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

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Abstract

We study the causal effect of house price increases on China's great birth rate decline since 2016, as well as on the country's marriage market and private educational investments. Using quasi-experimental increases in urban house prices in nearby unregulated prefectures—driven by capital spillovers resulting from house purchase restrictions in large metropolises—we find that the birth rate significantly decreased in these prefectures. This fertility reduction is driven by rural individuals who do not own urban homes, particularly in places where sex imbalance is notable, indicating competition in the marriage market, and where rural schools are scarce, signifying the role of educational opportunities. Our study provides the first causal evidence for a previously underexplored channel through which house prices impact fertility outcomes: by affecting the price of educational resources and marriage market benefits associated with homeownership. A back-of-the-envelope calculation suggests that this 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

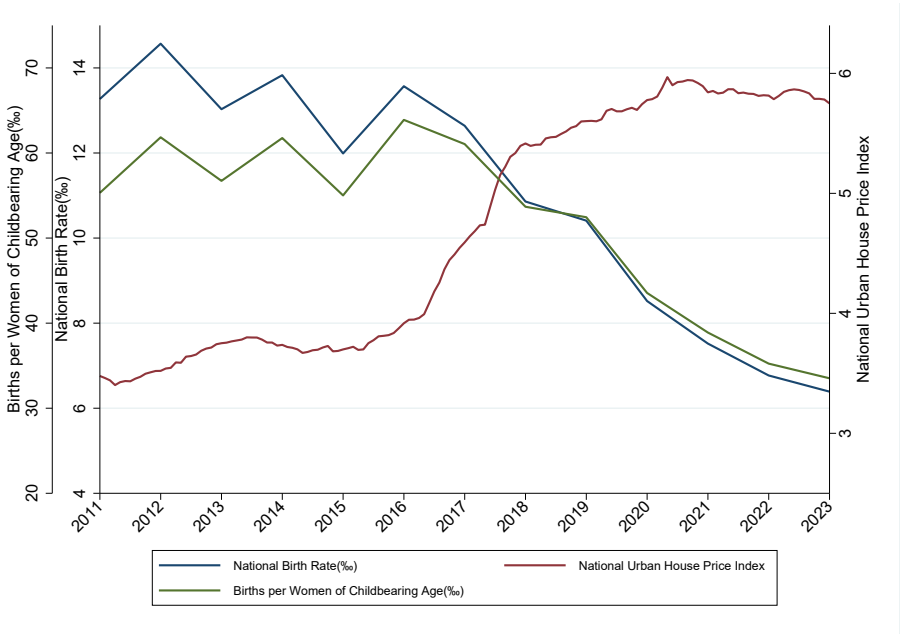
JEL Classification: D13, D15, J13, O15, R21, R31

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1. Introduction

China has experienced a significant decline in birth rates since 2016. Before this recent decline, the birth rate had averaged approximately thirteen per thousand since the early 2000s, remaining below the replacement level. Concerns over an again population led to the relaxation of the 'one-child' policy in 2013, but it failed to significantly boost birth rates. In fact, the birth rate fell rapidly from thirteen per thousand in 2016 to six per thousand recently. This precipitous decline—seen both in the raw birth rate and the birth rate for women of childbearing age (Figure 1)—has caused the country's population to peak in 2023. For China, the potential consequences include labor shortages, increased pressures on elderly care systems, and slower economic growth (Prasad, 2023). Globally, China's birth rate decline could have far-reaching repercussions, reshaping global demographics, and shifting the balance of economic and political power.

Figure 1: The Coincidental Timing of the House Price Surge and Birth Rate Decline



Notes: This figure plots the national raw birth rate and the birth rate for women of childbearing age (left axes) and the national urban house price index (right axis). Source: Annual data on the national birth rate from the China Statistical Yearbook; Annual data on births per women of childbearing age from author's calculation based on data from the China Statistical Yearbook and the Population Census; Monthly data on the national urban house price index from CityRE.

Interestingly, China's birth rate decline coincided with a quick rise in urban house prices. As shown in Figure 1, the national average urban house price index increased by 54 percent between 2016 and 2021, while the raw birth rate dropped by 45 percent, and the birth rate for women of childbearing age fell by 39 percent during the same period. This coincidence raises an important question: is there a causal link between the surge in urban house prices and China's birth rate decline?

Studying this potential link is essential and challenging, as demographic shifts can influence

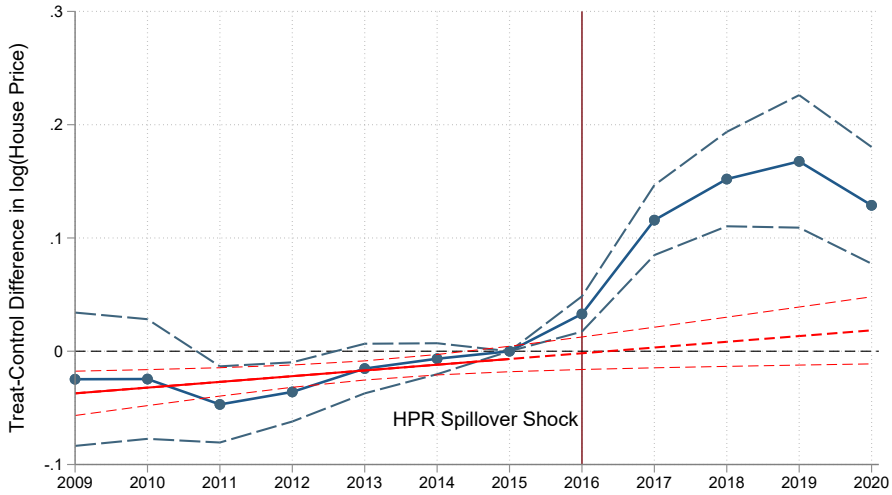
house prices, which themselves reflect future expectations. Identifying valid instruments for house prices is difficult. For example, one commonly used strategy adopts instruments related to supply elasticity (Saiz, 2010), but the interpretation of these instruments is complicated by the fact that cities with inelastic housing supply are often "superstar" cities (Van Nieuwerburgh and Weill, 2010; Gyourko, Mayer, and Sinai, 2013) that experience faster economic growth during boom episodes when house prices also rise. This can influence individuals' opportunity costs through economic fundamentals, thereby affecting birth rates beyond the direct effect of house prices (Davidoff, 2016).

In response, we use a unique quasi-experiment that increased house prices in some Chinese prefectures to identify the causal effect of house prices on China's birth rate decline.[1] The background of this quasi-experiment is the prevalence of investment purchases in China's housing market (Fang, Gu, Xiong, and Zhou, 2016; Wu, Gyourko, and Deng, 2016; Glaeser, Huang, Ma, and Shleifer, 2017) and the unintended consequences of policies aimed at controlling them. In 2016, major Chinese metropolises imposed house purchase restrictions to cool overheated housing markets by curbing local investment and speculation. We show in earlier work (Deng, Liao, Yu, and Zhang, 2022) that the unfulfilled investment demand from the regulated metropolises was redirected to nearby unregulated prefectures, our treatment group. This shock caused house prices to rise without any fundamental changes in these prefectures, compared to unregulated prefectures that are farther away, our control group.

Our identification strategy allows for different pre-existing trends at the treatment and control groups of prefectures. Our identification assumption posits that, absent the spillover effects from the house purchase restrictions, urban house prices and fertility outcomes in both the treated and control groups would have continued along their pre-existing trends. We use prefecture-level urban house price indices from CityRE, and we manually collected prefecture-level birth rates from annual Statistical Communiqués released by prefecture governments. Using the urban house price quasi-experiment arising from house purchase restiction spillovers, we find results consistent with the interpretation that the speculation-driven house price rise significantly affected China's birth rate decline. In our treated group—the nearby unregulated prefectures—urban house prices sharply took off from trend, with an average abnormal increase of 12.4% compared to the control group (Figure 2). And, the birth rate sharply fell off from trend, with an average abnormal reduction of 1.68 per thousand relative to the control prefectures (Figure 3).

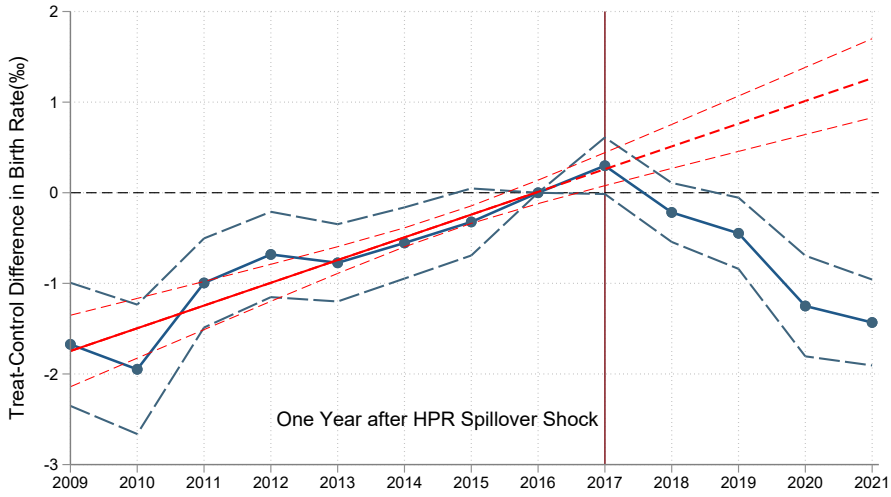
[1] "Prefectures" (or "prefectural cities") are administrative regions that encompass both urban areas and surrounding rural areas, similar to MSAs in the United States or départements in France. The findings of our study focus on individuals in both urban and rural parts of these prefectures.

Figure 2: Response to the HPR Spillover Shock: Urban House Prices



Notes: This figure plots the estimated response of log(urban house price index) to the house purchase restriction spillover shock. The response is estimated using event study regressions controlling for prefecture and year fixed effects and treatment-specific linear trends, and fiscal expenditure, average wage, population and per capita output growth. 95% confidence intervals are drawn based on standard errors clustered at the prefecture level. The timing of the house purchase restriction spillover shock is labeled by the vertical red line. Red upward sloping lines are the treatment-specific linear trend and its 95% confidence intervals. Data on the urban house price indices is from CityRE and available from 2009 to 2020 annually.

Figure 3: Response to the HPR Spillover Shock: Prefecture-level Birth Rate



Notes: This figure plots the estimated response of prefecture-level birth rate to the house purchase restriction spillover shock. The response is estimated using event study regressions controlling for prefecture and year fixed effects and treatment-specific linear trends, and fiscal expenditure, average wage, population and per capita output growth. 95% confidence intervals are drawn based on standard errors clustered at the prefecture level. One year after the house purchase restriction spillover shock is labeled by the vertical red line to take into account of the pregnancy delay. Red upward sloping lines are the treatment-specific linear trend and its 95% confidence intervals. The prefecture-level birth rate data is manually collected from Statistical Communiqués and available from 2009 to 2021 annually.

The economic magnitude of our estimated house prices' effect on the birth rate decline is substantial. We estimate a semi-elasticity of 8.8, that a 10% abnormal increase in urban house prices reduced the birth rate by 0.88 per thousand people. Using spatial decay of the spillover shock and a potential outcome framework, we estimate that this house price quasi-experiment alone reduced births by 2.46 million births, relative to pre-existing projections in the unregulated prefectures, or 10.4% of the aggregate birth reduction in the post-treatment period.

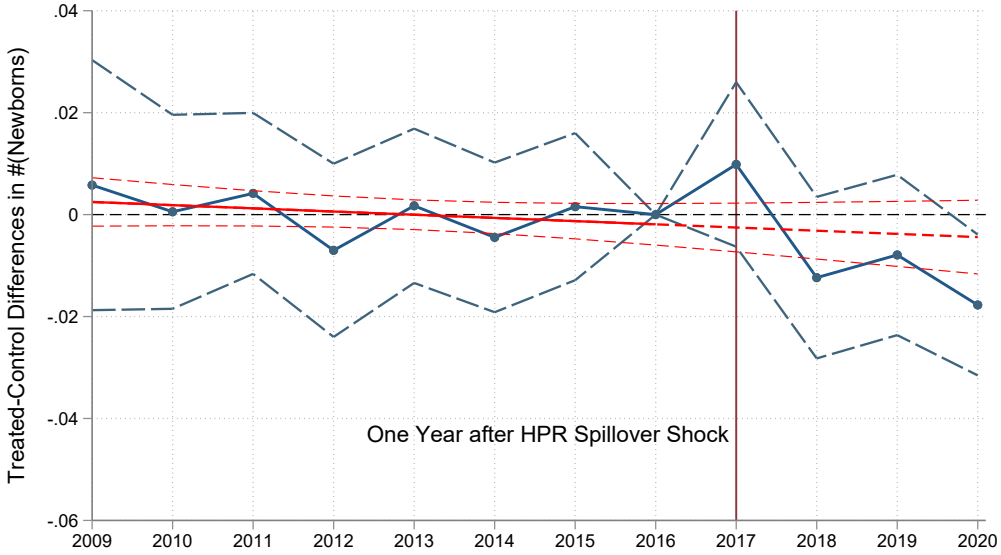
We found that the treatment decline in the birth rate after the urban house price shock could not be attributed to any other factor we considered. One potential concern is that the treated and control prefectures may not be fully comparable. By design, treated prefectures were closer to regulated metropolises and may have been economically more vibrant. Indeed, our analysis revealed that treated prefectures were larger, experienced faster economic growth, had higher house price levels, and similar wages, but lower fiscal expenditure per capita. To address these inherent differences, we controlled for these time-varying characteristics, in addition to prefecture fixed effects and prefecture-specific trends. Furthermore, we matched treated prefecture with control prefecture that were comparable in these characteristics and obtained similar results.

