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Right to Education (RTE) Act's Influence on Caste-based Enrollment Gaps and Segregation in India

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Section 12(1)(c) of the Right to Education (RTE) Act of India expanded affirmative action to primary schooling by requiring non-government-funded private schools to reserve 25% of their admissions for students from marginalized castes and economically disadvantaged backgrounds. Using variation in implementation of the policy across states in a difference-in-differences framework, this study assesses the clause's effectiveness in reducing caste-based enrollment gaps in private schools and promoting school integration. States implementing Section 12(1)(c) showed greater private school participation from Scheduled Caste (SC) and Other Backward Class (OBC) students, compared to privileged caste (OTH) students. There were no significant changes in segregation between private and public schools overall; however, early-implementing states exhibited small but statistically significant decreases in the uneven distribution of students by caste. I also found significant reductions in the share of RTE-mandated private schools where OBC students formed a significant majority. There was no evidence for reductions in enrollment gaps for Scheduled Tribe (ST) students or decreases in the share of private schools with a high concentration of privileged caste students, suggesting that Section 12(1)(c) is not influencing segregation at both ends of the socioeconomic spectrum of India. Overall, these results suggest that Section 12(1)(c) implementation is associated with improved access for some marginalized caste groups but its impact on school segregation is either weak or non-existent. More thorough implementation may be needed to achieve caste-based integration in private schools but must be balanced with support for government schools and students not participating in Section 12(1)(c).

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Introduction

In the context of a rapidly growing private school sector in India, which enrolled over 50% of urban students and around 30% of rural students in 2017-18, concerns have been raised over segregation across schools and classrooms (School Education in India, 2021). In fact, studies have shown that students from privileged castes attend private schools at higher rates, and that gaps in access to private schools by poverty level and caste are either stable or widening (Azam, 2017; Chudgar & Creed, 2016). Although there are disagreements on whether private schools offer superior quality of education, there is limited evidence that these schools may afford certain advantages such as better infrastructural facilities, greater access to English as a medium of instruction, and perhaps better adaptation to remote learning technologies (Agarwal, 2024; Chudgar & Quin, 2012; Gouda et al., 2013; Singh, 2015). Given the association between income and private school enrollment, access to private schools may also facilitate self-segregation with students from similar socio-economic backgrounds and thereby, improved outcomes through peer effects (Sacerdote, 2011). In this context, Section 12(1)(c) of the Right to Education (RTE) Act of 2009[1] marks a significant advancement by expanding affirmative action to primary education in private schools. The clause mandated that all private schools (with minimal exceptions) reserve 25% of their seats in Grade 1 for students from lower income households and disadvantaged groups.

Research on the implementation and effectiveness of Section 12(1)(c) is limited but developing. First, a range of policy reports show uneven implementation of the clause across states (Sarin et al., 2015; Verma et al., 2018). The National Commission for Protection of Child Rights (NCPCR), a statutory body of the Central Government of India, reported that only 16 states and union territories (UTs) had begun implementation of this clause by 2021, a decade after the law

[1] Officially the Right of Children to Free and Compulsory Education Act, 2009

came into effect (Sharma, 2021). Even among states that began implementation, many only filled a portion of the seats eligible under the law (Sarin et al., 2015; Verma et al., 2018). Researchers have also evaluated Section 12(1)(c)'s impact on various outcomes for students who are eligible for, receive admission, or enroll through the policy. Eligibility for Section 12(1)(c) and/or winning the lottery to receive admissions increased the likelihood of attending private schools, schools with English as the medium of instruction, schools that charge higher fees (which can be indicative of eliteness or higher quality), and schools with better digital facilities and higher teacher quality (Agarwal, 2024; Dongre et al., 2018; Romero & Singh, 2022). However, relatively privileged households within eligible groups were more likely to apply for and benefit from the program (Damera, 2017; Dongre et al., 2018; Romero & Singh, 2022). The evidence on learning outcomes is mixed, with limited evidence showing improvements in English language learning (Agarwal, 2024; Damera, 2017).

While research on this topic offers many insights into the law's implementation and impacts, the current literature is largely focused on individual outcomes for its direct beneficiaries. This paper contributes to the literature by answering two broader questions on Section 12(1)(c)'s influence at the district level: first, on caste-based enrollment patterns and second, on school segregation. Using state-level variation in Section 12(1)(c) implementation in a difference-in-differences (DiD) framework, I first assess impacts on caste-based enrollment gaps in RTE-mandated private schools by comparing the shares of privileged caste (OTH[2]) and marginalized caste students (Scheduled Castes/SC, Scheduled Tribes/ST, and Other Backward Classes/OBC[3]) who attend eligible private schools. Second, I examine the association between Section 12(1)(c) implementation and two dimensions of school segregation at the district level: the *evenness* of

[2] This group is also often referred to as 'General' category or 'Forward,' 'High,' or 'Upper' castes.
[3] Castes belonging to the OBC category generally enjoy higher economic and social status compared to SC and ST groups but also face discrimination due to their caste. This category does not fit neatly into the marginalized-privileged binary, and they are sometimes treated as the marginalized group in comparison to OTH students and sometimes as the "relatively privileged" group in comparison to SC and ST students.

student distribution across all schools and the levels of caste *concentration* in private schools that are mandated under Section 12(1)(c).

Literature Review

Affirmative action has been used across the world as a tool to improve opportunities for marginalized populations and redress the effects of historical oppression. Such policies have featured prominently in countries such as the U.S., post-apartheid South Africa, and India, which have long histories of systemic marginalization of certain populations. Although the term affirmative action is generally associated with admissions into higher education institutions in the U.S., a host of policies including scholarships for marginalized populations, busing for integration, or school vouchers can also be included under this umbrella term. In India, they also take the form of reservations (i.e., reserving a certain number of seats for marginalized groups), most prominently in higher education institutions, government jobs, and political offices. As these policies are politically controversial, understanding the effects of affirmative action policies, on the marginalized as well as non-beneficiaries, has been an important topic of study since their inception.

India, with an extensive set of policies spanning higher education, employment, and political office, has the world's largest and oldest[4] affirmative action system and has naturally attracted considerable scholarly attention over the years (Brown & Langer, 2015; Cassan, 2019). Much of the earlier research on educational affirmative action in India focused on its impacts at the higher education level because the policies were targeted at this level. Using data on engineering college admissions, Bagde et al. (2016) and Bertrand et al. (2010) found that caste-based reservation policies were effective in their targeting and provided long-term economic benefits to their beneficiaries. First, Bertrand et al. (2010) found that beneficiaries of the program

[4] Established by the Indian government in 1950.

came from lower-income and less-educated households than the "upper caste" students they displaced. Further, reservation policies improved access to higher education among marginalized students and the targeted students were more likely to choose competitive majors and equally likely to graduate as students enrolled through open admissions (Bagde et al., 2016). Finally, reservation policies were also associated with higher incomes after graduation for marginalized students who benefited from the program (Bertrand et al., 2010). However, both of these studies were based on small-scale surveys and focused on a limited number of post-secondary institutions focused on engineering education.

As Cassan (2019) points out, studies focusing on affirmative action impacts in higher education fail to capture a large proportion of marginalized students who do not reach tertiary education. Therefore, more recent literature has focused on impacts at primary and secondary levels of education. Initial studies in this strain were only able to map trends in the educational attainment of different groups. For example, using data from the National Sample Survey from 1983 to 2000, Desai & Kulkarni (2008) found declining gaps in primary school completion for Dalits (SC) and Adivasis (ST) compared to OBC and OTH caste groups. Similar trends were not found for Muslims who are also marginalized but do not have access to certain affirmative action benefits.[5] More recent research has also studied the causal effects of India's affirmative action policies using natural experiments. First, Cassan (2019) studied the impact of a natural experiment in which the government harmonized the lists of jatis belonging to the SC category in the 1970s, giving 2.4 million additional individuals access to caste-based reservations. Comparing those who had access to SC status since 1950 with those who newly gained the status, the author found that access to affirmative action policies caused an increase of about 0.5 education levels for children who were 12 or below at the time of gaining the status and about 0.7 education levels for children

[5] Members of SC, ST, and OBC groups generally follow a wide range of religions in India. However, Muslims and Christians belonging to the SC category do not enjoy the same benefits accorded to Hindu, Sikh, and Buddhist SC individuals. Muslims and Christians belonging to the OBC category are eligible for OBC benefits (Kumar, 2024; Patnaik, 2020; Salazar, 2021)

who were 6 or below at the time of implementation. However, most of the benefits went to males and there was only a negligible effect on higher education attainment. Second, Khanna (2020) focused on a law reserving federal government jobs for OBCs in the early 1990s. The study found that better prospects for employment arising from this law incentivized students to invest in education at lower levels. This resulted in an increase of 0.8 years of education for the average marginalized student.

While Cassan (2019) and Khanna (2020) estimate the general effects of a host of affirmative action policies that were mainly targeted at higher education or employment, the passage of the RTE Act in 2009, which expanded affirmative action to the primary education sector, warrants closer examination. Over the past decade, several studies have begun to understand how Section 12(1)(c) impacted its beneficiaries' school choices as well as academic and socio-emotional outcomes. In Gujarat, Dongre et al. (2018) compared children who benefited from this clause with their older siblings who did not, and found that the Section 12(1)(c) expanded the set of choices available to children. They found that, compared to their older siblings, children who were eligible under this clause were more likely to attend private schools, schools with English as the medium of instruction, and schools that charged higher fees (Dongre et al., 2018). Agarwal (2024, p. 26) also found that children who won the lottery under Section 12(1)(c) in Maharashtra attended schools with "better infrastructure facilities, digital facilities, teacher quality, and … a less diverse student composition," compared to students who lost the lottery.

However, several of these studies also note evidence of cream skimming[6], i.e., the program disproportionately benefited students from more privileged backgrounds, primarily because "applicants were drawn from more-educated and economically better-off households within eligible groups" (Damera, 2017; Dongre et al., 2018; Romero & Singh, 2022, p. 3). Although

[6] Creamy layer is a term used in India to refer to higher-income households who belong to historically marginalized caste groups. Cream skimming refers to instances when welfare programs mainly benefit the most advantaged layer of individuals belonging to eligible groups.

students who won the lottery were marginally more likely to attend private schools, around 75% of students in Romero & Singh (2022) and over 90% of students in Damera (2017) who lost the lottery also attended private schools as fee-paying students. Further, in Karnataka, Damera (2017) presents evidence that seats were being captured by ineligible individuals i.e., households that are above the poverty line but obtained the below-poverty-level (BPL) documentation through illegal means. Even after Romero & Singh (2022, p. 24) conducted a randomized intervention to ease constraints of income and application complexity, the most marginalized households were less likely to complete Section 12(1)(c) applications, primarily due to lack of documentation (to prove eligibility) among the poorest eligible households and lack of private schools in neighborhoods "with a higher proportion of quota-eligible disadvantaged groups." Dongre et al. (2018) note another issue that could limit the effectiveness of Section 12(1)(c) in promoting social integration, i.e., even when households applied for reserved seats in private schools, they did not apply for higher fee-charging, elite schools. The authors speculated that this could be due to higher non-tuition expenses in these schools or fear of discrimination. Taken together, this evidence suggests that, while Section 12(1)(c) may improve schooling options for its beneficiaries, its impact on economic and social integration may be limited.

