# Priced Out of Parenthood: Housing Costs, Household Formation, and Fertility in Spain*

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**Abstract**

This paper studies whether housing costs affect fertility by delaying household formation in Spain, where fertility is low and departure from the parental home is late. Over the last two decades, young adults' incomes have declined relative to those of adults aged 35-54, independent living has fallen, and homeownership among those living independently has declined. Households headed by young adults have also become less likely to be couples. Using a yearly municipality-level panel and a shift-share instrument that interacts national house-price movements with predetermined housing-supply constraints, we estimate the effects of local house prices. A 10 percent increase lowers birth rates by about 3 percent among women aged 20-34 and 2 percent among those aged 35-39, and marriage rates by about 5 and 11 percent, respectively. Higher house prices account for roughly half of the 2014-2022 decline in the implied total fertility rate of the typical municipality. We then evaluate a 2018 homeownership subsidy for young buyers in municipalities with fewer than 5,000 inhabitants. An event-study design exploiting the population cutoff shows that eligibility raises births by about 5 percent in 2021-2022 and reduces parental coresidence by about 2 percentage points in 2019-2021. We find no detectable price capitalization, while in-migration estimates through 2020 show no significant increase. The findings suggest that housing affordability constrains family formation in low-fertility settings.

***Keywords***: Housing affordability; fertility; household formation; parental coresidence; marriage; homeownership subsidies; Spain.

***JEL Codes***: J11, J12, J13, R21, R31, H31

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# 1 Introduction

Fertility has fallen dramatically across high-income countries and, increasingly, across middle-income countries as well (Delventhal et al., 2021). In many advanced economies, total fertility rates are now well below replacement, often between 1.2 and 1.5 children per woman. At the same time, the timing of parenthood has shifted later in the life cycle, and the share of women who remain childless has risen. As emphasized by Doepke et al. (2023), Bloom et al. (2024), and Kearney and Levine (2025), this demographic transition has far-reaching consequences for economic growth, the sustainability of social insurance systems, intergenerational inequality, and the organization of family life.

Housing affordability may be an important constraint on household formation and childbearing. Housing is not just another consumption good. Leaving the parental home, forming a couple, marrying, and having children all require space, stability, and the ability to finance a dwelling. Children increase the demand for housing services, and in countries where family formation is closely tied to independent residence, high housing costs may delay several decisions at once. Across 40 countries, Biljanovska et al. (2023) document a broad worsening of housing affordability since the COVID-19 pandemic. TheEuropean Bank for Reconstruction and Development (2025) shows that, for those between the ages of 25 and 34 in advanced EU economies, the share achieving the traditional markers of adulthood (finishing education, joining the labor force, moving out of the parental home, getting married, and having children) fell substantially between 2005 and 2023. Global survey evidence also points in the same direction: many individuals cite economic insecurity and housing costs as barriers to realizing their fertility intentions (United Nations Population Fund, 2025).

This paper asks whether housing costs affect fertility by delaying household and family formation. Four features make Spain a particularly informative setting for studying these questions. First, Spain's total fertility rate (TFR) fell to close to 1.1 in 2023, while the mean age at first birth reached about 31.5 years. Spain also stands out among high-income countries for high childlessness and a low share of women with two or more children (Guner et al., 2024). Second, the living-arrangements channel is likely to be quantitatively important: many young adults remain in the parental home well into their late twenties and early thirties, and youth unemployment and labor market instability are high. Third, Spain experienced large housing-market fluctuations over the last two decades, with substantial geographic variation across municipalities. The Banco de España's *Annual Report 2025* highlights housing-supply constraints and severe affordability problems, particularly for young adults (Banco de España, 2026, Chapter 2). Its calculations for 2024 imply that

a hypothetical household formed by two young earners still living with their families would face an average purchase price equivalent to 8.3 years of their combined net income, holding their incomes unchanged. Finally, Spain offers a recent policy experiment: a homeownership subsidy introduced in 2018 for young buyers in municipalities with fewer than 5,000 inhabitants. This allows us to study not only whether higher house prices reduce fertility and marriage, but also whether a policy that lowers the cost of housing for young adults moves fertility and household formation in the opposite direction.

We structure our analysis in three steps. First, we document the descriptive facts that motivate the analysis. Using the Human Fertility Database, the Spanish Labor Force Survey, and the Spanish Survey of Income and Living Conditions, we show that the most recent episode of fertility decline in Spain is intertwined with delayed transitions into independent living and couple formation. Young adults aged 20-34 spend longer in education, their personal income has declined relative to that of adults aged 35-54, and independent living has become less common. Among households headed by young adults, the share of couples has fallen, while single-person and other non-couple households have become more common. This change in household composition accounts for part of the decline in the share reporting a recent birth. Over the same period, homeownership among young adults living independently has fallen sharply, while renting has become more common.

Second, we estimate the causal effect of local house prices on births and marriages across Spanish municipalities. We combine administrative records of births and marriages from the Spanish Statistical Institute, municipal population from the *Padrón Continuo*, and municipal transaction prices from the Real Estate Registry Statistics. We use a shift-share instrument that interacts national house-price movements with a predetermined proxy for local housing-supply elasticity based on land availability around the year 2000. The idea is that national housing-demand shocks raise prices more in supply-constrained municipalities than in places where housing supply can expand more easily. We find that higher house prices reduce births and marriages. For women aged 20-34, a 10 percent increase in local house prices lowers the birth rate by about 3 percent and the marriage rate by about 5 percent. The crude-rate estimates for the broader municipal sample are larger, consistent with stronger responses in smaller municipalities. Marriage at ages 35-39 is particularly sensitive to prices, suggesting that housing costs bind well into the mid-thirties. A back-of-the-envelope calculation attributes roughly half of the 2014-2022 decline in our implied TFR measure for the typical municipality to higher house prices. Higher prices also shift transactions toward smaller dwellings, increase parental coresidence among young adults, and reduce homeownership among those living independently.

Third, we study the 2018 place-based homeownership subsidy for young buyers. The subsidy provided up to 10,800 euros, or 20 percent of the purchase price if smaller, for eligible buyers under 35 purchasing a primary residence in municipalities with fewer than 5,000 inhabitants. We exploit the population cutoff and the fixed start date in a municipal event-study design, comparing municipalities just below and just above the 5,000-inhabitant threshold. This policy analysis uses two types of data. Births, marriages, and house prices come from municipal administrative records. Leaving the parental home (emancipation), homeownership, and migration come from the *Panel de Hogares*, a longitudinal administrative household panel that identifies the municipality of residence, household composition, and cadastral ownership. Eligibility for the subsidy raises municipal births by about 5 percent in 2021-2022, relative to municipalities just above the cutoff, and reduces parental coresidence by about 2 percentage points in 2019-2021. We find no detectable capitalization in mean municipal transaction prices or increase in young-adult in-migration over the observed migration window, which ends in 2020. Homeownership among independent young adults moves in the expected direction, but the estimate is imprecise in the main bandwidth. Together, these findings are consistent with a household-formation channel, with the clearest response in parental coresidence.

The paper contributes to several literatures. First, it contributes to the literature on the economics of low fertility. Traditional explanations emphasized the quantity-quality trade-off and the opportunity cost of women's time, but in high-income countries the central issue increasingly appears to be compatibility between work, family, and institutions (Goldin, 2021; Doepke et al., 2023). For Spain, Guner et al. (2024) show that labor-market uncertainty and inflexible work schedules are important barriers to fertility. We complement this work by studying housing affordability as another constraint on the transition to parenthood.

Second, the paper contributes to the literature on housing and fertility. Demographic research has long emphasized that housing affordability and family formation are linked: Mulder and Billari (2010) argue that homeownership regimes with difficult market entry are unfavorable to fertility, and Kulu and Steele (2013) show that childbearing and housing transitions are closely interrelated over the family life course. In economics, Dettling and Kearney (2014) show that house-price increases have opposite effects on U.S. fertility depending on homeownership status: they reduce fertility among renters but increase it among homeowners through wealth effects. Lovenheim and Mumford (2013) and Daysal et al. (2021) also emphasize the housing-wealth channel, while Cumming and Dettling (2024) show that mortgage-payment shocks affect births. Dettling and Kearney (2025) study mortgage access and the baby boom, and van Doornik et al. (2024) provide experimental evidence

from Brazil that access to subsidized housing credit increases fertility. Hacamo (2021) shows that mortgage-market deregulation increased home purchases and fertility among young U.S. households, with improved access to housing space as a potential mechanism. Li (2024) provides cross-country evidence linking higher house prices to lower fertility, while van Wijk and Feijten (2025) document differences in fertility responses across tenure groups in the Netherlands. Our contribution is to study Spain, where delayed household formation is central, and to connect local price effects to births, marriages, housing space, and a policy that directly targets young people's access to housing.

Third, we contribute to the literature on household formation and the transition out of the parental home. Ermisch (1999) models leaving and returning to the parental home as responses to housing prices, parental resources, and young adults' own economic conditions. For Spain, Martínez-Granado and Ruiz-Castillo (2002) study the joint decisions to work, study, and leave the parental household, showing that these margins need to be analyzed together to understand young adults' household formation. Martins and Villanueva (2009) show that higher mortgage costs reduce household formation among young adults in Portugal, a Southern European context where home-leaving, marriage, and fertility are closely linked. Becker et al. (2010) use a sample of 13 European countries to show that over the period 1983 to 2004 coresidence increases by 1.7 percentage points following a 10 percentage point rise in the share of youths perceiving their job to be insecure and declines by 1.1 percentage points following the same increase in insecurity for older workers. Cooper and Luengo-Prado (2018) document the role of housing costs and business-cycle conditions in U.S. household formation, while Laeven and Popov (2017) show that young households in high house-price-growth areas were less likely to buy homes, marry, and have children. Chiocchio (2025) develops a structural model in which higher house prices increase parental coresidence and reduce marriage by limiting young adults' participation in the marriage market. Finally, Couillard (2025) estimates a dynamic model of the joint housing-fertility choice for the U.S., allowing for choices over location and house size, and finds that rising costs since 1990 account for 51 percent of the total fertility rate decline between the 2000s and 2010s.

Finally, the paper is closely related to Spain-specific evidence on independent living among young adults, marriage, and housing policy. Aparicio-Fenoll and Oppedisano (2015) study the 2008 Spanish rental subsidy, *Renta Básica de Emancipación*, and show that reducing rental costs increased living away from parents, cohabitation, and childbearing. Ahn and Sánchez-Marcos (2017) document the evolution of emancipation during Spain's boom-bust cycle and discuss the role of house prices, credit conditions, and rental subsidies in shap-

ing young adults' living arrangements. For marriage, González-Val (2022) studies Spanish provinces and large cities and finds a negative relationship between house prices and marriage rates. Luccioletti (2024) studies the same young-buyer subsidy, focusing on homeownership and internal migration. We complement that analysis by examining fertility and parental coresidence.

We build on this literature by estimating the effect of municipal house prices on births and marriages using supply-driven price variation and by evaluating a subsequent homeownership subsidy targeted at young buyers in small municipalities.

The rest of the paper is organized as follows. Section 2 describes the data. Section 3 presents the descriptive evidence on fertility, young adults' economic circumstances, living arrangements, household composition, and housing tenure. Section 4 estimates the effect of house prices on births, marriages, and transacted housing space. Section 5 evaluates the 2018 homeownership subsidy. Section 6 concludes.

# 2 Data Sources

The paper combines several complementary data sources, since no single dataset provides all the outcomes and geographic detail needed for the analysis. We first use survey data to document long-run trends in the economic circumstances, living arrangements, household composition, fertility, and housing tenure of young adults. We then construct a yearly municipality-level panel to estimate the effects of local house prices on births and on marriages. Finally, we use a longitudinal administrative household panel to study individual-level outcomes (emancipation, homeownership, and migration) and evaluate the place-based homeownership subsidy introduced in Spain in 2018. These sources combine broad geographic coverage with detailed information on household circumstances, and we keep definitions and sample restrictions consistent across analyses whenever possible. Appendix A provides detailed descriptions of the sources, sample restrictions, and variable construction.

The descriptive evidence in Section 3 relies primarily on two household surveys. We use the 2005-2024 waves of the Spanish Labor Force Survey (*Encuesta de Población Activa*, EPA) to document trends in education, employment, contract type, job tenure, independent living, household composition, and births among young adults. The Spanish Survey of Income and Living Conditions (*Encuesta de Condiciones de Vida*, ECV) constitutes Spain's component of the European Union Statistics on Income and Living Conditions (EU-SILC). We use its 2004-2023 waves to complement the EPA with detailed information on income, housing tenure, housing costs, and dwelling characteristics. In both surveys, we focus on individuals

aged 20-34, the ages at which education, labor-market entry, leaving the parental home, partnership formation, and first births are concentrated. We also use data from the Human Fertility Database to place Spain's fertility decline in comparative perspective using period and cohort fertility indicators.

For the analysis in Section 4, we build a yearly municipality-level panel covering 2014-2022, excluding 2020, that combines demographic outcomes, population counts, local house prices, and local controls. Births and marriages come from the vital statistics (*Movimiento Natural de la Población*) of the Spanish Statistical Institute (INE), which record the universe of registered births and marriages by year and by the mother's municipality of residence for births and the couple's municipality of residence for marriages. We use population counts from the *Padrón Continuo*, the continuously updated municipal population register, to calculate crude birth and marriage rates per 1,000 inhabitants. INE also publishes age-specific birth and marriage counts for municipalities with more than 10,000 inhabitants, which we use to construct rates for ages 20-34 and 35-39.

Local house prices are from the Real Estate Registry Statistics, available through the Banco de España Data Laboratory (BELab). These data are produced by the Spanish Association of Property and Commercial Registrars and are based on real-estate transaction records. From this dataset, we also extract the mean floor area, in square meters, of dwellings transacted in each municipality and year. We complement the price data with a local income control from the *Atlas de Distribución de la Renta de Hogares* (ADRH), produced jointly by INE and the Spanish Tax Agency. The ADRH provides municipality-level measures of income, age structure, household size, and household composition for 2015-2023.

A key component of the municipal analysis is the construction of a housing-supply measure used in the shift-share instrument. CORINE Land Cover 2000 is a pan-European inventory that classifies land cover around the year 2000. We use this edition because it predates our 2014-2022 estimation period, providing a measure of land availability that does not reflect housing development in response to the price and fertility changes we study. We combine it with topographic data from the Spanish National Geographic Institute to compute each municipality's share of buildable land. Land is classified as buildable if it is not already built, is not water or wetland, and has a slope below fifteen degrees. The share is defined as the area of buildable land divided by the area of potentially developable land, that is, built-up plus buildable land. It therefore measures how much room a municipality has to expand its housing stock rather than the share of its total surface area. This predetermined buildable-land share proxies for the elasticity of local housing supply. Interacted with national house-price movements, it provides supply-driven variation in local house prices.

To evaluate the 2018 homeownership subsidy for young buyers in small municipalities in Section 5, we use the *Panel de Hogares* of the Spanish Institute of Fiscal Studies (*Instituto de Estudios Fiscales*, IEF). The IEF Household Panel is a longitudinal administrative panel covering 2016-2023; our policy analysis uses 2016-2022. It links population records and household composition from INE with income, tax, and cadastral property information from the Spanish Tax Agency, but excludes the Basque Country and Navarre. Unlike the EPA and ECV, the IEF Household Panel identifies the municipality of residence and links individuals to the dwellings owned by household members. This allows us to study homeownership, emancipation, and internal migration within the municipal policy design. We aggregate the person-level records into annual municipality-level averages for young adults aged 20-34 and compare municipalities just below and just above the policy's population cutoff.

# 3 Overview

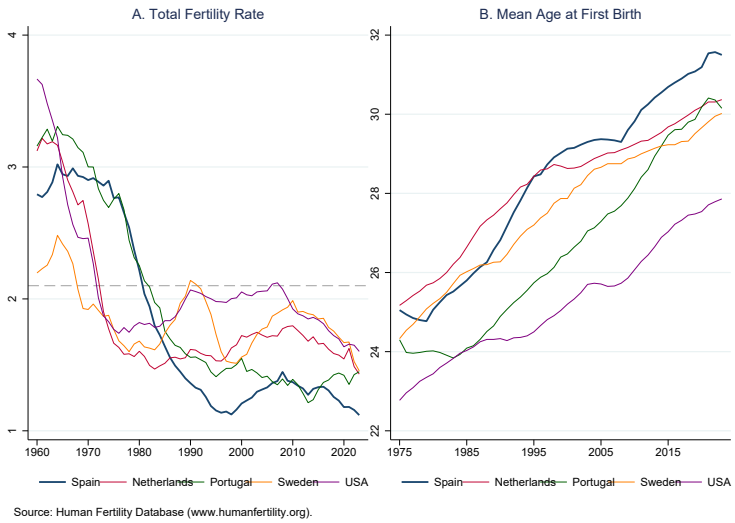
This section documents the main descriptive facts that motivate our empirical analysis. We begin by placing Spain's fertility decline in comparative perspective. We then describe how the economic conditions, living arrangements, household composition, and housing tenure of young adults have changed over the last two decades. Fertility has fallen sharply, young adults are less likely to live independently of their parents, and, among those living independently, both the share living as couples and the homeownership rate have declined.

## 3.1 Trends in Fertility

Figure 1 places Spain's fertility decline in comparative perspective using indicators from the Human Fertility Database. Panel A shows the total fertility rate (TFR) for Spain, Portugal, the Netherlands, Sweden, and the United States since 1960. Portugal provides a close Southern European comparison, while the Netherlands, Sweden, and the United States offer European and North American contrasts with generally higher fertility over recent decades. All five countries experienced substantial declines in fertility, although the timing and extent differ. Spain's TFR fell from approximately 3.0 in the mid-1960s to nearly 1.1 by 2023, with a temporary recovery during the 2000s. Portugal followed a broadly similar trajectory, while fertility in the other countries, particularly in Sweden and the US, remained higher from the 1980s onward. The dashed horizontal line marks the approximate replacement level of 2.1 children per woman; all five countries are below this level in the most recent years shown.

Panel B documents the postponement of parenthood. Spain's mean age at first birth rose by more than six years between the mid-1970s and 2023, reaching about 31.5 years, the highest level in the comparison group. The mean age at first birth has risen in the other countries as well, but remains lower than in Spain, especially in Sweden and the United States.

Figure 1: Cross-Country Comparison over Time



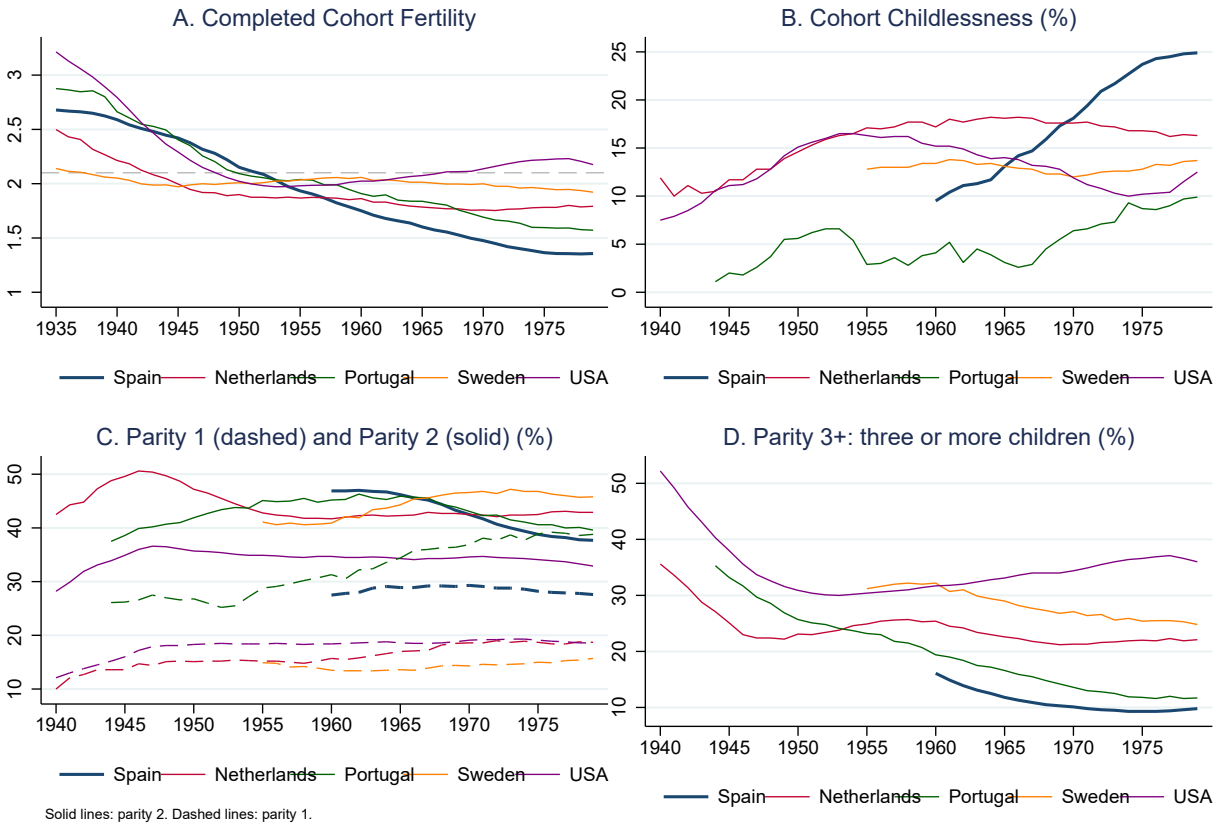

NOTES: Authors' calculations using the Human Fertility Database for Spain, Portugal, the Netherlands, Sweden, and the United States. Panel A reports the period total fertility rate: the number of children a woman would have if she experienced the age-specific fertility rates of that year throughout her reproductive life. Panel B reports the mean age at first birth, in years. The dashed horizontal line in Panel A marks the approximate replacement level of 2.1 children per woman.

Figure 2 shifts to a cohort perspective. Cohort measures summarize fertility among women born in the same year and, once childbearing is complete, are not reduced merely by births being postponed. Panel A reports completed cohort fertility (CCF), the average number of children per woman at the end of reproductive life. Spain's cohort fertility fell from roughly 2.7 children per woman born around 1935 to fewer than 1.4 per woman born in the mid-1970s. The Netherlands, Sweden, and the United States maintain higher cohort fertility than Spain for much of the sample, while Portugal follows a similarly low trajectory for recent cohorts.

