Household Mortgage Refinancing Decisions Are Neighbor Influenced, Especially Along Racial Lines*

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Abstract

Neighborhood peer effects have been shown to affect households' publicly observable decisions, but how they affect private decisions, like mortgage refinancing, remains unclear. Using precisely geolocated data and a nearest-neighbor research design, we find that households are 7% more likely to refinance if a neighbor within 50 meters has recently refinanced. Consistent with a word-of-mouth mechanism, social influence effects are stronger when neighbors live especially nearby, weaker when owners are non-occupants, and stronger when neighbors and owners are of the same race. Our results suggest an important role for neighborhood peer effects in explaining regional variation in refinancing activity.

JEL Classification: D12, D14, D71, H31, R23

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1 Introduction

Mortgage refinancing is important for households' wealth accumulation and for the transmission of monetary policy to household consumption (Amromin et al., 2020; Di Maggio et al., 2020; Keys et al., 2016; Wong, 2019). Recent empirical evidence has documented significant regional heterogeneity in refinancing activity that leaves some local economies better off than others when rates go down (Beraja et al., 2019). Prior work investigating spatial clustering in economic decisions has focused predominantly on choices that are conspicuous and publicly observable.[1] These settings make it difficult to disentangle the role of visual salience from other channels like word-of-mouth exchanges. This distinction is crucial for decisions such as refinancing, which are relatively private, inconspicuous choices. In this paper, we investigate the importance of word-of-mouth social interactions for explaining regional heterogeneity in refinancing.

This paper's contributions are twofold. First, we identify the importance of hyperlocal social networks in driving regional refinancing activity and document how local these effects can be. Using a nearest-neighbor research design to precisely geolocate millions of refinancing decisions, we investigate the impact of households' immediate neighbors (those who live within 50 meters) while including a local geography fixed effect and controlling for the refinancing decisions of neighbors slightly farther away (those within 100 meters or 250 meters). We show that the average owner-occupied household in Los Angeles County is 7% more likely to refinance if an immediate neighbor refinanced in the previous quarter. We also observe significant hyperlocal social influence effects over the whole time period, 2003 to 2012, and across the house price distribution suggesting that these effects are economically pervasive.

Second, we provide compelling evidence that neighbor social influence effects operate via word-of-mouth social interactions. Since neighbors' refinances are not publicly observable, the existence of hyperlocal spatial clustering in refinance activity is most consistent with a word-of-mouth transmission channel. To provide further evidence in support of this channel, we use three settings where we can plausibly vary the intensity of word-of-mouth social interactions: geographic proximity between neighbors, whether households are owner occupied or not, and racial similarity between neighbors.

[1] For example, extant work documents neighbor effects in car buying (Grinblatt et al., 2008; McShane et al., 2012), the purchase of investment properties (Bayer et al., 2021), exterior home improvements (Bollinger and Gillingham, 2012; Newman and Staelin, 1972), and foreclosure (Anenberg and Kung, 2014; Campbell et al., 2011; Gupta, 2019; Kalda, 2019; Towe and Lawley, 2013). A notable exception is Bayer et al. (2008) who find evidence that neighbors share job referrals.

First, we test whether the geographic proximity between a household and its neighbors influences the strength of our estimates. If hyperlocal clustering in refinancing operates primarily through word-of-mouth social interactions, then we would expect to find that the strength of hyperlocal influence is strongest among immediate neighbors and decays as the geographical distance between them increases. We find that a household's nearest neighbors, i.e., those who live less than 50 meters away, exert a social influence effect nearly twice as strong as that from neighbors between 50 and 100 meters away and more than three times as strong as that from neighbors who are between 100 and 250 meters away. These results illustrate not only the importance of a household's immediate neighbors on the decision to refinance, but also quantify the extent to which hyperlocal influence effects spatially decay.

In our second test, we focus on non-occupant owners and show that hyperlocal spillovers only occur for owner occupied households' refinance decisions. Intuitively, this test provides a natural falsification group allowing for a sharp contrast between two potential mechanisms. If correlations in refinancing activity between households and their neighbors were spurious and there was no causal peer effect – for example, if sorting and correlated shocks were behind the hyperlocal clustering in refinance activity – then we might expect to see a positive correlation for both non-owner-occupied and owner-occupied households. Alternatively, if the clustering was the result of social interactions, then we would expect the refinance decisions of non-occupant owners to be uncorrelated with the immediate neighbors, since non-occupants are less likely to interact with surrounding households. We find that households are only socially influenced by their most proximate neighbors if they actually occupy the home. For households that own multiple properties, we find that the refinance decisions on their second and third properties are uncorrelated with their immediate neighbors' refinance decisions. However, these same households' *are* socially influenced by the neighbors at their primary residences–presumably where hyperlocal social interactions are more likely to occur.

Our third test is motivated by the accumulating body of evidence that shows social interactions are more common between people of the same race and ethnicity (Ananat et al., 2018; Billings et al., 2019; Fletcher et al., 2020; Hellerstein et al., 2008; Ioannides and Datcher Loury, 2004; McPherson et al., 2001). We begin with simple sample splits and show that *on average* households are similarly affected by their neighbors regardless of their own race or their neighborhood's racial composition. However, these average effects mask two important relationships. Households are more than twice as influenced by neighbors of the same race than by neighbors of a different race. Furthermore, the strength of this relationship depends on the neighborhood's racial composition. While same-race neighbors matter everywhere, the social influence of neighbors of a different race is statistically and economically significant only in the most racially diverse neighborhoods and decays to zero in the most racially segregated neighborhoods.

In the final section of the paper, we investigate how the social influence effect varies over one of the fundamental reasons households might refinance, lower interest payments. We split the sample into two sets of households: those whose current interest rate is one percentage point, or 100 basis points (BPS), higher than the prevailing rate – who would potentially save a great deal in monthly payments by refinancing – and those whose interest rate is either the same or lower than the prevailing rate. We document that the effects of neighbor social influence are approximately twice as strong on households who stand to gain the most by refinancing. This finding suggests that complementarities may exist between the effects of social influence and what a household perhaps ought to do.

Our work contributes to several research agendas. First, we contribute to the large body of work that investigates the drivers of household mortgage refinancing. One of the biggest puzzles in this space is why so many households opt not to refinance, despite it being in their financial best interest to do so (see Agarwal et al. (2016); Andersen et al. (2020); Johnson et al. (2019); Keys et al. (2016); and Gomes et al. (2020) for a review of the literature). In some cases, the failure to refinance can be attributed to low creditworthiness, negative home equity, or liquidity concerns (Archer et al., 1996; Campbell, 2006; Caplin et al., 1993). Households planning to move may also sensibly choose not to refinance (Yang and Maris, 1993). Yet, many households still fail to refinance even in settings where refinancing to lower the interest rate is financially costless (Agarwal et al., 2013; Andersen et al., 2020). This suggests behavioral reasons for under-refinancing such as procrastination, inattention, inertia, and even suspicion of lenders' motives (Johnson et al., 2019; Keys et al., 2016). Other evidence finds that borrower sophistication (Agarwal et al., 2013) and financial literacy (Bialowolski et al., 2020; Campbell, 2006) significantly influence the likelihood of refinancing. Each of these potential reasons for under-refinancing suggest particular, and sometimes different, policy solutions. Our novel finding that word-of-mouth interactions between neighbors increases refinancing activity suggests that policymakers should leverage hyperlocal targeting interventions, especially in areas that historically under-refinance. Doing so could both immediately benefit households and trigger beneficial spillover effects. Moreover, broader interventions that promote community building, such as via neighborhood associations (Portney and Berry, 1997) or community churches (McRoberts, 2005), and encourage more frequent social interactions between neighbors are also likely to have meaningful welfare effects that could benefit households and neighborhoods alike.

Second, our results on the importance of racial ties contribute an important new piece of evidence to the literature investigating racial differences in mortgage choices, especially refinancing and prepayment, but also mortgage performance and pricing (see, for example, Berkovec et al. (1996, 1998); Bhutta and Hizmo (2021); Campbell (2006); Cheng et al. (2015); Gerardi et al. (2020, 2021)). Specifically, we show that minority households are less likely to refinance and, perhaps as a consequence, have fewer neighbors who have recently refinanced. This suggests a negative feedback loop – limited minority refinances beget even fewer minority refinances since the recently-refinancing-neighbor influence channel is shut down – and can potentially help explain why financial mistakes are particularly severe among minority households (Gerardi et al., 2020).

Third, we build on closely related papers that identify other types of peer effects in household mortgage decisions. Maturana and Nickerson (2019) and Bailey et al. (2018) show that households' co-workers and Facebook friends, respectively, are important sources of social influence. We investigate a new part of the household's social network and find that social interactions between neighbors are also influential in household mortgage decisions. Given the interest in understanding the drivers of regional variation in refinancing activity and its importance for local housing markets and household consumption (Beraja et al., 2019; Glaeser et al., 2014; Wong, 2019), identifying and quantifying the influence of regional peers (neighbors) is of first order importance.

Our results are also able to push against the largely negative effects of neighbor influence that much of the literature suggests, especially when documenting "Keeping up with the Jones" effects (Agarwal et al., 2020; Mitton et al., 2018). Refinancing typically *benefits* households, especially in our time period. Though neighbors may encourage household spending, this paper's results show they may also pass along information or encourage behavior that promotes substantial household savings.

It is worth noting that the importance of household neighbors is likely growing. The popularization of neighborhood-based apps such as Nextdoor and the shift to working from home, two trends amplified by the recent COVID-19 pandemic, mean that households and their neighbors will be increasingly connected (BLS, 2017; Brynjolfsson et al., 2020). Understanding the economic effects of neighbor influence, both positive and negative, will therefore be increasingly important going forward.

2 Data Description

We create a panel data set that follows households in Los Angeles County from 2008 to 2012. In this section, we describe the data sources, define neighbor activity, and summarize our final sample.

