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Essays in Development Economics

A Dissertation submitted in partial satisfaction of the
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in

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by

Arushi Kaushik

Committee in charge:

Professor Prashant Bharadwaj, Chair
Professor Karthik Muralidharan, Co-Chair
Professor Jeffrey Clemens
Professor Julie Cullen
Professor Craig McIntosh

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The Dissertation of Arushi Kaushik is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

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DEDICATION

I would like to dedicate this dissertation to my grandparents, *Baa (Smt. Rajesh)* and *Papaji (Sh. Parmeshwar Das Sharma)*, who encouraged me to pursue doctoral studies.

I would also like to thank my husband, *Ashwini Kumar Vashisht*, and family in India, who have been a perennial source of support through this journey and provided me with the courage to persevere through the tough times.

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VITA

2011 BA (Hons) Economics, University of Delhi, India

2013 MA Economics, University of Delhi, India

2013-2016 Assistant Professor, University of Delhi, India

2017-2021 Teaching Assistant, University of California San Diego

2022 Doctor of Philosophy, University of California San Diego

PUBLICATIONS

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ABSTRACT OF THE DISSERTATION

Essays in Development Economics

by

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Professor Karthik Muralidharan, Co-Chair

Chapter 1 studies the effect of reducing class-size, by hiring more tenured teachers, on students' scholastic performance. Using a maimonides' rule-kind of instrumental variable strategy and a longitudinal panel data from India, I find that a 10% reduction in pupil-teacher ratio (PTR) results in 0.02σ improvement in value-added scores. However, a cost effectiveness calculation suggests that reduction in PTR, by hiring more regular teachers, entails cost-benefit ratio of 2.8, meaning that for every INR 100 spent on hiring a regular teacher, the student gains only INR 35

in future income. Thus, despite of positive impact on student learning, it is an economically costly program.

Chapter 2 examines the long-term health and educational effects of the Bhopal Gas Disaster (BGD), one of the most serious industrial accidents in history, using geolocated data from India's National Family Health Survey-4 (NFHS-4) and the 1999 Indian Socio-Economic Survey [NSSO-1999]. A spatial difference-in-differences strategy estimated the relative effect of being in utero near Bhopal relative to other cohorts and to those further from Bhopal. We find that men who were in utero at the time were more likely to have a disability that affected their employment 15 years later, and had higher rates of cancer and lower educational attainment over 30 years later. These results indicate social costs stemming from the BGD that extend far beyond the mortality and morbidity experienced in the immediate aftermath.

In Chapter 3, I combine a novel dataset of >1 million patent applications from India with the firm level R&D expenditures to study the impact of weak Intellectual Property Rights (IPR) on innovation activities. With the announcement of TRIPS, the technology classes that were allowed to patent only the **process** innovations (a weak form of IPR) before, started showing uptick in innovation activities both in terms of patent counts and industry-level R&D expenditures, compared to technology groups that were already enjoying more *secured* **product** patenting provisions. Decomposing the industry-level impact reveals that the incremental innovations by smaller firms that require modest sums of R&D have been replaced by more expensive R&D projects by larger firms, following the adoption of TRIPS.

CHAPTER 1

Class-Size Effects in Developing Countries: Longitudinal Evidence from India[1]

Arushi Kaushik[2]
UC San Diego

Karthik Muralidharan[3]
UC San Diego

Abstract

This paper presents evidence on the negative effects of class-size on students' scholastic performance (as measured by value-added scores) in the context of developing countries. Using a unique annual panel dataset from 500 Indian schools and 73,000 students and applying a maimonides' rule[4] - kind of instrumental variable strategy, we find that a 10% reduction in pupil-teacher ratio (PTR) leads to a 0.02σ improvement in value-added scores. An attempt to replicate the results using test score levels- an oft-used outcome variable in the literature- leads to overestimation of the returns to class-size. This happens because test score level is a stock variable that captures the cumulative effect of all the previous investments made in the child. To the extent these investments tend to be highly correlated within a cohort in a school across years, a school fixed effect strategy removes this bias and gives us the same point estimates for both outcome variables. A cost effectiveness calculation suggests that reduction in PTR, by hiring more regular teachers, entails a cost-benefit ratio of 2.8, meaning that for every INR 100 spent on hiring a regular

[1] The authors thank the AP RESt team and Azim Premji Foundation who supported the collection of data used in this paper. Financial assistance for the project has been provided by the Government of Andhra Pradesh, the UK Department for International Development (DFID), the Azim Premji Foundation, and the World Bank. However, the findings, interpretations and conclusions expressed in this paper are those of authors and do not necessarily represent the views of any organization that helped in data collection.

[2] UC San Diego. Email: arkaushi@ucsd.edu
[3] UC San Diego, NBER, BREAD, and J-PAL. Email: kamurali@ucsd.edu
[4] Since the administrative rule, that is used as IV, mandates pupil teacher ratio (PTR) only at the school-level, our estimates are de-facto measuring the impact of school's average class-size on student performance.

teacher, the student gains only INR 35 in future income. Thus, despite of positive impact on students' test scores, it is an economically costly program. Higher test score gains have been achieved by other education interventions in the same setting at much lower costs.

JEL code: O15, I2, I210, I250

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1 Introduction

Huge expansion in school enrollment in developing countries since 2000s has been accompanied by massive expansion in public expenditure on education. Teachers' salaries constitute a major chunk of this budget (≈ 90% of non-capital expenditure in our sample), thus making class-size norms an important determinant of the cost of publicly provided education. For instance, in India, the Right to Education Act, 2009 brought down the targeted pupil-teacher ratio (PTR) from 40:1 to 30:1 thus, entailing an additional cost of Rs 35,000-40,000 crores per year[5] ($7.78-8.89 billion).

Despite of its importance, there is surprisingly little well-identified evidence on the effect of class-size on student achievement for developing countries. This lack of evidence arises from two important constraints -the difficulty of running experiments with regular civil service teachers and the lack of data. The best experimental estimates of reducing pupil teacher ratios (PTR)[6] come from Duflo et al. (2015) who experimentally vary the provision of a (locally-hired contractual) teacher and estimate the effect of PTR on scholastic achievements of students who are left behind

[5] Government expenditure on elementary education went up from Rs. 80313.91 crores in 2008-09 to Rs. 128595.63 crores in 2010-11 (MHRD, Educational Statistics at a Glance, 2012,2013). Assuming 80% of the budget is spent on teacher salaries (Accountability Initiative, 2016-17), Rs. 38,625.376 crore increase in budget can be attributed to teacher salaries.

[6] Throughout this paper, we will be using the terms- class-size and PTR- interchangeably.

with regular civil service teachers. These indirect estimates, of reducing PTR using regular teachers, are likely to be biased if experiment induces re-optimization of efforts and tasks between the two types of teachers within the school[7] as well as if there is an imperfect compliance with randomization protocol where students get re-allocated amongst teachers[8].

On the other hand, Urquiola (2006) uses observational data from Bolivia to study the same question. However, he uses cross-sectional variation to analyze the effect of class-size on student *test score levels* which reflect the *cumulative* effect of all the past investments, that have gone into improving the child's human capital, and not just the effect of *contemporaneous* inputs like class-size. A conceptually more appropriate outcome variable to study in this context is *value-added scores* that control for students' prior performance. The students' lagged test-scores provide a good summary measure of the effect of various educational inputs previously invested into the child, while also acting as a proxy for child's inherent ability. Thus, controlling for lagged test scores allows us to analyze the effects of contemporaneous investments while parsing out the effects of previous ones. The distinction between value-added scores and test score levels is especially important for the class-size question since the latter is a *flow* variable. From the cost-effectiveness perspective, it is important to compare a *flow* (cost of an additional class) with a *flow* (value-added scores) instead of a *stock* variable (test score levels).

For the first time, using an annual panel dataset from 500 schools and 73,000 students, this paper looks at the effect of class size on students' scholastic achievement as measured by *value-added scores*. We use Maimonides' like administrative rule as an instrumental variable (IV) to study this question. We replicate the results using a school fixed effects (FE) strategy that exploits

[7] This may happen if, e.g, the regular teachers are now spending more time in mentoring the contractual teachers than on teaching in classroom.
[8] This could happen, for instance, if parents insists on putting their child in specific classrooms where their friends are or with specific teachers.

a within-school variation in class-size across years instead of a cross-sectional variation of IV strategy. This is in line with the growing literature (Chetty et al. (2014); Deming et al. (2014); Singh (2015); Angrist et al. (2017)) that shows congruence between value-added estimates and quasi-experimental estimates.

We find a remarkably consistent negative effect of PTR on students' value-added scores from both school FEs (0.0185) and IV (0.0196) strategies. The point estimates suggest that a 10% reduction in PTR by hiring more regular teachers can lead to ≈0.02 σ improvement in test scores. This is almost 1/10th of the class-size effects reported by Angrist and Lavy (1999) for Israel and less than half of those for Bolivia (Urquiola (2006)). Thus, the downside (in terms of adverse effects on student test scores) of having bigger classrooms appears not to be as high in India as in other developing and developed countries.

In monetary terms, hiring a regular teacher reduces the PTR by 20%, generates a test score gain of 0.04σ and leads to additional annual future earnings worth INR 13,540 per student. However, since a regular teacher is a tenured government employee who cannot be removed before her retirement, her salary is not a one-time cost. The PDV of the cost of hiring a regular teacher turns out to be INR 30,690 per student. Even under a wide range of scenarios, the cost-benefit ratio lies above 1 and its mean value lies between 2.5-2.8. This means that for every INR 100 spent on hiring a regular teacher, the student earns only INR 35-40 in additional future income. Thus, despite of improvement in student learning, the test score gains are not economically significant enough to justify the costs of hiring regular teachers.

The existing literature has focused on understanding the effect of class-size on *test score levels*. As discussed before, these test-score levels capture the cumulative effect of all the past human capital investments in the child and not just the impact of contemporaneous class-size. In fact, when we re-run the analysis without controlling for lagged test-scores, the IV estimate of class-size turns out be ≈ 0.4-0.5- about twice the estimate from the *value-added* specification. Since the class-size (and other school investments) tend to be highly correlated within a cohort across school-years (≈ 0.7 in our sample), a school FE that absorbs the time invariant school-specific factors, can effectively control for these previous investments and can give us unbiased estimate of the contemporaneous class-size irrespective of whether we control for lagged test scores. This is what we find in our data. For either of the outcome variables- *test score levels* or *value-added scores*, the school FE strategy gives us a similar estimate of -0.2.

Our results are not sensitive to the functional form in which the main policy variable- *PTR* is introduced in estimating equations. Whether we use it linearly or in logarithmic form, the above results remain qualitatively same. However, since reducing PTR from 60 to 56 and from 20 to 16 should have differential effects on student performance, we prefer to use *ln(PTR)* to calculate the semi-elasticity of student test scores with respect to PTR.

Moreover, our measure of class-size, *PTR*, does not the confound the peer composition effect. Introducing additional controls for the socio-economic characteristics of the student body, e,g, proportion of students belonging to disadvantaged castes- Schedules Caste (SC) and Scheduled Tribe (ST) and the average household affluence of the students attending the school- does not change the point estimates on the class-size effect.

Introducing different functional forms of school enrollment in IV estimation, like the piecewise linear trend, reduces the precision of the estimate but not the magnitude. It should be noted that controlling for too flexible a functional form of enrollment would render our IV weak since such functional forms would start closely mimicking the logarithm of our IV- the administrative-rule predicted PTR[9], apart from the fact that statistical inferences for such specifications are often misleading (Gelman and Imbens (2019)). This is indeed what we find when we control for a quadratic in enrollment. Likewise, the point estimate of class-size effect when estimated on a smaller sample of schools having enrollments within the range of +/- 5 students of the thresholds, are exactly similar to our IV estimates but more nosier.

Our paper contributes to the limited literature in developing countries that looks at the effect of class-size on student performance. As noted before, the best estimates from low incoming countries are based on Urquiola (2006)'s study in Bolivia that exploits a variant of Maimonides' rule to look at the effect of class-size on *test score levels* which confound the effect of previous school investments into the child. The second set of results, based on indirect estimates of reducing *PTR* with regular teachers, come from Duflo et al. (2015), where students were randomly assigned to contractual and regular teachers. However, adherence to such randomization protocol is rarely perfect. In addition, any endogenous re-allocation of effort between these two types of teachers, after randomization, can confound the estimates.

We also speak to the vast education literature that study the same question in developed countries using *test score levels* as outcome variable (see, e.g.Angrist and Lavy (1999), Hoxby (2000), Dobbelsteen et al. (2002), Bandiera et al. (2010), Angrist et al. (2019)). To the best of our knowledge, this is the first paper studying the effect of class-size (a *flow* variable) on value-added scores (a *flow* variable), instead of test score level (a stock variable).

Finally, we contribute to a small yet growing literature on the cost effectiveness of various education interventions. Muralidharan and Sundararaman (2013) and Muralidharan and Sundararaman (2011) study the cost effectiveness of two such interventions in India- provision of

[9] In fact, the Kleibergen-Paap F-statistics (Kleibergen and Paap (2006)) for first stage falls rapidly as soon as higher order polynomials of school enrollment are introduced.

an extra contract teacher and linking teachers' pay to improvements in student performance, respectively. The present paper at- tempts to understand the cost effectiveness of reducing *PTR* via hiring more regular teachers.

The next section (Section 2) lays out the context and describes the data. Section 3 discusses the two empirical strategy- IV and School FEs. Section 4 presents the results from both the identification strategies and some robustness checks. Section 5 concludes.

2 Context

India has prioritized universalization of elementary schooling (for children in age group 6-14 years old) since early 1990s. A major component of various schemes[10], geared towards achieving this goal, has been norms on pupil-teacher ratios (PTR). The Sarva Shiksha Abhiyan (SSA) stipulated a PTR of 40:1[11] while ensuring that small schools (those with enrollment of less than 80) have a minimum of two teachers. The compulsory schooling law- the Right to Education Act (RTE), passed in 2009 and implemented in 2010[12], decreased this ratio to 30:1.

This paper uses data from the Indian state of Andhra Pradesh. With a population of 80 million, Andhra Pradesh is the 5th largest state in the country but compares similarly to all-India average on various development indicators, like gross enrollment in primary school, literacy and infant mortality, and most importantly on measures of service delivery such as teacher absence (Kremer et al. (2005)).

[10] The District Primary Education Programme (DPEP) was launched in 1994, later replaced by Sarva Shiksha Abhiyan (SSA) in 2001. Currently, SSA is a major vehicle to implement the objectives of Right To Education Act (RTE), 2009.
[11] Note that these PTRs are stipulated at school level, i.e, they specify number of teachers for a fixed level of student enrollment. While school-level PTR directly affects the average class-size, this effect may also get modulated by the probability of being put in a multi-grade classroom especially in smaller schools. However, in this paper, the terms-school PTR and class-size are used interchangeably to remain consistent with global literature on education but one should keep in mind that the estimates presented in the subsequent sections represent the reduced form effects of school PTR on student achievement.
[12] Since the law got implemented only in 2010, we will be using school PTR norms of 40:1, as laid down under SSA, given that our dataset runs from academic year 2005-06 to 2009-10.

2.1 Data

The data was collected as part of AP RESt (Andhra Pradesh Randomized Evaluation Studies)- a series of experimental studies that were conducted to understand the effectiveness of various inputs- and incentives- based school interventions in improving student test scores. It entails an annual panel dataset of 500 schools and 73,000 primary school (class1-5) students who were given independent assessments on maths and language (Telugu). The baseline assessment was conducted in June- July, 2005 and thereafter annual assessments were conducted for next 5 years, in March-April of the corresponding academic years to capture the end-of-year performance, till 2009-10. Scores are normalized (such that they have mean 0 and std dev 1) within each grade, subject and year of assessment.

The literature on class-size, both in developing (Urquiola (2006)) and developed countries (Angrist and Lavy (1999), Hoxby (2000) etc), have thus far used *test score levels* to study the effect of class-size which may be confounded since cohorts within a school receive similar investments over time. This emphasis on *test score levels* as the outcome variable is a result of lack of availability of student-level panel datasets. The annualized panel dataset of AP RESt allows us the unique opportunity to conduct the analysis for the first time in value-added form, thus purging the estimates of class-size effects of any past school investments. Moreover, since these assessments were created, administered and graded externally by an independent agency, there is a greater degree of confidence in the validity of these scores as measures of learning since they are free from teacher subjectivity which might crop- in if assessments are conducted by the government using the usual school-apparatus.

For the purpose of this study, we restrict our sample to schools that had a regular teacher (RT) throughout the year. The number of observations contributed by each school-year are shown in Table 1.11. The business as-usual way of achieving class-size norms is via hiring regular teachers till targeted PTR is met or by re-deploying the regular teachers to schools that witness higher or lower enrollment than planned. However, the administrative rules lays down detailed criteria regarding movement of teachers across schools with teachers getting transferred only every 3 years in AP. Likewise, rigid hiring procedures may make it difficult to hire new regular teachers just on-time to meet the needs of the impending academic year. Hence, often schools get contractual teachers to fill temporary position till they get a regular teacher. These contractual teachers differ from regular teachers on various dimensions (see Muralidharan and Sundararaman (2013) for details). However, schools hiring these contractual teachers do not differ systematically from those that have only regular teacher. In fact, as Table 1.2 shows the two types of schools are similar in terms of their student body (the socio-economic status of the households of students and average proportion of disadvantaged groups[13]) and school characteristics like infrastructural facilities and proximity to various public goods like roads, banks, post-office, bus-stop etc. The average PTR in both types of schools is 29:1. While non-RT schools seem to have higher school enrollment, they tend to compensate it by hiring more contractual teachers (hence, have a higher number of overall teachers) than RT-only schools!

The summary statistics for our sample schools are shown in Table 1.1. A typical school in this sample is small with a mean enrollment of 93 students. With average number of teachers being 3 per school, there are not sufficient number of teachers to be assigned separately for each grade. Consequently, approximately 50% of students are placed in classrooms with students from other grades, resulting in multigrade classrooms. A quarter of our sample consists of students from

[13] Here, by disadvantaged groups, we mean Scheduled Caste (SC) and Scheduled Tribe (ST).

disadvantaged groups- Scheduled Caste (SC) and Scheduled Tribe (ST) and about 50% of them are males.

3 The Empirical Model

Following Todd and Wolpin (2003), student scholastic achievement is modeled as a linear function of current and past school inputs. In particular, we will focus on a specific school input- the class size, as captured by pupil-teacher ratio. Student i's test scores in year (t) is given by:

$$Y_{ikt} = \beta_0 + \beta_1 ln(PTR)_{k,t} + \beta_2 ln(PTR)_{k,t-1} + \ldots\ldots + \beta_t ln(PTR)_{k,1} + \beta_X X_{ik} + \varepsilon_{ikt} \quad (1)$$

where, Y_{ikt} are test scores, for student i in school k and year of assessment (t). $PTR_{k,t}$ captures school-level pupil-teacher ratio. X_{ik} includes other controls. ε_{ikt} is, by definition, orthogonal to all current and past class-sizes and X_{ik}.

In the absence of data on all the previous class-sizes, estimating Equation 1 will bias the returns from the current class size (β_1):

$$Y_{ikt} = \alpha_0 + \alpha_1 ln(PTR)_{k,t} + \alpha_X X_{ik} + e_{ikt} \quad (2)$$

where the error term, $e_{ikt} = \varepsilon_{ikt} + \beta_2 ln(PTR)_{k,t-1} + \ldots\ldots + \beta_1 ln(PTR)_{k,t}$.

If, for any $n \in \{1,2,\ldots,t-1\}$, $corr(PTR_{k,t}, PTR_{k,n}) \neq 0$, then e_{ikt} will be correlated with $ln(PTR)_{k,t}$, thus, biasing α_1:

$$\alpha_1 = \beta_1 + (Z'Z)^{-1}.\sum_{n=1}^{t-1} \beta_n cov(\ln(PTR)_{k,t}, \ln(PTR)_{k,n}).$$

where $Z = (ln(PTR),X)$. In general, class size tends to be highly correlated within a school-cohort across years, as can be seen in Columns (1) and (2) of Table 1.3. Hence, any omission of previous class-sizes will bias the measured effect of the contemporaneous class-size.

One way to correct this omitted variable bias is by including lagged test-scores (Y_{ikt-1}):

$$Y_{ikt} = \gamma Y_{ik,t-1} + \theta_1 ln(PTR)_{k,t} + \theta_X X_{ik} + v_{ikt} \qquad (3)$$

If the following conditions are satisfied, then θ1 is an unbiased estimator of β_1.

1. $\beta_n = \gamma\beta_{n-1}$
2. $\varepsilon_{ikt} - \gamma\varepsilon_{ik,t-1}$ is iid (independent and identically distributed)
3. v_{ikt} is uncorrelated with the contemporaneous class-size ($ln(PTR)_{k,t}$)

The first two are the standard assumptions of the value-added models (see Todd and Wolpin (2003)). To ensure that the last assumption is satisfied, we estimate Equation 3 using an IV and a school FE strategy, as discussed below.

3.1 Empirical Strategy: OLS Specification

Equation 3 forms the basis of our baseline OLS specification:

$$Y_{ijkm,t} = \gamma_j Y_{ijkm,t-1} + \beta_1 ln(PTR)_{k,t} + \varepsilon_{ijkm,t} \qquad (4)$$

where, $Y_{ijkm,t}$ are normalized test scores, for student i, grade j, school k and mandal or block m, relative to the distribution of scores across schools in the same grade, subject and year of assessment (t), where $t \in \{1,2,3,4,5\}$. Y_0 refers to baseline test scores for assessments conducted in June-July, 2005. $Y_{ijkm,t-1}$ are lagged normalized test scores that gives Equation 4 a value-added form. These are set to 0 for students in grade 1 as they do not have baseline test. $PTR_{k,t}$ captures school-level pupil-teacher ratio. SEs are clustered at school level.

The main challenge in interpreting β_1 as the causal estimate of $ln(PTR)$ on value- added scores is the concern that there could be school-, parental-, student-level characteristics that could be correlated with both student learning trajectories as well as *PTR*[14]. We therefore augment Equation 4 with a full set of school and household controls X_{ijk} and then with district (α_d) fixed effects. The school controls include an infrastructure index and proximity index (that measures the proximity to various public transport systems). Household controls include household socio-economic status, parental literacy, student gender and caste[15].

However, there might still be concerns about other omitted variable biases. Therefore, in the next section we estimate Equation 4 with a Maimonides' rule-kind of instrumental variable strategy.

3.2 Instrumental Variable (IV) Strategy: Maimonides' Rule

We can exploit the administrative norms on PTR to use Angrist and Lavy (1999) kind of Maimonides' Rule as an instrument for actual class-size. The administrative rules target a PTR of 40:1 in big schools while small schools (with enrollment of less than 80 students) are assigned a minimum of two teachers. Thus, we can use school enrollment numbers to predict what the school-level PTR would look like if administrative rules are followed strictly which in turn can be used as an IV for actual PTR.

The first-stage therefore amounts to estimating:

$$ln(PTR)_{k,t} = \alpha_0 + \delta_1 ln(PTR^*)_{k,t} + \delta_X X_k + f(E_{k,t}) + \varepsilon_{k,t} \quad (5)$$

[14] Note that because our main policy variable- PTR- is measured at school-level. The endogenous re-allocation of students that take place within school, when students are sorted across classes, is not a main concern here.
[15] See Table 1.1 for more details.

where, $ln(PTR^{*})_{k,t}$ = school-level PTR as predicted by enrollment $E_{k,t}$ in year t. The procedure for making this prediction exploits the administrative rules that determine the allocation of number of teachers on the basis of school enrollment numbers.

The number of teachers assigned per school as function of enrollment is given by:

$$T_{k,t} = \begin{cases} 2, & E_{k,t} \leq 80 \\ int\left(\frac{E_{k,t} - 1}{40}\right) + 1, & E_{k,t} > 80 \end{cases}$$

Consequently, following this allocation rule, the predicted school-level PTR is:

$$PTR *_{k,t} = \begin{cases} \frac{E_{k,t}}{2}, & E_{k,t} \leq 80 \\ \frac{E_{k,t}}{nt\left(\frac{E_{k,t}-1}{40}\right)+1}, & E_{k,t} > 80 \end{cases} \qquad (6)$$

Figure 1.1 plots the actual and the administrative-rule predicted PTR against enrollment. According to Figure 1.1, the relationship between the actual and the predicted PTR appears stronger for schools with enrollment levels ≤ 200. Therefore, we will present IV results for this sub-sample only.

The class-size predicted by Maimonides' rule seems like a good IV for actual class- size. It satisfies both the conditions of an ideal instrument: relevance and validity. The relevance of the instrument, as formally tested in the first-stage, is discussed in subsection 4.1. Here we will discuss the validity of the instrument. Intuitively, the administrative rules should not have any independent effect on the value-added scores unless they are encouraging school principals and administrators to manipulate the enrollment numbers in such a fashion so as to obtain an additional teacher even before they hit the thresholds. However, manipulating the total enrollment numbers at the school-level is much harder than manipulating grade-level enrollments. Schools cannot turn away kids seeking admissions and teacher appointments are administratively made at the block- or district-level. The data too does not provide any evidence of manipulation of enrollment around thresholds (Figure 1.2).

