Premarital Property Rights and Marriage Timing: Evidence from Urban China

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Abstract

In 2011, China's Supreme People's Court strengthened individual property rights over premarital housing, reducing the expected redistribution of wealth upon divorce. We exploit this legal shock to estimate the effect of property rights on marriage timing in the presence of high housing costs. Using 2010 and 2015 census microdata, we employ a difference-in-differences strategy combined with entropy balancing. We find a distinct gender asymmetry: the reform caused a significant decline in marriage entry for young women in high-housing-price cities, while men's outcomes remained largely unchanged. Specifically, women in high-price areas experienced a roughly 3 percentage point decrease in marriage rates relative to low-price cities. These findings suggest that strengthening premarital property rights reduces the marital surplus for non-owners, incentivizing marriage delay.

Keywords: Family formation, Marriage timing, Housing prices, Premarital property rights, Gender differences, Urban China

1. Introduction

Marriage in urban China is closely intertwined with housing. Driven by a combination of cultural norms and skewed sex ratios, housing has evolved into

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a critical status good in the marriage market, with grooms often expected to provide a marital home as a signal of financial capacity (Wei and Zhang, 2011; Du and Wei, 2013). Consequently, for many couples, securing housing is not only a major financial decision but also a key prerequisite for union formation, often tied to the access of local public services, especially education for future children(Chen and Feng, 2013). This dynamic shapes bargaining within relationships and the timing of household formation.

Against this backdrop, legal rules governing the ownership and division of premarital housing assets can have first-order implications for marriage behavior. Economic theory suggests that family laws affect the gains from marriage and the allocation of resources by altering the threat points in bargaining (Becker, 1993; Stevenson and Wolfers, 2007). Changes in property rights regimes, in particular, have been shown to shift incentives regarding labor supply, savings, and divorce (Voena, 2015). In the Chinese context, such rules are particularly salient in cities where housing is expensive and premarital housing wealth constitutes the central component of household balance sheets (Glaeser et al., 2017).

This paper studies how a major legal clarification in 2011 affected marriage outcomes in the presence of large cross-city differences in housing costs. In 2011, China's Supreme People's Court issued a judicial interpretation that clarified the treatment of premarital housing assets and strengthened the protection of individual ownership claims in divorce-related property division. In economic terms, the clarification reduced the extent to which premarital housing wealth is expected to be shared within marriage. Because the value of these assets is mechanically higher in high-price cities, the reform plausibly altered marriage incentives more strongly in places where housing costs are high.

Our empirical analysis links repeated cross-sections of census microdata to a pre-determined measure of local housing prices. We use the 2010 housing price distribution to define baseline exposure and hold it fixed throughout

the analysis. The core outcome is an indicator for whether an individual has ever been married. The baseline comparisons focus on urban residents and emphasize cohorts close to the legal marriage age at the time of the reform, where behavioral responses should be most visible in the short to medium run. To improve the comparability of high- and low-price cities, we use entropy balancing (Hainmueller, 2012) as a design tool to reweight control cities so that they resemble treated cities in key pre-reform characteristics, including baseline demographic composition and, where relevant, earlier marriage outcomes. We then implement two complementary empirical exercises: (i) a two-wave comparison between 2010 and 2015, complemented by a four-wave (2000/2005/2010/2015) event-study framework that transparently describes the timing of marriage responses around the reform; and (ii) specifications that treat housing prices as a continuous exposure and trace how the post-reform housing-price relationship varies over the age profile.

The results reveal a clear gender asymmetry. Across specifications, marriage outcomes deteriorate in high-price urban areas after 2011 for women at early marriageable ages, while men exhibit much weaker and less systematic responses. In the high-versus-low comparisons, young women in high-price cities experience a relative decline in marriage outcomes after the reform, whereas the corresponding pattern for men is muted. The entropy-balanced event-study profiles show that once observable differences are accounted for, high- and low-price cities display similar pre-reform dynamics, and the post-reform divergence is concentrated among women. When housing prices are treated continuously, the post-period relationship between housing prices and marriage is most negative at early ages for women and attenuates with age; the age profile for men is comparatively flat. We further document that the female pattern is weaker among more-educated women, consistent with education relaxing the housing-related financial constraints that link housing costs to marriage timing.

We interpret these findings by focusing on how legal rules governing

premarital assets shape marriage incentives when housing is both valuable and central to union formation. By strengthening individual ownership claims over premarital housing wealth, the 2011 clarification reduced the expected transferability of premarital housing resources within marriage. This change is amplified in high-price cities where the economic stakes of premarital housing are larger. A central implication is that the reform can delay marriage for cohorts at marriageable ages by tightening the conditions under which marriage is privately beneficial, with responses concentrated where housing costs are high and among groups for whom housing affordability constraints are most binding. Throughout, the empirical patterns are best understood as partial-equilibrium responses: we hold baseline local housing-market conditions fixed and trace short- to medium-run changes in marriage behavior, abstracting from longer-run general-equilibrium adjustments in housing supply, migration, or matching.

This paper contributes to research on family behavior, housing markets, and legal institutions in two ways. First, it provides evidence that legal rules governing premarital property rights can shift marriage outcomes in economically meaningful ways in settings where marriage is tightly linked to high-value assets such as housing, complementing work that emphasizes how housing-market conditions shape marriage-market behavior and sorting (Wei et al., 2017; Sun and Zhang, 2020). Second, it documents substantial heterogeneity by gender and age, highlighting that the interaction between legal institutions and housing-market conditions can operate differently for women and men and can be concentrated at early marriageable ages. This emphasis on heterogeneous responses relates to a broader literature showing that family law affects household behavior through bargaining positions and outside options (Gray and Jeffrey, 1998; Chiappori et al., 2002; Rangel, 2006; Stevenson and Wolfers, 2006; Voena, 2015). As a complementary check on external consistency, we also present auxiliary evidence from province-level administrative marriage registrations, which displays post-reform patterns aligned

with the micro-level findings. More broadly, our focus on the 2011 regime shift connects to a growing body of evidence on how China's divorce-related property-division rules shape marriage-related behavior and intrahousehold outcomes (Zang, 2020; Dong, 2022; Li and Sun, 2023; Zhang et al., 2024).

2. Institutional Background

This section explains why the 2011 Supreme People's Court (SPC) judicial interpretation constituted a salient shift in the expected distribution of housing wealth within marriage, and why its incidence is plausibly gendered in urban China. The key point is that the interpretation did not introduce an explicit gender distinction in statutory language, but it changed the expected divorce-time allocation of the single most important household asset—housing—in a setting where (i) housing is commonly treated as a prerequisite for marriage and (ii) formal ownership is disproportionately registered in men's names.

2.1. Trends in the Average Age at First Marriage in China

Figure 1 plots China's average age at first marriage from 2004 to 2019. The series is relatively stable during 2004–2010, fluctuating only slightly and remaining at a comparatively low level. In contrast, following the issuance of the 2011 Judicial Interpretation of the Marriage Law, the aggregate statistical average age at first marriage begins to rise sharply and increases almost year by year thereafter, indicating a pronounced upward shift in the timing of first marriage at the national level.