Focusing on academic outcomes, Agarwal (2024) found that lottery-winning students had significantly higher test scores in English (0.18 SD units), but not Math, compared to their counterparts who were not successful in the lottery. They also note significant variation in the quality of private schools with students attending higher quality schools experiencing 0.5-0.7 standard deviation (SD) higher scores in English compared to Section 12(1)(c) beneficiaries who attended lower quality private schools (Agarwal, 2024). The author identifies that attending schools where English is the medium of instruction may be a crucial factor in improving students' language outcomes. In addition, Agarwal's (2024) study occurred during the COVID-19 pandemic, and they also cite effectiveness in adapting to remote education technologies as a key mechanism driving the test score improvements in English. In Karnataka, Damera (2017) found no overall evidence of improved test scores in Math, English, General Cognitive Ability (GCA), or the local language (Kannada) for lottery-winning students. However, they found a statistically significant treatment effects for girls in GCA and total scores. They suggest that Section 12(1)(c) could "alleviat[e] a binding constraint, in this case parental unwillingness to invest in girls relative to boys" (Damera, 2017, p. 19).

Finally, a few studies have investigated Section 12(1)(c)'s impacts on socio-emotional outcomes. Damera (2017) found that, after 1.5 years of enrollment, students who won the lottery under Section 12(1)(c) scored 0.11 SD higher on self-efficacy, one of four psychosocial outcomes tested. The other three outcomes studied—peer support, school support, and teacher support—did not show any statistically significant differences compared to children who lost the lottery, leading the author to conclude that children admitted through Section 12(1)(c) are not "at a psychosocial disadvantage due to discrimination/lack of integration at schools" (Damera, 2017, p. 13). However, they also note that the improvements in self-efficacy are driven by girls, non-Muslims, and non-SCs, which suggests that boys and marginalized groups do not experience the same self-efficacy benefits. While Damera (2017) focuses on students enrolled through Section 12(1)(c), Rao (2019) focuses on their peers who are fee-paying students at the same private institutions. Surveying students in four elite schools in Delhi, they found that rich students display more prosocial values, are more generous, and are less likely to discriminate after the implementation of the RTE policy and subsequent interactions with students from lower-income households (Rao, 2019). These changes were mainly driven by personal interactions in schools such as study groups assigned by name order. On the other hand, Joshi (2020) also studied social interactions between RTE and non-RTE students and finds that both groups were more likely to be friends with other students from their respective groups. While this is not an encouraging finding, the author also finds that RTE students with a higher share of non-RTE friends tend to score higher on tests.

Overall, literature on the academic and socio-emotional impacts of Section 12(1)(c) is mixed, but studies such as Agarwal (2024) and Damera (2017) highlight its potential for improving student outcomes for those who benefit from the program. Rao (2019) also emphasizes its promise of social cohesion if implemented according to the Act's intent. However, much of the existing literature also casts doubts on Section 12(1)(c)'s effectiveness in reaching the lowest-income households and the most marginalized groups, thereby limiting its capacity to promote social integration (Damera, 2017; Dongre et al., 2018; Romero & Singh, 2022). Finally, current literature is insufficient to understand how Section 12(1)(c) implementation affects government schools and the students who are enrolled in them. Further research is needed to fully understand the societal impacts of Section 12(1)(c).

Context on Section 12(1)(c) and its Implementation

The Indian Parliament passed the RTE Act in 2009 with planned implementation starting in April 2010. The Act provides a legal framework for implementing the constitutional amendment of 2002, which makes 'free and compulsory education' for children between 6 and 14 years of age a fundamental right. It also allocates funds for improving school access and quality while establishing various standards of quality education, such as minimum infrastructural facilities, etc. Section 12(1)(c) of the Act requires all unaided private schools to reserve 25% of their seats in Grade 1 each year for children from marginalized caste (and other disadvantaged) groups and lower-income households. The Act mandates the inclusion of SC and ST students as disadvantaged groups eligible under Section 12(1)(c). State governments were largely responsible for Section 12(1)(c)'s implementation, with both the Central and State governments sharing its costs in an approximately 65:35 ratio.[7] State governments were required to publish their rules concerning the

[7] With exceptions for Northeastern and Himalayan states which receive a higher Central government contribution (Correspondent, 2010).

income criteria for eligibility, eligibility for other disadvantaged or vulnerable groups, and determine the per-child costs for reimbursements. State governments were also given the authority to determine how applications and admissions were to be managed and by whom and finally, they were tasked with establishing a system for reimbursing private schools for Section 12(1)(c) costs.[8]

Despite its planned implementation in the 2010-11 school year, Section 12(1)(c) faced significant backlash from private schools, leading to a legal battle that ended in the 2012 Supreme Court ruling declaring that the clause pertaining to private schools is constitutional.[9] Even after the resolution of this legal hurdle, implementation of Section 12(1)(c) largely depended on state governments' initiative. Some states implemented the clause and some even extended the requirements of the Central government, while others skirted implementation. For example, the online RTE portal of Rajasthan allows minority-run private schools to participate in this clause voluntarily (Verma et al., 2018). In addition, Delhi and Chhattisgarh have voluntarily extended RTE to pre-primary admissions (Verma et al., 2018). On the other hand, Punjab had used its discretionary power to introduce a rule that Section 12(1)(c) would only apply to students who were not able to gain admission to a local government school, essentially watering down the clause and restricting the pool of students who would be able to avail of it (*Punjab Govt. Gazette Notification*, 2011). Uttar Pradesh and Himachal Pradesh also attempted to institute similar rules in 2013 and 2015 respectively, but these rules were struck down by their High Courts in 2016 (*Namita Maniktala vs State of H.P.*, 2016; *Public Interest Litigation (PIL) No. 3334*, 2016). In 2019 (after the study period), Karnataka also implemented a similar rule ("RTE Act Amended," 2019).

In addition to the legal hurdles, as the burden of implementation was given to the state governments, several states avoided implementation by simply not following through with their

[8] NCPCR, a statutory body of the Central government, introduced model guidelines in 2020-21 (*Standard Operating Procedures (SOP) for Implementation of Section 12(1)(c) of the RTE ACT, 2009: Model Procedures for Effective Implementation*, 2021).
[9] In the same verdict, the Supreme Court also declared that religious and minority-run institutions are exempt from this clause.

responsibilities under the Act. These states have regularly ignored calls for Section 12(1)(c) implementation by not releasing the rules or setting up the necessary policy and digital infrastructure necessary for processing RTE applications, admitting students, and reimbursing private schools. In 2018 (at the end of our study period) and 9 years after RTE's enactment, Indus Action reported that 5 states and union territories had not published the eligibility rules. Further, only 22 out of 29 states published the per-child costs which are required to receive reimbursements from the Central government (Verma et al., 2018).

Further, there is substantial variation even among states that implemented the clause. While the inclusion of SC and ST students under this clause is mandated by the law, substantial decision-making power was given to the states, including establishing income criteria for eligibility and including additional disadvantaged groups. Therefore, the income limits for qualification under economically weaker sections ranged widely (Accountability Initiative, 2015). To provide one example, the income limit was under 1 lakh rupees annually in Delhi and 2.5 lakhs in Rajasthan ("Reservation within Reservation," 2015; Verma et al., 2018). Groups qualifying as 'disadvantaged' involved relatively less variation as the inclusion of SC and ST in this group was mandated under the law. However, many states also included OBC students (with some providing restrictions against capturing seats by the creamy layer of OBC students), orphans, students with disabilities, and children affected by HIV/AIDS.[10] Further complicating matters, the guidelines provided by Haryana do not mention SC and ST students as a disadvantaged group and it is unclear if households belonging to these two groups automatically qualify based on their inclusion in the national law or if the state objects to their inclusion (*Haryana Government Notification*, 2011).

In addition to differences in eligibility criterion, there are several operational differences among states implementing the law. States were allowed to decide if the application process and

[10] See Appendix Table A2 for qualifying categories in each implementing state.

lotteries for reserved admissions under Section 12(1)(c) will be managed by the state government or by private schools themselves (Verma et al., 2018). This directly affected implementation as Verma et al. (2018) find that states in which private schools are in charge of managing applications and conducting lotteries have lower levels of participation. They also note that states that have implemented centralized, online systems for admissions and reimbursements have had greater success in scaling the program (Verma et al., 2018).

Finally, there are several systemic challenges associated with the design and implementation of Section 12(1)(c) that negatively impact its effectiveness. First, the process generally requires private schools to submit their proposals/budgets for reimbursement to be approved by the state governments. Such a reimbursement process often requires private schools to admit students and provide education before being approved for or receiving funds (Sarin et al., 2018). Further, Sarin et al. (2018) report that reimbursement funds are transferred from the state to district governments and then to private schools, leading to potential delays at various stages of the process. Second, reimbursement costs are set by the state governments based on the per-pupil expenditure in government schools, but private schools charge a wide range of tuition fees. In cases where private schools charge lower fees than the per-pupil cost determined by the government, the schools receive their tuition costs. However, if the private school tuition is higher than the pre-determined costs, the government reimburses only the pre-determined per-pupil cost (Sarin et al., 2018). This can be a disincentive for elite private schools to participate in the program. In fact, as the Supreme Court exempted minority-run educational institutions (defined as institutions established and administered by a religious minority or minorities (*National Commission for Minority Educational Institutions*, 2024), journalists have documented a sharp increase in the number of minority schools and it is speculated that elite private schools may be using this as a loophole (Mandal, 2024). It must be noted that minority schools have to be run by religious minorities, but this does not necessarily reflect the composition of their student bodies, especially in historically elite Catholic or convent schools of urban India (Dasgupta, 2018; Dayal, 2024; NCPCR, 2021). Overall, a combination of challenges in the reimbursement process and resistance by private schools has impeded widespread rollout of Section 12(1)(c) in many implementing states. Further, there remain many state governments that have resisted its implementation by not establishing the policy infrastructure mandated under the law or by diluting the law through rules that restrict eligibility.

Data and Methods

Datasets

The primary data used in this study comes from the District Information System in Education (DISE) dataset, a national census of public and private schools in India. The study sample has been limited to data from 19 major states and Delhi, excluding 8 smaller states and 6 union territories which represent less than 3% of the Indian population and have unique demographic characteristics (Kingdon, 2020).[11] I also exclude schools from the state of Jammu and Kashmir where the RTE Act was not applicable during the study period. Delhi, despite being a union territory, has been included in the study as it has a larger population than some of the major states such as Uttarakhand and Himachal Pradesh (according to 2011 Census).

School-level data from DISE have been used to calculate enrollment gaps and segregation among Grade 1 students at the district level. The study was restricted to Grade 1 students to fit with the design of Section 12(1)(c) which is applicable to Grade 1 in the first year of implementation, Grades 1 and 2 in the second year and so on. In other words, Section 12(1)(c) was designed to admit new students into Grade 1 in the first year of its implementation with the expectation that students admitted in the first year would be promoted to higher grades in

[11] Telangana, a state that was carved out of Andhra Pradesh in 2014, was merged with its former state to maintain consistency. Schools from districts created after 2011 were also merged into their old districts from Census 2011 to ensure consistency across 11 years of data.

upcoming years. Therefore, in order to maintain consistency in district-year comparisons, all dependent and independent variables have been calculated using only Grade 1 enrollment data. Data on around 11 million school-year observations across 13 years, including a little of over 2 million observations on RTE-mandated schools, were used to calculate the district-level metrics. Enrollment gaps at the district level were calculated based on data for all schools that enrolled at least one student in Grade 1. Segregation indices were calculated after excluding schools with fewer than 5 students as smaller schools do not offer sufficient potential for caste-based integration, a common practice in segregation literature (Greenberg & Monarrez, 2019). Smaller schools may also serve other educational goals such as improving access in remote areas.

