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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 instrumental variable (IV) and propensity score matching (PSM) approaches in Cook County, Illinois, we find evidence of significant non-pecuniary costs of foreclosure, particularly for foreclosed-upon homeowners. For all homeowners (IV and PSM), foreclosure causes housing instability, reduced homeownership, and financial distress. For marginal homeowners (IV) but not average homeowners (PSM), foreclosure also causes moves to worse neighborhoods and elevated divorce. We show that the difference between IV and PSM is due to treatment effect heterogeneity: marginal homeowners have more to lose than average homeowners. We find similar financial costs for landlords, although the non-financial effects we find for owners are absent. We find few negative effects for renters whose landlord forecloses. The contrast between our results for owners, renters, and landlords implies that the financial costs come from the financial loss while the non-financial costs for owners are due to a combination of eviction and financial loss rather than either individually. Our estimates imply that foreclosure is far more costly than current estimates imply, particularly for marginal cases that are most responsive to foreclosure mitigation policies, and that the costs are disproportionately borne by owners who lose their home.

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Appendices is available at http://www.nber.org/data-appendix/w27358

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 (2019)), and understanding and calibrating these costs can improve our knowledge 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. 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 for the universe of foreclosure filings in Cook County, Illinois, and leverage both an instrumental variables (IV) design based on random foreclosure judge assignment and a propensity-score-matched (PSM) event study design to identify

[1] This study is one of the most comprehensive to date and is still cited today. For instance, Ganong and Noel (2020) 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 is 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.

[2] See Immergluck and Smith (2006), Campbell, Giglio, and Pathak (2011), Harding, Rosenblatt, and Yao (2009), Anenberg and Kung (2014), Gerardi et al. (2015), Gupta (2019) for evidence of highly-localized foreclosure externalities. Mian, Sufi, and Trebbi (2015) and Guren and McQuade (2020) argue that foreclosures have larger market-level impacts.

the causal effects of foreclosures over 5 years. We find evidence of significant non-pecuniary costs for foreclosed-upon homeowners. For all owners, foreclosure causes housing instability, reduced homeownership, and financial distress including increased delinquency on other debts. Marginal homeowners – who are represented by the local average treatment effect of the judge IV – but not average households – who are represented by the average PSM effect – also move to worse neighborhoods in terms of average income and school test scores and are more likely to divorce. We analyze the discrepancy between IV and PSM for these outcomes and show that heterogeneous treatment effects can explain the difference: marginal households have more to lose. We also analyze the effects of foreclosure on landlords and their tenants. We find similar financial distress for landlords as we did for owners, but none of the non-financial costs are present. We find limited negative outcomes for renters.

Our results suggest that current estimates of the social costs of foreclosure for homeowners are far too low. In terms of mechanisms, our findings about the relative impact of foreclosures on owners, renters, and landlords suggest that some of the costs we find related to financial distress come from financial loss as they are present for owners and landlords but not renters. In contrast, the non-financial non-pecuniary costs of foreclosures that we observe only for owners are not caused by eviction or financial 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 foreclosure filings 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 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 by combining IV and PSM approaches. Our IV approach uses random foreclosure judge assignment as an instrument for foreclosure.[3] 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. Anecdotal evidence and an analysis of outcomes show that the main alternative to foreclosure is a loan modification or payment plan. Foreclosure cases in the Cook County Chancery Court are assigned to a "calendar" of cases, and each calendar is assigned

[3]Munroe 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.

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 (2020), Dobbie, Goldin, and Yang (2018)).

Our IV identification strategy infers 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 a case is in our sample, which shows that dropped observations during data cleaning are not inducing selection in our sample.

Our PSM identification strategy builds on Molloy and Shan (2013). A simple OLS event study that compares houses with a foreclosure filing that foreclose with those that do not foreclose suffers from the fact that many of cases that avoid foreclosure may not be a good control group. While some may have a loan modification, others may be sellers who have discontinued mortgage payments before they sell or non-foreclosure outcomes that are similar to foreclosure such as a short sale. PSM addresses this by conditioning on households that have similar *ex-ante* foreclosure probabilities based on observables. In practice, PSM eliminates some pre-trends in OLS, making the event study more convincing.

Both IV and PSM have strengths and weaknesses. IV leverages true random assignment but has far less power, especially for landlords and renters for whom we have more limited samples. IV also measures a local average treatment effect (LATE) for compliers to the instrument. PSM has much more power, but it relies on a selection on observables assumption that is stronger than the assumptions needed to infer a causal effect from IV. IV also allows us to disentangle the foreclosure itself from shocks that occur simultaneous with foreclosure, while PSM may be biased due to the presence of such shocks, such as unemployment shocks that trigger foreclosure (see, e.g., Ganong and Noel (2019), Bhutta and Shan (2017), Gerardi et al. (2015)). Finally, PSM finds an average treatment effect for the population, not a LATE for compliers.

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 – an indicator of housing instability – and are about 20 percentage points less likely to own their home. We also find evidence of financial distress. Owners who are foreclosed upon have more unpaid collections, which refer to defaulted unsecured debt, and they are more likely to have a foreclosure on their credit record, although interestingly their credit scores are not much worse. They also have modestly elevated bankruptcy rates. For these outcomes, IV and PSM are consistent, which reinforces our findings from either approach independently and assuages potential methodological concerns about each design on its own.

For other outcomes, we find large negative effects using IV but not PSM. In particular, IV reveals that homeowners move to worse neighborhoods in terms of average income (16% lower after 5 years) and middle school test scores. Over 5 years, owners are nearly 7 percentage points more likely to get divorced. We show that the difference between IV and PSM for these outcomes is due to IV measuring a LATE for compliers to the judge instrument instead of a population-average treatment effect like PSM. If marginal cases have more to lose – for instance if they live in a better neighborhood and have further to fall on a neighborhood quality ladder than the average case – IV and PSM will differ. We provide evidence for this interpretation by showing that there is heterogeneity in OLS effects that is consistent with our full-sample IV treatment effects. Given our evidence for treatment effect heterogeneity, after accounting for the gap between the IV LATE and the PSM ATE, IV and PSM reinforce one another even for outcomes for which they do not agree for the full population. We thus conclude that the negative treatment effects for neighborhood quality and divorce hold for marginal cases but not average cases.

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 ex-post inefficient transfer from homeowners to lenders. Our findings also broaden the case for aggressive foreclosure mitigation policy, although of course the potential costs in terms of a reduced default deterrent must also be considered.

We next turn to the landlords, for whom we mainly use PSM because of wide confidence intervals for IV. Landlords suffer similar adverse financial effects to homeowners. They experience an increase in unpaid collections, a modest increase in bankruptcy, and are more likely to have a foreclosure flag on their credit record, although again the decline in credit scores is modest. For non-financial outcomes, however, we do not see any of the adverse effects we find for owners. Landlords do not appear to move from their primary residence as a result of the foreclosure, and do not have the negative impacts on homeownership, neighborhood quality, or personal outcomes, although we do see some evidence of higher DUI convictions. We also do not see any evidence of elevated divorce. Given that landlords experience financial loss but not eviction from their primary residence, our results for landlords imply that the financial losses and credit losses from foreclosure can explain the negative financial effects we observe for owners but not the negative non-financial effects.

We finally turn to renters whose landlord is foreclosed upon. Renters are frequently evicted after a foreclosure, to the point that after popular outcry in 2009 the Federal Government intervened to limit renter evictions after landlord foreclosure.[4] We find that renters are more likely to move in marginal cases but not average cases, likely because marginal landlords who avoid foreclosure do not evict tenants. Even in cases where there are moves, these moves do not cause significant adverse outcomes. Indeed, we find almost no negative effects for renters, and the few negative effects we do find are transitory. 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. Our results echo some of the modest causal effects of evictions of low-income renters found by Humphries, Mader, Tannenbaum, and Van Dijk (2019) and Collinson and Reed (2019) using a similar judge instrument in Cook County and New York City, respectively. Together, our results for renters and landlords suggest that the non-financial negative effects we find for homeowners are due to the *combination* of eviction and a financial shock rather than either on its own.

Our findings about neighborhood quality and divorce for owners in particular highlight the importance of considering heterogeneous treatment effects in evaluating foreclosure policy. We are not aware of prior research that shows evidence of significant treatment effect heterogeneity. The prior literature uses ordinary least squares in an event study design with controls for observables. For instance, Molloy and Shan (2013) use PSM to show that foreclosure starts cause moves, but not to less desirable neighborhoods or more crowded living conditions. Brevoort and Cooper (2013) show that credit scores persistently decline after a foreclosure start. 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. These papers use credit report data and compare households whose lender initiates a foreclosure with households who are current on their mortgage due to the difficulty of observing a foreclosure completion (a lender

[4]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.

repossession of a house) in credit report data. By contrast, we use administrative court records and observe lenders repossessing homes through foreclosure. Our analysis thus compares households who have a foreclosure initiated by the lender but avoid losing their home due to judicial influence with households who are actually foreclosed upon. 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.

The gap between marginal and average cases is highly relevant for policy and welfare. In particular, our marginal cases are most informative for foreclosure mitigation policies that are similar to our judge instrument in that they they affect a relatively small group of marginal homeowners who have to take costly action to be treated. By contrast, our results for average homeowners are more relevant for the average effect of broad-based foreclosure mitigation policies for which take up is less costly. Nonetheless, for such broad-based policies, our findings of declines for marginal homeowners are still relevant for aggregate welfare calculations that place more weight on households with larger losses.

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, landlords, and renter, 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 Appendices A.4 and A.5. 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.[5] 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. 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 a new main judge that takes over the calendar.[6] Court documents describe random assignment beginning in 2005, which is when we 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 new calendars due to high caseloads, 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

[5] For 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 refiled.

[6] One 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 in Appendix B.1.2. Because we observe 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 Appendix B.1.2. Crucially, with the cleaned calendar, we are able to replicate stable random assignment probabilities that correspond to court documents as described in Appendix B.1.2.

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 (2019).

Fourth, we merge in annual snapshots of individual credit reports from TransUnion (TU). This allows us to observe VantageScore 3.0 (a credit score comparable to a FICO score that we subsequently refer to as the "VantageScore"), borrowing, debt payments, delinquency status, loan modifications, foreclosure flags, and death.

Fifth, we obtain ZIP code income from the IRS Statistics of Income Tax Stats and school test scores from the Illinois Board of Education.[7]

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.[8] 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 case because RIS may not report all of the parcel APNs. This affects roughly 10% of cases. We then merge in the DataQuick and CoreLogic deeds records using the APN.

We merge our case-level data with individual-level address histories from Infutor. To do so, we search the individual Infutor address histories to find homeowners and tenants who live at an address when the foreclosure filing begins as well as landlords who own foreclosed properties. We construct outcomes for each case-person measured in years from the initial foreclosure filing from five years prior to the foreclosure start until five years after the foreclosure start. We also merge in individual-level outcomes from RIS regarding crime, bankruptcy and divorce using first and last name and ZIP code.

We next identify homeowners, renters, and landlords. We are purposefully cautious in who

[7]We 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.

[8]We 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 Appendix A.3.

we define in each category, and drop 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 two criteria. First, a landlord is someone whose address matches the mailing address listed in the deeds records from prior transactions of the foreclosure property and whose last name matches the defendant name in the court case.[9] Second, 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 with a name match living on the same street or in the same ZIP code as the foreclosed property, we define them as 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.[10] We also restrict 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 introduce several sample restrictions to construct our final data set. We drop non-residential properties and cases that are not clearly classified as either a foreclosure or a dismissal. We also remove people who have Infutor address histories that are clearly duplicates, as described in Appendix A.2. 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 3.2.2 we confirm that these restrictions do not induce sample selection by showing that our instrument is uncorrelated with inclusion in our analysis sample or

[9] The mailing address of the owner is listed when the property transacts or when a new mortgage is issued. We filter to only the mailing addresses that are different from the property address. Sometimes this is a business address, which makes it hard for us to track these landlords.

[10] We use cases where where multiple individuals in Infutor match the first and last name of the landlord 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.

Table 1: Sample Construction Summary

Step	Cases	Case-People
All Foreclosure Cases 2005-2012 (Cook County Court)	275,401	
Cases Matching to a Single Property in RIS	247,370	
Residential Property Cases	244,831	
Drop Unclear Foreclosure Case Outcome	239,835	
Keep if Match to Someone in Infutor	229,346	1,125,977
Keep if Owner, Renter, or Landlord	183,509	388,723
Drop A Few Problematic Observations With No Leniency	183,494	388,677
Full Final Sample	183,494	388,677
Owners	124,951	248,494
Renters	15,850	80,132
Landlords	54,237	60,051
TU Subsample	174,388	316,514
Owners	121,346	222,966
Renters	13,142	39,595
Landlords	48,477	53,953

Notes: The top panel shows our sample size at each step in the data cleaning process. The bottom two panels show sample sizes broken down into owners, renters, and landlords for the full final sample and TU subsample, respectively. Note that the full final sample and full TU subsample rows are not the sum of the rows below because renter cases include the landlord cases and additional cases with duplicate landlords that are not in the landlord sample as described in Footnote 10.

classification as an owner, renter, or landlord.

We finally match our data set to the TransUnion credit reports to create a subsample of our main sample that we refer to as the ”TU subsample.” 81 percent of the people in our final sample match to a credit report, and 95 percent of our cases have at least one matched individual as indicated in the bottom panel of Table 1. The match rate is lowest for renters.

2.3 Outcomes For Properties With a Foreclosure

Before moving on, we validate our court-record-based foreclosure measure. 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.[11] A similar fraction are observed with a new mortgage without a sale in the deeds data (indicative of a refinancing or a loan modification, although not all modifications are recorded), 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 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. Indeed, in private conversations, lawyers who represent parties in Cook County foreclosure cases told us that the vast majority of dismissed cases are due to a settlement with the lender, usually in the form of a loan modification or payment plan. 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.

