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RACIAL DISPARITIES IN HOUSING RETURNS

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ABSTRACT

We document the existence of a racial gap in realized housing returns that is an order of magnitude larger than disparities arising from housing costs alone, and is driven by differences in distressed home sales (i.e., foreclosures and short sales). Black and Hispanic homeowners are both more likely to experience a distressed sale and to live in neighborhoods where distressed sales erase more house value. However, absent financial distress, houses owned by minorities do not appreciate at substantially slower rates than houses owned by non-minorities. Racial differences in liquid wealth and income stability are important determinants of differences in distress.

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1 Introduction

Racial wealth disparities in the US are large and persistent. The wealth of the median Black household is about one-tenth of median White wealth, and median Black wealth has rarely exceeded $20,000 since at least 1949.[1] At the same time, the Black homeownership rate has increased dramatically over the last century, from 23% in 1920 to 45% in 2021 (Collins and Margo 2011; Callis et al. 2021). Given that housing is the single-largest asset class held by middle-class households (Campbell, 2006), and that returns to housing are comparable to returns to other risky assets and significantly higher than returns to safe assets (Jordà et al., 2019), the wealth held by middle-class Black Americans has remained puzzlingly low.

While homeownership represents an attractive savings vehicle for Americans, who benefit from federal mortgage guarantees and tax deductions, mortgaged homeownership is different than most other savings vehicles because it requires sufficient income stability and liquidity to make monthly mortgage payments. This requirement may be particularly relevant for disadvantaged minorities, who are more likely to be financially distressed.[2] However, there is little evidence on the extent to which this requirement limits the effectiveness of homeownership as a savings vehicle for minorities.

This study is the first to estimate the racial/ethnic gap in housing returns using administrative data on individual housing transactions.[3] We find that Black and Hispanic homeowners realize substantially lower returns than White homeowners because minorities are both more likely to experience a distressed home sale (i.e., foreclosure or short sale) and have more home value eliminated during distressed home sales. Higher rates of illiquidity and income instability among minorities can explain a large share of the underlying differences in financial distress. In the presence of such underlying differences, the early-2000s expansion in credit supply exacerbated pre-existing racial gaps in returns. These results help explain why minority wealth has remained persistently low despite rising homeownership rates and decades of policies designed to improve homeownership opportunities for minorities.[4] Our findings suggest that policies that prevent financial distress by ameliorating upstream disparities, or that help minorities keep their homes when they become financially distressed, are important complements to policies that aim to narrow the wealth gap by promoting minority homeownership.

We document the existence of a substantial gap in housing returns using administrative data that links homeowner race and ethnicity to real estate transactions, which allow us to observe the purchase and sale prices received by each homeowner over a 30-year window. We consider two

[1]Kuhn et al. (2020) report wealth by race in 2016 dollars since 1949 from the Survey of Consumer Finances. Median Black wealth briefly exceeded $20,000 in the years prior to the Great Recession and the accompanying collapse in house prices. Bhutta et al. (2020) document similarly low levels of wealth for Hispanic households.

[2]Racial and ethnic disparities in financial distress were especially pronounced in the Great Recession, during which the foreclosure rate among new Black and Hispanic homeowners was nearly double that of their White counterparts (Bocian et al., 2010).

[3]For conciseness, we henceforth use race to refer to race and ethnicity collectively.

[4]Policies promoting minority homeownership date back to the 1968 Fair Housing Act, and have been supported by Republican and Democratic policymakers alike (Bush 2004; Warren 2019). Most recently, housing policy under the Biden-Harris administration has had the explicit goal of narrowing the racial wealth gap (White House, 2021).

complementary measures of annual housing returns: the unlevered return, which simply compares the sale price to the purchase price; and the levered return (i.e., internal rate of return), which takes into account mortgage debt and housing costs. We find that both the unlevered and levered returns realized by minority homeowners are substantially lower than those realized by White homeowners. Among mortgaged home purchases, which comprise about three-fourths of all purchases, we find that Black and Hispanic homeowners realize unlevered returns that are both 2.3 percentage points lower per year than White homeowners.[5]

The racial gap in housing returns can be statistically accounted for by distressed home sales (i.e., foreclosures and short sales); within non-distressed sales, the Black-White gap in annual returns falls to less than 40 basis points, and the Hispanic-White gap is inverted. Two distinct factors underlie the role of distressed sales. First, Black and Hispanic homeowners are twice as likely as White homeowners to experience a distressed sale during our sample period. Second, minority homeowners live in neighborhoods where distressed sales carry larger sale price discounts.[6] That is, within distressed sales, unlevered annual returns are several percentage points lower for Black and Hispanic homeowners. To unpack the explanatory power of distressed sales for the gap in returns, a simple decomposition indicates that distressed sale frequency (the former factor) is quantitatively much more important than distressed sale severity (the latter). Moreover, average returns for *non-distressed* sales among homeowners who purchase their homes in the same place and time are very similar across race, implying that on average, homes owned by minorities do not appreciate at substantially slower rates than those owned by non-minorities.

We find similar patterns when analyzing differences in levered returns, which take into account the fact that housing is typically purchased using debt. Our measure of levered returns allows us to capture both higher rates of leverage among minorities as well as lenders bearing the cost of underwater foreclosures. Mean annual levered returns for Black and Hispanic homeowners are 5.0 and 9.6 percentage points lower than for White homeowners, respectively. As with unlevered returns, racial disparities in levered returns can be accounted for by differences in distressed sales. For the sample of non-distressed sales, higher rates of leverage allow Black and Hispanic homeowners to realize levered returns that are substantially higher than White homeowners.

Our estimated gaps are robust to a number of alternative approaches to constructing returns. First, our baseline calculation of levered returns does not take into account racial differences in homeownership costs. Including prior estimates of racial differences in interest rates (Gerardi et al., 2020), taxes (Avenancio-León and Howard, 2019), rent-to-price ratios (Demers and Eisfeldt, 2022), as well as differences in home maintenance only slightly reduces estimated racial gaps in levered returns. Second, we find no evidence of differences in returns among cash purchases, which by definition cannot result in a distressed sale. Since cash purchases represent about one-fourth of all

[5] We analyze home purchases made in cash separately, and find no evidence for racial differences in returns among cash purchases.

[6] Recent work has shown that many mortgage defaults occur among homeowners with positive amounts of home equity (Low 2021; Ganong and Noel 2020b), meaning that distressed home sales tend to eliminate substantial amounts of homeowner wealth because these sales entail large price discounts (Campbell et al., 2011).

purchases, the racial gap among all purchases is moderately smaller than our baseline gap among mortgaged purchases. Lastly, we also find large racial gaps in alternative measures of housing returns such as net present value and total returns.[7]

To help interpret the magnitude of the racial gap in returns, we conduct a simple counterfactual exercise that estimates the contribution of the gap to differences in housing wealth at retirement age. The estimated contribution is substantial: equalizing housing returns reduces the Black-White gap in housing wealth at retirement by 37%. In contrast, equalizing rates of first-time home purchases over the life cycle has virtually no impact because the gap in returns nullifies the benefit of purchasing a home at an earlier age. Equalizing both returns and purchase rates reduces the gap by 49%. These calculations suggest that addressing the racial gap in returns is necessary in order for policies that promote homeownership to be effective in narrowing the racial wealth gap.

Although distressed sales can statistically account for gap in returns, distressed sales themselves are an endogenous result of both borrower and lender behaviors and therefore influenced by underlying factors like market conditions and homeowner financial stability. For example, it is possible that higher rates of minority distress occurred because minority homebuyers disproportionately purchased homes before the Great Recession, or because they tend to have lower incomes. We find that about one-third of the racial gap in unlevered housing returns can be attributed to purchase year and county, and about one-sixth of the remaining gap can be explained by income, family structure, gender, and leverage. To shed more light on the explanatory power of these factors, further heterogeneity analysis reveals that although the gap in returns exists even among the safest borrowers (i.e., high-income couples with low leverage), the gap is larger among less economically stable groups (e.g., single-headed households). In addition, even controlling for neighborhood, which absorbs both the causal effects of neighborhoods and selection of homeowners into neighborhoods, the racial gaps in returns remain economically meaningful.

The finding that minorities are more likely to experience distress relative to observationally similar White homeowners is puzzling given that, in principle, adequate credit screening by lenders could avoid higher rates of distress among minority homeowners, even if minority households are less financially stable on average. Adding to this puzzle, risky households should have a relative disincentive to become homeowners because distressed sales are very costly outcomes (Ganong and Noel, 2020b). Linking detailed data on financial characteristics from mortgage servicers and credit bureaus to our study sample, we show that about half of the racial gap in distress can be attributed to higher observable credit risk among minorities. This finding suggests that changes in lending standards, either by private lenders or by GSEs, can impact the racial gap in distress.

What drives the differences in distress that remain after controlling for observable credit risk? We show that differences in liquidity and income stability play an important role. Household surveys reveal that minority homeowners have substantially less liquid assets and face more income

[7]Note that our analysis pertains to financial returns. Homeownership offers many non-financial benefits, such as consumption value and access to desirable neighborhoods. To probe whether the latter benefit may compensate minority homeowners for lower returns, we analyze migration patterns associated with home purchase. Our results suggest limited scope for improvements in neighborhoods to compensate for lower returns.

instability than White homeowners. To document the importance of differences in liquidity, we estimate responses to liquidity shocks in the form of changes in monthly mortgage payments from adjustable-rate mortgage resets. We find that minorities are substantially more likely to default in response to a liquidity shock. Very little of this excess sensitivity can be predicted by characteristics observable at mortgage origination. Although this finding suggests that higher levels of minority risk are not observable to lenders, it remains puzzling why higher-risk minorities are not more likely to select out of homeownership. We analyze surveys of homeowners and find evidence that minority homeowners are not fully aware of the higher levels of risk they face, which can explain selection into homeownership by unobservably high-risk minorities.

In light of these higher levels of underlying risk among minorities, an important question is whether expansions in credit supply exacerbate racial disparities among homeowners. The primary credit supply expansion during our sample period occurred during the early 2000s, just prior to the Great Recession. To assess the impacts of this expansion on racial disparities, we compare outcomes for purchases that were financed through private securitization and portfolio lending, which exhibited a substantial credit expansion, to those that were financed by Fannie Mae, Freddie Mac, and FHA loans, which did not exhibit an expansion. We find that purchases that were financed by expanded private and portfolio lending exhibited substantially increased rates of distressed sales during the Great Recession. We calculate that over half of the increase in the racial gap in distress sales can be attributed to expansions in credit supply.

Since the racial gap in housing returns is ultimately a product of underlying differences in borrower characteristics like levels of income, liquidity, and income stability, closing the gap likely requires addressing upstream racial disparities, such as in hiring discrimination (Bertrand and Mullainathan 2004; Kline et al. 2021) and income stability (Ritter and Taylor 2011; Hardy et al. 2018). Moreover, our results lend support to recently-proposed policies designed to prevent distress among minority homeowners by intervening at the point of home purchase, such as by creating liquidity reserves (Goodman et al., 2023). In considering policies that can help already-distressed minority homeowners avoid foreclosure, we provide evidence that policies that provide payment flexibility would have disproportionate benefits for minority homeowners. In particular, we find that the causal impact of mortgage modifications for distressed homeowners is similar by race, suggesting larger benefits for minority homeowners (who are more likely to become distressed).

In the long run, racial disparities in homeownership and wealth accumulation will naturally change along with future economic conditions. This is indicated by the larger Black-White gap in foreclosures during the Great Recession, although the gap exists in every year of our sample window and persists to the present day. In contrast, the Hispanic-White gap in foreclosures exceeded the Black-White gap during the Great Recession, but has dissipated since 2017. Yet even after the Great Recession, we find that counties with higher unemployment rates and lower house price growth exhibited larger racial gaps in foreclosures. In light of the relatively favorable economic conditions in recent years, it would be premature to conclude that the economic relevance of these gaps has diminished. In particular, it is entirely plausible that future downturns may disproportionately increase distressed sales among minority homeowners, particularly given more financial fragility and higher employment cyclicality among minority workers (Hoynes et al., 2012).

By providing the first estimates of racial gaps in housing returns using data that capture distressed sales, this study reveals gaps that are substantially larger than prior estimates. Prior estimates relied on either neighborhood-level price appreciation (Anacker 2010; Kahn 2024) or homeowner self-reports of home value (Flippen 2004; Faber and Ellen 2016; and in subsequent work Wolff 2022), neither of which capture the impact of distressed sales. By using data on actual purchase and sale prices, we show that accounting for distressed sales is critical for accurately measuring racial differences in realized housing returns. Indeed, in our sample of homeowners who do not experience a distressed sale, realized returns are relatively similar by race, in line with prior estimates.

Prior studies have documented that minorities pay higher housing costs due to disparities in purchase prices (Myers 2004; Ihlanfeldt and Mayock 2009; Bayer et al. 2017), tax assessments (Avenancio-León and Howard, 2019), interest rates and fees (Bartlett et al. 2019; Bhutta and Hizmo 2019; Fuster et al. 2020; Ambrose et al. 2020), and refinancing behavior (Gerardi et al., 2020). We find that in annual dollar terms, the racial gap in housing returns is an order of magnitude larger than each of these previously-documented disparities in housing costs. Thus, although these disparities undoubtedly depress returns for minorities, their overall impact on housing returns is modest. One can draw a similar comparison with studies finding that minority homeowners pay higher prices for homes. For example, Bayer et al. (2017) find racial differences in purchase prices on the order of 2%, a difference that is well within our estimated 40 basis point Black-White gap in returns among regular sales, when annualized over the ownership spell.

This study also contributes to prior work documenting racial differences in mortgage default (Berkovec et al. 1994; Rugh and Massey 2010; Gascon et al. 2017; Bayer et al. 2016; Reid et al. 2017; Gerardi et al. 2020) and income instability and liquidity (Wrigley-Field and Seltzer 2020; Ganong et al. 2020; Ritter and Taylor 2011). We identify the economic mechanisms that, in spite of countervailing forces like credit screening, result in racial differences in distress among homeowners. We document the role of income instability and liquid asset holdings in creating the gap that remains after controlling for observable characteristics. Although we find no evidence that the disparities we document are generated by differential lender treatment of minorities, we do find that the expansion of credit supply contributed meaningfully to larger gaps in distress during the Great Recession. This finding contributes to prior work that has identified the early-2000s expansion of credit as an important driver of subsequent mortgage default in the general population (Mian and Sufi, 2009).

Lastly, we contribute to the policy debate concerning solutions to the widely-studied racial wealth gap (Blau and Graham 1990; Barsky et al. 2002; Gittleman and Wolff 2004; Altonji and Doraszelski 2005; Hamilton and Darity Jr 2010; Kuhn et al. 2020; Derenoncourt et al. 2021) and racial gaps in homeownership (Charles and Hurst 2002; Gupta et al. 2021). Our study documents that racial gaps in housing returns are quantitatively important contributors to the overall racial wealth gap. This finding illustrates the limitations of policies that focus solely on increasing minority homeownership as a tool to build minority wealth, and lends support to policies seeking to reduce the incidence of financial distress among homeowners (Campbell et al. 2020; Alexandrov et al. 2022; Goodman et al. 2023).

The remainder of this paper proceeds as follows. Section 2 describes the merged administrative data. Section 3 documents the raw racial gap in housing returns, and Section 4 presents gaps controlling for observable characteristics. Section 5 explores mechanisms. Section 6 discusses policy implications. Section 7 concludes.

2 Data and Measurement

We use a series of linked administrative data sources to measure both realized housing returns and race and ethnicity at the household level. This section describes our data sources and primary measures of housing returns. Appendix C provides additional details on the data sources and sample construction.

2.1 Data Sources

Measuring realized housing returns requires observing purchase and sale prices. To do so, we use a nationwide panel of millions of residential properties containing records of both transactions and mortgages. This data source is derived from the records of local governments and is assembled by ATTOM, a private data provider. We refer to this data source as the "property data". We observe homeowner race and ethnicity in the Home Mortgage Disclosure Act (HMDA) data. By law, lenders must disclose certain information about new mortgage loans, including the self-reported race and ethnicity of loan applicants, as well as applicant income. With the exception of mortgages originated by small financial institutions that are exempt from these reporting requirements, these data capture the near-universe of mortgage originations going back to the 1990s.

The property data, which link mortgages to properties, are merged to the HMDA records by matching on mortgage origination year, Census tract, dollar amount, and lender name. This linkage is very similar to those used in previous research (e.g., Bayer et al. 2017; Avenancio-León and Howard 2019). We restrict to HMDA mortgages that are unique to the year, Census tract, amount, and lender name, and we require an exact merge on year, tract, transaction amount, and a fuzzy string match on lender name.[8]

In order to compute the rate of return associated with a given ownership spell, we develop an algorithm for identifying repeat sales of properties. This approach is similar to that in Goldsmith-Pinkham and Shue (2020), who use a sample of repeat sales to analyze gender differences in returns. We identify property purchases by restricting to arm's length, full-consideration transactions. To identify the future sale of the property, we select the next arm's length, full-consideration transac-

[8] All administrative data linkages used in this study were performed by the Fisher Center for Real Estate and Urban Economics at UC Berkeley, also used by Avenancio-León and Howard (2019).

tion of that property. We compute measures of name similarity to ensure that the buyer of the first transaction is the same as the seller in the second transaction. Appendix C provides more details on this algorithm.

Our primary analysis sample is comprised of owner-occupied properties for which we observe an initial purchase price and the buyer's race. We refer to this sample as our baseline sample of ownership spells, which is restricted to homes purchased with a mortgage. While we can observe cash purchases (i.e., homes purchased without a mortgage) and compute subsequent returns, we are unable to measure self-reported homeowner race for cash purchases. This is because the HMDA records that contain race only exist for mortgaged purchases.

Estimating housing returns using finite data entails a standard censoring problem resulting from our inability to observe realized returns for homeowners in our sample who had not yet sold before the end of our sample period. We take a simple approach to this censoring problem, which is to impute the estimated value as of April 2020 using the FHFA county-level house price index. For homes that were unsold by March 2020, we compute housing returns as if the property were sold for the estimated value in April 2020. Overall, this approach is conservative in that it likely underestimates racial gaps in housing returns. We discuss the reasons for this underestimation and provide adjusted estimates in Section 3.

We build our baseline sample of ownership spells from a sample of 146.8 million arm's length home purchases in the property records from 1990 to 2020. To arrive at our analysis sample, we make a number of sample restrictions, which are illustrated visually in Appendix Figure A1. We drop 34.5% of observations for which a purchase price is not recorded in the data, 7.0% of observations where the buyer cannot be identified as a person or trust, and 12.5% for which our repeat sales algorithm cannot be implemented (e.g., due to a missing transaction between consecutive ownership spells), resulting in 67.6 million purchases (note that this retains properties that were unsold by the end of March 2020). We are able to link 23.5 million purchases to HMDA mortgages between 1990 and 2016. A large part of this reduction in sample is due to the exclusion of cash purchases (which comprise about one-fourth of all purchases), while another part is due to the temporal coverage of the HMDA data. Between 2000 and 2016, the years with the highest coverage, this linkage drops a smaller share (49.0%) of purchases. We drop transactions with prices that are less than $10,000, combined loan-to-value ratios of more than 102.5%, and ownership spells that last less than 12 months or with sales after March 2020, which yields a sample of 21.5 million properties, each of which represents a separate ownership spell.[9]

We additionally restrict to owner-occupant households where the primary mortgage applicant in the HMDA data identifies as Hispanic, Non-Hispanic Black, or Non-Hispanic White, which results in a sample of 16.2 million ownership spells starting between 1990 and 2016.[10] Due to our finite

[9]We exclude ownership spells lasting less than 12 months because such "flippers" often make substantial renovation expenditures, which we cannot observe in our data and could meaningfully impact the calculation of returns. Consistent with such behavior, average returns calculated including spells lasting less than 12 months are higher, although racial differences are fairly similar. Results are presented in the Online Appendix.

[10]Since our focus in this study is on historically disadvantaged minorities, our main analysis excludes Asian homeowners, who represent 5.9% of observations, as well as homeowners of other races/ethnicities. We present analysis

sample window, a larger share of later ownership spells remain unsold as of 2020. Appendix Figure A2 plots the share of properties that are unsold by 2020 for each purchase year. We drop properties that were purchased in 2015 or later because sale prices would have to be imputed for over 80% of these ownership spells. Together, these restrictions yields a baseline analysis sample of 13.6 million ownership spells. Henceforth, we refer to this sample as our baseline sample of ownership spells. Note that our baseline sample includes properties that remain unsold as of the end of our sample window. Panel A of Table 1 presents summary statistics for the baseline sample of ownership spells. Due to the relatively poor coverage of the property data prior to 2000 and in certain so-called non-disclosure states, about 97% of the ownership spells occur in or after 2000 in 40 states.[11] In our sample, 8% of households identify as Non-Hispanic Black, and 14% identify as Hispanic of any race.

We draw on a number of additional administrative data sources that are linked to the property data. To compute homeowner cash flows that are used for computing levered rates of return, we make use of mortgage servicing records from McDash. We measure a broad range of financial behaviors and outcomes appearing in credit bureau records from Equifax. To estimate the causal impacts of mortgage modifications, we measure modifications in mortgage records from Fannie Mae, Freddie Mac, and ABSNet. To analyze changes in neighborhood quality associated with purchasing a home, we analyze address histories provided by Infogroup. These data sources and their linkages to the property data are described as they arise throughout this paper, and in greater detail in Appendix C.

2.2 Identifying Distressed Home Sales

The property data allow us to separately analyze ownership spells that end in a distressed sale. There are two main types of distressed sales: foreclosures and short sales. If a borrower stops making mortgage payments, the lender can foreclose on the home and sell it to recover the outstanding mortgage balance. In contrast, a short sale occurs when the lender allows the homeowner to sell their home for less than the outstanding mortgage balance but does not hold the homeowner liable for the difference.

Foreclosures are readily identified from administrative documents that are captured by the property data. Legal regulations require documentation of foreclosures, such as notices of default and records from foreclosure auctions. We identify short sales using a flag in the property data derived from a proprietary algorithm constructed by the data provider. In Appendix C, we show that this algorithm classifies short sales as those that are likely to have yielded proceeds below the outstanding balance of the mortgage, and that this classification closely tracks aggregate rates of short sales over time measured using external survey data. Short sales by definition take place at prices below the outstanding principal balance, meaning that sales not classified by the algorithm

for these groups in the appendix.

[11] Sourcing data on transactions from local recorder offices generates imperfect geographic coverage in the baseline sample of ownership spells because non-disclosure states do not require that property sale prices be recorded publicly. These states are Alaska, Idaho, Kansas, Louisiana, Mississippi, Missouri (some counties), Montana, New Mexico, North Dakota, Texas, Utah, and Wyoming.

as short sales are unlikely to be false negatives. Therefore, the fact that the algorithm matches aggregate rates suggests that it accurately identifies short sales. In our baseline sample, 36% of distressed sales are classified as short sales, essentially identical to the 36% classified as short sales by Zhang (2019) using similar data on property sales. Since our baseline sample restricts to arm's length transactions, it excludes deeds in lieu of foreclosure, which represent only about 0.09% of transfers in the property data.

The rates of distressed sales in our sample are similar to external estimates. Appendix Figure A3 depicts the aggregate foreclosure rate over time in our data, which is similar to the trends reported in Corbae and Quintin (2015) using data from the Mortgage Bankers Association National Delinquency Survey.[12]

2.3 Comparison to American Housing Survey

Our primary results rely on a merge between the HMDA records and property transaction records. In light of the fact that this sample of 13.6 million ownership spells represents a relatively small share of the total number of purchases that occurred during this time period, we assess the representativeness of our sample by comparing it to an external representative sample from the 2015 American Housing Survey (AHS).

Reassuringly, our baseline sample is similar to the AHS sample along many dimensions. To ensure comparability of our spell-level sample to the AHS cross-section, we compare the subset of properties in our sample that were purchased before 2015 and remained unsold as of 2015, to homeowners in the AHS who were surveyed in 2015 and who purchased their homes with a mortgage. Section C.5 provides more details of the AHS sample. Appendix Table A1 shows that share of Black and Hispanic homeowners are similar in both samples, as is the share of properties that are single-family homes. The distribution of down payments also appears similar to the sample in the AHS, particularly after restricting to homeowners in the AHS who have moved since 2000, which more closely reflects the temporal coverage of our sample.

Given the way our sample was constructed, we expect certain modest differences in geographic coverage and homeowner characteristics. In terms of geographic coverage, the property data have very little coverage in states that do not publicly disclose real estate purchase prices (e.g., Texas). This is most clearly illustrated in a comparison of sample coverage across CBSAs (Appendix Table A2). Nonetheless, the distribution of our sample across large geographic areas (i.e., Census Division) is similar to the AHS, with modest differences largely due to limited coverage of non-disclosure states. However, the property data, even without the merge with HMDA, have less coverage in rural areas: the non-metropolitan share of the sample is 5% in the property data (3% after the merge to HMDA), compared to 13% in the AHS. Likely as a result, the average property in our baseline sample is valued at about 10% more than the national average in the AHS.

[12] Our quarterly foreclosure rates are lower than those in Corbae and Quintin (2015) due to differences in the definition of foreclosure. The National Delinquency Survey measures foreclosure starts, whereas our ownership spell data measures foreclosure sales. Since not all foreclosure starts end in a foreclosure sale, aggregate counts of foreclosure starts are naturally higher (ATTOM, 2023).

Another expected difference relative to the external sample is the tenure length of homeowners. Since our sample begins in 1990, and the property data have limited coverage before 2000, the distribution of tenure length in our sample is lower than the national average. In line with this difference in coverage, our sample appears very similar to the AHS after restricting to homeowners in the AHS that moved into their current residence in 2000 or later (Appendix Table A1, Column 4).

Overall, comparing our sample to the AHS confirms the dimensions on which we expect our sample to differ from a representative cross-section. We therefore view our results as being externally valid for homeowners who purchased their homes within the temporal window which is covered by our sample.

2.4 Measuring Housing Returns

In order to estimate racial disparities in housing returns, we construct measures of housing returns at the household level. This analysis represents an advancement over alternative approaches, which typically rely on local price indices (e.g., Anacker 2010; Kahn 2024) or on homeowner self-reports of home value (e.g., Flippen 2004; Faber and Ellen 2016) to study racial differences in housing returns.

We analyze two measures of housing returns: unlevered and levered returns. Analyzing each measure individually carries a trade-off: unlevered returns are more accurately measured in the property data but do not incorporate borrowing and limited homeowner liability in foreclosures, both of which are captured by levered returns. However, computing levered returns requires a number of additional assumptions. Therefore, we view unlevered and levered returns as complementary. In the results presented in the following section, both measures reveal qualitatively similar patterns.

We compute the annual unlevered rate of return for owner i, r_i^u using the following formula:

$$1 + r_i^u = \left(\frac{P_{i1}}{P_{i0}}\right)^{\frac{1}{T_{i1}-T_{i0}}} \tag{1}$$

In Equation 1, P_{i0} and P_{i1} are the property purchase and sale prices, respectively. $T_{i1} - T_{i0}$ denotes the length of the ownership spell in years. Given that transaction prices and dates are recorded in the property data, unlevered returns are both easy to compute and accurately measured.

The primary measurement limitations of unlevered returns are that unlevered returns do not capture homeowner leverage or the limited liability of homeowners in the event of a foreclosure. Minorities tend to purchase their homes with more leverage (i.e., with higher loan-to-value ratios), at least in part due to more binding financial constraints (Gupta et al., 2021). Ceteris paribus, more leverage creates higher returns, meaning that Equation 1 may understate the true rate of return for minority homeowners. Relatedly, lenders often have a limited ability to recoup losses associated with underwater home sales, meaning that Equation 1 may overestimate the magnitude of losses from distressed home sales.[13]

[13]In certain "no-recourse" states, lenders are legally prohibited from holding homeowners responsible for any

In order to capture both of these factors, we compute the levered rate of return for owner i as the interest rate that sets the net present value of cash flows equal to zero. Specifically, the monthly levered return is the value of r_i^l that solves the following equation:

$$0 = -DownPay_{i0} + \sum_{t=1}^{T_i-1} \frac{rent_{it} - pymt_{it}}{(1+r_i^l)^t} + \frac{\max\{0.01, rent_{iT} - pymt_{iT} + 0.95P_{iT} - UPB_{iT}\}}{(1+r_i^l)^{T_i}} \quad (2)$$

This formulation is also referred to as the internal rate of return. Intuitively, the levered return sets the homeowner's discounted monthly cash flows to zero. In the above, $DownPay_{i0}$ denotes owner i's down payment, $rent_{it}$ denotes the implicit rent received in month t, $pymt_{it}$ denotes the actual housing payment, P_{iT} denotes the property sale price, and UPB_{iT} denotes the outstanding principal balance at the time of sale. We assume that homeowners pay transaction costs of 5% of the sale price when selling their homes. The max operator captures the assumption that the homeowner is not liable for the difference between the sale price and the outstanding balance, and setting a floor of \$0.01 ensures that r_i^l is well-defined.[14]

We rely on a number of imputation strategies to compute the components of Equation 2 that are not observed in the property data. To measure the down payment, we calculate the difference between the sale price and the original loan amount in the recorder data, and add closing costs that are imputed using the 2018 and 2019 HMDA data. The down payment is therefore computed as the sum of equity at origination and closing costs.

The monthly payment is the sum of three components: the principal and interest payment, the tax and insurance payment; and maintenance costs. Principal and interest payments are computed assuming a 30-year fixed interest fully-amortizing mortgage, with interest rates imputed using mortgages originated in the same county and quarter in the McDash mortgage servicing data, distinguishing between first and second liens. Unpaid principal balance at sale is calculated using the same method. We impute tax and insurance payments using observed escrow payments in McDash and maintenance costs using the 2001 to 2013 Consumer Expenditure Surveys (CEX). We impute rents using the ratio of county-level median rents to median house prices in Census data, and use data from the American Housing Survey to adjust for composition bias using the methods developed in Demers and Eisfeldt (2022) and Gilbukh et al. (2017). Lastly, we also assume foreclosed homeowners do not make payments in the final months of their tenure, and impute the number of payment-free months using observed differences between last payments and REO in each state-year in the McDash data. See Appendix D for more details.

Our baseline sample restrictions (i.e., dropping purchases with combined loan-to-value ratios of more than 102.5% and ownership spells of less than 12 months) are designed to minimize the

difference between the outstanding principal balance and the proceeds from a home sale. Even in states that allow recourse, pursuing such a judgment is costly and lenders may have a limited incentive to pursue unpaid amounts using a legal judgment.

[14] We also calculate the net present value (NPV) of the cash flows defined in Equation 2, which allows us to relax these assumptions because the NPV does not require a positive final value in order to be well-defined. When calculating the NPV, we take into account limited liability for distressed home sales but not for non-distressed sales.

potential for measurement error in both of our measures of housing returns. We also winsorize annual returns at the 1% level. For 1.5% of the sample, levered returns cannot be computed because of missing imputed components. The levered rate of return is undefined for less than 0.01% of the remaining sample.

3 Unconditional Racial Gaps in Housing Returns

We uncover large racial differences in both levered and unlevered housing returns. Table 2, Column 1 presents means and standard deviations of housing returns for homeowners in our baseline sample of ownership spells, along with rates of distressed sales. Panel A reports that the mean annual unlevered returns for Black, Hispanic, and White homeowners are 0.5%, 0.6%, and 2.8%, respectively. Since these means are estimated on a sample of 13.6 million ownership spells, differences in mean returns are highly statistically significant. Differences in annual levered returns are also large, with mean levered returns of 1.6%, -3.0%, and 6.6% for Black, Hispanic, and White homeowners, respectively. There is large variation in realized returns: the standard deviations of unlevered and levered returns are 8.2% and 43.4%, respectively (consistent with the findings in Giacoletti 2021). Black and Hispanic returns exhibit markedly higher variance than White returns, with standard deviations of 9.3%, 11.9%, and 7.1%, respectively.

Distressed sales (i.e., foreclosures and short sales) play an important role in generating these differences. Among the sample of regular sales (Column 2), unlevered returns for Black homeowners are only 0.4 percentage points lower than returns for White homeowners, and returns for Hispanic homeowners are 1.5 percentage points higher. For levered returns, Black and Hispanic homeowners realize higher returns than White homeowners. Relative to unlevered returns, higher levered returns for non-distressed sales among minorities are driven by higher amounts of leverage among minorities (Appendix Figure A4). Distressed sales mediate racial gaps in housing returns in two ways. First, a much higher share of Black and Hispanic homeowners experience a distressed home sale (Table 2, Column 1, Panel C); and second, minorities also realize lower returns within the subsample of distressed sales (Column 3, Panel D). These patterns imply that distressed sales can almost entirely explain the racial gaps in housing returns in a statistical sense, which represents a key finding of this study.

While distressed sales can statistically account for the gap in returns, distressed sales themselves are an endogenous result of both borrower and lender behaviors and therefore influenced by underlying factors like market conditions and homeowner financial stability. We revisit the economic mechanisms underlying racial differences in distressed sales in Section 4, and devote the remainder of this section to evaluating various factors that may bias our estimates of these returns and to benchmarking the magnitudes of these overall differences.

3.1 Robustness to Potential Sources of Bias

In this section, we analyze five factors that could potentially bias our estimates of racial differences in housing returns.

Sample Selection One potential threat to the external validity of our results is if ownership spells that can be linked to the HMDA data systematically differ from those that cannot be linked. In support of the external validity of our results, we confirmed in Section 2.3 that our sample is similar across observable characteristics to a representative sample from the American Housing Survey. In addition to this exercise, we also relax the selectivity of our sample by estimating racial gaps in housing returns by imputing race from owner names and Census block using the approach in Imai and Khanna (2016) (see Appendix C for additional details). Because owner names and property addresses are measured in the property data, this approach does not require the merge with the HMDA data and therefore avoids dropping about half of our sample (see Appendix Figure A1 for a visual illustration).

Although inferring race from names potentially reduces the selectivity of our sample, it carries the cost of more measurement error relative to self-reported race. This is evident in Appendix Table A3, which compares self-reported and inferred race for homeowners where both are observed. The classification based on names is fairly accurate for White and Hispanic homeowners, but markedly less accurate for Black homeowners. Although this measurement error can be expected to attenuate estimated gaps, this exercise is nonetheless helpful for probing the external validity of our analysis.

Racial gaps in housing returns that are estimated by inferring race yields results that are similar to those we estimate using the smaller sample where self-reported race is observed, supporting the external validity of our baseline results. We compare estimated gaps in our baseline sample to those in the larger sample of ownership spells, which is not restricted to properties merged with HMDA. To ensure comparability of the two samples, we compare homes purchased between 2000 and 2014 with a mortgage, and where the owner is classified as Black, White, or Hispanic based on name and Census block, yielding a sample that is nearly twice as large than the sample merged with the HMDA data. Average realized returns are very similar between the two samples, at 2.2% with baseline restrictions and 2.5% in the larger sample (Appendix Table A4, Columns 1 and 2). Rates of distressed sales are also similar, at 14.8% and 15.1%, respectively. As expected, the estimated differences are slightly smaller when using imputed race, which entails more measurement error. For example, the Hispanic-White gap is 2.3 percentage points in our baseline sample, and 2.1 percentage points when imputing race in the larger sample (Columns 2 and 4). Moreover, the distribution of returns in the baseline sample is very similar to the sample where self-reported race is missing (Column 3), suggesting that missing race information is unlikely to be a source of bias. Together, these findings offer confidence that the analysis conducted with our baseline sample using self-reported race is externally valid.

Cash Purchases Our baseline estimates rely on self-reported race using mortgage records and therefore exclude homes purchased in cash, which comprise about one-fourth of home purchases.

While this exclusion does not affect the validity of our estimates for homeowners who purchase their homes with a mortgage, it is likely that racial differences in returns among cash purchases differ from those purchased with mortgages, particularly since homeowners without a mortgage cannot experience a foreclosure or short sale.

We are able to measure the returns of cash purchases in our larger sample of home purchases that do not make use of the HMDA merge, and we can compare returns by race using homeowner race inferred from homeowner names and Census block. We find no meaningful racial differences in returns among cash purchases, and that the distribution of returns associated with cash purchases is very similar to that of mortgaged purchases that do not end in a distressed home sale. This comparison is presented in Appendix Figure E3.

Informed by these patterns, we use the race-specific returns of mortgaged purchases not sold in a distressed sale as a proxy for the returns of cash purchases. To estimate race-specific rates of cash purchases, we use the 2013-2017 waves of the American Community Survey (ACS) to compute the share of households who have been living in their current residences for less than two years and who have unpaid mortgages. According to this measure, 76.5%, 78.6%, and 76.7% of White, Black, and Hispanic homeowners purchased their homes with a mortgage, respectively. To estimate average unlevered returns adjusted for cash purchases for each racial group, we take the weighted average of returns for our baseline sample and returns for the subsample of properties not sold in a distressed sale, weighting by the ACS cash purchase shares. Both samples include properties that were not sold by the end of our sample period. We make an analogous calculation for levered returns by computing a measure of the internal rate of return without leverage.[15] Similarly, we calculate standard deviations by computing the standard deviation of returns when combining the baseline sample and sample of non-distressed properties and weighting by cash purchase shares. To estimate rates of distressed sales adjusted for cash purchases, we assume that homes bought without leverage do not result in distressed home sales, and therefore proportionally reduce estimated rates of distressed sales by the race-specific cash purchase share.

We present the estimates of returns that adjust for cash purchases in Column 4 of Table 2. Adjusting for cash purchases yields slightly smaller estimates of racial gaps in housing returns: the Black-White (Hispanic-White) gap is 1.9 (1.4) percentage points. These gaps represent our preferred estimates for the racial gap in housing returns that includes cash purchases.

An alternative approach to factoring in cash purchases is to estimate returns in the sample that does not use the HMDA merge. The key disadvantage of this approach is that it requires inferring race from homeowner name and Census block, which attenuates gaps. In line with expectations, these gaps are similar to our preferred adjustment (Appendix Table A4, Column 5). The gaps are slightly smaller, which is an expected result of the relatively higher measurement error in

[15] Specifically, we compute the levered rate of return defined according to Equation 2 but exclude cash inflows and outflows from mortgage loans. We include implicit rents, tax and insurance payments, maintenance costs, a 5% transaction cost, and assume closing costs that are half of the imputed value for a loan with an LTV of 80% (given that a large share of closing costs are not associated with borrowing). We then use these modified levered returns among the subset of properties that were not sold in a distressed sale as a race-specific proxy of levered returns associated with cash purchases.

identifying Black homeowners by name, as is evident when comparing classifications of self-reported and imputed race (Appendix Table A3).

Imputation of Returns for Unsold Properties Our finite sample window naturally produces many ownership spells that are censored—that is, the properties were unsold as of March 2020. Our baseline approach to addressing this standard censoring problem is to use house price indices. Specifically, we impute the property value as of April 2020 by inflating the purchase price by the amount of house price growth from the FHFA county-level house price index (Bogin et al., 2019). Appendix Figure A5 compares the annual house price growth to the actual returns among properties where we observe a sale, indicating that this approach accurately approximates average returns among non-distressed sales. However, because some of the unsold properties will eventually be sold in a distressed sale (for a price substantially below its estimated market value), this approach overestimates housing returns for these properties; and because rates of distressed sales are higher for minorities, it underestimates racial gaps in returns.

We conduct two exercises to assess the magnitude of this bias. The first exercise modifies our baseline approach by discounting the imputed property value for unsold properties. To do so, we estimate the share of sales at each tenure length that end in a distressed home sale within our sample of properties where we observe a sale. Using these estimates, we compute the probability that an ownership spell owned by a homeowner of race r which remains unsold after t years eventually results in a distressed sale.[16] We assume that distressed properties are sold at 70% of the market value imputed by the house price index, which is informed both by the observed sale prices of distressed sales in our data (Appendix Figure A6), and by previous estimates of distressed sale discounts (Campbell et al., 2011). We then discount the imputed value of each unsold property by 30% multiplied by its estimated probability of being sold in a distressed sale.

We present estimates of returns that include this adjustment in Table 2, Column 5. In line with expectations, this adjustment leads to a modest reduction in returns, which is larger for minority homeowners. Thus, accounting for future distressed sales leads to slight increases in the estimated gaps. For example, the Black-White gap in unlevered returns increases by about 6%. This effect is slightly larger for gaps in levered returns due to the inherent non-linearities involved in their calculation: the Black-White gap in levered returns is about 20% larger when making this adjustment.

We conduct a second complementary exercise that addresses censoring and does not rely on house price indices (and therefore avoids the aforementioned bias). Consider a decomposition of the average returns for homeowner i:

[16] Specifically, we compute this probability as $p_{rt} = \sum_{i=t}^{30} Pr[\text{distressed sale}|\text{realized tenure length } i]_r \times Pr[\text{sale in year } i|\text{tenure length } \geq t]_r$. We assume that ownership spells lasting 30 years or more do not end in distress. We estimate these these components using the observed survival curve of distressed sales and rates of distressed sales by race and tenure length. The estimated probabilities are presented in Appendix Figure E2. See Appendix C for more details.

$$E[R_i|\text{race}_i = r] = \sum_{t=1}^{T} \left(E[R_i|\text{tenure}_i = t, \text{race}_i = r] \times Pr[\text{tenure}_i = t, \text{race}_i = r]\right) \quad (3)$$

In the above, average returns for race r are the sum of tenure-specific returns. To use this formula to compute average returns, we first produce Kaplan-Meier estimates of $Pr[\text{tenure}_i = t, \text{race}_i = r]$. We then compute $E[R_i|\text{tenure}_i = t, \text{race}_i = r]$ using the returns among properties where we observe a sale. Since we observe that annual returns stabilize for ownership spells lasting 15 years and longer, we use the realized returns of ownership spells with long tenure lengths to extrapolate annual returns outside of our sample window. That is, we assume that $E[R_i|\text{race}_i = r] = \bar{R}_r$ for tenure lengths lasting 20 or more years. This enables us to compute Equation 3 using only ownership spells where we observe a sale price. See Appendix E for plots of Kaplan-Meier estimates and tenure-specific returns, along with additional details on this approach.

This alternative approach to adjusting for censoring bias yields gaps that are very close to our baseline estimates. Appendix Table A5 shows that the estimated Black-White (Hispanic-White) gaps in mortgaged housing returns are 2.4 (2.1) percentage points for unlevered returns and 7.2 (8.9) percentage points for levered returns, which are similar to those estimated using our main approach in Table 2. Estimated gaps adjusting for cash purchases are also similar across the two approaches.

Although our primary approach to addressing censoring (i.e., imputing sale prices using house price indices) yields downward-biased estimates of racial gaps in housing returns, the two exercises above indicate that this bias is relatively modest. Moreover, a key advantage of our baseline approach is its simplicity and transparency. Accordingly, we take this approach throughout the remainder of the paper. In light of the downward bias, this approach should be viewed as being relatively conservative.

Racial Differences in Housing Costs Our baseline calculation of monthly costs in levered returns does not take into account racial differences in homeownership costs. Because we are unable to systematically measure the relationship between racial differences in housing costs and the other components of Equation 5, we do not incorporate such differences into our baseline measure of internal returns. However, it is valuable to assess the sensitivity of our estimates to incorporating these racial differences.

To probe the sensitivity of our estimates to incorporating additional racial differences in housing costs, we rely on the estimates of differences in interest rates and taxes in Gerardi et al. (2020) and Avenancio-León and Howard (2019). Both of these adjustments mechanically increase the racial gap in levered returns. On the other hand, prior work indicates that minorities live in neighborhoods with higher rent-to-price ratios (e.g., Desmond and Wilmers 2019). In order to incorporate such differences, we rely on the estimates in Demers and Eisfeldt (2022) of differences in rent-to-price across ZIP codes within MSAs. In addition, to measure differences in home maintenance expenditures, we analyze CEX data and estimate maintenance expenditures as a percentage of home value that are moderately lower for Black and Hispanic homeowners.[17] See Appendix D for more details on the adjustments.

Incorporating racial differences in housing costs leads to a modest reduction in the racial gap in levered returns. Compared to our baseline estimates, this additional adjustment for racial differences in housing costs causes the Black-White gap in levered returns to fall from 5.0 to 4.5 percentage points, while the Hispanic-White falls from 9.6 to 9.0 percentage points (Appendix Table A6, Panel A). These findings indicate that higher rents and lower maintenance expenditures counterbalance higher interest and taxes paid by minorities, and that racial differences in implicit rents appear to partially compensate minorities for the overall gap in housing returns. However, the exclusion of differences in housing costs from our baseline measure of internal returns appears to have a modest impact on the estimated disparities.

Home Improvements Many homeowners improve their homes during their tenure, and racial differences in home improvement expenditures are a non-trivial consideration when interpreting observed racial differences in housing returns. In our baseline sample, we do not directly observe such expenditures. When we incorporate racial differences in home expenditures in our returns calculation (see above), we find that differences in home expenditures and rents counteract racial differences in interest rates and property taxes, and lead to slight reductions in the racial gap in housing returns.

To further evaluate the quantitative importance of home improvement expenditures, we draw on two additional data sources. First, we analyze the sample of homeowners in the CEX from 2001 to 2013, previously used by Benmelech et al. (2017) to directly measure home improvement expenditures. Second, we use data from BuildFax covering 2000-2017 that contain information on building permits filed for the purpose of home improvement.[18] While the BuildFax data carry the advantage of being merged to the property data for ownership spells within that sample window, these data only capture larger improvements that entail a building permit. In contrast, the CEX is designed to capture all types of home improvement expenditures.

Minority homeowners appear to spend less on home improvement relative to White homeowners. In Appendix Table A7, we report results of regressing measures of home improvements on Black and Hispanic indicators. Annual home expenditures as a percentage of home value are about 21 basis points lower for Black homeowners and 6 basis points lower for Hispanic homeowners, relative to White homeowners. These differences become 20 basis points and 13 basis points, respectively, when including fixed effects interacting purchase year, state, and survey year. Minority homeowners are also less likely to file permits for home improvement, as measured by the annual number of permits filed and by the reported job costs of building permits as a percentage of home value. When

[17] Specifically, these adjustments assume maintenance expenditures that are 22 (7) percentage points lower for Black (Hispanic) homeowners after controlling for state and home value, interest rates that are 46 (27) basis points higher for Black (Hispanic) homeowners, property taxes that are 12% higher for both groups, and rent-to-price ratios that are 2.5 percentage points lower in the highest quintile of ZIP codes by home price relative to the lowest quintile.

[18] For the analysis of permit outcomes, we restrict to ownership spells with full coverage of the permit data between purchase and sale. Data provided by BuildFax. More information on the permit data can be found at http://www.buildfax.com.

scaled by the outcome means, the magnitude of the Black-White difference in permit filing is similar to the Black-White difference in expenditures measured in the CEX data. Hispanic homeowners are also less likely to file permits than White homeowners, with the Hispanic-White gap being slightly smaller than the Black-White gap.

While it is possible Black and Hispanic homeowners spend less on maintenance in anticipation of realizing a distressed sale, expenditure patterns on non-home durables suggest this is not likely to be a quantitatively important driver of differences in home expenditures. Appendix Table A7, Columns 7 and 8 present differences in monthly non-home durable expenditures. Controlling for home value, racial differences in non-home durable expenditures are about as large as differences in home expenditures, scaled by the outcome mean. This similarity suggests that differences in expenditure are driven by non-strategic factors.

These lower maintenance expenditures do not appear to depress housing returns for minority homeowners. There is essentially no average difference between actual sale prices and predicted home values for properties sold in regular sale. Appendix Figure A5 plots average realized returns for regular sales against county-level house price growth measured using the FHFA house price index, and shows that the realized returns and county-level growth track each other essentially one-for-one for Black, Hispanic, and White homeowners alike. If minorities' lower maintenance expenditures were depressing home values, one would expect minority returns to be lower than local house price growth. Consequently, the finding that minorities spend less improving their homes does not change the overall interpretation of our results. It does suggest that minority homeowners with non-distressed sales realize even higher returns relative to non-minorities, if one were to subtract out differences in home improvements.

3.2 Alternative Measures of Housing Returns

Racial gaps in housing returns appear across a range of alternative measures of housing returns. Another commonly analyzed measure of housing returns is the total return to housing, defined as the sum of capital gains and yields (i.e., rents). Appendix Table A6, Panel B presents estimates of total returns by race. Qualitatively similar patterns emerge: total returns for Black (Hispanic) homeowners are 2.0 (3.0) percentage points lower than for White homeowners. As with our baseline measures of returns, the gaps are slightly larger when accounting for distressed sales that will eventually occur outside of our sample window, and slightly smaller when accounting for cash purchases.

Two additional measures of housing returns are the net present value (NPV) of the home purchase, and the Sharpe Ratio of returns. The NPV is calculated using the same cash flows in Equation 2, but relaxing the floor on cash flows in the final period for non-distressed sales. Results presented in Appendix Table A8 reveal large racial differences in NPV and Sharpe Ratios.[19]

[19] The analysis of Sharpe ratios also confirms the previous findings in Giacoletti (2021) that investment in individual houses suffers from relatively large idiosyncratic shocks, and on a risk-adjusted basis, its financial performance in recent years has been inferior to investments in the stock market. Of course, households may derive other non-financial benefits from homeownership that compensate them for the excess volatility of their housing investment returns.

Comparisons in NPV that weight by the NPV of costs (i.e., upfront costs, mortgage payment, taxes and insurance, and maintenance), and comparisons that include additional foreclosure costs yield similar conclusions.

Lastly, we compute measures of returns that only include some of the components of Equation 2. Appendix Table A9 provides estimates of differences in housing returns, sequentially incorporating components of levered returns. This exercise offers a quantitative comparison of how differences in unlevered returns are affected by incorporating one-time transaction costs, monthly rent and upkeep costs, and leverage. These comparisons illustrate that the most important difference between our measures of levered and unlevered returns is the calculation of leverage itself, rather than monthly costs or one-time transaction costs.

