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Foreclosure Contagion and the Neighborhood Spillover Effects of Mortgage Defaults

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ABSTRACT

In this paper, I identify shocks to interest rates resulting from two administrative details in adjustable-rate mortgage contract terms: the choice of financial index and the choice of lookback period. I find that a 1 percentage point increase in interest rate at the time of adjustable-rate mortgage (ARM) reset results in a 2.5 percentage increase in the probability of foreclosure in the following year, and that each foreclosure filing leads to an additional 0.3 to 0.6 completed foreclosures within a 0.10-mile radius. In explaining this result, I emphasize price effects, bank-supply responses, and borrower responses arising from peer effects.

OVER 4 MILLION COMPLETED FORECLOSURES TOOK place between January 2007 and December 2010, and another 8.2 million foreclosures were initiated during the same period (Blomquist (2012)). While the foreclosure crisis remains historic in its aggregate cost to homeowners and investors, understanding the precise mechanisms and channels behind this wave of mortgage defaults remains a challenge. In this paper, I examine the role of neighborhood spillover effects from foreclosures as an important amplification mechanism behind this foreclosure crisis.

There are several plausible mechanisms through which foreclosures can affect the default patterns of their geographical peers. First, foreclosures reduce the market price of neighboring homes, which may induce those borrowers to default due to the negative equity that results. Alternatively, lenders may deny refinancing opportunities to prospective lenders from areas that have previously experienced foreclosure activity. A separate possible channel relies on the role of information. This channel operates through borrowers' reassessment of the costs of default after exposure to neighboring foreclosures,

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or through their reassessment of the stigma associated with mortgage nonpayment (as emphasized, for instance, in Guiso, Sapienza, and Zingales (2013)). Finally, a direct treatment effect involves foreclosures leading to an increase in crime, vandalization, and other forms of property depreciation that reduce the amenity value of the neighborhood.[1] Though the price effects of foreclosures have been previously studied,[2] we know comparatively little about the causal implications of foreclosure activity on neighborhood default behavior or about the precise mechanisms through which such peer effects operate.

The key contribution of this paper is the development of an empirical setting that allows for the causal estimation of foreclosure spillovers. Understanding foreclosure externalities is important to understanding the seemingly snowballing wave of foreclosures observed during the period of initial subprime mortgage defaults and the ensuing financial crisis, as well as the policy intervention designed to combat these spillovers. For instance, as Timothy Geithner argued in a speech in February 2009 introducing the Financial Stability Plan,[3] "As house prices fall, demand for housing will increase, and conditions will ultimately find a new balance. But now, we risk an intensifying spiral in which lenders foreclose, pushing house prices lower and reducing the value of household savings, and making it harder for all families to refinance." My work examines the role of foreclosure externalities and the extent to which they constitute an amplification mechanism that potentially motivates these federal housing relief efforts.

A central econometric challenge to understanding foreclosure contagion is the issue of reflexivity and the endogenous assignment of default, as emphasized by Manski (1993). On the one hand, observing that foreclosures appear to be geographically clustered is consistent with geographically proximate borrowers suffering a common shock (for instance, a local plant closure). On the other hand, clustering of foreclosures may be due to the fact that geographically proximate borrowers share common (possibly unobservable) characteristics that predict mortgage default.[4] Understanding the contribution of foreclosures to the default behavior of neighboring properties has thus proven to be a key challenge in prior literature on this subject.

To address this econometric issue, I introduce a novel instrument based on exogenous shocks to interest rates on adjustable-rate mortgage (ARM) loans. I argue that while these shocks impact the foreclosure of resetting households, they affect the default choices of neighboring properties only through the channel of default on treated households. ARMs in the United States—which were quite common among subprime and jumbo-prime borrowers during the boom

[1] For instance, see Immergluck and Smith (2005, 2006) on crime and local amenities.

[2] See, for instance, Campbell, Giglio, and Pathak (2011) or Mian, Sufi, and Trebbi (2015).

[3] This speech laid the groundwork for the Home Affordable Modification Program (HAMP) and a variety of other government programs. See http://www.treasury.gov/press-center/press-releases/Pages/tg18.aspx.

[4] For example, this seems to be the case among foreclosure completions in Phoenix, as shown in Internet Appendix Figure IA3. The Internet Appendix may be found in the online version of this article.

in house price appreciation—are characterized by an initial teaser rate that resets to a market interest rate (plus a margin term to account for risk) after an initial period that typically lasts two, three, or five years.[5] I focus on two previously unexplored aspects of the mechanics according to which ARMs reset: the choice of financial index and the choice of lookback date. When ARMs reset, the market interest rate component of the new payment is derived from the prevailing market interest rate according to an index (typically LIBOR or Treasury), taken a certain number of (lookback) days from the reset date. Importantly for my analysis, these interest rates are then fixed for a period of time between 6 and 12 months after initial reset.

I find variation in these contract terms that drives borrower payment amounts subsequent to mortgage reset. Focusing on the choice of financial index, I find that while LIBOR and Treasury rates tracked each other quite closely prior to the financial crisis, a large spread between rates emerged during the financial crisis that resulted in large differences in payments among borrowers linked to different indices. LIBOR borrowers resetting in January 2009, for instance, paid on average \$11,000 more than otherwise identical Treasury borrowers who reset the same month. Next, I find that substantial interday volatility in interest rates led to variation in payments paid by borrowers with different lookback terms—for instance, 15 lookback days instead of 45. I thus argue that both forms of interest rate variation, which are ultimately determined by administrative details of loan contracts, are unlikely to be related to other aspects of loan performance, and lead to substantial variation in payment terms after reset.

In the first stage of my analysis, I find that the size of the within-month interest rate shock resulting from these contract differences drives default and foreclosure rates among resetting mortgages. The first-stage results are substantial, suggesting that a 100 basis point increase in interest rates corresponds to a roughly 2.5% rise in the probability of experiencing a foreclosure in the subsequent 12 months—a substantial increase relative to a baseline foreclosure rate of 8%. These results are in line with existing work on mortgage resets (see, for instance, Fuster and Willen (2015)). I contribute to this literature by obtaining a tighter empirical setting using the within-month variation in interest rates as a shock to the reset window.

My setting provides a clean form of identification for local foreclosure spillovers. Focusing on a broad sample of resetting ARM holders, I develop a novel merge algorithm that links information on these loans—including their contract terms, credit scores, and loan performance—to deeds records containing precise geographical information based on where the borrowers live. I then construct neighborhoods that consist of all transacting properties within a radius of 0.10 miles around the resetting ARM holder to analyze the spillover effects of default.

I contrast my first-stage results, which use the interest rate variation induced by the idiosyncratic contract terms such as lookback date and index to predict

[5] Initial teaser lengths of 1, 7, or 10 years also exist but are less common.

the default rate among resetting loans, to reduced-form estimates, which use the ARM interest rate shocks to predict the default rates among loans *neighboring* the resetting loans. Since neighboring loans are typically set to very different contract terms, there is no ex ante reason to expect that these exogenous interest rate shocks should predict borrower loan performance in the absence of peer effects or foreclosure externalities. However, my reduced-form estimates suggest that higher within-month interest rates among resetting loans also predict foreclosures and defaults among properties in neighboring areas.

Combining the first-stage and reduced-form estimates, I employ an instrumental variables (IVs) specification in which the interest rate shock of the resetting mortgages serves as an instrument for the foreclosure of the ARM holder. Estimates using both index and lookback forms of interest rate variation point to substantial default spillovers. My preferred specifications suggest that a given foreclosure raises the probability of default among neighbors by around 2.1%—roughly a 66% change to the hazard rate of default. When I aggregate the estimates at a neighborhood level, I find that a given foreclosure leads to an additional 0.3 to 0.6 foreclosures in neighboring areas. Contrasting these estimates with OLS estimates on the change of foreclosures on neighboring default outcomes suggests much larger estimates of around one, which suggests that each foreclosure is associated with an additional foreclosure in neighboring areas. Finding an IV estimate that is much smaller than the OLS estimate suggests that common local exposure to shocks is an important determinant of foreclosure outcomes, but the magnitude and standard errors of my estimate rule out a null hypothesis of zero foreclosure contagion.

Consistent with the idea that my results are driven by active borrower decisions, as opposed to common exposure to local shocks, I find that delinquencies on nonmortgage revolving accounts do not vary around the mortgage delinquency window of the resetting loan. The product-specific delinquency response helps rule out the possibility that borrowers are exposed to an aggregate shock, as opposed to a real estate–centered channel. I also examine the geographical areas most subject to foreclosure spillovers and find that the largest responses are in a very tight radius around resetting properties.

My results are subject to two important caveats. First, I assume that within-month variation in interest rates impact neighboring peers of ARM holders only through the channel of default of the treated properties. This assumption may be violated if, for instance, there is local correlation in contract terms. I test this assumption by focusing on the default responses of neighboring fixed-rate mortgage holders, for whom in principle there should be no contagion of default effects except through exposure to local foreclosures as a result of resetting loan foreclosures.

Second, my results are based on a local average treatment effect (LATE) that is unique to neighborhoods surrounding ARM holders. This is the relevant neighborhood in several contexts, however, particularly in evaluating the impact of changes in interest rates on the default behavior of adjustable-rate loans. This population was also the target of many government relief efforts, including HAMP and Home Affordable Refinance Program (HARP), so understanding default spillovers in this context allows for better evaluation of federal housing policy targeting subprime and other low-income borrowers.

The unusually rich nature of my data set permits me to further analyze the precise mechanisms responsible for mortgage default contagion and to test for confounding factors. I first analyze the impact of the price effect, a key focus of prior literature, and find that sales of transacting properties fall by about 1% to 3% after the foreclosure of a resetting ARM; the results are smaller when I analyze the repeat-sale estimate of the sale price. These estimates are in line with prior literature on the size of pecuniary externalities resulting from foreclosures. Note that, given prevailing estimates in the literature on the elasticity of default with respect to house prices (e.g., Palmer (2015)) suggests that a 10% decrease in house prices leads to a 1% increase in the frequency of default, there is a possible role for channels beyond the house price channel to account for the size of the foreclosure spillovers that I observe.

Complementing the pure price channel, I examine a refinancing channel. I find that refinancing activity drops by one-third among neighboring properties after an instrumented foreclosure. I show that in the aftermath of local foreclosures, borrowers are no less likely to have mortgage inquiries on their credit accounts, which indicates that borrower demand for new credit does not decrease. The large change in access to market credit, therefore, likely reflects a bank supply response whereby banks interpret a nearby foreclosure as an indicator of the creditworthiness of local borrowers (possibly because they use the fire sale price of homes as an input in the property appraisal process) and hence cut access to credit.

I also examine the role of peer effects and borrower responses on mortgage debt. First, I document that foreclosure spillovers are observed in both defaults (as measured by a notice of default or payment delinquency) and foreclosures. Since foreclosure is a joint outcome of decisions made by the borrower and the lender, this finding suggests that borrower responses play a role in foreclosure spillovers. I also document that mortgage defaults pattern among neighboring properties appear to decline in the few quarters prior to foreclosure completion, at which point the mortgage default is highly visible—the borrower leaves the house, which is then typically vacant and subject to depreciation and vandalization. Finally, I find that foreclosure spillover effects are strongest in areas that previously experienced few recent foreclosures. Taken together, these observations are consistent with an information channel of default that relies on borrowers learning about the costs of default in reaction to a visible local foreclosure.

I conclude by examining the broader macroeconomic effect of my sample of foreclosures. First, I examine aggregate effects of census tract-level foreclosure activity on tract-level mortgage denials drawn from Home Mortgage Disclosure Act (HMDA) data. I find strong evidence that local foreclosure activity lowers the supply of new mortgages—both purchase mortgages and refinancings—in nearby areas. In areas hit by foreclosure, lenders are more likely to report that collateral problems contribute to credit denial. While this result is not supported by the same identification result as prior tests above, it does provide valuable context to my results that exogenous shocks to foreclosure activity (i) do not result in a decrease in the demand for credit in local areas (as measured by mortgage inquiries) but (ii) do result in a strong decrease in to refinancing volume in local areas. This overall pattern of results provides novel evidence on the role of a refinancing channel that amplifies the effects of foreclosure activity on local credit availability.

In a final set of tests, I examine the broader consequences of resetting ARM activity. To do so, I create an index of areas ranked by the fraction of mortgages that were resetting under conditions of higher reset interest rates and that were underwater at the time of reset. ARMs facing these conditions defaulted at high rates, as they faced an income shock concurrent with an equity shock that left them with neither a financial incentive to stay in the property nor an ability to refinance into a new mortgage (analogous to the classic "double-trigger" conditions of default). I rank geographical areas by the degree to which they contain borrowers facing the joint shocks of negative equity and rate reset. Under conditions of default externalities, the initial wave of defaults resulting from ARMs may spill over to affect other properties, resulting in an amplification of the initial shock to affect a broader set of properties and borrowers. When I analyze the subsequent macroeconomic performance of areas prone to double-trigger resets, I find evidence of lower home price recovery and greater foreclosures.

In sum, my findings point to the importance of neighborhoods as a source of shock and exposure to information for mortgage holders. While I am not able to fully distinguish between the various channels driving foreclosure contagion, my results provide causal evidence for the spillover effects of foreclosure activity during the Great Recession. In particular, I present novel evidence that the refinancing channel is an important externality of foreclosure activity. These results suggest that foreclosure contagion is an important amplification mechanism during the financial crisis, propagating shocks to household defaults.

My paper relates most strongly to existing studies on the local effects of foreclosures. Many of these papers examine the price impacts of foreclosures, including Harding, Rosenblatt, and Yao (2009), Campbell, Giglio, and Pathak (2011), Anenberg and Kung (2014), and Gerardi et al. (2015). Similar to these papers, I investigate the pecuniary externalities of foreclosures. But distinct from those papers, I additionally investigate alternative channels. My work is also closely linked to papers examining borrower default spillover effects from foreclosure, such as Towe and Lawley (2013), Guren and McQuade (2015), Munroe and Wilse-Samson (2013), Agarwal, Ambose, and Yildirim (2015), and Goodstein et al. (2011). Much of this literature emphasizes the role of foreclosures in aggregating housing downturns through fire-sale externalities and other spillovers. I build on this literature by quantifying one aspect of foreclosure externalities and emphasizing the spillover role that foreclosures have on nearby access to credit.

Some prior studies on foreclosure contagion, such as Towe and Lawley (2013), document that foreclosure rates in a particular state are correlated. My work differs by using an IV approach to obtain a causal interpretation of the contagion effect of foreclosures. Of this literature, the paper nearest to mine in focus is Munroe and Wilse-Samson (2013), who also analyze the contagion effect of foreclosures in the context of an instrument (in their case, the leniency of foreclosure judges). My paper differs by providing broad geographical coverage, rather than limiting attention to one county as in the case of Munroe, which improves the power of my estimates and allows me to rule out a broader set of estimates for the value of foreclosure spillovers. In particular, I am able to rule out null effects for a broader range of contexts. I contrast the quantitative nature of my estimates with those in the literature when discussing my main results below. I also provide novel evidence on the mechanisms behind this result—in particular, the role of nearby refinancing denials.

Additionally, my work relates to a growing literature that examines the consequences of changes in ARM rates, such as Bryne, Kelly, and O'Toole (2017), Di Maggio et al. (2017), and Fuster and Willen (2015). These papers use variation in market interest rates and the contract features of adjustable-rate loans to shed light on how mortgage payments affect household consumption and default choices. My paper extends this literature by providing evidence of cross-sectional variation in interest rates paid by similar borrowers due to novel contract features, and by examining externalities of household foreclosures. Moreover, closely related studies examine other regional consequences of rate reductions (due to mortgage renegotiation or refinancing), such as Agarwal et al. (2015, 2017), Ehrlich and Perry (2015), and Hurst et al. (2016). My work provides complementary analysis showing that payment increases (due to rate resets) contributed to the foreclosure crisis and house price declines, and highlights the contagion channel in propagating foreclosures during the financial crisis. Other related papers include those on peer effects, including in residential mortgages (such as Maturana and Nickerson (2017) and Bailey et al. (2018), which examine peer effects of socially connected but geographically distant ties), corporate defaults (Benmelech at al. (2014)), corporate bankruptcy (Bernstein et al. (2017)), and microfinance debt repayment (Breza (2014)).

