'I'll Have What She's Having': Identifying Social Influence in Household Mortgage Decisions*

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Abstract

We investigate whether household mortgage choices are influenced by social interaction. We build a database using household mortgage data and precisely geolocated real estate data and ask whether households are socially influenced by those with whom they are more likely to interact. Specifically, we test if households are especially affected by their hyperlocal peers (i.e., neighbors who live on the same census block) over and above their neighborhood peers (i.e., neighbors living on the same or adjacent census blocks). We find that households are 1.5% (9.8%) more likely to choose an adjustable rate mortgage (refinance) if the share of their block-peers with an adjustable rate mortgage (who have recently refinanced) increases by ten percentage points. Consistent with a social interactions mechanism, people new to their census block (i.e., purchase loans) show no effects from social influence while those who have had time to socially interact with their neighbors (i.e., refinance loans) do. Furthermore, households who move to a new neighborhood and later refinance their loan (i.e., movers) become more socially influenced over time. Finally, non-occupant owners' decisions about their second and third properties are never influenced by the households that live around the property but are influenced by the neighbors at their primary residence. We complement these empirical strategies in the lab by experimentally assigning peers and peer decisions in a variety of ways to demonstrate the robustness of these findings.

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1 Introduction

Decisions surrounding household mortgage choices have a substantial impact at both the household and public policy levels. Taking out a mortgage is one of the most important financial decisions households ever make. And since household mortgage decisions played such a crucial role in precipitating the Great Recession, there has been a significant push from policy makers to better understand the drivers of mortgage choices. Theory suggests that households making mortgage decisions should consider their private personal characteristics (e.g., their likelihood of moving in the near future or their risk tolerance) and the pricing and terms of the available mortgage options[2]. However, results from a 2015 Consumer Financial Protection Bureau (CFPB) survey of two thousand borrowers found that households often turned to additional, outside sources for information. Perhaps unsurprisingly, individuals consulted with a number of different sources perceived as experts, including lenders, real estate agents, financial planners, and housing counselors. More curiously, individuals also reported consulting their social networks when making mortgage decisions. Over half of the respondents reported that their friends, relatives, and co-workers were important sources of information (CFPB, 2015). Thus, according to this survey, peers meaningfully influence the most significant investment decisions that households make. Yet little empirical research exists that establishes whether social connections matter in practice; moreover, no work, at least to the best of our knowledge, investigate whether geographic peers (i.e., neighbors) play a meaningful role on on household mortgage choices.

Establishing whether one's neighbors socially influence decision-making is of particular interest given the conflicting evidence. Contrary to the CFPB survey, the research agenda of political scientist Robert Putnam argues that social connection is declining (see, e.g., Putnam (1995)). At the same time, social media use has increased nearly 500% in the last decade, potentially exacerbating these trends (Pew Research Center (2015)). Social scientists and health professionals alike assert that social media has created more diffuse social networks, decreasing the importance of the role that neighbors play. Combining the evidence that neighbors are socially less relevant, the theoretical predictions about how households ought to make mortgage decisions, and the results of the CFPB survey we are left without an answer to an ultimately empirical question: Are individuals' mortgage decisions, specifically what type of mortgage they should choose (adjusted rate mortage, ARM, or fixed rate mortage, FRM) and whether or not to refinance, socially influenced by their neighbors; and, if so, are these social influence effects economically meaningful?

Households' peers are not randomly assigned, so the challenges to identifying social influence are severe. Casual observation might mistake endogenous group formation or

[2]See, for example, Agarwal, Driscoll, and Laibson (2013); Campbell and Cocco (2003); Coulibaly and Li (2009)

correlated unobservables for social influence. We use a comprehensive dataset of all mortgage loans made in Los Angeles between 1990 and 2012. Our final dataset includes information about the borrowers, the lenders, the mortgage, and the property. Importantly, we know the exact latitude and longitude of the property securing the mortgage and whether or not the borrower lives there. This level of detail allows us to identify social influence by estimating the effects of the decisions made by a household's block peers (those neighbors on the household's residential block) while controlling for the decisions made by the household's neighborhood peers (those neighbors on the household's residential block or adjacent blocks).[3] By assuming that a household chooses a neighborhood in which to live, but does not or cannot choose the precise block, we can then say that block peers, conditional on neighborhood peers, are as if randomly assigned. We take this clean identification strategy to our detailed dataset and examine three decisions made over the life of the mortgage: whether to choose an ARM versus a FRM when purchasing, whether or not to refinance, and, conditional on refinancing, whether or not to choose an ARM or FRM. Our predictions are predicated on the idea that social interactions must occur between households for our hypothesized effects to become economically important. This implies that households are not initially influenced by the households on their block, but become so over time, presumably after they have interacted with those around them.

We find, consistent with our predictions, that the ARM choice of households making purchase loans is not influenced by the decisions of others on their block. This result is also important because it attenuates the possibility that households endogenously choose specific blocks based on matching to their new block peers. However, decisions made after the households have lived there, and social interactions are more likely to have occurred, are socially influenced. Specifically, we find that house are nearly 10% more likely to refinance and are 1.5% more likely to choose an ARM when refinancing if the share of their block-neighbors who have recently refinanced or who have an ARM increases by ten percentage points, respectively. We further leverage the richness of our dataset to provide an even more conservative test by focusing on households who move to new neighborhoods and then refinance their purchase loans. As predicted, the type of purchase loan households choose is not socially influenced, but the decisions to refinance and whether or not to choose an ARM when refinancing (made on average 1.9 years after the initial purchase loan decision) are significantly influenced by the decisions of their block peers.

Another implication of social interactions driving correlated mortgage decisions is that households who have limited social interactions with their block peers, regardless of the time spent owning the property, should not be socially influenced. To test this, we look at households who own second and third properties they do not occupy. Consistent with our social influence effect, non-occupant owners' choices of whether or not to refinance and whether to choose an

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[3] See Bayer, Ross, and Topa (2008) for the introduction of this method to the literature and Bayer, Mangum, and Roberts (2016) for an example of this strategy being used in real estate decisions in Los Angeles.

ARM or FRM when refinancing are not socially influenced by the households around the second home. Social interactions driving mortgage decisions, though, would predict that the investor-owners' decisions are influenced by the people they do socially interact with, specifically, the hyperlocal block peers at their primary residence. We document that this is indeed the case: when refinancing mortgages on their second and third properties, non-occupant owners' ARM decisions are socially influenced by the hyperlocal block peers at their primary residence.

Our paper makes a clear contribution to three areas of research. First, our work adds to the small literature examining the role of peers and social influence in household real estate decisions. Bailey, Cao, Kuchler, and Stroebel (2016) find that social influence from Facebook friends has important consequences on real estate purchasing decisions. Bayer, Mangum, and Roberts (2016) and Gupta (2016) find that nearby households play a role in the decision to purchase and default, respectively. Maturana and Nickerson (2016) use a sample of teachers in Texas to investigate the influence that co-workers have on refinance decisions. They find that teachers who are randomly assigned the same off-period during the day make decisions more like each other than teachers with different off-periods. Our paper brings to this literature a study that uses a universal sample of homeowners, a previously unexplored mortgage decision, and a previously unexplored peer group. It is important to note that each of these papers, ours included, uses just a subset of the households' social network and therefore produces conservative estimates of the true social influence effect. Thus, taken together, we believe that the role of social influence is likely larger than any of the estimates produced by extant research. Future work will need to find ways of simultaneously considering the influence of co-workers, family, friends, and neighbors to uncover the magnitude of the entire social influence effect.

We also add to a growing body of empirical work documenting various drivers of household mortgage decisions. DeFusco (2016), Demyanyk and Loutskina (2015), Fuster and Vickery (2014), and Di Maggio, Kermani, Korgaonkar (2015) provide evidence for the importance of government regulation and deregulation in mortgage decisions. Another line of research has concluded that lending standards affect household mortgage decisions (Agarwal, Amromin, Ben-David, Evanoff, 2015; Dell'Arriccia, Igan, Laeven, 2012; Mian and Sufi, 2009). Moreover, competition in the lender market can also play an important role (Amromin and Kearns, 2014; Scharfstein and Sundaram, 2014). Finally, other work has focused on individuals, finding that households' decisions are influenced by their expectations (Adelino, Schoar, and Severino (2016)), lack of education and understanding (Agarwal, Ben-David, and Yao (2014); Keys, Pope, and Pope (2014)), financial situation (Bhutta, Dokko, and Shan (2016); Palmer (2016), and strategic considerations (Mayer, Morrison, Piskorski, and Gupta (2014)). Our work documents that social influence, and more specifically the influence of neighbors, is yet another important factor for understanding how households make mortgage decisions.

Finally, our work contributes to the larger peer effects literature. Social influence is recognized as a critical factor in many important consumption and labor decisions (Bayer, Ross, and Topa (2008); Bollinger and Gillingham (2012); Goolsbee and Klenow (2002); Grinblatt, Keloharju and Ikäheimo (2008); Iyer and Puri (2012); McShane, Bradlow and Berger (2012)). It is important to note that much of the previous work investigates the peer effects in contexts where decisions are easily observable. For example, Grinblatt, Keloharju and Ikäheimo (2008) and McShane, Bradlow and Berger (2012) document the impact that peers have on new car purchases. Bollinger and Gillingham (2012) document that social interaction (peer) effects are important in the diffusion of solar photovoltaic panels. In both of these examples, the choices made are publicly observable—individuals can see which car is parked in a neighbor's driveway or whether a neighboring roof contains solar panels. We add to this literature a demonstration of the importance of peers even in cases where decisions are not publicly observable or identifiable. Our work suggests that peers are not a source of information due exclusively to visual salience but influence decision-making in ways consistent with word-of-mouth effects.

