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Abstract

This dissertation comprises three essays on policy program impact evaluation. The first essay (chapter 1) looks at the Employment Guarantee Act policy and its effect on the child sex ratio. The second essay (Chapter 2) investigates the effectiveness Productive Safety Net Program to buffer the negative impact of various shocks experienced by households on child's outcomes. The last essay (Chapter 3) investigates in No Detention policy of the Right to Education Act and its effect on test scores for students.

Limited economic opportunity for women reduces their household bargaining power and the economic value of daughters, amplifying son preference. Chapter 1, co-authored with Dr. Daniel Hicks, studies India's National Rural Employment Guarantee Scheme salary and mandated at least one-third of workers be women. In a setting where the gender gap in employment and wages are sizeable, NREGS represented both an income shock and a large relative improvement in the labor market for women. We use the staggered roll-out of NREGS to show that districts which implemented the program earlier experienced an improvement in child sex ratios in favor of girls. Although program implementation was non-random, we find impacts exist only in rural areas, not in the urban counterparts of the same district, where NREGS did not operate. Furthermore, effects are larger in middle and upper income districts and districts with the most skewed initial sex ratios, results which are inconsistent with an alternative selection story. Finally, the effects appear only for rural youth sex ratios, not for adult sex ratios, suggesting endogenous migration is not driving the results.

Many developing countries have opted for workfare programs to reduce the vulnerability of the poor and to lessen economic inequality. In chapter 2, I investigate the negative impact of household shocks on children's outcomes and assesses the effectiveness of Ethiopia's Productive

Safety Nets Program (PSNP) in providing a mechanism to cope with these shocks using a fixed-effect model, and a unique counterfactual group approach. Understanding the extent to which the PSNP offsets the negative impact of shocks on children's growth and welfare is important in accurately assessing the value of the program. In this paper, I find that while certain shocks reduce child school attendance, the implementation of PSNP appears to offset this effect. I find no robust evidence that it plays a similar role for child height or household labor outcomes.

The No detention policy act in 2010 guaranteed promotion to the next grade, and no student can be failed or expelled till grade 8. This paper investigates if education quality affected around the time of this law using test scores from Young Lives survey data. The test scores declined for students belonging to both higher and lower levels in test scores distribution.

Chapter 1
The Right to Work and to Live: The Implications of India's NREGS Program for Missing Women

1. Introduction

In many parts of the world, preferences for male children have manifested in skewed sex ratios, both at birth and for the population at large. The issue was brought to popular attention by Amartya Sen who calculated that millions of women were missing in the 1990s, and the subject has garnered heavy research attention since (Sen, 1990)[1]. In India, this problem has become particularly severe, worsening every decade since the 1960s, with the child sex ratio (i.e. among 0-6 year olds) falling as low as 91.4 females per 100 males as of 2011. These trends are depicted in Figure 1. A number of mechanisms have been cited as responsible for the growth in the demographic deficit of females emerging around the world, including neglect of females, increasing access to sex selective abortion, and infanticide (Jayachandran, 2017).

At the root of the issue is son preference, driven by a myriad of social, cultural, historical, and economic forces[2]. Among the economic determinants studied, the relative strength of labor market opportunities for women has been shown to have a large impact on sex ratios, likely by altering the bargaining power of working women within a family, as well as by increasing the expected future economic value of daughters to the household (Qian, 2008). Women's education, employment, and empowerment, as well as trends in urbanization have also been shown to help improve the sex ratio by increasing survival rates for girls (Gupta et. al., 2003).

[1] Das Gupta *et. al.* (2003) note that elevated rates of child mortality among women in China and India, including the practice of female infanticide, have been recorded for over a century.

[2] Sociocultural and historical factors that have been cited include patrilineal kinship and political structures, the practice of patrilocal exogamy, inheritance practices favoring males, the use of dowries, religious and cultural beliefs regarding the proper behavior of females and their role in society (see Jayachandran (2015) and Mitra (2014) for reviews).

The importance of labor market opportunities in influencing gender bias suggests an important avenue for future policy interventions seeking to reduce gender-based discrimination and help nudge the sex ratio back to biological norms. This paper examines the impact of India's National Rural Employment Guarantee Scheme (NREGS), one of the most ambitious workforce interventions ever implemented – along this gendered dimension – studying its impact on the sex ratio. Theoretically the impact of the program is ambiguous as the large income shock may relieve income constraints on sex-selective abortions and thus worsen the sex ratio, while at the same time it has the potential to improve income equality and close the gender wage gap, which should both raise the bargaining power of women in the household and the incentive to have daughters and to invest more heavily in them.

India's National Rural Employment Guarantee Act was proposed in 2005 and implemented over the course of three phases in the years followed. The program, NREGS, is now the largest public works program in the world, guaranteeing at least 100 days of paid work each year to any rural resident, and paying a fixed minimum wage to all workers, irrespective of gender. In the year 2010 alone, over 50 million households in India had an individual employed through NREGS, around one in five households.[3] As wages and labor force opportunities are worse for women than for men across India, the introduction of NREGS has helped to close the gender gap in the labor market. Azam (2011) estimates that after it was implemented, observed real earnings rose approximately 8% for women in NREGS areas relative to a 1% increase for men.

Our analysis faces several general threats to identification. One of which is that NREGS itself may have induced migration. Fortunately, this should manifest only in changes in the adult sex ratio, not in the sex ratio at birth. Second, a concern is that the NREGS implementation was targeted (focusing on poorer districts for the initial program expansions). We address this in several ways. To

[3] Using official data, Jha and Gahia (2013) show that average days of employment per household and percentage of households completing 100 days of work under NREGA has since deteriorated over time.

examine how the narrowing of the gender employment gap created by NREGS has influenced sex ratios at birth, we exploit the staggered roll out of the program and then contrast rural and urban areas in an exercise akin to that of a differences-in-differences approach (before vs. after implementation, comparing rural vs. urban areas) to help isolate the impact of the public works program from many other potentially confounding forces. We show that rural areas of districts which implemented the program earlier experienced an increase in the number of females born relative to the number of males. The changes we observe in youth sex ratios are robust to controlling for a large range of observable district characteristics.

While the effects we observe could in theory be attributable to differential trends in the sex ratio between early and late adopting districts, we present multiple pieces of evidence which support the alternative that NREGS had a direct causal effect on gender equality and on the sex ratio. First, we find that no similar pattern was evident in the urban counterparts of these very same districts when comparing before and after the implementation of NREGS. Employment through NREGS was only offered in rural areas, suggesting the programs effect is more likely to have influenced outcomes than differential trends in the sex ratio across regions of India. Furthermore, we aggregated job card records and show that the more intensively that NREGS was implemented the larger the impact of the effects we observe. Finally, while these effects are evident for child (age 0-6) sex ratios, no similar patterns are evident in either the rural or the urban areas when we contrast the adult sex ratios of early and late adopters, consistent with the public works program weakening son preference.

The results of our heterogeneity analysis are also not consistent with differential trends in early and late adopters driving the results. The program was explicitly targeted to districts which were poorer with the stated intention of improving gender parity, but this assignment was not mechanical. When we split the overall sample into income terciles, we find that the more job cards that were issued, the larger the improvement in the sex ratio, but only in higher income rural areas. Similarly, when we split the sample on the basis of initial sex ratios, we find an association between early NREGS exposure and improvements in the sex ratio in favor of women among districts both at the top and the bottom end of the distribution, with the largest impacts among the most skewed districts.[4]

Within economics, studies on the direct and indirect economic impacts of NREGS across a range of other outcomes exist – including studies on consumption, income, asset accumulation, and other socio-economic effects such as incentives for human capital investment. Some of these employ propensity score matching. For example, using cross-sectional data, Ravi and Englar (2009) find that take-up of the workfare program is associated with a significant increase in both food and non-food consumption expenditure. Similarly, Liu and Deininger (2010) use detailed panel data in Andhra Pradesh and find significant positive impacts on measures of nutritional intake and on consumption expenditure.[5]

Shah and Steinberg (2015) exploit within district variation to compare educational outcomes across different age cohorts before and after NREGS was implemented. They show that access to the works program decreased school enrollment and math scores slightly for 13 to 16 year old children (both for male and female children) suggesting that the labor market impacts of NREGA may have induced modestly lower human capital investments for a segment of the population. These findings are echoed by Li and Sehkri (2013) who show that enrollment decrease in NREGS districts.[6]

In qualitative analysis, Nayal (2009) finds that work fare recipients benefited through better access to employment, bargaining power, and safety in the work place. Nayal also finds a wide range of variation in the extent of NREGS participation among women across regions, with some parts of

[4] Districts which received NREGS in 2008, the last wave of implementation, were on average wealthier and had more skewed sex ratios.
[5] More recently, Banerjee (2015), finds a positive but insignificant impact on food consumption and female employment due to another employment guarantee policy Sawarnajayanti Gram Swarojgar Yojana (SGSY), but no impact from NREGA.
[6] The findings of Afridi et al. (2016) conflict with these results, suggesting an improvement in children's educational outcomes for participating mothers, although some of the difference may stem from their focus on the state of Andra Pradesh.

India seeing more rapid adoption of the program. Sudarshan and Bhattacharya (2010) argue that some of the variation in female uptake of NREGS may be driven by differences in the minimum wage offered across areas as well as to differences in the proximity of rural areas to their urban counterparts.

Imbert and Papp (2015) demonstrate an increase in rural wages following the implementation of NREGS. Perhaps most related to our own work, Berg et al. (2018) and Azam (2011) examine gendered differences in economic impacts in addition to showing overall wage and earnings effects. Berg et al. (2018) demonstrate that the gender wage gap is not impacted by the presence of NREGS. At the same time, Azam (2011) shows that women's earnings in NREGS districts increased 8% when compared to non-NREGS districts. In contrast, while those for men increased by only 1% on average after the introduction of the program.[7] Berg et al. (2018) reconcile these differences by noting that program disproportionately employs women.[8] Using child and household level panel data, Afridi et al. (2016) find an increase in female labor force participation after the implementation of NREGS and argue that this increase in earnings improves the bargaining power of mothers as evidenced by improvements in their children's educational outcomes.

The remainder of this paper is structured as follows. Section 2 provides a history of the structure and implementation of the National Rural Employment Guarantee Scheme. Section 3 provides an overview of the data. Section 4 analyzes the impact of the implementation of NREGS on gender ratios in India. Section 5 concludes.

[7] Using the implementation capture only the short run effects, as mentioned by Shankar (2008). Additional studies are needed to assess the longer-term general equilibrium impacts of the program.

[8] Berg et al. (2018) suggest that slack in the labor market for female labor in rural India may have prevented upward pressure on average wages, a theoretical argument along the lines of the original work of Lewis (1954).

2. Background: The National Rural Employment Guarantee Act

The National Rural Employment Guarantee Act (NREGA) was passed into law in India in 2005, guaranteeing the right to work for all rural individuals in India. NREGS offers adults at least 100 days of wage based unskilled manual labor employment over the course of the year. Three quarters of the material and labor costs of NREGS is born by the federal government, with the remaining quarter funded through state government budget allocation. The most common types of work under NREGA are jobs in road construction, irrigation, and water conservation.

Stated goals of the program include poverty reduction and the empowerment of trivialized communities, Schedule Caste (SC), Schedule Tribe (ST) and especially women.[9] (Ministry of Rural Development, 2013). In many areas, the implementation of NREGS created opportunities for female labor where few if any existed before, particularly for non-agricultural work. Social pressure within caste, tribe, or religious group is frequently cited as a deterrent to female participation in the workforce. In this regard, the status afforded to employment within government is especially beneficial to helping women overcome the hurdle imposed by social norms in general – work for the government by women is less stigmatized, offered during more acceptable hours, and exposes women to lower levels of harassment than they may otherwise face in the private sector (Khera and Nayak, 2009). Under NREGS equal wages are offered to men and women (Gulzar and Fayaz 2016), and although these wages vary by state, they average approximately $2 a day for the country.

Consistent with the stated goals of the program to improve social welfare, the implementation of NREGS was staggered in three phases, targeting less developed regions first. In first phase of 2006, the program was implemented initially in 200 districts classified as backward districts. In 2007, 130 additional districts were phased in to NREGA. Finally, in 2008 all of the remaining districts were

[9] The NREGA act stipulates that at least one third of the receivers of the program be women, with the intention of reducing gender inequality.

covered. Actual assignment is not clearly predicted by any one poverty measure, as other nuances such as the requirement that at least one district in each state receive NREGS early were in place.

Because not all districts were captured in the Census, and because some districts changed over time, we focus on a set of 559 consistent districts available in the 2001 and 2011 censuses as detailed in Table 1. Consistent with the previous literature, we refer to these districts as "wave 1," "wave 2," and "wave 3" respectively and for the analysis we contrast those in waves 1 and 2 with the late implementers in wave 3 as has become common practice in the literature examining NREGS. This gives us by wave, 181, 125, and 253 districts respectively.[10] Figure 2 presents a Choropleth map of India which illuminates the wide degree of geographic variation in the timing of the roll out of NREGS.

In order to participate in the program, individuals have to apply for a job card to a Gram Panchayat (local government organizations at the village level). If an individual can prove local residence within the area of Gram Panchayat, a job card is issued to record employment requested, employment provided, number of days worked, and payments issued. Job card are issued at the household level, and within 15 days of receipt, all adult members of the household are entitled to employment.[11] Local village meetings (Gram Sabha) are held and post job openings for public work. NREGA blocks middle men such as contractor or agent from becoming involved in the work assignments allotted through NREGA.

3. District Data

Our primary regional and demographic data comes from India's National Census, including district level aggregates for 2001 and 2011. The 2001 and 2011 district level estimates are further

[10] Appendix Table 1 lists the complete set of districts in each of the three phases of implementation. Shah and Steinberg (2015) provide a detailed description of the assignment process for districts across waves, arguing that actual allocation rule was generally based on income, but imperfectly so, and as such was subject to political economy motivations and policy implementation nuances.

[11] If the Gram Panchayat could not allocate work within 15 days, job card holders are entitled to receive unemployment compensation

disaggregated between rural and urban portions of each district -- a census distinction which classifies individuals living in villages as those who inhabit rural areas and those living in towns and cities as urban. The timing and methodology of the census are fortuitous for our analysis in the sense that the 2011 census is ideally timed to observe changes in the child sex ratio (those aged 0-6), a measure that would driven by births since the start of NREGA which was implemented in 2006, 2007, and 2008.

Means and standard deviations for all variables discussed in this section are presented in Table 2. As can be seen from columns (1) and (2) of the table, the overall sex ratio and the sex ratio at birth are both highly skewed in 2001 (Panel A) and 2011 (Panel B).12 Reading across the table, it is apparent that the parts of the country in which NREGS was first implemented are significantly different from the parts of the country where it was not implemented until 2008 (when it was run nationwide) along several observable dimensions. More precisely, as of 2011, early implementers had on average, a less skewed sex ratio, were more agrarian, and were more illiterate than districts which implemented the workforce program later.

Table 3 presents information on district level amenities including the number of schools of various levels, the number of health centers and hospitals, as well as the extent of roads in the area. With the exception of health centers, early NREGA areas tend to be more densely populated and to have fewer such amenities. We include these factors as controls in the analysis. At the district level we match these census measures with information on the timing and degree of implementation of NREGA provided by the Ministry of Rural Development as described in Section 2. The Appendix describes each of these data sources in more detail.

