The Effect of Foreclosures on Homeowners, Tenants, and Landlords

Rebecca Diamond[*], Adam Guren[†], and Rose Tan[‡§]

April 30, 2026

Abstract

Cost-benefit analyses of foreclosure typically ignore non-pecuniary costs to borrowers. Using random judge assignment for all foreclosure filings in Cook County, Illinois, from 2005 to 2012, we estimate the causal effects of foreclosure over 5-12 years for owners, renters, and landlords. Our IV addresses endogeneity arising from borrowers with greater private costs fighting foreclosure more aggressively. For owner-occupants, foreclosure causes reduced homeownership over a decade, financial fragility, moves to worse neighborhoods, and increased divorce. Landlords experience shorter-lived financial costs, but no non-financial impacts. Renters experience declines in neighborhood income. Our findings imply that foreclosure is far costlier than previously thought.

[*]Harvard University and NBER

[†]Boston University and NBER

[‡]Stanford University

[§]rdiamond@fas.harvard.edu, guren@bu.edu, rose2@alumni.stanford.edu. Zheyu Ni, Shizhe Zhong, Abhisit Jiranaphawiboon, Sara Johns, Analysia Watley, Max Grotz, and Gustav Yang, in particular, provided outstanding research assistance. Stijn Van Nieuwerburgh, Raven Molloy, Matt Notowidigdo, and seminar participants at Wisconsin, Toronto, NYU, BU, Opportunity Insights, the UEA meetings, the ASSA meetings, the NBER Summer Institute CF and UE programs, and the Stanford Cities, Housing and Society Workshop provided useful comments and discussion. Diamond acknowledges support from the Stanford Graduate School of Business and Harvard University, the National Science Foundation (CAREER Grant #1848036), and the Sloan Foundation. Guren acknowledges support from the National Science Foundation (Grant #1623801). Tan acknowledges support from the National Science Foundation Graduate Research Fellowship (Grant #1656518). Any opinions, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation. The authors have obtained IRB approval from Stanford University to conduct this research.

1 Introduction

How costly is foreclosure, and for whom? This question is crucial for policymakers as even small changes in these costs can shift the calculus behind foreclosure mitigation efforts and broader housing market interventions.

From 2007 to 2017, over six million homes were lost to foreclosure (Piskorski and Seru, 2021). Yet, despite the scale of the foreclosure crisis, evidence on the costs of foreclosure remains limited. Existing estimates typically focus almost exclusively on financial losses — property damage and transaction costs borne by lenders, price externalities borne by neighbors,[1] and legal and moving costs borne by the foreclosed-upon household. For instance, a U.S. Department of Housing and Urban Development (2010) study from the depths of the Great Recession, which is still widely cited (Ganong and Noel, 2020), found that foreclosed households bear only one-fifth of total social costs. However, this narrow lens omits the upheaval in families' lives and finances that dominates public concern about foreclosures.

We ask whether foreclosure imposes significant non-pecuniary costs beyond these financial estimates using novel, comprehensive data on every foreclosure filing in Cook County, Illinois, from 2005 to 2012. We track the consequences of foreclosure over five years for moving, housing, neighborhood, divorce, and financial outcomes, and over 12 years for homeownership. Our data allow us to separately estimate the consequences of foreclosure on owner-occupants, landlords, and renters living in the foreclosed property. To identify causal effects, we exploit random assignment of foreclosure cases to judges of varying stringency, an approach that avoids selection bias, which plagues OLS: Households with more to lose from a foreclosure actively avoid foreclosure, which biases OLS estimates toward zero and explains why prior studies have not found significant impacts of foreclosure on neighborhood quality, family structure, or default on non-mortgage debt.

We find large and persistent non-financial costs. Homeowners who are foreclosed upon experience sharp and sustained declines in housing, neighborhood quality, and family stability: They are 20 percentage points more likely to move, experience a 45 percentage point decline in homeownership which remains depressed for over a decade, live in neighborhoods with 10 percent lower average income, worse schools, and worse long-term outcomes for children, and experience a 170 percent increase in divorce rates. They also face financial distress over 2 to 3 years,

[1] See Immergluck and Smith (2006), Campbell et al. (2011), Harding et al. (2009), Anenberg and Kung (2014), Gerardi et al. (2015), Gupta (2019), Mian et al. (2015), and Guren and McQuade (2020)

including elevated bankruptcy (20 percentage points), fewer mortgage loans, and increased delinquency that spills over into non-mortgage debts. In contrast, landlords experience similar but shorter-lived financial fallout but avoid the non-financial consequences — and, in some cases, benefit from shedding unproductive debt by moving to *higher*-quality neighborhoods. Renters whose landlords are foreclosed upon move to lower-income neighborhoods. Our results are uninformative due to low statistical precision for most other renter outcomes.

Our findings imply that the full cost of foreclosure has been significantly underestimated. They also help explain a puzzle in household finance and macro models: The observed low levels of strategic default. Structural models require implausibly large default penalties — often 25–30 percent of lifetime consumption — to rationalize low default rates (Bhutta et al., 2017; Ganong and Noel, 2023; Kaplan et al., 2020).[2] Our results suggest that these large default costs are not moral or psychological but rooted in real, measurable social and economic consequences.

We build on existing studies of the longer-term impact of foreclosure by improving both data and identification. First, we use a better and more policy-relevant measure of foreclosure: Court-ordered foreclosure *completions*, rather than foreclosure filings or delinquencies observed in credit report data. This distinction is crucial because fewer than half of Cook County foreclosure filings result in actual foreclosure. Our variation is also closer to actual foreclosure mitigation policies, which aim to prevent foreclosures among delinquent households. Our comprehensive dataset, which we describe in Section 2, combines administrative case records, foreclosure and property records, divorce records, address histories merged with neighborhood characteristics, detailed and high-frequency credit reports, and loan servicing data — providing a more complete view of foreclosure's consequences across dimensions that previous work could not examine.

Second, we address endogeneity, which we demonstrate to be significant in Section 3. We show that traditional OLS and propensity score matching (PSM) methods underestimate the negative impact of foreclosure due to selection bias: Households that suffer more from foreclosure fight harder to avoid it. This biases the effects of foreclosure toward zero, contradicting the traditional narrative that foreclosure coincides with unobserved financial shocks that affect the household's ability to pay, biasing the estimated impact of foreclosure upward. We provide three pieces of evidence for significant selection. First, landlords facing only financial losses are much

[2] Ganong and Noel (2023) find that only 3 percent of defaults are strategic and require a 25 percent lifetime consumption penalty to match this in their model. Laufer (2018) structurally estimates a default penalty of 29% of permanent income. Bhutta et al. (2017) show households on average wait until -74 percent equity before defaulting, while frictionless models predict default at -20 percent. Kaplan et al. (2020) target a pre-crisis default rate in their calibration and find an average consumption-equivalent loss of 30 percent in the default period.

more likely to be foreclosed upon than owner-occupants who are also facing eviction. Second, although foreclosed and non-foreclosed delinquent households appear similar one year before filing, high-frequency data that previous studies have not considered reveal diverging borrowing and delinquency patterns in the 9 months before a foreclosure filing. Third, we show that proxies for the costliness of foreclosure to borrowers predict foreclosure more strongly than measures of borrowers' ability to pay. Since these observable proxies capture only a fraction of the variance in foreclosure outcomes, selection on unobservables remains substantial enough that no identification strategy relying on selection on observables — including OLS and PSM — can account for it.

We address this bias using a judge-based instrumental variables (IV) design presented in Section 4, which exploits quasi-random case assignment by the Cook County Chancery Court.[3] In Illinois, judges approve all foreclosures and can influence borderline cases toward foreclosure or alternatives like loan modifications or payment plans. The Cook County Chancery Court randomly assigns cases to "calendars," which are each assigned to a principal judge. We measure judge stringency at the calendar level using leave-out means (Kling, 2006; Dobbie and Song, 2015; Kolesar, 2013; Bhuller et al., 2020; Dobbie et al., 2018). Our IV strategy infers the causal effect of foreclosure by comparing cases randomly assigned to strict versus lenient calendars. Our instrument meets standard validity checks, including balance on observables, no pre-trends, and tests for monotonicity (Frandsen et al., 2023).[4] However, our approach is data-intensive, requiring a long panel for the second-largest county in the country to obtain statistically significant results, which limits our ability to examine treatment effect heterogeneity.

Section 5 presents results for owner-occupants, whom we refer to as homeowners. Foreclosure increases the probability of moving by 20 percentage points, and these moves impose lasting costs. Homeowners become 45 percentage points less likely to own their residence in subsequent years, with suggestive evidence of multiple moves and transitions to smaller homes. We find homeownership is significantly lower, even 12 years after foreclosure, consistent with Artigue et al. (2025). Owners move to worse neighborhoods — 10% lower average income after 3 to 4 years, lower school test scores, worse children's long-run outcomes — and divorce rates nearly triple. Financial distress persists for 2 to 3 years: Bankruptcy rises 20 percentage points, fewer households

[3] Munroe and Wilse-Samson (2013) also use Cook County judge assignment to study price externalities on neighboring properties, but do not examine effects on homeowners, renters, and landlords.

[4] Our instrument does not predict sample inclusion, so dropping observations in the process of cleaning the data does not induce selection.

hold first mortgages, and defaults on home equity loans and non-mortgage debt (especially credit cards) increase. Credit scores show modest declines, driven primarily by defaults rather than foreclosures. Non-financial outcomes prove more persistent than financial effects, demonstrating that foreclosure imposes costs far beyond traditional financial measures.[5]

Section 6 reveals that landlords suffer significant but shorter-lived financial distress (measured by default on other loans) without the adverse non-financial impacts homeowners face. The financial effects are less persistent than for owners, suggesting financial hardship concentrated in the first year or two, with improvement by years 3 to 4. Divorce rates do not increase. While landlords are modestly more likely to move following investment property foreclosure, they move to *better* neighborhoods, suggesting they benefit from shedding overhanging debt. Since landlords experience a financial loss but not an eviction, comparing their outcomes to homeowners reveals that financial losses account for the adverse financial effects for owners but not for the negative non-financial effects.

Section 7 examines renters whose landlords are foreclosed upon. Renters frequently face eviction after foreclosure — prompting federal intervention in 2009 through the Protecting Tenants at Foreclosure Act.[6] We find that renters whose landlords are foreclosed upon do not increase their frequency of moves but move to neighborhoods with 9.6 percent lower income, a similar magnitude to what we found for owners. We do not see an increase in divorce, and we find a short-lived decline in homeownership that eventually turns positive after 5 years. Due to limited power, most other outcomes have wide enough confidence intervals that our results are inconclusive. Because we cannot observe whether a move is an eviction and because data quality is lower for renters, we do not want to over-interpret these results. However, the fact that we do not observe the complete set of adverse outcomes observed for owners, together with our findings about landlords, suggests that homeowners' non-financial losses stem partly from eviction and partly from the *combination* of eviction and financial shock, rather than either alone.

Our findings contrast sharply with prior literature using OLS with controls or PSM.[7] These studies find results consistent with ours for some outcomes, such as increased moving and decreased homeownership, but not for others, such as neighborhood quality, credit, and delin-

[5]Our findings strengthen the case for aggressive foreclosure mitigation, though reduced default deterrence must also be considered. See Agarwal et al. (2023), Agarwal et al. (2017), and Gabriel et al. (2021) for more complete analyses of foreclosure mitigation programs.

[6]The PTFA grants tenants written eviction notice rights and the ability to serve out remaining lease terms.

[7]Many of these studies use credit report data and cannot tell if the foreclosed-upon owner is an owner-occupant or landlord; the exception being Artigue et al. (2025), who focus only on owners.

quency. The contrast between our findings and the literature aligns with the bias we find in OLS. Our results are consistent with the literature for "mechanical" outcomes, such as having one fewer mortgage or not being a homeowner, where there is little heterogeneity in the amount households have to lose. By contrast, our results differ for non-mechanical outcomes, such as neighborhood quality, where households living in better neighborhoods may have more to gain by fighting a foreclosure. For instance, Molloy and Shan (2013) use PSM to show that foreclosure starts as observed in credit bureau data cause moves, but not to less desirable neighborhoods or more crowded living conditions. Artigue et al. (2025) use a differences-in-difference design to compare households that receive mortgage modifications with households that do not, using credit report data in which households are observed every three years. They find that households receiving loan modifications experience a dramatic increase in homeownership even a decade later, but there is no impact on creditworthiness or neighborhood quality. Brevoort and Cooper (2013) show that credit scores persistently decline after a foreclosure start. Piskorski and Seru (2021) show that only a quarter of foreclosed-upon homeowners from 2007 to 2017 eventually purchased a home, taking an average of four years to do so. De Giorgi and Naguib (2024) show that 90-day delinquency has significant impacts on credit, ownership, and income 10 years after a delinquency. Barca et al. (2022) use PSM to show that kids who experience a foreclosure between ages 10 and 17 exhibit signs of credit scarring as adults. Been et al. (2011) uses OLS to show that foreclosed families' children switch schools and move to worse schools. Been et al. (2025) find that households with negative equity have higher test score growth. Currie and Tekin (2015) show foreclosure causes increased unscheduled and preventable hospital visits using ZIP-level timing variation, and a related public health literature shows that foreclosure causes physical and mental health deterioration (Tsai, 2015; Houle, 2014; Pollack and Lynch, 2009; Bennett et al., 2009). To our knowledge, no prior research examines landlords or renters.

Our results reveal that these OLS and PSM studies underestimate the negative consequences of foreclosure for *non-mechanical outcomes*. Our evidence of selection bias in OLS versus IV supports the costliness story: Households with more to lose fight foreclosure more aggressively, biasing OLS toward zero. Crucially, this bias is directly observable only in high-frequency data — foreclosed households' observables diverge only about 9 months before the foreclosure filing. This creates significant bias for non-mechanical outcomes, where private default costs may be heterogeneous, such as neighborhood quality and divorce. In contrast, there is less bias for mechanical outcomes that are similar across households, such as moving after a foreclosure or being unable to purchase after a foreclosure due to rules limiting mortgage borrowing by foreclosed-upon households. These patterns explain why many papers find homeownership impacts but miss the broader non-pecuniary costs we uncover using our judge IV.

Overall, our results call for a broader accounting of foreclosure's social costs and justify interventions that prevent not only financial loss but also deep and lasting personal harm.

2 Data

We construct a unique data set on the universe of foreclosures in Cook County, Illinois, from 2005 to 2012, with most outcomes measured through 2016, and homeownership as proxied by the presence of a primary mortgage measured through 2024. This section describes the data, its construction, and institutional background, with details in Appendix A.

2.1 Institutional Background

Illinois is a judicial foreclosure state, meaning that lenders must file a court case to seize a home as collateral. The final authority for the foreclosure rests with an Illinois Circuit Court judge in the Chancery Division.[8] After a borrower is at least 90 days delinquent, a lender can file for foreclosure. The borrower is notified and can contest the foreclosure, cure (that is, restart paying and become current on their loan), or apply for a loan modification or mediation. If contested, the process can drag on for years, although most foreclosure cases are resolved relatively quickly. Once a judge approves a foreclosure, the property is sold at auction, and the owner can be evicted. Alternatively, the case could be dismissed either because of a settlement, typically a loan modification or repayment plan, or if the lender did not follow the legal procedures of foreclosure.

Judges have considerable discretion in affecting the outcome of foreclosure cases.[9] Most notably, judges can push for non-foreclosure resolutions such as a loan modification or payment plan, a short sale, or a deed in lieu of foreclosure. Discussions with lawyers representing parties in Cook County foreclosure cases reveal that judges vary significantly in how much they push parties to settle. However, we cannot see the details of any settlement in the court data. Judges can also dismiss a case for legal errors or failure to follow proper procedure. There was enough

[8] Illinois is also a recourse state, meaning that lenders can go after an individual's assets if the property's value is less than the amount owed. However, Nelson et al. (2014) report that judges in Cook County rarely grant personal deficiency judgments.

[9] For details on Illinois Foreclosure Law and ways in which defense attorneys can contest a foreclosure, see Nelson et al. (2014), on which our summary is based.

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.[10]

The process for evicting renters following a landlord's foreclosure is less automatic. Anecdotally, lenders often evict renters when they repossess a property. However, tenant protections enacted during the financial crisis made it challenging to do so before the end of an existing lease.

While it is hard to gauge how representative our analysis of Cook County is of foreclosure experiences nationwide, a few points are worth benchmarking. First, we find that just under half of foreclosure cases result in a foreclosure within three years of filing. This is roughly in line with a survey by HOPE NOW, which in 2014 and 2015 found a ratio of foreclosure completions to foreclosure starts around 50 percent.[11] Second, Illinois had a high foreclosure rate despite being a judicial state; CoreLogic estimates that its peak foreclosure rate was the fourth highest nationally.[12]

2.2 Data Sources

We combine six primary data sources. First, we utilize administrative case records from 2005 to 2016 scraped from the Cook County Clerk of the Circuit Court's website (Cook County Clerk of the Circuit Court, 2005-2016). These case records provide a case number and a dated history with the judge's name for all filings and judgments, the name and address of the defendant, 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 Appendix A. Our primary measure of foreclosure is an indicator for a foreclosure judgment within 3 years of the initial filing, which provides a clear outcome for all cases, including those not resolved by 2016. In practice, 91 percent of cases conclude within three years.

Cases are randomly assigned to a calendar. One judge primarily handles each calendar, although other judges occasionally step in. When a judge leaves, the entire calendar is typically transferred to a new judge.[13] We begin our analysis in 2005 when the Court says it started to randomly assign cases and conclude with cases filed in 2012, allowing us to observe at least 5 years

[10]In 2013, the Supreme Court adopted new state-wide rules with clearer guidelines. See http://www.illinoiscourts.gov/media/pressrel/2013/022213.pdf.

[11]See https://www.housingwire.com/articles/36455-hope-now-145-million-solutions-available-to-homeowners-in-2015/.

[12]See https://www.alta.org/file/CoreLogic-Foreclosure-Report.

[13]We use the judge who issued judgments at the time of case filing to infer the originally-assigned calendar, as detailed in Appendix C. Our calendar variable yields stable random assignment probabilities that match those in court documents.

of outcomes. Over these 8 years, there are 77 year-calendars with an average of 3,600 cases each. 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, and divorce records for Cook County from the early 2000s to 2017 from Record Information Services (RIS) (Record Information Services, 1998–2017), which digitizes and cleans public records data in Chicago. 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 numbers (APNs) for the foreclosed properties. The data also includes the names of all defendants, as well as property and mortgage characteristics. We use the APN to link the court foreclosure records to RIS data. We also use APN to link to deeds and assessor data from DataQuick and CoreLogic (DataQuick, 2012; CoreLogic, 2020b), which provides each property's transaction and mortgage history, including buyer and seller names and unit characteristics. We also use deed data (CoreLogic, 2020a) to obtain the address to which property tax bills are sent, which helps identify landlords. Finally, we use individual-level divorce records from RIS.

Third, we obtain address histories for any individual who resided in Cook County between 1990 and 2016 from Infutor (Infutor Data Solutions, LLC, 2016). The dataset provides the exact street address where they lived, regardless of whether it is in Cook County, the months and years the individual lived at that location, the individual's name, and demographic information, including age and gender. We remove duplicates as best we can, as described in Appendix A.2. Diamond et al. (2019) show the data is of high quality, particularly for owners. It is generally of lower quality for transient households, such as renters, which may mean we do not observe all moves, but this should be independent of our instrument. However, potential data quality issues make us cautious in evaluating the magnitudes of our findings for renters.

Fourth, we merge in granular "tradeline" credit-report data and individual-level VantageScore 4.0 credit scores from a credit bureau. The tradeline data provide detailed information on each debt account for each individual, allowing us to construct measures of borrowing and delinquency by debt type. This avoids aggregation issues encountered with the more typically used borrower-level statistics in credit report data, as discussed by Gibbs et al. (2025). We clean the data, separating account types into primary mortgages, home equity (secondary) mortgages, credit cards, student loans, and auto loans. We create several outcomes, including a default share, the number of newly opened loans, a dummy for having a loan of a given type, and a dummy for having ever defaulted on a loan type since five years before the foreclosure filing. Crucially, the default share measures the proportion of active accounts at any point in a given year that have experienced a default, as measured by a foreclosure, collection, repossession, or charge-off. We also create a bankruptcy flag. Our credit report data extends through 2024, allowing us to examine outcomes up to 12 years after foreclosure.

Fifth, we obtain ZIP code income from the IRS Statistics of Income Tax Stats (Internal Revenue Service, 1998-2016) and school test scores from the Illinois Board of Education (Illinois State Board of Education, 2000-2016, 2018). We also merge in intergenerational mobility measures for kids who grew up in a tract in adulthood, including intergenerational income mobility, teen birth rates, and incarceration rates from Opportunity Insights' Opportunity Atlas (Chetty et al., 2025).

Sixth, we merge in CoreLogic's Loan-Level Mortgage Analytics (LLMA) servicing data (CoreLogic, 2020c). This data provides a more detailed picture of payments on the primary mortgage, but it is only available for a smaller subset of our data. We consequently use it primarily to assess bias in OLS rather than in our primary IV analysis, for which sample size is paramount.

2.3 Construction of Analysis Sample

We drop non-foreclosure cases from the Cook County Court data and merge with the RIS data using case numbers. We drop cases with multiple parcels in a single foreclosure case because RIS may not report all the parcel APNs, affecting roughly 10 percent of cases. We then merge the DataQuick and CoreLogic deed records by APN, to produce a case-level dataset.

We next merge our case-level data with individual-level address histories from Infutor. We identify all individuals in Infutor who reside at an address within a month before the foreclosure filing, as well as landlords who own foreclosed properties. We construct annual outcomes for each case-person, starting five years before and ending five years after the initial foreclosure filing. We then merge the various individual-level and neighborhood-level outcomes described previously.

We next identify homeowners, renters, and landlords. We are purposefully cautious in defining individuals in each category and drop those whose category we cannot determine with high confidence. Owners are individuals whose last name matches that of the foreclosure defendant and who reside at the foreclosure address in the month before the foreclosure filing. Landlords are individuals who are not living at the foreclosure address in the month prior to the foreclosure filing, whose name matches the defendant's, and whose address matches the property tax mailing address. Renters are individuals who live at a foreclosure address in the month before the foreclosure filing and have an identified landlord. For renters only, we limit to the most recently

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	
Have Foreclosure Outcomes and Judge Stringency	239,782	
Keep if Match to Someone in Infutor	186,435	382,066
Full Final Sample	186,435	382,066
Owners	133,346	264,174
Renters	43,021	47,247
Landlords	53,089	70,645
Credit Report Subsample	180,296	332,065
Owners	128,057	232,518
Renters	38,622	34,483
Landlords	50,559	65,064

Notes: The top panel shows our sample size at each step in the data cleaning process. The bottom two panels display the sample sizes broken down by owners, renters, and landlords for the full final sample and the credit report subsample. Note that the full sample totals are the sum of owners and landlords for cases (some cases with landlords also have renters, some do not) and the sum of owners, renters, and landlords for case-people. For the credit report subsample, we keep renters whose landlord we can find but does not match to the credit report data, so the total number of cases is not equal to the sum of owner and landlord cases that match.

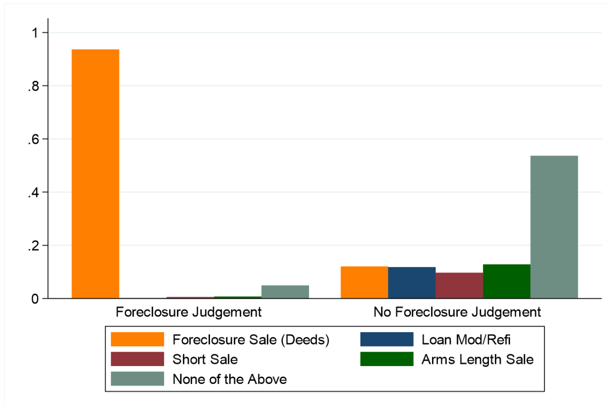
moved-in renter.[14]

We drop non-residential properties and cases that are not clearly classified as either a foreclosure or a dismissal. Table 1 summarizes how each data cleaning step and sample restriction affects our sample size. In Section 4.3, we confirm that these restrictions do not induce sample selection by showing that our instrument is uncorrelated with inclusion in our analysis sample or classification as an owner, renter, or landlord.

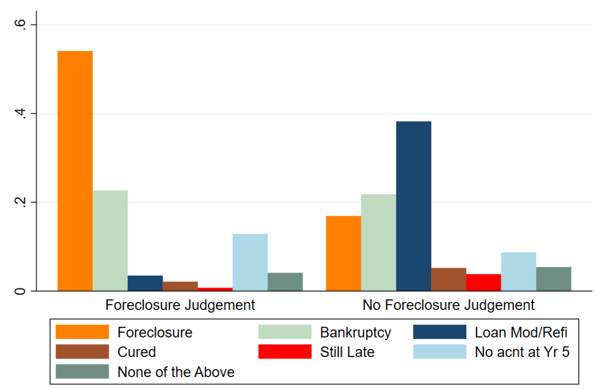
We finally match our data set to the credit reports to create a subsample of our main sample that we refer to as the "credit report subsample." The credit bureau that provides our data conducts this match for us. 87 percent of the people in our final sample match a credit report, and 97 percent of our cases have at least one matched individual, as indicated in the bottom panel of Table 1. Appendix Table A.1 provides summary statistics for the owner, renter, and landlord samples by foreclosure outcome for our main outcome variables.

[14]Infutor is less reliable for tracking renters, as their moves leave a smaller paper trail, particularly for transient, lower-income renters and in the very early years of the Infutor data (the 1980s and 1990s). Some renters' most recent addresses have not been updated since the early years of Infutor's data, suggesting Infutor lost track of them. Limiting our sample to renters who moved in most recently before the foreclosure filing is the simplest way to eliminate these problematic cases and yields a more plausible 20 percent annual move rate before the foreclosure filing.

Figure 1: Property Outcomes in Deeds Data By Foreclosure Judgment



(a) CoreLogic Deeds Data



(b) Credit Report Data

Notes: The figure shows property outcomes 5 years after the initial foreclosure filing, split by whether a foreclosure occurred within 3 years in the court judgments. Panel (a) splits outcomes by what we observe in the CoreLogic deeds data: a foreclosure deed or auction, an observed refinancing or mortgage modification (which shows up as a new lien and looks like a refinance), a short sale as categorized by CoreLogic, an arms-length sale as categorized by CoreLogic, or none of the above (no sale or refinancing). Panel (b) splits outcomes based on status codes in owners' and landlords' tradeline credit report data for their mortgages. "Modification" combines arrangements made to make partial payments, settlements, adjustments, loan modifications, and loan forbearance. For cases without foreclosure, bankruptcy, or modification codes, we determine their status at year 5 by checking whether the first mortgage is current ("cured"), still delinquent ("late"), or closed/paid off/inactive ("no account at year 5"). For both panels, categories are assigned hierarchically from left to right: A foreclosure code places a case in the foreclosure category regardless of other codes; bankruptcy takes priority over loan modification; and so on, so that the bars add to 100 percent.

2.4 Outcomes For Properties With a Foreclosure

We first document descriptive facts about how mortgage defaults resolve for cases that do and do not result in a foreclosure. Figure 1 shows outcomes five years after the foreclosure filing, split by whether we observe a foreclosure judgment within three years of filing. Panel 1a shows results from the CoreLogic deeds data on sales (including foreclosure sales) and liens. Panel 1b shows results in the credit report data on payments and account status. Crucially, in the credit report data, delinquency is administratively reported, but foreclosure requires manual entry by lenders and is thus not administrative. This explains why lenders rely less on foreclosure and more on payment in underwriting.

Panel 1a shows that over 90 percent of cases with a foreclosure judgment have an observed foreclosure sale in the deeds data within 5 years. By contrast, under 15 percent of cases without a foreclosure judgment within 3 years experience a foreclosure sale within 5 years.[15] Of those with no foreclosure judgment, about 12 percent have a loan modification or refinancing that is

[15]This is non-zero because of foreclosure judgments delivered after more than 3 years, refilings, and deed-in-lieu of foreclosure settlements that are coded as foreclosures.

significant enough that it is reported in the deeds data (most loan modifications do not necessitate changing the liens on a property and are thus not recorded in deeds data). Another 10 percent are sold as short sales or regular, arm's-length sales. Just over half of the cases with no foreclosure judgment have no visible outcome in the deeds data within five years.

