# Do Parents Selectively Time Birth Relative to Ramadan? Impact of Maternal Fasting on Height: Evidence from Matlab, Bangladesh

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**PRELIMINARY AND INCOMPLETE.PLEASE DO NOT CITE**

**Abstract**

When evaluating *in utero* nutrition shock, many studies assume parents do not selectively time birth relative to that event of shock. Fasting during Ramadan is one of the nutrition shocks. Using MHSS 1996 data we show that Muslim mothers who received free contraceptives timed the birth of their children relative to Ramadan. We also evaluate the consequence of allowing selection. When we do not control for selection, we find maternal fasting has adverse effect on height. However, when selection is controlled we do not find any statistically significant effect on height.

## 1 Introduction

There is a growing literature based on "Fetal Origin Hypothesis"(Barker 1990) which links adverse environment and the inadequate nutrition *in utero* to later life health outcomes. Numerous evidences from these studies show nine months *in utero* is very critical for individual health (Almond and Currie 2011). Adverse condition *in utero* can have both short term effect such as effect on birth weight and long term effects such as effect on cognition,

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obesity, cardiovascular disease, diabetes (Almond and Muzumder 2011, Almond and Currie 2011, Ewjik 2011). The early studies, however, did not take into account the endogenous exposure to adverse environmental and nutritional condition (Almond and Currie 2011). As a result they could not address potential confounders appropriately. To establish causal link between *in utero* nutrition shock and health outcomes researchers studied nutrition and health shocks *in utero* exogenous to mothers(Currie 2009). Some of the studies are based on severe historic events such as famine and spread of infectious disease and recent few studies are based on regular occurring event such as Ramadan (Almond 2006; Chen and Zhou 2007; Almond and Muzumder 2011; Ewjik 2011 and Majid 2012). The results from these studies conform with the hypothesis that adverse nutritional environment *in utero* has serious consequences on child health.

However, these studies are not free from limitations. One important methodological limitation is that they are based on the assumption that parents don't selectively time birth relative to those shocks. Thomas(2009) suggests that it is a strong identifying assumption and may not hold. Using data on famine in Bangladesh and Influenza Pandemic in USA, he finds that parents who gave birth relative to these events were systematically different from those who didn't. This raises a serious concern as we do not know whether the results are driven entirely by selection or shocks *in utero*.It is very important to examine the results to discern between selection effect and nutritional shock effects . This has important policy implications. Imagine we find that one particular *in utero* nutrition shock affects health outcomes negatively controlling for selective timing of birth. Then we have to form policies to address this issue. We know from the literature that nutrition shock *in utero* has persistent effect throughout an individual's life. This will create an inequality at birth which can not be reversed. Therefore, we have to take measures to save pregnant mother from such nutritional shock. On other hand, imagine we don't find any effect after taking care of selection. This would imply that the particular *in utero* nutrition shock under study has no effect on child health and we do not need to form any policies to tackle this.

In this paper we study the impact of *in utero* nutrition shock due to maternal fasting during Ramadan on child health accounting for selective timing of birth using Matlab Health and Socioeconomic Survey(MHSS) 1996. The quasi-random placement of family planning program in Matlab allows us to examine whether mothers selectively time birth relative to Ramadan. Empowered with this treatment, we instrument the time of birth relative to Ramadan to study the impact of nutrition shock due to maternal fast-

ing during Ramadan on child height. Typically, the literature studies birth weight to measure the impact of nutrition shock *in utero*. Unfortunately, MHSS does not have any information of child birth weight. However, Currie and Vogl(2012) argues height can be a good proxy for birth weight. Medical literature also suggests that height reflects the combination of both genotype and phenotype influences *in utero* (Martorell and Habicht 1986). Moreover, in economics literature height has been positively associated with wage and productivity. (Strauss and Thomas 2008). There is huge policy value of this study. Almond and Muzumder(2011) estimates roughly 1 billion of Muslims alive today were *in utero* during the month of Ramadan.

Broadly, we present two main results. First, we find that Muslim mothers time birth relative to Ramadan. This result is consistent with various robustness checks. Secondly, we instrument time of birth relative to Ramadan with the family planning program placement to get causal estimate of impact of maternal fasting on height. In contrast to earlier findings, we don't find any evidence adverse effect of maternal fasting on children height controlling for selection.

This paper makes some key contributions in the literature. First, we show parents selectively time birth relative to Ramadan using a rich data set. In our knowledge this is the first paper that shows selective timing of birth relative to Ramadan. Secondly, we find that community level time varying characteristics affect timing of birth.This suggests comparison of SES of the parents of an affected child with the parents of a non-affected child may not be adequate to determine selection in birth timing. Thirdly, we show that providing free contraceptives may help Muslim mothers to avoid pregnancy overlapped with Ramadan. Past literature has evaluated the free contraceptive program in Matlab on fertility, health and education. This paper is the first to evaluate the family planning program in the context of Ramadan fasting. Fourth and most importantly, the paper attempts to study the impact of nutrition shock *in utero* on health outcomes systematically taking into account selective timing of birth.

The rest of the paper is organized as follows. In section 2 we briefly the discuss existing literature on maternal fasting on different outcomes from epidemiology and economics. In section 3 we discuss the background of Matlab Family Planning and Child Health Program. In Section 4 we discuss the MHSS and compare it with other data sets used in the literature. In section 5 the we discuss the empirical strategy to study selective timing of birth relative to Ramadan and also how it affects health outcomes. In section 6 we provide the results and also discuss the contrast and similarity of these results with existing literature. In section 7 we make concluding

remarks.

## 2 Literature Review

The literature on maternal fasting on child health outcomes can be broadly divided into two categories. One category is epidemiological literature and another is economics literature. Almond and Muzumder(2011) presents a nice summary of epidemiological literature in their paper. Several studies suggest that fasting during pregnancy can lead to neurological impairments, higher blood pressure in later life (Hunter and Sadler 1987,Moore et al. 1989, Sheehan et al. 1985, Gluckman and Hanson 2005).

Almond and Muzumder(2011) notes some limitations of epidemiological studies. First of all, most of those studies are based on a small number of observations. Secondly, those studies compare the effect on fasters and non-fasters assuming that decision to fast is exogenous. Thirdly, those studies do not disentangle the fasting effect from the seasonality, as they are based on Ramadan overlapping with only one season.

The application of Intent To Treat (ITT)analysis distinguishes the study of Almond and Muzumder(2011) from the epidemiological studies on Ramadan. They were also were first to study the impact of maternal fasting during Ramadan on child outcomes in the economics literature. ITT anaylsis alllows them to get rid of the compliance problem related to fasting. Under the assumption that parents do not selectively time birth relative to Ramadan, it gives causal estimates of the impact of *in utero* nutrition shock. Moreover, unlike most epidemiological studies, the study of Almond and Muzumder(2011) was also based on a large number of cohorts.

Almond and Muzumder(2011) use data from Michigan, Iraq and Uganda. They study the impact of *in utero* nutrition shock on birth weight using data from Michingan and on various forms of disabilities using census data from Iraq and Uganda. They find children who were *in utero* during Ramadan have lower birth weights and are more likely to be disabled. Following Almond and Muzumder(2011), Ewjik(2011) finds that maternal fasting during Ramadan may increase the chances of developing health problem such as coronary heart disease and type 2 diabetes. Majid(2012) finds that maternal fasting during Ramadan leads to fewer hours worked and self-employment in later life. Ewjik(2011) and Majid(2012) use Indonesia Family Life Survey(IFLS) wave 3 and wave 4 respectively. Using English register data, Almond Muzumder and Ewjik(2012) finds that maternal fasting during Ramadan leads to lower test scores.

One obvious limitation in those papers is that they assume parents don't selectively time birth relative to Ramadan. Moreover,the above mentioned studies have serious some limitations with data. In Michigan and Iraq data, Almond and Muzumder(2011) didn't know the religion of the mother. They used Arab as proxy for Muslims in their Michigan data. The birth data from Uganda, Iraq and Indonesia were self-reported. This could be a serious problem as misreported birth dates will lead to wrong classification of birth relative to Ramadan. In comparison to past data sets used, with MHSS we can clearly identify the religion of the mother and get reliable birth data in a single data set for a sufficiently large of number of cohorts.

# 3 Matlab Family Planning and Child Health

Matlab is a *thana*(sub-district) in Chandpur District in Bangladesh. It is located 55 kilometers of South-East of Dhaka. The Demographic Surveillance System(DSS) has been operating in Matlab thana since 1966. Initially 132 villages were included in the system, and 101 villages were added in 1968.All households in the DSS area are within the Monitoring system. A typical village consists of several *baris*, or groups of houses around a central courtyard. In DSS area, the record of birth, death and migration(in and out) are collected from the start of the project. The enumeration of marital union and dissolution began in 1975(Razzaque and Streatfiled). In October 1977 the DSS area was contracted to 149 villages by excluding 84 villages. The family planning and health project was launched in 70 villages(treatment area)and the remaining villages were comparison area. No report of using randomization mechanism has been found (Schultz 2009). The figure (1)[1] in appendix also shows that the treatment area grouped into clusters.Schultz (2009) argues that the clustering of villages into treatment area retain the spillover effect. Table 3 in appendix presents 1974 census data which shows that the treatment and the comparison area were very similar except for few observable characteristics such as sources of drinking water, number of cows and age of both household head and spouses of household head. In our estimation strategy we will control household and biological sibling fixed effects to account for the household and mother level fixed unobservables.

Barham(2012) also describes the other treatments added to the treatment areas which are documented in Bhatia et al.(1980), Phillips et al.(1984) and Koening et al.(1990). In October 1977, the family planning program began in treatment areas through the provision of modern contraception.

[1]Figure 1, table 2 and 3 are reproduced from Bahram (2012)

From June 1978, pregnant women received tetanus toxoid vaccination and also pregnant women in their last trimester pregnancy received iron and folic acid tablets. From March 1982, the children aged from 9 months to 59 months in treatment area 1 received measles vaccine. This program was expanded to treatment area 2 on November 1985. From January 1986, DPT, polio and tuberculosis immunization were given to children under age 5. Later in 1986, Vitamin A supplementation for children under age 5 and nutritional rehabilitation for those who were nutritionally risky were added to treatment areas. In appendix section, I reproduced the table 1 from Barham(2012) which gives a summary of the programs introduced in the treatment areas and age cohorts the programs have affected.

# 4 Data

This paper uses Matlab Household and Socioeconomic Survey 1996 which was funded by National Institute of Aging and was collaborative effort of RAND, the Harvard School of Public Health, the University of Pennsylvania and the University of Colorado at Boulder . The primary sample was drawn from a probability sample of 2,883 *baris* from 7,440 *baris* in the DSS 1994 sample frame. *baris* usually consists of cluster of households in close physical proximity. In all *baris*, interviews were completed in 2,781 *baris* out of 2883 eligible *baris*. Within each bari, upto two households were randomly selected. For each *baris*, one household was randomly chosen and designated as primary household or $Status = 1$. If there are more than 2 households, the second household was randomly chosen and designated as $Status = 2$. Otherwise the second household was designated as $Status = 2$. Out of the 2,781 *baris*, 94 *baris* were inappropriately interviewed and therefore disregarded from analysis which leaves us with 2,687 *baris*. Out of these *baris*, 656 are one household *baris* and rest of them have two or more households. Ideally, there should be 2,013 households but the survey team could find only 1,677 households. The remaining $Status = 2$ households are purposive sample based on relationship to the first household. In this paper we limit our studies to only $Status = 1$ households or primary households.

In the survey mothers were asked about birth dates of each their children. Later the birth dates were matched with the DSS data sets for their consistency and accuracy. Although DSS started in 1966, during the data collection process of MHSS the events(i.e. birth, marriage) which took place from 1974 were linked to computerized system of DSS. Therefore, we have reliable birth dates for 22 birth year cohorts from 1974 to 1995. All birth

dates before 1974 are self reported (Menken et al. 1999). There are also some other limitations with this data. We get the birth data from the pregnancy history of the women interviewed in the MHSS 1996. This is a limitation because we can know about births prior to 1996, only if the women living in sampled household survived till 1996. Since the treatment area got maternal health care, one might argue that the women who survived in treatment area may not have survived in absence of maternal health treatment.Ronsmans et al.(1997) finds that maternal mortality from all causes declined in both treatment and control area from 1976 to 1993 and the difference is no significant between treatment and control areas. Moreover, if the survival of the women correlates with birth timing relative to Ramadan, this will create a downward bias. To illustrate, let's suppose there are two types of women high type and low type. High type avoids birth and low type doesn't avoid birth relative to Ramadan. The low type is also less likely to survive. The maternal health aspect of the treatment would make it more likely that the low type survives in the treatment area and therefore get included in the sample.

Another limitation of the data is for some births only month and year of birth is known and the birth dates are replaced with zero. It also varies considerably between treatment area and control area. There are 2086 births which had date *zeros* out of 8573 from 1974 to 1995. Out of the 2086 births, the treatment area had 856 births and control area had 1230 births. One possibility is that some of these births took place out of the treatment and control area. Another possibility is these births have date *zeros* because of birth data collection method. We will do the analysis both including and excluding these date zero births.

To study the impact on height we match the birth dates from the mother's pregnancy with the birth month and year of the individual surveyed in the Matlab. We later match anthropometric data for each individual. We limit our study to only single birth. There are few twin births in MHSS. We found only 28 twins in our data.

## 5 Ramadan Measures

The month of Ramadan is the 9th Month in the Islamic Calender Year. Islamic law does not require a pregnant woman to fast during pregnancy. However, evidence from different Muslim countries suggests that some Muslim mothers fast when they are pregnant(Almond and Muzumder 2011).

For a given year, we construct the dates Gregorian Calender which over-

laps with the Ramadan Month of Islamic Calender [2]. From our birth data we only know the date of birth but we don't know the gestation time. We observe how many months after Ramadan the individual 'i' was born. Generally, the gestation time for human is 266 days. For each date of birth we create century day code(CDC) following Almond and Muzumder(2011). We will denote $ramadan\_0$ if the individual 'i' was born during Ramadan, $ramadan\_1$ if the individual 'i' was born within 30 days after Ramadan, $ramadan\_2$ if the individual 'i' was born between 31 days and 60 days after Ramadan and so on. We also define dummy variable 1 if individuals were born between X and Y months after Ramadan as $RamadanXtoY$ where $X < Y$ and 0 otherwise. For example, $ramadan7to9$ would mean anyone who born between 180 to 270 days after Ramadan. $ramadan7to9$ would mean the individuals were most probably in the first trimesters when Ramadan overlapped with pregnancy. Similarly, $ramadan4to6$ and $ramadan1to3$ would mean individuals were most probably in second trimester and third trimester. We prefer to use $ramadan7to9$ in stead of first trimester because we only know the date of birth. At times birth takes place preterm and post-term. As a result, if we denote $ramadan7to9$ as first trimester, we would wrongly classify first trimester overlapped with Ramadan.

For individuals whose date of birth is not known or date of birth is replaced with *zero*, we match the month of Gregorian Calender year with Ramadan Month and replace the month of Gregorian Calender year with 1 if more than 50 percent or more of Ramadan days overlap with the Gregorian Month.

## 6 Estimation Strategy

Empirical strategy is divided in two sections. In the first section we test whether parents selectively time birth relative to Ramadan. In the second section we study the impact of maternal fasting of height. Most of the analysis is based on cohorts born from 1974 to 1995 because Schultz(2009) and Schultz and Joshi(2007) find that the treatment and control area were balanced in many dimensions. As a robustness check, analysis on birth cohorts born from 1963 to 1995 was also conducted.

[2]Following Almond and Muzumder(2011), we construct the Ramadan month from Institute of Oriental Studies at University of Zurich using their website http://www.oriold.uzh.ch/static/hegira.html

## 6.1 Selection on Timing of Birth

We run the following regression equation to test this assumption:

$$R_{ijhkmt} = \beta_0 + \beta_1 Post + \beta_2 Treated + \beta_3 Treatment + \beta_4 Hindu + \beta_5 Post * Hindu + \beta_6 Treated * Hindu + \beta_7 Treatment * Hindu + \beta_8 X + \alpha_j + \nu_h + \gamma_m + \delta_t + \tau_k + \epsilon_{ijhmt} \quad (1)$$

Equation (1) basically means whether individual i, born by mother j in month m and year t, was in utero during Ramadan or not. The *post* variable takes value 1 if the individual was born in July in 1978. The $Treated$ value is 1 if the person living in a village which gets the treatment and 0 otherwise. Although the program started in October,1977. The variable post takes value 1 from July 1978 and 0 otherwise. The reason is those who were born between October 1977 and July 1978 were conceived prior to program started. We can identify post even with year fixed effect because of the start of the program in middle of the year. The $Treatment$ is the interaction between $Treated$ and $Post$. $\alpha_j$ is the mother fixed effect, $\nu_h$ is household fixed effect, $\gamma_m$ is the month fixed effect, $\delta_t$ is the year fixed effect and $\tau_k$ is the village fixed effect. $R$ represents several outcome variables. From our discussion on measures of Ramadan, $R$ represents various Ramadan measures $ramadan\_0$ to $ramadan\_10$ and $ramadanXtoY$ for various values of X and Y where $X < Y$.The Hindu dummy is 1 if the the mother is Hindu and 0 otherwise. The interaction term of Hindu with Post, Treated and Treatment allows us to check robustness of the avoidance behavior of Muslim mothers relative to Ramadan. The variable $X$ represents community variables and interaction of the community level variables with post. The community level variables are whether the village has large market, electricity, post office, primary school and satellite health clinic.

## 6.2 Impact on Health

We want to know how Ramadan affects on health taking care of selective timing of birth relative to Ramadan. Therefore we are interested in following regression equation

$$Y^M_{iabjhkmt} = \mu_0 + \mu_1 Post + \mu_2 Treated + \mu_3 \hat{R} + \mu_4 Sexc + \alpha_j + \nu_h + \gamma_m + \delta_t + \tau_k + u_{iabjhkmt} \quad (2)$$

Where $Y^M_{iabjhkmt}$ is the height in centimeters and of the Muslim individual ‘i’,born by mother ‘j’ in month‘ m’ and year ‘t’ , who belongs to age group a,household h and village k. Sexc is the sex of the children.

However, the various treatments added to the treatment area make it hard to disentangle the benefit on health between avoiding birth relative to Ramadan and the treatments. In other words, we have to worry about the exclusion restriction of the instrumental variable. We construct the various age groups to capture the different treatment effects on the age group following Barham(2012) The study on height is only limited to 22 birth year cohorts. In this study age group 1 are the individuals who were born from January 1974 to September 1977, age group 2 are the individuals who were born in October 1977 to February 1982, age group 3 are the individuals who were born in March 1982 to December 1988 and the remaining individuals fall in age group 4. The treatment area is interacted with the age groups to capture age specific treatment effects. Following Barham(2012) we regress following regression equation:

$$Y_{iabjhkmt} = \rho_0 + \rho_1 T_i + \rho_3 sexc_i + \sum_{s=2}^{4} \pi_s d_{is} + \sum_{s=2}^{4} \phi_s d_{is} * T_i + \alpha_j + \nu_h + \gamma_m + \delta_t + \tau_k + \varepsilon_{iabjhkmt} \tag{3}$$

The omitted category is the age group 1. The $d_{is}$ is dummy for age group 2 to 4. The dummy variable takes value 1 if individual i belongs to age group s and 0 otherwise. The variable $T_i$ takes value 1 if individual i is from treated area and 0 otherwise. We will run the regression equation on Hindu population in our sample to get the treatment effects of the additional treatments in the treatment area. We will subtract the treatment effects on the specific age groups from the height of Muslim population to calculate the magnitude of impact of fasting on height.

We also estimate the impact of first, second and third trimester overlapping with Ramadan on height with following regression equation:

$$\begin{aligned} Y_{ijhkmt} &= \Gamma_0 + \Gamma_1 ramadan1to3 + \Gamma_2 ramadan4to6 + \Gamma_3 ramadan7to9 \\ &+ \Gamma_4 Hindu + \Gamma_5 Sexc + \Gamma_6 Hindu * Sexc + \Gamma_7 ramadan1to3 * Hindu \\ &+ \Gamma_8 ramadan4to6 * Hindu + \Gamma_9 ramadan7to9 * Hindu + \alpha_j + \nu_h + \gamma_m + \delta_t + \tau_k + \xi_{iabjhkmt} \end{aligned} \tag{4}$$

# 7 Results

## 7.1 Selective Timing of Birth

To study selective timing of birth we regress $ramadan_0$ to $ramadan_{10}$ on post, treated and treatment for Muslims controlling for birth month and year

fixed effects. The cohorts under study were born from year 1974 to 1995. The results are presented in table 1. We find that treatment has statistically significant positive effect on individuals being born in $ramadan\_5$ and negative effect on $ramadan\_9$. This leads us to construct variables $ramadan5to6$, $ramadan5to7$ and $ramadan8to9$ where for example $ramadan8to9$ takes value 1 if individual i is born either during $ramadan_8$ or $ramadan_9$ and 0 otherwise and regress these variables with the same independent variables. The birth pattern of Hindus relative to Ramadan works as a robustness check . We should expect the variables Hindu and post, treated and treatment interacted with Hindu should not be statically significant from zero. We also would like to check the robustness of our results with regression specifications with household fixed effects and biological mother fixed effects. The results are presented in table 2. The dependent variables from column 1 to 3, from 4 to 6 and from 7 to 9 are $ramadan5to6$, $ramadan5to7$ and $ramadan8to9$ respectively. All regression specifications include birth month and year fixed effects. In column 1 we find the coefficient of the treatment variable is positive and statistically significant from zero. On the other hand, in column 2 and 3 when household fixed effects and biological mother fixed effects are applied, it is positive but not statistically significant. In column 4 to 6, we find that the treatment coefficient is positive and statistically significant when regression specifications include birth month and year fixed fixed effects as well as household fixed effects. However, it is not statistically significant when biological fixed effects are applied. In column 7 to 9, the treatment coefficient is negative and statistically significant in all fixed effect regression specifications. As we would expect the variable Hindu and interaction terms with Hindu are not significantly different from zero in all columns and all regression specifications. Since $ramadan5to7$ incorporates $ramadan5o6$, we omit the analysis on $ramadan5o6$ in the following results.

In MHSS data we find that more births take place on date 1 than any other dates. Moreover, data shows unusually high amount of birth birth takes place on January 1st than any other day of the year. We try to check the further robustness of our results in table 2 by dropping these dates. In table 3 we replicate our exercise by dropping date 1. We find the the results are robust to dropping these dates. In table 4 we drop the births taken place on January 1st and we find that the treatment coefficient is statically significant for all regression specifications. The results in table 4 is even more robust than the results in table 2. This suggest there might be some misreporting of birth dates in Matlab data. We don't know which birth took place in Matlab and which didn't. It is quite possible some of these births actually took place out of surveillance area and were not registered in DSS.

Recall that we can identify the variable post in our regression equation even though we have year fixed effects because of the timing of the program. To check whether this does not corrupt our estimates we redefine post where post takes value 1 for 1979 and onward and 0 otherwise. This does not allow us to identify post as the variable post is absorbed by year fixed effects. We do same exercise of table 2 with redefined post variable. The results are presented in table 5. We find that results are consistent with our results in table 2.

So far we have limited our analysis to birth dates known in MHSS. In MHSS data there are some births which have dates recorded zero. The results are presented in table 6. We find that the treatment coefficient is negative for $ramadan8to9$ and positive for $ramadan5to7$ as in table 2. For dependent variable $ramadan5to7$ the treatment coefficient is positive and statistically significant when household fixed effects are included. However, it is not statistically significant in a regression specification with biological mother fixed effects. For variable $ramadan8to9$ the treatment coefficient is negative and significant in regression specification with month and year fixed effects. However, it is not statistically significant when household fixed effects and biological fixed effects are included in the regression specifications.

We again limit our analysis to population whose birth dates are known and further check the robustness by studying the birth cohorts born from 1963 to 1995. This gives us 33 birth year cohorts. Recall that it takes Ramadan around 32 to 33 years to complete a full circuit of western calender. The results are presented in table 7. The results conforms with the findings in table 2.

In table 8 we control for village level characteristics and do the same analysis in table 2 for cohorts born from 1974 to 1995. We find that even including village level characteristics and interacting them with post do not alter our findings.

In table 9 we study the relationship between timing of birth relative to Ramadan with mother education level. The primary takes value 1 if mother education ranges between more than 0 to less than 6 and 0 otherwise. The secondary education takes value 1 if mother has more than 5 years of education and 0 otherwise. The mother education variables are interacted with post treated and treatment variable. What we find is the primary educated are less likely give birth during $ramadan8to9$. The are also more likely to avoid birth using the contraceptive treatment.

In table 10 we study the avoidance behavior with mother education in years. The $mothereduy$ variable takes value the years of education completed by mothers. We find that for variable $ramadan8to9$ the treatment

coefficient is negative and statistically significant. However, the interaction terms with mother education in years with post treated and treatment is not statistically significant.

### 7.2 Discussion on Selective Timing of Birth

Based on the results from table 1 to 8 we can conclude that the Muslim mothers are selectively timing the birth of their children relative to Ramadan. Our results show mothers in the treatment area are around 6 percent less likely to give birth 8 or 9 months after Ramadan and more likely to give birth 5 to 7 months after Ramadan. The avoidance results are robust to inclusion of biological mother fixed effects. Biological mother fixed effects control mother level fixed unobservables that might affect the timing of birth and also affect health outcomes of the children. Ewjik(2011) and following Ewjik(2011), Majid(2012) motivates to solve the problem of selective timing of birth by controlling biological mother fixed effects. Our results suggest controlling biological mother time invariant unobservables may not be enough to take care of selection. We find that Muslims mothers are shifting the births few months ahead and avoiding conception just one or two months before Ramadan. The question is why do we observe such pattern? If the mothers worry that fasting during pregnancy could affect the child health negatively, they would choose a time period where the pregnancy doesn't overlap with Ramadan. Perhaps they don't do it because it is really hard to time birth in that way. The gestation time for human is 266 days and the Islamic Calender completes a year in 354 or 355 days. That leaves mothers 88 or 89 days to give birth to completely avoid Ramadan. Another possibility is that mothers are shifting the births ahead to avoid fasting during Ramadan. It could be possible that when child *in utero* is four or five months older at the time of Ramadan, the family members, friends and relatives are less likely to ask the mother to fast during Ramadan

In table 9 we study the association between mother education level with the timing of birth relative to Ramadan. We find the mothers who had some education but less than equal to primary education are more likely avoid conception 1 or 2 months before Ramadan than others. On other hand the mothers who have more than primary education do not time birth relative to Ramadan. This points out the avoidance behavior is not linearly related to mother education or in other words more education of mothers doesn't mean that they are more likely to avoid birth relative to Ramadan. However, this shouldn't mean that more educated mothers do not care about their children. It could be possible that they are informed about Islamic

rules about fasting for pregnant mothers. According to Islamic law it is not mandatory for Muslims mothers to fast during pregnancy. The mothers with more education are more likely to know about it than mothers with low education. However, if the mothers are somewhat educated like primary or below they might still suffer from the belief that they have to fast during Ramadan even if they are pregnant or they may belong to SES where the social pressure is high for fasting during Ramadan even with pregnancy. The mothers with primary and below education are more likely to avoid birth because they would have some idea about calender and time of Ramadan than mothers with no education. Therefore, they are more likely to use the contraceptive treatment to time birth relative to Ramadan .

### 7.3 Effect of Maternal Fasting on Height

In table 11 we study the impact of maternal fasting overlapped with first trimester second trimester and third trimester on children height for cohort born from 1974 to 1995. As discussed above ramadan7to9 should be interpreted as first trimester, ramadan4to6 should be interpreted as second trimester and ramadan1to3 should be interpreted as third trimester. We also interact the Hindu with these variables to cancel out any seasonality effect. We find that the coefficient of ramadan7to9, ramadan4t06 and ramadan1to3 all are negative. However, only ramadan7to9 and ramadan1to3 are statistically significant when we control for month and year fixed effects. Only coefficient of ramadan7to9 is robust to inclusion of village level fixed effects. These variables are not statistically significant when we control for household fixed effect and biological mother fixed effects. As we would expect, the variable Hindu and interaction hindu with Ramadan variables are also not significant in any regression specification.

Next we study the impact on height if the children were born 8 or 9 months after Ramadan or in other words we regress height on ramadan8to9 on height and compare with children born in Hindu families during the same time. The results are presented in table 12. We find the variable ramadan8to9 is negative and statistically significant and the result is robust to inclusion of village level fixed effects.

This demands us to examine the impact on height taking care of the selective timing of birth. The problem is that the exclusion restriction requirement of the instrument may not hold. We replicate the exercise of Barham(2012) to get the magnitude of the effect of these treatments on children height in centimeter for cohort born from 1974 to 1995. The results are shown in table 13. Column 1 and 2 show results on overall population,

column 3 and 4 show results on Muslim and column 5 and 6 show results only for Hindu. In Column 1 and 2 we find that only age group 3 have benefited from the later treatments in treatment area. This result matches with the findings of Barham(2012). We also find similar results for Hindu population. However, for Muslims only the column 3 and 4 show there is no effect.

In table 14 we show our results on height after taking care of selection. In column 1 and 2 we present first stage and second stage results respectively. In column 1 and 2 we disregard any exclusion restriction problem. We therefore instrument ramadan8to9 with treatment controlling for village level fixed effects. In column (2) we find that instrumeted variable $\hat{ramadan8to9}$ is negative but not significantly different from zero. We construct a variable *mheight* to tackle the problem of exclusion restriction. Under the assumption the treatments added to treatment area had linear effect on the height of the children, we can subtract the effect of those treatments from the height. Fortunately, the Hindus and Muslims in the area received the same treatment in the treatment area. So we can study the average treatment effect of those treatments on the Hindu population and subtract it from height of the Muslims living in the treatment area. The *mheight* is created by subtracting the effect of those treatments on Hindus from height for age group 3. Then we regress mheight on $\hat{ramadan8to9}$. The result is shown in column 3. We find that the coefficient is positive but not significantly different from zero. We then again construct another variable *mheight*2 by subtracting the average treatment effect on age group 3 from column 1 of table 14. The coefficient of $\hat{ramadan8to9}$ is positive but close to zero and not statistically significant from zero.

We also instrument *ramadan8to9* by interacting Muslim with treatment controlling for village level fixed effects. Column 1 in table 15 shows the first stage result. In column 2 we present the second stage results. We find that $\hat{ramadan8to9}$ is negative but not statistically different from zero.

### 7.4 Discussion on Impact on Height

In the previous section we present impact of maternal fasting on height taking care of the selection and with out taking care of the selection. When we do not take care of selection we find that fasting during first trimester affects height negatively. This findings matches the result of Almond and Muzumder(2011) who also find first trimester *in utero* to be very critical for health outcomes. However, this result is not robust to inclusion of household and biological mother fixed effects. Controlling for biological mother fixed

effects limit our analysis to mothers who had given atleast two births. This may not reflect the true effect of fasting. The same reasoning holds for household fixed effects. We also present the difference in difference result with respect to the Hindu population. The results confirm the the impact is only concentrated to Muslim population.

When we take care of selection, we do not find any impact of fasting on height. We try to take care of exclusion restriction by subtracting the treatment effect on the unaffected group the Hindu population. Our results show that subtracting the treatment effect makes our coefficient positive. This suggests violation of exclusion restriction would lead to upward bias of the impact of the maternal fasting. Since we find that $ramad\hat{a}n8to9$ is not significantly different from zero, this means if anything it would not be significantly different from zero even when we didn't have the exclusion restriction problem. We also present results interacting treatment with Muslim, the results do not differ.

The size of coefficient of $ramad\hat{a}n8to9$ is also of interest. In column 2 of table 18 it is $-6.125$ and in column 2 of table 17 it is $-7.444$. The absolute values of the coefficient are rather large compared to the absolute values of the coefficient in column 1 and 2 in table 15.

However, it should be noted that the absence of impact could be due to ITT analysis. The ITT estimate takes the average effect of those who comply and those who do not comply. As a result ITT would give the true estimate of impact when everyone in the sample complies. The relationship of birth pattern with mother education show that the mothers with more education are less likely to comply. As a result, the ITT analysis fails to capture the true impact of the nutrition shock *in utero* due to maternal fasting on child health outcomes.

One serious limitation in our study on height is that it is limited to mothers who were surveyed in MHSS 1996. The children whose mothers didn't survive may have suffered most from fasting and omitting those children might create a downward bias of the impact. Therefore, it is possible that fasting might have impact on the height of the children even after taking care of the selection. It is impossible for us to solve this problem with this data set, as the date of birth is only known from the pregnancy history of the mother. Another limitation is we can only instrument birth timing during $ramadan8to9$. Ideally we would like to instrument the timing of birth relative relative to Ramadan at various times. We only have one instrument. As a result we can instrument only one specific time period relative to Ramadan.

# 8 Conclusion

There is little doubt about the welfare implications of the impact of adverse condition *in utero* on health outcomes. We have seen proliferation of studies which have documented the consequences of *in utero* shocks. However, little was known about how much of those results were driven by selection. In our paper we show that disregarding the selection issue could mislead us about the health impact. In contrast to earlier papers, this paper shows parents selectively time birth relative to Ramadan. In Matlab we find that mothers who received free contraceptives are 6 to 7 percent more likely to avoid birth after 8 or 9 months after Ramadan and around 6 percent more likely to give birth after 5 to 7 months after Ramadan.

We study the impact of maternal fasting on height without taking care of selection. We find the overlap of first trimester *in utero* has significant adverse effect on height. This result is consistent with the results on birth weight studied by Almond and Muzumder(2011). They also find overlap of Ramadan with first trimester leads to lower birth weight. However, when we control for selection problem we do not find any effect of maternal fasting on height. This suggests the impact must have been driven by selection rather than *in utero* nutrition shock.

Policies are based often on empirical results. However, if the results do not represent the true magnitude of the problem it would be hard to design, target and benefit from policies. Moreover, there is an opportunity cost of every policy. This paper shows that selection problem in this literature may lead us to design policies which in reality may not exist or the magnitude is not as big as thought. Therefore, future research should be designed to get the exact magnitude of the impact for various types of *in utero* shock taking care of selection problem.

# 9 Tables

Table 1: Treatment Effect on Birth Timing for Muslims Born from Year 1974 to 1995

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | ramadan_0 | ramadan_1 | ramadan_2 | ramadan_3 | ramadan_4 | ramadan_5 | ramadan_6 | ramadan_7 | ramadan_8 | ramadan_9 | ramadan_10 |
| post | 0.194*** | 0.118*** | 0.118*** | 0.139*** | -0.0877* | -0.285*** | -0.163*** | -0.184*** | -0.0794** | -0.0603* | 0.111*** |
| | (0.0325) | (0.0277) | (0.0329) | (0.0343) | (0.0483) | (0.0444) | (0.0286) | (0.0381) | (0.0353) | (0.0357) | (0.0256) |
| treatment | 0.00639 | 0.00725 | 0.0130 | -0.0235 | 0.00913 | 0.0368* | 0.00443 | 0.0222 | -0.0236 | -0.0359** | 0.00137 |
| | (0.0225) | (0.0181) | (0.0155) | (0.0217) | (0.0201) | (0.0204) | (0.0164) | (0.0160) | (0.0161) | (0.0167) | (0.0201) |
| treated | 0.00471 | -0.00691 | -0.00932 | 0.0256 | -0.0219 | -0.0262 | -0.00794 | -0.00784 | 0.0217 | 0.0190 | -0.00213 |
| | (0.0203) | (0.0164) | (0.0169) | (0.0215) | (0.0179) | (0.0176) | (0.0145) | (0.0141) | (0.0151) | (0.0155) | (0.0175) |
| Constant | -0.000325 | 0.0339* | 0.0138 | 0.196*** | 0.157*** | 0.122*** | 0.135*** | 0.0764*** | 0.0631*** | 0.0937*** | 0.0882*** |
| | (0.0196) | (0.0176) | (0.0178) | (0.0331) | (0.0250) | (0.0249) | (0.0266) | (0.0211) | (0.0175) | (0.0223) | (0.0209) |
| Month& Year FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Mean of Dep. Var. | 0.083 | 0.067 | 0.076 | 0.092 | 0.105 | .102 | 0.092 | 0.094 | .078 | 0.074 | .074 |
| Observations | 5,935 | 5,935 | 5,935 | 5,935 | 5,935 | 5,935 | 5,935 | 5,935 | 5,935 | 5,935 | 5,935 |
| R-squared | 0.070 | 0.056 | 0.054 | 0.057 | 0.047 | 0.060 | 0.039 | 0.055 | 0.043 | 0.046 | 0.060 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Treatment Effect on Birth Timing for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>ramadan5to6 | (2)<br>ramadan5to6 | (3)<br>ramadan5to6 | (4)<br>ramadan5to7 | (5)<br>ramadan5to7 | (6)<br>ramadan5to7 | (7)<br>ramadan8to9 | (8)<br>ramadan8to9 | (9)<br>ramadan8to9 |
|---|---|---|---|---|---|---|---|---|---|
| post | -0.449*** | -0.449*** | -0.452*** | -0.632*** | -0.626*** | -0.618*** | -0.129*** | -0.0814 | -0.0747 |
| | (0.0446) | (0.0679) | (0.0693) | (0.0430) | (0.0610) | (0.0642) | (0.0446) | (0.0682) | (0.0692) |
| treatment | 0.0408* | 0.0527 | 0.0425 | 0.0635** | 0.0846** | 0.0666 | -0.0596*** | -0.0750** | -0.0658** |
| | (0.0238) | (0.0388) | (0.0407) | (0.0247) | (0.0412) | (0.0432) | (0.0223) | (0.0337) | (0.0321) |
| treated | -0.0344 | | | -0.0423* | | | 0.0417** | | |
| | (0.0215) | | | (0.0233) | | | (0.0207) | | |
| hindu | 0.0728 | | | 0.138 | | | -0.0632 | | |
| | (0.0911) | | | (0.110) | | | (0.0617) | | |
| posthindu | -0.0434 | -0.0607 | 0.00529 | -0.125 | -0.0651 | -0.0297 | 0.0656 | 0.0373 | -0.0335 |
| | (0.0901) | (0.137) | (0.111) | (0.120) | (0.172) | (0.172) | (0.0695) | (0.172) | (0.123) |
| treatedhindu | -0.0932 | | | -0.134 | | | 0.100 | | |
| | (0.0980) | | | (0.120) | | | (0.0733) | | |
| treatmenthindu | 0.0192 | 0.0268 | 0.000281 | 0.0740 | -0.0257 | -0.000289 | -0.0654 | -0.0173 | 0.0462 |
| | (0.0966) | (0.149) | (0.134) | (0.131) | (0.190) | (0.201) | (0.0871) | (0.186) | (0.145) |
| Constant | 0.254*** | 0.234*** | 0.232*** | 0.337*** | 0.326*** | 0.318*** | 0.154*** | 0.183*** | 0.192*** |
| | (0.0345) | (0.0488) | (0.0488) | (0.0340) | (0.0454) | (0.0476) | (0.0254) | (0.0337) | (0.0344) |
| Month& Year FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| HH FE | N | Y | N | N | Y | N | N | Y | N |
| Mother FE | N | N | Y | N | N | Y | N | N | Y |
| Mean of Dep Var. | 0.192 | 0.192 | 0.192 | 0.287 | 0.287 | 0.287 | 0.154 | 0.154 | 0.154 |
| Observations | 6,474 | 6,474 | 6,474 | 6,474 | 6,474 | 6,474 | 6,474 | 6,474 | 6,474 |
| R-squared | 0.099 | 0.381 | 0.402 | 0.144 | 0.429 | 0.449 | 0.082 | 0.386 | 0.403 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Treatment Effect on Birth Timing for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>ramadan5to7 | (2)<br>ramadan5to7 | (3)<br>ramadan5to7 | (4)<br>ramadan8to9 | (5)<br>ramadan8to9 | (6)<br>ramadan8to9 |
|---|---|---|---|---|---|---|
| post | -0.678*** | -0.709*** | -0.702*** | -0.190*** | -0.125 | -0.120 |
| | (0.0484) | (0.0643) | (0.0670) | (0.0497) | (0.0792) | (0.0809) |
| treatment | 0.0568** | 0.0912* | 0.0743 | -0.0748*** | -0.0904** | -0.0829** |
| | (0.0280) | (0.0461) | (0.0475) | (0.0274) | (0.0408) | (0.0395) |
| treated | -0.0360 | | | 0.0570** | | |
| | (0.0251) | | | (0.0254) | | |
| hindu | 0.212 | | | -0.00907 | | |
| | (0.173) | | | (0.0716) | | |
| posthindu | -0.194 | -0.0910 | -0.0440 | 0.0115 | -0.0189 | -0.0726 |
| | (0.185) | (0.266) | (0.249) | (0.0708) | (0.188) | (0.166) |
| treatedhindu | -0.212 | | | 0.0580 | | |
| | (0.180) | | | (0.0833) | | |
| treatmenthindu | 0.152 | 0.00470 | 0.00750 | -0.0152 | 0.0798 | 0.130 |
| | (0.192) | (0.285) | (0.280) | (0.0910) | (0.204) | (0.186) |
| Constant | 0.347*** | 0.334*** | 0.327*** | 0.175*** | 0.206*** | 0.216*** |
| | (0.0396) | (0.0566) | (0.0609) | (0.0309) | (0.0412) | (0.0433) |
| Month& Year FE | Y | Y | Y | Y | Y | Y |
| HH FE | N | Y | N | N | Y | N |
| Mother FE | N | N | Y | N | N | Y |
| Mean of Dep. Var. | 0.290 | 0.290 | 0.290 | 0.155 | 0.155 | 0.155 |
| Observations | 5,754 | 5,754 | 5,754 | 5,754 | 5,754 | 5,754 |
| R-squared | 0.147 | 0.447 | 0.467 | 0.086 | 0.417 | 0.432 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Treatment Effect on Birth Timing for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>ramadan5to7 | (2)<br>ramadan5to7 | (3)<br>ramadan5to7 | (4)<br>ramadan8to9 | (5)<br>ramadan8to9 | (6)<br>ramadan8to9 |
|---|---|---|---|---|---|---|
| post | -0.705*** | -0.731*** | -0.727*** | -0.156*** | -0.0943 | -0.0883 |
| | (0.0436) | (0.0580) | (0.0607) | (0.0469) | (0.0748) | (0.0766) |
| treatment | 0.0571** | 0.0915** | 0.0729* | -0.0649*** | -0.0864** | -0.0767** |
| | (0.0257) | (0.0401) | (0.0428) | (0.0241) | (0.0367) | (0.0349) |
| treated | -0.0375 | | | 0.0484** | | |
| | (0.0238) | | | (0.0224) | | |
| hindu | 0.194 | | | -0.0505 | | |
| | (0.141) | | | (0.0674) | | |
| posthindu | -0.177 | -0.104 | -0.0747 | 0.0437 | 0.00390 | -0.0297 |
| | (0.150) | (0.198) | (0.193) | (0.0683) | (0.160) | (0.139) |
| treatedhindu | -0.201 | | | 0.0830 | | |
| | (0.150) | | | (0.0795) | | |
| treatmenthindu | 0.139 | 0.0444 | 0.0834 | -0.0392 | 0.0500 | 0.0776 |
| | (0.159) | (0.216) | (0.225) | (0.0878) | (0.175) | (0.159) |
| Constant | 0.346*** | 0.326*** | 0.318*** | 0.182*** | 0.212*** | 0.224*** |
| | (0.0351) | (0.0512) | (0.0556) | (0.0266) | (0.0380) | (0.0388) |
| Month& Year FE | Y | Y | Y | Y | Y | Y |
| HH FE | N | Y | N | N | Y | N |
| Mother FE | N | N | Y | N | N | Y |
| Mean of Dep. Var. | 0.285 | 0.285 | 0.285 | 0.154 | 0.154 | 0.154 |
| Observations | 6,317 | 6,317 | 6,317 | 6,317 | 6,317 | 6,317 |
| R-squared | 0.146 | 0.435 | 0.455 | 0.083 | 0.394 | 0.410 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Treatment Effect on Birth Timing for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>ramadan5to7 | (2)<br>ramadan5to7 | (3)<br>ramadan5to7 | (4)<br>ramadan8to9 | (5)<br>ramadan8to9 | (6)<br>ramadan8to9 |
|---|---|---|---|---|---|---|
| treatment | 0.0553** | 0.0605 | 0.0393 | -0.0610*** | -0.0649** | -0.0549* |
| | (0.0253) | (0.0426) | (0.0446) | (0.0201) | (0.0316) | (0.0306) |
| treated | -0.0341 | | | 0.0415** | | |
| | (0.0231) | | | (0.0185) | | |
| hindu | 0.0238 | | | -0.00843 | | |
| | (0.0280) | | | (0.0318) | | |
| treatedhindu | -0.0253 | | | 0.0377 | | |
| | (0.0512) | | | (0.0480) | | |
| treatmenthindu | -0.0466 | -0.105 | -0.0477 | 0.0102 | 0.0176 | 0.0114 |
| | (0.0485) | (0.0752) | (0.0998) | (0.0496) | (0.0680) | (0.0739) |
| Constant | 0.350*** | 0.338*** | 0.330*** | 0.156*** | 0.185*** | 0.195*** |
| | (0.0341) | (0.0450) | (0.0480) | (0.0243) | (0.0340) | (0.0348) |
| Month& Year FE | Y | Y | Y | Y | Y | Y |
| HH FE | N | Y | N | N | Y | N |
| Mother FE | N | N | Y | N | N | Y |
| Mean of Dep.Var. | 0.287 | 0.287 | 0.287 | 0.154 | 0.154 | 0.154 |
| Observations | 6,474 | 6,474 | 6,474 | 6,474 | 6,474 | 6,474 |
| R-squared | 0.127 | 0.418 | 0.437 | 0.080 | 0.385 | 0.403 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Treatment Effect on Birth Timing for Born from cohorts 1974 to 1995 including date zero

