Foreclosure Contagion: Measurement and Mechanisms [1]

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Abstract

In this paper we ask whether foreclosures are contagious: does a completed foreclosure cause neighboring foreclosure filings? We estimate this relationship using administrative data on home foreclosures and sales in Cook County, IL, and instrument a completed foreclosure using random assignment of chancery-court judges. We find that a completed foreclosure causes between 0.5 and 0.7 additional filings within 0.1 miles, an effect that persists for several years. We interpret our results as evidence that contagion is driven by borrowers (rather than lenders). Moreover, contagion is not driven by borrowers in negative equity, but by borrowers learning from neighboring foreclosures. We also find that foreclosure disrupts local housing markets, increasing sales of neighboring lower-quality properties.

1 Introduction

The housing bubble and crisis of the last decade has resulted in an unusually large number of foreclosures in the United States. Completed foreclosures—when a mortgage borrower does not make payments on their loan and the lending institution claims the mortgaged property—increased dramatically starting in 2007 from 404,849 properties per year, peaking at 1.05 million completed foreclosures in 2010.[1] The length and severity of this crisis has increased academic interest in the consequences of home foreclosures and have raised questions about how and why foreclosures spread.

In this paper we ask whether home foreclosures are contagious: does one completed foreclosure increase the probability that geographically neighboring borrowers end up in the foreclosure process? The answer to this question informs our understanding of home foreclosures, borrower and lender behavior, and policy toward mortgages and foreclosure procedures. Foreclosure contagion is suspected of exacerbating the housing crises during the Great Depression and the recent financial crisis (Campbell (2013)). Identifying contagion in foreclosures will provide a better understanding of how and why such crises spread. Furthermore, the presence of contagion is relevant to policy makers concerned with mitigating the spread of home foreclosures.

Our chief contribution is to develop a randomly assigned instrument for foreclosures, which we apply to administrative data to achieve credible, policy-relevant estimates of foreclosure contagion. In Cook County, IL, where foreclosures cases are decided in court, we use the randomization of new foreclosure cases to fixed groups of judges as an instrument for a completed foreclosure. Intuitively, our estimates compare the neighborhoods around two types of properties going through the foreclosure process (i.e., situations in which a borrower is in default and the lender wants to claim the home as collateral): properties randomly assigned to "difficult" judges that, as a result, end up being foreclosed upon and sold at auction versus properties randomly assigned to "lenient" judges that dismiss the foreclosure case. Since our empirical strategy necessarily relies on the comparison of neighborhoods around homes in default that do and do not end in foreclosure, our estimates speak directly to the policy question of how strongly lenders should be incentivized to renegotiate delinquent loans.[2]

We develop a novel data set that matches administrative records of foreclosure *court cases* to

[1] We use "foreclosure filing" to refer to the initiation of the foreclosure process by the lender, and "completed foreclosure" to refer to a foreclosure proceeding ending with the mortgaged property being sold at auction. However, lenders are not always successful in foreclosing on a home, and so not all filings end in completed foreclosure—we refer to such unsuccessful foreclosure attempts as "dismissals."

[2] There is a developed literature that uses judicial bias as an instrument, as we do herein, including: Kling (2006) (sentencing propensities of judges to instrument for incarceration length); Autor and Houseman (2010) (job placement rates of non-profit contractors to instrument for receiving temporary help jobs); Chang and Schoar (2006) (judicial fixed effects to measure judge-debtor-friendliness); Dobbie and Song (2013) (judge discharge rates to instrument for bankruptcy protection); Doyle (2007) (placement frequency of child protection investigators to instrument for foster care); French and Song (2011) (allowance frequency of administrative law judges to instrument for receipt of disability insurance); Maestas et al. (2013) (allowance rates of disability examiners to instrument disability insurance receipt); and Aizer and Doyle (2011) (incarceration tendency of randomly-assigned judges to instrument for juvenile incarceration).

records on foreclosure *filings and auctions* for Cook County. This county, which contains most of the city of Chicago, is the second-most populous county in the U.S. and was relatively hard hit by the housing crisis: the surrounding MSA experienced the 12th largest decline in city-wide housing prices between 2007 and 2011, while 5.2% of the 1.9 million households in Cook County experienced a completed foreclosure. Our data covers the universe of foreclosure filings and completed foreclosures in Cook County between 2004 and 2011, allowing us to leverage the random assignment of foreclosure judges while observing the precise location of the associated property. We also use administrative data on residential housing sales to assess whether a completed home foreclosure lowers neighboring housing values.

Concrete evidence on foreclosure-related externalities and contagion has been elusive, owing to empirical challenges (Frame (2010)). Home foreclosures are known to be correlated with neighborhood characteristics and changes in housing prices and macroeconomic circumstances (Mian and Sufi (2009); Mian et al. (2011)). Existing studies that find negative housing price effects of foreclosure have relied primarily on local analyses that explicitly control for property and neighborhood characteristics (Campbell et al. (2011), Immergluck and Smith (2006), Schuetz et al. (2008), Pennington-Cross (2006), Leonard and Murdoch (2009), and Lin et al. (2007)) or repeat-sales analyses (Harding et al. (2009) and Gerardi et al. (2012)). Similarly, existing studies finding evidence of foreclosure contagion rely either on local analyses (Towe and Lawley (2013)) or aggregate analyses controlling for neighborhood and zip code characteristics (Goodstein and Lee (2010)). Few studies have taken a quasi-experimental approach to identifying the externalities associated with foreclosure.[3] Ours is the first study to use a randomly assigned instrument to estimate contagion and local price effects of home foreclosures.[4]

Using our instrumental variables strategy to compare neighborhoods with completed foreclosures to neighborhoods where foreclosure cases are dismissed, we find evidence of foreclosure contagion. Relative to dismissal of a foreclosure case, a completed foreclosure raises the probability of any new foreclosure filing within 0.1 miles by 10% per year and leads to about 0.5 new filings per year. We find that this foreclosure contagion effect is persistent, lasting for three to four years after the case is decided. Additionally, our estimates show that substantial contagion in foreclosure filings occurs even in neighborhoods with no recent foreclosures—the "first" completed foreclosure in a neighborhood substantially increases foreclosure filings in the following years. Moreover, our results demonstrate that contagion is not limited to new foreclosure filings—we also observe an increase in neighboring completed foreclosures, suggesting that contagion is costly and plays a role

[3] One exception is Mian et al. (2012) who exploit changes at state borders in policy toward foreclosure (judicial vs. non-judicial states) to instrument foreclosures, although this instrument is not randomly assigned (for example, Pence (2006) shows substantial changes in housing market conditions at the boundaries between judicial and non-judicial foreclosure states).

[4] There is a related line of research that focuses on non-pecuniary costs of foreclosure, such as whether foreclosures cause crime (Cui (2010); Goodstein and Lee (2010); Ellen et al. (2013)) or harm health (Currie and Tekin (2011)). Additionally, several existing studies address the question of own-price discounts of home foreclosures. Campbell et al. (2011) find a substantial discount for the sale of foreclosed properties (as well as other types of distressed sales), while Harding et al. (2012) argue that the observed foreclosure discount is a fair price for those properties—investors who purchase real-estate owned properties are not reaping unusually large profits.

in the geographic spread of foreclosures. Lastly, we find that a completed foreclosure influences the effort that neighboring borrowers and lenders put into ongoing foreclosure proceedings: a completed foreclosure increases the number of completed foreclosures even among neighboring cases that began prior to the event.

While we find evidence that a completed foreclosure lowers neighboring residential sale prices, our estimates are largely driven by selection into sale. Within the first year of a case ending in a foreclosure, *relative to a case that ends in dismissal*, the average price of neighboring housing sales drops by up to 40%. However, using a repeat-sales methodology to adjust for property quality, our estimates of this effect fall substantially. We interpret this as evidence that a completed foreclosure disrupts the housing market in terms of the types of homes that sell, causing a larger share of lower-quality homes to transact at correspondingly lower prices than the average home in the neighborhood. At the same time, due to the small size of our housing sales sample and the resulting imprecision of our housing price estimates, we cannot rule out a negative effect of completed foreclosure on neighboring home values (holding quality constant).

We find evidence consistent with the commonly held belief that foreclosure contagion is driven by an increase in borrowers defaulting on their loans in response to a neighboring foreclosure, rather than lenders filing for foreclosure against already delinquent borrowers. There is substantial evidence that lenders and mortgage servicers—third parties employed by creditors to manage loans—indiscriminately favored pursuing foreclosure on delinquent mortgages, rather than modification (see discussions in Adelino et al. (2009); Foote et al. (2008); Levitin and Twomey (2011)). We argue that in the absence of borrower-driven contagion, lenders would not exhibit positive foreclosure contagion. Given that we do observe positive contagion provides evidence that borrowers respond. Moreover, we find substantial contagion even among mortgages serviced by lenders known for automating foreclosure procedures and who are, thus, unlikely to respond to very local market conditions.

There are two prominent explanations of why a completed foreclosure will increase the probability that neighbors default on their own mortgages. The first hinges on a completed foreclosure lowering neighboring home values, thus increasing the likelihood that borrowers are "underwater" on their loans—i.e., owing more than the mortgaged property is worth (Campbell and Cocco (2011), Campbell (2013), Goodstein et al. (2011)). As one becomes further underwater on one's loan, the incentive to default on the mortgage increases: the loss on the asset grows large relative to the costs associated with foreclosure (primarily moving costs and a drop in credit score). The second explanation is that a completed foreclosure transmits information to neighbors (Guiso et al. (2013), Towe and Lawley (2013)). Specifically, a completed foreclosure may send a signal to neighbors about the future of the neighborhood (influencing the expected value of the property to the borrower), or about the foreclosure process itself (e.g., neighbors may learn about the likelihood of a mortgage modification if they default on their loans).

We find that contagion is concentrated among borrowers who are not underwater on their mortgages, which we interpret as evidence that contagion is not driven by foreclosures causing falling housing prices. Using information about housing values, and loan principal and outstanding balance, we construct a proxy for borrowers being underwater. We find that contagion is driven not by borrowers in negative equity (underwater), but by borrowers in positive equity. We expect underwater borrowers to be more sensitive to falling home values than those in positive equity (Campbell and Cocco (2011)), and, thus, interpret this difference in effects as evidence that contagion is not driven by falling home values. Rather, we expect that foreclosures convey information to neighbors: a neighboring foreclosure may act as a "wakeup call" for borrowers in positive equity, sending a strong signal about the future of the neighborhood and/or information about the foreclosure process itself, while those who are underwater on their loans may already be well informed about the foreclosure process and the consequences thereof.

We find that contagion varies substantially depending on whether borrowers have mortgages serviced by the same lender, which we interpret as further evidence that information plays an important role in driving contagion. Specifically, we find that when a completed foreclosure occurs there are significantly fewer new foreclosure filings among loans serviced by the same lender than loans serviced by different lenders. This difference may be driven by borrowers learning more and different information from the experience of neighbors whose loans are serviced by the same institutions: how long the foreclosure process takes, how actively the lender pursues foreclosure, and the probability of a loan modification.

Our results suggest that policies that keep delinquent borrowers in their homes, for example by encouraging lenders to modify delinquent loans, may reduce the spillovers associated with home foreclosures. We are able to speak to this question since our empirical strategy identifies foreclosure spillovers for the set of marginally delinquent loans for whom the idiosyncrasies of the overseeing judge matter (i.e., the cases that are likely to comply with the instrument)—these are the cases most likely to be influenced by policy interventions. In principle, our results provide support for government policies that encourage modification, such as the Treasury's Home Affordable Modification Program (HAMP) (even if there have been considerable (and well documented) problems in implementation). Of course any program must weigh general equilibrium considerations—these include the effects on ex-ante incentives for loan origination, and incentives for default by other borrowers. Our finding that contagion is minimal or even negative among borrowers with the same creditors, may provide evidence that these incentives for strategic default matter—we interpret this finding as demonstrating that borrowers update the probability of a modification downward and are discouraged from defaulting on their loans. As such, a policy that raises the cost of default (e.g., achieving a reduction in loan principal only by going through bankruptcy, as suggested by Levitin (2009)) or a more direct policy that targets the vacancy and neglect associated with REO properties may be preferred.

The rest of the paper proceeds as follows: In Section 2 we outline the judicial foreclosure process in the state of Illinois and randomization of judges to cases in Cook County. Section 3 sketches our data sources—administrative records on court cases, geocoded administrative records on foreclosure filings, and deed transfer records. In Section 4 we outline the empirical strategy, which exploits the random assignment of judges for quasi-experimental identification, the results of which we present in Section 5. Section 6 explores possible mechanisms, and Section 7 concludes.

2 Judicial Foreclosure in Illinois

Cook County, Illinois, provides a good context in which to study foreclosure contagion. Firstly, it was badly affected by the foreclosure crisis. Between 2002 and 2011, the county saw 302,166 foreclosure proceedings initiated by lenders ("foreclosure filings"), and 134,924 completed foreclosures. These trends are illustrated in Figure 1. The upper panel of the Figure shows a sharp increase in the number of foreclosure cases filed in Cook County from about 1000 per month in 2004 to more than 3000 filings per month in 2008. The lower panel demonstrate that foreclosure proceedings became more likely to end in a completed foreclosure: the completed foreclosure rate jumps from 45% for cases filed in 2004 to 65% in 2008. Secondly, the foreclosure process in Cook County, IL, goes through the court system, allowing us to instrument a foreclosure outcome using random assignment of judges to cases.

In Illinois, as in many so-called "judicial foreclosure" states, lenders must take delinquent borrowers to court in order to claim a mortgaged property. When a borrower has missed three mortgage payments (i.e., is in default), a lender or the third party servicing the mortgage may initiate the foreclosure process by filing for foreclosure on the associated property with the chancery court (we refer to this event as a **foreclosure filing**). If after ninety days the borrower has not made up all missed payments, the trial begins and the lender's attorney must establish that the borrower: has borrowed money from the lender; has signed a mortgage note promising the property as collateral; and is behind on payments. At the same time, the borrower may mount a defense, for example by disputing any of these facts or claiming that the lender has violated lending laws (e.g., the Truth-in-Lending Act). After hearing the arguments, the presiding judge decides the case, either dismissing the foreclosure action or filing a judgment of foreclosure. If the case is dismissed, the borrower typically continues to reside in the home. If a judgment of foreclosure is filed, then the case proceeds to a foreclosure auction, which we refer to as a **completed foreclosure**.[5] If the sale price does not cover the outstanding balance of the mortgage then the borrower is still considered in debt to the lender, although it is common for lenders to forgive this remaining debt. In the vast majority of cases (around 95% for Cook County), the lending institution purchases the property at auction for the amount of the outstanding loan—in doing so the lender need not record a loss on their balance sheet.[6]

A dismissal may refer to several possible outcomes, most of which result in the property remain-

[5] When a foreclosure judgement is made, a redemption period begins during which the borrower may pay off the entire outstanding mortgage (not just missed payments) plus late fees, attorney fees, court costs, and taxes. The redemption period ends either three months after the judgment or seven months after the initial foreclosure complaint is served, whichever is later.

[6] See statistics for Cook County compiled by the Woodstock Institute: blog.cookcountyil.gov/economicdevelopment/wp-content/uploads/2012/11/Wodstock-Institute-Foreclosure-Filings-2007-2012.pdf

ing occupied by the borrower. First, if a borrower makes all missed payments within 90-days of the filing, then the case is dismissed and the mortgage is reinstated. Second, rather than continuing to pursue an ongoing foreclosure case, the lender may modify the terms of the mortgage to make payments more affordable to the borrower. Third, a lender may accept a deed-in-lieu of foreclosure, in which the borrower forfeits the home to the lender without going through the courts. Fourth, the borrower may negotiate a short sale of their home: the lender accepts the proceeds from the sale of the home as payment for the mortgage. Deed-in-lieu of foreclosure and short sales are generally not an option when there are multiple liens on the property, a fact we exploit to confirm that our results are not driven by these outcomes in which delinquent borrowers lose their property (Agarwal et al. (2011)).[7] Fifth, the lender may "lose" the case by failing to adequately establish non-payment of the mortgage or that they are owed the debt, or the borrower's defense may be successful. Finally, a case may be dismissed because the lender does not take action in pursuing the foreclosure. In our data we cannot distinguish which of these outcomes occurs; we only know whether the case ends in dismissal or completed foreclosure. However, with the exception of a deed-in-lieu of foreclosure or a short sale, in all of these dismissal outcomes the house remains occupied by the borrower.

Foreclosure cases (as with all chancery court cases) are randomly assigned to a case calendar, which restricts the set of judges that will ever hear an action on the case. A case calendar is a weekly schedule of court-room/judge pairings, usually made up of two or three judges. Judges typically only hear cases associated with their case calendar. Similarly, since chancery court cases are only assigned to one calendar, only the associated judges will oversee an action on that case. When a case is filed, it is assigned a unique case number, sorted by property type (single-family home, condominium, commercial property, etc.), and randomly assigned to a case calendar.[8] As of 2010, there were 12 chancery court case calendars hearing foreclosure cases (there are additional calendars that hear only other chancery court cases). Judges are assigned to case calendars each year by the Chief Judge of the Circuit Court of Cook County (details of these assignments are outlined in the General Administrative Orders of the court available online at www.cookcountycourt.org/).[9]

There are several ways that a Cook County judge might influence the outcome of a foreclosure case, which is necessary for the validity of our instrument.[10] Firstly, the judge has discretion to

[7] Anecdotally, deed-in-lieu of foreclosure and short sales are uncommon in Cook County for the reason that, in both cases, creditors are typically taking a loss, while mortgage servicers will accrue lower fees (relevant in cases where the property is being managed by a mortgage servicer): in Illinois, by accepting a deed-in-lieu of foreclosure the lender must forgive all debt, while short sales typically transact at a price below the outstanding debt (Ghent and Kudlyak (2011)).

[8] Random assignment of cases to case calendars is performed by the Chancery Court computer system. As described on the Chancery Court's FAQ page "When a case is filed in the Law Division it is randomly assigned via a computer program to a calendar letter. You may contact the Information Desk in Law Division to obtain the Judge's information associated with the calendar letter." See www.cookcountyclerkofcourt.org/?section=FAQSPage

[9] Not all calendars hear foreclosure cases in equal proportion. For example, throughout our sample (2004–2011) three of the calendars (Chancery Court calendars #52, #53, and #54) hear both foreclosure cases and other chancery cases, while others hear exclusively foreclosure cases. Thus, while assignment is random, the probability of assignment to a given calendar is not uniform.

[10] There is substantial evidence of so-called judicial bias in many settings: Anderson et al. (1999) illustrate important *differences across judges in decision-making*—sometimes suggestive of some forms of bias (see for example, Abrams et al. (2008) or Yang (2012)), and sometimes more generally based on "personal assessments" of case-specific

determine how long a defendant has to find a lawyer and mount a defense. Secondly, even if a defendant does not mount a defense, the judge determines whether or not the lender successfully establishes that the borrower is behind on payments and that the debt is owed to the lender. Establishing these points is less trivial than it seems. Throughout the foreclosure crisis, there have been accounts of mistakes and wrongdoing in the prosecution of foreclosures, including wrongful foreclosures on current mortgages, and failures of banks to produce proper documentation, lenders initiating foreclosure proceedings without reviewing the history of the loan ("robosigning"), failures to adequately attempt to renegotiate the terms of the loan or follow government policy around modifications (c.f., Kiel (2012)). Similarly, it us up to the judge to evaluate a borrower's defense, for example by determining whether a mortgage is legal in the first place (e.g., is not in violation of (predatory) lending laws). Anecdotal evidence suggests that judges vary substantially in their leniency on these issues.[11]In our analysis of Cook County court records we find substantial variation by judge in the rate of completed foreclosure.

In what follows, we use the random assignment of foreclosure cases to case calendars to instrument the outcome that the case ends in foreclosure. As discussed above, judges may influence the outcome of a case. At the same time, the case calendar to which a foreclosure case is randomly assigned determines the possible judges who will ever hear the case. If judges vary sufficiently in their biases toward foreclosure, then the case calendar to which a case is assigned may influence whether a case ends in foreclosure or dismissal. Thus, our identification relies on the comparison of two delinquent borrowers going through the foreclosure process, one of whom is randomly assigned to a "lenient" case calendar and ends in dismissal, while the other is randomly assigned to a "strict" calendar and ends in foreclosure. To implement this study we require data on Cook County foreclosure cases, including case calendar assignment and the case outcome (foreclosure or dismissal).

3 Data

We use geocoded administrative data for Cook County from three sources: Cook County chancery court records, foreclosure filings, and deed transfer records. Publicly available chancery court records for 2004–2010 provide us with details of each foreclosure case, including the information necessary to construct our instrument: the case calendar to which the case is randomly assigned and the outcome of the case (dismissal or foreclosure). To study neighborhood outcomes, however, we need to know the location of the borrowers' homes. To this end, we match each chancery court foreclosure case record to the associated foreclosure filing record (2002–2011), which has been

information (Iaryczower (2009)). Berdejo and Chen (2010), for example, present evidence suggestive of unconscious judicial bias—illustrating priming effects on judges of wars (which suppress dissents)—as well as more partisan behavior before Presidential elections.

[11]The *Washington Post* observes, for example, that "[in] Suffolk and Nassau counties on Long Island and Kings County... which have among the highest rates of foreclosure in the state and where the 81 judges handling foreclosures have become infamous over the past few years for scrutinizing paperwork ... *the level of tolerance for document mistakes varies from judge to judge* ..." (emphasis added). "Some judges chastise banks over foreclosure paperwork", *Washington Post*, 9 November 2010.

provided to us by Chicago-based Record Information Services, Inc. (RIS). These records allow us to observe new foreclosure filings that occur around any given delinquent homeowner's property, which we use to study foreclosure contagion. To observe how completed foreclosures affect housing markets—prices and sales volumes—we rely on deed transfer records (1995–2008) provided to us by the Paul Milstein Center for Real Estate at the Columbia Graduate School of Business. These records allow us to observe the state of the housing market around each property going through the foreclosure courts. Finally, we bolster the information about each neighborhood using data from the 2000 Decennial Census, Zillow housing price indices, and IRS Statistics of Income.

The Cook County chancery court makes public all court records, which include details on each foreclosure case. We manually collected data for each of the 217,230 chancery court cases filed between January 1 2004 and June 30 2010 from the court's public electronic docket.[12] Each record identifies the case number (a unique identifier assigned by the court), the type of case (e.g., foreclosure vs. other chancery case), the plaintiff (lending institution or mortgage servicer), the defendant, and the case calendar. The records also include every action on the case (and corresponding date), although the action descriptions offer minimal detail.