The rest of the article is organized as follows. Section II introduces our baseline identification strategy and empirical approach. In Section III we describe the data sources and sample construction as well as the main summary statistics. Section IV displays the empirical results identifying the importance of hyperlocal block peers on household mortgage decisions. In Section V, we look at the aggregate implications of our results. Section VI concludes.

2 Data

2.1 Data Sources

Our database of mortgage transactions was originally compiled and published by DataQuick. This dataset is now available for purchase at the University Data Portal by CoreLogic. The data, acquired from mortgage deeds published by local tax assessor's office, is at the mortgage level and reports, for each loan, the names of the buyers and sellers, the date of the transfer, the type of transaction (purchase loan or refinance), the loan value, whether the interest rate on the loan is fixed or adjustable, the lender, and the latitude and longitude of the property. We use the latitude and the longitude to precisely map every mortgage transaction and outstanding loan in Los Angeles County. This allows us to define, for every household, the decisions their neighbors have recently made and the terms and lenders of their neighbors' outstanding mortgages. For our smallest geographic area, we use census blocks. Census blocks are roughly similar to city blocks and are populated by an average of twenty-five households.

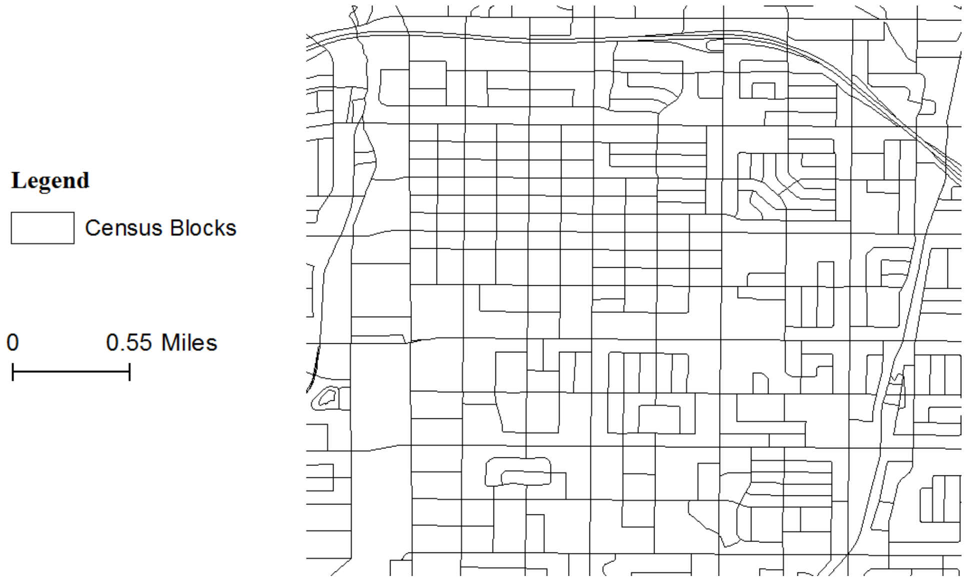

FIGURE 1: CENSUS BLOCK DELINEATIONS IN LOS ANGELES. This figure presents census blocks in a northern part of Los Angeles. Each census block corresponds to a city block and is populated by an average of twenty five households. For more information on how census blocks are constructed see https://www2.census.gov/geo/pdfs/reference/GARM/Ch11GARM.pdf. These TIGER/Line shapefiles can be downloaded here, https://www.census.gov/geo/maps-data/data/tiger-line.html.

The Home Mortgage Disclosure Act (HMDA) data is a separate mortgage level database that lists all mortgage applications made to qualifying lending institutions. This dataset includes information about the loan, including its purpose (purchase or refinance), dollar amount, the census tract of the underlying property, the year of the application, and whether or not it was approved. The dataset also details information on the applicants, specifically their race, sex, and income. 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.

2.2 Descriptions and Summary Statistics of the Main Sample

We merge these the two mortgage level datasets and the bridge file to create a rich, detailed dataset of all the mortgage transactions that take place in Los Angeles County. We focus on one area to ensure that recorded transactions use consistent coding rules. We chose Los Angeles as the focus sample of study for three reasons. First, Los Angeles is the second-largest city in the United States, and has a diverse ethnic and racial population. Second, Los Angeles is an important world economic center. And third, on a pragmatic level, Los Angeles was the largest city with long panel data and reliable, non-missing variables. We focus our attention on the time period between 2008 and 2011. We specifically use this period in order to avoid the bubble period and subsequent collapse of 2002 – 2007, given that a bubble is too anomalous a setting to confidently identify the importance and ubiquity of social influence in household mortgage decisions. We believe that future work should separately try to understand how the bubble and social influence were shaped by each other.

The goal of our study is to identify social influence in *household* mortgage decisions. We are consequently interested only in the decisions made by household borrowers as opposed to institutions or real estate investors. We consequently drop properties owned or being purchased by institutions (trusts, banks, business, and government and nonprofit organizations). We also drop properties that are simultaneously held with two or more other properties by the same owner. Our assumption is that owners owning more than three properties at one time are not what we are thinking of when we say household.[4] We also drop purchase mortgages where the house involved sells for less than $1 or the transaction is tagged as one not at arms-length. Observations that are missing key variables – lender code, date, location – are also dropped. The final sample is summarized in Table 1.

[TABLE 1 HERE]

Approximately 80% of the loans in our sample are refinance loans and 20% have adjustable interest rates.[5] The average loan amount is approximately $330,000, which, for purchase loans, translates to an average loan to value (LTV) of about 70%. Reassuringly, the median LTV is 80%, the cutoff above which many lenders require the borrower to purchase mortgage insurance. Activity is slower in 2008 and 2009 but roughly consistent during the entire time period. In Panel B of Table 1, we see that about half of all mortgage loans are made by banks and a quarter by mortgage companies. Our sample contains more than 2500 distinct lending institutions, the largest of whom is Wells Fargo who accounts for just 13% of lending activity.

In Panel C of Table 1 we present information on characteristics of the households. More than half of our loans have a one-to-one match in HMDA. We use a conservative matching algorithm and so we have a great deal of confidence in the loans that are matched. The income, race, and ethnicity variables come from HMDA.[6] Distance to property is the distance between the location of the property and the location of the tax address (the location where the property tax bill will be sent) of the borrower. In some cases, this occurs when a household is moving to a new home and uses their old address as the tax address. In others it is because the property in question is the borrower's second home.[7] In the majority of cases this value is less than just a

[4]This group of real estate buyers is interesting in its own right and the focus of work by Bayer, Mangum, and Roberts (2015), but is outside the scope of the current project.
[5] This is a heavier weighting to refinancing than normal due to the depressed housing market. Between 1990 and 2012 in Los Angeles, 70% of mortgage transactions were refinances.
[6] These variables serve as important controls, but our results are consistent across the sample of DataQuick mortgage data that does not match to HMDA.
[7] This definition is used to define households purchasing investment properties by Chinco and Mayer (2016) and DeFusco, Nathanson, and Zwick (2016)

few miles as households move locally from one part of Los Angeles to another or are purchasing investment properties near to them. For almost all refinances, the distance is 0 as the refinancer also lives in the property. Cases where the value is non-zero are second homes.

Our second sample uses the data from the first sample and fills out the panel for every quarter between 2008Q1 and 2011Q4. We are left with a panel of more than 1,000,000 outstanding mortgages. To ensure the most complete sample we go back to 1990, the beginning of coverage in DataQuick. Therefore, for each observation, unique at the property by quarter level, we know details of the outstanding mortgage loan, the borrowers, and whether or not they refinance so long as the owner of the property moved in after 1990 (18 years before the time period on which we estimate our models). The characteristics of the outstanding loans are very similar to those presented in table 1. What we gain in this panel data is a household by quarter dummy variable indicating if the outstanding loan was refinanced or not. See more details of the characteristics of the outstanding loans in Table 2.

[TABLE 2 HERE]

2.3 Measuring Neighbor Decisions

To identify social influence effects on a household we must construct measures of its neighbors' activities. In our first tests we e a household's propensity to choose an ARM or FRM as a function of the ARM choice its neighbors have made. For each census black by quarter we define the share of outstanding loans that are ARMs. For example, if a block in Los Angeles has 36 outstanding loans and 9 of them are ARMs, then the share of outstanding mortgages that are ARMs is 1/4. The variation in ARM share across census blocks is economically large. Some blocks have very low levels of ARM share while in other areas more than half of the outstanding mortgages are adjustable rate loans. The measure we use for peer refinancing is, because of the different nature of the problem, constructed differently. We look at the same geographic levels as before and count the number of peer loans that are refinanced and divide that by the number of peer loans outstanding. We are careful not to include the household itself in these rates. So the proportion of peer households that have refinanced in the last time period are *household dependent*. The peer refinancing rate over the sample is consistently between approximately 1% and 3%.

3 Strategies for Identifying Social Influence

3.1 Explaining the Plausibility of the Identifying Assumption

Identifying social influence effects is difficult. Households in similar stages of life, with similar incomes, using the same institutions, and facing the same market conditions will make similar decisions. And since households choose where to live based, in large part, on the people who will be living near them and the characteristics of the neighborhood there will be correlations between the mortgage decisions made by households and the decisions made by their neighbors. In short, there are two problems. First, similar households choose to live together and similar households make similar mortgage decisions (endogenous group formation). Second, households living in the same neighborhood share all the characteristics of that neighborhood and common shocks to that neighborhood, some of which will remain unobserved to the econometrician (correlated unobservables). A random assignment of households to peer groups fixes this problem.

