The Effect of China's One Child Policy on Sex Selection, Family Size and the School Enrollment of Daughters

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Abstract

I first document that the introduction of the One Child Policy dramatically increased sex-selection in certain regions, and that the Chinese government responded to this by allowing parents who had a daughter as their first child to try for a second child. Next, I show that the increase in family size caused by this relaxation in the One Child Policy *increased* school enrollment of first-born daughters. The analysis provides suggestive evidence that economies of scale in child rearing and short-term income demands contribute to the main results.

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1 Introduction

The effect of family size on child quality is a question of longstanding interest for economists. The effect is *a priori* ambiguous. On the one hand, a large literature in economics provides evidence that parents trade off the quantity of children with the quality of children, which implies that the quality of children declines as family size increases (e.g., Becker and Lewis, 1973; Becker and Tomes, 1976).[1] On the other hand, child psychologists such as Iacovou (2001) and Zajonc (1982) emphasize social interaction and learning-by-doing. They argue that increases in the number of children can increase the quality of children because it provides children opportunities to teach and learn from each other.[2] Alternatively, there may simply be economies of scale in costs for childcare for items such as clothes and textbooks such that an additional child lowers the marginal cost of quality for all children. In the rural Chinese context, this can be seen in Table 1A, which shows that average per child expenditures on household chores and child care for rural Chinese households decrease significantly with the number of children.

For policy makers in developing countries today, understanding this relationship is especially relevant as many governments have attempted to curb population growth as a way of increasing average human capital investment. Both China and India, the world's two most populous countries, have experimented with different family planning policies to limit family size. This study addresses the effect of family

[1]The textbook quantity-quality tradeoff argues that as women's wages rise, the cost of having children increases, and hence, parents will have fewer children. These models assume that parents equalize investment across children. Thus, reducing the number of children will naturally increase the *average* quality of children. The classic quality-quality model does not allow for differences across children.

[2]Iacovou (2001), a child psychologist, argues that the disadvantage could be because children benefit from social interactions with other children. Using detailed data on time use of children in the U.K., she finds that the one-child disadvantage decreases with the amount of time a child spends playing with other children after school. In the learning-by-doing discussed in Zajonc (1982), older children are predicted to benefit more from having additional siblings than the youngest child because it is assumed that children teach younger children and benefit especially from teaching.

size by examining the effect of increasing household size from one to two on school enrollment in rural China. To establish causality, I exploit region and birth year variation in relaxations of the One Child Policy.

There are two main difficulties. First, there is the possibility of parental heterogeneity. For example, if parents who value education more also prefer to have fewer children, then the correlation between quantity and quality will over-estimate potentially negative effects of family size. Endogeneity may also arise from the quality of the first child. For example, if parents are more likely to have a second child when the first child is of high quality, the correlational evidence will under-estimate the potentially negative effects of family size. To address these issues, past studies have carefully constructed strategies that exploit the exogenous variation in family size caused by multiple births or the sex composition of the first two children (Angrist, Lavy and Schlosser, 2010; Black et. al., 2006; Conley, 2004; Lee, 2003; Rosenzweig and Wolpin, 1980; Rosenzweig and Zhang, 2009).[3] While previous works provide important evidence, the strategies they employ cannot be applied to all contexts. Specifically, the estimates from using the sibling-sex composition instrument is most suitable for studying the effect of increasing the number of children from two to three, and cannot be an excludable instrument if parents practice sex-selection. Estimates from using the twins instrument can lack external validity to non-twin children.

The principal contribution of this paper is to address these problems and estimate the effect of increasing the number of children from one to two. I exploit regional and time variation in the relaxations of China's One Child Policy (OCP).

[3]The sibling sex composition methodology argues that parents prefer children of mixed sex. Therefore, they are more likely to have a third child if the first two are of the same sex. The twins methodology argues that the occurrence of twins (before the introduction of fertility treatments) is uncorrelated to individual characteristics. Hence, twinning is a plausibly exogenous source of variation for family size. Both methodologies examine the effect of an additional sibling for families with at least two children. Angrist, Lavy and Schlepper (2005, 2006) used both techniques and found that the results are similar.

I use the relaxation that allowed families to have a second child if the first child is a girl to instrument for the family size of first-parity children born before the relaxation was announced. Three facts are exploited: first, an individual is only affected by the relaxation if she is born in a relaxed area; second, amongst first-born children born in relaxed areas, only girls are affected; and third, a girl is more likely to gain a sibling due to the relaxation if she is younger at the time of the policy announcement. The instrument for family size is the triple interaction term of an individual's sex, date of birth and region of birth. The interaction between whether a girl was born in a relaxed area and her birth year estimates the effect of the relaxation on family size. The additional comparison with boys controls for region-specific changes in school provision (and other cohort changes) that affected boys and girls similarly. This strategy differs from previous methods in that it essentially compares one-child households with two-child households. Interpreting the 2SLS estimates as causal assumes that absent the introduction of the relaxation, the *difference* between households with first-born daughters and those with sons would have moved along parallel trends for villages that received the relaxation and villages that did not. This is the standard parallel trends assumption applied to a triple difference setting. I do not take this assumption as given and will carefully consider and provide evidence against potential caveats in the robustness section.

The OLS estimates show that for households with three or fewer children, the number of siblings is negatively correlated with school enrollment. However, for households with two or fewer children, having a younger sibling is positively correlated with the school enrollment of the eldest child. This is consistent with the descriptive evidence which shows that only children are, on average, less likely to be enrolled in school relative to children from two-child families, who are, on average, more likely to be enrolled in school relative to children from three child families.

The 2SLS results show that for households with two or fewer children, an additional child significantly increases the school enrollment of the first child by up to sixteen percentage points. The fact that the 2SLS estimate is larger in magnitude than the OLS estimate is consistent with the existence of parental heterogeneity in preferences for education and quality.

The main results show that there is a significant one-child disadvantage for the eldest child, which is consistent with the belief that children benefit from teaching younger siblings, and also with the possibility that there may be economies of scale in raising children. They do not unambiguously reject the Beckerian quantity-quality tradeoff model since that model makes predictions about the average outcomes of children, and I can only examine the outcomes for the eldest child. However, for quantity to have no average effect on quality given my findings for the eldest child, there would have to be inequality across children, which would also be inconsistent with a simple Beckerian case.

In addition to the main results, we attempt to investigate the mechanisms underlying them. First, I investigate the hypothesis that the positive effect of an additional child is driven by economies of scale in child rearing costs. Under the assumption that there are larger economies of scale school for children of the same sex (e.g., children can more easily share clothes if they are the same sex), I explore this hypothesis by examining whether the benefit of the second child is larger when the two children are the same sex. The results show that the benefits of a second child are almost entirely driven by households where the two children are of the same sex. This is consistent with the presence of economies of scale.

Second, I examine the hypothesis that the benefit of an additional child is driven by an increase in permanent income. For example, if adult children provide parents with income, then an additional child will increase the permanent income of the household. If parents can borrow against their children's future income, this could increase investment in schooling. Under the assumption that parents expect sons to earn more than daughters, I test this hypothesis by investigating if the benefits of a second child are larger when the second child is a boy. The results show the opposite pattern: the benefits are larger when the second child is a girl. Therefore, our results do not seem to be driven by increases in permanent income, which is perhaps not surprising since households in rural China are generally believed to be credit constrained. Note that because the sex of the second child is not random due to sex selection, these results should be interpreted very cautiously as only suggestive evidence.

Finally, I investigate the possibility that the main results occur due to binding income constraints. If the financial costs imposed by an additional child outweighs schooling costs, then parents pay increase their labor supply in the labor market and substitute public schools for self-provided child care. The data limits the extent to which I can investigate this hypothesis. I examine the effect of an additional child on mother's labor supply and school delay. The results are suggestive, but imprecisely estimated. They suggest that an additional child causes mothers to be *more* likely to enter the labor force and causes the elder child to enter school at younger ages. These results are consistent with the hypothesis that income demand caused by the additional child causes mothers to work and the first child into school. Interestingly, they suggest the possibility that public schools are being used as a form of low-cost childcare. This is important because it implies that classic frameworks for understanding the relationship between family size and children's schooling may be inadequate for contexts where schooling costs are low and can be used as a form of subsidized childcare by parents.

This study makes several contributions. First, it adds to the existing literature on the effects of family size. The results from this literature has been mixed.[4] Most of theses studies have focused on the effect of additional children conditional on there already being two children. I add to these studies by being the first to provide evidence for the one-child disadvantage (at least for the eldest child), which suggests that the effect of family size may be non-monotonic across family size. The finding that additional children benefit the schooling outcomes of the eldest child is similar to Angrist et. al.'s (2010) finding for Israel. The implication that the effects of family size may differ across birth orders supports the findings of Black et al. (2006) for Norway.

Second, this study provides an evaluation of the effects of the OCP, one of the most restrictive and large scale family planning policies ever undertaken. While demographers and sociologists have conducted many descriptive studies of the policy's impact on fertility and sex ratios, the lack of local enforcement data has heretofore prevented an examination of the causal effect of the OCP on child outcomes. The findings indicate that the OCP decreased female survival by up to ten percentage-points, and the relaxation was successful in reducing the sex selection to pre-OCP levels. Interestingly, the results also show that the previous rule on four-year birth spacing was well enforced, a fact that has received little attention from policy debates or academic studies. In rigorously evaluating the effects of the OCP, the first stage of this paper is closely related to a recent study by Ebenstein (2010), which uses Chinese Census data to show that regional sex ratios are closely linked to the level

[4] On the one hand, studies have found family size to have no effect or even a positive effect on child outcomes in Israel (Angrist, Lavy and Schlepper, 2005, 2006), Korea (Lee, 2003), the U.S. (Kessler, 1993), China (Guo and VanWey, 1991) and Africa (Gomes, 1984). On the other hand, the effect of family size on education has been found to be negative in the India (Rosenzweig and Woolen, 1980), France (Goux and Maurin, 2004), the U.S. (Conley, 2004; Berhman et. al., 1989; and Stafford, 1987), and China (Rosenzweig and Zhang, 2009).The studies described here all focus on cross sectional evidence. Alternatively, Bleakely and Lange (2005) examines time-series evidence in the American South. They find that increased schooling caused a decrease in fertility. See Schultz (2005) for a detailed critique of the empirical literature.

of fines for violating the OCP. More generally, this study adds to the large literature on the effects of family-planning-policy-induced fertility. Recent examples from the Chinese context include Dasgupta et. al. (2011), which study the effect of reduced fertility on the marriage market. Similarly, Banerjee et. al. (2010) examines the effect of reduced fertility on savings in urban China. Since family planning policies both reduced fertility and exacerbated the boy-biased sex imbalance in China, my work is also related to a recent study on savings by Wei and Zhang (2011).

