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Essays in Human Capital Development

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Dedicated to Shounak Sengupta and my parents.

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Essays in Human Capital Development

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This dissertation studies decisions pertaining to human capital investment, specifically education and health. Specifically, I examine human capital decisions through two key research questions. One, what is the effect of household structures on decisions pertaining to human capital development of infants? Two, what is the effect of education policies on education choices? Chapter 1 of the dissertation examines the former by assessing the role of grandparents in household decisions, and Chapters 2 and 3 study the latter question using education policies in India and United States, respectively.

Chapter 1 studies the role of grandparents in healthcare decisions made for infants. Using a unique research design, I show that a change to household structure caused by the death of the last living grandparent can be used to identify the effect of grandparents on household decisions, if one exploits the variation in the timing of these decisions relative to the death. This chapter highlights the importance of grandparents in household decisions, especially

in context of technology diffusion and human capital development. It also makes an important contribution to the literature by offering a novel empirical strategy that could be used to study the effect of family members on a variety of outcomes in an extended household setting.

Chapter 2 and Chapter 3 investigate how education policies affect educational outcomes of disadvantaged populations. In Chapter 2, I examine the effects of the world's largest free lunch program, the Mid Day Meal Scheme of India. Using an instrumental variable strategy, I explicitly incorporate the differential implementation levels of the policy across states. The findings of this paper show that India's free lunch program increased primary school enrollment in India, especially for girls and other disadvantaged populations. In Chapter 3, I study the effect of education policies on choices of students in higher education. In particular, I explore the impact of a policy change that allowed undocumented immigrants to be eligible for in-state tuition in Texas. Employing a difference-in-differences strategy, I find that the reduced college costs resulting from the in-state tuition policy decreased the gap in educational outcomes of undocumented immigrants and their US-born peers. The results of this chapter suggest that the in-state tuition policy increased the probability of graduating and graduating with advanced degrees from community colleges for undocumented immigrants.

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Chapter 1

Estimating the Role of Grandparents in Household Decisions

Grandparents play an important role in household decisions. The presence of grandparents influences household resources such as time endowment and income, which can have important implications for labor market outcomes and household production decisions. For instance, studies have found significant effects of old age pensions earned by grandparents on labor supply, educational attainment and nutritional outcomes (Duflo 2000, 2003, Bertrand et al. 2003, Edmonds 2006). Moreover, grandparents often have different education levels and preferences than young parents, which may also influence household decisions. Although household resource allocation has been studied extensively (Thomas 1990, Thomas and Chen 1994), very little is known about the influence of grandparents on household decisions.

Multigenerational families are an important example of a setting in which grandparents play a crucial role. In the United States, 19 percent of the population lives in multigenerational families and 10 percent of all children under the age of 18 live in the same household as at least one of their grandparents.[1,2] These statistics are much higher for developing countries in Asia, Sub-Saharan Africa, Central/South America and the Middle East, where multigenerational households are more common. For instance, in South Africa, 36 percent of the population and 42 percent of all children live in multigenerational households, while in India, 50 percent of all children live in the same household as their grandparents.[3] Given the high prevalence of multigenerational households, it is important to examine the role of grandparents in household decision-making.

I use a novel research design that exploits a change in household structure caused by the death of the last living grandparent. To examine the role of grandparents, many existing studies have used variations that are country-specific (such as old-age pension reforms). The variation used in this paper offers the advantage of broader applicability because all multigenerational families experience a transition in household structure after the death of the last living grandparent. A potential concern with using death as a variation is sample selection. It can be argued that deaths are not randomly distributed, and the timing of death of a grandparent might be confounded by other socioeconomic characteristics of the household. For instance, poorer families are more likely to have lower life expectancies and grandparents are likely to die younger

[1]A multigenerational family in the U.S. is defined as a household that includes two or more adult generations, or one that includes grandparents and grandchildren.

[2]Source: Pew Research Centre. Available at `http://www.pewsocialtrends.org/2013/09/04/children-living-with-or-being-cared-for-by-a-grandparent/`

[3]Source: Social profile of South Africa, 2002-2009 - Statistics South Africa and Global Perspectives on Multigenerational Households, and District Level Household Survey of India.

in poor families than in non-poor families. Using data from India, I show that the sample selection concerns can be addressed by comparing decisions made before to those made after the death of a grandparent. This research design can be used for any decision that a household makes frequently enough to be observable before and after the death, provided that the household has limited control over the timing of the decision.

I use data from India to analyze the influence of grandparents on neonatal care decisions. Neonatal care is an interesting outcome to illustrate my research design for two main reasons. First, grandparents play a significant role in neonatal care decisions. Grandparents typically are very involved in child care activities, and they are an important source of information for young mothers (Fikree et al 2001, Mumtaz and Salway 2007). However, they may have dated information about neonatal care best-practices, which can worsen a child's outcomes (Kerr et al. 2008, Aitken et al. 2016). Thus, the effect of grandparents on neonatal care outcomes is unclear a priori. Second, neonatal care is extremely important for child survival and human capital development. One-third of global neonatal deaths can be prevented by good neonatal care practices such as routine vaccination and early initiation of breastfeeding. India has a low incidence of good neonatal care practices and accounts for over 26 percent of global neonatal deaths. In this paper, I focus on two neonatal outcomes (i) an index of three vaccinations that physicians recommend should be administered at birth and (ii) the probability of feeding colostrum to a newborn.[4,5] High prevalence of multigenerational households and low incidence of good neonatal care practices makes India an interesting setting to study the role of grandparents on neonatal care decisions. Specifically, I use the variation in the timing of birth relative to the death of the last living grandparent to compare neonatal care decisions made for children born after the death with (i) children born before the death and (ii) children born in families in which the last living grandparent is alive. This approach enables me to control for differences across households in which the last living grandparent dies and households in which s/he is alive. In another specification, I use mother-fixed effects to separate out any time-invariant mother-specific characteristics and compare children born within a household.

My findings suggest that the presence of grandparents significantly worsens the neonatal care provided to newborns. In particular, children born after the death of the last living grandparent are 4.4-4.9 percentage points more likely to be immunized at birth and 3.4 percentage points more likely to be given colostrum.[6] These effects translate to a 33-36 percent increase in the probability of being immunized at birth and a 5.6 percent increase in the

[4]The three vaccinations are Bacillus CalmetteGurin (BCG), Oral Polio Vaccine (OPV), and Hepatitis B. The index takes a value of 1 if all three vaccines were given to the child and 0 otherwise. Details are provided in Section 4.

[5]Colostrum is the first breast milk produced by the mother after a child is born. It contains antibodies that help protect the newborn against diseases. In many cultures (including India), colostrum is considered impure (because of its "thick yellowish" appearance) and it is not given to newborns.

[6]The analysis sample includes children born in households in which either a grandparent is currently living within the household or the last living grandparent died during a three-year reference period of the survey.

colostrum outcome. In line with the findings of prior studies, I do not find any evidence for heterogeneity in results by the sex of the grandchild (Deaton 2003, Barcellos et al. 2015). I also find that the estimates for probability of immunization are higher in magnitude when a grandmother dies than when a grandfather dies, but these differences are not statistically significant.[7] Robustness tests show that the estimates are not sensitive to specifications, and are not driven by direct effects of death such as grief.[8] Suggestive evidence shows that the primary mechanisms driving the results are inter-generational informational gap and changes in bargaining power resulting from a change in household structure from a three- to a two-generation household. To support my empirical results, I present a conceptual framework that shows how differences in information, beliefs, and bargaining power allocation can affect household choices across different household structures.

This paper contributes to the literature in three important ways. First, this is the first paper to use the death of a grandparent to estimate causal effects of multigenerational households. Studies examining the role of grand-

[7]The lack of evidence for heterogeneity in results by the gender of the grandparent could be because of the fact that about 70 percent of the households in the sample had grandmothers as the last living grandparent.

[8]To separate the direct effects of death (such as changes in per-capita resources) from the effects of a change in household structure, I compare the death of the second-to-last grandparent to the death of the last living grandparent. Note that in case of the death of the second-to-last grandparent, there is no change in household structure. The findings suggest that children born after the death of second-to-last grandparent are less likely to get immunized. However, when the death results in a change in the household structure (from a three- to a two-generation household), the negative direct effects of the death are outweighed by the positive effects of the change in household structure.

parents have primarily used local policy variation to isolate the effect of cash transfers to grandparents on household decisions (Duflo 2000, 2003, Bertrand et al. 2003, Case 2004, Edmonds 2006).[9] The empirical methodology used in this paper has the advantage of being applicable to any context with multi-generational households and can be used to study the role of grandparents on a variety of outcomes such as nutrition and education. Second, this is the only paper to have estimated the causal impact of grandparents on immunizations and breastfeeding practices. The existing evidence on the relationship between household structure and health outcomes is restricted to qualitative or correlation analysis (Doan and Bisharat 1990, Dasgupta 1995, Gage et al. 1997, Griffiths et al. 2002, Bronte-Tinkew and Dejong 2005, Kumar and Ram 2013, Allendorf 2013). Third, findings of this paper show that grandparents are important factors in determining the demand for new health technologies such as immunization. Some studies have identified lack of information and socio-cultural factors as reasons for slow uptake of technology innovations (Rogers 1995, Hall 2004, Geruso and Spears 2015). The findings of this paper contribute to this literature by showing that the presence of older generations within a household is another important determinant of the rate of adoption of critical healthcare practices.

The evidence presented in this paper has important implications for

[9]Some other studies in sociology and psychology literature have examined the relationship between grandparents and adolescent grandchildren in developed countries (Matthews and Sprey 1985, Casper and Bryson 1988, Sands and Goldberg-Glen 2000, Ruiz and Silverstein 2007). But the evidence presented in these studies is not causal.

improving policy interventions aimed at reducing neonatal mortality. A recent WHO report emphasizes that preventable infant deaths can be reduced by raising awareness of neonatal care practices.[10] Typically, such strategic informational interventions are aimed at young parents, in particular, young mothers. This paper argues that informational interventions for young mothers may not translate into better neonatal outcomes, if they are not the primary decision-makers. Given the significant influence of grandparents on critical healthcare decisions, it is important to include older generations in healthcare informational interventions. Since India's neonatal deaths account for over a quarter of global neonatal deaths, adopting cost-effective interventions to reduce Indian child mortality can significantly contribute to the achievement of global child health goals.

The rest of the paper is organized as follows. Section 1.1 describes the setting of multigenerational households and neonatal care practices in India. Section 1.2 presents a conceptual framework. Section 1.3 describes the empirical strategy, followed by a discussion of the data in Section 1.4. Section 1.5 presents the empirical results. Section 1.6 presents the heterogeneity in the estimates and robustness checks. Section 1.7 discusses the possible mechanisms driving the results. Lastly, Section 1.8 concludes.

[10] Available at
http://www.who.int/maternal_child_adolescent/topics/newborn/every-newborn-action-plan-draft.pdf

1.1 Multigenerational Households and Neonatal Care in India

1.1.1 Multigenerational Families in India

In India, multigenerational households are very common and typically consist of older parents, their sons, unmarried daughters, sons' wives and their children (Coffey, Khera and Spears (CKS), 2013).[11] Bargaining power within a multigenerational household is a function of gender and age. Within a household, young women are subordinate to men and older women.[12] Thus, the presence of grandparents in a household adversely affects young women's autonomy in household decisions. According to IHDS (2005), 80 percent of Indian women between 15-44 years of age reported that they require permission from other senior members or their husbands to go to a health center. Panel A of Figure 1.1 shows that households in which the grandparents recently died, women's autonomy (measured by their ability to go to a health center without asking permission) is comparable to households in which grandparents are not permanent residents. In these households, less than 10 percent of women reported asking permission from a senior member to go to a health facility. As shown in Figure 1.1, for households in which grandparents are present, over

[11] Married daughters usually live with their husband's families. According to the Indian Human Development Survey (IHDS, 2012), about 98 percent married women indicate that they moved into the husband's parents' house after marriage. Over 64 of percent married women stated that they were currently living with their parents-in-law or lived with them until they died.

[12] In rural India, the status of young women drops drastically upon marriage and it remains low during the early reproductive years, until they become mothers-in-law (Dasgupta 1995, Allendorf 2013).

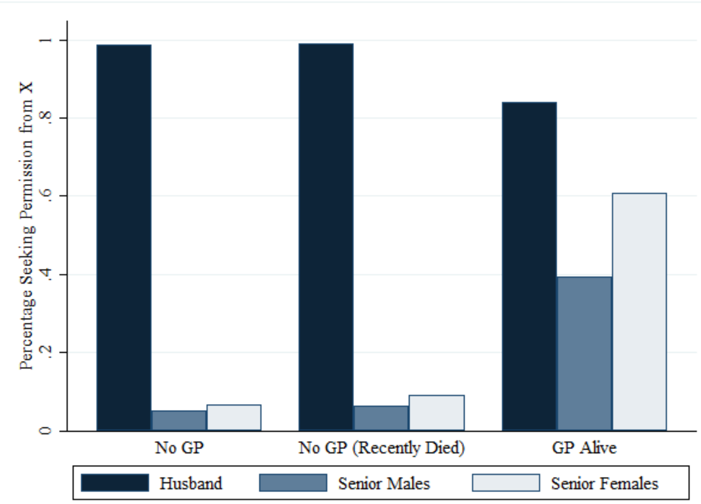
50 percent of women reported seeking permission from senior members to go to a health facility.[13] Figure 1.1 highlights the fact that, in rural India, older household members have a significant influence over young women's autonomy.

Resource allocation within a household is influenced by the relative bargaining strengths, information set and preferences of those involved in decision making (Behrman, 1995, 1998). Grandparents are different from young parents in two main ways. First, in low-income countries, grandparents typically have less education and less knowledge of healthcare advancements than young parents (Simkhada, 2010). According to the Indian Human Development Survey (IHDS, 2012), only 40 percent of grandfathers and 19 percent of grandmothers were literate, compared to a literacy rate of 79 percent for fathers and 61 percent for mothers.[14] Second, older generations during their reproductive years did not have easy access to good healthcare services, which contributes to their lack of knowledge about the importance, availability and affordability of healthcare services. Young parents, in contrast, are typically more educated and have more access to information about healthcare services. Also, most health awareness campaigns target young women for informational interventions. Given the generational information and education gap, it is likely that in families in which grandparents are the decision-makers and in

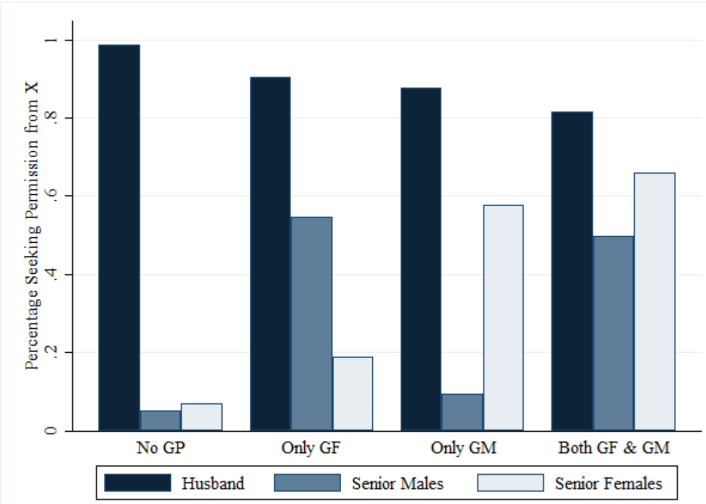
[13]In households in which mothers-in-law are present, about 59-65 percent of these women reported seeking permission from a senior female member or the mother-in-law. If I restrict the sample to families in which the only senior member is the mother-in-law, the percentage of women who seek permission from senior members is 64 percent.

[14]IHDS (2012) suggests that grandfathers and grandmothers had 3 and 1 years of education, on average, compared to the average of 7 and 5 years of education of fathers and mothers. Source: Author's calculations using IHDS, 2012.

Figure 1.1: Fraction of Married Women Seeking Permission to Go to Health Centers from Senior or Male Members across Different Household Structures



A: All Households



B: **Households with Grandparents**

Notes: This figure plots the proportion of women who stated that they need permission from other household members to go to a health center. The sample includes married women, aged 15-44 in the IHDS data for the years 2004-05. Different bar colors depict different household members from whom the women received permission to go to the health center. In Panel A, the first series includes a sample of current two-generation households in which a grandparent recently died; the second series include two-generation households that have had no deaths; and the third series includes three-generation households in which at least one grandparent is alive and living in the same household. In Panel B, GP stands for grandparents, GF denotes grandfathers and GM denotes grandmothers. The first series includes a sample of households that have no living grandparent; the second series includes households that have only grandfathers (but no grandmothers); the third series includes households that have only grandmothers (but no grandfathers); and the fourth series includes households in which both grandparents are alive and living in the same household.

families that have no grandparents, healthcare decisions will differ.

The association between the multigenerational family structure and women's health outcomes has been studied extensively by anthropologists and demographers (Jeffrey et al. 1989, Dasgupta 1990, 1995, Santow 1995, Barua and Kurtz 2001, Chorghade et al. 2006, Allendorf 2013). Allendorf (2013), who studied the effect of household structure on women's health, found that Indian women who live in extended families have better maternal health outcomes than those who live in nuclear families. However, as Allendorf notes, her analysis is limited because of sample selection and lack of data on transition of households from an extended to a nuclear family structure. In this paper I overcome these limitations and extend the literature by using a variation in the timing of birth relative to the timing of the death of the last living grandparent to determine how family structure affects the health outcomes of children. Moreover, I allow for lateral variation in household structure, i.e., my sample includes households that do and do not have married siblingswhich provides an additional opportunity to examine the relationship between heterogeneity in effects by household composition.

1.1.2 Neonatal Care in India

Each year in India 0.748 million babies die within the first month of birth, accounting for over a quarter of the world's neonatal deaths.[15] One-third

[15]Source: Unicef India. Available at `http://unicef.in/Whatwedo/2/Neonatal-Health-`

of newborn deaths in India are caused by neonatal infections which are preventable by vaccines and initiation of early breastfeeding. Despite large efforts aimed at achieving universal immunization coverage, the uptake of healthcare services in India remains low. Table 1.1 shows that 16 percent of the children who missed immunizations did so due to supply gaps. However, 65 percent of missed immunizations are attributed to demand-related reasons, with customary/family constraints and a lack of knowledge as the main causes of missed immunization. In this paper, I investigate whether the presence of grandparents contributes to this demand-side gap in neonatal care outcomes for children in India.

Existing evidence for the relationship between household structure and children's health outcomes is limited and ambiguous.[16] Some studies have found no association between household structure and children's health (Griffiths et al. 2002, Bronte-Tinkew and Dejong 2005), while others have found that the presence of grandparents is an important determinant of women's autonomy and children's health (Doan and Bisharat 1990, Gage et al. 1997). Studies that examine the low level of immunization coverage among infants have found that lack of knowledge, the mother's autonomy, and education are important determinants of improved immunization and feeding practices (Ku-

[16]Various psychology and sociology studies have examined the relationship between grandparents and grand-children (Matthews and Sprey 1985, Casper and Bryson 1988, Sands and Goldberg-Glen 2000, Ruiz and Silverstein 2007). Yet most of this literature studies behavior of adolescents in developed countries, where role of grandparents and household structures differ based on the status of parents (divorced or married). In India, divorces are uncommon and grandparents often have more bargaining power in the intra-household decisions than parents.

Table 1.1: Reasons for Not Getting Immunized

	All Families	Families With Death	Families Without Death
Supply Related	15.57	15.28	14.74
Demand Related	64.93	64.46	66.06
Family or Knowledge related	90.01	89.02	91.08
Financial Constraints	1.7	2.2	1.31
Time Constraint	8.29	8.79	7.61
Other	19.50	20.26	19.2
Observations	43,135	1,342	14,404

Notes: All statistics are in percentages. The sample includes married women aged 15-44 who did not have their children immunized. Supply-related reasons include: the place of immunization is too far, the auxiliary nurse midwife (ANM) is absent, the vaccine is not available, child were brought but not given immunization, and the waiting time was too long. Demand-related reasons include: too young to be immunized, unaware of the need for immunization, fear of side effects, no faith in immunization, mother too busy (time constraint), family problems, child ill and thus not brought for immunization, financial constraints, and the child is a girl. The other category includes an unknown place or time of immunization. This table shows that the majority of children are not immunized because of demand-related reasons; knowledge constraints are the main demand-related reason given for not immunizing children.

mar et al. 2010, Shroff 2011, Pavlopoulou et al. 2013). Steele et al. (1996) notes that a significant amount of unexplained variation in immunization uptake can be attributed to differences in knowledge and attitudes towards health care as well as power relationships within households. In this paper, I control for variation in parental attitudes by adding mother-fixed effects, and I investigate how power relationships and knowledge across generations affect the health outcomes of children.

1.2 Conceptual Framework

Let there be a three generation household that includes grandparents, parents and a child. For simplicity, let us assume that decisions related to health are taken only by female members, i.e., the grandmother and the mother. Let there be one final good, health of the child (H), that has two states: good health (H=1) and bad health (H=0). Let there be one binary input to health, immunization, $i \in \{0,1\}$, which enters into the health probability of the child:

$$E[H|i] = p(i) \text{ s.t. } p(1) > p(0)$$

Assume that the mother knows this true probability while the grandmother forms beliefs about the probability distribution s.t.

$$E_g[H|i] = p^g(i)$$

$$\text{s.t. } p^g(1) - p^g(0) < p(1) - p(0) \text{ and } p^g(0) = p(0)$$

Let there be an intangible cost, c, of changing prior attitudes about immunization and acquiring more information about the true probability distribution of health outcomes.[17] The grandmother has to incur this cost if she chooses i=1, but she does not have to incur this cost if she chooses i=0. Now, agent

[17] Mothers are more educated than grandmothers and young women are more likely to receive information about new born care through awareness campaigns and health workers. This is why I assume that the mothers know true health probability. Implications of this model will be similar if we assume that the mother and the grandmother have same dated beliefs about health probability but the cost of updating beliefs is higher for the grandmother than the mother.

j's utility maximization problem is:

$$\max_{i\in\{0,1\}} U(H) \text{ s.t. } E_j[H|i]$$

i.e.,

$$\max_{i\in\{0,1\}} p(i)\mathbb{1}(H=1) + (1-p(i))\mathbb{1}(H=0) - c_j$$

Case I: When the mother is the decision-maker, p(1)>p(0) and c=0. Thus, H(1)>H(0), and i^*=1.

Case II: When the grandmother is the decision-maker:

$$H(1) = p^g(1)[\mathbb{1}(H=1) - \mathbb{1}(H=0)] + \mathbb{1}(H=0) - c$$

$$H(0) = p^g(0)[\mathbb{1}(H=1) - \mathbb{1}(H=0)] + \mathbb{1}(H=0)$$

Thus, H(1)>H(0) and i^*=1 iff $[p^g(1) - p^g(0)] > c$.

