The Financial Consequences of Wanting to Own a Home*

Caitlin S. Gorback, McCombs School of Business, UT-Austin†
and
Gregor Schubert, UCLA Anderson School of Management‡

October 2026

Abstract

Does wanting to own a home build wealth? We use homeownership rates in immigrants' countries of origin (*HOCO*) as a source of exogenous variation in the desire to own. A 10pp higher *HOCO* raises U.S. homeownership by 1.5pp; the effect strengthens with time in the U.S., transmits across generations, and is not explained by origin-country economic conditions. High-affinity households respond more to credit expansions, default less, and accumulate more housing and total wealth by retirement, but these gains arise from buying early and holding on in favorable markets rather than from owning per se: an owner who bought at 28 ends within $25,000 of an otherwise identical renter, ahead in half of states and behind in the other half. We then price the preference. Tenure barely responds to the cost of owning, so observed origin gaps imply premia for owning of several thousand dollars a year in either direction—more dispersion than the single ownership wedge structural models calibrate—of which the home-country norm explains about a tenth. Policies that lower the price of owning therefore reach few households who would not have bought anyway.

*A previous version of this paper was circulated under the title "The Cultural Origins of Immigrant Homeownership". We thank discussants and participants at the 2022 Pre-WFA Real Estate Symposium, the Southern Economic Association Meeting, 2023 ASSA–AREUEA, the AREUEA Meetings in Dublin and Tokyo, the 2023 NBER Real Estate Summer Institute, the 2023 BFI Women in Empirical Microeconomics Conference, the 2023 Utah Eccles Innovations in Housing Affordability Summit, Tel Aviv University Conference on Low Income Housing Supply and Housing Affordability, the 2024 UEA Meeting, the 2025 Lone Star Finance Conference, the 2026 Texas Finance Festival, and the 2026 Miami Oktoberfest Finance Conference for valuable comments. We are also grateful for feedback from seminar participants at UCLA Anderson, Harvard University, the Federal Housing Finance Agency, UT Austin (Economics and McCombs), the University of Houston, CUNY Baruch, and the University of Nevada, Las Vegas. We also thank the UCLA Ziman Center for Real Estate's Rosalinde and Arthur Gilbert Program in Real Estate, Finance and Urban Economics for generous funding. The authors have no futher relevant and material financial relationships to disclose that might bear on this research.

†Email: caitlin.gorback@mccombs.utexas.edu

‡Email: gregor.schubert@anderson.ucla.edu.

In the United States, homeownership has long been treated by policymakers as a barometer of well-being and a pillar of the American Dream (Layton, 2021; Goodman and Mayer, 2018), and many costly policies aim to raise it on the premise that owning builds wealth. Whether it does, and for whom, is hard to learn from observed owners, who chose to own. Variation in the desire to own itself can do two things at once: shift tenure for reasons unrelated to a household's finances, and reveal what the choice is worth to those who make it.

In this paper, we use immigrants' affinity for homeownership, proxied by the homeownership rate in their country of origin by marital status (HOCO), as plausibly exogenous variation in tenure choice, conditional on rich household controls and local market conditions. We show that a stronger affinity causally raises homeownership among foreign-born U.S. residents, raises housing and total wealth at retirement, and shapes how households respond to credit conditions over the housing cycle. The wealth effects, however, are driven by local housing-market dynamics and by *how* high-affinity households own—buying earlier, holding on, paying down—rather than by the act of owning itself. We then use the same variation to put a dollar value on the preference for owning. It is large, widely dispersed across origins, and only weakly responsive to the price of owning, so policies that lower that price reach few households who would not have bought anyway.

We construct HOCO from harmonized census microdata for 85 origin countries, covering nearly 80% of the U.S. foreign-born population, following the origin-norm approach of (Blau, Kahn, and Papps, 2011). We combine this data with household microdata from the American Community Survey (ACS) data to document the passthrough of cultural norms of homeownership among immigrants in the U.S. Using household-level regressions, we find that HOCO significantly affects household tenure choice in the U.S., with immigrants from countries with higher homeownership more likely to own homes in the U.S., while those with lower rates opt instead to rent. The size of the effect suggests that a 10 pp increase in homeownership in a householder's country of origin is associated with a 1.5 pp increase in the likelihood of being a homeowner as a U.S. resident. The interquartile range of differences in *HOCO* among foreign-born residents causes a 2.8 pp difference in homeownership, for a relative magnitude, this equals 44% of the standard deviation in homeownership across U.S. states. The estimate is stable when including origin-country economic and institutional controls and controls for migrant selection, using alternative HOCO measures and samples. Fianlly, we show that the origin characteristics that could threaten the exclusion restriction are uncorrelated with HOCO or bias us toward zero. Consistent with a transmitted preference rather than lived experience, the effect is three-quarters as large for immigrants who arrived as children, strengthens with time in the U.S., and operates through a foreign-born spouse's HOCO as well as the householder's.

We then ask what a stronger affinity means for wealth at retirement. Using restricted-access Health and Retirement Study data, which link household balance sheets around age 60 to country of origin, we find that a one percentage point higher HOCO raises the probability of owning the primary residence at retirement by 0.5 percentage points, home equity by roughly \$1.7K, non-housing wealth by \$3.6K, and total wealth by \$5.3K; a one standard deviation difference in HOCO

(12 pp) corresponds to 19% more home equity and 37% more non-housing wealth relative to sample means. Two patterns point away from forced saving as the mechanism: high-affinity households are no more likely to hold stocks or risky financial assets, and the wealth effect is 90% smaller in states where housing is expensive and saving for a home is hardest. The gains appear instead to come from exposure to local housing-market dynamics, a possibility we test directly below.

A preference for owning also governs when households enter. Among renters in 2000, those from high-HOCO origins were more likely to own by 2005, and the gap was largest among households whose observables predicted low ownership before the boom. To isolate the role of credit, we build a group-by-commuting-zone panel over the boom, bust, and recovery (2000–2015) and use lender-driven shocks to local mortgage supply, in the spirit of Khwaja and Mian (2008) and Greenstone, Mas, and Nguyen (2020), as exogenous variation in credit availability. A one standard deviation credit-supply shock widens the ownership gap between high- and low-affinity groups by 1.06 percentage points over five years, against a baseline gap of 0.86 percentage points: credit expansions draw in the households already inclined to own.

High-affinity households also held on. They exited ownership less during the bust, and among those who bought at the 2006–2007 peak a one standard deviation higher HOCO was associated with a 38% lower probability of mortgage delinquency. Their realized housing returns were slightly higher than low-affinity buyers' within the same purchase cohort, but that difference is an order of magnitude smaller than the difference across cohorts: homes bought in the 2000s returned far less than homes bought in the 1990s or 2010s, whatever the buyer's affinity.

Whether the equity these households accumulated is wealth they would not otherwise have had is a separate question, and we answer it with a historical reconstruction. For a household born in 1955 we compare an owner who bought the median house in its state at each age from 25 to 55 and held it to 60 with an identical household that rented the same house and invested the difference. Home equity is always large, and the owner's wealth outside the house is always smaller than the renter's. The net is a coin flip whose odds are set by purchase year and state: positive on average for purchases in the mid-1990s, negative for the early 1980s and the mid-2000s, and negative almost everywhere against a renter who invests at the equity return.

We then ask what the preference for owning that drives these choices is worth, and who is on the margin that housing policy tries to move. We measure the user cost of owning relative to renting that each recent mover faced in its market and year, and take the response of tenure to that price from the 2015 cut in FHA mortgage insurance premiums (Bhutta and Ringo, 2021). The response is weak: a $1,000 change in the annual cost of owning moves the ownership rate by one to two percentage points. Dividing each origin group's ownership gap with otherwise identical natives by that response yields the group's premium for owning in dollars. The premia are large and dispersed: Vietnamese and Chinese movers behave as if owning were $14,000 to $18,000 a year cheaper for them than for natives, Indian and Puerto Rican movers as if it were $6,000 to $9,000 more expensive, with a standard deviation across origins of $6,000 a year—about 3% of house value, larger than the entire ownership wedge that structural models calibrate to match the

aggregate homeownership rate. The home-country norm explains about a tenth of this dispersion, roughly $2,000 a year across its interquartile range; the dispersion is as wide among immigrants who arrived as children and share natives' credit access and horizons, so it is not an artifact of constraints. The present value of the premium per point of *HOCO* is about what owning returned in home equity at retirement, and the premium predicts the behaviors behind that return—earlier purchase, longer tenure, paying off the mortgage.

These findings have direct implications for housing policy. Nearly every lever (the mortgage interest deduction, government mortgage guarantees, FHA and down-payment assistance, et.c) works by lowering the price of owning relative to renting. With tenure as insensitive to price as we estimate, a subsidy of $2,600 a year, roughly the pre-2018 value of the mortgage interest deduction to an itemizing household, raises ownership by two to five percentage points and pays 87 to 97% of its cost to households who would have owned without it; the fiscal cost of each additional owner is $20,000 to $100,000 a year. The households it reaches are those with the largest premium for owning, who buy in good years and bad; the households it is meant to reach have the smallest, and for several of the largest immigrant groups the binding constraint is horizon or credit access rather than price. Culture, which we cannot move, is nonetheless a useful benchmark: the $2,000 a year that the interquartile range of *HOCO* adds to the premium is worth one to two mortgage interest deductions and moves ownership by the same two to three points, at no fiscal cost. And while a stronger desire to own does raise wealth at retirement, it does so through exposure to housing market dynamics and through holding behavior rather than through systematic saving. Our reconstruction shows that the same purchase produced large gains or large losses depending on the state and year, so policies that bring marginal households into ownership buy them exposure to that lottery without the attachment that led high-affinity households to hold through its downturns. If the aim is retirement security, the case for pairing ownership policy with saving commitments that do not depend on the housing cycle is strong (Bernstein and Koudijs, 2024), and the case for expecting ownership alone to deliver it is weak.

Related literature & contributions. Our paper contributes to a growing literature linking past experiences and cultural norms to financial decision-making. Formative experiences shape financial preferences: those exposed to high inflation avoid adjustable-rate mortgages and own at higher rates (Botsch and Malmendier, 2020; Malmendier and Steiny Wellsjo, 2024). Homeownership also correlates across generations and places of origin (Charles and Hurst, 2003; Benetton, Kudlyak, and Mondragon, 2024; Ringo, 2020; Marcén and Morales, 2020), and historical shocks leave long-lived imprints on housing attitudes (Happel, Karabulut, Schäfer, and Tuzel, 2022). We show that origin-country norms causally affect homeownership, credit responses, and long-run portfolios among U.S. immigrants, a new channel through which cultural preferences shape wealth accumulation.

Our results also connect to work on marginal homebuyers and credit expansions, which finds that looser credit standards raised homeownership, especially among subprime borrowers, with mixed welfare consequences (Mian and Sufi, 2009; Bhutta, 2015; Bayer, Ferreira, and Ross, 2016).

We show that certain immigrant groups exhibit the same marginal behavior, with outcomes ranging from greater default exposure to sizable housing wealth gains.

Homeownership is central to household portfolio composition (e.g., Cocco, Gomes, and Maenhout, 2005; Yao and Zhang, 2005; Chetty, Sándor, and Szeidl, 2017), but empirical progress has been limited by data that rarely combine financial and housing information. Using Swedish administrative data, Sodini, Van Nieuwerburgh, Vestman, and von Lilienfeld-Toal (2023) show that access to ownership can raise total wealth and risk-taking, while Bernstein and Koudijs (2024) and Kovacs and Moran (2023) emphasize housing as a savings commitment. We complement this evidence with plausibly exogenous variation in ownership affinity, connecting the cultural determinants of tenure choice to exposure to credit cycles and to the composition of wealth at retirement.

Finally, our findings speak to structural models that rely on an unobserved ownership wedge to match high homeownership rates. The wedge, a rental-sector cost or a utility premium for owning, is calibrated to a single aggregate moment and is the same for every household (Kaplan, Mitman, and Violante, 2020; Mabille, 2023; Coven, Golder, Gupta, and Ndiaye, 2026); Greenwald and Guren (2021) allow it to vary. We estimate the wedge rather than calibrate it, in dollars, from the tenure choices of origin groups facing the same prices in the same markets, and find that its dispersion across groups exceeds its calibrated mean. The weak price response and wide dispersion are the configuration these models generate from borrowing constraints and moving shocks, which is why we report the premium relative to natives who share those frictions. Tracing part of the dispersion to home-country norms gives a micro-foundation for a parameter that has been a calibration device, and a reason to treat it as a distribution.

Section I describes the data. Section II establishes the effect of homeownership affinity on tenure choice. Section III examines implications for wealth and portfolios at retirement. Section IV studies lifecycle housing choices, including credit take-up, delinquency, and returns over the housing cycle. Section V prices the preference for owning, estimates its distribution across origins and its cultural component, and draws out what the estimates imply for who housing policy reaches. Section VI concludes.

I. Data

A. Measuring Affinities for Homeownership

The concept of cultural norms or an "affinity" is not ex-ante well defined, but can be thought of as the mass lived experience or expectations around a given institution in a specific culture (or in this case, country). For example, Blau et al. (2011) use the relative female-to-male labor force participation rates in origin countries as a measure of cultural norms around women's labor supply.

Analogously, we define homeownership norms to be the rate of homeownership in one's country of origin.[1] We use a number of different approaches to try to measure the reference homeownership rate in the country of origin that is relevant for understanding tenure choices by foreign-born U.S. residents of different ancestry.

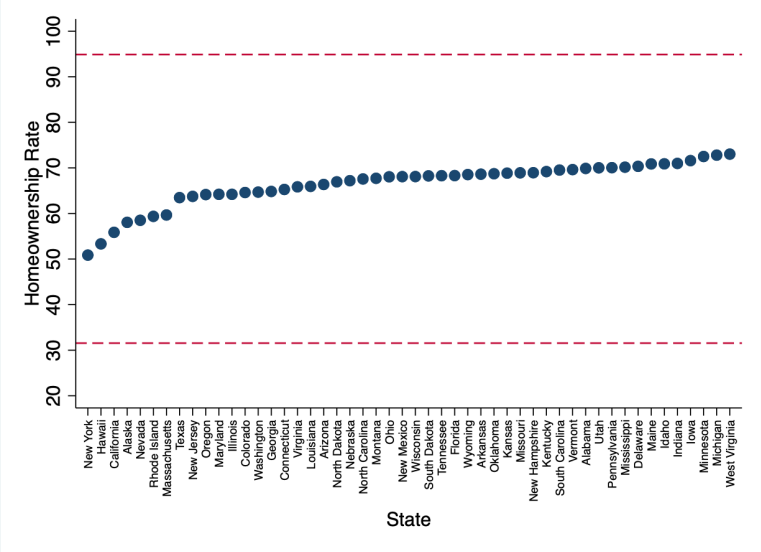
Homeownership rates in country of origin. One of the key concerns in measuring homeownership across countries is whether statistics provided by different national and international organizations measure the same concept and whether the sampled populations are representative of the underlying population. Moreover, to the degree that residents in different life stages will have different peers and reference groups in their home country, we need to take into account heterogeneity in homeownership within countries as well. To address these concerns, our main approach for constructing homeownership rates in the country of origin follows Marcén and Morales (2020) in using harmonized international census microdata from IPUMS (Center, 2020) to construct our own homeownership rates for comparable subgroups within each country. In particular, for each country we retain households where the head is 18-69 years old, and categorize each household further by the marital status of the household head.[2] We compute separate homeownership rates for each marital status in the expectation that homeownership reference points related to these major life events most cleanly transfer to the cultural expectations of U.S. residents. Where countries have multiple census years available in IPUMS, we retain the sample that is closest to the year 2015. After dropping 5 censuses that had implausibly low average homeownership rates,[3] we are left with homeownership data by marital status for 70 countries of origin in the US census data.

This baseline sample contains origin country data covering 72% of the birth places of all the foreign-born residents in the U.S. in the year 2000. Table HI shows example data for the largest country of origin groups in the U.S. covered by this data. Note that variation between households in the mapping to origin-country homeownership rates can come either from variation in the homeownership for different marital statuses in their country of origin, or from variation in the marital status distribution among household heads from that country residing in the U.S.

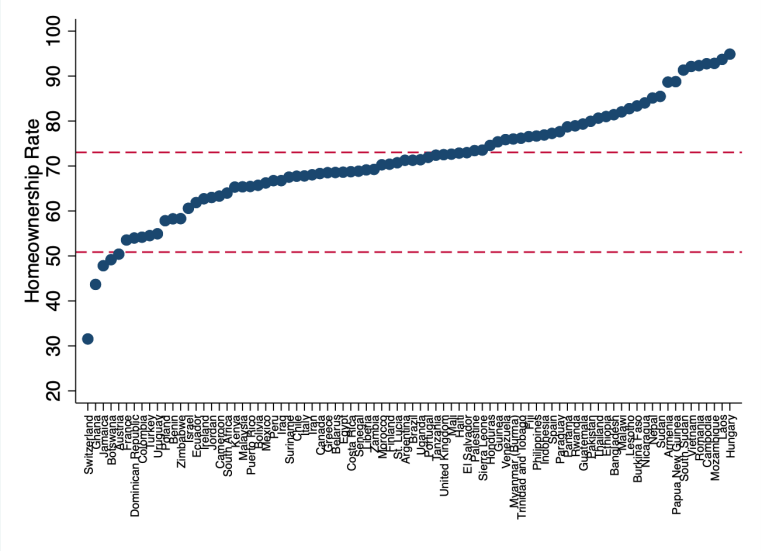
An important advantage of using country origins of foreign-born residents to study the effects of preferences for homeownership is that there is large variation in the cultural backgrounds of foreign-born residents relative to observable differences between U.S.-born residents: Figure 1 highlights the large amount of variation in homeownership when comparing rates across countries, relative to comparing U.S. states. In the U.S., New York stands out as the state with the lowest homeownership rate, at an average of 50.86%, driven by the large population share in New York City, which has a homeownership rate of around 30%.[4] Eight states have homeownership rates below 60%, twelve have rates above 70%, but for the most part, states hover in the 60% region, consistent with a national homeownership rate in the mid-60's. In contrast, there is much more cross-country variation in homeownership. Switzerland takes the bottom spot, with a homeownership rate of 31.5%. An additional 12 countries have mean homeownership rates below 60%. 57 countries have homeownership rates above 70%, and among those, 18 have rates higher than 90%.

Alternative homeownership statistics. As a robustness check, we also hand-collected data on average national homeownership rates (not disaggregated by marital status or age) from various international organizations and government statistics agencies. We were able to find data for 62 countries through this method.[5] In the 50 countries where these two samples overlap, the correlation between the national average homeownership rates obtained through both methods is 87%.

Figure 1: Variation in Homeownership Across U.S. States vs. Countries



(A) U.S. States



(B) Origin Countries

Notes: These graphs show the variation in homeownership rates across U.S. states, among U.S. residents, and 84 origin countries, among populations in those countries. The top graph shows the state-level homeownership rate using the average homeownership rate within a state across the 1970, 1980, 1990, 2000, and 2010 censuses. We also plot the country-level minimum and maximum homeownership rates, for ease of comparison to the bottom graph. The bottom graph draws on the IPUMS harmonized censuses among households with heads aged 18-69, for the available year closest to 2015. We plot the state-level minimum and maximum homeownership rates, for ease of comparison to the top graph. In both graphs, homeownership rates are ordered from smallest (L) to largest (R).

B. Other Origin-Country Characteristics

Country economic characteristics. In order to analyze the drivers of homeownership in origin countries, we also collect a cross-sectional data set of country characteristics from official data sources. In particular, we collect data on GDP per capita, inflation rates, urbanization rates, and domestic credit for 2000-2020 from the World Bank's World Development Indicators. In addition, we also use indices of property rights protection and investment freedom from the Heritage Foundation for the years 2000-2008.

Major religion by country. To assess the role of particular determinants of culture, we also collect a data set of the major religion of each country in our sample. We manually access data from the United Nations Statistics Division, Wikipedia, and the CIA World Factbook (in this order) on a country's population by religion. We combine different religious denominations into the categories Christianity, Islam, Buddhism, Hinduism, and other (which includes Atheists and Agnostics), and assign to each country the category with the largest population share. For the few cases (e.g. China) where official sources are not clear on which of the major religions best describes the majority practice in the country, we assigned "Other".

C. U.S. Tenure Choice and Household Finances

American Community Survey (ACS). In order to measure household-level tenure choices for foreign-born U.S. residents, we use microdata from the American Community Survey for the years 2005-2019.[6] This is a repeated cross-section of a representative sample of U.S. households, which contains ∼3.4 M households with foreign-born heads who are 18-69 years old whom we are able to match to homeownership rates in their countries of origin. The key variable in this data set for our analysis is the measure of whether the household owns the home that it lives in. In addition, this data allows us to measure the characteristics of the head's spouse, including their country of origin, which enables analyses that measure the effect of within-family differences in origin countries on household tenure choices. Moreover, we construct the following control variables at the household and individual level from this data: household income, education of the household head and their spouse, years since immigrating of the head and their spouse, age and gender of the head and their spouse, number of children living in the household, race of the household head, and marriage status.

American Housing Survey (AHS). We augment the ACS data with data from the American Housing Survey. The AHS is also a repeated cross-section, but instead of surveying a representative sample of households, its focus is on a sample of housing units. We use data from 2001-2019, covering ten waves of the biannual survey, which yields 175k households over 585k observations. We have HOCO for about 30k of the households with foreign-born heads. Like in the ACS, we observe many socioeconomic and demographic characteristics of the households, including household income, education of the household head, years since immigrating of the head, age and gender of the head, number of children living in the household, and marriage status. While the ACS microdata provides us with a public use microdata area (PUMA) which we can map to a commuting zone, the AHS data only provides 15 core-based statistical areas (CBSA), or a catch-all non-CBSA code.

The AHS also provides detailed data on mortgage origination and other financial characteristics of the household.

Panel Study of income Dynamics (PSID). One confounder of concern could be that immigrants from high homeownership countries come to the U.S. with higher levels of wealth than those from low homeownership origins. Wealth is not observable in either the AHS or ACS data. As such, we utilize a set of shared socioeconomic, demographic, location, and origin variables to impute wealth, using loadings calculated from the PSID. We use the 1997 and 2017 waves of the PSID to construct household wealth, as these two waves refreshed the PSID household sample, adding new immigrant households to the panel. We then regress household non-housing net worth on a suite of socioeconomic and demographic characteristics, as well as origin and destination fixed effects, to estimate their expected contributions to net worth. Finally, we use these coefficients to construct non-housing net worth estimates in either the AHS or ACS data. See Appendix C for details.

D. Portfolio Allocations

In order to study the long-run household portfolio consequences of an affinity for homeownership, we use data from the **Health and Retirement Study (HRS)**, which is provided by the University of Michigan (2024). This data set is uniquely useful for our question of how tenure choice preferences affect household portfolios in the long run, as it (1) is a nationally representative survey with detailed information about household finances, (2) captures household portfolios at the age of retirement, and (3) contains information about the country of origin of foreign-born participants. We obtain access to the restricted version of the HRS data, allowing us to link current participant location and national origins to their financial information. This data allows us to provide some of the first information about differences in the end-of-working-life portfolios of immigrants from different origin countries.

Within the HRS, we use the RAND HRS Longitudinal data (which combines and standardizes information from different waves) and, in order to make the included households comparable, focus on married / partnered households, for which we retain one person (the financial respondent where applicable). Among persons representing a household, we keep only respondents that are 55-65 years old at the time of at least one survey wave and we find the survey wave that comes closest to them being 60 years old. We construct a number of different variables characterizing respondents' retirement portfolio size and composition as well as real estate ownership, then merge this information with their country of origin information, including HOCO. See Appendix F for further detail on variable construction and data transformation in the HRS.

The key outcome variables that we focus on are whether households own real estate in their retirement portfolio, the value of that real estate, the value of different components of their household wealth and the share allocated to risky assets in the form of stocks.

E. Measuring Credit Supply

In order to measure local mortgage market outcomes, mortgage lending by lender, and to construct mortgage market liquidity shocks we use data collected under the **Home Mortgage Disclosure Act (HMDA)**. We obtain the loan-level data for 2007-2017 from the Consumer Financial Protection Bureau's website, which provides information on the lender and location of the property as well as property and loan characteristics, and supplement it with loan-level data for 1998-2006 available from the National Archives. We match lenders to their parent company using the crosswalk maintained by Robert Avery[7] and aggregate the data at the level of total loans and loan volume by lender-county-year. We retain only first-lien purchase loans that were originated for single-family (1-4 units) site-built properties that are owner-occupied, in order to focus our analysis on the type of lending that is most likely associated with a household's transition into homeownership. We construct a panel of lender-county-year data for 1998-2017.

II. Preferences for Homeownership and Tenure Choice

In this section, we establish that there is indeed an effect of homeownership in the country of origin (HOCO) on immigrants' tenure choice in the U.S., and that this effect is robust to plausible confounding effects. Appendix A provides a simple tenure-choice framework in which a stronger affinity for homeownership lowers the perceived cost of owning and therefore raises the probability of ownership.

In order to establish the baseline effect of homeownership in the country of origin on tenure choice in the U.S., we use household-level data to estimate specifications of the following form, among foreign-born U.S. residents:

$$HO_{ht} = \gamma HOCO_{ht}^{head} + \beta' X_{ht} + \zeta_{CZ,t} + \varepsilon_{ht} \tag{1}$$

Here, $HOCO_{ht}^{head}$ is the homeownership for householders, h, with the same marital status in the country of origin of the household head; X_{ht} is a vector of control variables consisting of household and individual characteristics; $\zeta_{CZ,t}$ represents fixed effects controlling for common variation at the commuting zone or year level, t. In fact, the full specification for most of our analyses flexibly includes commuting-zone-by-year level fixed effects, which control for the fact that immigrants might sort into cities with trends in housing markets or local labor markets that also affect homeownership rates.

A. Results: Baseline household level effects of HOCO on homeownership

To determine the size of the effect of country-of-origin homeownership on tenure choices among foreign-born U.S. residents, we estimate equation 1 in the household level ACS data. The results are shown in Table I. Each column shows the estimated effect of homeownership in the country of origin of the household head, matched by marital status, on whether the household owns their home

Table I: Baseline effects of Homeownership in Country of Origin on Tenure Choice

Dependent var.:	Household Homeownership$_{it}$ (in %)			
	(1)	(2)	(3)	(4)
HOCO (%)	0.897***	0.208***	0.168***	0.153***
	(10.33)	(3.21)	(3.05)	(3.15)
Observations	3,448,546	3,448,436	3,436,300	3,436,252
R-Squared	0.06	0.27	0.27	0.31
	Addl. controls & fixed effects			
Household Characteristics		X	X	X
Imputed HH Wealth			X	X
Commuting Zone × Year FE				X

Notes: This table shows the results of estimating Equation 1 in a pooled household level sample of households with foreign-born heads who are 18-69 years old, for the years 2000 and 2005-2019. The dependent variable is an indicator of homeownership in percent (so 100 indicates that the household owns their home). The independent variable is the homeownership rate in the country of origin (*HOCO*) of the household head for people with the same marriage status (single, married, separated/divorced, widowed). Household characteristic control variables in column 2 consist of: HH Income, (linear and ventile indicators), Quadratic function of Age of HH head and 5-year age group indicators, indicators for educational achievement of HH head, Indicators for # of children and # of relatives in the HH, Indicators for discretized years since immigration of head, marital status of HH head, and the interaction between marital status and years-since-immigration categories. Column 3 adds predicted non-housing household net worth as an additional control variable, and column 4 includes CZ-by-year fixed effects. T-statistics based on heteroskedasticity-robust standard errors clustered at the origin country level shown in parentheses. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

in the U.S. The first column shows the raw association between the two variables, while the second column flexibly controls for a large number of characteristics of the household, such as income, household size, children, years since immigration, and age and education of the household head. These control variables capture other factors that are known to drive tenure choices. Additionally, they address the concern that immigrants from countries with high homeownership rates may be incidentally selected with regard to characteristics that drive higher homeownership rates in the U.S, such as being high-income. That the relationship survives these controls and remains statistically significant, shown in column 2, suggests a direct link between the homeownership experiences in immigrants' origin country and their choices in the U.S.

One important concern is that immigrants from different countries arrive in the U.S. with different levels of (non-housing) wealth that are systematically related to homeownership in their country of origin,and which affect their ability to acquire a home. To control for this possibility, we use PSID data to estimate the relationship between household wealth and a rich set of household characteristics and then apply those estimates in the larger ACS sample to impute household net worth. While this is likely not a perfect proxy for wealth, when we include this control in the estimation in column 3 of Table I, the coefficient is only modestly reduced relative to the one estimated without this wealth proxy. Moreover, the analysis in Appendix Section A showed that the

gap in homeownership between low and high HOCO immigrants is quite stable along the marginal tax rate that households face, suggesting that household finances are likely not a key driver of the gap. Similarly, we show in Section II.B below that the effect is larger, the *longer* a migrant has been in the U.S., which makes wealth upon arrival an unlikely explanation.

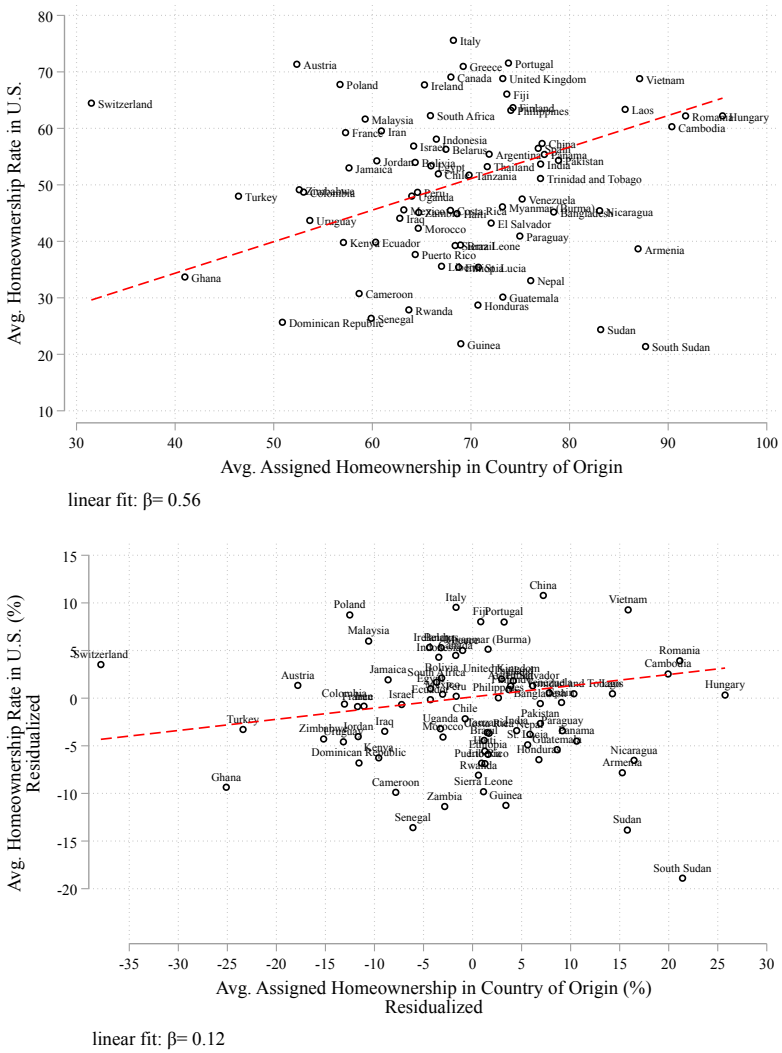
Additionally, we want to control for the possibility that immigrants from higher homeownership countries might be sorting into labor markets or housing markets (here captured by commuting zone boundaries) that are more or less conducive to homeownership on average or which experience rising homeownership rates during the years in the sample. Column 4 therefore includes commuting zone-by-year interacted fixed effects. That is, our preferred estimate in column 4 identifies only off within-labor-market variation in tenure choices within each year between immigrant households that are identical with regard to their observable characteristics noted above, except in the household head's country-of-origin. The estimated elasticity of homeownership with regard to *HOCO* in this specification is 15%, and is significant at the 1% level.

