Returns to Homeownership and Inequality:
Evidence from the First-Time Homebuyer Tax Credit

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Abstract

Homeownership is often promoted as a path to building wealth and reducing wealth inequality, as homeowners generally expect to benefit from price appreciation over time (i.e., a positive unlevered return). Yet, returns to housing are idiosyncratic. We observe minority householders, for example, who purchased and subsequently sold a home during the infamous U.S. boom-bust-recovery period (2000-2016) realized a negative unlevered return, on average. In this paper, we document new evidence of household-level demographic differences in housing returns and investigate a potentially important mechanism, upfront capital, that may ameliorate them. Using a unique national dataset linking internal American Community Survey (ACS) households to home transactions data, we test whether access to additional upfront capital increases returns for new homeowners. We then examine whether this constraint is particularly binding for minority households. Our research design exploits a tax policy shock, the 2008-10 First-time Homebuyer Tax Credit (FHTC), that defrayed upfront costs for new homebuyers who fell below an arbitrary income threshold. Results from a difference-in-differences analysis show the FHTC helped those in the eligible income cohort obtain relatively higher unlevered returns on their homes. The positive shock from the credit also enabled minority households in the eligible cohort, and Black householders in particular, to secure even higher returns. Taken together, the evidence suggests upfront capital is a more binding constraint for minority households to build wealth through housing capital gains.

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1. Introduction

Housing is a key sector of the economy, making up a large proportion of investment, personal consumption expenditures, and fixed assets in the United States.[1] Like other assets, neither homeownership nor housing wealth are equally distributed across all racial and demographic groups. Conditional on ownership, homes owned by White householders had a median asset value of $230K, while those owned by Black and Hispanic householders had $150K and $200K, respectively. Although Black households are far less likely (45%) to own a home than White households (74%),[2] housing still dominates the average Black household's asset portfolio (Derenoncourt et al. 2023). Recent literature has cited differences in homeownership rates and asset returns as key contributors to the racial wealth gap, along both the *extensive* margin in terms of portfolio choice/composition across asset types (Derenoncourt et al. 2023; Wolff 2022) and the *intensive* margin regarding return heterogeneity within an asset type (Xavier 2021; Kermani and Wong 2021).[3] In this paper, we document new evidence of significant heterogeneity in returns for a typical U.S. household's single largest asset, their home.[4] Given the evidence that returns differ by race, we thus investigate a critical channel, upfront capital, that can potentially raise returns to housing and we explore whether this constraint is particularly binding for minority households.

We answer these questions empirically by exploiting a policy shock to upfront capital, the 2008-2010 First-Time Homebuyer Tax Credit (FHTC). The policy offered new homebuyers with income below an arbitrary threshold up to $8,000 in the form of a refundable tax credit to help

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[1] Historically, residential investment has often been between 3 to 6% of GDP, and in 2021 it was 4.8% of GDP. It is a large share of gross private domestic investment (U.S. BEA 2022a). Expenditure on housing (i.e., housing services) typically constitutes around 10-12% of GDP and was about 10% of GDP and 15% of Personal Consumption Expenditures in 2021 (U.S. BEA 2022b). Residential housing made up approximately 49% of private fixed assets in 2021 (U.S. BEA, Table 1.1. - FAAt101)

[2] See the Federal Reserve's *Survey of Consumer Finances* here, which has homeownership rate and asset comparisons by group.

[3] Derenoncourt et al. (2023) construct a new data series showing the wealth gap is more pronounced than the numbers above initially imply, underscoring a key role of differential returns on assets. They find White households had more diversified portfolios with significantly higher proportions in equities and other assets, which have yielded a higher return than housing since 1950.

[4] The median homeowner had $225K of housing equity in their primary residence in 2019 (Bhutta et al. 2020).

defray upfront costs associated with purchasing a home (e.g., downpayment, closing costs, etc.). Because our research design requires household level data to determine whether the homebuyer would be in the eligible income cohort, we assembled a unique linked dataset that contains rich household-level income, demographic, and property information. Specifically, we linked internal Census records from the American Community Survey (ACS) with data from Zillow's Transaction and Assessment Dataset (ZTRAX) at an address-level. The data allow us to compare the returns of the eligible income cohort to ineligible households before and after the policy implementation, controlling for key demographic and property characteristics associated with the individual household that might otherwise confound difference-in-differences (DiD) estimates. Further, because we observe the race of the homebuyer, the data allow us to compare the effect of this shock across households of difference races, testing whether the injection of upfront capital for the eligible cohort had a larger impact on returns for minority homeowners.

Given the sizable initial costs associated with purchasing a home, prior literature suggests that the aforementioned racial wealth gap could make upfront capital a more binding constraint for minority households. For example, Sabelhaus and Thompson (2021) have documented substantial differences between Black and White households in intergenerational transfers like inheritance (and down-payment assistance from family (Charles and Hurst 2002)) that would help young homeowners with upfront capital.[5] Beyond initial costs, the proverbial adage that one must "spend money to make money"[6] applies broadly to homeownership, as lumpy expenses (e.g., maintenance and repair costs) arise complicating a low equity starting position. Recent work by Boar et al.

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[5] Based on PSID data from the 1990s, Charles and Hurst (2002) report that 54% of White households fully financed their down-payments from savings and 15% of households relied on family assistance. Nearly 90% of Black households relied on their savings for down-payments, while only 6% financed it through family assistance. More recent surveys from the National Association of Realtors show this gap has narrowed over time, however. For more recent estimates of wealth, inheritance, and family support trends by race, see Bhutta et al. (2020), which draws on data from the 2019 SCF.

[6] This adage has been traced back to Roman playwright Plautus over two millennia ago; but like many famous quotes, this may be apocryphal, as variations of this phrase may have originated elsewhere.

(2022) has shown that nearly four-fifths of new homeowners are still liquidity constrained, compounding the costliness of lumpy maintenance/repair shocks. Accordingly, an empirical assessment of an exogenous shock (i.e., FHTC policies) to a homeowner's capital constraints could facilitate a better understanding of the role of this channel in helping households build wealth.

The FHTC was passed by Congress in three phases and swiftly implemented over approximately two years (2008-2010). Although the initial phase effectively provided an interest-free loan (up to $7,500 that had to be paid back), our research design focuses on the latter phases where the tax credit essentially became a grant (non-repayable) to lower-income first-time buyers (up to $8,000). The eligibility criteria for this credit contained discrete, arbitrary cutoffs (described below); as a result, there exists a similarly situated cohort of households on the other side of the cutoff who were ineligible for the credit but still purchased a home during this period. Recent work by Hembre (2018) and Berger et al. (2020) leverage this setting to shed light on the credit's impact on homeownership rates using a difference-in-differences (DiD) approach. A missing link in this literature is determining whether, conditional on purchasing a home, a lumpy capital injection to help low-to-middle income homebuyers was able to facilitate a higher gross rate return when they eventually sold their homes relative to homebuyers that were ineligible.[7]

Initial DiD results show that the income cohort eligible for the FHTC obtained a higher gross ROR (around 1%, annualized) in comparison to an ineligible cohort. These results are robust to a number of relevant considerations, including comparing the "treated" income cohort to two different "control" groups not eligible for the policy (1 - a "control" cohort well-above the

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[7] In line with Goldsmith-Pinkham and Shue (2022), the rate of return [ROR] measure we focus on is the unlevered capital gain or loss generated from the difference in the initial purchase price and the eventual sale price. In the finance and real estate literature, price appreciation/depreciation, capital gain/loss, and unlevered return are often used interchangeably to refer to this difference in prices. We distinguish this as a gross return figure, rather than net, because it does not account for any other net cost or offsetting appreciation/depreciation associated with debt/financing, taxes, transactions costs, or opportunity costs. However, recent work by Sodini et al. 2023 has shown using data from Sweden that homeownership causes wealth accumulation primarily through house price appreciation. We return to this point later in the paper when we discuss how we measure different variations of ROR below.

threshold that would never be eligible for the policy; 2 - a "control" cohort just above the eligibility threshold, who would later become eligible). We take steps to ameliorate observable differences in the treatment and control groups in multiple ways, including entropy balancing and limiting the sample to those closer to the income-eligibility threshold. Further, we find minority households in the treated cohort, and Black households in particular, experienced substantially higher returns with income-eligibility in a triple difference (difference-in-difference-in-differences) analysis. Taken together, the evidence suggests that while the FHTC may have helped the eligible cohort build wealth via capital gains on their homes, the exogenous infusion of capital had a greater positive impact for Black households' returns to this asset.

These results contribute to multiple large literatures and provide useful insights into how U.S. housing market returns evolved over an important period. First, differential returns play an integral role in the sources of between-race levels and trends of wealth inequality for White and Black households;[8] and, our analysis of the FHTC contributes to this literature by advancing understanding of the role of upfront capital in raising returns to housing for minority households. While our results show that an injection of upfront capital helped the eligible lower income cohort more generally, the pronounced positive impact we observe for minority households highlights the degree to which ameliorating the capital constraint for purchasing the home allows households to build wealth and thereby mitigate the racial wealth gap. These results dovetail with large and growing literatures in real estate and urban economics evaluating racial differences in housing and

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[8] See Akbar et al. 2022 and Derenoncourt et al. 2023 for excellent discussions of the historical background. Not only does the existence of differential returns in housing provide some insight into the origins of existing wealth disparities, but our nationally representative analysis also suggests heterogeneity in returns (here by race, rather than income) is a necessary consideration when constructing estimates of wealth inequality (see Fagereng et al. (2016, 2020) for an illustration of heterogeneity by income in Norway, for example).

mortgage market outcomes, including homeownership rates,[9] lending,[10] appraisals,[11] tax assessments,[12] and sorting/location decisions.[13]

Second, this paper adds to a growing body of evidence that returns to homeownership are not homogenous across races by leveraging a unique dataset aptly suited to exploring this issue. Specifically, our initial descriptive statistics show White households generally realized significantly higher unlevered returns to housing than minority households during the infamous U.S. boom-bust-recovery period (2000-2016). These results hold after conditioning on numerous household-level demographic and property characteristics, including accounting for fine-grain location fixed effects. Though this heterogeneity in returns is broadly consistent with recent findings in prior literature using more aggregated data,[14] validation with household-level microdata is key given that recent work showed disparate patterns due to aggregation bias (e.g., Voorheis et al. 2023 and Colmer et al. 2023). Further, Voorheis et al. (2023) show that aggregation bias can be quite pronounced across the income distribution, especially for low-income households (particularly relevant for our analysis), where the microdata may sometimes reveal patently different results than data aggregated to even small geographic levels like block groups.[15]

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[9] See Carrillo and Yezer (2009), Gabriel and Rosenthal (2005), Deng, Ross, and Wachter (2003), Charles and Hurst (2002), Haurin and Rosenthal (2007), Gyourko, Linneman, and Wachter (1999), and Coulson (1999).

[10] See Bartlett et al. (2022), Zhang and Willen (2021), Ambrose, Conklin, Lopez (2021), Bhutta and Hizmo (2021), Bayer, Ferreira, and Ross (2018), Cheng, Lin, and Liu (2015), Kau, Keenan, and Munneke (2012), and Ladd (1998).

[11] See, for example, LaCour-Little and Green (1998).

[12] See Avenancio-León and Howard (2022), Hodge et al. (2017), and McMillen, D.P. and Weber (2008).

[13] See, for example, Shertzer and Walsh (2019), Christensen and Timmons (2018), Bayer and McMillan (2012), and Boustan (2012), among many others.

[14] See, for example, Akbar et al. (2022), Bayer et al. (2017), Blau and Graham (1990), Dawkins (2005), Chambers (1992), and Kahn (2021) for excellent work on housing return differences using more aggregated data. For recent literature on the broader topic of differences in wealth accumulation, see McKernan et al. (2014), Wolff (2017), and Piketty, Saez and Zucman (2018).

[15] One innovative approach in recent years has been for researchers to link housing microdata at the property transaction-level with anonymized data that contains demographic information, like the Home Mortgage Disclosure Act (HMDA) data, which contain some demographic information like race and income. Although the anonymized HMDA data do not disclose the associated names and addresses of the home transacted, they do contain geographic information that allow researchers to link aggregated data at some fine level of geography (e.g., census tract or zip code) or to probabilistically link a given transaction in the housing dataset by linking HMDA transactions with the same characteristics in the same location. Recent work by Ihlanfeldt et al. 2023 has documented concerning issues with using the HMDA data in this way, however. In this study, we evade this limitation with our data. We discuss the details of this linkage in Section 3 below; however, in the context of the literature, it is important to highlight this novel approach and the data's unique capacity to incorporate a rich set of household and property characteristics into our analyses of the ROR to homeownership. We return to this point in Sections 3 and 4.

2.   Background – Incentives, First-time Homebuyer Tax Credit, and the Wealth Gap

Given that a fundamental difference in the asset positions of Whites and non-Whites is the share of homeownership (and related returns), policies promoting homeownership have been advocated as a way to build wealth more generally and to reduce this racial gap.[16] In a recent survey article on homeownership, Goodman and Mayer (2018) remark that certain assumptions about the financial benefits of homeownership were widespread: "for decades, it was taken as a given that an increased homeownership rate was a desirable goal" (p. 31). Market dynamics in tandem with a host of policies have long incentivized homeownership across different channels.[17] On the banking and credit side, this included increased supply of credit that fueled spending (e.g., Mian and Sufi (2009, 2011)) and expansions of subprime lending (Mayer et al. 2009). On the demand side, there was price appreciation that begat more market entrants expecting further price appreciation (Foote et al. 2012) and lower perceived default risk (Gerardi et al. 2008).[18]

Another such policy incentivizing homeownership was the FHTC. Rather than subsidizing rates or mortgage terms that lower incremental costs, the FHTC essentially functioned as a lumpy injection of capital for qualified new homebuyers. After the housing market crash, Congress authorized the FHTC in three pieces of legislation (Housing and Economic Recovery Act (HERA) of 2008, American Recovery and Reinvestment Act (ARRA) of 2009, and the Worker, Homeownership, and Business Assistance Act (WHBAA) of 2009) that would allow first-time buyers (defined as not having owned a home within the last three years) a tax credit of up to $8,000.

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[16] One of the implications of lower homeownership rates and lower returns for minorities is the persistence of wealth inequality. Overall, the large literature on this topic presents many possible explanations for persistent racial disparities. See also Altonji et al. (2000), Charles and Hurst (2002), Barsky et al. (2002), and Killewald (2013) for further discussion of racial wealth gaps.
[17] On the policy side, for example, the Housing Act of 1968, HOME Investment Partnerships Program of 1990, Mortgage Revenue Bond Program, and Community Development Block Grant Program of 1974 promote homeownership by subsidizing low-income homeownership in the U.S. This list is far from exhaustive.
[18] Prior to the housing bust, a number of studies had shown that homeownership was associated with long-term wealth accumulation in a variety of contexts. See, for example: Belsky and Duda 2002; Haurin and Rosenthal 2004; Herbert and Belsky 2008.

While the initial phase of the program was slightly less ($7,500), it had to be repaid over the next 15 years, effectively functioning as a small interest-free loan to help new homeowners with a down-payment or closing costs. For buyers to be eligible for this credit, the legislation required a modified adjusted gross income to be below $75,000 for single-filer households and $150,000 for joint-filer households.[19] Analyses from Hembre (2018) and Berger et al. (2020) found that this initial phase was less effective at stimulating homeownership than the later phases (January 2009 through July 2010), where the statutes then stipulated that the amount up to $8,000 would *not* have to be paid back.[20] Following Hembre (2018) and Berger et al. (2020), our analysis in subsequent sections focus on this latter phase of the policy. We return to the details of how we use this policy setting as a source of quasi-experimental variation in Section 4.

