The Effect of House Prices on Fertility: Evidence from House Purchase Restrictions

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Abstract

We assess the causal effect of house price increases on the great birth rate decline in China from 2016 onward, and on the country's marriage market and private educational investments. Quasi-experimental increases in house prices, driven by the capital spillovers of house purchase restrictions in large cities to nearby unregulated cities, significantly reduced the birth rate in these cities. In the microdata, the individual-level effects were concentrated among rural people who do not own urban homes, especially when rural schools are spatially scarce. Both the marriage and the within-marriage margins contributed to their fertility effects. Private investments in children's education increased after the house price shock. A back-of-the-envelope calculation suggests that the positive house price shock accounted for a non-negligible share of the aggregate birth decline.

Keywords: house prices, fertility choice, marriage, urbanization, human capital investment

JEL Classification: D13, D15, J13, O15, R21, R31

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1. Introduction

The birth rate declined amid a surge in urban house prices in China from 2016 onward. National urban house prices increased by 54% between 2016 and 2021. Contemporaneously, the birth rate decreased from 13.6‰ in 2016 to an average of 9.3‰ during 2017–2021 and continued to drop further. This great birth decline sparked concerns of a looming demographic crisis for the country, with potential long-term consequences such as labor shortages, increased burden of elderly care, and slower economic growth. Globally, this trend could influence international markets, affect global demographics, and shift patterns of economic and political power.

Assessing the causal impact of house prices on fertility during this period is critical yet challenging, as demographic shifts can influence house prices, which in turn reflect future expectations. The first generation of studies on the house price and fertility relationship utilized indices of local house prices instead of family's housing wealth, assuming that house prices, external to an individual's decision, is exogenous to fertility. The second generation of studies instrumented house prices using the Saiz (2010a) supply elasticity strategy. However, it has been shown that inelastic cities tend to be superstar cities with more productive residents (Van Nieuwerburgh and Weill, 2010; Gyourko, Mayer, and Sinai, 2013), and these cities have different demand growth (e.g. Davidoff, 2016), complicating the interpretation of these instruments.

In response, we employ a unique quasi-experiment to estimate the causal effect of urban house prices on fertility. The quasi-experiment originates from unintended effects of policy interventions. In 2016, to cool down the overheated housing market, major Chinese cities implemented house purchase restrictions (HPRs) that curtailed local investment purchases. This policy redirected investment demand from these large, regulated cities to nearby unregulated cities—our treated group—resulting in a significant, exogenous shock to house prices in these areas. House prices increased significantly in these cities compared to farther away unregulated cities—our control group. Fundamentals did not diverge between the treated cities and the control cities.

Our identification assumption posits that, absent the house purchase restriction spillover treatment, urban house prices and fertility outcomes in the treated group and those in the control group would follow their pre-existing trends. Using a difference-in-differences estimation and controlling for city, time fixed effects, as well as time-varying controls and pre-existing trends, we find that in the four years after the house price shock, a period which saw a 12.4% average abnormal increase in treated cities' urban house prices, we observe an average abnormal reduction of 1.68‰ in treatment cities' birth rate relative to the control cities.

Instrumental variable analysis using the quasi-experiment yields a semi-elasticity of -8.8‰, implying that a 10% exogenous increase in urban house prices would reduce the birth rate by 0.88‰. Additionally, microdata analysis of women of childbearing age in treated cities showed a notable reduction of 0.028 (against a sample average of 0.060) in the annual average number of newborns per woman. These findings are robust across various specifications, including employing alternative distance cutoffs for treatment designation, as well as using a continuous distance specification that allows for a linear decay of treatment effects with distance.

Our subsequent analysis focused on the mechanisms underlying these changes in fertility. Institutionally in our setting, both urban and rural residents primarily own their homes, but with significant differences. Rural dwellings are non-tradable, but urban homes are tradable. Furthermore, urban homeownership is closely linked to educational opportunities.

We reestimate the treatment effect by homeownership status and by geographical location, urban or rural. Crucially, the significantly negative impact of urban house price shocks on fertility was pronounced among rural residents owning only rural homes. For individuals who do not own any local homes—a group that is ”mobile”—we observe a fertility reduction that was not statistically significant. For those owning urban properties, whether rural or urban residents, there was no pronounced fertility reduction. This pattern indicates that beyond the cost for space, other costs that urban house prices represent, such as access to education, played a significant role.

Delving a step deeper, in regions with scarce rural schools, indicated by longer travel distances to school, we observed a more substantial decline in fertility among rural dwelling owners with no urban homeownership, suggesting that limited access to educational resources significantly influenced fertility decisions. Moreover, we examined the impact of urban house price increases on marriage (Wei and Zhang, 2011; Wei, Zhang, and Liu, 2012) and within-marriage fertility decisions and find both to have decreased significantly among rural dwelling owners with no urban homeownership, especially when local marriage competition is intense, after the house price shock.

A back-of-the-envelope calculation suggests that our house price shock accounted for a significant part of the great birth rate decline in the aggregate in China. Using the rate of decay in the continuous distance specification and a potential outcome framework (e.g. Chodorow-Reich, 2019, 2020), we estimate that the house price surge due to spillovers of the house purchase restrictions accounted for 10.4% of the aggregate birth shortfall in the post-treatment period, or 2.46 million births. Furthermore, out of the 54% aggregate increase in urban house prices in the post-treatment period, if any 1% of the increase is due to an investment demand shock, it would additionally explain 2.07% of the aggregate birth decline.

We carefully examined several alternative explanations for the observed fertility decline and found no support. First, while an aging demographic in rural areas could be suspected, our analysis adjusts for age effects and we show that fertility and marriage impacts are most pronounced among the rural youth. Second, we considered increased rural-to-urban migration during the post period as a potential cause. We hypothetically redefined the ”treatment group” to include cities with significant post-period urbanization, and found that these cities actually exhibited higher birth rates. Third, we considered a potential spike and falling back in birth rates following the relaxation and eventual abolition of the one-child policy (OCP) between 2013 and 2015. However, our analysis using the OCP timing showed no significant divergence in fertility responses. These findings support our initial hypothesis that the fertility declines are primarily linked to the impacts of rising urban house prices, rather than other factors.

From the perspective of human capital accumulation, we explored whether the investment in children's education reacted significantly to undo the fertility reduction (e.g. Barro and Becker, 1989; Becker, Murphy, and Tamura, 1990; Galor and Weil, 2000). We observed a significant increase, approximately 58% above pre-shock levels, in educational expenditures by parents among rural dwelling owners with no urban homeownership. This suggests that increased private educational investments responded strategically to the prohibitive costs of alternative urban educational opportunities. This enhanced investments in education might offset some of the impacts of reduced fertility on aggregate human capital accumulation. Taken together, our causal estimates on birth rates, fertility, marriage, and educational investments highlight the complex interplay between educational opportunities, housing costs, and fertility decisions. By documenting this set of causal dynamics for the first time, our paper contributes to the literature on the role of inequality and local educational infrastructure in shaping human capital formation (e.g. De La Croix and Doepke, 2003; Chetty et al., 2014; Heckman and Landersø, 2022).

Our paper contributes to the literature on the real effect of house prices. We show that the causal effect of house prices can be important in the aggregate for fertility, a consequential outcome that complements the existing studies for corporate investment (Chaney, Sraer, and Thesmar, 2012; Martín, Moral-Benito, and Schmitz, 2021), entrepreneurship (Corradin and Popov, 2015; Schmalz, Sraer, and Thesmar, 2017), self-employment (Adelino, Schoar, and Severino, 2015), hiring (Bednarek et al., 2021), labor productivity (Bernstein, McQuade, and Townsend, 2021; Gu et al., 2021), and consumer spending (Mian, Rao, and Sufi, 2013; Aladangady, 2017; Guren et al., 2021b; Deng et al., 2022; Sodini et al., 2023).

Our paper also contributes to the quickly growing literature on investment or speculative demand in housing markets. DeFusco et al. (2018) document that spillovers between local housing markets in the United States are hard to square with local fundamentals. Chinco and Mayer (2016), Badarinza and Ramadorai (2018), Cvijanovic and Spaenjers (2018), Sá (2016), Gorback and Keys (2020), and Li, Shen, and Zhang (2020) show that demand from out-of-town investors is an important source of house price fluctuations, whereas Favilukis and Van Nieuwerburgh (2021) and Deng et al. (2022) studies the effect of out-of-town demand on city welfare and consumer spending. Nathanson and Zwick (2018) and DeFusco, Nathanson, and Zwick (2022) show the importance of speculative dynamics within the housing market, and Charles, Hurst, and Notowidigdo (2018, 2019) and Gao, Sockin, and Xiong (2020) study the effect of housing speculation on construction and local labor market outcomes.

Our paper creates a dialogue between quasi-experimental studies on the causal effect of house prices and the study of population dynamics and human capital investment. The potential effect of house prices on fertility and human capital investment has been proposed at least as early as Becker (1960). Yi and Zhang (2010) find house prices to negatively predict fertility in time-series data in Hong Kong. Clark (2012) find expensive house prices are associated with a fertility delay in the United States. Lovenheim and Mumford (2013), Clark and Ferrer (2019) and Daysal et al. (2021) find short-run home price rises to predict an increase fertility for owners in the United States, Canada, and Denmark, whereas Liu, Xing, and Zhang (2020) and Atalay, Li, and Whelan (2021) find it to predict reduction in fertility for renters in China and in the United

States. Using a Saiz (2010a) instrument, Dettling and Kearney (2014) finds a rise in home prices to increase fertility among owners and decrease it among renters in the United States, and Ge and Zhang (2019) Clark, Yi, and Zhang (2020), Liu, Liu, and Wang (2023) and Meng, Peng, and Zhou (2023) find it to reduce fertility in China. Most related to our study is Tan et al. (2023), who use a quasi-experiment in 2006 that reduced the down-payment ratio for urban homes no larger than 90 m^2. Using a regression discontinuity design, they find the associated housing wealth rise to increase fertility and child health for owners of such homes. Our study is unique in that we do not assume house prices are external to demographic dynamics, that we address the recent challenges to the Saiz (2010b) instrument, and that we use a recent quasi-experiment that informs about the causal effect of house prices on fertility at the city-level as well as the individual-level across comprehensive housing tenure and geographical statuses, allowing us to assess the aggregated causal effect of house prices on fertility and its disaggregated mechanisms.

The rest of this paper is organized as follows: Section 2 further introduces the institutional background and the shocks of the purchase restrictions. In Section 3, we explain the estimation strategy and data construction. Section 4 presents the estimate effect of house prices on city-level and individual-level fertility. Section 5 presents subsequent analysis on the mechanisms driving these changes in fertility. In Section 6, we entertain alternative explanations and discuss the aggregate significance of the house price shock's fertility effects. Section 7 concludes.

2. Institutional Background

This section describes (i) the great birth decline in China since 2016 and (iii) how the cost of urban homeownership is related to precursors and returns to fertility, and how that cost may be especially important for rural households given ownership and education resource differences.

2.1 The birth decline in China

China's birth rate hit its lowest level since the formation of the People's Republic of China 70 years ago. In 2021, the birth rate was 7.5‰. Before the most recent birth decline, China's birth rate hovered around a modest average of approximately 13‰ in this century. With concerns about an aging population, the "one-child" policy was partially relaxed in 2013 and fully relaxed in 2015. The change did not significantly boost birth rates. In 2016, the birth rate was 13.6‰. From 2016 to 2021, China's birth rate went downhill, declining by 1.2‰ per year on average. This was a great birth decline that precipitated the country's population peaking in 2023.

China's birth decline will impact the country's and the world's economy through implications on its production capacity and savings demand. Several reasons were discussed as potential underlying the birth decline in China. People frequently cite the high cost of living in Chinese cities as a key reason for not having children, among other main reasons such as the cost of education, and poor support for women in jobs. However, empirically identifying the causal effect remains challenging. For example, the high cost of living in cities is endogenously tied to marriage competitiveness (Wei and Zhang, 2011) and the latent demand for births would also affect the local demand for housing.