To put this number in perspective, note that it means that the interquartile range of *HOCO* across households in our sample, which is between 58 ppt and 76 ppt, is associated with a 2.8 ppt difference in homeownership rates. For comparison, this represents 44% of the standard deviation in homeownership across U.S. states, or 39% of the gap in homeownership rates between native and foreign-born households, conditioning on all the controls in Table I. This number is also quite similar to findings from Ringo (2020), who, using U.S. *county*-of-origin homeownership rates at age 14 as an instrument for an individual's likelihood of owning a home later in life, finds that a one-standard deviation increase in county-of-childhood homeownership rates is associated with a 2pp increase in homeownership.[8]

To visualize how different countries contribute to this effect, we can aggregate the residualized household homeownership rates and marriage status *HOCOs* to the origin country group level. We plot the relationship between the two variables in Figure 2. The top graph shows the raw averages of homeownership in the U.S. among foreign-born residents, plotted over the averages of the marital status matched *HOCOs*. The lower graph residualizes these variables with regard to the full set of control variables in column 4 of Table I before aggregating. While the marker sizes are not weighted by number of households in the U.S., both graphs use a weighted-linear fit overlaid in the dashed red line, which accounts for differential numbers of immigrants from origin countries. These graphs make it clear that homeownership rates vary widely between countries, with average rates conditional on marital status of more than 90% in Romania, Hungary, and Cambodia at the upper end, and rates below 50% in Turkey, Ghana, and Switzerland at the lower end.

It is reassuring that no obvious grouping of countries by geography or wealth emerges from these graphs. For instance, several low-income countries can be found both among the highest and lowest *HOCO* and U.S. homeownership countries. Moreover, the most visible outliers from the fitted linear relationship in the residualized graph are Switzerland and South Sudan, which only constitute 0.14% and 0.0009% of the U.S. foreign-born population.

Figure 2: Relationship between Homeownership in the U.S. and *HOCO* among Immigrants at the Origin Country Level



Notes: These graphs shows origin country averages of homeownership among U.S. households with foreign-born heads on the vertical axis, and the average of these households' associated homeownership in the country of origin by marital status on the horizontal axis. The left graph shows these averages for the raw ACS data from the years 2000 and 2005-2019, while the right graph residualizes both homeownership and *HOCO* at the household level before aggregating. The control variables for the residualization consist of: HH Income, (linear and ventile indicators), Quadratic function of Age of HH head and 5-year age group indicators, indicators for educational achievement of HH head, Indicators for # of children and # of relatives in the HH, Indicators for discretized years since immigration of head, marital status of HH head, and the interaction between marital status and years-since-immigration categories. The linear fits and slopes in each graph weight each country by the number of foreign-born residents with that origin in the sample. Note that the aggregation means that the regression slopes at the origin country level are merely illustrative here; they are not directly comparable to the household level effect estimates, which should be relied on for the quantitative magnitude of the effect.

B. *Mechanisms*

Sapienza (2020) notes that *"if the behavior is totally economic conditions... immigrants living in the same area... from different countries and different cultures, should, in principle behave the same way. If we observe that they behave differently... this is an opening for arguing that indeed culture plays a role."* Thus far, we have documented significant passthrough of immigrant's origin country homeownership norms to their U.S. decisions, controlling for socioeconomic and demographic characteristics, imputed wealth, and local market conditions; these immigrants with similar economic profiles in similar locations vary in their outcomes. In this section, we borrow from the economics of culture literature to further test whether endowed, exogenous, "cultural" forces or unobserved economic forces best explain the observed patterns.

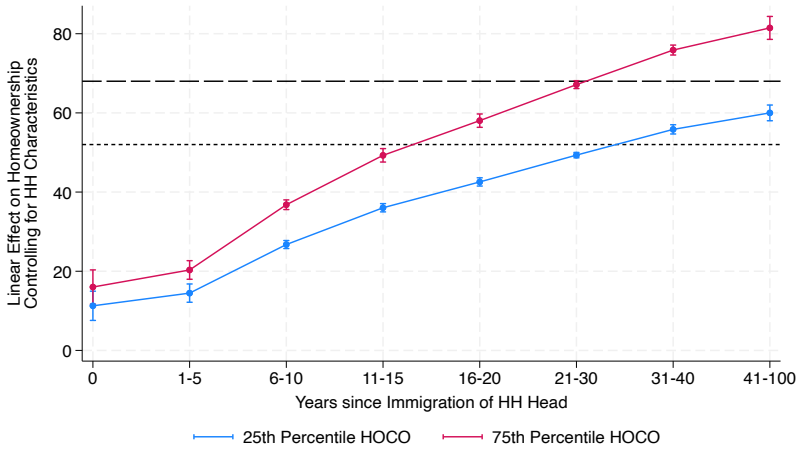
Time since arrival. Blau et al. (2011) note that assimilation profiles can help distinguish between cultural and economic frictions as driving variation between groups. For example, if those households with high and low HOCOs converge in their homeownership rates over time, the average differences we measured are likely masking the fact that economic adjustments take time; these households don't have different cultural affinities, they just have different economic disruption upon arrival. Over time, all immigrants exposed to the same broad economic setting converge to a similar homeownership rate. On the other hand, if immigrants coming from high and low HOCO countries show persistent differences in homeownership rates in the U.S. that do not converge over time, there is likely a persistent cultural friction preventing convergence.

We test these ideas in column 4 of Table II by comparing the *HOCO* effect among immigrants who arrived in the U.S. less than 10 years before the survey, and long-term residents who have been in the U.S. for a period of more than 10 years. The results show that the *HOCO* effect is significantly larger among long-term foreign-born residents, and smaller and not statistically significant among recent immigrants. In addition to showing that the effect lasts even years after arrival, this finding also allays some potential concerns with our *HOCO* effect estimation: on the one hand, this finding is not compatible with reverse causality, where immigrants move to the U.S. *because* they acquire a home, for instance as an investment, which may be driven by real estate dynamics in the country of origin. On the other hand, it makes it less likely that unobserved differences in the wealth of new arrivals from different countries– those not captured by the financial control variables – drive the effect.

We plot the full assimilation profile in Figure 3. The profiles plot the impact of additional years since immigration on a household's likelihood of owning a home. The upper red profile plots the profile at the 75^{th} percentile HOCO, and the lower blue profile the 25^{th}. The long-dashed horizontal line specifies the U.S. mean homeownership rate, while the lower short-dashed horizontal line marks the mean immigrant homeownership rate.

Both profiles exhibit an upward slope, consistent with it taking time to accrue the necessary savings to enter homeownership in a the U.S. setting with downpayment constraints. Both immigrant groups begin their stay in the U.S. with low homeownership rates, at 10-16 p.p. After beign in the U.S. 1-5 years, we already see higher HOCO immigrants diverging from lower HOCO immi-

Figure 3: Foreign Born Homeownership in the US, by Years in US (foreign-born only))



Notes: These graphs show the estimated homeownership rate among foreign-born households by years elapsed since arriving in the US, at the 25^{th} and 75^{th} percentiles of HOCO. We control for commuting zone-by-year fixed effects as well as household income, household head's age and age squared, educational attainment, and marital status, and cluster standard errors by commuting zone and year. Data from the ACS microdata 2005-2019. Horizontal dashed lines represent the mean homeownership rates among households with foreign-born heads (lower) and homeownership rates of households with US-born heads (upper).

grants in their homeownership rates, by 5.5 p.p. Rather than converge as immigrants assimilate into the U.S. economy, these groups' homeownership rates diverge, with persistent differences over the entire profile. While the 75^{th} percentile households attain the same rate of homeownership as U.S. households after 21-30 years, 25^{th} percentile household don't even hit the immigrant average rate until they've been in the country 31-40 years. Similar to Blau et al. (2011), we conclude that the figure provides significant evidence of persistent heterogeneity in cultural norms around homeownership, rather than convergence in economic incentives.

This interpretation is consistent with the structural life-cycle model of Le Blanc, Slacalek, and White (2025), who decompose cross-country homeownership gaps between Germany and four destination countries (Spain, the U.S., France, and Italy). Their decomposition shows that the origin-country homeownership rate serves as a baseline from which destination-country institutions, particularly the rental wedge and house price beliefs, shift behavior. Notably, the origin-country baseline itself is a quantitatively important component throughout the lifecycle. This structural finding mirrors our reduced-form evidence: immigrants carry their origin-country homeownership norm as a persistent reference point, and destination-country conditions do not erase it even after decades of exposure.

Second-generation immigrants. Motivated by the definition of culture adopted by Guiso, Sapienza, and Zingales (2006), "those customary beliefs and values that ... groups transmit fairly unchanged from generation to generation," we further test for evidence of HOCO passthrough among those who immigrated as children, as close as we can get to second-generation immigrants

in our sample. These immigrants did not choose to immigrate based on their own unobservable characteristics, but may nonetheless be culturally influenced by the attitude towards homeownership in their country of origin. While the ACS data does not allow us to identify countries of origin for the parents of residents born in the U.S.,[9] we can identify immigrants who came to the U.S. as children (18 years old or younger), and consider whether our results hold for this subgroup. Table II, column 1, shows that even though the sample size is substantially reduced for this analysis, the pass-through of HOCO to the children's decision to own a home as adults later in life is only modestly attenuated.[10] We estimate that immigrants who arrived as children have a 12% passthrough of homeownership attitudes from their country of origin. This is consistent with a related literature that has shown that an affinity for homeownership is transmitted from parents to children, which in this case ends up propagating the influence of the country of origin.

Origin of the spouse. If culture matters, we would expect married householders' tenure choices to also be influenced by the origin of their spouse. Marrying a U.S.-born spouse might indicate a greater desire to assimilate, which should be associated with a lower importance of cultural factors for the householder. In contrast, if the mechanism for HOCO effects operates through a greater level of sophistication with regard to the U.S. financial system, marrying a U.S.-born spouse would, if anything, indicate a greater ability to exploit this advantage. Column 5 of Table II includes only married householders and interacts the HOCO variable with whether the spouse is U.S.-born: we find that the HOCO effect is almost entirely undone for immigrants that marry a U.S.-born spouse, which seems aligned with a cultural explanation for the effect. If the spouse is not U.S.-born, the householder may also consider the HOCO-based preferences of their spouse when making tenure choices: Column 6 shows the independent effect of the spouse's affinity for homeownership, as well as an interaction for whether both spouses are from the same country. We find that the effect of a foreign-born spouse's HOCO is also significant and statistically indistinguishable from that of the householder.

Gender. An additional dimension of household characteristics that might matter for the transmission of HOCO into tenure choices is the gender of the household head. To explore this possibility, we estimate a specification that interacts the *HOCO* variable with indicators of the householder's gender, The results are shown in column 2 of Table II. We find that the effects of *HOCO* on households headed by men are smaller by about a quarter of the original effect size. While the gender of the householder may be associated with other differences in household structure, the fact that the transmission is significant and large for both households headed by both genders suggests that the effects are likely not driven by differences across immigrant groups in the prevalence of female-headed households.

Education. As higher education enables immigrants to attain more knowledge about their host country and greater exposure to other perspectives, it is commonly associated with higher rates of assimilation (Dustmann, 1996). In column 3 of Table II we explore whether this holds for homeownership effects: we find that the effect of *HOCO* on tenure choice is about a third smaller for college-educated householders.

Table II: Mechanisms for the Effect of Homeownership in Country of Origin on Tenure Choice

Dependent var.:	Household Homeownership$_{it}$ (in %)					
	(1) Immigrated as Child	(2) By Gender	(3) By Educ.	(4) By Years since Immig.	(5) US-born Spouse	(6) Same Origin Spouse
HOCO	0.122***	0.171***	0.178***	0.004	0.240**	0.162**
	(2.96)	(3.57)	(3.77)	(0.05)	(2.31)	(2.25)
HOCO × Male		-0.037***				
		(-4.59)				
HOCO × College Educ.			-0.075**			
			(-2.29)			
HOCO × LT Resident (> 10 Yrs)				0.196**		
				(2.46)		
HOCO × U.S.-born Spouse					-0.229***	
					(-3.26)	
HOCO × Same Origin Spouse						-0.058
						(-0.86)
Spouse HOCO						0.157***
						(3.12)
Observations	1,220,817	3,436,252	3,436,252	3,436,252	2,050,435	1,595,887
R-Squared	0.31	0.31	0.31	0.31	0.29	0.30
	Addl. controls & fixed effects					
Household Characteristics	X	X	X	X	X	X
Imputed HH Wealth	X	X	X	X	X	X
Commuting Zone × Year FE	X	X	X	X	X	X
Same Origin Spouse Indicator						X

Notes: This table shows the estimation results from additional specifications that vary particular aspects of the baseline regression in column 4 of Table I. Column 1 shows the results in a sample that is restricted to householders *who immigrated to the US when they were 18 years old or younger* as inferred from their age and immigration year. Columns 2-4 check for differential impacts by characteristics of the household head: whether the head is male, is college-educated, and whether the head has been in the U.S. longer than 10 years. Columns 5-6 estimate differential impacts by characteristics of the householder's spouse: Column 5 includes all married householders and interacts the HOCO variable with whether the spouse is U.S.-born. Column 6 includes households where the data also have information on the spouse's HOCO, and shows the independent effect of the spouse's affinity for homeownership, as well as an interaction for whether both spouses are from the same country.
T-statistics based on heteroskedasticity-robust standard errors clustered at the origin country level shown in parentheses. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

C. Robustness checks

Appendix D shows that the baseline estimate is stable to controls for origin-country economic and institutional characteristics (inflation history, credit conditions, property rights, cultural proximity), to controls for the average characteristics of each country's migrants, to alternative HOCO measures, to excluding the largest origin groups, and to replication in the American Housing Survey. Bounding the effect of unobservables following Oster (2019) yields a range of [0.12, 0.153] around the baseline. Origin characteristics that could threaten the exclusion restriction, such as savings rates and financial-market development, are uncorrelated with HOCO or bias the estimates

toward zero.

III. Implications for Wealth at Retirement

Many policies supporting or subsidizing homeownership in the U.S. are motivated by the assumption that homeownership leads to greater wealth. Theoretically, this need not be the case. Housing is a risky and undiversified asset. As such, many models imply that households should compensate for this by concurrently investing in safe assets, and lowering their exposure to other risky assets, such as stocks (Cocco, 2005; Yao and Zhang, 2005). Additionally, because households must save for a downpayment, this can preclude investing in alternative assets early on in the life cycle, losing out on many years of compounding returns. On the other hand, the wealth effects from unexpected gains to housing equity can loosen budget constraints enabling investment in other risky assets (Chetty et al., 2017; Sodini et al., 2023). Even in the absence of a diversified portfolio, housing may contribute to wealth formation through forced savings, (Bernstein and Koudijs, 2024; Kovacs and Moran, 2023). The ultimate contribution of housing to one's wealth at retirement is thus *ex ante* unclear, and an open empirical question. In this section, we use our HOCO-driven variation in homeownership to estimate the causal effect of owning a home on retirement portfolios.

A. Identification issues and empirical approach

Why do we need exogenous variation in households' desire to save for a home in order to study the degree to which trying to attain home ownership changes wealth accumulation? Consider two households at retirement: In one of them, the home was inherited from the head of household's parents and was given in part to make up for the fact that the head of household would not receive any cash from their parents. Moreover, this inheritance was communicated in advance, so the head of household consumed more and saved less during their working life in anticipation of inheriting a home. In the other household, no home was inherited. However, because the head of household had always wanted to own their own home, they worked harder and saved additional money throughout their working life in order to be able to buy a house. If we wanted to study whether saving for homeownership is associated with greater wealth at retirement, we need to be able to distinguish between these two households—and the fact that the former household will actually have lower financial resources (other than their home) at retirement *because* they inherited a home would bias our estimates of how wanting to own a home and not being guaranteed one changes behaviors—which is usually the situation of interest.

In this section, we therefore use the exogenous variation in the affinity for homeownership as a laboratory for studying how much the desire to own a home impacts wealth at retirement. Studying households at retirement has additional advantages over our earlier analysis that considered the tenure choices of all households. One potential issue with our earlier analysis showing that an affinity for homeownership causes foreign migrants to be more likely to own their home is that it may take time for a desire for homeownership to translate into realized portfolio choices. Conversely,

a greater desire to own a home might affect the *timing* of when a household acquires a home, but not the long run homeownership rate if we consider the full lifecycle. By focusing the analysis on the eventual outcome of decision during the working life, retirement wealth allows us to quantify the consequences of wanting to own a home for overall household wealth and other financial portfolio choices without having to figure out where in their dynamic wealth accumulation path a working age household is currently. Understanding whether an affinity for homeownership may affect retirement portfolios is also of particular important for policymakers because of the large role that real estate plays in retirement portfolios.

Threats to the exclusion restriction. Using *HOCO* as a source of exogenous variation in the desire to own a home requires that origin-country homeownership rates affect retirement wealth only through their effect on U.S. tenure choice. An omitted variable that violates this restriction must satisfy several demanding conditions simultaneously: it must be (i) *portable*, in the sense that immigrants carry it with them when they move to the U.S.; (ii) *persistent*, remaining relevant through decades of U.S. residence until retirement; (iii) *correlated with HOCO*; and (iv) a *causal contributor to wealth formation* conditional on our controls for education, income, age, and marital status. These conditions eliminate broad categories of potential confounders. Origin-country housing-market institutions, such as maximum loan-to-value ratios, property transfer costs, and tenant protections, which Le Blanc et al. (2025) identify as important drivers of cross-country homeownership differences, do not survive conditions (i) and (ii): they are not portable across borders and have no direct bearing on an immigrant's financial decisions in the U.S. Similarly, country-level development indicators such as GDP per capita and urbanization rates are properties of the origin economy, not of the individual immigrant, and their relevance diminishes conditional on observed U.S. income and education.

To assess what remains, we compute Pearson correlations (r) between *HOCO* and 20 origin-country characteristics spanning economic development, institutional quality, financial-market depth, savings behavior, and cultural attitudes toward homeownership (Appendix Figure II). The strongest correlates are markers of economic development: urbanization ($r = -0.48$, $N = 81$), GDP per capita ($r = -0.41$, $N = 81$), and property rights ($r = -0.37$, $N = 74$). None of those are portable or persistent for individual immigrants.

The characteristics most likely to satisfy all four conditions, namely, savings behavior and financial-market development, are either uncorrelated with *HOCO* or correlated in the wrong direction: gross domestic savings rates ($r = -0.18$, $N = 76$, $p = 0.13$), stock market capitalization as a share of GDP ($r = -0.31$, $N = 50$, $p = 0.03$), and domestic credit provision ($r = -0.10$, $N = 32$, $p = 0.58$) are all weakly negative (Appendix Figure III, panels b–d). Countries with higher homeownership rates have *lower* savings rates and *smaller* financial markets. To the extent that these variables contribute to wealth accumulation but are omitted from our regressions, the resulting bias would push the estimated effect of *HOCO* on wealth *downward*, making our estimates conservative.

Housing-market institutions, including the number of procedures, time, and cost required to

register property, drawn from the World Bank *Doing Business* reports, are uncorrelated with *HOCO* ($r < 0.06$ for all, $N = 78$; Appendix Figure III, panel e).

The only origin-country characteristic *positively* correlated with *HOCO* is the share of survey respondents reporting that they feel insecure without owning a home ($r = 0.44$, $N = 17$, from the 2024 Ipsos Global Advisor survey; panel f), which supports, rather than threatens, the cultural-transmission mechanism underlying our instrument. Adding to the cultural-transmission mechanism, the share of survey respondents agreeing with "I aspire to own my own home" also has a strong positive correlation with HOCO, though this relationship is not statistically significant ($r = 0.33$, $N = 17$)

Having established that plausible confounders either fail the portability and persistence requirements or carry the wrong sign to inflate our estimates, we now turn to how the effect of *HOCO* on U.S. homeownership evolves over an immigrant's tenure in the country.

Convergence of Homeownership. To see how a higher *HOCO* affects homeownership over an immigrant's time in the U.S, we plot the estimated effect of being in different categories of years since immigration on homeownership, controlling for other household characteristics, by *HOCO* in Figure 3. The resulting homeownership curves reveal two insights: on the one hand, time since immigration matters substantially for the level of homeownership—for both low and high *HOCO* householders, homeownership varies by a factor of three between foreign-born householders that recently arrived in the U.S. and those who have been in the country for more than 40 years. Moreover, a higher *HOCO* affects the initial homeownership rates in the first years after arrival, but it has an even greater effect on differences in homeownership in the long run: after several decades in the U.S., high *HOCO* foreign-born households on average exceed the homeownership rates of U.S.-born households, while low *HOCO* households on average have homeownership rates that are 15 pp lower than those of U.S.-born households even in the long run. The graphs also show that the homeownership rate curves tend to flatten after several decades in the country. This analysis motivates the use of retirement portfolio data to get an estimate of the *long-run* financial consequences of wanting to own a home.

HRS data. To estimate the size of *HOCO* effects on household retirement portfolios, we use confidential microdata from the Health and Retirement Study (HRS) that links detailed information about household balance sheets for individuals retiring in the U.S. with their country of origin if they are foreign-born. While the sample size of this data is substantially smaller than the ACS sample, this data is uniquely suited to answering the questions of interest, as by the time that a household reaches retirement age, we can expect their preferences to have been mostly translated into portfolio choices, and we are not aware of any other data set that combines detailed household balance sheet information with country of origin information. By comparing holdings of real estate and other assets upon retirement, we can study the effects of *HOCO* on *long-run* investment choices, and the long-run consequences for the overall wealth and portfolio allocation choices of immigrants. The HRS data and construction of individual variables is described in detail in Appendix Section F.

Estimation specification. Using the HRS data, we estimate regressions of the form

$$Y_h^{age60} = \gamma HOCO_h + \beta' X_h^{age60} + \alpha_t + \varepsilon_h, \tag{2}$$

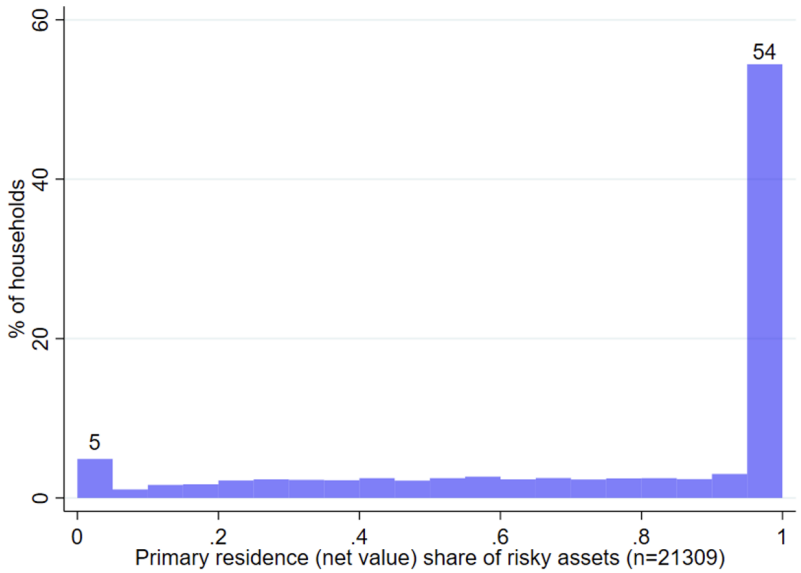
where Y_h^{age60} represents a household level financial characteristic measured in the survey wave where the financial respondent was closest to 60 years old; $HOCO_h$ represents the foreign-born financial respondent's country of origin homeownership rate matched to their marriage status; X_h^{age60} are demographic controls that capture non-HOCO differences between respondents at age 60 and consist of categorical variable dummies for respondent marriage status, age, total household income, a dummy for gender, a dummy for whether they are at least a high school graduate, and a dummy for whether the respondent was born in Mexico; α_t is a dummy indicating the survey wave that the respondent data at age 60 is from. The baseline specification also controls for total non-housing wealth of the household. The country of origin is determined from the location where a respondent was born and we restrict the analysis to respondents who did not live in the U.S. when they were in school, in order to focus on individuals who grew up abroad.

B. Homeownership in the retirement portfolio

Many households seem to show a special preference for real estate when allocating their portfolios at retirement age: Poterba, Venti, and Wise (2011) document that over 80% of households own a home at retirement, and housing represents about half of non-annuitized wealth. We can see this by considering a household's "discretionary" risky asset portfolio, i.e. funds that are not allocated through a retirement account, but rather can be moved freely by the household, and which are invested in either stocks, bonds, or real estate. Figure 4 shows that more than half of all retirees have *only* risky asset investments in the form of their primary residence–and do not hold any stocks or bonds outside of their retirement accounts. Thus, homeownership seems to occupy a special place in the set of asset classes that households could invest in—and cultural affinities for owning a home might be an important driver of this preference.

In order to quantify how much an affinity for homeownership affects realized tenure choices *at the end of the working life* of a household, we first estimate the effect of *HOCO* on whether or not a retiring household owns their primary residence. Column (1) of Table III shows this effect estimate: the estimated raw elasticity of realized retirement-age homeownership with regard to *HOCO* is 0.95 and statistically significant in spite of the relatively small sample size of the HRS. Statistically, we cannot rule out that households end up translating their *HOCO* almost one to one into eventual homeownership. Even with controls for demographic characteristics in column (2), the effect on retirement homeownership is larger than our preferred effect estimates from the ACS microdata, which might be due to retirement age immigrant households having had more time to establish themselves in the U.S. and realize their preferences for owning a home—in line with the time pattern in Figure 3. This result confirms that an affinity for homeownership results in actual differences in tenure choices, even in the long run. Columns (3) and (4) quantify how much the

Figure 4: Primary residence share of discretionary risky assets at retirement



Notes: This figure shows the distribution of respondents at (or near) age 60 in the Health and Retirement Study by the share of their "discretionary" risky assets (stocks, bonds, and real estate assets held outside of pensions or retirement accounts) that consists of the net value of the primary residence.

differences in tenure choice end up biasing the asset allocation in retirement portfolios: a 1 pp increase in a respondent's *HOCO* is associated with 1.7K more of a household's wealth consisting of the net value of their primary residence.

To visualize this effect of HOCO on retirement age tenure choices, and check for non-linearities, consider Appendix Figure IVIII: If we split the respondents with HOCO data into terciles of affinity, homeownership at age 60 increases monotonically with being in a tercile with greater affinity for it.[11]

C. Retirement wealth and portfolio composition effects

To provide a benchmark for our estimates of the effects of HOCO on retirement portfolios, and to document descriptive patterns that, to our knowledge, have not been quantified elsewhere, we first estimate the association of home ownership with different components of retirement portfolios.

Table IV, panel A, column (1) shows that homeownership is associated with significantly higher wealth among all HRS respondents around age 60, suggesting that homeowner households report on average $\sim$ \$212K higher total wealth, of which \$122K is in the form of the net value of the primary residence, as column (2) shows. Home-owning households are also significantly more likely to own stocks than renter households, with a 9.3 pp higher likelihood of participating in the stock market (column 4). Panel B shows qualitatively similar associations among the foreign-born households for which we have *HOCO* data, for whom total wealth among homeowners is larger to similar degree as in the full sample, but a larger share of that increase comes from the net value of the home itself.

Table III: Effect of HOCO on Real Estate Portfolio Holdings Among Foreign-Born at Age 60

Dep. var.:	Homeownership		Net Value of Primary Res.	
	(1)	(2)	(3)	(4)
HOCO	0.945***	0.463**	3121.529***	1716.930**
	(5.484)	(2.262)	(4.782)	(2.090)
Mean of dep. var.	56.1	56.1	110609.9	110609.9
R-squared	0.05	0.17	0.09	0.19
Observations	993	993	993	993
Survey Wave FEs	X	X	X	X
Demographic controls		X		X

Notes: This table shows the results of estimating Equation 2 in a sample of households with foreign-born financial respondents, who are (as close as possible to) 60 years old and did not live in the US while they were in school, in the Health and Retirement Study. The independent variable is the household's financial respondents homeownership in their country of origin (HOCO) matched to the respondent's marital status. The dependent variable in columns 1 and 2 indicates whether the respondent owns a primary residence worth at least $5K. The dependent variable in columns 3 and 4 represents the respondent's net value of wealth in their primary residence. Demographic controls include: categorical variable dummies for respondent marriage status, age, total household labor income, a dummy for gender, a dummy for whether they are at least a high school graduate, and a dummy for whether the respondent was born in Mexico. All regressions also include a dummy indicating the survey wave that the respondent data is from. Note: Standard errors are clustered at the country-of-origin-by-wave level. T-statistics shown in parentheses.

Moreover, foreign-born households show a smaller association of homeownership with stock market participation.

However, even though we are including demographic controls in these estimations, we cannot necessarily interpret these coefficients as causal evidence of the effects of homeownership: for example, households from wealthier families are also more likely to own their homes, and so the causality may be reversed. As we argued before, HOCO, on the other hand, is likely to be an exogenous characteristic from the perspective of individual households and is not driven by the wealth of a household at the end of its working life.