[12]Appendix Table 2 provides a similar set of summary statistics for rural and urban areas.

4. Analysis

4.1 Empirical Strategy and Baseline Results

Our empirical strategy exploits variation in the timing of NREGS implementation and thus in the length of exposure to the NREGS program to estimate the impact of the program on changes in fertility and sex ratios within districts over time. NREGS provides an equal floor to male and female earnings potential, so additional exposure to better labor market opportunities for women could improve their bargaining power and the sex ratio.

Formally our primary regression specifications take the general following form:

$$Sex\ Ratio_{i,2011} = \alpha + \beta_1 (NREGA\_Early)_i + \beta_2 (SR)_{i,2001} + X_{i,2001}\mathbf{\Gamma} + \varepsilon_{it} \qquad (1)$$

where identification comes from variation in the sex ratio over time within a district as a variation in exposure to the NREGS workfare program. Our coefficient of interest β1 captures the impact of earlier exposure to NREGS on the 2011 sex ratio conditional on the initial 2001 sex ratio. χ_i are a vector of district level controls discussed in Section 3, drawn primarily from the 2001 census. In later specifications, we consider an alternative measure of the intensity of NREGS implementation, the log of the number of job cards issued on a per capita basis across a whole district.

The results of estimating equation (1) are presented in Table 4 and estimated for the child sex ratio using census estimates for the rural areas only. We focus on just the rural areas to begin with as this is where we should expect to observe an impact if there is one operating through women's economic empowerment. Column (1) estimates the naïve association between early implementation of NREGA and the 2011 sex ratio. Early adopters have a 3.5% point greater amount of female births relative to male births in comparison to rural areas that adopted the program later. As can be seen from the mean of the dependent variable, both sets of districts still heavily favor male births. Turning to column (2), we can immediately see that the naïve association is largely just attributable to differences in the initial child sex ratio, and after controlling for the 2001 district sex ratio in these rural areas, the association becomes positive (and imprecisely different from zero).

Columns (3) and (4) include census controls for local amenities and demographics, factors which may influence son preference and the evolution of this preference over time, but which are also related to the timing of implementation (as discussed in Section 3). These controls do little to influence the estimated impact of NREGA on the sex ratio on their own. Column (5) which includes the full set of controls, produces a negative and significant association between having an addition 1 to 2 years of NREGA exposure on the child sex ratio as of the 2011 census. This is our preferred specification, and we include this full set of controls in future tables.

To interpret the magnitude of this effect, early adopters, conditional on their initial sex ratio and their observable census characteristics, saw improvements in their sex ratio relative to later adopters on the order of 0.543% points. A caveat of this coefficient, and of the estimates produced in our study across the board, is that we are estimating the impact of additional years of NREGS, not the overall program effect, which may be different and which by nature is more likely to have had even larger general equilibrium effects. Nevertheless, these results are informative – particularly when the impact of NREGS on the sex ratio is theoretically ambiguous.

4.2 Counterfactuals: Urban Areas and Adult Sex Ratios

The results of the previous section suggest that rural areas of districts exposed to 1 to 2 additional years of NREGS between 2001 and 2011 saw an improvement in their sex ratio in favor of girls by roughly a half a percentage point in contrast to those who did not get NREGS until 2008. A remaining concern is that this association could be the result of differential trends in the sex ratio between the rural parts of lower income districts which received NREGS earlier and the rural parts of those which received it later. To help alleviate this endogeneity concern, in Panel A of Table 5, we exploit the richness of the census data in more detail to produce comparable results where we should not expect to find an effect – essentially a set of placebos. Each coefficient presented represents an individual regression with the full control specification (6) of Table 4 – this time presenting the estimates for the total district, and then disaggregating the analysis to rural and urban populations within the district.

The first three columns present results for the child sex ratios (where we would expect to find an effect if there is one), while the last three columns present results for the overall population sex ratios (age 6+, where we should not expect to see an impact unless there was a trend difference). Thus, theoretically, if NREGS improved the sex ratio for women, the most likely place to observe an impact should be in the youth sex ratio of rural districts (or in the total district values, but driven by underlying rural effects). Impacts in urban regressions could reflect migration patterns in response to the program for instance (though these should be more likely to appear for the overall sex ratio, not the youth sex ratio).

Estimates in column (2) are negative and significant at the 10% level, suggesting that the child sex ratio, our closest proxy to sex ratios at birth, in districts with earlier access to NREGS moved in favor of women by approximately one half of a percentage point on average. In contrast, the effect in urban areas, and the effect for populations 6 and older, even in the rural areas, are either positive or close to zero, and are always insignificant. These results support the hypothesis that NREGS may have causally improved the at birth sex ratio in regions which had better access to it.

4.3 The Intensity of NREGS Implementation and Take-Up

We now undertake another falsifiable exercise to examine the impact of NREGS on the sex ratio in rural districts, this time exploiting the intensity of the treatment. If NREGS impacts the sex ratio through its labor market impacts and not simply by being correlated with district characteristics, then the effect should certainly be larger in places where the implementation was more widespread (i.e. where take-up of NREGS was greater). In order to examine this, we obtained estimates of the total number of job cards issued each year in each district using public records from the Ministry of Rural Development. We then construct a measure that is the log of the average number of job cards issued, per year, per capita by district.[13]

Table 6 replicates the exercise of Equation (1) where we now estimate the impact of job card intensity instead of NREGA timing. As can be seen from the table, districts which have more job cards issued have significantly more rapid improvements in the sex ratio in their rural areas, but only for the youth sex ratio. The overall magnitude, while significant, is only modest in size, with a doubling in job cards being associated with a half a percentage point improvement in the sex ratio in favor of women.

4.4. Heterogeneity Analysis: Income and Sex Ratios

NREGS had a social welfare purpose and was targeted on the poorest districts first. At the same time, income plays a number of roles in influencing son preference. First, income, both absolute and relative male/female earnings potential, is likely to play a key role in influencing son preference, both because it influences the income constraint on sex selective abortions to choose gender and because it alters bargaining power within the household as well as the opportunity cost of having girls relative to boys. NREGS provides rural individuals with work and income, which suggests that income is the channel through which exposure to the work fare program influences the sex ratio. For this

[13] Note that job cards are only issued in rural areas but that individuals could move between towns, cities, and their rural counterparts for work. In this section of the analysis, we apply the district's job cards issued to both the rural and urban areas under the assumption that impacts on urban areas if they appeared would also be greater when more nearby rural permits are issued.

reason, we do not include measures of district income in the main specifications as a control, as it is likely a "bad control" to borrow the language of Angrist and Pischke (2009).[14]

Instead, the myriad of mechanisms through which income may influence son preference and youth sex ratios suggest that it is a promising avenue for heterogeneity analysis. It is possible that the impacts of NREGS could vary based on the level of economic development of a region, as well as on the pre-existing level of son preference. In order to analyze distributional effects, we need some districts in both earlier and later waves with high and low values of these measures. Figures 3 and 4 plot the distribution of districts on the basis of nighttime satellite measured luminosity (a proxy for income) and on the 2001 census measure of sex ratio. These densities are broken apart into those districts in the early waves (1 and 2) of NREGS and those which did not receive NREGS until 2008, wave 3. As can be seen in both kernel densities, there is a significant amount of overlap for both distributions. This means that some higher income districts received NREGS earlier and some lower income districts still did not receive NREGS until 2008.

In Table 7, we use this fact and take the overall distribution and split it into terciles each of which contain some districts from all three waves. The analysis includes all the controls of the previous analysis and focuses only on rural parts of districts. In columns (1) – (3) we examine the role of income heterogeneity. When looking at the timing of NREGS, splitting the sample yields three negative but insignificant coefficients, the largest of which appears for middle income districts. In Panel B, we examine the impact of the intensity of job card issuance in these regions. The impact of NREGS intensity is significantly larger in the sub-sample of richer districts.

[14] Nevertheless, we include Appendix Table 3, which controls for satellite night lights (aggregated from the village level to the mean of lights at the district level) as a proxy for district incomes. As can be seen from the table, when we do this, our main specification in Panel A produces roughly the same negative magnitude on rural youth sex ratios, but the coefficient is significant at non-traditional levels of confidence. At the same time we continue to observe large impacts of the intensity of job card issuance per capita on the youth sex ratio in rural areas.

In Columns (4), (5), and (6), we examine the role of initial gender norms in the region, as measured by the initial sex ratio, in impacting the association between NREGS and the change in the sex ratio over time. Improvements in the sex ratio appear largest in the most skewed districts (which is consistent with the previous result, as richer parts of the country tend to have more skewed sex ratios on average). At the same time, we do not observe impacts for middle income districts, but do observe some evidence of them for the poorest. Although we cannot speak further to the underlying mechanism, these results are consistent with a story in which increased women's economic empowerment dampens son preference to greater extent than it is increased by a relaxed constraint on overall household income in these districts.

These results are also inconsistent with the primary selection concern, that districts which received NREGS had relatively less skewed sex ratios between 2001 and 2011 simply because trends favored lower income districts having this improvement. Instead, the effects we observe appear larger in richer districts.

5. Conclusion

This paper exploits the staggered roll-out of India's National Rural Employment Guarantee Scheme to examine the implications of the program for India's Missing Women problem. We show that in regions which implemented NREGA earlier, and in those which saw more job cards issued per capita, the child sex ratio significantly improved in favor of girls. This result suggests that son preferences may have been weakened, either through improved labor market opportunities for women either impacted attitudes towards daughters, reductions in household income constraints or vulnerability to shocks, or through improved bargaining power for women in general.

We present several pieces of evidence to suggest that the impacts we observe are likely caused and not just correlated with the implementation of NREGS. These results are robust to the inclusion of timevariant district controls, appear mostly for the child sex ratio and not the adult sex ratio, and appear in the rural areas of districts where NREGS was implemented, but not in urban areas where it was not. They are also larger where NREGS take-up was greater, and appear in sets of districts with characteristics which are inconsistent with alternative explanations stemming from selection effects.

It is important to note that the impacts we observe are estimated from differences in exposure to NREGS and unable to provide a perfect answer to policy questions concerning the value of work fare programs as the general equilibrium effects of the overall program for the full period which could be different. Nevertheless, our results highlight the fact that the general equilibrium impacts of large workfare programs may extend to include influencing son preference, and programs such as NREGS represent an important avenue through which future policy interventions seeking to reduce gender-based discrimination might be able to help nudge the sex ratio back toward biological norms.

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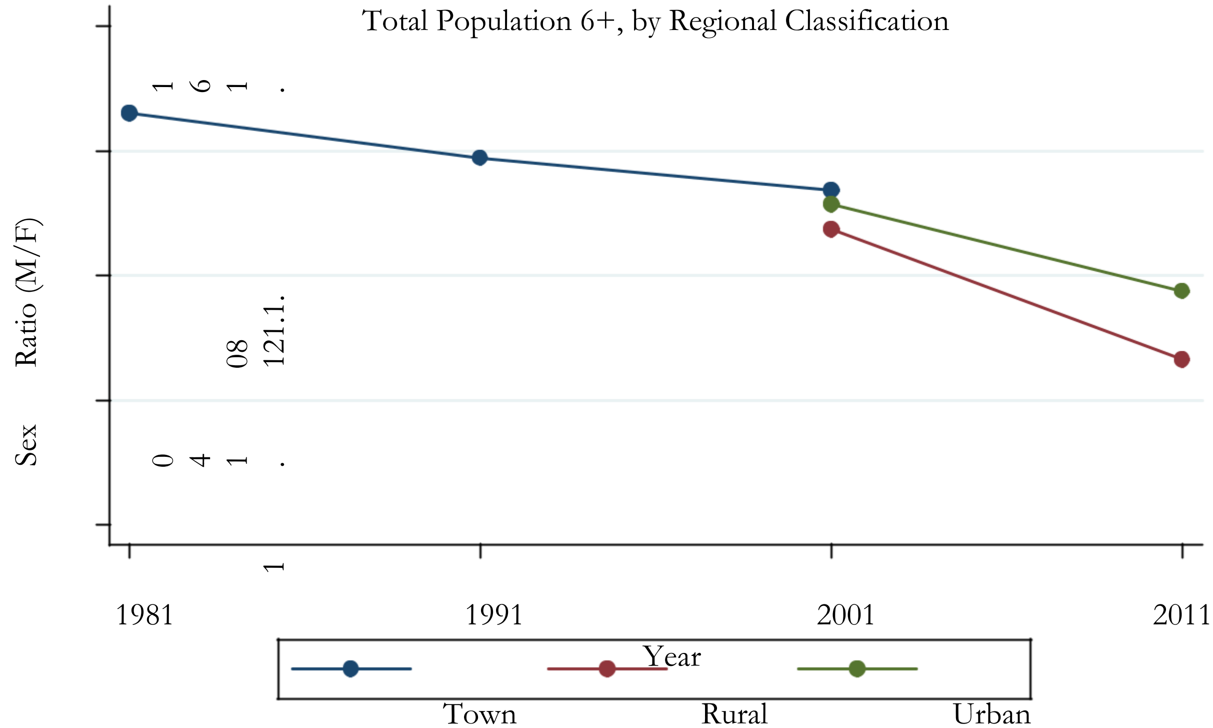
Figure 1: Sex Ratio Trends in India

Total Population 6+, by Regional Classification



Source: Author's Calculations using 2001 town records, as well as 2001 and 2011 district

