Housing Wealth and Fertility in China: A Regression Discontinuity Design *

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March, 2023

ABSTRACT

In this paper we examine the effects of housing wealth on fertility outcomes using a regression discontinuity design based on a 2006 Chinese housing policy. We show that the positive shock to housing wealth generated by this policy increased the likelihood of fertility by a significant margin of 3.6%. Our result implies that a 1% increase in housing wealth can raise the fertility rate by 0.34%. We also show that children born after the positive housing wealth shock exhibit better health conditions not only at birth but also in the long run. Moreover, we present suggestive evidences showing that both labor-market participation and parental health could explain the documented positive effects of housing wealth on fertility rates and child health.

JEL classification: J13, O18, R21

Keywords: Housing wealth; Fertility; Labor Participation; Health

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* We thank the seminar participants at Peking University, Renmin University of China, and the 5th International Conference of China Development Studies (ICCDS) at Shanghai Jiao Tong University. We thank Yanjiang Zhang for providing the data of housing transactions. The authors are responsible for all the errors. Declarations of interest: none.

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1. Introduction

In the literature, the fertility decision of a family has been found largely affected by the household economic resources (Jones et al., 2011; Lindo, 2010; Black et al., 2013; Brueckner and Schwandt, 2015; Kearney and Wilson, 2018; González and Trommlerová, 2021; Gallego and Lafortune, 2023).[1] Dating back to the 1960s, Becker (1960) introduced children in parental utility functions as durable goods. Because there are no substitutes for particular children, they are often regarded as normal goods, implying that an increase in family wealth should positively affect fertility choices.

Among all family economic resources, housing is often the largest component in the household asset portfolio (Iacoviello, 2011; Wolff, 2012; Bricker et al., 2019). Changes in housing wealth could have significant impacts on important household decisions, such as labor participation (Adelino et al., 2015; Li et al., 2020), migration (Hämäläinen and Böckerman,2004; Plantinga et al.,2013), and marriage (Wrenn et al., 2019; Chu et al.,2020; Sun and Zhang, 2020). In particular, there is a strand of literature studying the effect of housing wealth on fertility. Existing studies have found evidence that housing wealth increases fertility rates of developed economies such as the United States (Lovenheim and Mumford, 2013), Australia (Atalay et al., 2021), Japan (Mizutani, 2015), Canada (Clark and Ferrer, 2019) and Denmark (Daysal et al. 2021).

Empirical evidences on the influence of housing wealth on fertility rates in developing countries have, however, been rare and a consensus has not been reached.[2] China, one of the largest developing economies and the world's most populous country, has seen low fertility rates partly due to the one child policy implemented for decades. Although this policy restriction has been largely relaxed in recent years,[3] the birth rate in China still remains low. It is puzzling that

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[1] Lindo (2010) find that income shock caused by job displacement of a husband leaded to decrease in spousal fertility. Black et al. (2013) find that coal-rich US counties that experienced a boom due to rising energy prices had an increase in fertility. Kearney and Wilson (2018) look at the fracking boom and show that it increased fertility but not marriage. Gallego and Lafortune (2023) study the economic condition variation caused by commodity shocks and find positive shocks lead to increase in fertility. González and Trommlerová (2021) show the asymmetric fertility response to positive and negative economic shocks, and find an increase in fertility caused by introduction of child benefit policy, but a larger decrease in fertility upon the later cancellation of the policy.

[2] Past studies set in developing economies focus on the effects on fertility of housing size, housing quality, and housing costs. For example, Briggs et al. (2018) find that, in India, homeowning households experience higher completed fertility than renters do, whereas the effects of housing size are not significant. In another study, Paydarfar (1995) finds that, in Iran, females who reside in single-family housing units exhibit significantly higher actual and desired fertility than females who reside in multi-family housing units. According to Nelwamondo (2017), in the long run, a 1 percent increase in housing prices decreases the fertility rate by around 0.10% in South Africa.

[3] Following the launch of China's two-child policy in 2016, the government officially announced the three-child policy in May 2021, with the hope of increasing family size to solve the long-term negative influence of the one child policy that was effective for more than three decades.

the argument of housing wealth enhancing fertility rates does not seem to hold in the Chinese context, where we observe a declining birth rate against the backdrop of soaring housing prices over recent decades and a homeownership rate as high as 90% (Glaeser et al., 2017). As shown in Figure 1, while the average housing price nationwide increased from 2,000 to 7,000 *yuan* over the past two decades, the fertility rate exhibited a declining trend, although the curve appears to flatten out since 2010. Liu et al. (2020) use population census data from China and find that higher housing prices significantly reduce the fertility probability among renter families and those with self-built homes, but the response is nonsignificant for homeowners. Liu. et al. (2021) find that, among homeowning women, a 100,000-yuan increase in housing wealth results in a 14% decrease in the likelihood of giving birth.

[Insert Figure 1 Here]

Why so? In this paper, we aim to solve this puzzle by investigating the effects of housing wealth on fertility in China using a policy-driven discontinuity design. For the purpose of identification, we adopted *National Article Six (2006)*, a housing regulation that reduces the ratio of minimum down payments for individual home mortgage loans to 20% for those who buy self-occupied housing units with floor space less than or equal to 90 m^2. Since then, smaller houses (with under or equal to 90 m^2 in floor space) have become increasingly popular and more expensive in terms of unit price. The policy, which we shall explain in greater details later, creates a clear-cut discontinuity in housing size with respect to housing-wealth growth since 2006, particularly for households that purchased houses before 2006. Our main methodology involves comparing the fertility outcomes of households with houses just below (or equal to) and those with houses just above the policy threshold size (90 m^2) through the method of regression discontinuity (RD).

Using survey data from China Family Panel Studies 2018 (CFPS 2018), a representative household-level survey in China, we find that households that purchased commercial residential houses sized just below (or equal to) 90 m^2 in floor space before 2006 are more likely than those buying houses just above 90 m^2 in floor space before 2006 to bear children by a significant margin of 3.6% , after controlling for a vector of predetermined demographic and socioeconomic variables. We address threats to our identification strategy by testing the validity of the crucial assumption, i.e., non-full manipulation. Also, we address concerns that may threaten the validation and the interpretation of the results, including the choice of bandwidth, and the endogeneity issues of housing size with respect to fertility decisions.

To measure the elasticity of housing wealth on fertility, so as to facilitate a comparison with the existing literature (Lovenheim and Mumford, 2013; Dettling and Kearney, 2014), we use the abovementioned policy shock as an instrumental variable (IV), and estimate that a one-percentage-point increase in the annualized growth rate of housing prices increases the number of newborns by 0.069. Because the annualized housing price growth rate for houses just below (or equal to) 90 m^2 in floor space is 1.6% higher than the annualized housing price growth rate for those just above 90 m^2 after the policy shock, our estimated results imply that a 1% increase in housing wealth can raise the fertility rate by 0.34%. This result is robust to using the number of children born after 2006 as the dependent variable. We also conduct a series of robustness checks and placebo tests, including alternative fixed effects, a nonlinear model specification, the use of alternative cutoff points, and restricting sample to homes purchased after 2006.

We further investigate how changes in housing wealth affect children's health, including weight at birth, weight, and height as well as academic performance at the survey year. We find that children born in families experiencing housing wealth growth present higher birth weights and continue to weigh more than other children afterwards; and they are also taller, and perform better in reading and math.

We then discuss two mechanisms that might drive the above effects. Consistent with the findings of Li et al. (2020), we find that individuals who experience positive wealth shocks are more likely to quit the labor market. Interestingly, we find that males hit by the positive wealth shock are more likely to quit the labor market before the childbirth, suggesting husbands tend to provide greater pre-fertility support to wives by sparing more time in the family. However,, after the childbirth, females are more likely to quit the labor market , which suggests the positive wealth shock allows wives to stay at home full time for childcare. Overall, our findings support the notion that an increase in housing wealth, even without being cashed out, may lend greater confidence to couples in their financial conditions and encourage them to shift their attention from work to fertility for a while.

The second possible channel that might drive our findings is that housing wealth increase improves parents' physical and mental health. We find strong evidence that, before childbirth, individuals experiencing higher housing wealth growth report to be in better physical and mental health statuses. This is consistent with the notion that parental physical and mental health affects pregnancy and infant health (He et al., 2017; Fleming et al., 2018).

This paper contributes directly to previous studies on the effects of housing wealth on household fertility choices. While such effect has been investigated in the context of developed economies (Lovenheim and Mumford, 2013, Mizutani, 2015; Clark and Ferrer, 2019; Atalay et al., 2021; Daysal et al. 2021), the evidence in developing countries is relatively rare without a consensus reached (Nelwamondo, 2017; Briggs et al., 2018; Liu et al., 2020; Liu. et al.,2021), and the underlying mechanisms remain unclear. In our paper, we focus on an exogenous housing market policy shock which causes a change in housing wealth, and provide solid identification of a causal relationship between housing wealth and fertility rates in China. We also discuss two possible channels at the individual level: labor-market participation (Fang et al., 2013) and parental health (He et al., 2017; Fleming et al., 2018). There have been few micro-level empirical evidences in the literature supporting these channels. We find that policy-driven housing wealth growth is accompanied by decreased parental labor supply and improved parental physical and mental health, which might explain the observed positive effects of housing wealth on fertility rates.[4]

Our paper also contributes to the literature on household wealth shocks and children's well-being. Researchers find that reduced family resources and negative economic shocks increase the infant mortality rate, make preterm delivery more likely, decrease birth weights (De Cao et al., 2022),and hurt children's health (Baird et al., 2011; Schady and Smitz, 2010) These adverse early-life conditions could have long-lasting effects into adulthood (Almond and Currie, 2011; Currie and Almond, 2011; Almond et al., 2018). For example, higher birth weights are found to be correlated with higher adult schooling attainment, adult height, IQ, and earnings (Behrman and Rosenzweig, 2004; Black et al., 2007). Recent studies, such as Adhvaryu et al. (2019), find evidence that household income shocks at birth affect adult mental health. Very few studies have discussed the effect of housing wealth on children's health, whilst an exception is Daysal(2021), who finds that housing price increases are correlated with higher birth weight and fewer premature

[4] He et. al (2017) find that parental obesity has a significant impact on a newborn child's birth outcomes and health. Fleming et al. (2018) additionally find that parental environmental factors, including diet, body composition, and mental status, affect health and chronic disease risk for children and thus parents suffer from poor physical and mental health are advised to delay conception.

births. We not only find a housing wealth effect on child health conditions at birth, but also observe a long-term impact on children's weight, height, and academic performance.

Finally, previous studies on the relation between housing wealth and household fertility decisions are often challenged by various identification issues. For example, , fluctuations in housing wealth might be related to unobservable household-level characteristics, which may also affect fertility decisions. Daysal et al. (2021) rely on a key identification assumption according to which changes in home prices are unrelated to unobserved characteristics that also correlate with the likelihood of producing children. This could be true in the country in which their study is set, namely Denmark, but may not hold in other contexts such as China.[5] A common approach to resolving this issue involves using exogeneous instrumental variables such as a housing market boom or land supply elasticity to quantify the impact of housing wealth on fertility behaviors (Lovenheim and Mumford, 2013; Dettling and Kearney, 2014). Such regional-level price shocks however fail to capture price variations across communities within the same city. Moreover, home purchasing decisions may be influenced by fertility decisions. For example, households with strong intentions to produce children may choose to purchase homes where housing prices are lower (Liu et al., 2021). Moreover, households with children may sort into locations with better school or public transit access where home values appreciate more quickly. Our paper contributes to the literature by addressing these identification issues using a policy-driven regression discontinuity design, which could largely mitigate the sorting problem and thus identify the wealth effect more accurately.

The rest of the paper is organized as follows. In Section 2 we describe the background of Chinese housing-market policies. In Section 3 we summarize our data and the empirical framework. In Section 4 we present the main results pertaining to fertility outcomes and children's health as well as a bunch of robustness checks. In Section 5 we examine and discuss the possible mechanisms that could contribute to the positive effects of housing wealth on fertility and child health. In Section 6 we provide some further discussions. We conclude in Section 7.

[5] Housing prices are highly endogenous to macroeconomic variations such as differences in migration policy (Wang and Zhang, 2014), which may in turn simultaneously affect real estate prices and fertility decisions. As such, a carefully-designed empirical strategy must be implemented when we seek causal evidence of the impact of housing prices on fertility rates in large economies like China.

2. Background

The rapid development of a market economy since the Reform and Opening led China in the late 1990s to abolish the Soviet-type housing distribution model and adopt market-oriented housing-market reform, resulting in rapid expansion of the Chinese real-estate market as well as significant growth in housing prices. Data obtained from the National Bureau of Statistics of China indicate that the average annual growth rates for residential housing prices across 35 major Chinese cities were approximately 12.70% in the 2002–2010 period and 7.85% in the 2010–2019 period. Meanwhile, from a microeconomic perspective, housing assets have gradually occupied the largest share of Chinese household wealth. According to Xie and Jin (2015), housing assets accounted for 73.9% of total household wealth in 2012, which decomposes into 78.7% in urban China and 60.9% in rural China. Furthermore, Li et al. (2020) show that housing assets play a dominant role and account for over 60% of total household assets for a majority of households using 2017 data. We further pinpoint the effects of substantial changes in housing wealth on childbearing.

In the late 2000s, the Chinese government enacted a series of housing-market policies. In May 2006, the State Council approved a document, *Suggestions on Adjusting Housing Supply Structure to Stabilize Housing Price* (also called *National Article Six*), composed by the Ministry of Construction, Development and Reform Commission, the Ministry of Supervision, the Ministry of Finance, the Ministry of Land and Resources, the People's Bank of China, the State Administration of Taxation, the National Bureau of Statistics, and the China Banking Regulatory Commission. It includes a policy for adjusting homebuyers' financing that reduces down payments for individual home mortgage loans to 20% for those who buy self-occupied housing units with floor space less than or equal to 90 m^2, and since June 2006 the ratio must not be less than 30% for other types of buyers. This regulation was designed to restrain the rapid growth of housing prices while also considering the basic housing needs of low-income and middle-income individuals and families by promoting more affordable and smaller units. Subsequently, in November 2008, the Ministry of Finance and the State Administration of Taxation proposed reducing property taxes for first-time buyers of housing units with floor space less than or equal to 90 m^2 to encourage first-time homebuyers and relieve the negative consequences of the 2008 global financial crisis by reducing the tax burden. Meanwhile, discontinuity in the sizes (90 m^2) of housing units is thus generated by those policies, as apartments just below and those just above the 90 m^2 cutoff are likely to share similar attributes but experience differential growth rates in housing prices after 2006, because smaller units have become more popular. These facts lay the foundation of the empirical strategy we adopt in this paper.

3. Data and empirical framework

3.1 Data and variables

This study uses 2018 China Family Panel Studies (CFPS) data as the primary source for the empirical analysis. The CFPS is a nationally representative survey that covers communities, families, and individuals across 25 (of 31) Chinese provincial regions. The baseline survey was initiated in 2010 by the Institute of Social Science Survey of Peking University in China, which successfully surveyed and tracked 14,960 households and 42,590 individuals at a response rate of around 79 percent.