To get closer to estimating a causal effect of homeownership on retiree financial portfolios, we can therefore estimate the reduced-form effect of higher affinity for homeownership on these financial outcomes. We first consider whether high *HOCO* households end up with higher household wealth overall. In Table V, we find that households with higher *HOCO* indeed have significantly higher absolute wealth (column 1) not just in the form of the net value of their primary residence (as we showed in Table III) but also in the rest of their retirement portfolio (Table V, column 2). A 1 pp higher *HOCO* is associated with about a $5.3K higher total wealth, and $3.6K higher wealth in assets other than the primary residence. This effect is large, compared to a baseline average total wealth of $227K and wealth in other assets of $116K in the foreign-born sample. It implies that a standard deviation increase in *HOCO* (12 pp) is associated with increases relative to these averages of 28% and 37%, respectively. Moreover, this effect does not just stem from the extensive margin effects of being more likely to choose homeownership – in panel B, we show that differential affinity for homeownership also affects retirement portfolio wealth significantly

Table IV: Relation between Homeownership and Retirement Portfolios

Dep. Var.	(1) Total Wealth	(2) Net Value Primary Res.	(3) Total Wealth Excl. Primary Res.	(4) 𝟙[Owns Stocks]	(5) Stock Share of Total Wealth (%)
Panel A: Full HRS Sample					
Homeownership (%)	2119.035***	1224.186***	894.850***	0.093***	0.006*
	(9.735)	(10.168)	(7.042)	(8.792)	(1.770)
Mean of dep. var.	269031.3	96229.2	172802.1	16.6	4.3
R-squared	0.27	0.29	0.21	0.14	0.05
Observations	18606	18606	18606	18606	16013
Panel B: Foreign-born HOCO Sample					
Homeownership (%)	2167.273***	1738.517***	428.756**	0.048***	0.005
	(7.635)	(11.517)	(2.230)	(3.105)	(1.261)
Mean of dep. var.	226659.6	110609.9	116049.7	5.9	1.5
R-squared	0.40	0.36	0.33	0.19	0.09
Observations	993	993	993	993	766
Survey Wave FEs	X	X	X	X	X
Demographic controls	X	X	X	X	X

Notes: This table shows the results of estimating Equation 2 among those who are (as close as possible to) 60 years old in the Health and Retirement Study. The sample consists of all households (panel A) or of those with at least one foreign-born spouse for which we have HOCO data, and who did not live in the US while they were in school (panel B). The dependent variables are as follows: in column 1 it is the total respondent wealth; in column 2 the net value in dollars of the respondent's primary residence; in column 3 the total respondent wealth excluding the primary residence; in column 4 an indicator that is 100 if the respondent owns any stocks in their portfolio, and 0 otherwise; in column 5 the share of the respondent's total wealth in % that is invested in stocks (excl. households with negative total wealth, and winsorizing the ratio at 1% in each tail). Demographic controls include: categorical variable dummies for respondent marriage status, age, total household labor income, a dummy for gender, a dummy for whether they are at least a high school graduate, and a dummy for whether the respondent was born in Mexico. All regressions also include dummies indicating the survey wave that the respondent data is from, omitting the 2022 wave, such that all dollars are in 2022 terms. Note: Standard errors are clustered at the country-of-origin-by-wave level. T-statistics shown in parentheses.

conditional on being a homeowner—in fact, more so than in the full foreign-born sample. This is consistent with "wanting" to own a home, as proxied by *HOCO* driving higher accumulation of savings relative to those homeowners who did not acquire a home as a result of a strong affinity.

One potential explanation for this association between HOCO and retirement wealth could be differential preferences for risky assets that have higher returns – either associated with country of origin preferences, or as a result of real estate ownership affecting risk preferences – but in columns (3) we find no significant evidence that higher HOCO is associated with being more likely to own stocks. We do find marginally significant evidence in column (4) that a higher share of the retirement portfolio is being allocated to stocks for high HOCO households. This stands in contrast to the theoretical findings in Cocco (2005) and Yao and Zhang (2005), who suggest housing investment substitutes for risky assets. Instead, these portfolio results are more consistent with Chetty et al. (2017), who notes that this substitution effect works in opposition to the wealth effect for households with growing equity in their homes.

Table V: Effect of HOCO on Wealth and Risky Asset Portfolio Holdings Among Foreign-Born at Age 60

Dep. Var.	(1) Total Wealth	(2) Total Wealth Excl. Primary Res.	(3) 𝟙[Owns Stocks]	(4) Stock Share of Total Wealth (%)
Panel A: All, HOCO Sample				
HOCO	5307.805***	3590.875***	0.142	0.069*
	(3.231)	(2.943)	(1.310)	(1.739)
Mean of dep. var.	226659.6	116049.7	5.9	1.5
R-squared	0.36	0.34	0.18	0.09
Observations	993	993	993	766
Panel B: Homeowners, HOCO Sample				
HOCO	7829.725***	5573.619**	0.223	0.098*
	(2.632)	(2.477)	(0.926)	(1.718)
Mean of dep. var.	372737.6	174876.6	9.5	1.8
R-squared	0.35	0.34	0.20	0.11
Observations	557	557	557	539
Survey Wave FEs	X	X	X	X
Demographic controls	X	X	X	X
School outside US	X	X	X	X

Notes: This table shows the results of estimating Equation 2 in a sample of households with foreign-born financial respondents, who are (as close as possible to) 60 years old and did not live in the US while they were in school, in the Health and Retirement Study. The independent variable is the household's financial respondents homeownership in their country of origin (HOCO) matched to the respondent's marital status. Panel B further restricts the sample to only include homeowners. The dependent variables are as follows: in column 1 it is the net value in dollars of the total respondent wealth; in column 2 the total respondent wealth excluding the primary residence; in column 3 an indicator that is 100 if the respondent owns stocks in their portfolio, and 0 otherwise; in column 4 the share of the respondent's total wealth that is invested in stocks. Demographic controls include: categorical variable dummies for respondent marriage status, age, total household labor income, a dummy for gender, a dummy for whether they are at least a high school graduate, and a dummy for whether the respondent was born in Mexico. All regressions also include a dummy indicating the survey wave that the respondent data is from. Note: Standard errors are clustered at the country-of-origin-survey-wave level. T-statistics shown in parentheses.

Recent research by Sodini et al. (2023) on homeowners in Sweden seems to find a stronger result: being exogenously allocated homeownership after municipalities privatize existing units seems to increase not only housing wealth, but also significantly increases stock market participation. What could explain our more muted findings? While our point estimates are positive, our smaller sample size may lack power for detecting small effects on stock market participation. However, different effects for U.S. residents may also be explained by differences in direct stock market participation across countries: according to the Survey of Health, Aging and Retirement in Europe (SHARE), 44% of married couples in Sweden directly participate in the stock market. On the other hand, according to the Survey of Consumer Finances, only 26% of couples in the U.S do so. Owning a home in the U.S. context does not seem to significantly impact households' stock market participation, perhaps reflecting that for most households in the U.S. the effective barriers to participation outside

of a retirement or pension plan seem to be quite high. However, the difference could also be due to the nature of the variation studied: an increase in HOCO reflects a higher affinity for owning housing in particular, which should limit the tendency to try and diversify into other asset classes—while the allocation of homeownership in Sodini et al. (2023) is not associated with an affinity for the asset class.

Our findings are consistent with concurrent work in the U.S. context studying the impact of losing one's home during the Great Recession, the flip side of the unexpected homeownership studied in Sodini et al. (2023): Artigue, Bayer, Ferreira, and Ross (2024) find that households that marginally kept their homes vs. marginally lost their homes between 2010 and 2013 have 19% higher homeownership by 2022. This persistent difference in homeownership rates does not seem to translate into differences in other long-term outcomes: credit scores, credit balances and auto loans across the two groups are statistically indistinguishable. These findings in combination with our own suggest that homeownership in the U.S. does not necessarily impact risky asset portfolio allocations.

D. Regional housing markets and the HOCO effect on the retirement portfolio

Why does a higher *HOCO* affect *overall* wealth? For many policymakers, the notion that owning a home is a key mechanism for building wealth is assumed to be obvious. After all, those who bought a home in earlier decades are now wealthy! Recall, the White House emphasizing that homeownership is the way to build wealth and pass it on (Biden, Joseph R., 2024). Economists have questioned this assumption, highlighting that the returns to homeownership have been influenced by the good fortune of buying before a rise in house prices—which may be hard to replicate—and may look less favorable when considering the opportunity cost of not investing in other assets that have also seen high returns (Rappaport, 2010; Jordà, Knoll, Kuvshinov, Schularick, and Taylor, 2019). As households endogenously select into homeownership, simple comparisons between homeowners and renters, while common in the policy discourse, are not convincing evidence that homeownership *causes* greater wealth. To the degree that our *HOCO* measure is an exogenous driver of homeownership, conditional on other household characteristics, our results above provide a novel contribution by showing that a greater tendency to own a home caused higher wealth at retirement age.

However, it is important to understand the mechanism for this wealth effect of homeownership. While policy may make it easier or harder to buy a home, for example by lowering downpayments or subsidizing interest rate risk, these levers have limits. Housing policy as mediated through mortgages will only impact wealth creation if housing acts as a savings commitment device as in Kovacs and Moran (2023) or households adjust their consumption and leisure instead of substituting other forms of saving (Bernstein and Koudijs, 2024). If housing wealth accrual occurs instead through appreciation as in Sodini et al. (2023), homeownership policies that vary mortgage design will have less bite in terms of impacting household wealth, if—as is likely—they cannot also ensure favorable housing market dynamics after the purchase.

We can test whether the higher housing and overall wealth associated with a higher *HOCO* are due to homeownership encouraging higher savings, independent of the economic environment, or due to luck in having a fondness for an asset class that happens to have seen high appreciation rates in recent decades. While we do not have information on the exact timing and location of the purchase of the real estate that households own at the time of retirement, we can use the fact that most older households have low geographic mobility: we use the relative cost of housing in the household's current state as of the year 2000 as a proxy for how much saving is required to afford a home.

Mechanism hypotheses. If the dominant mechanism for how an affinity for homeownership affects total wealth is that it leads households to save *more* in order to afford a home, then we would expect overall savings and therefore total wealth to be higher in states that have relative high housing cost. In contrast, it may be the case that an affinity for homeownership was rewarded with a windfall due to economic changes in recent decades—explaining the higher total wealth resulting from it—but otherwise distorts financial decisions inefficiently by moving funds from higher return assets towards concentrated bets on housing. If the latter case plays an important role, we would expect high housing cost states to see a higher distortion in behavior and therefore less positive effects of homeownership on wealth.

In Table VI, we estimate how the association between homeownership and retirement portfolios (panels A and B), and between HOCO and retirement portfolios (panel C) varies across respondents in states with median house prices in the year 2000 that are in the lowest, middle, or high tercile.[12] We find that homeownership is associated with significantly lower total wealth, both in the primary residence and in other assets, and also lower stock market participation, for homeowners in higher housing cost states. This is mirrored by the HOCO effects in panel C: The effects of an affinity for homeownership on total wealth are 90% smaller (column 1) in the highest housing cost tercile than in the lowest cost tercile. A 10 percentage point increase in HOCO in a low cost state leads to an additional wealth increase of $85k, or 37% of the mean total wealth at retirement. In contrast, the same HOCO move in a high housing cost state only yields an additional $8k, or 3.6% increase in wealth. This provides substantial evidence that the financial distortion of allocating a large share of the household's portfolio to homeownership in many cases outweighs the positive effects of the motivation to save more in expensive housing markets in order to afford a home. Put differently, this shows that wanting to own a home has substantially less positive consequences for household wealth in states with high housing costs—making the welfare effects of a policy to encourage households to become homeowners highly dependent on the local econonomic context. This is also consistent with evidence by Hilber and Turner (2014) that changes in housing demand (due to the mortgage interest deduction in their study) tend to have a smaller effect on homeownership in more supply-constrained markets, which closely correspond to the higher housing cost states in our sample.

Housing markets do not just impact household wealth through the difficulty of affording a home, but also through windfalls in the house price changes after acquiring a home. We can estimate the

role of such windfalls, by proxying for the likely housing capital gains that a household experienced with the past house price growth in the state where the household resides at age 60. While we do not know the exact holding period for a household's home, this proxies for the overall price appreciation experienced in their local market.

In Table VII, we explore whether the effect of *HOCO* on the value of retirement portfolios varies with price growth in households' regional housing markets over the previous 20 years, relative to the survey wave year. In these estimations we also control for the level of housing cost in the year 2000.

As shown in Table VII, panels A and B, the association of homeownership with total wealth is significantly larger in states where housing markets experienced high price growth (top tercile). Moreover, that this is likely driven by windfalls due to price appreciation is suggested by the small and statistically not significant effects in column (3) for wealth other than in the primary residence.

However, this association may not be causal: our results in panel C of Table VII show that it is primarily the effect of an affinity for homeownership on non-primary residence wealth that is higher in high house price growth states. It is possible that households extract equity and shift it to other opportunities, leaving total wealth unchanged. Columns (4) and (5) do not support that these capital gains are transferred into stock purchases a la Sodini et al. (2023) or Chetty et al. (2017). Instead, household may use it to finance purchase of second homes (increasing their debt along with the windfall) or undertake risky entrepreneurial activities (Adelino, Schoar, and Severino, 2015; Manso, 2016) (an ex post poor decision should that equity extraction have preceeded the Great Recession), which do not seem to reliably increase their total wealth position. Unfortunately, we cannot observe a household's history of equity extraction in the HRS data to confirm this hypothesis.

These results show the wealth effects of wanting to own a home are highly contingent on local housing market dynamics interacting with household choices—an idea that we explore in more detail in the next section.

IV. Lifecycle Housing Choices and the Costs of Wanting to Own

The previous sections showed that households with higher *HOCO* are more likely to become homeowners, but Section III also showed that this greater attachment to ownership does not translate mechanically into higher wealth at retirement, especially in expensive markets. One reason is that homeownership is not a single decision, but a sequence of choices made over the housing cycle. Households decide when to enter homeownership, how aggressively to respond when credit becomes more easily available, and whether to remain homeowners when financial conditions deteriorate. Each of these decisions can affect the ultimate cost of owning a home. A simple framework in Appendix A provides intuition for these margins: a stronger affinity for homeownership should raise the value households place on owning, making them more likely to enter when financing constraints are relaxed and less willing to exit when ownership becomes financially costly.

Table VI: Effect of HOCO and Homeownership on Retirement Wealth By Regional Housing Cost in 2000

Dep. Var:	Total Wealth (1)	Net Value of Primary Res. (2)	Total Wealth excl. Prim. Res. (3)	𝟙[Owns Stocks] (4)	Stock Share of Wealth (5)
Panel A: HO Effect, Full Sample					
Homeownership x 1[1st Terc. Housing Cost]	3012.115***	1846.614***	1165.500***	0.118***	0.003
	(10.375)	(10.958)	(6.488)	(8.925)	(0.648)
Homeownership x 1[2nd Terc. Housing Cost]	-1037.995***	-797.161***	-240.834**	-0.017*	0.006
	(-7.205)	(-11.533)	(-2.147)	(-1.777)	(1.346)
Homeownership x 1[3rd Terc. Housing Cost]	-1490.048***	-1002.713***	-487.336***	-0.051***	0.006
	(-7.691)	(-10.400)	(-3.367)	(-4.763)	(1.078)
Mean of dep. var.	269471.2	96378.7	173092.5	16.6	4.4
R-squared	0.28	0.34	0.21	0.14	0.05
Observations	18479	18479	18479	18479	15895
Panel B: HO Effect, HOCO Sample					
Homeownership x l[1st Terc. Housing Cost]	3604.801***	2668.851***	935.950***	0.082***	0.003
	(7.986)	(11.313)	(2.940)	(3.423)	(0.298)
Homeownership x l[2nd Terc. Housing Cost]	-2441.175***	-1523.970***	-917.205*	-0.082**	-0.008
	(-4.065)	(-6.014)	(-1.866)	(-2.503)	(-0.710)
Homeownership x l[3rd Terc. Housing Cost]	-2782.042***	-1785.992***	-996.050***	-0.055	0.022
	(-5.864)	(-7.868)	(-2.600)	(-1.590)	(1.383)
Mean of dep. var.	228078.2	111135.3	116942.9	6.0	1.6
R-squared	0.44	0.46	0.34	0.20	0.10
Observations	978	978	978	978	753
Panel C: HOCO effect, HOCO Sample					
HOCO x 1[1st Terc. Housing Cost]	8479.743***	2766.912***	5712.830***	0.216	0.072
	(3.857)	(2.632)	(3.496)	(1.524)	(1.528)
HOCO x 1[2nd Terc. Housing Cost]	-6823.100**	-2117.829*	-4705.270**	-0.229	-0.056
	(-2.403)	(-1.894)	(-2.040)	(-1.515)	(-1.390)
HOCO x 1[3rd Terc. Housing Cost]	-7652.100***	-2309.759**	-5342.341**	-0.037	0.059
	(-2.828)	(-2.023)	(-2.330)	(-0.196)	(0.770)
Mean of dep. var.	228078.2	111135.3	116942.9	6.0	1.6
R-squared	0.38	0.23	0.35	0.19	0.10
Observations	978	978	978	978	753
Demographic controls	X	X	X	X	X
Survey Wave FEs	X	X	X	X	X

Notes: This table shows the results of estimating Equation 2 in a sample of households with foreign-born financial respondents, who are (as close as possible to) 60 years old and did not live in the US while they were in school, in the Health and Retirement Study. The independent variable is the household's homeownership or HOCO (assigned based on the financial respondent, if available, or their spouse, where applicable). The independent variable is interacted with terciles of *Housing Cost*, which is the rank of the state in median house prices as of the year 2000 Census, and assigned to each respondent based on their state of residence at the time. The terciles reflect the distribution among the full sample of respondents at age 60. The dependent variables are as follows: in column 1 it is the respondent household's total wealth; in column 2 it is the net value in dollars of the household's primary residence; in column 3 it is the total respondent wealth excluding the value of the primary residence; in column 4 it is an indicator for whether the respondent owns any stocks, scaled by 100; in column 5, it is the household's share of discretionary stock investments as % of total wealth, for all households with total wealth greater than zero - and winsorized with 1% in each tail. Demographic controls include: categorical variable dummies for respondent marriage status, age, total household labor income, a dummy for gender, a dummy for whether they are at least a high school graduate, and a dummy for whether the respondent was born in Mexico. All regressions also include a dummy indicating the survey wave that the respondent data is from. Note: Standard errors are clustered at the country-of-origin-survey-wave level. T-statistics shown in parentheses.

Table VII: Effect of HOCO and Homeownership on Retirement Wealth By Regional House Price Growth in Previous 20 Years

Dep. Var:	Total Wealth (1)	Net Value of Primary Res. (2)	Total Wealth excl. Prim. Res. (3)	𝟙[Owns Stocks] (4)	Stock Share of Wealth (5)
Panel A: HO Effect, Full Sample					
Homeownership x 1[1st Terc. 20-Yr House Price Growth]	1919.856***	1008.291***	911.565***	0.074***	0.010**
	(13.535)	(21.024)	(7.706)	(8.538)	(2.133)
Homeownership x 1[2nd Terc. 20-Yr House Price Growth]	607.150***	434.305***	172.846*	0.012	-0.006
	(3.553)	(4.023)	(1.714)	(0.999)	(-1.032)
Homeownership x 1[3rd Terc. 20-Yr House Price Growth]	1060.254**	806.282***	253.971	0.052***	-0.007
	(2.434)	(2.836)	(1.461)	(3.244)	(-1.460)
Mean of dep. var.	280532.9	102839.8	177693.1	15.7	4.3
R-squared	0.29	0.34	0.21	0.14	0.05
Observations	15290	15290	15290	15290	13005
Panel B: HO Effect, HOCO Sample					
Homeownership x 1[1st Terc. 20-Yr House Price Growth]	1306.836***	1096.163***	210.673	0.028	0.007
	(5.442)	(9.916)	(1.153)	(1.195)	(0.762)
Homeownership x 1[2nd Terc. 20-Yr House Price Growth]	1183.641**	694.779***	488.862	0.041	0.006
	(2.307)	(4.022)	(1.161)	(1.200)	(0.460)
Homeownership x 1[3rd Terc. 20-Yr House Price Growth]	2405.138***	1943.717***	461.421	0.027	-0.009
	(4.169)	(8.646)	(1.083)	(0.836)	(-0.700)
Mean of dep. var.	227167.2	110595.2	116572.0	5.9	1.6
R-squared	0.43	0.47	0.34	0.20	0.10
Observations	975	975	975	975	750
Panel C: HOCO effect, HOCO Sample					
HOCO x 1[1st Terc. 20-Yr House Price Growth]	2407.688	1508.936**	898.752	0.234*	0.113*
	(1.357)	(2.014)	(0.636)	(1.935)	(1.811)
HOCO x 1[2nd Terc. 20-Yr House Price Growth]	4104.839*	-538.203	4643.042**	-0.156	-0.058
	(1.666)	(-0.531)	(2.305)	(-1.149)	(-0.783)
HOCO x 1[3rd Terc. 20-Yr House Price Growth]	5403.997	885.770	4518.227*	-0.177	-0.082
	(1.571)	(0.527)	(1.800)	(-0.740)	(-1.097)
Mean of dep. var.	227167.2	110595.2	116572.0	5.9	1.6
R-squared	0.38	0.24	0.34	0.19	0.11
Observations	975	975	975	975	750
Demographic controls	X	X	X	X	X
Survey Wave FEs	X	X	X	X	X
Housing Cost Tercile FEs	X	X	X	X	X

Notes: This table shows the results of estimating Equation 2 in a sample of households with foreign-born financial respondents, who are (as close as possible to) 60 years old and did not live in the US while they were in school, in the Health and Retirement Study. The independent variable is the household's homeownership or HOCO (assigned based on the financial respondent, if available, or their spouse, where applicable). The independent variable is interacted with terciles of *20-Year State House Price Growth*, which is the growth rate in percent of the FHFA HPI measure of house prices between the year of the survey wave and 20 years earlier. It is computed at the state level and assigned to each respondent based on the survey year and their state of residence at the time. The terciles reflect the distribution among the full sample of respondents at age 60. The dependent variables are as follows: in column 1 it is the respondent household's total wealth; in column 2 it is the net value in dollars of the household's primary residence; in column 3 it is the total respondent wealth excluding the value of the primary residence; in column 4 it is an indicator for whether the respondent owns any stocks, scaled by 100; in column 5, it is the household's share of discretionary stock investments as % of total wealth, for all households with total wealth greater than zero - and winsorized with 1% in each tail. Demographic controls include: categorical variable dummies for respondent marriage status, age, total household labor income, a dummy for gender, a dummy for whether they are at least a high school graduate, and a dummy for whether the respondent was born in Mexico. All regressions also include a dummy indicating the survey wave that the respondent data is from. Note: Standard errors are clustered at the country-of-origin-survey-wave level. T-statistics shown in parentheses.

This section examines these lifecycle housing choices directly. We first show that households with higher *HOCO* respond more strongly to mortgage credit expansions, suggesting that they are more likely to enter homeownership when financing constraints are relaxed. We then show that these households are also less likely to fall delinquent on their mortgages during the housing bust, indicating that they hold on more tightly to homeownership during adverse shocks. Finally, we study whether this stronger attachment to ownership affects the returns households ultimately realize in housing. Taken together, these results help explain why a stronger desire to own a home need not mechanically generate greater wealth accumulation: the same preferences that increase entry into homeownership may also lead households to buy at less favorable times, remain exposed to housing risk for longer, or bear higher costs in order to avoid exiting ownership.

A. Credit Expansions and Entry into Homeownership

The previous section established that differences in homeownership rates in foreign-born residents' countries of origin pass through into actual tenure choices in the U.S. A key prediction of the framework in Section A is that these differences in affinity for homeownership should also shape how households respond to financial shocks. In particular, households with a stronger desire to own a home should respond more strongly to expansions in mortgage credit, with resulting differences in housing-market outcomes.

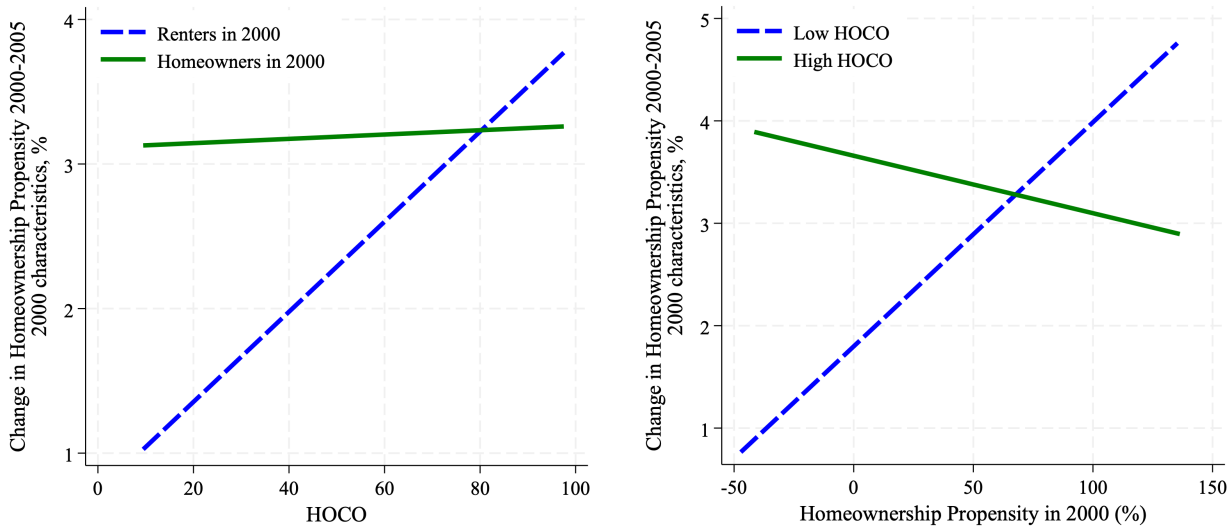
This question is central to the paper because differential responses to credit expansions can have important consequences for household finances. Prior work shows, for example, that minority households in the U.S. saw large increases in homeownership during the mid-2000s housing boom, followed by elevated delinquency and foreclosure risk during the bust (Bayer et al., 2016). More broadly, because homeownership is linked to mobility, child outcomes, and other dimensions of household behavior (Dietz and Haurin, 2003), heterogeneous responses to mortgage credit supply shocks can generate lasting differences across demographic groups. In this section, we show that foreign-born households with higher *HOCO* respond substantially more strongly to increases in mortgage credit supply than do otherwise similar households with lower *HOCO*.

A.1. Who benefited from the 2000–2005 credit expansion?

We begin with a descriptive exercise that provides intuition for the causal estimates that follow. Credit expansions should matter most for households that both want to own and are close to being able to do so; this suggests a particularly strong response among high-*HOCO* renters.

To examine whether this pattern is visible in the 2000–2005 housing boom, we use linear models to predict homeownership in the cross section in either 2000 or 2005 as a function of household characteristics, excluding *HOCO* itself.[13] Applying the 2005 coefficients to households' 2000 characteristics and comparing those predictions to predictions based on the 2000 coefficients gives a measure of how much easier it became to attain homeownership between 2000 and 2005 for households with a given set of characteristics.

Figure 5: Change in Homeownership Propensity during the 2000-2005 Credit Supply Expansion by HOCO



(A) HO advances by HOCO and Tenure

(B) HO advances by HO propensity in 2000

Notes: These graphs show how the homeownership propensity associated with particular demographics changed during the 2000-2005 boom as a function of different household characteristics. Each panel shows estimated linear fits between a household characteristic and the change in the homeownership propensity for different subgroups of foreign-born household heads. The propensity to own a home is the linear predicted homeownership rate for a household using the following demographic variables for the prediction: county fixed effects; Real HH Income (linear and ventile indicators); Quadratic function of Age of HH head and 5-year age group indicators, indicators for educational achievement of HH head, Indicators for # of children and # of relatives in the HH, Indicators for discretized years since immigration of head, marital status of HH head, and the interaction between marital status and years-since-immigration categories. The change in the homeownership propensity 2000-2005 in both graphs uses ACS 2000 data, and uses coefficients for predicting homeownership propensity estimated separately using cross-sectional year 2000 data and year 2005 data for foreign-born headed households to predict how likely a household with those characteristics would be to own a home. The change in this propensity holds household characteristics constant at their 2000 level and estimates the predicted change in homeownership due to changes in the coefficients between the two time periods. Low and High HOCO groups in the right panel are defined as households with foreign-born heads with HOCO below or above the median among all foreign-born heads. Renters and homeowners in the left panel are defined based on 2000 characteristics. Both panels exclude Mexican-origin household heads. The linear prediction for homeownership propensity does not constrain homeownership propensity to range from 0 to 100%.

Figure 5, panel A, shows that households with higher *HOCO* were more likely to have characteristics associated with increased homeownership during the boom. This pattern is especially strong for renters: high-*HOCO* renters—households that were culturally inclined to want to become homeowners but had not yet done so—experienced the largest increase in predicted homeownership. Panel B shows that this pattern was strongest among households that appeared more constrained before the boom. Among households with a low predicted probability of owning in 2000, those with high *HOCO* experienced a much larger increase in predicted homeownership between 2000 and 2005 than similarly constrained low-*HOCO* households. [14]

Taken together, these patterns suggest that high-*HOCO* renters were particularly likely to

benefit from the 2000–2005 credit expansion, and that this was especially true among households that were initially more constrained in their ability to buy a home. We next turn to a causal analysis of whether exogenous increases in mortgage credit supply generate larger increases in homeownership among high-*HOCO* households.

A.2. Estimating differential responses to exogenous credit supply shocks

To identify the causal effect of mortgage credit expansions on homeownership, we use county-level mortgage credit supply shocks based on local exposure to lender-specific national shifts in mortgage lending, following Gilchrist, Siemer, and Zakrajsek (2018) and Garcia (2020). Intuitively, counties that are more exposed to lenders whose national mortgage originations expand should experience greater local credit availability for reasons that are plausibly unrelated to local housing demand. We construct these shocks in stacked long differences over 2000–2005, 2005–2010, and 2010–2015, and further purge them of local lending-demand conditions. Appendix Section G provides the full construction, identifying assumptions, and geographic validation of the shock.

We first verify that the shock is relevant for local mortgage activity by estimating county-level regressions of the form

$$\Delta \ln \text{Loans}_{ct} = \alpha_c + \alpha_t + \beta \tilde{S}_{ct} + \gamma' X_{ct} + \epsilon_{ct},$$

where $\tilde{S}_{ct}$ is the county-level credit supply shock, and the specification includes county and period fixed effects as well as commuting-zone-by-period fixed effects.

