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**Published in:**
Humanities and Social Sciences Communications

**Published: 01/01/2024**

**Document Version:**
Final Published version, also known as Publisher's PDF, Publisher's Final version or Version of Record



**Published version (DOI):**
10.1057/s41599-024-03734-7

**Publication details:**
Chang, Z., Li, W., Diao, M., & Li, X. (2024). Marriage entry, divorce and reconciliation: The unintended consequence of the home purchase restriction policy in China. *Humanities and Social Sciences Communications*, *11*, Article 1229. https://doi.org/10.1057/s41599-024-03734-7

Humanities & Social Sciences Communications

ARTICLE

https://doi.org/10.1057/s41599-024-03734-7 OPEN

# Marriage entry, divorce and reconciliation: The unintended consequence of the home purchase restriction policy in China

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This study investigates the unintended socioeconomic consequences of housing market regulations, focusing on the home purchase restriction (HPR) policy implemented in cities across China. While the primary aim of the HPR policy is to curb property prices by limiting the number of properties a household can purchase, its broader impact on family formation, particularly marriage and divorce patterns, remains underexplored. Applying Becker's economic theory of marriage, which views marital decisions as rational choices influenced by socioeconomic factors, this study examines how the HPR policy affects family formation and dissolution across different city tiers and age groups in China. Our analysis reveals several key findings. First, the HPR policy effectively contributed to a decrease in house prices, aligning with its primary objective. However, it also led to delayed marriages among younger age groups, especially in first-tier cities, where housing affordability is a significant concern. Second, the policy inadvertently encouraged strategic divorces, allowing couples to circumvent purchase restrictions and acquire additional properties. This loophole undermines the policy's effectiveness in reducing housing demand and stabilizing prices. These results highlight the complex interplay between housing market regulations and family demographics, emphasizing the need for policymakers to consider the broader societal impacts of non-family-oriented policies. As China, like many other societies, faces challenges related to the second demographic transition—such as declining marriage and fertility rates—the findings of this study serve as a cautionary tale. Housing policies, while effective in addressing market concerns, can have significant and sometimes counterproductive effects on family stability and formation, potentially exacerbating the demographic trends a society aims to mitigate.

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## Introduction

In recent decades, the rise in delayed marriage, the increase in divorce, and the growth of cohabitation have been recognized as part of the second demographic transition (Zaidi & Morgan, 2017). Studies in this area explore the social, cultural, and economic factors contributing to this "less family" transition. According to Becker's (1973, 1974) marriage theory, marital status is a rational decision aimed at maximizing the utility between being single and being married, influenced by the wealth, education, and other socioeconomic characteristics of both partners. Applying economic principles to family studies, Becker attributed the erosion of the traditional family structure to women's career pursuit and men's difficulty in adapting to women's evolving roles in society. He also identified a shift towards individualistic lifestyle orientations that outweigh the opportunity costs of family commitments. In response to this less family trend, social scientists and policymakers have focused on developing policies directly targeting family structure to counteract declining marriage and fertility rates and increasing marital instability (Bergsvik et al., 2021; Huston & Melz, 2004; Karney & Bradbury, 2005). However, the unintended consequences of non-family-oriented policies on family demographics have often been overlooked. Notably, as homeownership is a critical factor in family formation (Hu & Wang, 2020; Mulder, 2006), policies aimed at housing affordability may significantly influence marriage trends.

As the affordable housing crisis has emerged as a global concern, municipalities worldwide have explored various approaches to intervene in the private housing market to bring prices down. One policy means is to arbitrarily curb demand by establishing purchase eligibility rules for homebuyers. For example, Florida passed a law in 2023 restricting foreigners from purchasing property. Many China cities implemented the home purchase restriction (HPR) policy since 2010 to limit the maximum number of properties a household can purchase and impose mortgage constraints on home finances. Numerous studies have examined the impact of this price-oriented regulation on local property markets, primarily focusing on whether it leads to a decrease in property prices (e.g., Li et al., 2017; Somerville et al., 2020; Sun et al., 2017; Wu & Li, 2018) or a slower price growth (e.g., Du & Zhang, 2015; Yan et al., 2022; Yu & Zhang, 2019). However, it is important to recognize that housing market regulations can have far-reaching socioeconomic impacts beyond mere price changes. Less attention has been paid to whether and to what extent these price-oriented housing regulations generate unintended socioeconomic consequences, such as marriage patterns, divorce risks, and birth rates, with the former two issues being the focus of our paper.

This study is a policy evaluation aimed at raising policymakers' awareness of the changes in family demographics caused by non-family-related policies. We expect that the HPR policy may affect marriages and marital commitments in two ways. On the one hand, because homeownership is considered a prerequisite for marriage in Chinese society (Fang & Tian, 2018; Mu et al., 2022; Zang, 2020), the policy-induced drop in house prices may encourage family formation. On the other hand, the policy may lead to marital dissolution because restrictions on purchase quotas and mortgage terms can make it economically advantageous for couples to divorce and become eligible to own additional properties. Our empirical analysis involves large and medium Chinese cities, categorized within the top three tiers of China's city classification system. We assess the policy's varying impacts on marriage entry by city tier and age group, as well as its impacts on divorce, remarriage, and marriage reconciliation by city tier.

This paper contributes to existing knowledge from both academic and public policy perspectives. First, our empirical synthesis connecting housing policies and family formation provides evidence supporting Becker's economic perspective on marriage, suggesting that couples make rational decisions on marriage and divorce in the Chinese marriage market. Second, our study offers empirical evidence to supplement the inconclusive findings in existing literature on the relationship between house prices and marriage stability. While some studies have found falling prices lead to higher divorce rates, others have observed lower divorce risks associated with declining prices. Third, we statistically identify a loophole in the housing purchase restriction policy, showing that couples can circumvent these restrictions through paper divorces. Our findings suggest that this loophole could undermine the policy's effectiveness in suppressing house prices by stimulating demand as more couples become singles. Fourth, we demonstrate that housing market regulations can have unexpected effects on the marriage market, such as delaying marriages among young people and jeopardizing marital stability. As many societies, including China, face the second demographic transition, they are seeking ways to encourage family formation, enhance family stability, and incentivize childbearing. The findings from this study serve as a caution to policymakers about the unintended consequences a housing policy may have on the demographic transition that a society seeks to reverse.

