Rationed Fertility: Theory and Evidence

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Abstract

We analyze the effect of *exogenous* changes in fertility on child quality both at and off the unrestricted optimal fertility level in a general model with rationed fertility. Besides the price and substitution effects analyzed in the literature, we show that a desired fertility change, representing a shift towards the optimal level, induces a positive income effect, while a forced fertility change, representing a shift away from the optimal level, induces a negative income effect. To empirically test the theory, we combine a natural experiment, *i.e.*, twins, with a policy change, *i.e.*, One-child Policy and find results that are consistent with the theoretical implications of our model. Our paper provides a theory of rationed fertility that reconciles recent empirical literature on the heterogeneous fertility effects on child quality, and advances our understanding of how population control policy affects human capital investment and economic development.

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Key words: Rationed Fertility; quantity-quality trade-off; intrahousehold resource allocation

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"Thus, voluntary versus mandatory population policies, which might lead to the same decline in fertility, could theoretically have a different effect on other family outcomes and on the distribution of welfare losses and gains."

– Schultz (2007, p. 3265)

1 Introduction

Population control policies had once been widely implemented in both developed and developing countries, and still exist in developing countries. These policies are believed to have profound implications for economic development (Schultz, 2007). Becker and Lewis's (1973) theory of child quantity-quality (Q-Q) trade-off predicts that the average quality of children is lower in larger families than in smaller ones.[1] Although the Q-Q theory has influenced policy practices worldwide, the empirical literature shows little consensus. Leibowitz (1974), Rosenzweig and Wolpin (1980), Hanushek (1992), and Rosenzweig and Zhang (2009) find that fertility has a negative effect on child quality; the negative effect found in Rosenzweig and Zhang (2009) is quite modest. On the contrary, Black, Devereux, and Salvanes (2005) and Angrist, Lavy, and Schlosser (2010) show that the effect of fertility on child quality, if any, is minimal. More recently, Mogstad and Wiswall (2016) and Brinch, Mogstad, and Wiswall (2016) demonstrate heterogeneous impacts of fertility on child education by specific margins of fertility change.[2]

Apart from the inconsistency in the results of empirical studies on fertility and child quality, theoretical analyses are scarce. To fill this gap, we formulate a general theory of the effect of *exogenous* changes in fertility on child quality, based on Becker and Lewis (1973) and Rosenzweig and Wolpin (1980). In particular, Becker and Lewis (1973) predict a negative *correlation* between child quantity and quality, pioneering the theory of child Q-Q trade-off. Specifically, child quantity and quality enter the household budget constraint in a multiplicative manner, so that an increase in child quantity raises the cost of child quality. If the income elasticity of child quality is larger

[1] We use fertility, child quantity, and family size interchangeably in this paper.
[2] We discuss the heterogeneous fertility effects in detail in Section 2.1.

than the income elasticity of child quantity, an increase in income lowers parental demand for child quantity while raising demand for child quality.[3] The insight from the Q-Q interactions is incorporated into macroeconomic models to interpret demographic transition and economic growth (e.g., Barro and Becker, 1989; Becker and Barro, 1988; Becker, Murphy, and Tamura, 1990; Galor and Weil, 2000; Galor, 2005). However, Becker and Lewis (1973) are silent on the impact of an exogenous change in child quantity on child quality, as both child quantity and quality are choice variables in their setup. Their framework does not allow a full comparative static analysis of the effect of child quantity on child quality.

Rosenzweig and Wolpin (1980) carry out a comparative static analysis of the effect of child quantity on child quality, using a method that is essentially an application of rationing theory (Tobin and Houthakker, 1950). In rationing theory, one variable is restricted or rationed when conducting a comparative static analysis involving two choice variables. Accordingly, Rosenzweig and Wolpin (1980) fix child quantity and theoretically model exogenous changes in fertility. They show that an exogenous increase in fertility does not necessarily reduce child quality in theory. Specifically, the effect of an exogenous change in fertility on child quality is decomposed into price and substitution effects. The price effect is always negative, as implied in the original Becker-Lewis setup: child quantity and quality are multiplicative in the household budget constraint, indicating that an exogenous increase in fertility makes child quality more costly, and thus decreases demand for child quality. Of the substitution effect, the sign cannot be predetermined: it is negative if child quantity and quality, both of which enter parental utility function, are net substitutes, and positive if child quantity and child quality are net complements. As such, the overall effect of fertility on child quality is inconclusive in Rosenzweig and Wolpin's (1980) model.

We utilize the rationing theory developed in Neary and Roberts (1980), and extend Rosenzweig and Wolpin's (1980) model. Specifically, Neary and Roberts (1980) generalize the original rationing theory in Tobin and Houthakker (1950) by providing a method of comparative static analysis both at and off the unrestricted optimal level of the rationed choice variable using the duality

[3] Becker and Tomes (1976) relax assumptions on the income elasticities of child quantity and quality.

technique. Therefore, relative to Rosenzweig and Wolpin (1980) who conduct a comparative static analysis of the effect of child quantity on child quality only at the optimal level of fertility, we are able to conduct the analysis both at and off the unrestricted optimal level.

Our theory decomposes the effect of an exogenous fertility change on child quality into three effects. In addition to the price and substitution effects in Rosenzweig and Wolpin (1980), we find an income effect, the sign of which is determined by the type of changes in fertility. Specifically, a change in fertility is classified as either a forced or a desired change, as shown in Figure 1. Defining n^o as the optimal level of fertility when it is not rationed, a forced change is a change away from n^o, while a desired change is towards n^o. For example, an increase from n^o to $n^o + 1$ is regarded as forced, while an increase from $n^o - 1$ to n^o is regarded as desired. Our model is more general than Rosenzweig and Wolpin's (1980) that considers only an infinitely small change in the neighborhood of n^o and ignores the income effect.

We show that a desired fertility change leads to a positive income effect on child quality, while a forced fertility change leads to a negative income effect. Specifically, a desired (forced) change in fertility raises (reduces) the maximal attainable utility of parents; the consequence is equivalent to that of an increase (a reduction) in parental income, which raises (reduces) child quality given that children are normal goods. Our theory predicts that compared with a desired fertility increase, a forced one is more likely to reduce child quality; likewise, compared with a desired fertility decrease, a forced one is less effective in promoting human capital investment. The type of fertility change in reality is determined by the specific constraint faced by the household. The additional income effect differentiates our theory from those in Becker and Lewis (1973) and Rosenzweig and Wolpin (1980).

In addition to the enriched theory, we present a novel way to test its implications. Specifically, we empirically test our theoretical implications by distinguishing between two types of exogenous changes in fertility, *i.e.*, forced and desired changes. Our analysis combines a natural experiment of twin births with the unprecedented One-child Policy (OCP) in China. From 1979 to 2015, the OCP was implemented and fertility was rationed. Some parents faced binding rationing of fertility, and thus could not achieve their optimal fertility level during this period. Twin births exogenously broke the OCP and shifted fertility back to the unrestricted optimum for the rationed families, for whom we regard the twinning-induced increase in fertility as desired. On the contrary, we treat the twinning-induced increase in fertility as forced for families for whom fertility is not rationed.[4]

Data for our empirical analysis are from the 1982 and 1990 China population censuses. We exploit two variations in the rationing of fertility to test our theory. First, we explore the differential treatments of the OCP: from 1979 to 1990, the OCP was applied to *Han* Chinese but not to ethnic minorities (Li, Yi, and Zhang, 2011). In other words, the OCP rations the fertility of *Han* Chinese, but not that of ethnic minorities. Twinning-induced increase in fertility was mainly a forced increase for ethnic minorities. In contrast, twinning-induced increase in fertility was a mix of forced and desired increases for *Han* Chinese. Based on the 1990 census data, we find that the twinning-induced increase in fertility significantly reduces educational achievement of minority children, but not that of *Han* children.

Second, we explore the implementation of the OCP in 1979. Since the OCP rations the fertility of *Han* Chinese after the OCP, but not before the OCP, twinning-induced increase in fertility for *Han* Chinese was mainly a forced increase before the OCP, but it was a mix of forced and desired increases after the OCP. Based on a combined sample of the 1982 and 1990 censuses, we find that for *Han* Chinese, the twinning-induced increase in fertility significantly reduces educational achievement before the OCP, but not after the OCP. The results from the second test are also validated by a robustness analysis using two unique surveys of Chinese twins. These empirical results are consistent with our theoretical implications.

Finally, to better understand how child quality can be maintained after a fertility increase, we study fertility effects on parental consumption and labor supply. We use the Chinese Child Twins Survey and find that in response to having twins (a mix of forced and desired fertility increases), parents work harder and consume less to maintain the same level of investment in their children. These findings are in line with Angrist, Lavy, and Schlosser (2010) and Galor (2012), who note that

[4] We discuss the econometric details for this intuitively appealing classification in Section 4.1.

an exogenous fertility increase may lead parents to reallocate their resources at margins other than child quality. For instance, Angrist, Lavy, and Schlosser (2010) conjecture that parents may reduce their own consumption or increase labor supply to maintain child quality. In addition, as proposed by Galor (2012, pp. 14-15), "...the effect of a change in an endogenous variable (*i.e.*, quantity of children) requires a careful examination of the adjustment made by the household due to the exogenously imposed non-optimal choice of quantity." Thus, our results also contribute to the literature on the role of intrahousehold resource reallocation in mediating the effect of exogenous fertility changes on child quality.

The rest of this paper is organized as follows. Section 2 discusses the literature. Section 3 develops the theory. Section 4 describes the data and our empirical strategy, the results of which are presented in Section 5. Section 6 studies parental responses to fertility, and section 7 concludes.

2 Literature

This section discusses major contributions to the literature. First, our paper proposes an economic theory that reconciles the heterogeneous fertility effects on child quality in recent empirical studies. Second, our findings have profound implications for population control policies. Finally, this paper offers a reasonable explanation for the modest effect of the OCP on child quality in China.

2.1 Heterogeneous Fertility Effects

Our paper provides an economic theory that reconciles three types of heterogeneous fertility effects documented in the literature. First, Mogstad and Wiswall (2016) show that the effect of fertility on child quality varies by the parity of fertility change. The effect of fertility on child quality is positive for small families, but negative for large families. They attribute this inverted U-shaped relationship between child quantity and quality to differential complementarities between child quantity and quality at different fertility levels, based on Rosenzweig and Wolpin (1980).[5]

[5] Rosenzweig and Wolpin (1980) first recognize that the substitution effect obscures the negative price effect of fertility on child quality. They conclude that "...the positive relationship between *N* (child quantity) and the shadow

Mogstad and Wiswall (2016) conjecture that complementarities may be stronger in smaller families. We offer a new theoretical foundation for this inverted-U relationship between child quantity and quality. Besides the price and substitution effects discussed in Mogstad and Wiswall (2016), the heterogeneous effects of fertility on child quality may result from the rationing income effect derived from our theory. If parents with low fertility are more likely to be rationed by some infertility constraints, a fertility increase in small families is more likely to be desired, and a fertility increase in large families is more likely to be forced.[6] The inverted U-shaped relationship between child quantity and quality is consistent with the rationing income effect.

Second, Brinch, Mogstad, and Wiswall (2016) find that the effect of fertility on child quality is correlated with parental returns of fertility change. They conclude: " …the effects of family size vary in magnitude and even sign, and that families act as if they possess some knowledge of their idiosyncratic return in the fertility decision." The rationing income effect sheds light on how "idiosyncratic return in the fertility decision (Brinch, Mogstad, and Wiswall, 2016, p. 27)" shapes the heterogeneous effects of fertility on child quality. As parents know their optimal level of fertility, they possess the knowledge of the return of fertility shocks at different birth parities. Specifically, an exogenous fertility increase is more likely to reduce child quality if it reduces the maximal attainable utility of parents.

Third, the results in our paper also help us understand the different estimates of local average treatment effects using different instrumental variables. For example, the distinction between "forced" and "desired" fertility changes proposed in our paper is analogous to the distinction between "unexpected" and "expected" fertility shocks raised in Black, Devereux, and Salvanes (2010), in the sense that a forced fertility change or an unexpected fertility shock is more likely to reduce child quality. Black, Devereux, and Salvanes (2010) use Norwegian data and find that a fertility increase induced by twinning has a negative effect on children's IQ scores, but a fertility increase induced by sex composition has no effect on children's IQ scores. They classify the fer-

price of Q (child quality) does not necessarily imply that an exogenous increase in N will reduce quality per child, since Q and N may be (strong) complements" (Rosenzweig and Wolpin, 1980, p. 232).

[6] We assume that theoretically parents can perfectly control fertility. In reality, parents may not be able to do so because of, say, some biological reasons.

tility change induced by twinning as an unexpected fertility shock and the fertility change induced by sex composition as an expected fertility shock. This finding supports our theory: compared with fertility change induced by the instrumental variable of gender composition, fertility change induced by twinning is more likely forced in the absence of fertility constraints.

2.2 Population Control Policies

The results in our paper have strong implications for population control policies. We show that a desired fertility reduction is more likely to improve child quality, while a forced fertility reduction is less likely to do so. This implication resonates the distinction between "voluntary" and "mandatory" population control policies (Schultz, 2007, p. 3265). A voluntary policy, such as distribution of contraception knowledge, helps parents achieve their optimal level of fertility. Thus the fertility decline induced by voluntary policies is likely to enhance child quality. On the contrary, a mandatory policy forces parents to deviate from their optimal level of fertility. Consequently, the fertility decline induced by mandatory policies does not necessarily enhance child quality.

The policy implications of our results are supported by existing empirical evidence. Rosenzweig and Schultz (1987), for instance, find that a fertility increase induced by imperfect birth control, which can be regarded as a forced fertility increase induced by a mandatory policy, reduces child quality in Malaysia. Joshi and Schultz (2013) find that a fertility reduction induced by home delivery of contraceptives, which can be regarded as a voluntary policy, boosts up child quality in Bangladesh. Pantano (2016) exploits a desired fertility reduction induced by the spread of the birth control pill in the U.S. He finds that children born in families with less unwanted fertility commit less crime, indicating that a desired fertility reduction raises child quality.

2.3 Effect of One-child Policy on Child Quality in China

Our paper provides an explanation of the modest effect of the OCP on child quality in China. Qian (2009), Liu (2014), and Li and Zhang (2016) use differential implementation of the OCP as an instrumental variable for fertility, and respectively find that fertility has a positive effect on school enrollment, a negative effect on child height, and a negative effect on educational attainment.[7] Notably, the estimates of the fertility effect in these studies are small in magnitude. Our explanation of the modest effect is that the fertility reduction induced by the OCP is forced, and is thus less likely to improve child quality. Correspondingly, the fertility increase induced by recent relaxations of the OCP is desired, and is thus less likely to reduce child quality.

3 Theory

In this section, we build on Rosenzweig and Wolpin's (1980) model using rationing theory in Neary and Roberts (1980), and formulate a general theory of *exogenous* fertility change. We first present and solve a model of rationed fertility. We then derive the effect of rationed fertility on child quality, and follow up with a simulation analysis.

3.1 Model Setup

We first consider the three-commodity interactive (q^2) model as in Becker and Lewis (1973) and Rosenzweig and Wolpin (1980). Parents maximize their utility by choosing the number of children (n), average child quality (q), and a composite good (s). The utility maximization problem is

$$\begin{aligned} \max_{n,q,s} \quad & U(n, q, s), \\ \text{subject to} \quad & \pi_{nq} nq + \pi_n n + \pi_q q + \pi_s s \leq y, \end{aligned} \tag{1}$$

where y is family income, π_s is the price of the composite good, π_{nq} is the price of increasing the quality of one child by one unit, π_n is the price of child quantity unrelated with child quality, and π_q is the price of child quality unrelated with child quantity. Following Becker and Lewis (1973), we call π_n the "fixed" price of child quantity, and π_q the "fixed" price of child quality.

We define $p_n = \pi_n + \pi_{nq} q$, $p_q = \pi_q + \pi_{nq} n$, and $p_s = \pi_s$. The terms p_n, p_q, and p_s are the "actual"

[7] The long-term socio-economic consequences of the OCP have been widely explored in the literature (Ebenstein, 2010, 2011; Li, Yi, and Zhang, 2011; Huang, Xu, and Jin, 2016; Huang, Lei, and Zhao, 2016; Huang, Lei, and Sun, 2016; Huang, 2016; Zhang, 2016).

prices of n, q, and s, respectively. To simplify notation, let $x = (n, q, s)$, $\pi = (\pi_{nq}, \pi_n, \pi_q, \pi_s)$, and $p = (p_n, p_q, p_s)$. When there is no restriction on the choice of x, p equals the shadow price of x at the optimal point. Since Becker and Lewis (1973) and Rosenzweig and Wolpin (1980) conduct the comparative static analysis only at the unrestricted optimal point, they do not distinguish p from the shadow price of x. To derive the implication of rationed fertility in the case of p not equal to the shadow price of x, a comparative static analysis off the unrestricted optimal point is necessary. Hence we call p the "actual" price of x to distinguish from the shadow price of x.

If $\pi_{nq} > 0$, child quality enters the price of child quantity, and vice versa (Becker and Lewis, 1973). If $\pi_{nq} = 0$, the q^2 model is reduced to the standard non-interactive (q^1) model.

We defined the indirect utility function and uncompensated demand functions as

$$V(\pi, y) = \max_{n,q,s}\{U(n, q, s)|\pi_{nq}nq + \pi_n n + \pi_q q + \pi_s s \leq y\}, \tag{2}$$

$$x(\pi, y) = \arg\max_{n,q,s}\{U(n, q, s)|\pi_{nq}nq + \pi_n n + \pi_q q + \pi_s s \leq y\}, \tag{3}$$

where $x(\pi, y) = (n(\pi, y), q(\pi, y), s(\pi, y))$.

The dual problem of the utility maximization problem in the q^2 model is the expenditure minimization problem,

$$\begin{aligned} \min_{n,q,s} \quad & \pi_{nq}nq + \pi_n n + \pi_q q + \pi_s s, \\ \text{s.t} \quad & U(n, q, s) \geq u. \end{aligned} \tag{4}$$

We define the expenditure function and compensated demand functions as

$$e(\pi, u) = \min_{n,q,s}\{\pi_{nq}nq + \pi_n n + \pi_q q + \pi_s s|U(n, q, s) \geq u\}, \tag{5}$$

$$x^c(\pi, u) = \arg\min_{n,q,s}\{\pi_{nq}nq + \pi_n n + \pi_q q + \pi_s s|U(n, q, s) \geq u\}, \tag{6}$$

where $x^c(\pi, u) = (n^c(\pi, u), q^c(\pi, u), s^c(\pi, u))$. Throughout the paper, we use superscript "c" to denote compensated demand functions (in the Hicksian sense) in the expenditure minimization problem.

By the duality theorem, $x(\pi, y) = x^c(\pi, V(\pi, y))$. Specifically, we define the optimal fertility level $n^o \equiv n(\pi, y) = n^c(\pi, V(\pi, y))$.

3.2 Rationed Fertility

We follow the rationing theory in Neary and Roberts (1980) to carry out the comparative static analysis of an exogenous fertility change. As in Becker and Lewis (1973), both n and q are choice variables, so a direct comparative static analysis of the effect of n on q is not feasible. Rather, we focus primarily on the effect of rationed fertility.

To begin with, we fix n at $n = \bar{n}$. We call the q^2 (or q^1) model with fixed fertility ($n = \bar{n}$) the restricted q^2 (or q^1) model. The utility maximization problem in the restricted q^2 model is

$$\begin{aligned} \max_{q,s} \quad & U(\bar{n}, q, s), \\ \text{subject to} \quad & \pi_n \bar{n} + (\pi_{nq}\bar{n} + \pi_q)q + \pi_s s \leq y, \end{aligned} \tag{7}$$

where $\bar{n}$ is the number of children mandated by some exogenous force. Note that the restriction on or rationing of fertility makes $\bar{n}$ a parameter rather than a choice variable. We define the indirect utility function and uncompensated demand functions in the restricted q^2 model as

$$\tilde{V}(\pi, y, \bar{n}) = \max_{q,s}\{U(\bar{n}, q, s) | \pi_n \bar{n} + (\pi_{nq}\bar{n} + \pi_q)q + \pi_s s \leq y\}, \tag{8}$$

$$\tilde{x}(\pi, y, \bar{n}) = \arg\max_{q,s}\{U(\bar{n}, q, s) | \pi_n \bar{n} + (\pi_{nq}\bar{n} + \pi_q)q + \pi_s s \leq y\}, \tag{9}$$

where a tilde "∼" denotes functions in the restricted model.