The rest of the paper proceeds as follows. Section I introduces the data, provides context on ARMs, and discusses the empirical strategy. Section II provides results on the spillover effects of foreclosures. Section III examines mechanisms for these results, while Section IV considers broader macroeconomic implications. Section V concludes.

I. Data and Empirical Strategy

A. Data

I construct a unique and unusually rich data set for the purpose of this study. I start with an administrative deeds data set from DataQuick, which contains geocodes taken at the address level for all transacted properties in select counties. These data include all properties with a commercial sale, purchase mortgage, or refinanced mortgage over the property's history. Coverage of this data set is close to universal among newly originated mortgages over the sample period of 2000 to 2010, including over 22 million transactions in total. I focus on a set of 54 counties, which cover roughly 40% of the total mortgage market over the period in question. This data set includes transactions-level information on sales, mortgages, and foreclosures on properties. For select counties, the data set also contains information on notices of default (i.e., an indicator of borrowers' failure to make payments).

Using a novel algorithm, I merge these data with BlackBox data on private-label securitized loans, which include loan performance information on default and foreclosure as well as administrative information on loan contract terms, and with credit bureau information from Equifax, which includes information on credit cards, revolving debt, and other mortgage liens. Section A of the Internet Appendix describes each data set and the merging procedure in greater detail. The complete linked data set allows me to observe mortgage transactions, payment decisions, exact interest rates paid, as well as credit scores and other information on precise geographic addresses. The final primary data set includes the most recent open mortgages within a 0.1-mile radius of resetting ARM borrowers, with mortgage data taken from both deeds data and BlackBox data.

B. Introduction to ARMs

The typical ARM in the United States is a hybrid ARM. For such loans, the interest rate is fixed for some initial period—often two, three, or five years—and resets on a predetermined schedule for the remainder of the loan. The most common reset frequencies are six months or one year, although other durations exist. At origination, the borrower agrees upon an initial rate and a series of guidelines that determine all future resets. Interest rates upon reset are based on the cost of funds to the bank, proxied by a financial index (typically either Treasury or LIBOR), plus a fixed margin agreed upon at origination. Resets are often subject to other rules, for example, per-period caps on the size of the rate change.

Though 30-year fixed-rate contracts dominate the U.S. mortgage market, ARMs comprise a substantial share of mortgages and were especially popular in the mid-2000s—particularly among subprime borrowers. Figure IA1, which can be found in the Internet Appendix, illustrates the reset structure of these ARMs. I exploit contract features particular to these types of mortgages in the identification strategy.

Resets on ARMs are commonly thought to have contributed borrower default decisions for both resetting loans and neighboring areas, though establishing the relative importance of reset rate shocks remains a challenge. For instance, Smith, Perwien, and Ratcliffe (2009) survey loan counsellors and emphasize the importance of rate reset shocks in explaining delinquency patterns among ARMs. They quote one counsellor as observing that "Before the resets [the borrowers] were fine. Once the resets kicked in, then they really started having problems making the payments. Without the resets, we probably would not have the volume [of foreclosures] that we have today" (p. 7). Sheila Bair, chairman of the Federal Deposit Insurance Corporation, noted in her Congressional testimony in December 2007 that $367 billion in hybrid loans, or about 1.7 million loans, were scheduled to reset for the first time during 2008 and 2009. As Bair explains, "This wave of mortgage resets, in combination with the decline in home prices and limited refinancing options, could prompt hundreds of thousands of additional mortgage foreclosures over the next two years. These foreclosures will hurt individual borrowers and their communities, as they potentially could place further downward pressure on home values."[6]

A critical aspect of the importance of ARM rate reset is borrower unawareness of or inattention to specific loan contract terms. For instance, Bucks and Pence (2008) document that borrowers often underestimate and fail to understand the nature of the interest rate recast upon reset. This may reflect "rational inattention" whereby the costs of information acquisition regarding precise contract terms are prohibitively expensive relative to the benefits of more accurately forecasting future loan payments. Alternatively, the financial sophistication of borrowers who select or are guided toward ARM products may have been more limited, consistent with a broader literature in household finance that finds that borrowers underestimate interest rates and fail to foresee the resulting financial consequences.[7] Whatever the reason, widespread confusion regarding the precise nature of ARM resets—including the teaser rates, the size of the potential interest rate reset, and prepayment penalties—raises the potential that the timing of ARM reset may represent a particularly important focal period for the timing of mortgage default.

ARM resets have increasingly been the focus of studies of household decision making in the context of interest rate shocks. Fuster and Willen (2015) find that downward interest rate shocks lead to a substantial reduction in the rate of mortgage default. Di Maggio et al. (2017) examine how household consumption responds to changes in monthly payments induced by ARM resets. My work builds on this literature by examining the *within*-month variation in interest rates paid by borrowers upon reset, providing a particularly clean form of identification with which to evaluate the nature of ARM resets. Even if borrowers are not fully attentive to the precise details of ARM reset, some borrowers may respond to the reset by anticipating its effects and defaulting earlier. Using the surprise or within-month variation in interest rates, as induced by variations in index choice or lookback period, provides a more plausibly exogenous form of variation in interest rate shocks.

It is important to keep in mind that ARM reset shocks can either benefit or hurt borrowers depending on prevailing interest rates.[8] Many ARM borrowers resetting in 2009 and 2010, in particular, typically reset to fairly low rates and might well have paid lower monthly mortgage costs relative to fixed-rate

[6] United States Congressional House Financial Services Committee, Hearing on Improving Foreclosure Prevention and Enhancing Enforcement, December 6, 2007, 110th Congress, 1st session, Washington (statement of Sheila C. Bair, Chairman, Federal Deposit Insurance Corporation).

[7] See, for instance, Lusardi and Tufano (2015).

[8] For this reason, ARM contracts may be preferred from a monetary policy perspective in enabling maximum pass-through of interest rates to borrowers as in Auclert (2015).

borrowers who may have been underwater and therefore unable to refinance to take advantage of lower prevailing interest rates. In addition, teaser rates on many ARM products were fairly substantial, and many borrowers of these products were expected to refinance into prime mortgages after building a credit history (see Mayer, Pence, and Sherlund (2009) and Foote, Gerardi, and Willen (2012) for a discussion of the role of ARM resets in driving the financial crisis). My identification strategy does not rely on ARM products being unique drivers of the foreclosure crisis as a whole: Rather, I focus on ARM resets to illustrate the broader transmission mechanism amplifying the effects of defaults through spillover effects.

C. Sample Creation

Figure IA2 in the Internet Appendix illustrates the basics of sample selection. First, I clean and analyze a comprehensive sample of ARMs in my sample of BlackBox private-label securitized loans. For these loans, I observe detailed information regarding the particular contract terms that prevail upon reset, including the date of reset, the precise interest rate upon reset, and the choice of financial index and lookback period. I keep ARMs that were current three months prior to their first reset in my main sample.[9]

Next, I link these loans to the broader Deeds sample, which consists of a comprehensive sample of transacting mortgages in a sample of 54 counties. Importantly for my purposes, the Deeds sample contains the precise address and geocodes of each property in question. This feature of the data enables me to construct neighborhoods of 0.10 miles in radius, as illustrated in Figure IA2 in the Internet Appendix, which define the parameters of my final data selection. I use 0.10 miles both to retain comparability with prior literature, and because it is a tightly defined area around the property that contains approximately 22 local properties.

The empirical strategy requires the computation of distances between households. I use a version of DataQuickDeeds data that include both street addresses and address-level geocodes that include the latitude and longitude of each property. I compute the great-circle distances using a standard Haversine formula.

I keep information on all properties in the radius around resetting ARMs, with data taken at the time of reset of the ARM. In my main analysis, I follow Dahl, Loken, and Mogstad (2014) and analyze neighborhoods with one and only one resetting ARM, but my results generalize to considering properties near multiple ARMs. For my key dependent variable, I analyze the default and foreclosure behavior of properties within two years following reset of the ARM in the center of each circle. In all analyses, I am careful to exclude the resetting ARM itself from the analysis of neighboring outcomes.

I use this basic sample creation procedure to analyze other variables and data sets. When analyzing the price effects of foreclosure, I keep sale data

[9] This happens after a period of two, three, or five years.

among mortgages that sell within a 0.10-mile radius of the resetting ARM. For a select sample of borrowers with private-label securitized loans, I am able to link up information from BlackBox and Equifax. The resulting sample is essentially a subset of the original BlackBox data that links together loans within a 0.10-mile radius. I use this sample to analyze precise information on the payment status of mortgage and revolving debts among neighboring loans.

D. Mechanisms

Why should mortgage activity depend on the behavior of peer borrowers? In this section, I discuss possible channels that I explore further in empirical analysis below.

D.1. Price Effects

A trend in the residential mortgages literature investigates pecuniary externality of foreclosure activity on the price of nearby homes (such as Harding, Rosenblatt, and Yao (2009), Campbell, Giglio, and Pathak (2011), Anenberg and Kung (2014), and Gerardi et al. (2015)). This literature finds that recent foreclosure activity depresses neighboring sale prices by roughly 1%, after adjusting for local trends. This price effect may be the result of a lower amenity value of the neighborhood, due to the visible abandonment of neighboring properties, associated increases in criminal activity (see Cui and Walsh (2015)), changes in neighborhood composition, or similar channels. The resulting price drop lowers the home equity position of neighboring borrowers, and hence makes mortgage default a more attractive proposition, particularly for borrowers already facing a negative equity position.

D.2. Learning Channel

Complementing the home price channel, foreclosures also induce a change in information that affects peer borrowers directly through a variety of channels other than effects on the contemporaneous price. First, individuals may perceive foreclosures as signaling information on *future* expectations of prices. Although evidence suggests that the price impact of foreclosure activity is short term in nature and prices exhibit mean reversion, borrowers may nonetheless perceive from the visible foreclosure of a neighboring property a negative signal about future prices, which is a factor in their default decision.

Second, the learning channel may also affect borrowers through direct peer information effects. Observing a salient nearby foreclosure can inform borrowers about the costs of defaulting, or even potentially about the existence of the default option. The direction of this effect is not obvious ex ante: Borrowers could learn that experiencing foreclosure is quite costly, entailing costs on future access to credit and moving expenses, among other consequences, and hence decide to continue with repayment. Alternatively, borrowers may learn that defaulting is not as costly as previously perceived, as lenders who foreclose do not usually come after borrowers for the difference in value between the mortgage and the recovery sale of the property. In this case, observing the experiences of nearby borrowers going through foreclosure may encourage strategic default.

D.3. Social Stigma

Finally, aside from affecting the information set or market prices observed by borrowers, foreclosures may lower default costs by lowering the social stigma of defaulting. In the context of residential mortgages, prior survey work (such as Guiso, Sapienza, and Zingales (2013)) examines the role of social attitudes toward default in inducing the majority of underwater borrowers to continue mortgage repayment, even though it may not be personally profitable. This literature emphasizes the fragility of social norms regarding debt repayment, which appear to have the potential to deteriorate in the presence of perceived violators of social norms (i.e., additional socially connected strategic defaulters). Observing neighboring foreclosure activity may thus weaken the social stigma against default and induce greater strategic default by borrowers for whom it is already profitable to stop making mortgage payments.

While the learning and social stigma channels are likely to be strongest in the case of observable foreclosures, they may also operate in the aftermath of serious mortgage delinquency among socially connected neighbors discussing personal financial situations. All mechanisms also predict effects on mortgage debts only, as the relevant diffused information and signals are mortgage debt specific. For these reasons, in subsequent analysis, I focus on serious default as well as foreclosures, and I investigate peer effects on non-mortgage delinquencies as a placebo check.

D.4. Refinancing Channel

Finally, banks may adjust their strategies in response to local foreclosure activity. In particular, they may respond to a nearby foreclosure by adopting more stringent standards on neighboring properties.

Banks may do so for a variety of reasons that overlap with the changes in price and information as in the case of residential borrowers. First, they may do so for the straightforward price channel effects—decreases in neighboring prices reduce the collateral value of housing real estate assets, and in response banks may offer smaller refinancing loans or may even deny them altogether. While this resulting denial of credit may be small, as neighboring home prices decrease by only about 1% in response to a nearby foreclosure, potentially more sizable impacts can arise if banks use the fire-sale price of the foreclosed property as a comparable unit in considering whether to extend credit to neighboring properties. Using the fire-sale price (which frequently sells at a deep discount on the order of 30%) would result in a drastically revised assessment of the value of real estate in local properties and the value of additional mortgage lending. While banks' stated policy is often to exclude forced sales from price comparisons, overworked lenders in this period had difficulty extracting a "true" market price from a wave of depressed home price information. Accepting foreclosure sales in appraisal information would result in a strong bias against extending additional refinancing in areas hit by foreclosure activity.

Regardless of whether credit rationing by banks in this manner was rational in the sense of being a careful response to changing local conditions as signaled by foreclosures, or whether it was an overreaction to the fire-sale prices of foreclosures, the key prediction of this channel is a reduction in refinancing activity. The resulting drop in credit through this channel could have large effects on the default behavior of nearby borrowers, even if they were otherwise unaffected (or unaware) of the original foreclosure. As interest rates were decreasing over this period overall (see Figure 4 Panel B), being able to access mortgage markets and obtain new credit was a reliable way to reduce monthly payments and thereby make mortgage payments more easily. Access to refinancing markets was most salient for neighboring borrowers nearing the end of teaser periods on ARMs. Many borrowers experienced a positive rate shock even though overall interest rates had declined since origination, and refinancing into another mortgage was important for such borrowers to avoid payment shock. A substantial thread of the literature examines the role of refinancing on reducing household default and boosting consumption over this period, particularly for ARM borrowers (such as Fuster and Willen (2015) and Di Maggio et al. (2017)). The bank supply response of foreclosure externalities could exacerbate foreclosure contagion by inducing a shutdown in credit in mortgage markets in response to local foreclosure activity.

E. Empirical Approach

The key prediction of the various mechanisms proposed above work through the externalities that foreclosures impose on neighboring properties. These effects of foreclosure on prices, information, and the supply of mortgage credit potentially induce different default behavior among peers. However, the main problem preventing straightforward identification through an OLS regression is the joint determination of defaults through peer effects. A positive correlation between defaults is consistent with the existence of peer effects through which the default choice of one individual is driven by the default choices of another. But individuals may simply be subject to common group-level shocks—for example, local plant closures—that affect the default behavior of individuals living in a particular neighborhood. These omitted variables are likely to bias the OLS estimate of the causal parameter of interest—the coefficient on nearby foreclosure on an individual's default—upward. This is the standard "reflection" problem as discussed in Manski (1993).

To resolve this identification challenge, an ideal experiment would randomly allocate individuals to neighborhoods of varying foreclosure intensity. I instead adopt a quasi-random approach, whereby I take advantage of an instrument that affects the defaults of particular mortgages, but that affects neighboring mortgages only through the channel of the treated individual. I argue later that the reset structure of ARMs can serve as such an instrument. The empirical strategy follows a two-step regression.

In the first stage, I regress the default behavior of a resetting ARM on a variety of controls specific to the loan and geographic area against attributes of mortgage reset. Specifically, for ARM i in zip code z and month t, I regress:

$$D_{iz,t\rightarrow t+12} = \alpha + \mu_{zt} + \beta' X_{izt} + \gamma' R_{it} + \varepsilon_{izt}. \tag{1}$$

Where $D_{iz,t\rightarrow t+12}$ is a foreclosure on a mortgage i in zip code z in the 12 months after reset date t.[10] I include fixed effects for month of reset and zip code X_{izt}. I also include a variety of other borrower controls, including the credit rating of the mortgage (e.g., subprime, jumbo-prime), the level of documentation, the credit score both at origination and reset, the reason for the mortgage (e.g., refinancing or purchase), and the current combined loan-to-value ratio (including first and second lines, with a measure of house prices adjusted to the zip code level provided by Zillow). A full list of covariates is included in Internet Appendix Section II.