We rely on foreclosure filings from RIS to identify the location of properties going through the foreclosure process. The RIS data span all 307,209 foreclosure cases filed between 2002 and 2011 in Cook County (although some records are for properties in neighboring counties). These records contain the same variables as the online chancery court records, except RIS does not collect the case calendar. However, RIS also collects information not included in the court records, such as the type of property (single-family, condo, etc.), details about the mortgage (type of mortgage, original loan principal, outstanding balance at time of foreclosure filing), and any additional lien holders identified on the filing.[13]The RIS data provide us with the address and latitude/longitude of the home under foreclosure. Finally, RIS also collects a record of each foreclosure auction between 2002 and 2011 (168,577 in total). This allows us to conclusively observe a foreclosure outcome and the date that the foreclosed property is sold.

We match the chancery court records to the RIS foreclosure filings by case id.[14] The resulting data set covers 174,187 foreclosure filings in Cook County filed between January 2004 and June 2010. For our analysis, we drop 847 filings associated with Veterans Affairs mortgages (VA), 12,755 filings made during the Cook County foreclosure moratorium of 2009,[15] and 12,365 filings made during the first or last year in which a case calendar hears foreclosure cases, as cases may be non-randomly assigned as the calendar makes the transition.[16] Ultimately, our results are not sensitive

[12]www.cookcountyclerkofcourt.org

[13]The lender's attorney is not required to identify additional lien holders.

[14]See the Data Appendix for more details on the cleaning process.

[15]Cook County enacted a moratorium on new foreclosure filings on April 16, 2009 to last through September 1, 2009. This moratorium applied to all new filings except those in which the borrower agreed not to mount a defense prior to filing. The effect of the foreclosure moratorium can be seen in Figure 1: in the upper panel, we see that the number of cases filed dips sharply during the moratorium. Interestingly, it seems as though the moratorium finished early—there is a spike in new filings before September 1, 2009. In the lower panel, we see a jump in the completed foreclosure rate for cases filed during the moratorium.

[16]Case calendars have been added over time to ease the burden on existing calendars. In our data, we observe the

to these sample restrictions, although the inclusion of filings on “new” case calendars brings our IV estimates closer in line with the OLS estimates, suggesting some non-random assignment to newly introduced calendars.

This matched data allow us to observe key dates and outcomes of foreclosure cases. For each record, we observe the date the case is filed, and whether and when the case is dismissed or foreclosed, the location of the home under foreclosure, and the above-mentioned details of the property and mortgage. We consider a case as ending in completed foreclosure if the RIS records indicate that a foreclosure auction occurs for that property and mortgage, and we consider the auction date to be the end of the case. We consider a foreclosure case as being dismissed if it does not have an associated foreclosure auction and if the chancery court data records a dismissal action, where we take the date of the dismissal action as the relevant “dismissal date” (see the Data Appendix for details of these variable definitions).

The majority of cases end in a completed foreclosure, while a small fraction of cases are unresolved due to right-censoring of our data. As can be seen in Table 1, which provides descriptive statistics (imposing the above-mentioned sample restrictions), 90,653 (61.2%) cases have an associated foreclosure auction, 50,140 end in dismissal (33.8%), and the remaining 7,427 foreclosure cases remain undecided due to right-censoring. The average length of a case is about 373.6 days, although this is significantly longer for cases that end in foreclosure (428.7 days vs. 274.9 for dismissals). Since the Cook County chancery court records are up to date as of the date of collection (early 2012), and the RIS foreclosure auctions are up to date through 2011, we do not observe the end of particularly long cases. This is especially true for cases filed in 2009 and 2010, from which 79.08% of the undecided cases originate. We omit these undecided cases from our analyses (as well as cases for which we observe the decision, but do not have data on our outcomes for that year).

Among dismissals, we see that only 12.0% of the borrowers “redefault”, suggesting that the dismissal outcome does not merely delay a completed foreclosure. We define redefault as a new foreclosure filing occurring against the same loan after the first case has been decided. Note that this definition of redefault is specific to a given loan and does not count future defaults to the same borrower on different loans and future defaults from different borrowers at the same property. Since dismissed cases make up our counterfactual in studying the neighborhood-level effects of completed foreclosures, this low rate of redefault is reassuring—in most instances dismissing a case does not merely delay the foreclosure (for example, while the lender finds a missing mortgage note), but provides a concrete resolution of the mortgage default within the time frame that we observe.[17]

To study the neighborhood-level effect of completed foreclosure on housing sales and prices we

addition of six new calendars to the foreclosure roster and the phase-out of 16 calendars (that move from hearing foreclosure cases to hearing exclusively other chancery cases). Unfortunately, the details of these phase-in and phase-out processes are not well publicized and we observe unusually low case assignment to these calendars during the phase-in periods. Our concern is that as new calendars are introduced to the foreclosure process they are restricted in the type of foreclosure cases that they hear.

[17]Notice that there is also a non-zero number of redefaults (1.3%) among loans that end in foreclosure. This is likely due to miscoding in the RIS data—for example, a foreclosure auction is scheduled and recorded by RIS, the case is dismissed before the auction takes place (and RIS misses this) and the borrower subsequently redefaults on the loan. Our results are not sensitive to discarding these observations.

rely on deed transfer records for Cook County from 1995–2008. These records cover the universe of real estate transactions and indicate the date of sale, the sale price, and the property type (residential, commercial, etc.). We restrict this data to residential real estate transactions between 2000 and 2008,[18] which leaves us with 862,215 residential real estate sales. The mean sale price for these transactions is $276,401, while the median is $215,000. We geocode the transactions using the reported property address (using Yahoo! Placefinder), allowing us to observe transactions near properties associated with foreclosure cases.

Finally, we add data from the 2000 Decennial Census, the IRS Statistics of Income (SOI), and Zillow. We match these data sources to the Cook County chancery court cases by census tract (Decennial Census) and zip code (IRS SOI and Zillow). The Census provides us with details (as of 2000) on the population density and race of the census tract in which each property is located. The IRS Statistics of Income provide a measure of zip-code-level income (mean adjusted gross income) derived from aggregated tax returns. These data are available for the 1998, 2001, 2002, and 2004–2008 tax years (for 2003, we use the mean of 2002 and 2004, while for 2009+ we use the observed adjusted gross income in 2008). Finally, Zillow provides zip-code-level housing price indices for 2000–2011.

4 Empirical Strategy

Our primary objective is to estimate whether and to what extent a completed home foreclosure is contagious, which we define in terms of the question: does one *completed* foreclosure cause new foreclosure *filings*? To this end, we compare the number of new foreclosure filings in neighborhoods around properties going through the foreclosure process that end in a completed foreclosure to properties that end in dismissal. An obvious concern is that there is non-random selection into completed foreclosure (versus dismissal); we deal with this endogeneity and omitted variable bias by instrumenting a completed foreclosure using the random assignment of foreclosure cases to chancery court case calendars.

For each property that goes through the foreclosure courts, we measure all outcomes annually within an x-mile radius of the property. We measure outcomes relative to the date that the case is decided (either the date of the foreclosure auction or the date of the court action in which the case is dismissed). For case i, let $d(i)$ be the time period in which the case is decided and $Y_{i,d(i)+t}$ be the outcome for property i measured within an x-mile radius of the property, t periods from the decision date. In practice, we measure time in terms of years: $d(i)$ is the year in which case i is decided, $d(i)+1$ is the year after the case is decided, and so on.[19] In our baseline specification we use a 0.1-mile radius around each property, although our results are not sensitive to taking smaller or larger radii (of the same order of magnitude). As an example of how we construct our outcomes, one measure of contagion we consider is the number of new foreclosure filings within a 0.1-mile

[18] See the Data Appendix for more details.

[19] We have also tried months and quarters. However, since home sales and foreclosure filings in small geographic areas are low-frequency events, estimates using these finer units of time end up being low-powered and imprecise.

radius of each property every year since the case is decided.

To achieve our goal of comparing cases filed at the same time that have different outcomes (owing to the random assignment of case calendars) we include several sets of fixed effects in our baseline specification. Filing-month fixed effects, $M_{m(i)}$, where $m(i)$ is the filing month associated with case i, allow us to compare foreclosure and dismissal among cases filed at roughly the same time (and, as explained below, we construct our instrument at the filing-month level). However, cases filed in the same month may be decided in different years. Since we do not want our estimates to be based on the comparison of cases decided in drastically different times (e.g., the onset of the financial crisis in 2008 versus the peak of the boom in 2006) we include year-of-observation fixed effects, $\psi_{d(i)+t}$.[20] In our baseline specification, we also include property-type fixed effects, Φ_i (single-family home, condo, etc.), as cases are sorted by property type prior to randomization to case calendar. Finally, we include a vector of covariates, X_i (loan principal at origination, a dummy variable for the lender/plaintiff being a "large" plaintiff (six largest plaintiffs each representing $\geq$ 7000 filings), a dummy variable for the plaintiff having a "large" attorney (three largest attorneys each representing $\geq$10,000 cases), whether the census tract has an above-median share of white residents, a set of dummy variables for the quartile of median census-tract income, and census tract population density). While these controls improve precision, our estimates are robust to excluding both the property-type fixed effects and the covariates. The resulting relationship we estimate is:

$$Y_{i,d(i)+t} = \beta_0 + \beta_1 F_i + \beta X_i + M_{m(i)} + \Phi_i + \psi_{d(i)+t} + u_{i,d(i)+t} \tag{1}$$

where F_i is an indicator for case i ending in foreclosure. Our goal is to estimate β_1 from Specification 1 separately for each value of $t \in \{0, 1, 2, 3, 4, 5\}$ for contagion and $t \in \{0, 1, 2\}$ for price and sales effects (due to data limitations).

We cluster our standard errors along two dimensions: filing month and census tract (Cameron et al. (2011)). Clustering on filing month captures correlation due to macroeconomic trends—cases filed in the same month may experience similar shocks. Since we also expect correlation between properties that exist in the same geographic area, we cluster at the census tract level. One issue with multi-way clustering that we occasionally encounter is invalid negative variance terms (and a non-positive-definite variance matrix). As suggested in Cameron et al. (2011), we conservatively address this by taking the maximum of the standard errors clustered only on filing month, clustered only on census tract, and clustered on filing month and census tract (and the minimum of the corresponding first-stage F-statistics).

[20] One concern is that the length of the case is itself endogenous. We have explored this in several ways: in Table A.1 of the appendix we add controls for the length of the case and find that our contagion results hold. In Table A.2 we estimate the baseline effects measuring the outcome as of the date that the foreclosure case is filed (rather than decided). While this measurement leads to somewhat noisier estimates (the treatment is diluted by cases that have not yet been decided) the results are generally consistent with our baseline contagion estimates.

4.1 Measuring Local Contagion and Prices

We define two outcomes to test for contagion. Firstly, we consider an indicator for whether any new foreclosure filing occurs within x miles of property i in year $d(i) + t$—how does a completed foreclosure affect the probability of observing any new foreclosure filing? Secondly, we consider the count of new foreclosure filings within x miles of property i in year $d(i) + t$—how does a completed foreclosure affect the total number of new filings? In both cases, we omit new foreclosure filings at the same address or associated with the same foreclosure case but at a different address (e.g., a loan taken with multiple properties as collateral). We also consider the effect of a completed foreclosure on the probability of any and total number of neighboring completed foreclosures.

We also examine the effect of a completed foreclosure on local housing prices, although here our estimates are hampered by sample size. Our measure of local housing prices for the property associated with case i is constructed by taking the log of the average sale price of all properties that sell within the x-mile radius of property i in the year of observation $d(i) + t$. Importantly, we omit the delinquent property itself to ensure that our price estimates are not influenced by an own-price discount of foreclosure (as found by Campbell et al. (2011)). One concern with this measure is that it does not account for selection into sale—the types of homes that sell after a completed foreclosure may be different from the types of homes that sell after a dismissal, and this selection may drive any observed price effects. Unfortunately, we do not observe any proxies for property quality in our data.

We take two approaches to addressing bias from selection into sale. Firstly, while we cannot estimate selection in terms of the types of homes that sell, we can observe whether the volume of sales itself changes. For each property going through the foreclosure process, i, we take as an outcome the count of sales that occur within x miles of property i in the year of observation $d(i)+t$ (again omitting sales at the delinquent property itself). A change in the quantity of sales in response to a completed foreclosure suggest that some sellers (or buyers) are selecting into or out of the market. At the same time, observing no significant response of quantity of sales does not prove that price effects are not driven by selection, but is reassuring—the influx of low (or high) quality properties would have to offset the drop in high (or low) quality properties.

Secondly, we study the subset of repeat-sales (about 44% of the sample) in our data in order to adjust for fixed property characteristics. We estimate the quality-adjusted home value by netting out property-specific fixed effects—details of this adjustment procedure are in the appendix. Our estimates of the effect of a completed foreclosure on the log of the mean quality-adjusted price will not be biased by selection into sales under two assumptions. Firstly, we assume that there is not differential occurrence of repeat sales around properties that end in completed foreclosure vs. dismissal. Since this analysis is restricted only to repeat sales, if this assumption is violated we will have selection into the sample. Secondly, we assume that the property characteristics that determine sale price are not changing differentially for properties near a completed foreclosure vs. a dismissal (i.e., the error in the repeat-sales adjustment process is invariant to the case outcome). Even if these two assumptions hold, our repeat-sales estimates may suffer from imprecision due to measurement error in the repeat-sales adjusted measure of home value.

Comparing the means of the various outcomes, as displayed in Table 2, shows suggestive evidence of foreclosure contagion and a foreclosure price effect. These averages are constructed using all concluded cases (with the above-mentioned sample restrictions) observed annually for five years after the case decision for contagion outcomes, and two years after for price and sales. Specifically, each observation represents a case-year (so a case that is observed for three years after the decision will appear three times). The means in the upper panel of Table 2 suggest foreclosure contagion. Properties associated with cases that are dismissed see 0.435 fewer new foreclosure filings per year than properties associated with foreclosures. There is also evidence that completed foreclosures disrupt the housing market. Properties that end in foreclosure tend to see a higher volume of neighboring sales (3.099 per year relative to 2.962 near dismissed homes). At the same time, these sales occur at a lower average price—$157,181.90 vs. $184,212.50—although this difference is not apparent in the repeat-sales-adjusted price. While these descriptive statistics suggest negative externalities of home foreclosure, these comparisons of means suffer from omitted variable bias and endogeneity of home foreclosure.

4.2 Instrumental Variables Approach and First Stage Regression

There are several reasons home foreclosures may be endogenous to neighborhood-level characteristics. A completed foreclosure is not a random event—it is the product of the choice of a borrower to default on a loan, the choice of a lender to pursue a foreclosure, and the actions of the associated attorneys and judges. The borrower default decision may be influenced by local housing prices, the type of mortgage a borrower has, and the borrower's financial position (both in terms of balance sheet, and cash flow). For example, foreclosures may be more likely to occur in neighborhoods with lower housing price levels and negative price growth (Campbell and Cocco (2011)). Similarly, the lender's decision to pursue a foreclosure versus a loan modification depends on the home value, the probability that the borrower re-defaults on a modified loan, the probability that the borrower brings him/herself out of delinquency without a modification, and, if the loan is serviced by a company that is not the creditor, the potential fees associated with foreclosure (Foote et al. (2008), Levitin and Twomey (2011)).

Descriptive empirical evidence suggests that observable borrower and neighborhood characteristics are correlated with home foreclosures. Table 1 shows means for various covariates broken down by case outcome, where the fourth column contains the p-value on the test of equality between the foreclosure and dismissal. Cases that end in foreclosure are significantly less likely to single-family homes (59.0% vs. 68.4%), more likely to have a plaintiff that is a "large institution" (47.3% vs. 47.2%) or have a plaintiff represented by a "large attorney" (68.7% vs. 68.2%), are less likely to have a conventional fixed-rate mortgage (65.3% vs. 66.5%), and tend to be in neighborhoods with lower median income (43,748.26 vs. 46,409.49), a lower share of white residents (43.3% vs. 46.8%), and a lower population density. While studies have attempted to control for omitted variable bias using very local fixed effects analyses (see summaries in Foote et al. (2009), Towe and

Lawley (2013)), ours is the first study to directly address the endogeneity of home foreclosure with a randomly assigned instrument.

We use a measure of the propensity to foreclose for each chancery court case calendar as an instrument for completed foreclosure. We construct our instrumental variable to capture the notion of judicial bias—the judges on some case calendars are more likely to foreclose than others, all else equal—by taking the "jackknife" or "leave-one-out" foreclosure rate for each case calendar, as is common in studies that use judicial random assignment as an instrumental variable (e.g., Kling (2006), Doyle (2007), Dobbie and Song (2013)). Specifically, for each case i, filed in month $m(i)$ and randomly assigned to calendar k, we take the foreclosure rate among all other cases j filed in that month and assigned to that calendar:

$$Z_i = \frac{\sum_{j \in K_{m(i)}, j \neq i} F_j}{n(K_{m(i)}) - 1} \tag{2}$$

where $K_{m(i)}$ is the set of all cases filed in month $m(i)$ and assigned to calendar k, $n(K_{m(i)})$ is the cardinality of set $K_{m(i)}$, and $F_j = 1$ if case j ends in a completed foreclosure.

A case calendar with "strict" judges whose cases end often in foreclosure will have a high value of the instrument, Z_i, while a calendar with "lenient" judges will have a low value. By omitting case i when constructing the instrument, we ensure that we are not regressing the outcome of the case on itself (resulting in a mechanical correlation in the first stage). Calculating this instrument at the filing-month level accommodates changing case-calendar rosters and attitudes of judges over time.[21] Failing to account for these changes may violate monotonicity of the instrument.

Our first-stage regression relates an indicator for a case ending in foreclosure to our measure of case-calendar strictness. For each case, we regress an indicator for the case ending in foreclosure (F_i) on the instrument. As with the second stage described in Specification 1, we include filing month fixed effects, $M_{m(i)}$, property-type fixed effects, Φ_i and year of observation fixed effects ($\Psi_{d(i)+t}$). One concern is that judge attitudes may respond to macroeconomic events, including the foreclosure boom, and the financial crisis. At the same time, the types of borrowers who are defaulting on their loans is likely also changing over time (Mian and Sufi (2009)). This concern is addressed by the inclusion of the filing month fixed effects. The resulting first-stage regression is:

$$F_i = \alpha_0 + \alpha_1 Z_i + \alpha X_i + M_{m(i)} + \Phi_i + \Psi_{d(i)+t} + v_i \tag{3}$$

We rely on the usual instrumental variables assumptions: the instrument influences the outcome of the foreclosure case (instrument relevance), the instrument is randomly assigned (instrument exogeneity), the instrument does not itself influence neighborhood outcomes (exclusion restriction), and an increase in the instrument is associated with an increase in the probability of the case ending in foreclosure (monotonicity). Table 3 presents OLS estimates of Specification 3 (omitting year of observation fixed effects, as these are only relevant in the full 2SLS procedure). Column 1 shows

[21] For example, we see in Figure 1 that the foreclosure rate changes over time.

a strong relationship between the case calendar foreclosure rate and the probability that a case ends in foreclosure—a one percentage-point increase in the case-calendar foreclosure rate increases the probability of a completed foreclosure by 0.556 percentage points (or about 0.91% off the mean foreclosure rate of 0.612). Including the case-specific covariates does not appreciably change this relationship. Moreover, this relationship is strong—the first-stage F statistic of the excluded instrument is 150. Thus, the assumption of instrument relevance seems valid.

We test exogeneity of the instrument by looking for systematic correlation between the instrument and pre-filing case characteristics. If the rules of the Chancery Court are followed, then the instrument should be randomly assigned and appear independent of case characteristics. We run two sets of regressions to check the assumption that the instrument, Z_i, is randomly assigned. First, we regress Z_i on a set of pre-treatment covariates (controlling for property type and filing month):

$$Z_i = \gamma_0 + \gamma X_i + M_{m(i)} + \Phi_i + e_i \tag{4}$$

where Z_i is the instrument, X_i is a vector of fixed or pre-treatment property and case characteristics, and $M_{m(i)}$ and Φ_i are filing month and property type fixed effects. Random assignment implies that none of the covariates predict the value of the instrument ($H_0: \ \gamma_i = 0$) and that the covariates do not jointly determine the value of the instrument ($H_0: \ \gamma_1 = \gamma_2 = ... = \gamma_k = 0$). Second, we regress each of these covariates on a full vector of case calendar dummy variables (again controlling for property type and filing month):

$$X_{ji} = \rho_0 + \sum_k \rho_k \kappa_{ki} + M_{m(i)} + \Phi_i + u_i \tag{5}$$

where X_{ji} is a given pre-treatment characteristic j observed for case i, and κ_{ki} is a vector of calendar-specific dummy variables such that $\kappa_{ki} = 1$ if case i is assigned to calendar k. We then test the joint significance of these dummy variables: $H_0: \ \rho_1 = \rho_2 = ... = \rho_k = 0$.

We present the results from these randomization-check regressions in Table 4. The first column presents the coefficient estimates from Specification 4 and the p-value for the joint significance test of the covariates. We see no evidence of systematic correlation between pre-treatment covariates and the instrument, and cannot reject the hypothesis that the covariates are jointly insignificant. The second column displays the p-value for the joint significance test of case calendar dummies for Specification 5, where the outcome variable is given by the row. Again, there is no systematic relationship between case calendar assignment and pre-treatment covariates, with the exception of loan principal. We conclude that, conditional on filing month and property type, case calendars are randomly assigned.

The assumption that the instrument does not itself influence neighborhood-level outcomes is reasonable. The outcomes we are studying are the result of the decisions of those not involved in the court case (e.g., neighboring home owners). Moreover, while foreclosure cases span many months, defendants will have minimal direct contact with the presiding judges.

Finally, we find no evidence of a failure of monotonicity. The assumption maintains that a higher value of the instrument—i.e., being assigned to a stricter case calendar—weakly increases the probability of foreclosure for all cases. One can imagine, however, a prejudiced judge who may be lenient toward delinquent wealthy borrowers, for example, but push for foreclosure against delinquent borrowers of lower social class. Then if there are disproportionately more of one type of borrower, a higher value of the instrument will not mean a higher probability of foreclosure for all cases. We explore this possibility by relating group-level foreclosure rates (e.g., foreclosure rate among cases in predominantly white vs. non-white census tracts) within each case calendar to the overall foreclosure rate for each calendar and find that foreclosure rates for sub-groups are all increasing with the overall case calendar foreclosure rate. A discussion of these results can be found in the appendix.[22]

4.3 Interpretation of the Two-Stage Least Squares Estimate

Our estimate captures the LATE for foreclosure cases in which judges are influential, compounds the effect of all subsequent completed foreclosures caused by the initial foreclosure, and is representative of neighborhoods with many foreclosure filings. The estimate does not represent the effect of a completed foreclosure relative to a mortgage that is in good standing; rather, the estimate represents the effect of a completed foreclosure relative to the effect of a foreclosure case being dismissed. We argue that this parameter is of central interest to policy makers.