In the design stage, the CFPS draws on many methods, tools, and successes of several advanced survey projects worldwide, including the Panel Study of Income Dynamics (PSID), National Longitudinal Surveys of Youth (NLSY), and the Health and Retirement Study (HRS). The CFPS consists of five types of questionaries: community/village, family roster, family, adult, and children, providing extensive data on houses/apartments (home ownership, housing floor size, purchase year, move-in year, housing costs, and current market prices), family economies, and individual demographic and socioeconomic details (birth year, birth month, ethnic status, marital status, marriage year, years of education, rural or urban residency (hukou), employment status, and health status). Most importantly, the CFPS surveys not just one or a few adults in a household; all family members (including children) who meet the requirements are required to fill out the corresponding personal questionnaire, providing highly accurate and comprehensive data. In addition, the unique design of the CFPS enables researchers to identify the most direct relationships between family members and to infer knowledge about the parents, children, spouses, siblings, and grandchildren of each interviewee in a given family. Hence, we can also infer the number of children of and additional information on children's birth years, gender, and so on.

This paper focuses on the latest CFPS wave (2018) in the primary analysis to mitigate the impact of the strict family-planning policy, which was entirely abolished in 2016. In other words, fertility outcomes before 2016 might still be largely restricted by the dominant OCP and partial two-child policy. To fulfill the crucial assumption of an RD design, namely non-full manipulation of the running variable (housing size), the analysis sample is limited to commercial residential units acquired before 2006. The analysis sample that was eventually selected includes 3,242 households obtained from the CFPS 2018. We treat childbirth as a joint parental decision and construct a sample of households; as supplementary evidence, we also restructure the data at the individual-parent level to capture the effects of individual characteristics on the father's and mother's side of a given family.

3.1.1 Outcome variable measures

We include two outcome variables to measure each household's or individual's fertility behavior after 2006: the number of children born after 2006 and a dummy variable that indicates the presence of newborns after 2006. Both could be constructed using children's birth years after 2006 with family roster data where detailed information on respondents' parents, spouses, and children are recorded.

3.1.2 Running variable

This paper focuses on the *National Article Six* policy, which states that houses sized below or equal to 90 m^2 enjoy lower down payments. The policy induced many households to pursue housing in the small-unit market and caused smaller homes to be priced higher in the later years of the sample period. Thus, in our discontinuity design that we introduce later, we choose housing size as the running variable, where 90 m^2 in floor space is used as the cutoff point.

3.1.3 Predetermined control variables

Predetermined variables are included in this study, serving two purposes. First, the outcome variable is fertility outcomes, which could be affected by many factors, such as age, hukou status, and years of education. Hence, we should consider those control variables to prevent or limit omitted-variable bias. Second, to ensure the validity of the RD design we must assume non-full manipulation, which requires these to be continuous variables at the cutoff point. Doing so indicates that the running variable cannot be fully manipulated, according to Lee (2008). To test this assumption, we lay out a set of predefined variables whose values were fixed characteristics or were decided before housing purchase years.

At the household level, we include the following predetermined controls: (1) *Previous Children*: the number of children already in a given household before the policy became effective, i.e., 2006; (2) *Previous Boy Dummy*: a dummy variable that indicates the presence of male children before 2006; (3) *Age(m)*: the mother's age as of 2006; (4) *Educ Dummy(m)*: an indicator of whether the mother has finished the national nine-year compulsory education; (5) *Married(m)*: a dummy variable that indicates whether the mother was married in 2006; (6) *Han*: a dummy variable that equals one if the parents are Han Chinese and zero if either member belongs to the ethnic minority; (7) *Urban Hukou(m)*[6]: a dummy variable that equals one if the mother has a non-agricultural residency hukou and zero if she has an agricultural residency hukou; (8) *House Age*: the age of a house as of 2006； (9) *School Zone*: a dummy variable to indicate the house is within 3km of a school.

For the analysis at the individual-parent level, the predetermined variable could be a bit different. While we control for *Previous Children, Previous Boy Dummy*, *House Age* and *School Zone* as in the household-level regressions, here we also control for a gender dummy, namely *Male*, that equals one if a respondent is male and zero if a respondent is female. Moreover, *Age*, *Educ Dummy*, *Married*, *Han*, and *Urban Hukou* are all measured at the individual level instead of at the mother level.

3.2 Summary statistics

Summary statistics for the outcome variable, the running variable, and the control variables included in the study are presented in Panel A of Table 1, where the number of observations, averages, standard deviations, minimum values, and maximum values of each variable are reported. Because homeowners whose houses are smaller than (or equal to) 90 m^2 in floor space have enjoyed the housing-price premium since the introduction of *National Article Six*, they are defined as the treatment group, while households with housing units greater than 90 m^2 in floor space are defined as the control group. Across all households, the average number of children prior to the shock is around 1.3 and the proportion of respondents having at least one boy before 2006

[6] In China in particular, fertility behavior cannot be discussed without mentioning the well-known mandatory household registration hukou system that legally documents birthplaces, addresses, and family composition. Chan and Zhang (1999) argue that, since the 1950s, workers who have migrated from rural areas to cities are excluded to the greatest degree from urban educational resources, social welfare programs, property purchases, and job opportunities because they lack non-agricultural hukou status. This policy serves not only as population mobility control but also as a major tool for maintaining the social and economic configuration and restricting free flows of resources. Fertility behavior is therefore largely constrained by parents' hukou status, especially for rural–urban immigrants.

is roughly 58.5%. Mothers' age averages about 45.7 years as of 2018. Around 55.6% of the mothers received education above or equal to 9 years as of 2018. Most of the observations of the sample population involved married couples. Moreover, about 13.3% of the houses are located close to school. Panel B of Table 1 reports the summary statistics of samples after restricting the housing sizes within the optimal bandwidth as used in the baseline regressions below.

[Insert Table 1 Here]

3.3 Empirical framework

Past studies rely heavily on evidence from local housing-market fluctuations, especially regarding housing prices, which previous studies have treated as exogenous to households. The underlying identification issue is, however, that households do not always buy houses at random. As a result, increases in housing wealth may be accompanied by variations in household characteristics, causing estimations to be significantly biased. In other words, it is challenging to distinguish the effects of changes in housing property prices from the effects of other housing features. Hence, we adopt an RD specification with the cutoff point derived from a particular series of housing policies and carefully address numerous identifying criteria that underlie the RD framework instead of using variations in local housing markets as shocks or instruments.

Specifically, to estimate the treatment effect of unexpected housing wealth shocks on fertility choices, this paper applies a sharp RD framework derived from the *National Six Rule*. Under the rule, down payments and property taxes were favorably lower for housing units below (or equal to) a chosen cutoff floor space (90 m^2), resulting in a discontinuity in growth rates of housing wealth for homeowners. In other words, whether a given household was eligible for favorable conditions and would experience faster growth in housing assets depended on housing-unit floor space. No one who occupied apartments that were larger than 90 m^2 in floor space had access to the advantageous conditions, while individuals who purchased apartments that were 90 m^2 or smaller in floor space before 2006 enjoyed this relatively favorable advantage.

With a reasonable research design, we are able to compare fertility decisions between households with houses just below (or equal to) and those with houses just above the cutoff point of 90 m^2 in floor space. This criterion enables us to capture the effects of changes in housing wealth on fertility while addressing the concern that the two groups may experience substantially different wealth conditions. That is to say, it is highly likely that homeowners with larger houses constitute a richer cohort, and we cannot simply compare the fertility rate between a household occupying 70 m^2 in floor space with another household occupying 150 m^2 in floor space, as their predetermined wealth endowments, observable or unobservable, may differ greatly. Therefore, an RD design where an appropriate bandwidth around 90 m^2 is assigned could satisfy the identification need. After the housing policies became effective, however, households could intentionally choose housing units with desired floor space to receive the treatment, requiring us to focus on housing units purchased before 2006.

This paper adopts a parametric approach to estimate the RD models. The outcome variable is fertility, specifically the number of children born after 2006 or the dummy for having children after 2006 for individual i, which is denoted as Y_{it}. The assignment variable is housing size, denoted as X_i. The assignment variable should be centered at the cutoff point, so let $\widetilde{X_i} = X_i - X_0$, where $X_0 = 90$. Let T_i=1 for individuals occupying housing units smaller than or equal to 90 m^2 in floor space and T_i =0 for those occupying housing units larger than 90 m^2 in floor space. Hence, define T_i as

$$T_i = \begin{cases} 1 & if\ X_i \leq 90 \\ 0 & if\ X_i > 90 \end{cases}$$

where Y_{i1} represents the fertility outcome of individual i at T_i=1 and Y_{i0} represents the fertility outcome of individual i at T_i=0. Hahn et al. (2001) show that the treatment effect (particularly, the intention-to-treat—ITT—effect) may be pinned down by assuming that $E(Y_{i0}|X_i = X)$ is continuous in X at X_0 as $\hat{\beta}_0 = \lim_{X \downarrow X_0}[Y_i|X_i = X] - \lim_{X \uparrow X_0}[Y_i|X_i = X]$.

Equation (1) represents the primary model of parametric estimation that could be used to estimate $\hat{\beta}_0$:

$$Y_i = \alpha + \beta_0 \cdot T_i + g(\widetilde{X_i}) + \pi' \cdot Z_i + \epsilon_i \quad (1)$$

$$where\ 90 - b \leq X_i \leq 90 + b$$

where $g(\widetilde{X_i})$ represents a polynomial function of the centered assignment variable $\widetilde{X_i}$, including the interaction term for the centered running variable and the treatment variable. Z_i represents a vector of predetermined and control variables for household or individual demographic and socioeconomic characteristics as described in Section 3.1. b denotes the estimated optimal bias-corrected bandwidths. In particular, we find the optimal bandwidths based on the data-dependent technique developed by Imbens and Kalyanaraman (2012), which aims at achieving an optimal balance between precision and bias. This type of "rule of thumb" bandwidth-selection method is able to raise the precision of estimation through carefully brining more observations into consideration, while mitigate the negative effects of bringing too many of them that eventually bias the estimate (Lee and Lemieux, 2010).

The coefficient β_0 to be estimated is the target of interest that indicates the housing cooling measures' marginal effects at the cutoff point for additional children. Specifically, the baseline model is settled as:

$$Y_i = \alpha + \beta_0 \cdot T_i + \beta_1 \cdot \widetilde{X_i} + \beta_2 \cdot T_i \cdot \widetilde{X_i} + \pi' \cdot Z_i + \epsilon_i \tag{2}$$

In addition to Z_i, we also include county-level fixed effects. Moreover, in view of the fact that the reported housing sizes exhibit an obvious pattern of bunching at multiples of ten, we include fixed effects indicating multiples of 10 in housing size, i.e., whether floor space is 70 m^2, 80 m^2, 90 m^2, 100 m^2, or 110 m^2. Because the assignment's cutoff point is 90 m^2, there is a chance that the estimated treatment impact reflects clustering rather than the true effects of housing wealth changes. Specially, regarding the bunching at the cutoff point 90 m^2, we perform a falsification test by randomly assigning a size value between 88 and 92 to the observations with exact 90 m^2 floor spaces and then rerunning the baseline regressions for 1,000 times in Section 4.4.5.

3.3.1 Test on household composition

The validity of the RD estimate relies on the assumption that $E(Y_{i0}|X_i = X)$ is continuous in X at X_0. In other words, at the point where treatment and result discontinuities occur, all predetermined variables other than the treatment and outcome variables are continuous. This indicates that the assignment variable, housing size, cannot be fully manipulated, as argued in Lee (2008). If true, this ensures that individuals who were just barely treated are equivalent to those who were just barely not treated, as treatment status could be considered effectively random. This study then employs a composition check to test the validity of this crucial assumption, following Lee and Lemieux (2010). As a result, this paper examines whether predetermined socioeconomic traits are smooth at the cutoff point. In particular, we would observe discontinuities in these predefined features at the cutoff point if full manipulation existed in the study.

For the household-level sample, we examined eight predetermined variables at the cutoff point of 90 m^2 in floor space: *Previous Children*, *Previous Boy Dummy*, *Age(m)*, *Educ Dummy(m)*, *Marital Status(m)*, *Han*, *Urban Hukou(m)*, *House Age, and School Zone*. As shown in Figure 2, all predefined variables show nonsignificant discontinuities at the cutoff point of 90 m^2. In Table 2, we show the results of regressions as specified in equation (2), where the dependent variables are household-level predetermined variables and the independent variable is *T*. For the predetermined control variables there are no statistically or economically significant discontinuities, which is in line with Figure 2.

[Insert Figure 2 Here]

[Insert Table 2 Here]

3.3.2 Test on density manipulation

The second standard check on the validity of our RD design is to test if the running variable can be manipulated around the policy threshold. McCrary (2008) proposes a formal density manipulation check, to test if the marginal density of the running variable is continuous without manipulation around the threshold. It is a method based on a nonparametric local-polynomial density estimator, which requires prebinning of the sample and introduces two tuning variables (the number of bins and optimal bandwidths). Cattaneo, Jansson, and Ma (2020) improve the manipulation test by introducing a nonparametric estimator, which does not require transforming the data or tuning the number of bins and benefits from the favorable features related to the local polynomial method.

In our case, if the housing-unit floor spaces are fully manipulated to enjoy a lower down payment and taxes, we would observe a discontinuity in the density distribution for the running variable, namely housing size, around the threshold of 90 m^2. We therefore conduct the manipulation testing that closely follows the local-polynomial density estimators as proposed in Cattaneo, Jansson and Ma (2020). Figure A1 shows graphically that there are no jumps in the density distribution on the left and right sides of the policy cutoff. To construct the density estimators, we adopt the estimated optimal bandwidth of 23.9 at the left-side of the cutoff point, and the optimal bandwidth of 25.5 at the right-side of the cutoff point. The test yields a p-value of 0.83, which means that we cannot reject the null hypothesis of the continuity of the assignment variable's density at the cutoff point.[7]

4. Empirical results

4.1 Baseline results

The relationship between our two fertility measures (the dependent variables) and the floor spaces of housing units (the running variable) is graphed in Figure 3. Each circle in a given size bucket stands for the average values of the two fertility measures, i.e., the number of children born after 2006 and the dummy indicating the presence of children after 2006. The vertical line represents the cutoff point at 90 m^2 in floor space. The two fitted lines around the cutoff point represent the estimated local linear regression with mean square error (MSE) optimal bandwidth. The area between the dashed lines is the 95% confidence interval. In the left-side plot, we observe a clear negative jump from the left side to the right side of the cutoff point. Specifically, families with housing units just below 90 m^2 in floor space have more children after 2006 than those with housing units just above 90 m^2 cutoff. A similar effect is observed in the right-side figure, whereas the discontinuity is weaker when plotted with raw data.

[Insert Figure 3 Here]

In the regression analysis below, we show the discontinuity effect of housing size on childbirth conditional on predetermined variables and several fixed effects. In terms of the individual-level sample, as shown in Appendix Figure A2, in both plots we observe a clear negative jump from the left side to the right side of the cutoff point, indicating that individuals with housing units just below 90 m^2 in floor space, after experiencing the positive wealth shock, produce more children than those with housing units just above 90 m^2 in floor space.