The key assumption in our methodology is that households may choose particular neighborhoods to live in, but are less likely to choose (or even be able to choose) a specific block. If true, then conditional on its neighborhood peers, a household's block peers are quasi-randomly assigned. Throughout this paper we will refer to the two areas of interest as blocks (census blocks) and neighborhoods (census block *and* the census blocks adjacent to it). Figure 1 presents an example of our identification strategy. Also, we call the two peer groups block peers (households that live on the same census block) and neighborhood peers (households that live in the same neighborhood). Our strategy uses within-neighborhood variation in activity to identify a block peer effect.

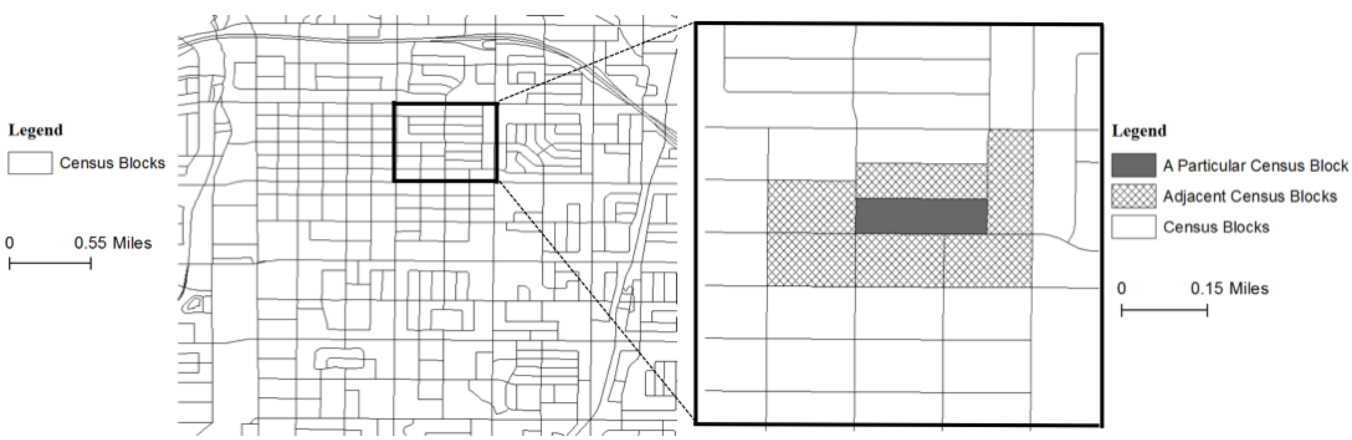

FIGURE 2: CENSUS BLOCK DELINEATIONS AND ADJACENT POLYGON NEIGHBORHOOD EXAMPLE IN LOS ANGELES. This left side of this figure presents census blocks in a northern part of Los Angeles. Los Angeles census blocks correspond closely to city blocks and are inhabited by an average of 25 households. This right side of this figure presents a zoomed in view of the census block and the adjacent census blocks. Combined, these make up a neighborhood. The average neighborhood in Los Angeles is made up of 9 blocks.

Our strategy is only possible because of our precisely geolocated data. Using the latitude and longitude of the property we are able to map every mortgage transaction to its census block. Our final dataset allows us to map, for example, Figure 3. In this case, we observe information about the outstanding loan for every property, specfcially whether the loan has an adjustable or fixed interest rate.

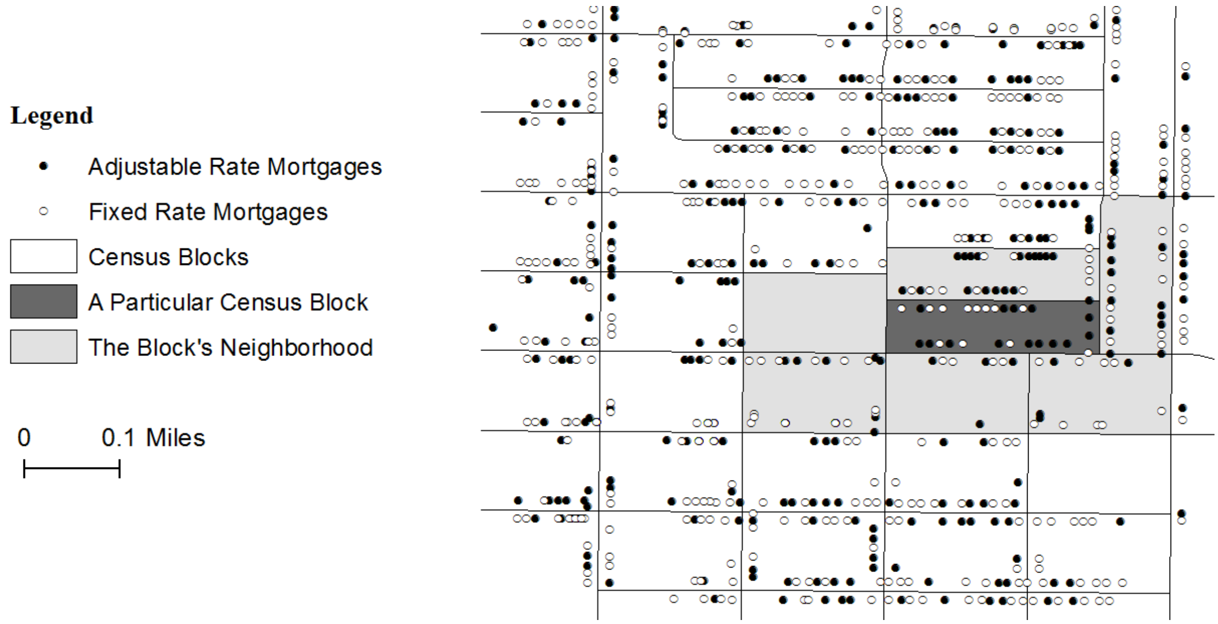

FIGURE 3: ADJACENT POLYGON NEIGHBORHOOD EXAMPLE. This figure presents a census block and the adjacent census blocks. Combined, these make up a neighborhood. The average neighborhood in Los Angeles is made up of 9 blocks. Each circle corresponds to a mortgage loan. Solid circles represent ARMs while unfilled circles represent FRMs, We draw a similar map for all of Los Angeles County and ask if households look even more like their black neighbors than their neighborhood noighbors. We take the robust result that they do as evidence of a social influence effect.

Our assumption means that, conditional on neighborhood, block peers are randomly assigned and consequently that endogenous group formation and correlated unobservables no longer bias estimations.[8] There are at least three reasons to assume the assumption is valid. First, households might be indifferent between properties located on adjacent blocks. Second, property availability constraints mean that buyers might not have the option to live on a specific block in their desired neighborhood. Third, as seen in Table 3, purchasing houleholds are no more similar to the block neighbors than they are to their adjacent-block neighbors in any meaningful sense.

[TABLE 3 HERE]

There are no perfect assumption validity tests, but we use the available data to try and confirm that our assumption is reasonable. In Table 3, we look at every house purchased with a mortgage between 2008Q1 – 2011Q4, and then compute the absolute value of the difference between the purchase loan / purchased property and the average of other houses on the block. Next, we compute the same difference but between the purchase loan and the average of houses in the adjacent blocks (*not* including the block itself, so that the two groups, block peers and

[8] It is this conditional-on-neighborhood-then-as-if-random observation that Bayer, Ross, and Topa (2008) made which enabled their design of this powerful identification strategy.

adjacent block peers, are mutually exclusive.). These absolute values are averaged over all purchase loans and presented in columns 1 and 2. The third column presents the difference between these two values. A negative value means the house is less different from its block than its neighborhood. The fourth column tests whether this difference in differences is statistically different from zero. The difference in differences are not economically meaningful. I.e., households are no more different from their neighborhood peers than their block peers. This evidence supports the assumption that a household, conditional on choosing a certain neighborhood, is randomly assigned a block and thus randomly assigned a block peer group.

In the event that households choose larger areas of interest first and then begin looking for specific households our assumption is especially valid. For example, households might choose where to live based on a close proximity to work or belonging to a particular school district. In these case, block peers and neighborhood peers will both be randomly assigned. We therefore view our working definition of neighborhood (block plus only adjacent blocks) as the location that households are non-randomly choosing as very conservative.

3.2 Describing Our Baseline Specifications

Our baseline strategy estimates the following linear probability model,

$$y_{it} = \alpha + \beta_1 * blockshare_i + \beta_2 * neighborhoodshare_i + \delta * \boldsymbol{X_i} + \tau_t + \varepsilon_{it} \qquad (1)$$

where y_{it} is the mortgage decision, always a binary outcome, made by household *i* at time *t*. Our parameter of interest is β_1 which is defined as the share of households on the block that have made the given decision (e.g., choose an ARM, refinance). The key control variable is *neighborhoodshare* which is defined as the share of households in the neighborhood that have made a given decision. Since the block peer decisions are included in the *neighborhoodshare* variable, the parameter β_1 picks up the outsized effect of block peers. And since block peers are ranom conditional on neighborhood peers, blockshare is random conditional on neighborhoodshare. We therefore consider a positive β_1 as evidence of hyper local social influence effects. We vary the controls across specifications. But often included are borrower- and loan-level controls, denoted $\boldsymbol{X_i}$, and time fixed effects, denoted γ_t. We also include geographic-level fixed effects which will be discussed in detail in the results section. We use the user create command *reghdfe* for estimation (Correia, 2016).