Finally, the suggestive results on mother's labor supply add to studies on the relationship between subsidized childcare and mother's labor supply. For a recent example, see Baker et. al.'s (2008) study of Quebec, where they find that universal child care significantly increase maternal labor supply. In a developing context, a recent study by Schlosser (2011) finds that subsidized pre-school increases the labor supply of Arab mothers in Israel.[5]

The paper is organized as follows. Section 2 discusses family planning policies and education in rural China. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 presents the empirical results. Section 6 offers concluding remarks.

2 Background

2.1 Family Planning Policies

In the 1970s, after two decades of explicitly encouraging population growth, policy makers in China enacted a series of measures to curb population growth. The policies applied to individuals of Han ethnicity, who make up 92% of China's population. Beginning around 1972, the policy "Later [age], longer [the spacing of births], fewer [number of children]" offered economic incentives to parents who spaced the birth

[5] Blau and Currie (2006) provides an overview of this literature.

of their children at least four years apart. The One Child Policy was formally announced in 1979. Actual implementation began in certain regions as early as 1978, and enforcement gradually tightened across the country until it was firmly in place in 1980 (Croll et. al., 1985; Banister, 1987).[6] Second births became forbidden except under extenuating circumstances. Local cadres were given economic incentives to suppress fertility rates. In the early 1980s, parts of the country were swept by campaigns of forced abortion and sterilization and reports of female infanticide became widespread (Greenlaugh, 1986; Banister, 1987).

Local governments began issuing permits for a second child as early as 1982. However, permits for a second child were not made widespread until the Central Party Committee issued "Document 7" on April 13, 1984. The two main purposes of the document were to: 1) curb female infanticide, forced abortion and forced sterilization; and 2) devolve responsibility from the central government to the local and provincial government so that local conditions could be better addressed. In other words, it allowed for regional variation in family planning polices. The document allowed for second births for rural couples with "practical" difficulties, and strictly prohibited coercive methods.[7] The main relaxation following *Document No. 7* is called the "1-son-2-child" rule. It allowed rural couples to have a second child if the first child was a girl (Greenlaugh, 1986). The explicit purpose of this relaxation was to decrease female infanticide of the first-born child.

White (1992) found that 5% of rural households were allotted second child permits in 1982. These permits were generally granted to regions with extremely high levels of infanticide. After Document 7, the permits expanded to 10% of the rural

[6] Past studies generally consider the One Child Policy to have only affected the family size of cohorts born after 1979/80. However, this paper will show that because of the previous four year birth spacing rule, the One Child Policy affected cohorts born in 1976 and after.

[7] Practical difficulties included households where a parent or first-born child was handicapped, or if a parent was engaged in a dangerous industry (e.g., mining).

population in 1984, 20% in 1985 and 50% by 1986.

Document No. 7 made provincial governments responsible for both maintaining low fertility rates and decreasing infanticide. While the exact process of granting permits is unclear, I use county level data on family planning policy to show in the next section that the probability for a county to obtain the 1-son-2-child relaxation is positively correlated with the rate of pre-relaxation sex-selection, and both are positively correlated with distance from the provincial capital. These facts most likely reflect that in order to maintain low aggregate fertility rates and decrease excess female mortality (EFM), provincial governments granted relaxations to regions that were distant from the administrative capital and where EFM was more prevalent. The higher prevalence of sex-selection in rural areas can be due to both more boy-preference in distant rural areas and the fact that geographic distance increases the provincial government's logistical difficulty of preventing EFM.[8] Issues of identification that arise from the correlation of obtaining a relaxation and sex-selection will be addressed explicitly in the section on robustness.

2.2 Rural Education

Rural primary schools are exclusively provided by the state in the period of this study. Relative to other developing countries, the cost of schools were very low. Nevertheless, during the time period of this study, there was much inequality in provision across regions – both across provinces and across counties within a province. This was a result in fiscal reforms that occured during early 1980s. The fiscal system reduced subsidies from rich regions to poor regions. The system of "eating from separate pots" (*fen zao chi fan*) devolved expenditure responsibilities from the central

[8] Levels of income between counties with some relaxation and counties with no relaxation are comparable in the CHNS data. This is consistent with the findings of Qian's (2008) study of rural China, which found that total household income had no effect on sex selection.

and provincial governments onto local governments in order to give the latter stronger incentives to generate revenue. The ratio of the per capita schooling expenditure in the highest spending province to the lowest spending province doubled in one decade.

Many rural schools were closed; rural enrollment rates dropped dramatically and did not recover until the mid to late 1990s (Hannum and Park, mimeo). Using spending data from Gansu province, they found that per capita school expenditure was positively correlated with income and that significant variation in school quality existed across counties. They found little variation within counties, suggesting that studies examining education outcomes should focus on variation at the county level.

Hannum (1992) showed that the difference in school provision between rich and poor areas was much greater for middle schools and high schools than for primary schools. This is consistent with the CHNS data used in this study, where primary school enrollment remained stable while middle school and high school dropout rates increased for poor areas (Hannum and Park, mimeo).

The CHNS data show that counties with some relaxation and counties with no relaxation had similar geographic access to schooling in 1989. However, the data does not reveal quality of schooling or the changes in school availability during the early 1980s. Because relaxed areas tend to be more rural, it is likely that the quality of schools declined in relaxed areas during the same time that the 1-son-2-child relaxation took effect. To control for this, I will compare outcomes for girls to boys within counties. The strategy is robust as long as the changes in school quality and the economic conditions that determine school quality in relaxed areas have the same impact on both boys and girls.

3 Data

This paper matches the 1% sample of the 1990 *Population Census* with the 1989 *China Health and Nutritional Survey* (CHNS) at the county level. The 1990 *Population Census* contains 52 variables including birth year, region of residence, whether an individual currently lives in his/her region of birth, sex, and relationship to the head of the household. The data allows children to be linked to parents. Thus, family size and birth order of children within a household can be calculated. Because the identification is partially derived from the region of birth, the sample is restricted to individuals who reported living in their birth place in 1990. The CHNS uses a random cluster process to draw a sample of approximately 3,800 households with a total of 16,000 individuals in eight provinces that vary substantially in geography, economic development, public resources, and health indicators. Most importantly, the survey provides detailed village and township level information on family planning policy enforcement. Since ethnic minorities were exempt from all family planning policies, I restrict the analysis to four provinces which are mostly composed of individuals of Han ethnicity. The matched dataset contains 21 counties in four provinces.[9] These provinces are in the middle and upper ranges of GDP and GDP growth during this period. The CHNS data is aggregated to the county level in order to be matched to the 1990 Census. Since the policy data is at the village and township level, the aggregated data set reports the percentage of the population in each county that is exposed to the relaxation.

For the analysis of family size and education, the sample is restricted to first-born children in cohorts born during 1962-1981. The reference group in the regression analysis is comprised of individuals born during 1962-1972. Those born after 1981 are excluded because after the relaxation, parents who preferred larger families may

[9]Liaoning, Jiangsu, Shandong and Henan.

have chosen to keep girls in order to have a second child so that the 2SLS estimate without excluding those born after 1981 will be biased by parental preferences and show that girls with larger family sizes are better off.

Panels A and B in Table 1B show that amongst first-born children, girls, on average, have more siblings, more educated parents and higher school enrollment. Panels C and D show that only children are more likely to be male, have more educated parents and are more likely to be enrolled in school. This is consistent with the identification concern that parents with more education may prefer to have fewer children and value education more.

To use individuals in counties without relaxations as a control group for individuals in counties with relaxations, I would like the two groups to have similar characteristics in every respect other than the relaxation. Table 1C compares first-born children born in counties with no relaxation and first-born children born in counties with some relaxation. It shows that the two types of counties have similar demographic characteristics. Each has approximately 55% males amongst first-born children. Family size and sex composition of siblings are similar. Children born in relaxed areas have slightly more educated parents. School enrollment in both counties with and counties without relaxations are approximately 50%. Panel B shows that in counties with some relaxations, 38% of first-born children are born in villages or townships with the relaxation. Counties with relaxations are further away from urban municipalities.

The treatment group comprises children who are nine to fourteen years old in 1990. In principle, they should be enrolled in primary school or junior high school. The control group comprises children who, in principle, should be in high school. The descriptive statistics show that children in counties with the relaxation must, on average, travel further to attend primary school. This biases against my finding a positive effect of the relaxation on school enrollment. The distances to middle schools and high schools are very similar between counties with and without the relaxation.

One potential concern with Chinese data on children is the fear that parents will misreport the number of children in order to evade the One Child Policy. Past studies have compared hospital birth records and population census data to show that misreporting is typically a problem for children under two years old and the data for older children are typically accurate (Zeng et. al., 1993). Since I use data from 1990 to study children born close before and after 1976 (who were around fourteen years old by the time the data was collected), misreporting should not affect my study.