The results of local linear regression estimates are reported in Table 3, with two dependent variables, namely the number of children and the having-children dummy, respectively, for each column. The baseline model is the regression of the fertility measure on the centered assignment variable $\widetilde{X}_i$, the treatment indicator T_i, the interaction term between the centered running variable and the treatment variable $\widetilde{X}_i * T_i$, a dummy for multiples of ten sqm,and county-level fixed effects. A vector of predetermined controls Z_i is then added. We carry out the regression at the

[7] We also adopt the standard method for manipulation testing provided in McCrary (2008) as robustness checks. The test yields an estimate of -0.0041, a standard error of 0.0435, and thus a p-value of 0.46, which is in line with our results under Cattaneo, Jansson and Ma (2020).

household level. Generally, the results show a consistent pattern such that households occupying apartments smaller than (or equal to) 90 m^2 in floor space are predicted to produce more children than those with larger apartments, a finding that is consistent with the pattern depicted in Figure 3. In column (1), our results show that households that experience the positive housing wealth shock tend to produce more children by a significant margin of 0.125, representing 27.7% of total fertility given that the average number of children is 0.451.

[Insert Table 3 Here]

In terms of fertility probability, we see that individuals in the treated group are 6.5% more likely to produce children than those in the control group, as shown in column (2). For the household-level analysis, we control for *Previous Children*, *Previous Boy Dummy*, *Age(m)*, *Educ Dummy(m)*, *Marital Status(m)*, *Han*, *Urban Hukou(m)*, *House Age, School Zone*. The effects remain quite stable when we add a handful of predetermined variables for columns (3) and (4).[8] When controlling for all variables, we show that the treated group, as compared with the control group with no policy benefit, after experiencing the positive housing wealth shock, are 3.6% more likely to produce children and this higher fertility rate is 0.089. As supplementary evidence, in Appendix Table A1, we report the results of carrying out the same regression at the individual-parent level, in which all control variables represent individual-level characteristics. Generally, the results show a consistent pattern, as we see in Table 3. The unconditional estimates reported in columns (1) and (2) show that owners occupying apartments with no more than 90 m^2 in floor space are predicted to produce more children than those occupying larger apartments, by a significant margin of 0.106, and they are 5.7% more likely to produce children than individuals in the control group. This effect is robust when we control for all predetermined variables.

4.2 Concerns on the baseline results

Although the composition and balanced checks have confirmed that there is no sign of manipulation on RD, which meets the basic requirement of the discontinuity design, there could be remaining concerns that may threaten the validation and the interpretation of the results.

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[8] With respect to concerns about households that own other housing units, we have three comments. First, in the baseline analysis we focus on households that have occupied the same houses since 2006 and for which there are records of all features of their current housing units. Second, our sample data, which were constructed from the CFPS, include information indicating ownership of other housing assets in the survey year; we cannot infer the existence of other housing assets in 2006. Third, to alleviate the effects of owning other housing units, we narrow the sample down to households that report in all five survey waves (2010, 2012, 2014, 2016 and 2018) having never owned other houses and rerun the baseline regression. The results are similar in magnitude and statistical significance to those reported Table 3.

4.2.1 The bandwidth

As mentioned, we use estimated optimal bias-corrected bandwidths that are consistent with Li et al. (2020). Under the same empirical setting and depending on varied outcome variables, Li et al. (2020) uses bandwidths of 13—42 around 90 m^2 in different specifications. Thus, the bandwidths in our baseline regressions (35.5 and 37.1) are quite comparable with the existing literature. To further check the sensitivity of the results to the bandwidth selection, in Table 4, we rerun the baseline regressions with a series of manually selected bandwidths: 30, 35, 40 and 45m^2. We show that, across all specifications, the estimated coefficients are still positive and statistically significant, suggesting that the baseline effect is not an outcome of manipulating the bandwidth.

[Insert Table 4]

One may still concern on the comparability between small house units with big ones. We argue that we have a focus on the jump around the cutoff point of 90 m^2 with respect to the policy shock, and we also control for *Size Diff*, a continuous variable measuring the size difference between the house size and 90 m^2, as well as *Size Diff*T* that captures the difference of trend that changes with size on two sides. Moreover, *Size Diff* may also help us to capture some predetermined but unobserved variables before 2006, such as wealth level of the family.

4.2.2 House size and fertility

A remaining concern is the endogeneity of house size with respect to fertility choices. On the one hand, people with fertility plans may choose houses with certain size or size-related characters; on the other hand, those who have childbirth plans may choose to buy houses, and thus create a sample selection bias against renters.

Regarding the first point, we believe for households planning to have a new child right before 2006, they may have greater incentive to buy larger houses since we document a positive relation between *ln(House Size)* and *Children Number* (or *Children Dummy*) in Appendix Table A2. In that case, our baseline finding could be underestimated since *T=1* stands for small-sized units. Since across all models, we have controlled for *Size Diff*, the concern on such downward bias could be alleviated.

Moreover, the cutoff point of 90 m^2 could be irrelevant to such reversed relation between the fertility plan and the house size, that is, there is no reason why people with fertility plans should buy a house right below or above 90 m^2, unless there is a structural break in terms of housing amenities or layout around 90 m^2. For housing amenities, we concern that small housing units may not be evenly distributed spatially. For example, central areas are likely to be highly dense and lack space. Small units may cluster in city centers with good facilities such as subway stations, schools, and sports centers. In the presence of positive shocks, houses that are located close to certain amenities tend to appreciate to a greater extent than other houses (Feng and Lu, 2013; Livy, 2017; Beracha et al., 2018; An et al., 2021). A typical example would be houses located near schools. For example, Feng and Lu (2013) show that the presence of one additional high-quality high school per square kilometer results in a 17.1% increase in average housing prices in the affected school district. In that case, the derived housing wealth growth may not result entirely from a policy effect on small units, but rather could be related in part to the growing value of the associated utility over time. The estimated effect might then be an overestimation.

To address this concern, we test a utility-based alternative story by checking the discontinuity of a handful of utilities on housing size. Specifically, we apply a set of RD regressions where the dependent variables represent whether a school is located within a walkable distance from a unit, whether the unit is located near a subway station, a sports center, or hospitals, and the distance to a town's central business district (CBD) (The result using *School Zone* as the dependent variable has been discussed in Table 2 and Figure 2). As in the baseline regression, here we use *T=1* as the major independent variable to indicate small units purchased before 2006 and we include all the usual control variables and fixed effects. The results are reported in Panel A of Table 5. We show that, across all specifications, the coefficients of *T=1* are neither statistically nor economically significant. That is, small units are, in general, evenly distributed spatially in relation to these key utilities.

[Insert Table 5 Here]

The survey data do not contain such information of house layout, so, to test the possible structural change of housing layout around 90m^2, we use an auxiliary data of 1,962,132 apartments in 110 cities from a nationally representative housing agency to check out the discontinuity of house layout at 90 m^2. Using this sample of houses built before 2006, we conduct an RD analysis where the dependent variables are *Room*, *South* and *Floor*. *Room* indicates the number of rooms, *South* is a dummy variable that indicates the house has at least a window opening to south, and *Floor* means the floor number. The independent variables are *T*, *Size Diff* and *T*Size Diff*. Fixed

Effects at building year and community are controlled. In Panel B of Table 5, we find that there is no significant jump in terms of housing layout around 90 m^2, which may reduce the concern that people buy houses right below 90 m^2 is due to a structural change in house layout at this threshold.

The second concern with the baseline effect is that people with fertility plans may choose to buy houses, and thus our results are subject to a selection bias against renters. To ensure that the possible selection bias on homeowners does not affect the main effect, we test the regressions in a Heckman two-stage model. In the unreported first stage, we conduct a Probit analysis where the dependent variable is a dummy of *Owner* to indicate the family bought instead of rented the house. The model's independent variables include all predetermined control variables as in Equation (2), namely, *Previous Children*, *Previous Boy Dummy*, *Age(m)*, *Educ Dummy(m)*, *Marital Status(m)*, *Han*, *Urban Hukou(m)*, and *School Zone*. Then, we re-run the specifications in Equation (2) when adding an inverse Mills ratio (*IMR*), and the results are shown in Appendix Table A3. While *IMR* is nonsignificant across both columns, the main effects are robust and consistent with the results in Table 3. Overall, our results suggest the selection issue may not be a major concern to our baseline results.

4.3 Economic magnitude

Provided that the policy allows for lower down payments and lower property taxes for smaller housing units (≤ 90 m^2 in floor space), these conditions favor smaller apartments, possibly raising the prices for those units and increasing the underlying housing wealth for homeowners. Combining the above-reported findings, it could be argued that greater housing wealth encourages childbearing behavior among policy-affected households. To support this argument, it is necessary to investigate whether there exists a discontinuity in housing-price growth rates at the cutoff point (90 m^2).

To estimate the effects of the policy on housing-price growth, we utilize the household-level sample with information indicating housing size, purchase year, housing price at time of purchase, and price at the time of the survey as well as province-city information. This paper adopts two measures of housing-price growth, i.e., the annualized growth rate and the continuously compounded annual rate of change. The annualized growth rate is calculated by the formula $r_1 \cong \left(\frac{price_t}{price_0}\right)^{\frac{1}{t}} - 1$, where $price_0$ denotes the price when a housing unit was purchased, $price_t$

denotes the price at the time of the survey, and t represents housing age. The continuously compounded annual rate of change is calculated using the formula $r_2 \cong ln\left(\frac{price_t}{price_0}\right)/t$.

The relationship between the annual housing-price growth rate and housing-unit floor space is presented in Figure 4. The annual growth rate shown on the left side of the cutoff point reveals a spike, suggesting that housing values for units with floor spaces smaller than (or equal to) 90 m^2 rose more quickly over the preceding decade than those with floor spaces larger than 90 m^2.[9]

[Insert Figure 4 Here]

The local linear regression estimates are reported in Panel A of Table 6, with two dependent variables—the annualized growth rate and the average annual growth rate at the household level, respectively—for each column. The model is the regression of the growth rate measures on the centered assignment variable $\widetilde{X_i}$, the treatment indicator T_i, the interaction term of the centered running variable and the treatment variable $\widetilde{X_i} * T_i$, a dummy of multiples of ten, purchase-year fixed effects, and county-level fixed effects. Consistent with the notion depicted in Figure 4, we see that housing prices in the treated group are higher and significant, both statistically and economically. In column (1), the reported coefficient estimate is 1.6%, which implies that housing prices of policy-affected homes (≤ 90 m^2) grew about 1.6% more rapidly than prices of control homes (> 90 m^2) in each year in our sample. This coefficient is 1.5% when we use the continuously compounded annual rate of change, as seen in column (2).

[Insert Table 6 Here]

It is worth noting that the ITT effect is effectively obtained from the RD estimation in Equation (2), which concludes that the difference between families within the treatment group and those within the control group is driven by the policy per se, regardless of what price treatment they may have received. We then perform an analysis that exploits the treatment variable (T_i) as an IV for the housing price growth rate in the regression of fertility measures on the growth rate of housing prices to validate the average treatment effect at the cutoff point. The IV estimation is also carried out at both the household and individual levels. All predetermined variables are added, including total housing values when purchased, the number of children prior to the housing wealth

[9] The discontinuity may not be significant at the 95% confidence level, according to the figures. We do not, however, rely merely on this unconditional test. We also show that housing-price growth for units immediately below 90 m^2 in floor space is significantly higher in regressions the results of which are reported in Table 6.

shock, a dummy of boys prior to the shock, gender, age, education, marital status, ethnicity, hukou status, housing age and school zone, and we further control for a dummy of housing size at multiples of ten as controls as well as county-level fixed effects.

In Panel B of Table 6 we report positive and statistically significant estimates, which are consistent with the baseline findings shown in Table 3. Using the annualized growth rate of housing prices as the independent variable and T as an instrumental variable, we show that a 1% annualized increase in housing wealth is associated with a 5.34% higher probability that children are produced between 2006 and 2018, or in terms of the number of children, the coefficient is 0.069. The estimations with the compounded annual rate of change in housing prices as the independent variable are similar in economic significance and magnitude, as can be seen in columns (3) and (4). Note that we also carry out the IV estimations with the individual-level sample and find similar results, as shown in Appendix Table A4.

Next, we carry out a back-of-the-envelope calculation to estimate the economic implications of the regression results. The CFPS is a biennial survey with missing data in reported total housing values, making it difficult to calculate changes in housing wealth directly and accurately. Also, the survey was first conducted in 2010, so we are unable to obtain reported housing values for the policy year, 2006. For these reasons, we use available information on housing-price growth rates, purchase years, and total value when purchased to interpolate total housing values in 2006 and further infer the effects of absolute changes in housing values on the number of newborns between 2006 and 2018.

According to the results reported in Panel A of Table 6, the estimated annual growth rate in housing prices is about 1.6% higher for policy-affected homes (≤ 90 m^2) than for control homes (> 90 m^2); therefore, between 2006 and 2018, housing prices for homes with floor spaces smaller than (or equal to) 90 m^2 should have been expected to increase 20.8% (=1.6%*13) faster than those with floor spaces larger than 90 m^2. The results reported in Panel B of Table 6 suggest that a one percentage point increase in the annualized growth rate of housing prices leads to 0.069 more newborns between 2006 and 2018. To ascertain the economic implication of absolute changes in housing values on the number of children born, we need the total housing values for 2006, which can be roughly estimated using the following formula:

$$Estimated\ total\ values\ \ in\ 2006_1 = total\ values\ when\ purchased * (1 +$$

$growth\,rate\,of\,housing\,prices * house\,age\,in\,2006)$.

As a result, the estimated average housing price in 2006 was RMB 84,593. Because we estimate that prices for policy-affected homes increased, on average, 20.8% faster between 2006 and 2018, the value change for policy-affected homes was RMB 17,595 (= 84,593 * 20.8%), resulting in 0.069 more newborns per household. An RMB 100,000 increase in housing values is then estimated to increase newborns per household by 0.392.[10] To facilitate the comparison of the economic magnitude of our result with that of other related studies, we show that the elasticity of fertility to housing value is 0.34 ($\frac{0.0534}{(0.7602*20.8\%)} = 0.34$).[11] The number is quite comparable in magnitude to what is found in prior literature. For example, the elasticity as estimated to be 0.13 in the United States(Lovenheim and Mumford, 2013) and 0.25 in Denmark (Daysal et al. 2021). Canada and Australia document higher elasticity of 0.46 and 0.57, respectively (Clark and Ferrer, 2019; Atalay et al., 2021).

It is worth noting that, in the first stage of the IV regression, the effects of the policy on housing-price growth may be underestimated because some houses were purchased much earlier than 2006. Thus, housing-price growth induced entirely by the policy could be larger. We show in Appendix Table A5 that, using a smaller sample of houses purchased in 2004 and 2005, the coefficient is 4.8%, which is statistically significant and economic larger. Correspondingly, we rerun the baseline regression and find that households that purchased housing under 90 m^2 in floor space before 2006 are 9.1% more likely to produce children, although the statistical significance is weak most likely because of the small sample size. In terms of economic magnitude, we may take this number as an upper bound of the main fertility effect.

4.4 Placebo and robustness checks

To provide additional evidence of the validity of the baseline findings, we carry out a series of robustness checks and placebo tests. The placebo tests include experiments with alternative cutoff points using a sample of housing purchases executed after the policy was implemented. The robustness checks include experiments with alternative provincial-level fixed effects, nonlinearity specifications, alternative bandwidths, and a probe of possible measurement error.

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[10] Under an alternative formula to for estimating housing value in 2006— $Estimated\,total\,values\,in\,2006_2 = total\,values\,when\,purchased * (1 + growth\,rate\,of\,housing\,prices)^{house\,age\,in\,2006}$—we obtain a highly similar result.