The expenditure minimization problem in the restricted q^2 model is

$$\begin{aligned} \min_{q,s} \quad & \pi_n \bar{n} + (\pi_{nq}\bar{n} + \pi_q)q + \pi_s s, \\ \text{subject to} \quad & U(\bar{n}, q, s) \geq u. \end{aligned} \tag{10}$$

The corresponding expenditure function and compensated demand functions are

$$\tilde{e}(\pi, u, \bar{n}) = \min_{q,s}\{\pi_n\bar{n} + (\pi_{nq}\bar{n} + \pi_q)q + \pi_s s | U(\bar{n}, q, s) \geq u\}, \tag{11}$$

$$\tilde{x}^c(\pi, u, \bar{n}) = \arg\min_{q,s}\{\pi_n\bar{n} + (\pi_{nq}\bar{n} + \pi_q)q + \pi_s s | U(\bar{n}, q, s) \geq u\}. \tag{12}$$

To derive testable implications, we carry out the analysis in four steps. First, we link the restricted q^2 model and the unrestricted q^2 model. Second, we define two types of fertility changes. Third, we link the restricted q^2 model and the restricted q^1 model. Finally, we decompose the derivatives of q and s with respect to $\bar{n}$ in the restricted q^2 model into the derivatives of q, s, and n with respect to prices in the unrestricted q^1 model.[8]

In the restricted q^2 model, the shadow price of child quantity does not equal p_n if $\bar{n} \neq n^o$. Specifically, if $\bar{n} < n^o$, parents prefer more children, so the shadow price of child quantity is higher than p_n; if $\bar{n} > n^o$, parents prefer fewer children, so the shadow price is lower than p_n. We adjust the fixed price π_n (as a component of p_n) to equate p_n with the shadow price of n, inducing parents to choose $n = \bar{n}$ in the *unrestricted* q^2 model. The *supporting fixed price* $\bar{\pi}_n$ is defined by

$$\bar{n} = n^c(\pi_{-n}, \bar{\pi}_n, u), \tag{13}$$

where $\pi_{-n} = (\pi_{nq}, \pi_q, \pi_s)$, and $n^c(\pi_{-n}, \bar{\pi}_n, u)$ is the compensated demand function of n in the unrestricted q^2 model defined by Equation (6). The supporting fixed price $\bar{\pi}_n$ makes parents choose $n = \bar{n}$ in the expenditure minimization problem in the unrestricted q^2 model. In the following analysis, functions derived from the unrestricted q^2 model are always evaluated at $(\pi_{-n}, \bar{\pi}_n, u)$.

Because the first order conditions of q and s in the expenditure minimization problem in the

[8] The decomposition of derivatives in restricted models into derivatives in standard models helps deliver testable implications. This approach is pioneered by Becker and Lewis (1973). They decompose income and price elasticities of q and n in the q^2 model, which they call “observed” elasticities, into income and price elasticities of q and n in the q^1 model, which they call “true” elasticities.

restricted q^2 model and those in the unrestricted q^2 model (when $\pi = \bar{\pi}_n$) are the same, we have

$$\tilde{q}^c(\pi, u, \bar{n}) = q^c(\pi_{-n}, \bar{\pi}_n, u), \tag{14}$$

$$\tilde{s}^c(\pi, u, \bar{n}) = s^c(\pi_{-n}, \bar{\pi}_n, u). \tag{15}$$

By Equations (13), (14), and (15), we connect the expenditure functions in the restricted q^2 model and those in the unrestricted q^2 model,

$$\tilde{e}(\pi, u, \bar{n}) = e(\pi_{-n}, \bar{\pi}_n, u) + (\pi_n - \bar{\pi}_n)\bar{n}. \tag{16}$$

Differentiating Equation (16) with respect to $\bar{n}$, we have

$$\begin{aligned} \frac{\partial \tilde{e}}{\partial \bar{n}} &= \left(\frac{\partial e}{\partial \pi_n} - \bar{n}\right)\frac{\partial \bar{\pi}_n}{\partial \bar{n}} + (\pi_n - \bar{\pi}_n) \\ &= \pi_n - \bar{\pi}_n. \end{aligned} \tag{17}$$

The second equality in Equation (17) holds because (i) $\frac{\partial e}{\partial \pi_n} = n^c$ by the Envelope Theorem (Shephard's lemma) and (ii) $n^c = \bar{n}$ by Equation (13).

Similarly, the indirect utility function in the restricted q^2 model is connected with that in the unrestricted q^2 model,

$$\tilde{V}(\pi, y, \bar{n}) = V(\pi_{-n}, \bar{\pi}_n, y + (\bar{\pi}_n - \pi_n)\bar{n}). \tag{18}$$

Differentiating Equation (18) with respect to $\bar{n}$, we have

$$\begin{aligned} \frac{\partial \tilde{V}}{\partial \bar{n}} &= \left(\bar{n} + \frac{\partial V/\partial \bar{\pi}_n}{\partial V/\partial y}\right)\frac{\partial \bar{\pi}_n}{\partial \bar{n}}\frac{\partial V}{\partial y} + (\bar{\pi}_n - \pi_n)\frac{\partial V}{\partial y} \\ &= (\bar{\pi}_n - \pi_n)\frac{\partial V}{\partial y}. \end{aligned} \tag{19}$$

The second equality in Equation (19) holds because (i) $\frac{\partial V/\partial \bar{\pi}_n}{\partial V/\partial y} = -n(\pi_{-n}, \bar{\pi}_n, y + (\bar{\pi}_n - \pi_n)\bar{n})$ by the Roy's Identity, (ii) $n(\pi_{-n}, \bar{\pi}_n, y + (\bar{\pi}_n - \pi_n)\bar{n}) = n^c(\pi_{-n}, \bar{\pi}_n, u)$ by the duality theorem, and (iii) $n^c = \bar{n}$

by Equation (13). We assume that the shadow price of income is always larger than zero, namely $\frac{\partial V}{\partial y} > 0$.

Based on Equations (17) and (19), we have the following definitions.

Definition 1 *When $\bar{n} < n^o$, $\bar{\pi}_n - \pi_n > 0$, an increase in $\bar{n}$ is a* ***desired*** *fertility increase.*

Definition 2 *When $\bar{n} > n^o$, $\bar{\pi}_n - \pi_n < 0$, an increase in $\bar{n}$ is a* ***forced*** *fertility increase.*

When fertility is rationed below the unrestricted optimum ($\bar{n} < n^o$), parents prefer more children. The shadow price of child quantity ($\bar{\pi}_n + \pi_{nq}\tilde{q}^c$) is higher than the actual price ($\pi_n + \pi_{nq}\tilde{q}^c$). As such, $\bar{\pi}_n - \pi_n = (\bar{\pi}_n + \pi_{nq}\tilde{q}^c) - (\pi_n + \pi_{nq}\tilde{q}^c) > 0$. By Equations (17) and (19), $\partial\tilde{e}/\partial\bar{n} < 0$ and $\partial\tilde{V}/\partial\bar{n} > 0$. An increase in n reduces the minimal expenditure to achieve a given utility level or raises the maximal utility attainable at a given income level. In this case, we call the fertility increase a desired one. Similarly, when fertility is rationed above the unrestricted optimum ($\bar{n} > n^o$), parents prefer fewer children. The shadow price of child quantity ($\bar{\pi}_n + \pi_{nq}\tilde{q}^c$) is lower than the actual price ($\pi_n + \pi_{nq}\tilde{q}^c$). As such, $\bar{\pi}_n - \pi_n = (\bar{\pi}_n + \pi_{nq}\tilde{q}^c) - (\pi_n + \pi_{nq}\tilde{q}^c) < 0$. By Equations (17) and (19), $\partial\tilde{e}/\partial\bar{n} > 0$ and $\partial\tilde{V}/\partial\bar{n} < 0$. An increase in n either raises the minimal expenditure necessary to achieve a given utility level or reduces the maximal utility attainable at a given income level. In this case, we call the fertility increase a forced one.

Forced versus desired fertility changes are illustrated in Figure 1. Desired fertility changes move fertility towards the unrestricted optimal fertility level (n^0): fertility increases at $\bar{n} < n^o$ and reductions at $\bar{n} > n^o$ are desired. On the contrary, forced fertility changes move fertility away from n^0: fertility increases at $\bar{n} > n^o$ and reductions at $\bar{n} < n^o$ are forced.

By the duality theorem, we link uncompensated and compensated demand functions,

$$\tilde{q}(\pi, \tilde{e}(\pi, u, \bar{n}), \bar{n}) = \tilde{q}^c(\pi, u, \bar{n}), \tag{20}$$

$$\tilde{s}(\pi, \tilde{e}(\pi, u, \bar{n}), \bar{n}) = \tilde{s}^c(\pi, u, \bar{n}). \tag{21}$$

Differentiating Equations (20) and (21) with respect to $\bar{n}$ and invoking Equation (17), we have

$$\frac{\partial \tilde{q}}{\partial \bar{n}} = \frac{\partial \tilde{q}^c}{\partial \bar{n}} + (\bar{\pi}_n - \pi_n)\frac{\partial \tilde{q}}{\partial y}, \tag{22}$$

$$\frac{\partial \tilde{s}}{\partial \bar{n}} = \frac{\partial \tilde{s}^c}{\partial \bar{n}} + (\bar{\pi}_n - \pi_n)\frac{\partial \tilde{s}}{\partial y}. \tag{23}$$

Following Becker and Lewis (1973) and Rosenzweig and Wolpin (1980), we further decompose $\frac{\partial \tilde{q}}{\partial \bar{n}}$ and $\frac{\partial \tilde{s}}{\partial \bar{n}}$ into derivatives in the q^1 model. The expenditure minimization problem in the restricted q^1 model is

$$\begin{aligned} \min_{q,s} \quad & \pi_n^* \bar{n} + \pi_q^* q + \pi_s^* s, \\ \text{subject to} \quad & U(\bar{n}, q, s) \geq u, \end{aligned} \tag{24}$$

where π_n^*, π_q^*, and π_s^* denote the actual prices in the non-interactive (q^1) model. We use the superscript "$*$" to denote functions in the q^1 model. The corresponding expenditure function and compensated demand functions are

$$\tilde{e^*}(\pi^*, u, \bar{n}) = \min_{q,s}\{\pi_n^* \bar{n} + \pi_q^* q + \pi_s^* s | U(\bar{n}, q, s) \geq u\}, \tag{25}$$

$$\tilde{x^{*c}}(\pi^*, u, \bar{n}) = \arg\min_{q,s}\{\pi_n^* \bar{n} + \pi_q^* q + \pi_s^* s | U(\bar{n}, q, s) \geq u\}, \tag{26}$$

where $\pi^* = (\pi_n^*, \pi_q^*, \pi_s^*)$ for notational brevity.

If $\pi_q^* = \pi_{nq}\bar{n} + \pi_q$, $\pi_n^* = \pi_n$, and $\pi_s^* = \pi_s$, the expenditure minimization problem in the restricted q^2 model (Expression (10)) is equivalent to that in the restricted q^1 model (Expression (24)). Hence compensated demands of q and s in the two models are equal,

$$\tilde{q^c}(\pi, u, \bar{n}) = \tilde{q^{*c}}(\pi^*, u, \bar{n}), \tag{27}$$

$$\tilde{s^c}(\pi, u, \bar{n}) = \tilde{s^{*c}}(\pi^*, u, \bar{n}). \tag{28}$$

Using Equations (27) and (28) as well as Equations (19), (24), and (29) in Neary and Roberts

(1980, pp. 32-34), we have[9]

$$\frac{\partial \tilde{q}}{\partial \bar{n}} = \pi_{nq} \frac{\partial \tilde{q^{*c}}}{\partial \pi_q^*} + (1 - \alpha_\Delta \epsilon_{n^* \cdot y}) \frac{\partial \tilde{q^{*c}}}{\partial \bar{n}} + (\bar{\pi}_n - \pi_n) \frac{\partial q^*}{\partial y}, \tag{29}$$

$$\frac{\partial \tilde{s}}{\partial \bar{n}} = \pi_{nq} \frac{\partial \tilde{s^{*c}}}{\partial \pi_q^*} + (1 - \alpha_\Delta \epsilon_{n^* \cdot y}) \frac{\partial \tilde{s^{*c}}}{\partial \bar{n}} + (\bar{\pi}_n - \pi_n) \frac{\partial s^*}{\partial y}, \tag{30}$$

where $\alpha_\Delta = \frac{(\bar{\pi}_n - \pi_n)\bar{n}}{y}$ and $\epsilon_{n^* \cdot y} = \frac{\partial n^*}{\partial y} \frac{y}{n^*}$. Note that α_Δ is the share of compensating income change,[10] $(\bar{\pi}_n - \pi_n)\bar{n}$, out of total monetary income y. $\alpha_\Delta > 0$ if child quantity is rationed below the unrestricted optimum ($\bar{n} < n^o$), and $\alpha_\Delta < 0$ if child quantity is rationed above ($\bar{n} > n^o$). The income elasticity of child quantity is given by $\epsilon_{n^* \cdot y}$. If $\bar{n} = n^o$, $\bar{\pi}_n = \pi_n$. Equations (29) and (30) are reduced to Equations (18) and (19) in Rosenzweig and Wolpin (1980, p. 231).[11] Importantly, Equation (29) enables us to derive the effect of $\bar{n}$, *i.e.*, rationed fertility, on q, even though a comparative static analysis of the effect of n is not possible.

3.3 The Effect of Rationed Fertility on Child Quality

In this subsection, we discuss the effect of rationed fertility $\bar{n}$ on child quality q.[12] We assume that a child is a normal good (e.g., Black et al., 2013). As such, $\frac{\partial q^*}{\partial y} > 0$ and $\frac{\partial n^*}{\partial y} > 0$.

The first term on the right hand side of Equation (29), $\pi_{nq} \frac{\partial \tilde{q^{*c}}}{\partial \pi_q^*}$, is the *rationing price effect*, *i.e.*, the response of child quality to fertility-induced change in its own price. The rationing price effect is negative if $\pi_{nq} > 0$, since $\frac{\partial \tilde{q^{*c}}}{\partial \pi_q^*}$ is an own-price effect (Hicks' substitution effect) that is always negative. Intuitively, given that $\pi_q^* = \pi_{nq}\bar{n} + \pi_q$, an exogenous increase in child quantity $\bar{n}$

[9] See Appendix A1 for details.

[10] Note that $(\bar{\pi}_n - \pi_n)\bar{n}$ appears in Equation (18). When π_n is adjusted to the supporting fixed price $\bar{\pi}_n$, income y should be adjusted to $y + (\bar{\pi}_n - \pi_n)\bar{n}$ to induce the unrestricted household to choose $\bar{n}$.

[11] Rosenzweig and Wolpin (1980) are the first to decompose derivatives of q and s with respect to $\bar{n}$ in the restricted q^2 model into derivatives of q, s, and n with respect to prices in the unrestricted q^1 model. They note that the method of decomposing derivatives in a restricted model into derivatives in an unrestricted model is analogous to the one used in rationing theory in Tobin and Houthakker (1950), which enables them to conduct a comparative static analysis only at the unrestricted optimal fertility level ($\bar{n} = n^o$). As a generalization of Tobin and Houthakker's (1950) rationing theory, Neary and Roberts (1980) apply the duality techniques to evaluate functions both at and off the unrestricted optimum. We adopt their duality techniques to extend Rosenzweig and Wolpin (1980)'s analysis. In this manner, we are able to evaluate derivatives of q and s with respect to $\bar{n}$ both at and off the unrestricted optimal fertility level (*i.e.*, $\bar{n} = n^o$ and $\bar{n} \neq n^o$), respectively.

[12] The effect of rationed fertility $\bar{n}$ on the composite good s can be analyzed in a similar way. We discuss the effect of rationed fertility on parental consumption and labor supply in Section 6.

by one unit shifts up the actual price of child quality by π_{nq}, and in turn reduces the demand for child quality ($\frac{\partial \tilde{q^{*c}}}{\partial \pi_q^*} < 0$). The negative rationing price effect is a direct implication of the interactive budget constraint formulated in Becker and Lewis (1973).

The second term on the right hand side, $(1 - \alpha_\Delta \epsilon_{n^* \cdot y})\frac{\partial \tilde{q^{*c}}}{\partial \bar{n}}$, represents the *rationing substitution effect*, *i.e.*, the effect of substitution between fertility and child quality. The substitution effect arises because both $\bar{n}$ and q enter the parental utility function. Note that $\frac{\partial \tilde{q^{*c}}}{\partial \bar{n}}$ can be further decomposed into derivatives in the unrestricted q^1 model (see Appendix A1):

$$\frac{\partial \tilde{q^{*c}}}{\partial \bar{n}} = \frac{\partial q^{*c}}{\partial \pi_n^*}\left(\frac{\partial n^{*c}}{\partial \pi_n^*}\right)^{-1}, \tag{31}$$

where $q^{*c}(p, u)$ and $n^{*c}(p, u)$ are standard Hicksian demand functions in the unrestricted q^1 model. Note that $\frac{\partial n^{*c}}{\partial \pi_n^*} < 0$ since it is an own-price effect (Hicks' substitution effect). Meanwhile, $\frac{\partial q^{*c}}{\partial \pi_n^*}$ is a cross-price effect; if q and n are net substitutes, then $\frac{\partial q^{*c}}{\partial \pi_n^*} > 0$, which implies $\frac{\partial \tilde{q^{*c}}}{\partial \bar{n}} < 0$; if q and n are net complements, then $\frac{\partial q^{*c}}{\partial \pi_n^*} < 0$, which implies $\frac{\partial \tilde{q^{*c}}}{\partial \bar{n}} > 0$. Rosenzweig and Wolpin (1980) first derive the substitution effect, and conclude that the effect of an exogenous increase in fertility on child quality is not necessarily negative if n and q are net complements. Mogstad and Wiswall (2016) use the substitution effect to interpret the heterogeneous marginal effects of $\bar{n}$ on q for changes in $\bar{n}$ at different fertility levels.

In the second term, $[1 - \alpha_\Delta \epsilon_{n^* \cdot y}]$ is a weighting coefficient, which equals one if $\bar{n} = n^o$. This special case is considered in Rosenzweig and Wolpin (1980). We expect α_Δ, the share of compensating income change out of total monetary income, to be small such that $1 - \alpha_\Delta \epsilon_{n^* \cdot y} > 0$. If child quantity is rationed below the unrestricted optimum, then $\alpha_\Delta \epsilon_{n^* \cdot y} > 0$, and the effect of substitution between $\bar{n}$ and q is mitigated. If child quantity is rationed above the unrestricted optimum, then $\alpha_\Delta \epsilon_{n^* \cdot y} < 0$, and the effect of substitution between $\bar{n}$ and q is magnified.

The third term, $(\bar{\pi}_n - \pi_n)\frac{\partial q^*}{\partial y}$, is the *rationing income effect* of an exogenous fertility increase. The rationing income effect differentiates our model from those in Becker and Lewis (1973) and Rosenzweig and Wolpin (1980). The shadow price of $\bar{n}$ is $\pi_{nq} q + \bar{\pi}_n$, and the actual price is $\pi_{nq} q + \pi_n$. As such, $\pi_{nq} q + \bar{\pi}_n > \pi_{nq} q + \pi_n$ if and only if $\bar{\pi}_n > \pi_n$. If $\bar{n}$ is rationed below the unrestricted optimum, the shadow price is greater than the actual price ($\bar{\pi}_n > \pi_n$). In this case, a fertility increase is *desired*.[13] It raises the maximal attainable utility level or real income of parents, and thus increases child quality ($(\bar{\pi}_n - \pi_n)\frac{\partial q^*}{\partial y} > 0$). On the contrary, if $\bar{n}$ is rationed above the unrestricted optimum, the shadow price is less than the actual price ($\bar{\pi}_n < \pi_n$), a fertility increase is *forced*.[14] It reduces the maximal attainable utility level or real income of parents, and thus reduces child quality ($(\bar{\pi}_n - \pi_n)\frac{\partial q^*}{\partial y} < 0$).[15]

In summary, the response of child quality to an exogenous fertility increase can be decomposed into three terms. The first term is the rationing price effect, which is always negative. The second term is the rationing substitution effect, which is positive if q and n are net complements, and negative if they are net substitutes. The third term is the rationing income effect, which is positive for a desired fertility increase, and negative for a forced fertility increase. Importantly, the rationing income effect gives rise to two implications, like two sides of the same coin.