Panel 1b shows outcomes in the credit report data. Only 60 percent of foreclosures appear in the credit report data because the resolution of a default is optionally reported in flags and notes in the credit report. This significant under-reporting cautions against relying directly on credit bureau-reported foreclosure indicators. Just over 20 percent of both foreclosed and non-foreclosed cases resolve as a bankruptcy in the credit report data. Cases that are not foreclosed primarily appear as having a loan modification or a payment plan, although some borrowers cure or remain delinquent.[16] This is consistent with conversations we had with lawyers representing parties in Cook County foreclosure cases who report that the vast majority of dismissed cases result from settlements with the lender, typically a loan modification or payment plan. These are the main alternatives to foreclosure in our data, making our results particularly relevant for policies that encourage settlements.

3 Evidence of Selection

In this section, we assess potential bias in approaches that use selection on observables (OLS and PSM) to identify the causal effects of foreclosure. We first formally present the OLS framework and sources of bias, and then evaluate whether OLS is biased and, if so, how.

3.1 OLS Regression Framework

Most of the literature summarized above regresses post-foreclosure outcomes on a foreclosure indicator (typically a delinquency in credit bureau data), controlling for observables. Formally, index individuals by i, event time in years relative to a foreclosure filing by $s \in \{-5, ..., 5\}$, let $Y_{i,s}$ be the outcome of interest, F_i be a foreclosure indicator, and X_i be a vector of observables. The canonical OLS event study regression is:

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

[16] Unfortunately, our data do not have details on these modifications and payment plans, so we cannot assess what margins these modifications affect or how judges of different leniency affect the characteristics of a modification.

where ξ is a date of filing fixed effect and ϕ is a ZIP-year fixed effect. β_s, the coefficient of interest, is the effect of foreclosure on Y at horizon s relative to foreclosure filing. β_s is zero in the year prior to foreclosure filing, either due to controlling for the outcome at that date or through a normalization when that control is not included.

OLS — and its cousin in using selection on observables, PSM — identify β_s through a cross-sectional comparison between homes with a foreclosure filing that are foreclosed upon and that are not foreclosed upon.[17] β_s is causal if foreclosure is random conditional on the observables; OLS and PSM seek to condition on "enough" observables that this holds. This condition can also be thought of in terms of a differences-in-differences design, in which case β_s is causal if foreclosed and non-foreclosed households follow parallel trends absent foreclosure and no omitted variable affects the foreclosed households at the time of filing.

It is also crucial to consider the population analyzed by OLS. Most existing studies consider the full population, with F indicating a foreclosure filing; consequently, the control group consists of households that do not default. We consider the population with a foreclosure court filing, and F is an indicator for a foreclosure judgment within 3 years of foreclosure filing. Our control group consists of households that default but do *not* experience a foreclosure judgment. Our population has the advantage of being closer to most foreclosure mitigation policies, which typically help households that are already delinquent or in default. For instance, the HAMP program provided mortgage modifications to households that had already defaulted to help prevent a foreclosure.

3.2 Sources of Bias in OLS

There are two principal sources of bias in OLS and PSM. First, borrowers with less ability to pay their mortgage may be more likely to foreclose. Prior literature focuses on the ability to pay (ATP) because recent research shows that these shocks explain default (Ganong and Noel, 2023; Bhutta et al., 2017; Gerardi et al., 2015). However, it is less clear that ATP shocks also cause foreclosure *conditional on default*. If an unobserved shock simultaneously causes foreclosure and other adverse outcomes, the ATP story would lead OLS to overstate the adverse effects of foreclosure, as the regression would attribute the shock's negative impacts to foreclosure.

Second, borrowers for whom foreclosure has a higher private cost — pecuniary or non-pecuniary — may be less likely to foreclose because they do more to avoid a foreclosure judgment. As with

[17] PSM is a variant of OLS that controls for an estimated predicted foreclosure probability (the propensity score) given observables. Like OLS, PSM relies on selection on observables.

the first source of bias, this is an omitted variable that manifests itself at the time of foreclosure and thus would not appear in a pre-trend. This "costliness" story would lead OLS to understate any adverse consequences of foreclosure, as it would induce a negative correlation between potential outcomes and treatment. Given that the literature has shown that strategic default is rare, we do not expect strong selection on private costs at the default stage.

3.3 Evidence of Selection

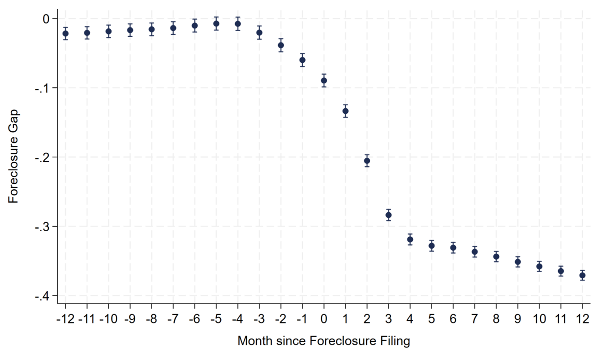
We present three pieces of evidence that there is significant bias arising primarily from the costliness story.

First, 35 percent of owner cases are foreclosed upon within three years, but 54 percent of landlord cases are (Appendix Table A.1). This suggests that owners, who not only incur financial losses like landlords but also face the risk of eviction, take steps to avoid foreclosure.

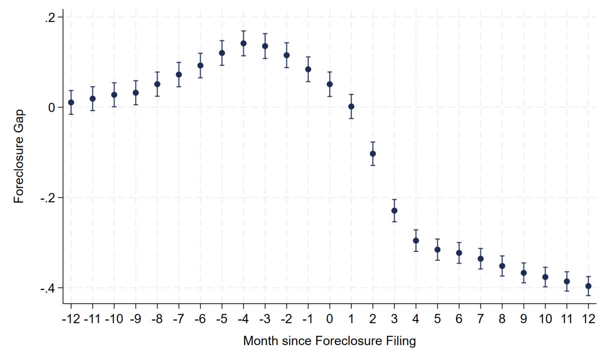
Second, Figure 2 shows the correlation between eventual foreclosure and credit report outcomes taken as snapshots in the credit report data at monthly frequency around the foreclosure filing for owners (panels a, c, e) and landlords (panels b, d, f), controlling for date-of-filing fixed effects. We observe high-frequency pretrends in the 9 months preceding the foreclosure filing that reveal significant endogeneity. Panels 2e and 2f show that default on non-mortgage accounts begins to rise 8 to 12 months before the foreclosure filing for households that eventually foreclose. Panels 2c and 2d show that this is followed by an increase in the number of newly originated first mortgages 4 to 12 months before filing, likely reflecting refinancing, as the number of active first mortgages does not rise for owners, but does modestly for landlords (Panels 2a and 2b). Finally, from 4 to 5 months before foreclosure to about 3 months after, the number of active first mortgages declines precipitously, especially for landlords who often have several primary mortgages on multiple investment properties, indicating either sales or defaults. The number of new first mortgages also plunges 3 to 4 months prior to foreclosure. Crucially, these pretrends for owners are not picked up in annual regressions as used by most of the literature, as shown in Appendix Figure A.1. We do see annual pretrends for landlords, who have not been evaluated in prior work.

Third, to disentangle the two stories, we examine the correlation between foreclosure and proxy variables for the ability to pay and the costliness of foreclosure. Many of these variables come from linking our data to mortgage loan servicing data (LLMA) from Corelogic. Since this data covers only a subset of our cases and requires a fuzzy merge, our linked sample is much smaller, so we use it only to investigate the mechanisms driving endogenous foreclosure.

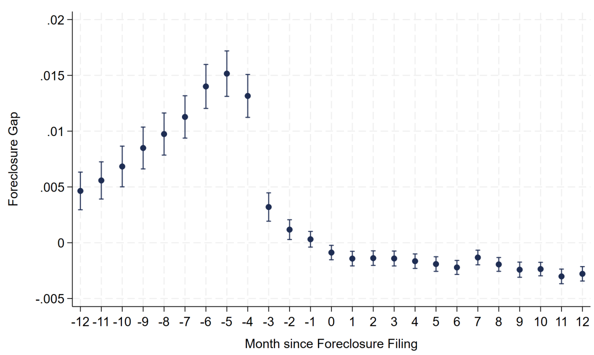
Figure 2: Monthly Correlation Between Foreclosure and Credit Report Outcomes



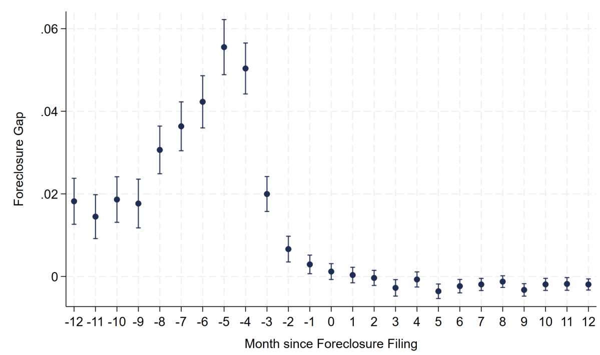
(a) Number of Active First Mortgages (Owners)



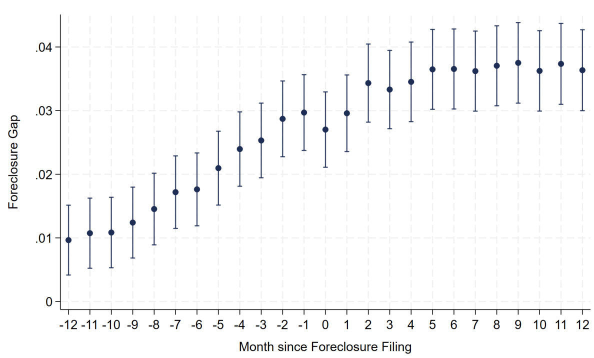
(b) Number of Active First Mortgages (Landlords)



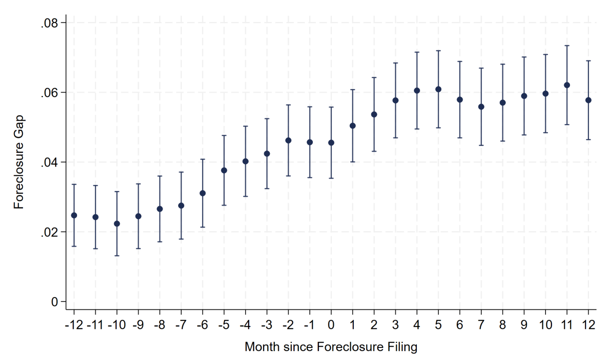
(c) Number of New First Mortgages (Owners)



(d) Number of New First Mortgages (Landlords)



(e) Share of Non-Mrtg Accts in Default (Owners)



(f) Share of Non-Mrtg Accts in Default (Landlords)

Notes: This figure shows the results of monthly regressions for the indicated group of the indicated outcome variable from credit reports on foreclosure, controlling for date-of-filing fixed effects. Formally, index individuals by i, event time in months relative to a foreclosure filing by $s = -12, ..., 12$, let $Y_{i,s,t}$ be the outcome at time t and time relative to foreclosure s, F_i be a foreclosure indicator, and $\xi_{t,s}$ be a date of filing fixed effect. We regress $Y_{i,s,t} = \beta_s F_i + \xi_{t,s} + \varepsilon_{i,s,t}$ clustering standard errors at the case level and plot β_s against s. Active first mortgages are the number of first mortgages that appear on the credit report as outstanding. New mortgages are the number of first mortgages originated in the prior month.

Table 2 summarizes the variables and their correlation with foreclosure. The last three columns indicate whether correlations support the costliness or ability-to-pay (ATP) stories (see Appendix B for details). If financially weaker households are more likely to foreclose, this supports the ATP story. Conversely, if financially stronger households or households that have lower default costs foreclose more, this indicates strategic default driven by heterogeneous private costs, as in the costliness story. The proxies include measures of financial weakness (credit scores, delinquency rates, proxies for income and mortgage payment shocks), direct indicators of strategic default (past mortgage modifications and payment patterns, which indicate whether a household has attempted payment or has walked away, going straight to full delinquency without attempting payment as in Keys et al. (2012)), and measures of private foreclosure costs (owner occupancy, LTV at foreclosure).

Column 1 of Table 2 shows regression coefficients from separate bivariate regressions of each indicated variable on foreclosure, controlling for the month of filing and ZIP-year fixed effects. Column 2 displays regression coefficients from an analogous multivariate regression. The bivariate regressions are limited to having the same sample as the multivariate regression.

The results reveal that foreclosure is associated with borrowers who face lower costs to foreclosure. Owner-occupancy, past loan modifications, chronic delinquency, and efforts to cure all negatively predict foreclosure, indicating that households who potentially have more to lose make greater efforts to avoid foreclosure. The ZIP income change between foreclosure filing and loan origination becomes insignificant once controlling for LTV, consistent with the costliness story in which local income affects house prices and, in turn, LTV ratios, which matter for how much a borrower loses by walking away from the mortgage. We find limited evidence for the ATP story: Mortgage interest rate differentials between origination and foreclosure filing (due to ARM rate resets) are insignificant, and while credit scores negatively affect foreclosure in the multivariate regression, as predicted by ATP, the effect is economically small, and is positive in the bivariate regression.

Despite these correlations favoring the costliness story, the proxies explain only a relatively low share of the variation in foreclosure, as indicated by a within R^2 excluding the fixed effects of 0.057 in the multivariate regression. Given that there is likely a significant component of the individual costliness of foreclosure that is unobserved, we see selection on observables as a hopeless exercise. We thus turn to an IV strategy.

Table 2: Correlation of Ability to Pay and Costliness Proxies With Foreclosure

Variable	Bivariate	Multivariate	Neg Supports	Pos Supports	Explanation
ZIP Avg Income Change	-0.032***	0.0009	ATP and Cost		ATP: Proxy for income
Since Origination	(0.002)	(0.0040)	No Cost w/LTV Control		Cost: Income→House Prices→
					Equity Lost in Foreclosure
6 Month Pre-Filing	0.025***	-0.0042	ATP	Cost	ATP: Financial Fragility Indicator
Vantage Score	(0.001)	(0.0032)			Cost: Strategic Default Indicator
Pre-Filing Share of	-0.017***	0.0019	Cost	ATP	ATP: Financial Fragility Indicator
Debts That Are Delinquent	(0.001)	(0.0028)			Cost: Strategic Default Indicator
Interest Rate Differential	0.001	-0.0004		ATP	ATP: Payment Shock
Since Origination	(0.002)	(0.0023)			
Owner Occupied	-0.050***	-0.0382***	Cost		Cost: Owner Also Evicted
Property Indicator	(0.003)	(0.0066)			
Late Prior to 6 Months	-0.051***	-0.0713***	Cost	ATP	ATP: Income Vol Measure
Pre-Filing Indicator	(0.001)	(0.0067)			Cost: Strategic Default Indicator
Mortgage Modification	-0.027***	-0.0376***	Cost		Cost: Revealed High Private Cost
in Past Indicator	(0.001)	(0.0074)			
Caught Up After Being	-0.045***	-0.0343***	Cost	ATP	ATP: Income Vol Measure
Late in Past Indicator	(0.001)	(0.0060)			Cost: Strategic Default Indicator
LTV at Foreclosure	0.0526***	0.0526***		Cost and ATP	ATP: Initial LTV → Fin Fragility
	(0.008)	(0.0032)			Cost: Equity Lost in Foreclosure
Observations		42,826			
Within R^2		0.018			

Notes: This table shows results for regressions of the proxy variables on an indicator for foreclosure within 3 years of filing. All variables not listed as indicators are standardized to have a mean of zero and a standard deviation of one; indicators are left as binary. The bivariate column indicates coefficients in a bivariate regression with the indicated variable on foreclosure, with month of filing and zip-year of filing fixed effects. The multivariate column shows coefficients from a multivariate regression that combines all variables. Because Vantage Score started in 2008, we set all variables with missing Vantage scores to a single value and include a dummy variable for missing Vantage scores. Vantage score is in hundreds. The last two columns indicate which story is supported by a negative or positive coefficient. For example, a negative coefficient on "Pre-Filing Share of Debts That Are Delinquent" supports the costliness story, while a positive coefficient supports ATP. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

4 IV Empirical Approach

Our instrument leverages differential randomly assigned judge stringency to provide exogenous variation in foreclosure. Our empirical approach follows the best practices of the empirical literature that uses the stringency of randomly-assigned judges for identification of causal effects in the study of incarceration, bankruptcy, disability insurance, foster care placement, and evictions (Kling, 2006; Doyle, 2007; Kolesar, 2013; Maestas et al., 2013; Dahl et al., 2014; French and Song, 2014; Dobbie and Song, 2015; Aizer and Doyle, 2015; Mueller-Smith, 2015; Dobbie et al., 2018; Bhuller et al., 2020; Norris et al., 2021; Bald et al., 2022; Collinson et al., 2024).

4.1 Econometric Approach

We use the leave-out probability of foreclosure in a case's calendar-year as an instrument for foreclosure. Formally, define the stringency $Z_{c(i)}$ of an individual i's case k assigned to calendar c in year t as:

$$Z_{c(i),t} = \frac{\sum_{j \in c,t,j \neq k(i)} F_{j,t}}{\sum_{j \in c,t,j \neq k(i)} 1}.$$

We then estimate the causal effect of foreclosure in a two-stage least squares framework for outcomes at horizon $s \in \{-5, ..., 5\}$ that builds on the OLS approach in equation (1):

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

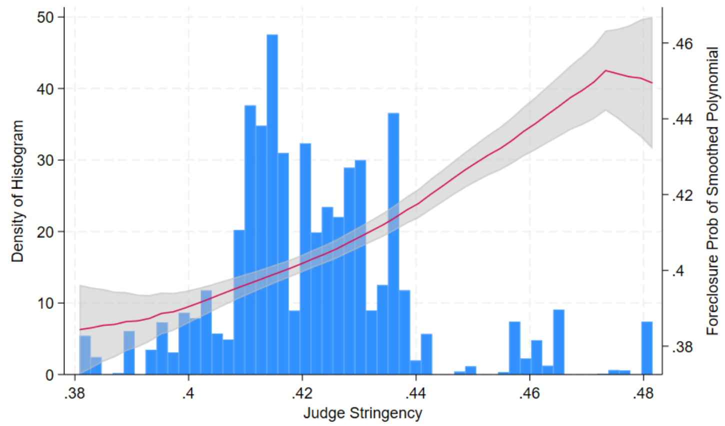
$$F_i = \Gamma Z_{c(i),t} + \alpha X_i + \zeta_t + \varphi_{z(i),t} + e_{i,t}. \tag{3}$$

As with equation (1), the regression controls for individual-level observables X_i, a date of filing fixed effect $\xi_{t,s}$, and a ZIP-by-year fixed effect $\phi_{z(i),t,s}$, and β_s is normalized to zero in year $s = -1$.[18] X_i includes $Y_{i,-1}$, the outcome in the pre-foreclosure year. We include this control to improve statistical power, but it is not necessary for our findings. We weight by the inverse of the number of people per case, so cases are represented equally. Standard errors are clustered at the case level.

Under three assumptions, β_s is the local average treatment effect of foreclosure at horizon s for "compliers" whose foreclosure status is affected by their judge. First, the instrument has to be relevant, that is, $\Gamma \neq 0$. Second, an exclusion restriction that the instrument is orthogonal to the second-stage error term — $Z_{c(i),t} \perp \varepsilon_{i,s}$ — must hold. This is ensured by random calendar assign-

[18] For the school outcomes, we use data on the closest school to the foreclosure address as described in the Appendix and use school-by-year rather than ZIP-by-year fixed effects.

Figure 3: Stringency Histogram and Nonparametric First Stage



Notes: The left axis shows a histogram of calendar-year stringency (probability of foreclosure) residualizing on month fixed effects (with the overall mean added back in). Because there is one observation per case, we do not weight. The right axis shows a local linear regression of the first-stage pooling owners, renters, and landlords into a single case-level regression with one observation per case. The local linear regression residualizes out month fixed effects and adds back the sample mean; the grey bands show a 95% confidence interval.

ment. Third, a monotonicity condition that all judges have the same ranking of the likelihood of foreclosure across cases must hold. In the judge IV context, monotonicity is a concern if, for instance, minority judges are more lenient towards minority defendants. We test these assumptions in the remainder of this section.

4.2 Heterogeneity in Calendar-Year Stringency and First Stage

Figure 3 overlays a histogram of judge stringency with a local linear first-stage regression for a sample that pools owners, renters, and landlords. The histogram on the left axis graphically displays the variation in judge stringency we use for identification. The foreclosure rate within 3 years across calendar-years ranges from 38 percent for the most lenient calendar-years to 48 percent for the most stringent calendar-years, with most of the variation falling between 40 percent and 44 percent. The 10 percent of cases whose outcomes are affected by the random assignment of the judge are the compliers.

The local linear regression in Figure 3 displays a smooth and monotonic upward-sloping first-stage relationship. Table 3 presents the first-stage regression used in our two-stage least squares framework (2) for various subsamples. The full-sample first-stage coefficient ranges from 0.74 to 0.90, with F statistics of 96.5 for owners, 32.2 for landlords, and 37.6 for renters, large enough that weak instruments are not a concern. The first stage for the credit report subsample is similar but

Table 3: IV First Stage Regression Coefficients and F Statistics

Dependent Variable: Foreclosed Within 3 Years				
Sample	All	Owner	Landlord	Renter
	(1)	(2)	(3)	(4)
Full Sample	0.746***	0.758***	0.740***	0.897***
Judge Stringency	(0.067)	(0.077)	(0.130)	(0.146)
F	124.1	96.5	32.2	37.6
N	186,365	133,279	53,013	42,929
Credit Report Sample	0.753***	0.768***	0.724***	0.824***
Judge Stringency	(0.068)	(0.077)	(0.136)	(0.158)
F	124.16	98.51	28.26	27.18
N	180,051	127,990	50,301	38,515

Notes: This table presents first-stage regression coefficients for all cases combined, as well as for owners, renters, and landlords separately. The top panel shows results for the full sample, while the bottom panel shows results for the credit report subsample. The regression is at the case-person level and includes month and zip-year fixed effects as in equation (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.

slightly less powerful for landlords and renters. The sample sizes differ from those in Table 1 due to singletons, which are dropped from this regression because they are absorbed by the fixed effects.

4.3 Verifying Random Assignment

Our IV approach relies on random assignment. We verify that the calendar is randomly assigned as the Court claims in three ways. First, Appendix C demonstrates that our cleaned calendar variable accurately replicates the published random assignment probabilities from the Cook County Courts. Second, there are no pre-trends in the results. Third, Table 4 presents several placebo tests in which we regress our instrument $Z_{c(k),t}$ on observables and test whether the coefficient is zero, as it should be under random assignment. The regressions take the form of the IV first stage (3), replacing the foreclosure dummy with the indicated observable. At the case level, judge stringency has no statistically significant effect on whether a case is in our sample or is categorized as an owner, renter, or landlord, which is reassuring not only for random assignment but also for concerns that our data cleaning and categorization of individuals result in a selected sample. At the case-person level, judge stringency has no statistically significant effect on age, gender, or having an credit report match. In Appendix Table A.2, we further show that none of the proxies

Table 4: Placebo Tests: Stringency Instrument Regressed on Observables

Dependent	Case-Level				Case-Person-Level		
Variable	In Sample	Owner	Renter	Landlord	Age	Male	Credit Match
Judge Stringency	0.008 (0.048)	0.088 (0.055)	-0.053 (0.060)	-0.078 (0.054)	0.007 (0.019)	0.058 (0.051)	-0.011 (0.032)
N	239,782	186,365	186,365	186,365	298,369	382,033	382,033

Notes: This table shows placebo regression coefficients for all cases matched to the Infutor data, including owner cases, landlord cases (some of which have renters), and cases without a defendant type. The first four columns present case-level regressions with dependent variables for being in the sample (matched to an owner or landlord), having an owner, having a renter, and having a 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.

for ability to pay or costliness in Table 2 are correlated with the instrument.

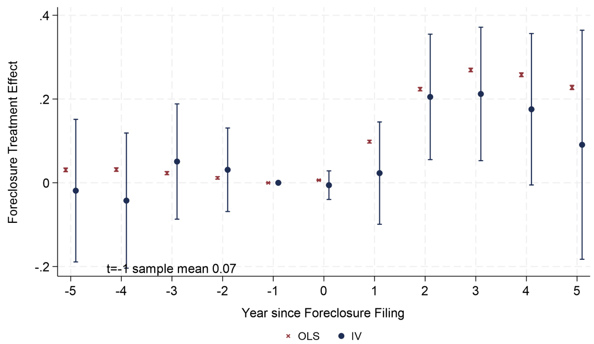
4.4 Verifying Monotonicity

Frandsen et al. (2023) provide a statistical test of monotonicity that examines how average judge-level outcomes vary as a function of the judge-level propensity to treat. Appendix C.4 shows that we cannot reject the null hypothesis of monotonicity holding for all of our primary outcome variables. We further demonstrate that the first stage is strong, positive, and monotonic when run separately by primary calendar judge and defendant characteristics, thereby assuaging the concern that judges behave differently towards defendants with whom they are similar.

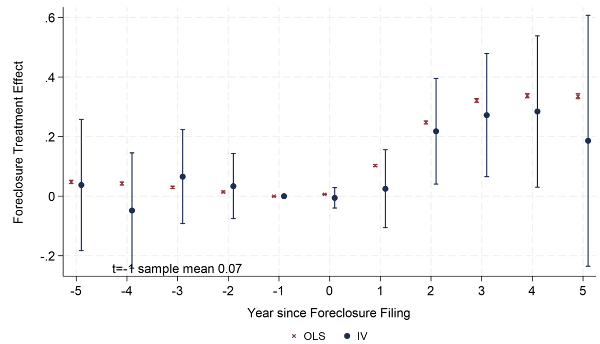
4.5 Accounting For the Judge's Effect on Time to Foreclosure

In the data, less stringent judges take slightly longer to decide cases. One might be concerned that the causal effects our IV approach attributes to foreclosure are instead due to the time to decision. To account for this, Appendix D runs regressions that include both foreclosure and time to judgment, instrumenting for judge time to judgment with the judge's leave-out average as we do for foreclosure. We find that the causal effects we attribute to foreclosure in the primary analysis are due to foreclosure, not to time to judgment.

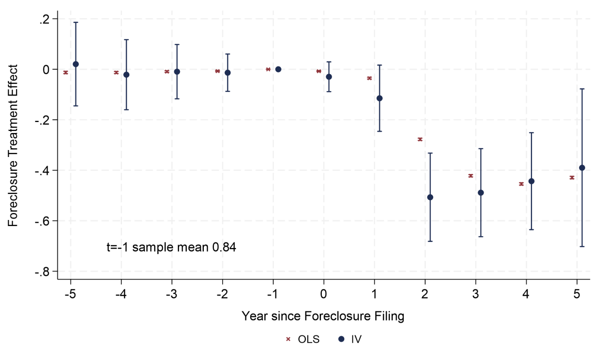
Figure 4: Owners: Moving, Homeownership, and House Characteristics



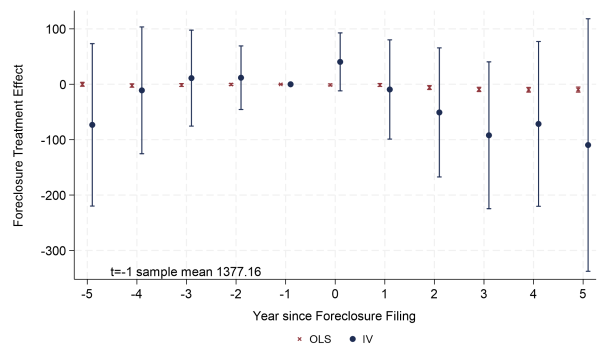
(a) Moved from Foreclosure Address



(b) Cumulative Number of Moves



(c) Owns Primary Residence



(d) Square Footage of Living Space

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

5 Results: Owners

Figures 4, 5, 6, and 7 and columns 1 and 2 of Table 5 show IV and OLS results for owners. The blue dots show the IV point estimates for each year $s \in \{-5, ..., 5\}$ since foreclosure with 95% confidence intervals indicated by blue bars. Red Xs show the OLS point estimates; we omit confidence intervals from the figures since they are so small. As a final specification check, none of our IV results show statistically significant pre-trends. We first analyze the IV results and then discuss the differences between OLS and IV across outcomes.