Figure 2: Year of NREGA Implementation

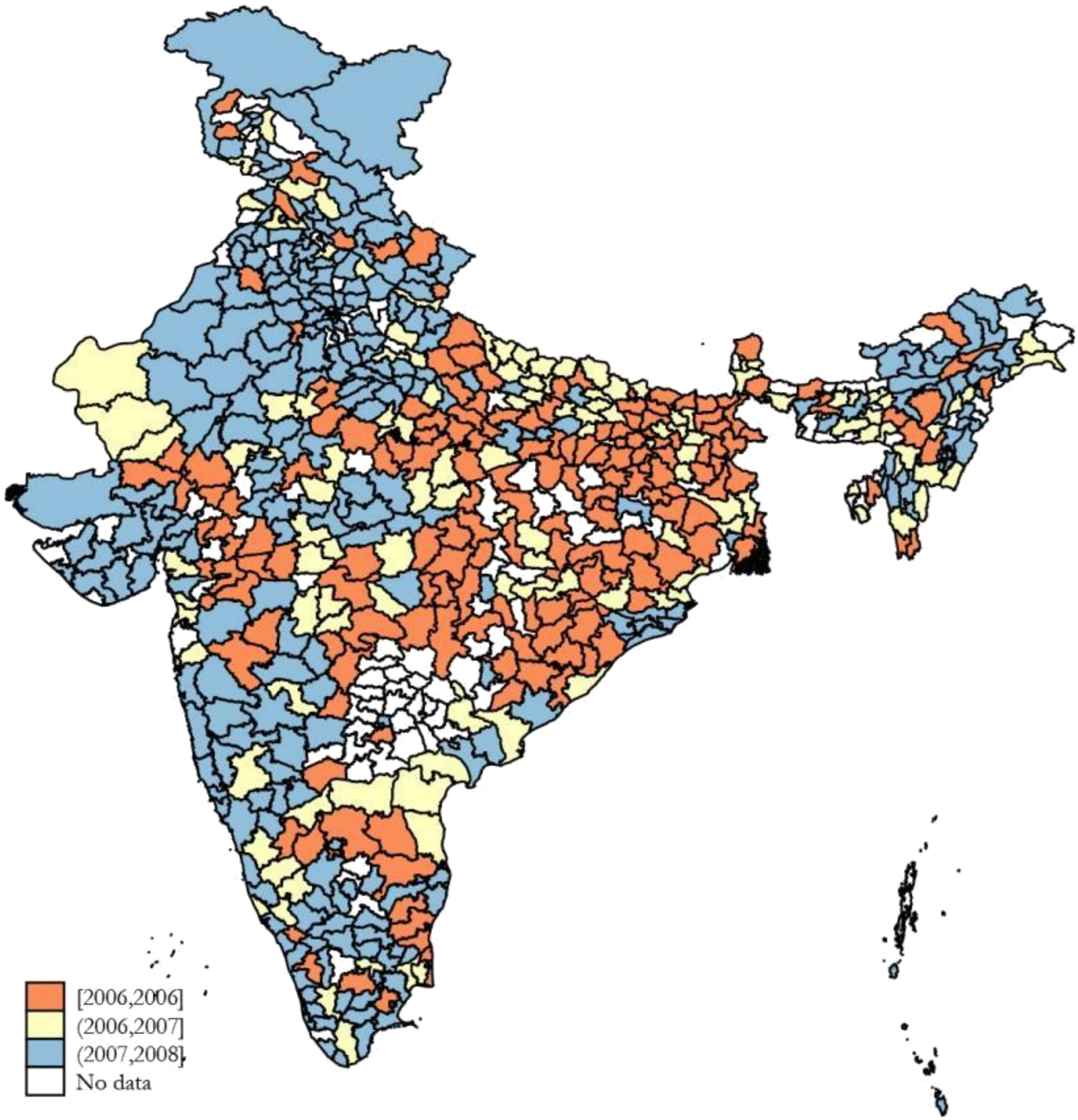
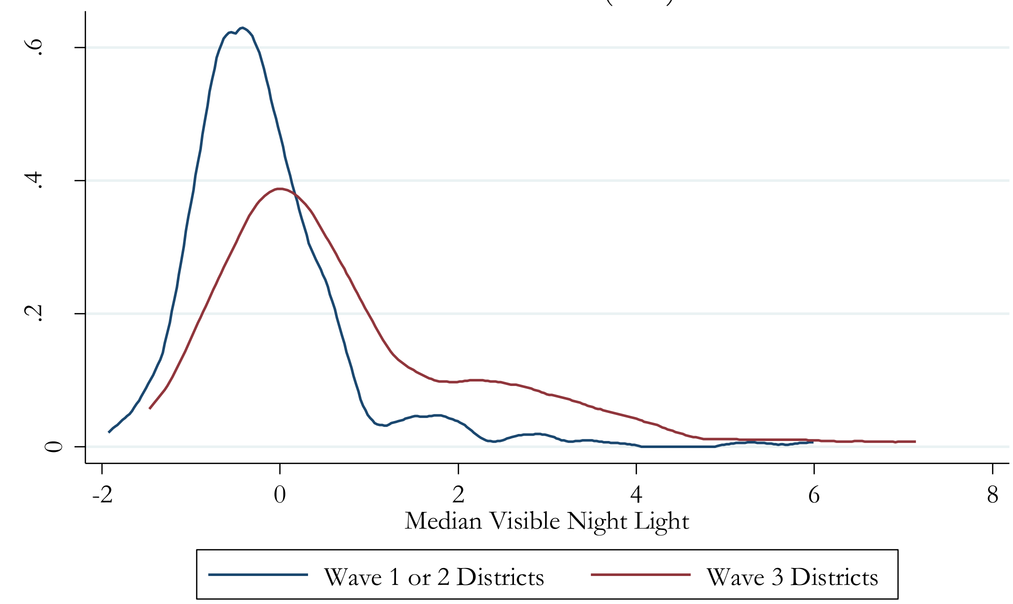


Figure 3: The Distribution of Nighttime Light Intensity

Indian Districts (2001)



Note: Figure excludes top 1% of districts based on luminosity for scaling.

Figure 4: The Distribution of Child Sex Ratios

Indian Districts (Census 2001)

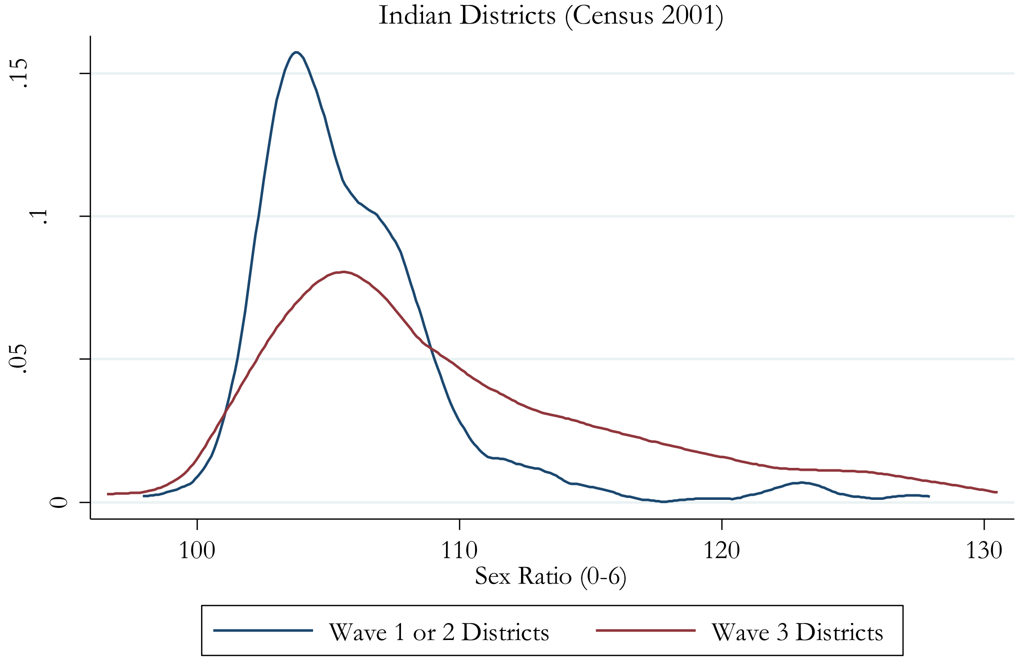


Table 1: NREGA Implementation Dates by District, Full Sample

Year Began	Obs	Percent
2006	181	32.4
2007	125	22.4
2008	253	45.3
Total	559	100.0

Source: Ministry of Rural Development, Government of India (2018).

Table 2: District Demographics

	Full Sample		2006 & 2007 Implementers		2008 Implementers		Early vs. Late: (3) vs (5)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Diff	SE
Panel A: District Demographics - 2001	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total Population	1727925	1344589	1789138	1313114	1710667	1354255	65,823	(69,651)
Total Households	324198	268645	336243	273051	320802	267596	12,671	(13,847)
Sex Ratio (Age 6+)	107.45	7.52	106.91	5.44	107.61	8.01	-1.14***	(0.44)
Sex Ratio (Age 0 to 6)	107.87	5.95	106.83	4.87	108.16	6.19	-1.46***	(0.31)
Scheduled Tribe or Caste (%)	0.31	0.22	0.31	0.21	0.31	0.22	0.00	(0.01)
Illiterate (%)	0.46	0.12	0.47	0.12	0.45	0.12	0.01*	(0.01)
Employed (%)	0.41	0.07	0.40	0.07	0.41	0.07	-0.01*	(0.00)
Agiculture (%)	0.18	0.08	0.19	0.07	0.17	0.08	0.01*	(0.01)
Panel B: District Demographics - 2011	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Diff	SE
Total Population	2001479	1601755	2060078	1583333	1984958	1608257	64,568	(82,066)
Total Households	410784	348592	425422	357042	406657	346461	15,613	(17,862)
Sex Ratio (Age 6+)	106.26	7.54	105.67	4.87	106.42	8.14	-1.14***	(0.39)
Sex Ratio (Age 0 to 6)	108.71	5.21	107.75	4.35	108.98	5.41	-1.37***	(0.28)
Scheduled Tribe or Caste (%)	0.32	0.22	0.32	0.21	0.32	0.22	0.00	(0.01)
Illiterate (%)	0.37	0.10	0.38	0.11	0.37	0.10	0.01	(0.01)
Employed (%)	0.41	0.07	0.41	0.07	0.41	0.07	-0.01*	(0.00)
Agiculture (%)	0.10	0.06	0.10	0.06	0.10	0.07	-0.00	(0.00)

Notes: Samples as defined in Table 1. District averages. Not population weighted. Sex ratio calculated as males per 100 females in the population. Panel A and B: Agricultural workers include those defined as working in agriculture or cultivation work. Panel C: Covers only populations in cities within the district.

Source: Author's calculations using 2001 and 2011 National Census of India, 2001 Census City Yearbook.
*** p<0.01, ** p<0.05, * p<0.1

Table 3: 2001 District Characteristics

	Full Sample		2006 & 2007 Implementers		2008 Implementers		Early vs. Late: (3) vs (5)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Diff	SE
Distric Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unpaved roads (Kaccha road)	162	409	115	157	175	455	-60.78***	(14.90)
Paved roads (Pakka road)	366	642	273	389	392	694	-118.71***	(27.75)
Hospitals	17	32	12	16	19	35	-6.45***	(1.27)
Health Centers	7	11	7	13	7	10	0.08	(0.71)
Secondary Schools	55	82	44	63	59	86	-14.68***	(4.03)
Middle Schools	82	115	69	95	86	120	-16.79***	(5.93)
Primary Schools	172	252	141	191	181	266	-39.39***	(12.31)
Banks	67	118	50	56	72	130	-22.07***	(4.61)
Density (Population/Area)	16953	23996	21365	29947	15722	21907	5,643.13***	(1,677.03)

Notes: Samples as defined in Table 1. District averages. Not population weighted.

Source: Author's calculations using 2001 National Census of India.
*** p<0.01, ** p<0.05, * p<0.1

Table 4: NREGS Implementation and Youth Sex Ratios in Rural Districts

	Baseline (1)	Initial Youth Sex Ratio (2)	District Characteristics (3)	Census Demographics (4)	Full Controls (6)
Wave 1 or Wave 2 NREGA	-3.552***	-0.400	-3.450***	-3.459***	-0.543*
	(0.465)	(0.286)	(0.498)	(0.481)	(0.306)
2001 Sex Ratio M/F (Total)		0.771***			0.748***
		(0.034)			(0.033)
Census2001 area			-0.002		-0.002*
			(0.002)		(0.001)
Unpaved (Kaccha road) in 01			-0.002***		-0.001***
			(0.001)		(0.000)
Paved roads (Pakka road) in 01			-0.000		0.000
			(0.001)		(0.000)
Census01 # Hospitals			0.010		0.011***
			(0.008)		(0.004)
Census01 # Healthcenters			-0.032		-0.019*
			(0.023)		(0.010)
Census01 # Secondary schools			0.002		-0.002
			(0.007)		(0.003)
Census01 # Middle schools			0.003		0.003
			(0.003)		(0.002)
Census01 # Primary schools			-0.001		-0.000
			(0.002)		(0.001)
Census01 number of banks			0.004**		0.001
			(0.002)		(0.002)
% Scheduled Tribe or Caste				-4.785***	-1.494*
				(1.062)	(0.792)
% Illiterate				0.488	2.011
				(2.201)	(1.269)
% Employed				-11.402***	-4.116*
				(4.156)	(2.464)
% Agriculture or Cultivation				14.715***	5.031
				(4.807)	(3.154)
Mean of Dep. Var.	108.4	108.4	108.4	108.4	108.4
Number of observations	574	574	564	574	564
R2	0.004	0.907	0.077	0.262	0.915

Note: OLS Regressions. Dependent variable is Sex Ratio in 2011 for 0-6 year old population. Includes a constant. Regressions are population weighted.
Source: Author's calculations using 2001 and 2011 National Census of India, 2001 Census City Yearbook.
*** p<0.01, ** p<0.05, * p<0.1

Table 5: The Association Between NREGS and Sex Ratios at Birth and Overall by Rural/Urban Status

	Youth Sex Ratio (0-6)			Sex Ratio (Age 6+)		
	(1)	(2)	(3)	(4)	(5)	(6)
NREGS Timing	**Total**	**Rural**	**Urban**	**Total**	**Rural**	**Urban**
Wave 1 or Wave 2 NREGA	-0.376	-0.543*	0.181	0.263	0.257	-0.100
	(0.282)	(0.306)	(0.240)	(0.166)	(0.191)	(0.263)
Initial SR Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Characteristics (Census)	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls (Census)	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var	108.9	108.4	110.4	106.1	105.4	107.9
Number of observations	571	564	571	571	564	571

Note: Dependent variable is Sex Ratio in 2011. Regressions are population weighted.

Source: Author's calculations using 2001 and 2011 National Census of India, 2001 Census City Yearbook. Ministry of Rural Development, Government of India 2018.
*** p<0.01, ** p<0.05, * p<0.1

Table 6: NREGS Take Up and Expenditure

	Youth Sex Ratio (0-6)			Sex Ratio (Age 6+)		
	(1)	(2)	(3)	(4)	(5)	(6)
NREGS Uptake	**Total**	**Rural**	**Urban**	**Total**	**Rural**	**Urban**
LN (Job Cards PC, Yearly Avg)	-0.432**	-0.597**	-0.025	0.054	0.075	-0.092
	(0.219)	(0.276)	(0.227)	(0.129)	(0.104)	(0.209)
Initial SR Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Characteristics (Census)	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls (Census)	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var	108.9	108.4	110.4	106.1	105.4	107.9
Number of observations	571	564	571	571	564	571

Note: Dependent variable is Sex Ratio in 2011. Regressions are population weighted.

Source: Author's calculations using 2001 and 2011 National Census of India, 2001 Census City Yearbook. Ministry of Rural Development, Government of India 2018.
*** p<0.01, ** p<0.05, * p<0.1

Table 7: The Heterogenous Impact of NREGS on Child Sex Ratios by District Characteristics (Rural Districts)

	Income Heterogeneity			Sex Ratio Heterogeneity		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Timing	**Lowest 33%**	**Middle 33%**	**Highest 33%**	**Lowest 33%**	**Middle 33%**	**Highest 33%**
Wave 1 or Wave 2 NREGA	-0.292	-0.379	-0.041	-0.302	0.157	-1.563**
	(0.436)	(0.392)	(0.679)	(0.414)	(0.291)	(0.654)
Panel B: Uptake						
LN (Job Cards PC, Yearly Avg)	-0.092	-0.304	-0.982**	-0.907***	0.463	-1.050**
	(0.349)	(0.321)	(0.418)	(0.350)	(0.295)	(0.455)
Initial SR Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Characteristics (Census)	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls (Census)	Yes	Yes	Yes	Yes	Yes	Yes
Night Lights (Village Level)	Yes	Yes	Yes	Yes	Yes	Yes

Note: Dependent variable is Sex Ratio in 2011. Regressions are population weighted. Sample size ranges from 164 to 197 observations.