Therefore, we proceed by using the predicted *ln(PTR)*, $\ln(\widehat{PTR})_{k,t}$ from Equation 5 as instruments in the second stage:

$$Y_{ijkm,t} = \gamma_j \, Y_{ijkm,t-1} + \beta_1 \ln(\widehat{PTR})_{k,t} + f(E_{k,t}) + \beta_2 X_{ijk} + \varepsilon_{ijkm,t} \qquad (7)$$

where X_{ijk} are the controls for time-invariant school and parental factors (like infrastructure, proximity indices, parents' literacy, household's affluence index, student gender and caste) and $f(E_{k,t})$ is a flexible function of enrollment, to control for the direct effects of enrollment (e.g., via funding made available to schools on the basis of enrollment numbers). In the main specification, we control for enrollment linearly. In robustness checks, we show the results controlling for a quadratic and the linear trend in enrollment as in Angrist and Lavy (1999). SEs are clustered at school-level.

β_1 is the 2SLS/IV estimate of the effect of *ln(PTR)* on value-added scores. In other words, a 10% increase in *PTR* leads to a $\frac{\beta_1}{10}$ σ improvement in *value-added scores* and not the *test-score levels* -the outcome variable studied in the literature so far. For comparison, we will also show the results without controlling for lagged test-scores $Y_{ijkm,t-1}$ in Equation 7. For simplicity, henceforth, we will call empirical specification like Equation 7 that controls for past test scores as *value-added specification* and the one that does not control for lagged scores as *level specification.*

As discussed in section 3, if we do not control for previous scores, past school in- vestments into the child do not get accounted for and the class-size variable, $PTR_{k,t}$ picks up the cumulative effect of all such previous investments, thereby biasing the coefficient of interest, β_1.

3.3 School Fixed Effects (FE) strategy

Recent work shows that panel data estimates are similar to experimental/quasi-experimental estimates (Chetty et al. (2014); Deming et al. (2014) ; Singh (2015); Angrist et al. (2017)). Therefore, we can use an alternative identification strategy based on school FE- to estimate the effects of class-size on student achievement.

The value-added specification, in this case, is as follows:

$$Y_{ijkm} = \gamma_j Y_{ijkm,t-1} + \beta_1 ln(PTR)_{k,t} + \beta_2 X_{ijk} + \alpha_k + \varepsilon_{ijkm,t} \qquad (8)$$

where, all other variables are as explained before and αk are school fixed-effects. The identifying variation in *PTR* now comes from variation within the same school over time which is driven by changes in cohort sizes over time and teacher transfers in and out of schools[16]. This specification eliminates concerns of unobserved heterogeneity across schools. SEs are clustered at school-year level.

One concern in interpreting β_1 in Equation 8 as causal effect of class-size is the fact that there might be endogenous movement of students across schools in response to previous year's performance of the student body of that school (as measured by lagged scores). To allay such concerns, in Table 1.4, we formally test if school PTR (Columns (1) and (2)) and enrollment

[16] Figure 3 shows the variation in PTR with- and without controlling for school FEs.

(Columns (3) and (4)), respectively, are responding to past test scores. As the tables suggest, there is no statistically significant correlation between lagged scores and school size. This intuitively makes sense since administrative rules restrict the admission in local primary schools to the households living in the vicinity, so there is limited room for shopping around for public schooling.

Like before, we will also show results from the level specification of Equation 8, without controlling for lagged scores $Y_{ijkm,t-1}$:

$$Y_{ijkm,t} = \alpha_1 ln(PTR)_{k,t} + \alpha_X X_{ijk} + \alpha_k + u_{ijk,t} \qquad (9)$$

As discussed in section 3, omission of past scores in estimating equation can lead to an omitted variable bias due to the past school investments into the students. Since these investments, including class size, tends to be highly correlated within a cohort in school over years, controlling for school FEs can absorb the time invariant school-specific component of *PTR* such that any year-on-year variation left in class- size is purely random (as seen in Columns (3) and (4) of Table 1.3). Since the new error term, $u_{ijk,t}$ is uncorrelated with $ln(PTR)_{k,t,}$ α_1 gives an unbiased estimate for β_1, without even controlling for lagged test scores! Consequently, the estimates of returns to class-size should not be different in the value-added and level specifications under the school FE identification strategy.

4 Results

Table 1.5 presents OLS results from Equation 4. Columns (1) and (2) do not include any fixed effects and indicate that a 10% reduction in PTR is associated with ≈0.023-0.025σ improvement in value-added scores. Since the school and household controls are available for a subset of student observations, we present separately the results with- and without- controls. Quite assuringly, the results are robust to inclusion of controls. Even introducing district and mandal FEs, in columns (3)-(4) and (5)-(6) respectively, reduce the point estimate of β_1 only slightly and it remains statistically indistinguishable from -0.2[17].

However, as noted before, these results might be a consequence of some omitted variable biases that are correlated with both class-size and student performance. Therefore, in next section, we present results from the IV strategy discussed in subsection 3.2.

4.1 IV Results

The first-stage results for IV are presented in Table 1.6 for the sub-sample of schools with 200 or less enrollment. The PTR predicted by the administrative rule explains about 70% of the variation in actual PTR, confirming that the administrative rule of prescribing a PTR of 40:1 is a relevant IV for actual PTR. The Kleibergen-Paap F-stat is high as well, suggesting that Maimonides' rule is not a weak instrument. The second stage results are presented in Table 1.7. The IV estimate of β_1 is around -0.2, i.e, a 10% fall in PTR brings about 0.02-0.025σ increase in value-added scores. (Table 1.7)[18].

The IV specification exploits the cross-sectional variation in actual PTR induced by the administrative rule via causing fall in actual PTR around the threshold enrollments. In the absence of any control for past school investments, our main policy variable of interest, PTR, tends to pick up not just the effect of current class-size but also the effect of previous school inputs. This happens because these past school investments tend to be highly correlated within cohorts across school-

[17] p-value for H0: $\beta 1 = -0.2$ is 0.00.
[18] Using a bootstrap procedure that preserves the error structure, we in fact tested the hypothesis that the gains in test-score levels is as high as twice that of value-added scores (${\beta_1}^{level} = 2{\beta_1}^{VA}$). The p-value (= 0.877) indeed turned out to be insignificant, which means that the estimate of β_1 using test-score level is biased upwards.

years as shown in the first two columns of Table 1.3. Thus, omission of lagged test scores (as is done in Columns (5)-(8) of Table 1.7) produces a biased IV estimate of β_1.

Similar results are found when we run the analysis subject-wise in Table 1.8. The IV estimate of β_1 is smaller for specifications that control for lagged test scores than those that do not.

4.2 FE Results

Column (1) of Table 1.3 suggests that previous class-size is highly predictive of cur- rent class size. And this statistically significant correlation does not go away even if we control for various school and household/student characteristics (as shown in Column (2)). However, since class-sizes tend to be highly correlated for a cohort across years within a school, as long as our regression specifications have school FEs to absorb this time-invariant part of cohort-specific class-size, such correlation between previous PTR and current PTR can be taken care of, which is what we find in Columns (3) and (4).

An important implication of the above discussion is that the OVB arising due to omission of lagged test scores in subsection 4.1 is automatically taken care of if we use within school variation, as is the case with school FE strategy. Therefore, both the value-added and level specification in this case should give us similar results, which is indeed what we find in Table 1.9. In both specifications (Columns (4) and (8)), the point estimate of β_1 is close to -0.2[19]- which is similar to what the value- added specification in IV framework gave us[20].

[19] p-value of test, H_0: ${\beta_1}_{level}^{FE}$= ${\beta_1}_{VA}^{FE}$= is 0.6345.

[20] A formal test of hypothesis: ${\beta_1}^{IV}$= ${\beta_1}^{FE}$ using bootstrap procedure yields a p-value of 0.943.

The point estimates for β_1 are again close to -0.2 when the analysis is done separately for maths and language scores in Table 1.10. Both value-added and level specifications are showing similar results.

4.3 Robustness Checks

4.3.1 Linear PTR

To compare our estimates of the effect of class-size on students' scholastic achievement with existing literature, we re-run the analysis using PTR linearly instead of *ln(PTR)* and instrumenting it with *PTR**, i.e, the *PTR* predicted by the Maimonides' rule. The results from IV strategy are presented in Table 1.13.

Since the previous literature has used test-score levels as the outcome variable, the appropriate Columns to look at are (3) and (4). The point estimate of class-size effect is ≈ -0.02 in the sub-sample of school with enrollment ≤ 200. These estimates are almost 1/10th of those reported in Angrist and Lavy (1999). But Angrist and Lavy (1999) comes from Israel- a middle income country which is relatively more developed than the country studied here. Therefore, a more appropriate comparison would be with Urquiola (2006) who looks at the same outcome variable but for Bolivia.

Urquiola (2006) finds that 1σ increase in class-size (which amounts to having 8 more classmates) leads to a 0.3σ decline in student test-score levels. A similar magnitude of change in class size in the Indian context would mean that test scores fall by 0.06-0.105σ- much lower than Urquiola (2006) estimates. Thus, it seems like that the cost (in terms of worsening student test scores) of having bigger classrooms in India is not as high as in other developing and developed countries.

As seen for *ln(PTR)* case, the IV strategy over-estimates the return from class-size if we use *level specification* instead of *value-added* one while the school FE strategy throws similar results under both specifications. Table 1.13 and Table 1.14 show similar results when we use *PTR* as our main policy variable[21].

4.3.2 Controlling for peer composition

One might be concerned that even if we control for past school investments via con- trolling for lagged test scores, our main policy variable, *PTR*, may still be picking up not only the effect of contemporaneous class-size but also of class composition (here, school composition). To allay such concerns, we introduce additional controls for peer composition- proportion of students in a school-year from disadvantaged caste- SC/ST and the average household affluence index. These should effectively control for the socio-economic background of the student body constituting the school.

The results are shown in Columns (5) and (6) of Table 1.15. Quite reassuringly, the point estimates remain similar to those in Table 1.7. A 10% reduction in PTR is expected to improve student test scores by 0.02σ.

4.3.3 Controlling for flexible functions of School Enrollment

[21] A formal test of hypothesis: ${\beta_1}^{IV} = {\beta_1}^{FE}$ using a bootstrap procedure that preserves the error structure yields a p-value of 0.266, implying that the two estimates are statistically indistinguishable.

Lastly, we intend to show how the IV results change as we control for the piece-wise trend in enrollment as suggested by Angrist and Lavy (1999). We introduce the following piece-wise trend as control for enrollment ($f(E_{k,t})$) in Equation 7:

$$f(E_{k,t}) = \begin{cases} \frac{E_{k,t}}{2}, & if\ E_{k,t} \leq 80 \\ 27 + \frac{E_{k,t}}{3}, & if\ 80 < E_{k,t} \leq 120 \\ 30.25 + \frac{E_{k,t}}{4}, & if\ 120 < E_{k,t} \leq 160 \\ 32.2 + \frac{E_{k,t}}{5}, & if\ 160 < E_{k,t} \leq 200 \end{cases} \tag{10}$$

The results are presented in Columns (1) and (2) of Table 1.15. Controlling for piece- wise linear trend in enrollment do not change the result that the point estimate of β_1 remains in the vicinity of -0.2, though it becomes noisier since we are only focusing on the sub-sample of schools with enrollment ≤ 200.

Introducing higher order polynomials of school enrollment as a control in Equation 7 weakens our instrument. The Kleibergen-Paap F-statistics for first stage falls rapidly as soon as higher order polynomials of school enrollment are introduced. This could be a result of the fact that introducing too flexible a function in enrollment will quickly start mimicking the *ln(PTR*)*, function thus rendering our instrumental variable weak. Therefore, we omit controlling for higher-order polynomial functions of school enrollment in Equation 7 .

If we restrict our sample to those schools[22] with enrollment in the range of +/- 5 of each threshold of administrative rule so that the identification comes only from discontinuities, the point

[22] To achieve maximum power, we include all the schools in the sample and not just those that have an enrollment of 200 or less.

estimates (given in Columns (3) and (4) of Table 1.15) turn out to be remarkably similar to our IV estimate of -0.2 (Table 1.7), though they are insignificant in this smaller sample. This builds the confidence that the identification is indeed coming from the discontinuities around the thresholds of the administrative rule.

4.4 Cost Effectiveness

In order to understand the economic significance of a program that aims at improving students' learning outcomes by reducing class size by hiring additional tenured teachers, we need to estimate the economic benefits due to students' learning gains attributable to the program and compare it with the cost of hiring such regular teachers. We proceed in three steps to do this. First, we estimate the present dis- counted value (PDV) of lifetime earnings per child Second, we project the gains in lifetime earnings that will occur due to improvement in student learning induced by smaller class-size. Finally, we combine this estimate with the cost of hiring an additional regular teacher to arrive at the cost-benefit ratio for the program.

Panel A of Table 1.12 shows the parameters required for calculating the PDV of earnings for children under status quo. We use NSS' Employment-Unemployment Survey of 2011-12 to estimate the labor force participation rate, average daily wage earned and number of days worked by a typical person in rural Andhra Pradesh. In 2011, the labor force participation rate in rural AP was relatively high (65%), the prevailing daily wage rate was INR 180, and an individual typically worked for approximately 292 days per year. We further assume that an individual enters the labor market at the age of 22 and retires in 65. Assuming an annual real wage growth rate of 5% and a discount rate of 3%, the PDV of earnings for a primary school child in 2010 turns out to be INR 2,604,572.

Next, we need to compute the long term gains in earnings due to improvement in student test score gains caused by reduction in PTR. In order to do this, we need to convert test score gains into changes in earnings. Chetty et al. (2011) provides a useful benchmark for this based on a long term follow-up of their STAR experiment on kindergarten class quality. Using this benchmark elasticity of 13% gains in earnings per standard deviation improvement in test scores, Panel B of Table 12 shows the projected gains in long term earnings due to reduction in class sizes. Hiring an additional regular teacher reduces PTR by 20%, leading to a treatment effect size of 0.04σ which in turn translates into $0.13 \times 0.04 \times 100 \approx 0.52\%$ increase in lifetime earnings of a child. Applying this rate to the PDV of earnings estimated in Panel A, we obtain an annual increment of INR 13,540 per child.

In Panel C of Table 1.12, we estimate the cost of hiring an additional regular teacher. The average monthly salary of a regular teacher in our sample is INR 10936. How- ever, a regular teacher is a tenured employee of the government and cannot be removed from her employment before the retirement at the age of 60. Therefore, we need to calculate the PDV of the lifetime salary of such a teacher. Regular teachers are appointed through public examinations and hence, they are relatively older than the contract teachers at the time of appointment. In our sample, the average age of a regular teacher at the time of appointment is 27 years. Moreover, their salaries are indexed to state-level inflation and are periodically revised through Pay Commissions. As such, we need to account for their wage growth as well. Finally, the opportunity cost of government funds is different from individuals. Using the average yield of 7.5% on government bonds prevailing between 2005 and 2011 as the discount rate and assuming an annual real wage growth rate of 5%, we arrive at the PDV of the lifetime salary of a regular teacher at INR 2,854,197. Assuming that average enrollment rate per school will remain at 93, this amounts to a projected cost of INR 30,690 per child. Consequently, the projected cost-benefit ratio per child of hiring an extra regular teacher is 2.8.

Figure 1.4 shows the sensitivity of our estimated cost-benefit ratio to various parameters listed in Table 1.12. Instead of assuming a single value for each parameter, we draw these parameters from a range of possible values, such that the preferred values listed in Table 1.12 lie in the middle of these ranges. We make 1000,000 independent draws of the parameters from either uniform or truncated normal distributions. The kernel densities of the resulting cost-benefit ratios are shown in Figures 1.4(a) and 1.4(b) respectively. The general result obtained above remains unchanged that cost is higher than the benefits from a regular teacher. For either distributions, 99% of scenarios gave cost-benefit ratio of ≥ 1. The 5-95 percentile range for Figure 1.4(a) is 1.2 to 5.3 and for Figure 1.4(b), the corresponding range is smaller and lies between 1.5 and 4.4. In both figures, the average cost-benefit ratio turns out to between 2.5:1 and 2.8:1, i.e., for every INR 100 spent on hiring a regular teacher, the student gains only INR 35-40 in future income. Thus, despite of improvements in student learning, such a program is economically costly.

5 Conclusion

We use two identification strategies- instrumental variable and school fixed effects- to understand the role of class-size on student performance. There are various ways of measuring the scholastic achievement of the students- the most prominently used in existing literature is *test score levels*. However, the analysis presented in this paper suggest that such an outcome variable confounds contemporaneous class-size effects with past human capital investments made by the schools into the child. Therefore, to understand the importance of a flow variable, like class-size, we analyze its effect on another flow variable- the *value-added scores* that control for lagged test scores.

The IV strategy suggest that the specification that do not account for previous school investments overestimates the returns to class-size by almost 100%. To the extent that these past investments (including past class-sizes) tend to be highly correlated within a cohort in a school across years, a school FE should be able to effectively absorb this time invariant part of school inputs. Consequently, for a school FE strategy, whether we use *test score levels* or *value-added scores* as an outcome variable, the class-size effects look similar.

Both the identification strategies suggest remarkably consistent negative effect of PTR on students' value-added scores. A 10% reduction in PTR by hiring more regular teachers can lead to 0.02 σ improvement in test scores. However, these point estimates are fraction of what the literature has found for other middle-income countries (e.g., for Israel, the effects are 10 times larger) and low-income countries (e.g., for Bolivia, the effects are twice the size here). Thus, it appears that the adverse effects of having bigger classrooms on student test scores, is not as high in India as in other developing and developed countries.

An important caveat that should be kept in mind while interpreting our estimates is that school PTR is capturing not only the effect of class-size that students' find themselves in but also the impact of multigrade teaching. An inadvertent feature of small schools in developing countries is that there are fewer teachers than grades in the school, leading to two or more grades being taught together by the same teacher at the same time. Smaller PTR is symptomatic of both smaller class-size as well as lower probability of a class being taught with another grade. This could partly explain why changing school PTR has such a miniscule impact on student performance in our context.

In monetary terms, reducing school PTR by hiring more regular teachers seems like an expensive investment. Hiring an additional regular teacher costs INR 30,690 annually per student whereas the student gains only INR 13,540 in future income due to the 0.04σ gains in test score resulting from smaller class size. A much higher improvement in students' scholastic performance has been achieved by less costly interventions in the same setting. For example, Muralidharan and Sundararaman (2013)'s contract teachers' program improved student scores by 0.152σ and costed only INR 192.15 per student per year. While another experiment that linked teachers' salaries to student performance (Muralidharan and Sundararaman (2011)) in India costed only INR 180 (including implementation costs) but achieved a much higher student learning gains of 0.2σ. Thus, it may be possible to achieve similar or much higher gains in student test scores in more cost-effective ways than by simply hiring more regular teachers.

Acknowledgements

Chapter 1 is currently being prepared for submission for publication of the material. Kaushik, Arushi; Muralidharan, Karthik. The dissertation author was the primary researcher and author of this material.

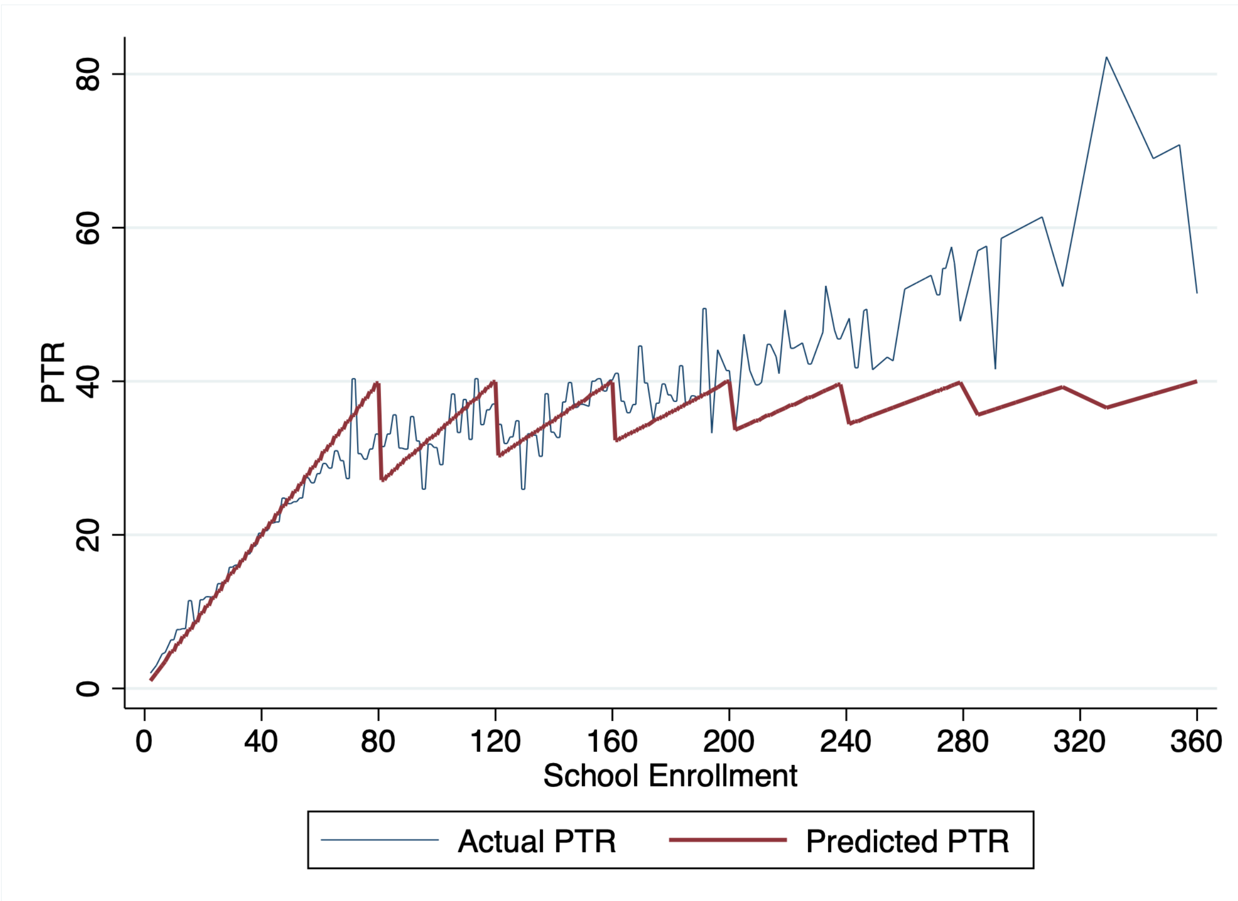


Figure 1.1: Actual and Predicted PTR

The graph shows actual PTR in blue and PTR predicted by administrative rule in Equation 6 (Maimonides' rule) in red at each enrollment level. The latter are used for instrumenting the former. Until the enrollment level of 200, the actual PTR closely follows administrative PTR. The correlation between the two weakens thereafter.