Implication 1 *Compared with a desired fertility increase, a forced fertility increase is more likely to reduce child quality.*

Implication 2 *Compared with a desired fertility reduction, a forced fertility reduction is less likely to increase child quality.*

3.4 Decomposing the Effect of Rationed Fertility on Child Quality: A Simulation

To illustrate the role of the rationing income effect in the determination of the heterogeneous effects of fertility on child quality, we simulate a parametric version of the restricted q^2 model. Following Mogstad and Wiswall (2016), we adopt a nested constant elasticity of substitution (CES) structure

[13] See Definition 1 in Section 3.

[14] See Definition 2 in Section 3.

[15] Equation (29) shows the effect of an infinitesimal change in $\bar{n}$ on q. However, fertility changes are discrete in reality. In particular, at $\bar{n} = n^o$, a fertility increase shifts child quantity away from the unrestricted optimum by one unit, *i.e.*, fertility increases from $\bar{n} = n^o$ to $\bar{n} = n^o + 1$; at $\bar{n} = n^o + 1$, parents would prefer fewer children ($\bar{\pi}_n < \pi_n$). Therefore, at $\bar{n} = n^o$, a fertility increase is forced.

for preferences,

$$U(\bar{n}, q, s) = U_1^{\theta} s^{1-\theta},$$

$$\text{and} \quad U_1 = (\alpha \bar{n}^{\rho} + (1-\alpha) q^{\rho})^{\frac{1}{\rho}},$$

where $\theta \in (0, 1)$, $\alpha \in (0, 1)$, and $\rho \in (-\infty, +\infty)$. U_1 is a CES aggregate of rationed child quantity $\bar{n}$ and child quality q. The elasticity of substitution between n and q is given by $\sigma = \frac{1}{1-\rho}$. The budget constraint is $\pi_{nq}\bar{n}q + \pi_n\bar{n} + \pi_q q + \pi_s s \leq y$. We set $\theta = 0.5$, $\alpha = 0.5$, $\rho = -3$, $\pi_{nq} = 1$, $\pi_q = 0$, $\pi_n = 0$, $\pi_s = 1$, and $y = 10$. Under this parametric setting, the unrestricted optimal fertility n^o is 1.83 children.

Figure A1 depicts child quality q against rationed fertility $\bar{n}$ where $\bar{n} \in [1, 5]$. When $\bar{n}$ is 1, q equals 1.61. When $\bar{n} < n^o$, q rises with $\bar{n}$. When $\bar{n} = n^o$, q is at the peak. When $\bar{n} > n^o$, q declines with $\bar{n}$. The inverted U-shaped line in Figure A1 is first shown in Figure 1 of Mogstad and Wiswall (2016, p. 161). While they ascribe the non-linear effect of $\bar{n}$ on q solely to the non-linear substitution effect, our generalized theory suggests that the heterogeneous effects of $\bar{n}$ on q are derived from a combination of three effects.

To interpret the heterogeneous effects using our general theory, we decompose the response of q to $\bar{n}$ by dividing Equation (29) by q. The semi-elasticity form is used to ensure that the results are robust to the unit of measurement of q. Figure 2 shows that the total effect is decomposed into three parts. The rationing price effect is negative; its magnitude declines with $\bar{n}$ but remains sizable for large $\bar{n}$. The rationing substitution effect is positive; its magnitude declines with $\bar{n}$ and approaches zero for large $\bar{n}$. The rationing income effect is positive for desired fertility increases ($\bar{n} < n^o$), negative for forced fertility increases ($\bar{n} > n^o$), and zero at the optimum ($\bar{n} = n^o$).

In our simulation, the total effect is primarily driven by the rationing income effect. The total effect is positive for fertility below the unrestricted optimum, and negative for fertility above the unrestricted optimum. Contrary to Mogstad and Wiswall's (2016) explanation, the inverted U-shaped relationship between q and $\bar{n}$ is not merely due to the rationing substitution effect. In fact, the rationing substitution effect is partly cancelled out by the rationing price effect, especially when

$\bar{n} < n^o$. Instead, the rationing income effect appears to drive the overall effect.[16]

4 Data and Empirical Strategy

4.1 One-child Policy, Twin Births, and Rationed Fertility

To test our theoretical implications, we combine twinning shocks with the unprecedented OCP.[17] If fertility is not rationed by the OCP (non-OCP regime), a twinning shock shifts fertility beyond a couple's unrestricted optimal level and thus induces a forced fertility increase. If fertility is rationed by the OCP (OCP regime), a twinning shock breaks the rationing of fertility, and helps the couple achieve the unrestricted optimal fertility level. Therefore, twinning shocks in the OCP regime are more likely to induce desired fertility increases relative to twinning shocks in the non-OCP regime. We discuss the two cases in the context of an instrumental variable interpretation below.

When the OCP is not in effect, fertility is not rationed, and parents can achieve their unrestricted optimal fertility level in the absence of twin births. Consider a couple who desires two children. If the second birth turns out to be twins, their fertility is shifted to three children, which is beyond the unrestricted optimal level of two children. The couple is a "complier" of twinning at the second birth as they would have stopped at two children in the absence of twinning. The twinning-induced fertility increase is a forced fertility increase for this couple. Now consider a different couple who desires three or more children. If the second birth turns out to be twins, their completed fertility would not change.[18] The couple is an "always taker" of twinning at the second birth. When twin birth is used as an instrumental variable for fertility, the estimated fertility effect on child quality

[16] We note that the perfect alignment of the sign of the rationing income effect with the sign of the total effect is a product of the specific parametric setting. This simulation illustrates that the rationing income effect is an important factor behind the heterogeneous effects.

[17] See Zhang (2016) for the evolution of the OCP.

[18] To simplify the discussion, we assume that twinning does not affect the desired fertility of parents. If this assumption is true, we should observe that twinning only shifts fertility at the parity of twin birth. Angrist, Lavy, and Schlosser (2010, p. 788) observe that "...twins instruments capture an average causal effect over a range of fertility variation that is close to the parity of the multiple birth." Even if this assumption is not true, our empirical strategy is still valid as long as the proportion of compliers that are forced to increase fertility when fertility is not rationed is similar across the treatment and comparison groups in our regression specifications.

captures the effect on "compliers", who are forced to increase fertility.[19] In the non-OCP regime, twinning-induced fertility increases capture forced fertility increases.

When the OCP is enforced, fertility is rationed, and some parents cannot achieve their unrestricted optimal fertility level in the absence of twin births. In rural China where we focus our empirical analyses, the OCP rations fertility at two, as shown in the statistical evidence presented below. The ration is not binding for a couple who desires two children, for whom twinning at the second birth induces a forced fertility increase. Importantly, the ration is binding for a couple who desires three or more children. In the absence of twinning, the couple's fertility is rationed at two children, which is below their unrestricted optimal level of three children. For this couple, the second birth being twins is an exogenous break of the OCP, shifting fertility back to the unrestricted optimal level (a desired fertility increase). In the OCP regime, twinning-induced fertility increases are a mix of forced and desired fertility increases. The theory implies that complying families in the non-OCP regime are more likely to reduce child quality than those in the OCP regime in the event of twinning at the second birth.

We exploit two variations in the OCP to test the theoretical implications. First, *Han* and minority Chinese received different treatments of the OCP. In the early stage of the policy, *Han* Chinese were targeted, whereas ethnic minorities, especially those in rural areas, were exempted until the early 1990s (Li, Yi, and Zhang, 2011). As such, fertility was rationed below the unrestricted optimal level for *Han* families who were subject to the OCP, but not rationed for minority Chinese. Second, the OCP was implemented in 1979. A couple's fertility was rationed by the OCP if the mother's primary childbearing period occurred after 1979.

Figure 3 shows trends of fertility distributions of *Han* and minority Chinese based on China's population censuses in 1982, 1990, 2000, and 2005. The sample contains rural married women born between 1930 and 1965, who are at least 40 years old at the time of each census. Nearly all women have at least one child (sub-figure A). Among women born before 1950, who were at least

[19] Under an implicit assumption of unrestricted fertility, Mogstad and Wiswall (2016, p. 173) write that, "The rationale for using twins as instruments is that for some families, twin births increase the number of siblings beyond the desired family size."

29 years old when the OCP was implemented, nearly all have at least two children (Sub-figure B). The probability of having at least two children starts to fall for women born after 1950. The decline in probability is larger for *Han* Chinese compared with minority Chinese. However, even for *Han* women born after 1950, over 80 percent have at least two children.

The rationing of fertility in rural China is most pronounced at the two-to-three fertility margin for *Han* Chinese (sub-figure C). Among women born before 1945, who were at least 34 years old when the OCP was implemented, over 80 percent have at least three children, conditional on having at least two children. The probability of having at least three children falls rapidly for women born after 1945. The decline in probability is larger for *Han* Chinese compared with minority Chinese. For *Han* women born in 1965, only 30 percent have three or more children. The fertility trends indicate that the OCP's rationing of fertility is effective at the two-to-three fertility margin in rural China.

Figure 3 also demonstrates that among women born before 1945, who had largely completed their fertility when the OCP was implemented in 1979, the fertility trends of *Han* and minority women were similar. The common trend suggests that minority Chinese is a valid comparison group for *Han* Chinese. Therefore, in the unique context of China's OCP, Implications 1 and 2 in Section 3 present two testable hypotheses.

Hypothesis 1 (Han versus minority) *Under the OCP regime, a twining-induced fertility increase has a larger negative effect on child quality for minority Chinese than for Han Chinese.*

Hypothesis 2 (Before versus after) *A twining-induced fertility increase has a larger negative effect on child quality for Han mothers whose primary childbearing period occurred prior to the implementation of the OCP, than for Han mothers whose primary childbearing period occurred after the implementation of the OCP.*

4.2 China Population Censuses

Our primary data sets are the one percent samples of the 1982 and 1990 China population censuses.[20] Both censuses were conducted by the National Bureau of Statistics. The 1982 census covers 10,039,191 individuals from 2,428,658 households, while the 1990 census covers 11,835,947 individuals from 3,152,818 households. Each data set contains a record for each household, which in turn includes a record for each individual residing in the household. The individual variables include demographic characteristics, educational attainment, ethnicity, marital status, and fertility.

4.2.1 Testing Hypothesis 1

To obtain the sample to test Hypothesis 1, we impose a series of restrictions on the 1990 census data. First, we drop urban families, because there are not enough observations of twin births in urban minority families, who became subject to the OCP from the late 1980s. We identify a family as urban if the household head has a non-agricultural *hukou*, and rural if the household head has an agricultural *hukou*.[21]

Second, we keep families with twins at the second birth, and families that do not have twin births but have at least two children. As shown in Figure 3, *Han* Chinese in rural areas were generally allowed to have two children. As such, rationing mainly occurs on the two-to-three fertility margin for rural *Han* families.

Third, we follow Li, Zhang, and Zhu (2008) to minimize sample selection. Census data do not record children who have left home. To ensure that no children have left home, we keep households where all children coreside. To minimize sample selection due to the requirement of the co-residence of all children, we keep households where the oldest child is younger than 17 years old, and households where the mother is younger than 35 years old. Consequently, the

[20] We extract the data sets from Minnesota Population Center (2014).

[21] In the early 1950s, the Chinese government established the *hukou* (household registration) system to control population flow. This system requires a person to be registered according to locality at birth. Each household has a registration certificate (*hukouben*) that records all members of the household. All administrative activities, such as land distribution, the issuance of ID cards, and the registration of a child in school, are based on the registration certificate. Until the early 1990s, the registration certificate was also used to distribute food, cooking oil, and clothing coupons. The *hukou* system has made moving across localities very restrictive in both urban and rural areas of China.

mothers in our data set were at most 24 years old when the OCP was implemented in 1979. We also drop households where the mother was younger than 15 at the first birth, as those entries either represent teen births or data error.

Fourth, we drop families in Tibet or Xinjiang, since the population policy in those two provinces is quite different from the OCP enforced in the rest of China.

Fifth, we keep children between the ages of six and 13,[22] whose primary school attendance and school enrollment status are taken as measures of child quality. The variable "Primary school attendance" equals one if the child has ever attended primary school, and equals zero otherwise. The variable "School enrollment" equals one if the child was enrolled in school during the census year, and zero otherwise.

The sampling frame yields 282,734 families with 457,164 children. Columns (1) and (2) of Table 1 show the descriptive statistics of the sample derived from the 1990 census. Families have on average 2.5 children, and 92 percent of them are *Han* families. A family is defined as a *Han* family if both parents are *Han*; otherwise, the family is defined as a minority family. The possibility of twinning at the second birth is 0.65 percent. On average, fathers are about 34 years old with 7.6 years of schooling, while mothers are 32 years old with 5.2 years of schooling. Children are on average 8.5 years old, and 50 percent of them are boys. The possibility of primary school attendance and the possibility of school enrollment are both around 0.8.

To examine Hypothesis 1, we first estimate the effect of rationed fertility on child quality for *Han* and minority Chinese, respectively. The estimated equation is,

$$Y_{ij} = \alpha_0 + \alpha_1 N_j + \mathbf{X_i}\alpha_2 + \mathbf{C_{ij}}\alpha_3 + \epsilon_{ij}, \tag{32}$$

where Y_{ij} is an indicator variable of primary school attendance or school enrollment status of child

[22] All empirical results are robust if we use children between the ages of six and 17. For children between the ages of six and 17, 95 percent of them are between the ages of six and 13. The low proportion of children between the ages of 14 and 17 suggest that families having children between the ages of 14 and 17 might be a selective sample. Moreover, primary school attendance, which is our main outcome variable, is not an informative indicator of child quality for children between the ages of 14 and 17. Hence we focus on children between the ages of six and 13, for whom primary school attendance can be taken as the main outcome of interest.

i in family j, and N_j is the number of children in family j. We use an indicator variable of twinning at the second birth T_j to instrument for N_j. $\mathbf{X_j}$ is a vector of family-level control variables, including maternal age at the second birth, parents' years of schooling, age, and age squared. $\mathbf{C_{ij}}$ is a vector of child-level control variables including the child's gender, age, age squared, and age cubed. We also control for prefecture fixed effects. The error term is given by ϵ_{ij}. Because fertility was rationed for *Han* Chinese but not for minority Chinese, Hypothesis 1 predicts that the magnitude of the negative fertility effect should be smaller for *Han* Chinese than for minority Chinese, *i.e.*, α_1 is larger for *Han* Chinese than for minority Chinese.

To directly test if α_1 differs for *Han* and minority Chinese, we include an interaction term of the *Han* indicator and child quantity,

$$Y_{ij} = \beta_0 + \beta_1 N_j + \beta_2 N_j \cdot Han_j + \beta_3 Han_j + \mathbf{X_i}\beta_4 + \mathbf{C_{ij}}\beta_5 + \epsilon_{ij}, \tag{33}$$

where Han_j is an indicator variable that equals one if both parents in family j are *Han* Chinese, and equals zero if either the father or mother belongs to a minority group. We use T_j and $T_j \cdot Han_j$ to instrument for N_j and $N_j \cdot Han_j$. This first-stage specification allows the effect of twinning on child quantity to differ between *Han* and minority families. The effect of rationed fertility on child quality for minority Chinese is captured by β_1, while the effect for *Han* Chinese is shown by $\beta_1 + \beta_2$. As discussed, β_1 captures the effect of forced fertility increases, while $\beta_1 + \beta_2$ captures the effect of a mix of forced and desired fertility increases. Hypothesis 1 implies that $\beta_2 > 0$.

When using twinning at the *n*th birth as the instrumental variable of child quantity, Black, Devereux, and Salvanes (2005, 2010) and Angrist, Lavy, and Schlosser (2010) include children born before the *n*th birth in the estimation sample. This method estimates a lower bound of the negative effect of rationed fertility on average child quality due to two reasons. First, parents reinforce lower-parity children's superior endowment over twins, cancelling out the negative effect on child quality (Rosenzweig and Zhang, 2009). Second, children born at lower birth parities share parental resources more exclusively, and are thus less likely to be affected by fertility increase at higher parities (Guo, Yi, and Zhang, 2016). As such we include all children in the estimation sample. However, including all children may overestimate the negative effect of rationed fertility on average child quality, since the included twins have inferior endowments compared with non-twins.[23] The instrumental variable estimate using only low-parity children serves as the lower bound of the negative effect of rationed fertility on child quality, while the instrumental variable estimate including all children serves as the upper bound. For each sample, $\beta_2 > 0$ is still a valid test as long as the magnitude of the potential bias does not differ significantly for *Han* and minority Chinese. We report the regression results using only first-born children in the Appendix. We further discuss the magnitude of the potential bias in Section 5.[24]

4.2.2 Testing Hypothesis 2

To test Hypothesis 2, we need a merged sample of the 1982 and 1990 censuses. The 1982 census sample in the merged data set contains mothers whose primary childbearing period occurs before the implementation of the OCP, while the 1990 census sample contains mothers whose primary childbearing period occurs after the implementation of the OCP. For ease of reference, we call the 1982 census sample the "before-OCP sample," the 1990 census sample the "after-OCP sample," and the merged sample the "before-after-OCP sample."

We impose a series of sample restrictions on the 1982 and 1990 censuses, which are similar to the restrictions we carried out for the 1990 census. The differences are as follows. First, because

[23] Twins have inferior endowment relative to non-twins, primarily because twins have lower birthweight than non-twins. See Behrman and Rosenzweig (2004) and Almond, Chay, and Lee (2005) for the impact of birthweight on individual outcomes. Rosenzweig and Zhang (2009) solve this problem by controlling for children's birthweight. However, the 1982 and 1990 censuses do not contain birthweight information.

[24] Another source of potential bias comes from the reporting of fake twins. Huang, Lei, and Zhao (2016) find that parents tend to report regularly spaced children as twins to escape the OCP. Parents who report fake twins have a stronger preference for child quantity than those who do not report. Such fake twinning is thus positively correlated with the preference for child quantity, which can be considered an omitted variable in Equation (32). To pin down the potential bias resulting from reporting fake twins, we rewrite Equation (32) as $Y = \alpha_0 + \alpha_1 N + \xi\mu + \sigma$, where μ represents the preference for child quantity. A larger μ means a stronger preference for child quantity. The instrument of twinning at the second birth (T) is positively correlated with the preference for child quantity (μ) for *Han* Chinese in the OCP regime, namely, $\text{Cov}(T, \mu) > 0$. The biology literature documents that organisms producing more offsprings tend to have lower offspring quality (Pianka, 1970). Parents having stronger preference for child quantity should have weaker preference for child quality, which means $\xi < 0$. The instrumental variable estimate of α_1 is $\hat{\alpha}_1 = \frac{\text{Cov}(Y,T)}{\text{Cov}(N,T)} = \alpha_1 + \xi\frac{\text{Cov}(\mu,T)}{\text{Cov}(N,T)}$. As $\text{Cov}(\mu, T) > 0$, $\text{Cov}(N, T) > 0$, and $\xi < 0$, we have $\hat{\alpha}_1 < \alpha_1$. The presence of fake twins thus introduces a downward bias for the instrumental variable estimate of α_1 for *Han* Chinese. As a result, the presence of fake twins biases the instrumental variable estimate of β_2 downward, making $\beta_2 > 0$ an even stronger test.

the 1982 census does not contain individual *hukou* status, we cannot define a family as rural based on the household head's *hukou*. To get a consistent definition in both censuses in the merged sample, we define a family as rural if the household head was employed in an agricultural sector. An inspection of the 1990 census shows that 86.79 percent of household heads with an agricultural *hukou* was employed in an agricultural sector, and 99.54 percent of household heads employed in an agricultural sector had an agricultural *hukou*. Second, because the 1982 census does not have information on individual school enrollment status, we use "Primary school attendance" alone as a measure of child quality in the merged sample.

The sampling frame yields 245,233 families with 398,762 children in the after-OCP sample, and 198,798 families with 386,306 children in the before-OCP sample. Because we use agricultural employment, and not agricultural *hukou*, to define a rural family, the after-OCP sample (columns (3)–(4) of Table 1) has a smaller sample size compared with the 1990 census sample used to test Hypothesis 1 (columns (1)–(2) of Table 1). The two samples are otherwise similar to each other. Columns (5)–(6) of Table 1 show the descriptive statistics of the before-OCP sample. Parents in the before-OCP sample are more likely to have more children and lower education levels compared with parents in the after-OCP sample. Children in the before-OCP sample are less likely to attend primary school than those in the after-OCP sample.