Source: Author's calculations using 2001 and 2011 National Census of India, 2001 Census City Yearbook. Ministry of Rural Development, Government of India 2018.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1: NREGS Implementation Dates

2006		2007		2008	
District	**State**	**District**	**State**	**District**	**State**
Kupwara	Jammu And Kashmir	Anantnag	Jammu And Kashmir	Srinagar	Jammu And Kashmir
Poonch	Jammu And Kashmir	Jammu	Jammu And Kashmir	Badgam	Jammu And Kashmir
Chamba	Himachal Pradesh	Kangra	Himachal Pradesh	Pulwama	Jammu And Kashmir
Sirmaur	Himachal Pradesh	Mandi	Himachal Pradesh	Leh Ladakh	Jammu And Kashmir
Hoshiarpur	Punjab	Amritsar	Punjab	Kargil	Jammu And Kashmir
Chamoli	Uttarakhand	Jalandhar	Punjab	Ramban	Jammu And Kashmir
Tehri Garhwal	Uttarakhand	Nawanshahr	Punjab	Udhampur	Jammu And Kashmir
Champawat	Uttarakhand	Udam Singh Nagar	Uttarakhand	Rajauri	Jammu And Kashmir
Sirsa	Haryana	Haridwar	Uttarakhand	Kathua	Jammu And Kashmir
Mahendragarh	Haryana	Ambala	Haryana	Lahul And Spiti	Himachal Pradesh
Karauli	Rajasthan	Sawai Madhopur	Rajasthan	Kullu	Himachal Pradesh
Sirohi	Rajasthan	Jaisalmer	Rajasthan	Hamirpur	Himachal Pradesh
Udaipur	Rajasthan	Barmer	Rajasthan	Una	Himachal Pradesh
Dungarpur	Rajasthan	Jalore	Rajasthan	Bilaspur	Himachal Pradesh
Banswara	Rajasthan	Tonk	Rajasthan	Solan	Himachal Pradesh
Jhalawar	Rajasthan	Chittorgarh	Rajasthan	Shimla	Himachal Pradesh
Kheri	Uttar Pradesh	Etah	Uttar Pradesh	Kinnaur	Himachal Pradesh
Sitapur	Uttar Pradesh	Budaun	Uttar Pradesh	Gurdaspur	Punjab
Hardoi	Uttar Pradesh	Farrukhabad	Uttar Pradesh	Kapurthala	Punjab
Unnao	Uttar Pradesh	Kanpur Dehat	Uttar Pradesh	Rupnagar	Punjab
Rae Bareli	Uttar Pradesh	Jhansi	Uttar Pradesh	Fatehgarh Sahib	Punjab
Jalaun	Uttar Pradesh	Ambedkar Nagar	Uttar Pradesh	Ludhiana	Punjab
Lalitpur	Uttar Pradesh	Sultanpur	Uttar Pradesh	Moga	Punjab
Hamirpur	Uttar Pradesh	Bahraich	Uttar Pradesh	Firozepur	Punjab
Mahoba	Uttar Pradesh	Shravasti	Uttar Pradesh	Sri Muktsar Sahib	Punjab
Banda	Uttar Pradesh	Balrampur	Uttar Pradesh	Faridkot	Punjab
Chitrakoot	Uttar Pradesh	Gonda	Uttar Pradesh	Bathinda	Punjab
Fatehpur	Uttar Pradesh	Siddharth Nagar	Uttar Pradesh	Mansa	Punjab
Pratapgarh	Uttar Pradesh	Basti	Uttar Pradesh	Sangrur	Punjab
Kaushambi	Uttar Pradesh	Sant Kabeer Nagar	Uttar Pradesh	Patiala	Punjab
Barabanki	Uttar Pradesh	Maharajganj	Uttar Pradesh	Chandigarh	Chandigarh
Gorakhpur	Uttar Pradesh	Mau	Uttar Pradesh	Uttar Kashi	Uttarakhand
Kushi Nagar	Uttar Pradesh	Ballia	Uttar Pradesh	Rudra Prayag	Uttarakhand
Azamgarh	Uttar Pradesh	Pashchim Champaran	Bihar	Dehradun	Uttarakhand
Jaunpur	Uttar Pradesh	Purbi Champaran	Bihar	Pauri Garhwal	Uttarakhand
Chandauli	Uttar Pradesh	Sitamarhi	Bihar	Pithoragarh	Uttarakhand
Mirzapur	Uttar Pradesh	Madhepura	Bihar	Bageshwar	Uttarakhand
Sonbhadra	Uttar Pradesh	Saharsa	Bihar	Almora	Uttarakhand
Sheohar	Bihar	Gopalganj	Bihar	Nainital	Uttarakhand
Madhubani	Bihar	Siwan	Bihar	Panchkula	Haryana
Supaul	Bihar	Saran	Bihar	Yamunanagar	Haryana
Araria	Bihar	Begusarai	Bihar	Kurukshetra	Haryana
Kishanganj	Bihar	Khagaria	Bihar	Kaithal	Haryana
Purnia	Bihar	Bhagalpur	Bihar	Karnal	Haryana
Katihar	Bihar	Banka	Bihar	Panipat	Haryana
Darbhanga	Bihar	Sheikhpura	Bihar	Sonipat	Haryana
Muzaffarpur	Bihar	Buxar	Bihar	Jind	Haryana
Vaishali	Bihar	South District	Sikkim	Fatehabad	Haryana
Samastipur	Bihar	East District	Sikkim	Hisar	Haryana
Munger	Bihar	Lohit	Arunachal Pradesh	Bhiwani	Haryana
Lakhisarai	Bihar	Changlang	Arunachal Pradesh	Rohtak	Haryana
Nalanda	Bihar	Mokokchung	Nagaland	Jhajjar	Haryana
Patna	Bihar	Wokha	Nagaland	Rewari	Haryana

Appendix Table 1: NREGS Implementation Dates

2006		2007		2008	
District	**State**	**District**	**State**	**District**	**State**
Bhojpur	Bihar	Peren	Nagaland	Gurugram	Haryana
Kaimur (Bhabua)	Bihar	Churachandpur	Manipur	Faridabad	Haryana
Rohtas	Bihar	Chandel	Manipur	Ganganagar	Rajasthan
Jehanabad	Bihar	Champhai	Mizoram	Hanumangarh	Rajasthan
Aurangabad	Bihar	Lunglei	Mizoram	Bikaner	Rajasthan
Gaya	Bihar	West Tripura	Tripura	Churu	Rajasthan
Nawada	Bihar	South Tripura	Tripura	Jhunjhunu	Rajasthan
Jamui	Bihar	West Khasi Hills	Meghalaya	Alwar	Rajasthan
North District	Sikkim	Ri Bhoi	Meghalaya	Bharatpur	Rajasthan
Upper Subansiri	Arunachal Pradesh	East Khasi Hills	Meghalaya	Dholpur	Rajasthan
Mon	Nagaland	West Jaintia Hills	Meghalaya	Dausa	Rajasthan
Tamenglong	Manipur	Barpeta	Assam	Jaipur	Rajasthan
Lawngtlai	Mizoram	Nalbari	Assam	Sikar	Rajasthan
Saiha	Mizoram	Darrang	Assam	Nagaur	Rajasthan
Dhalai	Tripura	Marigaon	Assam	Jodhpur	Rajasthan
Kokrajhar	Assam	Cachar	Assam	Pali	Rajasthan
Goalpara	Assam	Hailakandi	Assam	Ajmer	Rajasthan
Bongaigaon	Assam	Darjeeling	West Bengal	Bundi	Rajasthan
Lakhimpur	Assam	Coochbehar	West Bengal	Bhilwara	Rajasthan
Dhemaji	Assam	Bardhaman	West Bengal	Rajsamand	Rajasthan
Karbi Anglong	Assam	Nadia	West Bengal	Kota	Rajasthan
Dima Hasao	Assam	Hooghly	West Bengal	Baran	Rajasthan
Jalpaiguri	West Bengal	Deoghar	Jharkhand	Saharanpur	Uttar Pradesh
Dinajpur Uttar	West Bengal	East Singhbum	Jharkhand	Muzaffarnagar	Uttar Pradesh
Dinajpur Dakshin	West Bengal	Bargarh	Orissa	Bijnor	Uttar Pradesh
Maldah	West Bengal	Baleshwar	Orissa	Moradabad	Uttar Pradesh
Murshidabad	West Bengal	Bhadrak	Orissa	Rampur	Uttar Pradesh
Birbhum	West Bengal	Jajapur	Orissa	Meerut	Uttar Pradesh
24 Paraganas North	West Bengal	Anugul	Orissa	Baghpat	Uttar Pradesh
Bankura	West Bengal	Korba	Chhattisgarh	Ghaziabad	Uttar Pradesh
Purulia	West Bengal	Janjgir-Champa	Chhattisgarh	Bulandshahr	Uttar Pradesh
Medinipur West	West Bengal	Raipur	Chhattisgarh	Aligarh	Uttar Pradesh
24 Paraganas South	West Bengal	Mahasamund	Chhattisgarh	Hathras	Uttar Pradesh
Garhwa	Jharkhand	Datia	Madhya Pradesh	Mathura	Uttar Pradesh
Latehar	Jharkhand	Guna	Madhya Pradesh	Agra	Uttar Pradesh
Chatra	Jharkhand	Panna	Madhya Pradesh	Firozabad	Uttar Pradesh
Hazaribagh	Jharkhand	Damoh	Madhya Pradesh	Mainpuri	Uttar Pradesh
Koderma	Jharkhand	Rewa	Madhya Pradesh	Bareilly	Uttar Pradesh
Giridih	Jharkhand	Anuppur	Madhya Pradesh	Pilibhit	Uttar Pradesh
Godda	Jharkhand	Dewas	Madhya Pradesh	Shahjahanpur	Uttar Pradesh
Sahebganj	Jharkhand	East Nimar	Madhya Pradesh	Lucknow	Uttar Pradesh
Pakur	Jharkhand	Rajgarh	Madhya Pradesh	Kannauj	Uttar Pradesh
Dumka	Jharkhand	Harda	Madhya Pradesh	Etawah	Uttar Pradesh
Dhanbad	Jharkhand	Katni	Madhya Pradesh	Auraiya	Uttar Pradesh
Bokaro	Jharkhand	Chhindwara	Madhya Pradesh	Kanpur Nagar	Uttar Pradesh
Lohardaga	Jharkhand	Bharuch	Gujarat	Allahabad	Uttar Pradesh
Gumla	Jharkhand	Navsari	Gujarat	Faizabad	Uttar Pradesh
West Singhbhum	Jharkhand	Valsad	Gujarat	Deoria	Uttar Pradesh
Jharsuguda	Orissa	Buldhana	Maharashtra	Ghazipur	Uttar Pradesh
Sambalpur	Orissa	Akola	Maharashtra	Varanasi	Uttar Pradesh
Deogarh	Orissa	Washim	Maharashtra	Bhadohi	Uttar Pradesh
Sundargarh	Orissa	Wardha	Maharashtra	Tawang	Arunachal Pradesh
Kendujhar	Orissa	Thane	Maharashtra	West Kameng	Arunachal Pradesh

Appendix Table 1: NREGS Implementation Dates

2006		2007		2008	
District	**State**	**District**	**State**	**District**	**State**
Mayurbhanj	Orissa	Osmanabad	Maharashtra	East Kameng	Arunachal Pradesh
Dhenkanal	Orissa	Srikakulam	Andhra Pradesh	Papum Pare	Arunachal Pradesh
Ganjam	Orissa	East Godavari	Andhra Pradesh	Lower Subansiri	Arunachal Pradesh
Gajapati	Orissa	Guntur	Andhra Pradesh	West Siang	Arunachal Pradesh
Kandhamal	Orissa	Prakasam	Andhra Pradesh	East Siang	Arunachal Pradesh
Boudh	Orissa	Spsr Nellore	Andhra Pradesh	Upper Siang	Arunachal Pradesh
Sonepur	Orissa	Kurnool	Andhra Pradesh	Dibang Valley	Arunachal Pradesh
Balangir	Orissa	Belagavi	Karnataka	Tirap	Arunachal Pradesh
Nuapada	Orissa	Ballari	Karnataka	Longleng	Nagaland
Kalahandi	Orissa	Shivamogga	Karnataka	Zunheboto	Nagaland
Rayagada	Orissa	Chikkamagaluru	Karnataka	Dimapur	Nagaland
Nabarangpur	Orissa	Hassan	Karnataka	Phek	Nagaland
Koraput	Orissa	Kodagu	Karnataka	Senapati	Manipur
Malkangiri	Orissa	Kasaragod	Kerala	Bishnupur	Manipur
Korea	Chhattisgarh	Idukki	Kerala	Thoubal	Manipur
Surguja	Chhattisgarh	Karur	Tamil Nadu	Imphal West	Manipur
Raigarh	Chhattisgarh	Thiruvarur	Tamil Nadu	Imphal East	Manipur
Bilaspur	Chhattisgarh	Thanjavur	Tamil Nadu	Ukhrul	Manipur
Kabirdham	Chhattisgarh	Tirunelveli	Tamil Nadu	Mamit	Mizoram
Rajnandgaon	Chhattisgarh			Kolasib	Mizoram
Dhamtari	Chhattisgarh			Aizawl	Mizoram
Kanker	Chhattisgarh			Serchhip	Mizoram
Bastar	Chhattisgarh			North Tripura	Tripura
Sheopur	Madhya Pradesh			East Garo Hills	Meghalaya
Shivpuri	Madhya Pradesh			Dhubri	Assam
Tikamgarh	Madhya Pradesh			Kamrup	Assam
Chhatarpur	Madhya Pradesh			Nagaon	Assam
Satna	Madhya Pradesh			Sonitpur	Assam
Umaria	Madhya Pradesh			Tinsukia	Assam
Sidhi	Madhya Pradesh			Dibrugarh	Assam
Alirajpur	Madhya Pradesh			Sivasagar	Assam
Dhar	Madhya Pradesh			Jorhat	Assam
Khargone	Madhya Pradesh			Golaghat	Assam
Barwani	Madhya Pradesh			Karimganj	Assam
Betul	Madhya Pradesh			Howrah	West Bengal
Dindori	Madhya Pradesh			Ranchi	Jharkhand
Mandla	Madhya Pradesh			Kendrapara	Orissa
Seoni	Madhya Pradesh			Jagatsinghapur	Orissa
Balaghat	Madhya Pradesh			Cuttack	Orissa
Banas Kantha	Gujarat			Nayagarh	Orissa
Sabar Kantha	Gujarat			Khordha	Orissa
Panch Mahals	Gujarat			Puri	Orissa
Dohad	Gujarat			Durg	Chhattisgarh
Narmada	Gujarat			Dantewada	Chhattisgarh
Dang	Gujarat			Morena	Madhya Pradesh
Nandurbar	Maharashtra			Bhind	Madhya Pradesh
Dhule	Maharashtra			Gwalior	Madhya Pradesh
Amravati	Maharashtra			Sagar	Madhya Pradesh
Bhandara	Maharashtra			Neemuch	Madhya Pradesh
Gondia	Maharashtra			Mandsaur	Madhya Pradesh
Gadchiroli	Maharashtra			Ratlam	Madhya Pradesh
Chandrapur	Maharashtra			Ujjain	Madhya Pradesh
Yavatmal	Maharashtra			Shajapur	Madhya Pradesh