| VARIABLES | (1)<br>ramadan5to7 | (2)<br>ramadan5to7 | (3)<br>ramadan5to7 | (4)<br>ramadan8to9 | (5)<br>ramadan8to9 | (6)<br>ramadan8to9 |
|---|---|---|---|---|---|---|
| post | -0.738*** | -0.718*** | -0.720*** | -0.108** | -0.0676 | -0.0501 |
| | (0.0348) | (0.0446) | (0.0454) | (0.0416) | (0.0607) | (0.0620) |
| treatment | 0.0509** | 0.0594* | 0.0509 | -0.0475** | -0.0437 | -0.0385 |
| | (0.0228) | (0.0353) | (0.0385) | (0.0192) | (0.0294) | (0.0319) |
| treated | -0.0339 | | | 0.0316* | | |
| | (0.0213) | | | (0.0178) | | |
| hindu | 0.184* | | | -0.0853 | | |
| | (0.105) | | | (0.0579) | | |
| posthindu | -0.151 | -0.0862 | -0.0588 | 0.0583 | 0.0151 | -0.0476 |
| | (0.110) | (0.130) | (0.136) | (0.0663) | (0.122) | (0.0910) |
| treatedhindu | -0.218* | | | 0.155** | | |
| | (0.113) | | | (0.0673) | | |
| treatmenthindu | 0.158 | 0.0885 | 0.115 | -0.0983 | -0.0320 | 0.0193 |
| | (0.116) | (0.151) | (0.167) | (0.0803) | (0.134) | (0.112) |
| Constant | 0.322*** | 0.303*** | 0.298*** | 0.199*** | 0.231*** | 0.231*** |
| | (0.0307) | (0.0399) | (0.0439) | (0.0258) | (0.0319) | (0.0337) |
| Month& Year FE | Y | Y | Y | Y | Y | Y |
| HH FE | N | Y | N | N | Y | N |
| Mother FE | N | N | Y | N | N | Y |
| Observations | 7,914 | 7,914 | 7,914 | 7,914 | 7,914 | 7,914 |
| R-squared | 0.144 | 0.413 | 0.435 | 0.102 | 0.379 | 0.400 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Treatment Effect on Birth Timing for cohorts Born from 1963 to 1995

| | | | | | | |
|---|---|---|---|---|---|---|
| post | -0.586*** | -0.549*** | -0.537*** | -0.234*** | -0.209*** | -0.211*** |
| | (0.0442) | (0.0563) | (0.0577) | (0.0453) | (0.0607) | (0.0616) |
| treatment | 0.0598*** | 0.0640** | 0.0481 | -0.0333** | -0.0411* | -0.0251 |
| | (0.0183) | (0.0301) | (0.0329) | (0.0134) | (0.0241) | (0.0265) |
| treated | -0.0431*** | | | 0.0133 | | |
| | (0.0162) | | | (0.0109) | | |
| Constant | 0.248*** | 0.234*** | 0.265*** | 0.234*** | 0.278*** | 0.296*** |
| | (0.0484) | (0.0639) | (0.0685) | (0.0543) | (0.0762) | (0.0765) |
| Month& Year FE | Y | Y | Y | Y | Y | Y |
| HH FE | N | Y | N | N | Y | N |
| Mother FE | N | N | Y | N | N | Y |
| Observations | 7,475 | 7,475 | 7,475 | 7,475 | 7,475 | 7,475 |
| R-squared | 0.071 | 0.328 | 0.357 | 0.027 | 0.294 | 0.317 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Treatment Effect on Birth Timing for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>ramadan5to7 | (2)<br>ramadan5to7 | (3)<br>ramadan5to7 | (4)<br>ramadan8to9 | (5)<br>ramadan8to9 | (6)<br>ramadan8to9 |
|---|---|---|---|---|---|---|
| post | -0.697*** | -0.678*** | -0.659*** | -0.0953* | -0.0478 | -0.0400 |
| | (0.0572) | (0.0838) | (0.0886) | (0.0554) | (0.0788) | (0.0784) |
| treated | -0.0465* | | | 0.0378* | | |
| | (0.0277) | | | (0.0220) | | |
| treatment | 0.0576* | 0.0635 | 0.0407 | -0.0555** | -0.0826** | -0.0715* |
| | (0.0296) | (0.0470) | (0.0484) | (0.0234) | (0.0378) | (0.0369) |
| hindu | 0.105 | | | -0.0583 | | |
| | (0.123) | | | (0.0765) | | |
| posthindu | -0.0960 | 0.00471 | 0.0317 | 0.0575 | 0.0491 | -0.0379 |
| | (0.137) | (0.207) | (0.215) | (0.0885) | (0.213) | (0.153) |
| treatedhindu | -0.106 | | | 0.0883 | | |
| | (0.131) | | | (0.0843) | | |
| treatmenthindu | 0.0536 | -0.0797 | -0.0475 | -0.0523 | -0.0185 | 0.0580 |
| | (0.145) | (0.222) | (0.241) | (0.100) | (0.224) | (0.169) |
| Constant | 0.405*** | 0.329*** | 0.322*** | 0.119*** | 0.179*** | 0.188*** |
| | (0.0543) | (0.0459) | (0.0487) | (0.0409) | (0.0345) | (0.0349) |
| Month& Year FE | Y | Y | Y | Y | Y | Y |
| HH FE | N | Y | N | N | Y | N |
| Mother FE | N | N | Y | N | N | Y |
| Additional Comm. Var. | Y | Y | Y | Y | Y | Y |
| Mean of Dep. Var. | 0.286 | 0.286 | 0.286 | 0.155 | 0.155 | 0.155 |
| Observations | 6,431 | 6,431 | 6,431 | 6,431 | 6,431 | 6,431 |
| R-squared | 0.145 | 0.430 | 0.449 | 0.084 | 0.386 | 0.403 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Effect of Mother Education on Birth Timing for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>ramadan5to7 | (2)<br>ramadan8to9 |
|---|---|---|
| post | -0.693*** | -0.128** |
| | (0.0581) | (0.0595) |
| treated | -0.0345 | 0.0232 |
| | (0.0381) | (0.0306) |
| treatment | 0.0396 | -0.0343 |
| | (0.0386) | (0.0314) |
| primary | -0.00160 | -0.0729** |
| | (0.0499) | (0.0335) |
| postprimary | 0.00462 | 0.0851** |
| | (0.0519) | (0.0332) |
| treatedprimary | -0.0266 | 0.0656 |
| | (0.0628) | (0.0482) |
| treatmentprimary | 0.0353 | -0.0891* |
| | (0.0679) | (0.0486) |
| secondary | 0.0197 | 0.0269 |
| | (0.0903) | (0.0793) |
| postsecondary | -0.0377 | -0.00502 |
| | (0.0919) | (0.0713) |
| treatedsecondary | 0.0721 | -0.0626 |
| | (0.112) | (0.0915) |
| treatmentsecondary | -0.0409 | 0.0489 |
| | (0.122) | (0.0865) |
| Constant | 0.403*** | 0.141*** |
| | (0.0559) | (0.0455) |
| Month& Year FE | Y | Y |
| Additional Comm. Var. | Y | Y |
| Observations | 5,838 | 5,838 |
| R-squared | 0.145 | 0.084 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Effect of Mother Education on Birth Timing for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>ramadan5to7 | (2)<br>ramadan8to9 |
|---|---|---|
| post | -0.692*** | -0.119* |
| | (0.0622) | (0.0612) |
| treated | -0.0371 | 0.0423* |
| | (0.0304) | (0.0228) |
| treatment | 0.0466 | -0.0627** |
| | (0.0314) | (0.0243) |
| mothereduy | -0.00195 | -0.00128 |
| | (0.00236) | (0.00192) |
| postmothereduy | 0.000721 | 0.000453 |
| | (0.00241) | (0.00198) |
| treatedmothereduy | 0.00140 | 0.000345 |
| | (0.00283) | (0.00217) |
| treatmentmothereduy | -0.000712 | -0.000954 |
| | (0.00287) | (0.00236) |
| Constant | 0.405*** | 0.146*** |
| | (0.0588) | (0.0468) |
| Month& Year FE | Y | Y |
| Observations | 5,629 | 5,629 |
| Additional Comm. Var. | Y | Y |
| R-squared | 0.144 | 0.086 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Fasting Effect on Height(cm) for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>height | (2)<br>height | (3)<br>height | (4)<br>height |
|---|---|---|---|---|
| ramadan1to3 | -1.077* | -0.901 | -0.0794 | -0.117 |
| | (0.572) | (0.586) | (1.212) | (1.196) |
| ramadan4to6 | -0.690 | -0.576 | 0.628 | 0.761 |
| | (0.623) | (0.631) | (1.402) | (1.385) |
| ramadan7to9 | -1.207** | -1.163** | -0.312 | -0.197 |
| | (0.558) | (0.557) | (1.025) | (1.013) |
| sexc | 2.506*** | 2.484*** | 2.538*** | 2.647*** |
| | (0.318) | (0.335) | (0.544) | (0.553) |
| hindu | -0.800 | -0.622 | | |
| | (1.112) | (1.083) | | |
| sexchindu | 1.166 | 0.786 | -0.238 | -0.139 |
| | (0.923) | (0.982) | (1.502) | (1.505) |
| ramadan1to3hindu | 1.018 | 0.182 | 1.317 | 1.250 |
| | (1.149) | (1.208) | (1.833) | (1.811) |
| ramadan4to6hindu | 0.264 | -1.186 | 1.362 | 1.223 |
| | (1.623) | (1.616) | (2.402) | (2.420) |
| ramadan7to9hindu | -0.427 | -0.608 | 0.916 | 0.806 |
| | (1.484) | (1.426) | (2.107) | (2.102) |
| Constant | 159.1*** | 158.1*** | 159.1*** | 158.9*** |
| | (1.442) | (1.710) | (4.317) | (4.767) |
| Month& Year FE | Y | Y | Y | Y |
| Village FE | N | Y | N | Y |
| HH FE | N | N | Y | N |
| Mother FE | N | N | N | Y |
| Observations | 2,970 | 2,970 | 2,970 | 2,970 |
| R-squared | 0.890 | 0.898 | 0.964 | 0.966 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Fasting Effect on Height(cm) for cohorts Born from 1974 to 1995

| VARIABLES | (1)<br>height | (2)<br>height |
|---|---|---|
| ramadan8to9 | -1.131** | -1.130** |
| | (0.548) | (0.547) |
| sexc | 2.541*** | 2.548*** |
| | (0.305) | (0.331) |
| sexchindu | 1.003 | 0.265 |
| | (0.824) | (1.053) |
| ramadan8to9hindu | -1.772 | -1.276 |
| | (1.212) | (1.311) |
| Constant | 158.3*** | 157.5*** |
| | (1.329) | (1.636) |
| Month& Year FE | Y | Y |
| Village FE | N | Y |
| Observations | 2,970 | 2,970 |
| R-squared | 0.891 | 0.898 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Effect of additional treatments in treatment area on Height(cm) for cohort 1974 to 1995

| VARIABLES | (1) height | (2) height | (3) height | (4) height | (5) height | (6) height |
|---|---|---|---|---|---|---|
| treated | -1.336 | -1.428 | -1.056 | -1.129 | -1.768 | -2.269 |
| | (1.302) | (1.312) | (1.387) | (1.403) | (2.933) | (3.409) |
| ageg2 | -4.664*** | -2.274 | -4.488*** | -2.197 | -2.157 | 1.354 |
| | (1.394) | (1.405) | (1.513) | (1.525) | (4.232) | (4.666) |
| ageg3 | -8.498*** | -3.527 | -7.501*** | -2.619 | -17.72*** | -10.61* |
| | (2.283) | (2.311) | (2.445) | (2.502) | (5.528) | (5.620) |
| ageg4 | -79.48*** | -79.16*** | -71.18*** | -70.90*** | -75.07*** | -73.44*** |
| | (1.392) | (1.431) | (1.310) | (1.350) | (3.359) | (3.670) |
| ageg2*treated | 2.487 | 2.305 | 2.423 | 2.276 | -1.498 | -1.823 |
| | (1.696) | (1.693) | (1.760) | (1.759) | (3.719) | (4.280) |
| ageg3*treated | 2.644* | 2.728* | 2.329 | 2.427 | 5.198* | 5.391* |
| | (1.567) | (1.566) | (1.665) | (1.671) | (2.997) | (2.998) |
| ageg4*treated | 1.685 | 1.664 | 1.587 | 1.564 | -0.0700 | -0.0255 |
| | (1.280) | (1.273) | (1.311) | (1.311) | (3.157) | (3.256) |
| sexc | 2.363*** | 2.357*** | 2.302*** | 2.315*** | 3.136 | 2.769 |
| | (0.409) | (0.408) | (0.436) | (0.431) | (2.326) | (2.721) |
| sexc*treated | 0.290 | 0.333 | 0.325 | 0.340 | -0.920 | -0.843 |
| | (0.513) | (0.511) | (0.578) | (0.574) | (2.422) | (2.945) |
| Constant | 157.0*** | 158.8*** | 157.4*** | 159.1*** | 144.8*** | 148.0*** |
| | (1.056) | (1.209) | (1.049) | (1.258) | (3.193) | (4.101) |
| Year FE | Y | N | Y | N | | |
| Month& Year FE | N | Y | N | Y | | |
| Observations | 3,430 | 3,430 | 3,137 | 3,137 | 293 | 293 |
| R-squared | 0.887 | 0.890 | 0.885 | 0.888 | 0.929 | 0.936 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 14: First stage and Second Stage results on Height(cm) for cohort 1974 to 1995

| VARIABLES | (1)<br>ramadan8to9 | (2)<br>height | (3)<br>mheight | (4)<br>mheight2 |
|---|---|---|---|---|
| post | -0.00509 | -1.444 | 0.0437 | -0.695 |
| | (0.0899) | (2.178) | (2.130) | (2.150) |
| treatment | -0.162*** | | | |
| | (0.0505) | | | |
| sexc | 0.00764 | 2.444*** | 2.364*** | 2.404*** |
| | (0.0137) | (0.339) | (0.332) | (0.334) |
| $\hat{ramadan8to9}$ | | -7.449 | 8.388 | 0.529 |
| | | (8.870) | (8.812) | (8.838) |
| Constant | 0.163*** | 159.0*** | 156.5*** | 157.8*** |
| | (0.0489) | (1.973) | (1.997) | (1.984) |
| Month& Year FE | Y | Y | Y | Y |
| Village FE | Y | Y | Y | Y |
| Observations | 2,718 | 2,718 | 2,718 | 2,718 |
| R-squared | 0.163 | 0.897 | 0.894 | 0.896 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

Table 15: First stage and Second Stage results on Height(cm) for cohort 1974 to 1995

| VARIABLES | (1)<br>ramadan8to9 | (2)<br>height |
|---|---|---|
| post | 0.0105 | -1.640 |
| | (0.0818) | (2.074) |
| treatment | -0.163*** | |
| | (0.0497) | |
| sexc | 0.00840 | 2.524*** |
| | (0.0137) | (0.334) |
| hindu | -0.145** | -2.755 |
| | (0.0607) | (2.839) |
| posthindu | 0.0102 | 2.006 |
| | (0.0675) | (3.085) |
| treatedhindu | -0.0115 | -1.507 |
| | (0.0910) | (3.915) |
| treatmenthindu | 0.0310 | 0.592 |
| | (0.0968) | (4.151) |
| sexchindu | 0.00786 | 1.050 |
| | (0.0198) | (0.991) |
| ramadan8to9hindu | 0.893*** | 3.703 |
| | (0.0194) | (7.702) |
| $\hat{ramadan8to9}$ | | -6.125 |
| | | (8.628) |
| Constant | 0.166*** | 158.4*** |
| | (0.0470) | (1.906) |
| Month& Year FE | Y | Y |
| Village FE | Y | Y |
| Observations | 2,970 | 2,970 |
| R-squared | 0.233 | 0.898 |

Standard errors in parentheses clustered at village ID level
*** p<0.01, ** p<0.05, * p<0.1

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# 11 Appendix

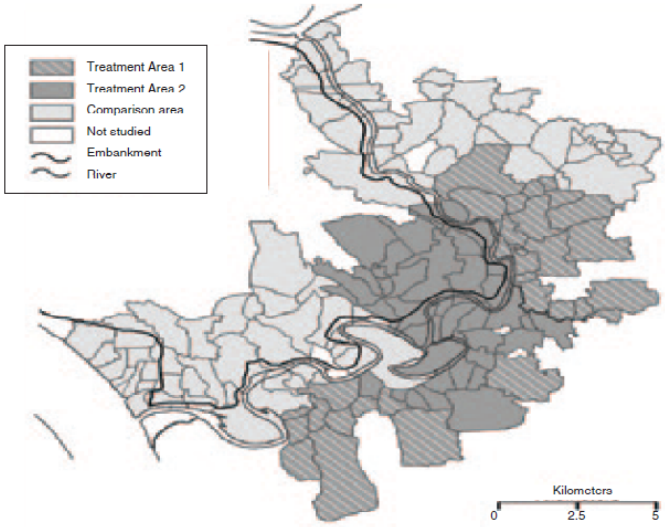


Figure 1. Map of Matlab Study Area

Table 3—1974 Baseline Characteristics

| | Treatment Area | | | Comparison Area | | | Difference in Means | | |
|---|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Obs. | Mean | SD | Obs. | Mean | T-stat | Mean/SD |
| *Panel A. Full Sample* | | | | | | | | | |
| Family size | 7.01 | (5.15) | 2,124 | 6.82 | (4.20) | 2,548 | 0.18 | 1.34 | 0.04 |
| Owns a lamp (=1) | 0.65 | (1.18) | 2,124 | 0.61 | (0.92) | 2,548 | 0.04 | 1.37 | 0.04 |
| Owns a watch (=1) | 0.16 | (0.94) | 2,124 | 0.16 | (0.62) | 2,548 | 0.00 | 0.05 | 0.00 |
| Owns a radio (=1) | 0.08 | (0.63) | 2,124 | 0.08 | (0.47) | 2,548 | 0.00 | 0.15 | 0.00 |
| Wall tin or tinmix (=1) | 0.32 | (1.08) | 2,124 | 0.31 | (0.78) | 2,548 | 0.01 | 0.27 | 0.01 |
| Tin roof (=1) | 0.83 | (0.66) | 2,124 | 0.84 | (0.69) | 2,548 | 0.00 | −0.10 | 0.00 |
| Latrine (=1) | 0.83 | (0.78) | 2,124 | 0.85 | (0.86) | 2,548 | −0.03 | −1.22 | −0.03 |
| Number of rooms per capita | 0.21 | (0.15) | 2,124 | 0.21 | (0.18) | 2,548 | 0.00 | 0.39 | 0.01 |
| Number of cows | 1.55 | (3.30) | 2,124 | 1.37 | (3.05) | 2,548 | 0.19 | 2.02 | 0.06 |
| Number of boats | 0.68 | (1.50) | 2,124 | 0.68 | (1.42) | 2,548 | −0.01 | −0.20 | −0.01 |
| Drinking water, tubewell (=1) | 0.31 | (1.41) | 2,124 | 0.16 | (0.93) | 2,548 | 0.15 | 4.35 | 0.11 |
| Drinking water, tank (=1) | 0.38 | (1.72) | 2,124 | 0.33 | (1.68) | 2,548 | 0.05 | 1.01 | 0.03 |
| Drinking water, other (=1) | 0.31 | (2.21) | 2,124 | 0.51 | (1.84) | 2,548 | −0.20 | −3.39 | −0.09 |
| HH age | 47.8 | (22) | 2,124 | 46.5 | (22) | 2,548 | 1.28 | 1.97 | 0.06 |
| HH years of education (edu.) | 2.52 | (6.72) | 2,124 | 2.34 | (5.25) | 2,548 | 0.17 | 1.25 | 0.02 |
| HH works in agriculture (=1) | 0.61 | (0.89) | 2,124 | 0.59 | (0.99) | 2,548 | 0.02 | 0.79 | 0.02 |
| HH works in fishing (=1) | 0.05 | (0.53) | 2,124 | 0.06 | (0.49) | 2,548 | −0.01 | −0.57 | −0.02 |
| HH spouse's age | 37.0 | (17) | 2,124 | 36.2 | (16) | 2,548 | 0.86 | 1.72 | 0.05 |
| HH spouse's years of edu. | 1.14 | (3.58) | 2,124 | 1.21 | (2.56) | 2,548 | −0.07 | −0.95 | −0.02 |
| *Panel B. Age 8–14* | | | | | | | | | |
| Family size | 6.59 | (3.09) | 188 | 6.87 | (3.20) | 304 | −0.27 | −0.97 | −0.08 |
| Owns a lamp (=1) | 0.63 | (0.52) | 188 | 0.60 | (0.69) | 304 | 0.03 | 0.63 | 0.05 |
| Owns a watch (=1) | 0.16 | (0.71) | 188 | 0.17 | (0.43) | 304 | −0.01 | −0.13 | −0.01 |
| Owns a radio (=1) | 0.09 | (0.51) | 188 | 0.09 | (0.29) | 304 | 0.01 | 0.14 | 0.01 |
| Wall tin or tinmix (=1) | 0.29 | (0.72) | 188 | 0.32 | (0.43) | 304 | −0.03 | −0.49 | −0.05 |
| Tin roof (=1) | 0.81 | (0.45) | 188 | 0.88 | (0.52) | 304 | −0.07 | −1.57 | −0.14 |
| Latrine (=1) | 0.85 | (0.39) | 188 | 0.85 | (0.52) | 304 | −0.01 | −0.13 | −0.01 |
| Number of rooms per capita | 0.21 | (0.08) | 188 | 0.21 | (0.18) | 304 | 0.00 | 0.09 | 0.01 |
| Number of cows | 1.52 | (2.51) | 188 | 1.38 | (2.22) | 304 | 0.14 | 0.60 | 0.06 |
| Number of boats | 0.67 | (0.98) | 188 | 0.67 | (0.92) | 304 | −0.01 | −0.07 | −0.01 |
| Drinking water, tubewell (=1) | 0.27 | (0.71) | 188 | 0.13 | (0.58) | 304 | 0.14 | 2.28 | 0.21 |
| Drinking water, tank (=1) | 0.37 | (0.84) | 188 | 0.31 | (0.99) | 304 | 0.06 | 0.75 | 0.07 |
| Drinking water, other (=1) | 0.36 | (1.12) | 188 | 0.56 | (0.97) | 304 | −0.21 | −2.09 | −0.18 |
| HH age | 48.9 | (17) | 188 | 46.3 | (18) | 304 | 2.58 | 1.61 | 0.15 |
| HH years of education | 1.92 | (3.84) | 188 | 2.10 | (3.15) | 304 | −0.18 | −0.61 | −0.05 |
| HH works in agriculture (=1) | 0.67 | (0.45) | 188 | 0.55 | (0.67) | 304 | 0.12 | 2.29 | 0.19 |
| HH works in fishing (=1) | 0.07 | (0.35) | 188 | 0.07 | (0.38) | 304 | 0.00 | 0.11 | 0.01 |
| HH spouse's age | 37.6 | (15) | 188 | 35.8 | (14) | 304 | 1.82 | 1.37 | 0.13 |
| HH spouse's years of edu. | 1.00 | (1.39) | 188 | 1.20 | (1.58) | 304 | −0.20 | −1.58 | −0.12 |

Table 1—MCH-FP Program Eligibility by Birth Year

| Birth cohorts | Birth cohort label[a] | Program eligibility[b] |
|---|---|---|
| October 1947–September 1972 | 25–49 | *Pre-intervention group* |
| October 1972–September 1977 | 20–24 | *No interventions, potential sibling competition* |
| | | i. Not eligible for child health interventions and unlikely to use family planning and maternal health interventions.<br>ii. Potentially affected by the program through sibling competition. |
| October 1977–February 1982 | 15–19 | *Intensive family planning and maternal health interventions* |
| | | i. Mother eligible for family planning, tetanus toxoid vaccine, folic acid, and iron in last trimester of pregnancy.<br>ii. Children under age five eligible for mainly late measles vaccination in Treatment Area 1.<br>iii. Potentially affected by sibling competition from younger groups. |
| March 1982–December 1988 | 8–14 | *Child health interventions added* |
| March 1982–October1985 | 12–14 | *Child health interventions added in Treatment Area 1* |
| | | i. Mother eligible for family planning, tetanus toxoid vaccine, folic acid, and iron in last trimester of pregnancy.<br>ii. Children under age five eligible for on-time measles vaccination in Treatment Area 1, but for late DPT, polio, and tuberculosis vaccination in entire treatment area. |
| November 1985–December 1988 | 8–11 | *Child health intervention extended to entire treatment area* |
| | | i. Mother eligible for family, tetanus toxoid vaccine, folic acid and iron in last trimester of pregnancy.<br>ii. Children under age five eligible for on-time vaccination (measles, DPT, polio, tuberculosis) and vitamin A supplementation.<br>iii. Nutrition rehabilitation for children at risk. |

# Early Life Circumstance and Mental Health in Ghana*

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**Abstract**

We study the impacts of income fluctuations in early life on adult mental health, an important determinant of utility that has been overlooked in the "fetal origins" literature. Combining a time series of real producer prices of cocoa with a nationally representative household survey in Ghana, we show that a one standard deviation rise in the cocoa price in early life decreases the likelihood of severe mental distress in adulthood by 3 percentage points for individuals born in cocoa-producing regions relative to the same birth cohort born in other regions. This effect is nearly half the mean prevalence of severe distress. Impacts on alternative measures of mental health are consistent with the results on mental distress. This effect is strongest for income fluctuations occurring in the year of birth. There are smaller but significant impacts for fluctuations during the first 4 years of life. We find little evidence of positive effects on physical health, wealth, or migration. Together, our results suggest that early life circumstances can dramatically impact overall utility through effects on mental health, even when impacts on other welfare measures are muted.

*Keywords: early life, mental health, endowments, commodity prices, Ghana*
*JEL Classification Codes: I12, I15, O12*

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# 1 Introduction

Mental health disorders account for 13 percent of the overall global disease burden (Collins et al., 2011). The economic losses of mental health disorders in low-income countries are staggeringly large: for example, depression is estimated to generate losses of 55.5 million disability-adjusted life years (DALYs) in low- and middle-income countries, compared to 10 million DALYs in high-income countries (Mathers et al., 2008).[1] Despite the costs of these disorders, both in terms of health and economic development, investment in prevention and treatment remains low relative to the disease burden in most countries (Collins et al., 2011).

Given the burden of mental health disorders in low-income countries, it is crucial to gain a better understanding of their origins. In this study, we ask: how does circumstance in early life affect psychological distress in adulthood? We examine this relationship using data from nationally representative household survey data from Ghana, which includes a module comprising the Kessler Psychological Distress Scale, an internationally validated measure of anxiety-depression spectrum mental distress (Kessler et al., 2002). We exploit variation in early life conditions induced by changes in the real producer price of cocoa. Cocoa is Ghana's chief agricultural export commodity, and its price is a key determinant of household incomes in the regions where it is grown. We show that in cocoa-producing regions of Ghana, low cocoa prices at the time of birth substantially increase the incidence of severe mental distress, as classified by the Kessler Scale. A one standard deviation drop in the cocoa price increases the probability of severe mental distress by 3 percentage points, or nearly 50 percent of mean severe distress incidence in the Ghanaian population.

Effects on alternative measures of mental health show remarkable consistency with the Kessler Scale results. We test whether the effect of these income shocks varies by their timing. We find that prices from occurring between the time of birth and four years after birth have significant impacts on adult mental health. These results are in line with other studies on long-run impacts of early life shocks in agricultural settings (e.g., Maccini and Yang (2009)). We do not find robust evidence that declines in the cocoa price adversely affect other measures of adult health and welfare, such as height, BMI, completed schooling, savings, and expenditures. Together, these findings suggest that some early life stressors may have long-run impacts on mental health even if they do not influence other measures of adult welfare.

Our study is closely related to the "fetal origins" literature in economics. Barker's original hypothesis – that access to nutrition in early life has long-run effects on health and

[1] In Ghana, where our study is based, Canavan et al. (2013) estimate that the productivity loss associated with mental illness is equivalent to 7 percent of the country's GDP.

well-being – has been affirmed and extended by a large body of empirical evidence in economics.[2] These studies show that changes in fetal programming can affect a wide variety of outcomes, including physical health (Currie, 2009; Hoynes et al., 2012); educational performance and attainment (Bharadwaj et al., 2013a,b; Bleakley, 2007); and labor market outcomes (Almond, 2006; Bhalotra and Venkataramani, 2012; Bleakley, 2010; Gould et al., 2011).

Though this literature has grown in many directions, to our knowledge no "fetal origins" study in economics has examined long-run impacts on mental health. Apart from being a key determinant of utility and an important endpoint in its own right (Daly et al., 2013; Kahneman and Deaton, 2010), mental health is also a potential mechanism through which some of the previously documented "fetal origins" impacts on human capital and earnings may arise (Kubzansky et al., 1997, 1998; Whang et al., 2009). Moreover, medical evidence suggests that some components of mental health are coded during fetal development (Shonkoff, 2011; Shonkoff et al., 2012). Changes to the fetal environment, if they alter or disrupt this coding process, may have long-lasting impacts on mental health (Huttunen and Niskanen, 1978; Mednick et al., 1988; Neugebauer et al., 1999; **?**).

As a growing segment of the fetal origins literature has already documented, early life trauma can have have outsized impact in low-income populations whose income smoothing and coping mechanisms are often limited. In particular, smallholder farm households in the developing world are exposed to a high degree of income uncertainty (Maccini and Yang, 2009; Townsend, 1994). The households we focus on (cocoa farmers), and millions like them, are commodity suppliers to the global market (Deaton, 1999). The wide and persistent price fluctuations that characterize these markets directly affect the livelihoods of smallholder suppliers, leaving households (and young children in particular) vulnerable to the deleterious effects of shocks (Benjamin and Deaton, 1993; Cogneau and Jedwab, 2012; Kruger, 2007; Miller and Urdinola, 2010). It is crucial, then, to study whether income shocks and their consequences constitute part of the origins of mental distress in low-income contexts, in order to devise policy solutions that address this problem.[3]

The rest of the paper is organized as follows. In section 2, we outline our empirical strategy. Section 3 describes the cocoa price data and our survey data. Section 4 discusses our results, and section 5 concludes.

[2] Barker's original contributions and the subsequent literature in economics are nicely reviewed in Almond and Currie (2011) and Currie and Vogl (2012).

[3] In this respect, our study is related to recent work documenting the short- and medium-run mental health impacts of natural disasters and crises (Frankenberg et al., 2008; Friedman and Thomas, 2009; Paxson et al., 2012; Rhodes et al., 2010).

# 2 Empirical strategy

In this section, we describe the empirical approach that we use to test for effects of cocoa price shocks at birth on adult mental health outcomes.

## 2.1 Intuition

The intuition for our identification strategy is that households in the cocoa-producing regions of Ghana experience changes in the real producer price of cocoa as income shocks, while households in regions that do not produce cocoa are unaffected by these fluctuations. Children born into households in cocoa-growing regions during periods of high cocoa prices will have more resources, owing both to the higher incomes of cocoa-producing households and to the dependence of non-agricultural activities in these regions on the cocoa sector. These resource booms could have large and lasting impacts on mental health through their effects during both gestation and infancy.

## 2.2 Motivation

To motivate this identification strategy, we present a graph in Figure 1 that depicts the strong correlation between cocoa price shocks during individual's year of birth and his mental health in later life. The solid line is the 3-year moving average of the log of the real producer price of cocoa. The dotted line is the 3-year moving average of the difference between the incidence of severe mental distress among individuals born in Ghana's cocoa-producing regions and its incidence among individuals born in the rest of Ghana. A clear negative correlation between the two time series is evident. That is, individuals born in the cocoa-producing regions of Ghana when incomes of cocoa-producers are high show low rates of severe mental distress relative to individuals born in the same year but in parts of Ghana that do not grow cocoa. When incomes in cocoa-producing regions fall, the pattern is reversed.

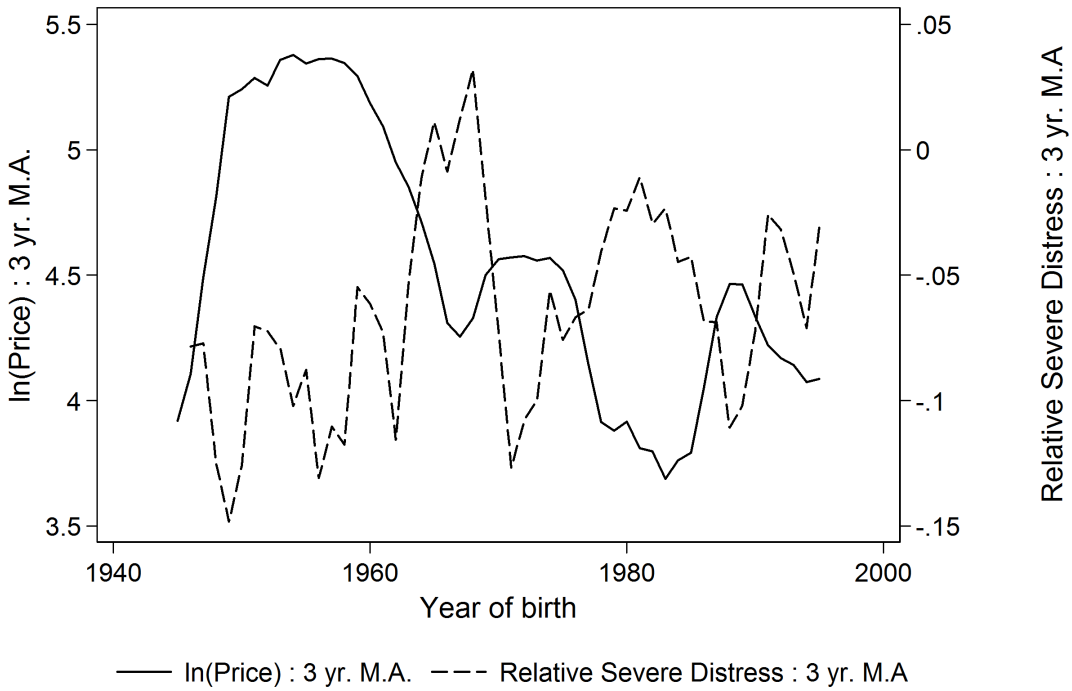
## 2.3 Specification

To test for the effects of cocoa price fluctuations in the year of birth on later-life mental health, we estimate the following equation:

$$Outcome_{irt} = \beta ln(CocoaPrice_t) \times CocoaProducer_r + x'_{irt}\gamma + \delta_r + \eta_t + \epsilon_{irt}. \quad (1)$$

Here, $Outcome_{irt}$ is the outcome for individual $i$, born in region $r$ in year $t$. In the main results reported in Table 2 below, we will use either the natural log of the individual's

Figure 1: Cocoa Prices at Birth and Severe Distress




response on the 10-question Kessler Psychological Distress Scale or a dummy for whether the individual's score was above 30, an indicator for severe distress. $CocoaPrice_t$ is the real producer price of cocoa in year $t$. We describe the source of the price data used and how the real producer price is calculated in section 3. $CocoaProducer_r$ is an indicator for whether cocoa is produced in region $r$. We discuss how this indicator is defined in section 3. $\beta$ is the coefficient of interest. We anticipate that the effect of beneficial shocks to parental income will reduce adult mental illness, leading to negative estimates of $\beta$. Throughout, we will refer to this composite variable $ln(CocoaPrice_t) \times CocoaProducer_r$ as the "Price Shock."

$x_{irt}$ is a vector of controls. In our preferred specification, this will include female, household head, the interaction of female and head, dummies for religion, and dummies for ethnicity. $\delta_r$ and $\eta_t$ are vectors of fixed effects for year and region of birth, respectively. In our baseline, we will cluster standard errors by enumeration area. This is the primary sampling unit of the outcome variables. As robustness checks, we will cluster, alternately, by region of birth or year of birth. In addition to these, we also report Cameron et al. (2011) standard errors clustered both by enumeration area and year of birth, or alternately by region of birth and year of birth.