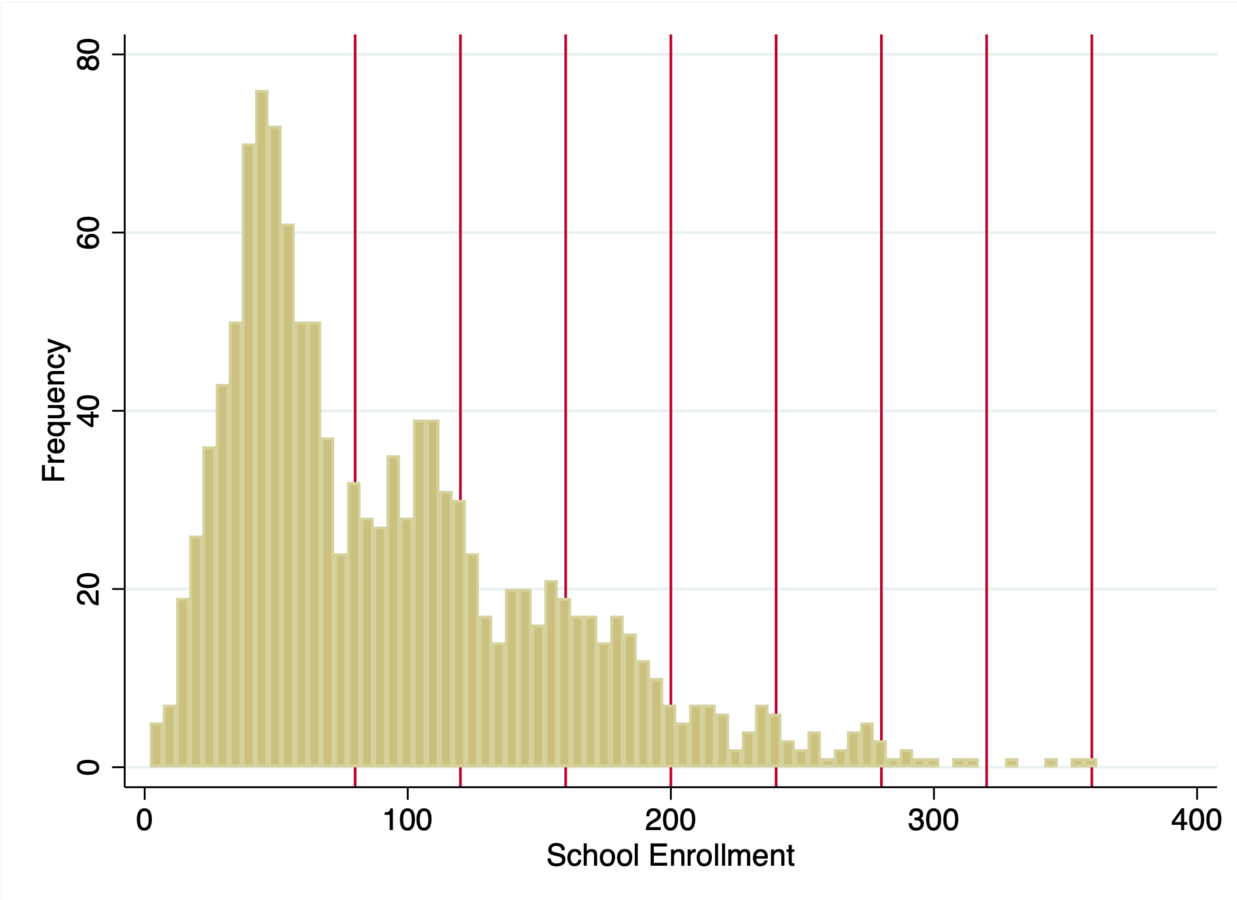
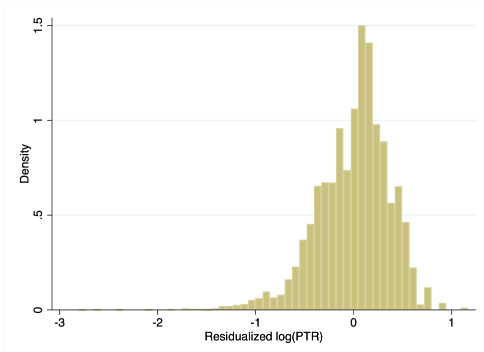
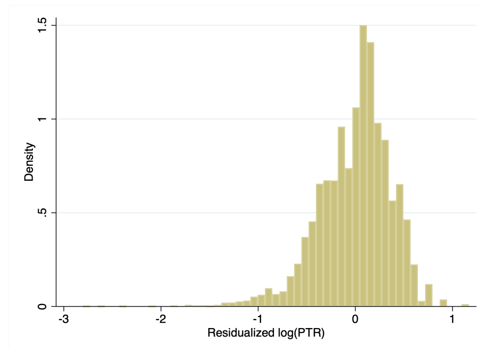


Figure 1.2: School Enrollment

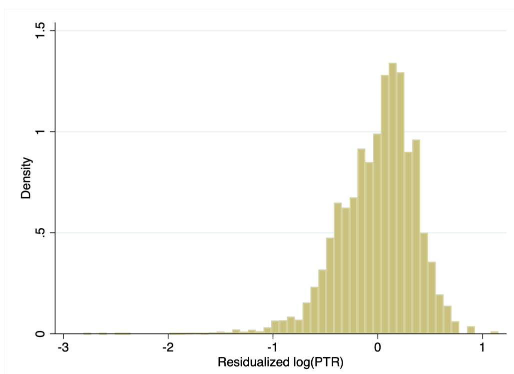
The graphs shows variation in school enrollment using a school-year as the unit of observation. The red vertical lines plot the threshold levels at which the administrative rule predicts hiring of a new teacher and hence a jump in PTR. There does not seem to be any manipulation of school enrollment around the cut-offs, in the sense that there does not seem to be a systematic effort on part of schools to push the enrollment just above the thresholds in hope of getting an extra teacher.



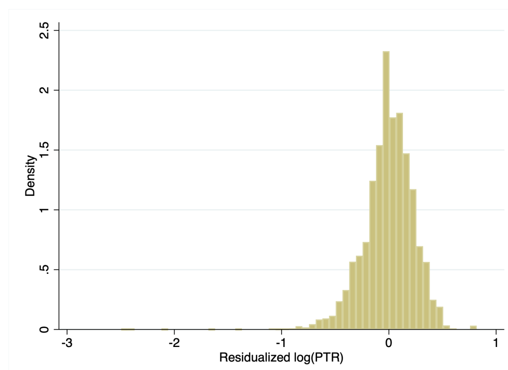
(a) PTR



(b) Residualized ln(PTR) w/o FEs



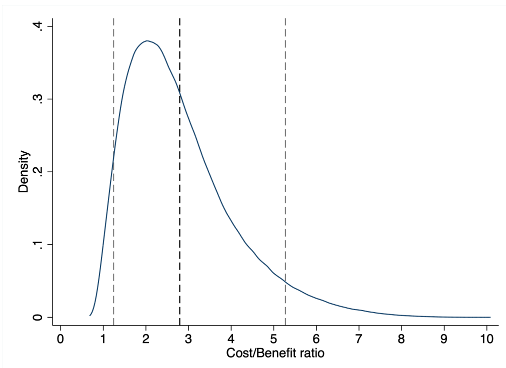
(c) Residualized ln(PTR) w/ District FEs



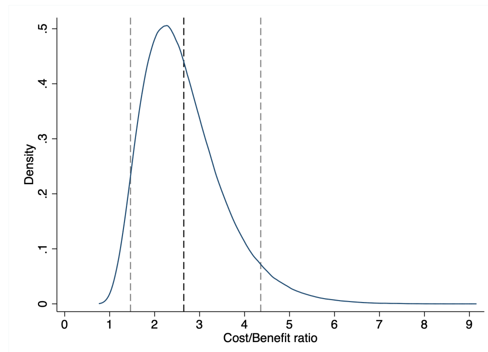
(d) Residualized ln(PTR) w/ School FEs

Figure 1.3: Variation in School PTR after controlling for various Fixed Effects

Figure 1.3 (a) shows variation in the main explanatory variable of interest, school PTR. Figure 1.3 (b) shows variation in residuals obtained after regressing ln(PTR) on a constant. Figure 1.3 (c) shows variation in residuals obtained after regressing ln(PTR) on district FEs. Figure 1.3 (d) shows variation in residuals obtained after regressing ln(PTR) on school FEs. Even in the most restrictive specification (1.3 (d)), there is enough variation left in residualized ln(PTR) to explain variation in student test scores.



(a) Uniform Parameters (mean=2.8, median=2.5)



(b) Truncated Normal Parameters (mean=2.65, median=2.5)

Figure 1.4: Sensitivity of Cost-Benefit Ratio to various parameters

The graphs show the sensitivity of estimated cost-benefit ratio to various parameters listed in Table 1.12. The parameters are drawn from a range of values with the preferred values of Table 12 located at the middle of the range: Days worked in a year range from 250 to 300; annual wage rate growth varies from 3 to 6%; the discount rate lies between 1.5% and 4.5%. The proportionate increase in earnings per σ range from 7 to 19%. In Figure 1.4 (a), the parameters are drawn from independent uniform distribution, while in Figure 1.4 (b) the draws come from truncated normal distribution. The graphs show kernel densities of cost-benefit ratio based on 1000,000 draws of parameter values. The grey lines show the values at first and 99th percentile while our preferred estimates from Table 1.12 are shown in black.

TABLES

Table 1.1: Summary Statistics

	mean	sd	min	max
School-Level Characteristics				
School Enrollment	93.4	62.2	2	360
No. of Teachers	3.05	1.32	1	8
School PTR	29.1	12.1	2	106
Infrastructure Index	2.92	1.28	0	6
Proximity Index	14.5	3.29	8	24
N	1,254			
Student-Level Characteristics				
SC	0.21	0.41	0	1
ST	0.04	0.20	0	1
OBC	0.56	0.50	0	1
Male	0.48	0.50	0	1
Household Affluence Index (0-7)	3.28	1.29	0	7
Parent Literacy Index (0-4)	1.19	1.04	0	4
N	89,681			

For school-characteristics, each school-year is taken as a unit of observation; for student-level characteristics, each student-year is used as an observation. This table provides summary statistics for schools that had only regular teacher/s throughout the year for whom the main variable ln(SchoolPTR) is not missing. The school infrastructure index is a sum of 6 binary variables indicating the existence of a brick building, a playground, a compound wall, a functioning source of water, a functional toilet, and functioning electricity. The school proximity index ranges from 8-24 and is a sum of 8 variables (each coded from 1-3) indicating proximity to a paved road, a bus stop, a public health clinic, a private health clinic, public telephone, bank, post office, and the mandal educational resource center. A higher value of the index indicates being further away from these facilities. The household asset index ranges from 0 to 7 and is the sum of seven binary variables indicating whether the household has an electricity connection, has a water source at home, has a toilet at home, owns any land, owns their home, has a brick home, and owns a television. Parental education is scored from 0 to 4 in which a point is added for each of the following: father's literacy, mother's literacy, father having completed 10th grade, and mother having completed 10th grade.

Table 1.2: Characteristics of schools with regular teachers (RT) only v/s other schools

	Other Schools	RT only Schools	Difference
School Enrollment	116.44	93.38	-23.06***
	(70.76)	(62.16)	(5.18)
No. of Teachers	3.75	3.05	-0.69***
	(1.46)	(1.32)	(0.11)
School PTR	29.61	29.14	-0.48
	(12.03)	(12.08)	(0.82)
Infrastructure Index	2.86	2.92	0.06
	(1.30)	(1.28)	(0.09)
Proximity Index	14.12	14.50	0.37
	(3.48)	(3.29)	(0.26)
Avg hh affluence	3.28	3.31	0.03
	(0.49)	(0.57)	(0.04)
Proportion of SC/ST students	0.25	0.28	0.03
	(0.28)	(0.30)	(0.02)
N	440	1254	1694

Here we compare the characteristics of schools that had only regular teachers throughout the year in Column (2) and those that didn't in Column (1). Sample includes only those school-year observations for whom the main variable *ln(SchoolPTR)* is not missing. Avg hh affluence and Proportion of SC/ST students respectively capture the average value of household index and proportion of SC and ST students in each school-year. SEs are reported in parentheses and clustered at school-level.

Table 1.3: Correlation between lagged class size and current class-size

	School PTR			
	(1)	(2)	(3)	(4)
Lagged of School PTR	0.661***	0.678***	-0.009	-0.041
	(0.028)	(0.030)	(0.115)	(0.114)
Observations	49,616	38,554	49,616	38,554
R-squared	0.374	0.400	0.603	0.644
School FEs	No	No	Yes	Yes
Controls	No	Yes	No	Yes

The table shows that there is a statistically significant correlation between lagged school PTR and current PTR values (Columns (1) and (2)) but only until School FEs are included (Columns (3) and (4)). Sample includes all schools that had only Regular Teacher in the whole academic year. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions in columns (2) and (4) include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index). Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.4: Past scores are not predictive of future PTR or school enrollments

	Δln(PTR)		School Enrollment	
	(1)	(2)	(3)	(4)
Lagged Normalized test scores	-0.005	-0.003	-2.379	-1.021
	(0.009)	(0.008)	(3.394)	(0.682)
Observations	105,857	105,857	176,870	176,870
R-squared	0.000	0.244	0.000	0.863
School FEs	No	Yes	No	Yes
Controls	No	No	No	No

The table shows that past test scores do not predict future class sizes (Columns (1) and (2)), nor they predict future enrollment levels (Columns (3) and (4)). Sample includes all schools that had only Regular Teacher in the whole academic year. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.5: OLS Results

	Normalized Scores			
	(1)	(2)	(3)	(4)
ln(School PTR)	-0.25***	-0.23***	-0.20***	-0.20***
	(0.05)	(0.05)	(0.05)	(0.05)
Observations	133,773	107,708	133,773	107,708
R-squared	0.25	0.26	0.27	0.28
District FEs	No	No	Yes	Yes
Controls	No	Yes	No	Yes

The table shows results corresponding to Equation 4. There is statistically strong negative correlation between class size and student test scores. Sample includes all schools that had only Regular Teacher in the whole academic year. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions in columns (3) and (4) include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index). Regressions in Columns (2) and (4) also include District FEs. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.6: First-Stage Results

	ln(School PTR)			
	VA Specification		Levels Specification	
	(1)	(2)	(3)	(4)
ln(PTR*)	0.735***	0.717***	0.727***	0.716***
	(0.054)	(0.054)	(0.051)	(0.051)
KP F-stat	90.68	83.06	77.32	73.30
KP LM stat	66.23***	65.58***	77.64***	78***
Observations	91,944	91,944	119,737	119,737
R-squared	0.565	0.588	0.574	0.593
District FEs	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes
School Enrollment	Yes	Yes	Yes	Yes

The table shows first-stage results corresponding to Equation 5. ln(PTR*) is strong predictor of ln(SchoolPTR), as seen in small p-value for Kleibergen-Paap LM test, making the administrative rule of Equation 6 a relevant instrument for school PTR. Moreover, the Kleibergen-Paap robust F-statistic has a sufficiently high value ensuring that it is not a weak instrument. Sample includes all schools that had only Regular Teacher in the whole academic year. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm.Columns (1) and (2) include lagged test scores while Columns (3) and (4) do not. Regressions include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index) as well other controls like school enrollment. Regressions in Columns (2) and (4) also include District FEs. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.7: IV Results: Value-Added and Level Specifications

	Normalized Scores			
	VA Specification		Levels Specification	
	(1)	(2)	(3)	(4)
ln(School PTR)	-0.255**	-0.196**	-0.482***	-0.416***
	(0.106)	(0.099)	(0.145)	(0.128)
School Enrollment	-0.000	-0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	91,944	91,944	119,737	119,737
R-squared	0.261	0.275	0.038	0.068
District FEs	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes

The table shows results corresponding to Equation 7, where ln(SchoolPTR) is instrumented with ln(PTR^*). Columns (1) and (2) include lagged test scores while Columns (3) and (4) do not. Sample includes all schools that had only Regular Teacher in the whole academic year and had enrollment ≤ 200. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index) as well other controls like school enroll- ment. Regressions in Columns (2) and (4) also include District FEs. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.8: IV Subject-wise Results: Value-Added and Level-Specifications

	Normalized Scores			
	VA Specification		Levels Specification	
	Maths	Language	Maths	Language
	(1)	(2)	(3)	(4)
ln(School PTR)	-0.147	-0.244***	-0.367***	-0.465***
	(0.114)	(0.092)	(0.142)	(0.120)
School Enrollment	-0.000	-0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	45,870	46,074	59,820	59,917
R-squared	0.243	0.314	0.070	0.069
First-Stage Statistics				
First Stage KP F-stat	82.73	83.31	73.33	73.24
First Stage KP LM	65.57***	65.59***	77.97***	78.02***
District FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

While columns (1)-(2) control for lagged test scores, columns (3)-(4) do not. ln(SchoolPTR) is instrumented with ln(PTR*). The table also shows first-stage statistics like Kleibergen-Paap (KP) F-stat and LM-stat. Both of them indicate that ln(PTR*) is a relevant and not weak instrument for ln(SchoolPTR). Sample includes all schools that had only Regular Teacher in the whole academic year and had enrollment ≤ 200. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index) as well other controls like school enrollment. All regressions also also include District FEs. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.9: School Fixed Effects Results: Value-Added and Levels-Specifications

	Normalized scores							
	VA Specification				Levels Specification			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln(School PTR)	-0.25***	-0.23***	-0.20***	-0.18***	-0.27***	-0.28***	-0.16**	-0.17**
	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Observations	133,773	107,708	133,773	107,708	187,190	139,774	187,190	139,774
R-squared	0.25	0.26	0.33	0.34	0.01	0.03	0.18	0.20
School FEs	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

The table shows results corresponding to Equation 8 and Equation 9. While columns (1)-(4) control for lagged test scores, columns (5)-(8) do not. Sample includes all schools that had only Regular Teacher in the whole academic year. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions in columns (3) and (4) include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index). Regressions in Columns (3), (4), (7) and (8) also include School FEs. Standard Errors are reported in parentheses and are clustered at school level. *** p$<$0.01, ** p$<$0.05, * p$<$0.01.

Table 1.10: School Fixed-effects Subject-wise Results: Value-Added and Levels-Specifications

	Normalized Scores			
	VA Specification		Levels Specification	
	Maths	Language	Maths	Language
	(1)	(2)	(3)	(4)
ln(School PTR)	-0.16**	-0.21***	-0.16**	-0.19***
	(0.08)	(0.07)	(0.07)	(0.07)
Observations	53,721	53,987	69,826	69,948
R-squared	0.33	0.37	0.22	0.20
School FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

While columns (1)-(2) control for lagged test scores, columns (3)-(4) do not. Sample includes all schools that had only Regular Teacher in the whole academic year. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions in columns (3) and (4) include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index). All regressions also include School FEs. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.11: Estimation Sample for School FE Strategy

No. of years with RT only	**5**	**4**	**3**	**2**	**1**	**Total**
No. of schools (all schools)	126	70	40	83	58	377
No. of school-year observations (all schools)	630	280	120	166	58	1254
No. of schools (only control schools)	43	22	14	12	8	99
No. of school-year observations (only control schools)	215	88	42	24	8	377

The table shows no. of school-year observations and no. of schools contributing to identifying variation. Sample includes all schools that had only Regular Teacher in the whole academic year. Most of the schools have data for more than a year.

Table 1.12: Cost-Benefit Analysis

Parameter	Source	Estimate
A. Projecting Future Earnings		
Labor Force Participation Rate	LFP for rural population of AP aged >=15, NSS 2011-12	0.65
Average Daily wage rate	Average wage for rural workers of AP aged >=15, NSS 2011-12	180
Days worked per-year when in labor force	For rural workers of AP aged >=15, NSS 2011-12	292
Current Annual Earnings when in labor force	Calculation	32,174
Annual real wage growth rate	Assumption[23]	0.05
Discount rate	Assumption	0.03
Average PDV of lifetime Earnings	Calculation	2,604,572
B. Impact Size		
Test score effect (sd)	Table 1.8 & 1.11 [24]	0.04
Earnings gain per sd of test scores	Estimates from literature linking test score gains to earnings [25]	0.13
Predicted gain in PDV of earnings per child	Calculation	13,540
C. Cost-Benefit Ratio		
PDV of lifetime salary of a regular teacher	Calculation[26]	2,854,197
Average Enrollment in a school	Data	93
Predicted cost of a RT per child	Calculation	30,690
Cost-Benefit Ratio	Calculation	2.8

The table presents cost-benefit calculations for a program that aim at reducing PTR by hiring a regular/tenured teacher. Panel A lists the parameters needed for calculating the present discounted value (PDV) of lifetime earnings of primary school children in rural Andhra Pradesh in 2010. Panel B presents the assumptions required to estimate the increase in earnings attributable to improvement in test scores induced by smaller class sizes. Panel C combines estimate from Panel B with the projected lifetime cost of hiring a regular teacher to calculate the cost-benefit ratio.

[23] According to NITI Aayog report, the real gross state domestic product in Andhra Pradesh grew 5.5% per year from 2011-12 to 2013-14.

[24] Hiring an additional regular teacher reduces the PTR by 20% which is associated with 2*0.02 σ increase in student test scores.

[25] Estimates are taken from Kline and Walters (2016), Appendix Table A.IV

[26] According to our data, average annual salary of a regular teacher is INR 131,232 and typical age at the time of appointment is 27 years. Age of retirement is 60. Further assume that an annual growth rate in salary is 5% and the discount rate at 7.5% which was the average yield on government bonds between 2005 and 2011.

Table 1.13: IV Results: Value-Added and Level Specifications with linear PTR

	Normalized Scores			
	VA Specification		Levels Specification	
	(1)	(2)	(3)	(4)
School PTR	-0.013**	-0.009*	-0.023***	-0.020***
	(0.006)	(0.005)	(0.007)	(0.007)
school enrollment	0.001	0.000	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	91,944	91,944	119,737	119,737
R-squared	0.259	0.273	0.026	0.056
	School PTR			
	VA Specification		Levels Specification	
	(1)	(2)	(3)	(4)
PTR*	0.667***	0.640***	0.647***	0.630***
	(0.088)	(0.087)	(0.083)	(0.081)
KP F-stat	25.28	22.94	21.44	20.73
KP LM stat	52.55***	49.42***	61.11***	60.59***
Observations	91,944	91,944	119,737	119,737
R-squared	0.433	0.459	0.448	0.470
District FEs	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes

Panel A of the table shows second-stage results similar to Equation 7, where instead of ln(SchoolPTR), we are using linear PTR, i.e., School PTR, which in turn, is instrumented with PTR*, i.e, PTR predicted by administrative rule and given in Equation 6. Panel B of the table shows First-stage results similar to Equation 5. Both KP F-stat and LM-stat indicate that PTR* is a relevant and not a weak instrument for School PTR. Columns (1) and (2) include lagged test scores while Columns (3) and (4) do not. Sample includes all schools that had only Regular Teacher in the whole academic year and had enrollment ≤ 200. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index) as well other controls like school enrollment. Regressions in Columns (2) and (4) also include District FEs. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.14: School Fixed-Effects Results: Value-Added and Level-Specifications with linear PTR

	Normalized Scores							
	VA Specification				Levels Specification			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School PTR	-0.007***	-0.007***	-0.006***	-0.006***	-0.007***	-0.008***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	133,773	107,708	133,773	107,708	187,190	139,774	187,190	139,774
R-squared	0.253	0.263	0.331	0.342	0.012	0.031	0.179	0.198
School FEs	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

The table shows results similar to Equation 8 and Equation 9, where ln(SchoolPTR) is replaced by School PTR. While columns (1)-(4) control for lagged test scores, columns (5)-(8) do not. Sample includes all schools that had only Regular Teacher in the whole academic year. To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions in columns (3) and (4) include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index). Regressions in Columns (3), (4), (7) and (8) also include School FEs. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.15: IV Results: Robustness Checks

	Normalized Scores					
	Piecewise Linear Trend		+/- 5 Discontinuity Sample		Peer Composition	
	(1)	(2)	(3)	(4)	(5)	(6)
ln(School PTR)	-0.22**	-0.17	-0.21	-0.22	-0.20*	-0.19*
	(0.11)	(0.11)	(1.67)	(1.84)	(0.11)	(0.10)
Observations	91,944	91,944	23,351	23,351	91,944	91,944
R-squared	0.26	0.28	0.20	0.26	0.27	0.28
First-Stage Statistics						
KP F-stat	69.44	65.77	0.187	0.179	76.89	79.95
KP LM-stat	58.62***	59.50***	0.535	0.522	65.16***	65.66***
District FEs	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Columns (1) and (2) of the table shows results corresponding to controlling for piecewise linear trend given in Equation 10 in Equation 7. Columns (3) and (4) restricts the sample to +/- 5 around threshold values of school enrollment in Equation 6 and presents results corresponding to Equation 7 for this restricted sample. Columns (5) and (6) controls for peer composition by including additional controls like average household affluence of the peers and proportion of SC/ST student body in the school in that year. In all regressions, ln(SchoolPTR) is instrumented with ln(PTR*). Sample includes all schools that had only Regular Teacher in the whole academic year and had enrollment ≤ 200 except for Columns (3) and (4). To increase the power, data from all treatment arms of APRESt experiment are pooled in and dummies are included for each treatment arm. Regressions in columns (3) and (4) include School controls (like infrastructure index and proximity index) and HH Controls (like gender, caste, hh affluence index and parental literacy index). Regressions in Columns (2), (4), (6) and (8) also include District FEs. All regressions control for lagged scores. Standard Errors are reported in parentheses and are clustered at school level. *** p<0.01, ** p<0.05, * p<0.1.

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CHAPTER 2

In Utero Exposure to Industrial Disasters: A Case Study of the Bhopal Gas Tragedy[27]

Gordon C. McCord[28]

Prashant Bharadwaj[29]

Arushi Kaushik[30]

Lotus McDougal[31]

Anita Raj[32]

Abstract

Background: Globalization and industrialization can increase economic opportunity for low- and middle-income nations, but these processes may also increase industrial accidents and harm workers. This paper examines the long-term, cohort-based health effects of the Bhopal Gas Disaster (BGD), one the most serious industrial accidents in history.

Methods: This retrospective analysis uses geolocated data on health and education from India's National Family Health Survey-4 (NFHS-4) and the 1999 Indian Socio-Economic Survey [NSSO 1999] to examine the health effects of exposure to the BGD among men and women aged 15-49 living in Madhya Pradesh in 2015-16 (women n=130,002; men n=7,031 [NFHS-4] and n=13,369 [NSSO-1999]). A spatial difference-in-differences strategy estimated the relative effect

[27] Author Contributions: PB conceptualized the project, GCM and AK organized data and conducted analysis, all authors contributed equally to data interpretation, writing, review and editing. GCM and AK have verified the underlying data. Authors declare no conflict of interest.
[28] Corresponding author. gmccord@ucsd.edu, School of Global Policy and Strategy, UC San Diego
[29] Department of Economics, UC San Diego
[30] Department of Economics, UC San Diego
[31] Center on Gender Equity and Health, Department of Medicine, UC San Diego
[32] Center on Gender Equity and Health, Department of Medicine, UC San Diego

of being in utero near Bhopal relative to other cohorts and to those further from Bhopal separately for each dataset.