2.1 Data Sources

Our primary data set uses data from two public sources: deeds registries and tax assessors' offices. These data are cleaned and standardized by the real estate data company, DataQuick Information Services (now CoreLogic Solutions). The deeds registries detail mortgage loans, recording the names of the borrowers (and, in the case of purchase loans, the sellers), the date of the origination, the purpose of the loan (purchase or refinance), the loan amount, whether the interest rate on the loan is fixed or adjustable, and the name of the lender. Local tax assessors' offices record ownership and property characteristics, e.g., square feet, year built, appraised value, and, crucially, the exact latitude and longitude, of every property in Los Angeles County.

We focus on one county to ensure that recorded transactions use consistent coding rules. We choose Los Angeles for three reasons. First, Los Angeles has a long panel of reliable, non-missing data, giving us a complete picture of every households' mortgage decisions between 1992 and 2012. Second, with over 10 million inhabitants, the population of Los Angeles County alone exceeds that of 41 individual states. And third, large not only in terms of population, Los Angeles also has the world's third-largest metropolitan economy and a nominal GDP of $700 billion, making it an important economic center.

We supplement the DataQuick data with Home Mortgage Disclosure Act (HMDA) data. HMDA is a mortgage level database listing all mortgage applications, both approved and denied, made to qualifying lending institutions. We successfully merge HMDA data into our main data set using loan purpose (purchase or refinance), loan amount, presence of a co-signer, census tract, lender name, and year of application for 37% of our sample.[2] For this subsample, we observe the homeowner's race, sex, and income since HMDA includes these variables.[3] We supplement the race data from HMDA with lists of the most common surnames for people who identify as Asian, Black, Hispanic, and White.[4]

Finally, we supplement these datasets with survey data on prevailing interest rates from the Monthly Interest Rate Survey (MIRS) conducted by the Federal Housing Finance Agency (FHFA). We use the yearly file for Los Angeles to provide an estimate for the interest rate likely being paid by the mortgage borrowers in our sample and the rate they could likely get if they were to refinance.

2.2 Variable Definitions and Sample Construction

To create our final panel data set, we clean the raw data in several steps. First, we ensure that we have a sample of households, as opposed to institutions or professional investors. We use the name cleaning algorithm developed by Bayer et al. (2021) to tag borrowers as either individuals or institutions. This algorithm uses the names of the borrowers and a rich set of keywords to determine if the borrowers are trusts, banks, businesses, government and nonprofit organizations, or individuals. The algorithm further cleans and standardizes borrower names.

We limit our sample to only those properties owned by individuals – as opposed to institutions – and further drop any homeowners who ever concurrently own four or more properties (for more on this interesting group of buyers see Bayer et al. (2021)). In addition, we drop observations in which the property securing the loan involves a condominium or a property divided into smaller properties and resold. We also drop transactions that were flagged as not at arms-length, where the house sold more than once in a single day, and if the sale price for the home was less than $1. We drop observations that are missing key information including the location of the property, the name of the buyer, the lender (allowing us to control for lender-level fixed effects), the amount of the loan, square footage, or an assessed value. Informal interviews with real estate agents suggested that our identifying assumption might be violated in the case of very expensive homes and new construction. Thus, we we omit homes assessed at more than $2,000,000 as well as new construction. Next, we

[2] HMDA uses a specific lender identification number to mark distinct lending institutions. This identification number is matched to the lender's name in the HMDA lender file compiled by Dr. Robert Avery.

[3] Since our deeds records include only approved loans, none of the denied loans from HMDA will match to our main data set.

[4] See, for example, https://namecensus.com/data/hispanic.html, accessed June 7, 2021.

drop loans originated by lenders that made fewer than 1,000 loans over the entire time series so that we can include a meaningful lender-level fixed effect. Finally, we restrict the sample to properties in census blocks that have between five and 100 owner-occupied properties. Neighborhoods with very few or very many households likely vary too substantially for our identifying assumption to be valid.

Second, we create a novel algorithm that assigns to each homeowner the property that it occupies. This algorithm takes as its key inputs the mailing address used at the time the loan was originated, site address of the property securing the loan, and purpose of the loan. We follow each homeowner by property over time and say that the house is not occupied if its mortgage is refinanced with different site and mailing addresses and is occupied if they are the same. Then, if the homeowners only ever own one home, we say that the home is owner occupied. Next, if we see they occupy one property, then we say they do not occupy any others. Finally, we assume that the property they purchased first is occupied, and any others purchased later are not. This algorithm is important as our analysis hinges on social interactions. We use owner-occupant status to identify primary residences, where owners are more likely to be interacting with their neighbors.

Third, we group all refinancing activity together (e.g., refinances that replace existing loans, cash-out refinances, home equity loans, and home equity lines of credit). We follow households over time and observe, each quarter, if they originated a new loan of one of these types. If they did, we say that they refinanced.

Fourth, we use the latitude and the longitude of the property to precisely map the universe of owner-occupied homes in Los Angeles County, outstanding mortgage loans, and new mortgage transactions. For each household, we tag the neighbors within 50 meters, 100 meters, and 250 meters. To measure peer refinance activity, we count the number of neighbors within each distance and in the same census block group that refinanced in the previous quarter. Third, we estimate the current loan-to-value (LTV) ratio for each household each quarter. We assume that borrowers repay following a standard 30-year repayment schedule. To calculate each home's current value, we adjust its 2011 appraised value at the same rate as the median house price in its ZIP code (from Zillow). The quotient gives the estimated current LTV.

To investigate whether hyperlocal social influence effects vary across racial lines, we use a two-step methodology. First, to assign each household a race, we use the race variable from HMDA for the set of mortgages in the DataQuick dataset that we uniquely match to HMDA. If the household's ethnicity is coded as Hispanic in the HMDA data, then we say the household is Hispanic, regardless of race. In this way, white and Black households are non-Hispanic white and non-Hispanic Black, respectively. Since we are only able to confidently match 37% of the DataQuick sample to HMDA, we use households' last names in our second step. Specifically, if a last name is used by people identifying as Asian, Black, Hispanic, or White more than 80% of the time, we say that households with that last name are of that race. If the household never has a loan that matches to HMDA and also has a name either commonly affiliated with more than one race or never commonly affiliated with any race, we leave the race variable empty. Using this two-step methodology, we are able to assign a race/ethnicity to 73% of the sample.

Finally, we focus on the time period 2008–2012 for our main tests, choosing this time period for two reasons. First, mortgages originated before 1992 are not in our raw data. Using a more recent part of the sample ensures that we know the outstanding mortgage characteristics of all households in the neighborhood. Second, 2008–2012 was marked by both depressed economic activity and very low interest rates, which make it similar in important respects to today's environment and our results relevant for today's policymakers. Though we focus on 2008–2012, we do expand our sample to include the time period between 2003–2012 in Section 4.2.3 and show that our results are similar over the whole time period.

2.3 Summary Statistics

[TABLE 1 HERE]

The final sample contains more than 17 million household-by-quarter observations and is described in detail in Table 1. The average probability of a household refinancing in a given quarter is 2.4%. The average household has 11 neighbors living within 50 meters, 0.19 of whom refinanced in the previous quarter.

3 The Identification Strategy and Tests of Assumption Validity

We use our rich dataset, described above, combined with a spatial empirical design to identify hyperlocal neighborhood social influence effects. Below we detail the strategy and then present evidence consistent with the validity of the test's identifying assumption – that households' very nearest neighbors are quasi-randomly assigned.

3.1 The Identification Strategy

Two well-known endogeneity issues confound the identification of neighborhood peer effects: endogenous group formation and correlated unobservables (Brock and Durlauf, 2001; Manski, 1993, 2000; Moffitt, 2001). First, households choose where to live partly based on similarities with those they will be living near. Since households in similar stages of life and with similar incomes will make similar refinancing decisions, there will likely be correlations between the decisions made by households and the decisions made by their neighbors. Second, households living nearby are likely to use the same financial institutions and face the same market conditions. Shared exposure to often unobservable local shocks may drive households to make the same decisions. Finally, if households are not only influenced by their neighbors but also influence them, then neighborhood peer effects may be difficult to detect.

To overcome these challenges, we use a nearest-neighbor research design that models households' refinancing decisions as a function of their very nearest neighbors' decisions while controlling for the decisions made by neighbors who live slightly farther away. In our preferred specification, the nearest neighbors are those households living within 50 meters, and the neighborhood is the census block group. Defined this way, our sample's average household has 11 nearest neighbors and lives in one of Los Angeles's 6,213 neighborhoods (each inhabited by an average of 246 households). The strategy assumes that *within* a neighborhood, a household's *nearest* neighbors are randomly assigned. If a household's nearest neighbors are conditionally random, then endogenous group formation and correlated unobservables no longer bias estimates of the social influence effect from these nearest neighbors.

We can therefore identify social influence effects using the following linear probability model:

$$Refi_{it} = \alpha + \beta_1 \times Nbr\ Refis_{i,t-1} + \delta \times X_i + \kappa_{lt} + \phi_{gt} + \epsilon_{it}, \tag{1}$$

where $Refi_{it}$ is binary variable equal to 100 if household i refinanced in quarter t. Our parameter of interest, β_1, estimates the effect of $Nbr\ Refis_{i,t-1}$, the count of hyperlocal neighbors who refinanced in the last quarter, on the household's likelihood of refinancing this quarter. X_i is a vector of variables that control for characteristics of the borrower, their property, and their outstanding mortgage. Specifically, we include controls for whether the outstanding loan is an ARM or FRM, whether it is a refinance or purchase loan, an estimate of its current LTV, how many quarters have passed since its origination, and whether there was a co-signer or not. We further control for the home's most recent assessed value, size, and age, all of which might affect the refinance decision. A large literature in household finance documents many factors that affect the refinance decision and we control directly for as many of these as we can (Agarwal et al., 2013; Andersen et al., 2020; Campbell, 2006; Keys et al., 2016). Some of these factors, like likelihood of moving away, are unobservable to us. But recall that our empirical design assumes that households do not look especially like their nearest neighbors with respect to these unobserved factors since the nearest neighbors are randomly assigned.