[11] Note that in our sample the mean values of the likelihood that at least one child is produced before 2006 are 0.7653 at the individual level and 0.7602 at the household level.

4.4.1 A falsification test with alternative cutoff points

The cutoff point for housing size inferred from housing-market policies that were in place from 2006 to 2008 is 90 m^2 in floor space. Here we rerun our regressions with alterative cutoff points at 80 m^2 and 100 m^2. The results are summarized in Panel A of Table 7. In columns (1) and (2), we report the estimation results when the cutoff is manually set at 100 m^2 and all other conditions remain unchanged. After the policy shock, the treated group and the control group exhibit almost no statistical differences in terms of newly produced children and the economic magnitude is rather trivial. We find similar results when we use 80 m^2 as the cutoff point, as shown in columns (3) and (4).

[Insert Table 7 Here]

4.4.2 Homes purchased after 2006

A critical assumption in RD design is non-full manipulation of the running variable (housing size). If the assumption is relaxed, the nonrandom nature of home purchases is likely to taint the impact of housing wealth on fertility. Hence, data on home acquisitions after the policies became effective are considered, when families may take advantage of the various treatment conditions regarding housing sizes by choosing apartments that are larger or smaller than 90 m^2 in floor space. In this subsection, we rerun the RD analyses using a sample of housing purchases made after 2006, when the favorable conditions were already announced and granted to smaller housing units, and households were able to consciously choose the floor spaces of the homes they purchased. The rationale behind this test is that, in an efficient market, news should be fully incorporated into asset prices when it is released. Even if the housing market may not be efficient, we may still postulate that households buying homes after the policy shock would not arbitrage to any great extent when the price is already artificially high.

The estimation results are reported in Panel B of Table 7. As expected, while the coefficient is not statistically significant, the economic magnitude is notably smaller than that of the baseline results.

We then repeat all of the placebo tests presented in Sections 4.4.1 and 4.4.2 with the individual-level sample and find consistent results, as shown in Appendix Table A6.

4.4.3 Alternative fixed effects

In the previous section, we include county-level fixed effects in the regression. In this test, we include in the regression provincial-level fixed effects in that cover an area approximately equal to that of 26 provinces in China to ascertain whether the baseline findings are sensitive to the scope of the region to be controlled for. In columns (1) and (2) of Panel A of Table 8, we control for provincial-level fixed effects instead of county-level fixed effects. As compared with the baseline results, here we observe a consistent pattern, with similar statistical significance and magnitudes, suggesting that the findings are robust to the selection of fixed effects at varying regional levels.

[Insert Table 8 Here]

4.4.4 Non-linear estimation

The baseline RD results are obtained using the local linear regression method. In a robustness check, we adopt an alternative non-linear approach and report the results in this subsection. To obtain the results reported in columns (3) and (4) of Panel A of Table 8, we add squared housing size to the RD regression and we find that the results remain robust.

4.4.5 Measurement error

The cutoff point used in our RD design is 90 m^2 in floor space. The running variable, reported housing size, exhibits a pattern of bunching at multiples of 10 m^2, which may raise the concern that the treatment effect could be caused by the bunching or misreporting of housing size. To address this concern, we first follow Li et. al. (2020) and include a dummy variable indicating multiples of 10 in the analyses throughout the paper, so that the effects of bunching could be differenced out. Second, housing sizes could be reported with errors. In a typical "rounding up" procedure, individuals may report a housing size of 90 m^2 when the actual size is only 88–89 m^2. Noting that houses with right 90 m^2 in floor space, according to the policy, are influenced by the positive shock, we may argue that the baseline effect is less likely to be driven by "rounding up" in reported housing size.

A remaining concern is that individuals may "round down" to report floor space of 90 m^2 when the actual size could be 91 or 92 m^2. To address this concern with misreporting, we carry out a falsification test. For all observations at 90 m^2 in floor space we randomly assign a size value between 88 and 92 and rerun the baseline regressions. We repeat this procedure 1,000 times and plot the distribution of the p-values. As shown in Panel (a) of Appendix Figure A3, for the RD regression at the household level, when *Children Number* is the dependent variable the coefficients of T are significant at the 10% level in over 99% of the cases. In Panel (b) of Appendix Figure A3 we report the simulation results using *Children Dummy* as the dependent variable. All coefficients of T are significant at the 3% level.

We see quite similar results of simulation when addressing the measurement error with the individual-level sample, as shown in Panels (c) and (d) of Appendix Figure A3.

4.5 Heterogeneous analysis

In this subsection, we explore the effects of the rich heterogeneity in treatment effects across subgroups of individuals defined by years of education, age, household financial condition, and birth order.

We first divide the sample into two subgroups sorted by reference to the median mother's years of education, which is 9 years. The regression results for the two fertility measures across the two subgroups are presented in Panel A of Table 9. The lower-than-median education-attainment (< 9 years) sample shows a positive and statistically significant estimate, whereas the well-educated group responds weakly. When the dependent variable is *Children Number*, we show that less educated mothers register a large magnitude of 0.125 compared with the 0.028 (statistically nonsignificant) estimate with the higher education (≥ 9 years) sample. Similar findings are documented when *Children Dummy* is the dependent variable. These findings imply that the fertility response in mothers with fewer years of education are more sensitive to the focal housing policies.

[Insert Table 9 Here]

We then consider a mother's age as the criterion for defining the two subgroups. We use 47 years as of 2018 (that is, 35years in 2006), as the cutoff for dividing the sample.[12] The regression outcomes between the two subgroups are presented in Panel B of Table 9. As expected, younger individuals respond more strongly to the positive housing wealth shock. In the regression of *Children Number* born after 2006, for individuals older than 47 years, the estimated coefficient is statistically nonsignificant and economically small; the main effect stems from the subgroup of individual mothers under 47 years of age, where the estimated coefficient is significantly larger. We conclude that the housing policies have had a positive impact on the probability that individuals below 47 years of age produce children and the effect is muted for the older cohort.

[12] Note that we obtain similar results when we use other ages, such as 42, 45, and 49 years, as cutoff points.

Overall, our results suggest that the fertility effects of positive housing wealth shocks are subject to an age constraint.

The third set of heterogeneity analyses is derived from cross-sectional differences in financial conditions at the household level. We divide the sample by the median per capita income as of 2018 and rerun the baseline regressions and report the results in Panel C. We find that households with higher per capita incomes are 8.7% more likely to produce new children between 2006 and 2018 if the housing unit is small. In contrast, we find no significant effect for the subsample of lower per capita incomes. Overall, our results suggest that, although the positive housing wealth shock may facilitate fertility, the strength of the effect depends on household financial conditions, which facilitates producing and raising children.

Lastly, we check the effects of heterogeneity in birth order. The sample is split by the number of children prior to the policy shock. As reported in Panel D of Table 9, when a household had no children before 2006, the wealth effect largely promotes fertility (note that, in the sample, almost all households produce at least one child between 2006 and 2018 if they had zero child prior to 2006, which is why we do not use *Children Dummy* as the dependent variable because it lacks variation).[13] If a household had one child before 2006, the wealth effect remains positive and significant, although the coefficient may be smaller in magnitude. In the case where a household already had two children before 2006, we see that the wealth effect is statistically nonsignificant and economically trivial.

4.6 Wealth effect on children's health at birth and future development

The impact of the housing wealth shock could be extended to children's post-natal health. Specifically, in this section we investigate whether children weigh more at birth and exhibit better physical and intelligence indicators in 2018. The results are shown in Table 10. First, we use birth weight as the dependent variable while keeping *T=1* as the major independent variable to indicate small units purchased before 2006, controlling for children's gender, a mother's age in the year of childbirth, and the same control variables and fixed effects as in the baseline regression. We show in column (1) that children born in families that experience the positive wealth shock are 0.26 kg heavier than those in the comparison group. We then replace the dependent variable with several

[13] The local implementation of the OCP varied widely across regions and ethnic groups (Zhang, 2017). For example, according to Ebenstein (2010), urban residents may choose to exceed the restricted number of children by paying a fee and facing a variety of financial penalties.

other attributes as of 2018 to indicate physical status and intelligence, namely height, weight, and academic record. Observing the results reported in columns (2) and (3), we see that, when controlling for gender and current age, children experiencing the positive wealth shock are 4.7 cm taller and 2.4 kg heavier than those in the comparison group. To measure intelligence, we use the parent-reported children's reading and math grades for the schooling sample. There are four grades in the questionnaire—"Excellent", "Good", "Pass" and "Fail"—for each subject. We define *Reading* and *Math*, two dummy variables, to indicate that a parent reports "Excellent" on the survey for a given subject.[14] The results reported in columns (4) and (5) show that children in a family that experiences the positive housing wealth shock are 7.8% more likely to perform well in Chinese and 11.6% more likely to perform well in math.

[Insert Table 10 Here]

5. Possible channels

In this section we aim to discuss two possible channels that might drive the causal relationship between greater housing wealth and rising fertility among households in China, they are, respectively, time allocation and health status.

The literature indicates that substantial gains in housing wealth should set people free to determine their own roles in the labor supply (Henley, 2004; Jacob and Ludwig, 2012; Zhao and Burge, 2017; Disney and Gathergood, 2018; Li et al., 2020), including opting out, a time-allocation choice that may further enable households to produce and raise children. In terms of labor supply, we focus on respondents' employment status before and after the childbirth.

Growth in household wealth may also improve individual health status. Using panel data with a sample of representative homeowners in the United Kingdom, Fichera and Gathergood (2016) find that higher housing prices during the housing boom, which provided owners with more wealth, reduce the risk of experiencing non-chronic health issues and enhance self-evaluated health. According to Zhang and Zhang (2019), housing assets are the primary mechanism through which home characteristics have a significant impact on subjective well-being in China. Therefore, we propose another channel through which household wealth may affect fertility: improved parental health. In terms of health status, we focus on respondents' physical health proxied by self-

[14] Similar results are obtained when we define *Reading* and *Math* as dummy variables when a parent reports "Excellent" or "Good" performance.

reported health scores, whether one has suffered from chronic disease within the preceding half-year, and whether one was hospitalized within the preceding year, as well as life satisfaction.

5.1 Labor supply

We apply a set of RD regressions with participation of the labor market and daily hours of housework as the dependent variables. As in the baseline regression, here the major independent variable is *T=1*, which indicates small housing units purchased before 2006, and we control for all sets of control variables and fixed effects. The results are shown in Table 11.

[Insert Table 11 Here]

In Panel A in Table 11, we report the results of the regression where the dependent variable is pre-birth labor participation. Pre-birth labor participation is a status variable that takes the value of 1 if a respondent was unemployed or had exited the job market and 0 otherwise. The estimated coefficient for the full sample is positive but not statistically significant. Dividing the sample by respondents' genders in columns (2) and (3), we see that the male sample, however, has a positive and statistically significant estimate, with a high magnitude of 0.107 for the female sample. We do not find significant results for the female sample, which is intuitive since most female may sacrifice their labor supply before giving birth to a child. The findings suggest that in a household that received the positive wealth shock, the husbands tend to provide greater pre-fertility support by sparing more time in the family.

In Panel B in Table 11, we report the results of the regression where the dependent variable is post-birth labor participation (as of 2018). The estimated coefficient for the full sample in column (1) is positive and statistically significant, which implies that policy-affected individuals are more likely to withdraw from the labor market after producing children. Dividing the sample by respondents' genders for columns (2) and (3), we see that the female sample, however, has a positive and statistically significant estimate, with a high magnitude of 0.171 as compared with -0.011 (which is statistically nonsignificant) for the male sample. This result suggests the positive wealth shock grants wives in a household to stay at home for childcare. Overall, our findings support the notion that an increase in housing wealth, even without being cashed out, may lend greater confidence that encourages people to shift their attention from work to fertility.

5.2 Parental health status

The second channel through which housing wealth gains might affect fertility involves physical and mental health. To test whether the focal policies positively affect health status, we use health variables in the survey year before a child is born. Enjoying better health increases the likelihood that households make progressive childbearing decisions; based on past studies (Bird and Fremont, 1991; Mirowsky and Ross, 2002; Spence, 2008), however, childbearing activities affect the health conditions of mothers negatively. Therefore, the health variables should reflect health conditions before childbirth. A similar RD design is applied based on the samples with self-reported health status, chronic disease, hospitalization, and subjective well-being as outcome variables. The regression results across five dependent variables are summarized in Table 12.

[Insert Table 12 Here]

CFPS respondents are asked to evaluate their health condition by choosing from 1 to 7, which represents from unhealthy to very healthy. In column (1) of Panel A in Table 12 we see that policy-affected individuals report better self-assessed health by a significant margin of 0.019 points, which is consistent with the previous discussion indicating that gains in housing wealth should boost physical health. In columns (2) and (3) we report regression results regarding another two dummy variables for physical health, i.e., an indicator of no chronic disease within the preceding half-year and an indicator of not having been hospitalized within the preceding year. The estimated coefficients are positive and statistically significant, implying that treated individuals are less likely to experience chronic diseases or hospitalization. We include a variable related to subjective well-being: satisfaction with life. In columns (4) of Panel A in Table 12, we report positive and statistically significant coefficient, implying that individuals report greater life satisfaction if they experience positive housing-wealth shocks.

We then split the sample by parental gender and report the results in Panels B and C in Table 12. We find that the housing wealth shock affects the physical health of both males and females, whereas improvement in subjective well-being is more significant for females, which could be an important driver of subsequent fertility behavior.

6. Further discussion

In this section, we provide further discussion of our results. We utilize an RD–difference-in-differences (DiD) design to further check the validity of our baseline effects. Finally, we use a longitudinal data structure to investigate the dynamics of the fertility response to the wealth shock.

6.1 Alternative specification with RD-DiD

To further test whether household fertility behavior differs before and after the housing wealth shock, we conduct an RD-DiD analysis and report the results in Table 13. The dependent variable is *Give_birth*, a dummy variable that takes the value of 1 if a household has a new child in a given year and 0 otherwise and the independent variable is interaction between *T=1* and *After,* which is an indicator of years after 2006. While controlling for other variables, including *T=1, T=1*Size Diff*, and *Size Diff*, we control for several fixed effects at the household and year levels as well as a dummy for *Multiples of 10* m^2. Year-level fixed effects absorb all time-varying variates that affect all households. Household-level fixed effects, then, could capture all the time-invariant characteristics of a household. Because the CFPS starts in 2010, we are not able to control for household-level variables that may change over time.

[Insert Table 13 Here]

For column (1) of Panel A we use the 2002–2008 window and show that households occupying small units are 1.7% more likely to produce children after the housing wealth shock as compared with their fertility behavior during the pre-treatment period and the behavior of the control group. We find consistent and similar results using alternative 2002–2012 and 2002–2018 windows. Moreover, we repeat the above analyses with the individual-level sample. As shown in Appendix Table A8, the results are largely unchanged.

6.2 Dynamic Effect with Longitudinal Data

To test the dynamics of the fertility response to the housing wealth shock, we expand the dataset by tracing fertility decisions in every year after 2006. That is, for each respondent, we manually construct a panel dataset spanning from 2006 through 2018 and create a dummy variable *Give_birth* that equals 1 if a respondent bears a child in a given year and 0 otherwise. We rerun the RD regression where the independent variable is *T=1* while we control for all predetermined variables and fixed effects, as in the baseline regression. And we control for year fixed effects in the tests.