2.2 Urban home ownership as a ticket to marriage and children's education

Urban home ownership in China is relevant for marriage prospects and for school quality—hence the ability to have children and the returns to having children. In China, home ownership is not merely a matter of housing—it's also critically important for social status and is often considered essential for marriage. Owning a home is commonly seen as a prerequisite for marriage, especially for men. This perspective stems from traditional views where men are expected to provide a stable and secure environment for their future family. The property ownership becomes a visible measure of financial stability and readiness to start a family.

Urban home ownership is also deeply intertwined with educational opportunities in China. In urban areas, access to public schools is often linked to property ownership within the school's catchment area. The system operates on a hierarchy of eligibility: the highest priority is given to residents who have all three qualifiers—local *hukou* (household registration, often eligible with property ownership), property ownership, and residential location. This is followed by those with two qualifiers , and lastly, those with only one, with *hukou* given priority. For individuals in the single-qualifier category, school places are contingent on availability after accommodating applicants with two or three qualifiers.

Between urban and rural areas, the educational gap is significant (Li, Loyalka, Rozelle, Wu, and Xie, 2015). Buying an urban home becomes a prevalent plan for rural individuals, not only for immersion into cities, but also for better marriage prospects and education opportunities for children. Over recent years, there has been a trend toward the closing of rural schools in China, which is largely driven by urbanization. This leads to the consolidation of schools, where several small rural schools are merged into larger regional ones. While this approach aims to manage resources more efficiently, it often results in increased travel distances for students and the loss of community-based education, further disadvantaging rural students. As of 2016, data from our sample shows that 17% of rural households have purchased urban homes, compared to 85% of urban households. There are other disadvantages, for example, rural homes are not tradable in China, but urban homes are tradable. Consequently, any positive shock to urban house prices could disproportionately heighten the barriers for rural individuals, affecting their marriage prospects and their ability to provide quality education for their children.

3. Empirical Strategy

This section first describes the house purchase restriction spillover quasi-experiment and how we use it to identify the causal effect of house prices on fertility. We further describe the treatment designation, the identification assumption, the regression model, and the data used in the tests.

3.1 The house purchase restriction spillover quasi-experiment

In China, a reform in 1998 marketized the supply and ownership of urban homes. In the ensuing period, urban house prices rose quickly, especially in large cities. The demand for owning urban homes transcended the need for shelter, as purchasing property also became a favored investment strategy. The urban housing market ”overheating” became a policy worry. Policy tools were designed and used to cool down local urban housing markets.

In September 2016, the local government of all Tier-1 and a large number of Tier-2 cities (the regulated cities) implemented a policy referred to as ”house purchase restrictions” (HPRs), aimed at limiting housing demand from speculators who often own multiple properties. In March 2017, these policies were reiterated and made more stringent in certain cases.

The HPRs limited the number of homes investors can buy in the regulated cities, and also reduced the credit availability on investment properties there. These measures included higher down payment requirements and increased mortgage rates, sometimes completely prohibiting investment purchases for those owning more than two or three homes. After the HPRs was implemented, the regulated city investors can still purchase properties in the unregulated cities. The nearby unregulated cities became natural destinations. The closeness facilitates information gathering and occasional monitoring of the investment properties.

This created a ”house purchase restriction spillover” shock that inflated the house prices in the nearby unregulated cities despite any changes in the fundamentals, as studied by Deng, Liao, Yu, and Zhang (2022). They located 22 regulated cities that adopted HPRs in late 2016 and early 2017, and studied housing market dynamics in nearby unregulated cities, compared to farther away unregulated cities. They found that house price increases in the regulated cities reduced after the HPRs were imposed. Immediately, online search activity for real estate in the nearby unregulated cities surged from the regulated cities.

House prices and transaction volumes in the nearby unregulated cities rose sharply. These cities’ bank deposits similarly increased, aligning with house price hikes, indicative of capital inflows. Moreover, the rise in home transaction volumes in the nearby unregulated cities paralleled the decline in transaction volumes in the regulated cities. No evidence suggested changes in rents or economic growth. Local governments of the nearby unregulated cities announced they are concerned of this phenomenon. The house purchase restriction spillover shock created a unique opportunity to study the causal effect of house prices on fertility, which in this study we exploit.

3.2 Designation of treatment status and plausibility of quasi-experiment assumptions

We designate unregulated cities closer than 250 km to the nearest regulated city as treated cities to the house purchase restriction spillover effect. The farther away unregulated cities are the control cities. The 250 km distance facilitates a travel time that is approximately 2–3 hours by car and approximately 1 hour by high-speed rail. Investment homes in the treated cities are closer for investors in the regulated cities to screen and occasionally monitor, and information regarding the treated cities' properties may be more readily available. These cities are at the level of commuting zones, meaning that they are also not near enough for commuting from the regulated cities.

All our results are robust to alternative choices of the distance cutoff (200 km or 300 km). The discrete designation of treatment status reduces noise in statistical estimations. But we acknowledge that there are no strong reasons to think that the HPR spillover effects should change discontinuously across the distance cutoff. Instead, we test and show that our results are also robust to a continuous distance specification, where we allow the HPR spillover effects to decay log-linearly with distance.

The treatment effectively shocked local urban house prices, as shown in Figure 1. Before the HPR spillover shock, urban house prices in treated cities increased stably, approximately 0.5% annually faster than the control cities. Strikingly, urban house prices in treated cities increased abnormally and sharply faster after the HPR spillover shock of late 2016 and early 2017. This urban house price movement in the post period was distinctively different from the pre-existing trends. The urban house price gap between the treated and control cities quickly rose to 9% relative to trend in 2017, and 12% relative to trend in 2018 and 2019.

[**Figure 1 about here**]

There are reasons to think that the treatment was external and plausibly exogenous. There is evidence that the treated cities' surge in urban house prices was led by external demand from the regulated cities. Using the same treatment designation, Deng et al. (2022) documented that web searches for real estate in the nearby unregulated cities from the regulated cities significantly increased immediately after the imposition of HPRs.

There is also no evidence that the surge in treated cities' urban house prices was correlated with the local fundamentals. Had the treatment been solely from out-of-town investment demand, it should not obviously affect rents or tradable economic output. Deng et al. (2022) tested responses in rents, local output growth, and employment growth. Indeed, they found estimates that are insignificant and close to zero. For example, they found rents to relatively reduce by 0.8% (t-value 0.67) in the treated cities, output growth to reduce by 0.2% (t-value 0.20), and employment growth to increase by 0.4% (t-value 0.96). They found some increase in local real estate construction investment (17.3%), as expected after an out-of-town demand shock. But it was not statistically significant (t-value 1.12). These findings provide initial support that the house purchase restriction spillover event constitutes a quasi-experiment in urban house prices.

3.3 Regression specification

Our identification assumption is that absent the house purchase restriction spillover treatment, urban house prices and fertility outcomes in the treated cities and those in the control cities would grow at their pre-existing trends. We follow Wolfers (2006) and Bilinski and Hatfield (2019) and use the following extension of the difference-in-differences regression model:

$$Y_{i,t} = \beta \times Treat_i \times Post_t + \Gamma X_{i,t} + \gamma(i)t + \alpha_i + \delta_t + \epsilon_{i,t}. \tag{1}$$

As standard, α_i is the city (individual) fixed effect, δ_t is the time fixed effect, and $X_{i,t}$ are the time-varying controls. β is the coefficient of interest that measures the treatment effect. To separate the treatment effect from pre-existing trends, we include $\gamma(i)t$, which delineates linear pre-existing trends. In all estimations, we included treatment group-specific trends, or included city-specific trends. The results are the same.

Following Bilinski and Hatfield (2019) , to make sure $\gamma(i)t$ are only estimated off of pre-treatment data, we saturate the model with post-period treatment-time interaction dummies, and use the average coefficient of these post-period treatment-time interaction dummies as the treatment effect estimate $\hat{\beta}$.

We designate the post timing according to the time period needed for a response in the $Y_{i,t}$. We use annual-level data. House prices in treated cities may react immediately after house purchase restriction spillovers. Because the house purchase restrictions were enacted in the regulated cities in September 2016, the initial full year when house prices in treated cities were impacted was 2017, and only a part of 2016 was impacted. Therefore, we assess treatment effects starting from 2017 and use data through 2015 to estimate pre-existing trends in house prices.

For fertility, taking into account the pregnancy period, the first full year when fertility was impacted in treated cities was 2018, and only a part of 2017 was impacted. Thus, we begin to assess treatment effects in 2018 and estimated pre-existing trends in fertility using data through 2016. We also used marriage and private educational investments as additional treatment outcomes, assuming these variables react as swiftly as house prices.

To compute a quasi-experimental estimate of the semi-elasticity of the urban house price increase on the birth rate, we construct an instrumental variables specification based on Model (1). Namely, we use the post-period treatment-time interaction dummies from the house purchase restriction spillover treatment as instruments for the natural logarithm of house prices, and estimate the predictive effect of last year's (log) local urban house price on this year's birth rate.

3.4 Data and summary statistics

We combine a city-level and a micro-level analysis to study the response of fertility to the house price shock. We use several data sources to ensure our results are not driven by errors in one particular data source.

At the prefectural city-level, we obtain annual birth rates from each city's annual Statistical Communiqué on Economic and Social Development. The cities' Statistical Communiqué are readily available on each city's statistical bureau website, and are archived in electronic text format by aggregation platforms. We manually downloaded and scraped each city's Statistical Communiqué from 2009 to 2021. In rare cases when the birth rate was missing from Statistical Communiqué, we fill data from city statistical yearbooks, to make the birth rate dataset as complete as possible. We obtain city-level constant-quality urban house price indices from CityRE. We obtain our city-level control variables from the City Statistical Yearbook.

At the micro-level, we constructed individual- and household-level datasets from the China Family Panel Studies (CFPS), a biennial panel dataset that samples approximately 16,000 households across 25 provinces. Our analysis incorporated data from six waves: 2010 (initial), 2012, 2014, 2016, 2018, and 2020 (latest). We utilized the CFPS data in two distinct ways. Firstly, leveraging CFPS data on children's birth years, we reconstructed an annual record of newborns for each individual in the CFPS from 2009 to 2020. We cross-verified this annual record of newborns using information on the same individual from different survey waves and find it to be highly consistent across waves. Consistent with the economics literature on fertility, we focused on women aged 15 to 44, a demographic that accounts for over 99% of births in the country. We utilized the CFPS's biennial records for additional economic controls, assuming that the values of these controls apply to all years within the biennial wave period; for example, controls from the 2010 wave were assumed to apply to both 2009 and 2010 annual observations.

Secondly, we also utilized the survey's biennial records on marriage statuses at the time of survey, as well as reported marriage history, which we used to construct a biennial individual-level dataset on new marriages. Because we used information in the transition from a "single" marital status to a "married" status to infer new marriage, this biennual individual-level dataset on new marriages covers the second survey waves forward, corresponding to the years 2012, 2014, 2016, 2018, and 2020. We also employed data from the CFPS on parents' private educational expenditures on their children, from which we constructed a biennial household-level dataset that covers the years 2010, 2012, 2014, 2016, 2018, and 2020.

Table 1 provides summary statistics of the analysis samples we use. The sample covers the unregulated cities that made up our treated group and control group of observations, and do not include observations in the regulated cities. We discuss some key observations from Table 1. The average birth rate in the sample is 10.72‰, and the average number of newborns to women of childbearing age (15-44) is 0.06. This translates to a total fertility rate of 1.80, which is below the replacement level. The share of urban inhabitants is 31% and the share of rural inhabitants is 69%. This reflects a modest level of urbanization in the unregulated cities during the sample period. The homeownership rate is 91%. Most of the urban inhabitants own their home, as do most of the rural inhabitants, but the rural owned dwellings cannot be traded. The multiple home ownership rate is 16%. In addition to some urban residents owning multiple urban properties, a portion of rural residents also own an urban home in addition to their rural dwelling.

[**Table 1 about here**]

4. The Effect of the House Price Shock on Fertility

This section reports quasi-experimental estimates of the house price shock, driven by the house purchase restriction spillover treatment, on treated cities' birth rates, and on fertility of treated cities' women of childbearing age.