Though the FHTC did not promote homeownership for racial minorities explicitly, there are several reasons to expect that targeting marginal first-time homebuyers would be particularly relevant for low-income minority households. [21] First, while the tax credit is provided to low-income first-time homebuyers in general, this group of homebuyers tends to include a higher share of minority households than higher-income first-time homebuyer groups (Herbert and Belsky 2008).[22] Second, loan service providers report that constraints with traditional loans from conventional lenders are more binding for minority households, especially Black and Hispanic buyers, and the tax incentive was more likely to be relevant where traditional market incentives

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[19] The subsequent legislation would later expand eligibility to $225,000 and $125,000 for joint and single filers, respectively, starting in November 2009. The initial (repayable) phase included homes purchased from April 9, 2008 through December 31, 2008, while the non-repayable credit phase ran from January 1, 2009 through July 2010.

[20] Both Hembre 2018 and Berger et al. 2020 document causal evidence that the FHTC increased homeownership, and Berger et al. (2020) found it increased median home prices by about $2,400 (or 1.1%).

[21] For example, in a discussion of the ARRA of 2009, the Congressional Black Caucus outlines the FHTC as one of the credits critical to economic development and welfare of African American communities during the policy period (Hackshaw 2009, p. 6).

[22] Herbert and Belsky (2008) make this point in their discussion of the income and racial distribution of first-time homebuyers using tabulations of first-time homebuyers over 1991-2003 using responses from the American Housing Survey (see the discussion of their Exhibit 3 on p. 15 of their review).

(including lending opportunities) were not sufficient to induce a home purchase. Finally, a number of interest groups advocated that the FHTC would be critical to the economic development and welfare of particular minority groups. Hence, given all of the above, it may not be surprising that Goodwin and Zumpano (2011) found that Black, Hispanic, and Asian homebuyers were more likely than White homebuyers to be making their purchase as a result of the FHTC using data from surveys of homebuyers during the policy period. Thus, this policy becomes an especially germane setting for understanding the effects of more general policies promoting homeownership and the role of upfront capital plays for minority homeowners in particular.

It is not clear *ex ante* what effect the FHTC might have on returns. On one hand, the FHTC may have helped liquidity constrained buyers build wealth through capital gains by enabling new homeowners to begin their homeownership path at an improved financial position (and at an opportune time near the bottom of a trough in home prices). On the other hand, there are a number of plausible scenarios consistent with the prior evidence in the literature which suggest mitigating circumstances for wealth accumulation. First, if the FHTC induced marginal homeowners into the market, they may have bid against others in the same income cohort on similar homes, inflating prices for homes (consistent with the evidence in Berger et al. 2020), which may not eventually sell for the same premium in a later period. Hence, one possible scenario might be that the gross return to this asset could be smaller for those eligible than for those outside the income-eligibility policy threshold. Second, those who actually took up the tax credit may have bid against those in the same income cohort who were not eligible as "first-time buyers," securing winning bids for better homes, where their greater gross return to homeownership could be offset by lower returns among the losing bidders, making the predicted overall impact more ambiguous. Third, even if the eligible cohort had greater returns overall, these returns may not have been universally shared; in

particular, if minorities which are underrepresented in the housing market (e.g., Hispanic and Black homeowners) earned lower returns than White homeowners, this could potentially exacerbate the racial wealth gap.[23] Or, if such a capital infusion was not sufficient to allay binding liquidity constraints in the face of maintenance/repair shocks, then homebuyers may have ended up worse off. Given the ambiguous predictions *ex ante*, it is thus an empirical question whether returns in low-to-middle income households eligible for this policy outpaced those in higher income cohorts, and whether eligible minority homeowners experienced higher returns.

3.   Data

3.1   Data Description and Sample Restrictions

One key contribution of this paper is the novelty of the data. We constructed a unique address-level dataset from internal American Community Survey (ACS) microdata matched with Zillow Transaction and Assessment Dataset (ZTRAX) data. Census Bureau staff matched ZTRAX assessment and transaction data on an address-level to an internal household-level address identifier used by the ACS.[24] This allowed us to link parcel-level ZTRAX housing data provided by Zillow, which is drawn from public tax assessor records at local municipalities and is organized by state.[25] We began by merging the provided assessment and transaction data so that the home characteristics of each parcel were associated with its sale history. The data was then cleaned and

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[23] Statistics from the Federal Housing Administration (FHA), with whom the vast majority of loans are for first-time homebuyers, suggest that the agency has "long been known to serve a disproportionately larger number and share of minority homebuyers, particularly African-American and Hispanic buyers" (Comeau et al. 2012, p. 2). The FHA attributes this to the fact that many low-income minority households do not have traditional loans provided by conventional lenders and instead tend to be served either by Government-Sponsored Enterprises (GSEs) like Fannie Mae, FHA, or subprime or other nontraditional conventional loans.

[24] The Census Bureau assigned the ZTRAX records a Master Address File Identifier (MAFID) at an address-level, which is an identifier unique to each address. Though imperfect, this matching process generally has a high rate of accuracy and is used internally at Census to link household-level data. The MAFID was then used to link the datasets; however, we take additional steps in culling the data to discard bad matches (described below).

[25] Data are provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results using this data were generated prior to the sunsetting of the Ztrax program. The authors' institution has since purchased data from another vendor that includes virtually the same raw data as well as expanded coverage of price and mortgage data (including states not covered in Ztrax), which can be used for future revisions.

collapsed such that each address (parcel) along with its associated property characteristics (e.g., square footage, lot size, bedrooms, bathrooms, etc.) and transaction information (sale price, date, type, and mortgage information) could be merged to household data from the ACS. For this analysis, we restricted our sample to single family residences, dropping other residential property types and transactions outside the scope of our analysis like commercial transactions, vacant land, and agricultural sales. It is also important to note that our analysis omits the states which do not mandate price disclosure, as the data is either missing or inconsistent for those states.[26]

Though the Census Bureau has multiple surveys which collect information on housing and households, the 5-year pooled samples from the ACS have the largest samples (3.5 million households per year), which include many questions on housing, in addition to demographic and labor market questions that are widely used across the social sciences. Moreover, it is representative of the U.S. population (with survey weights applied).[27] Finally, the ACS collects both individual and household-level data, but for this exercise we focus on only household(er) level data. Thus, any individual-level characteristics are those of the household head, facilitating the joining of these datasets at the property-level. We use the term householder hereafter when referring to individual demographic characteristics for this reason.

As this is a new linked dataset, we take additional steps to ensure the quality of the match. To do so, we compared characteristics of the home as reported by respondents in the ACS and by localities in the ZTRAX data. We not only limited the sample to single-family residences in both data sets,[28] but also ensured the observations were similar in terms of home characteristics.

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[26] These states are Idaho, Indiana, Kansas, Maine, Mississippi, Missouri, Montana, New Mexico, North Dakota, South Dakota, Texas, Utah, and Wyoming. However, we have recently obtained price data for these states from another proprietary dataset, which we plan to incorporate in a future revision.

[27] There have been many studies that utilize the American Housing Survey (AHS), since it contains many detailed housing questions. However, it is a much smaller sample and lacks some of the demographic information available in the ACS.

[28] Single family residences are by far the most common type of domicile in the United States; however, non-single family residences are much more likely to have a lower quality match due to messiness in unit number reporting (e.g., APT vs. Unit vs. #, or even

Following approaches similar to Nolte et al. (2023) and others using ZTRAX,[29] we sought to create a "Zillow-consistent" sample and reduce the possibility of matching-error by dropping observations if (1) the ACS and ZTRAX counties were different, (2) the difference in the number of bedrooms reported is more than two, and (3) the difference in the reported year built is greater than 10 prior to 2002 (ACS year built is in buckets) or greater than 5 after 2002 (finer buckets available in the later period). Further, we exclude observations when the timing of the survey and the timing of the home sales are more likely to be problematic for a clean match. Therefore, we also exclude: (1) when the home has likely been flipped or changes hands multiple times within a short period of time (i.e., bought and sold within the same year), or (2) the homeowners had moved into the home less than one year before the survey year to ensure the respondents are those who bought the home (since the ACS survey is conducted 12 months a year), and (3) the home was foreclosed (from information provided by Zillow). The foreclosure process can take months or even years, depending on state laws and legal processes that vary by jurisdiction, which itself can create invalid matches.[30] Our results in this paper should be interpreted in the context of more "normal" (non-distressed, non-flipped) sales, which may, if anything, understate racial differences in returns (Kermani and Wong 2021).[31]

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missing unit numbers entirely). Out of this practical consideration to maximize the quality of our dataset, we chose not to include these in our sample and instead focus on single family residences for this research question, while acknowledging the limitation for extrapolation of our results.

[29] For example, our data cleaning considerations are similar to those in Gindelsky et al. 2020, Moulton and Wentland (2018), and Chen et al. (2022), which use the same vintage of ZTRAX data.

[30] For example, a foreclosure may be recorded as an REO (real estate owned) sale, usually to the lender, but the homeowner in default may still be living in the home for quite a while until evicted. Or, the homeowner may have abandoned the home months or years before the REO sale. Either way, the inhabitant and the owner of the property may be misaligned. With more detailed mortgage data, which include relevant foreclosure dates, we can explore this issue further in future work.

[31] Kermani and Wong (2021) find Black and Hispanic households had a higher share of distressed sales and foreclosures during this period, which contribute to a substantial portion of their overall return differences. Their paper investigates an alternative (mortgage modification) channel and focuses on the role of distressed sales, leveraging a policy setting that expanded the availability of mortgage modifications for distressed homeowners. We view their concurrent working paper as complementary to our setting of the FHTC Tax Credit, as they explore the impact of addressing one of the drivers of returns (distressed sales) that target existing homeowners through a mortgage modification channel. We investigate an altogether different channel, focusing on how expansion of new homeownership through an exogenous lumpy injection of capital influences returns of more "typical" home transactions, excluding foreclosures and distressed sales. Like any issue of considerable magnitude, there may be multiple mechanisms/channels to explore, particularly with causal evidence from quasi-experimental settings.

Given the possible years for our matched sample (2008-2016) and our research question, we only included home purchases that took place after 2000. Not only were the transaction data in this period more complete in coverage, but they were closer to the surveyed years (which is important when matching household characteristics to the right buyers and sellers). We further culled the sample by dropping the following "outlier" observations: (1) the householder is younger than 20, (2) household income is less than $1,000, (3) sale price is less than $10,000 or greater than $10,000,000, (4), the aggregate rate of return is greater than 500% or the annualized rate of return is greater than 50%, or (5) the ratio of the sale price (in ZTRAX) to home value (in ACS) is greater than 300% or less than 30%. The resulting dataset yields about 149,000 observations that are consistent with the above criteria.

The largest restriction on the dataset is matching ACS respondents who purchased *and* sold a home within our sample period. There are generally sizable periods of time between when a household purchases and sells a home. According to a recent survey by the National Association of Realtors (NAR), the typical home seller has been in their home for about 8 years,[32] with a much lower tenure among younger homebuyers of 3-5 years for sellers under 40.[33] To calculate a return based on the initial purchase price and the eventual sale price, we can only use records from ACS respondents who were surveyed in between those transaction dates of a linked property in the ZTRAX sample.[34] Thus, because we are (by necessity) excluding many long-tenure homeowners, our results should be interpreted as reflecting the returns for homeowners closer to the average tenure. This is likely to be skewed younger than the total population. In our linked sample, the

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[32] NAR 2021 Profile of Home Buyers and Sellers. Prior to the Great Recession, NAR reports that average home tenure was historically 6-7 years, but in recent years it has typically fallen within 8 to 10 years.
[33] See NAR's 2020 Home Buyer and Seller Generational Trends report: https://www.nar.realtor/sites/default/files/documents/2020-generational-trends-report-03-05-2020.pdf
[34] One limitation of this approach is that it must drop observations for which we have a purchase price and no eventual sale price (i.e., they are still living in their home by the time our sample period ends). Or, we may have a sale price and not an initial purchase price (i.e., they had purchased the home prior to our sample period beginning – 2000).

average tenure for a household selling a home is five years. While this skews somewhat younger, this sample nonetheless represents an important step on the path of long-time homeownership. Repeat-buyers tend to roll over equity from their prior home sale into their next home, leading to a larger down payment, lower likelihood of having to pay private mortgage insurance (PMI), and lower likelihood of needing a loan/gift from a family member.[35]

From this data, we measure the gross or unlevered rate of return to homeownership in three different ways. We first use the simplest calculation, the nominal or gross rate of return (ROR), which is the difference between the initial purchase price and the eventual sale price where the difference is scaled by the initial purchase price. Our second measure is an inflation-adjusted ROR (IAROR), which we calculate by deflating the ROR using the Personal Consumption Expenditure (PCE) Index produced by BEA (cited as the Federal Reserve's preferred measure of inflation).[36] Given that households bought and sold these assets after holding them for different time periods, we then simplify comparisons of the dependent variable across households and provide a more standardized measure consistent with the asset returns literature for our third measure: an annualized ROR (defined in Table 1 using a standard compound annual growth rate formula). The latter (annualized) measure is our default for most of our analysis when we refer to the "return," unless otherwise stated. One limitation of this dataset is that it does not allow for a "net rate of return," as it does not include a host of potentially important idiosyncratic costs for the household, property-specific investments, and a variety of other costs associated with homeownership.[37] We thus emphasize that the results should be interpreted as gross or unlevered returns and continue to

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[35] For additional details on sources of down payments, see NAR's 2020 report on Downpayment Expectations & Hurdles to Homeownership.

[36] See, for example: https://www.federalreserve.gov/faqs/economy_14400.htm

[37] Kermani and Wong (2021) evaluate a sample from the Panel Study of Income Dynamics over 2001 through 2017, finding that expenditures on repairs (as a percentage of the home's value) is very similar across races. There is a moderate difference in home improvement expenditures, where White households spend a bit more on home improvements. We leave further analysis of a "net" measure of housing returns to future research.

refer to them as such throughout the paper. Recent research by Goldsmith-Pinkham and Shue (2022), for example, focus on a similar measure as the outcome of interest in studying the gender gap of housing returns.

3.2   Summary Statistics, Unconditioned, and Conditioned Differences in Returns

Table 2 provides descriptive statistics for homes purchased and sale within the broader 2000-2016 window of our linked dataset. The average gross ROR in this sample is 10.7% (annualized 0.5%). Thus, this average ROR did not outpace inflation over this period, where the inflation adjusted rate of return (IAROR) was negative, or about -4.3% for the average household. The average home in the sample has 3 bedrooms, was owned about 5 years, was built in 1980, and had a value of around $300,000. Average household income in the sample is $107,900. About half of the households surveyed in our linked sample were surveyed between 2008-2010. This is particularly helpful in examining the FHTC in our primary analysis. This sample of homeowners is mostly White (80%) and fairly young, with half of the householders under 40 (though the average age is 44). Our sample of householders are also well-educated, with about half attaining at least a bachelor's degree.

Notably, there are large differences in ROR by race in Table 2, documenting initial evidence that unlevered returns to housing are neither homogenous or nor universally positive (even in nominal terms). For example, White and Asian householders had an average annualized ROR of about 0.9% to 1%, yet Hispanic and Black householders had an average annualized return of -0.9% and -2.3%, respectively. While these raw average differences are unconditioned, we continue to observe significant differences in when we condition on a host of demographic characteristics of the household, property characteristics, and fixed effects for location, time, and tenure in a multivariate regression. In Appendix Table A1, we tabulate numerous multivariate

regression specifications,[38] where the coefficient estimates for race on returns tell a similar story as the unconditioned means from Table 2, Panel B, albeit with nominally different magnitudes. The three-percentage point unconditioned difference between the mean annualized ROR of White to Black homeowners (0.85% vs. -2.2%) in Table 2 falls to approximately 1.9-2.3 percentage points in the conditioned estimates in Table A1. Hispanic householders have a *less negative* return of about 1.1-1.6 percentage points lower than similar White householders' annualized returns in the conditioned estimates in Table A1, although Asian households experienced the least difference of all groups (about 0.4%).[39]

Finally, the differences in returns we observe are not primarily driven by differences in location either. Specifically, in Table A2 we explore whether the conditioned results are sensitive to alternative fixed effects, finer geographic controls, and interacted specifications. This is important given prior work by Meyers (2004), which found that racial differences in home prices depended on the level of location controls used. In Panel A of Table A2, the six columns vary location and/or time fixed effects as follows: 1) state-by-year interactions, 2) census tracts, 3) block groups, 4) zip code, 5) year-by-county, and 6) quarter-by-county fixed effect interactions. Census tracts and block groups are relatively small geographic units; so, while we lose data due to dropping singleton observations within these geographies in these specifications, the "within geography" interpretation here is that these differences persist even after accounting for time-invariant "neighborhood differences" in returns.[40] In our sample, accounting for finer location

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[38] We estimate equation A1 (described in the *Notes* in Table A1) on ROR, IAROR, and annualized ROR as dependent variables in columns (1) through (3), respectively, with a limited set of controls aside from the race categories (i.e., fixed effects for county, purchase quarter-by-year, survey year, and tenure). For the remainder of the analysis, we focus on annualized ROR, and we incrementally add controls to the analysis in columns (3) through (6) such that the final column includes the full set of controls.

