

Essays on Education and Youth Crime

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Summary

Chapter 1: Juvenile crime is a pressing issue in developing countries. In India, 16-18 year-olds are responsible for more than 75 percent of total juvenile crime. In 2016, the Indian government introduced adult punishment for heinous crimes committed by this group of juveniles. Comparing below and above 18- year-old boys in a difference-in-differences framework, this paper finds that the juvenile crime reform led to a significant decrease in murder and rape for the targeted group, providing evidence that some of the most serious violent crime can be deterred through harsher punishment. Overall, these results offer support for a punitive approach to youth crime in contraposition to a welfare approach that favours rehabilitation over punishment.

Chapter 2: In 2010, the Government of India engaged in one of the biggest primary schooling reforms by making primary schooling free and compulsory. This was aimed at increasing enrolment rates, along with making provision to hire more qualified teachers and improve school quality. Comparing districts with high- and low- pre-reform schooling provision in a difference-in-differences framework, this paper finds that the schooling reform led to an increase in total enrolment, number of schools and teachers per population, along with an improvement in quality indicators, including pupil-teacher ratios. Further, the results are larger for boys and younger pupils.

Chapter 3: Violent youth crime has been rising in the UK in the last 10 years. While youth crime has high social and economic costs, figuring out effective ways to deter youth from committing crime has been of utmost importance to policy makers. In this paper, I study the effect of school exclusion on youth crime. Comparing pupils excluded in a given term, with pupils not excluded till then, but experiencing an exclusion a few weeks after, this paper finds no evidence that school exclusions have important effects on the likelihood of youth committing crime in the short run. This result suggests that disciplinary policies against pupils with disruptive behavior may not be effective at tackling youth crime, but they do not exacerbate the phenomenon either.

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1 Chapter 1: Harsher Punishment and Juvenile Crime: Evidence from India

1.1 Introduction

Juvenile crime is a pressing issue in developing countries. For instance, crime figures released by the National Crime Records Bureau (NCRB) of India reveal that more teenagers are getting involved in crimes every year. From 2002 to 2012, rape cases have more than doubled (from 9200 to about 21400), murders have increased by 87 percent (from 3300 to about 6200) and kidnapping has increased five-fold. As per the NCRB, close to 75 percent of the total juvenile crimes and about 80 percent of the juvenile heinous crimes, extremely violent offences including murder and rape, were committed by 16-18 year olds.¹ Youth crime may have important short and long-term consequences for both the perpetrator and its victims. Juvenile incarceration reduces the probability of high school graduation (Aizer and Doyle, 2015). Among perpetrators, juvenile crime is also associated with negative cognitive development, lower productivity, and higher unemployment (Golan et.al., 2021), and with higher adult incarceration rates including for violent crimes (Bayer et.al., 2009; Aizer and Doyle, 2015). Among victims, it also results in injuries, deaths, disabilities, and other long term health consequences including problems related to mental health, behavioural disorders, and other chronic diseases.

¹Own computation based on data available at <https://www.ncrb.gov.in/crime-in-india.html>.

Tackling juvenile crime is challenging, and countries around the world have adopted different models to deal with young offenders. The main distinction is between a welfare model, focusing on the needs of the child, diagnosis, and treatment (for example, Belgium, France, and New Zealand) and a justice model (for example, the UK and the US) emphasizing formal procedures including punishment. According to the Society of Adolescent Health and Medicine, the welfare model is motivated by the idea of prioritizing the youth well-being and sees incarceration as a last resort for offenders committing most serious violent crimes and whose presence puts the community safety at stake (Moore et.al., 2016).² The justice model relies on the rationale that punishment helps in deterring crime, by increasing the cost of being caught, as set out in Becker's seminal economic model of crime (Becker 1968).

The debate regarding policy interventions for juvenile offenders is theoretically guided by these two pieces of thought. On the one hand, rehabilitative interventions emphasize the role of support to individuals to improve their educational and economic outcomes after contact with the criminal justice system, with the aim of enhancing public safety through a reduction in recidivism. The punitive approach, on the other hand, emphasizes the potential of harsh sentences to deter criminal behavior before it occurs. Due to longer incarceration, the punitive approach arguably also reduces crime due to incapacitation during the incarceration period.³

²For example, Georgia implemented a meditation program including rehabilitation and working with a social worker since 2010 instead of criminal prosecution for first time juvenile offenders, leading to a massive number of children receiving support outside of the justice system and close to 50 percent reduction in sentencing (Unicef, 2020). In China, there are both custodial and non-custodial measures for juvenile offenders and there is endorsement of suspended sentences for juveniles involved in minor offences.

³The topic of punishment and rehabilitation has also been addressed in a multidisciplinary literature in sociology and criminology where some studies find contrasting effects of punitive approach on

In this paper, I aim to contribute to the debate about the trade-off between leniency and deterrence by providing causal evidence on the effects of harsher punishment on youth crime. Until 2015, India did not punish juveniles who committed heinous crime, no matter how serious the crime was, due to the juvenile protection laws such as the Juvenile Justice (Care and Protection) Act (JJA), 2000. Instead, young people were sent to juvenile care centres until the age of 21 in accordance with the rehabilitative approach. In 2015, the Indian government introduced a reform establishing that any juvenile between the age of 16 and 18 years would be tried as adults if they committed a heinous crime. In India, rape and murder are the two crime types that are classified as heinous crimes for which the adult sentencing is at least 7 years in prison. This implies that from 2016 onwards, juveniles could face up to a lifetime in prison if sentenced for murder and rape after the reform.

Using NCRB administrative state-level crime data, I evaluate the effects of the reform by estimating a difference-in-differences model that compares crime committed by juveniles versus adults, before and after the reform. I find that both rape and murder decrease by 23.1 and 13.5 percent respectively for juveniles relative to adults after the reform suggesting that it has been successful in achieving its primary objectives. Further, event studies show that the effect seems to get stronger over time. I also find suggestive evidence that results are larger for states with high unemployment, low literacy, low GDP and high pre-reform juvenile heinous crime. The results are robust across a range of specifications with log and normalized dependent variables, and controlling for various individual and pair-wise fixed effects. Non-heinous crime like property crime, on the other hand, is not affected by the reform. However, I observe a significant recidivism for juvenile offenders treated by the reform (Wilson et.al., 2017; Bouchard and Wong, 2018).

decline in kidnapping. Although the reform did not increase punishments for this type of crime, this spillover effect may be explained by the fact that kidnapping is often accompanied by aggravating factors such as force, bodily injury, or sexual assault which are considered heinous crimes.

My paper contributes to several strands of the literature. First, I contribute to the academic debate about a rehabilitative versus punitive approach by focusing on youth crime to study whether harsher punishment deter juvenile crime. While an increasing number of papers show that tougher sentencing has a deterrent effect on adult crime (Marvell and Moody 1994; Levitt, 1998; Spelman 2000; Levitt and Lochner 2001; Abrams, 2012), there is little evidence on youth crime, despite the individual, social, and economic relevance of this phenomenon (Bayer et.al., 2009; Aizer and Doyle, 2015). My paper focuses on youth crime deterrence and provides evidence that, in the context of India, juvenile heinous crime can be deterred through harsher punishment.

Second, I contribute to the sparse literature that studies the impact of harsher punishment on youth crime. Most of these studies exploit the discontinuity in sentence severity at the age of criminal majority (Hjalmarsson (2009); Lee and McCrary (2017); Loeffler and Chalfin (2017); Lovett and Xue (2018); and Arora (2023)) and find negligible effects, most likely, because of a lack of awareness of the size of the sanction discontinuity. The 2016 Indian reform has been implemented across all states and has been extensively discussed in the media, thus offering evidence for a context where the change in sentencing is salient.⁴ The closest paper to mine is Damm et.al.(2017)

⁴My paper also relates to a wider economics literature on youth crime, which has investigated, for example, the effects of schooling on youth crime (Luallen, 2006; Anderson, 2014; Depew and Eren, 2016; Eren et.al., 2017), economic conditions and youth crime (Fleisher 1963, 1966), as well as neighborhood and peer effects in youth crime (Damm and Dustmann, 2014; Diaz et.al., 2021; Dustmann and Landerso, 2021; Dustmann et. al., 2023).

who find no crime deterrence effect of a Danish reform that reduces the age of criminal majority from 15 to 14, although they find some evidence of deteriorating effects on school outcomes. As pointed out by the authors, 14 year olds commit mostly property crime, for which even adults receive only mild punishment. Thus, this could explain why the Danish reform has little effect on youth crime. In India, significantly higher number of juveniles were getting involved in heinous crime for which adult punishment is very severe (up to life imprisonment/death penalty). Thus, the threat of punishment is likely to be much more severe for juveniles in the age of 16 and 18 in India after the JJA reform, and the deterrence effect is likely to be higher.

Third, the economic literature focuses on developed countries (Oka, 2009; Abrams, 2012; Damm et.al., 2017; Arora, 2023), while there is little evidence on the effect of harsher punishment in the context of developing countries, where a lack of economic opportunities may limit the impact of tougher sentencing. My study, thus offers a significant contribution to the currently sparse juvenile crime literature by being the first to evaluate the effectiveness of the harsher punishment reform in India. My findings suggest a deterrence effect in the context of developing countries, which is especially policy relevant when considering the large financial burden that state prisons bring to public expenses.

The remainder of the paper is structured as follows. Section 2 provides information on the institutional background and data used in our analysis. Section 3 describes our empirical strategy and estimation procedure. Section 4 presents the results and Section 5 concludes the paper.

1.2 Background and Data

1.2.1 Juvenile Justice in India

There has been a surge in juvenile crimes in India in the past few years leading to a dilemma in the justice system about the optimal approach to tackle this issue. The juvenile justice system in India has traditionally prioritised restoration and rehabilitation of children requiring support. To bring uniform laws for justice of juveniles, the Juvenile Justice Act, 1986 was enacted in line with the United Nations (UN) Minimum Rules for Administration of Juvenile Justice of 1985, dealing mainly with the rehabilitation of juveniles. Further, to protect the best interest of the juvenile offenders and their reintegration, the JJA, 2000 was enacted preventing any judicial proceedings and court trials against juveniles. Until December 2012, which witnessed a heinous act of gang rape in the Indian capital of New Delhi (Nirbhaya rape case) by four people including a minor, there used to be a reformatory approach towards treatment of juvenile offenders who were dealt with leniency in the name of reformation and rehabilitation. This event led to a public perception that the leniency emboldened some perpetrators to commit such crimes fearlessly. With an alarming number of young people getting involved in violent crimes like murder and rape, and a lack of well-equipped legal provisions to deal with such cases, the Indian government identified a need to update the existing Juvenile Justice Laws, adopting a more retributive approach to deal with such incidences.

Several public debates about finding a better way to prevent juveniles from committing such grave crimes led to the amendment of the existing JJA. Before this amendment, all juveniles committing a heinous crime (which in the Indian context includes murder

and rape), were kept in Juvenile Care Centres (JCC) until the age of 21 and released thereafter, while for non-heinous crimes like property crimes, they were either sent to JCCs or not punished at all depending on the case. However, after this new act came into force in January 2016, juveniles between the age of 16 and 18 years would be tried as adults if they committed a heinous crime, considering their mental capacity, while the same previous rule of sending to JCCs or no punishment would apply to all other younger juveniles (below 16 years). This meant that, instead of receiving no prison sentence at all, juveniles could be sentenced to a minimum of 7 years in prison for rape or murder, and in grave cases up to lifetime imprisonment. The JJA, 2015 also led to the introduction of Juvenile Courts which are special courts for juvenile offence trials only. Thus, the new act was rolled out with an aim of deterring heinous crimes among juveniles aged 16-18 years with careful consideration of individual case.

1.2.2 Data

This paper uses national and state level administrative data published by the NCRB, India, on crime incidence for above and below 18-year-olds from 2013 to 2021 for the 28 states and 7 union territories. The data set distinguishes various crime types: heinous crime, including murder, rape, attempt to murder; property crimes, including robbery and dacoity⁵ and other crimes like kidnapping that constitute non-heinous crime. I use data for 27 states and 1 UT (union territory, the capital city, New Delhi), omitting the state of Jammu and Kashmir as it was exempt from the JJA owing to its autonomous status, and the other 6 union territories contribute very little to the national juvenile crime numbers.

⁵Term used in India for an act of violent robbery committed by an armed gang.

The NCRB provides national level crime data for juveniles by classifying them into three age groups: below 12, 12-16 and 16–18-year-olds. However, at the state level, crime incidence is observed only for juveniles and adults, without further age group classification. Importantly, the younger age groups (12–16-year-olds and below 12-year-olds) contribute very little to overall juvenile crime⁶ (about 25 and 20 percent of overall and heinous juvenile crime respectively) Similarly, girls below 18 years, on average, commit less than 2 percent of total murders, attempt to murders and property crime, and less than 1 percent of total rape committed by juveniles, based on the NCRB juvenile crime data by gender. Thus, for the analysis at the state level, I compare below 18-year-old boys (treatment group) with above 18-year-old boys (control group).

I use the Census 2011 data to get population numbers by age group at the state level along with 2021 population estimates provided by the World Bank at the national level. I use state population proportions in 2011 for each age group and construct an estimate for state level population by age group for all the years from 2013 to 2021 taking into account the population projections which assume linear changes each year. I also use 2011 population data to construct weights for each state in order to perform weighted regression analysis. In the next subsection, I provide summary statistics for the outcomes of interest and introduce the identification strategy.

1.2.3 Descriptive Statistics

In this subsection, I present a descriptive analysis to motivate the identification strategy chosen for the empirical analysis. To analyze the effect of the reform, I compare the evolution of crime for below and above-18 years old, before and after the reform. Table

⁶I use National level data to check evolution of crime for the age group actually affected by the reform (16-18 year olds) and for all juveniles (below 18 year olds) and see similar evolution for the two groups suggesting that the younger groups had little contribution to heinous juvenile crime.

1.1 shows differences in the means of the outcomes of interest (crime rate, defined as crime per one million age-specific population for various crime types) for the treatment and control population for the pre-reform period for 27 states and 1 union territory.