The results obtained with the household-level sample are presented in Table 14. As the results reported in column (1) indicate, we find that the housing wealth effect holds in a longitudinal setting in which we trace fertility decision in each year after 2006. We then test household reactions to the wealth shock in specifications with interaction terms. The results reported in column (2) indicate that households respond to the wealth shock in the first five years following enactment of the policy by producing children. While the coefficient of the interaction term is quite significant, the coefficient of *T=1* becomes substantially nonsignificant. This finding suggests that households may have responded to the policy in the first five years following enactment but then held off.

[Insert Table 14 Here]

A remaining question is whether treated families would react more prominently to the relaxation of the OCP in 2013 and 2016.[15] By interacting *T=1* with *After 2013* and *After 2016*, respectively, and reporting the results in columns (3) and (4), we see that the baseline effect is not amplified when the OCP was relaxed. We report quite similar results in Appendix Table A9, where the individual-level sample is utilized. We argue that we do not see a positive effect when the OCP was relaxed because a proportion of respondents who had one child before 2006 were restrained by the OCP when they wished to have a second child. When the OCP was relaxed in 2013, these households may have aged beyond their most fertile years. In this scenario, the baseline effect should have been stronger for a subsample where respondents did not have children prior to 2006. We report supporting evidence in panel D of Table 9, where we show that the coefficient is as large as 0.472 for the subsample of households who had no children in 2006 as compared with coefficients of 0.113 and 0.012, respectively, for the subsamples of households with one child and those with two or more children as of 2006.

7. Conclusion

This paper studies the response of fertility behavior to housing wealth gains using a regression discontinuity method. The identification strategy is based on a housing-market policy, namely *National Article Six* issued in 2006 that led to differential growth rates of housing values at a cutoff point of 90 m^2 in floor space. In particular, the value of homes with no more than 90

[15] Regarding the evaluation of family-planning policies in China, in 1979 the leaders of the Chinese Communist Party, in fear of a Malthusian disaster, declared the one child policy (OCP) to control China's rapidly growing population. In spite of considerable propaganda, incentives, and penalties, however, the OCP met with severe opposition, especially in rural regions, where a family's financial well-being and the material stability were heavily interrelated with the number of male offspring. As a result, in April 1984, as a compromise, Central Document No. 7 was issued in rural regions that permitted any family to have a second child if the first was female, but in urban areas the OCP remained in place, although it became known as the "one-and-a-half child policy". In 2013, the OCP was replaced by the Partial Two-Child Policy, whereby couples were eligible to have a second child if either parent was an only child. In 2016, the OCP was officially abolished when the Universal Two-Child Policy was issued to entitle all couples to have two children. Out of concern over low fertility rates even after the implementation of the Universal Two-Child Policy, all couples became entitled to have three children following the approval of the Three-Child Policy in 2021.

m^2 in floor space grew more quickly than that of larger homes as a result of the unexpected regulatory change.

The findings point to an increase in the likelihood of having children and an improvement in child health as a result of the unexpected rise in housing wealth. A series of heterogeneity analyses also find differential impacts related to mothers' education, age, household income level, and birth order. We then discuss two channels that might drive the main effects. We find that individuals who experienced the positive wealth shock are more likely to quit the labor market; this effect is especially strong for males before child birth and females after child birth. Another channel through which housing wealth might boost fertility involves physical and mental health. As expected, we find strong evidence that, before producing a child, individuals experiencing housing wealth growth report better physical and mental health.

We then extend our discussion utilizing an RD-DiD framework, and we find quite similar findings, and the main findings also hold in a longitudinal setting in which we trace fertility decisions through each year after 2006. Moreover, we show that the effect is unlikely to be a response to the relaxation of the one child policy, as the effect is strongest in the first five years after the policy shock.

One important policy implication stemming from our study regards the significant population aging and low fertility problem that China is confronted with. According to the seventh national population census, the number of aging population (older than 60 years old) in China reached 264 million up to the end of 2020, accounting for 18.7% of the total population. In the meanwhile, the number of newborns in China fell to 12 million in 2020, with a decrease of 18% from 2019. In 2022 the population in the Chinese mainland fell for the first time in 61 years, recording a decline of 0.85 million in 2022. China's total fertility rate has been below 1.3, among the lowest in the world. China is in an urgent situation to raise fertility rate.

The findings in this paper suggest that policies aiming to ease the financial pressures of households are effective in encouraging fertility. For example, subsidies in housing may increase fertility in the short run with an elasticity of 0.34 based on our estimation. Moreover, the subsidy policy should not be limited to the aim of "more birth", but should be designed in a way that encourages "better birth". We documented that a positive housing wealth shock has intergenerational effects, as the affected couples produce babies that have better health and cognitive ability in both the short run and the long run. As such, our findings may justify why some Chinese cities roll out housing subsidies to attract high-end talents.[16] The policy, besides its role of boosting skill development and productivity, may also stimulate “better birth” in these cities. Thus, our paper may provide a new lens for cost-benefit analyses for such a policy.

Furthermore, based on the causal relationship between housing wealth and fertility in China identified by this paper, policy implications may be drawn for other developing economies, particularly in Southeast Asia and Africa, which are now on the fast track of economic growth and at an early stage of demographic structural changes, but hold concerns about declining demographic dividends in the future. A key takeaway is that prompt actions such as applying reasonable population policies and maintaining a stable housing market should be considered by policymakers to achieve sustainable population growth and balanced demographic structure.

[16] For example, the maximum subsidy to top-level talents in Suzhou's Industrial Park is 5 million yuan, in Nanjing's Jiangbei New Area or Taizhou is 3 million yuan. Apart from giving quota for subsidizing housing purchase, others cities may have issued subsidy based on housing price or deed tax.

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Figure 1. Average Residential Housing Prices and Aggregated Fertility Rates in China between 1999 and 2016

Notes: This figure plots the relationship between average housing prices and aggregated fertility rates. The x-axis represents years between 1999 and 2016. The primary (left-side) y-axis represents aggregate fertility rates, and the secondary (right-side) y-axis represents average commercial residential housing prices in China. The green line represents the trend in the aggregate fertility rate and the red line represents the trend in residential housing prices from 1999 to 2016.

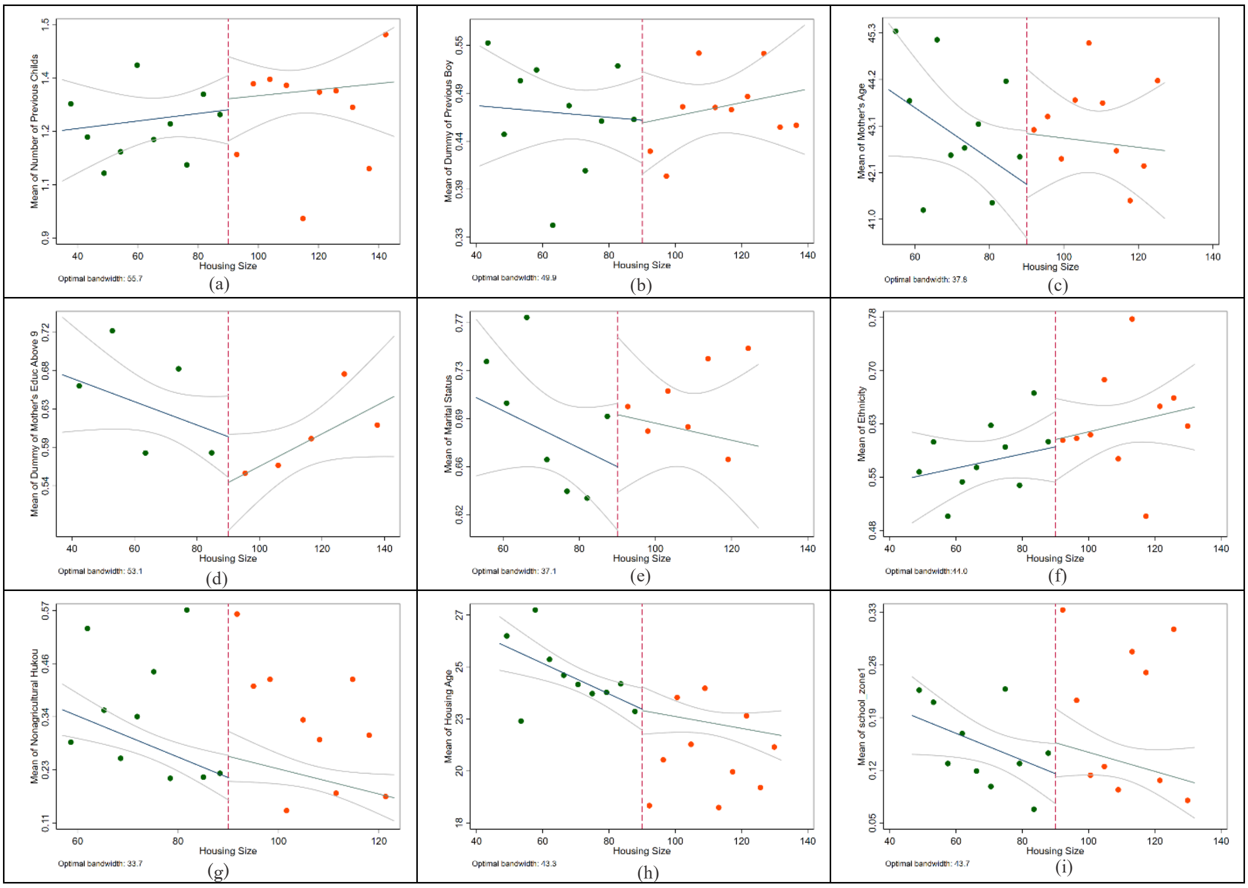

Figure 2. Discontinuity Checks of Predetermined Variables

Notes: In this table we report the results of checks of the discontinuity of predetermined variables with household-level data. The x-axis represents the floor spaces of housing units, while the y-axes of graphs (a)–(i) represent nine predetermined variables: the number of children prior to the shock, an indicator of boys prior to the shock, the mother's age, an indicator of mothers with years of education above or equal to 9 years, household marital status, an indicator of Han Chinese, an indicator of non-agricultural hukou, housing age, and an indicator of the apartment is in school zones. 1. The circles represent conditional mean values of the respective variables for ten bins on each side of the cutoff point. 2. The solid lines are the fitted values from the local linear regression with the optimal bandwidth while the dashed lines are 95% confidence intervals. 3. The vertical line is the 90 m^2 floor-space cutoff point for the running variable.

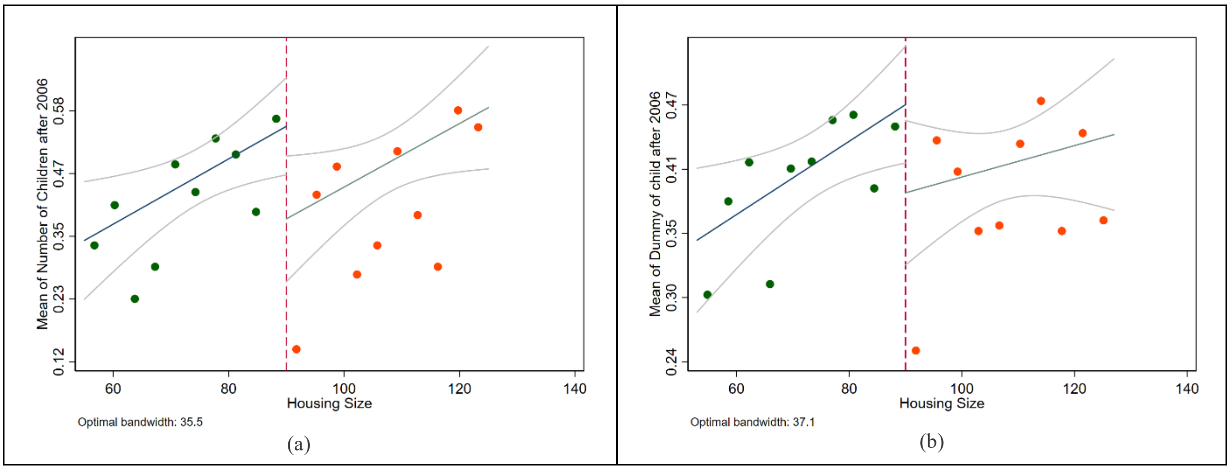

Figure 3. Housing Size Discontinuity and Fertility Outcomes

Notes: This figure plots the discontinuity of fertility outcomes with respect to housing size with the household-level sample. The x-axis represents the floor spaces of housing units, while the y-axes represent the two measures of fertility outcomes: the number of children born after 2006 for graph (a) and an indicator variable for producing a child after 2006 for graph (b). 1. The circles represent conditional mean values of the respective variables for ten bins on each side of the cutoff point. 2. The solid lines are the fitted values from the local linear regression with the optimal bandwidth while the dashed lines are 95% confidence intervals. 3. The vertical line is the 90 m^2 floor-space cutoff point for the running variable.

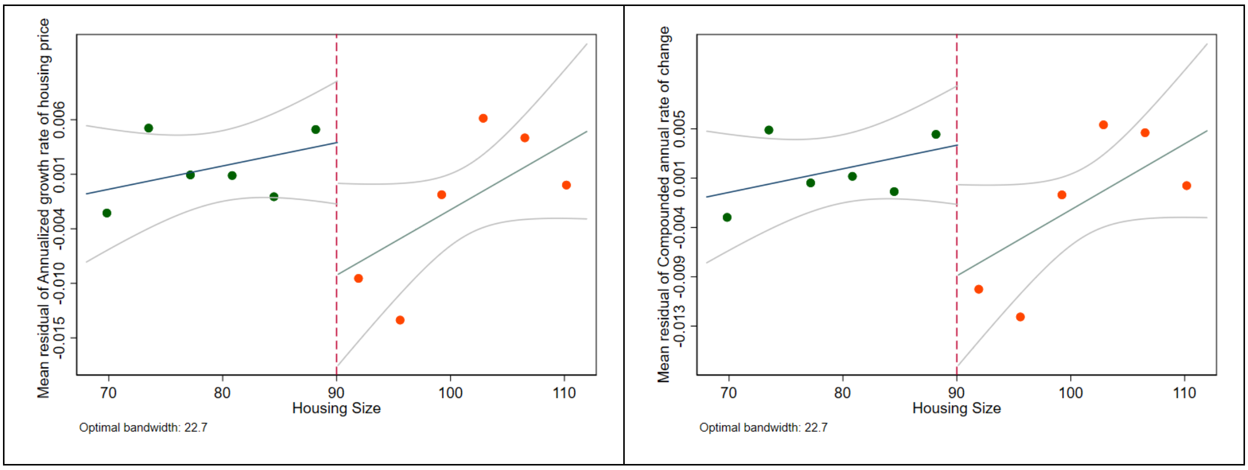

Figure 4. Housing Size Discontinuity and Price Growth

Notes: This figure plots the discontinuity of price growth with respect to housing size. 1. The x-axis represents the floor spaces of housing units while the y-axes of Panels (a) and (b) represent the two measures of housing-price growth: the annualized growth rate of housing prices and the average growth rate of housing prices. 2. The circles represent conditional mean values of the respective variables for ten bins on each side of the cutoff point. 3. The solid lines are the fitted values from the local linear regression with the optimal bandwidth while the dashed lines are 95% confidence intervals. 4. The vertical line is the 90 m^2 floor-space cutoff point for the running variable.