Findings: We document long-term, intergenerational impacts of the BGD, showing that men who were in utero at the time were more likely to have a disability that affected their employment 15 years later, and had higher rates of cancer and lower educational attainment over 30 years later. Women living up to 100km from the BGD had higher levels of pregnancy terminations than those living farther away.

Interpretation: These results indicate social costs stemming from the BGD that extend far beyond the mortality and morbidity experienced in the immediate aftermath. Quantifying these multigenerational impacts are important for policy consideration. Moreover, our results suggest that the BGD affected people across a substantially more widespread area than has previously been demonstrated.

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1 Introduction

Globalization and industrialization can increase economic opportunity for low- and middle-income nations, but recent cross-national evidence indicates that these processes may also be increasing industrial accidents and harming the workers of these nations (Blanton and Peksen (2017)). Evidence from India suggests high numbers of industrial accidents, with the highest associated attributable deaths in the world (Nalapat (2020), Sengupta et al (2016)). Despite recommendations for improved protections for workers and industry in India (Sengupta et al (2016)), the Ministry of Environment, Forest and Climate Change has proposed the Environmental

Impact Assessment (EIA) notification 2020, which will roll back environmental and worker protections in the country. Critics, including United Nations experts, have stated that this rollback violates the safety and health of workers and the environment and with potential impact on subsequent generations[33][34][35]. Unfortunately, little research is available on these multigenerational impacts, and inadequate recognition of these far-reaching repercussions can diminish the memory of such tragedies for policy consideration. This research provides insight into these impacts for children born to women survivors of one of the worst industrial disasters in history, the Bhopal Gas Disaster.

The Bhopal Gas Disaster (BGD) occurred in December 1984 and involved a methyl isocyanate (MIC) gas leak at a Union Carbide pesticide plant near the city of Bhopal, in Madhya Pradesh, India. The leak spread toxic gas in approximately a 7 km radius around the plant, exposing more than half a million people and resulting in up to 30,000 deaths in the region (Sharma (2005)). There is a broad spectrum of serious long term and chronic health consequences for hundreds of thousands of survivors, including children, manifesting across multiple systems (e.g. respiratory, neurologic, musculoskeletal, ophthalmic, endocrine) (Sharma (2005), Ganguly et al (2018), Mishra et al (2009), Dhara and Dhara (2002)). These impacts may be the tip of the iceberg however, given that MIC toxins affected groundwater (Mishra and Banerjee (2014)) and the reproductive health and other health outcomes of exposed women (Ganguly et al (2018)), factors suggesting generations not exposed to the toxic gas directly may nevertheless suffer adverse health and social impacts. Potential in utero effects cannot be understated. Studies document a 4-fold

[33] Explained: What is EIA 2020? How does it water down the existing policy? The Week. 2020 August 10, 2020. doi: https://www.theweek.in/news/biz-tech/2020/08/10/explained-what-is-eia-2020-how-does-it-water-down-the-existing-policy.html
[34] Explained Ideas: What ails with the draft EIA notification 2020? The Indian Express. 2020 August 12, 2020. doi: https://indianexpress.com/article/explained/draft-environmental-impact-assessment-notification-2020-shibani-ghosh-explained-ideas-6549609/
[35] UN experts to Govt of India: Environmental exemptions to industry unwarranted. September 4, 2020.

increase in the rate of miscarriage following the gas leak, as well as increased risk for stillbirth and neonatal mortality (Mishra et al (2009), Dhara and Dhara (2002)). Decades after the disaster, menstrual abnormalities and premature menopause have emerged as common problems among exposed women and their female offspring (Mishra et al (2009)). MIC has also been shown to damage human chromosomes (Ennever and Rosenkranz (1987)). Early clinical studies on the gas-affected population showed signs of increased chromosomal aberrations that could manifest as cancer (Malla et al (2011)), though population-level changes in cancer rates 8 years after the accident were suggestive but not significant (Dikshit and Kanhere (1999)).

This paper estimates the long-term health (specifically, adult cancer rates and disability) and human capital impacts (educational attainment) on individuals who were exposed to the BGD in utero or as children in 1984. Understanding the long-term impacts of industrial disasters on children, especially those in utero at the time of the disaster, is important for several key reasons. First, while the immediate damage to children in the form of direct health impacts is perhaps easier to assess, the long term, health related damages could take years to manifest and might not be part of the legal damages considered. Second, economic research over the last few decades has shown that interventions that improve outcomes for children have high rates of return (Magnuson and Duncan (2016)); to this second point, we include education as an important indicator of socioeconomic impact on these children, particularly as this may compound health impacts. Our findings offer timely information for consideration of potential rollbacks of policies related to social and environmental protections attached to industrialization in India and elsewhere.

2 Methods

2.1 Data

The principal data source is the fourth round of the Demographic and Health Surveys (DHS) in India, which was conducted between 2015-2016 (also called National Family Health Survey, or NFHS-4, in India) (IIPS (2017)). The DHS is a high-quality household survey that provides detailed information on health at the individual level. NFHS-4 is representative at both the national and state levels; all interviews in Madhya Pradesh were completed in 2015. The analytic sample for women includes women who were interviewed in Madhya Pradesh and born between 1960 and 1990 and had pregnancies between 1975 and 1990 (n=130,002 pregnancies). The analytic sample for men included men who were interviewed in Madhya Pradesh and born between 1960 and 1990 (n=7,031).

A second dataset that we use is the Integrated Public Use Microdata Series (IPUMS) from India for the year 1999, which presents harmonized data from the 1999 Socio-Economic Survey conducted by the National Sample Survey Organization (NSSO). These data contain employment information for a representative sample but only identifies the district of the respondent. This is the most recent NSSO survey in which respondents were asked how long they had been living at their current residence, a crucial question for this analysis. The survey sample includes people from ages 6-64, thus including those who were in-utero at the time of the BGD. The analytic sample for this manuscript includes men interviewed in Madhya Pradesh and born between 1960 and 1990 (n=13,369).

These two datasets could not be merged at the individual level, as the same respondents were not interviewed in each survey. To preserve the statistical power of individual microdata, datasets thus were analyzed separately.

2.2 Measures

For women, the outcome of interest was whether they ever had a pregnancy that ended before a live birth due to miscarriage, abortion or stillbirth, and if so, the year of the most recent terminated pregnancy.

For men, the outcomes of interest were cancer rates and educational attainment (from NFHS-4) and employment disability (from NSSO). Cancer was assessed as a general question "Do you currently have cancer", and educational attainment was assessed as the number of years that an individual attended school. We use the question on whether "the respondent was economically inactive because of disabilities or...other health-related reasons" to classify the person as suffering from employment disability. Given the prevalence of child labor, note that the employment-related questions of the survey are asked of all respondents, including children. Analyses assessing movers vs. non-movers are necessarily restricted to men, since patrilocality in India means that most women move to their husband's house after marriage (Odysseos (2015)). As 78% of Indian women aged 15-49 have married (IIPS (2017)), the sample size of non-mover females is both small and non-representative, and thus cannot be considered in this analysis.

For both men and women, distance from the Union Carbide plant was the primary predictor. NFHS-4 collected the latitude and longitude of each respondent's sampling cluster (henceforth, DHS clusters). The DHS clusters are villages or hamlets in rural areas and blocks or census tracts in urban areas; thus, they are much smaller geographical units than administrative units such as districts. These coordinates enable the calculation of distance from the Union Carbide plant in Bhopal for each respondent, within a privacy displacement radius of 2 km (urban) or 5-10 km (rural) (Figure 2.1).

In the case of district-level IPUMS data, we measure distance from Union Carbide plant to the nearest border of the district. For both datasets, we use the distance from the pant to form the treatment and control groups, since exposure to the toxic MIC gas would be more muted at greater distances.

2.3 Analysis

The analysis uses a spatial difference-in-differences strategy, where distance from the Union Carbide plant (we also refer to this as "distance from Bhopal") lends the spatial dimension. We compare people living close to Bhopal to those far away from Bhopal, and people who were young at the time of the BGD to those who were older at the time of BGD. This way, under some basic assumptions, we are able to net out any difference in outcomes that arise due to *time invariant* aspects of being close to Bhopal, as well as aspects of belonging to a specific birth cohort.

All analyses were conducted using birth year cohorts (Figure 2.2). The reference cohort includes those born between 1965-1974 (i.e. respondents who were more than 10 years old at the time of the BGD). Three other cohorts are considered: young children below 10 years old (i.e., respondents born between 1975-1984), fetuses (i.e. respondents born in 1985) and children not yet conceived at the time of BGD (i.e. respondents born between 1986-1990).

It is worth noting that all four cohorts have been affected by the disaster, including our reference cohort. Therefore, while older cohorts are being used as a reference category, our results speak to the *relative* impacts on those who were in utero at the time of the disaster.

Another caveat is in respect of interpretation of our estimates. Since we are comparing the population that is *currently* living in Madhya Pradesh instead of those who were *born* there, our estimates are affected by differential mortality and migration of the cohort that was in utero at the time of the accident vis-à-vis other cohorts. Both these factors can severely affect the composition of the population observed post-facto. For example, if the weakest children in utero were more likely to die from the incident, then our estimates could be considered a lower bound. While an outmigration of richer households from Bhopal will yield an upper bound on the estimated impact of BGD on in-utero group. In fact, migration in this area was relatively low and researchers found that 91% of the population remained in the same area subsequent to 1984 (Dhara and Dhara (2002)). Our own estimates in Figure S2 suggest that mothers of the in-utero cohort are not significantly different from mothers of other consecutive cohorts on various educational and health dimensions. Therefore, at best our estimates are understating the true effect of BGD on the in-utero group on account of differential mortality.

Linear regression models were used to estimate the relationship between proximity to Bhopal, birth cohort, and outcomes of interest, with the basic specification as follows:

$$(I) \quad Y_{itc} = \alpha_1 Bhopal_c + \beta_1 * T1_t * Bhopal_c + \beta_2 * T2_t * Bhopal_c + \beta_3 * T3_t * Bhopal_c + Cohort\ FE + \varepsilon_{itc}$$

where Y_{itc} is the outcome of interest for person i born in year t and living in location c. We consider three health and education outcomes, namely: the probability of suffering from cancer, the probability of suffering from employment disability, and years of education completed. $Bhopal_c$ takes the value of 1 if the respondent i is living near Bhopal (we define "near" Bhopal as being within 100kms following spatial patterns in miscarriages explained in Figure 3) and 0 otherwise. $T1_t$ is a dummy variable equal to 1 if the respondent was born between 1975-1984 (cohort of ages 0-10 at the time of BGD) and 0 otherwise. $T2_t$ is a dummy variable equal to 1 if the respondent was born in 1985 (in-utero cohort) and 0 otherwise. $T3_t$ is a dummy variable equal to 1 if the respondent was born between 1986-1990 (cohort not yet conceived at the time of BGD) and 0 otherwise. The omitted category consists of respondents who were older than 10 years at the time of the disaster and are living more than 100 kms away from Bhopal. All regressions include birth year fixed effects to standardize the outcome variable across ages. Standard errors are two-way clustered by birth year and district in order to account for spatial and serial autocorrelation.

β_1, β_2, and β_3 respectively capture the effect of BGD on young children below 10 years old, fetuses and not-yet-conceived children relative to those who were older than 10 at the time of BGD. The spatial difference-in-difference compares children in a cohort living near Bhopal to children in the same cohort living far from Bhopal. In order to interpret β_1, β_2, and β_3 as the causal effect of the BGP, the underlying identifying assumption is that in the absence of the disaster the cohorts near and far from Bhopal would have evolved similarly. Note that α_1 captures the effect of living near Bhopal vis-a-vis far from Bhopal for the reference cohort (1965-1974). This difference captures time invariant differences in the two geographical locations.

2.4 Ethical approval

All data used in this study is publicly available and de-identified. Ethical approval for NFHS-4 data collection was provided by the International Institute for Population Sciences and ICF. Ethical approval for the 1999 NSSO Socio-Economic Survey was provided by the Indian National Sample Survey Office. Ethical exemption for this analysis was provided by the University of California San Diego IRB.

3 Results

3.1 Miscarriages following the Bhopal Gas Disaster

Pregnancies during 1975-1984 and 1986-1990 have similar rates of termination, with marginally lower proportions very near Bhopal (around 7%) compared to around 9% at 200-250 km, consistent with an urban-to-rural gradient (Figure 2.3). The 1985 pregnancies, however, show a strikingly different pattern. Women who lived near Bhopal experienced much higher rates of pregnancy termination (more than double) compared to women who lived further away or compared to pregnancies in the other years. This abnormally high proportion of terminations extends as far as 100km from Bhopal.

3.2 Adult Cancer Rates among Exposed Children

Similar to trends seen with pregnancy terminations, children who were born in 1985 and who lived close to Bhopal during the disaster experience much higher rates of cancer as adults compared to adults who were born before or after the disaster and who lived further away from Bhopal (Figure 2.4). As in the case of pregnancy terminations, the abnormally high cancer rates appear to extend up to 100 km from Bhopal. Given the two spatial patterns in Figures 3 & 4, we use 100 km as the BGD exposure cutoff in our analysis.

The excluded cohort was born between 1960-1974, such that the coefficients are interpreted as the difference in cancer probability between the 1960-1974 and the other cohorts, and between men living within and beyond 100 km from Bhopal in 2015. Among all men in the NFHS-4 analytic sample, estimates suggest that the 1975-1984 and 1986-1990 birth cohorts have statistically indistinguishable cancer rates compared to the 1960-1974 cohort, while men born within 100 km of Bhopal in 1985 have a 2.1 percentage point higher risk than the other cohorts (Table 2.1). This is an 8-fold higher cancer risk compared to the other cohorts.

Column (2) limits the sample to men who have never moved, and were therefore residing within 100km of the Union Carbide plant during the BGD. The effect of being born in 1985 and near Bhopal is even stronger, with these men having a 6.2 percentage point higher cancer risk in 2015 compared to the other cohorts and to those living more than 100 km from Bhopal. This represents a 27-fold higher risk of cancer among adults who were in utero during the BGD, suggesting long-term health consequence of exposure, even while in the womb. Column (3) limits the sample to men who have moved at some point before the survey in 2015-16 (and who were therefore less likely to be exposed to the BGD), and finds no difference in cancer rates across cohorts.

3.3 Effects on Employment Disability

Men who were in utero during the BGD and who lived in districts with a border within 100 km Bhopal are 1 percentage point more likely to report employment disability when they are surveyed as adults, compared to the older cohorts and those living further from Bhopal (Figure 2.5; Table S1).

3.4 Effects on Educational Attainment

Relative to other cohorts, men who were in utero during the BGD attained approximately two fewer years of education, with the effects only evident among men who have never moved (Figure 2.6; Table S2).

4. Discussion

The Bhopal Gas Disaster was one the most serious industrial accidents in history, causing thousands of deaths at the time of the leak, and tens of thousands more deaths and serious health sequelae in the three decades since (Broughton (2005), Odysseos (2015)). This analysis documents long-term, intergenerational impacts of the BGD, showing that men who were in utero at the time of the BGD were more likely to have a disability that affected their employment 15 years later, and had higher rates of cancer and lower educational attainment over 30 years later. Importantly, these results indicate social costs stemming from the BGD that extend far beyond the mortality and morbidity experienced in the immediate aftermath. Moreover, our results suggest that the BGD affected people across a substantially more widespread area than has previously been demonstrated: up to around 100 km (approximately 60 miles) from the site, as opposed to the 4.5 km radius that was considered exposed by public health officials and researchers after the disaster

(Mishra et al (2009), Dhara and Dhara (2002), Dikshit and Kanhere (1999), Sathyamala and Jayaprakash (2012)). This study is also the first, to our knowledge, to use different birth cohorts to attribute causality of subsequent health and social consequences to the BGD.

We find that men currently living within 100 km of Bhopal and born in 1985 have an 8-fold higher risk of cancer than men of other birth cohorts; of those men, those who have never changed residence since the BGD have a 27-fold higher risk of cancer. While elevated cancer levels have been previously associated with MIC exposure (Dikshit and Kanhere (1999), Khan et al (2015), Senthilkumar et al (2011), Senthilkumar et al (2015)), our results identify long-term, birth-year based differences in cancer prevalence. This underscores the plausibility of multiple MIC exposure pathways, including teratogenic in utero exposure, and ongoing environmental exposure. Bhopal has ongoing cancer surveillance programs[36], and it remains clear that those exposed to MIC require ongoing and careful monitoring, support and treatment.

Employment disability was one percentage point more likely among men who were in utero in districts within 100 km of Bhopal during the BGD than those born prior or more distal to the BGD. This is a meaningful impact since baseline employment disability rates are quite low (0.4%), and men's employment at the time of study was nearly universal at 98% (IIPS (2017)). In a context of high poverty, with nearly 40% of Madhya Pradesh's population living beneath the poverty line[37], more than 90% informal employment nationally (thus lacking social protections and safety nets) (Mehrotra (2019)), and household reliance on male income due to low levels of female labor force participation and a highly skewed gender wage gap (IIPS (2017), ILO (2018)), this increase

[36] National Centre for Disease Informatics and Research. Area Cancer 2021.https://ncdirindia.org/Area_Cancer.aspx (accessed June 8 2021).
[37] Ministry of Social Justice and Empowerment, Department of Social Justice and Empowerment, Government of India. State-Wise Percentage of Population Below Poverty Line by Social Groups, 2004-05. 2021. http://socialjustice.nic.in/UserView/index?mid=76672 (accessed June 8 2021).

in disability or health-related economic inactivity risks substantial impact on the health, safety and well-being of affected families.

The finding that men who were in utero and within 100km of Bhopal during the BGD received more than two fewer years of education than other cohorts has tremendous human capital implications. This is a large impact both since the average number of years of education in the control group is only 5.6 years, and because education has such a direct association with subsequent wages and consumption (Fulford (2017)). We cannot ascertain whether the lower educational attainment is due to health or cognitive consequences of being exposed to the BGD in utero. Given that the BGD effect is much more evident in the 1985 cohort compared to older or younger children, it is unlikely that the effect operated through broader socioeconomic impacts or impacts on other family members that limited children's access to school. Regardless of the mechanism, the reduced educational attainment is evidence of long-term consequences of exposure to the accident that imply personal and social costs far beyond the immediate health impacts.

A few study limitations are worth outlining. First, as with any ecological exposure assignment, the cohort of in-utero children we assign to exposure will include a range of actual exposure to MIC gas. The interpretation of our estimates would be the effect of average exposure in the population we define as treated. Secondly, our estimates are affected by mortality and migration. An important consequence of a disaster, whether natural or man-made, is that mortality can severely affect the composition of the population observed post-facto. For example, if the weakest children in utero were more likely to die from the incident, then our estimates could be considered a lower bound. In our empirical context, any mortality effect of the disaster that symmetrically affects all cohorts within Bhopal (without affecting people away from Bhopal) will not affect our estimates. As such, it is differential mortality across cohorts due to the BGD that would have compositional effects on our study sample.

Likewise, a frequent human response to a disaster is migration to safer areas. Again, it would only be differential migration in response to the BGD (different migration across cohorts and across people living close or far from Bhopal) that would create a problematic compositional difference between our treated cohort and the others. In fact, migration in this area was relatively low and researchers found that 91% of the population remained in the same area subsequent to 1984 (Dhara and Dhara (2002)). Our own estimates in Figure S1 suggest that mothers of the in-utero cohort are not significantly different from mothers of other consecutive cohorts on various educational and health dimensions.

It is also important to note that the long-term consequences that we estimate could be the result of both direct effects from exposure as well as lack of subsequent mitigation of the effects through health, disability and education services. While disentangling these forces is important from a policy perspective, in this paper we simply highlight the total combined effect of being exposed to such disasters as a child. Finally, cancer reporting in the data comes from self-reports, which could be subject to biases; there are also very cases of self-reported cancer in the data. We think of our results on cancer as being consistent with the broader public health narrative on the consequences of the disaster, but more data through systematic cancer screening and place of birth information is key to firmly establish this link.

Understanding the short- and long-term damages caused by industrial disasters is key to gaining insight into the tradeoffs involved in making regulatory decisions. It is also crucial from a policy response perspective if policy makers wish to dedicate resources to mitigate harm done by such events or to compute legal damages. These concerns are particularly germane now, when there is evidence that the multiple health conditions suffered by many BGD survivors may make them more susceptible to the COVID-19 pandemic currently ravaging India (Malviya et al (2021), Dore (2020)). The evidence presented in this paper starkly highlights the long-term, intergenerational health and human capital effects of the BGD, and underscores the need for ongoing survivor support, as well as robust regulatory protections.

Acknowledgements

Chapter 2 is currently being prepared for submission for publication of the material. McCord Gordan; Bharadwaj, Prashant; Kaushik, Arushi; McDougal, Lotus; Raj, Anita. The dissertation author was the primary researcher and author of this paper.

FIGURES

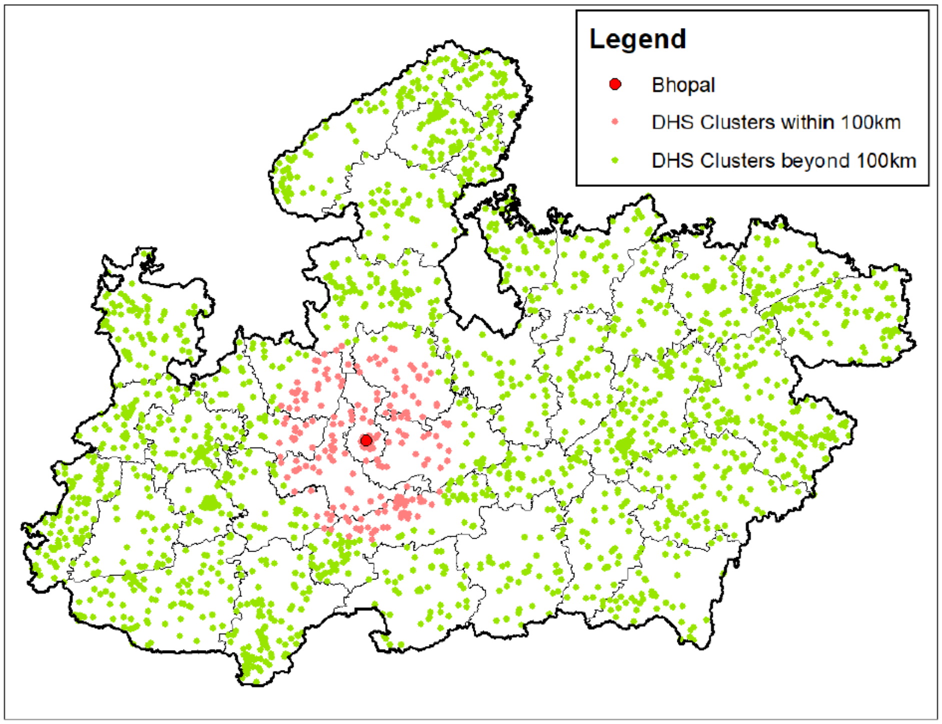
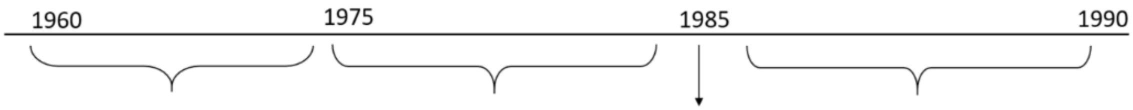


Figure 2.1: Madhya Pradesh and DHS cluster locations around Bhopal



1960-1974	1975-1984	1985	1986-1990
Reference Cohort	T1	T2	T3
>10 years old at the time of Bhopal tragedy	0-10 years old at the time of Bhopal tragedy	Fetuses at the time of Bhopal tragedy	Not yet conceived at the time of Bhopal tragedy

Figure 2.2: Description of all cohorts used in empirical estimations

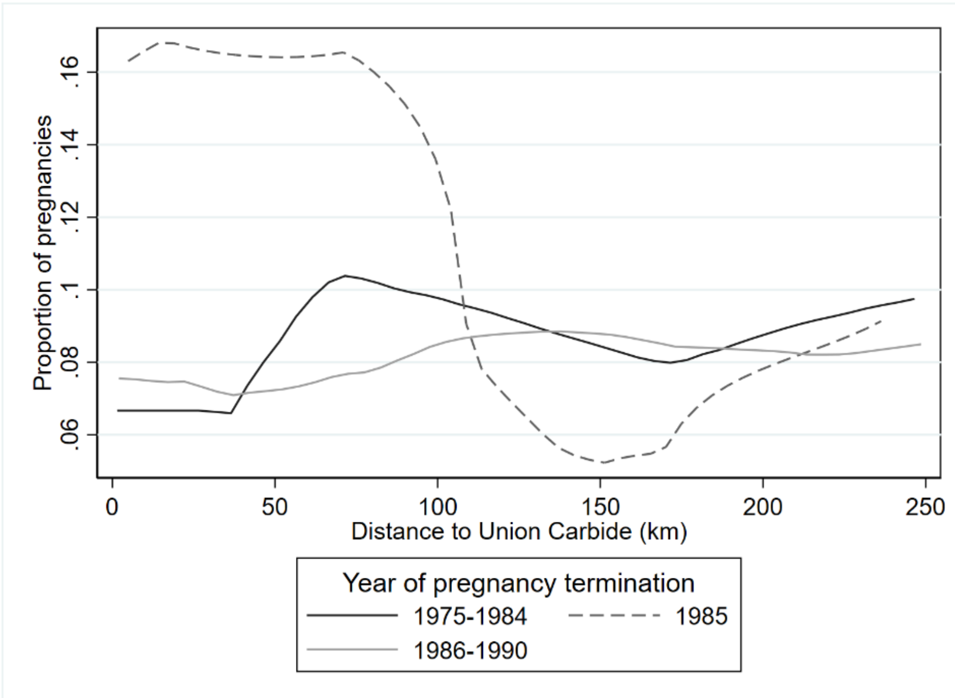


Figure 2.3: Most recent pregnancy terminations due to miscarriage, abortion or stillbirth among women aged 15-49 in Madhya Pradesh, by distance to Union Carbide