Panel B shows a sharp increase in childlessness in Spain: from around 10 percent among women born around 1960 to approximately 25 percent among those born in the mid-1970s. Panels C and D show declining shares of Spanish women with two children and with three or more children. Thus, Spain's fertility decline reflects both smaller families among mothers

and a rising share of women who do not become mothers.

Figure 2: Cross-Country Comparison over Cohorts




Source: Human Fertility Database (www.humanfertility.org).

NOTES: Authors' calculations using the Human Fertility Database for Spain, Portugal, the Netherlands, Sweden, and the United States. Panel A reports completed cohort fertility for women born between 1935 and 1979, subject to country-specific data availability. Panel B reports the percentage of women in each cohort who remain childless. Panel C reports the percentages with exactly one child (dashed lines) and exactly two children (solid lines). Panel D reports the percentage with three or more children. Cohort outcomes are measured for women observed close to the end of their reproductive lives.

## 3.2 Young Individuals

We next describe the economic circumstances and living arrangements of young adults in Spain, using the Labor Force Survey (*Encuesta de Población Activa*, EPA) and the Survey of Income and Living Conditions (*Encuesta de Condiciones de Vida*, ECV). We focus on ages 20-34, when education, labor-market entry, independent living, partnership formation, and childbearing often overlap. Independent living means residing outside the parental

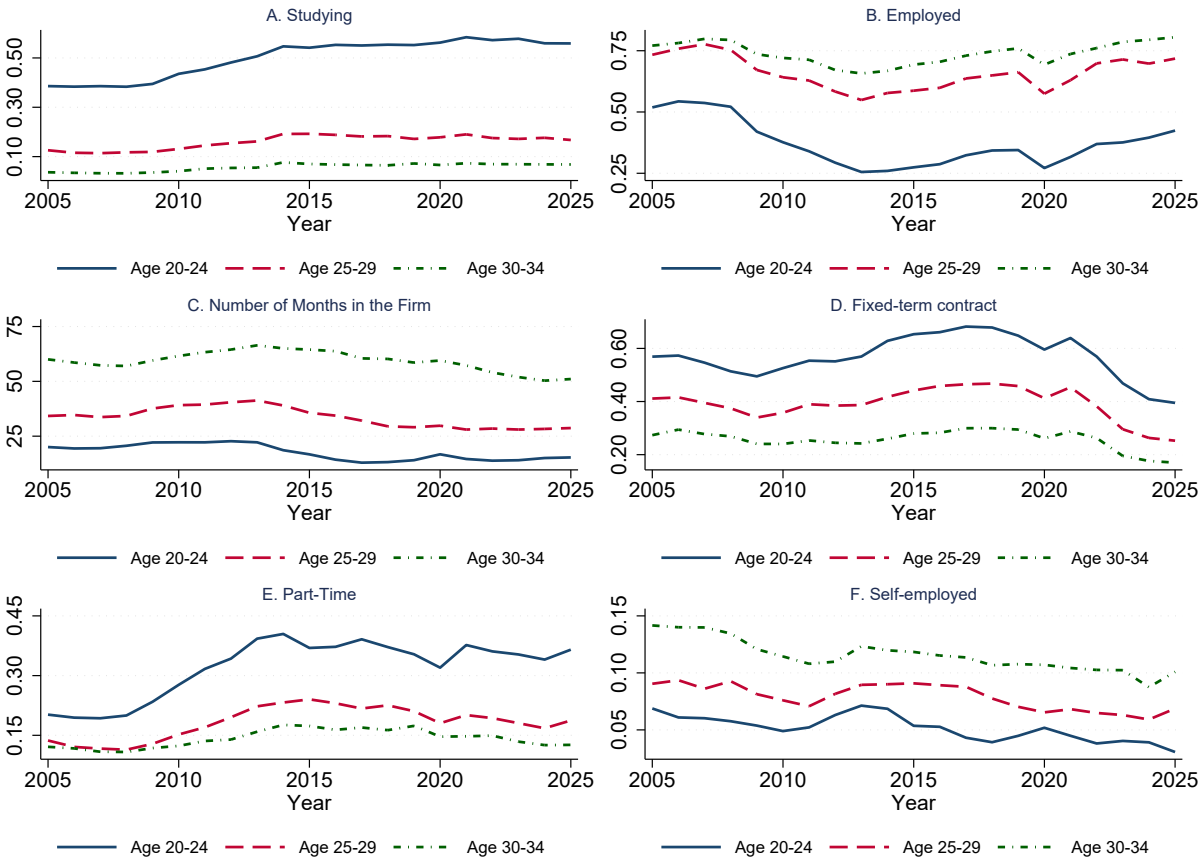
household; it does not necessarily imply living alone or financial independence. Appendix A.1 and Appendix A.2 describe the surveys.

**Labor Market Entry.** Figure 3 documents changes in education and employment. In Panel A, the share of 20-24 year-olds in full-time education rises from roughly 40 percent at the beginning of the sample to about 55 percent at the end. Panel B shows a pronounced fall and subsequent recovery in employment. By the end of the period, employment rates are about 70 percent among 25-29-year-olds and 80 percent among 30-34-year-olds. Among 20-24-year-olds, employment is around 40 percent, compared with about 50 percent at the start of the sample period. These patterns are consistent with young adults spending longer in education before entering employment.

The remaining panels show a mixed picture of employment stability and work intensity. In Panel C, average tenure in the current firm among 25-29 year-olds falls from roughly 45 months in the early 2010s to about 30 months at the end of the sample. Yet fixed-term employment also declines: in Panel D, the share of employees aged 25-29 on fixed-term contracts falls from around 40 percent in the mid-2010s to about 20 percent by the end of the period. Panel E shows that part-time work among employed 20-24-year-olds rises from roughly 20 to 35 percent across the sample. Self-employment remains below 15 percent in all three age groups and generally declines (Panel F). Thus, the recovery in employment and decline in temporary contracts coexist with shorter job tenure and a greater prevalence of part-time work among the youngest workers.

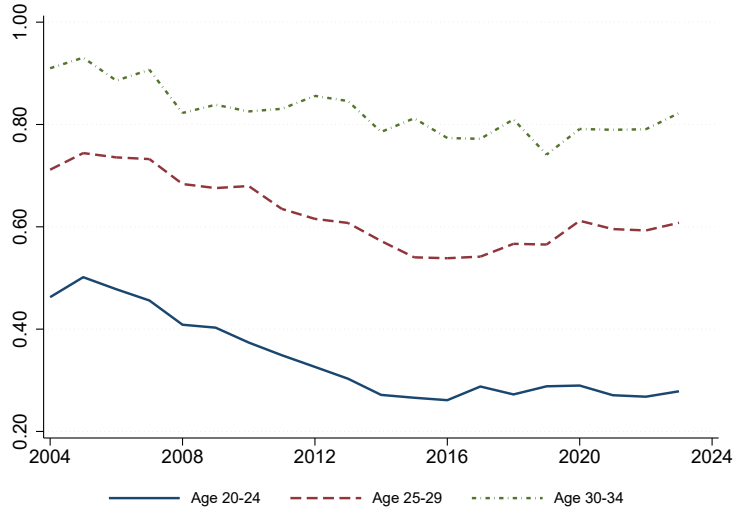
Figure 4 shows the mean net personal income for each younger age group relative to the mean net personal income among individuals aged 35-54. Means are calculated among individuals with positive personal income, including unemployment benefits. The ratio for 20-24 year-olds falls from roughly 0.45 in 2004 to around 0.30 in 2023; the corresponding ratios decline from about 0.70 to 0.60 for ages 25-29 and from about 0.90 to 0.80 for ages 30-34. Much of the decline occurs before the mid-2010s, followed by a partial recovery in the older groups. Longer education and more part-time work may contribute to the decline among the youngest adults.

Figure 3: Studying and Working




NOTES: Authors' calculations using the Spanish Labor Force Survey (EPA), 2005-2024. The sample includes individuals aged 20-34, shown separately for ages 20-24, 25-29, and 30-34. Panel A reports the share in full-time education. Panel B reports the employment rate. Panel C reports mean tenure in the current firm, in months, among employed individuals. Panel D reports the share of employees with fixed-term contracts. Panels E and F report the shares of employed individuals working part-time and in self-employment, respectively. Shares are expressed as proportions.

Figure 4: Personal Income Relative to Ages 35-54

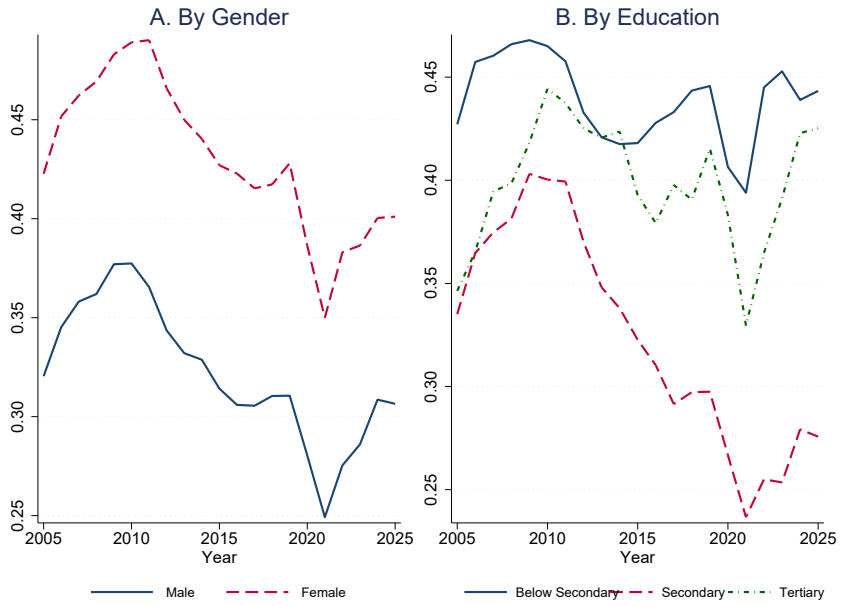



NOTES: Authors' calculations using the Spanish Survey of Income and Living Conditions (ECV), 2004-2023. The young-adult sample includes individuals aged 20-34. Each series reports mean net personal income in the indicated age group divided by mean net personal income among individuals aged 35-54 in the same year. Means are calculated among individuals with positive personal income, including unemployment benefits. The vertical axis reports a ratio, not an income level; a value of 0.60 means that the group's mean income is 60 percent of the reference group's mean income.

**Household Formation.** We now turn to living arrangements. Figure 5 shows independent living by gender and education. Women are consistently more likely to live independently than men, with a gap of roughly 10 percentage points over much of the period. Rates decline from their peaks around 2010 for both groups. By education, the fall is most pronounced among individuals with secondary education, while the series for those with below-secondary and tertiary education are more variable.

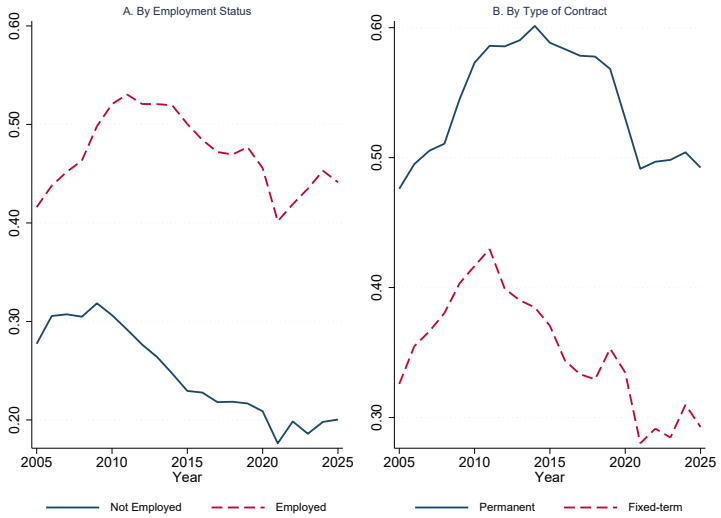
Figure 6 shows that independent living is closely associated with labor-market attachment. Employed young adults are much more likely to live independently than those who are not employed, and employees with permanent contracts are more likely to do so than those with fixed-term contracts. These persistent gaps highlight the importance of considering housing costs alongside young adults' employment circumstances.

Figure 5: Independent Living by Gender and Education




NOTES: Authors' calculations using the Spanish Labor Force Survey (EPA), 2005-2024. The sample includes individuals aged 20-34. Independent living means residing outside the parental household. Panel A reports the share living independently by gender. Panel B reports the corresponding shares by educational attainment: below secondary, secondary, and tertiary. The unit of analysis is the individual. Shares are expressed as proportions.

Figure 6: Independent Living by Labor Market Status

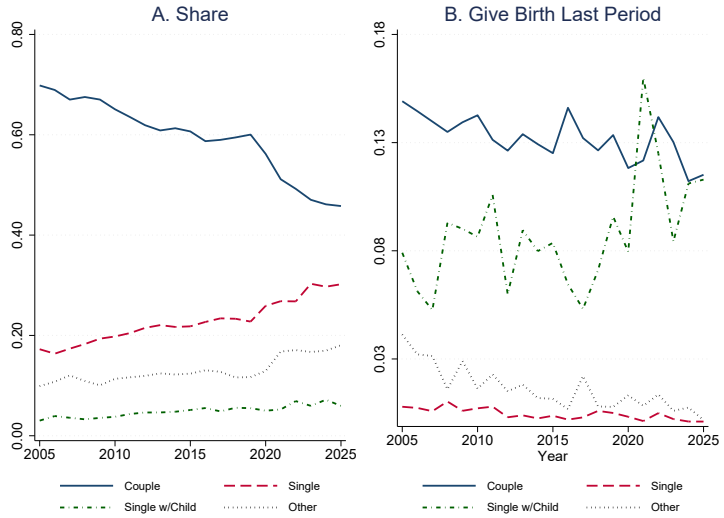



NOTES: Authors' calculations using the Spanish Labor Force Survey (EPA), 2005-2024. The sample includes individuals aged 20-34. Independent living means residing outside the parental household. Panel A reports the share living independently by employment status. Panel B restricts the sample to employees and reports the corresponding share by contract type. Permanent contracts are open-ended; fixed-term contracts are temporary employment arrangements. Shares are expressed as proportions.

**Fertility.** Household composition connects independent living to fertility. Figure 7 shows household composition among households headed by someone aged 20-34. Panel A shows a shift away from couple households toward living alone and other non-couple arrangements. Panel B reports, for each household type, the share of households that report that a child was born in the previous year. Births are much more common in couple households than in single-person and other household arrangements. The series for single adults with children is volatile and should be interpreted cautiously given the small size of this group.

The share of households headed by young adults that report a recent birth can fall even if the corresponding share within couple households changes little, simply because couples represent a declining fraction of these households. Figure 8 illustrates this accounting relationship. It compares the observed share with a counterfactual that fixes the distribution of household types at its 2005 level while allowing the within-type birth indicators to vary. The counterfactual lies above the observed series in later years, indicating that changes in household composition account for a substantial part of the decline in this measure. This descriptive decomposition does not identify the causes of household change. It motivates our question: whether housing costs reduce fertility partly by making independent living and couple formation more difficult.

Figure 7: Household Type and Births among Households Headed by Young Adults




NOTES: Authors' calculations using the Spanish Labor Force Survey (EPA), 2005-2024. The sample includes households with a head aged 20-34. Panel A reports their distribution across household types: couple, single-person household, single adult with a child, and other arrangements. Panel B reports, for each household type, the share of households who report that a child was born in the previous year. Shares are expressed as proportions.

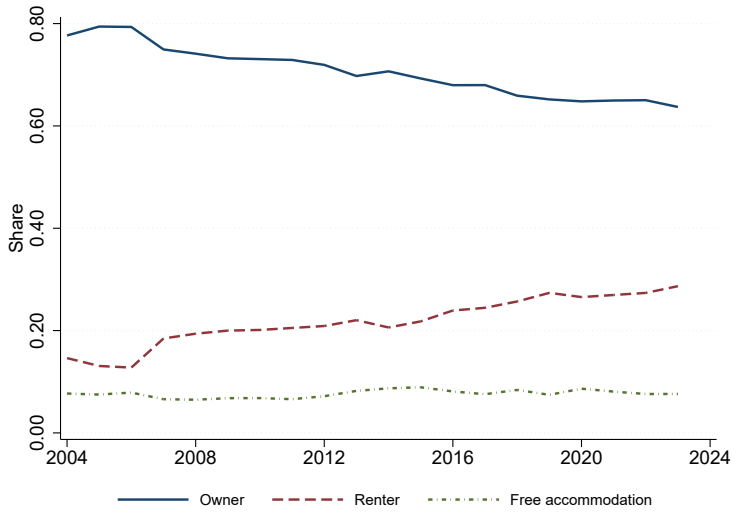
Figure 8: Share of Young Households Reporting a Birth Last Year




NOTES: Authors' calculations using the Spanish Labor Force Survey (EPA), 2005-2024. The sample includes households with a head aged 20-34. The solid line is the share who report that a child was born in the previous year. The dashed line combines each year's within-type shares with the distribution of household types among households with a young head in 2005. Shares are expressed as proportions.

**Housing Tenure.** Figure 9 documents housing tenure among young adults living independently. The share living in owner-occupied housing falls sharply, while the share renting at market rates rises by approximately 20 percentage points. Free accommodation (occupying a dwelling without paying rent) remains a smaller but persistent category. It may reflect housing support from parents or other relatives, although tenure status alone does not identify who provides the accommodation. Such support could help some young adults leave the parental home without facing market rents or the cost of purchasing a dwelling.

Figure 9: Housing Tenure among Young Adults Living Independently




NOTES: Authors' calculations using the Spanish Survey of Income and Living Conditions (ECV), 2004-2023. The sample includes individuals aged 20-34 who live outside their parental household. The figure reports their distribution by household tenure status: owner, renter at market rent, and free accommodation. Free accommodation means occupying a dwelling without paying rent. Shares are expressed as proportions.

Figure 10 reports housing costs (mortgage payments for owners and rent for tenants) as a share of household disposable income among independent young adults. Renters face higher cost burdens than owners, and both series decline in the latter part of the sample. These conditional measures need not track the housing costs faced by young adults who have not yet left the parental home. One possible explanation is selection: those able to live independently may increasingly have higher household incomes or greater family support. The figure does not establish this explanation, but it highlights a distinction between the costs paid by existing independent households and the affordability of forming a household in the first place. The following section examines how local house prices affect births and marriages.

Figure 10: Housing Cost to Household Income among Young Adults Living Independently




NOTES: Authors' calculations using the Spanish Survey of Income and Living Conditions (ECV), 2004-2023. The sample includes individuals aged 20-34 who live outside their parental household. The figure reports housing costs (mortgage payments for owners and rent for tenants) as a percentage of household disposable income, separately for owners and renters, using the housing-cost components reported in the ECV. The calculation is conditional on independent living and excludes young adults living with their parents.

# 4 House Prices, Fertility, and Marriage

In this section, we estimate the effect of local house prices on births and marriages across Spanish municipalities. The analysis combines administrative records of births and marriages from the Spanish Statistical Institute (INE), municipal population from the *Padrón Continuo*, and municipal house prices and transacted floor area from the Real Estate Registry Statistics for the period 2014-2022. We exclude 2020 because the COVID-19 pandemic disrupted civil weddings and the registration of vital events, with some events recorded late or attributed to the wrong municipality. We also use the IEF Household Panel for parental coresidence and homeownership outcomes over 2016-2022, again excluding 2020. We describe each data source in Section 2 and Appendix A.

The section proceeds in four steps. First, we document the geographic distribution of house prices, births, and marriages across Spain. We find that the raw correlations between house prices and both birth and marriage rates are positive. These correlations do not identify causal effects, as municipalities differ in labor markets, amenities, and population composition. Second, we estimate the effect of house prices using within-municipality variation over time and a shift-share instrument that isolates supply-driven movements in local

prices. The instrument interacts a national house-price shock with a predetermined measure of each municipality's housing-supply elasticity, based on buildable land. Third, we assess the robustness of the estimates to alternative specifications, samples, price measures, and definitions of the local housing market. Finally, we examine potential mechanisms: whether higher prices shift transactions toward smaller dwellings, increase parental coresidence among young adults, and reduce homeownership among those living independently.

A 10 percent increase in local house prices lowers birth rates by about 3 percent among women aged 20-34 and 2 percent among those aged 35-39. The corresponding marriage rates fall by about 5 and 11 percent. In the full estimation sample, crude birth and marriage rates fall by about 5 and 7 percent, respectively. The same price increase is associated with an approximately 2 percent decline in the mean floor area of transacted dwellings, a rise of about 1 percentage point in parental coresidence among young adults, and a decline of about 2 percentage points in homeownership among those living independently.

These findings provide the causal counterpart to the descriptive evidence in Section 3: when housing becomes more expensive, births and marriages decline.

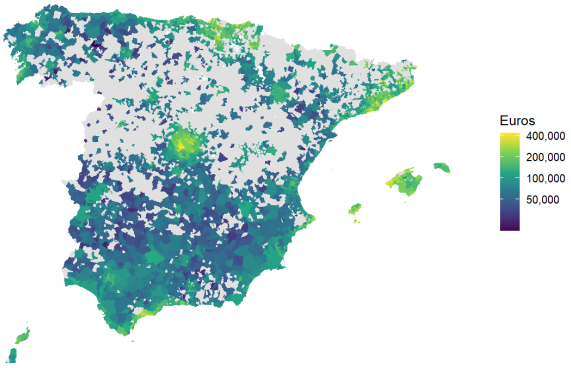
## 4.1 Descriptive Patterns

Figure 11 maps municipal house prices, crude birth rates, and crude marriage rates. House prices have a sharp geographical pattern. They are high in large metropolitan areas such as Madrid and Barcelona, in coastal and tourist destinations such as the Mediterranean coast and the Balearic Islands, and low across much of the rural interior.[1] To complement this evidence, Figure 12 plots the crude birth and marriage rates against local house prices across twenty price bins before any fixed effects or instruments. In the pooled municipality-year observations, higher prices are associated with higher birth and marriage rates: the correlations are +0.08 and +0.23, respectively. These positive correlations may reflect the fact that high-wage, high-amenity municipalities attract young households, potentially masking a negative effect of housing costs. To identify the effect of house prices, we exploit within-municipality variation and instrument local prices using predetermined housing-supply constraints interacted with national price movements.