All of the measures of housing returns that we have examined are financial, and therefore do not capture any non-financial benefits that, in theory, could compensate minorities for lower financial returns. Homeownership offers many benefits that are not captured by financial returns, such as the opportunity to locate in neighborhoods with desirable amenities, such as high-quality schools. If buying a home helps households locate in desirable neighborhoods (Bergman et al., 2019), such non-financial benefits have the potential to contribute to household wealth accumulation.

To assess the scope for such non-financial benefits to compensate for racial gaps in financial returns, we analyze racial differences in neighborhood upgrades realized upon home purchase. We combine address histories for our analysis sample with data from Chetty et al. (2018) that measures neighborhood-level characteristics, including measures of intergenerational mobility. We are able to identify the previous address of 3.3 million homeowners in the property data. See Appendix F for more details on these data, as well as a detailed description of our results.

We find that on average, homeowners of all racial groups move to higher-quality neighborhoods, as measured by poverty rates, school quality, intergenerational mobility, and incarceration rates. However, the upgrades realized by minority homeowners are limited relative to their White counterparts. While minority homebuyers do appear to make meaningful gains in terms of neighborhood poverty rates and school quality, minority homebuyers move to higher-poverty neighborhoods than White homebuyers of similar incomes. Moreover, we find no evidence of average upgrades in race- and income-specific measures of intergenerational mobility and incarceration rates. Taken together, these findings suggest limited scope for improvements in neighborhoods to increase wealth accumulation for minority homeowners beyond those realized by White homeowners.

3.3 Benchmarking Magnitude of Gaps in Returns

Our estimates of racial gaps in housing returns are substantially larger than previous estimates that rely on house price indices or self-reported home values. The key reason for this difference is that the racial gap in housing returns can be statistically accounted for by distressed sales, which are not well-reflected in house price indices and self-reported home values. For example, Kahn (2024) analyzes ZIP code house price indices and finds that over 5 year holding periods, Black homeowners earn returns that are 0.7 percentage points lower than the average homeowner, while

Hispanic homeowners earn returns that are 1 percentage point higher, a finding that resembles our estimates of Equation 5 among only regular sales (Table 2, Column 2).[20] In Appendix Figure A6, we plot the distribution of sale price as a function of home value implied by local price indices and find that measuring returns using these indices slightly underestimates the returns to regular sales and greatly overestimates the returns to distressed sales. Thus, using local indices to compare differences in housing returns across racial groups can overlook large differences in returns that are due to differences in distressed sales. Similar reasoning applies to the use of home values reported by homeowners (e.g., Flippen 2004), since homeowners presumably report the value of their homes if sold in a regular sale.

The gaps in housing returns are also large relative to previously-documented racial differences in housing costs. For instance, Gerardi et al. (2020) document post-origination interest rate disparities due to differences in refinancing behavior of over 40 basis points (a difference on the order of $500 annually for a $200,000 home). Avenancio-León and Howard (2019) find that inflated property assessments result in annual property tax costs that are 10-13% higher for minorities, amounting to a difference of $300 to $390 per year. In our sample, about half of properties are held for at least 10 years. The average mortgaged Black homeowner buys a home for $219,303 by making an initial payment of $21,675 (closing costs and down payment), and the average Hispanic homeowner buys a home for $256,474 by paying $30,172. Over a ten-year period, the racial gap in unlevered returns (Table 2, Column 1) applied to initial home values amounts to a difference of $5,920 per year for Black homeowners and $6,762 per year for Hispanic homeowners. A similar calculation for levered returns based on average housing costs amounts to $3,032 per year for Black homeowners and $7,500 per year for Hispanic homeowners.[21] In dollar terms, gaps in housing returns are an order of magnitude larger than any one of the previously-documented gaps in housing costs.

3.4 Implications for Racial Wealth Gap

Given the importance of housing for wealth accumulation in the US, our findings of large racial gaps in distressed sales and housing returns are likely to have direct implications for the racial wealth gap. However, there are two factors that could, in theory, limit the impact of the returns gap on the racial wealth gap. First, it is possible that distressed sale discounts have a limited impact on minority wealth—if distressed sales only occur when a property is underwater (i.e., the home is worth less than the outstanding mortgage balance), then the impact of higher rates of distressed sales on minority wealth could be limited. However, recent work indicates that this is not the case. Low (2021) and Ganong and Noel (2020b) show that many homeowners who default on their payments have positive equity. Similarly, we find that many foreclosures in our baseline sample involve borrowers with positive equity, but that much of that equity is destroyed after a distressed sale. Estimating home equity for foreclosed properties in our baseline sample, we find that over half

[20] In subsequent work, Wolff (2022) analyzes self-reported values in the SCF (which precludes the analysis of distressed sales) and does not find meaningful racial gaps in housing returns after controlling for observable characteristics.

[21] See Appendix D for figures used in this calculation.

of foreclosures for Black and White households, and more than one-third of Hispanic foreclosures, are above water. Moreover, comparing the property's sale price with its imputed value implies a distressed sale discount of 28% for White homeowners, 39% for Black homeowners, and 40% for Hispanic homeowners (Appendix Figure A6). These discounts are roughly in line with estimates of foreclosure discounts from prior work (e.g., Campbell et al. 2011). Therefore, the distressed sales that drive the returns gap directly erode Black and Hispanic wealth, meaning that the racial gap in housing returns translates into real differences in wealth accumulation.[22]

A second possibility in which the gap in returns would have limited effects on minority wealth accumulation is if the vast majority of real estate transactions were to occur within race (e.g., Hispanic sellers only selling to Hispanic buyers) and if foreclosures do not entail substantial property depreciation. If this were the case, then minority homeowners living in distressed neighborhoods would be able to take advantage of the availability of nearby discounted homes sold at foreclosure, potentially realizing high returns. We analyze this possibility in Appendix G, and find that this does not appear to be the case—the majority of distressed minority home sales involve buyers of a different race. Moreover, foreclosed properties may be subject to substantial depreciation, which further limits the returns net of renovation costs associated with buying discounted properties.

We estimate the contribution of racial gaps in housing returns to wealth disparities using a simple wealth accumulation equation that allows us to estimate a variety of counterfactual wealth gaps. For these calculations, we use the Panel Study of Income Dynamics (PSID) 2001-2017, which contains a uniquely long panel of outcomes for surveyed households. However, due to limited coverage of Hispanic households, we restrict our calculations to Black and White households. See Appendix C for more details on the PSID sample.

We compute average wealth held in the primary home at retirement age by a household of race $r \in \{Black, white\}$ using the following equation:

$$\hat{H}_{r,65} = \sum_{s \in \{cash, mort\}} \sum_{t=25}^{65} \left(p_{r,s,t} \times H_{r,s,t} \times R_{r,s}^{(65-t)} \right) \tag{4}$$

In Equation 4, $p_{r,s,t}$ denotes the unconditional probability of becoming a first-time home buyer at age t for race r, in a purchase of type s (mortgaged or cash). $H_{r,s,t}$ denotes the average house value at first-time home purchase, and $R_{r,s}$ denotes the average annual return on a home purchased by a homeowner of race r of purchase type s.

This formulation computes average primary housing wealth at retirement by inflating the value of households' first home at purchase at each age using mean housing returns for each race and purchase type. We do not explicitly model transitions out of homeownership through distressed sales, which are captured in the race-specific returns $R_{r,mort}$. We compute mean housing returns

[22] Note that our conclusions about the role of distressed sales do not depend on whether the discount is a "true" discount, or whether the lower price reflects depreciation of the property. Regardless of whether or not they involve substantial property depreciation, avoiding distressed sales saves housing wealth. In support of this interpretation, the quasi-experimental estimates of the impacts of modifications in Section 6 show that avoiding distressed sales increases housing returns.

adjusted for inflation and censoring from our baseline sample of ownership spells.[23] The panel structure of the PSID allows us to estimate probabilities of becoming a first-time home buyer $p_{r,s,t}$ and home values $H_{r,s,t}$, normalized to 2016 dollars. Average home value at first home purchase is \$208,621 for White homeowners and \$142,587 for Black homeowners.[24]

Despite its simplicity, Equation 4 yields estimates of primary housing wealth at retirement that are similar to those in the external data. As reported in Table 3, this framework yields average housing wealth at retirement for Black households of \$81,713, which is close to the average of \$89,872 for retirement-age households in the PSID. The Black-White wealth gap is \$169,389, which is only slightly smaller than the actual gap of \$182,771.

Equation 4 allows us to calculate racial gaps in housing wealth under counterfactuals in which we eliminate gaps in housing returns, rates of first-time home purchases over the life cycle, and home values at purchase. These counterfactuals are calculated by setting R_{Black}, p_{Black}, and H_{Black} to the corresponding values for White homeowners. Table 3 reports that equalizing housing returns reduces the gap by 37%. In contrast, equalizing first-time purchase rates reduces the gap by about 1%, and equalizing both purchase rates and initial home values reduces the gap by only 26%. Equalizing both returns and purchase rates reduces the gap by 49%.

The results of this exercise indicate that the gap in housing returns can explain a quantitatively large share of observed differences in housing wealth and that equalizing housing returns can substantially reduce racial wealth disparities.[25] Housing wealth held in the primary home comprises 43% of total net wealth for the average retirement-age Black household in the PSID sample, implying that the gap in housing returns can explain a large share of the overall racial wealth gap. While the homeownership rate among White households is substantially higher than that of Black households, our results indicate that the potential wealth-building benefits of increasing Black home purchase rates would be almost entirely negated by their lower returns. This finding illustrates the limitations of policies that focus solely on promoting homeownership, as well as the potential value of policies that help minorities avoid realizing low returns.

4 Gaps in Returns Conditional on Homeowner Characteristics

Although differences in distressed sales statistically explain the racial gap in housing returns, distressed sales are an endogenous outcome of both borrower and lender actions. Therefore, differences in distressed sales should be interpreted as a mediating factor between racial differences in economic

[23] Annual real returns adjusted for both inflation and censoring are 0.53% for mortgaged White homebuyers, -1.83% for mortgaged Black homebuyers, 2.00% for White cash homebuyers, and 2.15% for Black cash homebuyers.

[24] Using the sample of first-time home buyers, we regress house value on age for each combination of race and purchase type combination, and use the predicted values of the linear fit to compute $H_{r,s,t}$. To compute $p_{r,s,t}$, we first estimate the transition probability of becoming a first-time home buyer at age t for race r, denoted by $q_{r,t}$. We regress an indicator for a mortgaged purchase on age for each race, and denote the predicted values of the linear fit by $S_{r,t}$. Let $N_{r,t}$ denote the share of households of race r and age t who have never been homeowners. Then we can compute the transition probabilities as $p_{r,mort,t} = N_{r,t}q_{r,t}S_{r,t}$ and $p_{r,cash,t} = N_{r,t}q_{r,t}(1 - S_{r,t})$. We compute $p_{r,s,25}$ as the share of households aged 25 who are homeowners times the predicted share of purchases of type s.

[25] These findings echo recent work studying heterogeneity in returns to wealth more generally (Fagereng et al. 2020; Bach et al. 2020; Sakong 2020; Campbell et al. 2019).

primitives and racial gaps in housing returns. This section sheds light on the nature of those primitives by examining gaps conditional on observable factors.

A priori, many underlying factors may generate racial gaps in returns and distressed sales. For example, it is possible that the gap in returns and distress occurred because minorities were more likely to purchase homes immediately prior to the Great Recession. Similarly, if racial gaps in housing returns and distress are absent when comparing homeowners with similar characteristics like income or family structure, then one could conclude that the gap in returns is economically explained by racial differences in these factors.

To analyze the economic importance of timing, location, and homeowner characteristics, we estimate the share of the gap in returns and distressed sales that can be explained by these factors. Specifically, we estimate regressions of the following form:

$$Y_{it} = \alpha_0 \mathbb{1}\{\text{Black}_i\} + \alpha_1 \mathbb{1}\{\text{Hispanic}_i\} + \mu_{c(i),t} + \varepsilon_{it} \tag{5}$$

This specification regresses an outcome Y (e.g., annualized housing returns) for homeowner i purchasing a home in year t on race indicators and a vector of fixed effects μ, which capture homeowner characteristics $c(i)$. We cluster standard errors within purchase year, sale year, and county cells. Varying the level of fixed effects allows us to evaluate the share of the racial gaps in returns and distress that can be explained by observable factors.

It is important to note that controlling for these factors can absorb both causal effects and systematic selection. When controlling for purchase year and county, it is plausible that the explanatory power of those factors largely reflects the causal impacts of buying a home in a given year or county, as opposed to homeowner sorting along those dimensions. In contrast, when controlling for neighborhood, the additional explanatory power likely reflects sorting of residents across neighborhoods to a much larger extent than when controlling for county or purchase year. That is, the concentration of distressed sales in certain neighborhoods reflects both the impacts of neighborhoods and the impacts of differences in the economic characteristics of residents.

We present the results of estimating Equation 5 in Figure 1. Panel A presents estimates for unlevered returns, and Panel B presents estimates for distressed sales. In Panel C, we interact race indicators with an indicator that an ownership spell ends in a distressed sale. The first columns in each panel present raw differences by race, the analogue of the results presented in Table 2. We present estimates for levered returns in Appendix Figure A7.

Timing and Geographical Sorting Timing and sorting across counties can explain about one-third of the racial gaps in housing returns. The second set of bars in Figure 1 present estimates of Equation 5, which include fixed effects that interact purchase year and county. Compared to the raw Black-White (Hispanic-White) gap of 2.3 (2.3) percentage points, controlling for county and purchase year reduces the Black-White (Hispanic-White) gap to 1.5 (1.6) percentage points. Controlling for county and purchase year also explains a meaningful share of the gap in distressed sales (Panel B). Furthermore, within county and purchase year, distressed sales fully mediate the racial gap in housing returns (Panel C): the Black-White gap in housing returns among non-distressed shrinks to a miniscule 0.1 percentage points. We interpret these findings as evidence that local economic conditions have a strong impact on racial gaps in realized returns, in part by influencing the frequency and severity of distressed sales.

Much of the explanatory power of controlling for purchase year and county can be attributed to the Great Recession. In particular, racial gaps in housing returns are largest for ownership spells beginning just prior to the recession and ending just after (see Appendix Figure A8 for a heat map by purchase year and sale year). The Great Recession featured expanded racial gaps in distressed sales, which underlie the larger gaps in returns. Appendix Figure A3 plots aggregate foreclosure rates over our sample period. Although the racial gap in distressed sales was greatly exacerbated during the Great Recession, Black and Hispanic homeowners experienced higher foreclosure rates before and for most of the period after the Great Recession, suggesting that minority homeowners would likely have experienced lower returns even in the absence of the recession and collapse in house prices. Black homeowners continued to exhibit higher foreclosure rates through the end of 2020, while rates of distressed sales between White and Hispanic homeowners have converged since around 2016. Consistent with these patterns, gaps in returns are substantial even for purchases made many years before the recession (Appendix Figure A8). Although estimated gaps in more recent years are relatively modest (and inverted for Hispanic homeowners and levered returns), these gaps should be interpreted with caution given our finite sample window.

Unfavorable local economic conditions exacerbate racial differences in foreclosures, suggesting that the relatively modest gaps observed in recent years characterized by favorable conditions are unlikely to remain modest indefinitely. Appendix Figure A9 plots racial gaps in foreclosure rates at the county-year level against local unemployment rates and house price growth. Even after the Great Recession, counties with higher unemployment rates and lower house price growth exhibited larger racial gaps in foreclosures. This finding suggests that while the Great Recession clearly exacerbated the racial gaps in housing returns, it would be premature to conclude that the economic relevance of these gaps has diminished. It is very plausible that future downturns may disproportionately increase distressed sales among minority homeowners, particularly given more financial fragility and higher employment cyclicality among Hispanic and Black workers (Hoynes et al., 2012). We use Census employment and population data to show that Hispanic and Black workers experienced higher employment cyclicality both before and after the Great Recession (Appendix Figure A10). It therefore remains to be seen whether future economic downturns will exacerbate racial disparities in housing returns.

Homeowner Characteristics Income at home purchase, family structure, and leverage explain a small share of racial gaps in housing returns. In theory, racial differences in housing returns could plausibly arise solely from racial differences in household characteristics, such as income and family composition, or from differences in leverage that lead to different propensities to default (Gupta and Hansman, 2022). The third specification in Figure 1 estimates the racial gap including fixed effects that interact purchase year, county, the gender of the primary loan applicant, and an indicator for the presence of a co-applicant (a measure of family composition), and fixed effects that interact purchase year, county, and deciles of income at home purchase. In other words, we allow for differential time trends for residents of each county with different family structures and levels of income and leverage. The fourth specification additionally includes fixed effects that interact purchase year, county, and 1 percentage point bins of combined loan-to-value at origination. The inclusion of these controls has the modest effect of reducing the gaps in returns by around 0.3 percentage points.

We further unpack the role of these homeowner characteristics in Figure 2, which estimates Equation 5, simultaneously interacting race indicators with family structure, income, and leverage, and including fixed effects that interact purchase year, county, and the three dimensions of heterogeneity. To interpret these coefficients, note that the main coefficients in the top row represent gaps for the supposedly safest group of borrowers (low-leverage, high-income couples)—for this group, the Black-White (Hispanic-White) gap in unlevered returns is 0.7 (0.5) percentage points. In comparison, the gap for high-leverage, low-income, single male homeowners is 1.8 (1.7) percentage points.[26] Differences in returns by family composition play an important role for the Black-White gap, which is larger among single-headed households. While the Black-White and Hispanic-White gaps vary by family composition, income, and leverage, the gaps exist in each demographic category. These patterns indicate that homeowner characteristics partly explain the gap in returns because the gap varies by demographic groups, and not because all homeowners (regardless of race) within certain demographic groups exhibit lower returns on average.[27]

Neighborhoods In principle, the racial gap in returns could be a result of neighborhood-specific factors, like lower rates of house price appreciation, or more severe distressed sale discounts. The last specification in Figure 1 further controls for fixed effects that interact purchase year and Census tract, in addition to the controls for leverage, income, gender, and family composition. Adding this control for neighborhood explains about half (one-third) of the remaining gap in returns and distressed sales for Black (Hispanic) homeowners.

In light of systematic sorting of homeowners across neighborhoods, interpreting the explanatory power of Census tract warrants caution. As previously noted, this explanatory power consists of some combination of the causal impacts of neighborhoods and selection into neighborhoods. Consequently, controlling for tract may capture unobserved individual-level factors like job stability, liquid wealth holdings, and social ties, all of which are likely to be relevant for a homeowner's capacity for avoiding a distressed sale.

The patterns we document indicate that differences in neighborhoods cannot fully account for racial differences in returns and distress. Moreover, the finding that distressed sales continue

[26]The gap for this latter group can be calculated by adding the main, income, leverage, and family effects: $0.74 + 0.45 + 0.14 + 0.44 = 1.8$ percentage points.

[27]In Appendix Figure A11, we present gaps among an additional array of characteristics, including an property characteristics, an age proxy, debt-to-income ratio, and urban vs. rural status. Comparisons across these demographic characteristics illustrates a similar pattern: average returns among White homeowners are similar across these dimensions, but racial gaps tend to vary.

to fully mediate the racial gap in distressed sales implies that neighborhoods have no average impact on returns conditional on avoiding a distressed sale. To further illustrate this finding, Figure 3 estimates Equation 5, including the previous controls for purchase year, county, income, gender, family composition, and leverage, and interacting race indicators with neighborhood racial composition. Although racial gaps in housing returns are larger in minority neighborhoods (Panel A), interacting race and neighborhood quintile with sale type (Panel B) reveals that there is no racial gap in returns for regular sales, regardless of neighborhood racial composition. These results demonstrate that the racial gap in housing returns is created by a combination of higher rates of distressed sales among minorities and higher distressed sale penalties in minority neighborhoods, rather than lower levels of house price growth in minority neighborhoods.

One channel through which neighborhoods impact the racial gap in housing return is through neighborhood-specific distressed sale discounts. As is evident from Figure 1, Panels B and C, distressed sales mediate the racial gap in housing returns both because minority homeowners are more likely to experience a distressed sale and because minority homeowners experience larger house price penalties from distressed sales. To quantify the relative importance of these two factors, we conduct a simple threefold Blinder-Oaxaca decomposition of returns residualized within county and purchase year. This decomposition indicates that if Black homeowners were to experience the same rate of distressed sales as White homeowners (but not the same distressed sale penalty), the Black-White gap in unlevered housing returns would shrink by 84.6%. Analogous calculations for Hispanic homeowners indicate a reduction of 133.6%, which is driven by higher returns among Hispanic homeowners among regular sales. These figures indicate that the large majority of the gap is attributable to higher rates of distressed home sales, with a modest but quantitatively meaningful remainder attributable to lower returns conditional on distress.

In principle, the lower returns of minorities conditional on realizing a distressed sale could be driven by higher rates of depreciation for distressed minority homes or by more severe distressed sale discounts in neighborhoods in which minorities are located. We find evidence that the latter explanation is more quantitatively important. We estimate racial differences in distressed sale discounts, measured by dividing a distressed property's actual sale price by its imputed value (purchase price inflated by FHFA county-level home price index). Appendix Figure A12 presents estimates of Equation 5 for distressed sale discounts, and shows that the majority of the difference in distressed discounts can be accounted for by controlling for Census tract. Note that the remaining difference can be interpreted as an upper bound for the share of the distressed sale discount that can be attributed to racial differences in depreciation during foreclosure.[28]

[28] In addition, differences in the type of distressed sale do not appear to be important drivers of racial differences in distressed sale discounts, even though short sales comprise a larger share of distressed sales for White homeowners than they do for minorities. Previous research shows that short sales carry more modest penalties than foreclosures (Zhang, 2019), and in our sample, 42% of distressed sales for White homeowners are short sales, compared to only 25% and 29% for Black and Hispanic homeowners, respectively. However, racial differences in distressed sale discounts exist within both foreclosures and short sales, and racial returns gaps within foreclosures are only slightly smaller than those within all distressed sales (Appendix Figure A13). Moreover, in Appendix Figure A12, controlling for short sales only slightly reduces differences in sale discounts among distressed sales. These findings indicate that compositional differences are not behind the difference in distressed sale discounts.

Given that controlling for Census tract absorbs both characteristics of the neighborhood and characteristics of residents, we use a direct measure of neighborhood market conditions to provide evidence that such conditions can explain the magnitude of distressed sale discounts. We construct a measure of housing market thickness using multiple listing services (MLS) data, which allows us to measure the median days that for-sale properties have been listed in a given ZIP code-year.[29] Overall, Black and Hispanic homeowners are particularly likely to realized distressed sales in neighborhoods with relatively illiquid real estate markets (Appendix Figure A14, Panel A). Interacting race indicators with sale type and quintiles of market thickness reveals that differences in sale discounts between regular sales and distressed sales decrease substantially with market thickness, indicating that distressed sale penalties are larger in these ZIP codes. This finding is presented in Appendix Figure A14, Panel B, which compares sale discounts among properties that were sold within our sample window, and controls for county, purchase year, and sale year. For instance, distressed sale discounts experienced by Black homeowners are about 13 percentage points lower in the least-thick markets relative to the thickest markets. In contrast, market thickness has little impact on returns for regular sales. Together, these results indicate that market thickness is an important characteristic of neighborhoods that contributes to larger distressed sale discounts, particularly for Black homeowners.

The finding that average returns among non-distressed sales do not differ by neighborhood racial composition signals a departure from historical trends. Previous research has documented historical instances in which minorities experienced lower levels of neighborhood-level house price growth. For instance, Akbar et al. (2019) show that during 1930s and 1940s, Black families entering a previously-White neighborhood paid a house price premium of 28%, only to see the value of their homes fall as White homeowners left the neighborhood. Boustan and Margo (2013) document similar patterns of White flight and depressed home prices between 1940 and 1980. In contrast to these findings, we find that differences in neighborhood-level house price growth in recent decades have not disadvantaged minorities on average. This finding is also evident in the distribution of house price growth in the last two decades. Between 2001 and 2020, the distribution of local house price growth exhibits a thicker right tail in Census tracts with higher minority shares, indicating that minority neighborhoods were more likely to experience very rapid increases in house prices (Appendix Figure A15).

Our findings are consistent with studies of present-day racial disparities in purchases prices and home values. For example, Bayer et al. (2017) find that Black and Hispanic homebuyers pay 2% more for houses than their White counterparts. Our finding of a 39 basis point Black-White gap in returns among non-distressed sales (Table 2) is consistent with this finding when annualized over the ownership spell. Other work has documented substantial undervaluation of properties in Black neighborhoods in terms of the level of house prices, compared to houses in other neighborhoods with similar amenities but fewer minorities (Perry et al., 2018). Yet we find that this undervaluation does not seem to meaningfully affect average capital gains for minority

[29] The MLS data provide property-level listings with national coverage starting in 2011.

homeowners who avoid a distressed sale. To show this, Appendix Figure A5 plots returns that are predicted from the county-level house price index against realized returns for the sample of properties where we observe a non-distressed sale, which shows that returns closely track local house price growth regardless of race. Overall, these comparisons highlight the large quantitative importance of the racial gaps in returns that are revealed when factoring in the role of distressed sales.

Interpretation Taken together, these results indicate that differences in purchase timing and geography (i.e., county) can explain about one-third of the racial difference in returns and distress. Note that while additionally interacting sale year with purchase year and county offers some additional explanatory power (see Appendix Figures A16 and A17), this control is difficult to interpret given that sale timing is an endogenous outcome and related to the occurrence of distressed sales, which tend to occur in years soon after home purchase.

For much of the remainder of the gap, higher underlying propensities of minority homeowners to realize distressed sales appear to remain economically relevant after controlling for observable characteristics like income, family structure, and leverage. Moreover, even after controlling for neighborhoods, which captures sorting of households along other unobserved characteristics along with the causal impacts of neighborhoods, about one-third of the racial gap in returns remains unexplained. Therefore, other economic mechanisms that are not captured by these characteristics must underlie the remaining differences in distressed sales.

5 Why Are Black and Hispanic Homeowners More Distressed?

This section identifies the mechanisms that generate higher rates of minority distress that exist even conditional on characteristics like income and family structure.

A seemingly natural explanation for higher rates of minority distress conditional on observable characteristics like income and family structure is that minority homeowners face greater risk of financial distress, even conditional on these characteristics. Indeed, relatively high financial instability among minority households has been widely documented (e.g., Ritter and Taylor 2011; Ganong et al. 2020). Yet, the existence of differences in distress among minority *homeowners* is puzzling because multiple factors should prevent financially unstable households from becoming homeowners. For example, the mortgage underwriting process is designed to screen out households who are deemed too risky based on observable measures of risk like income and credit score, and borrowing costs are also designed to reflect risk. Moreover, since experiencing a distressed sale is a very costly outcome for borrowers (Ganong and Noel 2020b), those who are high-risk should have a relative disincentive to become homeowners.

In the presence of lender screening and incentives for risky households to self-select out of homeownership, there are three potential mechanisms that can generate higher levels of realized distress among minority homeowners. We summarize these three mechanisms below, and in Appendix J, we present a simple Roy model of selection into homeownership that formalizes these mechanisms. In this framework, households are characterized by both observable and unobservable risk of distress.

The first potential mechanism is that minority households have higher levels of observable risk on average, and that mortgage contract terms are set in such a way that does not overly discourage observably risky homeowners from purchasing homes. By setting the terms of a mortgage (e.g., interest rate, down payment) according to the level of observable household risk, lenders and policymakers can affect the level of realized distress conditional on observed risk. Therefore, changes in mortgage underwriting rules (both by lenders and by government agencies like the GSEs) can affect racial disparities in distress and returns.

The second potential mechanism is that minorities are unobservably riskier on average. In theory, higher levels of unobserved risk among minorities can lead to higher levels of distress conditional on becoming a homeowner, depending on the distribution of unobserved risk (Dobbie et al., 2021). Moreover, if minority households are unaware that they are unobservably riskier on average, then riskier minority households will suboptimally select into homeownership, increasing the level of realized distress among minority homeowners.

The third potential mechanism is that higher levels of distress among minority homeowners are a product of differential lender treatment. For example, lenders could offer different mortgage contracts to minorities, or apply different screening criteria to minorities. Any such differential lender treatment could lead to higher levels of risk among minority homeowners.

To empirically quantify the importance of each of these mechanisms, we leverage a dataset that links administrative mortgage servicing and credit bureau records to the property data. These data have the important advantage of allowing us to observe a wide range of financial behaviors and outcomes that reflect borrower risk. The mortgage servicing records are provided by McDash, and contain information on both mortgage characteristics measured at origination (e.g., loan-to-value ratio, property value, and borrower credit scores) and mortgage performance information including monthly balance, payment amount, delinquency, and foreclosure. The credit bureau data are provided by Equifax and are comprised of a monthly panel of outcomes for households in the mortgage servicing data, including balances and delinquencies on credit cards, auto loans, and mortgages, accounts in collections, and continually-updated credit scores. The mortgage servicing and credit bureau records cover the period 2005 to 2018 and capture between three- to four-fifths of the US mortgage market, depending on the time period. Summary statistics for our sample of property records linked with credit and mortgage servicing records are presented in Table 1, Panel B. Although the credit and mortgage servicing records allow us to observe a wider range of outcomes for our study sample, this linkage reduces the number of properties merged with our study sample by 57% (illustrated in Appendix Figure A1). Appendix C provides more details on the sample construction and linkage.

We analyze two key variables in these data. The first is mortgage default, which is defined as an indicator that a homeowner is 90 or more days past due on their primary mortgage and is measured in the servicing data. Importantly, this measures of default offers a more accurate measure of risk and financial distress relative to distressed sales, because the occurrence of a distressed sale depends on a lender's willingness to foreclose or accept a short sale. Since loan default occurs when a borrower does not make their loan payment, it solely reflects borrower behavior. The second key variable is an individual's credit score. Credit scores are designed by ratings agencies to predict the risk that a borrower defaults on a loan using the borrower's prior credit history. In doing so, credit scores indirectly capture economic instability prior to home purchase as reflected in prior outcomes (e.g., credit card delinquency). Largely due to regulation seeking to combat discriminatory treatment, standard credit scores do not use employment, family composition, neighborhood, or race to form predictions. Overall, credit scores can be interpreted as a backward-looking proxy for latent risk. We observe both the initial credit score used for underwriting, which is recorded in the serving data, and a dynamic credit score (Vantage 3.0), which is recorded in the credit bureau data and computed each month.

The remainder of this section uses these data to analyze the importance of each mechanism underlying racial differences in distress.

5.1 Differences in Observable Risk

Minorities who become homeowners tend to be riskier based on characteristics that are observable at home purchase. Figure 4, Panel A plots the distribution of credit scores at home purchase by race, which shows that credit scores are lower for minorities on average. Panel B plots rates of default separately by race and credit score at home purchase, and shows that default rates are higher for low-credit score borrowers. In addition, this figure reveals that minorities exhibit substantially higher rates of distress than non-minorities even conditional on credit score, consistent with the findings in Blattner and Nelson (2021). These two findings indicate that higher levels of minority distress are likely due to differences in both observable and unobservable risk.

To quantify the relative importance of unobservable versus observable risk, we estimate a version of Equation 5 with mortgage default as the outcome. We estimate this regression using a yearly panel consisting of the June credit and mortgage servicing records. Sequentially adding controls allows us to assess what share of the racial difference can be predicted using observable characteristics. Figure 4, Panel C shows that the raw racial gaps in financial distress measured by at least 90 days mortgage delinquency are 2.6 (1.8) percentage points for Black (Hispanic) homeowners. Controlling for purchase year and county modestly reduces the gaps to 2.2 (1.6) percentage points. The majority of this difference can be explained by four characteristics that are observable to lenders at home purchase: family structure, income, leverage, and credit score. Controlling for these factors reduces the gaps to 0.98 (0.94) percentage points, meaning that observable characteristics can explain about 55% (41%) of the gap in default. The bulk of this explanatory power comes from credit scores, which are specifically designed to predict default. In addition, a relatively small share of the gap can be explained by mortgage characteristics, suggesting that lenders offering minorities different contracts is unlikely to play a major role in explaining differences in distress. Similar patterns apply to non-mortgage default (Appendix Figure A18), indicating that the relative explanatory power of these characteristics applies to financial distress more generally.

These results indicate that the majority of the racial gap in default risk can be explained by differences in observable risk and choice of leverage. Although these observable characteristics are highly predictive of default, they do not capture all factors that may cause households to default. In particular, credit scores are backward-looking predictors of future default, and therefore may partly omit the risk of future events like job loss. If minorities face a higher latent probability of job loss, credit scores may underestimate the risk of default for that group (Blattner and Nelson, 2021). Thus, this omission leaves ample scope for risk that is unobserved to lenders to explain the remaining gap in default.[30] Since household liquidity and income stability are two factors that are not captured by credit scores but are key triggers for default (Low 2021; Ganong and Noel 2020b), we next analyze these factors as potential causes of excess minority default.

5.2 Unobserved Risk: Liquidity and Income Stability

Differences in unobservable default risk can arise from many different factors. We document that racial differences in liquidity and income stability represent important sources of risk, which are not captured in credit reports and not directly observable to lenders.

Differences in Liquidity and Income Stability in Survey Data To directly measure liquidity and income, we analyze a sample of homeowners in the Survey of Income and Program Participation (SIPP) surveyed between 1992 and 2017. SIPP reports income earned by all household members, and liquid wealth held in bank accounts, stocks, and bonds. See Appendix C for more details.

Racial gaps in liquid wealth are even larger than gaps in total wealth. Panels A and B of Figure 5 plot median total net wealth and median liquid wealth as a share of annual household income, by the race and age of the household head. While racial disparities exist for both measures of wealth, the gap in total wealth is roughly constant over the life cycle, while the gap in liquid wealth increases dramatically. At less than 20% of annual earnings for almost all age groups, median liquid wealth among Black and Hispanic homeowners is extremely low. Liquid wealth for minority homeowners is small in dollar terms as well: median liquid wealth for Black and Hispanic homeowners is $2,400 and $5,400, respectively, consistent with the findings in Ganong et al. (2020).

Minority homeowners also have lower and less stable incomes than White homeowners. Figure 5 plots median income over the life cycle (Panel C) and the likelihood of transitioning to unemployment as a function of income (Panel D). Not only do minority homeowners earn substantially less income at all ages, they are also 2 to 4 percentage points more likely to experience a transition to unemployment, conditional on pre-unemployment income.

Controlling for income stability and liquidity explains a large share of racial differences in mortgage delinquency. Homeowners in SIPP are asked whether they have missed mortgage payments in the last 12 months. Table 4 shows that Black homeowners are 5.9 percentage points more likely to have missed mortgage payments, relative to a mean of 4.25% for White homeowners. Hispanic

[30] One indication of this is that controlling for neighborhood in Figure 4 yields some modest explanatory power. However, legal prohibitions designed to curb lending discrimination via redlining have meant that underwriting generally does not take neighborhood into account directly.

homeowners are 3.7 percentage points more likely to have missed mortgage payments. Controlling for liquidity, job loss in the prior year, and income substantially reduces the impacts of the race indicators. Column 2 shows that the coefficients are 4.1 and 2.2 percentage points for Black and Hispanic homeowners, respectively. Comparing the difference in the coefficients in Columns 1 and 3 implies that liquidity and income stability explain about 30% and 41% of the racial gap in delinquency for Black and Hispanic homeowners, respectively. Columns 4 through 6 repeat the same exercise but include controls for the level of household income, current loan-to-value, and family composition. Comparing the coefficients in Columns 4 and 6 indicates that when including these additional controls, liquidity and income stability explain 29% and 70% of the gap for Black and Hispanic homeowners, respectively.

Although the analysis of homeowners in SIPP provides direct evidence that racial differences in income stability and liquidity are relevant for explaining racial differences in the risk of distress, this sample is substantially smaller than our administrative data, and also subject to measurement error inherent in survey-based measures of liquidity, delinquency, and job stability. Moreover, this exercise does not reveal the extent to which higher rates of illiquidity and income stability among minorities are indirectly observable to lenders through credit scores. Since illiquidity and income instability can lead to lower credit scores by placing households at higher risk of delinquency, which is recorded in credit reports, it is possible that lenders at least partly observe risk arising from illiquidity and income instability. These considerations motivate a second empirical exercise that makes use of our administrative data.

Differential Responses to Liquidity Shocks in Administrative Data We estimate the causal impacts of liquidity shocks by race using variation from adjustable-rate mortgage (ARM) resets, which create month-to-month changes in monthly payments. This variation has been applied extensively in prior research (e.g.,Fuster and Willen 2017 and Di Maggio et al. 2017). We estimate monthly event studies of the following form:

$$y_{it} = \alpha_i + \gamma_{s,c(i)} + \sum_{s \neq -3} \beta_s 1[t = e_i + s](\Delta P_i) + \varepsilon_{it} \tag{6}$$

In Equation 6, y_{it} denotes an outcome for the homeowner associated with mortgage i in month t, α_i denotes homeowner/mortgage fixed effects, and γ_s denotes event time fixed effects interacted with characteristics of homeowner i, respectively. Our baseline specification interacts event time with origination year. ΔP_i denotes the percent change in monthly principal and interest payments due to the ARM reset. Note that Equation 6 applies event time, rather than calendar time, fixed effects. Consequently, the estimator does not use already-treated units as controls for not-yet-treated units. This approach, which is possible because of the availability of a continuous regressor ΔP, circumvents the bias arising from treatment effect heterogeneity in staggered treatment designs documented in previous work (Sun and Abraham 2021; Goodman-Bacon 2021; Callaway and Sant'Anna 2021).

We construct a sample of homeowners with ARMs in the linked credit bureau and mortgage servicing data to estimate Equation 6. This sample consists of first-lien ARM mortgages held by Black, Hispanic, and White owner-occupants originated between 2000 and 2015. We also drop negative amortization loans and restrict to a panel of loans that is balanced between 12 months before and 12 months after a rate reset. These restrictions yield a sample of 200 thousand mortgages. Table 1, Panel C provides summary statistics.

Under the assumption that the primary reason for default is a lack of liquidity, the magnitude of the default response measures the extent of underlying illiquidity. This assumption is plausible given that default directly leads to foreclosure, and is supported by a large body of evidence indicating that liquidity is a key determinant of mortgage default (Ganong and Noel 2020b; Low 2021).

Black and Hispanic homeowners exhibit stronger default responses to payment shocks, relative to White homeowners. Figure 6 presents the results of estimating Equation 6. To maximize comparability between estimates, we re-weight Black and Hispanic homeowners to match the distribution of event years of White homeowners. The point estimates imply that in response to a 10% increase in monthly mortgage payments, White homeowners exhibit an increase in 90-day mortgage default of 3.0 percentage points after 12 months. In contrast, Black and Hispanic homeowners exhibit increases in 90-day default of 4.5 and 7.1 percentage points after twelve months, respectively.

Credit scores strongly predict the magnitude of default responses, but not racial differences. Appendix Figure A19 shows that impacts are more than twice as large for lower credit score, relative to higher credit score homeowners, with qualitatively similar patterns by income. However, credit scores fail to capture differences in default responses across race. Black and Hispanic homeowners exhibit larger impacts even conditional on credit score and income, suggesting that observable measures of risk may not fully predict excess default among minorities.

To demonstrate the inability of observable measures of risk to capture racial differences in default responses, Table 5 estimates excess default impacts for Black and Hispanic homeowners controlling for measures of risk by interacting γ_s with observable characteristics. Controlling for demographics, leverage, and credit score absorbs only a small share of racial differences in default responses. Census tract, which is not typically used in lending decisions, absorbs about one-fourth of the Black-White difference in sensitivity; however neighborhood is not typically used for making lending decisions. Together, these findings provide evidence that the majority of the excess risk of minorities created by illiquidity is not captured by measures of risk that are observable to lenders at home purchase.

Although our data do not allow us to measure the extent to which racial differences in income instability are predictable by characteristics at home purchase, evidence from a sample of homeowners in the Current Population Survey suggest that such differences may be very difficult to predict.[31] In Appendix Table A10, we document that controlling for standard observable characteristics including age, education, income, occupation, and family structure, Black and Hispanic homeowners are about 4-6 percentage points more likely to experience an income loss of more than

[31] See Appendix C for more details on the CPS sample.

30% in a given year. Importantly, these gaps are not significantly reduced when restricting to homeowners with five or ten years of work experience in their current job. This finding suggests that relatively straightforward screening criteria like requiring longer employment histories may not reduce racial gaps in distress (and would also exclude many safe borrowers from homeownership).

5.3 Unobserved Risk: Strategic Default

In theory, differences in strategic default could contribute to higher levels of unobserved risk among minority homeowners. We assess this possibility in two ways. First, we apply the research design in Gupta and Hansman (2022) to test whether the causal effect of leverage on default differs by race. In general, leverage and default risk exhibit a positive correlation, which reflects a combination of two factors. The first is adverse selection—riskier borrowers tend to select into higher-leverage contracts ex-ante. The second is the causal impacts of leverage—higher leverage ex-post can causally increase default by reducing incentives to avoid foreclosure or by making it harder to sell, refinance, or extract equity. Gupta and Hansman (2022) disentangle causal impacts from adverse selection by using quasi-random variation in leverage after origination. Specifically, this approach leverages variation in interest rates for option adjustable-rate mortgages (option ARMs). The interest rate adjustments of option ARMs are tied to prespecified interest rates rates, usually LIBOR or Treasury rates. During the 2008 financial crisis, these indices diverged, generating quasi-random variation in outstanding balances (and therefore leverage) among borrowers with the same initial leverage. Since the goal of our exercise is to assess whether strategic default varies by race, we focus on estimating racial differences in the causal impact of ex-post leverage. Accordingly, we implement a version of the approach in (Gupta and Hansman, 2022) that allows relatively flexible controls for variation in ex-ante leverage. See Appendix H for more details.

Consistent with the findings in Gupta and Hansman (2022), we find that ex-post leverage has a large causal impact on subsequent default; however, we find no evidence that these impacts are larger for minorities (results presented in Appendix H). Although leverage can causally impact default through channels other than strategic default (e.g., inhibiting the sale of a property), the finding that the causal impacts of leverage are not larger for minorities suggests that strategic default motives are not stronger for minorities. In addition, these findings offer a useful contrast with the analysis of responses to ARM payment shocks analyzed in Section 5.2. While monthly payment shocks directly impact household liquidity, the leverage shocks in the research design from Gupta and Hansman (2022) only affect the remaining principal of the mortgage. We find racial gaps in the former but not the latter, which represents further evidence that differences in liquidity play a central role in explaining higher rates of default among minorities.

Our second approach to assessing differences in strategic default by race is to analyze a sample of homeowners in the National Survey of Mortgage Originations (NSMO), which asks homeowners questions pertaining to strategic default. We analyze a sample of NSMO respondents who were asked about their primary residences, and with mortgages opened between 2013 and 2017. See Section C for more details on the NSMO sample.

Homeowners in NSMO are asked, whether they agree or disagree that, "It is okay to default or stop making mortgage payments if it is in the borrower's financial interest." Table 6, Column 1 estimates racial differences in the share of respondents who agree with this statement, controlling for credit score, age, income, and marital status. Compared to a mean of 6.1%, we estimate a statistically insignificant Black-White difference of 0.05 percentage points (s.e. 0.65), suggesting no meaningful Black-White differences in attitudes towards strategic default. Although Hispanic homeowners are 3.3 percentage points more likely to agree with this statement, Hispanic homeowners in this sample are not more likely to default (Appendix Table A12), possibly due to Hispanic homeowners experiencing relatively rapid house price growth during the survey period. Coupled with the absence of larger impacts of leverage for minorities, we interpret these collective findings as indicating that there is little role for differences in strategic default to explain higher rates of default among minorities.

5.4 Unobserved Risk: Information Frictions

Even if minorities exhibit higher levels of unobserved risk, risky minority homeowners should have a relative disincentive to become homeowners. One potential explanation for this lack of self-selection out of homeownership is that households are unaware of their own risk. Prior work studying workers' own perceptions of their income and employment risk (Manski, 2004) has found that although stated probabilities of unemployment risk are predictive of future outcomes, workers' predictions differ markedly from the distribution of realized outcomes. To assess the relevance of such information frictions for excess minority default, we analyze the sample of homeowners in the National Survey of Mortgage Originations (NSMO), which elicits homeowners' expectations about the future. Appendix Table A11 reproduces relevant questions from the survey.

We find evidence of relatively overoptimistic expectations among Black homeowners. When asked to rate house price growth in recent years on a three-point scale (significant decrease, little/no change, significant increase), Black homeowners' answers are lower than White homeowners' by a statistically insignificant 0.02 points, or 0.04 standard deviations (Table 6, Column 2). Yet, when asked about future house price growth on a five-point scale (decrease a lot, decrease a little, remain about the same, increase a little, increase a lot), Black homeowners report expectations that are about 0.07 points (0.1 standard deviations) more optimistic than White homeowners ($p < 0.01$), controlling for past experiences (Column 3). However, realized house price growth in the two years following origination is actually 1.1 percentage points lower for Black homeowners (Column 4; $p < 0.01$).[32]

Similarly, although Black homeowners are 2.8 percentage points more likely to report the occurrence of personal financial crises in the past ($p < 0.01$), expectations about the likelihood of future crises are similar for Black and White homeowners (Columns 5 and 6), a finding in stark contrast with both higher observed rates of financial distress among minority homeowners in our adminis-

[32] Realized house price growth is computed by dividing mark-to-market LTV at origination by LTV two years after origination.

trative data, and with realized 90-day default rates that are 2.5 percentage points higher among Black homeowners in the first two years after origination (Column 7). Lastly, Black homeowners are significantly more optimistic about their own future income growth, relative to past experiences, both elicited on a 3-point scale (Table 6, Columns 8 and 9). Specifically, their expectations are 0.3 standard deviations higher than White homeowners ($p < 0.001$), a finding that contrasts with our findings in SIPP and CPS that minorities are more likely to experience income losses.

Hispanic homeowners also tend to express more optimistic expectations than White homeowners, although there is less evidence that these expectations are inaccurate, at least for the time period covered by our NSMO sample (i.e., 2013-2017). The Hispanic-White differences estimated in Table 6 indicate that Hispanic homeowners have more optimistic expectations about house price growth and income growth, but unlike Black homeowners, Hispanic homeowners experienced higher levels of previous house price growth, subsequent house price growth, and previous income growth. Moreover, there is no Hispanic-White difference in realized default or past experiences of financial crises (consistent with our finding that Hispanic-White gaps in financial distress have abated in recent years). Although these findings do not reveal Hispanic-White differences in overoptimism, it is possible that these findings would have been different in earlier years during which Hispanic homeowners exhibited much higher rates of distress than White homeowners.

These stated expectations appear to translate into differences in financial decision-making. Appendix Table A12 correlates stated expectations with administrative measures of loan-to-value and debt-to-income ratios, and finds that those who have more optimistic expectations tend to take on more leverage. Taken together, these patterns suggest the existence of information frictions. If minority households underestimate their likelihood of experiencing financial distress, subsequent financial decision-making may be suboptimal and contribute to higher realized rates of distress among minority homeowners.

5.5 Credit Supply

In addition to the mechanisms discussed above, a number of credit supply factors could contribute to excess risk among minorities. First, credit supply expansions may lead to higher rates of minority distress if marginal borrowers are riskier or more likely to be minorities. Second, a given lender may observe signals of risk that are not observable to policymakers/regulators, and differentially grant credit to riskier minority applicants, offer different mortgage terms to minority applicants, or cater to populations with a disproportionate share of high-risk minorities.

General Expansion of Credit During our sample period, the primary instance of a credit supply expansion occurred in the early 2000s, immediately prior to the Great Recession. This period saw a substantial increase in the supply of credit to minorities, and was followed by large increases in foreclosure rates during the Great Recession, particularly among Black and Hispanic homeowners (see Appendix Figures A20 and A3). Consequently, the Black-White gap in distressed sales increased from about 11 percentage points among homes purchased in 2002, to 17 percentage points among homes purchased in 2006 (Appendix Figure A24). The rise in both the levels of distress and racial gaps in distress is plausibly driven by some combination of two factors: changes in credit supply that extended credit to riskier minority borrowers; and the impacts of the Great Recession, which entailed disproportionate job losses for minority households (Hoynes et al., 2012).

We assess the relative quantitative importance of the two factors by comparing outcomes for mortgages from different investor channels. We note that the expansion of credit to minorities in the early 2000s was driven by mortgages identified in the HMDA data as being funded by sources other than Fannie Mae, Freddie Mac, or the FHA. To show this, we build a county-level panel of purchase mortgage originations using the HMDA data, which cover the near-universe of originations.

Figure 7, Panel A, plots the change in home purchases with GSE and FHA mortgages by race and year. We regress the number of purchases in a given county and race (normalized to mean 2000-2003 levels) on race-by-year indicators and county fixed effects. The resulting patterns show that GSE/FHA purchases exhibit moderate declines between 2002 and 2007. In contrast, mortgages that are recorded in the HMDA data as not being sold (i.e., portfolio loans) or as being privately securitized exhibit substantial growth.[33] Figure 7, Panels B and C plot the excess growth of these loans within counties. We regress home purchases on investor-by-year-by-race indicators and fixed effects that interact investor, county, and race, such that the estimated coefficients represent within-county differences relative to GSE/FHA loans.[34] Although White homeowners also experience an increase in lending through these channels, the increase is substantially more pronounced among Black and Hispanic homeowners.

For all types of loans, homes purchased in the mid-2000s are more likely to end in distress, relative to those purchased in the early 2000s; however, the increase in distress is more pronounced among non-GSE/FHA loans. Panel D of Figure 7 plots rates of distressed sales by purchase year using our baseline sample of ownership spells, restricted to GSE/FHA loans (coefficients are from a regression analogous to that in Panel A). Black (Hispanic) homeowners purchasing in 2006 are 18 (31) percentage points more likely to experience a distressed sale relative to those purchasing in 2002, compared to 15 percentage points for White homeowners. Given that this channel does not exhibit an increase in supply during this time period, we interpret these patterns as indicating that at least part of the increases in distress are due to factors other than the pool of new homebuyers becoming riskier.

