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


Ziqian Liu — Peking University

Yu Zhang* — Peking University



First draft: April 15, 2024. This draft: May 3, 2024



**Abstract**

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



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

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



*We thank Nobuhiro Kiyotaki, Kai Li, and Michael Z. Song for their invaluable comments and suggestions. All errors are our own.


Send correspondence to Yu Zhang at yuzhang@gsm.pku.edu.cn.

# 1. Introduction

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

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

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

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

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

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

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

Delving a step deeper, in regions with scarce rural schools, indicated by longer travel distances to school, we observed a more substantial decline in rural fertility, suggesting that limited access to educational resources significantly influenced fertility decisions. Moreover, we examined the impact of urban house price increases on marriage (Wei and Zhang, 2011; Wei, Zhang, and Liu, 2012) and within-marriage fertility decisions and find both to have decreased significantly with the house price treatment. Indeed, the marriage effects are also especially true among rural residents owning only rural homes. .

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

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

From the perspective of human capital accumulation, we explored whether the investment in children's education reacted significantly to undo the fertility reduction (e.g. Barro and Becker, 1989; Becker, Murphy, and Tamura, 1990; Galor and Weil, 2000). We observed a significant increase, approximately 44% above pre-shock levels, in educational expenditures by parents, particularly among rural residents who only own rural homes. This suggests that enhanced investments in education might offset some of the impacts of reduced fertility.

However, the response of educational investment to rising urban house prices varied greatly depending on the accessibility of rural schools. In areas where rural schools were less accessible, we previously noted a significant reduction in rural fertility and now find no corresponding increase in educational investments, indicating a clear decline in human capital formation. Conversely, in regions with more accessible rural schools, we previously observed no significant reduction in fertility but now find a marked increase in educational investment. This pattern aligns with the notion that, in these areas, public and private educational resources acted complementarily. Increased private educational investments in these regions responded strategically to the prohibitive costs of alternative urban educational opportunities, thus mitigating the need for a reduction in fertility.

This dynamic empirically highlights the complex interplay between educational opportunities, housing costs, and fertility decisions. By documenting this dynamic for the first time, our paper contributes to the literature on the role of inequality and local educational infrastructure in shaping human capital formation (e.g. De La Croix and Doepke, 2003; Chetty et al., 2014; Heckman and Landersø, 2022).

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

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

Our paper creates a dialogue between quasi-experimental studies on the causal effect of house prices and the study of population dynamics and human capital investment. The potential effect of house prices on fertility and human capital investment has been proposed at least as early as Becker (1960). Yi and Zhang (2010) find house prices to negatively predict fertility in time-series data in Hong Kong. Clark (2012) find expensive house prices are associated with a fertility delay in the United States. Lovenheim and Mumford (2013), Clark and Ferrer (2019) and Daysal et al. (2021) find short-run home price rises to predict an increase fertility for owners in the United States, Canada, and Denmark, whereas Liu, Xing, and Zhang (2020) and Atalay, Li, and Whelan (2021) find it to predict reduction in fertility for renters in China and in the United States. Using a Saiz (2010a) instrument, Dettling and Kearney (2014) finds a rise in home prices to increase fertility among owners and decrease it among renters in the United States, and Ge and Zhang (2019) Clark, Yi, and Zhang (2020), Liu, Liu, and Wang (2023) and Meng, Peng, and Zhou (2023) find it to reduce fertility in China. Most related to our study is Tan et al. (2023), who use a quasi-experiment in 2006 that reduced the down-payment ratio for urban homes no larger than 90 $m^2$. Using a regression discontinuity design, they find the associated housing wealth rise to increase fertility and child health for owners of such homes. Our study is unique in that we do not assume house prices are external to demographic dynamics, that we address the recent challenges to the Saiz (2010b) instrument, and that we use a recent quasi-experiment that informs about the causal effect of house prices on fertility at the city-level as well as the individual-level across comprehensive tenure and geographical statuses, allowing us to assess the aggregated causal effect of house prices on fertility and its disaggregated mechanisms.

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

## 2. Institutional Background

This section describes (i) the great birth decline in China since 2016, (ii) the house purchase restriction spillover event that we use an exogenous shock to local house prices, and (iii) how the cost of urban homeownership is related to precursors and returns to fertility, and how that cost may be especially important for rural households given ownership and education resource differences.

### 2.1 The birth decline in China

China's birth rate hit its lowest level since the formation of the People's Republic of China 70 years ago. In 2021, the birth rate was 7.5‰. Before the most recent birth decline, China's birth rate hovered around a modest average of approximately 13‰ in this century. With concerns about an aging population, the "one-child" policy was partially relaxed in 2013 and fully relaxed in 2015. The change did not significantly boost birth rates. In 2016, the birth rate was 13.6‰.

From 2016 to 2021, China's birth rate went downhill, declining by 1.2‰ per year on average. This was a great birth decline that precipitated the country's population peaking in 2023.

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

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

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

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

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

### 2.3 The house purchase restriction spillover shock

In China, a reform in 1998 marketized the supply and ownership of urban homes. In the ensuing period, urban house prices rose quickly, especially in large cities. The demand for owning urban homes transcended the need for shelter, as purchasing property also became a favored investment strategy. The urban housing market ”overheating” became a policy worry. Policy tools were designed and used to cool down local urban housing markets. Since September 2016, the local government of all Tier-1 and a large number of Tier-2 cities (policy cities) implemented a policy referred to as ”house purchase restrictions” (HPRs), aimed at limiting housing demand from speculators who often own multiple properties. In March 2017, these policies were reiterated and made more stringent in certain cases.

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

This created a ”house purchase restriction spillover” effect that inflated the house prices in the nearby unregulated cities despite any changes in the fundamentals, as studied by Deng, Liao, Yu, and Zhang (2022). They located 22 policy cities that adopted HPRs in late 2016 and early 2017, and studied housing market dynamics in nearby unregulated cities, their treated cities, compared to farther away unregulated cities, their control cities. They found that house price increases in the policy cities reduced after the HPRs. Immediately, online search activity for real estate in treated cities surged from policy cities. House prices and transaction volumes in treated cities rose sharply. Treated cities bank deposits similarly increased, aligning with house price hikes, indicative of capital inflows. Moreover, the rise in home transaction volumes in treated cities paralleled the decline in transaction volumes in policy cities. No evidence suggested changes in rents or economic growth. Local governments of the treated cities announced they are concerned of this phenomenon. The HPR spillover shock created a unique opportunity to study the causal effect of house prices on fertility, which in this study we exploit.

## 3. Empirical Strategy

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

## 3.1 Designation of treatment status

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

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

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

**[Figure 1: House price experiment about here (finalized)]**

There were reasons to think that the treatment was plausibly exogenous. There was evidence that the treated cities' house price surge was led by external demand from the policy cities. Deng et al. (2022) documented that out-of-town web searches for real estate in the nearby unregulated cities significantly increased immediately after the imposition of HPRs in the policy cities, decaying linearly with distance. There was also no evidence that the treated cities' house price surge was correlated with their local fundamentals. Had the treatment been solely from out-of-town investment demand, it should not obviously affect rents or tradable economic output. They tested responses in rents, industrial output growth, and urban employment growth. Indeed, they found estimates that are insignificant and close to zero. For example, they found rents to relatively reduce by 0.8% (t-value 0.67) in the treated cities, industrial output growth to reduce by 0.4% (t-value 0.37), and employment growth to increase by 0.4% (t-value 0.96). They found some increase in local real estate construction investment (17.3%), as expected after an out-of-town demand shock. But it was not statistically significant (t-value 1.12). These findings provide suggestive support that the house purchase restriction spillover treatment shocked treated cities' house prices by external demand and was plausibly exogenous.

## 3.2 Regression specification

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

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

As standard, $\alpha_i$ is the city (individual) fixed effect, $\delta_t$ is the time fixed effect, and $X_{i,t}$ are the time-varying controls. $\beta$ is the coefficient of interest that measures the treatment effect. To separate the treatment effect from pre-existing trends, we include $\gamma(i)t$, which denote linear pre-existing trends. In all estimations, we included treatment group-specific trends, or included city-specific trends. The results are the same. Following Bilinski and Hatfield (2019) , to make sure $\gamma(i)t$ are only estimated off of pre-treatment data, we saturate the model with post-period treatment-time interaction dummies, and use the average coefficient of these post-period treatment-time interaction dummies as the treatment effect estimate $\hat{\beta}$.

We designate the post timing according to the time period needed for a response in the $Y_{i,t}$. We use annual-level data. House prices in treated cities may react immediately after house purchase restriction spillovers. Because the house purchase restrictions were enacted in policy cities in September 2016, the initial full year when house prices in treated cities were impacted is 2017, and only a part of 2016 was impacted. Therefore, we assess treatment effects starting from 2017 and use data through 2015 to estimate pre-existing trends in house prices. For fertility, considering a nine-month pregnancy period, the first full year when fertility was impacted in treated cities is 2018, and only a part of 2017 was impacted. Thus, we commence to assess treatment effects in 2018 and estimated pre-existing trends in fertility using data through 2016. We also used marriage and private educational investments as additional treatment outcomes, assuming these variables react as swiftly as house prices.