We then estimate differential responses of homeownership across groups within counties over time. Because the ACS is a repeated cross section rather than a panel, we aggregate the household data into county-group-period cells and study changes in local homeownership rates. We define five groups: U.S.-born, high-*HOCO* foreign-born, low-*HOCO* foreign-born, Mexican-born, and foreign-born with no *HOCO* data. High and low *HOCO* are defined relative to the population-weighted median *HOCO* among non-Mexican foreign-born households. For each county-group-period cell, we compute the average homeownership rate using ACS population weights.

At the county-group-period level, we estimate

$$\Delta HO_{c,g,t} = \phi \tilde{S}_{c,t} + \beta_g \tilde{S}_{c,t} \times \mathbb{1}[HOCO_g] + \alpha_c + \alpha_t + \zeta_{c,g} + \psi_{CZ,t} + \varepsilon_{c,g,t}, \tag{3}$$

where $\Delta HO_{c,g,t}$ is the change in homeownership for group g in county c over period t, $\mathbb{1}[HOCO_g]$ indicates the different demographic groups, α_c and α_t are county and period fixed effects, $\zeta_{c,g}$ are county-group fixed effects, and $\psi_{CZ,t}$ are commuting-zone-by-period fixed effects.

A.3. Affinity for homeownership and sensitivity to credit supply shocks

Table VIII reports the results. Columns (1) and (2) show that the credit supply shock has a positive and significant effect on county-level mortgage originations, confirming that the shock is relevant for local lending activity. Columns (3) and (4) show that, on average, positive credit supply shocks also increase local homeownership rates.

Columns (5) and (6) show that these effects are highly heterogeneous across groups. The omitted category is the low-*HOCO* foreign-born group, so the first-row coefficient captures the baseline effect of the shock for low-*HOCO* foreign-born households, while the interaction terms show how responsiveness differs for the other groups.[15] Across specifications, foreign-born residents with higher affinity for homeownership are significantly more responsive to credit supply shocks than are low-*HOCO* foreign-born households. By contrast, the responses of U.S.-born residents and low-*HOCO* foreign-born residents are positive but imprecisely estimated.

Comparing columns (5) and (6) to columns (3) and (4) shows that much of the total homeownership response to mortgage credit supply shocks is driven by the high-*HOCO* foreign-born group. Quantitatively, the coefficient in row 3 of column (6) implies that an exogenous 1 pp increase in national loan growth by locally active lenders raises homeownership among the high-*HOCO* cohort by nearly 4 pp. In the data, the five-year change in the homeownership gap between high- and low-*HOCO* households is 0.86 pp, while a one-standard-deviation credit shock of 0.28 increases this gap by 1.06 pp over a five-year period. These results suggest that households with a stronger affinity for homeownership take up credit expansions much more aggressively than other households.

Table VIII: Credit Supply Shocks and Homeownership by HOCO

Dependent var.:	Δ Log Loans$_{c,g,t}$ (in pp)		Δ Homeownership$_{c,g,t}$ (in pp)			
	(1)	(2)	(3)	(4)	(5)	(6)
Nonlocal Lending Shock$_{c,t}$	0.451***	0.404***	1.691***	0.918**	1.481	0.468
	(12.23)	(9.98)	(6.60)	(2.42)	(0.74)	(0.24)
Native $\times$ Nonlocal Lending Shock$_{c,t}$					0.193	0.458
					(0.09)	(0.23)
High HOCO $\times$ Nonlocal Lending Shock$_{c,t}$					3.914**	3.860**
					(2.10)	(2.06)
Observations	7,908	7,617	34,044	34,043	34,044	34,043
R-Squared	0.88	0.96	0.24	0.31	0.24	0.31
	Fixed Effects & Controls					
Period FE	X	X	X	X	X	X
County FE	X	X				
County $\times$ Group FE			X	X	X	X
CZ $\times$ Period FE		X		X		X

Notes: Columns 1-2 show county-by-period panle estimation results and columns 3-6 show estimation results using a sample of county-period-group cells, where the periods are 2000-2005, 2005-2010, and 2010-2015. Groups include: US natives, foreign-born w/out HOCO, above-median HOCO, below-median HOCO (reference group), and Mexican-born. Coefficients for Mexican and no-HOCO foreign born groups omitted for legibility. The dependent variable is the county (columns 1-2) or group-level (columns 3-6) change in lending or homeownership rates in percentage points. *Nonlocal lending shock* is a credit supply shock for the local number of mortgage loans originated. T-statistics based on heteroskedasticity-robust standard errors clustered at the commuting zone level shown in parentheses. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

B. *Holding On in Downturns: Mortgage Delinquency*

Why do homeowners with a high *HOCO* experience smaller declines in homeownership during the housing bust? While we would expect homeownership to increase during periods of easing credit supply, we do not necessarily expect homeownership to fall when credit dries up. Only those with adjustable or floating rate mortgages will experience the immediate effect of a contraction in credit through rising mortgage rates, but as the overall economy deteriorates, households' financial circumstances may also take a hit. Whether households exit from homeownership as a result depends on whether they are willing and able to endure the hardship associated with not defaulting on mortgage payments.

In Table IX, we explore how *HOCO* affects whether foreign-born households default on their mortgages over time, using data from the American Housing Survey. Using data from 2013-2017,[16] we regress an indicator for having at least one missed mortgage payment in the past three months on *HOCO*, whether the household bought at the peak of the credit cycle in 2006-2007, and the interaction of the two. We also include CBSA-by-year fixed effects, detailed socioeconomic and demographic controls, controls for years the householder has lived in the US, as well as controls for debt-to-income and house cost-to-income ratios, which capture the degree of financial pressure that the household may be experiencing over the cycle. This analysis captures whether households with comparable finances behave differently in terms of mortgage delinquency depending on how attached they are to remaining a homeowner.

In column (1) of Table IX, we find that a 1 pp higher *HOCO* is associated with 8% fewer missed payments, suggesting that overall, higher-affinity households seem to be more committed to paying their mortgage. In column (2), we split the sample by those who purchased at the peak of the cycle and explore heterogeneity by purchase date and *HOCO*. The raw delinquency rate in this sample of households that purchased at the peak of the boom is 11.6%, more than twice that of households that bought in other years, which is 5.6%.

We find that most of the variation in missed payments is driven by those households who purchased at the end of the housing boom; these households bought homes when they were most expensive and therefore often suffered large declines in their equity positions that may have resulted in their mortgages being underwater. We find that, for mortgages not originated at the peak of the boom, *HOCO* is still estimated to lower delinquency, but this estimate is no longer statistically significant. However, having a high *HOCO* significantly lowers delinquency rates among households that bought at the peak, undoing a substantial share of the peak buying effect: a one standard deviation higher *HOCO* (which corresponds to a 12 pp difference in this AHS sample) is associated with a 4.4 pp lower delinquency rate for peak boom-originated mortgages,[17] or a decline equal to 38% of the total delinquency rate for properties bought in those years. These results, in combination with Figure 6, suggest that high-affinity households have a high tendency to move into homeownership in all parts of the housing cycle and also hold on tighter to their homes during hard times than other households during market downturns. The same effects are shown visually in Appendix Figure IVII.

Table IX: Delinquencies and HOCO, by Purchase Period

	(1) $\geq$ 1 Missed Pmt.	(2) $\geq$ 1 Missed Pmt.
HOCO	-0.080**	-0.049
	(-2.02)	(-1.27)
Bought 2006-2007		0.317***
		(2.70)
Bought 2006-2007 X HOCO		-0.366**
		(-2.43)
R^2	0.032	0.039
Observations	4043	4043
	FEs & Controls	
CBSA X Year	X	X
Demographics	X	X
Years in US	X	X
CTI & DTI	X	X

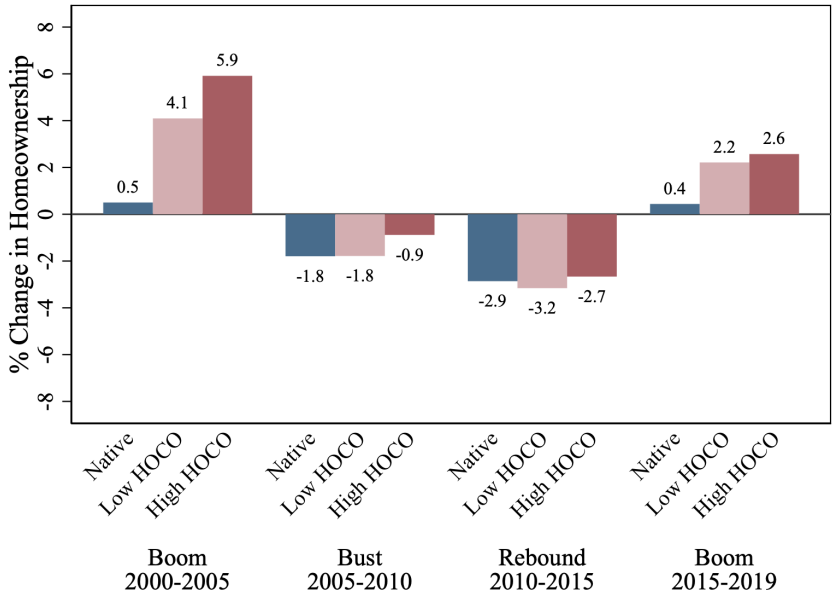
Notes: Data from the 2013 and 2017 waves of the AHS. Sample covers households with foreign-born heads. Left-hand side variable for columns (1) and (2) is whether household has missed at least 1 mortgage payment in the past three months. Missed payments are rare events among these populations, with a missed payment rate of 6.13%. Controls include household head education, race, ethnicity, marital status,, number of family members and children in the home, and quadratics in household income, years in the US of household head, and age of household head, as in prior specifications and add debt-to-income and cost-to-income ratios. We include year-by-CBSA fixed effects. T-statistics based on heteroskedasticity-robust standard errors clustered at the commuting zone level shown in parentheses. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

An important caveat, however, is that holding on tighter to homeownership during a downturn need not always improve broader long-run outcomes. Recent evidence on distressed borrowers during the Great Recession finds that households who were able to remain homeowners accumulated substantially more housing wealth, but experienced little long-run improvement in credit scores, consumption, or neighborhood quality relative to otherwise similar households who lost their homes after becoming seriously delinquent. In that light, a high *HOCO* may not only reduce mortgage delinquency; it may also induce households to remain in owner-occupied housing even when doing so is privately costly in the short run, and when the long-run benefits outside of housing wealth are limited. Our findings therefore suggest that a stronger attachment to homeownership can help households preserve ownership through adverse shocks, but may also lead some households to absorb substantial financial strain in order to remain in expensive homes.

C. Housing Cycle Timing and Realized Returns

How does the effect of a desire to own a home on homeownership vary across the housing cycle? The results above may raise the question of whether the *HOCO* effects that we find are

Figure 6: Homeownership Change by HOCO and Period

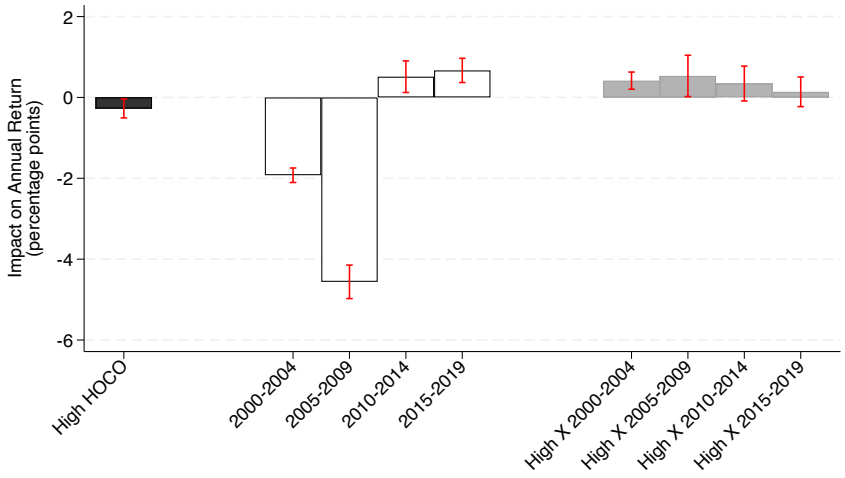


Notes: This figure shows the average change by period in the homeownership in percentage points among U.S.-born American residents, and foreign-born residents with a homeownership in their country of origin (*HOCO*) that is below-median or above-median. The graph and the assignment into HOCO groups exclude Mexican origin foreign-born and residents where we were unable to assign a *HOCO* score.

a phenomenon that is limited to housing booms. Table VIII shows that high-affinity household took up mortgage credit disproportionately over 2000-2015, when it was *relatively* more available in their county. Over this period, the national housing market also experienced severe boom-bust cycles in the *levels* of credit availability and in house prices that caused substantial dislocation in housing markets. A stronger affinity for homeownership may shape choices over the entire housing cycle, making households both more likely to buy in a broader set of market conditions and more likely to hold on during downturns.

In Figure 6 we show this differential responsiveness to the cycle visually in the raw homeownership changes by group. We break up the sample into the same periods that are also analyzed in the regression analysis: boom (2000-2005), bust (2005-2010) and rebound (2010-2015); and we additionally show data for the renewed boom period 2015-2019 that followed. The figure shows that the desire to own a home systematically affected group level behaviors across different parts of the housing cycle. During the housing boom, a time of expanding credit, we see that homeownership changes the most among high-HOCO groups, increasing by almost 6%. During both the housing bust and initial rebound period, as credit markets recovered from the financial crisis, and homeownership was falling nationwide, it was falling *the least* among the high HOCO group, and then again rose fastest during the 2015-2019 period, albeit at only less than half the rate observed in the prior boom, growing at 2.6%. This is consistent with mortgage credit supply generally being

Figure 7: Differential Annual Returns, by Year of Home Purchase



Notes: This figure shows the additional annual returns to homeownership for above-median HOCO households, relative to below-median HOCO households, by era of home purchase. The annual returns are imputed by first computing the annual appreciation in the real value of the home by converting the ratio between the current reported real (CPI-adjusted) market value of a respondent's home in the American Housing Survey in a particular year and the original real purchase price of the home into an annual growth rate in the real house price. Then, we run regressions that predict this expected annual appreciation on HOCO group-by-purchase period indicators and other variables, including the standard socioeconomic and demographic controls used in other specifications, as well as CBSA by year fixed effects. To control for differential returns to housing by quality, we additionally control for bedrooms, bathrooms, lot size, square footage, the year built, presence of a garage, and the unit size (i.e. single family, duplex, small multifamily). The differential returns shown in the figure correspond to the estimated coefficient estimates for the HOCO group-by-purchase period indicators in this prediction of annual returns due to real price appreciation.

less available after the Great Recession—but, nonetheless, high HOCO groups show a consistently higher tendency for moving into homeownership in all parts of the cycle.

While holding on more tightly to one's home may stabilize or improve the consumption flows that residents receive from owning their homes, they do not necessarily improve their financial returns. In Figure 7, we plot the annual housing return differentials by period of purchase, and HOCO group.

We compute predicted returns to holding a home as the annual appreciation in the real value of the home in American Housing Survey data since the time that a household bought the home that they are living in. The coefficients shown in the figure show what share of these returns is predicted by the HOCO group of the current owner and the original purchase period. These return differentials control for the same household and demographic characteristics, commuting zone by year fixed effects, and wealth as in Table I. Additionally, to allow for differential returns to houses of varying qualities, we control for standard hedonic characteristics including bedrooms, bathrooms, acreage, square footage, the presence of a garage, the year the unit was built, and unit type.[18]

The omitted group is foreign-born households with below-median HOCOs who bought in the

years before 2000. Historically, relative to low-HOCO households, high-HOCO households realized 27 basis points (bps) lower returns each year if they purchased prior to 2000. The second to the fifth bar in the figure show the baseline changes in returns for low-HOCO households that bought in later time periods. They clearly show the strong effect of differential timing relative to the real estate market cycle: homes bought during the boom years before the height of the recession in 2009 have low returns, whereas homes bought before and during the recovery in house prices during the 2010s experienced higher appreciation.

High-HOCO households who bought during the 2000s ended up realizing relatively higher annual returns than their low-HOCO peers, on the order of 42-53 basis points, but this advantage became smaller and not statistically significant for those buying during the 2010s. Importantly, these differential gains for high HOCO households are swamped by the negative returns realized for the cohort of buyers who purchased in the 2000s, which was on the order of 1.9-4.6 percentage points. Overall, a desire to own a home does not yield meaningfully different housing returns on average. Even though the greater willingness to hold on to one's home does seem to have modest benefits for the high-HOCO cohort that bought during the 2000s boom, these benefits do not outweigh buying at the wrong time. This importance of buying in the right market will resurface again in the next section when we analyze the implications for retirement portfolios of an affinity for homeownership.

D. The payoff to owning depends on when and where you bought

The returns in Section IV.C are gross: they tell us what a house bought in a given year was worth later, not what the household that bought it ended up with. Owning also commits the down payment, carries taxes, maintenance and interest that exceed rent in most years, and forgoes whatever those dollars would have earned elsewhere. To see whether the home equity that high-affinity households accumulate is wealth they would not otherwise have had, we need to net the two sides against each other over an entire ownership spell, and we need to do it for households who bought at different ages, in different states, into different rate and price environments.

Figure 8 does this with a simple thought experiment. Two households born in 1955 are identical in income and non-housing consumption. One buys the median standardized house in its state at the purchase age on the horizontal axis—at 25 in 1980, at 40 in 1995, at 55 in 2010—with 20% down and a 30-year fixed mortgage at that year's Freddie Mac rate, and holds it to age 60. The other rents the same house for the same years and invests every dollar by which its housing outlay falls short of the owner's. The owner refinances the way households actually do, slowly and incompletely: whenever the prevailing rate falls two points below the note rate, the balance is refinanced a year later (Berger, Milbradt, Tourre, and Vavra, 2025). House values follow the state's FHFA index, rents follow the state's hedonic rent-to-value ratio, property taxes are the rates recent buyers in the state paid (with Proposition 13 in California), and owner deductions are valued under the tax law of each year.[19] The three panels report, in 2019 dollars at age 60, the owner's wealth outside the house minus the renter's, the owner's home equity (the renter has none), and their sum. The two rows differ only in what the renter earns on the invested difference: a behavioral renter who earns

3.1% real (reflecting a portfolio allocation of 35% of a 7% equity return and 65% of a 1% deposit return), and an equity renter who earns 7%.

Three features of the figure matter for what follows. First, home equity is always large and positive and outside wealth is always negative: the owner finishes with $40,000 to $460,000 in the house and $20,000 to $640,000 less outside it than the renter twin. Home equity is not wealth creation until it is netted against the saving the renter did instead. Second, the net is a coin flip whose odds are set by timing and location. Against the behavioral renter, owning beat renting on average for the 1955 cohort only at purchase ages 40 and 45—the mid-1990s—and lost at every other age; at every age the spread across the eleven largest immigrant-destination states is $250,000 to $360,000. A Texan who bought at 40 finished $96,000 ahead of the renter twin; a Californian who bought at 50 finished $319,000 behind. A household that bought at 28, the age our retirement analysis assumes, finished $24,000 behind on average across all fifty-one states, ahead in half of them and behind in the other half. Third, against a renter who invests well, owning almost never wins: at a 7% real alternative return, the owner's deficit outside the house exceeds the equity inside it for nearly every state and purchase age, by as much as $1.7 million for a 1980 purchase in California. Refinancing helps—it adds $70,000 to $125,000 to the totals of households who bought into the 13% rates of the early 1980s—but it does not change the shape of the figure.

In sum, households from high-affinity origins buy earlier, respond to credit when it loosens, hold on through downturns and default less; each of those behaviors is a way of paying for ownership, and Figure 8 says that whether the payment was recovered depended on the state of the housing cycle and the local market at the moment it was made, which no household controls. Affinity does not know what year it is. That is what makes the question of the next section both interpretive and practical: if the decision to own is driven by a preference rather than by its financial terms, then households who hold the preference will buy in good years and bad, with or without a subsidy, and the households a subsidy must reach are the ones who do not.

V. Who Is the Marginal Homebuyer? The Price of Wanting to Own

The lifecycle evidence of Section IV paints a consistent picture: households from high-affinity origins buy earlier, respond to credit when it loosens, hold on through downturns, and default less. Figure 8 showed that each of these choices was a bet whose payoff depended on when and where it was placed. Taken together, the results raise a question that is as much about policy as about interpretation. Nearly every instrument in the US housing toolkit—the mortgage interest deduction, the government guarantee of long-term fixed-rate mortgages, FHA and down-payment assistance—works by lowering the price of owning relative to renting. Whether such a policy reaches anyone depends on how many households are near the margin at that price, and whether it reaches the *right* households depends on who those marginal households are. If the decision to own is driven mostly by a preference for owning rather than by its financial terms, the households who hold the

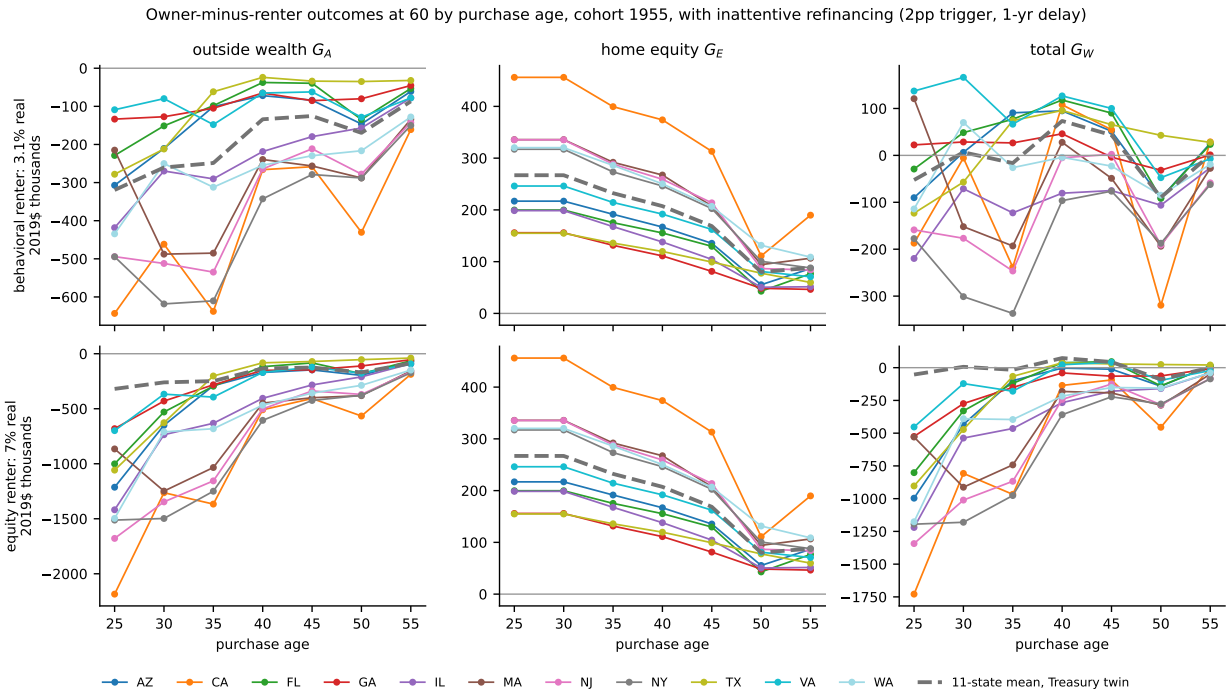


Figure 8: Owner-minus-renter wealth at age 60, by purchase age and state (households born 1955)

Notes: Each panel compares a household that buys the state's median standardized house at the purchase age on the horizontal axis and holds it to age 60 with an otherwise identical household that rents the same house and invests the cash-flow difference; both have the same income and non-housing consumption. Left: wealth outside the primary residence, owner minus renter—the owner's account is debited at purchase for the 20% down payment and 2.5% buying costs and each year for the excess of mortgage interest and principal, property tax, maintenance and depreciation (2.0%) and insurance (0.35%) over rent, net of the income-tax saving from owner deductions. Middle: home equity, the marked-to-market house value less the mortgage balance, no selling cost. Right: the sum. Rows differ in the renter's return on the invested difference: the behavioral renter earns 3.1% real (35% of a 7% equity return and 65% of a 1% deposit return); the equity renter earns 7% real (Jordà et al., 2019). The grey dashed line is the eleven-state mean when the renter earns the 10-year Treasury yield. The owner refinances the remaining balance whenever the prevailing 30-year rate is at least two points below the note rate, with a one-year delay and a 1% fee (Berger et al., 2025). House values: 2000 median tenure-neutral house value by state projected with the FHFA all-transactions index; rents: state hedonic contract rent-to-value ratios anchored in 2000 and 2017 and in the 1970–1990 censuses; mortgage rates: Freddie Mac PMMS in the purchase year; property taxes: state effective rates among owners who moved in within two years, with Proposition 13 in California; taxes: parametric federal and state schedules by law year. Thousands of 2019 dollars. States shown are the eleven largest foreign-born destinations.

preference will buy with or without the subsidy, in 2009 when entry was cheap and in 2006 when it was not, and the subsidy's dollars will go to them; the households it is meant to move are the ones without the preference, and a price cut moves them only a little.

This section puts a dollar value on the preference, estimates how it varies across origin groups, and asks how much of that variation the home-country norm explains. Subsection V.A builds the two objects the exercise requires—the price of owning relative to renting that each household faced when it chose, and the responsiveness of tenure to that price—and uses them to recover an origin-specific premium for owning. Subsection V.C draws out what the estimates imply for who a homeownership subsidy reaches.

A. The price of wanting to own

The structural wedge and what it leaves out. Quantitative models of housing cannot reproduce the observed homeownership rate from financial fundamentals alone. A risk-neutral household facing the user cost of owning and the rent on the same house is roughly indifferent, and once the models add borrowing constraints, transaction costs, moving shocks and house-price risk, too few households own. The literature closes the gap with a wedge that favors owning, calibrated to hit an aggregate ownership moment. The wedge takes two forms (Appendix Table HV summarizes how three recent papers define, identify, and size it).A *rental-sector cost* makes renting more expensive than owning the same unit: Kaplan et al. (2020) give rental companies a per-unit operating cost, chosen so that the model matches the 39% ownership rate of households under 35, and the calibrated value is just under 1% of the housing stock's value per year. A *utility premium* makes owning more valuable than renting the same unit. Kaplan et al. (2020) let an owner-occupied house deliver ωh units of services with $\omega = 1.015$, chosen to match the 66% aggregate rate in 1998, and report the implied consumption-equivalent gain for the median owner at about half a percent of consumption. Mabille (2023) and Coven et al. (2026) use an additive utility shifter for owners, calibrated respectively to regional ownership rates of 69% and 67% in low- and high-price metros and to California's 61% rate; neither is expressed in dollars.[20]

Three features of this approach are worth drawing out, because they define what our exercise adds. First, the wedge is identified from a single aggregate moment at a point in time—the national or regional ownership rate in a calibration year—so it is a scalar by construction. Second, it is a residual: it absorbs whatever the model's frictions do not explain, and its size therefore depends on which frictions the model includes. Third, and most important for us, it is assumed to be the same for every household. The ownership premium that these models need is exactly the object our reduced-form results say is heterogeneous: households from different origins, living in the same markets with the same incomes and family structures, make systematically different tenure choices. If the premium differs across groups, matching the aggregate rate recovers a weighted average that tells us little about who is on the margin. We use that heterogeneity to estimate the distribution of the premium rather than its mean, and to ask how much of the distribution is cultural.

Framework. We use the tenure-choice model of Appendix A. A household h owns when its premium for owning, γ_h, covers the annual financial cost of owning relative to renting the same house, Δ_h, up to an idiosyncratic shock:

$$\Pr(\text{own}_h) \;=\; \Lambda\!\left(\frac{\gamma_h - \Delta_h}{\sigma}\right), \qquad \gamma_h = \gamma_{i(h)} + \varepsilon_h, \tag{4}$$

where Λ is the logistic function, σ is the scale of everything that moves tenure other than its price, and γ_i is the component of the premium common to households from origin i. Two ingredients are needed to take (4) to data: the price Δ_h, and the exchange rate between probabilities and dollars, which is the response of ownership to Δ_h, $\beta \equiv \partial \Pr(\text{own})/\partial(\Delta_h/P)$, with $\sigma = \bar{P}(1-\bar{P})/|\beta| \cdot \bar{P}^*$.

We measure Δ_h as a share of the value of the house, P_h^*, because both the cost of owning and the premium for owning scale with the house a household occupies, so that the normalized gap is the price of owning a given house rather than that price times its size—the unit in which the structural literature states its wedge and in which β is not confounded by the fact that households who want larger houses are also more likely to own them.

The price of owning. Δ_h is the ex-ante annual user cost of owning the house an observationally similar owner in the same commuting zone occupies, net of the contract rent on that house:

$$\Delta_h \;=\; P_h^*\Big[\, r_t\theta + r_t(1-\theta) + \tau_c + (m+\delta) + \text{ins} + \rho - g_{s,t} + \tfrac{F}{T} \,\Big] \;-\; \text{tax}_h \;-\; \kappa_c(P_h^*)\, P_h^*. \qquad (5)$$

P_h^* is predicted from demographics among owners in the household's commuting zone, so that renters and owners are priced on the same bundle; $\kappa_c(P)$ is the contract rent-to-value schedule from separate rent and value hedonics (Coven et al., 2026); r_t is the 30-year mortgage rate in the year the household moved; τ_c is the effective property-tax rate paid by recent owners; tax_h is the household's own-minus-rent tax liability from TAXSIM under the law in force; ρ is a 2% housing risk premium (Himmelberg, Mayer, and Sinai, 2005); $g_{s,t}$ is the trailing 25-year real appreciation in the household's state plus 2.5% expected inflation; and F/T amortizes three sales over a 32-year horizon, the number implied by the owner move-in hazard in our own data. Table X lists each component by era. The cost parameters are those of the ledger behind Figure 8—maintenance and depreciation 2%, insurance 0.35%, buying and selling costs 2.5% and 7%, the same rents and the same refinancing rule—so that the ex-ante price here and the ex-post payoff there are built from one set of inputs; the user cost adds a risk premium and expected mobility, which belong in a decision price and not in a realized accounting, so that the premium we recover is not simply a relabeling of frictions the structural models carry explicitly.

The sample is recent movers—households in both tenures who moved into their current unit within two years of the survey—in the 2000 Census and the 2005–09, 2010–14 and 2015–19 ACS, 1.96 million households in 741 commuting zones. Recent movers are the households whose tenure reflects a decision made at the current Δ_h: a stayer's tenure is a decision made years earlier at a different price and rate, held in place by transaction costs and a locked-in mortgage. Restricting to movers also puts natives and immigrants on the same footing, since both have just chosen. Under this price, owning carried an ex-ante penalty for most movers in every era: Δ_h was positive for 96% of movers in 2000, when interest rates were 8%, and for 57% in 2015–19, when they were under 4% (Table X). This is the configuration in which the structural models need a premium, to justify high homeownership rates observed in the data.