The instruments are reset shock controls in R_{it}. In the tightest form of identification, these instruments are the relevant interest rates paid by borrowers of a given index type or lookback period. In the broadest sample, these instruments capture the size of the change in interest rates at the time of the reset shock. Standard errors are clustered at the census tract-year level.[11]

This specification yields the fraction of foreclosures among ARMs that can be attributed purely to the within-month variation in interest rates arising from index choice, lookback choice, or other forms of reset shocks. As an intermediate step, I also investigate the reduced-form specification, which investigates how these reset shocks serve as a treatment for neighboring loans:

$$D_{-i,z,t\rightarrow t+24} = \alpha + \mu_{zt} + \beta' X_{-i,zt} + \gamma' R_{izt} + \varepsilon_{-izt}. \tag{2}$$

The reduced-form (or intention-to-treat) specification asks how assignment into the relevant treated population—having a neighboring loan that is an ARM with a relatively higher reset shock—affects the future two-year-ahead foreclosure probability.[12] The notation X_{-i} indicates that these values are taken from the set of all neighbors of the resetting ARM *except* the resetter itself. While data taken for the resetting ARM come from the BlackBox data set of all

[10] To ensure comparability of results across regions with varying time to foreclosure completion, the definition of foreclosure that I use in the first stage is the foreclosure start. Subsequent results focusing on the timing of peer responses suggest that neighboring defaults begin prior to the completion of the foreclosure, suggesting that borrowers may respond to the physical abandonment of the property. Selecting foreclosure starts as the dependent variable in the first stage therefore allows for potential peer effects under a broader definition of foreclosure. Results using foreclosure completion as the dependent variable of the first stage yield similar results.

[11] Robustness checks with Conley standard errors yield similar estimates for standard errors.

[12] The foreclosure definition used for neighbors in both the reduced-form and final-IV specifications is foreclosure completion, which is the only measure available for the full set of neighbors observed in the Deeds data set. Results using notice of default are similar.

private-label securitized loans, information on neighbors comes from the Deeds data set, which provides comprehensive data for all transacting mortgages. Information at the Deeds level on $X_{-i,st}$ includes whether the neighbor's mortgage itself is an ARM, the current combined loan-to-value of local properties (again including subordinate liens and updating the price through a zip-code-level index), and an indicator for whether the property is owned by an investor.[13]

Finally, the IV specification combines the first-stage and reduced-form specifications into an estimate for how borrowers respond to the foreclosure decisions of their peers as instrumented by characteristics of the mortgage reset,

$$D_{-i,z,t\rightarrow t+24} = \alpha + \mu_{zt} + \beta' X_{-i,zt} + \delta' \hat{N}_{izt} + \varepsilon_{-i,zt}, \tag{3}$$

where $\hat{N}_{izt}$ captures the share of ARM defaults instrumented for by the within-month difference in interest rates paid by the resetting ARM-holder. The resulting two-stage IV estimation process calculates the effect of an additional foreclosure, as instrumented by higher interest rates paid by a resetting ARM holder, on the foreclosure or default propensity of all local properties. These specifications control for the actual index or lookback period of the resetting loan, allowing for any level differences between loans or neighbors of loans linked to these contract terms. The identification proceeds by using the precise interest rate spread induced by the choices in contract terms to instrument for neighboring foreclosure. The coefficient of interest, δ, captures the effect of foreclosure rates as instrumented by the quasi-exogenous interest rate shocks on neighboring default behavior.

This estimation process is subject to a number of limitations and is based on a number of assumptions that must be examined. First, the exclusion restriction requires that the instrument can affect properties only through the foreclosure of the treated property. An important test of this assumption comes from local fixed-rate mortgage properties, which pay constant interest rates and so by construction are unlikely to be affected by the interest rate dynamics governing the instrument.

The resulting estimates also reflect an LATE that is specific to regions surrounding the private-label securitized ARMs studied in this paper. It is worth bearing in mind, however, that this is the relevant group for a number of interesting analyses. For instance, in examining the effect of changes in interest rates on loan performance, ARMs correspond to the population exposed to fluctuations in interest rates and their localities correspond to the locally treated geographies facing this population's default choices. Additionally, the sample of subprime and jumbo-prime mortgages in question were highly targeted for a variety of federal programs, such as HAMP and HARP, which aimed to provide mortgage relief and were motivated at least in part by the goal of preventing default spillovers. Finally, the results, though estimated on a sample selected to

[13] This variable is generated following Chinco and Mayer (2016) by isolating properties in which the mailing address for taxes differs from the property address.

achieve the greatest identification, illustrate mechanisms and processes of social interaction that likely reflect patterns in the residential mortgage market that are broader yet.

II. Main Specification—Defaults

A. Interest Rate Variation

A.1. Index Choice

The first contract detail that I exploit is the financial index that determines the reset rate. There are several potential choices of index, but Treasury and LIBOR rates are most common. In my analysis, I focus on contrasting the one-year LIBOR rate and the one-year Treasury rate.

Prior to 2007, LIBOR and Treasury rates moved quite closely, with a spread of under 50 basis points. While loans tied to LIBOR typically had slightly lower margins to account for the spread, the difference in risk between the two was perceived to be low and both were viewed as valid proxies for the risk-free rate.[14] While borrowers may have had some choice of index type, it was typically a function of the lender's intentions with respect to the secondary market. Many investors in mortgage-backed securities (for instance, European banks) had a cost of funds denominated in LIBOR, and hence were interested in purchasing assets with a payment structure also determined by LIBOR. In my sample, the loan servicer explains more than 50% of the variance in index; however, many originators provided loans in both categories.

In the precrisis period, the precise choice of interest might lead to modest differences in interest rates for borrowers in the same product class resetting in the same month, but there was little ex ante reason to prefer one over the other. However, as Figure 1 demonstrates, the two rates diverged sharply during the crisis, reaching a maximum spread of over 3 percentage points and averaging over 1 percentage point between 2007 and 2009. The source of this discrepancy was predominately overnight risk. This interest rate spread is very similar to the traditional TED spread (the spread analyzed here differs slightly in that loans were indexed to 12-month LIBOR and Treasury indices).[15]

[14] Hull (2010) notes that "There is a small chance that an AA-rated financial institution will default on a LIBOR loan. However, they are close to risk-free. Derivatives traders regard LIBOR rates as a better indication of the 'true' risk-free rate than Treasury rates, because a number of tax and regulatory issues cause Treasury rates to be artificially low (p. 81)."

[15] Another possible source of the discrepancy may have been market manipulation of LIBOR rates. A class action suit filed against the 12 largest banks alleges rate manipulation on LIBOR-linked ARM contracts: "Throughout the Class Period, the LIBOR six-month rates on the first business day of each month are, on average, more than two basis points higher than the average LIBOR six-month rates throughout the Class Period. Additionally, from August 2007 through February 2009, the LIBOR six-month rates on the first business day of each month are, on average, more than seven and one-half basis points higher than the average LIBOR six-month rates. Finally, the LIBOR six-month rates on the first business day of each month are, the great majority of the time, higher than the five-day running average of the LIBOR six-month rate surrounding the first business day submissions throughout the Class Period." See Annie Bell Adams, et al.

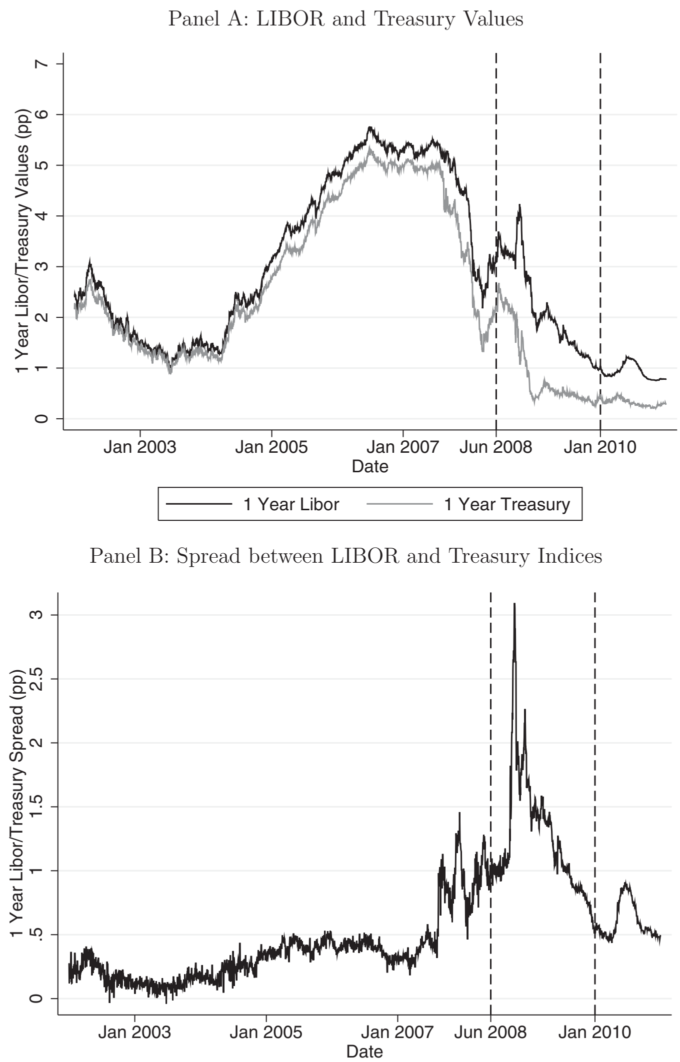


Figure 1. Index values. Panel A illustrates prevailing index rates on one year LIBOR and Treasury indices to which many adjustable-rate contracts were linked. Panel B illustrates the spread between these two indices. The spread reaches a maximum of over 300 basis points in late 2008. The areas highlighted by dashes indicate a period of large differences in interest rates between the two indices that are explored further in Figure 5.

Panel A of Table I summarizes information at the loan, neighborhood, and neighboring loans levels for both LIBOR- and Treasury-linked loans. The

v. Bank of America, et al. 12 Civ. 7461. However, on balance, banks were likely underquoting LIBOR rates. Any market manipulation is likely to be uncorrelated with borrower, originator, or neighborhood characteristics around the time of the reset and thus is unlikely to impact the identification strategy.

Table I
Summary Statistics

Index Sample

Index Indicator:	LIBOR	Treasury
Resetting Loan:		
Credit Score	870	902
Original Balance	468	623
Conforming	31%	9%
CLTV	83%	70%
Refi	49%	66%
Investor	9%	4%
Neighborhood:		
Density	24	22
Fraction Non-white	24%	22%
Neighboring Loans:		
CLTV	77%	67%
Mortgage Amount	319%	389%
Year Built	1971	1968
ARM	42%	48%
Investor	23%	20%
N	395k	287k

Lookback Sample

Lookback Length:	0	25	30	45
Resetting Loan:				
Credit Score	723	704	714	704
Original Balance (100$k)	266	259	263	262
Conforming	76%	81%	78%	77%
CLTV	87	107	99	92
Refi	49%	48%	49%	49%
Investor	9%	11%	7%	8%
Neighborhood:				
Density	22	23	22	22
Fraction Non-White	36%	36%	35%	37%
Neighboring Loans:				
CLTV	74%	83%	80%	78%
Mortgage Amount	207	241	218	213
Year Built	1967	1967	1967	1966
ARM	42%	39%	40%	41%
Investor	20%	23%	22%	21%
N	558k	334k	333k	581k

biggest point of difference is that LIBOR-linked loans tended to have a lower origination balance, while Treasury-linked loans were more commonly used above the conforming threshold. When we restrict attention to the jumbo market above the conforming threshold, we find quantitatively similar estimates. Index variation exists only among loans with an initial teaser length

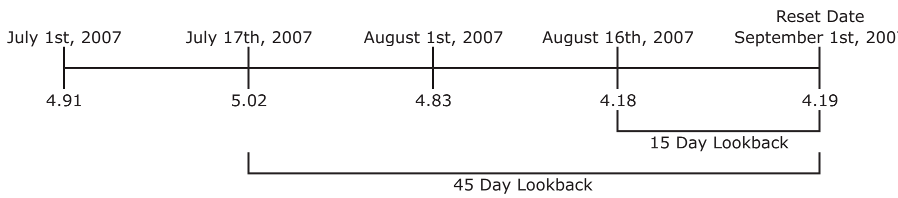


Figure 2. Illustration of lookback period. This graph illustrates the process by which lookback interest rates are calculated. In this setting, a mortgage with a lookback of 15 days will pay, for the next 6 or 12 months, an interest rate component of the payment drawn from the exact interest rate on the prevailing index (LIBOR or Treasury) exactly 15 days prior; in this example 4.18%. An otherwise comparable mortgage with a lookback of 45 days will pay 5.02%.

of five years, such that 5/1 ARMs were found disproportionately in the jumbo-prime market.

A.2. Lookback Period

The second contract detail that I focus on is the lookback period, a remnant of precomputer processing times for updating schedules. At each adjustment, it is necessary for the lender to compute a new interest rate using the relevant index value. To allow for processing and notification of the borrower, lenders typically do not take the contemporaneous value of the index on the day of reset, but instead use the index value at a set period in the past. However, there is no set standard for this period: 15, 25, and 45 days are all common durations. While there is some connection between originator identity and the precise choice of lookback period, there is again no strong ex ante reason to prefer one lookback period over another, and in general the choice of lookback period does not forecast future interest rates.

In periods with relatively low fluctuations in interest rates, the precise choice of lookback period makes little difference. However, when index rates are volatile, differences in lookback period can lead to significant differences in the reset rate actually paid ex post. Figure 2 illustrates the different effects of different lookback periods at a time of significant volatility: A loan resetting September 1, 2007, could experience a difference in rate of greater than 80 basis points depending on the lookback period chosen.

Figure 3 depicts the magnitude of interest rate variation in the sample due to variation in the lookback period. Panel A of this figure plots interest rates paid in each month of reset for three common lookback periods: 15 days, 30 days, and 45 days. Panel B translates these monthly interest rates into within-month spreads corresponding to the maximum difference between the minimal and maximum interest rates paid by holders of these three contract terms over the period 2007 to 2011. This figure, together with Figure 1, illustrates the basic time series of interest rate variation resulting from contract features that I exploit in this paper.

Figure 4 further illustrates the cross-sectional consequences of these interest rate spreads for borrowers' payments. This histogram summarizes interest

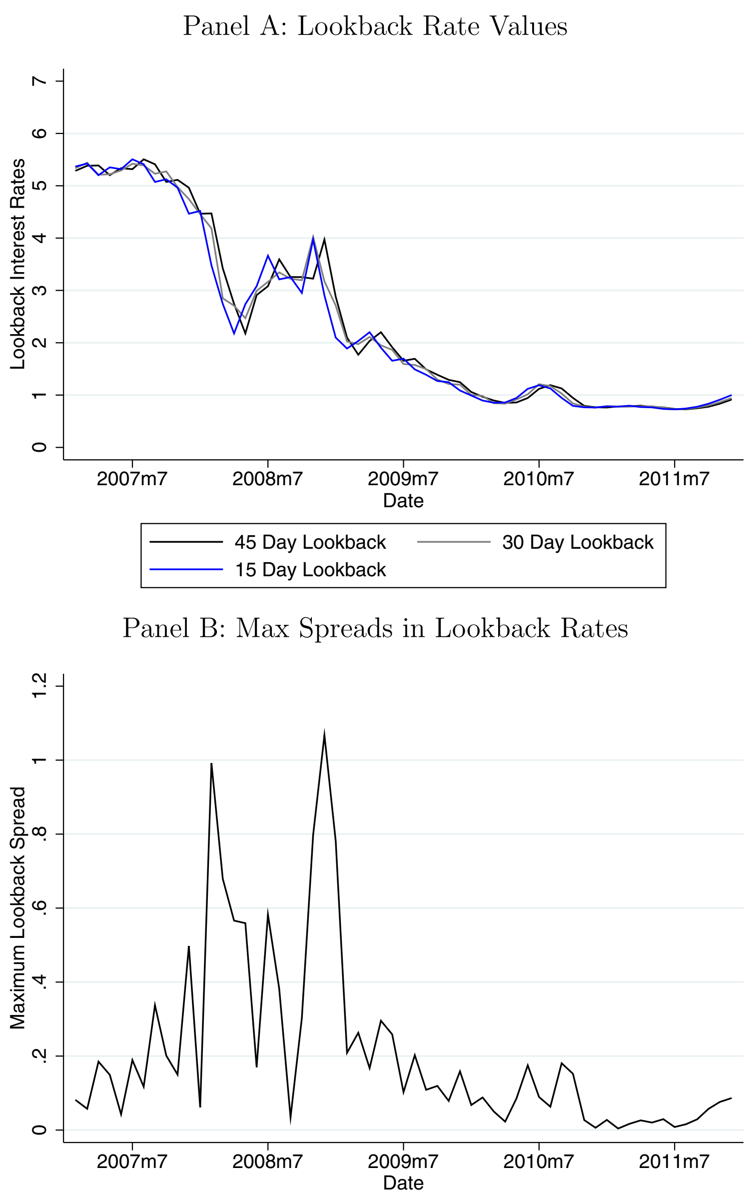


Figure 3. Variation in interest rates from lookback periods. This graph illustrates the differences in potential interest rates paid by adjustable-rate mortgages resetting within the same month due to lookback variation. Panel A illustrates different interest rates paid by holders of different lookback periods over the sample 2007–2011 among borrowers indexed to LIBOR loans for three common lookback periods. As discussed in Figure 2, holders of different lookback contracts will differ only in the reference period over which their interest rate is computed. Panel B highlights the variation exploited in this paper: this figure plots the maximum within-month difference in interest rates paid by different holders of lookback contracts. (Color figure can be viewed at wileyonlinelibrary.com)