To arrive at the quasi-experimental estimate of the effect of house price shocks on birth rates, we naturally modified Model (1) into an instrumental variables specification. Namely, we use the post-period treatment-time interaction dummies from the house purchase restriction spillover treatment as instruments for the natural logarithm of house prices, and estimate the predictive effect of last year's local urban (log) house price on this year's birth rate.

## 3.3 Data and summary statistics

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

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

At the micro-level, we constructed individual- and family-level datasets from the China Family Panel Studies (CFPS), a biennial panel dataset that samples approximately 16,000 households across 25 provinces. Our analysis incorporated data from six waves: 2010 (initial), 2012, 2014, 2016, 2018, and 2020 (latest). We utilized the CFPS data in two distinct ways. Firstly, leveraging CFPS data on children's birth years, we reconstructed an annual and cross-year verified record of newborns for each individual in the CFPS from 2009 to 2020. We applied a similar reconstruction approach to create an annual individual-level record of marriages. Consistent with the economics literature on fertility, we focused on women aged 15 to 44, a demographic that accounts for over 99% of births in the country.

Secondly, we utilized the CFPS's biennial records for additional economic controls, assuming that the values of these controls apply to all years within the biennial wave period; for example, controls from the 2010 wave were assumed to apply to both 2009 and 2010 annual observations. We also employed data from the CFPS on parents' private educational expenditures on their children, from which we constructed a biennial family-level dataset.

Table 1 provides summary statistics of the analysis samples we use. The sample covers the unregulated cities that make up our treated group and control group of observations, and do not include observations in the policy cities. The share of observations belonging to treated cities are 61%, 60%, and 59%, respectively, in the city-level, individual-level, and family-level datasets. This affirms the national representativeness of our datasets at both the individual-level and the family-level. The share of urban inhabitants is 28% and the share of rural inhabitants is 72%. This reflects a modest level of urbanization in the unregulated cities during the sample period. The homeownership rate is 93%. Most of the urban inhabitants own their home, as do most of the rural inhabitants, but their owned rural dwelling cannot be traded. The multiple home ownership rate is 16%. In addition to some urban residents owning multiple urban properties, a portion of rural residents also own an urban home in addition to their rural dwelling. The average birth rate in the sample is 10.72‰, and the average number of newborns to women of childbearing age (15-44) is 0.06. This translates to a total fertility rate of 1.80, which is below the replacement level.

**[Table 1 about here]**

## 4. The Effect of the House Price Shock on Fertility

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

## 4.1 City-level birth rate responses

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

**[Table 2 about here]**

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

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

**[Figure 2 about here]**

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

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

## 4.2 Individual-level fertility responses

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

[**Table 3 about here**]

Our analysis find individual-level results that confirm the city-level result. After the house purchase restriction spillover shock, the average number of newborns to each women of childbearing age in the treated cities abnormally reduces by 0.028. This abnormal reduction in newborns after the positve house price shock is statistically significant at the 1% level.

[**Figure 3 about here**]

Figure 3 visually captures the distinct downturn in individual-level newborns in the post-treatment period. In two out of three post-treatment years, the decline in newborns within treated cities was statistically significant from zero at the 5% level. Collectively, these event study estimated validate that the abnormal decrease in newborns in treated cities significantly diverged from the pre-treatment trend at the 1% level.

Curiously, no significant pre-trend differences were observed in the CFPS individual-level newborn data, whereas we observed positive birth rate pre-trends for treated cities in the city-level Statistical Communiqué data. Moreover, the reduction in newborns at the individual level appears to be economically substantial when considering the sample's overall average of 0.060, as Table 3 indicates. A plausible reason for the more pronounced individual-level decline may be the panel's focus on 'non-movers' present in the sampled cities since the survey's inception in 2010, who are disproportionately affected by rising urban homeownership costs. Conversely, the treated cities might have previously attracted migratory inflows, not captured by the panel survey, who settled and contributed to the birth rate prior to the house price shock.

## 4.3 Robustness checks

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

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

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

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

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 imposition of house purchase restriction spillovers. Hence, we expect a negative continuous treatment effect $\phi$ for urban house prices in unregulated cities, and a positive continuous treatment effect $\phi$ for birth rates and the number of newborns in unregulated cities.

**[Table 4 about here]**

**[Figure 5 about here]**

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

## 5. Mechanisms

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

### 5.1 Rural aspirations for urban homeownership and the fertility decline

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

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

[**Table 5 about here**]

Column (1) of Table 5 indicates that the effect on newborn numbers among the mobile subset is negative, albeit not statistically significant (-0.024 with an s.e. of 0.050). Crucially, column (2) demonstrates a statistically and economically meaningful decline in newborns among rural residents without urban property (-0.047 with an s.e. of 0.013). The responses for rural owners of urban homes and urban residents with property are positive, yet statistically non-significant. Moreover, we find a significant disparity in fertility responses between rural non-urban-owners (column 2) and urban owners (column 4), suggesting that despite both groups having stable housing, their fertility responses to the urban house price increase diverge, with no change for urban homeowners but a significant decrease for rural non-urban-owners.

[**Figure 6 about here**]

This finding aligns with the patterns in panels (a) and (b) of Figure 6, which reveal rural non-urban-owners' fertility responses as consistently deviating negatively from the trend across all post-treatment years, while urban homeowners exhibit insignificantly positive fertility responses. This dichotomy supports the notion that rural residents' intentions to purchase urban homes—and consequently their family planning—are thwarted by escalating urban housing costs.

## 5.2 The distance to rural schools and the fertility decline

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

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

[**Table 6 about here**]

Table 6 reports the results by the local spatial scarcity of schools. We find that, within urban samples, the fertility responses do not significantly differ whether schools are relatively near or distant. However, the spatial scarcity of schools matters crucially for the rural fertility reduction. We only observe a statistically significant reduction in the number of newborns among rural individuals in counties with a long average distance to schools. Furthermore, this ”rural, schools distant” group's fertility response is statistically different from the two ”schools near” groups at the 5% significance level, and different from the ”urban, school distant” group at the 10% level. These results indicate that, indeed, one factor driving the rural individuals' fertility reduction response to an positive urban house price shock is the gap in education resources.

## 5.3 The competitive marriage market and the fertility decline

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

**[Table 7 about here]**

Table 7 reports results from this test on marriage rate. Panel (a) estimate the average treatment effect. After the house purchase restriction spillover shock, the likelihood of new marriage for treated cities' individuals abnormally reduced by 0.033, significant at the 1% level. Figure 6 shows the event study for marriage rate, which shows a negative treatment response immediate after the house purchase restriction spillover shock. Panel (b) of Table 7 reports the heterogeneous treatment effects by housing tenure status in rural and urban areas. We find rural inhabitants who own no urban homes to have the significant marriage rate decline. In contrast, rural inhabitants who own urban homes have the largest positive point estimate, albeit statistically insignificant. The treatment effect for rural inhabitants who own no urban homes is significantly different from rural inhabitants who own urban homes, and from urban inhabitants that are urban homeowners, both at the 5% level.

**[Figure 7 about here]**

In addition, we also want to assess whether already married individuals also reduce their number of newborns. A priori, the education mechanism we previous discussed reduces the return to having children, which would affect both the number of newborns conditional on marriage as well as the marriage rate. We therefore conduct another test to assess whether we find treatment effect of the urban house price shock within the married sample. Table 8 reports results from conditioning the baseline fertility treatment effect tests on the married sample. We find an abnormal average fertility decline in treated cities within married individuals, as indicated by Panel (a). And we find the fertility decline in married individuals to be statistically significant among rural inhabitants who do not own urban homes. Indeed, the patterns are strikingly consistent across the baseline fertility treatment effects, the marriage rate treatment effects, and the fertility treatment effects among married individuals. Therefore, the house price shock reduced fertility through both (1) reducing new marriages and (2) reducing the number of newborns to married couples.

**[Table 8 about here]**

## 5.4 Effects on private educational investments

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

We use information from the 2010, 2012, 2014, 2016, 2018, 2020 biennial waves of CFPS and the family-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 family's expenditures as the family's educational investment on children.

[**Table 9 about here**]

Table 9 reports the treatment effect results on private educational investments. Panel (a) of Table 9 estimates the average treatment effect. Figure 7 visually depicts the event study coefficients. Education investments were normalized against the pre-treatment city-level average. Therefore, our preferred specification in column (4) implies an average 44.2% increase on the pre-treatment educational investments, which is economically significant. The average treatment effect indicates that educational investments significantly increased as house price increased and newborns decreased.