Figure 7, Panels E and F quantify the relative increase in distress among non-GSE/FHA loans by estimating a regression analogous to that in Panels B and C.[35] Both channels exhibit higher increases in distress than GSE/FHA loans. For example, the increases in distress among private/other

[33] The HMDA data categorize investor status based on information available in the calendar year after origination. Although this categorization is effective in allowing us to isolate a channel that does not exhibit a supply expansion (i.e., loans categorized as GSE/FHA), we note that this method of categorization may conflate other channels. This conflation is illustrated in Appendix Table A13, which compares investor status in HMDA to investor status in McDash. For example, mortgages labeled as unsold are often later privately securitized. Accordingly, in our decomposition exercise we group mortgages labeled as unsold, private, and other into a single category.

[34] Specifically, we estimate the following specification: $y_{crti} = \sum_{t' \neq 2002} \sum_{r' \in B,H,W} \sum_{i \in Private,Unsold} \beta_{t'r'i'} \mathbb{1}[t = t', r = r', i = i'] + \mu_{cri} + \mu_{crt} + \varepsilon_{crti}$, where c denotes county, r denotes race, t denotes year, and i denotes investor channel.

[35] See Footnote 34 for regression specification.

loans relative to GSE/FHA loans are 13, 15, and 10 percentage points higher for Black, Hispanic, and White homeowners, respectively. We interpret these findings as indicating that the increase in credit supply through these non-GSE/FHA channels also contributed to the increase in distress. Note that the patterns in Figure 7, which reflect within-county estimates, are also apparent in aggregate patterns presented in Appendix Figures A20 and A21.

We conduct a simple accounting exercise to quantify the relative importance of credit supply and non-credit supply factors. Let $d^r_{c,t}$ denote the fraction of ownership spells ending in distress for homes purchased in year t by homeowners of race r with mortgages originated through channel $c \in \{G, NG\}$, corresponding to GSE/FHA and non-GSE/FHA loans, respectively. Let $f^r_{NG,t}$ denote the non-GSE/FHA share of purchases among race r in year t. By construction, the share of purchases in year t among race r is given by $d^r_t = (1 - f^r_{NG,t})d^r_{G,t} + f^r_{NG,t}d^r_{NG,t}$. This equation can be rearranged to express the change in distressed sale rates for race r, relative to 2002 as

$$d^r_t - d^r_{02} = \left(d^r_{G,t} - d^r_{G,02}\right) + \left[f^r_{NG,t}\left(d^r_{NG,t} - d^r_{G,t}\right) - f^r_{NG,02}\left(d^r_{NG,02} - d^r_{G,02}\right)\right] \quad (7)$$

In the above, the first term captures the share of the increase in distress between 2002 and t that is not due to changes in credit supply. The second term captures the share that can be attributed to credit supply, and reflects both the effect of the expansion of non-GSE/FHA lending (holding fixed the differences in risk between GSE/FHA and non-GSE/FHA loans) and the change in relative riskiness of non-GSE/FHA loans between 2002 and t.

Intuitively, this decomposition treats originations and distress among GSE/FHA loans as a counterfactual without an expansion in credit supply. That is, increases in distress among GSE/FHA loans (which did not exhibit the same increase in credit supply) capture the impact of non-credit supply factors, like employment losses and decline in house prices stemming from the Great Recession. In doing so, this approach abstracts from (local) general equilibrium effects of the credit supply shock, and thus only considers the direct impact of lending standards on distress sales taking other economic factors such as house prices as given (Di Maggio and Kermani 2017; Mian et al. 2019; Greenwald and Guren 2021). Moreover, we may underestimate the share attributable to non-credit supply factors, to the extent that the reductions in GSE/FHA lending during this period (Figure 7, Panel A) are a result of riskier borrowers substituting into other channels.

Following our approach in Figure 7, we compute this decomposition within counties to exclude potentially confounding variation in county characteristics. For mortgages originated between 2004 and 2006, we find that about 42%, 30%, and 35% of the increase in distress relative to 2002 is attributable to credit supply factors for Black, Hispanic, and White homeowners, respectively. This finding is consistent with Palmer (2024), who finds that changes in credit standards can explain about 30% of the differences in default rates across purchase year cohorts among subprime loans, whereas declines in house prices can account for an additional 60%. See Appendix Table A14 for component values and year-by-year calculations, and Appendix K for more details on these calculations.

Thes preceding calculations decompose increases in distress within race. One can follow a similar logic to assess what share of the increase in the racial gap in distress is attributable to credit supply. Appendix Figure A24 shows that the Black-White gap in distressed sales rose by 6.2 percentage points between homes purchased in 2002 and 2006, but only by 2.4 percentage points for GSE/FHA loans. Through the lens of the decomposition in Equation 7, these differences imply that 61.5% of the overall 6.2 percentage point increase can be attributed to credit supply. Analogous calculations imply that 52.0% of the 12.2 percentage point increase in the Hispanic-White gap between 2002 and 2006 can be attributed to credit supply.

What accounts for the remaining increase in distress, which we interpret as not attributable to the direct impacts of expanded credit supply? General equilibrium effects, which are excluded from the preceding calculations, are a plausible explanation. For example, the credit supply expansions may have increased distress through their impact on local house prices (Favara and Imbs 2015; Di Maggio and Kermani 2017; Mian and Sufi 2022). We assess the potential magnitude of such a channel by building on the findings in Mian and Sufi (2022), who leverage ZIP code-level variation in lender reliance on non-core deposits (i.e., non-core-liability lenders, or "high NCL lenders"). High NCL lenders benefited disproportionately from the rise of private label securitization. Mian and Sufi (2022) document that areas with a larger share of high NCL lenders experienced a larger expansion in credit supply, and a subsequently stronger boom-bust cycle. We analyze impacts of NCL exposure on outcomes for GSE/FHA purchases, and interpret those impacts as capturing general equilibrium effects.[36]

We find evidence that general equilibrium effects can explain the bulk of the remaining increase in distress among GSE/FHA purchases. In Appendix Figure A22, we present the time path of distressed sales among homes purchased with GSE/FHA mortgages, with and without controlling for ZIP code NCL exposure (normalized to the average exposure in the bottom decile of ZIP codes). This comparison indicates that in the absence of credit supply shocks (i.e., in the bottom decile of exposure), GSE/FHA purchases exhibit a much smaller increase in distress throughout this time period. Black and White homeowners exhibit nearly no increase in distress. Similarly, directly controlling for changes in house prices yields a similar conclusion (A22, Panel C). These findings suggest that much of the increase in distress among supposedly safe GSE/FHA mortgages can be explained by the general equilibrium effects of the early-2000s credit supply expansion.

Taken together, these findings indicate that the early-2000s expansion of credit supply played an important role in the subsequent increase of distress during the late-2000s, both by extending credit to riskier borrowers and through general equilibrium effects. This expansion exacerbated racial gaps in distress and housing returns. Two points of interpretation are worth highlighting. First, these findings should be interpreted as explaining the *increase* in racial gaps in distressed sales in the 2000s, rather than the existence of those gaps, which are on the order of 6-10 percentage points both for homes purchased in 2002 and for homes in areas not exposed to the credit supply expansion (see

[36]This interpretation assumes that GSE/FHA lending standards did not change significantly during this time period, and also did not respond to local changes in credit supply. One potential violation of this assumption is if the relative relaxation of credit supply in non-GSE/FHA lending resulted in substitution of riskier borrowers to non-GSE products, which would lead our analysis to underestimate the impact of credit supply.

Appendix Figures A24 and A22). Second, it remains plausible that future economic downturns, even if they are not created by credit markets, will also exacerbate racial gaps. This possibility is suggested by the observation that racial gaps in foreclosures are larger when unemployment is higher, even in years after the Great Recession (Appendix Figure A9), and that Black and Hispanic employment rates are more cyclical, both before and after the Great Recession (Appendix Figure A10).

Differential Treatment in Lending Motivated by our finding that credit supply expansions had a meaningful impact on racial differences in distress, we evaluate the extent to which deliberate lender behavior generates these differences. We present two pieces of evidence that suggest that the role of differential treatment by lenders is likely limited.

First, we note that in Figure 4, Panel C, controlling for mortgage contract characteristics offers little explanatory power for racial differences in default. This finding suggests that differential treatment in terms of contract terms is not a significant contributor to racial differences in default. More generally, this finding is consistent with recent research finding that decades of enforcement of anti-discrimination provisions has been greatly limited the scope for individual lenders to racially discriminate (Bhutta et al., 2022).

Second, we use the mortgage servicing data to analyze differences in default by investor status conditional on a broad range of homeowner characteristics. Intuitively, if lenders deliberately extend credit to higher-risk minorities, either via differential treatment or specifically catering to high-risk minority populations, one would expect the lender to avoid keeping these higher-risk loans on their balance sheets, and instead sell them via securitization. Appendix Figure A23 presents the results of interacting race indicators with securitization status. We find that White mortgages that are held on balance sheets are less likely to be in default than those sold to a GSE, consistent with lenders holding less-risky loans on their balance sheets. However, Black and Hispanic mortgages that are held on balance sheets are substantially riskier than observationally similar loans that are securitized, even when controlling for the identity of the lender. If lenders are deliberately extending credit to higher-risk minorities, they appear to be bearing the costs of that risk themselves, suggesting that such behavior is unlikely.

A final remaining possibility is that differences in distress are generated by differential lender treatment of distressed homeowners. For example, lenders may be more likely to foreclose on minority homeowners who default on their mortgages. Results presented in Appendix I that analyze foreclosures and modifications using a sample of defaulted homeowners indicate that this is not the case. Rather, lenders appear to be somewhat more likely to modify defaulted loans for minority homeowners.

These findings contrast with a large body of evidence documenting differential treatment of minorities in other credit markets, such as credit cards (Cohen-Cole 2011; Firestone 2014), auto loans (Butler et al. 2023), and small business lending (e.g., Asiedu et al. 2012; Chernenko and Scharfstein 2022). Many of these studies detect substantial racial gaps in both access to credit and pricing. Although there is ample work documenting racial differences in mortgage markets (e.g., Bartlett et al.

2019; Ambrose et al. 2021; Giacoletti et al. 2022), recent work leveraging increasingly rich datasets suggests that the impact of differential treatment by mortgage lenders may be quantitatively small (Bhutta et al. 2022), consistent with decades of relatively robust anti-discrimination enforcement for which the HMDA data was originally created. Yet even if interventions that demonstrably reduce disparities in access, like stronger anti-discrimination enforcement (Butler et al. 2023) and automated underwriting (Bartlett et al. 2019; Howell et al. 2022), were to eliminate gaps in access to mortgage credit, such interventions would not necessarily mitigate higher rates of minority distress. In the following section, we discuss complementary areas for policy intervention.

6 Policy Implications

Since the 1968 Fair Housing Act, homeownership has been a key tool in the policy effort to combat racial inequalities. Republican and Democratic politicians alike have advocated for policies that increase homeownership among minorities (e.g., Bush 2004; Warren 2019), particularly as part of efforts to narrow the racial wealth gap (White House, 2021). However, because these policies are typically not designed to prevent financial distress among homeowners, nor to help financially distressed homeowners avoid foreclosure, they may be limited in their ability to help minorities build wealth. In this section, we discuss potential policies that can mitigate the racial gap in housing returns, distinguishing between policies that intervene before homeowners become distressed, and those that intervene afterwards.

Preventing Higher Rates of Distress Among Minority Homeowners Our findings indicate that most of minorities' higher rates of distress is due to factors that are upstream of the home purchase decision, like income stability and liquid wealth holdings. Regardless of whether the risk of financial distress created by these factors is observable to lenders, these results imply that addressing upstream disparities, like those in the labor market, is necessary to meaningfully reduce racial disparities in distressed sales and housing returns.

Setting aside upstream policies, our findings also imply that caution must be exercised when designing housing policies that extend credit to high-risk borrowers, because credit expansions can exacerbate racial disparities among homeowners. Consistent with the excess levels of minority default among high-risk borrowers documented in Section 5, racial gaps in housing returns and distressed sales are larger among these groups. This is evident in Appendix Figure A25, which plots average returns for White homeowners by credit score and leverage at origination, along with the Black-White and Hispanic-White gaps in housing returns, residualized within purchase year and county. In addition to illustrating larger gaps among high-risk groups, this figure also shows that annual returns are low for high-risk borrowers in general.

Although these findings suggest that extending credit to high-risk groups (regardless of race) may have limited benefits in terms of supporting wealth accumulation, tightening lending standards would disproportionately exclude minority households from homeownership, given higher levels of observable risk among minorities. This outcome conflicts with social preferences that place a high value on homeownership being broadly accessible, and on avoiding exclusionary policies in light of historical patterns of deliberate racial exclusion (e.g., mortgage redlining).

Since illiquidity is a key driver of excess minority distress, policies that mitigate such illiquidity offer a promising alternative to limiting access to homeownership. One such example is the use of mortgage reserve accounts, which are savings accounts that can be used in the event of a financial shock (Goodman et al., 2023). Such enhanced savings technologies could help to mitigate higher levels of financial distress among illiquid minority homeowners, particularly if those homeowners underestimate their likelihood of becoming distressed.

Intervening Among Already-Distressed Homeowners A second class of policies that can address racial gaps in housing returns are those that mitigate the consequences of financial distress after it occurs. In theory, any such policy may create additional costs due to moral hazard and adverse selection. However, as long as the policy does not entail behavioral responses that differ by race, the benefits of the policy would disproportionately accrue to minorities because of their greater risk of financial distress.

Mortgage modifications are an example of a policy that may be well-suited to preventing distressed sales from destroying minority housing wealth. When homeowners experience temporary cash flow shocks and become unable to make their monthly mortgage payments (e.g., after becoming unemployed), mortgage servicers can restructure the terms of the mortgage.[37] Previous research has found that modifications are useful for avoiding mortgage default (Ganong and Noel, 2020a), a precursor to distressed sales. Moreover, because minority homeowners are more likely to default, they receive a disproportionately large share of modifications.[38] Collins et al. (2015) find similar associations between modification and foreclosure across racial and ethnic groups in a sample of subprime loans originated between 2004 and 2006.

Although these findings are encouraging, they are not sufficient to conclude that modifications are effective for reducing the racial gap in housing returns. First, reductions in default need not increase housing returns. For instance, modifications could merely prolong and postpone the foreclosure process, exacerbating property depreciation and lowering housing returns. Second, it may be possible that modifications are less beneficial for minority homeowners, particularly considering that minorities experience higher levels of financial fragility.

We use our sample of ownership spells to provide evidence that mortgage modifications can causally increase returns for minority homeowners. In Appendix I, we leverage quasi-experimental variation in servicer propensity to modify loans to estimate the causal impacts of modifications. We

[37] Servicers can reduce monthly payments through a combination of principal forbearance, interest rate reductions, and term extensions. Large-scale mortgage restructuring has ample precedent in the modification subsidies provided by the Home Affordable Modification Program which, according to Agarwal et al. (2017), prevented an estimated 600,000 foreclosures between 2009 and 2012 by subsidizing modifications through incentive payments to servicers, borrowers, and investors. More recently, the mandatory payment forbearance mandated by the 2020 CARES Act has had a similarly large impact in preventing defaults (Cherry et al., 2021).

[38] Online Appendix Figure 3 shows that throughout the financial crisis and Great Recession, Black and Hispanic homeowners each accounted for approximately 20% of loan modifications, despite only comprising about 7% and 13% of open mortgages, respectively.

find that modifications cause economically large increases in housing returns for Black, Hispanic, and White homeowners alike. These findings suggest that since minorities are more likely to become financially distressed and thus eligible for modification, an expansion in mortgage modifications could reduce the gap in housing returns.[39]

Mortgage forbearance policy during the COVID-19 pandemic provides further evidence that large-scale mortgage restructuring can reduce the racial gap in housing returns. Appendix Figure A26, Panel A shows that the disproportionately high foreclosure rate among Black households fell sharply at the onset of the pandemic, likely due to a combination of forbearance and enactment of foreclosure moratoria (Cherry et al., 2021). Panel B indicates that the sharp reduction in the Black-White gap in distressed sales was followed by a convergence in housing returns. To quantify this convergence, we estimate Equation 5 separately for properties sold in the nine months before and after March 2020. Appendix Figure A27 shows that the Black-White gaps in unlevered returns and distressed sales fell by half after the onset of the pandemic. The Hispanic-White gap remained comparatively stable, in line with the fact that Hispanic and White foreclosure rates trended identically throughout this period.

Although these results provide evidence that a policy-led expansion of modifications would have disproportionate benefits for minorities, such an expansion of modifications could incur efficiency costs through moral hazard and adverse selection. In principle, such costs could be limited by tying modifications to well-defined life events, like job loss or divorce, an approach reflected in recent proposals to expand mortgage forbearance (Alexandrov et al., 2022). Moreover, our findings that minorities may underestimate their relatively high risk of distress and do not exhibit differences in strategic default suggest that racial differences in these efficiency costs may be limited.

Large-scale mortgage restructuring need not come at a large cost to taxpayers. Since previous research indicates that monthly payment reductions are the component of modifications that are most effective for reducing default, a large-scale expansion may only require implicit government loans to lower payments and lengthen terms for distressed homeowners (Ganong and Noel, 2020a). Expanding modifications could also ameliorate the well-documented negative house price externalities on nearby properties associated with distressed sales (Campbell et al. 2011; Anenberg and Kung 2014), and the corresponding reduction in foreclosures could yield additional economic benefits through residential investment and consumer demand (Mian et al., 2015). In addition, our results suggest that adoption of alternative mortgage contracts with built-in payment flexibility could also benefit distressed minorities and thus narrow racial gaps in housing returns.[40]

[39]Interestingly, mortgage servicers already disproportionately target modifications to Black (and to some extent Hispanic) homeowners despite the absence of any policy incentives to do so. Black homeowners who have defaulted on their mortgages are 3 to 7 percentage points more likely than observationally similar White homeowners to receive a modification. These patterns hold even controlling for the homeowner's neighborhood and servicer, suggesting that servicers internalize part of the larger distressed sale discounts among minority homeowners. Appendix I discusses these results in more detail.

[40]For further discussion of alternative mortgage contract designs, see Piskorski and Seru (2018), Campbell et al. (2020), Greenwald et al. (2021) and Guren et al. (2021). Depending on the extent of information asymmetries, privately-provided insurance contracts could in principle yield similar benefits as alternative mortgage contracts (Shiller and Weiss, 1999).

7 Conclusion

Homeownership has long been a central part of the American dream, and is the primary savings vehicle for middle-class households. Despite enormous changes to the homeownership opportunities available to minorities over the last century—including legal prohibitions on discrimination in housing—minority wealth has remained remarkably low. While policies that increase minority homeownership are widely viewed as helping minorities build wealth, we show that the financial returns to homeownership for minorities are severely limited by high rates of financial distress.

Our findings imply that policies that help homeowners stay in their homes in times of financial distress, or avoid financial distress entirely, are critical complements to policies that help households purchase homes. Because higher rates of illiquidity and income instability underlie higher rates of distressed home sales among minorities, ultimately closing the gap requires addressing labor market disparities. In addition, the existence of racial disparities in liquid wealth implies that redistributive policies like baby bonds and reparations may mitigate the racial gap in housing returns.

There is no reason why financial distress should only inhibit the returns on housing wealth. Other assets that are typically acquired using leverage may yield less net value to minorities in general. Indeed, rates of delinquency on student loans and auto loans are higher for minorities (Appendix Figure A28). Therefore, attempts to improve economic outcomes for minorities by expanding access to leveraged assets may have limited impacts if they ignore the risk of financial distress as well as its root causes.

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Table 1: Summary Statistics

	Mean	SD	p10	p90
Panel A. Baseline Sample of Ownership Spells				
Black Share	0.077			
Hispanic Share	0.137			
Income ($, Thousands)	90	116	34	155
Purchase Year	2007	5	2001	2013
Purchase Price ($, Thousands)	275	1134	98	495
Length of Ownership (Months)	108	61	34	192
Combined Loan-to-Value Ratio at Purchase (%)	87	15	69	100
Share Distressed	0.146			
N = 13,630,629 Ownership Spells				
Panel B. Credit Bureau and Servicing Records				
Term (Months)	338	62	180	360
Interest Rate (%)	5.21	1.32	3.62	6.75
Credit Score at Origination	718	66	632	795
Debt-to-Income Ratio	35	13	18	49
Mortgage 30+ Days Delinquent	0.073			
Mortgage 90+ Days Delinquent	0.044			
Any Non-Mortgage Loan 30+ Days Delinquent	0.186			
Any Non-Mortgage Loan 90+ Days Delinquent	0.171			
N = 74,254,097 Loan-Years				
Panel C. Adjustable Rate Mortgage Sample				
Black Share	0.106			
Hispanic Share	0.208			
Reset Year	2010	3	2007	2014
Years Since Origination	4	2	2	5
Change in Monthly Payment (%)	1.4	20.5	–24.0	28.4
N = 199,544 ARM Loans				

Notes: This table presents summary statistics for our main analysis samples. Panel A presents statistics at the level of the ownership spell for owner-occupied properties in our baseline sample of ownership spells. Panel B presents statistics at the loan-year level for a panel of homeowners with outcomes linked to CRISM mortgage servicing and credit bureau records. Outcomes in the yearly panel are measured as of June. Panel C presents statistics at the loan level for a sample of adjustable rate mortgages (ARMs) that are observed in the mortgage servicing records. The sample is restricted to first-lien ARMs held by Black, Hispanic, and White owner-occupants.

Table 2: Annualized Housing Returns by Race and Ethnicity

	Baseline	Regular Sales Only	Distressed Sales Only	Cash Adjustment	Distress Adjustment
	(1)	(2)	(3)	(4)	(5)
Panel A. Unlevered Returns (%, Annualized)					
Black	0.52 (9.32)	3.82 (5.47)	–11.06 (10.68)	1.23 (8.74)	0.19 (9.16)
Hispanic	0.58 (11.87)	5.72 (7.08)	–13.51 (10.89)	1.78 (11.15)	0.35 (11.71)
White	2.84 (7.11)	4.21 (5.53)	–7.45 (9.04)	3.16 (6.80)	2.65 (7.03)
Overall	2.36 (8.16)	4.36 (5.75)	–9.39 (10.09)	2.82 (7.71)	2.14 (8.06)
Black-White	–2.32	–0.39	–3.61	–1.94	–2.46
Hispanic-White	–2.26	1.51	–6.06	–1.39	–2.30
Panel B. Levered Returns (%, Annualized)					
Black	1.57 (47.11)	15.34 (39.06)	–46.54 (40.81)	2.87 (41.90)	–0.18 (47.03)
Hispanic	–3.04 (54.29)	18.44 (42.60)	–61.60 (36.73)	–0.34 (47.90)	–3.96 (54.07)
White	6.59 (40.62)	13.50 (34.81)	–45.13 (43.51)	6.87 (35.62)	5.91 (40.53)
Overall	4.88 (43.40)	14.21 (36.16)	–49.42 (42.19)	5.58 (38.11)	4.09 (43.30)
Black-White	–5.02	1.83	–1.40	–4.01	–6.09
Hispanic-White	–9.63	4.94	–16.47	–7.21	–9.87
Panel C. Distressed Sale (%)					
Black	22.2			17.4	31.2
Hispanic	26.7			20.5	32.2
White	11.7			9.0	16.6
Overall	14.6			11.2	19.9

Notes: This table presents estimates of means and standard deviations of housing returns and rates of distressed home sales by race and ethnicity. Panel A presents statistics for annual unlevered returns. Panel B presents statistics for annual levered returns. Panel C presents rates of distressed home sales. Column 1 presents estimates from baseline sample of ownership spells described in Section 2. Column 2 presents estimates for the subsample of properties not sold in a distressed sale, including properties not sold within the sample period. Column 3 present estimates for the subsample of properties sold in a distressed sale. Column 4 adjusts for cash purchases. Column 5 adjusts for bias from not observing future distressed sales among properties that remain unsold by the end of our sample window. See Section 3 for more details.

Table 3: Contribution of Returns Gap to Housing Wealth Disparities at Retirement Age

	PSID	**Model**	**Counterfactuals**			
	(1)	(2)	(3)	(4)	(5)	(6)
Black Wealth at Retirement	$81,713	$89,872	$156,658	$92,140	$137,933	$179,241
White-Black Difference	$169,389	$182,771	$115,984	$180,502	$134,709	$93,402
% Reduction in Gap	-	0%	36.54%	1.24%	26.30%	48.90%
Equal Returns	-		X			X
Equal Transition Rates	-			X	X	X
Equal Purchase Values	-				X	

Notes: This table presents estimates from our wealth accumulation equation (Equation 4). This equation allows us to compute the average household's housing wealth at retirement age by race, along with actual and counterfactual differences between Black and white households. These estimates illustrate the contribution of the gap in housing returns to observed racial wealth disparities at retirement. Column 1 presents estimates from households aged 63-67 in the PSID, including non-homeowners with no housing wealth. Column 2 presents baseline estimates for households at age 65 from the wealth accumulation equation, incorporating estimates of the racial gap in housing returns presented in Section 3 and purchase amounts and rates of first-time home purchases from the PSID. Columns 3 through 6 present estimates of counterfactual wealth disparities by equalizing annual housing returns, rates of first-time home purchases, and home values at purchase by race.

Table 4: Liquidity, Income Stability, and Racial Disparities in Mortgage Delinquency

	(1)	(2)	(3)	(4)	(5)	(6)
Black	5.87***	4.10***	3.87***	4.76***	3.49***	3.37***
	(0.45)	(0.45)	(0.45)	(0.45)	(0.45)	(0.44)
Hispanic	3.68***	2.17***	1.83***	2.33***	1.36*	0.69
	(0.56)	(0.54)	(0.54)	(0.54)	(0.53)	(0.53)
Liquidity and Layoff		X	X		X	X
Income			X			X
Additional Demographics				X	X	X
Observations	84,800	84,800	84,800	84,800	84,800	84,800

Notes: This table presents regressions of an indicator that a household has been delinquent on its mortgage in the past 12 months on different sets of covariates. Results in this table illustrate that a large share of the racial/ethnic differences in mortgage delinquency can be explained by differences in liquidity and income stability. *Liquid and Layoff Assets* includes the log of 1 plus the dollar amount of liquid assets, an indicator that the household has experienced unemployment in the last 12 months, and their interaction. *Income* includes log household income and its interaction with unemployment. *Additional Demographics* includes the number of household members, and indicator that the household is married, and the household's current loan-to-value ratio. Data come from a sample of homeowners in the Survey of Income and Program Participation (1992-2017) described in Section 5. Race/ethnicity is assigned according to the head of household. All specifications include state-by-year fixed effects. Standard errors are clustered at the household level and reported in parentheses. Table 16 in the Online Appendix reports coefficients for all regressors. *** p<0.001, ** p<0.01, * p<0.05

Table 5: Mortgage Default Responses to Monthly Mortgage Payment Shocks

	(1)	(2)	(3)	(4)	(5)
Panel A. Monthly Payment (%)					
Payment Shock	96.68	96.86	96.99	96.70	96.63
	(0.127)	(0.140)	(0.147)	(0.157)	(0.231)
Payment Shock × Black	–1.806	–1.940	–2.333	–2.695	–1.611
	(0.512)	(0.526)	(0.551)	(0.563)	(0.717)
Payment Shock × Hispanic	–0.197	–0.174	–0.617	–0.789	–1.052
	(0.313)	(0.352)	(0.360)	(0.377)	(0.491)
Panel B. 90-Day Default (%)					
Payment Shock	31.24	30.39	27.92	23.24	27.13
	(0.455)	(0.521)	(0.541)	(0.573)	(0.911)
Payment Shock × Black	13.73	14.07	12.40	11.01	8.199
	(1.714)	(1.877)	(1.914)	(2.028)	(3.011)
Payment Shock × Hispanic	41.32	40.14	37.74	37.98	38.59
	(1.190)	(1.288)	(1.330)	(1.415)	(2.054)
Number of Loans	199,543	185,714	172,413	155,679	104,950
Controls					
Origination Year	X	X	X	X	X
County		X	X	X	X
Payment Size		X	X	X	X
Demographics, Leverage			X	X	X
Credit Score				X	X
Tract					X

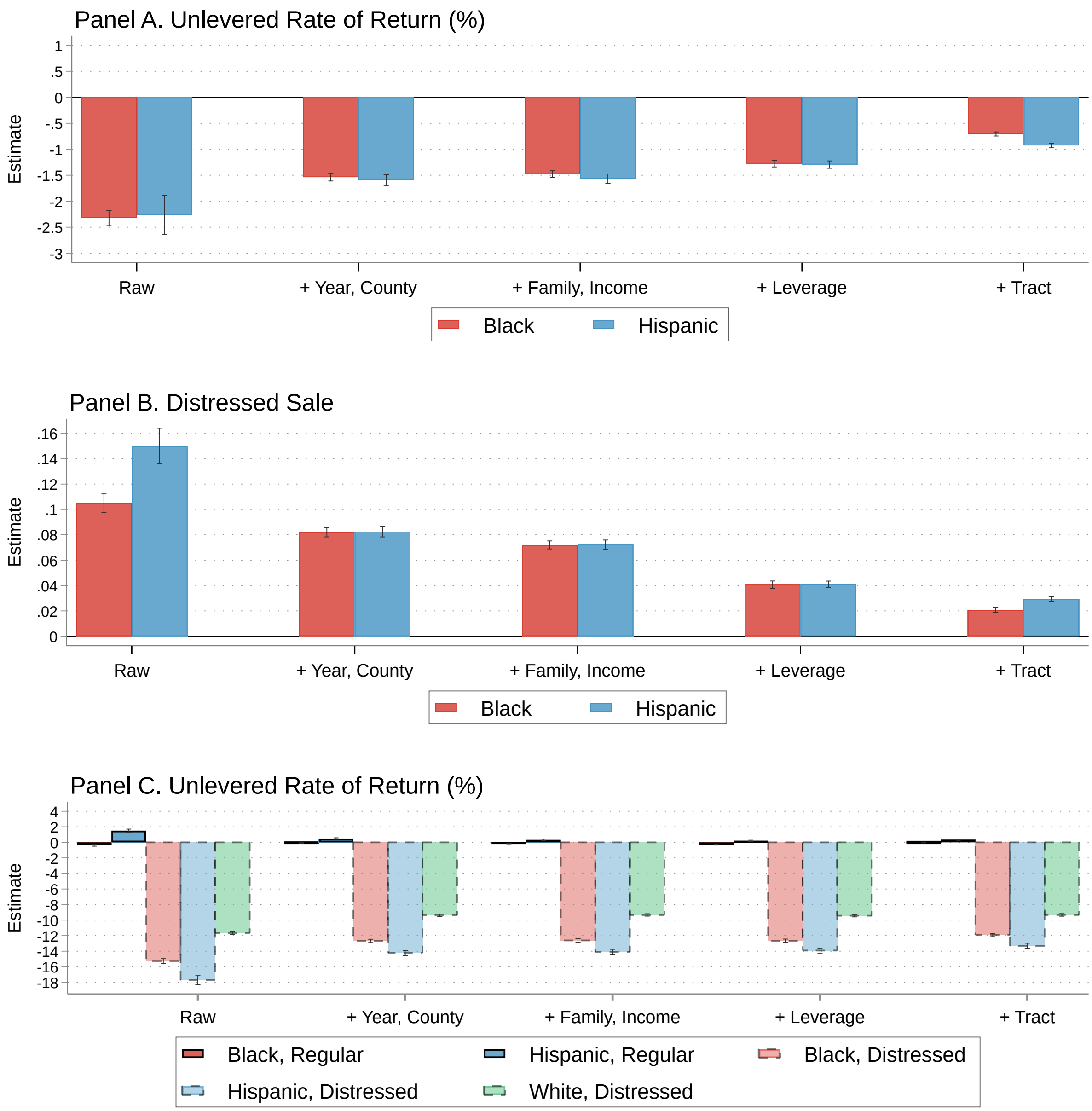
Notes: This table presents event study estimates of the impacts of monthly payment changes on normalized monthly principal and interest payments (Panel A) and an indicator that the mortgage is 90 or more days past due (Panel B). Monthly payments are normalized by the payment at event month -12. Coefficients correspond to estimated impacts at event month 12 (relative to event month -1), and are multiplied by 100 for legibility. Coefficients can be interpreted as impacts of a 100 percent increase in monthly payments. Each column presents a separate specification, corresponding to Equation 6. All specifications include loan and event month-by-origination year fixed effects. Additional controls are interacted with event time and origination year and are defined as follows: *County* and *Payment Size* denote county fixed effects interacted with deciles of principal and interest payment at event month -12; *Demographics, Leverage* denotes income decile, 1-point original LTV bins, and co-applicant status-by-gender; *Credit Score* denotes 10-point credit score bins; *Tract* denotes Census tracts. Data from panel of homeowners with adjustable rate mortgages described in Section 5. Standard errors are reported in parentheses and clustered at the loan level.

Table 6: Racial Differences in Attitudes and Expectations

	Strategic Default Attitudes (1)	Past House Price Growth (2)	Expected House Price Growth (3)	Actual House Price Growth (4)	Past Financial Crisis (5)	Expected Financial Crisis (6)	Actual 90-Day Default Rate (7)	Past Income Growth (8)	Expected Income Growth (9)
Black	0.0464	–2.454	7.350**	–1.143**	2.841**	–1.875	2.510***	1.398	13.61***
	(0.651)	(1.722)	(2.446)	(0.427)	(0.886)	(1.168)	(0.718)	(1.570)	(1.461)
	[0.002]	[–0.041]	[0.103]	[–0.068]	[0.122]	[–0.045]	[0.169]	[0.024]	[0.286]
Hispanic	3.251***	11.23***	10.74***	2.018***	0.939	–0.207	–0.265	3.245*	4.624***
	(0.763)	(1.461)	(1.881)	(0.418)	(0.678)	(1.069)	(0.466)	(1.460)	(1.261)
	[0.136]	[0.187]	[0.151]	[0.119]	[0.040]	[–0.005]	[–0.018]	[0.056]	[0.097]
Outcome Mean	6.103	30.53	–4.884	23.46	5.804	–81.86	2.259	12.82	19.11
Outcome SD	23.94	60.20	71.36	16.93	23.38	41.69	14.86	57.85	47.51
N	24777	24777	24777	19575	24777	24777	24777	24777	24777
Baseline Controls	X	X	X	X	X	X	X	X	X
Experience Control			X	X		X	X		X

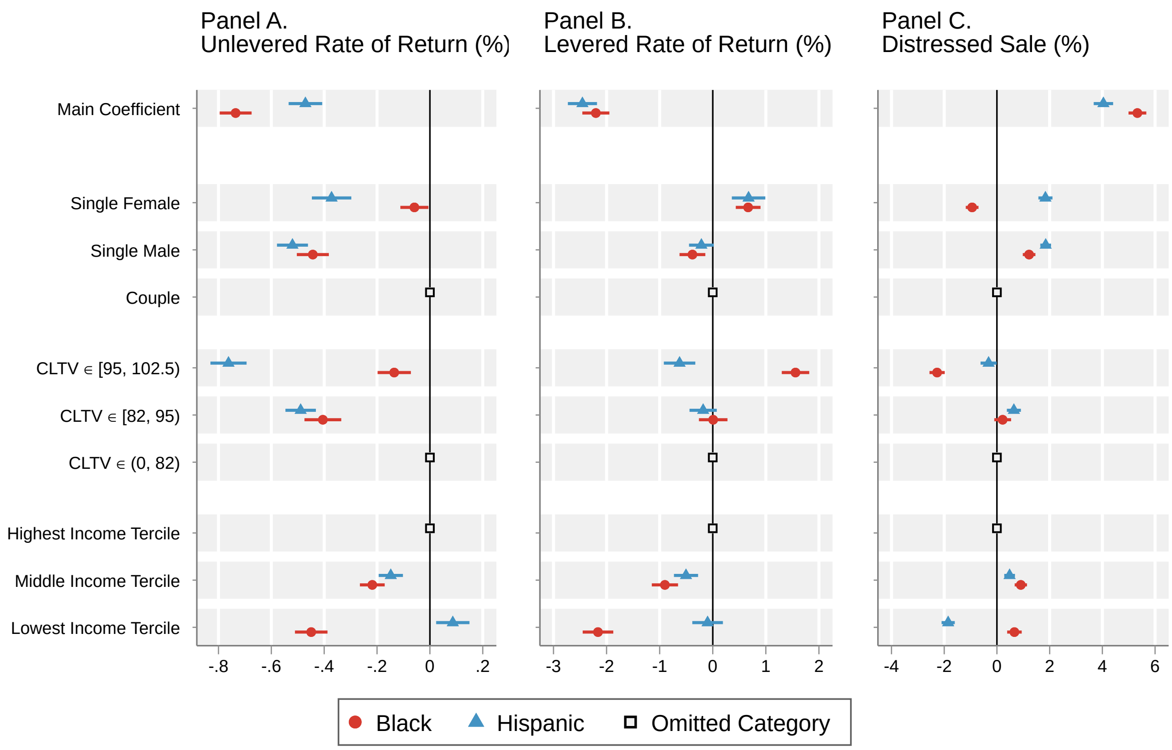
Notes: This table presents responses from sample of homeowners in the National Survey of Mortgage Originations (NSMO). Each column pertains to a regression of a given outcome on race indicators and a vector of controls. Robust standard errors reported in parentheses. Point estimates scaled to outcome standard deviation reported in brackets. *Baseline Controls* denotes survey wave fixed effects, fixed effects for the six bins of income reported in the survey, a quadratic in credit score, a quadratic in age, an indicator that the respondent is married, and an indicator that a respondent is female. *Experience Controls* denotes controls for past experience corresponding to a given outcome variable (e.g., controlling for past house price growth when estimting racial differences in expectations about future house price expectations). Each column corresponds to results for a different survey question. *Strategic Default Attitudes* pertains to the question, Do you agree or disagree with the following statements? It is okay to default or stop making mortgage payments if it is in the borrower's financial interest. *Past House Price Growth* pertains to the question, In the last couple years, how have [house prices] changed in the neighborhood where this property is located? *Expected House Price Growth* pertains to the question, What do you think will happen to the prices of homes in this neighborhood over the next couple of years? *Actual House Price Growth* is computed using administrative measures of mark-to-market LTV at origination divided by mark-to-market LTV after two years. *Past Income Growth* pertains to the question, In the last couple years, how has [household income] changed for you (and your spouse/partner)? *Expected Income Growth* pertains to the question, In the next couple of years, how do you expect the following to change for you (and your spouse/partner)? *Past Financial Crisis* pertains to the question, In the last couple of years, has [a personal financial crisis] happened to you (or your spouse/partner)? *Expected Financial Crisis* pertains to the question, How likely is it that in the next couple of years you (or your spouse/partner) will face [some other personal financial crisis]? *Actual 90-Day Default Rate* is computed using administrative measures as an indicator that the mortgage became 90 days past due at least once in the first two years after origination. Appendix Table A11 presents more information on outcome variables. All regressions use analysis weights. See Appendix Section C for additional information about the NSMO sample. *** p<0.001, ** p<0.01, * p<0.05

Figure 1: Racial Gap in Annualized Housing Returns



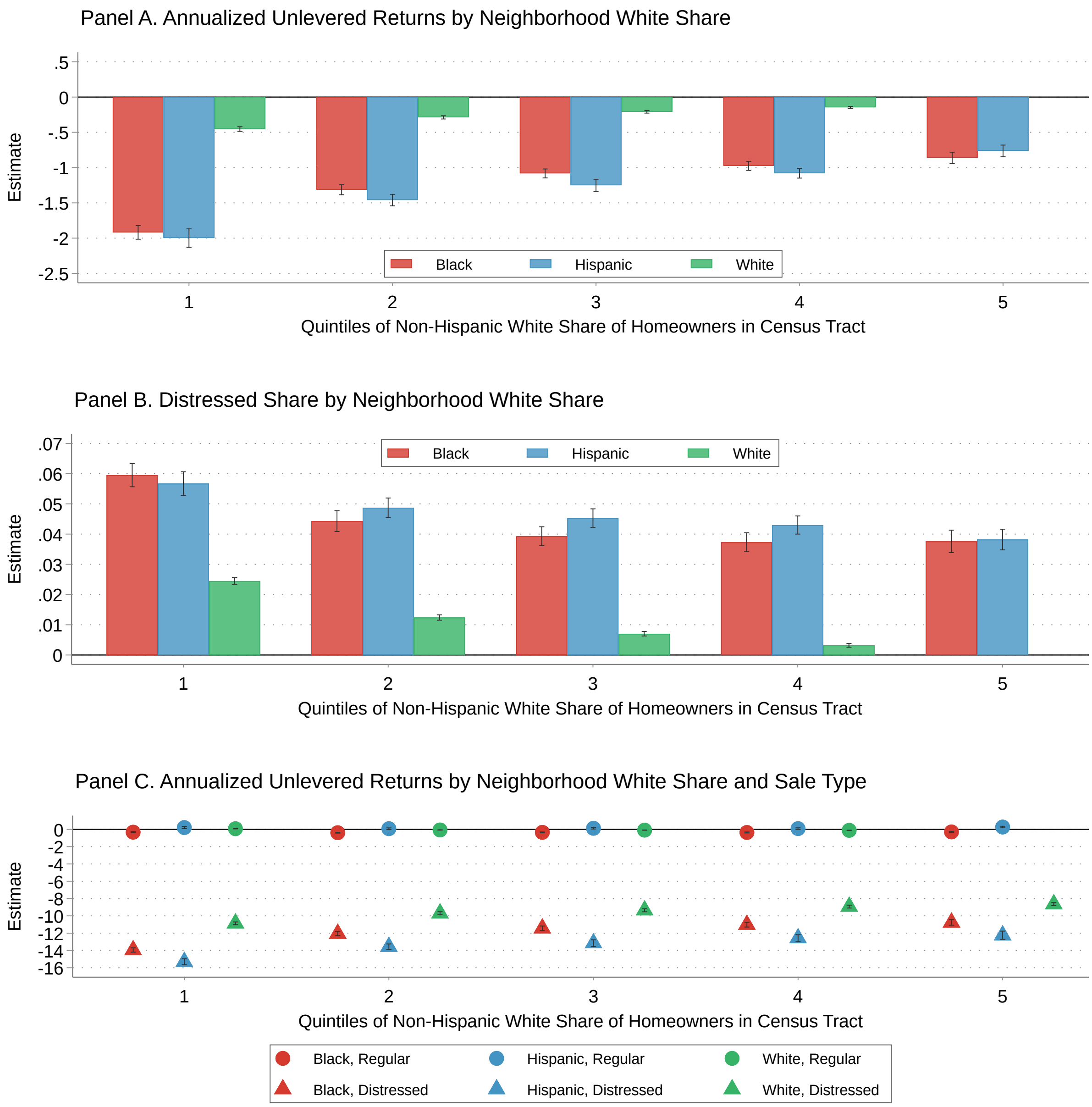
Notes: This figure presents estimates of racial gaps in housing returns and distressed sales. In Panels A and B, each pair of bars corresponds to a separate regression and indicates estimated coefficients of Black and Hispanic indicators from Equation 5 using a particular set of fixed effects. In Panel C, each set of bars corresponds to coefficients of race indicators interacted with an indicator that a property is sold in a distressed sale. *Raw* denotes estimates without controls. *+ Year, County* adds in purchase year-by-county fixed effects. *+ Family, Income* adds fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, and fixed effects that interact county, purchase year, and deciles of income at home purchase. *Leverage* adds fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. *Tract* adds fixed effects that interact purchase year and Census tract. The outcome in Panels A and C is the annualized unlevered rate of return (Equation 1), and the outcome in Panel B is an indicator that a homeowner experiences a distressed sale. Data are from baseline sample of ownership spells described in Section 2. Standard errors are clustered within purchase year, sale year, and county cells. Table 1 in the Online Appendix presents numerical values and additional statistics.

Figure 2: Heterogeneous Racial Gaps in Housing Returns and Distressed Sales



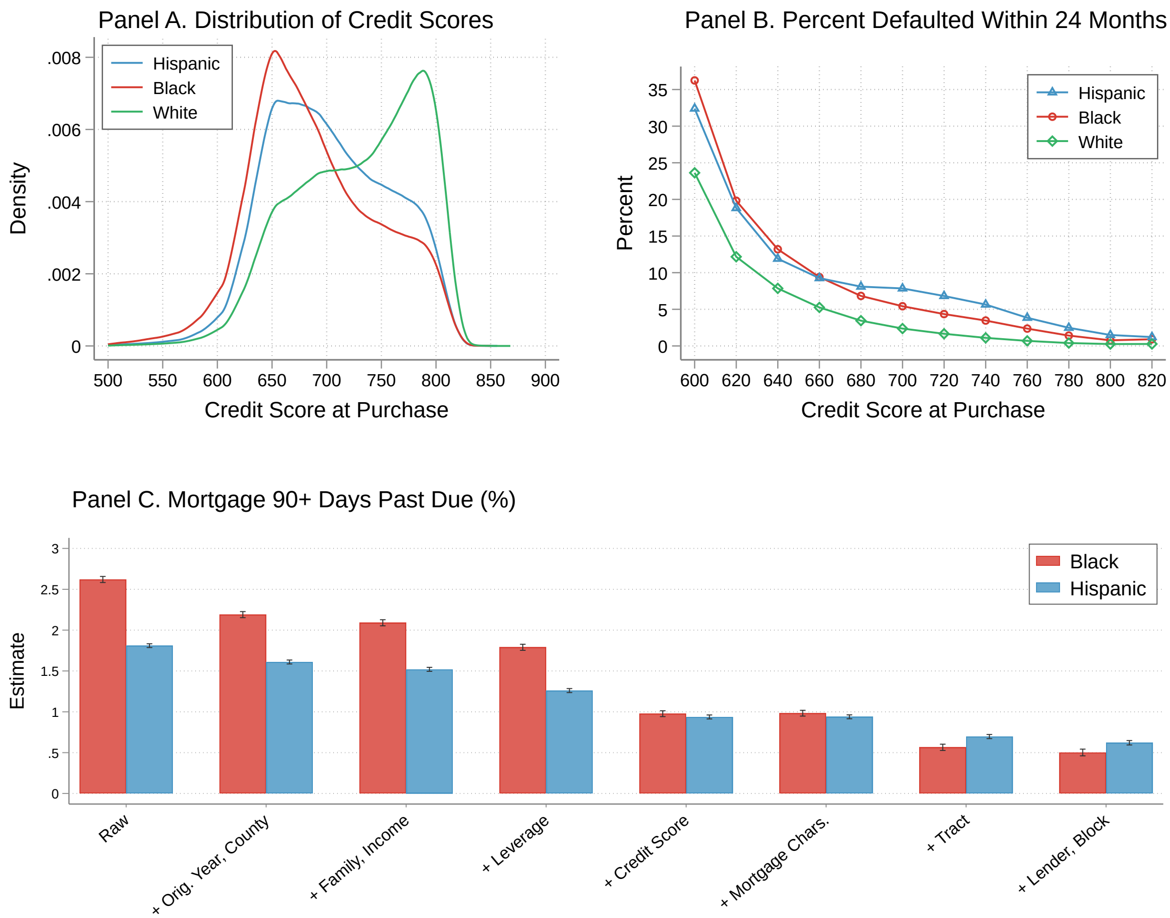
Notes: This figure documents heterogeneity in the racial gap in housing returns for annualized unlevered returns (Panel A), annualized levered returns (Panel B) and distressed sales (Panel C) across family structure, leverage at home purchase, and income at home purchase. Each panel presents estimates from a separate regression which includes fixed effects that interact purchase year, county, bins of current loan-to-value at purchase, bins of family structure, and terciles of income within purchase year and county (Equation 5). Points denote estimated coefficients of race/ethnicity indicators interacted with homeowner characteristics. *Couple* is defined as homeowners with a co-applicant in the HMDA data. Data are from baseline sample of ownership spells described in Section 2. Standard errors are clustered within purchase year, sale year, and county cells. Table 2 in the Online Appendix presents numerical values and additional statistics.

Figure 3: Racial Gaps by Neighborhood Demographics and Sale Type



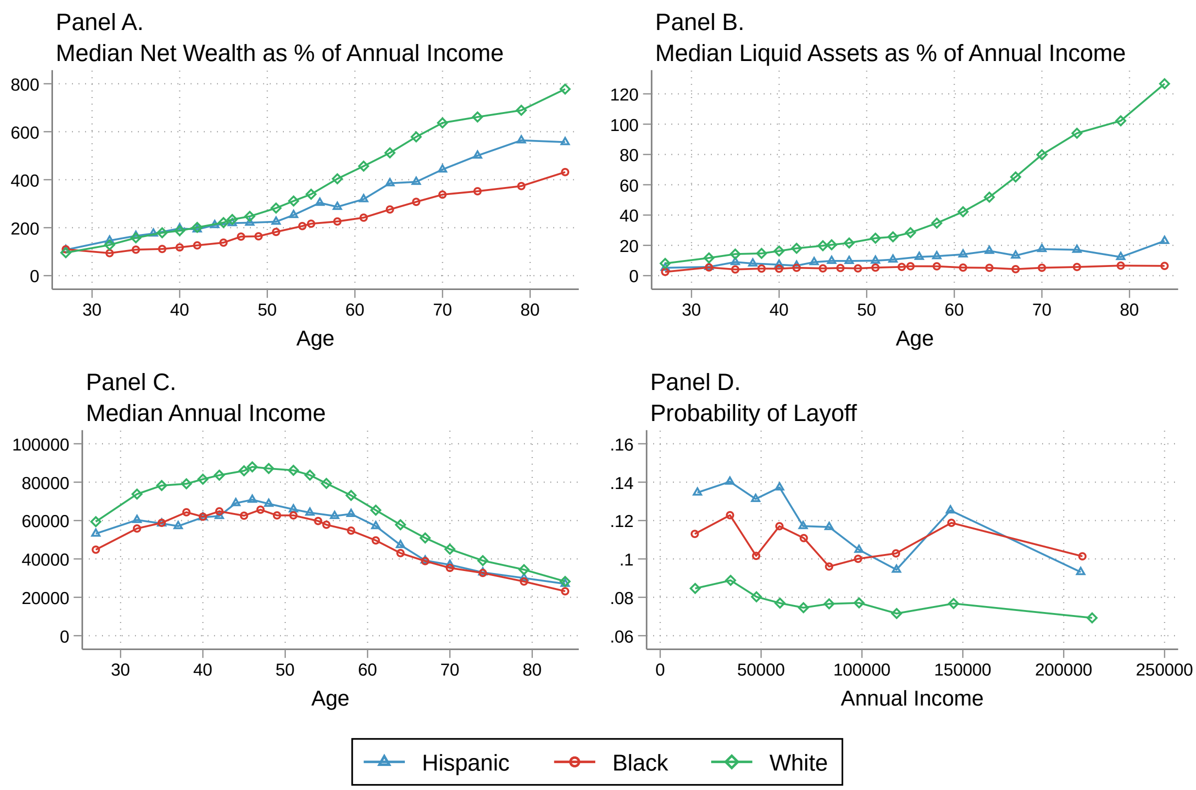
Notes: This figure presents estimates of racial gaps in annualized unlevered housing returns (Panels A and C) and distressed sales (Panel B) from regression specifications that compare homeowners with similar incomes, family structures, and leverage, living in the same county and buying their homes in the same year (Equation 5). Panel A presents regression coefficients that interact individual race/ethnicity with quintiles of the non-Hispanic White share of homeowners in the individual's Census tract. The omitted category is non-Hispanic White homeowners in neighborhoods with the highest non-Hispanic White share. Panel C presents regression coefficients that interact homeowner race/ethnicity with quintiles of the White share and homeowner's sale type (regular vs. distressed). Regular sales include properties that were not sold within our sample period. The omitted category in Panel C is White homeowners in neighborhoods with the highest White share whose property sale is not distressed. In all panels, quintiles are assigned within each county, such that higher quintiles contain neighborhoods in each county with the highest share of White homeowners. Data are from baseline sample of ownership spells described in Section 2. Standard errors are clustered within purchase year, sale year, and county cells. Table 3 in the Online Appendix presents numerical values and additional statistics.