The bulk of our analysis uses equation (1) on the sample of refinance loans. We use refinance loans and not purchase loans because refinance loans are made by households who have lived on the census block and are thus ‘at risk’ for social influence from their block peers. Purchase loans will prove useful in placebo and falsification tests, in ways further described below. If block peers are perfectly randomly assigned conditional on neighborhood peers, then our models cleanly identify a peer effect. But since we cannot be certain of the complete validity of our assumption we use two subsamples, namely, households moving to new neighborhoods and households that own but do not occupy second and third homes, to provide compelling supportive evidence. Analysis of each subsample provides evidence consistent with our hypothesis – that there exists a social influence effect operating over short distances.

Movers allow our research design to test two specific predictions consistent with our overall hypothesis. The first is that movers, when making purchase loans, do not behave like their new neighbors. Movers have not yet had meaningful interactions with their new block peers, and so have yet to be socially influenced by them. We expect that the neighborhood effects, estimated by β_2, will be significant because of endogenous group formation and correlated unobservables, but not necessarily social interaction. On the other hand, β_1, which estimates only a hyperlocal social influence effect will not be relevant for movers. The second prediction is that households that move will behave like their new peers after they have lived there and had time to interact and be socially influenced. This test is particularly compelling because many of the unobserved household characteristics are now no longer a concern. Households wealthy enough to purchase second homes, whether for investment or consumption, are different than the average American household. But they provide a useful group for testing for local social influence effects. Specifically, if households own second homes but do not occupy those houses, then their mortgage decisions should be unaffected by a local social influence effect.

Our empirical strategy can be summarized as follows. Households do not randomly choose where they live and therefore do not randomly choose their peers. However, conditional on narrowly defined neighborhoods, the specific block households live on look as if randomly assigned. This allows us to use a strategy that estimates peer effects by modelling household mortgage decisions as a function of their block peers' decisions while controlling for their neighborhood peers' decisions. We use households moving to new neighborhoods and properties owned by non-occupants as compelling falsification and placebo tests. However, there always remain lingering endogeneity concerns that the best empirical identification strategies cannot fully handle.

3.3 Using Laboratory Experiments for Causal Inference

We use a variety of laboratory experiments to reduce these concerns. Specifically, it is possible that our identified social influence effects could be due in part to unobservable correlations or sorting within neighborhoods. Experimentally manipulating peers serves to causally test our hypothesis in a way that would prove exceptionally challenging using conventional econometric techniques. In order to test our effect, we ran a pilot experiment using sixty people, randomly varying their existing peer group as well as, at least in some cases, their new peer group.

Participants were assigned one of two rooms of individuals. In all cases, participants were brought into a room of between 5-7 people and were told that they would be involved in a choice task where they would be asked to choose which mortgage option that they would prefer (FRM versus ARM) based on current mortgage rates and risks. They were told to read information regarding these two mortgage options and choose the option they wanted (FRM versus ARM). Participant's choices would then be recorded and the proportion of those choosing each option would be revealed at the end of each choice trial to all individuals in the room (i.e., one's peers). However, these proportions were randomly varied as a function of the experiment. In one room, it was revealed that between 72.4-84.8% chose a FRM; in the other room, it was revealed that this same majority percentage chose an ARM. After making their first decision, individuals were told that they would be making the ARM versus FRM mortgage decision in nine future periods/trials, being informed after each trial about the majority choosing a FRM (ARM).

In addition to exogenously varying information regarding what their nearby peers in their particular room have chosen, we also sought to replicate our moving result. We did this by splitting participants into one of three 'moving' treatment groups and then varying a) whether they switched rooms during their mortgage choice selection and b) at what point they switched rooms. Therefore, our three moving treatment groups were as follows: Participants either stayed in the same room for all ten sessions, switched rooms after two trials (i.e., they were exposed to one peer group and then moved to a different room with a new peer group that they spent a majority of their decision time with), or they switched after eight trials (i.e., they spent a majority of their time with the first peer group and moved later on to a new peer group). Our hypothesis was that participants who stay in the same place will conform to their peers the most. Participants who switch early (after trial two) should be more likely to conform to their new room (i.e., look like their new peers--replicating our earlier refinancing result). Participants that switch rooms much later are the least likely to conform with their new peers.

4 Results

In this draft, the bulk of interpretation and discussion is in the tables section. Click the links to read the detailed analysis and interpretation of each table. The (BACK) links next to the table title return to the body of the text.

4.1 Adjustable or Fixed

First, we explore the household decision to pick an adjustable or fixed interest rate mortgage.

[TABLE 4 HERE]

We build the model, as before, by adding neighborhood share and then more restrictive fixed effects. Across specifications (2) through (5) our main result is remarkably consistent. We use specification (3) as our preferred specification since the inclusion of the geography fixed effects do not meaningfully change the results but are incredibly restrictive, especially in the following subsample analyses. As before we test on the sample of refinance loans. Specification (3) estimates that increasing the share of block peers with adjustable rate mortgages by ten percentage points increases the average household's propensity of choosing an adjustable rate by 1.48%.

[TABLE 5 HERE]

In Table 5 we include demographic controls. The results from specification (1) to (2) are economically unchanged. In specifications (3) and (4) we estimate our model not with refinance loans, but with purchase loans. As expected, there are no significant block peer social interaction effects. DataQuick loans with HMDA matches, and therefore demographic information, are systematically different than those without matches. We therefore prefer specifications (1) and (3), which not only demonstrate the presence of social influence effects in refinances and not purchases, but also show a strikingly consistent neighborhood effect. This neighborhood effect is likely attributable to such features as loan availability and pricing and local macroeconomic conditions.

[TABLE 6 HERE]

In Table 6, we focus on our first subsample, non-occupant owners. Specifically, we compare those that do not occupy their property and those that do. The first group is owners whose mailing address zip code is different than the zip code of the property. For the second group, these zip codes are the same. We find that, when refinancing, non-occupant owners are completely unaffected by a hyper local social influence while the owner occupants are.

[TABLE 7 HERE]

In table 7, we ask if these non-occupant owners are influenced by the neighbors around their primary residence. It is these peers that the household most likely interacts with and is potentially socially influenced by. What we find is compelling. Borrowers are influenced by their hyperlocal peers even when making decisions about an investment property or second home in a completely different neighborhood. This result cannot be explained by the any of the alternative explanations that relate to local area effects like lender advertising.

[TABLE 8 HERE]

Finally, in table 8, we explore the set of households that (1) use a mailing zip code different from the property zip code when purchasing and (2) later refinance that loan. Some of these borrowers remain non-occupants, but many, when refinancing, now use the same mailing zip code as the property. We find that when purchasing there is no hyperlocal social influence effect nor is there a social influence for households that refinance as non-occupants. But for those households who moved to the property, hyperlocal social influence effects matter.

4.2 Refinance or Not

The third household decision where we test for the existence of social influence effects is the household's decision to refinance or not. We use our panel of households over time and model the probability that a household refinances in a given quarter on the share of their peers who have refinanced at some point in the previous six months. We present our key results in Table 9.

[TABLE 9 HERE]

We find that households are significantly more likely to refinance if their hyper local peers have recently refinanced – this effect over and above the effect at the neighborhood level. Specifically, we find that increasing the of block peers with adjustable rate mortgages by ten percentage points increases the average household's propensity of choosing an adjustable rate by 9.83%.

There is a concern that, because household lender choice is affected by peers, that this is just a follow-up lender effect and not a social influence effect. We deal with this problem in table 10 specifications (3) and (4) by including just those households whose outstanding loans were originated by a lender that originated more than 50,000 loans (the 18 largest lenders).

[TABLE 10 HERE]

We then include 17 lender fixed effects in the model and the results remain unchanged. We take this as very strong evidence against the lender effect story. As before, we look at non-occupant households for falsification.

[TABLE 11 HERE]

In table 18, we find that non-occupant refinance decisions are completely unaffected by the recent refinance decisions of the households living on the same block as the investment property / second or third home.

4.3 Results from the Lab

In the lab, we find preliminary evidence demonstrating that participants who stayed in the same room or stayed in their first room for a majority of the time were significantly more likely to select the same mortgage as their peers from this first room (FRM in Room A, ARM in Room B). In contrast, participants who switched early were more likely to conform to the majority choice of their new peers in later trials (trial six and onwards) than those of their first peer group. By randomly assigning peer group, peer choice, and even varying moving/switching within the trial period, we find early causal evidence for social influence effects on mortgage decisions in even a laboratory setting.

4.4 Overall Analysis of Results

We explore three different household mortgage decisions – ARM or FRM when purchasing, whether or not to refinance and ARM or FRM when refinancing. Our results confirm the hypothesis that households are socially influenced by their neighbors. Importantly, our estimates can only identify the social influence effects of block neighbors that occur over and above the effects of neighborhood neighbors. That is, included in our estimates of β_2 are social influence effects which our tests cannot identify. Therefore, our estimates of the size of social influence effects from β_1 are a lower bound of the true social influence effect from neighbors.

We use our rich dataset to explore several other groups of homeowners. The first group we look at is households taking out purchase loans. As new arrivals to the block, these households have not yet been subject to any social influence effects from their neighbors. We confirm this hypothesis by using our same strategy and documenting no cases of social influence effects. Next we focus on two special groups: households moving to new neighborhoods and borrowers who own but do not occupy second and third homes. We find that these households behave in all the ways we would expect. The movers do not originally behave like their neighbors, but then over time do behave like them. The investor-owned properties never behave like the households around the property, but the owner is influenced by the neighbors around his primary residence.

