subsection 5.2 we show that the main results hold up to a number of robustness checks and pass several placebo

[21] We obtained the specifics on reasons for the four-day adoption in rural Colorado by calling the relevant school administrators.

tests providing further support for the identifying assumption.

5 Results

5.1 Agency-Year Analysis

Table 2 reports estimates obtained from Equation 1 where each estimate comes from a separate regression. Columns 1-4 correspond to different crime outcomes (any crime, property, drug and violent) and Columns 5 and 6 report results where the outcome is crime counts by location (not-school and at-school). Panel A reports results for a parsimonious specification that includes only agency and year fixed effects while Panel B includes all fixed effects plus time-varying agency-level covariates. The point estimates from the two specifications are very similar, further adding to the plausibility of the identifying assumption.

The results in Columns 1 and 2 – for both Panel A and Panel B – show a significant increase in juvenile crime. There is a 19.6% increase in overall crime among high school age individuals in agencies that are treated. The effect is strongest for property crime; property crime increases by 26.6% in agencies containing a high school on a four-day week.

Column 3 reveals a noisy but positive relationship between the four-day week and drug violations. Given that violent crimes are a rare occurrence among juveniles, it is unsurprising we find essentially no effect on violent crimes as the point estimates are small in magnitude and indistinguishable from zero (Column 4).

Next, we ask the question: Are the observed changes in crime occurring at school or outside of school? To investigate this, we utilize a feature of the data that indicates the location of each crime and categorize crimes as either at-school or any other location (not-school). If the increase in crime is driven by the common perception that unsupervised students are more likely to misbehave (i.e., incapacitation), then we should expect to see crime increase outside of school among treated students. Columns 5 and 6 report estimates for any crime by location. In line with the proposed hypothesis, we show crimes occurring in non-school locations in treated agencies (Column 5)

increase by 18.2%. The point estimates for crimes occurring at school (Column 6) are large and positive but indistinguishable from zero. It is also the case that we cannot reject that the not-school and at-school coefficients are the same. As such, to further investigate the incapacitation hypothesis, in Subsection 5.3 we examine crime by location and day of week. Given that students are not in school on Friday, we expect a large decrease in crimes coded as at-school on this day.

5.2 Robustness Checks

We probe the robustness of the results by examining several alternative specifications and samples. To begin, one potential threat to the causal interpretation of the treatment effect is if the treatment coincides with an unobserved differential change in crime patterns between treatment and control agencies; e.g., changes in the local economy or law enforcement practices. To rule out the possibility of such a confound, as a placebo test we estimate Equation 1 using adult crime (age 19+) as the outcome. If juvenile crime is only increasing in areas because of the four-day week policy, then there should be no detectible relationship between the treatment and adult crime. Table 3 Panel A reports these results and confirms there is no relationship; evidence supporting the notion that the four-day school week policy is uniquely impacting juveniles and is not a proxy for other unobserved changes in the agency.

In Panel B we report results for police agencies only. Police agencies tend to serve more geographically concentrated areas while sheriff agencies primarily serve counties. Because there could be multiple high schools within a county where some are treated and some not, focusing on the subsample of police agencies allows us to more precisely code agencies as treated. We obtain similar results using this subsample. The only major difference is that drug violations now attain statistical significance at the five percent level, where in the full sample the estimates were positive but noisy.

Panel C reports results for an alternative mapping of schools to agencies. Here, using the exact location of a school and the agency office, we assign a school to an agency if they are within a five mile radius of each other, versus ten miles as in the main analysis. Again we obtain nearly

identical point estimates as those in our preferred specification (Panel B, Table 2).

As an additional sensitivity test and to further ensure that the estimated effect is not driven by some other unobserved factor, we conduct an agency-level placebo analysis. In this procedure agencies are randomly reassigned the treatment profile of a different rural agency. This reassignment maintains the characteristics of the treatment profile, the treatment turns on and can remain on or turn off, but the timing of treatment is randomly reassigned. If our main results are really capturing the effect of the policy on crime, the results for this specification should reveal no relationship between the treatment and crime outcome since the reassigned treatment does not reflect the true treatment in that agency-year. Figure 2a presents the distribution of the estimated treatment effects from estimating Equation 1 1,000 times with a balanced panel for the outcome any crime, each time with random reassignment of the treatment profile. The actual estimate is indicated with a red line, $\hat{\beta}_1 = 0.20$. Figure 2b reports the distribution of the corresponding t-statistics, where similarly the t-statistic corresponding to the actual estimate is represented by the red line (t-stat = 2.29). Of the 1,000 t-stats, 58 are greater than 2.29 in absolute value; this again suggests that the estimated coefficient is likely capturing the effect of the policy net of other confounding factors.