Table 1. Summary Statistics

Notes: In this table we report summary statistics for the household-level sample, regarding the treatment status *T*, two outcome variables, and all control variables. The number of observations, averages, standard deviations, and minimum and maximum values are included.

Panel A. Full sample

VARIABLE	Obs.	Mean	Std	Min	Max
T=1	3,242	0.319	0.466	0	1
Children Number	3,242	0.451	0.770	0	5
Children Dummy	3,242	0.305	0.461	0	1
Previous Children	3,242	1.344	0.985	0	9
Previous Boy Dummy	3,242	0.585	0.493	0	1
Age(m) (as of 2018)	3,242	45.73	9.722	21	60
Educ Dummy (m)	3,242	0.556	0.497	0	1
Married(m)	3,242	0.809	0.393	0	1
Han	3,242	0.718	0.450	0	1
Urban Hukou(m)	3,242	0.230	0.421	0	1
House Age	3,242	23.04	8.559	13	108
School Zone Dummy	3,242	0.133	0.339	0	1

Panel B. RD sample (optimal bandwidth=35.5 m^2)

VARIABLE	Obs.	Mean	Std	Min	Max
T=1	1,568	0.515	0.500	0	1
Children Number	1,568	0.435	0.770	0	5
Children Dummy	1,568	0.295	0.456	0	1
Previous Children	1,568	1.335	0.981	0	6
Previous Boy Dummy	1,568	0.575	0.494	0	1
Age(m) (as of 2018)	1,568	45.97	9.548	22	60
Educ Dummy (m)	1,568	0.543	0.498	0	1
Married(m)	1,568	0.823	0.382	0	1
Han	1,568	0.712	0.453	0	1
Urban Hukou(m)	1,568	0.241	0.428	0	1
House Age	1,568	23.20	8.626	13	108
School Zone Dummy	1,568	0.136	0.343	0	1

Table 2. Discontinuity Checks on Predetermined Variables

Notes: In this table we report the results of a check of the discontinuity of predetermined variables in regressions for the household-level sample. Specifically, we present the local linear regression results of the predetermined socioeconomic characteristics as a quantitative check on the manipulation of the assignment variable. The dependent variables associated with columns (1) through (9) are the nine predetermined variables: the number of children prior to the housing wealth shock, an indicator of boys prior to the shock, the mother's age, an indicator of mother's education years above or equal to 9 years, household marital status, an indicator of Han Chinese, an indicator of non-agricultural hukou, housing age, and an indicator of the apartment is in school zones. 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff*, *Size Diff**T=1*, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*Previous Children*	*Previous Boy Dummy*	*Age(m)*	*Educ Dummy(m)*	*Married(m)*	*Han*	*Urban Hukou(m)*	*House Age*	*School Zone*
T = 1	-0.037	0.017	-1.241	0.059	-0.017	-0.002	0.002	0.605	-0.039
	(0.082)	(0.053)	(0.958)	(0.039)	(0.041)	(0.024)	(0.041)	(0.750)	(0.031)
Observations	1,911	1,797	1,558	1,896	1,558	1,741	1,488	1,740	1,741
R-squared	0.212	0.131	0.104	0.265	0.117	0.215	0.516	0.249	0.132
Optimal bandwidth	55.7	49.9	37.8	53.1	37.1	44.0	33.7	43.3	43.3

Table 3. Housing Wealth Shock and Fertility—Regression Discontinuity

Notes: In this table we report the results of baseline regressions of the effects of the housing wealth shock on fertility using the household-level sample. The dependent variables are the two measures of fertility. Specifically, children born after 2006 are reported in columns (1) and (3), and dummies of children born after 2006 as dependent variables are reported in columns (2) and (4). 1. Local linear regressions are used with the optimal bandwidth. 2. All regressions control for a *Size Diff*, *Size Diff*T=1*, a dummy for multiples of ten sqm, county fixed effect, and columns (3) and (4) further include a list of control variables, namely *Previous Children, Previous Boy Dummy, Age(m), Edu Dummy(m), Married(m), Han, Urban Hukou(m), House Age* and *School Zone*. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
VARIABLES	Children Number	Children Dummy	Children Number	Children Dummy
T=1	0.125***	0.065**	0.089*	0.036**
	(0.009)	(0.025)	(0.046)	(0.016)
Size Diff	0.004**	0.001	0.004**	0.001
	(0.002)	(0.001)	(0.002)	(0.001)
*T*Size Diff*	-0.001	0.001	-0.003	0.000
	(0.005)	(0.002)	(0.002)	(0.001)
Previous Children			-0.269***	-0.123***
			(0.026)	(0.013)
Previous Boy Dummy			-0.250***	-0.151***
			(0.042)	(0.022)
Age(m)			-0.023***	-0.020***
			(0.003)	(0.002)
Educ Dummy(m)			-0.023	-0.008
			(0.038)	(0.018)
Married(m)			-0.351***	-0.167***
			(0.035)	(0.045)
Han			0.033	-0.002
			(0.027)	(0.019)
Urban Hukou(m)			-0.124**	-0.047
			(0.058)	(0.029)
House Age			0.001	0.002*
			(0.001)	(0.001)
School Zone			0.044	0.045*
			(0.026)	(0.022)
Constant	0.356***	0.269***	2.209***	1.540***
	(0.032)	(0.023)	(0.116)	(0.057)
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	1,568	1,600	1,568	1,600
R-squared	0.179	0.149	0.718	0.730
Optimal bandwidth	35.5	37.1	35.5	37.1

Table 4. Alternative bandwidth choices

Notes: This table presents the results of a robustness check with alternative instead of optimal bandwidths. The dependent variable is the number of children born after 2006 for columns (1), (3), (5), and (7). The dependent variable is a dummy for having children after 2006 for columns (2), (4), (6), and (8). The chosen bandwidths are 30, 35, 40, and 45, respectively. 1. Local linear regressions are used with fixed bandwidth. 2. In all regressions we control for all control variables in the baseline regression, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

VARIABLES	(1) Children Number	(2) Children Dummy	(3) Children Number	(4) Children Dummy	(5) Children Number	(6) Children Dummy	(7) Children Number	(8) Children Dummy
T = 1	0.088**	0.037**	0.089*	0.036**	0.083*	0.048***	0.087*	0.047***
	(0.041)	(0.016)	(0.046)	(0.016)	(0.044)	(0.011)	(0.047)	(0.012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,454	1,454	1,524	1,524	1,716	1,716	1,770	1,770
R-squared	0.706	0.720	0.710	0.722	0.709	0.723	0.712	0.724
Bandwidth	30	30	35	35	40	40	45	45

Table 5. The Discontinuity in Housing Location and Layout

Notes: Panel A reports the discontinuity of several locational utilities with respect to housing size. Local linear regressions are estimated with four dependent variables over columns (1)–(4): indicators of having subway stations, hospitals, sports ground within a walkable distance, and the distance to the town center(or central business district). Panel B presents the evidence of whether the discontinuity in housing layout and characteristics exists around the cutoff point of 90 m^2. The data in Panel B is sourced from a representative housing agency in China. Local linear regressions are estimated with three dependent variables over columns (1)–(3): the number of rooms, the indicator of whether the apartment has at least one window opening to south, and the number of floors on which the apartment is located. 1. Local linear regressions are used with the optimal bandwidth. 2.All regressions control for *Size Diff*, and *Size Diff*T=1*. In Panel A, we control for a dummy for multiples of ten sqm, county fixed effect, and a list of control variables as in the baseline regressions; In Panel B, we control for fixed effects at building year and community level. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

Panel A. The Discontinuity in Housing Location

	(1)	(2)	(3)	(4)
VARIABLES	Subway	Hospital	Sports	Distance to Town
T = 1	-0.001	-0.043	-0.363	-3.697
	(0.004)	(0.170)	(0.266)	(3.218)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Observations	4,797	5,458	3,584	3,983
R-squared	0.566	0.498	0.531	0.548
Optimal bandwidth	34.1	42.0	29.2	39.6

Panel B. The Discontinuity in Housing Layout

	(1)	(2)	(3)
VARIABLES	Room	South	Floor
T = 1	-0.001	0.004	-0.084
	(0.006)	(0.004)	(0.101)
Size Diff	0.023***	0.001***	0.005
	(0.001)	(0.000)	(0.007)
*T*Size Diff*	0.005***	0.002***	0.002
	(0.002)	(0.001)	(0.010)
Built year FE	Yes	Yes	Yes
Community FE	Yes	Yes	Yes
Observations	336,757	441,900	497,552
R-squared	0.631	0.450	0.741
Optimal bandwidth	8.4	11.5	14.4

Table 6. House Price Growth and IV Estimation

Notes: In this table we report the results of IV estimations. In Panel A we report the first-stage results, including an RD estimate of the annualized growth rate of housing prices in column (1) and an RD estimate of the average growth rate of housing prices in column (2). In Panel B we report the second stage of IV estimates, i.e. the regression of fertility outcomes on housing prices with household-level data. These regressions use the treatment status (*T=1*) as an instrument for the growth in housing prices. For columns (1) and (2), the target independent variable is the annualized growth rate of housing prices. The dependent variable for column (1) is the number of children born after 2006 and for in column (2) it is the dummy of children born after 2006. For columns (3) and (4), the target independent variable is the average growth rate of housing prices. The dependent variable for column (3) is the number of children born after 2006, and for column (4) it is the dummy of children born after 2006. 1. Local linear regressions are used with the optimal bandwidth. 2. All regressions control for a *Size Diff* and *Size Diff*T=1*, control variables in the baseline regressions, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

Panel A. First-stage Results

VARIABLES	(1) Annualized growth rate of housing prices	(2) Compounded annual rate of change in housing prices
T = 1	0.016**	0.015**
	(0.007)	(0.007)
Controls	Yes	Yes
Multiples of 10 sqm	Yes	Yes
County	Yes	Yes
Observations	1,634	1,634
R-squared	0.392	0.385
Optimal bandwidth	22.7	22.7

Panel B. IV Estimates with Household-level Sample

VARIABLES	(1) Children Number	(2) Children Dummy	(3) Children Number	(4) Children Dummy
Annualized growth rate of housing prices	0.0687*	0.0534*		
	(0.0415)	(0.0312)		
Compounded annual rate of change in housing prices			0.0708*	0.0558*
			(0.0429)	(0.0325)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	1,050	1,050	1,050	1,050
R-squared	0.553	0.478	0.565	0.490
Optimal bandwidth	22.7	22.7	22.7	22.7

Table 7. Placebo Tests

Notes: In this table we report the results of several placebo tests. Panel A presents the results of a falsification test with other cutoff points of 100 m^2 and 80 m^2 in floor space with household-level data. The dependent variables are the number of children born after 2006 for columns (1) and (3) and a dummy of children born after 2006 for columns (2) and (4). Panel B presents the results of a falsification test with a sample of homes purchased after 2006 when the policies were already effective and households could consciously choose housing size. The outcome variables are the number of children for column (1) and a dummy of children for column (2). For both Panels: 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff, Size Diff*T=1*, all control variables in the baseline regressions, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

Panel A. Other cutoffs

	(1)	(2)	(3)	(4)
VARIABLES	Children Number	Children Dummy	Children Number	Children Dummy
T100 = 1	-0.027	0.003		
	(0.045)	(0.015)		
T80 = 1			0.002	-0.016
			(0.041)	(0.031)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Observations	1,722	1,694	1,690	1,729
R-squared	0.709	0.720	0.709	0.725
Optimal bandwidth	45.1	43.7	43.9	47.3

Panel B. Housing purchased after 2006

	(1)	(2)
VARIABLES	Children Number	Children Dummy
T = 1	-0.009	0.008
	(0.040)	(0.016)
Controls	Yes	Yes
Multiples of 10 sqm	Yes	Yes
County	Yes	Yes
Observations	1,302	1,302
R-squared	0.721	0.776
Optimal bandwidth	37.6	37.4

Table 8. Robustness Check: Alternative Fixed Effects and Nonlinearity

Notes: In this table we report the results of a robustness check with alternative fixed effect for columns (1) and (2) as well as a nonlinear model specification for columns (3) and (4). The dependent variables are the number of children born after 2006 for columns (1) and (3) and a dummy of children born after 2006 for columns (2) and (4). 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff, Size Diff*T=1,* control variable in the baseline regressions, a dummy for multiples of ten sqm. Province fixed effect is used in columns (1)-(2) and county fixed effect is used in columns (3)-(4). 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
			Children Number	Children Dummy
VARIABLES	Children Number	Children Dummy	(Non-linear)	(Non-linear)
T=1	0.077**	0.034**	0.050	0.023**
	(0.037)	(0.016)	(0.034)	(0.011)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
Province	Yes	Yes	No	No
County	No	No	Yes	Yes
Observations	1,576	1,608	1,524	1,558
R-squared	0.644	0.684	0.710	0.720
Optimal bandwidth	35.5	37.1	35.5	37.1

Table 9. Heterogeneity Analysis

Notes: In this table we report the results of heterogeneity analyses. Panel A presents the RD estimates for two subgroups sorted by whether the mother's years of education are lower or higher than the median of 9 years. The dependent variables are the number of children for columns (1) and (3) and an indicator of having children for columns (2) and (4). Panel B presents the RD estimates for two subgroups sorted by whether the mother's age is above or below 47 years old (as of 2018), with the following dependent variables: the number of children for columns (1) and (3) and an indicator of having children for columns (2) and (4). Panel C presents the RD estimates for two subgroups, sorted by whether annual income per individual was above or below the 50th percentile. The dependent variables are the number of children for columns (1) and (3) and an indicator of having children for columns (2) and (4). Panel D presents the RD estimates for three subgroups, sorted by having zero, one, two, or more children prior to 2006, with the following dependent variables: the number of children for columns (1), (2), and (4) and an indicator of having children for columns (3) and (5). 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff, Size Diff*T=1,* control variables in the baseline regressions, a dummy for multiples of ten sqm,and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

Panel A. By Mother's Education Year (as of 2018)

	(1)	(2)	(3)	(4)
	Educ Year (Mother)>=9		Educ Year (Mother)<9	
VARIABLES	Children Number	Children Dummy	Children Number	Children Dummy
T = 1	0.028	-0.013	0.125*	0.062***
	(0.058)	(0.020)	(0.064)	(0.015)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Observations	787	826	746	897
R-squared	0.751	0.766	0.656	0.617
Optimal bandwidth	33.6	36.6	45.0	60.4

Panel B. By Mother's Age (as of 2018)

	(1)	(2)	(3)	(4)
	Age (Mother)<=47		Age (Mother)>47	
VARIABLES	Children Number	Children Dummy	Children Number	Children Dummy
T = 1	0.188*	0.077**	-0.005	-0.008
	(0.106)	(0.031)	(0.015)	(0.015)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Observations	640	640	954	954
R-squared	0.715	0.680	0.215	0.204
Optimal bandwidth	35.4	35.5	41.6	41.6

Panel C. Household Financial Condition (as of 2018)

	(1)	(2)	(3)	(4)
	Above 50 Percentile Income Per Person		Below 50 Percentile Income Per Person	
VARIABLES	Children Number	Children Dummy	Children Number	Children Dummy
T = 1	0.139**	0.087***	0.032	0.007
	(0.062)	(0.029)	(0.055)	(0.015)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Observations	625	657	958	958
R-squared	0.781	0.776	0.721	0.732
Optimal bandwidth	35.3	39.8	40.2	40.8