[39] Incorporation of property characteristic controls in column (4) in Table A1 changed return differences very little, while household demographic controls in column (5) accounted for somewhat more of this difference across racial groups. Indeed, it is clear from the regressions in Table A1 that the differences in returns are not primarily driven by differences in observable characteristics of the household or property they own.

[40] Tracts roughly approximate the size of what one might think of as a broader neighborhood, and block groups are subsets of that, although the exact geographic size varies depending on the population density of the area. Tracts, block groups, and blocks are

differences are more important for Black returns in terms of economic magnitude when going from county (-1.9%) to block group (-1.2%) fixed effects, as compared to Hispanic households, which do not change as much.[41] Overall, the boom-bust-recovery period of 2000-2016 did not produce great returns to housing, on average, but it was particularly lackluster for nonwhite racial minority householders who experienced significantly lower returns over this period.[42]

The initial results motivate further exploration into the nature and causes of household differences in returns, which are our focus for the remainder of the paper. However, it is worth a noting that we focus on households who have selected into homeownership, independent of the option to rent a home instead. A more complete analysis might account for differences in this potentially important opportunity cost, especially if rents appreciated dramatically differently for minority households over this period. For example, a lackluster return to homeownership for Black households could be the better option if rents were appreciating at very high rates (and, as a result, may be the *less detrimental* option for exacerbating wealth inequality). In the Appendix, we plot year-over-year changes in average rents for Black and White households over our sample period using public-use ACS data in Figure A1. The data show that price appreciation in the rental markets were generally similar for both White and Black households over this period. To keep the comparisons simple, we plot rents for the most common rental types (1 or 2-bedroom properties), though the results are similar for larger sized units. Furthermore, the typical rent increases were

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commonly used in hedonic real estate literature to account for location-specific (dis)amenities and neighborhood heterogeneity (see, for example, Bian et al. 2021, Turnbull et al. 2019, Brastow et al. 2018, or Moulton et al. 2018). For more information about the size, scope, and construction of these geographic units, see: https://www2.census.gov/geo/pdfs/reference/GARM/Ch11GARM.pdf

[41] We lose nearly 20% of the sample observations when we incorporate block group fixed effects, which is an inherent tradeoff when incorporating finer fixed effects and requiring non-singletons. We are reluctant to over-interpret magnitude differences across specifications with different samples and potential selection issues with dropping these observations. However, given the sample loss, we chose county fixed effects for the remainder of the analysis to balance these tradeoffs.

[42] We also observe other demographic differences in returns. Table A1 suggest that if the householder was married, younger, college educated, higher income, or had a smaller number of people living in the household, then they were associated with a significantly higher rate of return. Properties that are larger (i.e., more bedrooms, rooms), older, have a value closer to householder income, and do not have a mortgage, are all associated with significantly higher returns.

far below what would be required to equate to the negative return observed for Black households during the period.[43] We acknowledge further work to quantify the opportunity costs of homeownership would facilitate a broader understanding of the net return and rationality of homeownership choices along an extensive margin; but, for now the scope of the proceeding analysis focuses on quantifying the intensive margin and differences in unlevered returns. And, as we discuss in greater depth in the next section, our data is especially well-suited for this purpose.

4.   Methods

4.1.   FHTC within a Difference-in-Differences (DiD) Framework

Prior to evaluating whether the FHTC policy had a differential impact by race, we begin by first examining its overall impact on a household's gross return to selling its home (regardless of race). The FHTC provides a quasi-experimental policy shock to the U.S. housing market, as this temporary policy contained a discrete, arbitrary cutoff for eligibility by income (and other qualifications discussed in more detail below). Conceptually, this allows us to compare returns of those eligible as the "treatment group" to those who were *never* eligible or those who were *not initially* eligible as two different "control groups" before and after the policy was implemented.

For this analysis, we adapt the multivariate regression discussed in prior subsection (and tabulated in Appendix Table A1) to include the following difference-in-differences setup:

$$\text{ror}_{ht} = \beta_0 + \beta_1\,Post + \beta_2\,Eligible + \beta_3\,(Post * Eligible) + \sum \beta\,race_h +$$
$$\sum \gamma\,Household\,Demographics_h + \sum \delta\,Property\,Characteristics_h + \propto_h^{location} + \propto_t^{year-by-qtr.} + \varepsilon \quad (1)$$

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[43] For example, we observe it would take an annualized rental increase rate of over 12 percent to equate to the approximate 2 percent annualized negative rate of return to homeownership we observe for Black households if we use a simplified comparison of values near the mean household of our sample (a purchase price of $300,000, household income of $110,000, 33 percent of household income for determination of the initial annual rent ($36,300), and house ownership or rental tenure of 5 years). Given that the highest single year of rent increase was less than 6 percent, this suggests that escaping rent as an alternative did not offset the home value loss in this period for Black households.

where *ror* is the annualized rate of return (ROR) for a given household *h* when selling its home purchased in year *t*; *Post* for our default specification includes households *h* who purchased a home between January 1, 2009 and June 30, 2010, when the tax credit did not have to be paid back;[44] and, the *Treat* group includes those eligible for this policy by income and marital status who had a gross income less than $150,000 for joint-filers (or $75,000 for single and other filers) at the initial phase.[45] In this regression, β_3 is the diff-in-diff estimator on the homeowner's annualized gross rate of return, representing the marginal "post" effect of the policy treatment for the income-eligible cohort who purchased a home during the qualifying window relative to the control group's conditional $ror_{h,t}$.

The controls are defined as follows. Here *race* is defined as the race of householder *h*, which we code into mutually exclusive categorical dummy variables: White, Black (primary racial response), Asian (primary racial response), Hispanic (regardless of race), Other (all other racial responses).[46] We incorporate the following *Household Demographics* of the householder from the ACS and examine their impact on returns: age, education, marital status, and household size (see

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[44] The policy was passed and implemented in three phases. When the FHTC tax credit initially passed, the first phase (April 9, 2008-July 2009) included a tax credit up to $7,500 that had to be repaid over 15 years, which functioned as effectively a small interest free loan. Hembre (2018) and Berger, Turner, and Zwick (2020) found that this phase of the policy had little effect on incentivizing potential homeowners into homeownership. We exclude the period from our initial analysis where the repayable credit was the only option for new homebuyers (April 9-December 31 2008). The second and third phases made this credit effectively a grant up to $8,000 (or 10% of the purchase price), which did not have to be repaid unless the property was sold within 3 years and the seller made a capital gain on the home (but if the householder had died or sold the home within the 3 years without a capital gain, it also did not have to be repaid). The second phase was enacted on February 17, 2009 and included eligible first-time homebuyer purchases from January 1, 2009 through November 2009 (retroactively excluding first-time homebuyers from having to pay back the credit if they purchased the home after January 1, 2009), and the third phase extended this period through June 30, 2010 while expanding eligibility to higher earners. For illustration purposes in the Appendix (Table A5), we estimate a regression with all these moving parts, so-to-speak, which include all policy windows, all treated groups, and their corresponding interactions in an expanded form of the specification.

[45] The First-time Homebuyer Tax Credit did not literally require a household to be a homebuyer for the "first time." In addition to income-eligibility requirements, it required that the homebuyer not be a homeowner in the previous three years. We discuss below the limitation of not being able to identify who is a first-time homebuyer in the data.

[46] Although race and ethnicity are distinct individual attributes, here were use "race" as shorthand for both. Though there are numerous ways to characterize race and ethnicity, we chose this common breakout of the data, as institutions like the Census Bureau and Federal Reserve use a similar White/Black/Hispanic/Other categories when they report data on household assets in the SCF, for example. When the sample size is large in broad sample period (2000-2016) used in Appendix Table A1, we include the "Other" category; but, for most of our analysis, we omit this category when observation counts become sufficiently low.

Table 1 for variable descriptions and treatment). We explored multiple functional forms of these variables in untabulated tests, but chose the categorical and indicator variables for ease of interpretation and consistency with later stratifications.[47]

The remaining variables in equation (1) account for variation in returns to housing specific to the property characteristics, location, and time of the purchase/sale, which are commonly used in hedonic pricing models. Specifically, *Property Characteristics* come from both ACS and Zillow ZTRAX datasets, which include the following (with the corresponding dataset in parentheses): square footage of the living area of the home (ZTRAX), logged lot size (ZTRAX), number of years-owned or tenure fixed effects (ZTRAX), number of rooms (ACS), number of bedrooms (ACS), property age (ZTRAX), mortgage status (ACS), home value category (ACS), and quartiles of home value to income (ACS). We include fixed effects for tenure, or the number of years the household resided in their home between purchase and sale, to account for differences in holding periods of this asset. These variables and categorical thresholds are defined in more detail in Table 1. We include purchase quarter-by-year fixed effects for households who purchase a home in quarter *t*. To account for location-specific time-invariant heterogeneity, we include county fixed effects in our default specification. For robustness, we alter the location fixed effects to include different location and location-by-year interactions in later analyses. Given that these household characteristics are obtained via survey over an extended period of time, where the survey also changes subtly over time, we also include survey year fixed effects. We cluster robust standard errors by county.

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[47] We explore the sensitivity to using categories versus other forms of these demographic variables in another study using this linked data (Gindelsky et al. 2022), albeit covering a different topic. Our results are not sensitive to using categories (e.g., age buckets or a college education dummy) as opposed to a linear form of age or income, or a broader categorization of education.

At first glance, the specification above may appear oversimplified, given that the FHTC policy had multiple phases and expanded eligibility. To simply the comparisons for the diff-in-diff, we opt for a more straightforward approach by separately estimating two different stratifications throughout the proceeding analysis consistent with the specification (2) above. Specifically, we separately compare the treatment group with two different control groups: 1) never eligible (income > $225K for married households, >$125K for single/other filers), or 2) not initially eligible (married households making between $150-225K, singles making between $75-125K) for the full credit. In the first specification (i.e., "never eligible" control), we drop all observations from the second control group (i.e., "not initially eligible") so the treatment effect compares those eligible with only those who had never been eligible for the credit in any phase of the policy.[48] To limit the influence of outliers in the (likely) control group, we also drop household incomes greater than $500K, householders over 60 years old, all homes purchased for over $800K (since they would later be ineligible for the credit), and other ineligible sales (i.e., when the home was sold within 3 years of the initial purchase and the household had a capital gain).

One concern with this "never eligible" control group is that married households making more than $225K, for example, might be different than those making under $150K in ways unobservable to us even after accounting for a rich set of household controls. Therefore, these groups may have fundamentally different experiences in the housing market and thus trend differently prior to the policy. We examine common trends below to explore this possibility, but we also consider a second, alternative control group that is more comparable in terms of income.

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[48] There is also a phase-out portion of the credit, where households just outside the eligible cutoff can receive a partial credit. For simplicity, since these households do not receive the full credit, these households are lumped in with the control. We acknowledge that this may attenuate differences between the treatment and control group with part of the control group receiving a partial "dosage" of the treatment. However, this is not an issue with how we define the "never eligible" control cohort and our results are generally similar regardless of how we define the partial credit group.

In this second specification (with those "not initially eligible"), we compare the treated to those in the income range just over the initial treatment cutoffs (but under the "never eligible" cutoffs), excluding observations in the first ("never eligible") control group. Since this alternative control group would eventually become eligible, we exclude all observations during the policy window in which the second group becomes eligible in a later phase of the policy, shortening the *Post* period to be from January through November 2009 in a modified version of equation (2) above. If results stemming from the comparisons of the treatment to these control groups are similar, it may be indirect evidence that the unobservables associated with income (or potential differential trends) are not driving the underlying treatment effect. We explore other possibilities to address differences among the treatment and control households, like entropy balancing (Hainmueller 2012), in later analysis. We describe this in more detail in our results section below.

We follow both Hembre (2018) and Berger, Turner, and Zwick (2020) by focusing on the policy period where the credit did not have to be paid back as this is the period shown to have actually motivated marginal homebuyers with the credit. Thus, we drop homes purchased in the repayment period (April 9, 2008 through December 31, 2008). For robustness, we also vary the size of the windows of time prior to the treatment in our main analysis and in the Appendix. To clarify how the timelines and eligibility of these treatment and control groups compare, we illustrate the research design and corresponding diff-in-diff comparisons in Figure 1.[49]

4.2. Parallel Trends, Data Limitations, and Interpretations from this Approach

As we alluded to above, one concern about comparing the cohort directly affected by the policy (i.e., within the income-eligibility threshold) with those not eligible is that these groups

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[49] While some in the second group were eligible for a partial credit (a linear $20,000 income phase-out) in the initial phase, we exclude them in the first specification so the comparison is a cleaner "full eligible" versus "never eligible" interpretation.

may have empirically relevant differences, even beyond the household demographic factors for which we control for in the regression. For example, there may be differences in wealth (rather than income), tastes in homes/amenities, or other unobservable factors that could also lead to differences in gross returns to homeownership across these groups. However, for the interpretation to be causal, the diff-in-diff approach does not require treatment and control groups to be identical across observables or unobservables; rather, an important assumption for DiD is that the returns of the treatment and control group trended similarly prior to the policy period. We next examine this assumption directly by plotting the raw annualized rate of return on a home purchased over time for the treatment group and both control groups in the years leading up to the FHTC policy.

Figure 2 shows monthly averages of annualized ROR for homes purchased either a few years prior to the FHTC policy (since 2005 in the left column) or nearly a year and a half prior to the FHTC policy (since 2007 in the right column), where the latter is our default pre-policy period. The treatment group is depicted as the orange time series and the control groups are shown in blue (where the "not initially eligible" control group is in the top row and the "never eligible" group is in the bottom row). As the boom and bust in the U.S. housing market occurred around this time, the shape of the trends in Figure 2 match intuition, as households who purchased a home during this infamous period generally had a negative annualized ROR, which dipped more negative around the peak of the housing bubble in 2007. Overall, the trends in the raw data in Figure 2 suggest both the treatment group and control groups followed similar trends prior to the FHTC policy (since 2005 and since 2007). Homebuyers in both groups who purchased during the subsequent downturn had returns moving in the positive direction prior to the FHTC policy. While the treated group's time series is clearly shifted lower than both control groups, it follows a similar

trend over time. This is easier to see when we consider linear trend lines in both of the panels in the right column, showing an approximately parallel path.[50]

As we discussed in more depth in the data section above, one contribution of this study is the data. The data allows us to control for a variety of relevant household characteristics (like income, education, marital status, size of household, age, and race), which could contribute (either directly or indirectly) to heterogeneity in ROR, ultimately reducing model specification error. However, our data do not indicate whether the household had owned a home within the prior 3 years; so, the treatment cohort is determined by income eligibility.[51] Unlike an average treatment effect on the treated (ATT) estimator or the local average treatment effect (LATE), the interpretation of the treatment in our case is subtly different than a scenario where we knew with certainty that the individual household qualified *and* took-up the credit. Instead, the interpretation of this treatment is simply that they belong to the income-eligible cohort, whether or not they actually claimed the credit.[52]

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[50] We test whether these trends are statistically different from one another in untabulated regressions, where we regress these trends on the dependent variable (monthly average annualized ROR) along with group identifiers for treatment vs. control households and a corresponding interaction term. While there may appear to be slight visual differences in trends (perhaps due to the scale of the graph), in the regression results, we do not observe evidence to support a statistical difference in trends across the groups in any of these time series. This untabulated set of tests was approved by Census's Disclosure Review Board for the general public, but is omitted here for brevity and is available upon request.