[**Figure 7 about here**]

Panel (b) of Table 9 explores the heterogeneous treatment effects by housing tenure in rural and urban areas. Although the results lack the precision necessary for testing differences in effects, they indicate significant action among rural inhabitants who do not own urban homes. In this demographic, a decrease in the number of newborn children is accompanied by increased private educational spending on existing children. Collectively, panels (a) and (b) of Table 9 and our fertility treatment effect estimates suggest that as fertility declines in response to the urban house price shock, private educational investments increased, possibly offsetting the negative impact on human capital formation.

In panel (c) of Table 9, we explore the heterogeneous treatment effect estimates by spatial school scarcity in rural and urban areas. Similar to previous results, the data among urban families do not allow for effective testing of difference reponses. Among the rural households, the response of educational investment to rising urban house prices varied greatly depending on the accessibility of rural schools. Unlike the fertility effects, which were pronounced among rural residents in areas with scarce rural schools, we observed a significant increase in private educational investments among rural residents in counties where rural schools are relatively abundant. The effect among that group is statistically larger than among the rural residents in areas with scarce rural schools at the 5% level.

This observation, that in regions where rural schools were less accessible, there was a significant decline in rural fertility and a lack of increase in educational investments, clearly indicates a reduction in human capital formation. Conversely, areas with more readily accessible rural schools saw no notable decrease in fertility, yet experienced a significant rise in educational spending. However, concluding that human capital formation has increased in these areas would be premature, given the higher costs of alternative urban educational opportunities.

We may interpret these results as indicating that public and private educational resources appear complementary. In areas with more accessible rural schools, families inclined to augment accessible public education with private resources to optimize educational outcomes when urban alternatives are expensive or inaccessible. Conversely, in areas with fewer schools, the perceived return on educational investment (both public and private) may be lower, thus discouraging substantial private educational expenditure and leading to reduced fertility as a strategic adaptation to limited resources and opportunities. This notion underscores the close relationships among educational opportunities, homeownership costs, and fertility choices, emphasizing the significant impact of inequality and the local educational environment on human capital formation (e.g. De La Croix and Doepke, 2003; Chetty et al., 2014; Heckman and Landersø, 2022).

# 6. Discussions

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

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

## 6.1 Alternative Explanation 1: Age composition

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

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

Motivated by this hypothesis, we assessed the treatment effects within specific age groups. Table 10 presents these results.

**[Table 10 about here]**

Panels (a) and (b) of Table 10 detail the treatment effects on the number of newborns and the likelihood of new marriages, respectively, segmented by age group. We observed a significant treatment effect particularly in younger demographics. The negative impact on both the number of newborns and new marriages was statistically significant specifically within the 20-29 age group. Furthermore, when categorizing women of childbearing age into two groups—below advanced maternal age (under 35) and advanced maternal age (35 and over)—the significant negative treatment effects were confined to the group under 35.

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

## 6.2 Alternative Explanation 2: Local migration

Possibly, treated cities had more local rural-to-urban migration during the post-2016 period, which resulted in more fertile-age women moving to urban areas. Our hypothesis is in a sense opposite, that exogenous spike in house prices may have made urbanization in the treated cities more challenging. Hence, this alternation explanation is also unlikely to account for our findings. Nevertheless, we assess this alternative by redefining the "treatment" status based on the 2016-2021 change in urbanization rates and then conducted the main Difference-in-Differences (DID) analysis using the same post-treatment timing. The results are presented in Table 11.

**[Table 11 about here]**

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

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

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

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

**[Table 12 about here]**

The results are reported in Table 12. They show no significant effects associated with this placebo OCP treatment timing, neither in city-level birth rates nor in the individual-level fertility data. In three of the four specifications, the point estimates suggest that treated cities exhibited a insignificantly lower birth rate response to the OCP relaxation prior to our house price shock. Hence there was no differentially heightened response to OCP relaxation in treated cities to fall back from. Consequently, these findings suggest that the relaxation and abolishing of the one-child policy does not explain our estimated negative treatment effect of the house price shock on fertility.

## 6.4 Aggregate implications of quasi-experimental fertility effect estimates

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

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

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

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

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

## 7. Conclusions

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

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

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

# References

Adelino, Manuel, Antoinette Schoar, and Felipe Severino. 2015. "House prices, collateral, and self-employment." *Journal of Financial Economics* 117 (2):288–306.

Aladangady, Aditya. 2017. "Housing wealth and consumption: evidence from geographically linked microdata." *American Economic Review* 107 (11):3415–3446.

Atalay, Kadir, Ang Li, and Stephen Whelan. 2021. "Housing wealth, fertility intentions and fertility." *Journal of Housing Economics* 54:101787. URL https://www.sciencedirect.com/science/article/pii/S1051137721000371.

Badarinza, Cristian and Tarun Ramadorai. 2018. "Home away from home? Foreign demand and London house prices." *Journal of Financial Economics* 130 (3):532 – 555.

Barro, Robert J and Gary S Becker. 1989. "Fertility choice in a model of economic growth." *Econometrica: journal of the Econometric Society* :481–501.

Becker, Gary S. 1960. "An Economic Analysis of Fertility." In *Demographic and Economic Change in Developed Countries*. Columbia University Press, 209–240. URL https://www.nber.org/books-and-chapters/demographic-and-economic-change-developed-countries/economic-analysis-fertility.

Becker, Gary S., Kevin M. Murphy, and Robert Tamura. 1990. "Human Capital, Fertility, and Economic Growth." *Journal of Political Economy* 98 (5, Part 2):S12–S37. URL https://www.journals.uchicago.edu/doi/abs/10.1086/261723. Publisher: The University of Chicago Press.

Bednarek, Peter, Daniel Marcel te Kaat, Chang Ma, and Alessandro Rebucci. 2021. "Capital flows, real estate, and local cycles: Evidence from German cities, banks, and firms." *The Review of Financial Studies* 34 (10):5077–5134.

Bernstein, Shai, Timothy McQuade, and Richard R Townsend. 2021. "Do household wealth shocks affect productivity? evidence from innovative workers during the great recession." *The Journal of Finance* 76 (1):57–111.

Bilinski, Alyssa and Laura A Hatfield. 2019. "Nothing to see here? Non-inferiority approaches to parallel trends and other model assumptions." *arXiv preprint arXiv:1805.03273* .

Chaney, Thomas, David Sraer, and David Thesmar. 2012. "The collateral channel: How real estate shocks affect corporate investment." *American Economic Review* 102 (6):2381–2409.

Charles, Kerwin Kofi, Erik Hurst, and Matthew J Notowidigdo. 2018. "Housing booms and busts, labor market opportunities, and college attendance." *American Economic Review* 108 (10):2947–2994.

———. 2019. "Housing booms, manufacturing decline and labour market outcomes." *The Economic Journal* 129 (617):209–248.

Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. 2014. " Where is the land of Opportunity? The Geography of Intergenerational Mobility in the United States *." *The Quarterly Journal of Economics* 129 (4):1553–1623. URL https://doi.org/10.1093/qje/qju022.

Chinco, Alex and Christopher Mayer. 2016. "Misinformed speculators and mispricing in the housing market." *The Review of Financial Studies* 29 (2):486–522.

Chodorow-Reich, Gabriel. 2019. "Geographic cross-sectional fiscal spending multipliers: What have we learned?" *American Economic Journal: Economic Policy* 11 (2):1–34.

———. 2020. "Regional data in macroeconomics: Some advice for practitioners." *Journal of Economic Dynamics and Control* 115:103875.

Clark, Jeremy and Ana Ferrer. 2019. "The effect of house prices on fertility: evidence from Canada." *Economics* 13 (1). URL https://www.degruyter.com/document/doi/10.5018/economics-ejournal.ja.2019-38/html. Publisher: De Gruyter Open Access.

Clark, William A. V., Daichun Yi, and Xin Zhang. 2020. "Do House Prices Affect Fertility Behavior in China? An Empirical Examination." *International Regional Science Review* 43 (5):423–449. URL https://doi.org/10.1177/0160017620922885. Publisher: SAGE Publications Inc.

Clark, William AV. 2012. "Do women delay family formation in expensive housing markets?" *Demographic research* 27 (1):1.

Corradin, Stefano and Alexander Popov. 2015. "House prices, home equity borrowing, and entrepreneurship." *The Review of Financial Studies* 28 (8):2399–2428.

Cvijanovic, Dragana and Christophe Spaenjers. 2018. ""We'll always have Paris": Out-of-country buyers in the housing market." Working paper, Kenan Institute of Private Enterprise.

Davidoff, Thomas. 2016. "Supply Constraints Are Not Valid Instrumental Variables for Home Prices Because They Are Correlated With Many Demand Factors." *Critical Finance Review* 5 (2):177–206.

Daysal, N. Meltem, Michael Lovenheim, Nikolaj Siersbæk, and David N. Wasser. 2021. "Home prices, fertility, and early-life health outcomes." *Journal of Public Economics* 198:104366. URL https://linkinghub.elsevier.com/retrieve/pii/S0047272721000025.

De La Croix, David and Matthias Doepke. 2003. "Inequality and growth: why differential fertility matters." *American Economic Review* 93 (4):1091–1113.