5.3 Agency-Year-Day of Week Analysis

Aggregating the data to the agency, year, day of week level allows one to examine the effect of the four-day school week policy on specific days. Table 4 reports the corresponding summary statistics for this level of aggregation. Columns 1 and 2 report summary statistics for the outcome any crime where means for agencies that are untreated are reported in Column 1 and those that are treated with Friday off in Column 2. Similarly, Columns 3 and 4 report summary statistics for crimes taking place in non-school locations and, in Columns 5 and 6, for crimes taking place at school.

One reason to examine crime patterns at this finer level is to learn more about the underlying mechanism. Does crime increase on a specific day? One would expect crime to rise on Friday in treated agencies that have Friday off if an incapacitation effect exists. Incapacitation is a theory put forth by the literature that predicts when youth are kept engaged or supervised, as they are in

school, they are less likely to misbehave. It is also possible that crime will increase on Thursday if Thursday becomes the new Friday, or throughout the weekend (i.e., Saturday and Sunday) if there are spillover effects present.

To investigate this, we estimate the following model using Fixed-Effects Poisson QML separately for each day of the week:

$$Y_{atd} = \beta_0 + \beta_1 TF_{at} + \alpha X_{at} + \gamma_a + \delta_t + \psi_d + \varepsilon_{atd} \tag{2}$$

TF_{at} is a dummy variable equal to one if there is at least one school in the agency-year treated with Friday off. All other controls included are defined in the previous section.[22] In an attempt to get the cleanest day of week estimates and because the majority of treated agencies are on a Monday-Thursday schedule with Friday off (see Table 1), we drop the small share of agencies that are treated with Monday off.

Table 5 reports the point estimates from Equation 2 where each estimate comes from a separate regression and corresponds to the treatment effect for that particular day of the week. In some agency-years on a given day of the week there are zero crimes reported for the entire sample period, we drop these agencies to construct a balanced panel of 4,704 observations; 672 per day of the week. This allows for easy comparison of the treatment effect across days of the week holding constant the sample. Column 1 reports estimates for any crime taking place in any location and reveals that crime increases on Thursday and Saturday in treated agencies. The point estimate for Thursday is the largest; crime increases by about 34% relative to untreated agencies. This result is in line with an incapacitation theory as it is quite plausible that treated students, those with no school Friday, view Thursday (or Thursday night) as the start to the weekend. However, the standard prediction of incapacitation is that crime increases on the day off (Friday). While the point estimate for Friday is imprecisely measured, it is positive and quite large – second largest

[22] An alternative day of the week specification would be to include a fully interacted model where the treatment is interacted with each day of the week in a single regression. Using this approach, we obtain similar results to those produced by Equation 2. The main difference is that our preferred day of the week specification includes a richer set of controls by implicitly controlling for the interaction between day of the week and each right hand side variable. Perhaps most importantly, it implicitly includes a day of week by agency fixed effect.

after Thursday – suggestive that the main findings are consistent with incapacitation.

To further probe the mechanism, if the main results are operating through incapacitation, we should also expect to see at-school crime in the treated agencies decreasing on Friday and crime in non-school locations increasing. In Columns 2 and 3 we report estimates of the treatment effect by day of the week and location, not-school and at-school, respectively. Consistent with incapacitation, there is a dramatic decrease in any crime taking place at school on Friday for those agencies with Friday off, a 61% decline. The effect on any crime occurring in non-school locations on Friday is large and positive but imprecisely measured.[23]