Table 5: Summary of All IV and OLS Results at Years 3 and 4

	Owner		Landlord		Renter	
Specification:	IV	OLS	IV	OLS	IV	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Moved from Foreclosure Address	0.195***	0.265***	0.110	0.008**	-0.007	0.029***
	(0.075)	(0.002)	(0.149)	(0.004)	(0.128)	(0.004)
Cumulative Number of Moves	0.278***	0.331***	0.063	0.024***	0.043	0.036***
	(0.103)	(0.003)	(0.211)	(0.005)	(0.173)	(0.005)
Owns Primary Residence	-0.468***	-0.438***	-0.055	-0.052***	0.136	-0.010***
	(0.077)	(0.002)	(0.151)	(0.004)	(0.083)	(0.003)
Square Footage of Living Space	-82.632	-9.594***	-178.963	-7.259	126.970	-3.884
	(60.989)	(2.066)	(138.655)	(4.529)	(123.537)	(3.553)
Log Zip Code Average Income	-0.106***	0.014***	0.150	0.006**	-0.085	-0.001
	(0.040)	(0.001)	(0.096)	(0.002)	(0.059)	(0.002)
Elementary School Test Score Rank	-0.774	0.408***	15.140**	0.660***	2.202	-0.383**
	(3.261)	(0.089)	(7.044)	(0.175)	(5.376)	(0.165)
Middle School Test Score Rank	-6.630**	0.171**	-9.648	0.046	5.957	-0.422***
	(3.054)	(0.087)	(6.401)	(0.178)	(5.341)	(0.161)
High School Test Score Rank	-5.182	0.965***	-8.237	0.709***	6.606	-0.285
	(3.527)	(0.103)	(7.665)	(0.208)	(5.860)	(0.200)
Opportunity Atlas: Intergen Mobility	-0.021**	0.002***	0.025	0.001**	-0.009	0.000
	(0.010)	(0.000)	(0.017)	(0.001)	(0.017)	(0.001)
Opportunity Atlas: Frac of Pop in Jail	0.005***	-0.000***	-0.004	-0.000**	-0.000	0.000
	(0.002)	(0.000)	(0.003)	(0.000)	(0.004)	(0.000)
Opportunity Atlas: Teen Birth Rate	0.031**	-0.003***	-0.043*	-0.001	0.030	0.000
	(0.013)	(0.000)	(0.024)	(0.001)	(0.026)	(0.001)
Cumulative Number of Divorces	0.040*	0.001*	-0.048	-0.001	-0.017	-0.001
	(0.022)	(0.001)	(0.039)	(0.001)	(0.029)	(0.001)
VantageScore	-3.716	-1.237***	56.161*	-0.446	-4.144	-0.577
	(12.433)	(0.466)	(32.414)	(0.842)	(37.400)	(1.152)
Has Had Bankruptcy	0.223***	0.077***	0.094	0.103***	0.007	0.001
	(0.074)	(0.002)	(0.154)	(0.004)	(0.097)	(0.003)
Number of Active First Mortgages	-0.245**	-0.351***	-0.522*	-0.369***	-0.046	-0.005
	(0.103)	(0.002)	(0.284)	(0.007)	(0.163)	(0.004)
Ever Defaulted on a First Mortgage	0.201***	0.077***	0.242	0.102***	0.009	-0.000
	(0.074)	(0.002)	(0.165)	(0.004)	(0.073)	(0.002)
Ever Defaulted on a Home Equity Loan	0.104*	0.049***	-0.134	0.082***	0.017	0.001
	(0.063)	(0.002)	(0.161)	(0.004)	(0.060)	(0.002)
Has a Home Equity Account (excl. Mortgages)	-0.107**	-0.035***	-0.119	-0.016***	-0.096	-0.003
	(0.049)	(0.001)	(0.126)	(0.003)	(0.084)	(0.002)
Share of First Mortgage in Default	0.249	0.094***	-0.241	0.081***	0.041	-0.002
	(0.174)	(0.004)	(0.196)	(0.004)	(0.254)	(0.008)
Share of Non-Mortgage Accts in Default	0.192**	0.053***	0.142	0.047***	0.045	-0.006
	(0.080)	(0.002)	(0.134)	(0.003)	(0.162)	(0.005)
Share of Credit Card in Default	0.123	0.051***	0.214	0.043***	0.007	-0.002
	(0.086)	(0.003)	(0.147)	(0.004)	(0.148)	(0.006)
Share of Auto Loan in Default	0.287	0.034***	1.685	0.024***	-0.487	0.002
	(0.199)	(0.004)	(2.067)	(0.006)	(1.455)	(0.010)
Share of Student Loan in Default	-0.090	0.019***	-0.884	0.019***	-0.008	-0.031***
	(0.151)	(0.005)	(0.554)	(0.007)	(0.439)	(0.012)

Notes: This table summarizes our IV and OLS results for owners, landlords, and renters by presenting pooled results for years 3 and 4 relative to a base period of the year before foreclosure filing. For IV, we estimate equation (2) using two-stage least squares, with the first stage given by equation (3). For OLS, we estimate equation (1). Above the line are results using the full sample, while below the line are results for the credit report subsample. 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.

5.1 Moving, Housing, and Homeownership

Figure 5 shows outcomes for moving, home characteristics, and homeownership. Unsurprisingly, foreclosures cause homeowners to move out of their homes, as illustrated in Panel 4a. This picks up in year 2 and peaks in year 3, with a causal effect in years 3 and 4 of 19.5 percentage points. Panel 4b shows that the cumulative number of moves since the foreclosure filing (0.28 in years 3 to 4) is slightly larger than the indicator for having moved (0.20), suggesting that foreclosure leads to modest housing instability, although the difference is not statistically significant.

About half the owners who have a foreclosure ruling against them in the first 3 years have moved by year 5, indicating a non-trivial share continue to live at the foreclosed address after a foreclosure. This finding is in line with the existing literature and may reflect banks' incentives to delay evictions of foreclosed-upon owners so homes are not vacant and vulnerable to vandalism.[19] To ensure this finding is not an artifact of data quality, Appendix Figure A.6 replicates Panel 4a using address data from the credit bureaus instead of Infutor and finds comparable results. Indeed, Chetty et al. (2026) explicitly validate the Infutor data quality against the 2010 and 2000 restricted-use decennial Census and found that Infutor performed as well as the IRS tax records at tracking individuals' addresses over time.

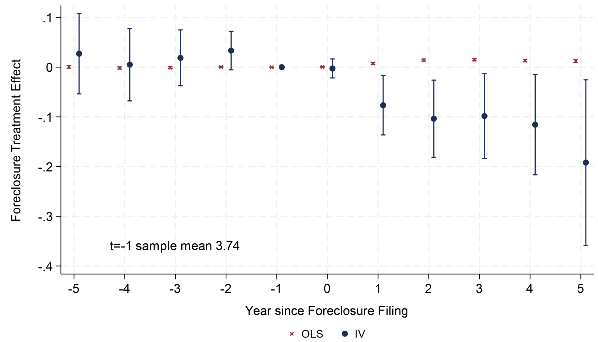
Foreclosed-upon homeowners are also substantially less likely to own their residence after a foreclosure, as seen in Panel 4c, which shows a peak 51-percentage-point effect in year 2. This is highly persistent: The point estimate is 44 percentage points at year 4 and 39 percentage points at year 5, although at 5 years, the confidence intervals widen. Finally, Panel 4d shows that foreclosure reduces home size by about 110 square feet or 7 percent at peak, although the effect is not statistically significant.

5.2 Neighborhood Quality

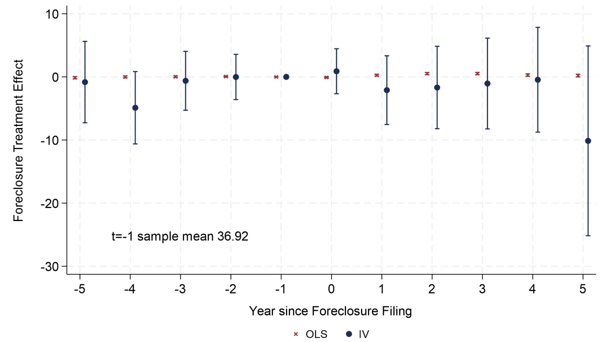
Figure 5 shows that foreclosure causes moves to worse neighborhoods across multiple dimensions. Income-based measures decline sharply: Average ZIP code income falls by 10 percent in years 3 to 4 and nearly 20 percent by year 5 (Panel 5a). School quality measures also worsen, though not all estimates are statistically significant. Middle school test score ranks decline by a

[19] Our 50 percent figure is in line with Molloy and Shan (2013), who find that only 55% of those who are foreclosed upon have moved to a different Census Block four years after the foreclosure filing (their Table 2), and Piskorski and Seru (2021), who report that 60% of those with a foreclosure between 2007 and 2017 have moved to a different ZIP code by 2017 (their Table 1b). Furthermore, in a 2013 report, RealtyTrac found that nationwide, the previous owner occupied 47 percent of bank-owned homes, and that this figure is particularly high in Chicago (see https://mortgageorb.com/realtytrac-47-of-bank-owned-homes-still-occupied-by-former-owner).

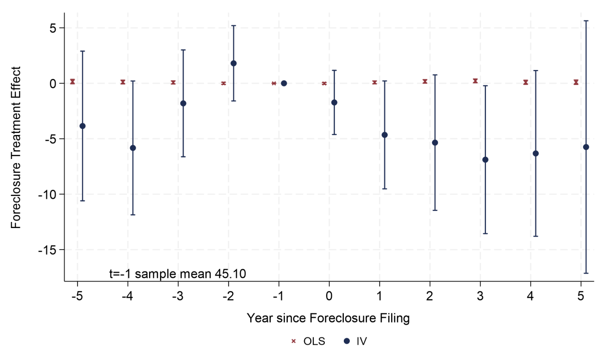
Figure 5: Owners: Neighborhood Characteristics



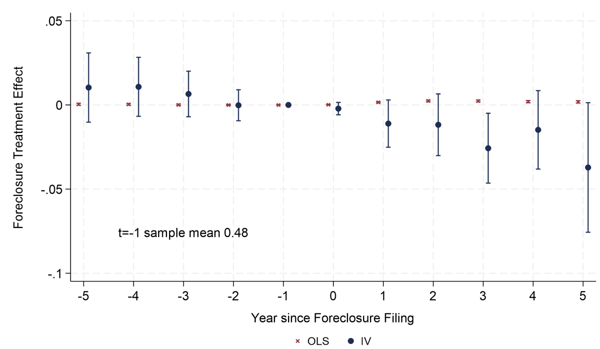
(a) Log ZIP Code Average Income



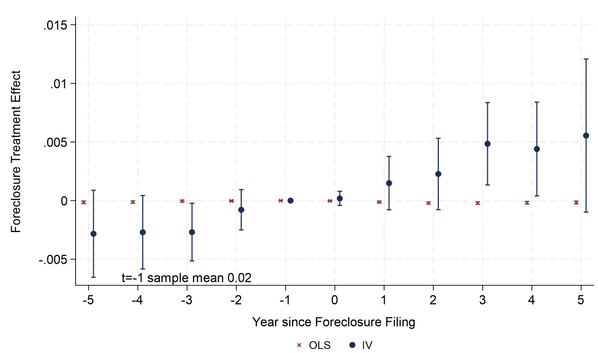
(b) Elementary School Test Score Rank



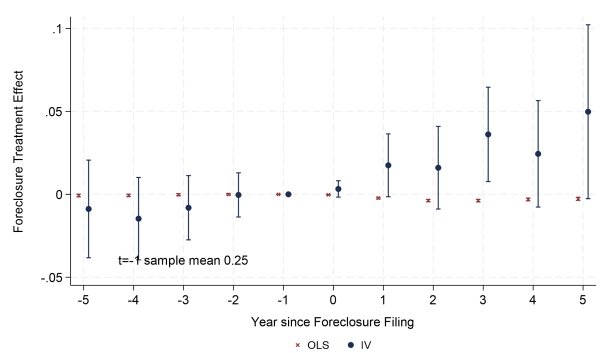
(c) Middle School Test Score Rank



(d) E[Adult Income Rank] of Kids



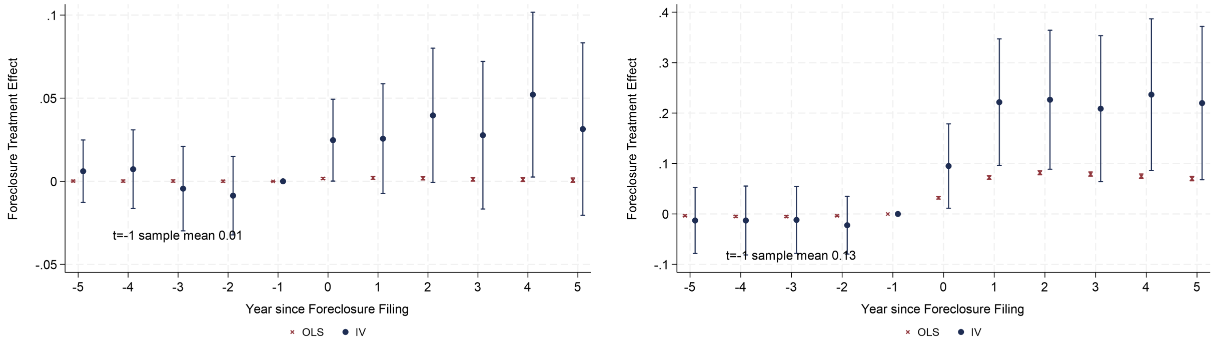
(e) Pct of Pop in Jail



(f) Teen Birth Rate

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case. For test scores, the dependent variable is the average percentile rank of the local school in math and reading (a coefficient of 1 indicates a 1-percentage-point change in the average rank). The Illinois Board of Education reports these percentages for math and reading separately, and we combine them into a single average index. The Opportunity Atlas variables are the mean percentile rank of children of children who grew up in the census tract in the national earnings distribution ("kfr" in their codebook), the teen birth rate of women who grew up in the census tract, and the percentage of the population who grew up in the census tract who were ever incarcerated.

Figure 6: Owners: Personal Outcomes



(a) Cumulative Number of Divorces

(b) Ever Had a Bankruptcy (Credit Report)

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case. "Ever had a Bankruptcy" indicates whether there has been a bankruptcy from year -5 through the stated year.

statistically significant 6.6 percentiles (Panel 5c), while elementary school scores and high school test scores both fall by a peak of 10 percentiles (Panel 5b and Appendix Figure A.4), though neither is significant. Finally, measures from the Opportunity Atlas (Chetty et al., 2025) indicate that children who grow up in destination neighborhoods have worse outcomes. In years 3 to 4, the mean percentile rank of children who grow up in the tract in the national income distribution falls by 2.1 percentiles (Panel 5d), the fraction of the tract population in jail rises by 0.5 percentage points (Panel 5e), and the tract teen birth rate increases by 3.1 percentage points (Panel 5f).

5.3 Personal Outcomes

Figure 6 shows that foreclosure causes elevated divorce and bankruptcy. Because divorce is relatively rare, Panel 6a shows the effect on the cumulative number of divorces since 5 years before foreclosure. Foreclosure causes a spike in divorce, reaching 4 percentage points in years 3 and 4. This is a 170 percent increase over the 3-year divorce probability for the non-foreclosure group. This effect is significant at the 10% level and does not condition on marriage, implying the conditional effect for married couples is nearly double what we find for the general population. Our economically large IV findings on divorce echo those of Charles and Stephens (2004), who find evidence of elevated divorce hazards after job layoffs.

Panel 6b shows results for an indicator for ever having a bankruptcy in the credit report data.

This increases by 9.5 percentage points in year 0 and 22.2 percentage points in year 1, after which it plateaus. This result is highly significant and represents an effect by years 3 to 4 that is 205% of the sample mean in the year before foreclosure filing.[20]

5.4 Financial Outcomes

Figure 7 shows results for selected financial outcomes from our matched credit report sub-sample, with additional credit results summarized in Table 5 and Appendix E. We find substantial contagion in default that lasts 2 to 3 years, but a limited effect of foreclosure on new borrowing.

Panel 7a shows a marked decline in the number of active first mortgage loans for owners.[21] Foreclosure causes a decline of 0.42 mortgages by year 2, which then mean-reverts partially, roughly in line with our homeownership results. This makes sense; the presence of a first mortgage is often used as a proxy for homeownership, which we already saw declines precipitously by a similar magnitude. Panel 7b shows that an increase in defaults on first mortgages primarily drives this. The probability a homeowner has ever defaulted on a first mortgage rises to 0.2 by year 2 before leveling off. The new mortgage margin matters less: The Appendix shows that the number of new mortgages opened falls modestly by 0.07 mortgages in year 2, but this is not statistically significant.

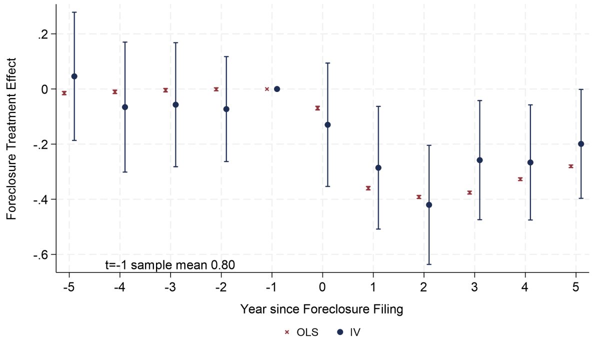
The last three panels demonstrate that foreclosure causes contagion, with defaults on other forms of credit beyond first mortgages. Panel 7c shows that the probability a household has ever defaulted on a home equity loan rises by 0.18 in year 1. Panel 7d shows that the share of non-mortgage accounts in default each year, which includes credit cards, student loans, auto loans, and other non-collateralized debt, peaks at 0.2 in year 2. Finally, Panel 7e shows the share of accounts in default for credit cards, which account for most of the defaults in year 1. The Appendix shows that at longer horizons, auto loans and student loans matter more, but the results are not statistically significant.

We do not see significant effects on credit scores or on new borrowing. Panel 7f shows that a homeowner's credit score falls by 22 points in year 1, which is significant at the 10% level, after which it mean-reverts. This is a relatively small effect economically. Furthermore, the Appendix shows that across essentially all types of debt, there is almost no statistically or economically

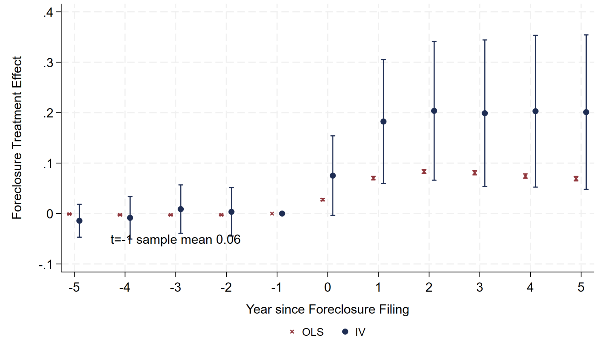
[20]Households may have an incentive to file for bankruptcy after a foreclosure to eliminate a deficiency balance or second lien debts.

[21]Mortgages are categorized as "first mortgage," or "home equity mortgage or line" by the credit bureau. The first mortgage category likely includes the primary mortgage being foreclosed upon, but may also encompass other mortgages, such as primary mortgages on different properties. All secondary mortgages are categorized as home equity.

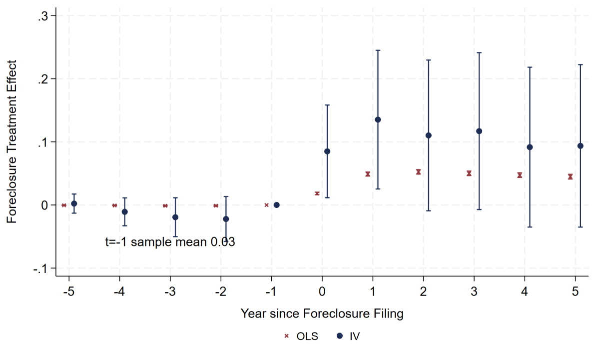
Figure 7: Owners: Financial Outcomes



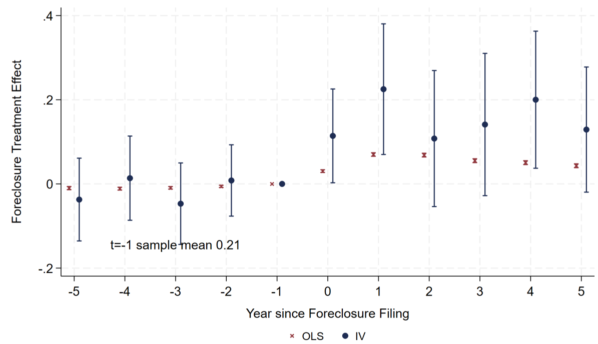
(a) Number of Active First Mortgages



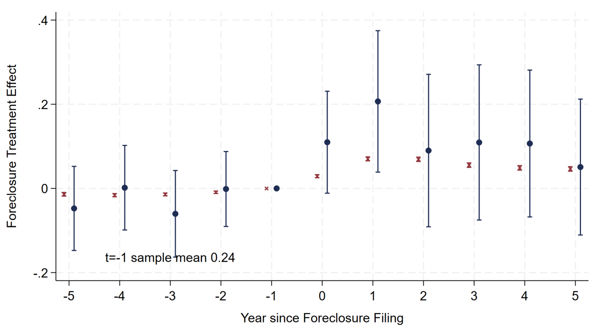
(b) Ever Defaulted on a First Mortgage



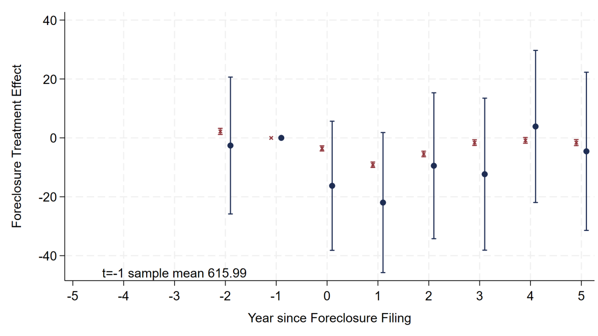
(c) Ever Defaulted on a Home Equity Loan



(d) Share of Non-Mortgage Accounts in Default



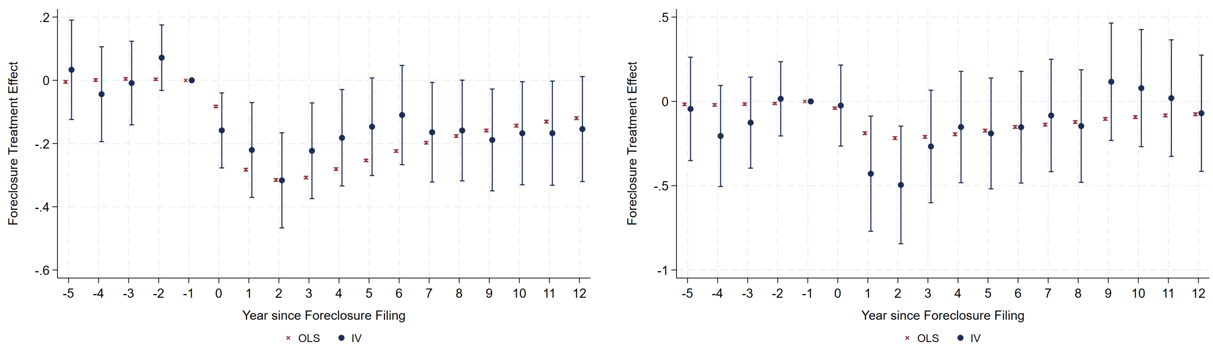
(e) Share of Credit Cards in Default



(f) Vantage Score

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

Figure 8: Long-Term Impact on Having a First Mortgage for Owners and Landlords



(a) Owners

(b) Landlords

Notes: Each panel shows IV and OLS results for the indicated outcome variable for the indicated group. 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

significant effect on new borrowing. This limited impact reflects the fact that the major decline in credit scores and borrowing occurs at *delinquency*, which affects both the treatment and control groups, rather than the judge's decision to foreclose. Indeed, we observe significant declines in new borrowing in both the treatment and control groups. This is also consistent with the fact that credit report data is of much higher quality for payments and delinquency than for actual foreclosure.

5.5 Long Term Effect on Homeownership

Our credit report data continues through 2024, allowing us to examine homeownership, proxied by having a first mortgage, up to 12 years after the foreclosure filing.[22] Figure 8a shows a peak decline in year 2 of 31.6 percentage points, which is a smaller decline than the version built from deeds records in Figure 4c. The point estimate rebounds after a few years, then falls again, but remains statistically significantly negative for most years out to year 12, with a 14-20 percentage-point decline in homeownership. OLS shows a similar decline, with smoother, more consistent mean reversion, but still a 12 percentage-point decline in homeownership at year 12. These findings are consistent with Artigue et al. (2025), as discussed below

[22] Infutor changed the way it reported data, making it difficult to go beyond 2017 for non-credit-report outcomes. We do not show long-term results for other financial outcomes because the outcomes stabilize before 5 years post-filing and are not interesting in years 6 to 12.

5.6 Comparing IV and OLS: Evidence of Selection Bias

Comparing OLS and IV sheds light on selection and bias. We find that OLS and IV are remarkably consistent (albeit with slightly different time paths) for some outcomes, such as moving, homeownership, and the number of mortgages. For others, such as neighborhood quality and divorce, we find no or small and positive effects in OLS but substantial adverse effects with IV. A third group, which includes bankruptcy, credit scores, and financial contagion, exhibits modest adverse effects in OLS and substantially worse effects in IV.

The lack of downward bias in OLS for some outcomes may be surprising given the evidence of selection and the costliness story presented previously. However, the outcomes for which we observe little to no bias are the more "mechanical" outcomes: All households lose their homes, have one fewer active mortgage, and are more likely to move due to foreclosure. On these dimensions, there is little scope for heterogeneity in foreclosure costs that would cause some households to fight the case more vigorously, so selection bias is limited under the costliness story, and OLS and IV are similar. By contrast, households in a better neighborhood have far more to lose in terms of neighborhood quality than households in a bad neighborhood, which opens the door to significant bias under the costliness story. In other words, limited bias for mechanical outcomes and significant bias for non-mechanical outcomes is *exactly* what one would expect to observe under the costliness story.

This heterogeneous bias across outcomes clarifies the differences between our findings and the existing literature. For moving and homeownership, our results are generally consistent with the existing literature. For instance, Molloy and Shan (2013) finds that foreclosure increases the probability of moving by 0.23 to 0.33 after 2 years, depending on the specification; we find an effect of 0.19. They find an effect of 60 to 70 percentage points, which is larger than the 48 percentage points we find, but this is based on a comparison of foreclosed and non-foreclosed groups. In contrast, we control for many more fixed effects in our specification. Our magnitudes are closer to Artigue et al. (2025), who find an effect of 36 percentage points at year 3, 30 percentage points at year 6, and 20 percentage points at year 12; we find an impact of 49 percentage points at year 3 and 39 percentage points at year 5 for ownership from the deeds data and an impact of 15 percentage points at 12 years using the presence of a first mortgage in the credit report as a proxy for ownership. However, these papers find no impact on credit, delinquency, or neighborhood quality, whereas we find significant adverse effects. Indeed, these differences between our paper and prior work for non-mechanical foreclosure outcomes likely stem from the fact that the other papers rely on selection on observables, which we argue ignores important selection bias.

Overall, our results indicate that foreclosure imposes significant non-pecuniary costs on homeowners that are far larger than prior estimates.[23] We now turn to landlords to assess whether households that experience a financial loss but not an eviction from their primary home experience similar outcomes.

6 Results: Landlords

We find that landlords experience severe but short-lived financial distress similar to owners. However, unlike owners, landlords show no adverse effects on divorce or neighborhood quality – if anything, they move to *better* neighborhoods. We present pooled results for landlords in columns 3 and 4 of Table 5, and Figure 9 shows results for each year relative to foreclosure for selected outcomes. We relegate figures for the other outcomes, which lack statistical power, to Appendix E.