DeFusco, Anthony, Wenjie Ding, Fernando Ferreira, and Joseph Gyourko. 2018. "The role of price spillovers in the American housing boom." *Journal of Urban Economics* 108:72–84.

DeFusco, Anthony A, Charles G Nathanson, and Eric Zwick. 2022. "Speculative dynamics of prices and volume." *Journal of Financial Economics* 146 (1):205–229.

Deng, Yinglu, Li Liao, Jiaheng Yu, and Yu Zhang. 2022. "Capital Spillover, House Prices, and Consumer Spending: Quasi-Experimental Evidence from House Purchase Restrictions." *The Review of Financial Studies* 35 (6):3060–3099. URL https://doi.org/10.1093/rfs/hhab091.

Dettling, Lisa J. and Melissa S. Kearney. 2014. "House prices and birth rates: The impact of the real estate market on the decision to have a baby." *Journal of Public Economics* 110:82–100. URL https://www.sciencedirect.com/science/article/pii/S0047272713001904.

Ding, Yanqing, Shaoda Wang, and Xiaoyang Ye. 2016. "Why Have Some Local Governments Closed More Rural Schools than Others?" *China Economics of Education Review* 1 (4):3–34.

Favilukis, Jack and Stijn Van Nieuwerburgh. 2021. "Out-of-town home buyers and city welfare." *Journal of Finance* .

Galor, Oded and David N Weil. 2000. "Population, technology, and growth: From Malthusian stagnation to the demographic transition and beyond." *American economic review* 90 (4):806–828.

Gao, Zhenyu, Michael Sockin, and Wei Xiong. 2020. "Economic consequences of housing speculation." *The Review of Financial Studies* 33 (11):5248–5287.

Ge, Yuhao and Xuemei Zhang. 2019. "The effect of housing price on family fertility decision in China." *Population Research* 43 (1):52.

Gorback, Caitlin S and Benjamin J Keys. 2020. "Global Capital and Local Assets: House Prices, Quantities, and Elasticities." Tech. rep., National Bureau of Economic Research.

Gu, Quanlin, Jia He, Wenlan Qian, and Yuan Ren. 2021. "Housing booms and shirking." *Available at SSRN 3189933* .

Guren, Adam, Alisdair McKay, Emi Nakamura, and Jón Steinsson. 2021a. "What do we learn from cross-regional empirical estimates in macroeconomics?" *NBER Macroeconomics Annual* 35 (1):175–223.

Guren, Adam M, Alisdair McKay, Emi Nakamura, and Jón Steinsson. 2021b. "Housing wealth effects: The long view." *The Review of Economic Studies* 88 (2):669–707.

Gyourko, Joseph, Christopher Mayer, and Todd Sinai. 2013. "Superstar cities." *American Economic Journal: Economic Policy* 5 (4):167–199.

Heckman, James and Rasmus Landersø. 2022. "Lessons for Americans from Denmark about inequality and social mobility." *Labour Economics* 77:101999. URL https://www.sciencedirect.com/science/article/pii/S0927537121000348. European Association of Labour Economists, World Conference EALE/SOLE/AASLE, Berlin, Germany, 25 – 27 June 2020.

Li, Hongbin, Prashant Loyalka, Scott Rozelle, Binzhen Wu, and Jieyu Xie. 2015. "Unequal access to college in China: How far have poor, rural students been left behind?" *The China Quarterly* 221:185–207.

Li, Zhimin, Leslie Sheng Shen, and Calvin Zhang. 2020. "Capital Flows, Asset Prices, and the Real Economy: A" China Shock" in the US Real Estate Market." *International Finance Discussion Papers 1286* .

Liu, Hong, Lili Liu, and Fei Wang. 2023. "Housing wealth and fertility: evidence from China." *Journal of Population Economics* 36 (1):359–395. URL https://link.springer.com/10.1007/s00148-021-00879-6.

Liu, Jing, Chunbing Xing, and Qiong Zhang. 2020. "House price, fertility rates and reproductive intentions." *China Economic Review* 62:101496. URL https://www.sciencedirect.com/science/article/pii/S1043951X20300936.

Lovenheim, Michael F. and Kevin J. Mumford. 2013. "Do Family Wealth Shocks Affect Fertility Choices? Evidence from the Housing Market." *The Review of Economics and Statistics* 95 (2):464–475. URL https://doi.org/10.1162/REST_a_00266.

Martín, Alberto, Enrique Moral-Benito, and Tom Schmitz. 2021. "The financial transmission of housing booms: evidence from Spain." *American Economic Review* 111 (3):1013–1053.

Meng, Lina, Lu Peng, and Yinggang Zhou. 2023. "Do housing booms reduce fertility intentions? Evidence from the new two-child policy in China." *Regional Science and Urban Economics* 101:103920.

Mian, Atif, Kamalesh Rao, and Amir Sufi. 2013. "Household balance sheets, consumption, and the economic slump." *Quarterly Journal of Economics* 128 (4):1687–1726.

Nathanson, Charles G and Eric Zwick. 2018. "Arrested development: Theory and evidence of supply-side speculation in the housing market." *The Journal of Finance* 73 (6):2587–2633.

Sá, Filipa. 2016. "The effect of foreign investors on local housing markets: Evidence from the UK." Working paper, King's College London.

Saiz, Albert. 2010a. "The Geographic Determinants of Housing Supply." *The Quarterly Journal of Economics* 125 (3):1253–1296. URL https://www.jstor.org/stable/27867510. Publisher: Oxford University Press.

———. 2010b. "The geographic determinants of housing supply." *Quarterly Journal of Economics* 125 (3):1253–1296.

Schmalz, Martin C, David A Sraer, and David Thesmar. 2017. "Housing collateral and entrepreneurship." *The Journal of Finance* 72 (1):99–132.

Sodini, Paolo, Stijn Van Nieuwerburgh, Roine Vestman, and Ulf von Lilienfeld-Toal. 2023. "Identifying the benefits from homeownership: A Swedish experiment." *American Economic Review* 113 (12):3173–3212.

Tan, Ya, Fan Zhang, Qinghua Zhang, and Geer Ang. 2023. "Housing Wealth and Fertility in China: A Regression Discontinuity Design." .

Van Nieuwerburgh, Stijn and Pierre-Olivier Weill. 2010. "Why has house price dispersion gone up?" *The Review of Economic Studies* 77 (4):1567–1606.

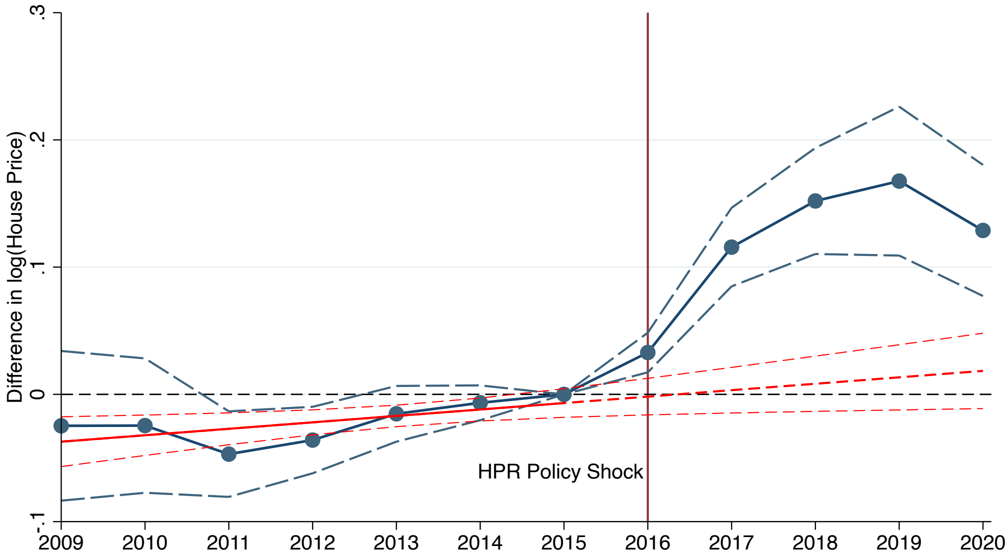
Wei, Shang-Jin and Xiaobo Zhang. 2011. "The competitive saving motive: Evidence from rising sex ratios and savings rates in China." *Journal of political Economy* 119 (3):511–564.

Wei, Shang-Jin, Xiaobo Zhang, and Yin Liu. 2012. "Status competition and housing prices." Tech. rep., National Bureau of Economic Research.

Wolfers, Justin. 2006. "Did unilateral divorce laws raise divorce rates? A reconciliation and new results." *American Economic Review* 96 (5):1802–1820.