2.2. The 2011 Judicial Interpretation on Marriage Law

China's 1980 Marriage Law, as amended in 2001, provides a broad framework for marital property rights (National People's Congress of the People's Republic of China, 2001). It distinguishes between property acquired during marriage, which is generally treated as jointly possessed, and premarital property, which is generally treated as separate, while emphasizing that the

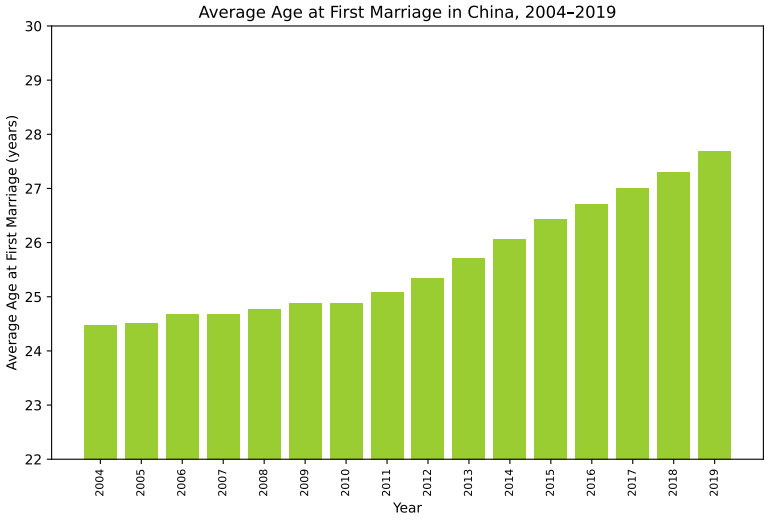


Figure 1: Average Age at First Marriage in China, 2004–2019
Notes: The figure reports the national average age at first marriage (years) by year.

division of marital property upon divorce should consider actual circumstances and protect the rights and interests of the wife and children. While these principles establish the general direction of the marital property regime, they were not designed to operationalize the division of the urban family home under modern market conditions, particularly when housing is purchased before marriage with mortgage repayment during marriage, or when parental contributions are involved and formal title registration is held by only one spouse. In practice, the combination of general joint-property language and pre-2011 judicial practice contributed to a widespread perception that marriage conferred a meaningful claim on housing wealth even when purchase and registration were asymmetric across spouses.

In August 2011, the Supreme People's Court (SPC) issued *Interpretation (III) of the Supreme People's Court on Several Issues Concerning the Application of the Marriage Law of the People's Republic of China* (Supreme People's Court of the People's Republic of China, 2011). This nationwide

judicial interpretation standardized court practice in marriage and divorce disputes and, in doing so, clarified the treatment of housing assets tied to premarital purchase, parental financing, and formal title registration. In the canonical case in which one spouse makes a premarital down payment, obtains a mortgage, and continues repayment during marriage with marital resources, with the property registered under that spouse's name, the interpretation allows courts to award the home to the registered owner upon divorce, treating the remaining loan as personal debt and limiting the non-registered spouse's claim to compensation for joint mortgage repayment and the corresponding appreciation. The interpretation similarly clarifies that housing purchased with funds from one side's parents and registered under that child's name is generally treated as that spouse's personal property, absent contrary agreement, and that when both sides' parents contribute but registration is held by one party, ownership may be allocated in proportion to contributions. In housing-reform transactions in which marital resources are used to purchase a unit registered under a spouse's parents' name, the non-registered spouse is typically limited to a creditor claim rather than a direct ownership share.

Although framed as a clarification intended to reduce adjudicative uncertainty, the economic content of the 2011 interpretation is that it places greater weight on formal registration and on the financing structure that determines whose name appears on the title. Relative to the pre-2011 regime, this shift reduces the expected share of housing wealth that a non-registered spouse can claim upon divorce, especially when housing is tied to premarital purchase or parental financing and registered under one spouse's name. Because the change operates through expectations about the division of housing wealth, it can affect marriage timing and formation even though it formally concerns divorce adjudication. These effects are likely to be strongest in high-price housing markets, where the value of the marital home is large, and among cohorts making marriage decisions around the time of the reform, for whom the interpretation altered the expected surplus from marriage at the relevant

margin.

2.3. Marriage Market Norms and Housing Assets

Urban marriage markets in China are characterized by strong housing-related norms. A widely documented feature is that the groom (and his family) is expected to provide housing as a precondition for marriage, while women's marriage prospects are less tightly linked to direct ownership of housing assets (Yu and Xie, 2015). In this setting, housing functions not only as a consumption good but also as a key marker of economic capacity in the marriage market.

These norms are reinforced by intergenerational transfers and ownership conventions. Parental resources are frequently mobilized to help sons purchase a marital home, and formal title registration commonly follows the financing side. As a result, men are substantially more likely than women to own housing property and, in particular, to hold sole title to the marital home (Wei et al., 2017).

Housing therefore occupies a first-order position in household wealth, especially in urban China. Rules governing the allocation of housing assets at divorce translate into large differences in expected economic security, and these differences scale directly with local housing prices. When housing values are high, even modest changes in expected claims to the marital home can materially affect marriage behavior at the margin.

Taken together, these institutional features imply that a legal rule strengthening the link between title, financing, and divorce-time ownership is unlikely to be neutral in incidence. Because housing ownership and titling are disproportionately concentrated on men, the 2011 judicial interpretation constitutes a plausibly gender-specific shock, with effects that should be most visible among women at early marriageable ages and in high-price cities.

3. Conceptual Framework

We provide a stylized framework to organize the empirical design. The goal is not to fully characterize equilibrium matching, but to clarify how the 2011 judicial clarification on premarital assets can shift marriage and fertility timing in a way that varies with local housing prices and age at the time of the policy.

Consider an urban marriage market in city c, where premarital housing wealth is scaled by a city-level housing price index $P_c > 0$. A potential husband is characterized by premarital housing endowment $k \geq 0$ and a match-quality component q. A woman of age a meets potential husbands over time and decides whether to enter marriage now or delay.

Let $\theta \in [0, 1]$ summarize how much of premarital housing wealth is expected to be effectively shared with the spouse in the event of divorce (through transfers or compensation). A lower θ corresponds to stronger protection of individual ownership of premarital assets. The 2011 judicial clarification is modeled as a reduction in θ, from θ_0 to $\theta_1 < \theta_0$, with the economic relevance amplified when P_c is higher.

The woman's net payoff from accepting a marriage offer at age a in city c is

$$U^f(k, q; c, a, \theta) = q + \theta P_c k - \kappa(a), \tag{1}$$

where $\kappa(a)$ is an age-dependent cost (or outside-option advantage) of remaining single at age a. We assume $\kappa(a)$ weakly decreases with age, capturing that delaying marriage becomes less attractive as age rises (for example because of social pressure or a tightening fertility window).

A woman accepts if $U^f \geq 0$, equivalently if

$$q \geq \kappa(a) - \theta P_c k. \tag{2}$$

The 2011 policy reduces θ, increasing the minimum match quality q required for acceptance by $(\theta_0 - \theta_1)P_c k$. This effect is mechanically larger in higher-P_c

cities and for matches involving men with larger premarital assets k[1].

Consequently, women must search longer to locate a partner whose non-pecuniary match quality q is sufficiently high to compensate for the reduction in secure housing wealth. This mechanism predicts that marriage entry at younger ages will decline, manifesting as delayed marriage rather than permanent singlehood, provided that the reservation threshold falls sufficiently with age as $\kappa(a)$ declines.

On the male side, the same reduction in θ increases the degree to which premarital housing is protected, raising the net value of marriage for men with higher k. This can increase men's willingness to marry, but realized marriages may still decline if the female acceptance margin becomes more restrictive.

4. Data

This study combines individual-level microdata from multiple waves of China's population census and related large-scale census samples. The core individual-level analysis relies on repeated cross-sections from the 2010 Population Census and the 2015 1% Population Sampling Survey, which provide harmonized information on demographic characteristics (e.g., age and gender), hukou-based residence status (urban versus rural), and marital status. These two waves form the basis of the main difference-in-differences analysis and all individual-level outcome regressions.

To support the dynamic analysis and the construction of entropy-balancing weights, we additionally draw on earlier census waves from 2000 and 2005. These earlier waves are drawn from nationally representative census or mini-

[1]A higher acceptance threshold reduces the set of matches that are acceptable, consistent with empirical evidence that search frictions play an important role in marriage markets. For example, Mansour and McKinnish (2017) document patterns of assortative matching that are difficult to reconcile with frictionless search and instead point to the relevance of search costs.

census samples collected using comparable enumeration procedures. Because detailed individual-level covariates are not fully harmonized across all four waves, the 2000 and 2005 data are not used directly in the main individual-level regressions. Instead, they are aggregated to the city level and used for two specific purposes: (i) to summarize pre-reform marriage outcomes when constructing entropy-balancing weights for the baseline DID and four-wave event-study analyses, and (ii) to describe the evolution of marriage outcomes at the city level in the event-study framework.