Another concern is that the negative but shrinking pre-trends in birth rates in treated prefectures compared to control prefectures could have affected our treatment effect estimates. One possible channel is that treated prefectures may have experienced a relatively growing population of women of childbearing age. To address this, we alternatively used the birth rate among women of childbearing age as the dependent variable and found quantitatively similar results, suggesting that the influence of this channel is limited.

We apply recent methodologies from the difference-in-differences literature that examine the robustness of quasi-experimental estimates to non-linearity in pre-existing trends. Specifically, we follow approaches by Bilinski and Hatfield (2020) and Rambachan and Roth (2023), which allows for same or different pre-existing trends, but require researchers to test robustness to richer function forms in trends and post-treatment changes in trends. By using long pre-period data and identifying sharp breaks from trend, we ensure that our estimates are robust to these tests.

We also considered the possibility that individual-level factors, such as education and income, could have affected birth trends before the house price shock. To address this, we utilized data from the China Family Panel Studies (CFPS), a nationally representative survey, and reconstructed annual birth records from its biennial survey waves. We estimated the treatment effect on the number of newborns at the individual level, controlling for these individual-level factors as well as individual fixed effects. Our analysis revealed a significantly negative treatment effect on individual-level births without observing significant pre-trends (Figure 4), further supporting our findings.

Figure 4: Preexisting Trends and Dynamic Responses: Newborns (Individual-level)



Notes: This figure plots the estimated response of number of newborns of each individual in treated prefectures relative to control prefectures, both before and after the house purchase restriction spillover shock. The response is estimated using event study regressions controlling for individual and year fixed effects and treatment-specific linear trends. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, away-from-home 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 prefecture level. One year after the house purchase restrictions in the regulated metropolises is labeled by the vertical red line to take into account of the pregnancy delay. Red upward sloping line is the pre-treatment linear trend and its 95% confidence intervals. Data on the number of newborns is reconstructed from the CFPS and available from 2009 to 2020 annually.

Why did the house price rise affect the birth rate? We explore this by using the individual-level data, and analyze treatment effects on the number of newborns to urban and rural individuals with different housing tenure statuses.

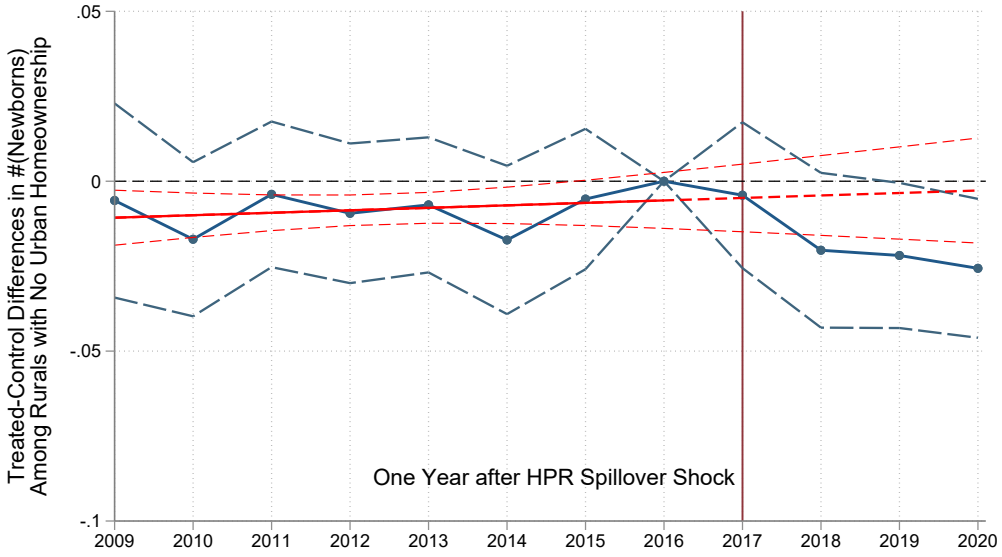
We found a significant negative treatment effect on birth rates *only* among rural individuals who own their rural dwellings but do not own urban homes (Figure 5), which is also the majority group in our sample. Notably, these individuals have land for shelter allocated by their villages according to the family size, so their shelter costs were not directly affected by the urban house price shock. Instead, the negative treatment effect on their birth rate is consistent with a new channel, that rural people's aspirations for urban homeownership drove the effect of urban house prices on the decline in birth rates.

Aligning with this new channel, we did not find a significant negative treatment effect among urban homeowners, which is likely due to the offsetting effects of two conventional factors: the substitution effect of needing future shelter space for new births, and the positive wealth effect.[2] We found imprecise but consistent effects among two other groups. Mobile individuals who

[2]By design, urban homeowners were unaffected by the hypothesis that rural people's aspirations for urban homeownership drive the effect of house prices on birth rates, and our results confirm that they do not explain the impact of house prices on the birth rate decline.

own neither rural nor urban homes locally exhibited an imprecise negative fertility effect, while rural individuals who own both rural and urban homes showed an imprecise but positive fertility effect. These results are in line with our new channel.

Figure 5: Preexisting Trends and Dynamic Responses: Newborns (Individual-level) among Ruralites with No Urban Homeownership



Notes: This figure plots the estimated response of number of newborns of each individual in treated prefectures relative to control prefectures, both before and after the house purchase restriction spillover shock. The sample is restricted to rural dwelling owners with no urban homeownership. The response is estimated using event study regressions controlling for individual and year fixed effects and treatment-specific linear trends. The individual control variables are age, age^2, education level, marital status, marital status$\times$spouse's education level, party membership, urban residence, away-from-home 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 prefecture level. One year after the house purchase restrictions in the regulated metropolises is labeled by the vertical red line to take into account of the pregnancy delay. Red upward sloping line is the pre-treatment linear trend and its 95% confidence intervals. Data on the number of newborns is reconstructed from the CFPS and available from 2009 to 2020 annually.

Our hypothesis of this new channel, that rural people's aspirations for urban homeownership explain urban house prices' impact on the birth rate decline, is related to the view that owning an urban home enhances (1) marriage prospects and (2) access to higher-quality education. We test both mechanisms and find supportive results.

First, we find evidence consistent with the interpretation that social norms in the marriage market exacerbated the house price shock's effect on the birth rate decline. We observe that the birth rate decline of rural individuals without urban homeownership was especially noticeable in areas with a severe local sex imbalance, as proxied by a high local sex ratio among marriage-age individuals. Additionally, our analysis revealed that rural individuals in treated prefectures who do not own urban homes experienced a significant delay in marriage following the house price shock. In China, urban homeownership—unlike rural housing, which is non-tradable—is a key factor for marriage competition (Wei and Zhang, 2011; Wei, Zhang, and Liu, 2017). These results

are consistent with the interpretation that the urban house price shock increased barriers to marriage, as dictated by marriage market norms that emphasize urban homeownership, thereby contributing to the decline in birth rates.[3]

Second, we find evidence consistent with the interpretation that urban homeownership as a gateway to overcoming the urban-rural gap in educational resources also played a key role in house prices' impact on the birth rate decline. We estimate that the impact of rising urban house prices on birth rates among rural individuals was also particularly pronounced in areas with limited access to rural schools, as indicated by longer home-to-school travel times. In China, owning an urban home is closely tied to access to educational opportunities, as public-school enrollment is often linked to property ownership within a school's catchment area and/or possession of an urban hukou, which is frequently obtained through urban homeownership. The existing literature has documented a significant urban-rural gap in educational attainment (e.g., Li, Loyalka, Rozelle, Wu, and Xie, 2015). Furthermore, the recent consolidation and closure of rural schools (Ding, Wang, and Ye, 2016) have increased travel distances and reduced local access to education, placing rural students who lack access to urban schools at an even greater disadvantage.

From the perspective of human capital accumulation, we also find results that are consistent with the mechanism that private investment in children's education reacted significantly to partially undo the fertility reduction (e.g. Barro and Becker, 1989; Becker, Murphy, and Tamura, 1990; Galor and Weil, 2000). Our additional results indicate that rural married couples in the treated prefectures without urban homeownership reduced childbearing, a behavior consistent with the interpretation of a strategic adaptation. Facing higher barriers to accessing public education tied to urban homeownership, these couples appeared to reduce their fertility. Among these rural couples, we observe a significant increase, approximately 58% above pre-shock levels, in private educational investments conditional on having children. Thus, simultaneously, they seemed to compensate for the growing difficulty of accessing urban public education by turning to private alternatives, a resource they had previously relied on less.

To provide a universal framework for interpreting our rich set of findings on the treatment effect of urban house prices, we build a theoretical model of fertility, housing, and education investment, building on standard models of fertility and human capital investment, e.g., Barro and Becker (1989) and De La Croix and Doepke (2003), judiciously adding institutional assumptions that link urban housing ownership to marriage prospects and public educational resources. Importantly, our model can rationalize the set of rich observed empirical patterns for the treatment effects on fertility, marriage, and educational investment, through a set of infra-marginal responses to a large increase in urban house prices.

How does our study contribute to the academic discussion on the birth rate? The literature has long suggested that house prices play a non-negligible role in determining fertility outcomes (e.g. Becker, 1960; Yi and Zhang, 2010; Lovenheim and Mumford, 2013; Dettling and Kearney,

[3]A joint survey by Wuhan University, CASS, Sina, and XinhuaNet found that 83% of rural respondents believe that young people from their hometown need to purchase an urban home in order to get married (https://k.sina.com.cn/article_1712686623_6615861f027011c3w.html). However, our individual-level dataset reveals that only 17% of rural individuals actually own an urban home. These statistics further support this interpretation.

2014). Some studies suggesting that house price increases can increase fertility through a wealth effect (e.g., Lovenheim and Mumford, 2013; Daysal, Lovenheim, Siersbæk, and Wasser, 2021; Ang, Tan, Zhai, Zhang, and Zhang, 2024). However, other studies have found that house price increases can also reduce fertility by raising the cost of sheltering (e.g., Yi and Zhang, 2010; Clark, 2012; Dettling and Kearney, 2014; Liu, Liu, and Wang, 2023; Meng, Peng, and Zhou, 2023).

The most important contribution of our study is to provide the first causal evidence for a previously underexplored channel through which house prices impact fertility outcomes—by affecting the price of educational resources and marriage market benefits associated with homeownership. This channel, which has received less attention, may be equally important in understanding the relationship between house prices and birth rates. It is linked to classical theories on inequality, public vs. private education, and human capital production (De La Croix and Doepke, 2003; De la Croix and Doepke, 2004). This channel also showcases how shocks to house prices interact with competitive social norms in the housing market, extending Wei and Zhang (2011). Our results suggest that this new channel cannot be ignored for understanding house prices' effect on China's recent birth rate decline.

Our study also makes a secondary contribution to the literature on the redistributive intergenerational mobility aspect of housing markets (e.g. Chen, Fang, and Tang, 2024). As Chetty, Hendren, Kline, and Saez (2014) suggest, a child's prospects for upward mobility are greatly influenced by relocating to the "high-opportunity" area. Heckman and Landersø (2022) show that family residential decisions are typically made early in children's lives, often before their birth. In the language of Chetty, Hendren, Kline, and Saez (2014), the rural areas in our study are the "low-opportunity" areas and the urban areas are the "high-opportunity" areas with amenities and benefits tied to urban homeownership. Our findings highlight the importance of considering rural individuals and their interactions with urban housing markets in understanding China's birth rate decline. In doing so, our study contribute to the literature on the redistributive intergenerational mobility aspect of housing markets by documenting for the first time how rising housing costs in the "high-opportunity" areas—which exacerbates the barriers to relocation—causally led to strategic fertility and human capital investment responses for people in the "low-opportunity" areas.

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 prefecture-level and individual-level fertility. Section 5 presents subsequent analysis on the mechanisms driving these changes in fertility. Section 6 presents a full and a simplified dynamic model on theoretical interpretation of mechanisms. In Section 7, we entertain alternative explanations and discuss the aggregate significance of the house price shock's fertility effects. Section!8 concludes.

2. Institutional Background

This section outlines the institutional background for our study. We focus on how in our research setting, anecdotally, homeownership costs in urban areas (the "high-opportunity" areas) matter for individuals in rural areas (the "low-opportunity" areas) due to their impact on marriage prospects and access to educational resources.

The rising cost of urban homeownership has been pointed to as a significant suspect for China's birth rate decline. This is because urban homeownership is seen as to matter for both marriage—a near prerequisite for fertility due to social norms—and access to high-quality public education for children—which is closely linked to the returns to having children.