The final (district-level) study sample is an unbalanced panel dataset of Indian districts from 2007-08 to 2017-18, which includes three academic years of data prior to the first implementation of Section 12(1)(c) and eight years of post-implementation data.24[12] The sample size for analyses under the non-strict treatment condition included around 5,400 district-year observations across 513 districts. Analyses under the strict treatment condition are based on around 3,850 observations across 361 districts (excluding states that qualified under the non-strict treatment condition but not under the strict condition).25 Districts that did not report any Grade 1 enrollment in RTE-mandated schools in at least one pre-implementation year were excluded from the analyses.

In addition to the DISE dataset, data were gathered from various non-DISE sources to determine the status of Section 12(1)(c) implementation across states. Table 1 presents some of the variables used to assess implementation, such as state-reported school participation rates and whether state applications for reimbursement have been submitted or approved. In addition, Table A1 in the Appendix presents data on states' reports of how many students were admitted through

[12] Data for these years was available upon request at www.schoolreportcards.in.

Section 12(1)(c) in each year. Data on these variables were gathered through a variety of sources including: State of the Nation reports published by various organizations from 2015 to 2017 (Sarin et al., n.d., 2015, 2017); Bright Spots reports published by various organizations from 2018 onwards (Dhariwal et al., 2021; Dhariwal & Middha, 2023; Fortunato et al., 2020; A. Kumar et al., 2019; Verma et al., 2018); responses to requests by the Indian Parliament, Central government data platforms, and news reports on the NCPCR26 report on Section 12(1)(c) implementation released in 2021 (Sharma, 2021).27[13] It must be noted that the available information on Section 12(1)(c) implementation is inconsistent across various sources and prone to mistakes. For example, using various sources of data, Sarin et al. (2015) deduced that some states like Rajasthan and Madhya Pradesh reported cumulative numbers of students admitted until 2014-15 while others like Uttarakhand reported the number of students admitted in 2014-15 alone (Table A1). States may have incentives to report higher levels of implementation to the Central government in order to show compliance with the law or receive higher reimbursement allocations. Therefore, care was taken to cross-check information across multiple non-DISE sources as well as compare them with data from DISE. In the main analysis, treatment status is defined based on information gathered through non-DISE sources while the supplementary analysis defines treatment status using a combination of DISE and non-DISE sources.

Outcome Variables

The study assesses the impact of Section 12(1)(c) implementation on seven dependent variables; the first three attempt to understand the policy's influence on caste-based enrollment gaps in private schools while the final four examine its influence on caste-based school

[13] In addition to these, the 'RTE In Your State' database maintained by RighttoEducation.in and Abhishek Bhattacharya (Centre for Civil Society) is a good resource for understanding each state's rules for Section 12(1)(c) implementation. However, this resource appears to include information from the published State Rules documents but not from memorandums or other communication released later. While there is some correlation between states that published rules and went on to implement Section 12(1)(c), there are discrepancies in this pattern. Therefore, I did not consider whether a state published rules or how clear the rules were as factors in deciding the implementation status.

segregation. To understand private school enrollment gaps between privileged and marginalized castes, I use three variables, i.e., OTH-SC, OTH-ST, and OTH-OBC gaps in RTE-mandated school enrollment.[14] These gaps were calculated by first measuring the percentage of each caste group's students in a district who attended RTE-mandated schools. Then, the share of each marginalized group's (SC, ST, and OBC) students attending RTE-mandated schools was subtracted from the share of OTH students attending RTE-mandated schools in the same district.

The final four variables focusing on school segregation include: a measure of evenness in caste distribution of students (Theil's H index calculated for the entire district) and three variables related to caste-based concentration in private schools (i.e., share of intensely segregated schools where OTH, OBC, or a combination of OTH+OBC students make up a 90% majority). The calculation of all four segregation measures excluded schools that enroll fewer than 5 students in Grade 1 as these schools do not offer significant opportunities for desegregation.

As mentioned, Theil's H is a measure of *evenness* in student distribution by caste across schools.[15] It compares each school's caste composition to the district composition and measures the extent to which a school's composition differs from the district's composition. It is important to note that the H index was calculated using data from all schools within a district, not just those mandated under the RTE. Since private schools do not operate in isolation, policies such as seat reservations or voucher programs can influence enrollment patterns in nearby government schools and may inadvertently exacerbate segregation. For this reason, the study calculates the H index at the district level rather than limiting the analysis to private schools alone. The H index ranges from 0 indicating no segregation (i.e., each school has the same composition of students as the district)

[14] Concept borrowed from Chudgar & Creed (2016)

[15] Theil's H index has been prioritized as a measure of caste-based segregation because, unlike other relative measures of segregation, the H index accounts for changes in population proportions of various caste groups and therefore can be interpreted independently. The Theil's H index was also chosen over similar absolute measures of evenness such as the dissimilarity index due to its wider use in recent school segregation literature (Hinrichs, 2010; Owens et al., 2016; Taylor et al., 2019; Taylor & Frankenberg, 2021) as well as its adaptability and greater decomposability in multigroup settings (Reardon & Firebaugh, 2002; Reardon & Yun, 2003).

to 1 indicating perfect segregation. Generally, researchers consider an H index of 0.10 or below to be low segregation, under 0.25 to be moderate segregation, 0.25 to 0.40 to be high segregation and anything above 0.40 to be extremely high (Ayscue et al., 2018; Reardon & Yun, 2003; Taylor & Frankenberg, 2021). Further, a change of 0.05 in the H index is comparable to around 10% of a district's students moving schools in a manner that either improved or worsened the even distribution of students. A change of 0.05 or above is considered to be "substantively meaningful" change in segregation (Reardon & Yun, 2001, p. 87, 2003). Theil's H is calculated using the formula given in Equation 1:

$$\boldsymbol{H = \sum_{i=1}^{n} \left[\frac{t_i(E-E_i)}{ET}\right]} \qquad \textbf{(1)}$$

where n is the number of units in a district, t_i is the total population of the school, and T is the total population of the district. Similarly, E_i is the entropy or the diversity of the school and E represents the diversity of the district.

The final three outcome variables focus on caste-based segregation in RTE-mandated schools and examine the *concentration* dimension of segregation (Massey & Denton, 1988). Often referred to as the intense segregation index, this measure essentially captures the share of schools that have high concentrations of students from a single caste group or two similar caste groups (Orfield et al., 2012; Orfield & Frankenberg, 2014). In this study, three such outcome variables are included: the share of intensely segregated schools in a district that are OTH-majority, OBC-majority, and OTH+OBC majority.28 For reference, schools in which OTH category students form a 90% majority are categorized as OTH-majority intensely segregated schools; schools in which the sum of OTH and OBC category students form a 90% majority are categorized as OTH+OBC majority schools.

Independent Variables/Treatment Status

The study relies on the uneven implementation of Section 12(1)(c) across states. A combination of factors was used to determine treatment status and, due to the wide variation in implementation levels across states, two different treatment status variables were created. The first treatment status variable (referred to as non-strict condition) mainly relied on the assessment of state-wise implementation conducted by NCPCR, a statuary body under the Central government. In 2021, NCPCR released a report identifying 16 states and UTs that are implementing Section 12(1)(c) and urging others to follow suit. All except two of the 16 states and UTs mentioned in the NCPCR report were classified as treatment states under the non-strict condition. Himachal Pradesh was excluded because there is no evidence (from sources other than the NCPCR report) that the State implemented the law before 2018 (i.e., end of study period; See Table 1). In addition, even though Uttar Pradesh meets many of the criteria, including applying for reimbursements from the central government, it reported that only 0.2% of eligible seats were filled by 2014-15. So, Uttar Pradesh was also not included under the non-strict treatment condition. Second, states qualifying under the second treatment condition (referred to as the strict treatment condition) were determined using a combination of state-reported school participation rates (which correlate with actual 2014-15 participation rates reported in Sarin et al. (2015)), high seat fill rate (above 20% after capping) in 2014-15 as reported in Sarin et al. (2015), and a consistent streak of applying and being approved for central government reimbursement. By these criteria, the states that qualify as treatment states under the strict condition are Chhattisgarh, Delhi, Karnataka, Madhya Pradesh, Rajasthan, Uttarakhand, and Tamil Nadu. In addition to the above-mentioned states, Assam, Bihar, Gujarat, Jharkhand, Maharashtra, and Odisha qualify under the non-strict condition. The control group is consistent across both conditions and includes the following states: Andhra Pradesh, Haryana, Himachal Pradesh, Kerala, Punjab, Uttar Pradesh, and West Bengal.

In the main analysis, it is assumed that all treated states began implementation in the 2010-11 academic year. This decision was necessitated by the fact that data on many factors associated with implementation, such as whether states sought reimbursement from the central government are not available prior to 2014-15. In addition, there was lack of consistency in implementation across various stages of the policy (lack of consensus between whether a state published rules vs established an application process vs actually enrolled children). This was especially true of non-DISE sources of information about Section 12(1)(c) implementation. Therefore, in the main analysis, 2010-11 is treated as the first year of implementation for all eventually treated states. Although not ideal, this method treats potentially untreated cohorts as treated cohorts. So, it is expected that it would result in estimates that are likely to be more conservative than the true impact and avoid the risk of overestimation. In addition, I also present supplementary analysis which takes the staggered nature of policy implementation into account. The supplementary analysis attempts to identify the first year of Section 12(1)(c) implementation for each state using DISE data. Details are provided in the Supplementary Analysis subsection below.

Table **1**: Compilation of non-DISE Sources on Section 12(1)(c) Implementation, Part #1

Variable	**Implementation Status**	**School Participation Rate**	**Status of Per-Child Cost**					**Funds approved for Reimbursement**					
Source	Report by NCPCR (as reported in The Print)	Sarin et al., 2015; as reported by states to Centre and published in RTI reply number F.No.15-1/2015-EE-4 (dated 15th May 2015)	Rajya Sabha Unstarred Question NO. 2496 (To Be Answered On 03.01.2019)					Open Government Data (OGD) Platform (data.gov.in)					
Year	2021	2014-15	2017-18 and Previous Years	2014-15	2015-16	2016-17	2017-18	2018-19	2019-20	2020-21	2021-22	2022-23	
Andhra Pradesh	No	0%	No	-	-	-	-	-	-	-	-	-	
Assam	Yes	33%	No	-	-	-	-	-	-	-	-	-	
Bihar	Yes	50%	Yes	NR	NR	NR	9000.3	9000.3	0	0	0	0	
Chhattisgarh	Yes	99%	Yes	3064.7	3133.3	NR	14030.3	14030.3	4949.6	9269.8	10644.8	7789.4	
Delhi	Yes	100%	Yes	NN	3481.95	NR	Did not Propose	5252.26	6294.3	9500.7	8799.1	7932.7	
Gujarat	Yes	26%	Yes	1303.3	5406.2	NN, NR	7033.5	7033.5	14218.2	19228.1	26593.8	33091.9	
Haryana	No	0%	No	-	-	-	-	-	-	-	-	-	
Himachal Pradesh	Yes	0%	No	-	-	-	-	0	0	6.8	19.0	13.8	
Jharkhand	Yes	23%	Yes	NN, NR	NR	NR	241.84	241.84	716.3	600.1	782.3	692.2	
Karnataka	Yes	78%	Yes	12355.2	16549.8	18246.8	29318.6	29318.6	14859	19999.5	19998.9	19180.6	
Kerala	No	0%	No	-	-	-	-	-	-	-	-	-	

Madhya Pradesh	Yes	88%	Yes	0	9707.8	14919.6	18712.1	18712.1	24000.8	26570.8	29646.4	34289.0
Maharashtra	Yes	6%	Yes	0	2470.0	1400.0	24428.0	24428.0	12000.0	8206.0	5000.0	4291.0
Odisha	Yes	39%	Yes	15.115	88.3	35.1	88.3	88.3	88.3	99.3	95.9	130.4
Punjab	No	0%	No	-	-	-	-	-	-	-	-	-
Rajasthan	Yes	78%	Yes	4171.2	8292.5	12453.4	23581.6	23581.6	17424.9	22647.6	29664.2	17044.9
Tamil Nadu	Yes	76%	Yes	NN, NR	18.6	NR	2770.0	2770.0	7078.4	11827.5	18499.1	23919.2
Uttar Pradesh	Yes	0.2%	Yes	5.26	121.50	NR	653.6	653.6	2586.5	2696.1	302.8	243.1
Uttarakhand	Yes	76%	Yes	4150.8	NR	3950.4	4714.6	4714.6	5786.4	14942.7	13890.8	10033.4
West Bengal	No	0%	No	-	-	-	-	-	-	-	-	-

Note: NN = Per Child Cost Not Notified; NR = Not Reimbursed to Private Schools

Note: Two columns in Table 1 report the same numbers for most states despite one source indicating that these funds were approved for reimbursement in 2017-18 and another source indicating that the same numbers were from 2018-19. This is another example of the inconsistencies in available data on Section 12(1)(c) implementation.