4.1 City-level birth rate responses

We first use the Statistical Communiqué data on city-level birth rates and the CityRE urban house price indices to estimate regression model (1). In addition to controlling for pre-existing trends, city fixed effects, and year fixed effects, we further control for the time-varying variables of log per capita fiscal expenditure, log per capita fiscal income, log population and log per capita GDP. Table 2 presents our findings, with odd-numbered columns adjusting for city-specific pre-existing trends and even-numbered columns accounting for treatment group-specific pre-existing trends. The two sets of results are quantitatively similar. Our preferred specifications are the even columns, controlling for treatment group-specific trends, following Bilinski and Hatfield (2019).

[Table 2 about here]

Urban house prices in treated cities abnormally increased by an average of 12.4% in the four years following the late 2016 house purchase restriction spillover shock, relative to control cities, as detailed in column (4) of Table 2. This estimation aligns with the patterns depicted in Figure 1 , which tracks pre-existing trends and dynamic responses in house prices. Specifically, the 12.4% increase is the average deviation of the house price event study coefficients for the four full years 2017 through 2020 after the house purchase restriction spillover shock in 2016, from the trend line established by pre-period data up to 2015. We see that house prices in treated cities, i.e. unregulated cities within 250 km of the nearest regulated city, swiftly surged away from control cities in after the house purchase restrictions were imposed in the regulated cities. The house price surge in treated cities relative to the control cities stabilized in 2018 and 2019, when the abnormal house price increase was approximately 16%, and adjusted slightly downwards in 2020. This downshift could be due to some treated cities imposing their own purchase restrictions in the third post-treatment year, curbing abnormal demand and possibly triggering further spillovers to control cities.

City-level birth rates in treated cities abnormally and significantly declined by an average of 1.68‰ compared to the control cities in the four years 2018 through 2021, the first full years after the house purchase restriction spillover shock and accounting for a pregnancy period delay, as detailed in column (2) of Table 2. Considering the average city-level birth rate was 10.72‰ during the sample period, the induced birth rate decline due to the house price shock is economically significant.

[Figure 2 about here]

Figure 2 visually depicts the abnormal decline in the birth rate of treated cities, which slowed down at about a 1.3‰ reduction in the third post-treatment year. Positive pre-trends in both birth rates and house prices were observed in treated compared to control cities, suggesting a potential reverse causality scenario where, absent the quasi-experimental house price shock, an upward trend in births could elevate house prices. Such pre-trend dynamics underscore the necessity for an exogenous house price shock to reliably estimate its impact on fertility. The abnormally low city-level birth rate in the treated cities was significantly different from trend in each of the four post treatment years.

The quasi-experimental estimate of the semi-elasticity of city-level birth rate with respect to urban house prices is -8.7‰, statistically significant at the 1% level, as reported in column (6) of Table 2. This semi-elasticity implies that an exogenous 10% rise in urban house prices is expected to cause a birth rate decrease of 0.87‰.

4.2 Individual-level fertility responses

We use the data on newborns to women of childbearing age to examine the effect of exogenous house price increase on fertility at the micro-level, corroborating our city-level findings. We applied regression model (1) to annual birth records spanning 2009-2020, reconstructed from the biennial CFPS dataset. In additional to controlling for individual fixed effects, year fixed effects, and potential pre-existing trend differences, we also control for time-varying individual and family characteristics such as age, age squared, marital status, party membership, urban residence, health score, housing tenure, family income, and mortgage debts. Table 3 reports the results.

[**Table 3 about here**]

The individual-level treatment effect qualitatively confirms the city-level treatment effect. After the house purchase restriction spillover shock, the average number of newborns to each women of childbearing age in the treated cities abnormally reduces by 0.027. This abnormal reduction in newborns after the positive house price shock is statistically significant at the 1% level.

[**Figure 3 about here**]

Figure 3 reports the event study coefficients that show an abnormal reduction in the treated cities' individual-level newborns in the post-treatment period. After the house purchase restriction spillover shock, the event-study coefficients in the number of newborns in the individual-level data were below the 95% confidence bands of the pre-existing trends in the post-treatment years of 2018, 2019, and 2020. This is consistent with the abnormal decrease in the individual-level fertility documented in Table 3.

4.3 Robustness checks

The results in Table 2 and Table 3 indicate that our results are robust whether we control for city-specific pre-existing trends or treatment group-specific pre-existing trends, and whether we control for time-varying city-level, individual-level, and family-level characteristics. We further assess the robustness of our results to (1) alternative distance cutoffs for designating the treatment status, and (2) using a continuous distance specification where we allow treatment effects to linearly decay with distance to the nearest regulated city.

First, we designate a unregulated city as a treated city if it is closer than 200 km (300 km) to the nearest regulated city. Panels (a) and (b) of Table 4 report the respective results. The estimates are quantitatively similar. They point to the same robust finding, that after the imposition of house purchase restrictions in the regulated city, the nearby unregulated cities saw urban house prices abnormally increase, birth rate abnormally decrease, and individual-level newborns abnormally decrease.

Second, we estimate the following modification to regression model (1):

$$Y_{i,t} = \phi \times \log(Distance_i) \times Post_t + \Gamma X_{i,t} + \gamma(i)t + \alpha_i + \delta_t + \epsilon_{i,t}. \tag{2}$$

Instead of using $Treat_i$ which is binary, we designate treatment status using $\log(Distance_i)$, which is continuous. The assumption is the longer the distance to the nearest regulated city, the weaker the external demand shock from the imposition of house purchase restriction spillovers. Hence, we expect a negative continuous treatment effect ϕ for urban house prices in unregulated cities, and a positive continuous treatment effect ϕ for birth rates and the number of newborns in unregulated cities.

[Table 4 about here]

[Figure 5 about here]

That is exactly what we find, as detailed in panel (c) of Table 4, and graphically depicted in Figure 5. The longer is the distance to the nearest regulated city that imposed house purchase restrictions, the abnormal increase in house prices will be smaller, as indicated by the negative ϕs in columns 1–2. Graphically, there is a linear decay in abnormal price increases from the highest response in nearest cities. The abnormal decrease in birth rates and in the number of newborns are also smaller with longer distance, as indicated by the positive ϕs in columns 3–6. Graphically, there is a linear dampening in abnormal fertility reduction from the strongest reduction in nearest cities. These robustness results improve our confidence that the baseline findings indicate a negative fertility effect of exogenous house price increases following the house purchase restriction spillovers.

5. Mechanisms

We next assess whether cost for living space or other costs urban house price represents, such as access to education, account for the fertility effects. Also, we examine whether the reduction occurred primarily in married couples, or there is also an effect on marriage itself, and whether the marriage margins of house prices' fertility effects associates with local sexual imbalance. Finally, from the perspective of human capital formation, we explore whether there is an intensive margin response in parents' expenditure on children's education that accompanies the extensive margin fertility decline.

5.1 Rural aspirations for urban homeownership and the fertility decline

The spillover from house purchase restrictions induced a significant uptick in urban house prices in treated cities. Institutional factors differentiate urban from rural homes: urban properties are title-holding and tradable, whereas rural dwellings, allocated by village collectives, cannot be sold or bought. This distinction means that urban and rural housing markets react differently to urban house price shocks. Moreover, as discussed in Section 2, urban home ownership is tightly interwoven with access to urban educational resources. Consequently, rural residents—and urban non-owners—may aspire to acquire urban homes to gain access to better schooling, suggesting potential variance in fertility responses to house price shocks based on urban home ownership.

We reanalyze our regression model (1) across four subsets of the individual-level sample: (1) those without ownership in either rural or urban homes—essentially mobile or unattached to the housing market, (2) rural inhabitants with rural dwellings but no urban property (rural non-urban-owners), (3) rural inhabitants with both rural dwellings and urban property, and (4) urban residents with urban home ownership. Table 5 summarizes their respective fertility responses as measured by the average annual number of newborns.

[**Table 5 about here**]

Column (1) of Table 5 indicates that the effect on newborn numbers among the mobile subsample is negative, albeit not statistically significant (-0.020 with an s.e. of 0.045). Crucially, column (2) demonstrates a statistically and economically meaningful decline in newborns among rural residents without urban property (-0.039 with an s.e. of 0.012). The fertility response of rural owners of urban homes is positive (0.011 with an s.e. of 0.054), and the fertility response of urban homeowners is negative but close to zero (-0.004 with an s.e. of 0.018), both statistically non-significant. These findings align with the patterns in panels (a) and (b) of Figure 6, which suggest rural non-urban-owners' fertility event study coefficients as consistently below the 95% confidence bands of the pre-existing trend across the post-treatment years of 2018, 2019, and 2020, while urban homeowners' fertility event study coefficients do not show this pattern. The fact that we find a significant reduction in fertility responses among rural dwelling owners with no urban homeownership (column 2) but essentially a nil effect among urban owners (column 4), suggests that the role of housing as shelter does not fully explain the fertility reduction in face of the abnormal house price increase we observe.

[**Figure 6 about here**]

5.2 The distance to rural schools and the fertility decline

We further assess whether the rural concentration of the negative effect of urban house price on fertility is associated with the scarcity of rural schools. The CFPS questionnaire asked student subjects (or their parents) how long a distance it takes from home to school. We proxy for spatial scarcity of schools, by calculating the county-level rural/urban-specific average distance from home to elementary school in the last pre-treatment year. We use individuals in counties with a higher distance than the national average as a proxy for individuals facing spatial scarcity of schools, and estimate the rural/urban gap in fertility treatment effects by subsamples of this dichotomy.

Ding, Wang, and Ye (2016) studied local government's incentives in rural school closures and consolidation, which has resulted in "some students facing longer distances to school and increased risks in traffic safety, a heavier financial burden on students' families, a shortage of boarding schools in rural areas, and overcrowded classes in some urban schools." They suggested two incentives that are (1) reducing education expenditure and facilitating educational management, and (2) encouraging the concentration of rural populations into urban areas, thereby promoting urbanization.

[**Table 6 about here**]

Table 6 reports the results by the local spatial scarcity of schools, where we focus on rural dwelling owners with no urban homeownership. We find that, among them, the fertility reduction is statistically and economically more significant in counties where rural schools are more distant. Whether the county has relatively distant schools or not is designated by the median distance from home to local schools, reported by rural dwelling owners with no urban homeownership. To ensure this designation is relevant, we further restrict the regression sample to those who stayed in the same county during our sample period. If the county median home-school distance is larger than national rural/urban-specific median in 2016, the survey wave just before the house price shock, this area is designated as "Schools Distant", and vice versa. We find that for the "School Distant" group of rural dwelling owners with no urban homeownership, their fertility reduction (-0.061 with a s.e. of 0.020) is statistically different (with a two-sided p-value of 0.092) from urban homeowners' fertility response, whereas for the "School Near" group, their fertility reduction is smaller in point estimate (-0.029 with a s.e. of 0.022) and corresponds to a smaller gap from urban homeowners. These results are consistent with the idea that one factor driving the rural individuals' fertility reduction response to an positive urban house price shock is the gap in education resources.

5.3 The competitive marriage market and the fertility decline

Wei and Zhang (2011) and Wei, Zhang, and Liu (2012) studied the role of homeownership in enhancing prospects in the marriage market, especially for men. Although we study an individual-level sample containing women of childbearing age, an increased urban house price would reduce the share of local males that could afford urban homeownership as a signal for their wealth and marriage eligibility, and would increase marriage frictions for women as well. We therefore expect the urban house price shock to reduce the rate of new marriages, thereby contributing to the fertility reduction. We therefore conduct a test to estimate the treatment effect of the urban house price shock on new marriage in the individual sample. We use the same women of childbearing age individual sample to be consistent with our other analysis. If we find treatment effect on local women's marriage rate, we do not expect the treatment effect on local males' marriage rate to be qualitatively different.

[**Table 7 about here**]

Table 7 reports results from this test on the marriage rate, using the biennial dataset on new marriages we constructed from the CFPS survey. Panel (a) estimate the average treatment effect. After the house purchase restriction spillover shock, the likelihood of new marriage for treated cities' individuals abnormally reduced by 0.038, significant at the 1% level. Figure 6 shows the event study for marriage rate, which shows a negative treatment response after the house purchase restriction spillover shock.