4 Empirical Strategy

Figure 1 plots the total number of children against the birth year of the first-born child. It shows that children born in more recent years have smaller family sizes. This reflects both the fact that parents of young children may not have finished having children and a decrease in family size over time. To reveal the commonly seen OLS evidence for the quantity-quality trade-off, I regress a dummy variable for school enrollment on dummy variables for the number of children in a household. Children from one-child households are the reference group. Figure 2A plots the coefficients. It shows that family size is negatively correlated with school enrollment regardless of whether county fixed effects are controlled for. However, this confounds the family size effect with several factors: 1) younger children are more likely to be in school; 2) younger children will have fewer siblings because their parents may not have finished having children; and 3) quantity and quality may be jointly determined by parental preferences. Controlling for birth years addresses the first two problems and causes the relationship between family size and school enrollment to become non-monotonic. Figure 2B plots the coefficients for family size when controlling for birth year fixed effects.[10] Relative to the reference group of children from one-child families, children from two-child families have higher school enrollment. However, the correlation between enrollment and family size is negative for households with two to five children.

The main second stage equation will control for birth county and birth year fixed effects. It can be written as the following.

$$enroll_{itc} = sibs_{itc}b + X'_{ict}\kappa + \sum_{l=1973}^{1981}(urban_c \times d_{il})\delta_l + \alpha + \gamma_t + \psi_c + \varepsilon_{itc} \quad (1)$$

School enrollment for individual i, born in county c, birth year t, is a function of: $sibs_{itc}$, the number of siblings he or she has; X_{ict}, individual characteristics; the interaction term between $urban_c$, distance to urban area, and d_l, a variable indicating whether an individual was born in year l; γ_t, birth year fixed effects; and ψ_c, county fixed effects.

This faces the problem that the number of children and investment in these children are jointly determined by parents. Hence, if parents who value education also prefer smaller households, then OLS will over-estimate the negative effect of an additional sibling on schooling. I address this by exploiting plausibly exogenous variation in family size caused by relaxations in the One Child Policy. Sex, date and region of birth jointly determine an individual's exposure to the 1-son-2-child relaxation. The relaxation allowed parents to have a second child only if the first-born child was a girl. Therefore, family size should be positively correlated with being a girl. Since parents are more likely to have a second child if the first girl was younger when the relaxation was announced, family size should be negatively

[10] Estimates for the coefficients plotted in Figures 2A and 2B are shown in Appendix Table A1.

correlated with the age of the first girl. The interaction between whether a girl was born in a relaxed area and her age estimates the effect of the relaxation on family size. The additional comparison with boys controls for changes in policies such as education provision that affected both boys and girls similarly. The instrument for family size is therefore the triple interaction of an individual's sex, year of birth and region of birth. Only the combination of the three is exogenous. The exclusion restriction for the instrument is that it must be correlated with family size and only affect school enrollment via the family size channel.

To understand the identification strategy, I first estimate the effect of the policy on family size for boys and girls separately. If the policy was fully enforced, it should increase the number of siblings for first-born girls for whom the One Child Policy prevented from having younger siblings. The relaxation should have no effect on the family size of boys. I estimate the following equation separately for samples of first born boys and girls born during 1962-1981.

$$sibs_{itc} = \sum_{l=1973}^{1981} (relax_c \times d_{il})\beta_l + \gamma_t + \alpha + \psi_c + \upsilon_{itc} \tag{2}$$

The number of siblings for individual i, born in county c, birth year t, is a function of: the interaction term of $relax_c$, the extent of relaxation in county c and d_{il}, a dummy indicating whether the individual was born in year l; γ_t, birth year fixed effects and ψ_c, county fixed effects. The reference group is comprised of individuals born during 1962-1972. It and all of its interaction terms are dropped. For all regressions, standard errors are clustered at the county level.

Then, to assess the statistical difference of the effect on boys and girls, I pool the data to estimate the first stage equation with the triple interaction terms on the right hand side.

$$
\begin{aligned}
sibs_{itc} = &\sum_{l=1973}^{1981} (relax_c \times girl_{itc} \times d_{il})\beta_l + \sum_{l=1973}^{1981} (relax_c \times d_{il})\delta_l \\
&+ \sum_{l=1973}^{1981} (girl_{itc} \times d_{il})\zeta_l + (relax_c \times girl_{itc})\lambda + girl_{itc}\kappa \\
&+ \alpha + \gamma_t + \psi_c + v_{itc}
\end{aligned}
\tag{3}
$$

The number of siblings for individual i, born in county c, birth year t, is a function of: the triple interaction term of $relax_c$, the extent of relaxation in county c, $girl_{itc}$, a variable indicating whether a child is a girl and d_{il}, a dummy variable indicating whether the individual was born in year l; the interaction term of $relax_c$ and d_{il}; the interaction term between $girl_{itc}$, and d_{il}; the interaction term between $relax_c$ and $girl_{itc}$; $girl_{itc}$; γ_t, birth year fixed effects; and ψ_c, county fixed effects. As before, the reference group of cohorts born 1962-1972 and all its interactions are dropped. β_l is the difference in the effect of being born in a relaxed area on family size between girls and boys born in year l. The estimates should be zero for earlier cohorts who were not affected by the One Child Policy and relaxation and positive for later affected cohorts. β_l is the effect of being born in a relaxed county on family size for an individual born in year l.

Like simple differences-in-differences estimators, cohort-invariant differences across regions are differenced out by the comparison across cohorts. Changes across cohorts which affect different regions similarly are differenced out by the comparison across regions. The triple difference adds the advantage that cohort-varying differences that affect boys and girls similarly across regions are also differenced out by the comparison between girls and boys within each cohort and region. The exclusion restriction is only violated if a change with differential impacts on areas with and without the relaxation *and* on boys and girls occurs at the same time the relaxation took effect. In other words, the 2SLS estimate will be biased only if there is a sex-specific change at the time of the relaxation in relaxed regions. For example, if local governments of relaxed regions implemented a program encouraging girls to attend school when the relaxation was enacted, then the 2SLS will confound the effects of this program with the effects of family size. There is little reason to think that such a change occurred.

The main concern with this strategy arises from the fact that the relaxations were introduced to curb sex selection. If the relaxation is strongly correlated with the extent of sex-selection for One Child Policy cohorts, two potential problems will arise. First, unobserved factors correlated with sex-selection may affect education investment differentially for boys and girls. This will bias the estimates if the factors driving sex-selection are time varying.[11] Second, there might be selection bias regarding the parents who choose to keep girls in relaxed regions. The main concern is that parents of girls in relaxed regions could have different unobservable characteristics from parents of girls in regions without the relaxation in such a way that would bias the 2SLS estimates upwards. For example, parents of girls in relaxed regions may, on average, have a higher consumption value for all things related to children, such as education relative to parents of girls in non-relaxed regions. Then, the 2SLS estimate will overestimate the true effect of family size on school enrollment. I investigate this by first examining the effect of the relaxation on the fraction of males by birth year using the following equation.

[11] For example, Qian (2008) finds that increasing relative adult male wages increases sex selection and that increasing relative adult male wages decrease girls' schooling relative to boys. This would cause a downward bias in the 2SLS estimates.

The CHNS does not have accurate data on individual income within the household since much of rural production is conducted at the household level and income cannot be accurately assigned to individual members. Consequently, I cannot directly examine the role of relative earnings in this study.

$$male_{itc} = \sum_{l=1969}^{1989} (relax_c \times d_{il})\beta_l + \gamma_t + \alpha + \psi_c + \upsilon_{itc} \tag{4}$$

The probability of being male for individual i, born in county c, birth year t is a function of: the interaction terms between $relax_c$, and birth year dummy variables, d_{il}; birth county fixed effects, ψ_c; and birth year fixed effects, γ_t. β_l is the correlation between being born in a relaxed county and the sex ratios of your cohort for each birth year l.

Then, to estimate the magnitude of the effect of the relaxation on sex ratios, I estimate the following equation using the sample of first-born children.

$$male_{itc} = \sum_{l=2}^{3} (relax_c \times post_{il})\delta_l + \alpha + \gamma_t + \psi_c + \varepsilon_{itc} \tag{5}$$

The probability of being male for individual i, born in county c, birth year t is a function of: the interaction term between $relax_c$, and $post_{il}$, a variable indicating the individual's cohort group; ψ_c, county fixed effects and γ_t, cohort group fixed effect. In the section on robustness, I will use the estimate of δ_l to compute bounds for the main results. The children are divided into three groups according to birth cohort. The reference group is comprised of individuals not affected by the One Child policy and the relaxation (born before 1978). The second group is comprised of children born after the One Child Policy but before the relaxation (1978-1981). The third group is comprised of children born after the relaxation (1982-1989). $\hat{\delta}_l$ is the effect of the One Child Policy on sex selection in relaxed areas relative to areas without the relaxation. For robustness, I use it to calculate the extent to which the main results can be driven by selection under certain assumptions.

5 Empirical Results

5.1 The Correlation between Family Size and Schooling

Panels A and B of Table 2 show the estimates from equation (1). All regressions control for the full set of double interaction terms from equation (3).[12] Panel A shows that among households with 3 or fewer children, an additional sibling is negatively correlated with the school enrollment of the first child by 1.1 percentage points. However, since the 2SLS will reveal the effect of increasing the number of children from one to two, the relevant OLS comparison should be on a sample of individuals with one or no sibling. Panel B shows that in this restricted sample, an additional sibling is *positively* correlated with the school enrollment of the eldest child by approximately 1.5 percentage-points. Estimates in both cases are statistically significant at the 1% significance level and robust to controls.

Note that the number of observations change slightly across the different estimates of the analysis because the control variables are not always available for the full sample. In the paper, I present results using the largest possible sample. All of the results are nearly identical when the estimates are repeated on a restricted sample where all controls are available for all observations. These results are not reported for brevity and are available upon request.