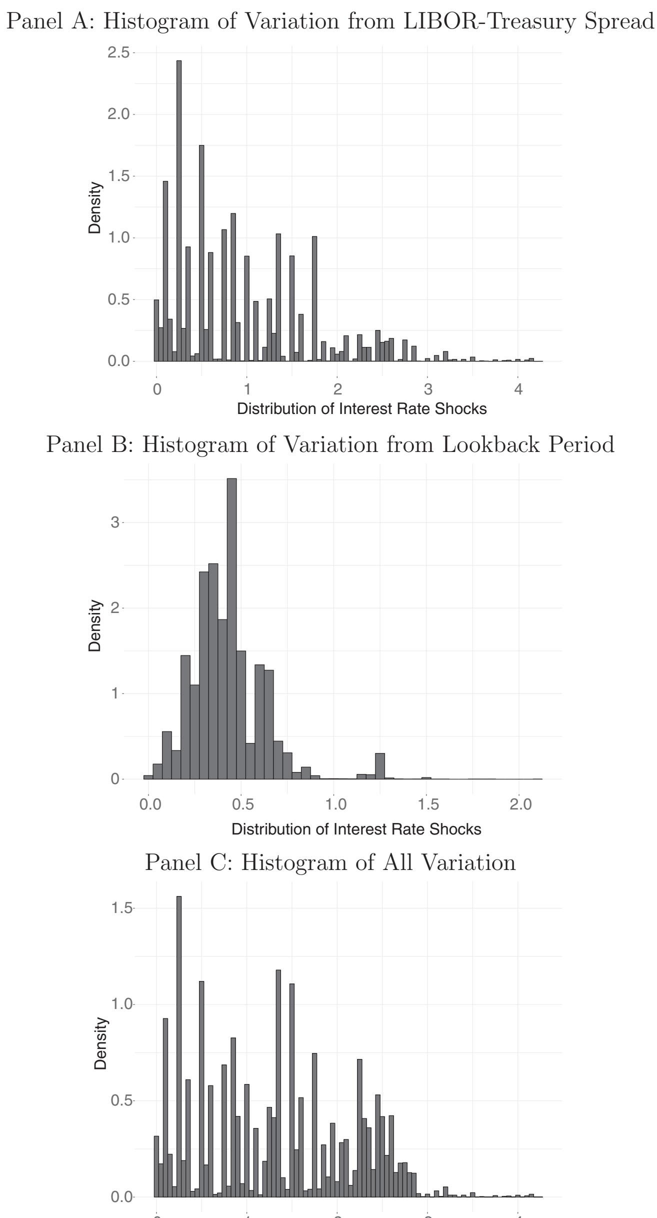


Figure 4. Histogram of interest rate shocks. These histograms illustrate a histogram of realized interest rate shocks resulting from the different forms of variation explored in the paper. Panel A explores the interest rate variation resulting from the LIBOR-Treasury spread (among 5/1 ARMs), by illustrating the histogram of within-month interest rate shocks. Panel B explores the distribution of interest rate shocks resulting from lookback period variation (among 2/28 and 3/27 ARMs). Panel C uses all variation in interest rate shocks among ARM borrowers.

rate shocks by collapsing all within-month interest rate shocks experienced by ARM borrowers over the sample period. Each entry corresponds to a particular loan and represents the difference in payments between the given borrower and borrowers of the minimum interest rate product in that month. Panel A plots the distribution of within-month variation in reset shocks for the variation induced by the spread between LIBOR and Treasury loans, Panel B focuses on the variation induced by the choice of lookback period, and Panel C employs a pooled measure of reset shocks. Panel A restricts attention to 5/1 ARM borrowers, for whom the LIBOR-Treasury spread is salient, while Panel B restricts attention to 2/28 and 3/27 ARM borrowers. Panel C includes all borrowers. Characterizing the variation in this manner helps illustrate that the cross-sectional variation in payments induced by the contract features that I examine in this paper can be substantial-especially when considering that these interest rates prevail over the following 12 months (for 5/1 loans) or six months (for 2/28 and 3/27 loans).

Panel B of Table I summarizes information at the loan, neighborhood, and neighboring loans levels for varying lookback periods. Loan variation on lookback period is relatively minor—the point of largest difference is found among loans with a 25-day lookback period, which reset to lower levels of equity than loans of other lookback periods.

B. Univariate Default Specifications

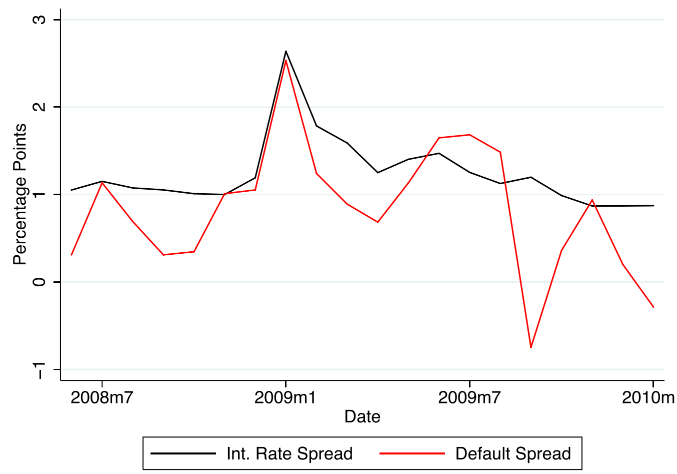
B.1. LIBOR and Lookback Variation

First, I illustrate my approach using a simple univariate approach that highlights the index variation in my sample. Recall that, upon reset, ARMs are tied to either a LIBOR or Treasury index. Monthly payments after reset are based on both the amortization component of the mortgage plus an interest rate component that is divided into a margin and an index component.

Panel A of Figure 5 illustrates the consequences of the interest rate divergence between LIBOR and Treasury rates among resetting loans in my sample. The black line represents the in-sample interest rate spread among loans in my sample. As can be seen, LIBOR-linked loans resetting in periods of high rate divergence between the LIBOR and Treasury indices did, in fact, pay higher interest rates after the reset. Importantly, these rates were fixed for a period of 12 months subsequent to reset. The component of mortgage payments attributable purely to the index divergence was sizable. In the month of greatest divergence, January 2009, resetting LIBOR-linked borrowers paid over 260 basis points more on an identical contract originated in the same month relative to a Treasury-linked borrower, translating into payments of over $11,000 in the subsequent year of the contract. This represents a substantial payment shock, especially in relation to the sorts of shocks previously studied in the literature.[16]

[16] For instance, Parker et al. (2013) analyze the consumption effects of stimulus checks ranging in size from $300 to $1,200.

Panel A: In-Sample LIBOR-Treasury Default Spread for Resetting Loan



Panel B: In-Sample LIBOR-Treasury Default Spread for Neighboring Loans

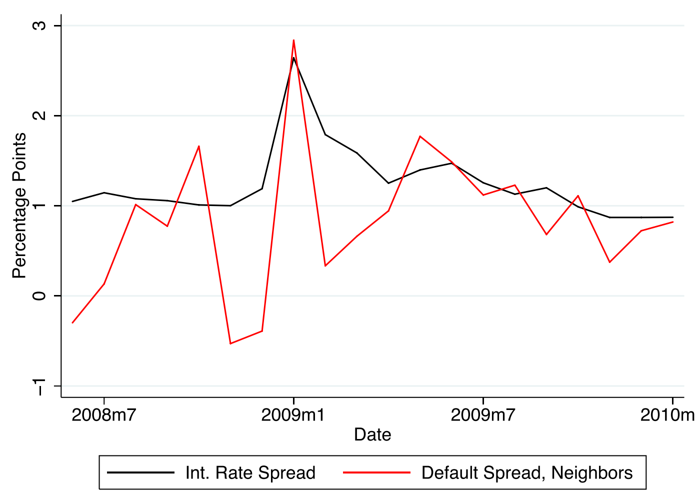


Figure 5. Univariate graphs: Interest rate and default spreads. These graphs illustrate the differences in interest rates and default behavior among loans linked to different indices along with their neighbors. The black line in both panels illustrates the difference in interest rates paid among loans indexed to LIBOR relative to interest rates paid among comparable loans indexed to a Treasury index. This spread reaches a maximum in January 2009, when LIBOR-linked loans could expect to pay, on average, 260 basis points more than otherwise identical Treasury-linked loans—corresponding to about $11,000 more in payments over the subsequent 12 months. Panel A illustrates how this interest rate differential corresponds to a difference in subsequent foreclosure rates among LIBOR-linked loans relative to Treasury-linked loans over the year following reset. Panel B illustrates how this interest rate differential translates into the probability of experiencing a foreclosure in the two years subsequent to the reset date among the *neighbors* of loans linked to either a LIBOR or Treasury index within a 0.10-mile radius. (Color figure can be viewed at wileyonlinelibrary.com)

The red line in Figure 5 Panel A illustrates the effect of the LIBOR-Treasury spread on subsequent loan performance. Specifically, it plots the difference between future foreclosure rates on LIBOR-linked loans relative to Treasury-linked loans over the year subsequent to reset. While the simple univariate graph shows some variation in default patterns unrelated to the underlying index-driven difference, the overall pattern of default divergence follows that of the index and peaks in exactly the month of greatest interest rate divergence (January 2009).

Panel B extends this comparison to examine the behavior of *neighboring* loans in neighborhoods around either a LIBOR- or Treasury-linked loan. The sample construction follows that described above. Here, the graph illustrates the difference in foreclosure rates among borrowers who live near either a LIBOR- or Treasury-linked loan over the two years following reset of the ARM. The novel finding is that neighbors appear to respond to interest rates among neighboring properties. The month of greatest divergence between loans neighboring LIBOR- and Treasury-linked loans again matches the January 2009 spike in interest rates paid by LIBOR- and Treasury-linked resetting loans.

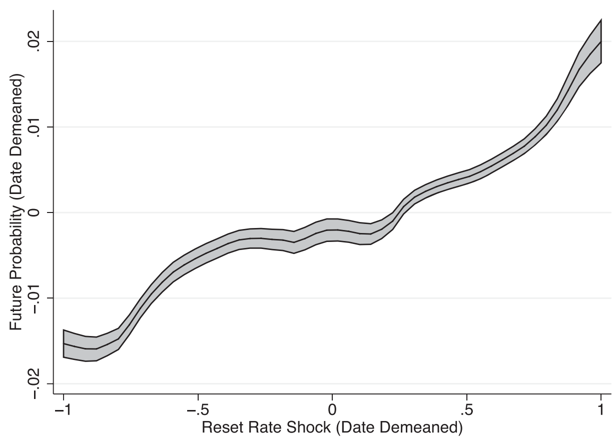
Overall, Figure 5 provides strong evidence that interest rate shocks at the time of reset influence borrowers' default-related decisions and, consequently, the foreclosure intensity faced by their neighbors.

B.2. All Interest Rate Variation

While in the previous two sections, I investigate particular sources of variation in interest rates paid conditional on month of reset due to specific contract terms, for reasons of power and generalizability in comparisons, here I also examine the response of ARM resets in general. Figure 6 plots a local polynomial smoothing of the effects of all of the within-month variation in reset shocks on default rates. Again, the key comparison is not between loans, which experience an upward reset of, say, 3% rather than 1% because they reset in a period of high rather than low interest rates. Instead, the date-demeaned component of reset shocks focuses on the within-month variation in reset shocks arising from a combination of different origination rates (between loans with different initial teaser rates), different lookback durations, different teaser lengths, and different index rates. The underlying assumption is that such differences in contract terms driving the subsequent shocks to within-month reset rates offer sufficient variation to examine how borrowers respond to these reset shocks, and thus, provide an instrument for analyzing neighboring responses to different foreclosure intensity. It is important to keep in mind that whatever factors may have driven borrowers of a particular ARM product to pick a particular contract menu, these decisions are out of the control of their neighbors, who must take the contract choices and subsequent default patterns of a borrower as given.

Panel A of Figure 6 provides evidence on the foreclosure response of an ARM borrower in response to higher reset shocks. Borrowers tend to default more in response to higher-than-average reset shocks for a given month. Panel B is

Panel A: Impact of within-month variation in Interest Rates on ARM Default Rates



Panel B: Impact of within-month variation in Interest Rates on Neighbor Default Rates

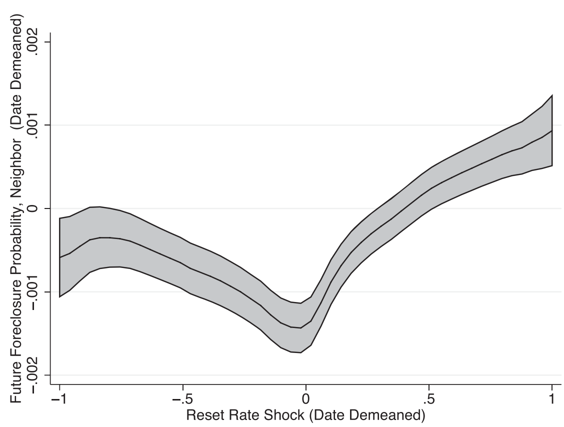


Figure 6. Default responses to interest rate hikes. These plots investigate the impact of within-month variation in reset rates on default behavior. Panel A illustrates a local polynomial smoothed plot of date-demeaned reset shocks against future date-demeaned foreclosure probabilities among resetting loans. Variation in reset rates, conditional on month, arises from differences in origination rates (due to differences in initial teaser term), index choice, and lookback period. Panel B uses the same measure of reset rate shocks among resetting adjustable-rate mortgages, but investigates the foreclosure responses among neighboring loans within a 0.10-mile radius in the subsequent two years.

perhaps more surprising. In this graph, I examine the foreclosure response of neighbors of an ARM product in response to a higher reset shock of the ARM holder. I find that neighbors of loans facing higher-than-average reset shock are themselves more likely to experience foreclosure over the two years following mortgage reset.

C. OLS Results

As an illustration, I first test for geographical clustering using OLS.

In Internet Appendix Table IA1, I present estimates on the relationship between completed foreclosures and the number of additional foreclosure completions around a property through a benchmark OLS calculation. The sample consists of all loans within a 0.10-mile radius around a resetting ARM holder, and the table reports results on foreclosure outcomes for neighbors over the two years following the ARM reset. The first column estimates the foreclosure response of borrowers as a function of prior foreclosures in the entire radius prior to reset. Additional controls include several variables taken at the level of both the resetting mortgage and the neighbors. Standard errors are clustered at the census tract-year level.

The results suggest that a prior foreclosure in the region is associated with a 3% higher probability of subsequent foreclosure among neighboring homes in the vicinity, or a 100% increase in the hazard rate of foreclosure. The second column estimates the foreclosure response in the two years after reset among neighbors as a function of all foreclosures of the resetting ARM holder in the period after reset. The estimate in this column is higher—each foreclosure results in a 4.9% higher chance of foreclosure among neighboring homes. The higher estimate in the second column is likely due to the estimated timing of the foreclosures. The first column examines the postreset foreclosure reaction of neighbors in response to a previous foreclosure prior to the reset date, while the second column examines foreclosure responses in the postreset window among both neighbors and the resetting loan.

Overall, the OLS results provide suggestive (though not causally definitive) evidence that defaults may be geographically clustered. These results are consistent with a sizable literature that examines geographical clustering of default patterns. The results in column (2) in particular point to very large contagion effects: Given an average of 22 houses in each 0.10-mile radius, the results suggest that each foreclosure is associated with another foreclosure over the following two years.