Table 1.1: Summary Statistics by treatment status

Variables	Overall Mean (1)	Above 18 years (2)	Below 18 years (3)	Difference (2)-(3) (4)
Heinous crime	50.12	151.29	9.16	142.13***
Property crime	18.54	56.10	3.18	52.92***
Rape	16.37	48.41	4.81	43.60***
Murder	15.08	45.79	2.24	43.55***
Attempt to murder	18.67	57.10	2.11	54.99***
Robbery	16.52	50.01	2.73	47.28***
Dacoity	2.02	10.83	0.25	10.58***
Kidnapping	33.36	102.14	3.51	98.63***
No. of States/UT	28	28	28	

Source: NCRB Juvenile and Adult Crime Data 2013-2015

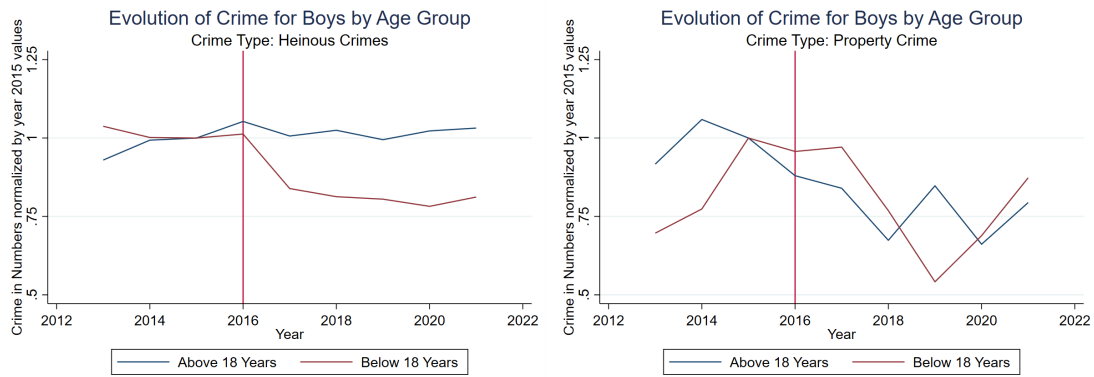
Notes: Crime rate defined per 1 million population.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

From table 1.1, it can be seen that the above and below 18 year olds differ significantly in terms of main outcome of interest which is heinous crime. Similarly, for other outcomes like the heinous crime subcategories of rape and murder, attempt to murder and other crimes like robbery, dacoity, property crime and kidnapping are also significantly higher for adults compared to juveniles in the pre-reform period. These are level differences, and below, I compare the crime evolution for the above and below 18 year olds, which matters for the difference-in difference approach that is discussed in the next section.

To capture the relative evolution of crime over time, I generate crime numbers normal-

Figure 1.1: Evolution of Heinous and Property Crime for Boys



Source: NCRB Juvenile and Adult Crime Data 2013-2021

ized to 1 in 2015 (one year prior to the enforcement of the JJA) by dividing the annual values of a given series of crime data by its 2015 value. I plot this relative evolution of heinous crimes (directly affected by the JJA) and property crimes (not directly affected by the JJA) in the left and right panel of figure 1.1 respectively. The evolution of heinous crime appears comparable across the two groups before the reform. In contrast, after the reform the two trends diverge. Importantly, this is not the case when looking at property crime (right-hand side graph), which was not targeted by the reform. The plots suggest a divergence in heinous crimes as the treatment group seems to witness a fall after 2016 while there seems to be no significant change for the control group. These graphs provide suggestive evidence that the JJA could be associated with significant lower juvenile crime for the targeted heinous crime types like murder and rape. In support of the identification strategy, the pre-reform dynamics to test for the parallel trends assumption are presented in the results section.

In the following section, I discuss the empirical strategy used in the analysis to study how the outcome variables are affected due to the reform using various specifications. Along with heinous crime, I also look at the various crime sub types to shed light on

how each of those individually are affected by the reform.

1.3 Methodology

To conduct my analysis, I use the difference in difference approach that exploits the state, year and age group variations in incidence of crime. The main regression specification is defined as follows:

$$\ln(Y_{ijt}) = \beta_0 + \beta_1.Reform_t * Below18_i + \beta_2.Population_{ijt} + Z_{ij} + \gamma_{jt} + \epsilon_{ijt}, \quad (1)$$

where Y_{ijt} refers to the numbers recorded of a given crime type, for age group i in state j in year t , $Reform_t * Below18_i$ is the interaction term of the reform and treatment group dummy, $Population_{ijt}$ refers to the age specific population in millions for state j in year t , Z_{ij} includes state by age-group fixed effects (FE) to control for unobserved state and age-group specific characteristics that do not vary across time and influence the outcomes of interest. γ_{jt} includes state by year fixed effects to control for any unique shocks or events that affect specific states in specific years, and ϵ_{ijt} is the error term. In all the regression results presented in the following sections, the standard errors are clustered at the state times age group level to take into account the within cluster correlation of the standard errors.

Further, I present variations of equation (1) that include individual state and year fixed effects; only state-by-year fixed effects; year and state-by-age fixed effects. Additionally, I present a variation of the baseline specification which weights the regression by the 2011 population of each state. Finally, I present results with the dependent variable being the non-logarithmic normalized crime numbers defined above (normalized to 1

in 2015) unweighted and weighted respectively.

The main coefficient of interest is β_1 , which conditional on the validity of the identification strategy, captures the impact of the reform on juvenile heinous crime. The difference-in-difference approach relies on the parallel trends assumption, in absence of the reform, juvenile and adult crime numbers would have evolved in parallel. I test this assumption by estimating an event-study specification in the next section. This identification strategy also requires that the timing of the juvenile crime reform does not coincide with other events that happen at the same time as the reform and affect the below and above 18 year olds differently. To the best of my knowledge, there are no other major events that happen at the same time of the crime reform that could affect the treatment and control group differently.

In 2010, the Indian government approved an ambitious reform that aimed at compulsory primary schooling for the 6-14 year olds was introduced in 2010 in India. However, there is no perfect overlap in who is treated by the RTE and the JJA reforms. In the section on heterogeneity analysis, I also provide evidence that the results were not driven by the education reform. Further, in this paper, I find that there are no significant effects of the JJA reform on non-heinous crimes, such as property crimes (which are not affected by the JJA reform). If the results were driven by the earlier education reform, then one would expect the effects across all crime types – not specifically for heinous crime, for which this paper find the strongest effects. The analysis in the next section also provides evidence to suggest that the effects were not driven by the COVID-19 pandemic.

1.4 Results

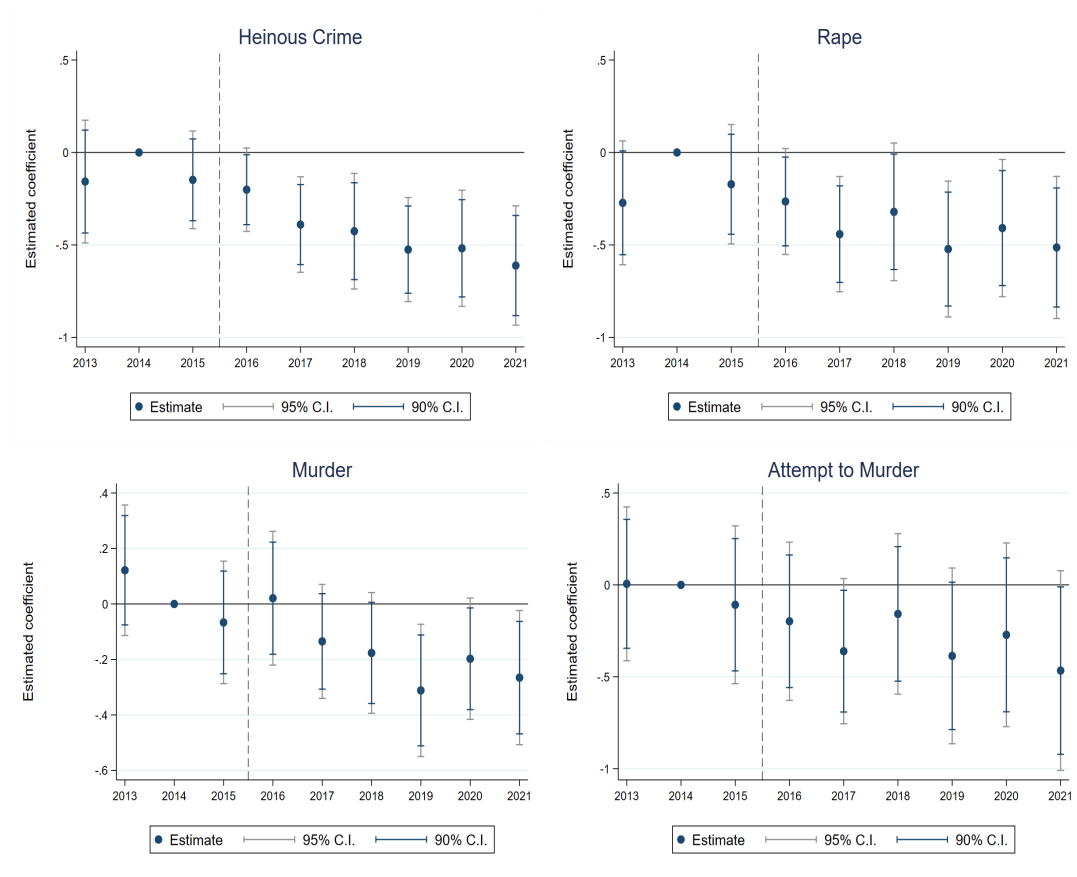
1.4.1 Event-Study Results

In figure 1.2, I present event-studies estimated using a dynamic specification of model 1, to explore the dynamic effects of the reform and test for differential pre-trends across treated and control groups. Testing the parallel trends assumption seems especially important in this context, as the treatment has not been randomly assigned. Here, 2014 is chosen as the reference year, to investigate potential anticipation effects that could take place in 2015, when the reform was discussed in parliament.

The event-study coefficients presented in figure 1.2 and appendix table 1.5 are obtained from a regression specification similar to equation (1), but instead of a Post indicator, now each year dummy (except for the reference year) is interacted with the treatment group dummy. The insignificant pre-policy year (2013 and 2015) coefficients suggest that the crime levels for below and above 18 year-olds seemed to evolve in parallel and thus, do not violate the parallel-trends assumption.

These graphs also exclude that the reform generated visible anticipation effects in 2015. In contrast, after 2016, the reform appears to decrease heinous crime for the below-18 group relative to adults, with the effect being especially visible for both rape and murder, and a bit noisier for attempt to murder. From these estimates, it can be inferred that the effect seems to build over the first years starting 2016 and remains relatively stable. Thus, I rule out the possibility of only COVID-19 driving these results.

Figure 1.2: Event Studies by Crime Type



Source: NCRB Juvenile Crime Data 2013-2021

1.4.2 Average Effect on Crime

In this subsection, I present the average effect (across all post-reform years, relative to the pre-reform years) of the reform on heinous crime followed by the crime sub types for various heinous and non-heinous crimes.

Table 1.2: Impact on Heinous crime

Explanatory Variables	Logarithm						Levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Below 18* post reform	-0.260*** (0.075)	-0.258*** (0.096)	-0.260*** (0.103)	-0.219*** (0.075)	-0.272*** (0.098)	-0.345*** (0.134)	-0.256*** (0.122)	-0.282*** (0.126)
Below 18 years	-3.523*** (0.075)	-3.499*** (0.096)	-3.512*** (0.103)	-	-	-	-	-
Age-specific population	-	0.002 (0.004)	0.001 (0.109)	0.054*** (0.018)	-0.015 (0.056)	-0.028 (0.053)	-0.017 (0.062)	-0.000*** (0.000)
Observations	504	504	504	504	504	504	504	504
R squared	0.958	0.958	0.969	0.980	0.990	0.991	0.763	0.776
State FE	✓	✓						
Year FE	✓	✓		✓				
State-by-year FE			✓		✓	✓	✓	✓
State-by-age FE	✓	✓	✓	✓	✓	✓	✓	✓
Weighted						✓		✓

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: The data varies at the state-age-group-year level. In columns 1-6, the dependent variable is the log number of crimes, while in columns 7-8, the dependent variable is the normalized number of crimes (normalized to 1 in 2015 by dividing each time series by its 2015 value). In both cases, the effect sizes have a similar interpretation (relative effect sizes compared to the pre-reform period).

Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

From Table 1.2, I find that heinous crime falls significantly for the below 18-year olds after the reform compared to the above 18-year olds. The specification in column (1) controls only for state and year fixed effects. The results show that heinous crime of below 18-year olds reduced by approximately 26 percent after the reform, relative to above 18-year olds. From column (2) onwards, I include the age-group specific population of each state as a control variable, to account for the fact that the population

size of age groups could have changed over time.⁷ Column 3 includes state-by-year FE, while column 4 includes year and state-by-age FE. In column 5, I include both of these fixed effects together and column 6 shows results of weighted regressions based on 2011 population weights. In the last two columns I present specifications where the dependent variable is expressed in levels (number of crimes normalized to 1 in 2015 by dividing each time series by its 2015 value). The first insight of this table is that, irrespective of the controls included and the functional form considered, the reform significantly decreases juvenile heinous crime, compared to adult crime.

In my preferred specification, displayed in column 5, the average effect is a 27.2 percent decline, significant at 1 percent. Running alternate specifications helps understand how the results vary under different assumptions, control variables or fixed effects and reveals which variables consistently matter, thus offering robustness to the findings. Table 1.2 shows that the results are robust across a range of specifications and provide evidence that the juvenile crime reform worked in achieving its aim of reducing heinous juvenile crime and are in line with Becker's model of crime (1968) along with other studies like Spelman (1994), Marvell and Moody (1994); Levitt (1996) which have found evidence that increasing adult punishment helps deter crime. However, my results are in contrast to the only causal evidence of such a reform on juvenile crime (Damm et.al., 2017) which finds no deterrence effects.

In table 1.3, I examine effects on crime sub types, using my preferred specification with the log dependent variable which includes two pairwise fixed effects interactions (i.e., the specification in column (5) of Table 1.2). The results for other specifications are

⁷I use age-specific population figures from Census 2011 and projections from World Bank data for 2021. I then infer population over years and age-groups by assuming a linear trend from 2011 to 2021 based on year on year projections.

presented in appendix tables 1.6-1.10 for different crime sub types.

Table 1.3: Impact on Various Crime Subtypes

Explanatory Variables	Heinous crime (1)	Rape (2)	Murder (3)	Attempt to Murder (4)	Kidnapping (5)	Property Crime (6)
Below 18* post reform	-0.272*** (0.098)	-0.231** (0.114)	-0.135* (0.077)	-0.130 (0.099)	-0.284** (0.138)	0.026 (0.107)
Age-specific population	-0.015 (0.056)	0.162** (0.071)	0.002 (0.004)	-0.083* (0.049)	0.292*** (0.018)	-0.008 (0.061)
Observations	504	504	504	504	504	504
R squared	0.990	0.983	0.993	0.990	0.983	0.985
State-by-year FE	✓	✓	✓	✓	✓	✓
State-by-age FE	✓	✓	✓	✓	✓	✓
Weighted						

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: The dependent variable is log number of crimes in the column headings.

Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

These results show that the reform significantly decreases both components of heinous crime, namely rape and murder by 23.1 and 13.5 percent respectively. The point estimates for attempt to murder are also negative, and the event studies point towards evidence of a decline in this outcome as well. Thus, the results go in the same direction but are less precisely estimated.

Property crime, which was not directly targeted by the reform, does not seem to be significantly different for the two age groups after the reform. Remarkably, however, the JJA appears to have spillover effect on kidnapping, an offence which is often committed together with the heinous crimes directly targeted by the reform. The results show a 28 percent fall in this crime for below 18-year-olds after the reform. Such spillover effects could be due to several reasons. When kidnapping occurs in conjunction with aggravating factors involving bodily injury or sexual assault, the perpetrators could be charged with higher punishment up to life imprisonment which is similar to that of

heinous crime. This may cause fear of being tried as an adult also for kidnapping. In some instances kidnapping is carried out as a means to achieve one of the heinous crimes, implying that when the heinous crime is deterred, the kidnapping that is complementary to it, is deterred too. Age-specific population seems to be associated with a significant increase in rape and kidnapping, a small significant decrease in attempt to murder while no significant effect on murder or property crimes.

1.4.3 Heterogeneity Analysis

In this subsection, I study whether certain states witness stronger effects of the reform than others based on factors like the pre-reform level of unemployment, state GDP, adult literacy rate and pre-reform juvenile heinous crime incidence in that state. Caruso (2015) finds association between higher unemployment and higher violent crime such as rape. Further, lower adult literacy can increase the likelihood of youth engaging in criminal activities as it might indicate low school enrollment rates, low levels of education and lower academic performance, all of which are known to be related to higher crime (Sabates and Feinstein, 2008; Cook and Kang, 2016). On the other hand, states with better economic conditions such as low unemployment and high state GDP are expected to have modern law enforcement thereby having lower crime incidence (Mishra and Verma, 2021). I hypothesize that the scope of a greater (deterrence) effect of the JJA reform would thus be in states with high unemployment, low adult literacy and low state GDP. Similarly, states with higher pre-reform incidence of heinous juvenile crime are expected to witness stronger effect of the JJA because when crime levels are high to begin with, there is more scope to reduce them.

To conduct this analysis, for a given factor, I create a dummy variable ($Factor_j$) to indicate whether a state is above or below the median for the respective factor. I then

augment my preferred baseline specification with interaction terms of this dummy with the reform and treatment dummy as follows:

$$\ln(Y_{ijt}) = \beta_0 + \beta_1.Reform_t*Below18_{jt}*(1 - Factor_j) + \beta_2.Reform_t*Below18_{jt}*Factor_j + \beta_3.Population_{ijt} + Z_j + \gamma_t + \epsilon_{ijt} \quad (3)$$

The coefficient β_1 then captures the reform effect for states where the factor is below the median, and the coefficient β_2 captures reform effects for states where the factor is above the median. I also report the difference between these two effects, and whether the difference is significant.

Broadly, the results in columns (1)-(4) of table 1.4 confirm my hypothesis across all outcomes. The reform effects indeed tend to be stronger for high unemployment, low literacy, low-GDP and high pre-reform juvenile heinous crime incidence states. However, the degree of the difference varies across these outcomes, and is statistically significant only for the low-literacy states. Thus, at least in this simple specification with an above-below-median split, low literacy seems to be the best predictor amongst these characteristics for stronger reform effects. These results suggest that increasing punishment for juvenile crimes seems especially effective in fragile social contexts. Further, column (5) shows that the effect is not driven by states that were mostly affected by the education reform. Thus, I rule out that the effects of the JJA could be driven by the education reform.

Table 1.4: Heterogeneous Effect on Heinous Crime

Explanatory Variables	High Unemployment (1)	Low Literacy (2)	Low State-GDP (3)	High Crime Incidence (4)	More Affected by Educational Reform (5)
Below 18*post-reform*(1-factor)	-0.248*** (0.093)	-0.119 (0.121)	-0.197* (0.106)	-0.186* (0.107)	-0.205 (0.116)
Below 18*post-reform*factor	-0.286** (0.098)	-0.465*** (0.132)	-0.305*** (0.126)	-0.392*** (0.148)	-0.362 (0.133)
P-value for test of equality of coefficients	0.791	0.017	0.495	0.171	0.298
Observations	504	504	504	504	504
R squared	0.991	0.991	0.991	0.991	0.991
State-by-year FE	✓	✓	✓	✓	✓
State-by-age FE	✓	✓	✓	✓	✓
Weighted					

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: The dependent variable is log number of crimes in the column headings.

Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

1.5 Discussion and Conclusion

Youth crime is a costly phenomenon with adverse effects on victims, perpetrators, and society more widely. Countries around the world have adopted welfare and justice models to tackle the global issue of rising youth crime, with no clear evidence to suggest if one approach is better than the other. In this paper, I offer fresh evidence on the effectiveness of a punitive approach towards juvenile crime by studying the effects of harsher punishment enforced through the Juvenile Justice Act of 2015 on heinous juvenile crime in India. The results show that the reform had, on average, a strong negative effect of about 27 percent on heinous juvenile crime, driven mostly by a decrease in cases of rape. Studying the dynamic impact of this reform, I find that the results become stronger over time. Thus, the reform seems to have achieved its objective of reducing heinous juvenile crime which was rising sharply since 2002. Further, states with lower youth literacy saw a stronger reform effect compared to states with relatively higher youth literacy. Such reforms could prevent more juveniles from engaging

in such crimes and could potentially help them focus on development, thereby achieving better labor market outcomes for themselves.

These results have important policy implications and indicate that stricter laws and punishment could be more useful in curbing heinous criminal activity among youth than a welfare model. This is especially relevant in a country like India where previously, a focus on youth well-being implied no formal adult punishment for juveniles committing heinous crime thereby continuing to witness massive increase in these crimes over time. By studying the impact of the JJA reform on juvenile crime, this study suggests that a justice model could help control juvenile crime, thereby preventing major social and economic problems like mental illness and behavioural issues. Further, as studies have found a positive link between youth crime and secondary school-dropouts, an effective reform like the JJA could tackle this issue in India where about 18 percent of the boys drop out of secondary school on average. Additionally, deterring youth crime could also prove to be a boon by improving poor productivity and employment outcomes for the youth (Golan et.al., 2021).

Further research could study the effect of this juvenile justice reform on college/university attainment along with labor market outcomes. It would be interesting to see how such reforms could help foster economic growth through youth crime control, enabling more and more young people to indulge in constructive work.

Outside of economics, recent research such as Wangombe (2019), suggests that restoration and rehabilitation models could prevent juveniles from committing a crime in the future (recidivism), while other research suggests ineffectiveness of deterrence in crime reduction (Howell, 2003; Schneider, 2012). India, that was following a reformative

approach until several juvenile heinous crime incidences led to a punishment based model, seems to have reduced juvenile crime incidence at least for the targeted crime types. For those individuals who still engage in crime despite the reform, they now receive a punishment rather than a rehabilitation. For them, it could have a negative (i.e., 'bad') effect on their recidivism due to exposure to criminal justice system. However, since they will get a minimum of 7 years sentence, it is beyond the scope of this study to look at recidivism for such individuals.

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1.7 Appendix

Event Studies and Additional Specifications for Crime Subtypes

Table 1.5: Dynamic effects of the JJA 2015, for Heinous Crime and Sub-crime

Explanatory Variables	Heinous Crime (1)	Rape (2)	Murder (3)	Attempt to Murder (4)
Year 2013*Below 18	-0.16 (0.23)	-0.27 (0.21)	0.12 (0.17)	0.19 (0.21)
Year 2015*Below 18	-0.15 (0.15)	-0.17 (0.22)	-0.07 (0.11)	0.14 (0.14)
Year 2016*Below 18	-0.20** (0.11)	-0.26** (0.15)	0.02 (0.12)	0.13 (0.12)
Year 2017*Below 18	-0.39*** (0.13)	-0.44*** (0.16)	-0.13 (0.10)	-0.07 (0.17)
Year 2018*Below 18	-0.43*** (0.16)	-0.32** (0.19)	-0.18* (0.11)	-0.18 (0.15)
Year 2019*Below 18	-0.53*** (0.14)	-0.52*** (0.19)	-0.31** (0.12)	-0.03 (0.14)
Year 2020*Below 18	-0.52*** (0.16)	-0.41*** (0.19)	-0.20* (0.12)	-0.07 (0.17)
Year 2021*Below 18	-0.61*** (0.16)	-0.51*** (0.19)	-0.27** (0.12)	0.15 (0.15)
Observations	504	504	504	504
R squared	0.99	0.98	0.99	0.99

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: The dependent variable is log number of crimes in the column headings.

Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.6: Impact on Rape

Explanatory Variables	Logarithm					Levels	
	(1)	(2)	(3)	(4)	(6)	(7)	(8)
Below 18* post reform	-0.356*** (0.105)	-0.347*** (0.107)	-0.347*** (0.103)	-0.344*** (0.096)	-0.309*** (0.131)	-0.247* (0.122)	-0.273* (0.174)
Below 18 years	-3.039*** (0.097)	-2.910*** (0.096)	-2.909*** (0.103)	-	-	-	-
Age-specific population	-	0.012 (0.004)	0.012 (0.004)	0.015 (0.019)	0.128 (0.069)	0.160 (0.128)	0.123 (0.084)
Observations	504	504	504	504	504	504	504
R squared	0.940	0.941	0.964	0.969	0.983	0.762	0.823
State FE	✓	✓					
Year FE	✓	✓		✓			
State-by-year FE			✓		✓	✓	✓
State-by-age FE				✓	✓	✓	✓
Weighted					✓		✓

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.7: Impact on Murder

Explanatory Variables	Logarithm					Levels	
	(1)	(2)	(3)	(4)	(6)	(7)	(8)
Below 18* post reform	-0.103 (0.089)	-0.100 (0.089)	-0.100 (0.103)	-0.091 (0.065)	-0.179** (0.096)	-0.243 (0.239)	-0.010 (0.195)
Below 18 years	-3.575*** (0.072)	-3.531*** (0.096)	-3.535*** (0.098)	-	-	-	-
Age-specific population	-	0.004 (0.003)	0.015 (0.014)	-0.042 (0.042)	-0.040 (0.048)	-0.110 (0.103)	0.015*** (0.077)
Observations	504	504	504	504	504	504	504
R squared	0.964	0.964	0.972	0.985	0.993	0.764	0.795
State FE	✓	✓					
Year FE	✓	✓		✓			
State-by-year FE			✓		✓	✓	✓
State-by-age FE				✓	✓	✓	✓
Weighted					✓		✓

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.8: Impact on Attempt to Murder

Explanatory Variables	Logarithm					Levels	
	(1)	(2)	(3)	(4)	(6)	(7)	(8)
Below 18* post reform	-0.066 (0.119)	-0.052 (0.118)	-0.053 (0.143)	-0.029 (0.079)	-0.345*** (0.098)	-0.256*** (0.122)	-0.282*** (0.126)
Below 18 years	-3.873*** (0.086)	-3.667*** (0.099)	-3.679*** (0.117)	-	-	-	-
Age-specific population	-	0.019*** (0.005)	0.018** (0.007)	0.048** (0.020)	-0.048 (0.0)	-0.017 (0.062)	-0.000*** (0.000)
Observations	504	504	504	504	504	504	504
R squared	0.938	0.939	0.948	0.980	0.991	0.764	0.776
State FE	✓	✓					
Year FE	✓	✓		✓			
State-by-year FE			✓		✓	✓	✓
State-by-age FE				✓	✓	✓	✓
Weighted					✓		✓

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.9: Impact on Kidnapping

Explanatory Variables	Logarithm					Levels	
	(1)	(2)	(3)	(4)	(6)	(7)	(8)
Below 18* post reform	-0.415*** (0.121)	-0.398*** (0.120)	-0.398*** (0.128)	-0.385*** (0.109)	-0.562*** (0.170)	-0.043 (0.136)	-0.429*** (0.153)
Below 18 years	-4.036*** (0.096)	-3.794*** (0.113)	-3.794*** (0.115)	-	-	-	-
Age-specific population	-	0.022 (0.004)	0.022 (0.005)	0.038 (0.025)	-0.270*** (0.072)	-0.059 (0.064)	-0.238*** (0.078)
Observations	504	504	504	504	504	504	504
R squared	0.950	0.952	0.966	0.972	0.988	0.664	0.676
State FE	✓	✓					
Year FE	✓	✓		✓			
State-by-year FE			✓		✓	✓	✓
State-by-age FE				✓	✓	✓	✓
Weighted					✓		✓

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 1.10: Impact on Property Crime

Explanatory Variables	Logarithm					Levels	
	(1)	(2)	(3)	(4)	(6)	(7)	(8)
Below 18* post reform	0.032 (0.124)	0.051 (0.121)	0.052 (0.143)	0.019 (0.093)	-0.115 (0.140)	-0.203 (0.235)	-0.431** (0.173)
Below 18 years	-3.436*** (0.090)	-3.163*** (0.105)	-3.153*** (0.121)	-	-	-	-
Age-specific population	-	0.029*** (0.004)	0.026*** (0.006)	-0.017 (0.020)	-0.098 (0.071)	0.041 (0.095)	-0.065 (0.064)
Observations	504	504	504	504	504	504	504
R squared	0.929	0.932	0.946	0.972	0.984	0.677	0.686
State FE	✓	✓					
Year FE	✓	✓		✓			
State-by-year FE			✓		✓	✓	✓
State-by-age FE				✓	✓	✓	✓
Weighted					✓		✓

Source: NCRB Juvenile Crime Data (2013-2021).

Notes: Standard errors in parentheses, clustered at state times age group level.

*** p<0.01, ** p<0.05, * p<0.1.

2 Chapter 2: School Resources and Primary Education Attainment: Evidence from India

2.1 Introduction

Primary school education forms the basis of development by equipping children with foundation skills, gearing them for labour market, helping them with empowerment and enabling them to escape poverty cycles (Elango et al., 2016). The World Bank has identified primary schooling as a crucial factor affecting socio-economic progress of developing and underdeveloped countries. At the same time, only getting children to school does not necessarily help reap the benefits of education, but quality learning is of utmost importance for children to thrive (Bonetti et al., 2018). Several developing countries have understood the importance of primary schooling and implemented various policies aimed at achieving higher primary school enrolment. Initiatives like the Education for All movement and the Millennium Development Goals have promoted universal primary education as an important policy. Despite the significant progress among the developing nations in raising the primary school enrolment rates, low-enrolment rates persist in various regions like South Asia (85.8 percent) and Sub-Saharan Africa (74 percent) as per the World Bank data as of 2012. School enrolment in countries like India is low due to the high direct and opportunity costs of schooling, inadequate infrastructure, shortage of teachers and problems like sanitation and safety (Birdsall, Levine,

and Ibrahim, 2005). In India, primary education has not received adequate attention from researchers yet, and there is not enough evidence on effectiveness of various government policies related to this topic which is of utmost importance for every country.