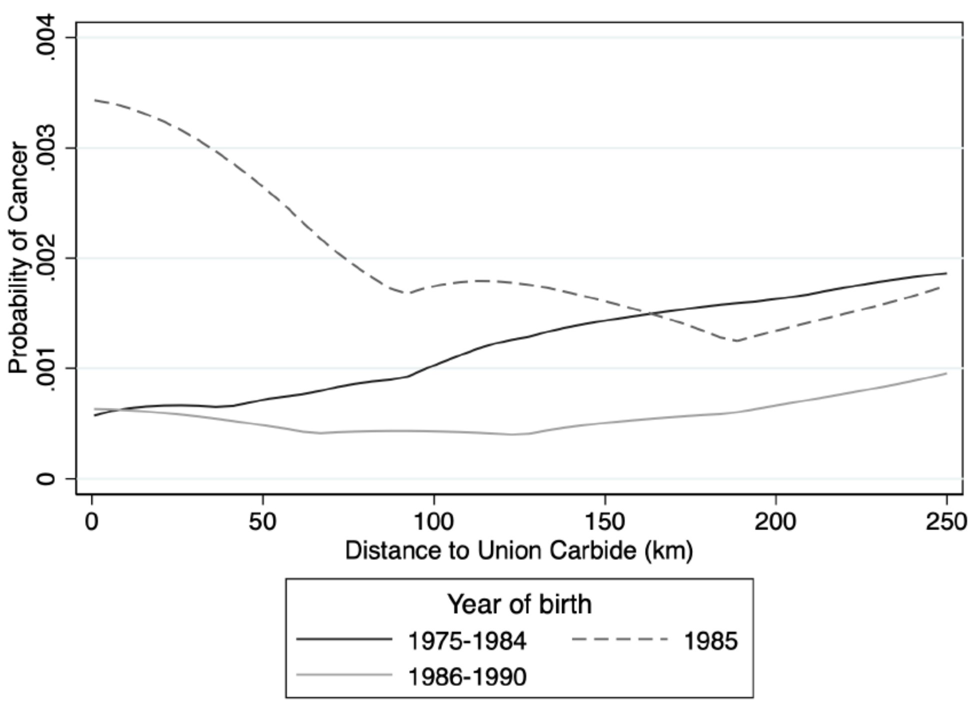


Figure 2.4: Self- reported cancer incidence among men and women living in Madhya Pradesh in 2015, by distance to Union Carbide plant

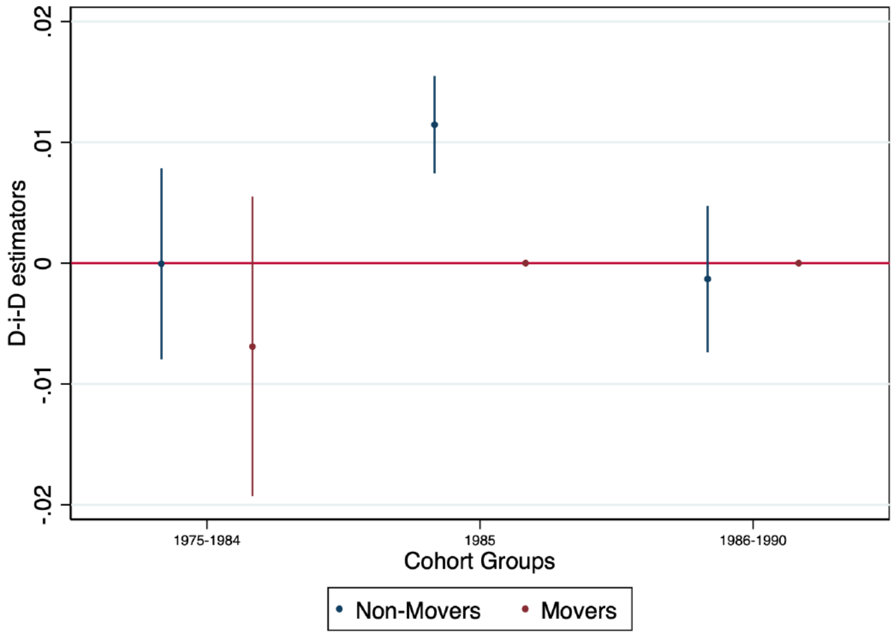


Figure 2.5: Employment disability, compared to reference group of men born from 1960-1974 and living >100 km from Bhopal

Bars represent 95% confidence intervals. Note that there were no individuals reporting employment disability in the 1985 and 1986-1990 cohorts among the *movers* subgroup.

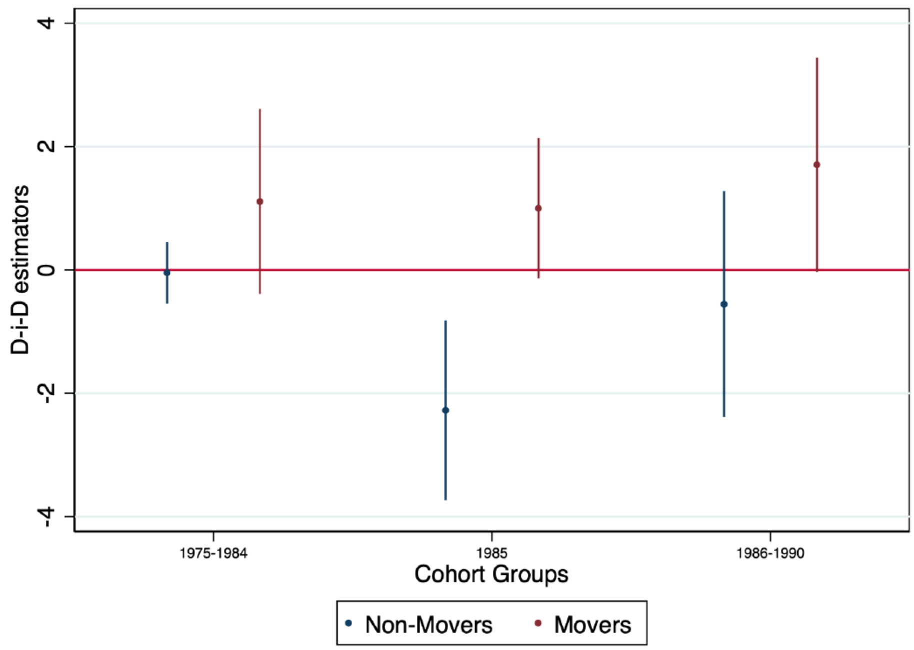


Figure 2.6: Years of education completed, compared to reference group of men born from 1960-1974 and living >100 km from Bhopal

Table 2.1: Probability of Suffering from Cancer among men born in Madhya Pradesh

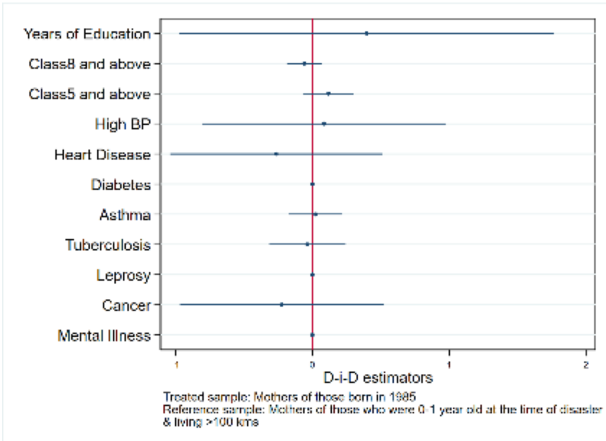
	Prob(Reports having Cancer)		
	(1)	(2)	(3)
	Pooled	Non-Movers	Movers
1975-1984	-0.00459	-0.00157	-0.00906
	(0.00355)	(0.00181)	(0.0113)
1985	0.0209***	0.0624***	-0.00906
	(0.00591)	(0.00505)	(0.0113)
1986-1990	-0.00373	0.00170	-0.0141
	(0.00340)	(0.00147)	(0.0121)
Observations	7,031	5,002	2,029
R-squared	0.007	0.012	0.014
Cohort FEs	Yes	Yes	Yes
Control Mean	0.00252	0.00228	0.00321

The regressions include Cohort FEs. Omitted category constitutes men born between 1960 and 1974 and those living 100 kms away from Bhopal. Sample includes men born between 1965-1990 and currently living in Madhya Pradesh. Column (1) uses the entire sample. Column (2) includes only those men that are living in their current residence for more than 32 years while Column (3) includes those who have lived for fewer years in the current residence.

APPENDIX

Figure S1 uses the Indian Human Development Survey (IHDS) in a robustness check to test whether mothers of those who were in-utero at the time of the BGD are different from those of other cohorts. The IHDS-I was conducted in 2004-05 and is representative at the district level. It is the only other survey apart from the DHS that collects health information for India. We compare mothers' education level and several (self-reported) health parameters, such as the probability of reporting to be suffering from a heart disease, high blood pressure, diabetes, asthma, tuberculosis, leprosy, cancer and mental illness (n=1,792).

A.



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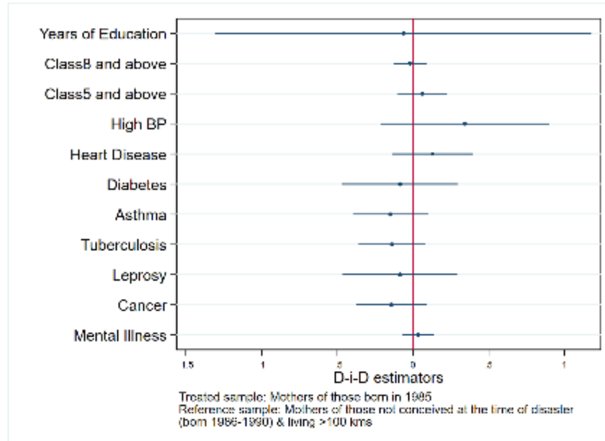

C.

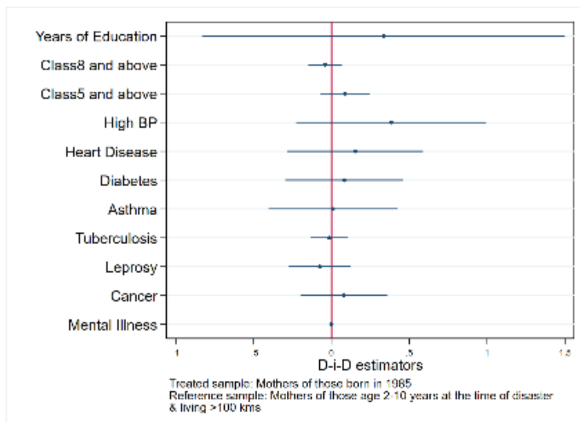

Figure S1: Characteristics of mothers of in-utero cohort compared to others

The figure shows difference-in-difference estimates for various outcome variables for the mothers of those who were in-utero (i.e, born in 1985). These estimates are obtained from regression equation similar to eq 1 where the control group includes mothers of non-mover men who were born between 1984 (i.e, < 1 year old at the time of BGD; panel A), non-mover men who were born between 1975-1983 (i.e, 2-10 years old at the time of BGD; panel B) and non-mover men who were born between 1986-1990 (i.e, not conceived at the time of BGD; panel C) and are living > 100kms away from Union Carbide plant. The figures depict estimates with 95% CI.

Table S1: Linear probability models (LPM) for employment disability among men living in Madhya Pradesh in 1999 within 100km of Bhopal, compared to men of the same cohort more than 100km from Bhopal

	Prob(Reported Employment Disability)		
	(1)	(2)	(3)
	Pooled	Non-Movers	Movers
1975-1984	-0.000632	-4.65e-05	-0.00690
	(0.00344)	(0.00388)	(0.00608)
1985	0.0111***	0.0115***	0
	(0.00216)	(0.00198)	(9.65e-11)
1986-1990	-0.00109	-0.00131	0
	(0.00257)	(0.00297)	(5.63e-11)
Observations	13,369	12,129	1,240
R-squared	0.003	0.003	0.024
Cohort FEs	Yes	Yes	Yes
Control Mean	0.00377	0.00426	0

The regressions include Cohort FEs. Omitted category constitutes men born between 1960 and 1974 and those living 100 kms away from Bhopal. Sample includes men born between 1965-1990 and currently living in Madhya Pradesh. Column (1) uses the entire sample. Column (2) includes only those men that are living in their current residence for more than 32 years while Column (3) includes those who have lived for fewer years in the current residence.

Table S2: Regression results on educational attainment among men born in Madhya Pradesh within 100km of Bhopal, compared to men of the same cohort more than 100km from Bhopal

	Years of Education Completed		
	(1)	(2)	(3)
	Pooled	Non-Movers	Movers
1975-1984	0.255	-0.0471	1.108
	(0.279)	(0.245)	(0.734)
1985	-0.577	-2.276***	0.999*
	(0.376)	(0.714)	(0.557)
1986-1990	0.221	-0.557	1.704*
	(0.601)	(0.897)	(0.851)
Observations	7,045	5,014	2,031
R-squared	0.048	0.055	0.047
Cohort FEs	Yes	Yes	Yes
Control Mean	5.833	5.561	6.600

The regressions include Cohort FEs. Omitted category constitutes men born between 1960 and 1974 and those living 100 kms away from Bhopal. Sample includes men born between 1965-1990 and currently living in Madhya Pradesh. Column (1) uses the entire sample. Column (2) includes only those men that are living in their current residence for more than 32 years while Column (3) includes those who have lived for fewer years in the current residence.

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CHAPTER 3

Effects of provision of secured Intellectual Property Rights in Developing countries: A case study from India

Arushi Kaushik[38]

Abstract

Weak property rights have shown to adversely impact economic decisions like agricultural investments and individual's labor supply choices. However, little is known about the impact of weak Intellectual Property Rights (IPR) on innovation activities. In this paper, I combine a novel dataset of >1 million patent applications from India with the firm level R&D expenditures to provide causal evidence on this question. With the announcement of TRIPS, the technology classes that were allowed to patent only the **process** innovations (a weak form of IPR) before, started showing uptick in innovation activities both in terms of patent counts and industry-level R&D expenditures, compared to technology groups that were already enjoying more *secured* **product** patenting provisions. Decomposing the industry-level impact reveals that the incremental innovations by smaller firms that require modest sums of R&D have been replaced by more expensive R&D projects by larger firms, following the adoption of TRIPS.

1 Introduction

Weak Property Rights have shown to adversely affect economic decisions like labor supply, agricultural investments etc. However, little is known about the impact of weak Intellectual

[38] Department of Economics, UC San Diego

Property Rights (IPRs) on innovation activities that are imperative for determining long term growth (Romer 1990).

IPR in the form of patent laws are one of the ways that governments use to correct the positive externalities involved in research activities due to the "public goods nature of ideas" (Williams and Sampat, 2019). These formal legal rights render the inventions as transferable assets, capable of being used as collateral to banks, for attracting venture capital funding and for licensing technologies. The provision of IPRs grant innovators temporary monopolies over their inventions to recoup the R&D costs by charging higher than competitive prices (Static Losses). In return, the innovators are required to share their invention publicly through patents, thereby supporting subsequent innovations (Dynamic Externalities). However, empirically it has been found that firms use such legal rights strategically to prevent follow-on innovations (Williams et al, 2019). Moreover, Moser (2005) posits that such laws merely bring the innovations, that would have taken place anyways, under the formal accounting system, without changing any ground reality. The net effect of introduction of such patent laws on R&D activities is, therefore, ambiguous. The trade-offs created by IPR laws get further complicated in the context of developing countries due to the additional uncertainty around the credibility of policy changes and the capacity of institutions to implement them, thus, making firms jittery about such expensive investments.

This paper attempts to answer the question- "Can the provision of *secured* IPRs encourage innovations in developing countries?", using the variation caused by TRIPS-induced legal amendments in India's IPR regime. Trade-Related Aspects of Intellectual property Rights (TRIPS) was signed by WTO (World Trade Organization) member countries in 1995. It introduced comprehensive IPR reforms in India and came into full effect in 2005. Prior to TRIPS, India's IPR environment was regulated by Indian Patents Act, 1970 that predominantly allowed "process" patenting (a weak form of IPR regulation) while granting "product" patenting only in a few categories. The implementation of TRIPS led to extension of "product" patenting to all the patent classes. This difference in the treatment of technology categories under Indian Patents Act, 1970 means that once TRIPS came in, different patent categories got affected differentially, thus, rendering a difference-in-difference strategy feasible.

Usually, such comprehensive and far-reaching legal reforms in the country are a result of many rounds of political negotiations between domestic players and the government. TRIPS was, however, different in the sense that it was lobbied by foreign corporations, especially, the pharmaceutical companies of US and Western Europe, on the pretext that it will attract enhanced R&D funding for tropical diseases. As such, these patent law reforms were exogenously imposed on the Indian players and were unrelated to the potential outcomes or any plans of such players with regard to their research endeavors. This lends TRIPS-induced legal amendments as a natural experiment that can be exploited to understand the impact of patent laws on innovation activities. Of course, the economic agents can still respond to the announcement of the policy changes and prepare themselves to take advantage of the opportunities presented by more secured IPRs. I will examine the timing of responses following both the announcement and implementation of TRIPS in 1995 and 2005 respectively.

I use two measures of innovations- count of patent applications and R&D expenditures. Use of patent counts in this context is tautological. Any changes in number of patent applications could be a mere artifact of the implementation of TRIPS that permitted filing of patent applications in certain technology categories that were hitherto not allowed. Hence, the patents counts could merely be capturing the mechanical effect of bringing such inventions, that would have taken place anyways, into formal books. Therefore, we need an independent metric with a longitudinal time series, that maps innovation activities closely. Since patent laws provide incentives for private firms more than the public sector players, it makes sense to focus on firm-level R&D efforts. Using the crosswalk provided by Lybbert and Zolas (2014), I match the patent classes to HS (Harmonized Systems) and then to NIC (National Industrial Classification) to map the patent classes to 3-digit industries and compare the outcomes of industries that uses patents from TRIPS-affected categories to those that uses patent from other classes.

I have curated a novel data set by scraping >1 million patent applications from the website of Indian Intellectual Office. The International Patent Classification (IPC), assigned by the Indian Patent Office, forms the basis of determining if an application belongs to a technology class that is affected by TRIPS or not. Categories like textiles, papers and fixed construction are used as controls while others like drug and pharmaceuticals, chemicals, food etc are included in treated group. For this study, I include the universe of all the patent applications filed by residents in India between 1990 and 2010. Firm-level information including their R&D expenditures, is obtained from Prowess database. For the period of study (1990-2010), this dataset contains information on a panel of more than 15,000 firms that are registered with the Ministry of Corporate Affairs. Inadvertently, the dataset is selecting the right-tail of firms that operate in the organized sector and potentially have resources to invest in R&D activities.

The paper presents four key findings. First, innovation activities, as measured by count of patent applications and R&D expenditures, have gone up in response to TRIPS. Compared to pre-1995, there is almost a 10-fold increase after the announcement of TRIPS in number of applications filed in TRIPS-affected categories vis-a-vis other technology groups and 125 times upsurge after the implementation of TRIPS. More importantly, this increase is contributed by “good” quality patents as reflected in rising no. of Indian applicants receiving patents abroad. According to WIPO (World Intellectual Property Organization), patents granted to residents of India by other countries went up by 3 times between 1995 and 2005 and by more than 9.5 times after 2005. This increased recognition of Indian patents abroad indicate that these are not mere imitations of previously existing patents and in fact, bring novelty with them.

Second, I find that the announcement of TRIPS provided credible enough signal to entrepreneurs about the change in IPR policy regime and encouraged them to adjust their R&D decisions accordingly. In the five years following the announcement of TRIPS, the average R&D expenditures by firms operating in TRIPS-affected industries, went up by a quarter million USD compared to other industries and relative to pre-1995. This difference-in-difference estimate further went upto USD 21 million after the adoption of TRIPS in 2005. Thus, the implementation of TRIPS had an independent impact, over and above the announcement effect of the policy. This could be a result of the formal conferment of legal rights in 2005 that enabled these firms to raise more funds from the capital markets or through licensing of technology. These additional funds could, in turn, be ploughed back into R&D.

Third, I decompose the impact of TRIPS on industry-level R&D expenditures into three effects- one that arises from differential entry and exit of firms in TRIPS-affected vis-a-vis other industries (*Compositional Changes*); two, that arises from change in proportion of firms that choose to invest in R&D (*Extensive Margin*); and three, the part that arises from increased R&D investments by a typical firm (*Intensive Margin*). After such a decomposition, I find that most of the increase in industry-level R&D expenditures is contributed by the Intensive Margin. And this holds even for the sample restricted to incumbent firms, implying that weak IPRs prior to TRIPS were a binding constraint on the firms' R&D decisions and it adversely affected the amount of private research funds that were available for innovation.

Fourth, in the long-term proportionately fewer firms are engaging in R&D activities in TRIPS- affected industries vis-a-vis other industries. Between 2000 and 2004, the relative probability of investing in R&D in such industries fell by 1.78% points as compared to pre-1995 period. This difference-in-difference estimate further fell to 2% point after 2005. This decline had been more consequential for smaller firms (those in first two quantiles of firm-size distribution) than for the larger firms. Adoption of TRIPS almost wiped out all the firms in the first quantile of the asset distribution that were previously engaged in R&D activities. The larger firms in the top quantile, on the other hand, are almost exclusively contributing to the increased Intensive Margin observed before. Thus, incremental innovations by small firms that require modest sums of money has been crowded-out by more expensive R&D projects by larger firms. This shift is symptomatic of the substitution away from process patenting and in favor of product patenting, induced by TRIPS.

This paper contributes to three strands of literature. First, the paper presents well-identified causal evidence on the impact of patent laws on innovation activities. The methodology used in this paper is an improvement over the cross-country variations in patent laws that the literature (Lerner (2009), Qian (2007), Moser (2005)) uses to study this question. Often country-specific characteristics and institutions jointly determine the effect of patent laws and failure to properly control for such differences could bias the results. Moreover, the use of exogenous variation created by TRIPS presents an even further improvement over the studies that use within-country variation like Sakakibara and Branstetter (2001). The political clout of dominant firms in influencing domestic policies could be a reason why the previous literature may have failed to find a conclusive effect of stronger patent laws on innovation activities, whether measured in patent filings (Lerner (2009)) or R&D investments (Sakakibara and Branstetter (2001)).

Second, this paper presents first estimates of the impact of TRIPS on domestic innovations in developing countries. The literature hitherto has exclusively focused on studying IPR laws in OECD countries, even while investigating the impact of TRIPS that was supposed to bring the IPR laws of developing countries at par with those of advanced economies (Kyle and McGahan (2012)). Most of the work so far has looked at the effect of TRIPS on transfer of technology (Brenstettter et al. (2006)), diffusion of drugs (Kyle and Qian (2014), Cockburn et al. (2016)) and pharmaceutical prices (Duggan et al. (2016), Chaudhuri et al. (2006)). But none on understanding its impact on developing countries' own research activities. My paper fills this gap in the literature. Developing countries often lack well-functioning IPR systems and the complementary resources to take advantage of the opportunities introduced by such laws (Qian (2007)). This could be the reason why initial attempts by Lerner (2009) and Brenstettter et al. (2006) failed to find any effect of TRIPS on patent filings by residents of such countries. However, their aggregate effects mask massive het- erogeneity among the developing countries depending on the availability of complimentary inputs and institutions, to make the TRIPS-reform usable. Not only India has a long history of working with formal IPR laws, it also has the necessary intellectual capital to take advantage of the opportunities unleashed by TRIPS. The findings of this paper are significant for developing countries since they imply that it is not the lack of entrepreneurial or innovative spirit that keeps their R&D investments constrained, rather it is the absence of "right" institutions and supporting resources that fail to provide appropriate incentives to economic agents to innovate in such countries.

Third, this paper contributes to the nascent literature that examines the impact of weak property rights on various economic decisions like agricultural investment (Huntington and Shenoy (2021), Hornbeck (2010)) and labor supply (Field (2007)). To the best of my knowledge, this is the first paper that studies how the absence of secured intellectual property rights affect R&D investment decisions of the firms. Innovation and research are consequential for determining the long-term growth rate of the economy and it is imperative to understand the factors that impede them. In addition, my paper looks at the downstream effect of the patent laws on the distribution of innovation activities amongst the firms. This complements the recent work by Autor et al. (2020) on superstar firms and the role of innovations in bolstering their product market concentration. The crowding- out of smaller firms from research activities by larger firms could have significant implications for the industrial structure in the long run if left unchecked.

