The Effect of Foreclosures on Homeowners, Tenants, and Landlords

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Abstract

How costly is foreclosure? Estimates of the social cost of foreclosure typically focus on financial costs. Using random judge assignment in Cook County, Illinois, we find evidence of significant non-pecuniary costs of foreclosure, particularly for foreclosed-upon homeowners. Foreclosure causes housing instability, reduced homeownership (30%), moves to worse neighborhoods in terms of income (17% lower) and school quality, and personal trauma such as divorce (7 percentage points higher). We find more limited and smaller negative effects for renters who are evicted due to landlord foreclosure and for landlords, suggesting the combination of eviction and the financial loss of foreclosure is particularly potent. Our estimates imply that foreclosure is far more costly than current estimates imply and that the costs are disproportionately borne by owners who lose their home.

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1 Introduction

How costly is foreclosure? This question has profound ramifications for public policy and household finance as even small changes in the social costs of foreclosure may dramatically change cost-benefit analyses of foreclosure mitigation programs and other policies to support housing markets. A household finance literature on bankruptcy and default also hinges on how costly default is and why. Indeed, most macro and household finance models require large default costs to rationalize low strategic default (Bhutta and Shan (2017), Gerardi, Rosenblatt, Willen, and Yao (2015), Ganong and Noel (2019b)), and understanding and calibrating these costs can improve our understanding of a host of issues related to housing markets and consumer default.

Despite the fact that six million homes were lost to foreclosure from 2007 to 2017 (Piskorski and Seru (2018)), our understanding of the social costs of foreclosure is surprisingly limited. Most estimates of the social costs of foreclosure focus exclusively on financial costs. For example, in the depths of the Great Recession, the U.S. Department of Housing and Urban Development (2010) analyzed a refinancing program for underwater borrowers and concluded that the social cost of a foreclosure is $51,061.\(^1\) Of this, $26,230 were costs borne by the lender including property damage and transaction costs, $14,531 was due to price externalities on neighboring homes,\(^2\) and just $10,300 – or one fifth of the total social cost – was borne by the foreclosed-upon household. Even then, these were purely financial: moving costs, legal fees, and administrative charges.

Although financial costs have been the focus of economists and policy-makers, they are not the costs of foreclosure that are typically in the public imagination or the popular press. Instead, the focus is typically on the plight of homeowners who lose their home and have ruined credit or the housing instability of renters who have been evicted after their landlord defaults. Given this popular conception of foreclosure, we ask whether existing estimates of the social costs of foreclosure are incomplete because they neglect significant non-pecuniary costs.

In particular, we use new and exhaustive data and leverage random foreclosure judge assignment in Cook County, Illinois, to identify the causal effects of foreclosures over the medium-run.

\(^1\)This study is one of the most comprehensive to date and is still cited today. For instance, Ganong and Noel (2019a) use the HUD study to estimate the total welfare benefits of foreclosure policy. This study is similar to a prior estimate by the U.S. Congress Joint Economic Committee (2007) that a foreclosure has a cost of $77,935. Of this, $50,000 were costs borne by lenders, $19,227 is losses to local governments due to foreclosures, and $1,508 is due to reduced neighbor home value. Just $7,200 – under one tenth – was legal and administrative costs to the foreclosed-upon homeowner.

We find evidence of significant non-pecuniary costs for foreclosed-upon homeowners, who are causally more likely to experience housing instability, less likely to own a home (30%), more likely to move to a worse neighborhood in terms of income (17% lower) and school quality, and more likely to experience personal trauma like divorce (7 percentage points higher). Perhaps surprisingly, we find fewer and smaller adverse effects for renters and landlords, although our estimates are less precise.

Our results suggest that current estimates of the social costs of foreclosure are far too low. In terms of mechanisms, our findings about the relative impact of foreclosures on owners, renters, and landlords suggest that the strong non-pecuniary costs of foreclosures are not caused by eviction or financial and credit loss alone but rather the combination of these effects.

Our paper improves on existing measures of the costs of foreclosures both in terms of data and identification. We construct a unique and comprehensive data set covering the universe of foreclosures in Cook County. We combine administrative foreclosure case records; foreclosure, property, crime, bankruptcy, and divorce records; comprehensive individual address histories merged to neighborhood and home characteristics; and soon will add credit reports. The resulting data set provides us with a more complete view of the consequences of foreclosure than prior work.

We also improve on identification relative to an existing literature on the post-foreclosure experiences of households, which uses ordinary least squares in an event study design with controls for observables. For instance, Molloy and Shan (2013) show that foreclosures cause moves, but not to less desirable neighborhoods or more crowded living conditions. Brevoort and Cooper (2013) show that credit scores persistently decline after foreclosure. Piskorski and Seru (2018) show that only a quarter of homeowners who were foreclosed upon from 2007 to 2017 eventually purchased a home, taking an average of four years to do so. All of these papers use credit report data. Finally, Currie and Tekin (2015) show that foreclosure causes an increase in unscheduled and preventable hospital visits using zip code level variation in the timing of foreclosure. While informative, the event study approach is likely to be biased, and the direction of the bias is unclear. On the one hand, OLS may conflate the impact of foreclosure with the impact of simultaneous shocks, including those that trigger a foreclosure (see, e.g., Ganong and Noel (2019b), Bhutta and Shan (2017), Gerardi et al. (2015)), which would lead to an upward bias. By contrast, borrowers with more to lose may be more vigorous in fighting a foreclosure, which may lead to a downward bias. Other unobservables may be important and lead to biases as well. Finally, an event study does not isolate the effect of foreclosure in marginal cases, which are likely to be the most relevant for policy.
To make progress with identification, we use random foreclosure judge assignment. In Illinois, the decision to foreclose lies exclusively with judges who hear a foreclosure case, and judges are able to influence the outcome in borderline cases. Foreclosure cases in the Cook County Chancery Court are assigned to a “calendar” of cases, and each calendar is assigned to a principal judge. Because assignment is at the calendar level, we measure judge leniency at the calendar level using a leave-out mean (see, e.g., Kling (2006), Dobbie and Song (2015), Kolesar (2013), Bhuller, Dahl, Løken, and Mogstad (2016), Dobbie, Goldin, and Yang (2018)).

We use judge leniency as an instrument for foreclosure, inferring the causal effect of foreclosure by comparing owners, renters, and landlords who are assigned a strict calendar of judges relative to those who are assigned a lenient calendar. Due to random assignment, individuals will on average be identical on all other dimensions, so we recover the causal effect of foreclosure. We verify random assignment by showing that judge leniency is not correlated with a battery of placebo outcomes and by showing that there are no pretrends prior to the foreclosure filing. We also show that our instrument does not predict whether an case is in our sample, which shows that dropped observations during data cleaning are not inducing selection in our sample. It is worth noting that our results pick up a local average treatment effect for marginal cases, which are likely the most policy relevant. A comparison of our IV results with ordinary least squares suggests that marginal cases are more likely to have adverse outcomes due to foreclosure, in part because the compliers to our instrument have larger mortgages and thus more to lose.

We first examine the causal effect of foreclosure on homeowners. We find that foreclosure increases the probability of moving by about 30 percentage points. As with almost all of the outcomes we consider for owners, most of the effect occurs in the first few years after the foreclosure case starts, and the effect is highly persistent. But these moves are not innocuous. Homeowners are more likely to experience multiple moves – a major indicator of housing instability – and are 30% less likely to live own their home. They also move to worse neighborhoods in terms of average income (17% lower after 6 years), and middle schools in their neighborhood of residence have worse outcomes on standardized tests. Owners are also more likely to experience personal trauma: over 5 years they are nearly 7 percentage points more likely to get divorced – 8 percentage

3Munroe and Wilse-Samson (2013) also use use random judge assignment in Cook County to study foreclosures. However, their focus is on price externalities of foreclosures on neighboring properties, and they do not examine the effects of foreclosure on homeowners, renters, and landlords.