Case III: When the mother and the grandmother both are involved in decision-making.

Let μ be the bargaining power of the mother and $(1-\mu)$ be the bargaining power of the grandmother.

$$H(i) = \mu[p(i)\mathbb{1}(H=1) + (1-p(i))\mathbb{1}(H=0)] +$$

$$(1-\mu)[p^g(i)\mathbb{1}(H=1) + (1-p^g(i))\mathbb{1}(H=0) - c]$$

Thus, i^*=1 iff

$$\mu[p(1) - p(0)] > (1-\mu)[c - (p^g(1) - p^g(0))]$$

Thus, given the differences in beliefs about the distribution over the health outcomes, the grandmother is more likely to choose no immunization than the mother. When both the grandmother and the mother are involved in decision-making, immunization is chosen only if the mother has enough bargaining power such that the product of her bargaining power and gain from immunization outweighs the product of the grandmother's bargaining power and the sum of the cost of immunization and her perceived loss from immunization.[18]

1.3 Estimation Strategy

The decision to live with a parent-in-law is endogenous. Thus, a simple comparison of households which have grandparents with those that don't will not capture the causal effect of the presence of grandparents. In order to compare three-generation households to two-generation households, I use the shock to household structure caused by the death of the last living grandparent. A potential concern is that the distribution of the timing of the death of a grandparent across households may not be random. Poorer families may have unobservable characteristics that are correlated with low life expectancy as well as child healthcare decisions. Summary statistics presented in Columns 1-3 of Table 1.2 show that there are some systematic differences between the households in which the last living grandparent died and the households in which no grandparent died during the reference period. For instance, the families that do not experience death in general are more educated and include

[18] In 3-generation Indian households, young mothers usually are not the decision makers, and so the analysis is closer to Case II than Case III.

younger mothers. In the estimation, I include controls for all these observables, but it is important to note that there is a concern for selection if we consider only those families which experienced death.

To address the sample selection concern, I use the timing of birth relative to the death as an exogenous variation. Using this variation, I employ two empirical strategies: (i) I compare children born after the death of the last living grandparents to children born before the death and to children in households where a grandparent is present; and (ii) I add mother-fixed effects.[19] In the former strategy I compare children across households, while in the latter, I compare children born within the same household.

To apply the first strategy, I include two indicators: one for whether the last living grandparent in the household died during the reference period and another for whether the child was born after the death of the last living grandparent. For children born in households in which the last living grandparent died, whether a child is born before or after the death is considered to be random. Thus, the indicator for whether a child is born after the death captures the reduced form effect of being born in a two-generation household without grandparents. The estimating equation for this approach is as follows:

$$Y_{imt} = \alpha_0 + \alpha_1 D_{mt} + \alpha_2 B_{imt} + \alpha_3 X_{imt} + \epsilon_{imt} \tag{1.1}$$

Y_{imt} is the health outcome of a child i born to mother m at time t. D_{mt} is an indicator that takes the value of 1 if the last living grandparent in the

[19]Black, Devereux and Salvanes (2014) and Persson and Rossin-Slater (2014) have used the death of a family member as a variation.

Table 1.2: Summary Statistics (Randomness of Timing of Birth)

Variables	No Death	With Death	Difference	Born before Death	Born After Death	Difference
Rural	0.780	0.770	0.010	0.770	0.770	0.000
Hindu	0.763	0.759	0.004	0.762	0.757	0.005
Muslim	0.152	0.196	-0.044***	0.191	0.200	-0.010
Christian	0.0474	0.0236	0.024***	0.0204	0.0264	-0.006
SC	0.202	0.229	-0.027*	0.252	0.208	0.044*
ST	0.138	0.107	0.032***	0.0995	0.113	-0.014
OBC	0.415	0.438	-0.023	0.402	0.472	-0.070**
Household Size	6.802	6.012	0.791***	6.193	5.844	0.350**
Number of DILs	1.265	1.092	0.173***	1.102	1.083	0.019
Rank of DILs	1.127	1.050	0.076***	1.048	1.053	-0.005
Number of Adult Males	1.815	1.211	0.604***	1.243	1.181	0.061
Number of Adult Females	2.200	1.173	1.026***	1.174	1.173	0.002
Age of Mother at Birth	23.58	24.86	-1.283***	24.29	25.39	-1.092***
Grandparent's Age	61.18	68.11	-6.929***	67.37	68.80	-1.438**
Male Child	0.495	0.486	0.009	25.45	11.81	-0.001
Mother's Schooling	7.885	7.579	0.306*	7.725	7.430	0.295
Father's Schooling	8.594	8.297	0.298*	8.441	8.162	0.279
Observations	10,536	1,528		734	794	

Notes: Column 1 includes households in which one grandparent is alive and no grandparent has died in the household. Column 2 includes households in which the last living grandparent died during the reference period. Column 3 shows the difference between the means from Column 1 and Column 2. Columns 4-6 include a sample in which the last living grandparent died. Column 4 shows summary statistics for children born before the death of the last living grandparent, and Column 5 shows the summary statistics for children born after the death. Scheduled Caste (SC), and Scheduled Tribes (ST), and Other backward classes (OBC) denotes the caste classifications of India. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

household died during the reference period and 0 otherwise. B_{imt} an indicator that shows whether child i was born after the death of the grandparent; it takes the value of 1 if the child was born after the death of the last living grandparent, and 0 if the child was born in the presence of a grandparent (i.e., those born before the death or into families that do not experience a death). Thus, the coefficient of this indicator compares outcomes of children born in two-generation households with those born in three-generation households. X_{imt} includes individual-level characteristics such as gender, an indicator for first male child, the mother's age at birth, dummies for child age, birth order, birth month, and birth year. I also include household-level characteristics such as paternal and maternal education, household size, type of residence (rural or urban), religion, and caste of the head of the household. α_1 captures the effect of being born in a household which experienced a death. The coefficient of interest, α_2, captures the effect of being born in the absence of a grandparent, i.e., in a two-generation household structure.

In the second empirical strategy, I add mother-fixed effects. Here I leverage the fact that many women in the sample had more than one birth during the reference period. For women who have had more than one birth, it is possible that one child is born before and another after the death of the last living grandparent. This provides a unique opportunity to compare children born to the same mother but in different household structures (two- versus three-generation households). Incorporating mother-fixed effects also allows me to separate out time-invariant factors specific to the mother or the household. The estimating equation is as follows:

$$Y_{imt} = \beta_0 + \beta_1 B_{imt} + \beta_2 X_{imt} + \phi_m + \epsilon_{imt} \tag{1.2}$$

All the variables are the same as in equation (1.1) except that this specification includes mother-fixed effect (ϕ_m). Adding mother-fixed effects alleviates the concern that the mother's (or the household's) unobservables are biasing the results.[20]

The key identifying assumption of this analysis is that the timing of the birth, conditional on death, is exogenous.[21] Columns 4-6 of Table 1.2 show that conditional on death, the observable characteristics of the sample of children born after the death are similar to those born before the death. In addition, I check whether the observable characteristics are associated with timing of birth relative to the death. Table A.1 shows the results of a regression of observable characteristics on an indicator for being born after the death for the sample of household that experienced the death of the last living grandparent. None of the observables (except the age of the mother at birth and the grandparent's age) significantly predict the timing of the birth

[20]If the birth of a child affects the probability of the death of a grandparent because of factors such as less time for elderly care, then there is a concern for reverse causality. To test that this is not the case, I estimate heterogeneity in results by the number of adult members. Since time constraint for elderly care is less likely to be binding in households with more adults, the issue of reverse causality would not be as prominent for larger households. I do not find any evidence for heterogeneity in results by the number of adult members.

[21]This would be problematic if the grandparents' presence directly affected the fertility decisions in the household. To alleviate this concern, Figure A.1 shows that the frequency of birth is smooth around the death of a grandparent.

relative to the timing of the grandparent's death.[22] While these covariates (except advice about immunization) are time invariant for a given household, the results show that conditional on death, the observables of a household are not correlated with the timing of birth.

Another important assumption is that the outcomes of children born before the death of the grandparent are not affected by the death. This assumption will be problematic if the grandparent dies after a long illness. In such a case, the children born before the death may have been affected by changes in time constraints (e.g., increased care for ill grandparent) or bargaining power. Thus, if the control group is also affected by the imminent death of the grandparent, the estimates would be biased. Ideally, using deaths caused by accidents would alleviate this concern. However, the dataset used does not provide any information about the cause of death. Using another nationally representative survey data (Indian Human Development Survey (IHDS), 2004-05), which includes the cause of deaths, I find that 80 percent of deaths of grandparents were due to unexpected accidents and short-term illness like heart attack, diarrhea, or fever (see Table A.3).[23] Hence, I expect the bias that can be attributed to the effect of death on control group to be small.

It is important to note that these strategies capture the reduced form

[22]By construction, the age of the mother is likely to be correlated with being born after a death in a household that has two or more children. Nevertheless, I add all of these variables to the regression equations.

[23]This only includes the deaths for which the causes were known. Missing values were excluded.

effect of the death of the last living grandparent. To separate the direct effects of the death from the effect of household structure change, I employ the same empirical strategies to compare households in which the second-to-last grandparent dies to households in which the last living grandparent dies.

1.4 Data

This paper uses the District-Level Household Survey (DLHS) of India for the years 2002-04 and 2007-08. This is a nationally representative dataset that includes immunization and birth history for children born to married women between the ages of 15 and 44 during a 3-year reference period. To supplement the analysis I also use the Indian Human Development Survey (IHDS) data for the years 2005 and 2012. IHDS is also a nationally representative dataset with detailed household level information.

The DLHS dataset includes information on deaths that occurred within a household since 2001 for 2002-04 round and since 2004 for 2007-08 round, amounting to roughly a 3-year long reference period. I use this information to create an indicator of whether a grandparent died in the family. Since the data does not contain information on the relationship of the deceased to the household head, I impute whether the deceased was a grandparent based on the age and gender of the deceased and the age and relationship among the living household members. Appendix A1 shows the details of the imputation. To confirm the accuracy of the imputed relationship of the deceased, I use the IHDS sample in which the relationship of the deceased is known, and I find that the correlation between the imputed relationship and true relationship is 0.998.

1.4.1 Outcome Variables

The data includes the immunization history of the last two live births that occurred during the three-year reference period. Information about whether the children received Oral Polio Vaccine (OPV), Bacillus CalmetteGuri (BCG), and Hepatitis B vaccination is used to create the key outcome variable, which is vaccination at birth.[24] Vaccination at birth is an indicator that takes the value one if the child received all three of these vaccines and zero otherwise.[25]

Another outcome of interest is whether colostrum is given to the newborn. The mother's first milk, colostrum, is rich in antibodies that protect the newborn from diseases. In developing countries, there is a widespread lack of awareness about its qualities and its key role in contributing to the health and growth of the newborn.[26] In many cultures (including those in India), colostrum is believed to be unclean due to its thick yellowish appearance, and it is often discarded. The DLHS dataset provides information about whether colostrum was given to the youngest child born during the reference period.

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[24] Bacillus CalmetteGurin vaccine is a vaccine primarily used against tuberculosis. Polio, BCG and Hepatitis B vaccines constitute the recommended vaccinations that are to be administered to the infant at the time of birth.

[25] In Table A.8 I present estimates for alternative definitions of the immunization index.

[26] Source: `http://www.who.int/nutrition/topics/world_breastfeeding_week/en/`

1.4.2 Sample Restrictions

Given that the immunization data is available for children born during the three year reference period, the sample is restricted to children aged 0-3 years. I exclude children born during the same month and year as the death because I do not know the exact death and birth dates and cannot determine whether these children were born before or after the death. Also, to reduce the immunization delays caused by grieving or care-giving for the terminally ill, I do not include children who were born a month before or a month after a grandparent death.[27]

To causally identify the effects of changes in household structure, I only include households in which the last living grandparent died during the reference period (treated group) and households in which exactly one grandparent is alive and no grandparent died during the reference period (control group).[28] I exclude families in which both grandparents are alive, although the results are not affected if I add these families back into the sample (see results in Section 1.7). I also exclude families in which the second-to-last-living grandparent died during my primary analysis, and I do so because these deaths do not produce a change in household structure. In a supplementary analysis,

[27]Results are similar if I include these children in the sample. The results do not change if I assign to the children born in the same month as the death either value 1 or 0 for the born after death variable.

[28]My decision to define the control group as families in which exactly one grandparent is alive is motivated by the thought that prior to death, the treated households, too, had exactly one grandparent. Thus, households that have one living grandparent are a comparable control group.

I include these excluded households to estimate the direct effects of death, in the absence of a change in household structure. Lastly, I exclude nuclear households in which there was no death during the reference period and no living grandparent in the household because I do not know why a grandparent is absent: the grandparent could have died long before the reference period or s/he could be living somewhere else.[29] Table A.2 compares the households in the analysis sample to an unrestricted sample that includes all household structures. Columns 1 and 2 of Table A.2 show that in most observables the sample of all households resembles the households that are included in my analysis.

In order to include mother-fixed effects, the sample used for immunization outcome analysis is restricted to mothers who had at least two births during the reference period. Column 3 of Table A.2 shows that women who had two or more children during the reference period are similar to the sample of all women who had children during the reference period (Column 2). While the differences in observables in Columns 2 and 3 are statistically significant for some observables, the magnitude of difference is not very large. I include these observables as covariates in the analysis to control for any observable differences.

[29] I also exclude households in which both grandparents died during the reference period. Very few households in the sample experienced two deaths and two births during the reference period. Table A.4 shows the sample restrictions imposed on different household structures.

1.5 Results

Figure 1.2 plots the binned residuals for the regression of probability of vaccination from Section 3, with the indicator for born after death excluded, against the time that passed between the birth of a grandchild and the death of a grandparent.[30] Panel A and Panel B of Figure 1.2 plot the residuals from regression in the equation 1.1 (no mother-fixed effects) and the equation 1.2 (with mother-fixed effects), respectively. As the figure shows, the residuals for the probability of vaccination jump after the death of the last living grandparent.

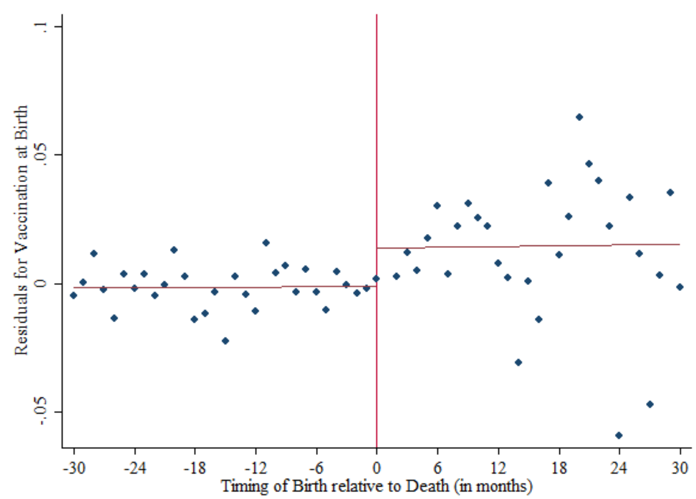
1.5.1 Results from Change in a Household Structure

Columns 1-3 of Table 1.3 present the results for the first estimating strategy. All specifications include demographic controls.[31] Standard errors are clustered by PSU level. Column 1 shows that being born after the death of the last living grandparent increases the probability of immunization at birth by 4.7 percentage points. By construction, the children born after the death also have a higher birth order. Thus, to ensure that the results are

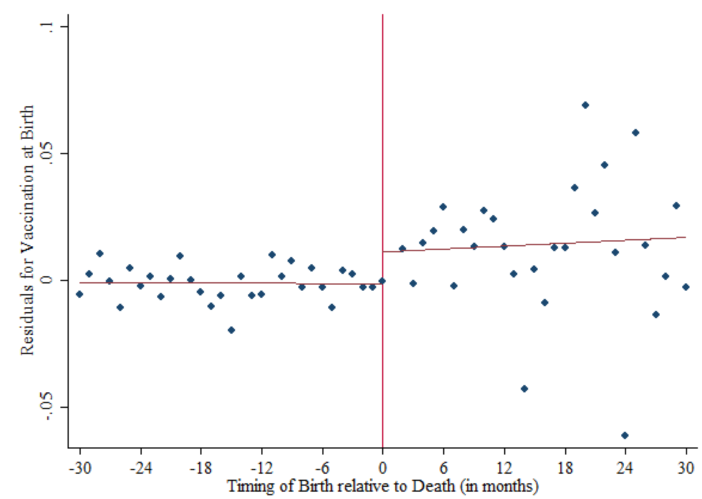
[30] For children born before the death of a grandparent or in households that had no death, the time passed between birth of a grandchild and death of a grandparent (x-axis) can be interpreted as time spent in the presence of a grandparent.

[31] Demographic controls include dummies for the child's age and gender, controls for the mother's age at birth, the age of the grandparent at the time of the child's birth, paternal and maternal education, an indicator of whether the child is a first male child, household size, type of residence (rural or urban), religion, caste of the head of the household, number of adult males, number of adult females, primary sampling unit (PSU) fixed effects and interaction of survey year- and survey month-fixed effects. The primary sampling unit consists of a village for rural areas and a block for urban areas.

Figure 1.2: Residuals against Timing of Birth w.r.t Timing of Death



A: **Comparison across Households**



B: **Comparison within Households**

Notes: This figure plots the binned residuals for the regression of probability of vaccination in equation 1 and equation 2, with the indicator for born after death excluded, against the time that passed between the birth of a grandchild and the death of a grandparent. The timing of birth relative to the timing of death is defined as the time difference between the birth of the grandchild and the death of a grandparent. Panel A plots the residuals for probability of vaccination for estimation equation 1, which does not control for mother-fixed effects. Panel B plots the residuals for probability of vaccination for estimation equation 2, i.e., it controls for mother-fixed effects. The sample for both figures includes households that experienced no death and had one living grandparent as well as households that experienced the death of the last living grandparent. In order to impute values for the x-axis for households that have had no deaths, the timing of birth relative to death for households with no death is defined as the negative of time spent in the presence of grandparent - i.e., for a 6 month-old in a household that had no death, the x-axis takes the value -6 because the child spent 6 months in the presence of the grandparent. Reference category is x=0.

not confounded by birth order effects, in the subsequent regressions I add birth order dummies. The results in Column 2 are similar to those shown in Column 1. It is possible that children born during a particular month or year are especially likely to be immunized (perhaps because of a local immunization drive or health camp). In Column 3, I add birth year- and birth month-fixed effects to control for these effects. Columns 1-3 of Table 1.3 show that the results are similar across specifications.

Columns 4-6 of Table 1.3 present the results of the second estimating strategy, which uses mother-fixed effects. In these specifications, I do not include controls for time-invariant household specific characteristics such as the death of a grandparent, maternal and paternal education, religion, and caste. The estimates suggest that children born after the death of a grandparent are 4.9 percentage points more likely to be immunized at birth. This is a large effect considering that the average immunization rate is 13.3 percent.[32] Going forward, for immunization results I will present only the specification that includes all the fixed effects (birth order, birth year and birth month).

Table 1.4 presents the results for the probability of giving colostrum to newborns. The sample used for this analysis includes all mothers who gave birth during the reference period. Since the data includes information about colostrum only for the youngest child born during the reference period, I cannot

[32]Note that the outcome variable is an index of *all* recommended vaccines to be administered at birth. Although the immunization rate for a particular vaccine might be higher, the mean for the index (i.e., receiving all of the recommended vaccines) is very low.

Table 1.3: Dependent Variable - Vaccination at Birth

	(1)	(2)	(3)	(4)	(5)	(6)
Born after Death	0.047**	0.045**	0.044**	0.053**	0.048**	0.049**
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.020)
Death of Grandparent	0.046	0.055	0.051			
	(0.043)	(0.044)	(0.044)			
Demographic Controls	✓	✓	✓	✓	✓	✓
Birth order dummies		✓	✓		✓	✓
Birth year and birth month dummies			✓			✓
Mother FE				✓	✓	✓
Mean of Dependent Variable	0.13	0.13	0.13	0.13	0.13	0.13
Observations	12,060	12,060	12,060	12,060	12,060	12,060

Notes: The sample includes mothers who had two or more births during the reference period. The unit of observation is the child. Demographic controls include the child's gender, child age dummies, child is a first male child, child is the first born, mother's age at birth, paternal and maternal education, whether the father lives in the household, the rank of the mother, the death of an older child, the number of daughters-in-law in the household, the presence of old individuals other than grandparents, the imputed age of the grandparents, household size, the number of children born during the reference period, the type of residence (rural or urban), religion, caste of the head of the household, the number of adult males, the number of adult females, primary sampling unit (PSU) fixed effects, and interaction of the survey year and survey month dummies. PSU is defined as a village for rural areas and a census block for urban areas. The treated group includes families in which the last living grandparent died, while the control group includes families in which exactly one grandparent is alive. Columns (1)-(3) show results from the estimation strategy including an indicator for death of the last living grandparent and an indicator for the child being born after the death of the grandparent. Columns (4)-(6) present results from empirical equation 2, which includes mother-fixed effects. Standard errors are clustered at PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

use the second approach for the colostrum outcome (because it is not possible to add mother-fixed effects). However, I can add household-fixed effects for the subsample of households that have two or more daughters-in-law who gave birth during the reference period. Like Table 1.3, Columns 1-3 subsequently add dummies for birth order, birth year and birth month. The estimates from Table 1.4 show that children born after the death of a grandparent are 3.6 percentage points more likely to be given colostrum. Given the baseline mean of 0.62, the estimates suggest that a child is 5.6 percent more likely to be fed colostrum if s/he is born after the death of the last living grandparent. Columns 4-6 show a 5.8-6.2 percentage point increase in the probability of giving colostrum to the newborn, but these estimates are not significant (due to the lack of power). As shown in Table 1.4, the results do not change significantly when other fixed effects are added. This suggests that the results are not driven by birth order or time effects.

1.5.2 Direct Effects of Death

As mentioned previously, the death of a grandparent can have direct effects on household decisions through a change in household resources (income and time), and stress caused by the death. The estimates presented in Section 1.5.1 show the reduced form effects of death which include direct effects of death as well as the effects of change in household structure. In this section, I separate the direct effects of death from the additional effects of a change in household structure.