Our preferred specification includes an additional vector of controls: $\delta_r \times t$. This set

of controls interacts the vector of region of birth fixed effects $\delta_r$ with a continuous year of birth variable $t$ to allow for region-of-birth-specific time trends. In subsequent robustness checks, we add quadratic region-of-birth-specific time trends. In an additional specification, we include rainfall and temperature measures in the region. To the degree that the price shock variable is picking up region-specific fluctuations in temperature or rainfall, effects on mental health outcomes could be due to direct effects of these fluctuations on health of the mother or other members of the household, in addition to household income fluctuations.

# 3 Data

In this section, we describe the data sources used in the analysis. Additionally, where necessary, we describe the construction of the variables of interest.

## 3.1 Cocoa prices and production

Our source of data for real producer prices of cocoa is Teal (2002). He calculates these using the following:

$$\frac{P_X^P}{P^C} = \frac{P_X}{P_M}\frac{P_M ER}{P^C}(1-t).$$

Here, $P_X^P$ is the cedi price received by cocoa producers, which is deflated by $P^C$, the price of domestic goods. This can be re-expressed as a function of $P^X$, the export price in foreign currency, $P^M$, the price of imports in foreign currency, $ER$, the official exchange rate, and the tax rate $t$, which encompasses both export duties and the difference between world cocoa prices and the lower prices often set by the monopolistic cocoa board. This real producer price represents a time-varying income opportunity available to households in the cocoa-growing regions of Ghana, but not available in other regions of the country.

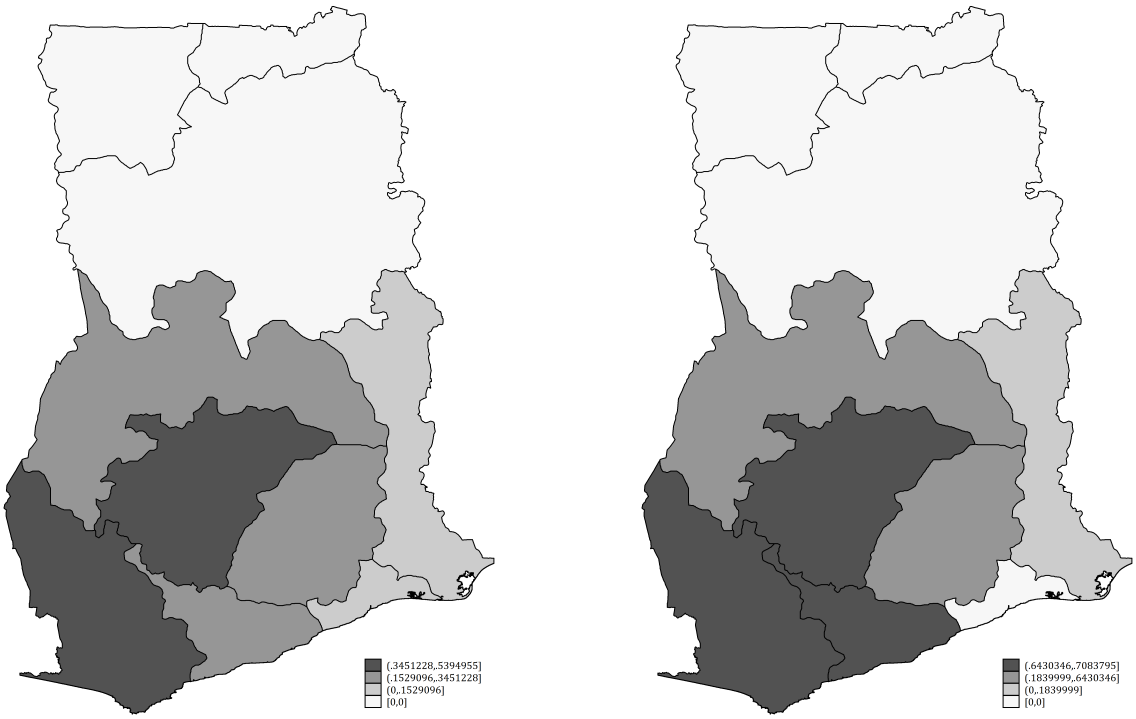
In our baseline specification, we interact these price shocks with an indicator variable for whether cocoa is produced in the respondent's region of birth. The data on cocoa production that we use to produce this baseline measure is computed directly from the EGC-ISSER Socioeconomic Panel Survey. These data were collected by the Economic Growth Center at Yale University and the Institute for Statistical, Social and Economic Research at the University of Ghana, Legon.

The data consist of a single cross-section, collected between November 2009 and April 2010, covering all of Ghana. Individuals were asked to list all plots of land, and what crops were grown on these plots. In Figure 2 below, we present a map of Ghana in which the 10 regions are shaded according to the percentage of farm acreage devoted to cocoa-growing.

As a robustness check, we discard Greater Accra and Volta from the analysis, as less than 20% of farm land in these regions is planted to cocoa.

Our baseline measure is an indicator for the presence of cocoa in a region. This overlaps closely with the area classified as suitable for cocoa production in the 1958 Survey of Ghana Classification Map of Cocoa Soils for Southern Ghana. Produced for the Survey of Ghana, this map classified Ochrosols, Oxysols and Intergrades as suitable for cocoa production, conditional on climatic suitability. We plot the fraction of households in each region that grow cocoa (left panel) and the fraction of land in the region suitable for cocoa production (right panel) in Figure 2.

Figure 2: Cocoa Production and Cocoa Suitable Soils by Region




The figure on the left depicts the fraction of land in the EGC-ISSER survey planted to cocoa in each region. The figure on the right depicts the share of all land in the region that is suitable for cocoa.

## 3.2 Mental health

Our principal measure of mental health is computed using the 10-question Kessler Psychological Distress Scale, or K10. These data were collected as part of the EGC-ISSER Socioeconomic Panel Survey, and are described in greater detail by Canavan et al. (2013).

The K10 was developed by Ron Kessler and Dan Mroczek in 1992 as a measure of anxiety-depression spectrum mental distress (Kessler et al., 2002). The questionnaire consists of 10 questions about negative emotional states experienced during the past 4 weeks. Respondents give 5-point answers ranging from "none of the time" to "all of the time." In particular, respondents are asked:

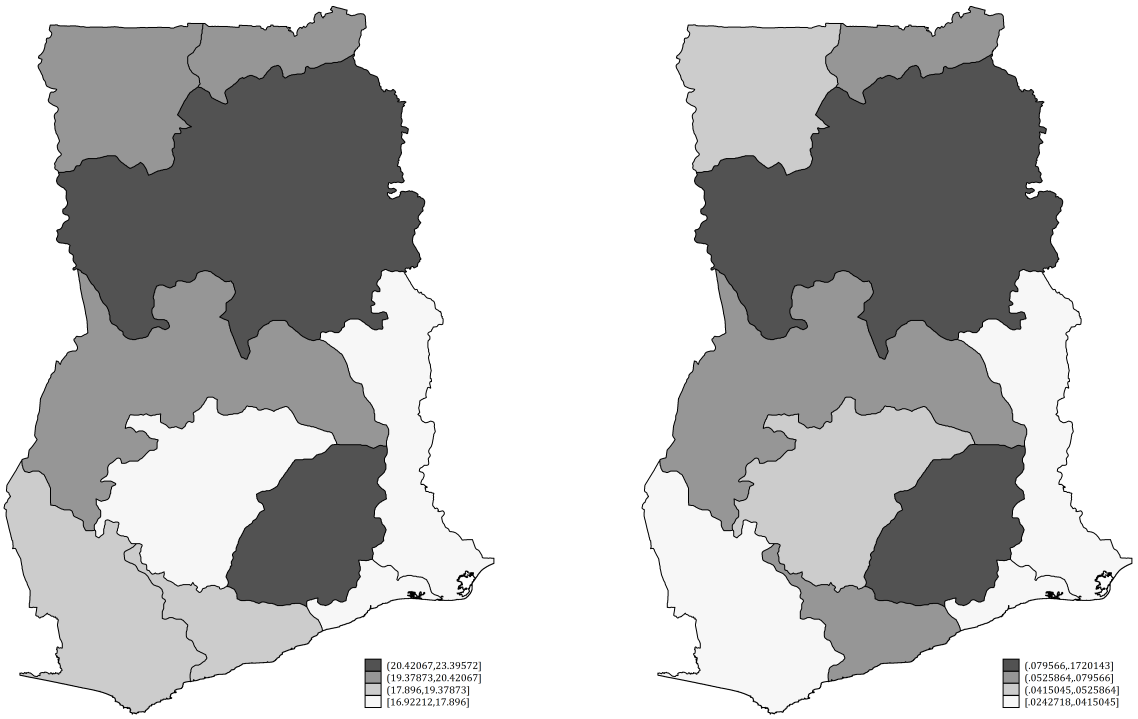
1. About how often did you feel tired out for no good reason?
2. About how often did you feel nervous?
3. About how often did you feel so nervous that nothing could calm you down?
4. About how often did you feel hopeless?
5. About how often did you feel restless or fidgety?
6. About how often did you feel so restless you could not sit still?
7. About how often did you feel depressed?
8. About how often did you feel that everything was an effort?
9. About how often did you feel so sad that nothing could cheer you up?
10. About how often did you feel worthless?

The survey methodology was developed and initially validated in the United States. It has been administered in a variety of contexts around the world, including in low-income populations in Australia and South Africa (Kilkkinen et al., 2007; Myer et al., 2008). Responses to the K10 have been shown to correlate with the Composite International Diagnostics Interview and with the probability of a Diagnostic and Statistical Manual for Mental Disorders (DSM-IV) mental disorder (Kessler et al., 2003). It is conventional to take a K10 score greater than or equal to 30 as an indicator for severe distress. Classifications of distress as measured by the K10 have been shown to be stable over time, suggesting that the scale captures a long run component of mental health (Lovibond, 1998).

In addition to the K10 questionnaire, individuals were asked several additional questions about their mental state. We use these to validate the K10 measures in section 4. These ask respondents to agree or disagree on a five-point scale with statements such as "I am someone who is depressed, blue" or "I am someone who is relaxed, handles stress well." We show that responses to these alternative measures of mental health follow the same response to early-life shocks as the more structured K10.

We retain individuals for our analysis who were born in Ghana, who have nonmissing responses on the region of birth and K10 questions, who are aged between 15 and 65 at the time of the survey, and whose self-reported ages are consistent with their self-reported years of birth within 5 years. This leaves us with a base sample of 7,741 individuals. We show mean means for individual's $K10$ scores and the indicator for severe distress by region of birth in Figure 3. Greater levels of distress correspond to darker shades of grey.

Figure 3: Mean K10 Score and Severe Distress by Region of Birth




The figure on the left depicts the mean K10 score over individuals in the sample. The figure on the right depicts the fraction of respondents whose scores indicate severe distress.

## 3.3 Additional controls

The bulk of our additional control variables are taken from the EGC-ISSER data. These include our principal individual controls – fixed effects for region and year of birth, an indicator for female, an indicator for household head, head, the intersection of female and head, dummies for religion, and dummies for ethnicity.

In addition to these controls, other variables that we interact with early-life shocks are also collected from the EGC-ISSER data. These include indicators for whether an individual's father was in agriculture or whether either of an individual's parents had any education.

We test whether early-life shocks predict additional outcomes also recorded in the EGC-ISSER data, including height in centimeters, body mass index (BMI), an individual's own education, whether an individual has migrated away from his or her region of birth, and the value of a household's savings.

In a robustness check, we control for rainfall and temperature shocks experienced during a respondent's year of birth. We take data on temperature and rainfall from the standard Willmott and Matsuura series available at `climate.geog.udel.edu/~climate/`. We merge this to the regions of Ghana by taking the average over grid points within a region. Regions not containing a grid point in the climate data are merged to the nearest point.

### 3.4 Summary statistics

Summary statistics on our variables of interest and principal controls are presented in Table 1. The statistics show that there is a great deal of variability in mental health. In particular, though only 7.4 percent of the sample appear severely distressed, the standard deviation of this binary variable is quite large at .26. Similarly, there is a great deal of variation in our price shock measure owing both to a highly variable cocoa price and a fact that not all regions produce cocoa.

## 4 Results

In this section, we present and discuss the results of the empirical analysis proposed in section 2.

### 4.1 Main result

We report our main estimates of (1) in Table 2. There is a negative impact of the price shock at birth on the log of the respondent's K10 score in adulthood. This effect becomes statistically significant once time trends are added for each region-of-birth and survives the addition of individual controls. The negative effect of cocoa prices on severe mental distress is robust across specifications.

The magnitudes of the effects on the log of the $K10$ score are moderate, while the impacts on severe distress are large. The real producer price of cocoa has varied widely over time, and a one standard deviation increase in the log price is equivalent to 0.55 log points. In column (3), this would reduce the log K10 score for an individual born in a cocoa-producing region by $-0.045 \times 0.55 = 0.025$. This is roughly 0.08 standard deviations, or 1% of the mean. For

severe distress, a one-standard-deviation price shock leads to a roughly 3 percentage point reduction in severe distress, which is nearly half the mean.

We also report in Table 2 alternative estimates of the standard error of the impact of the price shock. First, we report standard errors clustered by region of birth or by year of birth. Second, we report Cameron et al. (2011) standard errors clustered by both enumeration area and year of birth or by both region of birth and year of birth. Third, because the number of possible regions of birth is small, we report Moulton-corrected standard errors clustered by enumeration area or by region of birth (see Angrist and Pischke (2008)). Our estimates of the standard error do not change noticeably across specifications.

## 4.2 Timing

Although our baseline specification focuses on cocoa prices during an individual's year of birth, this is not necessarily the only age during which we would expect to find impacts on later-life mental health. For example, it is possible that parental incomes throughout childhood exert an influence later in life. If parents find it difficult to recover from adverse income shocks, shocks experienced before a child is born may also affect later-life outcomes.

In Figures 4 and 5, we test whether shocks in years other than an individual's year of birth affect later life mental health. We estimate (1) including both region-specific trends and controls. We replace the year-of-birth shock with price shocks experienced in other years. We report point estimates and 95% confidence intervals as a function of shock timing.

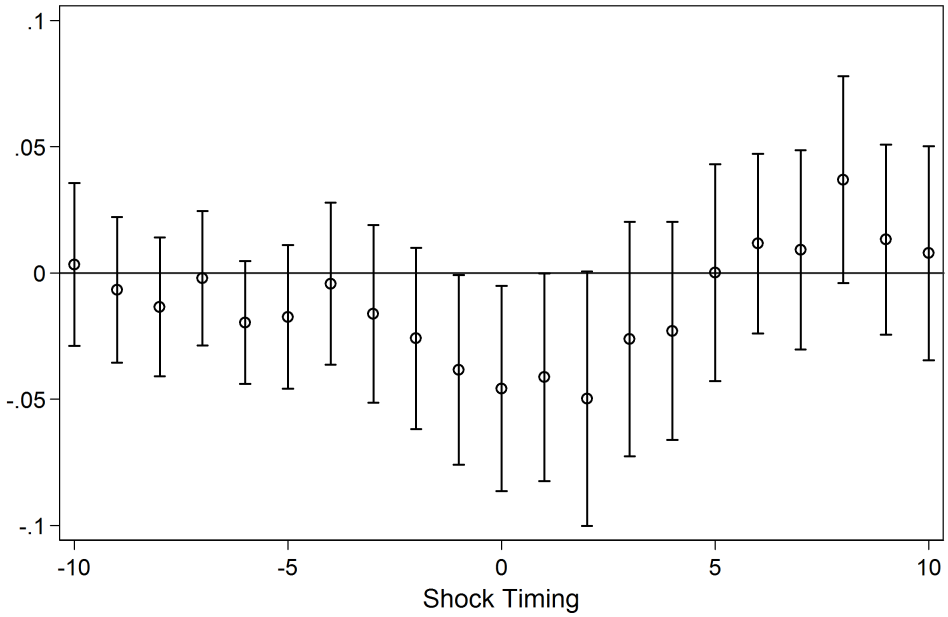
For both measures of mental health, these results suggest that shocks experienced in the first four years of an individual's life affect later-life mental health. The precision of these estimates are greater for severe mental distress than for the log of the individual's K10 score. In both cases, the effect is largest in an individual's year of birth. Although our point estimates suggest beneficial effects exist for higher prices experienced before birth, these are not statistically significant.

## 4.3 Alternative measures of mental health

We show in Table 3 that, in addition to mental health as measured by the K10 questionnaire, the impact of early-life cocoa price shocks is apparent for a variety of similar outcomes. This helps establish the validity of the K10 as a measure of mental illness, and the statistical robustness of our results.

First, individuals who received beneficial cocoa price shocks in their year of birth are less likely to report that they are the sort of person who is depressed, or "blue." Similarly, they are more likely to self-identify as relaxed. They are less likely to state that they tend to start

Figure 4: Effects on ln(K10) by Shock Timing




This figure depicts coefficient estimates and the 95% confidence interval for the main specification with region trends, controls, and standard errors clustered by enumeration area. The timing of the shock is in years relative to the year of birth.

quarrels, are disorganized, or are moody. These are traits we would expect from individuals who are less likely to experience mental distress as a result of favorable early-life events.

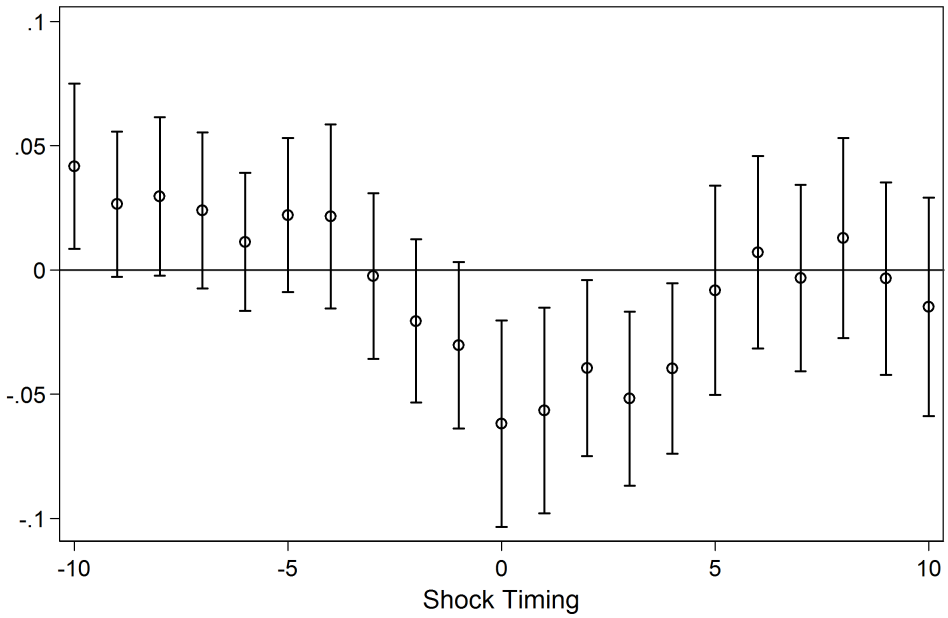
## 4.4 Robustness

We demonstrate the statistical robustness of our main result in Table 4. First, we replace the linear trends by region of birth with quadratic trends, and show that there is little effect on the results. Second, we discard the Volta and Greater Accra regions from our data. Cocoa is grown in both regions, but in small amounts, making it unclear whether these can be cleanly included in the treatment or control groups. This too does little to diminish our main result.

Third, we find that replacing the price shock in an individual's year of birth with the three-year moving average of the price of cocoa neither improves nor worsens the precision of our main result. Fourth, following on Figures 4 and 5, we show that cocoa prices averaged over the year of birth and first two years of an individual's life predict later-life mental health.

Fifth, including other early-life shocks that might be correlated with cocoa prices does not diminish our main results. Here, we control for region-level rainfall and temperature experienced in an individual's year of birth. Rainfall and temperature in the year of birth

Figure 5: Effects on Severe Distress by Shock Timing




This figure depicts coefficient estimates and the 95% confidence interval for the main specification with region trends, controls, and standard errors clustered by enumeration area. The timing of the shock is in years relative to the year of birth.

are themselves plausibly exogenous. While rainfall and temperature might have effects on a child's later-life outcomes through mechanisms other than parental income (e.g. maternal health), the stability in the effect of the price shock after controlling for rainfall and temperature supports the interpretation of the price shock as working through parental income.

Finally, because individuals may only know their ages imprecisely, we discard all individuals whose ages are divisible by 5. This reduces the sample by a third, and leads the estimated impact on the log K10 score to become insignificant. Estimates of the effect of price shocks on severe mental distress, by contrast, remain significant and become larger in magnitude than in the baseline.

We conduct additional robustness checks that are not reported here.[4] Our main results survive when we remove individuals born before 1960 or those who are less than 18 years old at the time of the survey. Using the real producer price without taking its logarithm still yields a significant effect on adult mental health. Defining cocoa-producing regions as those containing land suitable for cocoa changes little. Greater Accra moves to the control group, and the estimated effects are similar to the baseline. Similarly, if we interact the log

[4]These results are available upon request.

real producer price of cocoa with the fraction of land in a region that is planted to cocoa, we find a negative coefficient; cocoa prices matter more where cocoa is more important.

As additional solutions to possible age heaping, we collapse ages into three-year bins and use 2009 minus self-reported year of birth an alternative to self-reported year of birth. Our results survive including fixed effects for each region intersected with birth during the rule of Jerry Rawlings. We find significant effects of early-life cocoa price shocks even if the estimation is performed separately for household heads and non-heads. The effect on severe distress remains significant even when controlling for household fixed effects that compare two members of the same household.

## 4.5 Heterogeneity and intensity

In Table 5, we present tests for heterogeneous responses to cocoa price shocks. First, we show that the response of women to the price shocks is smaller than that for men. There are many possible mechanisms for this difference; women have higher average rates of mental distress in our sample. They may be less susceptible to shocks, or they may be treated differently by parents in response to income shocks.

Although there is some limited evidence that early-life cocoa price shocks matter most for the children of farmers, this interaction effect is small. This is consistent with other evidence that the growth of the non-agricultural sector in southern Ghana has largely been driven by cocoa (Jedwab, 2013). It is not only farmers that suffer from low prices.

While we find no evidence that children of educated fathers suffer less from adverse cocoa price shocks, there does appear to be a substantial mitigating effect of having a mother who has received any education. At least two interpretations are consistent with this result. First, it may be that more educated mothers are better able to shield their children from the effects of adverse income shocks. Second, it may be that educated mothers tend to live in households whose incomes are less dependent on cocoa prices.

Finally, we interact the cocoa price shock with an indicator for "Akan," Ghana's largest ethnic group and the country's dominant cocoa-producing ethnicity. The effect of cocoa price shocks is larger for this group than for others, reflecting the greater economic dependence of the Akan on cocoa.

## 4.6 Other Outcomes

In Table 6, we test whether impacts of cocoa price shocks in early life appear for other observable outcomes. In particular, we are concerned with whether the existence of poor mental health among adults exposed to adverse income shocks in their year of birth is

coincident with poor adult economic and physical health outcomes. We find little evidence of this. Although adult heights appear to improve with beneficial price shocks, this result is not robust across specifications. Neither is the positive effect of beneficial shocks on body-mass index, which appears only before controls or region-specific trends are added. Once trends are included, children receiving beneficial price shocks appear less likely to have ever attended school. This is consistent with greater returns to child labor that result from beneficial price shocks. There appear to be positive impacts of cocoa price shocks on both migration and income, but neither of these are statistically significant.

# 5 Conclusion

In this study, we seek to extend the literature on long-term impacts of early-life factors to include the study of adult mental health outcomes. We show among a nationally representative sample of households in Ghana that a one standard deviation increase in the cocoa price in early life reduces the likelihood of severe mental distress in later life by roughly 3 percentage points for individuals born in cocoa-producing regions relative to the rest of their birth cohort born in other regions. This is nearly half the mean. Expanding our analysis to price shocks in each of the 10 years before and after birth, we find that income shocks during the first 4 years of life matter for adult mental health. Effects are largest for the shocks during an individual's year of birth. The effects are driven by individuals from the Akan ethnic group that has been historically predominant in the production of cocoa, and are stronger for children born into agricultural households.

In addition to being of first-order importance for welfare or utility determination and measurement, mental health is potentially a key determinant of productivity, physical health, and economic decision-making. That is, mental health is important both as an outcome of economic shocks in its own right and as a potential mechanism for some of the large, later-life impacts of early-life factors measured in the literature to date.

Our results suggest two interpretations of the impacts on mental health. These are mutually compatible. First, mental health could be a mechanism that partly explains other economic impacts of early life shocks. If this is the case, the fact that we do not find large estimates of impacts on these economic outcomes suggests that we are considering a relatively mild shock compared to other studies in which early-life circumstance strongly affects economic livelihoods later in life. Had these studies been able to measure adult mental health, this suggests that they would have found larger effects than the ones we estimate here.

Second, mental health is a final outcome that depends partly on other economic and

health variables. In this case, we would also expect the mental health impacts of shocks that were considered by other studies to be greater than the ones we have found. The indirect effects of health and economic welfare on mental illness would be in addition to direct effects we have estimated. Previous measurements of the welfare importance of early-life factors, while already large, are underestimated to the degree that they do not include mental health.

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Table 1. Summary Statistics

| | (1) Mean | (2) s.d. | (3) Min | (4) Max | (5) N |
|---|---|---|---|---|---|
| *Mental Health* | | | | | |
| ln K10 | 2.92 | 0.31 | 2.30 | 3.91 | 7,815 |
| Severe distress | 0.074 | 0.26 | 0 | 1 | 7,815 |
| *Cocoa Price Shocks* | | | | | |
| ln(Cocoa price) X Region any cocoa: Year of birth | 3.30 | 2.01 | 0 | 5.52 | 7,741 |
| *Controls* | | | | | |
| Female | 0.55 | 0.50 | 0 | 1 | 7,815 |
| Year of birth | 1,973 | 13.7 | 1,943 | 1,997 | 7,815 |
| Head | 0.49 | 0.50 | 0 | 1 | 7,815 |
| Female X Head | 0.15 | 0.36 | 0 | 1 | 7,815 |
| *Real Producer Price Series* | | | | | |
| Real Cocoa Price | 105 | 60.1 | 31.1 | 251 | 55 |
| ln(Cocoa Price) | 4.50 | 0.55 | 3.44 | 5.52 | 55 |
| *Fraction of Farm Area Under Cocoa, By Region* | | | | | |
| Ashanti | 44.36% | | | | |
| Brong Ahafo | 31.80% | | | | |
| Central | 34.51% | | | | |
| Eastern | 26.20% | | | | |
| Greater Accra | 0.09% | | | | |
| Northern | 0.00% | | | | |
| Upper East | 0.00% | | | | |
| Upper West | 0.00% | | | | |
| Volta | 4.38% | | | | |
| Western | 53.95% | | | | |

Table 2. Main results

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | *ln(K10)* | | | *Severe Distress* | |
| Price shock | -0.023 | -0.045** | -0.045** | -0.052*** | -0.061*** | -0.062*** |
| | (0.017) | (0.021) | (0.021) | (0.017) | (0.021) | (0.021) |
| *S.E. clustered by* | | | | | | |
| *R.O.B.* | (0.021) | (0.016) | (0.016) | (0.005) | (0.014) | (0.013) |
| *Y.O.B.* | (0.012) | (0.019) | (0.018) | (0.013) | (0.018) | (0.019) |
| *C.G.M.: E.A. & Y.O.B.* | (0.015) | (0.020) | (0.023) | (0.015) | (0.019) | (0.023) |
| *C.G.M.: R.O.B. & Y.O.B.* | (0.021) | (0.018) | (0.020) | (0.006) | (0.032) | (0.020) |
| *Moulton: E.A.* | (0.019) | (0.026) | (0.026) | (0.014) | (0.019) | (0.019) |
| *Moulton: R.O.B.* | (0.015) | (0.020) | (0.020) | (0.013) | (0.018) | (0.018) |
| *Moulton: Y.O.B.* | (0.015) | (0.020) | (0.020) | (0.013) | (0.018) | (0.018) |
| Observations | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 |
| Y.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Trends | No | Yes | Yes | No | Yes | Yes |
| Controls | No | No | Yes | No | No | Yes |

*Notes:* ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

Table 3. Other personality outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *I am someone who is depressed, blue* | | | *I am someone who is relaxed, handles stress well.* | | |
| Price shock | -0.225*** | -0.281*** | -0.283*** | 0.168*** | 0.121 | 0.142* |
| | (0.060) | (0.093) | (0.091) | (0.057) | (0.076) | (0.074) |
| Observations | 6,978 | 6,978 | 6,978 | 7,010 | 7,010 | 7,010 |
| | *I am someone who starts quarrels with others* | | | *I am someone who tends to be disorganized* | | |
| Price shock | -0.084** | -0.087** | -0.100** | -0.234*** | -0.133** | -0.141** |
| | (0.042) | (0.042) | (0.041) | (0.058) | (0.065) | (0.067) |
| Observations | 7,003 | 7,003 | 7,003 | 6,992 | 6,992 | 6,992 |
| | *I am someone who has an assertive personality* | | | *I am someone who can be cold and aloof* | | |
| Price shock | -0.128* | -0.215** | -0.181** | -0.247*** | -0.288*** | -0.292*** |
| | (0.070) | (0.089) | (0.089) | (0.069) | (0.097) | (0.097) |
| Observations | 6,988 | 6,988 | 6,988 | 7,005 | 7,005 | 7,005 |
| | *I am someone who can be moody* | | | | | |
| Price shock | -0.255*** | -0.260*** | -0.275*** | | | |
| | (0.064) | (0.097) | (0.096) | | | |
| Observations | 6,994 | 6,994 | 6,994 | | | |
| Y.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Trends | No | Yes | Yes | No | Yes | Yes |
| Controls | No | No | Yes | No | No | Yes |

*Notes:* ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

Table 4. Robustness

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | *ln(K10)* | | | *Severe Distress* | |
| | | | *Quadratic region trends* | | | |
| Price shock | -0.023 | -0.064*** | -0.054*** | -0.052*** | -0.064*** | -0.054*** |
| | (0.017) | (0.022) | (0.021) | (0.017) | (0.022) | (0.021) |
| Observations | 7,746 | 7,746 | 7,746 | 7,746 | 7,746 | 7,746 |
| | | | *Drop regions with 0-25% of farmland under cocoa* | | | |
| Price shock | -0.029 | -0.043* | -0.043* | -0.053*** | -0.065*** | -0.064*** |
| | (0.018) | (0.022) | (0.022) | (0.018) | (0.022) | (0.022) |
| Observations | 6,243 | 6,243 | 6,243 | 6,243 | 6,243 | 6,243 |
| | | | *Price measured as 3 year moving average* | | | |
| Price shock | -0.021 | -0.041* | -0.042* | -0.043** | -0.042** | -0.042** |
| | (0.017) | (0.021) | (0.021) | (0.017) | (0.019) | (0.020) |
| Observations | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 |
| | | | *Price averaged over ages 0-2* | | | |
| Price shock | -0.026 | -0.057** | -0.056** | -0.053*** | -0.067*** | -0.066*** |
| | (0.018) | (0.025) | (0.025) | (0.018) | (0.023) | (0.024) |
| Observations | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 |
| | | | *Control for rainfall and temperature shocks* | | | |
| Price shock | -0.018 | -0.049** | -0.049** | -0.053*** | -0.061*** | -0.062*** |
| | (0.017) | (0.021) | (0.021) | (0.017) | (0.021) | (0.021) |
| Observations | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 |
| | | | *Discard possible age heaping* | | | |
| Price shock | -0.008 | -0.034 | -0.036 | -0.059** | -0.096*** | -0.096*** |
| | (0.018) | (0.024) | (0.024) | (0.024) | (0.024) | (0.025) |
| Observations | 5,375 | 5,375 | 5,375 | 5,375 | 5,375 | 5,375 |
| Y.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Trends | No | Yes | Yes | No | Yes | Yes |
| Controls | No | No | Yes | No | No | Yes |

*Notes:* ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

Table 5. Compliers

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | *ln(K10)* | | | *Severe Distress* | |
| | | | *Interact with "Female"* | | | |
| Price shock | -0.028* | -0.050** | -0.046** | -0.055*** | -0.064*** | -0.063*** |
| | (0.017) | (0.021) | (0.021) | (0.017) | (0.021) | (0.021) |
| Shock X Interaction | 0.002 | 0.002 | 0.000 | 0.007*** | 0.007*** | 0.005* |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 | 7,741 |
| | | | *Interact with "Father in Agriculture"* | | | |
| Price shock | -0.024 | -0.041* | -0.042* | -0.057*** | -0.051** | -0.052** |
| | (0.017) | (0.022) | (0.022) | (0.019) | (0.023) | (0.023) |
| Shock X Interaction | -0.011 | -0.010 | -0.010 | -0.008* | -0.009* | -0.009* |
| | (0.007) | (0.007) | (0.007) | (0.005) | (0.005) | (0.005) |
| Observations | 7,047 | 7,047 | 7,047 | 7,047 | 7,047 | 7,047 |
| | | | *Interact with "Mother any Education"* | | | |
| Price shock | -0.035* | -0.056** | -0.057** | -0.057*** | -0.048* | -0.050** |
| | (0.018) | (0.023) | (0.023) | (0.019) | (0.024) | (0.024) |
| Shock X Interaction | 0.023* | 0.022* | 0.022* | 0.014*** | 0.015*** | 0.015*** |
| | (0.013) | (0.013) | (0.013) | (0.004) | (0.004) | (0.005) |
| Observations | 6,466 | 6,466 | 6,466 | 6,466 | 6,466 | 6,466 |
| | | | *Interact with "Father any Education"* | | | |
| Price shock | -0.041** | -0.052** | -0.053** | -0.066*** | -0.057** | -0.059** |
| | (0.017) | (0.022) | (0.022) | (0.018) | (0.022) | (0.022) |
| Shock X Interaction | 0.013 | 0.012 | 0.012 | 0.003 | 0.003 | 0.003 |
| | (0.008) | (0.008) | (0.008) | (0.007) | (0.007) | (0.007) |
| Observations | 6,673 | 6,673 | 6,673 | 6,673 | 6,673 | 6,673 |
| | | | *Interact with "Akan"* | | | |
| Price shock | -0.007 | -0.032 | -0.034 | -0.040** | -0.049** | -0.048** |
| | (0.017) | (0.021) | (0.021) | (0.018) | (0.022) | (0.022) |
| Shock X Interaction | -0.022* | -0.019 | -0.020 | -0.019** | -0.020** | -0.021** |
| | (0.013) | (0.013) | (0.013) | (0.008) | (0.008) | (0.008) |
| Observations | 7,719 | 7,719 | 7,719 | 7,719 | 7,719 | 7,719 |
| Y.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Trends | No | Yes | Yes | No | Yes | Yes |
| Controls | No | No | Yes | No | No | Yes |

*Notes:* ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS and include the uninteracted "Interaction" variable. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

Table 6. Other outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *Height in cm* | | | *BMI* | | |
| Price shock | -0.785 | 1.093 | 2.480** | 1.336*** | 0.029 | -0.237 |
| | (0.823) | (1.312) | (1.099) | (0.488) | (0.476) | (0.522) |
| Observations | 7,374 | 7,374 | 7,374 | 7,374 | 7,374 | 7,374 |
| | *Ever attended school* | | | *Migrant* | | |
| Price shock | 0.112** | -0.148*** | -0.122*** | 0.051 | 0.106* | 0.093 |
| | (0.044) | (0.050) | (0.046) | (0.055) | (0.063) | (0.062) |
| Observations | 7,686 | 7,686 | 7,686 | 7,741 | 7,741 | 7,741 |
| | *Value of savings* | | | | | |
| Price shock | 88.227 | 194.417 | 238.514 | | | |
| | (59.069) | (211.527) | (218.554) | | | |
| Observations | 7,741 | 7,741 | 7,741 | | | |
| Y.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| R.O.B. Trends | No | Yes | Yes | No | Yes | Yes |
| Controls | No | No | Yes | No | No | Yes |

*Notes:* ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

# Developing Aspirations: The Impact of Child Sponsorship on Self-Esteem and Life Expectations

*Key Words: Economic Development, Child Sponsorship, Aspirations, Self-Esteem*
*JEL Codes: O15, O22, D03*

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October 4, 2013

Abstract: Recent research (Wydick, Glewwe, and Rutledge, 2013) finds positive and statistically significant impacts on adult life outcomes from child sponsorship, including large impacts on schooling outcomes, the probability and quality of employment, occupational choice, and community leadership. This paper uses data from four countries to explore whether these impacts may be due not only to a relaxation of external constraints, but also to higher aspirations among sponsored children. We use survey data from Bolivia, Kenya, India, and Indonesia, and psychological data from Indonesian children's self-portraits, to test whether sponsorship significantly affects psychological variables in children that are likely to foster better economic outcomes in the future. In preliminary studies in Bolivia, Kenya, and India, we found child sponsorship to be correlated with increases in educational and vocational aspirations and with self-esteem. Our main study in Kenya (which samples a separate population) and our study in Indonesia exploit an eligibility rule setting a maximum age for newly sponsored children. We use a child's age at program rollout in his or her village as an instrument for sponsorship to establish a causal link between sponsorship and higher levels of self-esteem, as well as educational and occupational aspirations. For those two countries we find a causal link between child sponsorship and large increases in educational and vocational aspirations among children in Kenya, and higher levels of happiness, self-efficacy, and hopefulness based on children's self-portrait data in Indonesia.

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This paper combines and replaces three previous University of San Francisco working papers: Ross, Phillip H. (2010) "An Investigation of Reference Point Shifts in a Child Sponsorship Program;" Ross, Phillip H. and Bruce Wydick (2011) "The Impact of Child Sponsorship on Self-Esteem, Life-Expectations, and Reference Points: Evidence from Kenya;" Glewwe, Paul and Bruce Wydick (2012) "Child Sponsorship and Child Psychology: A Note on Evidence from Children's Drawings in Indonesia." We would like to thank Wess Stafford, Scott Todd, Kate Heryford, Laine Rutledge, Herman Ramirez, Eliana Zeballos, Jennifer Meredith, Alistair Sim, Marcela Bakir, Boris Zegarra, Catherine Mbotela, Peter Ndungu, and other local Compassion staff and enumerators in Bolivia, Kenya, India, and Indonesia for logistical help and support in carrying out our field research. We also appreciate support and helpful comments from Alessandra Cassar and Pauline Grosjean. We are grateful to the University of San Francisco's graduate program in International and Development Economics for substantial funding and resources for this research.

# 1. Introduction

Child sponsorship programs transfer resources from sponsors in wealthy countries to children in developing countries, helping provide them access to healthcare, nutritious meals, tuition, and school uniforms. Wydick, Glewwe, and Rutledge (2013) find large and statistically significant impacts from Compassion International's child sponsorship program on adult life outcomes. These findings include an increase in schooling completion of 1.03-1.46 years, a 12-18 percentage point increase in secondary school completion over a baseline rate of 44.5 percent, and an increase in the probability of white collar employment of 6.6 percentage points over a baseline rate of 18.7 percent.

The emphasis of many child sponsorship programs such as that operated by Compassion, however, is not merely on the relief of external constraints such as access to healthcare and schooling, but on the relief of *internal* constraints. These internal constraints of the poor, which may be strongly manifested in children, may involve feelings of hopelessness, lack of empowerment, low aspirations, a diminished sense of self-efficacy, and low self-esteem. Above nearly all else, these programs claim to bring "hope" to children, and Compassion places a particular emphasis on the development of children's aspirations

In this paper we investigate the impacts of the Compassion International child sponsorship program on the self-esteem, life-expectations and other psychological characteristics of 1,382 children in Bolivia, India, Indonesia and Kenya. The question we address is whether the large impacts on adult life outcomes found in our study of formerly sponsored children could have been caused through psychological changes fostered by the program during the period when the children were sponsored. It is possible that the relief of external constraints from child sponsorship is solely responsible for these improved adult outcomes, and that changes in children's psychological traits due to the program are ancillary to the process. Indeed if we were to find no impact on children's psychology from child sponsorship, we could rule out impacts of the program on child psychological traits as a causal channel for the positive impacts from child sponsorship on adult life outcomes found in Wydick, Glewwe and Rutledge (2013). Thus we view a finding of significant psychological impacts on children as a critically important and necessary (but not sufficient) condition for a causal impact of heightened aspirations on adult outcomes.

A growing literature in behavioral economics explores the relationship between self-esteem and economic outcomes. Bénabou and Tirole (2003), for example, show that empowering and encouraging an individual can raise self-esteem, which may in turn raise achievement. Darolia

and Wydick (2011) find that actions such as parental praise designed to foster an increase in self-esteem result in academic achievement in university undergraduates above what natural ability would dictate.

Another important strand of the literature has sought to understand the role of internal constraints among the poor, especially in the areas of self-esteem and aspirations, and its effect on economic development. Much recent theoretical work in development economics has shown how low aspirations can lead to development traps (Dalton, Ghosal, and Mani, 2013; Bernard, Dercon, and Taffesse, 2011; Genicot and Ray, 2012). Ray (2006), for example, discusses how failed aspirations and poverty are reciprocally linked in a self-sustaining trap. Genicot and Ray (2012) demonstrate how aspirations failures can lead to a divergence in investment and thus growing income inequality.