5.2 The Effect of the 1-Son-2-Child Relaxation on Family Size

I first estimate equation (2) on separate samples for boys and girls. The estimates are shown in Table 3, columns (1) and (2). The estimates for girls are statistically significant at the 1% level for individuals born 1976 and later. This is consistent with

[12]The double interactions include the interaction term of $relax_c$ and d_{il}; the interaction term of $girl_{itc}$ and d_{il};the interaction term of $relax_c$ and $girl_i$; and $girl_{itc}$. The reference group is comprised of cohorts born during 1962-1972. The dummy variable for the reference cohort and all its interactions are dropped.

the fact that before the One Child Policy was introduced in 1979/1980, there was a four-year birth spacing law. Hence, the One Child Policy was binding for cohorts born four years previous to its introduction. The estimates for boys are statistically insignificant. The coefficients are plotted in Figure 3A. It shows that family size for boys and girls were similar for cohorts born 1973-1976, after which the family size for girls increased and the family size for boys remained the same.

The estimated coefficients for the triple interaction terms from equation (3) are shown in Table 3, column (5). They are statistically significant at the 5% level for the individuals born 1977-1981. Figure 3B plots the coefficients for the triple interaction term. It shows that the boy-girl difference in the effect of being born in a relaxed area on family size is zero for unaffected cohorts and positive for the affected cohorts. On average, the relaxation increased family size of first-born girls by approximately 0.25 children. The discrete change in the effect of the triple interaction term between individuals born before 1976 and those born afterwards is consistent with the claim that the One Child Policy was binding for cohorts born four years prior to its enactment. This is evidence for the effective enforcement of the previous four-year birth spacing.

5.3 The Effect of the 1-Son-2-Child Relaxation on Enrollment

I first estimate the effect of the relaxation on enrollment separately for boys and girls using an equation identical to equation (2) but replacing the dependent variable with enrollment, the outcome of interest. The reference group is comprised of individuals born during 1962-1972. The coefficients for girls and boys are shown in Table 3, columns (3) and (4). The estimates are statistically significant for girls. Figure 4A plots the estimates for boys and girls. The plot of the reduced form shows that girls affected by the relaxation (born 1976 and after) had higher education enrollment than boys, whereas girls unaffected by the relaxation (born before 1976) had lower school enrollment rates than boys.

The estimates in Figure 4A show that, relative to areas without the relaxation, enrollment for both boys and girls decreased after primary school. This is consistent with the hypothesis that school provision and quality in relaxed regions relative to regions without the relaxation declined during this period. I control for this by comparing the effect of the relaxation on enrollment for boys with the effect of the relaxation on enrollment for girls. I estimate an equation similar to equation (3) with school enrollment as the dependent variable. The reference group is comprised of individuals born during 1962-1972. The coefficients are shown in Table 3, column (6). The estimates show that for older cohorts not affected by the relaxation, individuals born in relaxed areas had on average 1% to 17% less school enrollment than areas without the relaxation. However, for cohorts affected by the relaxation, individuals born in relaxed areas were on average enrolled in school 5% more than individuals born in areas without the relaxation. The estimates are statistically significant at the 1% level. Figure 4B plots the triple difference reduced form estimates. It shows that school enrollment in relaxed areas was higher for girls of the affected cohort than for boys. Note that the year-by-year first stage and reduced form estimates use the full sample. Estimates for a sample restricted to households with three or fewer children are presented in Appendix Table A2.

5.4 The Effect of Family Size on Enrollment

Table 2 Panel C show the 2SLS estimates for households with three or fewer children. It shows that for a sample where 49% of individuals were enrolled in school, an additional sibling increased enrollment of the first child by approximately fourteen to sixteen percentage-points. The estimates are statistically significant at the 1%

level. Panel D restricts the sample to households with two or fewer children. The estimates show that for a sample where 54% of individuals were enrolled in school, an additional sibling increased enrollment of the first child by approximately twelve percentage-points. The estimates are mostly statistically significant at the 10% level.

5.5 Robustness

The main results show that eldest children with younger siblings are more likely to be enrolled in school than those without younger siblings. In this section, I consider and provide evidence against the concern that the instrument affects the school enrollment of the first child through channels other than family size (i.e., the exclusion restriction is violated).

5.5.1 Family Composition

Using the triple difference as an instrument for family size requires that the instrument does not affect any right-hand-side variable other than family size. However, if the relaxation also changed the sex composition of children in families of the affected cohort, then the 2SLS estimate will be confounded. I can explore this possibility by estimating equation (4) for the sex of second and higher parity births. If the percentage of males born after the relaxation was introduced in 1982 is similar between regions that received the relaxation and those that did not, then one will be less concerned about a change in composition. The coefficients and standard errors for second born children are shown in Table 4 columns (3) and (4). For third and higher parity births, they are shown in columns (5) and (6). They and their 95% confidence intervals are plotted in Figures 5B-5C, which show that the One Child Policy and subsequent relaxations did not affect sex ratios of higher order births in relaxed counties relative to counties without relaxations. Thus, the relaxation did not affect the sex composition of children.

I discuss Figure 5A when I discuss selection.

5.5.2 Marriage Market

One possible concern for the empirical strategy is that the instrument affected enrollment through channels apart from family size. In particular, the results may be driven by marriage market channels. The intensive boy-biased sex selection in regions that received the relaxation may have increased the value of girls in these regions. This may have a direct positive effect on the investment in girls' education apart from family size effects if there are positive returns to education on the marriage market. However, the increase in the value of girls in relaxed areas should also have resulted in an increase in female survival. Figures 5A-5C show that female survival in relaxed regions did not improve for any birth parity after the relaxation. This is inconsistent with the marriage market explanation.

5.5.3 Selection

Figure 5A plot the coefficients for the correlation between sex of first parity births and whether a region obtained a relaxation for each birth cohort (the coefficients and standard errors are shown in Table 4 columns (1)-(2)). It shows that sex ratios are higher for first-born children in regions that received the relaxation in the years leading up to the relaxation. This is consistent with the fact that the relaxation was motivated by the desire to curb son-biased sex-selection. The implication that the relaxation was more likely to be implemented in regions where parents had stronger preferences for boys raises the concern that parents who choose to have girls are in counties with the relaxation are different from parents who choose to have girls in counties without the relaxation on average. Specifically, if parents who chose to keep girls born under the One Child Policy in relaxed counties valued education more than parents who kept girls in counties without the relaxation, the 2SLS estimate will over-state the true effect of family size on school enrollment. This problem is partially addressed in the main estimation by excluding cohorts born after the relaxation (1982-1990), which has the advantage of excluding households that kept girls in order to have a second child. Thus, selection only concerns girls born during 1979-81 (after the One Child Policy was introduced, but before the relaxations were introduced). It is easy to see from Figures 3A, 3B, 4A and 4B that the magnitude of my main results will be similar if I exclude individuals born during 1979-81, the latter half of my treatment sample. Thus, it is highly unlikely that my main results are driven by sex-selection. I also address selection by constructing an alternative sample that removes selection for the 1979-81 cohorts to estimate the lower bound of the absolute value of the family size effect. Since this correction only affects 10% of the observations for half of the treatment group birth cohorts (e.g., Figure 5A shows that the 1979-81 cohort in relaxed regions have approximately ten percentage-points more girls), it makes little difference to the main results.[13]

5.6 Mechanisms

A second child can increase school enrollment of the first child for several reasons. Here, I consider some of the most obvious hypotheses. First, there could be economies of scale in schooling costs. These could include costs related to textbooks, school fees, clothes or food for school. Unfortunately, the data does not allow me to examine these costs directly. However, under the assumption that economies are larger when children are of the same sex, I investigate this hypothesis by examining whether the benefit of an additional child is larger when the two children are

[13] The results are not reported for brevity, but are available upon request. Please see the Appendix for a description of the construction of the alternative sample.

of the same sex. I separately estimate the 2SLS effect of family size on a sample excluding those where the first two children are the same sex, and a sample excluding those where the first two children are of different sexes.[14] The estimates for the two samples are shown in columns (1) and (2) of Table 5. The positive main effects are driven by households where the children are of the same sex. This should be interpreted cautiously since sex can be endogenously chosen by parents.[15]

Second, I explore the hypothesis that a second child increases school enrollment of the first through permanent income channels. In rural China, parents rely on children for income during old age. Therefore, an additional child can be seen as an increase to permanent income. This will lead to an increase in school enrollment if parents can borrow against children's future income. This seems unlikely to be true in rural China during the 1980s. However, to be cautious, I investigate this hypothesis under the assumption that parents expect sons to earn more than daughters. In this case, permanent income effects should cause the benefit of the second child to be larger when the second child is a boy. Hence, I divide the sample into those that do not have a younger sister and those that do not have a younger brother and examine whether the main effect differs by the sex of the younger child. The results are shown in Table 5 columns (3) and (4). They show that the effects are *larger* for those with a younger sister. This is inconsistent with the permanent income explanation. As with the previous set of results, these results should be interpreted cautiously since the sex of the younger sibling can be endogenously chosen by parents.

[14] Approximately 24% of the sample has siblings of the same sex.

[15] I also estimated the differential effect of family size across different age gaps between the first two children. The results showed that the benefits are larger when larger age gaps exist between children. Interpretation of this result is made difficult that age gaps can be a result of sex selection. Parents who want a son for a second child and who are constrained to have no more than two children will, on average, have further spacing between their children than parents who have weaker son preference or parents who have limited ability to sex select. If the ability to select is correlated with factors that also determine education,such as income, then the estimated interaction effect will reflect the influence of those factors.

Finally, I explore the possibility that having a second child increases the enrollment of the first by increasing the demand on cash income. This demand could arise for contemporaneous needs or for future needs such as tuition and fees for secondary education or costs associated with marriage. If income gains from increasing labor supply exceed schooling costs and parents are credit constrained, then to meet these needs, parents could send their eldest child to school and increase their labor in the labor market. This is a plausible explanation in rural areas where neighbors and relatives can offer assistance to take care of the youngest child. Unfortunately, the data does not allow a direct examination of childcare of the youngest child, schooling costs, or wages. However, I can examine this hypothesis with cruder measures by estimating the effect of a second child on mother's labor supply and school delay.