Table 1.4: Dependent Variable - Colostrum Given to Newborn

	(1)	(2)	(3)	(4)	(5)	(6)
Born after Death	0.037*	0.036*	0.034*	0.062	0.059	0.058
	(0.020)	(0.020)	(0.020)	(0.139)	(0.140)	(0.146)
Death of Grandparent	-0.047***	-0.046***	-0.045***			
	(0.017)	(0.017)	(0.017)			
Demographic Controls	✓	✓	✓	✓	✓	✓
Birth order dummies		✓	✓		✓	✓
Birth year and birth month dummies			✓			✓
Household FE				✓	✓	✓
Mean of Dependent Variable	0.62	0.62	0.62	0.61	0.61	0.61
Observations	48,157	48,157	48,157	3,308	3,308	3,308

Notes: The unit of observation is the child. Demographic controls include: the child's gender, child age dummies, whether the child is a first male child, whether the child is the first born, the mother's age at birth, paternal and maternal education, whether the father lives in the household, the rank of the mother, the death of an older child, the number of daughters-in-law in the household, the presence of old individuals other than grandparents, the imputed age of the grandparents, household size, the number of children born during the reference period, the type of residence (rural or urban), religion, the caste of the head of the household, the number of adult males, the number of adult females, primary sampling unit (PSU) fixed effects, and interaction of the survey year and the survey month dummies. The PSU is defined as a village for rural areas and a census block for urban areas. The treated group includes families in which the last living grandparent died, while the control group includes families in which exactly one grandparent is alive. Columns (1)-(3) include all mothers who gave birth during the reference period; these columns include PSU fixed effects. Columns (4)-(6) include households that have two or more daughter-in-laws who had a birth during the reference period, and household-fixed effects are included. Standard errors are clustered at PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

For colostrum outcome, it is unlikely that a mechanism other than changes in information and bargaining power is driving the results. This is because colostrum outcome is unlikely to be affected by changes in household resources and the practice of throwing away colostrum is driven by cultural myths. In other words, if the mechanism behind the results was a change in income, we would not expect any significant impact on the colostrum outcome.

In the case of the immunization outcome, it is comparatively more likely that the reduced form effects of Table 1.3 include the effect of changes in household resources, in addition to the effects of a change in household structure.[33] However, Table 1.1 shows that the majority of women stated that family and knowledge constraints are the primary reason that they do not immunize children. Thus, as suggested by Table 1.1, the contribution of changes in resources, such as income and time, on the estimates is likely to be small.

To empirically test if my results are driven by the direct effects of death, I compare the families in which the last living grandparent has died to families in which only one of the two living grandparents has died. In this strategy, both the control group and the treated group have experienced a death. This strategy separates the additional effect of a change in household structure (resulting from the death of the last living grandparent) from the direct effect

[33]Though the vaccinations studied in this paper are either free or subsidized, other costs could exist, such as transportation to the health center, bribes, and time costs associated with immunization.

of the death of a grandparent. But this specification raises the aforementioned sample selection concern because all households in the sample have experienced a death. The results from Table 1.5 show that the death of a grandparent has a negative direct effect on the probability of immunization. But this negative effect is outweighed by the positive effect of the change in household structure. Thus, the results from Table 1.5 confirm that children born after the death of the last living grandparent are more likely to get immunized than children born before the death, and these effects are not driven by the direct effects of death such as changes in resources.[34]

Another way to capture the direct effects of death is to compare families that experience the death of one of the two grandparents to families in which both grandparents are alive and that experienced no deaths during the reference period. If my primary estimates were driven by the direct effects of the death of a grandparent, then I would find similar effects following the death of the second-to-last living grandparent. According to Table A.5, the effect of being born after the death of the second-to-last grandparent is negative and significant. Also note that, although the samples used in Tables 1.3, 1.5 and A.5 are different (but overlapping), the magnitudes of reduced form and direct effect estimates are comparable across these specifications.

[34]Note that in households in which there are two living grandparents, grandfather is usually the first one to die (about 72 percent of my sample). To make sure that the difference in effects of death are not driven by the gender of the grandparent, I repeat this analysis with (i) subsample in which only grandmothers die, and (ii) subsample in which only grandfather die. The magnitude and significance of the results change, but the direction remains the same for each of these subsamples.

Table 1.5: Dependent Variable - Vaccination at Birth

	(1)	(2)
Born after Death*Last Living GP Died	0.108***	0.102***
	(0.030)	(0.030)
Born after Death	-0.047*	-0.032
	(0.027)	(0.028)
Last Living GP Died	-0.229*	
	(0.121)	
Demographic Controls	✓	✓
Birth order dummies	✓	✓
Birth year and birth month dummies	✓	✓
Mother FE		✓
Mean of Dependent Variable	0.15	0.15
Observations	2,986	2,986

Notes: The sample includes mothers who have had two or more births during the reference period in households in which one grandparent died. The treated group includes households in which the last living grandparent died; in the control households one grandparent is alive. The variable *Last Living GP Died* is an indicator for the treated group in which the last living grandparent died. The unit of observation is the child. Demographic controls include: the child's gender; child age dummies; whether the child is a first male child; whether the child is the first born; the mother's age at birth; paternal and maternal education; whether the father lives in the household; the rank of the mother; the death of an older child; the number of daughters-in-law in the household; the presence of old individuals other than grandparents; the imputed age of the grandparents; household size; the number of children born during the reference period; the type of residence (rural or urban); religion; caste of the head of the household; the number of adult males; the number of adult females; primary sampling unit (PSU) fixed effects; and the interaction of survey year and survey month dummies. PSU is defined as a village for rural areas and a census block for urban areas. Column (1) shows results from the estimation equation 1, which includes an indicator for the death of the last living grandparent and another indicator for the child being born after the death of the grandparent. Column (2) presents results from empirical equation 2, which includes mother-fixed effects. Standard errors are clustered at PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The evidence presented in this section suggests that while the direct effects of death are negative, the effects of a change in a household structure outweigh the negative effects of death on neonatal care outcomes. Hence, the evidence presented in this section indicates that presence of older generations within a household is an important determinant of adoption of neonatal care best-practices.

1.6 Heterogeneity in Results

In this section, I explore the heterogeneity in results by household composition, rank of the mother, gender of the grandparent, and the sex of the grandchild.

1.6.1 Number and Rank of Daughters-in-law

In households that have only one married couple in the second generation, the death of the last living grandparent results in a complete transfer of bargaining power to the young couple. However, in households that have more than one married couple, the redistribution of bargaining power among daughters-in-law (DIL) after the death of grandparents is unclear a priori. CKS (2013) states that in households that have more than one daughter-in-law the husband's higher birth parity entitles DILs to higher intra-household ranks.

Table 1.6 presents results for heterogeneity across different household structures and ranks of DILs.[35] Columns 1 and 2 show the results for immunization at birth; Columns 3 and 4 show the results for colostrum outcome. Panel A of Table 1.6 shows that the estimates for households that have one DIL and households that have two or more DILs are not statistically different. Similarly, Panel B of Table 1.6 shows that, conditional on being in a household that has more than one DIL, the results are not significantly different across ranks of the mother.[36, 37] Thus, I do not find any evidence supporting discriminatory behavior of in-laws based on the intra-household rank of the mother.

1.6.2 Gender of the Grandparent and the Grandchild

Table 1.7 shows the heterogeneity in results by the gender of grandparent and grandchild. In Panel A of Table 1.7, the estimates for immunization outcome are larger when a grandmother dies than when a grandfather dies, but the differences are not statistically significant. For the colostrum outcome, the effects of grandmother's and grandfather's death (Column 3) are not statistically different, but the effects of the grandmother's death are larger when household-fixed effects are added. These results should be interpreted with

[35]The rank of a mother is defined as her rank among the DILs in the household in order of age.

[36]The results of Table 1.6 are robust to inclusion of all household structures - i.e., including households that have only one DIL.

[37]The framework of Section 2 suggests that as long as the DILS share the same beliefs about the distribution of health outcomes, the effect of the death of a grandparent should not vary across households that have different number of DILS or ranks of DILS. The estimates support the information-gap and differential belief hypothesis presented in Section 2.

Table 1.6: Heterogeneity by Household Composition

	Vaccination at Birth		Colostrum	
	(1)	(2)	(3)	(4)
Panel A: Number of Daughters-in-Law				
Born after Death*1 DIL	0.042*	0.045**	0.034*	
	(0.022)	(0.022)	(0.020)	
Born after Death* $\geq$ 2 DILs	0.081*	0.101**	0.044	
	(0.043)	(0.045)	(0.043)	
2 or more DILs	0.028		0.010	
	(0.035)		(0.015)	
Mother FE		✓		
Mean of Dep Variable	0.13	0.13	0.62	
p-value (Equality of Interaction terms)	0.38	0.27	0.83	
Observations	12,064	12,064	48,157	
Panel B: Rank of the mother				
Born after Death*Rank1	0.112**	0.136**	0.045	0.081
	(0.053)	(0.058)	(0.090)	(0.136)
Born after Death*Rank $\geq$ 2	0.130**	0.126**	0.054	0.042
	(0.060)	(0.061)	(0.090)	(0.155)
Rank 1	0.107		-0.025	
	(0.086)		(0.019)	
Mother FE		✓		
Household FE				✓
Mean of Dep Variable	0.17	0.17	0.64	0.61
p-value (Equality of Interaction terms)	0.83	0.91	0.89	0.56
Observations	2,314	2,314	9,752	3,308

Notes: Columns 1-2 include mothers who have had two or more births during the reference period, Column 3 includes all mothers that had a birth during the reference period, and Column 4 includes households in which two or more daughter-in-laws had a birth during the reference period. Standard errors are clustered at the PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

caution because about 70 percent of households had grandmother as the last living grandparent.[38] Panel B of Table 1.7 shows that the effect of the death of a grandparent is similar for both male and female children.[39]

1.7 Robustness Checks

Placebo Deaths: As a falsification test to confirm that my results are not spurious, I compare (i) households in which a member from the same generation (not a grandparent) dies to (ii) households with no death during the reference period. To compare these to my main estimates, I restrict the sample to households that have one living grandparent. If my results were driven by direct effects of death and change in household structure had no effect on household decisions, then I should expect similar results from death of any family member. The estimates from this analysis (not presented in this paper) show that the death of a household member, who is not a grandparent, has a negative and insignificant effect.

Choice of Control Group: To show that my results are not sensitive

[38] This is because the sample includes households with only one living grandparent and women typically live longer than men.

[39] Table A.6 shows that girls born after the death of a grandmother have significantly higher probability of being vaccinated than girls who are born after the death of a grandfather or boys born after the death of grandmother. The p-value for differences in the coefficient for girls born after the death of the grandmother and the grandfather is 0.0254; for boys born after the death of a grandmother it is 0.0293. This suggests that boys are more likely to be vaccinated while grandmothers are alive. This finding also implies that in the case of grandsons, grandmothers might favor medical interventions over traditional wisdom. I caution that more detailed data and analysis on the information and beliefs is required to draw any conclusions about grandson preference.

Table 1.7: Heterogeneity by Gender

	Vaccination at Birth		Colostrum	
	(1)	(2)	(3)	(4)
Panel A: Gender of the Grandparent				
Born after Death*GM	0.052	0.059*	0.016	0.232
	(0.033)	(0.033)	(0.029)	(0.209)
Born after Death*GF	0.039	0.040*	0.051**	-0.174
	(0.024)	(0.024)	(0.026)	(0.151)
GM	0.032		-0.007	
	(0.040)		(0.014)	
Mother FE		✓		
Household FE				✓
Mean of Dependent Variable	0.13	0.13	0.62	0.61
p-value (Equality of Interaction terms)	0.74	0.64	0.36	0.12
Observations	12,060	12,060	48,157	3,308
Panel B: Gender of the Grandchild				
Born after Death*Girl	0.042	0.062**	0.031	0.083
	(0.030)	(0.030)	(0.030)	(0.139)
Born after Death*Boy	0.050*	0.041	0.038	-0.055
	(0.027)	(0.027)	(0.027)	(0.191)
Girl	-0.004	-0.001	-0.001	0.017
	(0.008)	(0.009)	(0.009)	(0.024)
Mother FE		✓		
Household FE				✓
p-value (Equality of Interaction terms)	0.83	0.58	0.88	0.37
Mean of Dependent Variable	0.13	0.13	0.62	0.61
Observations	12,060	12,060	48,157	3,308

Notes: GM denotes households in which the grandmother either died or is the only living grandparent Columns 1-2 include mothers who had two or more births during the reference period; Column 3 includes all mothers who had a birth during the reference period; and Column 4 includes households that have two or more daughters-in-law who gave birth during the reference period. Standard errors are clustered at the PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

to the choice of the control group, I include households in which one or more grandparents are alive. The estimates from Table A.7 confirm that my results are robust to the choice of the control group.

Robustness to Definition of Outcome Index: In India, different vaccines might have different levels of supply outreach. For example, because of the focus in India on the eradication of polio, during the late 1990s, Oral Polio Vaccine's outreach increased significantly more than other vaccines. Moreover, information about different vaccines is likely to differ across generations. For instance, because BCG and OPV have been a part of the mandatory immunization schedule for a longer period than Hepatitis B, information and perceptions about these vaccines could differ. To determine whether my results are sensitive to the choice of the index of immunization, Table A.8 shows the results for alternative definitions of the index. Panel A of Table A.8 shows the results for alternative definitions of the immunization index that use a combination of two out of the three vaccines to be administered at birth. Panel B presents the results for individual vaccines. The results of Table A.8 suggest that my estimates are robust to the definitions of the immunization index.[40]

Choice of Sample: Table A.9 presents the estimates using a different dataset. The IHDS dataset does not provide comparably detailed information about timing of death but it does provide more accurate information about

[40]The results for BCG vaccine are not significant but the sign is positive. This is probably because of widespread outreach for BCG vaccines. Given that mass campaigns for BCG were launched more than half a century ago (during the 1950s), it is likely that the information-gap about the importance of BCG vaccines differs little across generations (Lahariya, 2014).

the relationship of the deceased to the head of the household. IHDS also maps a longer time horizon, from 2001 to 2012. Due to data constraints in the IHDS sample, the outcome variable for this analysis is an index of two vaccines (OPV and BCG). Column 1 of Table A.9 presents the estimates for this new outcome variable using the original specification. Columns 2 and 3 show the estimates with a subset of controls using the DLHS and IHDS samples, respectively. Panel A of Table A.9 shows that when IHDS sample is used the estimates are larger and less precise. This lack of precision can be attributed to the small sample size of children born after the death in households that have experienced death. A possible explanation for the larger IHDS estimates is that it includes a longer time period. Panel B shows that the increase in the probability of vaccination associated with an additional year since death for children born after death is similar across the two dataset.

1.8 Discussion

The discussion in Section 1.5.2 showed that the direct effects of the death of a grandparent are negative, but these effects are outweighed by the positive effects of a change in household structure. In this section, I elaborate on the discussion of mechanisms through which the death of a grandparent can affect neonatal outcomes.

1.8.1 Time Constraints

Child health outcomes can be affected by the role played by grandparents in the household (whether or not the household structure changes); for example, are the grandparents care-receivers or care-givers? If the grandparents were care-receivers, their deaths should loosen the time constraint and increase the probability of immunization. To test this possibility, I estimate the heterogeneity in results by the number of adult males or females in the household (unmarried siblings of the parent or older siblings of the infants). If time constraint is the main mechanism that drives the effect of the presence of grandparents, then the presence of other adults in the household should reduce the burden of caring for the dying grandparent, and the estimates should become smaller as the number of adults in the household increases. The estimation results (not presented here for the sake of brevity) suggest that the effects do not differ by the number of adult males, or number of adult females in the household.

If the grandparents were care-providers, their deaths should result in a decreased probability of vaccination for children born after a death. The negative estimates from Table 1.5 and A.5 suggest that a set of grandparents might act as care providers. However, in the cases where only one grandparent is present, the time constraint imposed by the grandparent is unclear. Note that in households that have only one living grandparent, the grandparent is older than the grandparents found households in which both grandparents are alive. This could result in smaller negative effects from time constraint in households that have only one living grandparent because older grandparents are perhaps less able than younger ones to provide childcare.

1.8.2 Stress caused by Death

Another concern in this analysis is the confounding in-utero stress effect caused by the death of a family member. According to some studies, the death of a family member significantly increases in-utero stress and this stress affects the later health outcomes of children (Black et al 2014, Currie and Rossin-Slator 2013). Here I focus on neonatal care outcomes such as immunization and breastfeeding practices rather than biological health outcomes. Because these decisions are made once the child is born, my results are unlikely to be biased by the stress-effect on in-utero children caused by the death of a family member. Nevertheless, stress caused by death could result in a delay in vaccinations. Assuming that the stress or the grief effect of death is likely to lessen with time, I run my estimation with subsamples that exclude children born between 3, 6 and 9 months before and after the death, to allow for more grieving time. The results (not shown here) are similar to the estimates of Table 1.3. Thus, the results are unlikely to be driven by grief-effects.

1.8.3 Place of Delivery

An increase in immunization also could be driven by an increase in the number of births at a health facility. In other words, if the choice of the place of delivery is affected by the death of a grandparent, then my estimates might be capturing the effect of being born in a hospital. There are two ways in which the death of a grandparent can affect the choice of the place of delivery: (i) a change in financial resources and (ii) a change in the bargaining power of the mother. If the death leads to tighter budget constraint (fewer resources) there will be a decrease in deliveries at health facilities. Because immunization is positively correlated with delivery at a health facility, we should see an accompanying decrease in immunization. My findings suggest that the opposite is true. In fact, the probability of immunization is higher for children who are born after the death of the last living grandparent. On the other hand, if the death leads to an increase in available financial resources, then we should expect to see an increase in both the number of deliveries at health facilities and immunizations. Unfortunately, I do not know the place of delivery for all children in my sample, and I cannot empirically test if this is true. However, in families in which the last living grandparent has died, 13 percent of women state that financial constraints are the primary reason for not delivering the child at a health facility; in families that have not experienced a death, only 10 percent of women say this.[41] This suggests that a change in financial resources is not the primary reason to drive any changes in institutional deliveries.

As mentioned above, the death of a grandparent can loosen information and family constraints, and this can lead to more deliveries taking place

[41]Author's calculations. The statistics are derived from a subsample of DLHS data, including married women aged 15-44, who delivered the child at home.

at health facilities. When this is the case, the immunization estimates will be influenced by the effects of the absence of grandparents on the delivery outcomes. Given that the correlation between being vaccinated at birth and being born at a health facility is 0.25, not all of the increase in immunization can be attributed to the increase in institutional delivery.[42] Moreover, an increases in institutional deliveries, even if they lead to increased immunizations, are likely to be driven by changes in information and bargaining constraints. This suggests that the presence of grandparents has implications that extend beyond neonatal care outcomes to maternal health, which is beyond the scope of this paper.[43]

1.9 Conclusion

In this paper, I estimate the causal effect of the absence of grandparents on neonatal care outcomes in India. Using exogenous variation in the timing of

[42]This correlation is calculated using the subsample of youngest children born during the reference period for whom the information on place of delivery is available.

[43]Appendix Table A.10 shows the results for choice of the place of delivery as outcome. The information on the place of delivery is available only for the youngest child, and hence I cannot add mother fixed effects. In Panel A, I show the results for sample including all households in treated group and control group. In Panel B, I restrict the sample to only include households which have two or more DILs, and more than one DIL gave birth during the reference period. Columns 1-3 show results for empirical strategy one (without any household fixed effects). In Columns 4-6, I include household fixed effects. As shown in Appendix Table A.10, the direction of the results vary depending on the sample used. An explanation for this fluctuation in results is that, unlike the colostrum outcome, an important factor for the choice of the place of delivery is household income. Moreover, number of women present in the household to provide care during delivery could also affect the choice of place of delivery. The dynamics involved in the decision for the choice of place of delivery are more complicated and beyond the scope of this paper.

birth relative to the timing of death of a grandparent, I find that children born after the death are 4.4-4.9 percentage points more likely to be immunized at birth and 3.4 percentage points more likely to be given colostrum. I do not find any heterogeneity in results across the sex of the child or the grandparents. The results of this paper suggest that one way to improve neonatal care practices is to include older generations in healthcare informational interventions.

In addition to identifying the causal impact of grandparents on neonatal care, this paper has broad implications for future work. The results indicate that grandparents play an important role in critical household decisions, including those that affect child survival. My findings provide evidence that multigenerational households are slower to adopt new health technologies, likely because grandparents have different preferences for and knowledge about these technologies than parents. Thus, household structure is an important factor in assessing the demand for investments in children and technology diffusion. In future research, it would be useful to extend this analysis by studying the effect of grandparents on other important household decisions such as maternal health outcomes, nutritional outcomes and education outcomes of children.

Chapter 2

Impact of Free School Lunch Program in India

Food for Education programs have been used as a policy instrument to incentivize school participation for many years across the globe. School Feeding Programs (SFPs) are associated with various benefits including improved health, higher future productivity, better cognitive ability, gender equity and reduced child labor (Gundersen et al. 2012, Mirtcheva and Powell 2013). Given the wide range of benefits, SFPs have been adopted in various developing as well as developed nations. Based on a sample of 169 countries, a study by the World Food Programme (Burbano de Lara et al. 2013) estimates a potential annual investment of approximately \$47 - \$75 billion in school feeding programs around the globe, reaching out to over 368 million children. As noted by the WFP, India's school feeding program, known as the Mid Day Meal Scheme (MDMS), is the largest school feeding program in the world covering 113.6 million beneficiaries. This paper studies the impact of this nationally mandated free school lunch program on the probability of enrollment in primary schools in India.

The Mid Day Meal Scheme is a right-based program which entitles all children between the ages of 6-10 to free meals in public schools, and is aimed at incentivizing primary school attendance and achieving better nutritional outcomes for young children. Mid Day Meal Scheme is not only the largest school feeding program in the world, but is also among the top ten flagship programs of the Government of India. In 2016-17, MDMS was allocated $1.5billion in the annual federal budget.[1] Despite the large overall budgetary allocation, when compared to school feeding programs in other countries, MDMS has one of the lowest per child annual cost (see Table B.1). In light of increasing policy focus of child nutrition and universal primary education, the combination of low per child cost and large scale coverage makes MDMS a unique program to study the impact of school feeding programs.

While SFPs are generally considered to be effective in raising school attendance, the magnitude of effect varies depending on contexts (Alderman et al. 2012). An explanation for the variation in magnitude is that most of the literature includes small-scale studies which are difficult to generalize. Another caveat of this literature is that most existing studies estimate an intent-to-treat effect, which does not account for gaps in program implementation or uptake (Kremer and Vermeersch 2005, Kazinga et al. 2009, Jayaraman and Simroth 2011). This paper addresses these concerns in the literature by using a large dataset, and estimating a local average treatment effect to examine the impact of the world's largest SFP on school enrollment.