The exchange rate. Because β converts every ownership gap into dollars, we take it from a quasi-experiment rather than estimate it inside the model. Bhutta and Ringo (2021) study the January 2015 cut in the FHA's annual mortgage insurance premium, a 50 basis point reduction in the effective rate on 96.5%-LTV loans that was unanticipated and sharp. Home purchases among

FHA-reliant borrowers rose 14%, an extensive-margin semi-elasticity of about 19 per percentage point of rate, with no response among high-income borrowers; scaled to the whole market their estimates imply a 4 to 12% increase in purchases per point. In our units—one point of rate is 0.8 to 0.97% of house value in Δ_h/P_h^*, and 31% of movers own—this is β between -1.6 and -4.7 per unit of house value for the market as a whole and -6.2 for the constrained borrowers who drove their result, or σ between \$12,000 and \$35,000 per year, one to three times the imputed rent on the tenure-neutral house. We set $\beta = -1.7$, the low end of the market-wide range, so that the dollar magnitudes below are the largest consistent with their evidence; at the high end every dollar figure shrinks by a factor of 2.8, and the ordering of origins, the share of the premium explained by *HOCO*, and the pass-through in ownership units are unchanged.[21]

From ownership gaps to dollars. With β in hand, the premium for origin i is recovered from a single regression on the mover sample,

$$\text{own}_h \;=\; \sum_i \alpha_i \,\mathbf{1}[\text{origin}_h = i] \;+\; X_h'\theta \;+\; \lambda \log P_h^* \;+\; \mu_{c(h),t(h)} \;+\; e_h, \tag{6}$$

which is the paper's Equation (1) with origin indicators in place of *HOCO*, estimated on natives and immigrants together, with US-born households as the omitted group. X_h contains the controls of Table I—income and income quartile, age, marital status, children, family size, education—plus bins for years since arrival, so that α_i is net of the assimilation profile; $\mu_{c,t}$ are commuting-zone-by-year effects, so that α_i compares an origin group with natives who face the same prices, rents, rates and rental stock. The coefficient α_i is therefore a probability: the ownership gap, in percentage points, between group i and observationally similar natives who moved into the same market in the same year. The premium is that gap priced at the market's elasticity,

$$\tilde{\gamma}_i \;\equiv\; \gamma_i - \gamma_{US} \;=\; \frac{\alpha_i}{|\beta|}\,\bar{P}^*, \tag{7}$$

the annual subsidy that would close group i's ownership gap with natives if tenure responded to it at rate β.[22]

We report $\tilde{\gamma}_i$ rather than γ_i for a reason that the discussion of σ makes clear. The level of any group's premium absorbs the average borrowing constraint, liquidity need and expected mobility in its market; differencing against natives who moved into the same market in the same year, with the same demographics, removes the part of those frictions that is common. What remains in $\tilde{\gamma}_i$ is the part specific to the origin group—its taste for owning, but also any origin-specific difference in credit access, wealth, or expected horizon that the controls do not capture. We return to this below; it is the reason $\tilde{\gamma}_i$ should be read as a compensating variation, and the reason we test whether it behaves like a cultural trait before calling it one.

Origin premia. Figure 9 plots $\tilde{\gamma}_i$ for the origins with at least 2,500 mover households, at the lower bound of the β range (\$26K scale); at the upper bound every bar shrinks by a factor of about

Table X: Ingredients of the premium: the price of owning and the exchange rate

	2000	2005–09	2010–14	2015–19	Source
Panel A. Annual user cost of owning, % of house value (recent movers)					
Mortgage interest, r_t (move year)	7.44	6.13	4.28	4.09	Freddie Mac PMMS
Property tax, τ_c (buyers $\leq$2 yrs)	1.13	1.08	1.27	1.21	IPUMS PROPTX/VALUEH
Maintenance + depreciation	2.00	2.00	2.00	2.00	Poterba (1984); Himmelberg et al. (2005)
Insurance	0.35	0.35	0.35	0.35	Keys and Mulder (2024)
Housing risk premium, ρ	2.00	2.00	2.00	2.00	Himmelberg et al. (2005)
Expected appreciation, $-g_{s,t}$	-3.07	-3.32	-2.61	-3.63	FHFA trailing 25-yr real + 2.5%
Transaction costs, F/T (3 sales / 32 yrs)	0.89	0.89	0.89	0.89	Woodward and Hall (2012); Barwick, Pathak, and Wong (2017); ACS hazard
Tax saving, $-\text{tax}_h/P^*$	-0.72	-0.51	-0.30	-0.24	TAXSIM-35
User cost	**10.02**	**8.62**	**7.88**	**6.67**	
Contract rent, $\kappa_c(P^*)$	6.94	5.89	7.02	6.55	Hedonic, Coven et al. (2026)
Premium Δ/P^*, mean	**+3.08**	**+2.73**	**+0.86**	**+0.12**	
Share of movers with $\Delta > 0$	0.96	0.94	0.68	0.57	
Movers (household $\times$ CZ pieces)	340K	371K	538K	712K	
Panel B. Exchange rate $\beta = \partial \Pr(own)/\partial(\Delta/P^*)$ and scale σ					
FHA MIP cut, market-wide: $\beta \in [-4.7, -1.6]$; $\sigma \in$ [\$12K, \$35K]					Bhutta and Ringo (2021)
FHA MIP cut, FHA-reliant borrowers: $\beta = -6.2$; $\sigma =$ \$9K					Bhutta and Ringo (2021)

Panel A: weighted means over recent movers (moved into the unit within two years) in the 2000 Census and three ACS five-year files; the house is the tenure-neutral bundle P^* (mean \$202K). Mortgage and equity shares both carry r_t (the marginal down-payment dollar displaces mortgage debt). Panel B: $\sigma = \bar{P}(1-\bar{P})\bar{P}^*/|\beta|$ with $\bar{P} = 0.31$.

four, and the ranking is unchanged. The dispersion is large. Across 56 origins with a measured *HOCO*, the size-weighted standard deviation of $\tilde{\gamma}_i$ is \$6,000 per year, 3% of house value and 46% of the imputed rent on the house, with a tenth percentile of $-\$5,300$ and a ninetieth of $+\$8,700$. Vietnamese movers behave as if owning were \$18,100 a year cheaper for them than it is for natives; Chinese movers \$13,700; Canadians \$5,100; Mexicans \$3,900; Filipinos \$2,400. Puerto Rican and Indian movers behave as if it were \$5,600 and \$9,100 a year more expensive.[23] Measured in the literature's units, the cross-origin dispersion in the premium alone exceeds the entire average wedge that Kaplan et al. (2020) require, and it is dispersion that a single calibrated parameter cannot represent.

Two cautions about what $\tilde{\gamma}_i$ contains. First, it is a lower bound on the taste for owning for any group that is more constrained than natives. A household that cannot qualify for a mortgage cannot express its preference through tenure, so its group's ownership gap understates its premium; because foreign-born movers fail a payment-to-income test far more often than natives (45–67% against 31–49%), and because the ACS does not observe the liquid wealth that a down payment requires, the bias runs toward renting for immigrants as a whole.[24] For high-premium groups the true premium is therefore larger than Figure 9 shows; for the groups with negative premia, part

of the deficit is a constraint rather than a distaste. The constraint and horizon channels cannot, however, be the whole of the dispersion: among immigrants who arrived before age ten—who grew up in US schools and housing markets, face the same credit system and horizons as natives, and in most cases hold the same documents—the premia are as dispersed across origins as among adult arrivals ($5,900 against $6,500 per year). Second, $\tilde{\gamma}_i$ is scaled by β estimated on the full mover population. Foreign-born movers respond less to the premium than natives ($\beta = -1.1$ against -1.8), which in the model is what a binding constraint looks like: fewer households at the margin. Converting immigrant gaps at the immigrant elasticity would raise every $|\tilde{\gamma}_i|$ by about 60%.

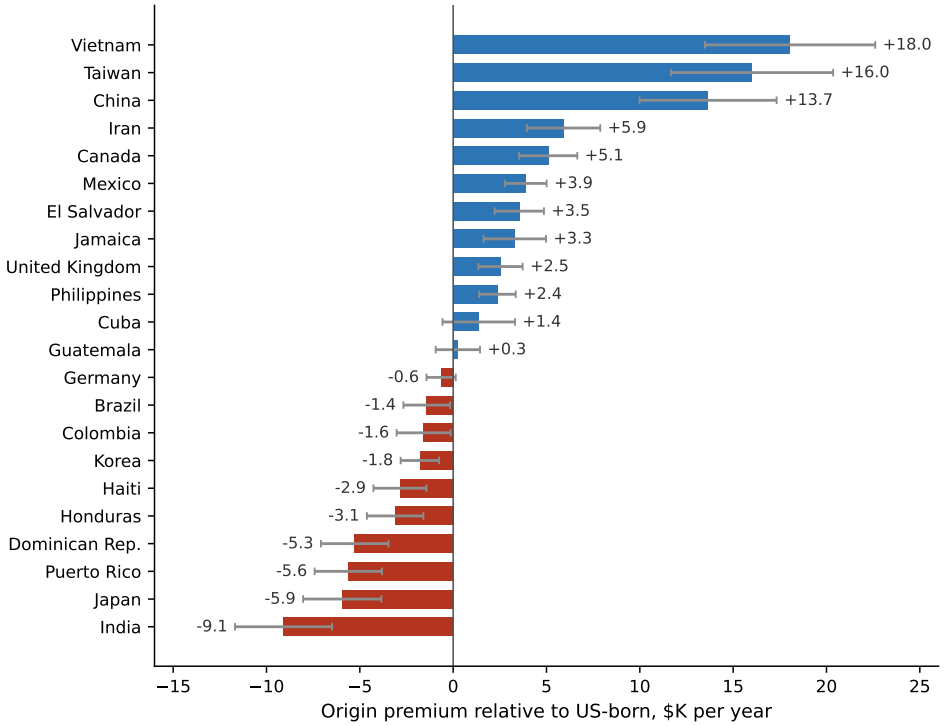


Figure 9: The premium for owning, by country of origin

Notes: $\tilde{\gamma}_i = \alpha_i \bar{P}^*/|\beta|$ from Equations (6)–(7) on recent movers in the 2000 Census and 2005–19 ACS, with $\beta = -1.68$; US-born households are the reference group. Bars show origins with at least 2,500 mover households; whiskers are delta-method standard errors that include the uncertainty in β. At $\beta = -6.2$ (national expected appreciation, or the FHA-reliant response in Bhutta and Ringo, 2021) all values shrink by a factor of 3.7.

How much of the premium is cultural? Figure 10 plots $\tilde{\gamma}_i$ against the origin country's homeownership rate. The slope is $156 per year per point of *HOCO* (s.e. $93), so the interquartile range of *HOCO* across origins, 69 to 82, corresponds to about $2,000 a year in the premium, roughly $40,000 over a working life at a 3% real discount rate. In ownership units the same regression gives 0.13 percentage points per *HOCO* point (s.e. 0.08), consistent with the 0.153 of Table I: the premium exercise reproduces the paper's first stage from a different sample and a different specification. *HOCO* explains 5% of the variance in $\tilde{\gamma}_i$ across origins, about 10% after

correcting for sampling error in the estimated premia. The remaining dispersion is origin-specific but not a home-country norm: networks, enclave housing markets, selection on who migrates, and the constraint and horizon differences discussed above.

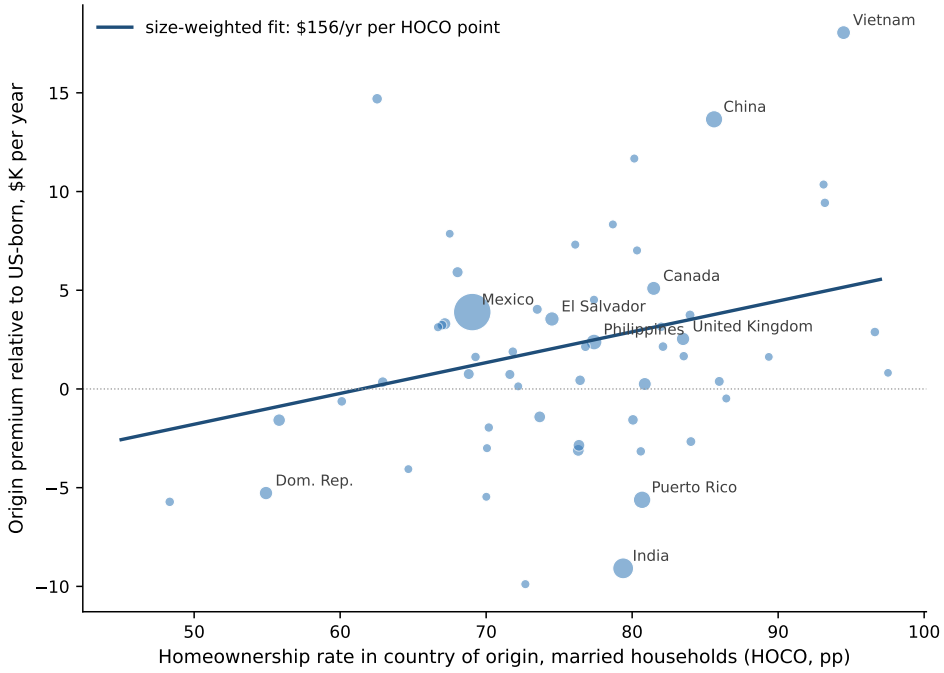


Figure 10: The premium for owning and homeownership in the country of origin

Notes: Each marker is an origin with at least 400 mover households and a measured *HOCO* (56 origins); marker area is proportional to the group's weighted size. The line is the size-weighted fit, \$156 per year per *HOCO* point (s.e. \$93), $R^2 = 0.05$. *HOCO* is the homeownership rate of married households in the census closest to 2015. Dropping India and Puerto Rico, origins with high rates of return, raises the slope to \$284 (s.e. \$65).

What carries into the policy discussion. Three facts from this subsection do the work in the next. Tenure responds weakly to its price: β between -1.7 and -6.6 per unit of house value, a scale σ of \$12,000 to \$35,000 per year, inside the range implied by the FHA premium cut. The premium for owning is large and widely dispersed across origins—a standard deviation of \$6,000 per year, or 3% of house value—and it is a lower bound where groups are more constrained than natives. And the part of that dispersion traceable to the home-country norm is about a tenth of it, worth roughly \$2,000 a year across the interquartile range of *HOCO*, with the rest origin-specific but not cultural. Each of these is a statement about where the margin is and who sits on it.

B. What owning does for those who own: the intensive margin

The premia of Section V.A describe the extensive margin: who becomes an owner, and what they were willing to pay to do so. A growing literature studies the other margin—what owning does for a household's balance sheet once it owns—and its findings are the ones a price subsidy

ultimately buys for the households it moves. Three channels organize that literature: forced saving through amortization, exposure to appreciation, and the opportunity cost of the capital committed. Our results speak to each, and they help reconcile several recent papers that appear, at first glance, to offer different views of how homeownership builds wealth; in practice they identify different margins of variation, and therefore different channels through which housing affects household balance sheets.

One channel is forced saving through amortization. Bernstein and Koudijs (2024) study a mortgage-market reform in the Netherlands that increased required amortization for first-time buyers. They show that faster principal repayment raises net worth nearly one-for-one, not because households rebalance other savings upward, but because they cut expenditures and increase labor supply. In their setting, housing wealth rises because the mortgage contract itself functions as a commitment device, a commitment device so valuable, its benefits often outweigh the gains of flexible equity extraction (Kovacs and Moran, 2023).

A second channel is capital gains through exposure to appreciation. Sodini et al. (2023) study a privatization setting in which households are quasi-randomly assigned into ownership under favorable terms. That design bundles tenure choice with access to a housing asset that can subsequently appreciate, so wealth can rise even absent a broader increase in saving effort. This is the mechanism that most closely matches our own evidence. We find that higher homeownership affinity increases homeownership at retirement and raises housing wealth, but that these effects are concentrated in places with strong prior house-price growth. Once we allow homeownership affinity to interact with regional housing-market dynamics, there is little remaining evidence that higher affinity raises other components of wealth through a general savings channel.

Our results therefore suggest a third, more limited channel: portfolio commitment without large net wealth creation. A greater desire to own a home pushes households toward housing on the extensive margin and increases their exposure to housing-market risk, but by the end of the working life this does not mechanically translate into substantially higher total wealth, especially in expensive markets. Instead, the wealth consequences of ownership appear to depend heavily on whether households happen to own in places and periods with unusually favorable price appreciation. As we note in the policy discussion, this implies that homeownership by itself is unlikely to be a reliable wealth-building policy lever unless it is coupled either with stronger savings incentives conditional on ownership or with favorable market dynamics after purchase.

This interpretation is also consistent with the broader long-run return evidence. In the United States, post-1980 real returns have been substantially higher for equities than for housing: 9.09 percent versus 5.66 percent (Jordà et al., 2019).[25] Thus, even when housing appreciates, shifting wealth into owner-occupied housing can still come with a meaningful opportunity cost relative to alternative assets. This matters particularly in our setting, where higher-affinity households become more exposed to housing but do not show commensurately stronger evidence of deliberate non-housing saving.

Even measured housing gains may overstate the true net return to owner-occupation. Stan-

dard repeat-sales house price indexes do not fully account for renovation expenditures, and recent evidence from Goodman and Choi (2026) argues that this can lead such indexes to overstate appreciation because improvement spending is capitalized into subsequent sale prices rather than netted out as a cost. This reinforces a broader point of interpretation: gross housing appreciation is not the same as net wealth creation. Once one accounts for maintenance, renovation, transaction costs, and the lower average return on housing relative to equities, it becomes easier to understand why a greater propensity to become a homeowner need not generate large differences in total late-life wealth, even if homeownership raises housing equity itself.

This channel-based interpretation helps reconcile the existing evidence, but it also underscores how modest our estimated total-wealth effects are relative to what standard portfolio models would suggest. Theoretical work in Cocco et al. (2005) and Yao and Zhang (2005) imply that owner-occupied housing, because it is large, risky, illiquid, and typically leveraged, should meaningfully reshape the rest of the household balance sheet by crowding out other risky assets. Chetty et al. (2017) confirm this empirically, showing that higher mortgage debt depresses stockholding while higher home equity raises it through a wealth effect. Yet in our setting, a higher affinity for homeownership translates into more housing at retirement without comparably strong evidence of a broader reallocation into risky financial assets. This suggests that the primary consequence of homeownership affinity may be increased exposure to undiversified housing risk rather than a large change in total long-run wealth.

Read together with Section V.A, these channels locate the wealth effects of homeownership on the intensive margin, among households who own and hold, rather than in the act of becoming an owner. That is also where our own evidence places them. The premium for owning predicts the behaviors the literature identifies as the ones that build wealth—buying earlier, staying longer, paying down the mortgage—and the mechanical return to switching tenure is close to zero on average: a household that bought at 28 finished within $25,000 of its renter twin across states (Figure 8), while the retirement wealth effect of a *HOCO* point is $1,700 to $3,100 in home equity alone. A policy that moves households across the extensive margin by lowering the price of owning therefore buys them exposure to these channels, not the holding behavior that made the exposure pay off for the households who wanted to own. The next subsection asks how many households such a policy moves, and who they are.

C. Policy implications: who a homeownership subsidy reaches

Begin with the arithmetic of a price subsidy. In the tenure model of Section V.A, a subsidy of s dollars a year lowers every household's Δ_h by s and raises the ownership rate by $\bar{P}(1-\bar{P})\,s/\sigma$; the share of its cost that goes to households who would have owned anyway is $\bar{P}/(\bar{P}+\Delta_h P)$. Consider a subsidy of $2,600 a year, the average value of the mortgage interest and property tax deductions to a mover who itemized in our pre-2018 samples. With σ between $12,000 and $35,000, it raises ownership among recent movers by 2 to 5 percentage points, and 87 to 95% of its cost goes to inframarginal households; on the stock of households, where two-thirds already own, the

inframarginal share is 93 to 97%, and the fiscal cost of each additional owner-year is $20,000 to $100,000. This is the quantitative content of the weak price response: the households who take up a homeownership subsidy are overwhelmingly the ones who were going to buy. Bhutta and Ringo (2021) reach the same conclusion from the other direction: the response to the FHA premium cut was concentrated among borrowers at a debt-to-income cliff and absent among high-income borrowers. Hilber and Turner (2014) reach it for the deduction itself: in their core specification the entire deduction raises ownership attainment by a net 3.2 percentage points, with gains for itemizing households where housing supply is elastic offset by losses where it is tight and the subsidy capitalizes into prices, at a cost of $28,000 per converted homeowner per year.

The premia of Figure 9 say who sits on the margin and who does not. In the model, the density of households a subsidy can move in group i is $P_i(1-P_i)/\sigma$: largest for groups near 50% ownership, smallest for groups far above or below it. Groups with a large positive premium are, at any given price, mostly inframarginal; a subsidy pays them to do what they were doing. Groups with a negative premium are the ones a subsidy would need to reach, and Section V.A showed that for several of them—Indian and Puerto Rican movers most clearly—much of the deficit is a constraint or a short horizon before returning to their origin rather than a distaste, which a price cut does not address and which a down-payment constraint makes binding regardless of price. The same logic applies to the *HOCO*-linked part of the premium: moving across the interquartile range of *HOCO* is worth about $2,000 a year, one to two mortgage interest deductions, and it moves ownership by the same two to three percentage points a deduction would, at no fiscal cost. Culture is not a policy lever, but it is a benchmark for one: it shows what a given dollar shift in the premium buys in ownership, and the answer is small.

Two further comparisons close the circle with the retirement results. The present value of the *HOCO*-linked premium is about $3,200 per *HOCO* point at the behavioral renter's 3% real rate ($2,700 at the real mortgage rate, $2,000 at the equity renter's 7%); the effect of a *HOCO* point on home equity at age 60 in the HRS is $1,700 to $3,100 (Table III). Households from high-affinity origins were willing to pay, in annual premium, about what owning returned them in home equity. That return did not come from the act of switching tenure—the owner-minus-renter net for a household that bought at 28 is roughly zero on average across states (Figure 8), so the mechanical contribution of the induced ownership to home equity per *HOCO* point is essentially nil—but from how high-premium households own. Across origins, each $1,000 of annual premium is associated with one percentage point more ownership at ages 25–34, 0.05 more years in the current home among owners, and 0.7 percentage points more owners with no mortgage, holding *HOCO* and years in the United States fixed. The object that prices the decision to own also predicts buying earlier, staying longer and paying off—the lifecycle behaviors of Section IV whose payoff Figure 8 shows to depend on the market. Policy that moves the extensive margin by price buys exposure to that payoff for the few households it moves; it does not buy the holding behavior that made the payoff positive for those who wanted to own.

Taken together with Section V.B, the evidence points to a narrower role for homeownership

policy than its rhetoric suggests. Homeownership builds wealth when it is paired with a strong savings commitment or with favorable capital gains, and Figure 8 shows how contingent the second condition is on timing and place. The households for whom it did build wealth are disproportionately those who wanted to own—who bought early, held through the bust and paid down—and they would have done so at a higher price. Lowering the price of owning reaches few of the others, and for several of the groups it is meant to reach the binding constraint is not price. If the aim is retirement security, the premium households place on owning is a reason to pair any ownership policy with saving commitments that do not depend on the housing cycle, and a reason to be modest about how many marginal buyers a subsidy creates.

VI. Conclusion

This paper studies how a household's desire to own a home shapes tenure choice, lifecycle housing decisions, and wealth accumulation at retirement. Using variation in homeownership norms across immigrants' countries of origin, we show that stronger homeownership affinity leads to persistently higher ownership rates in the United States. These differences reflect a latent preference for owning that is not explained by observable economic characteristics and that continues to influence behavior long after arrival.

We then examine the long-run consequences of these preferences for household balance sheets. A stronger desire to own increases the likelihood of owning at retirement and raises both housing wealth and total wealth. However, these gains do not appear to arise from systematic differences in saving or portfolio reallocation. Instead, they are closely tied to exposure to housing market costs and appreciation, indicating that the financial benefits of homeownership depend heavily on local market dynamics rather than on ownership itself.

We also show that homeownership affinity shapes how households interact with the housing cycle. Households with a stronger desire to own respond more aggressively to expansions in mortgage credit, are less likely to exit ownership during downturns, and default less. These patterns mean that preferences govern not only whether households own but when they enter and how long they hold, and a historical reconstruction of owner-minus-renter wealth by purchase age and state shows that the payoff to those choices was a lottery: large and positive for purchases in some years and states, large and negative in others, and negative almost everywhere against a renter who invested the difference well.

Finally, we put a price on the preference itself. Tenure responds weakly to the cost of owning—at the response implied by the 2015 FHA premium cut, a $1,000 change in the annual own–rent premium moves the ownership rate by one to two percentage points. Against that benchmark, the ownership gaps between origin groups and otherwise identical natives translate into premia for owning of several thousand dollars a year in either direction, with a dispersion across origins that exceeds the single wedge structural models calibrate to match the aggregate homeownership rate. About a tenth of that dispersion is traceable to the home-country norm, and the dispersion is as

wide among immigrants who arrived as children. The present value of the norm's contribution per point of *HOCO* is about what owning returned in home equity at retirement, and the premium predicts the behaviors that produced the return.

Taken together, our findings suggest that homeownership can contribute to wealth accumulation, but in a way that is contingent on market conditions and on the holding behavior of households who wanted to own in the first place. Policies that lower the price of owning reach few households who would not have bought anyway, pay most of their cost to the inframarginal, and expose the households they do move to housing risk without the attachment that carried high-affinity owners through its downturns. Understanding the preference for owning—how large it is, how it varies, and how little it responds to price—is central to evaluating housing policy as a tool for wealth building, and our estimates suggest the tool is both expensive and blunt.

REFERENCES

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

Angrist, Joshua D, and Jörn-Steffen Pischke, 2008, *Mostly harmless econometrics* (Princeton university press).

Artigue, Heidi, Patrick Bayer, Fernando Ferreira, and Stephen Ross, 2024, The long-term consequences of losing a housing during the great recession, *Working Paper* .

Barwick, Panle Jia, Parag A Pathak, and Maisy Wong, 2017, Conflicts of interest and steering in residential brokerage, *American Economic Journal: Applied Economics* 9, 191–222.

Bayer, Patrick, Fernando Ferreira, and Stephen L Ross, 2016, The vulnerability of minority homeowners in the housing boom and bust, *American Economic Journal: Economic Policy* 8, 1–27.

Benetton, Matteo, Marianna Kudlyak, and John Mondragon, 2024, Dynastic home equity?, *Working Paper* .

Berger, David, Konstantin Milbradt, Fabrice Tourre, and Joseph Vavra, 2025, Optimal mortgage refinancing with inattention, *American Economic Review: Insights* 7, 497–515.

Bernanke, Ben S., 2007, The importance of financial education and the national jumpstart coalition survey.

Bernstein, Asaf, and Peter Koudijs, 2024, The mortgage piggy bank: Building wealth through amortization, *Quarterly Journal of Economics* 139, 1767–1825.

Bhutta, Neil, 2015, The ins and outs of mortgage debt during the housing boom and bust, *Journal of Monetary Economics* 76, 284–298.

Bhutta, Neil, and Daniel Ringo, 2021, The effect of interest rates on home buying: Evidence from a shock to mortgage insurance premiums, *Journal of Monetary Economics* 118, 195–211.

Biden, Joseph R., 2024, Proclamation 10769: National homeownership month, 2024, Federal Register, 89 FR 48229.

Blau, Francine D, Lawrence M Kahn, and Kerry L Papps, 2011, Gender, source country characteristics, and labor market assimilation among immigrants, *The Review of Economics and Statistics* 93, 43–58.

Botsch, Matthew J., and Ulrike Malmendier, 2020, The long shadows of the great inflation: Evidence from residential mortgages, *Working Paper* .

Brueckner, Jan K, 1986, The downpayment constraint and housing tenure choice: A simplified exposition, *Regional Science and Urban Economics* 16, 519–525.

Center, Minnesota Population, 2020, Integrated public use microdata series, international: Version 7.3 .

Charles, Kerwin Kofi, and Erik Hurst, 2003, The correlation of wealth across generations, *Journal of Political Economy* 111, 1155–1182.

Chetty, Raj, Lázló Sándor, and Adam Szeidl, 2017, The effect of housing on portfolio choice, *The Journal of Finance* 72, 1171–1211.

Cocco, Joao F, 2005, Portfolio choice in the presence of housing, *The Review of Financial Studies* 18, 535–567.

Cocco, Joao F, Francisco J Gomes, and Pascal J Maenhout, 2005, Consumption and portfolio choice over the life cycle, *The Review of Financial Studies* 18, 491–533.

Coven, Joshua, Sebastian Golder, Arpit Gupta, and Abdoulaye Ndiaye, 2026, Property taxes and housing allocation under financial constraints, Technical report, National Bureau of Economic Research.

Das, Prashant, N Edward Coulson, and Alan Ziobrowski, 2019, Caste, faith, gender: Determinants of homeownership in urban india, *The Journal of Real Estate Finance and Economics* 59, 27–55.

Dietz, Robert D, and Donald R Haurin, 2003, The social and private micro-level consequences of homeownership, *Journal of Urban Economics* 54, 401–450.

Dustmann, Christian, 1996, The social assimilation of immigrants, *Journal of Population Economics* 9, 37–54.

Garcia, Daniel, 2020, Employment in the great recession: How important were household credit supply shocks?, *Real Estate Economics* .

Gilchrist, Simon, Michael Siemer, and Egon Zakrajsek, 2018, The real effects of credit booms and busts, Technical report.

Goodman, Laurie, and Jung Hyun Choi, 2026, Home price indexes don't fully capture home improvement spending, overestimating appreciation.

Goodman, Laurie, and Christopher Mayer, 2018, Homeownership and the american dream, *Journal of Economic Perspectives* 32, 35–58.

Greenstone, Michael, Alexandre Mas, and Hoai-Luu Nguyen, 2020, Do credit market shocks affect the real economy? quasi-experimental evidence from the great recession and "normal" economic times, *American Economic Journal: Economic Policy* 12, 200–225.

Greenwald, Daniel L, and Adam Guren, 2021, Do credit conditions move house prices?, Technical report, National Bureau of Economic Research.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales, 2006, Does culture affect economic outcomes?, *Journal of Economic Perspectives* 20, 23–48.

Happel, Jonas, Yigitcan Karabulut, Larissa Schäfer, and Selale Tuzel, 2022, Shattered housing, *Available at SSRN 4029064* .