[51] We use household income as reported in the ACS, not taxable income reported to the IRS, which exposes the estimates from this data to measurement error in two ways. First, there are documented differences between survey reported income and income claimed on taxes (Bee and Rothbaum 2019), however, AGI and total money income are correlated 99% (Gindelsky 2016) and measurement error at the median is classical (Bee and Rothbaum 2019). Second, the survey year does not necessarily coincide with the year of the home purchase, as the survey must have occurred *after* the initial purchase but *before* the eventual sale. We set the year of the home's purchase as our base year and adjust income based on PCE inflation (or average income growth for taxpayers in a similar strata of income during that period). For example, for a household surveyed in 2010 that purchased a home in 2009, we would adjust income for inflation (or the average growth in that strata) to 2009 dollars to represent income relevant for assessing the credit threshold limits in the year of purchase. On average, nominal income growth was slow during the period we analyze (e.g., nominal median household personal income grew 7% from 2007-2010 (BEA Distributional Accounts), but for an individual this might not be the case, as householders may have experienced a job change or promotion/demotion in a year that does not align with the year of the home purchase. This should be the case for both the treatment and control group, and thus the measurement error likely yields noisier results than if we had more precise income data.

[52] We also use age of the householder in our default specifications, which we use to exclude those from the sample who are least likely to be eligible, approximately 80% of those 60 are already homeowners.

One benefit of this approach is that the interpretation of the treatment group speaks to the more general policy objective concerning the effect of the tax credit on the broader income eligible group. That is, the broader measure can be interpreted as an estimate of the effect for the entire income cohort, whereas the policy eligibility and take-up is a narrower subset of that group.[53] On one hand, it is possible that the benefits/costs of those who took-up the credit were broadly offset by those who could not, where a null effect may represent this zero-sum outcome. After all, there are some zero-sum aspects of the housing market, where one winning bid on a home represents another potential buyer's losing bid (and thus buying elsewhere). On the other hand, an observed positive effect could represent an additive combination of: 1) a positive impact on those who received the credit, 2) a negative impact on those who did not receive the credit, but not enough to offset the positive impact on the former subset, and/or 3) a null or positive impact on those who did not receive the credit. While our measure of the treatment group does not disentangle these effects individually, it does help us answer a variation of the core question raised earlier in the paper: does a policy targeting a subset of low/medium income homebuyers help the overall cohort build (gross) home equity at a clip outpacing the ineligible higher-income homebuyer? Thus, for home equity to ameliorate the wealth gap among this cohort of earners, a broader measure of the policy effect on the low/middle-income cohort may be more suited to this purpose.

4.3.   FHTC and Differential Returns by Race

We modify the specification from equation (1) to explore heterogeneity in the FHTC policy effect on rate of return by race in a triple difference specification:

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[53] For example, an analysis of a change in the earned income tax credit (EITC) or minimum wage policy may not study the policy's effect exclusively on those who took up the credit or made minimum wage, rather it may include all low wage earners regardless of whether the credit or wage was binding.

$$\text{ror}_{ht} = \beta_0 + \beta_1 \, Post + \beta_2 \, Eligible + \beta_3 \, (Eligible * Post) + \beta_4 \, (Race * Post) + \beta_5 \, (Race *$$
$$Eligible) + \beta_6 \, (Race * Eligible * Post) + \beta_7 Race + \sum \gamma \, Household \, Demographics_h +$$
$$\sum \delta \, Property \, Characteristics_h + \propto_h^{location} + \propto_t^{quarter-by-year} + \varepsilon \qquad (2)$$

where the variables are defined the same as above, but where we separately interact Race (i.e., a binary indicator for nonwhite, Black, Hispanic, or Asian categories in the ACS) with our diff-in-diff parameters. Therefore, in this specification, β_6 is the diff-in-diff-in-diff (triple diff) estimator of interest, which estimates the relative rate of return on a home for a given racial category who purchased a home during the policy treatment window. We separately compare each estimated race effect against Whites as the comparison group. If these interactions are statistically significant and positive (negative), then the minority group in the treated income cohort has a higher (lower) return than comparable White households who have the same eligibility criteria and purchased a home during the policy window. If the β_6 coefficient is null and/or not statistically significant, then we interpret the policy as having a homogenous impact on a given racial minority household. In the context of the racial wealth gap, we interpret a positive and significant interaction coefficients as potentially narrowing this wealth gap in gross terms by yielding a relatively higher nominal return on this particular asset.[54]

From a parallel trends standpoint, we also compare relevant pre-trends in annualized ROR of the racial subgroups in Figure 3 below. As with our overall sample, a key identifying assumption for each minority subgroup is whether the control groups serve as reasonable counterfactuals by trending similarly prior to the policy shock. For a more apples-to-apples comparison in section 5, we depict the comparison of the treatment group and the *not initially eligible* group whose income

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[54] As we discussed above, perhaps not on net, if there are asymmetries in home improvement investments, transactions costs, or other costs that outweigh the gross return differences. While we have not yet seen evidence to suggest that the asymmetries would be large enough to substantially offset the effect sizes we observe, we leave it to future work to estimate racial differences in these costs.

is more comparable (i.e., just across the income threshold). A key takeaway from the top two panels in Figure 3 is that the treatment and control groups within Nonwhite and within White subsamples are not only trending similarly, but are also more or less on top of one another in absolute terms. Further, when we employ entropy balancing (section 5.4) to achieve better covariate balance (as evidenced by statistically indistinguishable means and variances across demographic covariates), our bottom two panels show a similar phenomenon among Black and Hispanic racial subgroups, where the trends are nearly identical. Like our earlier trend comparisons for the overall sample, we find none of these trends of the treatment group are statistically distinguishable from their corresponding control group at conventional levels. Taken together, these comparisons provide support that we have reasonable treatment and control groups for our difference-in-differences analyses, both for the overall sample and by race.[55]

5.   Results

5.1.   *Summary Statistics, Differences Across Groups, and Entropy Balancing*

Before we report the DiD regression results, we first compare summary statistics of the treatment and control groups tabulated in Table 3. The unconditioned, raw means of the annualized rate of return for the control groups and the treatment group are similar for the overall sample.[56] Property characteristics like age, number of years owned, and mortgage status are similar across groups, too. However, there are significant differences across many of the remaining variables,

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[55] We exclude Asian and Other races in this analysis because we do not find significant differences in our diff-in-diff-in-diff analysis for these subsamples. We discuss further in the results section. In a prior draft, we also explored trend comparisons excluding the lowest income group from the analysis for a more apples-to-apples comparison, which showed similar results and is available upon request.

[56] For conditioned estimates of differences in returns by race across relevant income groups, in Appendix Table A3 we re-estimate the default ROR regression from equation 1 for the full sample (columns 1 through 4 in Panel A) and the pre-FHTC sample (column 1 through 4 in Panel B), where we stratify the sample by: 1) lower income (half the eligibility cutoffs), 2) income eligible for all phases of the FHTC, 3) income eligible for the last phase (i.e., not initially eligible control group), and 4) never eligible income group, where income thresholds are defined in section 4.2. We find the most similar estimates among the FHTC eligible group and the not initially eligible control group (columns 2 and 3), while the largest differences among the lowest income and highest income stratifications.

particularly socioeconomic characteristics of the household. For example, while there are moderate differences in age of the householder, there are large difference in marriage rates, household size, income, and home value. These differences provide motivation for why we control for these variables, and DiD estimator is interpreted as a change in β while holding these factors constant.

To address concerns about selection issues, nonlinearities, and unobservables associated with some of these differences, we employ an entropy balancing technique proposed by Hainmueller (2012). Prior literature has paired matching and/or weighting in a pre-estimation step with a difference-in-differences analysis (e.g., Heckman et al. 1997), often using some variation of a matching approach like propensity score matching (PSM) to address differences in means between treatment and control (Caliendo and Kopeinig 2008). Over the last decade, Hainmueller's (2012) entropy balancing has become increasingly popular as a pre-estimation weighting step to balance covariates across moments, usually means and variances, with the benefit of avoiding the sample size reduction issues associated with other matching methods like exact matching, for example, and other issues with PSM (see King and Nielsen 2019). In Appendix Table A4, we tabulate the means and variances of the treatment and control samples before and after entropy balancing. This includes the overall sample in Panel A (used in Table 4) and samples used for our subsequent analysis of heterogeneous effects by race (in Panels B through D) that also balance covariates by race.[57] In regressions that use a balanced sample below, we use the not-initially eligible (Control 2) control group, since the raw differences in this group are generally closer to the treatment *ex ante*.

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[57] We use the "ebalance" program in Stata as described in Hainmueller and Xu (2013). We use the option to maintain the household weights designed for the ACS as the base weights. We follow other studies in applied microeconomics by balancing across two moments, mean and variance, for our treatment and control group (e.g., Marcus 2013). We also elected to keep covariates in our DiD estimation stage (though it is not required in entropy balancing) to reduce unexplained variance in the outcome. See Marcus (2013) for a nice discussion of these points in applying entropy balancing to a DiD policy setting.

Table 4 reports the DiD estimates from the specification laid out in equation (1) above, where we estimate the DiD design for the treated group relative to each control group separately.[58] That is, in the columns where we use households never eligible for the FHTC (Control 1) as the control group (col. (1)-(2)), we exclude all not initially eligible (Control 2) observations, to maintain a direct comparison. In the remaining columns (col. (3)-(6)), we exclude those never eligible (Control 1) from the sample. We have the additional restriction in the Control 2 sample that we omit control observations during the policy window in which they were eligible for the credit, as depicted in Figure 1. The results from the first two columns show that the income-eligible cohort had about a 1.0 to 1.2% higher annualized return compared to the never eligible control group, where the results are similar whether we use a sample going back to 2005 (column 1) or narrow the pre-treatment period to only include homes purchased since 2007 (column 2).

While the evidence of the parallel trends is strong for the never eligible group, the sizable differences in some of its covariates may still raise concerns about unobservables, particularly associated with high income households. We next turn to the results of a more comparable group, those not initially eligible (Control 2) for the FHTC, who are on the closer side of the eligibility cutoff. Column (3) reports a specification analogous to column (1), but with this control group from January 2005 through June 2010; and, columns (4)-(6) report the results compared to this group with entropy balancing across: demographic covariates only (col. 4), all demographic and

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[58] In Appendix Table A5, we estimate the effect of the policy windows separately. The initial column (1) draws from a broader sample that includes homes purchased in 2005 through 2010, where we have a DiD estimator for each policy period (A through D), which is an interaction between each respective Post variable and the treatment eligibility variable. We include controls and a full set of sample restrictions as in Table 4 in columns (2) and (3), respectively. We find mixed evidence that the initial period (A – April 2008 through December 2008), where the tax credit had to be paid back in subsequent periods, had a small positive effect. The next three phases have statistically significant effects, which we subsequently collapse into a single policy phase for the remainder of our analysis. Note that we have an interaction for the "not initially eligible" group during the window they were eligible (Post D – November 2009 through June 2010). As we discussed earlier, having both control groups in this regression will likely create some confusion about the treatment and control group comparisons, which is why we simplify our subsequent analysis.

property characteristic covariates (col. 5), and all covariates, but shortening the pre-treatment sample to 2007 (col. 6). Across these columns, we observe a positive and statistically significant coefficient for our DiD estimator of about 0.6% to 1.2%, suggesting that regardless of the control group, we find that income-eligible households who purchased a home during the main FHTC credit policy window (January 2009 through June 2010) had higher annualized returns.

In the Appendix, we alter these sample restrictions and specifications for robustness, finding broadly similar results for the policy effect, albeit with slightly different magnitudes for the DiD estimator. In Panel A of Appendix Table A6, we alter the pre-period length further, allowing purchases in the pre-period to go back two years (April 2006) or a single year (April 2007) prior to the initial FHTC policy phase. The DiD estimator is robust to alternative definitions of the pre-policy period, remaining statistically significant and lies within the same range as Table 4. Next, in Panel B, we tabulate the impact of iteratively adding controls to the default specifications for each control group. Finally, in Panel C, in order to examine the robustness of the results when we alter the fixed effects, we tabulate the results using census tract fixed effects (col. 2, 3, 5, and 6) and month-by-year fixed effects (col. 3 and 6). We find the results are qualitatively similar when we instead alter the model in these ways, although oversaturation and overfitting may become a concern when we incorporate increasingly finer fixed effects categories.

5.3.   FHTC Analysis – Diff-in-Diff Results by Race

In our next set of analyses, we consider differential returns by race for those income-eligible households for the FHTC. In Table 5, we report results that estimate equation (2) using the more comparable control group (not initially eligible – Control 2), where each race indicator is interacted with the corresponding DiD design variables. All columns have White householders as the reference group (excluding all other race categories) so that the comparison is more direct

with the White householder category. We have the same sample restrictions as columns (3)-(5) in the prior table.[59] In columns (1) and (2), we report the triple difference estimates for Nonwhite households without and with entropy balancing, respectively. The remaining columns continue to use entropy balancing as the pre-regression step.[60] Further, for more comparable groups prior to covariate balancing, we cut lower income households from the sample (i.e., halving the FHTC policy cutoffs – those with an adjusted household income less than $75K for married households ($37.5K for single)).[61] We call this subsample "Control 2a" in Figure 1 and subsequent tables. In columns (4)-(6), we break out race category comparisons for Black, Hispanic, and Asian householder categories, respectively.

The key results in the Table 5 show minority households, and Black households in particular, benefited from the First-time Homebuyer Tax Credit policies in 2009-10, where the interacted DiD estimators are statistically significant and positive in the first three columns. The evidence using these more comparable treatment and control groups suggests that income-eligible nonwhite homeowners realized a 1.5 to 2.0 percentage point higher annualized return (as shown by col. 1 and 2, respectively). The remaining columns show that this is likely driven primarily by Black households with a large (3.3 percentage points) higher annualized return, with a positive (but noisier) return among Hispanic homeowners and an insignificantly small (near 0) incremental benefit for Asian homeowners relative to the control group. For robustness, we re-estimate Table 5 using a shorter pre-FHTC period (beginning with purchases in January 2007), finding similar

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[59] Out of concern for thin cells, we do not include the "Other" race category here. We drop these from all subsequent samples.

[60] Following Hainmueller and Xu (2013), we balance covariates and their interactions with race categories when we want to balance moments across racial groups. To avoid an excessive number of interactions by race, we limit the covariate balance to the demographic variables, where selection issues are likely most binding. As we showed in Table 4 in a comparison across columns (4) and (5), we observe little benefit (or differences in coefficients) to balancing on property characteristics in this setting.

[61] This also excludes households potentially eligible for other pre-existing low-income and state-specific programs (like down-payment assistance for low income and minority households in particular). One such policy, the American Dream Downpayment Assistance Act of 2003, was a limited federal program administered by states that required FHTCs to have an income lower than 80% of their local's median income. The thresholds for the policy we examine exceed the median income (about $52K in 2008) of the U.S. generally, as well as the vast majority of locales in the U.S. during this period.

albeit somewhat larger effect sizes for DiD estimator interactions by race, but this contrast is particularly stark with Hispanic effect (a 2.7 percentage point higher annualized return), signifying a noisier effect that is more sensitive to specification than the effect for Black households. Taken together, the evidence from Tables 4 and 5 suggests that not only did the overall income-eligible cohort realize a higher gross annualized return to housing; but, minority homeowners in this cohort benefited as much or more than White households, potentially facilitating gains in wealth accumulation via housing capital gains for Black homeowners in particular.