Across all waves, we harmonize the definition of the key outcome variable—an indicator for whether an individual has ever been married—using consistent coding of census marital-status categories. Age is defined using birth-year information so that it is comparable across waves. City identifiers are standardized to ensure consistent linkage over time.

Each individual in the 2010 and 2015 samples is linked to a city-level housing price measure, defined at the prefecture-level city, based on their city of residence. Housing prices are measured in 2010 and held fixed throughout the analysis. This housing-price measure serves two roles. First, it defines exposure groups (high- versus low-price cities) used in the baseline DID and event-study analyses. Second, it is used as a continuous measure of local housing-market conditions in the gradient and age-heterogeneity specifications.

In sum, the individual-level outcome analysis is conducted using the 2010 and 2015 microdata, while the 2000 and 2005 waves enter only through city-level aggregates that inform the weighting procedure and the dynamic description of pre- and post-reform patterns.

Table 1: Descriptive Statistics by Census Wave

	2010	2015	Diff. (15–10)
Panel A: Women			
Number of cities	303	303	0
Age	32.15 (7.76)	32.68 (7.52)	0.53
Ever married	0.79 (0.41)	0.80 (0.40)	0.02
Rural indicator	0.72 (0.45)	0.58 (0.49)	-0.15
Urban indicator	0.28 (0.45)	0.42 (0.49)	0.15
Panel B: Men			
Number of cities	303	303	0
Age	32.36 (7.77)	32.76 (7.54)	0.40
Ever married	0.70 (0.46)	0.71 (0.45)	0.01
Rural indicator	0.73 (0.44)	0.64 (0.48)	-0.09
Urban indicator	0.27 (0.44)	0.36 (0.48)	0.09

Notes: The table reports unweighted descriptive statistics for the pooled census micro-samples used in the analysis. "Ever married" is an indicator for having ever been married. Rural/urban are hukou indicators. The sample includes individuals in the analysis-age window and cities included in the merged file.

5. Empirical Strategy

This section describes the empirical strategy used to study how the 2011 judicial clarification affected marriage outcomes in the presence of large cross-city differences in housing prices. The analysis combines a baseline difference-in-differences (DID) design with entropy balancing to improve comparability across cities, a four-wave event-study specification to trace the timing of responses, and complementary specifications that examine how post-reform effects vary with housing prices and across ages. Throughout, we focus on urban non-migrants close to the legal marriage age and interpret the estimates as short- to medium-run, partial-equilibrium responses to the reform, conditional on local housing-market conditions.

5.1. Baseline DID: high- vs. low-price cities

Our baseline design compares changes in marriage outcomes between 2010 (pre) and 2015 (post) across cities with different exposure to housing-market conditions. Let i index individuals, c prefecture-level cities, and t census waves. The outcome Married_{ict} is an indicator for whether individual i in city c and wave t has ever been married.

Exposure is defined using a pre-determined housing-price measure. Specifically, HighPrice_c equals one if city c's housing price in 2010 is above the national median, and zero otherwise. The post-reform period is captured by Post_t, which equals one for 2015 and zero for 2010. The baseline two-period DID specification is:

$$\text{Married}_{ict} = \beta_1 \,\text{Post}_t + \beta_2 \,\text{HighPrice}_c + \beta_3 \,(\text{HighPrice}_c \times \text{Post}_t) + \mathbf{X}'_{ict}\gamma + \alpha_c + \varepsilon_{ict}, \tag{3}$$

where $\mathbf{X}_{ict}$ includes individual controls for age, education, and ethnicity (Han and a missing indicator), and α_c are city fixed effects. Because only two waves are used, aggregate time effects are absorbed by Post_t. Standard errors are clustered at the city level.

The coefficient of interest, β_3, measures the differential change in marriage outcomes from 2010 to 2015 in high-price cities relative to low-price cities. Since city fixed effects absorb all time-invariant cross-city differences, including the level of housing prices, identification comes from within-city changes over time interacted with baseline housing-price exposure.

The baseline analysis focuses on urban non-migrants within narrow age windows around the legal marriage age—women aged 20–24 and men aged 22–26. Restricting attention to these cohorts sharpens interpretation by focusing on individuals for whom marriage-timing responses should be most visible in the years immediately surrounding the reform.

Interpreting β_3 as a causal effect requires a conditional parallel-trends assumption: absent the reform, marriage outcomes in high- and low-price cities would have evolved similarly between 2010 and 2015, conditional on observed covariates, city fixed effects, and the reweighting procedure described below.

5.2. Entropy balancing as a design choice

High- and low-price cities differ substantially in baseline demographics and marriage levels, raising concerns about comparability. To address this issue, we use entropy balancing (EB) as a preprocessing step to reweight control cities so that their observed characteristics match those of treated cities prior to the reform. Entropy balancing constructs weights that exactly satisfy a set of pre-specified balance conditions while remaining as close as possible to uniform weights in an entropy sense (Hainmueller, 2012).

In our preferred implementation for the baseline DID and four-wave event-study analyses, EB targets two sets of pre-reform moments: baseline demographic composition measured in 2010 (including age, education, and ethnicity shares) and marriage outcomes summarized from earlier census waves (2000 and 2005), in addition to the 2010 baseline. This multi-period balancing aligns treated and control cities not only in levels but also in their pre-reform outcome trajectories. Conceptually, this approach is closely related

to the synthetic control logic of selecting comparison weights to achieve a close pre-treatment fit (Abadie et al., 2010, 2015).

Importantly, entropy balancing is used here as a design tool rather than as a source of identification. A close pre-treatment fit is a choice that improves transparency and reduces sensitivity to baseline differences; it does not, by itself, validate the identifying assumption. Following the synthetic control literature, we therefore view pre-reform alignment as part of the empirical design rather than as a statistical test of parallel trends.

5.3. Dynamic effects: entropy-balanced event study

To complement the two-period DID estimate and to describe how marriage outcomes evolve around the reform, we estimate an event-study specification using four census waves (2000, 2005, 2010, and 2015). This specification can be interpreted as a dynamic decomposition of the same high-vs.-low comparison underlying the baseline DID. The event-study regression is:

$$\text{Married}_{ict} = \sum_{\tau \in \{2000,2005,2015\}} \beta_\tau \left(\mathbb{1}\{t = \tau\} \times \text{HighPrice}_c\right) + \mathbf{X}'_{ict}\gamma + \alpha_c + \delta_t + \varepsilon_{ict}, \tag{4}$$

where 2010 is the omitted reference wave and δ_t are wave fixed effects.

The coefficient β_{2015} captures the differential change in marriage outcomes by 2015 relative to 2010 and corresponds directly to the baseline DID estimand: β_{2015} in (4) is comparable to β_3 in (3). The coefficients for 2000 and 2005 summarize residual differences between high- and low-price cities in earlier waves relative to 2010. When EB weights are constructed to align pre-reform outcomes across multiple waves, these pre-period coefficients are mechanically attenuated by design. Accordingly, the event-study is used primarily to illustrate the timing and magnitude of post-reform divergence in a transparent way, rather than to interpret pre-period coefficients as formal tests of the identifying assumption.

The event-study sample and specification mirror the baseline DID: urban

non-migrants in the same narrow age windows, with identical controls, city fixed effects, and city-level clustering. This alignment ensures that the dynamic profile can be interpreted as a time decomposition of the same underlying estimand.

5.4. Continuous exposure and age heterogeneity

The binary high-vs.-low comparison provides a transparent summary of exposure to costly housing markets, but it does not capture the full variation in housing prices across cities. We therefore estimate complementary specifications that treat housing prices as a continuous measure and examine how post-reform effects vary across ages. These analyses serve two purposes. First, they assess whether the results reflect a broader housing-price gradient rather than a threshold at the median. Second, they exploit within-city comparisons across age groups to characterize how responses are distributed over the life cycle.

All specifications in this subsection apply entropy-balancing weights constructed using *only* baseline (2010) information. In contrast to the baseline DID and four-wave event-study analyses, which use multiple pre-reform waves to align levels and trends across cities, the gradient and age-profile specifications intentionally restrict the weighting stage to the 2010 cross-section. This design choice reflects the different identifying content of the analysis. Here, the goal is to trace how the *post-reform* relationship between housing prices and marriage outcomes varies across ages within the same city. Using baseline-only weights avoids mechanically constraining age profiles based on earlier outcome dynamics and allows the shape of the post-period age gradient to be driven by the data rather than by imposed balance on pre-reform life-cycle trajectories.