To examine Hypothesis 2, we first estimate Equation (32) for *Han* Chinese in both the before-OCP sample and the after-OCP sample. Twining at the second birth is used to instrument for child quantity. We then include an interaction term of the OCP indicator and child quantity,

$$Y_{ij} = \gamma_0 + \gamma_1 N_j + \gamma_2 N_j \cdot OCP_j + \gamma_3 OCP_j + \mathbf{X_i}\gamma_4 + \mathbf{C_{ij}}\gamma_5 + \epsilon_{ij}, \tag{34}$$

where OCP_j is an indicator variable that equals one if family j is in the after-OCP sample, and equals zero if family j is in the before-OCP sample. We use T_j and $T_j \cdot OCP_j$ to instrument for N_j and $N_j \cdot OCP_j$. This first-stage specification allows the effect of twinning on child quantity to vary in each census. The effect of rationed fertility on child quality in the pre-OCP sample is represented by γ_1, while the effect in the post-OCP sample is represented by $\gamma_1 + \gamma_2$. As discussed,

γ_1 captures the effect of forced fertility increases, while $\gamma_1 + \gamma_2$ captures the effect of a mix of forced and desired fertility increases. Hypothesis 2 implies that $\gamma_2 > 0$.

4.3 Chinese Twins Surveys

We use two unique twins surveys to conduct a robustness analysis. The first is the Chinese Adult Twins Survey (CATS), in which all children were born prior to the implementation of the OCP. The second is the Chinese Child Twins Survey (CCTS), in which all children were born after the implementation of the OCP. Comparing the estimated effect of rationed fertility on child quality using the two data sets, CATS and CCTS, allows a robustness test of Hypothesis 2.

CATS and CCTS have several advantages over census data. First, CATS and CCTS have better measures on child quality than the 1982 and 1990 censuses. CATS contains completed years of schooling of all children in each family, while CCTS contains expected years of schooling, parental investment in each child, and parental home tutorial time as measures of child quality.[25] Second, CCTS contains information on each child's birthweight, which enables us to control for intra-household resource allocation.

CATS was carried out by the Urban Survey Unit of the National Bureau of Statistics in June and July 2002 in five cities of China. The local statistical bureaus identified same-sex adult twins between the ages of 18 and 65 via various channels, including colleagues, friends, relatives, newspaper advertising, neighborhood notices, neighborhood management committees, and household records from the local public security bureau. Overall, these channels permit a roughly equal probability of contacting all twins in these cities, implying that the CATS sample is arguably representative. In addition, the Urban Household Survey sampling frame was used to obtain a comparable sample of non-twin adults between the ages of 25 and 65 in the same neighborhoods. See Li, Rosenzweig, and Zhang (2010) for a detailed description of CATS.

CCTS was carried out by the Urban Survey Unit of the National Bureau of Statistics in late 2002 and early 2003 in Kunming, the capital city of an underdeveloped province in China. The

[25] We also use CCTS to study the effect of rationed fertility on parental consumption and labor supply in Section 6.

Urban Survey Unit initially identified 2,300 households with twins between the ages of 7 and 18 from the 2000 population census as the target sample. Of this target sample, 1,694 households were successfully interviewed. The Urban Survey Unit also interviewed 1,693 neighboring households with non-twin children as a comparison group. CCTS contains comprehensive information on parental and child expenditures, parental labor supply, and child education. See Rosenzweig and Zhang (2009) for a detailed description of CCTS.

Appendix Table A1 and Table A2 present summary statistics for CATS and CCTS, respectively. Appendix A2 shows the corresponding empirical specifications for the analysis of these two data sets.

5 Empirical Results

5.1 Han versus Minority

Table 2 shows the tests of Hypothesis 1 using the 1990 census sample. Columns (1) and (2) show the instrumental variable estimates of Equation (32) for *Han* and minority Chinese, respectively.[26] For *Han* Chinese (column (1)), the fertility effect on primary school attendance is positive, although statistically insignificant. By contrast, an additional child reduces the likelihood of primary school attendance by 8.9 percentage points for children in minority families (column(2)). This negative effect is statistically significant at the five percent level.

To test if the fertility effect differs for *Han* and minority Chinese, we estimate Equation (33) using the pooled sample of *Han* and minority Chinese (columns (3) and (4)). In column (3), we add the same set of control variables as in columns (1) and (2). An additional child reduces the likelihood of primary school attendance by 9.6 percentage points more for children in minority families than children in *Han* families ($\beta_2 > 0$). The differential fertility impact is statistically significant at the ten percent level. In column (4), we allow the coefficients for the control variables to differ for *Han* and minority Chinese. The estimates are similar to those in column (3). Specifically,

[26] Twinning at the second birth is a strong predictor of child quantity in the first-stage regressions (Panel A of Table A3). The t-statistics of the coefficient of twinning are 41.4 and 12.8 for *Han* and minority Chinese, respectively.

an additional child reduces the likelihood of primary school attendance by 9.1 percentage points more for children in minority families than children in *Han* families ($\beta_2 > 0$). The differential fertility impact is statistically significant at the five percent level.

In columns (5)–(8), we use school enrollment status as an alternative measure of child quality. The estimates are similar to those in columns (1)–(4) where primary school attendance is used to measure child quality. An additional child reduces the likelihood of school enrollment more for children in minority families than children in *Han* families ($\beta_2 > 0$).

Because twins have inferior endowment compared with non-twins, including all children in the instrumental variable regressions may over-estimate the negative fertility effect on child quality. As a robustness check, we report the instrumental variable estimates using first-born non-twin children in Table A4. Consistent with Rosenzweig and Zhang (2009) and Guo, Yi, and Zhang (2016), the estimated fertility effect for first-born non-twin children is smaller in magnitude compared with the analogous effect for all children. Importantly, in each specification, the estimate of β_2 using only first-born children is similar in magnitude to the estimate using all children. Therefore, the potential bias due to the inferior endowment of twins compared with non-twins is unlikely to drive the estimate of β_2.

Overall, the comparison between *Han* and minority Chinese strongly supports Hypothesis 1. When the OCP was in effect, a twinning-induced fertility increase implies a larger reduction in child quality for minority Chinese than for *Han* Chinese.

5.2 Before versus After One-child Policy

Columns (1)–(4) of Table 3 show the tests of Hypothesis 2 using *Han* families in the before-after-OCP sample.[27] Column (1) shows the instrumental variable estimate of Equation (32) using the before-OCP sample. An additional child reduces the likelihood of primary school attendance by 5.5 percentage points for children of *Han* Chinese. By contrast, the fertility effect is smaller in magnitude and statistically insignificant for the after-OCP sample.

[27] See Panel B of Table A3 for the first stage estimates. The t-statistics of the coefficient of twinning are 20.8 and 37.6 for *Han* Chinese in before-OCP and after-OCP samples, respectively.

To test whether the difference is statistically significant, we estimate Equation (34) for *Han* Chinese using the before-after-OCP sample. In column (3), we add the same set of control variables as in columns (1) and (2). An additional child reduces the likelihood of primary school attendance by 7.0 percentage points more for children in the before-OCP sample than children in the after-OCP sample ($\gamma_2 > 0$). The differential impact is statistically significant at the one percent level. In column (4), we allow the control variables to have differential effects in the before-OCP and after-OCP samples. The estimates are qualitatively similar to those in column (4). An additional child reduces the likelihood of primary school attendance by 4.8 percentage points more for children in the before-OCP sample than children in the after-OCP sample ($\gamma_2 > 0$). The differential impact is statistically significant at the five percent level.

The tests on Hypothesis 2 above might be confounded by the time effect, e.g., the effect of economic development concurrent with the implementation of the OCP. After the implementation of the OCP, the negative fertility effect on child quality might shrink simply because parents became richer. To address this concern, we conduct placebo tests by estimating the fertility effects for minority Chinese before and after the OCP. The OCP is binding for *Han* Chinese but not for minority Chinese. If the time effect rather than the rationing income effect is driving the results, we should observe $\gamma_2 > 0$ for both *Han* Chinese and minority Chinese. If the rationing income effect rather than the time effect drives the results, we should observe $\gamma_2 > 0$ only for *Han* Chinese, but not for minority Chinese.

Columns (5)–(8) of Table 3 show the placebo tests.[28] An additional child reduces child quality for minority Chinese in both the before-OCP and after-OCP samples (columns (1) and (2)). The effect is statistically insignificant in the before-OCP sample and statistically significant in the after-OCP sample at the ten percent level. The magnitude of the negative fertility effect on child quality is larger in the after-OCP sample than in the before-OCP sample, but the difference is statistically insignificant (columns (7) and (8)). Hence the differential fertility effect for *Han* Chinese in the before-OCP and after-OCP samples is unlikely to be driven by the time effect.

[28] See Panel C of Table A3 for the first-stage estimates. The *t*-statistics of the coefficient of twinning are 3.3 and 10.7 for minority Chinese in the before-OCP and after-OCP samples, respectively.

Overall, results in this subsection support Hypothesis 2. A twinning-induced fertility increase reduces child quality for *Han* Chinese more in the before-OCP sample than in the after-OCP sample.

5.3 Robustness

Children in the CATS sample were born before the implementation of the OCP. The fertility distributions in CATS are smooth (Figure A2), indicating no rationing of fertility. We expect twinning-induced fertility increases to represent forced fertility increases. We define the n-plus ($n = 2, 3, 4$) sample that consists of (i) families with twins at the nth birth, and (ii) families without twin births but with at least n children. In the n-plus sample, twinning at the nth birth is used as the instrumental variable for child quantity. Panel A of Table 4 presents the instrumental variable estimates of Equation (A2.1) (similar to Equation (32)) using CATS samples. In the n-plus sample, an additional sibling reduces years of schooling of children by 0.84, 0.62, and 1.1 years when n equals 2, 3, and 4, respectively. These effects are statistically significant at the one percent level.[29] Overall, results using the CATS data show that a forced fertility increase significantly reduces child quality.

Children in the CCTS sample were born after the implementation of the OCP. The OCP rations fertility at one child in the urban sample, and at two children in the rural sample, as shown in Figure A3. We expect twinning-induced fertility increases to represent a mix of forced and desired fertility increases. Following Rosenzweig and Zhang (2009), we examine the effect of twinning at the first birth on child quality using the urban sample, and at the second birth using the rural sample. Panels B and C of Table 4 report the estimates for the urban and rural sample, respectively. The effects of rationed fertility on expected years of schooling of children are negative in both samples (column (1) in Panels B and C). However, the negative effects are statistically insignificant. We

[29] We also use an alternative specification including only children born before the nth birth when using the n-plus sample ($n = 2, 3, 4$). Estimates of the fertility effects including only low-parity children (Panel A of Table A5) are similar to estimates including all children (Panel A of Table 4). We also study the heterogeneous effects of rationed fertility on years of schooling by birth order. Following Mogstad and Wiswall (2016), we estimate the fertility effect for children born at each birth order in the 3-plus and 4-plus samples. Panel B of Table A5 consistently shows a negative impact of rationed fertility on years of schooling for children at all birth orders. The estimates are statistically significant except for the second-born children in the 3-plus sample.

also fail to detect a statistically significant negative fertility effect on child investment (column (2) in Panels B and C). In fact, having an additional child increases parental home tutorial time in the urban sample (column (3) in Panel B). Overall, we find that the negative effect of rationed fertility on child quality is at best modest in the CCTS sample.

The modest effects of rationed fertility on child quality in the CCTS sample are consistent with previous findings. For example, Li, Zhang, and Zhu (2008), Rosenzweig and Zhang (2009), Liu (2014), and Li and Zhang (2016) document that a fertility increase has a relatively small negative effect, if any, on child quality in the OCP regime. Contrasting the large negative effects of the forced fertility increase identified in the CATS sample with the modest effects of rationed fertility identified in the CCTS sample further supports Hypothesis 2.

6 Parental Responses

Why the estimated fertility effect on child quality, such as our estimates using CCTS, can be so small? Angrist, Lavy, and Schlosser (2010) and Galor (2012) conjecture that parents may respond to an exogenous fertility increase by adjusting their consumption and labor supply to maintain child quality. The effect of rationed fertility on parental consumption and labor supply is an integral part of the literature on the effects of child quantity on family behavior (Browning, 1992). While many studies have examined the effect of child quantity on parental labor supply, there are few studies that examine the effect on parental consumption. Data sets with information on parental consumption rarely contain a large sample of twins.

In Section A3 we use a simulation similar to the one in Section 3.4 to demonstrate the effect of rationed fertility on parental consumption. The substitution effect between child quantity and parental consumption drives intrahousehold resource reallocation (Figure A4 and A5). In Section A4 we extend the q^2 model (Expression (1)) to incorporate parental time allocation. We show that in response to a mix of forced and desired fertility increases, parents work harder and consume less to maintain investment in their children.

The empirical analysis in this part uses data from the CCTS, in which twining indicates a mix of forced and desired fertility increases. For parental consumption, outcome variables include the father's monthly cigarette and alcohol expenses, the mother's cosmetics expenses in the last six months, parents' clothing expenses in the last six months, and indicator variables of whether parents ever dined out without their children. To measure parental labor supply, we use seven variables that capture different aspects: (i) an indicator variable of labor force participation, (ii) the number of days worked in the last month, (iii) the number of hours worked in the last week, (iv) monthly labor income, (v) hourly earnings, (vi) an indicator variable of managing a private business, and (vii) an indicator variable of leaving home for more than 30 days in the last 180 days. Table A6 reports summary statistics on parental consumption and labor supply for urban and rural samples in the CCTS, respectively. In both samples, average annual income of families with twins is similar to that of families without twins. Compared with parents of non-twins, parents of twins appear to have lower consumptions and higher labor supply at various margins in both samples.

To study the effect of rationed fertility on parental consumption and labor supply, we estimate the following equation,

$$Y_j = \theta_0 + \theta_1 T_j + \mathbf{X}\theta_2 + \epsilon_j, \tag{35}$$

where Y_j is an outcome variable in family j; T_j is an indicator of twinning at the first birth for the urban sample, and at the second birth for the rural sample; $\mathbf{X}$ is a vector of parental controls, including maternal age at the first birth for the urban sample and at the second birth for the rural sample, parents' years of schooling, age, and age squared; and ϵ_j is the error term.

Table 5 presents the effect of twinning on parental consumption. Panels A and B show the results for the urban and rural samples, respectively. Thirteen out of 14 estimates of the coefficient on twinning are negative. Eight of the 13 negative coefficients are statistically significant. Urban fathers of twins spend 23 percent less on clothing, and are 7.1 percentage points less likely to dine out without children.[30] Urban mothers of twins spend 20 percent less on cosmetics and 29 percent

[30] A large coefficient on twinning itself is a poor approximation of the percentage change in the outcome variable

less on clothing, and are 6.4 percentage points less likely to dine out without children. Rural fathers of twins spend 46 percent less on clothing. Rural mothers of twins spend 37 percent less on cosmetics and 46 percent less on clothing. Interestingly, we do not detect significant reduction in fathers' consumption of cigarette and alcohol (columns (2) and (3)) in both urban and rural samples, as cigarette and alcohol contain addictive substances.

Parents may also increase their labor supply while reducing their own consumption in the event of a mix of forced and desired fertility increases. Table 6 shows the effects of twinning on parental labor supply in the two samples. We find that urban parents adjust their labor supply after an exogenous fertility increase. Specifically, conditional on labor force participation, urban fathers of twins, compared with urban fathers of non-twins, work four percent more hours in the week prior to the survey, and earn eight percent higher labor income. Urban mothers of twins are five percentage points more likely to participate in the labor force than urban mothers of non-twins. Conditional on labor force participation, urban mothers of twins work five percent more hours in the week prior to the survey, and are 5.9 percentage points more likely to manage a private business. In the rural sample, both parents are more likely to manage a private business after an exogenous fertility increase.[31]

In general, our results suggest that in response to a mix of forced and desired fertility increases, parents work harder and consume less to maintain child quality.

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induced by twinning when the outcome variable is in log scale. Let $\ln y_1$ denote the value of the outcome variable when the twinning indicator equals one, and $\ln y_0$ when the twinning indicator equals zero. Then $\ln y_1 - \ln y_0 = \ln \frac{y_1}{y_0} = \ln\left(1 + \frac{y_1 - y_0}{y_0}\right) \approx \frac{y_1 - y_0}{y_0}$ for small $\frac{y_1 - y_0}{y_0}$. Let $\gamma = \ln y_1 - \ln y_0$ denote the coefficient on twinning. We use $\frac{y_1 - y_0}{y_0} = e^{\gamma} - 1$ to calculate the percentage change for a large $|\gamma|$. Specifically, a coefficient estimate of twinning of -0.611 in column (4) in Panel B of Table 5 means a $e^{-0.611} - 1 \approx -46$ percent change in the outcome variable.

[31] We also check if parental responses to twinning differ by the gender of the twins. See Appendix A5 for the exact empirical specification. We find that the negative effect of twinning on parental consumption is stronger if there are more boys in the twin-pair (Table A7). But the response of parental labor supply to twinning does not display a clear pattern of gender difference (Table A8).

7 Conclusion

While there are many empirical studies on the quantity-quality trade-off of children that exploit exogenous variations in fertility and examine the effect of exogenous fertility change on child quality, few theoretical analyses exist in the literature on the quantity-quality trade-off. In this paper, we present a general theory of exogenous fertility change in the context of child quantity-quality tradeoff. We show that a desired fertility increase, which shifts fertility towards the optimal level, generates a positive income effect, while a forced fertility increase, which shifts fertility away from the optimal level, generates a negative income effect. Hence a desired fertility increase is less likely to reduce child quality compared with a forced fertility increase.

We design a novel empirical strategy to test our general theory by combining the natural experiment of twin births with China's unprecedented OCP. The OCP changes the composition of compliers of twin births. Twinning mainly induces forced fertility increases for parents whose fertility was not rationed by the OCP, while it induces a mix of forced and desired fertility increases for parents whose fertility was rationed by the OCP. The theory implies that a twinning-induced fertility increase reduces child quality more for parents whose fertility was not rationed, relative to parents whose fertility was rationed.

We exploit two variations of the OCP on the rationing of fertility. First, we exploit the differential treatment of the OCP on *Han* and minority Chinese. The OCP rations fertility of *Han* Chinese, but not or less so on that of minority Chinese. We find a that twinning-induced fertility increase reduces child quality of minority Chinese whose fertility was not rationed by the OCP, while it does not reduce child quality of *Han* Chinese whose fertility was rationed by the OCP. Second, we exploit the timing of the OCP. We find that a twinning-induced fertility increase reduces child quality for *Han* Chinese before the OCP, but not after the OCP. Both tests present empirical evidence remarkably consistent with our theory. The second test is also validated using data from two large surveys of Chinese twins and non-twins.

We also examine how parents respond to a mix of forced and desired fertility increases when they try to avoid reducing child quality. We find that parents work harder and consume less to maintain child quality. These findings also explain the small fertility effect on child quality in the literature. Further, our theoretical and empirical results help reconcile the heterogeneous fertility effects on child quality recently discovered by, for example, Mogstad and Wiswall (2016) and Brinch, Mogstad, and Wiswall (2016).

Our findings have strong implications for population control policies. Forced birth planning policies is unlikely to enhance child quality. If promoting human capital investment is one of the main objectives of population control, then "voluntary" population control methods are preferred to "mandatory" policy instruments.

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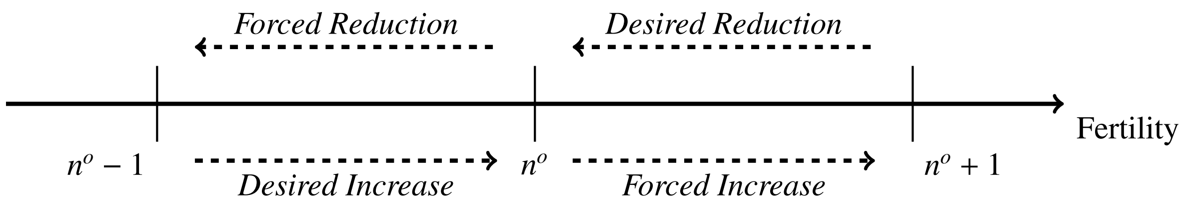

Figure 1: Forced versus desired fertility changes

Notes: n^o is the optimal fertility level by solving the utility maximization problem in Expression (1) when fertility is not restricted or rationed.

