THREE ESSAYS ON FERTILITY, LABOR MARKET PERFORMANCE, AND PARENTAL MENTAL HEALTH

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ABSTRACT

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In this dissertation research, the empirical analyses are developed to investigate the causal relationships among fertility, labor supply and parental health. As these three factors are closely intertwined, identifications of causality are achieved through various methods. The first and third chapters construct exogenous variation in fertility through a natural experiment, the One-Child Policy in China. Then the constructed exogenous variation is used as an instrument for fertility to identify the causals effects of fertility on female labor supply as well as parental mental health. The second paper analyses the impacts of job displacements on fertility. Several different specifications, including time trend model, fixed effect propensity score matching and regression with narrower definition of job displacement are used to verify the robustness of the causal effects.

I Chapter One, I try to answer the question, "Does fertility play a different role in female labor force participation in China than in the U.S.?" This chapter exploits plausibly exogenous variations in fertility created by the affirmative One-Child Policy in China to estimate the effect of having two or more children on the mother's labor force participation. Using a large data set from the 1990 Population Census, I find that OLS underestimates the negative effects of fertility, and 2SLS estimates imply that conditional on having one child, additional children decreases mother's female labor force participation by 8-15 percentage points in rural China. Recently, China relaxed its One-Child Policy to Two-Children Policy, our finding here provides a perspective for the potential effects of such policy relaxations on female labor supply.

Historical macro data show a negative association between unemployment and fertility. Individual level panel data is needed to explain the causal relationship behind the negative association. Using micro data from National Longitudinal Survey of Youth 1979, Chapter Two studies the effect of

job displacements on fertility in the U.S. After controlling individual time-invariant heterogeneity, the main regression results indicate that displacements of men will lead to reduced fertility in the following years, while the effect of displacements for women depends on the women's education levels. For women without college education, their fertility will increase four years after displacement. For women with college education, however, no significant effect on fertility is identified. The empirical findings are robust to several different specifications, including time trend model, fixed effect propensity score matching and regression with narrower definition of job displacement. There is an old saying, "More children, more blessings". Does having more children really promote mental health of the parents in China? To answer this question, Chapter Three exploits plausibly exogenous variations in fertility created by the affirmative One-Child Policy in China to estimate the long-term effect of having more children on the parent's mental health for people age 45 and above in rural areas. Using data from the 2011 China Health and Retirement Longitudinal Study (CHARLS), results show that, after controlling endogeneity in fertility, mothers with more children are found to have a higher probability of experiencing depression symptoms in rural China, while the effects on fathers are generally not significant.

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CHAPTER 1

FERTILITY AND FEMALE LABOR FORCE PARTICIPATION: EVIDENCE FROM THE ONE-CHILD POLICY IN CHINA

1.1 Introduction

Inspired in part by a contemporaneous increase in female labor force participation and a decrease in fertility in the U.S. (Figure B.1), many economists have investigated the effects of number of children on mother's labor supply (Klerman, 1999; Angrist and Evans, 1998; Bailey, 2006). Angrist and Evans (1998) and many others find negative effects of fertility on female labor force participation in the U.S.[1] On the other side of the world, China has implemented the One-Child Policy since 1979 aiming at curbing its fast population growth; however, surprisingly, with fewer children, it appears that women in China participate less in the labor force (Figure B.2). In this study, I examine the effects of fertility on female labor force participation in rural China.[2] Are the effects of fertility in China the same as found in the U.S., or are they opposite, as suggested by the information found in Figure B.1 and Figure B.2? This paper can help us answer this question.

Empirically analyzing the effect of fertility on female labor force participation is challenging due to the endogeneity of fertility decisions with respect to labor supply. Willis (1974) shows that female labor force participation and fertility are always jointly determined. For example, if omitted variables such as women's preferences for work are negatively correlated with their preferences for having more children, or if the time and effort spent on work discourages fertility (generating

[1] A negative correlation between fertility and female labor force participation has been found both over time and cross countries (Agüero and Marks, 2011).

[2] I exploit two sources of variation in fertility that arise due to the One-Child Policy: gender at first birth and ethnicity. Since gender at first birth is relevant for the implementation of the OCP only in rural areas (Qian, 2009), I focus on rural China for this study. Moreover since the penalties imposed on those who exceed their quota in urban areas may involve job loss, the OCP may directly affect labor force participation in these areas and thus I exclude urban areas from this analysis.

reverse causality), then the estimates of the effect of number of children on female labor supply would be biased downward. The bias could also go in the opposite direction; for example, some have found that fertility in developing countries is determined through a collective bargaining process at the household level and household members with more bargaining power have more influence on the total number of children (Rasul, 2008). In addition, men are generally found to prefer more children than women in developing countries (Mason and Taj, 1987). If women with less bargaining power are forced to work as well as have more children, then naive OLS regressions will underestimate the negative impact of children on mother's labor force participation.

To address this endogeneity problem, economists have exploited various sources of exogenous variation in family size such as twinning (Rosenzweig and Wolpin, 1980; Jacobsen et al., 1999), sex composition of the first two births (Angrist and Evans, 1998),[3] state-level access to contraception (Bailey, 2006), and state-level abortion laws (Klerman, 1999; Levine et al., 1999; Angrist and Evans, 2000). All of these U.S.-based studies found solid evidence of a negative effect of fertility on female labor supply.

In contrast, studies based on developing countries show mixed results on the effect of fertility on maternal labor supply. Using data from a randomized social experiment in Matlab, Bangladesh, Schultz (2009) found a negative effect for family planning programs (which lead to lower fertility) on the likelihood of "work for pay" for women. Using son-preference[4] as an instrument, Ebenstein (2009) indentified negative effects of fertility on maternal labor force participation in Taiwan. Based on data from Demographic and Health Surveys covering 59 developing countries (excluding China), using son-preference, the sex composition of first and second born children, and twinning as instruments, Porter and King (2010) reported that while in many developing countries women are less likely to work when they have more children, in some countries, mothers with more children are more likely to work due to reasons such as the financial costs of raising more children.

[3]They found parents of two children of the same sex have higher probability of having a third birth than parents of one boy and one girl.

[4]In east Asian countries, families are found to have strong preference for male children. In particular, families with two girls have higher probability to have third child.

Combining data from Demographic and Health Surveys with abortion legislation documents in each country, Bloom et al. (2009) confirmed these mixed results on the effects of fertility. In a cross-country study, Agüero and Marks (2011) analyzed 26 low- and middle-income countries, and using infertility as an instrument for family size. They found that number of children has no effect on labor force participation and work intensity.

I am aware of only one existing study on the impact of fertility on female labor supply in the context of China. Based on information from 1,315 women from the China Health and Nutrition Survey conducted in 1993, Lee (2002) used the sex of the first child as an instrument[5] and found no significant effects of fertility on rural female labor supply in China.[6] The main concern when using son-preference as an instrument for total fertility is that the gender of the first-born child may not satisfy the exclusion restriction condition, since previous work has shown that children's gender affects mothers' labor supply directly (Cruces and Galiani, 2007).

In order to curb rapid population growth, the Chinese government has implemented the One-Child Policy (OCP) since 1979, possibly the largest social experiment in human history. Under this policy, a married couple can have only one child in most areas of the country. However, there are several important exceptions. The first is that the OCP was not applied to non-Han Chinese until 1988. The second exception arose when, in some areas, it was observed that the OCP was associated with female infanticide, forced abortion and forced sterilization. To prevent these extreme cases, 19 provinces adopted the "1-boy-2-girl" rule in 1984, which stated that rural couples in these provinces were allowed to have a second child if the first child was a girl (Qian, 2009).

In this paper, I exploit two sources of exogenous variation in fertility generated by the One-Child Policy in China. More specifically, I use the differences in the policy's implementation

[5]With strong son-preference, the parents in China are found to have more children when they have no sons yet (Ebenstein, 2009).

[6]Lee (2002) also attempted to use the One-Child Policy, in particular, "local family planning rules" as instruments, but he admitted this instrument was potentially endogenous. I discuss this in more detail in Section 3.3.

between the Han and non-Han Chinese, as well as between rural couples with first-born girls and those with first-born boys in the 19 provinces that adopted the "1-boy-2-girl" rule to instrument for the likelihood a woman has two or more children. Thus the identification comes from variation in fertility generated by the OCP across cohorts of women (those affected and not affected by the policy during their fertile years), ethnicities (Han and non-Han), and families based on the gender of their first-born children. Assuming that without the variation in the OCP the differences between women in the unaffected (old) and affected (young) cohorts would be the same for the Han and non-Han, the differences-in-differences estimates based on ethnicity will capture the exogenous variation of fertility due to the variation in the OCP. Similarly, under the assumption that the differences between women in different age cohorts would be the same for women with either first-born boys or first-born girls without the variation in the OCP, DID estimates based on gender of first birth can capture the exogenous variations of fertility generated by the OCP. Using a large random sample from the 1990 China Population Census, the results show that OLS regressions underestimate the discouraging effects of children on female labor force participation. After accounting for the endogeneity of fertility, the likelihood of labor force participation for mothers with two or more children is 8 to 15 percentage points lower than for mothers with only one child in rural China in 1990.

This paper makes several contributions. First, it aims to fill the knowledge gap on effects of fertility on female labor supply in China, the most populous country in the world with a controversial population control. Along with the substantial drop in fertility experienced over the last 30 years, China witnessed a decline in female labor force participation, raising doubts as to whether the observed negative relationship between fertility and female labor force participation observed in developed countries extends to the developing country setting. By identifying the causal effects of fertility on female labor force participation in China, this research can enrich our understanding of the effects of fertility on female labor supply.

Second, the majority of past research focused on the effects of an additional child for the select sample of women who have had two children. We know little about the effect of increasing the number of children from one to two, which is important since some research shows that the effect of increasing the number of children is nonlinear (Black, Devereux, and Salvanes, 2004)(Figure G.1 confirms that in our dataset, the correlation between number of children and female labor force participation is nonlinear).

Third, there is an important limitation of using both same sex and twining as instruments to identify the effects of fertility on female labor force participation. Both strategies only identify the Local Average Treatment Effect (LATE) for women whose underlying desire to have more children is low. Using same sex as the instrument to family size estimates the LATE for women whose underlying desire to have more children is low but who are induced to have an additional child to balance the sex composition of the family. Similarly, using twinning as the instrument, estimates the LATE for women who prefer to have small families but happen to have an additional child due to "luck" (Agüero and Marks, 2011). Using an exogenous variation in the implementation of the One-Child Policy allows us to look at the LATE for women who have a high personal demand for children but are restricted by the policy. Considering the relatively higher demand for children in developing countries, the compliers in our strategy may be a more relevant population.

The remainder of the paper is laid out as follows. Section 2 provides background information on China's One-Child Policy. Section 3 introduces the estimation strategy and the construction of the instruments for family size. Section 4 describes the data and two sets of basic differences-in-differences estimates. Section 5 discusses the main regression results and explores the heterogeneous effects of fertility for women with different levels of education. Section 6 verifies the validity of the instruments. Section 7 provides additional robustness checks for the main results. Section 8 concludes.

1.2 The One-Child Policy in China

Between 1962-1970, the population growth rate in China reached 27.5‰ per year, and the total population reached 816 million in 1970 (Yang, 2004). To alleviate the social, economic, and environmental problems caused by increasing population pressure, the Chinese government began to curb population growth as early as 1972 using a policy known as “Later, Longer, and Fewer”. This policy encouraged people to get married and have children at a later age (the recommended age of first marriage was 25 years or above for men and 23 years or above for women), suggested longer birth spacing (at least three or four years), and also recommended that couples have at most 2 children. Implementation of this policy relied primarily on propaganda and social pressure (McElroy and Yang, 2000). As a result, the total fertility rate in China started to decrease in 1972 (Figure B.3).

In 1979, China initiated the well-known “One-Child Policy" (OCP), the implementation of which was more forceful . Under this policy, married couples were allowed only one child in most areas, except for those living in rural areas in five provinces (Hainan, Yunan, Qinghai, Ningxia, Xinjiang), who were allowed to have two children (Peng, 1996).[7] In practice, implementation of this policy in some regions began as early as in 1978, and enforcement was tightened nationally in 1980. In areas subjected to the OCP, a second birth was permitted only if the one child would cause the household “real difficulties”, such as poor health conditions of the first child. Couples who had an above-quota birth without permission were subject to penalties such as severe fines, job loss, and loss of access to public goods.[8] Local cadres were given economic and promotion incentives to implement the policy.

As a result of the OCP, in the early 1980s “parts of the country were swept by campaigns of forced abortion and sterilization and reports of female infanticide became widespread” (Greenhalgh, 1986). To prevent female infanticide, forced abortion and forced sterilization, and to better address region-specific conditions, the Central Party Committee issued “Document 7” in April 1984, allowing regional variation in family planning polices. The main change in the policy fol-

[7]There are no restrictions on number of children for rural couples in Tibet.

[8]In urban China, where most work in state-owned enterprises, and make use of public goods, job loss and lost access to public goods are the most commonly used punishments. In rural areas, where most people work on their own land and private enterprises, the one-time fine is the primary penalty used by local government officials. There is local variation in fines (Wei and Zhang, 2011).

lowing "Document 7" was the "1-boy-2-girl" rule in 19 provinces, which allowed rural couples in these 19 provinces to have a second child if the first-born was a girl (Qian, 2009). According to White (1991), these kinds of permissions began to be issued as early as 1982, suggesting that relaxation existed even before 1984.[9]

An important feature of the OCP was that it was only applied to Han Chinese before 1988. This affirmative rule had been made at the conception of the birth-control policy in China and recorded in all documents related to population policy (Peng, 1996). In 1988, when the population of the Zhuang ethnic group reached 10 million, they became subject to the OCP as well. Ethnic Manchus were similarly added in 1990 (Li, Zhang and Zhu 2005). With important variation across groups, the One-Child Policy gives us an opportunity to investigate the effects of exogenous change in family size.

1.3 Estimation Strategy

Following the literature, the main regression model we are interested in can be written as:

$$LFP_{ict} = \beta kids2_{ict} + \mathbf{X'_{ict}}\delta + \alpha_1 + \gamma_t + \psi_c + \varepsilon_{ict} \tag{1.1}$$

where LFP_{ict} is the labor force participation indicator for woman i in county c, age cohort t. Follow Maurer-Fazio, Hughes and Zhang (2005), $LFP_{ict} = 1$ if the woman had a job on the day of the census or if she was unemployed but was coded as "waiting for work". It would be ideal if we can measure women supply in both extensive margin and intensive margin. However, with the limited information provided in 1990 China Population Census, I focus only on the extensive margin in this study. $kids2_{ict}$ is a dummy variable equal to 1 if the woman has two or more children;[10]

[9] As the OCP got tightened nationally in 1980, and the "1-boy-2-girl" appeared as early as 1982, I do not expect the 1984 amendment to generate two separate cohorts with large differences. Mothers with first-born girls who were not allowed to have a second birth under the original OCP can continue to have the second child after the implementation of "1-boy-2-girl" rule.

[10] All women in our sample have at least one child; therefore, $kids2_{ict} = 0$ means the woman has only one child.

$\mathbf{X_{ict}}$ is a vector of woman i's characteristics, including her age, age at first birth, ethnicity, gender of the first child, and education levels for both her and her husband;[11] γ_t is the age cohort fixed effect, and ψ_c denotes the county fixed effect. We use the dummy variable $kids2_{ict}$ to measure fertility rather than number of children, since our DID estimates only apply to the discrete change from one child to two or more children. Angrist and Imbens (1995) indicates that the resulting estimated effects will be bigger than the true average per-unit effect when the treatment variable is incorrectly parameterized as binary, while the sign of the Average Causal Effect is still consistently estimated. As the effects of children on female labor supply are likely to be non-linear (Figure G.1), we take caution in interpreting our estimates of the coefficient on $kids2_{ict}$.

Women's labor force participation may affect fertility decisions and there might be unobserved factors (e.g. health) that affect LFP_{ict} and $kids2_{ict}$ simultaneously. For these reasons and others, we believe that the condition $cov(kids2_{ict}, \varepsilon_{ict}) = 0$ does not hold. As a result, the OLS estimator of β is not consistent. To address this endogeneity problem, I use two sets of differences-in-differences (DID) strategies to construct exogenous variation in fertility, and then use this variation to instrument $kids2_{ict}$ in Equation (1). The first DID strategy exploit the differences in the probability of having two or more children between Han and non-Han Chinese, for women affected and unaffected by the OCP; while the second strategy compares the differences between women with first-born girls and women with first-born boys, for affected and unaffected cohorts.

1.3.1 DID using ethnicity

As described in Section 2, before 1988 only Han Chinese were subject to the strict One-Child Policy, while non-Han couples were allowed to have two children. One attractive estimation strategy, therefore, is to use ethnicity to capture the exogenous variations in number of children. Unfortunately, knowledge about China as well as summary statistics shown in Section 4 suggest that there are many systematic differences between Han and non-Han Chinese. Therefore, one would

[11] Table A.1 reports the definition of the key variables (dependent variables, covariates, and instruments).

worry that ethnicity might directly affect women's labor force participation decisions, and thus the exclusion restriction will not hold if we use ethnicity alone as an instrument.

Using DID can help remove the first order differences between Han and non-Han Chinese. The DID method can be simply expressed as $(nonHan, After - Han, After) - (nonHan, Before - Han, Before)$. Under the assumption that without the OCP, the difference in female labor force participation between affected and unaffected cohorts would be the same for Han and non-Han individuals, DID estimates will capture the exogenous variations of $kids2_{ict}$ due to the variation in the OCP, which affects different ethnic groups differently.

Here, we use $After$ to represent females restricted by the OCP, that is, the young cohorts in our sample. $Before$ denotes the cohort not constrained by the OCP, that is, the old cohorts who probably had two or more children already before 1979/1980. Though the One-Child Policy was announced in 1979, there is no simple distinction between exposed/treated and non-exposed/non-treated individuals, as people choose their fertility timing differently. Figure G.3a depicts the cumulative distribution of age at second birth for the restricted provinces sample. It shows that over 90% of women with two or more children gave birth to their second child before the age of 30. Considering 1980 as the year that the OCP became nationally implemented, I assume that women older than 30 in 1980 were relatively less-constrained by the OCP. The cutoff age of 30 in 1980 means that the cutoff age is 40 in 1990. Thus, women older than 40 in the 1990 census will be regarded as $Before$ cohorts, while women age 40 or younger will be the $After$ cohorts. In regressions, rather than arbitrarily dividing the women into these two cohorts, I use age cohort dummies to allow for the most flexibility in the effect of the OCP on fertility. Using DID to capture the exogenous variation in fertility caused by exogenous variation in policy change is fundamentally equivalent to using interaction terms. Hence, the first stage regression of $kids2_{ict}$ on the interaction terms will have the following form:

$$kids2_{ict} = \sum_{l=16}^{44} (nonHan_{ict} \cdot d_l)\,\rho_l + \mathbf{X'_{ict}}\kappa + \alpha_2 + d_t + \theta_c + u_{ict} \tag{1.2}$$

where d_l, $l = 16 to 44$ are age dummies from 16 to 44 years of age.[12]

1.3.2 DID using gender of first-birth

In 1984, to prevent female infanticide, forced abortion and forced sterilization, an amendment to the original OCP was implemented in 19 provinces in 1984. Under the amendment, a rural couple is allowed to have a second child if their first child is a girl, also known as the "1-boy-2-girl" rule. This amendment provides us with another DID strategy to construct the exogenous variation in fertility.

Cruces and Galiani (2007) find that the gender of children directly affects women's labor supply in Mexico and Argentina. This might be true in Asia, especially in China, as well. In rural China, where the preference for sons is strong, boys are valued more highly than girls. It might be expected that the mothers with sons would spend more time on childcare than mothers with daughters. In this case, the gender of children will directly affect a mother's labor supply. However, we can remove the direct effect of first-born gender on female labor supply using DID. The DID method can be expressed as $(First\text{-}Born\ Girl, After - First\text{-}Born\ Boy, After) - (First\text{-}Born\ Girl, Before - First\text{-}Born\ Boy, Before)$. As above, $After$ represents the treated or young cohort, while $Before$ denotes the control or old cohort. In all regressions, I include age cohort dummies. The key assumption in this DID strategy is that without the variation in the OCP, the difference in female labor force participation decision between treatment and control cohorts would be the same for women whose first child is a son and those whose first child is a daughter. My first stage regression using interaction terms of age cohorts and gender of first birth is thus:

$$kids2_{ict} = \sum_{l=22}^{44} (First\text{-}Born\ Girl_{ict} \cdot d_l)\, \phi_l + \mathbf{X'_{ict}}\mu + \alpha_3 + d_t + \pi_c + v_{ict} \tag{1.3}$$

where d_l, $l = 16 to 44$ are age dummies from 16 to 44 years of age.

[12]Reasons for using these cohorts are discussed in Section 4.

1.3.3 Other research using the OCP to construct instruments for fertility

Although there exists only one study exploring the effects of fertility on female labor force participation in China, the strategy of using the variation in implementation of the OCP to instrument for fertility has been widely adopted in several studies exploring the effects of fertility on other outcomes in China. Short and Zhai (1998) show that in practice, the implementation of the OCP varied geographically. Some of the research tries to exploit these spatial variation in the implementation of the One-Child Policy. For example, in her study of the effects of number of children on school enrollment of the first-born, Qian (2009) used the implementation of the "1-boy-2-girl" rule at county level interacted with year and gender of first birth as instruments for number of children. However, when Lee (2002) tries to use local family planning rules as instruments for two or more children, he finds that the implementation of the "1-boy-2-girl" rules at the county level is significantly correlated with community location and infrastructure. Communities located far away from cities and/or with poor infrastructure are more likely to implement this rule. Therefore, these local rules may be endogenous to local labor market, so we do not use them as an instrument in our study.[13]

In other related studies, researchers use the year of the first birth to instrument for fertility. To estimate the effect of number of children on elderly parents' health, Islam and Smyth (2010) use the interaction terms of a rural/urban indicator and three period dummies corresponding to the year

[13] Lee (2002) mainly exploited three local rules, i.e., under three different scenarios, whether a community allows a couple to have a second child or not. The three scenarios are: 1) the first born child is a girl; 2) both husband and wife are from a one-child family; 3) one parent has "dangerous" job. Only first rule satisfies "relevance" as an instrument. However, when using both the first rule and gender of first child as instruments, Lee finds different results from using gender of first child as the only instrument. As a result, over-identification test is rejected. Assuming gender of first-birth is exogenous, Lee claimed that the implementation of the "first rule" at the county level is not a valid IV. He also tried to put "first rule" directly in OLS regression of number of children, and generated a significantly negative coefficients. In addition, he found that county-level "first rule" implementation is significantly correlated with community location and infrastructure. Communities located far away from cities and/or with poor infrastructures are more likely to implement this rule. Put all these together, he claimed local "1-boy-2-girl" rule at the county level is endogenous to the local labor market.

of the first birth. When investigating the effects of fertility on parent's saving behavior, Banerjee, Meng and Qian (2010) use a dummy for whether the first birth was before 1972 to capture the effect of the "Later, Longer, Fewer" policy on the number of children. The potential problem with this IV strategy is that the year of the first birth for a given woman is determined by the couple, and thus can be endogenous.[14]

To examine the impact of family size on mothers' health, Wu and Li (2012) use the interactions of a non-Han dummy and a measure of time exposure to the OCP. This strategy is similar to the one implemented in this paper. In particular, they assume that the effects of exposure to the policy is linear[15] and they choose an arbitrary year when the policy starts to take effect. Using our data, I can show that allowing for more flexible effects of the OCP on each age cohorts generates similar but different results from using their strategy (Table G.1 indicate that using identification strategy in Wu and Li (2012) generates less precise estimate for the effect of children on female LFP, and Table G.2 shows the regression results when using a modified strategy based on gender of the first-born, with the confidence interval for $\hat{\beta}_{2SLS}$ as wide as (-1.70, -0.019), surpassing the limit of -1).

Our DID method using ethnicity is motivated by a study by Li et al. (2005). In order to measure the effect of the OCP on fertility,[16] Li et al. (2005) use the interactions of a mother's birth cohorts and a Han dummy to identify the exogenous variation in number of children due to the OCP. Their robustness tests show that changes in other household decisions, such as marriage and the decisions whether to have any children, are not systematically different between Han and non-Han people during this period.

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[14]In the sample we use in this paper, IV results are very different when using year of first birth dummies instead of age cohort dummies, even after controlling for the mother's age at first birth. Regression results available upon requests.

[15]Figure B.5a shows this is not true, especially when including very young age cohorts.

[16]They use DID directly, rather than apply it as IV.

1.4 Data and Descriptive Statistics

The data used in this paper comes from a 1% sample of the 1990 China Population Census, the fourth census in China. For studies on fertility and labor supply, census data has the distinct advantages of large sample size and national representation. For this study, which relies on exposure to the One-Child Policy, the 1990 Census has three particular advantages. First, if we use the 2000 census to do the analysis, our control cohorts would be 50 years old at the time of the survey. Though 55 is the official retirement age for women in China, many women retire as early as 50 (Maurer-Fazio et al., 2011). As a result, the decision of whether or not to work and how it relates to fertility is not likely to be relevant for the control group. [17] Second, though there was a 1982 Census, some aspects of the OCP and important exceptions (including the 1-boy-2-girl rule) were not implemented until after 1982. Third, before the 1990s, the household mobility in China was almost zero due to the very strict household registration (Hukou) system. This helps to reduce the concern that families endogenously migrate in response to the OCP.

The 1990 China Population Census contains limited but essential information at the household and the individual level, including: age, sex, ethnicity, relationship to the household head, geographic location (at county level), education, employment status, marital status, and childbearing status. Detailed information about the census can be found in Wang (2000). Only three labor supply related questions are included in 1990 censuses: employment status, industry and occupation. I follow Maurer-Fazio et al. (2005) to define my dependent variable, the female labor force participation (LFP). $LFP_{ict} = 1$ if a woman has a job on the day of the census or if she is unemployed but is coded as "waiting to be employed".[18] Two types of childbearing questions were asked for

[17]The effects of number of children on female LFP depend heavily on the age group. Angrist and Evans (1998) shows that the negative effects of children on female labor supply will disappear for children age 13 or older.

[18]For individuals aged 15 and above, 1990 Population Census first asked their industries and occupations. For those who did not answer these two, they were asked questions further about their non-employment status, with choices listed as: 1. currently enrolled; 2. student; 3. housework; 4. waiting for schooling; 5. waiting to be employed; 6. retired/resigned; 7. lost ability to work; 8.other.

women aged 15 to 64: the birth history in the previous year and the number of sons and daughters ever born and the number of surviving. No other retrospective fertility information is collected in the 1990 Census. Similar to Angrist and Evans (1998), I match children to mothers within the households to get detailed information for the children.

The differences between urban and rural areas in China are huge. As noted in Section 2, the implementation of the One-Child Policy in urban areas may have direct impacts on female labor demand (as the most commonly-used penalty for above-quota birth is the job loss), which might confound the 2SLS estimates. Therefore, this paper will focus on the effects of family size on female labor force participation in *rural* China only. I include the households that are registered as agricultural households and also resided in the countryside in the study sample.[19] In order to link the children to their parents, the sample is restricted to women who are heads of the households or spouses of the household heads, with at least one child. I discard a small number of observations for which the age of the mother at first birth was under 15, as well as the observations for which the age of the first-born child is less than 1(Angrist and Evans, 1998; Cruces and Galiani, 2007). As older children are more likely to leave the household, existing literature usually restricts the sample to women less than or equal to 35 years old (Angrist and Evans, 1998; Cruces and Galiani, 2007). Unfortunately, I cannot follow that rule here. In order to implement differences-in-differences I have to include control cohort mothers who were older than 29 in 1980. This control cohort is aged 40 or older in 1990. Therefore, instead of 35, this study extends the upper bound for mother's age to 45. Angrist and Evans (1998) show that their results are not sensitive to increasing the upper bound age from 35 years old to 45 years old, and they found their results are not sensitive to that sample selection rule. In our sample, Figure G.2 shows that in the 1990 census for rural China, the average number of children is 3.3 for women aged 45. This is consistent with the trends of total fertility rates shown in Figure B.3, implying that the moving-out of children for mothers younger than 45 should not be a big concern. In addition, mothers for whom the number of linked children

[19] See two criteria for classifying the population into rural versus urban in Wang (2000). Here I use both.

did not match the reported number of surviving children are excluded.[20]

With the restrictions above, I obtained a sample of 824,609 women in 29 provinces. A.1 reports the definitions of the key variables (dependent variables, covariates, and instruments) and their summary statistics for the whole sample. The data show that a typical rural Chinese mother aged between 16 and 45 had an average 2.22 children in 1990. More than 75% of mothers in rural China at that time had two or more children. Among them, 9% are minority ethnic groups. And about 48% of the first-born children are girls. About 78% of these rural women have no more than primary school education, and the average education level of the rural women is lower than that of their husbands. The labor force participation of rural women is over 92%, which is higher than the findings in Maurer-Fazio et al. (2005), where the rural female LFP for women over 15 years old in 1990 is found to be 80.3%. The reason for the higher female LFP in this study is that we focus on women 45 or younger who have a relatively higher LFP rate. China is observed to have the highest female labor force participation in the world. People may think this is the result of the highly restricted population policy, however, as shown in Figure B.2, the female labor force participation was even higher in the 1970s before the One-Child Policy. There are at least two possible reasons for the historically high level of female labor force participation in China.[21] One possibility is that under the Communist rule in China, women were granted equal status to men through a series of laws. The other possibility is that individual labor allocation was not an individual choice before 1978. For example, in rural areas, people were assigned to work in collective agriculture by the village collectives; thus almost every adult had to work. For more details about the labor force participation in China, refer to Maurer-Fazio et al. (2005).

[20] This might cause a sample selection problem. To avoid sample selection issues, we can use the total reported fertility (survey question asked of all women) regardless of whether the children still live at home. The cost of this strategy which does not need to match mothers with their children is that I cannot get information for gender of the first-born. I tried this strategy for ethnicity DID, and the regression results are very similar to our main results when using DID based on ethnicity. Regression results available upon requests.

[21] In 2010, the female labor force participation rate in China was 67%, ranked first out of 35 countries (Hilda L. Solis, 2012).

As described in Section 2, different provinces in China enforced the One-Child Policy in different ways; thus I further divide this whole sample into three subsamples subject to different policy implementations. The first subsample is the “2-children provinces”, which consist of five provinces (Hainan, Yunan, Qinghai, Ningxia, Xinjiang) where all rural couples are allowed to have two children. In these provinces, there should be no differences between Han and non-Han couples and first-born boy and first-born girl couples in probability of having two children. This allows us to perform placebo tests using observations from this subsample to validate our instruments. Second, “1-boy-2-girl provinces” are the 19 provinces with an amended OCP, allowing rural couples to have a second child if the first child is a girl. We can employ the DID strategy along the dimension of the gender of the first-born child for this subsample. Third, “1-child provinces” include three municipalities (Beijing, Shanghai, and Tianjin) and the remaining two provinces (Jiangsu and Sichuan). In these provinces of high population density, all non-minority rural families are allowed to have only one child, without exception. In both the second and third groups (restricted provinces, hereafter), regardless of the “1-boy-2-girls” rule, the OCP is more strictly applied to ethnically Han Chinese, so I perform the DID according to ethnicity on this combined subsample. Table A.2 gives the summary statistics of major variables for each sample, and Table A.3 further compares the characteristics of Han and non-Han mothers in restricted provinces (“1-boy-2-girl provinces” and “1-child provinces”), as well as characteristics of mothers with first-born girls and first-born boys in “1-son-2-girl provinces”.

If people are allowed to move, couples with a stronger preference for a bigger family size might move to “1-boy-2-girl provinces” or even “2-children-provinces”, and this will contaminate our estimates of the effects of OCP on fertility and therefore the estimates of the effects of family size on female LFP. This is not likely a concern in our sample. Due to the strict household registration (Hukou) system, people were prevented from moving in general before 1990. In the 1990 Census, people were asked about their permanent residence in 1985 and 99.13% of the rural sample reported to live in the same province as in 1990 (97.76% reported to live in the same county). Therefore, we do not believe our analysis suffers from endogeneity caused by people’s preference and selective migration.

Table A.2 shows that, compared to the 1-boy-2-girl provinces, there are more minorities in the 2-children provinces, and fewer minorities in the 1-child provinces. In terms of the average number of children and the probability of having two or more children, 2-children provinces have a higher average and probability than 1-boy-2-girl provinces, which have a higher average and probability than the 1-child provinces. 1-child provinces, meanwhile, have higher female labor force participation than 2-children provinces (though not significantly), and 2-children provinces have higher female labor force participation than 1-boy-2-girl provinces.

Table A.3 shows that non-Han Chinese have more children and a higher probability of having two or more children than Han Chinese in the 1-child and 1-boy-2-girl provinces, while mothers with first-born girls are more likely to have additional children and bigger families in 1-boy-2-girl provinces. There are no significant differences in the age patterns of giving birth for mothers in these groups, especially for the timing of the second child, thus the interaction terms in the regression will mainly capture the variations due to the policy change rather than the differences in birth timing preferences (also seen in Figure G.3a). In general, however, Han and non-Han women have some different features. For example, the average education level for Han people is significantly higher than for non-Han people. DID is needed, therefore, to remove these level differences.

Table A.4 and Table A.5 show our basic DID estimates of having two or more children and labor force participation, based on ethnicity and gender of first-birth respectively. Though the One-Child Policy had an explicit implementation date of 1980, there is no simple distinction between no treatment and treatment for each individual, as people choose their fertility timing differently. As discussed in Section 3, I categorize mothers into treated and pre-treatment cohorts based on their ages. The cutoff age of 30 in 1980 means that the cutoff age is 40 in 1990. Women older than 40 but younger than 46 in 1990 census will be regarded as “Old (Pre-treatment) Cohorts”. On the other side, the “Young (Treatment) Cohorts” in Table A.4 and Table A.5 include mothers 40 years old or younger in 1990, i.e., age cohorts 16 to 40. The lower bound is 16, because we exclude women who had children when less than 15 years old and women whose first child was less than 1. This distinction of treatment status is consistent with the results from the regressions with a full set of age cohort dummies in Section 5.

Table A.4 shows that the probability of having two or more children decreases for both Han and non-Han Chinese after the One-Child Policy, but decreases significantly more for Han people by 3.7 percentage points. Although the fertility of the non-Han population was not officially constrained to only one child, there are other factors such as increased income, a monetary bonus for one-child families, and easier access to family planning services, that might have reduced overall fertility in this period, regardless of ethnicity. The right panel of Table A.4 shows that the DID estimate of the reduced form effect of the OCP on the labor force participation rate is -0.029. Similarly, Table A.5 shows that for both mothers of first-born girls and first-born boys, fertility decreased after the One-Child Policy took effect. In addition, the decline in the probability of having two or more children is larger for couples with first-born boys. On the other hand, the increase in labor force participation during this period is smaller for mothers with first-born girls.

One thing to notice from Table A.4 and Table A.5 is that the One-Child Policy did not lead to a large number of families with only one child in rural China. Even for the young cohorts with first-born boys, more than 70% still have more than one child. This fact may make exposure to the OCP a weak instrument for fertility, yet the huge sample size can resolve this problem to some extent. On the other side, weak enforcement of the OCP does not affect the validity of the instrument.[22] There are several possible reasons for the low compliance rate in rural China. First, with the relaxation of the OCP in 1984, many rural households with first-born girls were allowed to have a second child.[23] Second, it may be difficult to fully enforce the One-Child Policy in rural China, as the the only severe punishment in rural areas for above-quota births is a one-off fine. Moreover, even the fine may not be very effective in rural areas, because many poor farmers cannot afford to pay (Li et al., 2005). Third, rural households have strong incentives to disobey

[22]All our 2SLS regressions pass the Cragg-Donald weak instrument test at at least 5% level.

[23]This one does not explain why households with firstborn boys have more than one child.

the One-Child Policy, as children are valued inputs to farm labor (Schultz and Zeng, 1995) and for providing old age support, since social security and pension systems in rural China are very limited. However, in the urban areas where social security system are better developed, no farm labor is needed, and more severe punishments are implemented, so the probability of having two or more children is much lower. From the data in the 1990 Population Census, only about 30% of the urban couples have more than one child.

1.5 Regression Results

1.5.1 OLS estimates of the effect of additional children on female LFP

Panel A of Table A.6 show the results of estimating equation (1) using OLS. All regressions include age cohort dummies, mother's age at first birth, education levels for both parents, and county fixed effects. Standard errors are clustered at the county level. Columns (1) and (2) are based on samples of observations from restricted provinces (1-child provinces and 1-boy-2-girl provinces) and 1-boy-2-girl provinces respectively. Both regressions show no significant effects of additional children on women's labor force participation. Column (3) and Column (4) are based on observations in restricted provinces and 1-boy-2-girl provinces, except for mothers whose first birth occurred later than 1981. Chen, Li and Meng (2013) show that there was a jump in both the abortion rate and the sex ratio in 1982 (Figure G.4), though the sex ratio for first-birth looks stable over years. To be conservative here, I drop all samples with possibly non-exogenous first-born gender. More details about this sample selection rule are discussed in Section 6. In this highly skewed sample, both women and their children are older, so we expect the effects of children on mother's labor supply to be smaller (Angrist and Evans, 1998). The OLS estimates in Column (3) and (4) suggest that compared to mothers with only one child, mothers with two or more children have 1.3-1.5 percentage points higher probability to work. These OLS estimates are very different from the research findings in developed countries such as the U.S., where Angrist and Evans (1998) report their OLS estimates of the effect of three or more children on female LFP to be around -15%.