4We present some preliminary evidence that cases that receive a more lenient judge are more likely to receive a loan modification. We will observe these outcomes with far higher resolution in our in-progress analysis of credit report data, which will allow us to be more precise about the policy counterfactual to which our results correspond.
points for couples likely to be married – although we do not see negative outcomes for bankruptcy or crimes committed. We also find suggestive evidence of negative outcomes for a number of other outcomes with less statistical significance.

The persistent and negative outcomes we find for homeowners are clear evidence that foreclosure has significant costs beyond the financial costs that are traditionally considered. This implies that foreclosure is an inefficient transfer from homeowners to lenders and broadens the case for aggressive foreclosure mitigation policy, although of course the potential costs in terms of a reduced default deterrent also must be considered.

We next turn to renters whose landlord is foreclosed upon. These types of renters are frequently evicted by landlords, to the point that after popular outcry the Federal Government intervened. Nonetheless, renters are frequently evicted after a foreclosure: We find that three years after the foreclosure case begins the probability of moving rises by about 13 percentage points. This is smaller than the we find for owners, which makes sense because eviction is not as automatic for renters as it is for owners. Additionally, the estimates mean revert quickly for renters: Foreclosures trigger moves for renters mainly by pulling them forward in time rather than causing moves that would not have otherwise occurred. These moves do not have nearly as many or as strong negative effects as we observe for homeowners, although we do see a short-lived negative effect on elementary school test scores. Furthermore, renters do not experience the same personal trauma. The fact that renters do not experience the same degree of negative outcomes suggests that eviction on its own cannot explain the negative effects of foreclosure. This echoes modest causal effects of evictions of renters found by Humphries and Dijk (2018) and Collinson and Reed (2018) using a similar judge instrument in Cook County and New York City, respectively.

We finally turn to the landlords. We do not see that foreclosure causes landlords to move from their primary residence, and we do not see as many negative outcomes for the landlords generally, although the standard errors are wide and we see some evidence of worse local middle schools and higher incidence of DUI convictions. We do not see any evidence of elevated divorce; if anything landlords who are foreclosed upon are less likely to get divorced, although this treatment effect is not statistically significant. Our overall results for landlords imply that the financial and credit effects of foreclosure alone cannot explain the negative effects for homeowners. Together, our results for renters and landlords suggest that the homeowner effect is due to the combination of

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5 In 2009, the Protecting Tenants at Foreclosure Act, which gave tenants the right to receive written notice before eviction and serve out the remainder of their lease, was enacted. The PTFA was made permanent in 2018.
eviction and a financial shock that leads to the strong negative outcomes we observe for owners.

The remainder of the paper is structured as follows. Section 2 describes our data set and Section 3 describes our empirical approach. Sections 4, 5, and 6 describe our results for homeowners, renters, and landlords, respectively. Section 7 concludes.

2 Data

We construct a unique data set on the universe of foreclosures in Cook County, Illinois, from 2005 to 2012 with outcomes measured through 2016. Doing so requires linking together several data sets. This section describes how we create our data set and our analysis samples.

2.1 Data Sources

We combine five main types of data to create our data set. First, we use administrative case records for 2005 to 2016 scraped from the Cook County Clerk of the Circuit Court’s web site. These case records provide us with a case number and a dated history of all filings and judgments in each foreclosure case, the name and address of the defendant, the judge for each judgment, the filing date for the case, and the calendar to which the case is assigned. We parse the judgments to determine the ending date and outcome (foreclosure or dismissal) as described in the Appendix. Our main measure of foreclosure is an indicator for whether a case results in a foreclosure within three years of the initial filing, so cases that are still in progress have a definitive outcome. In practice 91.8% of cases result in a dismissal or foreclosure within three years, and our results are robust to using a longer time horizon.

Cases are randomly assigned to a calendar. A calendar is functionally equivalent to a courtroom overseen by a small group of judges that share cases. Each calendar is handled principally by one judge, although sometimes other backup judges are used. When a judge stops handling foreclosure cases, the entire calendar typically is transferred to the main judge that takes over the calendar. Court documents describe random assignment beginning in 2005, which is when we

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6For cases that are refiled (same property and owner at a future date), we use the outcome of the case 3 years after the initial filing including all re-filings. 9.6% of cases are refilings (same property and defendant), and 18.2% of initially denied foreclosure cases are refilled.

7One complication that we have to deal with is the non-random reassignment of cases to new calendars as the court expanded the number of judges and calendars due to the surge in foreclosures. The random assignment procedure and probabilities, the nature of calendar reassignment, and the active dates for each calendar are described by the Presiding Judge in a September 2010 memo available at https://www.dropbox.com/s/qrt671v6x8w7xb/FY10Q3%20Foreclosure%20update.pdf?dl=0. Because we observe
begin our analysis. We end with cases filed in 2012 so that we observe at least 5 years of outcomes. In these 8 years we observe 77 year-calendars. The average calendar has approximately 3,600 cases per year, although in 2008 before the court added calendars a single calendar might hear up to 6,400 cases in a year. There are no calendars with a small number of cases, and multiple calendars are active on every date we observe judgments.

Second, we obtain foreclosure, property, crime, bankruptcy, and divorce records for Cook County from the early 2000s to 2017 from Record Information Services (RIS), a firm that digitizes and cleans public records data in the Chicago area. RIS provides us with data on the universe of foreclosure cases in Cook County, including a crosswalk between case numbers and the assessor’s parcel number (APN) for the property being foreclosed upon. The data also includes the names of all defendants and property and mortgage characteristics. We use the APN to link the Court foreclosure records to RIS data on property characteristics as well as deeds data from DataQuick and CoreLogic that provide us with the transaction and mortgage history of each property, including buyer and seller names. We also obtain individual-level crime, bankruptcy, and divorce data from RIS that we use to measure additional outcomes at the person level.

Third, we obtain individual address histories and demographics from Infutor. Infutor provides us with the entire address history of any individual who resided in Cook County at some point between 1990 and 2016. The data include not only individuals’ Cook County addresses but also any other addresses within the United States at which that individual lived during 1990-2016. The data set provides the exact street address, the month and year in which the individual lived at that particular location, the name of the individual, and demographic information including age and gender. The data picks up moves at high frequency and is very high quality, as detailed by Diamond, McQuade, and Qian (Forthcoming).

Fourth, we are in the process of obtaining credit reports. This will allow us to observe all borrowing, mortgage status and modifications, and some data on spending and car purchases.

Fifth, we geocode each address an individual lives at in the Infutor data to 2010 Census Blocks and link it to neighborhood characteristics obtained from a variety of sources. We obtain some information from the Census, ZIP code income from the IRS Statistics of Income Tax Stats, and only the final calendar and only cases that take longer to resolve are reassigned, using the scraped calendar variable introduces non-random variation. We are, however, able to recover the original, randomly-assigned calendar by identifying the main judge handling cases for each active calendar and determining the original calendar for each case by observing judgments made by the main judge of the originally-assigned calendar. The algorithm we use to reassign calendars is described in the Appendix. Crucially, with the cleaned calendar, we are able to replicate stable random assignment probabilities that correspond to court documents.