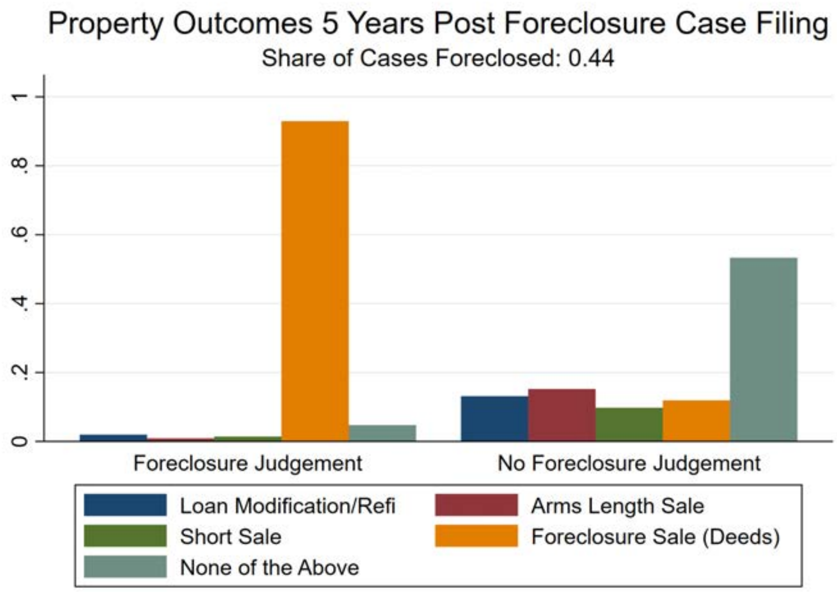
3 Empirical Approach

We estimate the effect of foreclosure using two complementary approaches: a propensity-score-matched event study (PSM) and an instrumental variables (IV) approach that exploits random judge assignment. Each approach has its benefits and drawbacks, so the two approaches are complementary. The judge IV has less power but is more convincing because of the clean nature of the instrument. It also identifies a LATE for marginal cases affected by the judge IV. PSM has more power, particularly for landlords and renters. The additional power also lets us analyze heterogeneity across various sub-samples. However, the identification assumptions are stronger as there is clear endogeneity in foreclosure and PSM requires strong selection on observables assumptions to be interpreted as causal. For the full sample, PSM identifies an ATE for the population. The difference between average and marginal effects will prove to be important for our results.

We first present an ordinary least squares (OLS) event study approach, since OLS is simplest and both of our main approaches build on OLS. We then present IV and PSM in turn.

[11] A 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 after the initial foreclosure filing separately for cases in which we infer a foreclosure or not based on the court 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.

3.1 Regression Framework

Consider the following individual-level regression which estimates the effect of foreclosure on various outcomes Y using an event study approach:

$$Y_{i,k,s,t} = \beta_s F_k + \gamma_s X_i + \xi_{m(k),s} + \phi_{z(k),s,t} + \varepsilon_{i,k,s,t}. \tag{1}$$

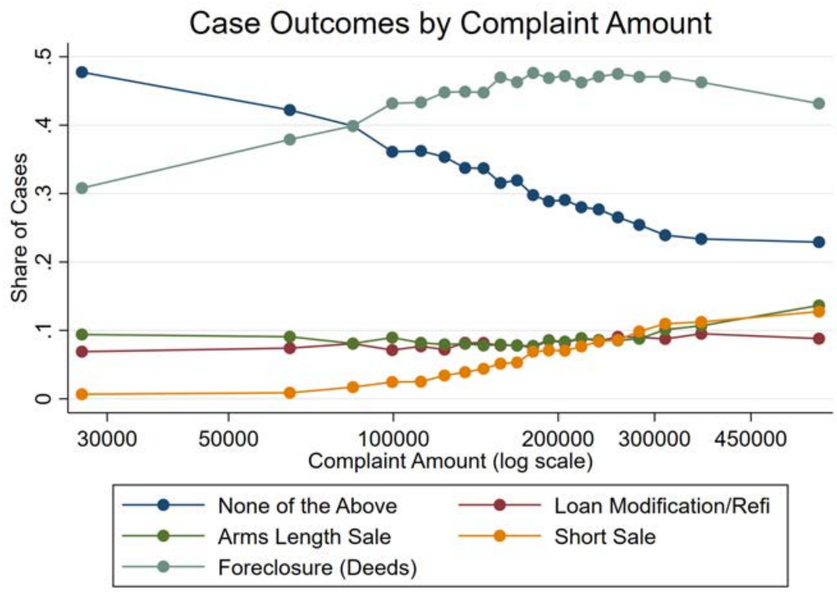
i indexes residents or owners of foreclosed-upon properties, k indexes foreclosure cases (which can involve multiple residents), t is the year in which the initial foreclosure filing occurs. $z(k)$ is the ZIP code of residence at the time of foreclosure filing, $m(k)$ is the month and year of the initial foreclosure filing, and F_k is an indicator for whether the foreclosure case resulted in a foreclosure within 3 years of the initial foreclosure filing. We estimate a separate regression for each event-year $s = -5, ..., 5$ and either plot the regression coefficients β_s to analyze the treatment effects or report a pooled coefficient for $s = 3$ and $s = 4$. To ensure that sample composition is not driving our results, we require that the panel be balanced from $s = -3$ to $s = 3$. The regression controls for individual-level observables X_i, a fixed effect for the month of the foreclosure start $\xi_{m(k),s}$, and a ZIP code-by-year fixed effect $\phi_{z(k),t,s}$. Unless otherwise indicated, X_i does not include any additional controls for location-based outcomes and includes the $Y_{i,k,t-1}$ 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. For owners and landlords, 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. For renters, we weight by person since there may be multiple units in the building and it is less clear how many households there are. Because this is an individual level regression but foreclosure is determined at the case level, standard errors are clustered at the case k level. We use the same weighting and clustering for PSM and IV appropriately modified as described below.

This regression performs a cross-sectional comparison between homes with a foreclosure filing that are foreclosed upon and not foreclosed upon. Such a cross-sectional comparison does not lead to causal estimates unless foreclosure is random. Prior studies such as Brevoort and Cooper (2013), Piskorski and Seru (2018), and Currie and Tekin (2015) have dealt with this by using an OLS event study design that is similar to a difference-in-difference strategy using sharp variation at the moment of foreclosure. Molloy and Shan (2013) use a propensity score matching approach similar to the one we use. The event study requires assuming that conditional on observables, foreclosed and non-foreclosed households would have followed parallel trends absent foreclosure and that the foreclosure is the only thing that happens in the year of foreclosure.[12]

There are three potential issues with OLS. First, OLS 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 (2019), 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 larger 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. These are just a few of

[12]The literature uses the full population rather than limiting to individuals who experience a foreclosure filing.

Figure 2: Property Outcomes By Complaint Amount



Notes: The figure shows case outcomes from the CoreLogic deeds data 5 years after 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 CoreLogic, a short sale as categorized by CoreLogic, or none of the above.

the observables for which there are clear differences between the foreclosure and non-foreclosure groups.

Second, 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 6% of cases. These cases are an odd control group for foreclosed-upon properties because they were unlikely to ever foreclose.

Third, OLS treats all non-foreclosure outcomes the same. Some look a lot like foreclosure, such as a deed in lieu of foreclosure (which the deeds data 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. This, too, is related to a heterogeneous likelihood of foreclosure.

One clear indicator that one if not all of these problems is present is the presence of pre-trends in some of our samples using OLS as shown in Appendix C.2. This is functionally a failed placebo test. Given these limitations with OLS, we pursue two different approaches: IV using random judge assignment and PSM following Molloy and Shan (2013).

3.2 Instrumental Variables Approach

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.[13] 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.[14] Most notably, judges can push for non-foreclosure resolutions such as a loan modification, a short sale, or a deed in lieu of foreclosure. Discussions with lawyers who represent parties in Cook County foreclosure cases reveal that judges are highly heterogeneous in the degree to which they push parties to settle. Judges can also 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 personally served or an "honest and well-directed" effort was not undertaken to find them.[15] Cases can also be dismissed if the borrower is found not sane or unable to enter into a contract, if the judge determines 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 "robosigning" 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.[16]

[13]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."

[14]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 is based.

[15]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.

[16]In 2013, the Supreme Court adopted new state-wide rules with clearer guidelines that (1) standardized fore-

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.[17] 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. (2020), Dobbie et al. (2018), Humphries et al. (2019), Collinson and Reed (2019)).[18]

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

$$Y_{i,k,s,t} = \beta_s F_k + \gamma_s X_i + \xi_{m(k),s} + \phi_{z(k),s,t} + \varepsilon_{i,k,s,t} \tag{2}$$

$$F_k = \Gamma Z_{k,c,t} + \alpha X_i + \zeta_{m(k)} + \varphi_{z(k),t} + e_{i,k,t}. \tag{3}$$

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

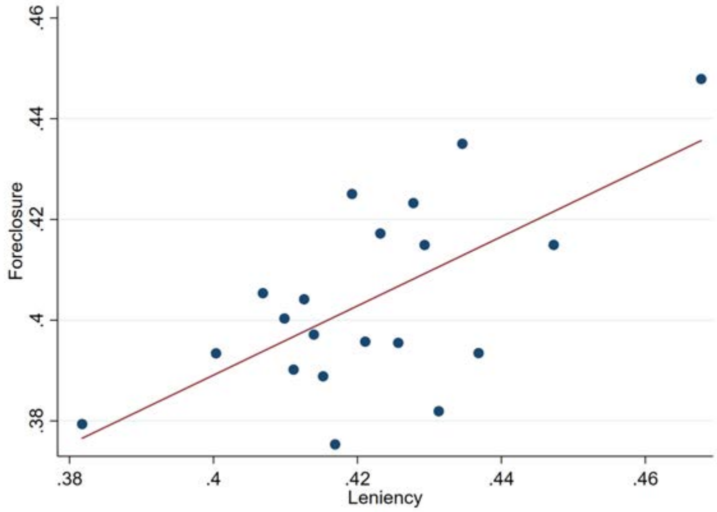
β_s is the local average treatment effect of foreclosure at horizon s for the variation induced by

closure 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.

[17] 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 our results are not sensitive to the frequency of the instrument.

[18] Some of the literature such as Collinson and Reed (2019) finds that judge leniency is correlated with time to decision and decompose the two effects. In our data, lenient judges do take longer. In Appendix D.3, we run regressions with both foreclosure and time to judgment instrumented by average judge leniency and average judge time to judgement. We find that the causal effects we attribute to foreclosure in the main paper are not driven by time to judgement, which has separate effects that are somewhat orthogonal to foreclosure.

Figure 3: First Stage Binned Scatter Plot



Notes: This figure is a binned scatter plot of the first stage for all observations in our analysis sample (owners, renters, and landlords combined). The binned scatter plot residualizes out month and ZIP-year fixed effects to be consistent with (3). We weight by the inverse of the number of people per case for owners and landlords so that each foreclosure case is weighted equally.

the instrument. This approach requires that the instrument be relevant – that is $\Gamma \neq 0$ – and that the instrument be orthogonal to the second stage error term – that is $Z_{k,c,t} \perp \varepsilon_{i,k,z,m,t}$ – which is satisfied by random calendar assignment. The IV approach also requires monotonicity – that is that all judges have the same ranking of the likelihood of foreclosure across cases. In the judge IV context, montonicity is a concern if, for instance, minority judges are more lenient for minority defendants and non-minority judges are more lenient for non-minority defendants. We evaluate the monotonicity assumption extensively in Appendix B.2 and find no evidence of a violation of monotonicity.

3.2.1 First Stage

The top panel of Table 2 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 3, 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. Thet 7% of

Table 2: IV First Stage Regression Coefficients and F Statistics

Dependent Variable: Foreclosed Within 3 Years				
Sample	All	Owner	Renter	Landlord
	(1)	(2)	(3)	(4)
Full Sample	0.692***	0.697***	0.945***	0.643***
Judge Leniency	(0.056)	(0.066)	(0.205)	(0.111)
F	152.5	112.1	21.3	33.3
N	183,494	124,951	15,850	54,237
TU Subsample	0.672***	0.668***	1.142***	0.723***
Judge Leniency	(0.060)	(0.069)	(0.341)	(0.117)
F	124.6	93.4	11.2	38.0
N	174,388	121,364	13,142	48,477

Notes: This table shows first stage regression coefficients for all cases (owners, renters, and landlords combined), owners, renters, and landlords. The top panel shows results for the full sample, while the bottom panel shows it for the TU subsample. The regression is at the case-person level. The first stage includes month and zip-year fixed effects as in (3). All standard errors are clustered by case, and we weight by the inverse of the number of people per case for owners and landlords so that each foreclosure case is weighted equally. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

cases have their outcome impacted by the random assignment of the judge are the cases we study using our IV design.

The bottom panel shows the first stage for the TU subsample. The F statistics are 93.38 for owners, 11.21 for renters, and 38.04 for landlords. The first stage is very similar for the full sample for all groups, but power falls precipitously for renters due to the lower match rate.

3.2.2 Verifying Random Assignment

It is crucial for our IV approach that we verify random assignment, which we do in three ways. First, the Appendix B.1.2 shows that our cleaned calendar variable replicates published random assignment probabilities from the Cook County Courts. Second, we show that there are no-pretrends 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 3 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.

Table 3: Placebo Tests: Leniency Instrument Regressed on Observables

	Case-Level				Case-Person-Level		
Dependent Variable	In Sample	Owner	Renter	Landlord	Age	Male	TU Match
Judge Leniency	-0.007	0.024	0.005	-0.004	2.239	0.008	-0.047
	(0.044)	(0.054)	(0.033)	(0.053)	(1.672)	(0.048)	(0.032)
N	244,831	183,494	183,494	183,494	290,369	388,648	388,648

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 four columns are case-level regressions with dependent variables for being in sample (matched to an owner or renter), owner, renter, and landlord. The next three 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.

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. The final column shows that judge leniency has no effect on whether we are able to match to credit reports in the TU subsample.

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.

3.2.3 Characteristics of Compliers

Our IV approach estimates a local average treatment effect for the compliers to random calendar assignment. Before moving on, we examine the characteristics of this group relative to the always takers and never takers of treatment. We define always takers as those who would foreclose even when assigned to the most lenient calendar. Never takers are defined as those who do not foreclose even when assigned to the most strict calendar. Compliers are those whose outcomes are impacted by the random assignment of the calendars in our sample. Table 4 shows summary statistics for

Table 4: Summary Statistics by Complier Type-Owners

Group Average	Mean Log ZIP Income at t (1)	Mean Log ZIP Income at $t-5$ (2)	Log Living Square Feet (3)	Elementary School Test at $t-5$ (4)
Compliers (7%)	3.76	3.77	7.27	41
Always Takers (31%)	3.71	3.66	7.24	33
Never Takers (62%)	3.75	3.69	7.27	35
Group Average	**Middle School Test at $t-5$ (5)**	**Outstanding Debt at t (6)**	**Debt At Purchase (7)**	**Elementary School Test t (8)**
Complier (7%)	48	\$212.3K	\$210.5K	41
Always Takers (31%)	44	\$197.9K	\$196.8K	36
Never Takers (62%)	45	\$192.5.K	\$195.3K	38
Group Average	**Middle School Test at t (9)**	**Self Represented (10)**	**Has Lawyer (11)**	**VantageScore at $t-5$ (12)**
Complier (7%)	50	0.084	0.060	610
Always Takers (31%)	42	0.047	0.038	605
Never Takers (62%)	44	0.055	0.039	597

Notes: This table shows summary statistics for compliers, always takers, and never takers of foreclosure within our homeowner sample.

these three groups within our homeowner sample.