In summary, Table 5 provides suggestive evidence that students who have Friday off are more likely to commit crime around the day of the week that they have off. We caution placing too much emphasis on the day of the week results, especially for those where the data is divided into 14 subgroups (Columns 2 and 3), as all of the estimates are somewhat imprecisely measured; a common issue when subsampling at such a fine level.[24] Further, although the estimated effects for Thursday and Saturday are significantly different from zero, we cannot reject that they are the same as the estimates from the other days of the week. An additional concern is that there may be reporting error in the day of the week that is coded. It could be difficult for law enforcement officials to determine if a crime is committed late at night or early the following day (11 p.m. Friday night versus 12:30 a.m. Saturday morning) if the crime is reported the following day. For example, suppose a group of youth steal some bicycles from a neighbor's yard late Friday night and it is discovered the following day. It becomes difficult to determine if the theft occurred Friday or Saturday.[25]

[23] We repeat the day of week and location analysis but instead estimate a linear probability model where the outcome is equal to one if there is observed crime in an agency-year-day of week, and zero otherwise. The corresponding results are reported in Table A2 and provide further support of an incapacitation mechanism. Crime appears to be increasing on Fridays and particularly in non-school locations.

[24] When subsampling by day of the week and at-school (Column 3), there are many agencies that have no observations for a particular day for the entire sample period which is why the number of observations vary across day of the week within this column. It is another reason to exercise caution when interpreting these results.

[25] Although NIBRS reports date and time of all offenses in theory allowing one to analyze the data at this finer level (agency-year-day of week-hour), point estimates obtained from such an analysis are too noisy to learn anything useful.

6 Conclusion

In summary, we find that the implementation of the four-day school week in rural areas leads to an increase in youth crime, particularly property crime. Overall crime increases by about 20% among high school age individuals in agencies that are treated. Our findings support the common belief that when youth are supervised, as they are in school, they are less likely to commit crime. The results from this study highlight the fact that policymakers should be aware of the unintended consequences associated with school schedules that result in more unsupervised time.

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Table 1: Summary Statistics

	Mean	Std. Dev.
Panel A: Treatment		
Any Four-Day in Agency	0.34	(0.47)
Any Four-Day in Agency (Friday)	0.28	(0.45)
Any Four-Day in Agency (Monday)	0.06	(0.24)
Panel B: Juvenile Crime (age 14-17)		
All Crime (per 1,000 students)	20.19	(23.91)
Property (per 1,000 students)	8.53	(12.45)
Drugs (per 1,000 students))	4.22	(5.95)
Violent (per 1,000 students)	4.52	(5.61)
Panel C: Controls		
Total Students	1,072.95	544.98
Unemployment (county-level)	5.82	(2.37)
Student/Teacher Ratio	16.13	(3.47)
Fraction Free/Reduced Lunch	0.32	(0.15)
Fraction Students White	0.73	(0.17)

Notes: The data are aggregated to the agency-year observation level and include only those agencies in rural counties (those counties outside of a MSA). Summer months (June-August) are excluded. All control and crime means are weighted by student population. $N = 836$.

Table 2: Effect of Four-Day School Week on Crime and Location

	Crime Type				Location	
	All Crime	Property	Drugs	Violent	Not-School	At-School
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Agency, Year Fixed Effects						
Four-Day	0.255***	0.315***	0.237	0.201	0.229***	0.274*
	(0.084)	(0.089)	(0.146)	(0.167)	(0.088)	(0.160)
Panel B: Agency, Year Fixed Effects and Time-Varying Controls						
Four-Day	0.196**	0.266***	0.252	0.076	0.182**	0.175
	(0.086)	(0.090)	(0.167)	(0.124)	(0.092)	(0.164)
N	836	825	763	808	836	728

Notes: Each coefficient comes from a separate regression. Each regression in Panel A includes year and agency fixed effects and each regression in Panel B includes year and agency fixed effects and agency-level time-varying controls (fraction eligible for free or reduced lunch, student-teacher ratio, unemployment rate, and fraction white). Each regression also includes total enrollment as the exposure variable with coefficient constrained to be one. Observations vary across outcomes because, in some agencies for some crime types and locations, zero crime counts are observed in all sample years. Standard errors are clustered at the agency level and are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Effect of Four-Day School Week on Crime for Various Samples