Panel (b) of Table 7 reports the heterogeneous treatment effects by housing tenure status in rural and urban areas. We find that rural dwelling owners with no urban homeownership is the only group to have a significant marriage rate decline (-0.028 with a s.e. of 0.016). In contrast, rural inhabitants who own urban homes have the largest positive point estimate (0.030 with a s.e. of 0.061), albeit statistically insignificant. For urban homeowners, we observe a negative coefficient close to zero (-0.002 with a s.e. of 0.033) that is also insignificant. These results confirm that the marriage margin of the treatment response to the abnormal house price increase also concentrates among rural dwelling owners with no urban homeownership.

[**Figure 7 about here**]

In addition, we also want to assess whether already married individuals also reduce their number of newborns. A priori, the education mechanism we previous discussed reduces the return to having children, which would affect both the number of newborns conditional on marriage as well as the marriage rate. We therefore conduct another test to assess whether we find treatment effect of the urban house price shock within the married sample. Table 8 reports results from conditioning the baseline fertility treatment effect tests on the married sample. We find an abnormal average fertility decline in treated cities within married individuals, as indicated by Panel (a). While the average treatment effect across rural and urban people is borderline insignificant, we find that the fertility decline in married individuals is statistically significant among rural dwelling owners with no urban homeownership, as reported in column (2), Panel (b) of Table 8. Indeed, the patterns are strikingly consistent across the baseline fertility treatment effects, the marriage rate treatment effects, and the fertility treatment effects among married individuals. These results are consistent with the idea that the house price shock reduced fertility through both (1) reducing new marriages and (2) reducing the number of newborns to married couples, especially among rural dwelling owners with no urban homeownership.

[**Table 8 about here**]

Moreover, we assess whether local variations in sexual imbalance of boys and girls among birth cohorts is associated with a stronger fertility reduction after the house price shock. We construct the local sex ratio measure as follows. We focus on the cohorts born in 1981-2000.

They constitute all the individuals above 20 and under 35 during the post period of the house price shock. 20 is the legal marriage age, and we find the effects on fertility and new marriage to concentrate in the group under 35 in Section 6.1. We use the 2000 Census Regional Statistics for the local number of boys and girls born in the cohorts 1991-2000, and the 1990 Census 1% Microdata to estimate the local number of boys and girls born in the cohorts 1981-1990. This way, we compute a local sex ratio measure for each prefecture. We split the sample of rural dwelling owners with no urban homeownership by whether the local sex ratio measure is above or below the median and separately estimate the fertility and marriage effects. Table 9 reports the results.

Indeed, prefectures with a higher local sex ratio are indeed associated with a stronger fertility reduction (columns 1 and 2). This gives the first indication that local variations in sexual imbalance of boys and girls among birth cohorts is an important mechanism for the result we find.

Further, for rural men, the competitive savings motive predict stronger marriage market competition should associate with a stronger reduction in marriage, because exogenously higher price of urban homeownership will reduce the share of rural men that can afford to it to signal and ensure marriage chances. Prefectures with a higher local sex ratio indeed display a sizable negative change in the rate of new marriage among rural men (columns 5 and 6). A priori, we do not expect stronger or weaker marriage market competition among men that the higher local sex ratio proxies for to affect rural women's marriage responses, and we find this to be the case among rural women (columns 3 and 4).

Prefectures with a higher local sex ratio also display a sizeable fertility reduction among the rural married women (columns 7 and 8). One possibility is that under more intense competition among men, women are in a position to increase bargaining power within the family, and the pronounced fertility reduction among married women in the high local sex ratio prefectures reflect its outcome. We interpret all these patterns as consistent with the mechanisms in Wei and Zhang (2011). They suggest that the competitive savings motive is a key mechanism for the fertility decline after the house price shock.

[**Table 9 about here**]

5.4 Effects on private educational investments

One view of fertility decline is that it does not necessarily lead to lower human capital formation (e.g. Barro and Becker, 1989; Becker, Murphy, and Tamura, 1990; Galor and Weil, 2000). Instead, quality investment may offset or even dominate the effect of fertility decline. We are interested in whether this happens. Ideally, one would assess the long-term quality investment into children, however this is infeasible given the recentness of our setting. Instead, our microdata provide information on parent's educational investment on children. Using this information, we estimate the treatment effect of the house price shock on parent's educational investment on children, which we consider as a measure of the intensive margin quality investment in the short term.

We use information from the 2010, 2012, 2014, 2016, 2018, 2020 biennial waves of CFPS and the household-level questionnaire. The questionnaire inquires about spending on children's education such as tuition, books, learning equipment, tutoring expenses for children younger than

14. We aggregate the household's expenditures as the household's educational investment on children.

[Table 10 about here]

Table 10 reports the treatment effect results on private educational investments. We observe a significant increase in parents' investment on children's education among rural dwelling owners with no urban homeownership (0.582 with a s.e. of 0.153). Consistent with this regression result, Figure 7 displays the event study coefficients for the treatment responses in educational investments for rural dwelling owners with no urban homeownership after the abnormal house price increase, and suggest a positive departure from pre-existing trends after the house price shock. We observe an increase in educational investments similar in size for the mobile population with neither rural owned dwellings nor urban homeownership, albeit statistically insignificant. These increases in educational investments are consistent with the idea that simultaneous to fertility declines in response to the urban house price shock, private educational investments increased as a strategic adaptation to limited resources and opportunities, possibly offsetting the negative impact on human capital formation.

[Figure 7 about here]

For rural inhabitants who own urban homes in addition to their rural dwellings, we find a sizable decrease in educational investments, and we find a smaller decrease in educational investments among urban homeowners. Both reductions are statistically insignificant. They are consistent with the possibility that housing investment serves as a potential substitute for human capital investment, so that after an unexpected positive shock in urban house prices, parents who have invested in urban homes may have reduced incentive to reduce human capital investments. Together, the pattern in the educational investment responses underscores the close relationships among educational opportunities, homeownership costs, and fertility choices, emphasizing the significant impact of inequality and the local educational environment on human capital formation (e.g. De La Croix and Doepke, 2003; Chetty et al., 2014; Heckman and Landersø, 2022).

6. Discussions

This section discusses (1) alternatives that may explain the fertility reduction in treated cities (especially among rural inhabitants) we observe, and how we empirically assess these alternatives, and (2) the aggregate implications of our quasi-experimental estimates.

We consider three major alternative explanations—age composition, local migration, and the relaxation of the one-child policy. The threat is that these alternatives, and not an exogenous house price increase, explains the fertility treatment effects. We describe each alternative in detail and test their explanatory power empirically.

6.1 Alternative Explanation 1: Age composition

A possible alternative explanation for the observed decline in fertility within the rural areas of treated cities could relate to differences in age composition between rural and urban areas. It is conceivable that a demographic shift has occurred in rural areas due to the migration of younger populations to urban centers. Such migration would naturally lead to a decline in the fertility rate in rural areas as the remaining population ages.

However, it is unlikely that this factor alone explains our results because we controlled for age and age squared in all individual-level regressions, and accounted for pre-existing trends in all our analyses. Therefore, any significant shift in age composition would have to coincide precisely with the timing of the treatment, and be particularly pronounced in the treated cities. In that case, if the variations in fertility were solely attributable to differences in age composition, we would expect to see no significant treatment effects within homogeneous age groups.

Motivated by this hypothesis, we assessed the treatment effects within specific age groups. Table 11 presents these results.

[**Table 11 about here**]

Panels (a) and (b) of Table 11 detail the treatment effects on the number of newborns and the likelihood of new marriages, respectively, segmented by age group. We observed a significant treatment effect particularly in younger demographics. The negative impact on both the number of newborns and new marriages was statistically significant specifically within the 20-29 age group. Furthermore, when categorizing women of childbearing age into two groups—below advanced maternal age (under 35) and advanced maternal age (35 and over)—the significant negative treatment effects were confined to the group under 35.

This pattern indicates that it is predominantly the younger individuals remaining in the rural areas of treated cities who are driving our results. Consequently, these findings lend no support to the age composition hypothesis as an explanation for the observed fertility decline.

6.2 Alternative Explanation 2: Local migration

Possibly, treated cities had more local rural-to-urban migration during the post-2016 period, which resulted in more fertile-age women moving to urban areas. Our hypothesis is in a sense opposite, that exogenous spike in house prices may have made urbanization in the treated cities more challenging. Hence, this alternation explanation is also unlikely to account for our findings. Nevertheless, we assess this alternative by redefining the "treatment" status based on the 2016-2021 change in urbanization rates and then conducted the main Difference-in-Differences (DID) analysis using the same post-treatment timing. The results are presented in Table 12.

[**Table 12 about here**]

The outcomes of these alternative treatment tests were either insignificant or pointed in the wrong direction. For instance, we observed that cities with a higher increase in urbanization rates from 2016 to 2021 exhibited a marginally significant rise in birth rates. Conversely, cities that saw a higher increase (or a lesser reduction) in the primary sector employment share during the same period showed an insignificantly lower birth rate. In other words, we found no evidence to suggest that a diminished agricultural presence reduces birth rates. Instead of local migration to urban areas reducing fertility, these patterns are consistent with individuals left behind in rural areas reducing fertility. Consequently, these findings lend no support to the local migration hypothesis as an explanation for the observed fertility decline.

6.3 Alternative Explanation 3: Relaxation and abolishing of the one-child policy (OCP) through 2013 to 2015

Another alternative explanation is that the fertility decline was possibly a falling back from the heightened births after the relaxation and abolishing of the OCP. In 2013, the government allowed couples in which at least one person is a single child to have two children. In 2015, the government allowed all couples to have two children. Suppose in response to the OCP relaxation, treated cities had a larger fertility increase before the HPR spillover shock. Then, a falling back in fertility may generate our result. Because the OCP was relaxed in all cities (policy, treated, and control), this is a priori unlikely.

Nevertheless, we address this alternative by examining whether in response to OCP relaxation, treated cities had larger fertility increase before the house purchase restriction spillover shock. We implement this by interacting the house purchase restriction spillover treatment dummies "ahead of time" with a post dummy that equals 1 for years after 2013, the first year the OCP was starting to be relaxed. We use a post period of 2014 to 2017, which covers four years since the OCP was relaxed and two years since the OCP was abolished and does not overlap with the post period of the house purchase restriction spillover treatment on house prices.

[Table 13 about here]

The results are reported in Table 13. They show no significant effects associated with this placebo OCP treatment timing, neither in city-level birth rates nor in the individual-level fertility data. In three of the four specifications, the point estimates suggest that treated cities exhibited a insignificantly lower birth rate response to the OCP relaxation prior to our house price shock. Hence there was no differentially heightened response to OCP relaxation in treated cities to fall back from. Consequently, these findings suggest that the relaxation and abolishing of the one-child policy does not explain our estimated negative treatment effect of the house price shock on fertility.

6.4 Aggregate implications of quasi-experimental fertility effect estimates

Lastly, it is crucial to consider the potential aggregate implications of our quasi-experimental estimates on the negative fertility effects. Researchers such as Guren et al. (2021a) have highlighted the complexities involved in interpreting the aggregate implications of quasi-experimental estimates. Chodorow-Reich (2019) and Chodorow-Reich (2020) advised on bounding the aggregate implications using a potential outcome framework. Our primary identification assumption posits that, in the absence of the house purchase restriction spillover treatment, urban house prices and fertility outcomes in both treated and control cities would have continued along their pre-existing trends.

Figure 4(a) graphically displays the results of a continuous distance specification that estimates how the abnormal increases in urban house prices in the non-regulated cities linearly decays with the log distance to the nearest regulated city. We constructed this figure as follow. We first estimate trend deviations in log urban house prices in each non-regulated city after the shock using a time-series regression for each non-regulated city. We then measured the linear decay of this abnormal increase with respect to log distance from the nearest regulated city. This process allows us to contrast the observed abnormal price increases against a hypothetical scenario where no treatment effect exists.

Notably, cities within 551 km of a regulated city displayed abnormally high house prices, decreasing log-linearly at a rate of -0.067 per log increase in distance during the post-treatment period. By calculating the average height under the log-linear line from the closest unregulated cities to the point where it crosses the horizontal axis, we estimate that the house purchase restriction shock led to an average urban house price increase of 8.4% over the four post-treatment years in these cities. Applying the estimated semi-elasticity of -8.76‰, this corresponds to an average birth rate reduction of 0.73‰ across these affected cities.