Firstly, as discussed in Doyle (2007), if there are heterogeneous treatment effects the parameter identified by a judicial random assignment instrumental variable (or in Doyle's case, rotationally assigned case workers) is the LATE for "marginal" cases—those where the judge is likely to have an influence. Intuitively, there are cases that will always end in foreclosure and cases that will always end in dismissal; the set of "compliers" with our instrument are the marginal cases where the judges on the case calendar have influence on the outcome.[23]

We find that the characteristics of the sub-population of loans that comply with our instrument are consistent with cases on the margin of foreclosure or dismissal, representing individuals who have a higher ability to pay than the typical delinquent borrower, but are facing difficult circumstances that could be mitigated through loan modification. We stratify the sample along several margins: tract-level quartile of income (from the 2000 Decennial Census), whether the loan is from a "large" lender, whether the mortgage is conventional, whether the zip code experiences positive price growth in the year that the case is filed, and a proxy for whether the property is worth less than the loan

[22] As suggested by Mueller-Smith (2013), we have also estimated our baseline specification by constructing the instrument separately for various sub-groups. If monotonicity is violated, then these results may differ substantially from the baseline estimates. While we do not see a substantial difference in our baseline results (see Table A.3 in the Appendix), these "monotonicity-robust" estimates are imprecise; constructing the group-specific instrument places high demands on the data, as splitting the data into filing-month/characteristics cells often yields few observations per cell.

[23] It is easy to conceive of situations where judges will not matter. For example, some sophisticated or well-to-do borrowers may always be able to renegotiate the terms of their mortgages (and a dismissal of the case), regardless of who the judge is. At the same time, other borrowers may resign themselves to walking away from their home and mortgage and choose not to appear in court at all (with a foreclosure as a result).

("underwater").[24] Our goal is to proxy characteristics of borrowers who are likely to benefit from loan modification; creditors may be more willing to modify in such situations (Adelino et al. (2009)), making them more responsive to judicial input.

We estimate the first-stage relationship for each subgroup and compare the estimate of the parameter on the instrument (judicial leniency) to the estimate of the same parameter for the full sample. Specifically, for each sub-sample, G, we estimate the first-stage:

$$F_i = \alpha_0 + \alpha_{1G} Z_i + \alpha X_i + M_{m(i)} + \Phi_i + \Psi_{d(i)+t} + v_i \quad \forall i \in G \tag{6}$$

We then take the ratio of the estimate of the first-stage relationship for group G to the estimate for the full sample from Specification 3: $\frac{\hat{\alpha_{1G}}}{\hat{\alpha_1}}$. As described by Angrist and Pischke (2008), the ratio of the sub-group-specific first stage to the full-sample first stage represents the relative likelihood that a complier belongs to the given subgroup.

We interpret our estimates of these ratios, presented in Table 5, as demonstrating that compliant cases are likely to be on the margin of completed foreclosure. The upper panel, which displays estimates for the four quartiles of tract-level income, show that compliers are more likely to be in the upper two quartiles of income than the general population of foreclosure cases. Taking income as a measure of a borrower's ability to repay their loan, these estimates suggest that compliers are more likely than the typical borrower to be able to resume payments if the case is dismissed.

At the same time, the compliant sub-population may benefit from a mortgage modification. Compliant borrowers are less likely to be in a zip code with positive price growth. Falling house prices may be largely responsible for the default crisis (Mayer et al. (forthcoming))—borrowers may be in default because they expect to lose money on their mortgages as housing prices fall and the value of the asset drops below the cost of the debt. A modification reducing either the principal of the loan or the interest rate may reduce the loss anticipated by the borrower making default (and foreclosure) less appealing. At the same time, compliant borrowers are not in dire straits—they are less likely to be underwater on their loans, so a modification may be more effective (home value is not so low that the mortgage is a lost cause) and may result in smaller losses to lenders than modification of more severely underwater loans. Additionally, compliers are less likely to have conventional loans. There is some suspicion that unconventional mortgages are responsible for many defaults during the crisis. For example, borrowers with low "teaser" interest rates or balloon payments may have been expecting to refinance their loans to avoid higher monthly payments, but found themselves without this option during the financial crisis. In such cases, a modification may be particularly effective (by mimicking the effect of a refinance).[25] Finally, it is interesting to note that the differential characteristics of the compliant population appear borrower specific—compliers

[24] We define a proxy for a borrower being underwater by relating the initial loan size to the outstanding debt when the foreclosure case is filed. We start with the initial loan principal, assume an 80% loan-to-value ratio at origination, and back out an estimate of the initial home value. We then adjust this value using zip-code-level housing price indices from Zillow to arrive at an estimate of the value of the home at the time of foreclosure. We deem the borrower to be substantially underwater if the lender's claim against the borrower is greater the estimated home value.

[25] At the same time, there is debate about the importance of unconventional loans in the default decision (c.f., Mayer et al. (forthcoming)), so this channel may be less relevant.

are no more or less likely to have a loan from a "large" lender.

Secondly, our LATE estimate does not simply identify the effect of a single completed foreclosure, but compounds the effects of all subsequent induced foreclosures. If foreclosures are contagious, then a completed foreclosure will lead to subsequent foreclosure filings. Naturally, some of these filings will, in turn, become completed foreclosures and themselves cause new foreclosure filings. Since our empirical strategy compares the local neighborhoods around cases in the foreclosure courts each year after the case is decided and does not control for the effects of subsequent foreclosures, our estimates will compound the effects of these subsequent foreclosures. We believe that this parameter is policy-relevant since it represents the overall consequences of the marginal foreclosure—given a delinquent borrower, what are the external costs of a completed foreclosure relative to a dismissal of the case?

Similarly, if there is foreclosure contagion, our LATE represent neighborhoods with several completed foreclosures. Recall that our unit of observation is the neighborhood around a property going through the foreclosure courts. This represents a selected sample—we only observe a neighborhood when a foreclosure filing is pursued against a property. Moreover, if completed foreclosures induce subsequent foreclosure filings resulting in additional observations in our data, neighborhoods with completed foreclosures will be over-represented in our sample. This does not affect the validity of the instrument—case calendars are still randomly assigned—but influences the interpretation of the LATE.

Finally, our estimates are conditional upon a foreclosure filing having occurred in the neighborhood. Our empirical strategy and data set necessarily rely on comparing neighborhoods around properties that are already going through the foreclosure process. Our estimates will not account for any externalities associated with a borrower default or a foreclosure filing. Many have argued that it is a completed foreclosure and subsequent real-estate ownership of the associated property that drives foreclosure-related externalities. While we cannot speak to any spillovers from borrower default, our estimates provide a well-identified answer to whether there are negative spillovers associated with the completed foreclosure itself. To date, existing estimates of foreclosure externalities have confounded these two channels.

The LATE represented by our estimates is a relevant parameter for the policy question of how best to address the problems of delinquent borrowers. Policymakers concerned with foreclosures can focus on several stages of the lending process: how easy it is to originate/obtain mortgages, how to prevent borrowers from defaulting, and what to do once a borrower has defaulted. Our parameter, which is estimated conditional on foreclosure filing, focuses directly on the latter question.[26] For example, if we find large negative externalities of a completed foreclosure relative to a dismissed case, then policymakers may want to consider incentivising lenders to renegotiate delinquent mortgages. Moreover, the LATE is relevant for cases on the margin of foreclosure and dismissal, and who are influenced by foreclosure court judges. These cases are also likely to be influenced by policies

[26] Of course, the usual partial-equilibrium caveat applies: any change to foreclosure policy may affect ex-ante incentives (e.g., Mayer et al. (2011)) and housing market outcomes (e.g., Pence (2006)), which are not captured in our reduced-form estimates.

discouraging foreclosure on delinquent loans.

A number of existing government policies to address the foreclosure crisis focused on incentivizing servicers to perform home mortgage modifications. These include the FDIC's Loan Modification Program, "Mod in a Box"; the GSE variant of this, "the Streamlined Modification Program," and most significantly the Treasury program—Home Affordable Modification Program (HAMP). While there have been important criticisms around implementation, our estimates help to address the question of whether these are sensible programs in principle.[27]

5 Neighborhood-Level Effects of Completed Foreclosure

We find robust evidence of foreclosure contagion that persists over several years. Neighborhoods around a completed foreclosure are 10% more likely to have at least one foreclosure filing in a given year relative to neighborhoods around a dismissed property and experience around 0.5 to 0.7 more total filings per year. We also find that residential properties that transact around completed foreclosures do so at a price discount (on the order of 30–40%), although this effect may be largely explained by negative selection into sale.

5.1 Contagion in Foreclosure Filings

Our estimates demonstrate that completed foreclosures are contagious. Table 6 presents our baseline 2SLS estimates of the effect of a completed foreclosure on the probability of observing any neighboring foreclosure filing in a year and on the annual count of neighboring foreclosure filings within 0.1 miles of the at-risk property. The 2SLS estimates show that a completed foreclosure increases the probability of observing any new filing within 0.1 miles by 0.052 percentage points in the year of the decision (a 7.4% increase in the mean for all dismissed cases). This effect increases over time to 8.2 percentage points (11.7%) in the second year after the decision, 9.0 percentage points (12.8%) in the third, and 24.7 percentage points (35%) in the fourth year out. Similarly, the 2SLS estimates show that a completed foreclosure causes 0.54 to 0.70 new foreclosure filings per year in the year the case is decided and the following three years. This contagion represents a 25–32% increase in total annual filings relative to an average of 2.161 filings per year around dismissed properties. Note that the instrument is strong in the year of the decision through the second year after the decision (F-stats around 200), although is relatively weak three, four and five years out owing to the smaller sample for these periods.

Our contagion estimates are generally not sensitive to the specification, sample, or geographic measurement of the outcome. The results are robust to excluding the covariates, omitting the property fixed effects, dropping cases decided in the summer months (the court automatically dismisses inactive cases during this time), using a monotonicity-robust instrumental variable, using the full sample and including the foreclosure moratorium, omitting each filing year one by one,

[27] Criticisms have centered around the incentives for servicers to perform these modifications and the lack of principal reduction in the modification plans.

dropping neighborhood-years with foreclosure filings above the 99th percentile, and restricting the sample to cases for which we observe at least three years post decision (see Tables A.3, A.4, A.5, and A.6 in the Appendix). This last specification is of particular interest, since it maintains a constant sample across observation-years, and demonstrates that the evolution of contagion over time is not driven by changes in the composition of the sample. We also estimate our baseline results measuring outcomes within 0.25 miles of the delinquent property and find that contagion (and price effects, discussed below) persist—See Table A.7. Note, however, that the estimates for price and any foreclosure filing are generally smaller (effects decline as we are further from the property), while the effects for total filings tend to be larger (as the radius goes the total base number of properties to file for foreclosure grows). Finally, we confirm that our estimates are driven by dismissals where the defendant retains possession of the property (rather than deeds-in-lieu of foreclosure or short sales). We estimate our baseline results on the sample of cases in which the plaintiff identifies that there are additional liens against the property. Although less precise, the point-estimates for this sample are comparable to the full-sample estimates and are not significantly different (see Table A.8 in the Appendix).

We further explore the validity of our estimates by applying the same 2SLS procedure to our contagion outcomes measured in the three years prior to the case being filed. If our instrumental variable is truly randomly assigned, we should not expect to see any effect of a case ending in foreclosure before the case has even started. We present these "pre-treatment" estimates in Table 7. Reassuringly, when instrumented by case calendar leniency, a case ending in foreclosure appears to have no relationship to local housing prices prior to the start of the case—the point estimates are close to zero and insignificant.

We examine the cumulative effect of a completed foreclosure in order to appreciate the full extent of contagion. Rather than using as an outcome the number of new foreclosure filings per year for each year since the decision, we instead consider the total number of new filings since the decision. These estimates are presented in the lower panel of Table 6 and show that a completed foreclosure leads to a significant divergence in foreclosure filings relative to a dismissal. As noted above, in the year of the decision a completed foreclosure causes 0.691 new filings. However, neighborhoods around completed foreclosures have experienced 2.09 more foreclosure filings by the second year after the decision, and 6.45 more filings by the fourth year after the case ends. One completed foreclosure may have a substantial impact on the composition of a neighborhood, at least in the short and medium term.

One concern with our findings is that they are specific to neighborhoods that are experiencing a wave of foreclosures. Firstly, our period of study (2004–2011) is largely made up of the housing crisis—the Cook County housing market peaked in 2006. Secondly, since we only observe neighborhoods where a foreclosure filing has occurred, and since we do find that foreclosures are contagious, there is likely selection into our sample—foreclosure filings (and, thus, observations in our data) are likely to be in neighborhoods with recent completed foreclosures. From a policy perspective, it is especially important to understand the cumulative impact of the first foreclosure in a neighborhood.

Nevertheless, we find that a completed foreclosure is contagious even in a neighborhood that has not experienced a foreclosure in recent years. We restrict our sample to cases where there have been no completed foreclosures in the two years prior to the decision (the results are similar if we restrict to cases with no filings within two years) and estimate the cumulative contagion effect of a completed foreclosure, presented at the bottom of the lower panel of Table 6. We find clear evidence that completed foreclosures are contagious even in neighborhoods with no other recent completed foreclosures, although these results are less precise than when we use the full sample, owing to a smaller sample size: a completed foreclosure leads to 1.3 more filings by the end of the first year after the decision, and almost four more filings by the third year out. Even the first completed foreclosure in a neighborhood has externalities (and that the contagion we observe is not an artifact of selection into the sample).[28]

5.2 Contagion in Completed Foreclosures

To better understand the costs of foreclosure contagion, we look for contagion in completed foreclosures. Above, we established contagion in foreclosure filings—the result of new borrower defaults and lenders pursuing foreclosure action (we take up the discussion of these two actions in more depth in Section 6). However, if we do not see contagion in completed foreclosures, then contagion in filings is unlikely to be a large contributor to the spread of a foreclosure crisis. Moreover, the costs of new filings that end in dismissal are, perhaps, smaller than the costs of new completed foreclosures (for example, owing to pecuniary externalities of completed foreclosure, moving costs associated with the displacement of homeowners, etc.).[29]

We find contagion in completed foreclosures. We estimate the baseline contagion IV regressions replacing the outcomes with an indicator for any neighboring completed foreclosure (within 0.1 miles of the property in the given year since the case is decided) and the count of completed foreclosures. We present these estimates in Table 8 and find that a completed foreclosure moderately increases the probability of observing any neighboring completed foreclosure (by 13.8 percentage points three-years out). Moreover, there is a notable increase in the number of neighboring completed foreclosures: one completed foreclosure causes between 0.28 and 0.56 additional completed

[28] Another concern relates to the length of cases—as seen in Table 1, cases ending in dismissal are significantly shorter than cases ending in completed foreclosure. A possible explanation is that foreclosure externalities are driven by borrower behavior while in default, and the effect is larger for cases ending in completed foreclosure since these cases are longer. To rule out this explanation, we estimate our baseline 2SLS estimates, adding flexible controls for the length of the case. We try three different sets of controls—log of the number of months, a quadratic in number of months, and dummy variables for the number of quarters of length—and present these results in Table A.1 in the Appendix. These estimates show contagion effects that are comparable to our baseline estimates, although the addition of these length-of-case controls reduces the precision of the estimates.

[29] One difficulty in studying contagion in completed foreclosures is that the response may be driven by judges. While this is not an issue when studying contagion in foreclosure filings—an event that depends only on the actions of the borrower and lender—judges have an influence over the outcome of a foreclosure case. We cannot explicitly rule out judge behavior as driving contagion in completed foreclosures. However, we do not expect judge contagion to be a dominant force—this would require judges to be well informed about recent events in the neighborhoods around the delinquent properties associated with their cases, which we find unlikely given the volume of cases (it would be costly to keep up on all outcomes) and judicial random assignment (judges are not specializing in neighborhoods).

foreclosures annually (or between 40 and 93 percent off of the mean). Thus, contagion appears to play an important role in the spread of foreclosures; mitigating completed foreclosures may reduce the depth and costs of a housing crisis.

The timing of contagion in completed foreclosures suggests that borrowers and/or lenders who are already involved in the foreclosure process respond to nearby events. Given how long the foreclosure process takes—from default to filing to completed court case—it may seem strange that we find contagion in completed foreclosures in the year of the decision. However, borrowers and lenders may respond at any stage of the foreclosure process. For example, a neighboring completed foreclosure may influence the effort a borrower puts into fighting an ongoing foreclosure case by conveying information about the costs of fighting foreclosure or the probability of a successful loan modification.

Indeed, we find that completed foreclosures influence cases that are ongoing. For each case, i, we split neighboring completed foreclosures into two groups: completed foreclosures among cases filed before case i is decided and cases filed after case i is decided. By focusing on the former group, we can observe how the outcome of case i influences ongoing foreclosure cases. We define as an outcome the count of completed foreclosures among cases filed before the decision (and an indicator for any completed foreclosure in this group as another outcome) and estimate our contagion model—the results are presented in the lower panel of Table 8. The estimates show that a completed foreclosure causes 0.11 to 0.6 new completed foreclosures per year among cases that were already filed before the decision in the first two years after the decision (these cases drive the contagion in the first two years, during which there is minimal contagion among newly filed cases). These results provide evidence that contagion acts not only through influencing the behavior of borrowers and lenders before filing (e.g., encouraging borrower default), but also by changing how borrowers/lenders approach an ongoing foreclosure case. In particular, it appears that borrowers may put less effort into resisting a completed foreclosure.

5.3 Housing Markets

Our two-stage-least-squares estimates demonstrate that a completed foreclosure disrupts local housing markets by inducing lower-quality properties to sell, resulting in a lower average sale price. We find that a completed foreclosure causes a drop in the average neighboring sale price (not adjusted for property quality or characteristics) of up to 41% in the following year. However, we find evidence suggesting that this drop in price is driven by an increase in sales of lower quality properties. We observe an increase in the number of neighboring sales in response to a completed foreclosure. Moreover, when we use repeat-sales to control for property characteristics, we find no evidence of an effect of completed foreclosure on average sale prices, suggesting that the unadjusted effect is driven by selection. Nonetheless, we cannot rule out a small negative effect on quality-adjusted property values.

Our baseline 2SLS estimates, presented in the first panel of Table 9, suggest that a completed foreclosure lowers the average neighboring sale price over several years. The columns of Table 9 present the baseline price and sales effects for the year in which the case is decided, and one and two years after. The estimates suggest that a completed foreclosure depresses neighboring residential sale prices (by 12.7% in the year of the foreclosure, 41.1% in the year after and 35.8% two years out). However, the precision of these estimates suffers from a smaller sample size than our contagion results (we only observe housing sales through June 2008 and only observe prices when a home sells): they are only significant in the year after the decision and the first-stage F-statistics are relatively low (around 20). Nonetheless, these estimates are not sensitive to the same robustness checks described in Section 5.1 (see Tables A.9, A.4, and A.11 in the appendix). Similarly, we see no pre-filing relationship between local housing markets and the eventual outcomes of the cases (see Table 7).

Our 2SLS estimates show suggestive evidence that a completed foreclosure influences the volume of residential housing transactions in a neighborhood. The point estimates of the effect of foreclosure on the probability of any sale are small and insignificant. On the other hand, the point estimates of the effect of a completed foreclosure on the cumulative number of neighboring residential sales since the case decision shows a large increasing trend—while this effect is not statistically significant, it appears as though a completed foreclosure may induce additional home sales. It may be the case, for example, that a foreclosure signals a future decline in the quality of the neighborhood (or sends a signal about the local economy), inducing homeowners. This increase in number of sales raises the question of whether the drop in sale price after a completed foreclosure is caused by selection into sale of lower quality (and thus, lower price) homes or a drop in the value of neighboring properties (conditional on quality). In particular, if this drive to sell in response to a completed foreclosure is stronger among those with lower-quality houses (for example, because they have less equity in their homes or are in a more precarious financial situation), then we would expect a decline in average neighboring sale price.

Our repeat-sales-adjusted price results suggest that there is negative selection into residential sales after a completed foreclosure, which may explain much of the negative sale-price effect associated with completed foreclosure. As explained in Section 3, we use a repeat-sales methodology to adjust reported sale prices for property quality. We then estimate the effect of a completed foreclosure on the log of the average neighboring quality-adjusted sale price. The point estimates of the effect of home foreclosure on the log of the mean repeat-sales adjusted price tend to be small: 0.059 in the year of the decision, 0.003 the following year, and are not significantly different from zero—controlling for property quality yields smaller price effects of completed foreclosure.[30] By pooling the unadjusted (i.e., baseline) and repeat-sales-adjusted price regressions and testing the cross-equation restriction that the effects of completed foreclosure are different (i.e., $H_0 : \beta_{\text{raw prices}} = \beta_{\text{repeat sales}}$), we find that the treatment effects for adjusted prices are significantly

[30] In Table A.10 in the appendix, we confirm that the difference between the baseline and repeat-sales adjusted estimates is not driven by a selected sample of repeat sales. We estimate the baseline price effects for two measures of neighboring unadjusted price: mean log sale price for all non-repeat sales, and mean log (unadjusted) sale price for all repeat sales. In both cases, the point estimates are comparable to the baseline price effects—large and negative—although the smaller sample size reduces precision.

smaller than for the unadjusted prices, although we cannot reject the hypothesis that the estimates are the same in the year of the decision or two-years out (p-values for this test are presented in the adjusted-price panel of Table 9).

We conclude that there is negative selection into sale—when a neighboring foreclosure occurs, the properties that do sell tend to be of a lower quality. The difference between our repeat-sale adjusted and unadjusted baseline sale price estimates, along with the earlier estimates that suggest an increase in sales volume after a completed foreclosure, suggest that lower-quality homes are more likely to transact after a completed foreclosure. This may be the case if owners of lower quality homes in a neighborhood are those who have lower income or wealth. Given a signal that the neighborhood is declining (i.e., a neighboring completed foreclosure), these owners may be eager to sell before the neighborhood "falls apart" in order to avoid the liquidity shock, both from the difficulty of selling an underwater property and the inability to borrow against an underwater home. Of course, we cannot conclude that all foreclosure-related pecuniary externalities are driven by this selection into sale. Existing studies of foreclosure-price externalities (e.g., Campbell et al. (2011)) find estimates one order of magnitude smaller than our own, and given the imprecision of our estimates, we cannot rule out that there are negative effects of completed foreclosure on the quality-adjusted value of neighboring properties.

6 Evidence of Contagion Mechanisms

While our reduced-form estimates provide clear evidence of contagion, interpreting the causes of contagion is difficult since a foreclosure filing is the result of a joint decision of the borrower, who defaults on his/her loan, and a lender or servicer, that chooses to pursue foreclosure on the borrower's home. Both parties may be influenced by a neighboring completed foreclosure, which may lower the value of the asset and alter the incentives for the borrower to repay and the lender to file for foreclosure. We argue that by studying heterogeneity in the treatment effect of foreclosures—across loan types, lender identity, and market conditions—we can gain insight into the importance of borrower and lender behavior.