In China, societal views hold that men are expected to provide a stable and secure environment for their future family. Property ownership becomes a visible measure of financial stability and readiness to start a family. While rural land for shelter is by regulation assigned by village committees based on family size, rural dwellings are not tradable and have little liquidity.[4] Hence, the only tradable residential properties are urban homes, and urban homeownership are valued in process of searching for marriage partners and seeking approval from their family members.[5]

Urban home ownership is also deeply intertwined with educational opportunities. In urban areas, access to public schools is often linked to property ownership within a school's catchment area. The system operates on a hierarchy of eligibility: the highest priority is given to residents with all three qualifiers—local *hukou* (household registration, often obtained via property ownership), property ownership, and residential location. This is followed by those with two qualifiers, then those with only one, with *hukou* prioritized. ndividuals in the single-qualifier category are admitted only if space remains after accommodating those with two or three qualifiers.[6]

The educational gap between urban and rural areas is significant (Li, Loyalka, Rozelle, Wu, and Xie, 2015). In the years preceding to 2016, rural school closures have accelerated, largely driven by urbanization. This has led to school consolidations, where smaller rural schools merge into larger regional ones. While intended to improve resource efficiency, these consolidations often increase travel distances for students and reduce access to community-based education, further disadvantaging rural children (Ding, Wang, and Ye, 2016; Haepp and Lyu, 2018).

As of 2016, data from our micro-level survey sample show that fewer than twenty percent of rural individuals own urban homes, compared to over ninety percent of urban individuals. Given the aforementioned background, urban homeownership seems a close complement to marriage and fertility in our research laboratory. Consequently, it can be hypothesized that a positive shock to urban house prices could disproportionately heighten barriers for rural indi-

[4] See http://www.hzjjs.moa.gov.cn/zjdglygg/202312/t20231215_6442846.htm for an example of the per-person land allocation rule, where each additional person is eligible for 30 square meters (approxmately 320 square feet). See https://www.yindu.gov.cn/2024/09-05/3099210.html for the explanation of the non-tradability of rural homes and rural land for shelter.

[5] See https://k.sina.com.cn/article_1712686623_6615861f027011c3w.html for recent survey results on the importance of urban homeownership as a prerequisite for marriage.

[6] See https://www.cqcs.gov.cn/bm/qjw_75248/zwgk_73772/fdzdgknr_73775/zcwj_bm/qtwj_bm/202104/W020211210550317427192.docx for an example of the "three qualifiers" rule, where property ownership is key to school eligibility.

viduals, affecting their marriage prospects and their ability to provide quality public 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 urban house prices on fertility. We outline the policy-induced house purchase restriction shock, describe our treated and control groups, and explain the difference-in-differences estimation strategy. We further describe 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 metropolises. 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 prefectures (the regulated prefectures) 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 house purchase restrictions limited the number of homes investors can buy in the regulated metropolises, and also reduced the credit availability on investment properties there. Appendix Tables A.1 and A.2 provide a list of the regulated metropolises that implemented the house purchase restrictions and the specific policy measures. 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 house purchase restrictions was implemented, the regulated prefectures' investors can still purchase properties in the other prefectures that are unregulated. The nearby unregulated prefectures 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 prefectures despite any changes in the fundamentals, as studied by Deng, Liao, Yu, and Zhang (2022). They located 22 regulated metropolises that adopted house purchase restrictions in late 2016 and early 2017, and studied housing market dynamics in nearby unregulated prefectures, compared to farther away unregulated prefectures. They found that house prices increases in the regulated metropolises reduced after the house purchase restrictions were imposed. Immediately, online search activity for real estate in the nearby unregulated prefectures surged from the regulated prefectures.

Urban house prices and transaction volumes in the nearby unregulated prefectures rose sharply.

These prefectures' bank deposits similarly increased, aligning with house price hikes, indicative of capital inflows. Moreover, the rise in home transaction volumes in the nearby unregulated prefectures paralleled the decline in transaction volumes in the regulated metropolises. No evidence suggested changes in rents or economic growth. Local governments of the nearby unregulated prefectures 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 prefectures closer than 250 km to the nearest regulated city as treated prefectures to the house purchase restriction spillover effect. The farther away unregulated prefectures are the control prefectures. 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 prefectures are closer for investors in the regulated prefectures to screen and occasionally monitor, and information regarding the treated prefectures' properties may be more readily available. These prefectures are at the level of commuting zones, meaning that they are also not near enough for commuting from the regulated metropolises.

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 house purchase restriction 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 house purchase restriction spillover effects to decay log-linearly with distance.

The treatment effectively shocked local urban house prices, as shown in Figure 1. Before the house purchase restriction spillover shock, urban house prices in treated prefectures increased stably, approximately 0.5% annually faster than the control prefectures. Strikingly, urban house prices in treated prefectures increased abnormally and sharply faster after the house purchase restriction 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 prefectures quickly rose to 9% relative to trend in 2017, and 12% relative to trend in 2018 and 2019 (Figure 2).

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

There is also no evidence that the surge in treated prefectures' 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, Liao, Yu, and Zhang (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 prefectures, 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 prefectures and those in the control prefectures would grow at their pre-existing trends. We follow Wolfers (2006) and Bilinski and Hatfield (2020) 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 prefecture (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 prefecture-specific trends. The results are the same.

Following Bilinski and Hatfield (2020), 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. Urban house prices in treated prefectures may react immediately after house purchase restriction spillovers. Because the house purchase restrictions were enacted in the regulated metropolises in September 2016, the initial full year when house prices in treated prefectures 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 prefectures 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 in-

crease 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 prefecture-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 prefecture level, we obtain annual birth rates from each prefecture's annual Statistical Communiqué on Economic and Social Development. The prefectures' Statistical Communiqué are readily available on each prefecture's statistical bureau website, and are archived in electronic text format by aggregation platforms. We manually downloaded and scraped each prefecture's Statistical Communiqué from 2009 to 2021. In rare cases when the birth rate was missing from Statistical Communiqué, we fill data from prefecture statistical yearbooks, to make the birth rate dataset as complete as possible. We obtain prefecture-level constant-quality urban house price indices from CityRE. We obtain our prefecture-level control variables from the Prefecture 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 and Appendix Table A.3

provides the variable definitions. The sample covers the unregulated prefectures that made up our treated group and control group of observations, and do not include observations in the regulated metropolises. 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 prefectures 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 presents the core empirical findings, estimating the causal effect of the house price shock on both birth rates at the prefecture level and newborns at the individual level. We report robust negative impacts of the positive house price shock on these fertility outcomes in treated prefectures. These results lay the foundation for the subsequent analysis of the mechanisms that drive this fertility response.

4.1 Prefecture-level birth rate responses

We first use the Statistical Communiqué data on prefecture-level birth rates and the CityRE urban house price indices to estimate regression model (1). In addition to controlling for pre-existing trends, prefecture 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 prefecture-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 (2020).

[Table 2 about here]

Urban house prices in treated prefectures abnormally increased by an average of 12.4% in the four years following the late 2016 house purchase restriction spillover shock, relative to control prefectures, as detailed in column (2) of Table 2. This estimation aligns with the patterns depicted in Figure 2, 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 prefectures, i.e. unregulated prefectures within 250 km of the nearest regulated city, swiftly surged away from control prefectures in after the house purchase restrictions were imposed in the regulated metropolises. The house price surge in treated prefectures relative to the control prefectures 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 prefectures imposing their own purchase restrictions in the third post-treatment year, curbing abnormal demand and possibly triggering further spillovers to control prefectures.

Prefecture-level birth rates in treated prefectures abnormally and significantly declined by an average of 1.68‰ compared to the control prefectures 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 (4) of Table 2. Considering the average prefecture-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 3 visually depicts the abnormal decline in the birth rate of treated prefectures, 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 prefectures, 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 prefecture-level birth rate in the treated prefectures was significantly different from trend in each of the four post treatment years.

The quasi-experimental estimate of the semi-elasticity of prefecture-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 prefecture-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 prefecture-level treatment effect. After the house purchase restriction spillover shock, the average number of newborns to

each women of childbearing age in the treated prefectures abnormally reduces by 0.025 (relative to a sample average of 0.061). This abnormal reduction in newborns after the positive house price shock is statistically significant at the 5% level.

Figure 4 reports the event study coefficients that show an abnormal reduction in the treated prefectures' 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 two of the three post-treatment years. 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 prefecture-specific pre-existing trends or treatment group-specific pre-existing trends, and whether we control for time-varying prefecture-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, (2) using a continuous distance specification where we allow treatment effects to linearly decay by the logarithm of the distance to the nearest regulated city, (3) matching the treated prefectures with control prefectures of similar pre-event characteristics, and (4) alternative assumptions regarding group-specific pre-existing trends.

Alternative Distance Cutoffs As the first set of robustness tests, we alternatively designate a unregulated prefecture as a treated prefecture if it is closer than 200 km (300 km) to the nearest regulated city. Panels (a) and (b) of Table 4 report the respective robustness 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 prefectures saw urban house prices abnormally increase, birth rate abnormally decrease, and individual-level newborns abnormally decrease.

Continuous-distance Specification In the second set of robustness tests, 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 spillovers from the imposition of house purchase restrictions. Hence, we expect a negative continuous treatment effect ϕ for urban house prices in unregulated prefectures, and a positive continuous treatment effect ϕ for birth rates and the number of newborns in unregulated prefectures.

[Table 4 about here]

That is exactly what we find, as detailed in panel (c) of Table 4, and graphically depicted in Figure 6. 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 ϕ's in columns 1–2. Graphically, there is a linear decay in abnormal price increases with the logarithm of the distance from the highest response in the nearest unregulated prefectures. The abnormal decrease in birth rates and in the number of newborns are also smaller with longer distance, as indicated by the positive ϕ's in columns 3–6. Graphically, there is a linear dampening in abnormal fertility reduction from the strongest reduction in nearest prefectures. 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.

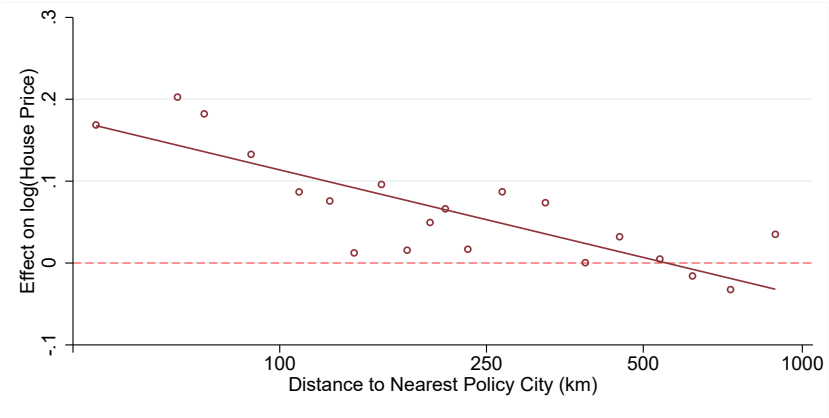
Difference-in-differences with Matching In the third set of robustness tests, we examine the robustness of the prefecture-level treatment effects to matching treated prefectures with control prefectures that were similar in observable characteristics. Appendix Table A.4 presents the results that are quantitatively unchanged. We match the treatment group prefectures with control group prefectures that are similar in the pre-event level (in 2015) of log urban house prices, log per capita local fiscal expenditure, log average wage income, log local population, and local per capita GDP growth. Before matching, the treatment prefectures, which are closer to the regulated metropolises, were larger, experienced faster economic growth, had higher house price levels, and similar wages, but lower fiscal expenditure per capita. After matching, no matched variable exceeds the Std-diff threshold of 0.25 or the variance ratio range of [0.5, 2.0] (Rubin, 2001), and we again find the house purchase restriction spillover shock led to a significantly lower birth rate in the treated prefectures.

Birth Rate among Women of Childbearing Age In the fourth set of robustness tests, we consider the robustness of the treatment birth decline to different trends in the number of women of childbearing age in the treatment prefectures. We use the 2010 and 2020 population census to compute the number of women of childbearing age in the treatment and control prefectures in the census years, and use linear interpolation and extrapolation to estimate the number of women of childbearing age in the 2009–2021 period except for the census years. We estimate the same event study regression in Figure 3 and report the robustness result in Appendix Figure A.2. The results are reassuringly unchanged—the birth rate among women of childbearing age in the treated prefectures diverged from the trend, declining sharply and persistently compared to control prefectures, one year after the quasi-experiment that abnormally increased urban house prices.