Perhaps the most fundamental factor that influences the household's refinance decision is how much rates have fallen since the current mortgage was originated. Note that since we include both a quarters-since-last-mortgage fixed effect and a current quarter fixed effect, we cannot estimate in this model the effect of potential interest rate savings. That is, the difference between the mortgage rate on the household's current mortgage and the prevailing mortgage rate is perfectly collinear to these two important fixed effects. We adjust our strategy to investigate how our results vary over this important factor in Section 6.

Another potential confounding factor is that households living near to each other might use the same lender and, for example, a lender might introduce a promotion that encourages refinancing. We would then mistake the effect of this promotion for a social influence effect. We therefore include a lender-by-quarter fixed effect, denoted κ_{lt}, that controls for variation in the likelihood of lenders encouraging their borrowers to refinance at particular points of time. Finally, we include a census block group-by-quarter fixed effect, ϕ_{gt}, to absorb the effects of sorting and common shocks. Since hyperlocal neighbors are *included* in the block group, β_1 picks up the *outsized* effect of hyperlocal neighbors.

Importantly, the effects that we identify using this strategy by construction underestimate the true magnitude of the neighborhood social influence effect. That is, our research design identifies the effect of immediate neighbors by controlling for the decisions of those slightly farther away. If these slightly more distant neighbors *also* influence refinancing decisions, then our estimates, which do not capture this effect, will, by construction, understate the full effect of neighbors' social influences.

Second, our estimate is also mechanically biased toward zero since we implicitly assume all households are "treated" with neighbor interactions. If a household only interacts with some, but not all, of their nearest neighbors, then what we estimate is really the treatment-on-the-treated effect, which will be less than the true social influence effect. This is especially important in private decisions because, unlike in public decisions in which everybody is likely to observe the neighbor's new car or foreclosure, the average household probably does not talk with every one of their neighbors.

Finally, at a broader level, our work focuses exclusively on isolating and quantifying the causal effects of social interactions from one part of the household's social network: their neighborhood peers. By isolating the effect of one peer group, and ignoring other peer groups like friends (Bailey et al., 2018) and co-workers (Maturana and Nickerson, 2019), our estimate understates the importance of social influence, more broadly, for households' refinance decisions.

3.2 Assumption Validity

The identifying assumption is that while households may choose to live in particular neighborhoods, they are less likely to choose, or be able to choose, specific parcels. We validate the assumption underlying our empirical strategy in four main ways.

First, we lean on the growing number of studies in urban and spatial economics that use a strategy similar to ours (Anenberg and Kung, 2014; Bayer et al., 2021, 2008; Campbell et al., 2011; Currie et al., 2015; Linden and Rockoff, 2008; Towe and Lawley, 2013). These papers conduct their own tests of the assumption's validity and show, overwhelmingly, that within small enough neighborhoods, the very nearest neighbors appear conditionally randomly assigned.

Second, we informally interviewed 41 real estate agents operating in Los Angeles. These interviews confirmed that home shoppers rarely search for specific parcels or even blocks. Specifically, 37 (90.2%) of the real estate agents reported that clients choose particular neighborhoods in which they want to live, and then try to buy a house somewhere in that neighborhood depending on what is available. The other four real estate agents, who notably focused more on high-end real estate, point out some exceptions. The few clients who focus on a particular block typically do so because they are looking for prestigious addresses or are interested in new development. In response, we exclude highly priced homes and new construction from our sample.

[TABLE 2 HERE]

Third, we design a balance test to measure the commonality between the nearest neighbors and those slightly farther away. For every block in the sample, we compare the block's residents with the residents of adjacent blocks along eight dimensions: the interest rate difference between outstanding loan and prevailing rate, a dummy for a down payment less than 20%, quarters lived there, assessed value, square feet, cosigners, applicant income, and race (defined using only HMDA). Block by block and variable by variable, we compare each block to its adjacent blocks with a t-test on the equality of means. We then store the p-values of each test and present them in Table 2. If a block and its adjacent blocks did not have significantly different averages of a given variable, we would see a large p-value and reject that the block was significantly different than those blocks adjacent to it along that dimension. Since we conduct so many tests, some p-values will suggest statistically significant differences, but this will just by chance. That is, even assuming that neighborhoods were homogeneous along a dimension, approximately 5% of blocks will have p-values less than 0.05.

In the top half of Table 2, we show that the distribution of p-values looks approximately as if drawn from a population of blocks that are not statistically different from their adjacent blocks. Two notable exceptions are home value and house size, since these characteristics often cluster at the block level. To take the problem of hyperlocal sorting very seriously, we drop from the sample all blocks where more than one of the variables of comparison is significantly different at the 5% level or more than two variables are significantly different at the 10% level. This procedure drops just over 30% of the sample. We confirm in Section 4.2.2 that our main results are robust to dropping these blocks.

Finally, we perform four more secondary tests and document results that cannot be explained solely with a hyperlocal sorting story. We show that (i) households' current LTVs and the difference between their outstanding mortgage rate and the current prevailing rate do *not* look especially like those of their nearest neighbors, (ii) that the effects are stronger the closer the neighbor lives, even within very short distances, (iii) that non-occupants (who are less likely to socially interact with their neighbors) show no signs of being socially influenced, and (iv) that neighbors of the same race exert much stronger social interaction effects than neighbors of different races.

4 Evidence of Hyperlocal Neighbor Peer Effects

In this section, we discuss our main results and a number of robustness tests.

4.1 Main Result

[TABLE 3 HERE]

Table 3 presents our estimations of the linear probability model described by Equation 1. Estimating this model without block group-by-quarter fixed effects, we find that each additional neighbor within 50 meters who refinanced in the previous quarter makes the average household 0.432 percentage points or 18% more likely to refinance this quarter. However, the omission of the geography-by-time fixed effect means that our social influence effect estimate is biased by the effects of both sorting on unobservables and unobserved correlated shocks. When we include this important fixed effect, and assume that the household's nearest neighbors are conditionally random, we control for the effects of endogenous sorting, in which households choose to live near households with similar characteristics, and correlated shocks, where households living near to each other might be similarly influenced to refinance by some external factor. This specification's estimation produces our headline results: Each additional nearby refinance makes households 0.170 percentage points or 7% more likely to refinance.

In the third specification, we further control for the household's income, race (from HMDA), and ethnicity (from HMDA). This limits the sample to just those mortgages in the deeds data matched to the HMDA data, but our main result is robust to the inclusion of these demographic controls. The sample used to estimate our preferred specification includes an average of 808,342 outstanding mortgages, 19,723 of which are refinanced each quarter. And the average household had 0.19 neighbors within 50 meters refinance in the previous quarter. Compared to the counterfactual in which there is no hyperlocal social influence effect, hyperlocal neighbors can be said to cause an additional 808,342 $\times$ 0.0017 $\times$ 0.19 = 260 refinances each quarter. In other words, we estimate that nearly 1.5% of mortgage refinancing in any given quarter is due to social influence effects from hyperlocal neighbors.

If we (i) assume that effect sizes are similar across the United States, and (ii) use the finding in Keys et al. (2016) that the average U.S. household could have saved an average of $11,500 in

December 2010 had they refinanced, then we estimate that peer effects might save U.S. households up to $175 million every quarter.[5] As in Keys et al. (2016) a third assumption we make in the previous exercise is that without the social influence effect, households would *never* have refinanced. However, this may be an extreme assumption.

It is perhaps more plausible that the social influence effect may be simply accelerating when households refinance. In this case, considering just monthly savings in mortgage payments may be more appropriate. The average household in our sample has a primary mortgage with an outstanding balance of $180,600 and is paying an interest rate of 6.18%. On average, households in our sample could lower their rate by 1.13 percentage points by refinancing, lowering their monthly payment from $1,172 a month to $1,054 a month. Therefore, even if the social influence effect merely speeds up by one year the time until refinance, neighbor peer effects can still be said to save the average household in our sample nearly $1,500.

4.2 Robustness of the Main Result

In this section, we conduct several checks to ensure that our main result is robust to alternative geographic specifications and holds during all time periods, including the periods prior to, during, and following the Great Recession. These tests allow us to rule out several competing drivers for our neighborhood peer influence effect.

4.2.1 Alternative Ways to Control for Slightly Farther Away Neighbors' Decisions

[TABLE 4 HERE]

In Table 4, we vary our preferred specification in several ways that illustrate the validity of the identifying assumption and demonstrate the robustness of our main result. In the first specification, we drop the block group-by-quarter fixed effect and instead control directly for the number of peers within 100 meters who refinanced last quarter. This strategy achieves the same central objective as our identification strategy: to control for activity at the neighborhood level and then test if, even

[5] In the fourth quarter of 2010, we observe that there were 48,000,000 mortgages outstanding. (See https://www.corelogic.com/downloadable-docs/corelogic-q4-2010-negative-equity-report.pdf, accessed August 13, 2020.) If these households were also 0.0017×0.19 percentage points more likely to refinance due to hyperlocal peer effects, then that translates to 15,504 additional mortgages being refinanced at lower rates, saving these households $11,500 each and U.S. households $178,209,000 in total each quarter.

within that geography, households behave especially like their closest neighbors, those with whom they are most likely to interact.

An advantage of this specification is that we can zoom in to very fine geographies. That is, we now assume that of the 26 neighbors that a household lives near, the 11 who live *nearest* are randomly assigned. We find that the average household is 0.08 percentage points or 3.3% more likely to refinance if a very nearby neighbor has recently refinanced, controlling for the refinancing activity of those living in a slightly larger area. Specification (4) mimics specification (1) but defines the larger neighborhood with a 250-meter radius instead of a 100-meter one. Ultimately, we prefer specifications that include a geography by time fixed effect as in Table 3 since this absorbs all commonalities between those living in the same area, not just those that actually manifest in different refinancing decisions.