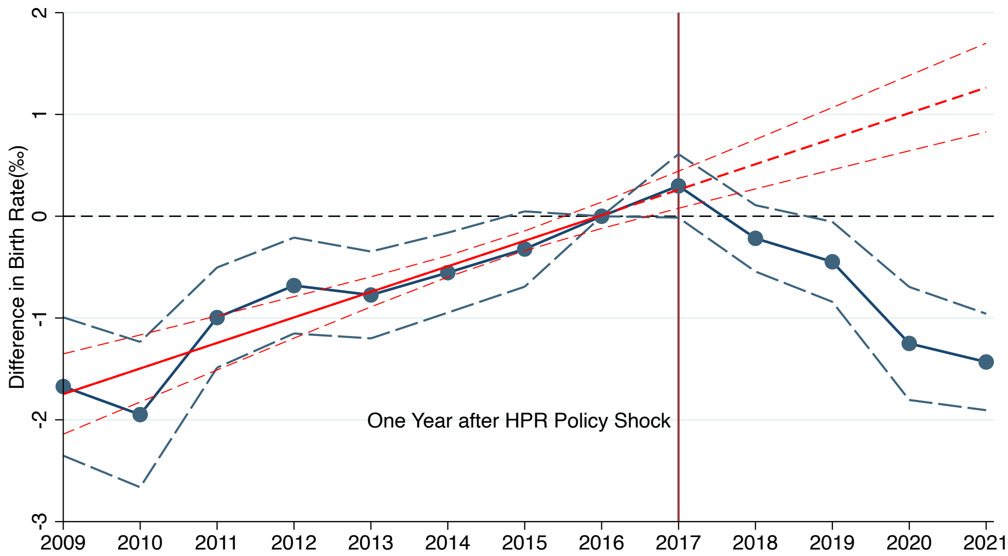
Yi, Junjian and Junsen Zhang. 2010. "The Effect of House Price on Fertility: Evidence from Hong Kong." *Economic Inquiry* 48 (3):635–650. URL https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1465-7295.2009.00213.x. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/j.1465-7295.2009.00213.x.

Figure 1: Preexisting Trends and Dynamic Responses: House Prices



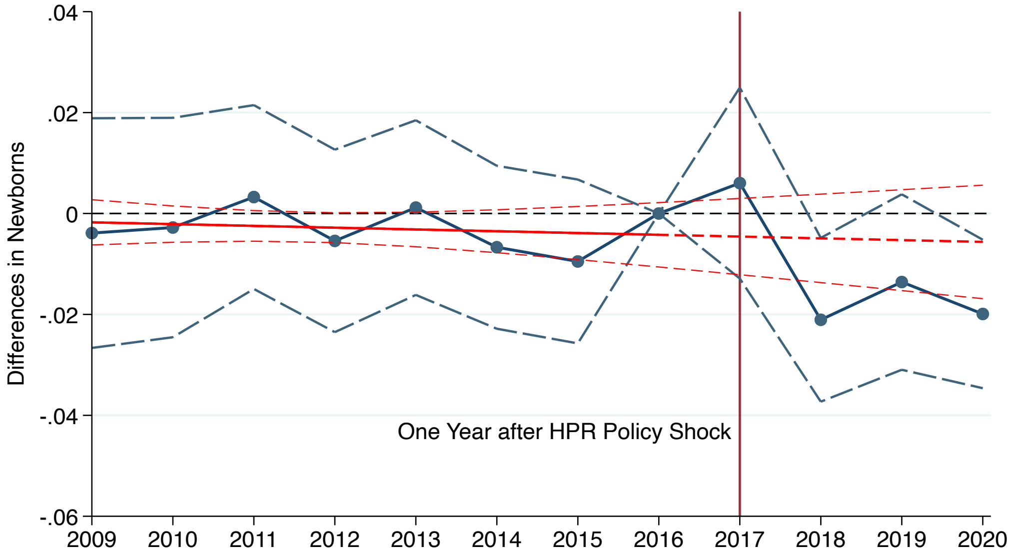

Notes: This figure plots the estimated response of house prices in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. The response is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

Figure 2: Preexisting Trends and Dynamic Responses: Birth Rate




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

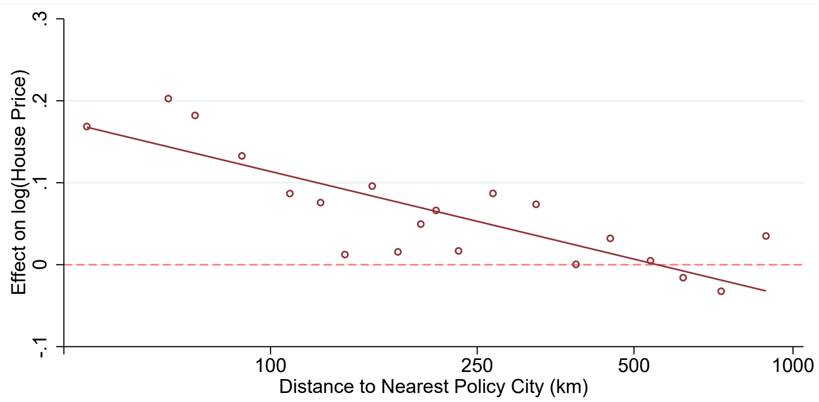
Figure 3: Preexisting Trends and Dynamic Responses: Newborns (Individual-level)



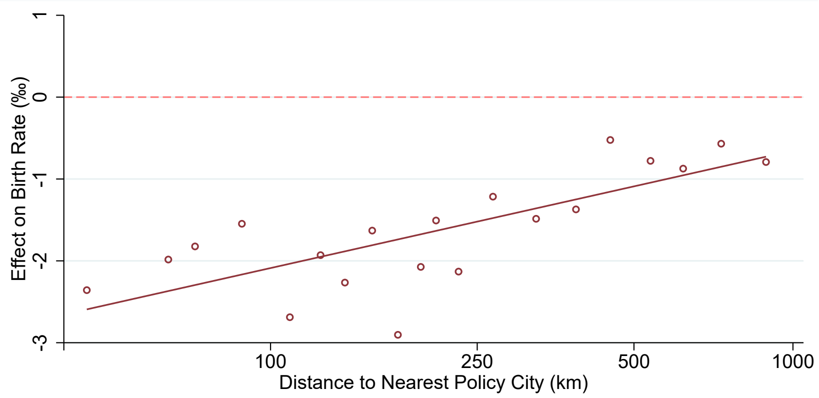

Notes: This figure plots the estimated response of number of newborns of each individual in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time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

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

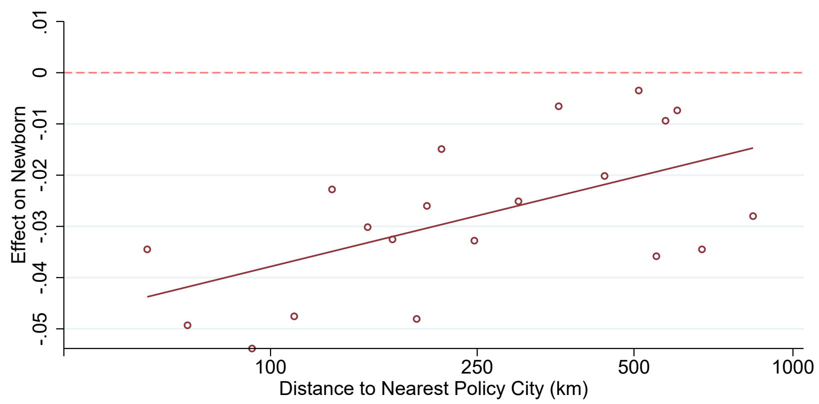
(a) Spillover Effects of House Purchase Restrictions: log(House Price)




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



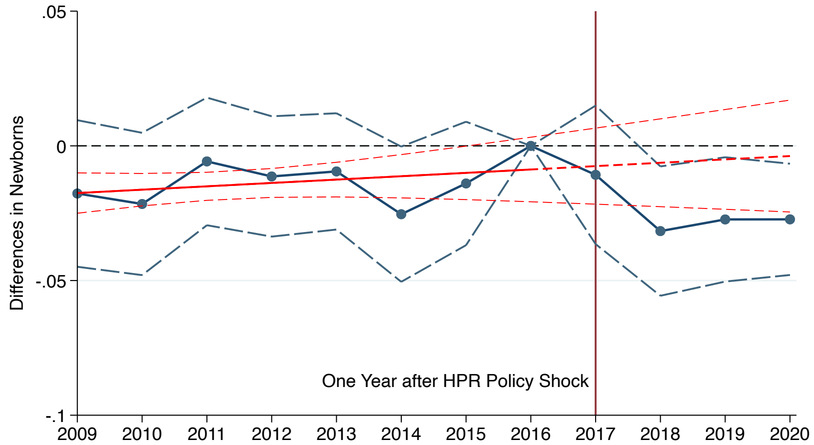

(c) Spillover Effects of House Purchase Restrictions: Fertility, Women of Childbearing Age



