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

Ziqian Liu
Foster School of Business
University of Washington

Yu Zhang*
Guanghua School of Management
Peking University

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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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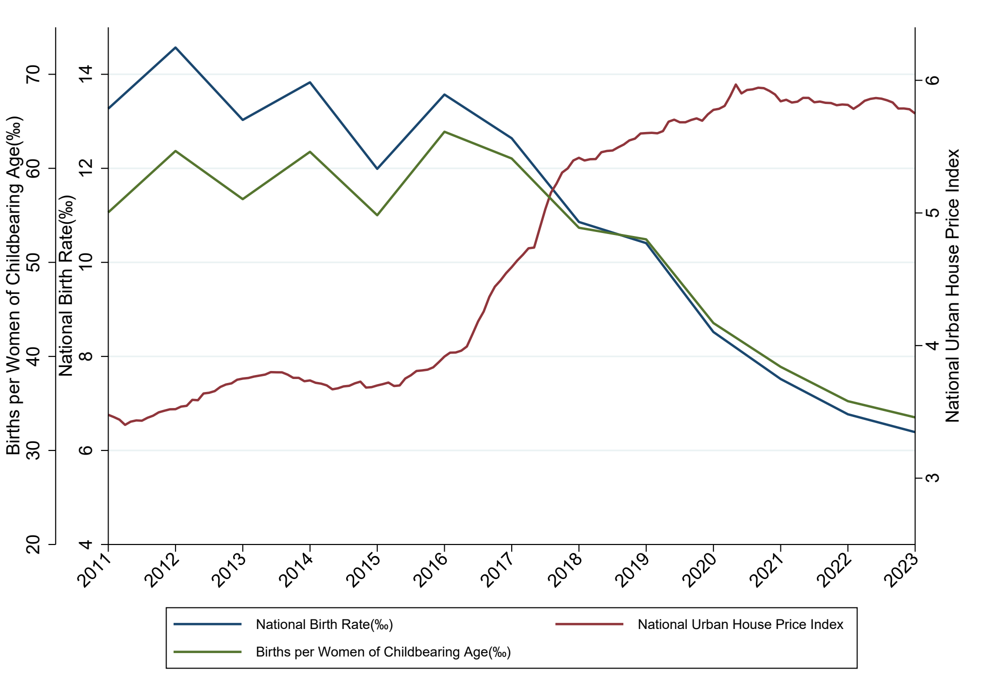
1. Introduction

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 aging 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 the authors' 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?

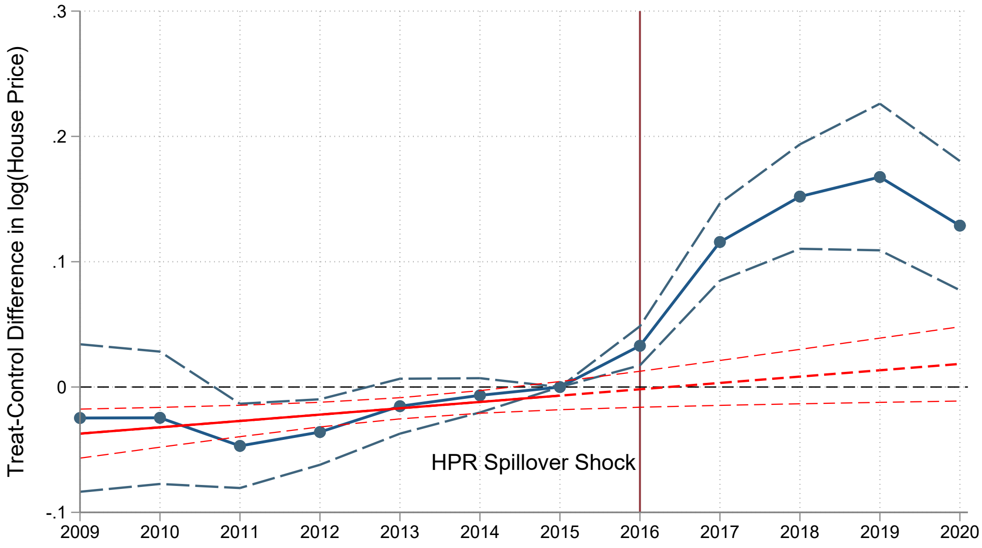
Studying this potential link is essential and challenging, as demographic shifts can influence house prices, which themselves reflect future expectations. Identifying valid instruments for house prices is difficult. For example, one commonly used strategy adopts instruments related to supply elasticity (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 in 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 collect prefecture-level birth rates from annual Statistical Communiqués released by prefecture governments. Using the urban house price quasi-experiment arising from house purchase restriction 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).

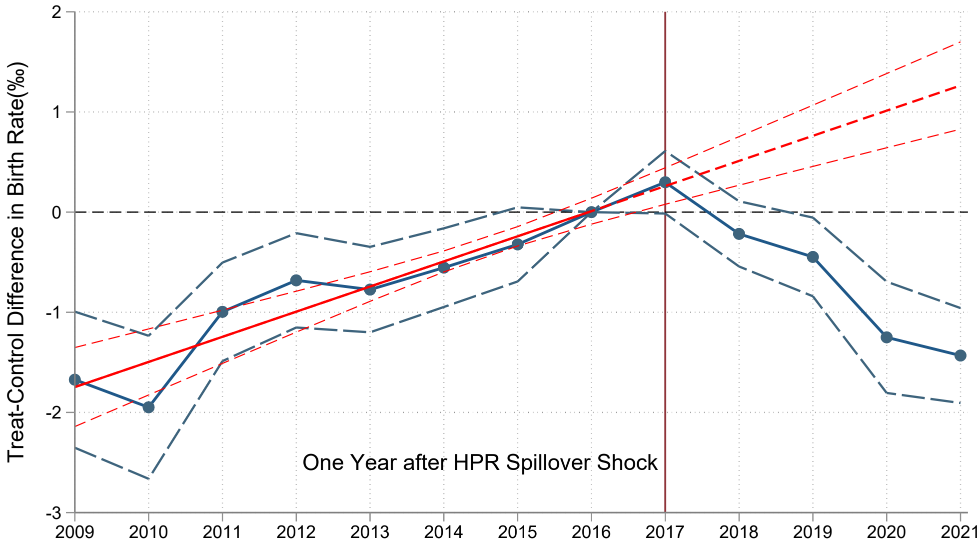
[1] "Prefectures" (or "prefectural cities") are administrative regions that encompass both urban areas and surrounding rural areas, similar to MSAs in the United States or départements in France. The findings of our study focus on individuals in both urban and rural parts of these prefectures.

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 red upward-sloping lines are the treatment-specific linear trend and its 95% confidence intervals. The vertical red line marks the implementation of house purchase restrictions in regulated prefectures. 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. The red upward-sloping lines are the treatment-specific linear trend and its 95% confidence intervals. The vertical red line marks one year after the house purchase restriction spillover shock to take into account the pregnancy delay. 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 observe 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, and had higher house price levels and similar wages, but lower fiscal expenditure per capita. To address these inherent differences, we control for these time-varying characteristics in addition to prefecture-fixed effects and prefecture-specific trends. Furthermore, we match treated prefectures with control prefectures that are comparable in these characteristics and obtain 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 use 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.