Appendix Table 1: NREGS Implementation Dates

2006		2007		2008	
District	State	District	State	District	State
Nanded	Maharashtra			Indore	Madhya Pradesh
Hingoli	Maharashtra			Vidisha	Madhya Pradesh
Aurangabad	Maharashtra			Bhopal	Madhya Pradesh
Ahmednagar	Maharashtra			Sehore	Madhya Pradesh
Vizianagaram	Andhra Pradesh			Raisen	Madhya Pradesh
Y.S.R.	Andhra Pradesh			Hoshangabad	Madhya Pradesh
Anantapur	Andhra Pradesh			Jabalpur	Madhya Pradesh
Chittoor	Andhra Pradesh			Narsinghpur	Madhya Pradesh
Bidar	Karnataka			Kachchh	Gujarat
Raichur	Karnataka			Patan	Gujarat
Chitradurga	Karnataka			Mahesana	Gujarat
Davangere	Karnataka			Gandhinagar	Gujarat
Wayanad	Kerala			Ahmadabad	Gujarat
Palakkad	Kerala			Surendranagar	Gujarat
Tiruvannamalai	Tamil Nadu			Rajkot	Gujarat
Villupuram	Tamil Nadu			Jamnagar	Gujarat
Dindigul	Tamil Nadu			Porbandar	Gujarat
Cuddalore	Tamil Nadu			Junagadh	Gujarat
Nagapattinam	Tamil Nadu			Amreli	Gujarat
Sivaganga	Tamil Nadu			Bhavnagar	Gujarat
				Anand	Gujarat
				Kheda	Gujarat
				Vadodara	Gujarat
				Surat	Gujarat
				Diu	Daman And Diu
				Daman	Daman And Diu
				Jalgaon	Maharashtra
				Nagpur	Maharashtra
				Parbhani	Maharashtra
				Jalna	Maharashtra
				Nashik	Maharashtra
				Raigad	Maharashtra
				Pune	Maharashtra
				Beed	Maharashtra
				Latur	Maharashtra
				Solapur	Maharashtra
				Satara	Maharashtra
				Ratnagiri	Maharashtra
				Sindhudurg	Maharashtra
				Kolhapur	Maharashtra
				Sangli	Maharashtra
				Visakhapatanam	Andhra Pradesh
				West Godavari	Andhra Pradesh
				Krishna	Andhra Pradesh
				Bagalkot	Karnataka
				Vijayapura	Karnataka
				Kalaburagi	Karnataka
				Koppal	Karnataka
				Gadag	Karnataka
				Dharwad	Karnataka
				Uttar Kannad	Karnataka
				Haveri	Karnataka
				Udupi	Karnataka

Appendix Table 1: NREGS Implementation Dates

2006		2007		2008	
District	**State**	**District**	**State**	**District**	**State**
				Tumakuru	Karnataka
				Kolar	Karnataka
				Bengaluru Urban	Karnataka
				Ramanagara	Karnataka
				Mandya	Karnataka
				Dakshin Kannad	Karnataka
				Mysuru	Karnataka
				Chamarajanagar	Karnataka
				North Goa	Goa
				South Goa	Goa
				Lakshadweep District	Lakshadweep
				Kannur	Kerala
				Kozhikode	Kerala
				Malappuram	Kerala
				Thrissur	Kerala
				Ernakulam	Kerala
				Kottayam	Kerala
				Alappuzha	Kerala
				Pathanamthitta	Kerala
				Thiruvananthapuram	Kerala
				Thiruvallur	Tamil Nadu
				Kanchipuram	Tamil Nadu
				Vellore	Tamil Nadu
				Dharmapuri	Tamil Nadu
				Salem	Tamil Nadu
				Namakkal	Tamil Nadu
				Erode	Tamil Nadu
				The Nilgiris	Tamil Nadu
				Coimbatore	Tamil Nadu
				Tiruchirappalli	Tamil Nadu
				Perambalur	Tamil Nadu
				Ariyalur	Tamil Nadu
				Pudukkottai	Tamil Nadu
				Madurai	Tamil Nadu
				Theni	Tamil Nadu
				Virudhunagar	Tamil Nadu
				Ramanathapuram	Tamil Nadu
				Tuticorin	Tamil Nadu
				Kanniyakumari	Tamil Nadu
				Pondicherry	Puducherry
				Karaikal	Puducherry
				South Andamans	And Nicobar Islands
				Nicobars	And Nicobar Islands

Source: Ministry of Rural Development, Government of India (2018).

Appendix Table 2: Rural / Urban Differences

	Full District			Rural			Urban		
Panel A: District Demographics - 2001	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Total Population	579	1715041	1336056	571	1257076	929290	569	483689	783535
Total Households	579	321789	267239	571	232249	175804	569	94379	164934
Sex Ratio (Age 6+)	579	107.47	7.53	571	106.70	8.46	569	112.12	13.31
Sex Ratio (Age 0 to 6)	579	107.86	5.96	571	107.48	6.03	569	110.13	6.52
Panel B: District Demographics - 2011	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Total Population	579	1987085	1593320	571	1390307	1065070	569	626595	995207
Total Households	579	407815	347035	571	280013	217059	569	133943	227614
Sex Ratio (Age 6+)	579	106.27	7.56	571	105.72	6.67	569	108.78	12.75
Sex Ratio (Age 0 to 6)	579	108.72	5.22	571	108.55	5.64	569	110.07	5.52

Source: Author's calculations using 2001 and 2011 National Census of India, 2001 Census City Yearbook. Ministry of Rural Development, Government of India 2018.

Appendix Table 3: Night Lights Control

	Youth Sex Ratio (0-6)			Sex Ratio (Age 6+)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Timing	**Total**	**Rural**	**Urban**	**Total**	**Rural**	**Urban**
Wave 1 or Wave 2 NREGA	-0.269	-0.452	0.270	0.312**	0.330*	-0.193
	(0.278)	(0.305)	(0.246)	(0.150)	(0.178)	(0.233)
Panel B: Uptake						
LN (Job Cards PC, Yearly Avg)	-0.617**	-0.729**	-0.261	-0.016	-0.002	-0.145
	(0.240)	(0.286)	(0.236)	(0.135)	(0.121)	(0.235)
Initial SR Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Characteristics (Census)	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls (Census)	Yes	Yes	Yes	Yes	Yes	Yes
Night Lights (Village Level)	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var	109.0	108.4	110.7	106.0	105.4	107.8
Number of observations	522	522	522	522	522	522

Note: Dependent variable is Sex Ratio in 2011. Regressions are population weighted.

Source: Author's calculations using 2001 and 2011 National Census of India, 2001 Census City Yearbook. Ministry of Rural Development, Government of India 2018.
*** p<0.01, ** p<0.05, * p<0.1

Chapter 2

How effectively does Ethiopia's Public Safety Net Program protect children against negative shocks?

1. Introduction

Households in less developed countries such as Ethiopia, when confronted with shock, becomes vulnerable to economic deprivation (Baulch and Hoddinott 2000). This is true in Ethiopia with an economy mainly dependent on agriculture and prone to drought every three to five years (World Bank, 2013). Studies show that along with food insecurity, severe and repeated economic, natural and health shocks in Ethiopia trigger reductions in household food consumption and savings, and it necessitates reliance on borrowing and selling assets and livestock. These shocks also increase the dropout rate of children from school, which may lead to long-term and intergenerational vulnerability (Guush, et. al., 2014, Andersson, 2011; Berhane, 2014). Theoretically, idiosyncratic shocks at household-level are more likely to insure itself against the negative impacts due to informal risk-sharing networks. However, common shock such as natural shocks affects the communities, renders the households weaker. The differential impact of types of shocks suggests the importance of studying role played by a range of shocks such as natural, family, and economic shocks.

Natural shocks have a substantial impact on consumption growth in Ethiopia and that could result in poverty traps (Derecon, 2004). Studies show that covariates' natural and economic shocks negatively affect savings and consumption, whereas household-level idiosyncratic shock such as health shocks prompts reliance on borrowing along with a reduction in savings[15] (Yilma et al., 2014). This negative effect of shock prompts the necessity of acquiring a better understanding of how households

[15] Idiosyncratic shocks are household level that does not affect other neighboring households. Covariate shocks are common shocks that affect other households in villages. (Yilma et.al., 2014)

could better mitigate the shocks. This paper investigates whether a food-security welfare program helps mitigate the impact of a range of shocks on a child welfare.

Launched in 2005 in Ethiopia, Productive Safety Net Program (PSNP) became the second-largest social safety net program in Africa that transfers food or cash or combination of both to chronically food insecure and poor households. In 2009, the PSNP provided benefits to nearly 7.6 million rural populations in Ethiopia, which is around one in ten. The Impact Evaluation in 2008 shows PSNP helped to reduce the food gap by improving the food security of the households, with beneficiary households displaying higher calorie intake on the order of 30% after being affected by drought as compared to non-PSNP beneficiaries (IFPRI/CSA, 2009b; Devereux S et al., 2008). Other research studies investigating the role of the PSNP have also focused at the household level, assuming child outcomes are a part of observed household level effect (Gilligan, et. al. 2009; Andersson, 2011, Cook and Kabeer 2009). Few studies focus directly on child welfare in the form of school attendance or cognitive skills (Berhane et al., 2015; Debela, 2015; Hoddinott, 2010), and there is no study to the best of my knowledge that looks at whether the PSNP provides protection or directly mitigates the negative effect of shocks on child welfare. This paper helps fill this gap in the literature and provides insights essential to design of safety net programs. In this paper, I focus on the impact of three types of shocks: natural, economic, and idiosyncratic shocks, looking at effects on children's height-for-age z-scores, hours spent on household chores, and school enrollment, assessing whether the PSNP provides a cushion for the outcomes at the time of shocks.

The estimates suggest that exposure to shocks to households is detrimental to children's height-for-age, and affects their time spent on household chores. PSNP significantly helps buffer negative impact of idiosyncratic (family) shock showing increased school enrollment for children as compared to children from the non-PSNP households. More specifically, the idiosyncratic shock is associated with lower height-for-age during childhood, lower school enrollment and decreased time allocation on household chores likely because children need to work outside to help family financially. However, PSNP does not significantly buffer the negative impact on school enrollment due to exposure to aggregate shock such as economic shock and natural shock. Furthermore, aggregate shock is associated with lower height-for-age and increased time allocation on household chores with a small increase to no significant effect on school enrollment. This is likely due to school feeding program by world food program. The estimates also suggest that the idiosyncratic shock shows decline in time allocation for household chores because children are more likely to participate in labor force.

The paper unfolds by providing a literature review in Section 2 and a brief background of the PSNP in Section 3. Section 4 describes the data and Section 5 explores the extent to which the PSNP mitigates the negative impact of various shocks. Section 6 concludes.

2. Literature review

Prior research has shown that risk and negative shock worsen the livelihood of poor people in developing countries (Baulch and Hoddinott 2000; Dercon and Krishnan 2000a; Yamano, Alderman, and Christiaensen 2003). Yilma (2014) shows that aggregate shocks negatively affect consumption and savings, whereas idiosyncratic shocks prompts reliance on borrowing along with a reduction in savings.

With the effort to provide protection for shock-induced poverty, many developing countries have opted for safety net programs since the 1990s, which evolved from the concept of providing a long-term mechanism to alleviate chronic poverty while simultaneously reducing social exclusion (Devereux and Sabates-Wheeler 2004; Barrientos and Hulme 2005; Cook and Kabeer 2009). There have been many studies on safety net programs (Ellis, White, Lloyd-Sherlock, Chhotray and Seeley 2008; Barrientos 2010; Dercon 2011; Arnold, Conway and Greenslade 2011) which reflects their critical role in alleviating poverty, improving consumption smoothing, and promoting long-term investment in physical and human capital. This paper aims to contribute to this growing body of literature in helping design a better public welfare program by studying the effectiveness of the safety net program PSNP in cushioning the ill effects on children of repeated shocks.

Many developing countries have been opting for workforce programs to protect rural households like NREGA in India and the *Juntos* program in Peru to help improve economic development in the short and long run. Comparatively there are fewer studies on Ethiopia's PSNP. While these programs have been heavily studied, there are fewer empirical evidence on PSNP that finds positive impacts on asset growth, income growth, household livestock, household welfare, participation in nonfarm labor, and access to credit (Gilligan, 2009; Andersson, 2011; Debela, 2015; Hoddinott, 2010). Although some literature covers its impact on women empowerment (Woldehanna, 2010; Tankha, 2010), a limited focus has been given to the impact on children.

Child poverty entails fundamental deprivations as a result of which children grow up without access to economic, social, cultural, physical, environmental and/or political resources that are vital to their development and well-being. Safety net programs have the potential to indirectly impact child growth and development by providing household income and food security, as well as to impact school attendance and performance by impacting household income. Research papers have shown an association between early-life health and adult outcomes in the long run (Baird et.al., 2016; Gertler et.al., 2016). Though most literature on childhood has focused on developed countries (Bhalotra & Venkataramani, 2011; Bleakley, 2007), however, Currie et al. (2012) shows that childhood health is more significant in determining the long-run outcome for children because health shocks are prevalent in developing countries. They argue that height captures early life experiences and use it as a proxy for the early life environment.

Children who belong to households with poor nutritional status suffer poor physical growth, cognition, education attainment, and earnings in the long (The Young Lives Determinants and

Consequences of Child Growth Project Team (2015)). The Young Lives team studies the association between child anthropometry and food security using cross-sectional and longitudinal data. They show that height-for-age z-scores (HAZs) and body mass index for z scores (BMI-Zs) for food-insecure household children are significantly lower in developing countries like Ethiopia, India, Peru, and Vietnam. Their paper emphasizes studying the food security program's impact with exogenous shocks, as rural Ethiopia is even more vulnerable to shocks leading to more food insecurity. Quisumbing (2003) uses panel data to study the free distribution of food (direct support without work) and food-for-work programs and shows both the programs have a direct positive effect on child weight for height.

Hoddinott et al. (2001) study child growth in times of drought in rural Zimbabwe, showing that among those 12 to 24 months in age, child's height declined by around 1.5 to 2 cm. The children impacted by drought as compared to the counterfactual group of children in the same age range not impacted by drought, displayed difference of even four years after the initial shock. Woldehanna (2009), in his paper using Young Lives data, shows that 8-year-old children also suffered from a deficiency in nutrition after individual household factors such as divorce and job loss. Young Lives data gives information on shocks experienced by the household at disaggregated levels such as household-level theft of cash, crops, livestock, housing, loss of a job, death of livestock, a shock to income source, family shock such as divorce, death or illness or severe injury of any family member in the household. In this study, I aggregate shocks in three types: natural shock, economic shock, and idiosyncratic (family) shock. I investigate the impact on child growth and welfare, to determine if PSNP provides cushion to such shocks offsetting the negative effects of shock[16]. A close paper to this approach is that by Yamano et al. (2005), in his paper used a nationally representative survey of the

[16] Natural shock consists of natural disasters such as drought, flooding. Economic shock consists of shock that affects a household's economic conditions such as job loss. Family shock consists of illness or death of a family member.

time period 1995-96 to study the responsiveness of food aids on child stunting in Ethiopia. He argues that food aid appears to significantly offset the negative impact of shocks on child growth for children in the age group 6-24 months[17].