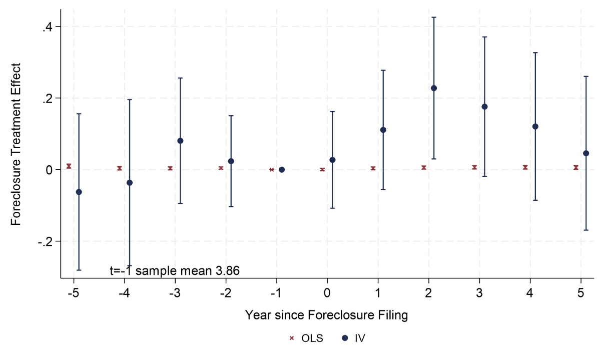
6.1 Non-Financial Outcomes

Landlords move to better neighborhoods, as shown in panels (a) and (b) of Figure 9. Their neighborhoods have 15 percent higher income in years 3 to 4, although this is insignificant, and elementary school test scores rank 15.1 percentiles higher (significant at the 5 percent level). The Appendix shows that other neighborhood quality measures are generally consistent with improvement, though none are statistically significant, and middle and high school test scores point in the opposite direction but are insignificant. These findings indicate that landlords can escape debt overhang that may prevent moves to better neighborhoods by shedding investment properties in foreclosure.

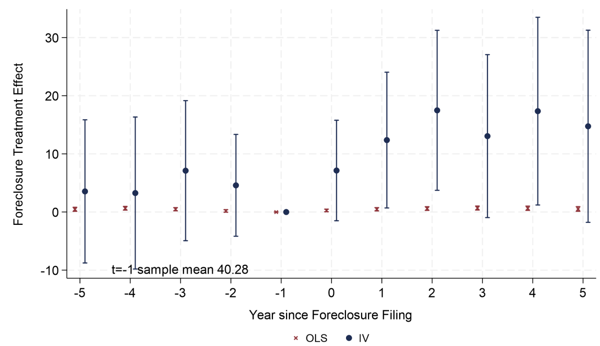
The Appendix shows additional results. Despite not being evicted from the foreclosed property, landlords are more likely to move, though the difference is statistically insignificant. There is also a modest, insignificant increase in homeownership in the short run. Beyond this, the results do not show clear post-foreclosure trends. Figure 8b shows a statistically significant decline in

[23] It is challenging to put a dollar figure on the costs we find because many of these outcomes do not have clear social welfare implications. For instance, it may be that divorces caused by foreclosure enhance social welfare (they should have occurred but were prevented by shelter concerns), or they could reduce social welfare (foreclosure destabilizes an otherwise healthy marriage).

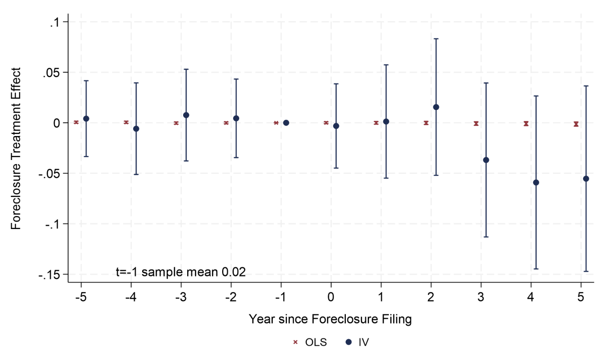
Figure 9: Landlords: Selected Outcomes



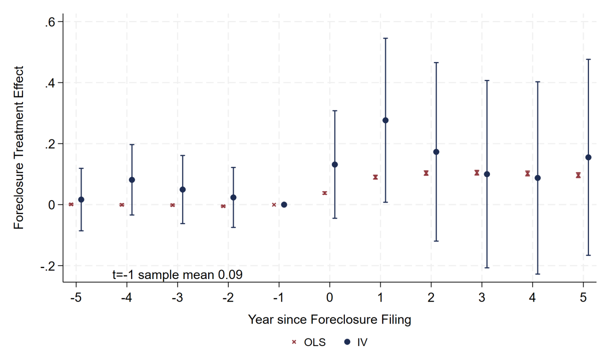
(a) Log ZIP Code Average Income



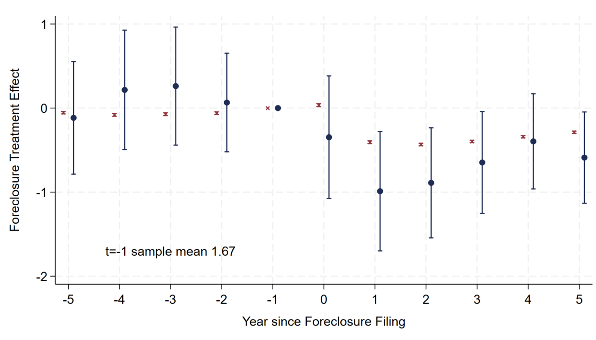
(b) Elementary School Test Score Rank



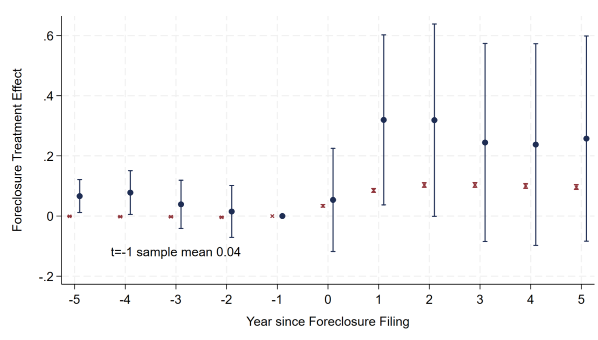
(c) Cumulative Number of Divorces



(d) Ever Had a Bankruptcy (Credit Report)



(e) Number of Active First Mortgages



(f) Ever Defaulted on a First Mortgage

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS 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. For test scores, the dependent variable is the average percentile rank of the local school in math and reading (a coefficient of 1 indicates a 1-percentage-point change in the average rank). The Illinois Board of Education reports these percentages for math and reading separately, and we combine them into a single average index. "Ever had a Bankruptcy" indicates whether there has been a bankruptcy from year -5 through the stated year.

having a mortgage in years 1 and 2. However, this rebounds quickly, and by year 4 and beyond, it is no longer significantly negative. Any impact of foreclosure on landlord homeownership is thus far more short-lived than it is for owners, for whom we found a long-term homeownership scar 12 years later.

Panels (c) and (d) show results for personal outcomes. The effect on divorce is near zero in the first few years, then declines to a negative, though not statistically significant, effect. Bankruptcy increases sharply in years 0 and 1, but the effect plateaus and is no longer statistically significant by years 3 to 4, suggesting foreclosure accelerates bankruptcy for landlords but does not increase its long-run likelihood.

Appendix E compares IV and OLS for landlords. The patterns of bias differ from owners, but this is because outcomes that are mechanical for owner-occupants, such as owning and moving, are *not* mechanical for landlords who do not live in the foreclosed-upon property. Once one adjusts which outcomes are mechanical and which are not, our results are consistent with the costliness story.

6.2 Financial Outcomes

Beyond the spike in bankruptcy, we see additional evidence of short-run financial distress for landlords as shown in panels (e) and (f). Landlords default on multiple investment properties when one is foreclosed upon due to a strict judge. Panel 9e shows a sharp decline in the number of first mortgages in year 1, falling by nearly one mortgage before mean-reverting to 0.3 by year 4. This far exceeds the 0.42-mortgage decline for owners, which largely reflects the mechanical loss of the foreclosed mortgage, suggesting landlords experience cascading defaults across multiple investment properties. Panel 9f shows that the probability of having ever defaulted on a mortgage rises to just above 0.3 for landlords, relative to 0.2 for owners. Together, Panels 9e and 9f suggest that landlords are both more likely to default and default on several properties when they lose a foreclosure case on one property.

The Appendix shows some evidence of short-term financial distress beyond primary mortgages. We see a slight increase in default share on non-mortgage accounts that is significant at the 10 percent level in years 0 and 1. This is primarily driven by a statistically significant increase in the short-run share of credit cards in default. As with first mortgages, this financial distress mean reverts more quickly for landlords and is concentrated in years 0 to 2. Landlord credit scores are not significantly affected in the short run, as is the case with owners. By year 4, however, landlords' credit scores are higher. This furthers our interpretation that foreclosure causes landlords to shed bad debts and *improve* their financial situation within 3 to 5 years.

Both owners and landlords experience financial losses. The fact that both groups experience short-term financial distress suggests that some of these outcomes are linked to monetary losses. However, the fact that landlords do not experience the other adverse outcomes we observe among owners suggests that these non-financial outcomes are unlikely to be solely due to financial loss.

7 Results: Renters

We find that renter foreclosures do not trigger additional moves but do lead renters to live in significantly lower-income neighborhoods. However, our statistical power is limited by a smaller sample size, greater outcome variance due to the non-automatic nature of eviction, and the possibility that Infutor may miss some moves for low-income renters. Consequently, while we find few other statistically significant adverse impacts of landlord foreclosure, we cannot rule out economically meaningful effects.

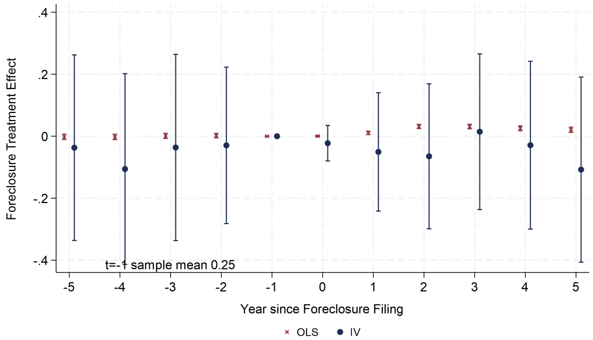
We show pooled results for renters in columns 5 and 6 of Table 5. Figure 10 shows results for each year relative to foreclosure for selected outcomes. We relegate figures for the other outcomes, which lack statistical power, to Appendix E.

7.1 Non-Financial Outcomes

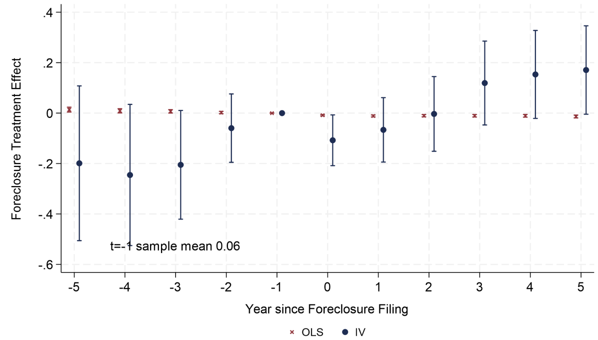
The main adverse impact for renters is a sharp decline in income-based measures of neighborhood quality (Panel 10c). By year 3, average neighborhood income is 9.6 percent lower, comparable to what we observe for owners, though only significant at the 10 percent level. To provide corroborating evidence, we examine other neighborhood quality measures; however, the confidence intervals are too wide to be informative, as shown in the Appendix.

The wide confidence intervals for our other outcomes also make it difficult to determine why we observe such a large decline in neighborhood income. We do not observe a substantial increase in move rates (Panel 10a), suggesting that the neighborhood income decline reflects worse destinations for movers rather than increased displacement. Several mechanisms could explain this decline, including renters accepting worse neighborhoods to transition to homeownership sooner (consistent with Panel 10b), selection bias if landlords in better neighborhoods fight foreclosure harder, and the disruption of sudden forced displacement, which may force renters to accept sub-

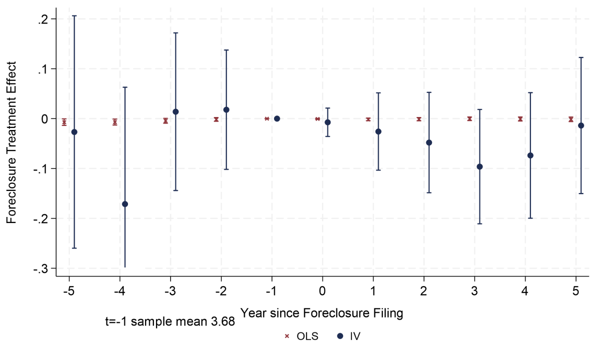
Figure 10: Renters: Selected Outcomes



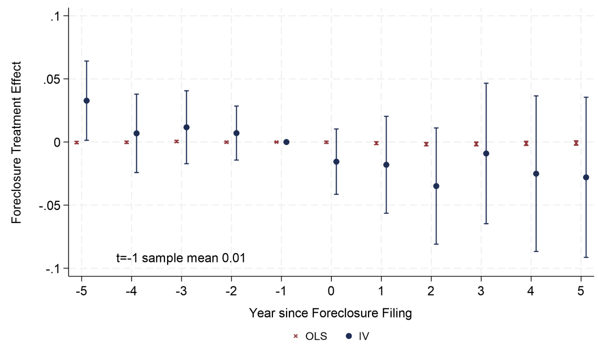
(a) Moved from Foreclosure Address



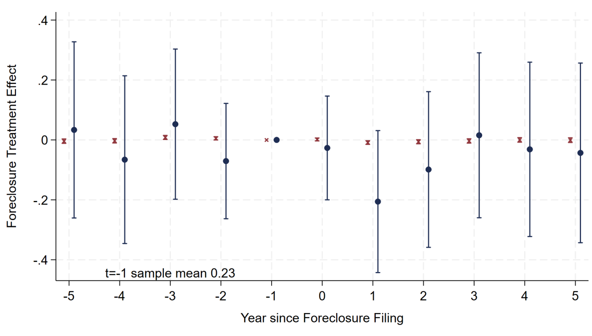
(b) Owns Primary Residence



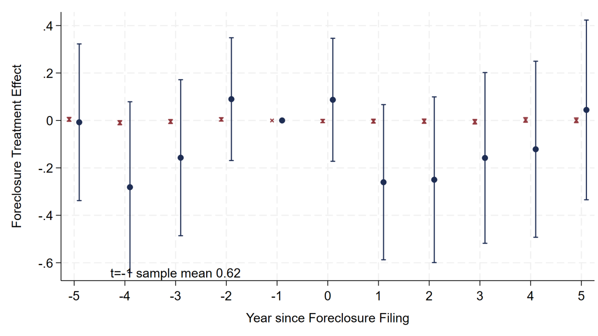
(c) Log ZIP Code Average Income



(d) Cumulative Number of Divorces



(e) Has a First Mortgage Account



(f) Has a Credit Card

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS 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.

optimal housing matches.

To our knowledge, our paper is the first to analyze outcomes for renters after their landlords have been foreclosed upon. The closest paper is Collinson et al. (2024), which examines the causal impact of renter eviction using random-judge assignment. They find significant effects on moving, homeless shelter usage, earnings, financial health, and health, but no effect on neighborhood quality. However, their setting differs fundamentally: They study renters evicted for their own non-payment, while we examine renters displaced by their landlord's distress. The former involves both housing loss and the renter's own financial crisis, while the latter represents an exogenous housing shock to potentially financially stable renters. This difference may explain why Collinson et al. find effects on earnings and health but not neighborhood quality, whereas we observe neighborhood effects but no significant financial or health impacts. While our results provide suggestive evidence of substantial effects on renter neighborhood income, the wide confidence intervals and data limitations indicate that a more comprehensive analysis of landlord foreclosure impacts on renters is an important avenue for future research.

Divorce rates among renters do not increase following a foreclosure, but instead decline slightly (Panel 10d), in sharp contrast to the significant increase we observed among owners.

7.2 Financial Outcomes

We observe a decline in having a mortgage (Panel 10e), mirroring the trend in Panel 10b. However, as with owning a primary residence in the deeds data, this is insignificant and short-lived. There is also a statistically insignificant but meaningful decline in having a credit card (Panel 10f). Beyond this, we see few significant financial effects for renters, and the wide confidence intervals make most results uninformative. The lack of clear financial impacts is consistent with the fact that renters are not the borrowers being foreclosed upon.

8 Conclusion

Using rich new data and leveraging random judge assignment in foreclosure cases, we provide new estimates of the causal impact of foreclosure on owners, landlords, and renters in the five years following a foreclosure filing. Owners who lose both their residence and their wealth experience substantial and persistent declines in neighborhood quality and homeownership, elevated divorce rates, and short-term financial distress. In contrast, landlords who suffer financial losses but retain their homes show short-term financial distress but, if anything, move to better neighborhoods. Renters whose landlord experiences a foreclosure move to lower-income neighborhoods, but our analysis of most other outcomes for renters is inconclusive due to limited statistical power. Overall, our findings provide comprehensive causal evidence on foreclosure's far-reaching consequences.

The contrast between our results for owners, landlords, and tenants helps shed light on the mechanisms underlying foreclosure. In particular, the evidence of financial distress among owners and landlords suggests that some of the negative financial costs of foreclosure stem from the loss of assets and credit. However, we observe non-financial costs for owners, such as elevated divorce rates, that are absent for landlords and renters. This suggests that the *combination* of eviction and a financial shock — rather than either on its own — explains the particularly sizable adverse non-financial effects for owners.

Our IV estimates also reveal substantial bias in conventional OLS approaches. For "mechanical" outcomes like moving and homeownership, IV and OLS align. But for "non mechanical" outcomes such as neighborhood quality and family stability — where households facing higher costs fight foreclosure more vigorously — OLS shows no effects or even slight improvements, while IV reveals significant adverse effects. This explains why prior studies using non-experimental methods found limited non-financial costs: Selection bias masked the true impact of foreclosure.

Our results suggest that conventional estimates that focus purely on financial costs significantly understate the social cost of foreclosure, particularly for foreclosed-upon owners. This alters the cost-benefit analysis of foreclosure mitigation programs and other forms of housing market support during a downturn. Because more lenient judges are more likely to push for loan modifications or payment plans, our results are particularly relevant for policies that seek to prevent foreclosures among delinquent borrowers through loan modifications, such as HAMP during the Great Recession. We leave a complete welfare analysis that monetizes these effects for future research.

Our results also help explain the significant utility costs of foreclosures in models of household default, which until now had been difficult to attribute to anything other than moral costs (Ganong and Noel, 2023; Laufer, 2018; Bhutta et al., 2017; Kaplan et al., 2020; Guren et al., 2021). Instead, the costs of foreclosure on households appear to be real and substantial.

References

AGARWAL, S., G. AMROMIN, I. BEN-DAVID, S. CHOMSISENGPHET, T. PISKORSKI, AND A. SERU (2017): "Policy Intervention in Debt Renegotiation: Evidence From the Home Affordable Modification Program," *Journal of Political Economy*, 125, 654–712.

AGARWAL, S., G. AMROMIN, S. CHOMSISENGPHET, T. LANDVOIGT, T. PISKORSKI, A. SERU, AND V. YAO (2023): "Mortgage Refinancing, Consumer Spending, and Competition: Evidence From the Home Affordable Refinance Program," *The Review of Economic Studies*, 90, 499–537.

AIZER, A. AND J. DOYLE, JOSEPH J. (2015): " Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges," *The Quarterly Journal of Economics*, 130, 759–803.

ANDERSON AND ASSOCIATES (2015): "Deciding Which Spouse Keeps the Family Home in a Divorce," https://www.andersondivorcelawchicago.com/chicagodivorceattorney/2015/04/10/family-home-after-divorce/, Accessed July 2019.

ANENBERG, E. AND E. KUNG (2014): "Estimates of the Size and Source of Price Declines Due to Nearby Foreclosures," *American Economic Review*, 104, 2527–51.

ARTIGUE, H., P. BAYER, F. V. FERREIRA, AND S. ROSS (2025): "Does Homeownership Matter? The Long-Term Consequences of Losing a House During the Great Recession," NBER Working Paper #33692.

BALD, A., E. CHYN, J. HASTINGS, AND M. MACHELETT (2022): "The Causal Impact of Removing Children from Abusive and Neglectful Homes," *Journal of Political Economy*, 130, 1919–1962.

BARCA, A., L. SANTUCCI, AND L.-A. SCHULTZ (2022): "Foreclosure Kids: Examining the Early Adult Credit Usage of Adolescents Affected by Foreclsoure," Working Paper, Federal Reserve Bank of Philadelphia.

BEEN, V., I. G. ELLEN, D. FIGLIO, A. A. NELSON, S. L. ROSS, A. E. SCHWARTZ, AND L. STIEFEL (2025): "The Effects of Housing Price Declines on Children's Educational Outcomes," *Education Finance and Policy*, 20, 312–343.

BEEN, V., I. G. ELLEN, A. E. SCHWARTZ, L. STIEFEL, AND M. WEINSTEIN (2011): "Does Losing Your Home Mean Losing Your School?: Effects of Foreclosures on the School Mobility of Children," *Regional Science and Urban Economics*, 41, 407–414.

BENNETT, G. G., M. SCHAROUN-LEE, AND R. TUCKER-SEELEY (2009): "Will the Public's Health Fall Victim to the Home Foreclosure Epidemic?" *PLOS Med*, 6, e1000087.

BHULLER, M., G. B. DAHL, K. V. LØKEN, AND M. MOGSTAD (2020): "Incarceration, Recidivism, and Employment," *Journal of Political Economy*, 128, 1269–1324.

BHUTTA, N., J. DOKKO, AND H. SHAN (2017): "Consumer Ruthlessness and Mortgage Default During the 2007 to 2009 Housing Bust," *The Journal of Finance*, 72, 2433–2466.

BREVOORT, K. P. AND C. R. COOPER (2013): "Foreclosure's Wake: The Credit Experiences of Individuals Following Foreclosure," *Real Estate Economics*, 41, 747–792.

CAMPBELL, J. Y., S. GIGLIO, AND P. PATHAK (2011): "Forced Sales and House Prices," *American Economic Review*, 101, 2108–31.

CHARLES, K. K. AND M. STEPHENS, JR. (2004): "Job Displacement, Disability, and Divorce," *Journal of Labor Economics*, 22, 489–522.

CHETTY, R., R. DIAMOND, T. B. FORSTER, L. KATZ, S. R. PORTER, M. STAIGER, AND L. TACH (2026): "Creating High-Opportunity Neighborhoods: Evidence from the HOPE VI Program," Working Paper, Harvard University.

CHETTY, R., J. N. FRIEDMAN, N. HENDREN, M. R. JONES, AND S. R. PORTER (2025): "The Opportunity Atlas: Mapping the Childhood Roots of Social Mobility," *American Economic Review*, Forthcoming.

COLLINSON, R., J. E. HUMPHRIES, N. MADER, D. REED, D. TANNENBAUM, AND W. VAN DIJK (2024): "Eviction and Poverty in American Cities," *The Quarterly Journal of Economics*, 139, 57–120.

COOK COUNTY CLERK OF THE CIRCUIT COURT (2005-2016): "Chacery Court Judgements," Scraped from https://www.cookcountyclerkofcourt.org/ in Aug, 2017.

COOK COUNTY GOVERNMENT (2025): "Single and Multi-Family Improvement Characteristics," Cook County Assessor's Office, https://datacatalog.cookcountyil.gov/Property-Taxation/Assessor-Single-and-Multi-Family-Improvement-Chara/x54s-btds/about_data (accessed on Sep 30, 2025).

CORELOGIC (2020a): "Corelogic Deed File," Purchased from CoreLogic.

——— (2020b): "Corelogic Property History," Purchased from CoreLogic.

——— (2020c): "Loan-Level Mortgage Analytics," Provided by CoreLogic.

CURRIE, J. AND E. TEKIN (2015): "Is There a Link Between Foreclosure and Health?" *American Economic Journal: Economic Policy*, 7, 63–94.

DAHL, G. B., A. R. KOSTØL, AND M. MOGSTAD (2014): " Family Welfare Cultures," *The Quarterly Journal of Economics*, 129, 1711–1752.

DATAQUICK (2012): "DataQuick Deeds," Purchased from Dataquick.

DE GIORGI, G. AND C. NAGUIB (2024): "Life After (Soft) Default," *European Economic Review*, 167, 104793.

DIAMOND, R., T. MCQUADE, AND F. QIAN (2019): "The Effects of Rent Control expansion on Tenants, Landlords, and Inequality: Evidence From San Francisco," *American Economic Review*, 109, 3365–3394.

DOBBIE, W., J. GOLDIN, AND C. S. YANG (2018): "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence From Randomly Assigned Judges," *American Economic Review*, 108, 201–40.

DOBBIE, W. AND J. SONG (2015): "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection," *American Economic Review*, 105, 1272–1311.

DOYLE, JOSEPH J., J. (2007): "Child Protection and Child Outcomes: Measuring the Effects of Foster Care," *American Economic Review*, 97, 1583–1610.

FRANDSEN, B., L. LEFGREN, AND E. LESLIE (2023): "Judging Judge Fixed Effects," *American Economic Review*, 113, 253–277.

FRENCH, E. AND J. SONG (2014): "The Effect of Disability Insurance Receipt on Labor Supply," *American Economic Journal: Economic Policy*, 6, 291–337.

GABRIEL, S., M. IACOVIELLO, AND C. LUTZ (2021): "A Crisis of Missed Opportunities? Foreclosure Costs and Mortgage Modification During the Great Recession," *The Review of Financial Studies*, 34, 864–906.

GANONG, P. AND P. NOEL (2020): "Liquidity versus Wealth in Household Debt Obligations: Evidence From Housing Policy in the Great Recession," *American Economic Review*, 110, 3100–3138.

——— (2023): "Why Do Borrowers Default on Mortgages?" *The Quarterly Journal of Economics*, 138, 1001–1065.

GERARDI, K., E. ROSENBLATT, P. S. WILLEN, AND V. YAO (2015): "Foreclosure Externalities: New Evidence," *Journal of Urban Economics*, 87, 42–56.

GIBBS, C., B. GUTTMAN-KENNEY, D. LEE, S. NELSON, W. VAN DER KLAAUW, AND J. WANG (2025): "Consumer Credit Reporting Data," *Journal of Economic Literature*, 63, 598–636.

GUPTA, A. (2019): "Foreclosure Contagion and the Neighborhood Spillover Effects of Mortgage Defaults," *The Journal of Finance*, 74, 2249–2301.

GUREN, A. AND T. MCQUADE (2020): "How Do Foreclosures Exacerbate Housing Downturns?" *Review of Economic Studies*, 87, 1331–1364.

GUREN, A. M., A. KRISHNAMURTHY, AND T. J. MCQUADE (2021): "Mortgage Design in an Equilibrium Model of the Housing Market," *The Journal of Finance*, 76, 113–168.

HARDING, J. P., E. ROSENBLATT, AND V. W. YAO (2009): "The Contagion Effect of Foreclosed Properties," *Journal of Urban Economics*, 66, 164–178.

HOULE, J. N. (2014): "Mental Health in the Foreclosure Crisis," *Social Science & Medicine*, 118, 1–8.

ILLINOIS STATE BOARD OF EDUCATION (2000-2016): "The Directory of Educational Entities," Downloaded from https://www.isbe.net/pages/data-analysis-directories.aspx (accessed May 7, 2020).

——— (2018): "The Illinois Report Card," Downloaded from https://www.isbe.net/reportcard (accessed May 8, 2019).

IMMERGLUCK, D. AND G. SMITH (2006): "The External Costs of Foreclosure: The Impact of Single-Family Mortgage Foreclosures on Property Values," *Housing Policy Debate*, 17, 57–79.

INFUTOR DATA SOLUTIONS, LLC (2016): "Consumer Reference Database," Purchased from Infutor.

INTERNAL REVENUE SERVICE (1998-2016): "Individual Income Tax Statistics," Downloaded from https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi (accessed Nov 28, 2017).

KAPLAN, G., K. MITMAN, AND G. L. VIOLANTE (2020): "The Housing Boom and Bust: Model Meets Evidence," *Journal of Political Economy*, 128, 3285–3345.

KEYS, B. J., T. PISKORSKI, A. SERU, AND V. VIG (2012): "Mortgage Financing in the Housing Boom and Bust," in *Housing and the Financial Crisis*, University of Chicago Press, 143–204.

KLING, J. R. (2006): "Incarceration Length, Employment, and Earnings," *American Economic Review*, 96, 863–876.

KOLESAR, M. (2013): "Estimation in an Instrumental Variables Model with Treatment Effect Heterogeneity," Working Paper, Princeton University.