5.4   Mechanism – where is the additional return coming from?

We next investigate whether the source of these returns is coming from buying the home for less, or selling it for more. Or, is it some combination of the two? To explore this, we adapt estimates of equation (2) to look at alternative dependent variables in Table 6: purchase prices (col. 1, 3, and 5) and eventual sale prices (col. 2, 4, and 6) for homes in which we observe both of these transactions for a given household surveyed. We revisit the significant effects from the last subsection for the Nonwhite and Black categories specifically. The results from columns (1) and (2) in Table 6 reveal that the FHTC policies enabled Nonwhite homeowners in this income cohort to purchase homes at a discount, but sell it at a price no different than comparable properties of White households.

Note that because we control for observable property characteristics, an alternative interpretation of this discount might be that minority households purchase homes that are lower quality in unobservable ways (e.g., lower quality countertops, lighting fixtures, window treatments, interior design, etc., which would not be directly accounted for by our controls). If this were the case, we would expect to see a similar discount on the back end when they sell the property with worse unobservables. However, note that the selling price effect is still close to zero

and statistically insignificant for Nonwhite households in Table 6 (col. 2). Given that we are using the same homes across specifications, one interpretation consistent with this evidence is that minority households may have been able to use this upfront capital (or otherwise freed up liquidity) to make such cosmetic changes in order to bring the prices in line with competing homes on the market of similar size.

Alternatively, another interpretation could be that the upfront capital may have enabled minority homeowners to participate in markets where homeowners were desperate to sell homes of standard quality, where they were able to sell the home at non-distressed prices later on.[62] The latter case is likely the case for Black homeowners, as illustrated by the very large discount tabulated in Table 6 in columns (3) and (5) (controlling for county fixed effects, the default in col. 3, and a much more stringent set of block group fixed effects in col. 5).[63] Either case, or some combination of the two, is consistent with upfront capital provided by the FHTC being more instrumental for Nonwhite homeowners, and Black homeowners most notably, which we might expect *ex ante* to be the case given well-documented wealth (and family wealth) differences.

We next turn to whether the *type* of house can provide useful information about where additional upfront capital has a greater differential return. Table 7 stratifies the sample of the Nonwhite, Black, and Hispanic triple difference specifications from Table 5 by newer (built 2000 and after) versus older homes (built prior to 2000). Like other physical capital assets, homes depreciate over time, as large maintenance/repair expenditures are more likely to come due as time goes on. Hence, while we do not have information in the ACS regarding maintenance and repair

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[62] Even though we exclude coded foreclosures and short sales, there may still be quasi-distressed sales where sellers are desperate to sell at a steep discount or have enough equity in the home that they can bear the equity loss without having to resort to foreclosure or short sales.

[63] Note that while the buy price and sell price estimates differ depending on the level of spatial fixed effects used in Table 6, the differential (and implied return) remains very similar across specifications. However, as we mentioned above, we express some caution with interpreting block group fixed effects in comparison with our sample in other specifications, given the drop in sample size due to limitations of thin cells dropping block groups without enough sales.

projects specifically, we use this to proxy for the notion that funds necessary to retain and enhance value in older homes are likely much larger than what can be covered by the $8,000 credit. Conversely, the $8,000 credit may more reasonably cover the types of costs needed to maintain and advance value in a newer home. We therefore confined the samples to newer homes in columns (1), (3), and (5), and older homes are in columns (2), (4), and (6) to allow all coefficients to vary on this dimension. Consistent with the capital constraints mechanism, we find that the positive impact of the FHTC on Nonwhite and Black housing returns is most pronounced and statistically significant among newer homes where the smaller amount of the credit may more reasonably play a role in returns. At the same time, we still observe positive, yet noisier, effects for the older home subsamples, which is consistent with the random shocks that exacerbate liquidity constraints as described by Boar et al. (2022). That said, we acknowledge that this is a simple stratification, and there could be a number of other relevant differences between older and newer homes.[64]

5.5   Unobservables, placebos, broader period tests – what is the likely direction of bias?

As with all causal inference empirical work, we should always have a critical eye when examining the research design and whether certain unobservable factors associated with the treatment (or control) classification are driving the key result. Like other policy shocks in the applied microeconomics literature, the arbitrary nature of the income thresholds for eligibility of the FHTC, at least on the surface, may allay some initial concerns about exogeneity. If these were not arbitrary (at least with respect to our outcome of interest), and there were relevant unobserved factors driving differences in returns for the treatment and control groups, we would first expect

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[64] Recall that the upfront capital provided by the FHTC is up to 10% of the purchase price of the home or a maximum of $8,000. Though this is a substantial sum for first-time homebuyers and most "starter homes," given that the average down-payment during this time was around 5-7% for most first-time homebuyers, it is not the kind of capital that is likely going to "flip" or substantially renovate homes. Indeed, older homes may have greater scope for large renovations with large returns on investment, so we caution against painting a picture with too broad of strokes here with respect to upfront capital and returns on investment. See:https://www.nar.realtor/blogs/economists-outlook/tackling-home-financing-and-down-payment-misconceptions

to see these show up in differences in trends prior to the policy. As we pointed out in section 4 above, the raw data plotted in Figures 2 and 3 allay the initial concern that these groups were trending in a way that predetermines our DiD results. However, these plots do not rule out the possibility that unobservables became relevant precisely around the timing of the policy shock. For example, what if during the FHTC policy period, those who ended up purchasing a home in higher income cohorts always underperformed the lower income cohorts? This could be due to a variety of reasons, possibly related to selection issues or perhaps macroeconomic factors during this period were somehow uniquely constraining for higher income individuals.

To investigate this possibility, we estimate false income thresholds or "placebo policies" in our main design in the Appendix. In Panel A of Table A8, we confine the sample to those not eligible, but instead assign a false eligibility to income groups above the threshold: 1) married $170-320k (single $95-170k) 2) married $200-350k (single $125-200k), 3) married $170-245k (single $95-145k), 4) married $200-275k (single $125-175k). In this case, we compare these placebo income groups to higher income groups in the prior control sample in our DiD design. If there are certain relevant unobservables or selection biases associated with income cohorts during the policy period, we would expect to see a positive and statistically significant effect for lower income cohorts, as they would outperform higher income cohorts in Table A8. In contrast to this explanation, none of the placebo interactions are statistically significant and all coefficient estimate magnitudes are near zero, ruling out the more fundamental concern than higher income groups always outperform during this period or some mechanical relation by income group.[65]

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[65] In future work, we plan to explore this issue even further. It is also possible the composition of the groups to change over time, as the lower income group could have positively selected households selecting into homeownership during a particularly volatile macroeconomic period in the US. To some extent, this underscores the need for having household level controls as we do currently (and the potential utility of entropy balancing insofar as these selection issues are present across observables). Based on feedback from a prior draft, we can also leverage geographic variation in the macroeconomic shock (e.g., areas may have experienced different shocks to unemployment) to evaluate whether the return differential is related to the treated cohort's ability to withstand economic shocks, which itself may be an alternative mechanism through which upfront capital may contribute to returns.

The evidence from outside the FHTC policy window also suggests that, if anything, the most likely expected bias in our DiD result is in the opposite direction of our findings, particularly for minority homeowners. We do not observe the wealth, family wealth, credit history, or a number of other potentially relevant financial factors for either our treated or control groups in our dataset. However, minority households in our treatment group are likely negatively selected on these attributes, as evidenced by Tables A1 and A3 in our appendix where we do a simple multivariate regression (as Table A3 is instead stratified by income cohort - panel A covers the entire 2000-2016 sample, while panel B is confined to the pre-FHTC period). Generally, we observe that higher income cohorts have higher returns in the broad sample of the data (Table A1) and minority households have the worst returns in the lowest income cohorts and perform better (or least less poorly) in the higher income cohorts (Table A3). In fact, Table A3 also shows the income cohorts that are most alike in terms of minority household performance are the ones we use throughout most of our FHTC analysis above – the eligible income cohort and the "not initially eligible" control group just over the income threshold. Thus, to the extent that unobservables are likely to bias our treatment, the evidence outside our policy window suggests that our findings may be somewhat conservative or perhaps understate the true effect size if similar headwinds are present.[66]

6.   Conclusion

The collective experience in the U.S. housing market during the boom-bust period in the first decade of the 21st century led many Americans to sour on the prospect of housing as a vehicle

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Additionally, examining variation in geographic regions more or less heavily influenced by the Financial Crisis would help to assess whether or how the effect with the FHTC may differ in more favorable economic periods outside the Financial Crisis.

[66] In Panel B of Appendix Table A8, we also explore additional placebo dates altering the timing for both the never eligible (Control 1) and not initially eligible (Control 2) DiD setups, which change the policy window to January through April 2007 outside of any federal FHTC. We estimate this in the 2005-2010 sample, including observations in the FHTC policy window (col. 1 and 2) and excluding observations purchased in the FHTC (col. 3 through 6). We find that none of these false treatment windows show statistically significant effects in any of these alternative specifications in Table A8. In untabulated tests we explore additional placebo windows finding similar null results, further supporting the interferences of the FHTC policy as being properly identified.

to building wealth. In their assessment of the recent literature and trends in the U.S. housing market, Goodman and Mayer (2018) concluded that, "while two decades of policies in the 1990s and early 2000s may have put too much faith in the benefits of homeownership, the pendulum seems to have swung too far the other way, and many now may have too little faith in homeownership" (p. 32-33). Taken together, our results support a more nuanced view of the housing market by knocking down strawmen on both ends of the pendulum.

On one end of the pendulum, the results from our initial descriptive statistics of the broader sample period (containing sales from 2000 to 2016) stand in stark contrast to the notion that homeownership is a universal panacea for building wealth. Using a unique sample of internal Census records linked with property-level data from Zillow's ZTRAX, the evidence from this rich data overwhelmingly suggests that the benefits of homeownership were not homogenous and uniformly shared across households. Specifically, we found Black and Hispanic households experienced significantly lower rates of returns to buying and selling single-family homes (relative to otherwise White households with similar observables). And, this return was even negative in nominal terms for Black households, on average. While our initial descriptive analysis is not causal in nature, these results would be difficult to reconcile with a view that housing is always a vehicle for building wealth in all circumstances, and one that would thus always reduce the racial wealth gap (provided that renting and other alternative assets are not worse). Strawmen notwithstanding, the more practical takeaway from our initial descriptive analysis of differential returns by race is that the magnitude of these differences can be quite large and meaningful, motivating future work on exploring the implications of differential returns to housing for wealth inequality and housing sector dynamics more generally. For example, non-financial returns to homeownership may

provide offsetting benefits (or exacerbating costs) over this period, which could be weighed against asset returns to homeownership when evaluating the net impact on wealth accumulation.[67]

On the other end of the pendulum, the results from our analysis of the FHTC show that not all homeownership policies of that era were disastrous for wealth accumulation among the low to middle end of the income distribution. Far from it – the evidence from our difference-in-differences analysis shows that the income-eligible cohort realized higher gross rates of returns to housing than non-eligible, high-income cohorts. More strikingly, when we compare cohorts of more similar income just above and below the eligibility threshold, minority households, and Black households in particular, realized substantially higher returns than White householders during the policy period. The results show the exogenous injection of capital from the FHTC was particularly impactful for minority households in the eligible income cohort. Provided that other factors do not swamp these gross returns, the evidence from our analysis of the FHTC suggests that mitigating initial capital constraints for homeowners in low to medium income households may have helped to reduce the racial wealth gap for this income cohort during this era.

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[67] Homeownership is often promoted as a vehicle for individual households to build wealth; however, we should note that this is a private good justification, as opposed to public good justifications that are particularly relevant for public policy considerations. There is a sizable literature suggesting that homeownership has a variety of positive spillovers or externalities, including improving social capital (DiPasquale and Glaeser 1999; Manturuk et al. 2010), political engagement (Engelhardt et al. 2010; McCabe 2013), tax compliance (Arbel et al. 2017), mobility (Sodini et al. 2016), neighborhood quality and other amenity value as measured by price appreciation (Coulson et al. 2003; Coulson and Li 2013; Hausman et al. 2022), and education benefits for children (Aaronson 2000; Green and White 1997). Additionally, there may be alternative aspects of the private good justification not captured with gross returns (e.g., avoiding fluctuations in rental prices) that influence the role of homeownership in building household wealth.

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Figure 1 – Treatment and Control Groups for a Diff-in-Diff Analysis of the First-time Homebuyer Credit
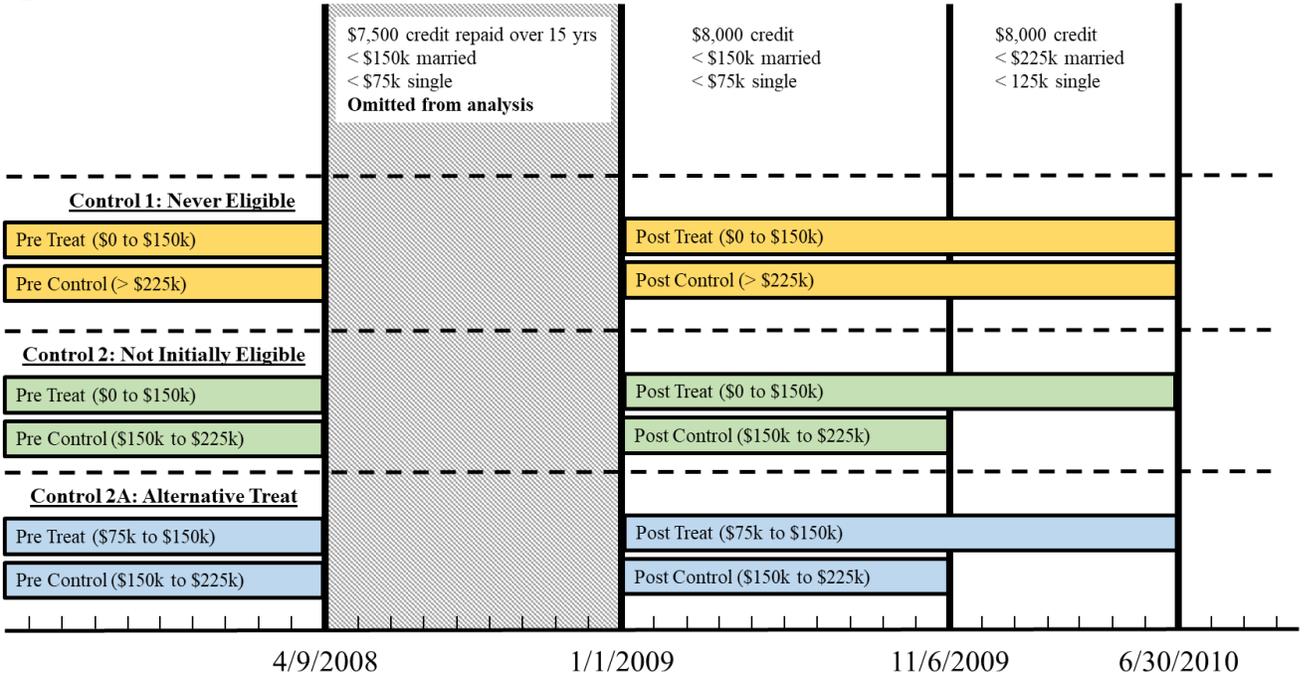
Treat and Control shown for married. Single groups can be determined using single income thresholds.