5 Robustness and Supporting Results

5.1 Alternative Definitions of Neighborhoods

We replicate all of our results using census block groups, as opposed to adjacent census blocks, as our measure of neighborhood. Census block groups are the next smallest geographic areas defined by the Census. They contain an average of ten blocks and 200 households. The assumption and identification strategy are the same as before. The assumption is that a household's block peers, conditional on its block group peers, are randomly assigned. And our model estimates how much more similar the decisions households make are to their block peers' decisions than to their block group peers' decisions. We replicate all of our key results using these definition of neighborhood. The drawback of this measure is that it is less likely that a household is as likely to choose any block in the block group as another block, weakening our identification assumption. The strength is from a modelling perspective: the block group fixed effects now has a very clear and intuitive economic meaning.

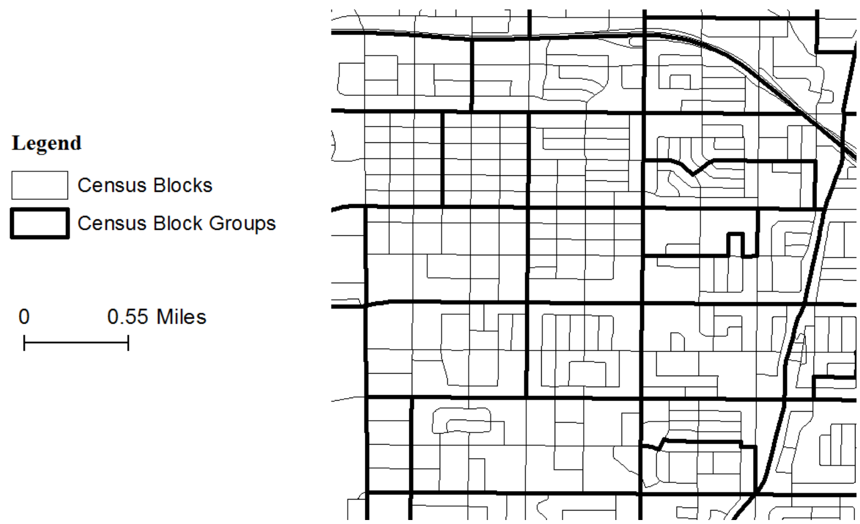

FIGURE 4: CENSUS BLOCK GROUP DELINEATIONS IN LOS ANGELES. This figure presents census blocks in a northern part of Los Angeles. Outline in a thick, black border are census block groups, the second smallest geographic delineation used by the Census.

Note that we use census block group fixed effects as important controls in some specifications in our primary results. However, we choose to use the block plus adjacent blocks as our preferred measure of neighborhood for two key reasons. First, using adjacent block neighborhoods removes the artificial census block group border. That is, we want to define a block that is adjacent to another block in a different census group as being in the block's neighborhood. Second, it is a more conservative definition. It might be the case that a household, when picking where to live, does prefer one part of the census block group to another, weakening our assumption. By defining a neighborhood as just a block and the adjacent blocks we make a stronger case for our assumption. The benefit of using census block groups as the larger area definition is that including census block group fixed effects becomes exceptionally powerful. For this reason, we replicate and present in the paper our key tests using this measure of neighborhood. These results can be found in Appendix A. Reassuringly, the results are robust to how we define neighborhood.

There is a remaining concern that using the census block as our smallest measure means that households directly across the street are not considered block peers, but only neighborhood peers. To be clear, this makes identifying our results harder as any social influence effects coming from these across-the-street peers are being absorbed in β_2. We solve this problem by defining hyper-local and local peers in one final way. For a random sample of households we define hyper-local households as those within .05 miles and local, neighborhood households as those within .2 miles. We then replicate our results using this measure. Defining these measures is computationally intensive, so we do so only a small sample. But even on this small sample, our results are again confirmed. These results are available upon request.

5.2 Measuring Community Involvement

We also consider how to incorporate the idea that some areas have more community involvement than others. This idea has been used effectively to provide evidence of social influence effects (see, e.g., Hong, Kubik, and Stein (2004)). Unfortunately, our identification strategy cannot incorporate measures of community involvement.

Our identification strategy identifies social influence effects by documenting differences in the effects from people living on the same block and people living in the same, slightly larger geographic region. We call the effects from these two groups β_1 and β_2, respectively. The strength of our strategy is that β_2 absorbs the effects of endogenous group formation and correlated unobservables. The weakness is that β_2 also absorbs any social influence effects that come from those neighbors who live nearby, but not on the same block. This weakness precludes using measures of community involvement to say something about β_1 for two reasons.

First, measures of community involvement would likely, but not necessarily, increase our estimates of β_1 and β_2. For examples, consider that having more churches causes there to be more local social interactions. Even if this is the case, we are unable to disentangle the increase in β_2 that comes from more social influence and the increase that comes from more churches being correlated with other measures like similar educations, similar life plans, the same access to local banks, and use of the same real estate agents. Another way to think about this problem is to consider what kind of measure of community would be required. We would need some measure or instrument that meant stronger community ties between people living on the same block, without also meaning stronger community ties between people living on adjacent block. Finding such a measure and using it say something about social influence effects seems a promising idea for future work.

The second problem is that these measures of community involvement do not have clean theoretical predictions. Our current hypothesis does. Social influence matters, people talk marginally more with people who live marginally closer, and we can therefore identify social influence effects by comparing people who live closer with those who live slightly farther away but are subject to the same neighborhood considerations. But when we look at the community measures we fall short of a clean theoretical prediction. Consider using the racial homogeneity of a neighborhood as a measure of higher community involvement. It might be the case that more homogenous areas have stronger ties as neighbors are more likely to socially interact. But it could also be the opposite, that those types of households that choose to live in racially diverse neighborhoods are more likely to be the types of people to value spending time and forming meaningful connections with their neighbors. Most measures of community involvement suffer from this same theoretical ambiguity problem. While incorporating measures of community involvement like presence of churches or racial homogeneity is initially promising, the design of our identification strategy and community measures' unclear theoretical predictions precludes our using them.

6 Social Welfare Implications and Potential Transmission Mechanisms

The identification of social influence effects naturally leads to two important questions. The first is whether their existence is social welfare increasing or decreasing and the second is what the underlying mechanisms are. While a rigorous answer to either is outside the scope of this project our results can shed some early light on answers to both.

The short answer to the first question – are social influence effects good or bad – is that it depends. Our results demonstrate that social influence effects are important in a variety of household mortgage decisions. If households are being influenced to borrow too aggressively then overall social welfare likely declines as these households are more likely to end up far underwater. If households, in talking with their neighbors, become aware that they can refinance their loans with lower interest rates than social welfare likely increases as their spending can increase. Whether or not social influence has a positive net effect will be challenging to answer and is likely best approached by analyzing a single important decision or time period at a time, and analyzing it in depth. We leave this to future research.

When it comes to why social influence effects matter, 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 an adjustable rate mortgage makes sense for them or that refinancing could save them money. An effect called social learning. 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 a fixed rate mortgage if their peers do because their peers can then help them refinance optimally. 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 these effects to future work. We can use the results from our lab experiments to shed some early light.

7 Conclusion

Our analysis shows that households making mortgage decisions are socially influenced by the decisions of their neighbors. We use household mortgage data, precisely geolocated real estate data, and a recently developed research design to identify the existence of social influence effects in three important household mortgage choices. Households are 9% more likely to choose an adjustable rate mortgage (ARM) and 13.5% more likely to refinance if the share of their block-neighbors with an ARM or who have recently refinanced increases from the 10^{th} percentile to the 90^{th} percentile, respectively. Non-owner-occupied households are never influenced by the hyperlocal block peers in their secondary residence, but are in fact influenced by the hyperlocal block peers in their primary residence. Moving households are not initially affected by their new neighbors but are socially influenced by their hyperlocal peers during the refinance decision. Finally, we complement our empirical strategies in the lab by experimentally assigning peers and peer decisions in a variety of ways. These findings contribute to the literature on the role of peer and neighborhood effects in consumption decisions. Importantly, it is worth nothing that we believe these percentages represent the lower bound of the importance of social influence effects. We account only for one's hyperlocal peers in our investigation, though there are undoubtedly other sources of peer influence.

Identifying the influence of peer effects and social influence in household mortgage decision-making has important implications both theoretically and substantively. Theoretically, we apply a new methodology that aids in establishing an issue of first order importance to the fundamental understanding of what drives household decisions. This methodology exploits plausibly exogenous variation in peer decisions across various types of mortgage decisions. Substantively, understanding how social influence can impact household mortgage decisions provides insight into an issue fundamental to consumer welfare. Most homebuyers are unable to buy homes outright, creating a need for mortgages and the financial intermediaries that offer them. Across the United States, nearly 50 million owner occupied, mortgaged households[9] owe a combined \$10 trillion in mortgage debt.[10] With thousands of lenders[11] offering fixed rate mortgages (FRMs) and adjustable rate mortgages (ARMs), households must decide what type of loan to apply for and where. And after making it through the loan origination process, households must everyday incorporate new information and choose whether or not to refinance. Poor lender choice can cost borrowers their livelihoods and neighborhoods their stability (Engel

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[9] Accessed September 21, 2016: http://factfinder.census.gov/faces/tableservices/jsf/pages/productview.xhtml?src=bkmk.

[10] Accessed September 21, 2016: https://www.federalreserve.gov/econresdata/releases/mortoutstand/current.htm

[11] Between 1990 and 2014, the Home Mortgage Disclosure Act data reports 23,406 unique mortgage lenders operating in the United States.

and McCoy, 2007; Agarwal et al., 2014). Keys, Pope, and Pope (2014) found that a fifth of households for whom refinancing was profitable failed to do so and cost themselves more than $10,000 each. Based on our results, we can conclude that social influence might be driving individuals who may not have considered the refinancing decision to consider doing so, thereby saving consumers money in the long term. Alternatively, there may be areas where few households refinance because few of their peers have recently refinanced, creating a cycle of under refinancing.