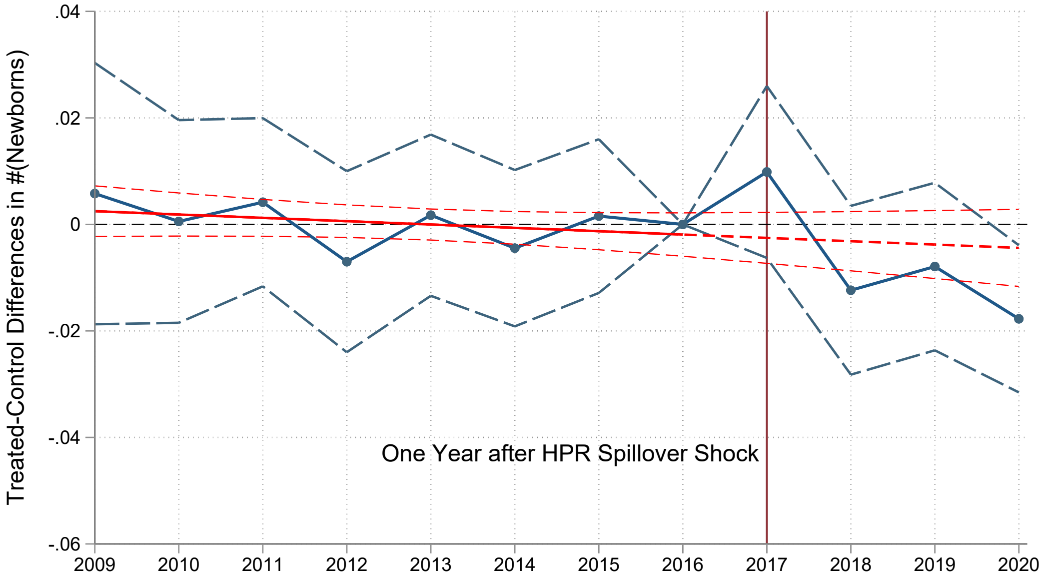
We apply recent methodologies from the difference-in-differences literature that examine the robustness of quasi-experimental estimates to non-linearity in pre-existing trends. Specifically, we follow approaches by Bilinski and Hatfield (2020) and Rambachan and Roth (2023), which allows for the same 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 the trend, we ensure that our estimates are robust to these tests.

We also consider 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 use data from the China Family Panel Studies (CFPS), a nationally representative survey, and reconstruct annual birth records from its biennial survey waves. We estimate 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 reveals a significantly negative treatment effect on individual-level births without observing significant pre-trends (Figure 4), further supporting our findings.

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 observe a significantly 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

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$\times$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. The red upward sloping line is the pre-treatment linear trend and its 95% confidence intervals. The vertical red line marks one year after the house purchase restrictions in the regulated metropolises to take into account the pregnancy delay. Data on the number of newborns is reconstructed from the CFPS and available from 2009 to 2020 annually.

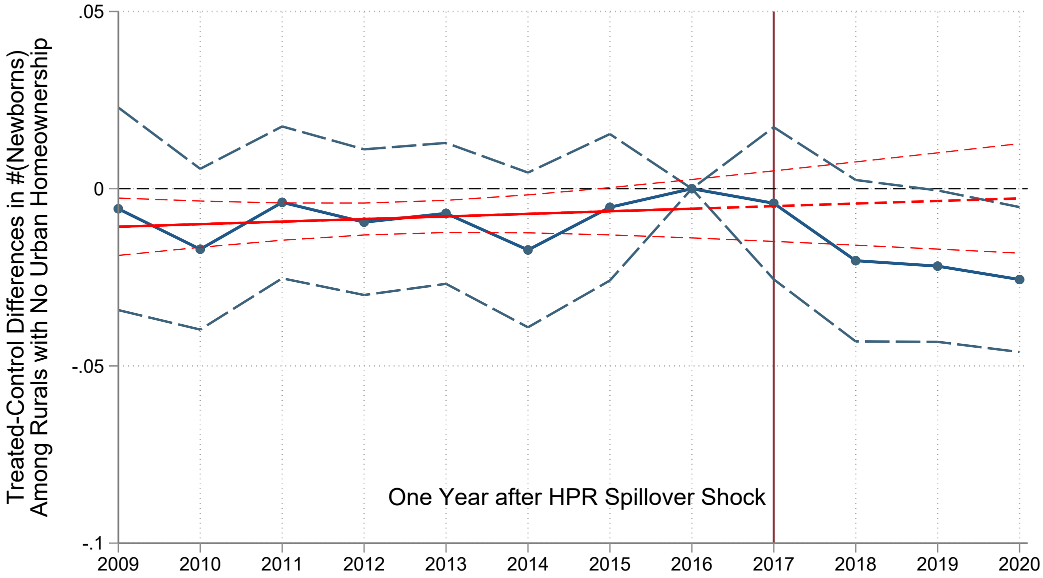
majority group in our sample. Notably, these individuals have land for shelter allocated by their villages according to the family size, so their shelter costs were not directly affected by the urban house price shock. Instead, the negative treatment effect on their birth rate is consistent with a new 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 do not observe a significantly 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 observe imprecise but consistent effects among two other groups. Mobile individuals who own neither rural nor urban homes locally exhibit an imprecise negative fertility effect, while rural individuals who own both rural and urban homes show a small, imprecise but positive fertility effect. These results are in line with our new channel.

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

[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.

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



Notes: This figure plots the estimated response of the 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×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. The red upward sloping line is the pre-treatment linear trend and its 95% confidence intervals. The vertical red line marks one year after the house purchase restrictions in the regulated metropolises to take into account the pregnancy delay. Data on the number of newborns is reconstructed from the CFPS and available from 2009 to 2020 annually.

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 reveals 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

[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.

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 appear 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 seem 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 unified 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, 2014). Some studies suggest 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 public education 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). Chetty, Hendren, Kline, and Saez (2014) suggest that 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. We document for the first time how rising homeownership 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.

Third, our study contributes to the finance literature on the real effects of house prices on investment behavior. Investment in children is one of the most important intertemporal investments. Existing studies have shown that shocks to house prices change relative returns, influencing corporate investment (Chaney, Sraer, and Thesmar, 2012; Martín, Moral-Benito, and Schmitz, 2021; Chen, Liu, Xiong, and Zhou, 2017), entrepreneurship (Corradin and Popov, 2015; Schmalz, Sraer, and Thesmar, 2017; Adelino, Schoar, and Severino, 2015), and equity investment (Qian, Tu, Wu, and Xu, 2020). Our theoretical model applies this insight within a multi-asset investment framework with heterogeneous returns, where the return on fertility—interpreted as intergenerational investment in human capital—increases with investment in housing ownership in high-opportunity areas. The analogy is that when homeownership requires a minimum size hurdle, house price shocks alter the relative returns on intergenerational investment in human capital. This dynamic asset allocation perspective enriches static models that consider only changes in relative prices.

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 estimated effect of house prices on prefecture-level and individual-level fertility. Section 5 presents subsequent analyses of the mechanisms driving these changes in fertility. Section 6 presents a full and a simplified dynamic model on the 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. In our research setting, 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.

In China, societal norms place an expectation on men to provide a stable and secure environment for their future families. Property ownership serves as a visible measure of financial stability and readiness to start a family (see, e.g., Chen and Zhang, 2019). Urban homes are the only tradable residential properties, making urban homeownership a key factor in marriage negotiations and family approval.[4] While rural land for shelter is allocated by village committees based on family size, rural dwellings are not tradable and have little liquidity.[5]

Urban homeownership is also closely linked to educational opportunities. A significant gap in educational attainment exists between urban and rural areas(Li, Loyalka, Rozelle, Wu, and Xie, 2015). Access to public schools in urban areas is often linked to property ownership within a school's catchment area. Eligibility follows a hierarchical system: highest priority is given to residents with all three qualifiers—local *hukou* (household registration, often tied to property ownership), property ownership, and residential location.[6] This is followed by those with two qualifiers, then those with only one, with *hukou* given priority. Individuals with only one qualifier are admitted only if space remains after accommodating those with two or three qualifiers.