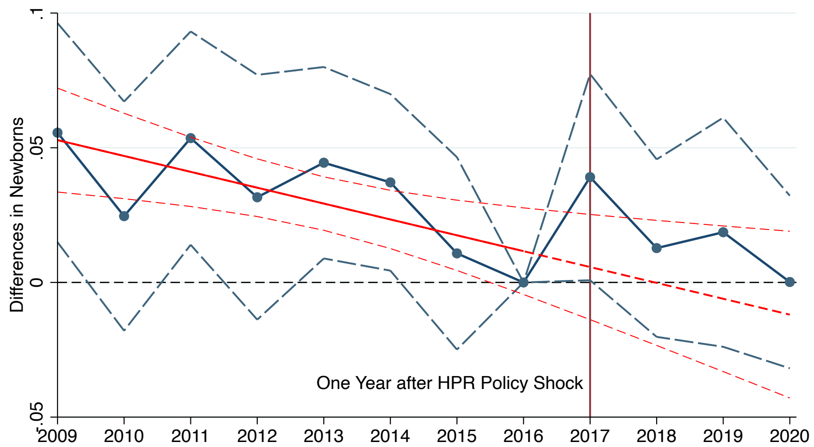

Notes: These figures plot the spillover effects of house purchase restrictions on the unregulated city as the distance from the nearest regulated city varies. The spillover effect on each city is defined as deviations in the variable of interest in post-shock periods (2016 for house price and 2017 for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

Figure 5: Preexisting Trends and Dynamic Responses: Rural Single House Owner, Urban House Owner

(a) Preexisting Trends and Dynamic Responses for Rural Single House Owner

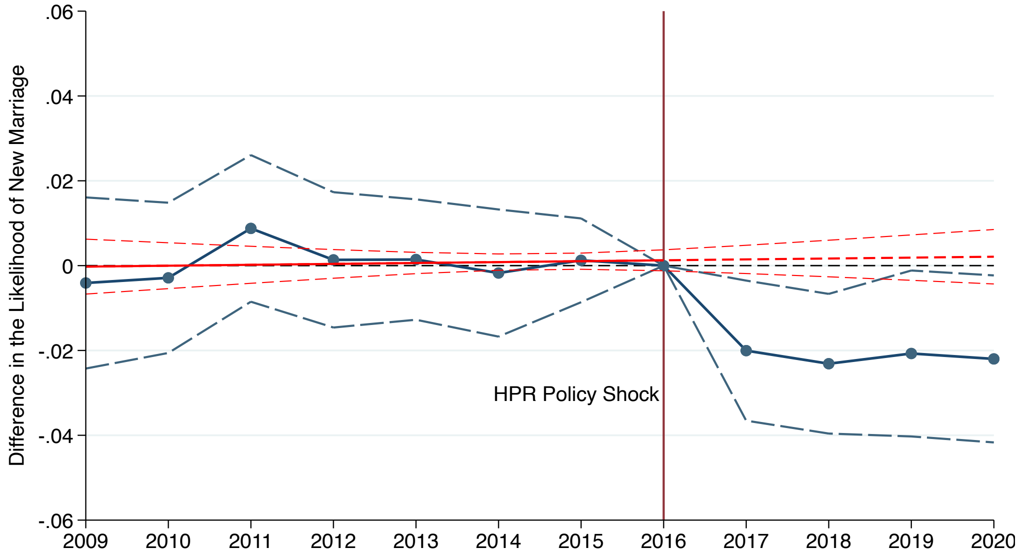



(b) Preexisting Trends and Dynamic Responses for Urban House Owner



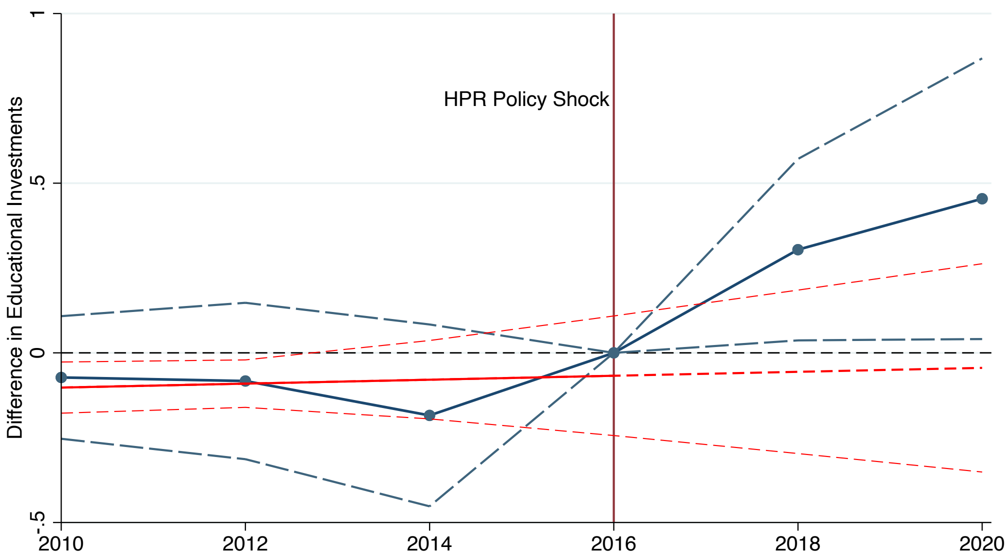

Notes: This figure plots the estimated heterogeneous response of individual-level number of newborns of rural single house owner and urban house owner in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences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

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




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

Figure 7: Preexisting Trends and Dynamic Responses: Educational Investments (Household-level)




Notes: This figure plots the estimated response of educational investments of each household in treated cities relative to control cities, both before and after the house purchase restrictions. The response is estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. The response is relative to the level of response in 2016. City fixed effects and time (year) fixed effects are added. The family control variables are urban residence, housing tenure, log per capita family net income, log total asset, and migratory status. 95% confidence intervals are drawn based on standard errors clustered at the city level. The house purchase restrictions in the regulated cities is labeled by the vertical red line. The sloped red solid line is the pre-treatment tread of the relative responses and the trends' 95% confidence interval , based on linear regression of the estimated responses on time. Data on educational investments is from 2010 to 2020 each two years.

Table 1: Summary Statistics

| | Count | Mean | Std. Dev. | 10th | 50th | 90th |
|---|---|---|---|---|---|---|
| *City-level Data* | | | | | | |
| 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 Data* | | | | | | |
| Treat | 62058 | 0.60 | 0.49 | 0 | 1 | 1 |
| Number of newborns | 62058 | 0.06 | 0.24 | 0 | 0 | 0 |
| New Marriage | 62058 | 0.04 | 0.2 | 0 | 0 | 0 |
| Age | 62058 | 30.85 | 8.52 | 19 | 30 | 42 |
| Education level | 62058 | 2.21 | 1.36 | 0 | 2 | 4 |
| Marital status | 62058 | 0.72 | 0.45 | 0 | 1 | 1 |
| Spouse's education level | 62058 | 1.30 | 1.32 | 0 | 1 | 3 |
| Ethnic minority | 62058 | 0.13 | 0.34 | 0 | 0 | 1 |
| Party membership | 62058 | 0.04 | 0.21 | 0 | 0 | 0 |
| Urban residence | 62058 | 0.28 | 0.45 | 0 | 0 | 1 |
| Migratory status | 62058 | 0.48 | 0.50 | 0 | 1 | 1 |
| Health score | 62058 | 2.58 | 1.14 | 1 | 3 | 4 |
| Housing tenure(not any or single) | 62058 | 0.93 | 0.26 | 1 | 1 | 1 |
| Housing tenure(single or multiple) | 62058 | 0.16 | 0.37 | 0 | 0 | 1 |
| Per capita family net income | 62058 | 60851 | 97031 | 8898 | 42000 | 120000 |
| Mortgage debts | 62058 | 2699 | 22439 | 0 | 0 | 0 |
| *Household-level Data* | | | | | | |
| Treat | 23671 | 0.59 | 0.49 | 0 | 1 | 1 |
| Educational Investment | 23671 | 1.04 | 1.82 | 0 | 0.40 | 2.85 |
| Urban residence | 23671 | 0.21 | 0.41 | 0 | 0 | 1 |
| Housing tenure(not own or own) | 23671 | 0.96 | 0.21 | 1 | 1 | 1 |
| Housing tenure(single or multiple) | 23671 | 0.15 | 0.36 | 0 | 0 | 1 |
| Log(per capita family net income) | 23671 | 10.10 | 1.23 | 8.58 | 10.31 | 11.37 |
| Log(total asset) | 23671 | 11.95 | 1.32 | 10.39 | 12.07 | 13.39 |
| Migratory status | 23671 | 0.48 | 0.50 | 0 | 0 | 1 |

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

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

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

Standard errors in parentheses

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

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

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

| | (1) Newborns | (2) Newborns | (3) Newborns | (4) Newborns |
|---|---|---|---|---|
| Treat × Post | -0.029*** | -0.031*** | -0.026*** | -0.028*** |
| | (0.009) | (0.010) | (0.010) | (0.010) |
| Mean | 0.060 | 0.060 | 0.060 | 0.060 |
| $R^2$ | 0.034 | 0.031 | 0.053 | 0.051 |
| Observations | 62058 | 62058 | 62058 | 62058 |
| Individual FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | yes | no | yes | no |
| Group Trend | no | yes | no | yes |
| Individual Controls | no | no | yes | yes |
| Family Controls | no | no | yes | yes |