Panel D. Birth Order

	(1)	(2)	(3)	(4)	(5)
	No Previous Child	One Previous Child		Two or More Previous Child	
VARIABLES	Children Number	Children Number	Children Dummy	Children Number	Children Dummy
T = 1	0.472**	0.113***	0.083**	0.012	0.013
	(0.194)	(0.036)	(0.035)	(0.049)	(0.018)
Controls	Yes	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes
Observations	301	554	571	610	929
R-squared	0.503	0.600	0.563	0.295	0.823
Optimal bandwidth	47.5	35.4	36.1	36.1	44.1

Table 10. Outcomes of Children Performance

Notes: In this table we report the results of local linear regressions of five dependent variables in columns (1)–(5). For column (1), *Weight_Birth* represents children's birth weights in kilograms and in the regression we control for children's gender and mother's age when giving birth. For column (2), *Weight* represents children's weight in kilograms as of survey year 2018. For column (3), *Height* represents children's height in centimeters as of survey year 2018. For column (4), *Reading* is an indicator variable that takes the value of 1 when children's reading performance is reported as "Excellent" and zero otherwise. For column (5), *Math* is an indicator variable that takes the value of one when children's math performance is reported as "Excellent" and zero otherwise. For columns (2)–(5) we control for children's gender and age. 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff, Size Diff*T=1,* control variables in the baseline regressions, birth order, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Weight_Birth	Weight	Height	Reading	Math
T = 1	0.258***	2.360**	4.671***	0.078	0.116**
	(0.036)	(0.997)	(1.310)	(0.061)	(0.044)
Control birth order	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes
Observations	1,123	813	805	571	563
R-squared	0.427	0.824	0.887	0.274	0.281
Optimal bandwidth	30.1	33.8	33.2	45.4	42.5

Table 11. Labor-Market Participation by Gender

Notes: In this table we report the discontinuity of labor-market participation with respect to housing size. Panel A reports the RD estimates of all samples, female subsample, and male subsample, with the dependent variable of a dummy indicating unemployed in columns (1)-(3). Panel B reports the RD estimates of all sample, female subsample, and male subsample in columns (1)-(3), respectively, with the dependent variable of daily hours of housework in columns (1)-(3). For both Panels, 1. Local linear regressions are used with the optimal bandwidth; 2. In all regressions we control for *Size Diff, Size Diff*T=1*, control variables in the baseline regressions, a dummy for multiples of ten sqm, county fixed effect; 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

Panel A. Pre-birth Employment Status

	(1)	(2)	(3)
VARIABLES	Unemployed	Unemployed	Unemployed
T = 1	0.092	0.052	0.107*
	(0.076)	(0.149)	(0.055)
Sample	All	Female	Male
Controls	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes
County	Yes	Yes	Yes
Observations	588	220	281
R-squared	0.327	0.403	0.383
Optimal bandwidth	44.3	37.8	49.9

Panel B. Post-birth Employment Status

	(1)	(2)	(3)
VARIABLES	Unemployed	Unemployed	Unemployed
T = 1	0.090**	0.171**	-0.011
	(0.034)	(0.065)	(0.043)
Sample	All	Female	Male
Controls	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes
County	Yes	Yes	Yes
Observations	848	413	482
R-squared	0.287	0.348	0.228
Optimal bandwidth	34.0	36.5	52.8

Table 12. Health Condition Before Giving Birth

Notes: In this table we report the discontinuity of parental health conditions with respect to housing size. Panel A presents the results using all samples; Panel B presents the results using the female sample; and Panel C presents the results using the male sample. We report the results of the local linear regression of four dependent variables in columns (1)–(4): three measure of self-rated health status (overall health scores, chronic disease, hospitalization) and one measure of subjective well-being prior to giving birth as outcome variables. 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff, Size Diff*T=1*, control variables in the baseline regressions, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
VARIABLES	Health Scores	No Chronic Disease	No Hospitalized	Satisfaction
Panel A. All Sample				
T = 1	0.019*	0.018**	0.017***	0.007**
	(0.010)	(0.007)	(0.006)	(0.003)
Observations	6,511	6,511	6,530	5,737
R-squared	0.263	0.289	0.277	0.106
Optimal bandwidth	46.9	46.9	47.9	36.8
Panel B. Female				
T = 1	0.014	0.010	0.012*	0.014**
	(0.009)	(0.006)	(0.006)	(0.006)
Observations	3,435	3,645	3,356	3,070
R-squared	0.253	0.280	0.257	0.098
Optimal bandwidth	46.0	50.5	42.5	38.9
Panel C. Male				
T = 1	0.018	0.019*	0.021**	0.006
	(0.014)	(0.012)	(0.011)	(0.005)
Observations	2,991	2,987	2,992	2,745
R-squared	0.302	0.322	0.320	0.107
Optimal bandwidth	43.0	42.9	43.5	39.0
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes

Table 13. Evidence from RD-DiD

Notes: In this table we report RD-DiD estimates using a longitudinal 2002–2018 dataset with the household-level sample. The dependent variable *Give_birth* is an indicator that equals one if a household produce a child in a given year and zero otherwise. The regression includes *(T=1)*After,* where *After* is an indicator of years after 2006 when the target policies became effective. We also include other controls, namely *(T=1)*After*Size Diff, After*Size Diff, (T = 1)*Size Diff, T=1* and *Size Diff*, a dummy for multiples of ten sqm, household fixed effect, year fixed effect, and county fixed effect. In column (1) we report results obtained using a sample consisting of children born between 2002 and 2008; in column (2) we report results obtained using a sample children born between 2002 and 2012; and in column (3) we report the results obtained using a sample of children born between 2002 and 2018. 1. Local linear regressions are used with the optimal bandwidth. 2. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
VARIABLES	*Give_birth*	*Give_birth*	*Give_birth*
*(T = 1)*After*	0.017**	0.018***	0.013***
	(0.008)	(0.005)	(0.004)
Children Birth Year	2002-2008	2002-2012	2002-2018
Other Controls	Yes	Yes	Yes
Household	Yes	Yes	Yes
Year	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes
Observations	13,164	22,020	38,290
R-squared	0.159	0.100	0.063
Optimal bandwidth	42.5	43.3	51.6

Table 14. Evidence from Longitudinal Data

Notes: In this table we report RD estimates using a longitudinal 2006–2018 dataset with the household-level sample. The dependent variable *Give_birth* represents an indicator that equals one if a household produce a child in a given year. For column (1), the regression includes *T=1*. For column (2)-(4), the regressions further include the interactions of *T=1* and *First 5 y*, an indicator of giving birth within the first five years, the interactions of *T=1* and *After 2013,* an indicator of giving birth after 2013, and the interactions of *T=1* and *After 2016,* an indicator of giving birth after 2016. 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff, Size Diff***T=1*, control variables in the baseline regressions, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
VARIABLES	*Give_birth*	*Give_birth*	*Give_birth*	*Give_birth*
T = 1	0.007*	0.000	0.012**	0.008*
	(0.003)	(0.006)	(0.005)	(0.004)
*(T=1)***First 5 y*		0.016*		
		(0.009)		
*(T=1)***After 2013*			-0.010	
			(0.010)	
*(T=1)***After 2016*				-0.004
				(0.010)
Controls	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Observations	19,068	19,068	19,068	19,068
R-squared	0.085	0.086	0.086	0.086
Optimal bandwidth	36.3	36.3	36.3	36.3

Appendix

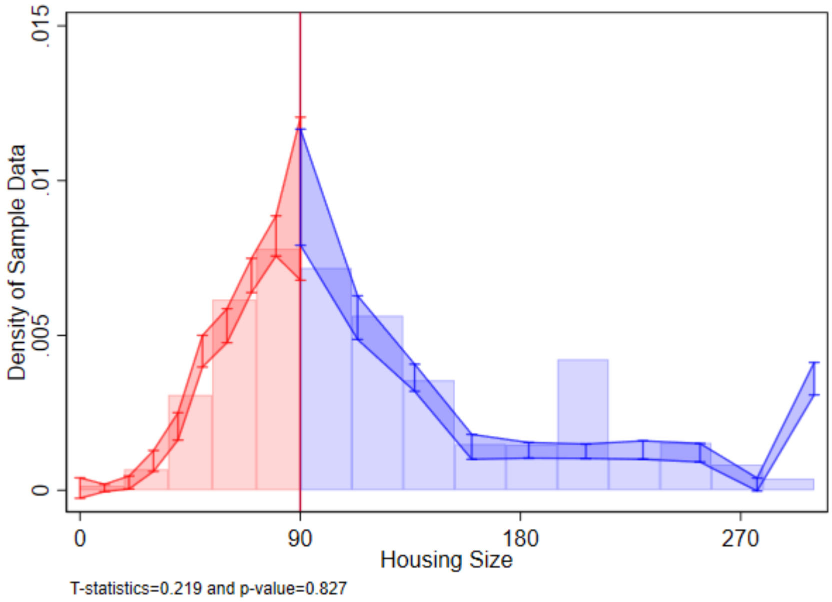

Figure A1. Density Distribution of Housing Size

Notes:1. The sample includes our sample households who purchased housing units before 2006 as reported in 2018 wave of the CFPS. 2. The vertical line represents the cut-off point, 90 m^2. 3. Shaded areas represent the 95 percent confidence intervals and corresponding error bars around the local polynomial estimates. 4. The optimal bandwidth to construct the density estimators is 23.9 at the left-side of the cutoff point and the optimal bandwidth is 25.5 at the right-side of the cutoff point.

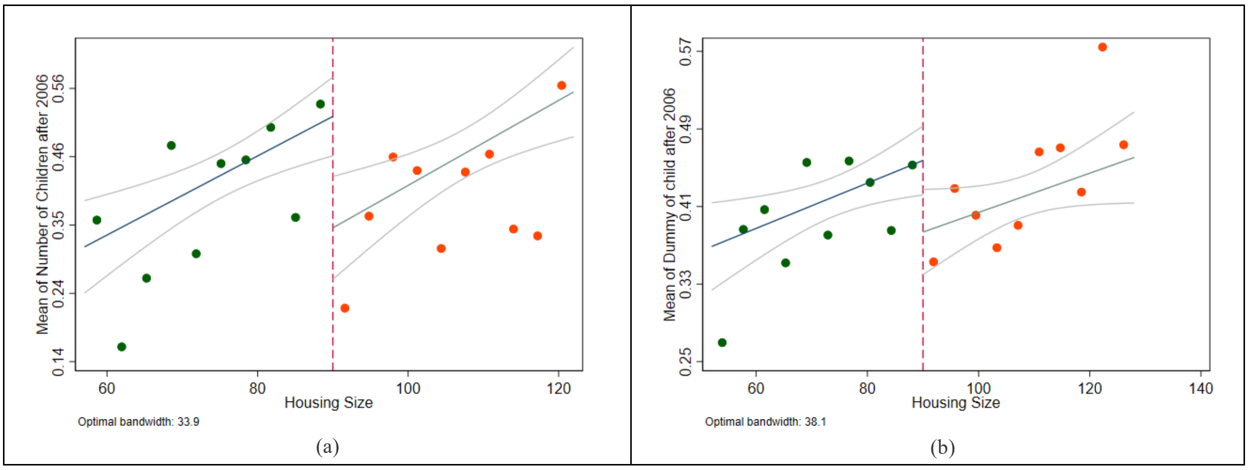

Figure A2. Housing Size Discontinuity and Fertility Outcomes Using Individual Sample

Notes: This figure plots the discontinuity of fertility outcomes with respect to housing size with the individual-level sample. The x-axis represents the floor spaces of housing units, while the y-axes represent the two measures of fertility outcomes: the number of children born after 2006 for graph (a) and an indicator variable for producing a child after 2006 for graph (b). 1. The circles represent conditional mean values of the respective variables for ten bins on each side of the cutoff point. 2. The solid lines are the fitted values from the local linear regression with the optimal bandwidth while the dashed lines are 95% confidence intervals. 3. The vertical line is the 90 m^2 floor-space cutoff point for the running variable.

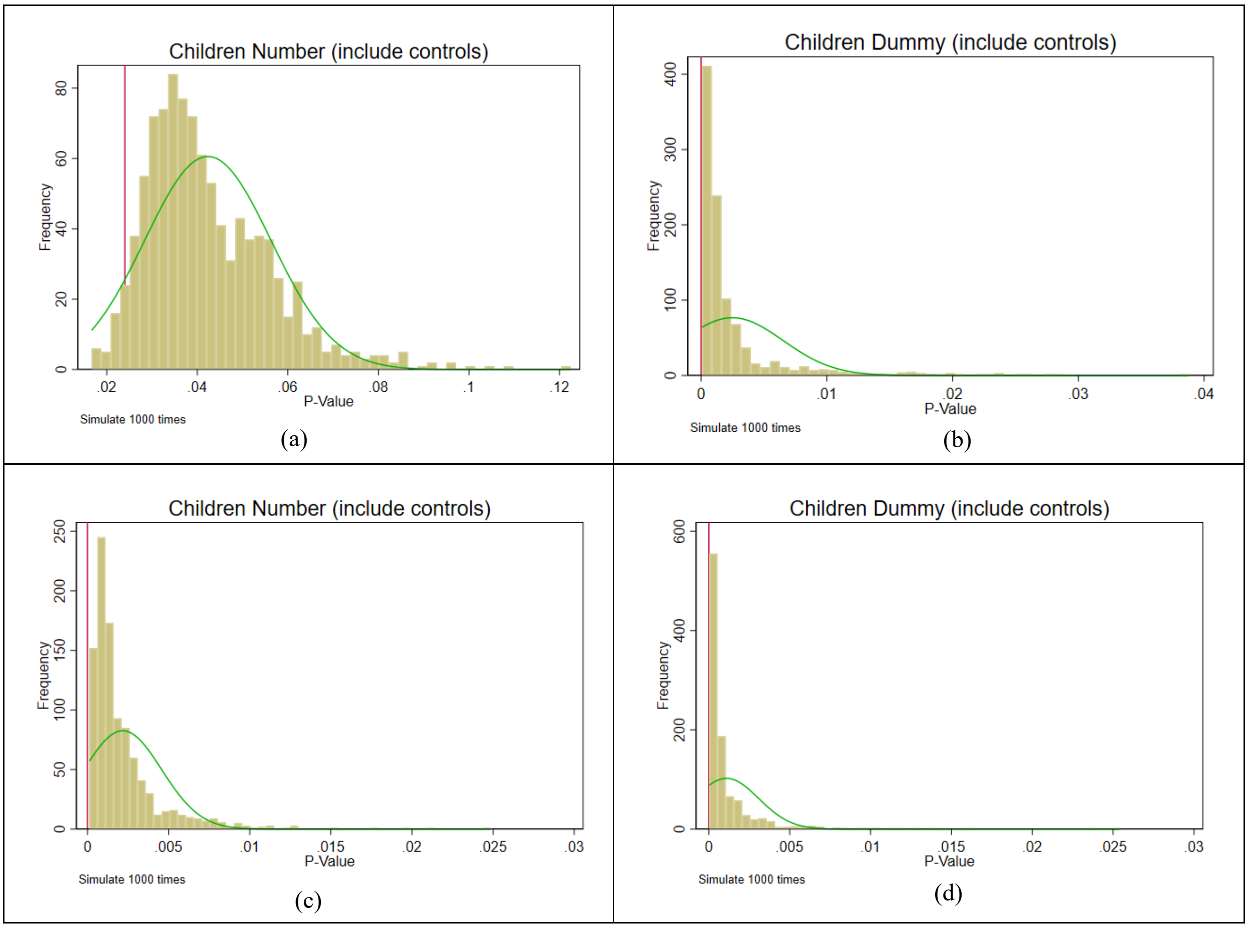

Figure A3. Self-Reporting Errors – Simulation Results

Notes: In this figure, we present the simulation results of randomly assigning floor spaces between 88 m^2 and 92 m^2 to housing units with sizes at 90 m^2, and rerunning the baseline regression model with control variables. Graphs (a)–(b) report the results using household-level data, and graphs (c)–(d) report the results using individual-level data. For graphs (a)–(d), the x-axis represents the p-value of the key estimate (*T=1*) for each simulation, the y-axis represents the frequency, the green curve represents the estimated kernel density, and the vertical line represents the baseline p-value. The dependent variables are the number of children born after 2006 in graphs (a) and (c), and the dummy of children born after 2006 in graphs (b) and (d).