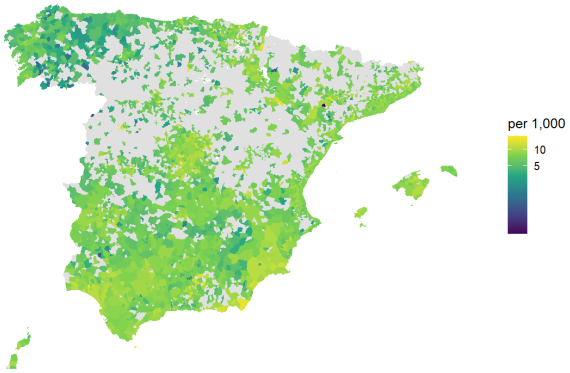
[1] Birth and marriage rates are spatially noisier because, in small municipalities, a few events can generate large changes in per-capita rates.

Figure 11: Geography of House Prices, Births, and Marriages

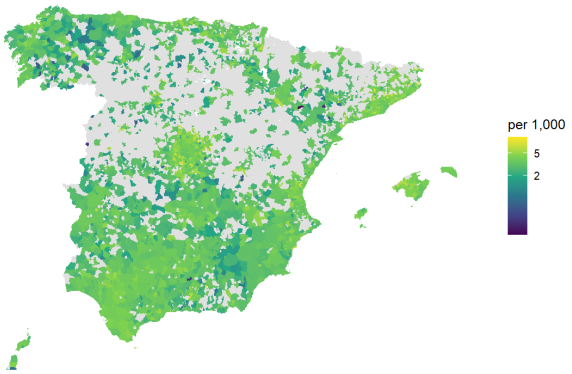
**Panel A: House price**




**Panel B: Birth rate**

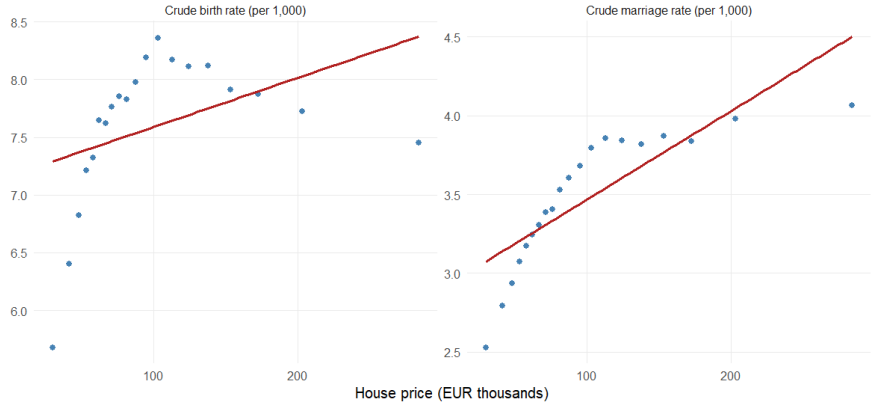



**Panel C: Marriage rate**




NOTES: Municipal house prices from the Real Estate Registry Statistics, vital statistics from INE, and municipal population from the *Padrón Continuo*. Panel A reports mean municipal transaction prices in current euros. Panels B and C report births and marriages per 1,000 inhabitants. Municipal values are averages over available observations in 2014-2022, excluding 2020, after requiring lagged-price and instrument data as in the full-sample crude-rate analysis. For the crude-rate panels, zero municipality-year rates are excluded, and positive rates are retained only within the first- and 99th-percentile cutoffs computed in the full price panel. Grey areas have no plotted data under these restrictions.

Figure 12: Raw Correlations between House Prices, Births, and Marriages




NOTES: Municipal house prices from the Real Estate Registry Statistics, vital statistics from INE, and population from the *Padrón Continuo*. The figure uses pooled municipality-year observations from 2014-2022, excluding 2020, with lagged-price and instrument data available. Current-year mean transaction prices are in thousands of euros, and crude birth and marriage rates are in levels per 1,000 inhabitants. Zero rates are excluded to match the baseline log-regression samples, while positive rates are retained only within the first- and 99th-percentile cutoffs computed in the full price panel. Twenty bins are formed from the distribution of current-year prices across these observations. Each dot shows the mean price and mean outcome within a bin; each line is an unweighted linear fit to the twenty bin means. The correlations in the text use the underlying, unbinned municipality-year observations, separately for births and marriages.

## 4.2 Empirical Strategy

The raw correlations between house prices and birth and marriage rates are unlikely to identify a causal effect, for two reasons. The first is reverse causality: family-formation decisions may themselves affect local housing demand and prices. A decline in the number of young people forming households, for instance, can reduce housing demand and depress local prices. The second reason is omitted-variable bias: local labor-market booms, migration shocks, or amenity changes can raise both house prices and the number of births or marriages. We use lagged house prices to allow housing conditions to precede the demographic outcomes. Municipality and year fixed effects absorb persistent differences across places and common aggregate shocks. Since lagged prices may still respond to anticipated household demand or unobserved local conditions, we also use an instrument based on housing-supply constraints.

Our estimating equation is

$$\ln Y_{mt} = \beta \ln P_{m,t-1} + \theta_m + \gamma_t + \varepsilon_{mt}, \tag{1}$$

where $Y_{mt}$ is a birth or marriage rate in municipality $m$ and year $t$, or the mean transacted floor area in the floor-area exercise, $P_{m,t-1}$ is the one-year-lagged mean transaction house price, and $\theta_m$ and $\gamma_t$ are municipality and year fixed effects. The coefficient of interest, $\beta$, is an elasticity, measuring the percentage change in the outcome associated with a 1 percent increase in local house prices in the preceding year. The sample is a municipality-year panel covering 2014 to 2022, excluding 2020, and standard errors are clustered at the municipality level.[2]

The fixed effects absorb a broad class of confounders. Year effects absorb shocks common to all municipalities, such as national movements in access to mortgages, interest rates, and aggregate housing demand. Municipality effects absorb time-invariant local characteristics, such as geography, amenities, average price levels, and persistent norms around family formation. Identification therefore comes from deviations in municipal house prices over time relative to the national trend. In the robustness checks, we also add a time-varying local income control, although it is available only for a shorter period.

We estimate equation (1) using crude and age-specific rates. Crude rates divide total births or marriages by total municipal population. They are available for all municipalities in the vital statistics, although our estimation sample is limited by house-price coverage. These rates reflect population composition as well as demographic behavior. Age-specific

[2]In Appendix B.10, we also report standard errors two-way clustered by municipality and year, following Cameron et al. (2011), to allow for correlated shocks across municipalities within a year. The IV estimates remain negative and statistically significant.

rates instead divide events involving women in a given age band by the number of women in that band. They therefore reduce the influence of broad age-composition changes, although composition within each band may still change. We report rates for ages 20-34 and 35-39, available for municipalities with more than 10,000 inhabitants.[3]

Within-municipality price changes may still be correlated with unobserved local conditions. We therefore instrument the lagged local price using a Bartik-style shift-share design, following Dettling and Kearney (2014), Chetty et al. (2017), and Basco et al. (2025). The instrument interacts the national house-price index with each municipality's predetermined buildable-land share, a proxy for local housing-supply elasticity:

$$\ln P_{m,t-1} = \pi \left( S_m \times D_{t-1} \right) + \alpha_m + \lambda_t + u_{mt}, \tag{2}$$

where $S_m$ is the area classified as buildable divided by the sum of built-up and buildable land in municipality $m$, measured using geographic information from around the year 2000, $D_{t-1}$ is the Spanish residential property price index in the first quarter of year $t-1$, and $\pi$ is the first-stage coefficient that measures how strongly supply-constrained municipalities transmit national price movements into local prices.[4] The intuition is that a common national housing-demand shock raises local prices more where supply is inelastic, that is, where buildable land is scarce, than where housing supply can expand more easily. Since higher values of $S_m$ indicate more buildable land, $\pi$ is expected to be negative.

The identifying assumption is that, conditional on municipality and year fixed effects, the interaction $S_m \times D_{t-1}$ affects births and marriages only through local house prices (Goldsmith-Pinkham et al., 2020). In particular, absent the induced price changes, municipalities with different buildable-land shares must not experience demographic changes systematically associated with this interaction. Measuring $S_m$ before the estimation period limits reverse causality, while the robustness checks in Section 4.6 address potential confounding geographic trends.

To construct the instrument, we proceed as follows. The supply term $S_m$ is buildable land divided by built-up plus buildable land in municipality $m$, measured around the year 2000. We start from *CORINE Land Cover 2000* and classify as buildable the land that is not already built, not water, and not wetland. We then remove steep terrain using the Spanish National Geographic Institute's 200-meter digital elevation model (*MDT200*), dropping land with a slope above fifteen degrees. The resulting measure captures how much room a mu-

[3] In 2023, 777 municipalities out of 8,139 have more than 10,000 inhabitants in Spain. About 80 percent of the Spanish population lives in these municipalities.

[4] Index obtained from Federal Reserve Economic Data (FRED), series `QESN628BIS`.

nicipality has to expand its housing stock and is the Spanish counterpart to the geographic supply-elasticity measures in Saiz (2010).

## 4.3 Sample, Descriptive Statistics, and First Stage

The estimation sample is a municipality-year panel covering 2014 to 2022 and excluding 2020.[5] Table 1 reports summary statistics. Age-specific rates are defined per 1,000 women in the relevant age band and are available only for municipalities with more than 10,000 inhabitants. The mean birth rate is approximately 59 births per 1,000 women in the age group 20-34, and 61 in the age group 35-39. Marriage rates are 23 and 18, respectively. The crude birth and marriage rates, defined as the number of events per 1,000 inhabitants, average 7.5 and 3.4, respectively. The mean municipal transaction price is about 103,000 euros.

Table 2 reports the first stage estimates. The instrument strongly predicts the local house price. Municipalities with more buildable land experience smaller price increases for a given national house-price movement, so the coefficient on $S_m \times D_{t-1}$ is negative. The first-stage $F$ statistic is 159 in the age-specific sample and 290 across all municipalities, well above conventional weak-instrument thresholds.[6]

## 4.4 Results

Table 3 reports the OLS and IV estimates, first-stage $F$ statistics, and number of municipality-year observations. The final column shows the approximate percentage change in each outcome from a 10 percent increase in house prices. Panel A covers municipalities with more than 10,000 inhabitants, for which age-specific rates are available; Panel B reports crude rates for all municipalities with house-price data.

The IV estimates are more negative than the OLS estimates. This difference is consistent with time-varying local conditions and household sorting that raise both prices and demographic outcomes. In the full municipal sample (Panel B), the IV elasticities are $-0.537$

[5]Municipal transaction-price coverage expands substantially from 2014 onward; see Appendix Table A.2. Outcomes are matched to the previous calendar year's price. For crude rates, the baseline excludes zeros and retains positive observations within the fixed first- and 99th-percentile cutoffs computed in the full price panel. Zero rates account for 0.21 percent of birth observations and 1.13 percent of marriage observations before these restrictions. Appendix B.1 describes the cutoffs and alternative treatments of zeros and extreme rates.

[6]The all-municipality column uses the crude-birth estimation sample. In the crude-marriage sample, the corresponding first-stage $F$ statistic is 287.

Table 1: Summary Statistics, Municipal Sample

| | $N$ | Municipalities | % of pop. | Mean | SD | Median |
|---|---|---|---|---|---|---|
| Birth rate, 20-34 (per 1,000 women) | 6,032 | 777 | 79.5 | 59.3 | 13.1 | 58.4 |
| Birth rate, 35-39 (per 1,000 women) | 6,032 | 777 | 79.5 | 61.4 | 13.5 | 61.0 |
| Birth rate, crude (per 1,000 pop.) | 22,391 | 3,162 | 95.1 | 7.5 | 2.4 | 7.5 |
| Marriage rate, 20-34 (per 1,000 women) | 6,032 | 777 | 79.5 | 23.1 | 7.0 | 22.4 |
| Marriage rate, 35-39 (per 1,000 women) | 6,026 | 777 | 79.4 | 18.3 | 6.4 | 17.9 |
| Marriage rate, crude (per 1,000 pop.) | 22,360 | 3,169 | 95.4 | 3.4 | 1.3 | 3.4 |
| Mean house price (euros) | 21,999 | 3,098 | 95.3 | 103,342 | 62,138 | 84,700 |
| Instrument $S_m \times D_{t-1}$ | 22,847 | 3,171 | 95.7 | 75.2 | 13.8 | 74.9 |

*Notes.* The table reports variable-specific summary statistics for 2014-2022, excluding 2020. $N$ is the number of observations available for each variable. "Municipalities" is the number of distinct municipalities entering each sample, and "% of pop." is the share of the Spanish population living in them, computed as covered municipal population over the window divided by the national total from the INE municipal population register. The age-specific rates for ages 20-34 and 35-39 are per 1,000 women in the corresponding age band and are defined only for municipalities with more than 10,000 inhabitants. The crude rates are per 1,000 inhabitants and cover the priced municipalities. House prices are mean municipal transaction values in current nominal euros.

Table 2: First Stage: Log House Price on the Shift-Share Instrument

| | Age-specific sample (>10k) | All municipalities |
|---|---|---|
| Instrument $S_m \times D_{t-1}$ | $-0.0157^{***}$ | $-0.0200^{***}$ |
| | (0.0012) | (0.0012) |
| First-stage $F$ | 159 | 290 |
| $N$ | 6,026 | 22,326 |

*Notes.* Dependent variable: lagged log mean municipal transaction price. The columns use the age-specific birth and crude-birth estimation samples, respectively, over 2014-2022, excluding 2020. The instrument is the interaction between the predetermined share of buildable land, $S_m$, and the first-quarter national house-price index in the lagged-price year, $D_{t-1}$. Both columns include municipality and year fixed effects. Standard errors, clustered at the municipality, are in parentheses. The reported first-stage statistic is the municipality-cluster-robust Wald $F$ for the excluded instrument. $^{***}p < 0.01$.

for the crude birth rate and −0.661 for the crude marriage rate. The results are robust to alternative treatments of zero rates and outliers (Appendix B.1).

Among municipalities above 10,000 inhabitants (Panel A), the IV birth-rate elasticity is −0.305 for women aged 20-34 and −0.196 for women aged 35-39, both statistically significant, implying that a 10 percent increase in house prices lowers birth rates by about 3.1 and 2.0 percent, respectively. The response is larger among younger women, for whom the housing constraint at the time of household formation is most likely to bind.[7]

The distinction between crude and age-specific rates matters for interpretation. Age-specific birth rates measure births per 1,000 women in the relevant age group, while crude rates use the total population and may also reflect changes in age composition. Among municipalities above 10,000 inhabitants (Panel A), the birth-rate elasticity is −0.305 for women aged 20-34 and −0.189 for the crude rate. Keeping the crude rate as the outcome, the elasticity is more negative in the full municipal sample (−0.537 in Panel B). Appendix Table B.2 shows that the crude birth response is strongest below 10,000 inhabitants (−0.71), smaller between 10,000 and 50,000 (−0.24), and statistically insignificant above 50,000. Crude marriage rates show a similar size pattern.

This heterogeneity is also important for population weighting. Our baseline gives each municipality equal weight and thus captures the response of the typical municipality. Population weighting instead places greater weight on large municipalities, where the crude birth-rate response is weaker. Accordingly, the population-weighted crude birth-rate estimate becomes positive but insignificant. The two age-specific birth elasticities and all three marriage elasticities, however, remain negative and statistically significant when weighted by population (Appendix B.3).

Finally, the estimated effects are economically sizable. Municipal house prices are approximately 62,000 and 138,000 euros at the 25th and 75th percentiles, a difference of 0.80 log points. Applying the full-sample crude-rate elasticities implies declines of about 35 percent in the crude birth rate and 41 percent in the crude marriage rate over that price difference.

[7]The smaller birth elasticity for women aged 35-39 (−0.196) is consistent with women in their late thirties being more likely to be homeowners. For owners, higher prices raise housing wealth, which may partly offset the higher cost of housing for prospective buyers (Dettling and Kearney, 2014). Our data do not allow us to test this wealth channel directly. In addition, if higher prices delay rather than prevent some births, part of the decline at younger ages can translate into births at older ages, which dampens the measured response for women aged 35-39.

Table 3: House Prices, Fertility, and Marriage: Municipal IV Elasticities

| **Outcome** | **OLS** | **IV** | **First-stage** $F$ | $N$ | **IV:** $+10\%$ **price** |
|---|---|---|---|---|---|
| *Panel A: Municipalities with more than 10,000 inhabitants* | | | | | |
| Birth rate, 20-34 | $-0.067^{***}$ | $-0.305^{***}$ | 159 | 6,026 | $-3.1\%$ |
| | (0.017) | (0.069) | | | |
| Birth rate, 35-39 | $-0.024$ | $-0.196^{**}$ | 159 | 6,026 | $-2.0\%$ |
| | (0.024) | (0.084) | | | |
| Birth rate, crude | $-0.037^{**}$ | $-0.189^{***}$ | 163 | 5,979 | $-1.9\%$ |
| | (0.015) | (0.065) | | | |
| Marriage rate, 20-34 | $-0.064^{**}$ | $-0.454^{***}$ | 159 | 6,026 | $-4.5\%$ |
| | (0.031) | (0.135) | | | |
| Marriage rate, 35-39 | $-0.171^{***}$ | $-1.052^{***}$ | 160 | 6,019 | $-10.5\%$ |
| | (0.048) | (0.175) | | | |
| Marriage rate, crude | $-0.039$ | $-0.380^{***}$ | 159 | 6,017 | $-3.8\%$ |
| | (0.026) | (0.101) | | | |
| *Panel B: All municipalities* | | | | | |
| Birth rate, crude | $-0.009$ | $-0.537^{***}$ | 290 | 22,326 | $-5.4\%$ |
| | (0.010) | (0.061) | | | |
| Marriage rate, crude | $+0.003$ | $-0.661^{***}$ | 287 | 22,289 | $-6.6\%$ |
| | (0.015) | (0.088) | | | |

*Notes.* Each row is a separate regression of the log outcome on the lagged log house price, equation (1), with municipality and year fixed effects. The IV column instruments the lagged log price with $S_m \times D_{t-1}$, as in equation (2). Coefficients are elasticities; standard errors, clustered at the municipality, are in parentheses. The first-stage $F$ uses the observations in each outcome's regression. The last column reports an approximation to the effect of a 10 percent increase in house prices ($100 \times 0.10 \times \hat{\beta}$). Panel A restricts all outcomes to municipalities with more than 10,000 inhabitants, for which age-specific rates per 1,000 women in the relevant age band are available. Panel B reports the crude rates per 1,000 inhabitants for all municipalities in the estimation sample. For the crude outcomes in both panels, zero rates are excluded because logarithms are undefined at zero; nonzero rates are retained only if they lie between the first- and 99th-percentile cutoffs computed in the full estimation panel. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## 4.5 How Much of the Fertility Decline Can House Prices Explain?

The estimated elasticity implies a sizable role for housing in Spain's recent fertility decline. To gauge its magnitude, we compute a back-of-the-envelope decomposition for the median municipality.[8]

We begin with the implied total fertility rate, using the age-specific rates available for municipalities with more than 10,000 inhabitants. Since most births occur between ages 20 and 39, we approximate the TFR as $15 \times r_{20\text{-}34} + 5 \times r_{35\text{-}39}$, where $r$ is the age-specific rate per woman and the weights, 15 and 5, are the widths in years of the two age bands. In this sample, the implied TFR falls from about 1.245 in 2014 to about 1.075 in 2022, a decline of 0.170. Over the same window and in these same municipalities, the median house price rose by about 31 percent, or 0.268 in log difference. Applying the age-specific IV elasticities in Table 3, $-0.305$ for ages 20-34 and $-0.196$ for ages 35-39, the price-driven component of the TFR change is about $-0.089$.[9] The post-2014 price increase therefore accounts for roughly 52 percent of the implied TFR decline. Absent that increase, the implied TFR in 2022 would have been about 1.16 rather than 1.075.

A decomposition based on the crude birth rate for the full sample of municipalities yields a similar share. Across these municipalities, the median house-price increase from 2014 to 2022 was about 24 percent, while the median decline in the crude birth rate was about 23 percent. Applying the crude-birth elasticity of $\beta = -0.537$, the price increase accounts for about 46 percent of the decline. Without that increase, the crude birth rate would have been about 7.4 rather than 6.6 per 1,000 inhabitants.[10] This figure is smaller both because the full sample of municipalities experienced a smaller price increase and because the crude rate confounds behavior with local age structure.

[8]Here "median municipality" refers to unweighted sample medians, not the outcomes of a single municipality. For the implied TFR calculation, each input is set to its unweighted median across municipalities in 2014 and 2022, so the resulting 'median municipality' is a synthetic benchmark rather than an observed one. The fertility approximation uses 736 municipalities with prices and both age-specific rates in 2014 and 2022: in each year, we take the median rate separately for each age band and combine them, while the price change is the log change in the yearly median prices. This descriptive comparison uses 2014 and 2022 transaction prices. The crude-rate calculation uses the median municipality-specific log changes in prices and crude birth rates among 2,303 municipalities observed in both years.

[9]The price-driven component is $15\, r_{20\text{-}34}\,(e^{-0.305\times 0.268} - 1) + 5\, r_{35\text{-}39}\,(e^{-0.196\times 0.268} - 1) \approx -0.089$, where $r_{20\text{-}34}$ and $r_{35\text{-}39}$ are the 2014 median age-specific rates per woman.

[10]The median municipality-specific log changes are 0.218 for house prices and $-0.257$ for the crude birth rate. The implied share is $(-0.537\times 0.218)/(-0.257) \approx 0.46$. The counterfactual rate is $6.6\times e^{0.537\times 0.218} \approx 7.4$ births per 1,000 inhabitants.