Figure 2 – Difference-in-differences Pre-Trend Analysis for the First-time Homebuyer Credit
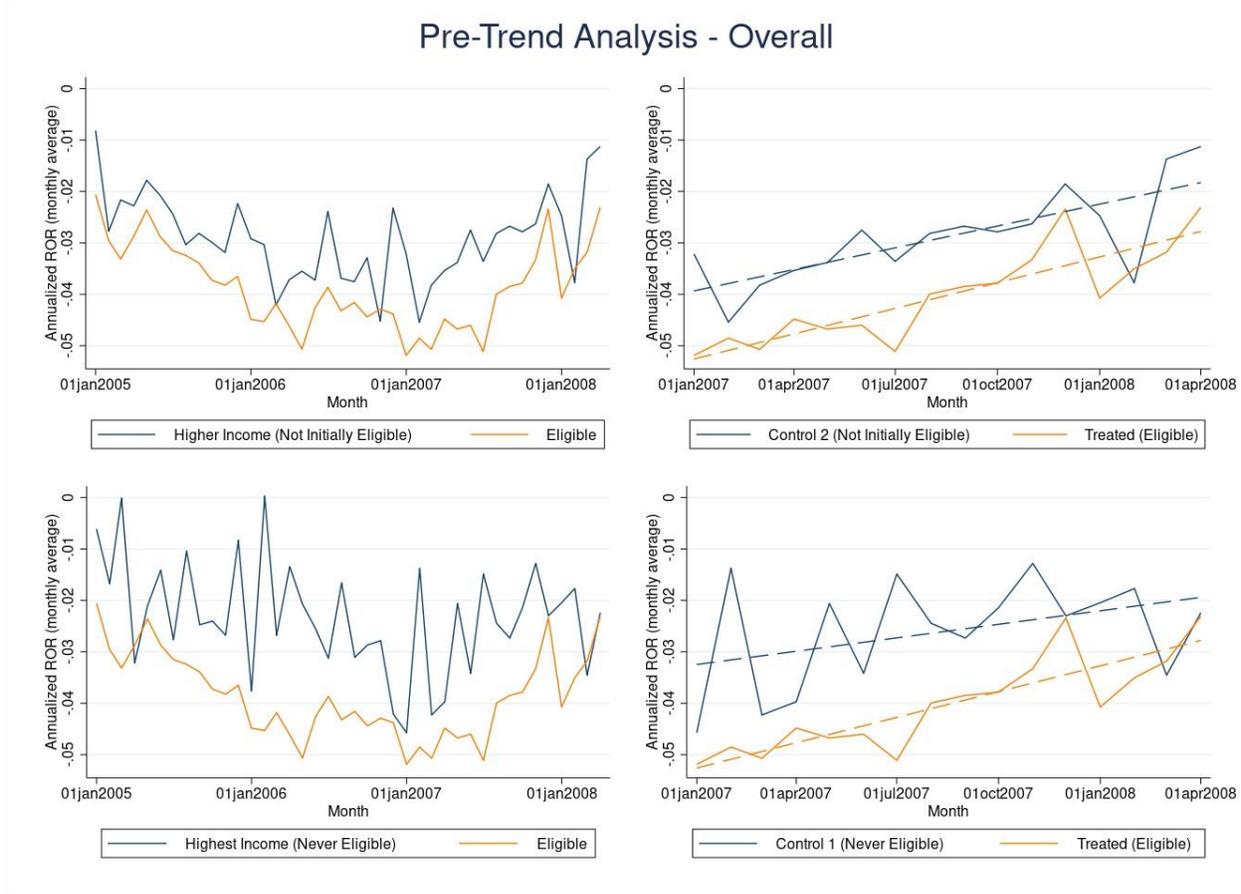
Notes: Figure 2 presents trends in annualized rates of return for households that would meet income threshold eligibility criteria for the First-Time Homebuyer Credit (eligible households) relative to alternative control groups for our difference-in-differences analysis in the periods leading up to credit policy period.

Figure 3 – Diff-in-Diff Pre-Trend Analysis for the First-time Homebuyer Credit by Race
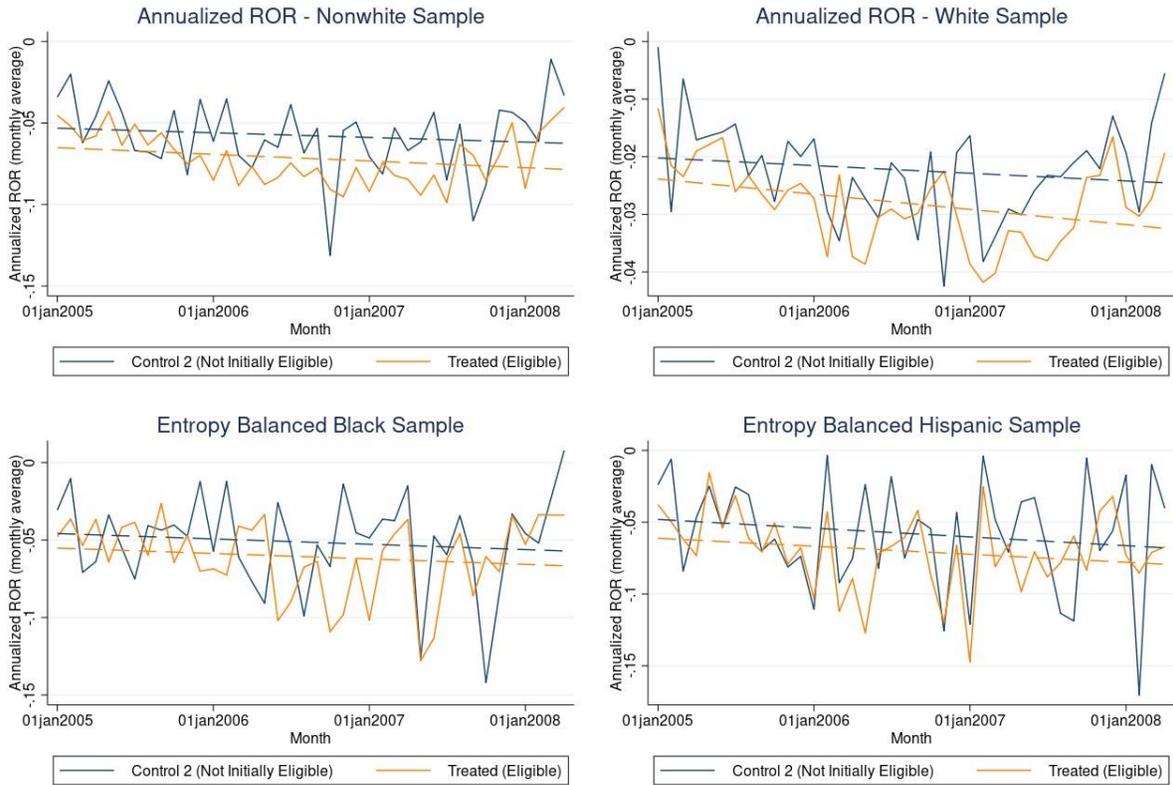
Notes: Figure 3 reports trends by race in annualized rates of return for households that would meet income threshold eligibility criteria for the First-Time Homebuyer Credit (eligible households) relative to those that would not initially meet eligibility thresholds in the periods leading up to credit policy period.

Table 1 – Variable Descriptions

Variable	Description
Dependent Variables	
Rate of Return (ROR)	(Sale Price – Buy Price)/Buy Price
Inflation-Adjusted ROR	ROR deflated by the Personal Consumption Expenditures (PCE) Index
Annualized ROR	(Sale Price/Buy Price)^(1/(Sale Year-Buy Year)) -1
Buy Price	Price a household initially paid for their home, as reported in ZTRAX then linked to ACS
Sale Price	Price a household eventually sold their home, as reported in ZTRAX then linked to ACS
Treatment Variables	
Eligible	Borrower is eligible for the FHTC based on adjusted household income (Dummy variable, Yes=1). Included as dummy and interacted with the credit window variables.
Credit Window	Period of time for which credit could be claimed (Dummy variables, Yes=1). Included as dummy and interacted with eligibility. **A:** April 9, 2008 – December 31, 2008 **B:** January 1, 2009 – June 30, 2009 **C:** July 1, 2009 – November 5, 2009 **D:** November 6, 2009 – June 30, 2010
Explanatory Variables	
Race	Race (ethnicity) of householder, as reported in ACS, coded into mutually exclusive categories (Dummy variables, Yes=1) White only Nonwhite (any of the below) Black (primary racial response) Asian (primary racial response) Hispanic (regardless of race) Other (all other racial responses)
Age	Age of the household head, as reported in ACS.
At least a Bachelor's	Dummy Variables: Yes=1 if education indicated Bachelor's degree or higher. Derives from the years of education of the householder, as reported in ACS.
Married	Dummy variable: Yes=1 if household head is married, as reported in ACS
Household Size	Number of household members, as reported in ACS.
Years Owned	Years since the householder owned the home, as reported in Zillow.
Property Age	Year purchased minus year the home was built, as reported in ZTRAX
Rooms	Number of rooms in the property, as reported in ACS
Ln(lot size)	Natural log of lot size measured in square feet, as reported in ZTRAX
Time in HH	Difference between sale date and initial purchase date, derived from ZTRAX. Discrete number of years are used as fixed effects.
Total sq. ft.	Square footage of the total living area in the home, as reported in ZTRAX. Scaled by 100 for coefficient interpretations.
Bedrooms	Number of bedrooms, as reported in ACS.
Income	Inflation-adjusted household income, as reported in ACS. Then, households are ranked (within state and survey year) and quintiles are constructed and used as below (Dummy variables, Yes=1) Low income (quintile 1) Medium income (quintiles 2, 3, and 4) High income (quintile 5)
Home Value	Inflation-adjusted home value, as reported in ACS. Then, households are ranked (within state and survey year) and quintiles are constructed and used as below (Dummy variables, Yes=1) Low value (quintile 1) Medium value (quintiles 2, 3, and 4) High value (quintile 5)
Home Val/Household Income	Home Value/Income, then divided into quintiles (ACS)
Mortgage status	Mortgage status of each household, as reported in ACS (Dummy variables, Yes=1, if household has a mortgage)

Table 2 - Summary Statistics for the Baseline Analysis Sample (2000-2016)

Panel A Descriptive Statistics (N = 149,000)

	Mean	St. Dev.
Annualized ROR	0.0053	0.0657
IAROR	-0.0426	0.3735
ROR	0.1068	0.4361
Married	0.6574	0.4746
Household Size	2.77	1.453
Age of Householder	44.29	13.78
At least a Bachelor's	0.5062	0.5
Household Income	107,900	103,800
Mortgage	0.8957	0.3057
Bedrooms	3.291	0.8001
Rooms	6.87	2.04
Property Age	25.69	19.37
Ln(lot size)	9.264	0.9077
Time in HH	5.022	3.122
Home Val/Household Income	0.4454	0.3372
Buy Price	285,800	259,300
Sale Price	305,800	297,200

Panel B Rates of Return for Single-Family Homes by Race (N = 149,000)

	Ann. ROR (Mean)	IAROR (Mean)	ROR (Mean)
White	0.0085	-0.0299	0.1218
Nonwhite	-0.0076	-0.092	0.0479
Black	-0.0231	-0.1923	-0.06
Hispanic	-0.0089	-0.0874	0.052
Asian	0.0095	-0.0118	0.1418

Panel C Representation by Category (N = 149,000)

Income Cohorts	% of the Sample in the Category
Lower: <$75k married; <37.5k single	23.75
Eligible: <$150k married; <75k single	54.75
Eligible2: >$150k & <225k married; >75k & <125k single	11.5

Race Categories	% of the Sample in the Category
White	79.7
Black	4.2
Asian	4.5
Hispanic	7.3
Other	4.3

Notes: Table 2 presents summary statistics for the sample of households in our baseline analysis examining determinants of ROR. Panel A reports the distribution of values for different ROR variables, a series of determinants we examine, and several component variables. Panel B provides means for our measure of ROR by race over the 2000-2016 sample period. Panel C outlines the distribution of households in two categories relevant to our later difference-in-differences analysis. Values are rounded in accordance with Census disclosure requirements. As values are rounded, the %'s in Panel C per category do not sum to exactly 100%.

Table 3 - Summary Statistics for the Difference-in-Differences Analysis (Overall) Sample

Panel A Descriptive Statistics for Different Groups

	FHTC Income-Eligible Group (N=34,000)		Control Group 1 – "Never Eligible" (N=3,000)		Control Group 2 – "Not Initially Eligible" (N=6,900)	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Annualized ROR	-0.0273	0.0719	-0.0161	0.0582	-0.0226	0.0608
IAROR	-0.1802	0.3561	-0.1419	0.3174	-0.1724	0.3101
ROR	-0.0786	0.3986	-0.0355	0.358	-0.0669	0.3517
Married	0.7216	0.4482	0.4589	0.4984	0.4816	0.4997
Household Size	3.012	1.449	2.539	1.5	2.582	1.457
Age of Householder	38.42	9.476	42.16	8.872	40.38	9.227
At least a Bachelor's	0.4571	0.4982	0.7694	0.4213	0.6671	0.4713
Household Income	76,420	37,610	250,000	94,120	145,200	51,600
Mortgage	0.9376	0.242	0.9466	0.2249	0.9534	0.2109
Bedrooms	3.218	0.7486	3.578	0.8479	3.381	0.7888
Rooms	6.615	1.892	7.782	2.314	7.164	2.099
Total sq. ft.	1841	803.8	2497	1097	2114	948.7
Property Age	26.79	19.86	24.61	20.54	25.92	20.78
Ln(lot size)	9.243	0.9064	9.358	0.9383	9.243	0.9292
Years in HH	4.072	1.977	4.074	2.038	4.152	2.038
Home Val/Household Income	0.4249	0.2654	0.7722	0.4593	0.5815	0.3203
Buy Price	244,900	138,700	415,300	177,600	335,300	164,600
Sell Price	215,000	1365,00	389,600	193,400	304,200	172,500

Panel B Race Category Breakouts by Group

Race Categories (% of the Group Subsample)

White	0.7687	0.8332	0.8113
Black	0.0547	0.0277	0.041
Asian	0.0397	0.0577	0.0511
Hispanic	0.0855	0.0488	0.0549
Other	0.0514	0.0327	0.0417

Notes: Table 3 presents summary statistics for the sample of households in our difference-in-differences analysis with the First-time Homebuyer Credit. Panel A reports the distribution of values for different ROR variables, a series of covariates, and several component variables. Panel B outlines the distribution of households by race. Values are rounded in accordance with Census disclosure requirements.

Table 4 - Difference-in-differences (DiD) Analysis with the First-time Homebuyer Credit

	Dependent Variable: Annualized ROR [Eq. (1)]					
	Control 1 Sample ("Never Eligible")	Control 1 Sample ("Never Eligible")	Control 2 Sample "Not Initially Eligible"	Control 2 Sample "Not Initially Eligible"	Control 2 Sample "Not Initially Eligible"	Control 2 Sample "Not Initially Eligible"
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Eligible	0.0123***	0.0096**	0.0055*	0.0098*	0.0121*	0.0123**
	(0.004)	(0.0039)	(0.0031)	(0.0054)	(0.0062)	(0.0057)
Post	-0.0072	-0.0044	-0.0007	-0.0066	-0.0114	-0.0148**
	(0.0057)	(0.0059)	(0.0045)	(0.0092)	(0.0098)	(0.0071)
Eligible	-0.0116***	-0.0119***	-0.0081***	-0.0123***	-0.0134***	-0.0141***
	(0.0017)	(0.0026)	(0.0012)	(0.0017)	(0.0018)	(0.0025)
Adj. R-Squared	0.4023	0.4247	0.4039	0.4267	0.4456	0.4964
N	37,000	17,500	41,000	41,000	41,000	17,500
Property Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Entropy Balanced Covariates	No	No	No	Demographic	Property	All
Purchase years in sample	2005-10	2007-10	2005-10	2005-10	2005-10	2007-10
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 4 report estimates of the difference-in-differences regression described by eq. (1), evaluating the impact of the First-time Homebuyer Credit. Columns differ by control group, whether entropy balancing was used for weighting, and the sample period used. The control group in Col. (1) and Col. (2) contains the income cohort who were never eligible for the credit. Col. (3)-(6) compare the treatment group to those not initially eligible (Control 2). Survey year, county, purchase quarter-by-year, and years owned fixed effects are used with all estimates. Prior to estimation, columns (4)-(6) are weighted using entropy balancing. Standard errors are clustered by county and reported in parentheses. Coefficients and N (obs) are rounded according to Census disclosure guidelines. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed).