The rest of the paper is organized as follows. Section 2 describes the TRIPS agreement and India's IPR regime prior to TRIPS. Section 3 provides descriptive evidence from count of patent applications, obtained from Indian Patent Office and WIPO. Section 4 presents results on industry-level R&D expenditures and lays down a framework to guide the empirical strategy in Section 5. Section 6 presents the results from this empirical analysis. Finally, Section 7 concludes.

2 Context

India has had experience of working with formal patent laws under the British rule. First such law was enacted in 1856 and aimed at encouraging "inventions of new and useful manufactures and to induce inventors to disclose secret of their inventions". After independence, the country passed its first modern law in 1970, called the **Patents Act, 1970**, which repealed all the previous IPR laws. This law remained in force till December 1994 when an ordinance was passed to give effect to some provisions of the TRIPS agreement. However, this ordinance ceased to operate after six months. The next 10 years saw passage of many ordinances and by-laws to give effect to the provisions of the TRIPS agreement.

2.1 Indian Patents Act, 1970

Prior to TRIPS, India's IPR regime was governed by Indian Patents Act 1970. Section 5 of the Act explicitly prohibited granting of patents for the "substances themselves" in case of inventions (1) claiming to be used as food, medicine or drug; (2) relating to substances "prepared or produced by chemical processes (including alloys, optical glass, semi-conductors and inter-metallic com- pounds)". However, methods and processes of manufacture for the same were patentable. Hence, these categories had to contend with a weaker form of IPRs, ie., process patenting while all other groups were allowed to patent both the process and product themselves. Infringement of process patenting is often hard to prove in the court of law. Moreover, many of the products in these technology groups can be reverse engineered, making it hard to use trade secrecy to maintain the competitive advantage. Consequently, the Act inadvertently promoted incremental innovations in the methods and processes of manufacturing while obliterating the incentives for disruptive inventions.

2.2 Trade-Related Aspects of Intellectual Property Rights (TRIPS)

TRIPS was signed by members of World Trade Organization (WTO) in 1995. The agreement laid down the **Standards**, rules for **Enforcement** and **Dispute Settlement Mechanism** (DSM) concerning Intellectual Property Rights. Thus, the subject matter of the patents, term of protection and the extent of rights of patentees are all defined by the agreement. In addition, it lays down a provision allowing disputes relating to non-compliance with the agreement to be resolved through the WTO Dispute Settlement Mechanism. This ensures that the provisions of the agreement are followed in letter and spirit. Developing countries were given several exceptions like a 10-year phase-in period to bring their domestic IPR laws in compliance with TRIPS' requirement and a provision of compulsory licensing in case of national health emergency.

In India's context, these provisions meant that the provision of product patenting will be extended to all the technology classes. Given the initial discrepancy in the treatment of patents relating to food, medicine and drugs, as well as alloys, optical glass, semi-conductors and inter-metallic compounds, this extension implies that different technology groups are affected differentially once the TRIPS came into effect. This gives us the opportunity to treat the subjects of inventions that were not allowed product patenting under Indian Patents Act, 1970 as 'treated' group and other categories as 'control' group.

This lends a perfect natural experiment to study the impact of provision of 'secured' IPRs on innovation activities. Usually, such major legal changes are influenced by the lobbying from special- interest groups, like domestic industries, thereby biasing the effects of such laws on outcomes that directly affect these politically influential groups. However, TRIPS was exogenously imposed on the Indian industries. It was lobbied by the pharmaceutical companies of US and western Europe, on the pretext that this will attract R&D funding for finding the treatment of tropical diseases. Of course, such promises never came to fruition (Kyle and McGahan (2012)). But the agreement inadvertently laid the foundations for building a local research base in the developing countries. Not only it brought the domestic laws of these countries at par with those of OECD, it also solved the enforcement problem by allowing dispute settlement through WTO's DSM.

Although TRIPS was announced in 1995, it came into full effect in India in 2005. India passed several pieces of legislation in the intervening period to comply with the requirements of TRIPS. In 1999, India passed a law accepting applications for product patents "in the areas of drugs, pharmaceuticals and agro-chemicals” even though such applications were to be examined only after December 31, 2004. This granted the applicants Exclusive Marketing Rights (EMR) to sell or dis- tribute these products in India. Another major legislative change in 2002 increased the patent term from 7 years to 20 years for food, medicine and drugs category and from 14 years to 20 years for all other categories.

3 Data

3.1 Evidence from Patent Counts

I have curated a novel data set by scraping >1 million patent applications from the website of Indian Intellectual Office. It contains universe of all the patent applications filed by residents as well as non-residents in India and contains the standard information required by Patent Cooperation Treaty (PCT). The Indian patent office assigns International Patent Classification (IPC) to each application according to the guidelines of the WIPO. These IPC categories determine the TRIPS- affected status of an application in my study. Consequently, technology categories like textiles, papers and fixed construction are used as controls while others like drug and pharmaceuticals, chemicals, food etc are included in treated group.

For the purpose of this analysis, I restrict the dataset to include the universe of all the patent applications filed by residents in India between 1990 and 2010. The period was marked by a sharp increase in patent applications. Between 1990 and 1994, the no. of patent applications filed by residents in India, even after aggregating across all the IPC categories and over all the five years, totaled up to 26 only. This no. began to increase after the announcement of TRIPS in 1995 and went upto ≈6,000 per annum after 2005.

Figure 3.1 shows the count of patent applications filed in TRIPS-affected categories vis-a-vis other IPC classes, relative to the year 1995. Compared to pre-1995, the count of patent applications for TRIPS-affected categories v/s others, increased 10-fold after the *announcement* of TRIPS in 1995 and by 125 times after the *implementation* of TRIPS in 2005. More importantly, this increase is contributed by “good” quality patents as reflected in the rising no. of applications by Indian residents obtaining patents abroad.

3.2 Quality of Patents

WIPO is one of the specialized agencies of United Nations that gathers and maintains annual data on the number of patent applications filed by residents of the member countries and track the grant of patents to such applicants in various countries, even outside of their domicile. These data are available by country and by technology groups.

Figure 3.2 depicts number of patents granted to Indian applicants abroad in TRIPS-affected technology categories versus other groups, relative to year 1995. According to WIPO data, the presence of Indian patentees abroad was almost negligible prior to 1995 but it changed once TRIPS was announced. The patents granted to residents of India by other countries went up by 3 times between 1995 and 2005 and by more than 9.5 times after 2005. Almost exclusively, all of this increase is contributed by TRIPS-affected patent classes of technology. This increased recognition of Indian patents abroad indicate that these are not mere imitations of previously existing patents and in fact, bring novelty with them.

3.3 Concordance

Patent counts are used as a measure of innovation since the seminal work of Griliches (1990). But recent works by Moser (2003, 2005) have raised concerns about what do these patent counts actually reflect. Examining the impact of IPR using patents as a proxy for innovation is tautological. The rise in patent counts, seen in Figure 3.1, can be a mere artifact of adoption of TRIPS that enabled the Indian patent office to formally accept applications in certain technology categories that were hitherto not allowed. Hence, it is possible that the patent counts are merely capturing the mechanical effect of bringing such inventions, that would have taken place anyways, into formal books. Therefore, we need an independent metric with a longitudinal time series, that maps innovation activities closely. Since patent laws provide incentives for private firms more than the public sector players, an obvious choice for such a variable would be firm-level R&D expenditures. This requires mapping of patent classes to industrial groups which is a non-trivial task. Moreover, evaluating the effectiveness of this mapping depends on what we intend to do with it.

Lybbert and Zolas (2014) provide one such mapping based on USPTO data. USPTO requires applicants to mention industries where the invention described in the patent application can find a practical use. Of course, any such mapping can only under-estimate the true extent to which a technology will be used in future e.g, the use of cameras in mobile phones was not imagined originally when a camera was invented. However, for my purposes, this mapping is ideal. Any industry that cannot be envisaged to make use of the described technology immediately, will also not have any incentive to contribute towards its research and development. In other words, all the industries that have any incentive to invest in R&D funding for developing a technology, are captured by this mapping.

Using the crosswalk provided by Lybbert and Zolas (2014), I match the International Patent Classification (IPC) to Harmonized Commodity and Description Coding System (HS) 2002. Using the concordance tables of Topolova (2010), I match these HS groups to National Industrial Classification (NIC), India's industrial classification system. Since the concordance tables from Topolova (2010) are available only for agriculture and manufacturing sectors, I end up mapping the patent classes to 85 three-digit non-services sector industries. Examples of some of the industries that get categorized as TRIPS-affected as a result of this mapping include Manufacturing of food products; Manufacturing of pharmaceuticals, medical chemicals and botanical products etc. While Manufacture of textiles, Manufacture of leather and related products etc. are examples of other industries that are presumed to be not affected by TRIPS. We can use this mapping to classify firms as operating in TRIPS-affected industries versus those operating in other (non-TRIPS) industries.

Next, I use Prowess data to investigate the effect of TRIPS on industry-level R&D expenditures. Prowess dataset consists of the firms that are incorporated with the Ministry of Corporate Affairs which may or may not be listed on the stock markets. Thus, we are already dealing with the right- tail of firms that exist in organized sector and potentially have the resources to invest in R&D. For the period of study, i.e., 1990-2010, the dataset contains a panel of more than 15,000 formal sector firms and includes information about their NIC codes, R&D expenditures, sales and assets.

4 R&D Expenditures at Industry level

The surge in patenting activity following the announcement and implementation of TRIPS is mirrored by the Industry-level R&D expenditures, as seen in Figure 3.4. Compared to pre-1995, the average R&D expenditures in TRIPS-affected industries went up by USD 2.2 million (58%↑) between 1995 and 2000 vis-a-vis non-TRIPS industries and this difference-in-difference estimate further went upto ≈ USD 21 million after 2005. Thus, the changes observed in Figure 3.1 represents a fundamental shift in innovation activities and do not represent a mechanical effect of TRIPS' allowance of product patent applications in new technology categories. Both patent counts and R&D investments, therefore, provide consistent evidence of the fresh impetus introduced by the TRIPS to the innovation culture in India.

To uncover the source of this increase and characterize who is engaging in R&D, I decompose the industry-level R&D expenditures into firm-level decisions in the next section.

4.1 Decomposition of Industry-level R&D

Total R&D at the industry level can be written as the no. of firms spending on R&D (P_i) times the average R&D expenditures made by such firms:

$$\sum_{f:f\in i} R\&D_f = P_i \ \mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f > 0\right] \qquad (1)$$

where, industry-level R&D, $\sum_{f:f\in i} R\&D_f$ will be estimated by $\mathbb{E}_i[R\&D_i|]$. The number of firms in industry *i* spending on R&D (P_i) can further be decomposed as the product of universe of all the firms in the dataset (*N*) times the probability that a firm chooses to operate in industry *i* ($\Pr(f \in i)$) times the probability that such a firm chooses to spend on R&D:

$$P_i = N \cdot \Pr(f \in i) \cdot \Pr\left(R\&D_f > 0 \mid f \in i\right) \qquad (2)$$

Combining equations (1) and (2), we have

$$\sum_{f:f\in i} R\&D_f = N \cdot \Pr(f \in i) \cdot \Pr\left(R\&D_f > 0 \mid f \in i\right) \cdot \mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f > 0\right] \qquad (3)$$

Thus, industry-wide R&D expenditures consists of three time-varying quantities- 1. whether a firm *f* chooses to operate in industry i, $\Pr(f \in i)$; 2. conditional on operating in industry i, does the firm *f* choose to spend on R&D, $\Pr\left(R\&D_f > 0 \mid f \in i\right)$; and 3. conditional on choosing to spend on R&D, how much does firm *f* spend on R&D, $\mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f > 0\right]$.

Consequently, the effect of TRIPS on industry-wide R&D is given by:

$$\Rightarrow \frac{d \sum_{f:f\in i} R\&D_f}{d\,TRIPS} = \underbrace{\frac{d \Pr(f\in i)}{d\,TRIPS}}_{\text{Compositional Changes}} \cdot b \cdot c + \underbrace{\frac{d \Pr\left(R\&D_f>0 \mid f\in i\right)}{d\,TRIPS}}_{\text{Extensive Margin}} \cdot a \cdot c + \underbrace{\frac{d\,\mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f>0\right]}{d\,TRIPS}}_{\text{Intensive Margin}} \cdot a \cdot b \qquad (4)$$

where $a = \Pr(f \in i)$, $b = \Pr\left(R\&D_f > 0 \mid f \in i\right)$ and $c = \mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f > 0\right]$.

That is, the impact of TRIPS on industry-wide R&D is a weighted average of three effects:

1. **Compositional Changes:** $\frac{d\ \Pr(f \in i)}{d\ TRIPS}$ measures the impact of TRIPS on the fraction of firms operating in an industry after netting the effect of exit and entry. TRIPS may attract high-tech firms to enter the industries that previously did not enjoy secured IPRs. Secured IPRs may reduce the entry cost of start-ups by giving them access to early-stage capital either by using the patented technology as collateral at banks or through venture capitalists. On the flip side, the same secured property rights could discourage the start-ups from entering the industry if "fundamental" research is already patented by big firms and it is difficult to patent the follow-on innovations done by newer and smaller firms. The fraction $\Pr(f \in i)$ captures the number of firms that continue operating in industry *i* excluding those that exit the industry, but inclusive of the firms that enter the industry *i*.
2. **Extensive Margin:** $\frac{d\ \Pr(R\&D_f > 0 \mid f \in i)}{d\ TRIPS}$ measures the impact of TRIPS on the proportion of firms that are operating in industry *i* that choose to allocate any money to R&D investments. The proportion of firms investing in R&D can go up both because more innovative firms enter such industries as well as due to the fact that many incumbent firms who were not investing in R&D earlier may now be attracted to do so once TRIPS provides security to the expected returns from such investments. However, the *discouragement effect* discussed above could show up here as well if follow-on innovations become hard to patent, causing some firms to opt out of research activities.
3. **Intensive Margin:** $\frac{d\ \mathbb{E}_{f:f \in i}[R\&D_f \mid R\&D_f > 0]}{d\ TRIPS}$ measures how much does the average R&D expenditures by firms, who choose to invest in R&D, change as a result of TRIPS. This effect could either go up or down, depending on which firms choose to undertake R&D

investments. If the increased investments come from larger firms, then the average R&D amount will go up. While, if most of the marginal increase is coming from smaller firms, then average R&D investments will fall[39].

The increase in R&D expenditures at industry-level, as seen in Figure 3.4, therefore, could be a result of more firms entering the TRIPS-affected industries vis-a-vis others (*compositional changes*); or it could arise from more firms deciding to invest in R&D without any changes in the average amounts of investment (*extensive margin*); or it could be a result of the same firms investing more funds into R&D (*intensive margin*). Of course, in reality, all the three effects are at work at the same time, which means that it is entirely possible for these three effects- *compositional changes*, *extensive margin* and *intensive margin*- to operate in different directions as long as the negative effects are compensated by the positive ones, yielding an overall positive impact on the industry-level R&D. For instance, consider a situation where the net entry of firms in TRIPS-affected industries is similar to that observed in other industries. If all the firms that are highly productive in doing R&D activities were already investing in it prior to TRIPS, then the observed rise in industry-level R&D will increasingly come from marginal or **fringe firms** starting their own research labs with lower average R&D investments than the amounts invested by the incumbent firms. Thus, the *Extensive Margin* effect will be positive, but the *Intensive Margin* effect will be negative. The opposite would be the case if more of the **larger firms** are encouraged to enter the R&D activities. Even if TRIPS induces only a few of them to invest in R&D, the deep pockets of such firms will ensure that the average amount invested by each of them is sufficient to achieve scale economies to begin any meaningful innovations in the respective industries.

[39] Of course, the implicit assumption here is that the firms contribute to R&D expenditures in proportion to their sizes.

Whether the increase in industry-level R&D expenditure of Figure 3.4 is coming from fringe firms or from large firms may not be a first-order concern for a government whose priority is to attract higher research funding. But this *Extensive Margin* effect may prove to be consequential for the industrial structure in the long-run as pointed out by the recent literature (Autor et al. (2020)) on superstar firms and the role that innovation may play in increasing the product market concentration of such firms.

A final point to note is that the increase in R&D investments at industry- or firm-level, per se, does not necessarily imply that there were binding constraints in terms of intellectual property protection prior to TRIPS. For example, if the announcement of TRIPS increases investors' attention to certain industries more than the others, then we will see an uptick on all the three margins- *compositional changes*, *extensive margin* and *intensive margin*- as more innovative domestic and foreign firms begin to set up their businesses in India in "TRIPS-affected" industries. However, it should have no impact on the R&D decisions of the incumbent firms who were already optimizing their R&D activities amidst no concerns regarding protection of IPRs in pre-TRIPS period. Thus, all we observe in this case is the effect of advertisement or salience created by TRIPS-announcement or implementation.

To disentangle how much of the effect is coming from weak intellectual property rights and how much of the observed change is an effect of salience or other factors like ease of barriers to entry, we need to focus on *incumbent* firms. This can help us to investigate the impact on Extensive and Intensive Margins arising due to greater protection provided by TRIPS to intellectual goods.

4.2 R&D Expenditures by Incumbents: A Simple Model

Here is a simple model, adapted from Budish et al. (2016) to understand the R&D decisions by incumbent firms.

Having decided to invest in R&D, a firm can invest $k \in K$, set of potential inventions. For a project k, the associated R&D cost is c_k and the probability of success is p_k. Without loss of generality, further assume that projects are arranged in increasing order of R&D costs, ie., if $l > k$, then $c_l \geq c_k$. If successful, the firm can earn monopoly profits, π_k on this invention for a period of time, $t = min\{t_{patent\_term}, t_{effective\_useful\_life}\}$. That is, the monopoly profits can be earned for atmost the time period the patent protection is available for (t_{patent_term}) or the time period before which the invention becomes obsolete due to introduction of a better technological substitute ($t_{effective\_useful\_life}$), whichever occurs earlier.

In the absence of product patenting, the expected profits from the invention are further threatened by the inability of the firm to defend patent infringement. Let q be the probability that patents can be defended in the court of law. Therefore, a firm will invest in a project k if and only if the net expected benefits are positive:

$$\underbrace{q \cdot [p_k \cdot t \cdot \pi_k]}_{\text{Expected Benefits}} \geq \underbrace{c_k}_{\text{Cost of Project } k} \tag{5}$$

Prior to 1995, for non-TRIPS industries, $q = 1$ and for TRIPS-affected industries, $q < 1$. After implementation of TRIPS in 2005, for both types of firms, $q = 1$. That is, $\frac{d\,q}{d\,TRIPS} > 0$ in the long run which means eq 5 will be satisfied for a project $k > j$ such that $c_k > c_j$. So, we have,

$$\frac{d\, q}{d\, TRIPS} > 0 \implies \frac{d\, c}{d\, TRIPS} = \frac{d\, \mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f > 0\right]}{d\, TRIPS} > 0 \qquad (6)$$

Therefore, in the long run as uncertainty about patent protection is resolved and q changes from $q < 1$ to $q = 1$ for TRIPS-affected industries, more expensive projects are undertaken by firms, which means the average R&D expenditures by firms that have decided to invest in R&D goes up by the amount $\frac{d\, \mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f > 0\right]}{d\, TRIPS}$.

However, between 1995 and 2005, R&D investments depend on entrepreneurs' expectation of what q is going to be: q^e. Therefore, during transition phase, a firm will invest in project k if and only if:

$$q^e \cdot [p_k \cdot t \cdot \pi_k] \geq c_k \qquad (7)$$

And similar to before, an upward updation of q^e will lead to increased investments in R&D by firms:

$$\frac{d\, q^e}{d\, TRIPS} > 0 \implies \frac{d\, c}{d\, TRIPS} = \frac{d\, \mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f > 0\right]}{d\, TRIPS} > 0 \qquad (8)$$

Therefore, it becomes imperative to figure out *when do the expectations get updated from $q^e < 1$ to $q^e = 1$ so that $\frac{d\, q^e}{d\, TRIPS} > 0$, i.e., when does the uncertainty resolve?* Does it happen when TRIPS is announced in 1995 (*Announcement Effect*) or does the uncertainty resolve when it is finally implemented in 2005 (*Implementation Effect*)?

4.3 Announcement v/s Implementation Effect

Rational economic agent acts when the information is received. Hence, it is sensible to expect firms to respond to the announcement of TRIPS in 1995 itself (**Announcement Effect**), especially when the stakes of being the *first-mover* are as high as in the innovation sector[40]. However, policy uncertainty is abound in developing countries. Too often, governments in developing countries "commit" to a decision that they either delay in implementing or altogether revert back later on. In such an environment, even rational firms may need a credible signal of policy change before they act. Moreover, if the firms believe that the local institutions, especially, the courts would not be able to interpret the provisions of the law in the letter and spirit, they may be reluctant to undertake expensive R&D investments. As such, we may not see any effect of TRIPS until 2005 (**Implementation effect**). Moreover, legal rights for ownership of IPR, that enables firms to collateralize the IPR assets or to license it to other businesses or use them for raising money from stock markets or venture capitalists, were conferred only in 2005. This could generate new sources of revenue which could further be ploughed back into more R&D. As such, the implementation of TRIPS in 2005 could have an independent effect. Thus, TRIPS could have both an *announcement* and *implementation* effect on firms' R&D decisions.

5 Empirical Strategy

Next, we need to empirically estimate the effect of TRIPS on *compositional changes* ($\frac{d\ \Pr(f \in i)}{d\, TRIPS}$), *extensive margin* ($\frac{d\ \Pr\left(R\&D_f > 0 \mid f \in i\right)}{d\, TRIPS}$) and *intensive margin* ($\frac{d\ \mathbb{E}_{f:f\in i}\left[R\&D_f \middle| R\&D_f > 0\right]}{d\, TRIPS}$) as well as track the timing of such changes.

[40] There are various channels through which first mover advantage works in innovation sector. e.g., Even though a patent in the pharmaceutical category can only be granted after 2005, there is still incentive for drug companies to file the patent application as early as possible since in the event of infringement of IPR, the restitution damages are calculated from the date of patent application filing and not from the date of grant. Another advantage of early patenting is that any follow-on innovation that uses the technology described in the earlier patent, must pay royalties to the former before patenting/using this follow-on technology.

5.1 Identification Strategy

To estimate the impact of TRIPS on these three margins, we will employ the difference-in-difference strategy and track the unfolding of these effects year-over-year to delineate the announcement v/s the implementation effect.

We compare the industries that are related to the patent categories that got differentially affected by TRIPS with those that did not. Recall that process patenting was available to all patent classes, but product patenting was restricted to few groups prior to TRIPS. Consequently, the research activities of the industries that should be affected by TRIPS are the ones that have strong incentives to invest in R&D of the technologies that can now be patented under the affected patent categories post-TRIPS implementation.

To obtain the *announcement* effect on these margins, we compare the outcome variables of firms operating in TRIPS-affected industries from pre-1995 period to post-1995 vis-a-vis those operating in other industries during the same time. The comparison with firms operating in non-TRIPS industries is useful for controlling for other macroeconomic factors that may be changing around the same time. While the pre- and post-TRIPS comparison within the same industry is imperative to ensure that the heterogeneity across industries in terms of innovation opportunities and operations gets accounted for.

To estimate the *implementation* effect, we make a similar comparison of firms operating in TRIPS- affected industries vis-a-vis others but this time, the outcomes of years 2000 to 2004 are compared with those of 2005-onwards. The time between 1995 and 2005 is an opportunity for firms to ramp-up their research capabilities to take advantage of the forces that will be unleashed by TRIPS in 2005.

5.2 Announcement Effect

The main specification is as follows:

$$Y_{f,i,t} = \alpha_i + \alpha_t + \alpha_1 * TRIPS - affected_i * Post1995_t$$
$$+ \alpha_2 * TRIPS - affected_i * Post2000_t + \varepsilon_{f,i,t} \quad (9)$$

where $TRIPS - affected_i$ = 1, if Industry i is affected by TRIPS and 0 otherwise; $Post1995_t$ = 1 if year is between 1995 and 1999, and 0 otherwise; likewise, $Post2000_t$ = 1 if year between 2000 and 2004 and 0 otherwise; α_i absorbs industry-specific time-invariant factors that may influence the outcome variables, e.g., typical R&D investments in pharmaceutical industry are much higher than in food industry; similarly, α_t captures year-specific shocks that are common to all industries, e.g, the financial crisis of 2008.

As motivated by the discussion in section 4.1, we will inspect the effect of TRIPS on three outcome variables, $Y_{f,i,t}$: $\Pr(f \in i)$, $\Pr\big(R\&D_f > 0 \mid f \in i\big)$ and $\mathbb{E}_{f:f\in i}\big[R\&D_f \big| R\&D_f > 0\big]$. The sample set for each of these outcomes will be different since these are conditional expectations[41]. Therefore, to capture the *Compositional changes*, $Y_{f,i,t}$= 1 if $f \in i$ and 0 otherwise and the eq 9 will be estimated on the entire sample of firms; to capture the *Extensive Margin Effect*, $Y_{f,i,t}$= 1 if R&D spending by firm *f* is positive in year *t* and 0 otherwise and the eq 9 will be estimated for sample of firms that are operating in industry *i* in year *t*; finally, to capture the *Intensive Margin Effect*, $Y_{f,i,t}$= firm-level R&D expenditures and the eq 9 will be estimated on the sample of firms that are spending on R&D[42] in year *t*.