In the years preceding 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 20% of rural individuals own urban homes, compared to over 90% of urban individuals. Consequently, it can be

[4] See https://www.chinadaily.com.cn/business/2012CEWC/2012-10/30/content_16010550.htm for results from a survey conducted in 2012 by house.ifeng.com, Horizon Research Consultancy Group and World Union Property Ltd, where two-thirds of the respondents bought homes before marriage. See also https://k.sina.com.cn/article_1712686623_6615861f027011c3w.html for results from a survey of rural individuals conducted in 2023 by CASS, Weibo, Wuhan University, and XinHuaNet on the importance of urban homeownership as a prerequisite for marriage, where 83% of rural respondents agreed with this view. Despite this widespread belief, only 17% of rural individuals in our micro-level sample actually owned an urban home.

[5] 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 (approximately 320 square feet). See https://www.yindu.gov.cn/2024/09-05/3099210.htmlfor an explanation of the non-tradability of rural homes and rural land for shelter.

[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.

hypothesized that a positive shock to urban house prices would disproportionately heighten barriers for rural individuals, 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. To curb overheating, policymakers in these large metropolises introduced restrictions to cool 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 could 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.[7]

After the house purchase restrictions were implemented, the regulated prefectures' investors could still purchase properties in the other prefectures that were 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 appeared to increase the house prices in the nearby unregulated prefectures without noticeable changes in the fundamentals,

[7] While the specific details varied across cities, HPRs were typically introduced as a policy package, combining multiple restrictions with a broader policy stance signaling the intent to curb speculative housing demand. Given this, we study the overall impact of the policy package rather than isolating the effects of individual measures.

as documented 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 price increases in the regulated metropolises were 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 about 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 within 250 km of the nearest regulated prefecture as treated prefectures to the house purchase restriction spillover shock, while those farther away serve as controls. The 250 km threshold corresponds to 2–3 hours by car or 1 hour by high-speed rail. Investment homes in the treated prefectures are easier for investors in the regulated prefectures to screen and occasionally monitor. These prefectures are at the level of commuting zones, meaning that they are also not near enough for commuting from the regulated prefectures.

Results remain robust to alternative distance cutoffs (200 km or 300 km). The discrete designation of treatment status reduces noise in 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 relevance of the treatment as an urban house price shock is shown in Figure 2. 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. Before the house purchase restriction spillover shock, urban house prices in treated prefectures increased stably, approximately 0.5% annually faster than the control prefectures. In contrast, the urban house price gap between the treated and control prefectures quickly rose to 9% relative to trend in 2017, 12% relative to trend in 2018 and 2019, and 9% relative to trend in 2020.

Several factors support the plausibility of exogeneity. Evidence indicates that the urban house price surge in treated prefectures was driven by external demand from regulated prefectures. Deng, Liao, Yu, and Zhang (2022) documented a sharp increase in web searches for real estate in nearby unregulated prefectures from regulated prefectures immediately after the imposition of house purchase restrictions. Consistent with external capital inflows, the urban house price increase in treated prefectures coincided with a rise in local bank deposits, suggesting purchases by non-local buyers.

There is no evidence that the house price surge in treated prefectures was driven by local economic fundamentals. If the increase were solely due to out-of-town investment demand, it should not significantly affect rents or tradable economic output. Deng, Liao, Yu, and Zhang (2022) tested responses in rents, output growth, and employment growth, finding estimates that were statistically insignificant and close to zero. Specifically, they found rents declined by 0.8% (t-value 0.67), output growth fell by 0.2% (t-value 0.20), and employment growth rose by 0.4% (t-value 0.96). While local urban real estate construction investment increased by 17.3%, it was not statistically significant (t-value 1.12). These findings suggest that the house purchase restriction spillover event constitutes a plausibly exogenous shock to urban house prices in the treated prefectures.

3.3 Regression specification

A challenge in estimating the effect of urban house prices on fertility is that different prefectures may follow different pre-existing trends. The urban economics literature has documented that cities exhibit heterogeneous growth paths (Blanchard and Katz, 1992; Glaeser, Scheinkman, and Shleifer, 1995), and recent applied-microeconometrics research stresses the need of allowing for flexible pre-trends in difference-in-differences designs (Rambachan and Roth, 2023).

Our identification strategy addresses this by allowing treatment-group-specific pre-trends, and we assess robustness to different ways of modeling them. Our identification assumption is that, absent the house purchase restriction spillover treatment, urban house prices and fertility outcomes in the treated and control prefectures would have continued along their pre-existing trends. To estimate treatment effects, we use the following extension of the difference-in-differences model from Wolfers (2006):

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

where α_i are prefecture (or individual) fixed effects, δ_t are time fixed effects, and $X_{i,t}$ are time-varying controls. The coefficient of interest, β, captures the treatment effect. In robustness checks, we alternatively control for $X_{i,t}$ using matching, and results remain unchanged.

To separate the treatment effect from pre-existing trends, we include $\gamma(i)t$, which delineates linear pre-existing trends. In all estimations, we include either treatment-group-specific trends or prefecture-specific trends. The results remain consistent. Following Bilinski and Hatfield (2020), we ensure that $\gamma(i)t$ is estimated only using pre-treatment data by saturating the model with post-period treatment-time interaction dummies. The treatment effect estimate, $\hat{\beta}$, is obtained as the average coefficient of these dummies. For robustness, we test alternative ways of modeling trends. We allow for **non-linear trends** following Bilinski and Hatfield (2020) and **post-treatment trend shifts** in the treatment group following Rambachan and Roth (2023). In both cases, the results remain robust.

We define the post-treatment period based on the expected response time of each outcome variable. For urban house prices, treated prefectures likely responded immediately after the house purchase restrictions were enacted in regulated metropolises in September 2016. Since the first full year affected was 2017, we define the post-treatment period for house prices as starting in 2017, using data through 2015 to estimate pre-trends.

For fertility, accounting for the pregnancy period, the first full year impacted was 2018, with partial effects in 2017. Thus, we assess treatment effects starting in 2018 and use data through 2016 to estimate pre-trends. We also analyze marriage rates and private educational investment, assuming these variables adjust as quickly as house prices.

To estimate the semi-elasticity of fertility to urban house prices, we use an instrumental variables (IV) approach 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 urban house prices, and estimate the predictive effect of last year's (log) local urban house prices on this year's birth rate.

3.4 Data and summary statistics

We combine prefecture-level and micro-level data to study the response of fertility to the house price shock. To ensure our results are not driven by measurement errors in any single source, we use multiple datasets.

At the prefecture level, we obtain annual birth rates from each prefecture's Statistical Communiqué on Economic and Social Development. These reports are published on local statistical bureau websites. We manually downloaded and scraped reports from 2009 to 2021. When birth rate data were missing, we supplemented them with prefecture statistical yearbooks to ensure completeness. We obtain prefecture-level constant-quality urban house price indices from CityRE and control variables from the Prefecture Statistical Yearbook.[8]

At the micro level, we construct individual- and household-level datasets using the China Family Panel Studies (CFPS), a biennial panel survey covering approximately 16,000 households across 25 provinces. Our analysis includes data from the 2010 (baseline), 2012, 2014, 2016, 2018,

[8]The CityRE index is based on urban house prices in both the prefectural city and county centers. Since some rural individuals purchase homes in county centers rather than the main city, this aggregation may introduce a mismatch and slightly underestimate the semi-elasticity. However, separate county-level house price indices are not available, making the prefecture-level index the best available measure.

and 2020 survey waves.

The CFPS classifies households as urban or rural based on their reported community type. Households in administrative village areas are classified as rural, while those in urban neighborhood communities are classified as urban. Treatment is assigned based on the household's location in the 2010 baseline survey. If the household resided in a treated prefecture in 2010, it remains assigned to the treatment group throughout the analysis, ensuring that migration after the initial survey does not affect treatment assignment. An alternative specification that assigns treatment based on residence in a treated prefecture in all survey waves produces similar results.