In this paper, I study the impact of a major educational reform in India, namely, the Right to Education Act (RTE, 2010), on primary school enrolment and school, classroom, and teachers' quality indicators. The RTE Act came into effect on April 1, 2010, and mandated that all children currently not in school should be admitted to an age-appropriate class and provided with special training to enable the child to come up to age-appropriate learning level. At the same time, the reform aimed at providing funding to states to improve school infrastructure and other quality indicators such as new classrooms, professionally qualified teachers and health and sanitation. To evaluate the RTE, I use district level education data from District Information System for Education (DISE data, 2007-2015). This administrative data set includes information on school enrolment, performance and quality indicators, number of schools, classrooms and teacher-student ratios. To study the impact of the RTE, I estimate a difference-in-difference model that compares districts with low- pre-reform school enrolment and districts with high pre-reform enrolment. My hypothesis is that the RTE reform would have larger impact on the former as these districts are more in need of improvement in enrolment, number of schools and classrooms to accommodate new students. My specification thus allows me to assess the degree of convergence between the two groups of districts due to the reform.

I find that the reform significantly increased enrolment per population for the low pre-reform school enrolment districts than the high pre-reform school enrolment districts. Further, the reform also led to a significant increase in the number of schools and qual-

ified teachers for the former than the latter along with lower teacher-student ratios. Other quality indicators like condition of classrooms, and presence of computers and libraries, also improved more for the districts with low pre-reform school enrolment. Analysis by gender suggests that enrolment increased for both boys and girls after the reform, however, the effect was larger for boys than girls, exacerbating the pre-existing gender difference in enrolment in the low pre-reform enrolment districts.

My work mainly contributes to the literature that studies the impact of an increase in resources on school outcomes such as Valente (2013); Glewwe et.al. (2020); Lavy (2020). Conditional Cash Transfer (CCT) programs are associated with an increase in enrolment but lower classroom availability (Das and Sarkhel, 2023). Similarly, enrolment also seems to increase with resources like merit based scholarships (Friedman et.al., 2016), in-kind transfers (Hidalgo et.al., 2013), and school construction (Duflo, 2001; Burde and Linden 2013; Kazianga et.al., 2013). Some descriptive studies on evaluation of RTE, such as Varghese (2022) use an event study comparing the state of Jammu and Kashmir (which was exempt from the RTE enactment owing to its autonomous status) with all other states as the treatment group. With a focus on children with disabilities (CWD), this study finds an increase in schooling among CWD. Shah and Steinberg (2019) provide trends in enrolment, test scores and school quality from the RTE but these are not causal estimates. They find that primary school enrolment seems to be increasing moderately over time while test scores seem to be decreasing. I contribute to these studies by exploiting new data and an alternative stronger identification strategy to study the causal effects of the RTE. My identification strategy uses a finer and more generalised difference-in-differences (DiD hereafter) approach, exploiting variation at district level within states. Proxying reform exposure at the district level has the advantage that I can control for time-varying state-level unobserved het-

erogeneity. I further contribute to these studies by establishing a relationship between increase in resources and a wider set of outcomes to measure quality of education. Besides enrolment of primary students, I also investigate the effect on number of schools and classrooms constructed over the years at the district level, the number of teachers (overall numbers and professionally qualified), along with other quality indicators like number of classrooms in good condition, schools with computers and library, pupil-teacher ratios, to estimate the causal impact. These variables are well-suited to study the effect of the RTE in terms of its success given its aims and expectations.

Due to the nature of the RTE reform, my work is related to three further strands of the literature. First, I contribute to studies that have evaluated legal mandates of making primary schooling compulsory. Introduction of a compulsory schooling reform has been associated with higher school enrolment (Oreopoulos, 2006; Elsayed, 2019; Cornelissen and Dang, 2022). My paper adds to this literature by studying a nation-wide reform that not only targeted enrolment but also aimed at improving the quality of education. Second, this paper relates to work that has evaluated abolishing fees for primary education. One of the major deterrents to access of education in different settings is found to be the school fee (Glewwe et. al., 2008). A review of the relevant literature suggests that reducing or abolishing school fees results in significant increase in primary enrolment (Lucas and Mbiti, 2012; Shi, 2016). Another study in China suggests that policies with tuition waivers, free textbooks, subsidies on living expenses had a significant positive effect on enrolment of rural girls due to increased enrolment of girls living in poor households (Chyi and Zhou, 2014). Third, this paper contributes to the literature that studies gender gaps in education in developing countries (Baird et.al. 2011; Lucas and Mbiti, 2012; and Burde and Linden 2013) by documenting persisting gender differences in household investment in children's education. My study sheds light on

whether any pre-existing gaps in primary enrolment are narrowed or exacerbated after the introduction of the reform.

The remainder of the paper is structured as follows. Section 2 provides information on primary education in India followed by the data used in this analysis in section 3. Section 4 describes the empirical strategy and estimation procedure. Section 5 presents the results, Section 6 presents robustness checks and Section 7 concludes the paper.

2.2 Primary Education In India

The National Policy on Education (NPE), 1986, aimed at expanding educational facilities all over India. But even after this initiative, there was a lack of implementation, problem of access to education, quality issues accumulating over the years. The overall primary school enrolment in India was about 84 percent in 2003 which increased to 91 percent in 2007 and stayed constant until 2010 (Unicef, 2021) which suggests that India has made some improvements in providing access to education. However, nearly half of the primary school going children did not achieve appropriate learning levels as per National Achievement Survey, conducted in India in 2017 by the National Council of Educational Research and Training. About 85 percent of the children in rural areas could not even read basic words (ASER, 2008). Although the primary school enrolment was increasing, the poor quality of schools and teachers was hampering learning outcomes. The Sarva Shiksha Abhiyan was enacted in 2001 to cover minor projects of the government aiming to improve curriculum and educational planning. It opened up a new approach to primary school interventions and subsequently led to the enactment of the 'Right to Education Act' 2009, approved by the Parliament of India on August 4, 2009, which came into force in April 2010. To the best of my knowledge, there are no

other policies coinciding with the RTE reform, that could affect high- and low-density districts differently.

By making free education a fundamental right for every primary age child, India stepped towards the United Nation's Education 2030 agenda which aims to ensure inclusive and equitable quality education and promote lifelong learning opportunities for all. The RTE Act was passed with the aim to have all the primary children (aged 6-14 years), irrespective of caste, gender, religion, financial background, to be educated, that is, with an aim of achieving 100 percent primary enrolment rates. Describing the importance of free and compulsory education for children in the age group of 6-14 years in India, this act made education a fundamental right for all children. RTE Act surveyed all the localities/neighbourhoods through means of qualitative interviews to identify the children who are eligible to avail education under this act but are not in school for reasons like lack of means or other reasons. These surveys helped set up facilities to provide free education to the ones not currently availing. The out of school children were admitted to a class that was age appropriate and provided with special guidance and training in order to cope up with the level at which he/she had been admitted. This took form of teachers spending extra time with those students to help them learn the pre-requisites for the class they would be admitted to, based on their age. The Act made it legally binding for state and local governments to follow the norms laid down under the Act. A school was to be established in neighbourhood as prescribed, within a period of three years from the commencement of the Act. The state can refuse to grant recognition to schools or withdraw funding that has been conferred, for schools that do not adhere to the prescribed minimum quality, standards, and rules. Under this act, no charges/fees are to be paid by the children in order to complete elementary education. The children would also receive resources like free uniforms and textbooks,

along with special educational resources for the children with disabilities. The reform lays down standards for Pupil-Teacher Ratios (number of children per teacher), classrooms, separate toilet facilities for boys and girls and safe drinking water availability. All kinds of discrimination, punishment and harassment is prohibited under this act. The government also aims to ensure that there is no urban-rural imbalance in terms of the specified Pupil-Teacher Ratio, and that there is an appointment of appropriately trained and academically qualified teachers. It also focuses on overall development of the children, and no holding back/expulsion until 8th standard. All the schools that are covered under the RTE, are under an obligation to have Management Committees, which would comprise of a headmaster and the parents. The role of these committees would be to plan the development of the schools and monitor their functioning. The act also has a Grievance Redressal mechanism to deal with the issue of non-compliance and take necessary actions.

2.3 Data

This study uses the District Information System for Education (DISE) data for the years 2007-15, published by the National Institute for Educational Planning and Administration (NIEPA). This data set covers all the schools (about 1.4 million) across a total of 604 districts in 28 States and 6 Union Territories in India. It provides information on key variables at the district, state and National levels. In particular, it includes information on elementary education in terms of the number of schools, classrooms, enrolment, and teachers, classified by school category and school management. There is grade-wise and level-wise enrolment data for each State. The data set also gives information about the condition of classrooms, whether they require minor or major repairs, by school category. It provides data on the various performance indicators by

school category; number of primary, upper primary schools, number of classrooms, single-teacher schools and so on. There is also data on quality indicators by schools category, teacher-pupil ratios, female teacher availability/ no female teacher schools, school and classroom infrastructure. I exclude the state of Jammu and Kashmir as it was not affected by the reform, and the state of Goa, due to missing data. Thus, this study has a total of 580 districts in the analyses.

The School Data reported here is taken from official school records (submitted by the School Head Master or the Head Teacher through the District and State level authorities), and could thus be referred to as administrative data. It is first checked at the cluster level by the Cluster Resource Centre Coordinator, before being submitted to the authorities at the National level. At the district level, the concerned authorities check if there are any inconsistencies in the data, after which the data is transmitted to the State level authorities. I complement this data set with population data for 6-14 year olds taken from the Census of India (2001 and 2011).

In the following subsections, I explain the construction of the treatment and control groups followed by summary statistics for the outcome variables of interest, enrolment, schools and classrooms per population.

2.3.1 Descriptive Statistics

In this subsection, I present the summary statistics with the aim of guiding the reader to the identification strategy chosen for the analysis. For the purpose of this paper, the analysis involves identifying the causal effect of the reform on school attainment. While the reform has been implemented at the same time across all Indian states, it seems reasonable to assume that states with low pre-reform enrolment may have been

more affected than those states which had achieved high enrolment rates already before 2010. This is because the former ones are more in need of a catch-up in terms of having higher enrolment, more schools and classrooms to accommodate new students. On the other hand, the high-density districts might already have a higher level of schooling to begin with, thereby leaving less scope for further increment. To study the impact of the reform, I thus compare its effects across so-called low-density and high-density districts. To construct the outcome variables per population (as densities), I use the enrolment density (absolute number per hundred population), school and classroom density (absolute number per ten thousand population),¹ using the population census data and determine the high- and low-density districts using the median value of these variables. The low-density districts (corresponding to below median districts) are thus the ‘treatment’ districts while the high-density districts (corresponding to above median districts) are ‘control’ ones. For the purpose of robustness checks, I also propose an alternative definition of treatment and control group by constructing the quartile densities and comparing the bottom quartile (treatment) with the top quartile (control) in the results section, for which I use the school density as explained above.

Table 2.1 shows differences in means of the outcomes variables for the treatment and control districts for the pre-reform period. It also shows differences in means for the other variables like proportion of urban districts, quality indicators like number of classrooms in good condition, number of schools with computers, library (overall and among high- and low-density districts). I find that the high- and low-density districts differ in terms of main outcomes of interest which are enrolment, schools, classrooms and teachers per ten thousand population, with the differences in means all being significant. The averages are higher for the high-density districts compared to the low-density

¹For the remaining of the paper, I simply use the term ‘outcome per population’ for each outcome.

districts. Similarly for quality indicators like the proportion of qualified teachers, number of classrooms in good condition, number of schools with computers and library, the high-density districts again have higher averages compared to the low-density districts.

Table 2.1: Summary Statistics by Treatment Status - Pre-reform period

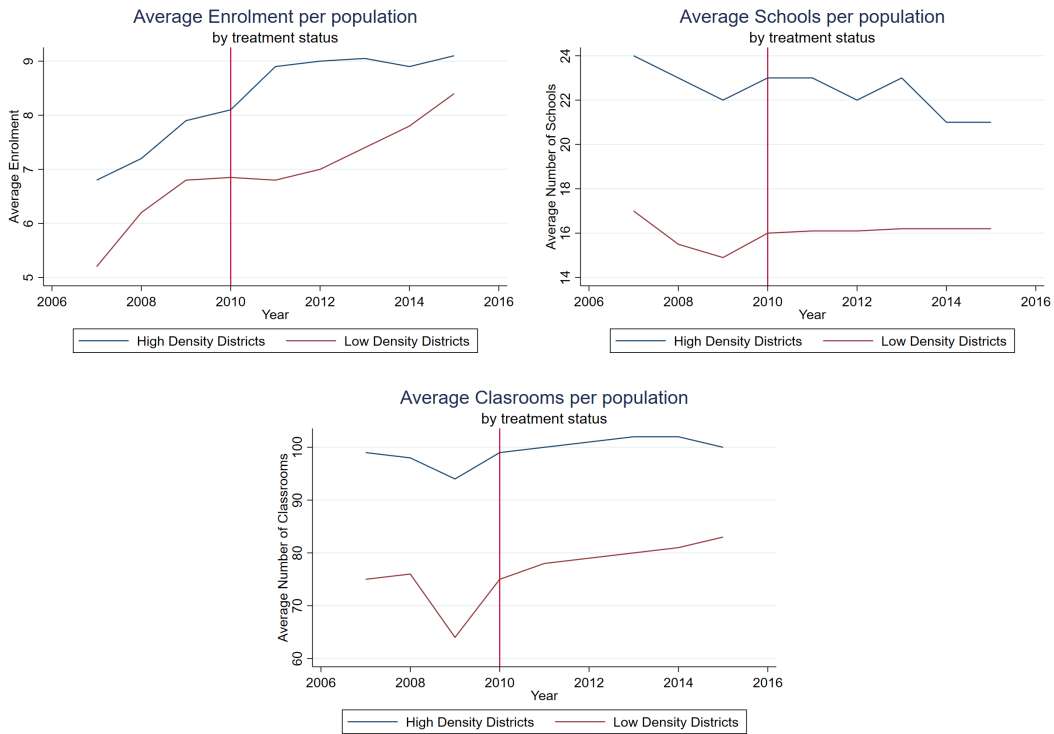
Variables	(1) Overall Mean	(2) High-density districts	(3) Low-density districts	(4) Difference (2)-(3)
Enrolment per hundred population	77.2	82.3	69.2	13.1***
Schools per ten thousand population	29.82	33.02	26.18	6.84***
Classrooms per ten thousand population	184.79	197.00	172.58	24.42***
Proportion of Urban Districts	32.30	36.18	28.64	7.54***
Teachers per ten thousand population	186.64	198.20	174.52	23.68***
Percentage of Qualified Teachers	66.28	71.18	61.86	9.32***
Percentage of Classrooms in good condition	23.21	28.60	17.82	10.78***
Percentage of Schools with Computers	7.84	10.28	5.40	4.88***
No. of Districts	580	290	290	

Source: School report card data 2007-2010 (DISE).