Table 5 - Difference-in-differences (DiD) Analysis – By Race

	Dependent Variable: Annualized ROR [Eq. (2)]					
	Control 2 Sample "Not Initially Eligible"	Control 2 Sample "Not Initially Eligible"	Control 2a Sample "Not Initially Eligible"	Control 2a Sample "Not Initially Eligible"	Control 2a Sample "Not Initially Eligible"	Control 2a Sample "Not Initially Eligible"
	(1)	(2)	(3)	(4)	(5)	(6)
Nonwhite x Post x Eligible	0.0151* (0.0079)	0.0208** (0.0094)	0.0131 (0.009)			
Black x Post x Eligible				0.0331* (0.0178)		
Hispanic x Post x Eligible					0.0088 (0.0135)	
Asian x Post x Eligible						0.002 (0.0082)
Adj. R-Squared	0.4008	0.4216	0.4176	0.4005	0.4109	0.3927
N	39,500	39,500	26,500	24,000	24,500	24,500
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contains Other DiD Variables in eq. (2)	Yes	Yes	Yes	Yes	Yes	Yes
Excludes Lowest Income Cohort	No	No	Yes	Yes	Yes	Yes
Entropy Balanced Covariates	No	Yes	Yes	Yes	Yes	Yes
Purchase years in sample	2005-10	2005-10	2005-10	2005-10	2005-10	2005-10
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 5 reports coefficients from estimating eq. (2), which represents a difference-in-differences analysis of the FHTC by race. Col. (3) through (6) in this table exclude low income households from the analysis, keeping households within a symmetric range around the income eligibility threshold for the FHTC, and the last four columns employ entropy weights to balance demographic covariates described in the text (and their interactions by race). All columns compare the new treated cohort to the second control group ("not initially eligible"). Columns also differ based on which race is being compared to White households (and White households are the omitted group in all regressions in this table). Sample restrictions are described in the text. Standard errors are clustered by county and reported in parentheses. All coefficients and N (obs) are rounded according to Census disclosure guidelines. For brevity, we do not tabulate controls and additional DiD parameters described in eq. (2), but these are available upon request. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Table 6 – Triple DiD Analysis by Race for a Household's Initial Purchase and Eventual Sale Price

Control Group in Sample: "Not Initially Eligible" Cohort (Control 2)

Dependent Var.	Buy Price	Sale Price	Buy Price	Sale Price	Buy Price	Sale Price
	(1)	(2)	(3)	(4)	(5)	(6)
Nonwhite x Post x Eligible	-0.0958* (0.0493)	0.0085 (0.0483)				
Black x Post x Eligible			-0.1538 (0.0942)	0.0278 (0.094)	-0.2460*** (0.0896)	-0.1485 (0.1065)
Adj. R-Squared	0.7575	0.7055	0.8245	0.7888	0.9044	0.8906
N	39,500	39,500	24,000	24,000	9,400	9,400
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contains Other DiD Variables in eq. (2)	Yes	Yes	Yes	Yes	Yes	Yes
Excludes Lowest Income Cohort	No	No	Yes	Yes	Yes	Yes
Entropy Balanced Covariates	No	No	Yes	Yes	Yes	Yes
Purchase years in sample	2005-10	2005-10	2005-10	2005-10	2005-10	2005-10
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Spatial FE	County	County	County	County	Block Group	Block Group
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 6 reports coefficients from estimating a variation of eq. (2), which is estimated on the (logged) initial sale price for which a household purchased their home (col. 1, 3, 5) and the (logged) price for which the home eventually sold (col. 2, 4, 6). Like Table 5, col. (3) through (6) in this table exclude low income households from the analysis and employ entropy weights to balance demographic covariates described in the text (and their interactions by race). All columns compare the new treated cohort to the second control group ("not initially eligible"). Columns also differ based on which race is being compared to White households (and White households are the omitted group in all regressions in this table). Sample restrictions are described in the text. Standard errors are clustered by county and reported in parentheses. All coefficients and N (obs) are rounded according to Census disclosure guidelines. For brevity, we do not tabulate controls and additional DiD parameters described in eq. (2), but these are available upon request. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Table 7 – Triple DiD Analysis by Race: Newer vs. Older Home Stratifications

Sample Stratification:	Homes Built 2000+	Homes Built Prior to 2000	Homes Built 2000+	Homes Built Prior to 2000	Homes Built 2000+	Homes Built Prior to 2000
	Dependent Variable: Annualized ROR [Eq. (2)]					
	(1)	(2)	(3)	(4)	(5)	(6)
Nonwhite x Post x Eligible	0.0300*** (0.0106)	0.0121 (0.0104)				
Black x Post x Eligible			0.0439** (0.0223)	0.0227 (0.0217)		
Hispanic x Post x Eligible					0.0292 (0.0199)	0.0086 (0.0154)
Adj. R-Squared	0.4283	0.4022	0.4497	0.4016	0.4538	0.4112
N	7,500	31,500	4,700	19,000	4,700	19,500
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contains Other DiD Variables in eq. (2)	Yes	Yes	Yes	Yes	Yes	Yes
Excludes Lowest Income Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Entropy Balanced Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Purchase years in sample	2005-10	2005-10	2005-10	2005-10	2005-10	2005-10
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 7 reports coefficients from estimating eq. (2) as in Table 5, instead stratifying the samples by whether the home was built in 2000 or later (i.e., newer) versus homes built prior to 2000 (i.e., older). Like Tables 5 and 6, Col. (3) through (6) in this table exclude low income households from the analysis and employ entropy weights to balance demographic covariates described in the text (and their interactions by race). All columns compare the new treated cohort to the second control group ("not initially eligible"). Columns also differ based on which race is being compared to White households (and White households are the omitted group in all regressions in this table). Sample restrictions are described in the text. Standard errors are clustered by county and reported in parentheses. All coefficients and N (obs) are rounded according to Census disclosure guidelines. For brevity, we do not tabulate controls and additional DiD parameters described in eq. (2), but these are available upon request. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Appendix Tables and Figures

Table A1 - OLS Regressions of Rates of Return for Single-Family Homes on Household and Property Characteristics

Dependent Variable:	ROR (1)	IAROR (2)	Ann. ROR (3)	Ann. ROR (4)	Ann. ROR (5)	Ann. ROR (6)
Black	-0.1127***	-0.0957***	-0.0233***	-0.0223***	-0.0197***	-0.0190***
Asian	-0.0196***	-0.0162***	-0.0041***	-0.0038***	-0.0038***	-0.0035***
Hispanic	-0.0588***	-0.0480***	-0.0162***	-0.0154***	-0.0124***	-0.0122***
Other	-0.0629***	-0.0511***	-0.0155***	-0.0148***	-0.0118***	-0.0118***
Married	0.0416***	0.0347***			0.0072***	0.0073***
Household Size	-0.0165***	-0.0139***			-0.0029***	-0.0030***
Age of householder	-0.0013***	-0.0012***			-0.0001***	-0.0002***
At least a Bachelor's	0.0253***	0.0214***			0.0052***	0.0052***
Medium Income	0.0610***	0.0474***			0.0078***	0.0084***
High Income	0.1036***	0.0811***			0.0132***	0.0139***
Has Mortgage	-0.0520***	-0.0468***		-0.0076***		-0.0091***
Bedrooms	0.0061**	0.0054**		0.0003		0.0012***
Rooms	0.0003	0.0002		0.0003***		0.0001
Total sq. ft. /100	0.0004	0.0003		0.0002***		0
Property age	0.0016***	0.0013***		0.0001***		0.0001***
Ln(Lot Size)	0.001	0.0009		-0.0002		-0.0004
Home Value Quartile 2	-0.0359***	-0.0301***			-0.0054***	-0.0050***
Home Value Quartile 3	-0.0469***	-0.0387***			-0.0065***	-0.0060***
Home Value Quartile 4	-0.0632***	-0.0520***			-0.0079***	-0.0074***
Home Value Quartile 5	-0.0938***	-0.0757***			-0.0126***	-0.0123***
Constant	0.1374***	-0.0083	0.0083***	0.0077**	0.0117***	0.0169***
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase Qtr.*Yr. FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Transaction Years	2000-2016	2000-2016	2000-2016	2000-2016	2000-2016	2000-2016
Adj. R-squared	0.2881	0.2961	0.3337	0.3382	0.3433	0.3459
N	149,500	149,500	149,500	149,500	149,500	149,500

Notes: Table A1 reports the estimation of the following multivariate regression at the household level using the broader 2000-2016 linked sample of transactions:

$$ror_{ht} = \beta_0 + \sum \beta\, race_h + \sum \gamma\, Household\ Demographics_h + \sum \delta\, Property\ Characteristics_h + \propto_h^{location} + \propto_t^{quarter-by-year} + \varepsilon \quad (A1)$$

Variables in eq. (A1) above are defined as in eq. (1). Columns differ based on dependent variable used (col. 1 ROR, col. 2 IAROR, and cols. 3-6 annualized ROR) or which groups of controls are included. Standard errors are clustered by county. All coefficients and N (obs) are rounded according to Census disclosure guidelines. Note that some coefficient estimates are zero due to Census rounding rules. We omit reporting standard errors for brevity in the table. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Table A2 – Regressions on Rates of Return & Home Prices by Varying Spatial F.E.

Panel A - Dependent Variable: Annualized ROR [Eq. (1)]

	(1)	(2)	(3)	(4)	(5)	(6)
Black	-0.0201***	-0.0121***	-0.0122***	-0.0133***	-0.0188***	-0.0219***
	(0.0018)	(0.0015)	(0.0021)	(0.0015)	(0.0017)	(0.0018)
Asian	-0.0021*	-0.0032**	-0.0027*	-0.0040***	-0.0041***	-0.0046***
	(0.0012)	(0.0013)	(0.0014)	(0.0011)	(0.001)	(0.0012)
Hispanic	-0.0100***	-0.0095***	-0.0095***	-0.0111***	-0.0112***	-0.0141***
	(0.0014)	(0.0012)	(0.0014)	(0.0013)	(0.0013)	(0.0016)
Other	-0.0106***	-0.0089***	-0.0090***	-0.0099***	-0.0108***	-0.0129***
	(0.0014)	(0.0013)	(0.0016)	(0.0014)	(0.0014)	(0.0015)
Adj. R-Squared	0.3525	0.4123	0.4425	0.3759	0.3944	0.2318
N	149,000	142,000	121,000	147,000	148,000	149,000
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes	Yes	No
Spatial FE	State	Tract	Block Group	Zip Code	County-by-year	County*Qtr time trend

Panel B

Dependent Variable:	Buy Price	Buy Price	Buy Price	Sale Price	Sale Price	Sale Price
	(1)	(2)	(3)	(4)	(5)	(6)
Black	-0.1299***	-0.0358***	-0.0085	-0.2802***	-0.1325***	-0.0956***
	(0.0137)	(0.0073)	(0.0074)	(0.0218)	(0.0099)	(0.0084)
Asian	-0.0092	-0.0186***	-0.0146***	-0.0354***	-0.0508***	-0.0419***
	(0.0066)	(0.0042)	(0.0047)	(0.0085)	(0.0064)	(0.0076)
Hispanic	-0.0389***	0.0016	0.0083*	-0.1200***	-0.0688***	-0.0518***
	(0.0093)	(0.0044)	(0.0049)	(0.0101)	(0.0054)	(0.0058)
Other	-0.0473***	-0.007	-0.0009	-0.1289***	-0.0729***	-0.0563***
	(0.0126)	(0.0059)	(0.0066)	(0.0115)	(0.0052)	(0.0056)
Adj. R-Squared	0.7731	0.8366	0.8583	0.7654	0.8463	0.8711
N	149,000	147,000	142,000	149,000	147,000	142,000
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes	Yes	No
Spatial FE	County	Zip Code	Tract	County	Zip Code	Tract

Notes: Panel A in Table A2 presents variations of Table A1 specifications, while Panel B presents (logged) buy and sell price dynamics, with varying spatial fixed effects across columns. Sample restrictions are described in the text. Standard errors are clustered by county and reported in parentheses. All coefficients and N (obs) are rounded according to Census disclosure guidelines. For brevity, we do not tabulate controls. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Table A3 – Regressions on Rates of Return by Income Cohort and Period

		Dependent Variable: Annualized ROR [Eq. (1)]		
		Panel A – Overall Sample		
	(1)	(2)	(3)	(4)
Black	-0.0233***	-0.0168***	-0.0181***	-0.0142***
	(0.0029)	(0.0027)	(0.0032)	(0.0051)
Asian	-0.0047**	-0.0051***	-0.0039	0.0050*
	(0.0022)	(0.0014)	(0.004)	(0.0027)
Hispanic	-0.0187***	-0.0065***	-0.0095**	-0.0049
	(0.0023)	(0.0018)	(0.0038)	(0.0034)
Other	-0.0133***	-0.0134***	-0.0106**	-0.0015
	(0.0032)	(0.0022)	(0.0049)	(0.0059)
Adj. R-Squared	0.3673	0.3655	0.3728	0.3513
N	34,500	46,000	17,000	9,400
Controls	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes
Spatial FE	County	County	County	County
Income Cohort	Lower	Eligible	Eligible2	Never Eligible
		Panel B – Homes Initially Purchased Prior to April 2008		
	(1)	(2)	(3)	(4)
Black	-0.0249***	-0.0180***	-0.0161***	-0.0119***
	(0.0031)	(0.0032)	(0.0036)	(0.0044)
Asian	-0.0097***	-0.0053***	-0.0013	0.0033
	(0.0021)	(0.0016)	(0.003)	(0.0025)
Hispanic	-0.0237***	-0.0104***	-0.0116***	-0.0151***
	(0.0034)	(0.002)	(0.0038)	(0.0048)
Other	-0.0213***	-0.0161***	-0.0225***	-0.0104*
	(0.0026)	(0.0024)	(0.0054)	(0.0062)
Adj. R-Squared	0.3421	0.3589	0.3476	0.3116
N	21,500	28,000	10,000	5,600
Controls	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes
Spatial FE	County	County	County	County
Income Cohort	Lower	Eligible	Eligible2	Never Eligible

Notes: Panel A in Table A3 presents variations of Table A1 specifications constrained to relevant income cohorts in subsequent analyses of the FHTC (see Table A1 for definitions of each group). Sample restrictions are described in the text. Standard errors are clustered by county and reported in parentheses. All coefficients and N (obs) are rounded according to Census disclosure guidelines. For brevity, we do not tabulate controls. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Table A4 – Sample Means & Variances: Before & After Entropy Balancing

Panel A – DiD Sample (Control 2)

	Treatment		Control Before		Control After	
	Mean	*Variance*	*Mean*	*Variance*	*Mean*	*Variance*
Black	0.05469	0.0517	0.041	0.0394	0.0548	0.0518
Asian	0.03967	0.0381	0.0511	0.0485	0.0397	0.0381
Hispanic	0.08553	0.0782	0.0549	0.0519	0.0855	0.0782
Other Race	0.05141	0.0488	0.0417	0.04	0.0514	0.0487
Married	0.7216	0.2009	0.4816	0.2497	0.7214	0.201
Household Size	3.012	2.101	2.582	2.124	3.012	2.101
Age	38.42	89.8	40.38	85.13	38.42	89.8
At least Bachelor's	0.4571	0.2482	0.6671	0.2221	0.4572	0.2482
Mortgage	0.9376	0.0586	0.9534	0.0445	0.9376	0.0586
Bedrooms	3.218	0.5603	3.381	0.6222	3.218	0.5603
Rooms	6.615	3.582	7.164	4.406	6.615	3.582
Total Sq. Ft. / 100	18.41	64.62	21.14	90.01	18.41	64.62
Property Age	26.79	394.3	25.92	411.2	26.79	394.3
Ln(lot size)	9.243	0.8216	9.243	0.8633	9.243	0.8216
Home Value/Household Income	0.4249	0.0704	0.5815	0.1026	0.4253	0.0707

Panel B – DiD Sample (Control 2 – Nonwhite vs. White Omitted)