For perspective, Spain's observed national TFR in 2022 lay about 0.94 points below the replacement level of 2.1. The sample-based housing component estimated above, 0.08 to 0.09 TFR points, is less than one-tenth of a gap of that size. House prices therefore explain a substantial share of the recent decline but only a small part of Spain's fertility shortfall.

## 4.6 Robustness

This section assesses the robustness of our estimated elasticities along several dimensions. We present the addition of a time-varying local income control here and summarize the remaining checks below; the details are in Appendix B.

**Local income.** One concern is that the IV price elasticity partly reflects changes in local income. The exclusion restriction requires the instrument to affect births and marriages through house prices rather than through other channels, such as income levels or the labor market. We find, however, that the instrument also predicts local income: its coefficient in a regression of log income per capita with municipality and year fixed effects is 0.0030, positive and statistically significant, and about one-sixth the absolute magnitude of its negative first-stage coefficient for log house prices. To address this concern, Table 4 adds log income per capita from the *Atlas de Distribución de la Renta de Hogares* as a control. Because the Atlas covers only the 2015-2023 period, the analysis uses a shorter sample. The table also reports estimates without the income control on the same sample, allowing us to isolate the effect of including income. Conditional on house prices, higher local income is associated with higher birth and marriage rates. Adding income attenuates the price elasticities, but all six estimates remain negative and statistically significant. The crude birth elasticity changes from $-0.52$ to $-0.48$, and the crude marriage elasticity from $-0.72$ to $-0.61$. These results suggest that the estimated negative effects of house prices are not driven by differences in observed local income.

**Exclusion restrictions.** The central exclusion restriction is that a municipality's share of buildable land affects births and marriages only through house prices. The main concern is that supply-correlated geography, such as coastal location, tourism, metropolitan status, or rurality, could directly drive demographic trends. To address this concern, we replace the common year fixed effects with autonomous-community-by-year fixed effects. All six estimates remain negative, although marriage at ages 20-34 is significant only at the 10 percent level. Furthermore, Appendix B.4 interacts year indicators with a coastal indicator, munic-

Table 4: Robustness, Local Income Control (IV Analysis)

| | **Birth rate** | | | **Marriage rate** | | |
|---|---|---|---|---|---|---|
| | 20-34 | 35-39 | crude | 20-34 | 35-39 | crude |
| $\ln P_{m,t-1}$ | $-0.279^{***}$ | $-0.198^{**}$ | $-0.478^{***}$ | $-0.293^{**}$ | $-0.802^{***}$ | $-0.605^{***}$ |
| | (0.070) | (0.085) | (0.064) | (0.137) | (0.167) | (0.094) |
| ln income per head | $0.245^{**}$ | $0.251^{*}$ | $0.280^{***}$ | $0.751^{***}$ | $1.839^{***}$ | $0.743^{***}$ |
| | (0.104) | (0.129) | (0.099) | (0.176) | (0.284) | (0.155) |
| $\ln P_{m,t-1}$, no income control | $-0.320^{***}$ | $-0.239^{***}$ | $-0.521^{***}$ | $-0.417^{***}$ | $-1.103^{***}$ | $-0.722^{***}$ |
| | (0.071) | (0.087) | (0.063) | (0.139) | (0.182) | (0.094) |
| Municipality and year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| First-stage $F$ | 141 | 141 | 248 | 141 | 142 | 241 |
| $N$ | 5,184 | 5,184 | 19,319 | 5,184 | 5,179 | 19,235 |
| Adjusted $R^2$ | 0.75 | 0.51 | 0.56 | 0.54 | 0.30 | 0.35 |

*Notes.* Instrumental-variable estimates of equation (1) adding log income per head from the *Atlas de Distribución de la Renta de Hogares*. The lagged log price is instrumented by $S_m \times D_{t-1}$. Each column is one outcome. The no-income-control row re-estimates the same specification without the income control on the identical Atlas-complete sample, so the two price rows are directly comparable. The first-stage $F$ statistic includes the income control. All specifications include municipality and year fixed effects. Standard errors, clustered at the municipality, are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

ipality size, and the share of second homes, allowing these characteristics to be associated with different annual demographic paths. The estimates again remain negative, with birth at ages 35-39 the only outcome that becomes imprecise.

**Alternative Price Measures and Rents.** Our baseline measure, the mean transaction price, may reflect changes in the composition of dwellings sold rather than changes in the price of a comparable dwelling. This concern may be particularly relevant in municipalities with few transactions. Appendix B.5 examines alternative specifications that use prices per square meter, restrict the sample to municipalities with more transactions, and employ a balanced panel. The age-specific estimates remain stable, although some crude-rate elasticities are smaller. Finally, Appendix B.6 uses municipal rents instead of purchase prices. The IV estimates remain negative and statistically significant for all six birth and marriage outcomes.

**Housing market boundaries.** The relevant housing market may extend beyond municipal boundaries because young adults can respond to high prices by moving to a cheaper nearby municipality. Appendix B.7 replaces own-municipality prices with an inverse-distance-weighted average of prices in other municipalities whose centroids lie within 30 kilometers and uses the corresponding neighbor-weighted instrument. All six elasticities remain negative and statistically significant, with broadly similar magnitudes. Similarly, Appendix B.8 shows that the crude birth and marriage elasticities remain negative and statistically significant when municipalities are aggregated into agricultural *comarcas*, groups of neighboring municipalities within a province defined by the Spanish Ministry of Agriculture.[11]

**Natives vs. Immigrants.** One concern is that immigrants may have higher fertility rates and disproportionately settle in less expensive municipalities. Changes in their geographic distribution could therefore contribute to the estimated negative effect of house prices on births. To partially address this concern, Appendix B.9 constructs crude birth rates using births to Spanish-nationality mothers and the corresponding Spanish-nationality population. The IV elasticity is $-0.240$, compared with $-0.171$ for all births in the comparable sample. The negative effect therefore persists when we restrict the analysis to Spanish nationals, suggesting that changes in nationality composition, which is used here as a proxy for immigration status, do not drive our results.

[11] Age-specific comarca estimates are not reported because the municipality-level age-specific data do not cover all municipalities within a comarca.

**Alternative Instruments.** Finally, we examine the robustness of our results to alternative definitions of the shift-share instrument in Appendix B.11. We first consider three alternative measures of the buildable-land share. Our baseline measure uses CORINE land-cover data, which has a minimum mapping unit of 25 hectares and may miss small built-up areas in rural municipalities. The alternatives are: (i) a stricter definition that includes only agricultural and forest or shrub land as buildable; (ii) a Saiz-style measure that restricts buildable land to areas within five kilometers of existing urban settlements; and (iii) a measure based on the finer-resolution *SIOSE 2005* land-cover data that excludes Natura 2000, the European Union's protected-area network, from otherwise buildable land. The crude birth and marriage elasticities remain negative and statistically significant under each alternative measure (Appendix Table B.11).

We also consider alternative national housing-market shifters. Our baseline instrument uses the Spanish residential property price index, available from FRED. Appendix Table B.12 replaces this index with national housing transaction volumes. The resulting crude birth and marriage elasticities are similar to the baseline estimates. We report the real mortgage rate and the three-month Euribor only as diagnostic exercises, since borrowing costs may affect family formation directly.

## 4.7 Mechanisms

Several mechanisms may underlie the negative effect of house prices on marriage and fertility. Table 5 examines three margins through which house prices may affect family formation: dwelling space, parental coresidence, and homeownership. For each outcome, we apply the same IV design as in the baseline analysis, instrumenting the lagged log house price with $S_m \times D_{t-1}$ and including municipality and year fixed effects. Panel A reports the floor-area result.[12] The IV elasticity is $-0.162$, implying that a 10 percent supply-driven increase in house prices reduces the average size of transacted dwellings by about 1.6 percent. This shift toward smaller dwellings is consistent with a housing-space constraint that can make independent living, couple formation, and childbearing more difficult.

Descriptive evidence from the IEF Household Panel helps relate this housing-space margin to family formation. Among independent young adults, those whose household includes a child under three occupy larger dwellings than those without, with median floor areas of

[12]The outcome is the log of the municipality-year mean floor area, in square meters, of dwellings transacted, as reported in the Real Estate Registry Statistics available through BELab. It measures the size of properties sold rather than the average size of the municipal housing stock.

about 93 and 85 square meters, respectively. Independent young adults living in a couple also occupy somewhat larger dwellings than those not living in a couple, with medians of about 89 and 83 square meters, while those who are legally married occupy even larger dwellings. These comparisons are descriptive, since cadastral floor area is available for about a quarter of young adults and is more frequently observed for owners, but they show that dwelling space is closely associated with dimensions of household formation that are relevant for childbearing.

Panel B examines these household-formation margins more directly. Parental coresidence is the share of all young adults aged 20-34 who live with a parent, while homeownership is the share of independent young adults whose household owns a dwelling.[13] The IV estimates are consistent with the housing-space evidence: higher house prices increase parental coresidence and reduce homeownership.

Table 5: Local House Prices, Housing Space, and Household Formation

| **Outcome** | OLS | IV | First-stage $F$ | $N$ |
|---|---|---|---|---|
| *Panel A: Housing space* | | | | |
| Transacted floor area ($m^2$) | $-0.008$ | $-0.162^{***}$ | 285 | 21,767 |
| | (0.007) | (0.031) | | |
| *Panel B: Household formation* | | | | |
| Lives with a parent | $+0.008$ | $+0.096^{*}$ | 242 | 16,135 |
| | (0.007) | (0.050) | | |
| Homeownership among independent young adults | $+0.001$ | $-0.166^{**}$ | 254 | 15,869 |
| | (0.012) | (0.075) | | |

*Notes.* Panel A covers 2014-2022, excluding 2020, and uses the log of mean transacted floor area. Panel B covers 2016-2022, excluding 2020, and uses the coresidence and homeownership shares in levels, measured from zero to one. All regressions include municipality and year fixed effects. The IV specifications instrument the lagged log house price with $S_m \times D_{t-1}$. Standard errors are clustered at the municipality. The coresidence sample contains 2,933 municipalities; the homeownership sample contains 2,917. The IEF Household Panel survey weight is used to construct each Panel B municipality-year outcome. Individuals are assigned to their annual INE municipality of residence. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

[13]Municipality-year outcomes are constructed using the IEF Household Panel weights.

### 4.8 Summary

The municipal IV estimates indicate that higher house prices reduce birth and marriage rates. A 10 percent increase in house prices lowers the birth rates of women aged 20-34 and 35-39 by about 3 percent and 2 percent, respectively, and the corresponding marriage rates by about 5 percent and 11 percent. In the full municipal sample, the estimated declines in crude birth and marriage rates are about 5 percent and 7 percent. Our back-of-the-envelope calculations suggest that higher house prices account for about half of the recent fertility decline in the typical municipality.

The negative estimates are broadly robust to controls for local income and supply-correlated geographic trends, alternative house-price and rent measures, different definitions of local housing markets, and alternative instruments and inference procedures. Across the crude-birth-rate calculation and the baseline, income-controlled, population-weighted, and autonomous-community-by-year age-specific calculations, higher house prices account for roughly 45-52 percent of the respective 2014-2022 fertility declines.

Consistent with a household-formation channel, higher house prices shift transactions toward smaller dwellings, increase parental coresidence among young adults, and reduce homeownership among those living independently. The next section examines whether a policy that lowers housing costs for young adults leads to changes in fertility and household formation in the opposite direction.

## 5 Can a Homeownership Subsidy Raise Fertility and Household Formation?

Section 4 shows that higher house prices reduce births and marriages. This section asks the complementary policy question: can a policy that lowers housing costs for young adults increase fertility and household formation? We study a Spanish place-based homeownership subsidy introduced in 2018 for young buyers in small municipalities. The policy provides a useful setting because eligibility is determined by a sharp municipal population cutoff, applies from a fixed start date, and directly targets the housing margin most closely connected to leaving the parental home.

We exploit the 5,000-inhabitant cutoff in a municipal event-study design. The analysis follows the outcomes highlighted by the house-price estimates in Section 4, births and marriages, and then studies the channels through which the policy may operate: house prices, homeownership, parental coresidence, and migration. The estimates are reduced-form ef-

fects of municipal eligibility, rather than treatment-on-the-treated effects of actual subsidy receipt. The main finding is that births increase by about 5 percent in treated municipalities after the reform, relative to municipalities just above the cutoff, while the share of young adults living with a parent falls by about 2 percentage points. We find no evidence that the subsidy capitalizes into local house prices, nor that treated municipalities attract young in-migrants from elsewhere. Homeownership among independent young adults moves in the expected direction, but the estimate is imprecise in the main bandwidth. Taken together, the evidence points to a modest demographic response to the subsidy, operating most clearly through reduced parental coresidence.

## 5.1 Policy Background

The subsidy was introduced in early 2018 under the *Plan Estatal de Vivienda 2018-2021* (State Housing Plan 2018-2021), regulated by Royal Decree 106/2018. The stated aims were to help young people leave the parental home and to slow rural depopulation. The program grants eligible young buyers up to 10,800 euros, or 20 percent of the purchase price if that amount is smaller, toward the purchase of a primary residence. To qualify, a buyer must be under age 35, have a gross annual household income below 19,400 euros, which corresponds to three times the IPREM reference income.[14] They must also buy a home priced at no more than 100,000 euros in a municipality with fewer than 5,000 inhabitants.[15] The grant carries a five-year residence requirement, which may be shortened if the buyer moves for work or sells the dwelling and reinvests the full proceeds in another primary residence.

The income, age, price, and municipality-size conditions jointly target lower-priced home purchases by young adults in small towns. These are precisely the households for whom the down payment and the transition to independent living are likely to bind. We are, to our knowledge, the first to study the policy's effects on fertility and household formation, the demographic margins the program was designed to support.[16]

The program is administered by the autonomous communities. Like the rest of the state

[14]The *Indicador Público de Renta de Efectos Múltiples* (IPREM) is Spain's reference income indicator for determining eligibility for means-tested public assistance.

[15]The median municipal transaction price in 2017 in treated municipalities was about 70,000 euros, and 77 percent had a mean price below the 100,000-euro ceiling.

[16]The same program has been studied by Luccioletti (2024), who uses it as quasi-experimental validation for a structural model of homeownership and internal migration. Using a control group similar to ours (young residents of municipalities just above the population threshold), he finds that the subsidy raised homeownership and reduced out-migration.

housing plan, it does not apply to the Basque Country or Navarre, which have distinct fiscal and housing policy arrangements. The IEF Household Panel also does not cover those two regions, so we exclude them from all event studies. The subsidy was later renewed under the successor *Plan Estatal para el acceso a la vivienda 2022-2025*, which raised the municipality-size threshold to 10,000 inhabitants and the price ceiling to 120,000 euros. For this reason, observations after 2022 are excluded: after the expansion, municipalities just above the original 5,000 cutoff, our control group, begin to be covered by the successor policy. We retain 2022 itself, the successor plan's first year, because births recorded in 2022 reflect conceptions in 2021, before the new rules could affect behavior.

## 5.2 Design

The main sample includes municipalities with 2017 populations between 2,500 and 7,500 inhabitants, a bandwidth of 2,500 on either side of the policy's eligibility cutoff. We compare municipalities with fewer than 5,000 inhabitants (the treatment group) with those at or above the cutoff (the control group), before and after the subsidy was introduced in 2018.[17]

For outcome $Y_{mt}$ in municipality $m$ and year $t$, we estimate

$$Y_{mt} = \sum_{y \neq 2017} \beta_y \, \mathbb{1}\{t = y\} \, \text{Below5k}_{m,2017} + \theta_m + \lambda_t + \varepsilon_{mt}, \tag{3}$$

where $\text{Below5k}_{m,2017}$ equals one if municipality $m$ had fewer than 5,000 inhabitants in 2017, $\theta_m$ and $\lambda_t$ are municipality and year fixed effects, and 2017 is the omitted reference year. The coefficients $\beta_y$ trace the treated-control difference relative to 2017: the pre-2018 coefficients test for differential pre-trends, the post-2018 coefficients estimate the effect of eligibility, and standard errors are clustered at the municipality. For births and marriages, the outcome $Y_{mt}$ is $\ln(\text{events} + 1)$.[18] For house prices, $Y_{mt}$ is the log mean transaction price, and for the IEF Household Panel outcomes it is the relevant municipal share. Identification requires

[17] Fixing treatment status using the 2017 population, before the policy, matters: assigning treatment by contemporaneous population would let the policy itself, by changing who lives where, move municipalities across the cutoff and contaminate the treatment indicator. The 2017 populations are taken from the *Padrón*. Treated and control municipalities differ in several pre-policy levels, including population, income, age composition, and the crude birth and marriage rates; Appendix C.1 reports these characteristics. We check robustness to the bandwidth in Appendix C.5.

[18] INE publishes age-specific rates only for municipalities above 10,000 inhabitants, so these outcomes are unavailable in our cutoff sample. The baseline uses $\ln(\text{total events} + 1)$. Municipal birth counts are right-skewed but rarely zero (Appendix C.2), so the +1 adjustment has little effect and the birth coefficients approximate within-municipality proportional changes.

treated and control municipalities to have followed parallel trends in the absence of the policy. The pre-policy coefficients provide evidence on the plausibility of this assumption, while municipality fixed effects absorb time-invariant differences between the groups.

## 5.3 Data

No single data source covers all outcomes, so we estimate equation (3) on several municipal panels that share the same 959 municipalities in the cutoff window (630 treated and 329 control). Births and marriages come from administrative vital-statistics records that cover the universe of municipalities every year and provide a long pre-period. House prices come from registry statistics, available for 952 of these municipalities, with thinner coverage in the earliest years. Homeownership, parental coresidence, and migration are drawn from the IEF Household Panel, which covers the same 959 municipalities and records who owns a home, who lives with a parent, and where individuals move. We collapse the person-level records into municipality-year averages for young adults aged 20-34 using the IEF calibration weights.[19] The event-study regressions give equal weight to municipality-year observations. Keeping all outcomes at the municipal level gives a common design across demographic, housing, and migration outcomes.

The IEF Household Panel results carry two caveats. First, the panel is designed to be representative of Spanish households nationally, not municipality by municipality; we therefore interpret the homeownership, parental-coresidence, and migration estimates as the average evolution of young adults near the cutoff rather than as precise municipal statistics. Second, the homeownership and parental-coresidence analyses use 2016-2022. With 2017 as the reference year, these outcomes provide only one pre-policy comparison, 2016 relative to 2017, limiting our ability to assess pre-trends. Migration outcomes use a shorter window, described below.

## 5.4 Results

**Fertility and Marriage.** We begin with the demographic outcomes, modeled as ln(events+ 1) as described above.[20] We again exclude 2020 from the vital-statistics event studies. Fig-

[19]For homeownership and parental coresidence, we assign each year's observations of people observed in 2017 to their 2017 municipality, keeping treatment assignment tied to pre-policy residence when people move. Using each year's residents instead yields a similar average post-treatment decline in parental coresidence, 1.96 rather than 1.99 percentage points; homeownership remains imprecise. Migration uses annual residence.

[20]The fertility result is robust to alternative functional forms: Appendix C.2 re-estimates births and marriages as crude rates per 1,000 inhabitants and as Poisson pseudo-maximum-likelihood (PPML) count

ure 13 reports the event-study estimates. The pre-policy birth coefficients are generally negative relative to 2017 and imprecisely estimated. The positive response emerges in the later post-reform years: treated municipalities have about 5 percent more births in 2021 and 4 percent more in 2022, both significant at the 5 percent level. For comparison, the maximum subsidy of 10,800 euros is about 15 percent of the 2017 median municipal transaction price in treated municipalities. Applying the crude birth-rate elasticity for municipalities below 10,000 inhabitants ($-0.71$; Appendix Table B.2), which matches the size of the targeted municipalities, to an equivalent price reduction yields an illustrative increase of about 11 percent in the crude birth rate. This calculation is only a benchmark, for two reasons. First, the elasticity describes a market-wide price change, whereas the subsidy reaches only eligible buyers, a small fraction of residents. Since our estimates are intent-to-treat effects averaged over all residents, the implied effect per eligible household is considerably larger. Second, unlike a price fall, a purchase subsidy lowers the entry cost for buyers without reducing existing owners' housing wealth, a channel the price elasticity nets out.[21]

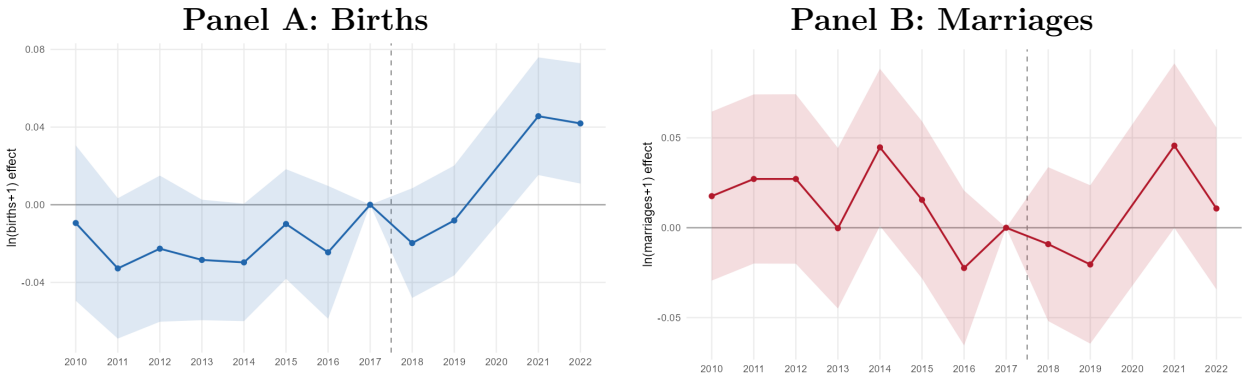
Marriages move in the same direction, with a point estimate of about 5 percent in 2021, but they are less precise and never significant. No comparable response appears at a placebo cutoff of 10,000 inhabitants, where no policy was introduced in 2018 (Appendix C.4). The birth response is positive across bandwidths around the cutoff, although the widest bandwidth shows signs of a pre-trend, which is why the main specification uses the 2,500-inhabitant bandwidth (Appendix C.5). We therefore interpret the birth response as a reduced-form increase of roughly 5 percent in eligible municipalities in 2021-2022.[22]

models with log-population offsets.