1.5.2 First Stage: the effects of the One Child Policy on fertility

As discussed in Section 3, OLS estimator of β in Equation (1) is likely to be inconsistent due to the endogeneity of fertility with respect to female labor force participation. The implementation of the One-Child Policy allows us to isolate the exogenous variation in fertility. Doing DID to capture the exogenous variations in fertility caused by exogenous variations in policy change is fundamentally equivalent to using corresponding interaction terms in a regression for the probability of having two or more children. Column (1) in Table A displays our first stage regression coefficients of "More than one child" ($kids2_{ict}$) on the interaction terms of age cohorts and ethnicity (ρ_l in Equation (2)). Since the OCP was nationally implemented in 1980, and most women with two or more children completed their second birth at or before age 30, only "Treatment (Young) Cohorts" who were at or under age 30 in 1980 would be restricted by the OCP and thus most likely to change their family size because of the restriction from the OCP. Therefore, we expect the interactions to be significantly positive in the first stage for cohorts younger than 40, but not significant for older age cohorts. This is exactly what we find: only interactions for age cohorts 40 or younger are positive, and only interactions for age cohorts 38 or younger are significantly positive. For women around 30 years old in 1990 the One-Child Policy decreased the probability of having additional children by about 8 percentage points more for Han women than it did for non-Han women. Notice here that some very young cohort dummies are not significant. There are two possible reasons for this: first, the small number of observations in those cohorts, and second, they are too young to have finished their life time fertility. The coefficients from Column (1) in Table A are plotted in Figure B.5a (the blue line).

Our second sets of DID estimates are based on the different policies on second births for Han couples with first-born girls and first-born boys in the 1-boy-2-girl provinces. Column (1) in Table A.8 displays our first stage regression coefficients on the interaction terms of age cohorts and whether the first-born was a girl (ϕ_l in Equation (3)). Similar to our findings in Table A.8, interactions are only positive for cohorts younger than 40. For women around 30 years old, the One-Child

Policy decreased fertility for mothers with a first-born boy by about 5 percentage points more than mothers with a first-born girl. The coefficients from Column (1) in Table A.8 are plotted in Figure B.5b (the blue line).

To increase the power of the instruments, I focus on mothers age 30 and above who are more likely to complete their second births in Table G.1. Using individuals age 40 and above as the control group, the first-stage reports larger F-statistics.

1.5.3 Reduced form: the effects of the One-Child Policy on female FLP

The reduced form regressions can be expressed as:

$$LFP_{ict} = \sum_{l=16}^{44} (nonHan_{ict} \cdot d_l)\, \tau_l + \mathbf{X'_{ict}} \zeta + \alpha_4 + d_t + \omega_c + u_{ict} \tag{1.4}$$

$$LFP_{ict} = \sum_{l=16}^{44} (First\text{-}Born\ Girl_{ict} \cdot d_l)\, \eta_l + \mathbf{X'_{ict}} \nu + \alpha_5 + d_t + \sigma_c + v_{ict} \tag{1.5}$$

where d_l, $l = 16 to 44$ are age dummies from 16 to 44 years of age. Individuals aged 45 in 1990 form the control group, and are omitted from the regression. One useful check of instrument validity is to see its effect on the untreated group. As the fertility of the "pre-treatment cohorts" are not affected by OCP, the coefficients τ_l and η_l are expected to be 0 for $l > 40$. Our findings in Column (1) of Table A and Table A.10 are close to this. For Column (1) in Table A, the interaction terms of age cohorts and non-Han are only significantly negative for women aged 41 or younger. And for Column (1) in Table A.10, the interaction terms of age cohorts and First-Born Girl are rarely significantly negative for women over 40. For women around age 30, compared to women at age 45, the decline of Han mothers' LFP is about 3 percentage points larger than that for non-Han mothers, and the decline in first-born boy mothers' LFP is about 2.3 percentage points larger for mothers with first-born boys than for mothers with first-born girls. The coefficients from Column (1) in Table A and Table A.10 are plotted in Figure B.5a (the orange line) and Figure B.5b (the orange line) respectively. (Columns (2) and (4) in Table G.1 show similar results.)

1.5.4 2SLS estimates of the effect of additional children on female LFP

Panel B in Table A.6 reports the 2SLS estimates of the effect of additional children on female labor force participation in rural China. Using DID based on ethnicity (gender of first birth) as instruments, the results show that, other things equal, for mothers under 46 years old and with one child, having additional children will decrease the possibility of working by 15.3% (8.4%) in rural China in 1990. These two estimates are close to the estimated effects of three or more children on female LFP in the U.S. (between -9.2% and -12% in Angrist and Evans (1998)), Mexico and Argentina (between -6.31% and -9.58% in Cruces and Galiani (2007)), and Taiwan (-12.6% in Ebenstein (2009)).(When focusing on mothers age 30 and above, and using individuals age 40 and above as the control group, Table G.2 display similar results, which suggest that, having additional children will decrease mother's labor force participation by 12.1%-12.9%.)

While the two instruments yield different effect sizes, the two coefficients are not statistically different from each other. Why do the two sets of IV generate such different estimates? The different powers of the two IV sets might be one reason. When using son-preference to estimate the effects of family size on female labor supply in Taiwan, Ebenstein (2009) shows that IV estimates will drop when the instrument is weaker. Based on simulated data from the structural model,[24] he shows that the estimates based on weaker instruments are smaller than the true average causal effect, and the stronger the instruments, the closer to the average causal effect the estimates are. The Cragg-Donald Wald F Statistic show that our instrument from DID based on gender of the first-birth is weaker than the instrument from DID based on ethnicity. Different local average treatment effects estimated by the two sets of IV and different sample for estimation might also be the reason.

Hausman tests show that the 2SLS estimates using DID based on ethnicity (gender of first birth) are statistically different from OLS estimates at 1% (5%) level. Our 2SLS estimates suggest a larger negative effect of children on female labor supply than OLS estimates. This differs from

[24] Ebenstein (2009) uses a model of a mother's joint determination of fertility and labor supply allowing for unobserved heterogeneity in both the benefits and costs of children.

most of the findings reported in the previous literature. There are at least two possible reasons for this finding. First, as in many other developing countries, the fertility of rural households in China might be determined through a collective bargaining process, in which only individuals with more bargaining power can decide the total number of children (Rasul, 2008). In this case, if women with less bargaining power are forced to work more as well as have more children, then the OLS estimates will underestimate the negative impacts of number of children on labor force participation. Second, controlling for the endogeneity of fertility removes the possible factors that promote fertility and female LFP simultaneously. For example, in rural China, if people with a higher earning capacity can afford to have more children financially and also have more opportunities to work, then this simultaneity will bias the OLS estimates up (Fang et al., 2010).

1.5.5 Heterogeneous Effects

In this section I explore whether the female labor force participation response to fertility is uniform or varies by mothers' education. Extending Gronau's (1977) model of market and home production, Angrist and Evans (1998) incorporate child quality effects to the model of fertility and female labor supply. Their model predicts that the labor supply of more educated women is more sensitive to fertility, and thus the negative effects of additional children on LFP is larger for women with higher education levels. Here we use exogenous variation in fertility brought by the OCP as instruments to explore how the labor market consequences of childbearing would vary with mothers' education. This is done by running separate regressions on women with at most primary school education (73.48% of the restricted provinces sample, 73.24% of the 1-boy-2-girl provinces sample), and women with at least junior high school education (26.52% of the restricted provinces sample, 26.76% of the 1-boy-2-girl provinces sample). Figure B.6a depicts the first stage and reduced form coefficients of interaction terms when using DID on ethnicity as instruments. It shows the effect of the OCP on fertility is larger for women with at least a junior high school education. On the other hand, the reduced form regression coefficients are close for women with different education levels. Figure B.6b represents the coefficients of interaction terms when using DID on gender of first birth as instruments, and it shows similar trend as Figure B.6a. Since Wald estimates equals to the ratio of reduced form coefficients to first stage coefficients, these results suggest that women with lower education to have larger negative 2SLS estimates of the effect of fertility. The 2SLS estimates in Table A.14 confirm this, though the difference between estimates for lower and higher educated women is not statistically significant.

These results contradict the predictions of the theoretical model, yet are consistent with the empirical findings in Angrist and Evans (1998), which suggest that the labor supply consequences of childbearing are smaller for more educated women. The results presented here are merely descriptive and should not be over-interpreted because many estimates are insignificant and education is correlated with other individual preferences that may affect the labor supply decisions.

1.6 Validity of Instruments

The key assumption underlying my DID estimation framework is that the instruments do not affect female labor supply through channels other than fertility. In other words, I assume that differences in female LFP between pre-treatment and treated non-Han minorities (mothers with first-born girls) should be the same as the differences between the pre-treatment and treated Han mothers (mothers with first-born boys) in the absence of the OCP. The validity of our DID instruments are extensively discussed and tested in this section.

1.6.1 DID using ethnicity

First, if the differential implementation of the One-Child Policy between Han and non-Han Chinese is endogenous, then the DID strategy in terms of ethnicity is not valid. Drawing on research in science studies and early documents in China, Greenhalgh (2003) concluded that the decision to exclude non-Han people from the One-Child Policy was driven by pure political considerations rather than by differential fertility rates or other economic factors. Therefore, we regard the exemption of non-Han people to be exogenous.

Second, if around the same time as the OCP there are other policies that altered the preferences and/or labor demands for Han and non-Han people differently, then the DID instruments will pick up those effects and violate the exclusion restriction. I am aware of only one ethnically-divided policy from 1978-1984 that may lead to such a concern. In March 1981, the State Council released the "Report on the 1981 National College Enrollment Conference". According to the report, ethnic minorities were able to enter college with lower grades and lower tuition fees. This preferential policy was accompanied by other local education policies favoring non-Han populations. These policy changes might have promoted the education levels of non-Han Chinese relative to the Han Chinese. This in turn might reduce the labor force participation for young non-Han Chinese, who might choose to go to school rather than work after the policy change. To test this possibility, I examined the change in education levels for non-Han people relative to Han people through the following regression:

$$Educ_{ict} = \sum_{l=16}^{44} (nonHan_{ict} \cdot d_l)\, \lambda_l + \alpha_6 + d_t + \omega_c + u_{ict} \tag{1.6}$$

Where $Educ_{ict}$ is the years of education for women i, in county c, age cohort t. Column (1) in Table A.11 shows the estimates of λ_l. The estimates indicate that the education level of non-Han was not promoted by the preferential policies. On the contrary, compared to women aged 45, the education disadvantage for non-Han relative to Han people was even bigger for the very young cohorts. Though I cannot explain why that is the case in this paper, I tried to exclude the young cohorts from the main regression and test the robustness of my findings. Table G.3 reports the effects of fertility on female LFP using DID using ethnicity as IV when excluding mothers younger than 27 (for the younger cohorts, non-Han mothers have significantly widened disadvantages in education). The results are similar to the main results in Table A and Table A, with 2SLS estimates dropping a little from -0.153 to -0.139, and these two estimates are not statistically different.

The third concern is that in order to have more than one child, some Han couples may have changed their ethnicity to non-Han after the implementation of the One-Child Policy. Although there is some anecdotal evidence of people changing their ethnicity (Scharping, 2003), such re-identification is not popular in China (Li et al., 2005). In fact, before the year 1981, when the State Council announced the Circular of Restoring and Correcting Ethnicity, it was almost impossible for people to change their registered ethnicity. Therefore, we can do a robustness check excluding mothers with first birth after 1981.[25] Column (3) and (4) in Table A.6, and Column (2) in Table A - Table A.10 are the regression results based on this restricted sample. This truncated sample has older children than that used in main regressions, and thus we may expect smaller negative effects of fertility on mother's labor force participation.[26] However, the 2SLS estimates in Table A.6 show no significant differences between the truncated and the main regression samples.

1.6.2 DID using gender of the first-birth

The "missing girl" problem is a big challenge for China's population. Since the 1980s, after the implementation of the OCP, the sex imbalance of children has increasingly favored boys (Li et al., 2011). Given this, we might be concerned that the sex of first-born children is endogenous due to sex selection. Though endogeneity might be true for the high parity births, for the gender of the first birth the threat is much less severe. Chen et al. (2013) find that access to the B-ultrasound is not associated with any significant change in the sex ratio of first births, while the increased local access to ultrasound technology is found to substantially increase the sex ratio. Their data from the 1992 Chinese Children Survey also implies that the abortion rate is really low for the first birth, and the sex ratio of the first birth is rather stable both before and after the implementation of the One-Child Policy. Figure B.4 depicts the percentage of girl births by the age of the first-born children in the 1990 Census. The first vertical line is for children age 6, who were born in 1984, the year of the relaxation of the OCP. The second vertical line denotes children age 11, who were

[25]Ideally, we should test with mothers older than 30 in 1981, but that would lead to a very small treatment group, so we compromise to exclude women with first-birth after 1981 only. The idea is that, it would be much more difficult to change ethnicity for both parents and child in the household.

[26]I check the age of youngest child in this sample, find that the average age of youngest child is 6.6, and over 50% of them are less than 6 years old.

born when the OCP was first announced in 1979. For all three subsamples of provinces, we do not see any significant change in sex ratio before and after the OCP or the the relaxation of the OCP.

Another way to test the effects of the OCP on sex ratio is to run DID regressions of first-born gender on ethnicity. As non-Han families are allowed to have two children, they do not have an additional incentive to have the first-born be a boy after the implementation of the OCP. Given no sex selection of the first-born for non-Han families due to the OCP, we can compare the first-born gender between Han and non-Han people. If there is no significant difference between them, then we are more confident that the gender of the first-birth in 1-boy-2-girl provinces is not endogenous after the implementation of OCP. The DID regressions can be expressed as:

$$First\text{-}Born\ Girl_{ict} = \sum_{l=16}^{44} (nonHan_{ict} \cdot d_l)\,\xi_l + \alpha_7 + d_t + \omega_c + u_{ict} \tag{1.7}$$

Column (2) in Table A.11 is the estimates of ξ_l, which are not statistically significant for all age cohorts. This provides evidence for the exogeneity of the gender of first birth.

Chen, Li and Meng (2013) show that there was a jump in both abortion rate and sex ratio in 1982 (Figure G.4), though the sex ratio for first-birth looks stable over years. To be conservative here, we can drop all samples with possibly non-exogenous first-born gender, and run a robustness check on the truncated sample excluding mothers with first-born after 1981. The regression results are reported in Column (3) and (4) in Table A.6, and Column (2) in Table A - Table A.10. As explained above, the robustness checks on the restricted sample generate results that are very similar to the results from the main regression.

1.6.3 Placebo tests on 2-Children provinces

If there are policy shocks or changes in social-economic variables other than the OCP in the same period that have affected the female labor force participation, then the DID method may confound the effect of these policies or changes. As a falsification test, we can run the first stage and reduced form regressions for observations in the "2-children provinces", where all couples are allowed to have 2 children. As there is no variation in terms of the eligibility of having a second child between Han and non-Han mothers, or mothers with first-born girls and first-born boys, we expect the interaction terms of non-Han (*First-Born Girl*) and age cohorts to be zero for both first stage and reduced form regressions.

Table A and Table A show the regression results in the "2-children provinces". Table A displays the first-stage and reduced-form coefficients when using DID based on ethnicity as IV, and Table A shows the coefficients of interaction terms when using DID based on gender of first birth as instruments. We can find that, interaction terms of age cohorts and non-Han/gender of first birth are rarely significant,[27] for both first stage and reduced form. Thus we find no evidence that the trends in fertility and female labor force participation for Han (first-born girl) and non-Han (first-born boy) mothers are different in these placebo provinces.

Figure B.5a shows the coefficients of interaction terms, $nonHan_{ict} \cdot d_t$, by age cohorts in first stage and reduced form regressions. The solid lines are from first stages, while dashed ones are from reduced forms. For the restricted provinces denoted by thicker lines, both first-stage and reduced-form coefficients are around zero for cohorts aged older than 41, and then they depart from each other as the age becomes younger. For 2-children provinces represented by thinner lines, both first-stage and reduced form coefficients are around zero for all cohorts, especially for the reduced forms. Similarly, the coefficients of interaction terms, $\textit{First-Born Girl}_{ict} \cdot d_t$, by age cohorts, for both first stage and reduced form regressions are depicted by Figure B.5b. While it shows similar patterns to Figure B.5a in the first stage coefficients, the decline of coefficients of reduced-form interactions for younger cohorts seem to be smaller. That's why our 2SLS estimate of effects on female LFP based on this set of IVs is in smaller magnitude.

[27] The interactions of age cohort and non-Han are significantly positive for age cohorts younger than 28. Doing further investigations into the data, I find that might due to the earlier births for non-Han in 2-Children provinces. Figure G.3b shows the different time patterns of second birth for Han and non-Han in 2-Children provinces, indicating non-Han usually have a second birth earlier in these placebo provinces. Meanwhile, Figure G.3a shows that's not a concern for restricted provinces.

1.7 Robustness Check

1.7.1 Using triple differences as instrument

If the trends of Han and non-Han Chinese (or mothers with first-born boys versus first-born girls) in terms of female labor supply are different in the absence of the OCP, then the instruments based on ethnicity (or first-born gender) will not be valid. However, we can still use the triple differences, $[(fb\_girl, After, Han - fb\_boy, Before, Han) - (fb\_boy, After, Han - fb\_boy, Before, Han)] - [(fb\_girl, After, nonHan - fb\_girl, Before, nonHan) - (fb\_boy, After, nonHan - fb\_boy, Before, nonHan)]$ to capture the exogenous variation in fertility caused by exogenous variation in the policy change. In this triple difference strategy, the assumption we need is the difference in trends of Han and non-Han Chinese is the same for mothers with first-born girls and first-born boys. This assumption is weaker as the effect of the OCP is identified off of differences across ethnicities, gender of the firstborn child, and cohort. The first stage regression can be expressed as:

$$\begin{aligned} kids2_{ict} = & \sum_{l=15}^{44} (nonHan_{ict} \cdot First\text{-}Born\ Girl_{ict} \cdot d_l)\, \zeta_l + \sum_{l=15}^{44} (nonHan_{ict} \cdot d_l)\, \vartheta_l \\ & + \sum_{l=15}^{44} (First\text{-}Born\ Girl_{ict} \cdot d_l)\, \sigma_l + nonHan_{ict} \cdot First\text{-}Born\ Girl_{ict} \cdot \tau \\ & + \mathbf{X'_{ict}} \nu + \alpha_8 + d_t + \varphi_c + w_{ict} \end{aligned} \tag{1.8}$$

The instruments here are the triple interactions of dummy variables for whether the household members are Han Chinese, whether the first born is a girl and age cohort. We can only run this on the sample of 1-boy-2-girl provinces. Column (2) in Table A.15 is the 2SLS regression results using triple difference as instruments. Table G.4 implies that the triple difference is not very powerful in the first stage. As a result, we don not have a significant 2SLS estimates in Table A.15. Though the estimate is not precise, the point estimate is still negative and larger than the OLS estimates in magnitude.

1.7.2 Use twinning as instrument

Some research use twinning as an instrument to deal with endogenous family size. For example, Angrist and Evans (1998) use both same-sex and multiple births as instruments for having three or more children. They find smaller negative effects of fertility on maternal labor supply using multi-birth as instrument, which they attribute to the fact that a third child due to twinning is older than third children in other cases. Additionally, "twinning" itself may affect mother's labor force participation (Rosenzweig and Wolpin, 2000).

To provide an additional robustness check, I also try to estimate the effects of additional children on female labor force participation using multi-birth as instrument. Month of birth is reported in the 1990 China Population Census, so I use this information to identify multi-births. In the whole sample, 0.38 percent of first births are multi-births. In the 1-boy-2-girl subsample, the incidence of multi-birth in first birth is similar, 0.39 percent. This occurrence rate is lower than the findings in Angrist and Evans (1998),[28] which implies that the manipulation of multi-birth was relatively low in rural China at that time. Column (3) in Table A.15 displays the regression results when using multi-birth as instrument. The 2SLS estimate is statistically significant at 5% level, but smaller than the coefficients from the main regression using DID estimators as instruments. It implies that for mothers with one child, the additional children due to "twinning" will decrease their labor force participation by 6.9 percentage points. The validity of the "twining" instruments and the differences in the local average treatment effects may contribute to the smaller effects I find here.

1.8 Conclusion

The importance of children in female labor supply decisions has long been recognized by economists. This paper examines the effect of having two of more children on mother's labor force participation

[28] In their 1980 married sample, identified by quarter of birth, the probability of multi-birth is 0.83 percent.

in rural China. It resolves the endogeneity problem by instrumenting fertility with exogenous variations caused by the One-Child Policy. By exploiting variation in the policy's implementation across ethnicities and gender of first-born children, I construct two sets of differences-in-differences. The DID estimates indicate that the One-Child Policy has negative effects on fertility for the targeted populations. Using these two sets of DID estimates as instruments, I find that having two or more children decreases the mother's labor force participation by 8-15 percentage points in rural China in 1990. Comparing data from three China Population Censuses (1982, 1990, and 2000), Maurer-Fazio et al. (2011) suggest that due to increased levels of income and more freedom of choice for labor allocation, the negative effects of young children on female labor supply have increased in China. If that is the case, the discouraging effects of children on female labor supply may be even larger now.

Recently there has been a call for the relaxation of the One-Child Policy (Feng, 2010). In November 2013, the Chinese Communist Party released "Decision on Major Issues Concerning Comprehensively Deepening Reforms", stating that "China will start to implement the two-child policy for the couples where either the husband or wife is from a single child family". This paper provides a perspective for the potential effects of such policy relaxations on female labor supply. With two or more children, women will be more likely to stay at home, rather than work, at least in rural areas.

CHAPTER 2

HOW DO JOB DISPLACEMENTS AFFECT FERTILITY IN THE U.S.

2.1 Introduction

Economic recessions affect people's behavior in many ways, mainly through reducing consumption. Recessions might affect people's fertility decisions as well, as shown by Figure D.1. The Great Depression is one example, where the fertility rate dropped from 4.11 children per women in 1921 to 3.14 children per woman in 1933.[1] The decline of the fertility rate during the energy crisis in 1970s and the recent recession of 2008 are two other examples. Even the media reports this trend. For instance, to show their concern about potential baby bust, an article in the Los Angeles Times in December 2008 stated: "Birth rates typically decline during economic downturns. Would-be parents struggle with the wisdom of waiting."

Figure D.2 depicts both unemployment rate and birth rate in the U.S. from 1973 to 2012.[2] Let's take a closer look at the three peaks of birth rates in Figure D.2. First, in 1990, the birth rate increased from below 16 in the 1980s to 16.7 per 1000, while the unemployment rate was 5.4% for women, and 5.2% for men in 1989, much lower than the average unemployment rates in 1980s. Later on, in 2000, both the male and female unemployment rate was as low as around 4%, and at the same time, the birth rate reversed its declining trends and reached a local peak at 14.4 per 1000. Before the recent recession, the unemployment rate was 4.5% in 2007, and the birth rate was 14.3 per 1000, higher than the rates after 2009, which are less than 13.

Inspired by the opposite trends in fertility and unemployment rate, scholars investigate the effects of recessions and unemployment on fertility decisions using macro data. Many researchers

[1] Here is the total fertility rate (TFR) for the U.S. TFR is the sum of age-specific birth rates for women who are 15 to 44 years old. The formula for TFR at year t can be written as $TFR_t = \Sigma_{a=15}^{44} BirthRate_{t,a} \cdot 1000$.

[2] Birth rate is the total number of births per 1,000 of a population in a year.

in demographics and economics (Becker (1960), Ben-Porath (1973), Adsera (2005),Adsera and Menendez (2009), Currie and Schwandt (2014), to name but a few) recorded a pro cyclical pattern in fertility that is, during the periods when unemployment rates are higher, birth rates are usually lower, and maternity is somewhat postponed.[3]

There are several concerns about analyzing the effects of aggregate unemployment level on fertility. The first challenge is that, with the general equilibrium effects, it is almost impossible to get some really exogenous change in unemployment level that is unrelated to change in fertility. The endogeneity problem of unemployment leads to doubt on causality. For instance, using data of women cohorts defined by state and year of birth, Currie and Schwandt (2014) analyze the effects of unemployment rate experienced by each cohort at different ages on their fertility. Their causal estimation might be biased, if there exists some state trends in women's rights movement, which raise female labor force participation, increasing unemployment rate, and reduce fertility at the same time. Or if there are some exogenous improvement in educational attainments at the state level, which lead to reduction in both unemployment rate and fertility rate. In addition, fertility response to unemployment might differ for different groups of individuals. For example, Dehejia and Lleras-Muney (2004) find that when unemployment increases, the negative effects on fertility for black women is larger than that for white women, while Hoynes (2002) shows that low skilled women experience greater cyclical effects than high skilled men. With these heterogeneous effects, the interpretation of the pro cyclical trends of fertility warrants more attention. To solve these two problems, Ananat et al. (2011) estimated the effect of county-level forced job loss due to a layoff or closing rather than unemployment. She argues that such job losses can be regarded as an exogenous shock to the workers, so the estimates can capture the causal effects. Additionally, she considers the effects separately for women with different demographics.

There are still some limitations to Ananat et al. (2011). First, we may want to know which

[3]There are some exceptions: Butz and Ward (1979) noticed that in 1970s in the U.S. fertility trends were likely to become counter-cyclical; Ermisch (1980) and Ermisch (1988) showed that counter-cyclical fertility trends emerge in the Federal Republic of Germany and Britain during the 1970s as well.

group changes their fertility facing the local economic downturn: are they the people who lose their jobs, or the people who face the risk of displacement but do not actually lose their job? Adsera (2011) argue that the strong feeling of economics instability rather than current income loss might have strong impacts on current fertility decisions. The above research cannot distinguish the effects for these two groups. Second, the static model cannot tell us whether the observed changes in fertility are temporary or permanent. That is, will people's lifetime fertility be changed due to the job loss? Using individual-level panel data can help answer these two questions.

Using data from the Panel Study of Income Dynamics (PSID), Lindo (2010) examined how a man's job loss affects his fertility. His OLS estimates show that male job displacement increases fertility in the year immediately after displacement, but the effect becomes negative after the second year. The total effect on fertility by the eighth year after job displacement is slightly negative, and the lifetime fertility is lower for the displaced group. However, when using a fixed effect model to control individual unobservables many coefficients become insignificant. As the main idea of Lindo's research is to show the effects of income on fertility, he does not look at the effect of female job displacement, which would be affected by substitution effects as well[4].

There are some research studying the fertility effects of job displacement in Europe using individual panel data. Del Bono et al. (2012) examined the effects of a woman's own job loss using Austrian administrative data from 1972 to 2002. Comparing the birth rates of displaced women with those unaffected by job losses, they find that job displacement reduces average fertility by 5% to 10%. The strong average response is mainly explained by the behavior of white collar women. Using Finnish data, Huttunen and Kellokumpu (2012) show similar negative effects of women's job loss on fertility, especially for highly-educated women. For every one hundred displaced women there are approximately four fewer children born. They also found that male job loss has no significant impact on lifetime fertility. These researchers use high-quality administrative data to identify the effects of exogenous job displacement on fertility in Europe, but it is

[4]The channels through which fertility can be affected by employment are discussed in more details in Section 2.

difficult to generalize their findings to the U.S., which has a different culture and sociology as well as labor market policy than Europe. In particular, maternity leave in the U.S. is much less generous than that in Austria and Finland, and the childcare system for children under two years old is less widespread and less developed in the U.S. With fewer maternity benefits for employed workers, the income effect of displacement will be smaller. With limited access to childcare, the time cost of children - and thus the substitution effect - would be greater for women in the U.S. Combining the smaller income effects which tend to be negative and the larger substitution effects which are usually positive, we may expect the effect of female job displacement on fertility to be more positive in the U.S. To test this conjecture and investigate the effects of job displacement in the U.S., we have to look at the data in the U.S.

In this paper, in order to explore the effects of job loss on fertility, I focus on exogenous job loss generated by job displacement for both men and women in the U.S. In particular, controlling the individual unobservables by a fixed effect model, I compare fertility for people with and without an experience of displacement, before and after the displacement. Data from the National Longitudinal Survey of Youth 1979 shows that, in the immediate years following displacement, there is no significant change in fertility for men or women. In the later years, however, male displacement has a negative effect on fertility, while female displacement has heterogeneous effects for women with different levels of education. The significantly positive effects are observed for women with no college education, and no significant effects are observed for women with at least some college education. Results of using some other specifications, including narrowly-defined "displacement" criteria and fixed effect propensity score matching confirm the robustness of the estimates.

This paper makes several contributions to the literature on analyzing the effects of displacement on fertility. First, to my knowledge, this paper is the first one to look at the effects of female displacement on fertility in the U.S with individual level data. Using PSID data, Lindo (2010) and Amialchuk (2013) showed that men's job displacement had negative effects on fertility, but they did not investigate the fertility consequences of women's displacement. Due to the different socially accepted gender expectations in raising children, I believe it is necessary to explore the effects of displacement for women separately. Second, based on the conceptual model, I find the implications of heterogeneous effects for women with different levels of education, and then test and confirm those implications in the empirical part. In addition, I check that my main results are robust to using a displaced group of individuals who lost their jobs due to workplace closings, and also robust to fixed effect propensity score matching. Workplace closings generate displacements that are more likely to be exogenous to individual characteristics. Propensity score matching generates a more similar non-displaced group for further fixed effect analysis.

The remainder of the paper is laid out as follows: Section 2 discusses the mechanisms through which displacements affect fertility, including both reviews of the related literature and a simple conceptual model of analyzing fertility decision. Section 3 describes the data and comparisons between displaced and non-displaced group. Section 4 introduces the estimation strategy, and the regression results. Section 5 and Section 6 present the robustness check results using narrower definition of displacement and fixed effect propensity score matching respectively. Section 7 concludes the paper.

2.2 Why Would Displacement Affect Fertility

2.2.1 Related Literature

Researchers have a continuing interest in the problems of displaced workers, and have produced a substantial literature on this topic. They have found many adverse and long-lasting consequences of job displacement. The most prominent consequence is the significant decreases in lifetime earnings (Ruhm (1991), Jacobson et al. (1993), Kletzer and Fairlie (2003) and Couch and Placzek (2010)). Even for young workers in the NLSY sample with less firm-specific capital, Kletzer and Fairlie (2003) found sizable long-term earning losses.

On the other hand, the existing literature shows that changes in earnings might change people's fertility decisions. Heckman and Walker (1990) estimate the relationship between earnings and fertility using retrospective fertility data from Sweden. They argue that because most Swedish earnings are set by collective bargaining agreements, they are exogenous to the fertility process. They find support for a positive effect of male earnings on fertility, while the effect of female earnings is found to be negative. Merrigan and Pierre (1998) find similar results using the same methodology with data from Canada. Schultz (1985) uses an instrumental variable strategy to identify the causal relationship. He uses world prices of grain, which is a male labor-intensive product, as the instrument for male wages, and prices of butter, which is a female labor-intensive product, as the instrument for female wages in his analysis of county-level fertility rates in Sweden from 1860 to 1910. He finds one quarter of the decline in fertility during that period can be explained by the increase in the female-to-male wage ratio. The doubling of real male wages had no significant effect on lifetime fertility, but it did induce earlier marriage and expedited fertility. More recently, using wide variation in energy prices in the 1970's as the instrument, Black et al. (2013) detected the positive effects of men's income on completed fertility. To sum up, it is generally found that male earnings have a positive effect on fertility, while female earnings reduce fertility.

Combining the substantial earning losses caused by displacement and the effects of earnings on fertility, it is reasonable to predict that job displacement will have some influence on fertility decisions. Besides changes in earnings, there are additional mechanisms through which job displacement may affect fertility decisions. For example, displacement is also shown to reduce job stability (Stevens (1997)), increase the hazard of divorce (Charles and Stephens (2004), Eliason (2012)) and have negative impacts on health, education, and labor market outcomes for the children of the displaced workers (Stevens and Schaller (2011), Oreopoulos et al. (2008)). In the conceptual model below, focus will center on the impact of induced change in earnings only.

2.2.2 Conceptual Model

The classic models of Becker (1960) and Mincer (1963) pioneered the association of fertility with parent's wages and household income. After that the model has been expanded greatly by other theorists (Willis (1973), Hotz et al. (1997)). Drawn from Jones et al. (2010), a standard static model of fertility decision is like this: Parents try to maximize their utility from consumption and quantity of children, subject to budget and time constraints. Children generate utility for the parents with the cost of time and money. Conventionally, women subsume the total time costs of the children, and men provide financial support. In this model, an increase in men's wages will only raise household income. For women, conversely, both household income and the price of children goes up with an increase of their wages, resulting in offsetting income and substitution effects on the demand of children. This could produce an ambiguous net effect. Therefore, even if the magnitude of earning loss caused by job displacement is similar for men and women, the effects on fertility might be different.[5]

With the introduction of market child care, the fertility effects for women with different earning levels will differ as well. Inspired by Singh et al. (1986) and Perry (2003), I analyze fertility decisions in a household (individual) production model. The individual generates utility from consumption (C) and quantity of children (n), so a mother's utility function can be written as $U(C,n)$. For each child, λ units of child care is required, and for n children, the $n\lambda$ units of child care can be obtained either from home production λ_h, or from market purchase λ_m. The home production function for child care $\lambda_h = f(K)$ is assumed to be increasing and concave, where K is the time input for home production. Market child care can be purchased at an exogenous price p_λ. For each mother, there are two constraints. First, is the budget constraint[6], and the second is the time constraint. In summary, with w denoting market wage for the individual, and L as the labor

[5]The concern that employers will invest less on women's firm-specific capital with anticipations of child-birth interruptions implies that earnings loss following job displacement should be smaller for women. However, empirical studies do not strongly support this conjecture (Jacobson et al. (1993), Kletzer and Fairlie (2003), Couch and Placzek (2010)).

[6]Husband earnings y_h is not included, since I regard that as exogenous unearned income, which will not affect the main conclusion.

supply in the market, the maximization problem can be expressed as:

$$
\begin{aligned}
max: &\quad U(c,n) \\
s.t.: &\quad C + p_\lambda \cdot \lambda_m = wL \\
&\quad \lambda_m + \lambda_h = n\lambda \\
&\quad \lambda_h = f(K) \\
&\quad K + L = T
\end{aligned}
\tag{2.1}
$$

Four constraints can be combined as the full income constraint:

$$
\begin{aligned}
C + p_\lambda \cdot n\lambda &= wT + \pi \\
\pi &= p_\lambda \cdot f(K) - wK
\end{aligned}
\tag{2.2}
$$

Now, the maximization problem can be solved in two steps. First, choose K to maximize π ($w = p_\lambda \cdot f'(K^*)$ for inner solution); second, given π^*, choose C and n to maximize utility. In this framework, a change in the women's wage rate might lead to different effects on the demand for children, depending on the level of the wage rate and how K^* is determined.

First, if w is so high that $w > p_\lambda \cdot f'(K^*)$ at the corner solution, marginal change in w will not change the level of K^*. Women will not produce any child care at home both before and after the change. So a decline in w will only reduce the income levels, and lower income will lead to a lower demand for children. Figure D.2 depicts this case graphically. In Figure D.2, the wage rate decreases from w_0 to w_1, and the number of children drops from n_0 to n_1 correspondingly.

Second, if w is so low that $w < p_\lambda \cdot f'(K^*)$ at the corner solution. Women will produce the entirety of child care at home, and use the remainder of available time to work on the market. In this case, a decrease in w will not change the amount of λ_m, which is always equal to zero, but K^* will be changed, as women have to trade off the benefit and cost of having children. Now, the first order condition for optimal K^* is $f'(K^*) = \frac{U_C'(C,n) \cdot w \cdot \lambda}{U_n'(C,n)}$. Decline in w will lead to decline in $f'(K)$, with concavity, K^* will rise. Intuitively, a lower wage rate reduces the relative cost for children, so the demand for children will be increased.

Third, if $w = p_\lambda \cdot f'(K^*)$ we have an inner solution and only part of the child care will be produced at home. Now if wage rate decreases, K^* will be increased, as the relative cost of home production is lower. However, smaller wT will exert a negative effect on the purchase of λ_m. Given these two factors, it is unclear whether the total effect on fertility will be positive or negative. For low-wage women, as they are purchasing very little child care from the market, the latter impacts are smaller, while the former effects are larger. Their total effects, therefore, will more probably be positive. For high wage women, the opposite should be true.

Perry (2003) also argues that the high-wage women have a stronger income effect while low wage women have a stronger substitution effect. Therefore, the demand for children will be reduced for high-wage women but increased for low-wage women when the wage rate decreases. Using industry and location specific wage variation as the instrument, Perry found that a 10% increase in earnings reduces total fertility by age 35 by .09 children for low-education women, who might face a lower wage offer. In the case of high-education women, meanwhile, a 10% increase in earnings will increase the total number of children they have by age 35 by about .03 children. Based on my conceptual mode, with the large earning loss caused by displacement, we can expect high-educated women who experience a job displacement to decrease their fertility, while low-educated displaced women will increase their fertility after experiencing a job displacement.

In the simple setting above, we use a static model assuming that the relevant unit of time for maximization is the individuals' lifetime. While static models can simplify the analysis of fertility decisions, dynamic models are attractive as they emphasize the inherently sequential nature of fertility decisions. A great deal of literature suggests that it is important to investigate the timing and spacing of births over the lifetime (Ward and Butz (1980), Hotz and Miller (1993), Amialchuk (2013)).