I use data from Indian Human Development Survey (IHDS), 2005,

[1] Government of India, http://mdm.nic.in/

and National Family Health Survey (NFHS), 1998-99, to study the impact of MDMS on school enrollment for children in primary school. I exploit the variation in the adoption of the policy across states and time, and use a difference-in-differences technique to evaluate the intent-to-treat (ITT) impact of the program on educational outcomes.[2] The ITT impact captures the average difference in the mean of outcomes of states which received the policy after 2001 (treatment group) and those which had the policy since the 1980s (control groups). However, there are two concerns with the ITT estimates in the context of the MDMS: (1) the level of implementation of the policy differed across and within states due to various factors such as bad governance, low political will, and insufficient budgetary allocations, and (2) the control states also benefited from this national policy. To address the concern of endogenous level of implementation, I evaluate the local average treatment effect (LATE) of the program on enrollment, using the policy as an instrument for fraction of schools *actually* offering free meals. While the ITT estimate gives the impact of the opportunity of being able to receive free meals in school, the LATE estimate captures the impact of actually receiving free school meals.

The findings of my analysis suggest that the program significantly increased the probability of primary school enrollment. The ITT effect estimates a 6 percentage point increase in gross as well as net primary school enrollment due to the policy.[3] This effect translates into 5.5% (7.3%) increase in the

[2]Prior to 2001, only a few states had a (state-funded) MDMS. These states constitute the control group for my analysis.

[3]Net primary enrollment is the ratio of number of students aged 6-10 years enrolled in

gross (net) primary school enrollment. The LATE estimate suggests 1 percent increase in the fraction of treated population increases the gross and net enrollment probability by 0.25 and 0.26 percentage points, respectively. To examine the extensive margin response of the policy, I study the effect of MDMS policy on enrollment in grade 1. My findings for the subsample for first graders suggest that while the gross enrollment effects are similar for first grader, the net enrollment effects are almost twice as large.[4] This suggests that the policy significantly increased the enrollment of young out-of-school children in grade 1. A grade-by-age analysis of enrollment reveals positive and significant effect of the program on on-time enrollment of children in primary school.

The analysis of heterogeneity in enrollment response reveals that the increase in probability of primary school enrollment is significantly higher for girls compared to boys. I also find that disadvantaged population groups of India (which include the Scheduled Castes and Scheduled Tribes (SC/STs) and Other Backward Classes (OBCs)) show higher increases in primary school enrollment probabilities relative to the high caste population.[5] The heterogeneity results for enrollment in grade 1 are similar to primary school enrollment results. These heterogeneous effects are in line with the findings of the

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school to the number of 6-10 year-olds in the sample.

[4]Net enrollment ratio in grade 1 is defined as the number of students aged 6 years enrolled in school to the number of 6 year-olds in the sample.

[5]The Scheduled Castes and Scheduled Tribes (STs) are two groups of historically disadvantaged people recognized in the Constitution of India. The criterion followed for specification of a community, as scheduled tribes are indications of primitive traits, distinctive culture, geographical isolation, shyness of contact with the community at large, and backwardness.

literature, and align with the expectations that girls and children from disadvantaged backgrounds are likely to be more responsive to free-lunch incentives provided by MDMS.

This paper contributes to the growing pool of literature analyzing the impact of school feeding programs on education outcomes. Various studies using randomized control trials have found the SFPs increased school participation, at least for girls (Kazianga et al. 2009, Kremer and Vermeersch 2004). While the results of this paper may not be as precise as those of randomized controlled trial studies, the results of this paper have the advantage of being generalizable. Moreover, in contrast to the existing studies, this paper estimates the local average treatment effect (LATE), explicitly incorporating the differential level of implementation of the policy across states, which is a feature of most government-run welfare programs in developing countries. This paper also extends the analysis to include effects of the program on on-time enrollment and extensive margin.

This paper also contributes to the literature studying the impact of the Mid Day Meal Scheme. This strand of literature is large but inconclusive, and is largely restricted to small survey studies which may not be generalizable to the national level (see Khera 2006). Moreover, the different nature of treatment, region of study (survey sample from different states) and the different estimation strategies used in these studies make the estimates incomparable.[6]

[6] For instance, Afridi et al. (2010) analyzed the treatment of offering freshly cooked meals compared to packaged snacks in urban area schools in one Indian state and found

The closest comparison of this paper in the MDMS literature is Jayaraman and Simroth (2011) which also uses a large dataset to study the impact of MDMS on enrollment.[7] However, by the virtue of my data and estimation strategy, my study has certain strengths over their analysis. First, their data does not have any information on mid day meal implementation at the school level, and hence relies on the ITT impact. Since these ITT estimates do not account for the actual treatment effect on the treated, they underestimate the program impact. In this paper, the use of instrumental variable estimation helps me address this issue. Second, Jayaraman and Simroth (2011) use official school level data and, thus, focus on children who are already enrolled in schools. In contrast, this paper uses an individual-level data which allows the analysis to include the out-of-school children to measure extensive margin response. Moreover, the enrollment statistics in their official school data might be subject to a greater degree of measurement error (Dreze and Kingdon 2001) than data obtained from individual-level surveys.[8] Lastly, my dataset allows for a wider coverage (19 states and 1 Union Territory (UT)) compared

a significant impact on the attendance for boys but no effect on student enrollment; while Afridi (2010) assessed the treatment of offering cooked meals compared to monthly dry rations in rural area schools in another Indian state and found a significant impact of the free lunch program on the daily school participation rate for girls, but insignificant results for boys.

[7]Chakraborty and Jayaraman (2016) is another study that uses a national individual-level dataset, but the focus of their paper is to study the effect of the policy on cognitive outcomes.

[8]It is plausible that schools overstate official enrollment figures to maintain performance evaluation criterion. Kingdon (2005) notes that enrollments figures in school-returns data are unreliable because failing/unpopular publicly funded schools exaggerate their student numbers in order to justify their existence.

to Jayaraman and Simroth (2011), which is restricted to 15 states.[9]

This paper has two important implications. One, the results of this paper highlight the importance of incorporating implementation imperfections when evaluating policies in developing countries. The results of this paper show that the intent-to-treat effects underestimated the true effect on the program, captured by the LATE estimates. Two, the findings of this paper suggest that the school feeding program had a greater response on the extensive margin than the intensive margin in India. This distinction on margins of response is difficult to capture when using administrative school data as it does not allow the researcher to observe children who are not currently enrolled. Using individual-level data, the analysis in this paper is able to capture the policy effect of different margins and across different demographic groups.

This paper is organized as follow. Section 2.1 describes the background of the policy and its implementation. Section 2.2 discusses the data and Section 2.3 describes the empirical strategy. Section 2.4 presents the empirical results and robustness checks. Section 2.5 presents a discussion of the results. Lastly, Section 2.6 concludes.

[9]A Union territory is a type of administrative division in the Republic of India. Unlike states, which have their own elected governments, union territories are ruled directly by the Union Government (Central Government).

2.1 Institutional Background

In August 1995, the Government of India launched the National Programme for Nutrition Support to Primary Education (also known as the Mid Day Meal Scheme), which mandated the provision of free cooked meals in all public primary schools across the country. Prior to 1995, only two states had the school free (cooked) lunch program - Tamil Nadu and Gujarat.[10] Under the national MDMS, the federal government was to bear the cost of providing raw grains and the state governments were responsible for financing the expenditure of cooking the grains and providing cooked meals in schools. In the interim, the states that could not make immediate budgetary or implementation arrangements were allowed to distribute monthly dry ration to the enrolled students.[11] Only three states (Kerala, Madhya Pradesh and Orissa), responded to the policy by offering free meals in some regions while the rest of the states did not respond to the policy at all.

After a 6 year lag in implementation, in late 2001, the Supreme Court of India directed all states to implement this program within 6 months. In practice, most states missed the new deadline and the implementation was still staggered across and within states (across public schools). The states' inaction with regards to policy implementation resulted in public campaigns

[10]Other states which introduced this program before 2001 had limited implementation (see Vishwanathan 2003). Kerala had an opt-in program.

[11]Dry ration refers to the practice of giving uncooked wheat or rice on a monthly basis, often based on the attendance of a pupil. Children received three kg of foodgrain per month if they had 80 per cent attendance in school. However, the attendance criterion is not strictly followed in practice.

expressing dissatisfaction with program implementation. As a result of these internal protests and the Supreme Court's order, the coverage of the program increased steadily after 2001 (Table B.2 in appendix shows the timing of adoption of policy for different states). Though there was no criterion dictating the staggered implementation level of the program across states, various studies have noted that the idiosyncratic timing and level of implementation of the mid day meal program was a combined function of organized campaigns by civil society activists, continued scrutiny by the media, and political and bureaucratic interest in the program (Jayaraman and Simroth 2011). Khera (2006) notes that "in many states, improvements in MDMS are linked to an increased political interest in the scheme".

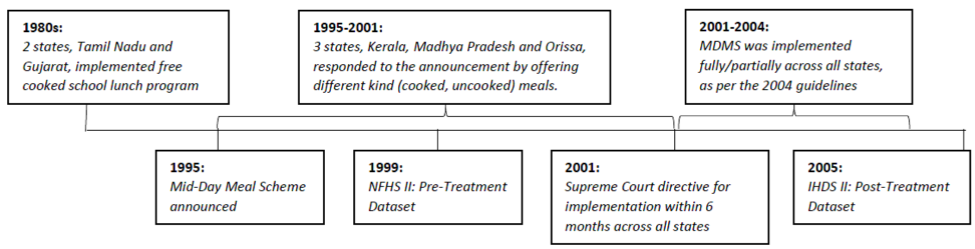
An important point to keep in mind for the purpose of this study is the distinction between policy adoption and its implementation intensity. The Supreme Court order made 2001 the effective year in which the policy was announced for all states in the sample. Hence, I use 2001 as the year of policy announcement. Since this announcement applied to all states, it is reasonable to treat this policy as an exogenous variation to the states. According to MDMS reports, by 2004, the program was fully or partially implemented across all 28 states and the 7 union territories (UT) of India. However, the level of implementation differed significantly across and within states. In other words, states had differential percentage of public primary schools offering free meals and different kind of meals - cooked or uncooked. The timeline and the states relevant for this study is discussed in more detail in Section 2.2.

The key objectives of the program included improving nutritional status of children, increasing the school enrollment and reducing drop-out, at least at the primary level. The secondary objectives of the program involved greater social assimilation of children from different castes, religion and gender (Chutani 2012). The rationale behind the program design was to incentivize the children from poor families to attend school, instead of assisting parents in home-production or their occupations, by providing them free lunch in school. The availability of at least one nutritious meal a day during early childhood was also aimed at curtailing malnutrition among children. This paper examines the performance of the program on its primary objective of increasing enrollment at primary school level.

2.2 Data

This paper uses two different nationally representative individual-level cross-sectional datasets: Indian Human Development Survey (IHDS), 2004-05 (henceforth 2005), and National Family Health Survey (NFHS), 1998-99 (henceforth 1999). Figure 2.1 illustrates the timeline of the policy announcement and implementation. As shown in the figure, two states, Tamil Nadu and Gujarat, implemented the free cooked meal policy in the 1980s. Figure 2.1 also shows that after MDMS was initially announced in 1995, only 3 states (Kerala, Orissa and Madhya Pradesh) responded to the announcement between 1995 and 2001. Since the exact implementation status of these states during 1998-99 is unclear, I drop them from the sample. I also drop 3 small states (Goa, Nagaland and Sikkim) as they had 0 fraction treated in 2005.[12] After dropping these 6 states, I have 19 states and 1 UT in my dataset (see Figure 2.2).[13] The results are similar if I add these states back to the sample.

Figure 2.1: Timeline for Mid Day Meal Policy and its Response



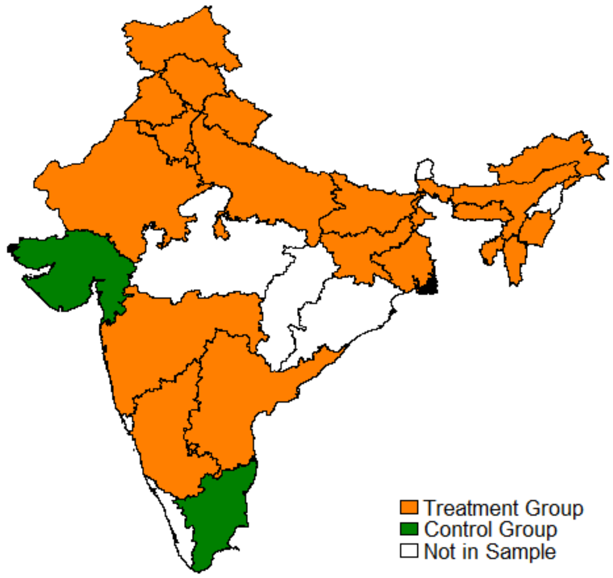
Note: Tamil Nadu originally introduced free school meal program at a small scale in 1925 in rural areas, but it was expanded to cover the entire state in 1982.

The key independent variable, fraction of students in primary schools receiving free mid day meal, is constructed using individual level IHDS dataset. The survey contains information on whether or not a child received mid day meals in school. I use this variable to construct the district-level fraction of children enrolled in schools who received any (cooked or uncooked) free meals. Using the IHDS data, Figure 2.3 plots the relationship between the district

[12]Kerala had an opt-in feeding program before 1995 and was, as suggested by the literature, the only state to significantly respond to the MDMS announcement in 1995. In the case of Orissa, JS (2011) suggests that the implementation of the mid day meal program started in 2001, but the NSS data statistics from Vishwanathan (2006) suggest that prior to 2001, a significant number of children between the ages of 7- 12 had access to school meals.

[13]The data is not available for any union territory except for Delhi (there are total 7 union territories in India). Also, in 2000, the number of states in India increased from 25 to 28 as three large states were split into 6 smaller states. In my sample I have combined the smaller states to represent the pre 2000 structure of states.

Figure 2.2: Treatment and control states

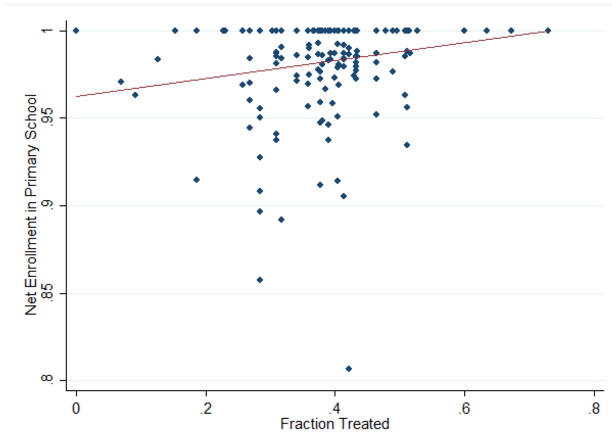


Note: This map reflects the states covered in the sample, states dropped from the sample. Note that the map does not show any Union Territories (for which the data is unavailable, except for Delhi).

net enrollment average and the fraction treated at district level for the sample of children between 6-10 years of age. Net enrollment ratio is defined as the number of students aged 6-10 years enrolled in school to the number of 6-10 year-olds in the district. Figure 2.3 shows that net enrollment increases with the fraction of students receiving free meals. This positive correlation substantiates the evidence presented in the literature (Dreze and Kingdon 2001, Motkuri 2009). I build on this is correlation to examine the causal impact of the free school lunches on enrollment in India.

Due to the lack of individual level information on free school meals in the pre-program time period, I use the state-level fraction of children getting

Figure 2.3: Relationship between district-level enrollment and fraction treated



Note: The sample is obtained from the IHDS data and includes 5-10 year-olds. The district net enrollment ratio is defined as the number of students of age 5-10 years enrolled in school to the number of 5-10 year-olds in the district. Fraction treated is defined as proportion of students enrolled in public primary schools receiving free mid day meals in a district.

free school meals presented in Vishwanathan (2006), calculated using the National Sample Survey (NSS) 1999-2000. Since, for control states, the program was introduced in the 1980s (long before the year of current study), I assume that the change in fraction of schools providing free lunches from 1999 to 2000 is negligible. I use the ratio of district population to state population as weights to create district level fraction treated from the state level statistics presented in Vishwanathan (2006). For the treatment states, the fraction is effectively 0 in the pre-program period (see Table 2.1, Vishwanathan (2006)).[14] An impor-

[14]For some states, the fraction treated was positive but very small (at most 2 percent). Incorporating this variation in the level of initial fraction treated in treated states does not change the results.

tant point to note here is that in the pre-treatment period, the coverage of the mid day meal was not 100 percent in the control states. Moreover, the fraction treated was different across the two control states. Hence, the control states also benefited from the new policy (maybe because additional funds from the federal government facilitated better implementation), imposing a downward bias on the ITT estimates. I correct for this bias using instrumental variable analysis with fraction treated as the endogenous variable and the presence of the policy as the instrument.

Other independent variables used in the analysis include child's gender, dummies for birth year, type of place of residence (rural or urban), household size, number of children under-5 in the household, number of siblings, caste and religion of the household head, and growth rate for state GDP (SGDP). The SGDP variable is constructed using data collated from the state reports, by the Planning Commission, Government of India.

The summary statistics for the treatment and control groups are presented in Table 2.1. The sample includes 6-17-year-olds who are either enrolled in primary school or are out of school. The pre-treatment statistics suggest that on average, the treatment group has a greater proportion of rural population and larger household sizes than the control group. The control group has higher state GDP growth rates compared to the treatment group. In the analysis, I add all these observable characteristics to control for any systematic differences between the control and treatment group.

Table 2.1: Descriptive Statistics

Variable	All		Treated Group		Control Group	
	Mean	sd	Mean	sd	Mean	sd
Pre-Treatment (1999)						
Age	9.59	2.83	9.60	2.83	9.54	2.82
Rural	0.68	0.47	0.69	0.46	0.56	0.50
State GDP	5.61	3.15	5.55	3.27	6.23	1.19
Male	0.49	0.50	0.49	0.50	0.49	0.50
Household Size	7.45	3.67	7.55	3.74	6.37	2.63
SC/ST	0.33	0.47	0.33	0.47	0.33	0.47
OBC	0.25	0.43	0.23	0.42	0.43	0.49
Hindu	0.71	0.45	0.70	0.46	0.89	0.32
Muslim	0.16	0.37	0.17	0.38	0.08	0.27
Christian	0.06	0.24	0.07	0.25	0.02	0.15
Uneducated mothers	0.66	0.47	0.66	0.47	0.58	0.49
Uneducated fathers	0.36	0.48	0.36	0.48	0.30	0.46
Number of Siblings	3.04	1.76	3.10	1.77	2.46	1.57
Fraction of Treated Districts	0.02	0.08	0.00	0.00	0.24*	0.15
Observations	37085		33830		3255	
Post-Treatment (2005)						
Age	9.42	2.59	9.43	2.60	9.26	2.47
Rural	0.67	0.47	0.68	0.47	0.52	0.50
State GDP	7.89	3.93	7.72	4.06	9.65	1.18
Male	0.47	0.50	0.47	0.50	0.48	0.50
Household Size	6.33	2.37	6.38	2.39	5.82	2.15
SC/ST	0.29	0.45	0.30	0.46	0.25	0.43
OBC	0.37	0.48	0.36	0.48	0.49	0.50
Hindu	0.75	0.44	0.73	0.44	0.88	0.33
Muslim	0.18	0.39	0.19	0.39	0.10	0.30
Christian	0.02	0.14	0.02	0.14	0.02	0.14
Uneducated mothers	0.54	0.50	0.55	0.50	0.43	0.49
Uneducated fathers	0.27	0.44	0.27	0.45	0.21	0.41
Number of Siblings	2.60	1.66	2.65	1.68	2.08	1.37
Fraction of Treated Districts	0.38	0.08	0.386†	0.08	0.42	0.06
Observations	17613		16085		1528	

Notes:The sample includes children aged 6-10 years. The sample includes 19 states and 1 UT, as the data is unavailable for the remaining 6 UTs and 5 states (Kerala, Madhya Pradesh, Orissa, Goa, Sikkim and Nagaland) were dropped from the sample. Panel A shows the the statistics based on NFHS 2 data for the pre-treatment year (1999) and Panel B shows the statistics from IHDS data for the post treatment year (2005). The Scheduled Caste/Scheduled Tribe (SC/ST) and Other Backward Classes (OBC) reflect the disadvantaged population groups (for definition refer to footnote 4). The State GDP (SGDP) growth rates are obtained from the data tables collated by the Planning Commission, Government of India.
*The level of fraction treated in the two control states, Tamil Nadu and Gujarat, was 41.5% and 12.7%, respectively.
†The level of fraction treated in the two control states, Tamil Nadu and Gujarat, was 44% and 40.5% in 2005, respectively.

2.3 Estimation Strategy

To identify the causal impact of the free lunch offered in schools on the probability of enrollment, I exploit the variation from the staggered adoption of the MDMS across the states. A few pioneering states implemented the program in the 1980s, while the others caught up after 2001. To estimate the causal effect of the program on enrollment, I use two estimation strategies: (i) difference-in-differences to estimate the ITT effect, (ii) instrumental variable estimation to explicitly incorporate the differential level of program implementation.

2.3.1 Intent-To-Treat Estimation

I use the exogenous policy variation across states and time to estimate the intent-to-treat impact. Specifically, I compare the change (over time) in outcomes among the states which recently got the free lunch program (treatment group) to the states which always had the free lunch program (control group). The estimating equation is as follows:

$$Y_{idst} = \alpha_0 + \alpha_1 X_{idst} + \alpha_2 Post_t + \alpha^{enroll} Treat_s * Post_t + \psi_d + \epsilon_{idst} \qquad (2.1)$$

where Y_{idst} is a binary variable for whether the child i in district d at time t is enrolled in school or not. X_{idst} are the individual level characteristics such as age, gender, caste, place of residence, etc. Since different districts, across and within states, might differ in terms of quality of governance, infrastructure, etc., I control for district fixed effects (ψ_d). The binary variable $Post_t$ takes a value of 1 for 2004-05 and 0 otherwise. The indicator variable, $Treat_s$, takes a value of 1 if the district is in a state which is in the treatment group and 0 otherwise. The control group includes the two states (comprising of 25 districts) which had universal primary public school free lunch program, while the other states (including 202 districts) constitute the treatment group. The error terms ϵ_{idst} account for the effect of all unobserved variables that vary across individuals, districts and time. The standard errors in this analysis are clustered at district level.

The coefficient on $Treat_s * Post_t$, α^{enroll}, measures the ITT effect of the program implementation on the outcome variables, i.e. the average difference in means between the treatment and control group.