Future research should investigate the conditions in which peer effects and social influence may improve mortgage decisions and benefit consumers and also conditions where peer effects could hurt mortgage decisions and increase suboptimal behavior. Future work should also examine the role of expertise. Does financial expertise buffer individuals from the negative consequences of social influence? Or might it decrease the benefits that social influence may afford? Are unsophisticated households more influenced by nearby experts and can nearby experts play an important role in improving social welfare? By establishing the existence and importance of social influence in household mortgage decision making, we provide an important stepping stone and open the door for a number of areas of inquiry.

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TABLE 1: SUMMARY STATISTICS ON MORTGAGE LOANS ORIGINATED IN LOS ANGELES BETWEEN 2008 AND 2011 (BACK)

	Mean	Std. dev.	Median	N
Panel A: Loan Characteristics				
Refinance (=1)	80.2%	39.9%		548,437
Adjustable Rate Mortgage (=1)	18.4%	38.8%		548,437
Loan Amount	329,485	256,280	291,830	548,437
Loan to Value (if purchase loan)	72.0%	30.7%	80.0%	108,793
Transaction Year 2008 (=1)	22.5%	41.7%		548,437
Transaction Year 2009 (=1)	24.0%	42.7%		548,437
Transaction Year 2010 (=1)	26.8%	44.3%		548,437
Transaction Year 2011 (=1)	26.7%	44.2%		548,437
Panel B: Lender Characteristics				
Bank Lender (=1)	49.7%	50.0%		548,437
Credit Union Lender (=1)	5.9%	23.5%		548,437
Mortgage Company Lender (=1)	22.6%	41.8%		548,437
Panel C: Borrower Characteristics				
Matched to HMDA loan (=1)	58.6%	49.3%		548,437
Distance to Property (miles)	0.36	2.68	0.00	548,211
Co-applicant (=1)	49.8%	50.0%		548,437
Applicant Income (1,000s)	141.26	213.07	101.00	298,179
Race, Asian (=1)	14.0%	34.7%		321,122
Race, Black (=1)	3.9%	19.3%		321,122
Race, White (=1)	60.3%	48.9%		321,122
Ethnicity, Hispanic (=1)	18.4%	38.7%		321,122

Sample: The sample starts with all mortgage loans originated between 2008Q1 and 2011Q4 in Los Angeles. We then follow Bayer Mangum, and Roberts (2016) by dropping observations where the property securing the loan is a condominium or was divided into smaller properties and resold. We also drop if the transaction was flagged as not at arms-length, if the house sold more than once in a single day, or if the sale price was less than $1. To ensure a reasonable panel, we either drop refinances that took place within 3 months of the previous refinance by that owner at that property or combine the information from the two transactions. We further assume that refinances that occurred within 90 days of the purchase loan were more likely to be piggy back loans and are treated as such in the sample. Next, we use an adjusted version of the name scrubbing algorithm used by Bayer, Mangum, and Roberts (2016) to tag those borrowers that are individuals as opposed to trusts or businesses. We also drop any borrowers that concurrently hold four or more properties. This removes professional investors whom this paper is not about. It also removes borrowers with common names giving us confidence that within the sample observations with same-named borrowers are indeed mortgages held by the same borrower. Finally, we drop observations that are missing key information like the location of the property, the name of the buyer, or the lender, or the amount of the loan. It is this sample, and subsamples of this sample, that we will use throughout the paper, except when noted otherwise.

Key Variables: *Refinance* means the loan is a refinance as opposed to a purchase loan. *Adjustable rate mortgages* are defined as those with adjustable or graduated interest rates; all mortgages in the final sample have either adjustable or fixed interest rates. The *loan-to-value* variable is only defined for purchase loans with a known sale price. The *bank, credit union*, and *mortgage company* lender tags are defined by dataquick. *Matched to HMDA* equals 1 if the dataquick loan has a unique match in HMDA. *Distance to property* is the distance between the mailing address of the borrower and the property securing the mortgage. *Co-applicant* indicates that there are two people on the mortgage contract. The *income, race*, and *ethnicity* variables are from HMDA.

TABLE 2. CHARACTERISTICS OF THE REFINANCE HOUSEHOLDS (BACK)

	Mean	Std. dev.	Median	N
Panel A: 2008Q1				
Refinance (=1)	2.8%			1,396,845
Outstanding Loan is an ARM (=1)	43.9%			747,935
Outstanding Loan is a Refinance (=1)	80%			1,383,939
Quarters Since Last Refinance/Purchase	17.49	17.55	11.00	1,383,939
Co-applicant (=1)	47.6%			1,396,845
Applicant Income (1,000s)	125.39	169.44	95.00	620,228
Distance to Property (miles)	6.90	103.07	0.00	44,096
Race, White (=1)	50.6%			676,405
Panel B: 2009Q4				
Refinance (=1)	1.8%			1,402,941
Outstanding Loan is an ARM (=1)	43.8%			750,791
Outstanding Loan is a Refinance (=1)	80%			1,388,308
Quarters Since Last Refinance/Purchase	22.02	18.39	16.00	1,388,308
Co-applicant (=1)	47.2%			1,402,941
Applicant Income (1,000s)	125.49	174.84	94.00	646,540
Distance to Property (miles)	7.96	113.47	0.00	33,972
Race, White (=1)	51.8%			699,940
Panel A: 2011Q4				
Refinance (=1)	2.9%			1,414,764
Outstanding Loan is an ARM (=1)	41.3%			758,484
Outstanding Loan is a Refinance (=1)	79%			1,398,463
Quarters Since Last Refinance/Purchase	26.30	20.23	22.00	1,398,463
Co-applicant (=1)	46.8%			1,414,764
Applicant Income (1,000s)	125.41	174.05	94.00	683,375
Distance to Property (miles)	8.55	117.52	0.00	48,689
Race, White (=1)	53.5%			734,977

Sample: The sample is all households with outstanding loans owned by people (as opposed to trusts or businesses) who own no more than three properties. Loans originated before 1992 are not included in the sample as data collection efforts had not yet begun. Each panel looks at a different cross section of all outstanding loans as of the first day of that quarter. Some of the older loans are missing the interest rate type variable which means we cannot determine if the outstanding loan is an adjustable or fixed rate mortgage. Approximately half of the loans do not have HMDA matches and so demographic variables are missing.

Key Variables: *Refinance* is a dummy equal to one if the outstanding loan was refinanced that quarter. *Quarters since last Refinance/Purchase* is the number of quarters since the outstanding loan was originated. *Distance to Property* is the distance between the address of the owner of the property and the property conditional on a refinance occurring that quarter.

TABLE 3: VALIDITY OF BLOCK VERSUS ADJACENT BLOCK NEIGHBORS ASSUMPTION (BACK)

	Average Absolute Value of Difference Between Purchased Household & Block	Average Absolute Value of Difference Between Purchased Household & Adjacent Blocks	Difference in Differences	T-Test on Difference	N
Panel A: Buyer Characteristics					
Co-applicants (=1)	0.479	0.482	-0.003	-6.51	83,990
Income (1,000s USD)	64.849	62.126	2.723	13.14	60,390
Race, white (=1)	0.460	0.464	-0.004	-6.77	74,534
Second home (=1)	0.024	0.024	0.001	5.19	83,990
Panel B: Property Characteristics					
Year Built (years)	8.02	10.20	-2.179	-87.07	83,207
Square Feet	531	599	-67.752	-30.87	83,990
2012 Assessed Value (USD)	191,916.30	198,852.10	-6,935.77	-11.26	83,990

Sample: The sample consists of every purchase loan in our main sample compared with all outstanding mortgages and properties in the sample. We compute the absolute value of the difference between the purchase loan / purchased property and the average of other houses on the block. We then compute the same difference but between the purchase loan and the average of houses in the adjacent blocks (those houses in the same neighborhood, but *not* on the same block). These absolute values are averaged over all purchase loans and presented in columns 1 and 2. The third column presents the difference between these two values. A negative value means the house is less different from its block than its adjacent blocks. The fourth column tests whether this difference in differences is statistically different from zero. To be clear, the two groups, block peers and adjacent block peers, are mutually exclusive.

Key Variables: The *year built* is the year that the house was constructed – largely between 1950 and 2010. The *2012 Assessed Value* is the value used to calculate the amount of property tax owed by the household. The average assessed value of a home purchased between 2008 and 2011 is just $358,000.

Interpretation: The difference in differences between a purchased home and the other homes on its block and a purchased home and the other homes on the adjacent blocks are not economically meaningful. This evidence supports the assumption that a household, conditional on choosing a certain census area, is randomly assigned a block. We will use both definitions of neighborhood – census block group and adjacent blocks – throughout the paper.