Figure IA1 in the Internet Appendix illustrates this effect, graphing completed foreclosures in Phoenix for the period 2006 to 2009. The massive rise in foreclosures and their spatial clustering in certain areas (for instance, in subprime-heavy Glendale located in the upper left part of the city) is certainly consistent with the hypothesis that an element of social interaction regarding mortgage default played a role in the amplification of the foreclosure wave. However, there are key econometric challenges in drawing a causal interpretation from the OLS evidence. In particular, local correlations in default rates may simply result from exposure to common local shocks (e.g., a local plant closing) or common unobserved variables (e.g., income). In other words, the standard endogeneity or reflection problem (as discussed, for instance, by Manksi (1993)) is particularly problematic in this context. Addressing this issue has proven to be difficult in prior work on peer effects, and in particular in prior work

Table II

Main Results

This table examines how within-month variation in interest rates, paid conditional on reset, predict future foreclosure behavior. The first-stage regression is $D_{iz,t\to t+12} = \alpha + \mu_{zt} + \beta' X_{izt} + \gamma' R_{it} + \varepsilon_{izt}$, the reduced form (intention-to-treat) specification is $D_{-i,z,t\to t+24} = \alpha + \mu_{zt} + \beta' X_{-i,zt} + \gamma' R_{izt} + \varepsilon_{-i,zt}$, and 2SLS (instrumental variable) specification is $D_{-i,z,t\to t+24} = \alpha + \mu_{zt} + \beta' X_{-i,zt} + \delta' \hat{N}_{izt} + \varepsilon_{-i,zt}$, where the notation X_{-i} indicates that these values are taken among the set of all neighbors of the resetting ARM *except* the resetter herself. Brackets contain the mean of the dependent variable for the sample in question. Standard errors are clustered at the tract-year level. The dependent variable in the first column is the foreclosure propensity of the resetting mortgage in the year after reset; in the next two columns it is the foreclosure probability of neighboring properties for the two years after reset. The last column shows the F-test for the excluded instrument in the first stage of the regression.

Dep Var: Foreclosure	First Stage	Reduced Form (ITT)	Second Stage (2SLS)	N (Clusters)	First-Stage F
Index Sample	0.024	0.0012	0.050	664k (14k)	173
	(0.003)	(0.0007)	(0.013)		
	[0.025]	[0.02]	[0.02]		
Lookback	0.025	0.0005	0.016	3.3m (45k)	969
	(0.002)	(0.00015)	(0.005)		
	[0.21]	[0.041]	[0.041]		
All Resets	0.025	0.0005	0.021	4.4m (58k)	1031
	(0.0013)	(0.00014)	(0.0049)		
	[0.17]	[0.034]	[0.034]		
Resetter Controls	Yes	Yes	Yes		
Neighbor Controls	Yes	Yes	Yes		
Fixed Effects	Zipcode	Zipcode	Zipcode		
Dep Var is Foreclosure of:	Resetter	Neighbor	Neighbor		
Key Regressor is:	Interest Rate	Interest Rate	Predicted Foreclosure		

on residual mortgage default spillovers.[17] To address this issue, I introduce a novel instrument related to administrative details associated with the reset of ARMs.

D. First-Stage Specification

In column (1) of Table II, I estimate the following first-stage equation:

$$D_{iz,t\to t+12} = \alpha + \mu_{zt} + \beta' X_{izt} + \gamma' R_{it} + \varepsilon_{izt}. \tag{4}$$

[17] For instance, see Campbell, Giglio, and Pathak (2011, p.15): "foreclosures are endogenous to house prices because homeowners are more likely to default if they have negative equity, which is more likely as house prices fall. Ideally, we would like an instrument that influences foreclosures but that does not influence house prices except through foreclosures; however, we have not been able to find such an instrument."

The first row reports the effect of a 100 basis point increase in within-month variation in interest rates arising from index choice on the foreclosure probability of the loan over the next year. The results suggest that each 100 basis point increase in interest rates is associated with a 2.5% increase in the probability of foreclosure occurrence (i.e., foreclosure starts) over the next year. The second row reports first-stage results using interest rate variation arising from the choice of lookback term. The results are very similar. The third row uses all within-month variation in reset rates at the time of reset and again yields similar results.

E. IV Results

In previous sections, I discuss the identification strategy. Here, I present evidence on results involving neighbor default as a function of resetter characteristics. Column (2) of Table II presents reduced-form, or intention-to-treat, estimates that examine the effect of higher within-month interest rates conditional on reset on the default behavior of neighbors. The reduced form provides a consistent estimate of the effect of having a peer exposed to the treatment group of resetting ARMs paying higher interest rates. Unlike the IV estimation, the reduced-form estimate does not require a monotonicity assumption (i.e., that higher within-month interest rates conditional on reset did not cause resetters to be less likely to foreclose). This regression includes controls at the level of the resetting loan (including controls for the current combined loan-to-value of the resetting loan, the credit score at origination and just prior to reset, loan attributes such as the purpose and type of loan, as well as a dummy indicating the servicer), neighboring loans (including the current combined loan-to-value, whether the occupant is an investor, and whether the mortgage is an ARM or a refinanced loan), and the neighborhood (including controls for density, past foreclosures in the area, and the fraction of the population that is nonwhite at the census tract level), as well as zip code and month of reset fixed effects.

The results in column (2) suggest that higher interest rates paid by resetting mortgages are associated with higher foreclosure rates among geographical peers. Given that these mortgage holders have completely separate mortgage terms from the resetting ARM holder, in particular, they may have a fixed-rate mortgage with constant payments, this apparent correlation points to the strength of peer interaction effects.

Column (3) combines the first-stage and reduced-form models into an IV specification in which the foreclosure rate of the resetter is instrumented by attributes of the reset. The first row, in which I examine peer effects arising from index variation, contains estimates comparable to the OLS results (see Internet Appendix Table IA1) of 0.05. These specifications control for whether the neighboring loan is itself LIBOR- or Treasury linked; the identification comes from the precise interest rate spread induced by the LIBOR-Treasury difference. Under the assumptions of the two-stage IV approach, this finding suggests that strong foreclosure spillovers lead to a substantial increase in foreclosure intensity as a result of nearby foreclosures.

The second row restricts attention to variation in within-month interest rates induced by choices in lookback period. These specifications also control for lookback dummies, which allows for level differences in foreclosure outcomes among neighbors as a result of different lookback choices for the resetting loan. In the first stage, the foreclosure of the resetting mortgage is instrumented by the variation in interest rates derived from the lookback period. The IV estimate calculates the foreclosure probability of neighbors as a function of this predicted foreclosure rate. The point estimate suggests that foreclosures driven by lookback interest rate variation result in a 1.6% increase in the foreclosure rate of neighbors in the two years after reset, relative to a somewhat higher baseline rate of 4.1%. This result suggests that the aggregate effect of each resetting foreclosure is 0.35 additional foreclosures.

My preferred estimates are in the third row of column (3), which combine all data and use all within-month variation in interest rates as shocks for the foreclosure of the resetting ARM, and which further use this instrumented foreclosure intensity to examine the peer effects in foreclosure. The point estimate suggests that foreclosures that are induced by attributes of the reset raise the foreclosure probability of neighbors in a 0.10-mile radius by 2.1%, relative to a mean of 3.4%, over the two years following reset. This finding represents a substantial increase in the hazard rate of foreclosure, although the estimate is substantially smaller than the OLS estimate derived from all foreclosures among the resetting property, which points to the importance of correcting for reflexivity in responses to foreclosure. These estimates correspond to a cumulative effect of each foreclosure of 0.46 additional foreclosures in the neighboring area over two years after a completed foreclosure (with a 95% confidence interval of (0.25, 0.67)). These effects are quite large in terms of economic significance and suggest that peer exposure to local foreclosures is a significant factor influencing household foreclosure outcomes.

My estimates are most comparable to the results of Munroe and Wilse-Samson (2013), who use a judicial instrument to establish the causality of foreclosure contagion. My point estimates are similar to their baseline specification, which shows that a completed foreclosure is associated with a yearly foreclosure hazard of around 0.54 to 0.69 additional foreclosures after a completed foreclosure. However, my estimates are more precise, although they are not able to rule out cumulative foreclosure effects as small as -0.054 or as large as 2.84 by the year after judicial decision. My estimates establish foreclosure estimates in a way consistent with prior research while ruling out the null hypothesis that foreclosures have no externalities, as well as extremely large contagion estimates.

The last column of Table II includes an F-statistic for the excluded instruments. The estimates range from 173 to 1031, which are quite high for this context and alleviate concerns regarding weak instruments in this investigation. The strength of this result likely arises from a combination of significant effect sizes in the first stage and a large sample size.

III. Mechanisms

A. Neighboring House Price Effects

In investigating the mechanisms through which foreclosure spillovers may impact neighbor responses, I first focus on the role of house prices. A foreclosed property typically sells quickly through auction at a distressed price. Foreclosures therefore result in an increase in the net supply of housing in local areas since defaulters typically rent subsequent to experiencing a foreclosure. Given that residential properties are typically differentiated products, this may result in measurable price impacts in the local neighborhood. Additionally, foreclosed properties typically remain vacant and depreciate through the foreclosure process, resulting in a decrease in the capitalized amenity value of local areas.[18] These channels suggest the possibility of foreclosures affecting house prices in local areas, which may in turn be driving the default decisions of borrowers. Lower house prices may induce borrowers to default for a variety of reasons, including strategic default by borrowers facing negative equity, or by borrowers facing other adverse shocks such as unemployment, in which case, the additional impact of local house price declines may lead to a double-trigger default.

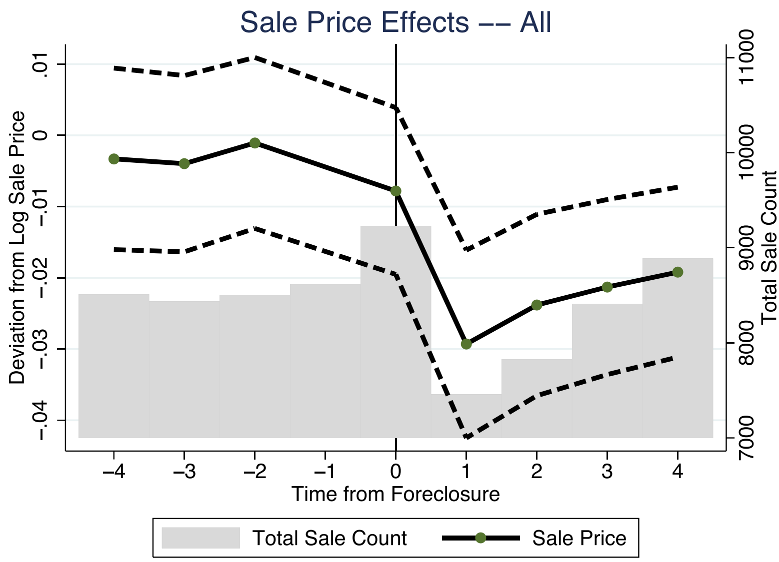
Figure 7 uses an event study framework to illustrate the effect of a foreclosure on sale prices of houses within a 0.10-mile radius around the foreclosed property both before and after the foreclosure completion. The estimates are based on a regression of the log sale price of properties on a variety of household characteristics,

$$\log(P_{it}) = \alpha + \mu_t + X_{it} + \sum_{k=-s}^{s-1} \mu_k \cdot 1[(t - T_i) = k] + \varepsilon_{it}, \tag{5}$$

where $\log(P_{it})$ is the log of the sale price of homes. Fixed effects control for the month of observation, the zip code, and a variety of property-specific controls. The key coefficient of interest is μ_k, which measures the change in sale price of the house during s quarters prior to and following the foreclosure in quarter T_i. This sample restricts attention to borrowers in the vicinity of a resetting ARM that experiences foreclosure. Standard errors are clustered at the census tract-year level. Panel A corresponds to the effect of the foreclosure on the log price of transacted properties. The estimates point to a sizable decrease in the sale price of transacted properties of around 1% to 4% after the sale. These estimates are comparable to those in prior research, such as Anenberg and Kung (2014) or Campbell, Giglio, and Pathak (2011), who estimate a roughly 1% effect on price after differencing out price impacts on more distant geographies. Panel B estimates the effect of foreclosures on the repeat-sale measure of prices, in which the dependent variable is now $\log(P_{i,t-q_i})/\log(P_{it})$, where $P_{i,t-q_i}$ is the last transacted sale on the property. The repeat-sale measure, as emphasized by Fisher, Lambie-Hanson, and Willen (2015), allows for a measure of price

[18] A price discovery channel may also suggest that the presence of a local foreclosure itself reveals some information about the future price dynamics of a neighborhood.

Panel A: All Effects



Panel B: Repeat Sales

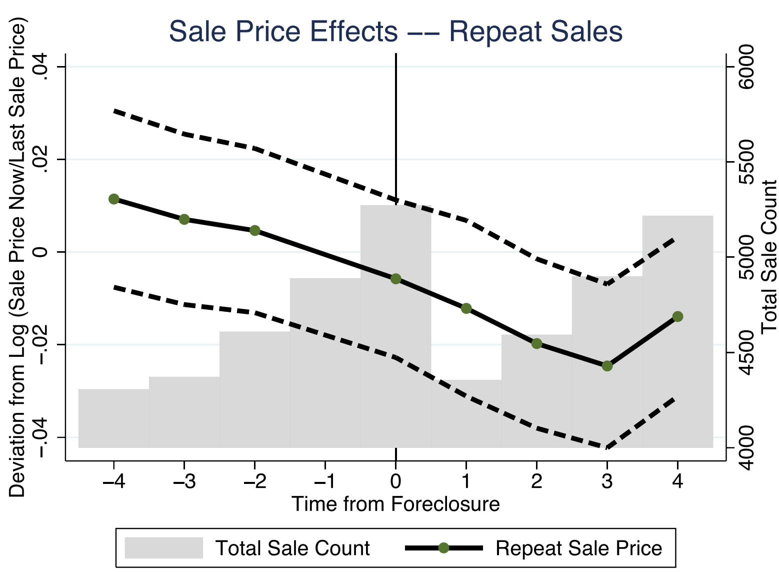


Figure 7. Estimates of impact of foreclosure activity on local sales. This graph illustrates the coefficients from a regression of log sale price among properties in the neighborhood of a resetting adjustable-rate mortgage (ARM) against an indicator of timing relative to a completed foreclosure on the resetting loan. Time to foreclosure is measured in quarters relative to a completed foreclosure date on the ARM subsequent to reset. The estimates are drawn from a regression of

impact that better controls for property-specific characteristics. The effect of foreclosures on the repeat-sale measure of price is lower, averaging around 1% and reaching statistical significance and a minimum of around 2% two quarters after the completed foreclosure sale.

The difference between the effect of the foreclosure on the standard price relative to the repeat-sale measure of price suggests an important role for compositional effects subsequent to foreclosure completions on the set of properties that transact. Lower priced properties disproportionately transact subsequent to foreclosures, suggesting a larger price impact than if changes in household sale choices were held fixed. Consistent with this interpretation, the histogram of completed sales in the neighborhood of foreclosed properties displayed in the background of Figure 7 shows that properties disproportionately do not transact in the quarter after a completed foreclosure has taken place, that is, in the quarter when price effects are also greatest.

Addressing the effect of changes in composition on the causal estimation of prices is a challenge that prior work in the area has not fully addressed. However, I emphasize several features of this bias. First, it appears that much of the selection effect comes from higher priced properties choosing *not* to transact after a nearby foreclosure, which suggests a downward bias on estimated price effects. Second, precise identification of the true treatment of foreclosure effects is not necessarily critical in the context of establishing peer effects, because nearby borrowers and lenders themselves take house prices as given and do not necessarily filter the true price impact separate from the change in composition, which is a challenge even for the econometrician. In particular, property value assessments take local comparable sales as given when establishing estimates of current market prices.

To gauge the potential importance of this channel, I perform a simple back-of-the envelope calculation. In particular, I assume a price effect of 1%, which would suggest that the price channel can explain perhaps 10% of the foreclosure contagion channel that I observe. Given the difficulties in accurately measuring the price channels, I emphasize the order of magnitude of this result: Even under reasonable adjustments to the precise estimation of the price effect, it seems unlikely that the price effect can explain the entirety of the foreclosure contagion channel.