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[41] Probabilities can be written as expectations of Indicator functions. As such, $\Pr(f \in i)$= $\mathbb{E}_{f:f\in i}[\mathbb{I}\{f \in i > 0\}]$and $\Pr\big(R\&D_f > 0 \mid f \in i\big)$ = $\mathbb{E}_{f:f\in i}\big[\mathbb{I}\{R\&D_f > 0\}\big]$. $\mathbb{I}$ is the indicator function taking value 1 if the statement in the brackets is true and 0 otherwise.

[42] The introduction of industry fixed effects (α_i) in eq 9 ensures that identifying variation for estimating Extensive and Intensive margin is contributed only by firms that are specific to the corresponding industry.

Standard Errors are clustered at 3-digit industry level to flexibly adjust for both serial correlation within the firm level outcomes over the years as well as to allow the outcomes to be correlated across the firms within the same three-digit industry. For announcement effect, the sample is restricted from 1990 to 2004.

α_1 and α_2 are the main parameters of interest that capture the *announcement* effect. α_1 compares the outcomes for the period between 1995 and 1999 with pre-1995 period for firms in TRIPS-affected industries vis-a-vis others. α_2 does the same but it compares outcomes for the period 2000-2004 with that of pre-1995. Important legislative changes such as accepting patent and Exclusive Marketing Rights applications from pharmaceutical companies; and extension of patent term, could affect firm's economic behavior in early 2000s. α_2 aims at capturing these effects. Even if firms do not find announcement of TRIPS itself to be a credible enough signal of policy change (i.e, α_1 = 0), the legislative changes brought in 1999 and 2002 could impact firm's beliefs about the government's intention to implement the same (i.e., $\alpha_2 \neq 0$).

5.3 Implementation Effect

The main specification is as follows:

$$Y_{f,i,t} = \beta_i + \beta_t + \beta_1 * TRIPS - affected_i * Post2005_t + \varepsilon_{f,i,t} \tag{10}$$

where, $Post2005_t$ = 1 if year is ≥ 2005, and 0 otherwise; $TRIPS - affected_i$ is as defined before; β_i and β_t are respectively the industry fixed-effects and the year fixed-effects. Similar to before, three outcome variables are considered, conditional on different sample spaces:

$$Y_{f,i,t} = \begin{cases} \mathbb{I}\{f \in i\}, for\ the\ entire\ sample\ of\ firms \\ \mathbb{I}\{R\&D_f > 0\}, for\ the\ sample\ of\ firms\ that\ are\ operating\ in\ period\ t \\ R\&D\ \ , for\ the\ sample\ of\ firms\ that\ are\ spending\ on\ R\&D \end{cases}$$

to capture the *compositional changes*, the *extensive margin* and the *intensive margin* effects respectively. Standard Errors are clustered at 3-digit industry level. For implementation effect, the sample is restricted from 2000 to 2010.

β_1 is the *implementation effect* of TRIPS. It compares outcomes for period between 2000-2004 with that of post-2005 for firms operating in TRIPS-affected industries vis-a-vis others. β_1 needs to be interpreted in conjunction with α_1 and α_2. If both α_1 and α_2 are zero, then announcement of TRIPS did not instill enough confidence among the firms to undertake the expensive research investments and they waited out till 2005 before committing any resources to R&D (i.e, $\beta_1 \neq 0$). We can safely rule out the case where the implementation of TRIPS in 2005 does not move firms' behavior on any of the three margins (i.e, β_1 = 0) since Figure 3.4 already shows that TRIPS had an impact on overall R&D. Consider another case where either $\alpha_1 \neq 0$ or $\alpha_2 \neq 0$, i.e, firms trusted the *announcement* of TRIPS and began ramping up their research capabilities after 1995 itself. In this case, β_1 captures the in- dependent effect of *implementation* of TRIPS, over and above the *announcement* effect of the policy in 1995. One instance where this could happen is when firms are able to raise higher resources from capital markets or through licensing of technology after establishment of property rights in 2005 (i.e, $\beta_1 > 0$).

5.4 Heterogeneity

5.4.1 Heterogeneity wrt firm size

As discussed in section 4.1 the observed increase in industry-level R&D investments could either be coming from a large number of **fringe firms** setting up their R&D shops or it could be a result of fewer **large firms** increasing the average R&D amount that each of them invest.

To examine from which part of the firm size distribution are the effects coming, for each year, we will divide up the entire universe of firms in every 3-digit industry into four quantiles according to their asset sizes. And then re-run equations (9) and (10) for each quantile separately. If we find *announcement* or *implementation* effect only for higher quantiles, then it supports the hypothesis that TRIPS mainly encouraged the **larger firms**, who have the resources and risk-appetite, to undertake more expensive and/or uncertain R&D investments. On the other hand, if the effects are evenly distributed across the quantiles or if the effects are concentrated among lower quartiles instead of the upper ones, then this gives more credence to the story about **fringe firms** being able to take advantage of the opportunities brought by TRIPS through greater access to capital markets as well as more protection for the expected returns on their intellectual goods.

5.4.2 Decomposition for Incumbents

In estimation of equations (9) and (10), the sample that contributes to identifying variation, consists of following types of firms:

1. **Incumbents firms**: firms who were operating in industry i in pre-TRIPS period as well in post-TRIPS period, i.e, f: $(f_{pre-TRIPS} \in i \wedge f_{post-TRIPS} \in i)$
2. **New firms**: firms who were not operating in industry i in pre-TRIPS period but began operations in post-TRIPS period, i.e, f: $(f_{pre-TRIPS} \notin i \wedge f_{post-TRIPS} \in i)$

The sample of **new firms** could further be sub-divided into:

(a) **Pure crowd-ins**: firms that did not operate in any other industry before, i.e., f : $(f_{pre-TRIPS} \notin any\_industry \wedge f_{post-TRIPS} \in i)$

(b) **Switchers**: firms that were operating in other industries before, i.e., $f:(f_{pre-TRIPS} \in j \neq i \wedge f_{post-TRIPS} \in i)$

The composition of our sample is given in Figure 3.3[43]. An overwhelming majority (72%) of the sample consists of **new firms**, followed by **incumbents** that constitute 22% of the firms. There are only few switchers and their numbers do not vary differentially between TRIPS-affected and other industries.

As discussed in section 4.1, in order to disentangle the constraints imposed by weak intellectual property protection from salience created by TRIPS announcement and ease of entry barriers caused by TRIPS implementation[44], we need to focus on the incumbent firms. Consequently, we re-estimate the *announcement* and *implementation* effects on Extensive and Intensive margins by restricting the sample to only the incumbents.

If these incumbent firms were already optimizing their R&D decisions prior to TRIPS and there were no concerns regarding IPR protection, then neither the *announcement* nor *implementation* of TRIPS should have any impact whatsoever. This means that all the three coefficients, α_1 or α_2 and β_1 should be zero for both the Extensive and the Intensive Margin. Conversely, if either $\alpha_1 > 0$ or $\alpha_2 > 0$ or $\beta_1 > 0$, especially for the Intensive Margin, then one can conclude that weak intellectual property protection was a binding constraint that impeded the firms' innovation activities prior to 1995.

[43] The Complete Exits are defined as firms that were operating in pre-TRIPS period but not after: i.e, $f:(f_{pre-TRIPS} \in i \wedge f_{post-TRIPS} \notin any\_industry)$. Since these firms drop out, they do not contribute to identifying variation and hence, are ignored for rest of the analysis.

[44] L egal Rights conferred by TRIPS render the IP assets transferable. Hence, they can be used as collateral to raise money from banks or from venture capitalist. This increased access to capital markets eases entry barriers for more productive/innovative firms.

6 Results

6.1 Overall Decomposition

Total Industry-level R&D Expenditures- The difference-in-difference estimates of columns 1 and 2 of Table 3.1 quantifies the amount of change in industry-level R&D expenditures post-*announcement* and *implementation* of TRIPS respectively. Compared to pre-1995, the amount of R&D expenditures in TRIPS-affected industries went up by USD 2.2 million in the five years following the announcement of TRIPS and by almost USD 3 million in the next five years. Moreover, the implementation of TRIPS in 2005 led to a further increase in the R&D expenditures by approximately USD 21 million. This latter effect is over and above the one observed after the announcement of TRIPS.

The rest of the columns of Table 3.1 decomposes this overall impact into three effects- *compositional changes*, *extensive margin* and *intensive margin*.

Compositional Changes- Figure 3.5 shows the effect arising due to compositional changes. The figure plots difference-in-difference estimates comparing the number of firms operating in TRIPS-affected industries with other industries for various years relative to 1995. Prior to 1995, the estimates are zero implying that parallel trends assumption is satisfied. The downward trend observed after 2002 is coming from increased entry of firms in two industries in the control group. Since the results are specific to just these two industries, I refrain from generalizing it to other industries. In fact, after removing these two industries from the sample, the difference-in-difference estimates look flat, indicating that there is no differential impact on the net (after accounting for entry and exit) number of firms operating in TRIPS-affected industries vis-a-vis others. This is exactly what columns 3 and 4 of Table 3.1 too are suggesting. They respectively capture the *announcement* and *implementation* effects of TRIPS due to compositional changes. All the coefficients are estimated to be precise zeros.

Extensive Margin- Figure 3.6 shows difference-in-difference estimates for the probability of investing in R&D. In the beginning of the last decade of 21st century, India was going through major liberalization reforms. It appears that the initial period is picking up the effect of those reforms, though overall all the pre-1995 estimates are still jointly significantly not different from zero. However, one can notice a distinct downward trend post-2005 compared to prior years. Columns 5 and 6 of Table 3.1 confirms this. Between 2000 and 2004, the probability of investing in R&D by firms operating in TRIPS-affected industries relative to pre-1995 period fell by 1.78% points compared to other industries. Since the implementation of TRIPS, such industries witnessed a further decline of 2% points, in addition to the previous 1.78 percentage point fall.

Intensive Margin- Figure 3.7 depicts the difference-in-difference estimates for Intensive Margin. Prior to 1995, there is no difference between the average amount of R&D investments made by firms operating in TRIPS-affected industries vis-a-vis others. With the *announcement* of TRIPS in 1995, the average R&D expenditure increased by almost a quarter million USD in the first five years, as can be seen in Column 7 of Table 3.1. This amount further went up above a million USD after the *implementation* of TRIPS in 2005 (Column 8 in Table 1). This *implementation effect* is over and above the *announcement effect* of the policy in 1995. This could be a result of the firms' increased ability to access capital markets or to license their in-house developed technology to outsiders, to generate novel sources of funding that could further be ploughed back into R&D.

In summary, although TRIPS reduced the cost of entry for innovative firms, I do not find any evidence of differential entry of firms in TIPS-affected industries vis-a-vis others. Otherwise for both Extensive and Intensive margin, I find that the announcement of TRIPS provided a credible enough signal to the entrepreneurs about the change in IPR policy regime and hence, encouraged them to adjust their R&D decisions. Moreover, in addition to this *announcement effect*, the *implementation* of TRIPS in 2005 has an independent effect, which is multiple times higher in magnitude than the initial *announcement effect*.

The increase in industry-level R&D is contributed by rising amount of average R&D expenditures made by firms operating in TRIPS-affected industries. However, in the long run proportionately fewer firms seem to be investing in R&D in TRIPS-affected vis-a-vis other industries. Thus, incremental innovations that require modest sums of money have been crowded-out by more expensive R&D projects. This shift is symptomatic of the substitution away from process patenting and in favor of product patenting, induced by TRIPS.

6.2 Heterogeneity wrt firm size

Tables 3.2 and 3.3 shed light on the types of firms that are engaging in R&D activities post-TRIPS. Table 3.2 focuses on the Extensive margin results for the four quantiles of the firm size. Table 3.3 does the same but for Intensive margin.

On inspection of the average proportion of firms that are choosing to invest in R&D activities in non-TRIPS industries in Table 3.2, one may notice that firms in higher quantiles are more likely to invest in R&D. Moreover, the decline in the probability of a firm in TRIPS-affected industry, to invest in R&D, that we first saw in Table 3.1, seems to be spread across all the four quantiles of firm size distribution. However, the implications of this fall are much worse for the first quantile where the decline of 2% points after the implementation of TRIPS, almost wiped out all the firms that were choosing to engage in R&D activities prior to TRIPS. Similarly, for second quantile, the 3.8% point fall following *announcement* of TRIPS, resulted in wiping out of almost half of all the firms that were previously engaged in R&D activities. Consequently, after the adoption of TRIPS, more firms in higher quantiles are left to invest in R&D than those in lower quantiles.

Thus, the Extensive margin is shifting towards bigger firms and the Intensive margin is showing a similar movement. In fact, the increase in average R&D expenditures, that was first seen in Columns 7 and 8 of Table 3.1, is almost exclusively coming from the top quantile. Therefore, smaller firms have been crowded-out of R&D activities by the adoption of TRIPS while larger firms have increased their average spending on innovation activities.

6.3 Decomposition for Incumbents

Figures 3.8 and 3.9 respectively present the Extensive and Intensive margin effects of TRIPS for incumbent firms. Both these figures look similar to those for the entire sample (Figures 3.6 and 3.7). The only difference is as opposed to the long-term decline in the proportion of firms spending on R&D that we saw for the entire sample (Figure 3.6), the incumbents seem to have stabilized at a level that was prevailing at the beginning of the decade of 1990s. This intuition is confirmed in Columns 1 and 2 of Table 3.4. None of the coefficients are statistically different from zero. On the other hand, the average R&D expenditures (Intensive Margin) by incumbents operating in TRIPS-affected industries has gone up by USD 188 thousand in the first five years after *announcement* of TRIPS and by USD 277 thousand in next five years relative to pre-1995 period and other non-TRIPS affected industries (Column 3 of Table 4). Moreover, after the *implementation* of TRIPS, this difference-in-difference estimate further went upto USD 1.7 millions (Table 3. 4).

This Intensive Margin effect for the incumbent firms is economically significant, in the sense that these firms were operating even before the *adoption* of TRIPS. Hence, their response to TRIPS' announcement and implementation is indicative of the fact that the protection of IPR was a concern among these firms that resulted in constrained optimization, severely depressing the amount of research funds that these firms were initially investing. It also confirms that the effects seen earlier in Table 1 are not a result of increased salience of TRIPS-affected industries following the announcement of TRIPS or a result of ease of entry barriers following implementation of TRIPS.

7 Conclusion and Discussion

This paper attempts to establish four facts. First, there is consistent evidence of a distinct improvement in innovation culture, as measured by increasing number of patent applications and industry- level R&D expenditures in India, following TRIPS. Moreover, Indian patents are increasingly recognized abroad, implying that these patents are not imitations of previously existing technologies. Second, *announcement* of TRIPS provided credible enough signal to firms about the change in IPR policy regime. The *implementation* of TRIPS introduced additional channels for R&D investments to respond and resulted in major thrust to innovation activities over and above the *announcement effect*. Third, most of the industry-level increase in R&D expenditures is coming from Intensive margin, i.e., through increased average R&D investments by firms operating in TRIPS-affected industries vis-a-vis other industries. And this true even about the incumbent firms separately. The latter fact implies that the absence of strong IPRs in pre-

TRIPS era adversely affected firms' R&D investment decisions. Finally, in the long term proportionately fewer firms engage in R&D activities in TRIPS-affected industries relative to others. Together with the fact that the intensive margin effect is coming mostly from larger firms, this shift is symptomatic of crowding-out of incremental innovations by smaller firms, that require modest sums of money, by more expensive R&D projects undertaken by larger firms.

The increase in overall Industry-level R&D expenditures, which is of magnitude USD 2 million and USD 21 million after the *announcement* and *implementation* of TRIPS respectively, is still minuscule compared to the R&D budgets of some of the top pharmaceutical companies in US and Western Europe[45]. However, when adjusted for purchasing power parity, this amount is high enough to achieve significant innovation goals geared towards solving the problems that are specific to developing countries as well as to adapt the technologies developed elsewhere to the local conditions in such countries. Moreover, the evidence presented in this paper should give confidence among other middle-income countries that intend to raise higher private funding for research. The findings of this paper prove that it is not the lack of entrepreneurial spirit that constrains innovation in such countries but the absence of appropriate institutions (like existence of well-functioning court system to interpret and enforce such laws) and complementary resources (like educated workforce to undertake research), that adversely affects the incentives of economic agents and acts as a drag on long-term economic growth.

Finally, the impact of patent laws on who ends up engaging in R&D activities and who opts out is crucial to understand the genesis of the superstar firm phenomenon (Autor et al. (2020)). On one hand, the provision of "secured" IPRs, in the form of product patenting instead of a much

[45] For example, Pfizer's R&D grant for development of Covid vaccine was USD 1.95 billion and that of Moderna is more than USD 2 billion.

weaker form of process patenting, encouraged **larger** firms to commit higher resources towards R&D. At the same time, it came at the cost of dropping out of **smaller** players from the R&D race altogether. In part, this substitution of more expensive R&D projects by bigger firms for incremental innovations by smaller firms, requiring modest sums of money, is a result of shift away from process patenting and in favor of product patenting, as induced by TRIPS. Countries whose priorities are to raise higher research funding may take these distributional effects as second order concern. However, when left unchecked they may have serious implications for the industrial organization structure since innovations fuel productivity growth, which in turn, could further strengthen the dominant positions of these larger firms.

FIGURES

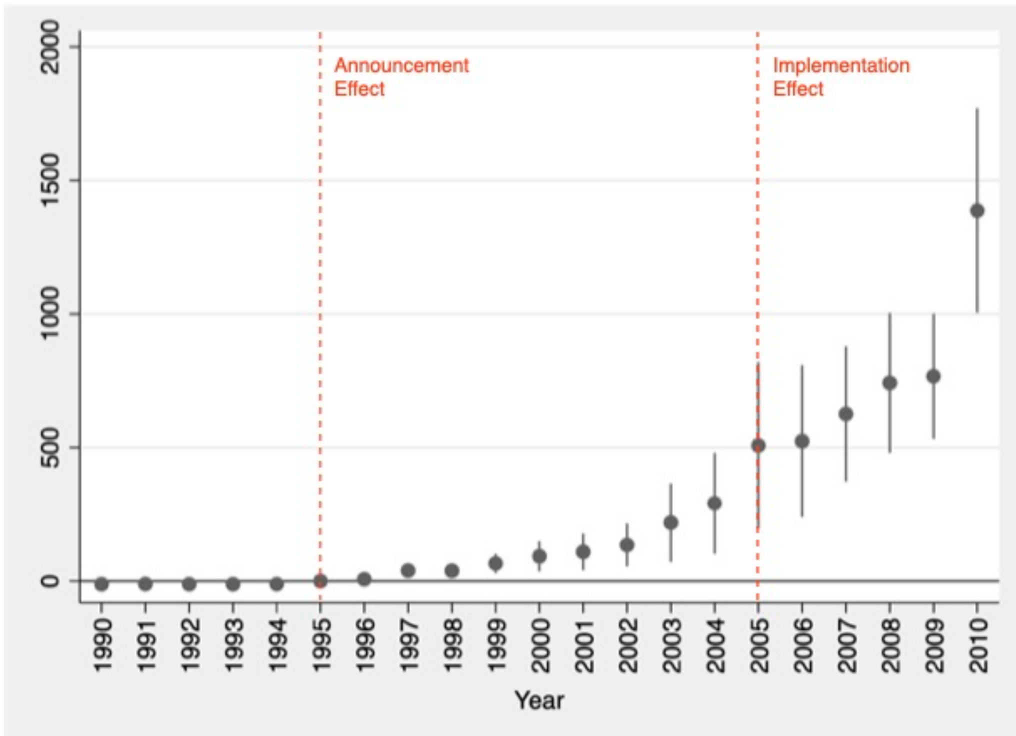

Figure 3.1: Count of Patent Applications in TRIPS v/s other technology categories

The figure plots 95% confidence intervals of difference-in-difference estimates that compare the count of patent applications filed by residents in India in TRIPS-affected category vis-a-vis other categories, relative to the year 1995. The data for this figure is scrapped from the website of Indian Intellectual Office.

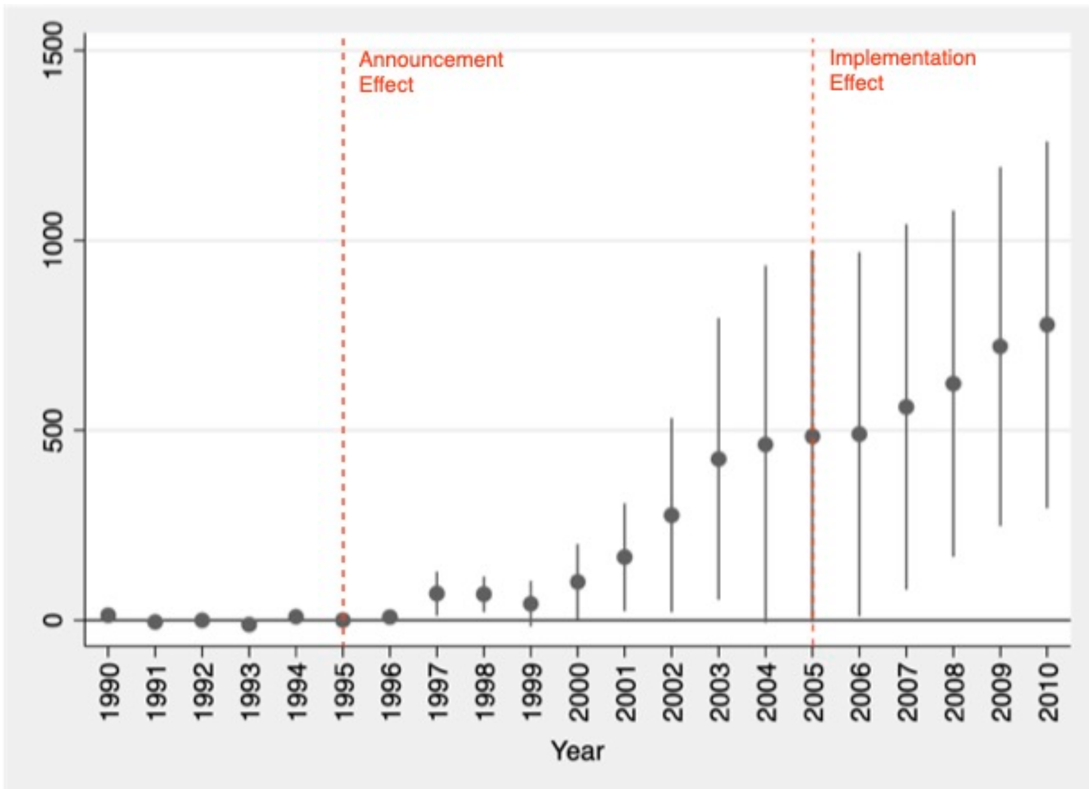

Figure 3.2: Count of Patents granted to Indian residents abroad in TRIPS v/s other technology categories

The figure plots 95% confidence intervals of difference-in-difference estimates that compare the no. of patents granted to Indian applicants abroad in TRIPS-affected category vis-a-vis other categories, relative to the year 1995. The data for the figure comes from WIPO.