8RIS’s data coverage begins in different years for different variables.
school test scores from the Illinois Board of Education.9

2.2 Construction of Analysis Sample

We begin with the Cook County Court data, which is at the case level. We categorize each case as a mortgage foreclosure or non-foreclosure case handled by the Chancery Court using the first judgment recorded and drop non-foreclosure cases.10 We merge this with the RIS data using case numbers. Before doing so, we clean the RIS data and drop all cases with multiple parcels as part of one foreclosure record because RIS may not report all of the parcel APNs. We then merge in the DataQuick and CoreLogic deeds records using the APN.

We merge our case-level data set with individual-level address histories from Infutor. To do so, we search the individual Infutor address histories to find homeowners and tenants who lived at an address when the foreclosure filing begins. We geocode every address in the Infutor address histories and link these to neighborhood-level outcomes using 2010 Census block groups. We use the combined data set to construct outcomes for each case-person measured in years from the initial foreclosure filing from five years prior to the foreclosure start until six years after the foreclosure start. We also merge in individual-level outcomes from RIS regarding crime, bankruptcy and divorce using first and last name as well as zip code.

We next identify homeowners, renters, and landlords. We are purposefully cautious in who we define in each category, and end up dropping many extraneous people who we cannot determine to be an owner, renter, or landlord with high confidence.

We define owners as individuals whose name matches the foreclosure defendant name and who live at the foreclosure address at the foreclosure filing date. We also include any cohabitants who have the same last name.

To find landlords of foreclosed-upon properties who do not live in the property, we use three criteria. First, a landlord is an owner who used to occupy the property subject to foreclosure, but who moved out at least 6 months prior to the foreclosure filing. Second, a landlord is someone whose address matches the mailing address listed in the deeds records from prior transactions of

9We have investigated intergenerational mobility measures including income mobility, teen birth, and share in prison from Opportunity Insights’ Opportunity Atlas (Chetty, Friedman, Hendren, Jones, and Porter (2018)). We have not found statistically significant results, but the data cover children who grew up in the 1990s and may not reflect tract-level conditions in the Great Recession.

10We do not use the Court provided case type because this variable only exists prior to 2007. More information on how we classify cases is provided in the Appendix.
the foreclosure property and whose last name matches the defendant name in the court case.\textsuperscript{11}

Third, for properties still not matched to a landlord, we find everyone who has ever lived in Illinois whose first and last name matches the defendant name. If a case matches to a unique person, we define them as the landlord. If a case is not matched to a unique person, but there is only one person living on the same street or in the same zip code as the foreclosed property, we define them as the landlord. For cases where multiple individuals in Infutor match the first and last name of the landlord, we flag the cases where we believe to have found the landlord in the Infutor data, but can’t be sure exactly who he or she is. These cases can be used in our renter sample since we are quite confident the owner does not live at the property. However, we do not include these duplicated matches in our landlord analysis since we are unsure exactly which person is the landlord.

We define renters as individuals for whom the foreclosure address matches the Infutor address at the foreclosure filing date and the foreclosure address has an identified landlord. We also our sample to renters living in condominiums and apartments, since these people are most likely to actually be renters. Since single family homes are so likely to be occupied by owners or family or friends of owners, we do not include them in our renter sample.

We finally introduce several sample restrictions to construct our final data set. We drop non-residential properties and cases that are not clearly either a foreclosure or a dismissal. We also remove people who have Infutor address histories that are clearly duplicates. Table 1 summarizes how each data cleaning step and sample restriction affects how many cases and case-people are in our sample. About half of the foreclosure cases that are dropped are because they are non-residential, match to multiple properties, have an unclear outcome, or do not match to anyone in Infutor and appear to be vacant. The other half of the cases that are dropped are because we cannot identify anyone in Infutor who we think is an owner, renter, or landlord. In Section 3.3 we confirm that these restrictions do not induce sample selection by showing that our instrument is uncorrelated with inclusion in our analysis sample or classification as an owner, renter, or landlord.

\textsuperscript{11}The mailing address of the owner is listed when the property transacts or when a new mortgage is issued. Sometimes this is a business address, which makes it hard for us to track these landlords.
Table 1: Sample Construction Summary

<table>
<thead>
<tr>
<th>Step</th>
<th>Cases</th>
<th>Case-People</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Foreclosure Cases 2005-2012</td>
<td>275,401</td>
<td></td>
</tr>
<tr>
<td>Residential Cases That Match to Single Property</td>
<td>244,831</td>
<td></td>
</tr>
<tr>
<td>Drop Unclear Foreclosure Case Outcome</td>
<td>239,814</td>
<td></td>
</tr>
<tr>
<td>Keep if Match to Someone in Infutor</td>
<td>226,374</td>
<td>1,048,393</td>
</tr>
<tr>
<td>Drop Cohabitants of Owners With Different Last Names</td>
<td>226,374</td>
<td>714,550</td>
</tr>
<tr>
<td>Keep if Owner, Renter, or Landlord</td>
<td>188,064</td>
<td>445,049</td>
</tr>
<tr>
<td>Drop Single Family and Townhomes from Renters (Final Full Sample)</td>
<td>183,494</td>
<td>388,677</td>
</tr>
<tr>
<td>Owners</td>
<td>124,951</td>
<td>248,494</td>
</tr>
<tr>
<td>Renters</td>
<td>15,850</td>
<td>80,132</td>
</tr>
<tr>
<td>Landlords</td>
<td>54,237</td>
<td>60,051</td>
</tr>
</tbody>
</table>

Notes: The table shows our sample size at each step in the data cleaning process.

2.3 Outcomes For Properties With a Foreclosure

Before moving on, it is worth considering what happens to a property that we identify as a foreclosure using the court records. To do so, Figure 1 shows outcomes 5 years after the foreclosure case filing at the property level in the deeds data for cases that have a foreclosure judgment within 3 years in the court data and cases that do not. For cases that have a foreclosure judgment, over 90 percent have a foreclosure sale within 5 years. Most of the rest have no observed change in the deeds data (a refinancing, arm’s length sale, or short sale). This is likely due to reversed judgments or appeals. For cases that do not have a foreclosure judgment, under 15 percent experience a foreclosure sale within 5 years. These are likely driven by some cases getting refiled and foreclosed on, as well as the borrower agreeing to a deed-in-lieu of foreclosure. A similar fraction receive a loan modification or refinance, are sold arms length, or have a short sale in which the bank lets the owner sell the house for less than their outstanding mortgage balance. Just over half of the cases with no foreclosure judgment have no visible outcome within five years in the deeds data. These include a mix of very different types of borrowers. First, they include households who began making mortgage payments again and cure out of delinquency. Second, they include households who are still not paying but their mortgage but for whom the bank has not refiled for foreclosure. Third, the bank and borrower could have renegotiated a loan modification without it showing up in the deeds data. Overall, the results in Figure 1 indicate that our primary dependent variable, which indicates a foreclosure judgment within three years, is picking up meaningful outcomes.

12A deed-in-lieu is an agreement between the lender and borrower where the borrower gives the house to the lender and the lender forgives all debt. The deeds records do not distinguish between deed-in-lieu and formal foreclosure, and so deed-in-lieu is included in the foreclosure category in Figure 1.
Figure 1: Property Outcomes in Deeds Data By Foreclosure Judgment

Notes: The figure shows property outcomes from the CoreLogic deeds data 5 years the initial foreclosure filing separately for cases in which we infer a foreclosure or not based on the judgments. We categorize outcomes based on whether the property has an observed refinancing or mortgage modification, an arms length sale not categorized as a short sale by CoreLogic, a short sale as categorized by CoreLogic, or none of the above.