Standard errors in parentheses

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

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

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

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

| | (1) log(House Price) | (2) log(House Price) | (3) Birth Rate(‰) | (4) Birth Rate(‰) | (5) Newborns | (6) Newborns |
|---|---|---|---|---|---|---|
| Treat×Post | 0.151*** | 0.142*** | -1.349*** | -1.454*** | -0.024** | -0.025** |
| | (0.029) | (0.029) | (0.324) | (0.310) | (0.010) | (0.010) |
| Mean | 8.544 | 8.544 | 10.723 | 10.723 | 0.060 | 0.060 |
| $R^2$ | 0.971 | 0.940 | 0.875 | 0.819 | 0.053 | 0.051 |
| Observations | 2589 | 2589 | 2589 | 2589 | 62058 | 62058 |
| City FE | yes | yes | yes | yes | no | no |
| Individual FE | no | no | no | no | yes | yes |
| Year FE | yes | yes | yes | yes | yes | yes |
| City Trend | yes | no | yes | no | yes | no |
| Group Trend | no | yes | no | yes | no | yes |
| City Controls | yes | yes | yes | yes | no | no |
| Individual Controls | no | no | no | no | yes | yes |
| Family Controls | no | no | no | no | yes | yes |

Standard errors in parentheses

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

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

| | (1) log(House Price) | (2) log(House Price) | (3) Birth Rate(‰) | (4) Birth Rate(‰) | (5) Newborns | (6) Newborns |
|---|---|---|---|---|---|---|
| Treat×Post | 0.160*** | 0.142*** | -1.564*** | -1.610*** | -0.027*** | -0.028*** |
| | (0.030) | (0.030) | (0.305) | (0.291) | (0.010) | (0.010) |
| Mean | 8.544 | 8.544 | 10.723 | 10.723 | 0.060 | 0.060 |
| $R^2$ | 0.971 | 0.940 | 0.877 | 0.819 | 0.053 | 0.051 |
| Observations | 2589 | 2589 | 2589 | 2589 | 62058 | 62058 |
| City FE | yes | yes | yes | yes | no | no |
| Individual FE | no | no | no | no | yes | yes |
| Year FE | yes | yes | yes | yes | yes | yes |
| City Trend | yes | no | yes | no | yes | no |
| Group Trend | no | yes | no | yes | no | yes |
| City Controls | yes | yes | yes | yes | no | no |
| Individual Controls | no | no | no | no | yes | yes |
| Family Controls | no | no | no | no | yes | yes |

Standard errors in parentheses

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

**(c) DID robustness check of using continuous distance specification**

| | (1) log(House Price) | (2) log(House Price) | (3) Birth Rate(‰) | (4) Birth Rate(‰) | (5) Newborns | (6) Newborns |
|---|---|---|---|---|---|---|
| log(Distance)×Post | -0.125*** | -0.113*** | 0.884*** | 0.953*** | 0.016** | 0.014*** |
| | (0.016) | (0.016) | (0.173) | (0.170) | (0.006) | (0.005) |
| Mean | 8.544 | 8.544 | 10.723 | 10.723 | 0.060 | 0.060 |
| $R^2$ | 0.973 | 0.944 | 0.876 | 0.819 | 0.053 | 0.051 |
| Observations | 2589 | 2589 | 2589 | 2589 | 62058 | 62058 |
| City FE | yes | yes | yes | yes | no | no |
| Individual FE | no | no | no | no | yes | yes |
| Year FE | yes | yes | yes | yes | yes | yes |
| City Trend | yes | no | yes | no | yes | no |
| Group Trend | no | yes | no | yes | no | yes |
| City Controls | yes | yes | yes | yes | no | no |
| Individual Controls | no | no | no | no | yes | yes |
| Family Controls | no | no | no | no | yes | yes |

Standard errors in parentheses

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

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

Table 5: Heterogeneous Treatment Effects by Policy Effectiveness Based on 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 | -0.024 | -0.047*** | 0.016 | 0.006 |
| | (0.050) | (0.013) | (0.048) | (0.019) |
| $R^2$ | 0.055 | 0.054 | 0.007 | 0.023 |
| Observations | 4112 | 39004 | 6234 | 12708 |
| Individual FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | no | no | no | no |
| Group Trend | yes | yes | yes | yes |
| Individual Controls | yes | yes | yes | yes |
| Family Controls | yes | yes | yes | yes |

Standard errors in parentheses

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

Notes: This table reports the heterogeneous treatment effects results comparing rural and urban population with various house status differently impacted by the house purchase restrictions. The sample consists of all unregulated cities, and the data span from 2009 to 2020. Regressions are at the individual-year level. The subsamples are mobile population who do not have any self-owned property in column (1), rural single house owner who live in rural area with only one self-owned property in column (2), rural multiple house owner who live in rural area with more than one self-owned property in column (3) and urban house owner who live in urban area having self-owned property, which means at least one, in column (4). Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2018, the first full year after the HPR spillover shock taking into account the pregnancy delay. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual control variables are age, $age^2$, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 6: Heterogeneous Treatment Effects by Policy Effectiveness Based on Proximity to Schools in Urban and Rural Areas

| | (1)<br>Rural,<br>Schools Distant | (2)<br>Rural,<br>Schools Nearby | (3)<br>Urban,<br>Schools Distant | (4)<br>Urban,<br>Schools Nearby |
|---|---|---|---|---|
| Dependent Variable: Number of Newborns | | | | |
| Treat×Post | -0.059*** | -0.004 | -0.010 | 0.003 |
| | (0.016) | (0.016) | (0.023) | (0.025) |
| $R^2$ | 0.051 | 0.050 | 0.030 | .036 |
| Obs | 23836 | 23608 | 7554 | 7060 |
| Individual FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | no | no | no | no |
| Group Trend | yes | yes | yes | yes |
| Individual Controls | yes | yes | yes | yes |
| Family Controls | yes | yes | yes | yes |

Standard errors in parentheses

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

Notes: This table reports the heterogeneous treatment effects results comparing rural and urban population averagely far from or close to schools differently impacted by the house purchase restrictions. The sample consists of all unregulated cities, and the data span from 2009 to 2020. Regressions are at the individual-year level. The subsamples are population living in rural area and local schools are averagely far from home in column (1), population living in rural area and local schools are averagely close to home in column (2), population living in urban area and local schools are averagely far from home in column (3) and population living in urban area and local schools are averagely close to home in column (4). Whether the county by rural/urban area have distant schools or not are designated by the average distance from home to local schools. If the area average home-school distance is larger than national rural/urban-specific average, this area is designated as ”Schools Distant”, and vice versa. Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2018, the first full year after the HPR spillover shock taking into account the pregnancy delay. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual control variables are age, age$^2$, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

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

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

| | (1)<br>New Marriage | (2)<br>New Marriage | (3)<br>New Marriage | (4)<br>New Marriage |
|---|---|---|---|---|
| Treat×Post | -0.041*** | -0.041*** | -0.033*** | -0.033*** |
| | (0.012) | (0.012) | (0.012) | (0.012) |
| $R^2$ | 0.111 | 0.106 | 0.127 | 0.123 |
| Observations | 66332 | 66332 | 62152 | 62152 |
| Individual FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | yes | no | yes | no |
| Group Trend | no | yes | no | yes |
| Individual Controls | no | no | yes | yes |
| Family Controls | no | no | yes | yes |

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

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

| | (1)<br>Does Not Own Any | (2)<br>Rural (Does Not Own Urban Home) | (3)<br>Rural (Does Own Urban Home) | (4)<br>Urban (Does Own Urban Home) |
|---|---|---|---|---|
| Dependent Variable: New Marriage | | | | |
| Treat×Post | -0.012 | -0.048*** | 0.029 | 0.007 |
| | (0.050) | (0.014) | (0.035) | (0.022) |
| $R^2$ | 0.576 | 0.223 | 0.282 | 0.265 |
| Observations | 4124 | 39068 | 6244 | 12716 |
| Individual FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | no | no | no | no |
| Group Trend | yes | yes | yes | yes |
| Individual Controls | yes | yes | yes | yes |
| Family Controls | yes | yes | yes | yes |

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

Notes: These tables show the results of the competitive marriage market. Panel (a) reports the difference-in-differences regressions of the likelihood of new marriage using CFPS data with respect to the spillovers from the imposition of house purchase restrictions. Panel (b) reports the heterogeneous treatment effects results, comparing rural and urban population with various house status, differently impacted by the house purchase restrictions. The sample consists of all unregulated cities. The new marriage data spans from 2009 to 2020. The subsamples in panel (b) are mobile population who do not have any self-owned property in column (1), rural single house owner who live in rural area with only one self-owned property in column (2), rural multiple house owner who live in rural area with more than one self-owned property in column (3) and urban house owner who live in urban area having self-owned property, which means at least one, in column (4). The dependent variables are the incidences of new marriage of each individual in each year. Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2017. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual control variables are age, age$^2$, education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 8: DID Estimated Effects of House Purchase Restrictions on Number of Newborns in Married Population