Duflo (2000) focuses on child welfare through investment in human capital through health and greater access to education due to social protection programs such as conditional cash transfers. The economic theory implies two types of effects of the safety net program: income effect and substitution effect. Income effect results from transfers received by household and substitution effect results from additional labor demand caused by public work programs. An increase in income could net an increase in the level of school enrollment if a child's schooling is considered as a normal good (Behrman & Knowles, 1999). However, if investments in children are reflective of luxuries, then an increase in income above a threshold may decrease child labor (Basu and Van, 1998). Public work programs are, however, different from social protection since it leads to substitution effects as demand for labor increases under public work. It is difficult to say which substitution effect dominates or income effects[18]. The magnitude of income and substitution effect depends on if a child is eligible for labor work, returns on his/her labor time, returns on schooling, and child's productivity in household chores. Additionally, another possibility is that the child increases time allocation in both schooling and child labor by reducing leisure time. Hence, public work's impact on a child's welfare is ambiguous. However, the theory gives a framework to study the possible impacts of public work. PSNP consists of both public work as well as direct support (in case of no adult available to work) to the household beneficiary, which makes the effect more ambiguous. This paper helps to provide empirical evidence to determine which effect is greater in the case when both types of programs are involved.

[17] Yamano et. al. (2005) note that estimates are not precise for age 24 to 60 months old children and also he suggests that according to a child's age, the results may be different.

[18] An increase in labor demand can alter a child's welfare depending on his/her opportunity cost of time allocation between schooling and child labor

3. Background on the PSNP

"It doesn't matter if it is raining here if it is raining in Canada." ~ a popular saying in Ethiopia (Devereux S., 2000).

Ranked 97th in the Global Hunger index in 2019, Ethiopia has been dependent on a consortium of donors from outside in support of food aid for decades*19*. Ethiopia infamously suffered a dramatic food shortage and famine in the mid-1980s resulting in the estimated death of over one million people. Since the mid-1990's, around 15 million people in rural Ethiopia have been affected by the food crisis in a country where livelihood is strongly dependent on the weather (World Bank, 2009). In response to chronic food insecurity Ethiopia received food aid from donors for more than 30 years. From 1997-2002 it cost an average of $265 million per year.

During the last 30-40 years, food aid became an institutionalized response to chronic food insecurity, instead of a response to transitory food security such as has occurred during droughts. However, previous food aid programs have failed to protect livelihoods and failed to lift households from poverty (Clay et al., 1999; Devereux, 2000; World Bank, 2009). Also, though Ethiopia received substantial amounts of food aid under previous emergency food programs, deliveries of food were often late, sometimes with reduced rations, and covered limited food insecure communities and households (DPPC 2000:10). As a result, the aid led to saving lives, but did not help alleviate poverty in the country, leaving the population vulnerable to subsequent shocks (World Bank, 2013). The Ethiopian government realized the need for a more comprehensive safety net program in early 2000 and in 2005 launched a new system, called Productive Safety Net Program (PSNP), that provides food and cash via public work or direct support to chronically food-insecure households. PSNP is an international flagship program with the help of donors designed to provide a productive safety net to

[19] "Latest Global Hunger Index Results". *Global Hunger Index*. Retrieved June 2019.

the poor and vulnerable populations as well as aid to food insecure households. The program was assisted by the World Bank and supported by Canadian International Development Agency (CIDA), British Department for International Development (DFID), the World Food Program (WFP), the United States Agency for International Development (USAID) and the Government of Ireland.

The stated objective of the PSNP is to provide food and cash transfers to food insecure populations in woredas (districts), prevent the drawdown of household assets, and through public works, create productive and sustainable assets for communities. These assets should contribute to severely degraded areas by increasing household productivity to allow food insecure households to resist shocks when resources for income are insufficient (Ethiopia, 2004). PSNP provides food and Works (PW) for households that have adults who can participate in labor-intensive work and Direct Support (DS) for households who cannot provide labor. In 2009, the public work wage rate was 10 birr per day (approximately US $0.34) for labor-intensive work and 3 kg of cereal food transfer. These transfers of food, cash, or food and cash are provided during the lean season of agriculture, which occurs between January to June, which is a relatively dry agricultural season in Ethiopia. The amount of transfer of food or cash is the same for PW and DS beneficiaries (World bank, 2005-09).

According to the World Bank's support to the program, the program has been implemented in phases. The first phase (2005-06) was intended for the transition of previous food relief programs to a productive safety net program where around 192 woredas were covered with a budget of US$203 million with a wage rate provided at 6 Birr/ day (approximately US $0.21). The second phase (2007-2009) covered around 234 woredas. By late 2009, the final phase, around 290 woredas were covered with around 7.57 million PSNP beneficiaries under a budget of US$374.6 million, and the wage rate provided was 10 Birr/ Day[20]. Since cash wage was supposed to cover the household purchases of food from the local food market, by design, the cash wage under PSNP was below the unskilled labor wages

[20] The additional woredas are the ones split from previous woredas overtime.

(Subbarao et al., 2013). The current wage rate under the public work of PSNP is, on average ETB23/day ($.77/day).

The PSNP beneficiaries are not self-selected but are targeted at the administrative and geographic levels. The woredas (districts) were targeted based on their historical food aid receipts before 2005. To do so, the Ethiopian government followed the list of woredas under the emergency response system of food aid in effect prior to 2005. Eligible households within woredas are selected by local officials decide which wards (kebeles) receive the program with priority given to kebeles, which have a higher number of eligible households. This process takes place annually, and, households are expected to be in the program for five years after being selected. Targeted beneficiaries are entitled but not forced to take up the program.

4. Data

The data analyzed come from the Young Lives Study. Young Lives is a multi-country panel 2016 that covers the years 2002, 2006, 2009, 2013, and 2016. Young Lives data is aimed at collecting outcomes for children born in poor households. Therefore, the data over samples children born in 2016 that covers the years 2002, 2006, 2009, 2013, and 2016. Young Lives data is aimed at collecting outcomes for children born in poor households. Therefore, the data over samples children born in poverty though the sample captures both who are in poor households as well as children who are out of poverty. As a result, the raw data is not perfectly representative of the full population but instead of the country's more vulnerable populations. Yet, sampling weights can be used to obtain national estimates.

The economic shocks studied in this analysis include death of livestock, loss of a job or other source of income, a large increase in input prices, a large decrease in output prices any dispute about an asset with a neighbor. Naturals shocks consist of drought, flooding, erosion, or crop failure.

Idiosyncratic shocks consist of death or illness of any parent or any member of household, divorce or separation, the birth of a new household member.

Table 1 provides summary statistics for the households for panel data of the five rounds from the year 2002 to 2016 used in the analysis. The sample size ranges from 10,000 to 14,000 based on the variable of interest for both non-PSNP and PSNP households. Column (1) of Table 2 provides summary statistics for the overall sample of individuals for entire panel su0vey data. Each child represents a one household. Just under half of the sample is of female children. Individuals are on average 10-year-old during the survey period. The average child height for PSNP participant households is 140.7 cm and for children of non-PSNP household is 126.1 cm. Hours spent on household chores for children in PSNP households are slightly higher than the hours spent by children in non-PSNP households. Column (2) and (3) of Table 1 provides summary for individuals and their characteristics according to their exposure to PSNP and the p-values from tests of equality of means listed in column (4) suggest that there are many statistically significant differences between these groups. Column (4) in Table 1 tests for differences in group means of the variables with non-PSNP and PSNP households. Table 1 shows that the non-PSNP and PSNP households are significantly different from each other in terms of parents age and education, wealth index and area of household, urban/rural area, since PSNP was predominantly rural safety net program and targets households that had lower economic standards with chronically and transitorily food insecurity. The PSNP beneficiary household reports around 11% higher school enrollment rate of children as compared to non-PSNP household children. The following rows in the same column reveal children from PSNP beneficiary households had older parents, lower education levels of parents, larger household size, lower wealth index, and larger share of PSNP beneficiaries came from the rural area. It shows that the PSNP beneficiary households are different from non-PSNP households. However, the differences do not establish a causal effect, so the fixed-effect strategy is important to study the causal effects.

Young Lives data contains questions regarding any shock experienced by households since the last survey round (around 3-4 years) that negatively affected their economic condition. For my analysis, I categorize the list of shock from the survey into three categories, economic shock, natural shock, and family shock. Economic shock comprises of loss of job or source of income or family enterprise and death of livestock, an increase in input prices, and a decrease in food availability. Natural shock includes shock due to crop failure and natural disasters such as drought, flooding, erosion, frost. Family shock consists of illness or death of any household member, divorce or separation, and birth of new household members.

5. Analysis

5.1 Empirical Strategy with Fixed Effect Panel Model

Selection is a key concern for estimating the impact of the program. PSNP beneficiary households are poorer and are concentrated in the rural region than non-participating households. The number of households ever get PSNP per round for the full Young Lives data is as follows:

Round	**Year**	**PSNP Beneficiary household**		**Total**
		No	**Yes**	
1	2002	2,999	0	2,999
2	2006	2,892	0	2,892
3	2009	2,153	742	2,895
4	2013	2,301	483	2,784
5	2016	2,362	309	2,671

I first employ a fixed effects model to compare variations within the household over time to see whether PSNP participation buffers the effect of shocks, using the entire Young Lives sample. This baseline empirical strategy uses the variation within households over the 15 years. I use a household-level panel data model that accounts for time-invariant individual heterogeneity, meaning identification is within household as a function of exposure to shock and PSNP take-up. Formally, the regression specification takes the following form:

$$Y_{i,t} = \alpha + \beta_1(Shock)_{i,t} + \beta_2(PSNP)_{i,t} + \beta_3(Shock * PSNP)_{i,t} + X_{i,t}\Gamma + \delta_i + \varepsilon_{it} \qquad (1)$$

$Y_{i,t}$ is child height, hours spent on household chores, or enrollment in school. Model variation coming from changes in the outcomes within the household over time is a function of variation in exposure to any shock, the PSNP safety net program, and the interaction between the two. The primary coefficient of interest here is β_3 that captures the average effect of shock and captures the impact of PSNP treatment when the household experiences any shock. δ_i is the individual level fixed effect. $X_{i,t}$ are the control variables demographics of children, household, and wealth index.

Table 2 shows the results for estimating equation (1) for round 1 to 5 in the Young Lives panel data. Column (1), (3), and (5) show a negative impact of economic, family, and natural shock on child's height by 0.007, 0.049 and 0.033 standard deviation, respectively. However, being a PSNP beneficiary does not help mitigate any shock's impact on height. This suggests even when the impact survey of 2008 showed an increase in calorie intake for PSNP household after shock (IFPRI/CSA, 2009b; Devereux S et al., 2008), children are not benefitting from it. But also height is a cumulative measure of health so slow to change. Children's time allocation in household chores is also seen to be increased because of economic (0.08 hours) and natural shocks (0.15 hours). Though it is expected that since public work compels adults to allocate their time in labor-intensive work, a child's time allocation in household chores might increase, however, PSNP is insignificant in changing their time allocation after shock. An important result from the fixed effect model is observed from column (6) that children's enrollment in the school declined by 12.8%, but PSNP household observed cushion for the family shock, causing an increase in school enrollment by 14.7%. This implies PSNP may have buffer negative impacts on child schooling.

As the PSNP beneficiary households are different than non-PSNP beneficiary households, it cannot be compared to each other. It is important to compare the PSNP beneficiary before and after the policy. Though the fixed-effect model takes care of any heterogeneity bias, however, there is a concern of endogeneity due to the intensity of shocks exposure, that only the households which are exposed to more intensity of shock are opting for PSNP. To account for this, I define a counterfactual group method.

5.2 Counterfactuals: Round 1 Older Cohort and Round 3 Younger Cohort

In counterfactual approach, I compared children OC age 8-12 who did not get PSNP and eventually get PSNP to YC age 8-12 who already got PSNP. The Young Lives data consists of 15 years comprising of 3000 children. Among those, 2000 children from the younger cohorts are aged between 6 and 18 months while the 1000 older cohort children are aged between 7.5 to 8.5 years as of 2002. This data structure makes it possible to compare outcomes for the same age group children using older children from round 1 (year 2002) and younger children from round 3 (year 2009). Since PSNP was implemented in 2005, I created a counterfactual group of individuals before PSNP existed, and the same-aged group after PSNP takes effect. The following model compares children of age group 7 to 8 years old from round 1 with the same age group children from round 3. The regression specifications for this model takes the following form:

$$Y_{i,t} = \alpha + \beta_1 (PSNP)_{i,t} + \beta_2 (Shock)_{i,t} + \beta_3 (PSNP * Shock)_{i,t} + X_{i,t}\Gamma + \varepsilon_{it} \qquad (2)$$

By comparing the same-age children from households before and after the policy, the counterfactual decreases the likelihood of endogeneity concerns that arises if only the households affected by major shocks became PSNP beneficiaries. The tradeoff is that it drastically reduces the sample size for data to be used for the model.

Figure 1(a) shows the frequency of shocks household experienced for full Young Lives data. For the full sample, economic shocks are most frequent (47%), while natural and family shocks affect around 41% of the households. I also categorize shocks into aggregate and idiosyncratic shocks. Aggregate shock contains information about whether the household suffered from drought, rain or flooding, erosion, hailstorms, crop failure, a large decrease in output prices, or large increase input prices. Nearly half of the population suffered from aggregate shock. Idiosyncratic shocks are the ones that when experienced by one household, is typically unrelated to neighboring households, and it consist of crime, theft of crop, death of a family member, loss of job or source of income, disputes with extended family or neighbors regarding land or asset. Idiosyncratic shocks are the most frequent (57.4%) household-level shock.

Figure 1(a). Incidence of shocks (percent of households) for full Young Lives data

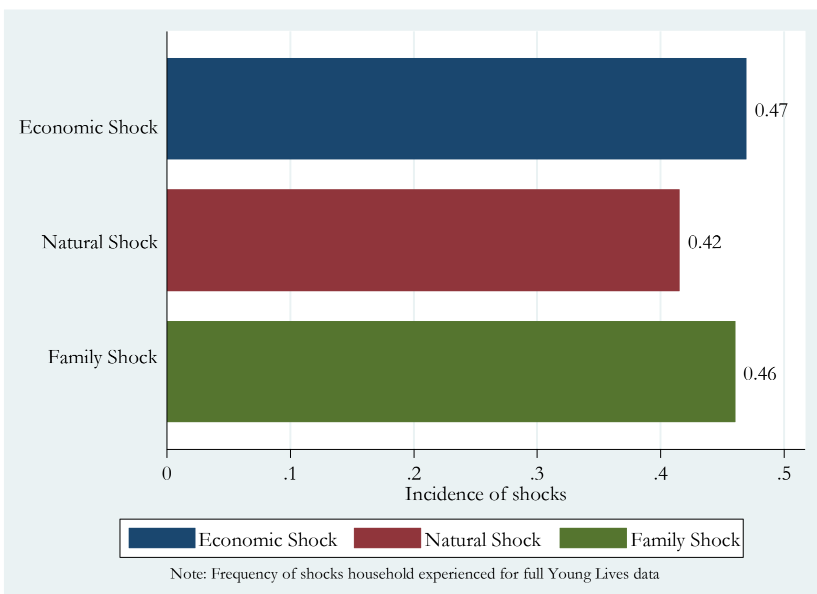


Figure 1(b) shows that in the sample for the counterfactual model, most of the households are affected by at least one economic (77%) and natural (76%) shock with half of the household affected by a family shock.