We first estimate a two-period DID-in-gradients model:

$$\begin{aligned}\text{Married}_{ict} = {} & \theta_1 \,\text{Post}_t + \theta_2 \, P_c + \theta_3 \,(P_c \times \text{Post}_t) + \theta_4 \,\text{Young}_i \\ & + \theta_5 \,(P_c \times \text{Young}_i) + \theta_6 \,(\text{Post}_t \times \text{Young}_i) + \theta_7 \,(P_c \times \text{Post}_t \times \text{Young}_i) \\ & + \alpha_c + \delta_{a_i} + \varepsilon_{ict}.\end{aligned} \tag{5}$$

where P_c is the city-level housing price measured in 2010, δ_{a_i} are age fixed effects, and Young_i indicates a gender-specific young group defined in the Results section. The coefficient θ_7 captures whether the post-period change in the housing-price gradient differs for the young group relative to older ages.

To characterize heterogeneity across the full age profile, we also estimate an age-by-gradient specification that allows the post-period housing-price gradient to vary flexibly by age:

$$\text{Married}_{ict} = \sum_{a \in \mathcal{A},\, a \neq a_0} \kappa_a \Big(P_c \times \text{Post}_t \times \mathbb{1}\{a_i = a\} \Big) + \alpha_c + \delta_{a_i} + \varepsilon_{ict}, \tag{6}$$

where $\mathcal{A}$ denotes the analysis age range and a_0 is an omitted reference age.

These specifications rely on within-city comparisons across age groups, which difference out city-level shocks that affect all ages similarly. Interpreting the age-specific coefficients as causal differential effects requires that, absent the reform, the housing-price gradient in marriage outcomes would have evolved similarly across ages, or that any age-specific deviations are not systematically correlated with baseline housing prices. This assumption is distinct from, and in some contexts weaker than, the across-city parallel-trends assumption underlying the binary high-vs.-low DID.

6. Results

This section presents four sets of findings. We begin with the main two-wave comparison between 2010 and 2015 for urban non-migrants close to the legal marriage age. We then document how the pattern evolves over time

using the four-wave entropy-balanced event study. Next, we show that the findings are not driven by the median split by examining the housing-price gradient and its age profile. Finally, we provide complementary province-level evidence using administrative marriage registrations.

6.1. Main results in urban non-migrants

Table 2: Main DID Estimates

	Female	Male
Post (2015)	0.045***	0.002
	(0.013)	(0.026)
High price city × Post (2015)	−0.031**	0.017
	(0.015)	(0.027)
City fixed effects	Yes	Yes
Age fixed effects	Yes	Yes
Weights	EB trimmed	EB trimmed
Clustered standard errors	City	City
Age range	20–24	22–26
Observations	19,310	17,572

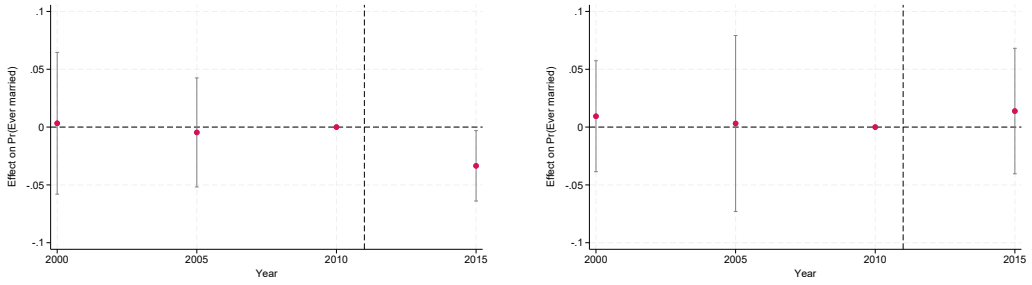
Notes. The dependent variable is an indicator for having ever been married. The table reports canonical two-period difference-in-differences estimates using the 2010 and 2015 waves. All specifications include city fixed effects, which absorb the main effect of the high-price indicator. The coefficient of interest is the interaction between the high-price indicator (defined using 2010 housing prices) and a post-reform indicator for 2015. Controls include age dummies, education categories, and ethnicity indicators. Urban samples are reweighted using entropy balancing to improve comparability between high- and low-price cities based on pre-policy characteristics and marriage outcomes. Standard errors are clustered at the city level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 2 summarizes the main results for urban non-migrants in narrow age windows around the legal marriage age (women aged 20–24 and men aged 22–26). The comparison is between 2010 and 2015, and it asks whether the post-reform change in marriage outcomes differs systematically between cities with high versus low baseline housing prices.

Two patterns stand out. First, marriage becomes more common between 2010 and 2015 for young women on average, but this increase is substantially smaller in high-price cities than in low-price cities. The gap implies a decline of roughly three percentage points in the likelihood of ever being married for women in high-price cities relative to their counterparts in low-price cities over the same period. Second, for men, the corresponding difference across high- and low-price cities is small and does not display the same clear post-reform divergence as for women.

These results point to a pronounced gender asymmetry: the post-reform adjustment is concentrated among young women in high-price urban areas, while men's marriage outcomes exhibit a much weaker response. The remainder of the section shows how this pattern appears in the time profile around the reform and in the way the housing-price relationship varies across ages.

6.2. Dynamic responses around the reform: entropy-balanced event study



(a) Female (b) Male

Figure 2: Entropy-balanced event study of marriage outcomes among urban non-migrants

Notes. The figure plots dynamic estimates of the interaction between the 2011 Marriage Law and high local housing prices, relative to 2010. Control cities are reweighted using entropy balancing to improve comparability with treated cities based on baseline characteristics and pre-policy marriage outcomes. Panel (a) reports results for women aged 20–24, and Panel (b) reports results for men aged 22–26, focusing on cohorts just above the legal marriage age. The vertical dashed line marks the 2011 reform.

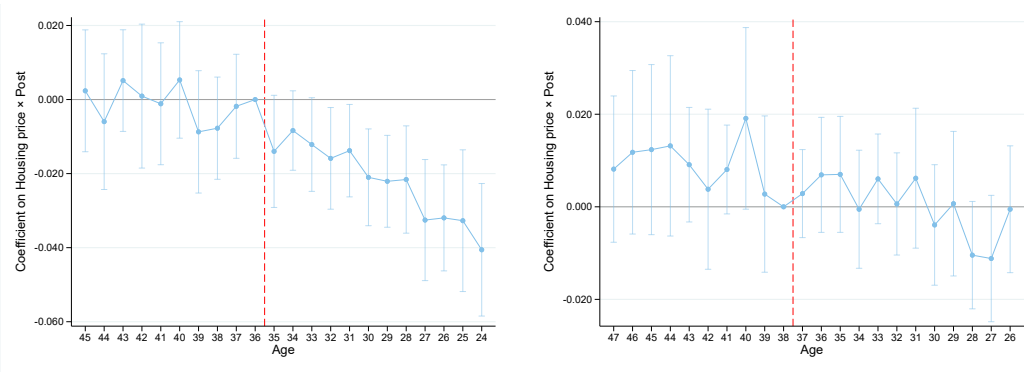
Figure 2 presents entropy-balanced event-study estimates that trace the evolution of marriage outcomes in high- and low-price cities across the four

census waves. The event-study is constructed to be fully aligned with the baseline DID analysis: it uses the same sample restrictions, controls, and city-level clustering, and the coefficient for 2015 corresponds directly to the two-period DID estimand reported in Table 2.

A defining feature of this figure is that control cities are reweighted using entropy balancing to match treated cities on baseline demographic composition and pre-reform marriage outcomes. As discussed in Section 5.2, this reweighting is a design choice intended to improve comparability across cities prior to the reform, rather than a statistical test of the identifying assumption. Consequently, the pre-reform coefficients for 2000 and 2005 are expected to be attenuated by construction and should be interpreted as reflecting the imposed balance rather than as independent evidence of parallel trends.