In a dynamic framework, individuals maximize the discounted sum of utility by jointly choosing the allocation of time, consumption, and the timing and number of children. The optimal choice from such a model generally entails the consumption smoothing and early births, due to the incentive to enjoy the utility from children earlier with a lower discount. How do earnings affect fertility in a dynamic model? This usually depends on whether the effect is transitory or permanent, and whether the individuals are credit-constrained or not (see e.g. Hotz, Klerman, and

Willis, 1993). For our study of displacement, much of the literature has shown that the loss of earnings is permanent, therefore, we can expect the lifetime fertility to be changed by the incidence of displacement. For lower-educated women, the static model suggests they might increase fertility after being displaced, but if we consider the credit constraint they might face after displacement, we would expect that increase in fertility to occur not immediately after displacement, but later in life. In addition, it might take time for individuals to learn the information about income loss from a job displacement, so the displaced individuals might adjust their fertility in response to income loss several years later after the displacement.

2.3 Data and Descriptive Statistics

For this study, I use public data from the National Longitudinal Survey of Youth 1979 (NLSY79), a nationally representative sample of young men and women aged 14 to 22 when first interviewed in 1979. After 1979, they were interviewed annually until 1994, and then biennially. I restrict the sample to observations to person-years below the age of 42 after which birth is rare(Lindo, 2010). The data from NLSY79 is well suited to this study as it includes both detailed labor market information and childbirth histories. For studies of job displacement, NLSY79 has several distinct advantages over other widely-used data sets. First, in comparison to CPS Displaced Worker Survey (DWS), the NLSY79 is longitudinal and includes both displaced and non-displaced individuals. This permits comparisons between workers who do and do not suffer a job displacement both before and after the displacements. Second, compared to the Panel Study of Income Dynamics (PSID), the NLSY79 records the reasons for job loss in more details by distinguishing layoffs from being fired (PSID puts these categories together as one choice). For my fertility analysis, the NLSY79 has two particular advantages. It samples both men and women (PSID includes only the head of the household, who are mostly men), enabling me to test the different effects for them, and the heterogeneous effects for women with different educational levels. Also, the NLSY79 follows people from a very young age, when they just started to enter the job market and have children, so we can see a complete history of displacement and fertility.

Each year, the NLSY79 gathers detailed employment information, including information on up to five jobs held during the interview period (approximately one year). If an individual was no longer working at their previously reported job and the reason for the job ending was "layoff" or "plant closure", then I categorize this as a job displacement (For the robustness check, I use narrowly defined displacement group which consists of people who lost their jobs due to plant closure only). For each job, there is information to link across interviews. Using this information, we can determine if the respondent reported being re-employed with same employer. If there was a match of employer, the reported job loss will not be counted as a displacement. Following the literature, to limit the analysis to workers with a reasonably strong attachment to the labor market, another restriction for "displacement" is that the lost job should be a full time one[7], that is, the individual must have worked an average of 25 or more hours per week when working at the job.

In this study, I only consider the effects of job displacement that occurred from 1984 to 1994. I do not include displacement information from 1979 to 1983 because of changes in the possible responses for the "reason left job" question. Prior to 1984, temporary and permanent layoffs were grouped together.Topel (1990) argues that the PSID might inaccurately measure displacements since the question focuses on an individual's last job. If a respondent has held and left another job after an initial displacement and before surveyed, the individual will be categorized as not displaced. Because the NLSY79 includes information for up to 5 jobs, this problem will be less severe. However, after 1994, when the survey was done biennially, this might become a concern. In addition, the recall errors will rise when the frequency of surveying drops[8]. Therefore, I only include displacement information until 1994 in the empirical analysis. (I also tried to drop observations with their first displacements after 1994, as well as using data from 1984 to

[7]Lindo (2010) argues that the strong attachment provides some reason to think that these workers have "something" to lose with job loss.

[8]For woman i's job j, the time when was the question of displacement answered might or might not be the time of displacement. Further, Jacobson et al. (1993) suggest that workers tend to report remote instances of displacement.

1992 only as robustness checks. Results are reported in Section 3.) To reduce the measurement errors in displacement, the respondents who had not been interviewed for two or more consecutive years from 1984 to 1994 are also excluded. I include only the first observed job displacement for each individual (if one exists) during the survey period, and I include it only if it meets the work experience restriction. Additional displacements for these individuals are not included separately, as I view future displacements as a potential cost of the initial displacement (Stevens, 1997).

A total of 7,659 individuals meet the screening criteria for the sample. Of those, 2,685 (35%) suffered at least one displacement from 1984 to 1994 .[9] Figure D.4 depicts the displacement rate in the NLSY79 sample by year. Notice here, I include all displacements no matter whether it is the first displacement for the individual or a subsequent displacement. The dashed line represents the official annual unemployment rate in the U.S. It shows a similar trend to the calculated displacement rate.[10] Figure D.4 suggests that displacement rate for our sample drops from 1980s to 1990s. Two possible reasons can lead to this decline trend, one of which is the improvement in the labor market in the U.S. over that period. Increase in the age of the followed individuals in the sample might also contribute to the declining trend of observed displacement rate. Figure D.5 shows the displacement rate in the NLSY79 sample by age. A trend of declining displacement rate can be found when age getting old.

Table C.1 compares the means of key variables for the displaced group and the non-displaced group. Regarding time-variant variables, following Lindo (2010), I calculated the means of the displaced group by using values three or more years prior to the time of first displacement. For the non-displaced group, all person-year values are used. As this treatment artificially reduces the age of the displaced group, I calculated the difference in means after adjusting to control age and

[9]The rate of job displacement calculated is higher than in Kletzer and Fairlie (2003) (24%). Part of the reason for the difference is the exclusion of non-consecutively interviewed individuals in our sample. If these individuals are included, the total displacement percentage will drop to 30%.I emailed authors for their code, but they cannot provide as it was wrote long time ago, so I cannot figure out what else might be the reason for the difference.

[10]Figure H.1 divides the total sample into three age cohorts. Among these cohorts a similar trend can be seen.

year fixed effects. It would be ideal if the pre-displacement characteristics of both groups were similar. As in that case, the exogeneity of displacements will be supported. That is not what we find here, however. Table C.1 indicates that some differences in the mean characteristics between the displaced and non-displaced groups are significant, e.g. the displaced group is more likely to be black, Hispanic and less educated, and their family income in the previous year year is about 6-8% lower than the never-displaced group. This suggests that these two groups may be different in terms of their unobserved characteristics as well. If the unobserved differences are correlated with fertility, the estimated effects of displacement will be biased. This fact underscores the potential importance of controlling for individual unobservables when estimating change in fertility due to job displacement. Therefore, we will focus on fixed effect models in our regression estimation, and try various specifications to verify the results.

Before estimating the effects of job displacement on fertility, we graphically represent the dynamic of fertility and its association with job displacement. Figure D.6 shows birth rates among men in two groups: those suffering at least one job displacement and never-displaced. For displaced men , the x-axis denotes time before and after job displacement. For non-displaced men, they do not have a year of displacement reference, and thus I generated a random year-of-displacement for each of them. After conducting the randomization with the probability for each year based on the conditional distribution of occurrence rates of displacement for the displaced group, I graphed both groups. The “fake event analysis” in Figure D.6 implies that displaced men have higher fertility before displacement compared to the never-displaced men. However, about 5 years after the “displacement”, displaced men display lower fertility. Figure D.7 shows the dynamics for women who are displaced at least once and never displaced. No significant differences can be detected for the two groups, both before and after the “displacement”. According to the analysis in Section 3, women with different levels of education might have heterogeneous effects on fertility. Therefore, I try to investigate the dynamics of fertility for women with and without college education separately in Figure D.8 and Figure D.9. For women with college education who never experienced a job displacement, fertility is higher than for their displaced counterparts, both before and after the "displacement". For women without college education who are never displaced, meanwhile, fertility was higher than their displaced counterparts before the "displacement", but the differences gradually disappear after the "displacement".

2.4 Regression Results

2.4.1 Fixed Effects Model

Although Figures 6-9 are informative, I am more interested in estimating the effects of displacement on fertility through controlling individual's unobserved heterogeneities. Following the literature about displacement effects (Jacobson et al. (1993), Kletzer and Fairlie (2003), Couch and Placzek (2010), etc.), especially studies about the displacement effects on fertility (Lindo (2010), Del Bono et al. (2012),Huttunen and Kellokumpu (2012)), I use the following fixed effect linear probability specification for regression analysis:

$$Birth_{it} = \Gamma\mathbf{D}_{it} + \mathbf{X}'_{it}\Delta + \alpha_1 + \gamma_t + \phi_i + \varepsilon_{it} \tag{2.3}$$

where $\mathbf{D_{it}} = (d_{it-2}, d_{it-1}, d_{it}, d_{it+1}, d_{it+3}, d_{it+5}, d_{it+7}, d_{it+8+})$. $Birth_{it}$ is an indicator for whether or not individual i has any additional children in year t. γ_t are year fixed effects that capture the general time pattern of fertility in the society. ϕ_i are individual fixed effects to capture individual time-invariant unobservables and other heterogeneity. $\mathbf{X}_{it}$ can include a vector of observed, time-varying individual variables, and here limits to age dummies.[11] $\mathbf{D}_{it}$ is a vector of dummy variables indicating the individual's displacement in a future, current or previous year, which can help us to capture the timing of the effects. To be specific, $\mathbf{D}_{it}$ includes indicators for two years prior to displacement, one year prior to displacement, the year of displacement, and indicators for subsequent

[11] I do not include wages, earning or other labor market outcome variables directly in the model because they are known to be endogenous to birth timing (Walker, 2002). Marriage status is important to fertility decision and should be controlled if it is exogenous. However, as shown by Charles and Stephens (2004) and Eliason (2012), job displacement generally increases the hazard of divorce, which has a detrimental effect on fertility. Therefore, including marriage variable may cause biased estimation on displacement impact. Robustness check using an alternative specification including marriage variables is done and reported in Table H.2.

years following a displacement (one year after the displacement, 2-3 years after the displacement, 4-5 years after the displacement, 6-7 years after the displacement, and eight or more years after the displacement). The omitted indicators are for three or more years prior to displacement and for the never-displaced. As it usually takes 9 months from a conception to a birth, we can regard the coefficient of d_{it+1} as the effects of displacement in year t on the conceptions in year t. Similarly, the coefficient of d_{it+3} represents the effects of displacement in year t on the conceptions in year $t+1$ and year $t+2$. Because we control the time trend γ_t and individual heterogeneity ϕ_i, this framework now compares changes in displaced workers' fertility to those of the non-displaced worker. This is essentially the same as the "difference-indifferences" technique, which uses a control group to capture the fertility changes that would have occurred in the absence of displacement. Based on the assumption that without displacement, change in fertility for the people in displaced group would be the same as that for the people in the non-displaced group, no matter how workers' permanent characteristics are related to their displacement status, the estimates of the displacement effects are consistent.

Table C.2 is the impact of displacements for the entire sample, men and women respectively. For men, the effects are negative (significantly negative at 10% level for 6 or more years following the displacement). The annual birth rate will be decreased by 1.5-1.6 percentage points 6 years after the displacement. Using PSID data for household head only, Lindo (2010) found a husband's displacement has a very small positive effect in the years immediately following the displacement, and a larger negative effect many years later. The magnitude of negative effect after four years is about 1.4-2.3 percentage points, similar to our estimates here. Further, the only significant effect in his estimates is the one for 8+ years after the displacement. For women, the effects reported in Column 3 are positive, but all coefficients are not significant.

Table C.3 shows heterogeneous effects for men/women with different levels of education. Columns 1 and 3 are people with high school educations or less. Columns 2 and 4 are for people with at least some college. For men, it seems the effects of job displacement on fertility are more negative for less educated people except for the effects during 6-7 years after the displacement, yet the differences between more and less educated men are not substantial. For women, my estimates imply that only lower-educated women show significantly positive effects four years after the displacement. Highly educated women will also increase their fertility if they suffer a job displacement, yet the positive effects are not significant. For women who did not go to college, if they lose a job, the probability of having additional children for each year will increase by 2.2-2.3 percentage points four or more years later. This is different from the findings in Europe by both Del Bono et al. (2012) and Huttunen and Kellokumpu (2012). Their studies find that job displacements have negative effects on fertility for women in Austria and Finland, and the negative effects are concentrated in high educated, white-collar women. Compared to our findings here, it is implied that the total effects of job displacement in the U.S. are more positive. This can be caused by either larger substitution effects (limited access to the market childcare) or smaller income effects (fewer maternity benefits for employed workers).

To test the differential effects of displacement for women with different levels of education, I include the interaction terms of a dummy variable of college education and indicators of years after the displacement in the regression. The coefficient of the interactions are reported in Table C.4. It is found that the effects of displacement are more negative for college educated women, and the differences are statistically significant for the effects during 2-5 years and 8+ years after the displacement.

One of the advantages of having panel data is that we can analyze the effects on total fertility. Follow (Lindo, 2010), I calculate the total fertility effect for the treated group in three steps. First, get the sum of the predicted post-displacement probability of births for the displaced people. Second, by setting the indicators of years after displacement equal to zero, I get the counterfactual probability of birth for the displaced people in each year after the displacement and add them together. Third, by averaging the differences between these two sums for each people in the displaced group, we can get the average total effects on the treated. The second to the last row in Table C.2 and Table C.3 reports the effects on total fertility. Standard errors are calculated through bootstrap. The results suggest a husband's job displacement has a total negative effects on fertility, reducing the total number of children by 0.11[12]. For women without college education, job displacement will lead to a 0.08 increase in total number of children. Similar to (Lindo, 2010), the estimated total fertility effects are not statistically significant.

2.4.2 Time Trend Model

By introducing ϕ_i, fixed effect model can give us consistent estimates even if there are some time-invariant differences between the displaced and non-displaced group. Now, suppose displaced and non-displaced workers are also differing in birth timing pattern. For example, displaced workers systematically have children earlier than the non-displaced ones. In this case, in order to get consistent estimates, we can apply the following specification, which allows heterogeneity in time trend by introducing $\omega_i \cdot t$ and $\eta_i \cdot t^2$:

$$Birth_{it} = \Gamma \mathbf{D}_{it} + \mathbf{X}_{it}' \Delta + \alpha_1 + \gamma_t + \phi_i + \omega_i \cdot t + \eta_i \cdot t^2 + \varepsilon_{it}$$

I use the quadratic form here to accommodate the inverse u-shape pattern we observed in Figures 6 through 9. To estimate this model, I use a generic quasi-difference technique(Wooldridge, 2010). First, for each individual i, regress $Birth_t$, $\mathbf{D}_t$ and $\mathbf{X}_t$ on t and t^2, and get the residuals as $\ddot{Birth}_{it}$, $\ddot{\mathbf{X}}_{it}$ and $\ddot{\mathbf{D}}_{it}$. Then, we can apply OLS to regress $\ddot{Birth}_{it}$ on $\ddot{\mathbf{X}}_{it}$, $\ddot{\mathbf{D}}_{it}$ and year dummies. Table C.5 shows the regression results. The estimated effects become larger when controlling for worker-specific time patterns[13], and thus we have more confidence that our previous estimates are not caused by the systematic differences in birth timing between displaced and non-displaced workers. The results in Table C.5 suggest that a husband's job displacement will reduce annual birth rate by 2.4 to 3.8 percentage points 4 years after the displacement. For women without college education, their job displacement will lead to 3-4.8 percentage points increase in annual birth rates one year after the displacement.

[12](Lindo, 2010) find that husband job displacement will reduce the total number of children by 0.098.

[13]Due to the similarity between quasi-difference and first difference, we get close results in Table C.5 and Table H.1, which reports regression results for first difference model.

2.4.3 Robustness Check

Considering the binary nature of $Birth_{it}$, I also try to use non-linear specification for regression analysis. The model can be expressed as:

$$Pr(Birth_{it} = 1) = \Phi(\Gamma \mathbf{D}_{it} + \mathbf{X}'_{it}\Delta + \alpha_1 + \gamma_t + \phi_i + \varepsilon_{it}) \tag{2.4}$$

Unfortunately, because of the incidental parameter problems, without further assumptions, I cannot get a consistent estimation for this model (Wooldridge, 2010).[14] Therefore, the Mundlak-Chamberlain approach (Mundlak, 1978) is introduced here, based on the additional assumption that:

$$\phi_i = \psi + \bar{\mathbf{X}}_i \xi + a_i,\, a_i | \mathbf{X_i} \sim Normal(0,\, \sigma_a^2).$$

where $\bar{\mathbf{X}}_i$ is the mean of time variant variables $\mathbf{X}_{it}$. This allows correlation between individual time-invariant characteristics and the means of $\mathbf{X}_{it}$. This methodology has been used successfully to estimate a correlated random effect model (Wooldridge, 2010). Now, the resulting model can be written as:

$$Pr(Birth_{it} = 1) = \Phi(Birth_{it} = \Gamma \mathbf{D}_{it} + \mathbf{X}'_{it}\Delta + \alpha_1 + \gamma_t + \psi + \bar{\mathbf{x}}_i \xi + u_{it}) \tag{2.5}$$

I use the random effect probit model to estimate (5). Table C.6 reports the average partial effects of displacement, which are similar to the coefficients reported in Table C.2 and Table C.3. However, the effects of women's job displacement are smaller and not significant any more. For men, Column 1 in Table C.6 suggests that the probability of having additional children will be reduced by 1.4 to 1.6 percentage points six years after a husband's job displacement. For women without college education, Column 3 in Table C.6 implies that, if they suffer a job displacement, their birth rate will increase by around 1 percentage points annually 4 years later, yet these effects are not significant.

[14] Fixed effect logit model can generate consistent estimators, however, marginal effects cannot be estimated since the individual fixed effects are not actually estimated. And the STATA program fails to converge here.

In regressions above, I only consider the effects of job displacement that occurred from 1984 to 1994. Since non-displaced people may encounter their first displacement after 1994 that will not be measured in our estimation, the effects estimated above will be underestimated. In order to check the magnitude of the possible bias, two other sample selections are implemented as robustness check. One is to drop all non-displaced people who report job displacements after 1994; the other is to use person-years from 1984 to 1992 only, as those observations include complete information of both birth and displacement. Results are reported in Table C.7 and Table C.8 separately. Estimated effects in Table C.7 are larger than the results reported in Table C.2 and Table C.3 for both men and women without college. When focusing on person-years from 1984 to 1992, the sample size is substantially reduced and lead to imprecise estimates for women. For men, the estimated effects are much larger now, and negative effects are observed for all periods including years prior to the displacement.

2.5 Plant Closure

The above estimates will be biased if firms selectively lay off employees whose performance was poor before the time of separation, and at the same time the performance is correlated with individual's fertility preferences for the future. One way to substantially reduce this kind of selection bias of displaced workers is to restrict analysis to workers who lose jobs as a result of workplace closings, as this type of job displacement is regarded to be more exogenous (Couch and Placzek (2010), Lindo (2010) and Del Bono et al. (2012)). Table C.9 displays the comparisons between the displaced and non-displaced groups when we restrict the displaced group to people who lose their jobs due to business closure. Compared with Table C.1, two groups are more similar, especially for women. More importantly, there is no significant differences in fertility between two groups.

Table C.10 shows the regression results based on this narrowly defined "displacement". The estimates here are similar to the main regression results in Table C.2 and Table C.3 in sign and magnitude, but are rarely statistically significant as a result of the small sample size of the displaced group. For the workplace-closing type of job displacement, it implies that women with no college education will increase their fertility 4 years after the displacement. The effect of male workplace-closing job displacement on fertility 6 years after the displacement is negative, yet the negative effects are not significant.

Charles and Stephens (2004) find that the divorce hazard increases after a spouse's job displacement, but that rise is found for displacement due to layoffs only but not firm closure. As our results imply that both types of job displacement can lead to the observed changes in fertility, we believe that the change in stability of marriage after job displacement is not the main reason for our observed changes in fertility behavior.

2.6 Propensity Score Estimates

Regression results from Section 5 generally show that fertility will be decreased if a husband suffers a job loss no matter his education level. In contrast to this, women with lower education will increase their fertility following a job displacement. We draw these conclusions from both fixed effect and time trend model. Further, this overall pattern is also robust to a correlated random effect probit model, different sample selection rules, as well as using a narrow definition of displacement that only includes workplace closings. There are still some other concerns worthy of investigation, however. As there is a non-random selection of which establishments are going out of business (Del Bono et al., 2012), workers might be selected into the closing firms. If this selection is also correlated with the preferences for fertility, then the estimated effects from narrowly-defined displacement still will be inconsistent. Therefore, we need some alternative ways to further control the heterogeneity among individuals. In order to do so, I introduce matching estimators to check the robustness of my findings. First, based on the propensity score, for each displaced worker, we choose a non-displaced individual who resembles him/her, and then we can use those pairs to calculate the fertility changes. Couch and Placzek (2010) use a similar method to calculate the earnings loss of displacement and get similar results to their fixed effect models.

The idea in using a matching estimator is to find a very similar control individual for each treated individual. To reduce the dimensionality of this problem, Rosenbaum and Rubin (1983) suggest that matches can be based on the predicted probability of the event. I include age and race dummies, as well as education, marital status, weeks worked and number of children 2 years before displacement as the predictors for "displacement". For time-variant dummies, Couch and Placzek (2010) use the level at the first year for all observations in estimating propensity scores, as they argue that the first year information provides substantial explaining power. This method is not appropriate here, as our sample commences at a relatively young age, of 14-22 in 1979. The sample does not have much in variation education, marital status, weeks worked and number of children in 1979, and thus cannot provide enough information for predicting the probability of displacement. Therefore, I follow the *inflated* method in Lechner (1999) to get the propensity scores. The *inflated* method artificially expands the non-displaced group by treating each non-displaced individual in each year during 1984 to 1994 as a separate observation with the respective displacement year. Now all non-displaced individuals have a displacement year, so we can easily use time-variant variables to calculate the propensity scores. Figure D.10 is the calculated propensities of displacement for women displaced in 1979 and their *counterparts*.[15] The distribution of probabilities is quite balanced, so we can proceed with further analysis.

After retrieving the propensity scores, I can find the best match (with the same propensity score or the closet propensity score) for each displaced person and calculate the difference in the change in fertility for each pair. In this calculation, first, the displaced person and their matched pair each have their own demeaned birth rate. Then, I can compare the difference for each pair, and average the DID across the sample for displaced people. This is referred to in the text as Fixed Effects Propensity Score Matching Estimator (FEPSME). The formula for FEPSME can be expressed as

[15] The estimation is based on a logit model.

follows:

$$FEPSME = \quad E\{[E[Y_{1it}|D_i=1,p(x_i)] - E[\bar{Y_{1i}}|D_i=1,p(x_i)]] - [E[Y_{0it}|D_i=0,p(x_i)] - E[\bar{Y_{0i}}|D_i=0,p(x_i)]]|D_i=1\}$$

While matching help us to eliminate the level differences, demeaning can further help to remove any systematic trend bias remaining across displaced and non-displaced individuals.

Table C.11 shows the preliminary results for the estimation of FEPSME. The standard errors are obtained through bootstrap. The smaller effects on fertility reported in Table C.11 are consistent with the idea that those who experience job displacements are systematically selected. Men are still shown to be less likely to have any additional children after job displacement, but these effects are only significant during 6-7 years after displacement. Less-educated women significantly reduce their probability of birth 4 years following the displacement, yet the effects we identified here is smaller than we found in Section 4.

2.7 Conclusion

The aim of this paper is to explore how fertility decisions are affected by job displacement in the U.S. by using micro data. Historical macro data shows a negative association between unemployment and birth rate, but there is a lack of casual analysis at micro level, especially for the effects of women's job loss in the U.S.

The major empirical results of this paper are as follows: Displacement of men will lead to reduction in fertility in the following years, while the effects of displacement for women depends on the women's education levels. For women with no college education, their fertility will increase after displacement; for women with college education, there is no significant effects on fertility after the job displacement. By introducing market childcare and household production model, the conceptual model can help to explain the heterogeneous effects for women.

The empirical findings are obtained through a fixed effect model and a time trend model which control the individual time-invariant heterogeneity and time trend heterogeneity, and are robust to several different specifications, including a correlated random effect model, different sample selection rules, a fixed effect propensity score matching model and a narrow definition of job displacement. One thing to be noticed is that job displacement (or job loss due to firm closure) is assumed to be exogenous in our estimation. To relax this assumption, we can try to use state by year unemployment rate as instruments and do 2SLS regressions in future research.[16]

Our results suggest that the short-run (1-3 years after the displacement) effects are quite small for both men and women. Therefore, job loss itself cannot fully explain the pro-cyclical trend in fertility that is observed with macro data. Some other mechanisms need to be explored to explain the causal relationships behind that trend. For instance, Adsera (2011) argue that the feeling of economics instability and the risk of job loss might have stronger impacts on current fertility decisions.

[16] The public NLSY79 data I use in this paper does not provide state information.

CHAPTER 3

THE MORE THE MERRIER? THE EFFECT OF FAMILY SIZE ON PARENT'S MENTAL HEALTH IN RURAL CHINA

3.1 Introduction

There is an old Chinese saying, "More children, more blessings". This represents the thoughts of most Chinese people that more children can generate more happiness for the parents. In fact, there are not many empirical studies of whether parenthood or number of children have effects on parental well-being, especially on mental health, in China or worldwide.

Mental health problem are costly to society both in terms of direct spending on treatment and through indirect costs such as the loss of productivity (Peng et al., 2013). According to the WHO, depression can result in disability, premature death, and severe suffering of those affected and their families (Demyttenaere et al., 2004). Hu et al. (2007) show that the total annual costs of depression in China is at least US $6.26 billion (at 2002 prices). In spite of the huge costs, mental health problems were neglected for a long time in China. In 2007, about 130 million people in China had mental illnesses, but among them only about 20% are diagnosed and treated (Lu et al., 2009). And the mental health problem in China is getting more widespread now. Data from the 2011 China Health and Retirement Longitudinal Study shows that over 40% of people age 45 and above, or about 140 million, show obvious symptoms of depression in 2011. Comparing to the estimated total number of people with mental illness in 2007, which is around 130 million for people at all ages, we see a huge increase. If the old saying is true, the decreasing fertility due to the One-Child Policy may contribute to the prevalence of depression to some extent because these people do not have as many children to bless them as they age. However, we need empirical evidences to confirm the effects of fertility on parent's mental health.

How would additional children affect the mental health of parents in the long run? The underlying mechanisms are complex, and at least three different channels have been suggested by the literature. The first chanel is the support effects, which include effects of both emotional and physical support from adult children. Dean et al. (1990) argue that expressive support from one's spouse and friends can reduce depression. Similarly, having children can generate a sense of gratitude and feelings of meaning in life (Evenson and Simon, 2005), which reduces the probability of experiencing depressive symptoms (Buber and Engelhardt, 2008). In China, having more children has been found to be associated with better support of aging parents receive (Pei and Pillai, 1999; Zimmer and Kwong, 2003), meanwhile, receiving more instrumental and financial support will lead to better mental health (Cong and Silverstein, 2008). The second channel is the budget effects. Additional children bring both direct costs (the consumption of the additional children) and opportunity costs (the reduction of parent's earning potential) to the family. Umberson and Gove (1989) indicate that, due to the economics costs and binding constraints, additional children can make their parents be vulnerable to mental diseases. Opportunity costs arise since having more children may increase the time spent on childcare and decrease maternal labor supply. Since employment history can directly affect one's health (Gove and Geerken, 1977), the effect of fertility on maternal health could work through the channel of labor supply. The third channel is the biological effects. Childbearing and nursing can have both negative and positive effects on a mother's health, which will consequently affect the mother's mental health (Kendig et al., 2007; Hurt et al., 2006) . As these three effects have different signs, the total effect of fertility on parent's mental health is an empirical question.

The empirical identification of the causal effect of fertility on parent's mental health is complicated by the endogeneity problem involved with the fertility decision. For instance, people with poor mental health may find it difficult for them to keep a stable marriage and to have more children (Buber and Engelhardt, 2008). In addition, people with different levels of mental health may have different preferences regarding fertility. As each individual chooses their optimal level of fertility, the number of children might be determined by the mental condition of the parent rather than the other way around (Kruk and Reinhold, 2014). With these endogeneity problems, the OLS estimator for coefficient of "number of children" will not be a consistent estimator of causal effects.

Research on the economics of happiness is always interested in the effects of children on parental happiness. Most of studies in this strand typically show negative or null effects of number of children on life satisfaction and/or subjective well-being (Di Tella et al., 2003; Alesina et al., 2004; Blanchflower, 2009; Gilbert, 2009). Based on these findings, economists claim that having more children does not make us happier (Angeles, 2010).

Comparing to the effects on happiness, existing literature about the effect of fertility on mental health are relatively thin, and most of them are provided by researchers in the field of public health, psychology and demography rather than economics. Their analyses usually ignore the endogeneity of fertility and generate surprisingly inconsistent results. Using data from the 1988 National Survey of Families and Households, Koropeckyj-Cox (1998) found in the U.S., childless women suffer greater rates of depression in middle and old age. On the other hand, using data from the U.S., Gove and Geerken (1977) and Burton (1998) both record the negative association between having children and mental health in the U.S.[1] Using multi-birth and sex composition of children as instruments for fertility, Kruk and Reinhold (2014) identify the negative effects of number of biological children on mental health for mothers in Europe. There are also a number of studies suggesting that the effects of number of children on parents' mental health are insignificant (Buber and Engelhardt, 2008; Mirowsky and Ross, 2002). For instance, Hank (2010) states no differences in mental health among middle-aged people with various numbers of children in Germany, and Kruk and Reinhold (2014) also finds no significant effects on father's mental health. To the best of my knowledge, Kruk and Reinhold (2014) is the only study accounting for endogeneity when looking at the mental health effects of fertility. The inconsistent findings in existing studies may in part be due to differences in characteristics of study group and variation in institutional context. More importantly, without carefully dealing with the endogeneity problem, differences in the selection of control variables will lead to different results as well.

[1]Gove and Geerken (1977) analyzed data from a survey conducted in Chicago, while Burton (1998) use a national probability sample.

The effects of fertility on mental health in China have not been fully explored. Silverstein et al. (2006) find that fewer children is associated with more depressive symptoms in China. On the other hand, Cong and Silverstein (2008) report no significant effects of "number of adult children" on parents' depression symptoms in China through their clustered regression analysis. Both treat fertility as an exogenous variable. In this paper, I will focus on the effects of additional children on mental health for people age 45 and above in rural China. As fertility is endogenously determined, I use the variation in the implementation of the One-Child Policy to construct exogenous variation in family size and do 2SLS.

In order to curb the rapid population growth, the Chinese government began to implement the One-Child Policy (OCP) in 1979. Under this policy, a married couple can only have one child in most areas. After the implementation of the OCP, female infanticide, forced abortion, and forced sterilization emerged in some places. To prevent these extreme cases, 19 provinces adopted the "1-boy-2-girl" rule in 1984, which means rural couples in these 19 provinces can have a second child if the first child is a girl (Qian, 2009).

In this paper, I use exogenous variations in fertility generated by the variation in the One-Child Policy in China. More specifically, I use the differences between couples with first-born girls and first-born boys. Based on data from the 2011 China Health and Retirement Longitudinal Study (first wave), results show that, for mothers age 45 and above in rural China, having more children has a negative effect on their mental health. The effect on father's mental health is also negative, but insignificant. (The effects are not statistically different for men and women.) By investigating the heterogeneous effects for people with different levels of education, I also find that the negative effects of fertility are stronger for mother with more education. This finding is consistent with the heterogeneous effects found in the first chapter of the dissertation, which indicates the negative effects on labor supply for mothers with more education is larger.

After identifying the effects on mental health, I look at one possible pathway of generating such effects. Wu and Li (2012) show that in China, having more children would decrease the resources allocated to mothers and thus affects their health outcomes, and poor physical health may lead to depression symptoms (Berkman et al., 1986). Two methods are applied to test the role of physical health. First, I use Self-Reported Health (SRH) as the indicator of physical health and apply the same instruments in the 2SLS. Second, SRH is added to the original 2SLS on mental health as control variables. Both strategies do not provide salient evidence on the pathway through physical health.

In this paper, I use scores on CES-D to measure an individual's mental health. As different people may have different scales of depressive feelings, they may give different answers for CES-D even under the same mental health status. To correct the possible bias in reported CES-D, I try to detect systematic scale bias using vignettes questions asked in CHARLS.

The remainder of the paper is laid out as follows. Section 2 provides background information on China's One-Child Policy and introduces the estimation strategy. Section 3 describes the data and summary statistics. Section 4 shows the main regression results including analysis of heterogenous effects for individuals with different levels of education. Section 5 discusses the possible reasons for these effects and tests the role of physical health using self-reported health. Section 6 presents a robustness check on the measurement bias on mental health. Section 7 concludes.

3.2 The One-Child Policy in China and Estimation Strategy

Due to very high fertility rates, the population growth rate in China reached 27.5‰ per year during 1962-1970, and the total population was 816 million in 1970 (Yang, 2004). To alleviate social, economic, and environmental problems caused by the huge population, the Chinese government began to curb population growth as early as 1972. The policy was summarized as "Later (late marriage and childbearing), Longer (birth spacing should be at least three years), and Fewer (two children should be enough)"(Qian, 2009). Implementation in that period relied primarily on propaganda, persuasion, and social pressure (McElroy and Yang, 2000).

And later on, in 1979, China began to implement a more restrictive policy, the "One-Child Policy" (OCP). Under this policy, a married couple can only have one child in most areas, except for couples living in the rural area in five provinces (Hainan, Yunan, Qinghai, Ningxia, Xinjiang)[2], who are allowed to have two children (Peng, 1996). In practice, implementation of this policy in some regions began as early as 1978, and the enforcement became nationally tightened in 1980. In areas subject to the OCP, a second birth was only permitted if one child would cause a household "real difficulties", e.g., very bad health condition of the first child. Couples who had an above quota birth without permission would be heavily fined[3]. Local cadres were given economic and promotion incentives to implement the policy. In the early 1980s, "parts of the country were swept by campaigns of forced abortion and sterilization and reports of female infanticide became widespread" (Greenhalgh, 1986).

To prevent female infanticide, forced abortion and forced sterilization, and to better address region-specific conditions, the Central Party Committee issued "Document 7" in April, 1984. "Document 7" allows regional variation in family planning polices. The main relaxation policy following "Document 7" is the "1-boy-2-girl" rule in 19 provinces, which allows rural couples in these 19 provinces to have a second child if the first born is a girl (Qian, 2009). But according to White (1991), these kind of permissions began to be issued as early as 1982. The different treatment of couples with first-born girls versus couples with first-born boys allows us to construct exogenous variation in fertility generated by the One-Child Policy.

Following the literature, the main regression model we are interested in can be written as:

$$CESD_{ict} = \beta kids2_{ict} + \mathbf{X'_{ict}}\delta + \alpha_1 + \gamma_t + \psi_c + \varepsilon_{ict} \tag{3.1}$$

where $CESD_{ict}$ is mental health indicator for woman i in county c, age cohort t[4]. $kids2_{ict}$ is a dummy variable that equals to 1 if the individual has two or more children. In some specifications we also use number of children ($nkids_{ict}$) to measure fertility. $\mathbf{X_{ict}}$ is a vector of individual i's characteristics, including gender[5], age, age when giving the first birth, gender of first child, ed-

[2]There are no restrictions on number of children for rural couples in Tibet.

[3]There are local variations in fines (Wei and Zhang, 2011).

[4]The measurement of $CESD_{ict}$ will be explained in detail in the next section.

[5]To explore differences in the effects by gender, we conduct analyses for whole sample, as well as for men and women separately.

ucation levels, number of siblings and self-reported health status during childhood; γ_t is the age cohort fixed effect, and ψ_c is the county fixed effect. Since mental health may affect individual's fertility decision, and people with poor mental health may have difficulties in maintaining a stable marriage and having more children, $cov(kids2_{ict}, \varepsilon_{ict}) \neq 0$. Therefore, the OLS estimator of β is not consistent. To address the endogeneity problem, in this paper, I use a set of differences-in-differences (DID) estimates to construct exogenous change in fertility, and then use these changes as instruments to $kids2_{ict}$ ($nkids_{ict}$) in Equation (1). The DID estimates will exploit differences in family size between people with first-born girls and first-born boys, before and after the OCP. The detailed explanation of this estimation strategy is as follows.