LAUFER, S. (2018): "Equity Extraction and Mortgage Default," *Review of Economic Dynamics*, 28, 1–33.

LAW OFFICES OF SCHLESINGER AND STRAUSS (2017): "Trust in an Illinois Divorce," https://illinois-family-lawyer.com/blog/property-division/trusts-illinois-divorce/, Accessed July 2019.

MAESTAS, N., K. J. MULLEN, AND A. STRAND (2013): "Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt," *American Economic Review*, 103, 1797–1829.

MIAN, A., A. SUFI, AND F. TREBBI (2015): "Foreclosures, House Prices, and the Real Economy," *The Journal of Finance*, 70, 2587–2634.

MIRABELLA, KINCAID, FREDERICK, & MIRABELLA, LLC (2015): "Division of Property Upon Divorce: The Impact of a Trust," https://www.mkfmlaw.com/dupageattorney/division-of-property-upon-divorce-trusts, Accessed July 2019.

MOLLOY, R. AND H. SHAN (2013): "The Postforeclosure Experience of US Households," *Real Estate Economics*, 41, 225–254.

MUELLER-SMITH, M. (2015): "The Criminal and Labor Market Impacts of Incarceration," Working Paper, University of Michigan.

MUNROE, D. J. AND L. WILSE-SAMSON (2013): "Foreclosure Contagion: Measurement and Mechanisms," Working Paper, Columbia University.

NELSON, P., M. HULSTEIN, AND T. WALSH (2014): "Mortgage Foreclosure Defense," http://www.cvls.org/sites/all/files/seminars/foreclosuredefense_053014/foreclosure_defense.pdf, Accessed July 2019.

NORRIS, S., M. PECENCO, AND J. WEAVER (2021): "The Effects of Parental and Sibling Incarceration: Evidence from Ohio," *American Economic Review*, 111, 2926–63.

PISKORSKI, T. AND A. SERU (2021): "Debt Relief and Slow Recovery: A Decade After Lehman," *Journal of Financial Economics*, 141, 1036–1059.

Pollack, C. E. and J. Lynch (2009): "Health Status of People Undergoing Foreclosure in the Philadelphia Region," *American Journal of Public Health*, 99, 1833–1839.

Record Information Services (1998–2017): "Court Cases Dataset," Purchased from Record Information Services, accessed Sep 8, 2018.

Tsai, A. C. (2015): "Home Foreclosure, Health, and Mental Health: A Systematic Review of Individual, Aggregate, and Contextual Associations," *PLOS One*, 10, e0123182.

U.S. Department of Housing and Urban Development (2010): "Economic Impact Analysis of the FHA Refinance Program for Borrowers in Negative Equity Positions," Washington DC.

Appendices

For Online Publication Only

A Data Construction

A.1 Overview of Construction of Sample

A.1.1 Overview of Data Sources

This project uses data from several sources:

- Record Information Services (RIS): RIS is a company that aggregates data from many sources for the Chicago area. We obtain several RIS datasets:
  - Court cases from 1998 to 2017: For each court case, this dataset contains the case number, the assessor's parcel number(s) (APN) of the property, defendant names, property address, time-varying property type (such as condominium, apartment, single-family home, etc), and some basic mortgage characteristics like loan amount.
  - Individual-level data: Divorce.
- Court cases from the Cook County Clerk of the Circuit Court (CC): We scrape information on each court case from 1998 to 2016 from the court's website. Information includes the case number, plaintiff and defendant names, case filing date, and case calendar. Each case has a list of judgments, and each item in the list contains the judgment itself, the judge's name, and the date.
- Infutor: Address histories for individuals who lived in Cook County, as well as name, age, gender, imputed race, and imputed immigration status.
- CoreLogic: Tax assessor and deeds data used to identify transactions, refinancings, and loan modifications, the owner of the property, the address, the square footage, and other characteristics. We also use similar data from DataQuick at times. DataQuick is a vendor that sold the same data and was bought by CoreLogic.
- Credit Report Data: Account-level credit records provided by a credit bureau, from which we create information on bankruptcy, the number of loans, delinquency, default, and collections status, and individual-level data on credit score.
- IRS Statistics of Income Tax Stats: Average annual income by ZIP code.
- Illinois State Board of Education: Elementary, middle, and high school test scores.
- CoreLogic Loan-Level Mortgage Analytics (LLMA) servicing data: This data provides a more detailed picture of payments on the primary mortgage, but it is only available for a smaller subset of our data. We use it to assess bias.
- Property Characteristics From Cook County Government Open Data (Cook County Government, 2025): This data provides the total number of units in a property, which is used to adjust the square footage of multi-family properties.

A.1.2 Data Cleaning

1. Clean RIS:
   - Deduplicate cases by keeping the latest input date for each case, which results in a unique observation for each case number.
   - Drop all cases that map to multiple APNs.
   - Standardize RIS defendant addresses using the method described in Section A.5.
   - Some RIS cases have missing APNs. We use case refilings to recover 158 observations from the full 1998-2017 RIS dataset by replacing missing APNs with those from other cases with the same defendant name and address.
   - Some RIS cases have APNs with typos. We replace roughly 5,000 of these using the APN from DataQuick.

2. Clean Cook County court cases (CC): First, we categorize cases into whether they are mortgage foreclosures, as detailed in Section A.3.1, and filter to only the mortgage foreclosure cases.
3. Merge RIS and CC using the court case number.
4. Constrain to the set of cases that have residential property types in RIS, which are single-family homes, condominiums, townhomes, and apartment buildings.
5. Drop cases for which we could not identify the foreclosure outcome. See Section A.3.2 for details.
6. Clean DataQuick Assessor data by dropping observations where multiple properties map to 1 APN or vice versa, which is only 2% of the data. This results in a dataset with unique APNs. Then, we standardize the DQ site address as described in Section A.5.
7. Clean CoreLogic deeds and transaction records. We drop all properties that map to multiple APNs, leaving a dataset with unique APNs.
8. Clean Infutor migration histories. Section A.2 explains the steps in detail.
9. Find all people in Infutor who satisfy the criteria for owner, landlord, and renter. These definitions are detailed in Section A.1.3.

A.1.3 Definitions of Owner, Renter, and Landlord

- Owner: An individual is classified as an owner if they appear in Infutor records before the foreclosure month and remain at the property until at least the month before foreclosure. For condominiums, the apartment number must also match. If the apartment number is missing in either Infutor or the foreclosure property address, ownership is assigned if DataQuick reports transactions for only one apartment under the individual's name. The identification process consists of the following steps:
    1. The Infutor and case addresses, city, and state must match, excluding the apartment number. Infutor provides both a standardized and a non-standardized address format; a match with either is sufficient. If no match is found, we relax the criteria by excluding city information and last name, capturing cases where city names are inconsistent across datasets.
    2. For condominiums, we also require the apartment numbers to match. If no exact match is found, we remove special characters and letters from apartment numbers and compare them again. If the apartment number is missing in either dataset, we check DataQuick for multiple apartment transactions under the individual's name. If they are associated with only one apartment, they are classified as an owner.
    3. Last names must either be identical or, if both exceed four characters, one name must be a substring of the other (e.g., "Smith" matches "Smith-Johnson").
- Landlord: Landlords are identified through three methods:
    1. An individual is classified as a landlord if they have lived at the DataQuick mailing address at any point in time. Address matching follows the same process as for owners, first considering full address, city, and state, and later allowing matches based on address and last name only. Both first and last names must match.
    2. We use the CoreLogic assessor's and deed data property tax mailing addresses for the foreclosure year, as well as the years before and after. The addresses are merged with Infutor to identify the residents associated with the mailing addresses. A landlord is classified as a landlord if the first and last name match that of a resident of the mailing address within one month of the foreclosure.
    3. We finally use unique names to match landlords in two ways:
        - First, we identify all individuals who have ever lived in Illinois and have the same first and last name as the main defendant, provided that fewer than ten people meet this criterion (to exclude common names such as "John Smith"). If exactly one such person exists, they are assigned as the landlord.
        - Second, we search for whether there is one person at the same address or ZIP Code. If exactly one person satisfies this condition, they are considered the landlord, since landlords often live near their rental properties.
- Renter: We identify renters only at addresses where we have positively identified a landlord using the above procedure. Renters are identified following the first step of the owner identification process. The apartment-matching procedure is similar, with the key difference that if an apartment number is missing in either dataset, we do not classify the individual as a renter, since ownership transactions cannot be verified. Additionally, the renter's last name must not match the last name in the case data, using the same substring-based matching rule as for owners.

A.2 Cleaning the Infutor Migration Data

The Infutor migration history contains address histories of individuals living in the U.S. from 1990 to 2016. Note that some addresses are from before 1990, but they are sparser. We extract a data set of everyone who has ever lived in Cook County. The cleaning steps are:

- Construct begin and end dates for each address.
  1. Infutor raw text data contains the following variables, all at the monthly level:
     - date_eff: Effective date is the "date of name/complete address combination" according to Infutor documentation. This variable is unique at the person-address level. The effective date is when the individual moved into the address. When Infutor obtains data that indicates an individual has a new address, they enter a new address and an effective date for that person. The effective date for the new address is also the move-out date of the old address. If Infutor never obtains new information about an individual, Infutor assumes that they continue to live at the same address.
     - addnum: This is the address history sequence number, where 1 is the most recent address, and 10 is the earliest address. Not everyone has 10 addresses, and no one has more than 10.
  2. Drop addresses with missing effective dates (drops 5%): We drop all addresses that do not have an effective date. These addresses tend to be the earliest addresses in a person's address history, coming from the beginning of the Infutor address sample (1980s and 1990s).
  3. Re-assign address numbers based on the revised effective date: Since we use effective date as the start of each address, the address numbers do not always sequentially align with the sequence of effective dates, so we re-assign new addnum's based on effective date so that addnum reflects the address sequence accurately.
  4. De-duplicate addresses by effective date (drops about 8%-12%): For a given person, some addresses have the same effective date, so we only need to keep the latest address.
     - Interpreted literally, it means that some people moved into a place and then moved out within the same month.
     - In practice, the same address is repeated twice, so it doesn't matter which one gets dropped.
  5. Create address date range: Use the effective date as the start date of each address. Use the effective date of the next addnum as the address's end date. For the final (most recent) address, the end date is imputed forward to be June 2017. We verify against Census data to determine that this method is the most sensible way to handle addresses without a final end date.
  6. De-duplicate by address id (drop 4%-7%): If a person has 2 or more consecutive addresses with the same Infutor-defined address id, we collapse them into one address and adjust the start and end months accordingly to reflect the expanded date range.
- Standardize addresses: We standardize street addresses and city names using the procedure described in Section A.5.
- Geocode addresses: We geocode every address in the Infutor address histories and link to 2010 Census block groups.

A.3 Cleaning the Cook County Court Data

A.3.1 Identifying Mortgage Foreclosure Cases

For each case, we scrape the list of judgments issued from the Cook County Clerk of the Circuit Court website. For a given case, if the first judgment exactly matches any of the phrases below, then we categorize the case as a mortgage foreclosure. There are 458,412 such cases between 1997 and 2017. The cases we count as foreclosure cases are:

- "mortgage foreclosure complaint filed"
- "owner occupied single family home or condominium - filed"
- "non owner occupied single family home or condominium - filed"
- "commercial, mixed commercial/residential or industrial - filed"
- "multi-unit residential mortgage foreclosure - (7 units or more) - filed"
- "owner occupied six units or less mortgage foreclosure - filed"
- "owner occupied, mixed commercial/residential - (6 units or less) - filed"
- "mortgage foreclosure disposed / sheriffs sale approved"

A.3.2 Identifying the Outcome of Each Case

We use court judgments to identify the outcome for each case and determine whether it results in foreclosure or dismissal. To identify foreclosures, we look for the judgment that says ”order for possession,” which means the owner must give up their house. To identify dismissals, we look for one of the following judgments:

- “case dismissed subject to consent decree - allowed”
- “dismiss by stipulation or agreement”
- “dismiss entire cause”
- “dismissed for want of prosecution”
- “general chancery - voluntary dismissal, non suit, dismiss by agreement”
- “mechanic lien - voluntary dismissal, non-suit, dismiss by agreement”
- “mortgage foreclosure dismissed for want of prosecution”
- “mortgage foreclosure motion defendant dismissed”
- “mortgage foreclosure motion plaintiff dismissed”
- “mortgage foreclosure voluntary dismissal, non-suit or dismiss by agreement”
- “mortgage foreclosure judgment for defendant”
- “voluntary dismissal, non-suit or dismissed by agreement”

In rare cases, judgments are overturned as indicated by an initial foreclosure judgment followed by a dismissal or vice versa. In these cases, we use the most recent judgment. Furthermore, some cases were dismissed and then reinstated (reinstated cases contain the judgment ”reinstate case - allowed -”), so we remove the dismissal categorization for those cases and rely on future judgments to classify the case. For our sample period from 2005 to 2012, this procedure enables us to categorize 97.8% of cases into foreclosure or dismissal. We drop the 6,104 cases that cannot be categorized using this algorithm.

A.3.3 Identifying the Begin and End Dates For Each Case

For each case, the online court record provides the filing date, which we treat as the case's start date. The court does not give an end date, so we assign one for each case based on the judgment date used to determine the outcome.

The main foreclosure outcome variable we use is whether the case results in foreclosure 3 years after the filing date, including case refilings.

A.4 Cleaning the IRS Zip Code Income

IRS contains annual income at the ZIP code level from 1998 to 2016. The data source is https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi. Here are the cleaning steps:

- Clean raw files and aggregate across all years.
- Define income as:
  - For each ZIP code, the files give the number of tax returns for each income bracket and the total Adjusted Gross Income (AGI) for that income bracket.
  - Income for a ZIP code (call it ”AGI”) is the total AGI divided by the total tax returns for each ZIP code in each year.
  - We also calculate ”log AGI” by taking the log of AGI and ”AGI percentile” by ranking ZIP codes by income for each year.
- For a few observations, if there is a ZIP-year duplicate, we keep the observation with the larger value.
- No data are available for 1999, 2000, or 2003, so we interpolate values for those years.

A.5 Address Standardization

We frequently merge addresses from sources such as Infutor, RIS, and DataQuick. We developed the following method to standardize all addresses prior to merging to increase match rates:

- Construct standardized street address variables: The standardized full address (add_std), standardized address without the apartment/unit number (add_std_noaptnum), and the standardized apartment/unit number (add_std_unitval):
    1. Use the Stata command stnd_address to extract the 1) street number and name, 2) PO Box, 3) building number, 4) floor number, and 5) unit number.
    2. Strip "apt" and "unit" strings and hyphens and other punctuation from the unit number so that only alphanumeric characters remain.
    3. Construct the standardized unit number variable add_std_unitval: the unit number is 3) building + 4) floor + 5) unit number. Most addresses only contain one of these fields. Only very rarely are 2 or more populated.
    4. Construct a version of the standardized address with no unit information, called add_std_noaptnum, which 1) street number and street name + 2) PO Box. This variable is useful because sometimes we do not want to require that the apartment numbers match.
    5. Construct a standardized address with unit information, called add_std, which is obtained by combining add_std_noaptnum and add_std_unitval.
- Construct standardized city name, called add_city: We use abbreviations from the USPS and list of military suffixes to unabbreviate any abbreviated city names. For example, "GDN" becomes "GARDEN" and "AFB" becomes "AIR FORCE BASE".

A.6 Cleaning the Credit Report Data

The credit report tradeline data is account-level. We describe how we collapse this data down to person-level aggregates in this section.

Matching Accounts Some accounts report the same loan multiple times, so we de-duplicate by matching loans with identical principal at origination.

Begin and End Date We begin by identifying a start and end date for each account. The credit bureau provides, for each report, the account's last payment date, last active date, official close date, and multiple account status/rating codes. We define the start date as the earliest of the account's report date, payment date, or last active date. We define an account's end date as the earliest of:

1. The later of the last active date + 6 months and the last payment date + 6 months.
2. The report date on which we first observe a collection, bankruptcy, charge-off, repossession, refinance, or foreclosure start code.
3. The last report date + 6 months.
4. The close date reported by the credit bureau. Accounts may be inactive well before it is closed.

This construction keeps an account "alive" as long as there is no clear evidence of default or closure, and it remains active within the past six months. Conversely, accounts with no evidence of recent activity or payments are treated as inactive.

Number of Active Mortgage Accounts After matching accounts and defining the beginning and end dates, we calculate the number of mortgage accounts active t years after foreclosure as the number of accounts with a start date before the foreclosure date and an end date after the foreclosure date plus $365 \times t$ days.

Bankruptcy and Delinquent/Default/Collection Variables We use account status codes to categorize a loan as in "default," which we define as bankruptcy, delinquency, default, or collection. These include codes 2-5: "Past due"; 6: "Collection account"; 7: "Included in Chapter 13"; 8: "Repossession"; 9: "Charged-off"; G: "Foreclosure process started"; M: "Included in Chapter 13 - retired"; S: "Dispute - resolution pending"; Z: "Included in Bankruptcy". We create an indicator for whether we ever observe any of these codes for each account type (Auto Lease/Loan, Credit Cards, Home Equity Lines or Loans, Student Loans, and all non-mortgage accounts) separately for each account. We do not restrict these observations to reports dated before the account's end date defined above. Because these status codes themselves cause the account to be treated as inactive, restricting to "active" reports would prevent us from observing the relevant default events.

We then create two variables for each account type:

- An indicator for whether any accounts in this category have ever been in default since five years before the foreclosure filing. This transitions from 0 to 1 upon the first occurrence of a bankruptcy, delinquency, default, or collection code.

- The share of accounts in the most recent report that are in default. That is, the numerator is the number of accounts with a bankruptcy, delinquency, default, or collection code in the last report in a 12-month period, and the denominator is the total number of reporting accounts open over the same period. The 12 months are measured from the time of the foreclosure filing.

We also construct a dummy variable indicating that at least one account of each type is active in each 12-month period and use it as an outcome when appropriate. Finally, with the same method, we create an additional indicator for whether an individual has ever had a bankruptcy on their credit record within five years of the foreclosure filing.

Vantage Score We use individual-level "Vantage40" variable, which is available since 2008.

A.7 Cleaning and Matching the LLMA Servicer Data

Table 2 regresses various proxy variables on foreclosure to assess selection and which story explains the selection we observe. Many of the proxy variables are from the CoreLogic LLMA monthly mortgage servicing data. In this section, we describe how we match the LLMA data to the RIS foreclosure case data, which form the backbone of our analysis.

The LLMA data for Illinois covers a subset of mortgages for which servicers share data with CoreLogic from 2004 to 2019. In the LLMA data, we can see the mortgage's "delinquency status" each month: current, 30 days delinquent, 60 days delinquent, 90 days delinquent, in foreclosure, real estate owned, sold, paid off, or status unknown. A foreclosure start in the mortgage servicing data (as opposed to a foreclosure filing in the court data) is defined as the first month in which the mortgage delinquency status is in foreclosure.

We match the RIS and LLMA data using a fuzzy merge process. We first take all the LLMA mortgages originated in Illinois and match them to the RIS filings based on mortgage amount at origination, month-year of loan origination, and property ZIP code. This initial match is many-to-many, allowing each LLMA mortgage to match multiple RIS filings and each RIS filing to match multiple LLMA mortgages. We then drop matches that (1) differ by more than two years in the original term length of the mortgage or (2) the foreclosure filing date does not fall within the period after the LLMA foreclosure start and before the LLMA foreclosure completion or cure. These two criteria ensure that most matches are one-to-one; after that, we drop any foreclosure cases that link to multiple different LLMA loans. Since not all mortgages are in the LLMA data, and some merges do not link to a unique mortgage in the LLMA data, we have a significantly smaller dataset of LLMA variables. We never co-mingle any CoreLogic or DataQuick deeds or assessor data with the LLMA data.

A.8 Constructing Residence Ownership and Square Footage Variables

To identify homeownership and the size of individuals' residences, we merge Infutor address records with CoreLogic deed records and compare the last names of Infutor residents with the listed property owners. We then construct two variables:

- Indicator for owning the residential property
  - An individual is considered to own a property from the recorded purchase date until the recorded sale date.
  - If a purchase record is observed but no sale record appears, we assume the individual continues to own the property through the end of our data, unless tax records indicate that they no longer owned the property as of 2016. In that case, we assign an imputed sale date of 2015.
  - If a sale record appears for an individual without a corresponding purchase record, we assume they purchased the property before 1975, the earliest year covered by the data.
- Square footage of the residential property
  - Building square footage is obtained from 2017 tax records via CoreLogic. We assign square footage only to years after the building's most recent construction, resulting in roughly 1 percent of property-year observations missing square footage because they occur before the latest reconstruction.
  - We calculate unit-level square footage by dividing the building's total square footage by the number of units in the building, using building-unit counts from the Cook County Government Open Data portal.[24]

[24] https://datacatalog.cookcountyil.gov/Property-Taxation/Assessor-Single-and-Multi-Family-Improvement-Chara/x54s-btds/about_data

A.9 Constructing School Quality Metrics

We use data from the Illinois State Board of Education to construct measures of neighborhood school quality. The procedure proceeds as follows:

- For each Infutor address, we identify the nearest elementary, middle, and high school using the Stata command `geonear`. We drop elementary schools located more than 3 miles away, and middle or high schools located more than 6 miles away, as such schools are unlikely to be part of the neighborhood environment.
- We average the schools' reading and math percentile ranking as the quality of the school.
- If multiple schools are located at the same distance from an address, we compute the neighborhood school quality as the average ranking across those schools. At the person-year level, distance ties occur for approximately 3 percent of elementary schools, less than 1 percent of middle schools, and 10 percent of high schools.

A.10 Summary Statistics

Table A.1 shows summary statistics by foreclosure outcome for our owner, renter, and landlord samples.

B Evidence of Selection Appendix

B.1 Annual Correlation Between Foreclosure and Credit Report Outcomes

Figure 2 in the main text showed high-frequency pre-trends related to foreclosure in monthly credit report data. Figure A.1 repeats this analysis in annual data and a 5-year window to mimic the yearly snapshots of the credit report data that are sometimes used. In particular, we calculate the number of active first mortgages at the time of the snapshot, the number of new first mortgages since the last snapshot, and the share of non-mortgage accounts that were active at any time since the previous snapshot that are in default. Figure 2 does this for monthly snapshots relative to the foreclosure filing date, while Figure A.1 does this for annual snapshots relative to the foreclosure filing date.

The figure shows that there are either no or minor pre-trends in the annual data. The fact that we see significant pre-trends in the monthly data cautions against relying on lower-frequency parallel trends, as lower-frequency analysis masks the pre-trends.

B.2 Details on Each Proxy For Costliness and ATP

Below, we provide details on each proxy in the LLMA analysis in Table 2 and explain in more detail why each story has the indicated sign for each proxy variable in Table 2.

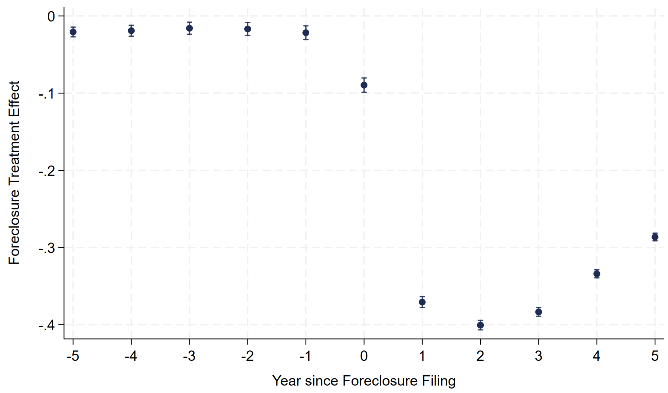
1. **The change in average ZIP code income from mortgage origination to foreclosure filing** according to the IRS Statistics of Income. A negative sign is consistent with ATP, under which a decline in income (proxied for by the ZIP average) should increase foreclosure. A negative sign is also consistent with the costliness story because lower ZIP income pushes down house prices and home equity, allowing borrowers to lose less by walking away from their mortgage in a foreclosure. Crucially, under the costliness story, there is no clear sign for this variable after conditioning on LTV or home equity.
2. **The individual's Vantage Score 6 months pre-filing**. A negative sign is consistent with ATP since individuals with lower credit scores have weaker financial positions and are more likely to foreclose. A positive sign is consistent with costliness because it indicates strategic default: Financially stronger households are more likely to default because they have low private costs of default.
3. **The share of debts that are delinquent one year before filing**. A negative sign is consistent with costliness: If individuals with fewer delinquent debts are more likely to default, they are strategically defaulting, revealing a low private cost of foreclosure. A positive sign indicates ATP because individuals with more delinquent debt tend to be financially weaker.
4. **The difference between the current and origination mortgage interest rate**. A positive sign is consistent with ATP because people whose debt financing costs increase due to an upward interest rate reset should be more likely to foreclose. There is no clear sign prediction for the costliness story.
5. **An indicator for whether the property is owner-occupied**. A negative sign indicates costliness, because an owner-occupant who is also evicted loses more in foreclosure than a landlord. This has no clear sign for ATP.

Table A.1: Summary Statistics by Case Outcome

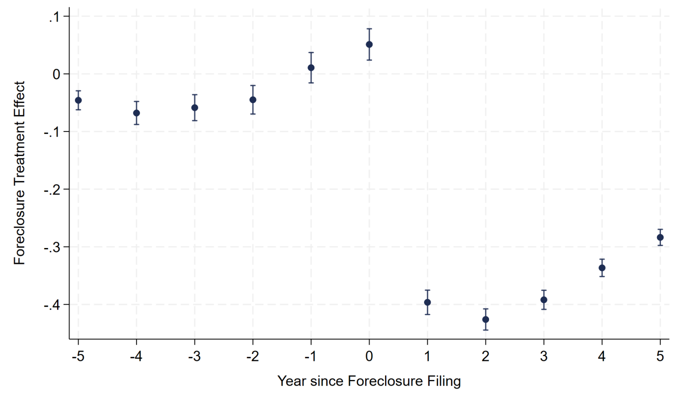
	Owner		Landlord		Renter	
Foreclosure Rate	0.349		0.543		Non Applicable	
	Owner		Landlord		Renter	
Case Foreclosed by Year 3	Yes	No	Yes	No	Yes	No
Moved from Foreclosure Address	0.093	0.070	0.175	0.139	0.255	0.242
	(0.290)	(0.256)	(0.380)	(0.346)	(0.436)	(0.428)
Cumulative Number of Moves	0.097	0.073	0.188	0.148	0.266	0.253
	(0.311)	(0.269)	(0.424)	(0.382)	(0.467)	(0.459)
Owns Primary Residence	0.783	0.858	0.503	0.515	0.055	0.067
	(0.412)	(0.349)	(0.500)	(0.500)	(0.228)	(0.250)
Square Footage of Living Space	1320.644	1386.411	1425.938	1460.358	1253.803	1303.594
	(518.598)	(583.295)	(633.116)	(673.659)	(485.032)	(531.861)
Log Zip Code Average Income	3.742	3.762	3.835	3.864	3.645	3.727
	(0.407)	(0.431)	(0.483)	(0.506)	(0.432)	(0.480)
Elementary School Test Score Rank	36.895	37.960	39.402	40.584	29.471	33.728
	(27.319)	(28.036)	(28.736)	(29.645)	(26.845)	(28.519)
Middle School Test Score Rank	44.024	45.287	48.187	49.788	40.494	43.978
	(30.273)	(30.537)	(30.949)	(31.097)	(30.294)	(31.090)
High School Test Score Rank	36.286	37.079	39.903	40.871	30.910	35.065
	(32.797)	(33.367)	(34.606)	(35.230)	(32.434)	(34.017)
Opportunity Atlas: Intergen Mobility	0.479	0.484	0.486	0.490	0.433	0.454
	(0.115)	(0.117)	(0.116)	(0.118)	(0.121)	(0.124)
Opportunity Atlas: Frac of Pop in Jail	0.019	0.018	0.018	0.017	0.027	0.024
	(0.020)	(0.019)	(0.019)	(0.019)	(0.024)	(0.023)
Opportunity Atlas: Teen Birth Rate	0.244	0.237	0.231	0.226	0.313	0.282
	(0.165)	(0.165)	(0.163)	(0.165)	(0.185)	(0.184)
Cumulative Number of Divorces	0.020	0.015	0.017	0.017	0.007	0.007
	(0.142)	(0.126)	(0.132)	(0.131)	(0.085)	(0.084)
VantageScore	624.500	617.269	648.532	646.474	616.273	631.846
	(94.915)	(94.291)	(98.468)	(100.014)	(113.062)	(114.083)
Has Had Bankruptcy	0.109	0.118	0.090	0.091	0.101	0.091
	(0.312)	(0.323)	(0.286)	(0.288)	(0.301)	(0.288)
Number of Active First Mortgages	0.789	0.793	1.688	1.640	0.222	0.239
	(0.857)	(0.825)	(1.615)	(1.515)	(0.526)	(0.539)
Ever Defaulted on a First Mortgage	0.050	0.056	0.042	0.046	0.026	0.023
	(0.218)	(0.230)	(0.200)	(0.210)	(0.160)	(0.150)
Ever Defaulted on a Home Equity Loan	0.024	0.026	0.024	0.029	0.009	0.009
	(0.154)	(0.160)	(0.154)	(0.168)	(0.093)	(0.093)
Has a Home Equity Account (excl. Mortgages)	0.282	0.234	0.427	0.394	0.080	0.088
	(0.450)	(0.423)	(0.495)	(0.489)	(0.272)	(0.284)
Share of First Mortgage in Default	0.021	0.022	0.018	0.018	0.006	0.006
	(0.136)	(0.141)	(0.122)	(0.125)	(0.076)	(0.076)
Share of Non-Mortgage Accts in Default	0.140	0.161	0.089	0.102	0.178	0.162
	(0.281)	(0.295)	(0.225)	(0.241)	(0.317)	(0.308)
Share of Credit Card in Default	0.143	0.166	0.093	0.105	0.168	0.151
	(0.300)	(0.313)	(0.244)	(0.260)	(0.328)	(0.317)
Share of Auto Loan in Default	0.049	0.052	0.034	0.038	0.063	0.057
	(0.199)	(0.205)	(0.167)	(0.175)	(0.227)	(0.217)
Share of Student Loan in Default	0.014	0.017	0.009	0.010	0.023	0.026
	(0.107)	(0.119)	(0.088)	(0.092)	(0.140)	(0.148)

Notes: This table shows foreclosure rates by group as well as summary statistics for the indicated key outcome variables separately by case outcome for owners, landlords, and renters.