The key identifying assumption of difference-in-differences approach is that the time trend in the absence of MDMS would have been the same in both treatment and control states. Ideally, to test this assumption, one would use the data from pre-2001 years to compare trends in the treatment and control states for the outcome variable. Due to lack of availability of such data, I instead use publicly available state-level statistics for adult literacy rate as an indicator of the general level of education to compare the time trends in treatment and control states.[15] Panel A of Figure 2.4 illustrates that the trends in literacy rate for the treatment and the control groups are parallel. The p-value for equality of slopes is 0.78, hence cannot reject the

[15]Source: Planning Commission, Government of India

equality of slope. Similarly, Panel B of Figure 2.4 shows the state-wise trends in infant mortality rate from 1997 to 2005 across the treatment and control groups. It is interesting to note that the trends are parallel before as well as after the policy was announced. This is not surprising because the policy did not affect the infant mortality rate. Panel C of Figure 2.4 show that trends in gross secondary enrollment are also parallel for the treatment and the control group.[16]

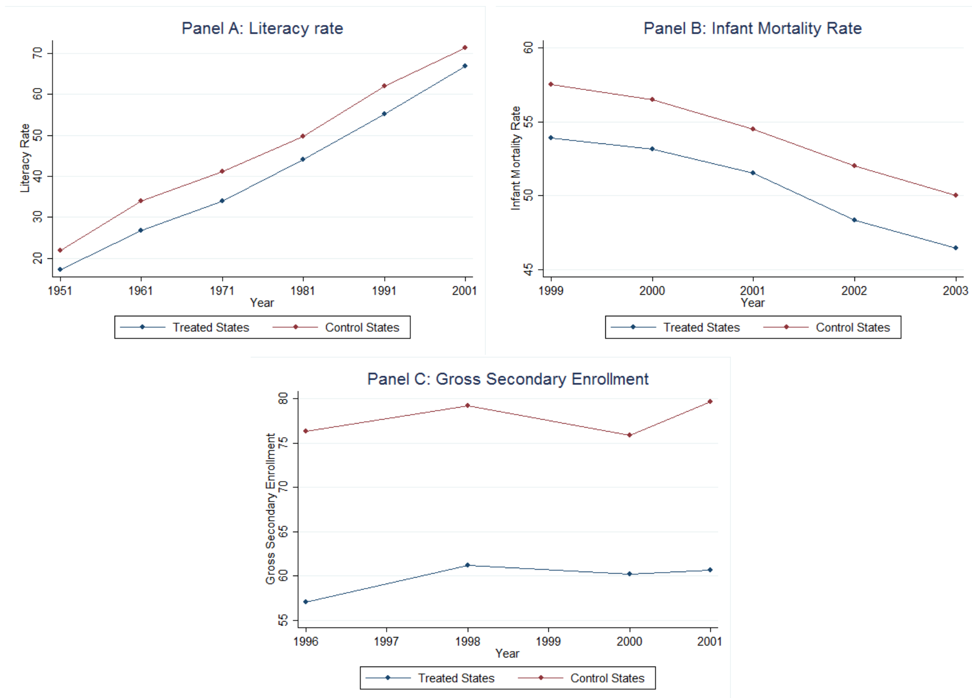
Two important points to note in context of MDMS are: (1) given the incomplete coverage in the control states during the pre-treatment period, the control group also increased program outreach after the policy was announced, and (2) the extent of implementation of the policy differed across and within states due to factors such as bad governance, low political will, insufficient budgetary allocations, etc. The ITT estimate does not address any of these concerns and hence underestimates the true policy impact. To incorporate these effects, I use instrumental variable estimation.

2.3.2 Instrumental Variable Analysis

One way to address the concern of the differential level of implementation across states is to use the level of policy implementation as the key independent variable instead of the presence of policy, i.e., regress enrollment on the fraction of children who received mid day meals, to capture the aver-

[16] The p-value for equality of slopes for infant mortality rate is 0.894 and for gross secondary enrollment is 0.472. Hence, we cannot reject the equality of slope for these trends.

Figure 2.4: Trends of treatment and control group



Note: This Figure plots the trends in literacy rate, infant mortality rate, and gross secondary enrollment rate for treated states and control states. Adult literacy rate is the percentage of people aged 15 and above who can, with understanding, read and write a short, simple statement on their everyday life. Data Source for Panel A is Economic Survey (2012-2013), Ministry of Home Affairs, Government of India. Infant Mortality Rate is the ratio of infant (less than a year old) death per 1000 live births. Data go Panel B is obtained from Sample Registration System (SRS) Bulletin, Volume 47 No.1 (Sep. 2013). Panel C denotes the trends in gross secondary enrollment for treatment and control states. The sample in Panel C includes data for 10 out of 18 control states.

age treatment effect. However, using the fraction of children who received the treatment might introduce an endogeneity bias. Although the program was launched nationally in 2001, the differential level of program implementation across the states might reflect that more proactive states had better on-ground implementation of the policy. To address this endogeneity concern, I use the presence of the policy as an instrument for fraction of schools offering free meals. I take advantage of the aforementioned state and time variation to apply difference-in-differences technique to estimate the fraction of schools that actually offered the free lunch in each district. I use this estimated fraction as a measure of the extent of implementation to evaluate the impact of MDMS on education outcomes among the children in India. Key assumptions for validity of the instrument are: (1) the instrument and the endogenous variable are correlated, and (2) exclusion restriction. While the first assumption is tested using the F-Statistic of the first stage, exclusion restriction is difficult to test empirically. Since the policy was announced at the national level it is plausible that the presence of policy does not affect the outcome variable through any channel other than the fraction of schools offering free lunch, maintaining the validity of the exclusion restriction. While it is not possible to test for correlation between the unobservables and the instrument variable, it is possible to check the correlation between the instrument and the observable characteristics. If observable characteristics significantly correlated with the instrument, then the exclusion restriction is less likely to hold. Table 2.2 shows that the observable characteristics do not significantly predict the presence of policy. Therefore, Table 2.2 provides suggestive evidence supporting the exclusion restriction.

To estimate the impact of the intensity of treatment on enrollment, I estimate the impact of fraction treated on probability of enrollment using the

Table 2.2: Exogeneity of Policy

	Rural	Household Size	Female	SC	ST
Treat*Post	0.066	-0.733***	0.008	0.033	0.040
	(0.081)	(0.190)	(0.028)	(0.034)	(0.026)
	OBC	SGDP	Hindu	Muslim	Christian
Treat*Post	0.051	-1.144	0.023	-0.031	0.002
	(0.049)	(0.696)	(0.029)	(0.029)	(0.007)
	Other Religions	Mother's Educ	Father's Educ	Siblings	Birth Year
Treat*Post	0.006	0.026	-0.017	0.078	0.319
	(0.006)	(0.030)	(0.031)	(0.085)	(0.362)
Observations	54698	54698	54698	54698	54698

Notes: The sample includes 19 states and 1 UT. Each cell denotes the coefficient from a regression of observable Y on Treat*Post variable. Robust standard errors (clustered at district level) are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

following equation:

$$Y_{idst} = \gamma_0 + \gamma_1 X_{idst} + \gamma_2 Post_t + \rho FractionTreated_{dt} + \psi_d + \mu_{idst} \tag{2.2}$$

where $FractionTreated_{dt}$ is the fraction of individuals receiving free lunch in primary schools in district d at time t and all other variables are the same as in equation (1). I estimate equation (2) by two-stage least square (2SLS) using the following as the first stage equation:

$$FractionTreated_{dt} = \beta_0 + \beta_1 X_{idst} + \beta_2 Post_t + \alpha^{frac} Treat_s * Post_t + \psi_d + e_{idst} \tag{2.3}$$

where $Treat_s * Post_t$ is the instrument for $FractionTreated_{dt}$.

Note that ρ is interpreted as the local average treatment affect among the compliers. Since in this case, the endogeneity is at an aggregated district level, the compliance to the treatment is at the district level.[17]

2.4 Empirical Results

2.4.1 Findings

Table 2.3 presents the ITT and LATE estimates for gross and net enrollment in primary school and grade 1. All the specifications include demographic controls such as religion, parents' education, household size, dummies for birth year and district fixed effects. Standard errors are clustered at the district level. Columns 1 and 2 present the results for gross and net enrollment in primary school, respectively. Columns 3 and 4 present the results for gross and net enrollment in grade 1, respectively. Panel A shows that the intent-to-treat estimates are positive and significant for all the outcome variables. In particular, the reduced form estimates suggest that MDMS increased the probability of enrollment in primary school by 6 percentage points, and enrollment in grade 1 by 5-11 percentage points. Panel B shows the results from instrumental variable strategy. The F-stat on the first stage shows that the presence of the policy strongly predicts the fraction of children who receive free meals in school. The first stage coefficient is below 1, which captures the imperfect implementation of the policy. The LATE effects from Panel B show

[17] Since the model is exactly identified, the LATE estimate is the same as Wald estimator, i.e. $\rho = \frac{\widehat{\alpha^{enroll}}}{\widehat{\alpha^{frac}}}$

that a one percent increase in the fraction of students getting meals increases the probability of primary school enrollment by about 0.25-0.26 percentage points; hence full coverage by the policy would increase the probability of enrollment by 25-26 percentage points. Similarly, a full coverage by the policy would increase the gross enrollment in grade 1 by 24 percentage points, and net enrollment in grade 1 by 46.7 percentage points. This estimate translates into a 32% increase in gross primary enrollment and 39% increase in gross grade 1 enrollment. Thus, the results of Table 2.3 show that the MDMS induced a larger extensive response by getting out-of-school children into schools and increasing enrollment in grade 1.

The dataset used does not allow for differentiation among the students who drop-out of school and those who never enrolled. Also, it does not offer any information about whether a student, who is older than the official age for the grade that he/she is in, enrolled at an older age or has been repeating grades. For example, the official age for grade 5 is 10 years, but a 12 year old could be studying in grade 5 because either he/she started school at the age of 8 years or he/she started school at 6 years of age but repeated grades for 2 years. Thus, an increase in gross enrollment could result from more children going to school (extensive margin response), reduced drop-out (intensive-margin response) or more students repeating grades. While it is difficult to completely separate the three components, I study the effect of the policy on on-time enrollment, i.e. being on-track in school according to recommended grade-age combinations. Table 2.4 presents a transition matrix that shows the effect on the policy on

Table 2.3: Impact on Probability of Enrollment

	Primary School		Grade 1	
	Gross Enrollment	Net Enrollment	Gross Enrollment	Net Enrollment
	Panel A: Reduced Form (ITT)			
Treat*Post	0.057***	0.063***	0.050*	0.113**
	(0.018)	(0.022)	(0.028)	(0.051)
	Panel B: IV (LATE)			
First Stage				
Treat*Post	0.232***	0.239***	0.207***	0.241***
	(0.035)	(0.035)	(0.031)	(0.033)
2SLS				
Fraction Treated	0.245***	0.262***	0.244**	0.467***
	(0.078)	(0.076)	(0.123)	(0.174)
Demographic Controls	✓	✓	✓	✓
District Fixed Effects	✓	✓	✓	✓
F-Stat for First Stage	45.16	47.31	43.71	52.33
Fraction Treated Mean	0.14	0.14	0.10	0.12
Pre-Reform Enrollment Mean	0.76	0.86	0.62	0.79
Observations	54687	37364	22701	7306

Notes: The sample includes 19 states and 1 UT. Demographic controls include type of residence, household size, number of children under 5 in the household, number of siblings, gender of the child, birth year dummies, caste and religion of the household head, mother's education and father's education. Robust standard errors (clustered at district level) are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

being enrolled in a particular grade at a specific age. Panel A of Table 2.4 shows that the ITT effect of MDMS on the probability of enrollment and Panel B shows he LATE estimates. Table 2.4 shows that the MDMS policy increased the on-time enrollment in all grades, and the LATE estimates are much larger than the ITT estimates for all grades. The positive and significant effect along the diagonal of the matrix suggests intensive margin response, i.e. more children continuing education on-time as a result of the MDMS.[18]

2.4.2 Heterogeneity in Results

The evidence in the existing literature suggests that girls benefit more than boys from the mid day meal program and the policy impact is also higher for the more disadvantaged populations such as SC/STs or OBCs. To test this hypothesis, Tables 2.5-2.6 show the heterogeneity in results by gender and with caste.

The results from Table 2.5 suggest that the program impact is larger for girls than boys. While the effect of the program is positive and significant for both boys and girls, the effect for girls, on average, is more than twice as large as the effect on boys. This difference in the magnitude is smaller for net enrollment in grade 1. This suggests that while the program is effective in increasing enrollment for all children, it has larger effects on girls in terms of

[18]Table 2.4 also shows that for some grades the probability of enrollment of older children increased, but the magnitude of the effect was usually smaller. Given data constraints, it is not possible to say whether this increase is due to children repeating grades, or enrollment of out-of-school children back into schools due to the incentives of MDMS.

Table 2.4: AgeXGrade Transition Matrix

	Grade 1	Grade 2	Grade 3	Grade 4	Grade 5
	Panel A: Reduced Form (ITT)				
6 years	**0.11**				
	(0.05)				
7 years	**0.05**	**0.11**			
	(0.02)	**(0.06)**			
8 years	0.01	**0.18**	**0.24**		
	(0.04)	**(0.04)**	**(0.08)**		
9 years	**-0.49**	0.03	0.05	**0.17**	
	(0.10)	(0.06)	(0.05)	**(0.08)**	
10 years or older	0.15	**0.15**	**0.21**	**0.14**	**0.07**
	(0.12)	**(0.05)**	**(0.02)**	**(0.02)**	**(0.02)**
	Panel B: 2SLS (LATE)				
6 years	**0.47**				
	(0.13)				
7 years	**0.26**	**0.48**			
	(0.10)	**(0.21)**			
8 years	0.10	**0.88**	**0.86**		
	(0.24)	**(0.19)**	**(0.26)**		
9 years	**-4.01**	0.19	0.23	**0.70**	
	(1.37)	(0.30)	(0.19)	**(0.25)**	
10 years or older	1.08	**1.22**	**1.41**	**0.64**	**0.28**
	(0.88)	**(0.30)**	**(0.20)**	**(0.10)**	**(0.11)**

Notes: Each cell denotes coefficient for the effect of MDMS on probability of being enrolled of a child of age X in grade Y. Standard errors are in parentheses. Coefficients in bold are significant at 10% or smaller level of significance.

continuing education at primary school level.

Table 2.6 presents the estimates for different castes. The results suggest that the impact on disadvantaged populations (including SCs, STs and OBCs) is statistically higher than the effect on other higher caste. A t-test does not reject the equality of coefficients of SC/STs and OBC, but rejects the equality of coefficients of SC/ST and other (higher) castes, and coefficient of OBC and other castes at less than 0.1 percent level of significance. This heterogeneity in results is not surprising as the disadvantaged groups belong to the more

Table 2.5: Heterogeneity by Gender

	Primary School		Grade 1	
	Gross Enrollment	Net Enrollment	Gross Enrollment	Net Enrollment
	Panel A: Reduced Form (ITT)			
Girls*Treat*Post	0.092***	0.092***	0.094***	0.128**
	(0.018)	(0.022)	(0.029)	(0.052)
Boys*Treat*Post	0.026	0.036*	0.013	0.098*
	(0.018)	(0.021)	(0.027)	(0.051)
p-value for equality	0.00	0.00	0.00	0.05
	Panel B: 2SLS (LATE)			
Girls*Fraction Treated	0.351***	0.354***	0.350***	0.511***
	(0.081)	(0.080)	(0.129)	(0.179)
Boys*Fraction Treated	0.139*	0.174**	0.116	0.419**
	(0.081)	(0.076)	(0.123)	(0.174)
p-value for equality	0.00	0.00	0.00	0.04
Demographic Controls	✓	✓	✓	✓
District Fixed Effects	✓	✓	✓	✓
Mean Enrollment for Girls	0.72	0.83	0.55	0.77
Mean Enrollment for Boys	0.81	0.88	0.69	0.81
Observations	54687	37364	22701	7306

Notes: The sample includes 19 states and 1 UT. Demographic controls include type of residence, household size, number of children under 5 in the household, number of siblings, gender of the child, birth year dummies, caste and religion of the household head, mother's education and father's education. Robust standard errors (clustered at district level) are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.6: Heterogeneity by Caste

	Primary School		Grade 1	
	Gross Enrollment	Net Enrollment	Gross Enrollment	Net Enrollment
	Panel A: Reduced Form (ITT)			
SC/ST*Treat*Post	0.081***	0.100***	0.080***	0.162***
	(0.020)	(0.022)	(0.030)	(0.052)
OBC*Treat*Post	0.085***	0.089***	0.083***	0.139***
	(0.020)	(0.023)	(0.029)	(0.053)
Other Castes*Treat*Post	0.013	0.016	-0.003	0.059
	(0.019)	(0.023)	(0.029)	(0.051)
F-test for equality	0.00	0.00	0.00	0.00
	Panel B: 2SLS (LATE)			
SC/ST*Fraction Treated	0.310***	0.368***	0.335***	0.616***
	(0.081)	(0.076)	(0.122)	(0.181)
OBC*Fraction Treated	0.334***	0.339***	0.347***	0.540***
	(0.079)	(0.079)	(0.121)	(0.184)
Other Castes*Fraction Treated	0.115	0.122	0.103	0.309*
	(0.077)	(0.078)	(0.115)	(0.178)
F-test for Equality	0.00	0.00	0.00	0.00
Demographic Controls	✓	✓	✓	✓
District Fixed Effects	✓	✓	✓	✓
Mean Enrollment for SC/ST	0.73	0.82	0.58	0.73
Mean Enrollment for OBC	0.74	0.84	0.57	0.77
Mean Enrollment for Other Castes	0.81	0.89	0.68	0.84
Observations	54687	37364	22701	7306

Notes: Notes: The sample includes 19 states and 1 UT. Demographic controls include type of residence, household size, number of children under 5 in the household, number of siblings, gender of the child, birth year dummies, caste and religion of the household head, mother's education and father's education. Robust standard errors (clustered at district level) are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

impoverished sections of the populations and have low initial enrollment rates. Thus, a larger response of the mid day meal program on these demographic groups is in line with our expectations. These results also align well with the results of other MDMS studies (Jayaraman and Simroth 2011).

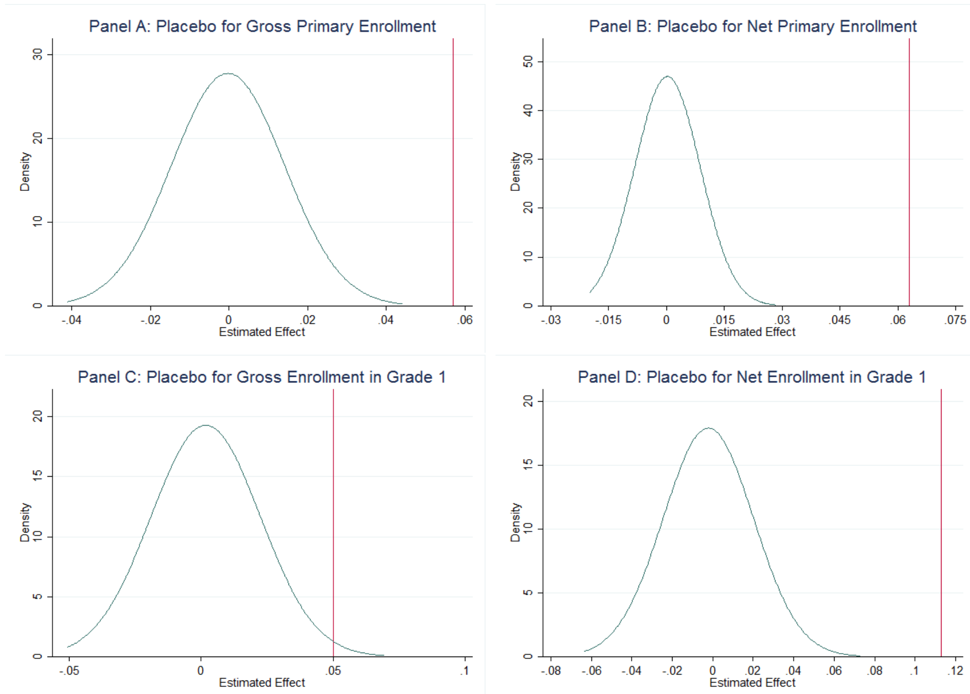
2.4.3 Placebo Test

To ensure that the results presented are not generated by chance, I perform a placebo test. Using data from control states and the states which were excluded from the sample, I randomly assign treatment to the same ratio of individuals as the original sample. Then I run the regressions for all the outcomes using this new dataset and store the estimates. This process is repeated 500 times and the results are summarized in Figure 2.5. Figure 2.5 plots the distribution of the estimates produced using the randomization and the vertical line depicts the actual coefficient estimated using the original sample. The figure shows that the true estimate is always above the 95th percentile of the placebo distribution. Except for Panel C, none of the placebo regressions estimated an effect which is larger than the actual estimates obtained in the analysis. This presents strong evidence that my estimates showing that the program led to an increase in probability of primary and grade 1 enrollment are not generated by chance.

2.5 Discussion

The evidence presented in this paper emphasizes that the ITT effects underestimate the true impact of a policy, especially if the program implementation is not uniform. The estimates for primary enrollment predict a 30-32% increase in primary enrollment, which is higher than 13-23% effect found in

Figure 2.5: Placebo Test



Note: Figure 4 plots the results for the Placebo regressions described in Section 5 for gross primary enrollment (Panel A), net primary enrollment (Panel B), gross enrollment in grade 1 (Panel C) and net enrollment in grade 1 (Panel D). The figure plots the distribution of placebo estimates generated using sample of states that were either the control group or were excluded from the sample. The vertical red line corresponds to the actual estimates from the analysis.

the literature. For enrollment in grade 1, the difference between the ITT and LATE estimates is much larger. The estimates from Table 2.3 show a 39-59% increase in the probability of enrollment in grade 1 compared to 15-36% enrollment increase found in the literature (Khera 2006). The large magnitude of impact though surprising, is not completely out-of-line in context of South Asian countries. A study by WFP showed over 100% increase in overall enrollment and about 200% increase in first grade enrollment of girls as a result of a take-home ration program in Pakistan between 1998/99 and 2003/04 (WFP Pakistan 2005). Moreover, unlike most existing studies that focus on children already in school (Adelman, Gilligan, and Lehrer 2008), this paper uses individual level household survey data to the extensive margin response. The large magnitude of LATE estimate for MDMS could be partially attributed to the choice of the outcome variable. Enrollment response might be much higher than the attendance response (outcome largely is studied in the literature) if the officially enrolled students did not regularly attend classes.

2.5.1 Other Similar Policies

If other policies were introduced during 1999-2005 and affected treatment and control states differentially, then the LATE estimate might capture a combined effect of all such policies. A key contender for such a policy in India is the Sarva Shiksha Abhiyan (SSA). SSA was introduced in 2001-02 and aimed to achieve universal elementary education. Most of the initiatives under SSA were focused on providing a better quality of education. SSA program implementation model included building new schools in regions with no public schools, improving the existing infrastructure of the schools, hiring more teachers and increasing grants for developing better course materials. Alternative schooling models such as the Alternative and Innovative Education (AIE) centers, mobile schools and Education Guarantee Scheme (EGS) schools were built to target out-of-school children.