Recent experimental fieldwork has also explored the importance of psychological variables to development. Kremer, Miguel, and Thornton (2009) analyze the impacts of a merit-based scholarship program in Kenya on the motivation of the students receiving the scholarship as well as spillovers onto teachers and other students; particularly relevant for this paper is that they test the effect of extrinsic rewards on intrinsic motivation. Using a randomized field experiment in South Africa, Bertrand et. al (2010) test psychological factors in credit and saving decisions while Duflo, Kremer, and Robinson (2011) explore nudges and fertilizer take-up among Kenyan farmers using models of procrastination from the psychology and economics literature. Chiapa, Garrido, and Prina (2012) use a difference-in-differences approach to evaluate the impacts of Mexico's PROGRESA program on parent's educational aspirations for their children. They find that the aspirations of parents for their children's education increased by almost half of a school year among high-exposure households, and that there is a positive correlation between parental aspirations and their children's educational attainment. However, they do not investigate the aspirations of the children themselves.

We seek to contribute to this emerging literature on the importance of aspirations to economic development. When positive impacts of child sponsorship became apparent during the course of our study on adult life outcomes (Wydick, Glewwe and Rutledge, 2013), we began exploring the role of aspirations development in sponsored children. We initiated small preliminary studies in several villages in India and Kenya, where our subjects for studies were not adults who had been sponsored when they were children, but instead were currently sponsored children. We then carried out a larger preliminary study in five villages in Bolivia. Although the longstanding existence of the program in these preliminary studies necessary for causal

identification on adult outcomes prevented the use of an age-eligibility instrument for studying causal impacts on children (since the program was rolled out in their villages before they reached the age cutoff point), the correlation between child sponsorship status and higher educational and vocational aspirations was sufficiently strong that they warranted further investigation. This led us to implement larger studies in Kenya and Indonesia where we were able to choose village sponsorship projects that had been rolled out sufficiently recently to allow us to estimate causal impacts via the age-eligibility-rule instrument used in Wydick, Glewwe and Rutledge (2013).

While in all four countries our surveys used direct questions related to measurement of self-esteem and aspirations, our study in Indonesia adds a new element taken from the psychology literature that we feel is especially relevant for children: the psychoanalysis of children's drawings (Koppitz, 1968; Klepsch and Logie, 1982; Furth, 2002). In this exercise, we asked 540 children living in the slums of Jakarta to "Draw a picture of yourself in the rain." Based on research in the child psychology literature, we coded attributes of these drawings that consistently display empirical correlations with diagnosed psychological phenomena in children. Using our vector of age-eligibility instruments to identify causal effects, we find that sponsored children's drawings reveal significantly greater levels of self-esteem and emotional health across a large number of drawing attributes. Combined with our direct survey data, which also find significant differences in educational and vocational aspirations, we find that child sponsorship strongly and positively impacts a wide array of psychological measures in children.

Our analysis consists of five parts: (1) the results from our preliminary studies in India, Kenya, and Bolivia that compare sponsored children to their non-sponsored siblings and peers, (2) the survey results from Kenya utilizing the age-eligibility instrument to compare sponsored children to their siblings, (3) survey results from Indonesia that utilize the same instrument to compare sponsored children to their siblings, and to compare differences between sponsored children and their siblings to differences between children that were on the sponsorship wait list and those children's siblings, (4) survey results based on pooled data from all four countries, and (5) the psychological analysis of drawings in Indonesia.

## 2. Description of survey and fieldwork

### 2.1 Description of the Compassion Program

Currently, Compassion supports over 1.3 million children in 26 countries, making it the third largest child sponsorship organization worldwide. In Wydick, Glewwe, and Rutledge (2013), we estimate that 9.14 million children are sponsored through various organizations

worldwide, and that this represents a transfer of approximately $3.4 billion dollars annually. These programs have been in existence for decades and typically involve a monthly payment of around $25-$40 that funds the provision of healthcare, education, clothing, food, and other needs for the sponsored child and/or the community in which he or she lives. Additionally, they foster a relationship between the child and the sponsor through the exchange of letters, photos, and gifts. (For a more detailed description of the Compassion program, see Wydick, Glewwe, and Rutledge, 2013).

One minor difference between the Compassion projects in this study and in our previous study, which involved adults who were sponsored in the 1990s or earlier, is that in most countries, the age-eligibility rule has been gradually lowered from 12 to 9 years of age. In this study we focus on the aspects of the program that seek to develop children's self-esteem and aspirations. These aspects, which make child sponsorship different from programs that provide only educational inputs, include the exchange of letters with sponsors, which exposes the children to a world outside of their village. It also includes the support network fostered by the Compassion program and its alumni who, directly or indirectly, influence the currently sponsored children through their own accomplishments. Compassion places a significant emphasis on self-esteem building, character development, and enhancement of self-expectations.

## 2.2 Survey Fieldwork

Our preliminary studies carried out in villages in India, Kenya, and Bolivia compare psychological variables such as the self-esteem and life aspirations of sponsored and unsponsored children in the same community, but we are unable to estimate a causal impact of the program on those variables. The results led us to implement more substantial studies in Kenya and Indonesia that were designed to exploit the eligibility rule to identify a causal relationship between sponsorship and psychological measures in children. We include these preliminary studies because ex-post we find that, although we use OLS estimates because we are not able to instrument for program participation, we find that the OLS and instrumental variable estimates to be quite similar in the larger Kenya and Indonesia studies and also quite similar to the OLS estimates from the non-instrumented preliminary studies. But since we lack instruments to identify causal impacts with certainty in these three preliminary studies, we group these villages together in our analysis.

Table 1 provides information on how the study was implemented in 14 villages across the four study countries. In each of the study sites, a survey questionnaire was used to obtain basic

information about the respondent such as age, gender, level of formal schooling, religion, sponsorship information and family characteristics such as language spoken at home and the highest level of education and occupation of each sibling and parent. In addition to this basic information, the survey questionnaires also included a series of questions designed to elicit the child's expectations for occupation and level of education and a battery of questions intended to measure self-esteem. Summary statistics for the data collected are shown in Table 2.

### *2.2.1 The Three Preliminary Studies*

Preliminary studies were carried out in seven villages in three countries from March through July of 2010. Data were collected from one village each in India and Kenya and five villages in Bolivia where the Compassion program was present. These preliminary studies were done in parallel with the Wydick, Glewwe, and Rutledge (2013) study, and so were carried out in some of the same villages. The villages for that study were chosen because their programs had been implemented between 1980 and 1992, which allowed the use of an age-eligibility rule to generate instrumental variables to estimate the impact of the program on formerly sponsored children who were now adults. To use the age eligibility rule to estimate the causal impact of the program on currently sponsored children would have required finding villages in which the program had been implemented at a much later date.

The sampled children in the three preliminary studies can be divided into three groups: currently sponsored children, non-sponsored siblings of sponsored children, and peers with similar socioeconomic backgrounds (usually a classmate) whose families had neither currently or formerly sponsored members. In Kenya the rule was one child per family, and in India two children per family. In Bolivia, the number of sponsored children was limited to less than half of the total number of siblings in a family. The survey was administered to 29 children in India, 90 in Kenya, and 151 in Bolivia, for a total sample of 270. In all, 126 (46.7%) were currently sponsored, 70 (25.9%) were siblings of sponsored children and 74 (27.4%) were peers from non-sponsored families. The respondents were recruited primarily through local schools that had both Compassion and non-Compassion students. An interviewer administered the survey to each child individually, and this was done without Compassion staff present. However, enumerators fluent in the local language were present to answer any questions the interviewers or respondents had about the survey.

### *2.2.2 The Second Kenya Study*

The second study in Kenya was carried out in three villages from May through July of 2011. These villages were randomly sampled from a list of all villages within a three-hour journey

by car from Nairobi with a Compassion program that was first implemented between 2002 and 2004. This time frame was chosen to exploit the age eligibility criteria of the program that newly sponsored children must be between the ages of three and nine years old. This allows us to use age at the time of program roll-out as an instrument for sponsorship. The survey questionnaire was written in English, but the questions were translated into Swahili or the local mother tongue at the discretion of the enumerators as to what they believed would be the most effective way to communicate with each child.

The survey sample consisted of three groups: currently sponsored children, the next oldest non-sponsored sibling and the next youngest non-sponsored sibling. Unlike the three preliminary studies, no children from non-sponsored families were surveyed. Within each village, 110 of the population of currently sponsored children between the ages of 12 and 16 were randomly sampled, for a total of 330 currently sponsored children. Of these, we successfully surveyed 326 (98.8%). Once locating the sponsored child, we would then interview the next oldest and next youngest child. There were 243 of these non-sponsored siblings of the 326 selected children between the ages of 10 and 18, of which we interviewed 237 (97.5%). Of the six who were not interviewed, two were mentally disabled, two were older siblings who had left the village because they had married, and two had left the village to find work. For these last four either we did not get permission from a parent to contact them or we could not locate them without a great amount of difficulty.

For 11 of the 326 (3.4%) currently sponsored children, the next youngest or next oldest sibling was also sponsored, even though the rule in Kenya was to allow only one sponsored child per family, and was not selected in our random sample of currently sponsored children to be surveyed. Four of these 11 sponsored siblings were sponsored due to the twin rule, which stipulates that if one twin is sponsored, the other must also be sponsored, and three were due to cases of extreme poverty in the family, in which case more than one child is allowed to be sponsored. The remaining four cases may have been due to some level of favoritism in one of the villages, as the local pastor had all of his age-eligible children sponsored. In these cases, we would interview this extra sponsored sibling provided they were between 10 and 18. If this extra sponsored sibling was older, we would then interview the next oldest after this extra sponsored sibling if they were 18 or younger. If the extra sponsored sibling was younger, then we would interview the next youngest after the extra sponsored sibling if they were 10 or older. In these 11 instances, the sponsored siblings were always contiguous in birth order, and there was never a third sponsored sibling contiguous in birth order that was between the ages of 10 and 18. Thus, in these instances, we have up to four children interviewed in a family, two sponsored and two non-sponsored.

In total, the survey was administered to 570 children: 333 that were sponsored, 154 next older non-sponsored siblings and 83 next younger non-sponsored siblings, all with the same mother and father within a household. The survey was administered to the children individually by enumerators who were university students or recent graduates; these enumerators were not affiliated with the Compassion program.[1] It was made clear to the child that the studies were confidential, independent of Compassion, and no one from Compassion or anyone else would know any of their responses. Most interviews took place in the children's schools and homes, away from any potential influences such as teachers, parents, and Compassion staff. For example, if interviewed at a school the enumerators would interview the children either in an empty room or somewhere outside that was far from being within earshot of any teachers or other school officials. If interviewed in their homes, parents and other siblings would be asked to wait either inside or outside (wherever the interview was not taking place) or the child was taken to the opposite side of the house. Surveys were never administered in the local church or Compassion center.

While most of the children were interviewed in the village they grew up in, some of those in secondary school were attending boarding school in another part of Kenya, which required up to a day of travel for an enumerator to reach. Additionally, a few older siblings that had left home to find work were located and interviewed in Nairobi or Nakuru.[2]

*2.2.3 Indonesia*

Researchers carried out Indonesia fieldwork in four Compassion project sites in the capital of Jakarta from May to July of 2012. The sites were selected for fieldwork based on the year of program implementation in order to gain maximum advantage of our age-eligibility-rule instrument. Two of these projects started in February 2003 and two in February 2007.

In Indonesia we were able to use children on the waitlist for sponsorship and their own siblings as quasi-controls in the sample. Each of the sites provided a list of sponsored children and waitlisted children from which subjects were randomly chosen for the study. Each randomly chosen child from these lists was instructed to bring one sibling with them to the research site.[3]

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[1] Since Compassion's implementing church partner often had a large role in the communities of these villages, and we hired enumerators that knew the members of the village well, a couple of the hired enumerators may have had some informal volunteer role in the church, but no affiliation with the Compassion program. Sponsored children would commonly participate in church activities outside of Compassion's program hours, and it is possible that one or two of the enumerators were involved in these activities and thus would have had some kind of relationship with some of the sponsored and non-sponsored children through the church and the community but outside of Compassion.

[2] Nakuru is Kenya's fourth largest city and the closest major city to Njoro, one of our selected villages.

[3] The sibling could be either sponsored or unsponsored, but had to be within the relevant age range so that 83.4% of children brought a proximate sibling in birthorder. Because of eligibility rules, in 57.7% of cases the sibling was not a sponsored child or on the waitlist, but the remaining cases of sponsored children, both children were sponsored.

In Indonesia, which had an upper limit of two sponsored children per family, data were gathered from 287 sponsored children, 112 siblings of sponsored children, 80 waitlisted children (of whom one is, and 79 are not, a sibling of a sponsored child), and 61 children who were siblings of waitlisted children.

The selected children and their siblings were asked to come at a specific day and time to the particular site. Each pair of children was then greeted by a graduate student researcher and the enumerator, who randomly selected one of the pair and asked that child to "Draw a picture of yourself in the rain." They were provided with a desk, a sheet of white paper, and a full set of 24 colored pencils, and were told that they have fifteen minutes to complete the drawing. Meanwhile the other child was administered a survey that included a group of questions about the subject's characteristics and living conditions, as well as questions about self-esteem, hopefulness about the future, social trust, spiritual depth, and reference points with regard to expected education and occupation, followed by a time preference game. Then the two children switched activities.

# 3. Empirical strategy

## 3.1 Establishing Causality

In order to estimate the impact of sponsorship on the variables of interest, we begin by using ordinary least-squares (OLS) with village fixed effects. This specification is used to avoid bias due to unobservable differences across villages, each of which consisted of different ethnic groups and different Christian denominations as implementing church partners. Therefore our initial specification identifies program impacts by comparing only differences within villages. More specifically, we estimate one of the following two equations:

$$y_{ij} = \alpha_j + \gamma T_{ij} + \boldsymbol{\beta}'\boldsymbol{X_{ij}} + e_{ij} \qquad (1)$$

$$y_{ij} = \alpha_j + \gamma T_{ij} + \boldsymbol{\beta}'\boldsymbol{X_{ij}} + \pi C_{ij} + e_{ij} \qquad (1')$$

where $T_{ij}$ is a dummy variable for current sponsorship of individual $i$ in village $j$, $\alpha_j$ is a village fixed effect, $\boldsymbol{X_{ij}}$ is a vector of control variables that includes age, age$^2$, gender, birth order, parent's education, and family size, and $C_{ij}$ is a dummy variable indicating a household with a sponsored child, which applies only to Indonesia, the only country where both sponsored and non-sponsored (i.e. waitlisted) households were surveyed. Equation (1) can be estimated using data from all five studies, while equation (1′) can be estimated using data for all studies *except* the data from the second Kenya study, which does not include children from non-sponsored households (which implies that $C_{ij}$ equals one for all observations).

As explained above, only the main (second) Kenya study and the Indonesian study can be used to estimate the causal impacts of child sponsorship. However, even after controlling for unobserved differences across villages, there remain two potential sources of bias when estimating the causal impact of the Compassion program: endogeneity in the selection of households into the program, and endogeneity in the selection of children within a particular household. We account for the former by including in our sample only families that were selected into the program in Kenya and Indonesia or were waitlisted in Indonesia in our analysis. That is, we estimate the average treatment effect of the sponsorship program on the treated (ATT), as opposed to the average treatment effect (ATE) on the general population.

We account for the latter source of bias by using instrumental variables that predict which siblings within program households are selected by their parents to participate in the program. More specifically, and consistent with Wydick, Glewwe, and Rutledge (2013), we find that a child's age at the time of program roll-out is strongly correlated with sponsorship, making it a natural instrument for sponsorship. Indeed the oldest eligible child is typically most likely to be sponsored upon introduction of the program into a village, with younger siblings of this child less likely, and older siblings having virtually no probability of sponsorship. As in Wydick, Glewwe, and Rutledge (2013), the instrumental variables are a vector of dummy variables for age at program rollout.

For these instrumental variable estimations, the first stage equations are

$$T_{ij} = \alpha_j + \boldsymbol{\varphi}'\boldsymbol{X_{ij}} + \boldsymbol{\lambda Z_{ij}} + u_{ij} \qquad (2)$$

$$T_{ij} = \alpha_j + \boldsymbol{\varphi}'\boldsymbol{X_{ij}} + \boldsymbol{\lambda Z_{ij}} + \delta C_{ij} + u_{ij} \qquad (2')$$

where $\alpha_j$, $T_{ij}$, $\boldsymbol{X}_{ij}$ and $C_{ij}$ are the same as in equations (1) and (1'), and $\boldsymbol{Z_{ij}}$ is a vector of dummy variables that indicate age (in years) when the program rolled out in village $j$, with all of those ten years and older grouped into one category. Equation (2) can be estimated using data from the second Kenya study and Indonesia (if non-sponsored households are excluded), while equation (2′) can be estimated using only data from the study in Indonesia since Kenya did not include children from non-sponsored households (which implies that $C_{ij}$ equals one for all observations).

Figure 1 shows the probability that a child in Kenya (main study only) and Indonesia, the two studies for which instrumental variables are available, was sponsored as a function of his or her age at the time the program was introduced in his or her village. It is clear that children from about age 3 to age 9 when the program was introduced in the area were far more likely to be

sponsored than other siblings. Regression estimates of equations (2) and (2') yield the probability of being selected for sponsorship within each household.

The second-stage equations are

$$y_{ij} = \alpha_j + \gamma \hat{T}_{ij} + \boldsymbol{\beta}' \boldsymbol{X_{ij}} + e_{ij} \quad (3)$$

$$y_{ij} = \alpha_j + \gamma \hat{T}_{ij} + \boldsymbol{\beta}' \boldsymbol{X_{ij}} + \pi C_{ij} + e_{ij} \quad (3')$$

where $y_{ij}$ is an outcome variable of interest, $\hat{T}_{ij}$ is the instrumented probability of being a sponsored child, and $\alpha_j$, $\boldsymbol{X}_{ij}$ and $C_{ij}$ are the same as in equations (1) and (1') (and (2) and (2')). Assuming age at program rollout is orthogonal to $y_{ij}$, controlling for age, sibling order, gender, and other characteristics, IV estimations remove bias due to intra-household selection among age-eligible children. We use standard errors clustered at the village level for OLS estimates; as a robustness check we also calculate standard errors using the wild bootstrap method (see Cameron et al. (2008) due to the relatively small (ranging from 3 to 14) number of village clusters in our OLS estimates. For IV estimates we cluster standard errors at the household level, since the number of instruments is larger than the number of clusters.

## 3.2 Summary Indexes

Our survey questionnaire provides multiple measures of the sampled children's psychological well-being. One potential problem with using each of these measures in separate regressions is that, even if the impact of sponsorship on all of these outcomes of interest were equal to zero, one is still likely to find a "significant" impact if one runs regressions for a large number of outcome variables. We address this problem of multiple inference by utilizing the summary indices proposed by Anderson (2008). Summary index tests are robust to over-testing and provide a statistical test for whether a program has a "general effect." They also have higher statistical power than tests of individual variables. Outcomes within an *a priori* grouping are demeaned and normalized, and then each element is weighted using the elements of the variable's corresponding row from the inverse of the covariance matrix that includes all variables within the relevant family (Anderson 2008).[4] Weighting each variable by the sum of its corresponding row (or column) entries of the inverse covariance matrix allows variables that contain more unique information to enjoy a higher weight in the summary index.

We construct three summary indices from the sampled children's responses to psychosocial questions: self-esteem, optimism, and aspirations. The first uses the standard questions from the

[4] Note that this is an efficient generalized least squares estimator (Anderson 2008).

Rosenberg (1965) Self-Esteem scale, the second uses questions from the General Social survey, and the aspirations index is generated based on responses to questions on hopes for adult occupation, expectations for adult occupation, and expected educational attainment.

### 3.3 Factor Analysis

To analyze the drawings done by the children in Indonesia, we use factor analysis as a data reduction tool in order to derive latent psychological factors from observable features of those drawings (those features are summarized in Table 13). Factor analysis is commonly used as a psychometric tool to create latent factors that summarize the common variation in observed sets of variables and is increasingly used by economists to avoid problems associated with over-testing and to uncover a general effect of a program based on a set of correlated variables. We apply factor analysis with a varimax rotation to the children's drawings to obtain three orthogonal factors related to children's psychological well-being: happiness, self-efficacy, and hopelessness.

## 4. Empirical Results

Table 2 presents summary statistics that combine the three preliminary studies (Bolivia, India and the first Kenya study), and then separately for the main (second) Kenya study and the Indonesia study. Since the summary indices are demeaned and normalized within villages, these values are not exactly equal to zero, but are very close. Some noticeable differences include the fact that the respondents in the main Kenya study were more likely to hope for and expect a white collar job (0.900) and (0.818), respectively, relative to the other countries. Respondents from the three preliminary studies also expect to achieve higher levels of education.

### 4.1 Bolivia and other Preliminary Study Sites

Table 3 provides summary statistics for the three preliminary study sites in Bolivia, India and Kenya (first study). The sample is restricted to children between the ages of 7 and 19 (this excludes only a few children). Those who were younger could not always fully grasp the meaning of the questions, and those that were older than 19 tended to report realized outcomes instead of aspirations or expectations. While not statistically significant, sponsored children have higher scores on the self-esteem and optimism indices by 0.067 and 0.016 standard deviations, respectively, and are 6.0 percentage points more likely to hope to have a white collar job. A major difference between sponsored and unsponsored children lies in *expectations* concerning a white

collar job; sponsored children are 15.0 percentage points more likely to expect such a job a difference that is significant at the 10 percent level. There is also a large and significant difference of 0.196 standard deviations in the aspirations index between sponsored and unsponsored children.

Table 4 presents the results from estimating equation (1) controlling for village fixed effects, age at time of survey and gender. The counterfactuals here are non-sponsored siblings and non-sponsored peers such as friends or classmates. The results indicate that sponsored children had higher values for each of our six dependent variables, although only one of these differences is statistically significant at conventional levels. More specifically, when assymptotic formulas are used for clustered standard errors none of these differences is statistically significant, but inference based on the wild bootstrap indicates that the estimate that sponsored children expect to achieve 0.40 more years of education is statistically significant at close to the 5% level (p-value of 0.057).

While the regression results in Table 4 suggest a causal impact of the Compassion program on children's aspirations, they could be biased due to selection of households into the program and selection of children to be sponsored within the households selected for the program. Even so, these findings of higher levels of aspirations among sponsored children motivated us to conduct follow up studies in Kenya and Indonesia. The data collected in these two follow up countries allow us to exploit the same eligibility criteria used in Wydick, Glewwe, and Rutledge (2013) and thus allow us to estimate causal effects of the sponsorship program. Even so, we believe that these simple OLS results are still informative, as the point-estimates for the countries where we are able to estimate causal impacts are not very different from the analogous OLS estimates.

## 4.2 Kenya

Table 5 provides summary statistics for the main (second) study in Kenya. These data allow us to exploit the age-eligibility rule because they were collected in villages where the sponsorship programs had been implemented more recently than in the three preliminary studies. Simple *t*-tests indicate that sponsored children were 0.137 standard deviations higher on the self-esteem index ($p < 0.05$), were 7.8 percentage points more likely to state that they expected to have a white collar job by ($p < 0.10$), expected to achieve 0.3 more years of education ($p < 0.01$) and were 0.122 standard deviations higher on the personal aspirations index ($p < 0.10$). While not statistically significant, sponsored children also had higher scores on the optimism index and were more likely to hope to have a white collar job as an adult.

Table 6 presents the results from estimating equation (1) for the main Kenya study, controlling for village fixed effects, age at time of survey, gender, birth order, family size, parents' education and missing parents' education. While all estimated impacts are in the expected direction, none is statistically significant.[5] Yet the sizes of some of these estimated impacts, even though statistically insignificant, are large; for example, the self-esteem index is estimated to increase by 0.17 standard deviations and there is a 7.2 percentage point increase in the probability of expecting to obtain a white collar job.

While these OLS results are similar to those found in the three preliminary studies, even if they were statistically significant they are not necessarily estimates of causal effects. In order to address this bias, we estimate equation (3) using a vector of age at program rollout dummy variables as instruments for sponsorship. The first stage results from equation (2) show that the instruments are strong, with an $F$-statistic of 72.12. The strong first-stage results stem from the fact that children over 9 years old at the time of project implementation had virtually no chance of being sponsored and that children who were roughly in the 4-9 age range when the program started in their village or neighborhood had a very high probability of being sponsored.

The IV estimations in Table 7 yield local average treatment effects that, interestingly, largely mirror those of the OLS estimations in Table 6, although they have higher statistical significance.[6] Sponsorship leads to an increase in the self-esteem index of 0.158 standard deviations ($p < 0.05$), which is almost identical to the OLS estimate of 0.166. The impact of sponsorship on optimism is positive but statistically insignificant, as was the OLS estimate. Sponsored children are 10.0 percentage points ($p < 0.05$) more likely to hope for a white collar job, which is about 2.5 times the magnitude of the (statistically insignificant) coefficient from the OLS estimations. Sponsored children are 9.3 percentage points ($p < 0.10$) more likely to expect a white collar job, which is similar to the OLS estimate of 7.2 percentage points. Sponsored children expect to achieve 0.275 additional years of education ($p < 0.10$), which is somewhat higher than the OLS estimate of 0.180. The final column of Table 7 shows that sponsorship caused children to increase their aggregate educational and vocational aspirations by 0.247 standard deviations ($p<0.01$), which is more than double the OLS estimate of 0.095. Overall, the IV estimations from

[5] This lack of statistical significance is found both for clustered standard errors using standard asymptotic formulas and for the wild bootstrap p-value. Also the t- value for the impact of the program on self-esteem is 2.43, it is not statisticall significant [p-value of 0.136] because the degrees of freedom is only 3, the number of villages (clusters) in the sample.

[6] Wild bootstrap p-values are not shown because no one has extended the wild bootstrap method to IV estimation. Note that the standard errors are clustered at the household level only, although in principle it would be desirable to cluster at the village level; clustering at the village level is not possible because the number of instruments exceeds the number of clusters.

the main Kenya study are either similar to, or somewhat larger than, the corresponding OLS estimates in Table 6, which suggests that the positive correlations between sponsorship and aspirations found in the three preliminary studies are also likely to be causal effects.

## 4.3 Indonesia Survey Results

To explore the external validity of the Kenya results, we collected similar data in Indonesia. Although additional respondents were surveyed, we restrict the analysis here to those between the ages of 7 and 19 for comparability with the three preliminary studies and the main (second) Kenya study. An important difference between the Indonesia data and the main (second) Kenya data is that the non-sponsored children in the Indonesia study also include children from non-treated households that were waitlisted for entry into the program but never actually had any child in their household sponsored. Table 8 provides summary statistics. Although not statistically significant, sponsored children had higher levels of self-esteem (0.014 standard deviations) and optimism (0.083). Surprisingly, they were 10.6 percentage points less likely to report that they expected to obtain a white collar job ($p$<0.10). On the other hand, they expected to achieve 0.53 more years of education than nonsponsored children ($p$ < 0.05). The unexpected result for expecting to obtain a white collar job may be partially due to Compassion choosing the neediest children for sponsorship.

Table 9 presents OLS estimations of equation (1') controlling for treated household, age, gender, birth order, size of family, and village fixed effects. The impacts of sponsorship are generally statistically insignificant, although the positive point estimate is relatively large for years of expected education (0.37 years), which is similar to the OLS estimates for the three preliminary studies in table 4 (0.40 years) but larger in magnitude than the OLS estimates in Table 6 for Kenya (0.18 years). Somewhat surprisingly, the coefficients on the self-esteem and aspirations indices, and on expecting to obtain a white collar job, are actually negative, and the last is marginally statistically significant.

Table 10 presents estimations of equation (3'), instrumenting for sponsorship with dummy variables for age at program rollout, and controlling for treated household, age, gender, birth order, family size, and village fixed effects.[7] The results largely mirror those in Table 9, both in sign and magnitude, and in (lack of) statistical significance. One notable exception is that the sign

[7] As with the Kenya IV estimates, the wild bootstrap cannot be used, and these results cluster the standard errors at the household level, due to the small number of villages (only 40). **IN FACT, THESE ARE NOT YET CLUSTERED AT THE HOUSEHOLD LEVEL; THIS WILL BE DONE NEXT WEEK ONCE WE GET THE HOUSEHOLD ID CODE VARIABLE STRAIGHTENED OUT.**

of the impact of the program on the aspirations index is now positive (although still insignificant), as one would expect. Overall, the survey results from Indonesia are all statistically insignificant, while a few of the results from the main Kenya study, and from three preliminary studies, are statistically significant.

Since the selection of the non-sponsored child was not random, as a robustness check we look at families that only had one or two children and thus there could have been no bias in the selection of the non-sponsored child in the family that participated. Tables A1 and A2 duplicate table 9 and 10 on this sub-sample. While the standard errors are much larger due to a much smaller sample size, the point estimates are very similar.

## 4.4 Combined Survey Results

Next, to maximize our sample size, we aggregate survey data across countries to see whether there are, on average, positive estimates across the study countries. Table 11 presents OLS estimations of equation (1), controlling for age, gender, and village fixed effects, that combine the data from all five studies in all four countries. The results indicate a strong positive impact of the Compassion program on self-esteem by 0.085 standard deviations ($p < 0.05$), years of expected education by 0.416 years ($p < 0.05$), and the general aspirations index by 0.106 std. dev. ($p < 0.10$). The wild bootstrap p-values indicate very similar levels of statistical significance for these three variables.

Of course, these OLS estimates could be biased, so Table 12 combines the data on sponsored households from the Indonesia study and the main Kenya study. Since waitlisted households were not surveyed in Kenya, these estimates exclude the waitlisted households in the Indonesia data. Again, equation (3) is estimated using dummy variables for age at time of program rollout to instrument for sponsorship, with one exception: We group all of those who were two or younger into one category.[8] As seen in Table 12, the coefficients on all the outcome variables are positive. The estimated impact of the Compassion program on children's expectations of obtaining a white-collar job in adulthood is a 7.7 percentage point increase, but it is statistically insignificant. In contrast, we do find statistically significant positive impacts on the optimism index (0.164 std. dev., with $p < 0.10$), hoping for a while collar job (11.5 percentage points, with

[8] This was done to unify the dummy variables across Indonesia and Kenya since in Kenya we never surveyed children that were younger than two at the time of sponsorship (see figure 1). As a check, Table A3 replicates Table 10 of the Indonesia IV estimation results except that for the instrument all of those younger than two are grouped into one dummy variable. The results of the two tables are very similar. **[AS WITH TABLE 10, HAVE NOT YET DONE THIS WITH CLUSTERED ERRORS AT THE HOUSEHOLD LEVEL; WILL BE DONE LATER IN OCTOBER.]**

$p<0.05$), years of expected education (0.61 years, with $p < 0.01$), and in their personal aspirations index (0.272 std. dev., with $p < 0.01$).

Overall, the power gained by combining results across our countries of study allows us to look at the overall impact of the program across the countries of the study. In particular, the 0.272 standard deviation increase caused by sponsorship on the aspirations index in column 6 of Table 12 appears to reflect significant overall impacts on aspirations from child sponsorship.

## 4.5 Indonesia Drawings

Each child who participated in the study in Indonesia was invited to sit at a small desk or table and was given a white sheet of paper with a new box of 24 colored pencils. The subjects were then asked to "Draw a picture of yourself in the rain." Table 13 provides summary statistics on the 20 drawing characteristics measured from these drawings. Children's self-portraits have been analyzed in a lengthy psychology literature, and often yield insightful information into the psychological makeup of children that is more difficult to obtain accurately from direct survey questions. The correlation between these drawings and their respective psychological attributes is taken from classic studies in the human figure drawing literature, including Koppitz, (1968), Klepsch and Logie (1982), Thomas and Silk (1990), and Furth (2002). A carrot symbol ("^") indicates that the measures for which a positive value represents a negative psychological outcome. These 20 characteristics were taken from the psychology literature and were chosen before any analysis of the drawings, and none were added or dropped after empirical analysis began.

As can be seen from simple *t*-tests, 11 of the 20 measures display statistically significant differences between sponsored and non-sponsored children, and 10 of these indicate an unequivocally more positive psychological outcome for sponsored children. (The remaining variable, "long arms," which describes a self-portrait with abnormally long arms is ambiguous; it has been associated with both emotional neediness as well as affection for others.)

Figures 2-4 provide examples of children's drawings that show variation in happiness, self-efficacy, and hopelessness, three factors we generated by conducting factor analysis on our drawing data. Figures 2A and 2B illustrate differences in happiness between two children of roughly the same age, where facial expression and body language display remarkable contrast between the two drawings, such that the drawing in 2A ranks in the only the 17th percentile in the Happiness factor, while the drawing on the right in 2B ranks in the 92nd percentile.

Figures 3A and 3B show two children's drawings ranking in the 8th percentile and 94th percentile, respectively, in Self-Efficacy/Optimism. Salient characteristics of the drawing in 3A with negative correlations to the latent factor include the use of a single color, the presence of lightning, and poor integration of body parts. These contrast to the multiple light colors used in 3B, the presence of a sun above the clouds, and the child using an umbrella to protect herself from the rain.

Figures 4A and 4B illustrate differences in our Hopelessness factor, where the drawing on the left in 4A was done by a teenage girl and the drawing on the right by a boy in elementary school. Note the missing facial features and hidden limbs in the girl's self-portrait on the left, all factors correlated with hopelessness and depression. In contrast, the bright colors used by the boy on the right in 4B, facial expression, full illustration of facial features and limbs, use of the umbrella are factors that have been empirically correlated with hopefulness in children (Klepsch and Logie, 1982, Furth, 2002).

Table 14 provides summary statistics on the three factors we assemble using the measures in Table 13, along with responses to questions from our optimism and self-esteem indices. These factors were created using factor analysis with the varimax rotation discussed in section 3.3, where the varimax rotation ensures that each of the factors exhibit a zero correlation between themselves. Because the drawing analysis was carried out only in Indonesia, we do not combine or compare results across countries, and so we do not restrict this sample by age. From simple *t*-tests, sponsored children scored 0.203 higher ($p < 0.10$) on the happiness factor, and 0.221 higher ($p < 0.05$) on the self-efficacy factor, and 0.338 lower ($p < 0.01$) lower on the hopelessness factor.

Table 15 shows rotated factor loadings from an analysis for which we allow for three factors. We give names to the three factors based on correlations between each factor and five variables in our survey (three that represent hope and two that represent self-esteem[9]) and the twenty drawing characteristic variables from the children's artwork. We labeled Factor 1 "Happiness" because it is very strongly positively correlated with a smiling self-portrait and negatively with a frowning or crying self-portrait and negatively correlated with a series of missing body and facial parts, the lack of which are correlated with emotional disturbance. We named Factor 2 "Self-Efficacy/Optimism" because it was strongly correlated with cheery colors, positive body language, and especially with the self-portrait figure holding an umbrella or taking

[9] These are "Do you believe that the future holds good things for you?" (hope1), "When you are old, will you have a good job and income?" (hope2), "Will your adult life be better than that of your parents?" (hope3), "Do you sometimes think that you do not have much to be proud of?" (self-esteem3) "At times do you think that you are not much good at all?" (self-esteem5).

shelter proactively from the rain. Factor 3 was a negative psychological factor that we called "Hopelessness" because, congruent with the existing empirical literature, it was strongly correlated with poor integration of body parts, missing facial features, drawn in a single color, and drawn as a monster figure, and was strongly correlated with our two (low) self-esteem questions.

Table 16 estimates equation (1') with and without village fixed effects, and equation (3'), which includes village fixed effects, on the happiness, self-efficacy, and hopelessness factors. Each of the coefficient estimations on all of the three factors are indicative of enhanced psychological well-being among sponsored children. All are significant at the 1% or 5% level except for the IV regression on self-efficacy. The wild bootstrap p-values, shown only for the OLS estimates with village fixed effects, show significance at the 10% level. These estimates show ranges between a 0.24 and 0.55 standard deviation positive impact in Factor 1 (Happiness), a 0.13 to 0.33 standard deviation positive impact in Factor 2 (Self-Efficacy), and a 0.35 to 0.88 standard deviation *decrease* in Factor 3 (Hopelessness) among sponsored children. For robustness, we test whether these results hold up when omitting drawing characteristics that could be affected by experience with drawing[10], since non-sponsored children may have less opportunity to draw. Tables A5-A7 duplicate Tables 14-16 and show very similar results. Overall, our analysis of children's self-portrait drawings provides additional evidence for a causal link between sponsorship and positive psychological impacts in the areas of self-esteem and aspirations.

## 5. Conclusion

This research seeks to explain the underlying mechanisms for the positive impacts on life outcomes of child sponsorship found in Wydick, Glewwe, and Rutledge (2013). A strong focus of Compassion's sponsorship program is in building the self-esteem and aspirations of sponsored children regarding educational and vocational outcomes. We test whether the program has a causal impact in these areas in order to investigate the possibility of a causal link between the development of aspirations among the poor and economic development. If such a causal link can be established, it would have significant implications for the way in which both researchers and practitioners think about how virtuous cycles of economic development occur among the poor in developing countries. It would mean that the relief of not only external constrains, which have long been the focus of development economics, but also internal constraints are key to the development process.

[10] These characteristics are long arms, poor integration of body parts, erasure marks/scribble outs, tiny head and short arms

Our analysis indicates that Compassion's child sponsorship program has large causal effects that lead to higher self-esteem and higher self-expectations for education and employment. Our analysis of children's drawings in Indonesia indicates large causal impacts on happiness, self-efficacy, and hopefulness about the future. These results account for both potential endogeneity due to family selection and intra-household selection among age-eligible children.

Seen in the broader sense of behavioral and development economics, this study suggests that when evaluating the impacts of programs it is important to consider not only the relief of external constraints, but also the addressing of internal constraints, the psychological factors that can lead to persistent poverty through low self-esteem and aspirations. If these two types of interventions are complements to each other, a combined intervention with children may be able to have a much greater impact than either would on its own. Greater understanding of factors such as enhanced aspirations and self-efficacy could lead to more effective international aid programs for children and a deeper understanding of why some programs have stronger impacts than others.