We construct an annual record of newborns for each individual using CFPS data on children's birth years. We verify the consistency of birth records across survey waves and find them to be highly reliable. The fertility analysis focuses on women aged 15 to 44, who account for over 99% of births in China. Economic controls are drawn from the biennial CFPS surveys, assuming values remain constant within each wave period. For example, controls from the 2010 survey apply to both 2009 and 2010 observations.

We construct a biennial dataset on new marriages using CFPS data on marital status transitions. If an individual was single in one wave and married in the next, the transition is recorded as a new marriage. This dataset covers survey years 2012, 2014, 2016, 2018, and 2020. The CFPS also provides data on private educational expenditures, which we use to construct a biennial household-level dataset covering the years 2010, 2012, 2014, 2016, 2018, and 2020.

The CFPS defines household members based on shared household finances and pooled living expenses rather than physical presence. Some individuals may be recorded as household members even if they are working away from home at the time of the survey. Since migrants often cannot afford housing in their work locations, their hometown housing market remains the relevant one for their fertility and homeownership decisions. This is consistent with our approach of defining treatment based on the baseline survey location of the household.

We classify individuals into four housing tenure groups based on CFPS survey responses. Rural dwelling owners without urban homeownership are those who own and reside in a home in a rural area but do not own urban property. Rural dwelling owners with urban homeownership own and reside in a home in a rural area and also own an urban property. Rural individuals can own only one rural dwelling in their home village; thus, these are identified as rural individuals with multiple homes. Urban homeowners own and reside in an urban home. Mobile individuals neither own a home in a local rural area nor an urban home. This group includes renters who were born outside the prefecture and do not own a local rural dwelling.

Table 1 presents summary statistics for the analysis sample and Appendix Table A.3 provides the variable definitions. The sample covers the treated and control groups of unregulated prefectures. Regulated prefectures are excluded from the analysis.

The average birth rate in the sample is 10.72 per thousand, and the average number of newborns per woman of childbearing age is 0.06, translating to a total fertility rate of 1.80. The urban population share is 31 percent, and the rural share is 69 percent, reflecting moderate urbanization in unregulated prefectures during the sample period.

[Table 1 about here]

4. The Effect of the House Price Shock on Fertility

This section presents the average treatment effect, estimating the effect of the house price shock on birth rates at both the prefecture and individual levels. We find a significant decline in fertility following the house price shock in treated prefectures. These results form the basis for the subsequent analysis of the mechanisms behind this fertility response.

4.1 Prefecture-level birth rate responses

We use Statistical Communiqué data on prefecture-level birth rates and 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 include time-varying controls, including log per capita fiscal expenditure, log per capita fiscal income, log population, and log per capita GDP.

Table 2 presents the results, 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 estimates are similar. Our preferred specifications are in the even-numbered columns, which control for treatment group-specific trends, following Bilinski and Hatfield (2020).

[Table 2 about here]

Urban house prices in treated prefectures rose abnormally by an average of 12.4% in the four years following the 2016 house purchase restriction spillover shock, relative to control prefectures, as shown in column (2) of Table 2. This estimate aligns with the patterns in Figure 2, which shows pre-existing trends and house price responses. House prices in treated prefectures, defined as unregulated prefectures within 250 km of the nearest regulated city, rose sharply relative to control prefectures after the house purchase restrictions were imposed in the regulated prefectures.

Prefecture-level birth rates in treated prefectures declined significantly by an average of 1.68 per thousand compared to control prefectures in the four years from 2018 to 2021, the first full years after the shock, accounting for the pregnancy period, as shown in column (4) of Table 2. Given an average birth rate of 10.72 per thousand in the sample period, this decline is economically meaningful.

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, significantly different from the pre-existing trend in each of the post-treatment years.

The estimated semi-elasticity of the birth rate with respect to urban house prices is -8.7 per thousand, statistically significant at the 1% level, as reported in column (6) of Table 2. A 10% exogenous increase in urban house prices is associated with a 0.87‰ decline in the birth rate.

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 addition 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 woman 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, (4) alternatively using the birth rate among women of childbearing age, and (5) alternative assumptions regarding group-specific pre-existing trends.

Alternative Distance Cutoffs As the first set of robustness tests, we alternatively designate an 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 A.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):

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

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 A.4 about here]

That is exactly what we find, as detailed in panel (c) of Table A.4, and graphically depicted in Figure 6. The longer 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 distances, 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 the 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.5 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, and 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 censuses 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.

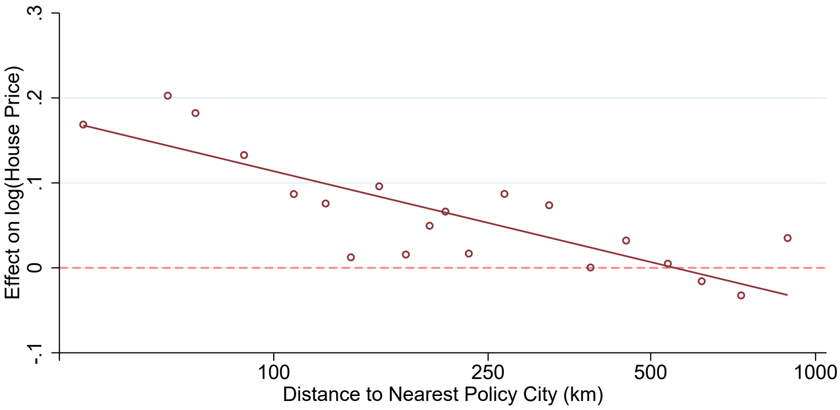
Perturbing Pre-existing Trend Assumptions In the fifth set of robustness tests, we examine the robustness of the treatment effects to alternative assumptions regarding group-specific pre-existing trends. First, we examine the robustness of the estimated treatment effects to an "one step up" test of the 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 using restricted cubic splines, allowing for non-linear group-specific trends. Appendix Table A.6 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:

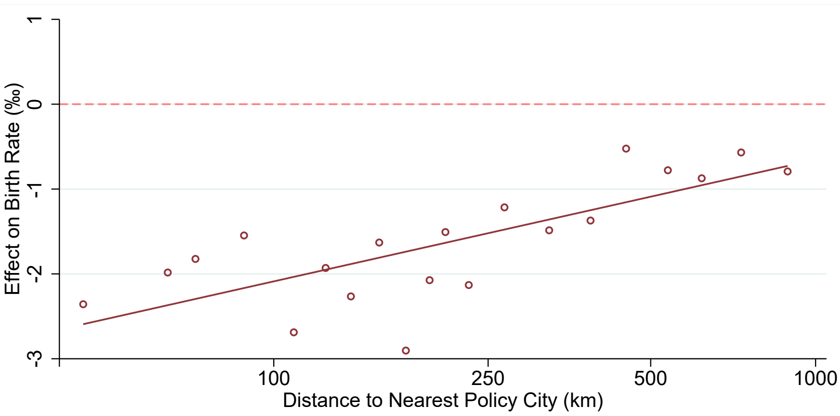
$$\Delta^{SD}(M) := \left\{\delta \ : \ \left|\left(\delta_{t+1} - \delta_t\right) - \left(\delta_t - \delta_{t-1}\right)\right| \ \leq \ M, \ \forall t\right\},$$

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

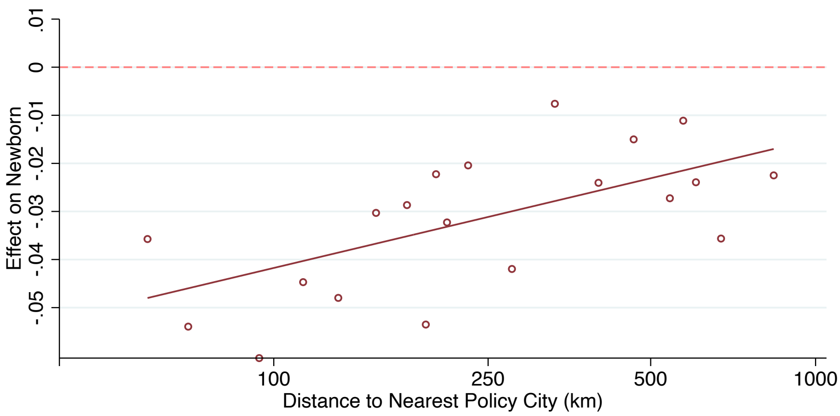
(a) Spillover Effects of House Purchase Restrictions: log(House Price)



(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 prices. Panel (b) plots the spillover effect on the prefecture-level birth rate. Panel (c) plots the spillover effect on the individual-level number of newborns.