Suppose we have four couples. Both Couple 1 and Couple 2 have their first births in the year 1984. Couple 1 has a girl, and Couple 2 has a boy. Because of the amended One-Child Policy, Couple 1 can have a second child, while Couple 2 was not allowed to. In 2011, we may observe these two couples have different mental health levels. However, we should not attribute all the differences to the variation in the number of children, as the gender of the first birth might directly affect people's mental health (Kohler et al., 2005). However, we can remove this gender difference by using the other two couples. Both Couple 3 and Couple 4 have their first births in 1974, Couple 3 has a girl and Couple 4 has a boy. As there was no One-Child Policy in implementation until 1979, both couples can have a second child if they want to. We also have their mental health status in 2011. The differences in mental health between Couple 3 and Couple 4 now can be considered as the "intrinsic differences between people with first-born girls and first-born boys". Now if we assume the "intrinsic differences between parents with first-born girls and first-born boys" are the same for two couples with first births in 1974 and two couples with first births in 1984, then we can use a differences in differences method to remove the "intrinsic differences" and get the remaining exogenous variation in fertility generated by the One-Child Policy. This estimation will work as long as we have these four couples, but in the real dataset, we have thousands of couples, with first births either before or after the implementation of the policy. The large sample will make our estimation more convincing and more precise. The DID method can be expressed as $(First\text{-}Born\ Girl, After -$

$First\text{-}Born\ Boy, After) - (First\text{-}Born\ Girl, Before - First\text{-}Born\ Boy, Before)$. $After$ represents the young cohort, who is affected by the One-Child Policy, while $Before$ represents the old cohort who is not affected. Considering the endogeneity problem involved with the timing of first birth, I decided to use age cohorts rather than the year of the first birth in regressions. The first stage for $kids2$ can be expressed as equation (2), the interaction terms of whether first-born is a girl and age cohorts are our instruments.

$$kids2_{ict} = \sum_{l=22}^{44} (First\text{-}Born\ Girl_{ict} \cdot d_l)\, \phi_l + \mathbf{X'_{ict}}\mu + \alpha_3 + d_t + \pi_c + v_{ict} \tag{3.2}$$

When using Equation (2) as first stage regression with respect to either $kids2$ or $nkids$, fairly small F-statistics are reported, which raise the concern of weak instruments. (Details are discussed in Section 4.) To solve this problem, according to the cutoff age observed from regression equation (2) as well as Table E.8 in the first chapter of the dissertation, I divide all parents into two age groups: parents age 62 and above as the $Before$ group, and parents below 62 as the $After$ group. Since the OCP was nationally implemented in 1980, and most women with two or more children completed their second birth at or before age 30, only $After$ group who were at or under age 30 in 1980 would be restricted by the OCP and thus most likely to change their family size because of the relaxation of the OCP. In line with this argument, I found only interactions for cohorts born after 1950 are positive in the first stage in chapter 1. As a result, I use age 62 as the cutoff age and the first stage for $kids2$ is written as equation (3). Now, the interaction term of two dummy variables, whether first-born is a girl and whether in the $After$ group (Age<62) is the single instrument.

$$kids2_{ict} = First\text{-}Born\ Girl_{ict} \cdot After_{ict} + \mathbf{X'_{ict}}\mu + \alpha_3 + d_t + \pi_c + v_{ict} \tag{3.3}$$

For the exclusion restriction to be true, we need to assume that without the OCP, the difference in mental health between parents with first-born girls and first-born boys would be the same in both age cohorts.

With its relaxation of the hukou (household registration) system and other restrictive regulations, as well as its rapid economic development, China has been experiencing a huge scale of labor migration since 1990s. According to the recent population census, more than 261 million rural residents in China lived in places other than their birthplaces in 2010 (NBSC 2012). Due to the high mobility of recent years, identification based on geographical variations in the OCP implementation has a small power. That is, when I tried to run first stage regression of fertility for One-boy-two-girl provinces, One-child provinces and Two-children provinces separately, the coefficients on interaction term ($First\text{-}Born\,Girl_{ict} \cdot After_{ict}$) are all positive. Therefore, I did not distinguish respondents from different provinces and focused on age dummies and gender of first-born only to construct exogenous variation in fertility.

3.3 Data and Summary Statistics

The data used in this paper come from the 2011 China Health and Retirement Longitudinal Study (CHARLS), which is a part of a set of longitudinal aging surveys including the Health and Retirement Study (HRS) in the United States and similar surveys in 20 other countries. CHARLS is a national representative data set of the residents in China age 45 and above, with no upper age limit (Zhao et al., 2012). 150 counties were randomly chosen from eight geographic regions across China. CHARLS contains a wide range of information on demographics, family structure/transfer, health status and functioning, etc. Detailed information of CHARLS can be found at *http://charls.ccer.edu.cn/en*. In this paper, we will only include individuals with agricultural household registration and residing in countryside. The sample is further restricted to people with first birth after the age of 15. With these restrictions, we obtain a sample of 9,657 individuals from 28 provinces, among them, 4,517 (46.8%) are men[6].

In this paper, the mental health of parents is measured by depressive symptoms based on a Chinese version of 10 item CES-D (Center for Epidemiologic Studies Depression Scale). A full list of the 10-item CES-D is provided in Table I.1. The 20 item CES-D is one of the most common screening tests for identifying depressive symptoms in the general population. The 10-item scale is the shorter version, which also provides a self-reported measure of an individual's depressive feelings

[6]When using $kids2$ to measure fertility, we restrict the sample to parents with at least one child.

and behaviors in the past week. It is designed for studies investigating the relationship between depression and other variables (Kohout et al., 1993), and its reliability and validity have been confirmed (Boey, 1999). The criterion for the assessment of mental health is the sum of individual symptoms[7]. The CES-D is a continuous measure of depressive symptoms, with score ranging from 0 to 30. Higher scores indicating higher levels of depression. The mean value of CES-D in the whole sample is 8.87, with a mean of 7.76 for men and 9.85 for women. Some literature regards 10 in CES-D as the threshold value for depression (Irwin et al., 1999). If we follow this criteria, then 39.8% of people in our sample have depression. Figure F.1 plots the average CES-D scores against the number of children, suggesting a nonlinear effect of fertility. Comparing to childless people, parents have better mental health when they have no more than 4 children. On the other hand, conditional on the presence of children, additional children will raise CES-D score in general.

Table E.1 gives the summary statistics of CES-D, number of children, as well as other control variables that are standard in the literature examining the determinants of mental health: age, gender, age at first birth, gender of the first birth, education level, number of siblings, and self-reported health status during childhood. For number of children, I only include biological children, and I include both children alive and those already deceased (Kruk and Reinhold, 2014). The average number of children is 2.96 in our sample, and over 88.6% of the respondents have more than one child. The average number of siblings that the respondents have is 3.9, which implies 4.9 is the average number of children of their parents. Comparing to the average number of children that they themselves have, we see shrinkage in family size over a generation. The average age of the respondents is 58.6, and the average age at first birth is 24. This indicates that most of the people in my sample had children several decades ago and that the results are the long-term consequences of fertility. On average, people in my sample have 4.7 years of education, only about half of them have completed primary school. 74.5% of the people report they had good, very good or excellent health when they were young.

[7]CES-D score equals to the sum of eight "negative" indicators plus the absence of two "positive" indicators. Detailed formula is reported in Table I.1

Table E.2 shows the basic DID estimates of probability of having two or more children, number of children and mental health, based on gender of first-birth. The “Young Cohort” in Table E.2 includes individuals born in or after 1950. Considering 1980 is the year that the OCP was nationally implemented, and most people with two or more children gave birth to their second child before the age of 30, it is reasonable to believe that people born in or after 1950 (who were above 30 in 1980) will be constrained by the OCP. The relaxation of OCP will therefore lead to differences in fertility for individuals in this cohort. The above two panels suggest that fertility decreases for both people with first-born girls and first-born boys, but the reduction is significantly greater for people with first-born boys. In terms of number of children, the difference between young and old cohorts for parents with a first-born boy is 0.05 bigger than that for parents with a first-born girl. Meanwhile, the bottom panel shows that, for parents of both first-born girl and first-born boy, the young cohort has a better mental health than the old cohort, but the improvement in mental health is bigger for the first-born boy parents, who have greater decrease in fertility.

3.4 Regression Results

Some previous studies show that due to different gender roles within a family, the effects of fertility on mental health are different for mothers and fathers (Islam and Smyth, 2010). As traditional caregivers, women may suffer more from having additional children. In addition, some research finds that the effects of marriage on depressive symptoms are different for men and women (Earle et al., 1997). In order to investigate the different effects for each group, besides the regressions based on the whole sample, I also run the regressions for men and women separately for all specifications.

Table I.2 and Table I.3 display our first stage regressions of $kids2_{ict}$ and $nkids_{ict}$ on the interaction terms of age cohort dummies and gender of first birth (ϕ_l in Equation (2)).One useful check of instrument validity is to see its effect on the untreated group, which is the old cohorts in this case. In Table I.2, we find the interactions are significant in the first stage for young cohorts, but not significant for old cohorts. Because the amended OCP allowed parents to have a second birth if the first one was a girl, we would expect the instruments to have larger power in explaining the discrete change in number of children from 1 to 2. Comparing Table I.2 and Table I.3, we find Table I.3 reports fewer significant terms for the young cohorts.

An important concern with the 2SLS is the weak instruments problem (Staiger and Stock, 1994). The Cragg-Donald Wald F statistics reported in Table I.2 and Table I.3 suggest that the interaction terms of age cohort dummies and gender of first birth tend to be pretty weak instruments, with F-statistics between 1.44 to 6.79 (Table I.4 and Table I.5 show that using interaction terms for cohorts age 61 to age 70 gives higher F-statistics, but still cannot pass the weak IV test at the 10% level.). Therefore, in the following work, I will not use them as my instruments. The purpose of Table I.2 and Table I.3 is to provide a basis for choosing cutoff age. From Appendix Tables 2-5, we can find that the interaction terms are mainly positive for cohorts younger than 62 (people born in or before 1950). This is consistent with my findings in Chapter 1 of the dissertation. Therefore, I am going to use 62 as the cutoff age to divide people into two groups, and use the interactions of group dummy and gender of first-birth as the single instrument. I also shift the cutoff age to 61, which yields similar results.

Table E.3 and Table E.4 show the first stage regressions for "Two or More Children" and "Number of Children" respectively. The coefficients on the instrument variable, the interaction terms of "younger than 62" and "first-born is a girl" are significant for both specifications. The F-statistics suggest that this instrument passes the weak instrument tests at the 1% level for all specifications. Table E.3 (Table E.4) indicates that compared to people age 62 and above, the difference in probability of having two or more children (number of children) between parents with first-born girls and first-born boys for people younger than 62 is 10.2% (0.263) larger.

Table E.5 reports the regression results of Equation (1) using OLS. The left panel uses "Two or More Children" ($kids2$) as a measure of fertility, while the right panel uses "Number of Children" ($nkids$). Both specifications suggest no effects of fertility on parents' mental health. Child birth decision is endogenous to mental health, and therefore OLS estimates might be biased. Table E.6

shows the regression results of 2SLS using the interaction of age group dummy and gender of first birth as the instrument. Again, the left panel is for $kids2$, while the right panel is for $nkids$. After controlling endogeneity in fertility, now the coefficients on fertility indicate that mothers with more children have higher risk of experiencing depression symptoms in rural China, while the effects on fathers are generally not significant. However, the different effects for men and women are not statistically significant, as the coefficients on the interaction term of fertility and gender are insignificant when I use a full model with all control variables interacted with gender. Comparing Table E.5 and Table E.6, we can find that without controlling endogeneity, OLS underestimates the negative effects of fertility on parents' mental health. The underestimation might be caused by either the negative effects of depression on marriage and fertility, or the negative correlation between poor mental health and preference for children. Let's take a closer look at the coefficients. The coefficients on $kids2$ in Column (3) implies that having additional children will raise CES-D by 9.83 for women with one child, and the coefficients on number of children in Column (6) suggests that having one more child will increase CES-D by 3.33, that's 33.8% of the average level for all women in our sample. In my sample, for mothers with more than 1 child, the average number of children is 3.31. For a mother with 1 child, using the coefficients in Column (6), we can calculate that the total effects on depression from additional 2.31 children is 7.69, similar to the estimated effects on $kids2$. [8]

The estimated effects of other control variables are largely as expected. Early childbearing, separation, divorce, and widowing will raise the symptoms of depression, while more education leads to better mental health. The findings are also consistent with the extensive literature showing that men have significantly better mental health than women.

As poor mental health is found to be positively associated with mortality rate, one may worry about the problem of sample selection. Individuals with higher CES-D tend to die early (Demyttenaere et al., 2004), which means that the exclusion of this group of people from our sample tends to bias

[8] The standard errors of 2SLS estimates are somewhat large, we tried to use TS2SLS to solve this. Unfortunately, the population in the census data are not the same as the population here, and the regression results are not satisfying.

the estimates upwards. Therefore, the estimated negative effects in Table E.6 can be viewed as a lower bound.

So far, I have focused on the average effects of fertility on men's and women's mental health. For individual, however, the effects on mental health may vary across people with different characteristics. There are different reasons to expect the effects to be heterogeneous (Cáceres-Delpiano and Simonsen, 2012). For example, I found the negative effects of child birth on female labor force participation to be stronger for more educated women (women with at least primary school education) in rural China in Chapter 1. If the employment is the main channel through which fertility affects mental health, then we would expect to see larger negative effects on mental health for more educated women when run regressions for women with different education levels separately. To check for this, I divide the sample into subgroups based on people's education level and run regressions on subgroups separately to check the heterogeneous effects. If an individual has less than 6 years of schooling, he/she is put into the less educated group; otherwise he/she belongs to the more educated group.

Table E.7 and Table E.8 present the 2SLS results of heterogeneous effects on mental health for people with different levels of education (The OSL estimates of heterogeneous effects are reported in Table I.7 and Table I.8). The sample size is much smaller for each subgroup, as a result, many coefficients are no longer significant. In terms of magnitude, there seems no heterogeneity in effects on mental health for men with different education levels. Meanwhile, the results from both Table E.7 and Table E.8 reveal a larger negative effects of fertility on women's mental health for the more educated group. For example, estimates from columns (3) and (6) in Table E.8 show that one more children will raise CES-D by 1.5 points for the less-educated group, and 5.3 points for the more-educated group. If we take this as true, then one possibility to explain the negative effects on mental health might be the factors of time allocation and employment. Women reduce their labor force participation when they have more children (as found in chapter 1), and fewer employment attachments will lead to poorer mental health. As both of these coefficients are insignificant, however, we cannot come to a firm conclusion that the negative effects on mental health are stronger for mothers with more education.

3.5 Physical Health and Living Arrangements

Given the evidence of negative effects of number of children on mother's mental health, we may want to further look at the possible pathways of generating such effects. As discussed in introduction part, three different channels have been suggested by the literature that can affect the mental health of parents in the long run. In this paper, we're going to investigate two of them, biological effects and support effects.

3.5.1 Self-Reported Health and Chronic Diseases

It has been shown that poor physical health may lead to depression symptoms (Berkman et al., 1986), and many studies suggest that fertility may lead to some physical health problems for the parents in the long run. Using data from NHIS during 1982-2003, Cáceres-Delpiano and Simonsen (2012) conclude that having more children will increase the likelihood of having high blood pressure and becoming obese for the mothers. In China, Wu and Li (2012) show that having more children would decrease the resources allocated to mothers and thus leads to both parents being underweight. Based on a sample of women from Shanghai, Zhang et al. (2009) provide evidence that having more children significantly increases the risk of stroke later in life. More recently, using pilot data from the China Health and Retirement Longitudinal Survey (CHARLS), Islam and Smyth (2010) find that having fewer children has a positive effect on self-reported health[9]. In this paper, in order to test the hypothesis that childbearing negatively impacts the mother's physical health, which in turn exerts long-term effects on mental health, I use two different methods. First, follow Islam and Smyth (2010), I use 2SLS to estimate the effects of fertility on the self-reported health (SRH), which is an indicator of physical health. The SRH question in CHARLS is phrased

[9]Due to the limited number of observation, they do not look at men and women separately.

as follows: "In general, how would you rate your health?" Respondents are asked to choose a point along a five-point scale, and two scales are randomly provided. One is "(1) excellent, (2) very good, (3) good, (4) fair, and (5) poor"; and the other is "(1) very good, (2) good, (3) fair, (4) poor, and (5) very poor". I combine the responses together, and recode the SRH as: excellent/very good=4, good=3, fair=2, and poor/very poor=1. The higher the score is, the better health is reported.[10]

Table E.9 shows highly similar results to Table E.6, which represents 2SLS results for mental health. The first row in Table E.9 indicates that women with more children have generally poorer self-reported health (only significant at 10% level), while the effects of fertility on men's mental health are insignificant. As self-reported health metrics are sometime argued to be biased due to subjectivity and measurement error (Mu, 2013), regressions on the diagnosis of some chronic diseases are used as the robustness check. Table E.10 reports 2SLS results on the diagnosis of three most common chronic diseases in our sample, i.e. arthritis or rheumatism (32.42%), hypertension (23.44%) and stomach or other digestive disease(17.87%). It suggests that additional children will not lead to higher probability of diagnosis of any of these chronic diseases.

The second way to test the role of physical health is to add physical health indicator to the original 2SLS on mental health. If the effects of fertility on mental health are due to the worsening of physical health caused by additional children, we expect the coefficient on fertility will be reduced once we condition on indicator of physical health.

Table E.11 shows the 2SLS results when we include the SRH as a control variable. The significantly negative coefficients on SRH suggest that physical health has a positive effect on mental health. For the other variables, most of the coefficients do not change. The coefficient on fertility is reduced somewhat but still significantly positive. This implies that there are still some other reasons for the negative effects on mental health.

[10]For ordered response models, we can use ordered probit/logit as well. To control for endogeneity, can apply Rivers-Vuong method, which gives us similar results to Table E.9.

3.5.2 Living Arrangements

Living arrangements affect support effects directly, as living together with children leads to higher emotional and physical support from adult children (Evenson and Simon, 2005). Buber and Engelhardt (2008) show that supports from children substantially reduce the probability of experiencing depressive symptoms . In China, having more children has been found to be associated with higher probability of coresidence with children (Zimmer and Kwong, 2003). In this paper, similar to the above analysis on physical health, I try two different methods to test the intermediating effects of living arrangements on mental health. First, 2SLS is applied to estimate the effects of fertility on living arrangements, then living arrangements indicator is added to the original 2SLS on mental health.

CHARLS asked the respondent the living situation of each of their child, I constructed a dummy variable called "living together" to measure the living arrangement. "living together" equals to one, if the respondents report having at least one child currently living in the same household, or the same or adjacent dwelling/courtyard; equals to zero for all other cases. In our sample, 51.27% of the respondents are living together with their children, and people with one child (51.55%) reports similar rate of "living together" to people with more than one children (51.24%).

Table E.12 indicates that different number of children does not lead to different living arrangements in our sample. The effects of co-residence with children on mental health are reported in Table E.13. It shows that living with children can help improve men's mental health at 10% significance level, while the effects of co-residence on women's mental health are insignificantly positive. After controlling living arrangements, the change in coefficients of fertility is very small comparing to the main results in Table E.6. This is consistent with the findings in Table E.12. Therefore, our sample cannot provide evidence on living arrangements as a pathway for fertility to affect parents' mental health.

3.6 Robustness Check

In this paper, I use scores on CES-D to measure individual's mental health. As CES-D is a self-reported measurement, it might be biased. Different people may have different scales of depressive feelings, and thus they may give different answers for CES-D even if they have the same mental health status. For example, if people with more children tend to have systematically different scales than people with fewer children, then the 2SLS estimates will be the sum of the true effects of fertility and the differences in scales. To check whether there are some systematic differences in scales, the best way is to compare peoples' scales directly. One way to get an individual's scale is to use vignette questions. If the same vignette questions are shown to different respondents, the only reason for respondents to give different responses is their heterogeneous scales.

In CHARLS, vignette questions covering six domains (body pain, sleep disorder, difficulty in mobility, cognition problems, shortness of breath, and mental problems) are included in the questionnaire. Respondents are asked to evaluate the health conditions of the hypothetical persons. They are given two randomly selected domains with three vignette questions for each. In our sample, only 810 people, 451 of them women, were asked about vignette questions on mental problems. I calculate vignette scores by summing up the scores from three questions. A higher score means people tend to have a lower threshold for reporting depressive symptoms, and thus are more likely to over-report their own depression. A lower score means people tend to have a higher threshold for reporting depressive symptoms, and thus are more likely to under-report their own depression. Figure F.2 and Figure F.3 plot vignette question scores against the two key variables in this study, number of children and the CES-D score, respectively. There is no salient evidence of any systematic bias in vignette scores in these two figures. Therefore, the measurement error in CES-D due to systematic differences in respondent's scales is not a big concern in this study. I attempted to control vignette scores in 2SLS regressions, but due to the very small sample size, most of the results are not precise.

3.7 Conclusion

The total effect of fertility on parent's mental health is an empirical question, which is complicated by the endogeneity problem involved with the fertility decisions. Most of the existing literature ignores the endogeneity and generate surprisingly inconsistent results. This paper is the first one to treat fertility as endogenous, and to identify the causal effect of fertility on parent's mental health in China using 2SLS. China's One-Child Policy, which came as a surprise to many families and had variations in its implementation, is s natural experiment. I construct exogenous variation in fertility through a DID strategy based on the OCP and use that as the instrument to do 2SLS. Using data from the 2011 China Health and Retirement Longitudinal Study, results show that, for women age 45 and above in rural China, having more children has a negative effect on their mental health. The effect on men's mental health is also negative, but insignificant.

After estimating the effects on mental health, I conduct further investigations to test the role of physical health in generating such effects using two methods. First, I use Self-Reported Health (SRH) as the indicator of physical health and apply the same instruments in the 2SLS. Second, I add SRH to the original 2SLS on mental health as control variables. The results suggest that physical health can help to explain a very limited part of the total effect on mental health.

I found stronger effects of fertility on mental health for more educated women when investigating the heterogeneous effects. This is in line with the findings in the first chapter that more educated women are more responsive to fertility change in terms of labor force participation decisions. These factors suggest that the change in employment attachment may contribute to the change in mental health. In the future, with better information on employment history, we can more thoroughly test this hypothesis.

China is now in a process of relaxing the One-Child Policy. With more children, this paper suggests that the mental health problems in China might get even worse. More resources will be needed to adequately address this problem.

APPENDICES

APPENDIX A

TABLES FOR CHAPTER 1

Table A.1: Descriptive Statistics, Women aged 16-45 with at least one child

	Definition	Means	S.D.
# of Children	Number of surviving children	2.223	0.0011
Kids2	=1 if mother has more than 1 child, =0 otherwise	0.756	0.0005
LFP	=1 if the woman has a job or is "waiting to be employed" on the day of the census, =0 otherwise	0.922	0.0003
Age	Mother's age in years on July 1st, 1990	32.436	0.0067
Age at 1st Birth	Mother's age in years when first child was born	22.784	0.0030
Age at 2nd Birth	Mother's age in years when second child was born	25.527	0.0041
non-Han	=1 if both mother and father are minority, =0 otherwise	0.090	0.0003
First-Born Girl	=1 if the first child is a girl, =0 otherwise	0.484	0.0006
Primary	=1 if mother's highest education achievement is primary school, =0 otherwise	0.476	0.0006
Junior	=1 if mother's highest education achievement is junior high school, =0 otherwise	0.218	0.0005
Senior	=1 if mother's highest education achievement is senior high school, =0 otherwise	0.042	0.0002
Primary_Husband	=1 if father's highest education achievement is primary school, =0 otherwise	0.415	0.0005
Junior_Husband	=1 if father's highest education achievement is junior high school, =0 otherwise	0.393	0.0005
Seniro_Husband	=1 if father's highest education achievement is senior high school, =0 otherwise	0.117	0.0004

Notes: Data is from 1990 China Population Census. Sample includes women aged 16-35 with at least one child who are household heads or the spouse of the household heads. Women whose first child is less than one year old are excluded.

Table A.2: Summary Statistics for Each Sample

	1-Child Prov[1]		1-Boy-2-Girl Prov[3]		2-Children Prov[2]	
Obs.	165,969		612,785		45,855	
	Means	S.D.	Means	S.D.	Means	S.D.
# of Children	1.826	0.0020	2.296	0.0013	2.689	0.0061
Kids2	0.608	0.0012	0.790	0.0005	0.843	0.0017
LFP	0.984	0.0003	0.901	0.0004	0.981	0.0006
Age	32.992	0.0153	32.319	0.0078	31.973	0.0289
Age at 1st Birth	22.971	0.0061	22.789	0.0035	22.047	0.0132
Age at 2nd Birth[4]	26.074	0.0103	25.486	0.0046	24.621	0.0158
non-Han	0.024	0.0004	0.083	0.0004	0.416	0.0023
First-Born Girl	0.482	0.0012	0.484	0.0006	0.486	0.0023
Primary	0.533	0.0012	0.469	0.0006	0.353	0.0022
Junior	0.218	0.0010	0.223	0.0005	0.142	0.0016
Senior	0.039	0.0005	0.044	0.0003	0.024	0.0007
Primary_Husband	0.469	0.0012	0.397	0.0006	0.450	0.0023
Junior_Husband	0.379	0.0012	0.405	0.0006	0.285	0.0021
Senior_Husband	0.094	0.0007	0.128	0.0004	0.064	0.0011

Notes: Data is from 1990 China Population Census.

[1] List of 1-Child Provinces: Hainan, Yunnan, Qinghai, Ningxia, Xinjiang.

[2] List of 2-Children Provinces: Beijing, Shanghai, Tianjin, Jiangsu, Sichuan.

[3] 1-Boy-2-Girl Provinces: All other provinces except Tibet, 19 provinces in total.

[4] This is based on mothers with at least two children. The sample sizes are 100,478 for 1-child provinces, 482,842 for 1-boy-2-girl provinces, 38,597 for 2-children provinces.

Table A.3: Han Vs. non-Han and First-Born Girl Vs. First-Born Boy

	1-Child Prov+1-Boy-2-Girl Prov				1-Boy-2-Girl Provinces			
	Han		non-Han		First-born Girl		First-Born Boy	
Obs.	723,949		54,805		296,183		315,777	
	Means	S.D.	Means	S.D.	Means	S.D.	Means	S.D.
# of Children	2.179	0.0011	2.413	0.0048	2.411	0.0019	2.187	0.0017
Kids2	0.748	0.0005	0.797	0.0017	0.810	0.0007	0.770	0.0007
LFP	0.919	0.0003	0.911	0.0012	0.901	0.0005	0.901	0.0005
Age	32.476	0.0072	32.286	0.0268	32.271	0.0111	32.362	0.0109
Age at 1st Birth	22.823	0.0031	22.893	0.0123	22.841	0.0051	22.738	0.0048
Age at 2nd Birth[1]	25.581	0.0044	25.666	0.0158	25.476	0.0065	25.494	0.0065
non-Han	n.a.	n.a.	n.a.	n.a.	0.083	0.0005	0.083	0.0005
First-Born Girl	0.484	0.0006	0.485	0.0021	n.a.	n.a.	n.a.	n.a.
Primary	0.486	0.0006	0.439	0.0021	0.470	0.0009	0.468	0.0009
Junior	0.224	0.0005	0.199	0.0017	0.223	0.0008	0.224	0.0007
Senior	0.043	0.0002	0.045	0.0009	0.044	0.0004	0.045	0.0004
Primary_Husband	0.412	0.0006	0.416	0.0021	0.397	0.0009	0.398	0.0009
Junior_Husband	0.403	0.0006	0.361	0.0021	0.407	0.0009	0.404	0.0009
Senior_Husband	0.121	0.0004	0.116	0.0014	0.128	0.0006	0.128	0.0006

[1] This is based on mothers with at least two children. The sample sizes are 539,737 for Han and 43,583 for non-Han respectively in the 1-child provinces and the 1-boy-2-girl provinces, 239,597 for first-born girl and 242,830 for first-born boy in the 1-boy-2-girl provinces.

Table A.4: DID Estimates Regarding Ethnicity

	Having 2 or More Children			Labor Force Participation		
	Old Cohorts[1]	Young Cohorts[2]	Difference	Old Cohorts[1]	Young Cohorts[2]	Difference
Han	0.964	0.691	-0.273	0.904	0.920	0.016
(s.d./s.e.)	(0.1851)	(0.4620)	(0.0018)	(0.2943)	(0.2706)	(0.0011)
non-Han	0.970	0.734	-0.236	0.923	0.910	-0.012
(s.d./s.e.)	(0.1707)	(0.4418)	(0.0060)	(0.2668)	(0.2856)	(0.0040)
Difference	0.005	0.043	0.037	0.019	-0.010	-0.029
(s.e.)	(0.0026)	(0.0021)	(0.0065)	(0.0041)	(0.0012)	(0.0040)

Notes: The sample is made of observations from the restricted provinces (the 1-child provinces and the 1-boy-2-girl provinces). Standard errors are in parenthesis.

[1] Old Cohorts are consisted of mothers older than 40 but younger than 46 in 1990.

[2] Young Cohorts are consisted of mothers age 40 or younger.

Table A.5: DID Estimates Regarding Gender of First Birth

	Having 2 or More Children			Labor Force Participation		
	Old Cohorts[1]	Young Cohorts[2]	Difference	Old Cohorts[1]	Young Cohorts[2]	Difference
First-Born Boy	0.967	0.713	-0.254	0.878	0.903	0.025
(s.d./s.e.)	(0.1775)	(0.4522)	(0.0027)	(0.3270)	(0.2958)	(0.0018)
First-Born Girl	0.971	0.756	-0.216	0.893	0.902	0.008
(s.d./s.e.)	(0.1672)	(0.4297)	(0.0027)	(0.3085)	(0.2975)	(0.0020)
Difference	0.004	0.042	0.039	0.015	-0.001	-0.016
(s.e.)	(0.0015)	(0.0012)	(0.0038)	(0.0028)	(0.0008)	(0.0027)

Notes: The sample is made of observations from 1-Boy-2-Girl provinces. Standard errors are in parenthesis.

[1] Old Cohorts are consisted of mothers older than 40 but younger than 46 in 1990.

[2] Young Cohorts are consisted of mothers age 40 or younger.

Table A.6: OLS and 2SLS Estimates of the Effect of Additional Children on Female LFP

	(1) Restricted	(2) 1-Boy-2-Girl	(3) Restricted	(4) 1-Boy-2-Girl
A: OLS				
kids2	0.000	-0.002	0.015	0.013
	(0.001)	(0.002)	(0.002)***	(0.004)***
non-Han	0.003		0.008	
	(0.008)		(0.008)	
First-Born Girl	-0.001	-0.001	0.000	0.000
	(0.000)	(0.001)*	(0.001)	(0.001)
Observations	778,754	561,921	396,607	282,125
B: 2SLS				
kids2	-0.153	-0.084	-0.157	-0.108
	(0.046)***	(0.036)**	(0.054)***	(0.041)***
non-Han	0.004		0.01	
	(0.008)		(0.008)	
First-Born Girl	0.008	0.003	0.01	0.005
	(0.003)***	(0.002)	(0.003)***	(0.002)***
Cragg-Donald Wald F statistic	16.286	11.732	44.735	19.708
Hansen J statitstic	31.466	31.008	30.539	26.566
Observations	778,710	561,875	396,580	282,095

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions controls age cohort dummies, mother's age at first birth, education levels for both parents, county fixed effects. Col (1) is on observations from the 1-child provinces and the 1-boy-2-girl provinces; Col (2) is on Han people only in the 1-boy-2-girl provinces; Col (3) is on observations from the 1-child provinces and the 1-boy-2-girl provinces, except for mothers with first birth later than 1981; Col (4) is on Han people only in the 1-boy-2-girl provinces, except for mothers with first birth later than 1981.

Table A.7: Coefficients of Interaction Terms for the 1st Stage Regressions Regarding Ethnicity

	(1)	(2)		(1)	(2)
age16*non-Han	-0.028		age31*non-Han	0.085	0.082
	(0.070)			(0.014)***	(0.012)***
age17*non-Han	0.001		age32*non-Han	0.081	0.084
	(0.054)			(0.014)***	(0.011)***
age18*non-Han	0.059		age33*non-Han	0.076	0.082
	(0.071)			(0.013)***	(0.011)***
age19*non-Han	0.072		age34*non-Han	0.066	0.079
	(0.039)*			(0.013)***	(0.011)***
age20*non-Han	0.005		age35*non-Han	0.053	0.065
	(0.028)			(0.012)***	(0.010)***
age21*non-Han	0.036		age36*non-Han	0.048	0.061
	(0.021)*			(0.012)***	(0.009)***
age22*non-Han	0.039		age37*non-Han	0.027	0.042
	(0.020)**			(0.012)**	(0.009)***
age23*non-Han	0.068	0.712	age38*non-Han	0.028	0.035
	(0.019)***	(0.263)***		(0.011)**	(0.009)***
age24*non-Han	0.049	0.096	age39*non-Han	0.012	0.019
	(0.019)***	(0.042)**		(0.011)	(0.009)**
age25*non-Han	0.052	0.087	age40*non-Han	0.004	0.01
	(0.019)***	(0.020)***		(0.011)	(0.008)
age26*non-Han	0.058	0.099	age41*non-Han	-0.006	0.006
	(0.019)***	(0.022)***		(0.011)	(0.008)
age27*non-Han	0.057	0.037	age42*non-Han	-0.012	-0.005
	(0.019)***	(0.022)*		(0.011)	(0.008)
age28*non-Han	0.055	0.062	age43*non-Han	-0.006	0.005
	(0.018)***	(0.016)***		(0.011)	(0.008)
age29*non-Han	0.081	0.051	age44*non-Han	-0.003	0.005
	(0.016)***	(0.017)***		(0.012)	(0.009)
age30*non-Han	0.079	0.053			
	(0.016)***	(0.015)***	*Observations*	778,754	396,607

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions controls age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, county fixed effects. Col (1) is on observations from the 1-child provinces and the 1-boy-2-girl provinces; Col (2) is on observations from the 1-child provinces and the 1-boy-2-girl provinces, except for mothers with first birth later than 1981.

Table A.8: Coefficients of Interaction Terms for the 1st Stage Regressions Regarding Gender of 1st Birth

	(1)	(2)		(1)	(2)
age16*First-Born Girl	-0.043		age31*First-Born Girl	0.05	0.06
	(0.017)**			(0.009)***	(0.008)***
age17*First-Born Girl	-0.034		age32*First-Born Girl	0.054	0.056
	(0.064)			(0.009)***	(0.007)***
age18*First-Born Girl	0.042		age33*First-Born Girl	0.053	0.064
	(0.045)			(0.009)***	(0.008)***
age19*First-Born Girl	-0.014		age34*First-Born Girl	0.047	0.064
	(0.025)			(0.009)***	(0.007)***
age20*First-Born Girl	0.004		age35*First-Born Girl	0.04	0.053
	(0.017)			(0.008)***	(0.007)***
age21*First-Born Girl	-0.016		age36*First-Born Girl	0.03	0.044
	(0.012)			(0.008)***	(0.007)***
age22*First-Born Girl	0.008		age37*First-Born Girl	0.017	0.029
	(0.01)			(0.008)**	(0.006)***
age23*First-Born Girl	0.018	1.021	age38*First-Born Girl	0.011	0.024
	(0.010)*	(0.011)***		(0.007)	(0.006)***
age24*First-Born Girl	-0.001	-0.086	age39*First-Born Girl	0.008	0.016
	(0.009)	(0.079)		(0.007)	(0.006)***
age25*First-Born Girl	0.012	0.037	age40*First-Born Girl	-0.001	0.008
	(0.009)	(0.030)		(0.007)	(0.005)
age26*First-Born Girl	0.019	0.036	age41*First-Born Girl	-0.002	0.002
	(0.008)**	(0.019)*		(0.007)	(0.005)
age27*First-Born Girl	0.016	0.047	age42*First-Born Girl	-0.001	0.002
	(0.008)**	(0.012)***		(0.007)	(0.005)
age28*First-Born Girl	0.006	0.039	age43*First-Born Girl	0.009	0.009
	(0.009)	(0.011)***		(0.008)	(0.006)

Table A.8 (cont'd)

age29*First-Born Girl	0.017	0.038	age44*First-Born Girl	0.009	0.007
	(0.009)*	(0.010)***		(0.008)	(0.006)
age30*First-Born Girl	0.038	0.045			
	(0.009)***	(0.009)***	*Observations*	561,921	282,125

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions controls age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, county fixed effects. Col (1) is on Han people only in the 1-boy-2-girl provinces; Col (2) is on Han people only in the 1-boy-2-girl provinces, except for mothers with first birth later than 1981.