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Importantly, for the validity of the DiD strategy, I need to establish that the evolution of the outcomes of interest was comparable across treated and control groups before the introduction of the reform (Parallel-trend assumption). Figure 2.1 provides descriptive evidence that this hypothesis holds in this context, while in the next section, I formally test the parallel trend assumption by estimating a series of event studies. In the follow-

Figure 2.1: Evolution of Enrolment, Schools and Classrooms per Population by treatment status (2007-2015)



Source: School report card data 2007-2015 (DISE)

ing figures I see graphically how the trends in these main outcomes of interest evolve over time for the high- and low-density districts.

From the three plots in figure 2.1, both school enrolment, number of schools and number of classroom seem to follow a comparable evolution in treated and control districts before the reform. Post 2010, there seems to be some level of convergence towards regional equality in terms of enrolment per population and classrooms per population between the high- and low-density districts, with the low-density district appearing to catch up with the high-density districts. For schools per population, the gap between the treatment and control group seems to narrow as well, although the increase in number of schools per population seems to be very small for the treatment group on average.

This suggests that classrooms in existing schools were being expanded rather than necessarily opening new schools to accommodate the increase in enrolment.

I further discuss how the outcome variables are affected due to the reform using various specifications explained in the following section. Along with the main outcomes of interest, I also look at the various quality indicators and perform regression analysis by age group and gender.

2.4 Identification Strategy

To conduct my analysis, I adopt a difference-in-differences strategy that compares the evolution of the outcome variables across low- and high-density districts. The regression model includes district fixed effects (FE) and state by year fixed effects as follows:

$$Y_{ijt} = \beta_0 + \beta_1.Reform_t * LowDensity_{ij} + Z_{ij} + \gamma_{jt} + \epsilon_{ijt} \quad (1)$$

where Y_{ijt} refers to the outcome variable (Enrolment per population, Schools per population, Classrooms per population, and Teachers per population), for district i in state j in year t , $Reform_t$ is a dummy variable, which switches to one in the post reform years. $Density_i$ is a dummy variable equal to one for low-density districts, $Reform_t * Density_i$ is the interaction term of the reform and density dummy, Z_{ij} includes the district fixed effects, γ_{jt} includes state-by-year fixed effects, and ϵ_{ijt} is the error term. The main coefficient of interest is β_1 that, under the validity of the identification strategy, should capture the impact of the reform. The standard errors are clustered at the district level to take into account that observations within the same cluster may be correlated.

The validity of my identification strategy relies on the assumptions of parallel pre-trends for the low- and high-density districts and that the timing of the roll out does not coincide with other events that happen at the same time as the reform and affect the high- and low-density districts differently. To the best of my knowledge, there was no other major reform introduced at the state or national level that could affect enrolment differently for the low- and high-density districts during the period of this study, that is 2007-15.

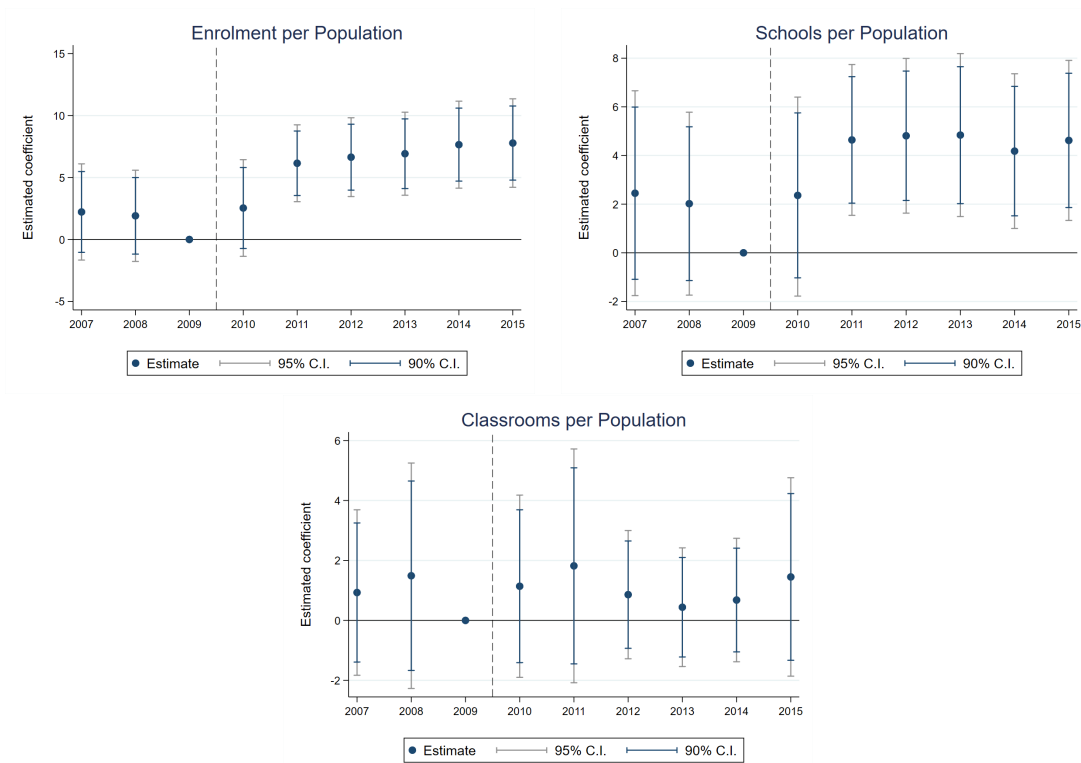
2.5 Results

In this section, I first present event-study exercises and then the average effects of the reform. In the next subsection, I perform the analysis by age and gender to study inequalities between males and females as well. The last subsection analyses the various school quality indicators like schools with computers, library, classrooms in good condition, teachers with professional qualification and pupil-teacher ratio.

2.5.1 Event-Study Results

In Figure 2.2, I present event-studies to explore the dynamic effects of the reform and test for pre-trends. Identification in the DiD estimation technique is based on the assumption of parallel pre-trends, which requires that the treatment and comparison groups exhibit no significant difference in evolution of the outcome of interest in the period before the policy is introduced. Testing this assumption seems especially important in this context, as the treatment group has not been randomly assigned. Here, 2009 is chosen as the reference year, which is one year prior to the enforcement of the JJA. The event studies presented in Figure 2.2 and Table 2.10 in the appendix are obtained from a regression specification similar to equation (1), but instead of a Post

Figure 2.2: Event Studies for Main Outcomes



Source: School report card data 2007-2015 (DISE)

indicator, now each year dummy (except for the reference year of 2009) is interacted with the treatment group dummy. The insignificant pre-policy year (2007 and 2008) coefficients suggest that the low- and high-density districts did not have a significantly different evolution of enrolment, school or classroom per population before the introduction of the reform and thus, satisfy the assumption of parallel pre-trends.

The event studies in figure 2.2 offer support for the parallel trends assumption due to insignificant estimates in the pre-reform years (2007 and 2008) for all outcomes. In the post-reform years, I find that the low-density districts have higher enrolment per population than the high-density districts and the effect seems to get stronger over years. I find similar results for schools per population, with the effect getting stronger

from 2010 to 2011 and remaining stable thereafter. Classrooms per population do not seem to be significantly affected over the years following introduction of the reform for the low-density districts compared to the high-density ones.

2.5.2 Main Outcomes

Table 2.2 presents the average effects of the reform on enrollment. Columns 1 and 2 in table 2.2 differ in terms of adding the state-by-year fixed effects. In column 3, the outcome is log of enrolment per population. Specification in column 2 with both district and state-by-year FE is my preferred specification for this study. Running alternate specifications helps understand how the results vary under different assumptions, control variables or fixed effects and reveals which variables consistently matter, thus offering robustness to the findings. In table 2.3, I present results for schools, classrooms and teachers per population using the my preferred specification.

Table 2.2: Impact on Enrolment

Explanatory Variable	Enrolment per Population (1)	Enrolment per Population (2)	log(Enrolment) per Population (3)
Low-density*post reform	7.74*** (1.27)	7.24*** (1.19)	0.048*** (0.03)
Observations	5220	5220	5220
R squared	0.554	0.602	0.312
State by Year FE		✓	✓
District FE	✓	✓	✓
Overall Mean	80.24	80.24	4.39

Source: School report card data 2007-2015 (DISE).

Notes: Standard errors in parentheses, clustered at district level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

From table 2.2, I find that the reform significantly increased enrolment rates in low-

density districts compared to high-density ones. On average, enrolment per population was up by about 4.8 percent relative to the pre-reform mean for the low-density districts after the reform (Specification 3). In other words, the reform appears to narrow the enrolment gaps between the low- and high-density districts within-state.

Table 2.3: Impact on Schools, Classrooms and Teachers

Explanatory Variable	Schools per Population (1)	Classrooms per Population (2)	Teachers per Population (3)
Low-density*post reform	4.71*** (0.89)	0.79 (1.25)	6.71*** (2.18)
Observations	5220	5220	5220
R squared	0.644	0.952	0.432
State by Year FE	✓	✓	✓
District FE	✓	✓	✓
Overall Mean	33.56	189.72	192.44

Source: School report card data 2007-2015 (DISE).

Notes: Standard errors in parentheses, clustered at district level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2.3 shows that the reform also leads to a significant increase in number of schools and teachers per population on average in treated districts compared to control ones after the reform. However, it does not seem to affect the number of classroom per pupil. Taken together, these results suggest that the schools have shrunk in terms of number of classrooms per school. This seems to be in line with the aim of the reform which to reduce the distance students need to travel from their home to reach the school. More schools being built could imply close proximity to neighbourhoods while the classroom numbers remain nearly the same on average. Alternatively, it is possible that some schools started running separate classes, like morning and even classes, for boys and girls. That would imply more students being accommodated in a lower number of

classrooms while not necessarily increasing the class size.

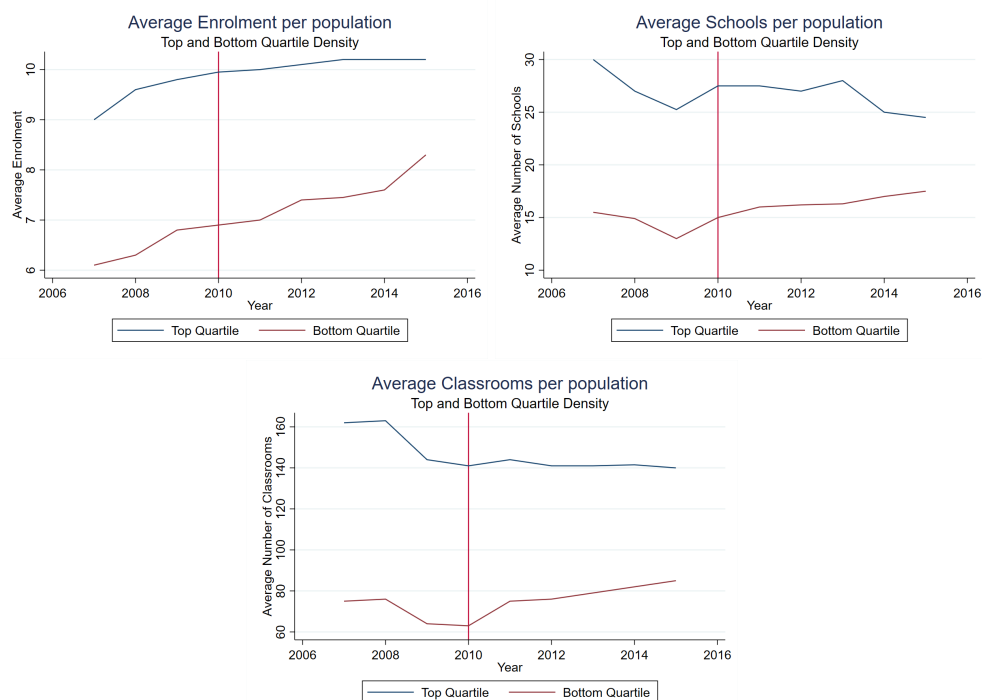
2.5.3 Robustness Checks

The results so far show that the reform has led to some closure of the pre-reform gap between the low- and high-density districts in terms of the main outcomes: primary enrolment, number of schools and classrooms per population. As a validation of these results, I perform a robustness check by varying the treatment and control group using school density quartiles. As the low-density districts, having lower pre-reform supply of schools were more affected by the reform than the high-density districts with relatively higher pre-reform supply, I compare the bottom quartile density districts with the top quartile density districts as they are the ones with a stronger difference in pre-reform supply of the main outcomes of interest.

Figure 2.3 shows that the outcome variables seem to evolve parallelly from 2007 to 2010 (pre-trends) and after 2010, seem to be converging to some extent towards regional equality between the bottom and top quartile density districts where the bottom quartile density districts seem to be catching up with the top quartile density districts.

Table 2.4 presents regression results for the above described specification where it can be seen that the results are very similar to the ones for the main analysis where I used the median to define low- and high-density districts. Thus, this analysis is robust to the definition of treatment groups.

Figure 2.3: Evolution of Enrolment, Schools and Classrooms per Population by Quartile Density (2007-2015)



Source: School report card data 2007-2015 (DISE)

Table 2.4: Impact on Enrolment (Varying the Treatment Group)

Explanatory Variable	Enrolment per Population (1)	Schools per Population (2)	Classrooms per Population (3)
Bottom Quartile*post reform	6.82*** (1.45)	5.62*** (1.78)	0.60 (1.40)
Observations	2760	2760	2760
R squared	0.918	0.627	0.952
State by Year	✓	✓	✓
District FE	✓	✓	✓

Source: School report card data 2007-2015 (DISE).

Notes: Standard errors in parentheses, clustered at district level.