Using a restricted sample of individuals who are currently enrolled in school, I repeat the main estimation with school delay as the dependent variable. It is measured as the difference between an individual's years of education and the years of education he/she should have had assuming that he/she began at age seven. The means are shown in Appendix Table A1 columns (5)-(8). Table 6 Panel A shows that, on average, first-born children of households with three and fewer children are 0.5 years ahead in schooling relative to the legal requirement. The sample means are similar for boys and girls. The OLS estimates in Panel A show that an additional sibling is correlated with being behind in school relative to the mean. But the estimates are not statistically significant. In contrast, the 2SLS estimates show that having a younger sibling causes the first child to attend school earlier. However, these estimates are also not statistically significant.

Next, I estimate the effect of having an additional child on mother's labor supply. The dependent variable is a dummy variable that equals one if the mother does not work outside of the home. The results are presented in Table 7. The estimates are negative and almost statistically significant at the 10% level. They suggest that an additional child causes the mother to be *less* likely to stay at home and *more* likely to participate in the labor market. Columns (7) and (8) show that the effect is statistically similar between those with a younger son and those with a younger daughter. These results are consistent with the hypothesis that parents view schools as an alternative source of child care for the first child and send her to school while the mother enters the labor force.

6 Conclusion

This paper estimates the effect of family size on school enrollment for first-born children. It resolves the problem of joint determination by exploiting the plausibly exogenous variation in family size caused by relaxations in the One Child policy. The results show that both the One Child Policy and the previous four-year birth spacing policy were well enforced; and that the 1-son-2-child relaxation increased family size for girls born in relaxed areas. Then, it uses the variation in family size caused by this relaxation to show evidence that a second child increased school enrollment of the first child. The empirical results provide empirical evidence for a novel insight about first born children, who have thus far been the focus of most existing empirical studies of quantity-quality. They show clearly that first born children benefit from having a younger sibling.

It is beyond the scope of this paper to provide conclusive evidence on the mechanisms driving the main effects. The empirical findings suggest that economies of scale in schooling and increased income demand from an additional child could play important roles. Interpreting these results outside of the context of rural China requires caution. This is especially true if parents in these other contexts do not have access to inexpensive public schooling or good labor market opportunities.

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Appendix – Selection Correction

To correct for the selection of parents who may value girls and education in the relaxed regions, I remove the "extra" boys from relaxed counties and replace them with girls that I construct so that for each cohort, the sex ratio is equivalent between counties with some relaxation and counties without any relaxation. Only boys who are not enrolled in school are removed. Added girls are assumed to be enrolled in school. This increases the average enrollment rate for boys born 1979-1981 in counties with the relaxation, and decreases average enrollment rate for girls in counties with the relaxation. 2SLS using this "stacked" sample will be biased against finding a positive effect of family size on school enrollment and allow me to estimate the lower bounds of the positive family size effect and investigate the extent to which the main results are driven by selection.

To estimate the number of "extra" boys, I first need to examine the extent of sex-selection in regions that received the relaxation before the relaxation was enacted. Recall that Figure 5A plots the coefficients and 95% confidence intervals. The estimates imply that 10.6 percentage-point more males were born in relaxed regions in the two years prior to the introduction of the relaxation. I use this difference to calculate the number of extra boys due to the One Child Policy. The estimates from the alternative sample are nearly identical to the results from using the uncorrected data. These estimates are not reported in the paper for brevity, and are available upon request.

Figure 1: The Number of Total Children in Household by Birth Year for Households with 3 or fewer children

Number of Total Children

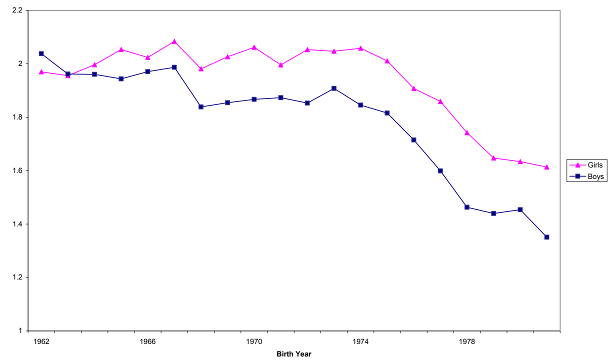

Figure 2A: Correlation between Family Size and School Enrollment by Family Size with No Birth Year Controls

Coefficient for the number of total children in the household

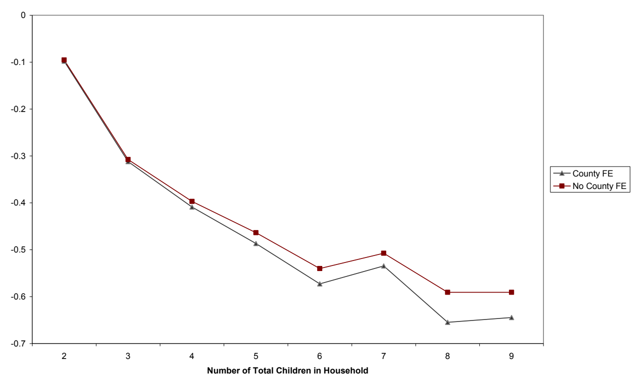

Figure 2B: Correlation between Family size and School Enrollment By Family Size with Birth Year Controls

Coefficient for the number of total children in the household

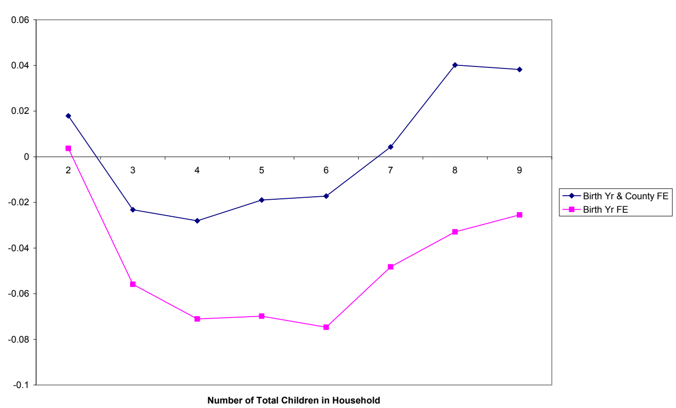

Figure 3A: The Effect of Relaxation on Family Size
Coefficients of the Interactions between
Born in a Relaxed Area * Birth Regions

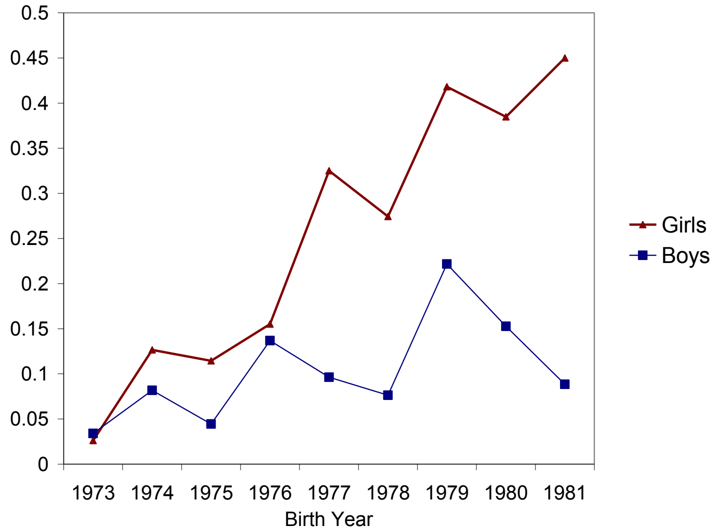

Figure 3B: The Effect of Relaxation on Family Size
Coefficients of the Interactions between
Dummy for Girl * Born in a Relaxed Region * Birth Year

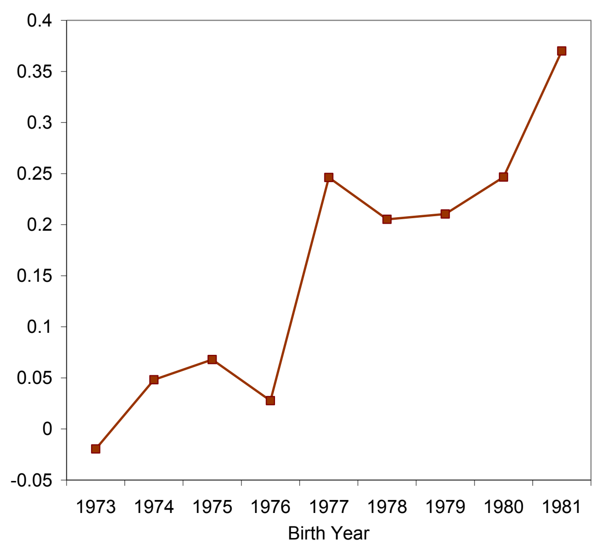

Figure 4A: The Effect of Relaxation on School Enrollment
Coefficients of Interactions between
Born in Relaxed Region * Birth Year

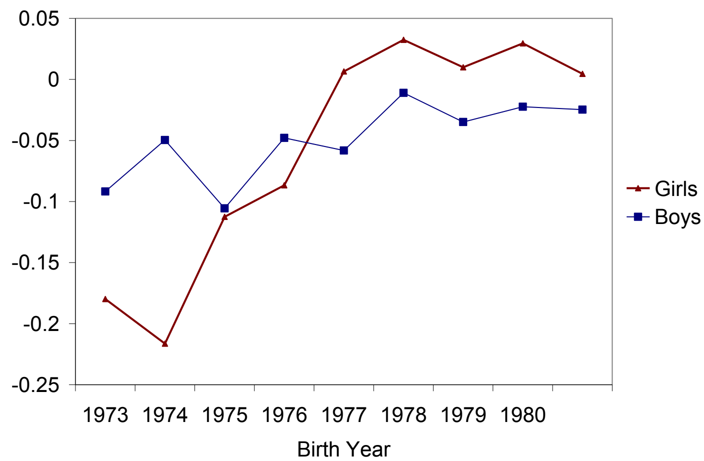

Figure 4B: The Effect of Relaxation on School Enrollment
Coefficients of Interactions between
Dummy for Girl * Born in Relaxed Region * Birth Year