	All Crime	Property	Drugs	Violent
	(1)	(2)	(3)	(4)
Panel A: Adult Crime (age$\geq$ 19) – Placebo Test				
Four-Day	0.023	0.050	-0.016	-0.019
	(0.103)	(0.121)	(0.131)	(0.097)
N	836	836	829	836
Panel B: Police Agencies Only				
Four-Day	0.141	0.186*	0.451**	0.081
	(0.088)	(0.098)	(0.189)	(0.174)
N	505	496	468	479
Panel C: Alternative Distance Measure- 5 mi. mapping (vs. 10 mi.)				
Four-Day	0.190**	0.267***	0.209	0.084
	(0.086)	(0.093)	(0.164)	(0.140)
N	834	823	761	806

Notes: Each coefficient comes from a separate regression and each regression includes year and agency fixed effects, and agency-level time-varying controls (fraction eligible for free or reduced lunch, student-teacher ratio, unemployment rate, and fraction white). Each regression also includes total enrollment as the exposure variable with coefficient constrained to be one. Observations vary across outcomes because, in some agencies for some crime types, zero crime counts are observed in all sample years. Standard errors are clustered at the agency level and are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Summary Statistics by Day of Week

	All Crime		Not-School		At-School	
	No Treat	Off Fri.	No Treat	Off Fri.	No Treat	Off Fri.
	(1)	(2)	(3)	(4)	(5)	(6)
All Days	3.32	1.97	2.74	1.69	0.84	0.39
	(4.21)	(2.91)	(3.58)	(2.55)	(1.60)	(0.97)
N	3,185	1,519	3,185	1,519	2,191	763
Monday	3.34	1.97	2.65	1.72	1.01	0.37
	(4.45)	(2.93)	(3.67)	(2.73)	(2.07)	(0.79)
Tuesday	3.51	2.14	2.73	1.80	1.14	0.50
	(4.34)	(3.23)	(3.56)	(2.70)	(1.84)	(1.02)
Wednesday	3.84	2.04	3.02	1.50	1.20	0.73
	(4.52)	(3.05)	(3.74)	(2.43)	(1.73)	(1.39)
Thursday	3.39	2.15	2.59	1.69	1.17	0.67
	(4.10)	(2.68)	(3.32)	(1.95)	(1.56)	(1.29)
Friday	4.07	2.06	3.28	1.89	1.12	0.22
	(5.03)	(2.54)	(4.28)	(2.29)	(1.70)	(0.66)
Saturday	2.84	1.80	2.74	1.71	0.13	0.14
	(3.63)	(2.52)	(3.53)	(2.40)	(0.50)	(0.62)
Sunday	2.23	1.59	2.15	1.55	0.11	0.07
	(2.82)	(3.33)	(2.70)	(3.21)	(0.47)	(0.35)
N	455	217	455	217	313	109