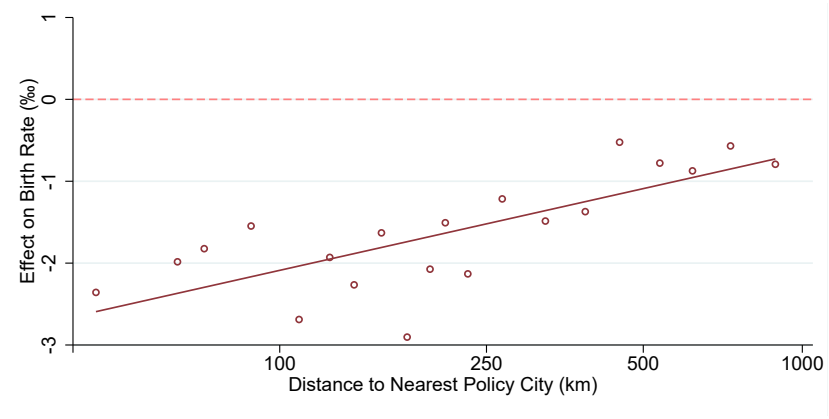
Perturbing Pre-existing Trend Assumptions In the fifth set of robustness tests, we examine the robustness of the treatment effects to alternative assumptions regarding group-specific pre-existing trends. First, we examine the robustness of the estimated treatment effects to an "one step up" test of the pre-trend assumption (Bilinski and Hatfield, 2020), specifically, the robustness to modeling the group-specific pre-existing trends in a more complex and flexible way us-

Figure 6: Spillover Effects of House Purchase Restrictions on House Prices, Prefecture-level Birth Rate and Individual-level Number of Newborns

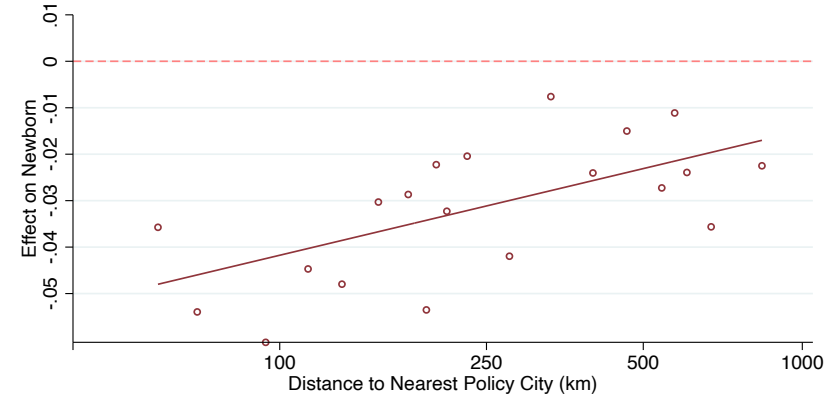
(a) Spillover Effects of House Purchase Restrictions: log(House Price)



(b) Spillover Effects of House Purchase Restrictions: Prefecture-level 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 prefecture as the distance from the nearest regulated city varies. The spillover effect on each prefecture 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 the prefecture-specific linear trend. Panel (a) plots the spillover effect on log urban house price. Panel (b) plots the spillover effect on prefecture-level birth rate. Panel (c) plots the spillover effect on individual-level number of newborns.

ing restricted cubic splines, allowing for non-linear group-specific trends. Appendix Table A.5 presents the results that show the estimated treatment effects are robust under non-linear group-specific trends in the form of restricted cubic splines.

Second, we examine the robustness of the estimated response of prefecture-level birth rate in treated prefectures relative to control prefectures after the positive urban house price shock, controlling for linear pre-existing trend difference (δ), to changes in the group-specific trend (Rambachan and Roth, 2023) in birth rates, in the form of:

$$\Delta^{SD}(M) := \left\{\delta \ : \ \left|\left(\delta_{t+1} - \delta_t\right) - \left(\delta_t - \delta_{t-1}\right)\right| \ \leq \ M, \ \forall t\right\},$$

i.e., allowing the differential trends in prefecture-level birth rate in treated prefectures relative to control prefectures to change each year by a maximum of M in the post period. Appendix Figure A.1 presents the result that shows that even allowing the group-specific trend to change annually by a bulk—three-fifths—of the entire pre-period average trend difference, the negative treatment effect of the house price shock on the birth rate is significant.

5. Mechanisms

We next assess whether the cost of living space or other costs urban house prices represent, such as access to education, account for the fertility effects. Also, we examine whether the reduction occurred primarily in married couples, whether there is also an effect on marriage itself, and whether the marriage margins of house prices' fertility effects are associated 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 prefectures. 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 (-0.031 with an s.e. of 0.044). Crucially, column (2) demonstrates a statistically and economically meaningful decline in newborns among rural residents without urban property (-0.038 with an s.e. of 0.012), significant at the 1% level. The fertility response of rural owners of urban homes is slightly positive (0.007 with an s.e. of 0.053), and the fertility response of urban homeowners is positive and close to zero (0.002 with an s.e. of 0.018), both statistically non-significant. These findings align with the pattern in Figure 5, 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. 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.

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 median 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 (-0.059 with a s.e. of 0.022) is statistically different (with a two-sided p-value of 0.083) from urban homeowners' fertility response, whereas for the "School Nearby" group, their fertility reduction is smaller in point estimate (-0.035 with a s.e. of 0.019) and corresponds to a smaller and statistically insignificant 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.

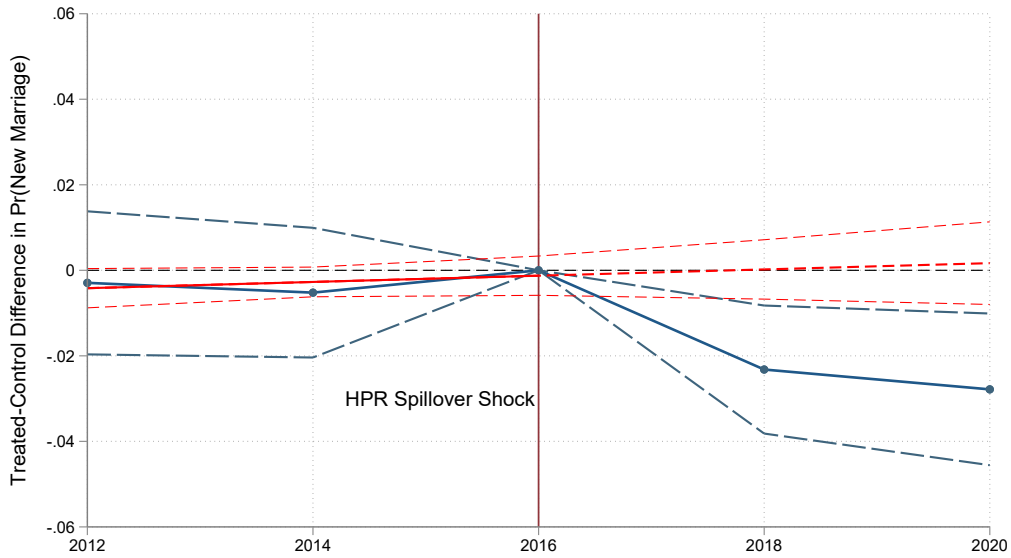
5.3 The competitive marriage market and the fertility decline

Wei and Zhang (2011) and Wei, Zhang, and Liu (2017) 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 of women of childbearing age.

[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 prefectures' individuals abnormally reduced by 0.039, significant at the 1% level. Figure 7 shows the event study for marriage rate, which shows a negative treatment response after the house purchase restriction spillover shock.

Figure 7: Preexisting Trends and Dynamic Responses in New Marriages (Individual-level)



Notes: This figure plots the estimated response of likelihood of new marriage of each individual in treated prefectures relative to control prefectures, both before and after the house purchase restriction spillovers shock. The response is estimated using event study regressions controlling for individual and year fixed effects and treatment-specific linear trends. The individual control variables are age, age^2, education level, party membership, urban residence, away-from-home 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 prefecture level. The house purchase restrictions in the regulated prefecture is labeled by the vertical red line. The sloped red solid line is the pre-treatment linear trend and its 95% confidence intervals. Data on new marriages is from the CFPS and available from 2012 to 2020 biennially.

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 (-0.033 with a s.e. of 0.017), significant at the 5% level. In contrast, rural inhabitants who own urban homes have the largest positive point estimate (0.030 with a s.e. of 0.060), albeit statistically insignificant. For urban homeowners, we observe a positive coefficient close to zero (0.001 with a s.e. of 0.031) 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.

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 prefectures within married individuals, as indicated by Panel (a) of Table 8. While the average treatment effect across rural and urban people is borderline insignificant, we find that the fertility decline in married individuals is statis-

tically 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 to concentrate in the group under 35 in Section 7.1. 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.

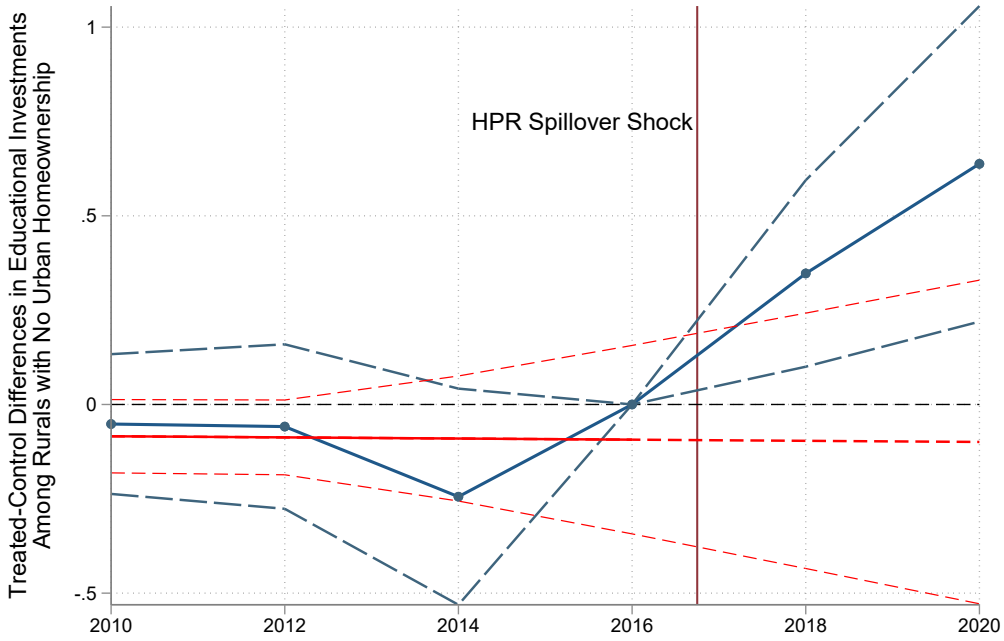
We use information from the 2010, 2012, 2014, 2016, 2018, 2020 biennial waves of CFPS and the household-level questionnaire. The questionnaire inquires about spending on children's education such as tuition, books, learning equipment, tutoring expenses for children younger than 14. We aggregate the household's 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 (0.568 with a s.e. of 0.169). 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. 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.

For rural inhabitants who own urban homes in addition to their rural dwellings, we find a sizable decrease in private educational investments, albeit the reduction is statistically insignificant. A reduction in private educational investments in this group is 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 make private 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, Hendren, Kline, and Saez, 2014; Heckman and Landersø, 2022).

Figure 8: 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 private educational investment on children, of each household in treated prefectures relative to control prefectures, both before and after the house purchase restrictions, among rural dwelling owners with no urban homeownership. The response is estimated using event study regressions controlling for household and year fixed effects and treatment-specific linear trends. The family control variables are urban residence, housing tenure, log per capita family net income, log total asset, and away-from-home status. 95% confidence intervals are drawn based on standard errors clustered at the prefecture level. The house purchase restrictions in the regulated metropolises is labeled by the vertical red line. The sloped red solid line is the pre-treatment linear trend and its 95% confidence intervals. Data on educational investments is from the CFPS and available from 2010 to 2020 biennially.

6. Model

In this section, we introduce a stylized dynamic model of fertility, housing, and education investment that rationalizes the observed empirical patterns. Our model builds on the standard fertility model (Barro and Becker, 1989; De La Croix and Doepke, 2003), and judiciously adds institutional assumptions that link urban housing ownership to marriage prospects and public educational resources. It demonstrates how rising house prices can lead to reduced fertility and increased educational investment, particularly through infra-marginal responses.

The model is based on the following assumptions. Individuals start their life cycle unmarried and resolve their marriage uncertainty during youth. During middle age, individuals make economic decisions. We assume that they are altruistic towards their children. Their utility depends on their own consumption, and separably on the number of children and utility of each child. They work, consume, purchase housing, produce offspring, and decide on investments in children's education. In old age, owned housing is divided among children through bequests. The

model serves as a lens to interpret the structural implications of our reduced-form findings.

6.1 Dynamic program of the full model

The Bellman equation of the dynamic program for middle-aged individuals is the following:

$$V(q_t, h_{t-1}^{ur}, h_{t-1}^{ru}, M_t) = \max_{\{c_t, h_t^{ur}, n_t, e_t\}} \ln(c_t) + \gamma \ln(h_t) + \alpha M_t + \beta a(n_t) n_t \cdot E_t V(q_{t+1}, (1-\delta)\frac{h_t^{ur}}{n_t}, h_t^{ru}, M_{t+1}). \tag{3}$$

The state variables for the dynamic program are $(q_t, h_{t-1}^{ur}, h_{t-1}^{ru}, M_t)$. In turn, they denote human capital, inherited urban housing, entitlement to rural housing, and marital status. The choice variables of the dynamic program are $(c_t, h_t^{ur}, n_t, e_t)$. In turn, they denote consumption, ownership of urban housing, number of offspring, and private investment in children's education.

The dynastic weight on offspring utility is $\beta a(n_t) n_t$. This is the product of the discount factor, the degree of altruism toward each child, and the number of children, the same as in Barro and Becker (1989) and Becker, Murphy, and Tamura (1990). The next generation's state variable for each offspring is $(q_{t+1}, (1-\delta)h_t^{ur}/n_t, h_t^{ru}, M_{t+1})$. The accumulated financial assets and ownership of urban housing increase the offspring's inherited financial assets and urban housing. Ruralites' offspring are entitled to rural housing. Private investment in education raises offspring's human capital. The number of children reduces the inheritance and private investment in education that each child receives.