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the log sale price of properties against a variety of household characteristics: $\log(P_{it}) = \alpha + \mu_t + X_{it} + \sum_{k=-s}^{s-1} \mu_k \cdot 1[(t - T_i) = k] + \varepsilon_{it}$, where $\log(P_{it})$ is the log of the sale price of homes, fixed effects control for the month of observation, the zipcode, and a variety of property-specific controls. The key coefficients of interests are μ_k, which measure the change in sale price of the house during s quarters prior to and following the timing of the foreclosure in quarter T_i. Standard errors are clustered at the tract-year level. Pane A illustrates the response of all prices; Panel B illustrate the change in sale prices using a repeat-sale measure which incorporates the last selling price of the house. Background histograms capture total sale counts in the quarter ended. (Color figure can be viewed at wileyonlinelibrary.com)

Table III
Spillover Impacts on Refinancing Outcomes

This table examines how within-month variation in interest rates, paid conditional on reset, predict future refinancing behavior for neighbors around a resetting ARM for the following two years. Results are estimated both on a Deeds sample, for which refinancing activity is drawn from subsequent mortgage transactions, as well as on a BlackBox sample, for which refinancing is measured as the prepayment on an existing mortgage. The specifications shown reflect the instrumental variable specification in which the first stage regresses all within-month variation in interest rates against foreclosure rates of the resetting property, and the second stage uses the predicted value of foreclosure from these reset characteristics against refinancing among properties in the 0.10 mi radius around the resetting ARM. Controls for neighboring properties include loan-to-value. Standard errors are clustered at the tract-year level.

	Refinancing (2SLS)	Refinancing (2SLS)	First-Stage F
Index Sample		−0.078	1,031
		(0.027)	
Lookback Sample		−0.062	969
		(0.099)	
All Resets	−0.035	−0.063	173 (458 BBX)
	(0.01)	(0.009)	
N (Clusters)	1.7m (26k)	4.4m (58k)	
Sample	BBX	Deeds	
Avg of Dep Var	[0.076]	[0.17]	

B. Refinancing Channel

Supposing that the price channel—a key focus of prior research in the area of foreclosure effects—is not likely to explain the full magnitude of my foreclosure contagion results, what are other likely channels through which foreclosures affected neighbor responses? One important channel complementing the role of house prices is the change in access to new credit as suggested by changes in refinancing activity.

Table III estimates a two-stage IV regression in which the outcome in the second stage is refinancing behavior among neighboring loans (again within a 0.10-mile radius), while foreclosure of the resetter remains the dependent variable in the first stage. Whereas refinancing opportunities respond in part to changes in house prices as captured in the previous section, separate estimation of the refinancing channel suggests a more precise isolation of this mechanism. In particular, lenders may respond both to the price effect as revealed through changes in transacting prices and to actual evidence of foreclosure activity in nearby areas. The reason may be that lenders view foreclosures as revealing information regarding the quality of the neighborhood or the borrowers who live within it.

Using the instrumented value of foreclosure among resetting loans from the variation in reset rates, I find substantial evidence that refinancing activity drops in areas that have experienced foreclosures. Column (1) estimates refinancing results on the BlackBox data set, for which refinancing is defined using an early prepayment indicator drawn from a sample of private-label securitized loans.[19] Additional controls in BlackBox specifications include a full set of controls for loan characteristics that are commonly measured on both resetting and neighboring loans, such as interest rates, credit score, loan purpose, and equity. My estimates on this sample suggest that instrumented foreclosure activity reduces the frequency of prepayment by 3.5%, lowering the hazard rate of prepayment among securitized loans by almost half.

Effect sizes are larger for the full sample of Deeds loans, as shown in column (2), for which information refinancing comes from mortgage transactions in the Deeds records. Depending on the specification (index variation, lookback variation, and all interest rate variation), I estimate a 6% to 8% decline in the future two-year propensity to experience a refinancing, relative to a mean of around 17%, or roughly a decrease of one-third of the hazard rate of refinancing.

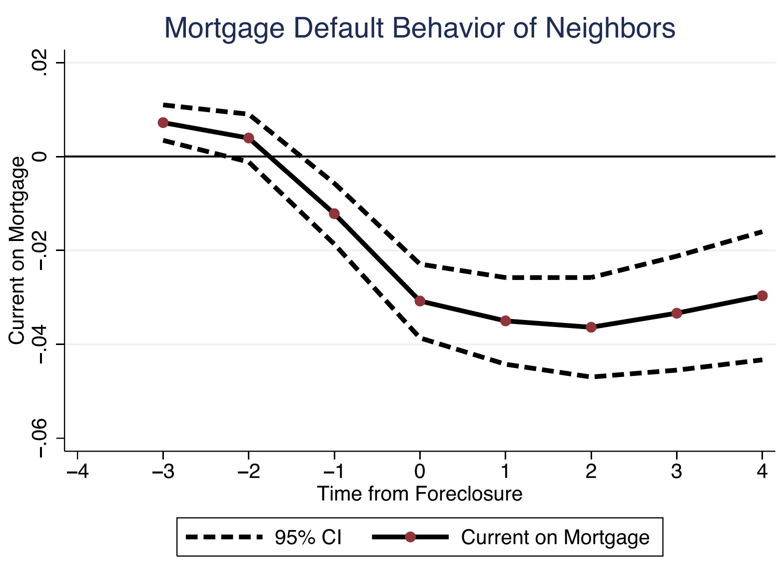
This refinancing effect may reflect either a borrower-driven response, if the local foreclosure reduces borrower propensity to seek out new refinancing, or a lender-driven response if lenders perceive local price drops or other foreclosure externalities make lenders more reluctant to extend credit in the vicinity of a foreclosed property. However, I emphasize that the size of the coefficient on this effect is much larger than my estimates for foreclosure spillovers, which suggests that even if all borrowers who ceased to make payments and wound up in foreclosure would have counterfactually received a new loan instead, there remains a substantial group of borrowers who neither refinance nor default. Instead, they simply continue to make payments on their old mortgage.

Although it is not fully conclusive, the presence of these borrowers is suggestive of a lender-driven response. A plausible mechanism linking lender refinancing responses lies in the role of appraisals. Though appraisal valuations of properties should, in principle, exclude distressed properties, in practice the task of filtering out distressed sales proves difficult for mortgage originators during the financial crisis. If fire-sale prices are included in financial institutions' assessment of borrower credit quality, then availability of credit in neighboring areas will drop by a greater amount and will amplify the effect of the initial foreclosure.

To further separate supply from demand-side components, in Figure 8, I illustrate the effect of foreclosures on neighboring mortgage inquiries. This field, taken from Equifax, is triggered when lenders access borrower credit files to verify creditworthiness, typically in this context of a nearby borrower applying for a refinancing. Mortgage inquiries on credit reports are highly predictive of subsequent prepayment (which generally indicates a mortgage refinancing). Lower mortgage inquiries in the aftermath of a neighboring foreclosure may thus indicate that borrowers show less interest in refinancing their mortgage.

[19] A downside of this measure is that it also includes early payment of the loan associated with the borrower prepaying and moving. Additionally, the sample of private-label securitized loans during this period typically faced difficulties in accessing refinancing markets throughout this period.

Panel A: Neighbor Responses on Mortgage Debt



Panel B: Neighbor Responses on Revolving Debt

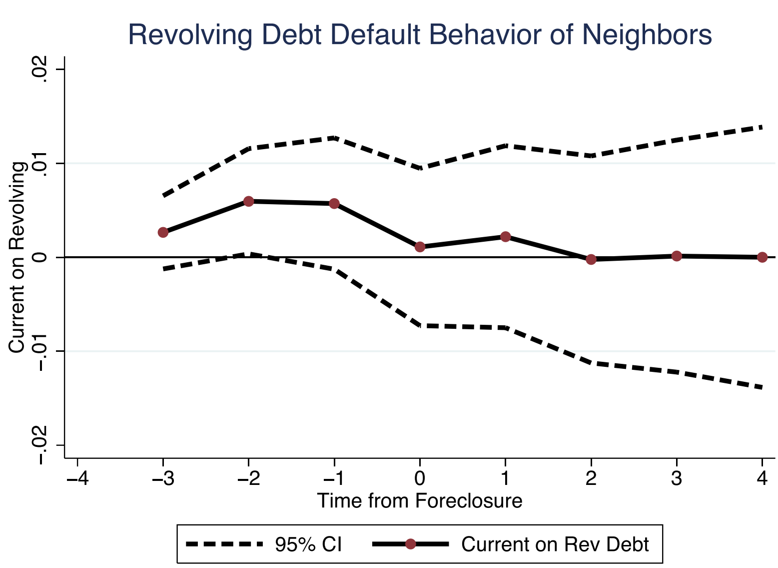


Figure 8. Mortgage inquiry responses to local foreclosures. This graph illustrates the outcome variable of mortgage inquiries, capturing changes in the demand for additional refinancing activity among neighboring borrowers. The sample includes BlackBox loans within 0.15 miles of a resetting property. Effect sizes are from an instrumental variable specification as illustrated as illustrated in the third column of Table II among all resetters, with a dependent variable of "mortgage inquiry" taken from Equifax. (Color figure can be viewed at wileyonlinelibrary.com)

Table IV

Spillover Effects on Loan Default

This table examines how within-month variation in interest rates, paid conditional on reset, predict future default behavior for neighbors around a resetting ARM for the following two years. Results are estimated both on a Deeds sample, for which default indicators are taken from a notice of default field, as well as on a BlackBox sample, for which default is taken based on payment information of other private label securitized loans. The specifications shown reflect the instrumental variable specification in which the first stage regresses within-month variation in interest rates against foreclosure rates of the resetting property, and the second stage uses the predicted value of foreclosure from these reset characteristics against household default in the 0.10-mile radius around the resetting ARM. Standard errors are clustered at the tract-year level.

	Notice of Default (2SLS)	Foreclosure Start (2SLS)	90+ DPD (2SLS)
All Resets	0.019	0.064	0.076
	(0.009)	(0.02)	(0.022)
N (Clusters)	1.17m (26k)	0.7m (54k)	0.7m (54k)
First-Stage *F*	505	548	548
Sample	Deeds	BBX	BBX
Avg of Dep Var	[0.076]	[0.19]	[0.25]

Instead, I find that neighboring credit inquiries appear roughly flat around the timing of neighboring foreclosures. Although there is a slight increase in mortgage inquiry levels in the quarters after foreclosure, this increase is not statistically significant from a year prior to the foreclosure. Observing that borrowers have no fewer mortgage inquiries on their credit reports, although refinancing activity decreases substantially, suggests that borrowers still *apply* for new credit at comparable rates, but lenders, who employ further credit checks, deny this new lending. In Section V.A, I explore the role of lenders in driving aggregate levels of lending activity in response to local foreclosures in a slightly different context.

C. Estimating Default Outcomes

A separate channel, which can also explain my results, is a neighboring peer effect channel whereby borrowers react to a local foreclosure by adjusting their own payment behavior. I report a range of results that illustrate the effects of local foreclosures on borrower behavior.

Table IV focuses on default outcomes among neighbors in the second stage while still using the foreclosure of the resetter in the first stage. Since foreclosure is a joint decision between borrowers and lenders, a potential concern is that spillover effects on foreclosures might be a lender-driven response. In column (1), I assess this channel using the notice of default as the dependent variable. A notice of default is a relatively automatic public notice delivered to borrowers stating that they are behind on payments and have some period of time to resume payment or lenders may initiate a foreclosure process. I find sizable effects of foreclosures of the resetting mortgage, as instrumented by reset characteristics, on neighbors' default decisions. However, since I do not have notice of default information for as many counties, the sample size is somewhat smaller and the lookback and index specifications do not reach statistical significance.

In column (2) of this table, I examine a variety of outcomes for the BlackBox sample. This sample consists of loans in the BlackBox data set that match to properties within 0.10 miles of a resetting ARM. While this sample is restricted in terms of coverage of loans present, with this sample I am able to control for a variety of additional loan characteristics. In particular, I control for the same characteristics as for the resetting loan, including the precise contract terms relating to reset (including whether the loan is an ARM, the exact interest rate, time from reset, as well as the index type and assigned lookback period), a variety of other loan-level details such as the combined current loan-to-value (including both first and second liens, with the home value adjusted to the monthly level using Zillow zip-code data), and the purpose and type of the loan. I also include additional borrower-level data such as lagged credit score and credit utilization. A key advantage of the BlackBox specification is that I am able to fully control for the same set of covariates for both resetter and local borrower, which allows me to control for any local correlation in lookback period or index choice.

The estimates suggest that completed foreclosures have a substantial effect on foreclosure start (a coefficient of 0.064) and delinquency outcomes (a coefficient of 0.076) among private-label securitized loans. These effects are sizable and isolate borrower delinquency, as opposed to a lender's decision to initiate foreclosure, as a key driver of foreclosure spillover effects. While the implied aggregated default estimates in this sample are quite large (i.e., the total spillover effects of foreclosure starts on the BlackBox sample suggest an additional 1.4 foreclosure starts in neighboring areas), these estimates entail a hypothetical neighborhood consisting of only subprime or jumbo-prime loans. Neighborhoods typically contain a mix of subprime and other borrowers. An advantage of calculations on the Deeds sample is that this sample consists of the universe of all neighboring borrowers, which allows for greater generalizability.

D. Timing of Default Responses and Nonmortgage Debts

In this section, I examine how repayment of mortgage debt varies around the timing of the local foreclosure using an event study framework. This analysis connects the precise payment decisions of borrowers to the foreclosure decisions of their neighbors. To evaluate neighbors' default decisions, I examine the payment behavior of neighbors who hold a mortgage in the BlackBox sample, restricting attention to those borrowers who have privately securitized mortgages. Specifically, I run the regression

$$O_{it} = \alpha + \mu_t + X_{it} + \sum_{k=-s}^{s-1} \mu_k \cdot 1[(t - T_i) = k] + \varepsilon_{it}, \tag{6}$$

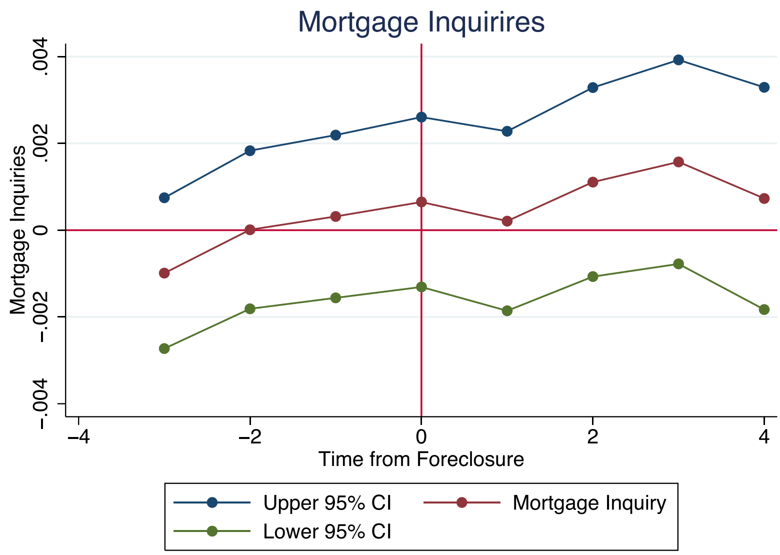


Figure 9. Effects by year. This graph illustrates the size of the spillover effects by year. Each point represents an instrumental variable regression as illustrated in the third row of the third column of column Table II among all resetters, broken out by the calendar year of reset. (Color figure can be viewed at wileyonlinelibrary.com)

where O_{it} is the outcome of making payments on either a mortgage or other revolving debt (e.g., credit cards). Fixed effects control for the month of observation, the zip code, and a variety of property-specific controls. The key coefficient of interest is μ_k, which measures the change in sale price of the house during s quarters prior to and following the timing of the foreclosure in quarter T_i. This sample is restricted to borrowers in the vicinity of a resetting ARM that experiences foreclosure. Standard errors are clustered at the census tract-year level.

Panel A of Figure 9 illustrates how borrower default patterns behave around the timing of the foreclosure completion of the resetting ARM by plotting the coefficients μ_k. I find that borrowers are less likely to be current the quarter prior to foreclosure completion, during which time the property is typically vacant and the foreclosure is visible to neighbors. Mortgage repayment rates continue to decline in the quarter in which the foreclosure is completed, and they flatten out thereafter.

The timing of the mortgage default highlights the role of specific channels and mechanisms driving my result, as discussed more broadly in Section II.D. The fact that neighboring responses appear to show up slightly before the foreclosure actually occurs suggests a role for information flows, peer effects, or expectations regarding home prices, since realized changes in home values should not appear at this point.