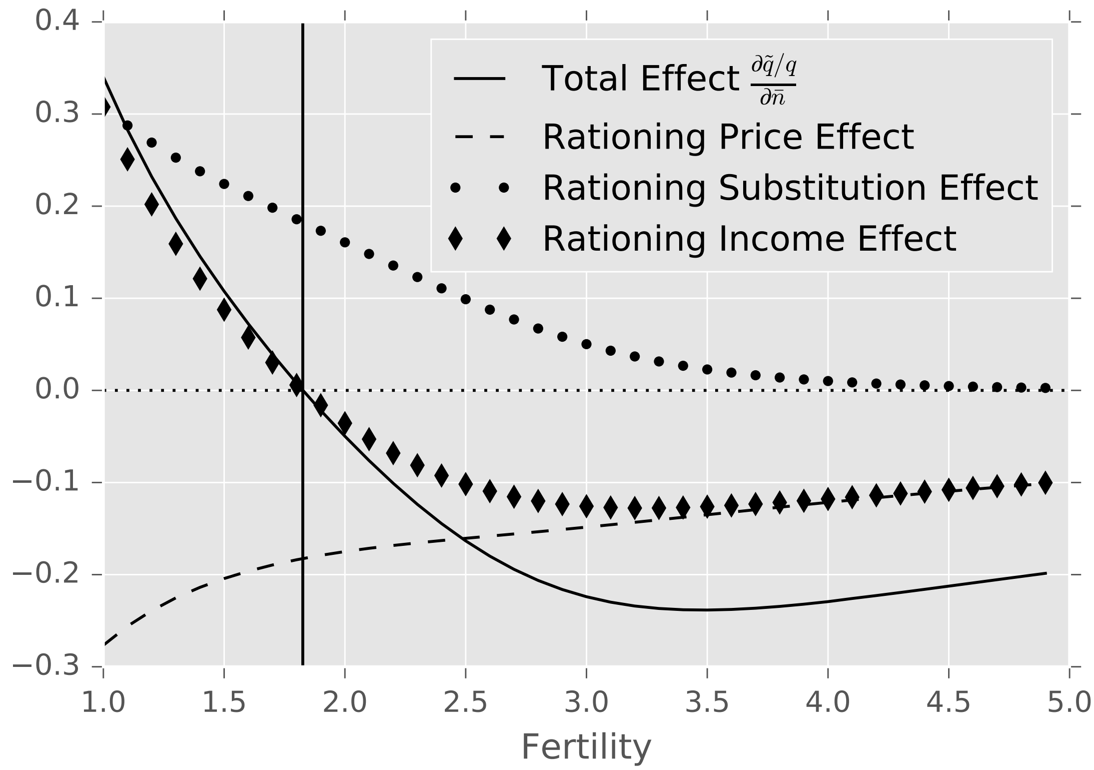

Figure 2: Decomposition of the marginal effect of rationed fertility $\bar{n}$ on child quality q

Notes: The x-axis shows the fertility of parents. The y-axis shows the semi-elasticity of child quality to child quantity ($\frac{\partial \tilde{q}/q}{\partial \bar{n}}$) and its decomposition terms (Equation (29) divided by q). In this figure, the Rationing Substitution Effect is $[1-\alpha_{\Delta}\epsilon_{n^*\cdot y}]\frac{\partial q^{\tilde{*}c}/q}{\partial \bar{n}}$, the Rationing Price Effect is $\pi_{nq}\frac{\partial q^{\tilde{*}c}/q}{\partial \pi_q^*}$, and the Rationing Income Effect is $(\bar{\pi}_n - \pi_n)\frac{\partial q^*/q}{\partial y}$. Using semi-elasticity forms, the results are insensitive to the unit of measurement of child quality q. The utility function adopts a nested CES parameterization: $U(\bar{n}, q, s) = U_1^{\theta} s^{1-\theta}$ and $U_1 = (\alpha \bar{n}^{\rho} + (1-\alpha) q^{\rho})^{\frac{1}{\rho}}$. The budget constraint is $\pi_{nq}\bar{n}q + \pi_n\bar{n} + \pi_q q + \pi_s s \leq y$. The model parameters are set at $\theta = 0.5$, $\alpha = 0.5$, $\rho = -3$, $\pi_{nq} = 1$, $\pi_q = 0$, $\pi_n = 0$, $\pi_s = 1$, and $y = 10$. The unrestricted optimal fertility n^o is 1.83, which is denoted by the vertical line.

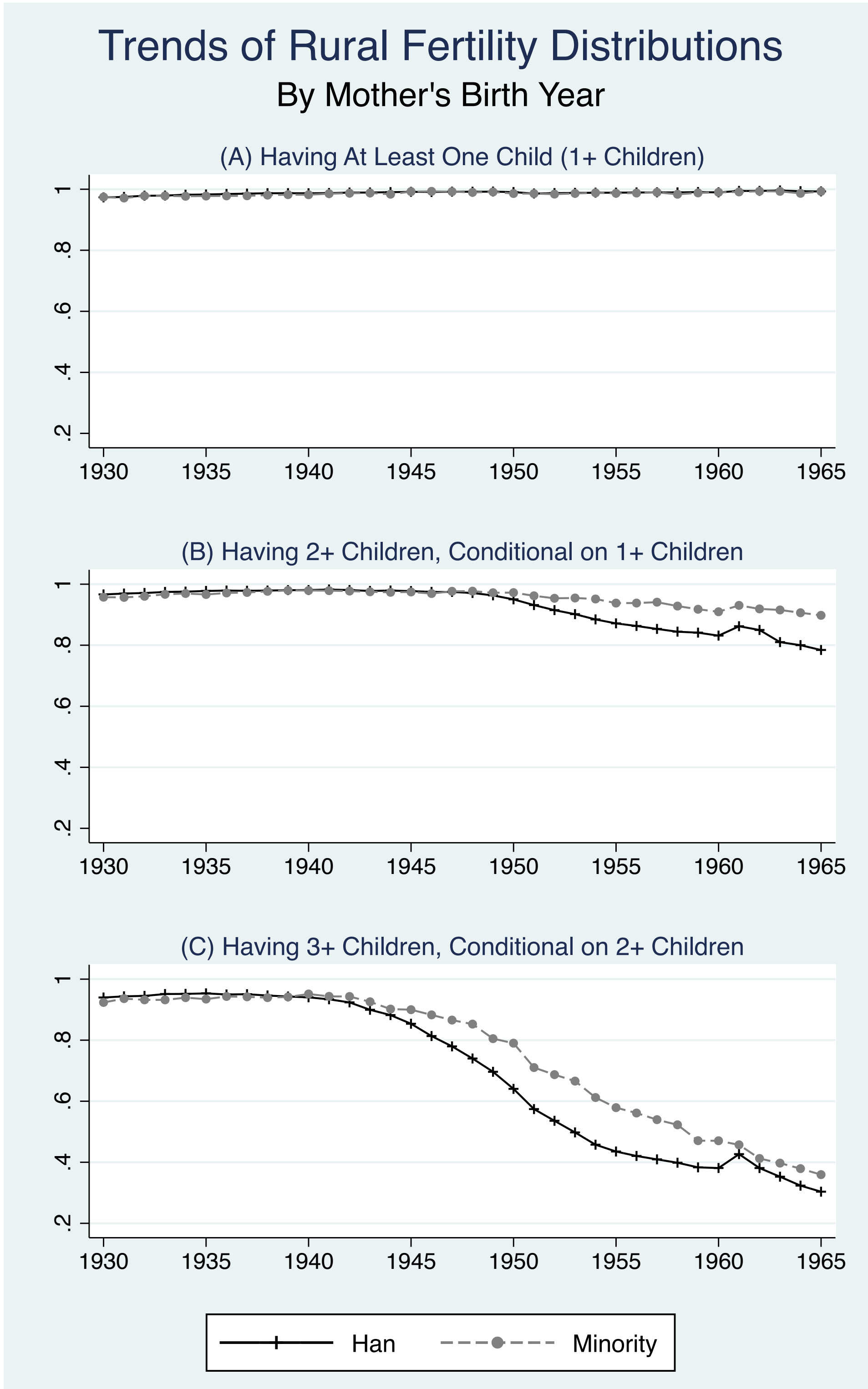

Figure 3: Trends of fertility distributions in rural China, Han versus minority

Notes: Based on the number of surviving children reported by rural married women aged 40 or above in the 1982, 1990, 2000, and 2005 China population censuses. We exclude mothers who had their first birth before the age of 15. We also exclude mothers in Xinjiang and Tibet.

Table 1: Summary statistics, 1982 and 1990 China population censuses

	Agri. Hukou		Agri. Employment			
	After-OCP (1990 Census)		After-OCP (1990 Census)		Before-OCP (1982 Census)	
Panel A. Family-level information	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Number of children	2.50	0.72	2.52	0.73	2.99	0.91
Both parents are Han	0.92	0.27	0.91	0.28	0.94	0.24
Twinning at the second birth	0.65%	0.08	0.61%	0.08	0.36%	0.06
Father's age	34.17	3.73	34.18	3.78	34.01	3.91
Mother's age	31.57	2.83	31.53	2.85	31.10	2.59
Mother's age at second birth	25.47	3.07	25.38	3.04	23.92	2.47
Father's years of schooling	7.56	2.96	7.42	2.96	5.95	3.00
Mother's years of schooling	5.21	3.62	5.06	3.60	2.94	3.29
Observations	282734		245233		198798	
Panel B. Child-level information	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Age	8.48	2.01	8.49	2.01	8.78	2.13
Male	0.50	0.50	0.51	0.50	0.51	0.50
Primary school attendance (dummy)	0.81	0.39	0.81	0.39	0.65	0.48
School enrollment (dummy)	0.81	0.39	-	-	-	-
Observations	457164		398762		386306	

Notes: This table shows summary statistics of the sample derived from the 1982 and 1990 csensuses. The sample is restricted to families with second-born twins or families without twins but with at least two children. In the 1990 census, we define a family as rural if the household head has an agricultural *hukou* (columns (1) and (2)) or is employed in agriculture (columns (3) and (4)). In the 1982 census, we define a family as rural if the household head is employed in agriculture (columns (5) and (6)). We drop families from Tibet and Xinjiang. Following Li, Zhang, and Zhu (2008), we restrict the sample to families with all children co-residing, with the oldest child below the age of 17, the mother below the age of 35, and maternal age at the first birth above the age of 15. We include children between the ages of six and 13 in the child sample.

Table 2: The effect of rationed fertility on child quality, Han versus Minority

Dependent variable	Primary school attendance				School enrollment			
Sample	Han (1)	Minority (2)	Pooled I (3)	Pooled II (4)	Han (5)	Minority (6)	Pooled I (7)	Pooled II (8)
Number of children (N)	0.002	-0.089**	-0.093*	-0.089**	0.003	-0.083**	-0.086*	-0.083**
	(0.010)	(0.039)	(0.051)	(0.039)	(0.010)	(0.039)	(0.051)	(0.039)
N * Han			0.096*	0.091**			0.089*	0.086**
			(0.053)	(0.040)			(0.053)	(0.040)
Han			-0.229	-0.399			-0.208	-0.378
			(0.149)	(0.284)			(0.149)	(0.290)
Observations	417496	39240	456736	456736	417496	39240	456736	456736

Notes: The table shows instrumental variable estimates. We use twining at the second birth as the instrumental variable of N in columns (1), (2), (5), and (6). We use twinning at the second birth and its interaction with Han as instrumental variables of N and $N \cdot Han$ in columns (3), (4), (7), and (8). The sample includes all children described in columns (1) and (2) of Panel B of Table 1. Additional control variables include mother's age at the second birth; the child's gender, age, age squared, and age cubed; parents' years of schooling, age, and age squared; and prefecture fixed effects. We allow these control variables to have separate coefficients for Han and minority in the "Pooled II" specifications in columns (4) and (8). Standard errors in parentheses are clustered at the family level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: The effect of rationed fertility on child quality, before versus after One-child Policy

Dependent variable	Primary schooling attendance							
Sample	Han				Minority			
	Before-OCP (1)	After-OCP (2)	Pooled I (3)	Pooled II (4)	Before-OCP (5)	After-OCP (6)	Pooled I (7)	Pooled II (8)
Number of children (N)	-0.055***	-0.007	-0.061***	-0.055***	-0.023	-0.076*	-0.037	-0.023
	(0.019)	(0.011)	(0.021)	(0.019)	(0.070)	(0.042)	(0.080)	(0.070)
N * OCP			0.070***	0.048**			-0.039	-0.053
			(0.026)	(0.022)			(0.107)	(0.082)
OCP			-0.064	-1.689***			0.263	-3.560***
			(0.080)	(0.153)			(0.368)	(0.487)
Observations	362830	361895	724725	724725	23020	36455	59475	59475

Notes: The table shows instrumental variable estimates. We use twining at the second birth as the instrumental variable of N in columns (1), (2), (5), and (6). We use twinning at the second birth and its interaction with OCP as instrumental variables of N and $N \cdot Han$ in columns (3), (4), (7), and (8). The sample includes all children described in columns (3)–(6) of Panel B of Table 1. Additional control variables include mother's age at the second birth; the child's gender, age, age squared, and age cubed; parents' years of schooling, age, and age squared; and prefecture fixed effects. We allow these control variables to have separate coefficients for the before-OCP sample and the after-OCP sample in "Pooled II" specifications in columns (4) and (8). Standard errors in parentheses are clustered at the family level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: The effect of rationed fertility on child quality, evidence from two Chinese twins surveys

Panel A. Chinese Adult Twins Survey (before One-child Policy)			
Dependent variable	Completed schooling years		
Sample	2-plus	3-plus	4-plus
	(1)	(2)	(3)
Number of children	-0.838***	-0.619***	-1.059***
	(0.220)	(0.203)	(0.280)
Observations	5149	4248	2824

Panel B. Urban sample of Chinese Child Twins Survey (after One-child Policy)			
Dependent variable	Expected schooling years	Child investment	Home tutorial time
	(1)	(2)	(3)
Twinning	-0.268	-0.067	0.263**
	(0.166)	(0.069)	(0.113)
Observations	1506	1532	1510
R-squared	0.28	0.12	0.14

Panel C. Rural sample of Chinese Child Twins Survey (after One-child Policy)			
Dependent variable	Expected schooling years	Child investment	Home tutorial
	(1)	(2)	(3)
Twinning on 1st-born	-0.202	-0.295	-0.217
	(0.286)	(0.145)	(0.178)
Twinning on 2nd-born	-0.082	0.153	0.027
	(0.251)	(0.098)	(0.163)
Observations	1181	1262	1136
R-squared	0.14	0.11	0.16

Notes: Panel A presents the instrumental variable estimates of rationed fertility on completed years of schooling using the CATS sample. The n-plus ($n = 2, 3, 4$) sample includes two types of families: (i) families without twins but with at least n children, and (ii) families with twin births at the nth birth parity. When using the n-plus sample, we use twinning at the nth birth as the instrumental variable for the number of children. Control variables include maternal age at the nth birth ($n = 2, 3, 4$), the child's gender and birth year dummy, parents years of schooling and birth year dummies, and city-district fixed effects. Panel B presents the OLS estimates of rationed fertility on child quality using the urban sample, and Panel C, the rural sample. In Panels B and C, "Child investment" and "Home tutorial" are in log scale. We add one to these outcome variables before taking logs to exploit the information of zero expenditure. The results are robust if we do not add one, that is, if we use the truncated samples. In Panel B, control variables include maternal age at the first birth; the child's gender, birthweight, age, age squared, and age cubed; and parents' years of schooling, age, and age squared. In Panel C, control variables include maternal age at the second birth; an indicator of being the first-born child; the child's gender, birthweight, age, age squared, and age cubed; the average birthweight of the second-born(s); an interaction term of the twinning indicator and the indicator of being a first-born child; an interaction term of the twinning indicator and the average birthweight of the second-born(s); gender of the first-born child; and parents' years of schooling, age, and age squared. Standard errors in parentheses are clustered at the family level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: The effect of rationed fertility on parental consumption (Chinese Child Twins Survey)

Dependent	Father's consumption				Mother's consumption		
variable	Cigarette	Alcohol	Clothing	Dinner out	Cosmetics	Clothing	Dinner out
				Panel A. Urban sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twinning	-0.102	-0.159	-0.264*	-0.071**	-0.218*	-0.344**	-0.064**
	(0.140)	(0.105)	(0.155)	(0.028)	(0.127)	(0.148)	(0.027)
Observations	1067	1067	1067	1062	1067	1067	1060
R-squared	0.03	0.02	0.07	0.08	0.16	0.11	0.08
				Panel B. Rural sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twinning	-0.091	0.027	-0.611***	-0.047	-0.470***	-0.614***	-0.020
	(0.141)	(0.119)	(0.167)	(0.029)	(0.129)	(0.165)	(0.019)
Observations	642	642	642	642	642	642	641
R-squared	0.02	0.03	0.10	0.03	0.10	0.12	0.02

Notes: This table presents OLS estimates of the effect of twinning on parental consumption using the CCTS sample. "Dinner out' is an indicator variable that equals one if the father or mother has ever dined outside without taking their children in the last month. Other outcome variables are in log scale. We add one to these outcome variables before taking logs to exploit the information of zero expenditure. The results are robust if we do not add one, that is, if we use the truncated samples. Panel A presents the results for the urban sample, and Panel B, the rural sample. Control variables include maternal age at the first birth for the urban sample and at the second birth for the rural sample; and parents' years of schooling, age, and age squared. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: The effect of rationed fertility on parental labor supply (Chinese Child Twins Survey)

Dependent Variable	Labor force participation	Days worked last month	Hours worked last week	Labor income	Earnings per hour	Private business	Out home one month
	Panel A. Urban father's labor supply						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twinning	-0.003	0.017	0.038*	0.077**	0.039	0.027	0.029
	(0.024)	(0.013)	(0.022)	(0.038)	(0.043)	(0.026)	(0.019)
Observations	1067	837	840	1030	834	845	1067
R-squared	0.09	0.06	0.03	0.20	0.23	0.12	0.03
	Panel B. Urban mother's labor supply						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twinning	0.051*	0.018	0.048**	0.076	0.044	0.059**	-0.002
	(0.028)	(0.014)	(0.024)	(0.055)	(0.051)	(0.029)	(0.013)
Observations	1066	674	676	903	665	681	1067
R-squared	0.13	0.08	0.03	0.14	0.29	0.15	0.01
	Panel C. Rural father's labor supply						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twinning	0.011	0.005	0.017	-0.021	0.047	0.114***	0.041
	(0.034)	(0.018)	(0.028)	(0.068)	(0.081)	(0.041)	(0.026)
Observations	642	530	528	629	529	532	642
R-squared	0.02	0.02	0.02	0.11	0.14	0.05	0.02
	Panel D. Rural mother's labor supply						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twinning	0.002	0.016	-0.007	0.015	0.069	0.142***	0.031**
	(0.036)	(0.020)	(0.033)	(0.069)	(0.085)	(0.041)	(0.016)
Observations	640	498	496	587	484	500	642
R-squared	0.02	0.01	0.02	0.11	0.13	0.07	0.02

Notes: This table presents the OLS estimates of twinning on parental labor supply. Outcome variables in columns (2)–(5) are in log scale. Outcome variables in columns (1), (6), and (7) are dummy variables. Panels A and B present the results for the urban sample, and Panels C and D, the rural sample. Control variables include maternal age at the first birth for the urban sample and at the second birth for the rural sample; and parents' years of schooling, age, and age squared. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Online Appendix for

"Rationed Fertility: Theory and Evidence"

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December 31, 2016

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A1 Decomposition Details

Differentiating Equations (27) and (28) with respect to $\bar{n}$,

$$\frac{\partial \tilde{q}^c}{\partial \bar{n}} = \pi_{nq} \frac{\partial \tilde{q}^{*c}}{\partial \pi_q^*} + \frac{\partial \tilde{q}^{*c}}{\partial \bar{n}}, \tag{A1.1}$$

$$\frac{\partial \tilde{s}^c}{\partial \bar{n}} = \pi_{nq} \frac{\partial \tilde{s}^{*c}}{\partial \pi_q^*} + \frac{\partial \tilde{s}^{*c}}{\partial \bar{n}}. \tag{A1.2}$$