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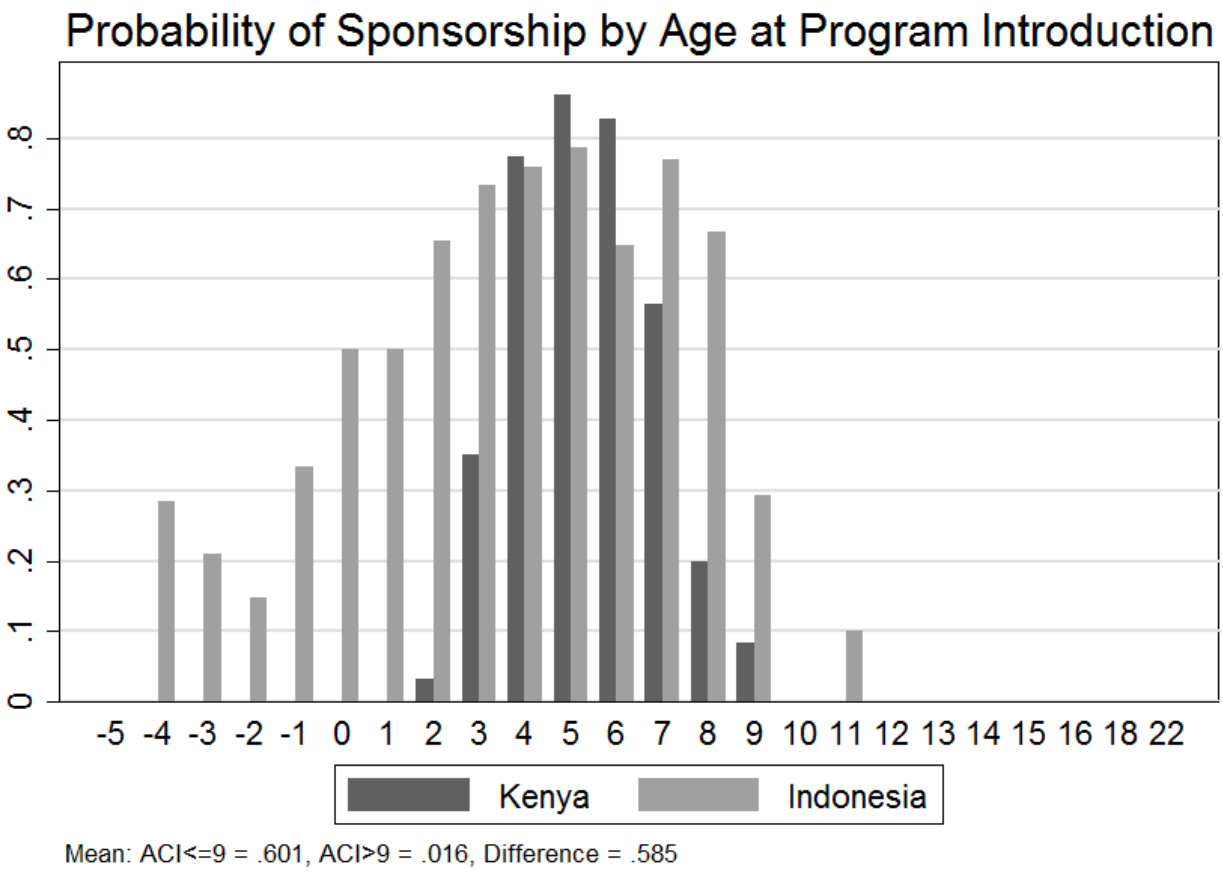


Figure 1. Discontinuity in sponsorship by age at time of program introduction

Figure 2a: Happiness, 17th percentile

Figure 2b: Happiness, 92nd percentile

Figure 3a: Self-Efficacy, 8th percentile

Figure 3b: Self-Efficacy, 94th percentile

Figure 4a: Hopelessness, 85th percentile

Figure 4b: Hopelessness, 7th percentile

**Table 1. Characteristics of Study Villages**

| County | Treatment Villages (year of program rollout) | Sample Size | Time of Investigation | Instrument? | Drawings? |
|---|---|---|---|---|---|
| India | Bangalore (1986) | 29 | March 2010 | No | No |
| Kenya | Thigio(1990) | 90 | May-June 2010 | No | No |
| Bolivia | Puntiti (1991), Chulla (1992), Vieche (1995), Los Olivios (1990), Pongunhoyo (1980) | 159 | June-July 2010 | No | No |
| Kenya | Rironi (2003), Isinya (2003), Njoro (2003) | 570 | May-July 2011 | Yes | No |
| Indonesia | (2003), (2003), (2007), (2007) | 542 | May-July 2012 | Yes | Yes |

**Table 2. Summary Statistics for Items Consistent across All Countries**

| | Bolivia, India and First Kenya Study (std. dev.) | Main (Second) Kenya Study (std. dev.) | Indonesia (std. dev.) | All Studies Combined (std. dev.) |
|---|---|---|---|---|
| Self Esteem Index | -0.003 | -0.002 | -0.001 | -0.002 |
| | (0.503) | (0.522) | (0.579) | (0.539) |
| Optimism index | -0.002 | -0.000 | 0.000 | -0.000 |
| | (0.698) | (0.736) | (0.701) | (0.715) |
| Hope for White Collar Job (%) | 0.706 | 0.900 | 0.566 | 0.746 |
| | (0.456) | (0.300) | (0.496) | (0.435) |
| Expect White Collar Job (%) | 0.618 | 0.818 | 0.573 | 0.689 |
| | (0.487) | (0.387) | (0.495) | (0.463) |
| Years of Education Expected | 17.136 | 15.449 | 15.089 | 15.670 |
| | (1.961) | (1.320) | (2.111) | (1.932) |
| Aspirations Index | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.702) | (0.632) | (0.637) | (0.648) |
| Age | 13.676 | 13.721 | 11.140 | 12.787 |
| | (1.860) | (1.976) | (2.817) | (2.600) |
| Male | 0.625 | 0.544 | 0.457 | 0.530 |
| | (0.485) | (0.499) | (0.499) | (0.499) |
| Family Size* | 3.139 | 3.249 | 3.415 | 3.300 |
| | (1.929) | (2.129) | (1.363) | (1.839) |
| Observations | *272* | *570* | *470* | *1,312* |

*Note:* *In column 1, family was collected only in Bolivia, not in India or the first Kenya study.

**Table 3. Summary Statistics for the Three Preliminary Studies**

| | Mean, All (std. dev.) | Mean, Sponsored (std. dev.) | Mean, Non-Sponsored (std. dev.) | Difference *t*-test (std. error) |
|---|---|---|---|---|
| Self Esteem Index | -0.003 | 0.033 | -0.034 | 0.067 |
| | (0.503) | (0.480) | (0.521) | (0.061) |
| Optimism index | -0.002 | 0.007 | -0.010 | 0.016 |
| | (0.698) | (0.682) | (0.714) | (0.085) |
| Hope for White Collar Job (%) | 0.706 | 0.738 | 0.678 | 0.060 |
| | (0.456) | (0.441) | (0.469) | (0.055) |
| Expect White Collar Job (%) | 0.618 | 0.698 | 0.548 | 0.150* |
| | (0.487) | (0.461) | (0.499) | (0.059) |
| Years of Education Expected | 17.136 | 17.381 | 16.925 | 0.456 |
| | (1.961) | (1.528) | (2.253) | (0.237) |
| Personal Aspirations Index | -0.000 | 0.105 | -0.091 | 0.196* |
| | (0.702) | (0.645) | (0.739) | (0.085) |
| Age | 13.676 | 13.952 | 13.438 | 0.514* |
| | (1.860) | (1.743) | (1.930) | (0.224) |
| Male | 0.625 | 0.635 | 0.616 | 0.018 |
| | (0.485) | (0.483) | (0.488) | (0.059) |
| Birth Order | 3.139 | 3.306 | 2.924 | 0.382 |
| | (1.929) | (1.940) | (1.908) | (0.316) |
| Family Size | 4.934 | 5.145 | 4.676 | 0.468 |
| | (1.962) | (1.933) | (1.981) | (0.320) |
| Mother's Education | 5.669 | 5.250 | 6.219 | -0.969 |
| | (4.091) | (3.683) | (4.544) | (0.676) |
| Father's Education | 7.779 | 7.537 | 8.095 | -0.559 |
| | (3.943) | (3.656) | (4.298) | (0.661) |
| Missing Mother's Education | 0.033 | 0.012 | 0.059 | -0.047 |
| | (0.178) | (0.108) | (0.237) | (0.029) |
| Missing Father's Education | 0.052 | 0.035 | 0.074 | -0.038 |
| | (0.223) | (0.186) | (0.263) | (0.036) |

The three preliminary studies were done in Bolivia, India and Kenya.
Full sample = 272: 126 sponsored, 66 non-sponsored siblings, 80 non-sponsored peers. *** p<0.01, ** p<0.05, * p<0.1

**Table 4. OLS estimations for the Three Preliminary Studies**

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope for White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | 0.059 | 0.003 | 0.065 | 0.134 | 0.401 | 0.199 |
| | (0.077) | (0.142) | (0.066) | (0.096) | (0.252) | (0.123) |
| | [0.413] | [0.969] | [0.321] | [0.203] | [0.057*] | [0.119] |
| Age | 0.031** | -0.021 | 0.019 | -0.011 | 0.168** | 0.042 |
| | (0.011) | (0.022) | (0.021) | (0.015) | (0.066) | (0.023) |
| Male | 0.104** | 0.179 | 0.105 | 0.185 | 0.161 | 0.261** |
| | (0.040) | (0.115) | (0.070) | (0.137) | (0.272) | (0.095) |
| Observations | 272 | 272 | 272 | 272 | 272 | 272 |
| Adjusted $R^2$ | -0.010 | -0.015 | 0.053 | 0.050 | 0.092 | 0.026 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors clustered at the village level in parentheses. Wild bootstrap p-values for the "Sponsored" variable shown in brackets. Village fixed effects are included in all regressions.

**Table 5. Summary Statistics for Main (Second) Kenya Study**

| | Mean, All (std. dev.) | Mean, Sponsored (std. dev.) | Mean, Non-Sponsored (std. dev.) | Difference, *t*-test (std. error) |
|---|---|---|---|---|
| Self Esteem Index | -0.002 | 0.055 | -0.082 | 0.137** |
| | (0.522) | (0.497) | (0.545) | (0.044) |
| Optimism index | -0.000 | 0.046 | -0.065 | 0.112 |
| | (0.736) | (0.683) | (0.801) | (0.062) |
| Hope for White Collar Job (%) | 0.900 | 0.919 | 0.873 | 0.046 |
| | (0.300) | (0.273) | (0.333) | (0.025) |
| Expect White Collar Job (%) | 0.818 | 0.850 | 0.772 | 0.078* |
| | (0.387) | (0.358) | (0.420) | (0.033) |
| Years of Education Expected | 15.449 | 15.574 | 15.274 | 0.299** |
| | (1.320) | (0.956) | (1.691) | (0.112) |
| Aspirations Index | -0.000 | 0.051 | -0.071 | 0.122* |
| | (0.632) | (0.642) | (0.613) | (0.054) |
| Age | 13.721 | 13.366 | 14.219 | -0.853*** |
| | (1.976) | (1.204) | (2.635) | (0.164) |
| Male | 0.544 | 0.547 | 0.540 | 0.006 |
| | (0.499) | (0.499) | (0.499) | (0.042) |
| Birth Order | 3.249 | 3.150 | 3.388 | -0.238 |
| | (2.129) | (2.180) | (2.051) | (0.181) |
| Family Size | 4.788 | 4.471 | 5.232 | -0.761*** |
| | (2.221) | (2.247) | (2.110) | (0.186) |
| Mother's Education | 7.633 | 7.771 | 7.442 | 0.329 |
| | (4.021) | (4.066) | (3.959) | (0.346) |
| Father's Education | 8.657 | 8.840 | 8.420 | 0.419 |
| | (3.936) | (4.021) | (3.822) | (0.364) |
| Missing Mother's Education | 0.025 | 0.030 | 0.017 | 0.013 |
| | (0.155) | (0.171) | (0.129) | (0.013) |
| Missing Father's Education | 0.165 | 0.192 | 0.127 | 0.066* |
| | (0.371) | (0.395) | (0.333) | (0.031) |

Full sample = 570: 333 sponsored children, 237 non-sponsored siblings of sponsored children
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

**Table 6. OLS Estimations for Main Kenya Study**

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope for White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | 0.166 | 0.105 | 0.041 | 0.072 | 0.180 | 0.095 |
| | (0.068) | (0.156) | (0.050) | (0.032) | (0.188) | (0.063) |
| | [0.131] | [0.867] | [0.897] | [0.131] | [0.369] | [0.131] |
| Age | 0.030 | 0.011 | -0.011* | -0.015* | -0.033 | -0.001 |
| | (0.011) | (0.008) | (0.003) | (0.004) | (0.043) | (0.018) |
| Male | 0.023 | 0.030 | -0.114** | -0.095 | 0.128 | -0.103 |
| | (0.052) | (0.171) | (0.026) | (0.050) | (0.140) | (0.096) |
| Birth Order | 0.002 | 0.011 | -0.007 | -0.011 | 0.079 | 0.025 |
| | (0.037) | (0.007) | (0.003) | (0.022) | (0.030) | (0.040) |
| Family Size | 0.003 | -0.026 | 0.008 | 0.018 | -0.131 | -0.022 |
| | (0.046) | (0.012) | (0.003) | (0.020) | (0.059) | (0.042) |
| Observations | 570 | 570 | 570 | 570 | 570 | 570 |
| Adjusted $R^2$ | 0.033 | -0.002 | 0.052 | 0.031 | 0.127 | 0.016 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors clustered at the village level in parentheses. Wild bootstrap p-values for the "Sponsored" variable shown in brackets. Village fixed effects and dummy variables for parent's education (including a dummy for missing parent's education) are included in all regressions.

**Table 7. IV Estimations for Main Kenya Study**

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope for White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | 0.158** | 0.021 | 0.100** | 0.093* | 0.275* | 0.247*** |
| | (0.065) | (0.093) | (0.043) | (0.053) | (0.155) | (0.087) |
| Age | 0.030*** | 0.007 | -0.008 | -0.014 | -0.028 | 0.007 |
| | *(0.011)* | *(0.016)* | *(0.007)* | *(0.009)* | *(0.026)* | *(0.014)* |
| Male | 0.023 | 0.032 | -0.115*** | -0.096*** | 0.127 | -0.105** |
| | (0.043) | (0.065) | (0.023) | (0.032) | (0.109) | (0.052) |
| Birth Order | 0.003 | 0.015 | -0.010 | -0.012 | 0.075 | 0.018 |
| | (0.021) | (0.030) | (0.011) | (0.013) | (0.059) | (0.026) |
| Family Size | 0.002 | -0.032 | 0.012 | 0.019 | -0.124** | -0.011 |
| | (0.023) | (0.031) | (0.012) | (0.014) | (0.061) | (0.024) |
| Observations | 570 | 570 | 570 | 570 | 570 | 570 |
| Adjusted $R^2$ | 0.033 | -0.005 | 0.044 | 0.030 | 0.125 | 0.003 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors clustered at the household level in parentheses. Village fixed effects and dummy variables for parent's education (including a dummy for missing parent's education) are included in all regressions. Dummies for age at program rollout are used as an instrument for sponsorship. F-statistic for first stage estimation: 72.12

**Table 8. Summary Statistics for Survey Questions for Indonesia**

| | Mean, All (std. dev.) | Mean, Sponsored (std. dev.) | Mean, Non-Sponsored (std. dev.) | Difference *t*-test (std. error) |
|---|---|---|---|---|
| Self Esteem Index | -0.001 | 0.003 | -0.005 | 0.008 |
| | (0.534) | (0.515) | (0.563) | (0.050) |
| Optimism index | 0.000 | 0.034 | -0.049 | 0.083 |
| | (0.701) | (0.691) | (0.714) | (0.066) |
| Hope for White Collar Job (%) | 0.566 | 0.545 | 0.596 | -0.051 |
| | (0.496) | (0.499) | (0.492) | (0.049) |
| Expect White Collar Job (%) | 0.573 | 0.529 | 0.635 | -0.106* |
| | (0.495) | (0.500) | (0.483) | (0.047) |
| Years of Education Expected | 15.089 | 15.307 | 14.777 | 0.530** |
| | (2.111) | (1.883) | (2.371) | (0.197) |
| Aspirations Index | -0.000 | 0.001 | -0.002 | 0.004 |
| | (0.637) | (0.627) | (0.652) | (0.060) |
| Age | 11.140 | 11.260 | 10.969 | 0.291 |
| | (2.817) | (2.351) | (3.374) | (0.264) |
| Male | 0.457 | 0.455 | 0.461 | -0.006 |
| | (0.499) | (0.499) | (0.500) | (0.047) |
| Birth Order | 3.415 | 3.493 | 3.302 | 0.191 |
| | (1.363) | (1.366) | (1.355) | (0.128) |
| Family Size | 3.530 | 3.493 | 3.583 | -0.091 |
| | (1.330) | (1.366) | (1.279) | (0.125) |

Full sample = 470: 277 sponsored children, 58 waitlisted children, 90 siblings of sponsored children, and 45 siblings of waitlisted children.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

**Table 9. OLS Estimations for Indonesia**

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope for White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | -0.042 | 0.060 | 0.013 | -0.089* | 0.374 | -0.043 |
| | (0.064) | (0.080) | (0.064) | (0.032) | (0.298) | (0.048) |
| | [0.558] | [0.442] | [0.930] | [0.070*] | [0.431] | [0.414] |
| Treated Household | 0.069 | 0.051 | -0.109 | -0.029 | 0.188 | 0.084* |
| | (0.036) | (0.122) | (0.071) | (0.049) | (0.244) | (0.027) |
| Age | 0.022 | 0.053*** | 0.000 | 0.005 | 0.082 | 0.027* |
| | (0.016) | (0.007) | (0.005) | (0.007) | (0.041) | (0.011) |
| Male | 0.013 | 0.139*** | -0.566*** | -0.442*** | -0.122 | -0.350*** |
| | (0.031) | (0.023) | (0.043) | (0.061) | (0.138) | (0.046) |
| Birth Order | 0.015 | -0.040 | 0.022 | 0.023 | -0.052 | -0.046 |
| | (0.090) | (0.061) | (0.020) | (0.050) | (0.302) | (0.091) |
| Family Size | -0.005 | 0.094** | -0.021 | -0.032 | -0.019 | 0.025 |
| | (0.073) | (0.029) | (0.026) | (0.049) | (0.332) | (0.092) |
| Observations | 468 | 468 | 421 | 459 | 468 | 468 |
| Adjusted $R^2$ | 0.002 | 0.058 | 0.325 | 0.216 | 0.032 | 0.076 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors clustered at the village level in parentheses. Wild bootstrap p-values for the "Sponsored" variable shown in brackets. Village fixed effects included in all regressions.

**Table 10. IV Estimations for Indonesia**

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope for White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | -0.084 | 0.120 | 0.036 | -0.128 | 0.399 | 0.045 |
| | (0.130) | (0.157) | (0.108) | (0.107) | (0.441) | (0.143) |
| Treated Household | 0.102 | 0.004 | -0.124 | 0.001 | 0.168 | 0.020 |
| | (0.124) | (0.154) | (0.097) | (0.097) | (0.455) | (0.136) |
| Age | 0.021** | 0.055*** | 0.000 | 0.004 | 0.083** | 0.029*** |
| | (0.010) | (0.012) | (0.008) | (0.008) | (0.036) | (0.010) |
| Male | 0.012 | 0.140** | -0.573*** | -0.443*** | -0.122 | -0.351*** |
| | (0.049) | (0.062) | (0.040) | (0.042) | (0.189) | (0.056) |
| Birth Order | 0.016 | -0.041 | 0.020 | 0.024 | -0.053 | -0.050 |
| | (0.061) | (0.057) | (0.050) | (0.054) | (0.250) | (0.070) |
| Family Size | -0.006 | 0.096* | -0.018 | -0.033 | -0.018 | 0.031 |
| | (0.061) | (0.057) | (0.052) | (0.057) | (0.261) | (0.074) |
| Observations | 468 | 468 | 422 | 459 | 468 | 468 |
| Adjusted $R^2$ | 0.001 | 0.057 | 0.333 | 0.215 | 0.032 | 0.075 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors in parentheses, controls for village fixed effects. Dummies for age at program rollout used as an instrument for sponsorship. F-statistic for first-stage estimation: 69.22

**Table 11. OLS Estimations Using Data from All Five Studies**

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope for White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | 0.085** | 0.090 | 0.032 | 0.036 | 0.416** | 0.106* |
| | (0.039) | (0.075) | (0.030) | (0.035) | (0.139) | (0.050) |
| | [0.085*] | [0.298] | [0.650] | [0.160] | [0.014**] | [0.056*] |
| Age | 0.026*** | 0.030** | -0.002 | -0.006 | 0.058* | 0.018 |
| | (0.007) | (0.010) | (0.004) | (0.006) | (0.032) | (0.011) |
| Male | 0.036 | 0.095 | -0.234*** | -0.172** | 0.005 | -0.138* |
| | (0.024) | (0.073) | (0.075) | (0.077) | (0.088) | (0.068) |
| Observations | 1,312 | 1,312 | 1,265 | 1,303 | 1,312 | 1,312 |
| Adjusted $R^2$ | 0.007 | 0.005 | 0.200 | 0.105 | 0.206 | 0.008 |

*** p<0.01, ** p<0.05, * p<0.1, Robust standard errors clustered at the village level in parentheses. Wild bootstrap p-values for the "Sponsored" variable shown in brackets. Village fixed effects included in all regressions.

**Table 12. IV Estimations for Indonesia and Kenya Combined**

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope for White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | 0.049 | 0.164* | 0.115** | 0.077 | 0.605*** | 0.272*** |
| | (0.068) | (0.093) | (0.052) | (0.058) | (0.211) | (0.079) |
| Age | 0.019** | 0.037*** | -0.003 | -0.004 | 0.036 | 0.017* |
| | (0.008) | (0.010) | (0.006) | (0.007) | (0.026) | (0.009) |
| Male | 0.031 | 0.070 | -0.277*** | -0.225*** | 0.029 | -0.201*** |
| | (0.034) | (0.047) | (0.024) | (0.027) | (0.102) | (0.040) |
| Birth Order | 0.013 | 0.027 | -0.004 | -0.004 | 0.083 | 0.021 |
| | (0.019) | (0.027) | (0.012) | (0.013) | (0.057) | (0.024) |
| Family Size | -0.011 | -0.023 | 0.007 | 0.007 | -0.150** | -0.028 |
| | (0.019) | (0.027) | (0.012) | (0.013) | (0.059) | (0.023) |
| Observations | 935 | 935 | 902 | 927 | 935 | 935 |
| Adjusted $R^2$ | 0.001 | 0.005 | 0.278 | 0.156 | 0.052 | -0.000 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors in parentheses, controls for village fixed effects. Dummies for age at program rollout used as an instrument for sponsorship. F-statistic for first-stage estimation: 63.68. Excludes waitlist households from Indonesia

**Table 13. Drawing Analysis of Psychological Factors Summary Statistics**

| | Mean, All (std. dev.) | Mean, Sponsored (std. dev.) | Mean, Non-Sponsored (std. dev.) | Difference *t*-test (std. error) |
|---|---|---|---|---|
| Huge Figure^ | 0.036 | 0.049 | 0.021 | 0.028 |
| | (0.187) | (0.215) | (0.144) | (0.016) |
| Monster^ | 0.074 | 0.045 | 0.109 | -0.064** |
| | (0.262) | (0.208) | (0.313) | (0.023) |
| Long Arms^ | 0.203 | 0.240 | 0.160 | 0.080* |
| | (0.403) | (0.428) | (0.367) | (0.035) |
| Shading | 0.253 | 0.250 | 0.256 | -0.006 |
| | (0.435) | (0.434) | (0.438) | (0.038) |
| Missing Mouth or Nose^ | 0.266 | 0.229 | 0.311 | -0.082* |
| | (0.442) | (0.421) | (0.464) | (0.039) |
| Frowning or Crying^ | 0.165 | 0.156 | 0.176 | -0.020 |
| | (0.372) | (0.364) | (0.382) | (0.033) |
| Dark Colors^ | 0.477 | 0.424 | 0.542 | -0.118** |
| | (0.500) | (0.495) | (0.499) | (0.044) |
| Single Color^ | 0.160 | 0.135 | 0.189 | -0.054 |
| | (0.367) | (0.343) | (0.392) | (0.032) |
| Weather | 0.072 | 0.066 | 0.080 | -0.014 |
| | (0.452) | (0.500) | (0.387) | (0.040) |
| Smiling | 0.679 | 0.733 | 0.613 | 0.119** |
| | (0.467) | (0.443) | (0.488) | (0.041) |
| Cheery Colors | 0.477 | 0.531 | 0.412 | 0.119** |
| | (0.500) | (0.500) | (0.493) | (0.044) |
| Tiny Figure^+ | 0.276 | 0.215 | 0.349 | -0.133*** |
| | (0.447) | (0.412) | (0.478) | (0.039) |
| Poor Integration of Body Parts^+ | 0.099 | 0.059 | 0.147 | -0.088*** |
| | (0.299) | (0.236) | (0.355) | (0.026) |
| Missing Arms or Hands^+ | 0.477 | 0.490 | 0.462 | 0.027 |
| | (0.500) | (0.501) | (0.500) | (0.044) |
| Missing Legs^ | 0.112 | 0.073 | 0.160 | -0.087** |
| | (0.316) | (0.260) | (0.367) | (0.027) |
| Erasure Marks or Scribble Outs^ | 0.078 | 0.066 | 0.092 | -0.026 |
| | (0.268) | (0.249) | (0.290) | (0.024) |
| Carrying Umbrella/Sought Shelter | 0.317 | 0.358 | 0.269 | 0.089* |
| | (0.466) | (0.480) | (0.444) | (0.041) |
| Body Language | 0.141 | 0.219 | 0.046 | 0.173* |
| | (0.802) | (0.812) | (0.781) | (0.070) |
| Tiny Head^ | 0.015 | 0.010 | 0.021 | -0.011 |
| | (0.123) | (0.102) | (0.144) | (0.011) |
| Short Arms^ | 0.219 | 0.191 | 0.252 | -0.061 |
| | (0.414) | (0.394) | (0.435) | (0.036) |

Full sample = 526: 288 sponsored, 79 waitlist, 112 sibling of sponsored, 47 sibling of waitlist, *** $p<0.01$, ** $p<0.05$, * $p<0.1$
^indicates this measure is taken as "negative" indicators and the rest are positive, +are used in the drawing self–esteem index.

**Table 14. Drawing Analysis Summary Statistics**

| | Mean, All (std. dev.) | Mean, Sponsored (std. dev.) | Mean, Non-Sponsored (std. dev.) | Difference *t*-test (std. error) |
|---|---|---|---|---|
| Happiness Factor | 0.000 | 0.092 | -0.111 | 0.203* |
| | (0.923) | (0.900) | (0.941) | (0.080) |
| Self-Efficacy Factor | -0.000 | 0.100 | -0.121 | 0.221** |
| | (0.955) | (0.947) | (0.953) | (0.083) |
| Hopelessness Factor | -0.000 | -0.153 | 0.185 | -0.338*** |
| | (0.762) | (0.657) | (0.838) | (0.065) |
| Age | 10.798 | 11.045 | 10.500 | 0.545 |
| | (3.428) | (2.547) | (4.244) | (0.300) |
| Male | 0.466 | 0.458 | 0.475 | -0.016 |
| | (0.499) | (0.499) | (0.500) | (0.044) |
| Birth Order | 3.398 | 3.488 | 3.288 | 0.200 |
| | (1.384) | (1.369) | (1.397) | (0.121) |
| Family Size | 3.530 | 3.488 | 3.581 | -0.093 |
| | (1.346) | (1.369) | (1.320) | (0.118) |

Full sample = 526: 288 sponsored, 79 waitlist, 112 sibling of sponsored, 47 sibling of waitlist, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

**Table 15. Rotated Factor Loadings**

| | Happiness | Self-Efficacy | Hopelessness | Uniqueness |
|---|---|---|---|---|
| Hopefulness Question 1 | 0.014 | -0.016 | -0.178 | 0.968 |
| Hopefulness Question 2 | 0.022 | -0.087 | -0.085 | 0.985 |
| Hopefulness Question 3 | 0.027 | -0.041 | 0.103 | 0.987 |
| Huge Figure | 0.005 | -0.037 | 0.019 | 0.998 |
| Monster | -0.044 | -0.059 | 0.428 | 0.812 |
| Long Arms | 0.046 | -0.014 | -0.067 | 0.993 |
| Shading | -0.009 | 0.144 | -0.084 | 0.972 |
| Missing Mouth or Nose | -0.390 | 0.129 | 0.316 | 0.732 |
| Frowning or Crying | -0.685 | -0.138 | -0.190 | 0.475 |
| Dark Colors | -0.048 | -0.928 | -0.033 | 0.135 |
| Single Color | -0.031 | -0.383 | 0.205 | 0.810 |
| Weather | 0.023 | 0.141 | 0.195 | 0.942 |
| Smiling | 0.896 | 0.011 | -0.134 | 0.179 |
| Cheery Colors | 0.082 | 0.921 | -0.017 | 0.145 |
| Tiny Figure | -0.138 | -0.026 | 0.105 | 0.969 |
| Poor Integration of Body Parts | -0.045 | 0.000 | 0.450 | 0.796 |
| Missing Arms or Hands | -0.268 | 0.054 | 0.133 | 0.908 |
| Missing Legs | -0.189 | 0.078 | 0.329 | 0.850 |
| Erasure Marks or Scribble Outs | 0.029 | -0.049 | 0.181 | 0.964 |
| Carrying Umbrella/Sought Shelter | 0.000 | 0.176 | -0.158 | 0.944 |
| Body Language | 0.706 | 0.187 | 0.071 | 0.462 |
| Tiny Head | 0.032 | -0.084 | 0.092 | 0.984 |
| Short Arms | 0.009 | -0.052 | 0.043 | 0.995 |
| Self Esteem Question 1 | -0.012 | 0.058 | 0.283 | 0.916 |
| Self Esteem Question 2 | -0.031 | 0.060 | 0.227 | 0.944 |

**Table 16. Estimations for Drawings**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | Happiness | | | Self-Efficacy | | | Hopelessness | |
| | (1', no fe) | (1', fe) | (3', fe) | (1', no fe) | (1', fe) | (3', fe) | (1', no fe) | (1', fe) | (3', fe) |
| Sponsored | 0.238** | 0.250** | 0.551*** | 0.320** | 0.327*** | 0.128 | -0.351** | -0.397** | -0.883*** |
| | (0.061) | (0.054) | (0.194) | (0.069) | (0.054) | (0.182) | (0.085) | (0.087) | (0.145) |
| | | [0.068*] | | | [0.068*] | | | [0.070*] | |
| Treated Household | -0.083 | -0.103 | -0.327* | -0.154 | -0.181 | -0.032 | 0.105 | 0.120 | 0.483*** |
| | (0.087) | (0.095) | (0.171) | (0.218) | (0.219) | (0.169) | (0.076) | (0.091) | (0.139) |
| Age | 0.010 | 0.013 | 0.017 | -0.041* | -0.037* | -0.040*** | -0.074*** | -0.071*** | -0.078*** |
| | (0.008) | (0.008) | (0.013) | (0.016) | (0.015) | (0.012) | (0.009) | (0.007) | (0.011) |
| Male | -0.171* | -0.169* | -0.165** | -0.299** | -0.300** | -0.303*** | 0.127 | 0.109 | 0.102 |
| | (0.062) | (0.059) | (0.082) | (0.083) | (0.089) | (0.081) | (0.069) | (0.067) | (0.063) |
| OLS | Yes | Yes | No | Yes | Yes | No | Yes | Yes | No |
| Village FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| IV | No | No | Yes | No | No | Yes | No | No | Yes |
| Level at which standard errors are clustered | Village | Village | Individual | Village | Village | Individual | Village | Village | Individual |
| Observations | 526 | 526 | 526 | 526 | 526 | 526 | 526 | 526 | 526 |
| Adjusted $R^2$ | 0.015 | 0.016 | -0.003 | 0.057 | 0.054 | 0.054 | 0.156 | 0.162 | 0.134 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses. Wild bootstrap p-values for the "Sponsored" variable shown in brackets. Dummies for age at program rollout used as an instrument for sponsorship. F-statistic for first-stage estimation of IV estimates: 38.23

# APPENDIX

Table A1. OLS Estimations for Indonesia – Family has 1 or 2 children

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | -0.078 | 0.066 | -0.146 | -0.165 | 0.734 | -0.060 |
| | (0.314) | (0.121) | (0.169) | (0.091) | (0.515) | (0.158) |
| Treated Household | 0.217 | 0.046 | 0.020 | 0.133 | 0.302 | 0.249 |
| | (0.185) | (0.144) | (0.118) | (0.091) | (0.623) | (0.179) |
| Age | -0.006 | 0.118*** | -0.011 | -0.008 | 0.136 | 0.071** |
| | (0.016) | (0.008) | (0.019) | (0.019) | (0.114) | (0.016) |
| Male | 0.135 | 0.250** | -0.591*** | -0.415** | -0.240 | -0.408** |
| | (0.151) | (0.061) | (0.073) | (0.089) | (0.408) | (0.077) |
| Birth Order | -0.037 | 0.067 | 0.118 | -0.144 | -1.099 | -0.074 |
| | (0.128) | (0.288) | (0.226) | (0.141) | (0.905) | (0.169) |
| Observations | 95 | 95 | 98 | 106 | 107 | 95 |
| Adjusted $R^2$ | -0.033 | 0.181 | 0.296 | 0.184 | 0.040 | 0.146 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors in parentheses, controls for village fixed effects

Table A2. IV Estimations for Indonesia – Family has 1 or 2 children

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | -0.121 | 0.044 | -0.174 | -0.282 | 1.790* | 0.202 |
| | (0.245) | (0.223) | (0.234) | (0.236) | (1.059) | (0.312) |
| Treated Household | 0.254 | 0.065 | 0.099 | 0.221 | -0.821 | 0.028 |
| | (0.242) | (0.248) | (0.226) | (0.225) | (1.050) | (0.299) |
| Age | -0.007 | 0.117*** | -0.020 | -0.015 | 0.407*** | 0.079*** |
| | (0.020) | (0.021) | (0.018) | (0.019) | (0.113) | (0.029) |
| Male | 0.133 | 0.249** | -0.576*** | -0.403*** | -0.466 | -0.395*** |
| | (0.101) | (0.125) | (0.096) | (0.098) | (0.462) | (0.129) |
| Birth Order | -0.038 | 0.066 | 0.091 | -0.140 | -1.383 | -0.067 |
| | (0.155) | (0.244) | (0.208) | (0.129) | (0.841) | (0.267) |
| Observations | 95 | 95 | 86 | 94 | 95 | 95 |
| Adjusted $R^2$ | -0.034 | 0.181 | 0.302 | 0.137 | 0.151 | 0.130 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors in parentheses, controls for village fixed effects. Dummies for age at program rollout used as an instrument for sponsorship. F-statistic for first-stage estimation: 366.07

**Table A3. Alternative IV Estimations for Indonesia – Children two and younger at program introduction grouped together**

| VARIABLES | (1) Self Esteem Index | (2) Optimism Index | (3) Hope for White Collar Job | (4) Expect White Collar Job | (5) Years of Education Expected | (6) Aspirations Index |
|---|---|---|---|---|---|---|
| Sponsored | -0.135 | 0.076 | 0.061 | -0.110 | 0.371 | 0.056 |
| | (0.137) | (0.163) | (0.112) | (0.110) | (0.435) | (0.148) |
| Treated Household | 0.142 | 0.039 | -0.143 | -0.012 | 0.190 | 0.011 |
| | (0.130) | (0.157) | (0.099) | (0.099) | (0.452) | (0.140) |
| Age | 0.020** | 0.054*** | 0.001 | 0.004 | 0.082** | 0.029*** |
| | (0.010) | (0.012) | (0.008) | (0.008) | (0.035) | (0.010) |
| Male | 0.011 | 0.139** | -0.573*** | -0.442*** | -0.122 | -0.351*** |
| | (0.049) | (0.062) | (0.040) | (0.042) | (0.190) | (0.056) |
| Birth Order | 0.017 | -0.040 | 0.020 | 0.024 | -0.052 | -0.050 |
| | (0.061) | (0.057) | (0.050) | (0.054) | (0.250) | (0.070) |
| Family Size | -0.008 | 0.094* | -0.017 | -0.032 | -0.019 | 0.031 |
| | (0.061) | (0.057) | (0.052) | (0.057) | (0.260) | (0.074) |
| Observations | 468 | 468 | 422 | 459 | 468 | 468 |
| Adjusted $R^2$ | -0.002 | 0.058 | 0.332 | 0.216 | 0.032 | 0.074 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, Robust standard errors in parentheses, controls for village fixed effects. Dummies for age at program rollout used as an instrument for sponsorship. F-statistic for first-stage estimation: 101.73

Table A4. Estimations for Drawings – Family has 1 or 2 Children

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | Happiness | | | Self-Efficacy | | | Hopelessness | |
| | (1', no fe) | (1', fe) | (3', fe) | (1', no fe) | (1', fe) | (3', fe) | (1', no fe) | (1', fe) | (3', fe) |
| Sponsored | 0.163 | 0.198 | -0.075 | 0.284 | 0.287 | 0.183 | -0.650*** | -0.761* | -1.067*** |
| | (0.226) | (0.217) | (0.268) | (0.248) | (0.127) | (0.364) | (0.209) | (0.289) | (0.318) |
| Treated Household | 0.170 | 0.255 | 0.468 | -0.142 | -0.146 | -0.065 | 0.404 | 0.442 | 0.680** |
| | (0.277) | (0.111) | (0.304) | (0.299) | (0.270) | (0.370) | (0.261) | (0.310) | (0.323) |
| Age | 0.050** | 0.053 | 0.048* | -0.026 | -0.026 | -0.028 | -0.102*** | -0.105** | -0.111*** |
| | (0.024) | (0.027) | (0.025) | (0.023) | (0.031) | (0.025) | (0.027) | (0.031) | (0.028) |
| Male | -0.295* | -0.319 | -0.345** | -0.255 | -0.247 | -0.257 | 0.112 | 0.035 | 0.006 |
| | (0.172) | (0.151) | (0.171) | (0.179) | (0.169) | (0.188) | (0.146) | (0.137) | (0.153) |
| OLS | Yes | Yes | No | Yes | Yes | No | Yes | Yes | No |
| Village FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| IV | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 108 | 108 | 108 | 108 | 108 | 108 | 108 | 108 | 108 |
| Adjusted $R^2$ | 0.071 | 0.120 | 0.160 | 0.015 | 0.013 | -0.016 | 0.215 | 0.254 | 0.248 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, robust standard errors in parentheses, dummies for age at program rollout used as an instrument for sponsorship. F-statistic for first-stage estimation: 10.09

Table A5. Alternative Drawing Analysis Summary Statistics

| | Mean, All (std. dev.) | Mean, Sponsored (std. dev.) | Mean, Non-Sponsored (std. dev.) | Difference *t*-test (std. error) |
|---|---|---|---|---|
| Unhappiness Factor | -0.000 | -0.091 | 0.110 | -0.200* |
| | (0.920) | (0.900) | (0.933) | (0.080) |
| Self-Efficacy Factor | -0.000 | 0.099 | -0.120 | 0.220** |
| | (0.955) | (0.952) | (0.947) | (0.083) |
| Hopelessness Factor | 0.000 | -0.105 | 0.127 | -0.232*** |
| | (0.754) | (0.660) | (0.838) | (0.065) |

Full sample = 526: 288 sponsored, 79 waitlist, 112 sibling of sponsored, 47 sibling of waitlist, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A6. Alternative Rotated Factor Loadings

| | Unhappiness | Self-Efficacy | Hopelessness | Uniqueness |
|---|---|---|---|---|
| Hopefulness Question 1 | 0.019 | 0.009 | -0.301 | 0.909 |
| Hopefulness Question 2 | 0.014 | -0.063 | -0.245 | 0.936 |
| Hopefulness Question 3 | -0.020 | -0.038 | 0.049 | 0.996 |
| Huge Figure | -0.008 | -0.043 | 0.006 | 0.998 |
| Monster | 0.066 | -0.044 | 0.127 | 0.978 |
| Shading | 0.030 | 0.166 | -0.147 | 0.950 |
| Missing Mouth or Nose | 0.354 | 0.100 | 0.423 | 0.686 |
| Frowning or Crying | 0.705 | -0.115 | -0.207 | 0.447 |
| Dark Colors | 0.052 | -0.931 | -0.053 | 0.128 |
| Single Color | 0.037 | -0.383 | 0.117 | 0.838 |
| Weather | -0.030 | 0.133 | 0.177 | 0.950 |
| Smiling | -0.882 | 0.018 | -0.217 | 0.175 |
| Cheery Colors | -0.078 | 0.926 | -0.022 | 0.136 |
| Tiny Figure | 0.147 | -0.016 | 0.033 | 0.977 |
| Missing Arms or Hands | 0.242 | 0.025 | 0.238 | 0.884 |
| Missing Legs | 0.145 | 0.032 | 0.445 | 0.780 |
| Body Language | -0.722 | 0.164 | 0.110 | 0.440 |
| Self Esteem Question 1 | 0.004 | 0.043 | 0.252 | 0.935 |
| Self Esteem Question 2 | 0.016 | 0.038 | 0.249 | 0.936 |

Table A7. Alternative Estimations for Drawings

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | Unhappiness | | | Self-Efficacy | | | Hopelessness | |
| | (1', no fe) | (1', fe) | (3', fe) | (1', no fe) | (1', fe) | (3', fe) | (1', no fe) | (1', fe) | (3', fe) |
| Sponsored | -0.233** | -0.241** | -0.501*** | 0.322*** | 0.334** | 0.165 | -0.248*** | -0.296*** | -0.867*** |
| | (0.106) | (0.052) | (0.192) | (0.101) | (0.060) | (0.179) | (0.076) | (0.027) | (0.161) |
| Treated Household | 0.076 | 0.095 | 0.289* | -0.164 | -0.191 | -0.065 | 0.103 | 0.108 | 0.534*** |
| | (0.129) | (0.091) | (0.170) | (0.124) | (0.220) | (0.167) | (0.101) | (0.093) | (0.144) |
| Age | -0.008 | -0.011 | -0.014 | -0.040*** | -0.036* | -0.038*** | -0.065*** | -0.061** | -0.069*** |
| | (0.013) | (0.007) | (0.013) | (0.012) | (0.015) | (0.012) | (0.011) | (0.015) | (0.012) |
| Male | 0.166** | 0.165* | 0.162** | -0.306*** | -0.305** | -0.307*** | 0.129** | 0.109 | 0.102 |
| | (0.080) | (0.067) | (0.081) | (0.081) | (0.095) | (0.081) | (0.063) | (0.080) | (0.064) |
| OLS | Yes | Yes | No | Yes | Yes | No | Yes | Yes | No |
| Village FE | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| IV | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 526 | 526 | 526 | 526 | 526 | 526 | 526 | 526 | 526 |
| Adjusted $R^2$ | 0.014 | 0.014 | 0.000 | 0.057 | 0.055 | 0.055 | 0.108 | 0.110 | 0.069 |

*** $p<0.01$, ** $p<0.05$, * $p<0.1$, robust standard errors in parentheses, dummies for age at program rollout used as an instrument for sponsorship. F-statistic for first-stage estimation: 38.23

# Land Reform and Sex Selection in China*

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**Abstract**

Following the death of Mao in 1976, abandonment of collective farming lifted millions from poverty and heralded sweeping pro-market policies. How did China's excess in male births respond to rural land reform? In newly-available data from over 1,000 counties, a second child following a daughter was 5.5 percent more likely to be a boy after land reform, doubling the prevailing rate of sex selection. Mothers with higher levels of education were substantially more likely to select sons than were less educated mothers. The One Child Policy was implemented over the same time period and is frequently blamed for increased sex ratios during the early 1980s. Our results point to China's watershed economic liberalization as a more likely culprit.