With these cities having a combined population of 840 million in 2016, this equates to an estimated shortfall of approximately 2.46 million newborns over four years. By contrast, the national birth rate during the four-year post-treatment period dropped by an average of 4.24‰, from a rate of 13.57‰ in 2016 to an average rate of 9.33‰ from 2018 to 2021. With a 2016 population of 1392 million, this translates into an overall shortfall of about 23.6 million newborns.

Therefore, the house purchase restriction spillover shock may account for approximately 10.4% of the aggregate birth decline—through its impact on urban house prices in unregulated cities—as suggested by this back-of-the-envelope calculation. Moreover, given that the national average urban house price index rose by 53.7% during the post-treatment period, if any 1% increase in national urban house prices stemmed from an aggregate investment demand shock, it would contribute an additional 2.06% to the aggregate birth decline, calculated as $1\% \times \frac{8.76‰}{4.24‰}$.

7. Conclusions

By leveraging spillovers from the imposition of house purchase restrictions in large cities, which redirected investment demand to nearby unregulated cities and exogenously increased their local house prices, we estimated the causal effect of house prices on fertility. We find the investment demand driven increase in urban house prices significantly reduced city-level birth rates, the number of newborns at the individual level, and marriage rates.

This impact was particularly pronounced among rural residents who solely own rural homes, likely due to their aspirations to acquire urban properties—a goal closely linked to marriage prospects and access to quality urban education for their children, which were compromised by rising prices. We also observed a positive treatment effect on private education investments, suggesting a strategic adaptation. The aggregate fertility effect size indicated by our quasi-experimental estimate is substantial.

Chetty et al. (2014) indicates that in the United States, a child's prospects for upward mobility are greatly influenced by relocating to the right areas and are negatively impacted by residential segregation. Heckman and Landersø (2022) shows that family residential decisions are typically made early in children's lives, often before their birth, citing Danish data. Our study highlights how surges in urban house prices can significantly influence family residential choices, affecting marriage and childbearing dynamics, especially for rural residents. As urban house prices rise, exploring how the remaining rural population decides between staying in declining rural areas or overcoming barriers to urbanization—and how these decisions affect individual economic behaviors and broader economic outcomes—is an essential area for future research.

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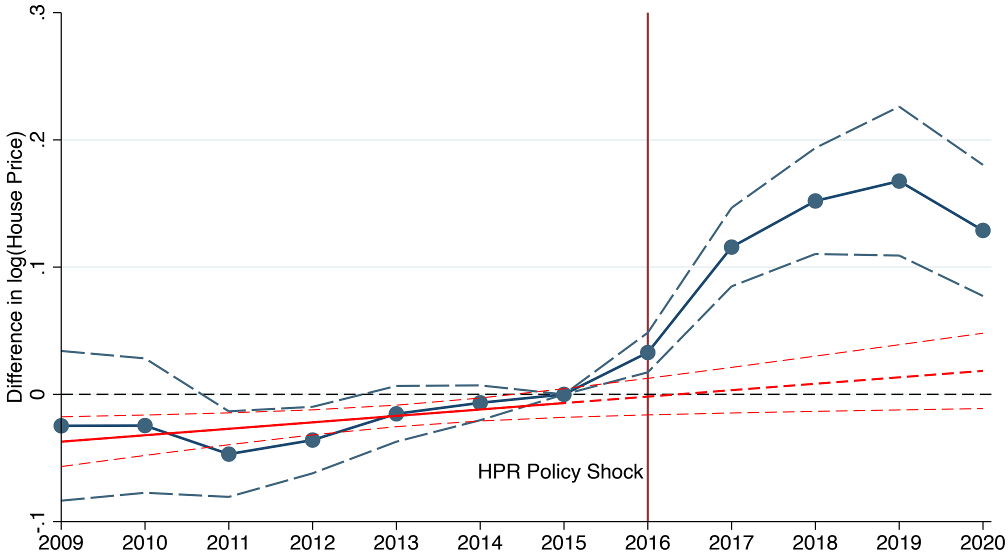
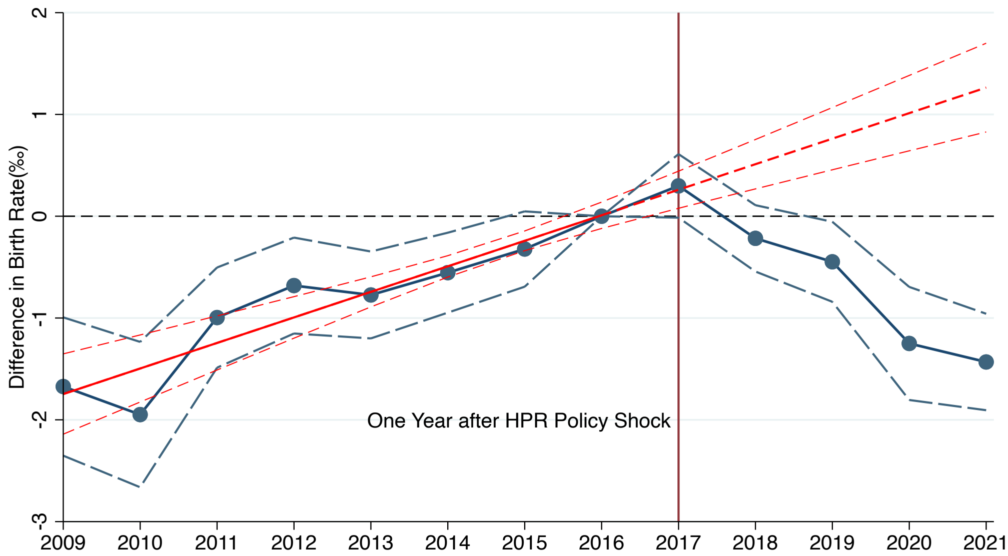
Figure 1: Preexisting Trends and Dynamic Responses: House Prices



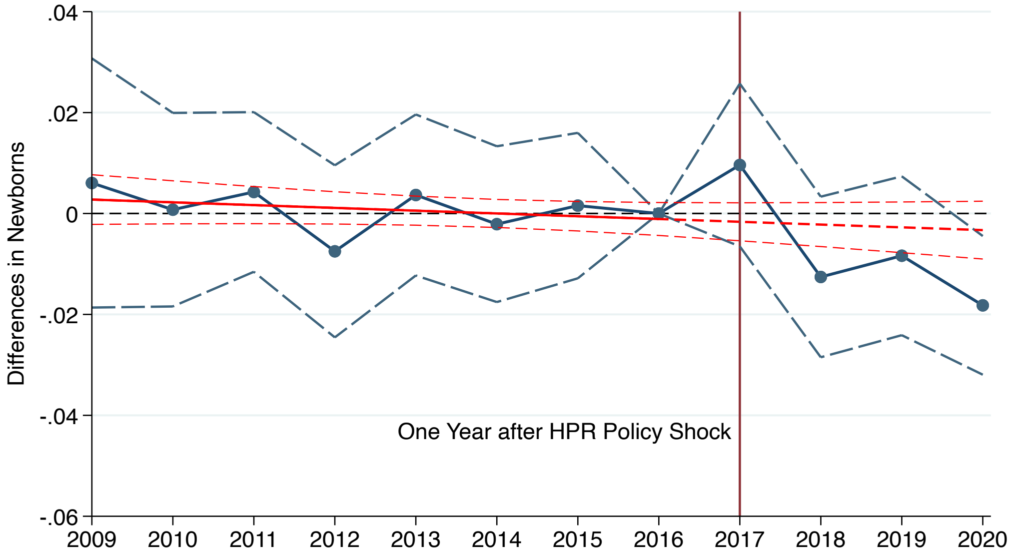
Notes: This figure plots the estimated response of house prices in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. The response is relative to the level of response in 2015. City fixed effects and time (year) fixed effects are included. Time-varying city-level control variables include log per capita local fiscal expenditure, log average wage income, log local population and local per capita GDP growth. 95% confidence intervals are drawn based on standard errors clustered at the city level. The dependent variable is log(House Price). The beginning of the two rounds of house purchase restrictions in the regulated cities is labeled by the vertical red line. Red upward sloping line is the pre-treatment tread of the relative responses and the trends' 95% confidence interval , based on linear regression of the estimated responses on time. The house price data is from 2009 to 2020.

Figure 2: Preexisting Trends and Dynamic Responses: Birth Rate



Notes: This figure plots the estimated response of city-level birth rate in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. The response is relative to the level of response in 2016. City fixed effects and time (year) fixed effects are included. Time-varying city-level control variables include log per capita local fiscal expenditure, log average wage income, log local population and local per capita GDP growth, all lagged one year. 95% confidence intervals are drawn based on standard errors clustered at the city level. One year after the house purchase restrictions in the regulated cities is labeled by the vertical red line to take into account of the pregnancy delay. Red upward sloping line is the pre-treatment tread of the relative responses and the trends' 95% confidence interval , based on linear regression of the estimated responses on time. The city-level birth rate data is from 2009 to 2021.

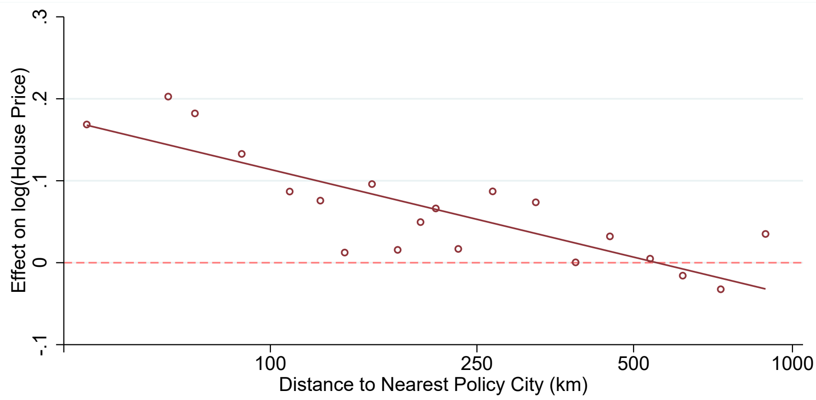
Figure 3: Preexisting Trends and Dynamic Responses: Newborns (Individual-level)



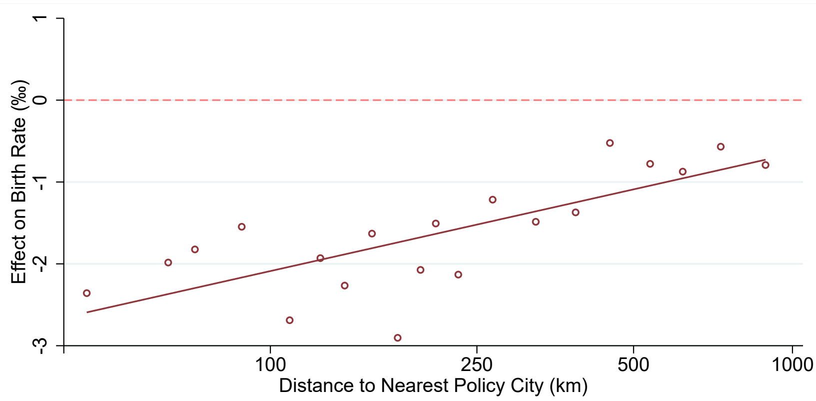
Notes: This figure plots the estimated response of number of newborns of each individual in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. The response is relative to the level of response in 2016. City fixed effects and time (year) fixed effects are added. The individual control variables are age, age^2, education level, marital status, marital status$\times$spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. 95% confidence intervals are drawn based on standard errors clustered at the city level. One year after the house purchase restrictions in the regulated cities is labeled by the vertical red line to take into account of the pregnancy delay. Red upward sloping line is the pre-treatment tread of the relative responses and the trends' 95% confidence interval , based on linear regression of the estimated responses on time. Data on the number of newborns is from 2009 to 2020.