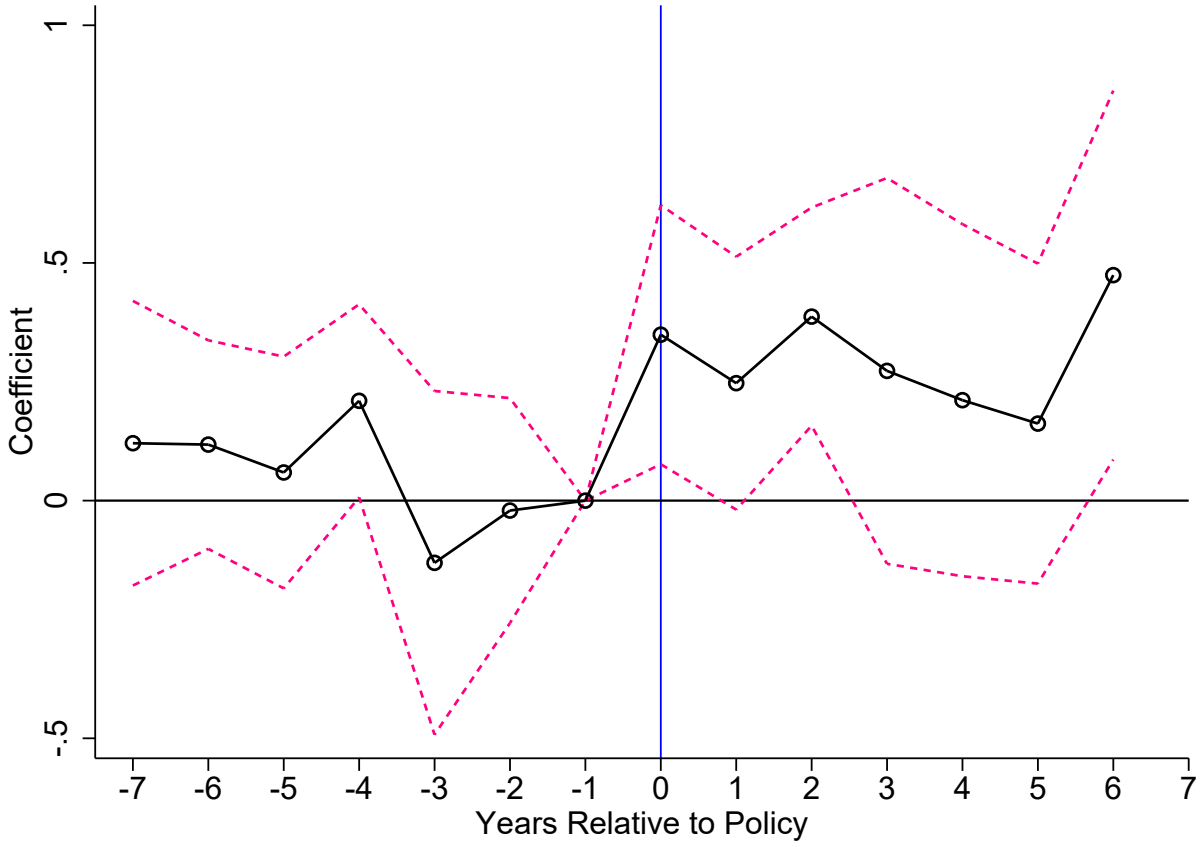
Notes: The data are aggregated to the agency-year-day of week observation level and include only those agencies in rural counties (those counties outside of a MSA). Reported means are interpreted as crimes per 1,000 students. All means are weighted by student population. Summer months (June-August) are excluded. Standard deviations are in parentheses.

Table 5: Effect of Four-Day School Week on Crime by Day of Week and Location

	All Crime (1)	Not-School (2)	At-School (3)
Four-Day Friday Off - Monday	0.244	0.216	-0.070
	(0.175)	(0.177)	(0.331)
N	672	672	442
Four-Day Friday Off - Tuesday	0.255	0.283**	0.135
	(0.156)	(0.126)	(0.643)
N	672	672	442
Four-Day Friday Off - Wednesday	0.180	0.050	0.375*
	(0.217)	(0.267)	(0.197)
N	672	672	442
Four-Day Friday Off - Thursday	0.337**	0.307**	0.260
	(0.146)	(0.156)	(0.235)
N	672	672	442
Four-Day Friday Off - Friday	0.270	0.338	-0.612**
	(0.219)	(0.230)	(0.299)
N	672	672	337
Four-Day Friday Off - Saturday	0.254**	0.206	0.705*
	(0.111)	(0.129)	(0.403)
N	672	672	267
Four-Day Friday Off - Sunday	-0.126	-0.108	-1.272
	(0.195)	(0.186)	(0.000)
N	672	672	254

Notes: Each coefficient comes from a separate regression and each regression includes year and agency fixed effects, and agency-level time-varying controls (fraction eligible for free or reduced lunch, student-teacher ratio, unemployment rate, and fraction white). Each regression also includes total enrollment as the exposure variable with coefficient constrained to be one. Observations vary across outcomes because, in some agencies, zero crime counts are reported in all sample years and days of week. Standard errors are clustered at the agency level and are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 1: Event Study