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 examine the mechanisms driving the fertility decline in response to rising urban house prices. Specifically, we assess whether the effect is driven by the cost of living space or whether urban house prices also represent other costs, such as education access or signaling devices in marriage market competition.

To do so, we first estimate heterogeneous treatment effects across housing tenure groups. We then test whether local scarcity of educational resources and local sex ratio imbalances amplify the treatment effects. Next, we analyze marriage rates and fertility conditional on marriage to determine whether reduced fertility is partly due to lower marriage formation. Finally, we explore human capital formation by examining whether there is an intensive margin response in parental spending on children's education that accompanies the extensive margin fertility decline.

5.1 Rural aspirations for urban homeownership and the fertility decline

One unique feature of our setting is that rural individuals who own local rural dwellings are not directly affected by the urban house price shock in terms of living space costs. Rural dwellings in our sample prefectures are not tradable, and village collectives allocate land for shelter only to rural individuals born locally, based on family size. Rising urban house prices do not change this eligibility.

However, urban homeownership is tightly linked to access to urban schools and marriage prospects. As a result, rural residents may aspire to acquire urban homes despite already owning rural dwellings, particularly to secure better schooling opportunities for children and improve marriage market prospects. This distinction allows us to test whether fertility reductions are primarily driven by housing as shelter or by the indirect costs of urban homeownership.

To assess this, We estimate heterogeneous treatment effects across four subsamples: (1) rural dwelling owners without urban homeownership, (2) rural dwelling owners with urban homeownership, (3) urban homeowners, who own only urban homes, and (4) mobile individuals, who own neither local rural dwellings nor urban homes. Table 4 presents the results.

[Table 4 about here]

We find a large and statistically significant fertility decline among rural dwelling owners without urban homeownership in column 2 of Table 4 (-0.038, s.e. $= 0.012$, 1% level), for whom the cost of living is not directly affected by the urban house price shock. This pattern is consistent with Figure 5, where the fertility event study coefficients for this group remain consistently below the 95% confidence bands in post-treatment years (2018–2020).

These findings suggest that fertility declines are not simply driven by higher living space costs. If urban house prices affected fertility only through the cost of shelter, we would not expect the decline to concentrate among rural dwelling owners, whose direct housing costs remain unchanged by the urban house price shock. Instead, the results support the interpretation that urban house prices also reflect indirect costs, such as access to education resources and marriage market signaling. If rural dwelling owners aspire to buy urban homes for these reasons, rising urban house prices increase their financial burden, ultimately reducing fertility. This interpretation highlights the importance of a new channel—the educational and social amenity cost of housing—in explaining the link between house prices and China's birth rate decline.

The heterogeneous treatment effects for other groups, though imprecise, are consistent with this new channel. Urban homeowners show a near-zero response ($+0.002$, s.e. $= 0.018$), likely because two opposing forces—higher living space costs for new births and positive wealth effects from homeownership—offset each other. Rural dwelling owners with urban homeownership show a small, imprecise positive effect ($+0.007$, s.e. $= 0.053$), consistent with the idea that they may benefit under this new channel, though their small sample size limits statistical power. Mobile individuals, who neither own a home in a local rural area nor an urban home—including renters born outside the prefecture who do not own a local rural dwelling—exhibit a negative but statistically insignificant reduction in fertility (-0.031, s.e. $= 0.044$). This decline is also consistent with the new channel but is not uniquely explained by it. To further study the new channel that the educational and social amenity cost of housing affects fertility, we next examine two detailed factors that may amplify the fertility response to rising urban house prices: (1) the role of educational access, where school scarcity may heighten the need for urban homeownership, and (2) marriage market competition, where local sex ratio imbalances may intensify homeownership pressures as a prerequisite for marriage.

5.2 The distance to rural schools and the fertility decline

We further assess the education access channel, whether the stronger fertility reduction among rural individuals without urban homeownership is linked to the scarcity of rural schools. We hypothesize that in areas where rural schools are more scarce, access to urban schools may be more important, hence a rise in urban house prices could have a stronger effect on reducing the return to having children and on fertility. The CFPS questionnaire asked student subjects (or their parents) how long it takes to travel from home to school. Since this question is only answered by households with school-age children, while our sample includes all women of childbearing age, we proxy for school scarcity at the county level (one level below the prefecture) by calculating the rural- and urban-specific average home-to-school distance in the last pre-treatment year. Using individual-level distances would restrict the sample in a way that could bias the results.

To ensure relevance, we calculate the county-level school distance designation specifically for rural dwelling owners without urban homeownership. Counties where this distance exceeds the national median are classified as "School Distant" areas, while the rest are classified as "School Nearby" areas.[9] We then estimate the fertility responses separately for these two groups. of areas.

Table 5 reports the results by the local spatial scarcity of schools, where we focus on rural dwelling owners with no urban homeownership. To ensure the designation is relevant, we further restrict the sample to individuals who remained in the same county throughout the sample period. We find that the fertility reduction is larger and more statistically significant in counties where rural schools are more distant.

[Table 5 about here]

Among rural dwelling owners with no urban homeownership in School Distant areas, the estimated fertility reduction is -0.059 (s.e. $= 0.022$), and the difference from urban homeowners' fertility response is statistically significant (two-sided p-value $= 0.083$). In School Nearby areas, the estimated fertility reduction is smaller in magnitude (-0.035, s.e. $= 0.019$) and the gap from urban homeowners is smaller and statistically insignificant. These results support the idea that rural individuals' fertility response to rising urban house prices is linked, in part, to the relative scarcity of educational resources.

5.3 The competitive marriage market and the fertility decline

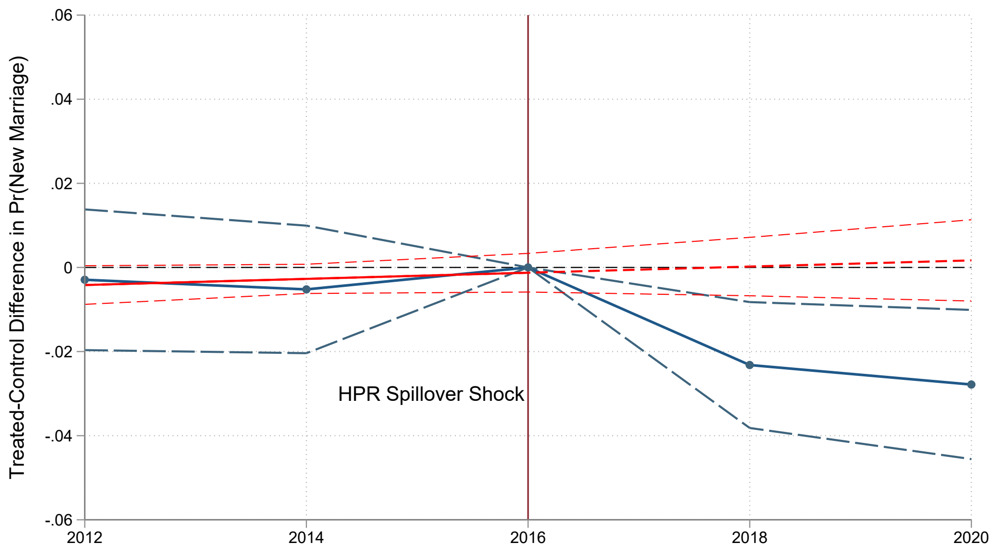
We next study whether marriage market norms may have amplified the fertility effects of rising urban house prices. We do so by examining whether the fertility effect of the urban house price shock is linked to marriage and the competition in the marriage market.