Methods

Difference-in-Differences and Event Study Analysis

The study employs a Difference in Differences (DiD) and event study framework, which attempts to identify the average treatment effect on the treated (ATT), although with significant limitations (See below). All analyses in this section are conducted at the district-year level with each district-year observation given equal weighting; this means that the estimates reflect the average treatment effect in the average treated district.[16] As mentioned above, the study assesses the influence of Section 12(1)(c) on seven dependent variables: three variables related to caste-based enrollment gaps and four related to school segregation. For each of these dependent variables, I present two sets of DiD estimations: the first set using two-way fixed effects (TWFE) and the second using doubly-robust inverse probability weighting (DRIPW). Each set of estimations has four models each. Model 1 only includes district- and year-fixed effects along with an interaction term indicating treatment status in the TWFE model. In Model 2, I also include two controls related to the supply and demand associated with RTE-mandated schools in a district, i.e., a control for the share of total schools that are RTE-mandated and another for the percentage of students who are attending RTE-mandated schools.[17] In Model 3, four additional control variables are included: total enrollment size of the district and the proportions of SC, ST, and OBC students in the district (with share of OTH students as the reference group). Finally, in Model 4, I also control for the share of urban enrollment in the district. Standard errors in all models are clustered at the state level because it is the level of policy implementation. Only Models 3-4 are presented for the final set of outcome variables, i.e., the share of intensely segregated OTH-majority, OBC-

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[16] These estimates can vary from weighted estimates that weight districts based on the number of marginalized students in the district, which would produce the average treatment effect in the district where the average treated student lives (Baker et al., 2025). The decision to present unweighted estimates was motivated by the difficulty of defining who a "treated student" would be in the context of significant policy variation around the inclusion of OBC students across states. However, this is a design choice that must be acknowledged for its implications on the estimates and their interpretation (Baker et al., 2025).

[17] These two variables do now show high levels of correlation and do not result in issues of multicollinearity.

majority, and OTH+OBC-majority schools. This is because the share of intensely segregated schools is highly correlated with the caste composition of districts. Therefore, only Models 3-4 which control for caste composition have been presented. They continue to be referred to as Models 3 and 4 for the sake of consistency. Finally, in addition to providing single DiD estimates that average policy effects across all post-implementation years, I also present event study analyses which illustrate the law's dynamic impacts over time as well as provide an additional check for parallel trends and pre-implementation trends. Event studies were conducted using both TWFE and DRIPW methods and under both non-strict and strict treatment statuses. All event study models presented in the paper are from Model 4 of the TWFE and DRIPW analyses, which include all the control variables discussed above.

Both TWFE and DRIPW estimates are provided in this paper because of TWFE's limitations in cases where there is imbalance in key covariates (as shown in Table 1) and parallel trends may be conditional on covariates, or situations in which covariates may be affected by treatment (See Baker et al. (2025); Cunningham (2024)). In this study, I do not expect that covariates would be affected by treatment given the small scale of Section 12(1)(c) implementation. However, Table 1 shows imbalance in covariates between treatment and control groups and unconditional parallel trends are not satisfied for all dependent variables (See Appendix Figures A1-A3 for raw trends graphs). Other noted issues with TWFE include its use of time-varying covariates (rather than baseline covariates) and its weighing methods of treatment effects among different covariate-strata (Baker et al., 2025). Further, TWFE results may not correctly identify the ATT in cases where treatment effects vary by covariate-strata, for example, if treatment is more effective in urban areas compared to rural areas (Baker et al., 2025). For these reasons, Baker et al. (2025) recommend the use of doubly robust DiD estimates (similar to DRIPW) in order to avoid several additional assumptions required by TWFE. However, as research on different methods of identifying ATT using DiD techniques is still developing

(Caetano & Callaway, 2024) and there is no consensus in the research community at this time, this paper presents both TWFE and DRIPW results.

TWFE estimates have been produced using the *reghdfe* package in Stata while DRIPW estimates were produced using the *csdid* package which implements the DRIPW method developed by Sant'Anna & Zhao (2020). It must be noted that the two estimates manage covariates differently. The instructions for the *csdid* package note that, "when using panel data, even if covariates are time-varying, only the base-period (earlier-period) values are used for the estimation." Therefore, in this study, the DRIPW estimates control for baseline values of covariates while the TWFE estimates control for time-varying covariates. Due to above-stated concerns with TWFE, additional emphasis has been placed on the DRIPW estimates in the discussion of results.

Robustness Check

I run a robustness check using a placebo group, i.e., private schools that are not mandated under Section 12(1)(c) because they are unrecognized or government-aided or religious institutions. Both TWFE and DRIPW estimates are presented for the robustness check under the non-strict and strict treatment conditions. This allows us to check whether the results indicate a general rise in private sector enrollment in treatment states, unrelated to the law's implementation. However, two crucial differences between the main sample and the placebo group must be noted: first, students attending RTE-mandated (recognized, unaided) schools are likely to be from higher-income households compared to students attending non-RTE-mandated private schools because schools in the placebo group charge lower tuition fees on average (Gouda et al., 2013). Second, in the DISE dataset, there are a little over 2 million school-year observations on RTE-mandated schools compared to only 600,000 observations on non-RTE-mandated schools. This may be because, as Kingdon (2020) points out, DISE undercounts unrecognized private schools because they are not registered with the government and may not wish to participate in governmentinitiated data collection processes. Therefore, for these two reasons, caution must be exercised when interpreting the results of this placebo check. However, a general rise in demand for private schooling is likely to impact both RTE-mandated and non-RTE-mandated schools, although not necessarily to the same extent.

Supplementary Analysis with Staggered Rollout

Although the RTE Act formally came into effect in 2010–11, not all eventually treated states began implementation in that year. In practice, many states began implementation between 2012 and 2014, following the Supreme Court's affirmation of Section 12(1)(c). For this reason, as a supplementary analysis, I repeated the TWFE and DRIPW analysis with an alternative treatment criterion that accounted for the staggered rollout. DISE data on Section 12(1)(c) implementation—specifically, the fill rate of new enrollments and the share of schools participating in the program—were used to approximate each state's first year of implementation. A state was considered to have begun implementation in the first year when DISE data indicated that at least 10% of eligible Grade 1 seats were filled under Section 12(1)(c). Importantly, this refers to 10% out of the 25% of Grade 1 seats that are legally mandated to be reserved—not 10% of all Grade 1 seats—thus representing a relatively low threshold. This cutoff was selected because, since DISE data are self-reported by school administrators, anomalies were common. For example, even in states like Andhra Pradesh—where no available evidence indicates that implementation occurred during the study period—some schools reported enrolling students under Section 12(1)(c), presumably because schools reported the number of students from households below the poverty line, number of students associated with other affirmative action programs, or due to data reporting errors. The 10% threshold was therefore chosen as a conservative benchmark to distinguish meaningful implementation from isolated or erroneous reports. Appendix Table A3 lists the first year of implementation for each state based on this criterion. It must also be noted that using a different criteria—the year in which at least 10% of schools in a state report participating in Section

12(1)(c)—resulted in the same year of first implementation for most states. This revised classification based on DISE data resulted in two key deviations from earlier analyses. Specifically, Haryana and West Bengal were found to have surpassed the 10% threshold in 2010–11 and 2012–13, respectively, even though prior assessments, including the NCPCR report, had classified both states as non-implementation states. As a result, these two states were included in the treatment group in the supplementary analysis based on DISE data.

As TWFE does not provide accurate estimates in cases with multiple implementation cohorts (Goodman-Bacon, 2021), this section presents a number of estimates that are robust to staggered rollout. Specifically, I present estimates generated using the approaches of Gardner (2021; hereafter referred to as Gardner estimates), Sun & Abraham (2021; abbreviated as SA estimates), and Callaway & Sant'Anna (2021; abbreviated as CS estimates). Gardner estimates were calculated using the *did2s* package (Butts & Gardner, 2022), SA estimates using *eventstudyinteract*, and CS estimates using *csdid*. Although all three methods account for staggered treatment rollout and heterogeneous treatment effects, they use different approaches. SA and CS methods are described as 2x2 aggregation methods which estimate the event-study average treatment effect while Gardner (2021) uses an imputation-based approach (Butts, n.d.; Butts & Gardner, 2022). Further, SA uses an interaction-weighted regression approach while CS combines regression and weighting techniques to generate doubly-robust estimates (Butts, n.d.). Given that literature on this topic continues to develop and there is no consensus yet, I present estimates calculated using all four approaches, including TWFE.

It must also be noted that I used different comparison groups across the various approaches. In the SA estimates, all treatment cohorts are compared to the never-treated group while the *csdid* command is used to compare treated groups with 'not yet' treated groups as controls. This was necessitated because, although 'not yet' treated cohorts were preferable as the control group, this option was not available in all the approaches and associated Stata packages.[18] So, different control groups were used for the SA and CS estimates in this section. All event study analyses presented in this section are from Model 4, i.e., they include all control variables. In addition to presenting event study estimates, I also present ATTs for each cohort using *csdid* to analyze heterogeneous treatment effects.

Limitations

The dataset and study design have several limitations. First, data on poverty or income status of students is not available in the DISE dataset during the period of study. Although caste is highly correlated with economic status, we also know that capture of affirmative action benefits by relatively wealthier or more educated individuals among eligible groups is a popular concern in India (Desai & Kulkarni, 2008; Dongre et al., 2018; Romero & Singh, 2022). Further, school segregation by economic status is a concern that has not received sufficient scholarly attention in the country. For these two reasons, the ability to study the intersections of caste and economic status would be profoundly informative. However, due to lack of available data, this study is unable to explore these questions. It must be noted that the DISE questionnaires asked school administrators for the number of students from below-poverty-level households enrolled in their schools. However, the responses to this question have not been made available to researchers in the downloadable datasets at www.schoolreportcards.in.