	Treatment		Control Before		Control After	
	Mean	*Variance*	*Mean*	*Variance*	*Mean*	*Variance*
Married	0.7219	0.2008	0.4855	0.2498	0.7218	0.2008
Household Size	2.977	2.033	2.562	2.021	2.976	2.033
Age	38.45	90.54	40.49	85.2	38.45	90.54
At least Bachelor's	0.4681	0.249	0.6732	0.22	0.4682	0.249
Home Value/Household Income	0.4267	0.0702	0.5814	0.1019	0.4269	0.0703
Nonwhite*Married	0.1283	0.1118	0.0662	0.0618	0.1282	0.1118
Nonwhite*Household Size	0.6335	2.205	0.4404	1.469	0.6334	2.205
Nonwhite*Age	7.422	250	6.116	218.3	7.421	250
Nonwhite*At least Bachelor's	0.0707	0.0657	0.0981	0.0885	0.0707	0.0657
Nonwhite*Home Val/Income	0.0755	0.0399	0.088	0.0635	0.0756	0.0399

Table A4 Cont. – Sample Means & Variances: Before & After Entropy Balancing

Panel C - DiD Sample (Control 2 – Black vs. White Omitted)

	Treatment		Control Before		Control After	
	Mean	*Variance*	*Mean*	*Variance*	*Mean*	*Variance*
Married	0.7116	0.2052	0.4886	0.2499	0.7116	0.2053
Household Size	2.851	1.809	2.516	1.925	2.851	1.809
Age	38.54	87.12	40.71	86.23	38.54	87.13
At least Bachelor's	0.5689	0.2453	0.6771	0.2187	0.5689	0.2453
Home Value/Household Income	0.4978	0.0679	0.5868	0.1037	0.4978	0.0679
Black*Married	0.0284	0.0276	0.0162	0.016	0.0284	0.0276
Black*Household Size	0.1695	0.601	0.1281	0.4447	0.1695	0.601
Black*Age	2.308	91.23	2.04	86.74	2.308	91.23
Black*At least Bachelor's	0.0285	0.0276	0.0296	0.02871	0.0285	0.0276
Black*Home Val/Income	0.0302	0.0212	0.0317	0.0324	0.0302	0.0212

Panel D - DiD Sample (Control 2 – Hispanic vs. White Omitted)

	Treatment		Control Before		Control After	
	Mean	*Variance*	*Mean*	*Variance*	*Mean*	*Variance*
Married	0.7228	0.2004	0.4852	0.2498	0.7228	0.2004
Household Size	2.889	1.87	2.527	1.952	2.889	1.871
Age	38.4	85.95	40.49	86.64	38.4	85.95
At least Bachelor's	0.5596	0.2465	0.6652	0.2227	0.5596	0.2465
Home Value/Household Income	0.4971	0.0667	0.5814	0.0955	0.4971	0.0667
Hispanic*Married	0.0474	0.0451	0.0207	0.0203	0.0474	0.0451
Hispanic*Household Size	0.2382	0.9573	0.1788	0.6406	0.2382	0.9573
Hispanic*Age	2.602	95.9	2.464	94.89	2.602	95.9
Hispanic*At least Bachelor's	0.0253	0.0247	0.0285	0.0277	0.0253	0.0247
Hispanic*Home Val/Income	0.0349	0.0224	0.0355	0.0244	0.0349	0.0224

Table A5 - DiD Analysis with the First-time Homebuyer Credit – Separately Estimating Phases

Dependent Variable: Annualized ROR [Eq. (1)]

	(1)	(2)	(3)
Eligible	-0.0041***	-0.0090***	-0.0121***
	(0.0012)	(0.0015)	(0.0016)
Post A	0.0042	0	-0.0038
	(0.0029)	(0.0028)	(0.0029)
Post A x Eligible	0.0021	0.0064***	0.0042**
	(0.0019)	(0.0019)	(0.0021)
Post B	-0.002	-0.0052	-0.0053
	(0.0047)	(0.0049)	(0.0063)
Post B x Eligible	0.0099***	0.0144***	0.0144***
	(0.0027)	(0.0032)	(0.004)
Post C	-0.0024	-0.0053	0.0017
	(0.0039)	(0.004)	(0.0053)
Post C x Eligible	0.0041	0.0077**	0.0005
	(0.0034)	(0.0036)	(0.0037)
Post D	-0.006	-0.0094	0.0029
	(0.0056)	(0.0062)	(0.0085)
Post D x Eligible	0.0143***	0.0208***	0.0088
	(0.0043)	(0.0048)	(0.0057)
Eligible 2	-0.0015	-0.0019*	-0.0037***
	(0.0009)	(0.0011)	(0.0013)
Post D x Eligible 2	0.0110***	0.0134***	0.0115*
	(0.0037)	(0.0041)	(0.0069)
Adj. R-Squared	0.3495	0.3015	0.3893
N	90,000	73,500	50,500
Controls	Yes	Yes	Yes
Sample Period	2005-2014	2005-2010	2005-2010
Survey Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes

Notes: Table A5 reports estimates of the eq. (1) difference-in-differences using the First-time Homebuyer Credit, separating out the policy phases that were either collapsed into a single period or, in the case of the initial phase A, dropped in the main analysis. Col. (1) examines all treatment periods within the same regression with a long sample. Columns (2) and (3) shorten the post-period to the default, and the latter column contains the same sample restrictions as the default regressions in Table 4. Standard errors are clustered by county and reported in parentheses. Coefficients and N (obs) are rounded according to Census disclosure guidelines. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Table A6 – DiD Alternative Specifications

Dependent Variable: Annualized ROR [Eq. (1)]

Panel A – FHTC DiD Results: Varying Pre-Treatment Time Windows

	(1)	(2)	(3)	(4)	(5)	(6)
Control Sample:	**Control 1**	**Control 2**	**Control 1**	**Control 2**	**Control 1**	**Control 2**
Post	-0.0047	-0.0006	-0.0044	-0.0022	-0.0053	-0.0032
	(0.0057)	(0.0046)	(0.0059)	(0.0046)	(0.006)	(0.0044)
Eligible	-0.0117***	-0.0096***	-0.0119***	-0.0106***	-0.0131***	-0.0114***
	(0.002)	(0.0014)	(0.0026)	(0.0017)	(0.0028)	(0.0018)
Post*Eligible	0.0112***	0.0062*	0.0096**	0.0069**	0.0102**	0.0072**
	(0.0038)	(0.0033)	(0.0039)	(0.0034)	(0.0042)	(0.0033)
Adj. R-Squared	0.4219	0.4231	0.4247	0.4265	0.4246	0.4261
N	24,500	27,000	17,500	19,500	16,000	17,500
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Spatial FE	County	County	County	County	County	County
Time Period	4/2006-6/2010	4/2006-6/2010	1/2007-6/2010	1/2007-6/2010	4/2007-6/2010	4/2007-6/2010

Panel B – FHTC DiD Results: Varying Spatial and Time Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Control Sample:	**Control 1**	**Control 1**	**Control 1**	**Control 2**	**Control 2**	**Control 2**
Post	-0.0088*	-0.0016	-0.0043	-0.0022	-0.0035	-0.0037
	(0.0048)	(0.0059)	(0.0071)	(0.0037)	(0.005)	(0.0063)
Eligible	-0.0126***	-0.0001	-0.0002	-0.0094***	-0.002	-0.0021
	(0.0016)	(0.0021)	(0.0021)	(0.0011)	(0.0013)	(0.0013)
Post*Eligible	0.0132***	0.0094**	0.0091**	0.0064**	0.0080**	0.0073**
	(0.0035)	(0.0041)	(0.0041)	(0.0028)	(0.0032)	(0.0033)
Adj. R-Squared	0.4625	0.505	0.5058	0.4638	0.5029	0.5037
N	37,000	27,000	27,000	41,000	31,000	31,000
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Qtr.*Yr.	Qtr.*Yr.	Month*Yr.	Qtr.*Yr.	Qtr.*Yr.	Month*Yr.
Spatial FE	County	Tract	Tract	County	Tract	Tract
Time Period	1/2005-6/2010	1/2005-6/2010	1/2005-6/2010	1/2005-6/2010	1/2005-6/2010	1/2005-6/2010

Notes: Table A6 provides alternative specifications estimating eq. (1) difference-in-differences. The Panel A regressions alter the pre-treatment period length. Panel B alters the spatial and time fixed effects for robustness. Standard errors are clustered by county and reported in parentheses. Coefficients and N (obs) are rounded according to Census disclosure guidelines. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Table A7 – Alternative Specifications for the DiD Analysis by Race

	Dependent Variable: Annualized ROR [Eq. (2)]			
	Control 2 Sample	**Control 2a Sample**	**Control 2a Sample**	**Control 2a Sample**
	(1)	(2)	(3)	(4)
Nonwhite x Post x Eligible	0.0354*** (0.0104)	0.0180** (0.009)		
Black x Post x Eligible			0.0403* (0.022)	
Hispanic x Post x Eligible				0.0272* (0.0154)
Adj. R-Squared	0.4543	0.4441	0.4355	0.441
N	18,500	12,500	11,500	11,500
Controls	Yes	Yes	Yes	Yes
Contains Other DiD Variables in eq. (2)	Yes	Yes	Yes	Yes
Excludes Lowest Income Cohort	No	Yes	Yes	Yes
Entropy Balanced Covariates	No	Yes	Yes	Yes
Purchase years in sample	1/2007-6/2010	1/2007-6/2010	1/2007-6/2010	1/2007-6/2010
Survey Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes

Notes: Table A7 reports coefficients from estimating eq. (2) like Table 5, but shortens the pre-treatment window to January 2007. Sample restrictions are described in the text and are the same as Table 5. Standard errors are clustered by county and reported in parentheses. All coefficients and N (obs) are rounded according to Census disclosure guidelines. For brevity, we do not tabulate controls and additional DiD parameters described in eq. (2), but these are available upon request. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Table A8 – DiD Analysis with Placebo Treatments and Placebo Windows

Panel A - Placebo Treatments (Dependent Variable: Annualized ROR [Eq. (2)])				
	Control 2 Sample	Control 2 Sample	Control 2 Sample	Control 2 Sample
	(1)	(2)	(3)	(4)
Post x Placebo1	0.0006			
	(0.0065)			
Post x Placebo2		-0.0019		
		(0.0108)		
Post x Placebo3			0.0033	
			(0.0049)	
Post x Placebo4				-0.0028
				(0.0063)
Adj. R-Squared	0.4097	0.4029	0.4107	0.4025
N	6,600	3,800	6,600	3,800
Property Controls	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes
Purchase years in sample	2005-10	2005-10	2005-10	2005-10

Panel B - Placebo Windows (Dependent Variable: Annualized ROR [Eq. (2)])				
	(1)	(2)	(3)	(4)
Control Sample:	Control 1	Control 2	Control 1	Control 2
Placebo Post*Eligible	-0.0002	-0.0013	-0.0003	-0.0028
	(0.005)	(0.0034)	(0.0048)	(0.0034)
Adj. R-Squared	0.4023	0.4039	0.4168	0.417
N	37,000	41,000	31,000	34,500
Controls	Yes	Yes	Yes	Yes
Survey Year FE	Yes	Yes	Yes	Yes
Years Owned FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Purchase Qtr*Yr FE	Yes	Yes	Yes	Yes
Excludes Purchases During FHTC Period	No	No	Yes	Yes
Time Period	1/2005-6/2010	1/2005-6/2010	1/2005-6/2010	1/2005-6/2010

Notes: Table A8 report estimates of the difference-in-differences regression described by eq. (2), but instead using placebo treatments (where income thresholds and sample restrictions are defined in the text for each placebo treatment) and a placebo post window (as described in the text). Standard errors are clustered by county and reported in parentheses. Coefficients and N (obs) are rounded according to Census disclosure guidelines. We define the variables in Table 1. The symbols ***, **, and * denote statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively.

Figure A1 – Changes to Average Monthly Rent by Year
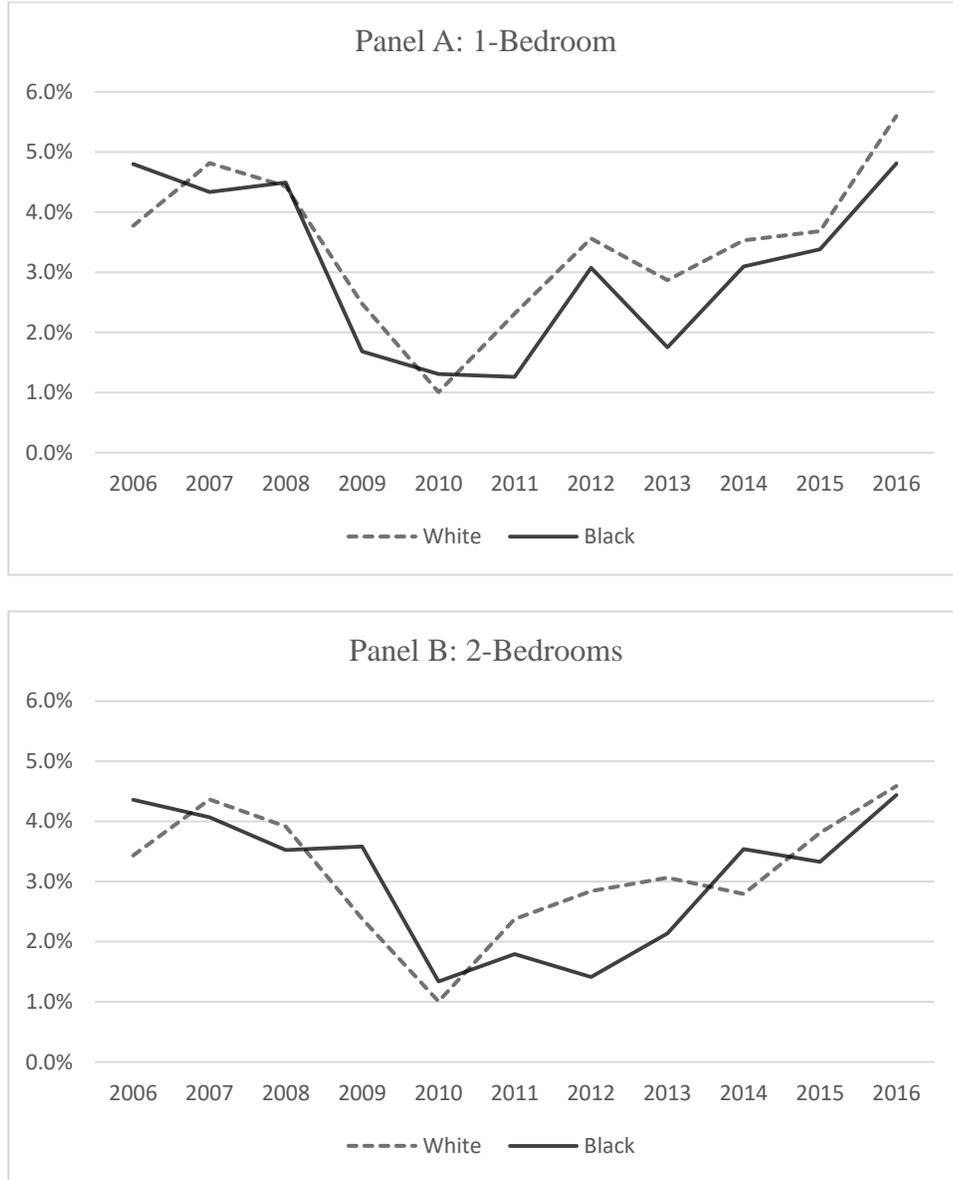
Notes: The top panel plots the year-over-year change in average monthly rent for ACS households, split out for White and Black households who reside in a 1-bedroom property. The bottom panel plots the same for 2-bedroom properties. *Source*: ACS Data via IPUMS (Census Bureau). Steven Ruggles, Sarah Flood, Matthew Sobek, Daniel Backman, Annie Chen, Grace Cooper, Stephanie Richards, Renae Rogers, and Megan Schouweiler. IPUMS USA: Version 14.0 [dataset]. Minneapolis, MN: IPUMS, 2023. https://doi.org/10.18128/D010.V14.0.