With the concentric circles neighbors in hand, we can combine the two strategies. Specifically, specification (2) adds a tract-by-quarter fixed effect and specification (3) a block group-by-quarter fixed effect. There are two findings of note. First, the "effect" of neighbors with 100 meters diminishes significantly as the geography fixed effects are included. This is entirely expected since the larger concentric circle and the geography fixed effect serve the same purpose in the model – to absorb reasons to refinance shared by people in the same neighborhood, either because of common characteristics of shared exposure to common shocks. Second, the outsized effect of the very nearest peers does *not* change, consistent with households being socially influenced by their especially proximate community.

In the Online Appendix, we explore the concentric circles approach in more detail. In the first specification of Appendix Table A1, we find that the effect of neighbors within 100 meters becomes especially small when the 250-meter disc is also included. However, the effect of the very closest neighbors does not change. This suggests two things. First, the very closest neighbors always matter, consistent with a social influence effect from these neighbors that matters over and above sorting and correlated shocks. Second, the fact that neighbors within 100 meters do not matter much once those within 250 meters are included suggests that endogenous sorting and correlated effects are happening at a geography of at least 250 meters. In other words, we conclude that the large, positive coefficient on neighbors within 50 meters is due to social influence and that the large, positive coefficient on neighbors within 250 meters is due to endogenous sorting and correlated shocks. The relatively small coefficient on neighbors within 100 meters suggests that social influence effects occur at even more local geographies while sorting and common shocks are relevant at broader geographies.

An important limitation of our design is one that biases our results toward zero. Households may socially interact with and be influenced by neighbors living in their block group but not within 50 meters (or within 100 meters but not within 50). Our strategy, however, combines the effect of these social interactions with the effects of endogenous group formation and correlated shocks. For this reason, our estimated effects are lower bounds of the true social influence effect of neighbors, since some of these effects are absorbed by the geography fixed effects or larger disc control variables. Appendix Table A2 reaches a similar conclusion. As finer and finer geography fixed effects are included, the estimated coefficient diminishes in magnitude. But even when block-by-quarter fixed effects are included, the estimated coefficient of the social influence effect remains economically and statistically significant. However, as just argued, this estimate is likely biased toward zero since households may socially interact with neighbors outside their closest 11.

4.2.2 Homogeneous Neighborhoods and Assumption Validity

In this section we re-visit the main threat to the validity of our empirical test. As discussed in detail in Section 3.2, our central assumption is that households do not sort to hyperlocal geographies conditional on slightly larger geographies. Our first test of this assumption repeats the same strategy described by Equation 1 but replaces the outcome variable with two important determinants of the refinance or not decision – current LTV and the difference in the loan's interest rate and the currently prevailing interest rate.

[TABLE 5 HERE]

Column (2) shows no significant similarity between a household's CLTV and the CLTV of its closest neighbors. In Column (1), the similarity is significant, but *negatively* so. This means that the average household's CLTV moves in the opposite direction from the nearest neighbors' CLTVs. Importantly, the difference is economically trivial. From column (1), we see that if the nearest neighbors average CLTV went up by 10.0 percentage points from the average, from 76% to 86%, then the household's CLTV would decrease by 0.07 percentage points. Such a small elasticity suggests that clustering by CLTV is unlikely to explain the economically large effect of nearby refinances on household refinances. The reasoning is similar in columns (3) and (4) when we investigate the interest rate difference between the household's current mortgage and the prevailing rate. From column (4), if the average rate difference of nearby neighbors was 0.50 percentage points higher (making refinancing, all else equal, more profitable), then we would predict the household's rate difference to be .002 percentage points or 0.2 basis points lower. Taken together, the results in columns (1) through (4) suggest that hyperlocal clustering in two determinants of refinancing is not occurring at levels high enough to explain our main result.

[TABLE 6 HERE]

Our second test follows from the commonality test we conducted in Table 2. To help rule out any possibility that hyperlocal clustering is driving our main results, we limit the sample to just neighborhoods in Los Angeles in which our assumption is especially likely to hold. Specifically, we limit our sample to the 70% of Los Angeles census blocks that are statistically indistinguishable from their adjacent census blocks as described in Section 3.2. We re-estimate our model on this homogeneous neighborhoods subsample in the first three columns of Table 6. We find that our main results are somewhat smaller, but still economically and statistically significant. For completeness we also estimate the model on just the sample of non-homogeneous neighborhoods. We confirm, as expected, that the estimates are larger in these neighborhoods, consistent with the estimates in this subsample being biased upwards by the effects of hyperlocal sorting. This difference between the estimates from the two samples also reassures us that our methodology for tagging non-homogeneous neighbors works as expected, and that we can therefore have confidence in our results from columns (1) through (3).

4.2.3 Variation in Effects over Time Periods and House Prices

We investigate whether the neighbor effect varies over time or whether it is limited to just the time period covered by our main sample (2008 – 2012). In addition, we test whether hyperlocal neighbor social influence effects are driven by households of a particular house price or whether they occur more broadly across the house price distribution.

[TABLE 7 HERE]

Our main analysis focuses on the time period between 2008 – 2012 for reasons discussed at the end of Section 2.2. However, limiting our results to this time period does bring up a concern about the external validity of our results. The periods prior to and during the recession were marked with unusual economic activity, particularly with regard to home mortgages. For example, large falls in house prices meant that many households ended up underwater on their mortgages, making them unable to refinance. In Table 7, we examine how our effects change over the boom and bust cycle by extending the sample back to 2003 and estimating our main regression year by year. We observe that a household's proximate peers are a significant source of influence in all years, including in the years prior to the recession, during the recession, and following the recession.

Next, to more deeply examine potential differences due to wealth effects, we categorize census tracts into one of five house price quintiles based on their average assessed house price. Appendix Table A3 shows that the effect of neighbors is similar across the house price distribution. This tells us that the importance of neighbors is not limited to just, for example, richer neighborhoods. Indeed, neighbors are similarly important across the whole house price distribution. The steadiness of our findings across time and space further suggests that factors which varied widely over the time period and/or county, like advertisements for low rates or household internet access, are unlikely drivers of our main results.

4.2.4 Variation over Current Loan-to-Value Ratios

In our final robustness subsection we re-visit current loan-to-value ratios and consider more explicitly their implications for this papers' conclusions. An important feature of our setting is that Los Angeles saw particularly severe house price declines in the late 2000s. One implication of this is that cash-out refinances were very rare during the years included in our main sample, assuaging concerns that the refinancing choices neighbors were making were publicly observable to their neighbors. A second implication is that many of the households in our sample were likely to have been pushed underwater and were therefore less able to refinance at all. Our model estimates will therefore have to be treated with caution when taken to settings with fewer underwater households.

To see why, look to Table A4, where we split our main sample into those households that have a significant equity cushion; those that are highly-leveraged, but not underwater; and those that are underwater. We find that social influence effects are strongest on those with high current LTVs who are nevertheless still able to refinance without putting extra money down. While underwater households appear to be significantly less socially influenced, an alternative interpretation is that these households are not even eligible to be affected by the treatment since they are locked out of the mortgage market. Our main estimate might therefore be biased downwards by the smaller than normal share of households eligible to refinance at all.

A final consideration is that, in the main sample, the neighbors who were refinancing were in financial positions that allowed them to refinance. I.e., they were likely to be relatively wealthy or high income. If households are especially likely to be influenced by neighbors they perceive as being financially secure, then our estimate might be larger than it would be in more normal times. As we show in Table 7, our estimated effect sizes are fairly stable over the whole boom bust cycle suggesting that neighborhood peer effects are always important. But future work should try to better understand how the financial situations of households and their neighbors affect their social interactions and how they might socially influence each other.

Overall, Section 4.2 demonstrates the importance of social interaction effects across various geographic specifications and modelling assumptions, over time, and in all neighborhoods, regardless of socioeconomic status. Assuming households' immediate neighbors are conditionally random, an assumption validated in a number of ways (see Section 3.2 and Section 4.2.2), our results are evidence of important neighbor social influence effects. In the next section, we will investigate more deeply the potential mechanism – word-of-mouth interactions. Doing so will also yield results that further suggest that a social interaction effect, and not hyperlocal sorting, is the best explanation for our main finding.

5 A Word-of-Mouth Channel

Much of the work on neighbor effects has focused on visual inference as a primary channel for information transmission. Foreclosures are typically observable via bank notices, non-occupancy, or dramatic cuts to home maintenance (Gupta, 2019; Towe and Lawley, 2013). Conspicuous consumption is, by definition, salient to neighbors with no face-to-face social interactions required. In our setting, visual influence is unlikely since the decision of whether or not to refinance is private. In some cases, refinances might be visually observable if they are cash-out refinances used to finance expansions, but these were exceedingly rare during the housing crisis time period of our sample.[6] This is suggestive of hyperlocal neighbor influence driven mainly via a word-of-mouth transmission mechanism. To provide more evidence consistent with this channel, we conduct three tests where word-of-mouth social interactions are relatively more or relatively less likely to occur.

5.1 Neighbor Proximity

[FIGURE 1 HERE]

In our first test, presented in Figure 1, we use our preferred geography fixed effect specification and vary the distance between the household and the neighbors of interest.[7] If hyperlocal clustering in refinancing operates primarily through word-of-mouth social interactions, then we would expect to find that the strength of hyperlocal influence is strongest among immediate neighbors and decays as the geographical distance between them increases. First, we estimate the effect of neighbors living within 50 meters, which corresponds to the first specification in Table 3. Then, in the second specification, we estimate the effect of those neighbors greater than 50 meters away but within 100 meters.