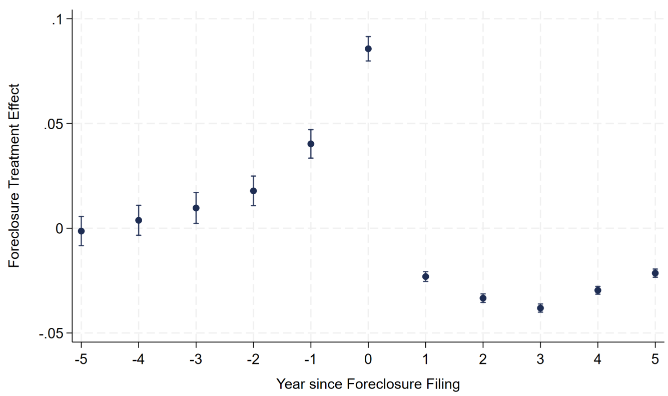
Figure A.1: Annual Correlation Between Foreclosure and Credit Report Outcomes



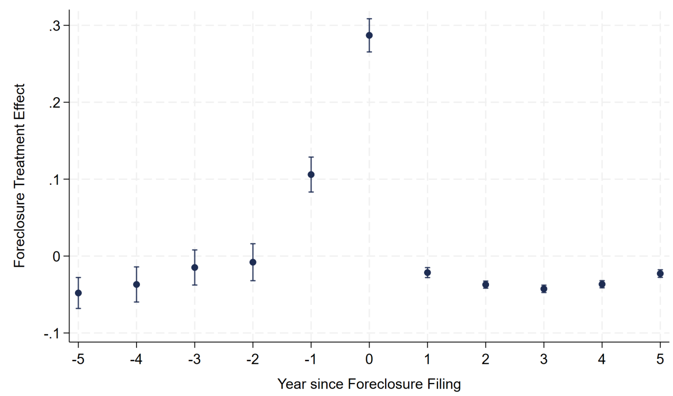
(a) Number of Active First Mortgages (Owners)



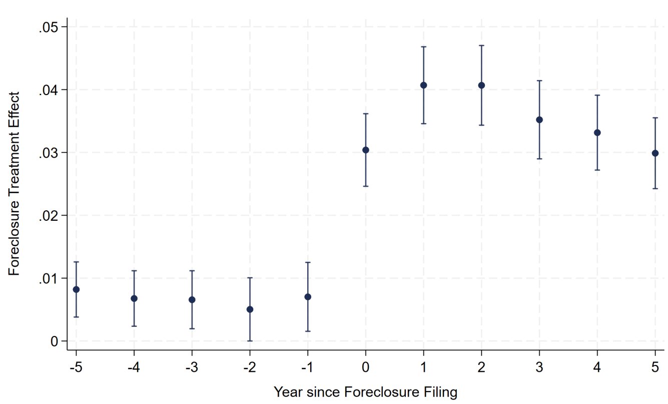
(b) Number of Active First Mortgages (Landlords)



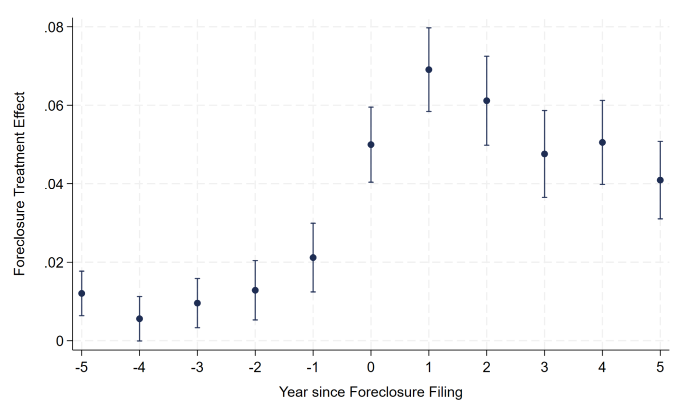
(c) Number of New First Mortgages (Owners)



(d) Number of New First Mortgages (Landlords)



(e) Share of Non-Mrtg Accts in Default (Owners)



(f) Share of Non-Mrtg Accts in Default (Landlords)

Notes: This figure shows the results of annual regressions for the indicated group of the indicated outcome variable from the credit report data on foreclosure, controlling for date of filing fixed effects. Formally, index individuals by i, event time in years relative to a foreclosure filing by $s = -5, ..., 5$, let $Y_{i,s,t}$ be the outcome at time t and time relative to foreclosure s, F_i be a foreclosure indicator, and $\xi_{t,s}$ be a date of filing fixed effect. We regress $Y_{i,s,t} = \beta_s F_i + \xi_{t,s} + \varepsilon_{i,s,t}$ clustering at the case level and plot β_s against s.

6. **An indicator for whether the borrower was late on the mortgage prior to 6 months before the foreclosure case was filed**. If this is the case, the borrower is chronically delinquent but trying to make sporadic payments when possible. A positive coefficient points toward ATP, indicating that income volatility matters for foreclosure. A negative coefficient is consistent with the costliness story because a borrower who is current 6 months prior and immediately goes delinquent without attempting to make a payment is likely strategically walking away from the mortgage. Indeed, Keys et al. (2012) propose a similar indicator as a measure of strategic default.

7. **An indicator for whether the borrower has had a mortgage modification in the past**. A negative sign is consistent with costliness, as the effort to obtain a mortgage modification indicates that foreclosure is privately costly for a household. This has no clear sign under ATP.

8. **An indicator for whether the borrower has caught up to being current on their mortgage after being late in the past**. This is similar to being late on the mortgage prior to 6 months before the case was filed: A positive coefficient indicates ATP, as chronic lateness indicates income volatility, whereas a negative coefficient is consistent with strategic default and the costliness story.

9. **The LTV at the time of foreclosure**. This should be positive under both stories. A borrower with a high LTV has less home equity to lose in foreclosure, so LTV should be positively correlated with foreclosure under the costliness story. It should also be positively correlated under ATP because borrowers with weaker finances will take out an initially higher LTV loan. We include this proxy despite its not having a clear correlation with any one story because controlling for LTV has a prediction for the ZIP income proxy as indicated above.

C Judge Instrumental Variable Appendix

C.1 Calendars and Judges

C.1.1 Overview

The Chancery Division of the Circuit Court of Cook County handles all mortgage foreclosure cases in Cook County. Since 2005, the court has randomly assigned cases to a "calendar." One can think of a calendar as a courtroom: There is a principal judge who typically handles cases, along with backup judges who are often shared across calendars. All mortgage foreclosure cases are heard at the same courthouse, and there is a single randomization process for all cases in Cook County.

In particular, mortgage foreclosure cases are one of eight types of cases heard by the Chancery Division. Before 2005, mortgage foreclosure cases were heard by the General Chancery Section. However, in February of 2005, the Mortgage Foreclosure/Mechanics Lien (MF/ML) Section was created to address increasing caseloads. At this point, the Court publishes rules for the random assignment of mortgage foreclosure cases. In particular, "one-fourth of all foreclosure cases were randomly assigned to the three mechanics lien judges, Calendars 52-54, and each of the three new judges, Calendars 55-57, received one-fourth of all foreclosure cases."

Starting in May 2006, additional calendars were introduced due to the high volume of cases. At that point, calendars 52, 53, and 54 received a third of 20 percent of the cases, while calendars 55, 56, 57, and 58 split the rest equally. Calendar 59 was added in July of 2007. On October 20, 2008, calendars 60-63 were added. Finally, on November 9, 2009, Calendar 64 was added, and all remaining cases from Calendars 52 through 54 were transferred to Calendar 64.

In the data, we find evidence of significant calendar reassignment beyond Calendar 64. In particular, as the court added calendars, they transferred existing cases to the new calendars. These cases were not randomly assigned since they were existing cases that had taken longer to decide. This is an issue because the final calendar is the only one we observe in our scraped data. Using this final calendar to create the instrument yields an invalid instrument due to non-random assignment.

We can nonetheless recover the randomly assigned initial calendar for all cases to produce a valid instrument. The key to doing so is to recall that we not only observe the final calendar but also the date and the judge for each judgment. We can look at cases that resolve quickly to identify which judge was the primary judge on each calendar for each month. We can then look at cases that resolve more slowly and are reassigned, and infer the initially assigned calendar from which the judge made the initial judgments in each case.

The following two subsections detail our algorithm for reassigning calendars and demonstrate that we replicate the court's random assignment probabilities.

C.1.2 Reassigning Calendars to the Original Calendar

We first identify the main judge on each calendar in each month. To do so, we use two definitions:

- A strict main judge makes at least 75% of the judgments on a calendar in a month based on the final calendar, and at least 75% of the judge's judgments in a month are on that calendar.
- A weak main judge makes at least 75% of the judgments on a calendar in a month based on the final calendar, or at least 75% of the judge's judgments in a month are on that calendar.

In finding the main judge, we limit ourselves to active calendars so that:

- Calendars 52, 53, and 54 finished having active judgments at the end of 2010 (even though they stopped taking new cases between November 2008 and February 2009).
- Calendar 58 begins in May 2006
- Calendar 59 begins in July 2007
- Calendars 60-63 begin in October 2008
- Calendar 64 begins in November 2009

A potential issue with this method of identifying main judges is that a judge could switch calendars mid-month and thus not appear as the main judge for either calendar that month. We manually reviewed the daily and weekly judgments and found this occurred only once, when Carolyn Quinn switched from calendar 54 to calendar 56 starting November 17, 2018. We manually adjust our main judges for this one case.

Once we have the history of the principal judges for each calendar, we determine the original calendar for reassigned cases. Recall that cases are reassigned only to new calendars to more evenly distribute the case load. The concern is that reassignment is non-random because cases that take longer may have different foreclosure probabilities. We thus determine whether a case is reassigned based on its starting date (the date of the initial judgment) before the final calendar is created. In this case, we know the case must have been reassigned to that calendar.

In particular, we count the number of judgments on each case made by the main judge on each calendar, and do not count calendars that are reassignments based on the day the case started relative to the calendar's creation date. If one calendar has the most judgments by the main judge of a potential original calendar, we set that calendar as the original calendar. We do this initially for the strict definition of a main judge. 96.01% of the cases have an original calendar assigned this way. The remaining 3.99% either have no judgments by a strict main judge of a potential original calendar or have a tie between two potential original calendars.

For these 3.99% of cases, we repeat the process using progressively weaker definitions of the main judge. First, we use the weak main judge definition above. This gets 3.33% of the 3.99% of cases that need to be reassigned. We then do three additional methods for the last 0.66% of cases. First, we include judges who are not the only main judge on a calendar. Then we go back to the strict main-judge definition and break ties by which of the tied main judges ruled first. We then repeat this tie-breaking procedure using the weak main judge definition. Ultimately, this algorithm assigns all cases to an original calendar.

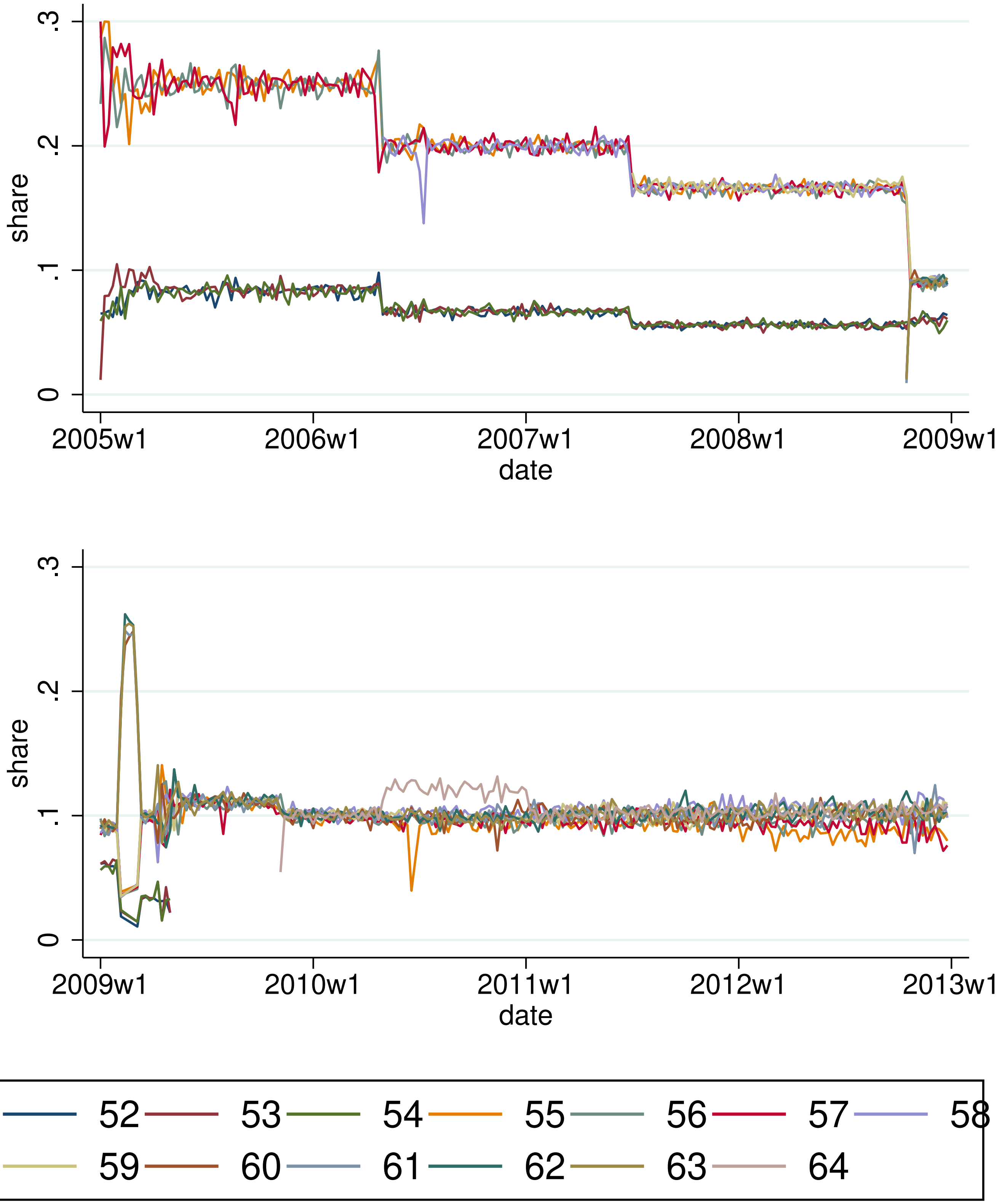
C.1.3 Relationship Between Our Assignment Probabilities and Published Assignment Probabilities

Figure A.2 shows the share of cases assigned to each calendar by week over our sample using the original calendar variable. The assignment probabilities are generally very stable, exhibit discrete jumps at the dates when new calendars are introduced, as documented in court records, and align with the published assignment probabilities during periods when they are public.

In particular, the court documentation indicates that from the beginning of 2005 until May 2006, calendars 52, 53, and 54 split a quarter of the cases three ways while calendars 55, 56, and 57 each took 25 percent, which is precisely what we observe. Then, in May 2006, calendar 58 was added, and calendars 52, 53, and 54 split 20% of the cases three ways, while calendars 55, 56, 57, and 58 each received 20 %. In July of 2007, Calendar 59 was added. Although we were unable to find published probabilities for this addition or subsequent additions, it seems they followed the same rule: Calendars 52, 53, and 54 split one-sixth of the cases, and each of the other calendars gets one-sixth. In October of 2008, court documents indicate that four additional calendars were added. We observe the randomization probabilities change at this time. However, rather than reducing the load on calendars 52, 53, and 54, the cases were divided among the remaining calendars. In early 2009, the randomization probabilities of these four calendars increased as their caseloads filled, while calendars 52-54 were phased out. Unfortunately, we could not find court documentation of this process. Finally, in November 2009, Calendar 64 was added, and the randomization probabilities for the remaining calendars are 10% for the rest of the sample, except for a period in 2010 when Calendar 64 has a higher probability, presumably to increase its caseload.

The fact that we have stable assignment probabilities (with some noise) that replicate published probabilities when available indicates that our case reassignment algorithm does a good job of identifying the original randomly assigned calendar. In the main paper, we also show several placebo tests to confirm random assignment conditional on the date a case starts.

Figure A.2: Share of Cases Assigned to Each Calendar By Week



Notes: The figure shows the share of cases assigned to each calendar in each week from 2005 to 2012.

C.2 Constructing the Instrument

Our instrument can be defined at any time frequency. Using a higher frequency has the disadvantage that Z is a noisier measure of stringency. However, a higher frequency may be beneficial if one is concerned that calendars may be more active in parts of the year when more cases are more likely to foreclose. This would imply that Z is not randomly assigned, leading to biased results. We use an annual Z to maximize power, but our results are not sensitive to the instrument's frequency.

C.3 Correlation of Instrument and Proxies for ATP and Costliness

Table A.2 repeats the analysis in Table 2 but replaces the foreclosure indicator with the judge stringency instrument. None of the proxies for ability to pay and costliness that we found to be correlated with foreclosure are correlated with the instrument, providing an additional test of the exclusion restriction.

C.4 Monotonicity

To obtain a valid LATE, IV requires a monotonicity assumption that an increase in the judge's average foreclosure probability must increase the foreclosure probability for all foreclosure cases. In other words, a strict judge must be strict for everyone, and a lenient judge must be lenient for everyone. One concern is that this may not hold for judge assignments if, for instance, some judges are stricter with certain types of defendants than with others. A commonly cited example of this is a case in which judges are lenient toward defendants of their own race and strict toward defendants of other races.

Table A.3 addresses these concerns by estimating the first stage equation (3) for different judge characteristics (gender and race) and defendant characteristics (gender, race, age, immigrant status, presence of an attorney, ZIP-Income Quintile, and Mortgage Size Quartile). We do not find any statistically significant negative coefficients, and with a few exceptions, where we have very little data and the standard errors widen, we have significant and positive coefficients.

To further test monotonicity and the exclusion restriction, we conduct a nonparametric test for the exclusion and monotonicity assumptions in judge IV designs based on Frandsen et al. (2023) and implemented in their Stata package "`testjfe`." The test examines how average judge-level outcomes vary as a function of the judge-level propensity to treat. In their appendix, Frandsen et al. propose a semi-parametric test with two test statistics — one based on the slope of the line and one on the fit of the relationship — that they implement using the testjfe package in Stata. Table A.4 shows p-values from the test for owners for several of the most significant outcomes in our analysis, measured three years after foreclosure filing. We use month of foreclosure filing fixed effects for this analysis. All of the p-values indicate that we cannot reject the null of the monotonicity and exclusion restrictions holding. We conclude that we find no significant evidence of a violation of monotonicity.

D Disentangling Foreclosure From Time To Foreclosure

One potential concern is that judges who are more or less likely to foreclose also engage in other actions that are actually causing our measured effects and that cannot be distinguished from foreclosure because all of the variation in the instrument is at the judge level. The most natural such story is that judges who do not foreclose take longer to decide the foreclosure case, and that it is the extended case duration rather than the foreclosure decision that gives the owner, renter, or landlord breathing room and causes differential outcomes. In this Appendix, we test this alternate story and find no evidence to substantiate it: Foreclosure matters, not time to foreclosure.

To reach this conclusion, we first define the length of a case as the difference between the date of the last and first judgment on the case. The median case takes about one year, so we use an indicator for whether a case takes over a year as our primary outcome variable.

We then augment our main IV and OLS empirical approaches to include both the foreclosure within three years indicator *and* an indicator for the case lasting over one year indicator as independent variables. In particular, the OLS regression we want to estimate instead of (1) is:

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

where L_k is the indicator for the case taking more than one year to resolve, β_s^F is the measured coefficient on foreclosure over horizon s, and β_s^L is the coefficient on the length of the case over horizon s. The concern is that F_k and L_k are correlated and that by omitting L_k we had a biased coefficient on F_k.

We also modify our IV method by defining a leave-out average of the probability that a case lasts over a year, just as we defined a leave-out average of foreclosure propensity to measure stringency. Define the stringency Z of case k

Table A.2: Correlation of Ability to Pay and Costliness Proxies With Judge Stringency

Variable	Bivariate	Multivariate
ZIP Avg Income Change Since Origination	0.00005	0.00003
	(0.00006)	(0.00015)
6 Month Pre-Filing Vantage Score	0.00011*	-0.00001
	(0.00005)	(0.00012)
Pre-Filing Share of Debts That Are Delinquent	-0.000010	-0.00015
	(0.000041)	(0.0001)
Interest Rate Differential Since Origination	-0.000057	-0.00006
	(0.000078)	(0.00009)
Owner Occupied Property Indicator	0.00005	0.00040
	(0.00025)	(0.000102)
Late Prior to 6 Months Pre-Filing Indicator	-0.000045	-0.00004
	(0.000045)	(0.00025)
Mortgage Modification in Past Indicator	0.000048	0.00015
	(0.000045)	(0.00028)
Caught Up After Being Late in Past Indicator	-0.000056	-0.00044
	(0.000046)	(0.00023)
LTV at Foreclosure	0.000034	-0.00008
	(0.000106)	(0.00055)
Observations		42,826
Within R^2		0.0003
P Value of Joint Test Coefficients are Zero		0.224

Notes: This table shows results for regressions of the proxy variables on our judge stringency instrument. All variables not listed as indicators are standardized to have a mean of zero and a standard deviation of one; indicators are left as binary. The bivariate column indicates coefficients in a bivariate regression with the indicated variable on foreclosure, with month of filing and zip-year of filing fixed effects. The multivariate column shows coefficients from a multivariate regression that combines all variables. Because Vantage Score started in 2008, we set all variables with missing Vantage scores to a single value and include a dummy variable for missing Vantage scores. Vantage score is in hundreds. The final line shows the p-value of a joint Wald test of whether all of the coefficients in the multivariate regression are zero. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

assigned to calendar c in year t as:

$$Z^L_{k,c,t} = \frac{\sum_{j \in c,t,j \neq k} L_{j,c,t}}{N_{(-k),c,t}},$$

while the corresponding foreclosure definition remains:

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

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

$$Y_{i,k,s,t} = \beta^F_s F_k + \beta^L_s L_k + \gamma_s X_i + \xi_{m(k),s} + \phi_{z(k),t,s} + \varepsilon_{i,k,s,t} \tag{5}$$

$$F_k = \Gamma^F Z^F_{k,c,t} + \Gamma^L Z^L_{k,c,t} + \alpha X_i + \zeta_{m(k)} + \varphi_{z(k),t} + e_{i,k,t} \tag{6}$$

$$L_k = \Omega^F Z^F_{k,c,t} + \Omega^L Z^L_{k,c,t} + \alpha X_i + \zeta_{m(k)} + \varphi_{z(k),t} + e_{i,k,t}, \tag{7}$$

Table A.3: Monotonicity Tests: First Stage Regressions by Defendants' and Judge's Characteristics

	Any Judge	By Main Judge's Characteristic			
		Male	Female	White	Non-white
Any Defendant					
By Gender					
- both male and female	0.765***	0.772***	0.655	0.903***	0.513*
	(0.116)	(0.134)	(0.343)	(0.146)	(0.261)
- male only	0.723***	0.751***	0.729	0.731***	0.611*
	(0.136)	(0.159)	(0.401)	(0.172)	(0.305)
- female only	0.622***	0.681***	0.574	0.657***	0.455
	(0.143)	(0.167)	(0.442)	(0.182)	(0.325)
By Race					
- white only	0.662***	0.734***	0.537	0.608***	0.861**
	(0.130)	(0.150)	(0.387)	(0.164)	(0.291)
- non-white	0.784***	0.779***	0.695**	0.890***	0.403*
	(0.080)	(0.092)	(0.221)	(0.099)	(0.171)
By Age					
- old	0.707***	0.745***	0.919	0.760***	0.291
	(0.162)	(0.189)	(0.516)	(0.207)	(0.378)
- young	0.686***	0.713***	0.538*	0.768***	0.554**
	(0.093)	(0.107)	(0.263)	(0.116)	(0.201)
By Immigrant Status					
- both immigrants and locals	0.786**	0.338	1.579	0.836*	0.353
	(0.283)	(0.342)	(1.197)	(0.380)	(0.777)
- immigrants only	0.971**	1.065**	-0.136	1.375**	0.830
	(0.310)	(0.382)	(1.191)	(0.421)	(0.888)
- locals only	0.590***	0.663***	0.606	0.869***	0.239
	(0.124)	(0.144)	(0.367)	(0.157)	(0.272)
By Presence of Attorney					
- no attorney	0.736***	0.749***	0.665***	0.804***	0.470**
	(0.069)	(0.079)	(0.189)	(0.085)	(0.148)
- has attorney	1.668***	1.378**	-1.840	1.720**	1.394
	(0.388)	(0.504)	(2.152)	(0.576)	(1.429)
By Zip-Income Quintile					
- 1st quintile	0.776***	0.822***	0.788	0.886***	0.631
	(0.169)	(0.196)	(0.497)	(0.214)	(0.375)
- 2nd quintile	0.773***	0.786***	0.427	0.814***	0.002
	(0.165)	(0.192)	(0.493)	(0.210)	(0.367)
- 3rd quintile	0.890***	0.941***	0.244	0.918***	0.886*
	(0.152)	(0.177)	(0.459)	(0.192)	(0.347)
- 4th quintile	0.773***	0.652***	1.357**	0.693***	0.525
	(0.148)	(0.173)	(0.452)	(0.187)	(0.335)
- 5th quintile	0.612***	0.636***	0.334	0.791***	0.327
	(0.143)	(0.166)	(0.423)	(0.179)	(0.323)
By Mortgage Size Quartile					
- 1st quartile	0.705***	0.732***	-0.165	0.775***	0.448
	(0.146)	(0.171)	(0.434)	(0.187)	(0.319)
- 2nd quartile	0.521***	0.663***	0.414	0.600***	0.144
	(0.138)	(0.160)	(0.426)	(0.175)	(0.312)
- 3rd quartile	0.972***	0.974***	1.095**	1.118***	0.912**
	(0.133)	(0.155)	(0.386)	(0.166)	(0.302)
- 4th quartile	0.791***	0.777***	0.635	0.808***	0.889**
	(0.135)	(0.156)	(0.395)	(0.170)	(0.300)

Notes: This table shows the baseline IV results (column 1) and IV results with both foreclosure and case length being over 1 year both included as regressors (columns 2 and 3). All 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.