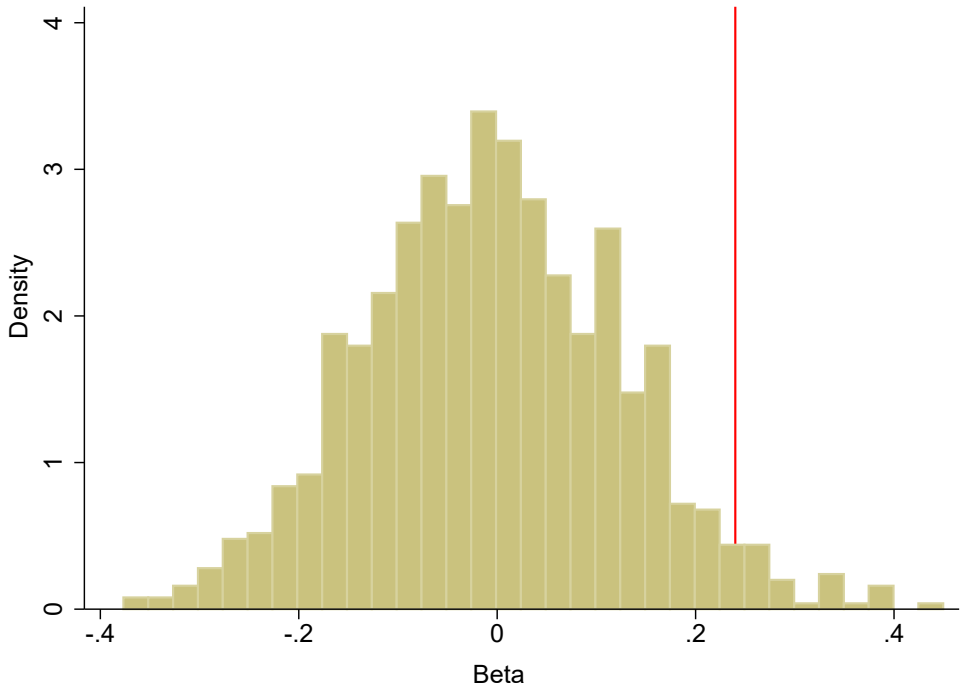
Notes: This figure plots θ_k, and 95% confidence intervals in dashed pink lines, from estimating the following equation.

$$Y_{at} = \beta_0 + \sum_{k=-16}^{-1} \theta_k S_{atk} + \sum_{k=1}^{15} \theta_k S_{atk} + \alpha X_{at} + \gamma_a + \delta_t + \varepsilon_{at}$$

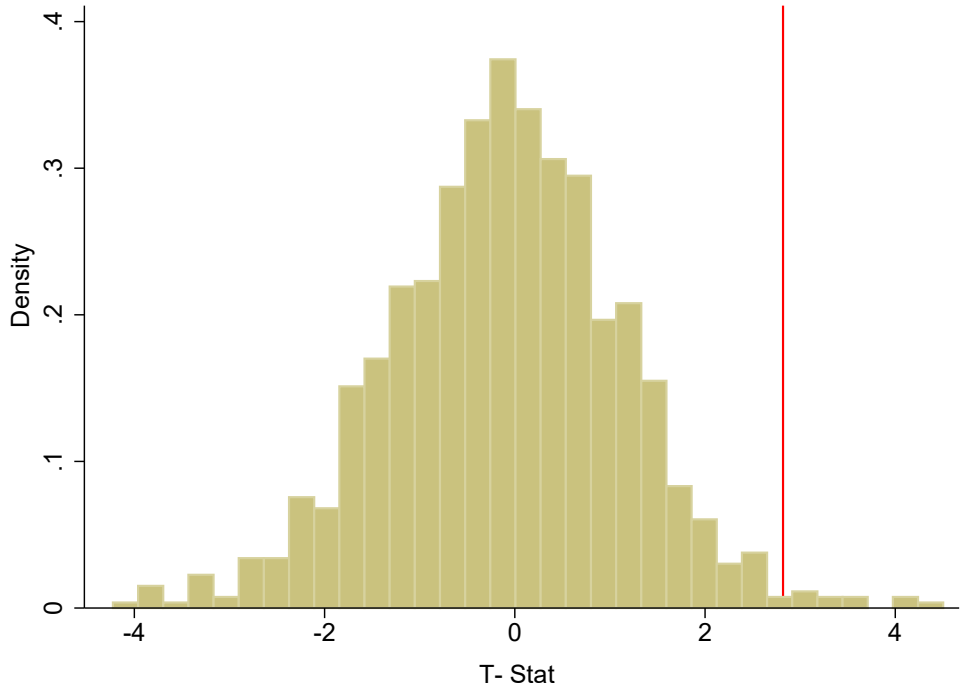
S_{atk} is an indicator for k years from the adoption of the four-day school week for agency a and the year t. All other controls included are defined in Equation 1. The omitted category is t-1, as that is the most recent pre-policy year. There are a total of 16 pre-policy years and 15 post years. The sample excludes agencies that become treated and then become untreated within the sample period.