Figure 4: Spillover Effects of House Purchase Restrictions on House Prices, City-level Birth Rate and Individual-level Birth Rate

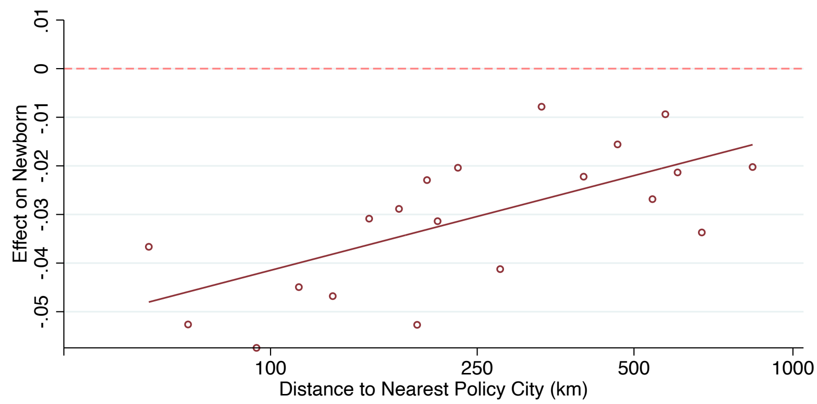
(a) Spillover Effects of House Purchase Restrictions: log(House Price)



(b) Spillover Effects of House Purchase Restrictions: Cities' birth rate



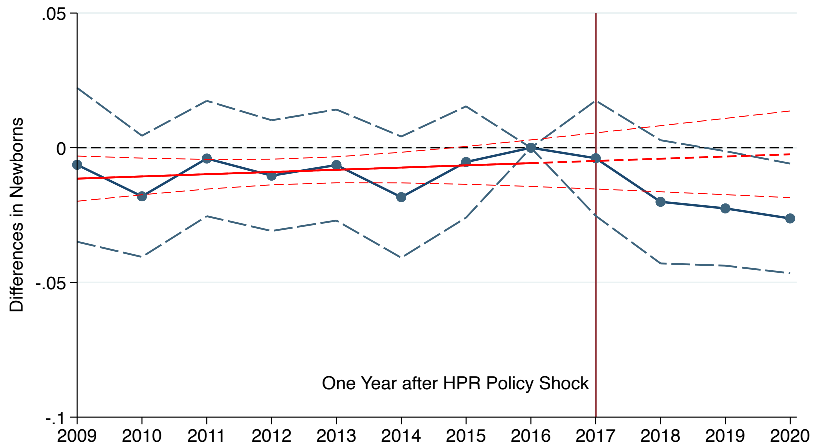
(c) Spillover Effects of House Purchase Restrictions: Fertility, Women of Childbearing Age



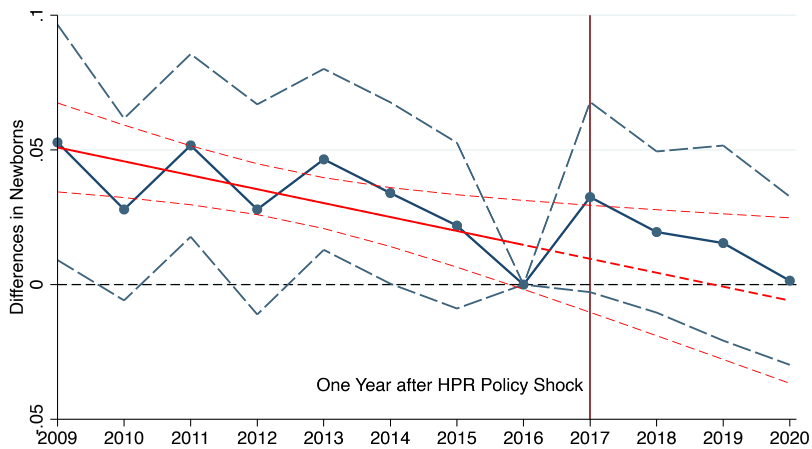
Notes: These figures plot the spillover effects of house purchase restrictions on the unregulated city as the distance from the nearest regulated city varies. The spillover effect on each city is defined as deviations in the variable of interest in post-shock periods (2016 for house price and 2017 for birth rate and number of newborns) from city specific trend estimated using pre-shock period data. Panel (a) plots the spillover effect on log house price. Panel (b) plots the spillover effect on cities' birth rate. Panel (c) plots the spillover effect on number of newborns of each individual.

Figure 5: Preexisting Trends and Dynamic Responses: Rural Dwelling Owners with No Urban Homeownership and Urban Homeowners

(a) Preexisting Trends and Dynamic Responses for Rural Dwelling Owners with No Urban Homeownership

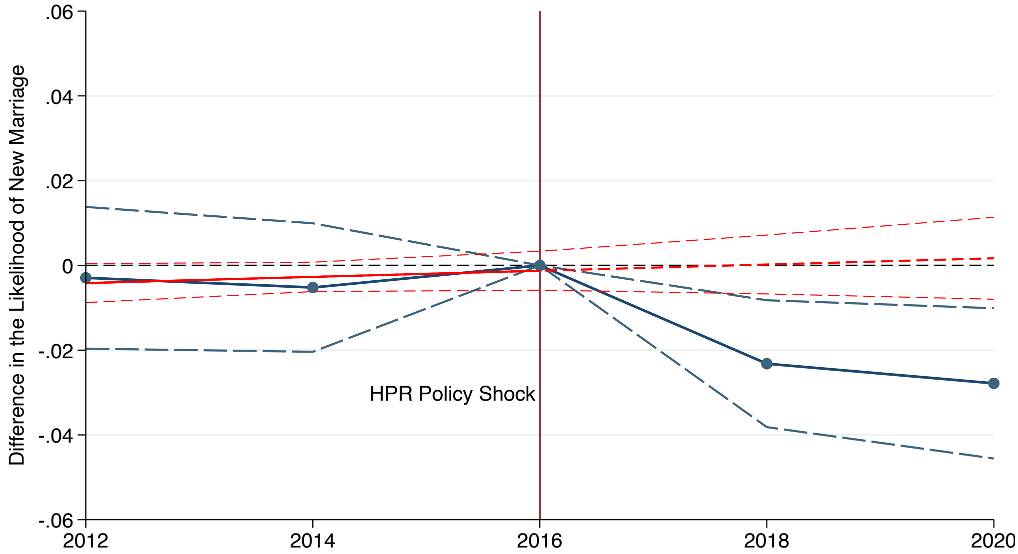


(b) Preexisting Trends and Dynamic Responses for Urban Homeowner



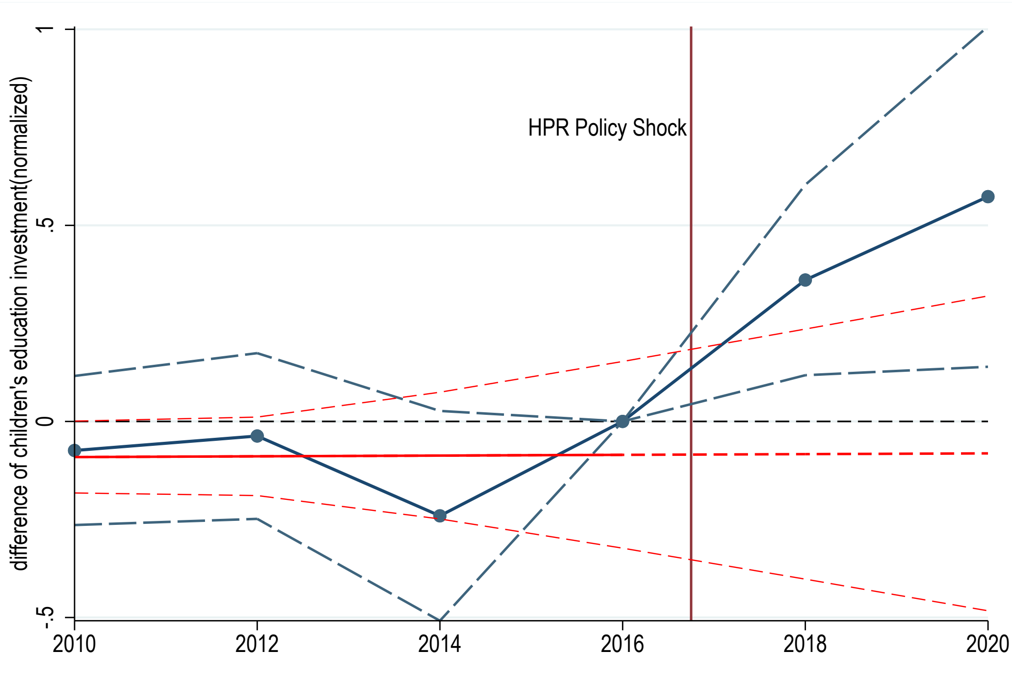
Notes: This figure plots the estimated heterogeneous response of individual-level number of newborns of (a) rural dwelling owners with no urban homeownership and (b) urban homeowners in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. The response is relative to the level of response in 2016. City fixed effects and time (year) fixed effects are added. The individual control variables are age, age^2, education level, marital status, marital status$\times$spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. 95% confidence intervals are drawn based on standard errors clustered at the city level. One year after the house purchase restrictions in the regulated cities is labeled by the vertical red line to take into account of the pregnancy delay. The sloped red solid line is the pre-treatment tread of the relative responses and the trends' 95% confidence interval , based on linear regression of the estimated responses on time. Data on the number of newborns is from 2009 to 2020.

Figure 6: Preexisting Trends and Dynamic Responses in New Marriages (Individual-level)



Notes: This figure plots the estimated response of number of new marry of each individual in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. The response is relative to the level of response in 2016. City fixed effects and time (year) fixed effects are added. The individual control variables are age, age^2, education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. 95% confidence intervals are drawn based on standard errors clustered at the city level. The house purchase restrictions in the regulated cities is labeled by the vertical red line. The sloped red solid line is the pre-treatment tread of the relative responses and the trends' 95% confidence interval, based on linear regression of the estimated responses on time. Data on new marry is from 2009 to 2020.

Figure 7: Preexisting Trends and Dynamic Responses in Parents' Investment on Children's Education, Among Rural Dwelling Owners with No Urban Homeownership



Notes: This figure plots the estimated response of educational investments of each household in treated cities relative to control cities, both before and after the house purchase restrictions, among rural dwelling owners with no urban homeownership. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. The response is relative to the level of response in 2016. City fixed effects and time (year) fixed effects are added. The family control variables are urban residence, housing tenure, log per capita family net income, log total asset, and migratory status. 95% confidence intervals are drawn based on standard errors clustered at the city level. The house purchase restrictions in the regulated cities is labeled by the vertical red line. The sloped red solid line is the pre-treatment tread of the relative responses and the trends' 95% confidence interval , based on linear regression of the estimated responses on time. Data on educational investments is from 2010 to 2020 each two years.