3 Empirical Approach

3.1 Regression Framework

We estimate the effect of foreclosure on various outcomes $Y$ using an event study approach. To do so, consider the following individual-level regression:

$$ Y_{i,k,z,m,t,s} = \beta_{s} F_k + \gamma_{s} X_i + \zeta_{m,s} + \phi_{z,t,s} + \epsilon_{i,k,z,m,t,s}, \quad (1) $$

which we estimate for $s = -5, ..., 6$. In this regression $i$ indexes residents or owners of foreclosed-upon properties, $k$ indexes foreclosure cases (which can involve multiple residents), $z$ is the ZIP code of residence at the time of foreclosure filing, $m$ is the date of the initial foreclosure filing, $t$ is the year in which the initial foreclosure filing occurs, and $F_k$ is an indicator for whether the foreclosure case resulted in a foreclosure within 3 years of the initial foreclosure filing. We run a separate regression for each event-year $s$ and plot the regression coefficients $\beta_{s}$ to analyze the treatment effects. The regression controls for individual-level observables $X_i$, a fixed effect for the exact date of the foreclosure start $\zeta_{m,s}$, and a zip code-by-year fixed effect $\phi_{z,t,s}$. Unless otherwise indicated, $X_i$
does not include any additional controls for location-based outcomes and includes the \( Y_{i,k,z,m,t-1} \) for person-based outcomes. We control for the \( t-1 \) outcome to enhance power, and our results are not significantly changed if we omit this control. We weight by the inverse of the number of people per case so that we do not overweight foreclosure cases with more people matched to the property. Because this is an individual level regression but foreclosure is determined at the case level, standard errors are clustered at the case \( k \) level.

This regression performs a cross-sectional comparison between homes that had a foreclosure case filed that are foreclosed upon and not foreclosed upon. We estimate a separate effect in each event year around the year of foreclosure filing. This cross-sectional comparison does not lead to causal estimates. Prior studies such as Molloy and Shan (2013), Brevoort and Cooper (2013), Piskorski and Seru (2018), and Currie and Tekin (2015) have dealt with this by performing a difference-in-differences analysis. However, this requires assuming that foreclosed and non-foreclosed households would have followed parallel trends absent foreclosure.\(^{13}\)

While useful, OLS may be biased for a number of reasons. First, it may conflate the effects of foreclosure with other omitted variables. The bias may go either direction. For instance, if financial shocks trigger foreclosure when a household is underwater as in the “double trigger” model of default (Ganong and Noel (2019b), Bhutta and Shan (2017), Gerardi et al. (2015)), then the OLS approach would ascribe the effects of the financial shock to the foreclosure, leading to an upward bias. By contrast, it may be the case that borrowers with the most to lose may be more likely to vigorously fight the foreclosure, leading to a downward bias. Similarly, lenders may make more effort to foreclose on larger mortgages. Indeed, Figure 2 plots probabilities of various outcomes for a property 5 years after the foreclosure filing for bins of the complaint amount, which is roughly equal to the amount of principal remaining on the defaulted mortgage plus missed payments. One can see that a foreclosure and a short sale are much more likely for higher complaint amounts, indicating that both lenders are more aggressive at pursuing the foreclosure and homeowners are more aggressive at pursuing alternatives to foreclosure when the stakes are higher.

The second source of bias in OLS is that the dismissed foreclosure cases may not be a good control group. For instance, we observe cases in which an above-water homeowner stops making mortgage payments, a foreclosure case is filed, and a sale occurs shortly thereafter, indicating that the homeowner stopped making payments while they were trying to sell. This occurs for about

\(^{13}\)The literature uses the full population rather than limiting to individuals who experience a foreclosure filing. Additionally, Molloy and Shan use a matching estimator rather than a linear control for observables, which is qualitatively similar but less parametric.
Figure 2: Property Outcomes By Complaint Amount

Notes: The figure shows case outcomes from the DataQuick deeds data 5 years the initial foreclosure filing by initial complaint amount. We categorize outcomes based on whether the property has an observed refinancing or mortgage modification, an arms length sale not categorized as a short sale by DataQuick, a short sale as categorized by DataQuick, or none of the above.

6% of cases. These cases are an odd control group for foreclosed-upon properties.

Third, OLS treats all non-foreclosure outcomes the same. Some look a lot like foreclosure, such as a deed in lieu of foreclosure (which CoreLogic codes as a foreclosure), in which the household transfers the property to the lender without a formal foreclosure proceeding in exchange for more favorable treatment. Other outcomes, such as loan modifications, look very different. Others, such as short sales, are somewhere in between. It is unclear what OLS is picking out, and ideally we would like more policy-relevant variation.

One approach to address some of these concerns about OLS is to saturate the regression with fixed effects. To evaluate how well this may work, Table 2 regresses the indicator for a case foreclosing within 3 years on several observables. Panel A shows that all of the observables we consider are highly correlated with a foreclosure judgment in a specification with controls only for date of filing fixed effects. Panel B adds year-by-ZIP code fixed effects to non-parametrically control for observables related to highly-local economic conditions. However, these fixed efforts barely move the coefficients (aside from age, which has a wide standard error), indicating that most of the endogeneity of foreclosure is happening due to unobservables within a date-location.
Table 2: Endogeneity of Foreclosure

<table>
<thead>
<tr>
<th></th>
<th>Panel A: Date FE Only</th>
<th></th>
<th>Panel B: Date and ZIP \times Year FE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent Var</td>
<td>Log Zip Income</td>
<td>Age</td>
<td>Owner Occupied Amount</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Foreclose within 3</td>
<td>-0.0361***</td>
<td>-0.0541</td>
<td>-0.193***</td>
</tr>
<tr>
<td>years</td>
<td>(0.00206)</td>
<td>(0.0652)</td>
<td>(0.00217)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>0.0868***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.00302)</td>
</tr>
</tbody>
</table>

|                      |                      |                      |                                      |
| Foreclose within 3   | 0.103                | -0.170***            | 0.0864***                            |
| years                | (0.0647)             | (0.00213)            | (0.00273)                            |
|                      | 187,439               | 322,136              | 188,067                              |
|                      | 187,988               |                      |                                      |

Notes: The table shows regressions of foreclosure within 3 years on the indicated dependent variable with the indicated fixed effects for our full sample. All standard errors are clustered by case, and we weight by the inverse of the number of people per case so that each foreclosure case is weighted equally. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Of course, one can include these observables as controls, but one is always limited by the available observables. For instance, Molloy and Shan (2013) have extremely rich data and are still only able to match on age, credit score, structure type, and mortgage balance.

Given the issues with OLS, the only way to overcome this identification concern is with an instrument that provides random or quasi-random variation in foreclosure.

3.2 Instrument

We use the leniency of randomly assigned foreclosure judges in Cook County as an instrument for foreclosure.

Illinois is a judicial foreclosure state, meaning that in order to seize a home as collateral, lenders must file a court case, and final authority for the foreclosure rests with an Illinois Circuit Court judge in the Chancery Division.\(^{14}\) While the law has clear guidelines and standards for a foreclosure to occur, there are grey areas that gives judges some discretion in affecting the outcome of foreclosure cases.\(^{15}\) Most notably, judges can push for non-foreclosure resolutions such as a loan modification, a short sale, or a deed in lieu of foreclosure. Judges can dismiss a case based on mistakes in mortgage paperwork or missing paperwork, if the lender cannot prove that they have provided the borrower with legally-required notices of delinquency, the defendant was not

\(^{14}\)Illinois is also a recourse state, meaning that lenders can go after an individuals assets if the property’s value is less than the amount owed on the mortgage. However, Nelson and Walsh (2014) report that “by custom, the judges in Cook County rarely grant [personal deficiency judgments], and instead grant only in rem deficiency judgments.”