The primary informational content of the event study lies in the post-reform dynamics. By 2015, marriage outcomes diverge between high- and low-price cities in a sharply gendered manner. Panel (a) shows that, for women aged 20–24, marriage outcomes decline in high-price cities relative to low-price cities following the reform. Panel (b) shows that the corresponding movements for men aged 22–26 are substantially smaller. This pattern mirrors the baseline DID results and illustrates how the post-reform adjustment unfolds over time.

Taken together, the event-study estimates provide a transparent dynamic decomposition of the main treatment effect. They show that the post-reform divergence emerges after 2011 and is concentrated among young women in high-cost housing markets. Consistent with the empirical strategy, these patterns should be interpreted as short- to medium-run, partial-equilibrium responses to the reform, conditional on baseline housing-market conditions. The event study does not incorporate broader general-equilibrium adjustments—such as housing supply responses, migration, or longer-run changes in matching—but instead clarifies the timing and gender-specific nature of the behavioral response identified in the baseline DID analysis.



(a) Urban women

(b) Urban men

Figure 3: Event-study estimates of the post-period housing-price gradient by age.
Notes: Each panel plots age-specific coefficients on the interaction between housing prices and the post period, shown relative to the omitted reference age (36 for women; 38 for men). Points are estimates and whiskers are 95% confidence intervals. The vertical dashed line marks the reference age. All specifications include city and age fixed effects; standard errors are clustered at the city level and entropy-balancing weights are applied.

6.3. Beyond the median split: housing-price gradients and age profiles

The high-versus-low comparison provides a transparent summary of exposure to costly housing markets, but it is natural to ask whether the results reflect a broader relationship with housing prices rather than a threshold at the median. We address this by examining how the association between housing prices and marriage outcomes changes from 2010 to 2015 when housing prices are treated as a continuous measure, and by mapping how this change varies across ages.

Table 3 reports a compact summary that focuses on a younger age group and asks whether the post-2015 relationship between housing prices and marriage differs at younger ages relative to older ages. The estimates again show a clear gender contrast: the post-period change in the housing-price relationship is more negative for women in the younger age range than for older women, while the corresponding pattern for men is noticeably weaker.

Figure 3 provides a more detailed view by plotting the age profile of the post-period housing-price relationship. For women, the relationship is

Table 3: Housing Prices and Marriage: Urban Women and Men

	Women	Men
Post period	0.179***	0.262***
	(0.025)	(0.030)
Post period × Housing price	0.019***	0.012***
	(0.004)	(0.004)
Young-group × Housing price	0.015***	0.008**
	(0.005)	(0.004)
Post period × Young-group	0.075***	-0.027
	(0.029)	(0.026)
Post period × Young-group × Housing price	-0.021***	-0.009**
	(0.005)	(0.004)
Constant	0.573***	0.554***
	(0.012)	(0.009)
City fixed effects	Yes	Yes
Age fixed effects	Yes	Yes
Weights	EB trimmed	EB trimmed
Clustered standard errors	City	City
Observations	86,564	77,512

Notes: The dependent variable is an indicator for having ever been married. Housing price is measured at the city level in thousand RMB. The young-group indicator is defined differently by gender: ages 24–35 for women and ages 26–37 for men. All specifications include city and age fixed effects and use entropy-balancing weights. Robust standard errors clustered at the city level are reported in parentheses. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

most negative at early ages and then attenuates steadily, becoming small at older ages. For men, the profile is comparatively flat and does not show a concentrated decline at early ages. This age pattern complements the narrow-window main results: the post-reform adjustment is strongest precisely when the marriage margin is most active for women, and it fades as age increases.

Overall, the gradient and age-profile evidence reinforces the main message from Table 2 and Figure 2: the post-reform change in marriage behavior is closely tied to exposure to costly housing markets, and it is disproportionately driven by women at early marriageable ages.

6.4. Province-level dynamic consistency

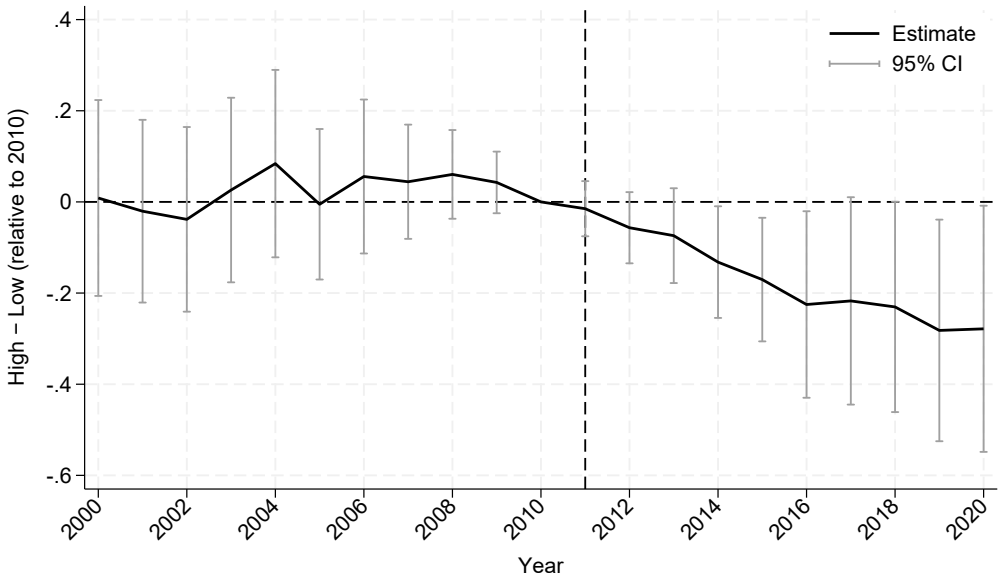


Figure 4: Province-level event study of marriage registrations

Notes. The figure plots differences in log marriage registrations between provinces with high versus low average city-level housing prices, relative to 2010. Treatment status is assigned based on average city-level housing prices in 2010 and held fixed over time. The vertical dashed line marks the 2011 reform. This figure provides macro-level dynamic consistency evidence and is not intended as a test of the micro-level parallel trends assumption.

Figure 4 provides complementary macro-level evidence using administrative data on province-level marriage registrations from 2000 to 2020. Provinces are classified by whether their average city-level housing prices in 2010 are above or below the national median, and this classification is held fixed over time. The figure plots differences in log marriage registrations between high- and low-price provinces relative to 2010.

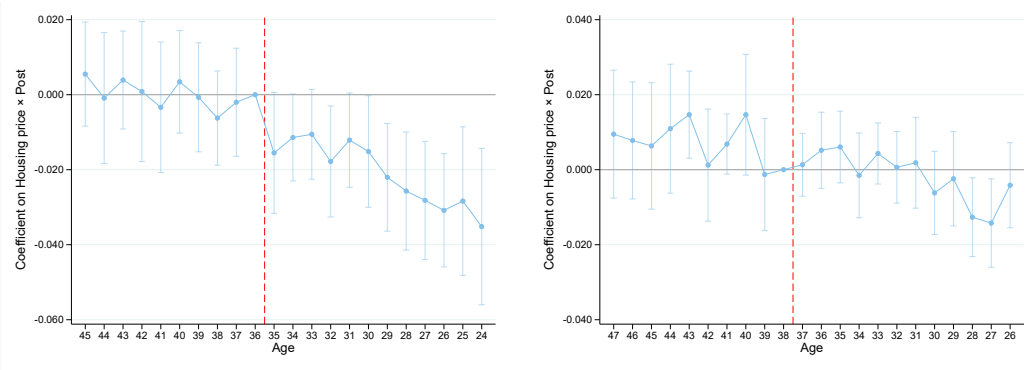
This province-level pattern is not meant to serve as a definitive benchmark for the individual-level design. Instead, it offers a useful consistency check: at an aggregate level, high- and low-price regions do not display sharply diverging dynamics prior to the reform, while a relative decline in marriage registrations appears after 2011 in higher-price areas. The direction of this macro pattern aligns with the micro evidence, where the post-reform adjustment is concentrated among young women in high-cost urban housing markets.