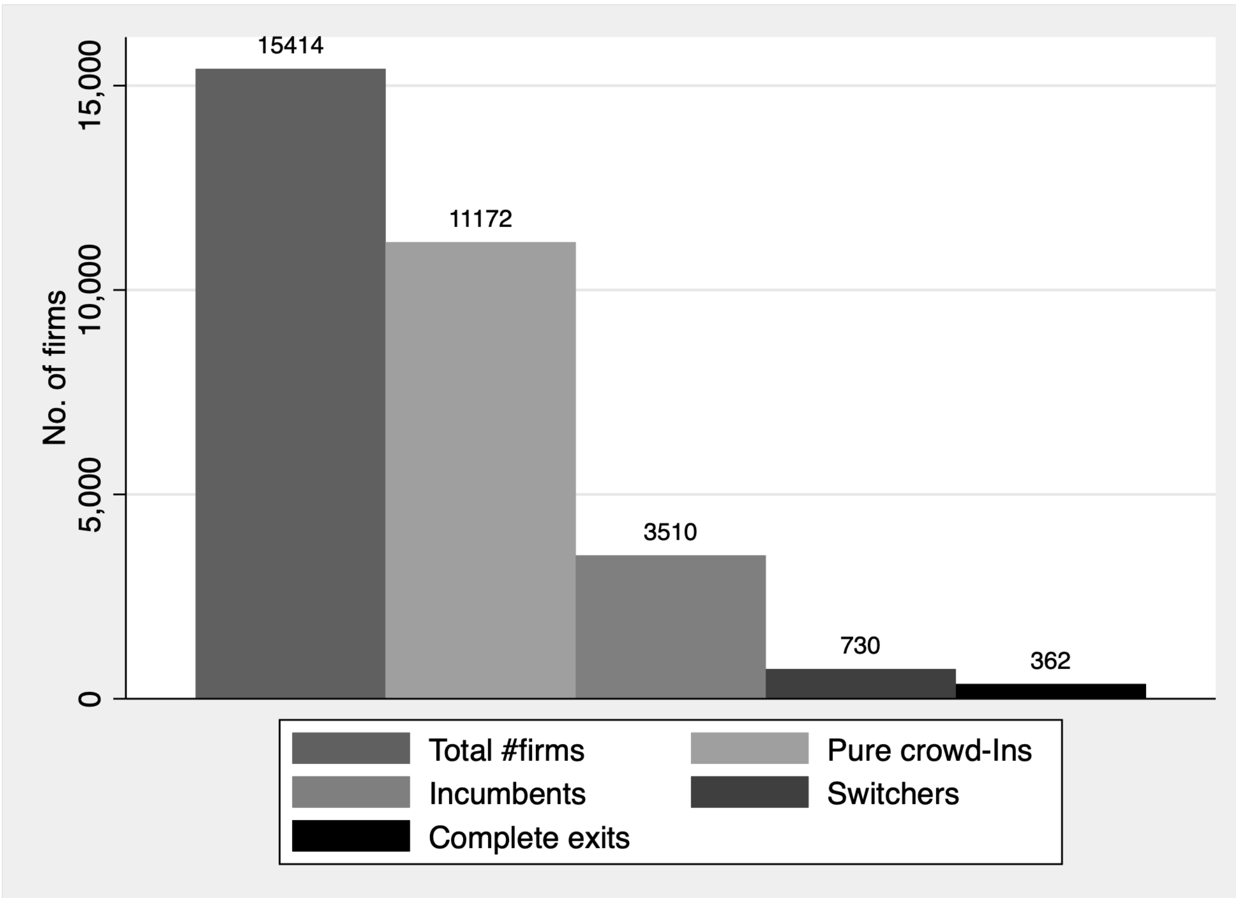

Figure 3.3: Different firms in the sample

The figure plots the composition of various types of firms that constitute the Prowess dataset for the study period.

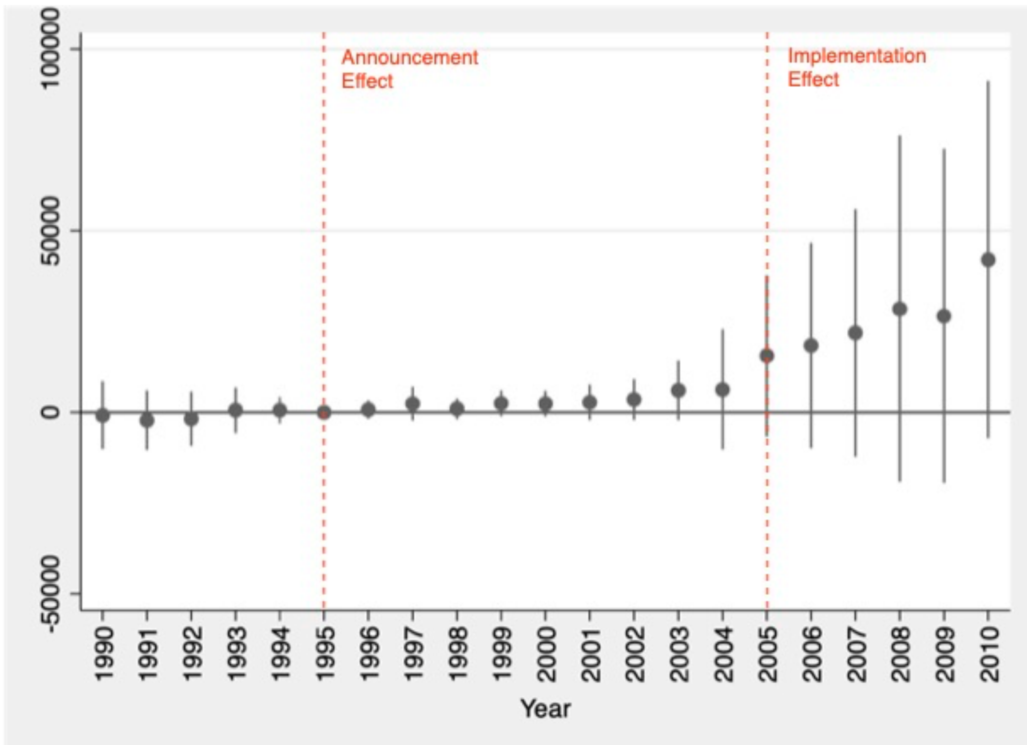

Figure 3.4: Industry-level R&D Expenditures of TRIPS v/s non-TRIPS industries

The figure plots 95% confidence intervals of difference-in-difference estimates that compare the industry-level R&D expenditures in TRIPS-affected industries vis-a-vis other industries, relative to the year 1995. The data for this figure comes from Prowess.

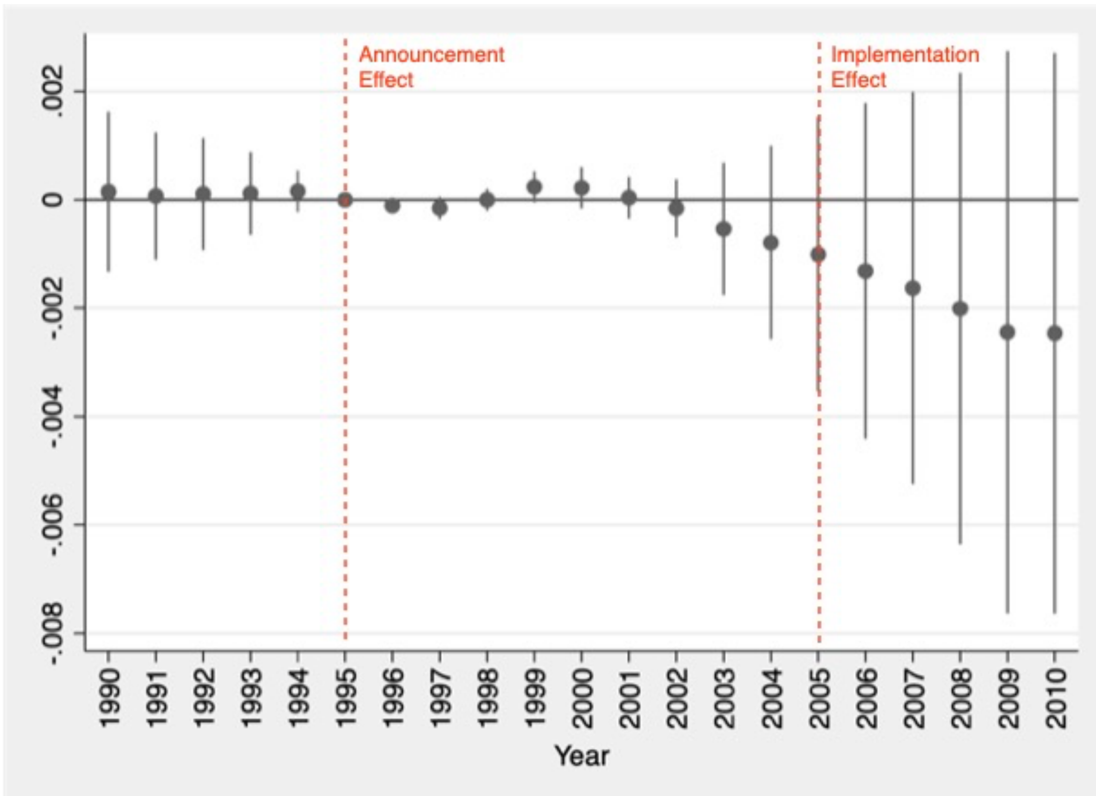

Figure 3.5: Compositional Change

The figure plots 95% confidence intervals of difference-in-difference estimates that compare the number of firms in TRIPS- affected industries vis-a-vis other industries, relative to the year 1995. The data for this figure comes from Prowess.

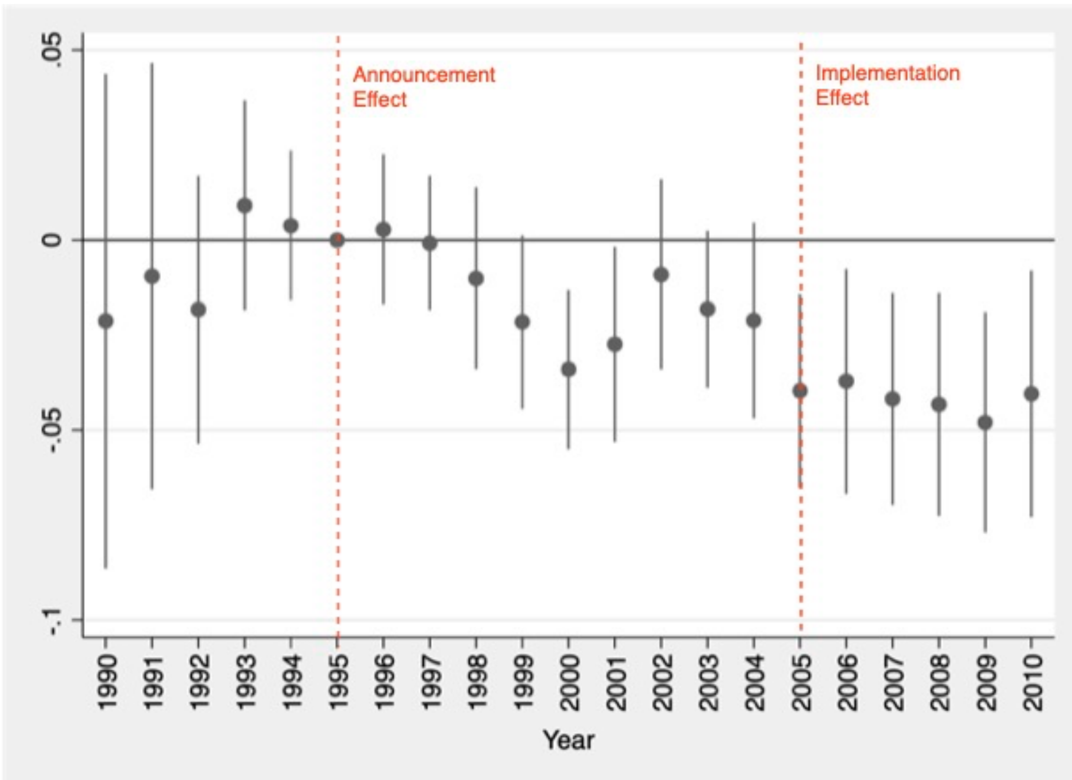

Figure 3.6: Extensive Margin

The figure plots 95% confidence intervals of difference-in-difference estimates that compare the proportion of firms investing in R&D in TRIPS-affected industries vis-a-vis other industries, relative to the year 1995. The data for this figure comes from Prowess.

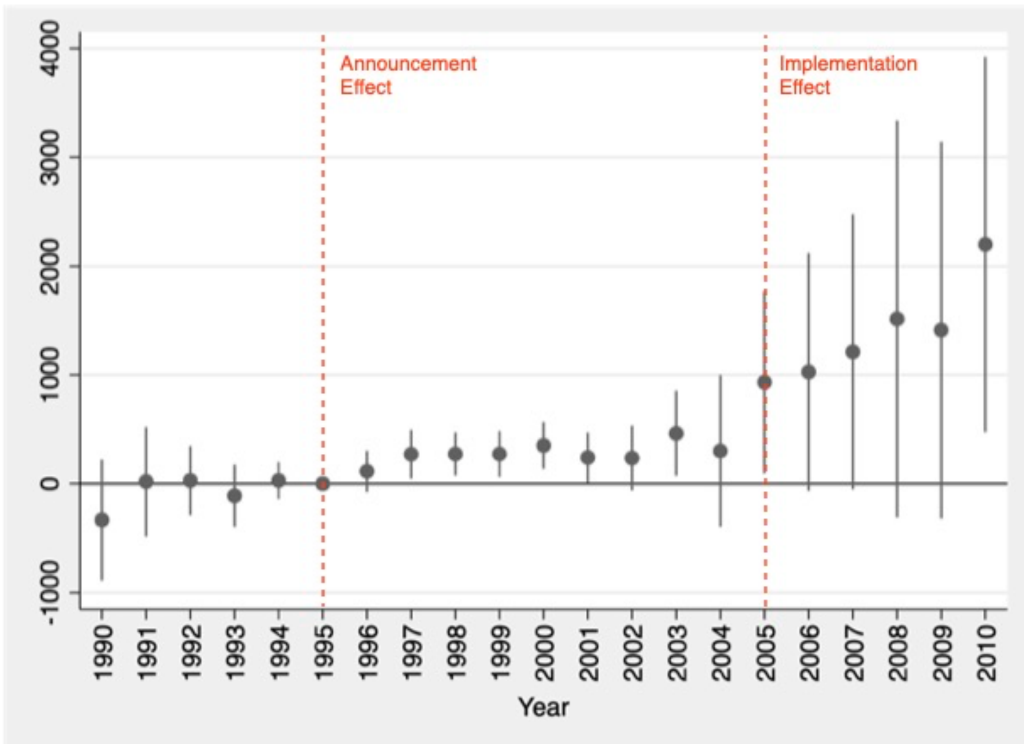

Figure 3.7: Intensive Margin

The figure plots 95% confidence intervals of difference-in-difference estimates that compare the average R&D expenditures by firms operating in TRIPS-affected industries vis-a-vis those operating other industries, relative to the year 1995. The data for this figure comes from Prowess.

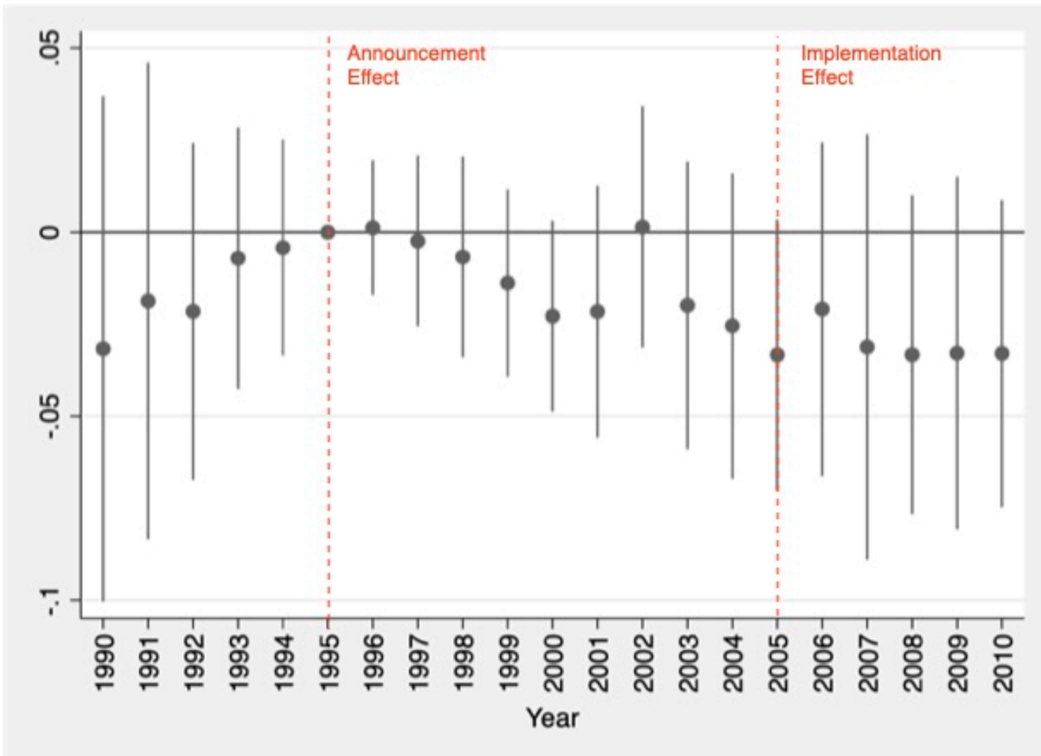

Figure 3.8: Incumbent Firms: Extensive Margin

The figure plots 95% confidence intervals of difference-in-difference estimates that compare the proportion of firms investing in R&D in TRIPS-affected industries vis-a-vis other industries, relative to the year 1995 for the sample restricted to incumbent firms. The data for this figure comes from Prowess.

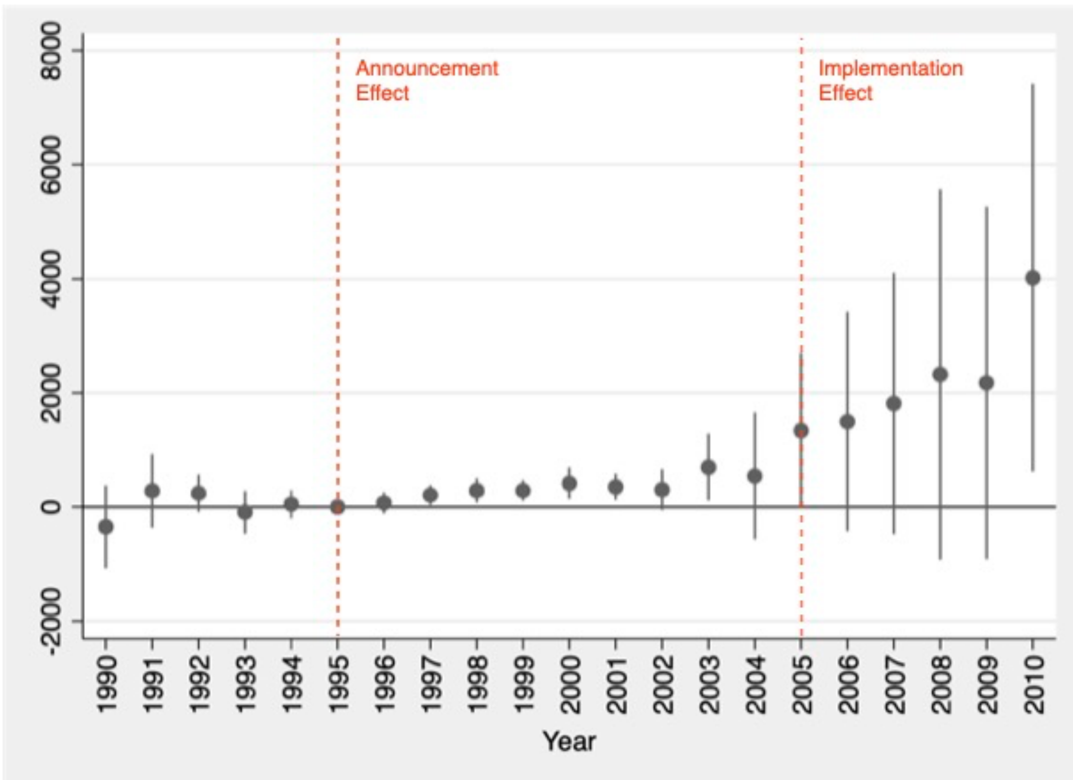

Figure 3.9: Incumbent Firms: Intensive Margin

The figure plots 95% confidence intervals of difference-in-difference estimates that compare the average R&D expenditures by firms operating in TRIPS-affected industries vis-a-vis those operating other industries, relative to the year

TABLES

Table 3.1: Decomposition of Industry-level R&D Expenditures

	Industry-level R&D (USD)		Compositional Changes		Extensive Margin		Intensive Margin	
	$\sum_{f:f\in i} R\&D_f$		$\Pr(f \in i)$		$\Pr(R\&D_f > 0 \mid f \in i)$		Firm-level R&D (USD) $\mathbb{E}_{f:f\in i}[R\&D_f \mid R\&D_f > 0]$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TRIPS* Post 1995	2.187e+06		-0.000123		-0.00294		228,837**	
	(2.716e+06)		(0.000471)		(0.0159)		(111,458)	
TRIPS* Post 2000	2.982e+06		-0.000240		-0.0178**		148,266	
	(2.424e+06)		(0.000385)		(0.00773)		(108,390)	
TRIPS* Post 2005		2.106e+07		-0.00156		-0.0194**		1.039e+06*
		(1.342e+07)		(0.00162)		(0.00900)		(470,958)
Observations	861	704	19,652,850	14,412,090	61,108	77,016	12,649	12,045
R- squared	0.568	0.698	0.008	0.013	0.093	0.108	0.105	0.165
Control Mean	3,796,271.61	5,673,508.26	0.002	0.004	0.14	0.14	332,808.36	365,311.31
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of clusters	70	72	85	85	83	85	70	72

The table shows estimates for *announcement effects* (Equation 9) and *implementation effects* (Equation 10) of TRIPS. Columns (1) and (2) show the effects on Industry-level R&D expenditures. Columns (3) and (4) show the effects on Compositional changes. Columns (1)-(4) are estimated on the entire sample. Columns (5) and (6) show the effects on Extensive Margin for the sample of firms that are operating in the respective industries. Columns (7) and (8) show the effects on Intensive Margin for the firms that choose to invest on R&D. The equations include industry and year FEs. SEs are clustered at three-digit industry level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.2: Heterogeneity wrt firm size: Extensive Margin

	Pr(R&D > 0)							
	Q1		Q2		Q3		Q4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TRIPS* Post 1995	-0.00218		-0.0376**		-0.00683		0.0262	
	(0.0147)		(0.0186)		(0.0229)		(0.0325)	
TRIPS* Post 2000	-0.0120		0.00508		-0.0215		-0.0431*	
	(0.00841)		(0.0143)		(0.0160)		(0.0230)	
TRIPS* Post 2005		-0.0204**		-0.0225		-0.0174		-0.0159
		(0.00835)		(0.0248)		(0.0151)		(0.0170)
Observations	16,374	20,273	14,553	18,542	14,785	18,861	15,396	19,340
R- squared	0.073	0.062	0.095	0.094	0.156	0.166	0.194	0.252
Control Mean	0.052	0.023	0.076	0.066	0.119	0.144	0.328	0.349
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of clusters	83	85	73	76	70	72	77	79

The table shows estimates for *announcement effects* (Equation 9) and *implementation effects* (Equation 10) of TRIPS on the Extensive Margin for the sample of firms that continue to operate during the period of analysis. For Columns (1) and (2), the sample consists of firms that lie in first quantile of the asset-size distribution. For Columns (3) and (4), the sample consists of firms that lie in second quantile of the asset-size distribution. For Columns (5) and (6), the sample consists of firms that lie in third quantile of the asset-size distribution. For Columns (7) and (8), the sample consists of firms that lie in fourth quantile of the asset-size distribution. The equations include industry and year FEs. SEs are clustered at three-digit industry level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Heterogeneity wrt firm size: Intensive Margin

	Firm-level R&D (in USD)							
	Q1		Q2		Q3		Q4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TRIPS* Post 1995	61,098		22,537		-7,283		308,704**	
	(50,486)		(36,780)		(25,905)		(123,578)	
TRIPS* Post 2000	123,890		-11,628		11,851		312,177	
	(109,497)		(23,113)		(15,407)		(226,763)	
TRIPS* Post 2005		227,876		116,897		71,090		1.841e+06*
		(219,779)		(127,742)		(77,717)		(1.008e+06)
Observations	1,059	675	1,705	1,262	3,413	3,276	6,472	6,832
R- squared	0.265	0.185	0.480	0.307	0.375	0.453	0.165	0.291
Control Mean	103,143.93	63,555.07	31,511.41	26,395.43	60,753.90	56,708.99	529,739.77	567,560.85
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of clusters	65	63	61	60	62	64	67	70

The table shows estimates for *announcement effects* (Equation 9) and *implementation effects* (Equation 10) of TRIPS on the Intensive Margin for the firms that choose to invest in R&D. For Columns (1) and (2), the sample consists of firms that lie in first quantile of the asset-size distribution. For Columns (3) and (4), the sample consists of firms that lie in second quantile of the asset-size distribution. For Columns (5) and (6), the sample consists of firms that lie in third quantile of the asset-size distribution. For Columns (7) and (8), the sample consists of firms that lie in fourth quantile of the asset-size distribution. The equations include industry and year FEs. SEs are clustered at three-digit industry level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Extensive and Intensive Margin Effects for Incumbent Firms

	Extensive Margin		Intensive Margin	
	Pr(R&D > 0)		Firm-level R&D (in USD)	
	(1)	(2)	(3)	(4)
TRIPS* Post 1995	0.00948		188,109*	
	(0.0192)		(106,942)	
TRIPS* Post 2000	-0.0133		277,115*	
	(0.0107)		(166,514)	
TRIPS* Post 2005		-0.0137		1.700e+06*
		(0.0105)		(894,476)
Observations	35,829	23,678	9,383	6,857
R- squared	0.114	0.129	0.128	0.213
Control Mean	0.14	0.25	334,359.30	436,645.72
Year FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
No. of clusters	74	73	65	63

The table shows estimates for *announcement effects* (Equation 9) and *implementation effects* (Equation 10) of TRIPS for the sample restricted to the incumbent firms. Columns (1) and (2) show the effects on Extensive Margin. Columns (3) and (4) show the effects on Intensive Margin for the firms that choose to invest in R&D. The equations include industry and year FEs. SEs are clustered at three-digit industry level. *** p<0.01, ** p<0.05, * p<0.1.

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