Table A.4: New Frandsen et al. Monotonicity and Exclusion Restriction Test for Owners

Outcome Variable	Combined P Value
Moved From Foreclosure Address	0.474
Cumulative Number of Moves	0.168
Log Zip Code Average Income	1.000
Middle School Test Score Rank	0.497
Cumulative Number of Divorces	1.000

Notes: This table shows p values from the Frandsen et al. (2023) test of the monotonicity and exclusion restriction conditions for judge IV designs. The table shows p values of the fit test and slope test for owners, where the test is for the null of the monotonicity and exclusion restriction conditions holding. The slope test only works for outcomes between -1 and 1, so for the slope test only, we normalize the outcome to fall in this interval. The tests control for dummies for ZIP code and month of the foreclosure case. The indicated outcomes are for 3 years after the foreclosure case occurs. The combined test evenly weights the fit and slope tests.

where equations (6) and (7) are both first stage equations. Intuitively, we are constructing two separate judge stringency instruments — one relating to foreclosure and one to case length — and using to instrument both foreclosure and the case length indicator. This approach uses variation in case length and foreclosure stringency that is orthogonal across judges to identify each coefficient separately. We cluster and weight these regressions as in the main text.

Figure A.3 shows results for three key outcomes for owners: moving, log zip code income, and divorce. For each outcome, the left panel shows the coefficients on foreclosure β_s^F while the right-hand panel shows the coefficients on whether the case is resolved in over one year β_s^L. Three things are of note. First, the confidence intervals for foreclosure are somewhat wider than in our baseline, reflecting reduced statistical power. Second, the point estimates for foreclosure are very close to our main results, indicating that the effects really were due to foreclosure, not case length. Third, the effects of a longer case are near zero, with one exception: Cases that take over a year to resolve see an increase in moving in year one that then goes away.

Tables A.5, A.6, and A.7 show pooled results for all outcomes for years 3 and 4 results for our main specification (same as Table 5) and the specification with both foreclosure and length of case for owners, renters, and landlords, respectively. One can see that, for both IV and OLS, adding case length does not significantly alter our results on foreclosure across nearly all outcomes, though it modestly reduces statistical power. We can also see that case length generally has economically insignificant effects. The exception is credit score, where it seems like a longer case leads to a lower credit score, but this is statistically insignificant.

Overall, our analysis of time to foreclosure indicates it is far less important than the primary foreclosure outcome. This assuages any concerns that our foreclosure effects were actually picking up the impacts of case length.

E Additional Outcomes

This appendix presents figures with additional neighborhood quality and financial outcomes for owners, landlords, and renters that are in Table 5 but not in the figures in the main text. For renters, this includes many outcomes with low statistical power that we did not show in the main text.

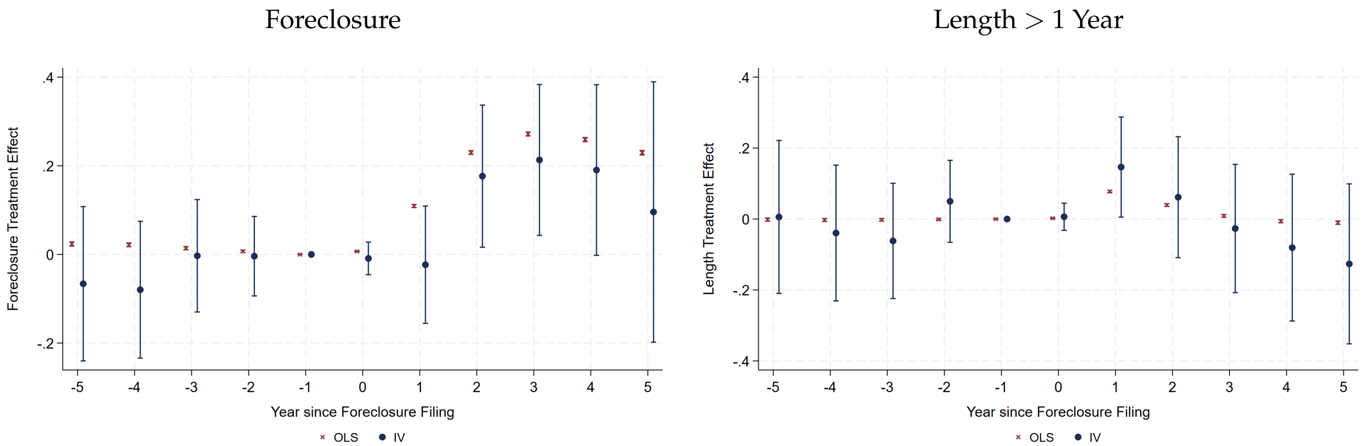
E.1 Comparing IV and OLS For Landlords

We see slightly different patterns of bias when comparing OLS and IV for landlords than we do for owners. In particular, we see no effects on moving, homeownership, home size, or any of the neighborhood quality outcomes. For the financial outcomes, OLS shows more minor adverse effects on bankruptcy, the number of active first mortgages, and having ever defaulted on a first mortgage than IV.

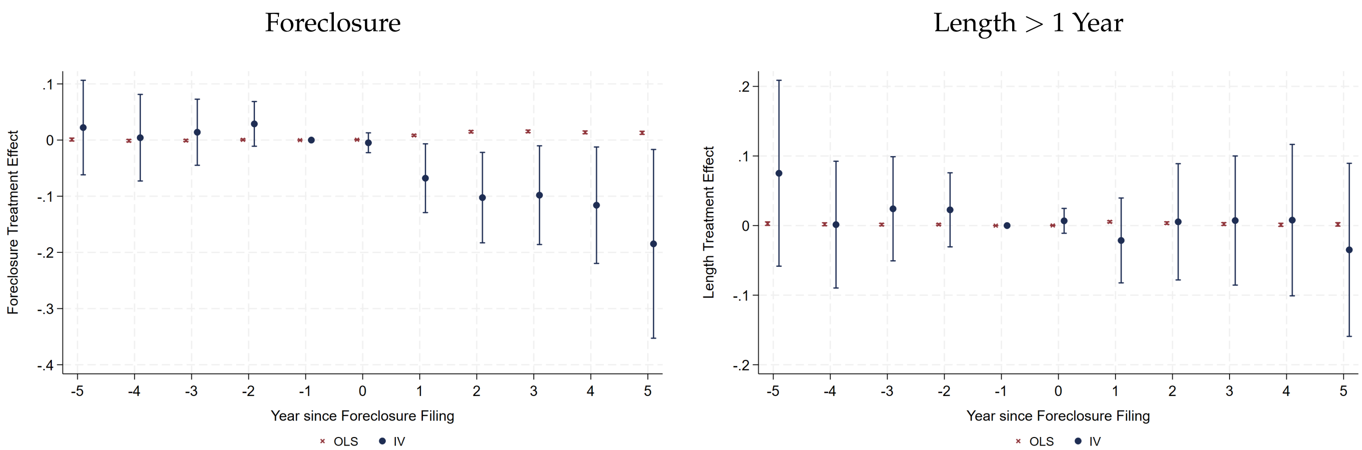
While the fact that these patterns differ from those we find among owners may at first seem perplexing, they are consistent with the costliness story once one realizes that outcomes that are mechanical for owner-occupants, such as owning and moving, are *not* mechanical for landlords. Indeed, the only mechanical outcome for landlords is a default on a primary mortgage on their credit report. Consequently, the fact that we find short-lived financial distress but long-term improvements in credit scores and moves to better neighborhoods is consistent with the downward bias in OLS stemming from landlords who could benefit from shedding bad debts in foreclosure doing less to fight their foreclosure cases.

Figure A.3: Foreclosure vs. Length to Foreclosure For Selected Owner Outcomes

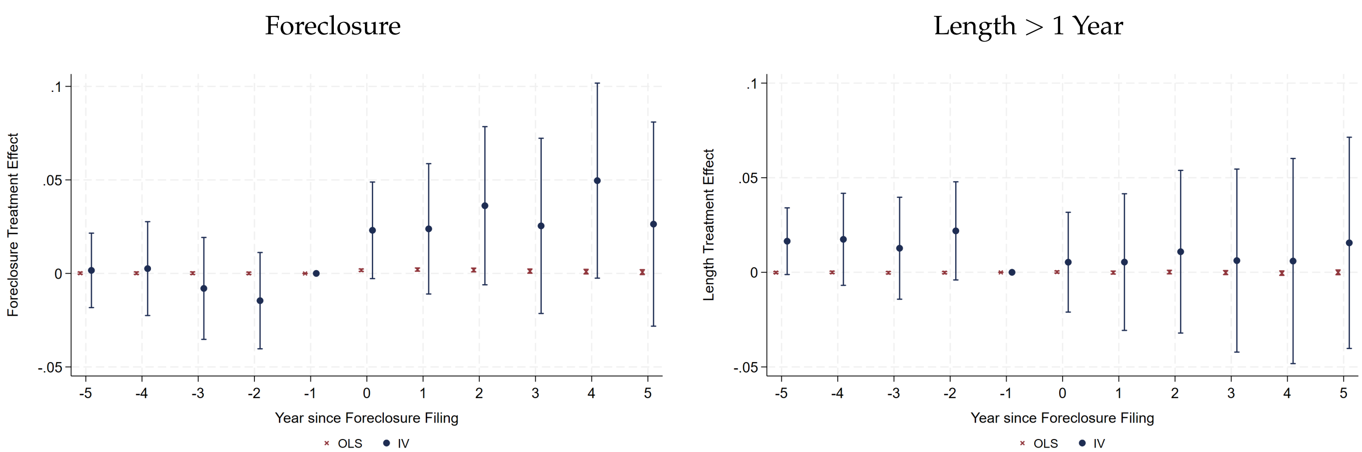
(a) Moved from Foreclosure Address



(b) Log Zip Code Average Income



(c) Cumulative Number of Divorces



Notes: Each panel shows IV and OLS results for the indicated outcome variable for all owners in our sample. The left panel shows foreclosure outcomes, while the right panel shows outcomes for cases lasting more than one year. Each dot indicates the IV point estimate for β_s^F or β_s^L estimated using equations (5), (6), and (7) and the bars indicate 95% confidence intervals. Each x indicates the OLS estimate for β_s^F or β_s^L estimated using equation (4). OLS 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.

Table A.5: Treatment Effects of Foreclosure and Time to Foreclosure Summary, Owners

Specification:	IV Baseline	IV Both	
Coefficient:	Foreclosure	Foreclosure	Case Length
	(1)	(2)	(3)
Moved from Foreclosure Address	0.195***	0.209***	-0.053
	(0.075)	(0.080)	(0.080)
Cumulative Number of Moves	0.278***	0.283***	-0.018
	(0.103)	(0.109)	(0.109)
Owns Primary Residence	-0.468***	-0.478***	0.049
	(0.077)	(0.080)	(0.080)
Square Footage of Living Space	-82.632	-71.284	-60.668
	(60.989)	(61.902)	(61.902)
Log Zip Code Average Income	-0.106***	-0.053	-0.034
	(0.040)	(0.038)	(0.038)
Elementary School Test Score Rank	-0.774	-0.521	-0.910
	(3.261)	(3.409)	(3.409)
Middle School Test Score Rank	-6.630**	-6.739**	0.461
	(3.054)	(3.157)	(3.157)
High School Test Score Rank	-5.182	-4.457	-2.844
	(3.527)	(3.645)	(3.645)
Opportunity Atlas: Intergen Mobility	-0.021**	-0.016	-0.018
	(0.010)	(0.010)	(0.010)
Opportunity Atlas: Frac of Pop in Jail	0.005***	0.005***	-0.001
	(0.002)	(0.002)	(0.002)
Opportunity Atlas: Teen Birth Rate	0.031**	0.028**	0.011
	(0.013)	(0.014)	(0.014)
Cumulative Number of Divorces	0.040*	0.038	0.007
	(0.022)	(0.024)	(0.024)
VantageScore	-3.716	-1.167	-14.801
	(12.433)	(13.147)	(13.147)
Has Had Bankruptcy	0.223***	0.210***	0.051
	(0.074)	(0.078)	(0.078)
Number of Active First Mortgages	-0.245**	-0.244**	-0.004
	(0.103)	(0.108)	(0.108)
Ever Defaulted on a First Mortgage	0.201***	0.201***	0.001
	(0.074)	(0.078)	(0.078)
Ever Defaulted on a Home Equity Loan	0.104*	0.125*	-0.086
	(0.063)	(0.067)	(0.067)
Has a Home Equity Account (excl. Mortgages)	-0.107**	-0.106**	-0.002
	(0.049)	(0.051)	(0.051)
Share of First Mortgage in Default	0.249	0.238	0.053
	(0.174)	(0.171)	(0.171)
Share of Non-Mortgage Accts in Default	0.192**	0.196**	-0.015
	(0.080)	(0.084)	(0.084)
Share of Credit Card in Default	0.123	0.132	-0.046
	(0.086)	(0.089)	(0.089)
Share of Auto Loan in Default	0.287	0.270	0.042
	(0.199)	(0.202)	(0.202)
Share of Student Loan in Default	-0.090	-0.083	-0.027
	(0.151)	(0.157)	(0.157)

Notes: This table shows the baseline IV results (column 1) and IV results with both foreclosure and case length being over 1 year both included as regressors (columns 2 and 3). All 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.

Table A.6: Treatment Effects of Foreclosure and Time to Foreclosure Summary, Landlords

Specification:	IV Baseline	IV Both	
Coefficient:	Foreclosure	Foreclosure	Case Length
	(1)	(2)	(3)
Moved from Foreclosure Address	0.110 (0.149)	0.053 (0.178)	0.102 (0.178)
Cumulative Number of Moves	0.063 (0.211)	-0.014 (0.255)	0.134 (0.255)
Owns Primary Residence	-0.055 (0.151)	0.073 (0.190)	-0.208* (0.190)
Square Footage of Living Space	-178.963 (138.655)	-261.084 (176.412)	160.822 (176.412)
Log Zip Code Average Income	0.150 (0.096)	0.158 (0.116)	-0.015 (0.116)
Elementary School Test Score Rank	15.140** (7.044)	14.180* (7.770)	2.016 (7.770)
Middle School Test Score Rank	-9.648 (6.401)	-11.483 (7.237)	4.931 (7.237)
High School Test Score Rank	-8.237 (7.665)	-14.476 (9.676)	11.071 (9.676)
Opportunity Atlas: Intergen Mobility	0.025 (0.017)	0.025 (0.019)	0.000 (0.019)
Opportunity Atlas: Frac of Pop in Jail	-0.004 (0.003)	-0.003 (0.003)	-0.002 (0.003)
Opportunity Atlas: Teen Birth Rate	-0.043* (0.024)	-0.049* (0.028)	0.012 (0.028)
Cumulative Number of Divorces	-0.048 (0.039)	-0.046 (0.048)	-0.003 (0.048)
VantageScore	56.161* (32.414)	67.069 (43.252)	-53.557 (43.252)
Has Had Bankruptcy	0.094 (0.154)	0.181 (0.184)	-0.158 (0.184)
Number of Active First Mortgages	-0.522* (0.284)	-0.702** (0.354)	0.324 (0.354)
Ever Defaulted on a First Mortgage	0.242 (0.165)	0.290 (0.198)	-0.087 (0.198)
Ever Defaulted on a Home Equity Loan	-0.134 (0.161)	-0.142 (0.191)	0.014 (0.191)
Has a Home Equity Account (excl. Mortgages)	-0.119 (0.126)	-0.188 (0.155)	0.126 (0.155)
Share of First Mortgage in Default	-0.241 (0.196)	-0.212 (0.259)	-0.044 (0.259)
Share of Non-Mortgage Accts in Default	0.142 (0.134)	0.210 (0.157)	-0.136 (0.157)
Share of Credit Card in Default	0.214 (0.147)	0.264 (0.174)	-0.091 (0.174)
Share of Auto Loan in Default	1.685 (2.067)	10.855 (90.296)	-5.910 (90.296)
Share of Student Loan in Default	-0.884 (0.554)	0.140 (0.988)	-2.334 (0.988)

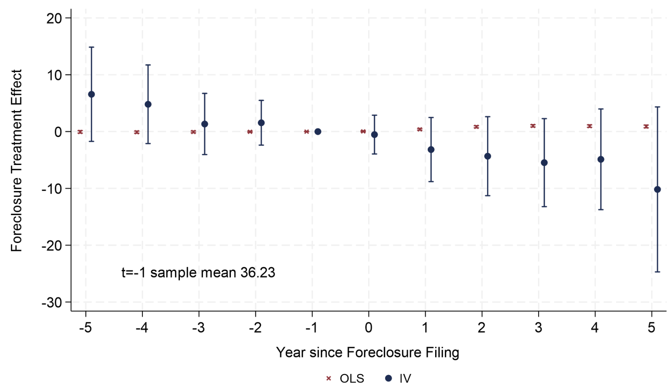
Notes: This table shows the baseline IV results (column 1) and IV results with both foreclosure and case length being over 1 year both included as regressors (columns 2 and 3). All regressions are weighted by the inverse number of people per case. All standard errors are clustered by case. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.7: Treatment Effects of Foreclosure and Time to Foreclosure Summary, Renters

Specification:	IV Baseline	IV Both	
Coefficient:	Foreclosure	Foreclosure	Case Length
	(1)	(2)	(3)
Moved from Foreclosure Address	-0.007	-0.030	0.069
	(0.128)	(0.146)	(0.146)
Cumulative Number of Moves	0.043	0.022	0.063
	(0.173)	(0.196)	(0.196)
Owns Primary Residence	0.136	0.169	-0.102
	(0.083)	(0.104)	(0.104)
Square Footage of Living Space	126.970	126.266	2.004
	(123.537)	(150.867)	(150.867)
Log Zip Code Average Income	-0.085	-0.113	0.068
	(0.059)	(0.076)	(0.076)
Elementary School Test Score Rank	2.202	0.722	3.380
	(5.376)	(6.994)	(6.994)
Middle School Test Score Rank	5.957	7.180	-3.455
	(5.341)	(6.112)	(6.112)
High School Test Score Rank	6.606	6.865	-0.675
	(5.860)	(7.209)	(7.209)
Opportunity Atlas: Intergen Mobility	-0.009	-0.008	-0.002
	(0.017)	(0.020)	(0.020)
Opportunity Atlas: Frac of Pop in Jail	-0.000	-0.001	0.002
	(0.004)	(0.004)	(0.004)
Opportunity Atlas: Teen Birth Rate	0.030	0.037	-0.017
	(0.026)	(0.031)	(0.031)
Cumulative Number of Divorces	-0.017	-0.016	-0.003
	(0.029)	(0.033)	(0.033)
VantageScore	-4.144	-4.223	0.980
	(37.400)	(43.287)	(43.287)
Has Had Bankruptcy	0.007	-0.003	0.038
	(0.097)	(0.111)	(0.111)
Number of Active First Mortgages	-0.046	0.039	-0.313**
	(0.163)	(0.200)	(0.200)
Ever Defaulted on a First Mortgage	0.009	0.023	-0.052
	(0.073)	(0.083)	(0.083)
Ever Defaulted on a Home Equity Loan	0.017	0.027	-0.036
	(0.060)	(0.068)	(0.068)
Has a Home Equity Account (excl. Mortgages)	-0.096	-0.083	-0.048
	(0.084)	(0.095)	(0.095)
Share of First Mortgage in Default	0.041	2.145	-1.656
	(0.254)	(13.130)	(13.130)
Share of Non-Mortgage Accts in Default	0.045	-0.011	0.133
	(0.162)	(0.224)	(0.224)
Share of Credit Card in Default	0.007	-0.037	0.174
	(0.148)	(0.172)	(0.172)
Share of Auto Loan in Default	-0.487	-5.860	2.067
	(1.455)	(55.226)	(55.226)
Share of Student Loan in Default	-0.008	-0.068	-0.146
	(0.439)	(0.495)	(0.495)

Notes: This table shows the baseline IV results (column 1) and IV results with both foreclosure and case length being over 1 year both included as regressors (columns 2 and 3). All 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 A.4: Owners: Additional Neighborhood Quality Outcomes



(a) High School Test Score Rank

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

F Additional Background

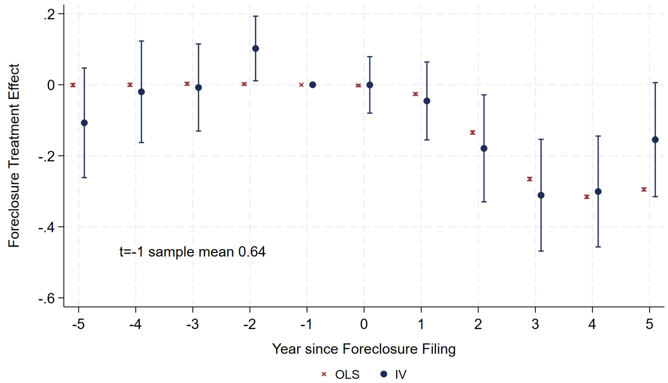
F.1 Divorce Laws in Illinois

Illinois is an "equitable division" state for contested divorces, meaning that the marital assets are divided "equitably" rather than 50/50. The judge weighs factors such as the length of the marriage, the financial resources of both spouses, their employability, and their contributions to the acquisition of the property. Assets are classified as either "marital property" belonging to both spouses or "separate property" belonging to an individual. Marital property is acquired during the marriage, and separate property is acquired before marriage. Gifts given to one spouse are considered separate property if not commingled with other assets belonging to both spouses, such as by depositing the funds into a joint bank account. These rules apply only to contested divorces. For uncontested divorces, all property may be split however the spouses want.

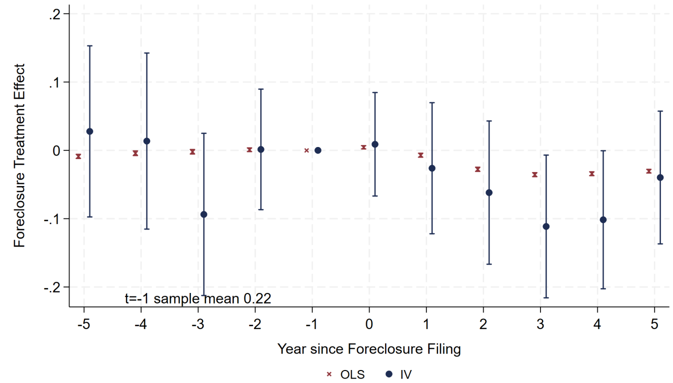
In a contested divorce, the spouse with primary custody of the children often keeps the house (Anderson and Associates, 2015). Sometimes, the award is temporary, and the house must be sold once the youngest child turns 18. Even if a spouse is given the home, they may still be required to buy out their partner's interest, or the higher-earning spouse may still be required to pay the mortgage as part of a support arrangement.

The house does not count as marital property if the spouse is a beneficiary of a revocable trust, because the property is under the grantor's control. If the spouse is a beneficiary of an irrevocable trust, then the property does not count as marital property as long as the property is not mingled with marital property. If the spouse is a grantor of a revocable trust, then the property counts as marital property unless it was acquired before the marriage or was a gift given to only one spouse and not commingled. Income derived from a trust counts toward the beneficiary spouse's income (Law Offices of Schlesinger and Strauss, 2017; Mirabella, Kincaid, Frederick, & Mirabella, LLC, 2015).

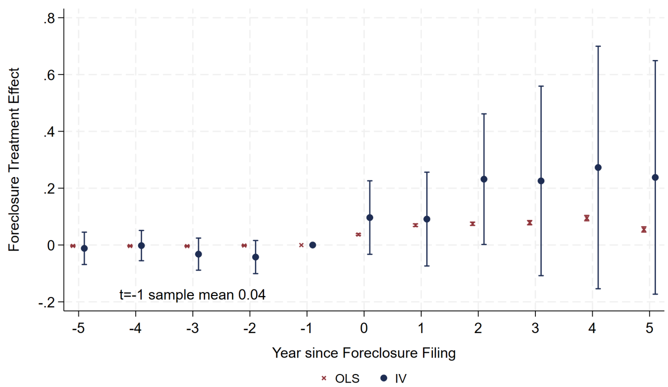
Figure A.5: Owners: Additional Financial Outcomes



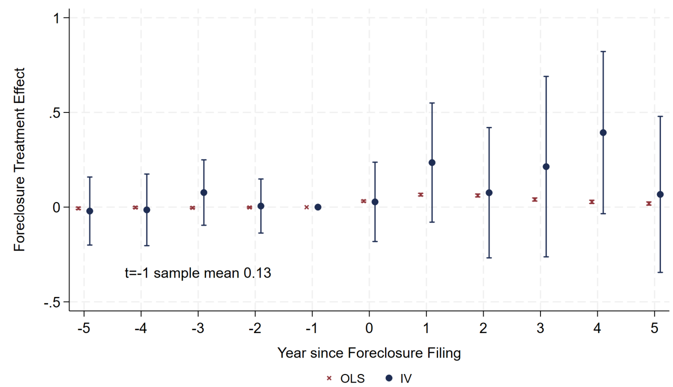
(a) Has a First Mortgage Account



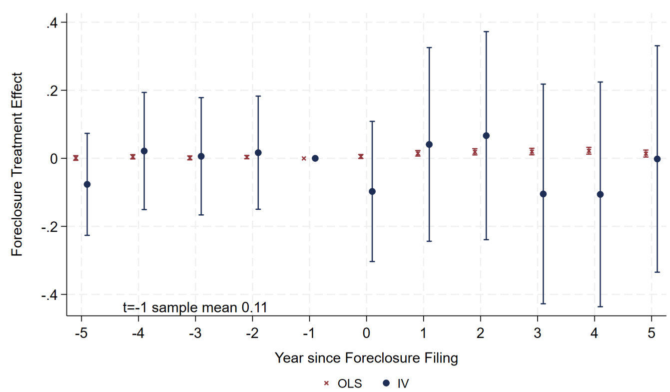
(b) Has a Home Equity Account



(c) Share of First Mortgages In Default



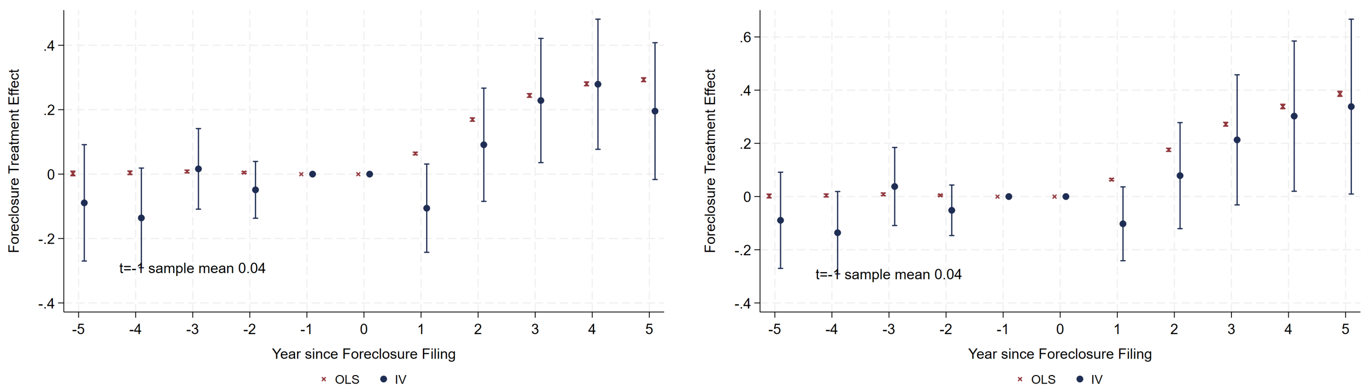
(d) Share of Auto Loans in Default



(e) Share of Student Loans In Default

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

Figure A.6: Owners: Moving According to Credit Report's ZIP

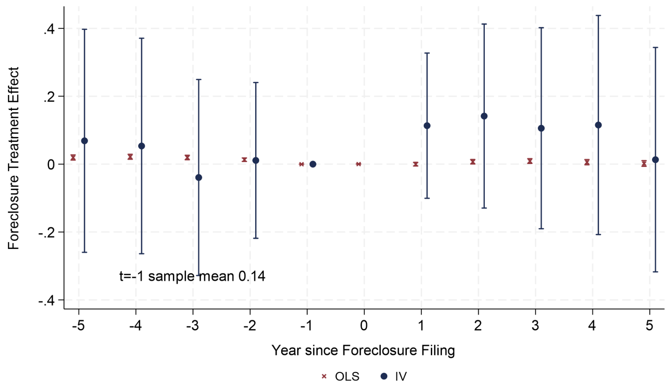


(a) Moved From Foreclosure ZIP (Credit Report)