Because the variation in the range of judge leniency is about 7 percentage points, it is not surprising the share of our homeowner cases that are compliers is about 7%. We find 31% are always takers and 62% are never takers. Compliers appear to have more to lose from a foreclosure. They have 7% more mortgage debt at the time of foreclosure than always takers and 10% more than never takers. They also took out larger mortgage at purchase than both always and never takers. They live in better neighborhoods than both the always and never takers, as measured by neighborhood income, and elementary and middle school test scores. This is especially apparent five years prior to treatment when compliers live in neighborhoods with 11% higher incomes and 8 percentage point higher elementary school test scores than always takers and 8% higher incomes and 6 percentage point higher elementary school tests than never takers. Compliers also appear to be more likely to fight the foreclosure case: 6.0% of them hire a lawyer, a compared to 3.8% and 3.9% of always and never takers, respectively. They also represent themselves in their cases 8.4% of the time, which is an indicator that they actively show up to court. By contract, always and

Table 5: Summary Statistics by Complier Type-Renters

Group Average	Mean Log ZIP Income at t (1)	Mean Log ZIP Income at $t-5$ (2)	Log Living Square Feet (3)	Elementary School Test at $t-5$ (4)
Compliers (10%)	3.54	3.60	7.73	22
Always Takers (53%)	3.56	3.50	7.65	21
Never Takers (37%)	3.64	3.57	7.68	23
Group Average	Middle School Test at $t-5$ (5)	Outstanding Debt at t (6)	Debt At Purchase (7)	Elementary School Test t (8)
Complier (10%)	50	$260.9K	$248.0K	21
Always Takers (53%)	47	$245.0K	$237.5K	23
Never Takers (37%)	47	$250.4K	$253.1K	26
Group Average	Middle School Test at t (9)	Self Represented (10)	Has Lawyer (11)	Vantage Score at $t-5$ (12)
Complier (10%)	59	-0.01	0.02	537
Always Takers (53%)	44	0.03	0.07	559
Never Takers (37%)	48	0.063	0.07	573

Notes: This table shows summary statistics for compliers, always takers, and never takers of foreclosure within our renters sample.

never takers only self represent 4.7% and 5.5% of the time, respectively.

Overall, the compiler analysis for homeowners suggests compliers tend to have more to lose from foreclosure than both always takers and never takers. This will prove important when we analyze the difference between IV and PSM, as IV estimates local average treatment effects for compliers while PSM estimates average treatment effects for the entire population.

Table 5 shows these same summary statistics among the renter-occupied cases we study. Unlike homeowners, renter compliers do not exhibit as clear selection selection patterns. They appear to have lower credit scores, live in larger buildings with larger mortgages, and have landlords who are very unlikely to use a lawyer or self represent themselves in the foreclosure case than always and never takers. There is no clear selection patterns in terms of neighborhood quality, as measured by neighborhood income or school quality. Table 6 shows these summary statistics for our landlord cases. Complier landlords are not selected based on neighborhood quality, having a lawyer, or self representing. They do appear to live in 5% to 7% smaller homes, but have larger mortgages and better credit scores than always and never takers.

Table 6: Summary Statistics by Complier Type-Landlords

Group Average	Mean Log ZIP Income at t (1)	Mean Log ZIP Income at $t-5$ (2)	Log Living Square Feet (3)	Elementary School Test at $t-5$ (4)
Compliers (6%)	3.86	3.76	7.32	37
Always Takers (51%)	3.84	3.74	7.37	37
Never Takers (43%)	3.89	3.81	7.40	39
Group Average	**Middle School Test at $t-5$ (5)**	**Outstanding Debt at t (6)**	**Debt At Purchase (7)**	**Elementary School Test t (8)**
Complier (6%)	39	$226.9K	$235.6K	37
Always Takers (51%)	49	$206.3K	$199.3K	41
Never Takers (43%)	52	$207.3K	$207.8K	42
Group Average	**Middle School Test at t (9)**	**Self Represented (10)**	**Has Lawyer (11)**	**Vantage Score at $t-5$ (12)**
Complier (6%)	45	0.033	0.006	644
Always Takers (51%)	49	0.027	.052	635
Never Takers (43%)	52	0.049	.056	638

Notes: This table shows summary statistics for compliers, always takers, and never takers of foreclosure within our landlord sample.

3.2.4 Advantages of Judge IV Approach

The key advantage of our judge IV approach is it provides clean evidence of causality with minimal additional assumptions. The judge assignment is truly random. The judge IV estimates a local average treatment effect for marginal cases that are affected by judge random assignment. This is likely to be the relevant treatment effect for policies that provide a mortgage modification to a small number of people after considerable legwork, much like the compliers must fight the case in court.

The main disadvantage of the judge IV approach is power. We have significant power for owners, but limited power for renters and landlords. Indeed, in some cases we obtain an imprecise zero with the IV approach. We also do not have sufficient power to analyze heterogeneity in the IV treatment effect in any meaningful way. We thus present a PSM design to complement our IV results.

3.3 Propensity-Score Matching Approach

The PSM approach generalizes the OLS event study design in equation (1) by turning the ZIP-time and date-of-filing fixed effects into ZIP-time-propensity-score-bin and date-of-filing-propensity-score-bin fixed effects:

$$Y_{i,k,s,t,p} = \beta_s F_k + \gamma_s X_i + \xi_{m(k),s,p} + \phi_{z,s,t,p} + \varepsilon_{i,k,s,t,p}, \tag{4}$$

where p indexes the propensity score bin. As with OLS, regressions are weighted by the inverse number of people per case for owners and landlords and standard errors are clustered by case. As with the OLS event study and IV, we present figures of the β_s coefficients and report a pooled β_s for $s = 3$ and $s = 4$ in a table.

To create propensity score bins, we run a regression of the foreclosure indicator for the case F_k on a number of observables three years prior to the case filing at $s = -3$:

$$F_k = \alpha X_{i,s-3} + \zeta_{m(k)} + \phi_{z(k),t} + e_{i,k,t}. \tag{5}$$

We use the predicted values of this regression based only on the observables, $\hat{\alpha} X_{i,s-3}$, to obtain propensity scores at the person level. We then split individuals into deciles based on their propensity score, which is what we use as the propensity score groups.[19]

By conditioning the fixed effects on propensity score deciles, our PSM estimates compare individuals who have similar propensities to be foreclosed upon but who receive different outcomes. This helps address the concerns about OLS. PSM has been found to be quite effective in non-experimental settings (Dehejia and Wahba (2002)). PSM also directly addresses the issues of a poor control group and poor counterfactual outcomes to foreclosure by conditioning on the propensity score so that cases that are very likely to foreclose are not compared to cases that are unlikely to foreclose. Appendix C.2 shows that the propensity score adjustment eliminates many of the pre-trends we observe in OLS.

The identifying assumption of the PSM approach is that (1) conditional on the propensity score fixed effects, the treatment and control groups have parallel trends and (2) that there are no other omitted shocks that occur at the time of foreclosure. This is a somewhat stronger assumption than IV because the propensity score control may not control for shocks that trigger a foreclosure. While

[19] The main advantage of using propensity score groups (similar to matching) rather than controlling for the propensity score is that doing so is more non-parametric.

the presence of parallel pre-trends is reassuring, it does not test for a violation of this assumption.

One issue is how to select the lagged observables $X_{i,s-3}$ that we use to create the propensity scores. To do so, we estimate equation (5) separately for owners, renters, and landlords for 11 different non-credit observables and 9 credit-related observables described in Appendix C.1. We pick the five observables with the most explanatory power for each subgroup and use these five variables as the $X_{i,s-3}$ in estimating the propensity score.[20] This procedure is quite successful based on its ability to reduce pre-trends.

3.4 Advantages of Propensity Score Matching Approach

The key advantage of our PSM approach is that it provides us with substantially more power, particularly for landlords and renters. It thus complements the IV approach by allowing us to study a number of outcomes and sub-groups for which we would otherwise have no significant evidence. This approach also provides us with an average treatment effect for the population rather than a local average treatment effect for compliers to the judge instrument.

The main disadvantage of the PSM approach is that it assumes selection on the observables is sufficient for a causal interpretation, and there may be unobservables for which the PSM approach cannot fully control. While PSM reduces pre-trends, it does not always remove them altogether indicating that the PSM method is imperfect.

Given the pluses and minuses of both approaches, we use the two approaches to complement one another. In particular, we obtain broadly similar results for IV and the PSM for a number of outcomes. For other outcomes, they look quite different. However, we are able to find sub-samples for whom OLSs look very similar to the IV results for the full population, and our selection of these sub-samples have a clear interpretation of being due to the difference between marginal and average cases.[21] IV and PSM thus complement one another even in cases where they may appear at first blush to disagree.

[20] For observations without matched credit report data, we substitute the sample average. This performs similarly to limiting ourselves to the TU sample.

[21] We use OLS rather than PSM for sub-samples because over-saturating the PSM regressions with fixed effects for small samples leads to very wide standard errors.

4 Results: Owners

Figures 4, 5, 6, and 7 and columns 1 and 2 of Table 7 show IV and PSM results for owners. Table 7 shows the pooled point estimate for years 3 and 4 relative to the base year. The figures show the IV point estimates for each year $s = -5, ..., 5$ since foreclosure indicated with blue dots and 95% confidence intervals indicated by bars. The PSM point estimates are shown by red Xs, and we omit confidence intervals from the figures since they are so small. For some outcomes, such as whether an owner had moved from their residence as of the date of foreclosure, the treatment effect is zero in year zero by construction. For other outcomes, we allow for a treatment effect in the year of the foreclosure filing by controling for the $t-1$ outcome, so the coefficient is zero in year -1. Importantly, none of our IV results have statistically significant pre-trends and none of our PSM results have economically significant pre-trends, which would be evidence against a causal interpretation of our PSM or IV approaches.

4.1 Moving, Housing, and Homeownership

Perhaps unsurprisingly, foreclosures cause homeowners to move out of foreclosed-upon home as shown in Panel 4a. Moving picks up immediately after the case filing and is 29% higher by years 3 and 4 for both IV and PSM. It is worth noting that as a raw mean, about half of the owners who have a foreclosure ruling against them in the first 3 years have moved by year 5.[22] For many results, PSM and IV are broadly similar, as they are for moving. The fact that these two methodologies agree reinforces our conclusions.

Our address history data allow us to take our analysis of moving one step further and examine housing instability. To do so, Panel 4b shows results for the cumulative number of moves since the foreclosure filing. There is some evidence of housing instability: Even though the causal effect of foreclosure on living at the original address is 0.29 in years 3 and 4, the cumulative number of moves is 0.39 according to IV and 0.35 according to PSM. For IV, this rises to 0.54 in year five. Foreclosure thus causes the average foreclosed-upon homeowner who leaves their home to move

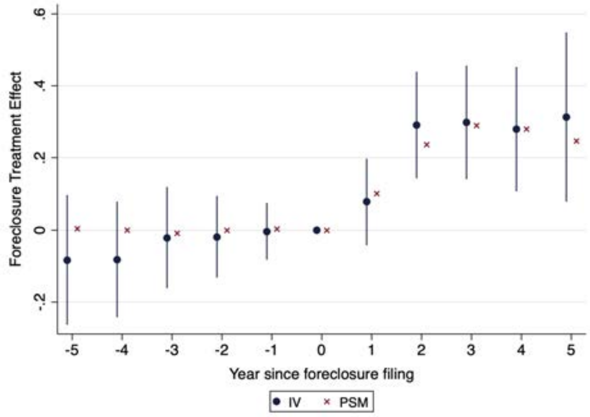
[22] While one might expect that all owners are evicted after a foreclosure, it turns out that many homes that are repossessed by a lender are still occupied by the previous owner, and our 50 percent number is in line with other studies. For instance, Molloy and Shan (2013) find that only 55% of those 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 (see https://mortgageorb.com/realtytrac-47-of-bank-owned-homes-still-occupied-by-former-owner).

Table 7: Summary of All IV and PSM Results at Years 3 and 4

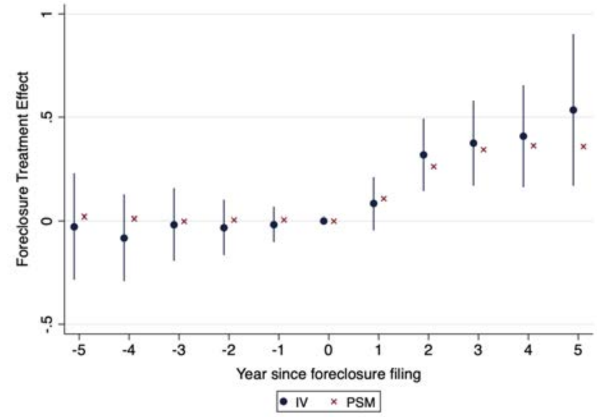
	Owner		Landlord		Renter	
Specification:	IV	PSM	IV	PSM	IV	PSM
	(1)	(2)	(3)	(4)	(5)	(6)
Moved from Foreclosure Address	0.289***	0.285***	0.050	0.017***	0.107*	0.005
	(0.081)	(0.003)	(0.161)	(0.006)	(0.063)	(0.005)
Cumulative Number of Moves	0.390***	0.352***	-0.042	0.031***	0.125	0.005
	(0.110)	(0.004)	(0.235)	(0.008)	(0.084)	(0.006)
Owns Primary Residence	-0.220**	-0.166***	0.339*	0.027	0.021	-0.013
	(0.106)	(0.004)	(0.204)	(0.036)	(0.061)	(0.013)
Log Square Footage of Living Space	-0.021	0.022***	-0.106	-0.009	-0.080*	0.004
	(0.051)	(0.002)	(0.117)	(0.007)	(0.048)	(0.004)
Log Zip Code Average Income	-0.106**	0.016***	0.127	-0.004	-0.044	-0.004**
	(0.041)	(0.001)	(0.088)	(0.003)	(0.032)	(0.002)
Elementary School Test Score Rank	-3.282	0.711***	0.324	0.045	-3.737	-0.154
	(2.769)	(0.097)	(6.803)	(0.310)	(2.416)	(0.147)
Middle School Test Score Rank	-6.143**	0.101	-6.317	0.001	2.251	-0.057
	(2.851)	(0.088)	(5.584)	(0.197)	(2.769)	(0.156)
High School Test Score Rank	-3.025	0.988***	-6.568	-0.085	0.253	-0.363**
	(3.107)	(0.101)	(5.884)	(0.226)	(2.288)	(0.176)
Cumulative Number of Divorces	0.065**	0.002*	-0.065	-0.001	0.005	-0.001
	(0.028)	(0.001)	(0.051)	(0.002)	(0.012)	(0.001)
Cumulative Number of Crimes Convicted	0.023	0.005	0.157	0.008	0.015	0.028***
	(0.100)	(0.004)	(0.181)	(0.009)	(0.102)	(0.010)
Cumulative Number of Bankruptcies	0.027	0.006***	0.095	0.015***	-0.007	-0.001
	(0.048)	(0.002)	(0.127)	(0.004)	(0.015)	(0.001)
Cumulative Number of DUI Convictions	0.017	-0.001	0.058*	0.003**	0.005	0.000
	(0.022)	(0.001)	(0.033)	(0.001)	(0.009)	(0.001)
VantageScore	-13.866	-3.538***	10.422	2.909*	-5.913	0.208
	(25.752)	(0.733)	(36.319)	(1.526)	(41.752)	(2.776)
Death	0.006	0.008***	0.022	0.002	0.021	-0.002
	(0.026)	(0.001)	(0.039)	(0.002)	(0.023)	(0.002)
Number of Foreclosures	0.085	0.210***	0.055	0.325***	0.016	-0.003
	(0.097)	(0.003)	(0.492)	(0.021)	(0.035)	(0.004)
Number of Unpaid Collections	1.255*	0.375***	0.379	0.264***	1.189	0.164*
	(0.667)	(0.021)	(1.198)	(0.053)	(0.778)	(0.088)
Number of Auto Loans	-0.152	-0.022***	-0.324	0.016	0.156	0.008
	(0.210)	(0.006)	(0.409)	(0.017)	(0.222)	(0.020)
Number of Mortgages 90+ DPDs	-0.288	-0.248***	-0.167	-0.344***	0.196	0.028
	(0.246)	(0.007)	(0.519)	(0.023)	(0.254)	(0.028)
Number of Mortgages with Loan Mod	0.036	-0.099***	-0.088	-0.100***	0.011	0.000
	(0.053)	(0.001)	(0.119)	(0.005)	(0.017)	(0.002)
Number of Open Mortgages	-0.260***	-0.310***	-0.226	-0.409***	-0.158*	0.002
	(0.095)	(0.003)	(0.321)	(0.014)	(0.084)	(0.007)
Open Mortgage Balance / RIS Amount	-0.416***	-0.307***	-0.004	-0.424***	0.021	-0.002
	(0.157)	(0.006)	(0.898)	(0.043)	(0.135)	(0.010)