Though the timing and objective of SSA overlap with the MDMS, it is unlikely that the SSA affected the treated and the control states differentially. However, new school established under the SSA could have contributed to the estimated increase in enrollment. A preliminary analysis of IHDS school level data (2005) suggests that less than two percent of sample public schools were opened after 2001. Also, the dataset used in this analysis does not include any EGS schools. Therefore, the presence of new schools driving the estimates seems unlikely. Nevertheless, it is plausible that the changes in infrastructure introduced by SSA might have coincided with the introduction of mid day meals in schools. To rule out this this concern, I rely on Jayaraman and Simroth's result, whereby they showed that schooling inputs such as classrooms, water, electricity, blackboards, teachers, etc. did not change differentially at the same time as the introduction of the mid day meal. To corroborate this, I use the IHDS school level data (2005) to estimate a correlation between the provision of free meals and school infrastructure in public schools. The results suggest a correlation coefficient smaller than 0.12. These findings, together with Jayaraman and Simroth's results, suggest that it is unlikely that my estimates are capturing the impact of the combined effects of SSA and MDMS.

Another potential concern is that if the program was rolled out first in districts with larger share of disadvantaged population, the impact estimated may be local to the level of implementation. The margin for response to welfare programs is higher in such backward districts. Thus, the impact estimate might become smaller as the program expands to other districts. Figure 2.6

plots the fraction treated against the fraction of disadvantaged population in a district. As is evident from Figure 2.6, fraction treated does not vary systematically with the proportion of backward population in a district. Hence, I do not find any evidence of systematic differences in implementation w.r.t. population demographics of a district.

Figure 2.6: Relationship between fraction treated and proportion of disadvantaged population

Note: Disadvantaged population is defined as the proportion of population belonging to the SC/ST/OBC group in a district. Fraction Treated is defined as the fraction of population getting free meals in schools at the district level. The graph is plotted for data in 2005 and sampling weights are used.

2.5.2 Back-of-the-Envelope

For any public policy or welfare program, it is important to weigh the costs against benefits and assess its viability and sustainability. However, in the case of education policy, it is difficult to measure the intrinsic social value of universal education. In this section, I discuss the benefits of SFPs and present a rough estimate of benefit-to-cost ratio of the MDMS using some back-of-the-envelope calculations.

The benefits from SFPs could be classified into three key categories: direct benefits to the beneficiary, benefits to the family of the beneficiary, and benefits to the society (externalities). Direct benefits to the beneficiary include better health and education resulting in increased employment opportunities in the future, higher productivity, better cognitive and non-cognitive skills and improved psychological well-being (WFP, 2013). Benefits to the family include reduced cost of schooling, reduced healthcare expenditure and household food security (reduced expenditure on food). Beside these direct benefits, school feeding programs have been associated with various indirect social benefits such as gender equity, better literacy rates, less child labor, poverty alleviation, and reduced crime rate (WFP, 2013). These indirect benefits are usually difficult to measure, making it harder to precisely estimate a cost-benefit ratio.

For benefit calculations, I restrict my analysis to the benefit that accrues from the increased expected wage resulting from a higher probability of being educated. I use the National Sample Survey of India (NSS), 2004-05, a nationally representative dataset, to obtain an estimate of benefit from increased enrollment probability. I assume that enrollment into primary school results in a positive probability of continuing education beyond first grade. I calculate the probability of schooling, separately for different levels of education. I also estimate the daily wage of workers with different levels of education. I use these estimates to obtain an expected value of the daily wage for males and females, conditional on enrolling in school. Using the estimates from this paper, the back-of-the-envelope calculations provide an estimate of expected annual wage benefit of about $14-17.[19]

The cost estimate is obtained by dividing the budgetary allocation for MDMS by the number of beneficiaries in 2005. The annual per child cost is estimated as $5.72. The resulting benefit to cost ratio is between 2.6:1 to 3:1, which is similar to the benefit-cost ratio of 4:1 derived in an analysis by Boston Consulting Group and WFP.[20] An important point to note here is that these calculations do not account for any social benefits or benefits accruing to the family and hence underestimate the true benefit of the program. Keeping in mind that the benefit estimate is a lower bound for true benefits of the program, the back-of-the-envelope calculation suggests that MDMS is a cost-effective and socially beneficial welfare policy.

2.6 Conclusion

Given the increasing popularity of school feeding programs in both developed and developing world, it is imperative to evaluate their impact to facilitate policy decisions. This paper contributes to the existing literature on impact evaluation of free lunch programs by explicitly incorporating the implementation imperfections in the analysis. The empirical evidence presented

[19] The exchange rate used is 43.487INR for 1USD (2004 rate), available at `http://www.forecasts.org/data/data/EXINUS.htm`

[20] Presentation available at http://documents.wfp.org/stellent/groups/public/documents/newsroom/wfp208643.pdf

suggests a positive and significant impact of the program on the probability of primary school enrollment. The ITT effect estimates a 5.5% (7.3%) and the LATE estimates a 32% (30%) increase in the gross (net) primary school enrollment. Analysis of on-time enrollment suggests that the program had a large effect on enrollment in grade 1, suggesting a large extensive margin response. I also find that the effect is larger for the socially disadvantaged groups as well as girls.

When interpreting the results, it is important to keep in mind the limitations of this analysis. First, due to the lack of identifiers for type of school (public or private) that the individual attends, the estimates might be biased as the sample may assign students who are not treated (as they attend private schools) to the treatment group. This will push the estimates towards zero, underestimating the true impact. Second, this paper does not distinguish between the different kinds of treatment - cooked meals versus uncooked meals, which might affect enrollment incentives differently. Third, due to the lack of data on attendance and cognitive outcomes such as test scores, the analysis does not incorporate other important educational gains that might be accrued from the program. It will be insightful to incorporate effects of differential types of treatment on enrollment and long-term educational outcomes in future research.

Chapter 3

College Costs and Educational Choices of Undocumented Immigrants: Evidence from Texas [1]

College cost is an important factor in post-secondary education decisions. College costs not only influence enrollment but also affect other important education decisions such as choice of major, type of degree, and timely graduation (Leslie and Brinkman 1988, Cabera and Nasa 2000, Denning 2014). These educational choices have long lasting effects on labor market outcomes, health, marriage and personality traits (Oreopolous et al. 2009). Since disadvantaged socio-economic subgroups are particularly sensitive to the price of college, changes in college costs can have a significant influence on their education decisions (Flores 2010a). In this paper, I study the impact of reduced college tuition on educational decisions of undocumented immigrants.

About 65,000 undocumented children, who were not born in the US but have been living in the US for over five years, graduate from American high schools every year.[2] While 25-30 percent of all 16 to 24 year-olds enroll

[1]The conclusions of this research do not necessarily reflect the opinion or official position of the Texas Education Research Center, the Texas Education Agency, the Texas Higher Education Coordinating Board, the Texas Workforce Commission, or the State of Texas.

[2]Undocumented refers to a foreign-born person without proper authorization or legal

in some college, only 10 percent of the undocumented immigrants in this age group do so (The UndocuScholars Project Report, 2015). One explanation for this enrollment gap is the high cost of postsecondary education. Unlike their US-born peers, the children of undocumented immigrants do not qualify for resident tuition, making college costs prohibitively high. In 2001, Texas became the first state to pass a policy allowing in-state tuition to undocumented immigrants (House Bill 1403). Currently, 20 states have legislation allowing undocumented children to qualify for in-state tuition.[3]

In this paper I estimate the effect of changes in college costs resulting from the in-state resident tuition reforms on educational decisions of undocumented immigrants in Texas. Specifically, I employ a difference-in-differences technique to estimate the impact of in-state tuition on probability of graduation, type of degree and choice of major for undocumented immigrants. Comparing undocumented Hispanic immigrants to the U.S. born Hispanics, I find that the reduced college costs significantly increased the graduation rate for undocumented immigrants enrolled in community colleges, but had no significant impact on students in four-year universities. The policy increased the probability of graduating with an associate degree or an advanced certificate, and the probability of graduating with an academic major from community colleges.[4] I also find that the policy had a larger impact for males than fe-

basis of residence in the United States. I use the terms undocumented and unauthorized interchangeably. The terms Latino and Hispanic are also used interchangeably. For additional information on these terms, see Bean and Lowell (2007).

[3]Source: `http://www.ncsl.org/documents/immig/InStateTuition_july212015.pdf`

[4]Majors in community colleges are categorized into academic, technical or technical-prep

males. The effect of the policy on timely graduation is sensitive to the choice of specification. The evidence presented in this paper suggests that the in-state resident tuition policy reduced the gap in opportunities for higher education, but its impact was primarily concentrated among students enrolled in community colleges.

This paper makes three important contributions to the literature. First, this paper extends the analysis of the effect of in-state tuition policy beyond enrollment to examine other important decisions such as major choice, degree obtained, and timely graduation. Although the in-state resident tuition policy has received much attention from various fields, most of these studies are qualitative in nature (Olivas 2004, 2008, Batalova and Fix 2006, Feder 2006, Perry, 2006, Castillo 2007, Flores and Chapa 2009), and quantitative evidence on the effect of such law changes on education outcomes is limited to enrollment (Kaushal, 2008, Chin and Juhn 2010, Flores 2010a, b, Amuedo-Dorantes and Sparber 2014, Potochnick 2014). Second, existing literature studying undocumented immigrants suffers from high measurement error due to lack of identifiers for the illegal immigration status in government surveys. Using unique administrative data from Texas allows me to identify undocumented immigrants with higher precision. Third, the existing literature has largely examined the impact of the in-state tuition policy on enrollment outcomes in any college, without distinguishing between the type of colleges and degrees (Flores 2006, Chin and Juhn 2010). Given the substantial differences across

major.

the cost of 2-year and 4-year public colleges, it is important to examine the effect of the policy on different types of institutions separately. Thus, another important contribution of this paper is to separate the effect of the policy on different channels of postsecondary education, i.e. 4-year public university degree and community colleges.

The rest of the paper is organized as follows. Section 3.1 discusses the institutional background details of the in-state tuition law of Texas. Section 3.2 discusses of the empirical strategy, followed by a description of the data in Section 3.2. Section 3.3 presents the empirical results. Lastly, Section 3.5 concludes.

3.1 Institutional Background

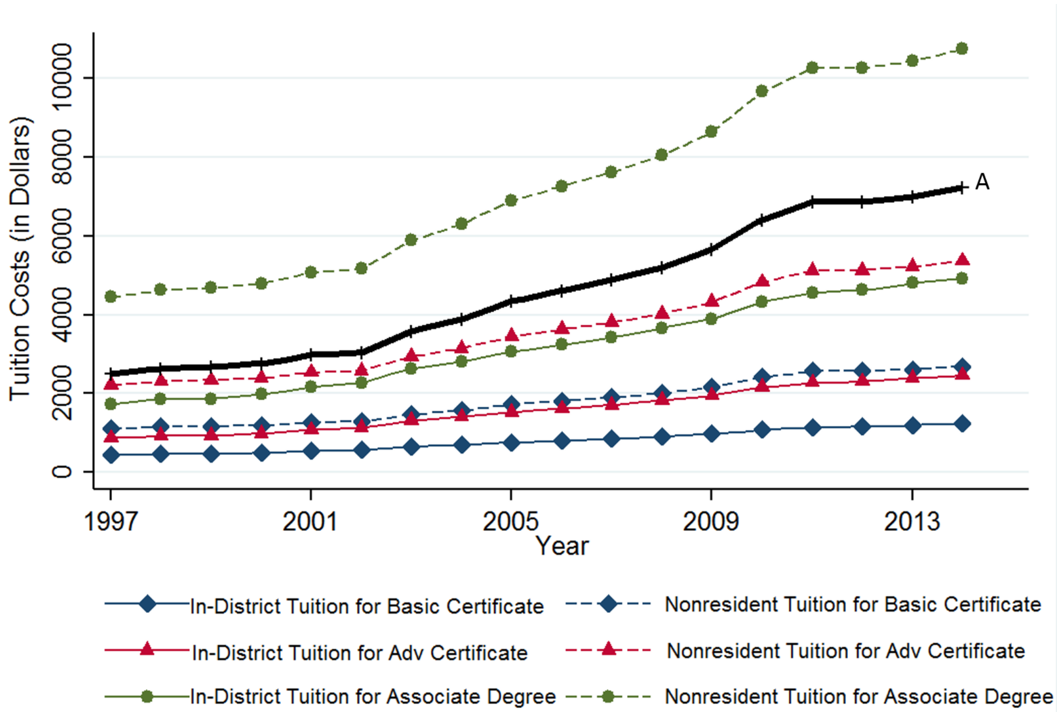
Education attainment rates for undocumented immigrants are lower than their US-born peers at all levels of education. Just 54 percent of undocumented youth have at least a high school diploma, compared to 82 percent for their US-born peers (Passel and Cohn, 2009). Among high school graduates, only 5-10 percent enroll in postsecondary institutions, and far fewer graduate with a degree (Department of Education Report, 2015). Financial barriers have been noted as one of the key explanations for the low education attainment among undocumented youth. Undocumented immigrants are ineligible for Title IV federal financial aid, making college education unaffordable for them. To improve college access, some states and public universities have passed policies allowing in-state resident tuition to undocumented youth.

In 2001, House Bill 1403 was passed in the Texas Senate, which allowed in-state resident tuition to any student that met the following criteria: (i) must have graduated from a public or private high school or received an equivalent of a high school diploma in Texas, (ii) must have resided in the state for at least 3 years as of high school graduation date or the date when they received equivalent of a high school diploma, (iii) must register as an entering student in an institution of higher education not earlier than the 2001 fall semester, and (iv) must provide the institution an affidavit stating that the individual will file an application to become a permanent resident at the earliest opportunity the individual is eligible to do so (HB 1403, 77th Leg., Reg. Sess. [Tex. 2001]).

While the in-state tuition reform is aimed at reducing the gap in opportunity for education, it does not address the gap between returns to education for undocumented immigrants and their U.S. born peers. The federal law prohibits employers from hiring an undocumented immigrant, making their labor market returns to college education lower compared to US-born students. However, hiring an independent contractor without seeking proof of immigration is within the bounds of the legal framework. Thus, postsecondary degrees which facilitate obtaining professional licenses for self-employment offer better labor market opportunities for undocumented immigrants. Given that the community colleges offer low-cost options of professional courses compared to most 4-year universities, I expect a greater strategic response to the change in college costs for students who attend community colleges.

Recent studies have found that the choice of major is sensitive to the price of college education (Stange 2012, Denning and Turley 2016). The cost of college is a function of the total number of credit hours taken, and different degrees/majors require a different number of credits. As a result, students face differential pricing based on their major and degree choice. For instance, the cost of getting an associate degree is substantially higher than getting a professional certificate. These differences get accentuated if a student has to pay nonresident tuition. Figure 3.1 plots the tuition costs for getting a certificate, advanced technical certificate, and an associate degree. As shown in the figure, the nonresident tuition for a certificate course is very similar to in-district tuition for an advanced technical certificate. Similarly, the nonresident tuition for an advanced technical certificate is very similar to in-district tuition for an associate degree. If students are financially constrained, qualifying for in-state resident tuition could allow them to afford previously unaffordable degrees. To illustrate this point, consider a student whose financial affordability is represented by line A. In the absence of in-state tuition policy, this student can at best afford an advanced technical certificate, but cannot afford an associate degree. However, with the in-state tuition policy, this student can now afford to get an associate degree. Thus, the change in college costs would not only affect enrollment, but are likely to influence the choice of degrees for students enrolled in colleges. In Section 3.4, I analyze the impact of the in-state tuition law on the probability of graduating with different types of degrees and majors.

Figure 3.1: Trends in College Tuition



Notes: This figure plots the trends in average college tuition for different degrees/certificate in Texas community colleges over time period 1997-2014. The solid lines represent average in-district tuition and dashed lines represent average nonresident tuition. The green series with circles shows average tuition costs for associate degree, with an average requirement of 60 semester credit hours (SCH). The red series with triangles shows average tuition costs for advanced technical certificates, with an average requirement of 30 SCH. The blue series with diamonds shows average tuition costs for basic certificates, with an average requirement of 15 SCH. Source: Statistics from Texas Association of Community Colleges.

3.2 Empirical Strategy

To estimate the causal effect of the tuition changes on educational choices, I employ the difference-in-differences technique using US-born Hispanics as the control group and the undocumented Hispanic immigrants as the treatment group. This empirical method will allow me to separate the time trends of the educational outcomes that would have existed in the absence of the policy. I estimate the following equation:

$$Y_{it} = \alpha_0 + \alpha_1 X_{it} + Post_t + UI_i + Post_t * UI_i + e_{it} \tag{3.1}$$

Where Y_{it} is the educational outcome for individual i at time t. The outcomes studied include probability of graduation, probability of graduating on time, graduating with an associate degree, an advanced certificate, or a basic skill certificate, and choice of major. X_{it} is a vector of control variables which includes an indicator for academic disadvantage, disability, if they are single parent, gender, age, limited English proficiency, and dummies for institute and year.[5] $Post_t$ is an indicator that takes value of 1 for years after the in-state policy was introduced, i.e., 2001 onwards, and 0 otherwise. UI_i is a variable used to identify if an individual is an undocumented immigrant. Identifying undocumented immigrants in the sample has been one of the long-standing limitations of the existing literature. In the literature, undocumented immigrants are usually defined as those individuals who are not US citizens.

[5]Academic disadvantage is an indicator variable which captures whether a student has college entry level skills in reading, writing or math, based on a local placement test.

In this paper, I use the following alternative definitions for the undocumented immigrant variable.

Definition 1: UI_i is as an indicator which takes the value of 1 for international Hispanic students who would pay nonresident tuition in the absence of the policy, and 0 for all other Hispanic students.[6] This definition is similar to the one used in the literature and categorizes all immigrants (documented as well as undocumented) as undocumented immigrants.

Definition 2: UI_i is defined as a predicted probability of being undocumented. Section 3.3 describes how this predicted probability is calculated.[7]

3.3 Data

In this paper, I use the individual-level administrative data collected by the Texas Education Resource Center (ERC). The ERC sources data from the Texas Higher Education Coordinating Board (THECB), which includes detailed information on the tuition status, waivers, majors, degree awarded, and demographic information for all students at Texas community colleges and public universities. Unlike the survey data used in the existing literature, the ERC administrative data has the unique advantage of identifying undocumented immigrants using markers for in-state tuition eligibility under the

[6]For years prior to 2001, these are students who paid nonresident tuition and after 2001, this includes students who pay nonresident or in-state under HB 1403.

[7]I use another definition in which UI is an indicator that takes value of 1 if the predicted probability of being undocumented is greater than 0.5, and 0 otherwise. The results (not presented here) are similar in sign and significance.

House Bill 1403, which is usually utilized for undocumented immigrants.

For my analysis sample, I use the students who enrolled as freshman in public colleges between 1995-2013 for community colleges, and 1995-2011 for four-year public universities.[8] I restrict my primary sample to include only Hispanics. Undocumented Hispanics constitute the treatment group and US born Hispanics form the control group. For year 2001 onwards, the data includes an identifier for individuals who are exempted from nonresident tuition under HB1403. Using the status of the tuition paid, I classify Hispanic students into US born (if they pay resident tuition) and undocumented Hispanics if they pay resident tuition under HB 1403. However, this identifier is available only after the year 2001.

To be able to classify students into different categories by residency status for the entire time-period (1995-2011), I predict the probability of a Hispanic student being an immigrant based on their place of residence, age intervals, whether they have a valid social security number, and whether they are internationals who pay or would have paid nonresident tuition in the absence of the policy.[9,10] I regress the subsample of years 2001-2013, for which I

[8]While the data is also available for students who enrolled until 2015, I use the information on students who are scheduled to graduate by 2015. Thus, for two-year colleges, I use data for students enrolled up until 2013, and for four-year universities, I use data for students enrolled up until 2011.

[9]Place of residence variable includes dummies for counties for those living in border counties, a dummy for those who live in non-bordering Texas counties, a dummy for students from Mexico, a dummy for international students, and a dummy for non-Texas US resident.

[10]As per the Social Security Administration, an individual needs to be a citizen or a documented immigrant to receive a social security number. In 2012, Deferred Action for Childhood Arrivals (DACA) allowed temporary work permits to be given to undocumented

have identifiers for undocumented immigrants, on these parameters, and then use the estimated coefficients to predict the probability of being undocumented for the entire sample. Panel A of Table 3.1 shows that the summary statistics for these characteristics for undocumented and US born Hispanic students. As is evident from this table, undocumented immigrants are more likely to not have a social security number and are more likely to be from Mexico than US born Hispanics.[11]

Note that the sample used only includes individuals who are already enrolled in college. Hence, the analysis of educational choices of undocumented immigrants is conditional on enrollment. According to the literature, the in-state resident tuition policy increased enrollment in college. Although I cannot estimate the effect of the in-state tuition law on enrollment because of data constraints, summary statistics from the data show that share of undocumented immigrants in community colleges increased from 0.5 percent in 2001 to 5.2 percent in 2013. Given the increase in share of undocumented immigrants in colleges, there is a concern that compositional effects could be driving the results. While I add demographic controls to alleviate this concern, the results should be interpreted with caution.

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immigrants who moved to the US as children. My results remain similar if I exclude the students who were enrolled in college after DACA was passed.

[11]To confirm that this model reasonable predicts the probability of being undocumented, I used a random 80 percent subsample for the 2001-2013 period to run the model, and used the estimated coefficients to predict the probability of the remaining 20 percent of the sample. This analysis found that anyone who had a predicted probability of higher than 0.85 was indeed an undocumented immigrant.

Table 3.1: Summary Statistics

	Community Colleges		4-Year Public University	
	Undocumented Immigrants	US-born Hispanics	Undocumented Immigrants	US-born Hispanics
Panel A				
Invalid SSN	0.76	0.04	0.76	0.05
Texas counties	0.84	0.99	0.85	0.94
Non-Texas US counties	0.00	0.00	0.00	0.01
Outside of US	0.16	0.00	0.15	0.06
From Mexico	0.13	0.00	0.12	0.01
Panel B				
Age	19.21	20.78	18.34	18.73
Female	0.53	0.56	0.53	0.53
Academically Disadvantaged	0.65	0.56		
Disable	0.01	0.02		
English Language Proficiency	0.07	0.09		
Displaced homemaker	0.01	0.02		
Single Parent	0.02	0.06		
Number of Observations	36,985	1,090,973	4,270	217,971

Notes: Sample in Columns 1-2 include students enrolled in community colleges from 2001-2013, and sample in COlumns 3-4 include students enrolled in 4-year public universities from 2001-2011 in Texas. The unit of observation is an individual. Information on demographics such as disability and single parent is not available for students enrolled in public universities.