A growing literature highlights the role of homeownership in shaping marriage market outcomes (Wei and Zhang, 2011; Wei, Zhang, and Liu, 2017). In a competitive marriage market, homeownership serves as a signal of financial stability and marriage eligibility. A rise in urban house prices reduces the share of men who can afford homeownership, increasing friction in the marriage market. For women of childbearing age in our sample, this makes finding a partner more difficult, reducing marriage rates and, ultimately, fertility.

Treatment effect on marriage rates To assess this, we estimate the effect of the house price shock on new marriages using an individual-level sample of women of childbearing age. We use

[9] Ding, Wang, and Ye (2016) analyzed local governments' incentives for rural school closures and consolidation, which have 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 highlight two key incentives: (1) reducing education expenditure and facilitating educational management, and (2) encouraging rural-to-urban migration to promote urbanization.

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



Notes: This figure plots the estimated response of the likelihood of new marriage 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 include age, age^2, education level, party membership, urban residence, away-from-home status, health score, and housing tenure. Family controls include per capita family net income and mortgage debt. Standard errors are clustered at the prefecture level, and 95% confidence intervals are shown. The sloped red solid line is the pre-treatment linear trend and its 95% confidence intervals. The vertical red line marks the implementation of house purchase restrictions in regulated prefectures.

the biennial CFPS survey data to construct biennial records on new marriages available from 2012 to 2020. Table 6 reports the results. We find that marriage rates respond in the same pattern as fertility rates.

Specifically, the average treatment effect, reported in Panel (a) of Table 6 and illustrated in Figure 7, shows that after the house purchase restriction spillover shock, the likelihood of new marriage among individuals in treated prefectures significantly reduced (-3.9 pp, 1% significance).

Panel (b) of Table 6 shows a large decline in marriage rates among rural individuals without urban homeownership (-3.3 pp, 5% significance). Urban individuals were unaffected (+0.1 pp), while rural individuals who own urban homes exhibited an imprecise but large increase (+3.0 pp), consistent with the interpretation that owning an urban home benefits marriage entry. The largest decline in marriage rate is observed among mobile individuals with neither rural nor urban homeownership (-12.1 pp, imprecise).

These findings suggest that rising urban house prices disproportionately affected those unable to signal financial stability through urban homeownership, making marriage formation more difficult. The resulting decline in new marriages directly contributed to the fertility reduction.

[Table 6 about here]

Marriage market competition and the treatment effects The effects of the urban house price shock on marriage formation and fertility may be even stronger in areas with higher marriage market competition. In some prefectures, cultural preferences have led to higher local sex ratios (more men than women), increasing the need for men to signal marriage eligibility through urban homeownership. If urban house prices rise, fewer men can afford this signal, intensifying marriage market frictions.

To test this, we construct a prefecture-level sex ratio measure based on the cohorts born between 1981 and 2000. These cohorts cover every sample individual aged 20–35—the primary marriage age—during the post-treatment period. We split the sample of rural dwelling owners without urban homeownership into high and low marriage competition groups based on whether the local sex ratio is above or below the national median. We reestimate the treatment effects in the high group and the low group. Table 7 reports the results.

[Table 7 about here]

The findings in Table 7 are consistent with the interpretation that social norms in the marriage market amplified the effect of the house price shock. We find that (a) prefectures with a higher sex ratio experience a stronger fertility decline (columns 1 and 2), (b) higher sex ratios are associated with a larger decline in new marriages among rural men (columns 5 and 6).

These results support the competitive savings motive proposed by Wei and Zhang (2011): in areas with more men relative to women, marriage market competition intensifies, making urban homeownership an even stronger prerequisite for marriage. Rising urban house prices therefore exacerbate marriage delays and fertility declines in high-competition areas.

In contrast, Table 7 reports no economically significant difference in rural women's marriage rate declines (columns 3 and 4). This is expected since marriage market competition affects men more directly. However, conditional on getting married, rural women without urban homeownership reduce their fertility more significantly in prefectures with high sex ratios (columns 7 and 8).

These findings also raise potential normative implications. While declining marriage rates and fertility could reflect constraints imposed by higher house prices, they may also indicate shifts in bargaining power within marriage (e.g., Doepke and Kindermann, 2019; Ashraf, Field, Voena, and Ziparo, 2022). In high-competition areas, women may face fewer constraints in marriage formation and may have greater autonomy in fertility decisions. Whether these changes reflect negative welfare consequences remains an open question.

Fertility decline among married couples In addition to possible shifts in bargaining power, the education access channel also predicts married couples without urban homeownership to reduce fertility. In Table A.7, we report the treatment effects of the urban house price shock on fertility among the married more systematically. While the overall fertility effect among married individuals is borderline insignificant (-0.019, s.e. $= 0.014$), the effect is significant for rural dwelling owners without urban homeownership (-0.032, 5% significance). The remaining groups show a consistent pattern as before: rural individuals with urban homeownership exhibit an imprecise increase, urban homeowners show no response, and mobile individuals have a large but imprecise decline.

Taken together, our findings suggest that the urban house price shock reduces fertility through both (1) marriage market frictions, which lower the rate of new marriages, and (2) lower returns to childbearing or shifts in bargaining power, which reduce fertility even among married couples. The strongest effects were observed among rural dwelling owners without urban homeownership, the group most affected by rising home prices as a marriage signal. In areas with higher male-to-female sex ratios, these patterns were even more pronounced. These results support the idea that the fertility response to rising urban house prices is associated with the role of urban homeownership (and the increasing difficulty of obtaining it) as a signaling device in the marriage market.

5.4 Treatment effects on private education investments

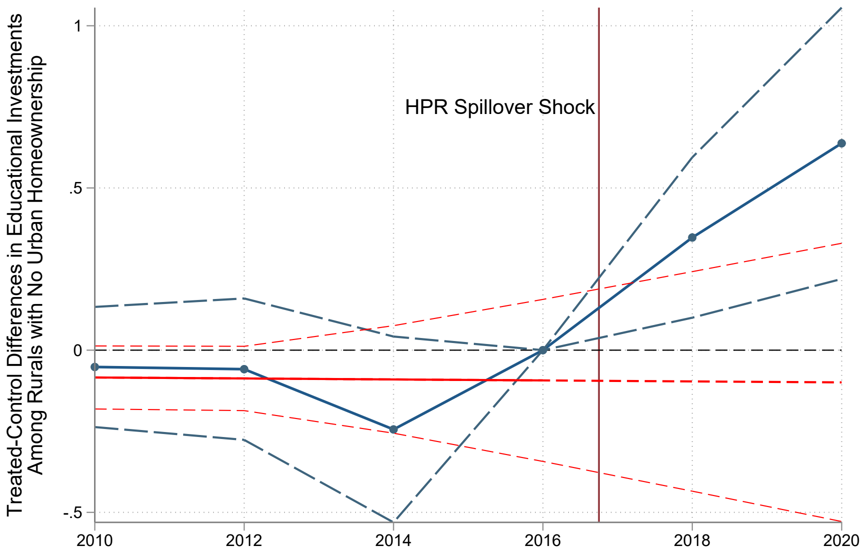
One view of fertility decline is that it does not necessarily lead to lower human capital formation (e.g. Barro and Becker, 1989; Becker, Murphy, and Tamura, 1990; Galor and Weil, 2000). Instead, quality investment may offset or even dominate the effect of fertility decline. We are interested in whether 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 in children. Using this information, we estimate the treatment effect of the house price shock on parent's educational investment in 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 in children.