The dynamic program is subject to a set of constraints and laws of motion. First, given urban house price p_t^{ut}, education cost p_t^e, and wage w_t, expenditures should not exceed the sum of income and inherited wealth: $c_t + p_t^{ur} h_t^{ur} + n_t e_t p_t^e \leq w_t q_t (1 - \phi n_t) + p_t^{ur} h_{t-1}^{ur}$. Second, urban housing has a minimum size: $h_t^{ur} \in \{0\} \cup [\underline{h}, \infty)$. Third, owned urban housing and entitled rural housing both produce housing consumption subject to an equivalent scale adjustment: $h_t = (h_t^{ur} + h_t^{ru}) \cdot h(n_t)^{-1}$. Fourth, only married individuals produce offspring: $n_t \in \{0, 1, 2, \ldots\}$ if $M_t = 1$ and $n_t \in \{0\}$ if $M_t = 0$. Fifth, private and public investments in education combine to produce human capital, the same as in De La Croix and Doepke (2003) and De la Croix and Doepke (2004), but urban housing ownership is related to more abundant public education investment (school quality): $q_{t+1} = (\bar{q}_t)^{1-\mu} \cdot (e_t)^{\mu}$, where $\bar{q}_t = \bar{q}$ if h_t^{ur} is zero and $\bar{q}_t = \bar{q}(1+\kappa) > \bar{q}$ if h_t^{ur} is not zero. Finally, an offspring's probability of getting married, π, is weakly increasing in inherited urban housing, the same as in Wei and Zhang (2011) and Wei, Zhang, and Liu (2017).

A higher p_t^{ur} can reduce fertility first by raising the marginal utility of consumption for those planning to buy more urban housing than they inherited ($h_t^{ur} > h_{t-1}^{ur}$), which is likely the case for ruralites, so that the shadow cost of fertility rises beyond the dynastic utility gain. This can be seen in Equation (4) below, the first-order condition for the number of offspring, where we

temporarily neglect integer restrictions on n_t, following Barro and Becker (1989).

$$
\begin{aligned}
[n_t]: \quad & \underbrace{\frac{1}{w_t q_t(1-\phi n_t) - p_t^{ur}\left(h_t^{ur} - h_{t-1}^{ur}\right) - n_t e_t p_t^e}}_{\text{MUC}} \cdot \left\{ \underbrace{\phi w_t q_t}_{\text{opp. cost}} + \underbrace{e_t p_t^e}_{\text{educ. cost}} \right\} \\
& + \underbrace{\gamma \frac{h'(n_t)}{h(n_t)}}_{\text{add'l. need for shelter}} + \underbrace{\beta a(n_t) n_t E_t\left[\frac{1}{c_{t+1}}\right] \cdot p_{t+1}^{ur} \cdot \frac{(1-\sigma)h_t^{ur}}{n_t^2}}_{\text{add'l. need for urban housing bequests}} \qquad (4) \\
& = \underbrace{\beta\left[a(n_t) + a'(n_t)n_t\right] E_t V(q_{t+1}, (1-\delta)\frac{h_t^{ur}}{n_t}, h_{t-1}^{ru}, M_{t+1})}_{\text{direct derivate of Barro-Becker dynastic utility}} \\
[h_t^{ur}]: \quad & \underbrace{\frac{1}{w_t q_t(1-\phi n_t) - p_t^{ur}\left(h_t^{ur} - h_{t-1}^{ur}\right) - n_t e_t p_t^e}}_{\text{MUC}} \cdot p_t^{ur} \qquad (5) \\
& = \underbrace{\frac{\gamma}{h_t^{ur} + h_t^{ru}}}_{\text{MUH}} + \underbrace{\beta a(n_t) \cdot E_t\left[\frac{1}{c_{t+1}}\right] \cdot (1-\delta) p_{t+1}^{ur}}_{\text{urban housing bequest value}}
\end{aligned}
$$

A higher p_t^{ur} potentially also reduces the dynastic utility gain from fertility by marginally reducing offspring's inherited assets. In the first-order condition for h_t^{ur}, when p_t^{ur} rises, ceteris paribus, h_t^{ur} has to decrease. This decrease in h_t^{ur} lowers $E_t V_{t+1}$, thereby reducing the Barro-Becker incentive for fertility. This can be seen in Equation (4) and (5) above.

Importantly, a higher p_t^{ur} can also cause an inframarginal effect on fertility and human capital. Ruralites exit the urban housing market, forgoing the benefits of school quality and marriage prospects, which affects fertility and educational investments. Without urban homeownership, the technology to produce dynastic utility through fertility is less efficient, reducing fertility (n_t). However, in this case, human capital becomes the key for intergenerational transfers. This can increase private educational investment (e_t) even under less efficient educational technology.

6.2 Dynamic program of the simplified model

While the previous model captures rich details and provides clear intuitions, it is challenging to derive analytical results on the inframarginal effect of an exogenous increase in p_t^{ur}. Here, we present a two-generation model to elucidate the inframarginal effect analytically.

We make simplification assumptions to focus on the mechanisms of action. We shut down utility from housing consumption and endogenous labor supply by setting $\gamma = 0$ and $\phi = 0$ since these mechanisms are not central to our results. We make h_t^{ur} fully depreciating by setting $\delta = 1$ to focus the benefits of urban homeownership on school access and marriage prospects. They are the main mechanisms of action in our reduced-form results. We assume a simple difference in the marriage probability by parents' urban homeownership: $\pi = \bar{\pi}$ if h_t^{ur} is zero and $\pi = \bar{\pi} + \sigma > \bar{\pi}$ if h_t^{ur} is not zero. We consider $n_t \in \{0, 1\}$ only, a simplification that still allows us to examine the decisions on fertility and private educational investment. The economy has two

generations. After the resolution of marriage uncertainty, generation t decides on consumption, urban housing purchase, fertility, and private educational investment: c_t, h_t^{ur}, n_t, and e_t. Because generation $t+1$ is the last generation, they resolve marriage uncertainty and consume, but make no home purchase and bear no children.

The simplified optimization problems for generation t and $t+1$ are as follows. We work from generation $t+1$ backward. Their utility is $V^{t+1}(q_{t+1}, M_{t+1}) = \ln(w_{t+1}q_{t+1}) + \alpha M_{t+1}$. Knowing this, we write the optimization problem that married generation t individuals solve, which is:

$$V^t(q_t, M_t) = \max_{\{h_t^{ur}, n_t, e_t\}} \ln(w_t q_t - n_t e_t p_t^e - p_t h_t^{ur}) + \alpha + \underbrace{\beta a(n_t) n_t \ln(w_{t+1}q_{t+1})}_{\text{offspring human capital}} + \underbrace{\beta a(n_t) n_t \alpha E_t\left[M_{t+1}\right]}_{\text{offspring marriage prospects}} \tag{6}$$

subject to

$$\begin{gathered} n_t \in \{0,1\},\, h_t^{ur} \in \{0\} \cup [\underline{h}, \infty), \\ q_{t+1} = \left(\bar{q}_t\right)^{1-\mu} \cdot \left(e_t\right)^{\mu}, \text{ where } \bar{q}_t = \bar{q} \text{ if } h_t^{ur} \in \{0\},\ \bar{q}(1+\kappa) \text{ if } h_t^{ur} \in [\underline{h}, \infty), \\ E_t\left[M_{t+1}\right] = \pi, \text{ where } \pi = \bar{\pi} \text{ if } h_t^{ur} \in \{0\},\ \bar{\pi} + \sigma \text{ if } h_t^{ur} \in [\underline{h}, \infty). \end{gathered}$$

In this simplified optimization problem, urban housing ownership matters inframarginally. Any h^{ur} greater than $\underline{h}$ is dominated by $h_t^{ur} = \underline{h}$. Thus, it suffices to analyze four options according to whether they buy an urban home and whether they have a child, and their respective conditional optimal strategies and values: (1) $h_t^{ur} = 0$, $n_t = 0$, (2) $h_t^{ur} = \underline{h}$, $n_t = 0$, (3) $h_t^{ur} = 0$, $n_t = 1$, (4) $h_t^{ur} = \underline{h}$, $n_t = 1$. Given the problem's simple form, analytically, the following are the optimal strategies and values corresponding to the four fertility and housing tenure options:

- Option 1 (do not buy an urban home and do not reproduce):
  $h_t^{ur} = 0$, $n_t = 0$, $e_t = 0$, $V_1 = \ln(w_t q_t) + \alpha$.
- Option 2 (do buy an urban home and do not reproduce):
  $h_t^{ur} = \underline{h}$, $n_t = 0$, $e_t = 0$, $V_2 = V_1 + \ln(1 - \Pi_t^{ur}) < V_1$,
  where $\Pi_t^{ur} \equiv \frac{p_t^{ur}\underline{h}}{w_t q_t}$ denotes the urban house price-to-income ratio.
- Option 3 (do not buy an urban home but do reproduce):
  $h_t^{ur} = 0$, $n_t = 1$, $e_t = \frac{\beta\mu}{1+\beta\mu}\frac{w_t q_t}{p_t^e}$, $V_3 = V_1 + A$,

  $$\text{where } A \equiv \underbrace{-\ln(1+\beta\mu)}_{\text{consumption reduction}} + \underbrace{\beta\ln(w_{t+1})}_{\text{offspring wage}} + \underbrace{\beta\mu\ln(w_t q_t) + \beta\mu\ln\frac{\beta\mu}{1+\beta\mu} - \beta\mu\ln p_t^e}_{\text{private educational investment}}$$
  $$+ \underbrace{\beta(1-\mu)\ln(\bar{q})}_{\text{public educational resources}} + \underbrace{\beta p_0 \alpha}_{\text{marriage prospects}}.$$
- Option 4 (do buy an urban home and do reproduce):

$$h_t^{ur} = \underline{h}, n_t = 1, e_t = \frac{\beta\mu}{1+\beta\mu}\frac{w_t q_t}{p_t^e}(1-\Pi_t^{ur}), V_4 = V_3 + B = V_1 + A + B,$$

$$\text{where } B \equiv \underbrace{\beta(1-\mu)\ln(q+\kappa)}_{\text{better public educational resources}} + \underbrace{\beta\sigma\alpha}_{\text{better marriage prospects}} + \underbrace{(1+\beta\mu)\ln(1-\Pi_t^{ur})}_{\text{Cost of urban homeownership}}.$$

Note that V_2 is strictly dominated by V_1, so the meaningful comparison is between V_1, V_3, and V_4. Let B' be the value of B after an exogenous increase in h_t^{ur}, the urban house price. By definition, $B' < B$. An exogenous increase in the urban house price reduces the value of buying an urban home and having a child. We have the following proposition.

Proposition 1 *A sufficiently large increase in the urban house price can cause inframarginal responses on housing tenure, fertility, and private educational investment in the simplified model focusing on school quality and marriage prospects. There is a one-to-one mapping between generation t rural individuals' preferences and endowment parameters and two strategic responses: (1) exit the urban housing market, with fertility unchanged, but increase private educational investment on children, (2) exit the urban housing market and reduce fertility.*

Proof of Proposition 1. The proof directly follows from the optimal strategies and values corresponding to the four fertility and housing tenure options above. Generation t rural individuals were in the urban housing market buying urban homes if and only if $B > 0$ (Option 4 $\succ$ Option 3) and $A + B > 0$ (Option 4 $\succ$ Option 1). On top of this condition, an exit from the urban housing market and fertility reduction occurs if and only if $A < 0$ and $A + B' < 0$. An exit from the urban housing market and an increase in private educational investment occur if and only if $A > 0$, and $A + B' < 0$. QED.

Given Proposition 1, with heterogeneity in rural individuals' preferences and endowments, this simplified model analytically generates that after a $h_t^{ut} \uparrow$ shock, fertility on average declines, but private educational investment in children born on average rises, Both patterns reflect strategic responses to prohibitive costs of better marriage prospects and alternative urban educational opportunities. In this way, the model offers a theoretical foundation and a lens to interpret our reduced-form results. This theoretical framework not only aligns with our empirical results but also supports our interpretation of the mechanisms identified earlier and informs the broader discussion to follow.

7. Discussions

This section discusses alternative explanations for the observed fertility decline and evaluates their plausibility relative to our findings. We also explore the aggregate implications of our quasi-experimental estimates of the effect of the house price shock on fertility.

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.

7.1 Alternative Explanation 1: Age composition

A possible alternative explanation for the observed decline in fertility within the rural areas of treated prefectures 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, or to other prefectures, such as the regulated prefectures. 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 rural aging alone accounts for our results. First, in all individual-level regressions, we controlled for age and age squared, so that even time-varying age composition should be controlled for. Even ignoring the age controls, any shift in the age composition of treated prefectures would need to coincide precisely with the timing of the treatment to account for the treatment effect, which is again highly unlikely.

Second, if fertility variations were solely attributable to changes in age composition, we should not expect any treatment effect within each age group. We directly test this prediction of the rural aging alternative hypothesis by estimating the treatment effects among the rural individuals with no urban homeownership conditioning on specific age groups. Table 11 detail the estimated treatment effects on the number of newborns. Contrary to the prediction of rural aging, the impacts are most pronounced conditional on the rural youth.

Specifically, in Table 11, when categorizing these women of childbearing age into two groups—those below advanced maternal age (under 35) and those of advanced maternal age (35 and over)—we find statistically significant negative treatment effects on both newborns and new marriages for the under-35 group. When we further refine the age groups, the largest and statistically significant treatment reduction in the number of newborns is observed within the 20–29 age group, and we find progressively less negative treatment effects on the 30-39 and the 40-44 age groups. These age group heterogeneity patterns are inconsistent with the rural aging hypothesis, whereas they are consistent with the interpretation that our main finding is the result of a fertility and marriage decline within the rural youth.