Figure 1(b). Incidence of shocks (percent of households) for counterfactual sample data

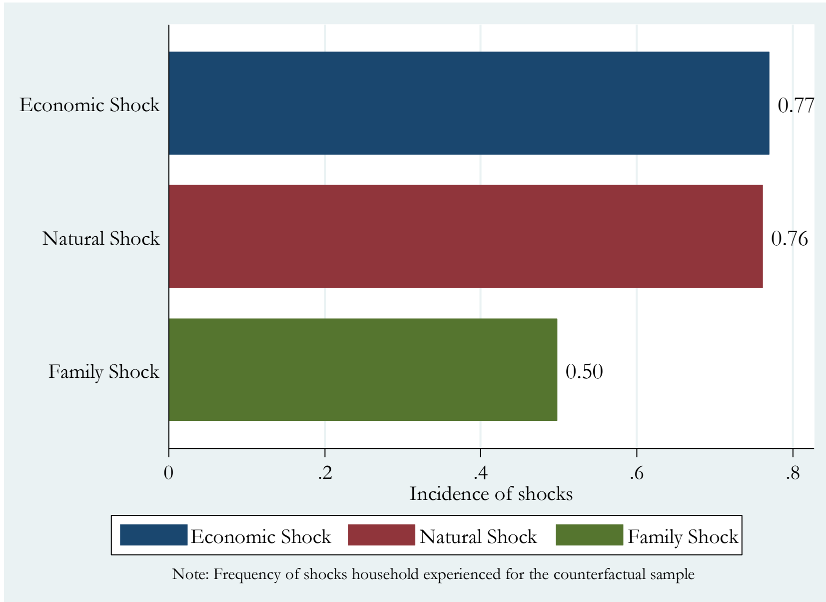


Table 4 presents the result of the counterfactual model equation (2). Though fixed effects show a negative impact of shocks on child's height, results of the counterfactual model in column (1), (4), and (7) of Table 4 shows the shocks are insignificant in affecting child's height. However, this could because it is difficult to observe an effect on a child's height in the short run. Column (3) shows that poor households are less likely to be enrolled in school. And it also suggests an economic shock to the household decreases children's enrollment in school by 4.5% and but being a participant in PSNP household increases school enrollment of children by 8.6%. Columns (1), (3), and (5) show the same results on children's height, suggesting PSNP improve child height even when households are exposed to any shock. Similar to previous results, it can be seen that hours spend on household chores increased for households exposed to any shock, and PSNP enables them to reduce the impact on the time allocation on work.

In summary, table 4 shows economic, family, and natural shocks negatively impact a child's height and increase their time allocation on household/domestic chores. Family shocks have a negative impact on school enrollment. PSNP helps buffer this negative impact of the family shock on enrollment in school for children.

6. Conclusion

Motivated by the idea that the food insecure households trigger different type of coping mechanism during different types of shock in comparison to other relative shocks, this paper investigates in role of Ethiopia's safety net program, PSNP, in mitigating these shocks for child welfare. The PSNP is one of the largest national social safety net programs in a developing country that targets the chronically food insecure households. The program aimed at alleviating poverty, assure food consumption, and prevent asset depletion through labor-intensive public work. This paper provides evidence on impact of PSNP in providing cushion for negative effects of shock.

PSNP is the program that targets household and not a specific household member. However, I find the spillovers to education of children are potential. The fixed-effect model shows that PSNP buffers the negative impact of an idiosyncratic family shock. However, when accounting for the endogeneity of selection bias of opting for the program depending on the severity of the shock, the counterfactual model result shows the negative impact of the economic shock on school enrollment is offset by PSNP. It follows that when households experience shock, PSNP household shows a positive effect on enrollment of children in school as opposed to a household that does not have exposure to PSNP at the time.

Additionally, though the counterfactual model is only taking into account short-run effects on height, the fixed effects model shows, in the long run, a child's height is negatively impacted by natural and family shock, however, PSNP does not help buffer the shock effects on height, and this might call for a need of transfer of more nutritious food under PSNP. Also, it can be concluded from these results that as adults from households allocate their time in work outside the household under the public work of PSNP, children substitute their time allocation on household chores by enrolling in school.

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Table 1: Summary Statistics for full Young Lives data

Variable	Overall (1) Mean (S.D.)	NON-PSNP (2) Mean (S.D.)	PSNP (3) Mean (S.D.)	Difference (4) Est. (S.E.)
Main outcome variable				
Height (cm)	127.683	126.078	140.737	-14.659***
	(30.732)	(31.578)	(18.006)	(0.829)
Domestic chores (Hours/Day)	1.882	1.856	2.047	-0.191***
	(1.670)	(1.672)	(1.650)	(0.046)
School enrollment indicator	0.762	0.748	0.860	-0.112***
	(0.426)	(0.434)	(0.347)	(0.012)
Explanatory variable				
Child's age	10.067	9.793	12.324	-2.531***
	(5.704)	(5.819)	(3.992)	(0.153)
Male indicator	0.524	0.523	0.525	-0.002
	(0.499)	(0.499)	(0.500)	(0.014)
Rural indicator	0.670	0.641	0.904	-0.262***
	(0.470)	(0.480)	(0.295)	(0.013)
Mother's age	36.498	36.165	39.256	-3.092***
	(8.548)	(8.610)	(7.471)	(0.230)
Mother's level of education	2.995	3.179	1.512	1.667***
	(4.057)	(4.148)	(2.825)	(0.110)
Mother disabled indicator	0.031	0.031	0.031	-0.000
	(0.172)	(0.172)	(0.172)	(0.005)
Father's age	45.596	45.264	48.346	-3.082***
	(9.852)	(9.918)	(8.823)	(0.265)
Father's level of education	4.632	4.846	2.777	2.070***
	(4.607)	(4.660)	(3.624)	(0.140)
Father disabled indicator	0.036	0.036	0.038	-0.003
	(0.187)	(0.186)	(0.192)	(0.005)
Household size	5.917	5.906	6.014	-0.109
	(2.118)	(2.141)	(1.911)	(0.057)
Wealth Index	0.320	0.325	0.280	0.044***
	(0.184)	(0.189)	(0.123)	(0.005)

Note: Summary statistics of the full sample spanning 2002, 2006, 2009, 2013, 2016. Standard deviation reported in parenthesis for columns 1 to 3. Column 4 shows standard errors for the difference in PSNP beneficiary and non-beneficiary households in the bracket.

Source: Young Lives data

Table 2: PSNP and shock effects using Fixed Effect Panel

	Economic shock			Family Shock			Natural Shock		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Height	Hours on Domestic Chore	Enrolled in School	Height	Hours on Domestic Chore	Enrolled in School	Height	Hours on Domestic Chore	Enrolled in School
Shock	-0.017	0.080**	0.037***	-0.049***	-0.148***	-0.128***	-0.033*	0.157***	0.000
	(0.015)	(0.038)	(0.012)	(0.014)	(0.040)	(0.011)	(0.018)	(0.045)	(0.014)
PSNP beneficiary HH	0.046	0.202**	0.206***	0.017	0.046	0.105***	-0.005	0.266***	0.196***
	(0.033)	(0.079)	(0.024)	(0.029)	(0.074)	(0.021)	(0.037)	(0.089)	(0.027)
Shock * PSNP	-0.051	-0.004	0.005	-0.001	0.143	0.147***	0.032	-0.097	0.028
	(0.041)	(0.094)	(0.029)	(0.041)	(0.096)	(0.027)	(0.044)	(0.101)	(0.031)
Child’s demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent's demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wealth Index	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	13,711	11,185	10,782	13,711	10,207	9,806	13,711	11,185	10,782
Mean (std. dev.) of dependent variable	127.683 (30.732)	1.882 (1.670)	0.762 (0.426)	127.683 (30.732)	1.882 (1.670)	0.762 (0.426)	127.683 (30.732)	1.882 (1.670)	0.762 (0.426)
R-squared	0.656	0.445	0.266	0.656	0.467	0.338	0.655	0.446	0.265

Note: Fixed-effect model with the full sample. Child’s demographic contains the child’s age, the parent’s demographics contain mother and father’s age, education, and information if the mother or father is disabled. The household characteristic has the size of the household, and the wealth index is from the data and has three components: housing quality, consumer durables, services. Includes a constant.
Source: Young Lives data
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Summary Statistics for counterfactual sample

Variable	Overall (1) Mean (S.D.)	NON-PSNP (2) Mean (S.D.)	PSNP (3) Mean (S.D.)	Difference (4) Est. (S.E.)
Main outcome variable				
Trimmed height	118.356	116.342	119.330	-2.988***
	(6.276)	(6.981)	(5.661)	(0.486)
Hours spent on domestic chores on a day	1.985	3.276	1.664	1.612***
	(1.901)	(2.558)	(1.542)	(0.180)
School Enrollment	0.972	0.970	0.973	-0.003
	(0.165)	(0.171)	(0.163)	(0.016)
Explanatory variable				
Child's age	7.555	7.355	7.656	-0.301***
	(0.521)	(0.479)	(0.513)	(0.039)
Male	0.535	0.536	0.534	0.002
	(0.499)	(0.500)	(0.499)	(0.039)
Rural	0.915	0.919	0.913	0.006
	(0.279)	(0.273)	(0.282)	(0.022)
Mother's age	34.986	34.515	35.222	-0.707
	(7.013)	(7.806)	(6.575)	(0.546)
Mother's level of education	1.022	0.809	1.125	-0.316
	(2.115)	(2.153)	(2.090)	(0.168)
Mother disabled	0.059	0.129	0.024	0.105***
	(0.236)	(0.336)	(0.154)	(0.018)
Father's age	44.468	43.615	44.896	-1.282
	(8.826)	(8.437)	(8.994)	(0.686)
Father's level of education	1.867	1.531	1.998	-0.467
	(2.981)	(3.176)	(2.895)	(0.274)
Father disabled	0.031	0.040	0.026	0.014
	(0.173)	(0.197)	(0.160)	(0.013)
Household size	6.398	6.347	6.423	-0.076
	(1.938)	(2.143)	(1.828)	(0.151)
Wealth Index	0.215	0.142	0.252	-0.110***
	(0.123)	(0.097)	(0.118)	(0.009)

Note: Summary statistics of data for the years 2002 and 2006 of older and younger cohorts respectively, used for the counterfactual model. Standard deviation reported in parenthesis for columns 1 to 3. Column 4 shows standard errors for the difference in PSNP beneficiary and non-beneficiary households in the bracket. The data ranges from 420 to 560 observations depending on missing data for dependent variables.

Source: Young Lives data

Table 4: PSNP and shock effects using Perfect Counterfactual

	Economic shock			Family Shock			Natural Shock		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Height	Hours on Domestic Chore	Enrolled in School	Height	Hours on Domestic Chore	Enrolled in School	Height	Hours on Domestic Chore	Enroll-- ed in School
Shock	-0.171	0.887	-0.045*	-0.134	0.419	0.033	-0.039	0.897	0.022
	(0.226)	(0.514)	(0.024)	(0.133)	(0.541)	(0.027)	(0.282)	(0.578)	(0.042)
PSNP beneficiary HH	-0.214	-1.252*	-0.066*	-0.060	-1.713**	0.013	-0.124	-1.156	0.009
	(0.283)	(0.682)	(0.033)	(0.152)	(0.598)	(0.029)	(0.242)	(0.797)	(0.046)
Shock * PSNP	0.197	-0.806	0.086***	0.036	-0.436	-0.038	0.084	-0.921	-0.015
	(0.269)	(0.526)	(0.028)	(0.205)	(0.548)	(0.025)	(0.279)	(0.627)	(0.052)
Child's demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent's demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wealth Index	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	562	495	420	561	495	420	562	495	420
Mean (std. dev.) of dependent variable	118.3 (6.27)	1.99 (1.90)	0.972 (0.165)	118.3 (6.27)	1.99 (1.90)	0.972 (0.165)	118.3 (6.27)	1.99 (1.90)	0.972 (0.165)
R-squared	0.021	0.203	0.018	0.024	0.199	0.010	0.020	0.203	0.009

Note: The counterfactual model with data for the years 2002 and 2006 of older and younger cohort respectively. Clustered errors at the village (kebele) level. Child's demographic contains the child's age and gender; parent's demographics contain mother and father's age, education, and information if the mother or father is disabled. The household characteristic has the size of the household, whether the household belongs to a rural region. The wealth index is from the data and has three components: housing quality, consumer durables, services. Includes a constant.
Source: Young Lives data
*** p<0.01, ** p<0.05, * p<0.1

Chapter 3
No Detention Policy in Schools and Education Quality

1. Introduction

In August 2009, India's Parliament enacted the Right to Education (RTE) Act which incorporated free and compulsory education in elementary schools. This law guarantees that each child should not be charged any expenses that might prevent them from attending elementary school. It obligated the central and state government to ensure admission and attendance of all children between the ages of 6 and 14 as a fundamental human right. With the passage of this act, India joined over 130 countries that guarantee free and compulsory education to children (The Hindu, 2016). One of the significant parts of the RTE is the No Detention Policy, which stipulates that no student until completion of elementary school (i.e., grade 8) will be held back, failed, or expelled from promotion to the next class. The Right to Education Act came into full effect in April of 2010.

The Ministry of Human Resource Development of India enacted the No Detention Policy intending to increase students' retention enrolled in schools by reducing the stress caused by exams needed to pass to be promoted to the next grade. Although there has been an increase in the retention rate for lower and upper primary classes after the policy, once past grade 8 students still face challenges as there is no safety net (Sabharwal, 2018). Using District Information System for Education (DISE) data Manisha Shah (2019) shows that after the RTE Act, the retention has increased till grade 8 however, there was an increase in the dropout rate in 9th and 10th grade. The finding raises concerns about the quality of education as students are not prepared to pass the examinations after automatically being promoted till 8th grade. The author also shows that the lower end of the distribution has manifested a rise in dropout rates in secondary school. A likely explanation for this could be that the quality of education lowered as there is less motivation for students to study in school through grade 8, affecting their quality of primary education. Similarly, low-performing students are less likely to leave during elementary school, making disproportionate exits later. Manisha Shah (2019) shows a dramatic decline in verbal and math test scores after 2010.

This paper examines the change in test scores after policy for the students at the low and high end of the test score distribution. I find that the score lowers for all the students in the tail of the distribution.

Assessing the distributional effects is vital to improving the policy so the quality of education would not deteriorate for any student. If quality is declining, it might suggest a need for better policy design to lower stress for students or more training programs for the teachers to keep students motivated.

Since, like most education policies, the No Detention Policy was implemented at the national level at one point in time, the causal inference becomes challenging due to a lack of a good counterfactual group. Using longitudinal Young Lives data pre and post-policy implementation, I analyze the movement in the test score distribution for the students exposed to the program and those not exposed to the program from previous unimpacted survey years.