Table 1: Summary Statistics

	Count	Mean	Std. Dev.	10th	50th	90th
City-level data (annual frequency)						
Treat	2589	0.61	0.49	0	1	1
Birth rate (‰)	2589	10.72	2.94	6.94	10.73	14.24
Log(CityRE house price)	2589	8.54	0.42	8.08	8.48	9.11
Log(Per capita fiscal expenditure)	2589	8.87	0.50	8.18	8.91	9.45
Log(Average wage)	2589	10.79	0.36	10.29	10.81	11.25
Log(Population)	2589	15.04	0.62	14.19	15.07	15.80
Log(Per capita GDP growth)	2589	0.09	0.13	0.01	0.09	0.19
Individual-level fertility data (annual frequency)						
Treat	79681	0.60	0.49	0	1	1
Number of newborns	79681	0.06	0.24	0	0	0
Age	79681	30.27	8.47	18	30	42
Education level	79681	2.24	1.38	0	2	4
Marital status	79681	0.67	0.47	0	1	1
Ethnic minority	79681	0.14	0.35	0	0	1
Party membership	79681	0.05	0.21	0	0	0
Urban residence	79681	0.31	0.46	0	0	1
Migratory status	79681	0.48	0.50	0	0	1
Health score	79681	2.57	1.14	1	3	4
Housing tenure (own any dwelling)	79681	0.91	0.28	1	1	1
Housing tenure (own multiple dwellings)	79681	0.15	0.36	0	0	1
Per capita family net income	79681	61000	91000	8720	42000	120000
Mortgage debts	79681	2879	21000	0	0	0
Individual-level marriage data (biennial frequency)						
Treat	30799	0.60	0.49	0	1	1
New marriage	30799	0.05	021	0	0	0
Age	30799	30.00	8.28	19	30	42
Education level	30799	2.36	1.40	0	2	4
Ethnic minority	30799	0.14	0.35	0	0	1
Party membership	30799	0.05	0.22	0	0	0
Urban residence	30799	0.31	0.46	0	0	1
Migratory status	30799	0.48	0.50	0	0	1
Health score	30799	2.73	1.10	1	3	4
Urban residence	30799	0.31	0.46	0	0	1
Housing tenure (own any dwelling)	30799	0.91	0.29	1	1	1
Housing tenure (own multiple dwellings)	30799	0.16	0.37	0	0	1
Per capita family net income	30799	65000	91000	9510	48000	130000
Mortgage debts	30799	3350	23000	0	0	1
Household-level education investment data (biennial frequency)						
Treat	28404	0.54	0.50	0	1	1
Educational investment	28404	1.38	2.57	0	0.46	3.74
Log(per capita family net income)	28404	10.29	1.41	8.75	10.53	11.61
Log(total asset)	28404	12.11	1.36	10.56	12.20	13.64

Notes: This table reports summary statistics for all the variables used in this paper. The city-level data combine information from the Statistical Communiqué on Economic and Social Development for each city and the CityRE constant-quality house price indices spanning from 2009 to 2020. The variable ”Birth Rate” is city-level birth rate in the next year. The annual individual-level data contain annual number of newborns, new marriage indicators, and age reconstructed from the biennial CFPS surveys, and survey wave control variables, spanning from 2009 to 2020. The household-level data are from the biennial CFPS surveys spanning from 2010 to 2020.

Table 2: DID Estimated Effects of HPR Spillovers on Birth Rates and House Prices (City-level)

	(1) log(House Price)	(2) log(House Price)	(3) Birth Rate(‰)	(4) Birth Rate(‰)	(5) Birth Rate for the Next Year (IV)	(6) Birth Rate for the Next Year (IV)
Treat × Post	0.138***	0.124***	-1.557***	-1.683***		
	(0.030)	(0.030)	(0.305)	(0.293)		
log(House Price)					-7.099***	-8.760***
					(2.233)	(2.555)
Mean	8.544	8.544	10.723	10.723	8.544	10.723
R^2	0.971	0.940	0.877	0.820	-0.392	-0.540
Observations	2589	2589	2589	2589	2589	2589
City FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
City Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
City Controls	yes	yes	yes	yes	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the difference-in-differences regressions of birth rate of cities and house prices with respect to the spillovers from the imposition of house purchase restrictions in the policy cities (HPR spillovers). The sample consists of all unregulated cities. The treatment group are the cities nearby the policy cities, with a cutoff of 250 km. Considering that the change in fertility occurs later than the change in house prices taking into account of the pregnancy delay, the timing of fertility data is one year later than that of the house price data. The birth rate data spans from 2010 to 2021, and the house price data spans from 2009 to 2020. The dependent variables are log CityRE house price index in each city in each year, in column (1) and column (2), birth rate in each city in each year, which unit is ‰, in column (3) and column (4). Column (5) and column(6) report IV estimation of the effect of house price on next year's fertility, instrumenting house price by policy spillover shocks. Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. In column (1) and column (2), Post is a dummy that takes the value 1 if the time is after or equal to year 2017. In column (3) and column (4), Post is a dummy that takes the value 1 if the time is after or equal to year 2018, taking into account the pregnancy delay. City Trend is a city-specific linear trend, and the results of controlling for it are in the odd columns. Group Trend is a treatment-group-specific linear trend, and the results of controlling for it are in the even columns. The city-level control variables are log per capita local fiscal expenditure, log average wage income, log local population and local per capita GDP growth. Standard errors are clustered at the city level.

Table 3: DID Estimated Effects of HPR Spillovers on the Number of Newborns (Individual-level)

	(1) Newborns	(2) Newborns	(3) Newborns	(4) Newborns
Treat × Post	-0.025***	-0.028***	-0.024**	-0.027***
	(0.010)	(0.010)	(0.010)	(0.010)
Mean	0.061	0.061	0.061	0.061
R^2	0.041	0.038	0.046	0.043
Observations	78408	78408	78408	78408
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
City Trend	yes	no	yes	no
Group Trend	no	yes	no	yes
Individual Controls	no	no	yes	yes
Family Controls	no	no	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the difference-in-differences regressions of number of newborns using CFPS data with respect to the spillovers from the imposition of house purchase restrictions (HPR spillovers). The sample consists of women of childbearing age in all unregulated cities. The treatment group are those in cities nearby the policy cities, with a cutoff of 250 km. The number of newborns data spans from 2009 to 2020. The dependent variables are number of newborns of each individual in each year. Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2018, the first full year after the HPR spillover shock taking into account the pregnancy delay. City Trend is a city-specific linear trend, and the results of controlling for it are in the odd columns. Group Trend is a treatment-group-specific linear trend, and the results of controlling for it are in the even columns. In column (3) and column (4), individual control variables and family control variables are added, while in column (1) and column (2) are not. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 4: DID Robustness Check: Different Designations of Treatment Status

(a) DID robustness check of using alternative distance cutoff: 200 km

	(1) log(House Price)	(2) log(House Price)	(3) Birth Rate(‰)	(4) Birth Rate(‰)	(5) Newborns	(6) Newborns
Treat×Post	0.151***	0.142***	-1.349***	-1.454***	-0.023**	-0.023**
	(0.029)	(0.029)	(0.324)	(0.310)	(0.010)	(0.010)
Mean	8.544	8.544	10.723	10.723	0.061	0.061
R^2	0.971	0.940	0.875	0.819	0.038	0.043
Observations	2589	2589	2589	2589	78408	78408
City FE	yes	yes	yes	yes	no	no
Individual FE	no	no	no	no	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
City Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
City Controls	yes	yes	yes	yes	no	no
Individual Controls	no	no	no	no	yes	yes
Family Controls	no	no	no	no	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) DID robustness check of using alternative distance cutoff: 300 km

	(1) log(House Price)	(2) log(House Price)	(3) Birth Rate(‰)	(4) Birth Rate(‰)	(5) Newborns	(6) Newborns
Treat×Post	0.160***	0.142***	-1.564***	-1.610***	-0.026***	-0.025***
	(0.030)	(0.030)	(0.305)	(0.291)	(0.010)	(0.010)
Mean	8.544	8.544	10.723	10.723	0.061	0.061
R^2	0.971	0.940	0.877	0.819	0.038	0.043
Observations	2589	2589	2589	2589	78408	78408
City FE	yes	yes	yes	yes	no	no
Individual FE	no	no	no	no	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
City Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
City Controls	yes	yes	yes	yes	no	no
Individual Controls	no	no	no	no	yes	yes
Family Controls	no	no	no	no	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(c) DID robustness check of using continuous distance specification

	(1) log(House Price)	(2) log(House Price)	(3) Birth Rate(‰)	(4) Birth Rate(‰)	(5) Newborns	(6) Newborns
log(Distance)×Post	-0.125***	-0.113***	0.884***	0.953***	0.013**	0.012***
	(0.016)	(0.016)	(0.173)	(0.170)	(0.006)	(0.005)
Mean	8.544	8.544	10.723	10.723	0.061	0.061
R^2	0.973	0.944	0.876	0.819	0.038	0.043
Observations	2589	2589	2589	2589	78408	78408
City FE	yes	yes	yes	yes	no	no
Individual FE	no	no	no	no	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
City Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
City Controls	yes	yes	yes	yes	no	no
Individual Controls	no	no	no	no	yes	yes
Family Controls	no	no	no	no	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the robustness of the difference-in-differences estimation of cities' urban house price, birth rate, and number of newborns of individual. Panels (a) and (b) uses alternative distance cutoffs of 200 km and 300 km, respectively, when designating the treatment group, and panel (c) uses a continuous distance specification when designating the treatment effect. The sample consists of all unregulated cities. The house price data span from 2009 to 2020. The birth rate data span from 2010 to 2021. The number of newborns data span from 2009 to 2020. In column (1) and column (2), regressions are at the city-year level and the dependent variables are birth rate of each city in each year. In column (3) and column (4), regressions are at the individual-year level and the dependent variables are the number of newborns of each individual in each year. Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2018 (2017) for birth rate and newborn (house price), which takes into account the pregnancy delay. City Trend is a city-specific linear trend, and the results of controlling for it are in the odd columns. Group Trend is a treatment-group-specific linear trend, and the results of controlling for it are in the even columns. The city-level control variables are log per capita fiscal expenditure, log average wage income, log local population and local per capita GDP growth. Standard errors are clustered at the city level. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 5: Heterogeneous Treatment Effects by Housing Tenure in Rural and Urban Areas

	(1) Does Not Own Any	(2) Rural (Does Not Own Urban Home)	(3) Rural (Does Own Urban Home)	(4) Urban (Does Own Urban Home)
Dependent Variable: Number of Newborns				
Treat × Post	-0.020	-0.039***	0.011	-0.004
	(0.045)	(0.012)	(0.054)	(0.018)
R^2	0.054	0.058	0.009	0.013
Observations	6259	46965	7334	17533
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
City Trend	no	no	no	no
Group Trend	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes
Family Controls	yes	yes	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the heterogeneous treatment effects results comparing rural and urban population with various house status differently impacted by the house purchase restrictions. The sample consists of all unregulated cities, and the data span from 2009 to 2020. Regressions are at the individual-year level. The subsamples are mobile population who do not have any self-owned property in column (1), rural dwelling owners with no urban homeownership, i.e. those who live in rural areas with only one rural property (which is non-tradable) and do not own urban housing in column (2), rural multiple homeowners, i.e. those who live in rural areas who own a rural property (which is non-tradable) but also at least one urban home in addition, in column (3), and urban homeowners, those who live in urban area and own at least one urban property, in column (4). Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2018, the first full year after the HPR spillover shock taking into account the pregnancy delay. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 6: Heterogeneous Treatment Effects by Proximity to Schools, among Rural Dwelling Owners with no Urban Homeownership

	Rural (Does Not Own Urban Home)	
	(1)	(2)
	Schools Distant	Schools Nearby
Dependent Variable: Number of Newborns		
Treat × Post	-0.061***	-0.029
	(0.020)	(0.022)
R^2	0.068	0.058
Observations	11478	12535
Individual FE	yes	yes
Year FE	yes	yes
City Trend	no	no
Group Trend	yes	yes
Individual Controls	yes	yes
Family Controls	yes	yes

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the heterogeneous treatment effects results comparing rural and urban population with various house status differently impacted by the house purchase restrictions. The sample consists of all unregulated cities, and the data span from 2009 to 2020. Regressions are at the individual-year level. We focus on rural dwelling owners with no urban homeownership, i.e. those who live in rural areas with only one rural property (which is non-tradable) and do not own urban housing. The subsamples are those rural dwelling owners with no urban homeownership in counties where local schools are averagely far from home in column (1), and those rural dwelling owners with no urban homeownership in counties where local schools are averagely close to home in column (2). Whether the county has distant schools or not are designated by the median distance from home to local schools, reported by rural dwelling owners with no urban homeownership. If the area median home-school distance is larger than national rural/urban-specific median, this area is designated as "Schools Distant", and vice versa. Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2018, the first full year after the HPR spillover shock taking into account the pregnancy delay. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual-level control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family-level control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 7: DID Estimated Effects of House Purchase Restrictions on New Marriage and Heterogeneous Treatment Effects

(a) DID Estimated Effects of House Purchase Restrictions on New Marriage

	(1) New Marriage	(2) New Marriage	(3) New Marriage	(4) New Marriage
Treat×Post	-0.037***	-0.039***	-0.037***	-0.038***
	(0.014)	(0.013)	(0.014)	(0.013)
R^2	0.044	0.042	0.050	0.048
Observations	29988	29988	29988	29988
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
City Trend	yes	no	yes	no
Group Trend	no	yes	no	yes
Individual Controls	no	no	yes	yes
Family Controls	no	no	yes	yes

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) Heterogeneous Treatment Effects on New Marriage by Housing Tenure in Rural and Urban Areas