Notes: This tables summarizes our IV and PSM results for owners, landlords, and renters by showing pooled results for years 3 and 4 relative to the base year. For IV, we estimate equation (2) by two-stage least squares with the first stage being equation (3). For PSM, we estimate equation (4) and create the propensity score as described in the main text. For owners and landlords, regressions are weighted by the inverse number of people per case. All standard errors are clustered by case. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

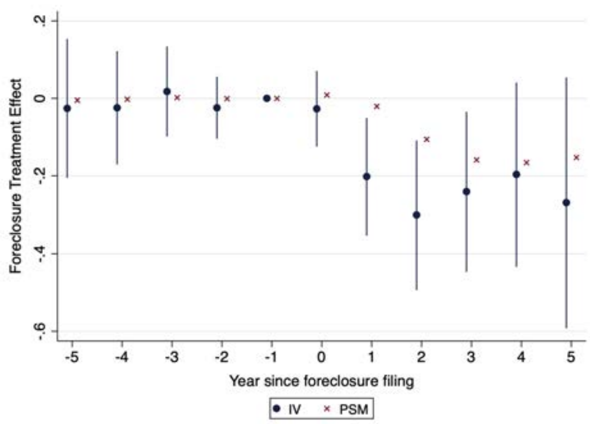
Figure 4: Owners: Moving and House Characteristics



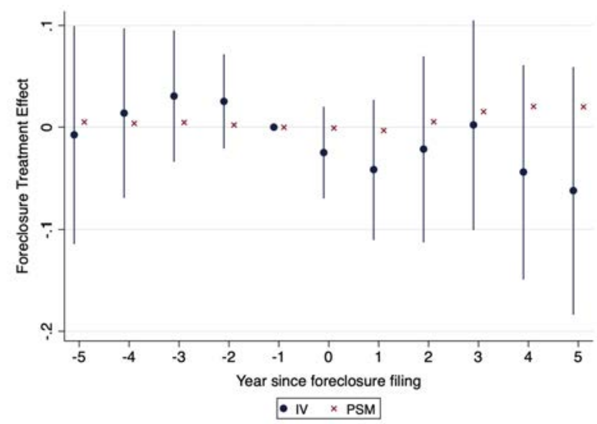
(a) Moved from Foreclosure Address



(b) Cumulative Number of Moves



(c) Owns Primary Residence



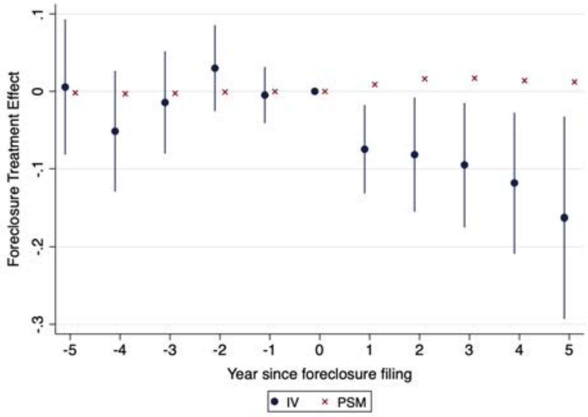
(d) Log Square Footage of Living Space

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all owners in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case.

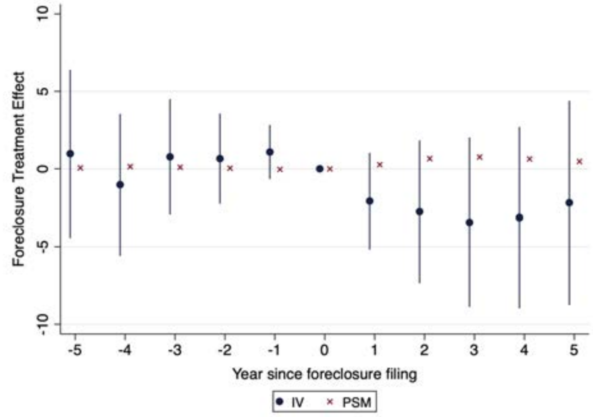
more than once. This result echoes findings by Collinson and Reed (2019), who show that eviction causes increases in residential instability, although they focus on homelessness rather than multiple moves.

Foreclosed-upon homeowners are also significantly less likely to own their residence after a foreclosure. Panel 4c shows estimates for whether the foreclosed-upon homeowner owns their primary residence in year t. IV finds a 30 percentage point fall by year two. This remains low: By years 3 and 4, homeownership drops 22 percentage points for IV and 17 percentage points for PSM. An effect that persists for at least five years is in line with what Piskorski and Seru (2018)

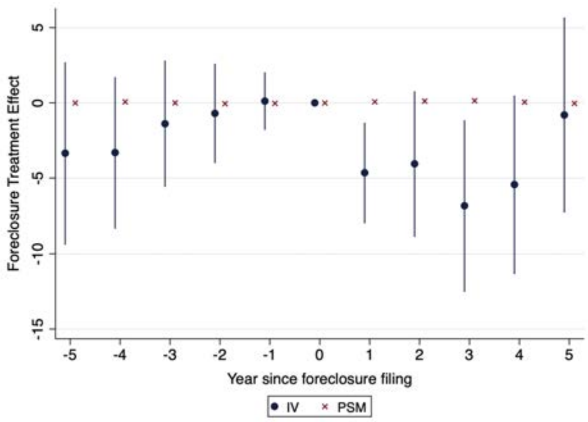
Figure 5: Owners: Neighborhood Characteristics



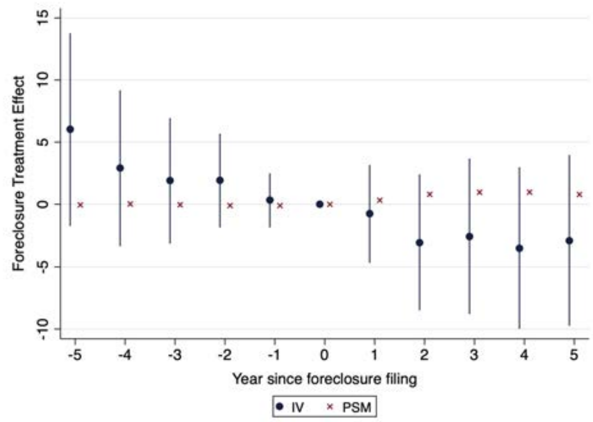
(a) Log ZIP Code Average Income



(b) Elementary School Test Score Rank



(c) Middle School Test Score Rank



(d) High School Test Score Rank

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all owners in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. 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.

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 evidence that foreclosure causes households to downsize to a smaller home. 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 for

IV and a significant but economically small positive effect on square footage effect for PSM, as shown in Panel 4d.

4.2 Neighborhood Quality

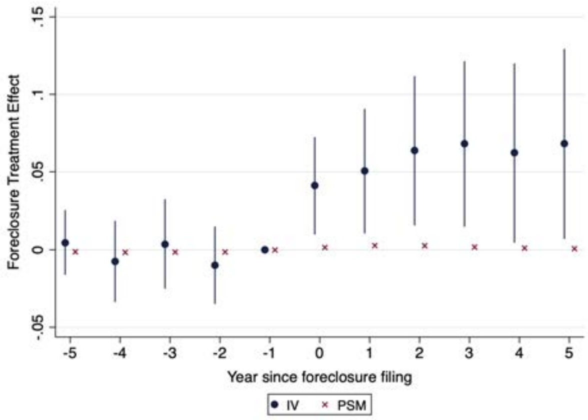
We find evidence that marginal foreclosed-upon homeowners move to worse neighborhoods, as shown in Figure 5. Panel 5a shows that for IV, foreclosures cause the log average income in the ZIP code in which the foreclosed-upon homeowner resides to fall by a statistically-significant 11% by years 3 and 4 and 16% by year 5. These are economically large effects that suggest that foreclosure causes owners to move to significantly lower-income neighborhoods. These effects widen over time, indicating that foreclosed owners fall further and further behind in neighborhood quality relative to where they would have been absent foreclosure.

However, we do not see a similar decline for PSM, for which ZIP average income rises by a statistically but not economically significant amount. This finding is consistent with Molloy and Shan (2013). We analyze the discrepancy between IV and PSM for several outcomes after we review all of our results for owners in Section 4.5 below. In that section, we present evidence that the discrepancy is due to differential treatment effects for marginal households (IV) and average households (PSM), which is consistent with marginal households being in nicer neighborhoods and have further to fall on a neighborhood quality ladder than average households.

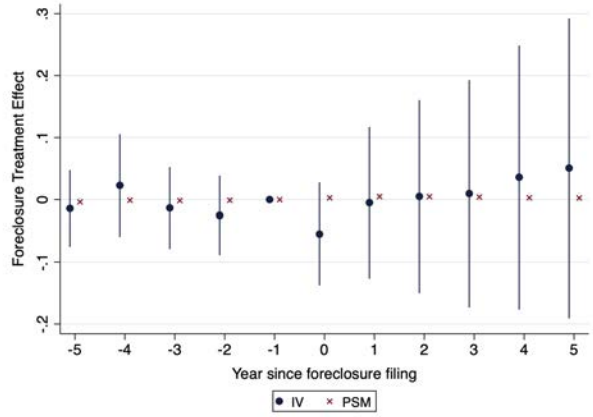
Another measure of neighborhood quality is school quality. Panels 5b, 5c, and 5d show the causal effect of foreclosure on the average (across reading and math) percentile rank of the local elementary, middle, and high school, respectively, on Illinois standardized tests. The results show that marginal households move to neighborhoods with 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 6 percentiles lower in years 3 and 4. These significant effects on middle school test scores hint at potential effect on the children of marginal foreclosed-upon households, but we cannot test long-run outcomes for children directly with our data.

As with ZIP code income, we do not observe large negative effects on neighborhood quality for average households using PSM. We ascribe this to the fact that neighborhood quality generally – which is picked up by income and school quality – declines for marginal foreclosed-upon owners but not the average foreclosed-upon owners.

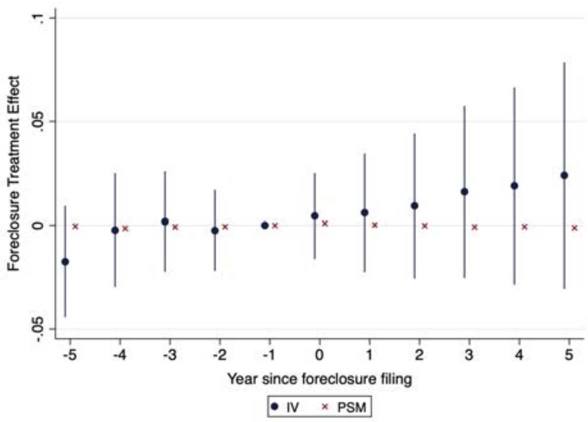
Figure 6: Owners: Personal Outcomes



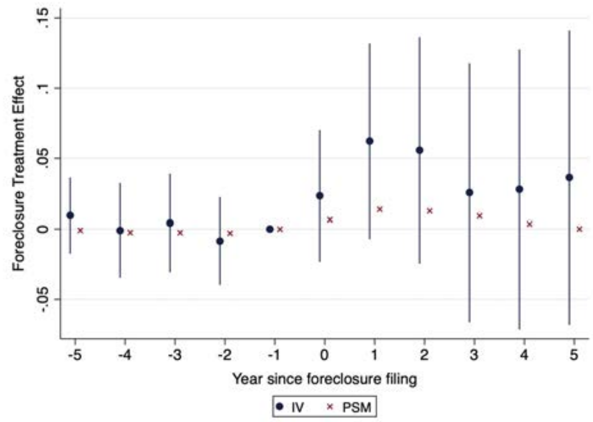
(a) Cumulative Number of Divorces



(b) Cumulative Number of Crimes Committed



(c) Cumulative DUI Convictions



(d) Cumulative Number of Bankruptcies

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all owners in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case. Because the filing of the foreclosure may cause negative outcomes, we measure outcomes relative to the period before the filing instead of the period of the filing itself. 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.

4.3 Personal Outcomes and Bankruptcy

We find evidence of elevated levels of personal trauma particularly for marginal households following a foreclosure by linking our results to RIS data on divorce, crime, DUI convictions, and bankruptcy, as shown in Figure 6. 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 6a shows that the judge IV reveals that for marginal households, foreclosure causes a 6.5 percentage point increase in the probability of divorce by years 3 and 4. This effect is significant at the 1% level and large: The mean default probability over five years in the non-foreclosure group is about 2.7 percent, so this is a more than two-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 (strictly two opposite-sex people living together one year before foreclosure filing), the year 3 and 4 IV estimate rises to 13 percentage points. Our IV findings on divorce echo those of Charles and Stephens (2004), who find evidence of elevated divorce hazards after job layoffs.

By contrast, we find a relatively small effect of foreclosure on divorce of 0.2 percentage points using PSM. We attribute this to treatment effect heterogeneity for marginal relative to average households, as we describe in Section 4.5.