Our key findings can be summarized as follows. First, the HPR policy contributed to a decrease in house prices. Second, the policy led to delayed marriages among younger age groups, especially in first-tier cities. Third, households employed strategic divorce and marriage reconciliation tactics to navigate the purchase restrictions. The rest of the paper is organized as follows: the next section presents the background of the HPR policy, followed by a literature review of the relationship between house prices and marriage, as well as between house prices and the HPR policy. The next section presents the empirical strategy and the summary of data and statistics. After presenting the empirical results on the impact of HPR on house prices, we explore the causal impact of HPR on family formation and dissolution. The final section concludes the paper.

## Policy background – Home purchase restriction policy

Over the past two decades, surging property prices in Chinese cities have prompted national and subnational governments to implement measures to mitigate potential economic and social problems arising from the affordability crisis. One approach focuses on the supply side, involving large-scale land development and the massive construction of public housing (Chen et al., 2014; Deng et al., 2011). Another approach addresses the demand-side, aiming to suppress speculative investments in the local property market. Higher returns on housing investments compared to the stock market in China have motivated both local and non-local households to purchase multiple properties for investment purposes (Fang et al., 2015). Examples of demand-side regulations include levying additional stamp duties on non-local homebuyers (e.g., in Hong Kong), imposing restrictions on minimum down payment for multiple homeowners, and establishing eligibility rules to directly control the number of potential buyers (e.g., in mainland China).

The home purchase restriction (HPR) policy promoted by China's central government in April 2010 is a restrictive intervention in cities' housing markets to curb speculative investments. Beijing was the first to adopt the policy on April 30 that year; and within a year, 46 cities followed suit, including other first-tier cities and twenty-eight provincial capital cities. With minor variations in provisions across municipalities, the HPR

policy typically sets eligibility rules for homebuyers based their residency status, focusing on purchase quotas and mortgage conditions. For example, only households with local residency status—defined as individuals with local hukou[1] or those without local hukou but able to prove a certain number of years of tax or social security payment records—are eligible to purchase a limited number of properties (typically 1 to 2) within municipal jurisdiction. Most HPR cities allow residents with local hukou to purchase at most one housing unit if unmarried, while a married couple (with at least one spouse having local hukou) can purchase up to two units. Residents without hukou can buy only one unit after working in the city for 3 to 5 years, provided they can demonstrate tax or social security payments. Eligible homebuyers also face restrictive mortgage conditions. Banks in cities experiencing rapid housing price increases are advised to suspend mortgage lending to households owning more than two properties. In most HPR cities, the minimum down payment rate for the first unit is 30%, rising to 50% for the second. First-tier cities set a 70% down payment requirement for the second unit.

However, the nationwide promotion of the HPR policy did not dictate which municipality should adopt the policy or, if it does, when and for how long. Contrary to the common belief that a city's decision is primarily influenced by its housing market conditions, local governments' adoption of the policy may have been driven by non-economic factors. Zou et al. (2022) identified four motivations: the desire to enhance housing affordability for residents, concerns about speculative investments by multiple homeowners and non-local buyers, pressure to emulate peer municipalities, and the need to showcase policies recommended by higher authorities. Some provincial capital cities with low housing prices still implemented HPR due to political pressure to benchmark other capital cities. Conversely, many coastal cities with high housing prices chose not to implement HPR, such as Zhongshan, Changzhou, Shantou, Yantai, and Ningbo. These cities, which did not impose restrictions, generally have lower political importance (Somerville et al., 2020). Interestingly, all municipalities except the four first-tier cities and Sanya in Hainan province rescinded the policy in 2014 due to a declining local property market; and then in 2016, fifteen of them reinstated the policy due to rapidly rising house prices. Among them, seven are new first-tier cities and eight belong to other tiers. This indicates that municipalities did rely on the policy to control local housing markets.

The HPR policy may also affect the marriage market. A unique provision of HPR allows a married couple who are both permanent residents to jointly own one property, or if they divorce, own two properties separately. This provision, initially designed to safeguard the homeownership rights of divorcees, creates a policy loophole. Some couples may exploit this loophole by signing a paper divorce to gain access to a lower down payment and mortgage rate for second home purchases, especially in large cities. For instance, a couple may choose to divorce and become two single individuals, with one spouse retaining the existing housing unit and the other, who has a hukou, then purchasing a new house as the first home with a lower down payment rate (e.g., 30%). After acquiring the new house, they would legally restore their marriage. In first-tier cities, the down payment rate for the second unit is higher, at 70%, compared to the 50% rate in other HPR cities. This strategic divorce and marital reconciliation tactics are relatively prevalent in first-tier cities, allowing households to save 40% on the down payment. Chinese local governments have recognized this policy loophole and revised related provisions in 2021. For example, starting from January 2021, Shanghai stipulated that if either party of a divorced couple purchases housing within three years of divorce, the number of housing units owned is calculated based on the total number of units held by the family before the divorce. Beijing also introduced similar conditions in August 2021.

Considering the intermittency and the newly added provisions of the policy in recent years, we set our study period from 2007 to 2016. This timeframe covers approximately 4–5 years before and 5–6 years after the policy implementation, depending on when each city enacted the policy. This period starts with the availability of China's official housing price statistics for major large- and medium-sized cities and ends with the restoration of the HPR policy in these cities. Also note that despite facing pressure of the 2008 global financial crisis and the central government's 4 trillion yuan stimulus program, China's housing market experienced only a slight drop and recovered to its pre-crisis level in less than a year (Fang et al., 2015).