[21]Note that the elasticity concerns the crude birth rate, while our policy estimates concern birth counts. The two coincide if the policy leaves population unchanged, which is supported by the migration evidence in Section 5.4.

[22]The positive birth response is also present outside Functional Urban Areas. The average post-treatment coefficient is $+0.038^{***}$ outside Functional Urban Areas (commuting zones), $+0.029^{*}$ inside them, and $+0.002$ in the Madrid and Barcelona Functional Urban Areas; see Appendix Table C.2.

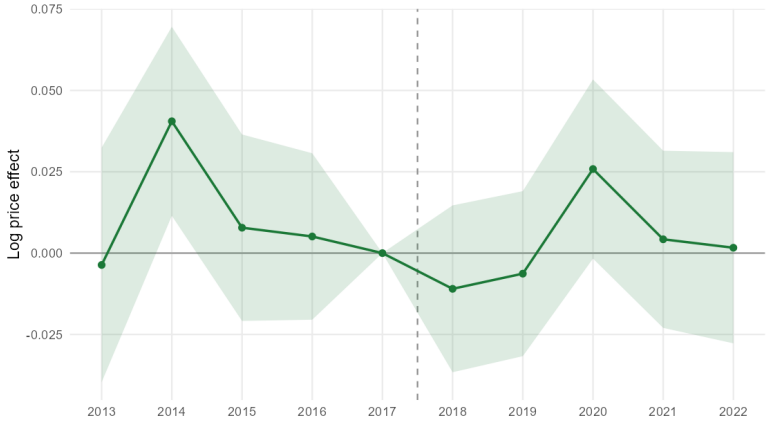
Figure 13: Fertility and Marriage at the 5,000-Inhabitant Cutoff




NOTES: Municipal event-study estimates of equation (3). Panel A reports estimates for $\ln(\text{births}+1)$; Panel B reports estimates for $\ln(\text{marriages}+1)$. The period is 2010-2022, excluding 2020; coefficients are in log points. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The sample includes municipalities with 2017 population between 2,500 and 7,500, excluding the Basque Country and Navarre. Municipality-year observations receive equal weight. The omitted year is 2017. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Shaded areas report 90 percent confidence intervals.

**House Prices.** A natural concern is that a demand-side buyer subsidy might capitalize into local house prices, transferring part of the grant to sellers rather than lowering the effective cost of homeownership for buyers. Figure 14 addresses this concern by presenting the event study of the log-mean municipal transaction price. The pre-trend is flat, and the post-2018 coefficients are small and statistically insignificant. We therefore find no evidence of price capitalization at the municipal level. This pattern is consistent with the subsidy improving affordability for eligible buyers rather than being fully absorbed by sellers.

Figure 14: House Prices at the 5,000-Inhabitant Cutoff




NOTES: Municipal event-study estimates of equation (3). The outcome is the log of the mean municipal transaction price from the Real Estate Registry Statistics. Coefficients are in log points. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The sample includes municipalities with 2017 population between 2,500 and 7,500, excluding the Basque Country and Navarre. Municipality-year observations receive equal weight. The omitted year is 2017. The event-study window is 2013-2022; earlier years are omitted because too few municipalities have transaction-price data before 2013. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Shaded areas report 90 percent confidence intervals.

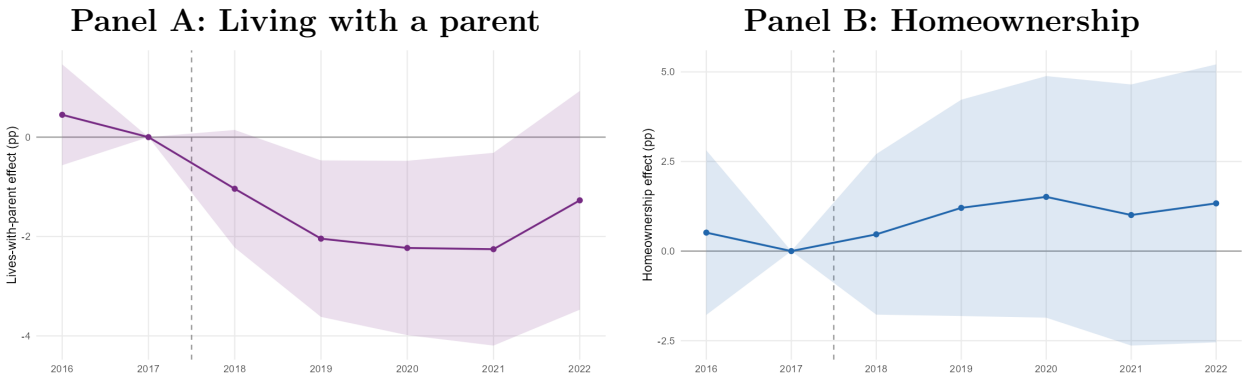
**Homeownership and Emancipation.** We next examine whether the subsidy helps young adults leave the parental home and acquire a dwelling. Using the IEF Household Panel, we construct annual municipality-level averages for adults aged 20-34, subject to the data limitations discussed above.[23] Panel A of Figure 15 reports the effects on the share living with a parent, so negative coefficients indicate greater independent living. Relative to control municipalities, this share falls by about 2 percentage points in 2019-2021. The 2016 pre-policy coefficient is close to zero. The 2019 and 2020 estimates are significant at the 5 percent level, and the 2021 estimate at the 10 percent level. When focusing only on young adults born in Spain, the average post-treatment decline is 1.6 percentage points (Appendix C.6).

Panel B reports the share of independent young adults whose household owns a dwelling, measured from cadastral records. Although homeownership is the outcome most directly targeted by the subsidy, the evidence is less conclusive. The estimates indicate an increase of about 1 percentage point in both 2021 and 2022, but neither is statistically significant within the main bandwidth of 2,500 inhabitants on either side of the cutoff. The estimates become statistically significant at wider bandwidths (Appendix C.5), which expand the sample but

[23]The Household Panel outcomes include 2020; the exclusion from the birth and marriage event studies concerns disrupted vital-event registration.

compare less similar municipalities. We therefore interpret the homeownership evidence as suggestive; the clearest household-formation response is the decline in parental coresidence.

Figure 15: Homeownership and Emancipation at the 5,000-Inhabitant Cutoff




NOTES: Municipal event-study estimates of equation (3) using the IEF Household Panel. Panel A reports the share of young adults aged 20-34 living with a parent. Panel B reports the share of independent young adults aged 20-34 whose household owns any residential dwelling, not necessarily the one it occupies; ownership is measured using cadastral records and excludes garages, parking spaces, storage rooms, and land. The period is 2016-2022; coefficients are in percentage points. IEF calibration weights construct the municipality-year shares; the regressions give equal weight to municipality-year observations. Individuals observed in 2017 remain assigned to their 2017 municipality throughout. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The sample includes municipalities with 2017 population between 2,500 and 7,500, excluding the Basque Country and Navarre. The omitted year is 2017. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Shaded areas report 90 percent confidence intervals.

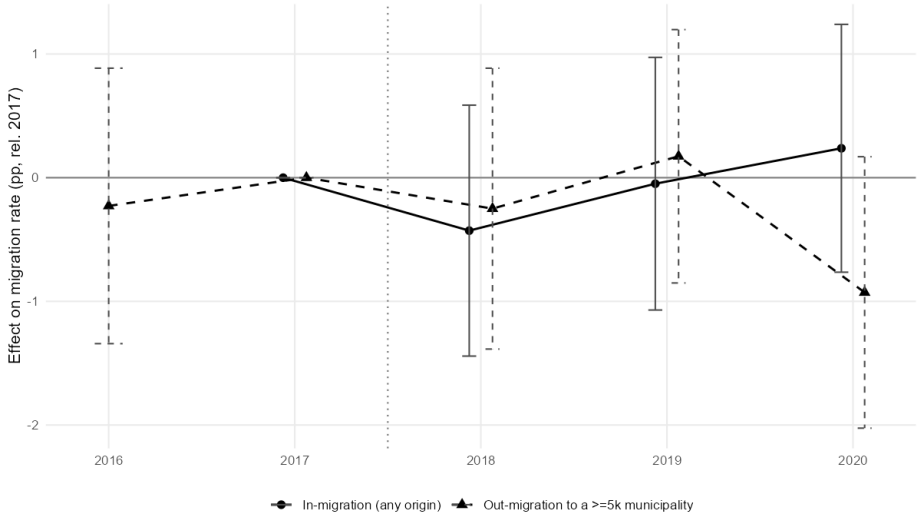
**Migration.** Slowing rural depopulation was one of the program's stated aims. The subsidy could support this objective by retaining young residents who would otherwise leave or by attracting young adults from other municipalities. We examine these two margins separately.

Using the IEF Household Panel, we compare treated and control municipalities on two migration flows among young adults aged 20-34. Out-migration measures departures to other municipalities with at least 5,000 inhabitants, which were ineligible under the original program. It records moves between the current year and the following year. In-migration measures arrivals from another municipality between the previous year and the current year. The event studies use 2017 as the reference year and report coefficients through 2020.

Figure 16 shows that the out-migration estimate is negative by 2020, indicating a decline of about 1 percentage point relative to the control-group trajectory, compared with a baseline rate of roughly 4.5 percent per year. The direction is consistent with the program's objective of retaining young residents, but the estimate is imprecise.

The in-migration estimates are small and statistically insignificant. We find no detectable increase in arrivals into treated municipalities over the observed period. This weighs against increased in-migration as an explanation for the rise in births but does not rule out changes in the composition of migrants or migration responses after 2020. The migration evidence, therefore, does not establish that the birth response arises entirely from changes in the behavior of incumbent residents.

Figure 16: Migration of Young Adults at the 5,000-Inhabitant Cutoff




NOTES: Municipal event-study estimates of equation (3) using the IEF Household Panel. We measure migration among adults aged 20-34 living in each municipality. The out-migration rate is the share who live the following year in a different municipality whose 2017 population was at least 5,000. The in-migration rate is the share who lived in a different municipality the previous year. For each rate, we include only people observed in both years and use IEF calibration weights. The regressions give equal weight to municipality-year observations. The figure ends in 2020: its final point uses the 2020-2021 transition for out-migration and the 2019-2020 transition for in-migration. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The sample includes municipalities with 2017 population between 2,500 and 7,500, excluding the Basque Country and Navarre. The omitted year is 2017. All specifications include municipality and year fixed effects. Coefficients are in percentage points. Standard errors are clustered at the municipality. Error bars show 90 percent confidence intervals.

Taken together, the policy evidence complements the house-price estimates in Section 4. Eligibility for the homeownership subsidy raises births by about 5 percent and reduces parental coresidence among young adults by about 2 percentage points. Homeownership among independent young adults moves in the expected direction but remains imprecisely estimated. The migration estimates suggest lower out-migration and show no detectable increase in in-migration through 2020, but neither effect is statistically significant. Overall,

the findings are consistent with the idea that housing affordability helps young adults leave the parental home and form families.

# 6 Conclusions

Spain combines very low fertility, late parenthood, delayed emancipation, and severe housing affordability pressures. This paper studies whether these facts are connected. We argue that housing costs can affect fertility not only through the direct cost of raising children, but also by delaying the earlier transitions that make childbearing more likely: leaving the parental home, forming a couple, marrying, and establishing an independent household.

We document three sets of results. First, young adults spend longer in education, their personal income has declined relative to that of adults aged 35-54, and independent living has become less common. Among households headed by young adults, the share of couples has fallen. This change in household composition accounts for part of the decline in the share reporting a recent birth. Homeownership among young adults living independently has also fallen, while renting has become more common.

Second, we estimate the effect of local house prices on births and marriages using municipality-level variation across Spain. A simple comparison across places is misleading: high-price municipalities are also high-income and high-amenity places that attract young households. We therefore use a shift-share instrument that interacts national house-price movements with predetermined local housing-supply constraints. The IV estimates show that higher house prices reduce both fertility and marriage. For women aged 20-34, a 10 percent increase in local house prices lowers the birth rate by about 3 percent and the marriage rate by about 5 percent, with the crude rates moving by somewhat more and the marriage response especially large at ages 35-39. The same price increases also shift transactions toward smaller dwellings, suggesting that housing costs constrain the space margin relevant for couple formation and childbearing. A back-of-the-envelope calculation attributes roughly half of the 2014-2022 decline in our implied TFR measure for the typical municipality to higher house prices.

Third, we study a policy that shifted housing costs in the opposite direction: the 2018 homeownership subsidy for young buyers in municipalities with fewer than 5,000 inhabitants. Using the population cutoff in an event-study design, we find that eligibility raises births by about 5 percent in 2021-2022 and reduces parental coresidence by about 2 percentage points in 2019-2021. We find no detectable capitalization in mean municipal transaction prices or increase in young-adult in-migration through 2020. Homeownership among independent

young adults moves in the expected direction but remains imprecisely estimated.

Taken together, the results suggest that housing affordability matters for family-formation policy. Policies aimed at fertility often focus on childcare, parental leave, or direct transfers after children are born. Those policies are important, but our evidence shows that constraints before parenthood also matter. In a setting like Spain, where many young adults remain in the parental home well into the ages at which first births usually occur, the cost of forming an independent household can be a first-order barrier to fertility.

The results also point to several limitations and directions for future work. The municipality-level analysis estimates effects on births and marriages, not directly on individual fertility intentions or completed fertility. The policy evaluation is a reduced-form effect of eligibility, not a treatment-on-the-treated effect of actual subsidy receipt. The subsidy analysis is also short-run and focused on small municipalities near the cutoff. Finally, although the Household Panel allows us to study emancipation, homeownership, and migration, it does not yet allow us to follow individual fertility responses over a long horizon.

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# A Data

This appendix describes the data sources in the order in which they are introduced in Section 2. We begin with the household surveys and the Human Fertility Database, which provide the descriptive evidence in Section 3. We then describe the municipal administrative records, housing-market data, local income data, and geographic and land-cover information used to study the effects of house prices on births and on marriages in Section 4. Finally, we describe the Household Panel used for the individual-level outcomes and the policy evaluation in Section 5.

## A.1 Labor Force Survey

The Spanish Labor Force Survey (LFS, *Encuesta de Población Activa*, EPA) is conducted quarterly by the Spanish National Statistics Institute (INE). The EPA is a large household survey representative of the Spanish resident population. Each household is interviewed for six consecutive quarters, which gives the survey a rotating-panel structure. In this paper, we use the repeated cross-sectional dimension of the EPA for the period 2005-2024.

The EPA provides detailed labor-market information for all household members aged 16 and older, including employment status, type of contract, part-time or full-time status, self-employment, and tenure in the current firm. It also contains information on household composition and each member's relationship to the household reference person. We use these household links to identify whether an individual lives independently of the parental household. The EPA allows us to document how education, employment, contract type, job tenure, and emancipation have evolved for young adults over the last two decades. For the descriptive analysis in Section 3, we focus on native-born individuals aged 20-34.

## A.2 Survey of Income and Living Conditions

We supplement the Labor Force Survey with the Spanish Survey of Income and Living Conditions (*Encuesta de Condiciones de Vida*, ECV), an annual household survey conducted by INE. The ECV constitutes Spain's component of the European Union Statistics on Income and Living Conditions (EU-SILC). We use the 2004-2023 waves. The ECV collects information at both the household and individual levels. The household register covers all household members, while the personal questionnaire, completed by adults aged 16 and older, records labor-market status, educational attainment, and income by source, including wages, self-employment income, unemployment benefits, and other transfers. The household file also

provides information on tenure status, housing costs, and dwelling characteristics.

Relative to the EPA, the ECV is better suited to analyzing the financial dimension of housing and household resources. We use the ECV to document young adults' personal income relative to that of individuals aged 35-54, changes in housing tenure, the prevalence of free accommodation, and housing costs relative to household disposable income. Personal income is measured among individuals reporting positive income, including unemployment benefits.

## A.3 Human Fertility Database

We use the Human Fertility Database (HFD) to place Spain's fertility decline in international and historical perspective. The HFD is an open-access demographic database produced by the Max Planck Institute for Demographic Research and the Vienna Institute of Demography. It provides harmonized fertility data for developed countries, based on official vital statistics and constructed using a common methodology to improve comparability across countries and over time.

In Section 3.1, we use HFD summary indicators for Spain, Portugal, the Netherlands, Sweden, and the United States. The period indicators include the total fertility rate and the mean age at first birth. The cohort indicators include completed cohort fertility, cohort childlessness, and the distribution of women by parity (number of children). These series are used only for the descriptive cross-country comparison in Figures 1 and 2; they are not used in the municipal house-price analysis or in the policy evaluation.

## A.4 INE Vital Statistics and the Padrón

The birth and marriage counts underlying the outcomes in Sections 4 and 5 come from the vital statistics (*Movimiento Natural de la Población*) of the Spanish Statistical Institute (INE). These records cover the universe of births and marriages registered in Spain and tabulate them by year and by the mother's municipality of residence for births and the couple's municipality of residence for marriages.

We use municipal population counts from the *Padrón Continuo*, the continuously updated municipal population register produced by INE, to calculate crude birth and marriage rates per 1,000 inhabitants.

INE also publishes births and marriages by age band of the mother or spouses, but only for municipalities with more than 10,000 inhabitants. For those municipalities, we construct age-specific birth and marriage rates for ages 20-34 and 35-39. These age-specific rates are

used in the municipal house-price analysis in Section 4. Because every municipality in the policy window of Section 5 has fewer than 10,000 inhabitants, the age-specific rates are unavailable for the policy analysis; in that case, the municipal demographic outcomes are total counts. Table A.1 defines the variables used in the analysis. We exclude the Basque Country and Navarre from the Household-Panel and policy analyses (Section 5), where the Panel does not cover them and the subsidy does not apply; they remain in the municipal price and vital-statistics data used in Section 4.

Table A.1: INE Vital Statistics and Population Variables

| **Variable** | **Definition and source** |
|---|---|
| Births | Live births in the year to mothers residing in the municipality, from INE vital statistics (*Movimiento Natural de la Población*). This is the main fertility outcome in the policy analysis. |
| Marriages | Marriages in the year assigned to the couple's municipality of residence, from INE vital statistics. |
| Crude birth rate | Births per 1,000 inhabitants. |
| Crude marriage rate | Marriages per 1,000 inhabitants. |
| Birth rate, 20-34/35-39 | Births to women in the age band per 1,000 women in that age band; published only for municipalities above 10,000 inhabitants and used in Section 4. |
| Marriage rate, 20-34/35-39 | Marriages involving women in the age band per 1,000 women in that age band; published only for municipalities above 10,000 inhabitants and used in Section 4. |
| Population | Total municipal population from the *Padrón Continuo* (INE), used to calculate crude rates; its logarithm enters as an offset in the Poisson (PPML) models. |

*Notes.* Our vital-statistics and population extract covers every Spanish municipality annually from 2010. Mean transaction prices and the buildable-land instrument are described in Sections A.5 and A.7. The Basque Country and Navarre are excluded from any analysis that uses the Household Panel.

The estimation windows differ across analyses according to data availability. The municipal house-price analysis in Section 4 uses outcomes for 2014-2022, excluding 2020, matched to house prices lagged by one year. Coverage of usable lagged prices in the municipal IV panel rises sharply by 2014. We exclude 2020 because of the pandemic-related disruptions discussed in Section 4. The policy analysis in Section 5 uses 2010-2022, excluding 2020, for

the vital-statistics outcomes and 2016-2022 for the Household Panel outcomes, which are first observed in 2016.

## A.5 House Prices

Our main house-price measure is the municipal mean transaction price from the Real Estate Registry Statistics, available through the Banco de España Data Laboratory (BELab). These data are produced by the Spanish Association of Property and Commercial Registrars and contain information on real-estate transactions, mortgages, and certifications. For the municipal analysis, we use the mean transaction price and the mean floor area, in square meters, of dwellings transacted in each municipality and year. The latter is the basis for the dwelling-size analysis in Section 4. The municipal price series are available from 2011 onward. Coverage in the merged IV panel is limited before 2014. The series provide the local house-price variation used in the municipal analysis in Section 4 and the policy analysis in Section 5.

Table A.2 reports the number of municipalities in the merged IV panel with a positive current-year mean transaction price. This count rises from 186 in 2011 to 801 in 2013, 2,417 in 2014, and about 3,000 by 2022. The IV panel also requires the previous year's price and the baseline supply and homeownership measures. Its greater coverage from 2014 onward supports the 2014-2022 estimation window in Section 4.

Table A.2: Municipal Price Coverage in the IV Panel by Year

| Year | Municipalities with current-year price |
|---|---|
| 2011 | 186 |
| 2012 | 752 |
| 2013 | 801 |
| 2014 | 2,417 |
| 2015 | 2,582 |
| 2016 | 2,662 |
| 2017 | 2,728 |
| 2018 | 2,817 |
| 2019 | 2,865 |
| 2020 | 2,880 |
| 2021 | 2,931 |
| 2022 | 2,997 |
| 2023 | 3,052 |
| 2024 | 3,077 |

*Notes.* Counts are municipalities with a positive current-year mean transaction price in the merged municipal IV panel. The panel also requires a previous-year price, the national house-price index, baseline homeownership, and the buildable-land share. The counts precede outcome-specific restrictions. INE vital statistics cover roughly 8,100 municipalities each year.

## A.6 Atlas de Distribución de la Renta de Hogares

We use the *Atlas de Distribución de la Renta de Hogares* (ADRH), produced jointly by the Spanish National Statistics Institute (INE) and the Tax Agency (AEAT), to measure local income and demographic composition. The ADRH is an administrative dataset that combines tax records, social security files, and population registers to produce annual estimates of household income at fine geographic resolution.

Our working extract covers 2015-2023 and is organized as a yearly municipality-level panel. It contains 8,139 municipalities and 73,251 municipality-year observations.[24] The income module provides per-capita and per-household measures of gross and net income. In the empirical analysis, we use net income per capita as the control for local purchasing power in Table 4. The ADRH also provides population counts, age composition, average household

[24]The dataset retains historical municipality codes, including municipalities that no longer exist following administrative reorganizations.

size, and the share of single-person households. These variables are not included as controls in the baseline specifications; the local control used in Table 4 is net income per capita.