Panel B of Figure 9 illustrates borrower repayment on *non*mortgage debt as captured by payment status on revolving debt, which includes credit cards. In

Table V
Heterogenous Effects

This table investigates how within-month variation in interest rates, conditional on reset, predicts foreclosure behavior. The specification is the same as in the third row and column of the main table II, and uses the instrumental variable specification on all loans. Each row subsets on a different sample. The first four cuts subset on different characteristics of the neighbors; the last specification subsets on the equity position of the resetting mortgage. Properties are taken within a 0.10-mile radius of resetting ARM and standard errors are clustered at the tract-year level.

Dep Var: Foreclosure	Second Stage (2SLS) Coefficient	*N* (Clusters)	First-Stage *F*
Sample of:			
- FRM	0.019	2.6m (56k)	995
	(0.006)		
- Apartment	0.048	0.5m (19k)	112
	(0.015)		
- Underwater	0.03	0.6m (44k)	400
	(0.019)		
- Investor	0.024	0.9m (53k)	320
	(0.009)		
- Reseter Underwater	0.023	1.7m (32k)	480
	(0.007)		

contrast to Panel A, Panel B shows no impact of the neighboring foreclosure on the payment status of nonmortgage debts. This powerful result helps rule out the possibility that borrowers are simply hit by some sort of aggregate shock that leads to generalized debt nonpayment. Instead, the product-specific aspect of mortgage nonpayment suggests that a real estate-specific channel drives my results. As discussed in Section II.D, this suggests a role either for changes in home prices (real or perceived), local information flows on the value of the default option, or changes in attitudes toward default.

E. The Pattern of Peer Strategic Responses

Table V reexamines my main specification along several dimensions. I follow the IV approach as outlined in column (3) of Table II, using all of the within-month variation in interest rates and focusing on a variety of samples. First, I find that my results persist when examining only those neighbors with fixed-rate mortgages. This specification avoids potential pitfalls relating to correlation in loan contracting terms across neighbors, as it is difficult to think of a reason why contract decisions resulting in different interest rates for adjustable-rate borrowers should have any effect on neighbors with fixed-rate contracts. The effects remain sizable among borrowers with fixed-rate contracts—I find an increase in the future two-year foreclosure probability of 1.9%, which corresponds to a cumulative effect of an additional 0.42 foreclosures in a hypothetical neighborhood consisting of only fixed-rate mortgages.

I also find larger effects among units in apartments and sizable effects on investor properties.[20] I find a very large 3% increase in foreclosure among underwater properties, although this specification is run only on the BlackBox sample and does not reach statistical significance.

While failing to repay bills seems an intuitive option, the household financial literature documents a relative unwillingness on the part of households to exercise their default option when doing so would improve household prospects. For instance, Fay, Hurst, and White (2002) find that a substantial fraction of households for which it would be financially profitable to declare bankruptcy choose not to do so. Among mortgage holders, the majority of those facing negative equity (i.e., owing more on their mortgage than it is worth) continue to make mortgage payments, even though it would be financially profitable for them to cease payments and walk away from the property.

To be sure, the decision to continue making payments on an underwater property is a complicated one involving a variety of other factors. Borrower repayment may be rationalized under a dynamic model incorporating the future credit costs of default, the local amenity values of the neighborhood, transaction costs in finding a new residence, or a variety of other factors (although the prospect of being able to live rent-free in the property subsequent to default is a factor which should encourage earlier default). Nevertheless, it seems likely that many borrowers are making decisions to continue mortgage payments that may be narrowly unprofitable from the perspective of maximizing the default option on the property.

In this context, my preferred interpretation is that default spillovers are high among borrowers for whom it is profitable to default and who have been previously dissuaded from defaulting for reasons of social stigma or misperception regarding the viability of the default option. The physical presence of a local foreclosure appears to trigger additional defaults among these borrowers.

Figure 10 illustrates default spillover effects by the year of observation. I focus on the year of ARM reset and follow the subsequent two-year foreclosure probability of neighboring loans using the baseline IV specification. Here, I employ all loans and examine variation induced by the reset amount. I find that spillover effects are fairly stable over the period 2006 to 2010, and I cannot reject the null that they are all the same.

Figure 11 illustrates default spillover effects by distance from the resetting property. In this analysis, I use a sample of BlackBox loans within 0.15 miles of a resetting loan and compute the baseline IV specification as illustrated in column (3) of Table II for properties at various distances away from the resetting loan. The estimates show a roughly linear relationship between distance from the resetting loan and the size of the foreclosure spillover effect. Among properties 0.10 to 0.15 miles from the resetting loan, the size of the spillover effect is no longer statistically significant.

Table VI reports evidence on spillover effects ordered by the intensity of

[20] Investor status is identified by contrasting the address field in Deeds records across the mailing address where tax bills are sent and the property address as in Chinco and Mayer (2016).

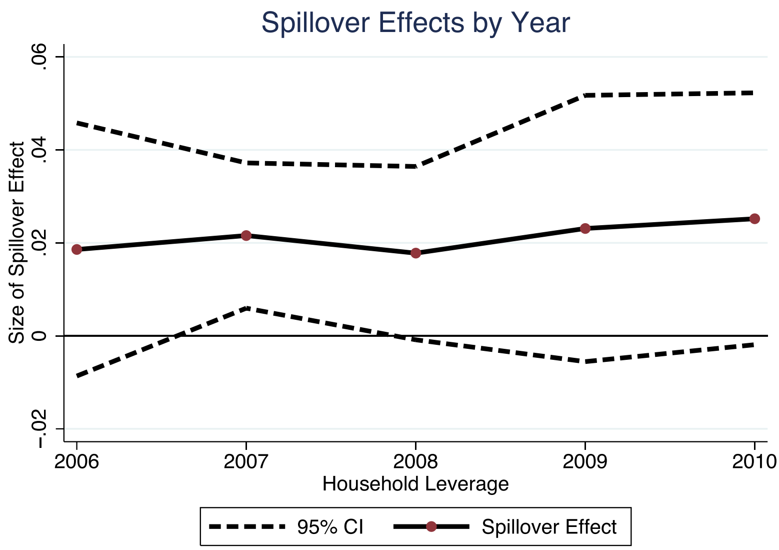


Figure 10. Effects by distance. This graph illustrates the size of the spillover effects by distance from the resetting loan. The sample includes BlackBox loans within 0.15 miles of a resetting property. Effect sizes are from an instrumental variable specification as illustrated as illustrated in the third column of Table II among all resetters, broken out by different levels of distance from the resetting loan. (Color figure can be viewed at wileyonlinelibrary.com)

prior foreclosures in the area. I find that foreclosure spillovers are strongest in areas that have experienced zero foreclosures or only one foreclosure within a broader radius (0.25 miles) in the previous two years. These results suggest that foreclosure spillovers are driven by learning about the cost of default, which manifests most strongly upon the initial set of foreclosures. An alternate hypothesis is that foreclosures impact peer responses by affecting the stigma attached to repaying debts, which might suggest that an increasing wave of local foreclosures is required to induce other borrowers to repay. I emphasize that the standard errors are large in this context and that I am not able to rule out a more S-shaped pattern of foreclosure spillovers. However, the point estimates I find are most consistent with a peer effect channel, which strongly weighs on the effect of initial local foreclosures.

The overall pattern suggested by these separate cuts is consistent with a neighboring peer default channel. I show that the default response to a local foreclosure (i) is linked to borrower decisions to cease debt payments, (ii) is linked to the timing of serious delinquency on neighboring properties, (iii) does not entail delinquency on nonmortgage debts, (iv) is highest among underwater properties, (v) is closely tied to the geography of foreclosed properties at a very tight radius, and (vi) is highest in areas that have previously experienced little foreclosure activity. These results together suggest a responsive default motive on the part of local borrowers, who learn from local foreclosure activity about

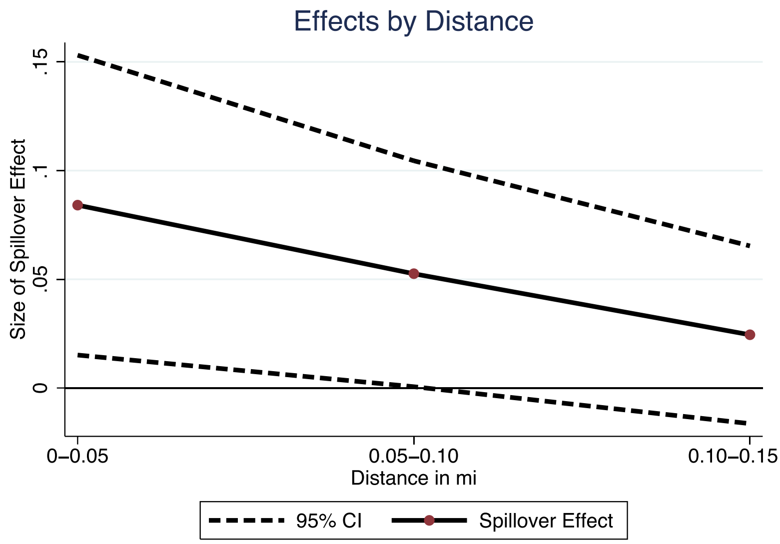


Figure 11. Timing of neighbor default responses. This graph illustrates the coefficients from a regression of neighbor mortgage default behavior among mortgages in the neighborhood of a resetting adjustable-rate loan which experiences a foreclosure. Mortgage default information is taken from BlackBox; revolving debt default information is taken from Equifax and both are only included for private-label securitized loans in the neighborhood of the resetting loan. I run the regression: $O_{it} = \alpha + \mu_t + X_{it} + \sum_{k=-s}^{s-1} \mu_k \cdot 1[(t - T_i) = k]\varepsilon_{it}$, where O_{it} is the outcome of making payments on either a mortgage or other revolving debt (i.e., credit cards), fixed effects control for the month of observation, the zipcode, and a variety of property-specific controls. The key coefficients of interests are μ_k, which measure the change in sale price of the house during s quarters prior to and following the timing of the foreclosure in quarter T_i. Standard errors are clustered at the tract-year level. Panel A illustrates the propensity to make payments on mortgages as a function of time from the foreclosure of the resetting loan. Panel B illustrates payment behavior on revolving debt as a function of time from the foreclosure of a resetting adjustable-rate loan. (Color figure can be viewed at wileyonlinelibrary.com)

the cost of default and cease debt repayment on their mortgage when it is profitable to do so.

IV. Discussion: Macroeconomic Channels

In the previous sections, I employ micro-level data to evaluate the effects of foreclosure spillovers on local outcomes. However, many of the effects of resets and foreclosures may aggregate and impact broader areas. In this section, I conduct two sets of tests to better understand the mechanisms and channels through which foreclosure contagion affects borrowers.

I first investigate the role of foreclosure activity on refinancing denial. While tests above explore the role of foreclosures on refinancing activity in very local areas, in this section, I use HMDA data to examine broader consequences of foreclosure activity on refinancing denial and credit shutdowns to isolate the role of bank reductions in credit. Next, I analyze the aggregate effects of reset

Table VI
Effects by Prior Foreclosures

This table highlights the size of the foreclosure spillover effect by the intensity of previous foreclosures. Effect sizes are from an instrumental variable specification as illustrated as illustrated in the third column of Table II among all resetters, broken out into different subsets based on the the number of previous foreclosures in a 0.25-mile radius around the resetting loan in the previous two years.

Dep Var: Foreclosure	Second Stage (2SLS)	*N*	First-Stage *F*
Prior 0	0.019 (.009)	2.4m (27k)	451
Prior 1	0.023 (.012)	0.7m (20k)	198
Prior 2	0.01 (0.018)	0.4m (14k)	76
Prior 3	0.010 (0.021)	0.3m (10k)	51
Prior 4	−0.01 (0.026)	0.2m (8k)	52

intensity on foreclosure activity in general to shed light on the aggregate role of mortgage payment shocks on default rates.

A. The Refinancing Denial Channel

As discussed in Section I.C, the impact of foreclosures on neighboring refinancing activity can be understood as a consequence of lender behavior in response to foreclosure activity. Banks are likely to respond to local foreclosure activity by cutting the supply of new credit, whether the result of a purely rational underwriting process (since foreclosures may signal lower prices or greater default inclinations among nearby residents) or the result of using the foreclosure fire-sale as an input in the appraisal process. The shutdown of mortgage credit happened in an environment in which interest rates were trending downwards overall, and some borrowers had teaser rates with reset triggers that left them facing payment shocks absent a refinanced loan. The refinancing channel was thus a contributing factor behind the externalities of foreclosures in neighboring loans.

Previous results in Section III.C suggest that one consequence of greater foreclosure activity is reduced refinancing activity in the local area, which is consistent with this channel. Additional supporting evidence comes from results on nonmortgage delinquencies, which did not increase in neighboring areas, indicating that attitudes toward default were concentrated among mortgage products, and results on mortgage inquiries, which did not drop, suggesting that borrowers did not display less interest in refinancing in the aftermath of a nearby foreclosures. However, these results cannot completely rule out the channel through which the decline in refinancing activity is driven by the decisions of borrowers rather than lenders.

In this section, I provide complementary evidence to support the role of the refinancing denial channel. To do so, I use separate data from the HMDA, which contain information on mortgage applications as well as originations, and thus allow me to understand bank decisions in the context of mortgage denials. Unfortunately, the data lack sufficient identifiers to be merged into my core data set, but I am able to analyze mortgage denials at the tract level as a function of tract-level foreclosures.

Figure 12 shows graphically the relationship between foreclosure activity (as measured by the Department of Housing and Urban Development (HUD) in the period from 2007 to the first half of 2008) and refinancing denials (as measured in HMDA) at the tract level. The strong positive correlation is suggestive of a link between lender actions on credit denials and local foreclosure activity.

I explore this relationship in a regression context in Table VII, reproduced below, which regresses foreclosure activity against credit denial at the tract level. The main specification is column (3), which can be interpreted as the impact of local foreclosure activity on refinancing credit denial rates, controlling for local price and employment trends. The results indicate that an additional standard deviation of local foreclosure activity is associated with either 0.57 standard deviations (in the overlapping sample of 2008) or 0.46 standard deviations (in the subsequent year, 2009) of refinancing denial.

Columns (4) and (5) explore reasons for credit denial and regress deviations from the mean of the frequency with which "Denied for collateral" and "Denied for credit history" are reported in HMDA as the reason for refinancing denial. The results suggest that both worsening collateral and credit history are reasons for decreased credit supply in high-foreclosure areas. While credit history denials may be indicative of other local reasons for a decline in borrower creditworthiness, refinancing denials for collateral reasons suggests that a decline in perceived home quality is also a reason for credit denials in high-foreclosure regions.

While these results cannot be interpreted purely causally, they do suggest a strong relationship between credit denials and prior foreclosure activity as measured at a highly local granularity. The role of denials for collateral reasons is suggestive of a link between local foreclosure activity and the perceived value of local housing collateral as a driver of lending decisions.