Figure 2: Placebo Test

(a) Histogram of Betas



(b) Histogram of T-Stats



Notes: The sample includes a balanced panel of 35 rural agencies observed each year from 1997 to 2014. Agencies are randomly re-assigned a treatment profile of a different agency. This alternative treatment variable is then used in Eq. (1) in place of the correctly assigned treatment. The treatment effect obtained with the balanced panel and the correctly assigned treatment profile is 0.20 and the corresponding t-statistic is 2.29. Each are denoted with a red line. There are 1,000 iterations of this re-assignment placebo procedure.

Appendix

Table A1: Effect of Four-Day School Week on Crime and Location - LPM

	Crime Type				Location	
	All Crime	Property	Drugs	Violent	Not-School	At-School
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Agency, Year Fixed Effects						
Four-Day	0.040	0.140**	0.143	0.066	0.048	0.172**
	(0.037)	(0.057)	(0.091)	(0.050)	(0.038)	(0.065)
Panel B: Agency, Year Fixed Effects and Time-Varying Controls						
Four-Day	0.039	0.134**	0.165*	0.060	0.046	0.186***
	(0.039)	(0.056)	(0.090)	(0.055)	(0.040)	(0.064)
N	836	836	836	836	836	836
Mean of Dep. Var.	0.84	0.66	0.57	0.65	0.82	0.47

Notes: Each coefficient comes from a separate regression where the model estimated is a linear probability model. Each regression in Panel A includes year and agency fixed effects and each regression in Panel B includes year and agency fixed effects and agency-level time-varying controls (fraction eligible for free or reduced lunch, student-teacher ratio, unemployment rate, and fraction white). Each regression is weighted by total enrollment. Standard errors are clustered at the agency level and are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A2: Effect of Four-Day School Week on Crime by Day of Week and Location - LPM

	All Crime (1)	Not-School (2)	At-School (3)
Four-Day Friday Off - Monday	0.077	0.043	0.146***
	(0.085)	(0.093)	(0.044)
Mean of Dep. Var.	0.59	0.55	0.22
Four-Day Friday Off - Tuesday	0.116	0.093	0.173***
	(0.092)	(0.102)	(0.050)
Mean of Dep. Var.	0.59	0.54	0.26
Four-Day Friday Off - Wednesday	0.054	0.022	0.292***
	(0.075)	(0.066)	(0.079)
Mean of Dep. Var.	0.61	0.57	0.28
Four-Day Friday Off - Thursday	0.082	0.021	0.199***
	(0.070)	(0.069)	(0.058)
Mean of Dep. Var.	0.60	0.56	0.28
Four-Day Friday Off - Friday	0.231***	0.263***	0.076
	(0.074)	(0.064)	(0.085)
Mean of Dep. Var.	0.62	0.58	0.25
Four-Day Friday Off - Saturday	0.099	0.100	0.020
	(0.096)	(0.095)	(0.074)
Mean of Dep. Var.	0.58	0.57	0.06
Four-Day Friday Off - Sunday	0.157*	160**	-0.034
	(0.078)	(0.079)	(0.035)
Mean of Dep. Var.	0.54	0.54	0.04

Notes: Each coefficient comes from a separate regression where the model estimated is a linear probability model. Each regression includes year and agency fixed effects and agency-level time-varying controls (fraction eligible for free or reduced lunch, student-teacher ratio, unemployment rate, and fraction white). Each regression is weighted by total enrollment. There are 672 observations per regression (i.e., per day of week) for a total of 4,704. Standard errors are clustered at the agency level and are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.