	(1) Does Not Own Any	(2) Rural (Does Not Own Urban Home)	(3) Rural (Does Own Urban Home)	(4) Urban (Does Own Urban Home)
Dependent Variable: New Marriage				
Treat×Post	-0.101	-0.028***	0.030	-0.002
	(0.080)	(0.016)	(0.061)	(0.033)
R^2	0.071	0.084	0.166	0.085
Observations	1325	16437	1615	5479
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
City Trend	no	no	no	no
Group Trend	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes
Family Controls	yes	yes	yes	yes

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: These tables show the results of the competitive marriage market. Panel (a) reports the difference-in-differences regressions of the likelihood of new marriage using CFPS data with respect to the spillovers from the imposition of house purchase restrictions. Panel (b) reports the heterogeneous treatment effects results, comparing rural and urban population with various house status, differently impacted by the house purchase restrictions. The sample consists of all unregulated cities. The new marriage data spans from 2009 to 2020. The subsamples in panel (b) are mobile population who do not have any self-owned property in column (1), rural dwelling owners with no urban homeownership, i.e. those who live in rural areas with only one rural property (which is non-tradable) and do not own urban housing in column (2), rural multiple homeowners, i.e. those who live in rural areas who own a rural property (which is non-tradable) but also at least one urban home in addition, in column (3), and urban homeowners, those who live in urban area and own at least one urban property, in column (4). The dependent variables are the incidences of new marriage of each individual in each year. Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2017. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual control variables are age, age^2, education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 8: DID Estimated Effects of HPR Spillovers on Number of Newborns Among the Married Population

(a) DID Estimated HPR Spillovers on Number of Newborns Among the Married Population

	(1) Newborns	(2) Newborns	(3) Newborns	(4) Newborns
Treat×Post	-0.032**	-0.035**	-0.020	-0.021
	(0.014)	(0.014)	(0.014)	(0.014)
Mean	0.075	0.075	0.075	0.075
R^2	0.067	0.064	0.082	0.081
Observations	54486	54486	54486	54486
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
City Trend	yes	no	yes	no
Group Trend	no	yes	no	yes
Individual Controls	no	no	yes	yes
Family Controls	no	no	yes	yes

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) Heterogeneous Treatment Effects Among the Married Population, by Housing Tenure in Rural and Urban Areas

	(1) Does Not Own Any	(2) Rural (Does Not Own Urban Home)	(3) Rural (Does Own Urban Home)	(4) Urban (Does Own Urban Home)
Dependent Variable: Number of Newborns				
Treat×Post	-0.034	-0.034**	0.022	0.001
	(0.065)	(0.016)	(0.080)	(0.025)
R^2	0.053	0.081	0.036	0.051
Observations	3616	32702	5428	12091
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
City Trend	no	no	no	no
Group Trend	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes
Family Controls	yes	yes	yes	yes

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: These tables reports the difference-in-differences results and heterogeneous treatment effects results of number of newborns in married population impacted by the house purchase restrictions. The sample consists of all unregulated cities, and the data span from 2009 to 2020. Regressions are at the individual-year level. The subsamples in panel (b) are mobile population who do not have any self-owned property in column (1), rural dwelling owners with no urban homeownership, i.e. those who live in rural areas with only one rural property (which is non-tradable) and do not own urban housing in column (2), rural multiple homeowners, i.e. those who live in rural areas who own a rural property (which is non-tradable) but also at least one urban home in addition, in column (3), and urban homeowners, those who live in urban area and own at least one urban property, in column (4). Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2018, the first full year after the HPR spillover shock taking into account the pregnancy delay. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 9: Treatment Effects Among Rural Dwelling Owners with no Urban Homeownership, by Local Sex Ratio

Dependent variables	Newborn		New marriage		New marriage (men)		Newborn (married)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local sex ratio	High	Low	High	Low	High	Low	High	Low
Treat×Post	-0.064***	-0.018	-0.030*	-0.031	-0.052	-0.012	-0.057**	-0.014
se	(0.017)	(0.017)	(0.016)	(0.029)	(0.040)	(0.031)	(0.023)	(0.022)
Individual FE	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
City Trend FE	no	no	no	no	no	no	no	no
Group Trend FE	yes	yes	yes	yes	yes	yes	yes	yes
Individual Control	yes	yes	yes	yes	yes	yes	yes	yes
Family Control	yes	yes	yes	yes	yes	yes	yes	yes
R2	0.0623	0.0458	0.0701	0.0967	0.0770	0.1257	0.0831	0.0700
Obs	21877	25088	7620	8817	7957	9137	15257	17445

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: DID Estimated Effects of HPR Spillovers on Parents' Investment on Children's Education, by Housing Tenure in Rural and Urban Areas

	(1) Does Not Own Any	(2) Rural (Does Not Own Urban Home)	(3) Rural (Does Own Urban Home)	(4) Urban (Does Own Urban Home)
Dependent Variable: Educational Investments				
Treat×Post	0.605	0.582***	-0.636	-0.269
	(0.962)	(0.153)	(1.039)	(0.610)
R^2	0.640	0.287	0.406	0.321
Observations	734	15285	1733	4217
Household FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
City Trend	no	no	no	no
Group Trend	yes	yes	yes	yes
Family Controls	yes	yes	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the DID estimated treatment effects of the house price shock, triggered by spillovers of house purchase restrictions in nearby regulated cities, on parents' investments on children's' education, separately for rural and urban populations with different housing tenure statuses. The educational investments data span from 2009 to 2020. Regressions are at the household-year level. The subsamples are mobile population who do not have any self-owned property in column (1), rural dwelling owners with no urban homeownership, i.e. those who live in rural areas with only one rural property (which is non-tradable) and do not own urban housing in column (2), rural multiple homeowners, i.e. those who live in rural areas who own a rural property (which is non-tradable) but also at least one urban home in addition, in column (3), and urban homeowners, those who live in urban area and own at least one urban property, in column (4). Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to the year 2017. City Trend is a city-specific linear trend, and the results of controlling this fixed effect are in columns (1) and (3) of panel (a). Group Trend is a treatment-group-specific linear trend, and the results of controlling this fixed effect are in columns (2) and (4) of panel (a), and all columns of panels (b) . The family control variables are urban residence, housing tenure, log per capita family net income, log total asset, and migratory status. Standard errors are clustered at the city level.

Table 11: The Treatment Effects of the House Price Shock on Different Age Groups

(a) Number of Newborns

	(1) Age: 15-19	(2) Age: 20-29	(3) Age: 30-39	(4) Age: 40-44	(5) Under 35	(6) 35 and Over
Dependent Variable: Number of Newborns						
Treat×Post	0.015	-0.033	-0.015	0.015	-0.028*	-0.007
	(0.010)	(0.022)	(0.016)	(0.010)	(0.016)	(0.009)
R^2	0.229	0.022	0.025	0.031	0.041	0.026
Observations	9404	27044	26934	16271	50793	29358
Individual FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
City Trend	no	no	no	no	no	no
Group Trend	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Family Controls	yes	yes	yes	yes	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) Likelihood of New Marriage

	(1) Age: 15-19	(2) Age: 20-29	(3) Age: 30-39	(4) Age: 40-44	(5) Under 35	(6) 35 and Over
Dependent Variable: New Marriage						
Treat×Post	0.003	-0.056	-0.011	-0.011	-0.049**	-0.011
	(0.024)	(0.034)	(0.022)	(0.020)	(0.019)	(0.017)
R^2	0.219	-0.032	0.127	-0.016	0.014	0.179
Observations	2867	9273	8527	4576	19288	9393
Individual FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
City Trend	no	no	no	no	no	no
Group Trend	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes
Family Controls	yes	yes	yes	yes	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: These tables report the treatment effects results of different age groups differently impacted by the house purchase restrictions. The sample consists of all unregulated cities, and the data of number of newborns and the incidences of new marriage both span from 2009 to 2020. Regressions are at the individual-year level. Panel (a) reports the heterogeneous treatment effects on number of newborns and panel (b) reports the heterogeneous treatment effects on the likelihood of new marriage. The subsamples in panels (a) and (b) are population aged from 15 to 19 in column (1), population aged from 20 to 29 in column (2), population aged from 30 to 39 (3) and population aged from 40 to 44 in column (4). According to the definition of advanced maternal age (AMA) which is over age 35, column (5) and column (6) report the results of under the AMA and over the AMA. Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to the year 2018 for the number of newborns, taking into account the pregnancy delay, and if the time is after or equal to the year 2017 for new marriage. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual control variables in panel (a) are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The individual control variables in panel (b) are age, age^2, education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 12: Placebo Test of the Local Rural-to-urban Migration Channel

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Alternative "Treatment Designation": $\Delta X_{2016-2021}$ >Median($\Delta X_{2016-2021}$)	$X =$ Urbanization Rate		$X =$ Share of Primary Industry GDP		$X =$ Share of Primary Industry GDP (Incl. Agri. Services)		$X =$ Share of Primary Industry Emp.	
Dependent Variable: Birth Rate(‰)								
Treat$_{Alternative}$×Post	0.627*	0.738**	0.200	-0.101	0.002	0.003	-1.294**	-1.304**
	(0.352)	(0.339)	(0.331)	(0.318)	(0.504)	(0.439)	(0.535)	(0.505)
R^2	0.876	0.828	0.872	0.825	0.872	0.825	0.872	0.824
Obs	2658	2658	2658	2658	2658	2658	2658	2658
City FE	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
City Trend FE	yes	no	yes	no	yes	no	yes	no
Group Trend FE	no	yes	no	yes	no	yes	no	yes
City Control	yes	yes	yes	yes	yes	yes	yes	yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the placebo test of local rural-to-urban migration's effect on birth rate. Regressions are at the city-year level. Treat is a dummy that takes the value 1 if the change of alternative ”treatment” designation variable from 2016 to 2021 is larger than the national median level. Post is a dummy that takes the value 1 if the time is after or equal to year 2018. The alternative ”treatment” designation variable in column (1) and column (2) is urbanization rate represented by the proportion of urban resident population to the whole resident population. The alternative ”treatment” designation variable in column (3) and column (4) is the proportion of primary industry GDP to GDP. The alternative ”treatment” designation variable in column (5) and column (6) is the proportion of primary industry GDP (including related services) to GDP. The alternative ”treatment” designation variable in column (7) and column (8) is the proportion of employments in the primary industry. City Trend is a city-specific linear trend, and the results of controlling for it are in the odd columns. Group Trend is a treatment-group-specific linear trend, and the results of controlling for it are in the even columns. The city-level control variables are log per capita fiscal expenditure, log average wage income, log local population and local per capita GDP growth. Standard errors are clustered at the city level.

Table 13: Placebo Tests of the One Child Policy Channel

	(1) Birth Rate(‰)	(2) Birth Rate(‰)	(3) Newborns	(4) Newborns
$\text{Treat}_{HPRSpillover} \times \text{Post}_{OCP}$	0.029	-0.263	-0.003	-0.005
	(0.321)	(0.293)	(0.016)	(0.016)
Mean	11.347	11.347	0.066	0.066
R^2	0.875	0.806	0.053	0.049
Observations	1799	1799	48022	48022
City FE	yes	yes	no	no
Individual FE	no	no	yes	yes
Year FE	yes	yes	yes	yes
City Trend	yes	no	yes	no
Group Trend	no	yes	no	yes
City Controls	yes	yes	no	no
Individual Controls	no	no	yes	yes
Family Controls	no	no	yes	yes

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the placebo test of one child policy's effect on birth rate or number of newborns. $\text{Treat}_{HPRSpillover}$ is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post_{OCP} is a dummy that takes the value 1 if the time is after or equal to year 2014 and before year 2017. Regressions in columns (1) and (2) are at the city-year level. Regressions in columns (3) and (4) are at the individual-year level. City Trend is a city-specific linear trend, and the results of controlling for it are in the odd columns. Group Trend is a treatment-group-specific linear trend, and the results of controlling for it are in the even columns. The city-level control variables are log per capita fiscal expenditure, log average wage income, log local population and local per capita GDP growth. All of these city-level control variables used in the regression of birth rate are lagged one period. The individual control variables are age, age^2, education level, marital status, marital status$\times$spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.