Keywords: Land Reform, Sex Selection, One Child Policy

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# 1 Introduction

Economic development has helped narrow key gender gaps over the past quarter century, including those in educational attainment, life expectancy, and labor force participation [World Development Report 2012]. On the other hand, perhaps the starkest manifestation of gender inequality – the "missing women" phenomenon – can persist with development, particularly if development reduces the cost of sex selection [Duflo, 2012]. Figure 1 shows the case in China. Despite the rapid growth of GDP per capita since 1980, the sex ratio at birth has increased from 1.06 in 1979 to 1.20 in 2000. In 2010, the sex ratio at birth remains 1.19, or about 500,000 more male births per year than the biological norm of around 1.05 per female.

In this paper, we reevaluate two prevalent beliefs about sex selection. First, China's One Child Policy (OCP) is routinely blamed for increased sex ratios. By reducing the number of random draws of child sex, the chance that parents obtain a son naturally is lowered, who then turn to sex selection, e.g. Ebenstein [2010]. In the current debate about relaxing or eliminating the OCP, its role in "missing girls" is frequently invoked [CNN, July 2012; NPR, April 2013; *New York Times*, May 2013].[1] While intuitive, this argument ignores the historic decline in fertility just prior to the OCP's introduction in 1979; fertility rates were comparatively steady from 1978-84. We explore whether the OCP's purported effect on "missing girls" is confounded by land reform, as both reforms proliferated 1978-84 in rural China. Second, OCP aside, previous findings on the perverse effect of development have usually focussed on particular factors that reduce the cost of sex selection (e.g. prenatal ultrasound). In this respect, increases in sex selection with "development" are not altogether surprising. By contrast, non-cost dimensions of economic development are generally thought to reduce sex selection, e.g. Jensen and Oster [2009]. Here, we consider a fundamental economic liberalization: how did the "world's largest anti-poverty program" [McMillan, 2002] affect de-selection of girls?

To evaluate these questions, we analyze new data on the rollout of the 1978-84 land reform in China to over 1,000 counties; previous work has focused on variation across 28 Chinese provinces [Lin, 1992]. The "Household Responsibility System" unraveled collectivized agriculture and marked a critical first step toward a market-oriented Chinese economy. While land user-ship rights were shifted from the collective to individual households, land ownership remained with the collective. Land was contracted to households for 3-5 years. Individual households could make their own input decisions and receive all income from the land after meeting the tax and quota sales obligations [Perkins, 1988]. The remarkable growth in agricultural output spurred by the reform has been well documented [McMillan et al., 1989; Lin, 1992]. Land reform is further recognized for its achievement in lifting *hundreds of millions* of rural households out of poverty [World Bank, 2000].

Using the 1990 population Census, we see a striking increase in the fraction male following land reform in families without a firstborn son (see event study in Figure 2B). Prior to land reform (year 0 and

[1] http://globalpublicsquare.blogs.cnn.com/2012/07/09/could-chinas-one-child-policy-change/
http://www.npr.org/2013/04/23/176326713/for-chinese-women-marriage-depends-on-right-bride-price
http://www.nytimes.com/2013/05/22/opinion/chinas-brutal-one-child-policy.html

before), we do not see trends in the sex ratio. Nor do we see substantial increases in sex ratios following land reform for the firstborn child (Figure 2A) or the second child if first child was male (Figure 2B, lower line). These raw patterns are replicated in a triple-difference regression framework.[2,3] Specifications that account for county-specific time trends and time-varying effects of county characteristics that primarily drive reform timing likewise deliver the same basic finding: following a first daughter, the second child is 5.5 percent more likely to be a boy following land reform. This translates into a 12 percent increase in the county-by-year sex ratio of the second child, or a doubling of the sex selection rate following a first-born daughter. Our results are also robust to including a full set of county-by-year interactions. Any potential confounder needs to mimic land reform rollout by county *and* differentially affect families with a first daughter.

As is well known, the OCP was introduced during the late 1970s and early 1980s, i.e., the same period as land reform. Although China's fertility rate fell dramatically during the 1970s, sex ratios did not increase (Figure 1). Once the OCP was introduced in 1979, fertility rates were comparatively flat (Appendix figures 1A & 1B), which limits the scope for OCP-regulated fertility to explain the aggregate sex ratio trends. Ebenstein [2010] found that higher sex ratios were associated with higher fines under the OCP at the province level (31 provinces). We collect the most comprehensive data on the initial introduction dates of the OCP at the county level between 1978 and 1985. We find that it was land reform, not the OCP, that increased sex ratios in the rural areas during the early 1980s (home to 86% of China's population at the time). The subsequent "1.5 Child" Policy arrived 3-5 years after the OCP (Figure 3a); controlling for it does not affect out impact estimates. Likewise, ultrasound diffusion would not confound the effect of land reform because it was unavailable in rural counties until the mid-1980s (Figure 4).

Fertility responses are of independent interest and might lead to endogenous sample selection and bias. We find a small positive response in the total number of births to land reform. However, on the margin that affects sample selection – the decision to have a second child and the birth interval between the first and second child – land reform had little effect. In contrast, we estimate a consistent, precisely-estimated, but *modest* fertility decline in response to our 1978-85 OCP county-level rollout measure, i.e. during the era of relatively stable national (and rural) fertility.

Finally, we consider economic and proximate mechanisms for the reduced form effect of land reform. Enhanced male productivity could spur sex selection, either through higher earnings of the father or so as to secure the future productivity increase of sons. Likewise, if sons received more land than daughters, this could induce sex selection. Our evidence is inconsistent with either a productivity or "direct remuneration" mechanism. Instead, it points to the best-established economic consequence of land reform in China: increased rural incomes. Just as children may be a normal good [Becker, 1960],

[2]We compare the sex of the second child born before and after the reform between families with a first girl and those with a first boy, using families with a first boy as our control group based on a previously-documented demographic regularity: the sex ratio of the first child is biologically normal, but it becomes abnormally male-biased at higher birth orders, especially among families with no previous son [Zeng et al. 1993].

[3]Standard errors are clustered at the county level.

so too may having a son. In consumer theory, goods with few close substitutes tend to be normal (e.g. Black et al. [forthcoming]). To the extent that a daughter is perceived as a poor substitute for a son, having a son may be normal. Consistent with the income mechanism, we find that the sex selection response was highly concentrated in: i) counties that experienced larger income gains from the reform, and; ii) families with more education. 53% of mothers who sex selected in response to land reform (the "compliers") had at least a high school education, despite making up just 4% of mothers having a second child.[4]

Turning to proximate mechanisms, parents might prefer to conceal sex selection behaviors, and as such detecting them an exercise in "forensic economics" [Zitzeqitz, 2012]. Some rural parents may have determined sex prenatally by traveling to provincial capitals, where ultrasound technology was introduced in the mid-1970s.[5] We estimate that ultrasound access in provincial capitals and excess female mortality after birth accounted for roughly half of the sex ratio increase that followed land reform.

The remainder of the paper is organized as follows. We summarize the background of land reform and the One Child Policy in Section 2 and preferences over the the sex composition of children in Section 3. The identification strategy follows in Section 4 and data in Section 5. Our main results are presented in Section 6. Section 7 considers economic mechanisms (why sex selection responded) and Section 8 proximate mechanisms (how). Section 9 concludes.

# 2 Background

## 2.1 The post-Mao land reform

Under collectivization implemented during the 1950s, workers received daily fixed work points and were paid at the end of the agricultural year [Lin, 1988]. The incentive to work was low and agricultural productivity was stagnant. From 1956 to 1977, there was virtually no change in grain output per capita [Zweig, 1987].

Following the death of Mao Zedong and the end of the Cultural Revolution, a small number of production teams in Anhui Province experimented with contracting land and assigning output quotas to individual households in late 1978 [Lin, 1987; Yang, 1996]. As the movement spread, communes were dismantled and the farm fields were contracted to households for individual cultivation for 3-5 years during 1978-83 (the lease was extended to 15 years nationally in 1984).[6] The land has continued to be owned by the collective. But the basic decision-making unit was shifted from the collective farm to individual households, who could make their own input decisions and receive all the residual income

[4]See Section 4.4.4 of Angrist & Pischke [2009] on estimating average complier characteristics.

[5]Using data on ultrasound machine diffusion by county from Chen, Li, and Meng [2013] and 1980 rail network data provided by Matthew Turner, we find larger increases in sex ratios in rural counties with railroad connections to provincial capitals, where ultrasound machines were available at the time of land reform (see Section 8.1).

[6]It was further extended to 30 years in 1993.

from the land after meeting the tax and quota sales obligations to the state [Perkins, 1988; Sicular, 1991]. Individuals of a former production team were entitled to use of an equal share of the land on a *per capita* basis [Kung and Liu, 1997]. A household received an additional plot for a newborn and lost one when a member passed away [Oi, 1999].

The initial response of the Central Committee of the Chinese Communist Party (CCP) to the new Household Responsibility System (HRS) was unfavorable. "Regulations on the Management of Rural People's Commune" passed by the CCP in the November of 1978 clearly stipulated that contracting to individual households was not permitted. But increased agricultural output quickly softened official resistance. The Party's prohibition was relaxed in September 1979 by allowing exceptions to households living in areas that were peripheral, distant, mountainous, and isolated due to transportation difficulties.[7] In September 1980, Central Document No.75 issued by the Central Committee further allowed poor and remote areas and production units heavily dependent on state subsidies to contract land and output quotas to households. By August 1981, the Central Committee's position on household farming was liberalized in a mission statement sent to fifteen provinces: "contracting to households is not only a means of relieving poverty but also a way of enhancing productivity; and it hasn't changed the production relations of the collective economy".[8] In January 1982, Central Document No.1 officially announced that "the HRS is the production responsibility system of the socialist economy", which first showed the CCP's willingness to popularize the HRS.

## 2.2 Variation in the county-level reform timing

The rapid rollout of the HRS is shown by the solid line in Figure 3A (See Section 5.1 for data description), which shows the fraction of counties that had introduced the HRS. Under two percent of counties pioneered reform in 1978. The vast majority reformed between 1979 and 1981, with the peak of 45 percent adopting in 1980. By 1984, all counties had adopted the HRS.

Before considering the effect of land reform, we explore what drove reform timing. The institutional history suggests two primary drivers: drought and poverty prior to reform. A severe drought led to large declines in agricultural production, which in turn provided the local government incentive to reform.[9] The negative production shock changed the cost-benefit calculation such that political risk-taking became more worthwhile: contracting land to individual households was not officially permitted in earlier years. Poor and remote counties were among the first permitted to adopt the HRS by the central government as a means to reduce national poverty rates.

The existing literature on HRS adoption at the province level provides three additional insights [Lin, 1987; Yang, 1996; Chung, 2000]. First, the diffusion of HRS was faster where reduction in monitoring

[7] *Agriculture Yearbook of China 1980*, 1981, Beijing, Agricultural Press.

[8] People's Daily, August 4th, 1981.

[9] Bai and Kung [2011] provide indirect evidence using province level data. They find that provinces that suffered more in the 1959-61 Famine started land reform earlier when struck by bad weather. The interpretation is that the Famine undermined local beliefs that collective farming could effectively cope with negative weather shocks.

cost was higher and thus productivity gains larger. Using size of production team to measure monitoring cost, previous studies show mixed results.[10] The second hypothesis is that provinces that suffered more from the 1959-61 Famine reformed earlier because they were more disenchanted with collective farming [Yang 1996; Bai and Kung 2011]. Lastly, Yang [1996] argues that provinces further from Beijing had more freedom to initiate reform earlier.

We first test the correlation between reform timing and its potential time-invariant determinants (measured prior to the reform). At the county level, poverty is captured by grain output per capita in 1977 that are collected from county gazetteers. Remoteness is measured by distance to provincial capital using a GIS map of the 1982 Census. Size of production team is proxied by the density of the labor force (aged 16-60) in 1977.[11] Famine intensity is measured by the average birth cohort size in 1953-1957 divided by the average cohort size in 1959-1961 using the 1982 Census.[12] We also calculate the distance to Beijing to proxy for discretion in local policy-making. Table 1A shows that counties that were initially poor, had larger production teams in 1977 and higher famine intensity in 1959-1961, and were located further from the central government adopted reform earlier, consistent with previous studies using provincial variation. The correlation between reform timing and the baseline sex ratio at birth in 1975-77 (from 1982 Census) is not statistically significant. This suggests that the underlying tendency to sex select (and its predictors) at the county level are uncorrelated with land reform timing. In the multivariate regression, controlling for grain output per capita in 1977 forces us to drop two thirds of the sample due to lack of data (we still have an order of magnitude more sample than previous studies). We omit grain output in the last column of Table 1A and find robust results for labor force density and famine intensity. The final note is on explanatory power. The $R^2$ is 0.095 when all initial controls are included. In a simple test on how much county fixed effects alone predict reform timing, we find that the increase in $R^2$ by adding county FE is very close to 0.095, suggesting our time-invariant observables may indeed capture the static predictors of reform timing.

Next, we test whether drought led to land reform by matching the county-level data on reform timing with county-by-year data on precipitation.[13] Land reform is an irreversible event, implying that drought prior to reform might affect the decision to reform, but drought after would not. Thus, we assign zero before reform, one to the first year of reform, and missing values after. In addition, the Chinese Academy of Agricultural Sciences [1984] suggests that the growth of rice, the No.1 grain in China by output, largely depends on rainfall at the beginning of the growing season, usually in March or April. In Table 1B, column 1 shows no correlation between the first year of reform and drought defined by average monthly precipitation in the whole growing season (March to September) in the reform year and the year preceding.[14] From columns 2 to 5, we measure drought by monthly precipitation from

[10] Lin [1987] finds that provinces with larger production teams reformed earlier, while Chung [2000] has the opposite finding.

[11] Density is calculated by population size aged 16-60 years in 1977 divided by area at the county level using 1982 Census.

[12] Meng et al. [2009] use a similar measure of famine intensity using the 1990 Census. See also Dyson (1991) on fertility response as a famine metric in South Asia.

[13] See Data Appendix.

[14] The month of reform is not recorded consistently. In data on reform year, a drought in the growing season is likely to

March to June separately. As expected, droughts in March and April of the reform year and one year prior have a strong and precisely estimated effect of hastening reform.

In all regressions on the effect of land reform, we control for the time-varying droughts in March and April in the current and prior year, as well as time-invariant determinants of reform timing by county interacted with time fixed effects (see equation (1) in section 4.1). This allows for characteristics that are correlated with reform timing to have their own idiosyncratic time effects [Acemoglu, Autor, & Lyle, 2004], e.g. factors correlated with distance from Beijing.

## 2.3 Land reform and grain output

Land reform rewarded individual effort more than collective farming. McMillan et al. [1989] used national, time-series data and suggest that over three-quarters of the productivity increase 1978-84 could be attributed to the incentive effects of the HRS. Using the reform rollout by province, Lin [1992] has a similar finding that the reform accounts for half of the output growth. Official statistics show that the rural poverty rate declined from 30 percent in 1978 to 5 percent in 1998 [World Bank, 2000].

Unfortunately, we do not observe household income in the Census microdata, nor is income data available from other sources for this period. Nevertheless, we provide the first quantitative evidence on the output gain from the 1978-84 land reform at the county level. We use grain production by county and year from the 1970s to the mid-1980s that we entered from hard-copy county gazetteers. Records on grain output in the 1970s are particularly scarce because in general county-level statistics have only been released systematically since the 1980s in China. These data are also arguably reliable because they were originally from local official archives (Xue, 2010).[15] There are 400 counties that report both the reform timing and the complete year-by-year grain production from 1974 to 1984. Data on other crops, especially cash crops, are rarely reported in the county gazetteers, nor are they available from any other data sources for the 1970s. Therefore, our analysis below presumably yields a conservative estimate of the overall output gain.

We plot grain output per capita by year relative to land reform in Appendix Figure 2. Time 0 indicates the first year of reform. The trend prior to land reform is relatively flat, consistent with the literature that agricultural productivity growth under the collectivized system was sluggish. There is a jump of grain output one year after the first reform year, suggesting that the first impacted harvest was one year after the reform. Additional detail on magnitudes is provided below (Section 7.1).

## 2.4 One Child Policy and subsequent "1.5 Child" Policy

### One Child Policy

affect reform at the second half of the current year or in the next year.

[15] Because the purpose of compiling county gazetteers is to accurately record local history rather than to report to the upper level government, local historians in the county gazetteer office have relatively little incentive to manipulate the grain output data.

The One Child Policy (OCP) was introduced over the same period as land reform. Prior to the OCP, the government had started a series of birth-planning propaganda campaigns in 1971 (Scharping, 2003). These campaigns focused on promoting "later, longer, and fewer", which referred to later marriage (minimum marriage age was 23 for women and 25 for men in rural areas), longer birth spacing (three to four years) and fewer children. A two-child norm was widely promoted. A popular slogan was: "One isn't too few, two are just fine, three are too much". During the Cultural Revolution, the government relied on ideological education and campaigns, which coincided with a large drop in average fertility. The total fertility rate decreased from almost 6 in 1970 to a little less than 3 in 1979, a nearly 50% decline (See Appendix Figure 1A from Cai (2008)). When economic reform started in 1978, the government set a population target of 1.2 billion in 2000 to maintain desired economic growth rates. Scientists hired by the government agued successfully that the population target could not be achieved under a two-child policy (Scharping, 2003).

In January of 1979, the OCP was officially announced. Departing from the propaganda campaign of the 1970s, the 1979 policy introduced a new system of financial incentives for birth control. The initial policy permitted one child in urban areas (home to approximately 14% of the Chinese population). Urban parents who gave birth to two children would suffer economic sanctions. Rural parents who had a third child were punished [Banister, 1987]. But introduction of the OCP between 1979 and 1982 did set explicit incentives for the second child in the rural areas. From our county-level OCP rollout data (see Section 5.1), 56% of counties introduced the OCP in 1979, and 97% had OCP by 1982.

Fertility was higher following the OCP's introduction than commonly believed. Nationally, the post-1979 total fertility rate (TFR) was fairly stable around 2.5 children per woman until 1988 (Appendix Figure 1A). We separate rural from urban TFR trends using the 10% sample of the 1988 national two-per-thousand Population Sampling Survey on Fertility and Contraceptives (Appendix Figure 1B). The rural TFR fell by nearly half from 1970 to 1977, and it "bottomed out" around 3 children, where it remained until 1986, the year the youngest cohort in our analysis sample were born. These trends are noteworthy given a common belief that the OCP had led to a large fertility decline in the 1980s (compared to fertility in the 1970s). Furthermore, fertility in rural areas remained steady and well *above* replacement levels during the HRS and OCP rollout period.

**"1.5 Child" Policy**

In 1984, the stated OCP was relaxed by national "Document 7" to allow second child permits to families with a first girl, the so-called "1.5 Child" Policy [Greenhalgh, 1986; Scharping, 2003].[16] Guangdong and Hainan are the only two provinces that started the 1.5 Child Policy prior to the national policy, in 1981-82 [Scharping, 2003]. By the time the 1.5 Child Policy was implemented in 1984, all counties had the HRS for at least one or two years (see Figure 3A). Our main potential confounder is thus the earlier One Child Policy. Indeed, when we control for the 1.5 Child Policy in Appendix Table 1, we find quite similar results for land reform.

[16]The stated policy was tightened to allow only a few types of rural families to have the second child in 1982, but we do not see any county governments revising their policies on this margin 1982-1984 in the county gazetteers.

# 3 Preferences for sex composition of children

Son preference in China has been well documented. Below, we cite three lines of evidence suggesting that if there are two children, a sex mix is most preferred, followed by two sons. Two girls are least preferred.

First, interviews conducted by demographers suggest that for rural parents, the vast majority report preferring two children if there were no fertility restriction, with "one son, one daughter" (Chu, 2001; Greenhalgh et al. 1994). Moreover, most rural women think that "having two sons is not perfect but acceptable". In Chu (2001)'s interviews, "rural women whose first child is a son usually take no measure to guarantee the sex of the second one, while those with a first girl would take steps to ensure the second is a son". These studies suggest that 1) son preference is non-monotonic; 2) preference for diversity could lead to sex selection.

Second, we discuss reasons why parents might prefer a sex mixture to all sons. Suppose parents prefer and can have two children. First, raising a son is more costly than raising a daughter, especially when it comes to marriage. In rural China, parents have to prepare a house and wedding for their son's marriage, while marrying a daughter may cost parents nothing (Chu, 2001). Second, there is disutility of having more than one son. While parents of one son can anticipate to live with him, two sons bring friction and uncertainty on whom to rely in their old age (Greenhalgh et al. 1994). Moreover, two sons might fight for splitting family wealth when they get married. Third, it may be the case that a daughter is beneficial in raising a son (Chen, Ebenstein, Edlund, Li, 2012).

Third, we consider the sex of children in the 1990 Census microdata. Following a first son, girls are actually slightly more common than biologically normal: Figure 2B shows that the sex ratio of the second children is consistently below the 1.05 norm when first child is son, a feature previously noted by Chen, Ebenstein, Edlund, Li (2012). That said, the pro-son bias after a daughter is stronger than the pro-daughter bias after a son. Nevertheless, a mixture seems preferred to two boys.

If sex mix is most preferred, the cheapest way to attain that *ex ante* is to not sex select with the first child, and sex select as necessary for the second child. And indeed, sex ratios are normal for the first child. Were one to sex select on the first child, one still bears a roughly even chance of having to sex select again with the second child to achieve a mix. This suggests that although childbearing and sex selection is a sequential "game", the action is hypothesized to be on the second child. This assumes that the decision to have the second child is unaffected by land reform, which we also provide evidence for below.

# 4 Empirical Strategy

## 4.1 Econometric Specification

We use the arrival of land reform by county as a natural experiment. We start the analysis with basic comparisons of sex ratios before and after the reform (i.e. without regression adjustment) in event study figures.

To estimate the effect of exposure to land reform on the probability of second child being male, our main estimation framework is a triple-difference. The first double differences are among birth cohorts born before and after the reform and between counties that reformed earlier and those that reformed later. The third difference is between families with a first girl and those with a first boy, specifically:

$$Boy^2_{ijt} = \alpha + \beta_1 Reform_{jt} + \beta_2 Girl^1_{ijt} + \beta_3 Reform_{jt} * Girl^1_{ijt} + \gamma_j + \delta_t + \phi_j * t + D'_{jt}\theta_t + D'_{jt-1}\lambda_{t-1} + \sum_{t=1975}^{1986} (X'_j * T_t)\rho_t + \varepsilon_{ijt} \quad (1)$$

where the subscript $i$ denotes the individual, $j$ the county of birth, and $t$ the year of birth. The superscript denotes birth order: 1 for the first child, and 2 the second. The dependent variable, $Boy_{ijt}$, is a binary outcome that is equal to 1 if the second child is a boy and 0 otherwise. The land reform indicator $Reform_{jt}$ is equal to 1 if the child was born one year after reform and 0 otherwise, which is determined by one's year of birth and county of birth. $Girl^1_{ijt}$ is an indicator that is equal to 1 if the first child is a girl and 0 otherwise. We interact the reform indicator with sex of the first child to get the key regressor, $Reform_{jt} * Girl^1_{ijt}$. The coefficient of interest is $\beta_3$. Standard errors are clustered at the county level.

To remove possible confounding differences among birth cohorts and between reform starters and followers, a comprehensive set of controls are included in the estimation. County fixed effects $\gamma_j$ and year of birth effects $\delta_t$ absorb the effects of time invariant county characteristics and birth cohort effects. County specific linear trends, $\phi_j * t$, account for county characteristics that change smoothly over time and that are correlated with the reform timing. Furthermore, we account for time-varying effects of county characteristics that are found to drive the reform timing: droughts in March and April of the current year are denoted by $D'_{jt}$, and droughts of previous year are denoted by $D'_{jt-1}$. The time-invariant determinants of the reform timing, $X'_j$, including labor force density in 1977, famine intensity in 1959-61 and distance to Beijing, are interacted with time fixed effects from 1975 to 1986, with 1974 omitted.

A more demanding approach enabled by the "first daughter" experiment is to control for county-by-year fixed effects to absorb *all* time-varying county characteristics:

$$Boy^2_{ijt} = \alpha + \beta_2 Girl^1_{ijt} + \beta_3 Reform_{jt} * Girl^1_{ijt} + \gamma_{jt} + \epsilon_{ijt} \quad (2)$$

where $\gamma_{jt}$ denotes the county-by-year fixed effects. The coefficient $\beta_1$ of the reform indicator $Reform_{jt}$ is no longer identified. Comparing $\beta_3$ from estimating equation (1) and (2) helps to infer whether time-varying county features omitted in equation (1) would bias the impact estimate. We will see that estimates without regression adjustment are quite similar to regression-adjusted estimates from estimating either (1) or (2).

## 4.2 Identification

The coefficient of interest, $\beta_3$, measures the effect of land reform on whether the second child is male in families with a first girl relative to that in families with a first boy. Two identifying assumptions underpin this triple-difference strategy:

1. The second births in families with a first boy provide the appropriate counterfactual.

2. There are no unobserved changes coincident with land reform by county and year that have differential effects on the sex of the second child depending on the sex of the first child.

The validity of the first assumption requires that the sex of the first child is not endogenous to the reform and the absence of pre-existing trends in the sex ratio of the second child in families with a girl versus those with a first boy. As noted in the Introduction, Zeng et al. [1993] documented that the sex ratio of the first births is biologically normal. That is, we have an observable metric of the exogeneity of the first-born child's sex in it's proximity to normal sex ratio of 1.05 – we don't think first-born *sons* are selectively aborted, which could offset deselection of girls and thereby yield a normal sex ratio on net. To be cautious, we also directly test whether the reform affected the sex of the first child and fail to find an effect. We also provide transparent evidence that there are no pre-existing trends in the sex ratio of the second births.

Concurrent changes by county might call into question the second identifying assumption. To confound the effects of land reform, other reforms should both follow the timing of land reform adoption by county *and* have had differential impacts on the sex of the second child depending on the sex of the first one. We have conducted a comprehensive reading of reform policies from the late 1970s to the mid-1980s. Two historic reforms might at first appear to pose confounding threats. First, price reform and market reform (aspects of the broader rural economic reform) might also lead to a stronger desires for sons. However, these were introduced in the same year nationwide: the increases in procurement prices and in bonuses for above-quota production occurred in 1979 [Sicular, 1991]; reductions in the planning of agricultural production and in the restrictions on interregional trade were also universal state interventions [Lin, 1992]. The effect of these sweeping reforms are absorbed by year of birth effects $\delta_t$. Second, using the second child following a first boy as our control group, we can difference out any effect of reforms that arrived at the same time as land reform, but whose effect would not depend on the sex of the first child.

The initial introduction of the OCP in 1978-1984 stands out as the most likely confounder for our triple difference approach. Previous studies at the provincial level find that higher fines under the OCP led to higher sex ratios, especially at higher birth orders with no older brothers [Ebenstein, 2010]. For our purposes, it is the timing of OCP introduction by county in 1978-1984, the same period when HRS was introduced, that poses a threat to our identification of the land reform effect. We therefore have compiled the most detailed data on the timing of OCP implementation by county, i.e. finer geographic resolution than previous studies using policy variations at the provincial level, e.g. Ebenstein (2010). Using data on the county-level timing of both land reform and the OCP, we can disentangle which reform is the more important driving force in increased sex ratios in the 1980s.

Conceptually, one might be concerned about the gender-specific revision of the OCP to the 1.5 Child Policy: only parents who had a girl first were allowed to have a second child under the latter policy. However, the 1.5 Child Policy did not start nationally until 1984 (except for Guangdong and Hainan provinces), i.e. after introduction of the HRS in 1978-1984. Because the 1.5 Child Policy did not coincide with the introduction of HRS, it is unlikely to confound our analysis of the land reform effect (see Figure 3A and Appendix Table 1).

A final note is on the introduction of ultrasound machines which increased sex ratios, especially following a first girl [Chen, Li and Meng, 2013]. Ultrasound machines did not arrive in rural areas until the mid-1980s, i.e. after the rollout of land reform. As a result, the county-level rollout of ultrasound machines would not confound our findings on land reform, when these birth cohorts were around age 5. Nevertheless, earlier introduction of ultrasound technology in provincial capitals could help shed light on how parents sex selected. In Section 8.1, we further investigate the role of ultrasound machines in provincial capitals below using data from Chen, Li and Meng [2013].

# 5 Data

## 5.1 Local reforms and ultrasound access

Our main data source for the county-level rollout design is the post-1949 county gazetteers that document local events and statistics about geography, politics, the economy and culture from 1949 to the 1980s. We conducted a comprehensive survey of all county gazetteers that have been published to date, covering 1835 counties. We compiled and digitized data on the county-level rollout of land reform and the OCP from these hard-copy county gazetteers. These records are originally from official sources, e.g., historical archives and policy documents of county governments (Xue, 2010).

### Land reform rollout (county-level)

We identified information on the year the HRS was introduced by county for 1242 counties, representing two-thirds of all counties that have ever published gazetteers.[17] Specifically, we use the reported year when collectively owned land was first contracted to individual households in a few villages for each county; it usually took 2-3 years to spread the HRS to the whole county. Because land reform occurred in rural areas, our sample includes locations that were rural counties at the time of the reform.[18]

### One Child Policy rollout (county-level)

For the OCP, we compiled data on the year the county government issued the first policy document to enforce rewards for the single child and penalties for above-quota, third births. There are 990 counties that report the timing of both land reform and the OCP.

In Figure 3A, the short-dotted line shows the fraction of counties that had introduced the OCP between 1978 and 1986, while the solid line represents HRS timing, both scaled by the Y-axis on the left. Despite similar timing in 1978-1984 in aggregate, land reform and the OCP show substantial difference in the county-level timing between 1978 and 1982. The county-level difference is visible in Figure 3B, showing the distribution of the difference between land reform start year and the OCP start year. Land reform came earlier than the OCP in 27% of counties, 25% in the same year, and in 48% the OCP came earlier. The correlation between HRS timing and OCP timing at the county level is -0.005. By 1982 when the OCP supposedly became restrictive on the second child in the rural areas, 99% of counties had already introduced the HRS.

### 1.5 Child Policy rollout (province-level)

The 1.5 Child Policy was announced as a national policy in 1984. County-level information on the Policy was rarely recorded. Instead, we obtained the rollout timing by province from two sources: 1) the chapter on birth planning policies in provincial gazetteers; 2) Sharping (2003) chapter 6.4.

Five provinces (Xinjiang, Yunnan, Ningxia, Qinghai and Shanghai) did not implemented the 1.5 Child Policy in the 1980s.[19] We plot the provincial rollout among the other 24 provinces in 1978-1986 with the long-dotted line in Figure 3A, scaled by the Y-axis on the right. By 1981 when Guangdong province started the 1.5 Child Policy, more than 90% of counties had completed land reform. By 1984 when the 1.5 Child Policy started to spread nationwide, all counties had already had the HRS for at least one or two years. To confound our results, the 1.5 Child Policy have to have had to particularly affect sex selection among three and four year olds (see also Appendix Table 1).

[17] The other one-third of counties either do not report the timing of HRS adoption or report it as "the late 1970s" or "the early 1980s", i.e. too vague to implement our identification strategy.

[18] City districts are defined and excluded by using the county code in the 1982 Census and the official definition.

[19] In the 1980s, Xinjiang, Yunnan, Ningxia and Qinghai issued second child permits to the entire rural population, and Shanghai did not revise the OCP to the 1.5 Child Policy (Scharping, 2003).

**Ultrasound technology adoption (county and province level)**

Because ultrasound diffusion increased sex ratios in China (Chen, Li, and Meng, 2013), we might be concerned that land reform is capturing the effect of ultrasound. We match our data on HRS rollout with the rollout of ultrasound technology by county (provided by Chen, Li and Meng [2013]) and show this is not the case. In Figure 4, the short-dotted line shows the fraction of counties that introduced ultrasound machines between 1978 and 1990. As noted above, the vast majority of counties acquired ultrasound machines after 1984. By 1982 when HRS was introduced in more than 99% counties, only 4% had ultrasound machines. During the rollout of land reform, there was little change in the local cost of sex selection through the introduction of ultrasound machines.

Although ultrasound technology was unavailable in the rural areas during land reform, it was introduced in provincial capitals as early as the 1960s. The first ultrasound machine arrived in Xi'an in Shaanxi province in 1965. Other provincial capitals started to acquire their first machine since the mid-1970s, which made prenatal sex determination possible. In Figure 4, the long-dotted line shows the rollout of ultrasound machines in provincial capitals, mostly between 1978 and 1984.[20] So during the rollout of land reform, one option for pregnant women was to travel to the provincial capital to ascertain fetal sex. In Section 8, we examine further whether and to what extent sex selection induced by land reform seemed to operate through ultrasound access in provincial capitals.

## 5.2 Microdata

To consider sex ratios, we use the 1 percent sample of the 1990 Census microdata.[21] Our analysis focuses on rural areas which were defined as counties in the 1982 Census, the definition closest to the time of land reform. Census data in China do not report county of birth, which forces us to use county of residence in 1990 to match the Census data with the county-level data on reform timing. There are 1065 counties (58 percent of all) that are matched with data on reform timing and county controls. Concerns about endogenous migration are circumscribed because internal migration had been under strict control under *Hukou* system until after the land reform we consider was completed; the first *Hukou* relaxation was in 1985 [Wang, 2005]. (Migration rates are described further later in this subsection.)

Implementing our research design requires information on one's birth order and the sex of previous children, which are not explicitly queried in the Census data. We use information on the relationship to the household head to identify his/her children and order them using their month and year of birth. To verify this order is complete, we require that the number of children linked to the household head is equal to the number of surviving births reported by their mother.[22] Our analysis sample includes second births born 1974-86.

[20] Interestingly, the rollout of ultrasound machines in non-capital cities was later, i.e. similar to the rollout to rural counties.

[21] Available at: https://international.ipums.org/international/index.shtml

[22] In our sample of counties matched with the land reform data, 87% of mothers report the number of surviving births that is equal to the number of children linked in the census.

A natural concern about imposing the sample restriction is whether families with an older first child living outside the household in 1990 are excluded (by the restriction that the number of surviving children equal the number of observed children). The oldest second child in the sample was age 16 in 1990. Using the average birth interval of 3 years, the oldest first child would be around 19, who were usually too young to leave their parents' home. Nevertheless, we test how large the sample bias would be by comparing the birth year distribution of the first child (who are matched to our second child) in the 1990 Census and the 10% sample of the 1988 national two-per-thousand Population Sampling Survey on Fertility and Contraceptives, the latter of which does not suffer from a sample selection problem as it reports year of birth, birth order, and sex of every birth. If we have excluded a substantial number of families with an older first child away, we would expect more older cohorts (precisely, first births before 1974) in the 1988 Fertility Survey compared to that in the 1990 Census. In Appendix Figure 3, the birth year distributions of first children before 1974 in these two dataset are nearly identical, reducing concerns about sample selection.

We impose two additional sample restrictions. First, we exclude families with multiple births, where birth order is more difficult to identify and interpret. Second, for the sub-analysis by parental education, we consider only children in two-parent families.

A reason for excluding children born 1987 and later is to reduce the possibility of under-reporting. Parents may underreport above-quota births following the introduction of the One Child Policy. Based on follow-up surveys conducted right after the Census in 1990, the National Bureau of Statistics reports that the underreporting rate is 0.7%. The rate is very low, but it is more common that children aged 0-4 in the Census year are underreported (Zhang and Zhao, 2006). Therefore, we focus on children born prior to 1987.[23]

In our sample of births, one is defined as a migrant if he/she did not reside in the same county in 1985, which is reported in the Census. The migration rate among individuals born in 1974-84 is 0.63 percent. Throughout our analysis, we use the 99.37 percent born 1974-84 who resided in the same county in 1985 and all births (irrespective of relocation since 1985) in 1985-86.

Summary statistics of the full sample and the two-parent sample are reported in Table 2. Roughly half the child sample was "exposed" to land reform. About 10% of their parents completed high school, with substantially higher completion rates among fathers.

# 6 Main results

## 6.1 Land reform and sex ratios: event study figures

We begin by plotting the sex ratio of the first child by birth timing relative to the year of reform in Figure 2A (raw/unadjusted figure). The sex ratio is very stable at the biologically normal rate of 1.05

[23] We checked the robustness of our results by including children born 1987-1990. Results are very similar to those in our main sample.

before and after the reform, supporting our use of families with a first boy as the control group. Land reform did not precipitate more sex selection for the first child, which might have been expected if sons (plural) were strongly preferred and their cost alone was an overriding deterrent.

Figure 2B shows our primary result: sex ratios of the second child for families with a first girl before and after land reform. For comparison, we plot families with a first boy separately (neither line is regression adjusted). Among these comparison families, little change in the (second child) sex ratio is observed in the pre- and post-reform periods. More importantly, there are no pre-existing trends for either families with a first boy or those with a first girl. Among the pre-reform cohorts, the sex ratio of the second child in families with a first girl is persistently higher than that in families with a first boy. The steady 10 percentage points gap suggests son preference as a culture, that is, parents with no previous son manifest a stronger desire for a subsequent son (and have some means of achieving it). Starting from one year after the reform, the sex ratio in families with a first girl increases dramatically, from around 1.15 to the peak of 1.3 six years after the reform. The sharp contrast between these two groups in the pre- and post reform periods suggests that land reform is the driving force behind rising sex ratios.

## 6.2 Land reform and sex ratios: regression estimates

When we estimate equation (1), we find the same estimates as the raw data displayed in Figure 2A. In column 1 of Table 3, the estimate of land reform on the sex of first child is economically very small (a 0.6 percent increase relative to sample mean) and not statistically significant.

Column 2 presents the estimate for the effect of land reform on the second child being male, with the full set of control variables listed in equation (1). We find an increase in the probability of being male of 2.9 percentage points among families with a first girl relative to families with a first boy, statistically significant at the 1 percent level.[24] The effect is sizable in magnitude, around 5.5 percent relative to the sample mean for all second births. Land reform's effect is slightly larger than the baseline level of son preference, as captured by the effect of having a first girl, which is an increase of 2.7 percentage points. In column 3, we implement a more demanding comparison by controlling for county-by-year fixed effects, i.e. equation (2). Notably, we get exactly the same point estimate and standard errors for reform interacted with the first child being a girl. This suggests that none of the omitted time-varying county characteristics in equation (1) affect our estimate of interest. For all subsequent estimations below, we use the main approach in equation (1).

Han Chinese (90% of population) are known to have stronger and more consistent son preference than ethnic minorities. We would therefore expect sex ratio impacts to be concentrated among the Han. In column 4, we find a 3.3 percentage points increase in the probability of being male among Han families with a first girl relative to Han families with a first boy (using column 2 specification). This

[24] We also estimated the trend break model suggested by the change in slope in Figure 2B. The probability of being male increases by 0.5 percentage points per year after the reform. Over 6 years, the increase is 3 percentage points, consistent with our estimate of the shift in level captured by equations (1) and (2).

suggests a larger effect of land reform on sex ratios among Han Chinese.

To translate the effect of land reform on male births to the effect on sex ratios, we estimate equation (1) on the sex ratio of all second births aggregated by county and birth year. In column 5, the sex ratio in families with a first girl increases by 0.15 following the reform, a precisely estimated increase of 12 percent that matches the magnitude in the (unadjusted) Figure 2B.

Just as in the event study figures, we do *not* find an increase in second sons following land reform. This is consistent with the preference for sex mix described above. Instead, we do find a small decrease in second sons (-0.01, significant at 10% level) in column 2. While smaller and less robust than our primary result, how might this decrease in sons be achieved? We think it is unlikely that sons were selectively aborted. Nor is there any anecdotal evidence of selective abortion of males until a female was achieved. Furthermore, prior to land reform the children that followed sons were, if anything, disproportionately female (below the biological normal sex ratio of 1.05, see Figure 2B). Thus, there was little/no scope for reducing the selective abortion of females following sons in the wake of land reform. Instead, *adopting* a second daughter is more likely [Chen, Ebenstein, Edlund and Li, 2012]. First, since lineage is traced through males, adopting a daughter may be less aversive than adopting a son. Second, Chen, Ebenstein, Edlund and Li (2012) find that the number of adopted girls increased significantly since 1979, while the number of adopted boys remained nearly constant from the 1970s to the 1990s. Third, most of children adopted at parity two are girls in families with a previous boy.