If we further consider the utility maximization problem in the restricted q^1 model, we have

$$\frac{\partial \tilde{q}}{\partial \bar{n}} = \pi_{nq} \frac{\partial \tilde{q}^*}{\partial \pi_q^*} + \frac{\partial \tilde{q}^*}{\partial \bar{n}}, \tag{A1.3}$$

$$\frac{\partial \tilde{s}}{\partial \bar{n}} = \pi_{nq} \frac{\partial \tilde{s}^*}{\partial \pi_q^*} + \frac{\partial \tilde{s}^*}{\partial \bar{n}}, \tag{A1.4}$$

$$\frac{\partial \tilde{q}}{\partial y} = \frac{\partial \tilde{q}^*}{\partial y}, \tag{A1.5}$$

$$\frac{\partial \tilde{s}}{\partial y} = \frac{\partial \tilde{s}^*}{\partial y}, \tag{A1.6}$$

where $\tilde{q}^*$ and $\tilde{s}^*$ are uncompensated demand functions of q and s in the restricted q^1 model when $\pi_q^* = \pi_{nq}\bar{n} + \pi_q$, $\pi_n^* = \pi_n$, $\pi_s^* = \pi_s$. Substituting Equations (A1.1) and (A1.5) into Equation (22), and Equations (A1.2) and (A1.6) into Equation (23), we have

$$\frac{\partial \tilde{q}}{\partial \bar{n}} = \pi_{nq} \frac{\partial \tilde{q}^{*c}}{\partial \pi_q^*} + \frac{\partial \tilde{q}^{*c}}{\partial \bar{n}} + (\bar{\pi}_n - \pi_n) \frac{\partial \tilde{q}^*}{\partial y}, \tag{A1.7}$$

$$\frac{\partial \tilde{s}}{\partial \bar{n}} = \pi_{nq} \frac{\partial \tilde{s}^{*c}}{\partial \pi_q^*} + \frac{\partial \tilde{s}^{*c}}{\partial \bar{n}} + (\bar{\pi}_n - \pi_n) \frac{\partial \tilde{s}^*}{\partial y}. \tag{A1.8}$$

Applying the Neary and Roberts (1980, pp. 32-34)'s Equations (19), (24), and (29), we decompose derivatives of q and s with respect to $\bar{n}$ in the restricted q^1 model to derivatives in the unrestricted

q^1 model,

$$\frac{\partial \widetilde{q^{*c}}}{\partial \bar{n}} = \frac{\partial q^{*c}}{\partial \pi_n^*}\left(\frac{\partial n^{*c}}{\partial \pi_n^*}\right)^{-1}, \tag{A1.9}$$

$$\frac{\partial \widetilde{q^{*c}}}{\partial \pi_q^*} = \frac{\partial q^{*c}}{\partial \pi_q^*} - \left(\frac{\partial n^{*c}}{\partial \pi_q^*}\right)^2 \left(\frac{\partial n^{*c}}{\partial \pi_n^*}\right)^{-1}, \tag{A1.10}$$

$$\frac{\partial \tilde{q}^*}{\partial y} = \frac{\partial q^*}{\partial y} - \frac{\partial \widetilde{q^{*c}}}{\partial \bar{n}}\frac{\partial n^*}{\partial y}. \tag{A1.11}$$

As for the comparative static analysis with respect to s, we have

$$\frac{\partial \widetilde{s^{*c}}}{\partial \bar{n}} = \frac{\partial s^{*c}}{\partial \pi_n^*}\left(\frac{\partial n^{*c}}{\partial \pi_n^*}\right)^{-1}, \tag{A1.12}$$

$$\frac{\partial \widetilde{s^{*c}}}{\partial \pi_q^*} = \frac{\partial s^{*c}}{\partial \pi_q^*} - \frac{\partial n^{*c}}{\partial \pi_s^*}\frac{\partial n^{*c}}{\partial \pi_q^*}\left(\frac{\partial n^{*c}}{\partial \pi_n^*}\right)^{-1}, \tag{A1.13}$$

$$\frac{\partial \tilde{s}^*}{\partial y} = \frac{\partial s^*}{\partial y} - \frac{\partial \widetilde{s^{*c}}}{\partial \bar{n}}\frac{\partial n^*}{\partial y}. \tag{A1.14}$$

Substituting Equation (A1.11) into Equation (A1.7),

$$\frac{\partial \tilde{q}}{\partial \bar{n}} = \pi_{nq}\frac{\partial \widetilde{q^{*c}}}{\partial \pi_q^*} + \left[1 - (\bar{\pi}_n - \pi_n)\frac{\partial n^*}{\partial y}\right]\frac{\partial \widetilde{q^{*c}}}{\partial \bar{n}} + (\bar{\pi}_n - \pi_n)\frac{\partial q^*}{\partial y}. \tag{A1.15}$$

Substituting Equation (A1.14) into Equation (A1.8),

$$\frac{\partial \tilde{s}}{\partial \bar{n}} = \pi_{nq}\frac{\partial \widetilde{s^{*c}}}{\partial \pi_q^*} + \left[1 - (\bar{\pi}_n - \pi_n)\frac{\partial n^*}{\partial y}\right]\frac{\partial \widetilde{s^{*c}}}{\partial \bar{n}} + (\bar{\pi}_n - \pi_n)\frac{\partial s^*}{\partial y}. \tag{A1.16}$$

Equations (A1.15) and (A1.16) in this paper are generalizations of Equations (18) and (19) in Rosenzweig and Wolpin (1980, p. 231).

A2 The Chinese Adult Twins Survey (CATS) and the Chinese Child Twins Survey (CCTS)

A2.1 The Chinese Adult Twins Survey

The Chinese Adult Twins Survey (CATS) was carried out by the Urban Survey Unit of the National Bureau of Statistics in June and July 2002 in five cities in China. The local statistical bureaus identified same-sex adult twins between the ages of 18 and 65 via various channels, including colleagues, friends, relatives, newspaper advertising, neighborhood notices, neighborhood management committees, and household records from the local public security bureau. Overall, these channels permit a roughly equal probability of contacting all twins in these cities, implying that the CATS sample is arguably representative. In addition, the Urban Household Survey sampling frame was used to obtain a comparable sample of non-twin adults between the ages of 25 and 65 in the same neighborhoods. Questionnaires were completed through face-to-face personal interviews. See Li, Rosenzweig, and Zhang (2010) for a detailed description of CATS.

We divide families in the CATS sample into two categories. "Twin family" refers to families formed by a pair of adult twins and their siblings and parents, in which the "children" are the adult twins and their siblings. "Non-twin family" refers to families formed by non-twin adults and their parents, in which the "children" are the non-twin adults. We impose a series of restrictions to form the estimation sample. First, we restrict the sample to households in which the youngest child was at least 25 years old, and the oldest child was at most 60 years old. This age restriction ensures that all children had completed their education at the time of the survey, and more importantly, that all children were born before the implementation of the One-child Policy (OCP). Second, following Black, Devereux, and Salvanes (2005, 2010) and Angrist, Lavy, and Schlosser (2010), we keep non-twin families with at least two children, as well as twin families with twins born at the second, third, and fourth birth parties. Third, we drop families in which mothers had their first birth before the age of 15. In total, we have 649 twin families (2,894 children) and 1,260 non-twin families (4,188 children).

Panel A of Table A1 describes the family-level information for twin and non-twin families. As expected, the average number of children in twin families is larger than in non-twin families. It is noteworthy that the fertility difference is more than one. The reason is that twin births at the second, third, and fourth parities shift the fertility distribution at different fertility margins (Figure A2). The dispersed fertility distribution in the CATS sample is consistent with the absence of the rationing of fertility in the non-OCP regime, supporting our hypothesis that twinning-induced fertility increases in the CATS sample are forced. Children in twin families are on average 41 years old, 1.8 years younger than those in non-twin families, as reported in Panel B of Table A1. Note that although these children are of a younger cohort, children in twin families have significantly fewer years of schooling than those in non-twin families. This pattern preliminarily suggests a negative effect of rationed fertility on child quality in the non-OCP regime.

To estimate the effect of rationed fertility on child quality, we first define the n-plus ($n = 2, 3, 4$) sample, which includes two types of families: (i) non-twin families with at least n children, and (ii) families with twin births at the nth birth parity. Using the n-plus sample, we then estimate Equation (A2.1),

$$Y_{ij} = \zeta_0 + \zeta_1 N_j + \mathbf{X}\zeta_2 + \epsilon_{ij}, \tag{A2.1}$$

where Y_{ij} is years of schooling of child i in family j; N_j is the number of children in family j; $\mathbf{X}$ is a vector of control variables, including child gender and birth year dummies; maternal age at the potential twin birth; parents years of schooling and birth year dummies;[1] ϵ_{ij} is the error term. We also control for district fixed effects.

We use twinning at the nth birth as an instrumental variable (IV) for fertility when using the n-plus sample. The first-stage estimation equation of Equation (A2.1) is

$$N_j = \phi_0 + \phi_1 T_j + \mathbf{X}\phi_2 + \mu_{ij}, \tag{A2.2}$$

where T_j is an indicator of twinning at the nth birth, and μ_{ij} is the error term.

[1] Maternal age at the potential twin birth using the n-plus ($n = 2, 3, 4$) sample is the mother's age at the nth birth.

As discussed in Section 4.2.1, we prefer to include all children in the estimation sample. Using only low-parity children underestimates the negative effect of child quantity on average child quality.

A2.2 The Chinese Child Twins Survey (CCTS)

The Chinese Child Twins Survey (CCTS) was carried out by the Urban Survey Unit of the National Bureau of Statistics in late 2002 and early 2003 in Kunming, the capital of an underdeveloped province in China. The Urban Survey Unit initially identified 2,300 households with twins between the ages of 7 and 18 from the 2000 population census as the target sample. Of this target sample, 1,694 households were successfully interviewed. The Urban Survey Unit also interviewed 1,693 neighboring households with non-twin children as a comparison group. CCTS contains comprehensive information on parental and child expenditures, parental labor supply, and child outcomes. See Rosenzweig and Zhang (2009) for a detailed description of CCTS.

All children in the CCTS sample were born during the OCP regime. In Kunming, the OCP was strictly enforced in urban areas.[2] Rural households, however, are exempted from the OCP, and subject to a "two children policy." We categorize families in the CCTS sample into twin and non-twin families. Following Rosenzweig and Zhang (2009), we use two sub-samples in the empirical analysis: (i) an urban (non-exempt) sample comprising non-twin families with at least one child and twin families with first-parity twins, and (ii) a rural (exempt) sample comprising non-twin families with at least two children and twin families with second-parity twins. In the urban sample, we keep the first-born children (or twins). In the rural sample, we keep the first-born non-twin children and the second-born children (or twins). We find that children above the age of 16 are likely to have left home at the time of the survey, and we thus exclude these children. This sample restriction yields 567 non-twin families and 500 twin families in the urban sample, and 364 non-twin families and 278 twin families in the rural sample.

We use expected years of schooling and parental investment in children, instead of completed

[2] Minority families in urban areas were exempted from the OCP and allowed to have two children. As such, we exclude families in which either parent is minority Chinese in the urban sample.

years of schooling, to measure child quality in the CCTS sample. Children in this sample are on average 11 years old, and thus have not completed their education. In our general theory, as well as in Becker and Lewis (1973) and Rosenzweig and Wolpin (1980), there is no distinction between investment in children and child quality (Expression (1)). As long as child quality monotonically increases with investment at relevant margins, investment in children is a good proxy for child quality.

Accordingly, we use three variables to measure child quality. The first is children's expected years of schooling. The second is parental monetary investment in children in the twelve months prior to the survey. The monetary investment includes parental investment in child education, health, clothing, and others. The third is parental time investment in children measured by the number of minutes of home tutorials per day during the workweek.[3]

Panel A of Table A2 reports summary statistics of the family-level information for the urban and rural samples. In the urban sample, twin families have on average 0.97 more children than non-twin families; the analogous number is 0.99 in the rural sample. The close-to-unity fertility differences between twin and non-twin families indicate a strict rationing of fertility in the CCTS sample. We compare fertility distributions in the CATS and CCTS samples in Section A2.3. Following the same logic as in Section 4.1, a twinning-induced fertility increase in CCTS represents a mix of forced and desired fertility increases.

Columns (1)–(4) in Panel B of Table A2 depict the measures of child quality in the urban sample. Urban twins have fewer expected years of schooling and less parental monetary investment than non-twins. The differences may result from lower endowment of twins compared with non-twins. Columns (5)–(8) depict the measures of child quality in the rural sample, demonstrating a comparable picture. Interestingly, urban twins receive more parental tutorial time than non-twins. However, a direct comparison of quality measures between twins and non-twins does not accurately portray the effect of rationed fertility on child quality. Twins are different from non-twins primarily because of their zero spacing and lower birthweight (Rosenzweig and Zhang, 2009). In

[3] We also have parental home tutorials per day on weekend. The results are insensitive to the choice of measure.

particular, the lower birthweight of twins reflects their inferior endowment (Behrman and Rosenzweig, 2004; Almond, Chay, and Lee, 2005). To deal with the confounding effect of parental reinforcement or compensation on the endowment difference between twins and non-twins, we follow Rosenzweig and Zhang (2009) to control for children's endowment, as measured by birthweight, when estimating the effect of rationed fertility on child quality.

As discussed earlier, non-twin families have almost exactly one fewer child than twin families in both rural and urban areas (Table A2). Therefore, it is not necessary to use an IV for fertility. Following Rosenzweig and Zhang (2009), we estimate the effect of rationed fertility on child quality using Equations A2.3 and A2.4.

Specifically, for the urban sample, we estimate

$$Y_{1j} = \eta_0 + \eta_1 T_j + \eta_2 BW_{1j} + \mathbf{X}\eta_2 + \epsilon_{ij}, \tag{A2.3}$$

where Y_{1j} is an outcome variable for the first-born child (or one of the twins) in family j; T_j is an indicator of twinning at the first birth in family j; BW_{1j} is the birthweight of the child; $\mathbf{X}$ is a vector of control variables, including maternal age at the first birth; the child's gender, birthweight, age, age squared, and age cubed; parents' years of schooling, age, and age squared; and ϵ_{ij} is the error term. In particular, Rosenzweig and Zhang (2009) show that the inclusion of BW_{1j} in the estimation deals with the confounding effect of parental resource reallocation, *i.e.*, reinforcement on child endowment arising from the lower birthweight of twins. Note that η_1 captures the effect of rationed fertility on the outcome variable Y_{1j}.

For the rural sample, we estimate

$$Y_{ij} = \delta_0 + \delta_1 T_j + \delta_2 T_j * F_{ij} + \delta_3 F_{ij} + \delta_4 BW^*_{2j} * F_{ij} + \delta_5 BW^*_{2j} + \mathbf{X}\delta_6 + \epsilon_{ij}, \tag{A2.4}$$

where Y_{ij} ($i = 1, 2$) is an outcome variable for the first- or second-born child in family j; T_j is an indicator of twinning at the second birth in family j; F_{ij} is an indicator of child i being the first-born child; BW^*_{2j} is birthweight (average birthweight) of the second-born child (twins) in family

j; $\mathbf{X}$ is a vector of control variables, including maternal age at the second birth; the first-child gender; the child's gender, age, age squared, and age cubed; parents' years of schooling, age, and age squared; and ϵ_{ij} is the error term. For the same reason mentioned above, we add BW^*_{2j} as a regressor. Note that $\delta_1 + \delta_2$ captures the effect of rationed fertility for the first-born children, and δ_1 for the second-born children.

A2.3 Fertility Distributions

We illustrate the binding rationing of fertility created by the OCP, by carefully examining the difference in fertility distributions between the CATS and the CCTS samples. Fertility is rationed by the OCP in the CCTS sample, but not in the CATS sample.

Sub-figure A of Figure A2 shows fertility distributions of non-twin and twin families in the CATS sample. The maximal fertility level is eight children for both types of families. Twinning at the second, third, and fourth parities shifts up the entire fertility distribution. Of non-twin families, 30.95 percent have two children, 31.19 percent have three children, 21.43 percent have four children, 10.48 percent have five children, and less than 10.00 percent have six or more children. Of twin families, none have two children, 26.81 percent have three children, 28.20 percent have four children, 25.89 percent have five children, 10.94 percent have six children, and less than 10.00 percent have seven or more children.

Twinning mainly shifts up fertility at the parity of occurrence in the CATS sample, representing a forced fertility increase. To check the effect of twinning at each birth parity, we investigate the n-plus sample ($n = 2, 3, 4$) introduced earlier, which consists of twin families with nth parity twins and non-twin families with at least n children. As can be seen from Sub-figure B, the largest shift in the fertility distribution happens at the two-to-three fertility margin in the 2-plus sample: 31.27 percent of non-twin families and 57.05 percent of twin families have three children. In the 3-plus sample, the largest shift in the fertility distribution happens at the three-to-four fertility margin, as shown in Sub-figure C, while in the 4-plus sample, the largest shift in the fertility distribution happens at the four-to-five fertility margin, as shown in Sub-figure D.

By contrast, fertility distributions in CCTS implies strict rationing of fertility. As shown in Figure A3, in the urban sample, 95.06 percent of non-twin families have only one child, while 98.4 percent of twin families stop at two children after the birth of first-parity twins, implying an overall OCP compliance rate of over 95.06 percent. In the rural sample, the "two children policy" compliance rate is 99.18 percent for non-twin families, and almost all twin families with second-parity twins stop at three children. The sharp fall in fertility when it passes the relevant birth quota suggests a binding rationing of fertility. Twinning at the first birth can be taken as an exogenous break of the OCP in urban areas, while twinning at the second birth, as an exogenous break of the "two children policy" in rural areas. As discussed in Section 4.1, twinning-induced fertility increases in CCTS represent a mix of forced and desired fertility increases.

Our theory implies that the magnitude of the negative effect of rationed fertility on child quality would be larger in the CATS sample than the CCTS sample. We examine this hypothesis in Section 5.3 in the main text.

A3 The Effect of Rationed Fertility on Parental Consumption: A Simulation

We use the parametric model in Section 3.4 to simulate parental consumption response to rationed fertility. Figure A4 depicts parental consumption s against rationed fertility $\bar{n}$ for $\bar{n} \in [1, 5]$. Note that s monotonically declines as $\bar{n}$ increases from $\bar{n} = 1$ to $\bar{n} = 5$. The decline of s is stronger at lower $\bar{n}$ than at higher $\bar{n}$. A higher $\bar{n}$ uses up a larger portion of parental income, leaving less leeway for parents to reduce s.

As in Section 3.4, we decompose the marginal effects of rationed fertility $\bar{n}$ on parental consumption s using Equation (30) divided by s. We use the semi-elasticity form to ensure the results are insensitive to the unit of measurement of s. Figure A5 shows that the total effect is decomposed into three parts. The rationing price effect is positive, and it remains sizable for the whole range of $\bar{n}$. The rationing substitution effect is negative; its magnitude declines with $\bar{n}$ and approaches zero for large $\bar{n}$. The rationing income effect is cancelled out by the rationing price effect for large $\bar{n}$. Overall, the total effect appears to follow the path of the rationing substitution effect.

A4 Theoretical Extensions on Parental Responses

We extend the model in Section 3 to incorporate parental time allocation. Parents care about children's average human capital (quality), the number of children, their own consumption, and leisure. Parents' utility function is $u = u(h, n, c, l)$, where h is the average human capital of children, c is parental consumption, and l is parental leisure. To enhance a child's human capital h, parents can either increase child expenditure q, or allocate more time to home tutorials t, *i.e.* $h = h(q, t)$. We use T to denote total parental time. Hence $d = T - t - l$ represents parental labor supply.

The parents' utility maximization problem is

$$\begin{aligned} \max_{q,t,c,l} \quad & u = u(h, c, l, n), \\ \text{subject to} \quad & h = h(q, t), \\ c + nq + \pi_n n + \pi_q q + w(t + l) &= wT + y, \end{aligned} \tag{A4.1}$$

where $U(q, t, c, l, n) = u(h(q, t), c, l, n)$ is the reduced-form utility function. Here, q no longer represents child quality; instead, q represents parental investment in each child.[4] Meanwhile, c, t, and l are components of the composite good s in the three commodity model of Section 3.

Differentiating the budget constraint with respect to $\bar{n}$, and multiplying both sides by $\frac{1}{wT+y}$, we have

$$\epsilon_{cn} b_c + b_n + \epsilon_{qn} b_q + \epsilon_{(t+l)n} b_{t+l} = 0, \tag{A4.2}$$

where $\epsilon_{kn} = \frac{\partial k}{\partial n}\frac{1}{k}$, $b_k = \frac{p_k k}{wT+y}$, $\forall k = c, n, q, t + l$. Note that $p_c = 1$, $p_n = q + \pi_n$, $p_q = n + \pi_q$, and $p_{t+l} = w$; ϵ_{kn} is the semi-elasticity of k with respect to $\bar{n}$, $\forall k = c, q, t + l$; and b_k is the budget share

[4] $\pi_{nq} = 1$ by construction. Child human capital h can also be multidimensional. Heckman (2007) emphasizes the importance of non-cognitive skills as a form of human capital.

of k, $\forall k = c, n, q, t + l$.[5]

If $\epsilon_{qn} = 0$, then $\epsilon_{cn} b_c + \epsilon_{(t+l)n} b_{t+l} = -b_n < 0$. Then either $\epsilon_{cn} < 0$ or $\epsilon_{(t+l)n} < 0$.[6] The theory has the following implication.