3.4 Empirical Findings

The key assumption in this analysis is that in the absence of a policy change, the US-born Hispanics will follow the same trend in their educational choices as that of the undocumented immigrants. To check whether the US-born Hispanics are a reasonable control group, Table 3.1 provide summary statistics of the observable characteristics of undocumented immigrants as well as US-born Hispanics. Panel B of Table 3.1 shows that the students from both the subgroups are similar in observable characteristics, except that undocumented Hispanic immigrants are younger and are more likely to be academically disadvantaged. In the analysis, I will control for all these observable characteristics.

Figure 3.2 and Figure 3.3 plot trends in the outcome variables for US-born Hispanics and undocumented students enrolled in community colleges. Figure 3.2 shows the trend for undocumented immigrant based on Definition 1, i.e., all non-citizens are assumed to be undocumented immigrants. For Figure 3.3, I use the predicted probability of being undocumented as per Definition 2. If the probability of being undocumented exceeds the sample mean of the predicted probability, then the student is categorized undocumented immigrant, and s/he is otherwise categorized as a US-born Hispanic. As shown in these figures, prior to the policy change, the trends for educational choices of undocumented Hispanic immigrants are similar to those of the US-born Hispanics, supporting the aforementioned parallel trend assumption. The probability of graduation, probability of graduating with associate degree and basic certificate for undocumented immigrants jumps at 2001, when the policy was implemented. Figures 3.2 and 3.3 suggest that the reduced tuition costs reduced educational gaps of undocumented immigrant students in community colleges.[12]

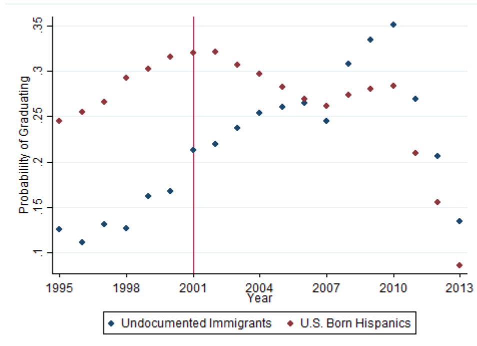
3.4.1 College Completion

Table 3.2 shows the results for estimating equation. The regressions include demographic controls such as age, gender, academic disadvantage, limited English language proficiency, and year fixed effects. Panel A shows the regression results using the first definition of undocumented immigrants, where any international student paying nonresident tuition is recorded as an undocumented immigrant. Panel B shows the results using Definition 2, i.e., the predicted probability of being an undocumented immigrant. Column 1 shows that the in-state tuition policy increased the probability of graduation for undocumented immigrants enrolled in community colleges by 12-14 pp. In contrast, Column 3 of Table 3.2 shows that the policy had no economically or statistically significant effect on the completion of degrees from four-year colleges. Given the larger cost and the lower returns of a four-year college degree for undocumented immigrants, it is not surprising that the in-state tuition policy did not have any impact on completion of four-year college degree.

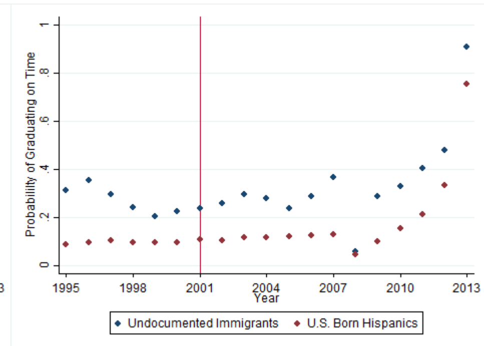
The estimates for probability of graduating on time shown in Column 2 and Column 4 are sensitive to the definition of the key independent variable.

[12]Plots for four-year public universities are similar and available upon request.

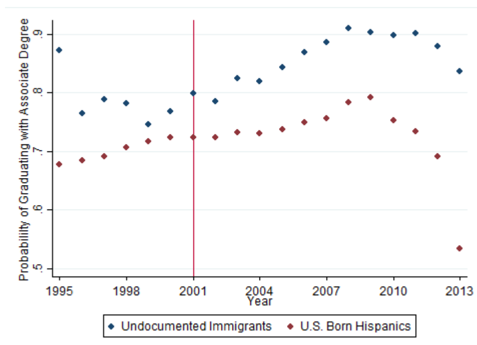
Figure 3.2: Trends for US-born Hispanics and Undocumented Immigrants (Based on Definition 1)



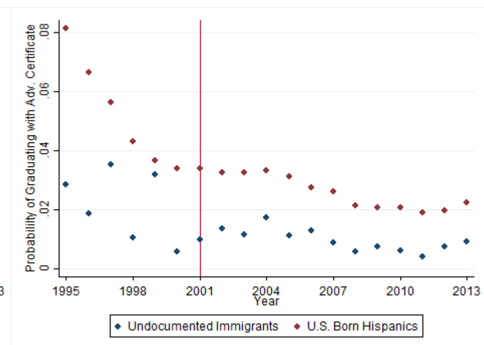
A: Graduation



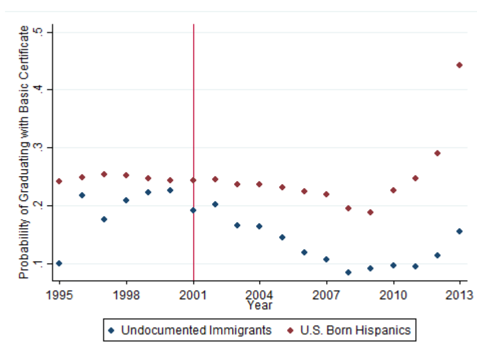
B: On-Time Completion



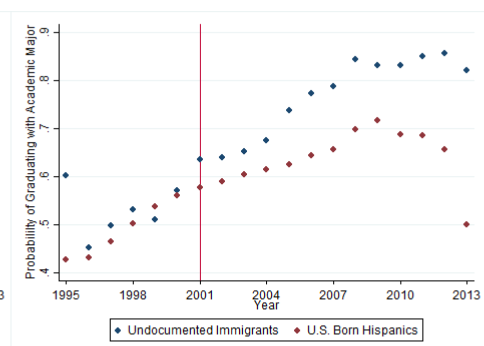
C: Graduating with Associate Degree



D: Graduating with Adv Certificate



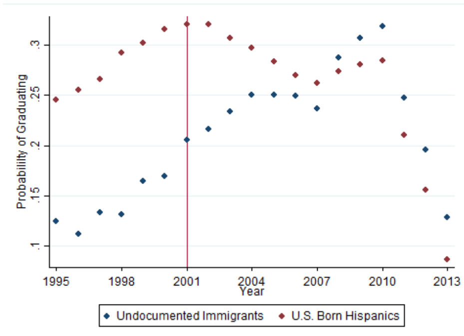
E: Graduating with Basic Certificate



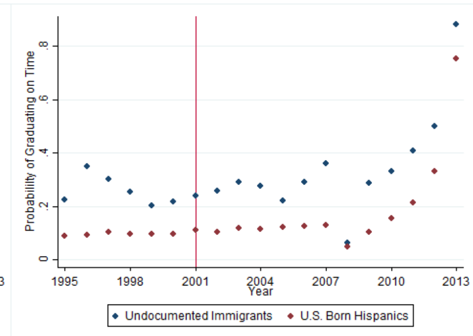
F: Graduating with Academic Major

Notes: This figure plots the time trends in the outcomes for US born Hispanics and the undocumented Hispanic immigrants enrolled in community colleges. Undocumented immigrants in this figure are defined using Definition 1, i.e., as all US noncitizens who paid nonresident tuition are categorized as undocumented immigrants.

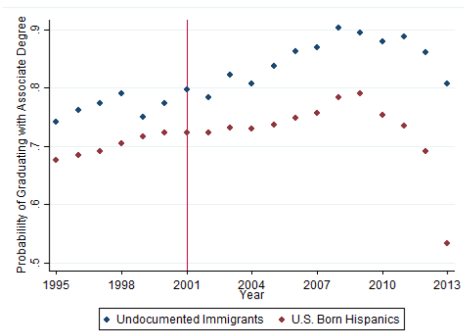
Figure 3.3: Trends for US-born Hispanics and UI (Based on Definition 2)



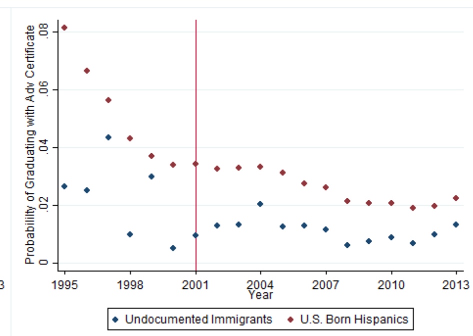
A: Graduation



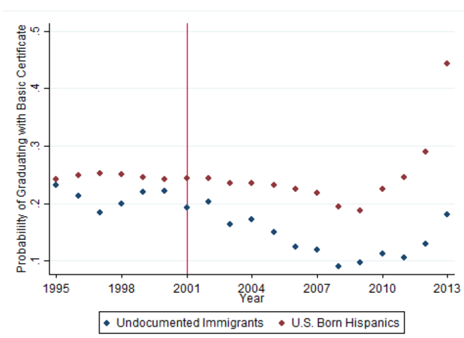
B: On-Time Completion



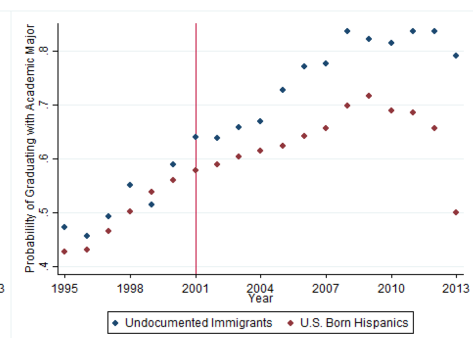
C: Graduating with Associate Degree



D: Graduating with Adv Certificate



E: Graduating with Basic Certificate



F: Graduating with Academic Major

Notes: Undocumented immigrants in this figure are defined based on the predicted probability of being undocumented calculated using region dummies, valid social security number, age interval for the student, and whether they are internationals who pay or would have paid nonresident tuition in the absence of the policy. The students who predicted probability is equal to or higher than the sample mean are categorized as undocumented immigrants, and others are categorized as US-born Hispanics.

Table 3.2: Regression Results

	Community College		4-year Public University	
	Graduation	On-time Completion	Graduation	On-time Completion
	(1)	(2)	(3)	(4)
Panel A: UI is defined as US noncitizens				
Post 2001*UI	0.139***	-0.005	0.007	-0.083***
	(0.003)	(0.009)	(0.009)	(0.016)
UI	-0.114***	0.143***	-0.146***	0.244***
	(0.003)	(0.009)	(0.007)	(0.012)
Mean of Dep Var	0.14	0.25	0.31	0.71
Panel B: UI is defined using Predicted Probability of Undocumented Immigrants				
Post 2001 * UI	0.122***	-0.037***	-0.005	-0.023
	(0.003)	(0.010)	(0.008)	(0.020)
UI	-0.158***	0.186***	-0.273***	0.189***
	(0.003)	(0.010)	(0.007)	(0.016)
Mean of Dep Var	0.27	0.10	0.42	0.60
Observations	1687230	429091	332025	139159

Notes: The unit of observation is an individual. Demographic controls include: student's age and gender, an indicator for academic disadvantage, an indicator for whether they are a single parent, diability, English language proficiency, and dummies for institute and year. For Columns 3-4, demographic controls do not include an indicator for academic disadvantage, an indicator for whether they are a single parent, diability and English language proficiency. Each Column correspond to an outcome. In Panel A, UI is defined as an indicator for US noncitizens, and in Panel B, UI is defined as the predicted probability of being an undocumented immigrant. Columns 1-2 use a sample of freshman year students enrolled in community colleges during 1995-2013. Columns 3-4 use a sample of freshman year students enrolled in four-year public universities during 1995-2011. Robust standard errors are in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Panel A shows a statistically insignificant effect of the policy on graduating from community colleges, but a negative effect of the policy on on-time completion of a bachelor degree. Panel B shows that the in-state tuition policy had a negative and significant impact on on-time graduation from community college of students with higher likelihood of being undocumented immigrants, but no significant effect on on-time graduation from four-year public universities. While the precision of the estimates is sensitive to the definition, the results suggest that the in-state resident tuition had a negative effect on on-time graduation. An explanation for this could be compositional effects. For instance, it is possible that the reduced college tuition increased the number of financially constrained students. These students might drop out of school for semesters to support families, which could result reduced on-time graduation. More detailed data will be required to understand what is driving the negative effect on-time graduation.

3.4.2 Type of Degree

Table 3.3 shows the regression results for students who graduated from community colleges. The estimates suggest that the in-state tuition policy increased the probability of graduating with an associate degree or an advance certificate for undocumented immigrants, and reduced the probability of graduating with a basic level-1 certificate. As mentioned earlier, Figure 3.1 shows that the in-state tuition policy significantly reduced the cost differential across different college degrees, which may have allowed some students to afford previously unaffordable degrees. The results shown in Table 3.3 are consistent with this expectation that as college costs decreases, students start opting for advanced certificates or associate degrees.

Table 3.3: Types of Degrees

	Graduating with an Associate Degree (1)	Graduating with an Advanced Certificate (2)	Graduating with a Basic Certificate (3)
Panel A: UI is defined as US noncitizens			
Post 2001*UI	0.059***	0.005	-0.064***
	(0.009)	(0.003)	(0.009)
UI	0.054***	-0.008**	-0.047***
	(0.009)	(0.003)	(0.008)
Mean of Dep Var	0.77	0.02	0.21
Panel B: UI is defined using Predicted Probability of Undocumented Immigrants			
Post 2001*UI	0.067***	0.008*	-0.075***
	(0.010)	(0.003)	(0.010)
UI	0.025**	-0.011***	-0.014
	(0.010)	(0.003)	(0.009)
Mean of Dep Var	0.70	0.05	0.25
Observations	429091	429091	429091

Notes: The unit of observation is an individual. Demographic controls include: student's age and gender, an indicator for academic disadvantage, an indicator for whether they are a single parent, diability, English language proficiency, and dummies for institute and year. Each Column correspond to an outcome. In Panel A, UI is defined as an indicator for US noncitizens, and in Panel B, UI is defined as the predicted probability of being an undocumented immigrant. Sample includes freshman year students enrolled in community colleges during 1995-2013. Columns 1-3 show the probability of graduating with an associate degree, advanced certificate and a basic certificate, respectively. Robust standard errors are in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

3.4.3 Choice of Major

Majors in community colleges are categorized into academic major or technical major.[13] Table 3.4 shows the effect of the in-state tuition policy

[13] A third category, technical-prep major, is included in technical major category for the purpose of this study.

Table 3.4: Dependent Variable: Probability of Choosing Academic Major

	Community College	4-year Public University
	Acadmic Major	STEM
	(1)	(2)
Panel A: UI is defined as US noncitizens		
Post 2001 * UI	0.107***	-0.006
	(0.011)	(0.016)
UI	-0.015	0.144***
	(0.010)	(0.012)
Mean of Dep Var	0.53	0.31
Panel B: UI is defined using Predicted Probability of Undocumented Immigrants		
Post 2001 * UI	0.100***	0.031
	(0.011)	(0.020)
UI	-0.019	0.086***
	(0.011)	(0.016)
Mean of Dep Var	0.49	0.19
Observations	429,091	139,159

Notes: The unit of observation is an individual. Demographic controls include: student's age and gender, an indicator for academic disadvantage, an indicator for whether they are a single parent, diability, English language proficiency, and dummies for institute and year. Each Column correspond to an outcome. In Panel A, UI is defined as an indicator for US noncitizens, and in Panel B, UI is defined as the predicted probability of being an undocumented immigrant. Columns 1 uses a sample of freshman year students enrolled in community colleges during 1995-2013. Column 2 uses a sample of freshman year students enrolled in four-year public universities during 1995-2011, and does not include the following demographic controls: an indicator for academic disadvantage, an indicator for whether they are a single parent, diability and English language proficiency. Robust standard errors are in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

on choice of major. Column 1 shows the probability of graduating with an academic major, and Column 2 shows the probability of choosing a STEM major at a 4-year university. The estimates show that in-state tuition policy resulted in an increase in probability of graduating with an academic major for undocumented immigrants enrolled in community colleges but had no significant impact on the probability of graduating with STEM major from a 4-year university.

3.4.4 Heterogeneity in Results

Table 3.5 and Table 3.6 show the heterogeneity in results by gender. Table 3.5 suggests that the policy had a larger effect on graduation rate of male compared to female undocumented immigrants at community colleges as well as four-year universities. However, the effect of the policy on on-time graduation was larger for females compared to male undocumented immigrants.

Table 3.6 shows that in-state tuition policy had a larger effect on educational choices of males than females. Male undocumented students were more likely to graduate, and graduate with associate degree or advanced certificates after the policy than female undocumented immigrants.

3.5 Conclusion

In light of recent reforms and debates concerning educational and labor market opportunities of undocumented immigrants, the question of how financial incentives affect their educational outcomes is important. This paper studies the effect of an in-state resident tuition policy in Texas on educational choices of undocumented immigrants. Using alternative specifications, I find that reduced college costs in community colleges, resulting from the in-state tuition policy, lead to an increase in the likelihood of graduation, the probability

Table 3.5: Heterogeneity in Results

	Community College		4-year Public University	
	Graduation	On-time Completion	Graduation	On-time Completion
	(1)	(2)	(3)	(4)
Panel A: UI is defined as US noncitizens				
Post 2001 * UI* Male	0.014***	-0.030***	0.032**	-0.011
	(0.003)	(0.006)	(0.011)	(0.019)
Post 2001 * UI	0.132***	0.009	-0.010	-0.078***
	(0.004)	(0.010)	(0.011)	(0.018)
UI	-0.114***	0.143***	-0.146***	0.245***
	(0.003)	(0.009)	(0.007)	(0.012)
Male	-0.042***	0.017***	-0.089***	-0.044***
	(0.001)	(0.001)	(0.002)	(0.002)
Panel B: UI is defined using Predicted Probability of Undocumented Immigrants				
Post 2001 * UI* Male	0.033***	-0.021***	0.078***	-0.008
	(0.003)	(0.006)	(0.010)	(0.022)
Post 2001 * UI	0.106***	-0.027**	-0.046***	-0.019
	(0.003)	(0.010)	(0.010)	(0.022)
UI	-0.158***	0.186***	-0.273***	0.189***
	(0.003)	(0.010)	(0.007)	(0.016)
Male	-0.043***	0.016***	-0.090***	-0.043***
	(0.001)	(0.001)	(0.002)	(0.002)
Observations	1687230	429091	332025	139159

Notes: The unit of observation is an individual. Demographic controls include: student's age and gender, an indicator for academic disadvantage, an indicator for whether they are a single parent, diability, English language proficiency, and dummies for institute and year. For Columns 3-4, demographic controls do not include an indicator for academic disadvantage, an indicator for whether they are a single parent, diability and English language proficiency. Each Column correspond to an outcome. In Panel A, UI is defined as an indicator for US noncitizens, and in Panel B, UI is defined as the predicted probability of being an undocumented immigrant. Columns 1-2 use a sample of freshman year students enrolled in community colleges during 1995-2013. Columns 3-4 use a sample of freshman year students enrolled in four-year public universities during 1995-2011. Robust standard errors are in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 3.6: Heterogeneity in Results - Types of Degrees

	Graduating with an Associate Degree (1)	Graduating with an Advanced Certificate (2)	Graduating with a Basic Certificate (3)
Panel A: UI is defined as US noncitizens			
Post 2001 * UI* Male	0.115***	0.010***	-0.125***
	(0.005)	(0.001)	(0.005)
Post 2001 * UI	0.004	0.001	-0.004
	(0.009)	(0.003)	(0.009)
UI	0.055***	-0.008**	-0.047***
	(0.009)	(0.003)	(0.009)
Male	-0.118***	-0.008***	0.127***
	(0.001)	(0.001)	(0.001)
Panel B: UI is defined using Predicted Probability of Undocumented Immigrants			
Post 2001 * UI* Male	0.095***	0.012***	-0.106***
	(0.005)	(0.001)	(0.005)
Post 2001 * UI	0.022*	0.003	-0.024*
	(0.010)	(0.003)	(0.010)
UI	0.026**	-0.011***	-0.015
	(0.010)	(0.003)	(0.009)
Male	-0.119***	-0.009***	0.127***
	(0.001)	(0.001)	(0.001)
Observations	429091	429091	429091

Notes: The unit of observation is an individual. Demographic controls include: student's age and gender, an indicator for academic disadvantage, an indicator for whether they are a single parent, diability, English language proficiency, and dummies for institute and year. Each Column correspond to an outcome. In Panel A, UI is defined as an indicator for US noncitizens, and in Panel B, UI is defined as the predicted probability of being an undocumented immigrant. Sample includes freshman year students enrolled in community colleges during 1995-2013.Robust standard errors are in parentheses. *** p < 0.001, ** p < 0.01, * p < 0.05.

of graduating with an associate degree or an advanced certificate compared to a basic certificate, and the probability of graduating with an academic major.

The evidence presented in this paper shows that reducing the gap in opportunities for higher education can result in improved educational outcomes for disadvantaged population groups. However, inadequate effort on the labor market integration of undocumented immigrants can restrict the benefits from any policy aimed at reducing the educational achievement gap. As shown in this paper, a reduction in college costs only affected the education outcomes of those enrolled in community college, but not in four-year public universities. One of the reasons for this differential impact could be that four-year college costs, even after allowing for in-state resident tuition, remain prohibitively high for undocumented immigrants. Another plausible explanation is that the returns to four-year college degree are not very high for undocumented immigrants due to their restrictive legal employment channels. Future research on the effect of in-state tuition policies on labor market outcomes will be helpful in improving our understanding of inequalities in opportunities for disadvantaged populations.

Appendices

Appendix A

Appendix to Chapter 1

The primary sample for the analysis is created using the District Level Household Survey (DLHS) for the year 2002-04 and 2007-08. The survey includes information about the birth and immunization history of the last two live births of all married women in the age group 15-44 (for 2004-05) and 15-49 (for 2007-08) who live in the household.

Restrictions based on Children's Characteristics

1. Since the reference period for data on deaths and births within the household is 3 years, I restrict the sample to include children who are 3 years old or younger.

2. I exclude children who were born during the same month as the death. I do this because I do not have the exact date of birth or death. Thus, for children born during the same month as the death, I cannot determine whether the child was born before or after the death. My estimates are similar if I include these observations in the sample.

3. I also exclude children who are born either one month before or one-month after the death. This allows for some grieving time. The results are similar but less precise when I include these children in the sample.