6.1 Distinguishing Borrower and Lender Response

Our goal is to distinguish whether contagion is driven by borrowers or by lenders. We argue that lenders would exhibit no response to a local completed foreclosure or even anti-contagion—fewer foreclosure filings in response to a completed foreclosure. This provides a weak test for the presence of borrower-driven contagion. We then explicitly test for borrower-driven contagion by examining contagion among loans from lenders known for automation of foreclosure proceedings (who are, thus, unlikely to respond to very local market conditions).

Lenders and mortgage servicers—third parties who are paid by creditors to collect mortgage payments and manage defaults—have three options when dealing with a delinquent borrower: do nothing, pursue foreclosure, or renegotiate the loan. The action lenders or servicers choose to take influences the observed number of foreclosure filings. Through foreclosure lenders acquire the mortgaged property and gain the benefit of the value of the property (servicers collect fees for managing the foreclosure). Lenders or servicers may instead modify the terms of a mortgage with the goal of making the payments more affordable—e.g., a lender might reduce the principal or interest rate on a loan in order to lower monthly payments. The benefit of modification to the lender is ensuring that a loan continues to be paid, at the cost of a lower lifetime value of repayment. Finally, when faced with a borrower who is not making payments, a lender or servicer can always do nothing, although this is generally not optimal.[31] Existing empirical studies have found that mortgage modification is uncommon (Adelino et al. (2009), Ding et al. (2009)). There are several (non-exclusive) explanations for why lenders and servicers prefer foreclosure to modification: borrowers may still redefault on a modified loan (Adelino et al. (2009)); lenders must modify infrequently in order to discourage strategic defaults among borrowers who can pay, but would like better terms on their loan (Foote et al. (2008)); foreclosing and purchasing the property at auction allows creditors to delay recognizing a loss on their balance sheets; modification requires coordination and agreement among all creditors (e.g., multiple holders of a mortgage-backed security or a second lien on the property), some of whom may need to accept a loss (Gelpern and Levitin (2009)).

These incentives to file for foreclosure against delinquent borrowers are unlikely to respond to neighboring completed foreclosures (unless borrowers themselves change their behavior), making lender-driven contagion unlikely. In the Appendix, we adapt the simple framework of Foote et al. (2008) and Adelino et al. (2009) to explore how the lender's incentive to modify or foreclose might change with a neighboring completed foreclosure. Intuitively, assuming that a completed foreclosure does not influence the behavior of neighboring borrowers (i.e., no borrower-driven contagion), but lowers neighboring home values, a lender would be weakly less inclined to foreclose on this home since the collateral is worth less relative to the value of the modified loan (although the lender may still foreclose for the reasons discussed in the preceding paragraph).[32] Moreover, mortgage servicers are not likely to respond to a neighboring foreclosure at all. Servicer compensation in foreclosure depends on fees incurred during the foreclosure process and not on the value of the home itself—servicers are indifferent to the value of the collateral. At the same time, since servicer compensation on a loan in good standing is typically a function of the monthly payments collected, modifying a mortgage typically entails lower future payments to the servicer and servicers do not typically recover fees for modifying a loan. Thus, when faced with a delinquent loan, pursuing

[31]In fact, there are two situations where inaction may be a good strategy. Firstly, if renegotiation and foreclosure are costly to the lender and if there is a high probability that the default will "self cure"—i.e., the borrower will resume making payments—then waiting may be beneficial. Secondly, accounting rules adopted in April 2009 allow creditors to keep a delinquent loan at face value on balance sheets if there is a reasonable chance that the loan may be repaid. Note, however, that if there is no borrower-driven contagion (i.e., borrowers do not change their probability of default in response to a neighboring foreclosure), then a neighboring completed foreclosure that lowers housing values will likely exacerbate these incentives to wait: foreclosures will be even less appealing as the associated property will yield a lower payoff.

[32]If a completed foreclosure has no influence on neighboring home prices and if we maintain the assumption that borrowers are unresponsive to a completed foreclosure, then lenders and servicers should not respond—the conditions of the neighboring loans are unchanged.

foreclosure is typically more valuable to servicers, and the decision to take this action is orthogonal to the value of the property. This is consistent with the discussion of Levitin and Twomey (2011) who note that servicers have no stake in the value of the property under consideration, and will take the action that maximizes the fees they collect, which is typically foreclosure.[33] In summary, under the assumption that borrowers do not respond to completed foreclosures, a neighboring completed home foreclosure should (weakly) discourage foreclosure filings—the collateral is less valuable to the lender and servicers are indifferent the home value.

The above-outlined servicer and lender incentives provide a simple test for the hypothesis of no **borrower-driven** contagion, which we reject. Specifically, this null hypothesis can be rejected if we observe positive contagion.

That we observe contagion in Table 6 suggests that borrowers respond. Note, that while this confirms the presence of borrower contagion, this does not rule out lender response—when borrower default probabilities are changing, lender response is ambiguous.

To further establish that contagion is driven primarily by borrowers, we study contagion among loans held by lenders and servicers who are known to have automated foreclosure filing processes. We identify foreclosure filings by all lenders/servicers investigated in the Independent Foreclosure Review Settlement conducted by the Office of the Comptroller of the Currency and the Board of Governors of the Federal Reserve System.[34] We then redefine our contagion outcomes in terms of new foreclosure filings among automating lenders only and re-estimate our baseline 2SLS contagion models. The new outcomes are: the count of new foreclosure filings among automating lenders and an indicator for any new foreclosure filing from these lenders. Because this restriction reduces our power—we are studying filings among a subset of lenders, which are lower frequency events—we pool all post-decision years ($d(i) + t$ where $t \in \{0, 1, 2, 3, 4\}$) and restrict the contagion effect to be constant across all years (see Table A.12 in the Appendix for annual estimates). If contagion is driven largely through lender response, then we should find no effect of a completed home foreclosure on the number of foreclosure filings among automated lenders. At the same time, if contagion is driven primarily through borrower response, then our estimates should be comparable to our baseline full-sample contagion estimates.

[33] Levitin and Twomey (2011) also point out that, although foreclosure is preferable to modification from the servicer's perspective, there is incentive to delay the start of the foreclosure process. When a borrower misses a payment, the servicer must forward payment to the creditor as an interest free loan that is repaid to the servicer when the borrower resumes payment or the property is sold at foreclosure auction. At the same time, the servicer can charge late fees that are paid when payments resume or the foreclosure sale occurs. Levitin and Twomey (2011) show how servicers can benefit by waiting several months to accrue late fees before beginning the foreclosure process. Of course, this wait period is unlikely to be influenced by a neighboring completed foreclosure, and so does not relate to contagion.

[34] Throughout the mid- and late-2000s, many lenders and servicers adopted automated foreclosure filing procedures (Levitin and Twomey (2011)). It is clear that in many cases the delinquent borrower's situation and background were not given close consideration. As investigations have revealed, employees of several mortgage servicers and financial institutions falsely testified that they had personally inspected mortgage borrowers' information, even though the processing speeds given personnel constraints made it impossible for this to be true (this was the so-called "robosigning" controversy, settled for $25bn in April 2012 between the federal government, 49 state attorneys general and the five largest servicers). See Kiel, P. "The Great American Foreclosure Story: The Struggle for Justice and a Place to Call Home", ProPublica, 10 April 2012.

We find that contagion is primarily borrower-driven—contagion in foreclosure filings persists when studying loans among automating lenders. Table 10 presents the pooled estimates for all filings (upper panel) and for filings among automating lenders. One concern is that the total number of loans in a neighborhood from automating lenders is necessarily smaller than the total among all lenders (as in the baseline estimates). If new filings are proportional to the existing number of loans, then we would expect a larger response in the total count of new filings for the baseline than for automating lenders. Indeed, this can be seen in column 2: we see a point estimate of 0.74 for the baseline (all filings) and a smaller estimate of 0.115 for the automating lenders. However, relative to the mean number of filings for each group (2.44 filings per year within a 0.1 mile radius for all filings, 0.37 for automating lenders), these point estimates are remarkably similar: a 30.3% increase in filings versus a 29.7% increase in filings. To further check this concern, we estimate the pooled models using as the outcome the log of the count of new filings. Here, we can see that the contagion effects are quite similar (0.094 versus 0.084). We also observe similar contagion in the probability of observing any new filing (0.062 versus 0.039). Thus, given the incentives of lenders and servicers, that the baseline estimates are inconsistent with the hypothesis of no borrower response, and the finding that contagion persists among automating lenders, we conclude that contagion is mainly borrower driven.

6.2 Borrower Incentives

To explore the mechanisms driving borrower contagion, we consider existing theoretical and survey research on borrower default and compare these predictions to estimates with our data and empirical strategy. Existing theory of optimal default suggests that borrowers will be more likely to default in response to a drop in the value of their home, especially when aggregate housing price growth is low. On the other hand, survey research suggests that learning about foreclosure through social networks may play an important role in the default decision. We look for evidence of these mechanisms by examining heterogeneity in the response to a neighboring completed foreclosure. We do not find that foreclosure contagion is stronger among those in negative equity—with home values below their outstanding mortgage debt—but is driven primarily by those in positive equity, which we interpret as evidence that contagion is driven through informational channels rather than a pure housing price effect. While we cannot conclude that the extent of foreclosure contagion varies systematically with social/demographic factors that proxy the strength of social ties in a neighborhood, we do find evidence of social contagion through a very specific network—neighbors with loans from the same lender.

6.2.1 Drop in Value of Home

It is commonly held that foreclosures are contagious because they lower neighboring property values (Campbell (2013)). Mortgage default theory (c.f., Campbell and Cocco (2011); Deng et al. (2000)) suggests that borrowers are more likely to default when the value of their home falls below the balance of their mortgage (putting the borrower "underwater"). Intuitively, if the market value of the home is greater than the outstanding debt, homeowners who are having difficulty making mortgage payments may sell the property and use the proceeds to pay off the debt. Conversely, underwater borrowers who are having difficulty making payments do not have the option to sell. Indeed, descriptive empirical work by Bajari et al. (2008) shows that aggregate housing price decline is one of the most important determinants of mortgage default. To the extent that foreclosures lower current and future neighboring home values foreclosure contagion may operate through pushing borrowers (further) underwater.[35]

We investigate whether foreclosure contagion is stronger among loans held by individuals who face high debt relative to the value of their home, as these individuals are likely most sensitive to falling home values (Campbell and Cocco (2011)). For each foreclosure filing, we create a proxy for the borrower being underwater at the time of filing. We then split the count of new foreclosure filings each year within 0.1 miles of each property into two groups: new filings among those who are underwater and new filings among those who are not. We estimate our baseline 2SLS contagion effects separately for each type of filing and test the hypothesis that there is more contagion among underwater borrowers than borrowers in positive equity.

We define a proxy for a borrower being underwater by relating the initial loan principal to the outstanding debt when the foreclosure case is filed. We start with the initial loan principal, assume an 80% loan-to-value ratio at origination, and back out an estimate of the initial home value. We then adjust this value using zip-code-level housing price indices from Zillow to arrive at an estimate of the value of the home at the time of foreclosure. We deem the borrower to be substantially underwater if the lender's claim against the borrower is greater the estimated home value.[36]

We estimate the baseline contagion specifications for contagion among underwater and non-underwater loans. For each foreclosure case we split the count of neighboring foreclosures in two—filings among borrowers that are underwater ($N_{i,u,d(i)+t}$) and filings among borrowers in positive equity ($N_{i,p,d(i)+t}$), and redefine our outcomes in terms of these two counts: $Y_{i,j,d(i)+t} = 1\left[N_{i,j,d(i)+t} > 0\right]$, $Y_{i,j,d(i)+t} = N_{i,j,d(i)+t}$, and $Y_{i,j,d(i)+t} = \log(N_{i,j,d(i)+t})$ for $j \in \{u, p\}$. We then estimate our baseline 2SLS specification jointly allowing the time-specific fixed effects to vary with

[35] The benefit of foreclosure is muted when borrowers have more than one mortgage taken against the property (e.g., a mortgage and a home equity loan). In Illinois (as in several other states), mortgages are "recourse" debt—if the foreclosure auction generates less revenue than the value of the outstanding debt, then the borrower still owes the creditor the balance. For example, if an individual owes $100,000, and the property sells at foreclosure auction for $75,000, then the individual continues to owe $25,000. As discussed previously, in the majority of cases the lender purchases the property in the amount of the outstanding debt to avoid writing down a loss (i.e., the property becomes REO), in which case borrowers no longer owe. However, if a borrower has more than one lien against the property, there is recourse for the second lien—even if the first mortgage is entirely covered by the foreclosure, the borrower is still in debt for the value of the second mortgage. Thus, a foreclosure will not always render borrowers debt free, although will reduce their debt burden to some extent.

[36] Because this proxy is imperfect, we have experimented with alternative definitions of underwater, including a claim greater than 110 or 120% of the estimated value, a claim greater than initial loan value, outstanding balance greater than local home values, etc. Our results are not terribly sensitive to these definitions.

the type of filings under consideration:

$$Y_{i,j,d(i)+t} = \beta_0 + \beta_u \cdot F_i \cdot 1[j = u] + \beta_p \cdot F_i \cdot 1[j = p] + \beta X_i + \Phi_i + M_{j,m(i)} + \psi_{j,d(i)+t} + u_{i,d(i)+t} \quad (7)$$

Pooling the estimates for both outcomes (underwater filings and non-underwater filings) allows us to test the null hypothesis that a completed foreclosure has the same effect on underwater borrowers as borrowers in positive equity ($H_0: \ \beta_u = \beta_p$). If falling property value is driving contagion, then after a completed foreclosure (lowering neighboring home values) we would expect disproportionately more filings among borrowers who are underwater on their loans.[37]

To draw conclusions about the role that underwater borrowers play in foreclosure contagion, we rely on two assumptions. First, we assume that, were it not for their differing loan to value ratios, borrowers in positive and negative equity respond similarly to a neighboring completed foreclosure. Second, we assume that lender behavior does not vary along these margins. In other words, we rely on the assumption that any observed heterogeneity is driven by whether or not the neighbors are underwater on their loans. These two assumptions allow us to conclude that heterogeneity in treatment effects is due to differing incentives for default due to low home value, as outlined above.

We find that contagion is more prevalent among borrowers who are *not* in negative equity. We present the 2SLS estimates of Equation 7 in the upper panel of Table 11. Again, we pool five years of post-decision observations ($t \in \{0, 1, 2, 3, 4\}$), restricting the treatment to be constant across all years (we present the yearly estimates in Table A.13 of the Appendix). Note that clustering our standard errors on the census-tract dimension accommodates correlation between the same property across different years of observation. These estimates suggest that foreclosure contagion is driven primarily by borrowers who are likely not underwater on their loans (or who are only slightly underwater), and the difference in point estimates is significant for the count and log of filings. Thus, we do not find evidence in support of the hypothesis that completed foreclosures encourage additional defaults among borrowers in negative equity.[38]

[37] Our empirical strategy addresses three concerns with this sort of heterogeneity analysis. By defining our outcomes in terms of the log count of filings and the probability of any filing for each group (in addition to the raw counts), we avoid the problem that we do not observe the base number of loans in each category—we will estimate the proportional response to a completed foreclosure among each loan type. Moreover, our randomly assigned instrument ensures that, in expectation, neighborhoods with completed foreclosures have similar numbers of underwater borrowers as neighborhoods with dismissals. Studies that rely on very local analyses or aggregate fixed effects analyses for identification will face this problem that neighbors are more likely to be underwater in neighborhoods where foreclosures are more likely to occur. Finally, one concern is that there is a mechanical effect of completed foreclosure on the number of underwater borrowers—lower house prices leading to more borrowers underwater—thus, even if the default rates are entirely unchanged it will appear as though there are more filings among underwater borrowers in response to a completed foreclosure. We avoid this problem since our measure of being underwater is defined using zip-code level aggregate housing-price indices that are not likely to be largely affected by the marginal completed foreclosure.

[38] We also examine heterogeneity by local price growth, loan type and by zip-code income level (a proxy for ability to pay). Deng et al. (2000) argue that borrowers derive option value from staying in their homes—even if an asset is performing poorly at the moment, if prices are expected to increase then it may be worth waiting. We adjust our baseline contagion specification by interacting the foreclosure effect with an indicator for positive zip-code-year price growth, but find no evidence that price growth matters for contagion—see A.14. Campbell and Cocco (2011) argue that the probability of default is decreasing in ability to pay (income relative to loan payments) and that incentives for default vary depending on the structure of loan payments. Since we do not observe mortgage payments, we proxy

We interpret the presence of contagion among borrowers in positive equity (and lack of contagion among those in negative equity) as evidence that a completed foreclosure may induce contagion by conveying information to neighbors, rather than by lowering property values. We are not arguing that falling home values are not an important factor in the default decision; borrowers are likely responsive to their financial position and local economic conditions. However, our estimates provide evidence that a neighboring foreclosure is not causing contagion among those whose financial position is most likely to be affected by a drop in housing value. Thus, it does not appear that the primary cause of foreclosure contagion is falling home values (perhaps, as suggested by Towe and Lawley (2013), the pecuniary externality of completed foreclosures is too small to induce a response).

It may be that a completed foreclosure in one's neighborhood sends an informational signal that is more meaningful for those who are not underwater. Those who are underwater to begin with may be sufficiently well informed about their own financial position, and have already thought through the benefits and costs of default. On the other hand, a neighboring completed foreclosure may send a meaningful signal to those in positive equity about the neighborhood (e.g., the neighborhood is in decline and these individuals may soon be in negative equity) or about the foreclosure process itself (e.g., how to walk away from a loan and the costs and benefits thereof). In the following section, we investigate whether contagion is more pronounced in situations where a foreclosure may send a stronger signal.[39]

6.2.2 Learning from Neighbors

Contagion may occur because borrowers learn about the foreclosure process and outcomes from their neighbors. Recent survey evidence suggests that borrowers learn from defaults and foreclosures within their social networks. Guiso et al. (2013) study a survey of 8,000 home owners about their hypothetical default behavior in the face of an underwater mortgage and find that social networks may matter. Firstly, a neighbor may learn about the foreclosure process, including the costs of default (and completed foreclosure), how long the process takes, and the probability of a positive resolution (e.g., mortgage modification). In this case, it is not clear whether the foreclosure event would increase or decrease the probability of neighboring filings (contagion vs. anti-contagion). For example, a foreclosure may lower neighbors' perception of the probability of renegotiating one's loan by defaulting, thus lowering the expected value of default and discouraging this behavior (Mayer

ability to pay with zip-code-level median income (from the IRS SOI), and find no discernible difference in contagion by income quartile. These results are plotted in Figure A.3 of the appendix. We also find no significant difference between the response among neighboring borrowers with conventional fixed-rate mortgages and those with alternative mortgage products—see Table A.15 in the appendix.

[39] Given our instrument, it is also conceivable that borrowers learn about the impact of a given judge on foreclosure outcomes. However, we do not expect that foreclosure contagion is driven by a completed foreclosure revealing information about a specific judge. Firstly, given the random assignment of judges to foreclosure cases, the probability that a given borrower ends up with a given judge is low—thus, the neighboring borrower's expectations about the outcome of default should not change substantially. Secondly, if learning about judges is the primary driver of foreclosure contagion, then we would not expect to observe differential contagion among individuals with the same lenders.

et al. (2011) find evidence of borrowers defaulting in response to an increase in the probability of modification). Secondly, a completed foreclosure may send a signal about the neighborhood. For example, a neighboring foreclosure may signal to borrowers in positive equity that the neighborhood is in decline, increasing the incentive to default.[40]

To assess the importance of these non-pecuniary channels, we first examine heterogeneity in the treatment effect across neighborhoods where we expect weaker and stronger social networks. If contagion is driven by borrowers learning about the foreclosure process or social norms, then we might expect to see stronger effects in these neighborhoods. We study how contagion varies with geographic-based proxies for neighbor connectivity, including racial homogeneity, population density, and housing type (drawing on the notion of social capital outlined by Glaeser and Sacerdote (2000)), although we find no systematic relationship between contagion and these proxies. We include a detailed discussion of these estimates in the Appendix. Of course, these proxies all pertain to geographic connectedness, which is only one dimension of an individual's network. Home owners also belong to networks of co-workers, friends, and family; a completed foreclosure in any of these networks may send information to the individual.

We also investigate a specific non-geographic social network—neighbors with loans from the same lender. Individuals with the same lender may be more likely to discuss the foreclosure (and/or mortgage renegotiation) process with one another. At the same time, a successful (or failed) mortgage renegotiation provides a stronger signal to individuals with loans from the same institution; it may be that a neighbor's foreclosure discourages default among those with loans from the same servicer/lender by lowering the perceved probability of a modification. To test for the presence of learning-based contagion, we test whether contagion is stronger/weaker among neighboring loans from the same lender. Under the assumptions that i) individuals are aware of (at least some of) their neighbors' lenders and ii) lenders are not reacting to local conditions, then the difference between contagion among same-lender and different-lender loans provides evidence of the importance of learning through social networks.

To test for a difference in contagion between same-lender and different-lender borrowers, we estimate our 2SLS contagion effects for each subset of neighboring filings. We split the count of neighboring foreclosures in two—filings with the same lender listed as the plaintiff ($N_{i,s,d(i)+t}$) and filings with any other lender listed as the plaintiff ($N_{i,o,d(i)+t}$), and redefine our outcomes in terms of these two counts: $Y_{i,j,d(i)+t} = 1\left[N_{i,j,d(i)+t} > 0\right]$, $Y_{i,j,d(i)+t} = N_{i,j,d(i)+t}$, or $Y_{i,j,d(i)+t} = \log(N_{i,j,d(i)+t})$ for $j \in \{s, o\}$. We then estimate our baseline 2SLS specification jointly allowing the time-specific fixed effects to vary with lender type:

[40] Guiso et al. (2013) identify a second channel for social contagion of foreclosure: learning about social norms. Many respondents (82.7%) feel they have a moral obligation to repay their debts—they may choose to continue paying even if it is not in their financial interest. However, a neighboring default may weaken respondents' sense of moral responsibility and increase their probability of default. We do not expect this social morality channel to be particularly strong in our context, as our estimates compare neighborhoods around properties where the borrower has already defaulted—the event that sends a signal about the moral obligation to repay.

$$Y_{i,j,d(i)+t} = \beta_0 + \beta_s \cdot F_i \cdot 1[j = s] + \beta_o \cdot F_i \cdot 1[j = o] + \beta X_i + \Phi_i + M_{j,m(i)} + \psi_{j,d(i)+t} + u_{i,d(i)+t} \quad (8)$$

Pooling the estimates for both outcomes (same-lender filings and other-lender filings) allows us to test the null hypothesis that a completed foreclosure has the same effect on mortgages held by the same lender as mortgages held by other lenders ($H_0: \ \beta_s = \beta_o$).