Second, Section 12(1)(c) exempts minority schools (defined as institutions established and administered by a religious minority or minorities (*National Commission for Minority Educational Institutions*, n.d.)) and religious schools; however, schools need to apply for minority status in order to be exempted and it is unclear which minority-managed schools may opt out of this program. DISE did not offer a way to distinguish between minority and non-minority schools,

[18] State package *eventstudyinteract* allows an alternative 'last treated' control group, but this was not the preferred control group and this option required dropping data from 2017-18 during which the last treated group received treatment.

except for Madrasas (Muslim religious schools) during this period of data collection. Therefore, schools identifying as unrecognized Madrasas or those recognized by Madrasa/Waqf boards have not been included as RTE-mandated schools, but the analysis likely includes other minority and/or religious schools that have been exempted from Section 12(1)(c) implementation. It must be noted that news reports show in an increase in minority institutions, especially among elite schools, following the passage of the RTE Act (Mandal, 2024). This, among other criticisms, has led the Supreme Court to reconsider the blanket exemption of minority-run schools (Kuntamalla, 2025). The current study is, however, unable to account for schools' minority status or any changes in status due to data constraints.

In addition to the limitations associated with the dataset, the implementation timeline for Section 12(1)(c) coincides with the timeline for RTE's Act's numerous other provisions. Even though most of RTE Act's provisions (such as improvements in school infrastructure, reductions in pupil-teacher ratio, etc.) applied to government schools and, to a lesser extent to government-aided private schools, these changes are likely to impact the environment in which unaided private schools function (See Shah & Steinberg (2019) for a comprehensive overview of RTE-induced changes and Varghese (2022)). Therefore, uneven implementation of these non-treatment provisions of the RTE Act can influence the estimates in this study. In addition, because of the wide scope of RTE Act's provisions, the interaction between the treatment and the various non-treatment policy changes cannot be easily disentangled.

Finally, it must also be noted that states that implemented Section 12(1)(c) are likely to fundamentally differ from states that are resistant to its implementation. Similarly, early implementation of the clause is likely to be an indication of greater motivation from the state governments. Greater motivation could, in turn, result from interactions between various factors; for example, larger marginalized caste populations or sharing same political party as the national government. While the DiD framework accounts for such non-random assignment of the treatment, these differences could also result in varying trends in control and treatment states. Therefore, care must be taken in the causal interpretation of these results, especially in light of evidence that existing DiD methods may not effectively estimate the ATT in a variety of real-world scenarios (Baker et al., 2025).

Descriptive Statistics

Table 2 presents descriptive statistics comparing control districts with treatment districts under two conditions (non-strict and strict) for the 2009-10 school year to assess baseline differences. It also calculates the standardized difference in means between the control and treatment groups.[19] I find statistically significant differences in baseline characteristics; the average non-strict and strict treatment districts have smaller total enrollments, lower shares of SC and OTH enrollment, and higher shares of ST and OBC enrollment, compared to the average district in control states. Higher proportions of OBC students may correlate with greater political support for Section 12(1)(c) implementation in treatment districts. On the other hand, non-strict treatment districts have a significantly lower average share of enrollment in RTE-mandated schools compared to control districts, while the opposite is true for strict treatment districts. Average literacy rate and share of overall urban population do not vary significantly between the three groups, although treatment districts have higher shares of enrollment in urban schools according to DISE data. The standardized difference values presented in Table 2 for several independent variables are above 0.25 in absolute terms, reiterating that the control and treatment groups are imbalanced on several of the district characteristics (Baker et al., 2025; Imbens & Rubin, 2015). The largest differences between control and both treatment groups lie in the differences in caste composition.

[19] Calculated using Stata command developed by Bayoumi (2022).

Table **2**: Baseline (2009-10) Descriptive Statistics by Treatment Status

	Control	Treatment	P-Value from T-test	Standardized Difference
Non-strict Treatment				
Total District Enrollment	61704.05	47127.22	0.000	0.369
Total Number of Schools	1991.05	1905.03	0.414	0.073
Share of Enrollment in RTE-mandated Schools	26.29%	21.73%	0.001	0.324
Percentage of RTE-mandated Schools	17.82%	13.78%	0.000	0.367
Share of SC Enrollment	27.96%	16.81%	0.000	1.069
Share of ST Enrollment	3.73%	19.88%	0.000	-0.945
Share of OBC Enrollment	36.71%	42.30%	0.005	-0.264
Share of OTH Enrollment	31.60%	21.01%	0.000	0.542
Literacy Rate – Census 2011*	63.35%	63.06%	0.760	0.029
Share of Urban Population – Census 2011*	26.74%	27.54%	0.676	-0.040
Share of Enrollment in Urban Schools	17.23%	21.91%	0.004	-0.284
Strict Treatment				
Total District Enrollment	61704.05	45674.54	0.000	0.417
Total Number of Schools	1991.05	1981.28	0.939	0.008
Share of Enrollment in RTE-mandated Schools	26.29%	30.57%	0.002	-0.337
Percentage of RTE-mandated Schools	17.82%	19.74%	0.095	-0.178
Share of SC Enrollment	27.96%	19.24%	0.000	0.844
Share of ST Enrollment	3.73%	17.12%	0.000	-0.822
Share of OBC Enrollment	36.71%	45.38%	0.000	-0.412
Share of OTH Enrollment	31.60%	18.27%	0.000	0.685
Literacy Rate – Census 2011*	63.35%	62.51%	0.427	0.084
Share of Urban Population – Census 2011*	26.74%	30.96%	0.057	-0.203
Share of Enrollment in Urban Schools	17.23%	25.74%	0.000	-0.500

Note: * indicates that variable is not included in the rest of the analysis. These variables from Census 2011 were only used to assess differences in control and treatment groups around the baseline year using an alternative dataset.

Results

This section presents TWFE and DRIPW estimates of Section 12(1)(c) implementation's influence on enrollment gaps in RTE-mandated private schools and caste-based segregation. For each outcome variable, the top panel presents 8 (4 TWFE and 4 DRIPW) estimates for the non-strict treatment condition and the bottom panel provides the same 8 estimates for the strict treatment condition. In each set of 4 estimates, Model 1 includes year and district fixed effects along with an indicator of RTE implementation in the TWFE model. Model 2 includes two additional controls for the share of schools in the district that are RTE-mandated and the percentage of students who are attending RTE-mandated schools. Model 3 also includes controls for the number of schools in the district, total enrollment, and proportions of various caste groups in the district student population. Finally, Model 4 includes controls for the share of enrollment in urban areas. DRIPW estimates from Model 4 are prioritized in the narration of results.

Section 12(1)(c) and Enrollment Gaps in RTE-Mandated Private Schools

Table 3 presents TWFE and DRIPW DiD estimates for three dependent variables related to enrollment gaps, OTH-SC, OTH-ST, and OTH-OBC gaps in RTE-mandated school enrollment. Focusing on the OTH-SC enrollment gap, Model 1 estimates are all negative, but the TWFE estimates are not statistically significant. On the other hand, the DRIPW Model 1 estimates indicate that the OTH-SC gap reduced by 2.26 points and 4.57 points in the non-strict and strict treatment states, respectively, compared to control states. Controlling for covariates related to district characteristics (which are treated differently under TWFE and DRIPW) increases the magnitude of the DRIPW estimates to 6.26 and 5.67 points for non-strict and strict treatment districts, respectively, and both estimates are statistically significant.

TWFE and DRIPW Model 4 estimates do not show a statistically significant (at the 95% level) impact of the treatment on OTH-ST enrollment gap in RTE-mandated private schools. This result indicates that Section 12(1)(c) implementation may not be benefiting all marginalized groups equally. However, it must also be noted that ST students are geographically concentrated in a small number of states, therefore, further analyses that weights districts based on the population of ST students will be needed for drawing stronger conclusions (Azam, 2017). It is also interesting to note that TWFE and DRIPW estimates vary in important ways. While Model 1 results from both

TWFE and DRIPW analyses are comparable, estimates for Models 2-4 diverge significantly and show different signs in both treatment conditions. This is likely due to the different ways in which the two methods manage covariates. TWFE Model 4 estimates are positive while DRIPW Model 4 estimates are negative, although neither is statistically significant at the 5% level.

TWFE and DRIPW estimates for treatment's influence on OTH-OBC gap in RTE-mandated private school enrollment differed as well. TWFE estimates show a positive and not significant impact for non-strict treatment districts and a negative and non-significant impact for strict treatment states (Model 4). On the other hand, according to DRIPW Model 4 estimate, non-strict treatment districts experienced a 3.54-point decrease and strict treatment districts experienced a 3.23-point decrease in the OTH-OBC enrollment gap after implementing Section 12(1)(c), compared to control states. The slightly larger reduction in the OTH-OBC gap in non-strict treatment states may be because states that qualified under the non-strict condition but did not qualify under the strict condition were more likely to include OBCs as a disadvantaged group under Section 12(1)(c) and more likely to not include restrictions against creamy layer of OBC students.[20] Overall, these estimates indicate that Section 12(1)(c) implementation may be an effective solution for bridging private school enrollment gaps in Grade 1 for SC and OBC students, but not for ST students. The larger reductions in the OTH-SC gap, compared to the OTH-OBC gap, indicate that the policy may be succeeding in targeting more marginalized groups.

[20] Out of 7 strict treatment states, 2 do not include OBCs, 4 include them but have restrictions against capture by creamy layer of OBCs, and 1 state includes them without any restrictions. Out of 6 states that qualify under non-strict condition but not under the strict condition, 4 include OBCs without any restrictions and 2 do not include OBCs as a disadvantaged group.

Table **3**: Estimates of Section 12(1)(c) Implementation's Influence on Caste-based Gaps in Private School Enrollment

Non-strict Treatment								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DEPENDENT VARIABLES	TWFE Model 1	TWFE Model 2	TWFE Model 3	TWFE Model 4	DRIPW Model 1	DRIPW Model 2	DRIPW Model 3	DRIPW Model 4
OTH-SC Enrollment Gap	-3.150	0.094	0.616	0.741	-2.257**	-4.132***	-5.753***	-6.259***
	(2.763)	(2.033)	(1.868)	(1.839)	(1.038)	(1.090)	(0.990)	(1.153)
OTH-ST Enrollment Gap	2.727	4.251*	4.210*	4.362*	2.564	1.199	-4.629***	-1.048
	(2.808)	(2.266)	(2.113)	(2.089)	(1.706)	(1.665)	(1.608)	(5.357)
OTH-OBC Enrollment Gap	-0.092	1.310	1.417	1.553	0.443	-0.880	-1.944**	-3.538***
	(1.749)	(1.923)	(1.683)	(1.629)	(0.933)	(0.946)	(0.918)	(1.224)
Strict Treatment								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DEPENDENT VARIABLES	TWFE Model 1	TWFE Model 2	TWFE Model 3	TWFE Model 4	DRIPW Model 1	DRIPW Model 2	DRIPW Model 3	DRIPW Model 4
OTH-SC Enrollment Gap	-5.347	-2.611	-2.301	-2.012	-4.570***	-3.093***	-4.771***	-5.666***
	(3.264)	(2.372)	(2.091)	(2.087)	(1.170)	(1.004)	(1.307)	(1.235)
OTH-ST Enrollment Gap	1.315	1.865	1.626	1.890	1.014	1.857	-1.708	-1.363
	(3.108)	(2.681)	(2.341)	(2.321)	(1.826)	(1.878)	(2.245)	(3.224)
OTH-OBC Enrollment Gap	-3.047	-1.770	-1.762	-1.452	-2.310**	-1.187	-2.318**	-3.226***
	(2.005)	(2.139)	(1.723)	(1.673)	(1.052)	(0.965)	(1.181)	(1.122)
Controls for Share of Mandated Schools and Mandated School Enrollment		X	X	X		X	X	X
Controls for District Enrollment and Caste Composition			X	X			X	X
Controls for District Urbanicity				X				X

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Section 12(1)(c) and Caste-based Segregation

To answer the second research question, I present both TWFE and DRIPW results of Section 12(1)(c) implementation's influence on school segregation using Theil's H and intense segregation indices. District-level segregation is measured using Theil's H index, which shows how evenly students of various caste groups are distributed across schools. The measure essentially compares each school to the district's caste composition and quantifies the extent to which each school's caste composition deviates from the district's composition. While other measures focus on enrollment or segregation in RTE-mandated private schools only, the H index has been calculated for all private and public schools enrolling at least 5 students in Grade 1 in a district. This was chosen instead of calculating the H index only for private schools because schooling choice programs influence enrollment patterns (and thereby, resource distribution) across both private and public schools.