*** p<0.01, ** p<0.05, * p<0.1.

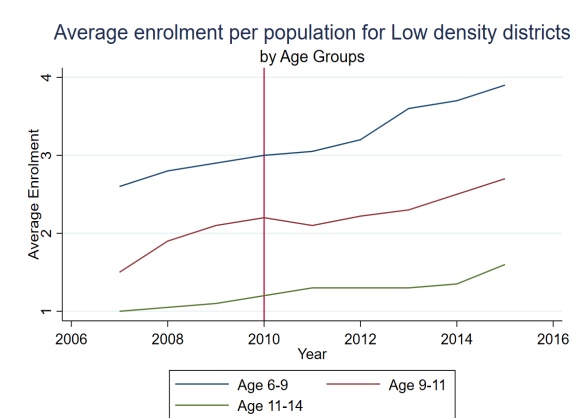
2.5.4 Enrolment Analysis by Age groups and Gender

The RTE mandates free and compulsory primary schooling. However, the parents are not obliged to necessarily send their kids to school as part of this reform and rather have them work in agriculture/other family business. A significant number of children in the age group of 6-14 are engaged in agricultural labor in rural India (Weiner, 2021) with majority being in the 6-11 year age group followed by 12-14 year olds. Thus, it seems important to study whether the reform had heterogeneous effects across age groups.

Further, according to the Global Education Monitoring (GEM) report (Unesco, 2018), primary education attainment has important implications especially for females, as it is linked with lower maternal deaths as a result of reduced gender inequality. As previously discussed, studies in China and Uganda found an increase in primary school enrolment for girls as a result of free/reduced fee educational policies, while a study in Kenya saw greater primary school completion rates for boys, leading to increased gender gap for primary school completion. According to a World Bank report on disparities in girls' education, gender gaps are the largest in low-income countries (Kattan and Khan, 2023) and girls are more likely to be deprived of opportunities including employment than boys in countries in Africa, South Asia and the Middle-East. My result show that the RTE increases overall primary school enrollment and it is thus important to understand whether this effect differs across genders.

To investigate heterogeneous effects across age groups, I look at three groups: lower primary (Ages 6-9), middle primary (Ages 9-11) and upper primary (Ages 11-14), using my preferred specification.

Figure 2.4: Average Enrolment per Population for Low-Density Districts by Age groups



*Source: School report card data 2007-2015 (DISE)

Figure 2.4 plots average enrolment per population for the low-density districts by age groups. I find that all the three age groups witness an upward trend after the reform from 2010 onwards, although the two youngest groups (6-9 and 9-11) have a relatively sharper increase compared to the 11-14 age group with the sharpest increase being observed among the youngest age group (6-9). This seems to be in line with my hypothesis. As the primary school entering age is 6 years, the youngest age group, which has to make a decision about entering primary school witnesses the most increase once the reform kicked in, while the upper primary age group (11-14) is not affected much as they probably already made their schooling decisions regardless of the reform.

From table 2.5, I find that all the three age groups saw an increase in the total enrolment per population in the low-density districts after the reform and the 6-9 age group seems to have the highest increase (about 10 percent higher than overall mean) as seen from the coefficient of the interaction term in regression (1). All these results are significant at the 1 percent level. The reform mandated enrolment for the 6-14 year olds (those in primary schooling) and the results suggest that the youngest group saw the

Table 2.5: Impact on Enrolment per Population by Age Groups

Explanatory Variable	Ages 6-9 Population (1)	Ages 9-11 Population (2)	Ages 11-14 Population (3)
Low-density*post reform	3.7** (0.42)	1.9** (0.28)	1.7*** (0.25)
Observations	5220	5220	5220
R squared	0.618	0.611	0.689
State by Year FE	✓	✓	✓
District FE	✓	✓	✓
Overall Mean	34.4	27.6	21.2

Source: School report card data 2007-2015 (DISE).

Notes: Dependent variable is Enrolment per Population.

Standard errors in parentheses, clustered at district level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

highest increase in enrolment per population. This could be true since the majority of these older children in the 9-11 and 11-14 year age groups might already be working as child laborers, while the parents of the younger ones, especially at the age of 6, faced a decision between sending the kids to school or have them work as laborers.

From the regression analysis in table 2.6 for the total enrolment per population by gender, I establish that the enrolment per population in the low-density districts for both the genders seems to go up after the reform compared to the high-density districts, but the increase was stronger for boys than girls (about 10 percent and 7 percent of the overall mean respectively). These differences are statistically significant at the 5 percent level. This finding suggests that the pre-reform enrolment gap (enrolment being higher for boys) between boys and girls exacerbates after the reform. This could be due to the lower willingness of households (especially in rural India) to send the girl child to school despite the schooling being free. Thus, although both genders witness increase

in enrolment, the magnitude of effects makes the existing enrolment inequality worse after the reform.

Table 2.6: Impact on Enrolment by Gender

Explanatory Variable	Boys (1)	Girls (2)
Low-density*post reform	4.7*** (1.59)	2.5*** (0.78)
Observations	5220	5220
R squared	0.82	0.68
State by Year FE	✓	✓
District FE	✓	✓
Overall Mean	46.35	33.89

Source: School report card data 2007-2015 (DISE).

Notes: Standard errors in parentheses, clustered at district level.

The above regressions are run separately for boys and girls. The pre-reform difference in average enrolment per population between boys and girls in low-density districts is 11.84 units.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2.5.5 Effect on School Quality Indicators

Despite key programs targeted at increasing primary school enrolment across developing countries, the rates remain low due to poor infrastructure, teacher shortages, sanitation issues (Birdsall, Levine and Ibrahim, 2005). Further, with increase in enrolment, there seems to be low level of student achievement and poor learning outcomes due to low skilled teachers (Ganimian and Murnane, 2016; Barrera-Osorio et.al., 2007). With an increase in primary enrolment, staff members become overworked, thereby af-

fecting quality of teaching (Muthaa and Mwirigi, 2015). Further, school and teaching quality positively influences children’s primary school progress. Students assigned to high quality teachers are more likely to attend college and earn higher salaries (Chetty et.al., 2014). Teachers also positively influence absences, suspensions, grade repetition and high school completion (Jackson, 2018). Thus, it is important to understand if the RTE was able to improve not just enrolment, but also ensure quality of education through improvements in school quality indicators.

Table 2.7 presents results for the analysis of the effect of reform on standard quality indicators like number of classrooms in good condition, schools with computers, library, and table 2.8 studies the effect on number of professionally qualified teachers and the pupil-teacher ratio.

Table 2.7: Impact on Schools Quality Indicators

Explanatory Variable	Classrooms in good condition (1)	Schools with Computers (2)	Schools with Library (3)
Low-density*post reform	2421.61** (338.36)	192.77** (39.47)	190.11*** (42.22)
Observations	5220	5220	5220
R squared	0.644	0.678	0.708
State by Year FE	✓	✓	✓
District FE	✓	✓	✓
Overall Mean	37124.48	2088.96	1864.22

Source: School report card data 2007-2015 (DISE).

Notes: Standard errors in parentheses, clustered at district level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.8: Impact on Number of Qualified Teachers and Pupil-Teacher Ratio

Explanatory Variable	No. of Qualified Teachers (1)	Pupil-Teacher Ratio (2)
Low-density*post reform	186.41*** (37.19)	-8.08*** (0.22)
Observations	5220	5220
R squared	0.715	0.628
State by Year FE	✓	✓
District FE	✓	✓
Overall Mean	1642.28	38.24

Source: School report card data 2007-2015 (DISE).

Notes: Standard errors in parentheses, clustered at district level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

These results suggest that all the quality indicators have improved in the low-density districts after the reform compared to the high-density districts. Compared to the pre-reform mean, low-density districts, seem to have about 10 percent more schools with computers, about 12 percent more schools with library, about 8 percent more good condition classrooms, about 12 percent more qualified teachers, and about 23 percent lower pupil-teacher ratios after the reform.

2.5.6 Heterogeneous Effect based on State-level characteristics

The analysis so far, controlling for state-by-year FE, has exploited within-state differences between low- and high-density districts. In this section, I bring in some state-level characteristics such as poverty rate, share in agriculture, and state-GDP to check whether the reform effects differ between states according to these characteristics.

Socio-economic factors like lack of financial support from government are negatively

associated with enrolment and retention of pupils (Namukwya and Kibirige, 2014). The RTE reform aims at free primary schooling along with provision for free textbooks, thereby offering relief to families from the financial burden of sending children to school. Similarly, engagement in domestic work (child labor) is also associated with lower enrolment in primary school (Namukwya and Kibirige, 2014). On the one hand, households in high-poverty states would witness greater increase in enrolment due to the reform by overcoming issues of poverty through free schooling. On the other hand, if child labor is higher in poor states, this could hinder the effect of the reform on enrollment. Further, states where relatively fewer households are engaged in agriculture maybe more likely to witness an increase in enrolment due to less dependence on children for employment in farms. On the other hand, states with a higher share of children in agriculture could have greater scope of increase in enrolment, so it is a priori ambiguous what we should expect along this dimension of heterogeneity. Further, states with higher GDP per capita are likely to have more resources for education expenditure and improve more school quality after the reform, with positive knock-on effects on enrollment.

To conduct the heterogeneity analysis, I create a dummy variable ($Factor_j$) to indicate whether a state is above or below the median for the respective measure. I then augment my preferred baseline specification with interaction terms of this dummy with the reform and treatment dummy as follows:

$$Y_{ijt} = \beta_0 + \beta_1.Reform_t * LowDensity_{ij} + \beta_2.Reform_t * Factor_j + \beta_3.LowDensity_{ij} * Factor_j + \beta_4.Reform_t * LowDensity_{ij} * Factor_j + Z_j + \gamma_t + \epsilon_{ijt} \quad (3)$$

The main coefficient of interest is β_4 which provides evidence whether the reform effect differs by state-level characteristics.

Table 2.9: Heterogeneous Effect on Enrolment per Population by State Characteristics

Explanatory Variable	High Poverty (1)	High State-GDP (2)	Low Share in Agriculture (3)
Low-Density*post-reform	3.36 (2.20)	4.05 (2.95)	3.66 (3.01)
Low-Density*post-reform*factor	3.78*** (0.48)	3.11 (2.63)	3.58 (2.24)
Observations	5220	5220	5220
R squared	0.716	0.428	0.776
State-by-year FE	✓	✓	✓
District FE	✓	✓	✓
Overall mean	80.24	80.24	80.24

Source: School report card data 2007-2015 (DISE).

Notes: Dependent variable is Enrolment per Population.

Standard errors in parentheses, clustered at district level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In table 2.9, I find that the reform effect in low-density districts is stronger in states with high-poverty while such districts do not react differently between states on the basis of GDP or share in agriculture. Since the states also received funding from the national level to comply with RTE as required, the level of GDP becomes an insignificant factor in this analysis. Thus, this study finds a positive effect on enrolment not only in low-density districts within-states but also in the poorest states.

2.6 Discussion and Conclusion

Understanding the importance of primary schooling in the early development of children, the government of India introduced a vital education reform (RTE) aimed at 6-14 year-olds, making primary education free and compulsory. This study aims to establish the casual link between the Right to Education Act of 2009, which was enacted by the Constitution in 2010, and enrolment at primary school level thereby offering evidence on the effectiveness of this policy in achieving higher enrolment, more number

of schools and classrooms, and improving quality of education through hiring more professionally qualified teachers, reducing pupil-teacher ratios, and better infrastructure for children.

As seen from the data, some districts saw greater increase in total number of schools, classrooms, total enrolment, number of teachers than others, while some districts saw less increase. From the difference in difference regression results, it is established that enrolment and number of schools seemed to have convergence between the treatment and the control group through the effect on low-density districts which were more likely to see an increase in these outcomes. This seems to be in line with the expectations that would suggest some form of compliance with the education reform.

Analysis by gender revealed that increase in enrolment per population for boys is higher than that for the girls in low-density districts after the reform, exacerbating the pre-reform gender differentials in enrolment. Further, the youngest group of 6-9 year olds saw greatest increase in the enrolment for low-density districts after the reform suggesting that the ones already above the minimum age for entering primary school (age groups 9-11 and 11-14) did not react much to the reform and were more likely to have already made their decision on schooling. Overall, the reform does seem to have a significant impact on the overall enrolment and number of schools after the reform for low-density districts. Further, there seems to be a significant positive effect on the quality indicators, implying better classrooms, more schools with computers and library, more professionally qualified teachers and lower pupil-teacher ratios on average after the reform.

The study has important policy implications. Quality schooling is linked to higher re-

turns to additional years of schooling (Card and Kreuger, 1992) and better labor market outcomes (Elango et.al, 2016). Thus, policies like the RTE, if implemented effectively, could help in the growth and development of the country through above discussed channels of labor market opportunities. Further, this study has found that the gender enrolment gap has worsened after the reform and thus, points at important results for the policymakers to further focus on targeting the female enrolment in particular.

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2.8 Appendix

Table 2.10: Dynamic Effects of the RTE 2010, on Enrolment, Schools and Classrooms per Population

Explanatory Variable	Enrolment per Population (1)	Schools per Population (2)	Classrooms per Population (3)
Year 2007*low density	2.22 (1.98)	2.45 (2.15)	0.93 (1.41)
Year 2008*low density	1.91 (1.88)	2.02 (1.92)	1.49 (1.92)
Year 2010*low density	2.54 (1.99)	2.36 (2.06)	1.14 (1.55)
Year 2011*low density	6.15*** (1.58)	4.64** (1.61)	1.82 (1.99)
Year 2012*low density	6.64*** (1.62)	4.81*** (1.67)	0.86 (1.09)
Year 2013*low density	6.92*** (1.71)	4.84*** (1.69)	0.44 (1.01)
Year 2014*low density	7.65*** (1.79)	4.18*** (1.62)	0.68 (1.05)
Year 2015*low density	7.78*** (1.82)	4.62*** (1.68)	1.45 (1.69)
Observations	5220	5220	5220
R squared	0.280	0.364	0.244

Source: School report card data 2007-2015 (DISE).

Notes: Standard errors in parentheses, clustered at district level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3 Chapter 3: School Exclusions and Youth Crime: Evidence from the UK

3.1 Introduction

Violent crimes by juveniles involving knives and other weapons have been rising in the UK. As per the Youth Justice Statistics for 2022-23, these offences resulting in a caution or conviction have increased by 23 percent in the last 10 years from about 2750 up to 3400. Further, the average custodial sentence length in months in England for violent crimes committed by juveniles has gone up by 7 months, from 13.5 months in 2013 to 20.5 months in 2023. Although the Youth Justice Board (YJB) data suggests a fall in overall offending during the covid-19 pandemic, the juvenile crime rate has been increasing after the end of 'stay-at-home' orders to help fight coronavirus in the UK by minimising socialising. Many economic outcomes are adversely affected later in life for these juvenile offenders. Offending in childhood is associated with higher likelihood of offending in early adulthood both for violent crimes (Bayer et.al., 2009; Aizer and Doyle, 2015) and drug offences (Eren and Mocan, 2021). Further, offences such as possession of weapons and drug dealing have the highest probability of persistence into adulthood (Rosenfeld et.al., 2012). Youth crime could also imply significant economic costs on the society due to increased costs of policing, maintaining juvenile criminal justice system. It could also lead to substantial costs on the victim including medical costs and damage to property. Further, the government spending is also likely

to provide additional support for affected families and other social services. Thus, because of the deleterious consequences of crime, it becomes critical to understand the various causes of youth crime and develop appropriate policies to prevent them.