Figure 4: Racial Differences in Observable Default Risk and Realized Default



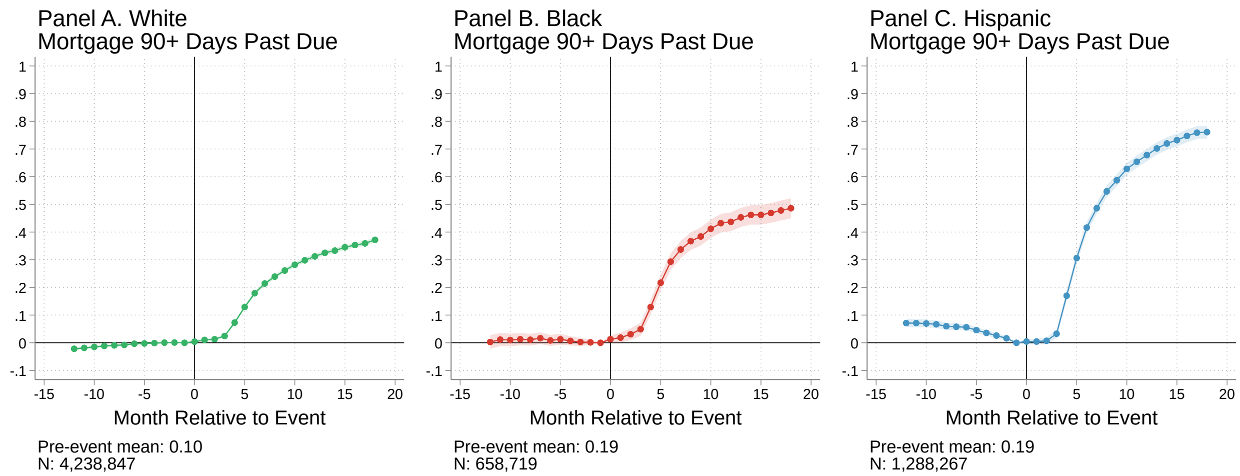
Notes: This figure presents the distribution of credit scores, 90-day mortgage default, and racial differences in default controlling for a range of observable homeowner characteristics (Equation 5). Panel A plots the distribution of credit scores at home purchase by race/ethnicity in kernel density form. Panel B plots the percent of homeowners who become 90 days past due within the first 24 months after home purchase as a function of credit score at purchase. Panel C decomposes racial differences in 90-day mortgage default into components that correspond to household, loan, and location characteristics. Each bar corresponds to the coefficient on a race/ethnicity indicator. Each pair of bars correspond to a separate regression with a particular set of covariates. *Raw* denotes a regression of the outcome on race/ethnicity indicators and current year fixed effects. *Orig. Year, County* adds fixed effects that interact current year, origination year, and county. *Family, Income* adds fixed effects that interact gender, the presence of a co-applicant on the mortgage, and current year, and fixed effects that interact deciles of income and current year. *Leverage* adds fixed effects that interact 1 percentage point bins of original loan-to-value and current year. *Credit Score* adds fixed effects that interact 10 point credit score bins and current year. *Mortgage Chars.* controls for a spline in current loan-to-value, log monthly payment, log interest rate, an indicators for interest-only loan, refinance, adjustable rate mortgage, and GSE loan, and adds fixed effects for mortgage term, deciles of original loan value, and deciles of debt-to-income. *Tract* adds Census tract fixed effects. *Lender, Block* adds lender and Census block fixed effects. Standard errors clustered at the loan level. Data are from a panel of homeowners with linked credit bureau and mortgage servicing records described in Section 5. Data in Panels A and B restrict to purchase mortgages. Table 4 in the Online Appendix presents numerical values and additional statistics.

Figure 5: Disparities in Wealth, Liquidity, and Income



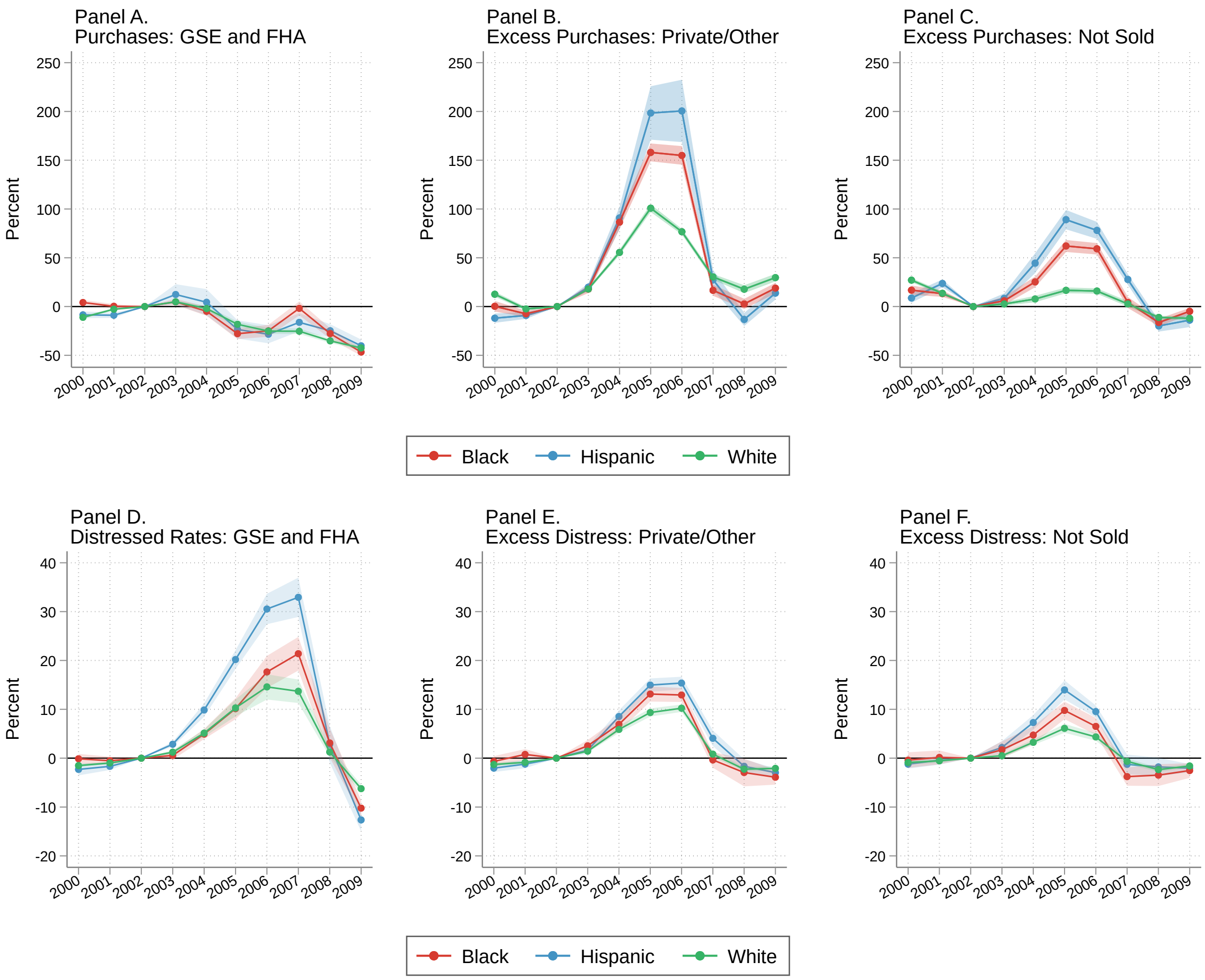
Notes: This figure presents binned scatterplots that illustrate racial disparities in wealth, liquidity, and income among homeowners. Panel A plots median net wealth as a percentage of annual income as a function of age. Panel B plots median liquid wealth as a percentage of annual income as a function of age. Panel C plots median annual income as a function of age. Panel D plots the share of households who have experienced an unemployment spell in the previous 12 months as a function of income in the prior year, restricting to households aged 25 to 65 who were employed homeowners in the prior year. Data come from sample of homeowners in the Survey of Income and Program Participation (1990-2017) described in Section 5. Race/ethnicity and age are assigned according to the head of household. Dollar values are adjusted to 2016 levels. Tables 5 and 6 in the Online Appendix present numerical values and additional statistics.

Figure 6: Effects of Liquidity Shock by Race/Ethnicity in 90-Day Default



Notes: This figure depicts the time path of monthly 90-day mortgage default rates around a change in monthly mortgage payments from adjustable rate mortgage resets that occurs in event month $t = 0$. Default rates are measured as an indicator that the homeowner's primary mortgage is 90 or more days past due. Each panel corresponds to a different racial group. All panels present event study coefficients from Equation 6, which interacts origination year with event time indicators. Event time indicators are interacted with the percentage change in the total monthly payment. The shaded region depicts 95 percent confidence intervals, with standard errors clustered at the loan level. Event coefficients are normalized to zero one month before the payment change ($t = -3$). Data are from panel of homeowners with linked credit bureau and mortgage servicing records with adjustable rate mortgages who experience a rate reset, described in Section 5. Table 7 in the Online Appendix present numerical values and additional statistics.

Figure 7: Differential Rates of Distressed Sales Following Credit Supply Shocks



Notes: This figure depicts the time path of home purchases and rates of distressed sale by race/ethnicity, mortgage investor channel, and purchase year, relative to homes purchased in 2002. The outcome in Panels A, B, and C is the number of home purchased in a given county and year, normalized to mean 2000-2003 levels. The outcome in Panels D, E, and F is the percent of properties (ownership spells) purchased in a given county and year that are later sold in a distressed sale. Panels A and D present estimates of the time path by race/ethnicity among purchases made with Fannie Mae, Freddie Mac, or the Federal Housing Adminsitration loans. Panels B and E present estimates of the relative difference between loans that were not originated through a GSE or the FHA, and that were sold within the HMDA calendar year, within a given race and county. For example, Panel E indicates that in 2005, homes purchased by Hispanic homeowners with Private/Other mortgages were about 15 percentage points more likely to end in a distressed sale relative to homes purchased by Hispanic homeowners with GSE or FHA mortgages. Panels C and F present estimates of the relative difference between purchases made with loans that were not sold within the HMDA calendar year (i.e., portfolio loans) and GSE and FHA loans, within race and county. Data for Panels A, B, and C come from HMDA mortgage origination records aggregated to the county level. Data for Panels D, E, and F come from baseline sample of ownership spells described in Section 2 aggregated to the county level. Counties are weighted by the mean number of purchases between 2000 and 2003. Standard errors are clustered within county. Table 8 in the Online Appendix present numerical values and additional statistics.

A Appendix Tables (For Online Publication)

Table A1: Comparison with External Representative Sample

	Baseline Sample (1)	AHS 2015 (2)	AHS 2015 (3)
Hispanic (%)	11.40	9.88	11.21
Non-Hispanic Black (%)	7.66	8.54	8.31
Single-Family Home (%)	84.85	90.23	88.93
Current Value			
Mean	305,113	273,585	280,952
SD	229,794	232,774	232,339
p10	107,954	79,117	84,673
p90	579,249	548,102	553,100
Tenure (Years)*			
Mean	6.89	15.43	7.38
SD	4.49	12.54	4.40
p10	1.00	2.00	2.00
p90	13.00	34.00	14.00
Down Payment (%)‡			
Mean	14.58	17.72	16.86
SD	18.45	20.72	20.51
p10	0.00	0.00	0.00
p90	30.50	30.50	30.50
Census Division (%)			
New England	6.89	5.50	4.80
Middle Atlantic	13.19	13.50	11.94
East North Central	15.31	17.24	16.00
West North Central	3.76	7.90	8.18
South Atlantic	24.73	18.42	19.49
East South Central	3.93	5.31	5.61
West South Central	2.48	10.45	11.49
Mountain	7.68	7.27	8.21
Pacific	22.03	14.41	14.29
N	9,533,818	24,869	15,140
Moved Since 2000			X

Notes: This table compares characteristics of our baseline analysis sample to those of a representative external sample from the 2015 American Housing Survey (AHS). Column 1 presents characteristics of our baseline sample, which we restrict to properties that were purchased before 2015 and remained unsold as of 2015 in order to maximize comparability with the AHS sample. Column 2 presents statistics for the sample of homeowners in AHS. Column 3 presents statistics for homeowners in AHS who have moved into their current properties since 2000. See Appendix C.5 for more details on the AHS sample.

* In the AHS, tenure length is measured by year the householder moved into the property. In the baseline sample, tenure is measured using the year the property was purchased.

‡Down payment is measured as a percentage of purchase price and is discretized into eight bins. To compute a numerical value, we take the midpoint of each bin, and apply the same procedure to construct an analogous variable in the baseline sample.

Table A2: Geographical Coverage Relative to External Representative Sample

CBSA (%)	Baseline Sample (1)	AHS 2015 (2)	AHS 2015 (3)
Atlanta-Sandy Springs-Roswell, GA	1.84	1.93	2.26
Boston-Cambridge-Newton, MA-NH	2.45	1.74	1.52
Chicago-Naperville-Elgin, IL-IN-WI	5.00	3.29	3.09
Dallas-Fort Worth-Arlington, TX	0.07	2.07	2.43
Detroit-Warren-Dearborn, MI	1.58	1.61	1.40
Houston-The Woodlands-Sugar Land, TX	0.00	1.85	2.19
Los Angeles-Long Beach-Anaheim, CA	4.64	2.96	2.56
Miami-Fort Lauderdale-West Palm Beach, FL	2.81	1.56	1.59
New York-Newark-Jersey City, NY-NJ-PA	5.57	5.40	4.80
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	3.38	2.26	2.06
Phoenix-Mesa-Scottsdale, AZ	2.36	1.38	1.66
Riverside-San Bernardino-Ontario, CA	2.14	1.11	1.24
San Francisco-Oakland-Hayward, CA	1.85	1.40	1.31
Seattle-Tacoma-Bellevue, WA	2.52	1.29	1.31
Washington-Arlington-Alexandria, DC-VA-MD-WV	3.79	2.25	2.39
All other metropolitan areas	56.76	53.58	54.25
Not in a metropolitan area	3.22	14.31	13.93
N	9,533,818	24,869	15,140
Moved Since 2000			X

Notes: This table compares the distribution of our baseline analysis sample distribution across CBSAs to that of a representative external sample from the 2015 American Housing Survey (AHS). Column 1 presents characteristics of our baseline sample, which we restrict to properties that were purchased before 2015 and remained unsold as of 2015 in order to maximize comparability with the AHS sample. Column 2 presents statistics for the sample of homeowners in AHS. Column 3 presents statistics for homeowners in AHS who have moved into their current properties since 2000. See Appendix C.5 for more details on the AHS sample.

Table A3: Cross-Tabulation of Self-Reported and Imputed Race

	Self-Reported Race						
	Hispanic	Black	White	Asian	Other	Missing	N
Race Imputed from Name							
Hispanic	72.7	1.5	14.4	3.4	0.6	7.4	2,102,238
Black	3.3	54.2	28.6	2.3	1.2	10.5	1,072,438
White	2.3	3.5	82.2	2.0	0.8	9.2	12,161,201
Asian	3.0	1.5	15.2	68.3	2.1	10.0	945,778
Other	6.3	8.1	37.3	32.8	5.1	10.5	39,292
Any Non-Missing	11.5	6.4	65.9	6.1	0.9	9.1	16,320,947
Missing	10.4	6.8	65.0	7.6	0.8	9.4	199,232

Notes: This table compares the sample distribution of self-reported and imputed race. Each number corresponds to the percent of homeowners self-reporting a given race/ethniciy, conditional on being categorized to a given race/ethnicity based on their name and Census block. Percentages are reported separately by race/ethnicity imputed from name. Data are from sample of property records linked to HMDA data described in Section 2.

Table A4: Unlevered Returns and Distressed Sales By Self-Reported and Imputed Race

	(1)	(2)	(3)	(4)	(5)
Panel A. Unlevered Returns (%, Annualized)					
Hispanic	0.39 (11.95)	0.48 (11.90)	0.62 (11.64)	0.84 (12.83)	2.60 (12.99)
Black	0.38 (9.34)	0.68 (10.08)	0.65 (10.30)	0.69 (11.93)	2.53 (13.39)
White	2.70 (7.11)	2.66 (7.13)	2.80 (7.41)	2.90 (8.75)	4.14 (10.32)
Asian	2.62 (7.77)	2.76 (7.30)	2.61 (7.32)	2.86 (8.48)	4.01 (9.16)
Overall	2.24 (8.17)	2.25 (8.18)	2.39 (8.24)	2.48 (9.62)	3.86 (10.84)
Missing Race	2.40 (8.24)	2.66 (8.07)	2.88 (8.05)	2.58 (9.65)	3.78 (10.91)
B-W Gap	–2.31	–1.99	–2.15	–2.21	–1.61
H-W Gap	–2.31	–2.18	–2.18	–2.06	–1.54
Panel B. Distressed Sales (%)					
Hispanic	27.19	27.08	26.96	26.84	22.06
Black	22.40	22.71	23.61	23.92	20.05
White	11.97	12.21	12.78	12.54	9.58
Asian	13.31	11.86	11.99	12.74	9.94
Overall	14.78	14.82	15.06	15.13	11.69
Missing Race	15.03	13.00	12.95	13.72	9.93
B-W Gap	10.43	10.49	10.83	11.38	10.47
H-W Gap	15.22	14.87	14.18	14.30	12.48
N	14,111,108	15,354,155	1,415,112	25,794,573	40,570,537
Sample	Baseline (HMDA)	Baseline (HMDA)	Missing HMDA Race	All Mortgaged Purchases	All Purchases (Incl. Cash)
Purchase Years	2000–2014	2000–2014	2000–2014	2000–2014	1990–2014
Race Assignment	Self-Report	Imputed	Imputed	Imputed	Imputed

Notes: This table presents means and standard deviations of annualized unlevered returns by self-reported and imputed race/ethnicity. Self-reported race is derived from HMDA records, while imputed race is imputed using homeowner names and Census block. *Baseline* denotes baseline sample of ownership spells used for main analysis. *Missing HMDA Race* denotes baseline sample restricted to observations that are merged to HMDA but where self-reported race/ethnicity is missing. *All Mortgaged Purchases* denotes sample of purchases made with a mortgage but not restricted to merge with HMDA data. *All Purchases (Incl. Cash)* denotes sample of all purchases in the property data, not restricted to merge with the HMDA data. Data come from property records and HMDA records described in Section 2.

Table A5: Returns and Distressed Sales with Alternative Adjustment for Censoring

	Mean (SD) (1)	Mean (SD) (2)
Panel A. Unlevered Returns (%, Annualized)		
Black	0.41 (9.24)	1.28 (8.79)
Hispanic	0.72 (11.48)	2.01 (10.89)
White	2.81 (7.02)	3.15 (6.68)
Overall	2.34 (7.99)	2.84 (7.55)
Black-White	–2.39	–1.87
Hispanic-White	–2.09	–1.15
Panel B. Levered Returns (%, Annualized)		
Black	–2.74 (43.76)	–0.27 (39.16)
Hispanic	–4.37 (52.19)	–1.07 (46.19)
White	4.50 (39.79)	5.43 (34.92)
Overall	2.78 (42.12)	4.14 (37.05)
Black-White	–7.23	–5.70
Hispanic-White	–8.86	–6.50
Panel C. Distressed Sale (%)		
Black	30.00	23.58
Hispanic	30.58	23.46
White	15.99	12.23
Overall	19.03	14.60
Adjustment	Censoring	Cash+censoring

Notes: This table presents estimates of means and standard deviations of housing returns and rates of distressed home sales by race and ethnicity. Panel A presents statistics for annual unlevered returns. Panel B presents statistics for annual levered returns. Panel C presents statistics for distressed home sales. Column 1 adjusts for censoring due to our finite sample window using the alternative method described in Section E. Column 2 adjusts for both censoring and cash purchases. See Appendix E for more details.

Table A6: Alternative Measures of Housing Returns by Race and Ethnicity

	Baseline	Regular Sales Only	Distressed Sales Only	Cash Adjustment	Distress Adjustment
	(1)	(2)	(3)	(4)	(5)
Panel A. Levered Returns Adjusted for Differences in Rents and Housing Costs					
Black	2.01 (46.85)	15.16 (39.30)	–43.95 (41.88)	3.29 (41.68)	0.19 (46.85)
Hispanic	–2.57 (53.97)	18.33 (42.92)	–59.54 (37.36)	0.06 (47.61)	–3.50 (53.76)
White	6.46 (40.99)	13.21 (35.44)	–44.07 (44.20)	6.74 (35.94)	5.78 (40.90)
Overall	4.88 (43.58)	13.95 (36.72)	–47.93 (42.84)	5.56 (38.27)	4.07 (43.49)
Black-White	–4.45	1.95	0.12	–3.45	–5.59
Hispanic-White	–9.03	5.12	–15.46	–6.67	–9.28
Panel B. Total Returns					
Black	4.44 (9.03)	7.62 (4.96)	–6.66 (11.03)	5.12 (8.43)	4.17 (8.93)
Hispanic	3.51 (11.54)	8.56 (6.38)	–10.25 (11.24)	4.69 (10.78)	3.32 (11.46)
White	6.49 (6.77)	7.80 (5.08)	–3.33 (9.30)	6.80 (6.44)	6.33 (6.74)
Overall	5.92 (7.86)	7.88 (5.25)	–5.46 (10.46)	6.38 (7.38)	5.75 (7.82)
Black-White	–2.04	–0.18	–3.33	–1.67	–2.16
Hispanic-White	–2.97	0.77	–6.92	–2.10	–3.00

Notes: This table presents estimates of means and standard deviations of alternative measures of housing returns by race and ethnicity. Panel A presents statistics for annual levered returns, incorporating racial differences in interest rates, maintenance costs, property taxes, and rents. This adjustment applies differences in costs and rents estimated in Avenancio-León and Howard (2019), Gerardi et al. (2020), and Demers and Eisfeldt (2022). Panel B presents statistics for annual total returns. Column 1 presents estimates from baseline sample described Section 2. Column 2 presents estimates for the subsample of properties not sold in a distressed sale, including properties not sold within the sample period. Column 3 present estimates for the subsample of properties sold in a distressed sale. Column 4 adjusts for cash purchases. Column 5 adjusts for bias from not observing future distressed sales among properties that remain unsold by the end of our sample window. See Section 3 for more details.

Table A7: Differences in Home Improvement Expenditures by Race

	Home Expenditures		Number of Permits		Permit Costs		Non-Home Durables	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black	–0.214***	–0.201***	–0.0207***	–0.0103***	–0.0529***	–0.0215***	–165.5***	–110.6***
	(0.0232)	(0.0274)	(0.000486)	(0.000483)	(0.00166)	(0.00187)	(8.815)	(11.83)
Hispanic	–0.0642***	–0.134***	–0.0138***	–0.00807***	–0.0457***	–0.0151***	2.094	–47.99***
	(0.0183)	(0.0257)	(0.000684)	(0.000424)	(0.00179)	(0.00148)	(8.107)	(11.66)
Outcome Mean	0.836	0.852	0.0646	0.0641	0.141	0.140	395.1	406.2
N	640,027	526,872	5,416,456	5,029,784	5,416,456	5,029,784	646,210	526,872
Controls		X		X		X		X

Notes: This table presents estimates of differences in home improvement expenditures by race/ethnicity. Each column corresponds to a separate regression. The outcome in Columns 1 and 2 is total annual home improvement expenditures as a percentage of home value, and *Controls* indicates regression including fixed effects that interact state, purchase year, and current year. The outcome in Columns 7 and 8 is total monthly non-home durable expenditures, and *Controls* indicates regression controlling for log home value and including fixed effects that interact state, purchase year, and current year. The data for Columns 1, 2, 7, and 8 are from the 2001 to 2013 waves of the Consumer Expenditure Survey, used in Benmelech et al. (2017). The outcome in Columns 3 and 4 is the number of building permits for home improvement filed per year of ownership, and *Controls* denotes fixed effects that interact purchase year, sale year, and state. The outcome in Columns 5 and 6 is the annual reported job costs filed with building permits, normalized as a percentage of purchase price per year of ownership. Permits are measured in BuildFax data. The data in Columns 3 through 6 are from baseline sample of ownership spells restricted to properties merged with Buildfax data. Standard errors are clustered at the level of the household/ownership spell. *** p<0.001, ** p<0.01, * p<0.05

Table A8: Racial Disparities in NPV and Sharpe Ratio

Outcome	NPV	NPV	NPV	NPV	Sharpe Ratio (Unlevered)	Sharpe Ratio (Levered)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Raw Differences						
Black	–6.313	–6.129	–12.13	–8.254	–0.318	–0.161
	(0.465)	(0.581)	(0.528)	(0.588)	(0.0113)	(0.0122)
Hispanic	–5.640	–8.970	–10.98	–11.72	–0.150	–0.255
	(0.783)	(0.783)	(0.955)	(0.836)	(0.0220)	(0.0193)
N	13,430,665	13,430,665	13,430,665	13,430,665	13,629,356	13,429,553
Panel B. Fixed Effect Estimates						
Black	–2.846	–1.286	–5.977	–2.235	–0.177	–0.0657
	(0.0836)	(0.0947)	(0.150)	(0.109)	(0.00404)	(0.00282)
Hispanic	–2.008	–1.480	–4.343	–2.690	–0.150	–0.0612
	(0.0970)	(0.0721)	(0.135)	(0.105)	(0.00362)	(0.00245)
N	12,841,120	12,841,120	12,841,120	12,841,120	13,026,345	12,840,998
Outcome Mean	11.46	5.807	7.049	4.286	0.0404	0.138
Outcome SD	45.75	38.30	52.58	40.61	1.219	1.216
Forecl. Costs	$0	$0	$50,000	$50,000	-	-
Cost-Weighted		X		X		

Notes: This table presents estimates of the racial gap in housing returns in net present value (NPV) and Sharpe ratios. Columns 1 through 4 present results for NPV, which is scaled as a percentage of the net present value of cash flows (i.e., upfront costs, mortgage payment, property taxes, insurance, and maintenance costs discounted by the 30-year fixed mortgage rate). Column 5 presents results for Sharpe ratio computed using unlevered returns. Column 6 presents results for Sharpe ratio computed using levered returns. For computing both Sharpe ratios, reference groups are properties purchased in the same county and year; risk-free rate is 10-year Treasury yield. Within each panel, columns correspond to separate regressions estimating Equation 5. Panel A presents raw estimates. Panel B applies fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant; fixed effects that interact county, purchase year, and deciles of income at home purchase; and fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. *Cost-Weighted* indicates that observations have been weighted by (strictly positive) net present value of costs (i.e., upfront costs, mortgage payment, taxes and insurance, and maintenance). *Forecl. Costs* denotes additional dollar cost of foreclosure paid in final month of homeownership spell. Data come from baseline sample of ownership spells described in Section 2. Standard errors are clustered within purchase year, sale year, and county cells.

Table A9: Decomposition of Levered Returns

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Raw Differences						
Black	–2.325	–2.207	–2.042	–7.906	–5.018	–4.702
	(0.0739)	(0.0745)	(0.0764)	(0.448)	(0.394)	(0.358)
Hispanic	–2.264	–2.284	–2.973	–8.704	–9.632	–10.33
	(0.194)	(0.196)	(0.196)	(0.995)	(0.761)	(0.606)
Panel B. Fixed Effect Estimates						
Black	–1.278	–1.155	–1.074	–7.141	–2.458	–0.517
	(0.0318)	(0.0313)	(0.0295)	(0.192)	(0.107)	(0.0850)
Hispanic	–1.295	–1.232	–1.203	–6.409	–2.666	–1.860
	(0.0359)	(0.0360)	(0.0357)	(0.170)	(0.115)	(0.116)
Outcome Mean	2.355	1.452	5.921	9.218	4.881	1.783
Outcome SD	8.161	7.938	7.861	53.70	43.40	30.66
N	13,630,629	13,630,629	13,430,665	13,427,973	13,430,541	13,430,541
Sale Costs		X	X	X	X	X
Closing Costs			X	X	X	X
Rent Minus Upkeep			X		X	X
Leverage				X	X	X
Mortgage Payment					X	X
Cost-Weighted						X

Notes: This table presents estimates of the racial gap in housing returns using six different measures of housing returns to illustrate the contribution of each component of levered returns. Within each panel, columns correspond to separate regressions estimating Equation 5. Panel A presents raw estimates, while Panel B applies fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, fixed effects that interact county, purchase year, and deciles of income at home purchase, and fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. Each column corresponds to different assumptions imposed on annualized housing returns. *Sale Costs* indicates that a 5% transaction cost upon property sale has been assumed. *Closing Costs* indicates that closing costs at purchase have been imputed. In Column 3, these have been imputed under the assumption of no leverage (i.e., half the closing costs associated with a mortgage with 80% LTV). *Rent Minus Upkeep* factors in implicit rents, property taxes, and maintenance costs. *Leverage* incorporates loan balances that offset up-front costs and repaid at sale. *Mortgage Payment* factors in monthly mortgage payments. *Cost-Weighted* indicates that observations have been weighted by (strictly positive) net present value of costs (i.e., upfront costs, mortgage payment, taxes and insurance, and maintenance). Data are from baseline sample of ownership spells described in Section 2. Standard errors are clustered within purchase year, sale year, and county cells.

Table A10: Racial Gaps in Large Income Losses in Current Population Survey

	(1)	(2)	(3)
Black	6.037***	6.231***	5.749**
	(1.125)	(1.455)	(1.909)
Hispanic	4.714***	2.718	4.149
	(1.179)	(1.608)	(2.270)
Outcome Mean	19.05	17.62	17.09
Outcome SD	39.27	38.10	37.65
N	39129	22583	13238
Job Tenure	Any Tenure	5+ Years	10+ Years

Notes: This table presents regressions of an indicator that a worker experiences a 30 percent loss in wage and salary income over 12 months on race indicators. All specifications control for fixed effects that interact year, state, and highest level of educational attainment; fixed effects for sex, marital status, income deciles; linear and quadratic terms in age; and fixed effects that interact 4-digit occupation code and year. All controls are measured at beginning of 12-month period. Column 1 presents estimates for all homeowners. Column 2 restricts to subsample with 5 or more years of experience in their current job. Column 3 restricts to subsample with 10 or more years of experience. Data come from sample of homeowners in CPS described in Section C.5. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table A11: Definitions for Outcomes from National Survey of Mortgage Originations

Outcome	***Definition/Example Question***	***Response Categories***
Strategic Default Attitudes	Do you agree or disagree with the following statements? It is okay to default or stop making mortgage payments if it is in the borrower's financial interest	1 = Agree; 0 = Disagree
Past House Price Growth	In the last couple years, how have the following changed in the neighborhood where this property is located? House price	–1 = Significant decrease; 0 = Little/no change; 1 = Significant increase
Expected House Price Growth	What do you think will happen to the prices of homes in this neighborhood over the next couple of years?	–2 = Decrease a lot; –1 = Decrease a little; 0 = Remain about the same; 1 = Increase a little; 2 = Increase a lot
Actual House Price Growth	Mark-to-market LTV at origination divided by mark-to-market LTV after two years	
Past Income Growth	In the last couple years, how have the following changed for you (and your spouse/partner)? Household income	–1 = Significant decrease; 0 = Little/no change; 1 = Significant increase
Expected Income Growth	In the next couple of years, how do you expect the following to change for you (and your spouse/partner)? Household income	–1 = Significant decrease; 0 = Little/no change; 1 = Significant increase
Past Financial Crisis	In the last couple of years, have any of the following happened to you (or your spouse/partner)? A personal financial crisis	1 = Yes; 0 = No
Expected Financial Crisis	How likely is it that in the next couple of years you (or your spouse/partner) will face... Some other personal financial crisis	–1 = Not at all; 0 = Somewhat; 1 = Very
Actual 90-Day Default Rate	Homeowner 90 days past due at some point in first two years after mortgage origination	

Notes: This table provides additional information about the outcomes constructed for the analysis of outcomes in the National Survey of Mortgage Originations (NSMO). Example questions correspond to wording as stated in NSMO questionnaire.

Table A12: Correlation Between Expectations and Contract Choice in NSMO Sample

	Ever 90 Days Past Due	Loan-to-Value	Debt-to-Income
	(1)	(2)	(3)
Black	2.454***	6.925***	2.660***
	(0.718)	(0.459)	(0.385)
Hispanic	–0.150	1.657***	3.055***
	(0.466)	(0.424)	(0.314)
Expected House Price Growth	–0.455*	0.698***	0.740***
	(0.226)	(0.193)	(0.138)
Past House Price Growth	–0.494*	–3.977***	–0.0545
	(0.202)	(0.224)	(0.156)
Expected Income Growth	0.607*	0.715**	0.865***
	(0.279)	(0.268)	(0.196)
Past Income Growth	–0.677**	1.677***	–0.939***
	(0.252)	(0.232)	(0.168)
Outcome Mean	2.259	78.25	35.84
Outcome SD	14.86	19.72	12.88
N	24777	24777	24777

Notes: This table presents regressions of two measures of leverage on race indicators, self-reported past experiences, and self-reported expectations among a sample of respondents in the National Survey of Mortgage Originations (NSMO). The outcome in Column 1 is an indicator that a homeowner is 90 days past due in the first two years after origination; the outcome in Column 2 is loan-to-value at origination; and the outcome in Column 3 is debt-to-income at origination. All specifications include survey wave fixed effects, fixed effects for the six bins of income reported in the survey, a quadratic in credit score, a quadratic in age, an indicator that the respondent is married, and an indicator that a respondent is female. Appendix Table A11 presents additional information on regressors. All regressions use analysis weights. See Appendix Section C for additional information about the NSMO sample. *** p<0.001, ** p<0.01, * p<0.05

Table A13: Initial Investor Status in HMDA Versus Investor Status in McDash

	McDash Investor			
	GSE and FHA	Portfolio	Private Securitization	Uncategorized
HMDA Investor				
GSE and FHA	89.4%	1.3%	8.5%	0.8%
Not Sold	15.8%	41.4%	41.1%	1.7%
Private/Other	23.7%	8.8%	64.9%	2.6%

Notes: This table compares the sample distribution of initial investor status recorded in HMDA to the investor status recorded in the McDash servicing records. Each number corresponds to the percent of mortgages assigned to a given investor status in McDash, conditional on a given investor category in HMDA. Data are from sample of property records linked to HMDA data and McDash mortgage servicing records described in Section 5, restricted to mortgages originated between 2004 and 2006 with k-nearest-neighbor proximity scores below 700 (approximately above-median closeness, see Appendix C.4 for distribution).

Table A14: Decomposition of Increase in Rates of Distressed Sales Relative to Homes Purchased in 2002

	Purchases	$f^r_{NG,t}$	$d^r_{NG,t} - d^r_{G,t}$	$d^r_{G,t} - d^r_{G,02}$	$d^r_t - d^r_{02}$	Credit Supply
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Hispanic Homeowners						
2002	337,088	0.65	0.03			
2004	508,044	0.76	0.09	0.10	0.15	32.6%
2005	597,850	0.85	0.13	0.20	0.29	30.6%
2006	571,578	0.85	0.15	0.31	0.41	25.9%
Pooled 2004–2006	2,014,560	0.79	0.11	0.17	0.24	29.6%
Panel B. Black Homeowners						
2002	237,711	0.66	0.04			
2004	332,143	0.77	0.09	0.05	0.09	46.8%
2005	370,648	0.84	0.13	0.10	0.19	46.0%
2006	372,595	0.84	0.14	0.18	0.27	34.8%
Pooled 2004–2006	1,313,097	0.79	0.11	0.09	0.15	42.4%
Panel C. White Homeowners						
2002	2,669,082	0.59	0.03			
2004	3,160,048	0.66	0.08	0.05	0.08	39.6%
2005	3,179,615	0.72	0.10	0.10	0.16	35.1%
2006	2,769,600	0.71	0.11	0.15	0.21	30.9%
Pooled 2004–2006	11,778,345	0.67	0.08	0.08	0.12	35.4%

Notes: This table presents the calculations used to decompose the increase in rates of distressed sales relative to homes purchased in 2002 using the decomposition represented in Equation 7. Rows correspond to statistics for homes purchased in a given year. Column 1 lists the number of purchase mortgage originations in the HMDA data. Column 2 presents the share of purchases with non-GSE and non-FHA mortgages. Column 3 presents the difference in distressed sale rates between GSE/FHA mortgages and other mortgages for a given race/ethnicity and purchase year. Column 4 presents the difference in rates of distressed sales among GSE/FHA mortgages relative to homes purchased in 2002 for a given race/ethnicity. Column 5 presents the overall difference in rates of distressed sales relative to homes purchased in 2002 for a given race/ethnicity and purchase year. Column 6 calculates the share of Column 5 that can be attributed to credit supply factors.

Table A15: Racial Disparities in Housing Returns (All Racial/Ethnic Groups)

Outcome	Unlevered Return (1)	Unlevered Return (2)	Levered Return (3)	Levered Return (4)	Distressed Sale (5)	Distressed Sale (6)
Panel A. Baseline						
Black	–2.325	–1.254	–5.018	–2.417	0.105	0.0395
	(0.0739)	(0.0312)	(0.394)	(0.105)	(0.00369)	(0.00143)
Hispanic	–2.264	–1.283	–9.632	–2.609	0.150	0.0398
	(0.194)	(0.0363)	(0.761)	(0.114)	(0.00713)	(0.00127)
Asian	–0.0691	–0.335	–3.998	–0.606	0.0133	0.00959
	(0.0781)	(0.0119)	(0.362)	(0.0574)	(0.00326)	(0.000552)
Other	1.045	–0.321	4.084	–0.619	0.0258	0.0130
	(0.0551)	(0.0205)	(0.276)	(0.108)	(0.00186)	(0.000943)
Missing	–0.344	–0.314	–2.477	–0.765	0.0311	0.0150
	(0.0407)	(0.0105)	(0.185)	(0.0505)	(0.00157)	(0.000494)
N	16,246,855	14,838,027	16,015,481	14,626,034	16,246,855	14,838,027
Panel B. Interacted						
Black × Reg.	–0.388	–0.296	1.833	–0.790		
	(0.0527)	(0.0230)	(0.452)	(0.118)		
Hispanic × Reg.	1.508	0.189	4.936	1.964		
	(0.0995)	(0.0436)	(0.567)	(0.220)		
Asian × Reg.	0.405	–0.117	–1.432	0.954		
	(0.0540)	(0.0135)	(0.226)	(0.0775)		
Other × Reg.	0.743	0.0876	5.266	1.202		
	(0.0489)	(0.0303)	(0.321)	(0.197)		
Missing × Reg.	0.285	0.0597	0.250	0.914		
	(0.0242)	(0.0112)	(0.125)	(0.0688)		
White × Distr.	–11.66	–9.435	–58.64	–45.74		
	(0.112)	(0.0804)	(0.421)	(0.426)		
Black × Distr.	–15.26	–12.65	–60.04	–46.57		
	(0.151)	(0.111)	(0.471)	(0.518)		
Hispanic × Distr.	–17.72	–13.91	–75.11	–55.60		
	(0.287)	(0.165)	(0.764)	(0.830)		
Asian × Distr.	–13.70	–10.66	–73.77	–53.55		
	(0.211)	(0.122)	(0.688)	(0.665)		
Other × Distr.	–12.51	–10.81	–62.31	–51.35		
	(0.188)	(0.146)	(0.720)	(0.772)		
Missing × Distr.	–13.17	–11.04	–64.62	–51.37		
	(0.171)	(0.113)	(0.591)	(0.622)		
N	16,246,855	14,838,027	16,015,481	14,626,034		
Outcome Mean	2.408	2.418	4.721	4.784	0.145	0.145
Outcome SD	8.148	8.119	43.21	43.36	0.352	0.352
Fixed Effects		X		X		X

Notes: This table presents estimates of racial gaps in housing returns and distressed sales. Columns 1 and 2 present results for unlevered returns. Columns 3 and 4 present results for levered returns. Columns 5 and 6 present results for an indicator that a property is sold in a distressed sale. In Panel B, *Distr.* is an indicator that a sale is distressed sale, and *Reg.* is an indicator that a sale is a non-distressed sale or is not sold in our sample window. *Fixed Effects* indicates the inclusion of fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant; fixed effects that interact county, purchase year, and deciles of income at home purchase; and fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. Race indicators pertain to self-reported race/ethnicity in HMDA. *Missing* denotes that no race/ethnicity was recorded in HMDA. Data come from sample of ownership spells described in Section 2, expanded to include additional racial/ethnic groups. Standard errors are clustered within purchase year, sale year, and county cells.

Table A16: Ownership Transitions by Race

	Next Owner				
	White	Black	Hispanic	Asian	Institutional
Panel A. Owner Race/Ethnicity (All Sales)					
White	80.96	3.91	8.62	6.19	6.03
Black	43.44	33.02	15.24	7.99	17.06
Hispanic	40.33	5.44	44.82	9.11	12.16
Asian	46.80	5.31	13.14	34.42	6.81
Panel B. Owner Race/Ethnicity (Distressed Sales)					
White	78.53	4.38	10.24	6.55	16.44
Black	44.94	31.32	14.60	8.91	27.26
Hispanic	44.16	5.47	38.68	11.44	20.82
Asian	47.93	5.90	14.75	31.08	16.21
Panel C. Sale Type					
Non-Distressed	90.63	88.63	85.83	87.86	
Distressed	9.37	11.37	14.17	12.14	

Notes: This table presents the transition matrix of homeowner characteristics across subsequent homeownership spells. Each number corresponds to the percentages of subsequent owners falling into a given category, conditional on original owner race/ethnicity (Panels A and B) or next owner race/ethnicity (Panel C). Panel A presents statistics for all sales, while Panel B presents statistics for distressed home sales. Panel C presents the share of purchases that are distressed or non-distressed by buyer race/ethnicity. Data are from subsample of baseline sample of ownership spells (described in Section 2), restricted to ownership spells with observed sale within sample window.

Table A17: Characteristics of Next Ownership Spell

	Institutional Next Owner (1)	Occupied by Next Owner (2)	Next Owner Tenure (3)	White Next Owner (4)
Black	0.0184	–0.00417	–1.388	–0.253
	(0.000708)	(0.00115)	(0.121)	(0.00270)
Hispanic	–0.00231	0.0118	–1.439	–0.235
	(0.000443)	(0.000882)	(0.119)	(0.00214)
Distressed	0.130	–0.0429	–6.101	–0.0352
	(0.000730)	(0.000738)	(0.0697)	(0.00103)
Black × Distressed	0.0708	–0.0204	–0.460	0.0188
	(0.00160)	(0.00208)	(0.167)	(0.00367)
Hisp. × Distressed	0.0474	–0.0178	0.712	0.0325
	(0.00124)	(0.00153)	(0.168)	(0.00310)
Outcome Mean	0.0765	0.909	75.04	0.728
N	6,663,541	3,054,278	6,663,541	2,744,976

Notes: This table presents estimates of Equation 5 for outcomes pertaining to the ownership spell immediately following the current spell, controlling for purchase year, sale year, county, leverage, family composition, and income. Specifically, these specifications include fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, fixed effects that interact county, purchase year, and deciles of income at home purchase, and fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. *Distressed* denotes an indicator that the current spell ends in a foreclosure or short sale. Each column corresponds to a separate regression and outcome. *Institutional Next Owner* is an indicator that the next owner is as a non-trust institution. Purchases by non-trust institutions are defined as those in which the buyer name in the property data is not that of a person or trust. *Occupied by Next Owner* is an indicator that the next owner lives in the property, conditional on a non-institutional next owner (i.e., the property is labeled as owner-occupied in the HMDA data). *Next Owner Tenure* is the number of months that the next owner holds the property. *White Next Owner* is an indicator that the next owner identifies as non-Hispanic White. Data are from subsample of baseline sample of ownership spells (described in Section 2), restricted to spells with an observed sale within sample window. Standard errors are clustered within purchase year, sale year, and county cells.

Table A18: Impact of Leverage on 90-Day Mortgage Default

	(1)	(2)	(3)
Current Leverage	0.780	0.655	0.786
	(0.196)	(0.168)	(0.158)
Current Leverage x Black	–0.145	–0.124	–0.132
	(0.029)	(0.026)	(0.021)
Current Leverage x Hisp	–0.021	0.003	–0.033
	(0.044)	(0.036)	(0.027)
Outcome mean	36.4%	36.3%	36.3%
F-Statistic	21.6	17.6	34.4
N	127,347	121,268	121,111
Baseline Controls	X	X	X
Credit/Loan Controls		X	X
Originator x Orig. Month FE			X

Notes: This table presents 2SLS estimates of the impact of leverage on 90-day mortgage default among homeowners with option ARM mortgages, using the design in Gupta and Hansman (2022). Each specification regresses an indicator that a homeowner has defaulted between 24 and 36 months on loan-to-value at 24 months interacted with race indicators, instrumenting using the leave-out mean of current loan-to-value among properties with the same origination month, index type, and original combined loan-to-value. *Baseline Controls* denotes origination month, index type, and ZIP code fixed effects, as well as linear terms in original combined loan-to-value interacted with race and three bins of original loan-to-value (less than or equal to 60, between 60 and 80, and more than 80). *Credit/Loan Controls* denotes linear terms in original home value interacted with 25k bins of value; 20-point bins of credit score at origination; and fixed effects for occupancy, property type, loan purpose, and documentation level. *Originator x Orig. Month FE* denotes fixed effects that interact originator and origination month. Standard errors are clustered at the originator level. See Appendix H for more details.

Table A19: Validation of Leave-Out Instrument for Current Leverage Using Option ARM Index

	Orig. Score (1)	Orig. Value (2)	No/Low Doc. (3)	Purchase (4)	Owner Occ. (5)
Current LTV Leave-Out Mean	–15.9055	–0.5196	0.0575	–0.7604	0.2390
	(10.7618)	(0.5783)	(0.2146)	(0.1311)	(0.0696)
Outcome Mean	702.679	5.373	0.820	0.256	0.852
Outcome SD	46.985	3.362	0.384	0.436	0.355
N	123,361	127,347	127,347	127,347	125,289

Notes: This table presents OLS regressions of mortgage characteristics on the leave-out instrument for current leverage used to apply the design in Gupta and Hansman (2022). Each column corresponds to a different characteristic. The main regressor is the leave-out mean of current loan-to-value among properties with the same origination month, index type, and original combined loan-to-value. All specifications include origination month, index type, and ZIP code fixed effects, as well as linear terms in original combined loan-to-value interacted with race and three bins of original loan-to-value (less than or equal to 60, between 60 and 80, and more than 80). *Orig. Score* denotes credit score at origination. *Orig. Value* denotes appraised value at origination in 100k. *No/Low Doc.* denotes an indicator that the original mortgage has no or low documentation. *Purchase* denotes an indicator that the mortgage purpose is for home purchase. *Owner Occ.* denotes an indicator that the property is owner-occupied. Standard errors are clustered at the originator level. See Appendix H for more details.