Implication A1 *In response to an exogenous fertility increase, parents will either reduce self-consumption or increase labor supply, or both, in order to maintain expenditure per child.*

A5 Parental Responses to Twinning, by the Gender of Twins

We check if parental responses to twinning differ by the gender of twins. Following Oliveira (2016), we define an indicator variable TG_j that equals one if family j has a pair of twin girls, and an indicator variable TB_j that equals one if family j has a pair of twin boys. We also define an indicator variable TGB_j that equals one if family j has a pair of mixed-gender twins. Specifically, we estimate

$$Y_j = \vartheta_0 + \vartheta_1 TG_j + \vartheta_2 TGB_j + \vartheta_3 TB_j + \mathbf{X}\vartheta_4 + \epsilon_j, \qquad \text{(A5.1)}$$

where Y_j is an outcome variable of parental consumption or labor supply; $\mathbf{X}$ is the same set of parental controls as in Equation (35); and ϵ_j is the error term. Note that ϑ_1, ϑ_2, and ϑ_3 capture the effect of having a pair of twin girls, mixed-gender twins, and twin boys, respectively. We show in Table A7 that the negative effect of twinning on parental consumption is stronger if there are more boys in the twin-pair. However, the response of parental labor supply to twinning does not display a clear pattern of gender difference (Table A8).

[5] $\epsilon_{(t+l)n} = \frac{b_t}{b_{t+l}} \epsilon_{tn} + \frac{b_l}{b_{t+l}} \epsilon_{ln}$. Note that $\epsilon_{ky} = \frac{\partial k}{\partial y} \frac{y}{k}$ is the income elasticity of k.

[6] $\epsilon_{(t+l)n} < 0$ means $\epsilon_{dn} > 0$ since $d = T - t - l$.

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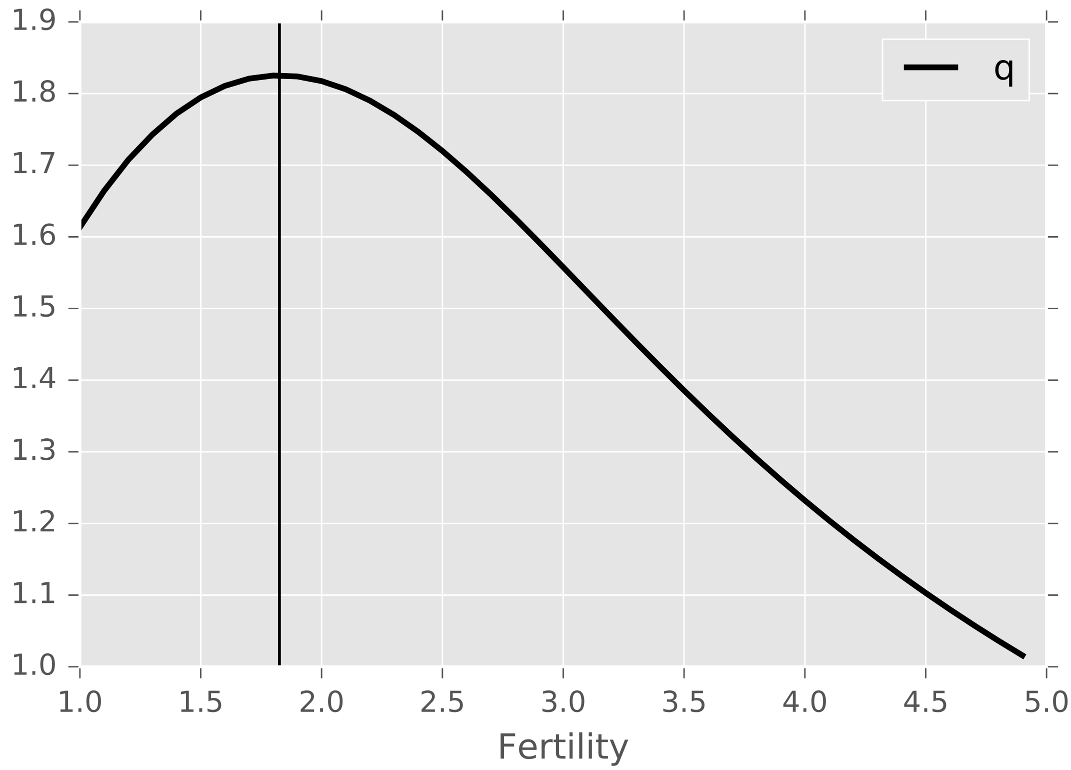

Figure A1: Rationed fertility and child quality: A simulation

Notes: The x-axis shows the fertility of parents. The y-axis shows the level of child quality q. The utility function adopts a nested CES parameterization. That is, $U(\bar{n}, q, s) = U_1^{\theta} s^{1-\theta}$ and $U_1 = (\alpha\bar{n}^{\rho} + (1-\alpha)q^{\rho})^{\frac{1}{\rho}}$. The budget constraint is $\pi_{nq}\bar{n}q + \pi_n\bar{n} + \pi_q q + \pi_s s \leq y$. The model parameters are set at $\theta = 0.5$, $\alpha = 0.5$, $\rho = -3$, $\pi_{nq} = 1$, $\pi_q = 0$, $\pi_n = 0$, $\pi_s = 1$, and $y = 10$. The unrestricted optimal fertility n^o is 1.83, which is denoted by the vertical line.

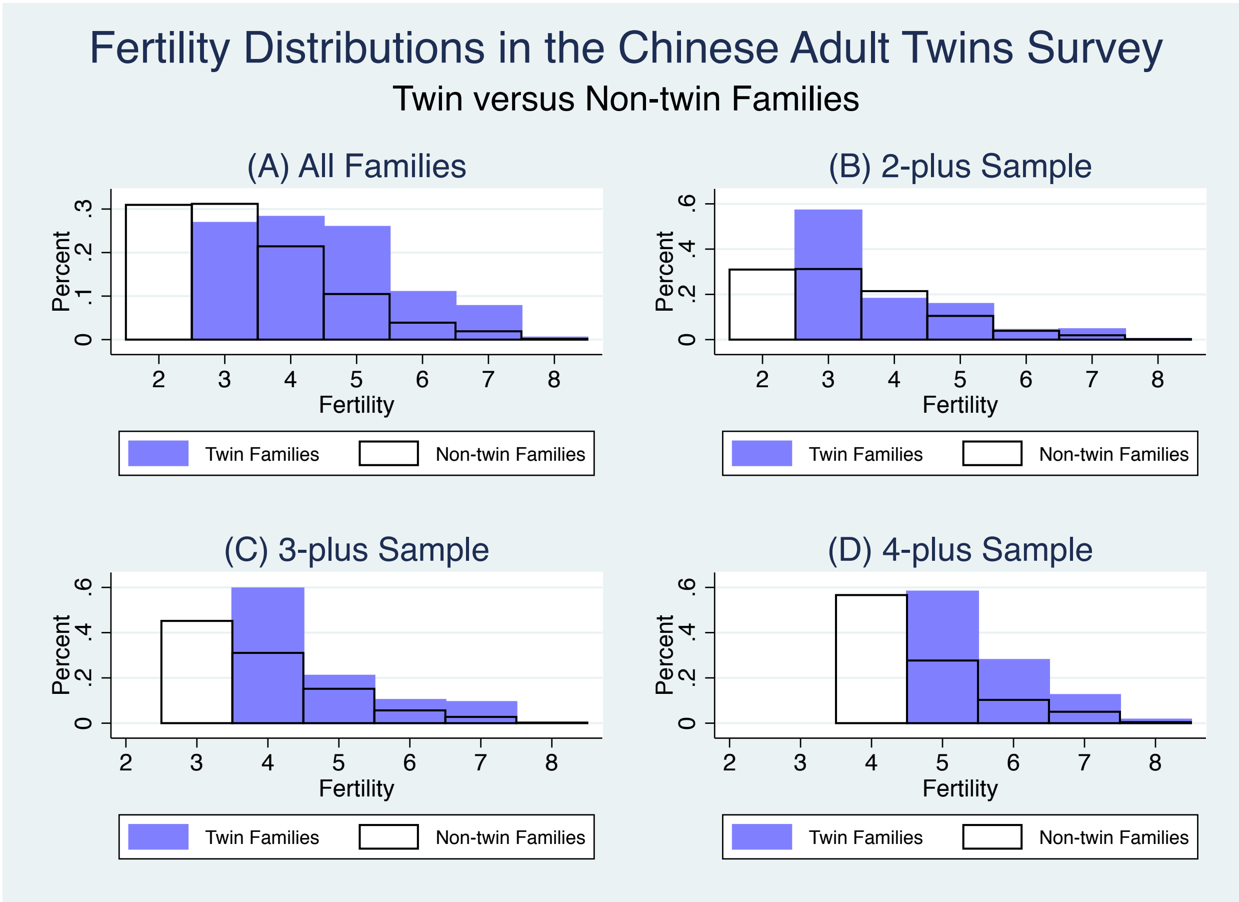

Figure A2: Fertility distributions in the Chinese Adult Twins Survey (CATS)

Notes: This figure presents fertility distributions of twin versus non-twin families in the CATS sample. In sub-figure A, the sample includes non-twin families with at least two children and twin families with twins born at the second, third, and fourth parities. Sub-figure B uses the 2-plus sample, which includes non-twin families with at least two children and twin families with second-parity twins. Sub-figure C uses the 3-plus sample, which includes non-twin families with at least three children and twin families with third-parity twins. Sub-figure D uses the 4-plus sample, which includes non-twin families with at least four children and twin families with fourth-parity twins.

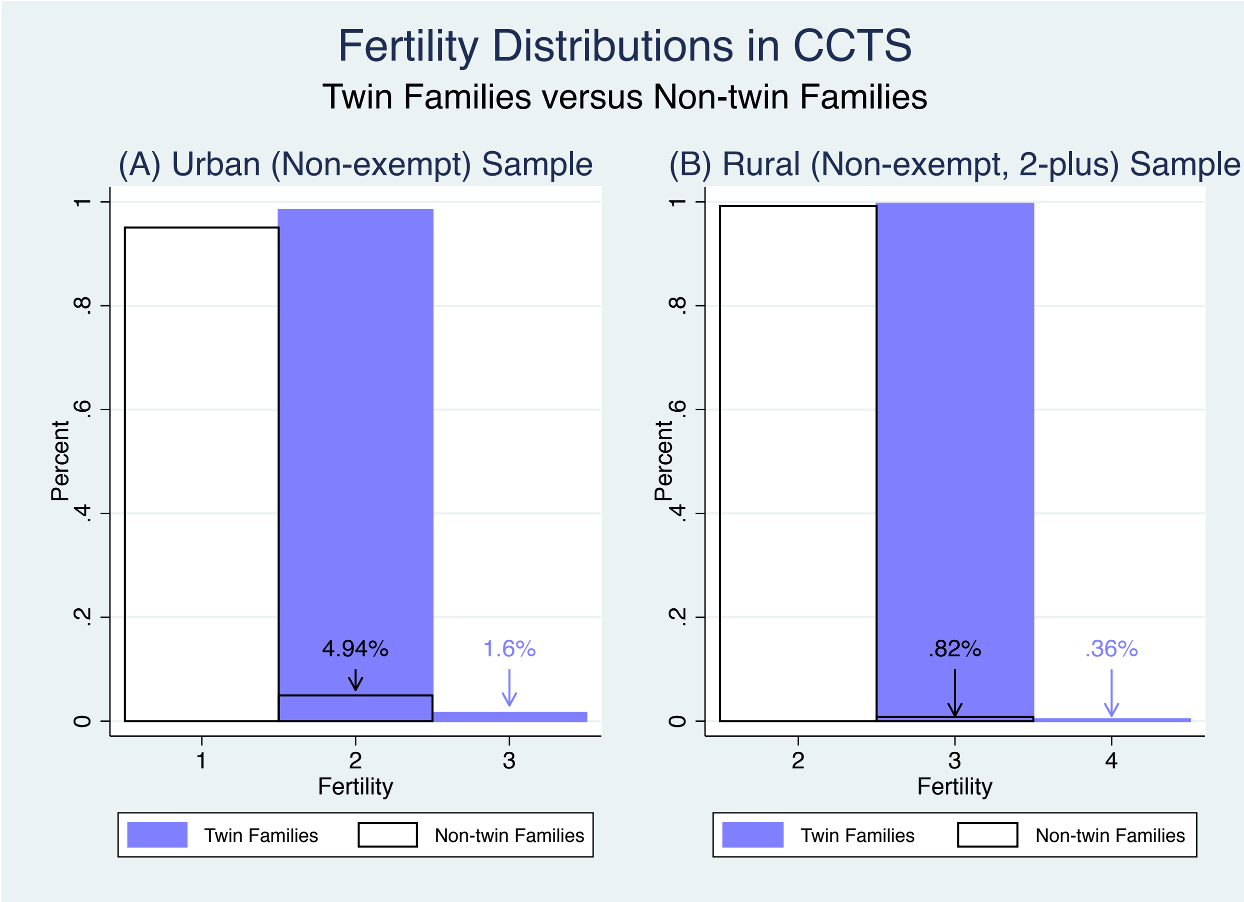

Figure A3: Fertility distributions in the Chinese Child Twins Survey (CCTS)

Notes: This figure presents the fertility distributions of twin versus non-twin families in the CCTS sample. Sub-figure A includes urban non-twin families and urban twin families with first-parity twins. Sub-figure B includes rural non-twin families with at least two children and rural twin families with second-parity twins.

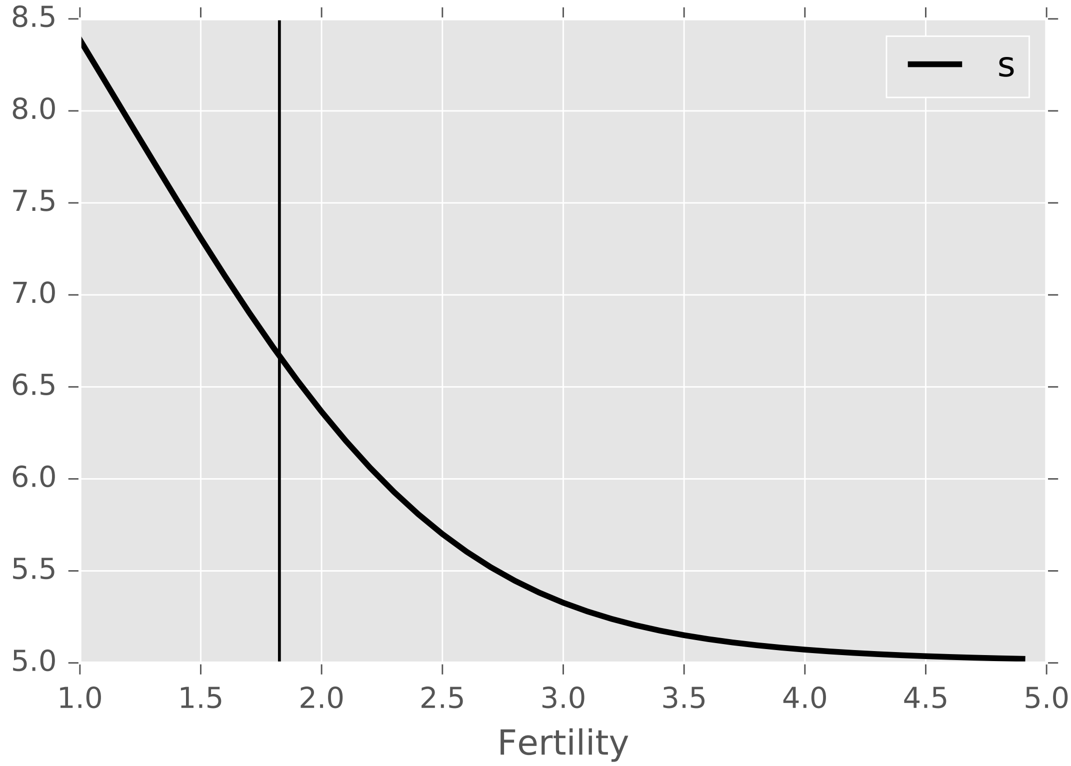

Figure A4: Rationed fertility and parental consumption: A simulation

Notes: The x-axis shows the fertility of parents. The y-axis shows the level of the composite consumption good s. The utility function adopts a nested CES parameterization. That is, $U(\bar{n}, q, s) = U_1^{\theta} s^{1-\theta}$ and $U_1 = (\alpha\bar{n}^{\rho} + (1-\alpha)q^{\rho})^{\frac{1}{\rho}}$. The budget constraint is $\pi_{nq}\bar{n}q + \pi_n\bar{n} + \pi_q q + \pi_s s \leq y$. The model parameters are set at $\theta = 0.5$, $\alpha = 0.5$, $\rho = -3$, $\pi_{nq} = 1$, $\pi_q = 0$, $\pi_n = 0$, $\pi_s = 1$, and $y = 10$. The unrestricted optimal fertility n^o is 1.83, which is denoted by the vertical line.

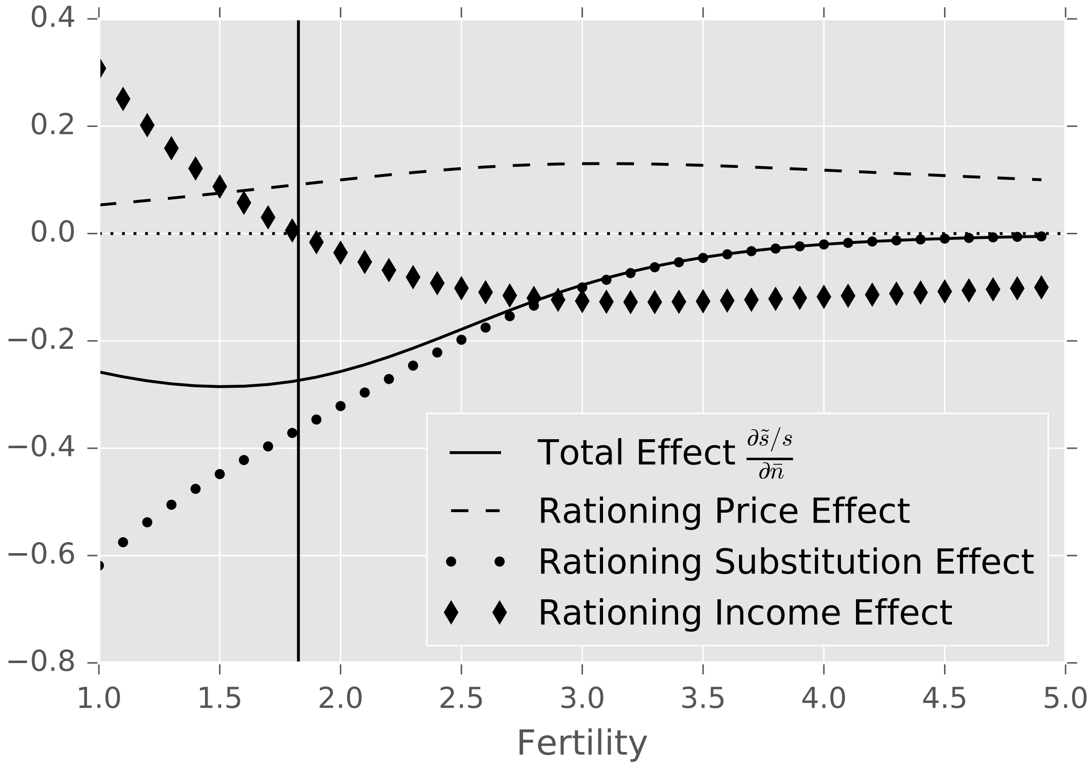

Figure A5: Decomposition of the marginal effect of rationed fertility on parental consumption. Notes: The x-axis shows the fertility of parents. The y-axis shows the semi-elasticity of the composite consumption good to child quantity ($\frac{\partial \tilde{s}/s}{\partial \bar{n}}$) and its decomposition terms (Equation 30 divided by s). In this figure, the Rationing Price Effect is $\pi_{ns}\frac{\partial s^{\tilde{*}c}/s}{\partial \pi_s^*}$, the Rationing Substitution Effect is $[1 - \alpha_{\Delta}\epsilon_{n^* \cdot y}]\frac{\partial s^{\tilde{*}c}/s}{\partial \bar{n}}$, and the Rationing Income Effect is $(\bar{\pi}_n - \pi_n)\frac{\partial s^*/s}{\partial y}$. Using semi-elasticity forms, the results are insensitive to the unit of measurement of child quality s. The utility function adopts a nested CES parameterization: $U(\bar{n}, q, s) = U_1^{\theta} s^{1-\theta}$ and $U_1 = (\alpha \bar{n}^{\rho} + (1-\alpha) q^{\rho})^{\frac{1}{\rho}}$. The budget constraint is $\pi_{nq}\bar{n}q + \pi_n \bar{n} + \pi_q q + \pi_s s \leq y$. The model parameters are set at $\theta = 0.5$, $\alpha = 0.5$, $\rho = -3$, $\pi_{nq} = 1$, $\pi_q = 0$, $\pi_n = 0$, $\pi_s = 1$, and $y = 10$. The unrestricted optimal fertility n^o is 1.83, which is denoted by the vertical line.