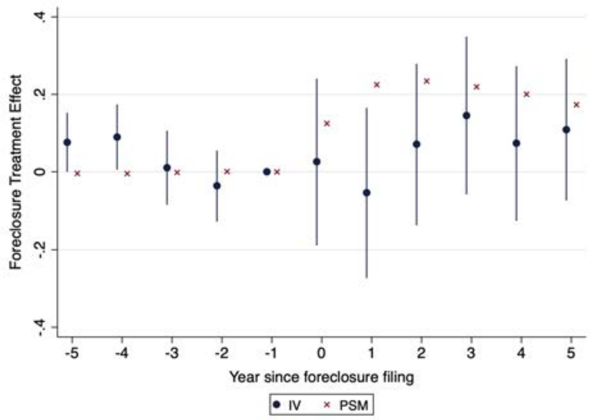
Panels 6b, 6c, and 6d show results for the cumulative number of drug, property, and violent crimes committed by the foreclosed-upon homeowner, the cumulative number of convictions for driving under the influence of alcohol, and the cumulative number of bankruptcy declarations. Crime and DUIs are insignificant or economically small for both IV and PSM. We find weak evidence of a small short-run increase in bankruptcy that mean reverts in the long run. In particular, IV finds that bankruptcy rises 6.2 percentage points for marginal households in year 1, which is significant at the 10% level, while PSM finds a rise of 1.3 percentage points in years 1 and 2 and is back to zero by year 5.

4.4 Financial Outcomes

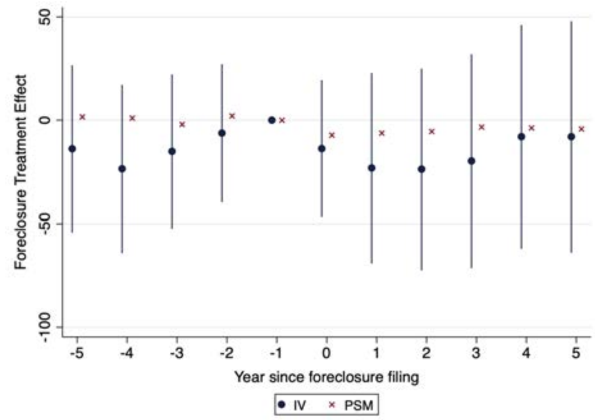
Figure 7 shows results for selected financial outcomes from our matched credit report sub-sample, with additional credit results summarized in Table 7.

Panel 7a shows that we are 24 percentage points more likely to observe an activated foreclosure flag in a homeowner's credit record by year 2 using PSM, but we do not observe a statistically significant effect in IV (although the confidence intervals are wide). The fact that we do not find a stronger effect on foreclosure may be surprising at first blush, but it turns out that foreclosure in the administrative judicial records and foreclosure in the credit report data are only loosely

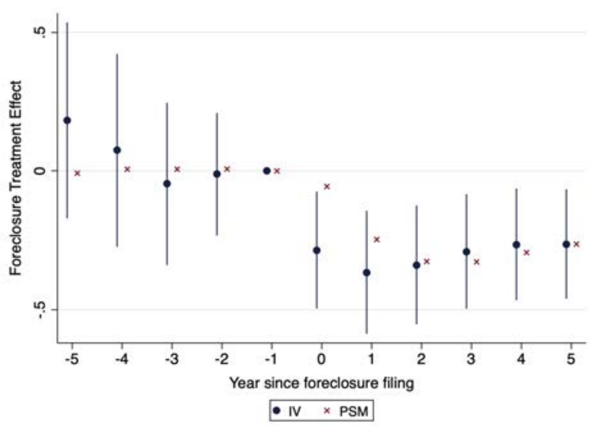
Figure 7: Owners: Financial Outcomes



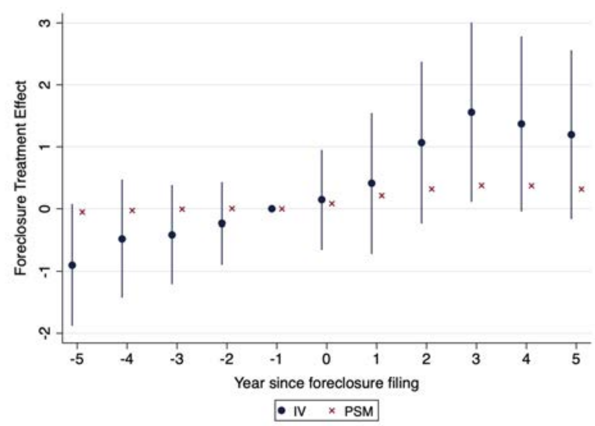
(a) Cumulative Number of Foreclosures



(b) VantageScore



(c) Number of Open Mortgages



(d) Number of Unpaid Collections

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all owners in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case.

correlated. This may be because while payments and debt amounts are automatically filled from lenders computer systems into credit reports, foreclosures require manual entry on the part of lenders and thus may have more error. This is why many papers that use credit report data to study foreclosure use delinquency measures rather than the foreclosure flag.

Foreclosure also does not have a huge effect on an owner's VantageScore, as shown in Panel 7b. The IV results are insignificant, while OLS shows a decline of 7 points in year zero and 3 points by year 3. It is important to note that these results do not imply that foreclosure does not affect one's VantageScore but instead implies that most of the effect comes from delinquency and the bank's foreclosure filing rather than the judge's decision to foreclose. Consequently, the negative effect on VantageScores is not differential for our treatment group relative to the control group, which also was delinquent and experienced a foreclosure filing.

The fact that credit scores do not significantly respond to foreclosure means that if loss of access to credit is driving some of our results, it is due to limitations on borrowing related to flags on the household's credit report rather than the credit score. Our results also stand in contrast to Humphries et al. (2019), who find using a similar methods that evictions of low-income renters have negative effects on credit scores.

Panel 7c shows a marked decline in the number of open mortgage loans for owners. One can see a significant negative treatment effect of about -0.3 by years 3 and 4 in both PSM and IV, which, reassuringly, has a magnitude in line with the positive treatment effect on moving in Panel 4a and the negative effect on homeownership in Panel 4c. The number of mortgages past due similarly falls using PSM, but the IV results are imprecise.

Our most intriguing finding from the credit report data is that foreclosure causes additional financial distress. Panel 7d shows the number of unpaid collections. Collections show up on a credit report due to failure to pay unsecured debt and thus do not include mortgage debt.[23] We find that foreclosure causes the number of unpaid collections to rise, peaking at over 1.5 (significant at 5% level) three years after the foreclosure filing for IV and 0.37 unpaid loans for PSM. The number of unpaid collections falls slightly for both specifications beyond 3 years but is still economically large at 5 years post-foreclosure. These results echo findings by Humphries et al. (2019), who find that eviction causes an increase in delinquency on other debts for low-income renters. The fact that an exogenous foreclosure filing causes additional delinquency for homeowners shows that rather than stabilizing a household's finances, foreclosure causes additional financial instability.

Columns 1 and 2 of Table 7 show a number of other outcomes in the credit report data. We observe an economically small decline in auto loans using PSM. We also observe a decline in mortgages with a loan modification of 0.10 by years 3-4 with PSM, which confirms our finding in Section 3 that many households that experience a foreclosure filing but not a foreclosure receive a loan modification. Finally, we find a 0.8% increase in death using PSM. All of these results are imprecise with IV.

[23] Collections can include credit card debt, student loans, auto loans, utilities, services, government debts, and medical debts. See https://www.experian.com/blogs/ask-experian/credit-education/report-basics/how-and-when-collections-are-removed-from-a-credit-report/ for details.

4.5 IV vs. PSM

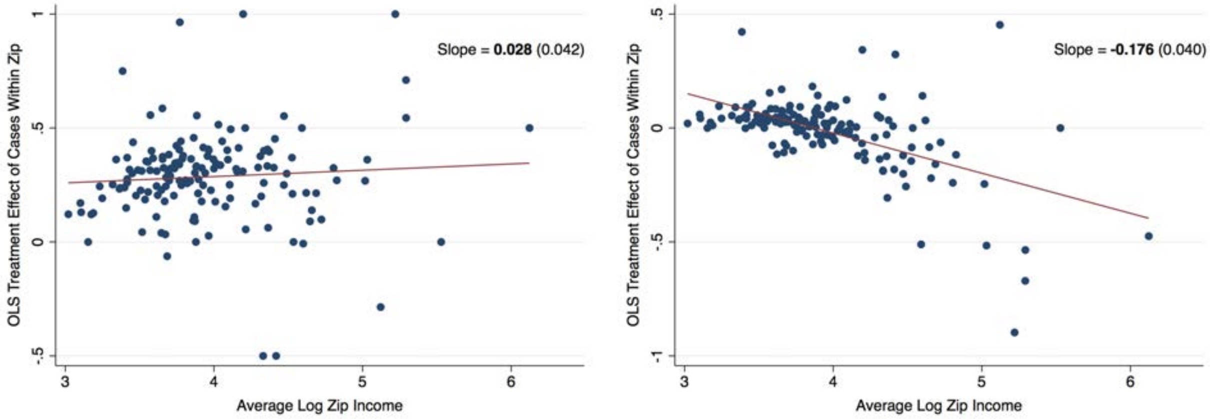
For some results, IV and PSM are consistent and reinforce each other, while for others, IV and PSM differ. In this section, we provide evidence that the inconsistencies are due to treatment effect heterogeneity: Marginal households have more to lose for these outcomes than average households. This makes sense, as compliers with our treatment have to go to court and challenge the ruling, and those with more to lose should all else equal be more likely to take on the costs of fighting the foreclosure in court.

In particular, we show that for outcomes for which IV and PSM are inconsistent, there is significant heterogeneity according to observables in OLS to the point that we can find subgroups for whom OLS is consistent with IV for the population. We use OLS rather than PSM for the heterogeneity analysis to preserve power when looking at smaller sub-samples. The fact that we find that IV is consistent with OLS when we constrain OLS to focus on subgroups who have more to lose suggests that IV and PSM differ due to the fact that IV picks up a treatment effect for marginal households and PSM for average households rather than due to biases in either approach.

To make this more concrete, we first focus on two outcomes that differ with respect to the discrepancy between IV and PSM. Recall that IV and PSM match closely for moving from the foreclosure address but IV finds economically and statistically significant negative results for ZIP income while PSM finds an economically-insignificant positive effect. Our interpretation is that for moving, IV and PSM are similar because the lender repossesses the house and forces the homeowner out with the same frequency regardless of how much a homeowner has to lose; indeed, this is the lender's decision. By contrast, for ZIP income, households with a lot to lose tend to be in nicer neighborhoods and have a long way to fall on the neighborhood quality later. The average foreclosure, however, is in a worse neighborhood and does not have far to fall on the neighborhood quality ladder – and in fact may actually improve neighborhoods after moving out due to regression to the mean.

Figure 8 provides some initial evidence consistent with this interpretation. Each panel shows a scatter-plot of OLS treatment effects of foreclosure on log average ZIP code income 3 years after foreclosure filing ($s = 3$) estimated separately for each ZIP code with over 200 foreclosure filings in Cook County against each ZIP code's log average income. Panel 8a shows that while there is some heterogeneity in the OLS effect of moving across ZIP codes, there is no systematic heterogeneity according to ZIP income. By contrast, Panel 8b shows that high income ZIP codes have significant

Figure 8: Evidence of Treatment Effect Heterogeneity in OLS



(a) OLS Heterogeneity by ZIP For Moved

(b) OLS Heterogeneity by ZIP For ZIP Income

Notes: Each panel shows scatters of heterogeneity in the OLS treatment effect when (1) is estimated separately for each ZIP code with over 200 foreclosure filings. The y-axis shows the OLS coefficient estimated using equation (1) for log ZIP income three years post foreclosure. The x-axis shows log ZIP income calculated as a weighted average of log ZIP incomes in each year weighted by the number of foreclosures each year. The red line shows a linear best fit.

negative treatment effects, while lower income ZIP codes have slightly positive treatment effects. This is consistent with a neighborhood quality ladder and implies that our IV compliers have ”more to lose” for this outcome in the sense that they tend to be in better neighborhoods. This finding echoes our analysis of complier characteristics in Section 3.2.3.

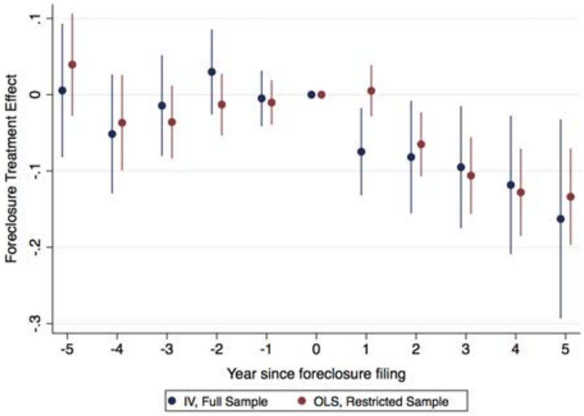
To elaborate on these findings, Figures 9 shows that for four of the main outcomes for which IV and PSM disagree, there are sub-samples based on observables for which OLS is consistent with our full-sample IV results.[24] This implies that treatment effect heterogeneity can explain the difference between IV and PSM.

Panel 9a shows results when Log ZIP Code Average Income – which is one of our starkest results and our preferred measure of neighborhood quality – is the outcome variable. In this case, the OLS restricted sample limits to owners in the top 5% of ZIP codes by average income and the top 25% of mortgage sizes of foreclosure cases county-wide. One can see that the OLS and IV results are quite consistent in magnitude, which is evidence of significant treatment effect heterogeneity in line with the neighborhood ladder interpretation.

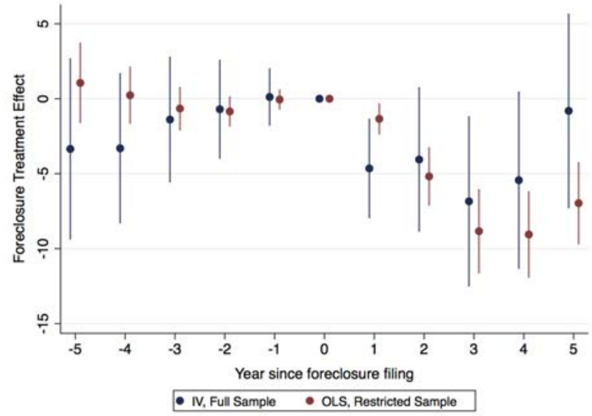
Similarly, Panel 9b shows similar results for middle school test scores. In this case, the OLS sub-sample conditions on areas with high test scores and high incomes. Again we see similar magnitudes to our IV results using OLS for households with the most to lose in terms of school

[24] Appendix C.2 shows that this is not due to OLS differing from PSM.