[Table 11 about here]

We observe economically significant treatment effects among younger populations in the treated prefectures. Specifically, when categorizing women of childbearing age into two groups—those below advanced maternal age (under 35) and those of advanced maternal age (35 and over)—we find statistically significant negative treatment effects on both newborns and new marriages for the under-35 group. When we further refine the age groups, the 20–29 age group shows the largest treatment effects on these outcomes, though the estimates are less precise.

These patterns suggest that the decline in fertility is primarily driven by the younger population in treated prefectures, who are experiencing reduced marriage and fertility rates, rather than by aging or older individuals reducing their fertility. Therefore, our findings do not support the age composition hypothesis as a plausible explanation for the observed fertility decline.

7.2 Alternative Explanation 2: Local migration or out-of-prefecture migration

Possibly, treated prefectures 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 prefectures 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 prefectures with a higher increase in urbanization rates from 2016 to 2021 exhibited a marginally significant rise in birth rates. Conversely, prefectures 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.

Another possibility is that the treated prefectures had more women of childbearing age migrating out of these prefectures to other prefectures, after the urban house price shock, so that the decline in the birth rate is the result of sorting on the remaining individuals driven by the migration response. We examine this possibility by estimating the response in out-of-prefecture migration among women of childbearing age after the shock. Appendix Table A.6 presents the results that show no statistically significant response in out-of-prefecture migration, so that there is no evidence that this possibility could explain the observed birth rate decline in the treated prefectures.

7.3 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 prefectures had a larger fertility increase before the house purchase restriction spillover shock. Then, a falling back in fertility may generate our result. Because the OCP was relaxed in all prefectures (regulated, treated, and control), this is a priori unlikely.

Nevertheless, we address this alternative by examining whether in response to OCP relaxation, treated prefectures 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 prefecture-level birth rates nor in the individual-level fertility data. Hence there was no differentially heightened response to OCP relaxation in treated prefectures 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.

7.4 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, McKay, Nakamura, and Steinsson (2021) 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 prefectures 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 prefectures linearly decays with the log distance to the nearest regulated prefecture. We constructed this figure as follow. We first estimate trend deviations in log urban house prices in each unregulated prefecture after the shock using a time-series regression for each unregulated prefecture. 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, prefectures 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 prefectures 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 prefectures. Applying the estimated semi-elasticity of -8.76‰, this corresponds to an average birth rate reduction of 0.73‰ across these affected prefectures.

With these prefectures 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 prefectures—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 $1\% \times \frac{8.76‰}{4.24‰}$.

8. Conclusions

By leveraging spillovers from the imposition of house purchase restrictions in large regulated metropolises, which redirected investment demand to nearby unregulated prefectures 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 prefecture-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. This impact was also exacerbated by the interaction between increased urban house prices and social norms that intensify marriage market competition, as well as by the scarcity of rural schools. We also found a positive treatment effect on private education investments among these rural people, suggesting a strategic adaptation to the changing educational options. The aggregate fertility effect size indicated by our quasi-experimental estimate is substantial.

Chetty, Hendren, Kline, and Saez (2014) indicate 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) show 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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Table 1: Summary Statistics

	Count	Mean	Std. Dev.	10th	50th	90th
Prefecture-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	80,177	0.61	0.49	0	1	1
Number of newborns	80,177	0.06	0.24	0	0	0
Age	80,177	30.27	8.48	18	30	42
Education level	80,177	2.24	1.38	0	2	4
Marital status	80,177	0.67	0.47	0	1	1
Ethnic minority	80,177	0.14	0.35	0	0	1
Party membership	80,177	0.05	0.21	0	0	0
Urban residence	80,177	0.31	0.46	0	0	1
Away household member	80,177	0.48	0.50	0	0	1
Health score	80,177	2.57	1.14	1	3	4
Housing tenure (own any dwelling)	80,177	0.91	0.29	1	1	1
Housing tenure (own multiple dwellings)	80,177	0.15	0.36	0	0	1
Per capita family net income	80,177	60,783	91,948	8,740	42,000	120,000
Mortgage debts	80,177	2,880	21,451	0	0	0
Individual-level marriage data (biennial frequency)						
Treat	30,989	0.61	0.49	0	1	1
New marriage	30,989	0.05	021	0	0	0
Age	30,989	29.99	8.29	19	30	42
Education level	30,989	2.36	1.40	0	2	4
Ethnic minority	30,989	0.14	0.35	0	0	1
Party membership	30,989	0.05	0.22	0	0	0
Urban residence	30,989	0.31	0.46	0	0	1
Away household member	30,989	0.48	0.50	0	0	1
Health score	30,989	2.73	1.09	1	3	4
Housing tenure (own any dwelling)	30,989	0.90	0.29	1	1	1
Housing tenure (own multiple dwellings)	30,989	0.16	0.37	0	0	1
Per capita family net income	30,989	65,503	92,484	9,560	48,400	128,300
Mortgage debts	30,989	3,375	23,603	0	0	0
Household-level education investment data (biennial frequency)						
Treat	28,354	0.55	0.50	0	1	1
Educational investment	28,354	1.38	2.57	0	0.46	3.73
Per capita family net income	28,354	53,706	92,718	6,400	37,500	110,000
Total asset	28,354	394,703	1,018,000	38,742	200,000	848,200
Mortgage	28,354	2,178	17,354	0	0	0

Notes: This table reports summary statistics for all the variables used in this paper. The prefecture-level data combine information from the Statistical Communiqué on Economic and Social Development for each prefecture and the CityRE constant-quality house price indices spanning from 2009 to 2020. The variable "Birth Rate" is prefecture-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 (Prefecture-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	10.723	10.723
R^2	0.971	0.940	0.877	0.820	/	/
Observations	2589	2589	2589	2589	2589	2589
Prefecture FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Prefecture Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
Prefecture 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 the 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 cities are the unregulated cities near the regulated cities, with a cutoff of 250 km. The control group cities are the unregulated cities farther away from the regulated cities. Considering that the change in fertility occurs later than the change in house prices, taking into account the pregnancy period, the sample period of the 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 prefecture in each year, in column (1) and column (2), birth rate in each prefecture 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 prefecture 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. Prefecture Trend denotes prefecture-specific linear trends, and the results of controlling for it are in the odd columns. Group Trend denotes treatment-group-specific linear trends, and the results of controlling for it are in the even columns. The prefecture-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 prefecture 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	-0.023**	-0.026**	-0.022**	-0.025**
	(0.010)	(0.010)	(0.010)	(0.011)
Mean	0.061	0.061	0.061	0.061
R^2	0.041	0.037	0.045	0.042
Observations	80177	80177	80177	80177
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Prefecture 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 the number of newborns with respect to the positive house price shock induced by the spillovers from the imposition of house purchase restrictions in the regulated metropolises (the HPR spillover shock) in the individual-level data. The sample consists of women of childbearing age in all unregulated prefectures. The treatment group are those in prefectures nearby the regulated metropolises, with a cut-off 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 prefecture 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. Prefecture Trend denotes prefecture-specific linear trends, and the results of controlling for it are in the odd columns. Group Trend denotes treatment-group-specific linear trends, 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, away-from-home 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 prefecture 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***	-0.020*	-0.022**
	(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	0.045	0.042
Observations	2589	2589	2589	2589	80177	80177
Prefecture FE	yes	yes	yes	yes	no	no
Individual FE	no	no	no	no	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Prefecture Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
Prefecture 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***	-0.021**	-0.023**
	(0.030)	(0.030)	(0.305)	(0.291)	(0.011)	(0.011)
Mean	8.544	8.544	10.723	10.723	0.061	0.061
R^2	0.971	0.940	0.877	0.819	0.045	0.042
Observations	2589	2589	2589	2589	80177	80177
Prefecture FE	yes	yes	yes	yes	no	no
Individual FE	no	no	no	no	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Prefecture Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
Prefecture 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***	0.012*	0.011**
	(0.016)	(0.016)	(0.173)	(0.170)	(0.007)	(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	0.045	0.042
Observations	2589	2589	2589	2589	80177	80177
Prefecture FE	yes	yes	yes	yes	no	no
Individual FE	no	no	no	no	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Prefecture Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
Prefecture 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 prefectures' urban house prices, birth rates, and number of newborns of individuals. Panels (a) and (b) use 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 prefectures. 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 prefecture-year level, and the dependent variables are the birth rate of each prefecture 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 prefecture 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 (2017) for birth rate and newborn (house price), which takes into account the pregnancy delay. Prefecture Trend denotes prefecture-specific linear trends, and the results of controlling for it are in the odd columns. Group Trend denotes treatment-group-specific linear trends, and the results of controlling for it are in the even columns. The prefecture-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 prefecture level. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, away-from-home 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 prefecture level.

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

	(1)	(2)	(3)	(4)
	Does Not Own Any	Rural (Does Not Own Urban Home)	Rural (Does Own Urban Home)	Urban (Does Own Urban Home)
Dependent Variable: Number of Newborns				
Treat × Post	-0.031	-0.038***	0.007	0.002
	(0.044)	(0.012)	(0.053)	(0.018)
R^2	0.047	0.058	0.006	0.017
Observations	6621	47715	7446	18078
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Prefecture 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 restriction spillover shock. The sample consists of all unregulated prefectures and the data span from 2009 to 2020. Regressions are at the individual-year level. The subsamples are the 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 prefecture 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, the first full year after the HPR spillover shock, taking into account the pregnancy delay. Prefecture Trend denotes prefecture-specific linear trends, and Group Trend denotes treatment-group-specific linear trends. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, away-from-home 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 prefecture 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	-0.059***	-0.035*
	(0.022)	(0.019)
R^2	0.061	0.066
Observations	11924	12218
Individual FE	yes	yes
Year FE	yes	yes
Prefecture 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 dwelling owners with no urban homeownership in counties where rural schools are spatially scarce, versus those in counties where rural schools are spatially less scarce. The sample consists of all unregulated prefectures 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 is determined by the median distance from home to local schools, as reported by rural dwelling owners with no urban homeownership with school-going children. If the county median home-school distance is larger than the national rural/urban-specific median, this county is designated as "Schools Distant" and vice versa. Treat is a dummy that takes the value 1 if the prefecture 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. Prefecture Trend denotes prefecture-specific linear trends, and Group Trend denotes treatment-group-specific linear trends. The individual-level control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, away-from-home 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 prefecture 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	-0.038***	-0.040***	-0.038***	-0.039***
	(0.013)	(0.013)	(0.013)	(0.013)
R^2	0.040	0.038	0.045	0.046
Observations	30989	30989	30989	30989
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Prefecture 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	-0.121	-0.033***	0.030	0.001
	(0.084)	(0.017)	(0.060)	(0.031)
R^2	0.103	0.078	0.153	0.070
Observations	1471	16835	1655	5770
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Prefecture 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 in the regulated metropolises (the HPR spillover shock). Panel (b) reports the heterogeneous treatment effects results, comparing rural and urban populations with various housing tenure statuses, differently impacted by the house purchase restrictions. The sample consists of all unregulated prefectures. The new marriage data spans from 2009 to 2020. The subsamples in panel (b) are the 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 marriages of each individual in each year. Treat is a dummy that takes the value 1 if the prefecture 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. Prefecture Trend denotes prefecture-specific linear trends, and Group Trend denotes treatment-group-specific linear trends. The individual control variables are age, age^2, education level, party membership, urban residence, away-from-home 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 prefecture level.

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

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

	(1) Newborns	(2) Newborns	(3) Newborns	(4) Newborns
Treat×Post	-0.030**	-0.033**	-0.019	-0.019
	(0.015)	(0.015)	(0.014)	(0.014)
Mean	0.076	0.076	0.076	0.076
R^2	0.067	0.064	0.082	0.080
Observations	55653	55653	55653	55653
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Prefecture 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	-0.065	-0.032**	0.017	0.007
	(0.062)	(0.016)	(0.077)	(0.024)
R^2	0.051	0.080	0.033	0.055
Observations	3835	33187	5507	12461
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Prefecture 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 report the difference-in-differences results and heterogeneous treatment effects results on the number of newborns in the married population impacted by the house purchase restriction spillover shock. The sample consists of all unregulated prefectures and the data span from 2009 to 2020. Regressions are at the individual-year level. The subsamples in panel (b) are the 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 prefecture 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, the first full year after the HPR spillover shock, taking into account the pregnancy delay. Prefecture Trend denotes prefecture-specific linear trends, and Group Trend denotes treatment-group-specific linear trends. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, away-from-home 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 prefecture 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	-0.061***	-0.018	-0.038**	-0.035	-0.057	-0.015	-0.053**	-0.012
se	(0.018)	(0.017)	(0.017)	(0.029)	(0.042)	(0.032)	(0.023)	(0.022)
Individual FE	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Prefecture 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	0.061	0.048	0.064	0.091	0.074	0.123	0.082	0.070
Obs	22159	25556	7762	9073	8207	9263	15449	17738

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 separately for rural dwelling owners with no urban homeownership living in areas with high versus low sex imbalance in the local marriage market, as proxied by sex ratio among the local marriage age population. The data on the number of newborns and the incidences of new marriages both span from 2009 to 2020. Regressions are at the individual-year level. Column (1) and column (2) show the effect on number of newborns among women aged 15-44. Column (3) and column (4) show the effect on the likelihood of new marriage among women aged 15-44. Column (5) and column (6) show the effect on number of newborns among men aged 15-44. Column (7) and column (8) show the effect on the likelihood of new marriage among men aged 15-44. Treat is a dummy that takes the value 1 if the prefecture 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 newborns, taking into account the pregnancy delay, and if the time is after or equal to the year 2017 for new marriages. Prefecture Trend denotes prefecture-specific linear trends, and Group Trend denotes treatment-group-specific linear trends. The individual control variables are age, age^2, education level, party membership, urban residence, away-from-home status, health score, and housing tenure. The family control variables are urban residence, housing tenure, log per capita family net income, log total assets, and migratory status. Standard errors are clustered at the prefecture level.