7. Robustness Checks

7.1. Accounting for housing-price growth

A potential concern with the baseline event-study-style design is that the estimated post-period housing-price gradients by age might partly reflect contemporaneous housing-market dynamics rather than differences in the pre-period price level. In particular, marriage timing could respond more strongly to *changes* in local housing affordability—e.g., rapid price appreciation tightening down-payment constraints—than to the level of housing prices per se. To address this, we augment the baseline specification by adding a parallel set of interactions constructed from a city-level housing-price growth measure (log price growth between the two waves).

Relative to the baseline event-study that focuses on the post-period shift in the housing-price *level* gradient across ages, this robustness analysis introduces an additional channel: it allows the post-period marriage response to vary with housing-price *growth*, and lets this growth-based response differ between



(a) Urban women

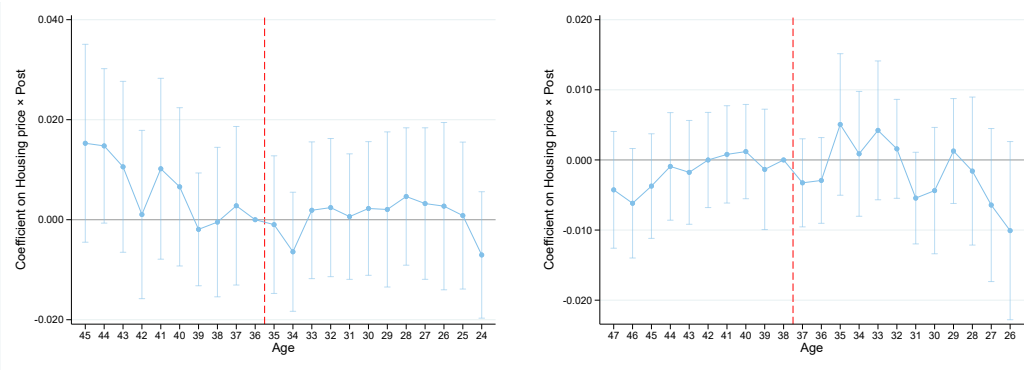
(b) Urban men

Figure 5: Event-study estimates controlling for local housing-price growth.
Notes: Each panel plots the age-specific coefficients for the post-period housing-price *level* gradient after additionally controlling for an analogous set of post-by-age interactions constructed from city-level housing-price growth. Points show coefficient estimates and whiskers indicate 95% confidence intervals. The vertical dashed line marks the reference age (36 for women; 38 for men). All specifications include city and age fixed effects, apply entropy-balancing weights, and cluster standard errors at the city level.

the younger-age group (ages 24–35) and the rest of the sample. Concretely, the regression includes both (i) the full post-by-young-group interaction with the housing-price level and (ii) an analogous post-by-young-group interaction with housing-price growth. The coefficient on the level-based triple interaction is therefore interpreted as the post-period change in the price-level gradient for the younger-age group *net of* any differential effects associated with local price appreciation.

7.2. Placebo test using the rural sample

As a placebo exercise, we replicate the baseline analysis on the rural group. The motivation is that housing and marriage markets operate very differently in rural areas: housing is less likely to be acquired through market transactions at prevailing urban prices, and access to housing is more often tied to family resources, homestead arrangements, or locally provided housing rather than to formal mortgage and down-payment constraints. Consequently, cross-city variation in urban housing prices is much less relevant for rural



(a) Rural women

(b) Rural men

Figure 6: Placebo event-study estimates in the rural sample.

Notes: Each panel plots age-specific post-period housing-price gradient estimates relative to the omitted reference age (marked by a vertical dashed line). Points show coefficient estimates and whiskers indicate 95% confidence intervals. All specifications include city and age fixed effects, apply entropy-balancing weights, and cluster standard errors at the city level. Because housing institutions and marketization differ sharply between rural and urban areas, any analogous pattern in the rural sample is expected to be substantially weaker.

women's marriage timing and partner matching.

Consistent with this institutional contrast, the rural estimates show substantially weaker post-period housing-price gradients than those observed in the urban sample, and the age-specific pattern highlighted in the baseline analysis does not appear in the rural group. This placebo result strengthens the interpretation that the urban findings are not driven by spurious time trends correlated with housing prices, but instead reflect mechanisms that are specific to urban housing-market conditions and financing constraints.

7.3. Permutation Test: Randomly Re-assigning City Housing Prices

To assess whether the baseline triple-interaction estimate could arise mechanically from coincidental correlation between city-level housing prices and changes in marriage outcomes across waves, we conduct a randomization (permutation) inference exercise. The test holds the individual-level data, the city and age fixed effects, and the entropy-balancing weights fixed, but repeatedly

breaks the empirical link between each city and its baseline housing price by randomly re-assigning housing prices across cities. For each permutation, we re-estimate the baseline two-period difference-in-gradients specification and record the coefficient on the triple interaction that captures the differential post-period housing-price gradient for the younger age group.

This procedure generates an empirical reference distribution for the triple-interaction coefficient under a sharp null in which housing prices are unrelated to post-period changes in marriage patterns once the sample composition, fixed effects, and weights are held constant. We compute a two-sided permutation p-value as the fraction of permutations for which the absolute permuted coefficient is at least as large as the absolute true estimate. In the urban female sample, the permutation p-value equals 0.0020, implying that only 0.2% of the 1,000 random re-assignments produce an estimate as extreme as the one observed in the data. This provides strong evidence that the baseline estimate is unlikely to be driven by chance alignment between city housing prices and time variation in marriage outcomes.

7.4. Excluding cities with home-purchase restrictions

A potential concern is that cities adopting home-purchase restrictions ("purchase-limit" policies) experienced policy-driven disruptions in housing transactions and access that could mechanically alter the relationship between local housing prices and marriage outcomes. To address this, we re-estimate the baseline two-period difference-in-gradients specification after excluding all purchase-limit cities from the analysis sample. The specification, weighting scheme, and inference remain the same as in the baseline: we include city and age fixed effects, apply entropy-balancing weights, and cluster standard errors at the city level.

Table 4 shows that the key post-period change in the housing-price gradient for the younger-age window remains negative and precisely estimated for both women and men. The magnitude is larger for women, consistent with the main finding that post-period housing affordability constraints are more

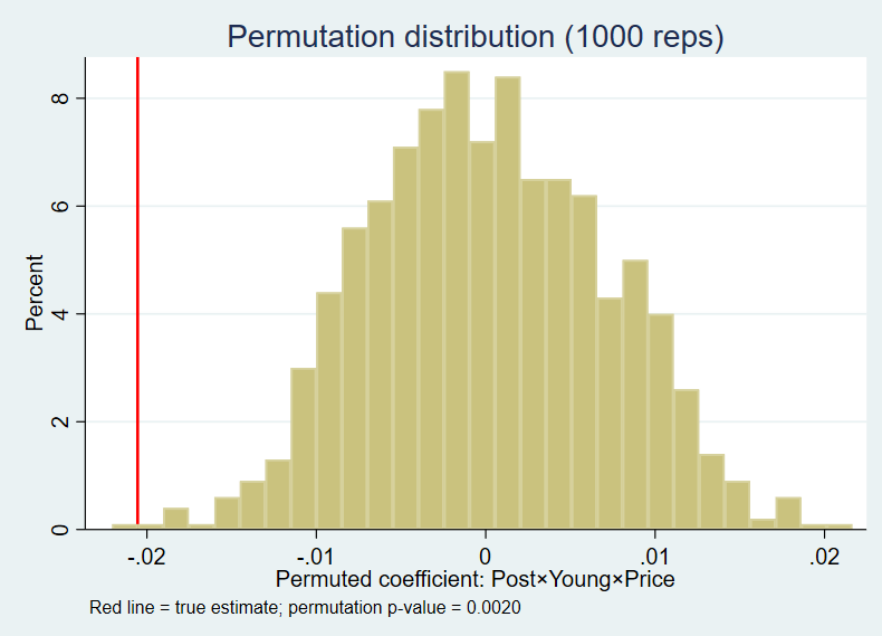


Figure 7: Permutation distribution of the triple-interaction estimate (urban women). *Notes:* The figure plots the distribution of the triple-interaction coefficient obtained from 1,000 permutations in which baseline housing prices are randomly re-assigned across cities, while the individual sample, entropy-balancing weights, and fixed effects are held fixed. The vertical line marks the true estimate from the observed data. The two-sided permutation p-value is computed as the share of permutations with an absolute coefficient at least as large as the absolute true estimate (here, $p = 0.0020$).

salient for women at early ages. Overall, dropping purchase-limit cities does not overturn the central gender contrast: the post-period housing-price gradient for early ages is substantially more negative for women, while the corresponding pattern for men is smaller in magnitude.