Table A20: Differential Modification and Foreclosure Rates by Race

	(1)	(2)	(3)	(4)	(5)
Panel A. Modified					
Black	6.733***	5.772***	5.047***	3.897***	2.545**
	(0.565)	(0.395)	(0.362)	(0.528)	(0.956)
Hispanic	–0.541	0.829	1.570***	0.178	–0.133
	(0.782)	(0.547)	(0.427)	(0.343)	(0.481)
Panel B. Foreclosed					
Black	–3.958***	–4.714***	–4.140***	–4.064***	–2.780**
	(0.928)	(0.481)	(0.524)	(0.592)	(0.986)
Hispanic	6.744***	2.011***	0.660	1.503***	2.041**
	(0.923)	(0.559)	(0.450)	(0.370)	(0.642)
Panel C. Self-Cured					
Black	–3.223***	–1.442***	–1.116***	0.147	0.562
	(0.574)	(0.235)	(0.225)	(0.213)	(0.578)
Hispanic	–5.543***	–1.907***	–1.553***	–0.861***	–1.026**
	(0.631)	(0.173)	(0.135)	(0.134)	(0.346)
N	1,117,521	1,025,022	957,788	335,551	55,341
Controls	Baseline	Borrower, Mortgage	County-Time, Servicer FE	Tract-Time FE	Servicer-Tract-Time FE

Notes: This table presents regressions in which the outcomes capture the events following a homeowner becoming 90 days delinquent on their mortgage, and shows that minorities are more likely to receive a loan modification after becoming delinquent. The three outcomes are *Modified*, an indicator that the delinquency resulted in the homeowner's loan terms being modified (μ = 20.0%); *Foreclosed*, an indicator that the delinquency resulted in a foreclosure (μ = 63.8%); and *Self-Cured*, an indicator that the delinquency resulted in the homeowner becoming current or paying off the loan (μ = 14.0%). The specification in Column 1 includes fixed effects for the quarter of default. Column 2 interacts quarter of default with origination year, adds fixed effects for number of years remaining in term, indicators for the mortgage being an ARM, interest-only, or negative-amortization, and adds decile fixed effects for current LTV, original credit score, original income, and original loan amount. Column 3 applies fixed effects that interact county, quarter of default, and origination year, and adds a servicer fixed effect to the controls in (2). Column 4 applies fixed effects that interact tract, quarter of default, and origination year, in addition to the controls in (2). Column 5 applies fixed effects that interact tract, servicer, quarter of default, and origination year in addition to the controls in (2). Data comprised of homeowners who have become delinquent and are observed in the Fannie Mae, Freddie Mac, and ABSNet data, described in Section 6. Coefficients are scaled by 100 and are interperetable as the percentage point differences in the likelihood of each outcome. Standard errors are two-way clustered at the servicer level and within origination year, county, and year of default cells. *** p<0.001, ** p<0.01, * p<0.05

Table A21: Impacts of Modifications on Distressed Sales and Annualized Housing Returns

Outcome	Modification	Unlevered Return	Unlevered Return	Levered Return	Distressed Sale
Specification	OLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Servicer Instrument	0.606***				
	(0.0558)				
Modification		5.496***	11.24***	35.88***	–0.362***
		(0.282)	(2.536)	(9.189)	(0.0981)
Black × Modification		0.979**	–2.825	–11.91	0.103
		(0.331)	(2.144)	(8.160)	(0.104)
Hispanic × Modification		2.692***	3.223	–1.692	0.0150
		(0.412)	(1.830)	(6.012)	(0.0688)
Outcome Mean	0.181	–12.58	–12.58	–54.16	0.759
N	130,635	130,354	130,354	130,246	130,354

Notes: This table presents estimated treatment effects of mortgage modifications. Results indicate that modifications reduce the likelihood of distressed sales and increase housing returns for homeowners of all racial groups. Column 1 presents estimates from the first stage OLS regression. Column 2 presents OLS estimates of the impact of modifications on unlevered returns. Columns 3 through 5 present treatment effects of modifications interacting the servicer instrument and modification indicator with race/ethnicity indicators. The outcome in Column 1 is an indicator that a homeowner receives a modification within 12 months of default. The outcome in Columns 2 and 3 is the annualized unlevered rate of return. The outcome in Column 4 is the annualized levered rate of return. The outcome in Column 5 is an indicator that the ownership spell ends in a distressed sale. *Modification* denotes an indicator that a homeowner receives a modification within 12 months of default. All specifications include interacted fixed effects for purchase year, default year, tract, an indicator for negative amortization loan, and an indicator for interest-only loan. Data comprised of homeowners who have become delinquent and are observed in the Fannie Mae, Freddie Mac, and ABSNet data, described in Section 2. Standard errors are two-way clustered at the servicer level and within origination year, county, and year of default cells. *** p<0.001, ** p<0.01, * p<0.05

Table A22: Impacts of Modifications, Robustness and Heterogeneity Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Unlevered Returns (%, Annualized)						
Modification	11.24***	9.411***	13.30***	10.58***	9.844***	10.22***
	(2.536)	(2.348)	(2.982)	(2.420)	(2.469)	(2.547)
Black × Mod.	–2.825	–3.675	–3.339	–1.805		
	(2.144)	(2.845)	(2.963)	(2.457)		
Hispanic × Mod.	3.223	4.122	2.592	4.467		
	(1.830)	(2.643)	(2.269)	(2.298)		
Distressed Tract × Mod.					5.139**	
					(1.670)	
Single Applicant × Mod.						2.939*
						(1.460)
Outcome Mean	–12.58	–13.62	–13.63	–13.60	–12.59	–12.58
N	130354	71888	102048	80417	130298	130354
Panel B. Distressed Sale						
Modification	–0.362***	–0.309**	–0.400**	–0.317**	–0.295**	–0.337***
	(0.0981)	(0.109)	(0.130)	(0.102)	(0.0914)	(0.0967)
Black × Mod.	0.103	0.197	0.107	0.133		
	(0.104)	(0.157)	(0.130)	(0.113)		
Hispanic × Mod.	0.0150	0.0255	0.0827	0.0334		
	(0.0688)	(0.0921)	(0.0850)	(0.0842)		
Distressed Tract × Mod.					–0.0965	
					(0.0515)	
Single Applicant × Mod.						–0.00794
						(0.0643)
Outcome Mean	0.759	0.773	0.776	0.775	0.759	0.759
N	130354	71888	102048	80417	130298	130354
Controls	Baseline	Score	LTV	Income	Baseline	Baseline

Notes: This table presents robustness exercises for the analysis of the impacts of mortgage modifications along with heterogeneous impacts by neighborhood and household characteristics. The outcomes are the unlevered rate of return (Panel A) and an indicator that the ownership spell ends in a distressed sale (Panel B). Column 1 presents the baseline specification. Columns 2 through 4 interact baseline fixed effects with terciles of credit score at origination, LTV in the month of default, and income at origination, respectively. Column 5 presents heterogeneity results for distressed Census tracts. The share of sales that are distressed sales is computed for each tract-year, and distressed tracts are defined as those in the highest quartile. Column 6 presents heterogeneity results for an indicator that the household listed a single individual on their mortgage application. The baseline specification includes interacted fixed effects for purchase year, default year, tract, indicator for negative amortization loan, and indicator for interest-only loan. Data comprised of homeowners who have become delinquent and are observed in the Fannie Mae, Freddie Mac, and ABSNet data, described in Section 2. Standard errors are two-way clustered at the servicer level and within origination year, county, and year of default cells. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

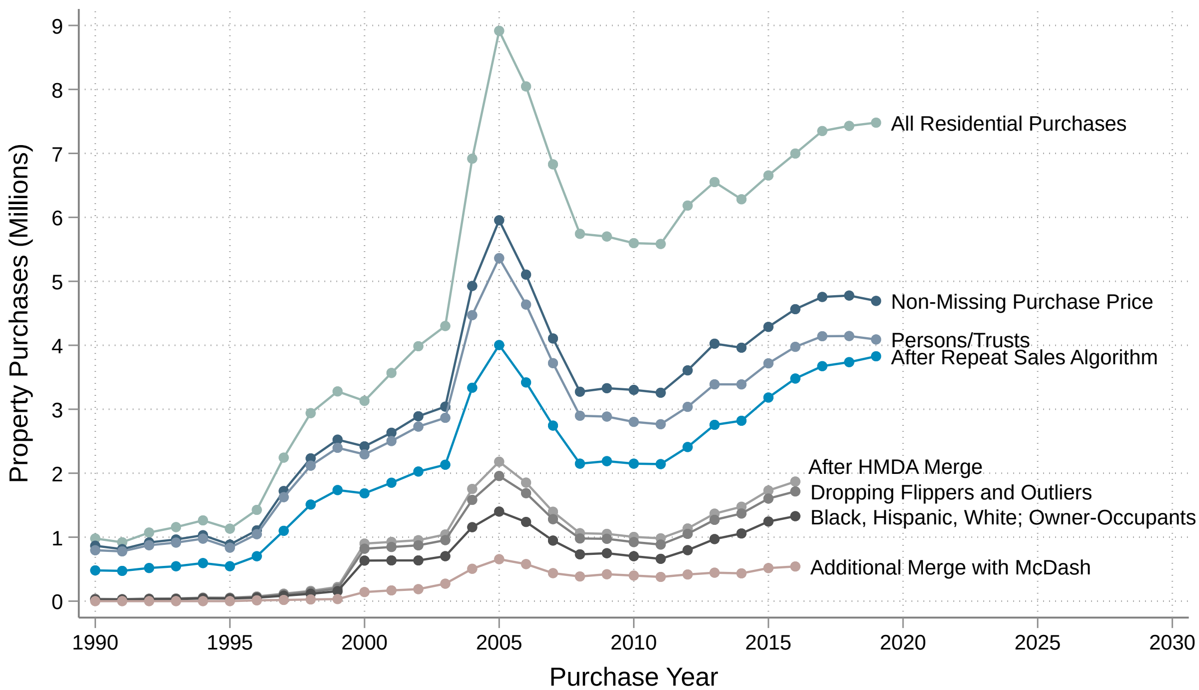
Table A23: Modification Treatment Effects Placebo Outcomes

	Distressed Sale (1)	Unlevered Return (2)	Levered Return (3)
$\hat{\gamma}_{s(i),t}$	0.00510	–0.0222	1.122
	(0.0100)	(0.360)	(1.309)
Black $\times\hat{\gamma}_{s(i),t}$	–0.0167	–0.204	–0.0665
	(0.0116)	(0.466)	(1.447)
Hispanic $\times\hat{\gamma}_{s(i),t}$	–0.00101	–0.234	–1.761
	(0.00847)	(0.347)	(1.158)
Outcome Mean	0.708	–10.23	–48.41
N	130,354	130,354	130,246

Notes: This table presents placebo exercises for the analysis of the impacts of mortgage modifications. Each column estimates the reduced-form impact of the server instrument and its interactions with race/ethnicity indicators on placebo outcomes. The placebo outcomes are defined using the predicted values from a regression of the true outcome (e.g. indicator that ownership spell ends in a distressed sale) on a vector of individual characteristics measured prior to the realization of the true outcome. The vector of characteristics includes loan type (i.e. conventional, FHA, VA); loan purpose; indicators for ARM, interest-only, and negative amortization; term; deciles of credit score, income, interest rate, and amount at origination; current year, and data source (i.e. Fannie Mae, Freddie Mac, ABSNet). Each placebo regression includes interacted fixed effects for purchase year, default year, tract, indicator for negative amortization loan, and an indicator for interest-only loan. Data comprised of homeowners who have become delinquent and are observed in the Fannie Mae, Freddie Mac, and ABSNet data, described in Section 6. Standard errors are two-way clustered at the servicer level and within origination year, county, and year of default cells. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

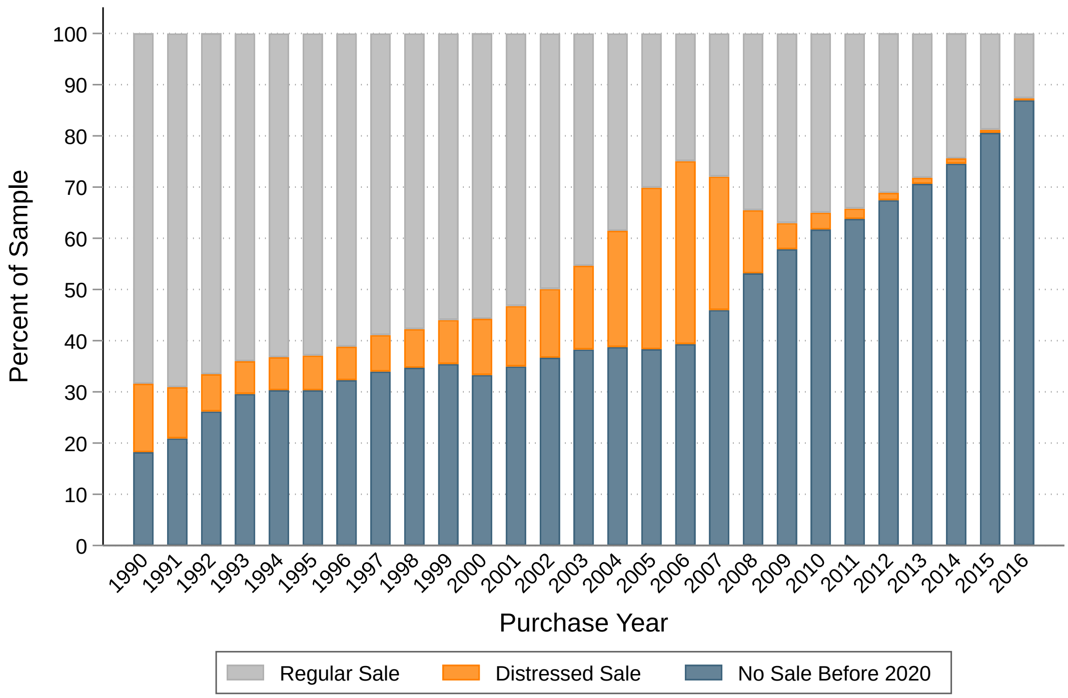
B Appendix Figures

Figure A1: Changes in Sample Size Due to Sample Restrictions



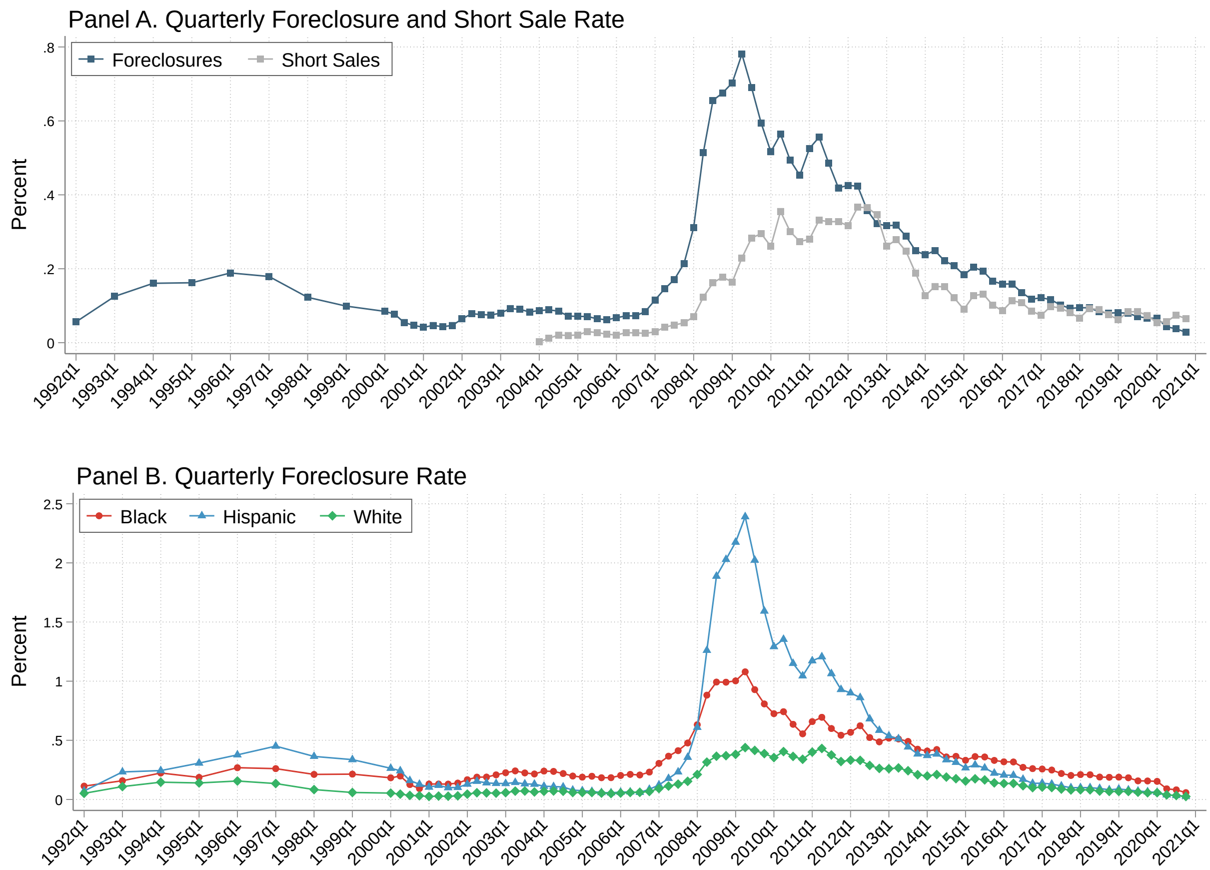
Notes: This figure illustrates the changes in sample size created by the sample restrictions we make to arrive at our baseline sample of ownership spells. Each point represents the number of properties that were purchased in a given year in a given subsample of the raw property data. *All Residential Purchases* denotes all arm's length, full consideration purchases in the ATTOM property data, for which the property use code is labeled as residential. *Non-Missing Purchase Price* restricts to purchases where a transaction price has been recorded. *Persons/Trusts* restricts to purchases made by persons or trusts. *After Repeat Sales Algorithm* drops properties for which the repeat sales algorithm cannot be implemented (e.g., due to a missing transaction between consecutive ownership spells), but retains properties that were not sold by the end of March 2020. *After HMDA Merge* restricts to mortgaged purchases where the mortgage can be linked to a record in the HMDA data. *Dropping Flippers and Outliers* drops ownership spells lasting less than 12 months, purchases with prices that are less than $10,000 or with combined loan-to-value ratios of more than 102.5%. *Black, Hispanic, White; Owner-Occupants* restricts to purchases made by owner-occupants identifying as Hispanic, non-Hispanic Black, or non-Hispanic White. This sample represents our baseline analysis sample used to estimate racial gaps in housing returns. *Additional Merge with McDash* restricts to properties where the mortgage has been linked to the McDash servicing records. See Appendix C for more details.

Figure A2: Composition of Baseline Sample by Purchase Year and Sale Type



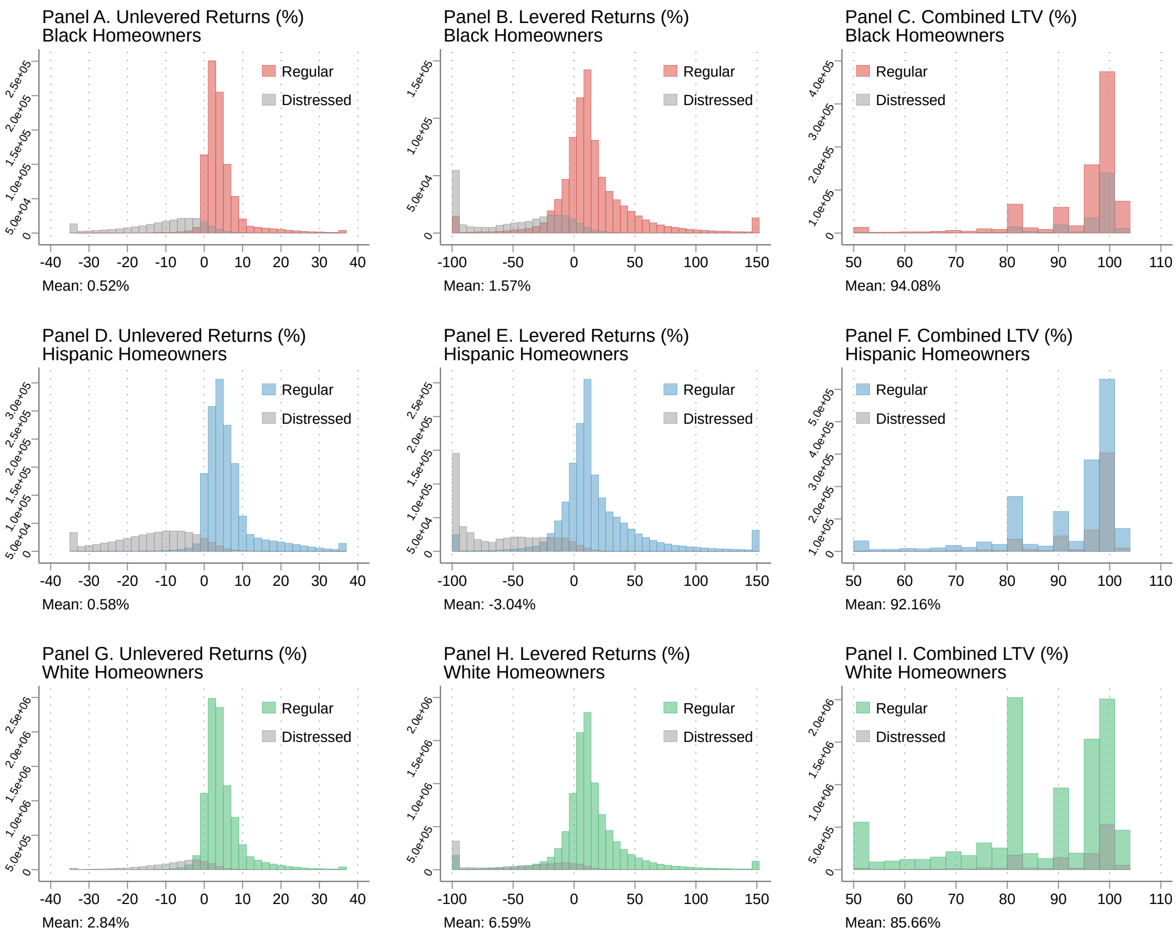
Notes: This figure illustrates the composition of sale types in our baseline sample of ownership spells. Each bar plots the percent of ownership spells belonging to a given type beginning in a given year. *Regular Sale* denotes properties that were purchased in a given year and sold by March 2020 in a non-distressed sale. *Distressed Sale* denotes properties that were sold by March 2020 in a distressed sale. *No Sale Before 2020* denotes properties that were not sold by March 2020. See Section 2 for more details on the baseline sample.

Figure A3: Time Series of Aggregate Distressed Sales



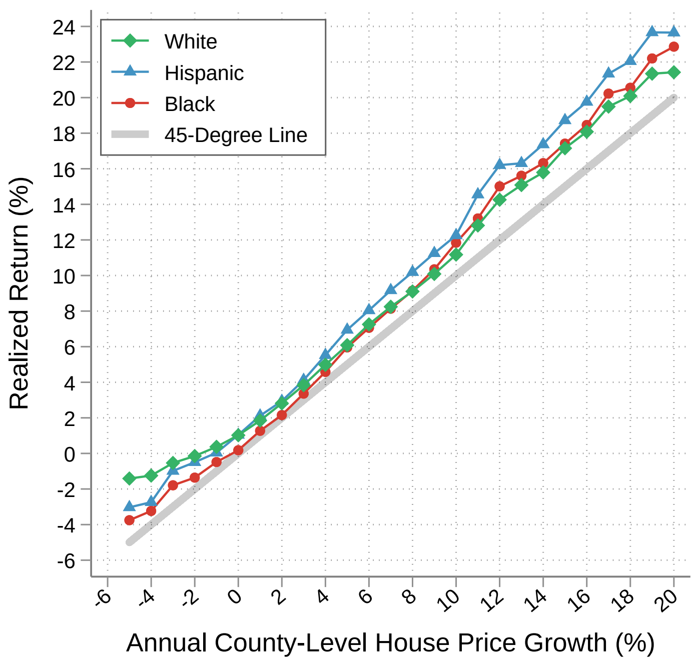
Notes: This figure plots aggregate trends in distressed sales and housing returns from 1992 to 2020, and illustrates higher rates of distressed sales among minority homeowners throughout this period. Panel A plots quarterly foreclosure and short sale rates, defined as the percent of ownership spells beginning prior to a given quarter and ending in a foreclosure or short sale in that quarter. Panel B plots the quarterly foreclosure rate by race/ethnicity. Data are from baseline sample of ownership spells described in Section 2, and includes sales that occurred after March 2020.

Figure A4: Distribution of Annualized Returns and Combined Loan-to-Value Ratio



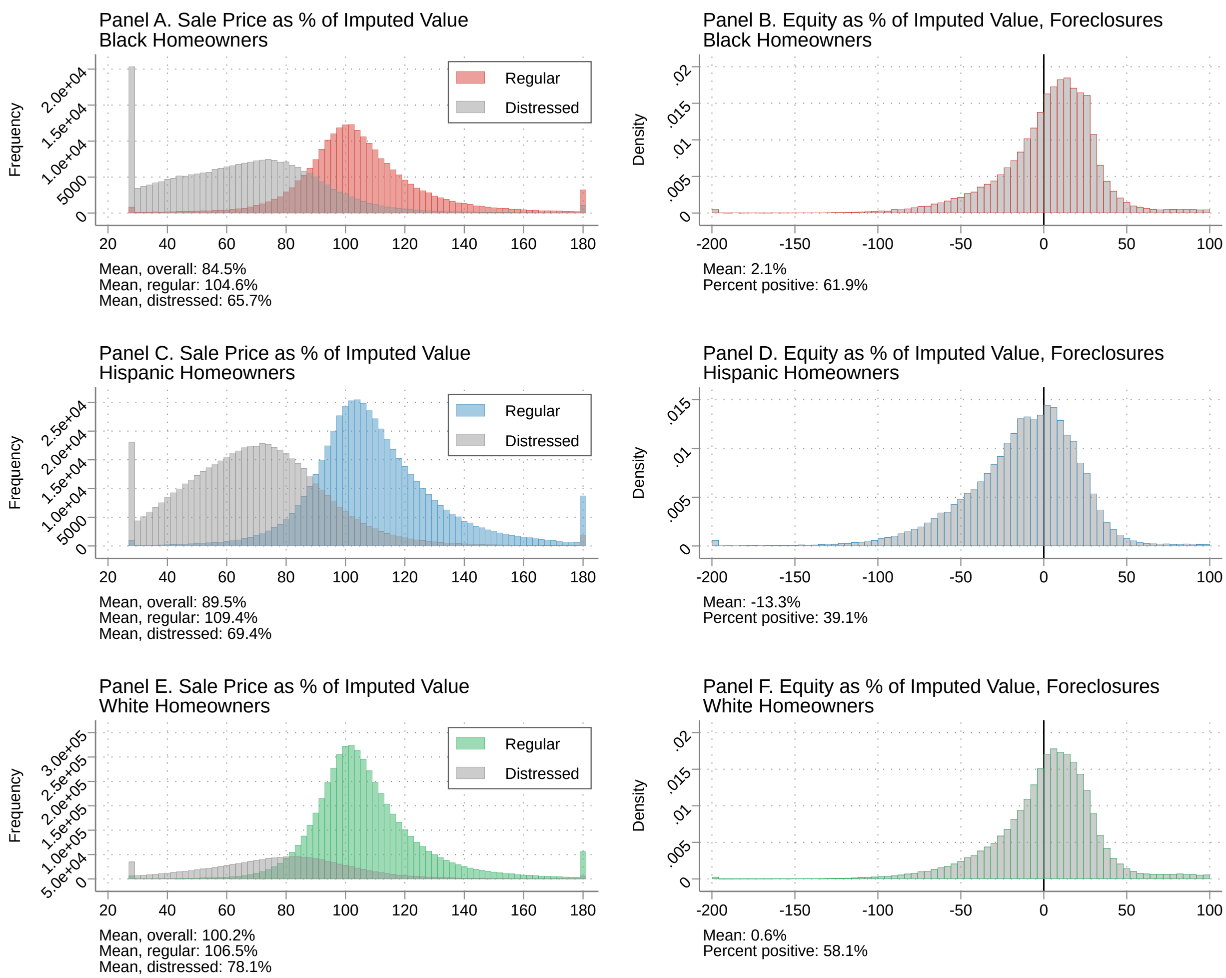
Notes: This figure plots the distribution in frequencies of annualized unlevered returns (i.e., sale price divided by purchase price, Equation 1), annualized levered returns (i.e., internal return, Equation 2), and combined loan-to-value ratio at origination. Distributions are plotted in separate panels for Black, Hispanic and White homeowners. For each race/ethnicity, the distributions for regular sales and distressed sales are presented separately within each panel. Distributions are winsorized for clarity. Data are from baseline sample of ownership spells described in Section 2.

Figure A5: Realized Returns vs. Local House Price Growth Among Non-Distressed Sales



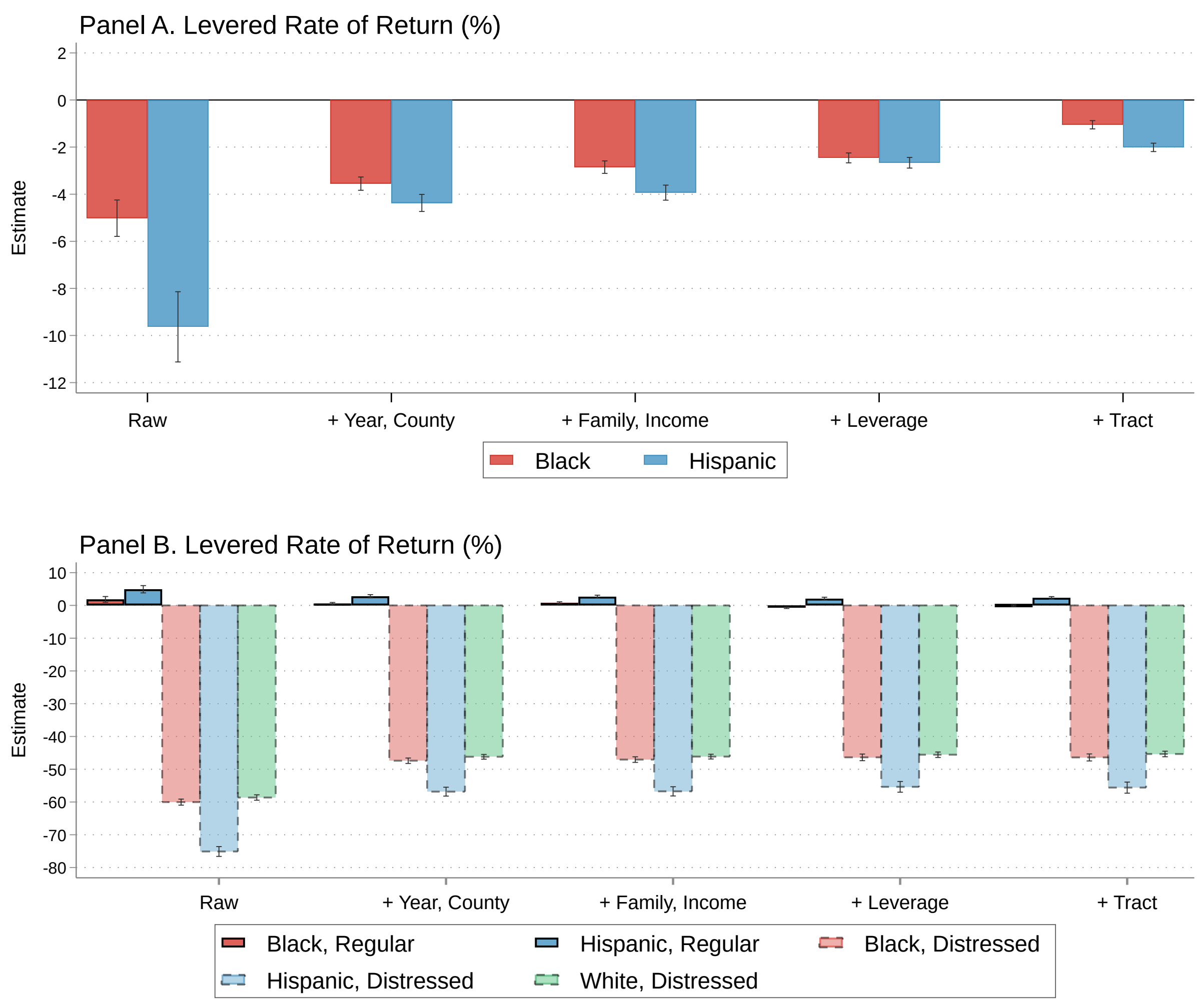
Notes: This figure presents a binned scatterplot of realized annual unlevered returns (Equation 1), plotted against annual local house price growth, computed for the years between property purchase and sale using the FHFA county-level house price index (Bogin et al., 2019). Data come from baseline sample of ownership spells described in Section 2, restricted to properties that are sold within our sample window in a non-distressed sale.

Figure A6: Sale Price and Equity as Percentage of Imputed Value



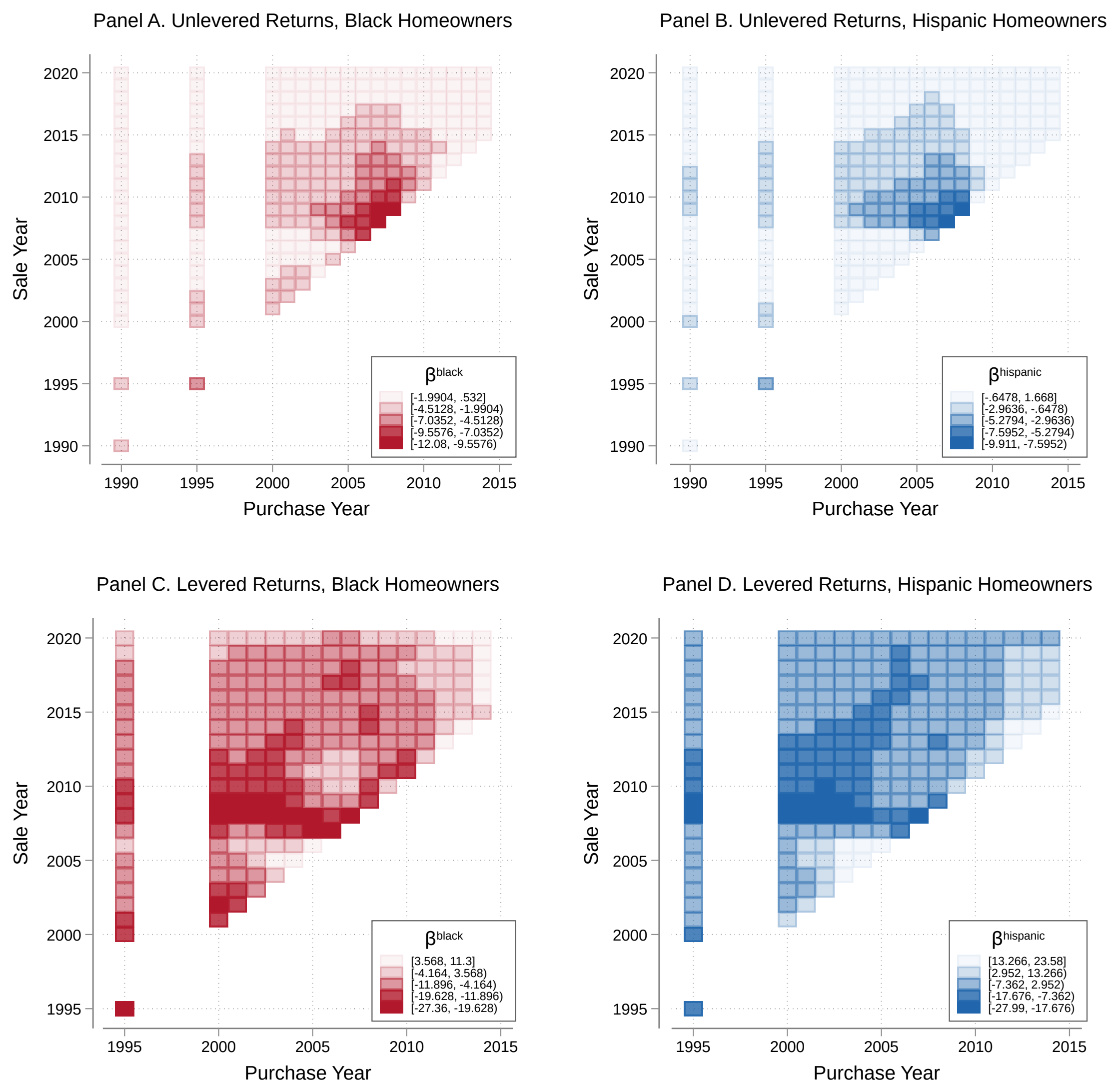
Notes: This figure plots the distribution of sale prices (Panels A, C, and E), and home equity at sale (Panels B, D, and F) as a percentage of the property's imputed value at time of sale. In Panels A, C, and E, imputed value at the time of sale is computed by inflating purchase prices using the FHFA county-level house price index (Bogin et al. 2019). Distributions are presented separately for regular sales and distressed sales. Data are from sample of homeowners with observed purchase and sale prices in baseline sample of ownership spells described in Section 2. Data for Panels B, D, and F come from mortgage records from McDash, which contain unpaid principal balance at sale, and are restricted to foreclosed homes. Equity is computed by subtracting unpaid principal from imputed property value, calculated by inflating value at origination using the FHFA house price index. Distributions are plotted separately for Black (Panels A and B), Hispanic (Panels C and D), and White (Panels E and F) homeowners.

Figure A7: Racial Gaps in Levered Housing Returns



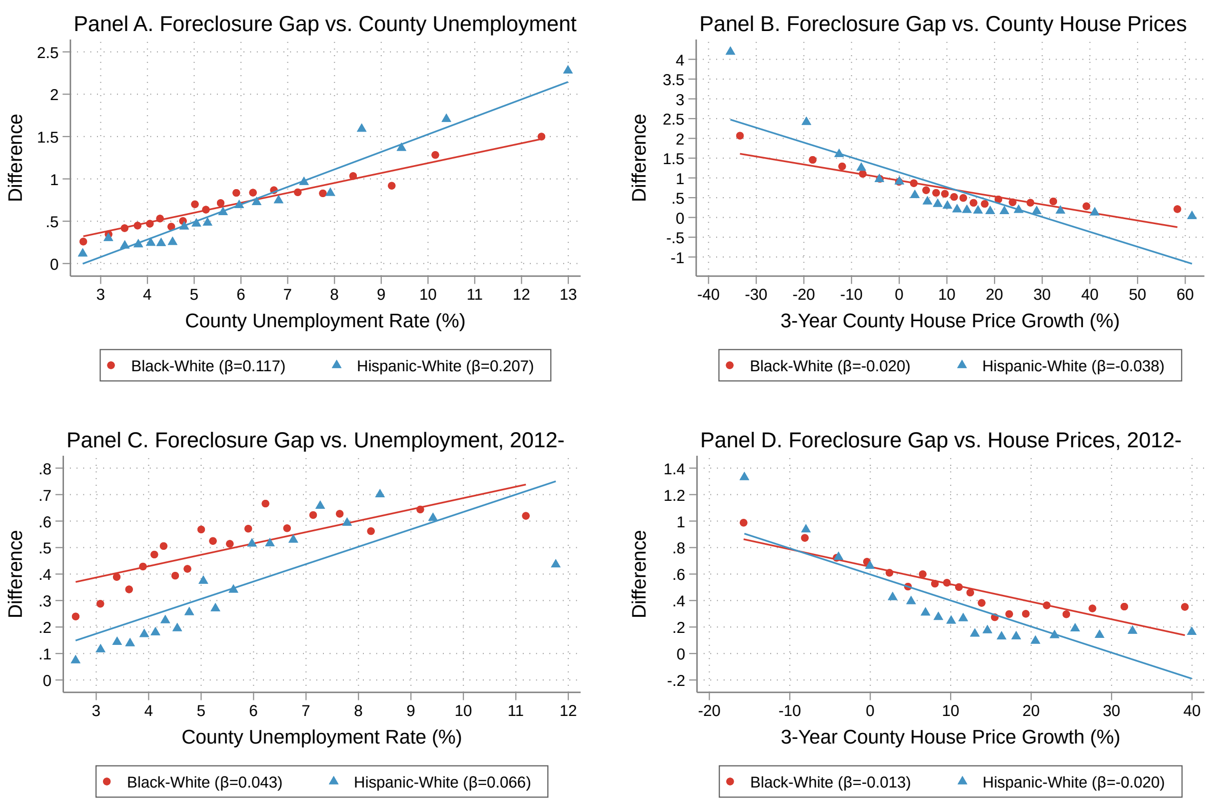
Notes: This figure presents estimates of racial gaps in levered housing returns (Equation 2). In Panel A, each pair of bars corresponds to a separate regression and indicates estimated coefficients of Black and Hispanic indicators from Equation 5 using a particular set of fixed effects. In Panel B, each set of bars corresponds to coefficients of race indicators interacted with an indicator that a property is sold in a distressed sale. *Raw* denotes estimates without controls. *+ Year, County* adds in purchase year-by-county fixed effects. *+ Family, Income* adds fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, and fixed effects that interact county, purchase year, and deciles of income at home purchase. *Leverage* adds fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. *Tract* adds fixed effects that interact purchase year and Census tract. Standard errors are clustered within purchase year, sale year, and county cells. Data are from baseline sample of ownership spells described in Section 2. Table 1 in the Online Appendix presents numerical values and additional statistics.

Figure A8: Heat Map of Returns Gap by Purchase and Sale Year



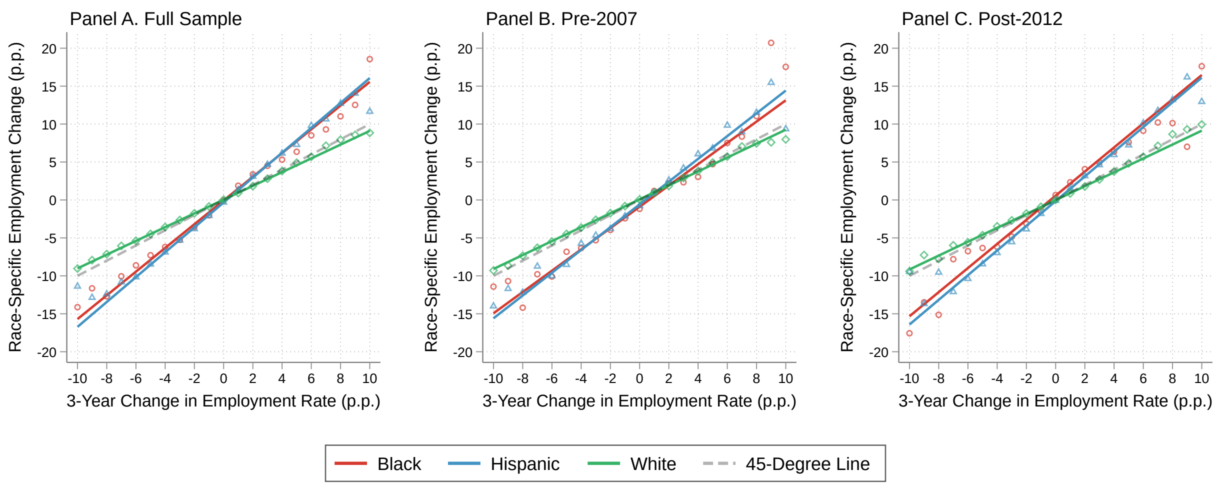
Notes: This figure presents estimates of racial gaps in unlevered housing returns split by year of purchase and year of sale for unlevered returns (Panels A and B) and levered returns (Panels C and D). Within each panel, the color of a square indicates the size of the estimated coefficient, with each square corresponding to a coefficient for race/ethnicity indicators in separate regressions estimated within purchase year-by-sale year cells. Regressions are estimated as in Equation 5 with county fixed effects. For purchase year, 1990 denotes period 1990-1994 and 1995 denotes period 1995-1999. For sale year, 1990 denotes 1990-1994 and 1995 denotes 1995-2001. Data are from baseline sample of ownership spells described in Section 2.

Figure A9: Racial Gap in Foreclosure Rate Under Different Local Economic Conditions



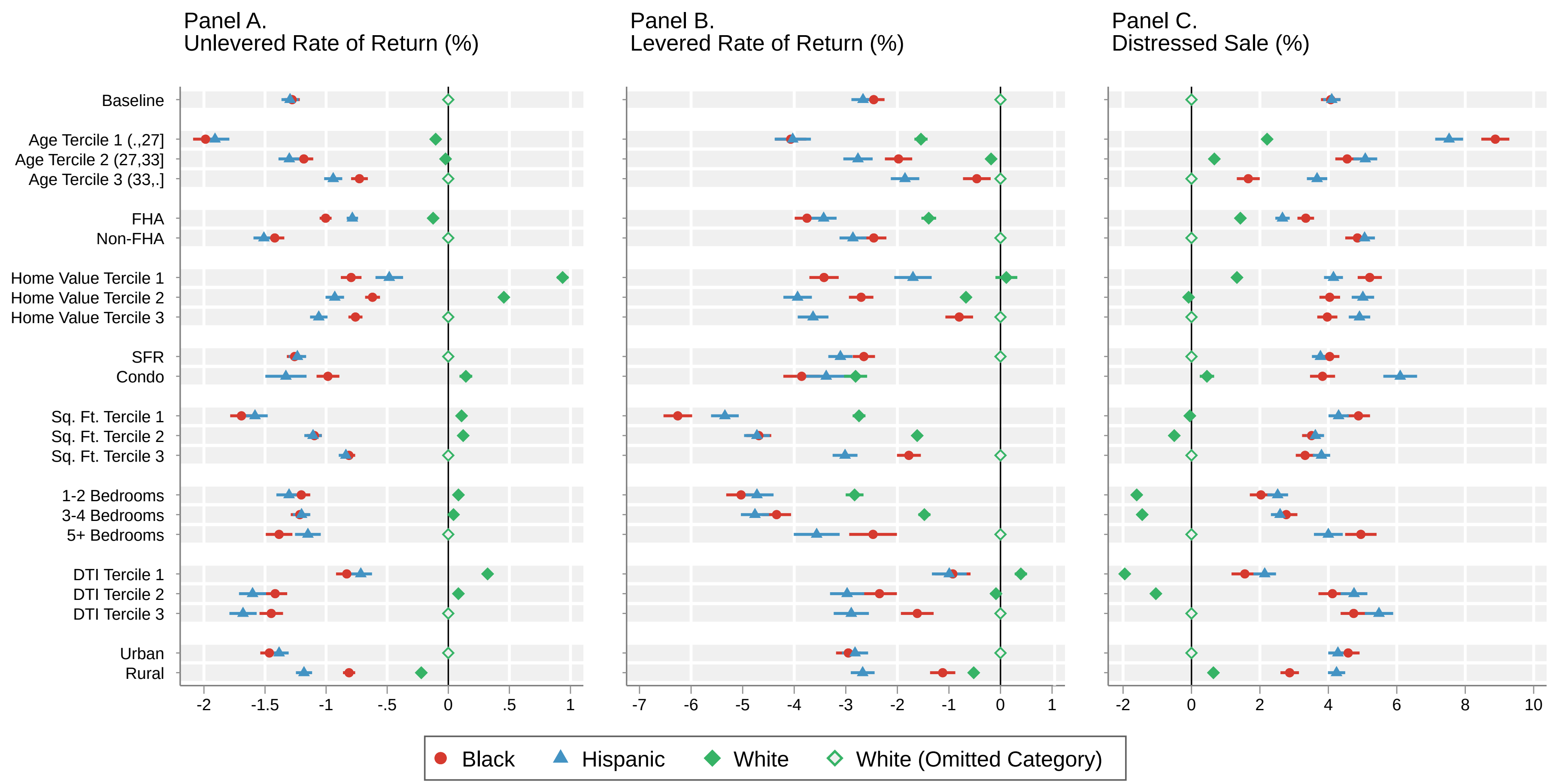
Notes: This figure plots racial differences in foreclosure rates as a function of local economic conditions. The foreclosure rate is computed as the share of properties in our baseline sample that end in a foreclosure in a given year and county. We compute the Black-White and Hispanic-White difference in foreclosure rates for each county and year. Each panel presents a binned scatterplot of racial differences in foreclosure rates against a measure of local economic conditions. In Panels A and C, local economic conditions are measured as the county unemployment rate from BLS. In Panels B and D, local conditions are measured as house price growth in the preceding 3 years. Panels C and D restrict to county-years in 2012 and later. β denotes slope of estimated linear fit. Data are from baseline sample of ownership spells described in Section 2.

Figure A10: Racial Differences in Employment Cyclicality



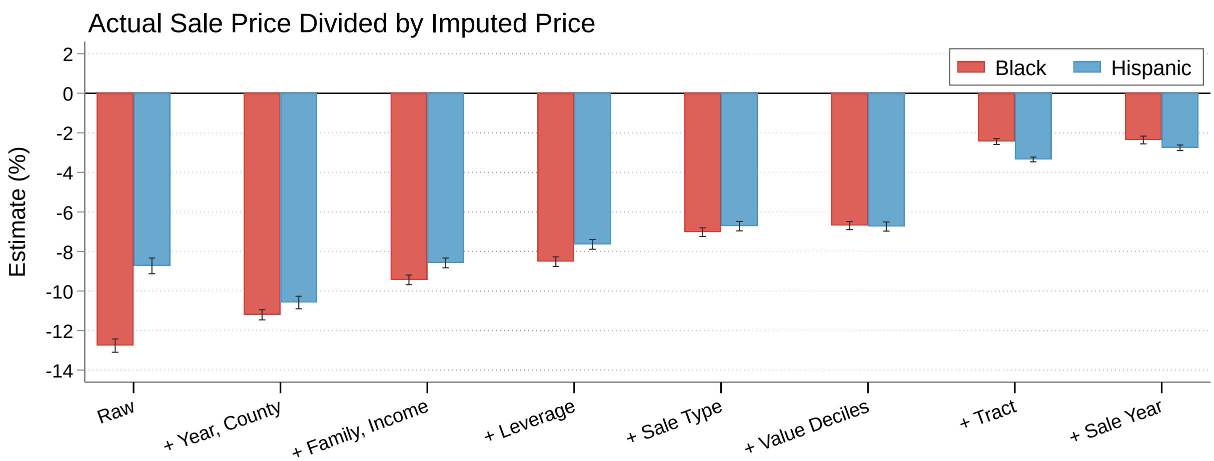
Notes: This figure plots the relationship between race-specific changes in county-level employment rates and changes in employment rates pooling the three racial groups. Panel A presents results including data from 2000 to 2019. Panel B restricts to years in 2007 and before. Panel C resetricts to years in 2012 and after. Yearly employment counts are measured using Quarterly Workforce Indicator data from the US Census. Yearly working-age population counts are derived from Census intercensal popualtion estimates. Binned scatterplot and linear fit restrict to changes in employment rate between -10 and 10 percetnage points. County-race-year observations are weighted by working age population.