The 2SLS estimates of β_s and β_o from Specification 8, presented in Table A.17 (pooling all years; see Table A.17 for yearly estimates), suggest that lender-specific local networks matter. The estimates show that contagion is primarily driven by foreclosure filings among loans held by different lenders—0.061 percentage point increase in the probability of any new filing on a loan from a different lender versus an (insignificant) increase of 0.006 for loans from the same lender, 0.770 additional foreclosure filings from different lenders versus a drop in filings of 0.184 among loans held by the same lender, and an increase of 0.085 in the log of total filings among different lenders versus 0.008 for filings on loans from the same lender. In the first two cases, the difference between the estimates are statistically significantly different from zero.

We interpret these same-plaintiff results as evidence that transmission of information through local social networks matter in foreclosure contagion, but also that individuals learn different things depending on the identity of their neighbor's borrower. Given that there is general contagion among loans from other lenders, it appears that borrowers experience anti-contagion when a neighbor from the same lender ends up in a completed foreclosure. A possible explanation for this difference is that the neighboring foreclosure sends different information to different individuals: contagion among those with different lenders is consistent with the foreclosure sending a broad signal about the general direction of the neighborhood (i.e., the value of the neighborhood is deteriorating), while anti-contagion among borrowers with the same plaintiff is consistent with borrowers revising downward the probability of a positive outcome (i.e., not losing their home) when a neighboring borrower with the same lender is unsuccessful.

7 Conclusion

We provide clean estimates of the effect of a completed foreclosure on neighboring residential sale prices and on neighboring foreclosure filings in Cook County, IL. We exploit a randomly assigned instrument—the set of judges who hear a foreclosure case—to compare neighborhood-level outcomes around a delinquent property that ends in completed foreclosure to a delinquent property whose foreclosure claim is dismissed. We offer three key findings.

Firstly, we find robust evidence of foreclosure contagion. A completed foreclosure leads to about 0.5 to 0.70 more foreclosure filings per year within 0.1 miles and increases the probability of observing any neighboring foreclosure filing by about 10%. Moreover, a completed foreclosure causes between 0.25 and 0.5 new completed foreclosures per year. These contagion effects persist for at least four years after the cases is decided.

Secondly, we find contagion among loans held by lenders who are known to automate foreclosure filings and are likely unresponsive to very local conditions, which we interpret as evidence that foreclosure contagion is driven by borrowers.. Additionally, contagion is not driven by borrowers with underwater loans who are likely to be affected by a drop in housing value, while it borrowers react differentially to a neighboring foreclosure depending on whether they have the same lender. These results suggest that learning plays an important role in contagion, rather than the pecuniary externality of the neighboring foreclosure.

Thirdly, we find evidence that completed foreclosures disrupt local housing markets. After a completed foreclosure, the mean residential sale price dips by as much as 40%. However, this drop is largely explained by selection into sale—in the wake of a completed foreclosure, the composition of residential sales is skewed toward lower quality (and thus, lower price) homes. Nonetheless, we cannot rule out that foreclosures do influence the value of homes, conditional on quality.

While our instrumental-variables method provides clean identification of the effect of foreclosure, the resulting estimates are of a particular parameter that helps inform foreclosure policy. Our estimates represent the effect of a completed foreclosure on the neighborhoods around properties that are most likely to be influenced by foreclosure judges. These are the cases that are most likely to be influenced by policy. At the same time, our estimates compare the neighborhood-level effect of a completed foreclosure relative to a delinquent mortgage that does not end in foreclosure. This is the relevant parameter for assessing policy that addresses how easily and how often lenders should be able to foreclose on delinquent borrowers. Finally, while there may be concerns about the external validity of our estimates, which are derived from a housing crisis in one of the worse-hit cities, it is in exactly such circumstances that policymakers and economists must worry most about foreclosure contagion. In sum, our estimates of foreclosure contagion suggest room for policy that seeks alternative solutions for delinquent borrowers.

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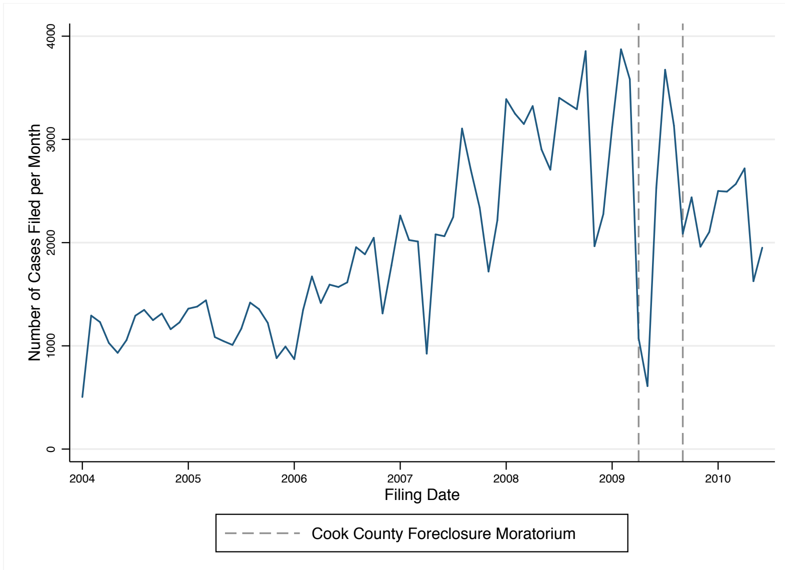
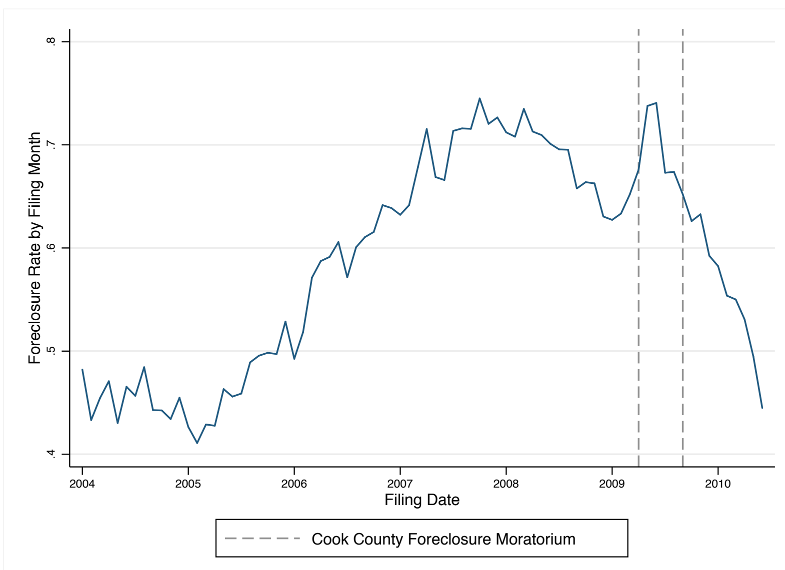
Figure 1: Foreclosure Cases Over Time in Cook County, IL

(a) New Foreclosure Filings



(b) Share of Filed Cases Ending in Completed Foreclosure



Notes: Upper panel presents the monthly count of new foreclosure filings in Cook County over time. Lower panel presents the share of cases filed in a given month that end in a completed foreclosure. Cook County suspended all new foreclosure filings starting April 16, 2009, except for so-called "consent foreclosures:" foreclosure filings in which lender and borrower had already agreed to foreclosure prior to filing. This "moratorium" was scheduled to end on September 1, 2009, although appears to have ended earlier, given the spike in filings prior to Sept. 1.

Table 1: Descriptive Statistics: Pre-Treatment Characteristics

	All	Dismissed	Foreclosed	P-Value
Case Resolved	0.950	1.000	1.000	.
Ends in Foreclosure	0.612	0.000	1.000	.
Days to Decision	373.554	274.931	428.665	0.000
Prob Redefault	0.049	0.120	0.013	0.000
Single-Family Property	0.623	0.684	0.590	0.000
Conventional Mortgage	0.647	0.665	0.653	0.000
Loan Principal	237328.100	219520.200	237733.000	0.892
Complaint Amount	229621.000	211420.400	231905.600	0.449
Large Plaintiff	0.471	0.472	0.473	0.065
Large Attorney	0.685	0.682	0.687	0.032
Median Income (tract)*	44859.720	46409.490	43748.260	0.000
Share White (tract)*	0.449	0.468	0.433	0.000
Population (tract)*	5434.558	5472.077	5411.712	0.000
N	148220	50140	90653	

Notes: Means of variables from matched foreclosure case and foreclosure filing data. *Indicates data is from the associated property's census tract (2000 Decennial Census). The sample omits VA mortgages, filings made during the 2009 Cook County foreclosure moratorium, and filings during the first calendar year in which a case calendar hears foreclosure cases.

Table 2: Descriptive Statistics: Outcomes

	All	Dismissed	Foreclosed	P-Value	N
Neighboring Filings	2.423	2.161	2.596	0.000	475127
Any Neighboring Filings	0.734	0.702	0.755	0.000	475127
Neighboring Foreclosures	0.748	0.603	0.844	0.000	475127
Any Neighboring Foreclosure	0.365	0.326	0.390	0.000	475127
Neighboring Sales	3.038	2.962	3.099	0.006	133176
Any Neighboring Sale	0.708	0.683	0.728	0.000	133176
Mean Neighboring Sale Price	169326	184213	157182	0.000	81371
Mean Repeat-sale Adjusted Price	122896	123240	122621	0.205	56306

Notes: Means of outcome variables from matched foreclosure case and foreclosure filing data. Sample omits VA mortgages, filings made during the 2009 Cook County foreclosure moratorium, and filings during the first calendar year in which a case calendar hears foreclosure cases. The sample is further restricted to cases for which we observe an outcome within five years of the case being decided; each observation represents a given year since the decision. Outcomes are measured within 0.1 miles of the associated property.

Table 3: First Stage Regression of Foreclosure Outcome on Propensity to Foreclose

	No Property Controls	Controls
Z_i	0.556***	0.554***
	(0.045)	(0.046)
1st-Stage F	150.000	147.400
N	140667	140667

Notes: ***Indicates significance at the 1% level. First-stage regression of indicator for case ending in foreclosure on leave-one-out case-calendar-filing-month foreclosure rate, and filing month and property type fixed effects. Additional (unreported) controls in second column include share of tract that report race as white in 2000 decennial census, income quartile from decennial census, whether plaintiff is a "large plaintiff" (six largest plaintiffs each representing $\geq$ 7000 filings) or attorney is a "large attorney" (three largest attorneys each representing $\geq$10,000 cases), whether mortgage is adjustable rate, size of initial loan, and census tract population. Reported standard errors (and corresponding F statistics) are the maximum of the standard error (minimum of F-stat) clustered on the census tract, clustered on filing month, and multi-way clustered on tract and filing month.

Table 4: Balance of Covariates

	Coefficient	p Value
Adj. Rate Mortgage	0.0005240 (0.0006250)	0.3837
Loan Principal	-0.0000033 (0.0000025)	0.0343**
Large Plaintiff	-0.0005760 (0.0003830)	0.9438
Large Attorney	-0.0007000 (0.0007400)	0.1370
Median Income (tract)	-0.0002720 (0.0001710)	0.6566
Share White (tract)	-0.0002550 (0.0005690)	0.4025
Population Density (tract)	0.0558000 (0.0353000)	0.1364
Price (Zip code)	0.0000507 (0.0000406)	0.8564
p Value	0.4280	
N	143276	

Notes: **Indicates significance at the 5% level. First column represents the coefficient from a regression of the instrument (case-calendar-filing-month foreclosure rate) on the given pre-treatment covariates, controlling for filing month and property type fixed effects. The reported p value in the first column is from a joint significance test for the given variables. Second column reports p values from the following procedure: the given covariate (for that row) is regressed on a full set of case calendar dummies (plus filing month and property type fixed effects), where the p value is for a joint significance test of the case calendar dummies. For all regressions, standard errors are clustered as in Table 3 (and the baseline 2SLS specification).

Table 5: Complier Characteristics

Quartile of Income			
1	2	3	4
0.785	1.012	1.059	1.066
Loan Characteristics			
Large Lender	Conventional Mortgage	Positive Zip Code Price Growth	Underwater
1.002	0.896	0.794	0.781

Notes: Ratio of the first-stage estimate (effect of instrument on indicator for completed foreclosure) for given subgroup to the first-stage estimate for the full sample. Income quartile is given by the tract-level quartile of median tract income from the 2000 Decennial Census. Large lender is an indicator for the plaintiff being one of the six most prominent banks in the sample, each representing $\geq$ 10% of filings. Zip-code-level annual price growth is taken from Zillow housing price indices for Cook County. Underwater is a proxy for the outstanding balance of the mortgage being greater than the estimated value of the home, as described in the text.

Table 6: Baseline Contagion Estimates

Years Since Decision		0	1	2	3	4	5
Any Filing per Year	2SLS	0.052*	0.012	0.082***	0.090*	0.247**	0.140
		(0.028)	(0.027)	(0.027)	(0.053)	(0.112)	(0.133)
	1st-stage F	238.300	224.200	205	24.620	19.060	12.970
	N	130199	118566	93143	67379	41958	23831
Total Filings per Year	2SLS	0.691*	0.670*	0.536***	0.657**	1.551	0.538
		(0.393)	(0.368)	(0.183)	(0.319)	(0.983)	(0.987)
	1st-stage F	238.300	224.200	205	24.620	19.060	12.970
	N	130199	118566	93143	67379	41958	23831
		Cumulative Effects					
Cumulative Filings	2SLS	0.691*	1.395*	2.090***	4.522***	6.446**	5.610
		(0.393)	(0.732)	(0.794)	(1.169)	(3.105)	(4.610)
	1st-stage F	238.300	224.200	205	24.620	19.060	12.970
	N	130199	118566	93143	67379	41958	23831
Cumulative Filings No Recent Foreclosures	2SLS	0.622	1.266*	1.594**	3.820***	4.076	2.693
		(0.380)	(0.667)	(0.703)	(1.429)	(2.918)	(4.611)
	1st-stage F	172.500	159.700	138	20.910	12.790	7.515
	N	71925	66995	55105	43186	27395	14948

Notes: ***Indicates significance at the 1% level, **5%, and *10%. OLS estimates of case ending in completed foreclosure on the given outcome are from a regression of the outcome . 2SLS estimates of the effect of complected foreclosure on given outcome (measured within 0.1 miles of the property in the given year since the case is decided), on an indicator for the case ending in foreclosure, filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3,- instrumenting the foreclosure outcome with the leave-one-out case-calendar-filing-month-specific foreclosure rate. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Filings outcomes represent new foreclosure filings within the given radius and time period only.

Table 7: Pre-Filing Estimates

Years Before Case is Filed	3	2	1
Any Filing	0.003	0.009	0.018
	(0.029)	(0.029)	(0.022)
1st-stage F	400	400	400
N	140672	140672	140672
Total Filings per Year	0.020	0.084	0.214
	(0.087)	(0.122)	(0.139)
1st-stage F	400	400	400
N	140672	140672	140672
Any Completed Foreclosure	-0.027	0.045*	-0.004
	(0.023)	(0.025)	(0.030)
1st-stage F	159.400	182.100	164.300
N	140672	140672	140672
Total Completed Foreclosures per Year	-0.057*	0.075	0.110
	(0.033)	(0.065)	(0.071)
1st-stage F	159.400	182.100	164.300
N	140672	140672	140672
log(price)	0.007	0.037	0.007
	(0.038)	(0.042)	(0.049)
1st-stage F	164.900	166.500	136.400
N	109123	102220	84126

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year before the case is filed) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes are measured as discussed in the text and in the notes to Tables 9 and 6.

Table 8: Baseline Completed Foreclosures

Years Since Decision	0	1	2	3	4	5
Any Completed Foreclosure	-0.048	0.002	0.045	0.138***	0.033	0.174
	(0.031)	(0.030)	(0.038)	(0.051)	(0.106)	(0.144)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
Total Completed Foreclosures	0.280*	0.435***	0.312***	0.558***	0.243	0.063
	(0.164)	(0.160)	(0.117)	(0.185)	(0.316)	(0.334)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
	Contagion in Completed Foreclosures Filed Prior To Decision					
Any Completed Foreclosure	-0.043	0.135***	0.073***	0.025	-0.010	0.003
	(0.031)	(0.028)	(0.019)	(0.016)	(0.017)	(0.015)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831
Total Completed Foreclosures	0.303*	0.609***	0.111***	0.031	-0.010	0.003
	(0.164)	(0.118)	(0.028)	(0.019)	(0.017)	(0.015)
First-stage F	238.300	224.200	205	24.620	19.060	12.970
N	130199	118566	93143	67379	41958	23831

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year since decision) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3.

Table 9: Baseline Housing Market Estimates

Years Since Decision		0	1	2
log(price)	2SLS	-0.127	-0.411**	-0.358
		(0.105)	(0.203)	(0.395)
	1st-stage F	22.500	18.840	14.390
	N	43079	26047	12241
log(price) (repeat-sales)	2SLS	0.059	0.003	-0.251
		(0.066)	(0.146)	(0.201)
	1st-stage F	11.130	6.314	7.355
	p-value	0.107	0.030	0.763
	N	30482	17916	7904
Any Sale	2SLS	0.014	0.026	0.035
		(0.046)	(0.127)	(0.152)
	1st-stage F	24.620	19.060	12.970
	N	67379	41958	23831
Total Sales (Cumulative Over Years)	2SLS	0.220	6.562	9.964
		(0.914)	(4.574)	(9.255)
	1st-stage F	24.620	19.060	12.970
	N	67379	41958	23831

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of complected foreclosure on given outcome (measured within 0.1 miles of the property in the given year since the case is decided), on an indicator for the case ending in foreclosure, filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3,- instrumenting the foreclosure outcome with the leave-one-out case-calendar-filing-month-specific foreclosure rate. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Sales outcomes represent residential transactions within the given radius and time period only (total sales represents the total count of sales since the decision year), while price represents the mean sale price of these transactions. Repeat-sales adjusted prices are estimated as outlined in the text; associated p-value is for the cross-equation test of equality between the repeat-sales adjusted and unadjusted (first panel) price effects.

Table 10: Response Among Loans with Lenders Known for Automating Foreclosure Proceedings

	Outcome Variable	Any Filing	Total Filings	Log Filings
Baseline	Effect of Completed Foreclosure	0.062***	0.735**	0.094*
		(0.022)	(0.320)	(0.050)
	First-stage F	131.900	131.900	147.100
	N	311116	311116	235325
Automating Lenders	Effect of Completed Foreclosure	0.039*	0.115	0.084*
		(0.022)	(0.073)	(0.049)
	First-stage F	131.900	131.900	138
	N	311116	311116	86054

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Sample is restricted to cases where multiple lien holders are listed in the foreclosure filing. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property each year since decision; one observation per year) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes for "Automating Lenders" are measured based on new foreclosure filings by lenders implicated in the Office of the Comptroller of the Currency and the Board of Governors of the Federal Reserve System Independent Foreclosure Review Settlement, while "Baseline" includes new foreclosure filings among all lenders. Both samples are restricted to cases decided between 2007 and 2010 (during which automation of foreclosure filings is thought to be most common).

Table 11: Filings from Borrowers in Positive vs. Negative Equity

Outcome Variable	Any Filing	Total Filings	Log Filings
Filings Among Non-Underwater Borrowers	0.045**	0.715***	0.108***
	(0.020)	(0.242)	(0.041)
Filings Among Underwater Borrowers	0.032**	-0.086	0.008
	(0.016)	(0.060)	(0.040)
First-stage F	84.220	84.220	87.690
p val for difference between groups	0.569	0.000***	0.005***
N	902490	902490	404152

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year since decision) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes are for two separate counts of new foreclosure filings by either borrowers in negative equity or borrowers in positive equity (as defined in the text). P-value tests the significance between the two responses (positive vs. negative equity)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

Table 12: Response Among Borrowers with the Same Lender

Years Since Decision	Any Filing	Total Filings	Log Filings
Any Filing Among Different Lender	0.061***	0.770***	0.085**
	(0.022)	(0.245)	(0.041)
Any Filing Among Same Lender	0.006	-0.184***	0.008
	(0.015)	(0.056)	(0.045)
First-stage F	87.490	87.490	93.560
p val for difference between groups	0.052*	0.000***	0.109
N	796112	796112	330880

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year since decision) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes are measured based on new foreclosure filings by either different lenders than that in the observed case or the same lender. P-value tests the significance between the two responses (same lender vs. other lender)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

A Online Appendix—Not for Publication

A.1 Data Appendix

A.1.1 Cleaning Court Records

We collected all chancery court case records filed between January 2004 and June 2010 (inclusive). We extract from each record the associated case number and the case calendar to which the case is assigned. The records also contain a list of case actions, the lawyer who initiated this action, the associated judge, and the date. We extract this list of actions (simple text descriptions, e.g., “Amend complaint or petition - allowed” or “Dismiss by stipulation or agreement”) and the corresponding dates.

We identify a case as ending in a dismissal if an action occurs containing one of the following descriptions: "mortgage foreclosure motion plaintiff dismissed", "mortgage foreclosure voluntary dismissal, non-suit or dismiss by agreement", "mortgage foreclosure motion defendant dismissed", "mortgage foreclosure dismissed for want of prosecution", "dismissed for want of prosecution", "general chancery - dismissed for want of prosecution", "general chancery - voluntary dismissal, non suit, dismiss by agreement", "mortgage foreclosure voluntary dismissal, non-suit or dismiss by agreement", or "mortgage foreclosure judgment for defendant"; or an action containing any of the following: "case dismissed", "voluntary dismissal", "declaratory judgment voluntary dismissal", "dismiss entire cause" and not "denied", or "dismiss by stipulation or agreement" and not "denied". For dismissed cases, we consider the end of the case to be the date of this “dismissal” action (in the case of multiple such actions, we take the final).

A.1.2 Cleaning RIS Data

Record Information Services, Inc. provided us with details of foreclosure filings and foreclosure auctions for Cook County from 2002 through 2011. RIS is a private data provision company that collects publicly available records on all foreclosure filings in the five counties of Chicago. RIS employees manually input data on each foreclosure filing. From the foreclosure filings, we extract the associated chancery court case number, unique loan ID, the filing date, details of the associated loan (origination date, principal at origination, outstanding claim at time of foreclosure filing, a general indication of mortgage type—conventional, adjustable rate, etc.), details of the associated property (latitude and longitude, census tract, zip code, property type—condo, single family, etc.), and the parties involved (defendant name, plaintiff—the lender or servicer—identity, plaintiff law firm).