Putting these pieces together (and still taking the -.010 coefficient in column 2 at face value), our findings have several implications. First, observing fewer two-son families after reform is consistent with the absence of sex selection for the first child. Second, the fear of having two girls is substantially larger than that of having two boys. Finally, the net increase in sons (through abortion of females or other non-adoption means) following a first girl would be 2 percentage points ($.029 - .010$). Two percentage points is also how much the fraction male increased in absolute terms following female and land reform: one third of the .029 DDD increase in sons following girls may have been “offset" by the increased supply of girls to other families. But this would not imply that the 2 percentage points absolute increase in fraction male following a girl as achieved by fully 1 percentage point of the *parents* giving up a daughter for adoption, as on average it will take giving up more than one second daughter for adoption to give birth to a second son (among parents not practicing sex-selective abortion). From the perspective of child welfare, we do not think that it is appropriate to conclude that the net effect of .02 captures land reform's effect on girls. After all, adopted girls are treated much more harshly on average than non-adopted girls (Chen, Ebenstein, Edlund and Li, 2012). Instead, we allow that some of land reform's effect of increased “sex mix" of children may have been achieved via both sex selective abortion and an expanded adoption market for girls.

## 6.3 The One Child Policy and sex ratios

We present three sets of results to distinguish the effect of land reform from that of the OCP and its later revision (the 1.5 Child Policy in the mid-1980s).

First, the data we digitized on the county-level rollout of land reform and the OCP permits a horse race between these two reforms. We focus on rural counties, home to 86% of China's population at the time, and we use the sample of 990 counties that report the timing of both land reform and the OCP.[25] We assign treatment status to the OCP as 1 for individuals born one year after the OCP or later and 0 otherwise. In Table 4A, the first three columns report the results using our main strategy in equation (1). Column 1 shows a similar estimate in this subsample as in column 1 of Table 3. In column 2, we find that the second birth in families with a first girl is 2.4 percentage more likely to be male after the introduction of OCP, which is precisely estimated. Thus, at first blush it appears that "phase 1" of the OCP increased sex ratios. This initial finding is consistent with the common argument that the OCP increased sex ratios (which has likewise not accounted for land reform). However, when we take the additional step of controlling for both land reform and the OCP in column 3, the estimate for land reform is robust while estimates for OCP become much smaller and statistically insignificant. Indeed, the point estimate on the OCP by first girl interaction term falls by an order of magnitude. In column 4-6, we repeat this horse race controlling for the full set of county-by-year fixed effects. Again, results are very robust indicating that it was land reform, not the OCP, that increased sex ratios in the 1980s.

The OCP applies to Han Chinese, not to ethnic minorities (see, e.g. Li, Yi, and Zhang, 2011). One might be concerned that columns 1-6 average over Han and (otherwise dissimilar) ethnic minorities. In column 7-9, we repeat the column 1-3 specifications in the subsample of Han Chinese. When both land reform and the OCP are included in column 9, a larger land reform effect is found among Han. Again, we fail to find an effect of the OCP on sex ratios among Han Chinese.

Second, we consider possible interactive effects between land reform and the OCP. In Figure 3B, land reform occurred prior to the arrival of the OCP in 27% of counties. In column 1 of Table 4B, we present the effect of land reform on second child being male in these counties where land reform came earlier than the OCP. The estimated effect of land reform is a 2.6 percentage points increase in the probability of being male. In column 2, we report the estimates in the other 73% of counties where OCP was already enforced when land reform occurred. The land reform effect is slightly larger, a 3.1 percentage points increase. To test whether the difference in these two samples is statistically significant, we interact Land reform*Girl first with the indicator of land reform coming after OCP in the whole sample in column 3. There is suggestive (but not overwhelming) evidence that more sex selection follows land reform if the OCP was in place.

Finally, we consider whether our land reform estimates are altered by allowing for the rollout of the 1.5 Child Policy by province. Appendix Table 1 reports the results. As one would expect, the gender-specific 1.5 Child Policy is indeed being captured: the probability of being male among second births

[25] Sex ratios in rural and urban areas were similar during the early 1980s and increased by comparable amounts 1978-84.

following a first girl increased. When land reform, the OCP, and the 1.5 Child Policy are all included, the estimated effect of land reform, (2.8 percentage points) is very similar to that in column 3 of Table 4A without controlling for the 1.5 Child Policy. Therefore, and as suggested by the timing shown in Figure 3A, the land reform effect on sex ratios does not appear confounded by the later revision of the OCP.

## 6.4 Fertility responses to land reform and the One Child Policy

Fertility responses are of independent interest, and could also complicate interpretation of the sex selection results. First, if land reform increases the desire to have more than one child, our sample of second births would be endogenously selected (see, e.g. McCrary and Royer, 2011). Another concern is about the timing of the second child. After the reform, parents might want to have the second child sooner in order to receive another plot of land earlier, which would generate selection on birth year.

We first test the effect of land reform on fertility. In Table 5A, the number of births by county and year increased by 2 percent due to land reform, while it is decreased by 2 percent by the OCP. We take the former as suggesting that having children is a normal good [Becker, 1960].[26] The effect of the OCP in reducing fertility is small, consistent with Appendix Figures 1A & 1B showing that the major national fertility decline occurred prior to the OCP. The small fertility effect of the OCP also helps to explain our null finding that the increased sex ratios were not caused by the initial introduction of the OCP.

On the margin of having a second child, it is not obvious *a priori* how land reform would affect the decision. Parents may desire more children to secure more land, but the rule of land distribution only applied for authorized births after the OCP was introduced. As a reward for compliance with the OCP, a single child received double plots of land, while as a punishment for non-compliance, above-quota births either did not receive land, or in some cases their parents' land allotment was revoked (various issues of county gazetteers). There are 73% of counties in our sample that introduced the OCP prior to or the same year as land reform, where land distribution favored the first (and single) child. To test whether land reform affected the decision to have a second child, we focus on couples during peak conception likelihood for a second child. We assign treatment status based on the year of birth of the first child and the average 3-year birth interval we find in the Census. We assume that two years after the first birth, parents made the decision whether to have a second. Suppose land reform came in year 0; the first group of parents whose decision was affected were those who had the first child in year -2. Thus, we assign 1 to the first child born 2 years prior to land reform or later and 0 otherwise.

Empirically, we find that the decision to have a second child is affected by the OCP but not land reform. In column (1)-(3) of Table 5B, controlling for the OCP, the effect of land reform on having a second child is very small and statistically insignificant, reducing concerns about endogenous sample selection. Moreover, if the "1.5 Child Policy" (which conditions on sex of first born) coincided with land

[26] See Section 7.

reform, we would have observed a larger likelihood in having the second following a first girl with land reform. Our finding here further discounts the "1.5 Child Policy" as a confounder. In stark contrast to the sex ratio results, the effect on having a second child all loads onto the OCP and is statistically significant at the 1% level. However, the net effect of OCP on having the second child is economically small, -0.004 (-0.027+0.046*0.5) relative to sample mean of 0.82, and consistent with the absence of fines for the second when OCP was first introduced.

Regarding the timing of fertility (conditional on having a second child), we test whether land reform shortened the birth interval between the first and second child. We assign treatment status according to year of birth of the second child. From column (4) to (6), there is little change in the birth interval induced by land reform when both reforms are controlled for. Overall, we do not find evidence that fertility responses would confound our findings, along with evidence that the OCP had a quite modest (although statistically significant) fertility effect.

# 7 Economic Mechanisms

Why did land reform increase sex selection? A common feature of land reform in other settings is that sons inherit land. This is unlikely to explain the increased sex selection we find because China's reform did not privatize land ownership. Intergenerational transfer was (and remains) impossible. *A priori*, two remaining mechanisms are most plausible:

1. Increases in household **income** following the reform increase the demand for a son or make a son more affordable. Just as children may be a normal good [Becker, 1960], so too may having a son. In consumer theory, goods with few close substitutes tend to be normal (e.g. Black et al. [forthcoming]). In cultures with a strong son preference, a daughter is a poor substitute for a son, so achieving a son may be expected to be a normal good. Moreover, sex selection and raising a son become more affordable as income increases.

2. If males have greater **productivity** in agricultural production, land reform could increase male earnings disproportionately. There are two distinct channels through which this could increase sex ratios: i) fathers' higher earnings induced more sex selection, or; ii) parents selected sons in order to obtain the disproportionate income increase ten or more years in the future, once the son became old enough to start working.

Empirically, having a second son became more common following land reform, but only after a first daughter. The economic mechanism should account for why having sons (plural) did not increase.

## 7.1 Income mechanism

As noted above, land reform's best documented effects in the existing literature are its positive impacts on agricultural output and income. To test for the income mechanism, we would like to compare the

sex of the second child in households with larger income gains and those with smaller gains after land reform. Unfortunately, no household-level income data are available from the 1970s to the early 1980s in China. Alternatively, we test two related predictions: 1) better educated parents who possibly gained more from land reform might sex select more; 2) higher sex ratios are observed in counties that gained more economically from the reform.

We first examine whether sex selection behavior following the reform differs by parental education. In column 1 of Table 6, we find that mothers with higher education levels were more likely to have a boy after the reform. The largest effect is found among mothers with a high school education, who are 7.5 percentage points more likely to have a son relative to those with no formal schooling. Similar to the calculation on the likelihood being a complier (Section 4.4.4 of Angrist and Pischke, 2009), we calculate the fraction of sex selectors following land reform by maternal education. We first estimate the benchmark effect of land reform on sex in the subsample of mothers with no formal schooling to be 0.016 (statistically significant at the 1 percent level). Among mothers who sex select due to land reform, 53% of them had a high school education, 27% a middle school education, and 20% a primary school education or no schooling (versus 4%, 13%, and 31% in mothers with a second child). In column 2, the education gradient among fathers is most apparent at the level of high school education, and the magnitude is smaller than that of mothers. When we control for both parents' education levels in column 3, estimates for mothers' education are robust, especially for high school education, while estimates for fathers' education are no longer statistically significant.

Better educated parents might capture larger income increases from land reform, which in turn spur more sex selection. Education improved the uses of household-supplied inputs and contributed to higher agricultural profits under the HRS (Yang and An, 2002). In Appendix Table 2, we find that counties with larger fraction of educated workers indeed have larger increase in grain output following reform. Furthermore, our education findings are consistent with a "first mover" advantage in sex selection, whereby high status parents would respond more strongly with selection because they are less susceptible to the marriage market consequence of imbalanced sex ratio (given hypergamy, women "marrying up", [Edlund, 1999]). The challenge lower status families might face in finding a wife for their son might temper their sex selection behavior.

Next, using grain output data at the county level, we test the income hypothesis between counties that benefited more from reform and those that benefited less. In Panel A of Appendix Table 3, we report the estimated effect of land reform on grain output per capita in our grain sub-sample. In column 1, on average, HRS adoption increases grain output by 2.6 percent at the 10 percent significance level.[27] We stratify the sample by the change in grain output before and after reform. Column 2 shows a precisely estimated output increase of 9.2 percent in counties above the median change in grain output at the

[27]The magnitude is smaller than the effect size found using provincial level data by Lin (1992). The outcome measure in Lin (1992) is the value of agriculture output, while ours uses only grain output thereby excluding changes in the price of grain (from price reform), as well as changes in cash crop production and price of cash crops. The effect size based on grain production and our more finely-focussed identification strategy presumably captures the lower bound of income change induced by the reform.

1 percent significance level, while column 3 shows a 3.9 percent decrease at the 10 percent significance level in counties below the median. Only counties above the median experienced an increase in grain output after the reform. In the subsample of counties with grain (and land reform) data, we present the estimated effect of land reform on the second child being male in Panel B. In column 1, the magnitude of the increase in the probability of being male, 1.3 percentage points, is smaller than that in our full sample, and it is also less precisely estimated. This indicates that we might underestimate the effect on male births using this grain-matched subsample. Column 2 shows a precisely estimated increase in probability male of 2.7 percentage points for counties above the median of the change in grain output, which doubles the overall effect in column 1. In contrast, the estimate in column 3 for counties below the median is very small in magnitude and not statistically different from zero, and has negative sign.[28]

To summarize, our evidence on the heterogeneous treatment effects of land reform – more sex selection among better educated parents and in counties with larger income gains – supports the income mechanism.

## 7.2 Productivity mechanism

Qian [2008] found that increases in female-specific income, as captured by the relative price increase of tea following post-Mao price reform, increased the survival rate of girls. If either higher paternal income or demand for sons' future labor were the primary force to sex select following land reform, we would expect more skewed post-reform sex ratios where the agricultural production was more male intensive.

We use two approaches to capture gender-specific productivity at the county level. First, we ascertain which crops were more or less male-labor intensive using the occupation and industry codes in the 1982 Census microdata. Overall, agricultural labor was fairly evenly divided between men and women. In Appendix Table 4 (Panel B), the county-level mean of male agricultural labor is 0.52 with a standard deviation of 0.026 across counties. It is so largely because grain production, which employed 95% of agricultural labor, was fairly gender neutral. Nevertheless, there is substantial variation in the county-level mean of males growing cash crops across counties (mean 0.52 and standard deviation of 0.23). Our first approach is to use the fraction of men growing cash crops by county to proxy for demand for male labor at the time of the reform. Among the main cash crops, cotton was the most female labor intensive: 35% of workers who grew cotton were male. Fruit appears to have been most male labor intensive: 69% of workers who grew fruit are male.

A potential concern is that crop choices might change after the reform when households could make their own production decisions. To provide a relatively exogenous measure for gender specific income, our second approach uses crop suitability indices based on agro-climate conditions from the FAO Global Agro-Ecological Zones (GAEZ) 2012 database. FAO calculated an estimate of the potential yield of each crop and crop suitability in each 0.5-degree-by-0.5-degree grid cell, given an assumed level of

[28] If parents thought sex selection was "bad" but wanted to do it anyways, they might increase their practice during the disorder right after land reform. If this alternative channel dominated, we would expect the same increase in sex ratios regardless of changes in grain output.

crop management and input use.[29] We aggregate the crop suitability indices to the county level. We focus on three sets of crops: 1) cotton, a female intensive crop; 2) fruits including citrus and banana, male intensive crops; 3) grain including wheat and wetland rice, the gender neutral crops. Our second approach is to compare the land reform effect on sex between "cotton friendly" counties and "fruit friendly" counties.

In Table 7, we attempt to isolate male income. Column 1 reports the coefficient on the interaction of land reform, the first child being a girl, and the fraction of male workers growing cash crops by county. It is statistically insignificant and economically very small: an increase of 0.02 percentage points, that is, a 10 percent increase in the fraction of male workers leads to a 0.2 percent increase in the probability of second child being male. The estimate is fairly precise (standard error of .0002). This estimate is unchanged in column 2 when we control for the interaction term with the fraction of male workers growing grain. In column 3, we compare the reform effect between counties more suitable for female-intensive crop and those more suitable for male-intensive crops, while suitability of gender-neutral crops is controlled for. None of these estimates are statistically significant. One index of a male-intensive crop, citrus, has a positive sign. However, the index of the female-intensive crop, cotton, also has a positive sign. Thus, we do not see much heterogeneity according to gendered agricultural earnings (*cf* heterogeneity by maternal education or grain output).

Overall, neither gender-specific income nor demand for future gender-specific labor appears to be the mechanism for our sex selection effect. Alternatively, evidence in this subsection is consistent with an increase in total household income.

## 7.3 Other economic mechanisms

This subsection examines another four possible channels through which land reform might affect sex ratios. None of these mechanisms is supported by our empirical evidence.

### 1. Was land distribution male biased?

Men and women had equal rights in land distribution. However, absent central oversight of women's land rights after marriage, there is anecdotal evidence that local rules might favor males. For example, when a daughter married out of her village, her plot of land was taken back by the village; getting a new plot in the village she married might not be automatic (Bossen, 2002). If women in fact received less land because of expropriation at marriage, it is perhaps less surprising to observe rising sex ratios following a reform that so directly favors males. If expropriation was common practice across China, we would expect that on average families with more males would have more land *within the village*, the administrative unit where land allocation and reallocation (due to household demographic changes)

[29] The crop suitability indices are based on intermediate input level. Water supply is rain-fed. Each index scales from 1 to 7, the higher the more suitable. Scale 1 indicates water, not suitable or very marginal, 2 for marginal, 3 for moderate, 4 for medium, 5 for good, 6 for high, and 7 for very high.

were implemented.

Unfortunately, we do not observe land holdings in the 1990 Census data. We test whether men had more land in two rural household surveys in the 1980s: the 1989 Chinese Health and Nutrition Survey (CHNS) that covers nine provinces and the 1986-89 Rural Fixed Point Survey that is nationally representative.[30] Using the CHNS 1989 wave in Panel A of Appendix Table 5, we find that, within village, having more male members has a very small effect on size of land farmed by the household (a 50% increase in the fraction of males increases household land size by 0.1 *mu*, or a 3% increase compared to the sample mean), which is not statistically significant. Furthermore, we test whether possible land reallocation in a 4-year window favored families with an increase in the fraction of adult males (if daughters "marry out") using the 1986-89 Rural Fixed Point Survey. From the household-level fixed effect estimator in Panel B, we find no evidence that changes in household land size are correlated with changes in the fraction of male labor.

One might argue that parents *feared* losing the land of a daughter, despite the lack of empirical evidence to support the expectation. We do not think it is plausible because of the short duration of land leases when the HRS was introduced. As documented in various county gazetteers, the initial reform granted a 3-5 year lease to individual households. In 1984, the central government officially extended the lease to 15 years. If parents had any expectation on the land rights of their children, it would not be beyond 15 years, when their children would still be too young to get married.

### 2. Extension of land lease in 1984

The subsequent extension of land leases to 15 years in 1984 might have substantially changed families' expectation of future income. If families waited until the extension to respond with sex selection, we should observe a large increase in sex ratios in 1984. We plot sex ratios of the second child by year of birth in Appendix Figure 4. There is no obvious change in the slope of the sex ratio following a first girl; the first increase in sex ratios occurred a few years before 1984. In Appendix Table 6, we interact the indicator of born 1984-86 with the girl first dummy to capture the effect of the land lease extension in 1984. We observe an increase of 1.3 percentage points in the probability of second child being male, statistically significant at the 10% level. However, including this interaction has little change to the estimate of the (larger) land reform effect (0.03, consistent with Table 4A).

### 3. Increase in demand for old age support

Another interpretation is that land reform destroyed the financial basis of the "state pension system". Its destruction then forced parents to rely on sons (instead of the collective or state) for old age support. If demand for sons were driven by collapse of collective support, we would expect that initially poor families, or families that gained less from the reform, were more in need of financial support from sons,

[30] The Chinese Household Income Project Survey (CHIPS) 1988 also has information on household land size and gender composition. We do not use CHIPS 1988 because the smallest administrative unit is county, and therefore we cannot conduct the analysis within village.

and thus were more likely to select sex. Because we do not have a income or wealth measure prior to reform, we cannot test this hypothesis at the household level. At the county level, our findings in Section 7.1 show the opposite: counties that experienced more output gains have a substantially larger increase in sex ratios after the reform. Furthermore, in Appendix Table 7, we present evidence on heterogeneous effects by initial economic conditions at the county level. Similarly, initially-rich counties also had more boys born after the reform. An increase in demand for old age support can not be easily reconciled with these findings.

**4. Collapse of rural medical system**

The rural medical system of Mao's era also came to its end after the reform. A resulting concern is that parents might respond to the negative healthcare shock differently for boys and for girls. If the cutoffs in health care supply had any effect on child survival, it would be the opposite to the effect of income growth. Although we cannot directly separate these two offsetting channels, we can test the net effect of the reform on infant health outcomes in the UNICEF 1992 Chinese Children Survey (no health indicators in the census data). The survey covers 522,371 households from 1088 counties in 29 provinces.

In Appendix Table 8, Panel A reports estimates for all births. We find that postneonatal mortality decreased by 0.3 percentage points (37.5% relative to sample mean), and birth weight increased by 34 grams (statistically significant at the 5% level, but a 1% effect relative to sample mean). These findings indicate that the impact of the change in health care supply, if any, would not offset the health benefits of land reform. To compare the effects on health outcomes with our main estimates on sex ratio, we focus on the second births in Panel B. Using the sample of all second births, there is little evidence that the effects of land reform on health outcomes differ by the sex of the first child.

We do not find evidence that the large increases in sex ratios coincided with a major deterioration in childhood health caused by compromised rural healthcare. Again, the large improvement in birth outcomes is consistent with increased income and reduced poverty improving health.

# 8 Proximate Mechanisms

How did land reform increase sex selection? Small deviations from normal sex ratios (around 1.05) occur "naturally" due to biology, e.g. Norberg [2004]; Almond & Edlund [2007]. Large increases in population sex ratios are generally accepted as behavioral, i.e. they reflect discriminatory decisions made in response to knowledge of offspring sex [Duflo, 2012]. Sex selection behavior includes sex-selective abortion, infanticide, adoption, and differential investment, including neglect and abandonment. Parents might prefer to conceal such behaviors, and as such detecting them a sleuthing exercise in

"forensic economics" [Zitzeqitz, 2012]. In general, direct observation of such behaviors is impossible.[31] Compounding matters, we only observe the sex of children in census microdata, not at birth, making it more difficult to distinguish prenatal versus postnatal behaviors. A convenient feature of our study from a forensic perspective is that the sex ratio has both ordinal and cardinal properties: ratios substantially above 1.05 were presumably achieved through a combination of these responsive behaviors. Below we provide indirect evidence related to two proximate mechanisms: sex-selective abortion following prenatal ultrasound and postnatal mortality. Their analysis and the omission of other mechanisms below is dictated by the data available for this time period.

## 8.1 Ultrasound availability in provincial capitals

Was sex-selective abortion possible? Land reform generally preceded the arrival of ultrasound machines in rural China, while ultrasound was largely available in provincial capitals from the late 1970s (Figure 4). We consider rail access as it was the main means of long-distance transportation at that time.

Using a digitized national map of railroad networks in 1980 (generously provided by Matthew Turner),[32] we define railroad access by whether a railroad line passed through a rural county. Every county on a railroad line was connected to the capital city of the same province. 36% of counties had railroad access. We assign access to ultrasound technology as 1 if a county was connected by railroad to the provincial capital that had ultrasound machines available one year after land reform or earlier, and 0 otherwise. Counties that are assigned 0 either had no railroad passing through or they had railroad linked to the provincial capital but ultrasound machines were not available there yet, or both.

In column 1 of Table 8, the land reform effect on sex is 2 percentage points higher if parents could take the train from their home county to the provincial capital to access ultrasound machines. When we compare the estimate of land reform, 0.024, to our main estimate 0.029 in Table 3, prenatal sex determination through our measure of rail access to ultrasound could explain 17% of the increase in sex ratios induced by land reform.

A potential concern is that railroad access might also help peasants to connect to a larger input/output market and hence increase their income, another interpretation of the results in column 1. To isolate the effect of access to ultrasound in provincial capitals from other channels, we include the interaction of land reform, girl first ,and railroad to province capital in column 2. Absent ultrasound technology in the provincial capital, rail access does not seem to increase sex ratios following land reform. The effect of access to ultrasound technology is larger (.025) once the railroad access is accounted for, suggesting that the main channel railroad access contributed to higher sex ratios is through access to ultrasound technology.

[31] A possible exception is Gu and Li [1996], who observed the sex of aborted fetuses in southern *Zhejiang* province, finding more female fetuses were aborted following a female live birth.

[32] Digitized from SinoMaps Press (1982) and used in Baum-Snow, Brandt, Henderson, Turner and Zhang (2012).

### 8.2 Excess female mortality after birth

The UNICEF 1992 Chinese Children Survey allows us to consider postnatal mortality. The Survey will miss female infanticide to the extent that their live births were not reported in the Survey. Following a first daughter, we do not find an effect of land reform on the overall mortality of second births 1977-1986 (column 1, Appendix Table 9). However, this masks heterogeneity by gender of the second child. Male mortality decreased 1.6 percentage points in column 2, mirrored by an increase of 1.6 percentage points in female mortality after land reform in column 3. Using these point estimates and the roughly 3% baseline mortality rate, a back-of-the-envelope calculation indicates that excess female mortality induced by land reform would increase the sex ratio from 1.05 to 1.07. The sex ratio in our main sample (Table 4A) increased from 1.06 prior to land reform to 1.13 after the reform. Therefore, roughly 29% of girls were missing due to postnatal excess female mortality.

In sum, we find that sex-selective abortion via "provincial" ultrasound and excess female mortality accounted for 46% of the increase in sex ratios following land reform. This suggests that remaining selection methods, e.g. infanticide, abandonment, prenatal sex determination by other technologies or locations, etc., might account for a little more than half of the sex ratio imbalance.

## 9 Discussion

We find that the post-Mao land reform increased the number of missing girls by more than 1.24 million over its first six years. In so doing so, we challenge two core beliefs about sex selection.

First, the argument that the One Child Policy (OCP) raised sex ratios is plausible *a priori*: fewer parents can have a son by chance if families are small. But fertility rates were cut in half during the 1970s (Appendix Figure 1A & 1B), i.e. prior to the introduction of OCP incentives and penalties. This historic fertility decline was not reflected by an increase in sex ratios (Figure 1). Furthermore, we collect the most comprehensive county-level dataset to date and find that while the OCP did reduce fertility in rural counties (home to 86% of China's population at the time), its impact was very small. Whatever modest impact it appears to have on sex selection is eliminated once land reform is accounted for. In the current debate about relaxing or eliminating the OCP, its role in "missing girls" is frequently invoked [CNN, July 2012; NPR, April 2013; New York Times, May 2013].[33] To the extent that the introduction of the rural OCP is taken as evidence for this connection, our findings suggest otherwise. Indeed, fertility in Hong Kong and Taiwan is well below replacement levels in the absence of a OCP, so the opportunity to have a son by chance may not change appreciably even if the OCP is relaxed or eliminated.

Second, it is commonly argued that development will help eliminate gender disparities [World Development Report 2012]. While previous work has shown that lowering the cost of sex selection can increase sex selection, this usually refers to a narrow facet of development: diffusion of prenatal sex

[33] http://globalpublicsquare.blogs.cnn.com/2012/07/09/could-chinas-one-child-policy-change/
http://www.npr.org/2013/04/23/176326713/for-chinese-women-marriage-depends-on-right-bride-price
http://www.nytimes.com/2013/05/22/opinion/chinas-brutal-one-child-policy.html

determination technologies. Indeed, policy-makers in Asia have considered restricting access to such technologies as a solution to high sex ratios. India started to ban ultrasound in prenatal sex determination as early as 1994 and China issued a similar law in 2003. But prenatal sex determination technology continues to evolve and may be increasingly difficult to regulate.[34] While banning its use may send an important message, it is unclear whether it will provide much of a practical obstacle. In our analysis, sex selection increased even when ultrasound access did not. Our findings suggest that given a cultural preference for sons [Almond, Edlund, & Milligan, 2013], development more generally may not eliminate "missing girls", and therefore the phenomenon is more intractable than realized.

[34]For example, see Devaney et al. [2011] on recent advances in non-invasive fetal sex determination.

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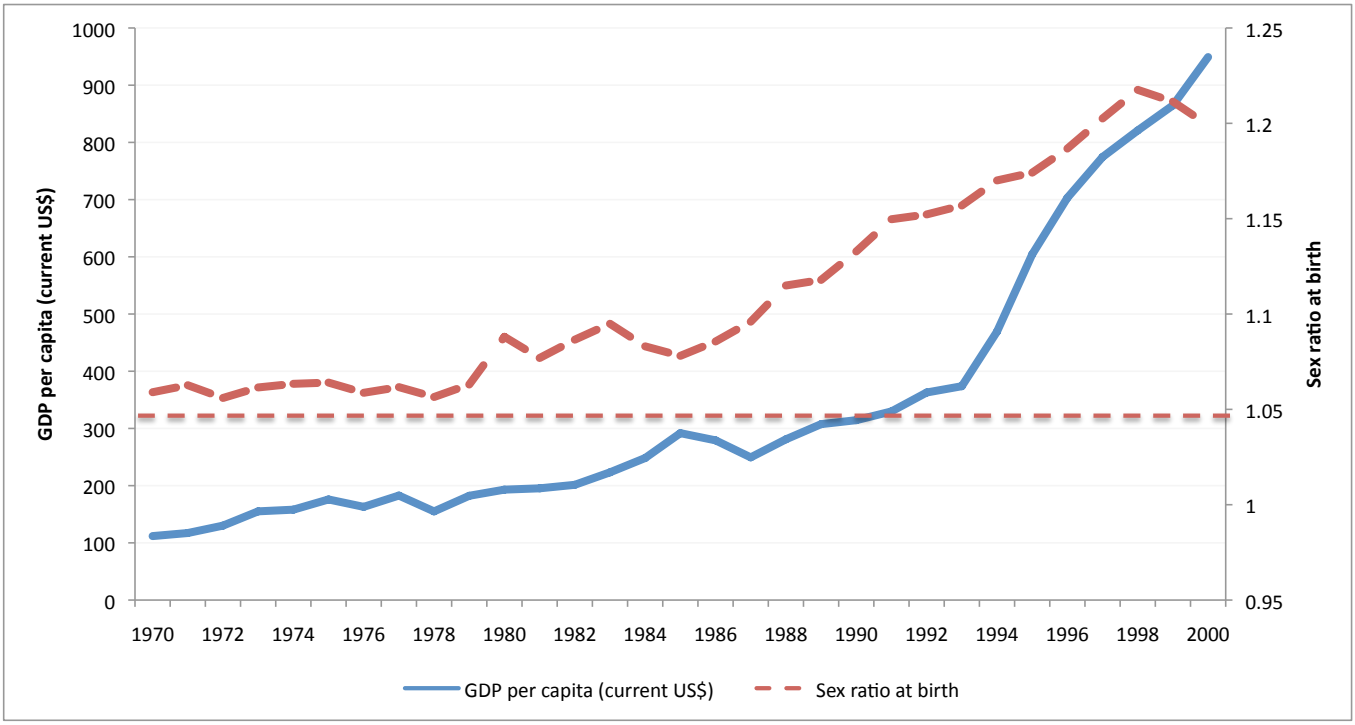
Figure 1: GDP per capita and sex ratio at birth in China: 1970-2000




Notes: 1) Data on GDP per capita (current US$) are from World Bank; 2) Data on sex ratios at birth in 1970-1981 are from the 1% sample of the 1982 Census, 1982-1989 data are from the 1% sample of the 1990 Census, and 1990-2000 data are from the 1% sample of the 2000 Census. 3) The horizontal line is at sex ratio of 1.05, the biologically normal rate.

Figure 2A: Sex ratio of the first child

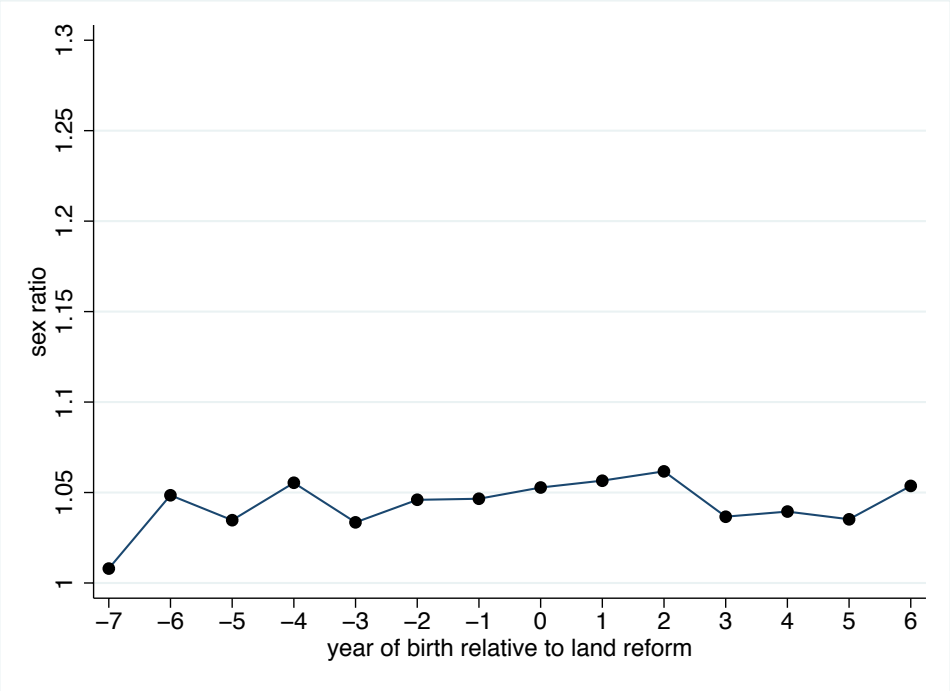
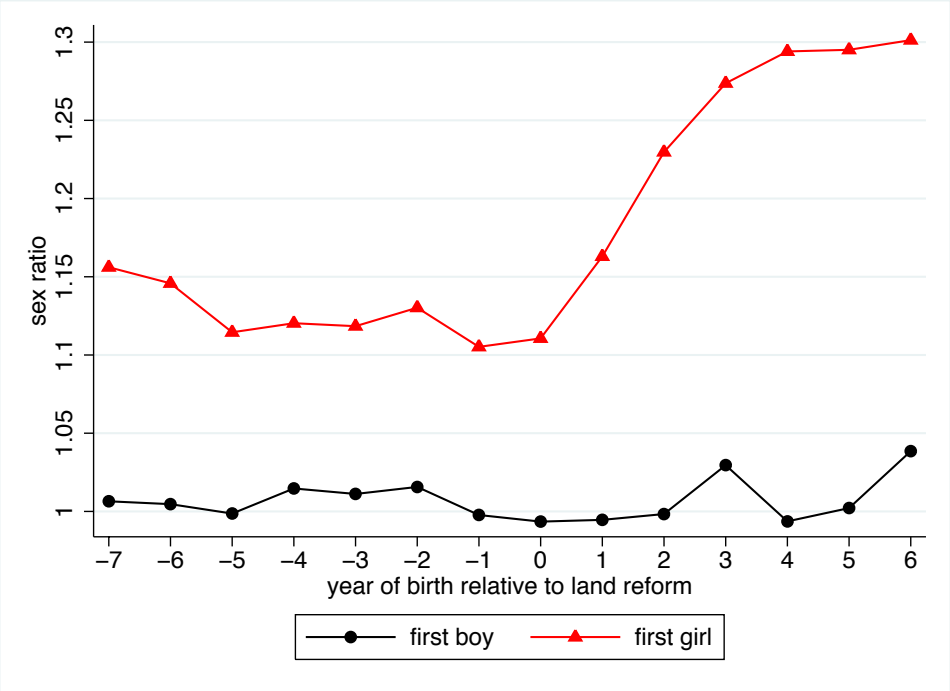



Figure 2B: Sex ratio of the second child




Note: Figure 2A and 2B are unadjusted figures, plotting sex ratios by the year of birth relative to land reform.

Figure 3A: County-level rollout of land reform and the One Child Policy, and Provincial rollout of the 1.5 Child Policy

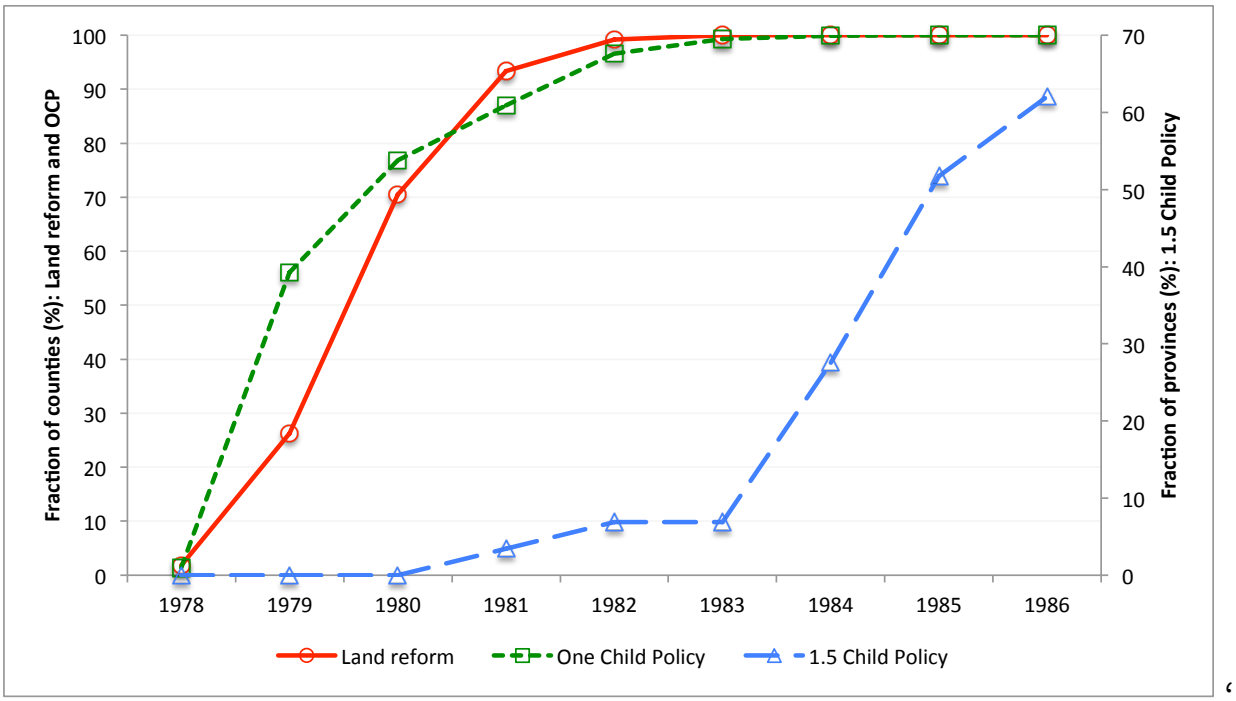
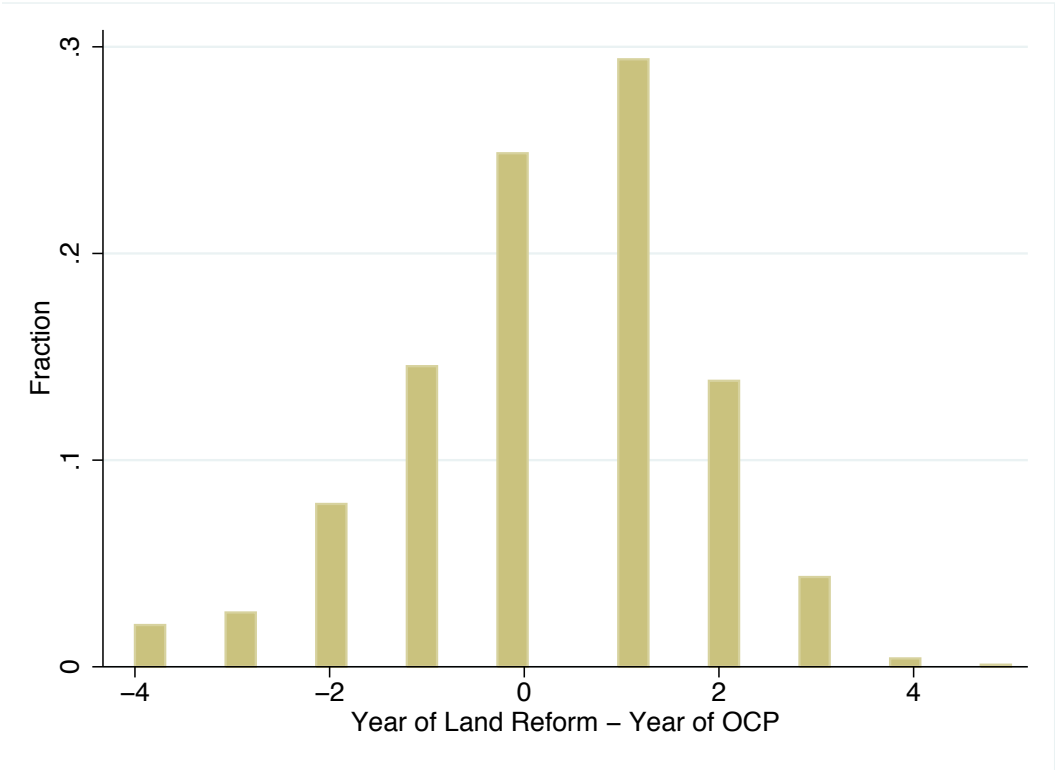



Figure 3B: Difference between land reform start year and the OCP start year




Note: Figure 3B shows the distribution of the difference between land reform start year and the OCP start year.