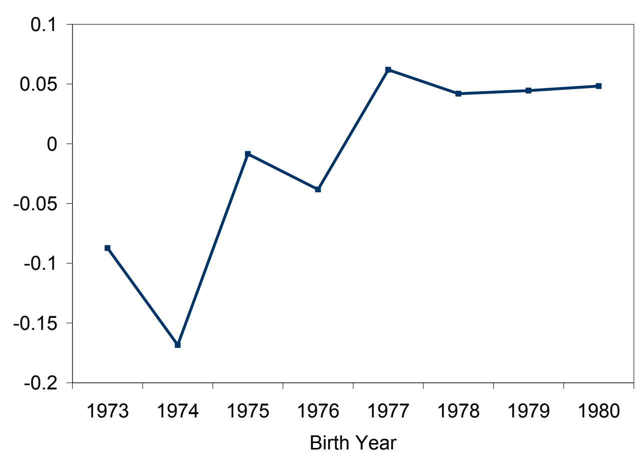

Figure 5A: The Effect of Relaxation on Sex Ratios of First Born Children and 95% Confidence Intervals

Coefficients of the Interactions between Born in Relaxed Region * Birth Year

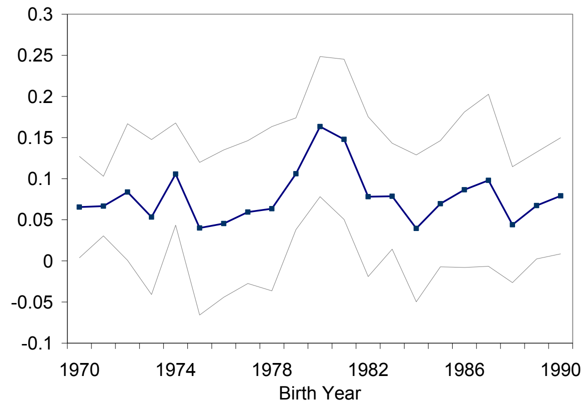

Figure 5B: The Effect of Relaxation on Sex Ratios of Second Born Children and 95% Confidence Intervals

Coefficients of the Interactions between Born in Relaxed Region * Birth Year

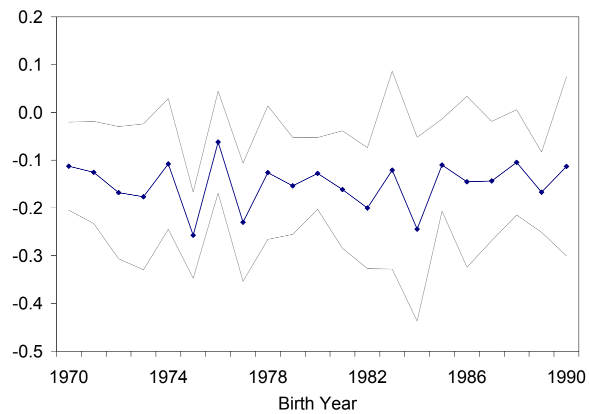

Figure 5C: The Effect of Relaxation on Sex Ratios of Later Born Children and 95% Confidence Intervals

Coefficients of the Interactions between Born in Relaxed Region * Birth Year

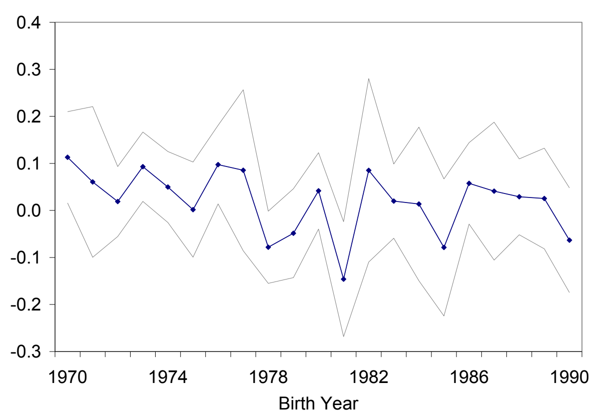

Table 1A: Time Expenditure on Household Chores and Child Care by Household Size

	Number of Children below Age 15 in Household		
Per Child Time and Money Expenditure	1	2	3
Buying and Preparing Food Last Week (Hours)	21.60	11.01	7.66
Laundry Time Last Week (Hours)	5.05	2.70	1.86
Child Care Time Last Week (Hours)	13.12	8.97	6.37
Child Care Cost Last Month (RMB)	24.53	10.26	11.49

Sample includes all rural households that report having children under the age of 15 in the 1989 CHNS Household Survey.

Table 1B: Descriptive Statistics
CHNS 1989 and 1% Sample of China Population Census

Variable	Obs	Mean	Std. Err.	Obs	Mean	Std. Err.
		A. Girls			**B. Boys**	
Han	11938	0.943	(0.002)	14518	0.949	(0.002)
# Siblings	11938	0.908	(0.007)	14518	0.759	(0.006)
Sisters	11938	0.359	(0.005)	14518	0.413	(0.005)
Brothers	11938	0.550	(0.006)	14518	0.345	(0.004)
Enrolled	11938	0.504	(0.005)	14518	0.477	(0.004)
Mother's Education	11551	6.252	(0.040)	13944	5.805	(0.036)
Father's Education	10872	8.191	(0.036)	13305	7.729	(0.034)
Mother at Home	11938	0.116	(0.003)	14518	0.135	(0.003)
Relaxation	11938	0.243	(0.003)	14518	0.238	(0.003)
Agricultural	11927	0.597	(0.004)	14481	0.608	(0.004)
		C. <=3 Children			**D. Only Child**	
Sex	16723	0.512	(0.004)	9733	0.611	(0.005)
Han	16723	0.939	(0.002)	9733	0.958	(0.002)
Enrolled	16723	0.430	(0.004)	9733	0.591	(0.005)
Mother's Education	16231	5.469	(0.031)	9264	6.952	(0.048)
Father's Education	15427	7.600	(0.029)	8750	8.530	(0.044)
Mother at Home	16723	0.129	(0.003)	9733	0.121	(0.003)
Relaxation	16723	0.272	(0.003)	9733	0.186	(0.003)
Dist to Prov. Capital	16723	169.968	(1.147)	9733	134.971	(1.607)
Dist to Big City	15806	7.711	(0.081)	9369	10.667	(0.116)
Agricultural	16694	0.709	(0.004)	9714	0.423	(0.005)

Sample of cohorts born 1962-1981

Table 1C: Descriptive Statistics for Counties with Relaxation

	No Relaxation			Some Relaxation		
Variable	**Obs**	**Mean**	**Std. Err.**	**Obs**	**Mean**	**Std. Err.**
A. Demographic						
Sex	9915	0.555	(0.005)	16541	0.545	(0.004)
Han	9915	0.967	(0.002)	16541	0.934	(0.002)
# Siblings	9915	0.833	(0.007)	16541	0.822	(0.006)
Sisters	9915	0.385	(0.006)	16541	0.391	(0.004)
Brothers	9915	0.448	(0.006)	16541	0.431	(0.004)
Enrolled	9915	0.459	(0.005)	16541	0.507	(0.004)
Mother's Education	9558	5.153	(0.042)	15937	6.520	(0.034)
Father's Education	9055	7.518	(0.038)	15122	8.187	(0.032)
Mother at Home	9915	0.110	(0.003)	16541	0.136	(0.003)
Agricultural	9905	0.700	(0.005)	16503	0.546	(0.004)
B. Infrastructural						
Relaxation	9915	0.000	0.000	16541	0.384	(0.003)
Dist to Prov Capital	9915	178.135	(1.340)	16541	144.480	(1.264)
Dist to Big City	8634	2.106	(0.017)	16541	12.311	(0.091)
Dist to Primary School	9915	0.245	(0.007)	15281	0.420	(0.004)
Dist to Middle School	9914	1.014	(0.010)	15281	1.607	(0.011)
Dist to High School	9914	4.989	(0.089)	15281	4.470	(0.069)

Sample of households with <=3 children amongst cohorts born 1962-1981.

Table 2: OLS and 2SLS Estimates of the Effect of Family Size on School Enrollment

	Dependent Variable: School Enrollment					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	All	All	All	All
OLS						
A. Households with <=3 children						
Sample Mean of Dep Var	**0.489**					
# Siblings	-0.011	-0.011	-0.012	-0.010	-0.011	-0.011
	(0.004)	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)
Observations	26456	26456	26456	25495	26456	25495
R-squared	0.69	0.69	0.69	0.70	0.69	0.70
B. Households with <=2 children						
Sample Mean of Dep Var	**0.539**					
# Siblings	0.016	0.016	0.013	0.018	0.016	0.015
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Observations	21321	21321	21321	20497	21321	20497
R-squared	0.70	0.70	0.70	0.71	0.70	0.71
2SLS						
C. Households with <=3 children						
# Siblings	0.161	0.161	0.167	0.146	0.159	0.140
	(0.058)	(0.059)	(0.062)	(0.043)	(0.062)	(0.046)
Observations	26456	26456	26456	25495	26456	25495
R-squared	0.64	0.64	0.64	0.66	0.64	0.66
D. Households with <=2 children						
# Siblings	0.122	0.122	0.134	0.097	0.125	0.104
	(0.065)	(0.065)	(0.064)	(0.063)	(0.067)	(0.062)
Observations	21321	21321	21321	20497	21321	20497
R-squared	0.69	0.69	0.69	0.70	0.69	0.70
Controls						
Han	N	Y	Y	N	N	Y
Distance to Urban	N	N	Y	N	N	Y
Mother's Education	N	N	N	Y	N	Y
Household Income	N	N	N	N	Y	Y

All regressions control for the full set of interaction terms and birth year and county fixed effects.
Standard errors are clustered at the county level.