We find that when one more of these neighbors refinances, the average household is just 3.7% more likely to refinance in the following quarter, an effect nearly twice as weak as when the refinancing neighbor is within 50 meters. This decreases again to just 2.1% when we look at neighbors within 250 meters but farther than 100 meters away. Neighbors who live nearest are those with whom a given household is most likely to socially interact and therefore most likely to be socially influenced by. In addition to providing evidence consistent with word-of-mouth social interactions, these results allow us to quantify the relative strength of hyperlocal ties versus broader neighborhood level ties and the extent to which their effects spatially decay. Finally, while recent evidence has documented *regional* variation in refinancing rates (Beraja et al., 2019), our results suggest that refinancing variation occurs even at hyperlocal levels.

[6] See the 2013 First Quarter Refinance Report from Freddie Mac online at http://www.freddiemac.com/fmac-resources/research/pdf/RefiReport2013Q1.pdf, accessed May 11, 2020.

[7] See the corresponding regression coefficients in Table A5.

5.2 Non-Occupant Owners

In our next test, we investigate the effects of neighbor social influence on a natural falsification group, non-occupant – or "investor" – owners. Recall that our sample omits properties owned by business or professional investors. That is, all properties in our sample are owned by individuals (or couples) who own at most three homes at any one time. We define an owner as a non-occupant if the property address is different than the mailing address where the tax bill is sent. Chinco and Mayer (2016) show that this measure is not perfect, but our hypothesis is that these owners are less likely to occupy the home full-time, are therefore less likely to interact with those living near it, and, consequently, are less likely to be socially influenced to refinance when those living near the secondary home refinance.

[TABLE 8 HERE]

In the first specification of Table 8, we add investor-owned properties to the sample used previously. We then interact the last quarter refinances of nearby households with a dummy variable equal to 1 if the household is owner occupied. What we find is that households are only socially influenced by their neighbors if they actually live there. Indeed, the main effect of neighbor influence is a fairly precisely estimated zero.

In specification (2), we limit the sample to just homes owned by "investors." Some of these homes are owner occupied, homes that are investors' primary residences, and some of them are not. The main effect of neighbors is now positive, potentially because even non-occupant owners interact with households who live around the second or third home. Importantly, the peer effect nearly triples when the home is occupied by its owner. When interpreting the first specification, one might be concerned that the significant interaction effect is due simply to households who own second and third properties being very different than households who own just one. For example, it is possible that households that own multiple properties may have more experience or expertise, or may have weaker social ties (Bianchi and Vohs, 2016) making these households less susceptible to social influence effects. Our second specification allows us to rule out this concern as we test our effects among the subset of households who own multiple properties. Here, we test whether a household's immediate neighbors influence refinance decisions, as a function of whether the property is a primary residence, presumably where hyperlocal social interactions are more likely to occur, or not.

In the third specification, we take full advantage of the fact that in many instances we observe the investors' primary residences and replace the geography fixed effect with an owner-by-quarter fixed effect. In this way, we look within owner at the two or three properties they own and show that they are more than doubly influenced by the neighbors at their primary residence than their second and third homes.

In both Figure 1 and Table 8, when households are less likely to talk to a neighbor, either because the neighbor lives farther away or because the household does, we find that the social influence effect is weaker. Since refinance decisions are private and in no way visually salient, our body of evidence is most consistent with a social influence effect that requires a word-of-mouth mechanism to propagate.

5.3 Racial Ties

Our third set of tests is motivated by research from multiple disciplines that finds that social interactions are more common between people who share salient characteristics, especially race and ethnicity (Ananat et al., 2018; Billings et al., 2019; Fletcher et al., 2020; Hellerstein et al., 2008; Ioannides and Datcher Loury, 2004; McPherson et al., 2001). Moreover, homophily is most likely to have an effect when individuals face uncertainty or in novel and less familiar settings (Galaskiewicz and Shatin, 1981; Ibarra, 1993; Kanter, 2008), as is likely the case with complex mortgage refinancing decisions (Agarwal et al., 2013; Chen and Ling, 1989; Gomes et al., 2020). If a word-of-mouth social influence effect is the driver of our documented neighbor effect, then we should expect to find that households are more influenced by same-race neighbors, with whom they are more likely to interact, than neighbors of a different race. If, on the other hand, hyperlocal sorting or visual influence were the cause, then the race of the neighbor should be irrelevant.

[TABLE 9 HERE]

We begin this investigation with simple sample splits by household race and ethnicity. We measure race by first using race and ethnicity variables from HMDA and then using last names that are mostly unique to one race or ethnicity (this procedure is described in more detail in Section 2.2). Table 9 presents the results of this analysis. Looking to the row of estimated magnitudes of the neighbor peer effect, we find that social influence effects are economically important and statistically similar for people of all races and ethnicities. Next, looking at the second-to-last row that presents the share of households that refinanced in a given quarter, we find that Black and Hispanic household are much less likely to refinance than white households, consistent with prior work (Berkovec et al., 1996, 1998; Gerardi et al., 2020). Specifically, 3.33% of white households refinance each quarter on average, while just 1.86% of Black households and 2.18% of Hispanic households do.

The final row of Table 9 also merits some discussion. This row tells us that both the average Black household and the average Hispanic household had just 0.16 nearby neighbors recently refinance. The average white household, on the other hand, had 0.22 nearby neighbors refinance. In other words, the average white household had 37.5% more nearby neighbors refinance than the average Black or Hispanic household. In some sense, this result is not surprising since (i) Black and Hispanic households are themselves less likely to be refinancing, as we just saw, and (ii) households typically live in neighborhoods with those of the same race (Ananat et al., 2018; Billings et al., 2019; Hellerstein et al., 2008). But the implications are important. It is possible that racial differences in baseline refinancing rates are a function of the relatively smaller spillover effects. That is, much like how neighborhood refinances beget more neighborhood refinances through a social influence effect channel, lower levels of refinancing activity lead to even less refinancing going forward. A further possibility we leave to future research is that differences in either the frequency or content of word of mouth discussions between hyperlocal neighbors in white neighborhoods versus Black and Hispanic neighborhoods may play a role in the relatively lower level of Black and Hispanic refinancing.

[TABLE 10 HERE]

Many neighborhoods in Los Angeles are racially segregated. But this is not true everywhere. In this next analysis, we classify neighborhoods by their racial diversity using an adapted Herfindahl-Hirschman index methodology to calculate a measure of racial diversity. We sum the square of each race's share in the census block group with lower scores indicating that the neighborhood is racially mixed and higher scores indicating that the neighborhood is more racially homogeneous. We then split the sample into four groups and re-estimate Equation 1 in each subsample. In Table 10, we present our initial findings: households are socially influenced by the nearest neighbors regardless of neighborhood racial composition. Next, we explore whether households are differentially influenced by neighboring households of the same race versus those that are a different race.

[TABLE 11 HERE]

To create Table 11, we count refinances by same-race neighbors and refinances by different-race neighbors separately. In column (1), we show that the average household is significantly more affected by same-race neighbors than different race-neighbors. In the second set of results in this table, we split the sample as in Table 10 and document that, across all neighborhoods, regardless of racial diversity, households are similarly affected by households of the same race. However, as racial diversity increases, so too does the social influence effect of opposite-race neighbors.

These results allow us to reach two conclusions. First, word-of-mouth effects are the most likely mechanism behind the social influence effects we document. The finding that same race neighbors' decisions are especially important is far more consistent with households interacting with same-race neighbors than with hyperlocal sorting or a visual salience of neighbor refinances which should not manifest in a difference between same-race and different-race neighbor refinances. Second, our findings suggest that social influence effects may be an important factor in understanding racial disparities in mortgage markets more broadly (see, for example, Bartlett et al. (2021); Bhutta and Hizmo (2021); Kau et al. (2019)).

To be clear, our results here should be interpreted with caution. While a households' very nearest neighbors are random, conditional on a slightly larger neighborhood, that neighborhood choice is *not* random. In this case, the kinds of households who choose to live in racially diverse neighborhoods are likely different in many ways than households who choose to live in neighborhoods where one race or ethnicity is in the clear majority. Our results motivate a deeper, more dynamic analysis that accounts for structural influences. This work, in line with McPherson et al. (2001) and Ingram and Morris (2007), will be necessary to understand the origin of these network ties, particularly as they exist across racial lines. Finally, our finding that the effects of different-race neighbors increase in neighborhood racial diversity suggests, very tentatively, a possible benefit of living in diverse neighborhoods. But again, more work will be needed to pin down causality.

6 Variation in Potential Interest Payment Savings

Many factors contribute to the household's decision of whether or not to refinance including when they plan to move (Yang and Maris, 1993), their ability to qualify for and pay the closing costs of a new mortgage (Archer et al., 1996; Campbell, 2006; Caplin et al., 1993), and their available mental bandwidth for solving a complex finance problem, a problem particularly severe for financially unsophisticated households (Allen et al., 2019; Boyce et al., 2012; Bucks and Pence, 2008). A limitation of this study is that many of these factors are unobservable to us. To the extent that households' nearest neighbors are quasi-randomly assigned, though, the unobservable nature of these variables should not bias our results. That is, the reasons households might choose to refinance should not be spatially clustered, an assumption we do our best to validate in Table 5. In this final section, we investigate whether the neighbor social influence effect is different for people who stand to save a lot by refinancing compared to people who do not.

[TABLE 12 HERE]

Our sample period of 2008 to 2012 is particularly notable as a time when mortgage rates dropped significantly and many households could save considerable amounts of money by refinancing. To estimate the interest rate likely being paid by the mortgage borrowers in our sample, we supplement our main sample dataset with MIRS data containing the yearly file for Los Angeles. We split the sample into two sets of households. Our classification differentiates households whose current interest rate is 1%, or 100 basis points (BPS), higher than the prevailing rate (who would potentially save a great deal in monthly payments by refinancing) to households whose interest rates is either the same or lower than the prevailing rate.

In Table 12, we document that social influence effects appear to be about twice as strong on households who stand to gain the most by refinancing.