\(^{15}\)For details on Illinois Foreclosure Law and ways in which defense attorneys can contest a foreclosure, see Nelson and Walsh (2014), on which our summary below is based.
personally served or an “honest and well-directed” effort was not undertaken to find them.\textsuperscript{16} Cases can also be dismissed if the borrower is found not sane or unable to enter into a contract, if the judge terminates that a reasonable borrower could not understand the loan terms, or if a judge finds fraud, deceptive business practices, or violations of the Truth in Lending Act, provisions that mattered significantly more after the exposure of “robo-signing” practices by lenders. Finally, in 2010 Cook County instituted a free foreclosure mediation program which all defendants have the right to request and which pauses the foreclosure proceedings. All of these subjective decisions make it possible for judges to affect outcomes by being systematically strict or lenient. Indeed, there was enough non-uniformity in existing practices that in 2011 the Illinois Supreme Court convened a Special Supreme Court Committee on Mortgage Foreclosures to mitigate “abuses and uncertainty” in the foreclosure process.\textsuperscript{17}

Given this discretion, our instrument takes advantage of differences in judge leniency to provide variation in foreclosure. Crucially, the assignment of cases to a calendar is random. Given this, we compute leniency at the calendar-year level and use this as our instrument.\textsuperscript{18} Define the leniency $Z$ of case $k$ assigned to calendar $c$ in year $t$ as:

$$Z_{k,c,t} = \frac{\sum_{j \in c,t, j \neq k} F_{j,c,t}}{N_{(-k),c,t}},$$

where again $F_j$ is an indicator for foreclosure within 3 years for case $j$ and $N_{(-k),c,t}$ is the number of cases on calendar $c$ in year $t$ leaving out $k$. $Z$ is thus simply the mean probability of foreclosure for case $k$’s calendar leaving out observation $k$ itself. A leave out mean is needed to ensure that the instrumented variable is not part of the instrument and is typically used in an empirical literature that uses the leniency of randomly-assigned judges for identification of causal effects (e.g., Kling (2006), Dobbie and Song (2015), Kolesar (2013), Bhuller et al. (2016), Dobbie et al. (2018), Humphries and Dijk (2018), Collinson and Reed (2018)).

\textsuperscript{16}Unfortunately the court records do not provide us information about what alternatives to foreclosures a judge recommends. All we see is whether a foreclosure is approved or denied. We determine whether a non-foreclosure resolution occurs based on deeds records, and in the future we will also use credit report data.

\textsuperscript{17}In 2013, the Supreme Court adopted new state-wide rules with clearer guidelines that (1) standardized foreclosure mediation programs, (2) requiring that prior to a foreclosure all plaintiffs must show all the chain of title to the mortgage debt, and (3) requiring plaintiffs file an affidavit attesting that they have complied with all available loss mitigation programs. See http://www.illinoiscourts.gov/media/pressrel/2013/022213.pdf and https://www.isba.org/ibj/2013/04/lawpulse/newsupremecourtrulespromoteforeclos

\textsuperscript{18}Our instrument can be defined at any time frequency. Using a higher frequency has the disadvantage that $Z$ is a noisier measure of leniency. However, a higher frequency may be beneficial if one is concerned that calendars may be more active in parts of the year in which more cases are more likely to foreclose. This would imply that $Z$ is not randomly assigned and provide biased results. We use an annual $Z$ to maximize power, but show in the Appendix that our results are not sensitive to the frequency of the instrument.
Figure 3: IV vs. OLS: Property-Level Outcomes

Notes: This figure shows regression coefficients from estimating equation 1 for OLS and equations 2 and 3 for IV for the $Y$ variables indicated at the bottom of the figure. “Lose Home” combines a foreclosure and short sale.

We use the instrument to estimate the causal effect of foreclosure in a two-stage least squares framework for $s = -5, ..., 6$:

$$ Y_{i,k,z,m,t,s} = \beta_s F_k + \gamma_s X_i + \zeta_{m,s} + \phi_{z,t,s} + \epsilon_{i,k,z,m,t,s} $$  \hspace{1cm} (2)

$$ F_k = \Gamma Z_{k,c,t} + \alpha X_i + \zeta_m + \phi_{z,t} + \epsilon_{i,k,z,m,t}. $$  \hspace{1cm} (3)

As with the OLS specification in equation (1) above, we weight by the inverse of the number of people per case and cluster our standard errors by case because the instrument variation is at the case level.

This approach requires that the instrument be relevant – that is $\Gamma \neq 0$ – and that the instrument be orthogonal to the first stage error term – that is $Z_{k,c,t} \perp \epsilon_{i,k,z,m,t}$ – which is satisfied by random calendar assignment within months. $\beta_s$ is the local average treatment effect of foreclosure at horizon $s$ for the variation induced by the instrument.

To shed light on the variation picked up by our instrument, Figure 3 shows OLS and IV regressions where $Y$ is an indicator for various property-level outcomes in the deeds data. One can see that relative to OLS, IV has a more negative coefficient on loan modifications, indicating that
relative to OLS, the IV control group is more likely to get a loan modification. This seems to be the main alternative to foreclosure that we can observe that is induced by the instrument, which suggests our variation is highly policy-relevant. We hope to obtain a much sharper picture of the alternatives to foreclosure when we add credit report data to our analysis.

Table 3 shows our first stage regression for three different samples: all properties that we can classify as an owner or renter, owners, and renters. We obtain a first-stage coefficient of between 0.64 and 0.95, with an F statistic of 112.10 for owners, 21.27 for renters, and 33.26 for landlords, indicating that we have a powerful instrument. A binned scatter plot of the first stage is shown in Figure 4, which reveals a linear first stage not driven by outliers. The median foreclosure probability is 42%, with the most lenient 5% of calendar-years foreclosing just under 38% of the time and the most strict 5% of calendar-years foreclosing just over 46% of the time. Indeed, this implies that about 8% of cases have their outcome impacted by the random assignment of the judge. These are the cases we study.

3.3 Verifying Random Assignment

It is crucial that we verify random assignment, which we do in three ways. First, the Appendix shows that our cleaned calendar variable replicates published random assignment probabilities
Table 3: First Stage Regression Coefficients and F Statistics

<table>
<thead>
<tr>
<th>Dep Var: Foreclosed Within 3 Years</th>
<th>Sample</th>
<th>All</th>
<th>Owner</th>
<th>Renter</th>
<th>Landlord</th>
</tr>
</thead>
<tbody>
<tr>
<td>Judge Leniency</td>
<td>0.692***</td>
<td>0.697***</td>
<td>0.945***</td>
<td>0.643***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.056)</td>
<td>(0.066)</td>
<td>(0.205)</td>
<td>(0.111)</td>
<td></td>
</tr>
<tr>
<td>F</td>
<td>152.5</td>
<td>112.10</td>
<td>21.27</td>
<td>33.26</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>183,49</td>
<td>124,951</td>
<td>15,850</td>
<td>54,237</td>
<td></td>
</tr>
</tbody>
</table>