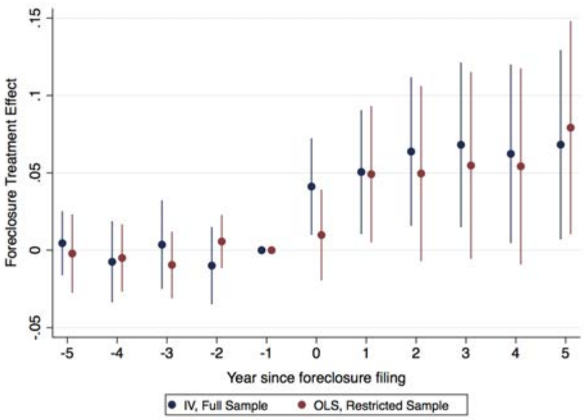
Figure 9: Owners: OLS Subsample



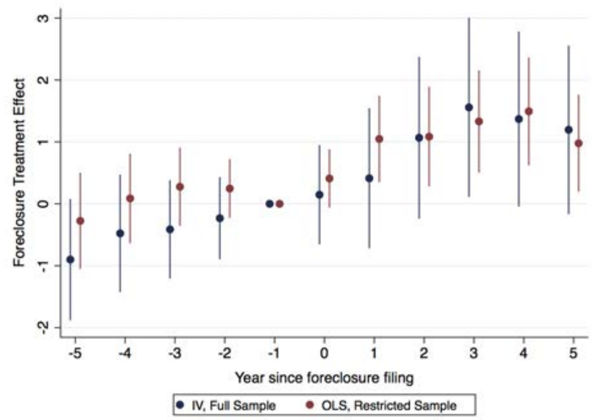
(a) Log ZIP Code Average Income



(b) Middle School Test Score Rank



(c) Cumulative Number of Divorces



(d) Number of Unpaid Collections

Notes: These figures display the treatment effects of foreclosure using full IV sample (blue) and restricted OLS subsample (red). ZIP average log income in (a), middle school test score rank in (b), cumulative number of divorces in (c) and number of unpaid collections in (d) are chosen because they exhibit large differences or have inconsistent signs in IV and OLS estimates, as shown in Figures 5a, 5c, 6a, and 7d, respectively. In (a), we condition cases on owners living in expensive neighborhoods, defined as belonging to the top-20th income ventile, and owning a high-value property, defined as belonging to the top-4th mortgage size quartile. In (b), we restrict to cases featuring owners who live in top-20th middle school score rank ventile and top-10th ZIP income decile neighborhoods one year prior to foreclosure filing. In (c), we focus on cases with strictly two opposite-sex owners owning/living at the same property one year prior to foreclosure filing and also condition on the top-5th income quintile and the top-4th mortgage size quartile. In (d), we only include owners who own primary residence, have a VantageScore lower than the median of 554, and belong to the bottom-1st mortgage size quartile and the bottom-1st ZIP income ventile one year before foreclosure filing. Of 248,494 case-owners in the full sample, the restrictions leave 7,321 observations in (a); 2,681 observations in (b); 2,633 observations in (c); and 1,874 observations in (d). Because these subsamples are quite small, we use OLS instead of PSM.

quality.

Panel 9c shows results when divorce is the outcome variable. In this case, the OLS restricted sample is limited to couples with large mortgages living in expensive ZIP codes. For this group, OLS provides a clean event study with a divorce effect as large as IV.[25]

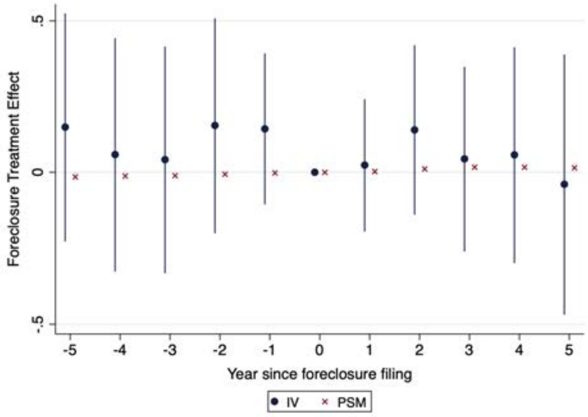
Finally, Panel 9d shows results for the number of unpaid collections, for which IV and PSM provide the same sign but different magnitudes for the full population. For financial distress, the OLS restricted sample corresponds to people in lower income areas with smaller mortgages. Again, for this group we see a clean event study with a magnitude as large as IV.

Our finding of significant treatment effect heterogeneity for these outcomes has important implications for the interpretation of our results. In particular, our results for marginal households are informative for policies that are similar to our judge instrument in that they affect a relatively small group of marginal homeowners who have to take costly action to be treated (recall 7% of our sample are compliers). By contrast, our results for average homeowners are more relevant for the average effect of a more sweeping foreclosure mitigation policy that does not require costly action to take up. This does not, however, mean our IV results are irrelevant for such a policy, as our findings imply there are marginal households who would have large non-pecuniary costs of foreclosure for things like neighborhood quality and divorce even if the average household does not. This heterogeneity is important to keep in mind when calculating the welfare benefits of broad-based policies, as a concave welfare function would imply a larger decline in welfare using the full distribution of outcomes instead of an average outcome.

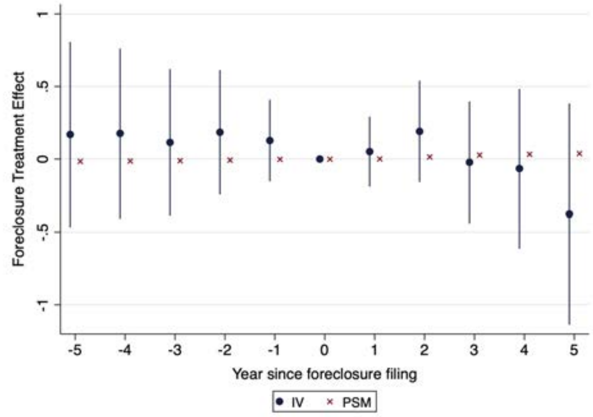
4.6 Summary of Results For Owners

Together, our results present a picture of foreclosure causing significant non-pecuniary costs for foreclosed-upon homeowner. Marginal and average homeowners move to less stable housing arrangements, are less likely to own a home, and have increased financial instability. Marginal homeowners also move to worse neighborhoods and have elevated divorce rates, but do not seem to downsize to a smaller home. The fact that owners prioritize the size of their home over their neighborhood is consistent with Bilal and Rossi-Hansberg (2020). Our point estimates are economically large and imply that foreclosure has far higher social costs than previously thought.

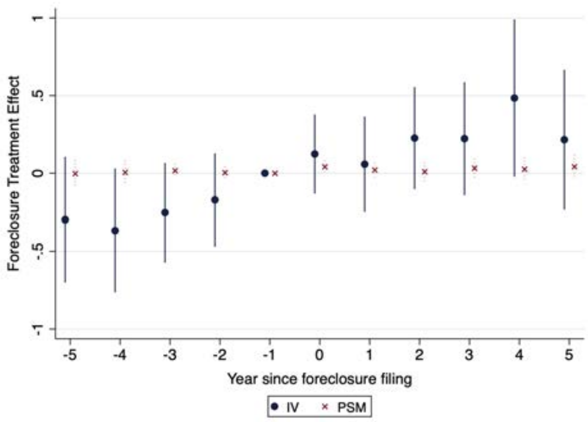
Figure 10: Landlords: Moving and House Characteristics



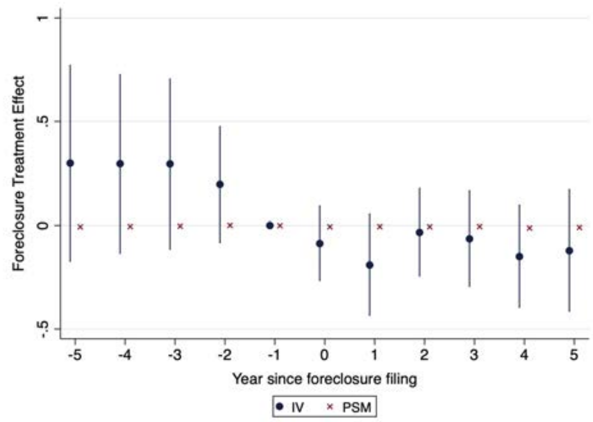
(a) Moved from Address At Time of Foreclosure



(b) Cumulative Number of Moves



(c) Owns Primary Residence



(d) Log Square Footage of Residence

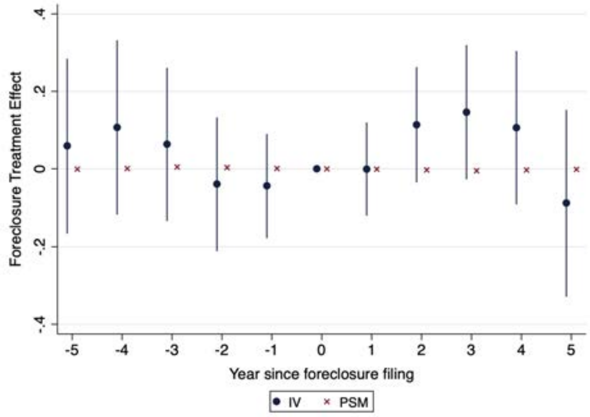
Notes: Each panel shows IV and PSM results for the indicated outcome variable for all landlords in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case.

5 Results: Landlords

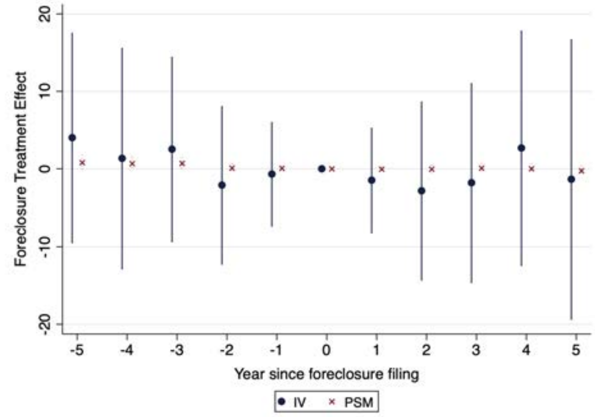
Figures 10, 11, 12, and 13 repeat the same analysis conducted in Figures 4, 5 and 6, and 7 for landlords. Similarly, columns 3 and 4 of Table 7 show pooled results for 3 and 4 years after foreclosure for landlords. For the landlord sample, the IV results tend to be imprecise, so we rely more on PSM results. We find landlords also experience similar negative financial outcomes to owners but

[25]One concern is that our divorce effects for marginal owners reflects strategic legal motives to divorce and not actual separations. We discuss Illinois divorce law as it relates to foreclosure in Appendix E.1. Based on our analysis, this explanation appears unlikely.

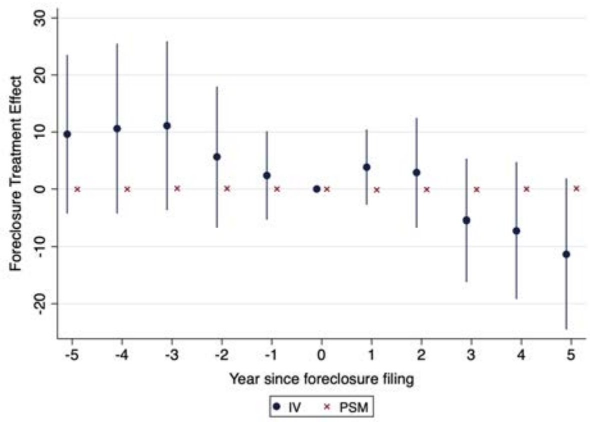
Figure 11: Landlords: Neighborhood Characteristics



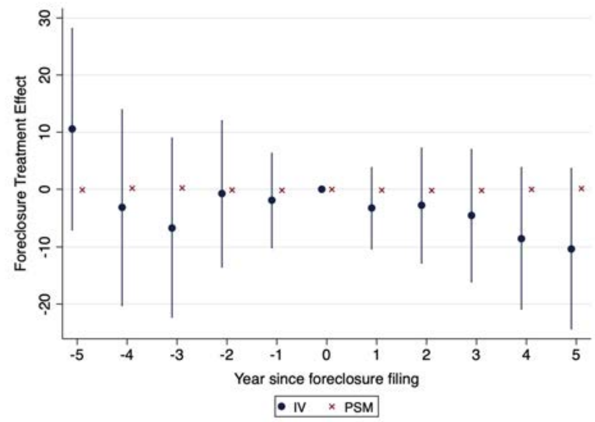
(a) Log ZIP Code Average Income



(b) Elementary School Test Score Rank



(c) Middle School Test Score Rank



(d) High School Test Score Rank

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all landlords in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. 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.

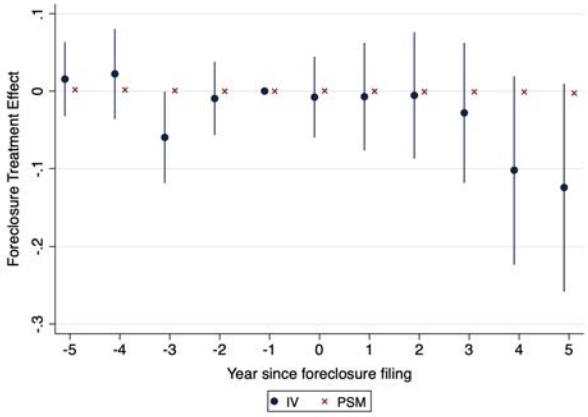
not similar negative non-financial outcomes.

5.1 Non-Financial Outcomes

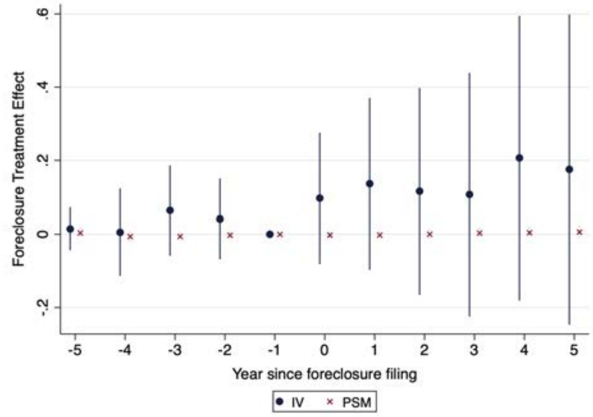
We find very small and largely economically insignificant effects on non-financial outcomes for landlords.