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

	(1)	(2)	(3)	(4)
		Rural	Rural	Urban
	Does Not Own Any	(Does Not Own Urban Home)	(Does Own Urban Home)	(Does Own Urban Home)
Dependent Variable: Educational Investments				
Treat×Post	0.342	0.568***	-0.669	0.004
	(0.801)	(0.169)	(1.082)	(0.510)
R^2	0.881	0.279	0.387	0.296
Observations	913	16768	1823	5145
Household FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Prefecture 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 metropolises, on parents' investments in children's education, separately for rural and urban populations with different housing tenure statuses. The educational investment 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 prefecture 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. Prefecture Trend denotes prefecture-specific linear trends, and the results of controlling this fixed effect are in columns (1) and (3) of panel (a). Group Trend denotes treatment-group-specific linear trends, 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 assets, out-of-home status (equals one if any household member is currently living out of the prefecture), and mortgage debt. Standard errors are clustered at the prefecture level.

Table 11: The Treatment Effect of the House Price Shock on the Number of Newborns s Among Rural Dwelling Owners with no Urban Homeownership, by Different Age Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	Age: 15-19	Age: 20-29	Age: 30-39	Age: 40-44	Under 35	35 and Over
Dependent Variable: Number of Newborns						
Treat×Post	0.001	-0.075**	-0.047*	0.011	-0.056***	0.001
	(0.014)	(0.030)	(0.025)	(0.012)	(0.018)	(0.015)
R^2	0.214	0.018	0. 038	0.018	0.052	0.020
Observations	6165	16232	15046	9951	30167	17534
Individual FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Prefecture 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: This table reports estimates of the urban house price shock's treatment effect on the number of newborns, among rural dwelling owners with no urban homeownership, segmented by different age groups. The sample consists of all unregulated prefectures, and the data on the number of newborns span from 2009 to 2020. Regressions are at the individual-year level. The age group subsamples are conditional on age from 15 to 19 in column (1), conditional on age from 20 to 29 in column (2), conditional on age from 30 to 39 in column (3), and conditional on age 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 conditioning on age under the AMA and over the AMA. Treat is a dummy that takes the value 1 if the prefecture 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. Prefecture Trend denotes prefecture-specific linear trends, and Group Trend denotes treatment-group-specific linear trends. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, away-from-home 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 prefecture 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		$X =$ Output Share of Secondary & Tertiary Industries		$X =$ Output Share of Secondary & Tertiary Industries (Excl. Agri. Services)		$X =$ Emp. Share of Secondary & Tertiary Industries	
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
Prefecture FE	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Prefecture Trend FE	yes	no	yes	no	yes	no	yes	no
Group Trend FE	no	yes	no	yes	no	yes	no	yes
Prefecture 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 prefecture-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 the year 2018. The alternative "treatment" designation variable in column (1) and column (2) is the urbanization rate represented by the proportion of the urban resident population to the whole resident population. The alternative "treatment" designation variable in column (3) and column (4) is the proportion of secondary and tertiary industries' output to total output. The alternative "treatment" designation variable in column (5) and column (6) is the proportion of secondary and tertiary industries' output (excluding agriculture-related services) to total output. The alternative "treatment" designation variable in column (7) and column (8) is the proportion of employment in the secondary and tertiary industries. Prefecture Trend denotes prefecture-specific linear trends, and the results of controlling for it are in the odd columns. Group Trend denotes treatment-group-specific linear trends, and the results of controlling for it are in the even columns. The prefecture-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 prefecture 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	0.009	0.008
	(0.321)	(0.293)	(0.012)	(0.012)
Mean	11.347	11.347	0.066	0.066
R^2	0.875	0.806	0.047	0.043
Observations	1799	1799	62911	62911
Prefecture FE	yes	yes	no	no
Individual FE	no	no	yes	yes
Year FE	yes	yes	yes	yes
Prefecture Trend	yes	no	yes	no
Group Trend	no	yes	no	yes
Prefecture Controls	yes	yes	no	no
Individual Controls	no	no	yes	yes
Family Controls	no	no	yes	yes

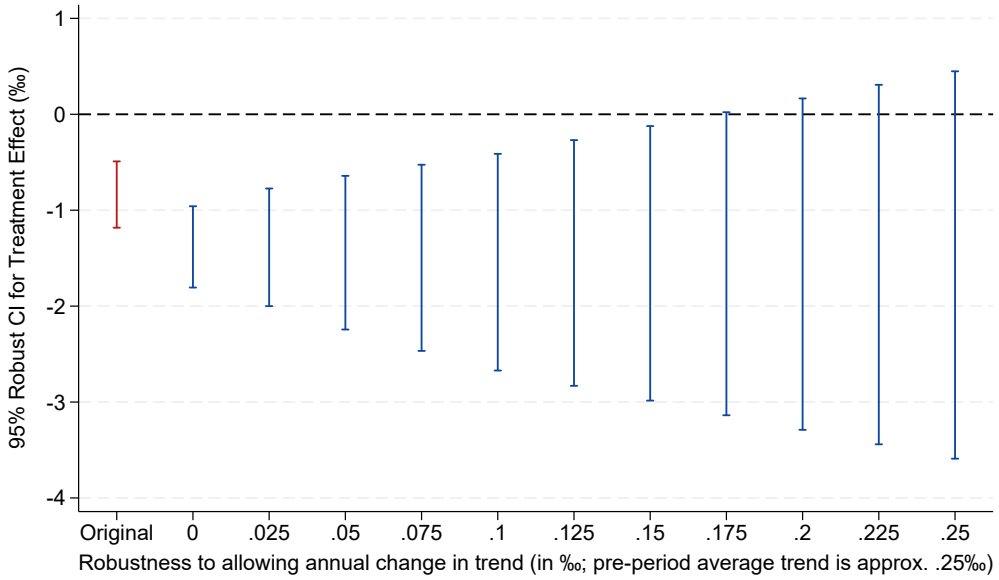
Standard errors in parentheses

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

Notes: This table reports the placebo test of one child policy's effect on the birth rate or the number of newborns. $\text{Treat}_{HPRSpillover}$ is a dummy that takes the value 1 if the prefecture 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 prefecture-year level. Regressions in columns (3) and (4) are at the individual-year level. Prefecture Trend denotes prefecture-specific linear trends, and the results of controlling for it are in the odd columns. Group Trend denotes treatment-group-specific linear trends, and the results of controlling for it are in the even columns. The prefecture-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 prefecture-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, away-from-home 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 prefecture level.

Appendix Figures and Tables

Figure A.1: Rambachan and Roth (2023) Test for Preexisting Trends and Dynamic Responses in the Birth Rate

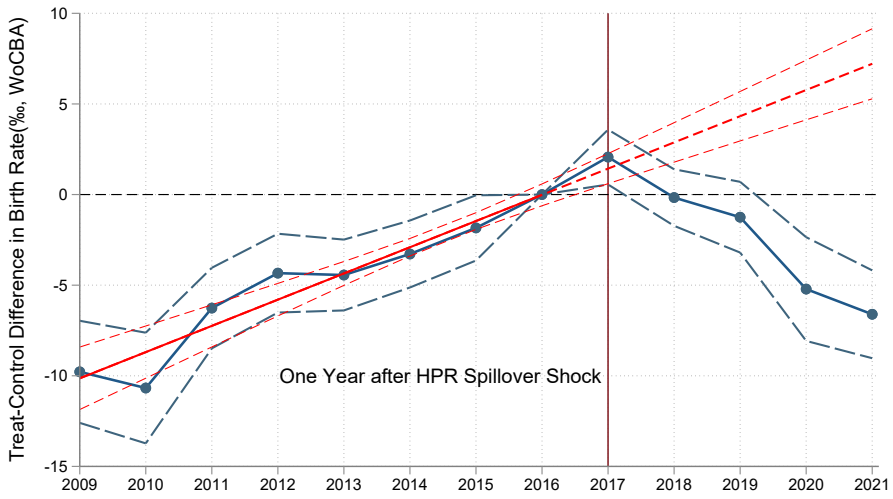


Notes: This figure plots the robustness of the estimated response of prefecture-level birth rate in treated prefectures relative to control prefectures after the positive urban house price shock, controlling for linear pre-existing trend difference (δ), to changes in the group-specific trend (Rambachan and Roth, 2023) in birth rates, in the form of:

$$\Delta^{SD}(M) := \left\{\delta \,:\, \left|\left(\delta_{t+1} - \delta_t\right) - \left(\delta_t - \delta_{t-1}\right)\right| \,\leq\, M,\ \forall t\right\},$$

i.e., allowing the differential trends in prefecture-level birth rate in treated prefectures relative to control prefectures to change each year by a maximum of M in the post period. The horizontal axis represents different assumptions about the sensitivity parameter M. Specifically, "Original" corresponds to the parallel trend assumption, "0" corresponds to our baseline assumption of linear trend differences, and "0.025", ..., "0.25" are different values of M that allows changes to the group-specific trends after the treatment shock. For example, $M = 0.25$ means that the group-specific trends in the prefecture-level birth rate in treated prefectures relative to the control prefectures are allowed to change by up to 0.25 births per thousand people per year. To put this number in context, the estimated pre-period differential trends in the prefecture-level birth rate in treated prefectures relative to the control prefectures *is* 0.25 births per thousand people per year. We observe in this figure that even allowing the group-specific trend to change annually by a bulk—three-fifths—of the entire pre-period average trend difference, the treatment effect is significant.

Figure A.2: Response to the HPR Spillover Shock: Prefecture-level Birth Rate among Women of Childbearing Age



Notes: This figure plots the estimated response of prefecture-level birth rate among women of childbearing age to the house purchase restriction spillover shock. The birth rate among women of childbearing age is the prefecture level number of births divided by the number of women of childbearing age (15-44). The number of women of childbearing age is estimated using data from the 2010 and 2020 population census. The response is estimated using event study regressions controlling for prefecture and year fixed effects and treatment-specific linear trends, and fiscal expenditure, average wage, population and per capita output growth. 95% confidence intervals are drawn based on standard errors clustered at the prefecture level. One year after the house purchase restriction spillover shock is labeled by the vertical red line to take into account of the pregnancy delay. Red upward sloping lines are the treatment-specific linear trend and its 95% confidence intervals.

Table A.1: First Round of House Purchase Restrictions

City	Policy Shock	Date Effective
Beijing	• Raise the down payment: from 35% to 40% for the 1st house; from 35% to 50%-70% for the 2nd house.	2016.9.30
Changsha	• Price-cap regulation: the average transaction price cannot increase further.	2016.11.25
Chengdu	• Raise the down payment: from 35% to 40% for the 2nd house.	2016.10.9
Fuzhou	• Raise the down payment: to 30% for the 2nd house.	2016.10.14
Guangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.1
Haikou	N/A	N/A
Hangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house in the prefecture center. • Raise the down payment: from 30%-40% to 50% for the 2nd house.	2016.9.20
Hefei	• Restrictions on resident purchases: cannot own more than 2 houses. • Raise the down payment: to 40%-50% for the 2nd house.	2016.10.1
Huizhou	N/A	N/A
Jinan	• Raise the down payment: from 20% to 30% for the 1st house; from 20% to 30%-40% for the 2nd house.	2016.10.2
Nanchang	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.8
Nanjing	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.9.25
Qingdao	N/A	N/A
Sanya	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.1
Shanghai	• Decrease credit supply (by rationing).	2016.10.19
Shenzhen	• Restrictions on purchases: cannot own more than 1 house. • Raise the down payment: to 30%-50% for the 1st house.	2016.10.4
Shijiazhuang	• Raise the land tax: to 3% for the 2nd house.	2016.10.1
Tianjin	• Restrictions on non-resident purchases: cannot own more than 1 house. • Raise the down payment: to 40% for the 1st house purchased by nonresidents.	2016.9.30
Wuhan	• Restrictions on non-resident purchases: cannot own more than 1 house. • Raise the down payment: to 25% for the 1st house; to 50% for the 2nd house.	2016.10.3
Wuxi	• Raise the down payment: to 40% for the 2nd house.	2016.10.2
Xiamen	• Restrictions on non-resident purchases: those who own 1 house can only purchase additional houses with areas larger than 180 m^2. • Restrictions on resident purchases: those who own 2 houses can only purchase additional houses with areas larger than 180 m^2. • Raise down payment: to 30% for the 1st house; to 40% for the 2nd house.	2016.10.5
Zhengzhou	• Restrictions on non-resident purchases: those who own 1 house can only purchase additional houses with areas larger than 180 m^2. • Restrictions on resident purchases: those who own 2 houses can only purchase additional houses with areas larger than 180 m^2. • Raise down payment: to 30% for the 1st house; to 40% for the 2nd house.	2016.10.2

Notes: This table enumerates the policy changes in the first round of house purchase restrictions in the regulated prefectures, the same as in Deng, Liao, Yu, and Zhang (2022). The policy information is collected from prefecture government announcements and the China Index Academy (a company collecting information on China's real estate market). The resident and non-resident purchases mentioned above are at the household level instead of the individual level. Nonresidents typically must reside in the prefecture and pay taxes for a certain period before being able to purchase a home.