Hilber, Christian AL, and Tracy M Turner, 2014, The mortgage interest deduction and its impact on homeownership decisions, *Review of Economics and statistics* 96, 618–637.

Himmelberg, Charles, Christopher Mayer, and Todd Sinai, 2005, Assessing high house prices: Bubbles, fundamentals and misperceptions, *Journal of economic perspectives* 19, 67–92.

Jordà, Òscar, Katharina Knoll, Dmitry Kuvshinov, Moritz Schularick, and Alan M Taylor, 2019, The rate of return on everything, 1870–2015, *Quarterly Journal of Economics* 134, 1225–1298.

Kaplan, Greg, Kurt Mitman, and Giovanni L Violante, 2020, The housing boom and bust: Model meets evidence, *Journal of Political Economy* 128, 3285–3345.

Keys, Benjamin J, and Philip Mulder, 2024, Property insurance and disaster risk: New evidence from mortgage escrow data, Technical report, National Bureau of Economic Research.

Khwaja, Asim Ijaz, and Atif Mian, 2008, Tracing the impact of bank liquidity shocks: Evidence from an emerging market, *American Economic Review* 98, 1413–42.

Kovacs, Agnes, and Patrick Moran, 2023, Financial innovation, the decline in household savings, and the trade-off between flexibility and commitment, *Working Paper* .

Layton, Don, 2021, The homeownership rate and housing finance policy– part 1: Learning from the rate's history, Technical report, Joint Center For Housing Studies, Harvard University.

Le Blanc, Julia, Jiri Slacalek, and Matthew N White, 2025, Housing wealth across countries: The role of expectations, institutions and preferences .

Mabille, Pierre, 2023, The missing homebuyers: Regional heterogeneity and credit contractions, *The Review of Financial Studies* 36, 2756–2796.

Malmendier, Ulrike, and A. Steiny Wellsjo, 2024, Rent or buy? Inflation experiences and homeownership within and across countries, *Journal of Finance* 79, 1977–2023.

Manso, Gustavo, 2016, Experimentation and the returns to entrepreneurship, *The Review of Financial Studies* 29, 2319–2340.

Marcén, Miriam, and Marina Morales, 2020, The effect of culture on home-ownership, *Journal of Regional Science* 60, 56–87.

Mian, Atif, and Amir Sufi, 2009, The consequences of mortgage credit expansion: Evidence from the us mortgage default crisis, *The Quarterly journal of economics* 124, 1449–1496.

Oster, Emily, 2019, Unobservable selection and coefficient stability: Theory and evidence, *Journal of Business & Economic Statistics* 37, 187–204.

Pfeffer, Fabian, Bob Schoeni, Arthur Kennickell, and Patricia Andreski, 2016, Measuring wealth and wealth inequality: Comparing two u.s. surveys, *Journal of Economic and Social Measurement* 41, 103–120.

Poterba, James, Steven Venti, and David Wise, 2011, The composition and drawdown of wealth in retirement, *Journal of Economic Perspectives* 25, 95–118.

Poterba, James M, 1984, Tax subsidies to owner-occupied housing: an asset-market approach, *The quarterly journal of economics* 99, 729–752.

Rappaport, Jordan, 2010, The effectiveness of homeownership in building household wealth, *Economic Review-Federal Reserve Bank of Kansas City* 35.

Ringo, Daniel R., 2020, Home ownership as a labor market friction, *Real Estate Economics* 49, 111–133.

Ruggles, Steven, Sarah Flood, Ronald Goeken, Josiah Grover, Erin Meyer, Jose Pacas, and Matthew Sobek, 2020, Ipums usa: Version 10.0 .

Sapienza, Paola, 2020, Women in economics: Paola sapienza - 1. does culture affect economic outcomes?

Schubert, Gregor, 2021, House price contagion and U.S. city migration networks, Technical report, University of California Los Angeles.

Sinai, Todd, and Nicholas S Souleles, 2005, Owner-occupied housing as a hedge against rent risk, *The Quarterly Journal of Economics* 120, 763–789.

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

University of Michigan, 2024, Health and retirement study.

Woodward, Susan E, and Robert E Hall, 2012, Diagnosing consumer confusion and sub-optimal shopping effort: Theory and mortgage-market evidence, *American Economic Review* 102, 3249–3276.

Yao, Rui, and Harold H. Zhang, 2005, Optimal consumption and portfolio choices with risky housing and borrowing constraints, *The Review of Financial Studies* 18, 197–29.

Notes

[1]An alternative would be to use the homeownership rate in an individual's U.S. *county* of origin, as in Ringo (2020). We do not do so because migration links within the U.S. connect local housing and markets, inducing comovement in outcomes across places (Sinai and Souleles, 2005; Schubert, 2021). County-of-origin homeownership rates may then capture not only childhood tenure norms but also later exposure to correlated markets. By contrast, cross-country housing returns are uncorrelated, making *country*-of-origin homeownership norms a cleaner source of variation in norms (Jordà et al., 2019).

[2]We distinguish between 4 marital status categories: single or never-married, married, separated or divorced, and widowed.

[3]We dropped any sample with an average homeownership rate of less than 33% that could not be independently verified by an online search, which led us to drop: Argentina 1991, Benin 2002, Ethiopia 1994, Togo 2010, and Papua New Guinea 1990.

[4]According to a report from the New York City Comptroller's office, "Spotlight: New York City's Homeowner Housing Market," March 2024. Accessed via `https://comptroller.nyc.gov/wp-content/uploads/documents/Spotlight-New-York-Citys-Homeowner-Housing-Market.pdf`

[5]This hand-collected comparison sample covers origin countries comprising 87% of all the foreign-born in the U.S. and 9.2% of the total U.S. population.

[6]This data was accessed through IPUMS (Ruggles, Flood, Goeken, Grover, Meyer, Pacas, and Sobek, 2020).

[7]We downloaded this crosswalk from Neil Bhutta's website. The mapping to lender parent companies is only available through 2017 at which point the lender identification codes in HMDA change. Therefore, we limit the analysis to the years before for which we can consistently aggregate to parent companies over time.

[8]One may wonder why we did not use variation across counties within the US as our laboratory. First, our preferred dataset does not have data more granular than state for place of birth among US resident. Second, Figure 1 highlights that comparing across countries yields wider variation in homeownership when comparing to states. Third, the only dataset we are aware of that maps more granular place-of-birth, at the county level, to adult locations is the National Longitudinal Survey of Youth 1979 (NLS79) or 1997 (NLS97). Neither of these samples has much variation in expected homeownership over the credit boom period we study in the paper (2000-2005), due to the participants having already largely transitioned to homeownership (NLS79), or being too young to have started (NLS97), as highlighted in Figure IV. In sum, using the immigrant sample ACS as our core dataset yields high variation in HOCO, broad coverage of lifecycle demand for housing, and large sample sizes with detailed geographic variation within the US for current locations.

[9]There is an "ancestry" variable, but it does not allow us to distinguish between the children of recent immigrants and those of families with a long history in the country.

[10]See Appendix Table HIII for the full results for the children of immigrants with different combinations of control variables corresponding to the columns in Table I.

[11]This figure corresponds to the same sample and control variables as used in column (2) of Table III. For the same analysis but without including any control variables, i.e. a raw comparison of group averages, see Appendix Figure IIX, which shows very similar patterns.

[12]Terciles are assigned by sorting respondents in the full sample by the assigned median housing cost rank of their state in the year 2000.

[13]The prediction uses the following household characteristics: county fixed effects; real household income (linear and ventile indicators); a quadratic in age of the household head and 5-year age group indicators; indicators for educational attainment of the household head; indicators for the number of children and relatives in the household; indicators for discretized years since immigration of the head; marital status of the head; and interactions between marital status and years-since-immigration categories.

[14]See Appendix Table HIV for the coefficient estimates corresponding to the linear models shown in Figure 5.

[15]We also estimate separate coefficients for the Mexican-born and no-*HOCO*-data foreign-born groups, but omit them from the table for concision, as we do not have sharp ex ante predictions for those groups.

[16]The AHS only initiated questions on missed rent or mortgage payments in 2013, and asks them every 4 years, such that researchers can only observe these in the 2013, 2017, and 2021 waves.

[17]Computed as $0.12 * (-0.366) = -4.4\%$.

[18]The type of unit includes whether a home is in one of the following structure types: a mobile/manufactured home, single family detached, singly family attached, 2 unit, 3-4 units, 5-9 units, 10-19 units, 20-49 units, 50+ units.

[19]The exercise is a conditional reconstruction on historical state paths, not an observed household history: it holds the housing bundle, income and consumption fixed across tenure, allows one purchase and no move, marks the house to market at 60 with no selling cost, and uses a parametric tax schedule. Maintenance and depreciation are 2.0% and insurance 0.35% of value, buying costs 2.5%, the same values as the user cost of Section V. Appendix **??** gives the two-account ledger, the refinancing rule, the construction of the state environments, and the companion figures by birth cohort and for state pairs. The rent-to-value ratio is the same hedonic contract-rent schedule used to price the user cost in Section V, aggregated to the state.

[20]A utility shifter is in utils and cannot be dollarized without the model's marginal utility of consumption at the calibrated allocation. Mabille (2023) reports that his *amenity* shifter of 0.508 is equivalent to 35.6% of per-period utility, which would put the ownership shifter of 0.82–0.90 near 60% of per-period utility; we do not use this conversion.

[21]We do not use the framework to rationalize the *level* of ownership. The 65% of the stock who own is the outcome of a persistent state—transaction costs keep a household an owner for years after the move at which it chose—and a model of the choice at a move, estimated among movers who own at 31%, is not the instrument for it; asked to match the movers' own rate, the framework needs no mean premium at all. The comparison with the structural wedge is therefore about dispersion, not the mean: the standard deviation of the premia across origins in Figure 9 is three times the entire wedge Kaplan et al. (2020) require.

[22]Equation (7) is the LPM counterpart of inverting (4) at the group mean, $\gamma_i = \bar{\Delta}_i + \sigma \operatorname{logit}(\bar{P}_i)$. The level that formula produces is negative for every group of movers—young, mobile, constrained households rent for reasons Δ does not price—and is not a statement about taste; the difference from natives is. Standard errors on $\tilde{\gamma}_i$ are by the delta method and include the uncertainty in β.

[23]India is the clearest case in which $\tilde{\gamma}_i$ bundles something other than taste. Its origin homeownership rate is high (79%), yet its movers own far less than natives with the same income and family structure, and the gap is concentrated among high earners: our fit over-predicts ownership for high-income immigrants generally and for Indian households in particular. Unlike the other large groups that carry the *HOCO* relationship—Vietnamese, Filipino, Mexican and Chinese movers, the great majority of whom are permanent residents or citizens—a large share of recent Indian arrivals are in the US on temporary employment visas with uncertain paths to permanent residency. A household that expects to stay five years faces transaction costs of $9.5\%/5 \approx 1.9\%$ of house value per year rather than the 0.9% we assign, close to $4,000 a year on the tenure-neutral house, and may be unable to obtain conventional financing at all. Both push its tenure toward renting for reasons that are costs, not preferences, and both are differenced out only to the extent natives share them, which they do not. Puerto Rican movers, who are citizens, illustrate the horizon channel without the visa channel: circular migration shortens the expected stay. We keep both groups in every estimate; the *HOCO* slope rises from $156 to $284 per point when they are excluded.

[24]Formally, a binding constraint truncates the choice: $\Pr(\text{own}) = \Pr(\text{feasible}) \cdot \Lambda((\gamma - \Delta)/\sigma)$, and a lower feasibility rate for group i lowers α_i for any γ_i. The imputed-wealth control of Table I, column 3, absorbs part of this; we have not yet carried it into (6).

[25]These are pretax returns, so overstate returns to both housing and equities depending on the prevailing property, dividend, and capital gains tax regime.

INTERNET APPENDIX

Appendix A. Theoretical Framework

We first develop a stylized model of tenure choice incorporating an affinity for housing into a user cost model, and later add borrowing constraints. We consider housing consumption to be exogenous in order to keep the model tractable and extend the model in Brueckner (1986).

To provide the basic intuition, first consider a static tenure choice problem: a household decides whether to rent, in which case its consumption x^R, net of rent payments Q and taxes t is

$$x^R = (1-t)y - Q,$$

where y is the household income. If the household decides to become a homeowner instead, it has to make a down-payment αP, where P is the house price, at the beginning of the period, which has an opportunity cost r.[26] The household finances the remainder $(1-\alpha)P$ of the house price with mortgage payments at interest rate r, which are tax-deductible, sells the house again at the end of the period for price P and receives back its equity αP. Moreover, the household experiences a cultural cost of being a homeowner ϕ, which represents the degree to which cultural benefits of homeownership (e.g. pride, sense of security, control premium) offset the additional costs of homeownership (e.g. mental burden, lower mobility, time cost of maintenance). Households from groups with a relatively high cultural affinity for homeownership will experience a lower cost of homeownership. Consumption for homeowners is therefore

$$\begin{aligned} x^H =& (1-t)(y - \alpha r P - r(1-\alpha)P) - \alpha P + \alpha P - \phi \\ =& (1-t)y - (1-t)rP - \phi \end{aligned}$$

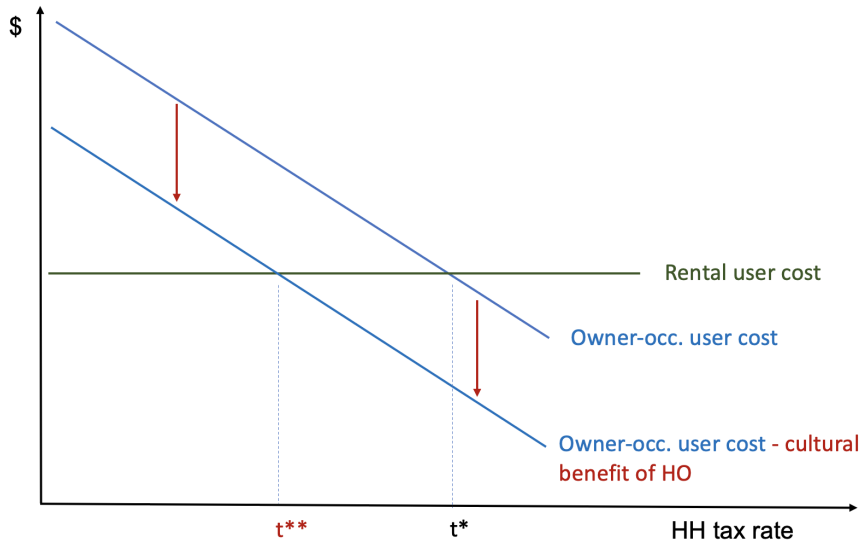
Note that the initial equity payment and later return of principal net out and that the need to finance the equity payment means that the household effectively pays interest on the full purchase price. For now, we are implicitly assuming that the household has no wealth or financing constraint in coming up with the downpayment - and we will modify this assumption later. Imposing a zero profit condition for landlords, such that $Q = rP$, this means that households are indifferent between owning or renting if

$$trP = \phi,$$

and households with relatively high tax advantages (high t) or high affinity for homeownership (low ϕ) will choose to own rather than rent. A common way of visualizing this "user cost" approach to tenure choice is to plot user costs for owners and renters over tax rates. Figure 11 illustrates the trade-off in this static version of the model: the owner-occupied user cost declines with the household's marginal tax rate, while the rental cost does not vary with tax rates. Therefore, there is some tax rate t^* above which households choose to own and below which they rent. However, the location of this indifference point is shifted by the cultural affinity for homeownership. A household

with a lower mental cost (higher affinity) for homeownership will have an owner-occupied user cost curve that is shifted down for all tax rates, leading to a lower indifference point t^{**} above which a household chooses to own rather than rent. That is, *higher affinity for homeownership results in higher homeownership rates.*

Figure 11: Static tenure choice with cultural affinity for homeownership



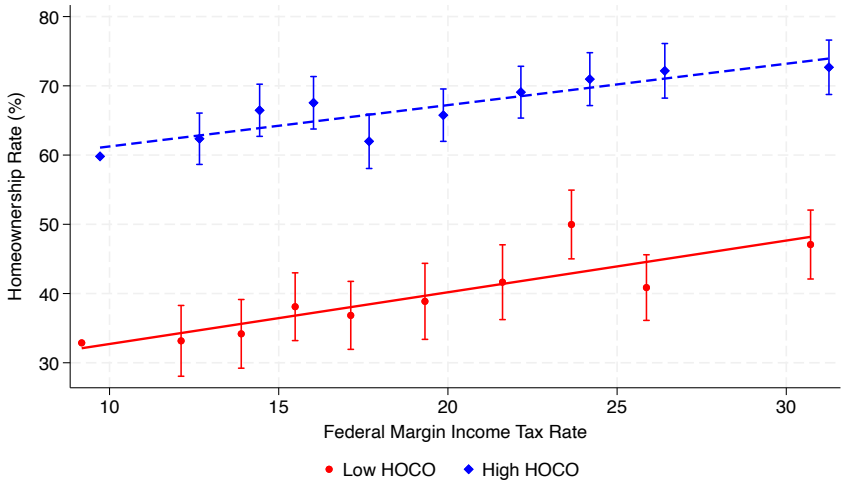
Notes: This figure shows the user-cost vs. tax rate profile for households that rent or own, as outlined in Section A. Here "cultural benefit of homeownership" is analogous to ϕ.

The model built thus far has two testable implications we take to the data. First, those households with a high affinity for homeownership should have lower user costs than those with lower affinities; however, since ϕ is unobserved by the researcher, along with many other standard inputs to user costs not outlined above (such as the opportunity cost of downpayments, or the household's risk premia), we cannot credibly construct household level user costs without significant noise. Instead, we study how homeownership rates vary with affinities in Section II. Intuitively, households with lower user costs of owning will more often select into homeownership.

The model's second implication is that between t^{**} and t^{*} the probability that a household owns its home is increasing in ϕ; below t^{**} everyone rents, and above t^{*} everyone owns. These thresholds are of course simplifications for two representative households, such that empirically we should observe, for a given tax rate, immigrant households with stronger cultural norms favoring homeownership will own at higher rates all along the tax schedule.

To test this implication of the model, we use the NBER's TAXSIM model (v35) to compute federal marginal tax rates for all immigrant households in our sample. Figure 12 plots a binned scatterplot of homeownership rates by federal marginal income tax rate, splitting our immigrant sample population into those with above-median HOCOs and below-median HOCOs. Importantly, the tenure choice model outlined above has a household deciding whether to own or rent the *same* unit, therefore we include CBSA-by-year fixed effects to proxy for location and labor market, as

Figure 12: Homeownership Rate by Federal Marginal Income Tax Rate



Notes: This table shows a binned scatter of homeownership rates by federal marginal income tax rate, split by immigrants with above- or below-median HOCO. Socioeconomic characteristics incorporated into the TAXSIM model used to produce marginal federal income tax rates. We control for CBSA-by-year fixed effects, as well as housing unit characteristics: number of bedrooms, bathrooms, lotsize in acres, unit size in ft^2, decade built, structure type (i.e. single family or multifamily), and presence of a garage or carport. Data from the AHS 2001-2019.

well as a suite of property characteristics including bedrooms, bathrooms, acreage, square footage, the home's age, type (i.e. single- or multi-family), and the presence of garage or car-port. These controls adjust for the fact that the researcher cannot observe the counterfactual rental (purchase) price a homeowner (renter) considered before buying (renting).

The figure shows that immigrants with stronger cultural norms for housing do select into homeownership at higher rates along the entire tax schedule. This gap between above- and below-median HOCO marginally attenuates at the top marginal income tax rates as predicted by the model, since the benefits of owning are highest here for all groups. These findings suggest that high-HOCO households may end up being observationally similar to marginal homebuyers, as they are selecting into homeownership at lower marginal tax rates, and thus lower incomes due to tax rate progressivity. Moreover, these results support the idea that differences in homeownership associated with affinities stemming from different origins are likely not explained by differences in financial circumstances, which we would expect to be strongly correlated with imputed tax rates.

Saving for downpayments. In order to evaluate the role of changes in credit supply and how they interact with an affinity for homeownership, we need to make the model dynamic and introduce a role for the downpayment constraints α which did not matter in the static version. We do so by adding a period in which households need to save for the downpayment in the subsequent period, following Brueckner (1986). Now, households first live through a period 0 in which all households are renters, earn their income and can save for period 1. In period 1, they have the choice to

become homeowners as before, but now have to finance the downpayment αP from their (weakly positive) savings s in the previous period. Moreover, any excess savings are invested with taxable returns r during period 1 and are available to be consumed at the end of the period.

Thus, in period 0, consumption of all households - whether future renters (R) or homeowners (H) - is given by

$$x_0 = (1-t)y_0 - s - Q$$

where households may choose different savings rates s^R or s^H depending on their optimal tenure choice in the following period. The period 1 consumption amounts then become

$$\begin{aligned} x_1^R &= (1-t)y_1 + (1+(1-t)r)s^R - Q \\ x_1^H &= (1-t)y_1 + (1+(1-t)r)s^H - (1-t)rP - \phi, \end{aligned}$$

where x_1^R and x_1^H denote the consumption of households that choose to rent and own, respectively, and $s^H \geq \alpha P$ needs to hold for the homeowner.[27] Each household maximises the objective function

$$u(x_0) + \theta u(x_1)$$

where $u(\cdot)$ is a strictly concave utility function and θ is the discount factor. We assume that owning always dominates renting for households for whom the downpayment constraint is not binding (i.e. for which $s^R \geq \alpha P$), and focus on the households for which the constraint is binding ($s^H = \alpha P$). The owner-renter utility differential for these constrained households is given by

$$\Omega = u(x_0^H) + \theta u(x_1^H) - u(x_0^R) - \theta u(x_1^R).$$

Credit supply shocks. If we think of a mortgage credit supply change as a change in the downpayment requirement α, we can derive the resulting change in the homeowner utility for constrained households (substituting $s^H = \alpha P$):

$$\frac{\partial \Omega}{\partial \alpha} = P\left(-u'(x_0^H) + \theta(1+(1-t)r)u'(x_1^H)\right) < 0,$$

where the inequality follows from the fact that when the downpayment constraint is binding, s^H is higher than optimal and moves further away from its optimal level if the downpayment requirement increases. That is, higher downpayment requirements - a tightening of mortgage credit - reduce the utility from homeownership holding other parameters constant.

How does this effect of a tightening of mortgage credit supply vary with household affinity for homeownership? Differentiating with regard to the cultural cost of homeownership ϕ, we find

$$\frac{\partial \Omega}{\partial \alpha \partial \phi} = -P(1+(1-t)r)u''(x_1^H) > 0,$$

so for households where the cost ϕ is larger, i.e. they have a *lower* affinity for homeownership,

the negative impact of an increase in downpayments is mitigated, while higher affinity households experience a larger utility decline after mortgage credit tightens. Intuitively, households with a higher consumption benefit from being homeowners in the later period would benefit more from smoothing their consumption by shifting some consumption towards the earlier period when they are renters. However, a downpayment constraint requires them to lower consumption in the earlier period in order to save so they can afford to become homeowners. A higher downpayment constraint – and therefore higher required savings – lowers relative consumption in the earlier renter period even more for future homeowners. This leads some households to stay renters and forgo the painful period of low consumption and high savings that becoming a homeowner entails. This decline in the relative attractiveness of becoming a homeowner is stronger for high homeownership propensity households because they will already, on average, have chosen homeownership even at lower consumption levels in the first period, and pushing that level of early period consumption lower is therefore more painful for them.

Mortgage default for homeowners. How does behavior change *after* households enter homeownership? We are interested in how a higher affinity might impact the likelihood of defaulting on a mortgage among homeowners, when faced with similar financial circumstances. Assume that two otherwise identical homeowners (with the same observable income and tax rates) are faced with a circumstance where they can default on their mortgage. For simplicity, assume that the mortgage costs have increased, such that if the household does not default and become a renter again, it would reduce disposable income of the household by δ. We can think of these costs as being associated with mortgage structure, such as ARM resets, or variable HELOC rates (Bernanke, 2007; **?**). Thus, the current homeowners face a choice between the following consumption bundles:

$$\begin{aligned} x^{\text{default}} &= (1-t)y - Q \\ x^{\text{no default}} &= (1-t)y - (1-t)rP - \phi - \delta, \end{aligned}$$

where the utility gain from defaulting is given by

$$\Delta(y,\phi) = u(x^{\text{default}}(y,\phi)) - u(x^{\text{no default}}(y,\phi)).$$

Note that, with a strictly concave utility function $u(\cdot)$, a higher affinity (lower homeownership cost ϕ) household, always has a lower incentive to default, ceteris paribus, as they get more utility from being a homeowner for a given income, i.e.

$$\left.\frac{\partial \Delta(y,\phi)}{\partial \phi}\right|_y > 0.$$

Now, assume that default becomes more attractive (δ increases) because of a further increase in adjustable mortgage rates, or a decline in local income opportunities combined with an inability to

move without defaulting (**?**). Then,

$$\left.\frac{\partial \Delta(y,\phi)}{\partial \delta \partial \phi}\right|_{y} > 0.$$

That is, for a given level of income, a shock to the mortgage carrying cost that makes default more attractive has a bigger impact on low affinity households. The intuition is that high affinity households with the same income are experiencing higher levels of utility and thus lower changes in marginal utility from a given change in income while remaining a homeowner. This means that *low affinity homeowners are more likely to increase their defaults* when they encounter economic conditions that make it financially harder to pay their mortgages.

This simple model shows (1) how homeownership affinity can increase homeownership, (2) that households with high affinities respond more strongly to mortgage credit expansikons than do their low-affinity peers, and (3) that high affinity households should have lower mortgage default rates, both on average, and in response to shocks that increase the incentive to default. We will test these hypotheses empirically in sections II and IV.

Appendix B. HOCO Stability

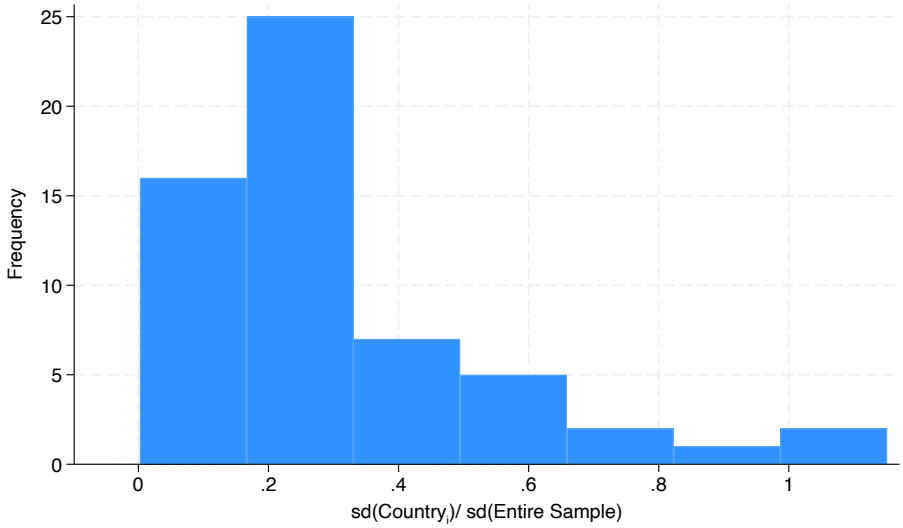
One concern in assigning HOCOs to immigrants in the U.S. is that the cultural norms around homeownership can change over time. We can check for origin-country stability in the IPUMS data, among the set of countries for which we have at least two years of data ove the available surveys between 1999 and 2016. Among the 85 countries in the harmonized international census microdata, 31 countries have two years of data, 21 have three, 2 have six, and 4 have five years. Among this set, on average, countries have 9 years between surveys, with a minimum gap of 4 years, and a maximum gap of 15 years.

Our first exercise is to estimate how well a country's earlier survey predicts a country's later survey. To do this, we set up a country-by-survey year panel, and regress current mean HOCO on the previous survey's HOCO for a given country. The exercise yields a correlation coefficient of 0.88, presented in column (1) of Table XI. We can additionally control for year fixed effects, noting that major macroeconomic disruptions such as the Global Financial Crisis may have dislocated some housing markets, as in the U.S. Column (2) in Table XI shows that controlling for years does little to impact the persistence of countries' HOCOs.

The results in Table XI show that, within a country, homeownership norms are quite stable over time. In the next exercise, we explore whether the concept of homeownership is stable across countries. To do so, we construct the standard deviation of HOCO within a country, and compare it to the standard deviation in HOCO across the entire sample. The goal of this exercise is to gauge whether some countries experience more volatility in homeownership than others.

Figure 13 shows the histogram of the 85 individual countries' HOCO variation relative to the entire sample variation. Of the 85 countries, half exhibit a magnitude of variation approximately one-quarter the entire sample magnitude, showing that most of the variation in HOCO comes from

Figure 13: Relative Variation in HOCO Across Countries



Notes: This figure shows the ratio between a country's HOCO variation, measured in standard deviations, and the sample HOCO variation. Sample includes 85 countries which have at least two survey waves observed in IPUMS international census microdata between 1999 and 2016.

cross-country variation rather than within-country variation. Only three countries exhibit more variation in their own HOCO than the sample as a whole: South Africa, Hungary, Vietnam, all of which have small population shares among U.S. immigrants. We conclude that, for the majority of countries, homeownership seems stable.

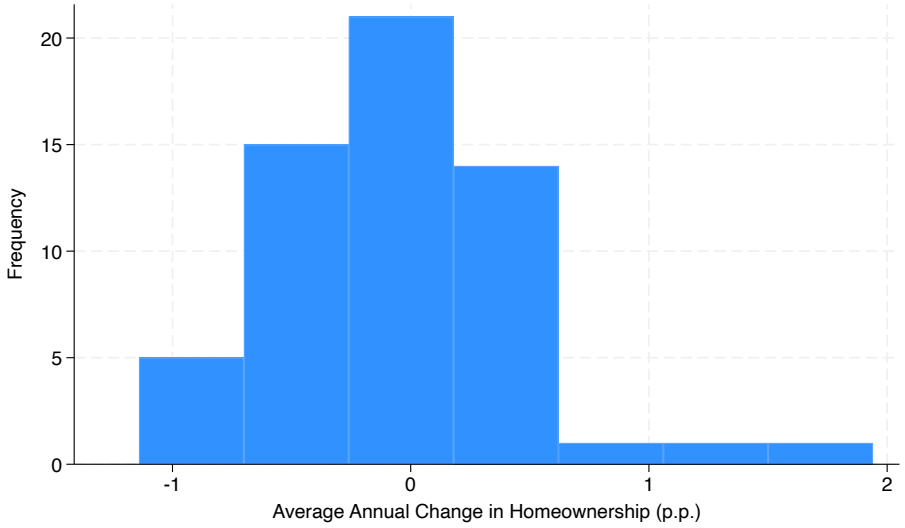
Finally, we estimate the average annual change in homeownership calculated between HOCO across survey years for a given country.[28] Figure 14 plots the histogram of these annual percent changes. The average country sees next to no HOCO change, at -0.07 p.p. per year. Four countries do have annual changes in homeownership above or below 1 p.p. per year, and these correspond to Ghana, Belarus, Hungary, and Vietnam.[29]

Taken together, these exercises show that homeownership is quite stable within and across countries over time.