Table 4: Robustness: Excluding Purchase-Limit Cities (Urban Women and Men)

	Women	Men
Post period	0.169***	0.208***
	(0.026)	(0.024)
Post period × Housing price	0.018***	0.017***
	(0.004)	(0.004)
Young-group × Housing price	0.014***	0.012***
	(0.004)	(0.003)
Post period × Young-group	0.090***	-0.012
	(0.028)	(0.019)
Post period × Young-group × Housing price	-0.020***	-0.011***
	(0.004)	(0.003)
Constant	0.590***	0.572***
	(0.010)	(0.009)
City fixed effects	Yes	Yes
Age fixed effects	Yes	Yes
Weights	EB trimmed	EB trimmed
Clustered standard errors	City	City
Observations	73,065	65,584

Notes: The dependent variable is an indicator for having ever been married. Housing price is measured at the city level in thousand RMB. The young-group indicator is defined by gender (women: ages 24–35; men: ages 26–37). All specifications exclude cities that implemented home-purchase restrictions, include city and age fixed effects, apply entropy-balancing weights, and cluster standard errors at the city level (in parentheses). Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

8. Heterogeneity analysis

8.1. Heterogeneity by Educational Attainment

This section examines whether the post-period housing-price gradient for early-age marriage differs by educational attainment among urban women. We extend the baseline two-period difference-in-gradients framework by allowing the key post-period gradient for the younger age group to vary smoothly with years of schooling. Intuitively, this specification asks whether the marriage-delay pattern in expensive housing markets is concentrated among less-educated women (who may face tighter affordability constraints) or whether it is similarly strong among more-educated women.

The estimates show a clear education gradient. The post-period housing-price effect for the younger age group is negative on average, but it becomes significantly less negative as education increases. In particular, the coefficient on the four-way interaction (post period $\times$ younger-age group $\times$ housing price $\times$ education) is positive and statistically significant, implying that additional schooling attenuates the negative post-period housing-price gradient at younger ages. Quantitatively, an additional year of schooling reduces the magnitude of the negative post-period gradient by about 0.7 percent points (per thousand RMB of baseline housing price). This pattern is consistent with a mechanism in which education raises earnings potential and relaxes housing-related financial constraints (down payment, credit access, and parental transfers), thereby weakening the extent to which high housing costs delay marriage at young ages.

8.2. Heterogeneity by Bride-price norms

Table 6 examines whether the post-period housing-price gradient in marriage entry for younger adults varies across regions with different customary bride-price norms, using a coarse province-level classification constructed from news and newspaper reports. The estimated housing-price gradient for younger relative to older adults is economically meaningful and statistically

Table 5: Education Heterogeneity in the Post-Period Housing-Price Gradient (Urban)

	Women	Men
Post period × younger group	0.069***	0.019
	(0.020)	(0.034)
Post period × younger group × housing price	-0.011***	-0.002
	(0.003)	(0.005)
Post period × younger group × housing price × educ	0.007***	0.005
	(0.002)	(0.003)
City fixed effects	Yes	Yes
Age fixed effects	Yes	Yes
Weights	EB trimmed	EB trimmed
Clustered standard errors	City	City
Observations	86,564	77,512

Notes: The dependent variable is an indicator for having ever been married. Housing price is measured at the city level (thousand RMB). Education is years of schooling (continuous). The main parameter of interest is the four-way interaction between the post period, the younger-age indicator, baseline housing prices, and education. All specifications include city and age fixed effects, apply entropy-balancing weights, and cluster standard errors at the city level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

significant for urban women in provinces classified as having mid bride-price norms, while the corresponding gradients in low- and high-tier provinces are smaller in magnitude. Formal tests indicate that the gradient in mid-tier provinces differs from that in low-tier provinces for women, whereas the high–low difference is not statistically significant. For urban men, the implied tier-specific gradients are negative in all three tiers, but tests of differential effects across tiers are not statistically significant overall, consistent with limited evidence of tier-based heterogeneity for men.

These patterns are plausible in the Chinese marriage market. First, marriage-entry negotiations and household formation costs are often more salient for women, in part because housing is frequently framed as a prerequisite for marriage and because expected asset security within marriage affects the perceived value of entering a union. Second, bride-price customs reflect broader local norms about marriage-related transfers and bargaining arrangements, but the mapping from "higher bride price" to greater sensitivity to housing prices need not be monotone: in high-tier regions, large transfers and extensive parental involvement may partially substitute for housing-price variation (e.g., through intergenerational financing or bundled marriage packages), dampening incremental sensitivity to local housing prices. By contrast, in mid-tier regions, transfers are sizable yet more likely to be binding for a broader share of households, so variation in housing prices may translate more directly into marriage-entry constraints for younger women. For men, the weaker tier-based differences are consistent with the idea that the relevant margin is not primarily their marriage-entry response to housing-price gradients, but rather broader cohort-level shifts in marriage timing and local marriage-market conditions that do not align tightly with this coarse tier proxy.

Table 6: Bride-Price Tiers and Heterogeneity in the Post-Period Housing-Price Gradient (Urban)

	Women	Men
Post period × younger group	-0.042	-0.073***
	(0.064)	(0.025)
Implied post-period housing-price gradient for younger vs. older adults		
Low-tier provinces	-0.009	-0.007*
	(0.008)	(0.004)
Mid-tier provinces	-0.050***	-0.025***
	(0.009)	(0.010)
High-tier provinces	-0.014***	-0.008**
	(0.005)	(0.004)
Difference vs. Low tier (Mid) [p-value]	0.001	0.072
Difference vs. Low tier (High) [p-value]	0.604	0.858
Joint test: Mid = High = 0 [p-value]	0.002	0.189
City fixed effects	Yes	Yes
Age fixed effects	Yes	Yes
Weights	EB trimmed	EB trimmed
Clustered standard errors	City	City
Observations	86,564	77,694

Notes: The dependent variable is an indicator for having ever been married. Housing price is measured at the city level (thousand RMB) and interacted with indicators for the post period and younger age group; the table reports the implied tier-specific post-period housing-price gradient for younger relative to older adults, computed from the fully interacted specification. Bride-price tiers are constructed from publicly available news and newspaper reports and assigned at the province level. P-values test whether the housing-price gradient differs between the mid/high tiers and the low tier, and the joint p-value tests both differences simultaneously. All specifications include city and age fixed effects, apply entropy-balancing weights, and cluster standard errors at the city level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

9. Conclusion

This paper examines how the 2011 judicial clarification of premarital property rights in China reshaped marriage behavior in urban areas, and how the response varied with local housing-market conditions. The reform strengthened the protection of individually owned premarital assets—especially housing—and thereby plausibly shifted expectations about marital wealth sharing and bargaining. Empirically, we combine repeated cross-sections of census microdata with city-level housing prices measured prior to the reform. Our baseline design compares changes in marriage outcomes between 2010 and 2015 across cities with different baseline housing costs, and we complement this with a four-wave (2000–2015) entropy-balanced event-study design, continuous-exposure specifications, and age-profile analyses. Together, these approaches allow us to characterize not only whether marriage outcomes changed differentially in high-cost housing markets after the reform, but also when in the life cycle such changes were most pronounced.