Table A.2: Second Round of House Purchase Restrictions

City	Policy Shock	Date Effective
Beijing	• Raise the down payment: to 60%-80% for the 2nd house. • Decrease credit supply: stop providing mortgages lasting longer than 25 years.	2017.3.17
Changsha	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses. • Raise the down payment: to 30% for the 1st house; to 35%-40% for the 2nd house.	2017.3.18
Chengdu	• Restrictions on purchases: each family can only own 1 house.	2017.3.23
Fuzhou	• Raise the down payment: to 50% for the 2nd house. • Restrictions on resale: owner must hold a house for 2 years before resale.	2017.3.28
Guangzhou	• Raise the down payment: from 30% to 40%-70% for families that have ever applied for mortgages.	2017.3.17
Haikou	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resale: owner must hold a house for 2 years before resale.	2017.4.14
Hangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house in the prefecture area. • Restrictions on resident purchases: cannot own more than 2 houses in the prefecture area.	2017.3.3
Hefei	• Increase mortgage rate by 10%.	2017.3.20
Huizhou	• Increase mortgage rate by 10%.	2017.3.20
Jinan	• Raise the down payment: to 60% for the 2nd house. • Increase the mortgage rate by 10%. • Restrictions on resale: owner must hold a house for 2 years before resale.	2017.4.19
Nanchang	• Restrictions on non-resident purchases: raised criteria for purchases. • Restrictions on resident purchases: cannot own more than 1 house.	2017.3.8
Nanjing	• Restrictions on non-resident purchases: raised criteria for purchases. • Raise the down payment: from 30%-40% to 50% for the 2nd house.	2017.3.15
Qingdao	• Raise the down payment: from 20% to 30% for the 1st house; from 30% to 40% for the 2nd house.	2017.3.16
Sanya	• Raise the down payment: from 30%-40% to 50% for the 2nd house.	2017.3.11
Shanghai	• Decrease credit supply (by stricter rationing).	2017.3.17
Shenzhen	• Increase mortgage rate by 10%.	2017.3.20
Shijiazhuang	• Raise the down payment: to 30%-40% for the 1st house; to 50%-60% for the 2nd house.	2017.3.17
Tianjin	• Restrictions on non-resident purchases: raised criteria for purchases. • Restrictions on resident purchases: each individual cannot own more than 1 house. • Raise the down payment: to 40% for the 1st house purchased by nonresidents.	2017.3.31
Wuhan	• Increase mortgage rate by 10%.	2017.3.20
Wuxi	• Increase mortgage rate by 10%.	2017.3.20
Xiamen	• Restrictions on resident purchases: an individual can only own 1 house.	2017.3.24
Zhengzhou	• Restrictions on non-resident purchases: raised criteria for purchases.	2017.3.17

Notes: This table enumerates the policy changes in the second round of house purchase restrictions in the regulated prefectures, the same as in Deng, Liao, Yu, and Zhang (2022). The policy information is collected from prefecture government announcements and the China Index Academy (a company collecting information on China's real estate market). The terms "resident" and "non-resident" refer to household-level ownership except where otherwise indicated. Typically, nonresidents must live in the prefecture and pay taxes for a set period before they can purchase a home.

Table A.3: Variable Definitions

Variable	Definition
Treatment Variables	
$Treat$	Discrete indicator of treatment status. $Treat = 1$ if an unregulated prefecture is within 250 km of the nearest regulated city (i.e., one that implements house purchase restrictions on investment purchases), and 0 otherwise (control group). In robustness checks, we define $Treat$ with alternative distance cutoffs (200 km or 300 km).
$log(Distance)$	Continuous treatment measure: the logarithm of the distance (in km) from an unregulated prefecture to the nearest regulated city.
$Post$	Time indicator. For house price tests, $Post = 1$ from 2017 onward (the first full year after regulated cities implemented purchase restrictions). For birth rate tests, $Post = 1$ from 2018 onward, to account for pregnancy delays following the shock.
Prefecture-level Data	
$BirthRate$	Annual birth rate (per thousand) at the prefecture level, from Statistical Communiqués.
$log(HousePrice)$	Logarithm of the urban house price index at the prefecture level, from CityRE.
Control Variables	
$log(FiscalExpenditure)$	Logarithm of per capita fiscal expenditure at the prefecture level.
$log(AverageWage)$	Logarithm of the average wage at the prefecture level.
$log(Population)$	Logarithm of the population at the prefecture level.
$GRPGrowth$	Growth rate of per capita gross regional product at the prefecture level.
Individual-level Data	
$Newborns$	Number of newborns an individual has in a given year (tracked annually for women of childbearing age).
$NewMarriage$	Dummy=1 if the individual has entered a new marriage between interview waves, and 0 otherwise (measured biennially).
Control and Moderating Variables	
Age	Age of the individual.
$EducationLevel$	Highest education level (0=Illiterate, 1=Primary school, 2=Junior high school, 3=Senior high school, 4=Associate's, 5=Bachelor's, 6=Postgraduate).
$MaritalStatus$	Dummy=1 if the individual is married, and 0 otherwise.
$SpouseEducationLevel$	Highest education level of the individual's spouse, defined in the same categories as $EducationLevel$.
$EthnicMinority$	Dummy=1 if the individual is an ethnic minority, and 0 otherwise.
$PartyMembership$	Dummy=1 if the individual is a party member, and 0 otherwise.
$AwayHouseholdMember$	Dummy=1 if the individual is classified by CFPS as living away from the household's primary prefecture, and 0 otherwise. Household membership is determined by economic linkages (e.g., being a primary financial supporter or primarily supported by the household).
$HealthScore$	Self-reported health status (1=Most healthy, 2=Healthy, 3=Fairly healthy, 4=Average, 5=Least healthy).
Household-level Data	
$EducationalInvestment$	Total household spending on education for children under age 16 in the survey year (measured biennially).
Control and Moderating Variables	
$UrbanResidence$	Dummy=1 if the household's primary residence is in an urban area (under street office/resident committee jurisdiction), and 0 if in a rural area (under village committee jurisdiction).
$HousingTenure_Own$	Dummy=1 if the household owns at least one residence, and 0 otherwise.
$HousingTenure_Multiple$	Among property owners, dummy=1 if the household owns multiple properties, and 0 if it owns only one. Because rural individuals may only legally own one rural dwelling, if this variable equals 1 for a rural resident, it implies they own both a rural dwelling and at least one urban property.
$log(Income)$	Logarithm of per capita household net income.
$log(TotalAsset)$	Logarithm of total household assets.
$MortgageDebts$	Household's total outstanding mortgage debt in the survey year.

Notes: This table provides brief descriptions of the main variables used in the analysis. The prefecture-level birth rates are from Statistical Communiqués, and the prefecture-level urban house price indices are from CityRE. All other prefecture-level variables are from the Prefecture Statistical Yearbook. All individual- and household-level variables are from the China Family Panel Studies (CFPS).

Table A.4: DID Robustness Check: Matching Treatment Prefectures with Control Prefectures of Similar Covariates

(a) DID robustness check: Treatment Prefectures and Matched Control Prefectures

	(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	0.103***	0.101***	-1.288***	-1.514***		
	(0.029)	(0.030)	(0.285)	(0.292)		
log(House Price)					-6.706**	-10.028***
					(2.766)	(3.35)
Mean	8.544	8.544	11.156	11.156	11.156	11.156
R^2	0.969	0.939	0.877	0.811	/	/
Observations	2953	2953	2953	2953	2953	2953
Prefecture FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Prefecture Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
Prefecture Controls	yes	yes	yes	yes	yes	yes

Standard errors in parentheses

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

(b) Balance Test of Treatment Prefectures and Matched Control Prefectures

	Before Matching				**After Matching**			
Variable	**T Mean**	**C Mean**	**Std-diff**	**Var-ratio**	**T Mean**	**C Mean**	**Std-diff**	**Var-ratio**
$log(HousePrice)$	8.537	8.416	0.395*	1.138	8.537	8.498	0.132	1.285
	(0.315)	(0.296)			(0.315)	(0.278)		
$log(Population)$	15.167	14.826	0.555*	0.579	15.167	15.118	0.098	1.334
	(0.526)	(0.692)			(0.526)	(0.456)		
$GRPGrowth$	0.050	0.017	0.501*	0.301*	0.050	0.046	0.083	1.125
	(0.045)	(0.082)			(0.045)	(0.042)		
$log(AverageWage)$	10.849	10.867	-0.113	0.802	10.849	10.843	0.039	1.601
	(0.152)	(0.169)			(0.152)	(0.120)		
$log(FiscalExpenditure)$	8.928	9.116	-0.616*	0.820	8.928	8.955	-0.106	1.646
	(0.291)	(0.321)			(0.291)	(0.226)		

Notes: Panel (a) of this table reports the robustness checks of the difference-in-differences regressions of the prefecture-level birth rates and house prices with respect to the spillovers from the imposition of house purchase restrictions in the regulated metropolises (HPR spillover shock) to matching. We match the treatment group prefectures with control group prefectures that are similar in the pre-event level (in 2015) of log urban house prices, log per capita local fiscal expenditure, log average wage income, log local population, and local per capita GDP growth. The treatment group are the unregulated prefectures near the regulated prefectures, with a cutoff of 250 km. The control group are the unregulated prefectures farther away from the regulated cities. The dependent variables, the post-treatment timing, and the regression controls are identical to those in Table 2. Panel (b) report results of balance tests of the treatment prefectures and the matched control prefectures. The treatment prefectures, which are closer to the regulated metropolises, were larger, experienced faster economic growth, had higher house price levels, and similar wages, but lower fiscal expenditure per capita. Before matching, four variables exceed the standardized difference (Std-diff) threshold of 0.25 and one variable exceeds the variance ratio range of [0.5, 2.0]. After matching, no matched variable exceeds the Std-diff threshold of 0.25 or the variance ratio range of [0.5, 2.0]. Asterisks (*) indicate variables that exceed the standardized difference (Std-diff) threshold of 0.25 or the variance ratio range of [0.5, 2.0] (Rubin, 2001).

Table A.5: DID Robustness Check: "One Step Up" Test of Using Restricted Cubic Spline to Control for Pre-existing Trend Differences

	(1) log(House Price)	(2) log(House Price)	(3) Birth Rate(‰)	(4) Birth Rate(‰)	(5) Newborns	(6) Newborns
Treat×Post	0.64*	0.082**	-1.503***	-1.268***	-0.026**	-0.028**
	(0.033)	(0.032)	(0.409)	(0.368)	(0.012)	(0.012)
Mean	8.544	8.544	10.723	10.723	0.061	0.061
R^2	0.980	0.940	0.889	0.820	0.046	0.042
Observations	2589	2589	2589	2589	80177	80177
Prefecture FE	yes	yes	yes	yes	no	no
Individual FE	no	no	no	no	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Prefecture Trend	yes	no	yes	no	yes	no
Group Trend	no	yes	no	yes	no	yes
Prefecture 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 the prefecture-level urban house prices, birth rates, and number of newborns of individuals to an "one step up" test of the pre-trend assumption (Bilinski and Hatfield, 2020), specifically, the robustness to modeling the trend difference in a more complex and flexible way using restricted cubic splines. The sample consists of all unregulated prefectures. 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 prefecture-year level, and the dependent variables are the birth rate of each prefecture 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 prefecture 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 (2017) for birth rate and newborn (house price), which takes into account the pregnancy delay. Prefecture Trend denotes prefecture-specific restricted cubic splines, and the results of controlling for it are in the odd columns. Group Trend denotes treatment-group-specific restricted cubic splines, and the results of controlling for it are in the even columns. The prefecture-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 prefecture level. The individual control variables are age, age^2, education level, marital status, marital status×spouse's education level, party membership, urban residence, away-from-home 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 prefecture level.

Table A.6: Out-of-Prefecture Migration after the House Price Shock

Dependent Variable	(1) Migration Out-of-Prefecture	(2) Migration Out-of-Prefecture	(3) Migration Out-of-Prefecture	(4) Migration Out-of-Prefecture
Treat × Post	0.010	0.009	0.008	0.008
	(0.044)	(0.044)	(0.045)	(0.044)
Mean	0.084	0.084	0.084	0.084
R^2	0.581	0.505	0.626	0.548
Observations	38125	38125	38125	38125
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Prefecture 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 presents difference-in-differences regressions examining out-of-prefecture migration in response to the positive house price shock induced by the spillovers from house purchase restrictions. The sample comprises women of childbearing age in all unregulated prefectures. We define out-of-prefecture migration as an indicator variable equal to one if the individual's household primary prefectural residence changed from the initial 2010 survey; this information is available biennially. The out-of-prefecture migration tests assess whether the urban house price increase significantly prompted women of childbearing age to move away from treated prefectures. The treatment group are those in prefectures nearby the regulated cities, with a cutoff of 250 km. Treat is a dummy that takes the value 1 if the prefecture 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 same as in the individual-level number-of-newborn tests. Prefecture Trend denotes prefecture-specific linear trends, and the results of controlling for it are in the odd columns. Group Trend denotes treatment-group-specific linear trends, 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, away-from-home 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 prefecture level.