TABLE 4. CHOOSING AN ARM WHEN REFINANCING AS A FUNCTION OF THE SHARE OF PEERS WITH ARMS (BACK)

	dependent variable: new mortgage is an ARM (=1)				
	(1)	(2)	(3)	(4)	(5)
sample	*owner-occupied refinances*				
Share of all outstanding block loans with adjustable rates	0.277***	0.0275***	0.0273***	0.0160**	0.0194***
	(45.04)	(3.94)	(3.99)	(2.39)	(2.65)
Share of all outstanding neighborhood loans with adjustable rates		0.796***	0.577***	0.965***	0.0588**
		(56.840)	(37.660)	(56.000)	(2.420)
Bank Lender	0.196***	0.189***	0.168***	0.186***	0.163***
	(125.380)	(122.290)	(107.980)	(117.070)	(88.060)
Credit Union Lender	0.242***	0.237***	0.215***	0.242***	0.215***
	(76.660)	(75.490)	(68.770)	(75.900)	(61.050)
Mortgage Company Lender	0.0307***	0.0252***	0.0226***	0.0234***	0.0195***
	(19.590)	(15.990)	(13.960)	(14.390)	(10.010)
Outstanding Loan is an ARM (=1)	0.102***	0.0923***	0.0667***	0.0853***	0.0611***
	(63.670)	(58.690)	(42.750)	(54.400)	(35.090)
Co-applicants (=1)	-0.0109***	-0.00996***	-0.00278**	-0.00882***	-0.00216
	(-7.87)	(-7.24)	(-2.08)	(-6.25)	(-1.40)
Quarter Fixed Effects			Y		
Census Group Fixed Effects				Y	
Quarter-by-Group Fixed Effects					Y
N	338,042	338,042	338,042	337,905	319,507

Sample: The sample consists of all the refinance loans from the sample detailed in Table 1 with the further restriction that the mailing address zip code of the borrower and the zip code of the property securing the loan are the same. This further restriction means that the sample used here only includes households who live at or very near the property and are therefore subject to a social influence effect from the households living around the property.

Models: Linear probability models. The dependent variable is a dummy equal to 1 if the loan is an ARM. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively. Standard errors are clustered at the census block level.

Key Variables: The *Share of all outstanding block loans with adjustable rates* is defined as the number of outstanding mortgage loans on the block that have adjustable rates divided by the total number of outstanding mortgage loans. The *Share of all outstanding neighborhood loans with adjustable rates* is defined as the number of outstanding mortgage loans in the neighborhood with adjustable interest rates divided by the total number of outstanding mortgage loans in the neighborhood. Note that the block loans are included in the neighborhood loans.

Interpretation: In this and the following tests we explore the hypothesis that households are more likely to choose adjustable rate mortgages if their neighbors have adjustable rate mortgages. We include two key variables: neighborhood share as a control and block share as the variable of interest. As discussed at length in Table 4, we include progressively more restrictive fixed effects and find a consistent effect coming from block peers in models (2) - (5). This effect is a peer effect over and above neighborhood share effects and is consistent with a social influence that operates at hyper local levels. Using our preferred specification, (3), we find that increasing the share of block peers with adjustable rate mortgages by ten percentage points increases the average household's propensity of choosing an adjustable rate by 1.48%. [.27 percentage points increase from a base probability of 18.4% (from table 1)].

TABLE 5. CHOOSING AN ARM AS A FUNCTION OF THE SHARE OF PEERS WITH ARMS (BACK)

	dependent variable: new mortgage is an ARM (=1)			
	(1)	(2)	(3)	(4)
sample	*owner-occupied refinances*		*purchase loans*	
Share of all outstanding block loans with adjustable rates	0.0273***	0.0221***	0.00734	0.0145
	(3.99)	(2.79)	(0.84)	(1.30)
Share of all outstanding neighborhood loans with adjustable rates	0.577***	0.481***	0.575***	0.604***
	(37.660)	(25.800)	(29.630)	(21.990)
Bank Lender	0.168***	0.107***	0.117***	0.0663***
	(107.980)	(58.180)	(54.200)	(23.870)
Credit Union Lender	0.215***	0.0628***	0.119***	0.0814***
	(68.770)	(19.610)	(15.080)	(8.710)
Mortgage Company Lender	0.0226***	0.0205***	0.0380***	0.0213***
	(13.960)	(10.910)	(23.310)	(8.370)
Outstanding Loan is an ARM (=1)	0.0667***	0.0694***		
	(42.750)	(37.180)		
Co-applicants (=1)	-0.00278**	-0.00599***	0.00208	-0.0105***
	(-2.08)	(-3.63)	(1.170)	(-4.30)
Quarter Fixed Effects	Y	Y	Y	Y
Income Quartile Controls		Y		Y
Race and Ethnicity Controls		Y		Y
N	338,042	185,652	108,541	62,348

Sample: In models (1) and (2), the sample consists of all refinance loans from the sample detailed in Table 1 with the further restriction that the mailing address zip code of the borrower and the zip code of the property securing the loan are the same. In models (3) and (4), the sample is all purchase loans detailed in Table 1.

Models: Linear probability models. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively. Standard errors are clustered at the census block level. Model (1) is identical to model (3) in the previous table.

Key Variables: New to this table are the income quartile controls, there are 4, and race and ethnicity controls. These variables are defined in table 1. The sample falls dramatically as we are only able to confidently match approximately half of the loans in the CoreLogic sample to HMDA.

Interpretation: This table provides two key insights. The first is that, in model (2), the inclusion of more household level controls does not qualitatively change our results. The second is that, as before, households taking our purchase loans, who have therefore not been lived there and been exposed to their neighbors, are not affected by any hyper local social influence. Households moving to a new area have not yet socially interacted with the inhabitants and so are not influenced by their decisions. This result is consistent with a social influence effect that operates at local level. Note that, as before, households taking out purchase loans do behave similarly to other borrowers in the neighborhood. This is because of a number of larger area-level effects that influence ARM choice including, for example, availability, advertising, and prices.

TABLE 6. CHOOSING AN ARM, COMPARING NON-OCCUPANT OWNERS AND OCCUPANT OWNERS (BACK)

	dependent variable: new mortgage is an ARM (=1)			
	(1)	(2)	(3)	(4)
sample	*refinances by non-occupant owners*		*refinances by occupant owners*	
Share of all outstanding block loans with adjustable rates	0.00548	-0.0862*	0.0273***	0.0221***
	(0.14)	(-1.95)	(3.99)	(2.79)
Share of all outstanding neighborhood loans with adjustable rates	0.122	0.383***	0.577***	0.481***
	(1.520)	(4.010)	(37.660)	(25.800)
Bank Lender	0.173***	0.0341**	0.168***	0.107***
	(19.420)	(2.460)	(107.980)	(58.180)
Credit Union Lender	0.184***	-0.00138	0.215***	0.0628***
	(4.440)	(-0.04)	(68.770)	(19.610)
Mortgage Company Lender	0.0629***	0.0143	0.0226***	0.0205***
	(5.720)	(0.880)	(13.960)	(10.910)
Outstanding Loan is an ARM (=1)	0.103***	0.0858***	0.0667***	0.0694***
	(9.480)	(6.320)	(42.750)	(37.180)
Co-applicants (=1)	-0.0191**	-0.0128	-0.00278**	-0.00599***
	(-2.16)	(-1.27)	(-2.08)	(-3.63)
Quarter Fixed Effects	Y	Y	Y	Y
Income Quartile Controls		Y		Y
Race and Ethnicity Controls		Y		Y
N	6,699	3,311	338,042	185,652

Sample: In models (1) and (2), the sample consists of all refinance loans from the sample detailed in Table 1 with the further restriction that the mailing address zip code of the borrower and the zip code of the property securing the loan are *not* the same. In models (3) and (4), we require that the two zip codes be the same. In this way we compare households that do not occupy the property with those that do.

Models: Linear probability models. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively. Standard errors are clustered at the census block level.

Interpretation: We find that households refinancing mortgage loans secured by properties they do not occupy, i.e., second or third homes or investment properties, are not significantly affected by a hyper local social influence. Households who do not live in the area interact less with the inhabitants and so are not influenced by their decisions. This result is consistent with a social influence effect that operates at local level.

TABLE 7. CHOOSING AN ARM, ARE NON-OCCUPANT OWNERS AFFECTED BY THEIR ACTUAL NEIGHBORS? (BACK)

	dependent variable: new mortgage is an ARM (=1)			
	(1)	(2)	(3)	(4)
sample	*refinances by non-occupant owners*			
Share of all outstanding block loans on owner's block with adjustable rates	0.115***	0.0720+	0.115***	0.0719+
	(2.78)	(1.53)	(2.78)	(1.53)
Share of all outstanding neighborhood loans in owner's group with adjustable rates	0.0455	0.298***	0.0398	0.260***
	(0.550)	(3.290)	(0.470)	(2.900)
Share of all outstanding block loans on property's block with adjustable rates			0.00334	-0.0899**
			(0.090)	(-2.04)
Share of all outstanding neighborhood loans in property's neighborhood with adjustable rates			0.094	0.317***
			(1.150)	(3.350)
Bank Lender	0.175***	0.0357***	0.174***	0.0352**
	(19.750)	(2.590)	(19.560)	(2.540)
Credit Union Lender	0.197***	0.00115	0.187***	-0.000487
	(4.730)	(0.030)	(4.530)	(-0.01)
Mortgage Company Lender	0.0655***	0.0146	0.0650***	0.0151
	(5.960)	(0.900)	(5.900)	(0.930)
Outstanding Loan is an ARM (=1)	0.104***	0.0844***	0.102***	0.0849***
	(9.560)	(6.250)	(9.350)	(6.260)
Co-applicants (=1)	-0.0198**	-0.0134	-0.0189**	-0.0117
	(-2.25)	(-1.33)	(-2.14)	(-1.16)
Quarter Fixed Effects	Y	Y	Y	Y
Income Quartile Controls		Y		Y
Race and Ethnicity Controls		Y		Y
N	6,734	3,314	6,694	3,308

Sample: The sample consists of all refinance loans from the sample detailed in Table 1 with the further restriction that the mailing address zip code of the borrower and the zip code of the property securing the loan are not the same.

Model: Linear probability models. Coefficients significant at the 15%, 10%, 5%, and 1% levels are marked with a +, *, **, and ***, respectively. Standard errors are clustered at the census block level.

Key Variables: The *Share of block loans around the owner's block* looks at the block of the non-occupant owner's primary residence. This captures the share of mortgages held by his hyperlocal peers that are ARMs.