Table A.9: Coefficients of Interaction Terms for the Reduced Form Regressions Regarding Ethnicity

	(1)	(2)		(1)	(2)
age16*non-Han	0.025		age31*non-Han	-0.031	-0.031
	(0.062)			(0.011)***	(0.013)**
age17*non-Han	-0.097		age32*non-Han	-0.023	-0.022
	(0.089)			(0.010)**	(0.011)**
age18*non-Han	-0.025		age33*non-Han	-0.029	-0.029
	(0.018)			(0.010)***	(0.010)***
age19*non-Han	-0.01		age34*non-Han	-0.03	-0.029
	(0.026)			(0.010)***	(0.010)***
age20*non-Han	-0.022		age35*non-Han	-0.029	-0.026
	(0.016)			(0.010)***	(0.010)**
age21*non-Han	-0.028		age36*non-Han	-0.027	-0.027
	(0.013)**			(0.010)***	(0.010)**
age22*non-Han	-0.03		age37*non-Han	-0.02	-0.017
	(0.011)***			(0.010)**	(0.010)*
age23*non-Han	-0.038	-0.104	age38*non-Han	-0.033	-0.031
	(0.011)***	(0.023)***		(0.010)***	(0.010)***
age24*non-Han	-0.033	-0.038	age39*non-Han	-0.021	-0.02
	(0.010)***	(0.033)		(0.010)**	(0.010)*
age25*non-Han	-0.032	-0.063	age40*non-Han	-0.025	-0.023
	(0.010)***	(0.021)***		(0.010)**	(0.011)**
age26*non-Han	-0.034	-0.037	age41*non-Han	-0.027	-0.028
	(0.010)***	(0.024)		(0.010)***	(0.010)***
age27*non-Han	-0.035	-0.025	age42*non-Han	-0.01	-0.007
	(0.010)***	(0.019)		(0.011)	(0.011)
age28*non-Han	-0.032	-0.049	age43*non-Han	-0.013	-0.014
	(0.010)***	(0.015)***		(0.010)	(0.010)
age29*non-Han	-0.03	-0.045	age44*non-Han	-0.011	-0.011
	(0.011)***	(0.015)***		(0.010)	(0.010)
age30*non-Han	-0.029	-0.022			
	(0.010)***	(0.013)	*Observations*	778,754	396,607

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions controls age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, county fixed effects. Col (1) is on observations in 1-Child Provinces and 1-Boy-2-Girl Provinces; Col (2) is on observations in 1-Child Provinces and 1-Boy-2-Girl Provinces; except for mothers with first birth later than 1981;

Table A.10: Coefficients of Interaction Terms for the Reduced Form Regressions Regarding Gender of 1st Birth

	(1)	(2)		(1)	(2)
age16*First-Born Girl	0.049		age31*First-Born Girl	-0.023	-0.017
	(0.090)			(0.008)***	(0.009)*
age17*First-Born Girl	-0.018		age32*First-Born Girl	-0.021	-0.016
	(0.072)			(0.008)**	(0.008)*
age18*First-Born Girl	-0.05		age33*First-Born Girl	-0.023	-0.021
	(0.031)			(0.008)***	(0.008)***
age19*First-Born Girl	-0.036		age34*First-Born Girl	-0.021	-0.02
	(0.018)**			(0.008)***	(0.008)**
age20*First-Born Girl	-0.03		age35*First-Born Girl	-0.02	-0.018
	(0.012)**			(0.008)**	(0.008)**
age21*First-Born Girl	-0.016		age36*First-Born Girl	-0.023	-0.023
	(0.009)*			(0.008)***	(0.008)***
age22*First-Born Girl	-0.015		age37*First-Born Girl	-0.018	-0.017
	(0.008)*			(0.008)**	(0.008)**
age23*First-Born Girl	-0.02	-0.098	age38*First-Born Girl	-0.019	-0.017
	(0.008)**	(0.008)***		(0.008)**	(0.008)**
age24*First-Born Girl	-0.022	0.061	age39*First-Born Girl	-0.018	-0.018
	(0.008)***	(0.071)		(0.008)**	(0.008)**
age25*First-Born Girl	-0.022	-0.045	age40*First-Born Girl	-0.015	-0.016
	(0.008)***	(0.034)		(0.009)*	(0.008)*
age26*First-Born Girl	-0.023	-0.034	age41*First-Born Girl	-0.013	-0.013
	(0.008)***	(0.019)*		(0.009)	(0.009)
age27*First-Born Girl	-0.024	-0.047	age42*First-Born Girl	-0.014	-0.015
	(0.008)***	(0.014)***		(0.009)	(0.009)*
age28*First-Born Girl	-0.023	-0.023	age43*First-Born Girl	-0.018	-0.018
	(0.008)***	(0.012)*		(0.009)*	(0.009)**

Table A.10 (cont'd)

age29*First-Born Girl	-0.015	-0.003	age44*First-Born Girl	-0.013	-0.014
	(0.008)*	(0.011)		(0.010)	(0.010)
age30*First-Born Girl	-0.026	-0.038			
	(0.008)***	(0.009)***	*Observations*	561,921	282,125

Notes: Standard errors clustered at county level are reported in brackets.* significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions controls age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, county fixed effects. Col (1) is on on Han people only in 1-Boy-2-Girl Provinces; Col (2) is on Han people only in 1-Boy-2-Girl Provinces, except for mothers with first birth later than 1981.

Table A.11: Heterogeneous Effect of Additional Children on Female LFP

	(1) ≤Primary	(2) ≥Junior	(3) ≤Primary	(4) ≥Junior
A: OLS				
kids2	0.005	-0.01	0.004	-0.012
	(0.001)***	(0.002)***	(0.002)**	(0.003)***
non-Han	0.005	-0.006		
	(0.008)	(0.009)		
First-Born Girl	0	-0.002	-0.001	-0.002
	(0.001)	(0.001)*	(0.001)	(0.001)*
Observations	572,222	206,532	426,719	161,725
B: 2SLS				
kids2	-0.185	0.06	-0.059	-0.035
	(0.053)***	(0.077)	(0.045)	(0.037)
non-Han	0.007	-0.008		
	(0.008)	(0.010)		
First-Born Girl	0.01	-0.007	0.002	-0.001
	(0.003)***	(0.006)	(0.002)	(0.002)
Observations	572,196	206,471	426,690	161,663

Notes: Standard errors clustered at county level are reported in brackets.* significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions are on observations from 2-children provinces. All regressions controls for age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, and county fixed effects. Col (1) is on mothers with at most primary school education in the restricted provinces; Col (2) is on mothers with at least junior high school education in the restricted provinces; Col (3) is on mothers with at most primary school education in the 1-boy-2-girl provinces; Col (4) is on mothers with at least junior high school education in the 1-boy-2-girl provinces; Col (1) and (2) in Panel B use DID based on ethnicity as instruments. Col (3) and (4) in Panel B use DID based on gender of first birth as instruments.

Table A.12: Coefficients of Interaction Terms for the Regressions of Education and Gender of First Birth

	(1) Education	(2) First-born Girl		(1) Education	(2) First-born Girl
age16*non-Han	-0.764	0.559	age31*non-Han	-0.087	0.013
	(0.777)	(0.187)***		(0.152)	(0.023)
age17*non-Han	-1.537	-0.039	age32*non-Han	0.155	0.009
	(0.977)	(0.182)		(0.140)	(0.022)
age18*non-Han	-0.274	-0.074	age33*non-Han	0.022	0.023
	(0.445)	(0.081)		(0.139)	(0.021)
age19*non-Han	-0.93	0.044	age34*non-Han	0.004	-0.002
	(0.319)***	(0.053)		(0.140)	(0.022)
age20*non-Han	-0.52	-0.019	age35*non-Han	0.052	0.009
	(0.192)***	(0.033)		(0.132)	(0.022)
age21*non-Han	-0.337	0	age36*non-Han	0.054	-0.006
	(0.163)**	(0.026)		(0.127)	(0.022)
age22*non-Han	-0.372	0.009	age37*non-Han	0.058	-0.005
	(0.150)**	(0.024)		(0.136)	(0.022)
age23*non-Han	-0.396	0.004	age38*non-Han	0.043	0.001
	(0.149)***	(0.023)		(0.132)	(0.021)
age24*non-Han	-0.434	0.014	age39*non-Han	0.071	0.011
	(0.147)***	(0.022)		(0.136)	(0.022)
age25*non-Han	-0.413	-0.005	age40*non-Han	0.105	0.008
	(0.140)***	(0.020)		(0.129)	(0.022)
age26*non-Han	-0.331	0.004	age41*non-Han	0.032	-0.003
	(0.147)**	(0.021)		(0.135)	(0.023)
age27*non-Han	-0.234	0.001	age42*non-Han	0.08	-0.012
	(0.145)	(0.020)		(0.138)	(0.024)
age28*non-Han	-0.21	0.006	age43*non-Han	0.217	-0.028
	(0.146)	(0.022)		(0.153)	(0.024)
age29*non-Han	-0.12	-0.019	age44*non-Han	0.233	-0.02
	(0.159)	(0.023)		(0.152)	(0.025)
age30*non-Han	-0.215	0.004			
	(0.153)	(0.022)	*Observations*	778,754	778,754

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. Both regressions are on observations from the restricted provinces. Col (1) controls for age cohort dummies, ethnicity dummies, and county fixed effects; Col (2) controls for age cohort dummies, ethnicity dummies, mother's age at first birth, education levels for both parents, and county fixed effects.

Table A.13: Coefficients of Interaction Terms for the 1st Stage Regressions Regarding Ethnicity (2-Children Provinces)

	(1) First Stage	(2) Reduced Form		(1) First Stage	(2) Reduced Form
age16*non-Han	0.07	-0.011	age31*non-Han	-0.013	-0.002
	(0.031)**	(0.011)		(0.034)	(0.013)
age17*non-Han	-0.211	0.005	age32*non-Han	-0.015	0.009
	(0.196)	(0.012)		(0.033)	(0.011)
age18*non-Han	0.056	-0.019	age33*non-Han	-0.02	0.005
	(0.120)	(0.029)		(0.032)	(0.011)
age19*non-Han	0.007	0.006	age34*non-Han	-0.02	-0.007
	(0.095)	(0.018)		(0.029)	(0.011)
age20*non-Han	0.083	0.014	age35*non-Han	-0.034	0.009
	(0.055)	(0.021)		(0.026)	(0.012)
age21*non-Han	0.223	0.017	age36*non-Han	-0.015	-0.006
	(0.051)***	(0.014)		(0.026)	(0.011)
age22*non-Han	0.154	0.008	age37*non-Han	-0.022	-0.003
	(0.040)***	(0.013)		(0.023)	(0.010)
age23*non-Han	0.112	0.003	age38*non-Han	-0.026	0.004
	(0.036)***	(0.013)		(0.024)	(0.012)
age24*non-Han	0.135	0.005	age39*non-Han	-0.017	-0.002
	(0.040)***	(0.013)		(0.024)	(0.013)
age25*non-Han	0.108	0	age40*non-Han	-0.008	0.019
	(0.039)***	(0.012)		(0.022)	(0.013)
age26*non-Han	0.085	0.001	age41*non-Han	-0.031	0.024
	(0.043)**	(0.012)		(0.020)	(0.015)
age27*non-Han	0.077	0.001	age42*non-Han	-0.029	0.005
	(0.040)*	(0.014)		(0.025)	(0.015)
age28*non-Han	0.039	0	age43*non-Han	-0.026	0.002
	(0.039)	(0.011)		(0.027)	(0.017)
age29*non-Han	0.025	0.007	age44*non-Han	-0.043	-0.011
	(0.043)	(0.013)		(0.028)	(0.019)
age30*non-Han	-0.001	-0.003			
	(0.035)	(0.011)	*Observations*	45,855	45,855

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. Both regressions are on observations from 2-Children Provinces. All regressions controls age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, and county fixed effects.

Table A.14: Coefficients of Interaction Terms for the 1st Stage Regressions Regarding Gender of the First-Birth (2-Children Provinces)

	(1) First Stage	(2) Reduced Form		(1) First Stage	(2) Reduced Form
First-Born Girl	0.067	-0.002	age31*First-Born Girl	-0.012	-0.007
	(0.027)**	(0.018)		(0.029)	(0.019)
age17*First-Born Girl	0.389	0.01	age32*First-Born Girl	-0.063	0.003
	(0.304)	(0.021)		(0.026)**	(0.022)
age18*First-Born Girl	-0.207	0.001	age33*First-Born Girl	-0.036	0
	(0.128)	(0.019)		(0.026)	(0.019)
age19*First-Born Girl	0.237	0.002	age34*First-Born Girl	-0.039	-0.001
	(0.137)*	(0.018)		(0.026)	(0.019)
age20*First-Born Girl	-0.072	-0.026	age35*First-Born Girl	-0.064	0.003
	(0.091)	(0.038)		(0.026)**	(0.016)
age21*First-Born Girl	-0.021	0.012	age36*First-Born Girl	-0.066	0.002
	(0.057)	(0.027)		(0.028)**	(0.018)
age22*First-Born Girl	-0.096	0.003	age37*First-Born Girl	-0.043	0.005
	(0.051)*	(0.019)		(0.028)	(0.020)
age23*First-Born Girl	-0.072	0.005	age38*First-Born Girl	-0.063	0.017
	(0.038)*	(0.020)		(0.031)**	(0.021)
age24*First-Born Girl	-0.064	0.015	age39*First-Born Girl	-0.045	0.005
	(0.038)*	(0.019)		(0.028)	(0.022)
age25*First-Born Girl	-0.041	-0.008	age40*First-Born Girl	-0.027	0.003
	(0.034)	(0.018)		(0.030)	(0.022)
age26*First-Born Girl	-0.05	-0.001	age41*First-Born Girl	-0.053	0.005
	(0.036)	(0.019)		(0.030)*	(0.031)
age27*First-Born Girl	-0.036	0.01	age42*First-Born Girl	-0.051	0.03
	(0.031)	(0.021)		(0.027)*	(0.022)
age28*First-Born Girl	-0.054	-0.002	age43*First-Born Girl	-0.05	-0.012

Table A.14 (cont'd)

	(0.033)	(0.019)		(0.030)*	(0.024)
age29*First-Born Girl	-0.038	0.019	age44*First-Born Girl	-0.061	0.007
	(0.031)	(0.020)		(0.036)*	(0.026)
age30*First-Born Girl	-0.053	-0.001			
	(0.034)	(0.019)	*Observations*	26,774	26,774

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. Both regressions are on observations from 2-Children Provinces. All regressions controls age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, and county fixed effects.

Table A.15: Robustness Check of the Effect of Additional Children on Female LFP

	(1) OLS	(2) 2SLS (Triple difference as IV)	(3) 2SLS (Twinning as IV)
kids2	-0.002	-0.07	-0.069
	(0.002)	(0.081)	(0.040)**
Observations	612,785	612,749	612,749

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, **significant at 5% level, ***significant at 1% level. All regressions controls for age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, and county fixed effects. All regressions are based on observations from the 1-boy-2-girl provinces. Col (1) is results from OLS; Col (2) is results from 2SLS using triple difference as instruments; Col (3) results from 2SLS using twinning as instruments.

APPENDIX B

FIGURES FOR CHAPTER 1

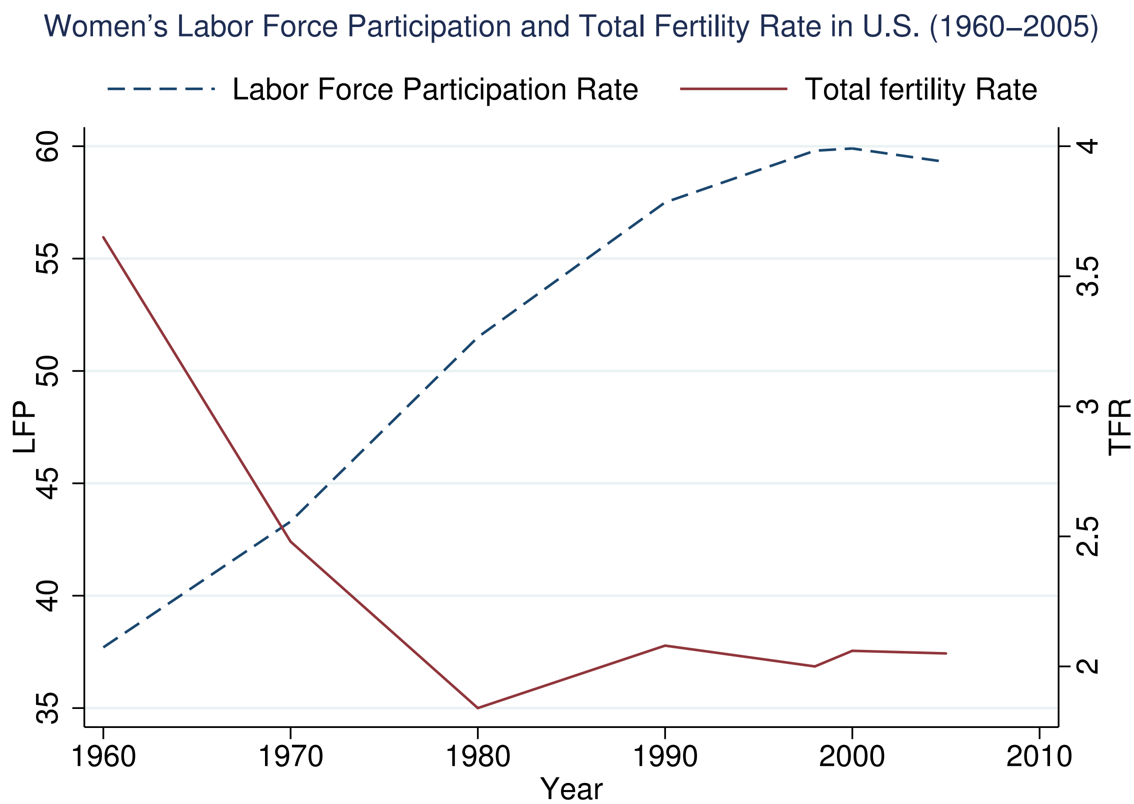


Figure B.1: Female Labor Force Participation and Total Fertility Rate in the U.S.
Data Sources: U.S. Bureau of Labor Statistics and World Bank

Figure B.2: Female Labor Force Participation and Total Fertility Rate in China

Data Sources: Maurer-Fazio et al. (2005) and the World Bank.

Measurement of female labor force participation is for the female population ages 15 and older.

The female LFP rates are for both rural and urban female together. According to Maurer-Fazio et al. (2005), the rural female LFP are 64.0, 59.6, and 56.3 in 1982, 1990, 2000 in China.

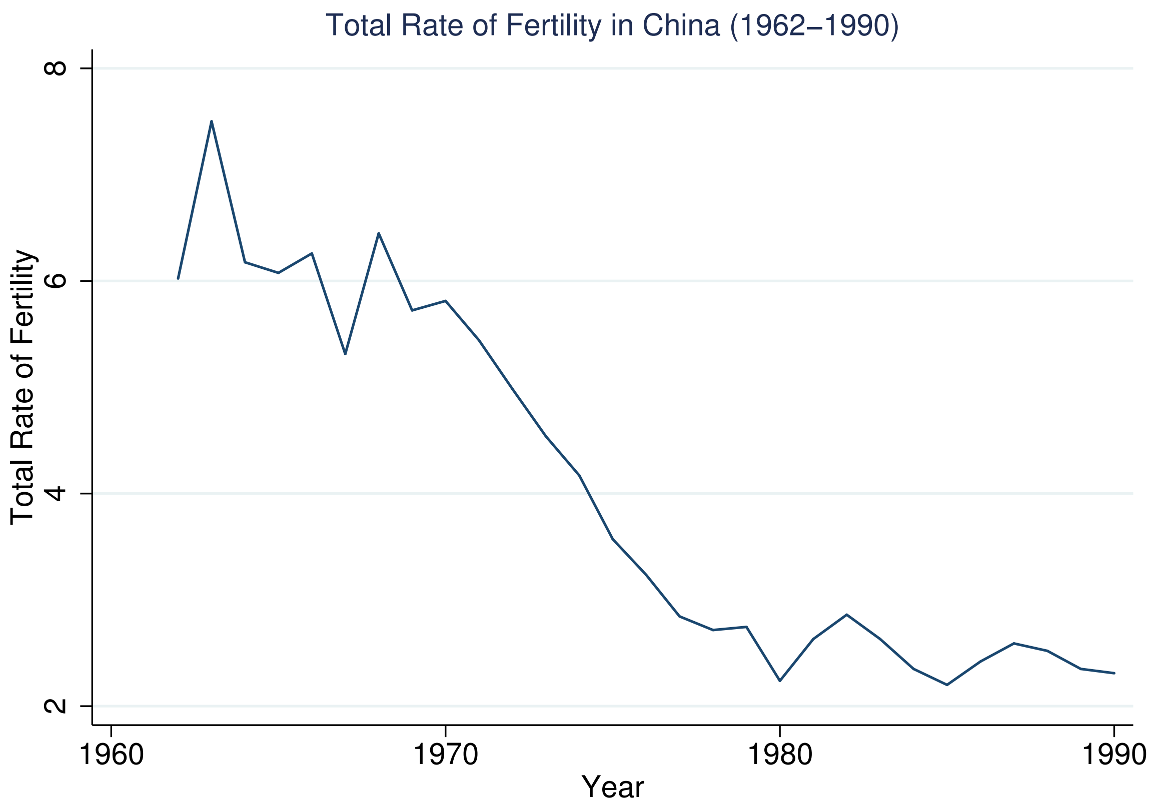
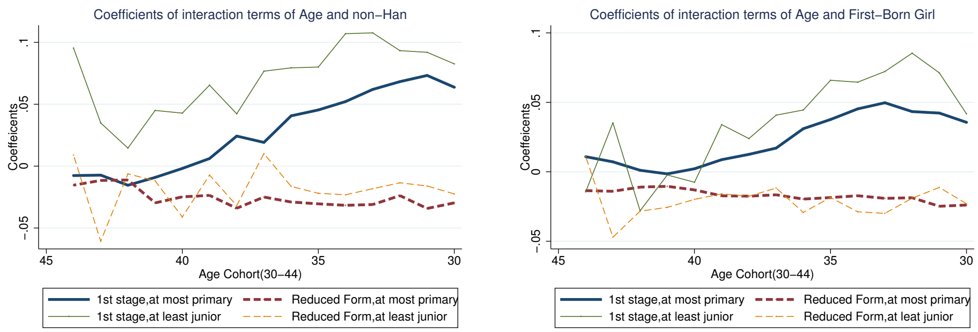


Figure B.3: Total Fertility Rate in China

Data Source: Yang (2004)

Total fertility rate (TFR) of a population in a period is the average number of children that would be born to a woman over her lifetime if she were to experience the exact current age-specific fertility rates through her lifetime, and she were to survive from birth through the end of her reproductive life (Yang, 2004).



a: Ethnicity as instruments   b: Gender of first birth as instruments

Figure B.4: Coefficients of Interactions of Age Cohorts and Ethnicity/First-Born Girl (Heterogenous Effects for Women with Different Education Levels)

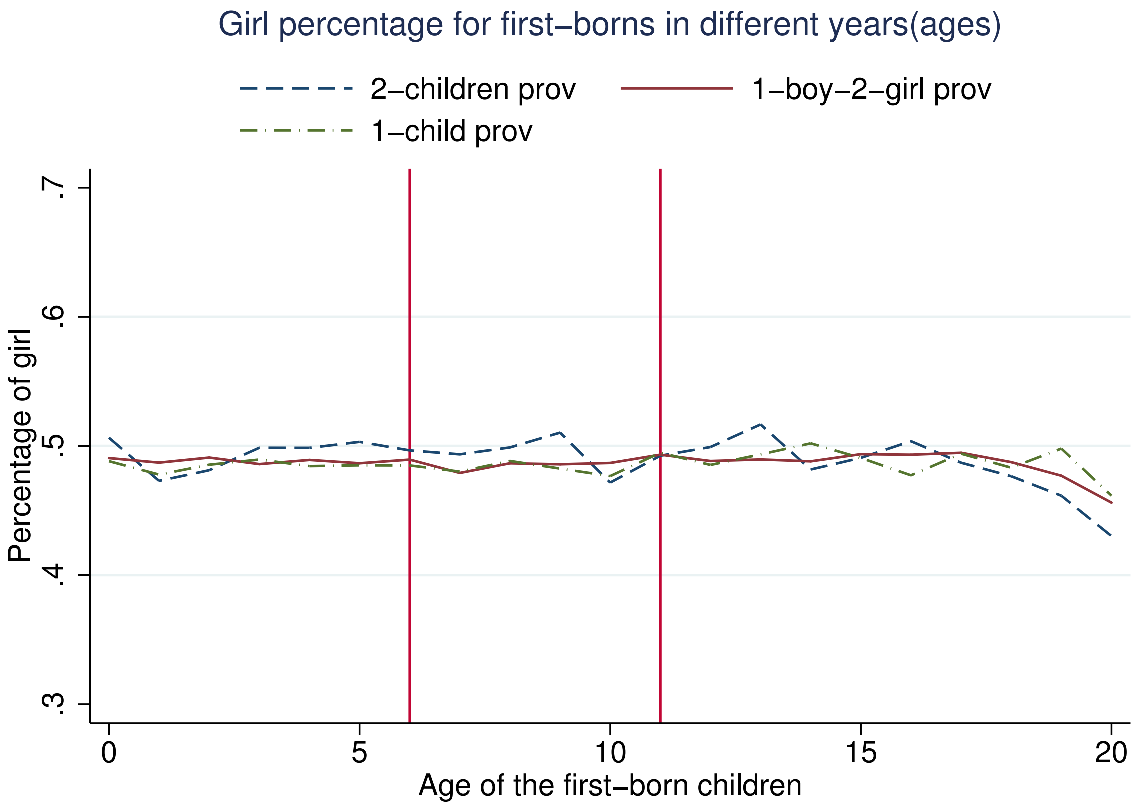
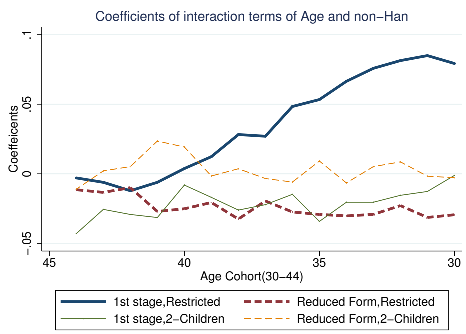
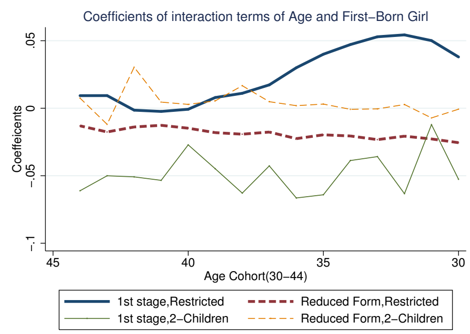


Figure B.5: Girl Percentage for First-Borns in Different Years(Ages)
Data Source: 1% sample from the 1990 China Population Census



a: $nonHan_{ict} \cdot d_t$



b: $First\text{-}Born\ Girl_{ict} \cdot d_t$

Figure B.6: Coefficients of Interactions of Age Cohorts and Ethnicity/First-Born Girl

Fig6a: blue line–coefficients of $nonHan_{ict} \cdot d_t$ from Column (1), Table 7
green line–coefficients of $nonHan_{ict} \cdot d_t$ from Column (1), Table 12
orange line–coefficients of $nonHan_{ict} \cdot d_t$ from Column (1), Table 9
red line–coefficients of $nonHan_{ict} \cdot d_t$ from Column (2), Table 12
Fig6b: blue line-coefficients of $First\text{-}Born\ Girl_{ict} \cdot d_t$ from Column (1), Table 8
green line–coefficients of $First\text{-}Born\ Girl_{ict} \cdot d_t$ from Column (1), Table 13
orange line–coefficients of $First\text{-}Born\ Girl_{ict} \cdot d_t$ from Column (1), Table 10
red line–coefficients of $First\text{-}Born\ Girl_{ict} \cdot d_t$ from Column (2), Table 13

APPENDIX C

TABLES FOR CHAPTER 2

Table C.1: People's Characteristics by Their Displacement Status

	Men			Women		
	Displaced[1]	Never Displaced[2]	Adjusted Differencesc[3]	Displaced[1]	Never Displaced[2]	Adjusted Differences[3]
Age	20.05	29.80	9.75***	20.46	29.86	9.40***
Hispanic	0.21	0.17	-0.04***	0.21	0.17	-0.04***
Black	0.33	0.27	-0.06***	0.33	0.29	-0.04**
More than high school educ	0.26	0.46	0.20***	0.32	0.44	0.11***
Single	0.84	0.39	-0.03***	0.68	0.31	-0.01
Married	0.13	0.51	0.03***	0.25	0.51	0.02
Divorce	0.03	0.11	-0.01	0.06	0.17	-0.01
Family income last year	18449.52	52541.77	4515.26***	18261.94	43810.14	2991.00***
Age at 1st marriage	25.77	25.16	-0.61***	23.19	23.43	0.23
Fertility in a Given Year						
Had an additional child	0.07	0.06	-0.01**	0.10	0.06	0.00
Number of children	0.25	1.18	-0.05**	0.55	1.44	-0.07**
Observations	1590	2081		1095	2893	

Note: Data is from NLSY79. For selection restriction, refer to Section 3.
[1] For time-variant variables, means are calculated by using values three or more years prior to the first displacement.
[2] For time-variant variables, means are calculated by using values of all person-years.
[3] To get the adjusted differences, age and year fixed effects are controlled.

Table C.2: Impact of Displacement on the Probability of Having an Additional Child (Fixed Effect)

	All Sample	Men	Women
Displacement year - 2	-0.001	-0.013	0.015
	(0.008)	(0.011)	(0.013)
Displacement year - 1	0	0.007	-0.011
	(0.008)	(0.010)	(0.011)
Displacement year	0.003	0.011	-0.01
	(0.007)	(0.010)	(0.010)
Displacement year + 1	0.007	0.006	0.004
	(0.007)	(0.010)	(0.011)
Displacement year + 2-3	0.002	-0.003	0.003
	(0.006)	(0.009)	(0.009)
Displacement year + 4-5	0.004	-0.005	0.009
	(0.006)	-0.009	(0.009)
Displacement year + 6-7	0	-0.015	0.011
	(0.006)	(0.008)*	(0.009)
Displacement year + 8+	-0.001	-0.016	0.007
	(0.005)	(0.008)**	(0.008)
Total Effect on the Treated	-0.005	-0.110	0.033
	(0.082)	(0.126)	(0.104)
Obersations	156,439	75,205	81,234

Notes: Standard errors clustered at individual level are reported in brackets. *significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, year and individual fixed effects. Standard errors for total effect on the treated are obtained through bootstrap.

Table C.3: Heterogeneous Impact of Displacement on the Probability of Having an Additional Child (Fixed Effect)

	Men Without College Education	Men with College Education	Women without College Education	Women with College Education
Displacement year - 2	-0.019	-0.003	0.007	0.044
	(0.013)	(0.017)	(0.016)	(0.023)*
Displacement year - 1	0.011	-0.008	-0.015	0.012
	(0.013)	(0.016)	(0.014)	(0.018)
Displacement year	0.005	0.022	-0.008	-0.002
	(0.012)	(0.017)	(0.013)	(0.018)
Displacement year + 1	0.004	0.008	0.012	0.005
	(0.012)	(0.017)	(0.013)	(0.018)
Displacement year + 2-3	-0.009	0.017	0.015	-0.002
	(0.011)	(0.014)	(0.011)	(0.014)
Displacement year + 4-5	-0.005	0.01	0.022	0.008
	(0.011)	(0.015)	(0.011)**	(0.015)
Displacement year + 6-7	-0.005	-0.014	0.023	0.017
	(0.011)	(0.013)	(0.010)**	(0.015)
Displacement year + 8+	-0.005	-0.003	0.022	0.011
	(0.010)	(0.012)	(0.009)**	(0.012)
Total Effect on the Treated	-0.015	-0.005	0.083	0.004
	(0.106)	(0.141)	(0.109)	(0.149)
Obersations	47,284	27,921	48,438	32,796

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, year and individual fixed effects.
Standard errors for total effect on the treated are obtained through bootstrap.

Table C.4: Interaction Terms of College Education and Displacement Status

College*Displacement year + 1	-0.019
	(0.020)
College*Displacement year + 2-3	-0.029
	(0.015)**
College*Displacement year + 4-5	-0.026
	(0.016)*
College*Displacement year + 6-7	-0.017
	(0.016)
College*Displacement year + 8+	-0.023
	(0.012)*
F-statistic	4.58
N	81,234

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
Regression controls age, age square, displacement indicators, year and individual fixed effects.

Table C.5: Impact of Displacement on the Probability of Having an Additional Child (Time Trend Model)

	Men	Women	Women without College Education	Women with College Education
Displacement year - 2	-0.017	0.01	0.007	0.018
	(0.012)	(0.014)	(0.017)	(0.025)
Displacement year - 1	-0.005	-0.01	-0.009	-0.011
	(0.013)	(0.013)	(0.015)	(0.023)
Displacement year	-0.006	-0.002	0.008	-0.018
	(0.013)	(0.013)	(0.016)	(0.023)
Displacement year + 1	-0.01	0.014	0.03	-0.016
	(0.014)	(0.014)	(0.017)*	(0.024)
Displacement year + 2-3	-0.021	0.017	0.035	-0.021
	(0.013)	(0.013)	(0.017)**	(0.022)
Displacement year + 4-5	-0.024	0.026	0.045	-0.011
	(0.014)*	(0.015)*	(0.018)**	(0.025)
Displacement year + 6-7	-0.035	0.032	0.048	-0.002
	(0.015)**	(0.015)**	(0.019)**	(0.026)
Displacement year + 8+	-0.038	0.03	0.048	-0.011
	(0.016)**	(0.017)*	(0.021)**	(0.027)
Obersations	71,534	77,246	46,063	31,183

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, year and individual fixed effects, interaction terms of individual fixed effect and year, and interaction terms of individual fixed effect and year square.

Table C.6: Impact of Displacement on the Probability of Having an Additional Child (Correlated Random Effect Probit Model)

	Men	Women	Women without College Education	Women with College Education
Displacement year - 2	-0.01	0.006	0.001	0.036
	(0.007)	(0.009)	(0.010)	(0.022)
Displacement year - 1	0.004	-0.012	-0.013	0.007
	(0.008)	(0.007)*	(0.008)	(0.018)
Displacement year	0.006	-0.013	-0.009	-0.005
	(0.007)	(0.007)*	(0.008)	(0.015)
Displacement year + 1	0.003	-0.004	0.004	-0.001
	(0.008)	(0.008)	(0.010)	(0.016)
Displacement year + 2-3	-0.004	-0.006	0.006	-0.009
	(0.006)	(0.007)	(0.008)	(0.012)
Displacement year + 4-5	-0.006	-0.003	0.01	-0.002
	-0.007	(0.007)	(0.009)	(0.014)
Displacement year + 6-7	-0.014	-0.002	0.012	0.004
	(0.006)**	(0.008)	(0.010)	(0.015)
Displacement year + 8+	-0.016	-0.01	0.008	-0.007
	(0.006)**	(0.007)	(0.009)	(0.012)
Total Effect on the Treated	-0.010	-0.041	0.029	-0.018
	(0.010)	(0.056)	(0.053)	(0.041)
Obersations	75,205	81,234	48,253	32,796

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, mean of age, mean of age square, mean of displacement indicators, and year fixed effects.

Table C.7: Impact of Displacement_Excluding People Suffering First Displacement After 1994

	Men	Women	Women without College Education	Women with College Education
Displacement year - 2	-0.015	0.015	0.007	0.044
	(0.011)	(0.013)	(0.016)	(0.023)*
Displacement year - 1	0.006	-0.011	-0.015	0.013
	(0.010)	(0.011)	(0.014)	(0.018)
Displacement year	0.009	-0.01	-0.007	-0.001
	(0.010)	(0.011)	(0.013)	(0.018)
Displacement year + 1	0.003	0.004	0.014	0.006
	(0.010)	(0.011)	(0.013)	(0.018)
Displacement year + 2-3	-0.007	0.003	0.017	0.001
	(0.009)	(0.009)	(0.011)	(0.014)
Displacement year + 4-5	-0.01	0.015	0.033	0.01
	-0.009	(0.009)	(0.011)***	(0.015)
Displacement year + 6-7	-0.02	0.017	0.032	0.021
	(0.009)**	(0.009)*	(0.011)***	(0.015)
Displacement year + 8+	-0.021	0.009	0.027	0.011
	(0.008)***	(0.008)	(0.010)***	(0.012)
Total Effect on the Treated	-0.162	0.059	0.102	0.024
	(0.116)	(0.109)	(0.141)	(0.143)
Obersations	64,274	68,219	39,862	28,357

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, year and individual fixed effects.
Standard errors for total effect on the treated are obtained through bootstrap.

Table C.8: Impact of Displacement for Obseravations during 1984-1992

	Men	Women	Women without College Education	Women with College Education
Displacement year - 2	-0.019	0.009	0.006	0.033
	(0.011)*	(0.013)	(0.016)	(0.024)
Displacement year - 1	-0.003	-0.022	-0.021	0.003
	(0.012)	(0.012)*	(0.015)	(0.021)
Displacement year	-0.002	-0.029	-0.015	-0.03
	(0.012)	(0.013)**	(0.016)	(0.020)
Displacement year + 1	-0.008	-0.007	0.015	-0.017
	(0.013)	(0.014)	(0.018)	(0.023)
Displacement year + 2-3	-0.02	-0.012	0.013	-0.022
	(0.013)	(0.013)	(0.017)	(0.020)
Displacement year + 4-5	-0.026	-0.007	0.027	-0.022
	(0.014)*	(0.016)	(0.020)	(0.026)
Displacement year + 6-7	-0.04	-0.002	0.029	0.009
	(0.016)**	(0.019)	(0.023)	(0.033)
Displacement year + 8+	-0.05	-0.026	0.017	-0.033
	(0.023)**	(0.029)	(0.034)	(0.053)
Total Effect on the Treated	-0.071	-0.013	0.017	-0.008
	(0.057)	(0.048)	(0.595)	(0.087)
Obersations	28,251	30,141	17,586	12,555

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, year and individual fixed effects.
Standard errors for total effect on the treated are obtained through bootstrap.

Table C.9: Comparisons between Non-displaced People and People Lost a Job Due to Firm Closure

	Men			Women		
	Displaced[1]	Never Displaced[2]	Adjusted Differencesc[3]	Displaced[1]	Never Displaced[2]	Adjusted Differences[3]
Age	20.74	29.80	9.06***	20.61	29.86	9.25***
Hispanic	0.18	0.17	-0.02	0.19	0.17	-0.01
Black	0.34	0.27	-0.07***	0.28	0.29	0.01
More than high school educ	0.30	0.46	0.16***	0.33	0.44	0.11***
Single	0.81	0.39	-0.04**	0.66	0.31	0.01
Married	0.15	0.51	0.05***	0.26	0.51	0.01
Divorce	0.04	0.11	-0.01*	0.07	0.17	-0.02
Family income last year	19851.42	52541.77	4699.83***	18534.52	43810.14	2719.02***
Age at 1st marriage	25.67	25.16	-0.49	22.78	23.43	0.67**
Fertility in a Given Year						
Had an additional child	0.08	0.06	-0.01	0.09	0.06	0.01
Number of children	0.30	1.18	-0.03	0.52	1.44	-0.03
Observations	445	2081		377	2893	

Note: Data is from NLSY79. For selection restriction, refer to Section 4.
[1] For time-variant variables, means are calculated by using values three or more years prior to the first displacement.
[2] For time-variant variables, means are calculated by using values of all person-years.
[3] To get the adjusted differences, age and year fixed effects are controlled.