## Literature review – relationships of house prices, marriage, and home purchase restriction policy

A substantial number of family policy studies have probed various factors that contribute to changes in marital status, such as family values and relationship skills (Karney & Bradbury, 2005), as well as adverse conditions outside of marriage like financial difficulties and lack of social support (Conger et al., 1999). The most influential view originates from Gary Becker, a Nobel Prize laureate in economics, who developed a theory of marriage applying economic principles to family behavior. In his seminal papers, Becker (1973, 1974) posited that individuals make rational decisions about marriage based on a cost-benefit analysis to maximize personal utility or satisfaction. People marry to increase their well-being, considering factors such as income, companionship, children, and social status. Based on this assumption, Becker developed economic models that yield several implications about behavior in the marriage market. One implication concerns marriage stability and dissolution. Decisions about staying in or leaving a marriage depend on the comparative costs and benefits. Higher costs of divorce—emotional, financial, or social—tend to increase marriage stability. As circumstances change, such as shifts in income, health, or preferences, the benefits and costs of marriage can also shift, affecting its stability. In this sense, our discussions on the causal effect of housing policies on marriages are grounded in the economic rationale of marriage theory.

Among those internal and external factors influencing family formation, accessibility to homeownership has been highlighted as a key condition for marriage (Grinstein-Weiss et al., 2014; Hu & Wang, 2020; Mulder, 2006). Related to homeownership is the level of house prices in the property market. Consequently, one stream of literature focuses on establishing a relationship between house prices and marriages under the assumption that a couple's marital commitment would be affected by family asset losses or gains from staying married versus divorcing.

For example, Rainer and Smith (2010) studied the UK's housing market slump in the 2000s and found that negative house price shocks significantly contributed to higher divorce rates for owner-occupier couples, especially young low-income couples with high mortgage debt. They argued that the negative shocks directly reduced the wealth of those families, who then expected to gain less from marriage than from divorce. Similarly, Farnham et al. (2011) examined the US housing market in the 2000s and found that declining house prices significantly decreased the divorce risk for homeowners due to high transaction costs of home sales during a housing downturn. Alternatively, Klein (2017)'s study, covering the period from 1980 to 2010 in the US, failed to establish a statistically significant correlation between marital stability and the housing market slump; instead, Klein found stronger marital commitments linked to rising house

prices. Conversely, in an Iranian study, Farzanegan and Gholipour (2016) revealed that an increase in housing cost destabilized marital status.

These results, although inconclusive, at least suggest a strong statistically significant correlation between house prices and marriages. Additionally, these studies using econometric methods—either ordinary least squares regression (Rainer & Smith, 2010) or fixed effects models (Farnham et al., 2011; Farzanegan & Gholipour, 2016)—are limited in revealing the causal relationship between house prices and marital status.

In the Chinese context, only a few studies have raised concern about the potential negative impacts of the booming housing market on delayed marriage and marital stability. Using data from 259 cities between 2000 and 2005, Wrenn et al. (2019) asserted that the increasingly unaffordable properties were partially responsible for the declining initial marriage rates and the rising age at first marriage. Alternatively, Zheng et al. (2018), using provincial-level data from 1997 to 2015, focused on divorce rates and house prices. They found that in the eastern region, rising housing prices caused more divorces in the short run; and in the long run, the causal direction reversed, with higher divorce rates leading to higher house prices. Interestingly, their study period overlaps with the implementation of the HPR policy, but Zheng et al. (2018) did not explicitly discuss the policy impact.

While these studies offer perspectives on the housing market and marital stability, there is a scarcity of research on the policy dimension. Little is known about how government interventions specifically targeting the housing market could lead to unexpected social outcomes, such as delayed marriages, low birth rates, and increased divorces. The study most closely related to this paper is by Alm et al. (2022), who attempted to determine how housing market regulations affected strategic divorce in Chinese cities. In their difference-in-differences models where the same set of cities during the regulated and deregulated periods were set as the control and treatment groups, Alm et al. (2022) found a positive relationship between the implementation of HPR (a dummy factor) and divorce propensity. The limitation of their study is the measurement of divorce. Rather than using actual divorce data, they relied on divorce-related keyword searches on Baidu (a Google equivalent in China) to measure divorce propensity.

One widely adopted analytical tool for estimating the causal effects of new policies, especially in housing studies, is the difference-in-differences (DID) method (e.g., Chang & Li, 2021; Wu & Li, 2018; Yan et al., 2022). Essentially, DID models compare the outcomes of a group exposed to the policy intervention (treatment) and another group not exposed (control), both before and after the intervention. This comparison operates under the assumption that the differences between the treatment and control groups would remain the same over time if the policy never existed. One criticism of DID studies is policy endogeneity, which can render treatment and control groups uncomparable and generate unreliable causal estimates. To reduce this selection bias in DID studies, propensity score matching has been popularly used for choosing the control group closely mirroring the treatment group in observed covariates (Yan & Ouyang, 2018). However, this matching method also raises concerns regarding its effectiveness in reducing the distribution imbalance of treatment confounders between treated and control groups (King & Nielsen, 2019). Admittedly, a city's decision on implementing the HPR policy may be endogenously determined by its local property market conditions, meaning that only cities with high house prices would adopt HPR. To identify matching cities in our study, we adopt a special approach by predicting a city's intention of policy implementation back in 2010 based on pre-policy conditions of key socioeconomic indicators. This approach allows us to create a treatment group, consisting of cities that did adopt HPR after 2010, and a *comparable* control group, consisting of cities that could have adopted the policy but did not do so.