Figure 10 shows that landlords do not move, have multiple moves, change their ownership

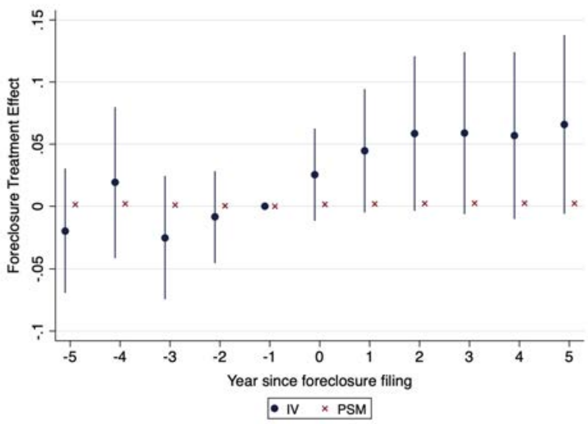
Figure 12: Landlords: Personal Outcomes



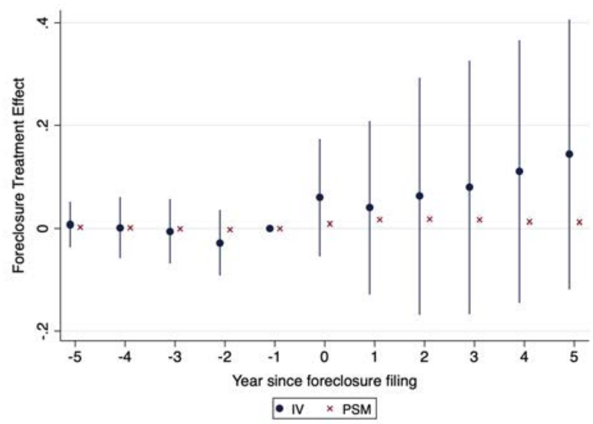
(a) Cumulative Number of Divorces



(b) Cumulative Number of Crimes Committed



(c) Cumulative DUI Convictions



(d) Cumulative Number of Bankruptcies

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all landlords in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. 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.

status, or reduce the size of their residence. Table 7 shows the PSM effect on moving is an economically-small 1.7 percentage points, and the cumulative number of moves is 0.031. While IV does find a borderline-significant positive effect on owning one's house for a few years, the standard errors are wide. As with owners, we see almost nothing on square footage of living space.

Given that we do not see a meaningful effect on moving, it is unsurprising that we see no significant effects on neighborhood quality using PSM as shown in Figure 11. IV occasionally finds a borderline significant effect, but the standard errors are wide.

Panel 12 shows results for personal outcomes. PSM shows relatively little, while IV shows a borderline significant decline in divorce in years 4 and 5 and a 5.8 percentage point increase in DUI convictions that is significant at the 10 percent level. We also see an increase in the cumulative number of bankruptcies of 1.5 percentage points for PSM, which is larger than for owners but still economically small. IV finds a larger bankruptcy effect of nearly 10 percentage points but is statistically insignificant.

5.2 Financial Outcomes

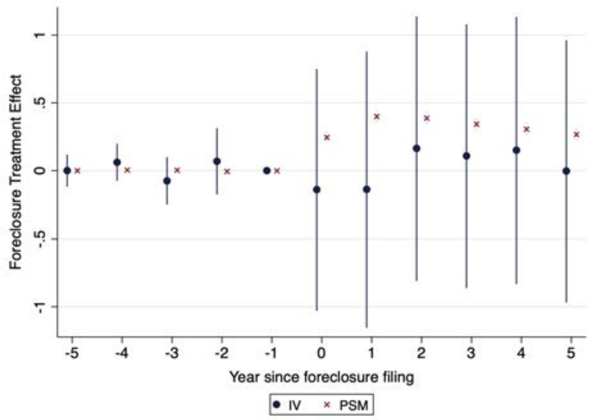
Figure 13 shows financial outcomes for landlords. We can see from the figure that their financial situation is akin to that of owners. In particular, in Panel 13a, PSM reveals an increase in the probability of a foreclosure flag on one's credit report of about 40 percentage points by year 2, which is a bigger effect than we observe for owners. Again, there is a relatively small decline in VantageScores, which fall by 6 points in year 0, are back to insignificant by year 2, and rise relative to year -1 by 3 points in years 3 and 4.

Panel 13c shows that the number of open mortgages falls by 45 percentage points in year two using PSM. This is more than for owners, for which the PSM effect peaks at 33 percentage points. Because it is unlikely that the foreclosure flag is turned on more frequently for landlords, this may reflect that landlords default on additional mortgages as a result of the foreclosure. Panel 13d shows that the number of unpaid collections rises by 0.27 by years 3 and 4 for landlords using PSM. As with owners, this is clear evidence of spillovers from the foreclosure to delinquency on other non-secured loans. This effect is slightly smaller than for owners, who have a PSM effect of 0.38 by years 3 and 4. Together, these results show that landlords experience significant financial distress.

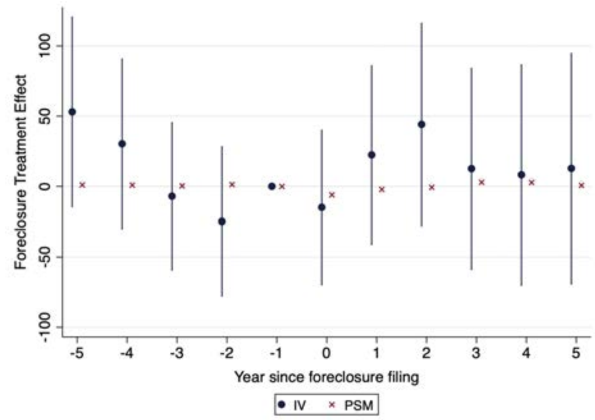
5.3 Summary of Results for Landlords

Overall, our results for landlords suggest far more benign effects of foreclosure than we observe for owners with the exception of financial outcomes, which are less severe for unpaid collections but more severe for the reduction in the number of mortgages and the small increase in the number of bankruptcies. For non-financial outcomes, we do see small effects and a potential increase in DUIs and decrease in divorce.

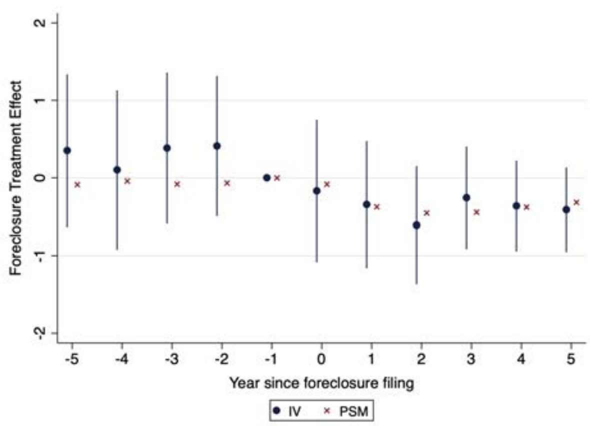
Figure 13: Landlords: Financial Outcomes



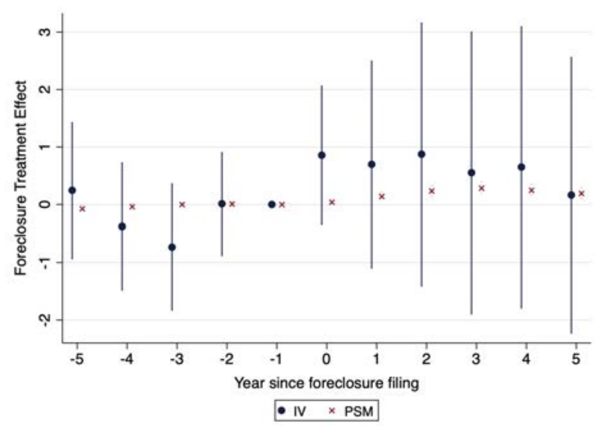
(a) Cumulative Number of Foreclosures



(b) VantageScore



(c) Number of Open Mortgages

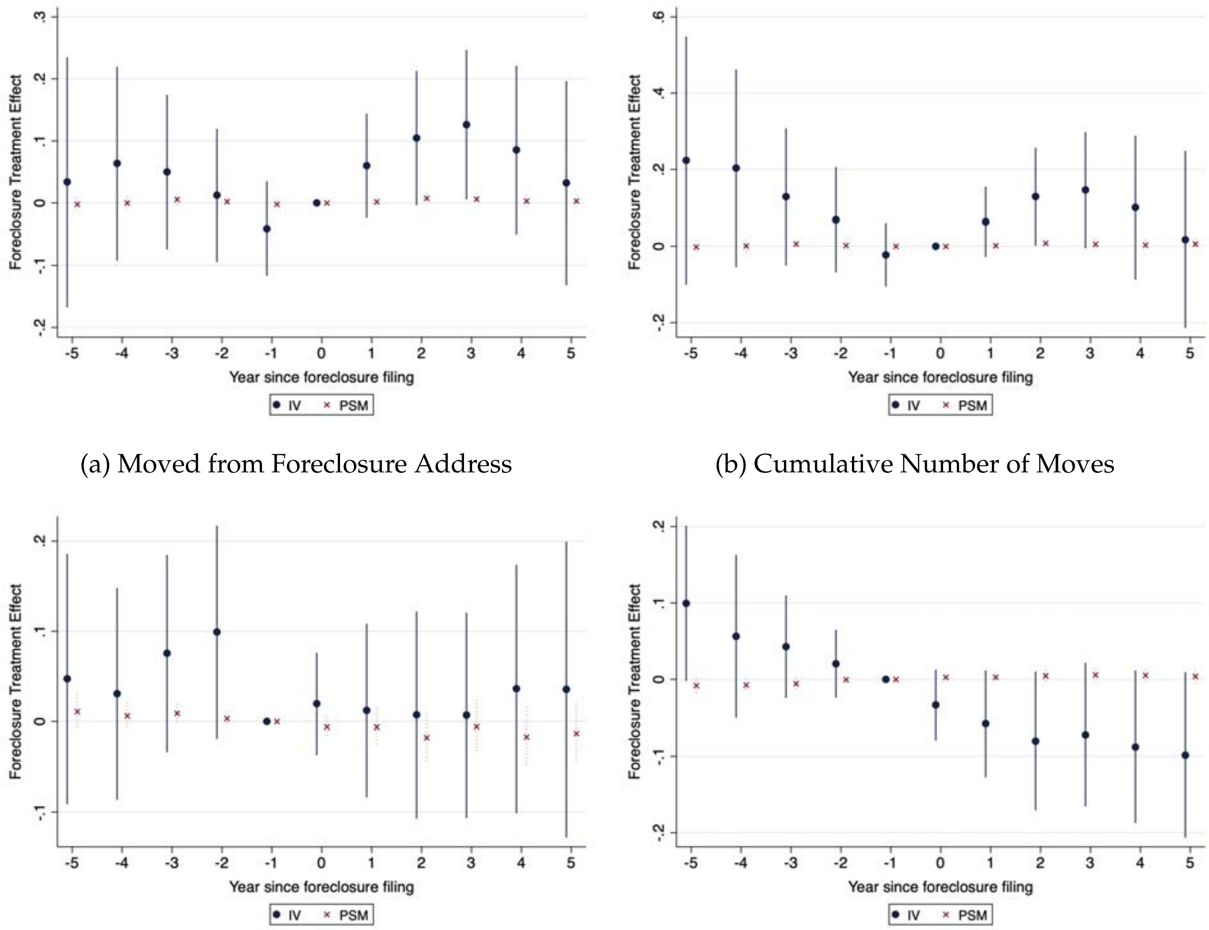


(d) Number of Unpaid Collections

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all landlords in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse of the number of people in each case.

Both owners and landlords experience a financial loss of assets and potentially a decline in credit access, although any decline in credit access must be mediated through something other than credit scores. The fact that both of these groups experience financial distress suggests that these outcomes are linked to the financial loss. However, the fact that the other adverse outcomes we observe for owners are not borne by landlords implies that these non-financial outcomes are likely not due solely to the financial loss. To assess whether they may be caused by the owner's eviction, we next turn to analyze the renters whose landlord if foreclosed upon.

Figure 14: Renters: Moving and House Characteristics



(a) Moved from Foreclosure Address

(b) Cumulative Number of Moves

(c) Owns Primary Residence

(d) Log Square Footage of Residence

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all renters in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case. Given the potentially-large number of units in each building, regressions are person weighted.

6 Results: Renters

Figures 14, 15, 16, and 17 repeat the same analysis conducted in Figures 4, 5, 6, and 7 for renters. Similarly, columns 5 and 6 of Table 7 show pooled results for 3 and 4 years after foreclosure for renters. Because our renter sample is small, we have wider standard errors and less statistical precision for IV and rely largely on PSM as we do for landlords. While we still find a few negative effects for renters, the effects are far smaller and less economically significant than for foreclosed-upon homeowners or landlords.

Panel 14a shows the causal effect of a landlord being foreclosed upon on the probability a renter leaves the foreclosure address. We find a significant effect on moving in IV but not PSM. For IV, moving rises to 12.6 percentage points over 3 years and is significant at the 5% level, while PSM finds no significant effect.

The IV results conform with media reports 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.[26] The effect mean reverts fully by year five, 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.

We do not find a positive effect on moving for PSM. This could be the case for two reasons. First, it could be that lenders are more likely to evict tenants when they repossess a unit in marginal cases. Second, it may be that landlords are more likely not to evict tenants if they avoid foreclosure in marginal cases. In Appendix D.1, we show that white landlords with black tenants closely match IV. The fact that landlord race matters suggests that it is likely that landlords are less likely to evict if they avoid foreclosure in marginal cases because landlord characteristics should not matter once the lender repossesses the property.