Table C.10: Impact of Job Loss Due to Firm Closure on the Probability of Having an Additional Child (Fixed Effect)

	Men	Women	Women without College Education	Women with College Education
Displacement year - 2	-0.001	0.011	0.013	0.014
	(0.017)	(0.020)	(0.025)	(0.034)
Displacement year - 1	0.014	-0.014	-0.002	-0.039
	(0.018)	(0.016)	(0.021)	(0.022)*
Displacement year	0.028	-0.018	-0.008	-0.031
	(0.018)	(0.015)	(0.019)	(0.024)
Displacement year + 1	0.015	-0.019	-0.018	-0.008
	(0.018)	(0.014)	(0.017)	(0.027)
Displacement year + 2-3	0.01	-0.004	0.008	-0.017
	(0.013)	(0.011)	(0.014)	(0.021)
Displacement year + 4-5	0	0.018	0.034	0
	(0.012)	(0.012)	(0.016)**	(0.020)
Displacement year + 6-7	-0.014	0.007	0.016	0.001
	(0.011)	(0.011)	(0.012)	(0.021)
Displacement year + 8+	-0.012	0.01	0.016	0.013
	(0.008)	(0.008)	(0.009)*	(0.014)
Total Effect on the Treated	-0.020	0.038	0.054	0.016
	(0.095)	(0.085)	(0.101)	(0.098)
Obersations	53,976	67,578	39,350	28,228

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, year and individual fixed effects.

Table C.11: Heterogeneous Impact of Displacement on the Probability of Having an Additional Child (Fixed Effect Propensity Score Matching)

	Men Without College Education	Men with College Education	Women without College Education	Women with College Education
Displacement year - 2	0.006	-0.013	-0.037	0.012
	(0.012)	(0.011)	(0.014)***	(0.019)
Displacement year - 1	-0.007	-0.021	-0.029	0.010
	(0.01)	(0.01)**	(0.013)**	(0.022)
Displacement year	0.036	-0.012	-0.011	-0.026
	(0.011)***	(0.009)	(0.013)	(0.016)
Displacement year + 1	-0.005	-0.005	-0.015	-0.014
	(0.01)	(0.01)	(0.012)	(0.019)
Displacement year + 2-3	-0.002	-0.007	-0.007	-0.009
	(0.007)	(0.007)	(0.008)	(0.013)
Displacement year + 4-5	-0.005	0.005	0.015	-0.010
	(0.008)	(0.006)	(0.008)*	(0.012)
Displacement year + 6-7	-0.017	0.009	0.018	0.009
	(0.007)**	(0.007)	(0.008)**	(0.011)
Displacement year + 8+	-0.001	0.003	0.010	-0.003
	(0.003)	(0.002)	(0.003)***	(0.005)
Total Effect on the Treated	-0.035	-0.003	0.007	-0.040
	(0.030)	(0.007)	(0.006)	(0.045)

Note: Data is from NLSY79.
Standard errors are obtained through bootstrap and reported in the parenthesis.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.

APPENDIX D

FIGURES FOR CHAPTER 2

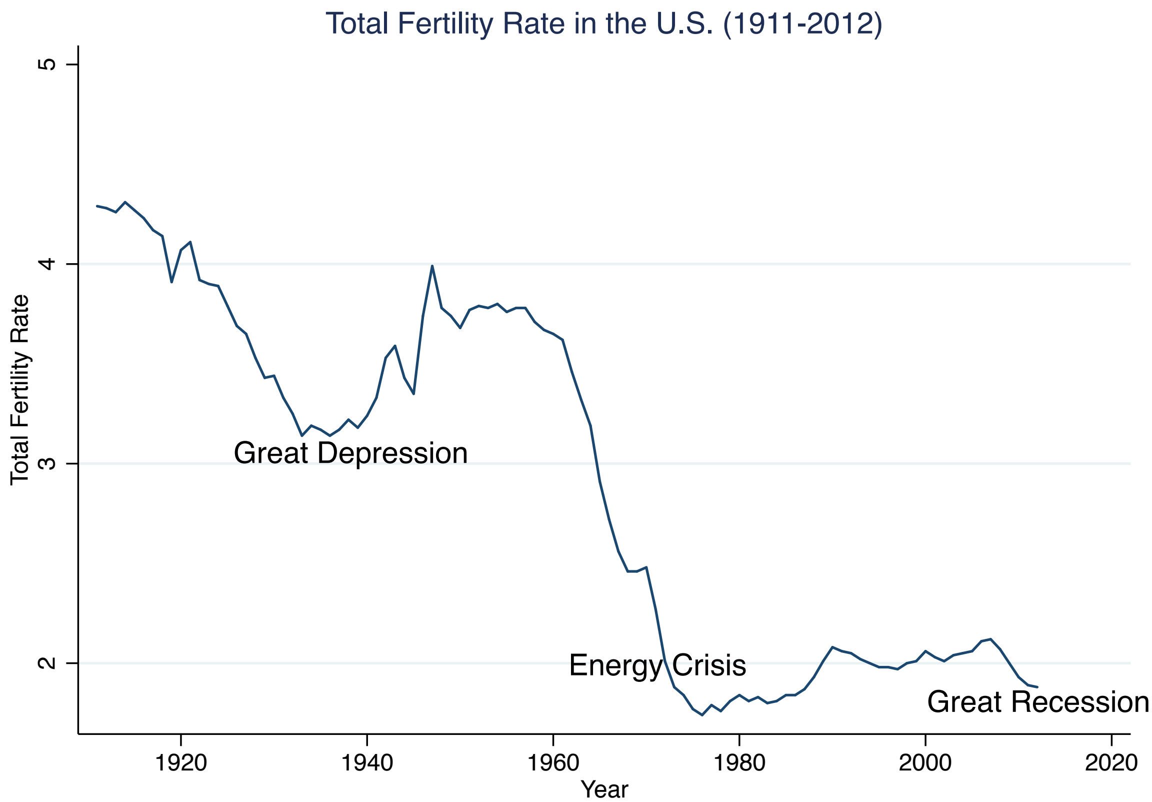


Figure D.1: The U.S. Fertility Rate Has Fallen During Recessions
Data Sources: OECD Demography Data.

Figure D.2: Unemployment and Birth Rate in the U.S.
Data Sources: National Vital Statistics System and Bureau of Labor Statistics.

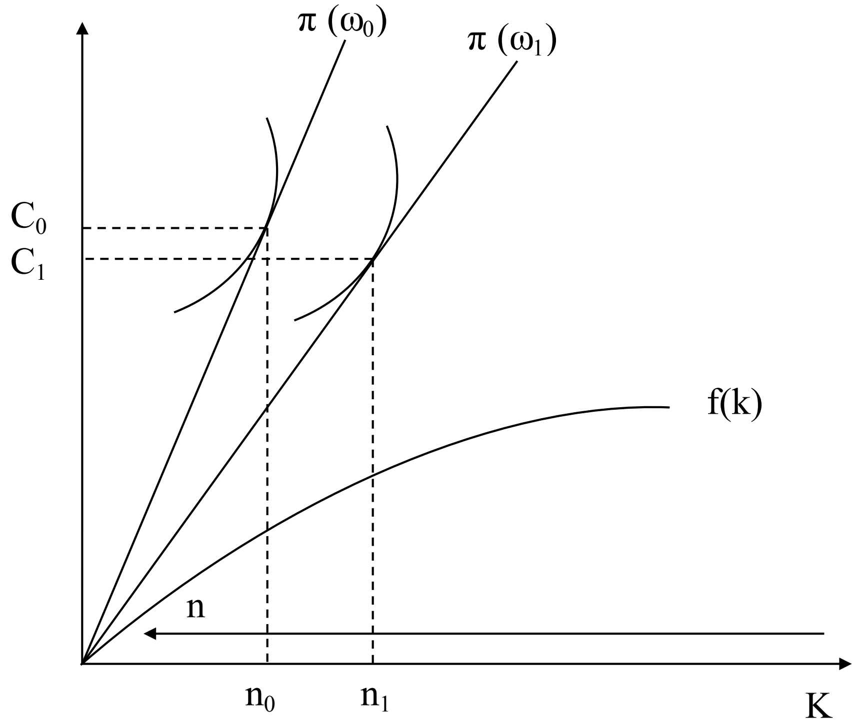


Figure D.3: Effects of Wage Decrease for Women with Very High Wage

Notes: The origin for two difference curves is at the northeastern corner, with n-axis denoting the horizontal axis.

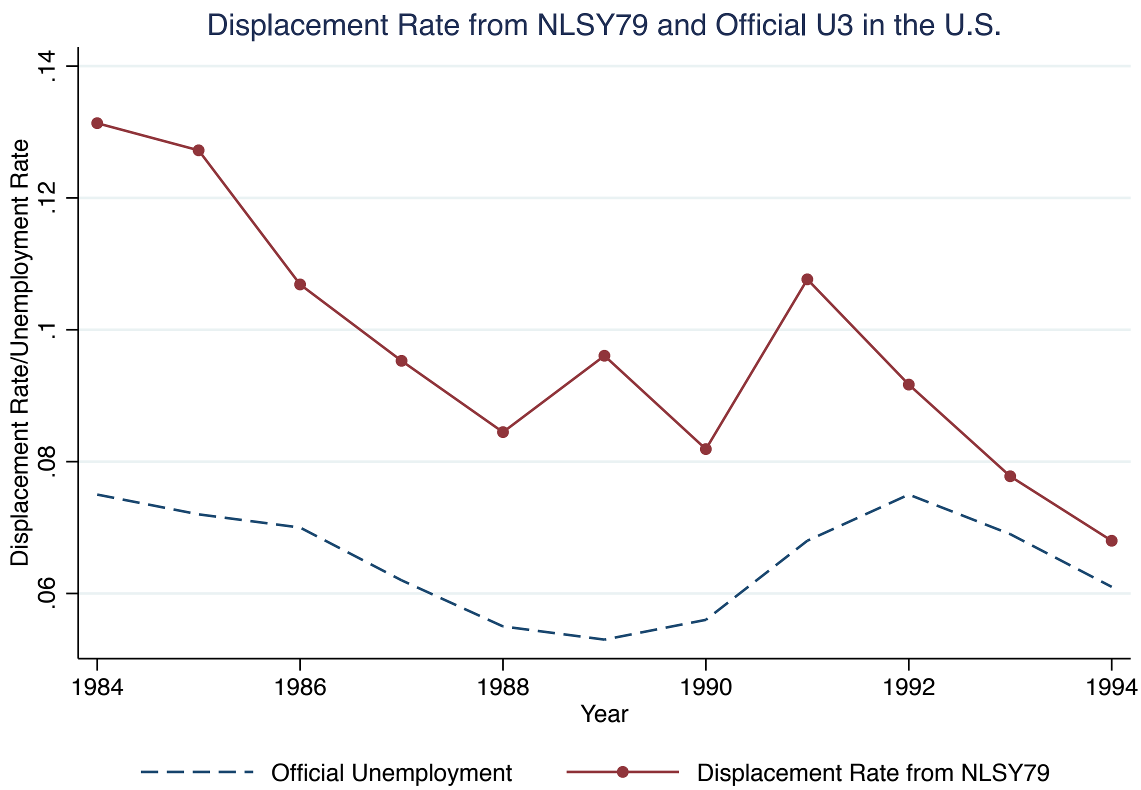


Figure D.4: Displacement Rate from NLSY79 Vs. Official Unemployment in the U.S. (1984-1994)
Data Sources: NLSY 79 and Bureau of Labor Statistics

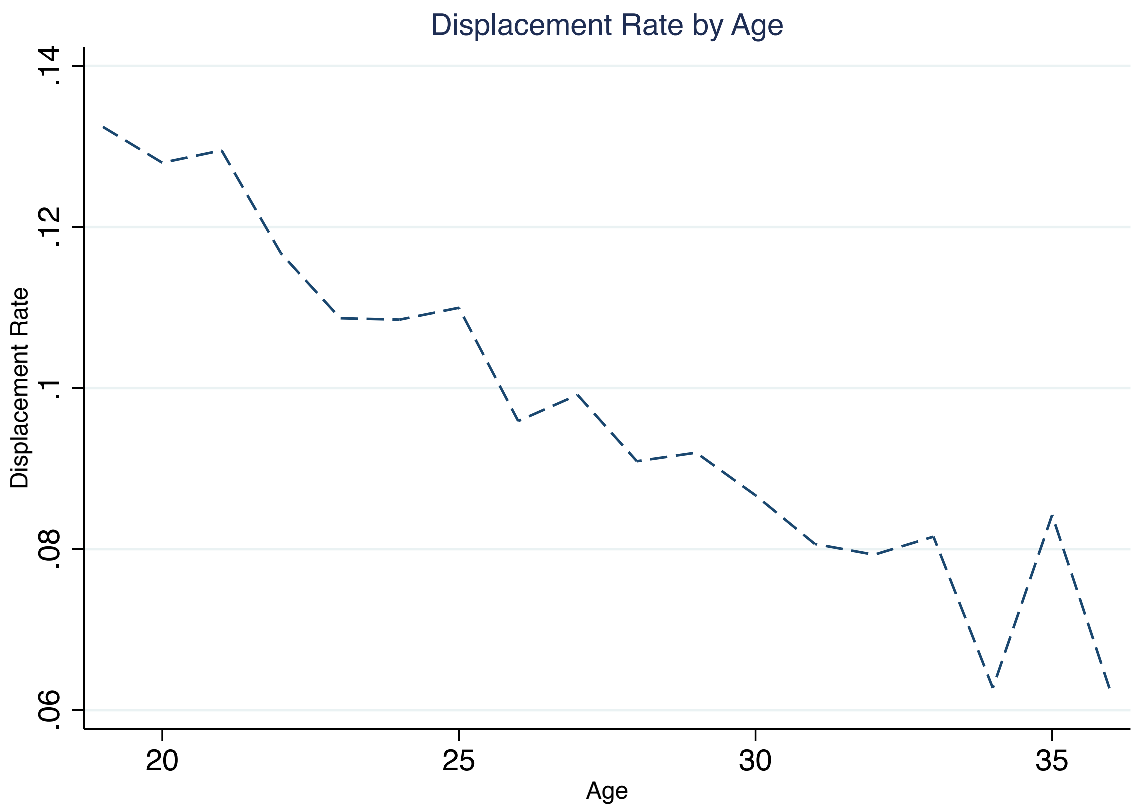


Figure D.5: Displacement Rate for Different Age Cohorts. (1984-1994)
Data Sources: NLSY 79

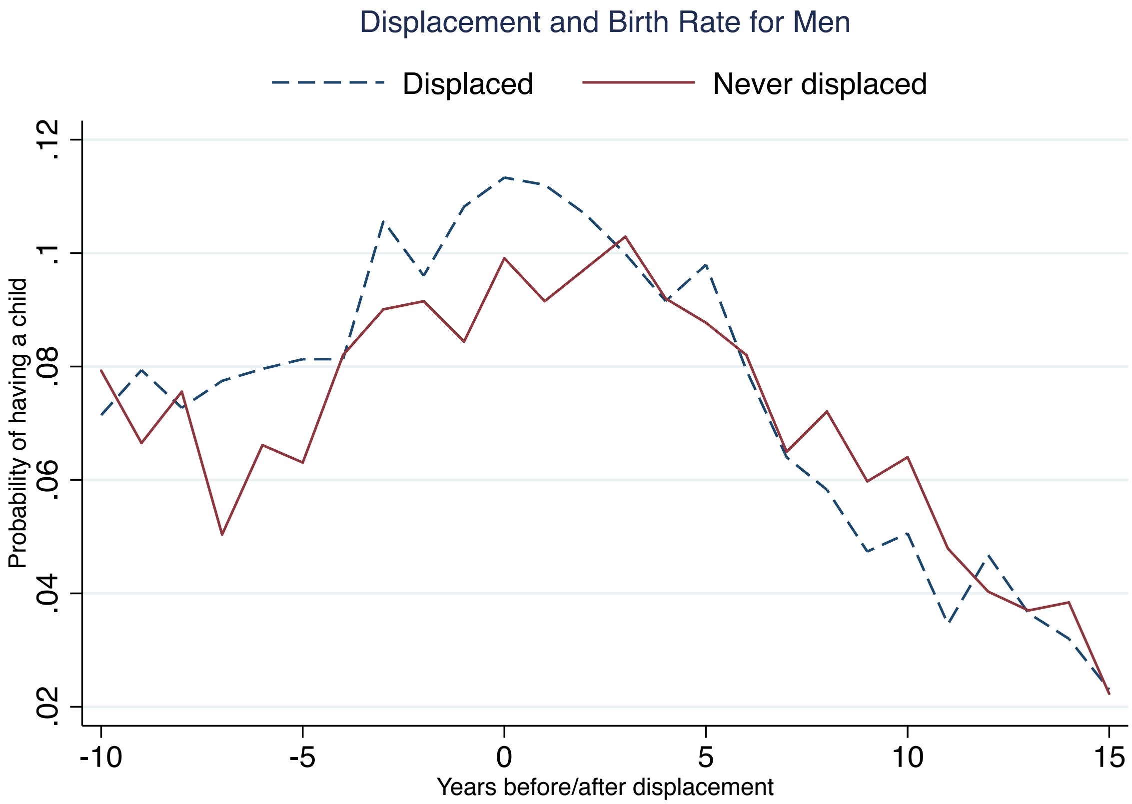


Figure D.6: Displacement and Birth Rate for Men

Notes: For displaced men , the x-axis denotes time before and after job displacement. For non-displaced men, the x-axis denotes time before and after a fake job displacement. The year of the fake displacement is generated by randomization with the probability for each year based on the distribution of occurrence rates of displacement for the displaced group.

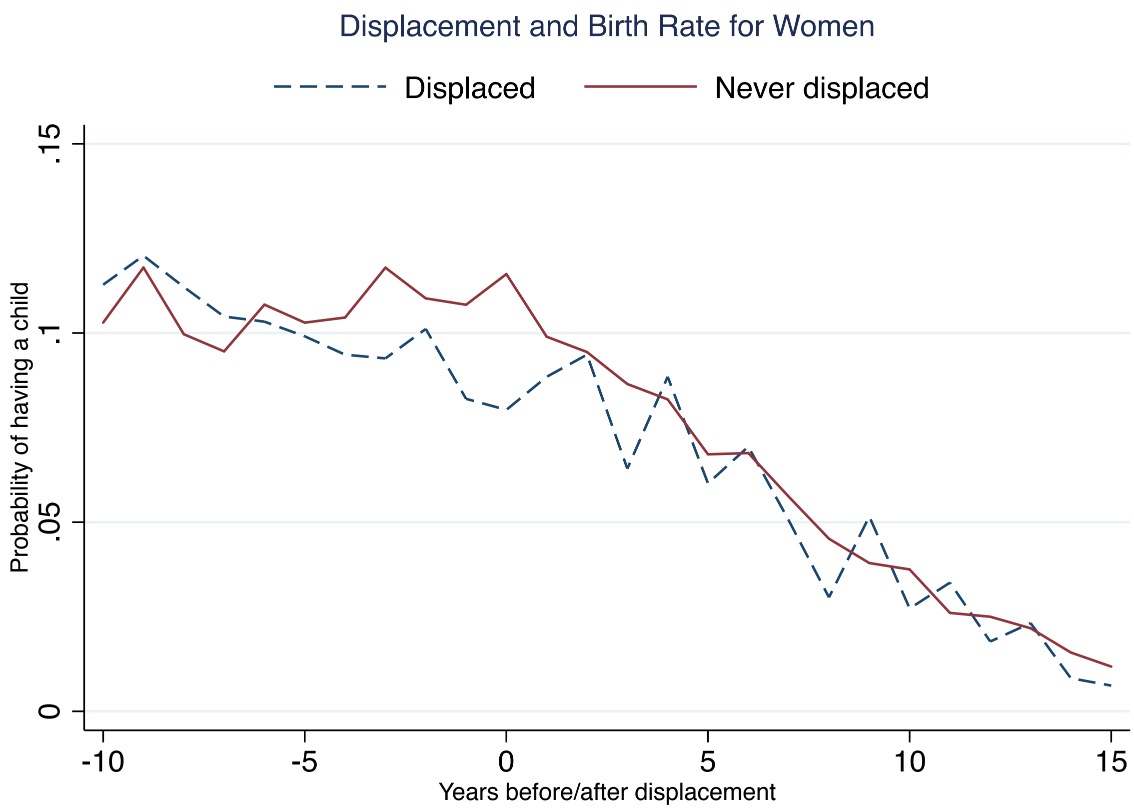


Figure D.7: Displacement and Birth Rate for Women

Notes: For displaced women , the x-axis denotes time before and after job displacement. For non-displaced women, the x-axis denotes time before and after a fake job displacement. The year of the fake displacement is generated by randomization with the probability for each year based on the distribution of occurrence rates of displacement for the displaced group.

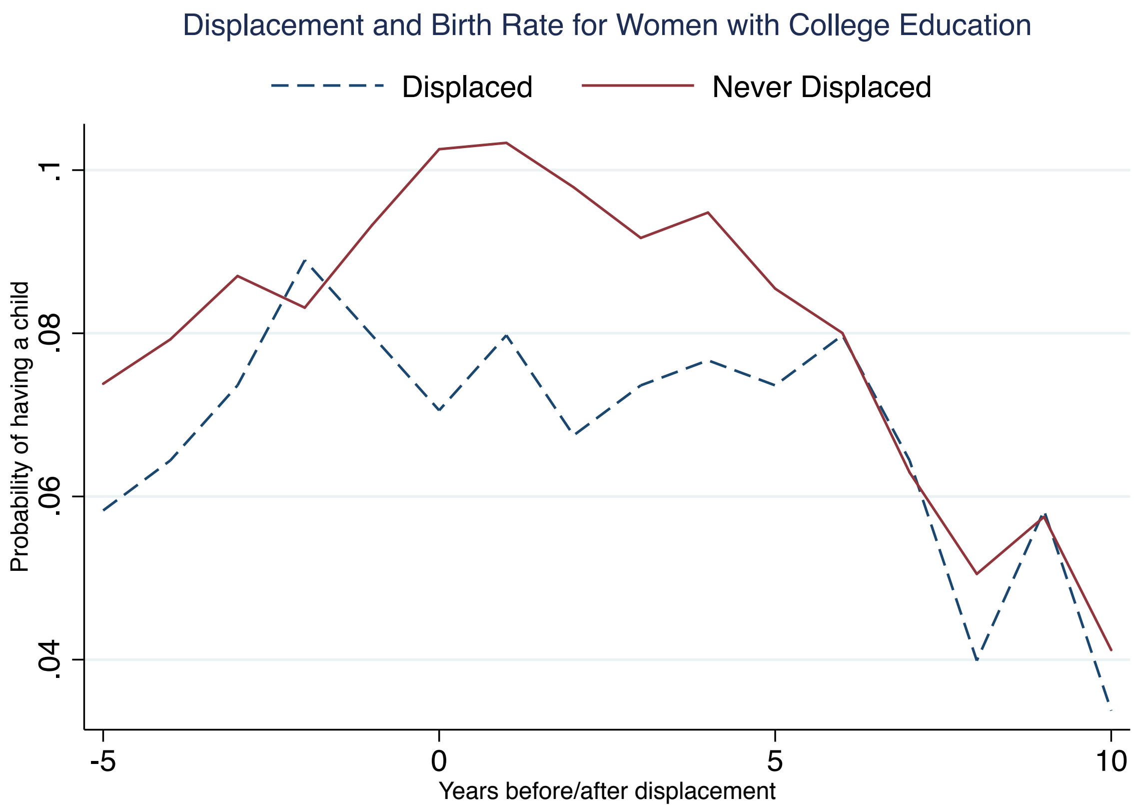


Figure D.8: Displacement and Birth Rate for Women with College Education

Notes: For displaced women , the x-axis denotes time before and after job displacement. For non-displaced women, the x-axis denotes time before and after a fake job displacement. The year of the fake displacement is generated by randomization with the probability for each year based on the distribution of occurrence rates of displacement for the displaced group.

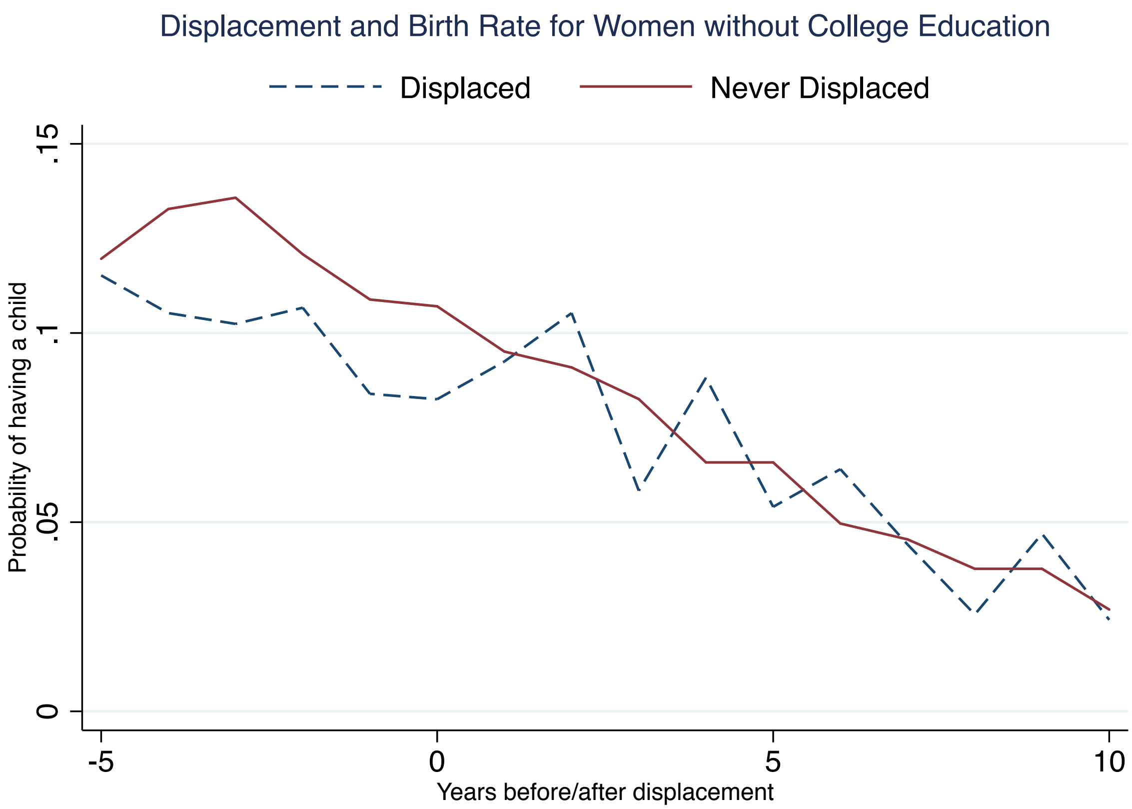


Figure D.9: Displacement and Birth Rate for Women without College Education

Notes: For displaced women , the x-axis denotes time before and after job displacement. For non-displaced women, the x-axis denotes time before and after a fake job displacement. The year of the fake displacement is generated by randomization with the probability for each year based on the distribution of occurrence rates of displacement for the displaced group.

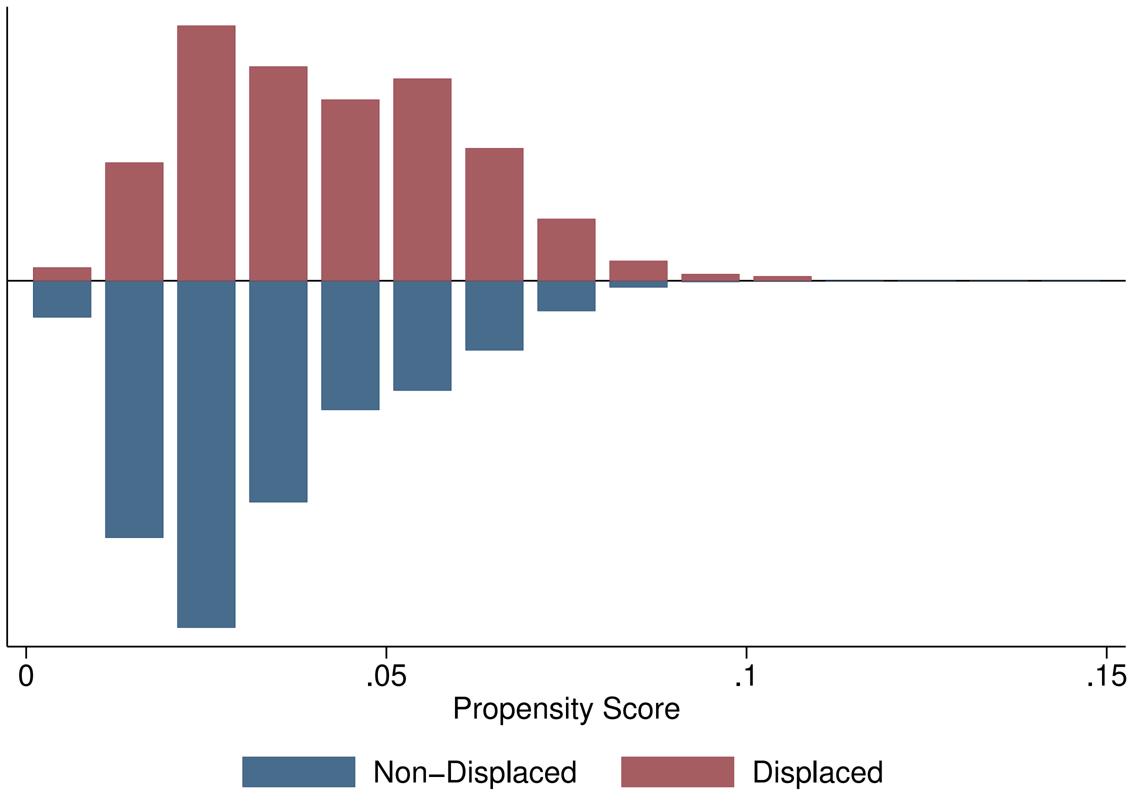


Figure D.10: Propensity Histogram by Displacement Status in 1984_Men
Data Sources: NLSY 79

APPENDIX E

TABLES FOR CHAPTER 3

Table E.1: Summary Statistics for Key Variables

	(1)	(2)	(3)
Men	0.468	1.000	0.000
	(0.005)	(0)	(0)
CES-D	8.872	7.764	9.845
	(0.065)	(0.088)	(0.093)
CES-D$\geq$10	0.398	0.324	0.463
	(0.005)	(0.007)	(0.007)
# of Children	2.956	2.867	3.034
	(0.015)	(0.022)	(0.022)
# of Children$>$1	0.886	0.876	0.894
	(0.003)	(0.005)	(0.004)
First-Born Girl	0.474	0.477	0.472
	(0.005)	(0.007)	(0.007)
Age	58.591	59.061	58.178
	(0.094)	(0.135)	(0.13)
Age at 1st Birth	24.024	25.162	23.024
	(0.041)	(0.062)	(0.051)
Year of Schooling	4.677	6.125	3.404
	(0.04)	(0.054)	(0.053)
At Least Primary School	0.504	0.666	0.361
	(0.005)	(0.007)	(0.007)
# of Siblings	3.921	3.838	3.994
	(0.02)	(0.029)	(0.027)
Married	0.849	0.894	0.810
	(0.004)	(0.005)	(0.005)
Good Health during Childhood	0.745	0.751	0.740
	(0.004)	(0.006)	(0.006)
Observations	9657	4517	5140

Notes: Data is from 2011 CHARLS.
Col (1) is on the whole sample; Col (2) is for men only; Col (3) is for women only.

Table E.2: DID Estimates Regarding Gender of First Birth

Probability of Having 2 or More Children			
	Old Cohorts	Young Cohorts	Difference
First-Born Boy	0.947	0.802	-0.145
(s.d./s.e.)	0.2233	0.3986	0.0094
First-Born Girl	0.957	0.872	-0.084
(s.d./s.e.)	0.2037	0.2970	0.0075
Difference	0.009	0.070	0.061
(s.e.)	0.0067	0.0082	0.0121
Number of Children			
First-Born Boy	3.981	2.230	-1.751
(s.d./s.e.)	1.6833	1.0064	0.0351
First-Born Girl	4.134	2.587	-1.546
(s.d./s.e.)	1.6777	1.1402	0.0380
Difference	0.152	0.357	0.205
(s.e.)	0.0527	0.0250	0.0516
CES-D-10 Score			
First-Born Boy	9.975	8.315	-1.660
(s.d./s.e.)	6.5607	6.2628	0.1858
First-Born Girl	9.509	8.571	-0.938
(s.d./s.e.)	6.4873	6.3816	0.1924
Difference	-0.466	0.256	0.722
(s.e.)	0.2197	0.1568	0.2674

Notes: Old Cohorts include individuals 62 years old and above; Young Cohorts include individuals below 62.

Table E.3: First Stage Results for Two or More Children

	(1)	(2)	(3)
Age<62*First-Born Girl	0.102	0.101	0.106
	(0.017)***	(0.019)***	(0.020)***
First-Born Girl	0.008	0.008	0.007
	(0.009)	(0.011)	(0.012)
Age<62	-0.231	-0.17	0.578
	(0.033)***	(0.037)***	(0.057)***
Men	0.009		
	(0.006)		
Year of Schooling	-0.001	-0.003	0.001
	(0.001)	(0.001)**	(0.002)
# of Siblings	0.002	0.002	0.002
	(0.002)	(0.002)	(0.003)
Temporary Seperated	0.011	0.008	0.009
	(0.016)	(0.019)	(0.025)
Seperated	-0.163	-0.016	-0.251
	(0.062)***	(0.062)	(0.085)***
Divorced	-0.191	-0.207	-0.19
	(0.078)**	(0.140)	(0.090)**
Widowed	-0.03	-0.007	-0.045
	(0.010)***	(0.010)	(0.019)**
Never married	-0.609	-0.104	-0.696
	(0.104)***	(0.273)	(0.087)***
Age Cohort FE	Y	Y	Y
Cragg-Donald Wald F	84.62	39.24	43.23
R-squared	0.12	0.11	0.14
Observations	9,657	5,140	4,517

Notes: Standard errors clustered at county level are reported in brackets. *significant at 10% level; **significant at 5% level; ***significant at 1% level. Col (1) is for the whole sample; Col (2) is for men only; Col (3) is for women only.

Table E.4: First Stage Results for Number of Children

	(1)	(2)	(3)
Age<62*First-Born Girl	0.263	0.262	0.256
	(0.059)***	(0.079)***	(0.070)***
First-Born Girl	0.123	0.121	0.141
	(0.055)**	(0.074)	(0.063)**
Age<62	-4.011	-3.779	0.209
	(0.095)***	(0.134)***	(0.169)
Men	-0.046		
	(0.021)**		
Year of Schooling	0.003	0.004	0.001
	(0.004)	(0.004)	(0.006)
# of Siblings	0.001	0	0.002
	(0.006)	(0.009)	(0.009)
Temporary Seperated	-0.012	0.004	-0.014
	(0.055)	(0.061)	(0.089)
Seperated	-0.105	0.307	-0.433
	(0.201)	(0.297)	(0.277)
Divorced	-0.642	-0.977	-0.477
	(0.201)***	(0.378)**	(0.202)**
Widowed	-0.08	0.001	-0.296
	(0.054)	(0.063)	(0.077)***
Never married	-1.748	-0.87	-1.917
	(0.306)***	(0.155)***	(0.412)***
Age Cohort FE	Y	Y	Y
Cragg-Donald Wald F	33.4	15.33	18.53
R-squared	0.45	0.47	0.44
Observations	9,657	5,140	4,517

Notes: Standard errors clustered at county level are reported in brackets. *significant at 10% level; **significant at 5% level; ***significant at 1% level. Col (1) is for the whole sample; Col (2) is for men only; Col (3) is for women only.