## Research design and data

**Empirical strategy**. This study examines the HPR's causal impact on family formation patterns using city-level data from 2007 to 2016 in China. Our initial sample size of 125 includes all cities designated as first, second, and third-tier cities in 2010. This sample covers nearly all cities that implemented the HPR policy during the study period. We utilize a logit regression model, as represented in Eq. (1), to identify the socioeconomic variables that possess substantial predictive power concerning a city's adoption of the HPR policy.

$$HPR_i = a_0 + a_1 X_i + \varepsilon_i \tag{1}$$

where $HPR_i$ is a binary variable that equals 1 for cities that implemented HPR during the study period, 0 otherwise. $X_i$ represents a vector of socioeconomic variables for cities in 2010, including GDP, population, fixed asset investment, annual municipal revenue, and housing affordability index. $\varepsilon_i$ is an error term.

The outcomes obtained from Eq. (1) enable us to differentiate between cities that implemented HPR and those that had economic justifications for the policy but refrained from doing so. Then we can match cities into treatment and control groups, ensuring the treatment-control pairs have comparable initial socioeconomic conditions but opposite HPR policy choices. Eventually, 71 cities remained for the subsequent regression analyses over ten years. We proceeded to investigate the causal impact of HPR on family formation with Eq. (2):

$$Y_{it} = a_0 + a_1 HPR_{it} + trend_{it} + \mu_i + \tau_t + \varepsilon_{it} \tag{2}$$

where $Y_{it}$ represents the outcome variable in city $i$ during year $t$ (e.g., housing price, marriage entry, divorce, remarriage, and reconciliation). $HPR_{it}$ is an event dummy that equals 1 if city $i$ implemented HPR in year $t$, and 0 otherwise. $trend_{it}$ signifies a linear trend of the outcome variable specific to city $i$ to control the city's endogenous factors overtime like unemployment and foreign direct investment. $\mu_i$ is the city-fixed effect, capturing all observable or unobservable time-invariant factors for city $i$. $\tau_t$ is the year-fixed effect, capturing all factors that are consistent across all cities in each year. $\varepsilon_{it}$ stands for the error term.

Equation (3) is utilized to assess the pre-policy parallel trend assumption using the standard event study method. To determine the validity of this assumption, the coefficients for each year before the implementation of HPR are examined. The parallel trend assumption holds if these coefficients are not significantly different from 0.

$$Y_{it} = b_0 + \sum_{k \neq -1} B_k HPR_i \times 1\left[Year_t = k\right] + trend_{it} + \mu_i + \tau_t + \varepsilon_{it} \tag{3}$$

where $1[Year_t = k]$ is an event time indicator, which equals 1 for each year before and after the HPR policy. $Year_t = -1$ serves as the reference point for comparison. All other variables remain consistent with previous specifications.

**Regarding heterogeneous treatment effects**. Equation (2) is commonly known as the two-way fixed effect (TWFE) DID model in empirical research. Recent scholarship has cautioned the potential biases arising from heterogeneous treatment effects when using this conventional approach (Goodman-Bacon, 2021; Sun & Abraham, 2021). De Chaisemartin and D'Haultfoeuille (2020) explained how the TWFE DID model essentially calculates a weighted average of individual treatment effects and introduced the concept of the "negative weighting" problem, which implies

that a TWFE DID estimate might appear negative even when all individual treatment effects are positive. Such anomalies become more likely when treatments are administered at disparate intervals over an extended period, leading to significant heterogeneous treatment effects.

Keeping these challenges in mind, we retain Eq. (2) as our primary analytical method for two reasons in the context of staggered DID research designs. First, following the guidelines outlined by De Chaisemartin and D'Haultfoeuille (2020), we scrutinize the individual treatment effect weights and confirm that they are all positive in our regression, eliminating any "negative weighting" bias. Second, for robustness, we apply the estimators recommended by De Chaisemartin and D'Haultfoeuille (2020), and these estimates produce results that align with our baseline estimations.

**Data and summarized statistics**. We gathered data from multiple sources. Information about cities' HPR status, including the implementation dates for 46 cities from 2007 to 2016, was manually compiled from municipal government websites and Baidu Baike, the Chinese equivalent of Wikipedia. Family formation statistics were obtained from multi-year statistical yearbooks published by China Civil Affairs, allowing us to extract data on variables such as the annual number of marriages, divorces, and remarriages, and reconciliations. Socioeconomic statistics, such as GDP, population, fixed asset investment, and annual municipal revenue came from China City Statistical Yearbooks. Housing price data were also compiled from multi-year city yearbooks, which publish average prices of commodity housing sales in major cities. Additionally, we acquired the housing affordability index defined as the ratio of housing expenditure to income from Li et al. (2020).

Our sample covers all Chinese cities belonging to the first three tiers, a city classification system commonly adopted in literature and accepted by the general public. The first tier refers to the four most populous cities with significant economic importance, namely Beijing, Shanghai, Guangzhou, and Shenzhen. The second tier includes Tianjin and Chongqing, 24 provincial capital cities, and nine other cities that are the country's major industrial and commercial centers. The rest are grouped as lower tier cities (Fang et al., 2015). Given the country's urbanization rate above 50% since 2011, a "New Tier 1" city category is introduced to identify fast-growing second-tier cities. This new first tier consists of 15 cities out of the original second tier, as proposed by Yicai Global, a Chinese business news outlet. Consequently, our regression models explore the impact of HPR on three city groups: Tier 1, New Tier 1, and other Tiers (comprising the remaining cities in Tiers 2 and 3).

Table 1 presents the definitions and summary statistics of the variables for Chinese cities from 2007 to 2016. Column (1) displays statistics for all 125 cities, while column (2) reports figures for the subset of 71 cities selected for the DID regressions. Columns (3) and (4) provide statistics for the treatment and control groups, respectively, with 40 cities in the treatment and 31 cities in the control group. On average, the treated cities exhibit a higher level of socioeconomic status than cities in the control group.

## Home purchase restriction and house prices

**Predicting a city's intention to implement HPR**. We begin our analysis by identifying socioeconomic variables that exhibit a strong predictive power for a city's decision on implementing HPR. In our sample comprising 125 Tiers 1–3 cities in 2010, we conduct a series of logit regression analyses using Eq. (1), and the results are summarized in Table 2.