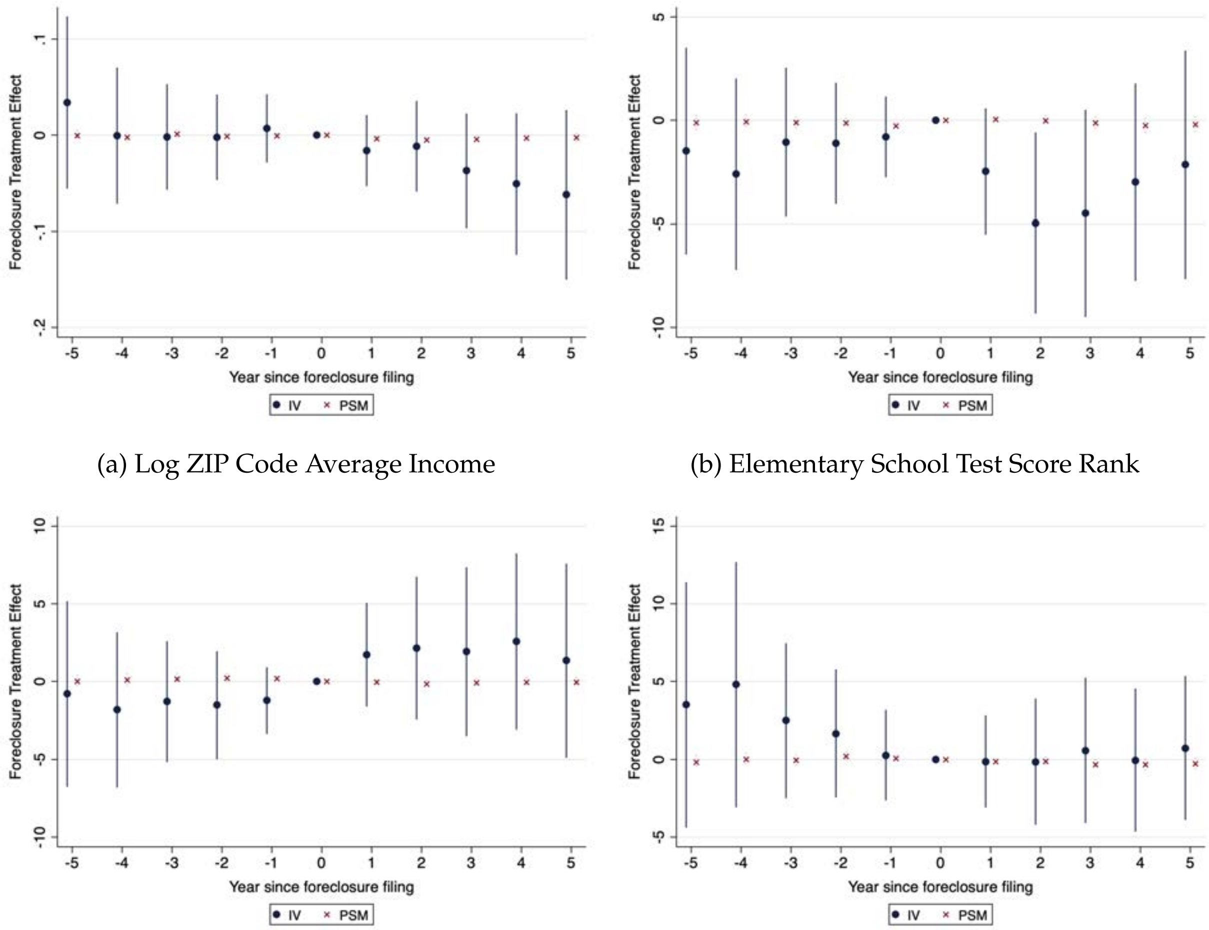
However, for renters, any additional moves due to the foreclosure do not seem to lead to the same degree of housing instability as for owners. Panel 14b shows that the IV and PSM treatment effects on the cumulative number of moves is not significantly larger than the respective estimates for moving in Panel 14a, 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 of one's residence for renters. Panel 14c shows that the probability that a renter owns is economically and statistically insignificant. Square footage falls by about 8 percentage points in IV but the standard errors are very large, while square footage rises slightly in PSM (Panel 14d). Given the limited statistical precision, we do not put much stock in these findings.

Figure 15 shows results for neighborhood quality. PSM detects a temporary decline in log ZIP income, but it is only half a percentage point in magnitude. IV detects a larger decline that expands over time, but the standard errors are wide. Panels 15b, 15c and 15d show that there is a significant negative effect on elementary school test scores for IV, which fall by five percentiles

[26] We do not have power to examine the causal effect before and after the passage of expanded tenant protections.

Figure 15: Renters: Neighborhood Characteristics



(a) Log ZIP Code Average Income

(b) Elementary School Test Score Rank

(c) Middle School Test Score Rank

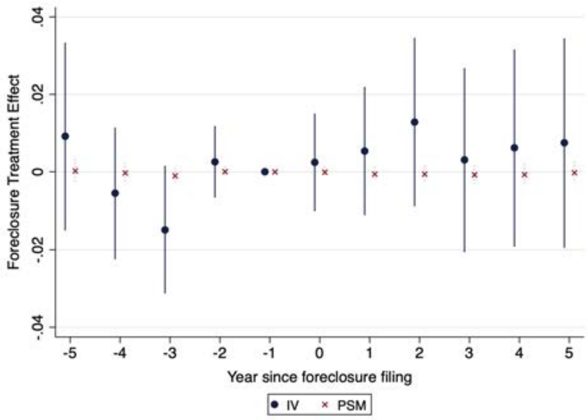
(d) High School Test Score Rank

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all renters in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case. Given the potentially-large number of units in each building, regressions are person weighted. 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.

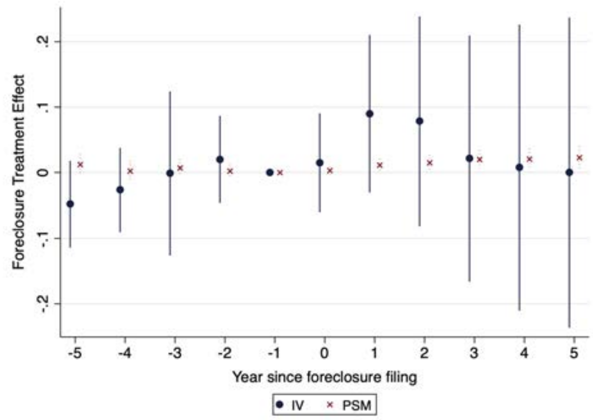
by year 2 but mean revert by year five. We do not find a negative effect on middle school or high school test scores. We do not observe a similar effect for PSM. PSM does detect a decline in high school test scores, but it is economically small. Overall, there is no convincing evidence of declines in neighborhood quality.

Figure 16 shows results for personal outcomes for renters. We do not see the marked and significant increase in divorce we observed using IV for owners. We do not see anything for

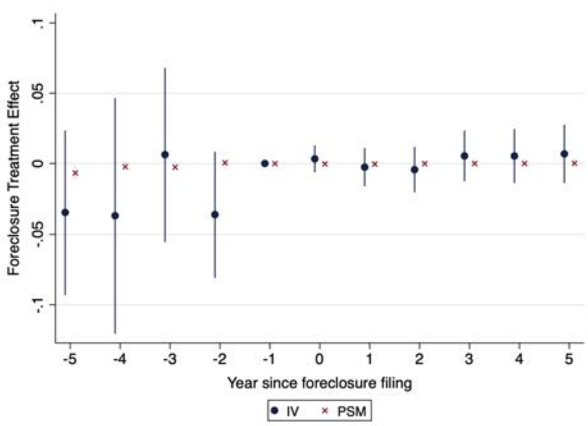
Figure 16: Renters: Personal Outcomes



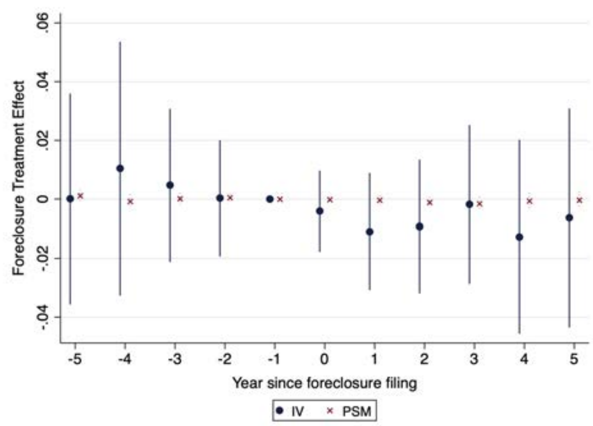
(a) Cumulative Number of Divorces



(b) Cumulative Number of Crimes Committed



(c) Cumulative DUI Convictions



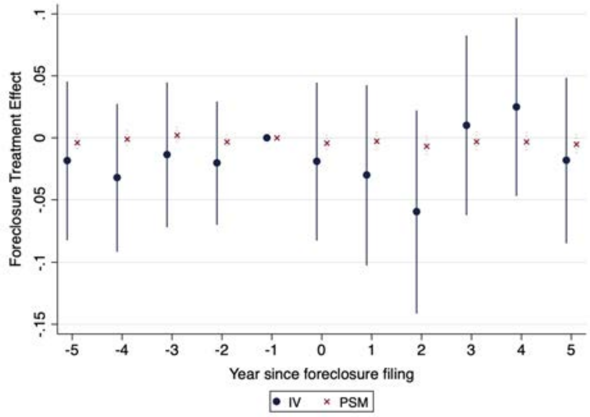
(d) Cumulative Number of Bankruptcies

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all renters in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case. Given the potentially-large number of units in each building, regressions are person weighted. 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.

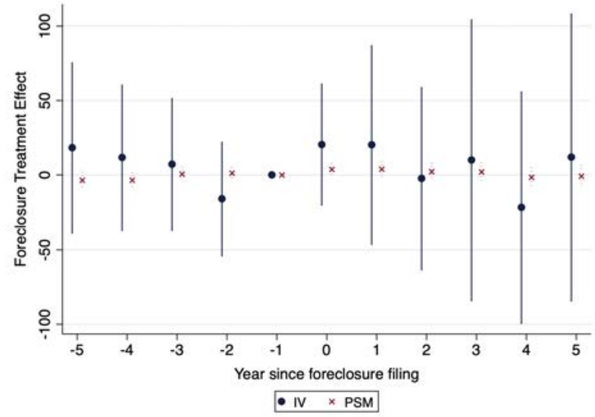
bankruptcy or DUIs. The one thing we do see using PSM is an economically small rise in the cumulative number of crimes committed of about 0.03.

Finally, Figure 17 shows results for financial outcomes for renters. We do not find significant effects on having a foreclosure flag on one's credit record, VantageScore, or mortgage borrowing, although the effect on the number of open mortgages is negative and significant for IV only in year 5. We do observe an upward trend in unpaid collections for PSM, but we do not put much stock in this result because it is the only PSM result with a pre-trend. Furthermore, using the basic

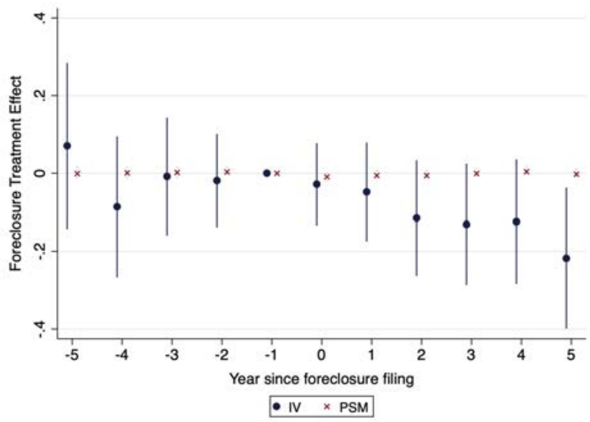
Figure 17: Renters: Financial Outcomes



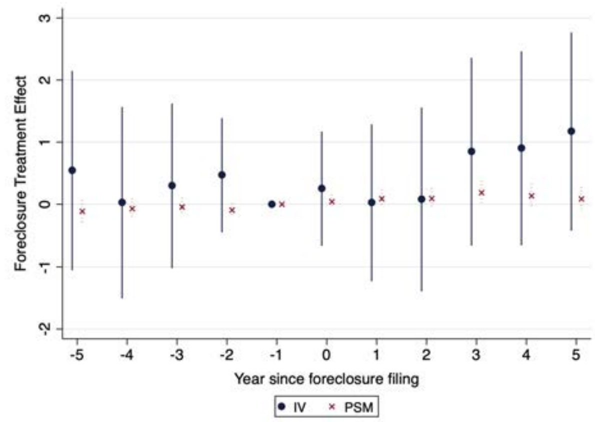
(a) Cumulative Number of Foreclosures



(b) VantageScore



(c) Number of Open Mortgages



(d) Number of Unpaid Collections

Notes: Each panel shows IV and PSM results for the indicated outcome variable for all renters in our sample. Each dot indicates the IV point estimate for β_s estimated using equations (2) and (3) and the bars indicate 95% confidence intervals. Each x indicates the PSM estimate for β_s estimated using equation (4). PSM confidence intervals are small enough that they are not shown. Standard errors are clustered by case. Given the potentially-large number of units in each building, regressions are person weighted.

OLS model in equation (1), we find a precise zero effect, which is not the case for any other PSM results. The rise in unpaid collections in IV is statistically insignificant.

Overall, our results for renters whose landlord is foreclosed upon suggests that their eviction has at best limited adverse effects. Because PSM does not detect an effect on moving and IV, which does detect a short-lived effect, has wide confidence intervals, we see our evidence as mostly suggestive and not as clear-cut as it is for owners and renters. However, we can reject the large non-pecuniary costs we observe for owners. Since both renters and owners experience evictions to some degree, this provides some evidence that the large negative effects we observe for owners are not due solely to eviction.

The limited causal effects of renter evictions triggered by foreclosure are in line with the somewhat modest effects found by Humphries et al. (2019) and Collinson and Reed (2019) in their analysis of the causal effects of renter eviction which also use random judge assignment in eviction cases for low-income renters. These papers, however find bigger effects on credit access and credit scores, unpaid debt, homelessness, and health outcomes than we do. One reason for the discrepancy may be that they study evictions due to renter delinquency rather than evictions due to delinquency by one's landlord, which is far less correlated with the renter's financial situation.

7 Conclusion

Using rich new data and two empirical approaches, IV with random judge assignment to foreclosure cases and PSM, we find significant negative causal effects of foreclosure for foreclosed-upon homeowners. For average homeowners, we find evidence of increased moving, housing instability, reduced homeownership, and financial distress. For marginal homeowners, we observe these effects in addition to large declines in neighborhood quality and elevated levels of divorce. We show that heterogeneous treatment effects can explain the discrepancies between PSM and IV; PSM measures effects for average households while IV measures effects for marginal households, who have more to lose. We also examine outcomes for landlords and renters whose landlord is foreclosed upon. We find evidence of financial distress for landlords but not the negative non-financial outcomes we observe for owners. We find few negative outcomes for renters, but our empirical designs have less power.

Our results suggest that conventional estimates that focus purely on financial costs significantly understate the social cost of foreclosure for foreclosed-upon owners, particularly for marginal cases. 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 work.

The differences between our results for owners, landlords, and tenants also shed light on the mechanisms underlying foreclosure. In particular, the fact that we find similar financial distress for owners and landlords suggests that the negative financial costs of foreclosure are due to the loss of assets and potentially credit loss – although the credit loss would have to be mediated by something other than credit scores. However, we see non-financial costs for owners – and particularly marginal owners – that are not present for landlords or for renters. This implies that it is the *combination* of eviction and a financial shock – rather than either on its own – that explains the large negative non-financial effects for owners.

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.

Our results have important implications for policy. First, our results imply that foreclosure mitigation can avert large non-pecuniary costs to society and that these costs are largely borne by foreclosed-upon homeowners. Second, our findings about treatment effect heterogeneity and the significant gaps between marginal and average households implies that different policies may have different implications. In particular, policies that do not affect as many people may still help mitigate the largest welfare losses from foreclosure because marginal households that are more likely to take up foreclosure mitigation policies have more to lose. More broad-based policies, on the other hand, may have smaller average treatment effects, but may still improve aggregate welfare significantly due to treatment effect heterogeneity. An important path for future research is to study the long-term impact of various foreclosure mitigation policies on owners, renters, and landlords.

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