2. Data and summary statistics

The Young Lives Panel Survey data of India consists of interviews of 3,000 children of two cohorts and their families. The survey is conducted every three years since 2002, consisting of child, household, and community level questionnaires. The data sample in this study consists of 3,000 observations, divided into the younger and older cohort. The older cohort consists of around 1,000 children born between 1994-95 and 1996-97, and the younger cohort consists of about 2,000 children born between 2001-2002 and 2004-05. Both the cohorts are surveyed from 2002 to 2016 every three to four years.

For this study, I use household-level data for the years 2009 (Round 3) and 2013 (Round 4) and examine the changes in test scores before and after the exposure to the policy for younger cohort children. I later compare test scores differences with old cohort children from 2006 (Round 2) and 2009 (Round 3) as a placebo.

The Young Lives data for India covers the Andhra Pradesh state. It is sampled at 20 clusters (Mandals) and uses a pro-poor sample with nearly an equal number of boys and girls in urban and rural communities. In terms of child education and literacy, Andhra Pradesh ranks low compared to the national average and has the highest number of child labor in India (Reddy et. al., 2003).

Table 1 shows the summary statistics for the raw scores for math and verbal tests for the young cohort in 2009 (Round 3) and then 2013 (Round 4), respectively, for the same individuals. Children in age group 7 and 8 of the young cohort show a slight increase in the math score in 2013, and children of 9 years old age group show a decline in mean math test score. I make the test scores comparable across different years, standardizing scores by age to have mean one and variance zero using the z-score normalization method for both math and verbal test scores.

Table (2) shows the summary statistics for the differences between standardized scores from Round 3 (R3) to Round 4 (R4), suggesting at mean the math score declined and the verbal test score slightly improved on average. Nevertheless, the story may be different for students from the higher and lower proportion of the distribution. The expectation is that policy might increase their scores for students from the lower percentile as it eliminates the exam stress to get promoted to the next grade. In contrast, for children in higher-level distribution, the score might go down as there is not enough motivation to invest more time in study. To study this, I created a dummy variable for each child for which decile of the test scores they belong to in the previous year (2009).

Figures 1 and 2 show the distributions of differences in standardized test scores of the year and 2013 for both verbal and math test scores, respectively. We see the shift in the distribution to the right. Both the Figure 1 and 2 shows that after the policy implementation there are fewer high performers and more low performers.

3. Analysis

To investigate the quality of education, I regress changes in the young cohort's test score from 2009 to 2013, which is before and after policy implementation on different test score deciles of students from 2009. The coefficients will give us the change in test scores compared to the average performing students (50th percentile). The second regression regress the difference in test scores for the old cohort from the year 2006 and 2009, which is before the policy implementation and is a placebo group for the analysis. In the third regression, I create a dummy variable for the young cohort and add an interaction term to use the old cohort's students from 2006 to 2009 as a comparison group.

For young cohort pre and post-policy:

$$\Delta Test\ Score_{i(2013-2009)} = \alpha + \beta_1 Decile_{i,10} + \beta_2 Decile_{i,20} + \beta_3 Decile_{i,30} + \beta_4 Decile_{i,40} + \beta_5 Decile_{i,60} + \beta_6 Decile_{i,70} + \beta_7 Decile_{i,80} + \beta_8 Decile_{i,90} + \beta_9 Decile_{i,100} + \varepsilon_{it} \tag{1}$$

For old cohort before the policy (Placebo):

$$\Delta Test\ Score_{i(2009-2006)} = \alpha + \beta_1 Decile_{i,10} + \beta_2 Decile_{i,20} + \beta_3 Decile_{i,30} + \beta_4 Decile_{i,40} + \beta_5 Decile_{i,60} + \beta_6 Decile_{i,70} + \beta_7 Decile_{i,80} + \beta_8 Decile_{i,90} + \beta_9 Decile_{i,100} + \varepsilon_{it} \tag{2}$$

Interaction between young and old cohort specified above:

$$\Delta Test\ Score_{i(2013-2009)} = \alpha + \beta_1\left(YC_i * Decile_{i,10}\right) + \cdots + \beta_4\left(YC_i * Decile_{i,40}\right) + \beta_5\left(YC_i * Decile_{i,60}\right) + \cdots + \beta_9\left(YC_i * Decile_{i,100}\right) + YC_i + \beta_1 Decile_{i,10} + \cdots + \beta_4 Decile_{i,40} + \beta_5 Decile_{i,60} + \cdots + \beta_8 Decile_{i,90} + \beta_9 Decile_{i,100} + \varepsilon_{it} \tag{3}$$

Where $\Delta Test\ Score_{i(2013-2009)}$ is the difference between standardized scores for both math and verbal test; $Decile_{i,j}$ are indicator variables for individual's deciles for the test scores in round 3, j=10-40, 60-100. The regression model is akin to having individual-level fixed effects since it is a first difference regression.

4. Regression Results

Table (3) shows the regression results for the difference in verbal test scores. From Column (1) of Table (3), we observe the difference in test scores before and after policy. The verbal test score declines for students from the last four deciles as compared to average-performing students. The low-performing children show improvement in verbal test scores as compared to students performing at average. The decline in test scores for the children in the higher level of test score distribution (70^{th}-100^{th} decile) suggests that the motivation to study may affect these children. Column (2) shows the decline in test score distribution as compared to average-performing students. However, this decline is lower than that of before and after policy. Column (3) shows when compared with the placebo group, the verbal test scores declined for both high and low-performing students.

In particular, column (1) of the table (3) shows that after the policy implementation, children from the lower decile shows, on average, a 0.35 standard deviation increase in verbal test score as compared to average-performing (50th decile) students. And students who belong to the highest decile show a 1.28 standard deviation decline in test scores compared to average students due to lack of motivation. However, column (3) shows the result for regression equation (3). Compared to the placebo group, both lower and higher distribution students who are exposed to the policy show a decline in test scores.

Table (4) shows the regression results for the difference in math test scores. Column (1) suggests that after the policy implementation, students from the lowest decile of test score distribution on average have a 0.40 standard deviation increase in math test score as compared to average (50th decile) students. And students who belong to the highest decile show a 1.02 decline in test scores compared to average students due to lack of motivation. However, column (3) suggests that compared to the placebo group from lower and higher distribution shows a decline in test scores compared to the old cohort who were not covered by the policy. And this decline in test scores is more for students from higher score distribution.

5. Conclusion

This paper demonstrates validity for Manisha Shah's finding of test score decline after 2010 and extended to studying the student's scores in the end distribution that is for both highest performer and lowest performer. The analysis contributes to the literature on the effect of the Right to Education Act, 2009. The study results suggest that the score declines for students from both the highest and lowest performers from the distribution.

References

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Shah, Manisha, and Bryce Steinberg. "The right to education act: Trends in enrollment, test scores, and school quality." AEA Papers and Proceedings. Vol. 109. 2019.

Figure 1: Distribution of standardized math score for the years 2009, 2013 of the young cohort (before and after policy)

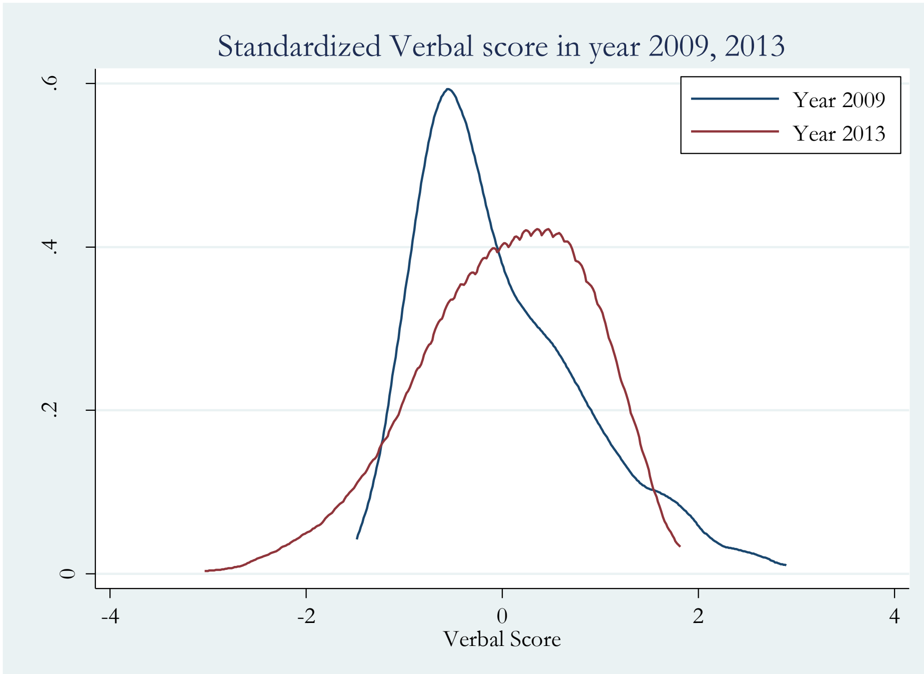

Figure 2: Distribution of standardized math score for the years 2009, 2013 of the young cohort (before and after policy)

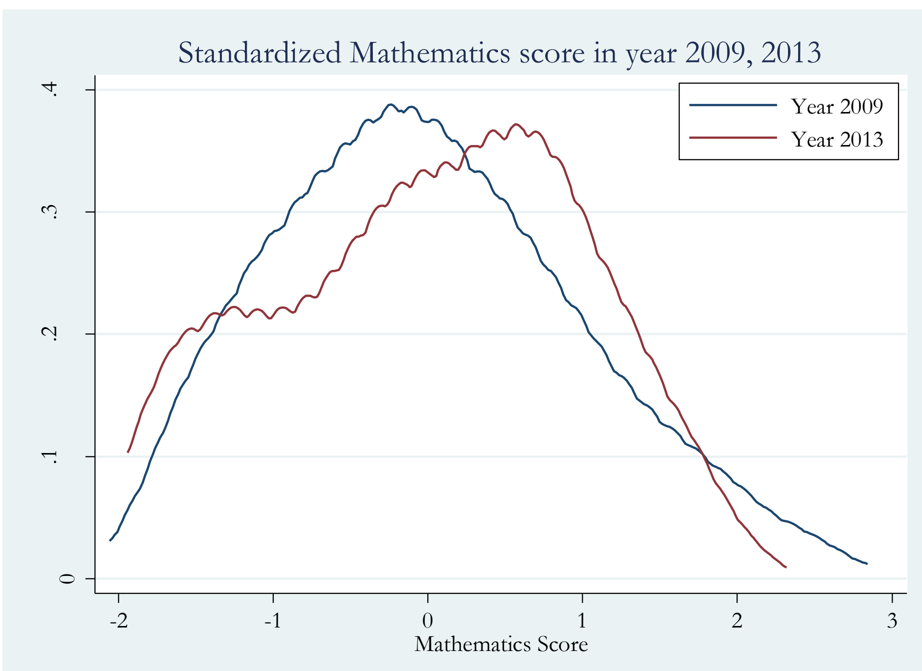

Table 1: Young cohort 2009, 2013 Summary statistics

Panel A: Young cohort 2009

	Math 2009		Local Language 2009		English 2009	
Age 2009	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
7	11.48	6.16	60.47	32.02	4.96	3.26
8	12.13	6.47	57.97	30.07	5.51	3.39
9	14.11	6.37	78.89	33.63	7.11	3.59

Panel B: Young cohort 2013

	Math 2013		Local Language 2013		English 2013	
Age 2013	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
11	12.98	6.69	13.44	4.52	14.06	4.42
12	12.72	6.59	13.37	4.48	13.51	4.38
13	11.50	5.37	14.00	3.59	13.13	4.22

Table 2a: Summary statistics for differences in standardized test scores (R4 - R3) YC

	Mean	S.D.	N
Difference in verbal score	0.00950	0.892	1,792
Difference in Math score	-0.0233	0.909	1,847

Table 2b: Summary statistics for differences in standardized test scores (R3 – R2) OC

	Mean	S.D.	N
Difference in verbal score	-.0235453	0.869	836
Difference in Math score	-.099614	0 .917	911

Table 3: Estimates of changes in verbal score

Difference in verbal score	(1) **For Young Cohort (year 2009-2013)**	(2) **For Old Cohort (year 2006-2009)**	(3) **For interaction YC*Decile**
Decile10 verbal	0.35***	1.24***	-0.89***
	(0.09)	(0.12)	(0.15)
Decile20 verbal	0.12	0.53***	-0.41***
	(0.08)	(0.13)	(0.15)
Decile30 verbal	0.13*	0.10	0.04
	(0.08)	(0.11)	(0.14)
Decile40 verbal	-0.03	-0.11	0.08
	(0.08)	(0.11)	(0.14)
Decile60 verbal	0.01	-0.19*	0.20
	(0.08)	(0.11)	(0.14)
Decile70 verbal	-0.16**	-0.28**	0.12
	(0.08)	(0.11)	(0.13)
Decile80 verbal	-0.36***	-0.44***	0.08
	(0.08)	(0.12)	(0.14)
Decile90 verbal	-0.70***	-0.46***	-0.24*
	(0.08)	(0.11)	(0.13)
Decile100_verbal	-1.28***	-0.45***	-0.83***
	(0.08)	(0.11)	(0.13)
Constant	0.22***	0.03	0.03
	(0.06)	(0.08)	(0.08)
Observations	1,792	836	2,628
R-squared	0.26	0.30	0.27

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

Note: Regression 1 shows difference in test scores for young cohort after the policy implementation. Regression 2 shows difference in test scores for old cohort before the policy implementation. Regression 3 shows the difference in test scores for young cohort as compared to old cohort

Table 4: Estimates of changes in math scores

Difference in math score	(1) For Young Cohort (year 2009-2013)	(2) For Old Cohort (year 2006-2009)	(3) For interaction YC*Decile
Decile10 math	0.40***	1.29***	-0.89***
	(0.08)	(0.10)	(0.13)
Decile20 math	0.24***	0.55***	-0.31**
	(0.08)	(0.10)	(0.13)
Decile30 math	0.20**	0.37***	-0.17
	(0.08)	(0.10)	(0.13)
Decile40 math	0.14*	0.27**	-0.13
	(0.08)	(0.14)	(0.16)
Decile60 math	-0.02	-0.01	-0.01
	(0.10)	(0.10)	(0.14)
Decile70 math	-0.17**	-	-0.17**
	(0.08)		(0.08)
Decile80 math	-0.40***	0.07	-0.47***
	(0.08)	(0.11)	(0.14)
Decile90 math	-0.51***	-	-0.51***
	(0.08)		(0.08)
Decile100 math	-1.02***	-0.06	-0.96***
	(0.08)	(0.15)	(0.16)
Constant	0.09	-0.34***	-0.34***
	(0.06)	(0.08)	(0.08)
Observations	1,847	911	2,758
R-squared	0.20	0.17	0.19

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

Note: 1. Regression 1 shows difference in test scores for young cohort after the policy implementation. Regression 2 shows difference in test scores for old cohort before the policy implementation. Regression 3 shows the difference in test scores for young cohort as compared to old cohort
Note: 2. Decile_70, decile_90 for math is – since there are no observations in both the deciles.