Interpretation: In the previous table, we find that households who are refinancing a mortgage loans on secured by properties they do not occupy, i.e., second homes or investment properties, are not significantly affected by a hyper local social influence from the people that live immediately around the property. In this table we ask if these non-occupant owners behave like the households that live around their primary residence, where they *do* actually live. We find that households do behave like their neighbors, even when making decisions about mortgages on properties not in the neighborhood. This result is weakened somewhat by the inclusion of the demographic controls, but we attribute some of this to the size of the sample. In models (3) and (4), we include the shares of ARMs in the area around the property, as well as the area around the primary residence. Our results are unchanged. Households behave like their hyperlocal peers, even when making decisions on properties not near those peers.

TABLE 8. TESTING FOR SOCIAL INFLUENCE IN BORROWERS WHO MOVE TO A NEW PROPERTY (BACK)

	dependent variable: new mortgage is an ARM (=1)		
	(1)	(2)	(3)
sample	*purchase loans with different site and mailing zip codes*	*refinances by borrowers who continued to have different zips*	*refinances by those who used the same zip code for the refinance*
Share of all outstanding block loans with adjustable rates	0.0178	0.00989	0.0356*
	(0.75)	(0.23)	(1.92)
Share of all outstanding neighborhood loans with adjustable rates	0.752***	0.519***	0.477***
	(17.770)	(6.710)	(13.750)
Bank Lender	0.245***	0.363***	0.305***
	(32.110)	(26.680)	(52.240)
Credit Union Lender	0.0872***	0.278***	0.196***
	(3.010)	(7.390)	(13.780)
Mortgage Company Lender	0.102***	0.108***	0.0757***
	(15.230)	(7.570)	(11.470)
Outstanding Loan is an ARM (=1)		0.160***	0.127***
		(14.060)	(26.590)
Co-applicants (=1)	-0.0556***	-0.0194*	-0.0243***
	(-9.84)	(-1.90)	(-5.60)
Quarter Fixed Effects	Y	Y	Y
N	25,978	7,799	42,266

Sample: This sample consists of all borrowers who made purchase loans sometime between 2002Q1 and 2011Q4 and then later refinanced that purchase loan. The longer time period is necessary to do this test because the sample of households that moved in and refinanced between 2008Q1 and 2011Q4 is too small. All those loans being refinanced in models (2) and (3) are refinanced purchase loans used to estimate (1). I.e., the average purchase loan (of which there are 25,978) made in this sample is refinanced approximately 2 times (7,799 + 42,266).

Models: Linear probability models. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively. Standard errors are clustered at the census block level.

Interpretation: As before, we find that households taking out purchase loans are not affected by the choices of households who live near the property. They have not lived near them before and are therefore not socially influenced by them. In this case we require that the mailing zip and site zip of the purchase loan borrower are different. We then follow the households that later refinance – some of whom were investors and remained non-occupants (model 2), but many others of whom did move to the property (model 3). We find that those did not move were not affected by the people living around the property but those who did move, and were therefore more likely to interact with these new neighbors, were socially influenced. This result also demonstrates that our previous results were not simply picking up some differences between purchase loans and refinance loans or the types of people that refinance as compared to the types of people that do not (and therefore only appear in the purchase sample).

TABLE 9: HOUSEHOLD PROPENSITY TO REFINANCE THEIR MORTGAGE IF THEIR NEIGHBORS RECENTLY HAVE (BACK)

	dependent variable: household refinanced this quarter (=1)				
	(1)	(2)	(3)	(4)	(5)
sample			*all households*		
Share of all block loans that have refinanced in the last 2 quarters	0.136***	0.0290***	0.0285***	0.0217***	0.0260***
	(88.84)	(16.50)	(16.24)	(12.30)	(14.57)
Share of all neighborhood loans that have refinanced in the last 2 quarters		0.370***	0.424***	0.139***	0.153***
		(109.870)	(116.950)	(35.720)	(26.220)
Outstanding Loan in an ARM	-0.00789***	-0.00778***	-0.00806***	-0.00786***	-0.00807***
	(-64.87)	(-63.82)	(-66.29)	(-63.38)	(-65.26)
Outstanding Loan is a Refinance	-0.795***	-0.794***	-0.791***	-0.793***	-0.788***
	(-146.62)	(-146.77)	(-146.68)	(-149.94)	(-148.33)
Co-applicants (=1)	0.00529***	0.00427***	0.00409***	0.00316***	0.00327***
	(45.760)	(36.840)	(35.550)	(26.100)	(27.110)
Quarters Since Last Transaction FE	Y	Y	Y	Y	Y
Previous Lender Type FE	Y	Y	Y	Y	Y
Quarter Fixed Effects			Y		
Census Group Fixed Effects				Y	
Quarter-by-Group Fixed Effects					Y
N	10,985,347	10,985,347	10,985,347	10,985,345	10,984,645

Sample: The sample is that described in Tables 2 and 3.

Models: Linear probability models. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively. Standard errors are clustered at the household level.

Interpretation: The key refinancing result. A household is more likely to refinance as the share of its block neighbors who have recently refinanced increases, controlling for the share of larger neighborhood peer households who have recently refinanced. This result is robust to time, area, and time-by-area fixed effects. As before, we adopt model (3) as our preferred specification. We find that increasing the of block peers with adjustable rate mortgages by ten percentage points increases the average household's propensity of choosing an adjustable rate by 9.83%. [.285 percentage points increase from a base probability of 2.9% (from table 1)].

TABLE 10: HOUSEHOLD PROPENSITY TO REFINANCE, INCLUDING LENDER FIXED EFFECTS (BACK)

	dependent variable: household refinanced this quarter (=1)			
	(1)	(2)	(3)	(4)
sample	*all households*			
Share of all block loans that have refinanced in the last 2 quarters	0.0285***	0.0307***	0.0281***	0.0324***
	(16.24)	(10.80)	(11.54)	(8.16)
Share of all neighborhood loans that have refinanced in the last 2 quarters	0.424***	0.457***	0.405***	0.457***
	(116.950)	(78.260)	(79.770)	(55.610)
Outstanding Loan in an ARM	-0.00806***	-0.0124***	0.00273***	0.00181***
	(-66.29)	(-62.04)	(15.750)	(6.210)
Outstanding Loan is a Refinance	-0.791***	-0.845***	-0.705***	-0.825***
	(-146.68)	(-165.50)	(-87.55)	(-96.69)
Co-applicants (=1)	0.00409***	0.00589***	0.00418***	0.00815***
	(35.550)	(30.950)	(25.660)	(29.800)
Quarters Since Last Transaction FE	Y	Y	Y	Y
Previous Lender Type FE	Y	Y		
Originator of Outstanding Loan FE			Y	Y
Quarter Fixed Effects	Y	Y	Y	Y
Race & Income Controls		Y		Y
N	10,985,347	4,768,915	5,582,289	2,421,770

Sample: The sample is that described in Tables 2 and 3.

Models: Linear probability models. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively. Standard errors are clustered at the household level.

Key Variables: In models (3) and (4) we include 18 *Originator of Outstanding Loan Fixed Effects* for the 18 lenders in our sample with more than 50,000 mortgage originations.

Interpretation: The key refinancing result is robust to controlling demographic controls. In specifications (3) and (4) we include 18 lender fixed effects on a sample that includes only those 18 lenders with more than 50,000 mortgage originations. The results remain unchanged. This result allows us to rule out a lender fixed effect story wherein households make similar decisions to their neighbors only because they share the same lender. If anything, the social influence effect becomes stronger after including the specific lender fixed effect.

TABLE 11: HOUSEHOLD REFINANCE, FALSIFICATION TEST USING NON-OCCUPANT HOUSEHOLDS (BACK)

	dependent variable: household refinanced this quarter (=1)				
	(1)	(2)	(3)	(4)	(5)
sample	*non-occupant owned households*				
Share of all block loans that have refinanced in the last 2 quarters	0.0359***	0.00632	0.00656	0.00000326	0.00181
	(5.94)	(0.92)	(0.96)	(0.00)	(0.22)
Share of all neighborhood loans that have refinanced in the last 2 quarters		0.109***	0.0954***	0.120***	0.0233
		(7.870)	(6.530)	(7.200)	(0.900)
Outstanding Loan in an ARM	-0.00547***	-0.00540***	-0.00558***	-0.00538***	-0.00542***
	(-8.20)	(-8.09)	(-8.35)	(-6.53)	(-6.98)
Outstanding Loan is a Refinance	-0.657***	-0.657***	-0.656***	-0.702***	-0.648***
	(-20.14)	(-20.14)	(-20.16)	(-27.67)	(-20.00)
Co-applicants (=1)	0.00188***	0.00172***	0.00184***	0.00167**	0.00196***
	(3.290)	(3.020)	(3.250)	(2.330)	(2.760)
Quarters Since Last Transaction FE	Y	Y	Y	Y	Y
Previous Lender Type FE	Y	Y	Y	Y	Y
Quarter Fixed Effects			Y		
Census Group Fixed Effects				Y	
Quarter-by-Group Fixed Effects					Y
N	326,544	326,544	326,544	326,501	306,528

Sample: The sample is that described in Tables 2 and 3, except with the added restriction that the mailing address zip code on the outstanding loan and the refinance loan are not the same as the property's zip code. In this way, we can look at the sample of households that are not owner occupied.

Models: Linear probability models. Coefficients significant at the 10%, 5%, and 1% levels are marked with a *, **, and ***, respectively. Standard errors are clustered at the household level.

Interpretation: A household that does not live at the property is not at all affected by the decisions of the households that live around the property. This result provides further evidence for a social influence effect operating at a hyper local.