We identify a case as ending in a completed foreclosure if there is an associated foreclosure auction record in the RIS data. For completed foreclosures, we use the date of the foreclosure auction as the end-date of the case. If there is both a dismissal action in the court records and an associated foreclosure auction, we consider the case to have ended in a completed foreclosure, although, our results are not sensitive to this decision. Relatedly, there is a field in the RIS data that indicates the outcome of the auction, including if the auction is canceled. Since this information is missing for half of the years and since it is not indicated why a cancellation occurs, we do not code canceled auctions as dismissals in our analysis sample. Again, however, our baseline results are not sensitive to coding canceled auctions as dismissals. We consider a borrower to have redefaulted if a foreclosure filing is brought against the same loan ID. Note that this will not count new filings at the same property for different loans (e.g., if the home owner has filings against mortgage and a home equity loan, we count these as distinct filings). However, if a second filing against the same loan ID occurs within 180 days of the initial filing, we consider this to be the same case, taking the first date as the true filing date (and merging info from the two filings).

We construct a unique ID for each plaintiff and attorney as follows. For plaintiffs and attorneys on more than ∼100 cases, we manually checked the names for consistency and constructed a unique ID number. We then identified "large plaintiffs" as those plaintiff-IDs associated with greater than 7000 cases, and "large attorneys" as those attorney-IDs associated with greater than 10000 cases. We identify by name all plaintiffs implicated in the Independent Foreclosure Review Settlement conducted by the Office of the Comptroller of the Currency and the Board of Governors of the Federal Reserve System.

We match the RIS and chancery court records by case number. We discard non-matches, which arise due to several factors: non-foreclosure chancery court cases (e.g., name changes, mechanic's liens) will not appear in the RIS foreclosure filings; differing date ranges between the two data sources (2004–2010 for court records, 2002–2011 for RIS); and differing geographies (RIS data includes some cases in neighboring counties).

A.1.3 Cleaning Census Data

We merge in the following census-tract-level data from the 2000 Decennial Census: median tract income, population, land area, and share of population that identifies as each census-designated race. We construct tract-level income quartiles (i.e., what quartile of median income does a given tract fall into), an indicator for being a predominantly white tract (share white is greater than the median share), population density (and associated quartiles), and a Herfindahl-Hirschman index for each race (i.e., the sum of the squared share of each race in the tract). We merge the census tract data to the RIS data using census tract FIPS codes.

A.1.4 Cleaning Deeds Records

These data are collected from the county recorder for Cook County, IL. The records were collected by an anonymous private firm and made available to us by the Paul Millstein Center for Real Estate at the Columbia Graduate School of Business. These data include the date of each sale property transaction, the type of property, the address, the price of the sale, and an indicator for the property being residential. We drop all transactions with sale price or address missing. We drop duplicate records—multiple sales with identical sale prices that occur at the same property within 30 days of one another. We keep only residential sales. We geocode these deeds records based on the property address using Yahoo! Placefinder.

A.1.5 Defining Outcomes

Using the cleaned and matched RIS and court records, for each foreclosure case we calculate the distance between the associated property and the properties associated with all other foreclosure filings. We then count the number of new filings around each property (i.e., within the given radius; 0.1 miles in the baseline) in each calendar year, omitting from the count new filings at the same property or filings associated with the same case—e.g., a given loan may be tied to multiple properties, which we do not want to include in the count. If there are multiple foreclosure filings at a neighboring property, we include each of these in the count (although we have found that our results are not sensitive to treating these as a single new filing). For each calendar year, we then construct an indicator for there being any new foreclosure filing. We then identify the year that the case is decided and define our contagion outcomes relative to that year: number of filings and indicator for any filing in the year the case ends, number and indicator one year after the case ends, two years after, and so on. We follow the same procedure to get a count of new completed foreclosures in each calendar year: calculate the distance between the property associated with each case and each completed foreclosure (from the RIS auction data), and count the auctions that occur within the given radius in the given calendar year (where this date is based on the auction date).

We also construct several sub-counts of new foreclosure filings. Foreclosure filing records in the RIS data have a field reporting additional lien holders that are listed on the foreclosure claim (reporting additional lien holders is optional for plaintiffs). For each filing we create an indicator for additional lien holders. We then construct the same contagion outcomes, but only counting filings with multiple lien holders. Similarly, we construct our contagion outcomes using the annual count of new filings from plaintiffs implicated in the Independent Foreclosure Review Settlement and, as described in the text, use this to investigate contagion among lenders known for the bulk processing of delinquent loans into foreclosure filings. Thirdly, we construct an indicator for the borrower being underwater on their loan: we take the principal of the mortgage at origination, conservatively assume an 80% loan-to-value ratio to back out the value of the property, adjust the value of the property using Zillow monthly zip code housing price indices for the year of origination and the year of the foreclosure filing, and compare the adjusted value of the property to the claim made against the borrower by the plaintiff. If the claim is larger than the value of the property, then we consider the borrower to be "underwater". For each property associated with a foreclosure filing, we then find the annual count of filings against underwater borrowers within the given radius. We have experimented with other ways to construct this underwater indicator—claim larger than loan principal at origination, claim larger than 110% of estimated value, and so on—and find little difference in our results. Finally, we construct contagion outcomes for filings from the same lender. We restrict our sample to the set of cases for which we cleaned the plaintiff name (i.e., filings with plaintiffs who appear on approximately more than 100 filings). For each case within this subsample, we identify all filings within the given radius and create an indicator for the neighboring filing having the same plaintiff. We then find the annual count of new filings from the same plaintiff and new filings from other plaintiffs.

We construct our housing market outcomes in a very similar way. For each foreclosure filing, we calculate the distance between the associated property and all residential sales in the deeds records. For each calendar year, we take the mean sale price of all sales occurring within that year within the given radius (0.1 miles in the baseline) of the property associated with the foreclosure filing. We then use as an outcome the log of this sale price. In the following section, we discuss how we use repeat sales do adjust this average sale price for fixed property quality. We also use as an outcome the count of residential sales that occur within the given radius in the given calendar year and an indicator for any sale occurring.

A.2 Adjusting Price Data for Property Quality Using Repeat Sales

In our deeds records, we first identify all repeat sales: of the 1,330,949 residential sales we observe between 1995 and 2008, there are 585,756 (44.01%) properties that transact more than once, which leaves us with 216,068 transactions during the relevant period of 2004 – 2008 (43.56% of the 496,055 residential transactions in this period). For each property, k, we assume that the sale price in year t, P_{kt}, is a function of the property's time-invariant characteristics, δ_k, the year of sale, and whether or not there is a recent foreclosure nearby, $F_{i(k)t}$:

$$\log(P_{kt}) = \alpha_0 + \alpha\delta_k + \Psi_t + \alpha_F F_{i(k)t} + \epsilon_{kt} = \alpha_0 + \alpha\delta_k + \Psi_t + e_{kt} \tag{9}$$

where Ψ_t is a year-specific fixed effect and we denote for convenience $e_{kt} \equiv \alpha_F F_{k(i)t} + \epsilon_{kt} = \log(P_{kt}) - \alpha_0 - \alpha X_k - \Psi_t$. We want a measure of the sale price, P^*_{kt}, that removes the influence of property characteristics, but allows price to vary with foreclosure:

$$\log(P^*_{kt}) = \beta_0 + \phi_t + e_{kt}$$

To achieve this, we estimate a simple price regression that controls for property and year of sale for all repeat sales in our sample: $\log(P_{kt}) = \beta_0 + \theta_k + \Psi_t + e_{kt}$, where θ_k is a vector of property fixed effects and Ψ_t is a vector of year-of-sale fixed effects. Property fixed effects absorb the influence of the (time-invariant) property characteristics. Using the OLS parameter estimates and residuals from this model, we then estimate $P^*_{kt} = \exp\left(\hat{\beta}_0 + \hat{\Psi}_t + \hat{e_{kt}}\right)$. Using these quality-adjusted sales prices, we then construct a quality-adjusted measure of sale price for each property i going through the foreclosure courts by taking the log of the average of all P^*_{kt} that transact within x miles of property i in the relevant year of observation.

A.3 Monotonicity of Instrument

A failure of monotonicity occurs if a higher value of the instrument means a higher probability of foreclosure for some cases, but a lower probability for others. As discussed in the main text, a failure of monotonicity may arise if judges treat different types of borrowers and lenders differently.

We examine this possibility by relating foreclosure rates for each case calendar for different subgroups to the overall value of the instrument for that case calendar. We want to check that a higher value of the instrument for the case calendar is associated with a higher foreclosure rate for the sub-groups. We first calculate the overall foreclosure rate by case calendar and filing year, and de-mean these estimates by filing year. [41]We then take a given covariate (e.g., the borrower is from a predominantly white neighborhood) and calculate the foreclosure rate by case calendar, filing year, and the value of the covariate (e.g., foreclosure rate by case calendar, filing year, and whether predominantly white neighborhood), and again de-mean by filing year. We plot the de-meaned group-specific foreclosure rates against the de-meaned general case-calendar-filing-year foreclosure rate and display these plots in Figures A.1 and A.2. A failure of monotonicity as we described above suggests that for certain subgroups a higher general case-calendar foreclosure rate is associated with a higher group-specific foreclosure rate, while for other subgroups a higher general case-calendar foreclosure rate is associated with a lower group-specific foreclosure rate. We construct six such plots: i) comparing foreclosure rates between properties in census tracts where the share of white residents is greater than the median to those below the median, ii) comparing properties in each quartile of median tract-level income, iii) comparing foreclosure rates among conventional (fixed-rate) mortgages vs. unconventional mortgages (adjustable rate, interest only, etc.), iv) comparing foreclosure rates by property type, v) comparing cases where the plaintiff is a large lender to those with smaller lenders, and vi) comparing foreclosure rates among cases where the lender's attorney is a large vs. smaller attorney (as previously defined). Figure A.1 shows that there is no evidence of a failure of monotonicity. In all cases, there is a clear positive relationship between the overall case-calendar-filing-year foreclosure rate and the group-specific case-calendar-filing-year foreclosure rate—a higher value of the instrument is associated with a higher foreclosure rate in each subgroup. Thus, in terms of observables—property type, loan type, whether the plaintiff is a large bank or employs a large attorney, and census tract demographics—the monotonicity assumption appears valid.

A.4 Lender Response to a Completed Foreclosure

For any given mortgage, divide the remaining life of the loan into three periods ($t \in 0, 1, 2$). Consider a mortgage in which the borrower owes a payment of m_t in each period, t and the mortgage is fully paid back as of $t = 2$. Suppose the borrower misses their payment in the current period ($t = 0$). With probability α_1 the borrower will still be delinquent the following period and the lender can

[41]Recall that our estimates all include filing date fixed effects, so the relevant comparison is within filing date, although the figures are similar if we do not de-mean. We use filing year for this exercise to decrease noise in the foreclosure rate estimates; the story does not change if we use filing month, although the associated figures are noisier.

foreclose on the property, in which case the lender recovers the value of the home, P, less the costs of foreclosure, λ (e.g., legal fees). However, with probability $1-\alpha_1$, the borrower will recover in period 1 and will resume making payments. Then the value of the unmodified loan to the lender is:

$$V_u = \alpha_1 \cdot (P-\lambda) + (1-\alpha_1) \cdot \left[m_1 + \frac{m_2}{R}\right] \tag{10}$$

where R is the discount rate. The lender may instead choose to modify the loan, which reduces subsequent loan payments to $m_1' < m_1$ and $m_2' < m_2$.

Lenders will be willing to modify a mortgage when modification is very effective in reducing the probability of non-payment or when the necessary reduction in the value of the loan is small. By lowering payments, modification reduces the probability of default to $\alpha_1' < \alpha_1$.[42]Then the value of the modified loan is:

$$V_M = \alpha_1'(P-\lambda) + (1-\alpha_1')\left[m_1' + \frac{m_2'}{R}\right]$$

Thus, the lender will choose modification when the value of the modified loan is larger than the unmodified loan, or the difference between the two is positive:

$$V_M - V_u = (\alpha_1 - \alpha_1') \cdot \left[m_1' + \frac{1}{R}m_2' - (P-\lambda)\right] - (1-\alpha_1) \cdot \left[m_1 + \frac{1}{R}m_2 - (m_1' + \frac{1}{R}m_2')\right] > 0 \tag{11}$$

Since $\alpha_1 - \alpha_1' > 0$, $1-\alpha_1 > 0$, and $\left[m_1 + \frac{1}{R}m_2 - (m_1' + \frac{1}{R}m_2')\right] > 0$, even if the probability of re-default under modification is zero (i.e., even if $\alpha_1' = 0$), modification is still not optimal if the modified payments are too low. For example, lenders will never modify if the net-of-foreclosure-cost value of the property is greater than the present value of the modified mortgage payments (i.e., $m_1' + \frac{1}{R}m_2' < (P-\lambda)$). When the value of the modified payments are high enough, lenders are more inclined to modify when it is very effective in reducing the probability of redefault (i.e., the smaller is α_1').

Assuming that the default probabilities are constant (which shuts down the borrower contagion channel), a neighboring completed home foreclosure—which lowers the value of the property under consideration—discourages new foreclosure filings by lenders. Taking the derivative of $V_M - V_u$ with respect to the value of the home, we see that $\frac{\partial V_M - V_u}{\partial P} = \alpha_1' - \alpha_1 < 0$, relying on the assumption that modification lowers the probability of future default. A drop in the value of the home encourages mortgage modification—selling the property at auction is relatively less appealing to the lender than the modified mortgage.

We derive similar conditions for modification by mortgage servicers. Mortgage servicers are

[42] Adelino et al. (2009) allow home prices to change across periods, however, for the purpose of this paper little generality is lost by assuming no price growth. Similarly, Adelino et al. (2009) operate under the assumption that modification guarantees payment in period 1 with a non-zero probability of default in period 2. Again, this does not fundamentally change the implications of the framework.

typically employed by lenders to collect mortgage payments and to manage mortgage defaults. When a mortgage is current, servicers receive a share of the interest payments that they collect. However, when a borrower is delinquent, servicers are required to forward payments to the holder of the debt while they manage the default (either by modifying the loan or seeking foreclosure). While managing a default, the servicer must incur all associated costs (e.g. legal fees). If the default ends in foreclosure, the servicer is reimbursed for all foreclosure-related expenses and fees. Thus, the value to the servicer of foreclosing on the delinquent loan (as in the above framework) is:

$$V_u^s = \alpha_1 \cdot \Pi + (1 - \alpha_1) \cdot \left[\rho \cdot m_1 + \rho \cdot \frac{m_2}{R}\right] \tag{12}$$

where Π is the total value of all foreclosure-related fees charged by the servicer and where ρ is the share of the mortgage payment that is returned to the servicer. When the loan is modified, the servicer is generally not reimbursed for any related expenses, and so the value of the modified loan to the servicer is:

$$V_M^s = \alpha_1' \cdot \Pi + (1 - \alpha_1') \left[\rho \cdot m_1' + \rho \cdot \frac{m_2'}{R}\right] - C_M$$

where C_M is the cost of modification (e.g., time/labor spent in negotiations). Notice that if the modification is successful, the servicer receives a lower monthly payment for servicing the mortgage. Then the servicer will prefer to modify when:

$$V_M^s - V_u^s = (\alpha_1' - \alpha_1) \cdot \left[\rho m_1' + \frac{\rho}{R} m_2' - \Pi\right] - (1 - \alpha_1) \cdot \rho \left[m_1 + \frac{1}{R} m_2 - \left(m_1' + \frac{1}{R} m_2'\right)\right] - C_m > 0$$

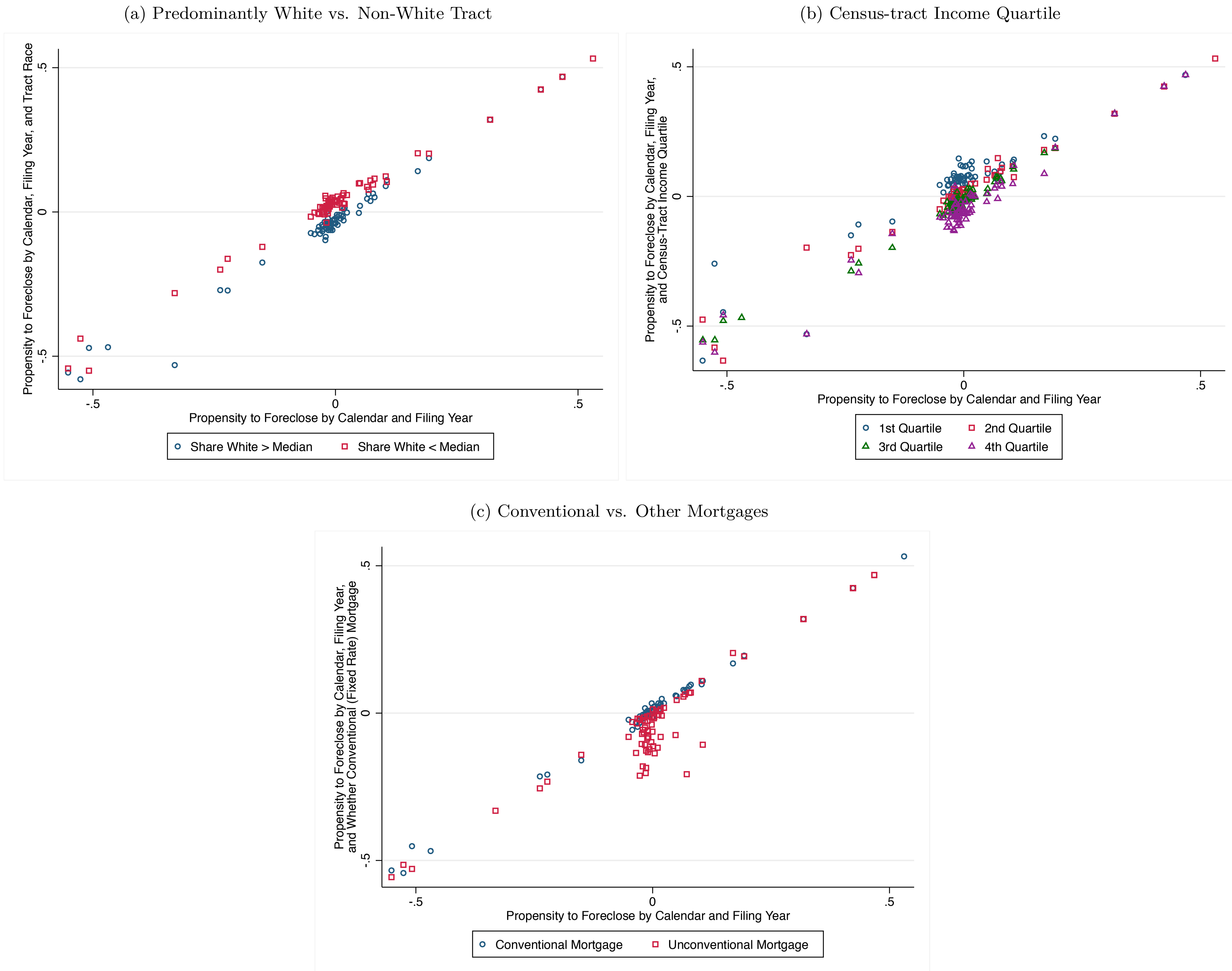
The servicer's incentives are similar to the lender's incentives, although as pointed out by Levitin and Twomey (2011), since servicers' fees have seniority over all other claims against a property, servicers are "indifferent to the amount of the [foreclosure] sale proceeds." In other words, since the payoff to a servicer of a completed foreclosure is the foreclosure fees, the servicer does not care about the value of the property (as long as it is high enough to cover their fees). Moreover, high costs of modification (which are not reimbursed) will push the servicer in favor of foreclosure. Notice, then, that if the probabilities of foreclosure (α_1 and α_1') are invariant to a neighboring completed foreclosure, the servicer will not experience foreclosure contagion ($\partial \left(V_M^s - V_u^s\right) / \partial P = 0$).

In summary, under the assumptions that a neighboring completed home foreclosure lowers the housing value P and that borrowers are unresponsive to the neighboring foreclosure so that α_1 and α_1' are unchanged, we should observe "anti-contagion" in home foreclosures: if only lenders and servicers are responding, a completed home foreclosure should discourage neighboring foreclosure filings. Of course, if a neighboring completed foreclosure has no influence on housing values and borrowers are unresponsive, then lenders and servicers should not respond (we should see no contagion at all).

A.5 Estimates by Proxies for Social Connectedness

We find little systematic relationship between the extent of foreclosure contagion and several proxies for social connection. Our first attempt to proxy social connectedness is to stratify by neighborhood diversity, operating under the assumption that neighbors with similar background maintain stronger social ties. For each census tract, we calculate a Herfindahl index of neighborhood diversity using race-population shares from the 2000 Decennial Census. We then estimate the baseline treatment effect for each diversity quartile to examine whether contagion is stronger in less diverse (high-index) census tracts. We also estimate the baseline specification and interact the treatment effect with an indicator for the foreclosure taking place in a census tract where a single race makes up more than 75% of the population. Our second set of proxies draw on the notion of social capital outlined by Glaeser and Sacerdote (2000), who argue that social connections are higher when residents live in close proximity to one another. For example, studying survey data, Glaeser and Sacerdote (2000) find that there are higher levels of civic participation among residents of large condo buildings than single-family housing. We proxy neighbor proximity in two ways. First, we interact the foreclosure treatment effect with an indicator that the home undergoing foreclosure (the unit of observation) is a condominium unit. Second, we stratify census tracts by population density (again using data from the 2000 Decennial Census). In both of these cases, while we continue to find evidence of contagion, there is no systematic relationship between foreclosure contagion and our proxies for social connection. Our social-contagion estimates can be found in Table A.16 and Figure A.3 of the Appendix.