Table A.8 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 siz-

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 in 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 sloped red solid line is the pre-treatment linear trend and its 95% confidence intervals. The vertical red line marks the implementation of house purchase restrictions in regulated prefectures. Data on educational investments is from the CFPS and available from 2010 to 2020 biennially.

able 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).

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:

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

The state variables for the dynamic program are $(q_t, h^{ur}_{t-1}, h^{ru}_{t-1}, M_t)$. In turn, they denote human capital, inherited urban housing, entitlement to rural housing, and marital status. The choice variables of the dynamic program are $(c_t, h^{ur}_t, n_t, e_t)$. 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 $(q_{t+1}, (1-\delta)h^{ur}_t/n_t, h^{ru}_t, M_{t+1})$. 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^{ut}_t, education cost p^e_t, and wage w_t, expenditures should not exceed the sum of income and inherited wealth: $c_t + p^{ur}_t h^{ur}_t + n_t e_t p^e_t \leq w_t q_t (1 - \phi n_t) + p^{ur}_t h^{ur}_{t-1}$. Second, urban housing has a minimum size: $h^{ur}_t \in \{0\} \cup [\underline{h}, \infty)$. Third, owned urban housing and entitled rural housing both produce housing consumption subject to an equivalent scale adjustment: $h_t = (h^{ur}_t + h^{ru}_t) \cdot h(n_t)^{-1}$. 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^{ur}_t is zero and $\bar{q}_t = \bar{q}(1+\kappa) > \bar{q}$ if h^{ur}_t 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).

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

$$
\begin{aligned}
[h_t^{ur}]: \quad & \underbrace{\frac{1}{w_t q_t(1-\phi n_t) - p_t^{ur}\left(h_t^{ur} - h_{t-1}^{ur}\right) - n_t e_t p_t^e}}_{\text{MUC}} \cdot p_t^{ur} \\
& = \underbrace{\frac{\gamma}{h_t^{ur} + h_t^{ru}}}_{\text{MUH}} + \underbrace{\beta a(n_t) \cdot E_t \left[\frac{1}{c_{t+1}}\right] \cdot (1-\delta) p_{t+1}^{ur}}_{\text{urban housing bequest value}}
\end{aligned} \tag{5}
$$

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. Instead of marriage probability being a weakly increasing function in owned urban housing in the general model, 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

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

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, 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 A.9 details 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 A.9, when categorizing rural individuals with no urban homeownership into two groups—those under 35 (below advanced maternal age) and those aged 35 or above—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 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.

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: Migration

Another potential alternative explanation for our findings is that migration patterns in treated prefectures changed after the house price shock, affecting fertility. We consider two possible migration channels in Tombe and Zhu (2019). First, local rural-to-urban migration, the idea that the house price shock could have coincided with increased local migration, leading to more women of childbearing age moving to urban areas, where fertility is typically lower. Second, out-of-prefecture migration, the idea that individuals from treated prefectures may have moved to other prefectures after the house price shock, altering the composition of the remaining population and affecting local birth rates.

Local Migration If the fertility decline were driven by an increase in local rural-to-urban migration, we would expect to see a higher urbanization rate in treated prefectures coinciding with lower birth rates (e.g., Wang, Liao, Wang, and Yip, 2024). However, our hypothesis suggests the opposite: that rising urban house prices made urbanization more difficult, restricting access to social amenities that support fertility.

To test this, we conduct a placebo test by redefining treatment status based on changes in urbanization rates from 2016 to 2021. We then re-estimate our main difference-in-differences analysis using this "placebo treatment" while maintaining the original post-treatment period.[10] The results, presented in Table A.10, show that (1) prefectures with greater increases in urbanization rates experienced higher, not lower, birth rates, and (2) while prefectures with greater shifts

[10]This test helps rule out a potential confounder, the 2014 hukou reform, which relaxed household registration requirements in prefectures with urban populations between 1 and 5 million. If this policy had significantly increased urbanization in treated prefectures, it could have contributed to fertility declines. However, our urbanization placebo test (Table A.10) finds no evidence that increased urbanization is associated with lower birth rates. Instead, we observe a positive correlation between urbanization and fertility, suggesting that the hukou reform is unlikely to drive our results.

toward non-primary sector output did not show significant changes in birth rates, prefectures with greater shifts toward non-primary sector employment also exhibited higher, not lower, birth rates.

These findings directly contradict the local migration hypothesis. If urbanization were reducing fertility, we would expect a negative relationship between urbanization and birth rates—but we observe the opposite. This pattern aligns with the idea that urbanization facilitates fertility by improving access to social amenities. However, when urban house prices rise sharply, fewer individuals can afford to move into cities, restricting access to these benefits and contributing to lower fertility.

Out-of-Prefecture Migration Another possibility is that individuals, particularly women of childbearing age, migrated out of treated prefectures after the house price shock, leading to a compositional change in the remaining population and driving fertility decline. To test this, we estimate the change in out-of-prefecture migration rates after the shock, using whether an individual migrated from the prefecture they were first surveyed in as the dependent variable.

The results, presented in Appendix Table A.11, show no statistically significant response in out-of-prefecture migration. Specifically, the estimated migration response is 0.8 percentage points (p-value$= 0.86$), compared to a sample mean of 8.4 percentage points. The effect is not only economically small but also statistically insignificant.

These results are consistent with two considerations: (1) the treated prefecture individuals were likely "sticky" to their hometowns, a pattern observed in prior research (Wang, Guo, and Ming, 2020), and (2) the regulated prefectures had been the more likely migration destinations of the treated prefecture individuals, but urban house prices in the regulated prefectures had already risen before the house purchase restriction implementation, making migration more difficult.

To conclude, both tests provide no evidence that migration explains the decline in birth rates. Instead, the findings align with the idea that rising urban house prices created barriers to marriage and childbearing, particularly for rural individuals.

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 increases 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.

The results are reported in Table A.12. 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 abolishment 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.[11]

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 decay with the log distance to the nearest regulated prefecture. We constructed this figure as follows. 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

[11] Our analysis focuses on the fertility effects of urban house price increases in unregulated prefectures, where the quasi-experimental variation allows for credible identification. We do not estimate counterfactual fertility trends for regulated prefectures, as the house purchase restrictions directly affected their housing markets, leaving no comparable control group. Moreover, in regulated prefectures, the policy primarily slowed house price growth rather than reducing price levels, making it unlikely that the same event would have led to higher fertility there.

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 house purchase restrictions in large regulated metropolises, which redirected investment demand to nearby unregulated prefectures and exogenously raised local house prices, we estimate the causal effect of house prices on fertility. We find that the investment-driven rise in urban house prices significantly reduced prefecture-level birth rates, individual-level fertility, and marriage rates.

This effect was particularly pronounced among rural residents who own only rural homes, likely reflecting their aspirations to acquire urban property—a goal closely tied to marriage prospects and access to quality urban education. The fertility decline was further amplified by social norms that intensified marriage market competition and by the scarcity of rural schools. We also find that affected individuals increased private education spending, suggesting a shift in investment priorities in response to changing educational access.

Chetty, Hendren, Kline, and Saez (2014) show that in the United States, a child's prospects for upward mobility are strongly influenced by residential location and constrained by segregation. Heckman and Landersø (2022), using Danish data, find that family residential choices are made early in children's lives, often before birth. Our findings highlight how rising urban house prices in high-opportunity areas can shape these choices, affecting marriage, childbearing, and human capital investment. If access to urban homeownership remains a major determinant of educational and economic opportunities, housing market dynamics may have lasting consequences for demographic and economic mobility.