Table 3: The Effect of Relaxation on Family Size and School Enrollment by Size

Coefficients for columns (1)-(4) are the interaction terms between born in a relaxed region * year of birth.

Coefficients in columns (5)-(6) are the triple interactions between dummy for girl * born in a relaxed region * year of birth.

	Dependent Variables						
	# Sibs		Enrollment			# Siblings	Enrollment
	(1)	(2)	(3)	(4)		(5)	(6)
	Girls	Boys	Girls	Boys		All	All
Sample Mean of Dependent Variable	**1.153**	**0.922**	**0.473**	**0.456**	**Sample Mean of Dependent Variable**	**1.028**	**0.464**
relax*born 1973	0.026	0.034	-0.180	-0.092	relax*girl*born 1973	-0.020	-0.087
	(0.110)	(0.116)	(0.082)	(0.071)		(0.099)	(0.037)
relax*born 1974	0.127	0.082	-0.216	-0.050	relax*girl*born 1974	0.048	-0.168
	(0.115)	(0.107)	(0.098)	(0.078)		(0.073)	(0.070)
relax*born 1975	0.115	0.045	-0.112	-0.106	relax*girl*born 1975	0.068	-0.009
	(0.071)	(0.139)	(0.078)	(0.048)		(0.132)	(0.056)
relax*born 1976	0.155	0.137	-0.087	-0.048	relax*girl*born 1976	0.028	-0.038
	(0.128)	(0.157)	(0.062)	(0.030)		(0.170)	(0.074)
relax*born 1977	0.325	0.096	0.007	-0.058	relax*girl*born 1977	0.246	0.062
	(0.136)	(0.101)	(0.055)	(0.037)		(0.116)	(0.061)
relax*born 1978	0.274	0.076	0.032	-0.011	relax*girl*born 1978	0.205	0.042
	(0.152)	(0.161)	(0.028)	(0.027)		(0.171)	(0.022)
relax*born 1979	0.418	0.222	0.010	-0.035	relax*girl*born 1979	0.210	0.044
	(0.158)	(0.159)	(0.034)	(0.025)		(0.183)	(0.022)
relax*born 1980	0.385	0.153	0.030	-0.022	relax*girl*born 1980	0.247	0.048
	(0.180)	(0.128)	(0.033)	(0.028)		(0.168)	(0.018)
relax*born 1981	0.450	0.088	0.005	-0.025	relax*girl*born 1981	0.370	0.029
	(0.186)	(0.154)	(0.035)	(0.030)		(0.194)	(0.014)
Observations	13271	15500	13271	15500	Observations	28771	28771
R-squared	0.26	0.23	0.70	0.69	R-squared	0.25	0.69

Regressions in columns (1)-(4) include county and birth year fixed effects.

Regressions in columns (5)-(6) include controls for relax*girl, relax*birthyear, girl*birthyear, girl, birthyear fixed effects and county fixed effects.

Standard errors clustered at county level.

Table 4: The Effect of Relaxation on Sex Ratios by Birth Parity
Coefficients are the interaction terms between born in a relaxed region * birth year.

	Dependent Variable: Dummy for Male											
	1st Born				2nd Born				3+ Born			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Sample Mean of Dep Var	**0.530**				**0.509**				**0.495**			
Relax * Born 1970	0.065	(0.032)	0.064	(0.031)	0.113	(0.047)	0.110	(0.046)	0.113	(0.049)	0.112	(0.049)
Relax * Born 1971	0.067	(0.018)	0.066	(0.018)	0.126	(0.055)	0.124	(0.054)	0.061	(0.082)	0.055	(0.081)
Relax * Born 1972	0.084	(0.042)	0.086	(0.043)	0.168	(0.071)	0.170	(0.071)	0.019	(0.038)	0.018	(0.037)
Relax * Born 1973	0.053	(0.048)	0.056	(0.049)	0.177	(0.078)	0.178	(0.077)	0.093	(0.038)	0.091	(0.037)
Relax * Born 1974	0.106	(0.032)	0.103	(0.033)	0.108	(0.070)	0.111	(0.069)	0.050	(0.039)	0.047	(0.039)
Relax * Born 1975	0.027	(0.047)	0.025	(0.049)	0.257	(0.046)	0.256	(0.047)	0.002	(0.052)	0.002	(0.052)
Relax * Born 1976	0.045	(0.046)	0.050	(0.047)	0.062	(0.054)	0.066	(0.055)	0.097	(0.043)	0.094	(0.044)
Relax * Born 1977	0.059	(0.044)	0.057	(0.045)	0.230	(0.063)	0.225	(0.063)	0.085	(0.087)	0.081	(0.087)
Relax * Born 1978	0.063	(0.051)	0.056	(0.052)	0.126	(0.071)	0.122	(0.071)	0.078	(0.039)	0.080	(0.039)
Relax * Born 1979	0.106	(0.035)	0.101	(0.036)	0.154	(0.052)	0.154	(0.051)	0.048	(0.048)	0.050	(0.049)
Relax * Born 1980	0.163	(0.044)	0.162	(0.044)	0.128	(0.038)	0.126	(0.039)	0.042	(0.041)	0.041	(0.041)
Relax * Born 1981	0.148	(0.050)	0.145	(0.051)	0.161	(0.063)	0.169	(0.064)	0.146	(0.062)	0.144	(0.063)
Relax * Born 1982	0.078	(0.050)	0.077	(0.050)	0.200	(0.065)	0.200	(0.065)	0.085	(0.099)	0.073	(0.098)
Relax * Born 1983	0.079	(0.033)	0.081	(0.034)	0.121	(0.106)	0.120	(0.105)	0.020	(0.040)	0.007	(0.037)
Relax * Born 1984	0.040	(0.046)	0.038	(0.046)	0.244	(0.098)	0.241	(0.098)	0.014	(0.083)	0.003	(0.083)
Relax * Born 1985	0.070	(0.039)	0.066	(0.039)	0.110	(0.049)	0.114	(0.048)	0.079	(0.074)	0.079	(0.076)
Relax * Born 1986	0.086	(0.048)	0.089	(0.048)	0.145	(0.091)	0.158	(0.092)	0.058	(0.044)	0.047	(0.049)
Relax * Born 1987	0.098	(0.053)	0.097	(0.053)	0.144	(0.064)	0.145	(0.062)	0.041	(0.075)	0.038	(0.075)
Relax * Born 1988	0.044	(0.036)	0.038	(0.036)	0.105	(0.056)	0.108	(0.057)	0.029	(0.041)	0.028	(0.041)
Relax * Born 1989	0.067	(0.033)	0.075	(0.033)	0.167	(0.043)	0.167	(0.044)	0.025	(0.055)	0.021	(0.055)
Relax * Born 1990	0.079	(0.036)	0.077	(0.037)	0.113	(0.096)	0.118	(0.095)	0.063	(0.057)	0.064	(0.057)
Han	N		Y		N		Y		N		Y	
Observations	44754		44754		23306		23306		14495		14495	
R-squared	0.01		0.01		0.01		0.01		0.01		0.01	

Regressions include county and birthyear fixed effects.
Standard errors clustered at the county level.

Table 5: OLS and 2SLS Estimates of the Effect of Family Size on School Enrollment by Sex Composition of Siblings

	Dependent Variable: Fraction enrolled in School			
	(1)	(2)	(3)	(4)
	Different Sex	Same Sex	Younger Sister	Younger Brother
OLS				
A. Households with <=3 children				
Sample Mean of Dep Var	**0.511**	**0.419**	**0.534**	**0.536**
# Siblings	-0.053	-0.011	-0.009	-0.005
	(0.011)	(0.005)	(0.004)	(0.004)
Observations	6218	19277	17785	18601
R-squared	.55	0.6	0.73	0.72
B. Households with <=2 children				
Sample Mean of Dep Var	**.483**	**0.554**	**0.378**	**0.423**
# Siblings	0.000	0.005	0.018	0.015
	(0.000)	(0.006)	(0.005)	(0.006)
Observations	3907	16590	14979	16213
R-squared	0.55	0.6	0.73	0.72
2SLS				
C. Households with <=3 children				
# Siblings	0.058	0.176	0.190	0.049
	(0.225)	(0.082)	(0.049)	(0.048)
Observations	6383	20073	18514	19316
R-squared	0.57	0.67	0.65	0.71
D. Households with <=2 children				
# Siblings	0.000	0.112	0.125	0.078
	(0.000)	(0.058)	(0.063)	(0.052)
Observations	4018	17303	15641	16857
R-squared	0.58	0.72	0.72	0.71
Controls				
Han	Y	Y	Y	Y
Distance to Urban	Y	Y	Y	Y
Mother's Education	Y	Y	Y	Y
Household Income	Y	Y	Y	Y

All regressions control for the full set of interaction terms and birth year and county fixed effects. Standard errors are clustered at the county level. Column (1) excludes individuals whose next youngest sibling is of the same sex. Column (2) excludes individuals whose next youngest sibling is of different sex. Column (3) is restricted to the sample individuals who do not have a younger brother. Column (4) is restricted to the sample individuals who do not have a younger sister.