Table E.5: OLS Results for Effects of Fertility on Parent's Mental Health

	Two or More Children			Number of Children		
	(1)	(2)	(3)	(4)	(5)	(6)
Fertility	0.214	0.083	0.32	0.099	0.059	0.126
	(0.236)	(0.322)	(0.341)	(0.068)	(0.086)	(0.089)
Age at First Birth	0.035	0.065	-0.018	0.043	0.07	-0.008
	(0.018)*	(0.024)***	(0.026)	(0.018)**	(0.024)***	(0.026)
Men	-1.74			-1.747		
	(0.135)***			(0.133)***		
Year of Schooling	-0.15	-0.187	-0.105	-0.15	-0.193	-0.103
	(0.021)***	(0.030)***	(0.032)***	(0.021)***	(0.029)***	(0.032)***
# of Siblings	0.031	0.068	-0.005	0.025	0.073	-0.02
	(0.036)	(0.046)	(0.055)	(0.035)	(0.045)	(0.055)
Temporary Seperated	0.593	1.219	0.309	0.518	1.091	0.271
	(0.310)*	(0.529)**	(0.364)	(0.305)*	(0.518)**	(0.363)
Seperated	5.735	6.7	3.763	4.846	5.41	3.489
	(1.294)***	(1.494)***	(2.261)*	(1.146)***	(1.460)***	(2.111)*
Divorced	3.886	5.365	1.343	3.906	5.399	1.442
	(1.112)***	(1.379)***	(1.772)	(1.100)***	(1.371)***	(1.802)
Widowed	1.325	1.643	1.332	1.337	1.708	1.306
	(0.272)***	(0.480)***	(0.313)***	(0.267)***	(0.472)***	(0.313)***
Never married	1.848	2.567	1.663	1.54	1.664	1.812
	(1.903)	(3.137)	(2.296)	(1.532)	(1.771)	(2.134)
R-squared	0.07	0.05	0.03	0.07	0.05	0.03
Observations	9,462	4,406	5,056	9,657	4,517	5,140

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

Table E.6: 2SLS Results for Effects of Fertility on Parent's Mental Health

	Two or More Children			Number of Children		
	(1)	(2)	(3)	(4)	(5)	(6)
Fertility	8.328	4.925	9.834	3.021	1.901	3.33
	(2.980)***	(3.486)	(4.197)**	(1.255)**	(1.506)	(1.602)**
Age at First Birth	0.144	0.134	0.106	0.324	0.24	0.307
	(0.043)***	(0.054)**	(0.061)*	(0.122)***	(0.141)*	(0.161)*
Men	-1.85			-1.609		
	(0.144)***			(0.153)***		
Year of Schooling	-0.14	-0.189	-0.077	-0.162	-0.198	-0.117
	(0.023)***	(0.031)***	(0.038)**	(0.025)***	(0.032)***	(0.036)***
# of Siblings	0.019	0.064	-0.024	0.031	0.068	-0.008
	(0.037)	(0.048)	(0.058)	(0.041)	(0.048)	(0.064)
Temporary Seperated	0.545	1.161	0.303	0.528	1.11	0.231
	(0.324)*	(0.555)**	-0.383	(0.348)	(0.530)**	(0.427)
Seperated	6.659	7.6	3.577	5.007	6.095	2.303
	(1.451)***	(1.674)***	(2.324)	(1.312)***	(1.558)***	(2.418)
Divorced	5.562	6.369	3.425	5.825	6.426	4.684
	(1.453)***	(1.734)***	(2.549)	(1.516)***	(1.709)***	(3.221)
Widowed	1.483	1.809	1.365	1.569	2.289	1.268
	(0.280)***	(0.505)***	(0.322)***	(0.328)***	(0.684)***	(0.372)***
Never married	4.512	4.796	2.79	6.631	5.223	4.551
	(2.629)*	(3.714)	(4.573)	(2.579)**	(3.323)	(3.015)
R-squared	-0.05	0	-0.11	-0.19	-0.06	-0.27
Observations	9,441	4,373	5,033	9,637	4,484	5,118

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

Table E.7: 2SLS Estimates of Effects of Two or More Children on Parent's Mental Health by Parents' Education

	Less Than Primary School			At Least Primary School		
	(1)	(2)	(3)	(4)	(5)	(6)
# of Children>1	6.593	10.597	6.401	7.427	5.866	9.601
	(6.675)	(9.082)	(8.598)	(3.460)**	(4.689)	(5.069)*
Age at First Birth	0.118	0.169	0.075	0.14	0.148	0.091
	(0.077)	(0.098)*	(0.103)	(0.063)**	(0.085)*	(0.103)
Men	-1.931			-1.816		
	(0.233)***			(0.182)***		
Year of Schooling	0.033	-0.105	0.082	-0.187	-0.241	-0.062
	(0.069)	(0.129)	(0.092)	(0.047)***	(0.061)***	(0.084)
# of Siblings	-0.05	0.001	-0.057	0.076	0.095	0.075
	(0.062)	(0.095)	(0.087)	(0.049)	(0.062)	(0.095)
Temporary Seperated	0.831	1.77	0.538	0.264	0.557	0.064
	(0.406)**	(0.875)**	(0.495)	(0.523)	(0.774)	(0.714)
Seperated	5.971	9.499	2.54	8.167	7.459	13.518
	(1.695)***	(2.558)***	(2.596)	(2.301)***	(2.536)***	(0.880)***
Divorced	5.155	5.083	4.682	6.184	6.842	0.903
	(2.500)**	(4.643)	(3.196)	(1.838)***	(2.046)***	(1.754)
Widowed	1.308	2.083	1.189	1.968	1.844	2.121
	(0.367)***	(0.796)***	(0.400)***	(0.461)***	(0.727)**	(0.676)***
Never married	3.755	4.372	2.53	7.829	7.026	
	(2.835)	(5.276)	(3.810)	(3.741)**	(4.961)	
R-squared	-0.02	-0.13	-0.03	-0.06	-0.04	-0.15
Observations	4,670	1,424	3,207	4,741	2,908	1,769

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

Table E.8: 2SLS Estimates of Effects of Number of Children on Parent's Mental Health by Parents' Education

	Less Than Primary School			At Least Primary School		
	(1)	(2)	(3)	(4)	(5)	(6)
# of Children	1.439	2.567	1.503	4.659	3.517	5.329
	(1.415)	(2.192)	(1.871)	(2.584)*	(5.999)	(3.270)
Age at First Birth	0.183	0.31	0.145	0.466	0.577	0.456
	(0.136)	(0.201)	(0.186)	(0.252)*	(0.580)	(0.314)
Men	-1.737			-1.679		
	(0.241)***			(0.231)***		
Year of Schooling	0.044	-0.084	0.095	-0.181	-0.196	-0.109
	(0.067)	(0.131)	(0.096)	(0.053)***	(0.083)**	(0.100)
# of Siblings	-0.034	-0.074	-0.014	0.069	0.14	-0.138
	(0.057)	(0.101)	(0.074)	(0.056)	(0.081)*	(0.130)
Temporary Seperated	0.704	1.818	0.415	0.154	0.014	-0.302
	(0.415)*	(0.968)*	(0.523)	(0.525)	(1.099)	(0.804)
Seperated	4.79	8.148	1.809	6.75	6.608	13.524
	(1.531)***	(2.282)***	(2.839)	(2.304)***	(3.356)**	(7.316)*
Divorced	4.679	4.743	4.776	7.408	8.596	2.57
	(2.204)**	(5.018)	(3.326)	(2.026)***	(3.546)**	(5.005)
Widowed	1.444	2.949	1.215	1.938	3.265	0.705
	(0.408)***	(1.104)***	(0.425)***	(0.479)***	(2.048)	(1.165)
Never married	3.863	7.244	2.798	8.404	8.909	
	(3.110)	(5.844)	(2.883)	(4.532)*	(7.373)	
R-squared	-0.03	-0.26	-0.05	-0.46	-0.83	-0.5
Observations	4,765	1,470	3,257	4,842	2,974	1,805

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

Table E.9: 2SLS Results for Effects of Fertility on Parent's Self-Reported Health

	Two or More Children			Number of Children		
	(1)	(2)	(3)	(4)	(5)	(6)
Fertility	-0.664	-0.148	-0.981	-0.275	-0.094	-0.358
	(0.374)*	(0.503)	(0.518)*	(0.154)*	(0.205)	(0.191)*
Age at First Birth	-0.011	-0.004	-0.015	-0.028	-0.009	-0.037
	(0.006)*	(0.008)	(0.008)*	(0.015)*	(0.019)	(0.019)*
Men	0.154			0.129		
	(0.020)***			(0.021)***		
Year of Schooling	0.01	0.015	0.001	0.012	0.016	0.005
	(0.003)***	(0.004)***	-0.005	(0.003)***	(0.004)***	(0.004)
# of Siblings	0.006	-0.004	0.014	0.005	-0.002	0.01
	(0.005)	(0.007)	(0.007)**	(0.005)	(0.007)	(0.007)
Temporary Seperated	0.002	-0.04	0.006	0.001	-0.027	0.003
	(0.041)	(0.065)	(0.053)	(0.041)	(0.064)	(0.054)
Seperated	-0.326	-0.319	-0.067	-0.228	-0.255	-0.013
	(0.134)**	(0.197)	(0.163)	(0.112)**	(0.179)	(0.190)
Divorced	-0.006	0.131	-0.19	-0.049	0.114	-0.33
	(0.185)	(0.235)	(0.242)	(0.187)	(0.237)	(0.256)
Widowed	0.055	0.134	0.012	0.035	0.098	0.01
	(0.032)*	(0.060)**	(0.040)	(0.035)	(0.089)	(0.043)
Never married	-0.477	-0.346	-0.193	-0.874	-0.587	-0.374
	(0.286)*	(0.242)	(0.136)	(0.326)***	(0.414)	(0.324)
R-squared	0.01	0.04	-0.06	-0.05	0.03	-0.15
Observations	9,439	4,372	5,032	9,635	4,483	5,117

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

Table E.10: 2SLS Estimates of Effects of Fertility on Diagnosis of Chronic Diseases

	Arthritis or Rheumatism		Hypertension		Digestive Diseases	
	# of Children>1	# of Children	# of Children>1	# of Children	# of Children>1	# of Children
All Sample	0.346	0.127	-0.115	-0.031	-0.016	-0.013
	(0.222)	(0.084)	(0.186)	(0.071)	(0.175)	(0.067)
Men Only	0.128	0.036	0.052	0.038	-0.032	-0.024
	(0.273)	(0.109)	(0.259)	(0.105)	(0.240)	(0.098)
Women Only	0.279	0.109	-0.093	-0.022	-0.089	-0.037
	(0.297)	(0.104)	(0.267)	(0.095)	(0.267)	(0.096)

Notes: Standard errors clustered at county level are reported in brackets.
All regressions controls age, age square, gender of first birth, age at first birth, sex, education, number of siblings, marital status, self-reported health during childhood, and county fixed effects.

Table E.11: 2SLS Results for Effects of Fertility on Parent's Mental Health (Controlling Self-Reported Health)

	Two or More Children			Number of Children		
	(1)	(2)	(3)	(4)	(5)	(6)
Fertility	6.773	4.657	7.237	2.404	1.742	2.426
	(2.685)**	(3.293)	(3.743)*	(1.111)**	(1.433)	(1.431)*
Age at First Birth	0.118	0.127	0.067	0.261	0.224	0.214
	(0.038)***	(0.051)**	(0.054)	(0.107)**	(0.134)*	(0.142)
Men	-1.497			-1.325		
	(0.135)***			(0.145)***		
Year of Schooling	-0.115	-0.158	-0.073	-0.134	-0.166	-0.103
	(0.021)***	(0.029)***	(0.035)**	(0.023)***	(0.030)***	(0.033)***
# of Siblings	0.032	0.056	0.013	0.042	0.065	0.018
	(0.035)	(0.045)	(0.054)	(0.038)	(0.045)	(0.059)
Temporary Seperated	0.55	1.079	0.317	0.53	1.055	0.237
	(0.300)*	(0.513)**	(0.352)	(0.322)	(0.501)**	(0.388)
Seperated	5.907	6.954	3.406	4.502	5.589	2.281
	(1.426)***	(1.778)***	(2.000)*	(1.306)***	(1.638)***	(2.109)
Divorced	5.546	6.644	2.924	5.716	6.678	3.851
	(1.325)***	(1.565)***	(2.488)	(1.399)***	(1.565)***	(3.085)
Widowed	1.621	2.085	1.413	1.664	2.498	1.318
	(0.264)***	(0.470)***	(0.301)***	(0.300)***	(0.641)***	(0.333)***
Never married	3.405	4.104	2.285	4.682	4.091	3.616
	(2.540)	(3.669)	(4.859)	(2.343)**	(3.194)	(3.580)
Self-Reported Health	-2.309	-2.048	-2.616	-2.232	-2.039	-2.487
	(0.089)***	(0.122)***	(0.115)***	(0.097)***	(0.127)***	(0.137)***
R-squared	0.09	0.1	0.07	0	0.05	-0.02
Observations	9,439	4,372	5,032	9,635	4,483	5,117

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

Table E.12: 2SLS Results for Effects of Fertility on Co-residence with Children

	Two or More Children			Number of Children		
	(1)	(2)	(3)	(4)	(5)	(6)
Fertility	-0.281	-0.261	-0.276	-0.106	-0.101	-0.098
	(0.246)	(0.290)	(0.294)	(0.094)	(0.115)	(0.107)
Age at First Birth	0.007	0.007	0.008	0.001	0.002	0.002
	(0.004)*	(0.004)*	(0.005)*	(0.009)	(0.011)	(0.011)
Men	-0.002			-0.009		
	(0.008)			(0.009)		
Year of Schooling	-0.003	-0.005	-0.001	-0.002	-0.005	0
	(0.002)	(0.002)*	(0.003)	(0.002)	(0.002)**	(0.002)
# of Siblings	0	0.003	-0.002	-0.001	0.003	-0.003
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Temporary Seperated	0.006	-0.039	0.03	0.002	-0.043	0.028
	(0.028)	(0.045)	(0.032)	(0.028)	(0.047)	(0.032)
Seperated	-0.18	-0.252	-0.022	-0.14	-0.225	0.018
	(0.087)**	(0.116)**	(0.109)	(0.083)*	(0.108)**	(0.112)
Divorced	-0.152	-0.159	-0.159	-0.168	-0.166	-0.201
	(0.086)*	(0.105)	(0.121)	(0.096)*	(0.110)	(0.153)
Widowed	0.095	0.051	0.117	0.095	0.029	0.122
	(0.021)***	(0.032)	(0.023)***	(0.021)***	(0.047)	(0.023)***
Never married	-0.301	-0.351	-0.134	-0.316	-0.365	-0.194
	(0.169)*	(0.229)	(0.093)	(0.181)*	(0.248)	(0.100)*
R-squared	0.01	0.01	0.02	0.03	0.02	0.02
Observations	9,590	4,460	5,095	9,590	4,460	5,095

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

Table E.13: 2SLS Results for Effects of Fertility on Parent's Mental Health (Controlling Living Arrangements)

	Two or More Children			Number of Children		
	(1)	(2)	(3)	(4)	(5)	(6)
Fertility	8.162	5.065	9.421	2.914	1.882	3.188
	(2.992)***	(3.492)	(4.244)**	(1.239)**	(1.458)	(1.608)**
Age at First Birth	0.14	0.136	0.1	0.319	0.243	0.302
	(0.041)***	(0.052)***	(0.058)*	(0.120)***	(0.138)*	(0.162)*
Men	-1.833			-1.617		
	(0.142)***			(0.150)***		
Year of Schooling	-0.141	-0.188	-0.084	-0.162	-0.195	-0.124
	(0.023)***	(0.031)***	(0.038)**	(0.024)***	(0.032)***	(0.036)***
# of Siblings	0.016	0.064	-0.029	0.027	0.068	-0.012
	(0.038)	(0.048)	(0.059)	(0.041)	(0.048)	(0.064)
Temporary Seperated	0.529	1.11	0.286	0.52	1.073	0.231
	(0.327)	(0.562)**	(0.383)	(0.347)	(0.534)**	(0.427)
Seperated	6.589	7.578	3.524	4.919	6	2.319
	(1.449)***	(1.671)***	(2.329)	(1.298)***	(1.540)***	(2.406)
Divorced	5.494	6.368	3.254	5.718	6.394	4.447
	(1.448)***	(1.730)***	(2.531)	(1.496)***	(1.688)***	(3.180)
Widowed	1.506	1.861	1.361	1.596	2.35	1.254
	(0.282)***	(0.509)***	(0.323)***	(0.326)***	(0.681)***	(0.366)***
Never married	4.194	4.797	2.226	6.361	5.134	3.893
	(2.633)	(3.694)	(4.256)	(2.554)**	(3.230)	(2.769)
Self-Reported Health	-0.335	-0.348	-0.272	-0.495	-0.459	-0.394
	(0.177)*	(0.206)*	(0.216)	(0.204)**	(0.243)*	(0.251)
R-squared	0.04	0	0.1	0.17	0.06	0.24
Observations	9,398	4,351	5,012	9,590	4,460	5,095

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

APPENDIX F

FIGURES FOR CHAPTER 3

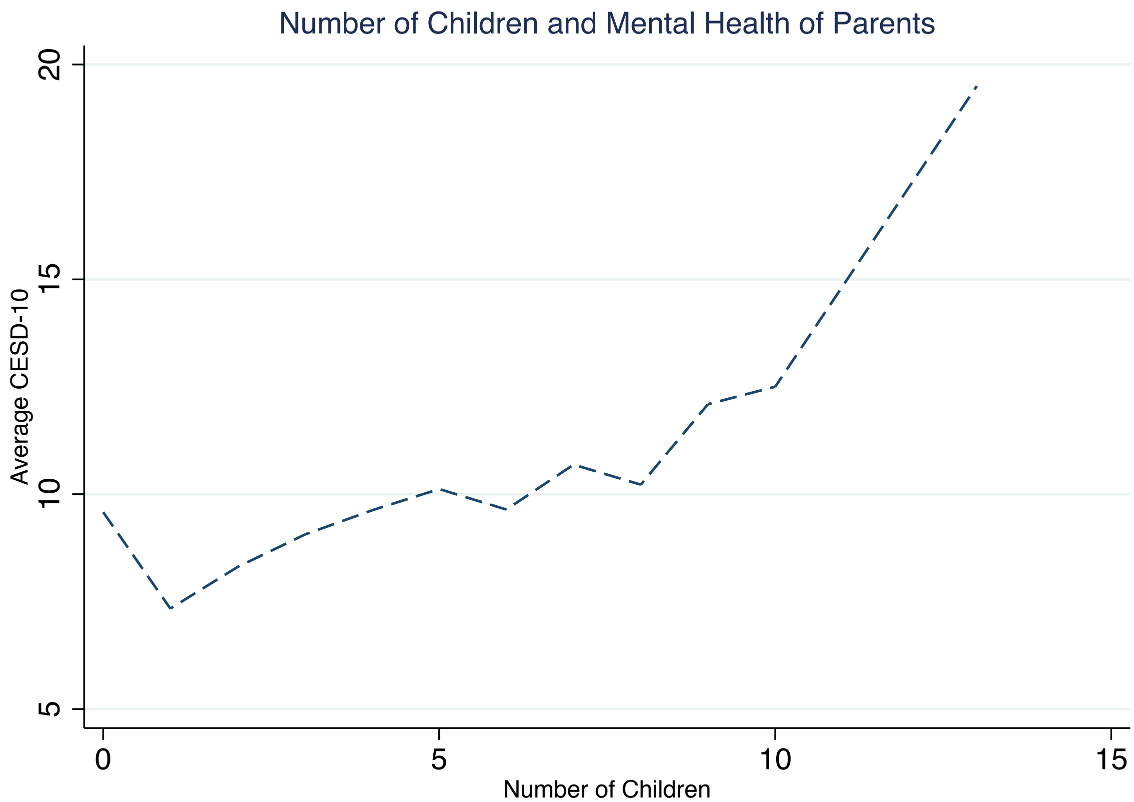


Figure F.1: Number of Children and Parent’s CES-D Score
Data Sources: China Health and Retirement Longitudinal Survey (2011)

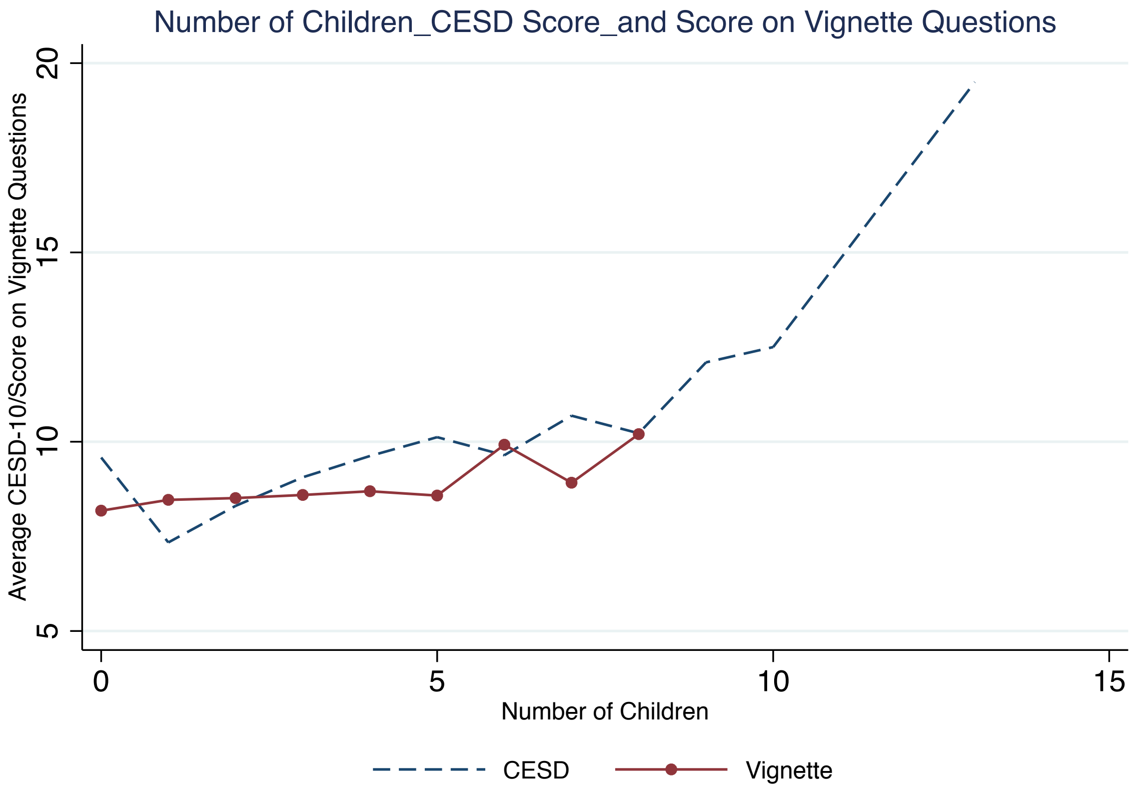


Figure F.2: Number of Children and Parent's Vignette Question Score
Data Sources: China Health and Retirement Longitudinal Survey (2011)

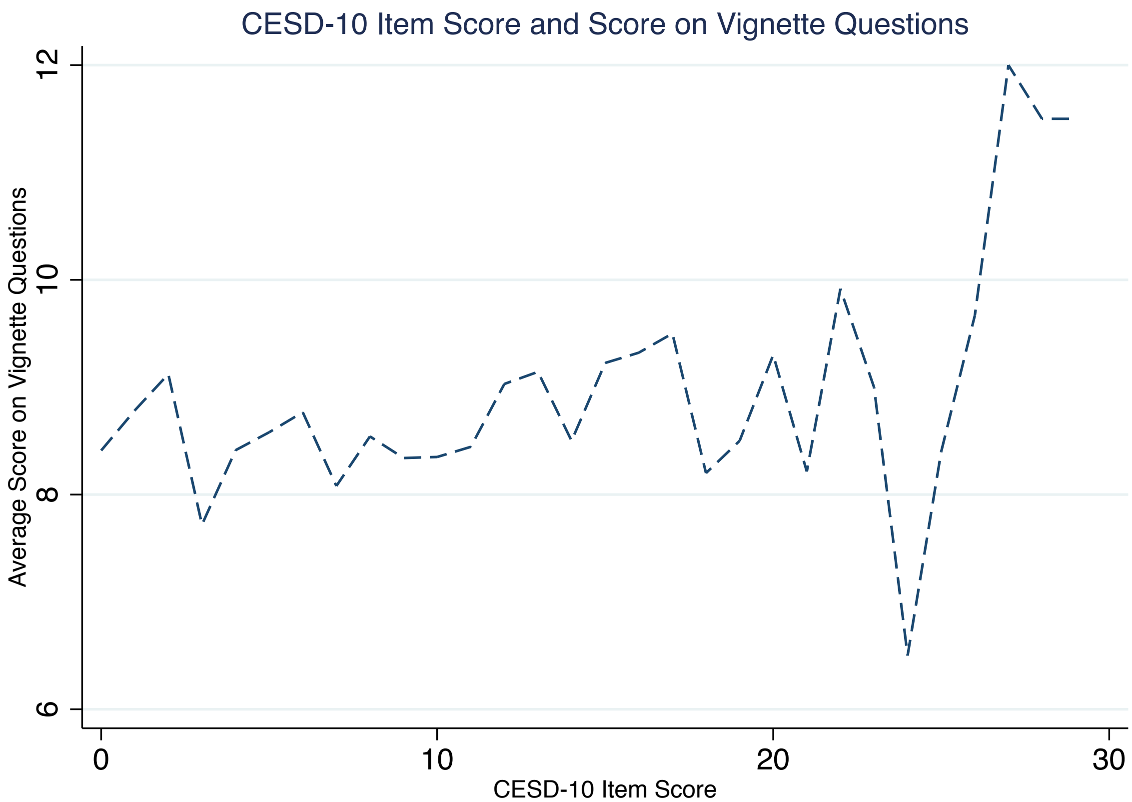


Figure F.3: CES-D Score and Vignette Question Score
Data Sources: China Health and Retirement Longitudinal Survey (2011)

APPENDIX G

APPENDICES FOR CHAPTER 1

Table G.1: Coefficients of Interaction Terms for First Stage and Reduced Form when Focusing on Mothers$\geq$30

	(1) First Stage	(2) Reduced Form		(1) First Stage	(2) Reduced Form
age30*non-Han	0.079	-0.012	age40*First-Born Girl	0.043	-0.012
	(0.014)***	(0.006)**		(0.006)***	(0.004)***
age31*non-Han	0.089	-0.014	age31*First-Born Girl	0.058	-0.01
	(0.011)***	(0.006)**		(0.006)***	(0.004)***
age32*non-Han	0.088	-0.005	age32*First-Born Girl	0.06	-0.007
	(0.010)***	(0.005)		(0.006)***	(0.003)**
age33*non-Han	0.082	-0.012	age33*First-Born Girl	0.062	-0.01
	(0.010)***	(0.005)**		(0.006)***	(0.003)***
age34*non-Han	0.072	-0.013	age34*First-Born Girl	0.055	-0.007
	(0.009)***	(0.005)***		(0.006)***	(0.003)**
age35*non-Han	0.058	-0.012	age35*First-Born Girl	0.047	-0.007
	(0.008)***	(0.005)***		(0.005)***	(0.003)**
age36*non-Han	0.052	-0.01	age36*First-Born Girl	0.036	-0.009
	(0.007)***	(0.005)*		(0.004)***	(0.003)***
age37*non-Han	0.033	-0.002	age37*First-Born Girl	0.022	-0.005
	(0.006)***	(0.005)		(0.004)***	(0.004)
age38*non-Han	0.029	-0.016	age38*First-Born Girl	0.015	-0.006
	(0.006)***	(0.004)***		(0.003)***	(0.003)*
age39*non-Han	0.014	-0.003	age39*First-Born Girl	0.011	-0.005
	(0.006)**	(0.005)		(0.003)***	(0.004)
Observations	475,344	475,344	*Observations*	339,118	339,118

Table G.1 (cont'd)

Notes: Standard errors clustered at county level are reported in brackets. *significant at 10% level; **significant at 5% level; ***significant at 1% level. Col (1) and (2) are on observations from restricted provinces (1-Child Provinces and 1-Boy-2-Girl Provinces); Col (3) and (4) are on observations from 1-Boy-2-Girl Provinces. All regressions controls age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, county fixed effects.

Table G.2: OLS and 2SLS Estimates of the Effect of Additional Children on Female LFP when Focusing on Mothers$\geq$30

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
kids2	0.01	-0.121	0.009	-0.129
	(0.002)***	(0.046)***	(0.003)***	(0.037)***
non-Han	0.009	0.011		
	(0.008)	(0.008)		
First-Born Girl	-0.001	0.008	-0.001	0.006
	(0.001)	(0.003)***	(0.001)	(0.002)***
Cragg-Donald Wald F statistic		46.886		80.629
Observations	475,344	475,304	339,118	339,085

Notes: Standard errors clustered at county level are reported in brackets. *significant at 10% level; **significant at 5% level; ***significant at 1% level. All regressions controls age cohort dummies, mother's age at first birth, education levels for both parents, county fixed effects. Col (1) and (2) are on observations in 1-Child Provinces and 1-Boy-2-Girl Provinces, using interactions of age cohort and ethnicity as instruments; Col (3) and (4) are on Han people only in 1-Boy-2-Girl Provinces, using interactions of age cohort and gender of first birth as instruments.

Table G.3: Fertility on Female LFP: T_i·non-Han as IV

	(1) First Stage	(2) Reduced Form	(3) OLS	(4) 2SLS
kids2			0	-0.255
			(0.001)	(0.090)***
non-Han	-0.137	0.04	0.003	0.006
	(0.039)***	(0.012)***	(0.008)	(0.008)
First-Born Girl	0.059	-0.001	-0.001	0.015
	(0.003)***	(0.000)	(0.000)	(0.005)***
T_i·non-Han	0.166	-0.042		
	(0.044)***	(0.010)***		
Observations	778,754	778,754	778,754	778,710

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions controls age cohort dummies, mother's age at first birth, education levels for both parents, county fixed effects. All regressions on observations from 1-Child Provinces and 1-Boy-2-Girl Provinces.

Follow Wu and Li (2012), I use interaction of first-born girl dummy and a measure of time exposure to OCP as an instrument. The value of T_i can be expressed as below,

$$T_i = \begin{cases} 1 & for\, women\, younger\, than\, 18\, at\, 1979 \\ \frac{55-age\, at\, 1979}{55-18} & for\, women\, betIen\, 18\, and\, 55\, at\, 1979 \\ 0 & for\, women\, older\, than\, 55\, at\, 1979 \end{cases}$$

Table G.4: Fertility on Female LFP: T_i·First-Born Girl as IV

	(1) First Stage	(2) Reduced Form	(3) OLS	(4) 2SLS
kids2			-0.002	-0.86
			(0.002)	(0.429)**
non-Han	0.011	0.002	0.002	0.011
	(0.004)**	(0.008)	(0.008)	(0.010)
First-Born Girl	0.024	0.016	-0.001	0.037
	(0.008)***	(0.005)***	(0.001)*	(0.019)**
T_i·First-Born Girl	0.023	-0.019		
	(0.010)**	(0.005)***		
Observations	612,785	612,785	612,785	612,749

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions controls age cohort dummies, mother's age at first birth, education levels for both parents, county fixed effects. All regressions on observations from 1-Boy-2-Girl Provinces.

Follow Wu and Li (2012), I use interaction of first-born girl dummy and a measure of time exposure to OCP as an instrument. The value of T_i can be expressed as below,

$$T_i = \begin{cases} 1 & for\,women\,younger\,than\,18\,at\,1979 \\ \frac{55-age\,at\,1979}{55-18} & for\,women\,betIen\,18\,and\,55\,at\,1979 \\ 0 & for\,women\,older\,than\,55\,at\,1979 \end{cases}$$

Table G.5: Fertility on Female LFP: DID Based on Ethnicity as IV

	OLS	2SLS	First Stage	Reduced Form
kids2	0.005	-0.139		
	(0.002)***	(0.044)***		
age29*non-Han			0.083	-0.03
			(0.016)***	(0.011)***
age30*non-Han			0.08	-0.03
			(0.015)***	(0.010)***
age31*non-Han			0.088	-0.032
			(0.014)***	(0.011)***
age32*non-Han			0.085	-0.023
			(0.013)***	(0.010)**
age33*non-Han			0.08	-0.03
			(0.013)***	(0.010)***
age34*non-Han			0.07	-0.031
			(0.012)***	(0.010)***
age35*non-Han			0.057	-0.03
			(0.012)***	(0.010)***
age36*non-Han			0.051	-0.028
			(0.011)***	(0.010)***
age37*non-Han			0.031	-0.019
			(0.011)***	(0.010)*
age38*non-Han			0.028	-0.033
			(0.011)***	(0.010)***
age39*non-Han			0.013	-0.021
			(0.010)	(0.010)**
age40*non-Han			0.005	-0.025
			(0.010)	(0.010)**
age41*non-Han			-0.003	-0.028
			(0.010)	(0.010)***
age42*non-Han			-0.012	-0.01
			(0.010)	(0.011)
age43*non-Han			-0.003	-0.014
			(0.010)	(0.010)
age44*non-Han			-0.002	-0.011
			(0.011)	(0.010)
Observations	593,411	593,358	593,411	593,411

Notes: Standard errors clustered at county level are reported in brackets. * significant at 10% level, ** significant at 5% level, *** significant at 1% level. All regressions are on observations from restrcited provinces (1-child provinces and 1-boy-2-girl provinces), excluding mothers younger than 27. All regressions controls age cohort dummies, ethnicity dummies, gender of first birth, mother's age at first birth, education levels for both parents, county fixed effects.

Table G.6: Coefficients of Tripple Interaction Terms

	(1) First Stage	(2) Reduced Form		(1) First Stage	(2) Reduced Form
age16*non-Han*First-Born Girl	0.058		age31*non-Han*First-Born Girl	-0.035	0.005
	(0.012)***			(0.014)**	(0.010)
age17*non-Han*First-Born Girl	-0.047		age32*non-Han*First-Born Girl	-0.022	-0.011
	(0.094)			(0.012)*	(0.007)
age18*non-Han*First-Born Girl	0.036		age33*non-Han*First-Born Girl	-0.021	0.006
	(0.157)			(0.012)*	(0.009)
age19*non-Han*First-Born Girl	-0.04		age34*non-Han*First-Born Girl	-0.028	-0.002
	(0.087)			(0.013)**	(0.010)
age20*non-Han*First-Born Girl	-0.079		age35*non-Han*First-Born Girl	-0.045	-0.012
	(0.048)*			(0.011)***	(0.008)
age21*non-Han*First-Born Girl	0.002		age36*non-Han*First-Born Girl	-0.028	0.007
	(0.035)			(0.010)***	(0.008)
age22*non-Han*First-Born Girl	-0.009		age37*non-Han*First-Born Girl	-0.015	-0.003
	(0.026)			(0.010)	(0.008)
age23*non-Han*First-Born Girl	0.039	-0.004	age38*non-Han*First-Born Girl	-0.031	0.015
	(0.025)	(0.011)		(0.009)***	(0.008)*
age24*non-Han*First-Born Girl	-0.015	0.011	age39*non-Han*First-Born Girl	-0.011	-0.01
	(0.019)	(0.009)		(0.011)	(0.011)
age25*non-Han*First-Born Girl	-0.009	-0.01	age40*non-Han*First-Born Girl	-0.01	-0.011
	(0.017)	(0.008)		(0.011)	(0.011)
age26*non-Han*First-Born Girl	-0.037	0.013	age41*non-Han*First-Born Girl	0.001	0.003
	(0.015)**	(0.008)*		(0.011)	(0.011)
age27*non-Han*First-Born Girl	-0.022	0.005	age42*non-Han*First-Born Girl	0.012	0.011
	(0.014)	(0.006)		(0.013)	(0.013)
age28*non-Han*First-Born Girl	0.002	0.01	age43*non-Han*First-Born Girl	0.009	-0.029

Table G.6 (cont'd)

	(0.016)	(0.009)		(0.013)	(0.014)**
age29*non-Han*First-Born Girl	0.02	0.004	age44*non-Han*First-Born Girl	-0.009	0.002
	(0.020)	(0.012)		(0.015)	(0.014)
age30*non-Han*First-Born Girl	0.008	0.007			
	(0.016)	(0.011)	*Observations*	612,785	612,785

Notes: Standard errors clustered at county level are reported in brackets.
* significant at 10% level, ** significant at 5% level, *** significant at 1% level.
All regressions controls age cohort dummies, ethnicity, gender of first birth, interactions of age and ethnicity, interactions of age and gender of first birth, mother's age at first birth, education levels for both parents, county fixed effects.

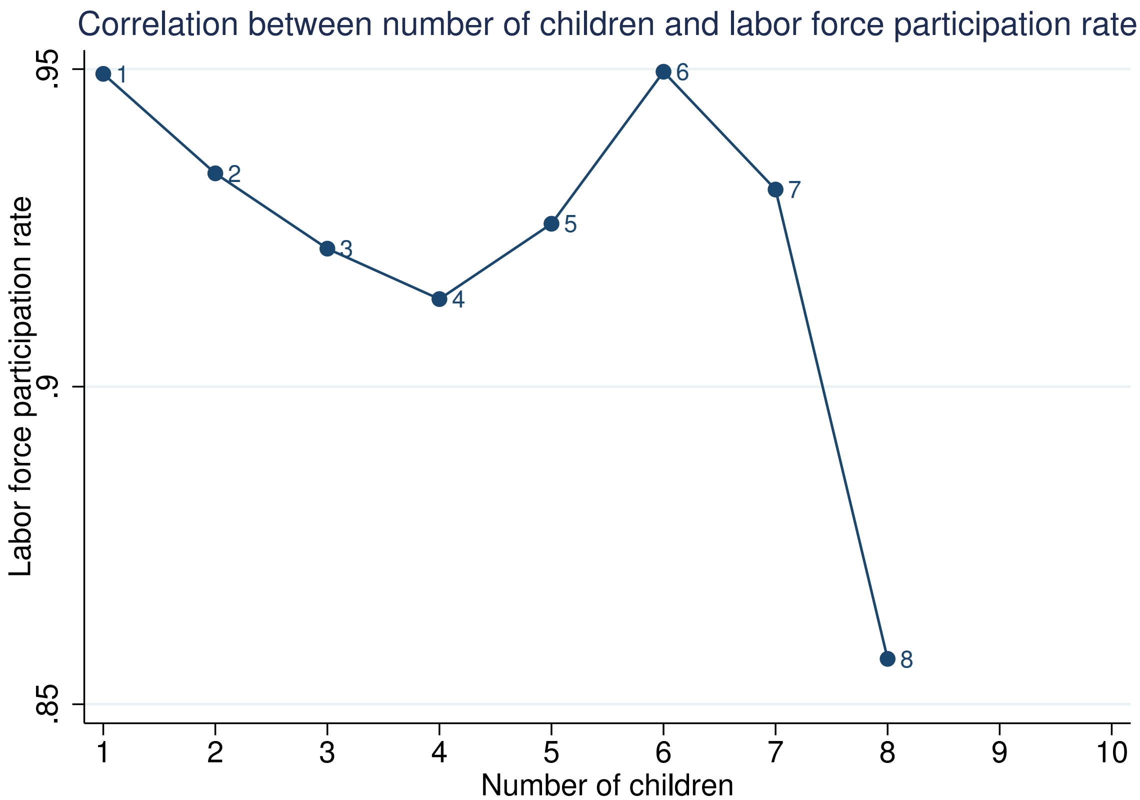


Figure G.1: Correlation between number of children and labor force participation rate
Data Source: 1% sample from 1990 China Population Census.