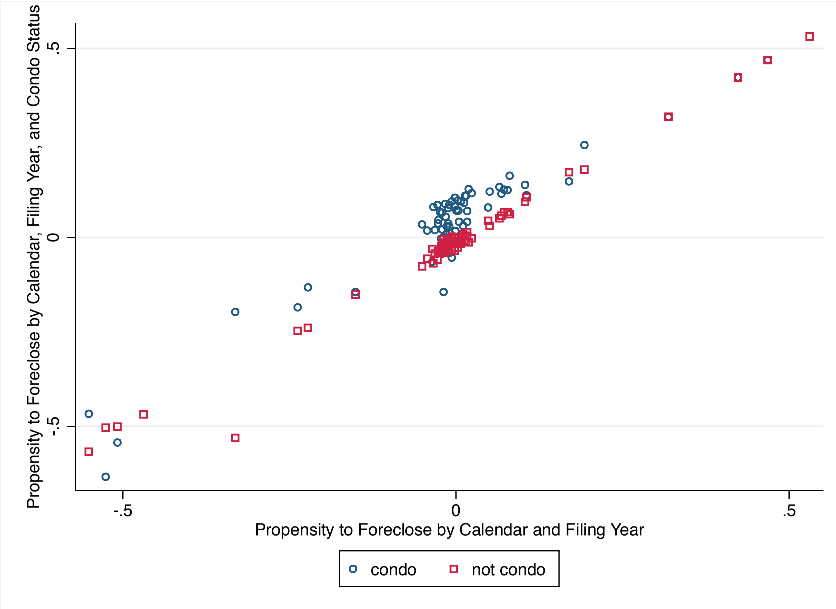
Figure A.1: Calendar-Group-Specific Foreclosure Rates vs. Calendar-Specific Rates



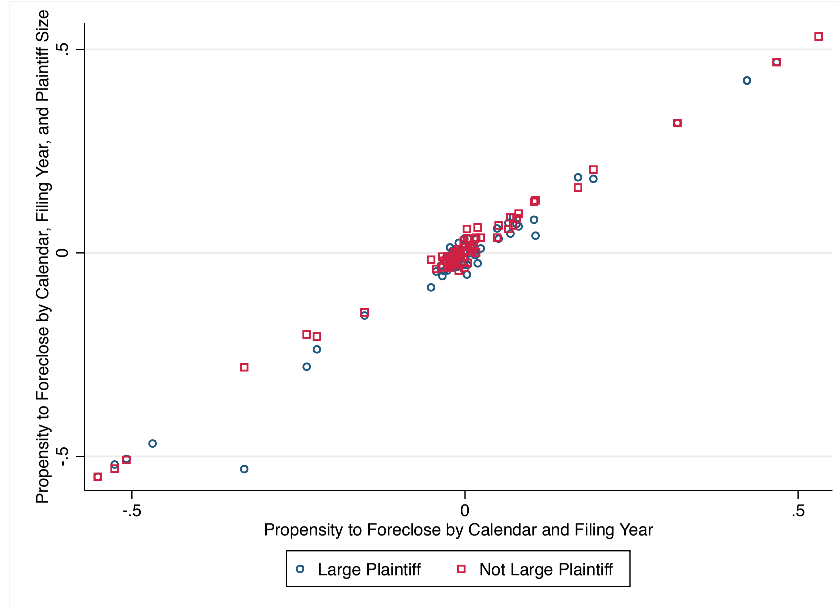
Notes: Filing-year foreclosure rates are calculated for each indicated sub-group by case calendar, demeaned by filing-year, and plotted against overall foreclosure rates for the given calendar and filing year (again, demeaned by filing year). A predominantly white census tract has an above-median share of white residents as of the 2000 Decennial Census, income quartiles are calculated at the census tract level using median income from the 2000 Decennial Census, conventional mortgage includes all standard fixed-rate mortgages (while unconventional mortgages includes adjustable-rate mortgages, balloon-payment mortgages, reverse mortgages, and interest-only mortgages; we exclude VA mortgages).

Figure A.2: Calendar-Group-Specific Foreclosure Rates vs. Calendar-Specific Rates

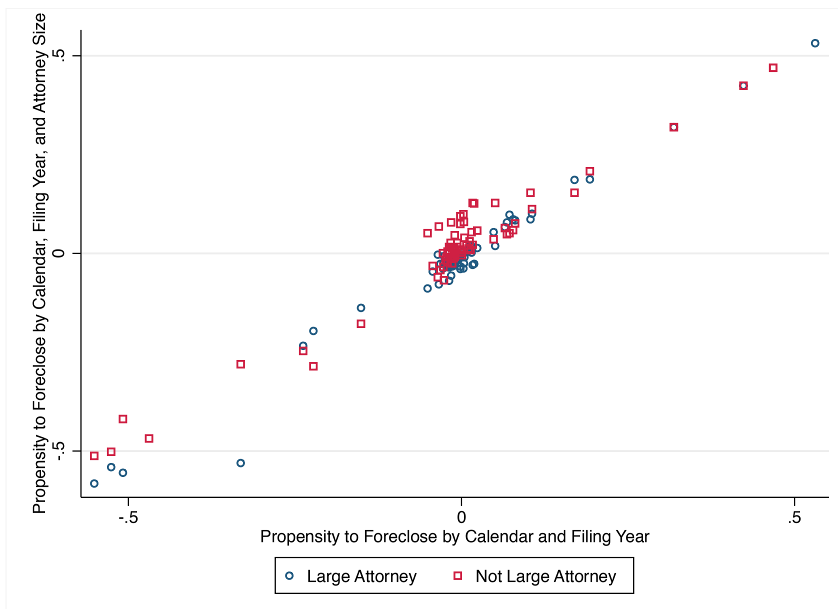
(a) Condominiums vs. Non-Condominiums



(b) Large Lender vs. Other Lender



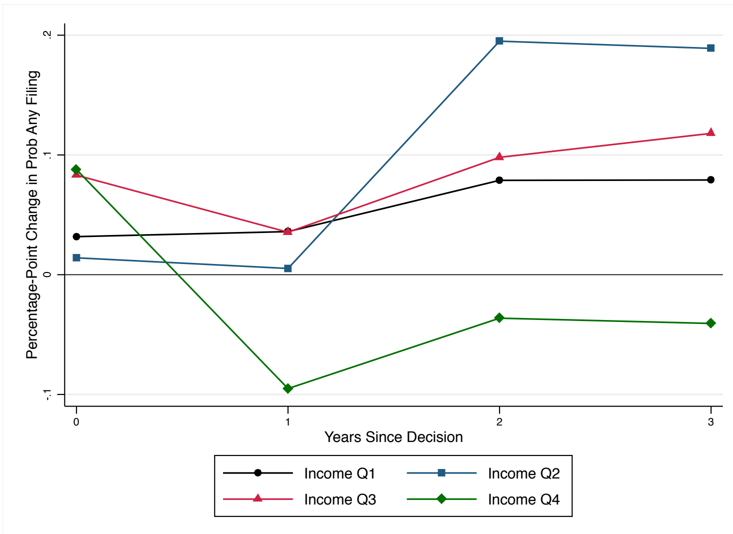
(c) Large Attorney vs. Other Attorneys



Notes: Filing-year foreclosure rates are calculated for each indicated sub-group by case calendar, demeaned by filing-year, and plotted against overall foreclosure rates for the given calendar and filing year (again, demeaned by filing year). A large lender is a plaintiff who appears on more than 7000 of the foreclosure cases filed in cook county, while a large attorney appears on greater than 10,000 cases.

Figure A.3: Contagion Estimates by Income Quartile

(a) Effect of Foreclosure on Any New Foreclosure Filings



(b) Effect of Foreclosure on Log New Foreclosure Filings

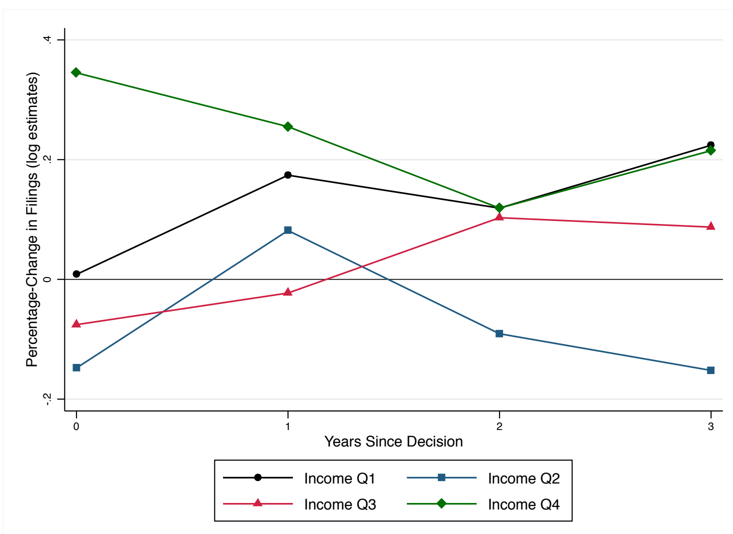
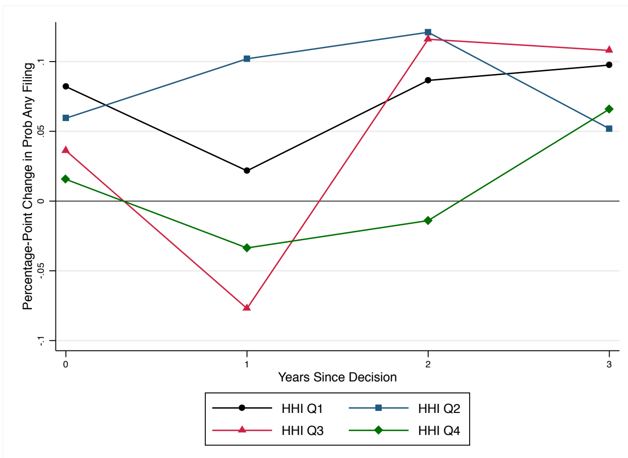
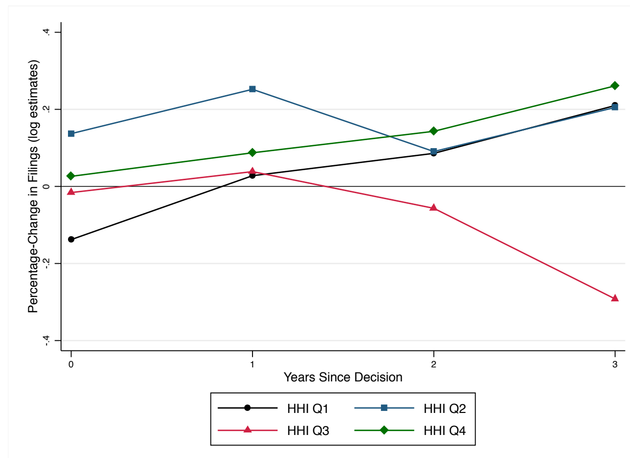


Figure A.4: Contagion Estimates by Social Connection Proxy

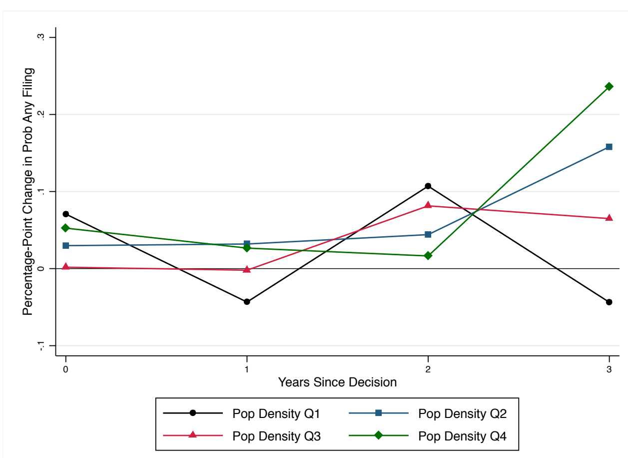
(a) Effect of Foreclosure on Any New Foreclosure Filings by Census Tract Diversity



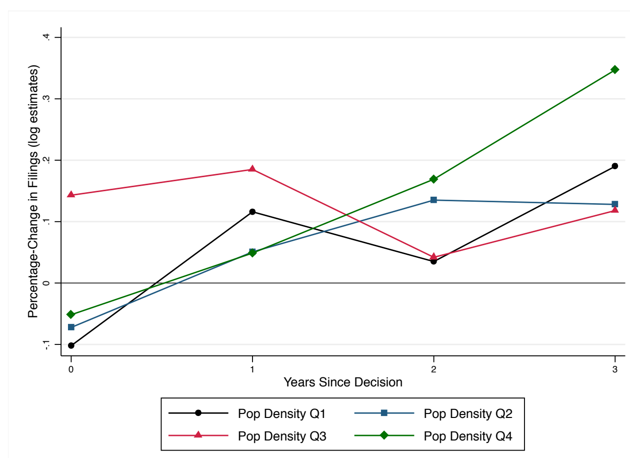
(b) Effect of Foreclosure on Log New Foreclosure Filings by Census Tract Diversity



(c) Effect of Foreclosure on Any New Foreclosure Filings by Census Tract Density



(d) Effect of Foreclosure on Log New Foreclosure Filings by Census Tract Density



Notes: 2SLS estimates (as described in Tables 9 and 6) performed separately for each value of the given quartile. Population density is calculated as census-tract population (as of 2000) over census-tract area. Diversity is measured with a Herfindahl-Hirschman index over the shares of each 2000 Decennial Census-designated race in the tract.

Table A.1: Controlling for Length of Case

Years Since Decision		0	1	2	3	4	5
Log Length of Case	Any Filing	0.049	-0.006	0.107**	0.125	0.357**	0.167
		(0.042)	(0.040)	(0.043)	(0.086)	(0.177)	(0.163)
	First-stage F	149.500	133.900	114.600	24.810	10.740	8.804
	N	129834	118201	92804	67076	41733	23666
	Total Filings	0.728	0.782	0.669**	0.928*	2.308	0.627
		(0.553)	(0.562)	(0.299)	(0.511)	(1.522)	(1.196)
	First-stage F	149.500	133.900	114.600	24.810	10.740	8.804
	N	129834	118201	92804	67076	41733	23666
	log(Price)	-0.102	-0.516	-0.390			
		(0.176)	(0.341)	(0.442)			
	First-stage F	19.320	7.391	12.570			
	N	42880	25890	12155			
Quadratic in Length of Case	Any Filing	0.058	0.008	0.105***	0.124	0.401**	0.245
		(0.036)	(0.036)	(0.037)	(0.076)	(0.196)	(0.264)
	First-stage F	175.100	163.900	163.200	27.100	14.840	4.805
	N	130199	118566	93143	67379	41958	23831
	Total Filings	0.783	0.791	0.677***	0.890**	2.497	0.883
		(0.496)	(0.501)	(0.255)	(0.451)	(1.599)	(1.895)
	First-stage F	175.100	163.900	163.200	27.100	14.840	4.805
	N	130199	118566	93143	67379	41958	23831
	log(Price)	-0.113	-0.638	-0.511			
		(0.158)	(0.427)	(0.685)			
	First-stage F	21.270	6.922	7.338			
	N	43079	26047	12241			
Quarterly Length Dummies	Any Filing	0.053	0.001	0.106**	0.141	0.463**	0.214
		(0.042)	(0.040)	(0.041)	(0.096)	(0.226)	(0.225)
	First-stage F	127	123.400	104.800	24.880	14.800	8.341
	N	130199	118566	93143	67379	41958	23831
	log(# of Filings)	0.808	0.842	0.672**	0.990*	2.819	0.803
		(0.567)	(0.556)	(0.288)	(0.576)	(1.956)	(1.660)
	First-stage F	127	123.400	104.800	24.880	14.800	8.341
	N	130199	118566	93143	67379	41958	23831
	log(Price)	-0.105	-0.619	-0.462			
		(0.197)	(0.404)	(0.562)			
	First-stage F	19.450	9.194	12.400			
	N	43079	26047	12241			

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1-miles of the property in the given year) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, case-level controls as outlined in Table 3, and the indicated controls for length of the case—log of the total months, a quadratic in total months, or number-of-quarter fixed effects. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes are measured as discussed in the text and in the notes to Tables 9 and 6.

Table A.2: Contagion Estimates Measured Since Case Filing

Years Since Filing		0	1	2	3	4	5
Any Filing	2SLS	0.057***	0.032	0.026	0.025	0.076	0.024
		(0.021)	(0.021)	(0.022)	(0.026)	(0.076)	(0.087)
	1st-stage F	388.100	388.100	399	255.500	30.450	22.310
	N	140683	140683	121171	98032	59967	36685
Total Filings	2SLS	0.631*	0.867**	0.334	0.276*	0.023	0.456
		(0.354)	(0.343)	(0.260)	(0.152)	(0.440)	(0.728)
	1st-stage F	388.100	388.100	399	255.500	30.450	22.310
	N	140683	140683	121171	98032	59967	36685

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1-miles of the property in the given year since the case is filed) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes are measured as discussed in the text and in the notes to Table 6.

Table A.3: Robustness of Contagion Estimates By Specification

Years Since Decision		0	1	2	3	4	5
No Covariates	Any Filing	0.066**	0.029	0.096***	0.113**	0.253**	0.160
		(0.028)	(0.027)	(0.027)	(0.052)	(0.107)	(0.130)
	First-stage F	242.600	225	212.400	25.470	20.900	14.130
	N	130199	118566	93143	67379	41958	23831
	Total Filings	0.739*	0.741**	0.603***	0.796**	1.616*	0.621
		(0.388)	(0.375)	(0.196)	(0.320)	(0.954)	(0.972)
	First-stage F	242.600	225	212.400	25.470	20.900	14.130
	N	130199	118566	93143	67379	41958	23831
No Property-Type FEs	Any Filing	0.071**	0.029	0.095***	0.096*	0.244**	0.150
		(0.028)	(0.027)	(0.027)	(0.053)	(0.112)	(0.133)
	First-stage F	232.700	220.200	201	24.080	18.910	13.190
	N	130199	118566	93143	67379	41958	23831
	Total Filings	1.190***	1.065***	0.876***	0.868**	1.488	0.805
		(0.453)	(0.398)	(0.197)	(0.338)	(0.939)	(0.941)
	First-stage F	232.700	220.200	201	24.080	18.910	13.190
	N	130199	118566	93143	67379	41958	23831
Drop Summer Decisions	Any Filing	0.068**	0.018	0.104***	0.089	0.171	0.085
		(0.034)	(0.032)	(0.034)	(0.058)	(0.117)	(0.137)
	First-stage F	179.200	168.800	162.300	25.500	19.180	14.600
	N	105638	95487	75004	55342	34719	19464
	Total Filings	0.914**	0.851**	0.633***	0.563	1.126	0.100
		(0.430)	(0.406)	(0.230)	(0.354)	(0.932)	(1.078)
	First-stage F	179.200	168.800	162.300	25.500	19.180	14.600
	N	105638	95487	75004	55342	34719	19464
Monotonicity-Robust IV	Any Filing	0.083	0.118**	0.026	-0.088	-0.130	-0.417
		(0.062)	(0.057)	(0.062)	(0.109)	(0.296)	(0.662)
	First-stage F	45.180	43.800	37.200	14.260	4.733	1.432
	N	127521	115977	90795	65515	40534	22821
	Total Filings	3.471***	2.067***	1.041	-0.263	-1.259	-7.345
		(1.113)	(0.675)	(0.674)	(1.149)	(3.539)	(9.501)
	First-stage F	45.180	43.800	37.200	14.260	4.733	1.432
	N	127521	115977	90795	65515	40534	22821

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates as described in the notes to Table 6, with the following adjustments. No covariates omits the case-level controls as outlined in Table 3; "No Property-Type FEs" omits the property-type (condo/single-family/multi-family/apartment) fixed effects; "Drop Summer Decisions" drops cases that are decided in June, July, or August (during which the courts dismiss inactive cases); "Monotonicity-Robust IV" indicates that the instrument is constructed by calendar/filing month/income quartile/large plaintiff/"white" census tract cells.

Table A.4: Robustness of Estimates by Sample

Years Since Decision		0	1	2	3	4	5
Include Moratorium	Any Filing	0.056**	0.009	0.083***	0.093*	0.280**	0.160
		(0.028)	(0.028)	(0.027)	(0.054)	(0.115)	(0.137)
	First-stage F	188.700	179.600	202.400	24.060	18.630	12.440
	N	140912	127802	94495	67749	42249	24048
	Total Filings	0.649	0.649*	0.553***	0.700**	1.757*	0.693
		(0.399)	(0.375)	(0.184)	(0.318)	(1.019)	(1.013)
	First-stage F	188.700	179.600	202.400	24.060	18.630	12.440
	N	140912	127802	94495	67749	42249	24048
	log(Price)	-0.122	-0.418**	-0.378			
		(0.106)	(0.210)	(0.409)			
	First-stage F	21.490	18.220	14.660			
	N	43343	26236	12343			
Full Sample	Any Filing	0.078***	0.064***	0.071***	0.058***	0.035	0.017
		(0.022)	(0.019)	(0.018)	(0.021)	(0.025)	(0.022)
	First-stage F	202.200	201.500	225.100	144.800	125	127.600
	N	152559	139324	105628	77785	50704	31508
	Total Filings	0.402**	0.363**	0.410***	0.376**	0.360**	0.241
		(0.204)	(0.177)	(0.124)	(0.163)	(0.157)	(0.204)
	First-stage F	202.200	201.500	225.100	144.800	125	127.600
	N	152559	139324	105628	77785	50704	31508
	log(Price)	-0.159***	-0.199***	-0.222***			
		(0.038)	(0.036)	(0.036)			
	First-stage F	131.300	121.900	132.700			
	N	50879	32750	17952			
Filings < 99th Percentile	Any Filing	0.048*	0.008	0.081***	0.089	0.251**	0.140
		(0.028)	(0.027)	(0.028)	(0.055)	(0.115)	(0.135)
	First-stage F	247.600	229.100	207.200	24.470	18.120	12.920
	N	127046	116434	91674	66454	41416	23533
	Total Filings	0.144	0.234	0.395***	0.318	0.887	0.231
		(0.194)	(0.174)	(0.128)	(0.225)	(0.561)	(0.568)
	First-stage F	247.600	229.100	207.200	24.470	18.120	12.920
	N	127046	116434	91674	66454	41416	23533
Constant Sample	Any Filing	0.094**	0.059	0.168***	0.090*		
		(0.048)	(0.046)	(0.061)	(0.053)		
	First-stage F	24.620	24.620	24.620	24.620		
	N	67379	67379	67379	67379		
	Total Filings	1.243**	1.567***	1.056***	0.657**		
		(0.552)	(0.395)	(0.391)	(0.319)		
	First-stage F	24.620	24.620	24.620	24.620		
	N	67379	67379	67379	67379		

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates as described in the notes to Tables 9 and 6, with the following sample adjustments (recall that the baseline sample omits cases filed during the foreclosure moratorium of 2009, omits filings against VA loans, and omits cases filed during the first year of a case-calendar's existence). "Include Moratorium" maintains the baseline sample, but includes cases filed during the 2009 foreclosure moratorium. "Full Sample" relies on the set of all matched foreclosure cases in Cook County for 2004–2010. "Filings < 99th Percentile" drops observations where the number of new foreclosure filings near a case in a given year is greater than the 99th percentile. "Constant Sample" restricts the sample to cases that are observed for at least three years after the decision.