In column (1), we include the natural logarithms of a city's GDP, population, and the amount of fixed asset investment as independent variables. The results indicate that both GDP and population are strongly correlated with the HPR dummy. In column (2), we introduce an additional control variable, local government revenue. Interestingly, the results show that local revenue is the only variable significantly related to the decision to implement HPR, with coefficients for other variables no longer statistically significant. Building upon the previous models, in column (3), we include the city's housing price factor. Here, housing price emerges as the only significant variable for explaining a city's adoption of HPR. This finding aligns with the primary goal of HPR, which is to cool down overheated housing markets. Cities with high housing prices in 2010 are much more likely to implement HPR. Finally in column (4), we add the 2010 affordability index as an additional control variable. Remarkably, the coefficient for affordability is not only substantial but also robust, while coefficients on all the other variables (including the housing price factor) become relatively small and insignificant. As we progressively include more control variables from columns (1) to (4), the pseudo-$R^2$, which measures the goodness-of-fit of the model, increases from 32.7% to 62.8%. Consequently, this stepwise logit regression exercise strongly suggests that a city's inclination to implement HPR can be reliably predicted based on its 2010 affordability index.

**Policy impact on house prices**. Before delving into the examination of how HPR affects family formation, it is imperative to explore the magnitude of policy's impact on house prices. This step validates that our DID model can produce policy-price relationship results consistent with those of similar existing studies, ensuring the robustness of the chosen DID model for subsequent policy impact regressions involving family pattern variables. In Table 3, we present the causal impact results derived from Eq. (2), where the dependent variable is the natural logarithm of average house prices, and standard errors are clustered at the city level.

In column (1) of Table 3, a basic two-way fixed effect DID model is applied to the full sample of 125 cities, which are simply separated into HPR cities (treatment) and non-HPR cities (control). The results suggest that HPR can drive down the house prices by about 5.8% at a 1% statistically significant level. To account for idiosyncratic city trends, we introduce the city linear trend to the full sample regression in column (2). We find that HPR lowers average housing prices by 7.9% for cities in the treatment group compared to those in the control group.

To bolster the comparability of city pairs within DID models and ensure more robust estimates, we incorporate an additional criterion for selecting cities in the control group—the affordability index as of 2010. We find that more than 90% of the 44 HPR cities had an affordability index above 0.36 in 2010. Thus, we choose cities with an affordability score greater than the cutoff point of 0.36. Ultimately, a sample of 71 cities remains, with 40 in the treated group and 31 in the control group. The results for this reduced sample are presented in column (3) of Table 3. HPR has contributed to an average reduction of 9.1% in housing prices at a 1% statistically significant level. Our findings that the HPR policy has contributed to a reduction in house prices are consistent with similar city-level studies (Li et al., 2017; Wu & Li, 2018; Yan et al., 2022). This indicates that cities that have forcefully controlled housing demand through HPR are successful in cooling down their housing markets.

For additional validation of the DID estimation, we examine the parallel trend assumption using Eq. (3). The results depicted in Fig. 1 confirm that the parallel trend assumption holds. House

**Table 1 Variable definition and summarized statistics.**

| | Variable | Description | Mean and standard deviation | | | |
|---|---|---|---|---|---|---|
| | | | [1] 125 cities | [2] 71 cities | [3] Treatment (40 cities) | [4] Control (31 cities) |
| | HPR*i* | Binary, 1 for HPR cities in 2007–16, 0 otherwise | 0.352 (0.478) | 0.563 (0.496) | | |
| | HPR*it* | Binary, 1 for HPR cities at year *t*, 0 otherwise | 0.151 (0.358) | 0.244 (0.430) | | |
| | H_Price | Average housing price (Yuan/square meter) | 5,018 (3,653) | 6,036 (4,110) | 7,979 (4,959) | 4,119 (1,388) |
| Control variable | GDP | Annual GDP (billion Yuan) | 295 (350) | 387 (427) | 539 (484) | 191 (219) |
| | Population | Population (million) | 5.38 (3.68) | 6.06 (4.33) | 6.14 (3.19) | 5.95 (5.46) |
| | Fixed | Fixed asset investment (billion Yuan) | 177 (182) | 224 (215) | 288 (212) | 140 (190) |
| | Revenue | Annual municipal revenue (billion Yuan) | 19.14 (54.83) | 41.52 (69.54) | 61.64 (84.19) | 15.56 (27.29) |
| | Affordability | Ratio of annual housing expenditure to income | 0.41 (0.20) | 0.50 (0.22) | 0.58 (0.26) | 0.38 (0.08) |
| City classification | Tier 1 | Binary, 1 for four Tier 1 cities, 0 otherwise | 0.032 (0.176) | 0.056 (0.231) | 0.100 (0.300) | 0 (0) |
| | New Tier 1 | Binary, 1 for 15 new Tier 1 cities, 0 otherwise | 0.120 (0.325) | 0.169 (0.375) | 0.275 (0.447) | 0.032 (0.177) |
| | Other Tiers | Binary, 1 for Tiers 2&3 cities, 0 otherwise | 0.848 (0.359) | 0.944 (0.231) | 0.900 (0.300) | 1.000 (0) |
| Family formation indicator | Marriage* | Number of marriages | 100.70 (73.21) | 115.89 (79.09) | 124.48 (71.31) | 107.49 (85.26) |
| | M_first | Number of first marriages | 87.07 (59.26) | 99.35 (62.73) | 105.65 (58.18) | 93.20 (66.38) |
| | M_20_24 | Number of marriages of 20–24 years old | 31.91 (26.42) | 35.00 (28.98) | 30.41 (21.06) | 39.48 (34.46) |
| | M_25_29 | Number of marriages of 25–29 years old | 36.97 (27.80) | 42.93 (29.64) | 50.64 (30.99) | 35.40 (26.19) |
| | M_30_34 | Number of marriages of 30–34 years old | 11.54 (9.96) | 13.69 (11.06) | 17.02 (11.74) | 10.43 (9.27) |
| | M_35_39 | Number of marriages of 35–39 years old | 6.25 (5.79) | 7.31 (6.43) | 8.38 (5.56) | 6.25 (7.02) |
| | M_over_40 | Number of marriages of >40 years old | 14.03 (14.26) | 16.96 (16.06) | 18.03 (14.92) | 15.92 (17.05) |
| | Divorce | Number of divorces | 11.15 (12.51) | 13.19 (14.44) | 16.27 (13.34) | 10.17 (14.84) |
| | Remarriage | Number of remarriages | 13.64 (17.88) | 16.53 (20.56) | 18.82 (18.05) | 14.30 (22.55) |
| | Reconciliation | Number of remarriages to original spouse | 1.37 (12.27) | 1.67 (2.68) | 2.26 (3.12) | 1.07 (2.00) |