Figure A11: Heterogeneous Racial Gaps



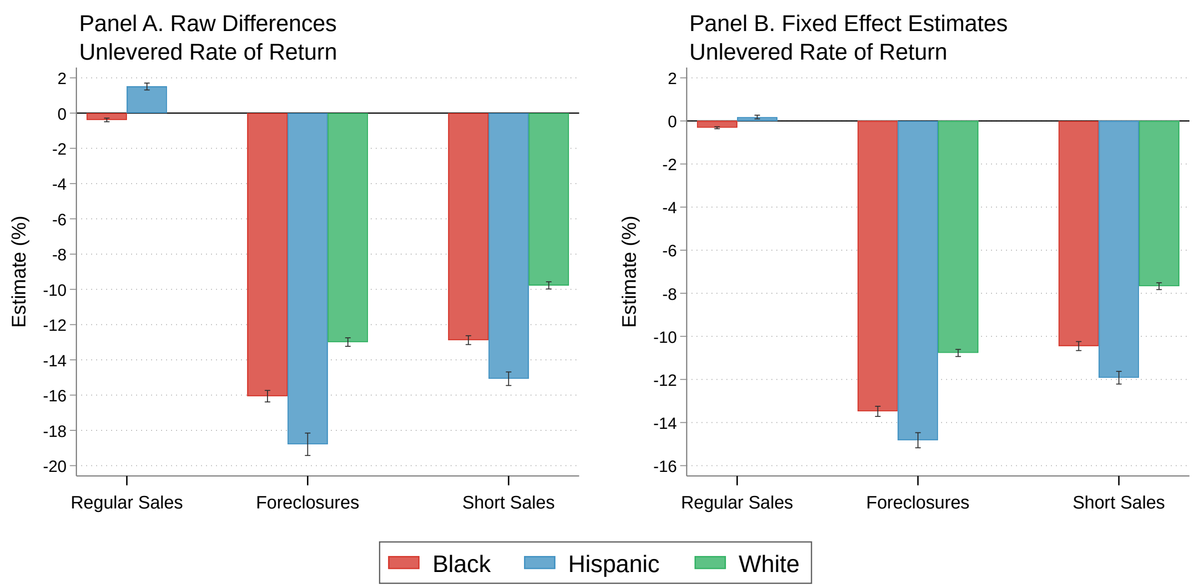
Notes: This figure documents heterogeneity in the racial gap in annual unlevered housing returns (Panel A), levered housing returns (Panel B), and distressed sales (Panel C). Each dimension of heterogeneity provides estimates from a separate regression (Equation 5). Regressions include fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, fixed effects that interact county, purchase year, and deciles of income at home purchase, and fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. Points denote estimated coefficients of race/ethnicity indicators interacted with homeowner characteristics (e.g., indicators for income tercile). *Baseline* denotes the full analysis sample. *Age* refers to a proxy of age defined as the age of the homeowner's oldest trade in the Equifax data plus 18 years. *FHA* denotes indicates the mortgage is identified as a loan from the Federal Housing Administration in the property data. *Home Value Tercile* denotes tercile of purchase price within county-year. *SFR* and *Condo* indicate that the property is a single-family residence and condominium, respectively. *Sq. Ft.* denotes interior square footage. *Bedrooms* refers to the number of bedrooms in the property. *DTI Tercile* denotes tercile of back-end debt-to-income ratio, measured at origination in mortgage servicing records provided by McDash. *Urban* denotes tracts in which all constituent Census blocks are urban, according to 2010 Census definitions, while *Rural* denotes tracts with at least one rural block. Data are from baseline sample of ownership spells described in Section 2. Standard errors are clustered within purchase year, sale year, and county cells.

Figure A12: Racial Gaps in Distressed Sale Discounts



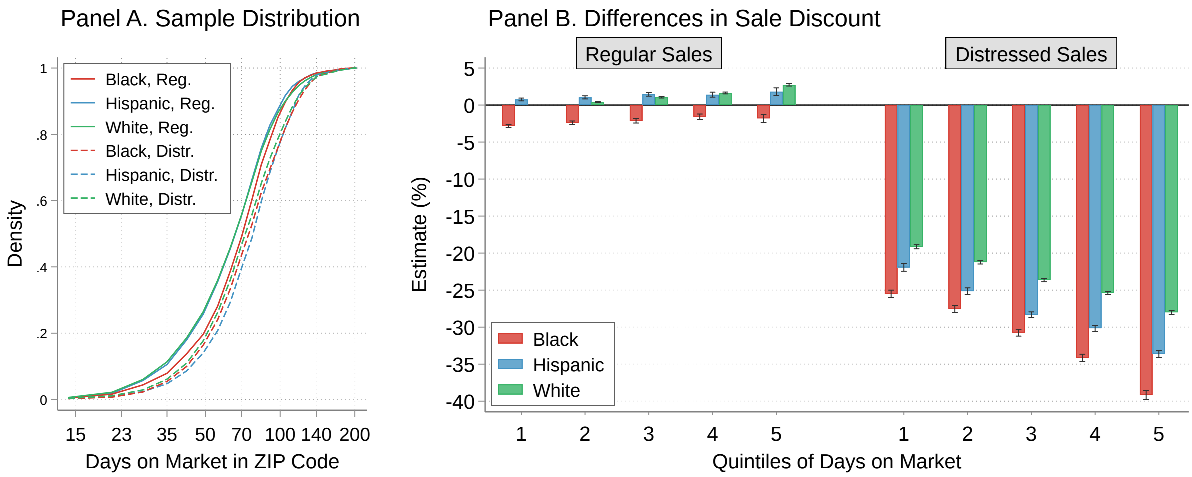
Notes: This figure presents estimates of racial gaps in distressed sale discounts. Each pair of bars corresponds to a separate regression and indicates estimated coefficients of Black and Hispanic indicators from Equation 5 using a particular set of fixed effects. The outcome is a property's actual sale price divided by its imputed value, computed by inflating purchase price by the FHFA county-level home price index. Sample is restricted to properties sold in a distressed sale. *Raw* denotes estimates without controls. *+ Year, County* adds in fixed effects that interact purchase year, sale year, and county. *+ Family, Income* adds fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, and fixed effects that interact county, purchase year, and deciles of income at home purchase. *Leverage* adds fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. *Sale Type* adds fixed effects that interact purchase year, county, and an indicator that a sale is a short sale. *Value Deciles* adds fixed effects that interact purchase year, county, and deciles of property purchase price within county and purchase year. *Tract* adds fixed effects that interact purchase year and Census tract. *Sale Year* adds fixed effects that interact purchase year, sale year, and Census tract. Standard errors are clustered within purchase year, sale year, and county cells. Data are from baseline sample of ownership spells described in Section 2, restricted to distressed sales.

Figure A13: Gap in Housing Returns by Distressed Sale Type



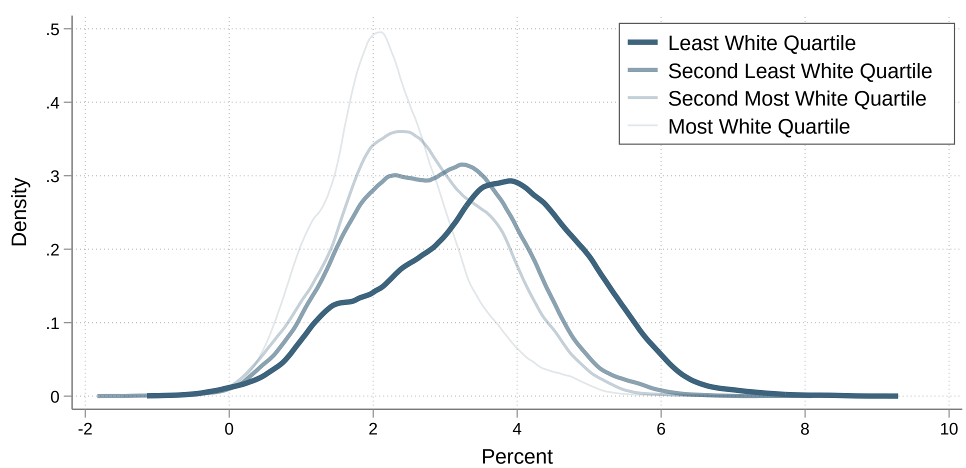
Notes: This figure presents estimates of racial gaps in housing returns. Each panel contains estimates from a separate regression (Equation 5). Bars depict estimated coefficients of Black and Hispanic indicators interacted with the sale type (regular, foreclosure, short sale). Panel A presents raw differences. Panel B includes fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, fixed effects that interact county, purchase year, and deciles of income at home purchase, and fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. The outcome in both panels is the unlevered rate of return (Equation 1). Data are from baseline sample of ownership spells described in Section 2. Standard errors are clustered within purchase year, sale year, and county cells.

Figure A14: Heterogeneity in Sale Discounts by Market Thickness



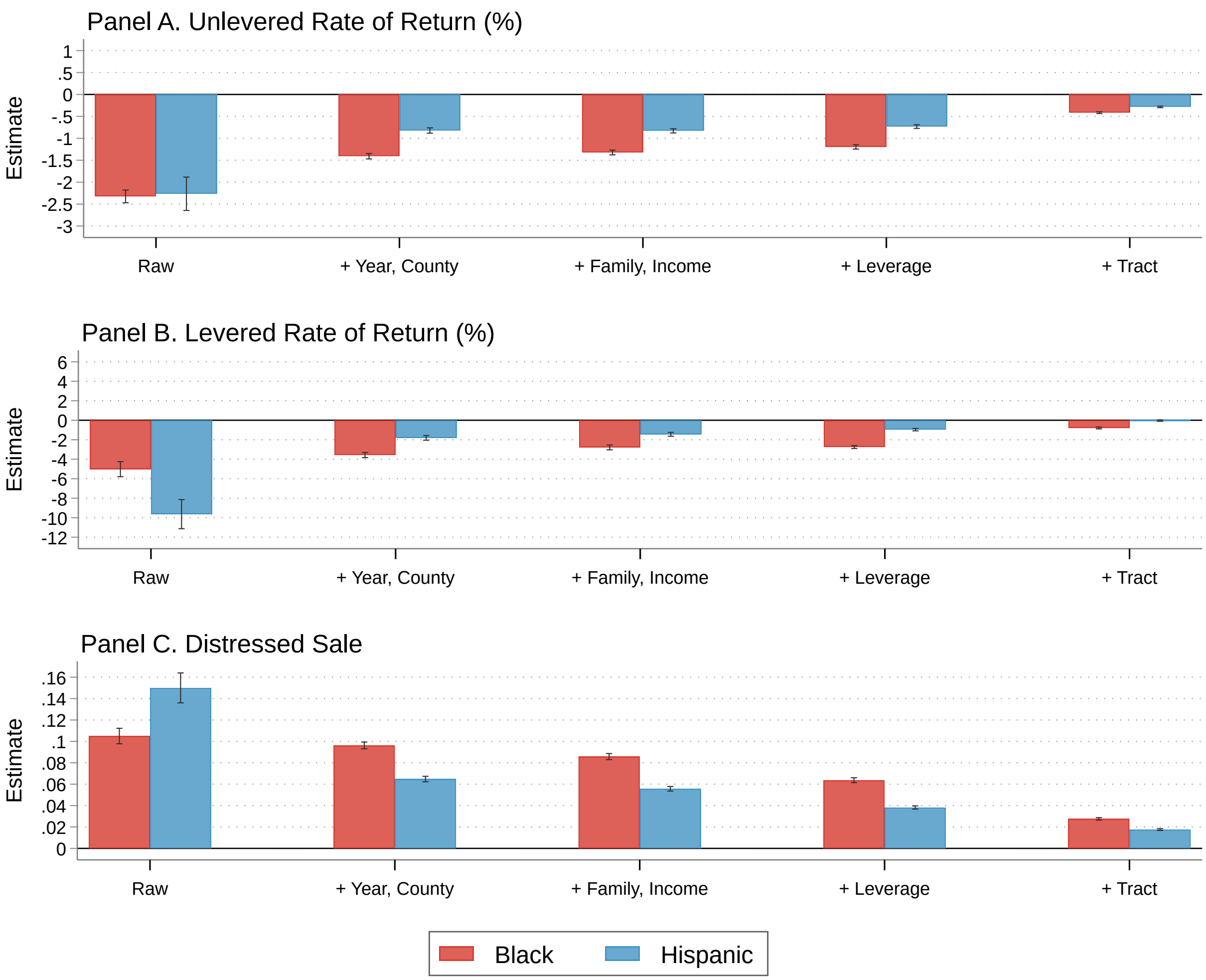
Notes: This figure presents racial differences in exposure to housing market thickness and estimates of racial gaps in annualized unlevered housing returns split by housing market thickness and sale type. Housing market thickness is measured by the median days on market of homes sold in a ZIP code, computed using MLS data. Panel A plots the CDF of days on market, split by racial group and sale type (distressed vs. regular). Panel B presents coefficients from a regression that interacts race indicators, sale type, and quintiles of days on market (Equation 5). The omitted category is White homeowners living in the quintile of ZIP codes in the thickest housing markets. The outcome is a property's actual sale price divided by its imputed value, computed by inflating purchase price by the FHFA ZIP-level home price index. The specification includes fixed effects that interact county, purchase year, and sale year. Data are from baseline sample of ownership spells described in Section 2, restricted to properties that are sold within sample window and ZIP-years merged with the MLS data. Standard errors are clustered within purchase year, sale year, and county cells.

Figure A15: Average House Price Growth 2001-2020 by Census Tract Demographics



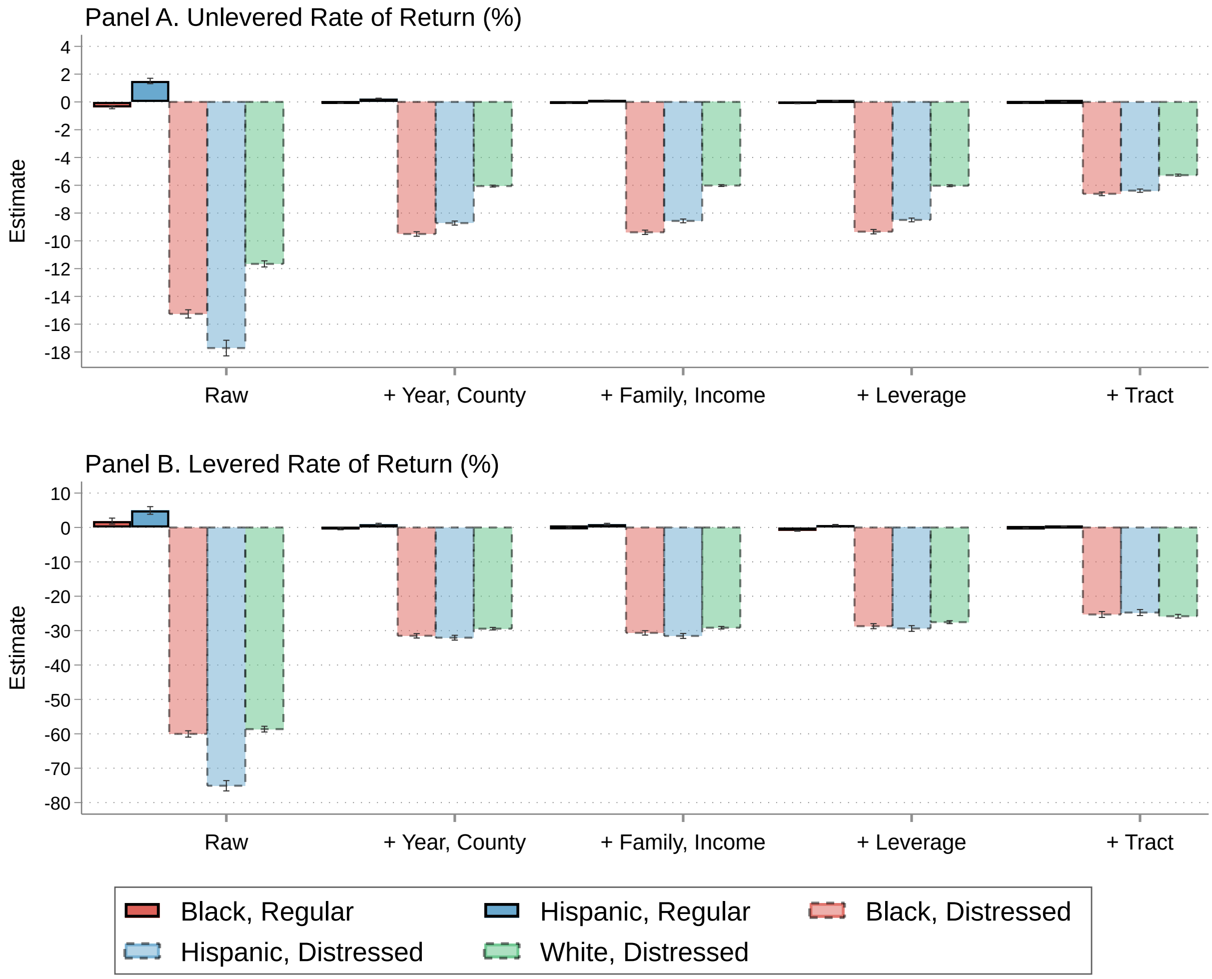
Notes: This figure presents the distribution of annual house price growth between 2001 and 2020 in kernel density form for US Census tracts. Tracts are categorized into quartiles of the share of homeowners in each tract identifying as non-Hispanic White in the 2010 Census. Tract house price growth is measured using tract-level FHFA house price index (Bogin et al. 2019). These distributions indicate that tracts with more minority homeowners were more exposed to rapid levels of house price growth between 2001 and 2020.

Figure A16: Racial Gaps Controlling for Sale Year



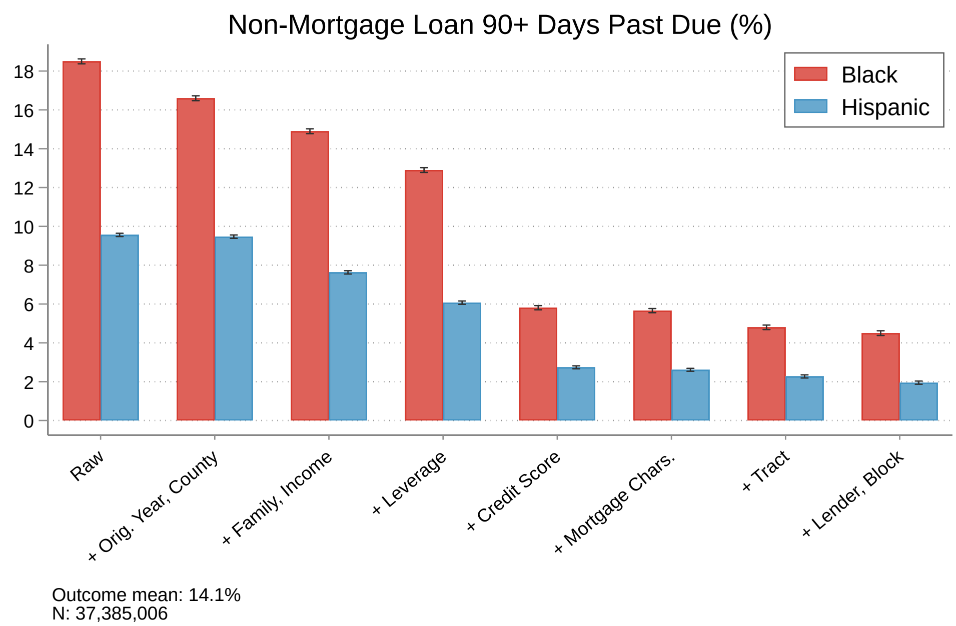
Notes: This figure presents estimates of racial gaps in housing returns and distressed sales. Each pair of bars corresponds to a separate regression and indicates estimated coefficients of Black and Hispanic indicators from Equation 5 using a particular set of fixed effects. *Raw* denotes estimates without controls. *+ Year, County* adds in fixed effects that interact purchase year, sale year, and county. *+ Family, Income* adds fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, and fixed effects that interact county, purchase year, and deciles of income at home purchase. *Leverage* adds fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. *Tract* adds fixed effects that interact purchase year, sale year, and Census tract. The outcomes are the annualized unlevered rate of return (Panel A), the annualized levered rate of return (Panel B), and an indicator that a property is sold in a distressed sale (Panel C). Standard errors are clustered within purchase year, sale year, and county cells. Data are from baseline sample of ownership spells described in Section 2.

Figure A17: Racial Gaps Controlling for Sale Year, Continued



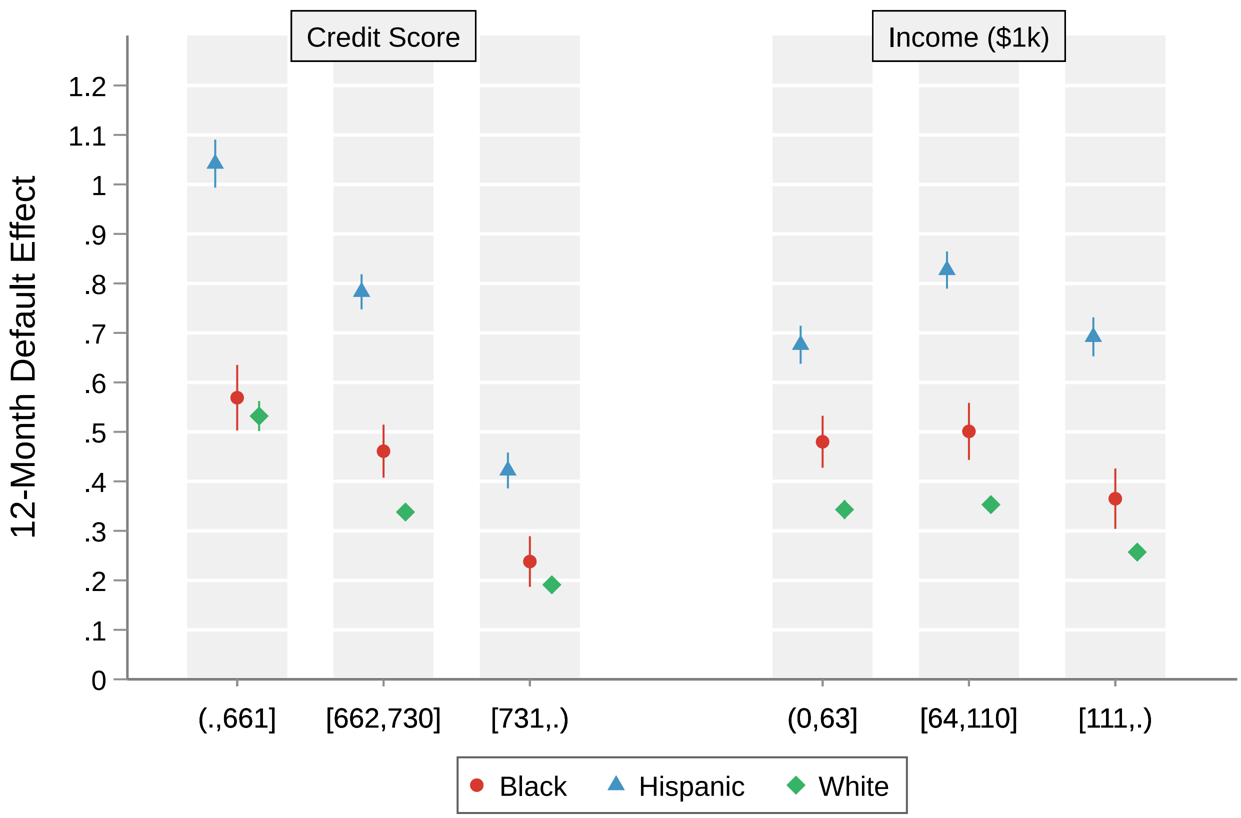
Notes: This figure presents estimates of racial gaps in housing returns and distressed sales. Each set of bars corresponds to coefficients of race indicators interacted with an indicator that a property is sold in a distressed sale. *Raw* denotes estimates without controls. *+ Year, County* adds in fixed effects that interact purchase year, sale year, and county. *+ Family, Income* adds fixed effects that interact county, purchase year, gender, and an indicator for the presence of a mortgage co-applicant, and fixed effects that interact county, purchase year, and deciles of income at home purchase. *Leverage* adds fixed effects that interact county, purchase year, and 1 percentage point bins of combined-loan-to-value at purchase. *Tract* adds fixed effects that interact purchase year, sale year, and Census tract. The outcomes are the annualized unlevered rate of return (Panel A), the annualized levered rate of return (Panel B), and an indicator that a property is sold in a distressed sale (Panel C). Standard errors are clustered within purchase year, sale year, and county cells. Data are from baseline sample of ownership spells described in Section 2.

Figure A18: Racial Disparities in Non-Mortgage Default



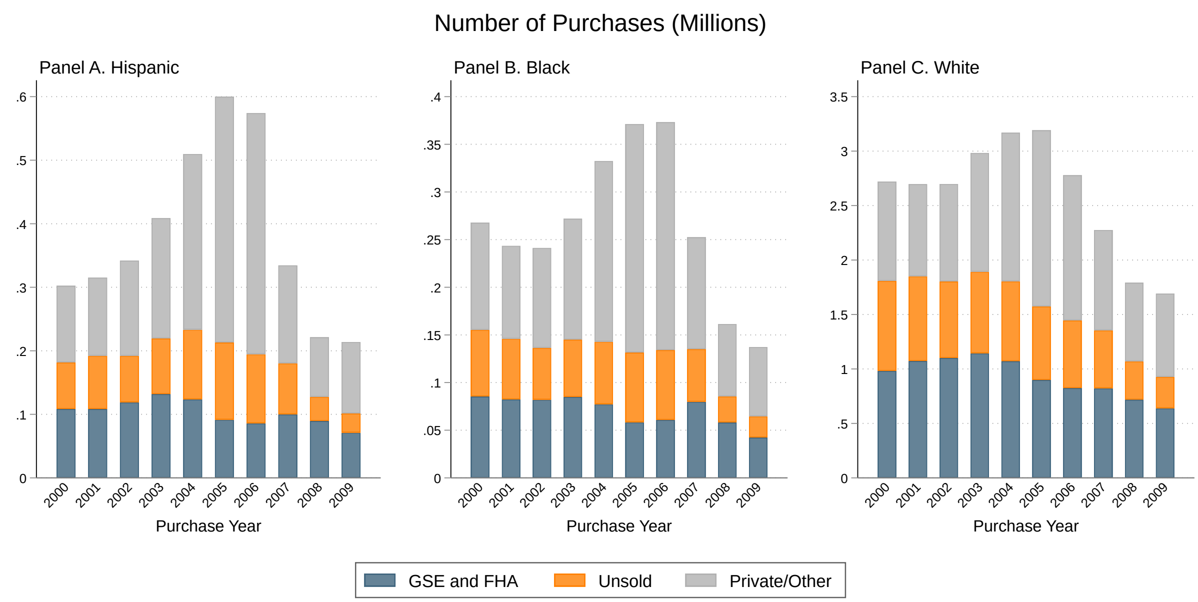
Notes: This figure presents estimates of racial differences in financial distress controlling for a range of observable homeowner characteristics (Equation 5). These estimates decompose racial differences in 90-day non-mortgage default into components that correspond to household, loan, and location characteristics. Each bar corresponds to the coefficient on a race/ethnicity indicator. Each pair of bars correspond to a separate regression with a particular set of covariates. *Raw* denotes a regression of the outcome on race/ethnicity indicators and current year fixed effects. *Orig. Year, County* adds fixed effects that interact current year, origination year, and county. *Family, Income* adds fixed effects that interact gender, the presence of a co-applicant on the mortgage, and current year, and fixed effects that interact deciles of income and current year. *Leverage* adds fixed effects that interact 1 percentage point bins of original loan-to-value and current year. *Credit Score* adds fixed effects that interact 10 point credit score bins and current year. *Mortgage Chars.* controls for a spline in current loan-to-value, log monthly payment, log interest rate, an indicators for interest-only loan, refinance, adjustable rate mortgage, and GSE loan, and adds fixed effects for mortgage term, deciles of original loan value, and deciles of debt-to-income. *Tract* adds Census tract fixed effects. *Lender, Block* adds Census block fixed effects. Standard errors are clustered at the loan level. Data are from a panel of homeowners with linked credit bureau and mortgage servicing records described in Section 5.

Figure A19: Heterogeneous Effects of Payment Shock on 90-Day Default



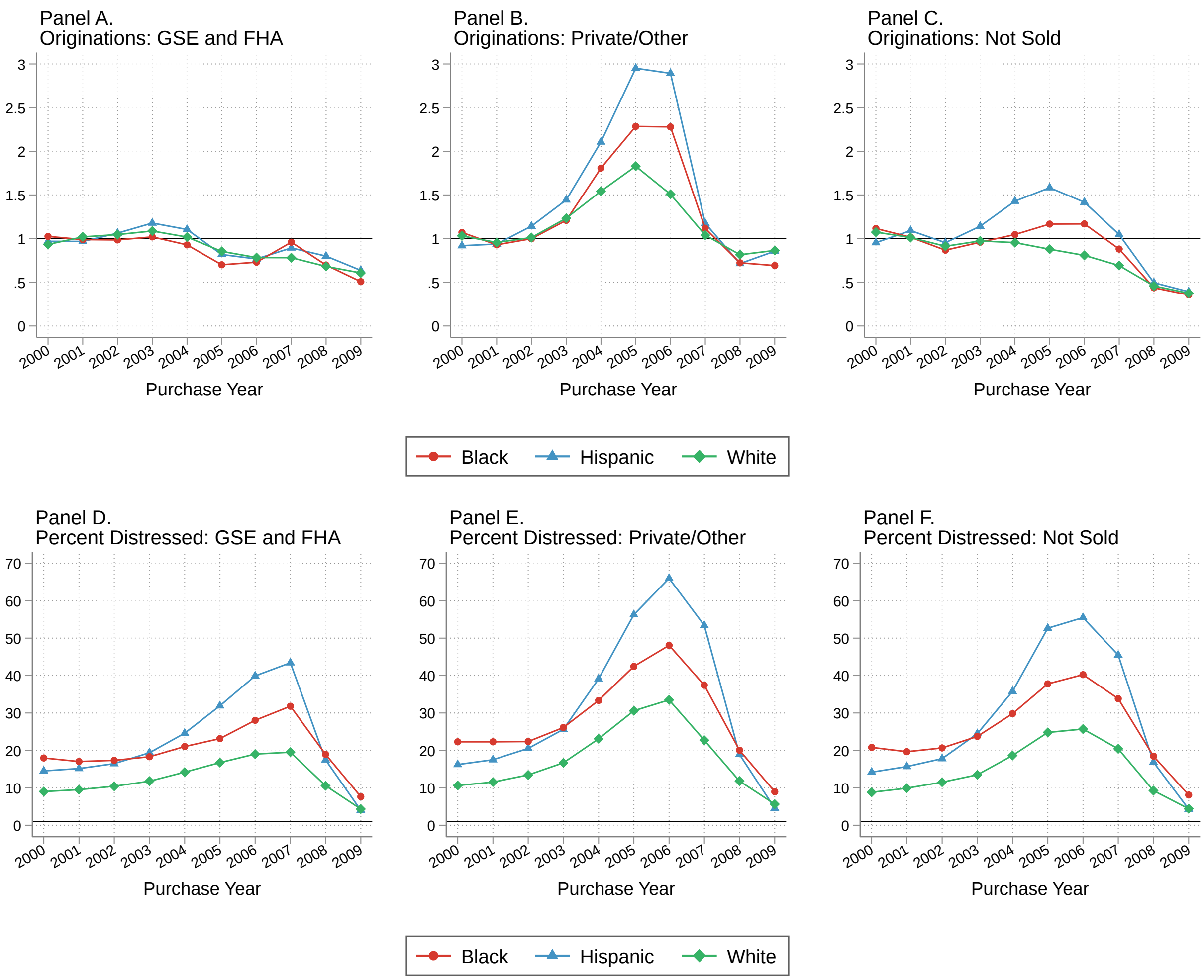
Notes: This figure documents heterogeneous impacts of monthly payment shocks on monthly 90-day mortgage default rates following the reset of an adjustable rate mortgage. Default rates are measured as an indicator that the homeowner's primary mortgage is 90 or more days past due. Coefficients correspond to effects after 12 months and are scaled to reflect percentage point impacts of a 1 percent increase in monthly payments. *Credit Score* splits the sample by terciles of credit score at origination. *Income ($1k)* splits the sample by terciles of annual income at origination. Standard errors are clustered at loan level. Data come from panel of homeowners with linked credit bureau and mortgage servicing records with adjustable rate mortgages who experience a rate reset. See Section 5 for more details.

Figure A20: Changing Composition of Purchases by Mortgage Investor Channel



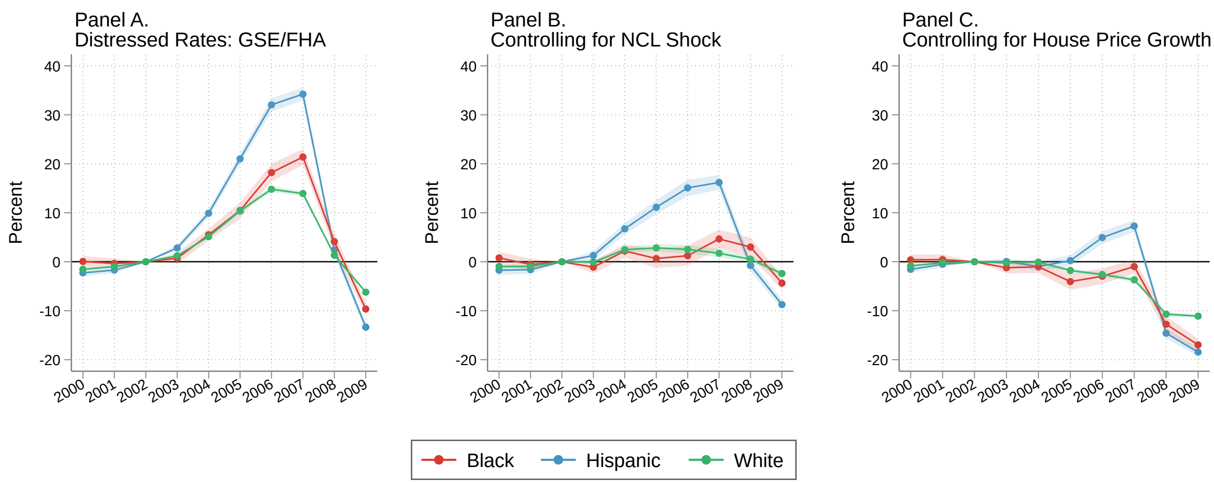
Notes: This figure presents stacked bar charts representing the number of purchases each year from 2000-2009 by race/ethnicity and mortgage investor channel. Each bar represents the total number of purchases for a given racial group in a given year. Mortgage investor channel is identified from HMDA records. *GSE and FHA* denotes mortgages that were purchased by Fannie Mae, Freddie Mac, or the Federal Housing Adminsitratiion. *Unsold* denotes mortgages that were not sold off of the originator's balance sheet in the calendar year covered by the HMDA data. *Private/Other* denotes all other loans. Data come from baseline sample of ownership spells described in Section 2.

Figure A21: Aggregate Trends in Purchases and Distressed Sales by Mortgage Investor Channel



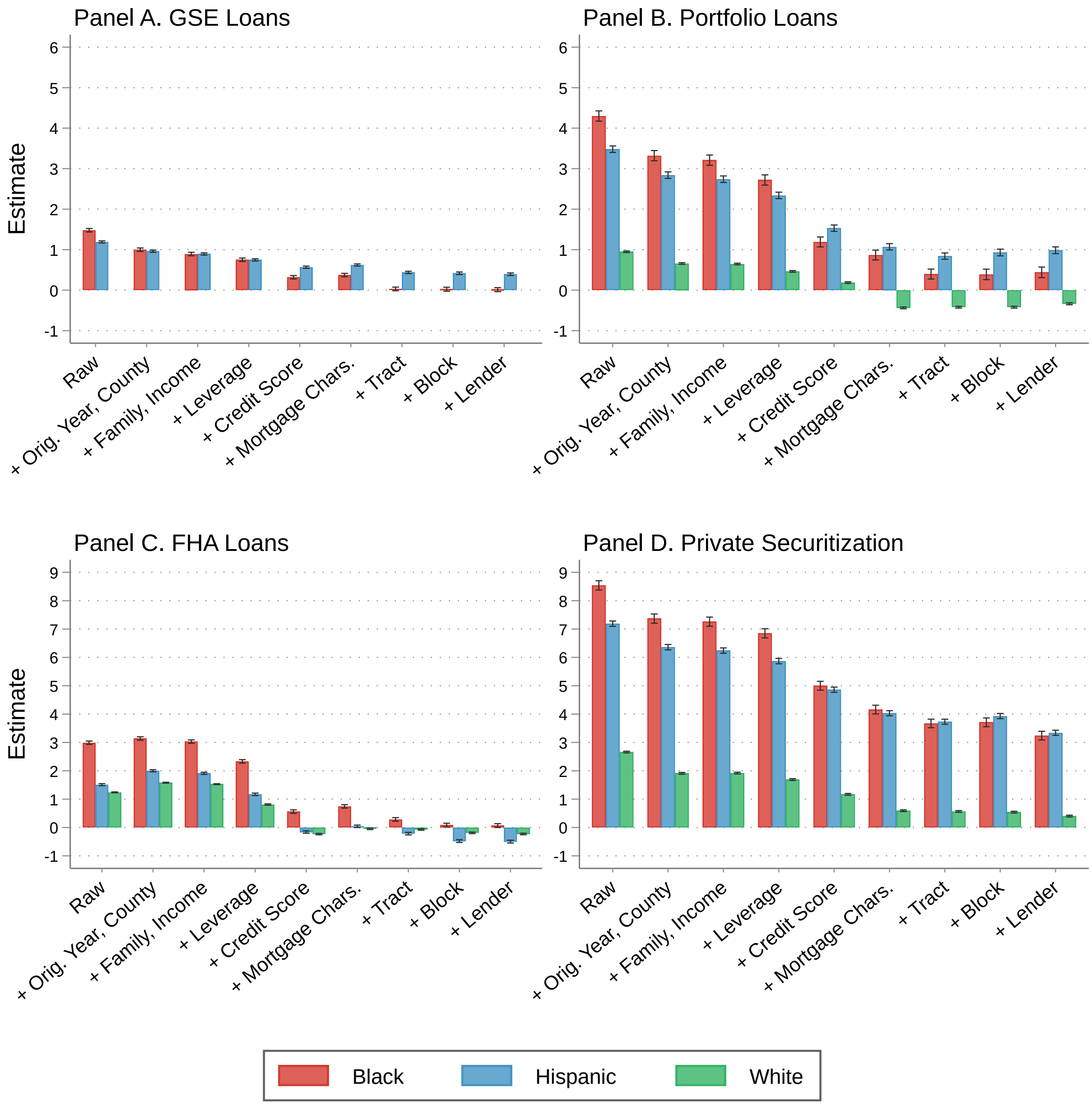
Notes: This figure presents aggregate trends by race/ethnicity and mortgage investor channel. Panels A, B, and C plot the number of purchase mortgage originations in a given year by race using HMDA data. Originations are normalized to the mean between 2000-2002. Panels D, E, and F plot the percent of homes purchased in a given year that end in distress using data from baseline sample of ownership spells described in Section 2. Mortgage investor channel is identified from HMDA records. *GSE and FHA* denotes mortgages that were purchased by Fannie Mae, Freddie Mac, or the Federal Housing Adminstration. *Unsold* denotes mortgages that were not sold off of the originator's balance sheet in the calendar year covered by the HMDA data. *Private/Other* denotes all other loans.

Figure A22: Impacts of Credit Expansion and House Prices on GSE/FHA Home Purchases



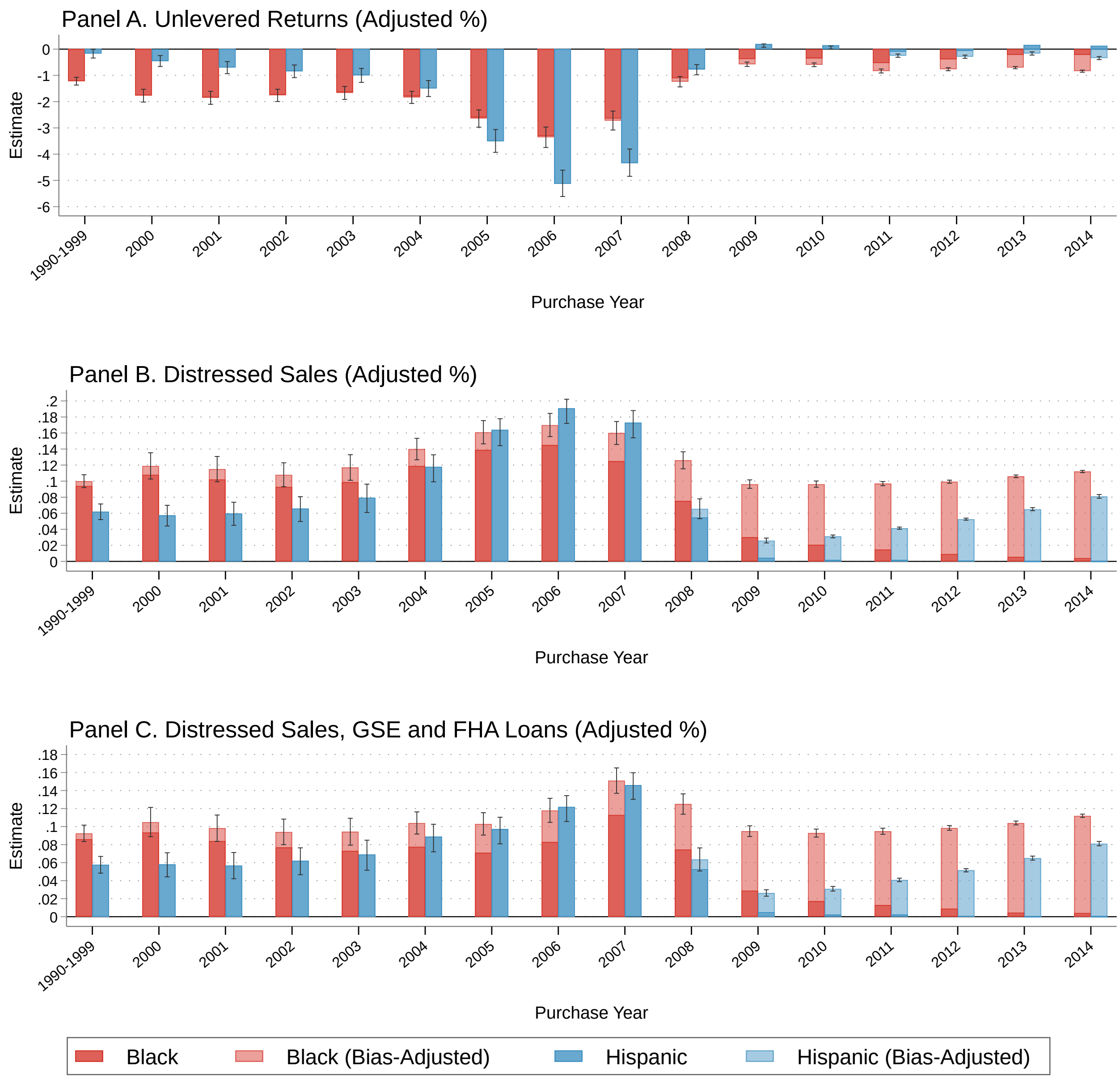
Notes: This figure depicts the time path of rates of distressed sales by race/ethnicity among homes purchased with GSE/FHA loans by purchase year, relative to homes purchased in 2002. The outcome is the percent of properties (ownership spells) purchased in a given ZIP code and year that are later sold in a distressed sale. Panel A presents estimates of the time path by race/ethnicity without controls, analogous to Panel D of Figure 7. Panel B adds controls for the early-2000s expansion of credit by interacting year indicators with the 2002 exposure of ZIP codes to non-core-liability (NCL) lenders from Mian and Sufi (2022), normalized to the bottom decile of exposure. Coefficients in Panel B can be interpreted as the relative increase in distressed sales for homes purchased in a given year by a given race relative to homes purchased in 2002, in the bottom decile of exposure to NCL lenders. Panel C controls for a linear spline in house price growth in the five years following purchase with a knot at 0. Data are from baseline sample of ownership spells described in Section 2 aggregated to the ZIP code level. ZIP codes are weighted by the mean number of purchases between 2000 and 2003. Standard errors are clustered within ZIP code.

Figure A23: Racial Differences in Mortgage Default Interacted with Mortgage Investor Channel



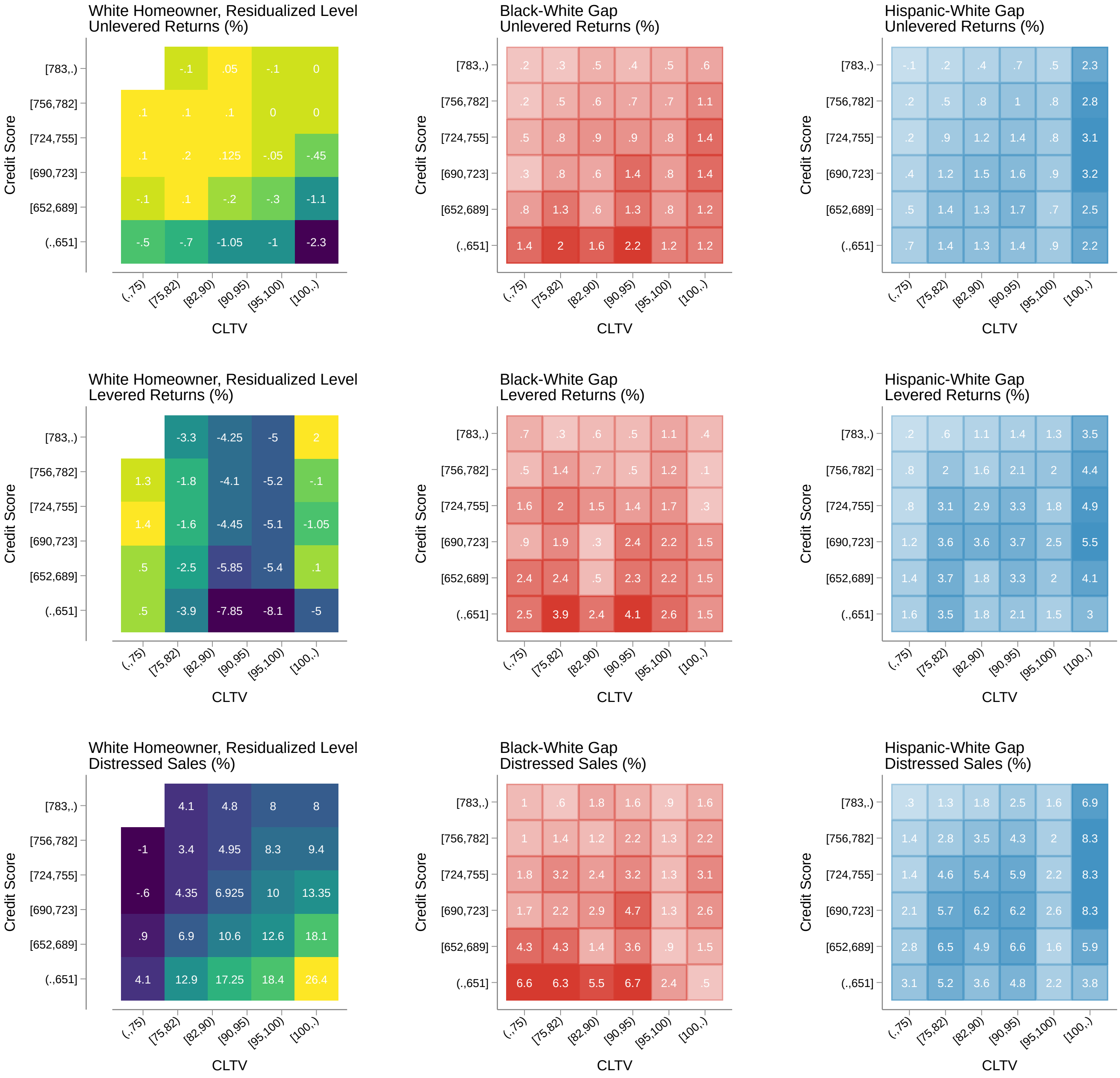
Notes: This figure presents estimates of racial differences in financial distress across mortgage investor channel, controlling for a range of observable homeowner characteristics. Estimates are derived from nine separate regressions, with eleven coefficients for each regression split across four panels, such that each panel presents coefficients for race indicators interacted with a given investor channel. Panel A corresponds to mortgages purchased by a government-sponsored enterprise (GSE); Panel B to mortgages held on a lender portfolio; Panel C to FHA mortgages; Panel D to privately securitized mortgages. The omitted category is mortgages held by White homeowners that were purchased by a GSE. *Raw* denotes a regression of the outcome on race/ethnicity indicators and current year fixed effects. *Orig. Year, County* adds fixed effects that interact current year, origination year, and county. *Family, Income* adds fixed effects that interact gender, the presence of a co-applicant on the mortgage, and current year, and fixed effects that interact deciles of income and current year. *Leverage* adds fixed effects that interact 1 percentage point bins of original loan-to-value and current year. *Credit Score* adds fixed effects that interact 10 point credit score bins and current year. *Mortgage Chars.* controls for a spline in current loan-to-value, log monthly payment, log interest rate, an indicators for interest-only loan, refinance, and adjustable rate mortgage, and adds fixed effects for mortgage term, deciles of original loan value, and deciles of debt-to-income. *Tract* adds Census tract fixed effects. *Block* adds Census block fixed effects. *Lender* adds lender fixed effects. Standard errors are clustered at the loan level. Data are from a panel of homeowners with linked credit bureau and mortgage servicing records described in Section 5.