Households may be influenced by their neighbors for at least three reasons. First, they may learn from their neighbors—a household may not have been aware that refinancing was an option for them or that refinancing could save them money on their monthly mortgage bill. The second potential mechanism, social utility, exists when a household's utility is increased if they make the same decision as a peer. For example, a household might choose to refinance when their peers do because their peers can then help them refinance optimally next time. Finally, households might suboptimally herd with their neighbors. Also called social conformity, this phenomenon occurs when a household ignores its own private information and instead follows the crowd. While we leave uncovering the precise mechanism(s) driving our identified effects to future work, we can use the results from Table 12 to shed some early light on a learning story as opposed to a social utility or herding story. That is, if the economic mechanism was a purely a social utility or herding one, then what a household ought to do would not interact with the social influence effect. Our finding suggests that complementarities may exist between what a household ought to do and if other nearby households engage in that activity.

7 Conclusion

Choosing when to refinance a mortgage is one of the most important decisions a household faces. Furthermore, the decision can be complex, and most households have limited experience and expertise to guide them. Information asymmetry between lenders and borrowers renders traditional sources of influence – the media, the lenders themselves – less reliable, at least from the household's point of view. A natural question emerges: Whom do households turn to for information when deciding whether or not to refinance, and how is this information transferred? In this paper, we look at one source of influence: hyperlocal, neighbor peer groups. Previous work has shown that neighbors do socially influence each other's publicly observable decisions. But there is scant evidence on the importance of neighbor social influence effects on households' private economic decisions.

Using precise data on where households are located, and thus with whom households are more likely to socially interact, we test whether word-of-mouth, social interactions can have a meaningful impact on household financial decision-making. We find that households are 7% more likely to refinance for each additional neighbor within 50 meters who refinanced in the previous quarter. We find that the magnitude of neighbor peer effects are stronger when the neighbors are more proximate. Furthermore, non-occupants, i.e., households who are less likely to interact with their hyperlocal neighbors, show less evidence of being socially influenced. Next, we show that, on average, households are twice as influenced by neighbors of the same race than neighbors of a different race. Furthermore, while the effect of same-race neighbors is the same everywhere, the social influence effect of different-race neighbors is increasing in neighborhood racial diversity. Our key contribution is evidence that social interactions between neighbors, especially same-race neighbors, matter for household wealth accumulation.

Our work has clear implications for policy. The size of potential savings from optimally refinancing can save households thousands of dollars. However, many households fail to refinance. Our results suggest that community-based targeting may be an important strategy to influence households and consequently their neighbors as well. While our work documents that proximate communities do indeed matter, identifying why and exactly how word-of-mouth social influence effects propagate still needs to be better understood. One possibility is that households share their own decisions when they perceive those decisions to have been good to manage impression concerns and make themselves feel smart or helpful as in Berger (2014). Finally, it is quite plausible that households are socially influenced by their neighbors – particularly those of the same race – not just when making refinance decisions. Hyperlocal social influence effects may be at play in other important household financial decisions that we do not explore in this paper, such as whether to use a fintech lender or whether to prepay a mortgage.

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Figure 1: Neighbor Social Influence Effects Decay over Distance

This figure illustrates how one additional neighbor refinancing influences a household's likelihood of refinancing in a given quarter as a function of the distance between the household and the refinancing neighbor. The solid lines reflects the strength of the neighbor social influence effect, while the dashed lines represent the 95% confidence interval. Effect sizes and standard errors are from models estimated in Table A5.

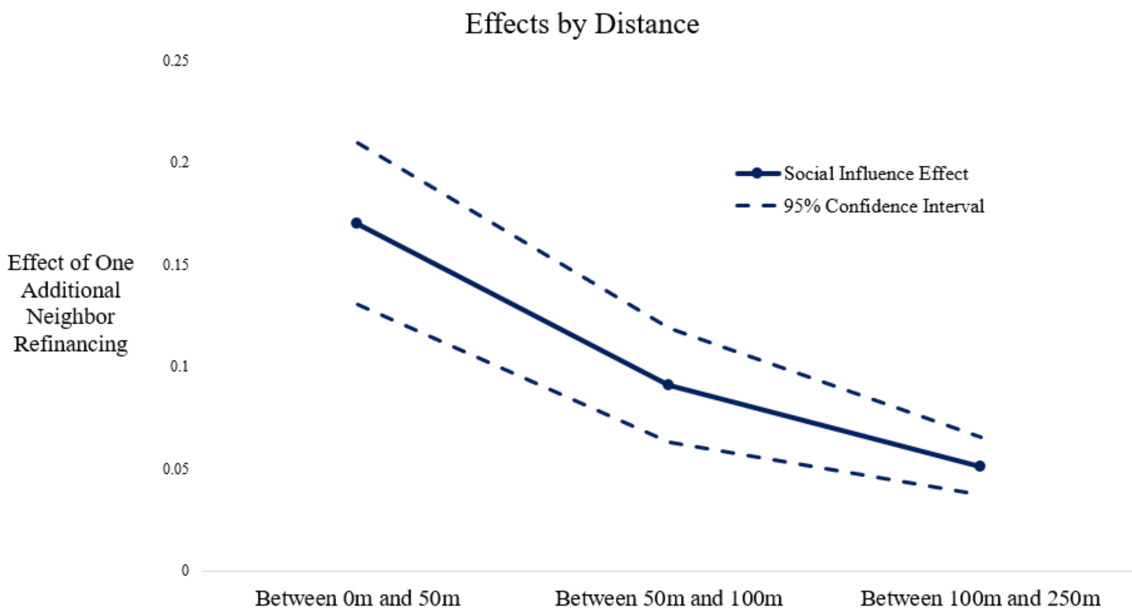

Table 1: Describing the Sample of Households

This table describes the sample that we use for our tests of social influence on the refinance decision. Each quarter, we observe whether or not the household refinanced, the refinancing decisions of their neighbors, and the characteristics of their outstanding loan. We also observe time invariant characteristics about owners and their homes. Adjustable rate mortgages are defined as those with adjustable or graduated interest rates; all mortgages have either adjustable or fixed interest rates. Current LTV is defined as the estimated outstanding loan balance on the household's primary mortgage divided by the estimated current house price. The interest rate difference is the difference between the current interest rate and the rate that was prevailing the year their last mortgage was originated. Co-signer indicates that there are two people on the mortgage contract. The income variables are from HMDA and are defined at the time of the mortgage application. We describe our construction of the race/ethnicity measure in Section 2.2. Property characteristics are from the county assessor's office.

	Mean	Std. Dev.	N
Refinanced this Quarter			
Household Refinanced this Quarter (=1)	2.4%	15.2%	17,582,305
Neighborhood Activity			
Nbrs within 50m Refi'd Last Qtr	0.19	0.46	17,582,305
Nbrs within 100m Refi'd Last Qtr	0.52	0.83	17,582,305
Nbrs within 250m Refi'd Last Qtr	2.49	2.51	17,582,305
Nbrs within 50m	10.63	11.27	17,582,305
Nbrs within 100m	25.95	17.90	17,582,305
Nbrs within 250m	117.42	59.88	17,582,305
Outstanding Loan Characteristics			
ARM (=1)	45.1%	49.8%	17,582,305
Refinance (=1)	81.6%	38.7%	17,582,305
Quarters Since Origination	19.2	15.0	17,582,305
Current LTV	76%	54%	17,582,305
Interest Rate Difference (ppts)	-1.10	0.90	17,582,305
Borrower Characteristics			
Co-Signers (=1)	64.5%	47.8%	17,582,305
Owner Occupied (=1)	92.0%	27.2%	17,582,305
Applicant Income (1,000s)	$125	$167	6,620,648
Asian (=1)	19.8%	39.9%	12,781,753
Non-Hispanic Black (=1)	4.6%	21.0%	12,781,753
Hispanic (=1)	38.5%	48.7%	12,781,753
Non-Hispanic White (=1)	37.1%	48.3%	12,781,753
Property Characteristics			
2011 Assessed Value	$351,484	$353,921	17,582,305
Square Feet	1795	1262	17,582,305

Table 2: Assumption Validity – Repeated Equality of Means Tests

In this table, we present p-values from t-tests on the equality of means between each block and blocks adjacent to it. To create this table we focus on the third quarter of 2010. We then, block by block and variable by variable, compare each block to its adjacent blocks with a t-test on the equality of means. For example, if a block has a very different mean of one variables than the blocks adjacent to it, the t-test will produce a low p-value. We store the p-values of each test and then list key moments of their distribution in this table. We first presents the results for all blocks in Los Angeles and then again for just those blocks in homogeneous neighborhoods (neighborhoods where no more than one variable is significantly different between the block and the adjacent blocks at the 5% level and no more than two variables are significantly different at the 10% level).