Restrictions based on Household Characteristics

1. I exclude from the analysis households that do not identify a grandparent as a current resident and that record no deaths of any old household member. I do this because in these cases I cannot separate households in which the grandparents are alive but live separately in another household from households in which the grandparents died prior to the reference period. Households that do not identify a grandparent as a current resident and record no deaths of an old member constitute a 42 percent of the total sample.

2. I only focus on households in which a death results in a structural shift in the household compositionthat is, when household composition shifts from a 3-generation unit to a 2- generation unit. Thus, the control group consists of households in which one grandparent is alive while the treatment group consists of households in which the only living grandparent died during the reference period.

3. I drop households that have grandparents who are less than 50 years of age. I do this because if a household member younger than 50 died, it is difficult to isolate whether s/he was a grandparent. The IHDS sample suggests the grandparents are usually over 50 years of age.

4. I drop households that have only 2 members.

5. I exclude visiting members and households in which women live in their natal homes.

6. I exclude households in which both grandparents died during the reference period. I do this because there are few such households and most have received very sparsely spaced death shocks.

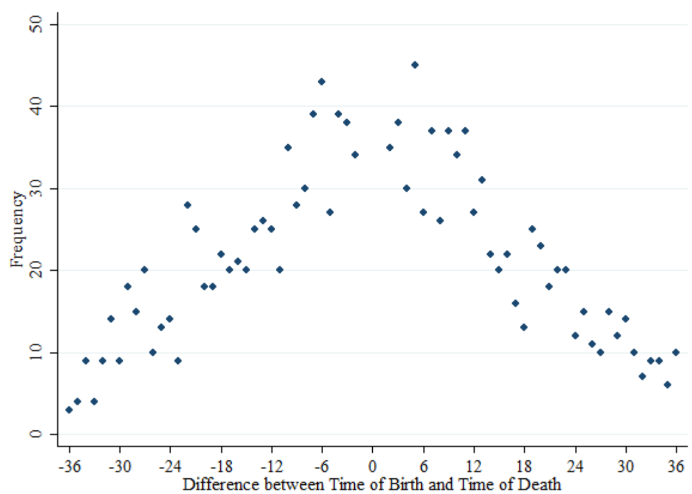
Imputed Grandparent's Death: The survey provides detailed information about the members who live in a household, including their relationship to the household head, age, gender, etc. With regards to the death history, the survey provides information about the month and year of death and the gender and age of the deceased, but it does not identify his/her relationship to the head of the household. To address this data constraint, I impute whether the deceased was a grandparent based on the gender and age of the deceased and the gender, age and his/her relationship to the head of the household of the living members. For instance, if a 55 year-old women has died and the current head of the household is a 60 year-old grandfather whose oldest son is 35, then I conclude that the women who died was the grandmother.

To check the accuracy of this imputation, I use IHDS data which includes information on the relationship of the deceased to the head of the household. I use the imputation algorithm to predict whether the deceased is the grandparent using IHDS data. The correlation between the predicted relationship and the true relationship (whether the deceased is a grandparent) is 0.9979. This confirms that my imputed indicator for the deceased being a grandparent is very accurate.

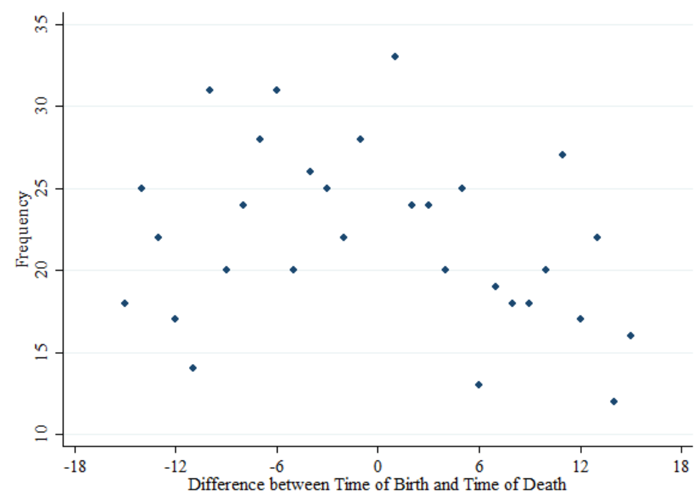
Other Robustness Checks: The humped shape of the data in Figure A.1 is by construction and can be explained using a simple example. For instance, if the deaths and births were noted only for 6 months before the survey and the survey was taken on 1st July, the number of children born one month before and one month after the death will be greater than the number of children born 5 months before or after the death. This is so because for a family that experienced a death in May, the children born two months after the death will not be observed in the data; in contrast, in families that experienced death in April, children born three months after the death will not be observed in the data. Thus, given the constant reference period, the probability of being included in the survey is higher if the birth occurred close to the date of the death; in other words, the probability of being in the data is inversely related to the length of time that has passed since the date of death.

To confirm that the data structure is not driving the estimates, I create a balanced panel for a children who were born 15 months before or after the death. Figure A.1 shows the frequency of births for the sample of households that experienced the death of the last living grandparent. As shown in the figure, the observations are randomly distributed in this new balanced panel. I also run my primary regressions using this balanced sample. The estimates from this new sample (not shown here) vary in magnitude and significance (due to reduced power) across specifications, but are positive across all specifications.

Figure A.1: Density of Births Relative to Timing of Death



A: **Analysis Sample (Unbalanced)**



B: **Balanced Sample**

Notes: This figure plots the frequency of births against the timing of birth relative to the timing of death. The timing of birth relative to the timing of death is defined as the time difference between the birth of the grandchild and the death of a grandparent. The sample includes households in which the last living grandparent died and the mother had at least two births during the reference period. Panel A uses the sample used in the analysis, i.e., the unbalanced panel. The hump-shape of the plot is by construction. Given the 3-year reference period for the death and birth history, the likelihood that a birth that occurs closer to the death is captured in the survey is higher than the likelihood that births that occur long before or after from the deaths are captured. Panel B includes a balanced panel of children born 15 months before or after the death. The banaced Panel in Panel B smoothens the hump shape.

Table A.1: Randomness of Timing of Birth

	Rural	Hindu	Muslim	Christian	SC
Born after Death	0.000	-0.013	0.012	0.000	-0.007
	(0.000)	(0.012)	(0.012)	(0.000)	(0.008)
Observations	1,873	1,873	1,873	1,873	1,873
	ST	OBC	Household Size	Number of DILs	Rank of Mother
Born after Death	0.001	0.004	-0.029	0.000	0.001
	(0.001)	(0.008)	(0.039)	(0.000)	(0.001)
Observations	1,873	1,873	1,873	1,873	1,873
	Number of Adult Males	Number of Adult Females	Mother's Age at Birth	Grandparent's Age	Child is a Male
Born after Death	-0.003	-0.000	1.661***	1.844***	0.043
	(0.004)	(0.002)	(0.149)	(0.152)	(0.050)
Observations	1,873	1,873	1,873	1,873	1,873
	Mother's Education	Father's Education	Mother's Literacy	Father's Literacy	Received Advice About Immunization
Born after Death	0.000	-0.025	-0.011	-0.015	-0.006
	(0.000)	(0.036)	(0.012)	(0.015)	(0.023)
Observations	761	1,192	1,871	1,828	1,336

Notes: The sample is restricted to only those households that experienced the death of the last living grandparent. Coefficients represent the regression of different observables on the timing of birth. The results are weighted using population weights and include primary sampling unit (PSU) fixed effects. A PSU is defined as a village for rural areas and a census block for urban areas. Standard errors are clustered by the PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: Summary Statistics

	All Households	Households in Analysis Sample (All Mothers)	Mothers with at least two births during reference period
Panel A: Household Characteristics			
Rural	0.77	0.77	0.79
Hindu	0.75	0.78	0.77
Muslim	0.14	0.14	0.15
Christian	0.06	0.04	0.04
SC	0.19	0.19	0.21
ST	0.18	0.14	0.14
OBC	0.39	0.41	0.42
Household Size	6.78	7.05	6.85
Number of DILs	1.27	1.27	1.27
Rank of DILs	1.16	1.16	1.15
Number of Adult Males	1.82	1.76	1.77
Number of Adult Females	1.86	2.11	2.09
Age of Mother at Birth	24.60	24.73	23.80
Age of Grandparent	61.19	62.47	62.00
Male Child	0.52	0.53	0.50
Age of Child (in months)	18.20	18.27	18.43
Child Birth Order	2.79	2.76	2.93
Mother's Schooling	8.35	8.44	7.85
Father's Schooling	8.99	8.98	8.58
Panel B: Sample Variation			
Born after Death	0.01	0.07	0.06
Death of last living grandparent	0.03	0.14	0.12
Observations	345,741	62,772	15,909

Notes: Column 1 includes all households irrespective of their household structure: whether or not the grandparent is present and whether or not there was a death in the family. Columns 2 and 3 include households in which either the last living grandparent died during the reference period or was alive and resided in the same household. Column 2 includes married women who had at least one birth during the reference period, while Column 3 includes married women who had at least two births during the reference period. Scheduled Caste (SC), Scheduled Tribes (ST), and other backward classes (OBC) denote the caste classifications of India.

Table A.3: Causes of Death - IHDS Sample

Causes	Percent	Percentage of Known Cause (Excluding Others)
Accident	2.09	4.5
Short-term Illness		
Fever	15.08	32.5
Diarrhea	1.97	4.3
Heart attack	17.29	37.3
Long-term Illness		
Cancer	5.20	11.2
TB	4.57	9.9
HIV-AIDS	0.19	0.4
Other	53.61	

Notes: This table shows the summary statistics for the cause of death of grandparents drawn from the Indian Human Development Survey data for 2004-05. The first column shows the distribution of the causes of death for all responses. The second column shows the distribution of the causes of death for all responses except those that stated "other." This table shows that most grandparents die as a results of short-term illnesses or accidents.

Table A.4: Household Structure

	No GP Died	One GP Died	Two GP Died
No GP in Household	Not in Sample		
One GP in Household	Control Group	Treated Group	
Two GP in Household	Robustness	Robustness	Not in Sample

Notes: This table shows which household structures are included in the sample. Rows show the household structure at the beginning of the reference period and Columns show the number of deaths during the reference period. Each cell denotes whether a particular combination of household composition and deaths during the reference period was included in the sample. For instance, Column 1 and Row 1 shows that households in which there were no grandparents and no one died are not included in the sample.

Table A.5: Effect of Death- One of the Two Living Grandparents Die

	(1)	(2)
Born after Death	-0.043*	-0.047*
	(0.024)	(0.024)
Death of a Grandparent	0.012	
	(0.030)	
Demographic Controls	✓	✓
Birth order dummies	✓	✓
Birth year dummies	✓	✓
Birth month dummies	✓	✓
Mother FE		✓
Mean of Dependent Variable	0.15	0.15
Observations	24,138	24,138

Notes: Sample includes mothers who had two or more births during the reference period. The treated group includes households in which one of the two living grandparents die, while in the control households grandparents are alive. The unit of observation is the child. Demographic controls include the childs gender, child age dummies, whether the child is a first male child, whether the child is the first born, the mothers age at birth, paternal and maternal education, whether the father lives in the household, the rank of the mother, the death of an older child, the number of daughters-in-law in the household, the presence of old individuals other than grandparents, the imputed age of grandparents, household size, the number of children born during the reference period, the type of residence (rural or urban), religion, the caste of the head of the household, the number of adult males, the number of adult females, primary sampling unit (PSU) fixed effects, and interaction of survey year and survey month dummies. PSU is defined as a village for rural areas, and a census block for urban areas. Column (1) shows results from the estimation strategy 1, which includes an indicator for the death of the last living grandparent and another indicator for the child being born after the death of the grandparent. Column (2) presents results from empirical strategy 2, which includes mother-fixed effects. Standard errors are clustered at the PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.6: Heterogeneity by Gender of Grandparent and Grandchild

	(1)	(2)
Born after Death*Female*GM	0.093**	0.119***
	(0.042)	(0.044)
Born after Death*Female*GF	-0.019	-0.001
	(0.033)	(0.034)
Born after Death*Male*GM	0.008	-0.002
	(0.041)	(0.040)
Born after Death*Male*GF	0.083***	0.072**
	(0.028)	(0.029)
Female*GM	0.039	
	(0.039)	
Male*GM	0.050	
	(0.038)	
Female*GF	0.017	0.031*
	(0.014)	(0.017)
Death of Grandparent*Female	0.039	0.014
	(0.029)	(0.028)
Death of Grandparent	0.030	
	(0.047)	
Demographic Controls	✓	✓
Birth order dummies	✓	✓
Birth year and birth month dummies	✓	✓
Mother FE		✓
F-test	0.01	0.02
Observations	12,060	12,060

Notes: Dependent variable is an indicator for vaccinations at birth. GM denotes the households in which either the grandmother died or is the living grandparent. The treated group includes families in which the last living grandparent died, while the control group includes families in which exactly one grandparent is alive. Column (1) shows results from an estimation strategy that includes an indicator for the death of the last living grandparent and another indicator for the child being born after the death of the grandparent. Column (2) presents results from empirical strategy 2, which includes mother-fixed effects. Standard errors are clustered at the PSU level. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1.

p-value for BornAfterDeath*Female*GM = BornAfterDeath*Male*GM is 0.0293.

p-value for BornAfterDeath*Female*GM = BornAfterDeath*Female*GF is 0.0254.

p-value for BornAfterDeath*Male*GM = BornAfterDeath*Male*GF is 0.1001.

p-value for BornAfterDeath*Female*GF = BornAfterDeath*Male*GF is 0.0791.

Table A.7: Alternative Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
Born after Death	0.049**	0.047**	0.047**	0.056***	0.048**	0.048**
	(0.020)	(0.020)	(0.019)	(0.020)	(0.020)	(0.019)
Death of Grandparent	-0.014	-0.015	-0.016			
	(0.026)	(0.026)	(0.026)			
Demographic Controls	✓	✓	✓	✓	✓	✓
Birth order dummies		✓	✓		✓	✓
Birth year and birth month dummies			✓			✓
Mother FE				✓	✓	✓
Mean of Dependent Variable	0.14	0.14	0.14	0.14	0.14	0.14
Observations	34,740	34,740	34,740	34,740	34,740	34,740

Notes: Dependent variable is an indicator for vaccinations at birth. The treated group includes families in which the last living grandparent died, while the control group includes families in which at least one grandparent is alive. The sample includes mothers who have had two or more births during the reference period. The unit of observation is the child. Demographic controls include the child's gender, child age dummies, whether the child is a first male child, whether the child is the first born, the mother's age at birth, paternal and maternal education, whether the father lives in the household, the rank of the mother, the death of an older child, the number of daughters-in-law in the household, the presence of old individuals other than grandparents, the imputed age of grandparents, household size, the number of children born during the reference period, the type of residence (rural or urban), religion, the caste of the head of the household, the number of adult males, the number of adult females, primary sampling unit (PSU) fixed effects, and the interaction of the survey year and the survey month dummies. PSU is defined as a village for rural areas and a census block for urban areas. Columns(1)-(3) show the results from the estimation equation 1, which includes an indicator for the death of the last living grandparent and an indicator for whether the child was born after the death of the grandparent. Columns(4)-(6) present the results from empirical equation 2, which includes mother-fixed effects. Standard errors are clustered at the PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.8: Alternative Outcome Definitions

Panel A: Alternative Indices	OPV and BCG		OPV and HepB		HepB and BCG	
Born after Death	0.052**	0.060**	0.038*	0.043**	0.042**	0.049**
	(0.027)	(0.028)	(0.020)	(0.020)	(0.020)	(0.020)
Death of Grandparent	0.067		0.045		0.063	
	(0.047)		(0.045)		(0.044)	
Mother FE		✓		✓		✓
Observations	12,060	12,060	12,060	12,060	12,060	12,060
Panel B: Individual Vaccines	OPV		BCG		HepB	
Born after Death	0.042*	0.048*	0.004	0.025	0.033	0.039**
	(0.024)	(0.025)	(0.023)	(0.023)	(0.020)	(0.020)
Death of Grandparent	-0.003		0.172***		0.059	
	(0.037)		(0.053)		(0.046)	
Mother FE		✓		✓		✓
Observations	12,060	12,060	12,060	12,060	12,060	12,060

Notes: Columns 1-6 of Panel A show the regression results for the immunization index (excluding Hepatitis B, BCG and Oral Polio Vaccine, respectively). Columns 1-6 of Panel B show the regression results separately for each of the three vaccines: Oral Polio Vaccine, BCG and Hepatitis B, respectively. The sample includes mothers who have had two or more births during the reference period. The unit of observation is the child. Demographic controls include the child's gender, child age dummies, whether the child is a first male child, whether the child is the first born, the mother's age at birth, paternal and maternal education, whether the father lives in the household, the rank of the mother, the death of an older child, the number of daughters-in-law in the household, the presence of old individuals other than grandparents, the imputed age of grandparents, household size, the number of children born during the reference period, type of residence (rural or urban), religion, the caste of the head of the household, the number of adult males, the number of adult females, primary sampling unit (PSU) fixed effects, and interaction of survey year and survey month dummies. The PSU is defined as a village for rural areas and a census block for urban areas. The treated group includes families in which the last living grandparent died, while the control group includes families in which exactly one grandparent is alive. Columns 1, 3 and 5 the show results from estimation equation 1, which includes an indicator for the death of the last living grandparent and another indicator for whether child was born born after the death of the grandparent. Columns 2, 4 and 6 present results from empirical equation 2, which includes mother-fixed effects. Standard errors are clustered at the PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.9: Vaccination at Birth Indicator (without HepB vaccine)

	Original Specification	DLHS (New Controls)	IHDS
Panel A			
Born after Death	0.052**	0.050*	0.168
	(0.026)	(0.026)	(0.212)
Death of Grandparent	0.058	0.054	0.030
	(0.045)	(0.048)	(0.144)
Mean of Dep Var	0.059	0.058	0.462
Observations	12,373	12,373	1,668
Panel B			
Born after Death*Years since Death	0.021	0.021	0.042
	(0.015)	(0.015)	(0.036)
Death of Grandparent	0.071	0.062	0.013
	(0.049)	(0.048)	(0.144)
Mean of Dep Var	0.059	0.058	0.462
Observations	12,373	12,373	1,668

Notes: This table shows the regression results for a modified vaccination at birth index, which includes only the Oral Polio Vaccine and BCG. Column 1 reproduces the main results. All specifications include dummies for birth order, birth month, birth year, and child age, including primary sampling unit (PSU) mixed effects. The PSU is defined as a village for rural areas and a census block for urban areas. Columns 2 and 3 specifications do not include controls for the number of newborns but includes a control for per capita household income at the time of the survey. Panel A presents results similar to the original specification. Panel B presents the results for the interaction of those born after death with years since death. Standard errors are clustered at the PSU level. *** p<0.01, ** p<0.05, * p<0.10.

Table A.10: Dependent Variable - Delivery at Home

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All households in the Analysis Sample						
Born after Death	0.014	0.010	0.009			
	(0.016)	(0.016)	(0.015)			
Death of Grandparent	-0.025*	-0.018	-0.017			
	(0.013)	(0.013)	(0.013)			
Mean of Dependent Variable	0.58	0.58	0.58			
Observations	50,042	50,042	50,042			
Panel B: Households with Two or more DILs who gave Birth during the Reference Period						
Born after Death	-0.050	-0.032	-0.052	-0.029	0.006	-0.032
	(0.087)	(0.086)	(0.093)	(0.093)	(0.099)	(0.107)
Death of Grandparent	-0.099	-0.124	-0.089			
	(0.088)	(0.088)	(0.086)			
Mean of Dependent Variable	0.55	0.55	0.55	0.55	0.55	0.55
Observations	4488	4488	4488	4488	4488	4488
Demographic Controls	✓	✓	✓	✓	✓	✓
Birth order dummies		✓	✓		✓	✓
Birth year and birth month dummies			✓			✓
Household FE				✓	✓	✓

Notes: This table shows the results for the outcome variable - whether the child was delivered at home or at a health facility. The sample in Panel A includes all the households in the analysis. The sample in Panel B is restricted to only include households which have two or more DILs, and more than one DIL gave birth during the reference period. The unit of observation is the child. Demographic controls include the child's gender, child age dummies, child is a first male child, child is the first born, mother's age at birth, paternal and maternal education, whether the father lives in the household, the rank of the mother, the death of an older child, the number of daughters-in-law in the household, the presence of old individuals other than grandparents, the imputed age of the grandparents, household size, the number of children born during the reference period, the type of residence (rural or urban), religion, caste of the head of the household, the number of adult males, the number of adult females, primary sampling unit (PSU) fixed effects, and interaction of the survey year and survey month dummies. PSU is defined as a village for rural areas and a census block for urban areas. The treated group includes families in which the last living grandparent died, while the control group includes families in which exactly one grandparent is alive. Columns 1-3 show results for empirical equation 1 (without any household fixed effects). In Columns 4-6, I include household fixed effects. Standard errors clustered at PSU level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix B

Appendix to Chapter 2

Table B.1: Top Ten School Feeding Programs

Country	Reported or Estimated Number of Beneficiaries(in 000's)	Estimated Annual Cost per Child (in USD)
India	113,600	13*
Brazil	47,271	30
United States	45,000	389
China	26,000	-
Japan	9,770	799
South Africa	8,821	64
Egypt	7,002	-
Mexico	5,164	59
Turkey	4,209	-
Venezula	4,031	189

Notes: *Cost estimates for India were unavailable in the WFP study and hence been obtained by dividing the total budget allocated for MDMS by the reported number of beneficiaries. Source: State of School Feeding Worldwide, 2013. World Food Programme (WFP).

Table B.2: Time of implementation

States	Implementation Year	States	Implementation Year
Andhra Pradesh	2003	Madhya Pradesh	2003
Arunachal Pradesh	2004	Maharashtra	2003
Assam	2005	Manipur	2004
Bihar	2005	Meghalaya	2003
Chhattisgarh	2002	Orissa	2004
Delhi	2003	Punjab	2003
Gujarat	1984	Rajasthan	2002
Haryana	2004	Sikkim	2002
Himachal Pradesh	2004	Tamil Nadu	1982
Jammu and Kashmir	2004	Tripura	2003
Jharkhand	2003	Uttar Pradesh	2004
Karnataka	2003	Uttaranchal	2003

Notes: The second column contains the year when the midday meal scheme was implemented with full coverage throughout the state, but for Assam, Bihar, Karnataka and Madhya Pradesh, it shows the year when the mid-day meal was launched in pilot districts; these dates were collected from state midday meal scheme audit and budget reports. Note: As shown by the IHDS data, it is not necessarily true that fully coverage mentioned in the budget reports translates into full coverage on ground. Source: Jayaraman and Simroth (2011) and state government reports.

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