*All figures of family formation indicators are in thousands. Standard deviation is in parentheses.

**Table 2 Results of Logit Regression.**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| lnGDP | 2.014** (0.833) | −0.804 (1.291) | −1.675 (1.319) | 0.427 (1.559) |
| lnPopulation | −1.565** (0.629) | −0.779 (0.771) | 0.205 (0.937) | −1.590 (1.104) |
| lnFixed | 0.595 (0.834) | −0.419 (0.761) | 1.426 (1.159) | 0.933 (1.109) |
| lnRevenue | | 3.323*** (1.240) | 1.120 (1.504) | 1.055 (1.488) |
| lnH_Price | | | 4.783*** (1.271) | 1.239 (1.927) |
| Affordability | | | | 16.400*** (4.275) |
| Pseudo $R^2$ | 0.327 | 0.404 | 0.523 | 0.628 |
| Observations | 125 | 125 | 125 | 125 |

The dependent variable is HPR. The standard errors are reported in parentheses and clustered at city levels. *, **, and *** refer to the statistically significant levels at 10%, 5% and 1%, respectively.

prices dropped immediately after the implementation of the HPR policy and rebounded four years later, coinciding with when most cities abolished HPR in late 2014. In addressing the potential biases arising from Heterogeneous Treatment Effect (HTE), we apply the HTE-robust estimators introduced by De Chaisemartin and D'Haultfoeuille (2020). Results in column (4) of Table 3 show that the coefficient is largely consistent with the previous estimations. Because the new estimator reduces the observation size from 626 to 173, we consider the results from column (3) as the baseline for future regressions, with a sample of 71 cities.

## Home purchase restriction and the marriage market

**Policy impact on marriage entry**. The implications of the HPR policy on family formation in China, where housing property holds significance in the marriage market, are complex. The household

eligibility rules specified in the HPR policy can have opposite effects on the patterns of family formation. Couples without a local hukou may delay their marriage plans due to their ineligibility to purchase a home until either has worked and paid social security taxes in the city for a certain number of years (e.g., five years in first-tier cities). Conversely, HPR may make housing more affordable, thereby inducing marriages among hukou holders. In this section, we examine how HRP affects marriage entries using Eq. (2), with the dependent variable being the natural logarithm of the annual number of marriages in each city on various occasions.

Table 4 presents the average effect of HPR on marriage entries. In column (1), the implementation of HPR is observed to reduce the number of marriages by 2.8% compared to cities without restrictions, but this result is not statistically significant. Moving on to column (2), we specifically examine the impact of HPR on first-time marriages, and the coefficient is −3.6%, though not statistically significant. We further analyze the total number of marriages, disaggregated by age group in columns (3)-(7). Column (3) reveals that the number of marriages among people aged between 20–24 declined by 9.9% in cities with HPR, which is statistically significant at the 5% level. In China, the earliest legal marriage age is 20 for females and 22 for males, and non-local individuals in this bracket may have to postpone their marriage plans for about 3–5 years if they need to own a house before getting married. These may well-explain the HPR-induced 9.9% reduction in marriages for people in their early twenties. For the other age groups, HPR appears to decrease the number of marriages, but these results are not statistically significant. The event study results for column (3) are shown in Fig. 2.

**Table 3 The impact of home purchase restrictions on house prices.**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| HPR | −0.058*** | −0.079*** | −0.091*** | −0.070*** |
| | (0.016) | (0.019) | (0.029) | (0.022) |
| City-fixed effect | Yes | Yes | Yes | Yes |
| Year-fixed effect | Yes | Yes | Yes | Yes |
| City linear trend | No | Yes | Yes | Yes |
| Observations | 1,113 | 1,113 | 626 | 173 |
| R-squared | 0.960 | 0.973 | 0.967 | N.A. |
| Number of cities | 125 | 125 | 71 | N.A. |

The dependent variable is lnH_Price. The standard errors are reported in parentheses and clustered at city levels. *, **, and *** refer to the statistically significant levels at 10%, 5% and 1%, respectively.

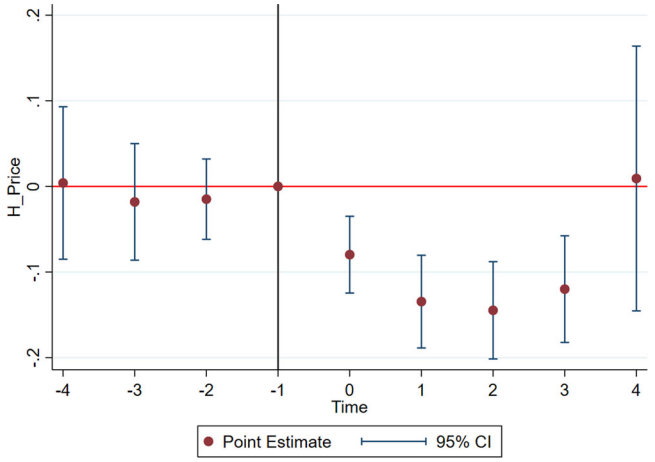


**Fig. 1 Event study for house prices.** The *x*-axis represents event time, with zero indicating the implementation year of the HPR policy, and the years before (−) or after (+) relative to time zero. The *y*-axis displays the coefficient of the housing price variable, showing the cumulative effects of the policy on house prices. The vertical bars around each coefficient represent the 95% confidence intervals. The figure illustrates a negative impact on house prices following the implementation of the HPR policy.