We link the ADRH data to the municipality-level panel by municipality code and year. The resulting matched dataset combines house prices, fertility and marriage outcomes, and the local income control. It is used for the specification in Section 4 that adds a time-varying local income control to the instrumental-variable estimates.

## A.7 CORINE Land Cover and the Buildable-Land Share

The instrument in Section 4 interacts the national house-price index in the first quarter of the municipal price year with a predetermined measure of each municipality's room to expand its housing stock. We denote this measure by $S_m$. It captures land availability using land cover around the year 2000 and topographic constraints. Following Saiz (2010) and Basco et al. (2025), we use geographic constraints to proxy for local housing-supply elasticity. The key idea is that a common national housing-demand shock should raise prices more in municipalities with scarce developable land than in those where housing supply can expand more easily.

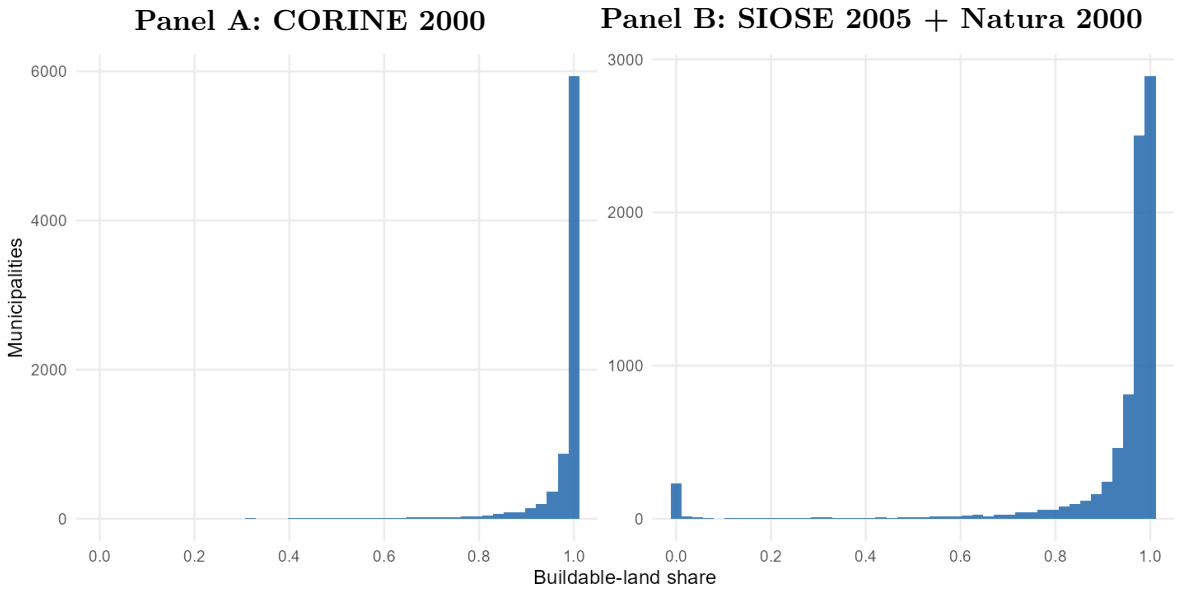
Land cover comes from the *CORINE Land Cover 2000* dataset of the Copernicus Land Monitoring Service, a pan-European inventory of land-cover classes at 100-meter resolution. We use the 2000 edition because it predates our 2014-2022 estimation period, providing a measure of land availability that does not reflect housing development in response to the price and fertility changes we study. We classify land as buildable if it is not already built and is not classified as water or wetland; in practice, this excludes artificial-surface classes, water bodies, and wetlands. We then add a terrain constraint using the Spanish National Geographic Institute's digital elevation model at 200-meter resolution (*Modelo Digital del Terreno*, MDT200), which is based on bare-earth elevation. We drop land with a slope above fifteen degrees, which is too steep to develop at reasonable cost. The buildable share in municipality $m$, $S_m$, is then defined as the buildable area divided by the potentially developable area, that is, built-up plus buildable land.

The coarse resolution of CORINE is relevant for rural Spain. Its 25-hectare minimum mapping unit can miss the small built-up patches typical of small villages and may therefore record little or no built land in some municipalities. We therefore construct several alternative versions of $S_m$ and show in Appendix B.11 that the estimated elasticities are robust to these choices. The alternatives include a stricter definition that counts only agricultural and forest or shrub cover as buildable; a Saiz-style measure that counts buildable land only within

a five-kilometer buffer of existing urban patches; and a measure based on the finer-grained *SIOSE 2005* land cover, whose one-hectare minimum mapping unit captures small villages more accurately. SIOSE records the fraction of each mapped area covered by buildings or pavement, which we classify as built-up. The buildable area excludes this built-up portion, water, wetlands, and Natura 2000 land that would otherwise qualify. It does not apply the CORINE terrain-slope restriction.

Figure A.1 shows the distribution of the buildable share across municipalities under two land-cover sources. Both distributions have a large mass near one: many municipalities can, in principle, develop a large fraction of their potentially developable land, while a tail of mountainous and coastal municipalities is much more constrained. The two sources differ in how sharply they pile up at one. The coarse CORINE map has a 25-hectare minimum mapping unit. Under the CORINE-based measure, about 72 percent of municipalities have a buildable-land share of at least 0.99. This means that almost all of the area classified as built-up or buildable is classified as buildable. The finer SIOSE 2005 map, with Natura 2000 land excluded from otherwise buildable area, is less saturated: about 31 percent of municipalities have a share of at least 0.99. In the baseline analysis, the CORINE share is interacted with the common national price shock to construct the instrument used in Section 4.

Figure A.1: Distribution of the Buildable-Land Share




NOTES: The figure shows the distribution of the buildable-land share across municipalities with mapped values. This share is the supply component of the shift-share instrument and is defined as buildable land divided by built-up plus buildable land. The left panel uses CORINE Land Cover 2000, excluding built-up land, water, wetlands, and land with a slope above fifteen degrees from the buildable area. The right panel uses the finer SIOSE 2005 measure, which excludes buildings and pavement, water, wetlands, and Natura 2000 protected land without a separate slope restriction. Overlapping exclusions count once. The CORINE measure has a larger mass at one. The vertical-axis scales differ across panels.

## A.8 Household Panel

For the individual-level analysis and for the homeownership, emancipation, and migration outcomes in the policy evaluation of Section 5, we use the *Panel de Hogares* (Household Panel) of the Spanish Institute of Fiscal Studies (*Instituto de Estudios Fiscales*, IEF). The Household Panel is a longitudinal administrative panel of a representative sample of Spanish households, observed annually from 2016 to 2023. The panel links administrative records from two main institutions. The National Statistics Institute (*Instituto Nacional de Estadística*, INE) provides population records, household composition, and annual municipality of residence. The State Tax Administration Agency (*Agencia Estatal de Administración Tributaria*, AEAT) provides information on income, taxes, and cadastral property.

The Household Panel jointly identifies municipality of residence, household composition, and cadastral ownership. This allows us to study homeownership, emancipation, and internal migration within a municipal design. Because the EPA and ECV do not release the

municipality of residence, they cannot support this design even where they measure tenure or living arrangements. For the policy analysis, we aggregate person-level records into annual municipality-level averages for the relevant young population. For the homeownership and parental-coresidence outcomes, we follow individuals observed in 2017 and assign their observations to their 2017 municipality. Migration uses the annual municipality. The panel does not cover the Basque Country or Navarre, which maintain separate tax systems.

We measure homeownership from cadastral records rather than from self-reported tenure. A household is coded as owning a dwelling if any of its members is the registered economic owner (the *titularidad económica*, the beneficial ownership recorded for tax purposes, as distinct from bare legal title) of at least one residential dwelling. The cadaster records garages, parking spaces, storage rooms, and land as separate properties. We therefore keep only units classified as dwellings, so that owning a lock-up garage or a parking space does not count as homeownership. This household-level cadastral measure is the homeownership variable used in Sections 4 and 5. Because it is built from property registers, it records whether the household owns any dwelling, not whether it owns the specific home it occupies; we therefore interpret it as a measure of property ownership rather than of owner-occupancy or residential tenure.

Emancipation is defined from household composition: a young person is classified as emancipated if he or she no longer coresides with a parent. Migration is measured from changes in the municipality of residence between consecutive years. Two limitations are worth noting. First, the Household Panel does not contain educational attainment. Second, it reports total household liabilities, but does not separately identify mortgage debt, so mortgage leverage cannot be measured directly. Table A.3 summarizes the key variables.

We use the IEF household calibration weights when constructing annual municipality-level averages. The panel is not designed to provide representative estimates for each municipality, so these weighted averages should not be interpreted as exact municipal population statistics.

Table A.3: Household Panel: Key Variables

| **Variable** | **Definition and source** |
|---|---|
| Owns a dwelling (household) | One if any member of the person's household is the registered economic owner of at least one residential dwelling in the cadaster, with garages, parking spaces, storage rooms, and land excluded (IEF/AEAT cadastral records). Reported among independent young adults when used in the homeownership analysis. |
| Independent/emancipated | One if the person does not coreside with a parent (household composition, INE). |
| Lives with a parent | One if the person coresides with a parent; the complement of emancipation. |
| In-migration | One if the young person lived in a different municipality in the previous year, that is, moved into the current municipality from another municipality. |
| Out-migration ($\geq$5,000) | One if the young person moves in the following year to a different municipality with 5,000 or more inhabitants. |
| Municipality of residence | Census municipality code of the household (INE), available in the Household Panel but not in the EPA or EU-SILC. |
| Household income | Gross and net household income from tax records (AEAT). |
| Calibration weight | IEF household calibration weight, applied to every rate and share computed from the panel. |

*Notes.* The panel covers 2016-2023 and excludes the Basque Country and Navarre. The policy analysis in Section 5 focuses on young adults aged 20-34 and uses the 2016-2022 window. The panel records only total household liabilities, so mortgage debt cannot be separated from other debt, and it contains no measure of educational attainment.

# B Additional Results: House Prices, Fertility, and Marriage

This appendix reports additional results and robustness checks for the municipal house-price analysis in Section 4. Appendices B.1, B.2, and B.3 examine the treatment of zero rates, heterogeneity by municipality size, and population weighting. Appendix B.4 considers autonomous-community-by-year fixed effects and supply-correlated geographic trends; Appendices B.5 and B.6 examine alternative measures of house prices and rents. Appendices B.7 and B.8 alter the geographic definition of the local housing market, and Appendix B.9 restricts the crude birth rate to Spanish nationals. Finally, Appendices B.10 and B.11 examine two-way clustered inference and alternative definitions of the buildable-land share and national housing-demand shifters.

## B.1 Zero Rates and Functional Form

Among the 22,847 municipality-years entering the Section 4 price panel, 47, or 0.21 percent, have a zero crude birth rate and 259, or 1.13 percent, have a zero crude marriage rate. The baseline dependent variable is ln(rate), so zero rates must be excluded. We separately apply first- and 99th-percentile cutoffs computed from the full price panel. For births, these cutoffs are 1.695 and 14.846 births per 1,000. Because the lower cutoff is positive, all zero birth rates are already excluded by the percentile rule, and the separate zero restriction removes no additional birth observations. For marriages, the cutoffs are 0 and 7.773 marriages per 1,000. The first percentile equals zero because zero marriage rates account for more than 1 percent of the sample. The percentile rule would therefore retain observations equal to zero, and the separate zero restriction is required for the log specification. Table B.1 first adds zero observations back using ln(1 + rate), while retaining the same fixed cutoffs for positive rates. The final column uses the same transformation but removes the percentile restrictions, thereby retaining all non-missing crude-rate observations, including zeros. The estimates remain negative and statistically significant in both specifications. The number of observations increases only modestly in the untrimmed specification because municipal house-price coverage, rather than zero or extreme crude rates, is the main constraint on the estimation sample.

Table B.1: Robustness to Zero Crude Rates and Tail Trimming

| | ln(rate) | ln(1 + rate) | |
|---|---|---|---|
| **Outcome** | Baseline | Fixed cutoffs | No trimming |
| Crude birth rate | $-0.537^{***}$ | $-0.513^{***}$ | $-0.494^{***}$ |
| | (0.061) | (0.055) | (0.057) |
| $N$ | 22,326 | 22,377 | 22,791 |
| Crude marriage rate | $-0.661^{***}$ | $-0.529^{***}$ | $-0.543^{***}$ |
| | (0.088) | (0.066) | (0.067) |
| $N$ | 22,289 | 22,563 | 22,791 |

*Notes.* Each cell reports an IV coefficient from a specification with municipality and year fixed effects and standard errors clustered at the municipality. The lagged log house price is instrumented with $S_m \times D_{t-1}$. The baseline excludes zero rates and restricts positive rates to the fixed first- and 99th-percentile cutoffs. The second column adds zero observations using ln(1 + rate), while retaining the same cutoffs for positive rates. The final column removes the percentile restrictions and includes all non-missing crude rates, including zeros. Coefficients in the first column are elasticities. Coefficients in the second and third columns are semi-elasticities and are not directly comparable in magnitude. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.2 Heterogeneity by Municipality Size

Table B.2 divides municipalities by their baseline population and reports the equal-municipality IV estimates. The crude birth and marriage responses are largest below 10,000 inhabitants and decline with municipality size. Above 50,000 inhabitants, the crude estimates are close to zero, while the birth rate for ages 20-34 and the marriage rate for ages 35-39 remain negative.

Table B.2: Heterogeneity by Baseline Municipality Size: Equal-Municipality IV

| **Outcome** | $< 10{,}000$ | 10,000–50,000 | $> 50{,}000$ |
|---|---|---|---|
| Birth rate, 20-34 | — | $-0.34^{***}$ | $-0.23^{*}$ |
| | | (0.089) | (0.121) |
| Birth rate, 35-39 | — | $-0.18$ | $-0.14$ |
| | | (0.110) | (0.129) |
| Birth rate, crude | $-0.71^{***}$ | $-0.24^{***}$ | $+0.00$ |
| | (0.148) | (0.082) | (0.121) |
| Marriage rate, 20-34 | — | $-0.51^{***}$ | $-0.03$ |
| | | (0.173) | (0.283) |
| Marriage rate, 35-39 | — | $-1.03^{***}$ | $-0.93^{***}$ |
| | | (0.232) | (0.251) |
| Marriage rate, crude | $-0.83^{***}$ | $-0.41^{***}$ | $-0.21$ |
| | (0.203) | (0.125) | (0.210) |

*Notes.* Each cell reports the IV elasticity of the indicated rate with respect to the lagged log house price. The instrument is $S_m \times D_{t-1}$, and all regressions include municipality and year fixed effects and give each municipality-year equal weight. Municipality size is measured using population in the first observed year of the Section 4 panel. Age-specific rates are unavailable below 10,000 inhabitants. For the crude-birth specifications, the first-stage $F$ statistics are approximately 62, 118, and 38 across the three bins. Municipality-clustered standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.3 Population Weighting

The headline estimates weight each municipality equally. Population weighting instead concentrates weight on a few large cities; in our sample the largest 5 percent of municipalities house more than half the population, and the single largest houses about 7 percent. Table B.3 reports both estimands. The age-specific elasticities remain negative and significant when population-weighted. The crude birth elasticity changes from $-0.537^{***}$ to $+0.013$ with a standard error of 0.077, making it statistically indistinguishable from zero.

Table B.3: Robustness to Population Weighting

| **Outcome** | **Unweighted** | **Population-weighted** |
|---|---|---|
| Birth rate, 20-34 | $-0.305^{***}$ | $-0.271^{***}$ |
| | (0.069) | (0.082) |
| Birth rate, 35-39 | $-0.196^{**}$ | $-0.227^{**}$ |
| | (0.084) | (0.104) |
| Birth rate, crude | $-0.537^{***}$ | $+0.013$ |
| | (0.061) | (0.077) |
| Marriage rate, 20-34 | $-0.454^{***}$ | $-0.708^{*}$ |
| | (0.135) | (0.365) |
| Marriage rate, 35-39 | $-1.052^{***}$ | $-1.396^{***}$ |
| | (0.175) | (0.450) |
| Marriage rate, crude | $-0.661^{***}$ | $-0.621^{**}$ |
| | (0.088) | (0.242) |

*Notes.* IV elasticities from equation (1). The unweighted column reproduces the headline estimates from Table 3. The population-weighted column weights observations by municipal population from the *Padrón Continuo*. The age-specific rows use municipalities with more than 10,000 inhabitants. The weighted first stage is strong, with first-stage $F \approx 43$ for the crude-rate specifications. Standard errors, clustered at the municipality, are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.4 Regional Fixed Effects and Geographic Trends

The identifying assumption in Section 4 requires that, absent house-price changes, municipalities with more and less buildable land would have followed parallel demographic trends. Table B.4 reports two checks. Column (2) replaces the common year fixed effects with autonomous-community-by-year fixed effects, absorbing policies and shocks shared by municipalities within the same autonomous community and year. All six estimates remain negative, although marriage at ages 20-34 is significant only at the 10 percent level. The first-stage $F$ statistic in the crude-birth sample is 147.

Column (3) interacts year indicators with three characteristics that may be correlated with buildable land: an indicator for a coastline within 25 kilometers, the municipality's mean log population over the estimation window, and the share of second homes in the 2011 Population and Housing Census. These controls allow coastal, larger, and second-home-intensive municipalities to follow different annual paths. The age-specific samples are unchanged. Requiring non-missing geographic characteristics reduces the crude-birth and

crude-marriage samples to 17,476 and 17,545 municipality-years. On these matched samples, the baseline crude elasticities are $-0.483$ for births and $-0.598$ for marriages; adding the geographic trends changes them to $-0.259^{***}$ and $-0.485^{***}$. The other age-specific estimates remain negative and statistically significant, with birth at ages 35-39 the only exception. In the matched crude-birth sample, the first-stage $F$ statistic falls from 289 to 130 but remains strong.

Table B.4: Robustness to Regional Fixed Effects and Geographic Trends

| | (1) | (2) | (3) |
|---|---|---|---|
| **Outcome** | Headline | Community×year | Geographic trends |
| Birth rate, 20-34 | $-0.305^{***}$ | $-0.260^{**}$ | $-0.364^{***}$ |
| | (0.069) | (0.110) | (0.099) |
| Birth rate, 35-39 | $-0.196^{**}$ | $-0.270^{**}$ | $-0.151$ |
| | (0.084) | (0.132) | (0.115) |
| Birth rate, crude | $-0.537^{***}$ | $-0.541^{***}$ | $-0.259^{***}$ |
| | (0.061) | (0.095) | (0.084) |
| Marriage rate, 20-34 | $-0.454^{***}$ | $-0.353^{*}$ | $-0.366^{*}$ |
| | (0.135) | (0.203) | (0.192) |
| Marriage rate, 35-39 | $-1.052^{***}$ | $-0.516^{**}$ | $-1.042^{***}$ |
| | (0.175) | (0.239) | (0.244) |
| Marriage rate, crude | $-0.661^{***}$ | $-0.717^{***}$ | $-0.485^{***}$ |
| | (0.088) | (0.140) | (0.129) |
| First-stage $F$ (crude sample) | 290 | 147 | 130 |

*Notes.* Each cell reports an IV elasticity. Column (1) includes municipality and year fixed effects. Column (2) replaces the common year effects with autonomous-community-by-year fixed effects. Column (3) restricts the sample to municipalities with non-missing geographic characteristics and interacts year indicators with an indicator for a coast within 25 kilometers, mean log population over the estimation window, and the share of second homes in the 2011 Population and Housing Census. The age-specific samples in columns (1) and (3) are identical; column (2) uses slightly fewer observations. The crude-birth and crude-marriage samples in column (3) contain 17,476 and 17,545 municipality-years; on the same samples without the geographic trends, the corresponding estimates are $-0.483^{***}$ and $-0.598^{***}$. The first-stage $F$ statistics are computed in the crude-birth sample. Standard errors, clustered at the municipality, are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.5 Robustness to the House-Price Measure

The headline analysis uses the mean municipal transaction price. A mean transaction price can reflect the composition and liquidity of the dwellings that sell rather than the price of a constant-quality home. Table B.5 re-estimates the IV elasticities using three alternatives. The first is the mean price per square meter, which measures transaction values per unit of floor area. The second is a high-liquidity subsample restricted to municipalities in the top tercile of their mean annual transaction count over the estimation window. The third is a balanced panel restricted to municipalities with a reported mean price in every estimation year from 2014 to 2022, excluding 2020.

The age-specific elasticities remain negative and significant across all three alternatives: the birth elasticity at ages 20-34 ranges from $-0.28$ to $-0.31$, while the birth elasticity at ages 35-39 and both age-specific marriage elasticities are similarly stable. The crude elasticities are more sensitive, especially in the high-liquidity subsample, where the crude birth elasticity attenuates to $-0.18$ and the crude marriage elasticity to $-0.37$; the crude birth elasticity is $-0.48$ under the price-per-square-meter measure and $-0.44$ in the balanced panel. This reinforces the reading in the main text: the age-specific results are robust to how the local price is measured, while part of the larger crude elasticities reflects the composition and liquidity of transactions.