Figure A24: Gaps in Returns and Distress by Purchase Year



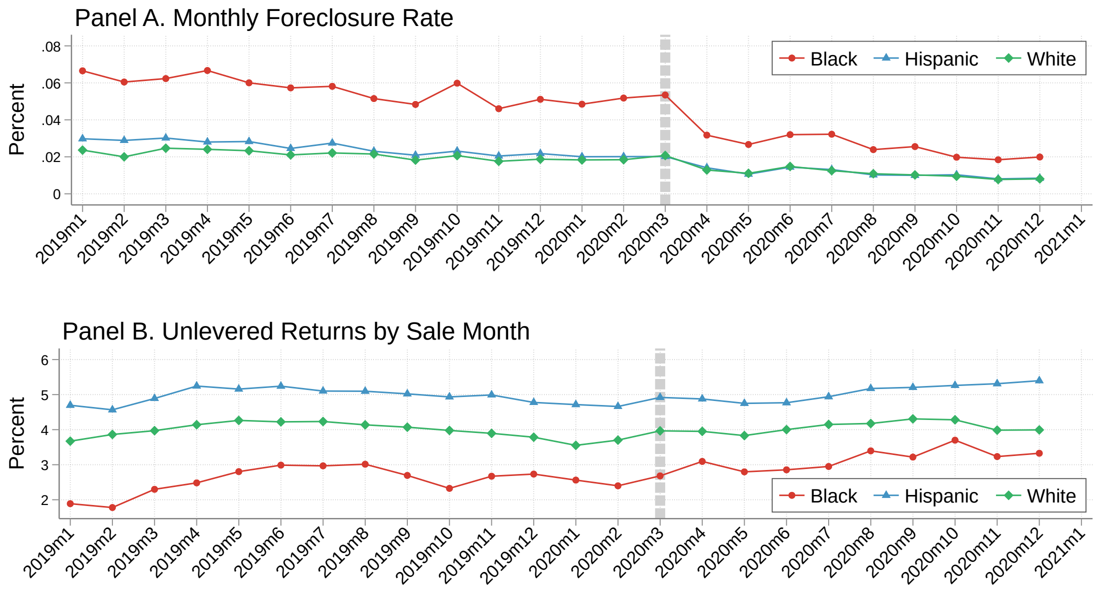
Notes: This figure presents estimates of racial gaps in annualized unlevered housing returns and distressed sale rates from regression specifications that compare homeowners living in the same county (Equation 5 estimated with county fixed effects). Each pair of bars corresponds to a separate regression, and each bar denotes an estimated coefficient corresponding to a race/ethnicity indicator. The outcome in Panel A is annualized unlevered housing returns, and the outcome in Panels B and C is distressed sale rate. In all panels, outcomes are adjusted to account for distressed sales that occur out of our sample window using the procedure described in Section 3.1. Darkly shaded region denotes size of unadjusted gaps. Panel C restricts to purchases that were originated via a GSE or the FHA. Data are from baseline sample of ownership spells described in Section 2. Standard errors are clustered within purchase year, sale year, and county cells.

Figure A25: Heat Map by Credit Score and Leverage



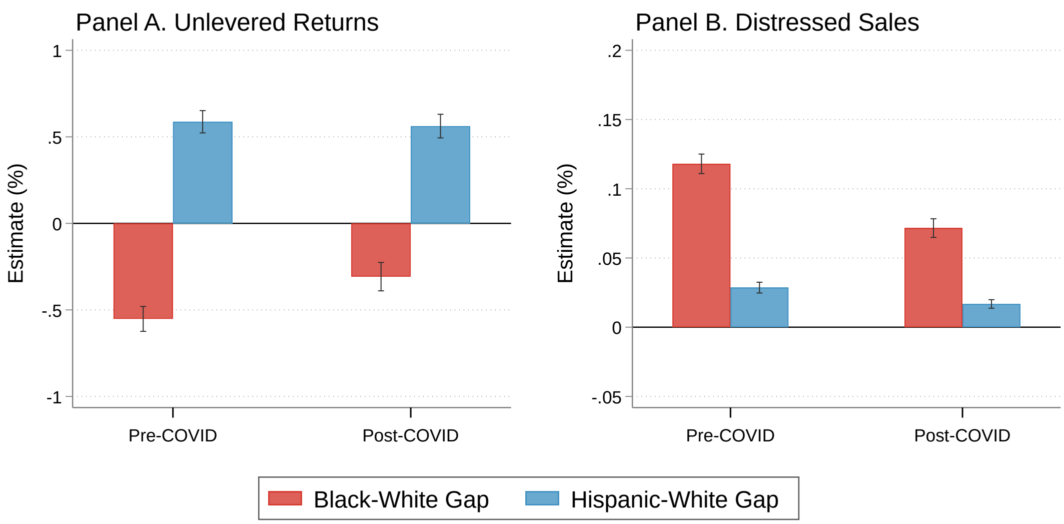
Notes: This figure presents estimates of racial differences in housing returns and distressed sales among homeowners by credit score and initial leverage. Credit score is measured at home purchase, and leverage is defined as combined loan-to-value (CLTV) at purchase. The first row presents results for annual unlevered returns. The second row presents results for annual levered returns. The third row presents results for distressed sales. The first column presents average outcomes for White homeowners, residualizing by county and purchase year and normalizing to the average value for the highest-score and lowest-CLTV cell. The second and third columns present Black-White and Hispanic-White differences, respectively. Within each panel, the color of a square indicates the size of the estimated coefficient, with each square corresponding to a coefficient for race/ethnicity indicators estimated from a separate regression within each score-CLTV cell. Regressions in Columns 2 and 3 are estimated as in Equation 5 with fixed effects that interact county and purchase year. Data are from baseline sample of ownership spells described in Section 2, restricted to properties merged to the McDash servicing records.

Figure A26: Aggregate Time Series of Foreclosures and Returns During the COVID-19 Pandemic



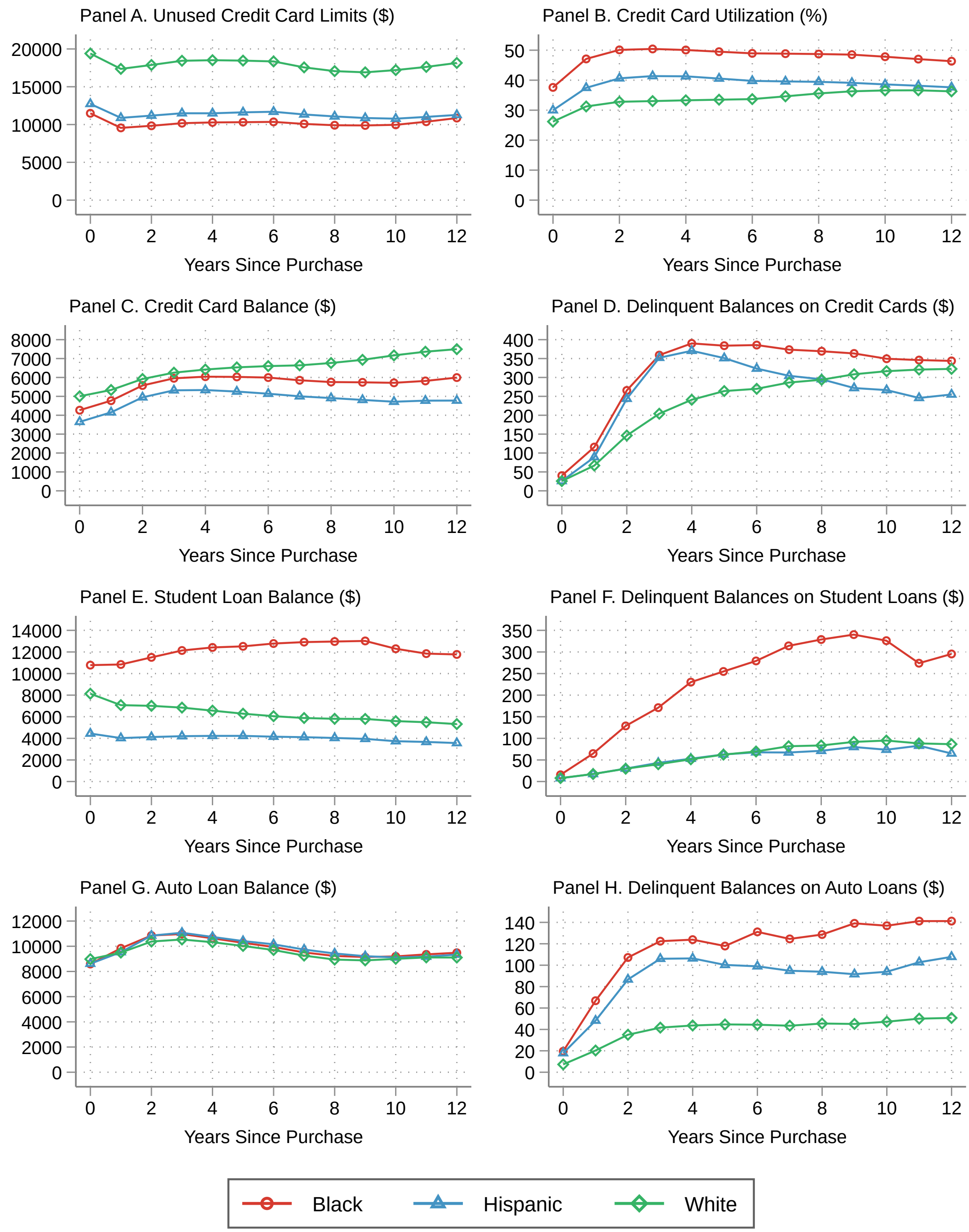
Notes: This figure plots aggregate trends in foreclosures and housing returns around the onset of the COVID-19 pandemic. Panel A plots the monthly foreclosure rate by race/ethnicity. Panel B plots average annualized unlevered returns among properties sold in a given month, by race/ethnicity. Data are from baseline sample of ownership spells described in Section 2, extended through 2020. Panel B restricts to properties with an observed sale.

Figure A27: Changing Racial Gaps After the COVID-19 Pandemic



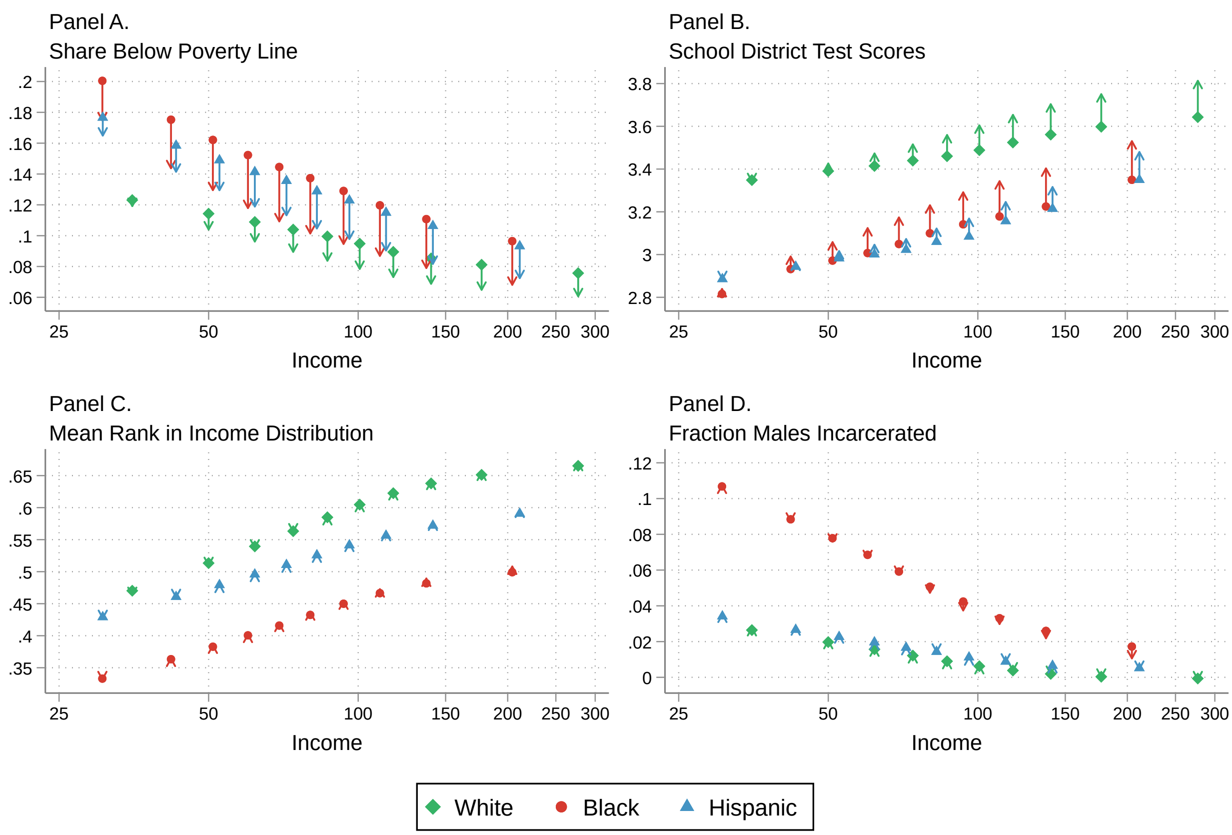
Notes: This figure plots racial gaps in housing returns immediately before and after the onset of the COVID-19 pandemic. Both panels present estimates of Equation 5 separately for the nine months immediately before and after the COVID pandemic (i.e., July 2019 to March 2020 and April 2020 to December 2020). This specification compares homeowners living in the same county and buying and selling their homes in the same year. Panel A plots results for annualized unlevered returns, and Panel B plots results for annualized levered returns. Data come from baseline sample of ownership spells described in Section 2, expanded to include all months in 2020 as well as ownership spells that begin in 2015 and 2016. Standard errors are clustered within purchase year, sale year, and county cells.

Figure A28: Racial Disparities in Financial Distress by Tenure Length



Notes: This figure presents financial outcomes by race/ethnicity as a function of the number of months since home purchase. The financial outcomes are dollar amount of unused credit card limits (Panel A), credit card utilization in percent (Panel B), credit card balances (Panel C), balances 30 or more days past due on credit cards (Panel D), student loan balances (Panel E), student loan balances 30 or more days past due (Panel F), auto loan balances (Panel G), auto loan balances 30 or more days past due (Panel H). Credit card utilization is conditional on having an open credit card. Homeowners without credit cards are coded as having $0 in unused limits. Data are from a panel of homeowners with linked credit bureau and mortgage servicing records described in Section 5.

Figure A29: Upgrades in Neighborhood Quality from Home Purchase



Notes: This figure depicts changes in neighborhood quality associated with home purchases, illustrating the modest upgrades in neighborhood quality achieved by minority homeowners relative to the neighborhood quality achieved by White homeowners. Each panel corresponds to a different measure of neighborhood quality. Panel A measures the share of homeowners in the Census tract below the federal poverty line in the 2006-2010 ACS. Panel B measures school district standardized 3rd grade math test scores in 2013. Panel C measures the mean rank in the national income distribution of children born in 1978-1983 to parents of the same race/ethnicity and income percentile as that reported by the homeowner in their mortgage application. Panel D measures the 2010 incarceration rate of male children that were born in 1978-1983 to parents of the same race/ethnicity and income percentile as that reported by the homeowner in their mortgage application. In each panel, homeowners are binned by race/ethnicity and to decile of income at home purchase (deciles computed within race/ethnicity). The base of each arrow corresponds to the quality of neighborhoods from which homeowners depart and the head of each arrow corresponds to the neighborhoods at which homeowners arrive after purchase. Income is measured in 2015 dollars. Homeowner-level data on neighborhood migration come from sample of homeowners linked to address histories described in Section 2. Data on neighborhood characteristics come from Chetty et al. (2018).

C Data Appendix

This section provides additional details on the various data sources we analyze, as well as the algorithm that identifies consecutive sales of properties used to construct the baseline sample of ownership spells. All administrative data linkages were conducted by the Fisher Center for Real Estate and Urban Economics at UC Berkeley.

C.1 Property Records

Our baseline sample is constructed from a linkage between the HMDA mortgage origination records and the ATTOM property transaction records. The ATTOM property data, which link mortgages to properties, are merged with the HMDA records by matching on transaction year, Census tract, dollar amount, and lender name, and allowing December transactions to match with January transactions. While the ATTOM data cover purchases made through December 2020, the merge between ATTOM and HMDA only covers ownership spells starting in 2016 and earlier. This means that while our baseline sample captures ownership spells in which the property was purchased in, for instance, December 2016 and sold in March 2020, it does not capture ownership spells in which the property was originally purchased in January 2017 and sold at a later date.

We rely on a proprietary algorithm from ATTOM to identify short sales, which appears to closely track external survey-based measures of short sales. Appendix Figure C1, Panel A shows that we can closely replicate this algorithm by defining short sales as those that are likely to have yielded proceeds below the outstanding balance of the mortgage. Panel B plots this percentage over time and shows that the ATTOM categorization closely tracks short sales measured using a monthly survey of real estate agents, reported in Campbell Communications (2011). These surveys are widely referenced by industry professionals (e.g., Mahon 2010). Since short sales by definition take place at prices below the outstanding principal balance, the patterns in Appendix Figure C1 suggest that the algorithm accurately identifies short sales. In addition, Zhang (2019) uses a sample of property transactions provided by DataQuick and classifies 36% of distressed sales as short sales, essentially identical to the 36% classified as short sales in our data. Ferreira and Gyourko (2015) take an alternative approach, categorizing short sales as those with sales proceeds below 90% of the unpaid principal balance. Replicating our analysis following this approach yields very similar results.

Our baseline approach assigns race and ethnicity using the self-reported race and ethnicity of the borrower in the HMDA records. While this offers a nearly ideal measure of homeowner race/ethnicity, this information is missing for about 10% of our main analysis sample, as reported in Appendix Table C1. To probe the extent of potential bias created by this missing information, we impute race/ethnicity using the methods developed by Imai and Khanna (2016) to impute homeowner race/ethnicity from the last name and Census block of the homeowner in the property records, although it classifies a relatively high share of self-reported Black homeowners as White. Appendix Table A3 shows that race/ethnicity imputed from homeowner name aligns closely with self-reported race/ethnicity. Reassuringly, racial/ethnic composition of homeowners with observed self-reported race/ethnicity appears to be similar to that with missing self-reported race/ethnicity, as shown in Appendix Table C1.

Table C1: Cross-Tabulation of Self-Reported and Impuetd Race

	Race Imputed from Name						
	Hispanic	Black	White	Asian	Other	Missing	N
Self-Reported Race							
Hispanic	80.4	1.9	15.0	1.5	0.1	1.1	1,900,047
Black	3.0	54.5	39.7	1.3	0.3	1.3	1,066,134
White	2.8	2.8	91.8	1.3	0.1	1.2	10,888,521
Asian	7.0	2.4	24.5	63.4	1.3	1.5	1,018,691
Other	9.3	8.5	66.1	13.6	1.4	1.1	145,599
Any Non-Missing	13.0	6.4	73.5	5.7	0.2	1.2	15,018,992
Missing	10.3	7.5	74.4	6.3	0.3	1.3	1,501,187

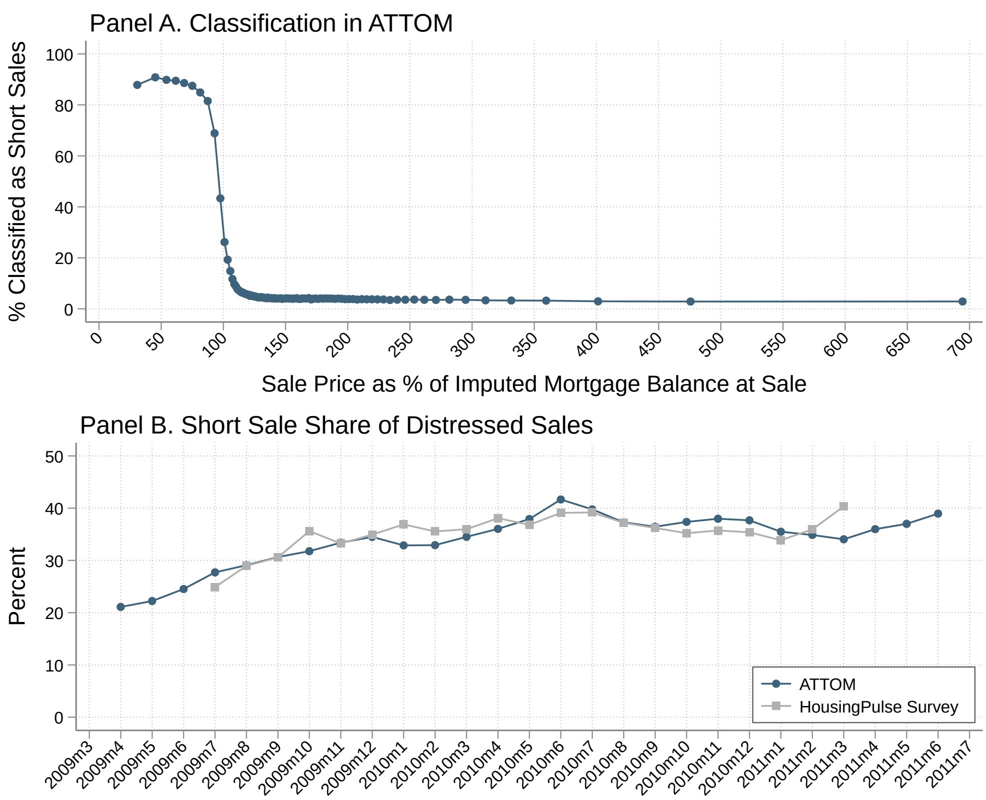
Notes: This table compares the sample distribution by self-reported and imputed race. Each number corresponds to the percent of homeowners assigned to a given race/ethniciy based on their names. Percentages are reported separately by homeowner self-reported race/ethnicity measured in the HMDA data.

To compare our baseline estimates of racial differences in housing returns to those estimated using homeowner name, we construct a sample of ownership spells in the property data that does not restrict to properties merged with the HMDA, but retains the other sample restrictions. Specifically, we drop ownership spells lasting less than 12 months, transactions of less than \$10,000, buyers that are neither persons nor trusts, and purchases with combined loan-to-value of more than 102.5%.

Estimates of racial differences in housing returns and distress using homeowner name are presented in Appendix Table A4. Comparing the average returns among homeowners with missing vs. non-missing self-reported race/ethnicity indicates that the distribution of returns is similar in both samples. This is also the case when computing the racial gap in housing returns by imputing race/ethnicity from names, within the subsample of homeowners with missing self-reported race/ethnicity. Consequently, estimates of Equation 5 which impute race/ethnicity from names when self-reported estimates are missing yield very similar to the baseline estimates.

Although our primary focus is on comparing Black and Hispanic homeowners to White homeowners, differences among other racial/ethnic groups are also of scientific interest. We present estimates of returns and rates of distressed sales for these groups in Appendix Table A15. Average unlevered returns for Asian homeowners are similar for White homeowners (Column 1, Panel A); however, when controlling for county, purchase year, and demographics, returns are markedly lower (Column 2, Panel A). Levered returns are also lower for Asian homeowners (Columns 3 and 4, Panel A). Distressed sales appear to play an important role in explaining these differences: for example, average unlevered returns among non-distressed sales are higher for Asian homeowners (Column 1, Panel B). However, controlling for timing, demographics, and neighborhood, returns among Asian homeowners not realizing a distressed sale are lower than those of White homeowners (Column 2, Panel B). These differences indicate ample scope for future research to uncover mechanisms determining realized housing returns among Asian homeowners.

Figure C1: Short Sale Classification



Notes: This figure illustrates the algorithm used to identify short sales and compares the results of that algorithm to an external measure of short sales. Panel A plots the percent of property sales that are classified by ATTOM as short sales, as a function of the sale price as a percentage of the imputed mortgage balance at sale. Sample excludes sales classified as foreclosures based on the sale documentation. Data are from a sample of homeowners with observed purchase and sale prices in baseline sample of ownership spells described in Section 2. Panel B plots the percentage of distressed sales that are classified as short sales by ATTOM in the baseline sample, along with the percentage classified as short sales in the HousingPulse Survey as reported in Campbell Communications (2011). See Appendix D for discussion of imputation of mortgage balance at sale.

C.2 Algorithm for Identifying Repeat Sales of Properties

We develop an algorithm for identifying repeat sales of properties, allowing us to calculate realized housing returns for each ownership spell. This algorithm distinguishes transaction records that represent mortgages used to purchase a property, transfers of ownership, and mortgage refinances. We identify property purchases by restricting to arm's length, full-consideration transactions that are recorded as home purchases in HMDA. To identify the future sale of a given purchased property among the set of all future transactions of that property, we drop transactions in which the new buyer's name is similar to the original buyer name, and select the first subsequent arm's length full-consideration transaction. A natural language processing algorithm classifies names as individuals, trusts, and non-trust institutions (e.g., banks, governments). We restrict to purchases in which the buyer is a person or trust, and to sales in which the seller in the second transaction is the same as the buyer in the first transaction, excluding distressed sales from the requirement because distressed sales are typically executed by institutions rather than individuals. We drop transactions with prices that are less than $10,000. For the purposes of computing housing returns, we drop purchases with combined loan-to-value ratios of more than 102.5% and ownership spells that last less than 12 months. This algorithm yields our baseline sample of ownership spells, consisting of 13.6 million ownership spells purchased by Black, Hispanic, or White owner-occupants between 1990 and 2014. Note that this sample includes spells where the property remained unsold by the end of March 2020.

Ownership spells in our baseline sample fall into one of two categories: those where the purchaser retains ownership through March 2020 and those where the owner sells or loses ownership of the property. For the former category, we impute sales using house price indices. For the latter category, we require the spell to end in an arm's length, full consideration transaction. Two consequences of this requirement are worth highlighting. First, this requirement excludes many transactions that are categorized as deeds in lieu of foreclosure. Specifically, 90% of transactions that are labeled deed-in-lieu are not labeled as arm's length, full consideration transactions. These are therefore dropped from our main analysis sample. Transactions labeled deed-in-lieu comprise only about 0.09% of all residential transactions in the property data. Second, the restriction to arm's length transactions drops transfers that occur within-family.

C.3 Standalone HMDA Mortgaage Origination Records

In Section 5, we make use of the HMDA mortgage origination records, which cover the near-universe of mortgage originations (e.g., to construct Panels A through C of Figure 7). For our analyses, we restrict to owner-occupied purchase mortgages. We follow a procedure similar to that in Bhutta and Keys (2022). Specifically, we identify first and second liens by grouping together loans that were originated in the same year and Census tract, by the same lender, to borrowers of the same income. Since information on liens is available starting in 2004, we use these groupings to restrict to first liens for years prior to 2004. In addition, we use information on first liens starting in 2004 to verify that this approach yields accurate results.

C.4 Linkages to Additional Administrative Data Sources

The HMDA and ATTOM datasets lack a number of variables of interest, such as measures of underlying financial well-being, characteristics of the mortgage contract, and loan modifications. In order to observe these variables, we turn to linkages with the Equifax, McDash, Fannie Mae, Freddie Mac, and ABSNet datasets. These additional datasets allow us to observe important information on our study sample. However, the linkages between the ATTOM property data and these datasets are different than between the ATTOM, HMDA, and Infogroup datasets. In particular, they likely entail non-negligible amounts of measurement error generated by imprecision in the linkages. In the remainder of this section, we provide more details on these linkages and the strategies used to minimize measurement error.

The Equifax and McDash data allow us to observe a wide range of financial outcomes and behaviors. These data are known as the Equifax Credit Risk Insight Servicing McDash Database (CRISM). The CRISM data contain two components: mortgage servicing records from McDash and credit bureau records from Equifax. The McDash data contain information on both mortgage characteristics measured at origination and loan performance. The Equifax data are comprised of information from the Equifax credit bureau records of borrowers of mortgages captured in the McDash data. The Equifax data are at the monthly level and capture a broad range of financial outcomes and behaviors, including balances and delinquencies on credit cards, auto loans, and mortgages, as well as accounts in collections.

We rely on a k-nearest neighbors algorithm developed by the Fisher Center to link the ATTOM property records with the CRISM, Fannie Mae, Freddie Mac, and ABSNet datasets. To create the linkage to the CRISM dataset, the algorithm proceeds as follows. Within each US county, the algorithm creates a stable linkage between transactions in ATTOM and loans in McDash, matching records ("neighbors") along a vector of attributes. These attributes include the loan amount, the value of the property, the origination date, the purpose of the loan (e.g., purchase or refinance), whether the loan ended in distress (e.g., foreclosure), the loan lien type, the interest rate, and the date the loan was paid off. The data merge we use is similar to merges between the CRISM and HMDA datasets used in the previous work (e.g., Gerardi et al. 2020).

We apply a number of sample restrictions to mitigate the potential impact of measurement error. First, we restrict to matches for which the algorithm chose the nearest neighbor (e.g., as opposed to the second-nearest neighbor). Second, we restrict to matches for which there are no other close matches. Our measure of match closeness comes from a score generated by the algorithm that denotes the closeness of the match along the vector of matched attributes. For the merge with the CRISM data, the 10th, 25th, 50th, and 75th percentiles of this score are 270, 639, 919, and 1,850, respectively (lower scores denote closer matches). We drop any match with a neighbor with a score within 200 of the score of the chosen match. Third, we restrict our analysis to matches with scores at or below 2,000 (slightly above the 75th percentile of the score).

We use the Fannie Mae, Freddie Mac, and ABSNet datasets allow us to study mortgage modifications. Fannie Mae and Freddie Mac publish publicly-available mortgage databases containing subsets of purchased or guaranteed single-family conventional fixed rate mortgages originated since 2000 and 1999, respectively. To complement these databases, we include loans in the ABSNet Loan database. The ABSNet data are sourced from reports to securitization trustees and cover over 90% of loans collateralized through private-label residential mortgage backed securities. In addition to observing loan modifications, these data sources also record the identity of the mortgage servicer. To focus on a sample of homeowners who are eligible to receive a mortgage modification, we construct a sample of loans that become 90 or more days past due.

As with the credit bureau and mortgage servicer records, we rely on a k-nearest neighbors algorithm developed by the Fisher Center to link mortgage modifications in the Fannie Mae, Freddie Mac, and ABSNet data to our main study sample. This algorithm is the same as that used for the CRISM datasets and uses a similar vector of matched attributes (e.g., loan amount, property value, origination date). We apply analogous restrictions to minimize measurement error, restricting to nearest-neighbor matches with no other close match and low distance scores. For the merge with the Fannie Mae, Freddie Mac, and ABSNet data, the 10th, 25th, 50th, and 75th percentiles of this score are 435, 499, 824, and 1,929, respectively.

The merge score provides a convenient way to test whether measurement error significantly impacts our results. In unreported results, we replicate our analyses that use the CRISM, Fannie Mae, Freddie Mac, and ABSNet datasets using more stringent restrictions on merge quality, restricting to matches with a score of 700 or less (approximately the median score), which leaves our findings largely unchanged but entails some loss in precision due to the use of a smaller sample. The robustness to these sample choices indicates that measurement error is not likely to significantly affect the conclusions we draw from the analysis using the CRISM, Fannie Mae, Freddie Mac, and ABSNet data sources.

We use address histories from Infogroup to identify the neighborhoods that homeowners depart from when they purchase a home and move to a new neighborhood. The Infogroup data were originally collected for business marketing purposes and are comprised of a yearly panel of households from 2006 to 2019. Infogroup links households and individuals over time and space. Each record provides a household address and names of household members. These data are linked to the ATTOM transaction records using addresses and names reported in property transaction records.

C.5 External Data Sources

This section provides additional details on the construction of the external data sources from the Survey of Income and Program Participation (SIPP), the Panel Study of Income Dynamics (PSID), the Current Population Survey (CPS), the Federal Reserve Bank of New York Survey of Consumer Expectations (SCE), the American Housing Survey (AHS), and the National Survey of Mortgage Originations (NSMO).

For the SIPP sample, we draw on waves between 1991 and 2018. All results use person weights, and dollar-denominated values are deflated to 2016 dollars. In Table 4, we restrict the sample to homeowners with positive liquid wealth. Our final sample includes 1.1 million observations corresponding to 423 thousand households, with households defined as the unique combination of SIPP sample units/address IDs, and family IDs. To construct a liquid wealth variable, we follow Chetty et al. (2017) and define liquid wealth as the sum of assets held in stocks, bonds, checking accounts, and savings accounts, excluding retirement, accounting for changes to variable construction and variable names across panels in some years. We winsorize this variable at the 1% level. Our annual unemployment variable is an indicator that measures whether in the prior

12 months, any earner in the household had no job in a month, was on layoff, or was looking for work in all weeks. Income is the annualized monthly income for the household, and delinquency corresponds to the question, "Was there any time in the past 12 months when (you/your household) did not pay the full amount of the rent or mortgage?"

For the PSID sample, we construct a dataset at the family (household) level using PSID waves between 2001 and 2017. We restrict the sample to households who are consistently reported in the survey for consecutive panels. That is, we drop 8,252 observations (1,593 households) that have gaps in the years for which data is provided. All results use the Core/Immigrant family longitudinal weights, and dollar-denominated values are deflated to 2016 dollars. Our combined sample includes 68,582 observations (14,554 households) for the years 2001 to 2017.

For the CPS sample, we construct a dataset at the individual level using interviews between 2002 and 2018. We restrict to Black, White, and Hispanic individuals aged 20 to 55 and who were employed working at least 30 hours per week as of their first interview. We restrict to individuals who report being homeowners two months after their first interview. Unemployment is measured 12 months after first interview, whereas income loss is measured by comparing income measured 2 and 14 months after the first interview. For about two-thirds of the sample, we compare interview months 0 to 12, and for the remaining one-third we compare interview months 2 to 14. All results use Annual Social and Economic Supplement Weights. Our combined sample includes 39,129 individuals.

For the SCE sample, we construct a dataset at the individual level using interviews between 2013 and 2022. We restrict to a sample of homeowners aged 20 to 65. We restrict to individuals who were surveyed in at least 10 out of the first 12 months. The resulting sample includes 4,832 individuals. All results use included SCE sampling weights.

For the AHS sample, we use the 2015 survey wave to construct a sample that can be compared to the primary analysis sample. We restrict to owner-occupied properties surveyed in 2015, occupied by homeowners who have lived in their properties for at least one year. To restrict to properties purchased with a mortgage, we keep properties where the homeowner reported making a down payment less than 100% of the purchase price, or who have a current mortgage open. We also drop mobile homes and vacant properties. The resulting sample includes 24,869 households. Continuous outcomes are Winsorized at the 1% level. All results use included AHS sampling weights.

For the NSMO sample, we use respondents surveyed about their primary residence with mortgages opened between 2013 and 2017. We restrict to respondents identifying themselves as Hispanic of any race, Non-Hispanic Black, and Non-Hispanic White. All results use included NSMO analysis weights. The resulting sample includes 24,777 respondents. We construct several outcome variables from existing responses in NSMO. To assess financial knowledge, NSMO respondents were asked about their ability to explain various financial concepts. To form an index of financial knowledge, we combine the following six questions into a standardized index: "How well could you explain to someone the..." (1) "process of taking out a mortgage", (2) "difference between a fixed- and an adjustable-rate mortgage", (3) "difference between a prime and subprime loan", (4) "difference between a mortgage's interest rate and its APR", (5) "amortization of a loan", (6) "consequences of not making required mortgage payments". Respondents select among three answer categories: "very", "somewhat", and "not at all". Other derived outcomes are presented in Appendix Table A11 and are coded as indicated in the table.

D Constructing the Internal Levered Rate of Return

This section describes the calculations used to estimate the levered (internal) rate of return (Equation 2). This is the return that satisfies the following equation:

$$0 = -DownPay_{i0} + \sum_{t=1}^{T_i-1} \frac{rent_{it} - pymt_{it}}{(1+r_i^l)^t} + \frac{\max\{0.01, rent_{iT} - pymt_{iT} + 0.95P_{iT} - UPB_{iT}\}}{(1+r_i^l)^{T_i}}$$

We draw on a variety of data sources to compute each component of this equation. $DownPay_{i0}$ denotes the homeowner's down payment and is the sum of initial equity and closing costs. Equity is measured directly as the difference between purchase price and loan values in the ATTOM dataset. To estimate closing costs, we use the 2018 to 2019 HMDA data, which contain information about closing costs paid for originated mortgages, including points. We restrict to owner-occupied purchase mortgages. For primary (secondary) mortgages, we restrict to loans with LTV less than or equal to 102.5% (35%). We compute total closing costs as total loan costs minus lender credits. For primary mortgages, we regress closing costs as a share of the loan amount on indicators corresponding to five LTV bins: (0,82%), [82%,90%), [90,95%), [95%,100%), and [100%, 102.5%]. These categories allow our calculations to reflect higher closing costs for higher-LTV mortgages (e.g., due to higher mortgage insurance costs). We also include log loan amount and the interaction of the LTV bins with LTV. For secondary mortgages, we estimate analogous regressions but using two LTV bins, corresponding to a cutoff of 20%. We then impute closing costs for each transaction in ATTOM using the coefficients from these regressions. When we use these imputations to compute statistics weighted by total upfront costs, we winsorize total upfront costs at the 99.9% level.

Table D1: Summary Statistics and Validation for Imputed Values

Panel A. Summary Statistics	Mean	SD	p10	p90
Principal and Interest Payment	1315	993.9	498	2378
Escrow Payment	386	752.6	174	647
Primary Balance at Sale	183815	141198.4	67880	330836
Interest Rate, First Lien	0.057	0.0116	0.040	0.070
Interest Rate, Second Lien	0.084	0.0125	0.071	0.096
Monthly Rent	1490	6435.6	626	2513
Rent-to-Price Ratio	0.070	0.0156	0.050	0.091
Annual Maintenance (% of Home Value)	0.817	0.1097	0.676	0.955
Closing Costs	4881	1846.3	2823	7203
Panel B. Validation with McDash	β	Constant	SE	R2
Primary Principal and Interest Payment	1.0338	84.799	0.0005	0.7404
Escrow Payment	0.9645	19.398	0.0007	0.5983
Interest Rate, First Lien	1.0090	–0.001	0.0002	0.7625
Primary Balance at Sale	1.0198	–3010.133	0.0002	0.9740
Combined Loan-to-Value	0.8586	10.701	0.0003	0.7284
Primary Loan-to-Value	0.9171	5.525	0.0002	0.8184

Notes: This table presents summary statistics (Panel A) and validation (Panel B) of the imputed values used to construct the internal rate of return (Equation 2). Panel A plots means, standard deviations, and 10th and 90th percentiles. Panel B plots regression coefficients derived from regressing the value observed in the merged McDash data on its imputed value, trimming variables at the 1% level. Note that in the ATTOM data, loan-to-value and combined loan-to-value are measured from recorder documents and thus not imputed using external sources. SE refers to the standard error of the slope coefficient. See Appendix Section D for more details.

When imputing monthly rents $rent_{it}$, we must calculate rents for properties that are actually owner-occupied. A well-known measurement challenge is that the stock of rental properties differs from that of owner-occupied properties, such that simply comparing local rents to local home values yields biased estimates of rent-to-price ratios (Demers and Eisfeldt, 2022).

To construct a debiased measure of local rent-to-price ratios (henceforth, R:P), we begin by using US Census data to construct a yearly series of county-level rents and home values for each year 1996-2020. Data from the Decennial Census and the American Community Surveys yield median rents and home values for 2000 and 2010-2020. We interpolate house prices using the FHFA house price index and rents using HUD fair market rents. Dividing the two series and winsorizing at the 1% level yields a county-by-year R:P.

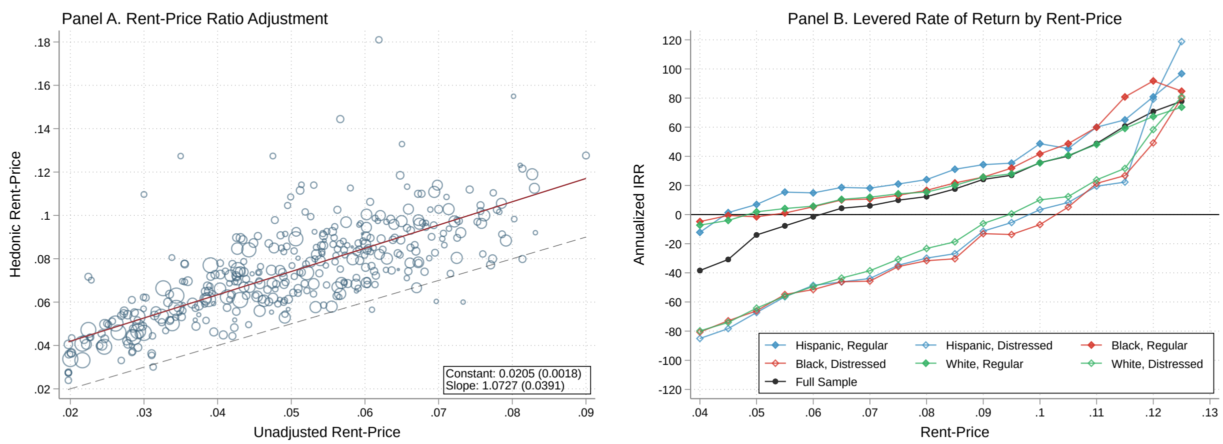
As previously stated, this measure of R:P is biased due to compositional differences between rented and owner-occupied properties (i.e., differences in property characteristics and quality). To address this bias, we follow the procedures in Demers and Eisfeldt (2022) and Gilbukh et al. (2017) to construct MSA-level R:P using hedonic regressions. We use a sample of rented and owner-occupied properties in the 1999-2013 waves of the American Housing Survey (AHS). For each rented property, we regress log rents on interactions of MSA and property age, availability of air conditioning, presence of a basement, bathrooms, bedrooms, all rooms, number of floors, urban/rural, presence of a garage, structure type, lot size, unit square footage, and tenure length. We then use the predicted values from this regression to impute rents for owner-occupied properties, which yields an estimate of R:P for each owner-occupied observation. Following the procedure in Gilbukh et al. (2017), we restrict to a sample of rental units with a similar owner-occupied property in that MSA (and vice versa), and adjust our estimates to account for the transformation of the prediction from logs to levels. Lastly, we restrict to observations with tenures of five years or less.

We collapse our data to the MSA-year level, allowing us to compare the hedonic R:P constructed from AHS to the unadjusted R:P constructed from county-level Census data (i.e., computed by dividing median rent by median house price). Figure D1, Panel A compares the two series. The unadjusted R:P is highly correlated with the hedonic R:P, and the relationship between the two is well-summarized by a level increase in the unadjusted R:P. To construct our measure of imputed rents, we therefore apply the linear adjustment in Panel A to our county-level measure. This allows us to compute R:P for all county-years in the data while also adjusting for compositional bias.

To analyze the potential influence of racial differences in imputed rents, we rely on estimates in Demers and Eisfeldt (2022) of differences across ZIP codes within MSAs in R:P. Because ZIP codes are a much smaller level of geography than MSAs (the smallest level of geography in the AHS), the adjustment using AHS data is not well-suited to removing the compositional bias in ZIP-level R:P. Since Demers and Eisfeldt (2022) use ZIP-level data on R:P ratios from Corelogic that were not accessible for this study, we apply the estimated cross-ZIP differences from Demers and Eisfeldt (2022), Figure 6. We assign each ZIP code to a quintile of house prices within each county-year based off of observed purchases in the property records. We then assume that R:P is 1.25 percentage points higher in the lowest-price quintile, 0.5 percentage points higher in the second quintile, 0.5 percentage points lower in the fourth quintile, and 1.25 percentage points lower in the highest-price quintile. This adjustment calculates relatively higher implicit rents for minorities.

Monthly housing costs $pymt_{it}$ are comprised of three components: principal and interest payments, tax and insurance payments (i.e., escrow), and maintenance costs. To impute monthly mortgage payments (and UPB_{iT}), we apply standard amortization formulas assuming a 30-year term. We impute interest rates using a sample of fixed interest mortgages contained in McDash. We impute interest rates separately for first- and second-lien mortgages. For first-lien mortgages, we regress interest rates on the full interaction of LTV and the five LTV bins used to calculate closing costs as well as closing quarter-by-county fixed effects and log loan amount. For second-lien mortgages, we regress interest rate on LTV and the interaction of the two LTV bins used to compute closing costs, as well as state-by-closing year fixed effects. Coarser fixed effects for second liens are used to compensate for relatively fewer observations of second lien mortgages. For missing county-quarter cells, we impute using the mean value of the fixed effects in that quarter. We then impute interest rates for each transaction in ATTOM using the coefficients and fixed effects from

Figure D1: Rent-Price Adjustment



Notes: This figure illustrates the imputation of rent-price ratios and the relationship between rent-price and average realized returns. Panel A plots the hedonically-estimated rent-price ratio (measured in the American Housing Survey) as a function of the unadjusted rent-price ratio (measured using Census median county house prices and rents) by MSA. Solid line denotes linear fit. Panel B plots realized annual levered housing returns as a function of the imputed price-to-rent ratio. See Appendix D for more details.

these regressions. Due to limited temporal coverage in the McDash data, if a home was purchased in 1996 or 1997, we impute using the 1998 values and adjust using changes in the average US 30-year fixed interest mortgage rate. When incorporating racial differences in interest rates into our calculation of levered returns, we draw on the estimates in Gerardi et al. (2020) and assume that interest rates are 46 and 27 basis points higher for Black and Hispanic homeowners, respectively.

To impute escrow payments, we use a sample of first-lien mortgages where escrow is observed in the McDash data. We measure escrow payments 18 months after the closing month and regress escrow payment as a share of property value on the full interaction of LTV and the five LTV bins used previously, as well as log loan amount and county-by-closing year fixed effects. We use the predicted values from this regression to impute escrow payments in the main data. If a home was purchased in 1996 or 1997, we impute using the 1998 values due to limited temporal coverage in the McDash data. When we incorporate property taxes that are 12% higher for minorities, we do so by assuming that property taxes comprise two-thirds of monthly escrow payments. When incorporating racial differences in tax payments into our calculation of levered returns, we draw on Avenancio-León and Howard (2019) and assume that minorities pay property taxes that are 12% higher.

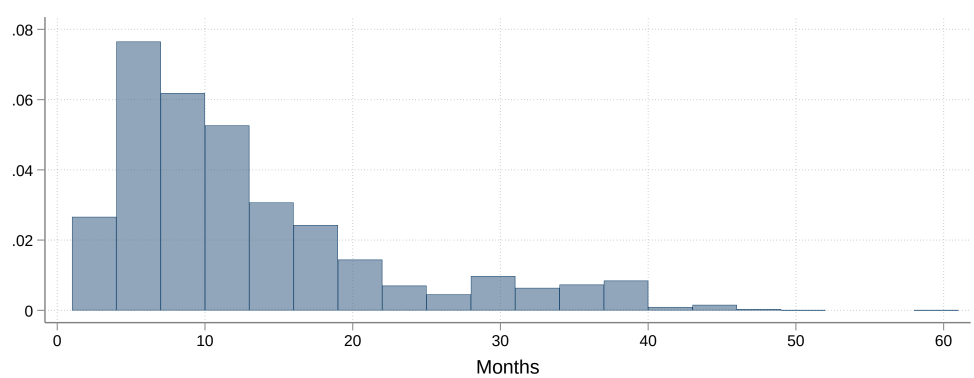
To impute maintenance costs, we analyze a sample of homeowners in the 2001-2013 waves of the Consumer Expenditure Survey (CEX). We regress total annual home expenditures as a percentage of property value on log property value and state fixed effects. We use the predicted values of this regression to impute maintenance costs. To incorporate racial differences in expenditures in our calculation of levered returns, we regress home maintenance expenditures as a percentage of home value on indicators for Black and Hispanic homeowners, and log property value, and estimate that expenditures are 22 and 7 percentage points lower for Black and Hispanic homeowners, respectively. Note that these differences are smaller than those in Appendix Table A7 because our baseline imputation procedure controls for state and home value, meaning that part of these racial differences in home expenditures are already embedded in our baseline imputations.

Our calculations also factor in growth over time in each of these imputed components. While monthly mortgage payments stay fixed over time, we allow imputed rents to change according to rent growth in our county-level rent series. All other user costs are assumed to grow at the rate of inflation.

Table D1, Panel A provides summary statistics on the imputed variables described in this section. To assess the accuracy of our imputations, we compare the imputed values to the actual values as reported in the McDash data. Table D1, Panel A presents statistics from regressions of actual values on imputed values. The regression coefficients are close to 1, and the R-squared values indicate that our imputations capture a large share of the observed variation.

In the months leading up to a foreclosure, homeowners by definition do not make required mortgage payments, and likely do not make tax payments and home improvement expenditures. To incorporate this behavior, we measure the number of months between the last payment and the REO month for foreclosures in the McDash data. We impute the number of months in a given year that a foreclosed homeowner lives in their home without making payment using the median of this difference by state and year, minus four months. We subtract four months because not all homeowners stay in their homes until the REO month. Figure D2, Panel A plots the distribution of the number of months in our sample. This measure is highly correlated with those used by Mian et al. (2015) and Ghent and Kudlyak (2011) (results available upon request).

Figure D2: Distribution of Imputed Months Without Payment in Foreclosure



Notes: This figure presents the distribution of the imputed number of months that a foreclosed homeowner lives in their home without making mortgage, tax, or home improvement payments. Data consist of ownership spells in baseline sample of ownership spells (described in Section 2) ending in a foreclosure.

We assume that in the event of a foreclosure, there is no surplus revenue to be redistributed to the property owner (i.e., the final term evaluates to 0.01). This latter assumption is justified by the fact that in the Freddie Mac single-family loan database, only about 3% of foreclosures have net sales proceeds listed as covered (i.e., there exist surplus funds that could potentially be returned to the homeowner). Imposing a floor of \$0.01 ensures that r^l is well-defined. Lastly, we assume that 5% of the sale price goes towards paying closing costs.

In Section 3.2, we calculate the racial gap in housing returns in annual dollars over a 10-year period. To compute this gap for unlevered returns, we compare the difference in home values between the unlevered returns for a given race, and the counterfactual home values after ten years using the returns achieved by White homeowners. To calculate an analogous difference for levered returns, we compound average cash flows over a ten year period. For Black (Hispanic) homeowners, average upfront costs are \$21,675 (\$30,172), average monthly taxes and insurance payments are \$347 (\$387), average annual maintenance costs are \$1,688 (\$1,932), average monthly mortgage payments are \$1,167 (\$1,377), and average monthly rents are \$1,220 (\$1,254). We let taxes, insurance, and maintenance costs grow by average inflation (2.6%). Average county-level rental growth for Black (Hispanic) homeowners is 3.8% (4.3%).