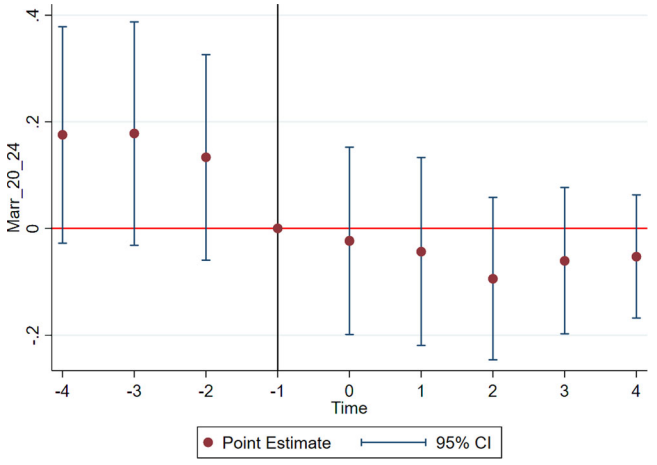


**Fig. 2 Event study for marriage entries among 20–24 year olds.** The *x*-axis represents event time, with zero indicating the implementation year of the HPR policy, and the years before (-) or after (+) relative to time zero. The *y*-axis displays the coefficient of the number of marriages among 20–24-year-old variable, showing the cumulative effects of the policy on marriage entries. The vertical bars around each coefficient represent the 95% confidence intervals. The figure illustrates a negative impact on marriage entries among 20–24 year olds following the implementation of the HPR policy.

**Table 4 Impact of Home Purchase Restrictions on Marriage Entry.**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| **Dependent variable** | **lnMarriage** | **lnM_first** | **lnM_20_24** | **lnM_25_29** | **lnM_30_34** | **lnM_35_39** | **lnM_over_40** |
| HPR | −0.028 (0.034) | −0.036 (0.034) | −0.099** (0.043) | −0.025 (0.043) | −0.005 (0.052) | −0.049 (0.057) | −0.070 (0.065) |
| City-fixed effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| City linear trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 708 | 708 | 708 | 708 | 708 | 708 | 708 |
| R-squared | 0.897 | 0.888 | 0.891 | 0.865 | 0.863 | 0.859 | 0.829 |
| Number of cities | 71 | 71 | 71 | 71 | 71 | 71 | 71 |

The standard errors are reported in parentheses and clustered at city levels. *, **, and *** refer to the statistically significant levels at 10%, 5% and 1%, respectively.

**Table 5 Impact of Home Purchase Restrictions on Marriage Entry by City Tier.**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent variable | lnMarriage | lnM_first | lnM_20_24 | lnM_25_29 | lnM_30_34 | lnM_35_39 | lnM_over_40 |
| HPR*Tier 1 | 0.018 (0.060) | −0.014 (0.062) | −0.103 (0.111) | −0.130** (0.058) | 0.184*** (0.063) | 0.145* (0.080) | −0.154 (0.120) |
| HPR*New Tier 1 | −0.007 (0.053) | −0.008 (0.049) | −0.073 (0.064) | 0.023 (0.066) | 0.070 (0.079) | −0.076 (0.064) | −0.096 (0.087) |
| HPR*Other Tiers | −0.046 (0.039) | −0.053 (0.041) | −0.110** (0.052) | −0.026 (0.051) | −0.076 (0.059) | −0.075 (0.063) | −0.042 (0.082) |
| City-fixed effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effect | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| City linear trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 708 | 708 | 708 | 708 | 708 | 708 | 708 |
| R-squared | 0.897 | 0.888 | 0.891 | 0.865 | 0.865 | 0.860 | 0.830 |
| Number of cities | 71 | 71 | 71 | 71 | 71 | 71 | 71 |

The standard errors are reported in parentheses and clustered at city levels. *, **, and *** refer to the statistically significant levels at 10%, 5% and 1%, respectively.

**Table 6 Impact of Home Purchase Restrictions on Strategic Divorce and by City Tier.**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Dependent variable** | **lnDivorce** | **lnRemarriage** | **lnRecon-ciliation** | **lnDivorce** | **lnRemarriage** | **lnRecon-ciliation** |
| HPR | −0.024 (0.041) | 0.069 (0.071) | −0.048 (0.169) | | | |
| HPR*Tier 1 | | | | −0.014 (0.093) | 0.107 (0.104) | 0.554*** (0.152) |
| HPR*New Tier 1 | | | | −0.023 (0.078) | 0.019 (0.114) | 0.394** (0.132) |
| HPR*Other Tiers | | | | −0.026 (0.041) | 0.083 (0.074) | −0.350 (0.245) |
| City-fixed effect | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effect | Yes | Yes | Yes | Yes | Yes | Yes |
| City linear trend | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 708 | 708 | 678 | 708 | 708 | 678 |
| R-squared | 0.929 | 0.825 | 0.644 | 0.929 | 0.825 | 0.651 |
| Number of cities | 71 | 71 | 71 | 71 | 71 | 71 |

The standard errors are reported in parentheses and clustered at city levels. *, **, and *** refer to the statistically significant levels at 10%, 5% and 1%, respectively.

The average effect of HPR on marriage entry may not be uniform across cities. To investigate this further, we analyze the data by city tier. Table 5 presents the results for first-tier, new first-tier, and other-tier (remaining 2nd and 3rd tier) cities. Interesting patterns are found for first-tier cities by age group. The HPR policy has caused a 13% reduction in the number of marriages for the 25–29 age bracket at a 5% significance level. For the next two age brackets (i.e., people in their 30s), the positive coefficients for the interaction terms present an interesting story: marriages increase by 18.4% for people aged 30–34 and 14.5% for those aged 35–39, suggesting that HPR may have shifted people getting married at older ages, from the 25–29 to the 30–39 age groups.