Because I am unable to connect my key instrument to HMDA data, this section is more descriptive than the previous well-identified results. However, I believe that the pattern of results in this context help illustrate the broader role for refinancing denials as a propagation mechanism during the financial crisis. I document (i) the effect that local foreclosure activity has on neighboring refinancing volume, (ii) the fact that local borrowers appear to exhibit no less desire to try to refinance their properties, as measured by subsequent credit inquiries on their account, and (iii) the fact that aggregate refinancing denials

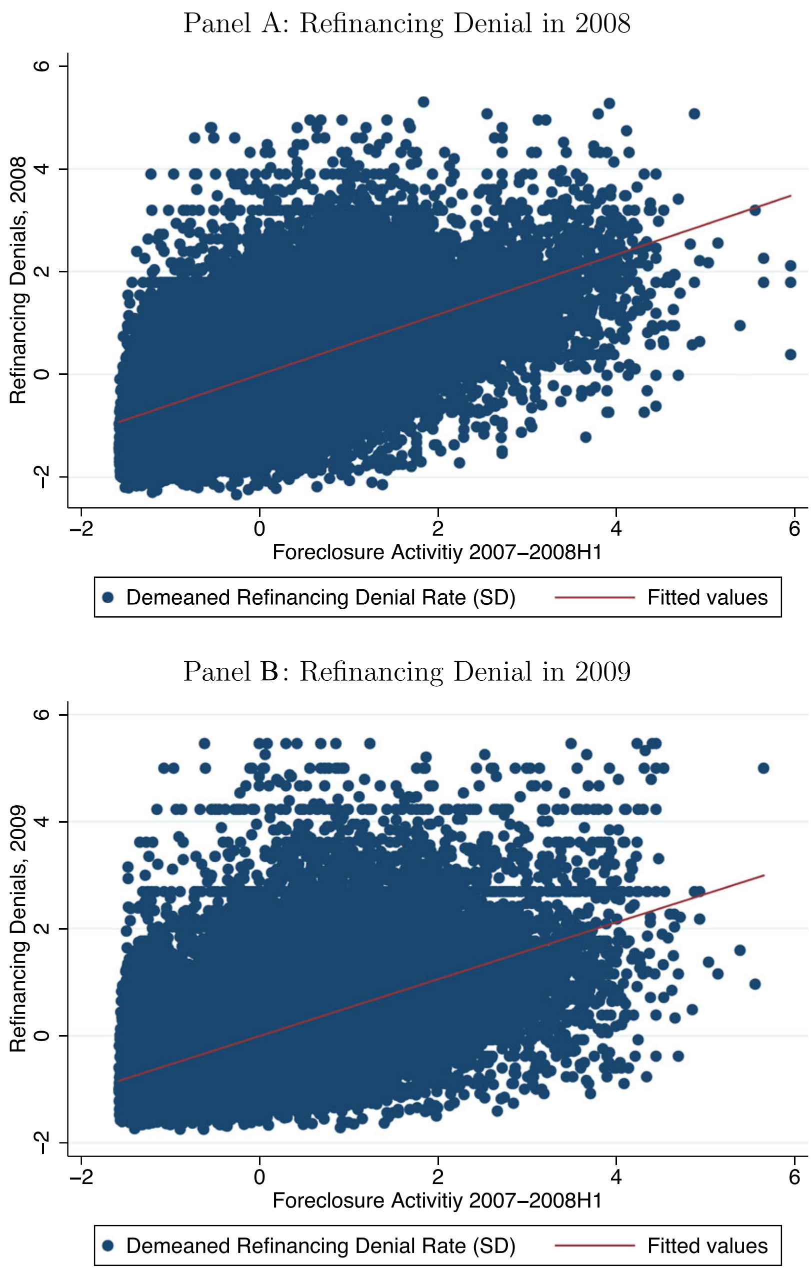


Figure 12. Aggregate estimates of refinancing denial and foreclosure activity. This graph illustrates the relationship between foreclosure activity and refinancing denials as measured at the tract level. Foreclosure activity is measured from HUD over the period 2007 to the first half of 2008. Refinancing denial is measured in the HMDA data in both 2008 (Panel A) and 2009 (Panel B). A fitted linear regression illustrates the positive relationship between local foreclosure activity and subsequent credit denials. (Color figure can be viewed at wileyonlinelibrary.com)

Table VII

Aggregate Estimates of Foreclosure Activity and Refinancing Denial

This table examines the aggregate consequences of foreclosure activity on credit denial at a more aggregate level. Foreclosure activity is measured at the tract level from HUD data over the period 2007 to first half of 2008, and is reported as standard deviations from the mean. Credit denial is measured at the tract level using HMDA data in either 2008 (Panel A) or 2009 (Panel B), and is also expressed as standard deviations from the mean. Columns 2 and 3 focus on refinancing denials; while columns 4 and 5 explore some reasons for credit denial. Additional controls include home price changes from 2007 (at the tract level) and unemployment (at the MSA level).

Panel A: Refinancing Denial in 2008

	(1)	(2)	(3)	(4)	(5)
Sample:	Purchase	Refinancing	Refinancing	Refinancing	Refinancing
Dependent Variable:	Denied	Denied	Denied	Denied for Collateral	Denied for Credit History
Foreclosure Rate	0.59**	0.57**	0.58**	0.33**	0.34**
	(106.92)	(113.20)	(133.64)	(66.46)	(80.46)
Unemployment			Yes	Yes	Yes
Δ Home Prices			Yes	Yes	Yes
N. of cases	65,199	64,881	30,901	30,901	30,901
R^2	0.34	0.32	0.44	0.24	0.20

Panel B: Refinancing Denial in 2009

	(1)	(2)	(3)	(4)	(5)
Sample:	Purchase	Refinancing	Refinancing	Refinancing	Refinancing
Dependent Variable:	Denied	Denied	Denied	Denied for Collateral	Denied for Credit History
Foreclosure Rate	0.49**	0.50**	0.46**	0.32**	0.31**
	(90.17)	(91.40)	(115.42)	(65.08)	(81.75)
Unemployment			Yes	Yes	Yes
Δ Home Prices			Yes	Yes	Yes
N. of cases	65,061	64,773	31,232	31,232	31,232
R^2	0.24	0.25	0.44	0.32	0.25

Marginal effects; *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. ** and *** denote significance at 5% and 1% levels, respectively.

react strongly to local foreclosure activity, even when controlling for local prices and unemployment rates.

B. Aggregate Effects of Reset Intensity and Foreclosures

In previous sections, I focus on micro-level data to evaluate the effects of foreclosure spillovers on local outcomes. However, many of the effects of resets and foreclosures can be expected to aggregate and impact broader areas. While identification is more difficult when examining these aggregate effects, a key motivation in understanding foreclosure spillovers is evaluating the broader macroeconomic consequences of household default decisions. The aggregation of externalities resulting from foreclosure presents a possible motivation for a variety of federal debt relief programs—including federal assistance in mortgage modification, refinancing, as well as the central bank's choice of interest rates, which directly feeds into the payment decisions of ARM holders.

I analyze the aggregate effects of foreclosures by focusing on regional variation in the fraction of local resetting ARMs. Since interest rates paid at the time of reset can be either higher or lower than the original rate, and even interest rate shocks may not be binding for borrowers with equity, I focus on what I refer to as double-trigger ARMs: those facing both an interest rate hike at the time of reset and close to negative equity at the time of reset (i.e., a cumulative loan-to-value ratio of at least 90). Borrowers resetting under these conditions face both a payment shock in meeting the higher loan payment subsequent to reset, and, due to their lack of equity, a payment constraint that is more likely to be binding, since refinancing is more difficult for borrowers with this amount of leverage.

First, I categorize counties and zip codes along an index by the quarterly count of private-label mortgages facing a double-trigger reset. Over my sample, there is sizable variation in local geographies facing resetting ARM shocks. The variation is driven by a combination of different originations in these product categories, different initial teaser length durations resulting in changes in timing of when initial resets occur, different interest rates inducing mortgages to reset under conditions of high interest rates, different leverage choices among households, and prereset changes in house prices, which also affect the loan-to-value calculation.

If foreclosures have important aggregate consequences, geographic areas experiencing high exposure to double-trigger resets should experience greater foreclosures resulting from these resets, which may also impact local house prices and foreclosures among nonresetting mortgages in local areas. The presence of externalities resulting from mortgage default acts as an amplification mechanism of the initial reset shock inducing more foreclosures in areas.

To test this hypothesis, I regress the quarterly count of resetting ARMs against aggregate counts of foreclosures and price changes in the following quarter. If the external effects of foreclosures are large, I expect areas with a greater intensity of double-trigger default to experience more foreclosures and

Table VIII

Broader Macroeconomic Effects

This table illustrates the aggregate impact of mortgage resets. I identify "double-trigger" resets as those which face (i) an upward adjustment in interest rates at the time of reset and (ii) conditions of near-negative equity (current combined loan to value of at least 90). Mortgage holders facing such conditions faced a combination of payment shock, difficulty in accessing financial markets for refinancing, and relatively high value of exercising their mortgage default option for strategic reasons—inducing these borrowers to default in greater proportion. I aggregate the number of such double trigger resetters in each quarter of observation among zip codes and counties, and regress a one-standard deviation rise in double-trigger reset intensity against foreclosure and price impacts in the zip code or county the following quarter. I control for zipcode or county fixed effects, quarter of observation fixed effects, and cluster standard errors at the county or zipcode level.

	Price	Foreclosure	Price	Foreclosure
1 *SD* in Double Trigger Resets	-0.0029^{***} (0.0008)	552^{***} (73)	-0.0033^{***} (0.00016)	11^{***} (0.61)
Level:	County	County	Zip	Zip
Avg of Dep Var:	−0.0064	1330	−0.0067	27
N (Clusters)	746 (54)	1,026 (54)	35k (2527)	48k (2529)

** and *** denote significance at 5% and 1% levels, respectively.

greater price declines in subsequent quarters. I run the following specification:

$$O_{it} = \mu_{it} + \beta \cdot DT_{i,t-1} + \varepsilon_{it}, \tag{7}$$

where i indexes either the zip code or county, t indicates the quarter of observation, O_{it} is the outcome variable in question (either aggregate foreclosures or the local price changes), and $DT_{i,t-1}$ is the count of local double-trigger ARM resets, normalized by subtracting the mean and dividing by the standard deviation. The coefficient β measures the effect that a one-standard-deviation increase in double-trigger resets in a quarter has on price changes and aggregate foreclosures the following quarter. To account for other changes in the economic environment, I include dummies for quarter of observation and fixed effects for the zip code or county, that is, I am not exploiting the fact that certain areas simply originated a greater fraction of ARMs than others. I use only the variation in the *timing* of those resets and their severity as captured by resets that face upward interest rate shocks in conditions of negative equity. Standard errors are clustered at either the county or zip-code level.

Table VIII reports evidence consistent with the hypothesis that foreclosure contagion has macroeconomic effects observable at the zip-code or county level. A one-standard-deviation increase in the relative frequency of double-trigger resets results in around a 0.3% drop in house price measured at the zip-code or county level, relative to a mean of an approximately 0.6% drop in house price. A one-standard-deviation increase in double-trigger rests leads to 11 additional foreclosures at the zip-code level the following quarter (relative to a mean of

Table IX
Placebo Test: Prior Foreclosures

This table performs a comparable instrumental variable analysis of reset characteristics on foreclosures as in the main analysis—but uses as a dependent variable in the second stage *prior* foreclosures among neighboring loans in the two years prior to reset. If the identification strategy is valid, within-month variation in interest rates should not predict foreclosures in the period prior to ARM reset.

	BBX Sample	Deeds Sample
All Resets	−0.00007	−0.0005
	(0.0027)	(0.0001)
N (Clusters)	1.9m (57k)	4.4m (58k)

27), or an additional 552 foreclosures at the county level (relative to a mean of 1,330).

These effects are economically sizable and suggest a substantial role for foreclosure externalities in amplifying the effects of an initial set of foreclosures into larger economic losses for geographies affected by a greater-than-average number of adverse ARM resets. To be sure, this analysis lacks the tight identification of the microeconomic approach above, which relies on within-month variation in interest rates paid upon the time of reset. While I am able to control for a range of economic conditions through time and geography fixed effects, areas experiencing a wave of adverse reset shocks may face other negative economic shocks at the same time. As such, these results should be seen as a descriptive illustration of the potential magnitude of the size of the aggregate consequences of foreclosure spillovers.

V. Robustness

A. Placebo Test: Prior Foreclosures

A key identifying assumption is that within-month variation in interest rates, conditional on other observables (such as zip code and characteristics of both resetting and neighboring mortgages) have no impact on neighboring mortgage default patterns *except through* the channel of foreclosures on the resetting ARM. While this assumption cannot be tested directly, I provide some evidence in its support. One such test is in Figure 10, which shows that default spillover effects are concentrated among loans within a very small radius directly surrounding the resetting loans. While it is likely that mortgage holders are commonly exposed to local shocks, for instance, the closure of a local plant, it is more difficult to imagine real economic shocks that are correlated with within-month variation in interest rates as induced by the lookback period or index type, but only at extremely small distances around resetting ARMs.

Another robustness check is in Table IX, which performs a similar regression as in the main specification, but with the key dependent variable being foreclosure status among loans neighboring the resetting ARM in the two years *prior* to the resetting ARM. This serves as a placebo check of the main analysis—if the instrument is valid, it should not predict foreclosures prior to the time when the resetting ARM is actually paying the differential interest rates. Table IX provides evidence consistent with this intuition. Column (1) reports estimates for the BlackBox sample of loans neighboring the resetting ARM; column (2) reports evidence for the full Deeds sample. Estimates of the instrumented peer effect are insignificant for the BlackBox sample and are significant but negative and very small for the Deeds sample (−0.0005). If anything, the estimates from the timing regressions suggest that the effect size may be slightly underestimated due to the assignment of treatment in areas with a slightly lower propensity for foreclosure.

VI. Conclusion

In this paper, I shed light on an important mechanism behind the massive wave of foreclosures experienced during the recent financial crisis: default spillovers and peer effects. In studying foreclosure externalities, I employ two novel instruments influencing foreclosures: the choice of lookback period and the choice of financial index. I argue that these commonly overlooked features of ARM contracts allow for a quasi-experimental setting in which mortgages resetting in the same month are shocked at different interest rates for reasons that are highly unlikely ex ante to be closely related to default decisions of neighbors.

Consistent with prior literature on ARM resets, I first establish that the within-month variation in interest rates among resetting ARMs predicts default and foreclosure behavior among resetting loans. Next, I use the resulting spike in foreclosure rates among loans facing relatively higher interest rates (which are fixed for a period of 6 to 12 months) as a local shock to the foreclosure intensity experienced by their geographical neighbors. I find compelling evidence for the existence of foreclosure spillover effects: each foreclosure leads to an additional 0.3 to 0.5 foreclosures in the 0.10-mile radius around a foreclosed property. These effects are economically large and point to foreclosures' large peer effects.

Consistent with other prior literature, I find that foreclosures lead to drops in reported prices among neighboring homes, which can explain a portion of my result. In addition to the price channel, I examine the role of a refinancing channel. I find that refinancing activity drops by a third in areas around foreclosures, potentially due to mortgage originators using local distressed sales in appraisal decisions. Consistent with this interpretation, I find that mortgage inquiries do not decrease in the aftermath of foreclosure sales, suggesting that demand for credit among local borrowers remains stable. However, in examining more aggregated data at the tract level, I find that foreclosure activity results in substantial increases in loan denials by lenders. These results together highlight the role of a novel refinancing channel through which credit supply falls in foreclosure-prone areas.

I also emphasize the role of informational and learning features of nearby mortgages. The presence of a nearby foreclosure provides local residents with important information both on the quality of the neighborhood as well as the viability of mortgage default. Consistent with this interpretation, I document that foreclosure spillovers are (i) concentrated in a tight geography around foreclosed properties, (ii) highest among underwater properties, (iii) do not manifest in defaults on nonmortgage debts, and (iv) highest in areas that have previously not experienced a foreclosure. This pattern is most consistent with an informational view of default in which borrowers strategically default in reaction to local foreclosures after learning about the cost of default in response to a visible nearby foreclosure.

Finally, I document other regional consequences of mortgage defaults. I focus on ARMs that reset under adverse circumstances—negative equity and upward rate resets—that are strongly predictive of future delinquency. In the presence of spillover effects, these defaults should impact borrowers in these areas more broadly. I document that zip codes and counties with a greater prevalence of adverse mortgage resets experience greater price drops and more foreclosures in the following quarter. These results provide complementary evidence on mortgage resets in connection with prior work, which documents positive effects of rate *decreases* among resetting ARMs on mortgage performance and local economic outcomes. While I am primarily interested in mortgage resets as an instrument to understand mortgage resets in general, my results also highlight the nature of interest rate–linked mortgage contracts as a key aspect of the monetary policy transmission mechanism.

Overall, these results highlight an important amplification channel associated with lender and borrower responses to defaults that resemble a traditional bank run. ARMs in particular can be seen as a form of repeated short-term borrowing, as borrowers typically refinance at the end of their teaser period. Continued repayment rates among loans featuring these contract details was predicated on the availability of continued access to market credit. When ARMs reset under conditions of low prices, however, refinancing is a challenge and borrowers facing payment shocks often default. To the extent that such defaults triggered price decreases and a reduction in refinancing activity among neighboring homes, they led to a cascading wave of additional defaults and further price drops in surrounding areas. In aggregate, the dynamics of peer interactions in the residential mortgage market may help to explain how an initially small shock to the mortgage market grew to a much greater shock on households broadly, and potentially point to important roles for public policy aimed at addressing foreclosures.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication code.