Several factors could affect youth crime such as poverty (McAra and McVie, 2016), unemployment (Gronqvist, 2011) along with other factors like inequality, and poor mental health. Additionally, young people could be driven into crime due to easy access to weapons, peer pressure, substance abuse, lower literacy levels. Access to illegal labor markets also increases the probability of children becoming criminals (Sviatschi, 2022). School exclusions could be one of the risk factors affecting youth crime. Theoretically, the effect of school exclusions on criminal activity is ambiguous. On the one hand, exclusion policies could stigmatize excluded kids and expose them to criminal activity if excluded pupils end up being placed in alternative provision schools with other disruptive peers. It can also reduce young individuals' perception of their chances of long-term educational and economic success, thus increasing their perceived returns of illicit activities relative to legal activities. On the other hand, such policies could act as deterrent and limit negative spillovers of misbehaviour onto other students. This issue is of wider concern in other countries too, especially in the US (Kupchik et.al., 2015). However, there is no clear evidence that school exclusions have a causal effect on youth crime. Pirrie et.al. (2011) study a small group of permanently excluded kids on the basis of qualitative interviews and suggest negative outcomes including prolonged periods out of education; poor physical and mental health; and involvement in crime. Hicks et.al. (2024) find that students assigned to schools with stricter discipline after a sudden boundary change in Charlotte-Mecklenburg schools in the US, were more likely to have ever been arrested as adults. However, it is unclear if the stricter discipline caused more students to engage in crime as youth which could be a

mechanism for translating into adult incarceration. In this paper, I aim to contribute to this scant literature by offering causal evidence on the effect of school fixed term and permanent exclusions of young individuals aged 12 to 16 in English schools on their probability to engage in criminal activity immediately after experiencing an exclusion. Further, the rich dataset will allow me to study whether engaging in crime as youth translates to adult incarceration.

In the UK, children can be excluded from school on a temporary or permanent basis due to misbehaviour or any form of disruptive behaviour in or outside school. The temporary or fixed term exclusion is up to 45 school days in one school year while the permanent one implies that pupils are no longer allowed to attend the school that they are excluded from. Both these types of school exclusions have been on a rising trend since 2013 (DfE, 2019). Moreover, the rate of school exclusions in the UK is about 10 times more than that of any other European country (Kupchik et.al., 2015). I use unique administrative data from England which link the school records of all students in English state schools with all criminal offences that these students have been cautioned or convicted with. With this rich data, I evaluate the effects of the school exclusion policies by estimating a difference-in-differences model that compares crime committed by kids who are excluded in one term versus the kids who have not yet experienced an exclusion but will experience one within the following two terms.

My results show that the probability of committing an offence significantly increases during the week a pupil is excluded and in the week after. However, the exclusion is also preceded by a surge in crime in the 2-3 weeks prior to this event. Therefore it seems to suggest that crime drives exclusions, rather than vice versa. Further, I find that the exclusions do not exacerbate the pre-existing increase in number of offences.

This suggests that school exclusions could have an ambiguous effect on youth crime.

This study contributes to the literature on disciplinary policies and crime. Disruptive kids could be dealt with leniency or strict disciplinary policies such as school exclusions. Teachers who reduce suspensions and improve attendance have been associated with substantially reducing recidivism (Rose et.al., 2022), while strict school principals in the US, generate more juvenile justice complaints (Sorensen et.al., 2022). Hicks et.al. (2024) find that students assigned to schools with stricter discipline are more likely to engage in crime as adults. My paper provides evidence on the short term effects of school exclusion (in the weeks following an exclusion) on youth offending in England and explores the dynamics over time.

This paper also contributes to the literature on the effects of education on crime by investigating the relationship between school exclusions and juvenile crime in England. Lower level of schooling has been found to significantly increase the probability of incarceration and arrest (Lochner, 2004). Sabates and Feinstein (2008) find that higher rate of students staying in school reduces crime. Admission to any secondary school has a sizeable negative effect on the propensity to commit crime (Berthelon and Kruger, 2010; Huttunen et.al., 2023). Anderson (2014) finds that policies aimed at keeping the youth in school such as increasing the minimum dropout age reduces both property and violent crime among high-school children. However, Jacob and Lefgren (2003) previously found decrease in property crime during school session days but an increase in violent crime attributing to the incapacitation and concentration effect.

Direct peer interaction is a mechanism through which social multipliers occur in criminal behaviour (Damm and Dustmann 2014; Billings et.al., 2014). Disadvantaged neigh-

bourhood peers (charged for drug possession) influence disadvantaged youths' propensity to commit criminal offences in Copenhagen (Rotger and Galster, 2019). Criminal behaviour is more strongly related to current neighbourhood conditions (Sciandra et.al., 2013). Crime rates of gangs in the neighbourhood are associated with an increase in the likelihood that boys commit crimes before age 19 (Dustmann et.al., 2023). Thus, policies aiming to increase school segregation based on behaviour could increase crime through denser criminal network formation. Alternately, removing criminal leaders from a school could reduce criminal activity and individual's propensity to become a criminal (Diaz et.al., 2021). There is no clear evidence so far to suggest whether exclusion policies help reduce crime or lead to an increase in delinquency for the youth and my research aims to inform this debate. The rich data I use will allow me to explore any heterogeneous effects across dimensions such as gender, age, ethnicity, or type of offence.

There is also a literature on school choice and youth crime. For example, Deming (2011) finds that attending a preferred school is associated with lower arrests for serious crimes and lower incarceration days; Dobbie and Fryer (2015) find a negative effect of being offered admission to high performing charter schools on males' students probability of being incarcerated. With my data I can contribute to this literature by following pupils after a permanent exclusion, document selection patterns by type of institution of destination, and, conditional on this, study heterogeneous effects by type of institution of destination.

This paper has several important policy implications. Youth crime negatively affects labour force participation (Brugard and Falch, 2013). Youth with a criminal record is more likely to face barriers to employment, as employers may be hesitant to hire such

individuals. Conviction at, or before the age of 17 is associated with a lower employment rate, limited occupational choices and lower wage growth rate even after 10 years in the labour market (Golan et.al., 2021). Disruptive education due to criminal activities could in turn affect productivity and employability of those individuals in the long run. Crime could cause psychological problems, diminishing the ability of the individual to secure and maintain employment (Wong and Ramakrishnan, 2017). These poor labour market outcomes can increase recidivism, trapping the individual into a vicious cycle of crime (Golan et.al., 2021). The findings of my analysis will have important implications for the design of effective policies to tackle youth crime, including disciplinary strategies and school organizational practices, a topic of primary public interest.

The remainder of the paper is structured as follows. Section 2 provides information on the school exclusion policies in the UK followed by data used in the analysis in section 3. Section 4 describes our empirical strategy and the results, and Section 5 concludes the paper.

3.2 School Exclusions in the UK

Exclusions in the UK are governed by a combination of the Education Act 2002 and various Education Department regulations. Fixed term exclusions are also referred to as suspensions and are for a specific period of time. A pupil can be suspended for one or more of such periods up to a total of 45 school days in a school year. Such suspensions can also be for parts of the school day such as being excluded from the school premises during lunch time (counted as half a day of exclusion) if a pupil's behaviour is disruptive during that time. In any case, the head teacher is legally required to notify parents. Permanent exclusion from school may happen only in response to a serious

breach of a school's disciplinary policy or to safeguard education and welfare of other pupils, where allowing the pupil to be in school would significantly affect education, other pupils or staff in the school. In such cases, the child gets removed from the school roll. Permanent exclusions could follow multiple fixed term exclusions and other measures taken in school including separate teaching or consultation with parents. It is also legal to exclude a child on grounds of behaviour outside of school as set out by schools' behaviour policy.

For fixed term exclusion that last for longer than 5 days or permanent exclusions, the child is provided access to education by putting in place an alternative provision (AP). These alternative provisions could be attending another mainstream school, a Pupil Referral Unit (PRU) or online tuitions. The PRU's or AP institutions are specifically designed to particular needs of those children who are excluded, by offering more tailored support and small class sizes. They offer more flexible curriculum that can be adapted to the abilities and interests of the students to enable them to re-engage with learning. Support services like mentoring and counselling are also offered in an attempt to resolve underlying issues contributing to the student's exclusion.

In most of the counties across the UK, the Local Authority (LA) is primarily responsible to provide alternative education to the excluded children, while in some counties, the schools themselves are assigned this responsibility. The LA allocates funding for AP to the schools which then self-govern the best ways to fulfil the requirements of the excluded children. In the event of a school not being able to provide alternative education on time, or the AP offered is below the desired standard, the LA could themselves put relevant provisions in place by reclaiming funding from the schools. The time spent finding a suitable PRU and navigating through the waiting lists can disrupt academic

progress, adjusting to home-schooling or online tutoring. Children facing such challenges could become isolated from the dynamics of a classroom, potentially worsening educational aspects.

3.3 Data

This paper uses pupil-level data from England and Wales, provided jointly by the Department for Education (DfE) and the Ministry of Justice (MoJ). It comprises administrative records of the full population of English pupils in state schools and is then matched to offenders who have been cautioned or sentenced for any offence recorded on the Police National Computer (PNC). Although the data measures cautions and convictions, I will refer to this as a shortcut as “committed crime” in the remainder of this paper. I focus on birth cohorts of pupils born between 1st September 1995 and 31st August 1996 up to those who are born between 1st September 2003 and 31st August 2004 and follow each of these target cohorts from grade 7 up until grade 11. For each pupil included in this sample, I use the Spring Census data which provides measure of pupils’ characteristics such as age, gender, ethnicity, eligibility for free-school meal (FSM), and having emotional/behavioral issues.

I also use fixed-term, or permanent exclusion data for the target cohorts along with criminal records. The former are key to identify the treatment, whether a pupil has been excluded, and its timing. It allows me to follow the same pupil over time and therefore compare their propensity to commit a crime before and after experiencing an exclusion. In the next subsection, I provide summary statistics for the outcomes of interest and introduce the identification strategy.

3.3.1 Descriptive Statistics

In this subsection, I present a descriptive analysis of the data with the aim of guiding the reader to the identification strategy chosen for the empirical analysis. I start with the proportion of exclusions and criminal offences among youth in table 3.1 to investigate the correlation of crime¹ and exclusions with socio-demographic characteristics.

From panel A, I find that boys are over two times more likely than girls to be ever excluded, and over than three times more likely to ever commit crime. 6.9% of boys and 3% of girls got ever excluded during grades 7-11. 1.11% of boys and 0.35% of girls in these age groups ever got convicted with crime. These small numbers suggest that crime and exclusions are relatively rare events. Further in panel B, I find that kids eligible for free school meals are about three times more likely to be excluded (10.70%), and about four times more likely to ever commit crime (1.91%), than the ineligible kids (3.94% and 0.52% respectively). In panel C, I find that blacks and mix ethnicity have a higher likelihood of being ever excluded and ever committing crime compared to whites, asians and other ethnicity.

In table 3.2, I present pupil characteristics by status of exclusion. Ever excluded kids seem to be older, more likely to be male, eligible for free-school meals, and more likely to have emotional/behavioural issues than the never excluded kids. These differences are all statistically significant, and many of them are large. For example, free school eligibility is 14.75% for never excluded pupils, and 33.56% for excluded pupils. Further, black pupils are significantly more likely to ever experienced an exclusion. Mean overall crime also seems to be significantly higher for the ever excluded kids than the

¹Throughout the paper, I simply use 'crime' to refer to being cautioned or convicted.

Table 3.1: Proportion of Exclusions and Criminal offences

Panel A: By Gender				
	Boys	Girls	Difference	
	(1)	(2)	(1)-(2)	
	(1)	(2)	(3)	
% Ever excluded	6.91	3.01	3.90***	
% Ever committed crime	1.11	0.35	0.76***	
Observations	12,833,226	12,242,321		

Panel B: By FSM Eligibility				
	FSM-Yes	FSM-No	Difference	
	(1)	(2)	(1)-(2)	
	(1)	(2)	(3)	
% Ever excluded	10.70	3.94	6.76***	
% Ever committed crime	1.91	0.52	1.39***	
Observations	3,934,899	21,140,648		

Panel C: By Ethnicity					
	White	Asian	Black	Mix	Other
	(1)	(2)	(3)	(4)	(5)
% Ever excluded	4.88	3.34	8.10	7.23	4.96
% Ever committed crime	0.75	0.33	1.05	1.20	0.67
Observations	19,674,862	2,302,445	1,266,590	1,079,807	751,843

Source: DfE-MoJ share data 2007-2020.

*** p<0.01, ** p<0.05, * p<0.1.

Table 3.2: Pupil characteristics by Exclusion Status

Variables	Never Excluded	Ever Excluded	Difference (2)-(1)
	(1)	(2)	(3)
Mean Number of offences	0.03	0.52	0.49***
Mean Age	13.40	13.68	0.28***
% Male	50.15	70.68	20.53***
% White	78.57	76.50	-2.07***
% Asian	9.34	6.12	-3.22***
% Black	4.89	8.18	2.39***
% Mix ethnicity	4.20	6.23	2.03***
% Other	3.00	2.97	-0.03***
% Free Meal	14.75	33.56	18.81***
% Emotional Issue	4.19	22.09	17.90***
Observations	23,821,243	1,254,304	

Source: DfE-MoJ share data 2007-2020.

*** p<0.01, ** p<0.05, * p<0.1.

never excluded ones, showing a strong correlation between exclusions and crime.

In panel A of table 3.3, I present summary statistics socio-demographic characteristics by crime status. Additionally in panel B, I condition on having at least one exclusion, to motivate the identification strategy.