Three findings emerge. First, among urban non-migrants near the legal marriage age, marriage outcomes for women decline in high-price cities relative to low-price cities between 2010 and 2015, while the corresponding estimates for men are small and statistically weak. Second, the four-wave entropy-balanced event study shows that pre-reform differences between high- and low-price cities are limited once baseline composition and pre-reform marriage patterns are aligned, while a clear post-reform divergence appears by 2015 for women. Third, moving beyond a median split, we document a post-period shift in the housing-price gradient in marriage outcomes that is concentrated at early ages for women and attenuates with age; the comparable age profile for men is substantially flatter. Complementary province-level evidence from administrative marriage registrations exhibits a directionally consistent post-2011 relative decline in higher-price areas, reinforcing the interpretation that the micro patterns are not driven solely by idiosyncratic sampling variation.

These patterns are consistent with a mechanism in which the reform

reduced the extent to which premarital housing wealth is expected to be shared within marriage, raising the effective match-quality threshold for women in contexts where housing is most salient for marital negotiations. The results further indicate meaningful heterogeneity: the negative post-period housing-price gradient at early ages is less pronounced among more-educated women, suggesting that economic capacity and bargaining resources can partly offset housing-related constraints or expectations in marriage formation. Overall, the evidence highlights a sharp gender asymmetry in marriage responses to an institutional shock that alters the distribution of property rights over a central household asset.

The analysis has two broader implications. Substantively, it underscores that legal rules governing asset ownership can affect marriage-market behavior, not only outcomes within existing relationships. More generally, it emphasizes the interaction between institutional change and local market conditions: the same legal reform can have very different behavioral consequences across cities when the underlying asset—housing—varies widely in value. For policymakers, the findings suggest that reforms intended to clarify and protect individual property rights may generate unintended effects on family formation in high-cost urban environments, with potentially unequal consequences across gender and socioeconomic groups.

Several limitations merit emphasis. While we deploy a range of strategies to improve comparability and probe alternative explanations—including entropy balancing on pre-reform moments, pre-trend diagnostics, within-city age comparisons, controls for local housing-price growth, placebo tests in rural samples, permutation inference, and exclusions of purchase-limit cities—the design cannot fully rule out all coincident, housing-related forces that differentially affected marriage in high-price cities during the 2010–2015 period. Future work could strengthen identification and illuminate mechanisms by linking administrative marriage records to finer measures of housing transactions, premarital homeownership, and parental transfers, or by leveraging

additional policy variation that directly alters housing affordability or credit access. More broadly, tracing longer-run adjustments in matching patterns, co-residence arrangements, and subsequent household outcomes would help clarify the general-equilibrium and dynamic consequences of property-rights reforms in rapidly urbanizing economies.

Data Availability

The individual-level data used in this study come from the *2015 1% Population Sample Survey of China* and the *2010 Sixth National Population Census (1% micro sample)*. Because of confidentiality agreements with the National Bureau of Statistics (NBS), the authors are not permitted to release the raw micro-data. Researchers may apply for access through the NBS data application system (https://data.stats.gov.cn). Aggregated data tables and variable definition files generated during the study are available from the corresponding author upon reasonable request.

Code Availability

All Stata do-files and LaTeX scripts used for data cleaning, analysis, and replication are currently being curated and will be deposited in a public repository once the article is formally published. Researchers who wish to inspect the code during the review process may obtain it from the corresponding author upon reasonable request.

Declaration of Interests

We declare that we have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work, we used OpenAI's ChatGPT in order to improve language clarity and readability. After using this tool, we reviewed and edited the content as needed and take full responsibility for the content of the publication.

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Author Contributions

Z.W. and R.Z. jointly designed the study and contributed to all major aspects of the research, including conceptualization, empirical analysis, and writing. Both authors proofread the manuscript, revised it critically for important intellectual content, and approved the final version.

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A.1. Additional Placebo Tests

Table A1 and Figure A1 present placebo analyses based on rural non-migrants. Both exercises apply the same difference-in-differences and event-study specifications used in the urban baseline to a population for whom urban housing markets and the 2011 judicial interpretation are expected to be substantially less relevant.

The limited salience of the reform for rural marriage decisions reflects fundamental institutional differences between urban and rural households. Rural residents typically hold rights to homestead land, and marriage-related asset considerations are closely tied to these non-tradable land-use rights rather than to market-priced urban housing. Although the legal framework does not prohibit rural residents from purchasing urban housing, in practice rural households are far less likely to own urban real estate prior to marriage, and premarital housing wealth plays a much smaller role in rural marriage arrangements. As a result, variation in urban housing prices—and the clarification of premarital property rights in 2011—is expected to have only a weak connection to rural marriage behavior.

Consistent with this institutional background, the dynamic patterns for rural non-migrants differ markedly from those observed in the urban sample. As shown in Figure A1, pre-reform marriage dynamics for rural populations exhibit greater volatility and do not display the same degree of stability achieved in the entropy-balanced urban sample. This lack of smooth pre-reform dynamics does not pose a threat to the identification strategy of the main analysis, which focuses on urban non-migrants and explicitly improves comparability through reweighting.

The post-reform estimates for rural non-migrants also contrast sharply with the urban results. Neither the DID estimates in Table A1 nor the event-study coefficients in Figure A1 show a systematic decline in marriage outcomes in high-price areas relative to low-price areas. If anything, the estimates suggest modest increases in marriage rates for both women and

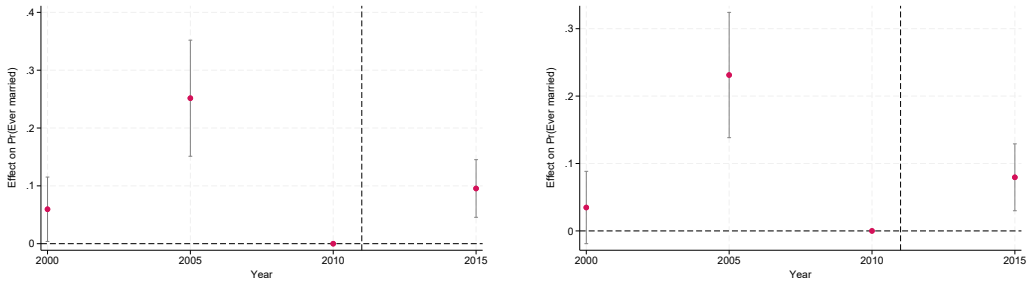
men. We interpret these patterns cautiously. One possible explanation is that rising urban housing prices and tightening marriage-related constraints in cities may induce relative shifts in marriage timing or location choices among rural households, whose marriage decisions are less directly tied to urban housing ownership. More broadly, these results underscore that the mechanisms linking housing markets, legal institutions, and marriage behavior differ sharply across urban and rural contexts.

Taken together, the rural placebo evidence reinforces the interpretation that the main findings are driven by institutionally specific channels operating in urban housing markets, rather than by nationwide trends correlated with housing prices. The absence of a parallel post-reform decline in rural marriage outcomes supports the focus on urban populations for whom premarital housing wealth and its legal treatment are central to marriage decisions.

Table A1: Placebo DID Estimates: Rural Non-migrants

	Female	Male
Post (2015)	-0.033^{***}	0.000
	(0.010)	(0.010)
High price city × Post (2015)	0.039^{***}	0.016
	(0.013)	(0.012)
City fixed effects	Yes	Yes
Age fixed effects	Yes	Yes
Weights	EB trimmed	EB trimmed
Clustered standard errors	City	City
Age range	20–24	22–26
Observations	41,240	45,365

Notes. This table reports placebo difference-in-differences estimates using rural non-migrants only. The specification mirrors the main DID regressions but excludes entropy-balancing weights. The dependent variable is an indicator for having ever been married. All regressions include city fixed effects. Controls include age dummies, education categories, and ethnicity indicators. Standard errors are clustered at the city level. The coefficient of interest is High price city × Post (2015). Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.



(a) Female

(b) Male

Figure A1: Placebo event study for rural non-migrants. The figure applies the same event-study specification as in the urban baseline to rural non-migrant individuals, for whom housing market institutions and the legal environment governing marriage differ substantially. Panel (a) reports results for women aged 20–24, and Panel (b) reports results for men aged 22–26. Estimates show no systematic post-2011 divergence between high- and low-price areas, supporting the interpretation that the main results are not driven by spurious nationwide trends correlated with housing prices.