Table A1: Summary statistics, the Chinese Adult Twins Survey (CATS)

	Panel A. Family-level information			
	Non-twin Family (Obs.=1260)		Twin Family (Obs.=649)	
	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)
Number of children	3.32	1.24	4.46	1.23
Father's age	73.57	8.90	71.65	9.25
Mother's age	70.05	8.37	67.95	8.69
Father's years of schooling	8.54	3.21	8.75	3.39
Mother's years of schooling	7.49	2.57	7.63	2.71
	Panel B. Child-level information			
	Non-twin Family (Obs.=4177)		Twin Family (Obs.=2894)	
	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)
Age	43.03	7.54	41.21	8.07
Years of schooling	11.55	2.72	11.38	2.64
Male	0.49	0.50	0.51	0.50

Notes: This table presents summary statistics of the sample derived from the Chinese Adult Twins Survey (CATS). "Twin family" refers to families formed by a pair of adult twins, siblings of the adult twins, and their parents. The adult twins and their siblings are "children" in twin families. Similarly, "Non-twin family" refers to families formed by non-twin adults and their parents. The non-twin adults are "children" in non-twin families. Panel A presents the family-level information and Panel B presents the child-level information. "Years of schooling" is computed as follows: six years for "primary school or below"; nine years for "junior middle school"; 12 years for "senior middle school" or "technical high school"; 15 years for "three-year college"; 16 years for "four-year college"; and 19 years for "graduate school or above."

Table A2: Summary statistics, the Chinese Child Twins Survey (CCTS)

	Urban (non-exempt) sample				Rural (exempt) sample			
	Panel A. Family-level information							
	Non-twin (Obs.=567)		Twin (Obs.=500)		Non-twin (Obs.=364)		Twin (Obs.=278)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(7)	(8)	(9)	(10)
Number of children	1.05	0.22	2.02	0.13	2.01	0.09	3.00	0.06
Father's age	38.43	4.68	38.90	5.05	37.20	4.72	40.20	4.74
Mother's age	36.12	4.13	36.52	4.54	35.28	4.41	37.99	4.35
Mother's age at birth†	24.70	2.99	25.46	3.53	26.79	3.81	27.10	3.76
Father's schooling years	11.03	3.31	10.79	3.36	8.42	2.65	8.02	2.43
Mother's schooling years	10.66	3.03	10.27	3.26	7.36	2.50	6.83	2.39
	Panel B. Child-level information							
	Non-twin (Obs.=532)		Twin (Obs.=1000)		Non-twin (Obs.=514)		Twin (Obs.=748)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(7)	(8)	(9)	(10)
Age	11.44	2.71	11.48	2.88	11.74	2.84	11.93	2.93
Male	0.52	0.50	0.50	0.50	0.50	0.50	0.55	0.50
Birthweight (kg)	3.15	0.47	2.42	0.49	3.11	0.48	2.66	0.49
Expected schooling years	15.78	2.35	15.11	2.47	13.17	2.19	12.27	2.48
Child investment (¥/year)	2139.11	2409.27	1960.42	2302.67	882.30	990.20	772.61	730.35
Home tutorial (minutes/day)	20.66	21.12	23.70	22.51	10.99	14.81	9.08	15.09

Notes: † Mother's age at birth is mother's age at the potential twin birth, which equals mother's age at the first birth for the urban sample, and at the second birth for the rural sample. ¥ stands for Chinese Yuan. Expected years of schooling is computed from the following rule: three years for "below primary school"; six years for "primary school"; nine years for "junior middle school"; 12 years for "senior middle school"; 13 years for "vocational secondary school"; 15 years for "three-year college"; 16 years for "four-year college"; 19 years for "graduate school or above."

Table A3: First-stage regressions when all children are included, China population censuses

Panel A. Han versus Minority						
Sample	Han	Minority	Pooled I		Pooled II	
Dependent variable	N	N	N	N * Han	N	N * Han
	(1)	(2)	(3)	(4)	(5)	(6)
Twinning	0.746***	0.829***	0.779***	0.134***	0.829***	
	(0.018)	(0.065)	(0.068)	(0.043)	(0.065)	
Twinning * Han			-0.029	0.603***	-0.082	0.746***
			(0.070)	(0.047)	(0.067)	(0.018)
Han			-0.070***	2.719***	1.987***	2.497***
			(0.008)	(0.006)	(0.618)	(0.177)
Observations	417496	39240	456736	456736	456736	456736
R-squared	0.33	0.43	0.35	0.64	0.35	0.66
Panel B. Before versus after One-child Policy, Han						
Sample	Before-OCP	After-OCP	Pooled I		Pooled II	
Dependent variable	N	N	N	N * OCP	N	N * OCP
	(1)	(2)	(3)	(4)	(5)	(6)
Twinning	0.602***	0.751***	0.573***	0.019**	0.602***	
	(0.029)	(0.020)	(0.029)	(0.008)	(0.029)	
Twinning * OCP			0.228***	0.698***	0.150***	0.751***
			(0.036)	(0.022)	(0.035)	(0.020)
OCP			-0.471***	2.682***	0.108	2.587***
			(0.003)	(0.002)	(0.310)	(0.196)
Observations	362830	361895	724725	724725	724725	724725
R-squared	0.48	0.33	0.43	0.87	0.47	0.90
Panel C. Before versus after One-child Policy, Minority						
Sample	Before-OCP	After-OCP	Pooled I		Pooled II	
Dependent variable	N	N	N	N * OCP	N	N * OCP
	(1)	(2)	(3)	(4)	(5)	(6)
Twinning	0.630***	0.790***	0.630***	0.069	0.630***	
	(0.191)	(0.074)	(0.198)	(0.049)	(0.191)	
Twinning * OCP			0.216	0.660***	0.160	0.790***
			(0.212)	(0.089)	(0.205)	(0.074)
OCP			-0.586***	3.032***	-0.146	0.891
			(0.012)	(0.009)	(1.197)	(0.619)
Observations	23020	36455	59475	59475	59475	59475
R-squared	0.49	0.43	0.49	0.85	0.52	0.88

Notes: Panel A presents the first-stage regressions of the IV estimates in Table 2. Panel B and C present the first-stage regressions of the IV estimates in Table 3. Standard errors in parentheses are clustered at the family level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: The effect of rationed fertility on quality of first-born children, Han versus Minority

Panel A. IV estimates

Dependent variable	Primary school attendance				School enrollment			
Sample	Han	Minority	Pooled I	Pooled II	Han	Minority	Pooled I	Pooled II
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of children (N)	0.013	-0.084**	-0.084*	-0.084**	0.015*	-0.075**	-0.076*	-0.075**
	(0.009)	(0.037)	(0.045)	(0.037)	(0.009)	(0.037)	(0.045)	(0.037)
N * Han			0.097**	0.097**			0.091**	0.090**
			(0.046)	(0.038)			(0.046)	(0.038)
Han			-0.213*	-0.118			-0.196*	-0.031
			(0.118)	(0.323)			(0.119)	(0.331)
Observations	239737	21548	261285	261285	239737	21548	261285	261285

Panel B. First-stage estimates

Sample	Han	Minority	Pooled I		Pooled II	
Dependent variable	N	N	N	N * Han	N	N * Han
	(1)	(2)	(3)	(4)	(5)	(6)
Twinning	0.808***	0.872***	0.848***	0.104***	0.872***	
	(0.011)	(0.048)	(0.051)	(0.029)	(0.048)	
Twinning * Han			-0.038	0.697***	-0.064	0.808***
			(0.052)	(0.031)	(0.049)	(0.011)
Han			-0.055***	2.499***	1.644***	0.445***
			(0.006)	(0.004)	(0.580)	(0.166)
Observations	239737	21548	261285	261285	261285	261285
R-squared	0.27	0.37	0.28	0.64	0.29	0.65

Notes: We use twining at the second birth as the IV of N in columns (1), (2), (5), and (6). We use twinning at the second birth and its interaction with *Han* as IVs of N and $N \cdot Han$ in columns (3), (4), (7), and (8). The sample includes first-born children of families described in columns (1) and (2) of Panel A of Table 1. Additional control variables include mother's age at the second birth; the child's gender, age, age squared and age cubed; parents' years of schooling, age, andage squared; and prefecture fixed effects. We allow these control variables to have separate coefficients for Han and minority in "Pooled II" specifications in columns (4) and (8). Standard errors in parentheses are clustered at the family level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: The effect of rationed fertility on years of schooling of low-parity children (Chinese Adult Twins Survey)

Panel A. For children born prior to the parity of twinning			
Sample	2-plus	3-plus	4-plus
	(1)	(2)	(3)
Number of children	-0.762***	-0.504**	-1.202***
	(0.280)	(0.231)	(0.276)
Observations	1487	2083	1736

Panel B. For children born at each birth order					
Sample	3-plus	3-plus	4-plus	4-plus	4-plus
	1st-born	2nd-born	1st-born	2nd-born	3rd-born
	(1)	(2)	(3)	(4)	(5)
Number of children	-0.651**	-0.397	-1.125***	-1.299***	-1.262***
	(0.291)	(0.290)	(0.382)	(0.389)	(0.361)
Observations	1035	1048	569	590	577

Notes: This table presents IV estimates of rationed fertility on completed years of schooling of low-parity children using the CATS sample. When using a sub-sample of the n-plus sample, we use twinning at the nth birth as the IV for the number of children. The sample used in each column of Panel A includes children born prior to the nth parity in the n-plus sample ($n = 2, 3, 4$). Control variables include maternal age at the nth birth ($n = 2, 3, 4$); the child's gender, birth order, and birth year dummies; parents years of schooling and birth year dummies; and city-district fixed effects. Panel B presents IV estimates of Equation (A2.1) for children born at each birth order in the 3-plus and 4-plus samples. Column (1) presents the estimates for the sub-sample of first-born children in the 3-plus sample, and column (2), the estimates for the sub-sample of second-born children in the 3-plus sample. Columns (3), (4), and (5) present the estimates for the sub-samples of first-, second-, and third-born children in the 4-plus sample, respectively. Control variables include maternal age at the nth birth ($n = 2, 3, 4$); the child's gender and birth year dummies; parents years of schooling and birth year dummies; and city-district fixed effects. Standard errors in parentheses are clustered at the family level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Summary statistics of the Chinese Child Twins Survey, parental consumption and labor supply

	Urban (Non-exempt) Sample				Rural (Exempt) Sample			
	Non-twin (N=567)		Twin (N=500)		Non-twin (N=364)		Twin (N=278)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(7)	(8)	(9)	(10)
Family income (¥/year)	15475.31	12718.49	15252.51	13649.34	10704.67	8845.79	11032.37	9888.22
Paternal Expenditure								
Cigarette expenses (¥/month)	103.58	101.88	98.56	128.38	58.82	65.51	47.49	54.93
Alcohol expenses (¥/month)	17.09	39.31	15.89	45.65	11.69	15.23	13.15	23.42
Clothing expenses (¥/six months)	246.21	391.24	211.73	361.59	124.20	177.34	87.77	168.39
Dinner out without children (dummy)	0.37	0.48	0.27	0.45	0.19	0.39	0.11	0.31
Maternal Expenditure								
Cosmetics expenses (¥/six months)	146.49	281.76	100.67	164.89	18.12	39.14	10.10	32.93
Clothing expenses (¥/six months)	339.57	461.80	264.67	377.01	118.55	146.37	82.32	109.33
Dinner out without children (dummy)	0.33	0.47	0.24	0.43	0.07	0.26	0.05	0.21
Paternal Labor Supply								
Labor force participation (dummy)	0.80	0.40	0.79	0.41	0.83	0.37	0.82	0.38
Days worked last month	24.11	5.03	24.58	4.79	25.88	4.32	26.38	4.64
Hours worked last week	46.68	14.74	48.42	15.67	48.73	13.65	49.57	14.35
Labor income (¥/month)	873.48	658.39	930.58	920.29	538.34	678.73	472.28	435.73
Earnings per hour (¥/hour)	6.82	6.49	6.78	5.49	3.48	4.12	3.38	3.62
Private business (dummy)	0.18	0.38	0.21	0.41	0.21	0.41	0.33	0.47
Out home one month (dummy)	0.09	0.29	0.12	0.33	0.10	0.30	0.12	0.32
Maternal Labor Supply								
Labor force participation (dummy)	0.63	0.48	0.65	0.48	0.78	0.42	0.78	0.41
Days worked last month	23.50	5.11	24.30	4.64	25.76	4.96	26.53	4.57
Hours worked last week	43.85	13.66	45.99	14.25	47.38	15.01	48.06	15.06
Labor income (¥/month)	589.45	522.88	621.41	706.38	285.13	296.91	267.47	245.65
Earnings per hour (¥/hour)	5.74	4.21	5.91	5.49	2.17	3.41	2.16	2.42
Private business (dummy)	0.16	0.37	0.23	0.42	0.20	0.40	0.35	0.48
Out home one month (dummy)	0.05	0.21	0.04	0.21	0.02	0.14	0.04	0.20

Notes: ¥ stands for Chinese Yuan. "Dinner out' is an indicator variable that equals one if the father or mother has ever dined outside without bringing their children in the last month. "Private business" is an indicator variable that equals one if the father or mother has a private business. "Out home one month" is an indicator variable that equals one if the father or mother has left home for more than 30 days in the last 180 days.

Table A7: Twinning on parental consumption, gender effect (Chinese Child Twins Survey)

Dependent	Father's Consumption				Mother's Consumption		
Variable	Cigarette	Alcohol	Clothing	Dinner out	Cosmetics	Clothing	Dinner out
	Panel A. Urban sample						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twin girls	-0.179	-0.096	-0.351*	-0.054	-0.291*	-0.438**	-0.037
	(0.186)	(0.140)	(0.207)	(0.038)	(0.174)	(0.196)	(0.037)
Twin girl-boy	-0.048	0.055	-0.008	-0.056	0.035	-0.061	-0.073*
	(0.253)	(0.193)	(0.268)	(0.046)	(0.213)	(0.257)	(0.043)
Twin boys	-0.050	-0.333**	-0.301	-0.096***	-0.269	-0.387*	-0.088**
	(0.187)	(0.136)	(0.214)	(0.036)	(0.169)	(0.207)	(0.035)
Observations	1067	1067	1067	1062	1067	1067	1060
R-squared	0.03	0.02	0.07	0.08	0.16	0.12	0.08
	Panel B. Rural sample						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twin girls	0.095	0.053	-0.455*	-0.056	-0.339*	-0.404	-0.024
	(0.206)	(0.167)	(0.261)	(0.038)	(0.196)	(0.261)	(0.026)
Twin girl-boy	-0.256	-0.152	-0.839***	-0.082**	-0.455**	-0.734***	-0.040*
	(0.212)	(0.185)	(0.262)	(0.038)	(0.198)	(0.253)	(0.024)
Twin boys	-0.112	0.120	-0.574***	-0.019	-0.566***	-0.679***	-0.005
	(0.179)	(0.158)	(0.222)	(0.038)	(0.157)	(0.222)	(0.027)
Observations	642	642	642	642	642	642	641
R-squared	0.02	0.03	0.10	0.04	0.11	0.12	0.02

Notes: This table presents the OLS estimates of the gender effect of twinning on parental consumption using the CCTS sample. "Twin girls" is an indicator variable that equals one if the family has a pair of twin girls. "Twin boys" is an indicator variable that equals one if the family has a pair of twin boys. "Twin girl-boy" is an indicator variable that equals one if the family has a pair of mixed-gender twins. "Dinner out" is an indicator variable that equals one if the father or mother has ever dined outside without bringing their children in the last month. Other outcome variables are in log scale. We add one to these outcome variables before taking logs to exploit the information of zero expenditure. The results are robust if we do not add one, that is, if we use the truncated samples. Panel A presents the results for the urban sample, and Panel B, the rural sample. Control variables include maternal age at the first birth for the urban sample and at the second birth for the rural sample; and parents' years of schooling, age, and age squared. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: Twinning on parental labor supply, gender effect (Chinese Child Twins Survey)

Dependent Variable	Labor force participation (1)	Days worked last month (2)	Hours worked last week (3)	Labor income (4)	Earnings per hour (5)	Private business (6)	Out home one month (7)
Panel A. Urban father's labor supply							
Twin girls	0.022	-0.014	0.020	0.045	0.036	0.008	0.022
	(0.030)	(0.017)	(0.031)	(0.047)	(0.056)	(0.032)	(0.026)
Twin girl-boy	-0.031	0.031	0.060	0.154**	0.121	-0.033	0.047
	(0.048)	(0.025)	(0.038)	(0.073)	(0.089)	(0.048)	(0.035)
Twin boys	-0.015	0.045***	0.049*	0.071	0.004	0.074**	0.027
	(0.033)	(0.016)	(0.028)	(0.054)	(0.059)	(0.037)	(0.026)
Observations	1067	837	840	1030	834	845	1067
R-squared	0.09	0.07	0.03	0.20	0.23	0.12	0.03
Panel B. Urban mother's labor supply							
Twin girls	0.068*	0.006	0.040	-0.014	0.012	-0.003	-0.004
	(0.036)	(0.019)	(0.029)	(0.085)	(0.062)	(0.034)	(0.018)
Twin girl-boy	0.018	0.016	0.059	0.196**	0.169	0.071	0.009
	(0.051)	(0.032)	(0.044)	(0.088)	(0.109)	(0.058)	(0.023)
Twin boys	0.048	0.032*	0.053	0.121*	0.028	0.125***	-0.006
	(0.038)	(0.019)	(0.034)	(0.067)	(0.071)	(0.042)	(0.017)
Observations	1066	674	676	903	665	681	1067
R-squared	0.13	0.08	0.03	0.14	0.30	0.16	0.02
Panel C. Rural father's labor supply							
Twin girls	0.005	0.032	0.003	0.046	0.093	0.150**	0.019
	(0.050)	(0.020)	(0.037)	(0.093)	(0.117)	(0.064)	(0.037)
Twin girl-boy	-0.015	-0.019	0.006	-0.061	0.010	0.114*	0.036
	(0.052)	(0.037)	(0.044)	(0.105)	(0.129)	(0.068)	(0.040)
Twin boys	0.031	0.002	0.033	-0.042	0.040	0.091*	0.059
	(0.042)	(0.023)	(0.039)	(0.090)	(0.106)	(0.052)	(0.036)
Observations	642	530	528	629	529	532	642
R-squared	0.02	0.02	0.02	0.11	0.14	0.05	0.02
Panel D. Rural mother's labor supply							
Twin girls	-0.008	0.005	-0.043	0.216**	0.215*	0.168**	0.016
	(0.054)	(0.032)	(0.050)	(0.089)	(0.116)	(0.066)	(0.021)
Twin girl-boy	-0.049	0.022	0.023	-0.072	-0.081	0.152**	0.046
	(0.057)	(0.037)	(0.049)	(0.113)	(0.142)	(0.073)	(0.028)
Twin boys	0.038	0.018	-0.001	-0.068	0.061	0.121**	0.032
	(0.044)	(0.024)	(0.046)	(0.091)	(0.108)	(0.053)	(0.021)
Observations	640	498	496	587	484	500	642
R-squared	0.02	0.01	0.02	0.12	0.13	0.07	0.02

Notes: This table presents the OLS estimates of the gender effect of twinning on parental labor supply. "Twin girls" is an indicator variable that equals one if the family has a pair of twin girls. "Twin boys" is an indicator variable that equals one if the family has a pair of twin boys. "Twin girl-boy" is an indicator variable that equals one if the family has a pair of mixed-gender twins. Outcome variables in columns (2)–(5) are in log scale. Outcome variables in columns (1), (6), and (7) are dummy variables. Panels A and B present the results for the urban sample, and Panels C and D, the rural sample. Control variables include maternal age at the first birth for the urban sample and at the second birth for the rural sample; and parents' years of schooling, age, and age squared. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.