Notes: This table shows first stage regression coefficients for all cases (owners, renters, and landlords combined), owners, renters, and landlords. The regression is at the case level. The first stage includes month and zip-year fixed effects as in (2). All standard errors are clustered by case, and we weight by the inverse of the number of people per case so that each foreclosure case is weighted equally. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 4: Placebo Tests: Leniency Instrument Regressed on Observables

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Case-Level</th>
<th>Case-Person-Level</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>In Sample</td>
<td>Owner</td>
</tr>
<tr>
<td>Judge Leniency</td>
<td>-0.007</td>
<td>0.024</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.054)</td>
</tr>
<tr>
<td>N</td>
<td>244,831</td>
<td>183,494</td>
</tr>
</tbody>
</table>

Notes: This table shows placebo regression coefficients for all cases that we match to the Infutor data, including owners, renters, and cases that cannot be matched to owners or renters. The first three columns are case-level regressions with dependent variables for being in sample (matched to an owner or renter), owner, and renter. The next two columns are for individual-level outcomes. All regressions include month and zip-year fixed effects as in (2). All standard errors are clustered by case, and we weight by the inverse of the number of people per case so that each foreclosure case is weighted equally. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

from the Cook County Courts. Second, we show that there are no-pre-trends when we present the results. Third, we present a number of placebo tests. In particular, if assignment is random the leniency instrument \( Z_{k,c,t} \) should be independent of case-level and individual-level observables, such as whether an observation is included in our main analysis sample, whether it is classified as an owner, renter, or landlord, gender, and age.

Table 4 shows the results of these placebo test regressions, which take the form of equation (1) with the indicated observable as the outcome \( Y \). In all cases we find no significant effects and precise standard errors.

The first set of regressions are case-level. The first column shows that judge leniency has no effect on whether a case is included in our analysis sample. The next three columns show that judge leniency has no effect on whether we categorize an individual as an owner, renter, or landlord. The second set of regressions are case-person level regressions with individual-level outcomes. Again, if the cases are randomly assigned, then judge leniency should have no effect
on any individual-level variables. The regressions show that judge leniency has no effect on the age or gender of an owner, renter, or landlord.

In addition to addressing concerns about random assignment, the fact that our instrument cannot predict whether a case is included in our sample and whether we have found an owner, renter, or landlord assuages concerns that our sample restrictions lead to a selected sub-sample of the data. If this were the case, one would see a treatment effect of the instrument on inclusion and whether the person is an owner renter or landlord.

4 Results: Owners

Figures 5, 6, and 7 show our instrumental variables results for owners. Each figure shows point estimates with dots and 95% confidence intervals indicated by bars. The treatment effect is zero in year zero by for these outcomes by construction, as by definition, no owner has moved from their residence in event year 0. Further, since we control for zip-code of residence in year zero, they also can be no treatment effects on zip code-level neighborhood outcomes in event-year zero. For other personal outcomes, we want to allow for a treatment effect of the foreclosure filing, so we control for the $t - 1$ outcome and the coefficient is zero in year $-1$. Importantly, none of our results have significant pre-trends, which would be evidence against random assignment.

One would expect that being foreclosed upon would cause homeowners to move out of the home from which they were evicted. Panel 5a shows that this is in fact the case. Within one year of the filing, the causal effect of foreclosure on the probability of having moved from the foreclosure address is 7.7 percentage points, and it rises to 29.8 percentage points in year two (once most cases have resolved) and stays around 30% through year six. It is worth noting that about half of the owners who have a foreclosure ruling against them in the first 3 years have moved by year 5 (as a raw mean not IV). While one might expect that all owners are evicted after a foreclosure, it turns out that many homes that are repossessed by a lender are still occupied by the previous owner, and our 50 percent number is in line with several other estimates of the fraction of foreclosed-upon homeowners who have moved from their home at the time of foreclosure.19

Our address data allow us to take this one step further and examine housing instability in

19For instance, Molloy and Shan (2013) find that only 55% of those who are foreclosed upon have moved to a different Census Block four years after the foreclosure start (their Table 2) and Piskorski and Seru (2018) report that 60% of those with a foreclosure between 2007 and 2017 have moved to a different ZIP code by 2017 (their Table 1b). In a 2013 report, RealtyTrac reports that nationwide 47 percent of bank-owned homes are still occupied by the previous owner, and that this number is particularly high in Chicago.
Figure 5: Owners: Moving and House Characteristics

(a) Moved from Foreclosure Address  
(b) Cumulative Number of Moves  
(c) Owns Primary Residence  
(d) Log Square Footage of Living Space

Notes: Each panel shows IV results for the indicated outcome variable for all owners in our sample. Each dot indicates the point estimate for $\beta$ estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case.

addition to whether the household leaves their original address. Panel 5b shows the cumulative number of moves since the foreclosure case was filed. We see clear evidence of housing instability: Even though the causal effect of foreclosure on living at the original address is roughly 30 percentage points in years two through six, the cumulative number of moves is 0.41 in year 4, and 0.91 by year six, implying that foreclosure causes the average foreclosed-upon homeowner who leaves their home to move multiple times. In other words, foreclosure causes moves to unstable living situations.

These homeowners are also significantly less likely to own their residence. Panel 5c shows causal effects for whether the foreclosed-upon homeowner owns their primary residence in year $t$. This dips 30 percentage points by year two and remains low, although we lose power starting
Figure 6: Owners: Neighborhood Characteristics

Notes: Each panel shows IV results for the indicated outcome variable for all owners in our sample. Each dot indicates the point estimate for $\beta_s$ estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case. Log average ZIP code income comes from the IRS. For schools, the dependent variable is the average percentile rank of the local school on math and reading (a coefficient of 1 means a change in the average rank of 1 percentage point). The Illinois Board of Education reports these percentages for math and reading separately, and we combine them into a single average index.

in year six. An effect lasting at least five years is in line with what Piskorski and Seru (2018) find for how long it takes foreclosed-upon households to return to homeownership, and it is also consistent with GSE guidelines for how long after a foreclosure a foreclosed-upon household is unable to obtain a mortgage.

Interestingly, we do not find any evidence that foreclosure causes households to downsize to a smaller home in terms of square footage. Indeed, we find an economically-small and statistically insignificant negative causal effect of foreclosure on the square footage of the a foreclosed-upon
household’s home, as shown in Panel 5d. We do, however, find evidence that foreclosed-upon homeowners move to worse neighborhoods, as shown in Figure 6. Most dramatically, Panel 6a shows that foreclosures the log average income in the ZIP code in which the foreclosed-upon homeowner resides to fall by 17.1% over 6 years. This result is significant at the 5% level in years, 1 through 5 and the 10% level in year 6. These are economically large effects that suggest that foreclosure causes owners to move to significantly lower-income neighborhoods. These effects appear to widen over time, indicating that foreclosed owners fall further and further behind in neighborhood quality relative to where they would have been absent foreclosure. This may be driven by the fact that many neighborhoods are essentially 100% owner occupied and that landlords often run credit check on new tenants. These barriers could severely limit the neighborhood options available to foreclosed-upon households.

Similarly, Panels 6b, 6c, and 6d shows the causal effect of foreclosure on the average (across reading and math) percentile rank of the local elementary, middle, and high school, respectively, on Illinois state standardized tests. The results show that households move to neighborhoods with significantly worse schools middle schools, with insignificant negative effects for elementary and high schools. In terms of magnitude, the percentile rank of the local middle school is 5 to 8 percentiles lower in the first four years after a foreclosure. The quality of the local elementary and high schools decline less and the standard errors are wider. These significant effects on middle school test scores hint at potential effect on the children of foreclosed-upon households, but we cannot test long-run outcomes for children directly with our data.