I find that the kids who ever committed crime seem to be older, more likely to be male, eligible for free-school meal, and more likely to have emotional/behavioural issues on average, than the kids who never committed crime. Black pupils are significantly more likely to ever commit crime. Further, these differences are all statistically significant and again large. For free school meal, the difference is now even larger than it was above for the exclusions in table 2. Conditioning on having at least one exclusion, the correlation of crime with socio-demographic factors gets weaker. For example, for free school meal eligibility, the difference drops by almost two thirds to 9 percentage points and for emotional issues, the difference drops by over one third to 18.53 percentage

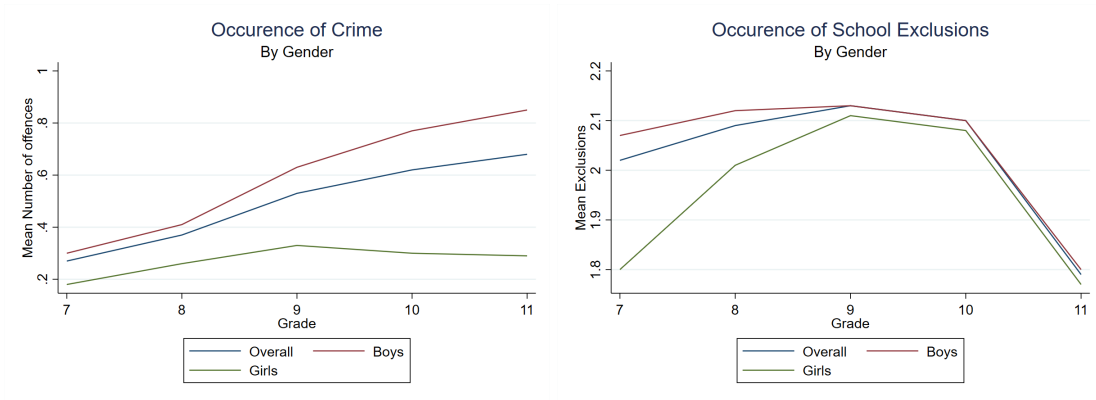
Table 3.3: Pupil Characteristics by Crime Status

Panel A: Unconditional			
Variables	Never Committed Crime (1)	Ever Committed Crime (2)	Difference (2)-(1) (3)
Mean Age	13.42	13.93	0.51***
% Male	50.99	76.90	25.91***
% White	78.46	79.02	0.56***
% Asian	9.22	4.07	-5.15***
% Black	5.04	7.21	2.17***
% Mix ethnicity	4.28	6.99	2.71***
% Other	3.00	2.71	-0.29***
% Free Meal	15.51	40.61	25.10***
% Emotional Issue	4.87	34.76	29.89***
Observations	24,890,334	185,213	
Panel B: Conditional on ever excluded			
Variables	Never Committed Crime (1)	Ever Committed Crime (2)	Difference (2)-(1) (3)
Mean Age	13.67	13.84	0.17***
% Male	70.00	79.38	9.38***
% White	76.37	78.19	1.82***
% Asian	6.30	3.96	-2.34***
% Black	7.56	8.22	0.66***
% Mix ethnicity	6.12	7.53	1.41***
% Other	2.99	2.77	-0.22***
% Free Meal	32.90	41.90	9.00***
% Emotional Issue	20.72	39.25	18.53***
Observations	1,161,947	92,357	

Source: DfE-MoJ share data 2007-2020.

*** p<0.01, ** p<0.05, * p<0.1.

Figure 3.1: Occurrence of number of offences and school exclusions by gender

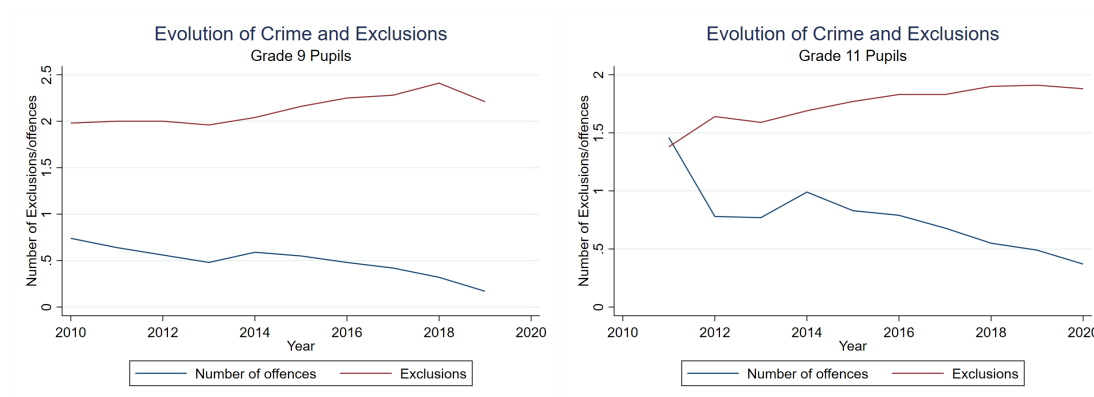


Source: DfE-MoJ share data 2007-2020.

points.

In figure 3.1, I plot the occurrence of exclusions and number of offences for ever excluded pupils across grades in the left and right panels respectively, for the overall sample, boys, and for girls. These plots suggest that total crime on average seems to be higher for higher school grades with the increase mainly driven by boys. Total exclusions seem to increase slightly across grades on average, with a significant drop in grade 11. The evolution of mean fixed and permanent exclusions (overall and by gender) are provided in appendix tables 3.4-3.6.

Figure 3.2: Evolution of School Exclusions and Number of offences



Source: DfE-MoJ share data 2007-2020.

Next, I plot the evolution of crime and exclusions for the grade with highest mean exclusions (Grade 9) and highest mean number of offences (Grade 11) in the left and right panel of figure 3.2 respectively. The graphs show that both grades witness an increase in number of exclusions over time, while the number of offences seem to decrease.

3.4 Event-Study Analysis

To study the relationship between school exclusions and youth crime, I compare pupils who have their first exclusion in the autumn term (treated), with the pupils who have not experienced an exclusion by then, but do so after a buffer period of at least 16 weeks, during the spring or summer term (control). This ensures that the control individuals had never been excluded in some previous year, and every pupil would only be considered once, either as treated, or as control. For the treated pupils, I keep weekly observations from week -8 to week +8, relative to the week of exclusion. For control pupils, and for the year of their exclusion, I keep all weekly observations from 8 weeks before the autumn term (earliest week that could theoretically be included amongst the treated) until 8 weeks after the autumn term (latest week that could theoretically be

included amongst the treated). Thus, the treated and control pupils are roughly observed in similar calendar months, around the autumn term. During the sample period, treated had an exclusion, but controls did not, however the controls will experience an exclusion a few weeks later. Thus, I exploit the potentially idiosyncratic differences in the timing of exclusions between treated and control group. I run event study regressions, where the outcome variable is the number of offences. The main regression specification is defined as follows:

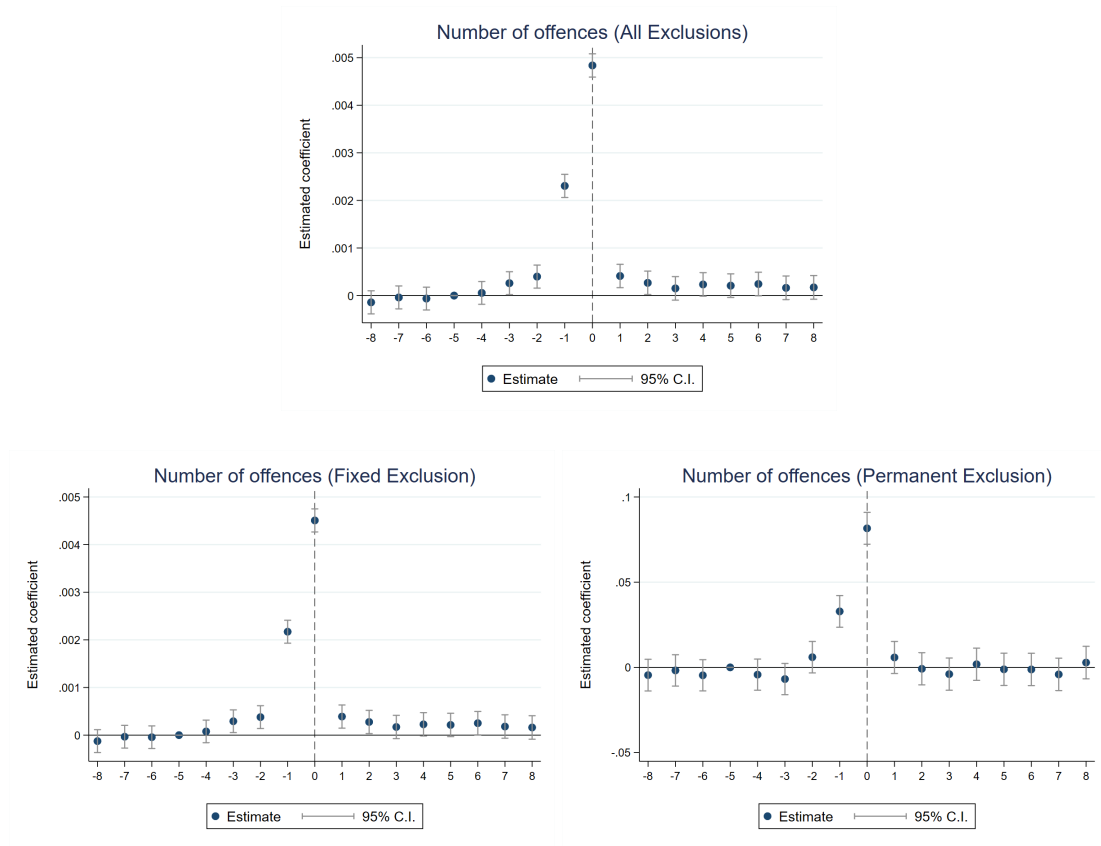
$$Y_{ijt} = \beta_0 + \sum_{s=-8}^8 \gamma_j \cdot event_{i,j+s} + treated_i + Z_j + \epsilon_{ijt}, \quad (1)$$

where Y_{ijt} refers to the numbers recorded of a given crime type, for pupil i in week j in year t ; γ_j are the coefficients for the period of 8 weeks before and after the event; $event_{i,j+s}$ is a dummy variable that for treated pupils is equal to one in the first week of their exclusion, and equal to zero otherwise; $treated_i$ is an indicator for the treatment group; Z_j includes the week fixed effects (cumulative weeks for all years); and ϵ_{ijt} is the error term. I express the event study coefficients relative to period -5 by normalising $\gamma_{-5}=0$.

The coefficients since the event has occurred (γ_j for $j \geq 0$) capture the dynamic effects of the treatment as these effects build over time. The terms γ_j for before the event has occurred (for $j < 0$) provide a placebo test. In the absence of any anticipation effects, model mis-specification, or omitted confounding variables, these pre-event terms should be equal to zero.

The event studies presented in figure 3.3 do not provide clear evidence about the effect of exclusions on crime. The graphs show that crime was already increasing in the weeks just before the exclusion happened, and the effect on crime does not exacerbate

Figure 3.3: Event Studies for Crime Types



Source: DfE-MoJ share data 2007-2020.

in the short term after the exclusion for all, fixed and permanent exclusions. However, for the fixed-term exclusions, the crime level stays more elevated up to week 2 which would be consistent with some crime happening during the exclusion time as a result of the exclusion.

There could be several reasons why the present approach has not identified conclusive effects of exclusions on crime. First, the causality can run both ways, and the event-studies may be dominated by reverse causality, of crime on exclusions. This could be removed by re-running the event studies on a sample of children that did not commit any crime until the date of the exclusion. Second, effects might concentrated among

specific groups of children. For this, heterogeneity analysis would be useful by gender, FSM, ethnicity, grade and behavior/special needs. Third, the analysis could focus on some particular exclusions happening due to reasons like persistent disruptive behavior or verbal abuse, which are less likely to be crime based. Fourth, the effect of permanent exclusions may depend on the type of school or educational provision the child gets transferred (for example, alternative provision). To address this, the treatment event could be redefined as a permanent exclusion with transfer to specific types of provision or other mainstream school.

3.5 Conclusion

Youth crime is very costly for both victims and perpetrators, and can be influenced by numerous factors. In this paper, I explore whether disciplinary policies such as school fixed or permanent exclusions for pupils in grade 7 to 11, increase offending. While the evidence from short-term event studies has not found conclusive evidence on the effect of exclusions on offending, it would be interesting to explore the long term effects of such exclusions.

There are several important policy implications of this research. Studies have found youth crime to have negative effects on employment, wages, psychological issues, which could trap individuals in vicious cycle of crime. Thus, findings from my study will help inform key policy decisions with regards to tackling youth crime, including disciplinary actions. The results from my study offer support for disciplinary policies against pupils with disruptive behavior, and are likely to decrease concern among parents and policymakers regarding higher involvement in crime by pupils who are excluded.

Future research could explore long run effects (up to age 18 of pupils) of these school exclusion policies adopting a propensity score matching strategy to identify a suitable comparable group for pupils who are excluded from school in the pool of pupils who do not experience this event. If significant results are obtained, it would be then interesting to study the mechanisms through which a school exclusion may lead a young person to engage in criminal behaviour. In particular, by considering changes in the probability of school absences after a fixed-term exclusion and changes in the characteristics of the school and school peers following a permanent exclusion. Finally, it would also be interesting to study heterogeneous effects across gender, as well as pupils from different ethnic groups, and across different types of offences. To study the effect of school choice on offending, it would be interesting to study effects for target cohorts after the exclusion from a mainstream school, if they are consequently placed in one of the alternate provision institutions. This would allow comparison whether the impact of exclusion on criminal activity depends on the type of institution into which the student is placed following the exclusion.

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3.7 Appendix

Table 3.4: Occurrence of Crime and Exclusions across Grades

School Grade	Mean Number of Offences (1)	Mean Exclusions (2)	Mean Fixed Exclusions (3)	Mean Permanent Exclusions (4)
7	0.27	2.02	1.99	0.02
8	0.37	2.09	2.06	0.03
9	0.53	2.13	2.09	0.04
10	0.62	2.10	2.06	0.04
11	0.68	1.79	1.77	0.02
Observations	1,254,304	1,254,304	1,254,304	1,254,304

Source: DfE-MoJ share data 2007-2020.

*** p<0.01, ** p<0.05, * p<0.1.

Table 3.5: Occurrence of Crime and Exclusions across Grades- Boys

School Grade	Mean Number of Offences (1)	Mean Exclusions (2)	Mean Fixed Exclusions (3)	Mean Permanent Exclusions (4)
7	0.30	2.07	2.04	0.03
8	0.41	2.12	2.09	0.03
9	0.63	2.13	2.09	0.04
10	0.77	2.10	2.06	0.04
11	0.85	1.80	1.78	0.02
Observations	886,614	886,614	886,614	886,614

Source: DfE-MoJ share data 2007-2020.

*** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: Occurrence of Crime and Exclusions across Grades- Girls

School Grade	Mean Number of Offences (1)	Mean Exclusions (2)	Mean Fixed Exclusions (3)	Mean Permanent Exclusions (4)
7	0.18	1.80	1.78	0.02
8	0.26	2.01	1.99	0.03
9	0.33	2.11	2.08	0.03
10	0.30	2.08	2.05	0.03
11	0.29	1.77	1.76	0.01
Observations	367,690	367,690	367,690	367,690

Source: DfE-MoJ share data 2007-2020.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.