Table B.5: Robustness to the House-Price Measure (IV Elasticities)

| **Outcome** | Mean price (headline) | Price per $m^2$ | High-liquidity (top-tercile) | Balanced panel (2014-2022) |
|---|---|---|---|---|
| Birth rate, 20-34 | $-0.305^{***}$ | $-0.282^{***}$ | $-0.304^{***}$ | $-0.308^{***}$ |
| | (0.069) | (0.064) | (0.070) | (0.069) |
| Birth rate, 35-39 | $-0.196^{**}$ | $-0.181^{**}$ | $-0.214^{**}$ | $-0.203^{**}$ |
| | (0.084) | (0.077) | (0.086) | (0.084) |
| Birth rate, crude | $-0.537^{***}$ | $-0.480^{***}$ | $-0.183^{**}$ | $-0.439^{***}$ |
| | (0.061) | (0.055) | (0.073) | (0.059) |
| Marriage rate, 20-34 | $-0.454^{***}$ | $-0.420^{***}$ | $-0.438^{***}$ | $-0.458^{***}$ |
| | (0.135) | (0.126) | (0.139) | (0.136) |
| Marriage rate, 35-39 | $-1.052^{***}$ | $-0.976^{***}$ | $-1.059^{***}$ | $-1.059^{***}$ |
| | (0.175) | (0.159) | (0.179) | (0.176) |
| Marriage rate, crude | $-0.661^{***}$ | $-0.596^{***}$ | $-0.372^{***}$ | $-0.564^{***}$ |
| | (0.088) | (0.078) | (0.113) | (0.088) |

*Notes.* IV elasticities of equation (1). The mean-price column reproduces the headline estimates. The price-per-square-meter column instruments the previous calendar year's log price per square meter, including the 2020 price for 2021 outcomes. The high-liquidity column restricts the sample to municipalities in the top tercile of mean annual transaction counts. The balanced-panel column keeps municipalities with a price in every estimation year from 2014 to 2022, excluding 2020. The age-specific rows use municipalities with more than 10,000 inhabitants. Standard errors, clustered at the municipality, are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.6 Rents

Municipal rents come from the state rental-price reference system, SERPAVI. For collective housing and single-family housing, SERPAVI reports the median monthly rent per square meter and the number of rental units used to construct the statistic. We combine the two typologies using their published unit counts. When only one typology is available, we use that series. Publication requires at least ten rental observations, so the analysis is restricted to municipalities with more than 10,000 inhabitants.

Table B.6 instruments each rent measure with the same supply shifter used for purchase prices. All six rent elasticities are negative and statistically significant. The rent and purchase-price elasticities should not be compared as effects of the same object: rent is a monthly flow, the purchase price is a capitalized stock, and the instrument moves rents less

strongly than prices.

Table B.6: Rent and Purchase-Price IVs on the Same Sample

| **Outcome** | Unit-weighted rent per $m^2$ | Total rent | Purchase price |
|---|---|---|---|
| Birth rate, 20-34 | $-1.37^{***}$ | $-1.22^{***}$ | $-0.33^{***}$ |
| | (0.296) | (0.261) | (0.067) |
| Birth rate, 35-39 | $-0.87^{***}$ | $-0.78^{***}$ | $-0.21^{***}$ |
| | (0.302) | (0.266) | (0.073) |
| Birth rate, crude | $-0.95^{***}$ | $-0.85^{***}$ | $-0.23^{***}$ |
| | (0.261) | (0.230) | (0.060) |
| Marriage rate, 20-34 | $-1.67^{***}$ | $-1.48^{***}$ | $-0.40^{***}$ |
| | (0.540) | (0.484) | (0.127) |
| Marriage rate, 35-39 | $-4.35^{***}$ | $-3.89^{***}$ | $-1.06^{***}$ |
| | (0.679) | (0.610) | (0.156) |
| Marriage rate, crude | $-1.63^{***}$ | $-1.47^{***}$ | $-0.40^{***}$ |
| | (0.402) | (0.358) | (0.091) |

*Notes.* The sample contains 711 municipalities. The crude-birth and crude-marriage regressions contain 5,562 and 5,594 municipality-years, respectively. The age-specific regressions contain 5,531 municipality-years, except marriage at ages 35-39, which contains 5,524. In the crude-birth sample, the first-stage $F$ statistics are 95 for unit-weighted rent per square meter, 113 for total rent, and 205 for purchase prices. All specifications include municipality and year fixed effects. Municipality-clustered standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

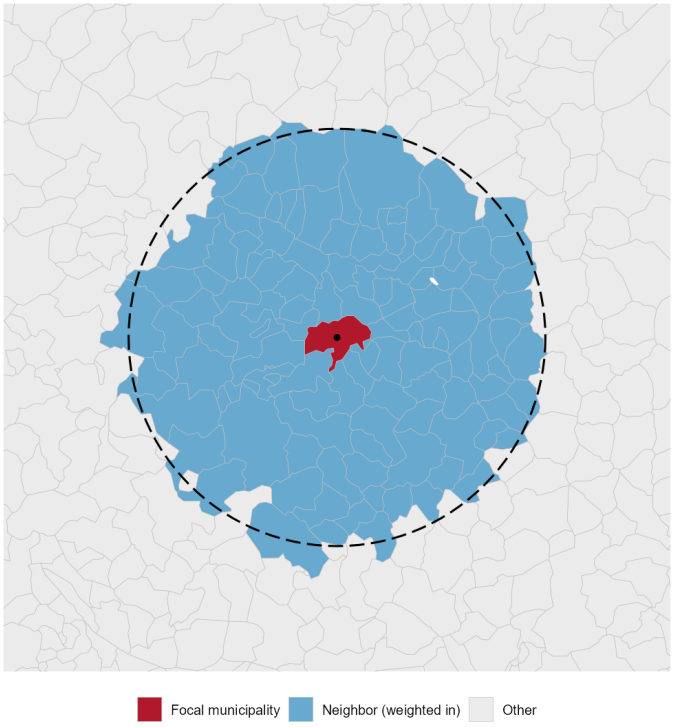
## B.7 Robustness to the Geographic Definition of the Local Market

The relevant housing market for a young household may extend beyond the municipality of residence. Young adults may respond to high prices in one municipality by moving to a cheaper nearby municipality. To address this concern, we replace the municipality's own price with a distance-weighted average of nearby prices. For each municipality, we compute an inverse-distance-weighted average of the mean transaction prices of all other municipalities whose centroids lie within 30 kilometers, excluding the focal municipality itself. We then take the log of this neighbor price and lag it by one year, exactly as we do for the own price, and instrument it with the corresponding neighbor-weighted supply shock.

Figure B.1 illustrates the construction for Salamanca. While the main analysis considers only prices in the municipality of Salamanca, the alternative analysis considers prices within the dashed circle. Table B.7 reports the results. The neighbor-price elasticities are negative

and statistically significant across all six outcomes and broadly similar in magnitude to the own-price estimates. The crude birth elasticity is $-0.55$, and the crude marriage elasticity is $-0.56$. The main conclusion is therefore not an artifact of municipal boundaries.

Figure B.1: Construction of the Neighbor Price




NOTES: The figure illustrates the construction of the neighbor price for Salamanca. The focal municipality is shown in red. Neighboring municipalities are those whose centroids lie within 30 kilometers of the focal municipality and are shown in blue. The neighbor price is the inverse-distance-weighted average of these municipalities' mean transaction prices; it enters the regression in logs and is lagged by one year, as the own price is. The dashed circle marks the 30-kilometer radius.

Table B.7: Robustness to the Geographic Definition of Price: Own versus Neighbor Price (IV)

| **Outcome** | **Own price** (headline) | **Neighbor price** (≤30 km, IDW) |
|---|---|---|
| Birth rate, 20-34 | $-0.305^{***}$ | $-0.424^{***}$ |
| | (0.069) | (0.064) |
| Birth rate, 35-39 | $-0.196^{**}$ | $-0.218^{***}$ |
| | (0.084) | (0.075) |
| Birth rate, crude | $-0.537^{***}$ | $-0.548^{***}$ |
| | (0.061) | (0.050) |
| Marriage rate, 20-34 | $-0.454^{***}$ | $-0.424^{***}$ |
| | (0.135) | (0.124) |
| Marriage rate, 35-39 | $-1.052^{***}$ | $-0.934^{***}$ |
| | (0.175) | (0.150) |
| Marriage rate, crude | $-0.661^{***}$ | $-0.565^{***}$ |
| | (0.088) | (0.073) |

*Notes.* IV elasticities from equation (1). The own-price column reproduces the headline estimates from Table 3. The neighbor-price column replaces the municipality's own lagged log price with the log of an inverse-distance-weighted average of the mean transaction prices of municipalities whose centroids lie within 30 kilometers, excluding the focal municipality, lagged by one year. The neighbor price is instrumented with the corresponding neighbor-weighted supply shock. IDW denotes inverse-distance weighting. The age-specific rows use municipalities with more than 10,000 inhabitants. Standard errors, clustered at the municipality, are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.8 Aggregation to Comarcas

We aggregate the municipal panel into the agricultural comarcas defined by the Ministry of Agriculture, territorial groupings of neighboring municipalities. The geographic crosswalk covers all 8,131 municipalities and all 326 comarcas. For each comarca-year, births, marriages, and population are summed across municipalities. The crude rates are the summed events divided by the summed population. The comarca price is the transaction-weighted mean of municipal prices, with the number of transactions per municipality-year as weights. The comarca supply share is the sum of buildable land divided by the sum of built and buildable land. It is interacted with the national house-price shifter.

Table B.8 reports the crude outcomes. Age-specific outcomes are not reported because

the underlying municipality-level age-specific data do not cover all municipalities within a comarca. The aggregation produces estimates close to the municipal results, although the comarca first stage is weaker.

Table B.8: Comarca Aggregation and the Municipal IV

| **Outcome** | Comarca IV | Municipal IV | Comarca first-stage $F$ |
|---|---|---|---|
| Crude birth rate | $-0.368^{***}$ | $-0.537^{***}$ | 49 |
| | (0.121) | (0.061) | |
| Crude marriage rate | $-0.491^{***}$ | $-0.661^{***}$ | 14 |
| | (0.111) | (0.088) | |

*Notes.* The comarca regressions cover 2,381 comarca-years from 319 comarcas for births and 2,388 comarca-years for marriages. They include comarca and year fixed effects and use the full-panel municipal crude-rate cutoffs. The municipal column reproduces the Section 4 estimates. Standard errors in parentheses are clustered by province for comarcas and municipality for the comparison. The first stage is re-estimated in each outcome sample: the birth and marriage coefficients are $-0.060$ and $-0.047$, with province-clustered standard errors of 0.0086 and 0.0124. The $F$ statistics differ because the two rate exclusions retain different comarca-years. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.9 Births to Spanish-Nationality Mothers

We construct a crude birth rate for Spanish nationals using births to Spanish-nationality mothers from the INE natality microdata. The denominator is the municipal population multiplied by the Spanish population share reported in the *Atlas de Distribución de la Renta de Hogares*. The municipality of residence is suppressed in the natality microdata for municipalities with fewer than 10,000 inhabitants, so this exercise is restricted to larger municipalities. The Atlas does not provide a nationality-by-age denominator, which prevents an age-specific version of this exercise.

Table B.9: Crude Birth-Rate IVs for Spanish Nationals and All Residents

| **Crude birth rate** | Equal-municipality IV |
|---|---|
| Spanish-nationality births / Spanish-nationality population | $-0.240^{***}$ |
| | (0.067) |
| All births / total population, comparable sample | $-0.171^{***}$ |
| | (0.065) |

*Notes.* The sample covers 775 municipalities with more than 10,000 inhabitants over 2015-2022, excluding 2020. The Spanish-nationality and all-birth regressions contain 5,169 and 5,242 municipality-years, respectively. The lagged log house price is instrumented with $S_m \times D_{t-1}$. Regressions include municipality and year fixed effects and give each municipality-year equal weight; municipality-clustered standard errors are in parentheses. The corresponding first-stage $F$ statistics are 150 and 152. The nationality-specific rate is trimmed at its own first and 99th percentiles after zero rates are excluded; the all-birth comparison uses the crude-birth cutoffs from the main estimates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.10 Robustness of the Standard Errors

The headline standard errors in Section 4 are clustered at the municipality level, allowing the errors of a given municipality to be arbitrarily correlated over time. A natural concern is that shocks may also be correlated across municipalities within a year, for example, because of national business-cycle conditions, aggregate housing demand, or common demographic shocks. Table B.10 therefore re-estimates the main OLS and IV regressions with standard errors two-way clustered by municipality and year, following Cameron et al. (2011). This inference procedure allows for both within-municipality serial correlation and within-year cross-sectional correlation.

The IV estimates remain negative and statistically significant for all six outcomes under two-way clustering. Two-way clustering changes the standard errors only modestly in this window, and every estimate, including the two crude rates, retains significance at the 1 or 5 percent level.

Table B.10: Robustness to Two-Way Clustering by Municipality and Year

| | IV | | | OLS | | |
|---|---|---|---|---|---|---|
| **Outcome** | $\hat{\beta}$ | Muni. | Muni. × year | $\hat{\beta}$ | Muni. | Muni. × year |
| Birth rate, 20-34 | −0.305 | (0.069)*** | (0.056)*** | −0.067 | (0.017)*** | (0.017)*** |
| Birth rate, 35-39 | −0.196 | (0.084)** | (0.070)** | −0.024 | (0.024) | (0.028) |
| Birth rate, crude | −0.537 | (0.061)*** | (0.059)*** | −0.009 | (0.010) | (0.009) |
| Marriage rate, 20-34 | −0.454 | (0.135)*** | (0.156)** | −0.064 | (0.031)** | (0.040) |
| Marriage rate, 35-39 | −1.052 | (0.175)*** | (0.179)*** | −0.171 | (0.048)*** | (0.079)* |
| Marriage rate, crude | −0.661 | (0.088)*** | (0.147)*** | +0.003 | (0.015) | (0.017) |

*Notes.* Each row reports the headline OLS and IV estimates of equation (1) for one outcome. The columns labeled "Muni." report standard errors clustered at the municipality level, as in the headline tables. The columns labeled "Muni. × year" report standard errors two-way clustered by municipality and year. Stars indicate the significance of the same coefficient using the standard error in that column. The IV estimates instrument the lagged log house price with $S_m \times D_{t-1}$, as in equation (2). The age-specific rates for ages 20-34 and 35-39 are defined only for municipalities with more than 10,000 inhabitants; the crude rates cover all municipalities in the estimation sample. The sample covers eight years (2014-2022 excluding 2020). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B.11 Robustness of the Instrument

The headline instrument interacts a broad measure of buildable land with the national house-price index. Both components involve choices: the supply share can be defined using different land-cover sources and restrictions, and the national shifter can be measured using different aggregate housing-market series. This appendix shows that the crude birth and marriage elasticities remain negative and significant with the alternative supply shares and the national transaction-based shifter. We use the same IV specification as in Section 4 and report the two crude outcomes, which cover the full municipal sample.

### B.11.1 Alternative Supply Shares

Table B.11 replaces the baseline broad buildable-land share with three alternatives. The *strong* measure restricts buildable land to agricultural and forest or shrub cover, excluding sparsely vegetated terrain. The *5 km buffer* measure, following Saiz (2010), counts buildable land only within five kilometers of existing urban patches, capturing land that a municipality could plausibly develop. The *SIOSE 2005 + Natura 2000* measure uses a finer land-cover map and excludes protected land from otherwise buildable area, as described in

Appendix A.7.

The estimates are stable across supply-share definitions. The broad and strong measures deliver nearly identical elasticities. The five-kilometer buffer produces slightly larger magnitudes. The SIOSE-based measure yields crude birth and marriage elasticities of −0.561 and −0.567, both significant at the 1 percent level, with a crude-birth first-stage $F$ statistic of 116. The estimates therefore do not hinge on a particular definition of buildable land.

Table B.11: IV Elasticities across Alternative Supply Shares: Crude Rates

| | **Broad** (headline) | **Strong** | **5 km buffer** | **SIOSE 2005** + Natura 2000 |
|---|---|---|---|---|
| Crude birth rate | $-0.537^{***}$ | $-0.535^{***}$ | $-0.603^{***}$ | $-0.561^{***}$ |
| | (0.061) | (0.061) | (0.069) | (0.085) |
| Crude marriage rate | $-0.661^{***}$ | $-0.668^{***}$ | $-0.699^{***}$ | $-0.567^{***}$ |
| | (0.088) | (0.088) | (0.096) | (0.142) |
| First-stage $F$ | 290 | 282 | 248 | 116 |
| $N$ | 22,326 | 22,326 | 20,310 | 22,326 |

*Notes.* Each cell reports the IV elasticity of the crude rate with respect to the lagged log house price. The lagged log price is instrumented with $S_m \times \text{HPI}_{t-1}$, where $S_m$ is the indicated supply-share measure and $\text{HPI}_{t-1}$ is the first-quarter national house-price index in the lagged-price year. All specifications include municipality and year fixed effects. Standard errors, clustered at the municipality, are in parentheses. The first-stage $F$ and $N$ rows use the crude-birth sample; the marriage sample is marginally smaller. The five-kilometer buffer share is missing for some municipalities, which lowers their number of observations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

### B.11.2 Alternative Demand Shifters

Table B.12 holds the supply share fixed at the baseline broad measure and replaces the national house-price index with three alternative national series: national housing transactions, the real mortgage rate, and the three-month Euribor. For each we report the first-stage coefficient on the interaction $S_m \times D_{t-1}$, the first-stage $F$, and the IV crude birth and marriage elasticities. The house-price index and transactions are demand-quantity shifters that rise with housing demand, and both enter the first stage negatively: where buildable land is more abundant, a national demand shock raises local prices less. Their IV elasticities are similar, so the result is not specific to the house-price index.

The two interest-rate shifters enter the first stage with positive signs, opposite to the

demand-quantity shifters. This is expected: higher borrowing costs move inversely to housing demand, so the interaction flips. We report them as diagnostics, not as valid alternative instruments, because interest rates can affect household formation directly through borrowing costs and so do not satisfy the same exclusion restriction; their IV elasticities remain negative and significant.

Table B.12: IV Elasticities and First Stage across Alternative Demand Shifters (Crude Rates)

| | **HPI** (headline) | **Transactions** | **Mortgage** rate | **Euribor** (3m) |
|---|---|---|---|---|
| Crude birth rate | $-0.537^{***}$ | $-0.556^{***}$ | $-0.481^{***}$ | $-0.521^{***}$ |
| | (0.061) | (0.065) | (0.076) | (0.084) |
| Crude marriage rate | $-0.661^{***}$ | $-0.676^{***}$ | $-0.795^{***}$ | $-0.901^{***}$ |
| | (0.088) | (0.090) | (0.116) | (0.119) |
| First-stage coef. ($S_m \times D_{t-1}$) | $-0.020$ | $-5.9 \times 10^{-6}$ | $+0.081$ | $+0.068$ |
| First-stage $F$ | 290 | 267 | 210 | 212 |
| $N$ | 22,326 | 22,326 | 22,326 | 22,326 |

*Notes.* Each cell reports the IV elasticity of the crude rate with respect to the lagged log house price, instrumented with $S_m \times D_{t-1}$, where $S_m$ is the baseline broad buildable-land share and $D_{t-1}$ is the indicated national shifter aligned with the lagged-price year. HPI denotes the first-quarter national house-price index used in the headline specification; Transactions denotes national housing transactions. The first-stage coefficient is the loading of the lagged log price on $S_m \times D_{t-1}$; its units differ across shifters, so only its sign and the $F$ are comparable. The reported first stage and $N$ use the crude-birth sample; the marriage regressions have 22,289 observations. The demand-quantity shifters (HPI, transactions) load negatively; the interest-rate shifters (real mortgage rate, three-month Euribor) load positively because rates move inversely to demand, and are shown as diagnostics rather than valid instruments, since rates may affect household formation directly through borrowing costs. All specifications include municipality and year fixed effects. Standard errors, clustered at the municipality, are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

# C Additional Results: The Homeownership Subsidy

This appendix reports additional results for the policy analysis in Section 5. Appendix C.1 compares treated and control municipalities in the pre-policy year. Appendix C.2 examines alternative functional forms for births and marriages. Appendix C.3 splits the birth effect by commuting-zone status. Appendix C.4 reports a placebo test at a population cutoff where no policy was introduced. Appendix C.5 presents the bandwidth checks, and Appendix C.6 reports the parental coresidence event study for adults born in Spain.

## C.1 Balance

Table C.1 compares treated and control municipalities in 2017 within the ±2,500-inhabitant bandwidth. The groups differ in several levels. Population is the running variable and is unbalanced by construction; treated municipalities are also older and have lower income, house prices, birth rates, and marriage rates. These differences do not provide the identifying variation. Municipality fixed effects absorb time-invariant levels, and the design relies on parallel pre-treatment trends. The flat pre-2018 coefficients in the main event studies, followed by the break after the 2018 reform, provide the relevant evidence for this assumption.

Table C.1: Balance at the 5,000-Inhabitant Cutoff, 2017

| Variable | Treated | Control | Difference | Robust SE | $p$-value | Norm. difference | $N$ |
|---|---|---|---|---|---|---|---|
| Population | 3,527 | 6,090 | −2,563 | 50.00 | $< 0.001$ | −3.51 | 959 |
| Birth rate per 1,000 | 7.58 | 8.04 | −0.46 | 0.15 | 0.003 | −0.20 | 959 |
| Marriage rate per 1,000 | 3.36 | 3.65 | −0.29 | 0.09 | 0.001 | −0.22 | 959 |
| Mean house price | 84,712 | 98,985 | −14,274 | 3,641 | $< 0.001$ | −0.28 | 920 |
| Income per head | 14,776 | 15,505 | −729 | 228 | 0.001 | −0.22 | 959 |
| Share under 18 | 16.87 | 18.17 | −1.30 | 0.26 | $< 0.001$ | −0.34 | 959 |
| Share over 65 | 20.96 | 18.73 | +2.22 | 0.41 | $< 0.001$ | +0.36 | 959 |
| Homeownership share (2011) | 0.813 | 0.805 | +0.008 | 0.005 | 0.101 | +0.11 | 913 |
| Buildable-land share | 0.955 | 0.936 | +0.019 | 0.006 | 0.003 | +0.21 | 913 |

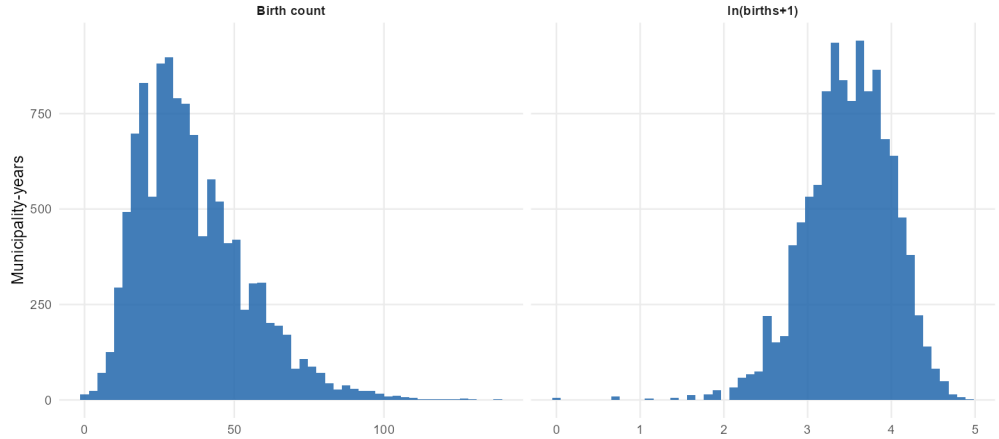
*Notes.* The table compares municipalities in the main 2017 cutoff sample, excluding the Basque Country and Navarre. Treated municipalities have populations between 2,500 and 5,000 inhabitants, while control municipalities have populations between 5,000 and 7,500. Differences and heteroskedasticity-robust standard errors come from separate regressions of each covariate on the treatment indicator. The normalized difference divides the treated-control difference by the square root of the mean of the two group variances. Birth and marriage rates are crude rates per 1,000 inhabitants. Age-share levels are percentages, and their differences are percentage points. House prices and income per head are in euros. The homeownership share is the proportion of dwellings classified as owned in the 2011 municipal census tenure table. The number of observations is lower for house prices, homeownership, and buildable land because of missing values.