Table A1. Baseline Regression with Individual-level Sample

Notes: In this table we report the results of baseline regressions of the effects of the housing wealth shock on fertility using the individual-level sample. The dependent variables are the two measures of fertility. Specifically, children born after 2006 are reported in columns (1) and (3), and dummies of children born after 2006 as dependent variables are reported in columns (2) and (4). 1. Local linear regressions are used with the optimal bandwidth. 2. All regressions control for a *Size Diff*, *Size Diff*T=1*, a dummy for multiples of ten sqm, county fixed effect, and columns (3) and (4) further include a list of control variables, namely *Previous Children, Previous Boy Dummy, Age(m), Edu Dummy(m), Married(m), Han, Urban Hukou(m), House Age* and *School Zone*. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
VARIABLES	Children Number	Children Dummy	Children Number	Children Dummy
T=1	0.106***	0.057**	0.087***	0.043***
	(0.022)	(0.021)	(0.022)	(0.009)
Size Diff	0.003	0.001	0.003**	0.001***
	(0.003)	(0.001)	(0.001)	(0.000)
*T*Size Diff*	-0.000	0.001	-0.001	0.000
	(0.005)	(0.002)	(0.002)	(0.001)
Previous Children			-0.250***	-0.121***
			(0.020)	(0.011)
Previous Boy Dummy			-0.248***	-0.157***
			(0.041)	(0.023)
Male			0.004	0.014
			(0.012)	(0.008)
Age			-0.022***	-0.019***
			(0.002)	(0.002)
Edu Dummy			-0.001	-0.004
			(0.019)	(0.011)
Married			-0.328***	-0.159***
			(0.034)	(0.047)
Han			0.060	0.040*
			(0.035)	(0.021)
Urban Hukou			-0.104***	-0.035*
			(0.034)	(0.018)
House Age			0.001	0.002**
			(0.001)	(0.001)
Constant	0.345***	0.251***	2.099***	1.456***
	(0.047)	(0.024)	(0.107)	(0.036)
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	3,172	3,339	3,172	3,339
R-squared	0.162	0.130	0.701	0.718
Optimal bandwidth	33.9	38.1	33.9	38.1

Table A2. The Regression of Fertility Outcomes on Housing Sizes

Notes: In this table we report the results of OLS regressions of fertility outcomes on housing sizes. The dependent variables are the two measures of fertility. Specifically, the number of children born after 2006 is reported in columns (1) and (2), and the dummy of children born after 2006 are reported in columns (3) and (4). Baseline optimal bandwidths are included for columns (1) and (3), and full sample is used for columns (2) and (4). 1. All regressions control for a list of baseline respondent characteristics, a dummy for multiples of ten sqm, and county fixed effect. 3. Robust standard errors reported in parentheses: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
VARIABLES	Children Number after 2006	Children Number after 2006	Children Dummy after 2006	Children Dummy after 2006
Ln(House Size)	0.035	0.032**	0.009	0.020**
	(0.064)	(0.016)	(0.037)	(0.010)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	1,568	3,242	1,568	3,242
R-squared	0.717	0.717	0.731	0.739
Bandwidth	35.4	No	37.1	No

Table A3. Results of Heckman's Second-Stage Regression

Notes: In this table we report the examination of the sample selection bias with two-stage Heckman estimation. The regression results of the first stage are showed in column (1), and the results for the second stage are reported in columns (2) and (3). IMR represents the inverse Mill's ratio, which is obtained from the first stage. For columns (2) and (3), the same baseline regression specification with additional independent variable, IMR, is implemented. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	First-Stage	Second-Stage	
VARIABLES	Housing Owner Before 2006 Dummy	Children Number	Children Dummy
IMR (λ)	–	-0.511	-0.574
		(5.126)	(2.246)
T=1	–	0.089*	0.036**
		(0.046)	(0.016)
Size Diff	–	0.004**	0.001
		(0.002)	(0.001)
*T*Size Diff*	–	-0.003	0.000
		(0.002)	(0.001)
Previous Children	-0.063***	-0.250	-0.102
	(0.024)	(0.186)	(0.081)
Previous Boy Dummy	-0.047	-0.236*	-0.135**
	(0.043)	(0.132)	(0.056)
Age(m)	0.021***	-0.029	-0.027
	(0.003)	(0.064)	(0.029)
Educ Dummy(m)	0.131***	-0.062	-0.052
	(0.037)	(0.378)	(0.175)
Married(m)	0.095*	-0.382	-0.202*
	(0.056)	(0.308)	(0.115)
Han	0.085**	0.007	-0.031
	(0.035)	(0.255)	(0.112)
Urban Hukou(m)	-0.004	-0.123**	-0.045
	(0.043)	(0.054)	(0.028)
School Zone	0.052	0.027	0.026
	(0.049)	(0.163)	(0.071)
Constant	-0.902***	2.893	2.308
	(0.096)	(6.857)	(3.000)
Multiples of 10 sqm	–	Yes	Yes
County FE	–	Yes	Yes
Observations	6,395	1,568	1,600
R-squared	–	0.718	0.730
Optimal bandwidth	–	35.5	37.1

Table A4. IV Estimation for the Individual-level Sample

Notes: In this table we report the second-stage results of IV estimations using the individual-level sample. These regressions use the treatment status($T=1$) as an instrument for the growth in housing prices. For columns (1) and (2), the target independent variable is the annualized growth rate of housing prices. The dependent variable for column (1) is the number of children born after 2006 and for column (2) it is the dummy of children born after 2006. In columns (3) and (4), the target independent variable is the average growth rate of housing prices. The dependent variable for column (3) is the number of children born after 2006, and for column (4) it is the dummy of children born after 2006. 1. Local linear regressions are used with the optimal bandwidth; 2. In all regressions we control for all control variables in the baseline regression, a dummy for multiples of ten sqm, and county fixed effect; 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

VARIABLES	(1) Children Number	(2) Children Dummy	(3) Children Number	(4) Children Dummy
Annualized growth rate of housing prices	0.0446**	0.0466***		
	(0.0188)	(0.0164)		
Compounded annual rate of change in housing prices			0.0460**	0.0486***
			(0.0199)	(0.0173)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	2,138	2,138	2,126	2,126
R-squared	0.628	0.513	0.635	0.524
Optimal bandwidth	21.5	21.5	20.2	20.2

Table A5. Sample of Houses Purchased in 2004-2005

Notes: In this table we report the RD estimates using a subsample of houses purchased only in 2004 and 2005. The dependent variable for column (1) is the annualized growth rate of housing prices. The dependent variable for column (2) report is the dummy of children born after 2006. 1. Local linear regressions are used with the optimal bandwidth; 2. All regressions control for *Size Diff, T*Size Diff,* control variables in the baseline regressions, a dummy for multiples of ten sqm, and county fixed effect; 3. Standard errors in parentheses are double clustered at the housing size and provincial level: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)
VARIABLES	Annualized growth rate of housing prices	Children Dummy
T = 1	0.048*	0.091
	(0.023)	(0.084)
Sample purchase year	2004-2005	2004-2005
Controls	Yes	Yes
Multiples of 10 sqm	Yes	Yes
County FE	Yes	Yes
Observations	489	352
R-squared	0.606	0.884
Optimal bandwidth	45.5	40.4

Table A6. Placebo Tests for Individual-level Sample

Notes: In this table we report the results of several placebo tests with individual-level sample. Panel A presents the results of a falsification test with other cutoff points of 100 m^2 and 80 m^2 in floor space. The dependent variables are the number of children born after 2006 for columns (1) and (3) and a dummy of children born after 2006 for columns (2) and (4). Panel B presents the results of a falsification test with a sample of homes purchased after 2006 when the policies were already effective and households could consciously choose housing size. The outcome variables are the number of children for column (1) and a dummy of children for column (2). For both Panels: 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff, T=1*Size Diff*, control variables in the baseline regressions, a dummy for multiples of ten sqm, and county fixed effect; 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

Panel A. Other cutoffs

	(1)	(2)	(3)	(4)
	Individual Level			
VARIABLES	Children Number	Children Dummy	Children Number	Children Dummy
T100 = 1	0.016	-0.009		
	(0.053)	(0.016)		
T80 = 1			0.029	-0.003
			(0.032)	(0.020)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Observations	3,146	4,200	3,643	3,855
R-squared	0.699	0.711	0.691	0.712
Optimal bandwidth	40.0	58.3	49.8	54.9

Panel B. Purchased after 2006

	(1)	(2)
	Individual Level	
VARIABLES	Children Number	Children Dummy
T = 1	0.009	0.013
	(0.032)	(0.017)
Controls	Yes	Yes
Multiples of 10 sqm	Yes	Yes
County	Yes	Yes
Observations	2,572	2,611
R-squared	0.713	0.784
Optimal bandwidth	33.5	34.2

Table A7. Robustness Checks for Individual-level Sample

Notes: In this table we report the results of several robustness checks with the individual-level sample. Panel A presents the results of a robustness check with alternative provincial fixed effects for columns (1) and (2) as well as a nonlinear model specification for columns (3) and (4). The dependent variables are the number of children born after 2006 for columns (1) and (3) and a dummy of children born after 2006 for columns (2) and (4). Panel B presents the results of a robustness check with alternative instead of optimal bandwidths. The dependent variable is the number of children born after 2006 for columns (1), (3), (5), and (7). The dependent variable is a dummy for having children after 2006 for columns (2), (4), (6), and (8). The chosen bandwidths are 30, 35, 40, and 45, respectively. Panel C presents the results of a robustness check after removing observations at 90 m^2 in floor space. The dependent variables are the number of children born after 2006 for column (1) and a dummy for children born after 2006 for column (2). For all Panels: 1. Local linear regressions are used with the optimal or alternative bandwidth. 2. In all regressions we control for *Size Diff, T=1*Size Diff*, control variables in the baseline regressions, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

Panel A. Alternative fixed effect and nonlinearity

	(1)	(2)	(3)	(4)
VARIABLES	Children Number	Children Dummy	Children Number (Non-linear)	Children Dummy (Non-linear)
T = 1	0.090***	0.046***	0.054***	0.025**
	(0.024)	(0.015)	(0.018)	(0.011)
Controls	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	No	No
County	No	No	Yes	Yes
Observations	3,193	3,360	3,123	3,290
R-squared	0.636	0.681	0.694	0.712
Optimal bandwidth	33.9	38.1	33.9	38.1

Panel B. Alternative bandwidth choices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Children Number	Children Dummy	Children Number	Children Dummy	Children Number	Children Dummy	Children Number	Children Dummy
T = 1	0.077***	0.034**	0.082***	0.037***	0.078**	0.042***	0.079**	0.042***
	(0.017)	(0.013)	(0.023)	(0.012)	(0.030)	(0.010)	(0.031)	(0.011)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,054	3,054	3,195	3,195	3,580	3,580	3,692	3,692
R-squared	0.692	0.714	0.696	0.716	0.694	0.715	0.697	0.715
Bandwidth	30	30	35	35	40	40	45	45

Panel C. Self-reporting errors – exclude obs. at 90 m^2

VARIABLES	(1) Children Number	(2) Children Dummy
T = 1	0.078***	0.034*
	(0.022)	(0.019)
Controls	Yes	Yes
Multiples of 10 sqm	Yes	Yes
County	Yes	Yes
Observations	3,074	3,097
R-squared	0.704	0.719
Optimal bandwidth	38.1	39.2

Table A8. Evidence from RD-DiD with Individual-level Sample

Notes: In this table we report RD-DiD estimates using a longitudinal 2002–2018 dataset with the individual-level sample. The dependent variable *Give_birth* is an indicator that equals one if a household produce a child in a given year and zero otherwise. The regression includes *(T=1)*After,* where *After* is an indicator of years after 2006 when the target policies became effective. We also include other controls, namely *(T=1)*After*Size Diff, After*Size Diff, (T = 1)*Size Diff, T=1* and *Size Diff*, a dummy for multiples of ten sqm, household fixed effect, year fixed effect, and county fixed effect. In column (1) we report results obtained using a sample consisting of children born between 2002 and 2008; in column (2) we report results obtained using a sample children born between 2002 and 2012; and in column (3) we report the results obtained using a sample of children born between 2002 and 2018. 1. Local linear regressions are used with the optimal bandwidth. 2. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
VARIABLES	Give Birth	Give Birth	Give Birth
*(T = 1)*After*	0.013	0.016*	0.017***
	(0.009)	(0.008)	(0.006)
Children Birth Year	2002-2008	2002-2012	2002-2018
Other Controls	Yes	Yes	Yes
Household	Yes	Yes	Yes
Year	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes
Observations	28,764	46,620	68,800
R-squared	0.157	0.097	0.063
Optimal bandwidth	45.1	41.6	39.2

Table A9. Evidence from Longitudinal Data with Individual-level Sample

Notes: In this table we report RD estimates using a longitudinal 2006–2018 dataset with the individual -level sample. The dependent variable *Give_birth* represents an indicator that equals one if a household produce a child in a given year. For column (1), the regression includes *T=1*. For column (2)-(4), the regressions further include the interactions of *T=1* and *First 5 y*, an indicator of giving birth within the first five years, the interactions of *T=1* and *After 2013,* an indicator of giving birth after 2013, and the interactions of *T=1* and *After 2016,* an indicator of giving birth after 2016. 1. Local linear regressions are used with the optimal bandwidth. 2. In all regressions we control for *Size Diff, Size Diff*T=1*, control variables in the baseline regressions, a dummy for multiples of ten sqm, and county fixed effect. 3. Standard errors reported in parentheses are double clustered at the housing size and provincial levels: ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
VARIABLES	Give Birth	Give Birth	Give Birth	Give Birth
T = 1 (Indi-level)	0.006***	-0.000	0.010**	0.006**
	(0.001)	(0.004)	(0.004)	(0.002)
*(T=1)*First 5 y*		0.015**		
		(0.007)		
*(T=1)*After 2013*			-0.008	
			(0.009)	
*(T=1)*After 2016*				0.001
				(0.009)
Controls	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Multiples of 10 sqm	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
Observations	37,236	37,236	37,236	37,236
R-squared	0.083	0.083	0.083	0.083
Optimal bandwidth	30.0	30.0	30.0	30.0