Table 6: OLS and 2SLS Estimates of the Effect of Family Size on Education Delay
Coefficients for the number of siblings a first born child has

	Dependent Variable: Actual Years of Education - Supposed Years of Education							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	All	All	All	All	All	Younger Sister*	Younger Brother*
Households with <=3 children								
A. OLS								
Sample Mean of Dep Var				**0.489**			**0.534**	**0.536**
# Siblings	-0.121	-0.121	-0.127	-0.076	-0.125	-0.095	-0.121	-0.115
	(0.026)	(0.026)	(0.029)	(0.021)	(0.021)	(0.024)	(0.027)	(0.026)
Observations	12940	12940	12940	12715	12940	12715	9817	10306
R-squared	0.63	0.63	0.63	0.64	0.63	0.65	0.68	0.68
2SLS								
B. 2SLS								
# Siblings	0.297	0.299	0.274	0.303	0.274	0.213	0.179	0.504
	(0.440)	(0.439)	(0.438)	(0.469)	(0.433)	(0.454)	(0.436)	(0.606)
Observations	12940	12940	12940	12715	12940	12715	10008	10499
R-squared	0.61	0.61	0.62	0.63	0.62	0.65	0.65	0.63
Controls								
Han	N	Y	Y	N	N	Y	Y	Y
Distance to Urban	N	N	Y	N	N	Y	Y	Y
Mother's Education	N	N	N	Y	N	Y	Y	Y
Household Income	N	N	N	N	Y	Y	Y	Y

Sample restricted to individuals enrolled in school.
All regressions control for the full set of interaction terms and birth year and county fixed effects.
Standard errors are clustered at the county level.
*Column (7) is restricted to the sample individuals who do not have a younger brother.
*Column (8) is restricted to the sample individuals who do not have a younger sister.

Table 7: The Effect of Family Size on Female Labor Supply

	Dependent Variable: Dummy Variable for Mother who Stays at Home							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	All	All	All	All	All	Younger Sister*	Younger Brother*
Households with <=3 Children								
Sample Mean of Dep. Var				**0.126**			**0.124**	**0.123**
A. OLS								
# Siblings	-0.029	-0.029	-0.029	-0.036	-0.028	-0.036	-0.036	-0.038
	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)
Observations	26456	26456	26456	25495	26456	25495	17785	18601
R-squared	0.13	0.13	0.13	0.19	0.13	0.19	0.20	0.20
B. 2SLS								
# Siblings	-0.142	-0.144	-0.144	-0.122	-0.139	-0.119	-0.137	-0.141
	(0.098)	(0.098)	(0.098)	(0.088)	(0.098)	(0.088)	(0.055)	(0.079)
Observations	26456	26456	26456	25495	26456	25495	18514	19316
R-squared	0.08	0.08	0.08	0.16	0.09	0.17	0.09	0.10
Controls								
Han	N	Y	Y	N	N	Y	Y	Y
Distance to Urban	N	N	Y	N	N	Y	Y	Y
Mother's Education	N	N	N	Y	N	Y	Y	Y
Household Income	N	N	N	N	Y	Y	Y	Y

All regressions control for the full set of interaction terms and birth year and county fixed effects.

Standard errors are clustered at the county level.

*Column (7) is restricted to the sample individuals who do not have a younger brother.

*Column (8) is restricted to the sample individuals who do not have a younger sister.

Appendix

Figure A1: The correlation between family size and education delay
Coefficients for dummy variables for total number of children in the household

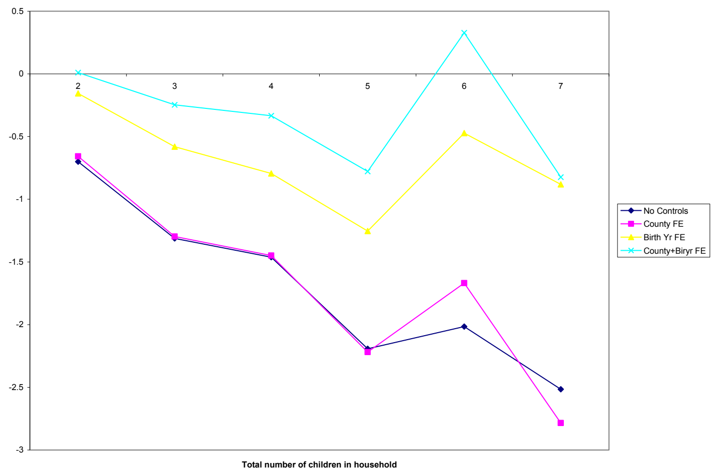

Table A1: The Correlation between Family Size and School Enrollment

Coefficients of dummy variables for the total number of children in each household

	Dependent Variables							
	School Enrollment				Actual Years of Edu - Supposed Years of Edu			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample Mean for Dependent Variable		**0.464**				**-2.470**		
Total Kids = 2	-0.095	-0.098	0.004	0.018	-0.700	-0.657	-0.156	0.010
	(0.051)	(0.050)	(0.004)	(0.006)	(0.115)	(0.140)	(0.033)	(0.042)
Total Kids = 3	-0.308	-0.312	-0.056	-0.023	-1.312	-1.297	-0.581	-0.247
	(0.057)	(0.057)	(0.011)	(0.009)	(0.168)	(0.172)	(0.099)	(0.062)
Total Kids = 4	-0.397	-0.409	-0.071	-0.028	-1.462	-1.449	-0.794	-0.334
	(0.058)	(0.059)	(0.011)	(0.009)	(0.175)	(0.203)	(0.126)	(0.074)
Total Kids = 5	-0.464	-0.487	-0.070	-0.019	-2.192	-2.219	-1.254	-0.778
	(0.055)	(0.056)	(0.018)	(0.015)	(0.290)	(0.362)	(0.198)	(0.163)
Total Kids = 6	-0.540	-0.573	-0.075	-0.017	-2.015	-1.668	-0.472	0.328
	(0.051)	(0.056)	(0.024)	(0.020)	(0.310)	(0.359)	(0.285)	(0.197)
Total Kids = 7	-0.507	-0.534	-0.048	0.004	-2.515	-2.784	-0.881	-0.823
	(0.061)	(0.063)	(0.047)	(0.046)	(0.364)	(0.325)	(0.450)	(0.202)
Total Kids = 8	-0.591	-0.655	-0.033	0.040	0.000	0.000	0.000	0.000
	(0.039)	(0.048)	(0.006)	(0.016)	(0.000)	(0.000)	(0.000)	(0.000)
Total Kids = 9	-0.591	-0.644	-0.025	0.038	0.000	0.000	0.000	0.000
	(0.039)	(0.048)	(0.006)	(0.020)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.591	0.594	0.478	-0.001	2.015	1.995	1.680	-5.029
	(0.039)	(0.034)	(0.015)	(0.016)	(0.075)	(0.080)	(0.066)	(0.724)
Controls								
County Fixed Effect	N	Y	N	Y	N	Y	N	Y
Birth Year Fixed Effect	N	N	Y	Y	N	N	Y	Y
Observations	28771	28771	28771	28771	13338	13338	13338	13338
R-squared	0.08	0.08	0.71	0.72	0.07	0.08	0.60	0.62

All standard errors clustered at county level.

Columns (5)-(8) restricted to sample of children enrolled in school.

Table A2: The Effect of Relaxation on Family Size and School Enrollment by Size for Households with 3 or Fewer Children
Coefficients for columns (1)-(4) are the interaction terms between born in a relaxed region * year of birth.
Coefficients in columns (5)-(6) are the triple interactions between dummy for girl * born in a relaxed region * year of birth.

	Dependent Variables						
	# Siblings		School Enrollment			# Siblings	School Enrollment
	(1)	(2)	(3)	(4)		(5)	(6)
	Girls	Boys	Girls	Boys		All	All
Sample Mean of Dependent Variable	**0.908**	**0.759**	**0.500**	**0.480**	**Sample Mean of Dependent Variable**	**0.826**	**0.489**
Relax* Born 1973	0.006	0.074	0.209	0.106	Relax*Girl*Born 1973	0.083	0.101
	(0.080)	(0.099)	(0.079)	(0.074)		(0.086)	(0.041)
Relax* Born 1974	0.225	0.105	0.230	0.035	Relax*Girl*Born 1974	0.129	0.194
	(0.070)	(0.060)	(0.103)	(0.078)		(0.067)	(0.076)
Relax* Born 1975	0.211	0.148	0.100	0.124	Relax*Girl*Born 1975	0.063	0.023
	(0.087)	(0.111)	(0.063)	(0.054)		(0.059)	(0.037)
Relax* Born 1976	0.317	0.158	0.074	0.040	Relax*Girl*Born 1976	0.179	0.033
	(0.070)	(0.133)	(0.060)	(0.033)		(0.104)	(0.079)
Relax* Born 1977	0.509	0.242	0.015	0.060	Relax*Girl*Born 1977	0.287	0.074
	(0.088)	(0.119)	(0.043)	(0.038)		(0.105)	(0.050)
Relax* Born 1978	0.493	0.145	0.032	0.015	Relax*Girl*Born 1978	0.361	0.045
	(0.117)	(0.151)	(0.029)	(0.027)		(0.133)	(0.023)
Relax* Born 1979	0.571	0.275	0.016	0.040	Relax*Girl*Born 1979	0.315	0.056
	(0.117)	(0.162)	(0.034)	(0.024)		(0.157)	(0.019)
Relax* Born 1980	0.525	0.260	0.026	0.026	Relax*Girl*Born 1980	0.268	0.049
	(0.153)	(0.154)	(0.034)	(0.026)		(0.176)	(0.019)
Relax* Born 1981	0.551	0.168	0.003	0.028	Relax*Girl*Born 1981	0.411	0.030
	(0.135)	(0.174)	(0.035)	(0.028)		(0.174)	(0.016)
					Relax*Girl*Born 1982		
Observations	11938	14518	11938	14518		26456	26456
R-squared	0.18	0.20	0.70	0.68		0.19	0.69

Regressions in columns (1)-(4) include county and birth year fixed effects.
Regressions in columns (5)-(6) include controls for relax*girl, relax*birthyear, girl*birthyear, girl, birthyear fixed effects and county fixed effects.
Standard errors clustered at county level.