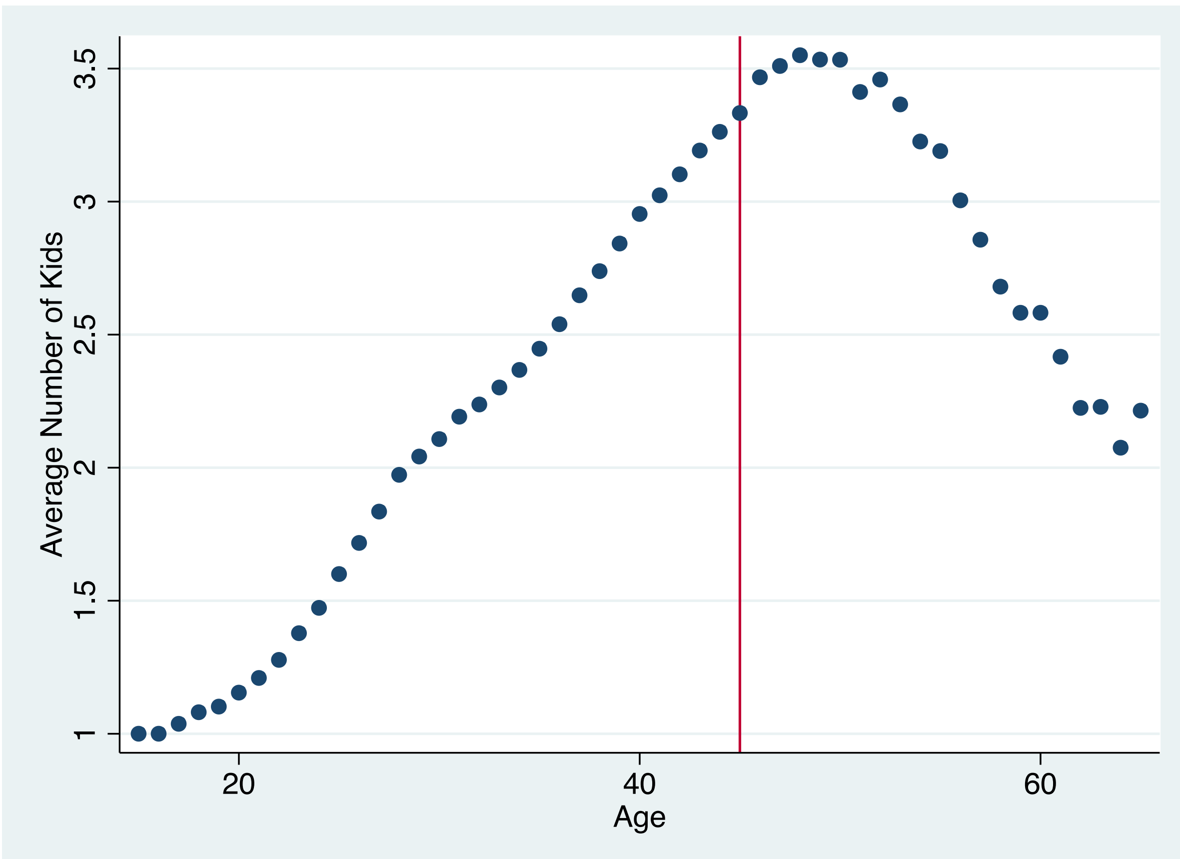
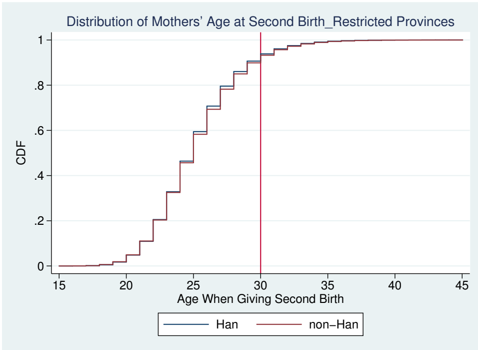
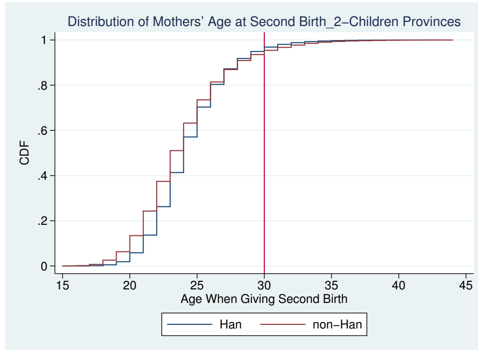


Figure G.2: Average number of children for women at different age.
Data Source: 1% sample from the 1990 China Population Census.



a: Restricted provinces



b: 2-Children provinces

Figure G.3: Cumulative Distribution of Age When Giving Second Birth
Data Source: 1% sample from the 1990 China Population Census

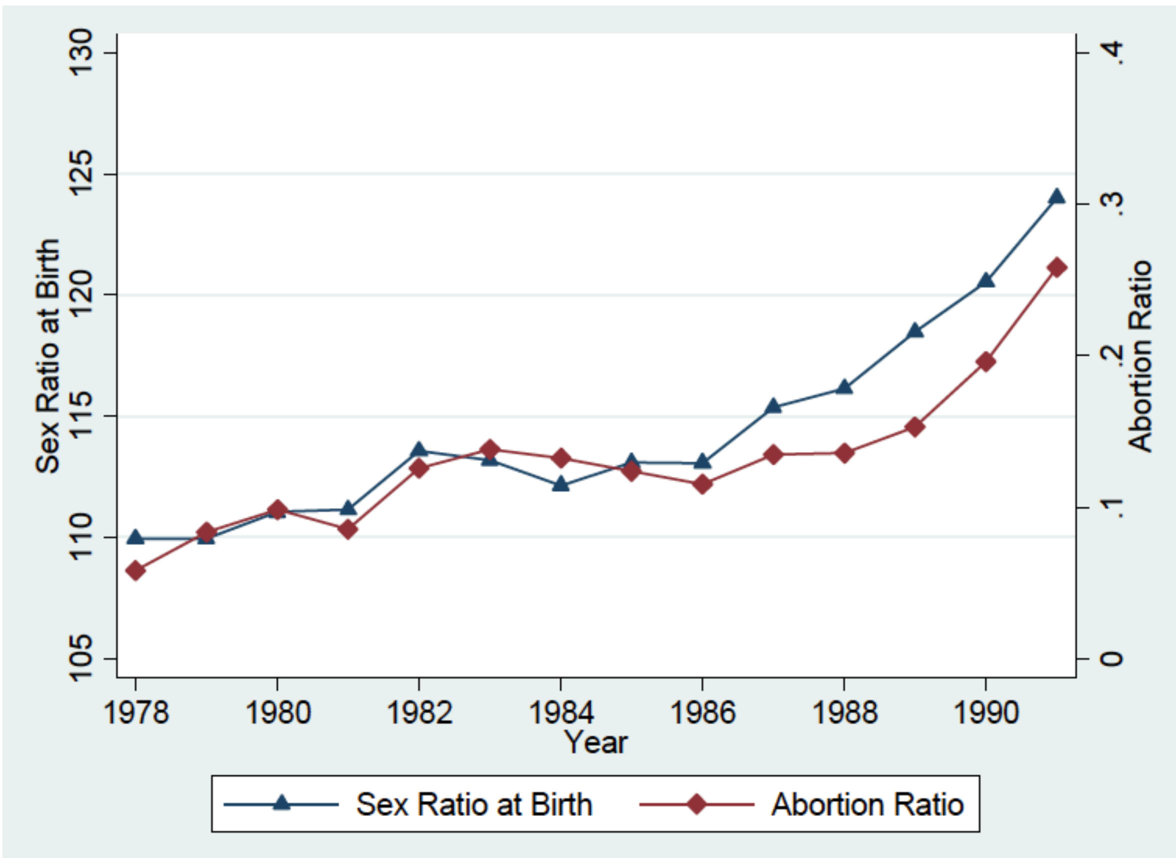


Figure G.4: Sex Ratio at Birth and Abortion Rate in China (1978-1991)
Picture Source: Chen et al. (2013)

APPENDIX H

APPENDICES FOR CHAPTER 2

Table H.1: Impact of Displacement on the Probability of Having an Additional Child (First Difference)

	Men	Women	Women without College Education	Women with College Education
Displacement year - 2	-0.023	0.006	0.011	0.010
	(0.016)	(0.017)	(0.022)	(0.032)
Displacement year - 1	-0.005	-0.024	-0.006	-0.027
	(0.016)	(0.016)	(0.022)	(0.030)
Displacement year	-0.004	-0.021	0.013	-0.039
	(0.016)	(0.017)	(0.025)	(0.032)
Displacement year + 1	-0.009	-0.006	0.041	-0.036
	(0.017)	(0.017)	(0.027)	(0.035)
Displacement year + 2-3	-0.016	0.006	0.072	-0.050
	(0.018)	(0.018)	(0.033)**	(0.038)
Displacement year + 4-5	-0.023	0.035	0.119	-0.046
	(0.023)	(0.023)	(0.041)***	(0.052)
Displacement year + 6-7	-0.036	0.054	0.147	-0.034
	(0.026)	(0.027)**	(0.049)***	(0.063)
Displacement year + 8+	-0.036	0.041	0.144	-0.053
	(0.029)	(0.030)	(0.054)***	(0.072)
Obersations	71,534	77,246	46,063	31,183

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, individual and year fixed effects.

Table H.2: Impact of Displacement on the Probability of Having an Additional Child (Including marriage status)

	Men	Women	Women without College Education	Women with College Education
Displacement year - 2	-0.013	0.016	0.008	0.043
	(0.011)	(0.013)	(0.016)	(0.023)*
Displacement year - 1	0.007	-0.01	-0.015	0.01
	(0.010)	(0.011)	(0.013)	(0.018)
Displacement year	0.011	-0.008	-0.007	0
	(0.009)	(0.010)	(0.013)	(0.017)
Displacement year + 1	0.008	0.006	0.012	0.007
	(0.010)	(0.011)	(0.013)	(0.017)
Displacement year + 2-3	0	0.003	0.014	-0.001
	(0.008)	(0.009)	(0.011)	(0.014)
Displacement year + 4-5	0.001	0.009	0.021	0.008
	(0.008)	(0.009)	(0.011)**	(0.014)
Displacement year + 6-7	-0.008	0.011	0.02	0.017
	(0.008)	(0.008)	(0.010)**	(0.014)
Displacement year + 8+	-0.01	0.008	0.019	0.014
	(0.007)	(0.007)	(0.009)**	(0.011)
Married	0.11	0.099	0.064	0.122
	(0.004)***	(0.004)***	(0.005)***	(0.005)***
Divorce	0.042	0.042	0.023	0.045
	(0.005)***	(0.004)***	(0.006)***	(0.007)***
Obersations	75,205	81,234	48,438	32,796

Notes: Standard errors clustered at individual level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, individual and year fixed effects.

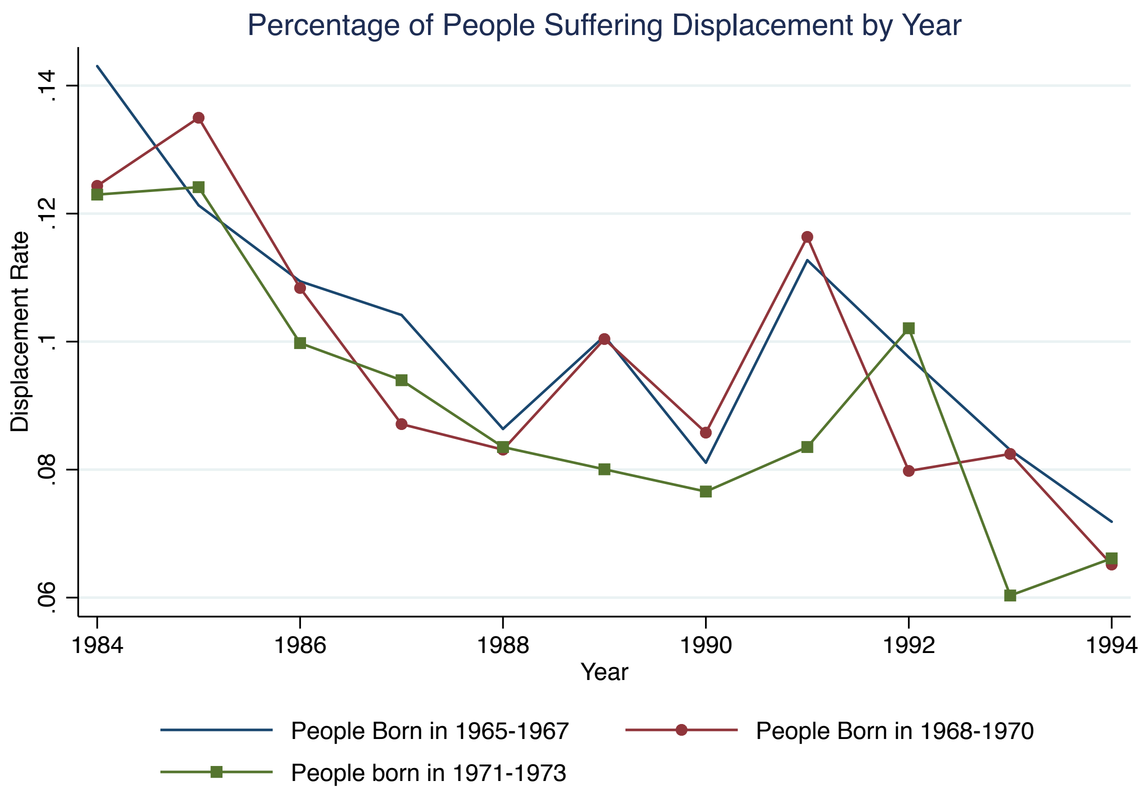


Figure H.1: Displacement and Birth Rate for Men with College Education
Data Sources: NLSY79.

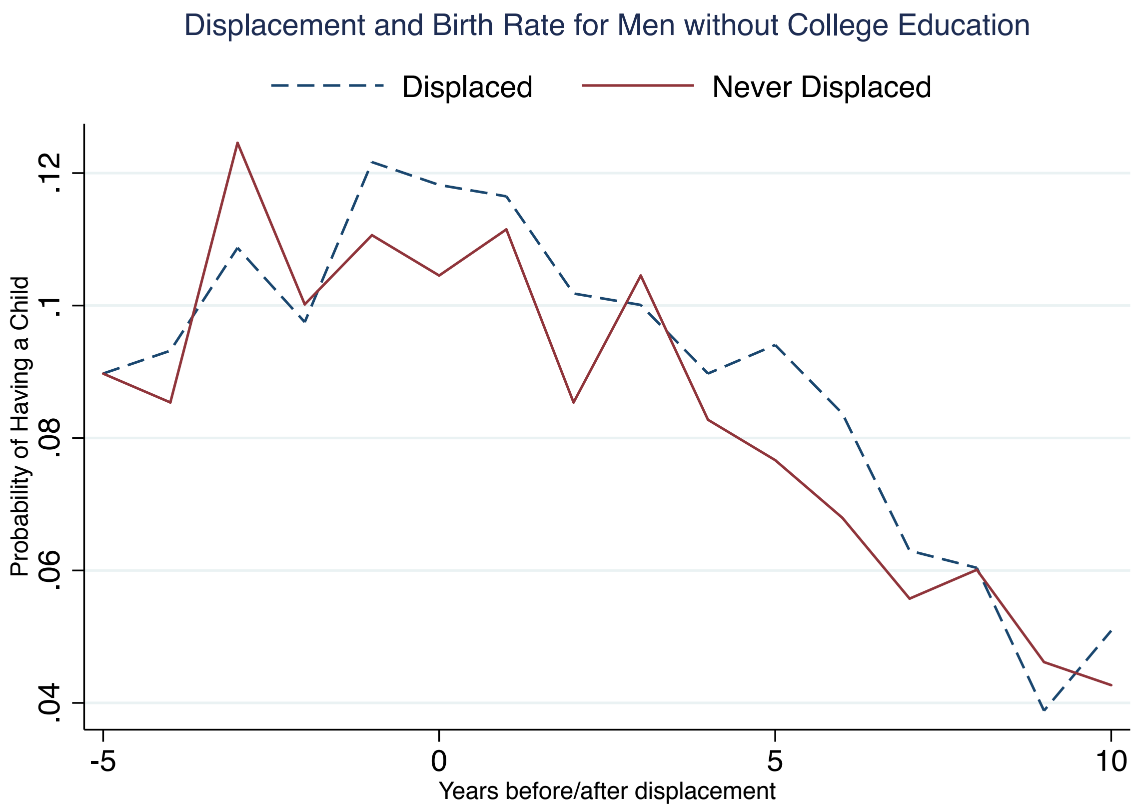


Figure H.2: Displacement and Birth Rate for Men with College Education
Data Sources: NLSY79.

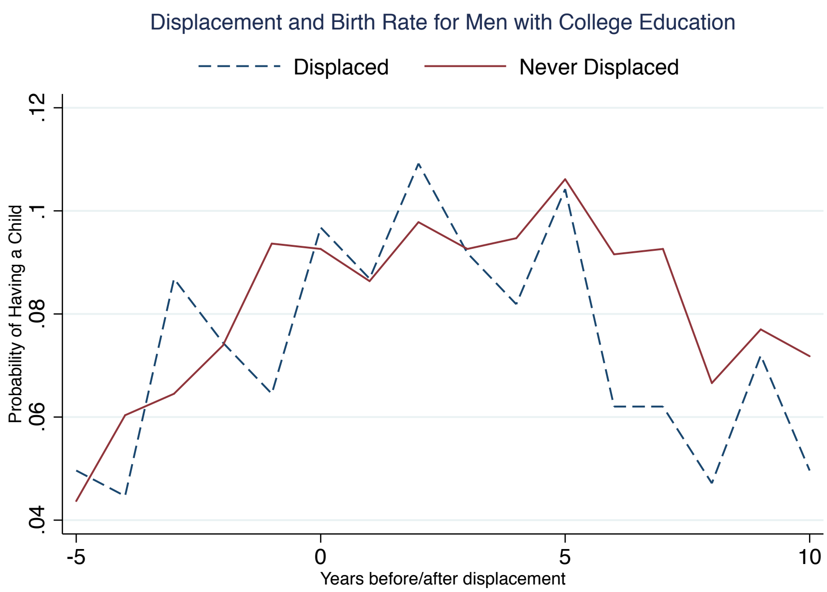


Figure H.3: Displacement and Birth Rate for Men without College Education
Data Sources: NLSY79.

APPENDIX I

APPENDICES FOR CHAPTER 3

Table I.1: List of CES-D-10 items to measure mental health

Item	Description
1	I was bothered by things that don't usually bother me.
2	I had trouble keeping my mind on what I was doing.
3	I felt depressed.
4	I felt everything I did was an effort.
5	I felt hopeful about the future.
6	I felt fearful.
7	My sleep was restless.
8	I was happy.
9	I felt lonely.
10	I could not get "going."

Notes: The 10 items above refer to how individual have felt and behaved during the last week. Individuala are requested to choose the appropriate response as: (1) Rarely or none of the time (<1 day) (2) Some or a little of the time (1-2 days) (3) Occasionally or a moderate amount of the time (3-4 days) (4) Most or all of the time (5-7 days)
For each individual, we use this formula to calculate CES-D-10:
CES-D-10=item1-1+item2-1+item3-1+item4-1+item6-1+item7-1+item9-1+item10-1+4-item5+4-item8

Table I.2: Coefficients of Interaction Terms for the 1st Stage Regression on Two or More Children

	(1)	(2)	(3)		(1)	(2)	(3)
age45*First-Born Girl	0.174	0.142	0.17	age59*First-Born Girl	0.051	0.026	0.046
	(0.062)***	(0.063)**	(0.045)***		(0.030)*	(0.035)	(0.027)*
age46*First-Born Girl	0.16	0.14	0.135	age60*First-Born Girl	0.031	0.02	0.052
	(0.047)***	(0.049)***	(0.040)***		(0.025)	(0.030)	(0.020)**
age47*First-Born Girl	0.154	0.153	0.209	age61*First-Born Girl	-0.005	0.028	0.04
	(0.037)***	(0.043)***	(0.032)***		(0.026)	(0.030)	(0.024)*
age48*First-Born Girl	0.206	0.208	0.157	age62*First-Born Girl	-0.006	-0.043	0.027
	(0.037)***	(0.036)***	(0.031)***		(0.025)	(0.027)	(0.022)
age49*First-Born Girl	0.095	0.109	0.132	age63*First-Born Girl	-0.033	0.014	-0.013
	(0.037)**	(0.041)***	(0.030)***		(0.026)	(0.028)	(0.027)
age50*First-Born Girl	0.136	0.175	0.17	age64*First-Born Girl	-0.062	-0.059	-0.024
	(0.038)***	(0.043)***	(0.031)***		(0.031)**	(0.047)	(0.022)
age51*First-Born Girl	0.132	0.088	0.138	age65*First-Born Girl	0.012	0.029	0.022
	(0.043)***	(0.044)**	(0.032)***		(0.027)	(0.040)	(0.027)
age52*First-Born Girl	0.084	0.151	0.093	age66*First-Born Girl	0.026	0.01	-0.006
	(0.042)**	(0.036)***	(0.041)**		(0.031)	(0.038)	(0.023)
age53*First-Born Girl	0.038	0.093	0.071	age67*First-Born Girl	-0.037	-0.038	-0.058
	-0.036	(0.038)**	(0.038)*		(0.032)	(0.030)	(0.023)**
age54*First-Born Girl	0.107	0.114	0.054	age68*First-Born Girl	-0.014	0.003	-0.031

Table I.2 (cont'd)

	(0.036)***	(0.036)***	(0.031)*		(0.030)	(0.024)	(0.025)
age55*First-Born Girl	0.059	0.134	0.069	age69*First-Born Girl	-0.008	-0.039	-0.031
	(0.033)*	(0.036)***	(0.034)**		(0.040)	(0.042)	(0.033)
age56*First-Born Girl	0.097	0.084	0.057	age70*First-Born Girl	-0.007	-0.031	0.013
	(0.034)***	(0.036)**	(0.031)*		(0.029)	(0.038)	(0.024)
age57*First-Born Girl	0.04	0.073	0.052				
	(0.03)	(0.029)**	(0.030)*	*Cragg-Donald Wald F*	6.79	3.13	4.1
age58*First-Born Girl	0.024	0.039	0.045	*R-squared*	0.13	0.13	0.12
	(0.030)	(0.039)	(0.031)	*Observations*	9,462	4,406	5,056

Notes:
Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age cohort dummies, gender of first birth, age at first birth, sex, education, number of siblings, marital status, self-reported health during childhood, and county fixed effects.
Col (1) is on the whole sample; Col (2) is for men only; Col (3) is for women only.

Table I.3: Coefficients of Interaction Terms for the 1st Stage Regression on Number of Children

	(1)	(2)	(3)		(1)	(2)	(3)
age45*First-Born Girl	0.416	0.51	0.508	age59*First-Born Girl	0.266	-0.078	-0.131
	(0.176)**	(0.167)***	(0.225)**		(0.144)*	(0.169)	(0.174)
age46*First-Born Girl	0.218	0.319	0.263	age60*First-Born Girl	0.233	0.071	-0.073
	(0.129)*	(0.153)**	(0.154)*		(0.139)*	(0.149)	(0.171)
age47*First-Born Girl	0.212	0.299	0.377	age61*First-Born Girl	0.217	0.029	0.023
	(0.144)	(0.165)*	(0.169)**		(0.151)	(0.169)	(0.174)
age48*First-Born Girl	0.318	0.407	0.197	age62*First-Born Girl	0.385	0.009	0.022
	(0.126)**	(0.146)***	(0.161)		(0.144)***	(0.164)	(0.181)
age49*First-Born Girl	0.219	0.274	0.133	age63*First-Born Girl	0.162	0.098	0.093
	(0.129)*	(0.137)**	(0.170)		(0.166)	(0.166)	(0.193)
age50*First-Born Girl	0.449	0.419	0.336	age64*First-Born Girl	-0.148	-0.021	0.022
	(0.132)***	(0.168)**	(0.177)*		(0.176)	(0.184)	(0.174)
age51*First-Born Girl	0.351	0.126	0.209	age65*First-Born Girl	0.083	0.16	0.215
	(0.146)**	(0.166)	(0.189)		(0.189)	(0.185)	(0.214)
age52*First-Born Girl	0.423	0.656	0.054	age66*First-Born Girl	0.13	0.025	-0.033
	(0.153)***	(0.170)***	(0.174)		(0.167)	(0.187)	(0.241)
age53*First-Born Girl	0.237	0.282	0.068	age67*First-Born Girl	-0.299	0.027	-0.253
	(0.141)*	(0.161)*	(0.169)		(0.203)	(0.184)	(0.220)
age54*First-Born Girl	0.273	0.163	0.002	age68*First-Born Girl	0.029	0.294	-0.035

Table I.3 (cont'd)

	(0.142)*	(0.189)	(0.177)		(0.223)	(0.209)	(0.235)
age55*First-Born Girl	0.269	0.24	-0.063	age69*First-Born Girl	-0.114	0.088	0.046
	(0.139)*	(0.148)	(0.193)		(0.231)	(0.245)	(0.262)
age56*First-Born Girl	0.353	0.172	-0.101	age70*First-Born Girl	-0.081	0.188	0.13
	(0.133)***	(0.160)	(0.161)		(0.217)	(0.191)	(0.219)
age57*First-Born Girl	0.291	0.075	-0.037				
	(0.137)**	(0.163)	(0.175)	*Cragg-Donald Wald F*	2.59	1.44	1.72
age58*First-Born Girl	0.33	0.178	-0.08	*R-squared*	0.45	0.42	0.45
	(0.145)**	(0.174)	(0.175)	*Observations*	9,657	4,517	5,140

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age cohort dummies, gender of first birth, age at first birth, sex, education, number of siblings, marital status, self-reported health during childhood, and county fixed effects.
Col (1) is on the whole sample; Col (2) is for men only; Col (3) is for women only.

Table I.4: Coefficients of Interaction Terms for the 1st Stage Regression on Two or More Children

	(1)	(2)	(3)		(1)	(2)	(3)
age45*First-Born Girl	0.185	0.155	0.175	age54*First-Born Girl	0.118	0.129	0.061
	(0.061)***	(0.061)**	(0.044)***		(0.035)***	(0.029)***	(0.029)**
age46*First-Born Girl	0.171	0.153	0.141	age55*First-Born Girl	0.07	0.149	0.076
	(0.045)***	(0.046)***	(0.039)***		(0.031)**	(0.032)***	(0.031)**
age47*First-Born Girl	0.165	0.166	0.215	age56*First-Born Girl	0.108	0.099	0.063
	(0.036)***	(0.041)***	(0.030)***		(0.033)***	(0.032)***	(0.028)**
age48*First-Born Girl	0.217	0.222	0.163	age57*First-Born Girl	0.052	0.088	0.058
	(0.035)***	(0.032)***	(0.029)***		(0.029)*	(0.025)***	(0.027)**
age49*First-Born Girl	0.106	0.124	0.138	age58*First-Born Girl	0.035	0.054	0.052
	(0.035)***	(0.037)***	(0.027)***		-0.028	(0.034)	(0.028)*
age50*First-Born Girl	0.147	0.189	0.177	age59*First-Born Girl	0.062	0.042	0.053
	(0.038)***	(0.043)***	(0.029)***		(0.028)**	(0.031)	(0.025)**
age51*First-Born Girl	0.143	0.102	0.144	age60*First-Born Girl	0.042	0.034	0.059
	(0.042)***	(0.041)**	(0.029)***		(0.023)*	(0.024)	(0.017)***
age52*First-Born Girl	0.095	0.166	0.1	age61*First-Born Girl	0.006	0.043	0.047
	(0.041)**	(0.033)***	(0.038)***		(0.023)	(0.025)*	(0.020)**
age53*First-Born Girl	0.049	0.108	0.078	*Cragg-Donald Wald F*	2.671	1.666	1.49
	-0.035	(0.035)***	(0.036)**	*Observations*	9,462	4,406	5,056

Table I.4 (cont'd)

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age cohort dummies, gender of first birth, age at first birth, sex, education, number of siblings, marital status, self-reported health during childhood, and county fixed effects.
Col (1) is on the whole sample; Col (2) is for men only; Col (3) is for women only.
Cohorts age above 62 are used as control.

Table I.5: Coefficients of Interaction Terms for the 1st Stage Regression on Number of Children

	(1)	(2)	(3)		(1)	(2)	(3)
age45*First-Born Girl	0.39	0.46	0.489	age54*First-Born Girl	0.248	0.099	-0.019
	(0.148)***	(0.149)***	(0.197)**		(0.109)**	(0.156)	(0.113)
age46*First-Born Girl	0.191	0.267	0.243	age55*First-Born Girl	0.242	0.177	-0.085
	(0.096)**	(0.130)**	(0.104)**		(0.114)**	(0.099)*	(0.138)
age47*First-Born Girl	0.185	0.242	0.357	age56*First-Born Girl	0.327	0.106	-0.123
	(0.113)	(0.136)*	(0.118)***		(0.107)***	(0.118)	(0.103)
age48*First-Born Girl	0.291	0.349	0.177	age57*First-Born Girl	0.263	0.013	-0.06
	(0.088)***	(0.110)***	(0.103)*		(0.102)**	(0.125)	(0.118)
age49*First-Born Girl	0.194	0.215	0.113	age58*First-Born Girl	0.302	0.114	-0.102
	(0.094)**	(0.100)**	(0.112)		(0.114)***	(0.142)	(0.115)
age50*First-Born Girl	0.423	0.361	0.314	age59*First-Born Girl	0.236	-0.142	-0.152
	(0.101)***	(0.128)***	(0.116)***		(0.109)**	(0.129)	(0.122)
age51*First-Born Girl	0.323	0.063	0.186	age60*First-Born Girl	0.206	0.005	-0.093
	(0.114)***	(0.128)	(0.133)		(0.105)*	(0.112)	(0.114)
age52*First-Born Girl	0.395	0.593	0.033	age61*First-Born Girl	0.189	-0.036	0.003
	(0.123)***	(0.146)***	(0.116)		(0.122)	(0.127)	(0.126)
age53*First-Born Girl	0.211	0.219	0.049	*Cragg-Donald Wald F*	9.635	4.854	5.442
	(0.108)*	(0.127)*	(0.108)	*Observations*	9,657	4,517	5,140

Table I.5 (cont'd)

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age cohort dummies, gender of first birth, age at first birth, sex, education, number of siblings, marital status, self-reported health during childhood, and county fixed effects.
Col (1) is on the whole sample; Col (2) is for men only; Col (3) is for women only.
Cohorts age above 62 are used as control.

Table I.6: 2SLS Results for Effects of Having Two Children on Parent's Mental Health

	(1)	(2)	(3)
Having 2 Children	8.004	4.195	7.191
	(5.131)	(5.662)	(7.019)
Age at First Birth	0.217	0.167	0.155
	(0.129)*	(0.149)	(0.188)
Men	-1.816		
	(0.226)***		
Year of Schooling	-0.173	-0.276	-0.112
	(0.032)***	(0.045)***	(0.055)**
# of Siblings	0.01	0.086	-0.051
	(0.067)	(0.091)	(0.094)
Temporary Seperated	0.761	1.88	0.745
	(0.496)	(0.841)**	(0.570)
Seperated	9.569	8.566	6.978
	(2.349)***	(2.901)***	(3.565)*
Divorced	7.284	8.846	3.514
	(1.963)***	(2.197)***	(3.074)
Widowed	2.294	2.272	1.204
	(0.655)***	(1.085)**	(0.713)*
Never married	4.432	7.332	-3.546
	(3.632)	(4.841)	(4.726)
R-squared	-0.12	0.03	-0.1
Observations	4,209	1,995	2,158

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
Observations are parents with either 1 or 2 children.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) is on the whole sample; Col (2) is for men only; Col (3) is for women only.

Table I.7: OLS Estimates of Effects of Two or More Children on Parent's Mental Health by Parents' Education

	Less Than Primary School			At Least Primary School		
	(1)	(2)	(3)	(4)	(5)	(6)
# of Children>1	-0.039	-0.029	-0.112	0.216	0.055	0.325
	(0.442)	(0.837)	(0.523)	(0.275)	(0.362)	(0.478)
Age at First Birth	0.045	0.061	-0.001	0.016	0.048	-0.063
	(0.023)*	(0.038)	(0.031)	(0.029)	(0.034)	(0.060)
Men	-1.848			-1.739		
	(0.217)***			(0.174)***		
Year of Schooling	0.035	-0.035	0.065	-0.194	-0.23	-0.136
	(0.067)	(0.108)	(0.087)	(0.043)***	(0.055)***	(0.072)*
# of Siblings	-0.029	-0.022	-0.026	0.073	0.104	0.018
	(0.055)	(0.084)	(0.069)	(0.048)	(0.058)*	(0.085)
Temporary Seperated	0.797	1.508	0.54	0.41	0.81	-0.075
	(0.416)*	(0.836)*	(0.505)	(0.473)	(0.695)	(0.652)
Seperated	5.349	8.29	2.731	7.138	6.469	14.155
	(1.507)***	(1.890)***	(2.544)	(1.915)***	(2.080)***	(0.766)***
Divorced	3.65	2.118	3.409	4.925	5.785	-1.37
	(1.589)**	(3.217)	(2.146)	(1.564)***	(1.632)***	(2.258)
Widowed	1.261	2.056	1.17	1.759	1.517	2.126
	(0.358)***	(0.726)***	(0.388)***	(0.439)***	(0.655)**	(0.683)***
Never married	2.439	2.947		-0.015	0.832	
	(1.779)	(3.267)		(0.425)	(0.518)	
R-squared	0.04	0.05	0.02	0.06	0.05	0.04
Observations	4697	1464	3233	4765	2942	1823

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

Table I.8: OLS Estimates of Effects of Number of Children on Parent's Mental Health by Parents' Education

	Less Than Primary School			At Least Primary School		
	(1)	(2)	(3)	(4)	(5)	(6)
# of Children	-0.004	-0.233	0.044	0.252	0.234	0.307
	(0.082)	(0.147)	(0.106)	(0.100)**	(0.109)**	(0.189)
Age at First Birth	0.045	0.052	0	0.039	0.067	-0.015
	(0.024)*	(0.040)	(0.032)	(0.028)	(0.034)**	(0.059)
Men	-1.836			-1.778		
	(0.214)***			(0.172)***		
Year of Schooling	0.036	-0.014	0.059	-0.193	-0.232	-0.121
	(0.067)	(0.107)	(0.086)	(0.042)***	(0.054)***	(0.070)*
# of Siblings	-0.042	-0.02	-0.041	0.078	0.119	-0.022
	(0.054)	(0.083)	(0.068)	(0.047)*	(0.057)**	(0.086)
Temporary Seperated	0.708	1.234	0.491	0.29	0.632	-0.214
	(0.413)*	(0.846)	(0.499)	(0.464)	(0.679)	(0.652)
Seperated	5.046	7.399	2.709	4.685	4.489	5.978
	(1.481)***	(1.950)***	(2.552)	(1.876)**	(2.097)**	(5.853)
Divorced	3.63	1.881	3.503	5.012	5.886	-1.237
	(1.572)**	(3.177)	(2.147)	(1.542)***	(1.614)***	(2.234)
Widowed	1.264	1.986	1.141	1.779	1.579	2.056
	(0.353)***	(0.700)***	(0.388)***	(0.434)***	(0.645)**	(0.675)***
Never married	0.938	0.094	1.856	2.854	3.188	
	(1.158)	(1.520)	(2.106)	(3.730)	(3.414)	
R-squared	0.04	0.05	0.02	0.06	0.05	0.04
Observations	4,792	1,509	3,283	4,865	3,008	1,857

Notes: Standard errors clustered at county level are reported in brackets.
*significant at 10% level; **significant at 5% level; ***significant at 1% level.
All regressions controls age, age square, self-reported health during childhood, and county fixed effects.
Col (1) and Col (4) are on the whole sample; Col (2) and Col (5) are for men only; Col (3) and Col (6) are for women only.

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