Models 1-2 in Table 4 suggest that treatment districts experienced marginal reductions in caste-based segregation (or evenness in the distribution of students), however, these reductions are likely due to differences in district characteristics rather than an effect of Section 12(1)(c) implementation. It must also be noted that the magnitude of the statistically significant estimates in Theil's H index is on the lower side. Reardon & Yun (2001, 2003) point out that a change of 0.05 in the H index is considered to be substantively meaningful and equivalent to around 10% of students in the district moving schools in a manner that increased evenness (or decreased segregation). Statistically significant (at the 5% level) TWFE and DRIPW estimates in our analysis ranged between 0.014 to 0.026, which would be roughly equivalent to 2-6% of students in a district moving schools in a way that decreased segregation. Further, the H index treats all four caste groups equally and gives equal importance to diversity among SC and ST groups (which are both marginalized groups) as it does to diversity among ST and OTH groups (which are generally on the opposite ends of the socioeconomic spectrum). Overall, given that neither of the Model 4 estimates is statistically significant, it can be concluded that implementation of Section 12(1)(c) did not substantially impact evenness in the caste distribution across schools in treatment districts.

Table 4 also presents analyses of intense segregation in RTE-mandated schools, i.e., estimates for Section 12(1)(c) influence on OTH-majority, OBC-majority, and OTH+OBC-majority intensely segregated schools, respectively.[21] Only Models 3 and 4 are presented in this analysis because the number and percentage of intensely segregated schools is highly correlated with the caste composition of districts. Therefore, only models that control for either baseline caste composition or changes in caste composition are presented. Overall, these results show that there are no significant changes in the share of schools where OTH students form a 90% majority. However, DRIPW Model 4 estimates indicate that the share of OBC-majority intensely segregated schools reduced by 2.77 points and 2.01 points in the non-strict treatment and strict treatment districts, respectively. These results indicate uneven implementation of Section 12(1)(c) across private schools and perhaps non-participation among elite private schools. They also echo the findings of Dongre et al. (2018) who show that Section 12(1)(c) applicants do not prefer elite private schools, likely due to the fear of discrimination and/or additional costs such as extra-curricular fees not borne by Section 12(1)(c). I do not find a significant reduction in the share of schools where OTH+OBC students together form a 90% majority.

[21] Schools in which OTH students make up at least a 90% majority are referred to as OTH-majority intensely segregated schools. Schools in which neither OTH or OBC students form a 90% majority but both groups combined form such a majority are referred to as OTH+OBC-majority intensely segregated schools.

Table **4**: Estimates of Section 12(1)(c) Implementation's Influence on Caste-based Segregation

				Non-strict Treatment				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DEPENDENT VARIABLES	TWFE	TWFE	TWFE	TWFE	DRIPW	DRIPW	DRIPW	DRIPW
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Theil's H Index	-0.026**	-0.026**	-0.014	-0.014	-0.015***	-0.014**	-0.009	-0.000
	(0.012)	(0.012)	(0.009)	(0.009)	(0.005)	(0.006)	(0.013)	(0.010)
Share of OTH-majority			2.227	2.284			-0.714	-1.329
Intensely Segregated Schools			(2.179)	(2.180)			(1.560)	(1.601)
Share of OBC-majority			-1.678*	-1.659**			-3.675**	-2.768**
Intensely Segregated Schools			(0.808)	(0.792)			(1.793)	(1.333)
Share of OTH+OBC-majority			0.092	0.095			3.216	1.401
Intensely Segregated Schools			(1.488)	(1.490)			(4.095)	(2.943)
				Strict Treatment				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DEPENDENT VARIABLES	TWFE	TWFE	TWFE	TWFE	DRIPW	DRIPW	DRIPW	DRIPW
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Theil's H Index	-0.030*	-0.028*	-0.012	-0.011	-0.021***	-0.017***	-0.006	-0.000
	(0.015)	(0.015)	(0.010)	(0.010)	(0.005)	(0.005)	(0.008)	(0.006)
Share of OTH-majority			3.580	3.680*			-0.354	-0.966
Intensely Segregated Schools			(2.059)	(2.067)			(1.185)	(1.136)
Share of OBC-majority			-1.760*	-1.738*			-2.664***	-2.010**
Intensely Segregated Schools			(0.982)	(0.961)			(1.003)	(0.981)
Share of OTH+OBC-majority			-2.371*	-2.408*			-0.726	-1.473
Intensely Segregated Schools			(1.199)	(1.197)			(1.933)	(1.574)
Controls for Share of Mandated Schools and Mandated School Enrollment		X	X	X		X	X	X
Controls for District Enrollment and Caste Composition			X	X			X	X
Controls for District Urbanicity				X				X

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Event Study Analysis

In this section, I present event study graphs to understand the dynamic impacts of Section 12(1)(c) over the years. Figures 1 and 2 present the TWFE and DRIPW event study estimates with coefficients normalized to the reference year, which is one year prior to the implementation of Section 12(1)(c), i.e., 2009-10. Event study results largely concur with the DiD estimates presented above. In Figure 1, DRIPW-based event study results show that non-strict treatment states experience statistically significant declines in OTH-SC and OTH-OBC gaps in private school enrollment. For both dependent variables, estimates show statistically significant reductions within one or two years of implementation but follow a U-shaped pattern in which the impact subsides around the sixth year of implementation, according to DRIPW estimates. Differences in OTH-OBC enrollment gap are no longer statistically significant starting in 2015-16. Further, TWFE-based event study results do not show any significant reductions in enrollment gaps under the non-strict treatment condition. In addition, non-strict treatment states also experience lower shares of OBC-majority intensely segregated schools. However, this effect is not consistent across all years.

For strict treatment districts (Figure 1), both TWFE and DRIPW event study estimates show statistically significant reductions in OTH-SC and OTH-OBC enrollment gaps. These reductions become statistically significant in the third year after implementation and follow a U-shaped pattern similar to results in Figure 1. This may indicate that the policy requires a period of implementation before producing significant changes or that many states had not begun implementation until later. Although the DRIPW-based DiD estimate for the share of OBC-majority intensely segregated schools is statistically significant, the corresponding event study graph shows no consistent pattern of significance. On the other hand, TWFE-based event study estimates show a significant reduction in the share of OTH+OBC intensely-segregated schools, starting in the fourth year of implementation, i.e., 2013-14.

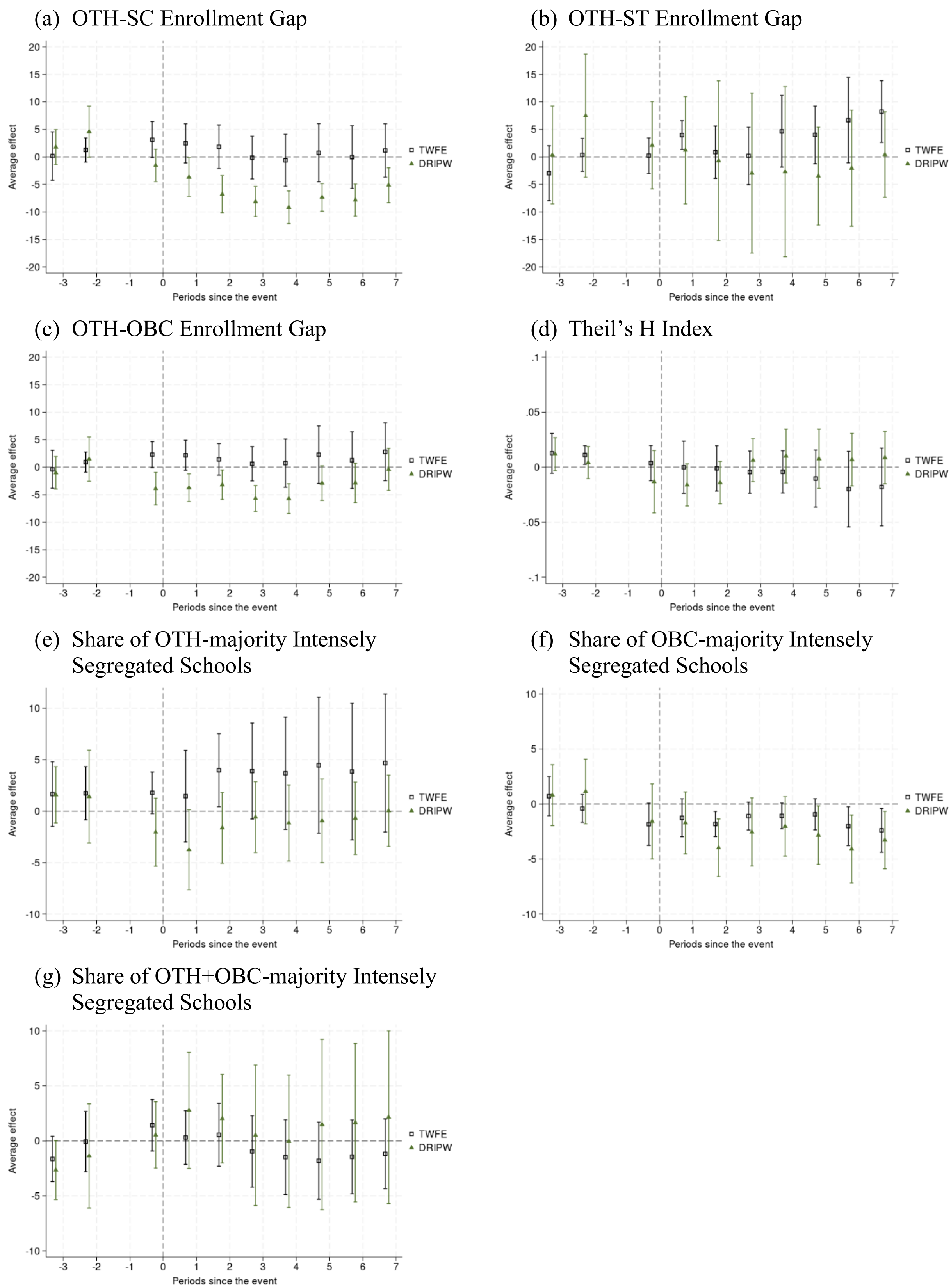
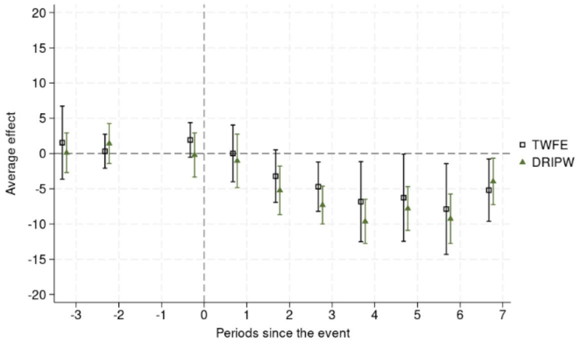
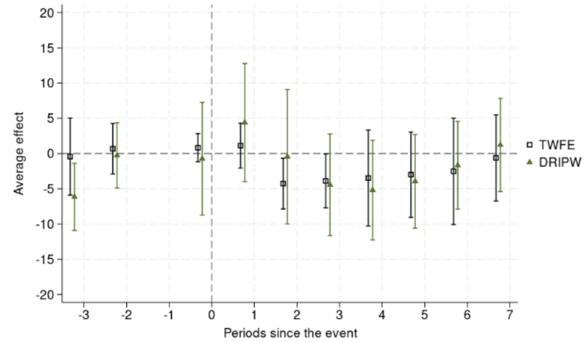