Our definition of the internal levered rate of return in Equation 2 factors in many relevant cash flows, but it does not take into account substantial indirect costs associated with foreclosure that have been documented in previous work (Diamond et al. 2020; Ganong and Noel 2020b). Moreover, imposing a positive floor on terminal cash flows mechanically limits the losses of homeowners who voluntarily choose to sell an underwater property at a loss. As a result, we may underestimate the size of the racial gap in returns. To relax these assumptions, we define measures of the net present value (NPV) of the home purchase, calculated using the same cash flows in Equation 2, but relaxing the floor on cash flows in the final period for non-distressed sales. We use the US average 30-year fixed mortgage rate in the month of home purchase to discount cash flows in our NPV calculation.

In addition, we compute Sharpe ratios for ownership spells in our study sample. For this calculation, we use 10-year Treasury yields as the risk-free rate, and compute the standard deviation of excess returns within county-purchase year cells. We calculate Sharpe ratios using both unlevered and levered returns.

We provide estimates of racial differences in NPV scaled as a percentage of the net present value of cash flows (i.e., upfront costs, mortgage payment, property taxes, insurance, and maintenance costs discounted by the 30-year fixed mortgage rate) and Sharpe ratios in Appendix Table A8. In addition to our baseline NPV calculations, we also provide two alternative variations of NPV. The first variation calculates NPV imposing a \$50,000 non-pecuniary cost paid at foreclosure, which captures a consumption-equivalent utility cost of foreclosure as in Ganong and Noel (2020b). The second variation of NPV weights observations by strictly positive NPV of cash flows. The results of the NPV regressions (weighted and unweighted, with and without consumption-equivalent utility costs), as well as those for Sharpe Ratios are qualitatively similar to results for levered and unlevered returns.

E Alternative Adjustment for Censoring

We estimate racial returns gaps using a sample of property transactions between January 1990 and March 2020. The finite sample window means that certain ownership spells are censored, in that some ownership spells remain unsold by the end of our sample period. The share of censored spells by purchase year is illustrated in Appendix Figure A2. Our baseline approach to addressing censoring is to impute sale prices for unsold properties using local house price indices. In this section, we present an alternative approach to adjusting our estimates of annual returns to address sample censoring. While this approach avoids the bias in our baseline approach associated with not observing distressed sales that occur outside of our sample window (discussed in Section 3), our baseline approach has the advantage of being simpler to implement and therefore more transparent. Reassuringly, both approaches yield similar estimates.

Our alternative adjustment for censoring decomposes average returns for homeowners of race r as follows:

$$E[R_i|\text{race}_i = r] = \sum_{t=1}^{T} (E[R_i|\text{tenure}_i = t, \text{race}_i = r] \times Pr[\text{tenure}_i = t, \text{race}_i = r]) \tag{8}$$

In the above equation, the expected return for a home purchase for a homeowner i of race r can be decomposed into the weighted sum of expected returns by tenure lengths t.

We compute each component in Equation 8 using our baseline sample of ownership spells, restricted to properties for which we observe a sale by March 2020. Since we no longer rely on house price indices, we additionally include properties purchased in 2015 and 2016, but retain the remaining sample restrictions in our baseline analysis sample described in Section 2.

To estimate $Pr[\text{tenure}_i = t, \text{race}_i = r]$, we use standard tools from survival analysis. Using the sample of properties purchased with mortgages for which we can observe the race of the buyer, Figure E1, Panel A plots non-parametric Kaplan-Meier estimates of the survival functions for our sample of ownership spells in the baseline sample of ownership spells separately by race. The survival curve for Black homeowners is somewhat higher than that of White homeowners throughout the first 30 years of tenure lengths, while the curve for Hispanic homeowners is lower for the first 12 years. Panels B through D plot the distribution of tenure length conditional on sale, and reveal that average tenure lengths for non-distressed sales are somewhat shorter than for distressed sales.

Given that ownership spells in our sample occur between 1990 and 2020 (and mostly between 2000 and 2020), estimating $E[R_i|\text{tenure}_i = t, \text{race}_i = r]$ requires extrapolating returns outside of the 30 year window. Figure E2 plots unlevered and levered returns and the distressed share of sales by tenure length. Plotted points correspond to the realized values in the data. This figure shows that in our sample, annual housing returns stabilize between years 15 and 20. In unreported results, we find that this plateau occurs even among properties purchased in the 1990s, suggesting that the plateau is not merely an artifact of changing sample composition. Accordingly, we extrapolate returns from year 20 onward using the sample-weighted average of housing returns among properties held for 19 years and longer. To extrapolate distressed sales, we assume that the distressed share of sales declines linearly to 0 between years 19 and 30. These extrapolated values are illustrated in Figure E2.

Combining the non-parametric estimates of the distribution of tenure lengths ($Pr[\text{tenure}_i = t, \text{race}_i = r]$) with the extrapolated values of returns by tenure length ($E[R_i|\text{tenure}_i = t, \text{race}_i = r]$), we can estimate returns corrected for censoring by race. These are presented in Appendix Table A5, Column 1.

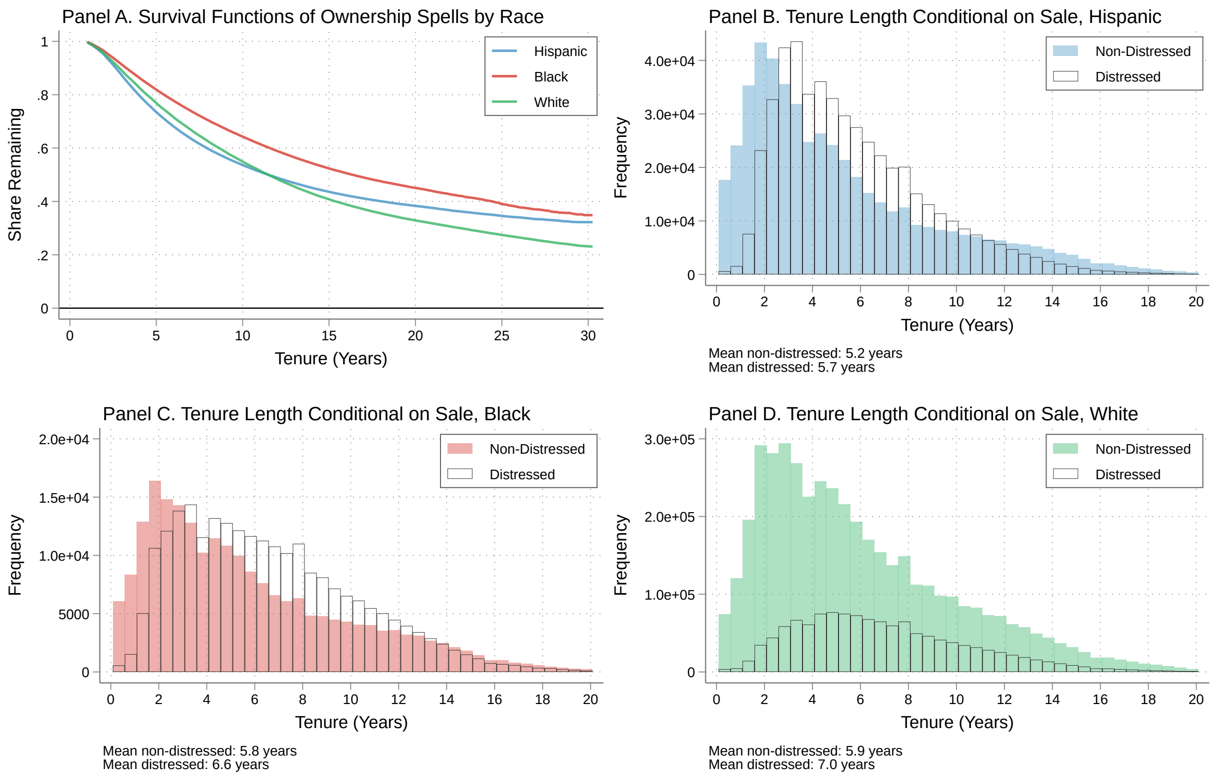
Reassuringly, racial differences in housing returns are similar when we adjust for censoring using our baseline approach (Table 2) and our alternative approach (Appendix Table A5, Column 1). In particular, average unlevered returns are quantitatively very similar by race, yielding similar racial gaps. In addition, racial differences in levered returns are also quantitatively similar. Although average levels of race-specific levered returns are somewhat different, this is partly attributable to much larger variance in levered returns, as well as to the conservative bias in our baseline estimates associated with coding unsold properties as not ending in a distressed sale (comparing Columns 1 and 5 of Table 2). Lastly, the share of properties ending in distressed sale is markedly lower using our baseline adjustment for censoring; however, this also reflects the conservative bias in our baseline estimates. After adjusting our baseline estimates for this downward bias, average rates of distressed sales are very similar across the two approaches (Table 2, Column 5).

E.1 Adjustment for Cash Purchases

We apply an adjustment for cash purchases to our alternative adjustment for censoring. We take an approach that is conceptually similar to our baseline adjustment for cash purchases, which uses the returns of mortgaged properties not sold in a distressed sale as a proxy for the returns of cash purchases. As discussed in Section 3, we observe home purchases made in cash in the ATTOM data but cannot measure the self-reported race and ethnicity of cash buyers. For non-cash buyers in our baseline approach, we measure homeowner race and ethnicity using the HMDA mortgage origination data, which do not cover home purchases made in cash. To construct a race-specific measure of housing returns that is adjusted for cash purchases, we begin by using the American Community Survey (ACS) to construct a race-specific measure of the share of owner-occupied home purchases that are made in cash. In the 2013-2017 ACS data, we restrict to homeowners that have been living in their current residences for less than two years. Among this subsample, 76.5%, 78.6%, and 76.7% of White, Black, and Hispanic homeowners have an outstanding mortgage, respectively. We take this as our measure of the share of homeowners buying a house with a mortgage.

To construct a race-specific measure of housing returns for cash purchases, we observe that the unlevered housing returns associated with properties purchased in cash are very similar to those purchased with a mortgage and sold in a non-distressed sale. Figure E3, Panel A indicates that these returns track each other closely throughout the distribution of tenure length. While returns among cash properties appear to be higher for short tenure lengths, this is likely to be at least in part driven

Figure E1: Survival Function by Race

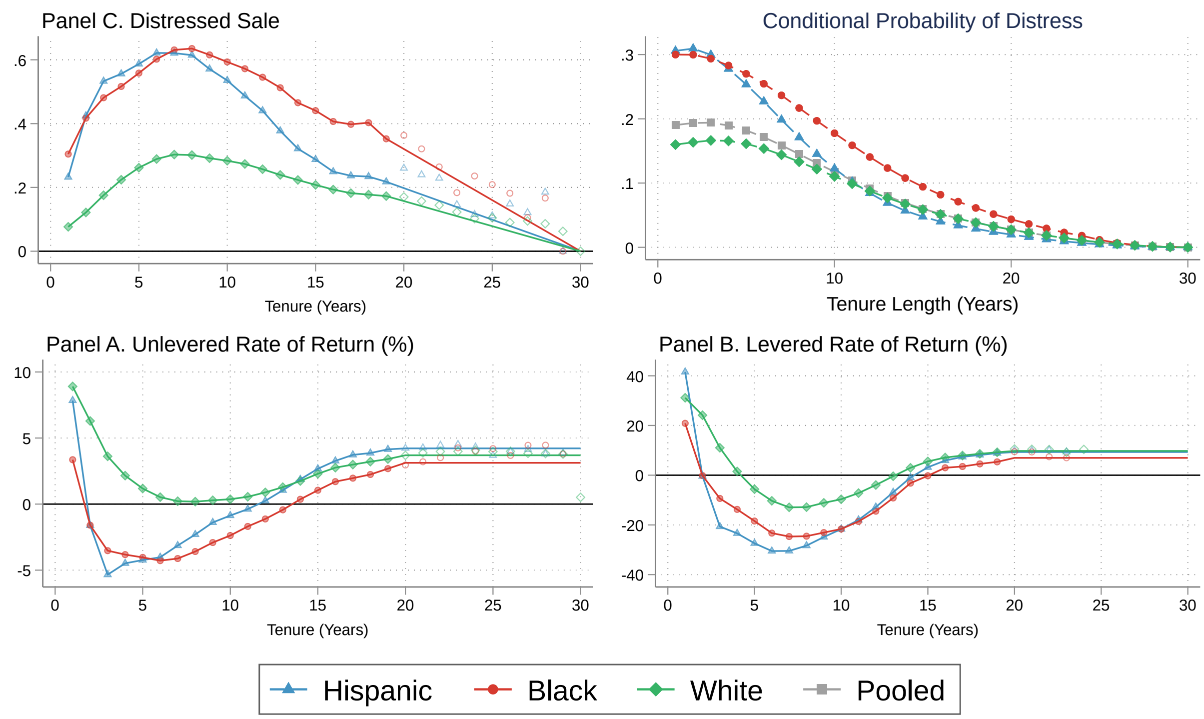


Notes: This figure illustrates the distribution of tenure length associated with ownership spells in which the property was purchased with a mortgage, by race/ethnicity. Panel A plots Kaplan-Meier survival functions by race/ethnicity. Panels B through D plot the distribution of tenure length conditional on sale, separately for distressed and non-distressed sales by race/ethnicity. Data for Panel A come from sample of properties with observed purchase prices, including those that have not yet been sold by the end of March 2020. Data for Panels B through D restrict to properties with observed sale prices.

by non-owner-occupant investment properties that are not the focus of our analysis. Therefore, we view the race-specific returns associated with properties bought with mortgages and sold in non-distressed sales as a reasonable proxy for the race-specific returns associated with cash properties. Figure E3, Panel C further validates this assumption by imputing race/ethnicity from homeowner last name and Census block using the methods developed by Imai and Khanna (2016), and shows that average unlevered returns among cash purchases are very similar by imputed race/ethnicity. Our race-specific estimates adjusted for cash purchases are computed as the weighted average of returns for mortgaged and cash properties, with weights corresponding to the race-specific estimates of the mortgaged purchase shares from the ACS. We proxy for the annual unlevered rate of return for cash purchases using the unlevered returns for mortgaged properties sold in non-distressed sales.

To adjust levered returns for cash purchases, we compute a measure of the internal rate of return without leverage. Specifically, we compute the internal rate of return defined according

Figure E2: Extrapolated Returns and Rates of Distressed Sales



Notes: This figure plots the extrapolated values of annual unlevered housing returns (Panel A), annual levered returns (Panel B), and share of ownership spells ending in a distressed sale (Panel C), as a function of tenure length, separately by race/ethnicity. Points correspond to average values observed in the data. Lines correspond to extrapolated values. Since outcomes are only extrapolated for tenure lengths longer than 19 years, extrapolated values are equal to observed values at earlier tenure lengths.

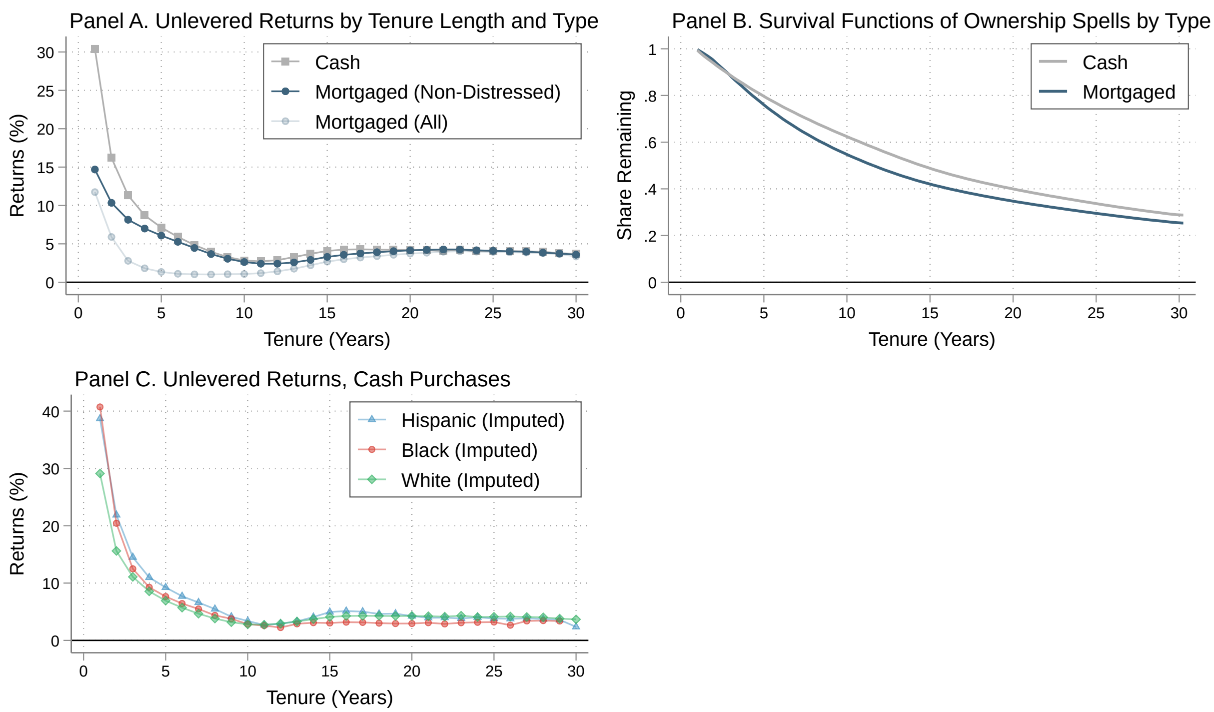
to Equation 2 but exclude cash inflows and outflows from mortgage loans. We include implicit rents, tax and insurance payments, maintenance costs, a 5% transaction cost, and assume closing costs that are half of the imputed value for a loan with an LTV of 80% (given that a large share of closing costs are not associated with borrowing). We then use these modified internal returns among the subset of properties that were not sold in a distressed sale as a race-specific proxy of internal returns associated with cash purchases. Lastly, we assume that homes bought without leverage do not result in distressed home sales.

To adjust cash purchases for censoring, we compute an analogous version of Equation 8 for cash purchases. First, we estimate race-specific survival functions for cash purchases. Figure E3, Panel B presents Kaplan-Meier survivor functions among both cash and mortgaged purchases. Since the survival curves differ slightly, we adjust the race-specific survival functions for mortgaged properties by the ratio of the survival functions of cash and mortgaged properties at each tenure length. This adjustment yields race-specific tenure probabilities for cash purchases ($\Pr[\text{tenure}_i = t, \text{race}_i = r]$). Second, we use the race- and tenure-specific returns associated with non-distressed cash purchases as proxies for the returns of cash purchases ($E[R_i|\text{tenure}_i = t, \text{race}_i = r]$). Combining the two using

Equation 8 yields race-specific estimates of returns associated with cash purchases adjusted for censoring. To combine censoring-adjusted returns for cash purchases and distressed home sales, we take the weighted average of estimates for cash and mortgaged purchases that have been adjusted for censoring, with weights corresponding to the estimates of cash purchases by race from the ACS.

The estimates of race-specific housing returns using our alternative adjustment for censoring and cash purchases are presented in Table A5, Column 2. These results can be compared to our baseline adjustment for cash purchases, which is presented in Table 2, Column 4. As with results that have not been adjusted for cash purchases, estimated racial gaps in both housing returns and distressed sales are very similar between the two approaches. Moderate differences in race-specific average levered returns and rates of distressed sales appear to be caused by the anticipated bias in our baseline approach. In our baseline approach, we treat properties that remain unsold as of the end of our sample window as if they are not sold in a distressed sale. Since some of these properties will eventually be sold in a distressed sale, this approach leads to an upward bias in returns, and since minorities are more likely to realize a distressed sale, a downward bias in the racial gap in housing returns. Consequently, our baseline approach should be viewed as conservative. Given that the estimates of the gap are of a very similar magnitude, and that our baseline approach is simpler to implement and therefore more transparent, we present results using the baseline approach throughout the paper.

Figure E3: Annual Housing Returns and Tenure Lengths by Purchase Type



Notes: This figure compares housing returns and tenure lengths between home purchases made in cash and those made with a mortgage. Panel A indicates that housing returns among cash purchases are similar to the non-distressed returns realized by mortgaged properties not sold in distressed sales. Panel B plots Kaplan-Meier survival functions for cash and mortgaged purchases. Panel C plots average annual unlevered returns by tenure length and race/ethnicity imputed from homeowner name and Census block. Data for Panel A come from sample of properties with observed purchase and sale prices, including properties purchased in cash. Data for Panel B include properties that have not yet been by the end of March 2020. Data for Panel C come from sample of cash purchases with observed purchase and sale prices.

F Do Non-Financial Benefits Compensate for Lower Returns?

Homeownership offers many benefits that are not captured by financial returns. For instance, homeownership may provide an opportunity to locate in neighborhoods with desirable amenities, such as high-quality schools, and previous research has indicated that neighborhoods can have a causal impact on intergenerational income mobility (Chetty et al., 2016). Moreover, buying a home may help households surmount the various barriers they face when attempting to find and move to a desirable neighborhood (Bergman et al., 2019). Consequently, it is possible that non-financial benefits can contribute to household wealth accumulation. To the extent that these non-financial benefits differ by race, they may compensate for the racial gap in financial returns.

While estimating the total impact of homeownership on saving and wealth accumulation is outside of the scope of this study, we analyze one potential dimension of non-financial benefits in the form of neighborhood upgrades realized upon home purchase. We combine address histories for our analysis sample with data from Chetty et al. (2018) that measures neighborhood-level characteristics, including measures of intergenerational mobility. We use address histories linked to our property data to identify the previous address of each household. Specifically, the address histories are comprised of a panel of individual addresses spanning 2006 to 2019. This panel was constructed by Infogroup, a private firm that collects address information on US residents from a variety of sources, including real estate transfers, voter registration files, and telephone directories. We are able to identify the previous address of 3.3 million homeowners in the property data. This linkage allow us to compare the characteristics of neighborhoods from which homeowners depart upon purchasing a home to those of the neighborhoods to which they move. See Appendix C for more details on the address history data.

On average, homeowners of all racial groups move to higher-quality neighborhoods. However, the upgrades realized by minority homeowners are limited relative to their White counterparts. In Figure A29, we plot the average size of neighborhood upgrades by race, binning homeowners into deciles of income computed within each racial group. Panel A depicts improvements in exposure to poverty, as measured by the share of individuals in the Census tract below the federal poverty line. Homebuyers of all races and incomes move to lower-poverty neighborhoods on average. Black and Hispanic homeowners move to neighborhoods with poverty rates that are 2 to 3 percentage points lower than their previous neighborhood, compared to only about 1 to 2 percentage points lower for White homeowners. However, despite minority homebuyers achieving larger absolute gains than their White counterparts, Black and Hispanic homebuyers of similar incomes move to higher-poverty neighborhoods than White homebuyers of similar incomes. This pattern is especially pronounced at lower levels of income, for which the poverty rates in neighborhoods to which minorities move are higher than in neighborhoods from which White homeowners depart.

Figure A29, Panel B illustrates a similar pattern for school quality, measured using 3rd grade math test scores (measured in 2013 at the school district level and reported in grade equivalent units). Homebuyers of nearly all racial and income groups move to school districts with higher test scores, but homebuying does not allow Black and Hispanic homebuyers to catch up to White homebuyers. The average minority homeowner arrives in a neighborhood with lower-quality schools than the neighborhood from which the average White homeowner with a similar income departs.

Because poverty level and school quality measure neighborhood quality by the average characteristics of residents, they are not ideal measures of the benefits realized by individual households. For example, moving a family to a district with high test scores does not guarantee that the children's test scores will improve. To better measure race-specific non-financial benefits to homeownership, we use the race- and income-specific estimates of intergenerational mobility and incarceration rates from Chetty et al. (2018). We assign each homeowner the statistic that pertains to their tract, race, and income percentile in the national distribution of income measured in 2015 dollars (income at home purchase is reported in the HMDA data).

We find no evidence of non-financial benefits in the form of average neighborhood upgrades for measures of neighborhood quality that are race- and income-specific. Figure A29, Panel C presents results for intergenerational mobility, measured as the mean rank in the national income distribution of children born to parents of a given race and income percentile. There is effectively no average change in neighborhood intergenerational mobility for any race or income group. Similarly, Panel D presents results for incarceration, measured as the share of male children born between 1978 and 1983 in each tract that are incarcerated in 2010. All income and racial groups experience negligible changes in race- and income-specific incarceration rates. This is especially notable for lower-income Black homebuyers who live in areas with the highest incarceration rates of Black men.[41]

These findings suggest little scope for improvements in neighborhoods to increase wealth accumulation for minority homeowners beyond those realized by White homeowners. There remain other channels through which homeownership can increase wealth accumulation, such as by providing a commitment device to save through mortgage amortization (Bernstein and Koudijs, 2021). However, the large magnitude of the racial gap in housing returns coupled with the limited gains in neighborhood quality strongly suggest that the total impact of homeownership on wealth accumulation is lower for Black and Hispanic homeowners.

G Racial Differences in Ownership Transitions

Are minority homeowners living in distressed neighborhoods able to take advantage of the availability of nearby discounted homes being sold at foreclosure? In theory, if all real estate transactions occur within race (e.g., Hispanic sellers only selling to Hispanic buyers) and foreclosures do not entail substantial property depreciation, then higher rates of distressed sales need not depress aggregate housing returns for minority homeowners. We analyze racial transitions by analyzing the ownership spell that occurs following a given spell in our baseline sample. Using the subsample

[41] These results are based on a sample of homebuyers that includes households that owned their previous property. Our data only allow us to observe whether a household is a first-time homebuyer through the linkage with administrative data from Fannie Mae and Freddie Mac (described in Section 6) which explicitly record this information but are linked to a smaller subset of homeowners. Online Appendix Figure 4 repeats these exercises for the sample of first-time homebuyers and yields similar results albeit with some loss in precision.

for which race and ethnicity is observed in the HMDA data for two consecutive ownership spells, Appendix Table A16 shows that the majority of distressed minority home sales involve buyers of a different race/ethnicity. Specifically, 79% of distressed White homeowners sell to a White buyer, 31% of Black homeowners sell to a Black buyer, and 39% of Hispanic homeowners sell to a Hispanic buyer. While minority homebuyers are disproportionately likely to purchase distressed homes owned by minorities, most individual buyers of distressed minority homes are not minorities. Nonetheless, minority homebuyers are slightly more likely than non-minorities to purchase distressed homes. Appendix Table A16, Panel C indicates that Black (Hispanic) homeowners are 2 (5) percentage points more likely to purchase a distressed home, relative to White homeowners.

What role do buyers outside of the neighborhood play in purchasing distressed sales? Appendix Table A16 indicates that homes owned by minorities are on average more likely to be purchased by institutional buyers, especially in distressed sales. In Appendix Table A17, we examine patterns using granular fixed effects (i.e., county, purchase and sale year, income, gender, family composition, and leverage). Distressed homes are substantially more likely to be purchased by an institutional buyer or as an investment property (i.e., not owner-occupied). Institutional and investment buyers appear to have an even larger presence in buying minority-owned distressed homes. In addition, buyers of distressed homes own the home for less time than buyers of non-distressed homes, suggesting that distressed properties are more likely to be flipped.

Purchasing a distressed home can require substantial additional investment in order to counteract recent property depreciation. Since we do not observe this investment, we are unable to measure the net discount associated with distressed home sales. Nonetheless, our results suggest that net discounts associated with distressed properties owned by minorities, to the extent that they exist, may disproportionately benefit outside investors and buyers of other racial groups. By the same reasoning, since minority homebuyers are slightly more likely to purchase distressed homes (Appendix Table A16, Panel C), it is possible that these homebuyers may be more able to benefit from these net discounts.

H Racial Differences in the Impact of Leverage

This section describes our analysis of racial differences in strategic default. To estimate these differences, we apply the design in Gupta and Hansman (2022), which estimates the causal impact of leverage on mortgage default. In general, leverage and default likelihood exhibit a positive correlation, which reflects a combination of adverse selection and strategic default. Adverse selection can create a positive correlation between default and leverage if riskier borrowers select into higher-leverage contracts. Similarly, strategic default can also create this correlation because higher leverage after home purchase reduces incentives to avoid foreclosure. Gupta and Hansman (2022) estimate the shares of this correlation attributable to strategic default and adverse selection by using quasi-random variation in leverage after origination. This approach leverages variation in interest rates for option adjustable-rate mortgages (option ARMs). The interest rate adjustments of option ARMs are tied to prespecified interest rates rates, usually LIBOR or Treasury rates. During the 2008 financial crisis, these indices diverged, generating quasi-random variation in outstanding balances (and therefore leverage) among borrowers with the same initial leverage.

The finding that mortgage contract terms have little explanatory power for the racial gap in default (Figure 4) suggests that racial differences in adverse selection are largely driven by measures of observable risk like credit score, as well as unobserved measures of risk, rather than ex-ante leverage choice. Accordingly, the goal of our analysis is to evaluate whether sensitivity to ex-post leverage varies by race. To do so, we closely follow Gupta and Hansman (2022), extending the design to compare effects by race and to control flexibly for racial differences in original leverage. In particular, we construct a panel of homeowners with option ARMs in the ABSNet data, with outcomes measured 24 to 36 months following loan origination. We restrict to loans originated between 2004 and 2007, with original combined loan-to-value ratios between 50% and 100%. To estimate differential effects by race, we restrict to loans merged with the HMDA data for which the borrower identifies as Black, Hispanic, or White.

For each homeowner, we construct a leave-out instrument for the borrower's leverage using average leverage of other similar borrowers with mortgages with the same interest rate index. Specifically, for homeowner j in month t, we compute:

$$Z_{it} = \frac{1}{n_{I(i)\times m(i)} - 1}\Big[\Big(\sum_{j=1}^{n_{I(i)\times m(i)}} LTV_{jt}\Big) - LTV_{it}\Big] \tag{9}$$

where $n_{I(i)\times m(i)}$ denotes the total number of loans originated in month $m(i)$. To form groups $I(i)$, we compare mortgages active in month t with the same interest index, originated in the same month, and with the same original combined loan-to-value. We then estimate the following specification by 2SLS, using $\hat{Z}_{it}$ as an instrument for LTV (and its interactions with race):

$$D_{i,t+1} = \alpha LTV_{it} + \alpha_B LTV_{it} \times \mathbb{1}\{\text{Black}_i\} + \alpha_H LTV_{it} \times \mathbb{1}\{\text{Hispanic}_i\} + L_i'\eta + X_i'\beta + \varepsilon_{it} \tag{10}$$

In the above, $D_{i,t+1}$ is an indicator that homeowner i is 90 or more days past due on their mortgage at least once in the following twelve months, LTV_{it} denotes current loan-to-value,[42] and L_i denotes a vector of flexible controls for original combined loan-to-value. Specifically, this vector contains linear terms in original combined loan-to-value interacted with race and three bins of original CLTV (less than or equal to 60, between 60 and 80, and more than 80). We also include a vector of controls X_i, which includes fixed effects for origination month interacted with originating institution, rate index, ZIP code, 20-point bins of original credit score, occupancy, property type, loan purpose (i.e., purchase vs. refinance), and documentation level. Estimated by 2SLS, $\hat{\alpha}$ captures the causal effect

[42] We measure LTV by the homeowner's current loan balance by an estimate of the current property value. Following Gupta and Hansman (2022), we compute current property value by inflating value at origination by Zillow's ZIP home value index.

of leverage on default.

We present results of this analysis in Appendix Table A18. The instrument appears to satisfy the relevance assumption, yielding F-statistics between 18 and 34, depending on the set of included controls. We find that a 1 percentage point increase in current leverage raises subsequent default rates by about 0.7-0.8 percentage points. Consistent with Gupta and Hansman (2022), these findings imply that current leverage has a substantial impact on subsequent default rates. In addition, our estimates are not statistically different from those in the original study (see Gupta and Hansman 2022, Table 3). Examining the interaction of current leverage and race indicators, we find no evidence that the causal impact of leverage is higher among Black or Hispanic homeowners, relative to White homeowners. Instead, Black homeowners appear to exhibit significantly smaller responses than White homeowners. Among Hispanic homeowners, we find no statistically significant differences relative to White homeowners. Together, these results suggest that racial differences in strategic default are not drivers of higher rates of default among Black and Hispanic homeowners.

The identifying assumption in this analysis is that the variation in leverage captured by the leave-out instrument is uncorrelated with default, except through its effect on current leverage. To support this identifying assumption, we conduct a validation exercise to assess the degree of correlation between the instrument and borrower- and loan-level characteristics. Specifically, we regress these characteristics on the leave-out instrument, including the minimal set of controls used in our main analysis (i.e., origination month, index type, and ZIP code fixed effects, and linear terms in original combined loan-to-value interacted with race and bins of original loan-to-value). Appendix Table A19 presents the results of this exercise. Reassuringly, the leave-out instrument appears to be uncorrelated with original credit score, original value, and documentation level. Although there appears to be some correlation with an indicator that the mortgage is a purchase mortgage and that the property is owner-occupied, this imbalance does not appear to meaningfully affect our estimates. This is apparent when controlling for these baseline characteristics when estimating Equation 10—Columns 2 and 3 of Table A18 show very similar estimates to those derived from applying the minimal set of controls. These findings support the validity of this design, in line with expectations given the results of analogous exercises conducted in Gupta and Hansman (2022).

I Analysis of Mortgage Modifications

In this section, we document that minority homeowners are more likely to receive modifications than observationally similar White homeowners. We then estimate the causal impacts of mortgage modifications on housing returns.

I.1 Racial Differences in Receipt of Mortgage Modifications

We analyze a sample of homeowners who have become 90 or more days past due in the Fannie Mae, Freddie Mac, and ABSNet datasets. We classify the outcome of a homeowner's first 90-day delinquency into one of three categories: modified, foreclosed, or self-cured. We directly observe modifications and foreclosures, and we define a loan as self-cured if the borrower makes three consecutive payments or pays off the loan. The outcome of the delinquency is defined as whichever of these three events occurs first. In this sample, 62% of 90-day delinquencies result in foreclosure, 16% are resolved by the borrower self-curing, and 21% result in a modification.[43]

We estimate the following equation:

$$\mathbb{1}\{\text{Delinquency Outcome}_i\} = \alpha_0 \mathbb{1}\{\text{Black}_i\} + \alpha_1 \mathbb{1}\{\text{Hispanic}_i\} + \mu_{f(i)} + \varepsilon_i \tag{11}$$

The outcome is defined as an indicator that the 90 day delinquency of homeowner i ended in modification, foreclosure, or self-cure. $\mu_{f(i)}$ denotes fixed effects which vary by specification. The values of $\hat{\alpha}_0$ and $\hat{\alpha}_1$ capture the extent to which delinquent Black and Hispanic homeowners are more or less likely than White homeowners to end their delinquency in a given outcome.

Appendix Table A20, Column 1 estimates a baseline version of Equation 11 with fixed effects for quarter of default, and shows that Black homeowners are about 7 percentage points more likely to receive a modification relative to White homeowners. The Black-White difference is driven by relatively lower foreclosure and self-cure rates. Hispanic homeowners are slightly less likely to receive a modification, and appear to be much less likely to self-cure. Adding in granular fixed effects that capture borrower characteristics (e.g., credit score, income) and mortgage characteristics (e.g., current LTV, origination year, interest type) results in a 5.0 and 1.6 percentage point higher modification rates for Black and Hispanic homeowners, respectively (Column 3). Notably, even when comparing homeowners within the same servicer, Census tract, and time period, Black homeowners are 2.5 percentage points more likely to receive a modification than White homeowners (with no statistically significant difference between Hispanic and White homeowners).

These results indicate that even conditional on defaulting on payments as well as a wide range of observables, Black (and to some extent Hispanic) homeowners appear to receive favorable treatment from mortgage servicers. These findings are similar to those in Collins et al. (2015), who document similar patterns in a sample of subprime loans originated between 2004 and 2006. This behavior is consistent with mortgage servicers internalizing the higher house price penalties associated with foreclosures on minority-owned properties. Such discrimination could arise from an equilibrium in which mortgage servicers attempt to maximize value for investors and anticipate that the value of avoiding a foreclosure for a Black or Hispanic homeowner is higher than for a White homeowner.

I.2 Empirical Strategy: Variation from Mortgage Servicers

To estimate the causal impacts of mortgage modifications on housing returns, we use data from Fannie Mae, Freddie Mac, and ABSNet to measure modifications for properties in our baseline sample of ownership spells. Fannie Mae and Freddie Mac publish publicly-available data on single-family conventional mortgages originated since 2000 and 1999, respectively. These data contain mortgage characteristics, monthly payment information, the identity of the mortgage servicer, and

[43] Note that a small fraction of loans (less than 2%) end in bankruptcy or repurchase. These observations are kept in the analysis dataset but not captured in the three main categories of outcomes.

records of loan modifications. ABSNet provides similar data for over 90% of residential mortgages collateralized through private-label mortgage-backed securities.

We restrict attention to a sample of 1.2 million loans in which the homeowner has become 90 days delinquent on their mortgage and whose homes are observed in the property data. After first becoming 90 days delinquent, 62% of homeowners subsequently experience a foreclosure, 21% receive a modification before a foreclosure occurs, and 16% catch up on their payments without a modification. Appendix C provides additional details on the mortgage modifications sample.

We leverage quasi-experimental variation in servicers' propensities to modify mortgages, which has been shown in prior work to vary both across servicers and within servicers over time (Piskorski et al. 2010; Agarwal et al. 2017; Aiello 2019; Korgaonkar 2020). To construct a measure of servicer modification propensity, we estimate equations of the following form on the sample of homeowners who have become 90 days delinquent:

$$\mathbb{1}\{\text{Mod}_{it}\} = \mu_{f(i)} + \gamma_{s(i),t} + \varepsilon_i \tag{12}$$

In Equation 12, i denotes homeowner, t denotes year, $\mu_{f(i)}$ denotes a vector of fixed effects that includes fixed effects for the Census tract interacted with origination year and current year; the source of the data (i.e., Fannie Mae, Freddie Mac, or ABSNet); deciles of credit score at origination; an indicator that the loan is interest-only; and an indicator that the loan is a negative amortization loan. The vector also includes fixed effects capturing deciles of the original loan amount, current LTV, and years remaining in the loan term, and an indicator that the loan is an adjustable-rate mortgage. $\gamma_{s(i),t}$ denotes servicer-by-year fixed effects. The outcome is defined as an indicator that the loan received a modification within 12 months of first becoming 90 days delinquent. We estimate a separate $\gamma_{s(i),t}$ for each state, restricting the sample to loans outside of that state and outside of any ZIP codes and commuting zones that overlap with that state. The estimated $\hat{\gamma}_{s(i),t}$ provide a plausibly exogenous measure of servicer propensities to modify loans.

The estimated servicer propensities serve as an instrument for modifications. We estimate the following specification by 2SLS, using $\hat{\gamma}_{s(i),t}$ as an instrument for $\mathbb{1}\{\text{Mod}_{it}\}$:

$$r_i = \alpha_0 \mathbb{1}\{\text{Mod}_{it}\} + \mu_{f(i)} + \varepsilon_i \tag{13}$$

Equation 13 regresses an outcome r_i (e.g., the rate of return realized by homeowner i) on indicators that i received a modification within 12 months of default interacted with race indicators. $\mu_{f(i)}$ denotes a vector of fixed effects. In our baseline specification, this vector includes interacted fixed effects for Census tract, purchase year, year of default, and indicators for interest-only loan and negative amortization loan, as well as servicer fixed effects. We two-way cluster standard errors at the level of the servicer and within county, origination year, and default year cells. Table A21, Column 1 presents the first stage, indicating that the instrument has a large and statistically significant impact on modifications.

Under the exclusion assumption that the servicer modification propensity affects realized returns only through receipt of modification, estimating Equation 13 by 2SLS recovers the causal impacts of modification receipt by race. The exclusion assumption is plausibly satisfied in this setting because homeowners are unlikely to be aware of their mortgage servicer's propensity to modify loans. Moreover, the inclusion of servicer fixed effects controls for potential bias from systematic sorting into servicers and leverages within-servicer over-time differences in modification propensities.

We find that modifications reduce the likelihood of experiencing a distressed home sale for all racial groups. Table A21, Column 2 presents naive OLS estimates, which indicate that a modification for a White homeowner is associated with a 5.5 percentage point increase in housing returns (and with slightly higher returns for Black and Hispanic homeowners). 2SLS estimates in Column 3 report that modification increases annual returns by 11.2 percentage points for White homeowners, indicating that the OLS estimates are downward biased. A possible reason why the OLS estimates are downward biased is if servicers allocate modifications to distressed neighborhoods where foreclosures are particularly costly. While the 2SLS point estimates are smaller for Black homeowners, the difference between Black and White homeowners is relatively modest compared to the overall effect and not statistically significant. Moreover, point estimates suggest modestly larger impacts for Hispanic homeowners, but these are also not statistically significant. Analyzing impacts on levered retruns (Column 4) yields similar results. These findings imply that modifications have economically large impacts on housing returns for minorities.

Modifications increase housing returns by helping homeowners avoid distressed sales. 2SLS estimates in Table A21, Column 5 indicate that modifications reduce the share of ownership spells ending in a distressed sale by 36 percentage points, with statistically insignificant differences across racial groups. These findings are consistent with Collins et al. (2015), who find similar associations between modification and foreclosure across racial and ethnic groups in a sample of subprime loans originated between 2004 and 2006.

We conduct two robustness exercises in order to validate our research design. First, we probe the sensitivity of our estimates to alternative sets of controls. In Appendix Table A22, we interact our baseline fixed effects with terciles of credit score at origination (Column 2), loan-to-value ratio at default (Column 3), and income at loan origination (Column 4). The estimated impacts of modifications are quantitatively similar across specifications. Second, we conduct a placebo exercise in which we regress the outcome of interest (e.g., indicator for a distressed sale) on a vector of characteristics measured prior to default and use the predicted values to define an index.[44] Intuitively, if our exclusion restriction holds, then the modification instrument should have no impacts on these outcomes measured prior to default. Forming an index using these predicted values concisely summarizes the relationship between the vector of characteristics and the outcome. In support of our empirical design, Appendix Table A23 regresses the index on the modification instrument and shows that the estimated coefficients are small and statistically insignificant.

[44] The characteristics include loan type (i.e., conventional, FHA, VA) fixed effects; loan purpose (i.e., purchase or refinance) fixed effects; indicators for adjustable-rate, interest-only, and negative amortization; fixed effects for deciles of credit score, income, interest rate, and loan amount at origination; current year fixed effects, and data source fixed effects.

To evaluate whether an expansion of modifications could be made more cost-effective by targeting specific types of homeowners, we analyze heterogeneous effects of modifications. Appendix Table A22, Columns 5 and 6 interact the modification indicator with measures of market distress and family size, respectively. For each homeowner i who is first delinquent in year t, we compute the share of property sales in year t (excluding the sale of homeowner i) that are distressed sales. We define homeowners in distressed tracts as those in the top quartile of this measure. Similarly, we define single applicants as those whose loan application registered in the HMDA data does not include a co-applicant. The impact of modifications on unlevered returns is 5.1 percentage points larger in distressed tracts and 2.9 percentage points larger for single-applicant households. These results suggest that targeting distressed neighborhoods and households may be particularly cost-effective.

J Roy Model of Selection into Homeownership

We formalize the hypotheses that can explain higher rates of minority default using a simple Roy model of selection into homeownership. Consider an agent who is offered a mortgage contract based on their observable risk of default, indexed by $\hat{\varepsilon}$. Intuitively, $\hat{\varepsilon}$ represents measures of riskiness like credit score and income. The mortgage contract has a vector of features $\vec{X}(\hat{\varepsilon})$ which depend on observable risk (e.g., higher interest rates for riskier borrowers). One can interpret the contract $\vec{X}(\hat{\varepsilon})$ as the household's most preferred choice of contract and property given a menu of available contracts and lenders. The agent also has an idiosyncratic risk of default ε_u, which is unobserved to the lender and mean-zero such that observed risk accurately reflects realized risk on average (i.e., $E[\varepsilon_u|\hat{\varepsilon}] = 0$).

The agent chooses to become a homeowner if the net financial benefits exceed the expected costs of foreclosure. Normalizing $\hat{\varepsilon} + \varepsilon_u$ to denote the probability that the agent defaults on their mortgage, this decision can be expressed as

$$B(\vec{X}(\hat{\varepsilon})) > (\hat{\varepsilon} + \varepsilon_u) \times C \tag{14}$$

In the above, C denotes the costs of foreclosure borne by the agent. Letting F denote the CDF of ε_u, the share of homeowners with risk type $\hat{\varepsilon}$ who choose to become homeowners is given by

$$F\left(\frac{B(\vec{X}(\hat{\varepsilon}))}{C} - \hat{\varepsilon}\right)$$

Lastly, let $\mathcal{R}$ the set of agents belonging to a racial minority and $\mathcal{H}$ denote the set of agents who choose to become homeowners.

In this environment, agents with low levels of unobserved risk select into homeownership. There are three potential mechanisms that can generate higher levels of realized risk among minorities, such that $E[\hat{\varepsilon}+\varepsilon_u|\mathcal{R}\cap\mathcal{H}] > E[\hat{\varepsilon}+\varepsilon_u|\mathcal{H}]$. The first potential mechanism is that minority homeowners have higher observable risk on average, and that $\vec{X}$ is set in such a way that does not overly discourage risky homeowners from purchasing homes. As the agent's problem illustrates, if interest rates for risky homeowners were to increase, this would decrease the benefits of homeownership and therefore reduce the average risk ε_u among agents who opt into homeownership. By setting $\vec{X}$, the lender (or a policymaking regulator) can affect the level of realized risk conditional on observed risk. Thus, if minority homeowners are observably riskier on average, then higher levels of minority distress are influenced by contract features $B(\vec{X}(\hat{\varepsilon}))$.

The second potential mechanism is if minorities are unobservably riskier on average, such that $E[\varepsilon_u|\hat{\varepsilon}, \mathcal{R} \cap \mathcal{H}]$. Higher levels of unobserved risk among minorities need not lead to higher levels of unobserved conditional on becoming a homeowner; rather, this will occur only for certain distributions of unobserved risk ε_u (Dobbie et al., 2021). In addition, if minorities are unaware that they are unobservably riskier on average, and solve Equation 14 for a value of ε_u that is lower than its true value, then this will naturally lead to higher minority risk ex-post.

The third potential condition entails differential lender treatment. If lenders set different contract features for minorities, or find other ways of altering the benefits of homeownership, then $B(\vec{X}(\hat{\varepsilon}))$ would be higher, thus increasing $E[\varepsilon_u|\hat{\varepsilon}, \mathcal{R} \cap \mathcal{H}]$. Intuitively, this formulation can nest a broad range of lender actions, including disproportionate advertising to higher-risk minorities, which increases $B(\vec{X}(\hat{\varepsilon}))$ by lowering search costs.

K Decomposition of Distress During Great Recession

In this section, we provide addition details of the methods used to calculate the decomposition in Equation 7, which we reproduce below for convenience:

$$d_t^r - d_{02}^r = \left(d_{G,t}^r - d_{G,02}^r\right) + \left[f_{NG,t}^r \left(d_{NG,t}^r - d_{G,t}^r\right) - f_{NG,02}^r \left(d_{NG,02}^r - d_{G,02}^r\right)\right]$$

This equation decomposes the increase in distressed sales for homes purchased in year t for race r, relative to those purchased in 2002 into two components. The first term captures the share of the increase in distress between 2002 and t that is not due to changes in credit supply. The second term captures the share that can be attributed to credit supply, and reflects both the effect of the expansion of non-GSE/FHA lending (holding fixed the differences in risk between GSE/FHA and non-GSE/FHA loans) and the change in relative riskiness of non-GSE/FHA loans between 2002 and t.

In this decomposition, $d_{c,t}^r$ denotes the fraction of ownership spells ending in distress for homes purchased in year t by homeowners of race r with mortgages originated through channel $c \in \{G, NG\}$, corresponding to GSE/FHA and non-GSE/FHA loans, respectively. $f_{NG,t}^r$ denotes the non-GSE/FHA share of purchases among race r in year t. By construction, the share of purchases in year t among race r is given by $d_t^r = (1 - f_{NG,t}^r)d_{G,t}^r + f_{NG,t}^r d_{NG,t}^r$.

To compute the first component $(d_{G,t}^r - d_{G,02}^r)$, we regress the share of properties purchased with GSE/FHA loans ending in a distressed sale and purchased in year t among race r. We weight by the mean number of GSE/FHA purchases within race-county cells. Specifically, we estimate the following specification for a given race r:

$$d^r_{G,ct} = \sum_{t' \neq 2002} \beta^r_{t'} \mathbb{1}[t = t'] + \mu_c + \varepsilon_{crt}$$

In the above μ_c denotes county fixed effects. The coefficients $\beta^r_{t'}$ represent the empirical counterparts of $d^r_{G,t} - d^r_{G,02}$.

To compute the second component, we first compute $d^r_{NG,t} - d^r_{G,t}$. We estimate the following regression on a panel at the county-year-investor level, separately for each race r:

$$d^r_{i,ct} = \sum_{t'} \beta^r_{t'} \mathbb{1}[t = t'] \mathbb{1}[i = NG] + \mu_{ct} + \varepsilon_{cti}$$

In the above, μ_{ct} denotes county-year fixed effects. i denotes investor status (i.e., GSE/FHA vs. non-GSE/FHA). We weight by the mean number of purchases in a given county-year-investor cell between 2000 and 2003. The estimated β^r_t coefficients represent the empirical counterparts of $d^r_{NG,t} - d^r_{G,t}$.

To compute $f_{NG,t}$, we compute the mean share of non-FHA/GSE loans for each year-race cell, weighting counties by the number of purchases between 2000 and 2003 for a given race. The results of the above calculations are presented in Appendix Table A14, and discussed in Section 5