Finally, we find evidence of elevated levels of personal trauma following a foreclosure by linking our results to RIS data on divorce, crime, DUI convictions, and bankruptcy, as shown in Figure 7. Because these events are relatively rare, we look at the effect on the cumulative number of events since the foreclosure filing rather than the probability of an event occurring in a given year. We also measure these outcomes relative to the year before foreclosure filing rather than the year the case is filed because while the case may not be completed, preliminary judgments by a stricter judge may cause some personal trauma immediately rather than at the final judgment, and we would like to detect any immediate effects.

Panel 7a shows that foreclosure causes a significant rise in the probability of divorce by 4.1 percentage points in the year of foreclosure filing and a cumulative 6.8 percentage points over 2 year to five years. This effect is significant at the 1% level in most years and large: The mean default probability over five years in the non-foreclosure group is about 2 percent, so this is a
more than three-fold increase in divorce. Furthermore, this figure is for all owners, regardless of whether they are married. When we condition on cases in which we suspect the owners are married (two people with the same last name), the causal effect rises to 7.8 percentage points. Panel 7b shows that foreclosure causes an increase in the cumulative number of drug, property, and violent crimes committed by the foreclosed-upon homeowner. While this increases by an economically-meaningful 6.3 percentage points over 5 years, the results are statistically insignificant. Panel 7c shows that the cumulative probability of having been convicted of driving under the influence of alcohol, which rises by an insignificant 2.3 percentage points. Finally, Panel 7d shows the proba-
Table 5: Summary Statistics by Treatment Type

<table>
<thead>
<tr>
<th>Group Average</th>
<th>(1) Mean ZIP Log Income at t</th>
<th>(2) Not At Address at t − 3</th>
<th>(3) Months at House at t</th>
<th>(4) Male</th>
</tr>
</thead>
<tbody>
<tr>
<td>Overall</td>
<td>3.76</td>
<td>0.24</td>
<td>84.38</td>
<td>0.44</td>
</tr>
<tr>
<td>Complier (7%)</td>
<td>3.77</td>
<td>0.22</td>
<td>75.92</td>
<td>0.47</td>
</tr>
<tr>
<td>Never Taker (58%)</td>
<td>3.78</td>
<td>0.24</td>
<td>86.19</td>
<td>0.44</td>
</tr>
<tr>
<td>Always Taker (35%)</td>
<td>3.73</td>
<td>0.25</td>
<td>82.65</td>
<td>0.44</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Group Average</th>
<th>(5) Age at t</th>
<th>(6) Outstanding Debt at t</th>
<th>(7) Debt At Purch</th>
<th>(8) Single Family Home at t</th>
</tr>
</thead>
<tbody>
<tr>
<td>Overall</td>
<td>48.15</td>
<td>$198.6k</td>
<td>$199.2k</td>
<td>0.70</td>
</tr>
<tr>
<td>Complier (7%)</td>
<td>45.07</td>
<td>$218.3k</td>
<td>$212.1k</td>
<td>0.67</td>
</tr>
<tr>
<td>Never Taker (35%)</td>
<td>48.57</td>
<td>$195.4k</td>
<td>$198.4k</td>
<td>0.73</td>
</tr>
<tr>
<td>Always Taker (58%)</td>
<td>47.92</td>
<td>$198.8k</td>
<td>$196.7k</td>
<td>0.64</td>
</tr>
</tbody>
</table>

Notes: This table shows summary statistics for our outcomes of interest for our overall sample of foreclosure cases, as well as among the compliers to the treatment of judge leniency, among “always takers” who always foreclose (those who always foreclosed even when assigned to the most lenient calendar), and “never takers”, who never foreclosure even if assigned to the strictest calendar.

ability of declaring bankruptcy. We find that foreclosure causes bankruptcy to rise 6.2 percentage points in the year after the foreclosure filing, which is significant at the 10% level. However, the standard errors widen over time.

Together, our results present a picture of foreclosure causing significant non-pecuniary costs for the foreclosed-upon homeowner. They move to less stable, are less likely to own, move to worse neighborhoods, and experience significant personal trauma. Our point estimates are economically large and imply that foreclosure has far higher social costs than previously thought.

4.1 Comparison of IV With OLS

Before moving on, it is worth comparing our IV estimates with OLS estimates for a few of our main outcomes. There are reasons to think that IV may be larger or smaller than OLS. In particular, if OLS conflates the causal effect of foreclosure with the effects of shocks that trigger foreclosure, OLS will be biased upwards in absolute value. If households with more to lose are more likely to fight a foreclosure, OLS will be biased downward in absolute value. Additionally, OLS picks up an average outcome, whereas IV picks up a local average treatment effect for marginal cases. It may be that the effect of foreclosure is bigger for marginal cases that would not have experienced as bad outcomes without foreclosure, in which case IV would be larger in absolute value.
Figure 8: Owners: IV vs. OLS

(a) Moved From Foreclosure Address
(b) Cumulative Number of Moves
(c) Log ZIP Code Average Income

Notes: Each panel shows IV results for the indicated outcome variable for all owners in our sample in blue and OLS results in red. Each dot indicates the point estimate for \( \beta \) estimated using equations (2) and (3) for IV and (1) for OLS, and the bars indicate 95% confidence intervals. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case.

To investigate the differences in characteristics between the cases influenced by judge leniency (the compliers to the treatment) and those that do or do not foreclosure, regardless of judge assignment, we measure summary statistics for these subgroups in Table 5. Compliers have more to lose: They have 10% more mortgage debt at the time of foreclosure than the average household and took out a 6% larger mortgage at purchase. This is consistent with the graphical evidence presented in Figure 2. They are also about 3 years younger, 3 percentage points more likely to be male, and have lived at property 8.5 months less than the rest of the sample. Interestingly, the never takers and always takers of foreclosure look quite similar in many dimensions, although the never takers are more likely to be living in a single family home.
Figure 8 compares the IV and OLS results for moving from the foreclosure address, the cumulative number of moves, and the log ZIP code average income. We can see that the IV results are generally somewhat larger in magnitude than OLS, especially several years after the foreclosure. For instance, for moving and the number of moves IV gives somewhat larger effects than OLS, suggesting that the marginal cases are more likely to have a real shot at staying in their home and avoiding housing instability. In some cases, IV detects large negative outcomes where OLS does not. For instance, for log ZIP code average income, we see no effect for OLS consistent with the results of Molloy and Shan (2013), while we observe large negative effects for IV. This is suggestive of treatment effect heterogeneity and particularly large negative outcomes for marginal cases, which are likely the most policy-relevant cases. Given these findings, it is worth keeping the nature of our local average treatment effect in mind when interpreting our results.

5 Results: Renters

Figures 9, 10, and 11 repeat the same analysis conducted in Figures 5, 6, and 7 for renters. Because our renter sample is smaller, we have wider standard errors and less statistical precision. While we still find some negative effects for renters, by and large the effects are fall smaller and less significant than for foreclosed-upon homeowners.

Panel 9a shows the causal effect of a landlord being foreclosed upon on the probability a renter leaves the foreclosure address. We can see that over three years this rises to 12.6 percentage points and is significant at the 5% level. This shows that lenders often evict renters in foreclosed-upon properties, although the effect is smaller than for owners likely due to the fact that eviction is not as automatic and tenant protection laws help renters stay in their homes. The effect mean reverts fully by year six, likely because the move triggered by foreclosure tends to pull a future move forward rather than create moves that would not have otherwise occurred. This makes sense given that renters have much higher baseline moving rates than owners.