Table A.5: Effect of Completed Foreclosure on Any New Filing, Omitting Each Filing Year

Years Since Decision	0	1	2	3	4	5
No 2004	0.044	0.014	0.078***	0.076	0.233*	0.167
	(0.028)	(0.027)	(0.027)	(0.054)	(0.130)	(0.174)
First-stage F	221.700	208.300	190.800	21.330	15.180	10.010
N	124342	112717	87309	61592	36265	18391
No 2005	0.046*	0.009	0.072**	0.109*	0.349**	-0.735
	(0.028)	(0.027)	(0.028)	(0.063)	(0.171)	(1.183)
First-stage F	258.600	248	247.400	22.270	8.089	0.937
N	116682	105064	79676	54020	28854	11860
No 2006	0.053*	0.006	0.078***	0.060	0.189*	0.233*
	(0.027)	(0.026)	(0.027)	(0.047)	(0.103)	(0.132)
First-stage F	267.700	255	249	28.580	26.780	13.700
N	112963	101380	76056	50592	26403	17411
No 2007	0.061**	0.015	0.073***	0.072	0.316*	0.140
	(0.029)	(0.028)	(0.028)	(0.052)	(0.185)	(0.133)
First-stage F	176.900	167.300	172.700	21.770	8.266	12.970
N	107066	95565	70614	46462	34353	23831
No 2008	0.079	0.003	0.224***	0.242*	0.247**	0.140
	(0.073)	(0.072)	(0.081)	(0.126)	(0.112)	(0.133)
First-stage F	62.950	55.030	34.530	16.530	19.060	12.970
N	93508	83050	62869	56850	41957	23831
No 2009	0.055*	0.018	0.078***	0.090*	0.247**	0.140
	(0.028)	(0.028)	(0.029)	(0.053)	(0.112)	(0.133)
First-stage F	232.200	218	173.100	24.620	19.060	12.970
N	110228	101633	89191	67379	41958	23831
No 2010	0.043	0.012	0.082***	0.090*	0.247**	0.140
	(0.028)	(0.027)	(0.027)	(0.053)	(0.112)	(0.133)
First-stage F	245.500	224.400	205	24.620	19.060	12.970
N	116405	111987	93143	67379	41958	23831

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates as described in the notes to Table 6, although omitting cases filed in the given year.

Table A.6: Effect of Completed Foreclosure on New Filings, Omitting Each Filing Year

Years Since Decision	0	1	2	3	4	5
No 2004	0.658	0.642*	0.462**	0.531*	1.464	-0.253
	(0.403)	(0.376)	(0.182)	(0.318)	(1.181)	(0.253)
First-stage F	221.700	208.300	190.800	21.330	15.180	20.300
N	124342	112717	87309	61592	36265	13557
No 2005	0.667	0.653*	0.531***	0.840**	1.609	0.571
	(0.407)	(0.386)	(0.189)	(0.341)	(1.157)	(1.206)
First-stage F	258.600	248	247.400	22.270	8.089	1.423
N	116682	105064	79676	54020	28854	8716
No 2006	0.695*	0.650*	0.501***	0.597**	1.524	-0.003
	(0.396)	(0.373)	(0.175)	(0.286)	(0.946)	(0.231)
First-stage F	267.700	255	249	28.580	26.780	24.660
N	112963	101380	76056	50592	26403	12983
No 2007	0.659	0.686*	0.528***	0.529*	1.882	-0.062
	(0.427)	(0.405)	(0.187)	(0.314)	(1.594)	(0.217)
First-stage F	176.900	167.300	172.700	21.770	8.266	24.500
N	107066	95565	70614	46462	34353	17628
No 2008	1.118*	0.635	1.192**	1.178	1.551	-0.062
	(0.654)	(0.517)	(0.573)	(1.012)	(0.983)	(0.217)
First-stage F	62.950	55.030	34.530	16.530	19.060	24.500
N	93508	83050	62869	56850	41957	17628
No 2009	0.749*	0.768**	0.524***	0.657**	1.551	-0.062
	(0.394)	(0.378)	(0.199)	(0.319)	(0.983)	(0.217)
First-stage F	232.200	218	173.100	24.620	19.060	24.500
N	110228	101633	89191	67379	41958	17628
No 2010	0.627*	0.667*	0.536***	0.657**	1.551	-0.062
	(0.375)	(0.367)	(0.183)	(0.319)	(0.983)	(0.217)
First-stage F	245.500	224.400	205	24.620	19.060	24.500
N	116405	111987	93143	67379	41958	17628

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates as described in the notes to Table 6, although omitting cases filed in the given year.

Table A.7: Baseline Estimates for 0.25 Mile Radius

Years Since Decision		0	1	2	3	4	5
Any Filing per Year	2SLS	0.017	-0.003	0.016	0.026	0.032	-0.064
		(0.012)	(0.011)	(0.011)	(0.016)	(0.041)	(0.052)
	1st-stage F	238.300	224.200	205	24.620	19.060	12.970
	N	130199	118566	93143	67379	41958	23831
Total Filings per Year	2SLS	2.187***	2.358***	1.693***	1.219	2.665	0.762
		(0.777)	(0.785)	(0.520)	(0.764)	(1.792)	(2.217)
	1st-stage F	238.300	224.200	205	24.620	19.060	12.970
	N	130199	118566	93143	67379	41958	23831
log(price)	2SLS	-0.161**	-0.269*	-0.410*			
		(0.067)	(0.144)	(0.220)			
	1st-stage F	24.760	17.870	16.860			
	N	63639	39508	21860			

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates as described in the notes to Tables 6 and **??**, although defining outcomes using a 0.25 mile radius around delinquent property.

Table A.8: Response Among Loans with Multiple Claimants

Years Since Decision	0	1	2	3	4	5
Any Filing	0.076*	0.007	0.087**	0.071	0.112	0.058
	(0.039)	(0.038)	(0.039)	(0.075)	(0.143)	(0.164)
First-stage F	210.100	213.600	175.800	23.360	16.810	12.410
N	65366	59917	48271	34865	21252	11377
95% CI Lower Bound	0.154	0.081	0.163	0.217	0.392	0.379
Total Filings per Year	0.820	0.669	0.454	0.470	2.026	0.707
	(0.575)	(0.511)	(0.286)	(0.495)	(1.634)	(1.267)
First-stage F	210.100	213.600	175.800	23.360	16.810	12.410
N	65366	59917	48271	34865	21252	11377
95% CI Upper Bound	1.947	1.671	1.015	1.440	5.229	3.190
log(price)	0.035	-0.172	-0.198			
	(0.147)	(0.231)	(0.346)			
First-stage F	19.580	24.230	18.530			
N	22348	13423	6071			
95% CI Upper Bound	-0.254	-0.625	-0.876			

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Sample is restricted to cases where multiple lien holders are listed in the foreclosure filing. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year since decision) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes are measured as discussed in the text and in the notes to Tables 9 and 6.

Table A.9: Robustness of Price Estimates By Specification

Years Since Decision		0	1	2
No Covariates	log(Price)	-0.153	-0.439**	-0.374
		(0.113)	(0.214)	(0.394)
	First-stage F	23.290	20.600	15.210
	N	43079	26047	12241
No Property-Type FEs	log(Price)	-0.113	-0.416**	-0.361
		(0.106)	(0.204)	(0.397)
	First-stage F	22.140	19.140	14.580
	N	43079	26047	12241
Drop Summer Decisions	log(Price)	-0.127	-0.482**	-0.549
		(0.122)	(0.240)	(0.361)
	First-stage F	22.860	18.330	9.793
	N	31977	19176	8837
Monotonicity-Robust IV	log(Price)	-0.200	-0.929*	-0.337
		(0.256)	(0.502)	(0.678)
	First-stage F	12.080	5.523	3.730
	N	41593	24902	11517

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates as described in the notes to Table 9, with the following adjustments. No covariates omits the case-level controls as outlined in Table 3; "No Property-Type FEs" omits the property-type (condo/single-family/multi-family/apartment) fixed effects; "Drop Summer Decisions" drops cases that are decided in June, July, or August (during which the courts dismiss inactive cases); "Monotonicity-Robust IV" indicates that the instrument is constructed by calendar/filing month/income quartile/large plaintiff/"white" census tract cells.

Table A.10: Sub-Sample Price Effects

Years Since Decision		0	1	2
log(price)	Repeat Sample, Unadjusted	-0.073	-0.395	-0.322
		(0.115)	(0.247)	(0.440)
	1st-stage F	22.070	11.470	13.980
	N	30482	17916	7904
log(price)	Non-Repeat Sample	-0.235	-0.291	-0.705
		(0.146)	(0.274)	(0.518)
	1st-stage F	24.040	16.870	9.938
	N	30522	17925	8143

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates as described in the notes to Table 9, although using the given sample.

Table A.11: Effect of Completed Foreclosure on Log of Mean Sale Price, Each Filing Year

Years Since Decision	0	1	2
No 2004	-0.068	-0.418*	-0.207
	(0.111)	(0.252)	(0.464)
First-stage F	17.390	12.490	12.150
N	38223	21575	8701
No 2005	-0.140	-0.282	-2.818
	(0.127)	(0.405)	(2.370)
First-stage F	15.530	5.944	1.824
N	32114	16558	6011
No 2006	-0.143	-0.393**	-0.156
	(0.101)	(0.182)	(0.341)
First-stage F	28.770	29.790	13.780
N	30601	17345	9770
No 2007	-0.113	-0.531*	-0.358
	(0.112)	(0.292)	(0.395)
First-stage F	17.230	9.746	14.390
N	32369	22664	12241
No 2008	-0.206	-0.411**	-0.358
	(0.206)	(0.203)	(0.395)
First-stage F	13.530	18.830	14.390
N	39009	26046	12241

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates as described in the notes to Table 9, although omitting cases filed in the given year.

Table A.12: Response Among Loans with Lenders Known for Automating Foreclosure Proceedings

Years Since Decision	0	1	2	3	4	5
Any Filing per Year	0.024	0.038	0.068	0.091	0.059	0.009
	(0.030)	(0.030)	(0.051)	(0.111)	(0.153)	(0.680)
First-stage F	269.300	161.600	25.060	19.060	12.970	2.765
N	94735	83564	67028	41958	23831	9579
Total Filings per Year	0.121	0.115	0.139	0.033	0.226	0.357
	(0.103)	(0.091)	(0.086)	(0.224)	(0.283)	(1.240)
First-stage F	269.300	161.600	25.060	19.060	12.970	2.765
N	94735	83564	67028	41958	23831	9579
Log of Filings per Year	0.094	0.089	0.064	0.034	0.174	0.024
	(0.068)	(0.068)	(0.084)	(0.281)	(0.225)	(0.396)
First-stage F	312.200	139.700	18.160	8.790	10.440	5.390
N	27570	23426	17780	11119	6159	2551
Log of Cumulative Filings since Decision	0.083	0.155**	0.258***	0.252	0.680**	1.911
	(0.079)	(0.070)	(0.083)	(0.228)	(0.319)	(1.454)
First-stage F	221.700	167.300	22.240	20.410	15.760	3.238
N	27929	36395	34539	23998	14714	6317

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Sample is restricted to cases where multiple lien holders are listed in the foreclosure filing. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property each year since decision; one observation per year) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes for "Automating Lenders" are measured based on new foreclosure filings by lenders implicated in the Office of the Comptroller of the Currency and the Board of Governors of the Federal Reserve System Independent Foreclosure Review Settlement, while "Baseline" includes new foreclosure filings among all lenders. Both samples are restricted to cases decided between 2007 and 2010 (during which automation of foreclosure filings is thought to be most common).

Table A.13: Filings from Borrowers in Positive vs. Negative Equity

Years Since Decision	0	1	2	3	4	5
Any Filing From Non-Underwater Borrower	0.043	0.013	0.074**	0.089*	0.217*	0.168
	(0.028)	(0.029)	(0.029)	(0.052)	(0.111)	(0.155)
Any Filing From Underwater Borrower	0.022	0.038	0.026	0.071	0.085	0.094
	(0.023)	(0.026)	(0.025)	(0.051)	(0.100)	(0.141)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.495	0.498	0.180	0.697	0.292	0.681
N	260398	237132	186286	134758	83916	47662
Total Filings From Non-Underwater Borrowers	0.846**	0.754**	0.620***	0.591**	1.410*	0.637
	(0.368)	(0.339)	(0.170)	(0.285)	(0.837)	(0.790)
Total Filings From Underwater Borrowers	-0.155**	-0.085	-0.083	0.066	0.141	-0.098
	(0.077)	(0.074)	(0.056)	(0.092)	(0.232)	(0.290)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.005***	0.008***	0.000***	0.053*	0.088*	0.141
N	260398	237132	186286	134758	83916	47662
Log of Filings From Non-Underwater Borrowers	0.060	0.142**	0.126**	0.160*	0.270	-0.112
	(0.059)	(0.058)	(0.053)	(0.086)	(0.273)	(0.229)
Log of Filings From Underwater Borrowers	-0.033	0.015	-0.006	0.076	0.156	-0.256
	(0.095)	(0.061)	(0.047)	(0.101)	(0.235)	(0.184)
First-stage F	49.820	104.100	88.260	14.960	7.718	11.420
p val for difference between groups	0.239	0.029**	0.035**	0.433	0.719	0.510
N	112137	105777	84497	62019	39722	22737

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year since decision) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes split the counts of new foreclosure filings by either borrowers in negative equity or borrowers in positive equity. P-value tests the significance between the two responses (positive vs. negative equity)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).

Table A.14: Interaction with Positive Price Growth

Years Since Decision	0	1	2	3	4	5
Any Filing	0.046	0.017	0.068**	0.076	0.227**	0.121
	(0.028)	(0.028)	(0.027)	(0.051)	(0.112)	(0.134)
Interaction of Foreclosure with Positive Price Growth	-0.042	-0.052	0.528*	0.094	-0.125	-0.087
	(0.065)	(0.045)	(0.308)	(0.080)	(0.141)	(0.109)
First-stage F	75.470	98.680	3.115	12.140	9.705	6.768
N	127980	116456	91358	65987	41063	23280
Total Filings	0.697	0.605*	0.255	0.609*	1.566*	0.385
	(0.549)	(0.364)	(0.232)	(0.324)	(0.946)	(0.999)
Interaction of Foreclosure with Positive Price Growth	-0.342	1.648	17.110*	2.593	-7.903*	-1.292
	(2.215)	(1.268)	(10.130)	(2.196)	(4.072)	(4.832)
First-stage F	75.470	98.680	3.115	12.140	9.705	6.768
N	127980	116456	91358	65987	41063	23280
Log of Filings in Zip Codes/Years with Negative Price Growth	-0.003	0.135**	0.069	0.176*	0.202	-0.093
	(0.074)	(0.061)	(0.054)	(0.091)	(0.225)	(0.217)
Interaction of Foreclosure with Positive Price Growth	0.156	0.084	1.805	0.328	-0.879	-0.021
	(0.281)	(0.150)	(1.182)	(0.238)	(0.606)	(0.386)
First-stage F	51.130	96.730	2.502	14.250	8.214	13.970
N	93583	85496	67258	48491	30449	17273

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year since decision) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3, but allowing for the interaction of the foreclosure treatment with an indicator for positive price growth in that zip code in that year (from Zillow price indices). Standard errors and first-stage F statistics are as outlined in the notes to Table 3.

Table A.15: Response Among Borrowers with Conventional Mortgages

Years Since Decision	0	1	2	3	4	5
Any Filing Among Borrowers with Non-Conv. Mortgages	0.022	0.050*	0.034	0.087	0.302**	0.060
	(0.028)	(0.030)	(0.030)	(0.054)	(0.133)	(0.158)
Any Filing Among Borrowers with Conventional Mortgages	0.019	0.023	0.094***	0.074	0.131	0.189
	(0.033)	(0.032)	(0.031)	(0.048)	(0.117)	(0.187)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.952	0.528	0.088*	0.821	0.209	0.529
N	260398	237132	186286	134758	83916	47662
Total Filings Among Borrowers with Non-Conv. Mortgages	0.226	0.391**	0.145	0.365*	0.894	0.223
	(0.212)	(0.181)	(0.093)	(0.193)	(0.598)	(0.491)
Total Filings Among Borrowers with Conventional Mortgages	0.465**	0.278	0.392***	0.292*	0.656	0.315
	(0.210)	(0.222)	(0.120)	(0.173)	(0.434)	(0.586)
First-stage F	119.200	112.100	102.500	12.310	9.534	6.486
p val for difference between groups	0.099*	0.503	0.017**	0.623	0.448	0.831
N	260398	237132	186286	134758	83916	47662
log(# of Filings) Among Borrowers with Non-Conv. Mortgages	-0.032	0.119**	0.075	0.119	0.242	-0.084
	(0.054)	(0.049)	(0.051)	(0.081)	(0.251)	(0.210)
log(# of Filings) Among Borrowers with Conventional Mortgages	0.125**	0.065	0.089*	0.134	0.231	-0.060
	(0.058)	(0.066)	(0.052)	(0.083)	(0.201)	(0.187)
First-stage F	117.800	96.760	72.820	14.810	8.370	13.850
p val for difference between groups	0.001***	0.357	0.791	0.838	0.959	0.885
N	136995	126760	100375	73225	45688	25633

Notes: ***Indicates significance at the 1% level, **5%, and *10%. The count of new foreclosure filings around each property is split in two: filings among borrowers with conventional mortgages and filings among borrowers with unconventional mortgages. The contagion outcomes are redefined for each group (any filing within each group, count of filings within each group, log of filings within each group). The baseline 2SLS estimates are presented for each outcome (as in Table 6), although the—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects). P-value tests the significance between the two responses (conventional vs. non-conventional).

Table A.16: Response by Proxy for Neighborhood Connectedness

Years Since Decision	0	1	2	3	4	5
Main Effect of Completed Foreclosure on Any Filing	0.037	0.031	0.090**	0.105	0.241*	0.185
	(0.040)	(0.038)	(0.038)	(0.066)	(0.141)	(0.185)
Interaction with Single Race > 75% of Tract	0.025	-0.038	-0.020	-0.034	-0.008	-0.095
	(0.054)	(0.052)	(0.053)	(0.093)	(0.212)	(0.260)
First-stage F	108.600	99.770	84.300	9.773	9.330	5.559
N	130199	118566	93143	67379	41958	23831
Main Effect of Completed Foreclosure on Total Filings	0.404	0.834	0.570*	1.346**	1.677	-0.096
	(0.558)	(0.662)	(0.296)	(0.529)	(1.047)	(1.512)
Interaction with Single Race > 75% of Tract	0.534	-0.338	-0.132	-1.385**	-0.394	1.216
	(0.813)	(0.650)	(0.369)	(0.630)	(1.416)	(1.810)
First-stage F	108.600	99.770	84.300	9.773	9.330	5.559
N	130199	118566	93143	67379	41958	23831
Main Effect of Completed Foreclosure on Log of Filings	0.011	0.172*	0.090	0.276**	0.252	-0.277
	(0.094)	(0.088)	(0.082)	(0.121)	(0.273)	(0.345)
Interaction with Single Race > 75% of Tract	0.009	-0.082	-0.009	-0.257	-0.119	0.380
	(0.110)	(0.108)	(0.104)	(0.166)	(0.391)	(0.423)
First-stage F	95.590	96.070	72.730	11.620	7.097	10.310
N	95155	86961	68482	49422	31016	17628
Main Effect of Completed Foreclosure on Any Filing	0.060**	0.014	0.074**	0.074	0.250**	0.155
	(0.029)	(0.028)	(0.029)	(0.057)	(0.116)	(0.137)
Interaction with Property Being a Condo	-0.045	-0.013	0.042	0.092	-0.017	-0.074
	(0.041)	(0.035)	(0.044)	(0.066)	(0.109)	(0.085)
First-stage F	120	112.400	102.400	12.400	9.603	6.414
N	130199	118566	93143	67379	41958	23831
Main Effect of Completed Foreclosure on Total Filings	0.563	0.261	0.079	0.549*	1.529*	0.248
	(0.392)	(0.296)	(0.192)	(0.331)	(0.916)	(1.032)
Interaction with Property Being a Condo	0.704	2.236**	2.446***	0.615	0.117	1.453
	(1.656)	(0.986)	(0.869)	(1.326)	(2.005)	(1.776)
First-stage F	120	112.400	102.400	12.400	9.603	6.414
N	130199	118566	93143	67379	41958	23831
	0.082	0.125**	0.024	0.106	0.112	-0.051
Main Effect of Completed Foreclosure on Log of Filings	(0.075)	(0.063)	(0.057)	(0.094)	(0.227)	(0.231)
	-0.321	0.036	0.352**	0.307	0.501	-0.061
Interaction with Property Being a Condo	(0.196)	(0.146)	(0.161)	(0.219)	(0.456)	(0.240)
	139.100	107.200	97.200	14.660	8.312	12.390
First-stage F	95155	86961	68482	49422	31016	17628
N	95155	86961	68482	49422	31016	17628

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year since decision) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3, but allowing for the interaction of the foreclosure treatment with an indicator for the property being a condo. Standard errors and first-stage F statistics are as outlined in the notes to Table 3.

Table A.17: Response Among Borrowers with the Same Lender

Years Since Decision	0	1	2	3	4	5
Any Filing Among Different Lender	0.062**	0.037	0.073***	0.092*	0.287**	0.070
	(0.029)	(0.027)	(0.028)	(0.054)	(0.119)	(0.163)
Any Filing Among Same Lender	0.001	0.014	0.020	0.013	0.048	0.029
	(0.021)	(0.021)	(0.019)	(0.029)	(0.074)	(0.094)
First-stage F	124.700	112.400	112.700	13.060	8.506	4.524
p val for difference between groups	0.104	0.488	0.126	0.146	0.040	0.794
N	231920	211018	164628	117636	70910	38210
Total Filings Among Different Lenders	0.816**	0.887***	0.683***	0.742**	1.112	-0.081
	(0.362)	(0.343)	(0.182)	(0.347)	(0.700)	(1.108)
Total Filings Among Same Lender	-0.194***	-0.185***	-0.160***	-0.116	0.024	0.063
	(0.061)	(0.062)	(0.050)	(0.083)	(0.190)	(0.254)
First-stage F	124.700	112.400	112.700	13.060	8.506	4.524
p val for difference between groups	0.007	0.002	0.000	0.012	0.018	0.860
N	231920	211018	164628	117636	70910	38210
log(# of Filings) Among Different Lender	0.023	0.127**	0.121**	0.182**	0.145	-0.053
	(0.067)	(0.054)	(0.053)	(0.090)	(0.226)	(0.246)
log(# of Filings) Among Same Lender	0.046	0.001	0.033	-0.154	-0.425	0.952
	(0.087)	(0.080)	(0.072)	(0.103)	(0.394)	(3.326)
First-stage F	55.050	107.600	41.270	15.800	5.857	0.056
p val for difference between groups	0.838	0.162	0.157	0.000	0.121	0.750
N	98694	88191	67543	47654	28798	15282

Notes: ***Indicates significance at the 1% level, **5%, and *10%. 2SLS estimates of the effect of completed foreclosure on the given outcome are from a regression of the outcome (measured within 0.1 miles of the property in the given year since decision) on an indicator for the case ending in foreclosure (instrumented by case calendar/filing month foreclosure rate), filing month, property type, and year of observation fixed effects, and case-level controls as outlined in Table 3. Standard errors and first-stage F statistics are as outlined in the notes to Table 3. Outcomes are measured based on new foreclosure filings by either different lenders than that in the observed case or the same lender. P-value tests the significance between the two responses (same lender vs. other lender)—estimates are performed simultaneously (pooling both outcomes and allowing for differing filing month and year of observation fixed effects).