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

| | (1)<br>Newborns | (2)<br>Newborns | (3)<br>Newborns | (4)<br>Newborns |
|---|---|---|---|---|
| Treat×Post | -0.036** | -0.038*** | -0.026** | -0.027** |
| | (0.014) | (0.014) | (0.013) | (0.013) |
| Mean | 0.078 | 0.078 | 0.078 | 0.078 |
| $R^2$ | 0.051 | 0.048 | 0.062 | 0.061 |
| Observations | 47236 | 47236 | 47236 | 47236 |
| Individual FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | yes | no | yes | no |
| Group Trend | no | yes | no | yes |
| Individual Controls | no | no | yes | yes |
| Family Controls | no | no | yes | yes |

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

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

| | (1)<br>Does Not<br>Own Any | (2)<br>Rural<br>(Does Not Own<br>Urban Home) | (3)<br>Rural<br>(Does Own<br>Urban Home) | (4)<br>Urban<br>(Does Own<br>Urban Home) |
|---|---|---|---|---|
| Dependent Variable: Number of Newborns | | | | |
| Treat×Post | -0.052 | -0.051*** | 0.035 | 0.014 |
| | (0.064) | (0.016) | (0.067) | (0.024) |
| $R^2$ | 0.060 | 0.062 | 0.015 | 0.033 |
| Observations | 2934 | 29600 | 5012 | 9690 |
| Individual FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | no | no | no | no |
| Group Trend | yes | yes | yes | yes |
| Individual Controls | yes | yes | yes | yes |
| Family Controls | yes | yes | yes | yes |

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

Notes: These tables reports the difference-in-differences results and heterogeneous treatment effects results of number of newborns in married population impacted by the house purchase restrictions. The sample consists of all unregulated cities, and the data span from 2009 to 2020. Regressions are at the individual-year level. The subsamples in panel (b) are mobile population who do not have any self-owned property in column (1), rural single house owner who live in rural area with only one self-owned property (2), rural multiple house owner who live in rural area with more than one self-owned property in column (3) and urban house owner who live in urban area having self-owned property, which means at least one, in column (4). Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to year 2018, the first full year after the HPR spillover shock taking into account the pregnancy delay. City Trend is a city-specific linear trend, and Group Trend is a treatment-group-specific linear trend. The individual control variables are age, $age^2$, education level, marital status, marital status×spouse's education level, party membership, urban residence, migratory status, health score, and housing tenure. The family control variables are per capita family net income and mortgage debts. Standard errors are clustered at the city level.

Table 9: DID Estimated Effects of House Purchase Restrictions on Educational Investments and Heterogeneous Treatment Effects

**(a) DID Estimated Effects of House Purchase Restrictions on Educational Investments**

| | (1)<br>Educational Investments | (2)<br>Educational Investments | (3)<br>Educational Investments | (4)<br>Educational Investments |
|---|---|---|---|---|
| Treat × Post | 0.518** | 0.496*** | 0.497** | 0.442** |
| | (0.250) | (0.188) | (0.224) | (0.198) |
| $R^2$ | 0.252 | 0.245 | 0.275 | 0.268 |
| Observations | 25880 | 25880 | 23671 | 23671 |
| Household FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | yes | no | yes | no |
| Group Trend | no | yes | no | yes |
| Family Controls | no | no | yes | yes |

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

**(b) Heterogeneous Treatment Effects - Housing tenure in Rural and Urban Areas**

| | (1)<br>Does Not Own Any | (2)<br>Rural (Does Not Own Urban Home) | (3)<br>Rural (Does Own Urban Home) | (4)<br>Urban (Does Own Urban Home) |
|---|---|---|---|---|
| Dependent Variable: Educational Investments | | | | |
| Treat×Post | 0.920 | 0.582*** | -0.669 | 0.391 |
| | (1.280) | (0.161) | (1.288) | (0.656) |
| $R^2$ | 0.491 | 0.283 | 0.385 | 0.283 |
| Observations | 536 | 14899 | 1617 | 3808 |
| Household FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | no | no | no | no |
| Group Trend | yes | yes | yes | yes |
| Family Controls | yes | yes | yes | yes |

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

**(c) Heterogeneous Treatment Effects - Proximity to Schools in Rural and Urban Areas**

| | (1) Rural, Schools Distant | (2) Rural, Schools Nearby | (3) Urban, Schools Distant | (4) Urban, Schools Nearby |
|---|---|---|---|---|
| Dependent Variable: Educational Investments | | | | |
| Treat×Post | 0.055 | 0.779*** | 1.079* | -0.196 |
| | (0.272) | (0.190) | (0.549) | (0.859) |
| $R^2$ | 0.251 | 0.303 | 0.278 | 0.318 |
| Observations | 8562 | 10039 | 2238 | 2038 |
| Household FE | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes |
| City Trend | no | no | no | no |
| Group Trend | yes | yes | yes | yes |
| Family Controls | yes | yes | yes | yes |

Standard errors in parentheses

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

Notes: These tables show the results of the regressions of educational investments. Panel (a) reports the difference-in-differences regressions of educational investments using CFPS data with respect to the spillovers from the imposition of house purchase restrictions. Panels (b) and (c) report the heterogeneous treatment effects results, comparing rural and urban population with various house tenure and rural and urban population with different proximity to local schools, differently impacted by the house purchase restrictions. The sample consists of all unregulated cities. The educational investments data span from 2009 to 2020. Regressions are at the household-year level. The subsamples in panel (b) are mobile population who do not have any self-owned property in column (1), rural single house owner who live in rural area with only one self-owned property in column (2), rural multiple house owner who live in rural area with more than one self-owned property in column (3) and urban house owner who live in urban area having self-owned property, which means at least one, in column (4). The subsamples in panel (c) are population living in rural area and local schools are on average far from home in column (1), population living in rural area and local schools are on average close to home in column (2), population living in urban area and local schools are on average far from home in column (3) and population living in urban area and local schools are on average close to home in column (4). Treat is a dummy that takes the value 1 if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value 1 if the time is after or equal to the year 2017. City Trend is a city-specific linear trend, and the results of controlling this fixed effect are in columns (1) and (3) of panel (a). Group Trend is a treatment-group-specific linear trend, and the results of controlling this fixed effect are in columns (2) and (4) of panel (a), and all columns of panels (b) and (c) . The family control variables are urban residence, housing tenure, log per capita family net income, log total asset, and migratory status. Standard errors are clustered at the city level.

Table 10: The Treatment Effects of the House Price Shock on Different Age Groups

**(a) Number of Newborns**

| | (1) Age: 15-19 | (2) Age: 20-29 | (3) Age: 30-39 | (4) Age: 40-44 | (5) Under 35 | (6) 35 and Over |
|---|---|---|---|---|---|---|
| Dependent Variable: Number of Newborns | | | | | | |
| Treat×Post | 0.009 | -0.043** | -0.013 | 0.016 | -0.035** | -0.007 |
| | (0.014) | (0.020) | (0.019) | (0.011) | (0.014) | (0.010) |
| $R^2$ | 0.188 | 0.034 | 0.033 | 0.032 | 0.048 | 0.032 |
| Observations | 7134 | 20862 | 20798 | 13264 | 38544 | 23514 |
| Individual FE | yes | yes | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes | yes | yes |
| City Trend | no | no | no | no | no | no |
| Group Trend | yes | yes | yes | yes | yes | yes |
| Individual Controls | yes | yes | yes | yes | yes | yes |
| Family Controls | yes | yes | yes | yes | yes | yes |

Standard errors in parentheses

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

**(b) Likelihood of New Marriage**

| | (1) Age: 15-19 | (2) Age: 20-29 | (3) Age: 30-39 | (4) Age: 40-44 | (5) Under 35 | (6) 35 and Over |
|---|---|---|---|---|---|---|
| Dependent Variable: New Marriage | | | | | | |
| Treat×Post | -0.015 | -0.066* | -0.011 | -0.002 | -0.052** | -0.000 |
| | (0.023) | (0.036) | (0.012) | (0.002) | (0.020) | (0.003) |
| $R^2$ | 0.292 | 0.105 | 0.619 | 0.219 | 0.091 | 0.714 |
| Observations | 7148 | 20922 | 20806 | 13276 | 38620 | 23532 |
| Individual FE | yes | yes | yes | yes | yes | yes |
| Year FE | yes | yes | yes | yes | yes | yes |
| City Trend | no | no | no | no | no | no |
| Group Trend | yes | yes | yes | yes | yes | yes |
| Individual Controls | yes | yes | yes | yes | yes | yes |
| Family Controls | yes | yes | yes | yes | yes | yes |

Standard errors in parentheses

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

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

Table 11: Placebo Test of Local Rural-to-urban Migration

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

Standard errors in parentheses

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

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

Table 12: Placebo Tests of One Child Policy

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

Standard errors in parentheses

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

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