## C.2 Robustness of the Fertility and Marriage Results to Functional Form

Figure C.1 shows the distribution of municipal birth counts and of $\ln(\text{births}+1)$ in the main cutoff sample. Birth counts are right-skewed, which motivates the log-count specification used in Section 5.4. Counts are almost never zero, so the +1 transformation matters little in practice.

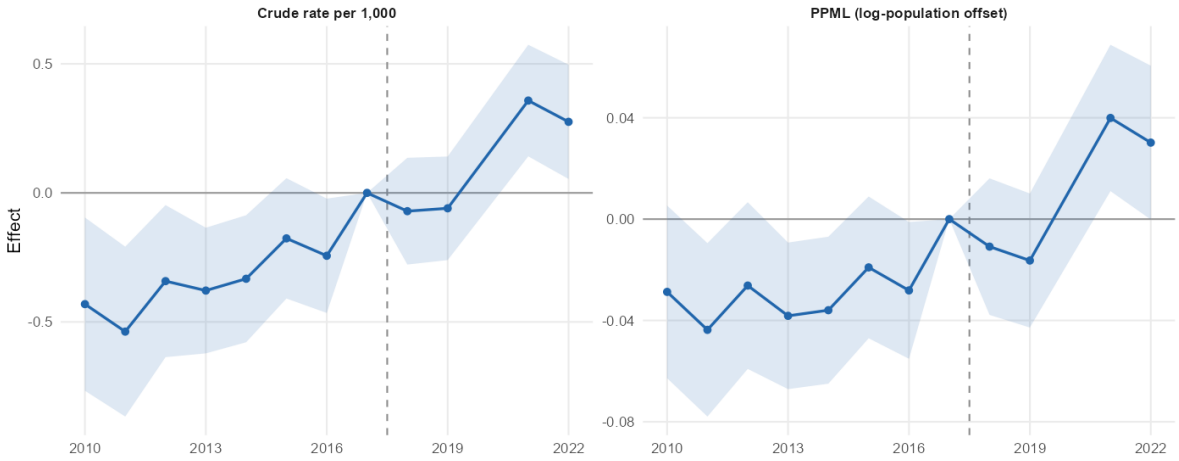
Figures C.2 and C.3 re-estimate the births and marriages event studies using two alternative functional forms: the crude rate per 1,000 inhabitants and a Poisson model with a log-population offset. The birth response is stable across these alternatives. The crude-rate specification displays a mild pre-trend, which is one reason we prefer the log-count specification in the main text.

Figure C.1: Distribution of Municipal Birth Counts



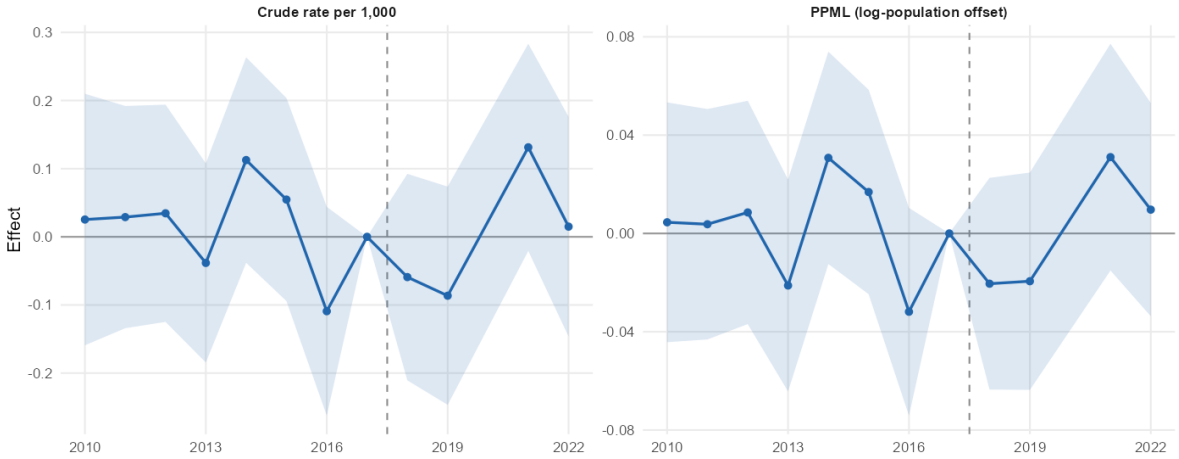

NOTES: The figure shows the distribution of municipal birth counts and of $\ln(\text{births}+1)$ in the main cutoff sample. The sample includes municipalities with 2017 populations between 2,500 and 7,500 inhabitants, excluding the Basque Country and Navarre, over the period 2010-2022. The year 2020 is excluded. Birth counts come from INE vital statistics. The right-skewed distribution of counts motivates the log-count specification used in the main text.

Figure C.2: Births: Robustness to Functional Form




NOTES: Municipal event-study estimates at the 5,000-inhabitant cutoff. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The sample includes municipalities with 2017 population between 2,500 and 7,500, excluding the Basque Country and Navarre. The figure reports two alternatives to the main $\ln(\text{births} + 1)$ specification: the crude birth rate per 1,000 inhabitants and a Poisson (PPML) count model with a log-population offset. The omitted year is 2017. The year 2020 is excluded. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Confidence intervals are at the 90 percent level.

Figure C.3: Marriages: Robustness to Functional Form




NOTES: Municipal event-study estimates at the 5,000-inhabitant cutoff. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The sample includes municipalities with 2017 population between 2,500 and 7,500, excluding the Basque Country and Navarre. The figure reports two alternatives to the main $\ln(\text{marriages} + 1)$ specification: the crude marriage rate per 1,000 inhabitants and a Poisson (PPML) count model with a log-population offset. The omitted year is 2017. The year 2020 is excluded. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Confidence intervals are at the 90 percent level.

## C.3 Birth Effects by Commuting-Zone Status

Table C.2 asks whether the increase in births is concentrated in municipalities located inside commuting zones. We use the OECD and Eurostat Functional Urban Areas, which group cities with their surrounding journey-to-work areas. The effect is somewhat larger outside these areas and is approximately zero in the Madrid and Barcelona Functional Urban Areas. The birth response is therefore not driven by municipalities inside the two largest commuter belts.

Table C.2: Birth Effect by Functional Urban Area Membership

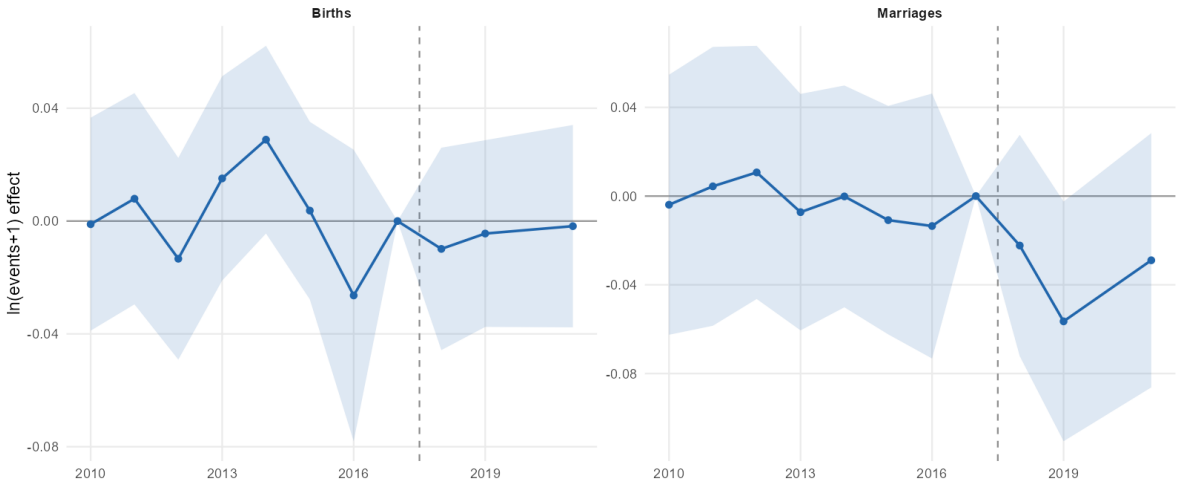
| Municipality group | Average post effect | SE | Municipalities | Treated |
|---|---|---|---|---|
| Outside Functional Urban Areas | $+0.038^{***}$ | (0.014) | 453 | 304 |
| Inside any Functional Urban Area | $+0.029^{*}$ | (0.017) | 506 | 326 |
| Madrid and Barcelona Functional Urban Areas | +0.002 | (0.035) | 130 | 83 |

*Notes.* Functional Urban Areas are journey-to-work commuting zones. Each row estimates a separate municipality-level specification on the indicated subsample. The outcome is ln(births+1). Coefficients are in log points. The coefficient is the interaction between treatment, defined by 2017 population below 5,000, and an indicator for the post-treatment years 2018-2022, excluding 2020. The main ±2,500-inhabitant bandwidth is used. All specifications include municipality and year fixed effects and cluster standard errors at the municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## C.4 Placebo at a Fake Cutoff

If the birth response reflected a generic post-2018 pattern among small municipalities rather than the homeownership subsidy, similar effects might appear at nearby population thresholds where no policy was introduced. Figure C.4 therefore repeats the fertility and marriage event studies at a placebo cutoff of 10,000 inhabitants. The placebo-treated group consists of municipalities with a 2017 population between 7,500 and 10,000, and the placebo control group consists of municipalities with a 2017 population between 10,000 and 12,500. Neither group was eligible for the 2018 program because it did not meet the 5,000-inhabitant threshold. The placebo birth path is flat throughout, and the marriage path is noisier; the placebo birth estimates do not show the post-2018 increase observed at the true 5,000 cutoff. This supports interpreting the effect at 5,000 as a policy effect rather than a small-municipality artifact.

Figure C.4: Placebo Event Study at a 10,000-Inhabitant Cutoff

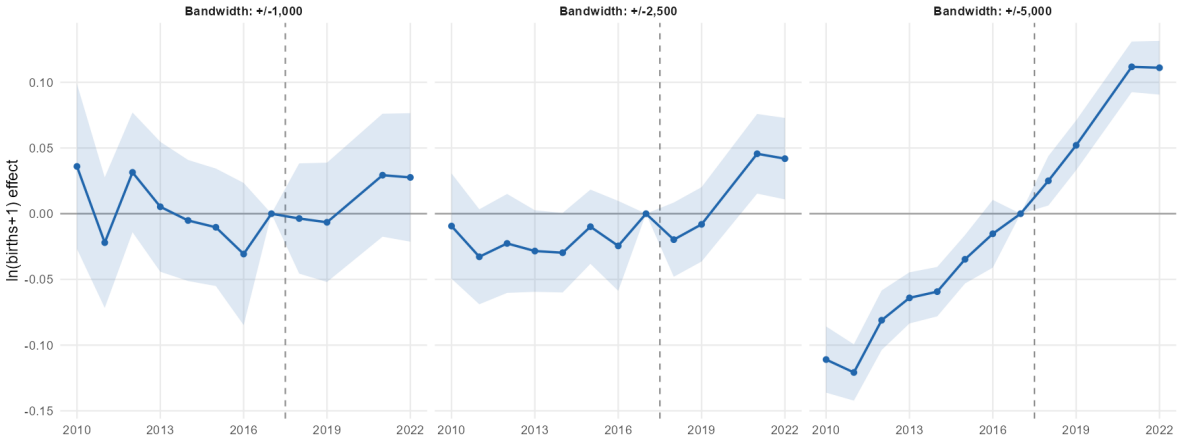



NOTES: Placebo municipal event-study estimates at a fake 10,000-inhabitant cutoff. No home-ownership subsidy was introduced at this cutoff in 2018. The placebo-treated group consists of municipalities with a 2017 population between 7,500 and 10,000; the placebo control group consists of municipalities with a 2017 population between 10,000 and 12,500. The left panel reports $\ln(\text{births} + 1)$ and the right panel reports $\ln(\text{marriages} + 1)$. The omitted year is 2017. The estimation period is 2010-2021, excluding 2020. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Confidence intervals are at the 90 percent level.

## C.5 Robustness to Bandwidth

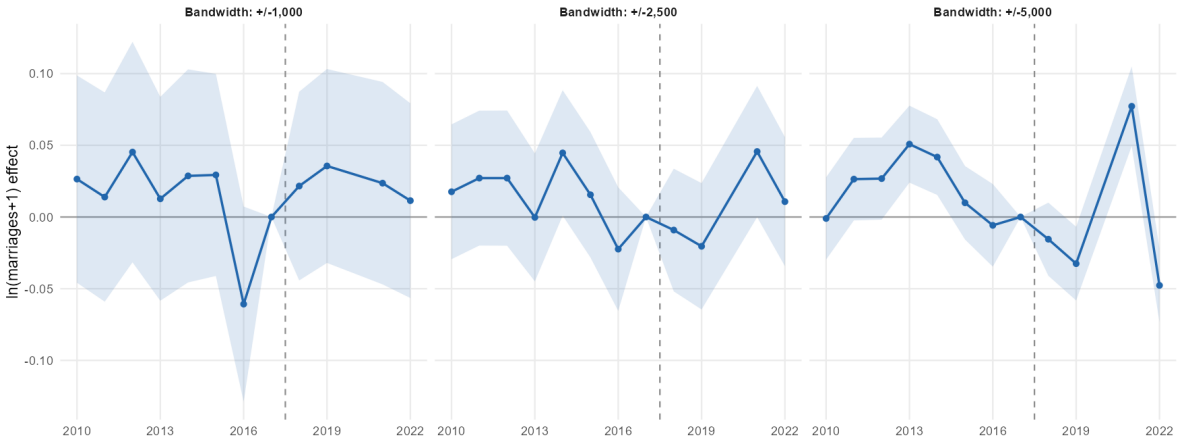
We re-estimate the event studies on bandwidths of 1,000, 2,500, and 5,000 inhabitants around the 5,000-inhabitant cutoff. Figures C.5 and C.6 show the full event-study paths for births and marriages. The birth point estimate increases with bandwidth, but the widest bandwidth introduces a visible pre-trend as less comparable municipalities enter the sample, which is why the main specification uses the 2,500-inhabitant bandwidth; the marriage paths are noisier and mostly insignificant at every bandwidth. Figures C.7 and C.8 report the same check for the Household Panel outcomes: the parental-coresidence response is robust across bandwidths, while the homeownership response is insignificant in the tight bandwidth and becomes significant only as the bandwidth widens, which increases precision but compares municipalities farther from the cutoff.

Figure C.5: Births: Robustness to Bandwidth



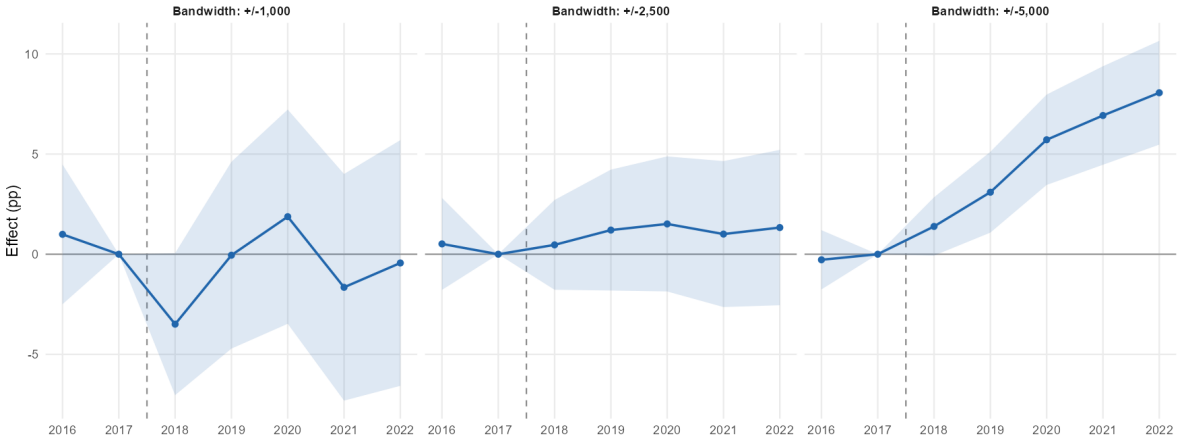

NOTES: Municipal event-study estimates for $\ln(\text{births} + 1)$ at the 5,000-inhabitant cutoff. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The figure reports bandwidths of 1,000, 2,500, and 5,000 inhabitants around the cutoff. The omitted year is 2017. The year 2020 is excluded. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Confidence intervals are at the 90 percent level. A visible pre-trend appears at the widest bandwidth, which is why the main text uses the 2,500-inhabitant bandwidth.

Figure C.6: Marriages: Robustness to Bandwidth



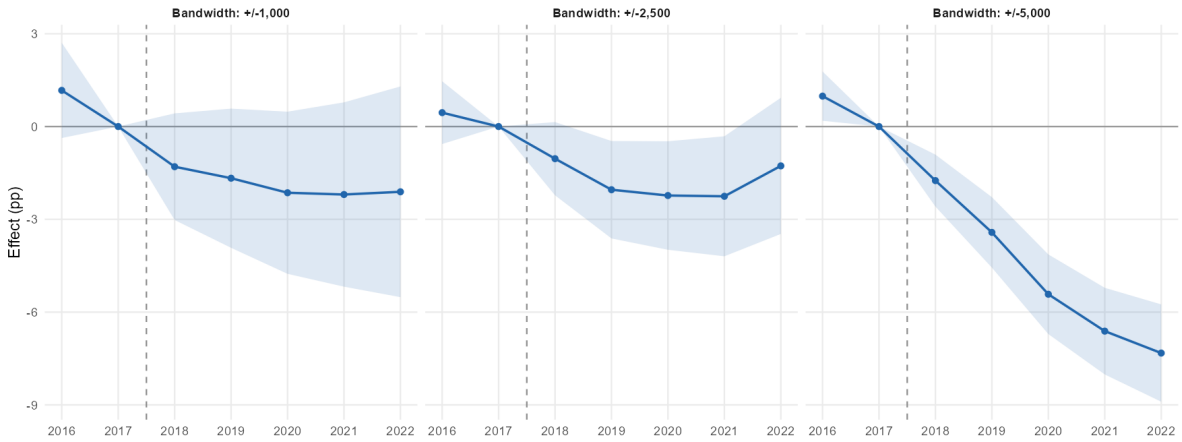

NOTES: Municipal event-study estimates for ln(marriages + 1) at the 5,000-inhabitant cutoff. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The figure reports bandwidths of 1,000, 2,500, and 5,000 inhabitants around the cutoff. The omitted year is 2017. The year 2020 is excluded. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Confidence intervals are at the 90 percent level. The marriage paths are noisier than the birth paths and mostly insignificant at every bandwidth.

Figure C.7: Homeownership among Independent Young Adults: Robustness to Bandwidth




NOTES: Municipal event-study estimates using the Household Panel. The outcome is the share of independent young adults aged 20-34 whose households own any residential dwelling, not necessarily the one they occupy; ownership is measured using cadastral records and excludes garages, parking spaces, storage rooms, and land. The estimation period is 2016-2022. Coefficients are in percentage points. IEF calibration weights construct municipality-year shares; the regressions give equal weight to municipality-year observations. Individuals observed in 2017 remain assigned to their 2017 municipality throughout. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The figure reports bandwidths of 1,000, 2,500, and 5,000 inhabitants around the cutoff. The omitted year is 2017. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Confidence intervals are at the 90 percent level.

Figure C.8: Living with a Parent: Robustness to Bandwidth




NOTES: Municipal event-study estimates using the Household Panel. The outcome is the share of young adults aged 20-34 living with a parent. The estimation period is 2016-2022. Coefficients are in percentage points. IEF calibration weights construct municipality-year shares; the regressions give equal weight to municipality-year observations. Individuals observed in 2017 remain assigned to their 2017 municipality throughout. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The figure reports bandwidths of 1,000, 2,500, and 5,000 inhabitants around the cutoff. The omitted year is 2017. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality. Confidence intervals are at the 90 percent level.

## C.6 Parental Coresidence among Adults Born in Spain

Figure C.9 repeats the parental-coresidence event study for adults aged 20-34 who were born in Spain. The post-treatment path closely follows the result for all young adults. The equal-municipality average post-treatment effect is $-1.61$ percentage points, with a standard error of 0.94 and a $p$-value of 0.087. This result indicates that the decline in parental coresidence is not due only to changes in the immigrant composition of treated municipalities.

Figure C.9: Living with a Parent among Adults Born in Spain




NOTES: Municipal event-study estimates using the Household Panel. The outcome is the share of adults aged 20-34 born in Spain who live with a parent. The figure displays coefficients for 2017-2022. Coefficients are in percentage points. IEF calibration weights construct municipality-year shares; the regression gives equal weight to municipality-year observations. Individuals observed in 2017 remain assigned to their 2017 municipality throughout. Treatment is defined by 2017 municipal population below 5,000 inhabitants. The sample uses the main ±2,500-inhabitant bandwidth and excludes the Basque Country and Navarre. The omitted year is 2017. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality, and confidence intervals are at the 90 percent level.