One plausible explanation is that people in first-tier cities typically get married at a later age due to unaffordable housing prices and the need to accumulate sufficient wealth to buy a property (Jones, 2017). Another explanation is that non-hukou holders in first-tier cities are not eligible for home purchase until they meet the five-year residency conditions, which typically happen to college graduates in their 30s. A third explanation is that people in their 30s may have a substantial investment motivation to buy second homes, which could have increased the number of marriages through strategic divorce and marriage reconciliation tactics. For new first-tier cities, no significant pattern is observed for the policy effect on marriage entry. For other-tier cities, all coefficients are negative, indicating that HPR has reduced the number of marriages in these cities. Specifically, the negative coefficient for the 20–24 age bracket at a 5% statistically significant level indicates that HPR cities have on average 11% fewer marriages compared to non-HPR cities.

**Policy impact on strategic divorce**. The previous section provides evidence suggesting that the HPR policy leads to delayed marriages in first-tier cities and reduced marriages in second and third tier cities. In this section, we analyze the impact of the policy on divorce, remarriage, and marital reconciliation (remarriage to the original spouse).

Columns (1) to (3) in Table 6 present the policy impact in general for all cities, failing to show significant impacts on the numbers of divorce, remarriage, or marriage reconciliation. But when cities are analyzed by each tier, some interesting patterns deserve attention. The coefficients for the number of divorces and remarriages (columns 4 & 5) for all city tiers are insignificant, suggesting that HPR has not contributed to divorce or remarriage in general. However, column (6) indicates that the number of marital reconciliations increased by 55.4% in first-tier cities and 39.4% in new first-tier cities, with 1% and 5% statistically significant levels, respectively. This unusually rise in reconciliation numbers caused by the HPR policy provides direct evidence of strategic divorce in these cities. Moreover, the degree of strategic divorce decreases as we move down the city tier. For other-tier cities, the result is not significant. We think that the high housing investment return and favorable down payment ratio may explain why strategic divorce is more prevalent in first-tier and new first-tier cities. Different from Alm et al. (2022)'s discovery of couples' divorce intention due to the HPR policy, our findings provide strong evidence that the policy has triggered strategic divorces in large cities in China.

## Conclusion

This study provides empirical evidence on the relationship between housing markets and family formation aligning with

Becker's marriage theory (1973, 1974). There are mixed results regarding the directional relationship between prices and marriages. Higher divorce rates are associated with a price shock in the UK (Rainer & Smith, 2010) and lower divorce risks with declining prices in the US (Farnham et al., 2011). In the Chinese context, high house prices are associated with declining marriage rates, delayed marriages (Wrenn et al., 2019), and higher divorce rates (Zheng et al., 2018). Although our paper does not directly measure the relationship between house prices and marital status, it implicitly suggests that housing policy can affect marriage decisions. We find that the HPR policy has caused a reduction of marriages among people in their twenties, and people in first tier cities tend to delay marriages to their thirties. We also find that couples have used the strategic divorce tactic to circumvent housing regulations to acquire a second home, implying that increasing family wealth (through acquiring a second home) is desirable for married couples. Our paper suggests that high house prices (or more wealth) are beneficial for marriage stability and that couples facing the potential benefit from acquiring a second property may strategically choose to divorce to expand family wealth. In this sense, our findings are more consistent with Rainer and Smith (2010) than with Farnham et al. (2011).

On the policy side, this study contributes to the housing regulation literature by investigating the effects of housing demand shocks on family formation. While the home purchase restriction policy effectively stabilizes the housing market and reduces house prices, it also places restrictions on home buyers. These restrictions have ripple effects on family formation decisions, impacting marriage rates among young people and the likelihood of divorce among couples without a local hukou. Policymakers should consider how individuals may respond to housing regulations like the HPR policy when designing such measures. The study suggests that the HPR policy has contributed to delayed marriages, potentially contributing to lower birth rates. Given the use of city-level data, we are unable to explore the underlying mechanisms behind each household's marriage decisions, which is a limitation of our research. To better understand these specific mechanisms, future research should obtain micro-level household survey data that includes information on individual household property ownership, family circumstances, and background.

## Data availability

The datasets generated and analysed during the current study are available from the China Statistics Database through the library E-Resources of the City University of Hong Kong, but restrictions apply to the availability of these data, which were used under license for the current study and so are not publicly available. The data are, however, available from the corresponding author on reasonable request.

Received: 22 January 2024; Accepted: 2 September 2024;
Published online: 17 September 2024

## Note

1 Hukou is a system of household registration used in China, which indicates a person's permanent residency in a city. Hukou has been utilized by policymakers to differentiate preferential treatments towards permanent residents. See Chan (2009) and Song (2014) for a comprehensive review of the hukou system.

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## Acknowledgements

This work was supported by the Beijing Normal University & Hong Kong Baptist University United International College Startup Grant [grant number UICR0700063-23]. The part of work by Mi Diao was supported by the Shanghai Municipal Natural Science Foundation [grant number 23ZR1465100] and Fundamental Research Funds for the Central Universities in China.

## Author contributions

Zheng Chang: Writing – original draft, Writing – review & editing, Methodology, Formal analysis, Data curation, Funding acquisition, Conceptualization. Weifeng Li: Writing – review & editing, Investigation, Conceptualization. Mi Diao: Writing – review & editing, Investigation, Conceptualization. Xin Li: Writing – original draft, Writing – review & editing, Methodology, Formal analysis, Conceptualization.

## Competing interests

The authors declare no competing interests.

## Ethical approval

This paper does not contain any studies with human participants performed by any of the authors.

## Informed consent

This article does not contain any studies with human participants performed by any of the authors.

## Additional information

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