An open question is how China's birth rate will evolve as the housing market cools. Most of our treated cities—primarily Tier 3 cities (Rogoff and Yang, 2024)—saw a surge in house prices from 2016 to 2021 but have since faced downward pressure. However, several factors complicate the fertility response. First, local governments actively manage land and housing prices, often sustaining high costs through policy interventions (Chang, Wang, and Xiong, 2023). Second, declining expectations of house price appreciation may increase the perceived user cost of homeownership, discouraging marriage and childbearing. Third, shifting social norms may have a lasting impact. If high housing costs have already led many to delay or forgo marriage and children, these behaviors may persist even if prices decline.

Our results do not necessarily imply a need for policy intervention to boost birth rates. However, if one were tasked with designing such policies, our findings suggest that addressing housing affordability alone may not be sufficient. Decoupling school enrollment from homeownership—such as by reforming property-linked school admission policies—could lower barriers to family formation. More broadly, if homeownership serves as a proxy for financial security in marriage markets, expanding alternative sources of economic security, such as social security reforms, could reduce this reliance. Whether such measures can shape long-term demographic trends remains an open question 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. 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. 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 4: Heterogeneous Treatment Effects by Housing Tenure in Rural and Urban Areas

	(1) Does Not Own Any	(2) Rural (Does Not Own Urban Home)	(3) Rural (Does Own Urban Home)	(4) Urban (Does Own Urban Home)
Dependent Variable: Number of Newborns				
Treat × Post	-0.031	-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 subsamples are (1) mobile individuals, who own neither local rural dwellings nor urban homes, (2) rural dwelling owners without 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, (3) rural dwelling owners with urban homeownership, 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, and (4) urban homeowners, those who live in urban area and own at least one urban property. Variable definitions are the same as in Table 3. Standard errors are clustered at the prefecture level.

Table 5: 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 6: 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 are (1) mobile individuals, who own neither local rural dwellings nor urban homes, (2) rural dwelling owners without 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, (3) rural dwelling owners with urban homeownership, 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, and (4) urban homeowners, those who live in urban area and own at least one urban property. 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 7: 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
	(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

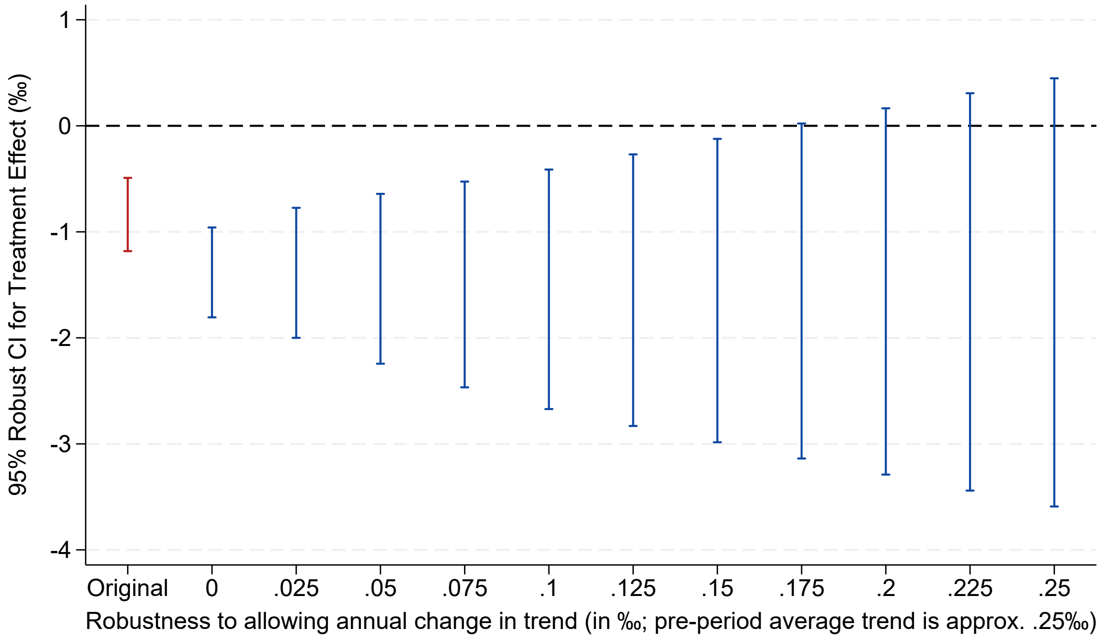
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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.

Online Appendix—Not for Publication

Additional Figures and Tables

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

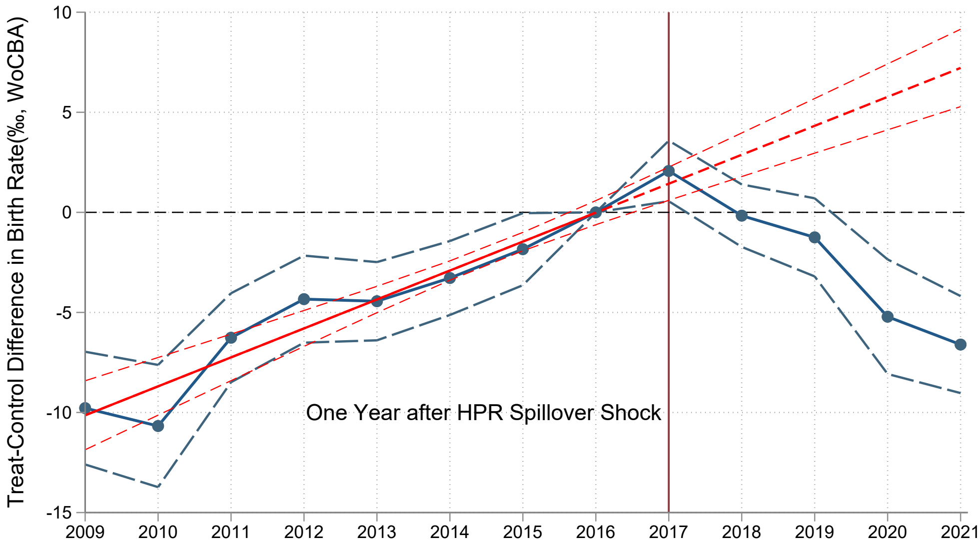


Notes: This figure plots the robustness of the estimated response of prefecture-level birth rate in treated prefectures relative to control prefectures after the positive urban house price shock, controlling for linear pre-existing trend difference (δ), to changes in the group-specific trend (Rambachan 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 allow 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 censuses. 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 red upward-sloping lines are the treatment-specific linear trend and its 95% confidence intervals. The vertical red line marks one year after the house purchase restriction spillover shock to take into account the pregnancy delay.

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, 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, 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.
$EthnicMinority$	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: 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)	(2)	(3)	(4)	(5)	(6)
	log(House Price)	log(House Price)	Birth Rate(‰)	Birth Rate(‰)	Newborns	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 A.5: 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.6: 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.7: 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 are (1) mobile individuals, who own neither local rural dwellings nor urban homes, (2) rural dwelling owners without 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, (3) rural dwelling owners with urban homeownership, 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, and (4) urban homeowners, those who live in urban area and own at least one urban property. 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 A.8: 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	(Does Not Own	(Does Own	(Does Own
	Own Any	Urban Home)	Urban Home)	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 (1) mobile individuals, who own neither local rural dwellings nor urban homes, (2) rural dwelling owners without 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, (3) rural dwelling owners with urban homeownership, 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, and (4) urban homeowners, those who live in urban area and own at least one urban property. 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 A.9: 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 A.10: 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 A.11: 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.

Table A.12: 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.