Figure **1**: Event Study Results for Non-Strict Treatment Condition

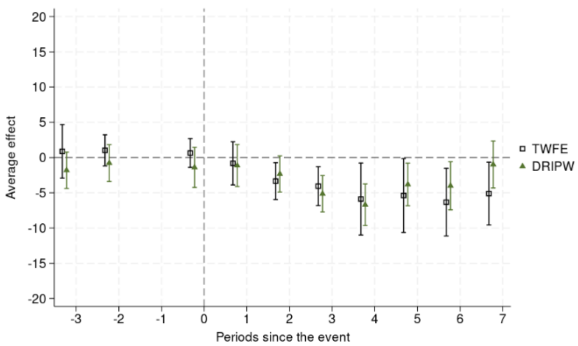
(a) OTH-SC Enrollment Gap



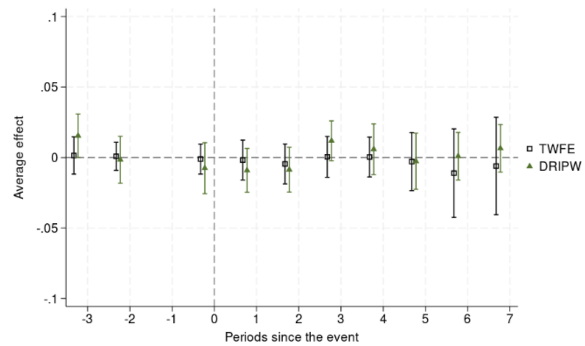
(b) OTH-ST Enrollment Gap



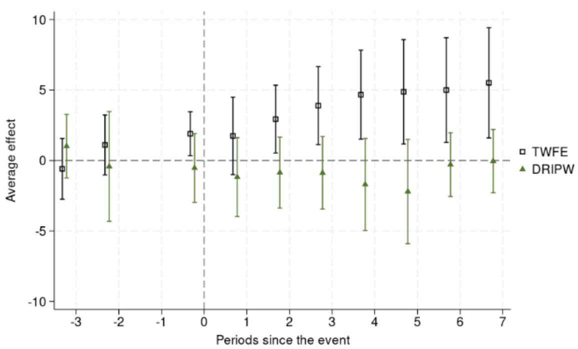
(c) OTH-OBC Enrollment Gap



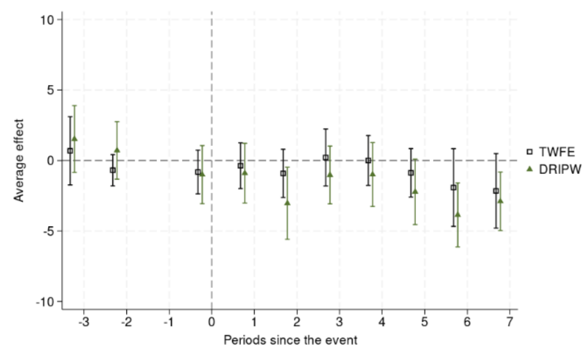
(d) Theil's H Index



(e) Share of OTH-majority Intensely Segregated Schools



(f) Share of OBC-majority Intensely Segregated Schools



(g) Share of OTH+OBC-majority Intensely Segregated Schools

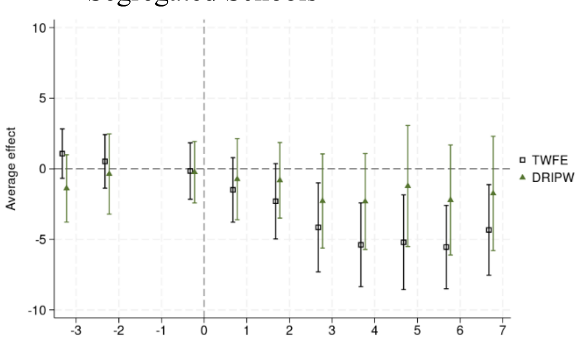


Figure **2**: Event Study Results for Strict Treatment Condition

Robustness Check

To check the robustness of the TWFE and DRIPW findings presented above, I repeated the main analysis with a placebo group of private schools that are not mandated under Section 12(1)(c), i.e., government-aided, unrecognized, or religious private schools. If the results from the main analysis were occurring due to a general increase in the prevalence of private schooling in treated states or differences in the trajectories of private sector growth between treatment and control states, it is likely that we would see similar effects for government-aided and unrecognized private schools. This is especially true when we consider that parents often do not recognize differences among private schools (Chudgar et al., 2023). However, we must note two important caveats: first, the populations attending RTE-mandated (recognized, unaided) schools and non-RTE-mandated schools are likely to differ in many demographic characteristics including household income, primarily because schools in the placebo group charge lower fees on average (Gouda et al., 2013). Second, fewer non-RTE-mandated schools are represented in DISE compared to recognized and unaided private schools (See Data and Methods for further discussion). Despite these caveats, a rise in the demand for private schools can be expected to extend to all private schools and, therefore, government-aided, unrecognized, and religious private schools can act as a reasonable placebo group. For both treatment conditions, Table 5 does not show statistically significant estimates that are consistent in direction with the main analysis. DRIPW Model 4 shows a statistically significant association between treatment status and the share of OBC-majority intensely segregated schools in the placebo group, but the sign is reversed compared to the main analysis. Overall, the results of the placebo test provide reasonable confidence that the results from the main analysis are unique to RTE-mandated private schools and thereby, more likely to be a causal effect of Section 12(1)(c) implementation.

Table **5**: Estimates of Section 12(1)(c) Influence in a Placebo Group (Unrecognized, Government-aided, or Religious Private Schools)

Non-strict Treatment								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DEPENDENT VARIABLES	TWFE	TWFE	TWFE	TWFE	DRIPW	DRIPW	DRIPW	DRIPW
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
OTH-SC Enrollment Gap	1.448	0.463	0.198	0.161	1.167	1.538	0.890	1.784
	(2.793)	(1.509)	(1.274)	(1.282)	(0.868)	(1.076)	(1.455)	(1.527)
OTH-ST Enrollment Gap	2.501	0.113	0.076	0.075	1.944	1.528	-1.253	-0.074
	(3.159)	(1.165)	(1.159)	(1.157)	(1.398)	(1.449)	(1.649)	(1.701)
OTH-OBC Enrollment Gap	1.449	0.787	0.357	0.334	1.344**	1.755*	0.837	1.294
	(1.694)	(1.083)	(1.033)	(1.043)	(0.664)	(0.934)	(1.097)	(1.358)
Share of OTH-majority Intensely			-1.676	-1.777			-4.019	-5.120
Segregated Schools			(3.129)	(3.055)			(2.753)	(3.590)
Share of OBC-majority Intensely			-0.066	-0.051			-1.949	-0.690
Segregated Schools			(5.376)	(5.400)			(2.069)	(2.189)
Share of OTH+OBC-majority Intensely			0.846	0.810			1.903	2.436
Segregated Schools			(1.720)	(1.680)			(2.301)	(1.896)

Strict Treatment								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DEPENDENT VARIABLES	TWFE	TWFE	TWFE	TWFE	DRIPW	DRIPW	DRIPW	DRIPW
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
OTH-SC Enrollment Gap	-0.248	0.625	0.365	0.441	-0.436	-0.984	-1.154	-1.872
	(2.507)	(1.654)	(1.369)	(1.364)	(0.810)	(0.839)	(1.121)	(2.001)
OTH-ST Enrollment Gap	0.859	0.249	0.242	0.240	0.257	-1.228	-3.452**	-2.819
	(2.817)	(1.189)	(1.067)	(1.068)	(1.356)	(1.272)	(1.658)	(1.837)
OTH-OBC Enrollment Gap	-0.211	0.165	-0.166	-0.112	-0.060	-0.275	-0.786	-1.728
	(1.282)	(1.086)	(0.991)	(0.998)	(0.620)	(0.693)	(0.940)	(1.962)
Share of OTH-majority Intensely			1.221	1.362			-1.409	-5.807
Segregated Schools			(3.241)	(3.263)			(2.559)	(4.796)
Share of OBC-majority Intensely			6.678	6.734			4.212*	6.663**
Segregated Schools			(6.839)	(6.854)			(2.423)	(2.877)
Share of OTH+OBC-majority Intensely			-0.694	-0.670			1.480	2.200
Segregated Schools			(1.964)	(1.929)			(2.068)	(2.178)

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: Please note that I do not include Theil's H index in this portion as this measure was calculated for all schools in the district in the main analysis and would not change based on the type of private schools studied.

Supplementary Analysis with Staggered Rollout

As mentioned above, although the RTE Act formally came into effect in 2010–11, not all eventually treated states began implementation in 2010-11. Therefore, this section divides states into various cohorts based on their 'first year of implementation' (See Methods for details) and re-calculates the DiD and event study estimates in Table 6 and Figure 3. Figure 3 presents event study estimates using TWFE as well as methods proposed by Callaway & Sant'Anna (2021), Gardner (2021), and Sun & Abraham (2021). SA and Gardner estimates confer with TWFE results and show statistically significant reductions in OTH-SC and OTH-OBC enrollment gaps as well as the share of OTH+OBC-majority intensely segregated schools, but CS estimates are not statistically significant for any of the outcome variables. The fact that TWFE, SA, and Gardner estimates are only statistically significant in later years also indicates that cohorts that began implementation early may be driving these estimates.

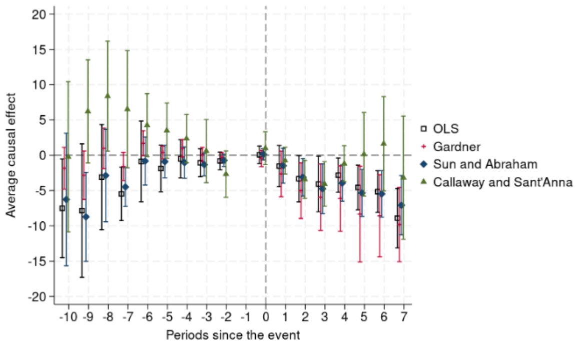
Further examination of the CS results using cohort-wise analysis (Table 6) also shows that statistically significant treatment effects are restricted to a few early-implementing cohorts. The 2011-12 cohort (Rajasthan, Uttarakhand, Chhattisgarh) and 2013-14 cohort (Maharashtra, Tamil Nadu) show reductions of 5.63 and 9.72 points in the OTH-SC gap, respectively. In addition to these two cohorts, the 2010-11 cohort (Delhi, Karnataka, Madhya Pradesh, Haryana) also shows significant reductions in the OTH-OBC enrollment gap. On the other hand, 2015-16 cohort (Gujarat) shows a significant increase in the OTH-OBC gap. No cohorts show significant reductions in the OTH-ST enrollment gap. Surprisingly, 5 out of the 7 cohorts also show statistically significant reductions in the Theil's H index, perhaps overturning earlier conclusions about treatment's impact on caste-based segregation. However, other dimensions of segregation also need to be assessed because, as mentioned above, the four-group Theil's H index used in this study equally weights diversity between SC and ST groups as it does with diversity between ST and OTH groups. The analysis of intense segregation also emphasizes the need for measuring multiple dimensions of segregation, because we observe no statistically significant (at the 5% level) reductions in the share of OTH-majority intensely segregated schools. On the other hand, the 2011-12, 2013-14, and 2015-16 cohorts experience a significant increase in such schools. Only one cohort (2015-16) shows significant reductions in the share of OBC-majority schools, which is surprising given that DiD estimates from the main analysis show statistically significant reductions in this outcome variable. On the other hand, although DiD estimates from the main analysis did not show a reduction in OTH+OBC-majority intensely segregated schools, many of the earlier cohorts show statistically significant estimates in this outcome variable (Table 6). The 2017-18 cohort (Jharkhand, Punjab) does not show any significant outcomes, while the 2014-15 (Assam, Bihar, West Bengal) and 2016-17 (Odisha) cohorts show weaker results for all outcome variables except Theil's H index. It is unclear whether the lack of significant results for the 2016-17 and 2017-18 cohorts is due to shorter implementation time or lower quality of implementation.