Figure 4: Rollout of land reform and ultrasound technology

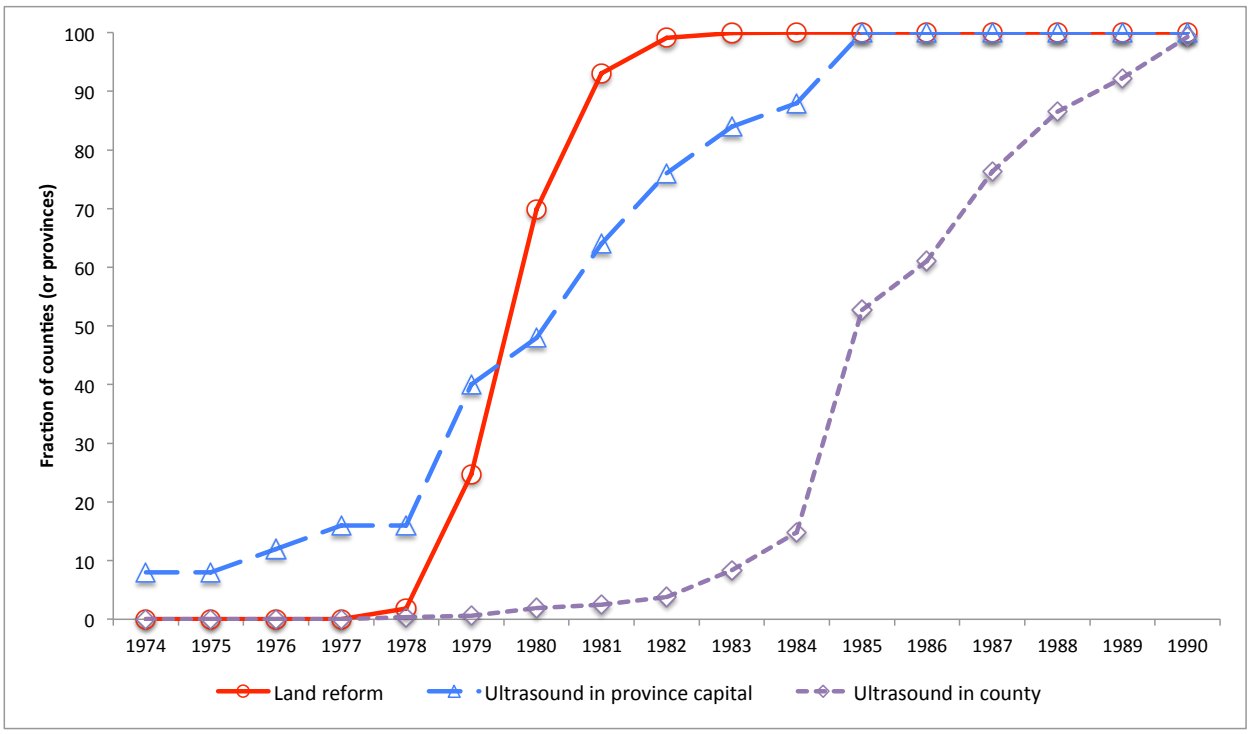


Table 1A: Time-invariant determinants of reform timing

| | Dependent variable: first year of land reform (1978-1984) | | | | |
|---|---|---|---|---|---|
| | Univariate | | | Multivariate | |
| | | Obs | R-squared | | |
| ln (grain output per capita 1976) | 0.250** | 481 | 0.011 | 0.400*** | |
| | [0.121] | | | [0.126] | |
| ln (distance to province capital) | 0.075** | 1,201 | 0.003 | -0.003 | -0.039 |
| | [0.036] | | | [0.061] | [0.039] |
| ln (labor force density 1976) | -0.147*** | 1,117 | 0.044 | -0.172*** | -0.149*** |
| | [0.022] | | | [0.045] | [0.028] |
| ln (famine intensity 1959-1961) | -0.494*** | 1,189 | 0.033 | -0.291** | -0.349*** |
| | [0.081] | | | [0.144] | [0.089] |
| ln (distance to beijing) | -0.074* | 1,201 | 0.003 | -0.127 | -0.134*** |
| | [0.038] | | | [0.078] | [0.041] |
| ln (sex ratio at birth 1975-77) | -0.135 | 1,193 | 0.001 | -0.198 | -0.235 |
| | [0.144] | | | [0.214] | [0.145] |
| Observations | | | | 438 | 1,114 |
| R-squared | | | | 0.096 | 0.072 |

Notes: The dependent variable is the first year of land reform, which varies from 1978 to 1984. For univariate analysis, each estimate is from a separate regression. Multivariate regressions include all independent variables. Data on grain output per capita in 1976 are collected from county gazetteers: only 438 counties report this information. Distance to Beijing and distance to province capital city are in kilometers and are obtained from a GIS map of 1982 Census. Labor force density in 1976 is calculated by population size aged 16-60 in 1976 divided by area. Using the 1982 Census, we measure the 1959-61 famine intensity by the average cohort size born in 1953-1957 divided by the average cohort size born in 1959-1961. Sex ratios at birth for birth cohorts 1975-77 are from the 1982 Census. Robust standard errors are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 1B: Droughts (time-variant) and reform timing

| | Dependent variable=1 for the first year of reform, 0 before reform and missing after the first year | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | March-September | March | April | May | June |
| Drought in year t | -0.011 | -0.021*** | -0.037*** | -0.006 | -0.004 |
| | [0.009] | [0.008] | [0.008] | [0.008] | [0.009] |
| Drought in year t-1 | 0.001 | -0.026*** | -0.027*** | 0.004 | -0.009 |
| | [0.008] | [0.007] | [0.008] | [0.008] | [0.008] |
| County FE | X | X | X | X | X |
| Year FE | X | X | X | X | X |
| County linear trend | X | X | X | X | X |
| Observations | 7,306 | 7,306 | 7,306 | 7,306 | 7,306 |
| R-squared | 0.768 | 0.769 | 0.769 | 0.768 | 0.768 |

Notes: The dependent variable is 1 for the first year of reform, 0 prior to the reform, and missing value after the first year. Drought is a dummy variable which is equal to 1 if the average monthly precipitation is below the bottom 20th percentile in the precipitation distribution during 1957-1984 and 0 otherwise. We include two drought indicators, one in the current year and another the year before. In the first column we measure drought using monthly average precipitation from March to September. Each of the other column headings presents the single month in which drought is measured. All regressions include county fixed effects, year effects and county linear trends. The sample includes 1194 counties and the time span is from 1975 to 1984. Robust standard errors are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 2: Summary Statistics

| | Births between 1974 and 1986 | | | |
|---|---|---|---|---|
| | Full sample | | Two-parent sample | |
| | first child | second child | first child | second child |
| Boy | 0.511 | 0.523 | 0.511 | 0.523 |
| Girl first | | 0.507 | | 0.507 |
| Exposed to land reform | 0.541 | 0.511 | 0.545 | 0.519 |
| | | | | |
| Mother No formal schooling | | | 0.468 | 0.522 |
| Mother Primary school | | | 0.260 | 0.308 |
| Mother Middel school | | | 0.197 | 0.133 |
| Mother High school | | | 0.075 | 0.037 |
| Father No formal schooling | | | 0.327 | 0.324 |
| Father Primary school | | | 0.169 | 0.268 |
| Father Middle school | | | 0.343 | 0.294 |
| Father High school | | | 0.160 | 0.114 |
| | | | | |
| Observations | 371762 | 279069 | 349351 | 260529 |

Table 3: Land reform and sex ratio

| | Male=1 | | | | Sex ratio (county-year) |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | First child | Second child | | | Second child |
| | | | | Han only | |
| Land reform*Girl first | | 0.029*** | 0.029*** | 0.033*** | 0.151*** |
| | | [0.004] | [0.004] | [0.004] | [0.030] |
| Land reform | 0.003 | -0.010* | | -0.014** | -0.021 |
| | [0.004] | [0.006] | | [0.006] | [0.044] |
| Girl first | | 0.027*** | 0.027*** | 0.028*** | 0.136*** |
| | | [0.003] | [0.003] | [0.003] | [0.020] |
| County FE | X | X | | X | X |
| YOB FE | X | X | | X | X |
| Initial control*YOB FE | X | X | | X | X |
| Spring drought in t and t-1 | X | X | | X | X |
| County-specific linear trends | X | X | | X | X |
| County * YOB FE | | | X | | |
| Dependent variable mean | 0.511 | 0.523 | 0.523 | 0.524 | 1.27 |
| Observations | 371762 | 279069 | 298755 | 248670 | 24,255 |
| R-squared | 0.006 | 0.011 | 0.052 | 0.012 | 0.131 |

Notes: Column (1) reports estimate for the effect of exposure to land reform on the probability of first child being male; column (2) and (3) for the effect on second child being male for all second births, column (4) for the effect on second child being male for Han Chinese only. Column (5) reports results on sex ratio of all second births by county, birth year and sex of the first child. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on reform timing and initial controls. Regressions in column (1), (2), (4) and (5) include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects, and droughts in March and April of the current year and the preceding year. Regression in column (3) includes county-by-year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 4A: Land reform versus the One Child Policy

| | male=1 | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | All | | | | | | Han | | |
| | Main specification | | | County-by-year FE | | | Main specification | | |
| Land reform*Girl first | 0.030*** | | 0.031*** | 0.030*** | | 0.033*** | 0.035*** | | 0.038*** |
| | [0.005] | | [0.008] | [0.005] | | [0.008] | [0.005] | | [0.008] |
| Land reform | -0.012* | | -0.013* | | | | -0.016** | | -0.017** |
| | [0.007] | | [0.007] | | | | [0.007] | | [0.008] |
| OCP*Girl first | | 0.024*** | -0.002 | | 0.024*** | -0.004 | | 0.027*** | -0.004 |
| | | [0.004] | [0.008] | | [0.004] | [0.008] | | [0.005] | [0.008] |
| OCP | | -0.017*** | -0.004 | | | | | -0.017*** | -0.001 |
| | | [0.006] | [0.007] | | | | | [0.007] | [0.007] |
| Girl first | 0.025*** | 0.028*** | 0.025*** | 0.024*** | 0.027*** | 0.025*** | 0.026*** | 0.028*** | 0.026*** |
| | [0.003] | [0.003] | [0.003] | [0.003] | [0.003] | [0.003] | [0.003] | [0.004] | [0.004] |
| Observations | 224600 | 224600 | 224600 | 241547 | 241547 | 241547 | 199423 | 199423 | 199423 |
| R-squared | 0.011 | 0.011 | 0.011 | 0.051 | 0.051 | 0.051 | 0.012 | 0.012 | 0.012 |

Notes: The sample in column (1)-(6) includes all second births between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and OCP, and the sample in column (7)-(9) includes all second births of Han ethnicity. Regressions using our main specification in column (1)-(3) and (7)-(9) include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Regressions in column (4)-(6) include county-by-year fixed effects. Robust standard errors clustered at the county level are reported in brackets.
* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 4B: Land reform effect before and after the OCP came in

| | male=1 | | |
|---|---|---|---|
| | (1) Before OCP came in | (2) After OCP came in | (3) whole sample |
| Land reform*Girl first | 0.026*** | 0.031*** | 0.025*** |
| | [0.009] | [0.005] | [0.009] |
| Land reform | 0.018 | -0.026*** | 0.007 |
| | [0.013] | [0.007] | [0.010] |
| Girl first | 0.027*** | 0.025*** | 0.027*** |
| | [0.006] | [0.004] | [0.006] |
| Land reform*Girl first* 1{After OCP came in} | | | 0.006 |
| | | | [0.010] |
| Observations | 55685 | 168915 | 224600 |
| R-squared | 0.012 | 0.011 | 0.011 |

Notes: Column (1) includes counties where land reform occurred before the OCP came in, and column (2) includes counties where land reform occurred the same year or later than the OCP. Column (3) includes all counties. 1{After OCP came in} is assigned 1 if land reform occurred the same year or later than the OCP. Land reform*1{After OCP came in} and Girl first*1{After OCP came in} are also controlled for. 1{After OCP came in} is absorbed by county fixed effects. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 5A: Fertility response (1) - number of births

| | Number of births by county and year | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Land reform | 2.333** | | 2.277** |
| | [1.101] | | [1.104] |
| OCP | | -2.824** | -2.783** |
| | | [1.101] | [1.097] |
| Dependent variable mean | | 90 | |
| Observations | 11137 | 11137 | 11137 |
| R-squared | 0.948 | 0.948 | 0.949 |

Notes: The sample is at the county-birth year level, including birth cohorts between 1974 and 1986 in counties that are matched with data on timing of land reform and the OCP. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 5B: Fertility response (2) - decision to have a second child and birth interval

| | Have second child=1 | | | Birth interval between 1st and 2nd | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Land reform*Girl first | 0.026*** | | -0.009 | -0.026* | | 0.021 |
| | [0.004] | | [0.008] | [0.015] | | [0.026] |
| Land reform | -0.016*** | | 0.001 | 0.057** | | 0.032 |
| | [0.005] | | [0.006] | [0.025] | | [0.028] |
| OCP*Girl first | | 0.038*** | 0.046*** | | -0.039** | -0.056** |
| | | [0.004] | [0.009] | | [0.015] | [0.026] |
| OCP | | -0.023*** | -0.027*** | | 0.004 | 0.014 |
| | | [0.004] | [0.006] | | [0.024] | [0.028] |
| Girl first | 0.040*** | 0.030*** | 0.031*** | -0.181*** | -0.174*** | -0.175*** |
| | [0.004] | [0.003] | [0.003] | [0.011] | [0.011] | [0.011] |
| mean of dependent variable | | 0.82 | | | 2.9 | |
| Observations | 298310 | 298310 | 298310 | 224600 | 224600 | 224600 |
| R-squared | 0.343 | 0.343 | 0.343 | 0.087 | 0.087 | 0.087 |

Notes: The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and the OCP. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 6: Treatment effect heterogeneity, by parental education

| | Dependent variable: Male=1 | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Land reform*Girl first*Mother High school | 0.075*** | | 0.062** |
| | [0.024] | | [0.025] |
| Land reform*Girl first*Mother Middle school | 0.031** | | 0.026* |
| | [0.013] | | [0.014] |
| Land reform*Girl first*Mother Primary school | 0.01 | | 0.008 |
| | [0.009] | | [0.010] |
| Land reform*Girl first*Father High school | | 0.044*** | 0.027 |
| | | [0.016] | [0.017] |
| Land reform*Girl first*Father Middle school | | 0.015 | 0.005 |
| | | [0.011] | [0.012] |
| Land reform*Girl first*Father Primary school | | 0.005 | 0.001 |
| | | [0.010] | [0.011] |
| Land reform*Girl first | 0.017*** | 0.016* | 0.014 |
| | [0.006] | [0.009] | [0.009] |
| Observations | 260529 | 260529 | 260529 |
| R-squared | 0.007 | 0.007 | 0.007 |

Note: Land reform*Parental eduation, Girl first*Parental education and Parental education are also controlled for.
This table reports estimate for the effect of exposure to land reform on the probability of second child being male by parental education. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on reform timing and initial controls. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 7: Treatment effect heterogeneity, by the fraction of male workers or crop suitability

| | Male=1 | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| **A. % Male workers by county in the 1982 Census (Appendix Table 1)** | | | |
| Land reform*Girl first*% Male growing cash crop | 0.0002 | 0.0002 | |
| | [0.0002] | [0.0002] | |
| Land reform*Girl first*% Male growing grain | | -0.0005 | |
| | | [0.0005] | |
| **B. Average crop suitability index by county from FAO GAEZ** | | | |
| Land reform*Girl first*Cotton suitability index | | | 0.005 |
| | | | [0.005] |
| Land reform*Girl first*Citrus suitability index | | | 0.011 |
| | | | [0.011] |
| Land reform*Girl first*Banana suitability index | | | -0.002 |
| | | | [0.011] |
| Land reform*Girl first*Wheat suitability index | | | 0.006 |
| | | | [0.006] |
| Land reform*Girl first*Wetland Rice suitability index | | | -0.014 |
| | | | [0.014] |
| Observations | 256605 | 256096 | 278522 |
| R-squared | 0.011 | 0.011 | 0.012 |

Notes: The fraction of male workers growing cash crop or grain by county is constructed using occupation and industry codes in the 1982 Census microdata (see also Appendix Table 1). Average crop suitability index by county is aggregated using data from the FAO GAEZ Data Portal version 3.0 (2012 May). The suitability index (for intermediate input level rain-fed) is from 1 to 7, the higher the more suitable. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on reform timing and initial controls. Regressions in column 1 and 2 also include fraction of male*land reform, fraction of male*girl first, and girl first*land reform. Regression in column 3 also includes each crop index*land reform, each crop index*girl first, and girl first*land reform. All regressions include girl first, land reform, county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 8: Railroad access to province capital cities that had ultrasound machines

| | male=1 | |
|---|---|---|
| Land reform*Girl first*Railroad to province capital where ultrasound came in 1 year after land reform or earlier | 0.020** | 0.025* |
| | [0.010] | [0.013] |
| Land reform*Girl first*Railroad to province capital | | -0.006 |
| | | [0.011] |
| Land reform*Girl first | 0.024*** | 0.025*** |
| | [0.005] | [0.005] |
| Observations | 279069 | 279069 |
| R-squared | 0.011 | 0.011 |

Notes: In column (1), Land reform, Girl first, Land reform*Railroad to province capital that had ultrasound and Girl first*Railroad to province capital that had ultrasound are also controlled for. In column (2), additionally, Land reform*Railroad to province capital and Girl first*Railroad to province capital are also controlled for. The sample includes counties that are matched with county-level data on land reform. The regression includes county fixed effects, year of birth effects, county-specific linear trends, and initial county controls interacted with birth year effects. Robust standard errors clustered at the county level are

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

# Appendix

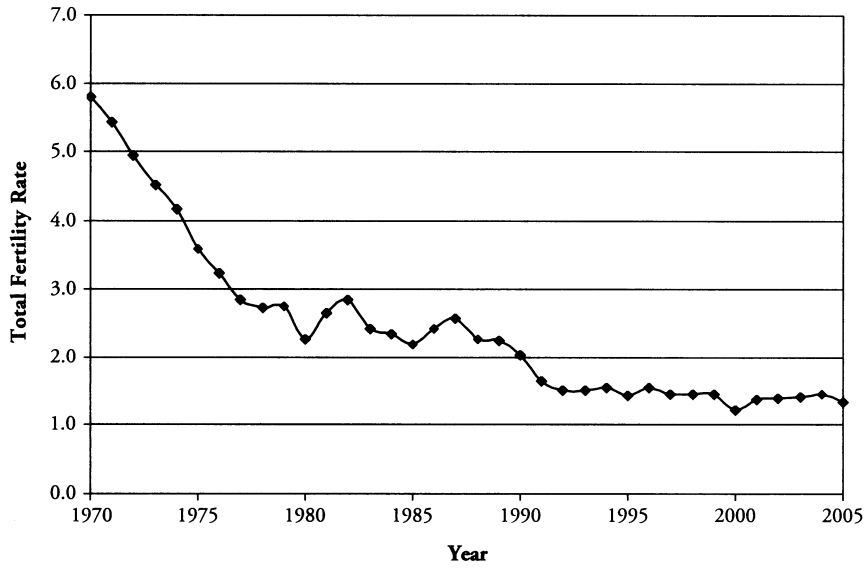
## Data Appendix: Precipitation Data

We use the Global Surface Summary of Day data produced by the National Climate Data Center (NCDC). Throughout China, daily data on the total precipitation amount (to 0.01 inches) are available from 225 weather stations from 1956 to 1964 and 536 stations from 1973 to 1984. In each year, we assign each county in the 1982 Census the precipitation data from the nearest weather station using longitude and latitude. Because the number of weather stations increases overtime, a county might be assigned different stations in different years, with relatively closer stations in more recent years.

To construct the measure of drought in March, for example, we first generate the distribution of total precipitation in March from all years during 1956-1964 and 1973-1984 for each county. We then define drought in March as a binary variable that is equal to 1 if the monthly precipitation is below the bottom 20 percentile of the distribution for each county in each year and 0 otherwise. For drought in the whole growing season, we calculate the average monthly precipitation from March to September and use its distribution to define drought.

Appendix Figure 1A: Total Fertility Rate, 1970-2005 (Cai, 2008)

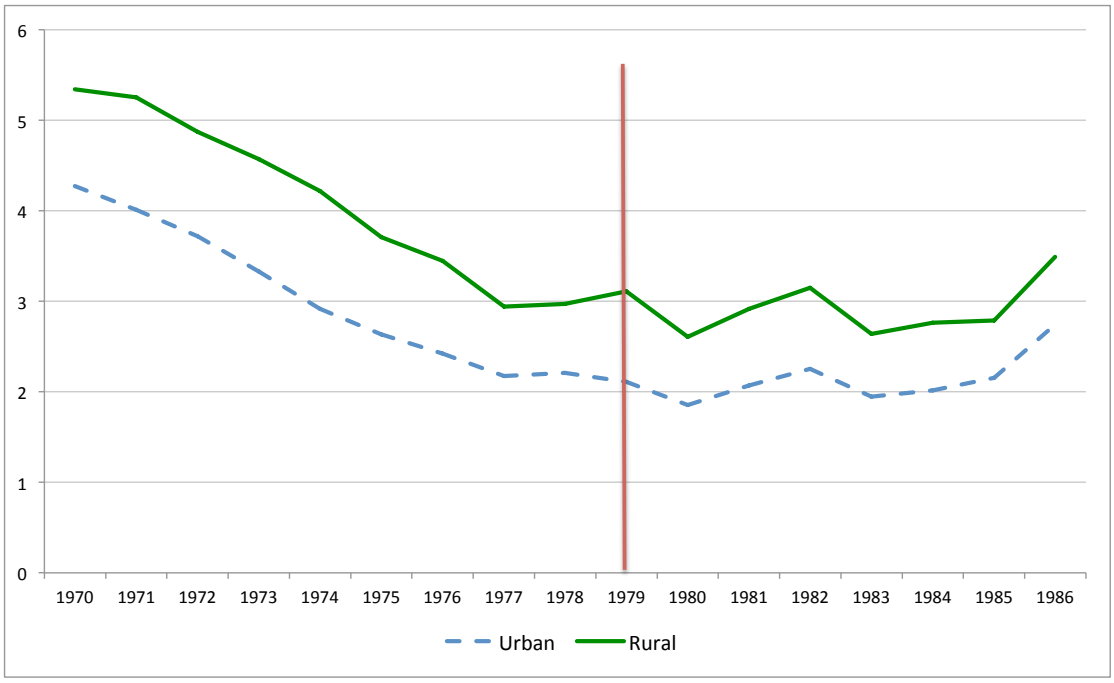
**Figure 1. Reported Total Fertility Rate: China 1970–2005, Unadjusted**




*Sources:* Guo (2004); NBS (1995–2006); Yao (1995).

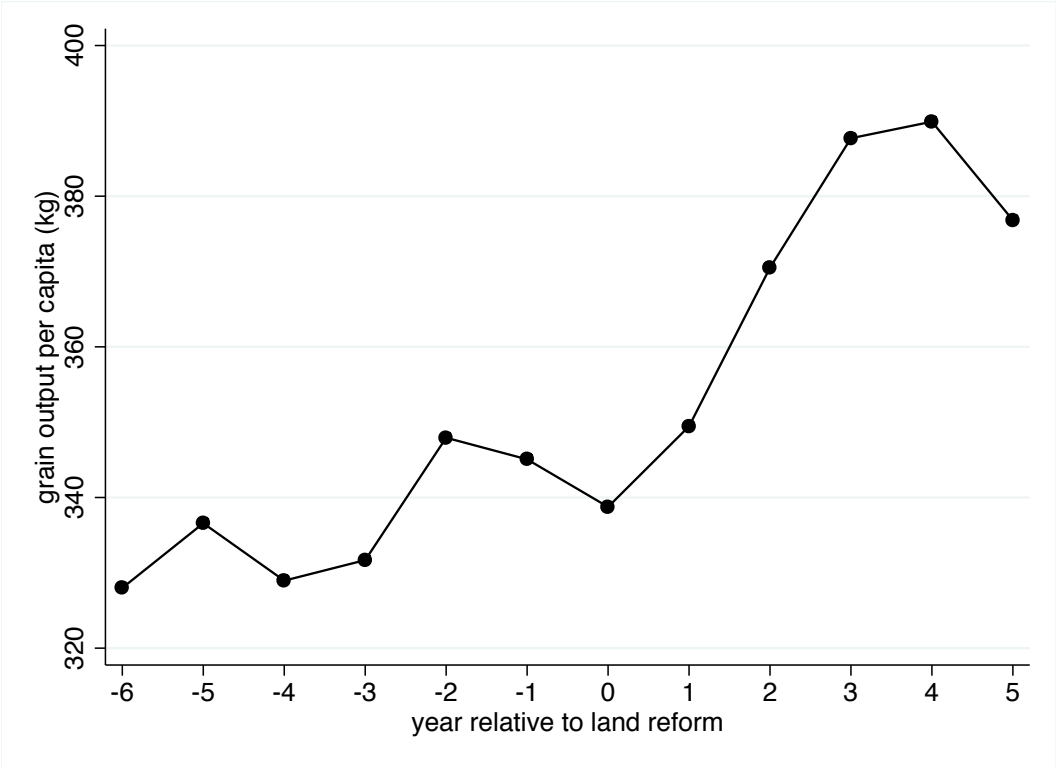
*Notes:* Data for 1970–1992 are from Yao's (1995) compilation: 1970–1981 data are based on the 1982 National One-per-thousand Population Sampling Survey on Fertility; 1982–1987 data are based on the 1988 National Two-per-thousand Population Sampling Survey on Fertility and Contraceptives; 1988–1992 data are based on the 1992 Fertility Sampling Survey in China; 1993 data are from Guo (2004), which is based on the 1997 National Survey on Fertility and Reproductive Health; 1994–2005 are from *China Population Statistical Yearbook* (NBS 1995–2006).

Appendix Figure 1B: Total Fertility Rate by Rural/Urabn, 1970-1986



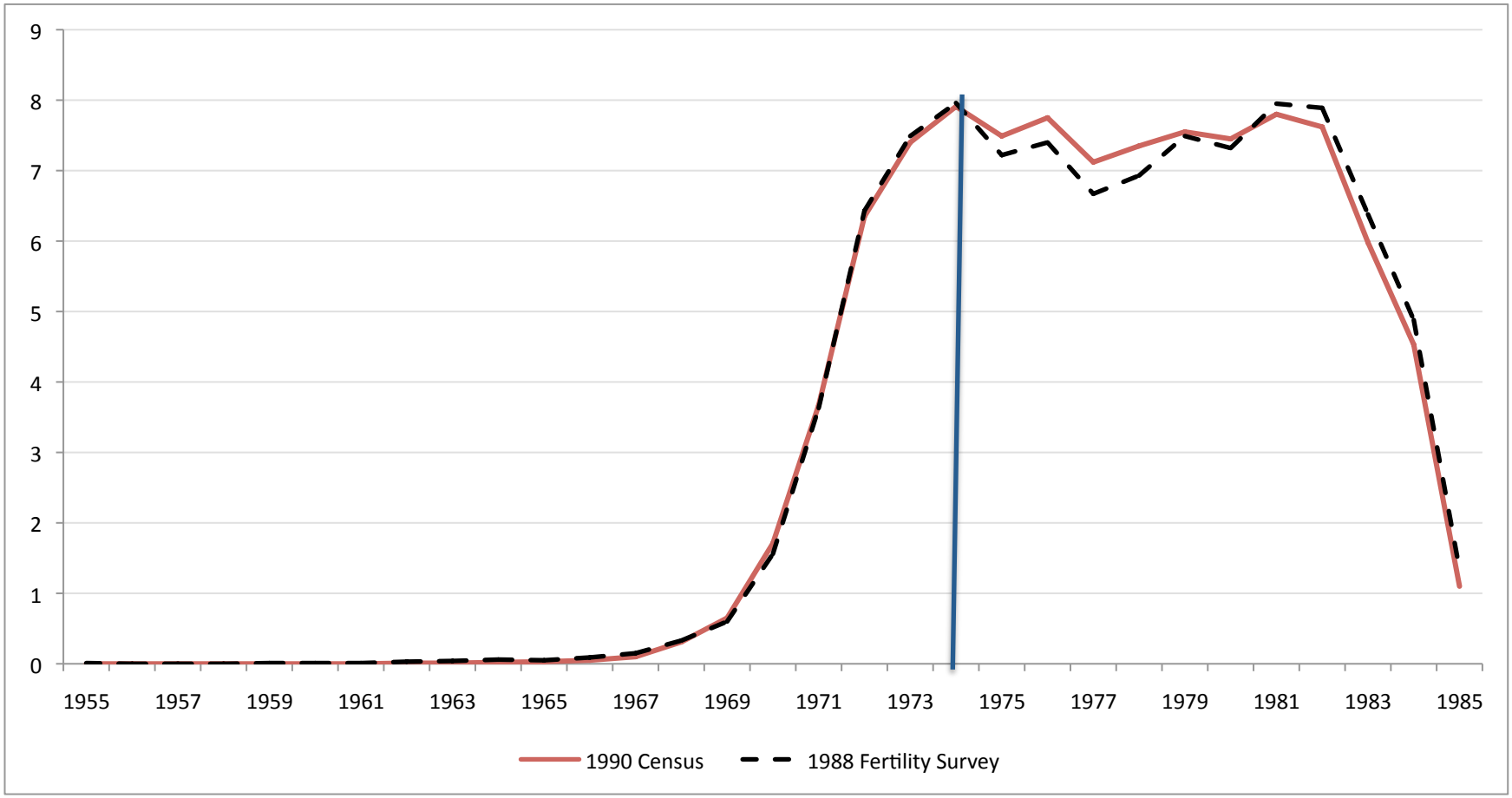

Note: Appendix Figure 1B is plotted by the authors using data from the 10% sample of the 1988 National Two-per-thousand Population Sampling Survey on Fertility and Contraceptives. The vertical line is at year 1979.

Appendix Figure 2: Grain output per capita



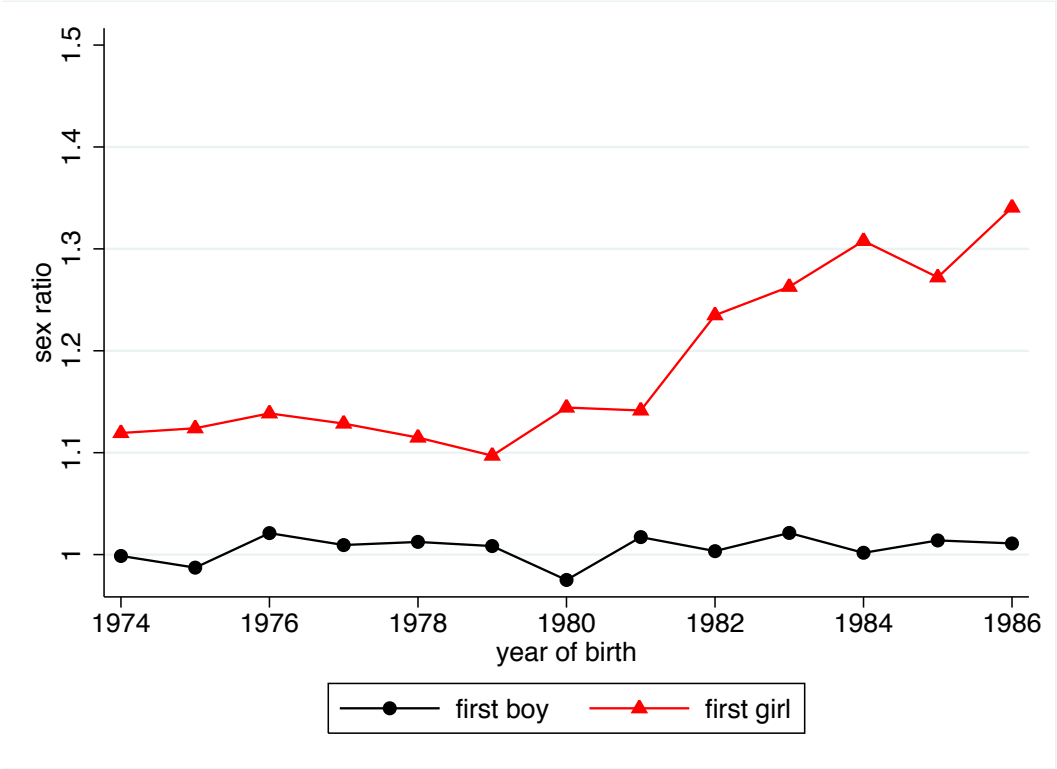

Note: The sample includes 400 counties that we have data on both land reform timing and grain output per capita from the 1970s to 1980s.

Appendix Figure 3: Frequency of birth year distribution of the first child




Note: The solid line is from the 1% sample of the 1990 Census. The dotted line is from the 10% sample of the 1988 National Two-per-thousand Population Sampling Survey on Fertility and Contraceptives.

Appendix Figure 4: Sex ratio of the second child, by year of birth



Appendix Table 1: Land reform, the OCP and the 1.5 Child Policy

| | **Male=1** |
|---|---|
| Land reform*Girl first | 0.028*** |
| | [0.008] |
| Land reform | -0.011 |
| | [0.007] |
| OCP*Girl first | -0.004 |
| | [0.008] |
| OCP | -0.002 |
| | [0.007] |
| 1.5 Child Policy*Girl first | 0.023*** |
| | [0.009] |
| 1.5 Child Policy | -0.012 |
| | [0.008] |
| Girl first | 0.026*** |
| | [0.003] |
| Observations | 224600 |
| R-squared | 0.011 |

Notes: 1.5 Child Policy is assigned 1 if one was born after the 1.5 Child Policy started in the province of birth and 0 otherwise. The sample includes all second births between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and OCP. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 2: Grain output by the fraction of educated workers

| | ln(grain output per capita) | | |
|---|---|---|---|
| Land reform*% High school | 0.008* | | |
| | [0.004] | | |
| Land reform*% Middle school | | 0.004** | |
| | | [0.002] | |
| Land reform*% Primary school | | | 0.001 |
| | | | [0.001] |
| Land reform | 0.022 | -0.009 | 0.034 |
| | [0.035] | [0.043] | [0.061] |
| Observations | 2,093 | 2,093 | 2,093 |
| R-squared | 0.906 | 0.906 | 0.906 |

Notes: Estimation in this table uses the sample of counties that are above the median of productivity change. All regressions control for county fixed effects, year effects, county-specific linear time trends, determinants of reform timing interacted with time fixed effects and droughts in March and April in year t and t-1. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 3: Treatment effect heterogeneity, by changes in grain output

| | Sample: 400 counties | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Full sample | Change in grain output above median | Change in grain output below median |
| | **Panel A: ln(grain output per capita)** | | |
| Land reform | 0.026* | 0.092*** | -0.039* |
| | [0.015] | [0.019] | [0.021] |
| Observations | 4,188 | 2,093 | 2,095 |
| R-squared | 0.874 | 0.905 | 0.818 |
| | **Panel B: Male=1** | | |
| Land reform*Girl first | 0.013* | 0.027*** | -0.004 |
| | [0.007] | [0.009] | [0.010] |
| Land reform | -0.015 | -0.029*** | 0.003 |
| | [0.010] | [0.013] | [0.017] |
| Girl first | 0.029*** | 0.027*** | 0.033*** |
| | [0.005] | [0.007] | [0.008] |
| Dependent variable mean | 0.521 | 0.524 | 0.519 |
| Observations | 93335 | 53243 | 40092 |
| R-squared | 0.011 | 0.011 | 0.013 |

Notes: Estimation in this table uses the sample of 400 counties that report grain data. Panel A reports reports estimates of land reform on log grain output per capita by county and year (1974-1984), and panel B reports estimates of land reform on second child being male at the individual level. Column (1) reports the estimate using the full sample, column (2) a subsample of counties above median of the change in grain output in capita before and after the reform, and column (3) a subsamle of counties below median. All regressions control for county fixed effects, year effects, county-specific linear time trends, determinants of reform timing interacted with time fixed effects and droughts in March and April in the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 4: County-level mean of male workers by crop in the 1982 Census

| | Obs | Mean | Std. Dev. |
|---|---|---|---|
| **A. county-leve mean of agricultural workers for each crop (all counties)** | | | |
| Grain | 1065 | 0.945 | 0.136 |
| Cash Crops | 1065 | 0.050 | 0.132 |
| Cotton | 1065 | 0.033 | 0.126 |
| Fruit | 1065 | 0.002 | 0.011 |
| **B. county-level mean of male workers for each crop (counties that grow some particular crop)** | | | |
| All Crops | 1065 | 0.519 | 0.026 |
| Grain | 1062 | 0.545 | 0.098 |
| Cash crops | 935 | 0.515 | 0.227 |
| Cotton | 232 | 0.348 | 0.236 |
| Fruit | 407 | 0.692 | 0.331 |

Notes: This table shows the summay statistics of county-level mean in the 1982 Census microdata. These counties can be matched with the county-level data on reform timing and the 1990 Census. The sample of individuals is restricted to agricultural workers. We use the unharmonized codes for occupation (OCC) and industry (IND) in the 1982 Census from IPUMS International to identify the crop an agricultural worker grows, e.g. fruit=1 if OCC==614&IND==14. We then obtain the county-level mean and report the mean and standard deviation across counties.

Appendix Table 5: Land size and gender

| | **Total amount of cultivated land for household (mu=1/6 acre)** |
|---|---|
| | A. Chinese Health and Nutrition Survey 1989 |
| % Male members | 0.002 |
| | [0.004] |
| Village FE | X |
| dependent variable mean | 3.1 |
| Observations | 2495 |
| R-squared | 0.438 |
| | B. Rural Fixed Point Survey 1986-1989 (Household-level Panel Data) |
| % Male labor | 0.002 |
| | [0.002] |
| dependent variable mean | 7.6 |
| Observations | 9,762 |
| No. of households | 2,460 |
| R-squared | 0.000 |

Note: in Panel A, village fixed effects are controlled for. In Panel B, we report household fixed effect estimator using household-level panel data from 1986 to 1989.

Appendix Table 6: Extension of land lease in 1984

| | **Male=1** |
|---|---|
| Land reform*Girl first | 0.030*** |
| | [0.008] |
| Land reform | -0.012* |
| | [0.007] |
| OCP*Girl first | -0.003 |
| | [0.008] |
| OCP | -0.003 |
| | [0.007] |
| 1{Born in 1984-1986}*Girl first | 0.013* |
| | [0.008] |
| Girl first | 0.025*** |
| | [0.003] |
| Observations | 224600 |
| R-squared | 0.011 |

Notes: 1{Born in 1984-1986} is assigned 1 if one was born in 1984-1986 and 0 otherwise. The sample includes all second births between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and OCP. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 7: Heterogeneity by grain output in 1977

| | Dependent variable: Male=1 | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Full sample | Grain ouput in 1977 above median | Grain ouput in 1977 below median |
| Land reform*Girl first | 0.017** | 0.022** | 0.011 |
| | [0.007] | [0.009] | [0.011] |
| Land reform | 0.002 | -0.01 | 0.017 |
| | [0.009] | [0.012] | [0.015] |
| Girl first | 0.027*** | 0.028*** | 0.025*** |
| | [0.005] | [0.006] | [0.007] |
| Observations | 99024 | 52633 | 46162 |
| R-squared | 0.011 | 0.011 | 0.011 |

Notes: Column 1 reports estimate for the effect of exposure to land reform on the probability of second child being male in the full sample; column 2 for the effect in counties above the median of grain output in 1977; column 3 for the effect in counties below the median. The sample includes individuals born between 1974 and 1986 in 400 counties that are matched with the county-level data on reform timing and grain output. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 8: Land reform and infant health (UNICEF 1992 Chinese Children Survey)

| | UNICEF 1992 Chinese Children Survey | | |
|---|---|---|---|
| | Neonatal mortality | Post-neonatal mortality | Birth weight |
| **Panel A: All births** | | | |
| Land reform | -0.0001 | -0.003** | 33.969** |
| | [0.002] | [0.001] | [15.320] |
| Observations | 107934 | 107934 | 28876 |
| R-squared | 0.015 | 0.019 | 0.158 |
| **Panel B: Second births** | | | |
| Land reform*Girl first | 0.002 | -0.002 | 10.811 |
| | [0.004] | [0.003] | [26.845] |
| Observations | 31892 | 31892 | 8598 |
| R-squared | 0.029 | 0.03 | 0.231 |

Notes: Using the UNICEF 1992 Chinese Children Survey, we report estimated effects of land reform on infant health outcomes. Panel A includes all births, and Panel B for the second births. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 9: Land reform and child mortality (UNICEF 1992 Chinese Children Survey)

| | Died in 1977-1986=1 (UNICEF 1992 Chinese Children Survey) | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | All | Male | Female |
| Land reform*Girl first | 0.000 | -0.016* | 0.016* |
| | [0.006] | [0.009] | [0.008] |
| Land reform | -0.001 | 0.013 | -0.014* |
| | [0.005] | [0.009] | [0.007] |
| OCP*Girl first | 0.000 | 0.01 | -0.017** |
| | [0.006] | [0.010] | [0.008] |
| OCP | 0.006 | 0.008 | 0.008 |
| | [0.005] | [0.008] | [0.007] |
| Girl first | 0.002 | 0.002 | 0.006 |
| | [0.003] | [0.005] | [0.006] |
| dependent variable mean | 0.028 | 0.029 | 0.027 |
| Observations | 31733 | 16817 | 14916 |
| R-squared | 0.045 | 0.062 | 0.082 |

Notes: Using the UNICEF 1992 Chinese Children Survey, we report estimated effects of land reform on child mortality of second births in 1977-1986. Column (1) reports the estimate for all second births, column (2) for male births and column (3) for female births. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.