	5^{th}	25^{th}	50^{th}	75^{th}	95^{th}	Count
	All Neighborhoods					
Homeowner Demographics						
Co-Signers (=1)	0.034	0.214	0.470	0.733	0.947	52,760
Income (1,000s)	0.040	0.253	0.504	0.745	0.948	37,987
HMDA Race (White = 1)	0.052	0.252	0.468	0.698	0.938	36,479
Tenure (Quarters)	0.025	0.204	0.455	0.725	0.943	52,729
Loan Characteristcs						
Outstanding Loan > 80% CLTV (=1)	0.052	0.250	0.459	0.695	0.935	35,532
Interest Rate Difference (ppts)	0.042	0.239	0.491	0.745	0.949	52,284
Property Characteristics						
2011 Assessed Home Value	0.008	0.169	0.418	0.683	0.936	52,774
Square Feet	0.002	0.125	0.382	0.670	0.932	52,469
	Homogeneous Neighborhoods					
Homeowner Demographics						
Co-Signers (=1)	0.058	0.245	0.493	0.744	0.950	44,850
Income (1,000s)	0.067	0.284	0.523	0.756	0.951	32,187
HMDA Race (White = 1)	0.072	0.267	0.477	0.703	0.939	30,847
Tenure (Quarters)	0.055	0.239	0.483	0.738	0.946	44,824
Loan Characteristcs						
Outstanding Loan > 80% CLTV	0.078	0.272	0.473	0.697	0.935	29,557
Interest Rate Difference	0.065	0.263	0.507	0.754	0.951	44,470
Property Characteristics						
2011 Assessed Home Value	0.045	0.230	0.463	0.708	0.941	44,864
Square Feet	0.019	0.191	0.436	0.700	0.939	44,586

Table 3: Neighbor Peer Effects on the Household's Decision to Refinance

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its neighbors' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are as defined in Table 1, except for the race and ethnicity variables. We use only data from HMDA to measure race and ethnicity. Hispanic is a dummy variable. The race variable is a categorical variable with five categories, Native American is the omitted group. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)		
Sample	*Owner-Occupied Households*		
	(1)	(2)	(3)
Nbrs within 50m Refi'd Last Qtr	0.432***	0.170***	0.155***
	(0.049)	(0.020)	(0.026)
Number of Neighbors within 50m	-0.0197***	-0.0187***	-0.0184***
	(0.002)	(0.002)	(0.002)
Outstanding Loan is an ARM	-0.859***	-0.892***	-1.110***
	(0.143)	(0.149)	(0.183)
Outstanding Loan is a Refinance	-0.0526	-0.258***	-0.233**
	(0.068)	(0.077)	(0.104)
Co-Signers	0.577***	0.523***	0.632***
	(0.046)	(0.043)	(0.065)
Natural Log 2011 Assessed Value	0.657***	0.0677	0.111
	(0.122)	(0.073)	(0.165)
Natural Log Square Feet	0.570***	0.585***	0.776***
	(0.049)	(0.049)	(0.087)
Current LTV	-0.00302***	-0.00426***	-0.00462***
	(0.001)	(0.001)	(0.001)

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Home Built 1931-1950	0.293***	-0.0479**	-0.0619
	(0.047)	(0.023)	(0.046)
Home Built 1951-1970	0.218***	-0.238***	-0.226***
	(0.062)	(0.026)	(0.059)
Home Built 1971-1980	-0.208**	-0.700***	-0.648***
	(0.074)	(0.048)	(0.076)
Home Built 1981-1990	-0.580***	-0.870***	-0.785***
	(0.073)	(0.064)	(0.082)
Home Built 1991-2000	-0.623***	-0.748***	-0.665***
	(0.075)	(0.065)	(0.075)
Home Built 2001-	-0.677***	-0.662***	-0.524***
	(0.092)	(0.078)	(0.110)
Natural Log Owner Income			-0.141**
			(0.060)
Hispanic			-0.324***
			(0.053)
Asian			0.177*
			(0.101)
Black			-0.271***
			(0.066)
Pacific Islander			-0.234**
			(0.096)
White			-0.0556
			(0.054)
Fixed Effects			
Quarters Since Last Mortgage	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y
Block Group × Quarter		Y	Y
N	16,166,843	16,166,526	3,826,715
Sample Means			
Refi'd This Quarter	2.44	2.44	2.87
Nbrs within 50m Refi'd Last Qtr	0.19	0.19	0.20

Table 4: A Concentric Circles Research Design

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its neighbors' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables included in the first and second specifications in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)					
Sample	*Owner-Occupied Households*					
	(1)	(2)	(3)	(4)	(5)	(6)
Nbrs within 50m Refi'd Last Qtr	0.0801***	0.0878***	0.0829***	0.140***	0.129***	0.120***
	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)	(0.015)
Nbrs within 100m Refi'd Last Qtr	0.313***	0.122***	0.0889***			
	(0.039)	(0.018)	(0.014)			
Nbrs within 250m Refi'd Last Qtr				0.169***	0.0761***	0.0615***
				(0.021)	(0.010)	(0.008)
CoreLogic Controls	Y	Y	Y	Y	Y	Y
Fixed Effects						
Quarters Since Last Mortgage	Y	Y	Y	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y	Y	Y	Y
Tract × Quarter		Y			Y	
Block Group × Quarter			Y			Y
N	16,166,843	16,166,690	16,166,526	16,166,843	16,166,690	16,166,526
Sample Means						
Refi'd This Quarter	2.44	2.44	2.44	2.44	2.44	2.44
Nbrs within 50m Refi'd Last Qtr	0.19	0.19	0.19	0.19	0.19	0.19

Table 5: Assumption Validity – Falsification Tests

This table reports the estimated relationship between two characteristics of a household's outstanding mortgage and the average of their nearby neighbors' outstanding mortgage. The specification mimics the main specifications in Table 3 and Table 4 and the sample is the same. CoreLogic control variables include a dummy for if the outstanding mortgage is an ARM, a dummy for if the outstanding mortgage is a refinance, a dummy for the presence of co-signers on the loan, controls for size, current LTV (except columns (1) and (2)), and house age. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Sample	*Owner-Occupied Households*			
Dependent Variable	Current LTV		Int Rate Diff	
	(1)	(2)	(3)	(4)
Nbrs within 50m Dep Var Avg	-0.00742**	0.00243	-0.00406*	0.00434
	(0.003)	(0.003)	(0.002)	(0.003)
Nbrs within 100m Dep Var Avg		-0.0454*		0.0843***
		(0.024)		(0.009)
CoreLogic Controls	Y	Y	Y	Y
Fixed Effects				
Outstanding Lender	Y	Y	Y	Y
Census Block Group	Y		Y	
N	15,950,017	15,950,018	15,963,819	15,963,820
Sample Means				
Dependent Variable	75.05	75.05	-1.10	-1.10
Key Independent Variable	76.08	76.08	-1.13	-1.13

Table 6: Estimating the Main Specifications in Homogeneous and Non-Homogeneous Neighborhoods

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its neighbors' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1. The first three columns further restrict the sample to just owner-occupied households and census blocks statistically indistinguishable from their surrounding census blocks. The second set of three columns uses the mutually exclusive, in this case non-homogeneous neighborhoods, sample. Control variables are defined in Table 1. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)					
Sample	*Owner-Occupied Households*					
Subsample	*Homogeneous Neighborhoods*			*Non-Homogeneous Neighborhoods*		
	(1)	(2)	(3)	(4)	(5)	(6)
Nbrs within 50m Refi'd Last Qtr	0.422***	0.121***	0.104***	0.444***	0.214***	0.194***
	(0.053)	(0.018)	(0.027)	(0.045)	(0.024)	(0.035)
CoreLogic Controls	Y	Y	Y	Y	Y	Y
HMDA Controls			Y			Y
Fixed Effects						
Quarters Since Last Mortgage	Y	Y	Y	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y	Y	Y	Y
Block Group × Quarter		Y	Y		Y	Y
N	11,206,461	11,206,168	2,686,039	4,952,741	4,951,952	1,136,229
Sample Means						
Refi'd This Quarter	2.49	2.49	2.91	2.31	2.31	2.77
Nbrs within 50m Refi'd Last Qtr	0.19	0.19	0.20	0.18	0.18	0.19

Table 7: Homogeneity of the Neighbor Peer Effect Size Over Time

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its neighbors' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)									
Sample	*Owner-Occupied Households*									
Subsample, Years	*2003*	*2004*	*2005*	*2006*	*2007*	*2008*	*2009*	*2010*	*2011*	*2012*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Nbrs within 50m Refi'd Last Qtr	0.176***	0.0580**	0.0917***	0.0785**	0.162**	0.131**	0.179**	0.240**	0.135**	0.158**
	(0.029)	(0.016)	(0.015)	(0.024)	(0.041)	(0.028)	(0.032)	(0.066)	(0.027)	(0.048)
CoreLogic Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fixed Effects										
Quarters Since Last Mortgage	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Block Group × Quarter	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	2,133,039	2,417,551	2,652,140	2,790,388	2,970,306	3,194,793	3,263,633	3,266,491	3,242,501	3,199,108
Sample Means										
Refi'd This Quarter	13.65	9.93	8.73	6.91	4.85	2.09	2.22	2.50	2.52	2.85
Nbrs within 50m Refi'd Last Qtr	1.06	0.86	0.74	0.65	0.42	0.21	0.15	0.16	0.18	0.25

Table 8: A Falsification Test – Non-Occupant Owners

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its neighbors' recent refinancing decisions. Specification (1) mimics the second specification in Table 3 but uses a larger sample that includes non-owner occupied homes. Specifications (2) and (3) use only the sample of "investor"-owned properties, those properties whose owners own either two or three properties simultaneously. Control variables are defined in Table 1 and include all variables included in the first and second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)		
Sample	*All Properties*	*'Investor' Owned Properties*	
	(1)	(2)	(3)
Nbrs within 50m Refi'd Last Qtr	0.0156	0.110*	0.155**
	(0.037)	(0.057)	(0.062)
Owner Occupied	0.569***	0.469***	0.653***
	(0.058)	(0.045)	(0.061)
Nbrs within 50m Refi'd Last Qtr × Owner Occupied	0.166***	0.195**	0.235**
	(0.042)	(0.073)	(0.091)
CoreLogic Controls	Y	Y	Y
Fixed Effects			
Quarters Since Last Mortgage	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y
Block Group × Quarter	Y	Y	
Owner × Quarter			Y
N	17,582,305	1,828,172	1,159,277
Sample Means			
Refi'd This Quarter	2.36	1.74	1.66
Nbrs within 50m Refi'd Last Qtr	0.19	0.17	0.17

Table 9: Splitting the Sample by Household Race

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its neighbors' recent refinancing decisions across for subsamples of census block groups in Los Angeles County. As described in the text, households are classified into one of four races: Asian, Non-Hispanic Black, Hispanic, and Non-Hispanic White using both HMDA race and ethnicity variables and surnames commonly associated with just one race or ethnicity. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)			
Sample	*Owner-Occupied Households*			
Subsample, Household Race	*Asian*	*Non-Hispanic Black*	*Hispanic*	*Non-Hispanic White*
	(1)	(2)	(3)	(4)
Nbrs within 50m Refi'd Last Qtr	0.168***	0.154***	0.148***	0.184***
	(0.036)	(0.053)	(0.024)	(0.024)
CoreLogic Controls	Y	Y	Y	Y
Fixed Effects				
Quarters Since Last Mortgage	Y	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y	Y
Block Group × Quarter	Y	Y	Y	Y
N	2,303,961	502,286	4,430,280	4,351,628
Sample Means				
Refi'd This Quarter	2.46	1.86	2.18	3.33
Nbrs within 50m Refi'd Last Qtr	0.20	0.16	0.16	0.22