Appendix C. Wealth Imputation

In this section, we discuss the imputation of household non-housing networth using the PSID and ACS data. The PSID structure follows an initial set of households recruited in 1968, adding new households as the children of the original households form their own households, or as households otherwise split. This structure does not accomodate population representativeness as immigration evolves over time. As such, we limit the PSID sample to the 1997 and 2017 waves. These refresh the sample of households being surveyed by adding new households that had migrated to the United States between between 1968 and 1997 and between 1997 and 2017, respectively. We end up with

Figure 14: Average Annual Change in HOCO Across Countries



1,046 immigrant households whose heads are between the ages of 18 and 69, in keeping with our other sample restrictions.

We construct non-housing networth following Pfeffer, Schoeni, Kennickell, and Andreski (2016) and predict networth using a suite of socioeconomic and demographic variables. We add up checking and savings accounts, the value of business assets, stocks, IRAs, private annuities, vehicle networth (value less remaining auto loans) and any other real estate that is not the primary residence. We then compute the household's liabilities, adding credit card debt, loans from relatives, medical debt, legal debt, student debt and other non-specified debt, excluding primary and secondary mortgages on the primary residence. Non-housing networth is just non-housing assets less non-housing liabilities.

The predictive variables include fourth order polinomial in household income, race and hispanic status of household head and spouse (if applicable), household head's level of education, age and gender, household head's age interacted with education to control for differential income profiles, whether there are children present in the home, whether the householder is married, as well as the interaction between marriage and children. Finally, we include census division fixed effects to control for differential non-housing wealth accumulation patterns across U.S. geographies, as well as continent fixed effects for immigrants' country of origin reflecting the fact that those immigrating from Europe may have differential wealth profiles to those from South America, for example. We are limited in the amount of geographic variation we can control for as our sample of $\sim 1,000$ immigrant households hail from only 38 states. Additionally, only 22 states have more than 10 households, limiting the residual variation should we control for state directly. For similar reasons, we cannot control for country of origin; the PSID only provides 16 broad regions which we map to continents as many of these regions have very few households. Observations are weighted by the cross-sectional family weight provided by the PSID.

Figure IIII plots the predictive ability of our model. The regression's R^2 is 0.22, suggesting that many wealth determinants have not been directly accounted for. Nonetheless, the model fits the data quite well up to networths of $800,000, suggesting that the remaining wealth determinants are most predictive of the right tail (we have only 17/1,1049 households with non-housing networths above $800,000), not the main sample.

Next, we store the point estimates associated with each of our socioeconomic and demographic predictors of wealth and apply them to our sample of migrants in the ACS data. This allows us to impute non-housing networth as the sum of the model inputs times their respective parameters. Figure IIV compares the actual distribution of non-housing networths observed in the PSID to the imputed non-housing networths we build using the ACS data and PSID-derived coefficients. The ACS immigrant sample, composed of nearly 4 million households, yields a distribution with more networth variation than that in the PSID, which is based on only about 1,000 households. Nonetheless, visual inspection of the two distributions shows many similar characteristics: mean networth near 0, with a significant right tail after about $500,000, and smalll mass of households with negative networth between $0 and -$500,000.

Appendix D. Robustness Checks for HOCO Passthrough

In this section, we explore different specifications that control for potential omitted variables and alternative explanations *other than country of origin* that could explain this association between *HOCO* and homeownership among immigrants in the U.S.

Origin country characteristics. An important concern with regard to the mechanism for our findings in Table I is that they may reflect omitted variable bias arising from immigrant households bringing other aspects of the lifestyle or economic situation of their country of origin with them, which then incidentally affect their homeownership in the U.S., without a particular preference over the latter. We consider a number of plausible home country characteristics that might be driving homeownership both in the U.S. and the origin country – their unconditional correlations with origin country average homeownership rates are shown in Appendix Figure II: On the one hand, we consider economic characteristics, such as GDP per capita, urbanization rate, annual inflation rate, and domestic credit to private borrowers as a share of GDP. These capture the idea that low wealth, exposure to economic risk, urban lifestyles, or a lack of access to credit may all keep homeownership in the country of origin artificially low, and this lack of resources and lifestyle choices may be replicated in the immigrants' experiences in the U.S. Note, for example, that homeownership is unconditionally negatively correlated with urbanization at a country level as shown in Appendix Figure II.

On the other hand, the legal system and property rights may make homeownership a more or less attractive investment relative to the security of, and access to, other assets. The persistence of the resulting attitudes towards homes as part of a household portfolio may be part of the cultural differences that explain immigrants' homeownership in the U.S. To capture these dimensions, we consider indices of property rights and investment freedom created by the Heritage Foundation. As can be seen in Appendix Figure II, these indices are highly positively correlated with GDP per capita and urbanization, and associated with lower inflation. Moreover, some studies suggest that religion may play a role in tenure choices, for example through their positions on usury (e.g. Das, Coulson, and Ziobrowski (2019)), so we also test how these results change when including fixed effects for which of the major religions (Buddhism, Christianity, Islam, Hinduism, Other) predominates in the country of origin.

We test the importance of these confounding country-of-origin characteristics by including all of them as control variables in the estimation. The results are shown in column 1 of Table XII: the *HOCO* effect on U.S. homeownership is consistently positive and significant in all specifications, and the magnitude of the coefficient is slightly bigger and more precisely estimated even when all country characteristics are controlled for. Among the country characteristics ((see Appendix Table HII for the full set of coefficients), only urbanization has a significant (and positive) effect on U.S. homeownership when all of our baseline controls are already included. These results suggest that each country's homeownership norms evolve through a complex system of institutions, culture, etc., none of which completely explain the variation in norms.

Immigrant group selection. Another potential concern with our estimates above is that

immigrants from particular countries are *selected* with regard to their tendency to buy a house when in the U.S. For example, if migrants from high homeownership origin countries tend to be selected to be particularly wealthy or more educated than average, e.g. because only the elites are able to pay the costs associated with migrating, then these selected characteristics might drive homeownership in the U.S. This concern should be mitigated by the fact that our baseline regressions already directly control for foreign-born households' income, education, and household size, such that any such selection would have to be orthogonal to these observables. However, to additionally ensure that our results are robust to the *average* characteristics of migrants from that origin, column 2 of Table XII shows a version of our regression where we also control for the average share of college-educated household heads, mean household income, mean number of children and mean household size among migrants from that origin country. In addition, we control for the GDP per capita of the origin country, in case immigrant selection is driven by the economic opportunities in the country of origin. Given that some of these migrant characteristics may actually be affected by homeownership, they may represent "bad controls" (Angrist and Pischke, 2008) that shut down the causal channel of interest – and the estimates including these controls should therefore be interpreted with caution. As the results in column 2 show, while the estimated effect size is reduced when including these additional controls, it is nonetheless statistically significant and implies an elasticity of 14% of U.S. tenure choice with regard to the household *HOCO*.

Largest origin groups. As Table HI shows, the share of U.S. migrants from different origin countries is highly concentrated. As a result, it is important to establish whether our *HOCO* effect estimates are driven by idiosyncrasies of the biggest origin countries or also hold for the smaller foreign-born origin groups in the U.S. In column 3 of Table XII, we therefore exclude any households with heads from one of the five largest immigrant groups in the U.S. as of the year 2000 (Mexico, Philippines, India, China, Canada) from the sample. The results show the estimated *HOCO* elasticity for this smaller sample is of a similar size, at 15%, and significant at a 1% level, which suggests that the large origin countries are not driving the baseline results.

HOCO measurement. Our measure of homeownership preferences assigns *HOCO* measured in international census microdata by household head marital status. In column 4, we show that our results are not due to mismeasurement of *HOCO* in this international census microdata. We hand-collect average national homeownership rates from official government sources or international organizations like the OECD. This sample covers a smaller number of countries (62, compared to 74 in our microdata sample). However, the *HOCO* constructed from the national average homeownership rates in this alternative data set still has an effect on U.S. tenure choice that is significant at the 10% level, and the estimated effect size of 15% is again very similar to our baseline estimate using the microdata *HOCO* by marital status.

Alternative household sample. The other concern around sample selection may be that the American Community Survey household data has a sampling methodology or differential response rates that correlate both with *HOCO* and U.S. tenure choices. In column 5, we instead use data from the American Housing Survey, which collects a much smaller sample using a different methodology

focused on being representative of housing units in the U.S. and tracking them over time. When we estimate our baseline effect in this separate data set of only $\sim$ 46K foreign-born householders, we obtain a significant estimated *HOCO* elasticity of 13%, so our baseline effect does not seem to be driven by methodological idiosyncrasies of the ACS sample.

Oster Bounds. Finally, we formally test to see how much unobserved variation could impact our point estimates using the two tests proposed in Oster (2019). Intuitively, these tests ask two questions: (1) how much more important (δ) do the unobserved variables need to be to nullify our estimated effect ($\beta = 0$), and (2) what is the estimated impact (β) should the unobservables and observables have equal importance ($\delta = 1$)? To implement these tests, we must first choose a total amount of variation we attempt to explain. While the most conservative approach would be to explain all of the variation in tenure choice, $R^2_{max} = 1$, given that the real world includes measurement error and other randomness, we instead follow Oster (2019) and set $R^2_{max} = 1.3 \times \tilde{R}^2$ from Table I, column (4), which includes our full set of observed covariates.[30] We first calculate how much more important the unobserved variables need to be to yield an impact of HOCO on tenure of 0, and find a $\delta = 4.11$. This test suggests that the unobservables need to be more than 4 times as important as the observables we already have, many of which have been well-documented as key drivers of homeownership as in Goodman and Mayer (2018)'s review, to wipe out our effect. For the second test, we assume equal importance, again likely conservative given the robust set of observable controls and fixed effects available, and calculate an impact of HOCO on tenure of 0.12. This provides a tight bounded set for our point estimate of [0.12, 0.153] relative to our baseline estimate in Table I, column (4).

To summarize, this section shows that the effects of origin country homeownership vary in a way that is consistent with a "cultural" explanation for this financial behavior. For example, foreign-born householders who are more likely to assimilate in the U.S. because they went to college or married a U.S.-born spouse are less likely to act in line with their HOCO. At the same time, the effects persist long after the householder immigrated, or if they arrived as a child, and the spouse's origin matters to a similar degree. All of these factors point to an effect that is more latent than the economic circumstances of the householder when they first arrive in the U.S.

Appendix E. Data Sources for Exclusion-Restriction Analysis

Appendix Figures II and III examine the cross-country correlates of homeownership in country of origin (*HOCO*), measured as the variable `ho_all` computed from harmonized IPUMS International census microdata covering 84 countries. The analysis draws on the following data sources, each averaged over 2000–2019 where panel data are available to obtain a country-level cross-section.

Economic development. GDP per capita (PPP, constant 2017 international $), urbanization rate (% of population), and inflation (consumer prices, annual %) are from the World Bank World Development Indicators (WDI), matching 79–81 of 84 *HOCO* countries. Domestic credit provided

Table XI: HOCO Stability Over Time

	(1) $HOCO_t$	(2) $HOCO_t$
$HOCO_{t-1}$	0.88***	0.89***
	(0.06)	(0.06)
Year FE		✓
R^2	0.73	0.77

Notes: This table estimates the impact of a country's past survey's HOCO on the country's current estimated HOCO. The sample includes 85 countries for which we observe at least two censuses in the harmonized census microdata in IPUMS. Survey years span 1999 to 2016. Observations equally weighted. Standard errors in parentheses, *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table XII: Effects of Homeownership in Country of Origin on Tenure Choice: Robustness Checks and Additional Specifications

Dependent var.:	Household Homeownership$_{it}$ (in %)				
	(1) Orig. Ctry Controls	(2) Mig. Char. Controls	(3) Excl. 5 largest Orig. Ctries	(4) Gov. source HOCO Data	(5) AHS Data
HOCO: by marriage status	0.167***	0.144***	0.151***		0.134**
	(5.35)	(3.05)	(2.98)		(2.53)
HOCO: gov. sources				0.148*	
				(1.94)	
Observations	2,060,006	3,436,252	1,627,032	3,485,350	16,320
R-Squared	0.30	0.32	0.33	0.31	0.31
	Addl. controls & fixed effects				
Household Characteristics	X	X	X	X	X
Imputed HH Wealth	X	X	X	X	X
Commuting Zone × Year FE	X	X	X	X	
CBSA × Year FE					X
Origin Ctry Characteristics	X				

Notes: This table shows the estimation results from additional specifications that vary particular aspects of the baseline regression in column 4 of Table I. Column 1 includes origin country characteristics and indicators for whether the householder belongs to different major religions (Buddhism, Christianity, Islam, Hinduism, Other). Column 2 controls for average migrant characteristics by origin country. Column 3 excludes the five largest immigrant groups in the U.S. as of the year 2000 (Mexico, India, China, Vietnam, Philippines) from the sample. Column 4 uses hand collected data from governmental reports for $HOCO$, rather than estimating it from IPUMS microdata. Column 5 uses the AHS sample instead of the baseline ACS sample to estimate HOCO effects. T-statistics based on heteroskedasticity-robust standard errors clustered at the origin country level shown in parentheses. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

by the financial sector (% of GDP) is also from the WDI ($N = 32$).

Institutional quality. Property rights and investment freedom indices (each scaled 0–100) are from the Heritage Foundation Index of Economic Freedom ($N = 74$).

Financial markets. Stock market capitalization as a share of GDP is from the World Bank WDI (indicator CM.MKT.LCAP.GD.ZS), averaged over 2000–2019 ($N = 50$).

Savings behavior. Gross domestic savings as a share of GDP is from the World Bank WDI (indicator NY.GDS.TOTL.ZS; $N = 76$). Household net saving rates (% of disposable income) are from the OECD National Accounts database ($N = 17$).

Housing-market institutions (OECD). Maximum loan-to-value ratios, transaction costs (% of property value), tenant protection indices, and property tax revenue (% of GDP) are from the OECD Housing Database, using the most recent available observation per country ($N = 14$–19). Le Blanc et al. (2025) provide a structural decomposition of how such institutional factors—particularly the rental wedge and house price beliefs—drive cross-country homeownership gaps.

Housing-market institutions (World Bank). The number of procedures, time (days), and cost (% of property value) required to register a property transfer are from the World Bank *Doing Business* historical data (DB04–DB20; $N = 78$). The quality of land administration index (0–30 scale) is available from DB16 onward. We construct a Transfer Costliness Index by standardizing the first three sub-indicators to mean zero and unit standard deviation and averaging the resulting z-scores.

Cultural attitudes. The share of respondents agreeing that "it is hard to feel secure in life without owning a home" and "I aspire to own my own home" are from the Ipsos Global Advisor survey "Home Ownership: A Growing Aspiration" (2024), based on nationally representative online surveys of adults aged 16–74 in 29 countries ($N = 17$ matched to *HOCO*).

Country-name harmonization. Country names are harmonized across sources to match the IPUMS International naming conventions used in the *HOCO* data. Key mappings include Czech Republic→Czechoslovakia, Korea (Rep.)→Korea, Russian Federation→Russia, Lao PDR→Laos, and Hong Kong SAR (China)→Hong Kong.

Appendix F. Health and Retirement Study Data

The overall goal of our analysis using Health and Retirement Study data is to estimate the effect of the homeownership in foreign-born respondents' countries of origin on their portfolio holdings at the end of their working life (around age 60). The two data sources used in this analysis are:

1. RAND HRS Longitudinal File (provided by HRS)
2. Homeownership by marriage status and country of origin ("HOCO"), collected from public data sources.

We transform and restrict the RAND HRS data in the following way to obtain our analysis subsample:

- Merge RAND HRS and HOCO data based on country of birth information (only available in the restricted access files) and also by marriage status
- Identify which survey waves occur closest to age 60 for each respondent and keep only respondents that are 55-65 years old at the time of at least one wave
- Retrieve all variables of interest from the survey wave when the respondent is (closest to) 60 years old (see variable definitions below)
- All final analyses also subset the sample to only include the foreign-born (for whom we have HOCO data), to only include one respondent per household (in couples, the financial data respondent) and only include respondents that are married or partnered at the time of the wave (in order to make households comparable)
- Some analyses additionally subset the sample to exclude respondents born in Mexico to show that the results are robust to excluding this largest country of origin; some analyses also subset only to respondents who did not live in the US when they were going to school.
- Analyses that concern wealth shares additionally condition on the numerator and denominator of portfolio share measures being non-negative and positive, respectively.

The analyses shown in the corresponding results tables consist of cross-sectional regressions that regress the portfolio holdings of the respondent's household at age 60 on their HOCO and additional household-level control variables – and the tables only report the coefficient on the HOCO variable. The sample sizes of the regression samples are indicated in the row "Observations" in the tables. The smallest regression sample represents 41 different countries of origin. The independent variable is continuous, so it represents an average effect for the full group indicated by that sample size.

Country HOCO terciles. For some analyses, we tabulate average homeownership rates by tercile of HOCO (low, medium, high), with or without controlling for the effect of demographic control variables. The HOCO tercile groups (Low, Middle, High) shown in the homeownership figures consist of the following numbers of respondents from the HRS:

The first group represents respondents from 9 different countries, the second and third group represent 16 different countries each. For reasons of data privacy, we are not allowed to provide further details on the countries included in each group.

Hoco Tercile	Respondents	Percent
1	49	21.97
2	97	43.50
3	77	34.53
Total	223	100.00

Definitions of key outcome and independent variables in HRS analyses

We use or construct the following variables for the HRS analysis, with variable names in the HRS RAND Longitudinal data noted in parentheses where applicable:

HOCO: Homeownership in country of origin, manually collected from government statistics agencies or computed from public IPUMS data and imported into the VDI at the country-by-marriage-status level. Continuous variable. The HOCO for a household is computed by first determining whether the main respondent is foreign-born and went to school outside of the U.S. and a HOCO is available for them. If all of these are true, we assign the household the main respondent's HOCO. If, and only if, these are not true, we check the same conditions for the spouse (if applicable) and assign the spouse's HOCO to the household. If neither the main respondent nor their spouse fufill these conditions, no HOCO is assigned. Intuitively, this means that HOCO in the HRS data captures the effect on the affinity for homeownership of at least one foreign-born (and raised outside of the U.S.) respondent in the household.

Homeownership at Age 60: Binary indicator for whether the gross value of the respondent's primary residence (*HwAHOUS*) at the time of the survey wave closest to their 60^{th} birthday is greater than \$5K.

Home Value Share of Total Wealth: Percentage measure that indicates the ratio of the net value of the respondent's primary residence (*HwATOTH*) to total wealth (*HwTOTB*) at the time of the survey wave closest to their 60th birthday. Set to missing if the numerator is negative, or the denominator is non-positive.

Total Wealth: Total wealth (*HwTOTB*) in dollars at the time of the survey wave closest to their 60th birthday.

Net Value of Primary Residence: Net value of the respondent's primary residence (*HwATOTH*) in dollars at the time of the survey wave closest to their 60th birthday.

Total Wealth excl. Primary Residence: Total wealth (*HwTOTB*) in dollars minus net value of the respondent's primary residence (*HwATOTH*) in dollars at the time of the survey wave closest to their 60th birthday.

1[Owns Stock]: Binary indicator for whether the gross value of the respondent's assets in stocks (*HwASTCK*) at the time of the survey wave closest to their 60th birthday is greater than \$5K.

Stock share of Wealth excl. Real Estate: Percentage measure that indicates the ratio of respondent's assets in stocks (*HwASTCK*) to total non-housing wealth (*HwTOTN*) at the time of the survey wave closest to their 60th birthday. Set to missing if the numerator is negative, or the

denominator is non-positive.

Definitions of additional control variables used in HRS analyses:

Demographic controls: These control variables (included where indicated in the tables and figures) are all for the respondent characteristics at age 60 and consist of categorical variable dummies for respondent marriage status ($RwMSTAT$), a continuous variable for age ($RwAGEY$), a dummy for gender ($RwAGENDER$) and dummies for the survey wave that the Age 60 data for the respondent is from. It also includes a dummy for whether the respondent was born in Mexico. Additionally, it controls for continuous total household income ($HwITOT$) and an indicator for respondent education, which is a binary for whether they are a high school graduate ($RwAEDUC$ is 3, 4, 5).

Non-housing wealth controls: These control variables (included where indicated in the tables and figures) are all for the respondent characteristics at age 60 and consist of the total non-housing wealth ($HwTOTN$) in dollars.

School outside US: This is a binary indicator for whether the foreign-born respondent lived in a US state when they went to school. It is constructed from the restricted access geographic variable WHRLIV10 and is a binary indicator. Where indicated, the analyses exclude anyone who lived in the U.S. when they went to school from the sample.

Appendix G. Construction of the Mortgage Credit Supply Shock

This appendix describes the construction of the county-level mortgage credit supply shock used in Section IV. Our goal is to isolate changes in local mortgage credit availability that arise from counties' differential exposure to lenders experiencing national shifts in mortgage lending, rather than from local changes in housing demand.

Our baseline approach follows Gilchrist et al. (2018) and Garcia (2020). The key idea is that local mortgage lending may vary either because local demand changes or because lenders change their willingness or ability to originate mortgages for reasons unrelated to local market conditions. Counties that are more exposed to lenders experiencing larger national expansions or contractions in lending should therefore experience larger shifts in local credit supply.

To isolate lender-specific shifts in mortgage lending, we estimate the following decomposition separately for each long-difference period:

$$\Delta \ln L_{c,j,t} = \alpha_{c,t} + \eta_{j,t} + \varepsilon_{c,j,t}, \tag{G1}$$

where $\Delta \ln L_{c,j,t}$ is the log change in the number of mortgage originations by lender j in county c over period t, $\alpha_{c,t}$ are county-by-period fixed effects, and $\eta_{j,t}$ are lender-by-period fixed effects. We keep lenders with activity in at least three counties within a period. Following Gilchrist et al. (2018), observations are weighted by the geometric mean of the lender's market share in the county and the county's share in the lender's activity at the start of the period. Because we are interested in counties' exposure to relative lender expansions and contractions, we re-center the estimated lender effects in each period so that their loan-origination-weighted mean is zero.

We estimate these shocks in long differences over 2000–2005, 2005–2010, and 2010–2015. Using long differences both increases power and aligns the credit shock measure with the boom, bust, and rebound phases of the housing cycle studied in the main text. Within each period, the county-by-period fixed effects absorb changes in lending driven by local economic or housing-market conditions, while the lender-by-period fixed effects capture changes in lenders' aggregate mortgage activity across the markets in which they operate.

We then aggregate the lender shocks to the county level using lenders' initial market shares:

$$S_{c,t} = \sum_{j \in C} \left(\frac{L_{c,j,t}}{\sum_{j \in C} L_{c,j,t}} \right) \hat{\eta}_{j,t},$$

so that counties more exposed to lenders experiencing above-average national lending growth receive larger positive shocks.

Although equation G1 removes contemporaneous county-level demand shifts, a remaining concern is that counties exposed to particular lenders may differ systematically in ways that are correlated with longer-run housing demand. To further purge the raw county shock of such demand-

related variation, we residualize $S_{c,t}$ with respect to county-period measures of local loan demand:

$$S_{c,t} = \gamma_1 \hat{\alpha}_{c,t}^{\text{Loan}} + \gamma_2 \hat{\alpha}_{c,t}^{\text{Lend}} + \lambda_c + \lambda_t + \chi_{c,t}, \tag{G2}$$

where $\hat{\alpha}_{c,t}^{\text{Loan}}$ and $\hat{\alpha}_{c,t}^{\text{Lend}}$ are county-period fixed effects obtained from versions of equation G1 estimated using, respectively, the log change in loan counts and the log change in lending volume as the dependent variable. These terms capture extensive- and intensive-margin local lending conditions. We also include county and period fixed effects and weight the regression by county population from the IRS. Our baseline credit supply shock is the residual

$$\tilde{S}_{c,t} = \hat{\chi}_{c,t}.$$

The identifying assumption is that exposure to lender-specific national lending shifts is not systematically correlated with within-county relative changes in housing demand across high- and low-*HOCO* groups, conditional on the controls included in the main text. A violation would require that counties exposed to particular lenders also experience differential changes in the relative demand for homeownership across *HOCO* groups that are not captured by overall local lending conditions. We view this as unlikely given the construction above. In the main specifications, we further mitigate this concern by including county-group fixed effects, which absorb permanent differences across places in the relative homeownership trajectories of different *HOCO* groups.

Appendix Figure IVI shows the geographic distribution of the resulting shocks for 2000–2005 and 2010–2015. The figure highlights substantial cross-county variation in exposure, including within regions and states, as well as meaningful turnover in the locations receiving the largest positive and negative shocks across periods. This variation underlies the identifying power of the estimates in Section IV.

Appendix H. Additional Tables

Table HI: Homeownership rates for selected countries of origin computed from international census data

Rank	Origin	$\%\text{Pop}^{00}_{USA}$	% Married	% $HOCO^{All}$	% $HOCO^{Married}$
		Census 2000 Birthplace			
1	Mexico	3.24	67	66	69
2	Philippines	0.51	64	77	77
3	India	0.47	75	79	79
4	China	0.38	71	74	86
5	Canada	0.38	55	68	81
⋮	⋮	⋮	⋮	⋮	⋮
	United States	86.5	54	67	83

Notes: This table summarizes our homeownership rates, by country of origin ($HOCO$) for the largest country-of-origin groups among foreign-born immigrants residing in the U.S. These homeownership rates are computed from harmonized country census microdata (obtained through IPUMS) by marriage status for household heads. Only countries for which this data was available are shown. The table is ranked by the population share of each immigrant group among U.S. household heads who are 18-69 years old in 2000 (according to Decennial Census data), and also shows the share of household heads from that origin who are married. It shows the average homeownership rate in the country of origin, as well as the rate among married household heads. We also provide the US native-born population data for the year 2000. Data for homeownership by marriage status in China is from the 2016 China Family Panel Studies Survey. Due to a lack of population weights by marriage status in the origin country, we approximate China "All" population marriage rates by taking a weighted average of married and single marriage rates, using prevalence of these statuses among Chinese-born US residents. Indian data is obtained from the 2015-2016 wave of the Demographic and Health Survey. Full table of 70 countries with homeownership data available upon request. Population shares of the 5 largest country-of-origin groups for which we could not compute homeownership data by marriage status: Germany, 0.47%; Cuba, 0.36%; South Korea, 0.29%, Japan, 0.21%, Taiwan, 0.15%.

Table HII: Effects of HOCO on Tenure Choice: Country Characteristic Controls

Dependent var.:	Household Homeownership$_{it}$ (in %)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HOCO (%)	0.092***	0.165***	0.157***	0.163***	0.108**	0.179***	0.168***	0.167***
	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)
GDP per capita (2021 USD)		0.082						-0.098
		(0.05)						(0.13)
Urban population (% of total)			0.008					0.118**
			(0.04)					(0.05)
Consumer price inflation (annual %)				-0.147				0.012
				(0.12)				(0.24)
Domest. credit by fin. sector (% of GDP)					0.030***			0.022
					(0.01)			(0.02)
Property rights index						0.032		0.106
						(0.05)		(0.10)
Investment freedom index							-0.001	-0.074
							(0.06)	(0.07)
Observations	3,436,252	3,436,252	3,385,624	3,223,662	2,100,486	3,175,701	3,175,701	2,060,006
R-Squared	0.32	0.31	0.31	0.31	0.30	0.31	0.31	0.30
	Addl. controls & fixed effects							
Household Characteristics	X	X	X	X	X	X	X	X
Imputed HH Wealth	X	X	X	X	X	X	X	X
Commuting Zone × Year FE	X	X	X	X	X	X	X	X
Major religion FEs	X							X

Notes: This table shows the results of estimating the same specification as column 4 of Table I, and additionally controlling for country-of-origin characteristics. Each column adds in one characteristic as a control variable. On the one hand, we add Major religion fixed effects (column 1) and 2000-2020 averages of economic characteristics from the World Bank's World Development Indicators: GDP per capita (column 2), urbanization rate (column 3), annual inflation rate (column 4), and domestic credit to private borrowers as a share of GDP (column 5). On the other hand, we include indicators of property rights (column 6) and investment freedom (column 7) from the Heritage Foundation, which are averaged over 2000-2008, and for which higher numbers indicate more rights. Column 8 jointly includes all of these characteristics and fixed effects. Heteroskedasticity-robust standard errors clustered at the origin country level shown in parentheses. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table HIII: Effects of HOCO on Tenure Choice: Immigrated as Children

Dependent var.:	Household Homeownership$_{it}$ (in %)			
	(1)	(2)	(3)	(4)
HOCO (%)	0.995***	0.147***	0.138***	0.122***
	(0.09)	(0.05)	(0.04)	(0.04)
Observations	1,224,869	1,221,160	1,221,160	1,220,817
R-Squared	0.08	0.26	0.29	0.31
Household Characteristics	No	Yes	Yes	Yes
Commuting Zone FE	No	No	Yes	No
Commuting Zone × Year FE	No	No	No	Yes

Notes: This table shows the results of estimating Equation 1 in a pooled household level sample of households with foreign-born heads who are 18-69 years old, for the years 2000 and 2005-2019, and *who immigrated to the US when they were 18 years old or younger* as inferred from their age and immigration year. The dependent variable is an indicator of homeownership in percent (so 100 indicates that the household owns their home). The independent variable is the homeownership rate in the country of origin (*HOCO*) of the household head for people with the same marriage status (single, married, separated/divorced, widowed). Household characteristic control variables consist of: HH Income, (linear and ventile indicators), predicted non-housing household net worth, Quadratic function of Age of HH head and 5-year age group indicators, indicators for educational achievement of HH head, Indicators for # of children and # of relatives in the HH, Indicators for discretized years since immigration of head, marital status of HH head, and the interaction between marital status and years-since-immigration categories. Heteroskedasticity-robust standard errors clustered at the origin country level shown in parentheses. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table HIV: Changes in homeownership propensity 2000-2005 by HOCO

Dep. Var.:	Δ Predicted Homeownership 2000-2005 (%)			
Sample:	Renters	Owners	Low Hoco	High HOCO
	(1)	(2)	(3)	(4)
HOCO	0.031***	0.001		
	(2.822)	(0.182)		
Homeownership propensity (2000)			0.022***	-0.006
			(4.321)	(-1.173)
Constant	0.731	3.114***	1.798***	3.659***
	(1.020)	(5.516)	(4.503)	(6.492)
Observations	177,432	201,270	183,135	195,567

Notes: This table shows the estimated coefficients for the lines of best fit shown in Figure 5. This tables shows the linear estimate of the association between a household's HOCO (columns 1 and 2) and predicted propensity to own a home in the year 2000 (columns 3 and 4) with the change in predicted homeownership, holding the household's demographics constant, over the 2000-2005 period. Columns 1 includes only renter households as of 2000, and column 2 includes only homeowners in the sample. Column 3 only includes households with below median HOCO in the sample, and column 4 only includes above median HOCO households. The predicted homeownership in 2000 and 2005 is computed from a linear prediction of homeownership based on household demographics in the cross-section in each year - see Figure 5 for further details. T-statistics based on standard errors clustered at the country of origin level are shown in parentheses.