Table **6**: Supplementary Analysis with Staggered Rollout of Section 12(1)(c): CS Estimates by Cohort (Not Yet Treated as Control Group)

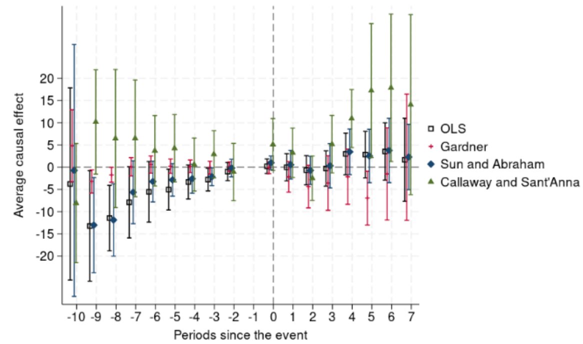
Treatment Group	(1) OTH-SC Gap	(2) OTH-ST Gap	(3) OTH-OBC Gap	(4) H Index	(5) OTH 90	(6) OBC 90	(7) OTH+OBC 90
Group	-3.280***	0.571	-1.873***	-0.015***	-0.040	-0.563*	-2.122***
Average	(0.571)	(0.915)	(0.521)	(0.003)	(0.525)	(0.325)	(0.515)
Group 2011	-2.090	-0.414	-3.508***	-0.023***	-0.681	-0.413	-3.483***
	(1.319)	(1.568)	(1.183)	(0.006)	(1.337)	(0.625)	(1.112)
Group 2012	-5.634***	-0.127	-2.502***	-0.012**	1.262**	0.072	-2.748***
	(1.106)	(1.736)	(0.875)	(0.005)	(0.641)	(0.542)	(0.642)
Group 2014	-9.724***	-2.343	-5.254***	-0.007	1.152**	-0.846	-2.454***
	(1.293)	(1.954)	(1.097)	(0.005)	(0.468)	(0.553)	(0.696)
Group 2015	-0.710	1.943	1.126	-0.025***	-2.621*	-1.267*	1.837*
	(1.064)	(1.679)	(1.063)	(0.005)	(1.505)	(0.737)	(1.095)
Group 2016	-1.270	3.343*	2.287**	-0.016***	1.349**	-2.079***	-2.244**
	(1.276)	(1.994)	(1.054)	(0.004)	(0.582)	(0.681)	(0.919)
Group 2017	-0.349	2.820	-0.117	-0.025***	-0.915	0.096	-1.504*
	(1.021)	(2.442)	(1.060)	(0.004)	(0.764)	(0.706)	(0.780)
Group 2018	0.951	4.697	1.072	0.006	0.795	-0.114	-2.569
	(1.218)	(3.000)	(1.090)	(0.006)	(1.116)	(1.166)	(1.779)
Observations	5,382	4,881	5,382	5,382	5,382	5,382	5,382

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

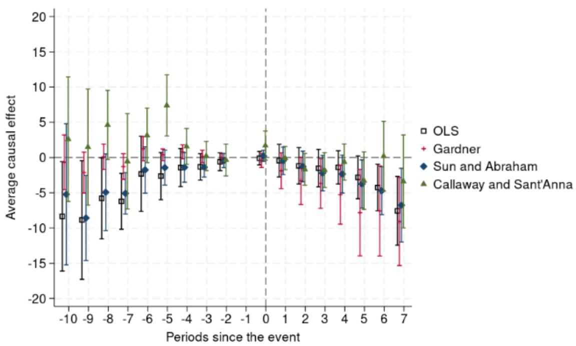
(a) OTH-SC Enrollment Gap



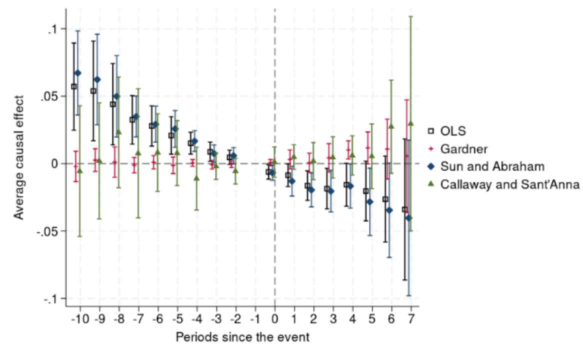
(b) OTH-ST Enrollment Gap



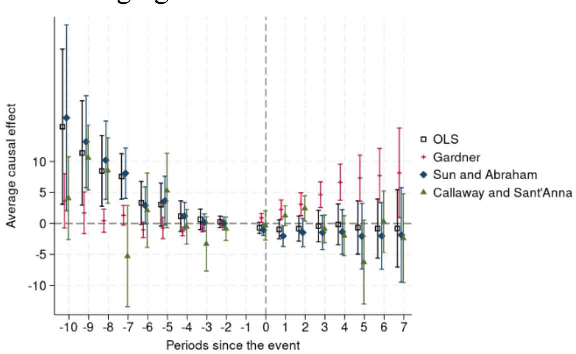
(c) OTH-OBC Enrollment Gap



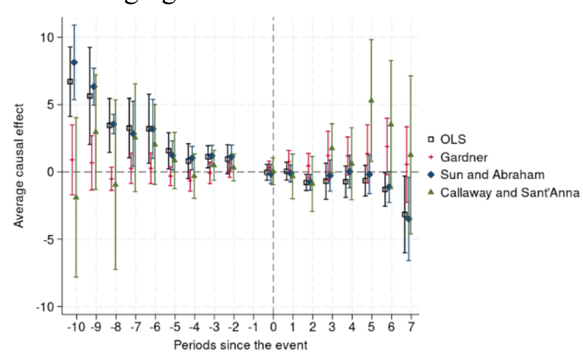
(d) Theil's H Index



(e) Share of OTH-majority Intensely Segregated Schools



(f) Share of OBC-majority Intensely Segregated Schools



(g) Share of OTH+OBC-majority Intensely Segregated Schools

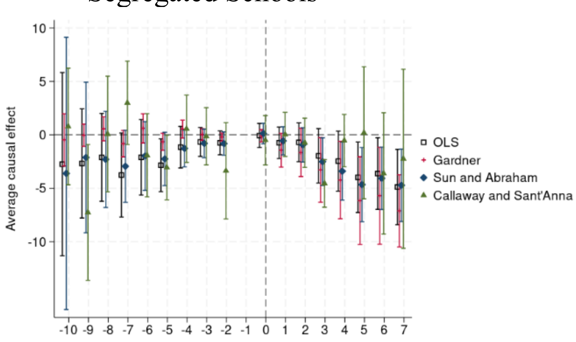


Figure **3**: Supplementary Event Study Analysis with Staggered Rollout of Section 12(1)(c)

Conclusion and Discussion

The private school sector in India is rapidly growing. Between 2007-08 and 2017-18, the number of unaided private primary schools in India and their share of total enrollment nearly doubled (Edara, 2025).[22] The growth of the private sector raises concerns about segregation across schools as students from lower-income households, students from marginalized castes, and girls attend private schools at lower rates compared to their counterparts (Azam, 2017; Chudgar & Creed, 2016). It is unclear whether higher achievement levels in private schools are because of a higher quality of education offered in these schools or a concentration of privileged students (Chudgar & Quin, 2012). However, irrespective of which mechanism is at play, segregation by caste or class can negatively impact students either through peer effects or distribution of resources or both.

In this context, I analyze the influence of Section 12(1)(c) implementation on enrollment gaps in private schools and caste-based segregation. The analysis indicates that states that have implemented the law show significant reductions in RTE-mandated private school enrollment gaps for SC and OBC students. In addition, the analyses show either weak or no influence on the evenness of student distribution across all schools and inconsistent evidence on concentration of OBC students in private schools. Most of the significant results are driven by states that implemented Section 12(1)(c) in its early years. Overall, the results related to reduced caste-based enrollment gaps in RTE-mandated private schools are encouraging, but it is unclear whether the benefits of Section 12(1)(c) extend beyond its direct beneficiaries, i.e., in promoting social integration. Irrespective of variations across models and methods, two sets of results are consistent. Implementing states do not show significant reductions in the OTH-ST enrollment gap or the share of OTH-majority intensely segregated schools. Few models even indicate an increase in the share

[22] See Tables 1-2 and 1-3.

of OTH-majority intensely segregated schools. These results indicate that Section 12(1)(c) is not likely improving access for the most marginalized group of students (i.e., ST students) and not likely disrupting elite schools in the country. It also suggests that the policy may not be effectively addressing entrenched patterns of segregation—particularly the continued ability of elite families to maintain exclusive schooling spaces.

The results can be situated within the broader literature on Section 12(1)(c) impacts. The reduction in caste-based enrollment gaps for SC and OBC students, but not for ST students, can be reconciled with prior evidence that relatively better off students are more likely to benefit from this program (Dongre et al., 2018; Romero & Singh, 2022). However, lack of data on the economic status of students is a significant limitation of this study that precludes analysis of dynamics within SC and OBC groups. Future research on this topic must also explore economic, gender-based, and religion-based segregation in private schools.

In considering the impact of Section 12(1)(c), it is also important to recognize the vast diversity among private schools in the quality of instruction and resources offered. Agarwal (2024) shows that beneficiaries who attended higher-quality private schools show significantly higher achievement in English language test scores. At the same time, Dongre et al. (2018) reports that Section 12(1)(c) applicants are hesitant about applying to higher fee-charging or elite private schools. In this vein, the current study finds that districts implementing Section 12(1)(c) showed no reduction, and in some cases showed increases, in the share of schools where OTH students form a 90% majority. This finding supports earlier studies suggesting that elite schools may not be participating in the affirmative action program or that more self-segregated schools may be propping up as a reaction to Section 12(1)(c) implementation.

Finally, prior research results showing that a large percentage of Section 12(1)(c) applicants who lose the lottery also enroll in private schools as fee-paying students (Damera, 2017; Romero & Singh, 2022) underscore the importance of analyzing Section 12(1)(c)'s impacts beyond its direct beneficiaries, which was attempted in this study. Section 12(1)(c)'s limited impacts on caste-based segregation raise questions about its ability to promote social integration. In the U.S., voucher and charter school programs, especially in non-metropolitan areas, have been associated with increased segregation and reduced hiring in nearby schools (Cordes & Laurito, 2023; Jackson, 2012; Kotok et al., 2017; Monarrez et al., 2022; Rich et al., 2021). The results of the current study are not sufficient to draw broad conclusions regarding whether, or in which areas, Section 12(1)(c) may follow a similar trajectory as similar programs in the U.S. However, it encourages discussion on the broader impacts of Section 12(1)(c). Policymakers must be cautious about encouraging private school growth using government resources, especially if it redirects investment away from government schools while the relatively-privileged students exit the public school system.

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