Table 10: Comparing The Effect Across Neighborhood Racial Homogeneity

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its neighbors' recent refinancing decisions across for subsamples of census block groups in Los Angeles County. As described in the text, households are classified into one of four races: Asian, Non-Hispanic Black, Hispanic, and Non-Hispanic White. We then calculate a racial Herfindahl-Hirschman Index that sums the square of each race's share in the census block group. Lower scores mean the neighborhood is more racially mixed. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)			
Sample	*Owner-Occupied Households*			
Subsample, Nbhd Diversity	*Least Diverse*	*2*	*3*	*Most Diverse*
	(1)	(2)	(3)	(4)
Nbrs within 50m Refi'd Last Qtr	0.175***	0.160***	0.178***	0.161***
	(0.028)	(0.029)	(0.024)	(0.020)
CoreLogic Controls	Y	Y	Y	Y
Fixed Effects				
Quarters Since Last Mortgage	Y	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y	Y
Block Group × Quarter	Y	Y	Y	Y
N	4,280,337	3,300,263	4,605,973	3,960,475
Sample Means				
Refi'd This Quarter	2.59	2.53	2.45	2.18
Nbrs within 50m Refi'd Last Qtr	0.20	0.20	0.19	0.18
Nbhd Racial HHI	[0.60, 1.00]	[0.50, 0.60)	[0.40, 0.50)	[0.00, 0.40)

Table 11: Comparing the Effect of Neighbors of the Same Race and Neighbors of a Different Race

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its neighbors' recent refinancing decisions across for subsamples of census block groups in Los Angeles County. As described in the text, households are classified into one of four races: Asian, Non-Hispanic Black, Hispanic, and Non-Hispanic White. We then calculate a racial Herfindahl-Hirschman Index that sums the square of each race's share in the census block group. Lower scores mean the neighborhood is more racially mixed. Next, we separately count the number of refinances made by neighbors of the same race as the household and the number made by neighbors of a different race. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)				
Sample	*Owner-Occupied Households*				
Subsample, Nbhd Diversity	*All*	*Least Diverse*	*2*	*3*	*Most Diverse*
	(1)	(2)	(3)	(4)	(5)
Same Race Nbrs within 50m Refi'd Last Qtr	0.240***	0.222***	0.224***	0.231***	0.299***
	(0.028)	(0.038)	(0.047)	(0.041)	(0.050)
Diff Race Nbrs within 50m Refi'd Last Qtr	0.0965***	0.004	0.0793*	0.122***	0.137***
	(0.022)	(0.036)	(0.041)	(0.038)	(0.031)
CoreLogic Controls	Y	Y	Y	Y	Y
Fixed Effects					
Quarters Since Last Mortgage	Y	Y	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y	Y	Y
Block Group × Quarter	Y	Y	Y	Y	Y
N	11,638,664	3,180,234	2,407,378	3,223,729	2,813,701
Sample Means					
Refi'd This Quarter	2.65	2.80	2.79	2.65	2.37
Same Race Nbrs within 50m Refi'd Last Qtr	0.09	0.12	0.10	0.07	0.05
Diff Race Nbrs within 50m Refi'd Last Qtr	0.07	0.04	0.07	0.08	0.09
Nbhd Racial HHI	all	[0.60, 1.00]	[0.50, 0.60)	[0.40, 0.50)	[0.00, 0.40)

Table 12: Variation in Potential Interest Payment Savings

This table compares the social influence effect between two sets of households. Our classification differentiates households whose current interest rate is 1%, or 100 basis points (BPS), higher than the prevailing rate (who would potentially save a great deal in monthly payments by refinancing) to households whose interest rates is either the same or lower than the prevailing rate. The sample is of refinance-or-not decisions made between 2008 and 2012. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)		
Sample	*≤ 100 BPS Difference*	*>100 BPS Difference*	*All*
	(1)	(2)	(3)
Nbrs within 50m Refi'd Last Qtr	0.0779***	0.154***	0.0893***
	(0.012)	(0.022)	(0.016)
>100 BPS Difference (=1)			-1.100***
			(0.313)
Nbrs within 50m Refi'd Last Qtr × >100 BPS Difference			0.0631**
			(0.029)
CoreLogic Controls	Y	Y	Y
Fixed Effects			
Block Group × Quarter	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y
N	1,362,740	8,626,550	9,991,611
Sample Means			
Refi'd This Quarter	2.14	2.39	2.36
Nbrs within 50m Refi'd Last Qtr	0.20	0.19	0.19

ONLINE APPENDIX

"Household Mortgage Refinancing Decisions are Neighbor Influenced, Especially Along Racial Lines"

October 27, 2021

This is the Online Appendix for "Household's Mortgage Refinancing Decisions Are Neighbor Influenced, Especially Along Racial Lines."

- Appendix A contains the supplemental tables.

A Supplemental Tables

Table A1: A Concentric Circles with Multiple Rings Approach

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its peers' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)		
Sample	*Owner-Occupied Households*		
	(1)	(2)	(3)
Nbrs within 50m Refi'd Last Qtr	0.102***	0.0801***	0.140***
	(0.014)	(0.013)	(0.015)
Nbrs within 100m Refi'd Last Qtr	0.0429***	0.313***	
	(0.008)	(0.039)	
Nbrs within 250m Refi'd Last Qtr	0.163***		0.169***
	(0.020)		(0.021)
CoreLogic Controls	Y	Y	Y
Fixed Effects			
Quarters Since Last Mortgage	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y
N	16,166,843	16,166,843	16,166,843
Sample Means			
Refi'd This Quarter	2.44	2.44	2.44
Nbrs within 50m Refi'd Last Qtr	0.19	0.19	0.19

Table A2: Varying the Geography Fixed Effect

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its peers' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)		
Sample	*Owner-Occupied Households*		
	(1)	(2)	(3)
Nbrs within 50m Refi'd Last Qtr	0.214***	0.170***	0.0831***
	(0.026)	(0.020)	(0.012)
CoreLogic Controls	Y	Y	Y
Fixed Effects			
Quarters Since Last Mortgage	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y
Tract × Quarter	Y		
Block Group × Quarter		Y	
Block × Quarter			Y
N	16,166,690	16,166,526	16,150,385
Sample Means			
Refi'd This Quarter	2.44	2.44	2.44
Nbrs within 50m Refi'd Last Qtr	0.19	0.19	0.19

Table A3: Homogeneity of the Effect Size over Local House Prices

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its peers' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)				
Sample	*Owner-Occupied Households*				
Subsample, Tract Avg HP	*Least*	*2*	*3*	*4*	*Most*
	(1)	(2)	(3)	(4)	(5)
Nbrs within 50m Refi'd Last Qtr	0.117***	0.0885***	0.105***	0.128***	0.156***
	(0.019)	(0.025)	(0.020)	(0.021)	(0.022)
CoreLogic Controls	Y	Y	Y	Y	Y
Fixed Effects					
Quarters Since Last Mortgage	Y	Y	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y	Y	Y
Block Group × Quarter	Y	Y	Y	Y	Y
N	6,018,708	6,403,805	7,201,419	7,236,549	6,796,652
Sample Means					
Refi'd This Quarter	3.78	4.12	4.31	4.50	4.80
Nbrs within 50m Refi'd Last Qtr	0.39	0.44	0.46	0.49	0.43
House Price	$194,440	$244,530	$300,454	$398,502	$827,691

Table A4: Heterogeneity of the Effect Size over Current Loan-to-Value Ratio

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its peers' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3, except current loan-to-value ratio. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)		
Sample	*Owner-Occupied Households*		
Subsample, Current LTV	*< 80%*	*80% - 100%*	*> 100%*
	(1)	(2)	(3)
Nbrs within 50m Refi'd Last Qtr	0.175***	0.195***	0.133***
	(0.024)	(0.037)	(0.018)
CoreLogic Controls	Y	Y	Y
Fixed Effects			
Quarters Since Last Mortgage	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y
Block Group × Quarter	Y	Y	Y
N	9,857,509	1,895,358	4,270,024
Sample Means			
Refi'd This Quarter	2.59	3.01	1.86
Nbrs within 50m Refi'd Last Qtr	0.20	0.19	0.17

Table A5: Increasing Radii

This table reports the estimated relationship between a household's decision of whether or not to refinance in a given quarter and its peers' recent refinancing decisions. Linear probability models are estimated using the sample described in Table 1 further restricted to just owner-occupied households. Control variables are defined in Table 1 and include all variables detailed in the second specification in Table 3. Standard errors are two-way clustered at the census tract and year-quarter level and reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively.

Dependent Variable	Household Refinanced This Quarter (=100)		
Sample	*Owner-Occupied Households*		
	(1)	(2)	(3)
Nbrs within 50m Refi'd Last Qtr	0.170***		
	(0.020)		
Nbrs between 50m and 100m Refi'd Last Qtr		0.091***	
		(0.014)	
Nbrs between 100m and 250m Refi'd Last Qtr			0.051***
			(0.007)
CoreLogic Controls	Y	Y	Y
Fixed Effects			
Quarters Since Last Mortgage	Y	Y	Y
Outstanding Lender × Quarter	Y	Y	Y
Block Group × Quarter	Y	Y	Y
N	16,166,526	16,166,526	16,166,526
Sample Means			
Refi'd This Quarter	2.44	2.44	2.44
Nbrs Refi'd Last Qtr	0.19	0.33	2.00