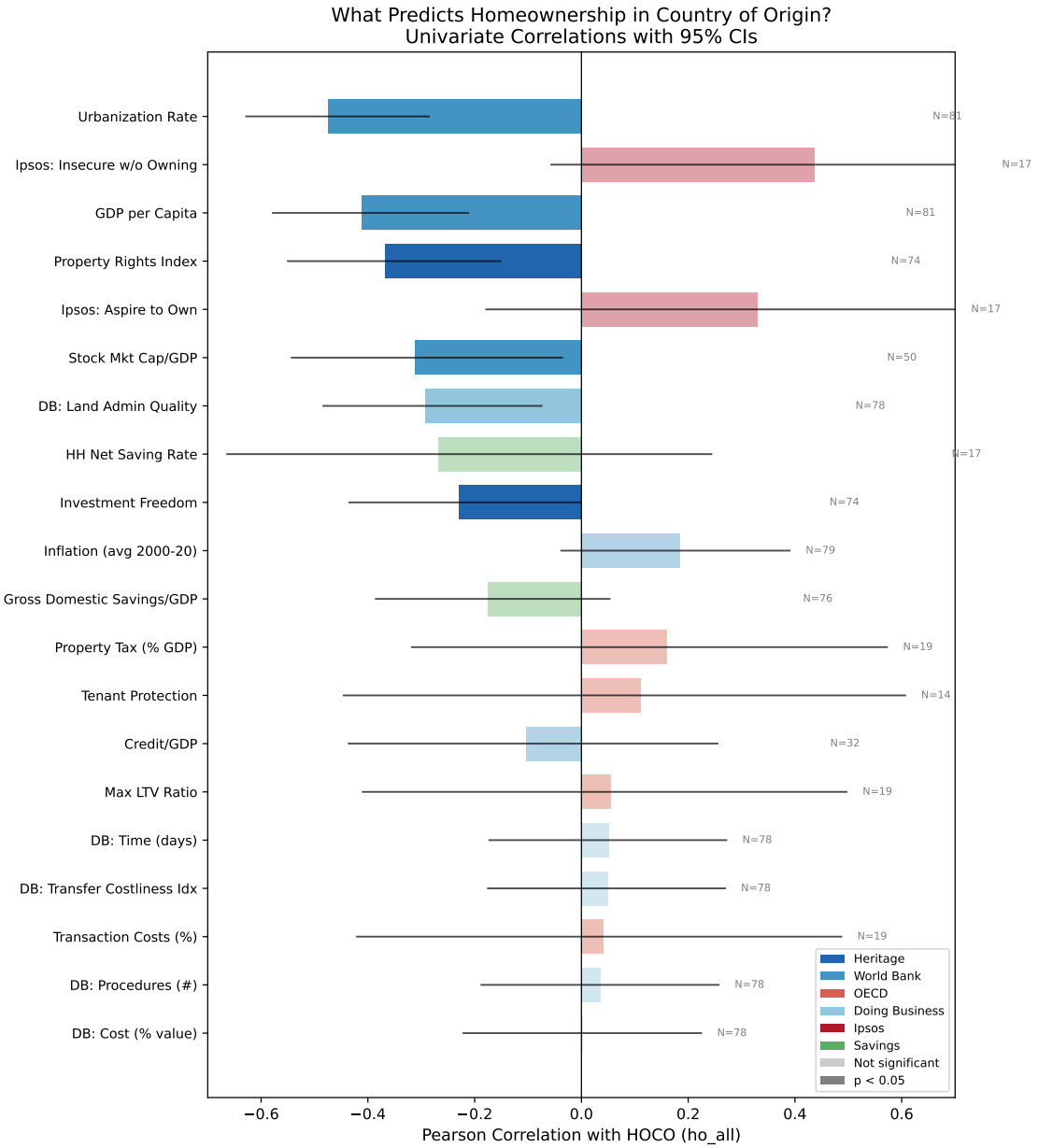
Table HV: The ownership wedge in the structural literature

Paper	Device	Identified from	Magnitude (as reported)
Kaplan et al. (2020)	Rental-company operating cost per unit rented (cost side)	Ownership rate of households < 35: 39% (1998)	$\approx 0.8\%$ of house value per year
Kaplan et al. (2020)	Owner-occupied house yields ωh services (utility side)	Aggregate ownership rate: 66% (1998)	$\omega = 1.015$; $\approx 0.5\%$ of consumption for the median owner
Mabille (2023)	Additive utility shifter for owners, by region	Ownership rates in low- / high-price MSAs: 69% / 67%	0.82 / 0.90 utils (annualized); not dollarized
Coven et al. (2026)	Additive utility shifter for owners	California ownership rate: 61% (2019 ACS)	0.529 utils per period; not dollarized
This paper	Origin-specific premium γ_i in the own–rent user-cost gap	Origin groups' ownership relative to natives within CZ×year, converted by a market-level price sensitivity	Distribution: SD \$6.0K/yr across origins ($\approx 3\%$ of house value); HOCO-linked part $\approx$ \$2K/yr across the HOCO IQR

Magnitudes are as reported by each paper; "% of house value" and "% of consumption" are the papers' own units. In our sample the tenure-neutral house is worth ≈\$202K and its imputed rent ≈\$13K per year, so 1% of value is \$2K per year or 15% of imputed rent.

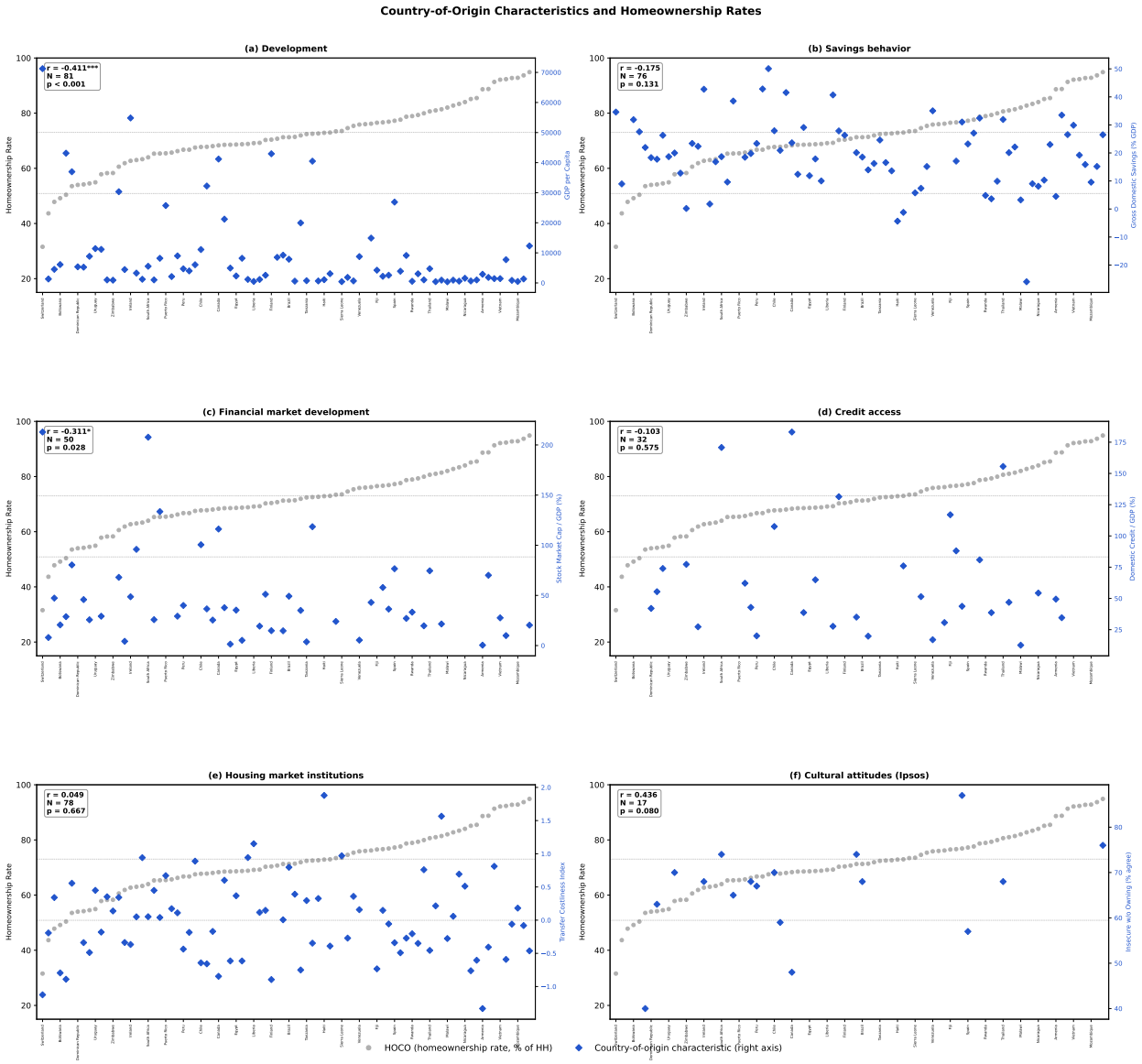
Appendix I. Additional Figures

Figure II: Correlates of Homeownership in Country of Origin



Notes: This figure reports Pearson correlation coefficients (r) between homeownership in country of origin (*HOCO*, measured as `ho_all` from IPUMS International census microdata) and 20 country-of-origin characteristics, sorted by absolute magnitude. Horizontal bars show point estimates; black lines show 95% confidence intervals computed via the Fisher z-transformation. Dark shading indicates statistical significance at the 5% level; light shading indicates $p > 0.05$. Sample sizes (N) vary across variables due to differential country coverage and are reported to the right of each bar. Colors indicate data source: Heritage Foundation (dark blue), World Bank WDI (medium blue), OECD Housing Database (red), World Bank *Doing Business* (light blue), Ipsos Global Advisor survey (dark red), and national savings statistics (green). See Appendix E for data source details.

Figure III: Origin-Country Characteristics and HOCO: Selected Scatterplots



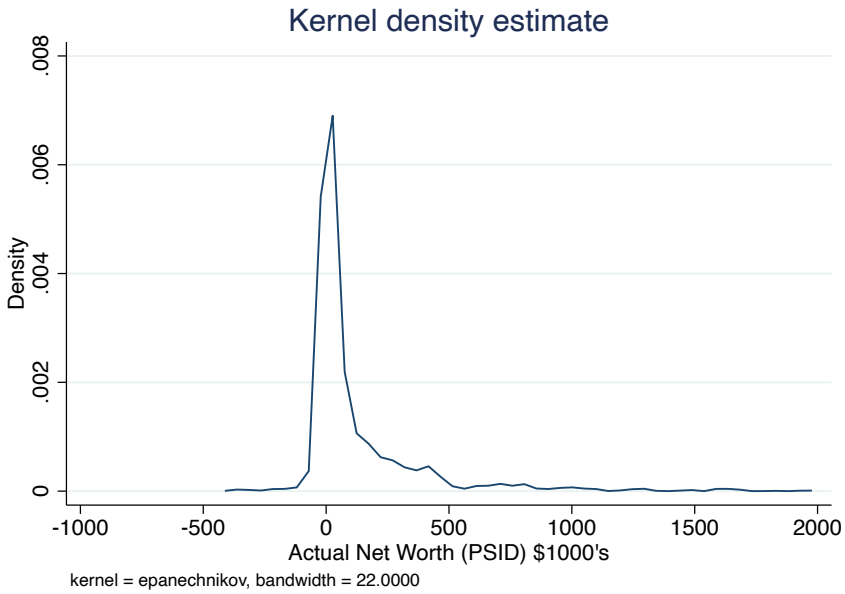
Notes: Each panel plots homeownership in country of origin (gray circles, left axis) against one origin-country characteristic (blue diamonds, right axis), with countries ordered along the horizontal axis by ascending *HOCO*. Dashed horizontal lines mark the 25th and 75th percentiles of the *HOCO* distribution. Pearson correlation coefficients (r), sample sizes (N), and p-values are overlaid. The six variables are selected to represent the primary categories of potential exclusion-restriction threats: (a) economic development, (b) savings behavior, (c) financial-market development, (d) credit access, (e) housing-market institutions, and (f) cultural attitudes toward homeownership. See Appendix E for data source details.

Figure IIII: Actual vs. Predicted Net worth in the PSID

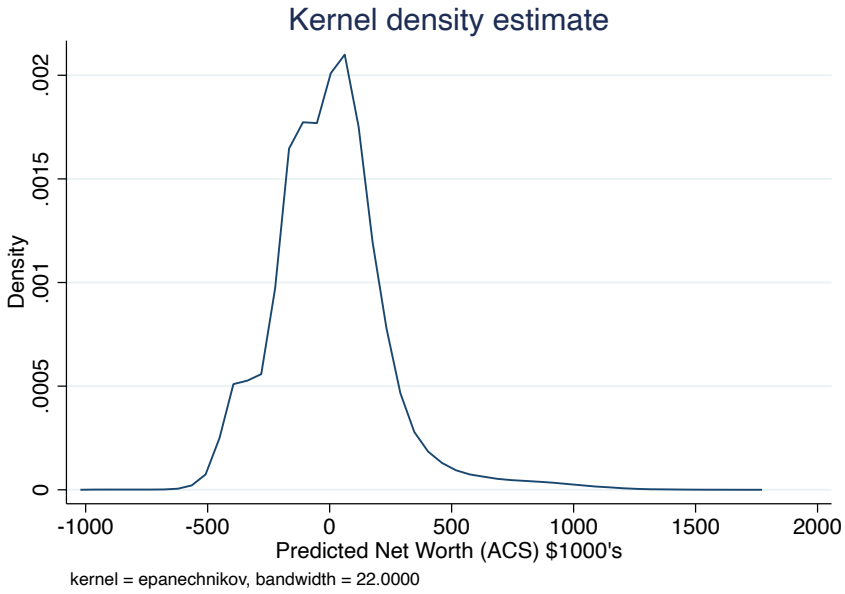


Notes: These figure plots a scatterplot of actual non-housing network vs. predicted housing net worth as in section C. Weoverlay the scatterplot with the 45 degree line which would reflect perfect prediction. Data comes from the 1997 and 2017 PSID waves, restricted to immigrant households. Net worth constructed according to Pfeffer et al. (2016), excuding assets and liabilities associted with the primary residence. Distribution of actual non-housing net worth trimmed at $2 million for ease of inspection due to the long right tail in wealth, which drops 8/1,049 households from the figure.

Figure IIV: Distributions of Networths, by Dataset



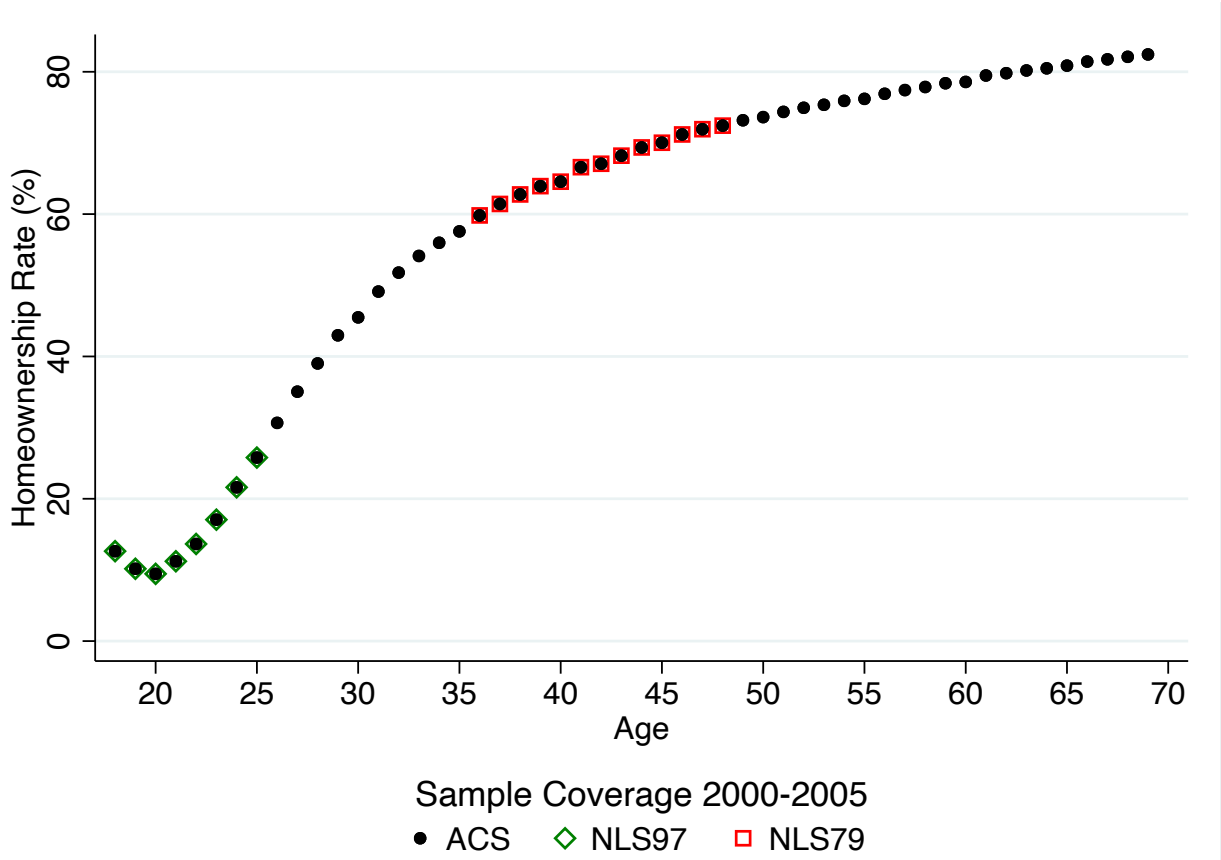
(A) PSID



(B) ACS

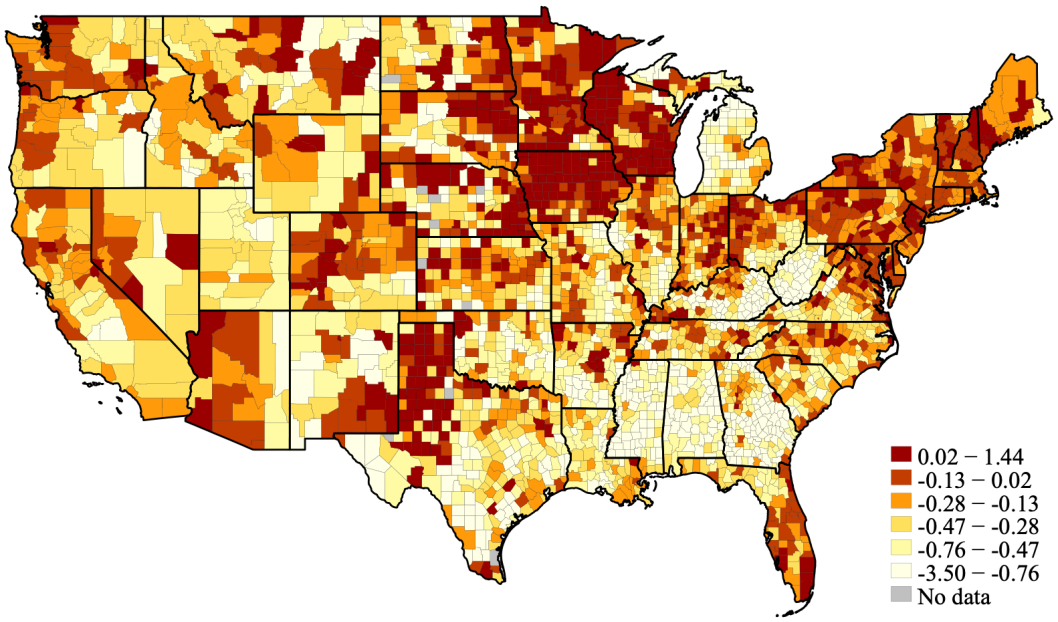
Notes: These figures plot the kernel density distributions of non-housing net worths from the PSID and ACS immigrant samples. We show actual non-housing net worth for the PSID, and imputed net worth according to Appendix C for the ACS sample. Distribution of actual non-housing net worth in the PSID trimmed at $2 million for ease of inspection due to the long right tail in wealth, which drops 8/1,049 households from the figure.

Figure IV: Sample Coverage of Candidate Datasets

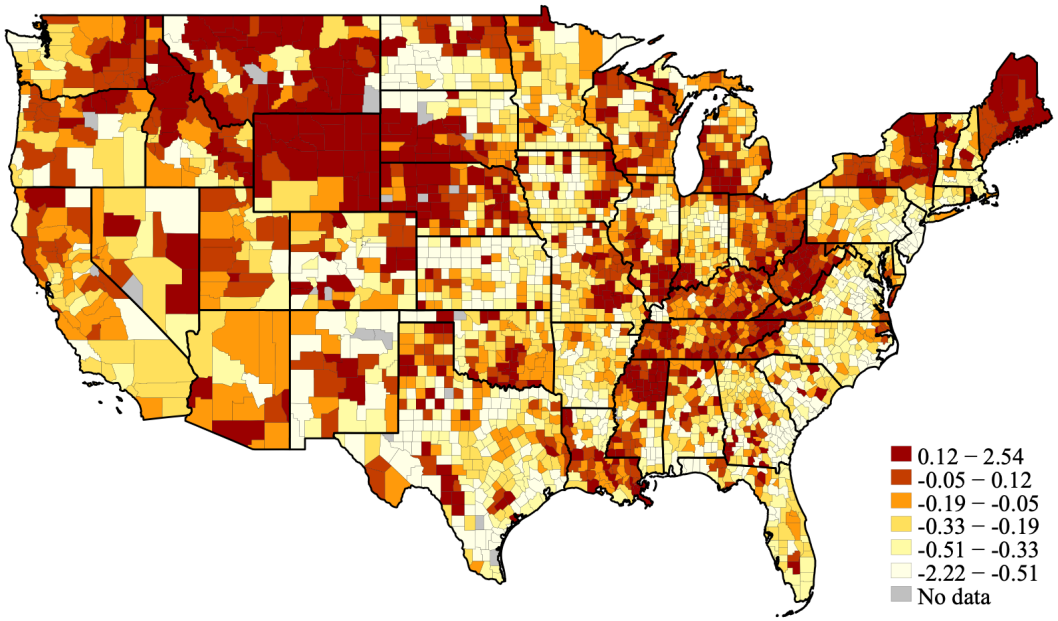


Notes: This plot shows the available sample coverage between 2000 and 2005, our period of study for credit shock analysis as in Section IV The green diamonds highlight the ages available for study in the National Longitudinal Survey of Youth, 1997 (NLS97) between 2000-2005. The red squares note the ages available for the NLS79. The solid dots plot the ages available using the US Census and ACS microdata.

Figure IVI: Spatial Distribution of credit supply shocks



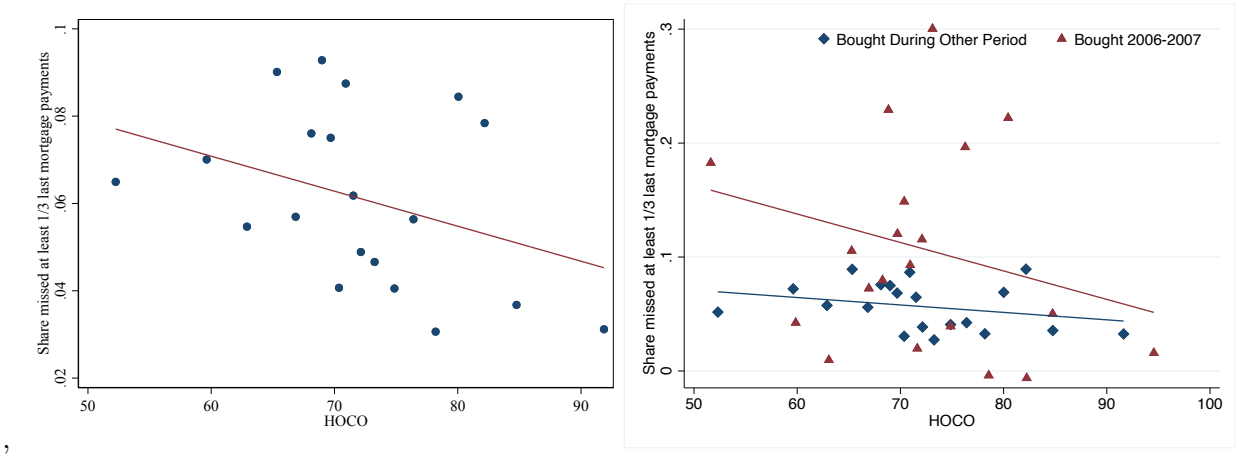
(A) Credit supply shocks: 2000-2005



(B) Credit supply shocks: 2010-2015

Notes: These maps plot the distribution of credit supply shocks at the county level. Darker colors have higher values of implied loan growth, and lighter colors denote lower values.

Figure IVII: Delinquencies by HOCO, and period of Home Purchase



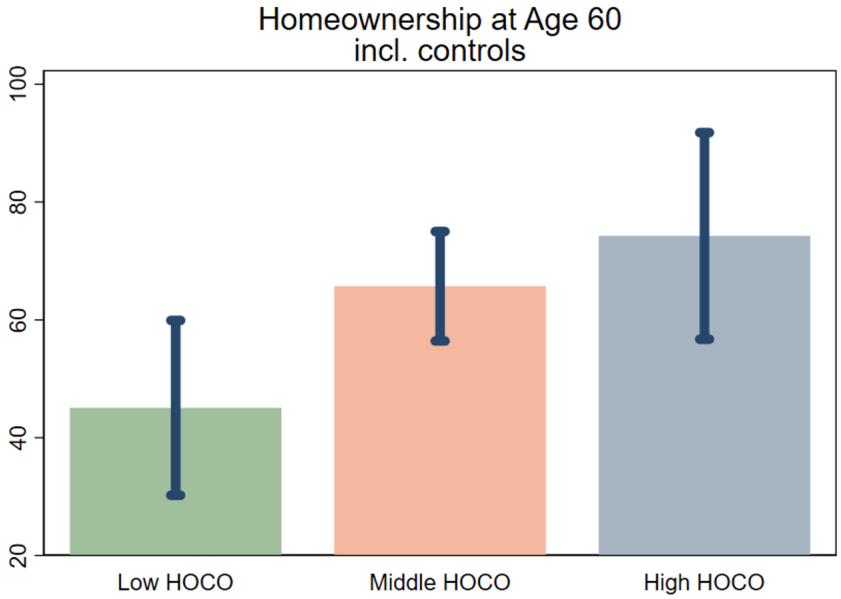
,

(A) Delinquencies by HOCO

(B) Delinquencies by HOCO, period Bought

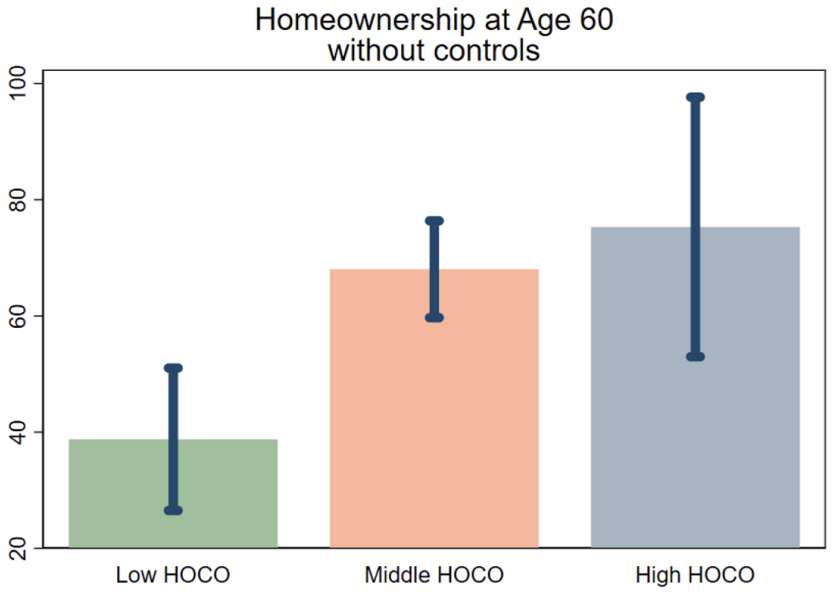
Notes: Binscatter using data from the 2013 and 2017 waves of the AHS. Sample covers homeowners with foreign born heads. Controls include household head education, race, ethnicity, marital status, number of family members and children in the home, and quadratics in household income, years in the US of household head, and age of household head, as in prior specifications and add debt-to-income and cost-to-income ratios. We include year-by-CBSA fixed effects. Heteroskedasticity-robust standard errors clustered at the commuting zone level shown in parentheses. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Figure IVIII: Homeownership by HOCO Tercile at Retirement Age, with controls



Notes: This figure shows the average homeownership in percentage points among foreign-born respondents in the Health and Retirement Study with a homeownership in their country of origin that is in the lowest, middle, or highest tercile of homeownership in the country of origin (*HOCO*). The graph and the assignment into terciles exclude Mexican origin foreign-born and residents where we were unable to assign a *HOCO* score. The homeownership rates are adjusted for the effect of the same control variables as included in column 2 of Table III.

Figure IIX: Homeownership by HOCO Tercile at Retirement Age



Notes: This figure shows the average homeownership in percentage points among foreign-born respondents in the Health and Retirement Study with a homeownership in their country of origin that is in the lowest, middle, or highest tercile of homeownership in the country of origin (*HOCO*). The graph and the assignment into terciles exclude Mexican origin foreign-born and residents where we were unable to assign a *HOCO* score. The homeownership rates are adjusted for the effect of the same control variables as included in column 2 of Table III.