However, for renters, these moves do not seem to lead to the same degree of housing instability as for owners. Panel 9b treatment effect on the cumulative number of moves is not significantly larger than the indicator for moving in Panel 9a, indicating that eviction following a landlord foreclosure does not trigger multiple moves.

We also do not observe significant negative results for homeownership or for the square footage

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20We do not have power to examine the causal effect before and after the passage of expanded tenant protections.
Notes: Each panel shows IV results for the indicated outcome variable for all renters in our sample. Each dot indicates the point estimate for $\beta$ estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case.

of one’s residence for renters. Panel 9c shows that the probability that a renter owns is economically and statistically insignificant. Square footage falls by about 10 percentage points (Panel 9d), but the standard errors are very large. Given the limited statistical precision, we do not put much stock in these findings.

Figure 10 shows the results for the neighborhood in which the renters live. There is a downward trend in neighborhood income, which falls by about 6%. However, this result is not statistically significant and quantitatively two-and-a-half-times smaller than the significant effect we find for owners. Panels 10b, 10c and 10d show that there is a significant negative effect on elementary school test scores, which fall by five percentiles by year 2 but mean revert by year six. We do not find a negative effect on middle school or high school test scores.
Figure 10: Renters: Neighborhood Characteristics

Notes: Each panel shows IV results for the indicated outcome variable for all renters in our sample. Each dot indicates the point estimate for $\beta$ estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case. Log average ZIP code income comes from the IRS. For schools, the dependent variable is the average percentile rank of the local school on math and reading (a coefficient of 1 means a change in the average rank of 1 percentage point). The Illinois Board of Education reports these percentages for math and reading separately, and we combine them into a single average index.

Finally, Figure 11 shows results for personal outcomes for renters. We do not see the marked and significant increase in divorce we observed for owners. We see an insignificant short-term rise in the cumulative number of crimes committed, and the outcomes for bankruptcy and DUIs are very noisy, and we do not put much stock into them.

Overall, our results for renters whose landlord is foreclosed upon suggests that their eviction has some adverse causal effects but that the non-pecuniary costs are not nearly as high as they are for foreclosed-upon homeowners. The limited causal effects of renter evictions triggered by
Figure 11: Renters: Personal Outcomes

Notes: Each panel shows IV results for the indicated outcome variable for all renters in our sample. Each dot indicates the point estimate for $\beta_s$ estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case. The outcome variables come from public records collected by RIS. Crimes includes drug, property, and violent crimes. Bankruptcies includes both Chapter 7 and Chapter 13.

foreclosure are in line with the somewhat modest effects found by Humphries and Dijk (2018) and Collinson and Reed (2018) in their analysis of the causal effects of renter eviction which also use random judge assignment in eviction cases.

Together with our results for homeowners, our results for renters also provide some evidence as to the mechanism behind the negative effects we estimate for homeowners. In particular, they suggest that eviction alone cannot explain the negative effects we observe for homeowners.\textsuperscript{21}

\textsuperscript{21}Of course, this may be because of treatment effect heterogeneity: Marginal homeowners may be more likely to avoid bad outcomes than the renters leasing from marginal landlords. We plan to explore this in future work.
6 Results: Landlords

Figures 12, 13, and 14 repeat the same analysis conducted in Figures 5, 6, and 7 for landlords. As with our renter sample, we have wider standard errors and less statistical precision due to the smaller size of our sample.

On the whole, our results do not show much evidence of significant negative causal effects of foreclosure for landlords, although there are a few notable exceptions. We do not find any significant evidence that they move, although they do seem more likely to own their primary residence and we see a moderate but insignificant decline in the size of their home over time. We see a short-term increase and long-run decrease in the average income of their neighborhood,
Figure 13: Landlords: Neighborhood Characteristics

(a) Log ZIP Code Average Income
(b) Elementary School Test Score Rank
(c) Middle School Test Score Rank
(d) High School Test Score Rank

Notes: Each panel shows IV results for the indicated outcome variable for all landlords in our sample. Each dot indicates the point estimate for $\beta_\delta$ estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case. Log average ZIP code income comes from the IRS. For schools, the dependent variable is the average percentile rank of the local school on math and reading (a coefficient of 1 means a change in the average rank of 1 percentage point). The Illinois Board of Education reports these percentages for math and reading separately, and we combine them into a single average index.

but again the results are insignificant. Intriguingly, Panel 14a shows a negative and economically meaningful negative causal effect of foreclosure on landlord divorce, especially after year four. However, these results are not significant.

We do, however, see three main negative effects with the limited statistical power we have. Panel 13c shows a delayed decline in middle-school test scores that is significant at the 10% level in years five and six. Panel 14c shows a positive 6 percentage point effect on the cumulative number of DUI convictions that is significant at the 10 percent level in years 1 through 5. Panel
Figure 14: Landlords: Personal Outcomes

(a) Cumulative Number of Divorces
(b) Cumulative Number of Crimes Committed
(c) Cumulative DUI Convictions
(d) Cumulative Number of Bankruptcies

Notes: Each panel shows IV results for the indicated outcome variable for all landlords in our sample. Each dot indicates the point estimate for $\beta_s$ estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case. The outcome variables come from public records collected by RIS. Crimes includes drug, property, and violent crimes. Bankruptcies includes both Chapter 7 and Chapter 13.

14d shows an increase in bankruptcy that gradually ramps up over time, although these results are highly statistically insignificant.

Overall, our results for landlords suggest far more benign effects of foreclosure than we observe for owners, although we cannot rule out some negative effects on school quality, DUIs, and possibly bankruptcy and neighborhood income. This suggests that the financial consequences of foreclosure alone cannot explain our results for owners. When combined with our results for renters, we are drawn to the conclusion that it is the interaction of eviction and the financial shock – and not either on its own – that explains our large negative effects for owners. Of course, it
could be that the landlords and renters are different from owners, something we plan to explore in future work.

7 Conclusion

Using rich new data and random judge assignment to foreclosure cases, we find significant negative causal effects of foreclosure for foreclosed-upon homeowners. Foreclosure causes housing instability, large reductions in homeownership (30%), moves to worse neighborhoods (17% lower income), and significant personal trauma such as divorce (increasing 7 percentage points). We find substantially more muted effects for renters who are evicted after their landlord is foreclosed upon, although there are some negative effects and our standard errors are wide.

Our results suggest that conventional estimates that focus purely on financial costs significantly understate the social cost of foreclosure, at least for the type of marginal cases that make up our local average treatment effect. This changes the cost-benefit analysis for foreclosure mitigation programs and other types of housing market support in a downturn. We leave putting a precise dollar figure on the bundle of negative outcomes we find foreclosure to cause to future work. We also leave an analysis of effect of these negative outcomes on foreclosure deterrence to future works.

Our results also shed light on the mechanisms underlying foreclosure. The fact that we find fewer and more muted effects for renters and landlords suggests that the eviction component of foreclosure and the financial shock component of foreclosure cannot explain the negative outcomes we observe for homeowners alone. Instead, it suggests that the interaction of these two effects is particularly potent. More broadly, our results justify large utility costs of foreclosures in models of household default, which until now had been difficult to justify as anything besides psychic costs. Instead, the costs of foreclosure on households appear to be very real and substantial.

In ongoing work, we are adding credit report data, which will allow us to look at financial outcomes and the ability to borrow in much more detail. It will also help us understand exactly how these households are avoiding foreclosure, as we will be able to see loan balances, monthly payments, and loan modifications.
References