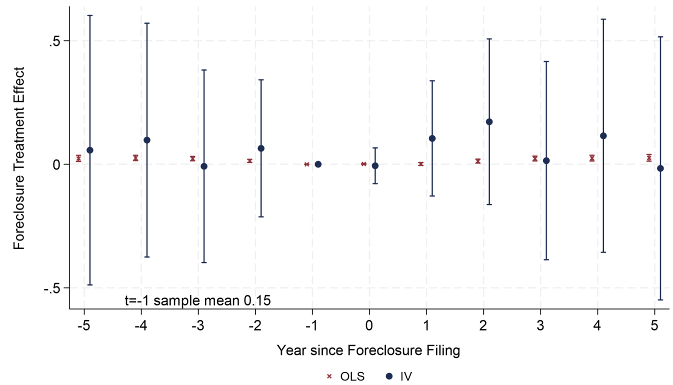
(b) Cumulative Number of Moves (Credit Report)

Notes: Each panel shows IV and OLS results for the indicated outcome variable for owners. The sample is restricted to owners whose credit report ZIP matches the foreclosure property ZIP at filing. 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

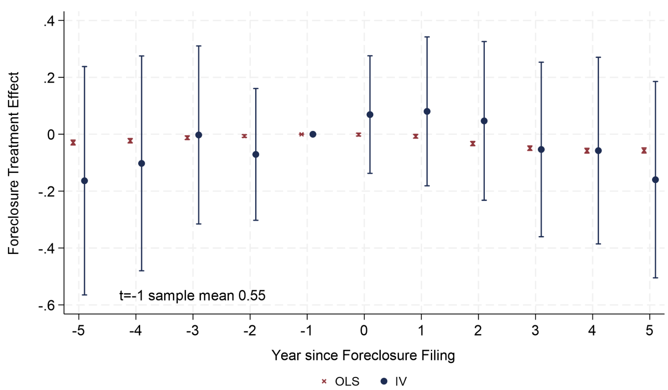
Figure A.7: Landlords: Moving, Homeownership, and House Characteristics



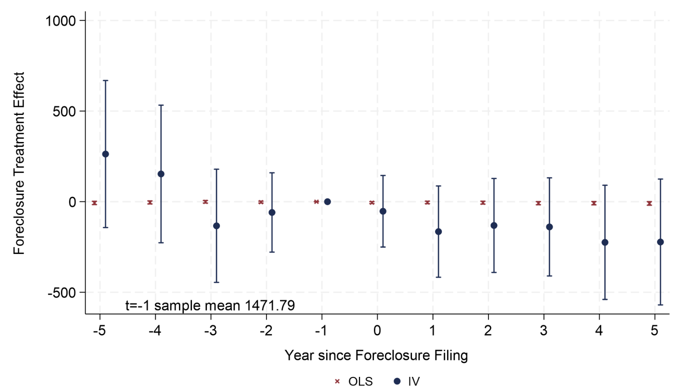
(a) Moved from Address At Time of Foreclosure



(b) Cumulative Number of Moves



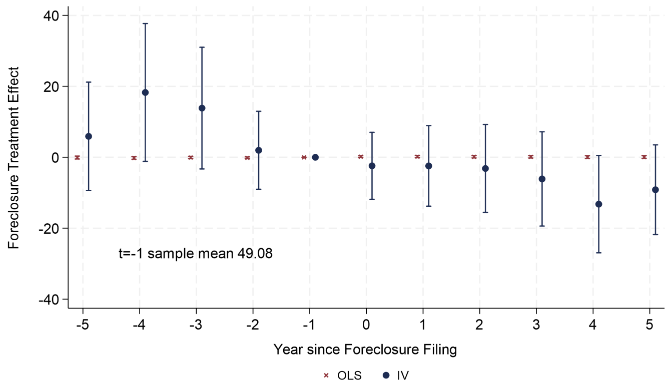
(c) Owns Primary Residence



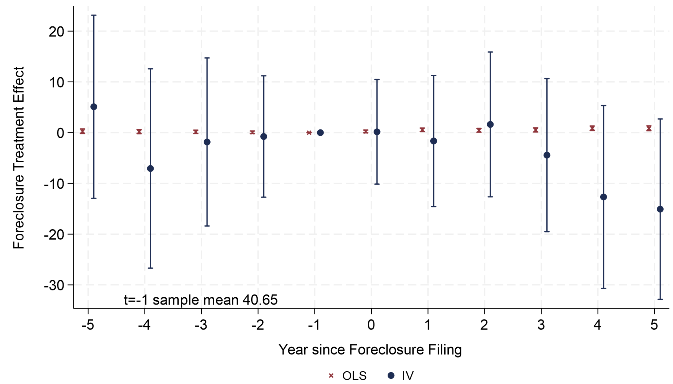
(d) Square Footage of Residence

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS 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.

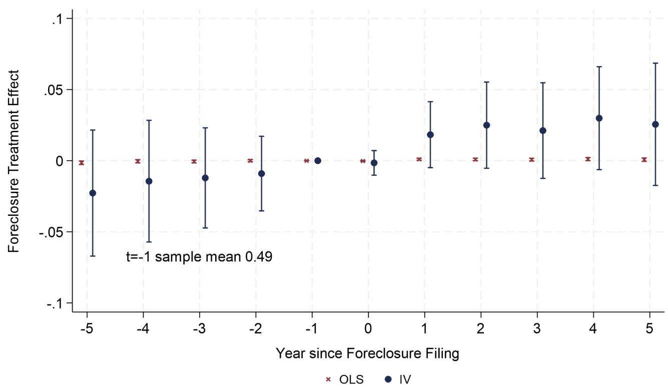
Figure A.8: Landlords: Neighborhood Characteristics



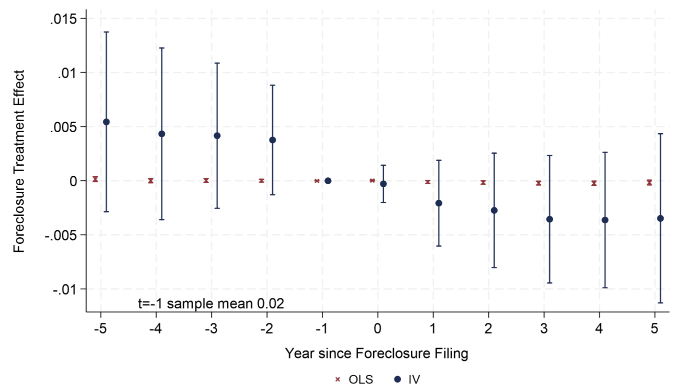
(a) Middle School Test Score Rank



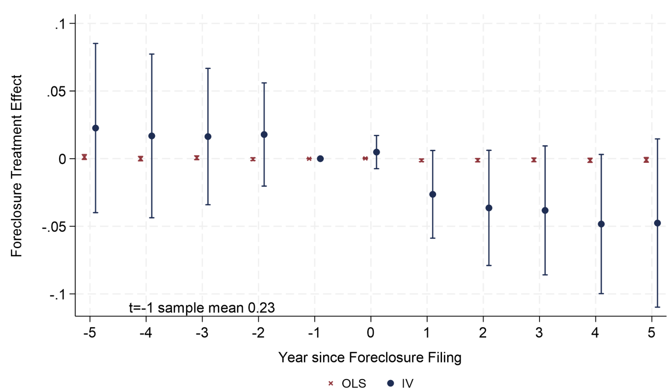
(b) High School Test Score Rank



(c) E[Adult Income Rank] of Kids



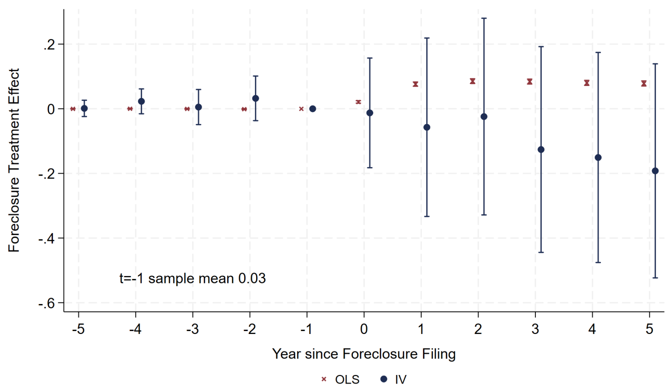
(d) Pct of Pop in Jail



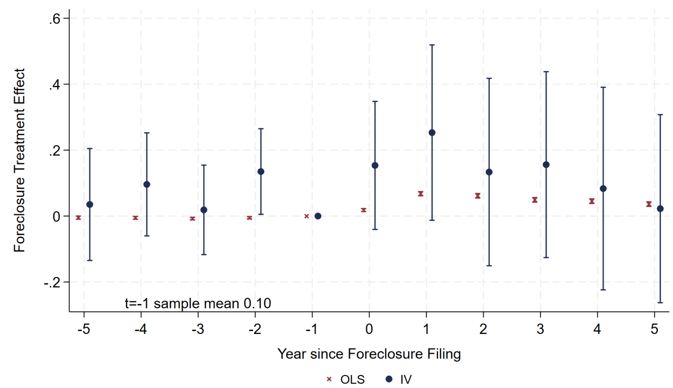
(e) Teen Birth Rate

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case. For test scores, the dependent variable is the average percentile rank of the local school in math and reading (a coefficient of 1 indicates a 1-percentage-point change in the average rank). The Illinois Board of Education reports these percentages for math and reading separately, and we combine them into a single average index. The Opportunity Atlas variables are the mean percentile rank of children of children who grew up in the census tract in the national earnings distribution ("kfr" in their codebook), the teen birth rate of women who grew up in the census tract, and the percentage of the population who grew up in the census tract who were ever incarcerated.

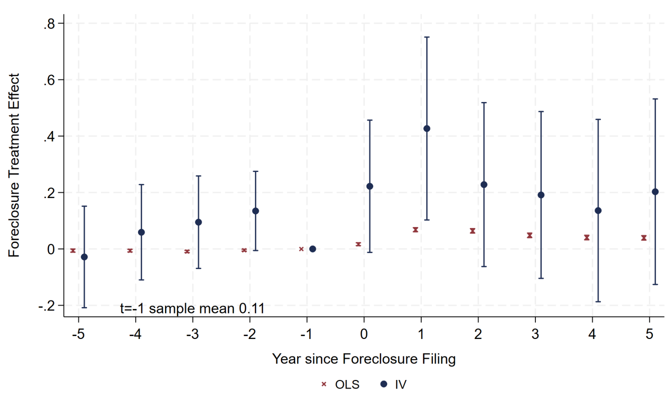
Figure A.9: Landlords: Financial Outcomes



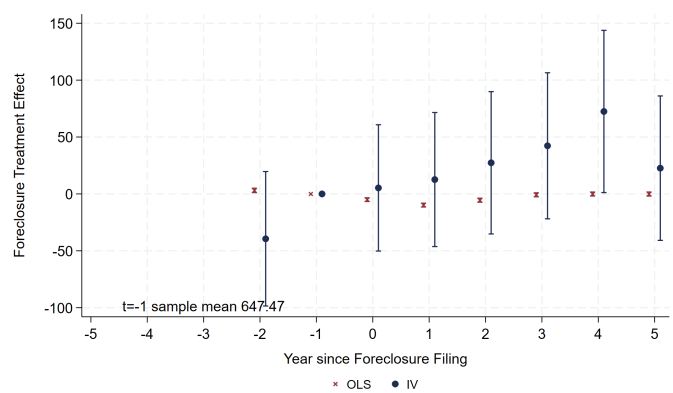
(a) Ever Defaulted on a Home Equity Loan



(b) Share of Non-Mortgage Accounts in Default



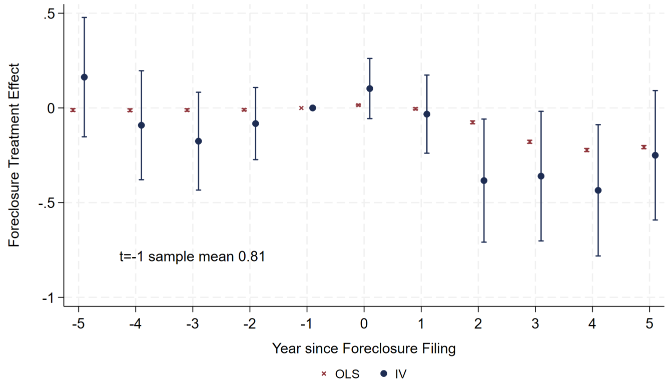
(c) Share of Credit Cards in Default



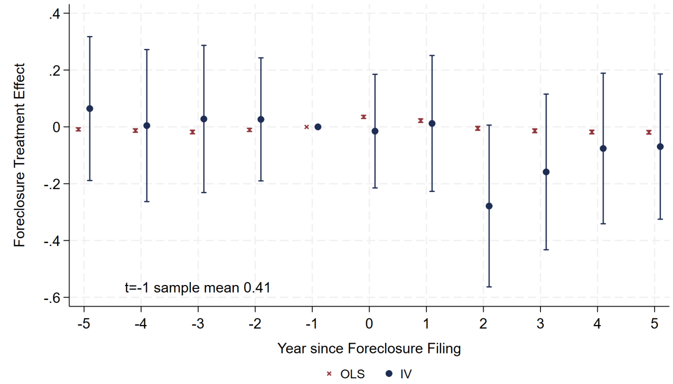
(d) Vantage Score

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS 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.

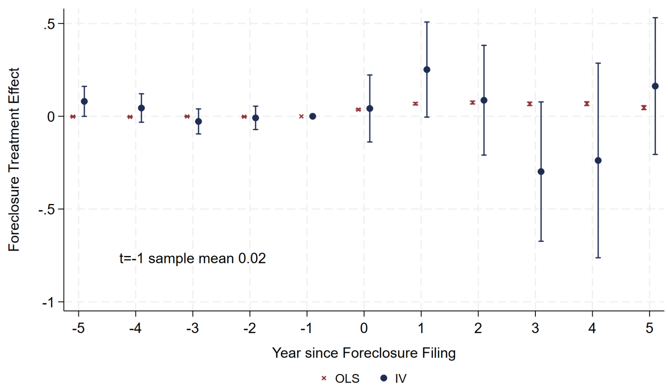
Figure A.10: Landlords: Additional Financial Outcomes



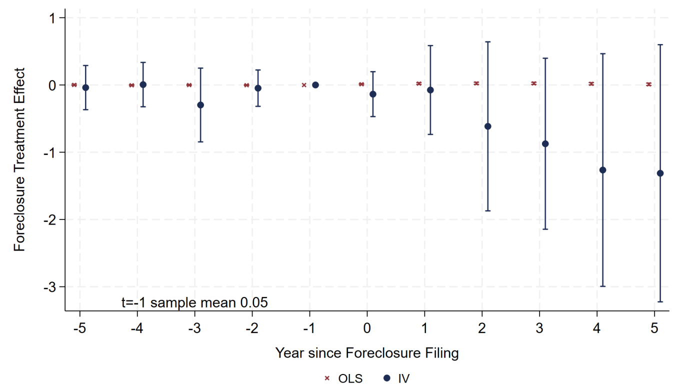
(a) Has a First Mortgage Account



(b) Has a Home Equity Account



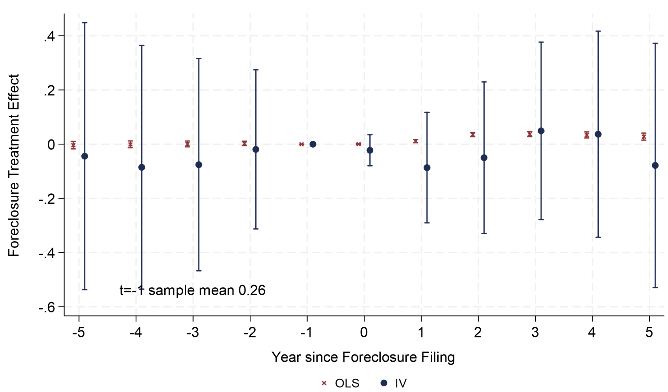
(c) Share of First Mortgages In Default



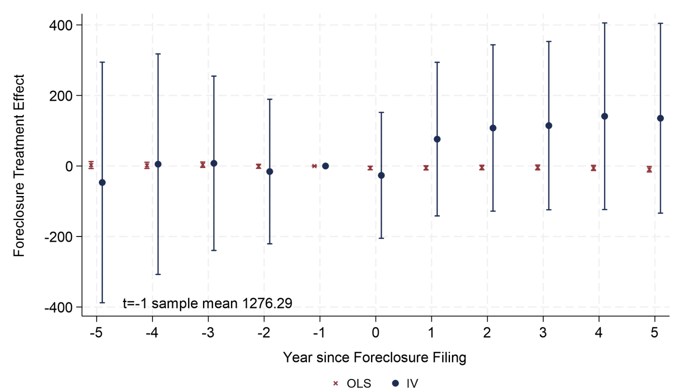
(d) Share of Student Loans In Default

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

Figure A.11: Renters: Moving and House Characteristics



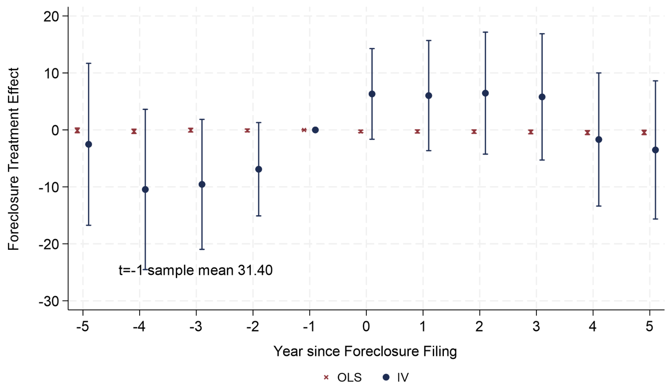
(a) Cumulative Number of Moves



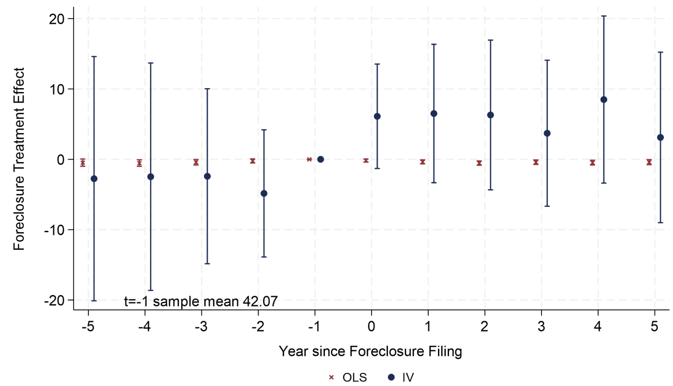
(b) Log Square Footage of Residence

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

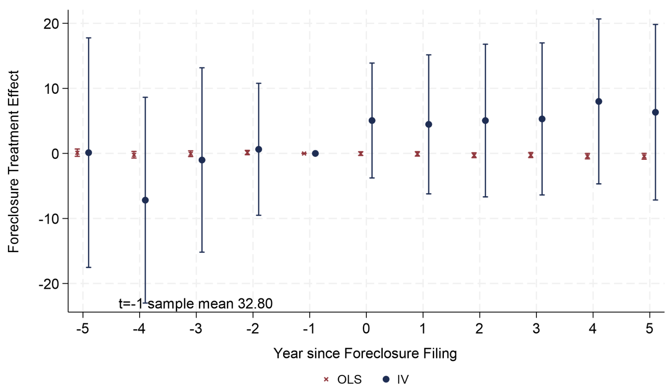
Figure A.12: Renters: Neighborhood Characteristics



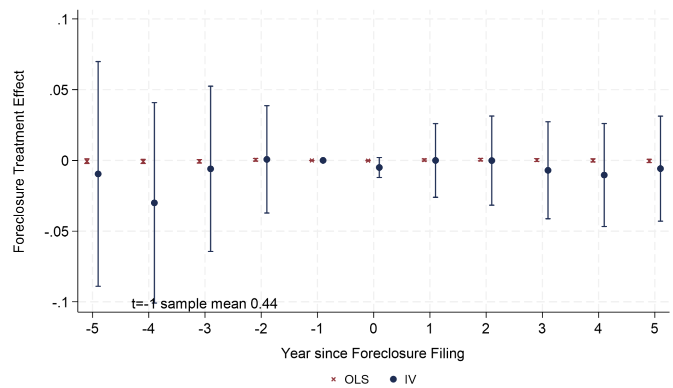
(a) Elementary School Test Score Rank



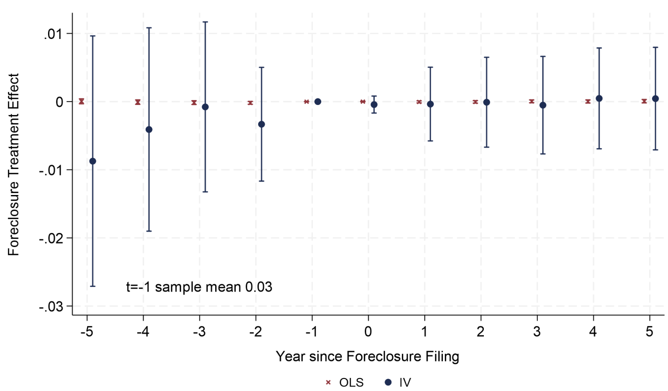
(b) Middle School Test Score Rank



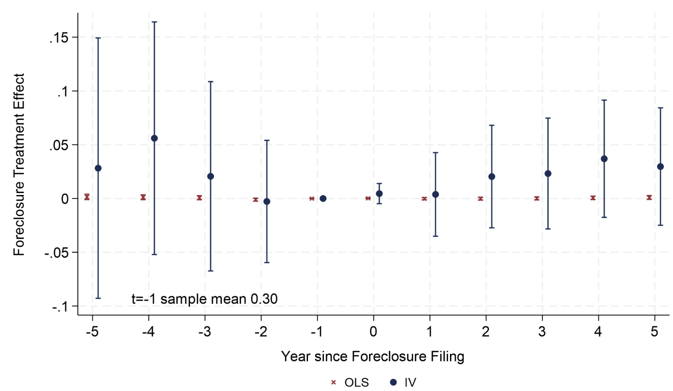
(c) High School Test Score Rank



(d) E[Adult Income Rank] of Kids



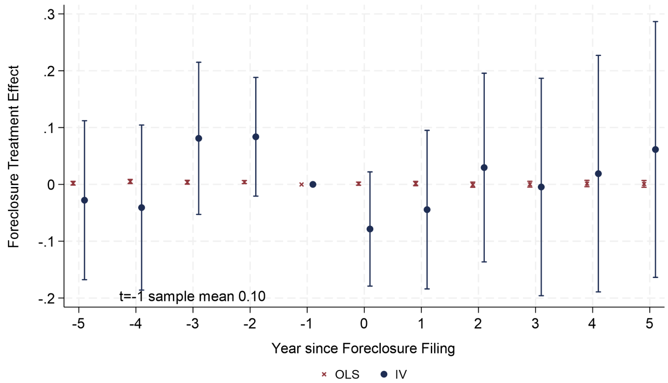
(e) Pct of Pop in Jail



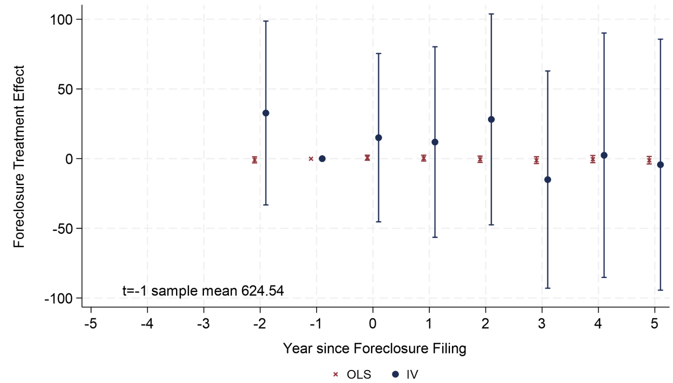
(f) Teen Birth Rate

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse 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.

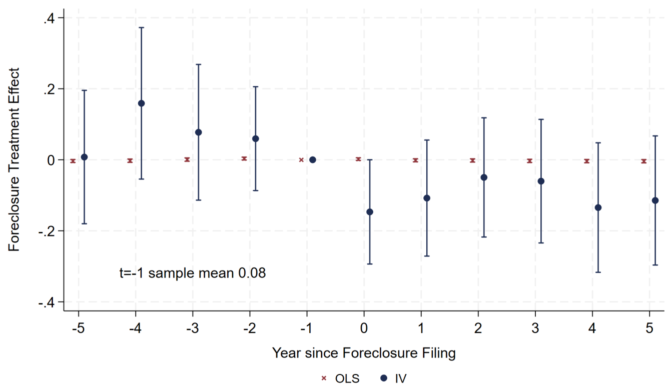
Figure A.13: Renters: Financial Outcomes



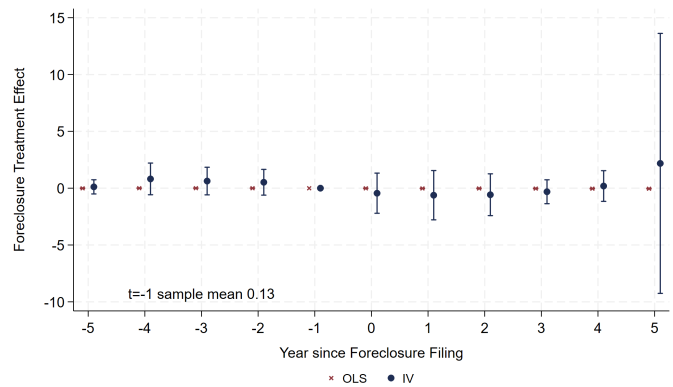
(a) Ever Had a Bankruptcy



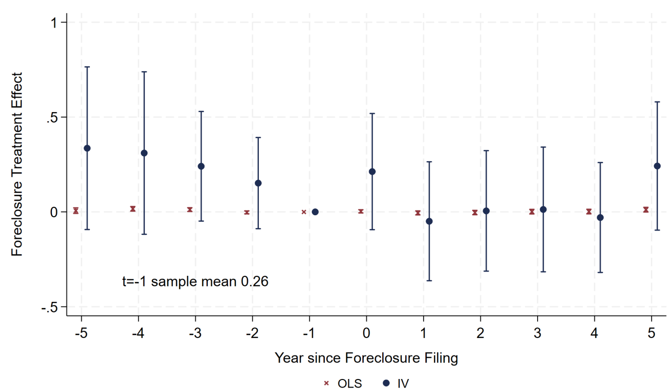
(b) VantageScore



(c) Has a Home Equity Account



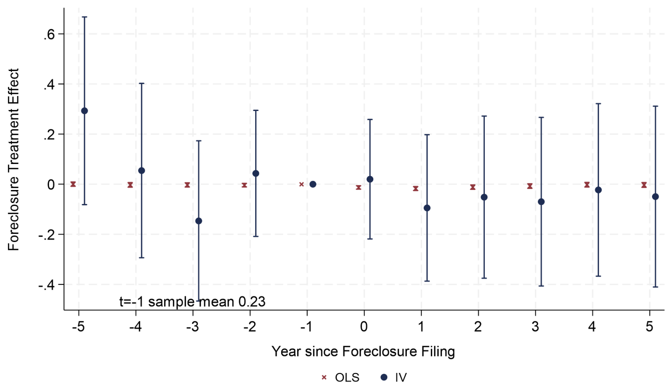
(d) Share of Student Loans in Default



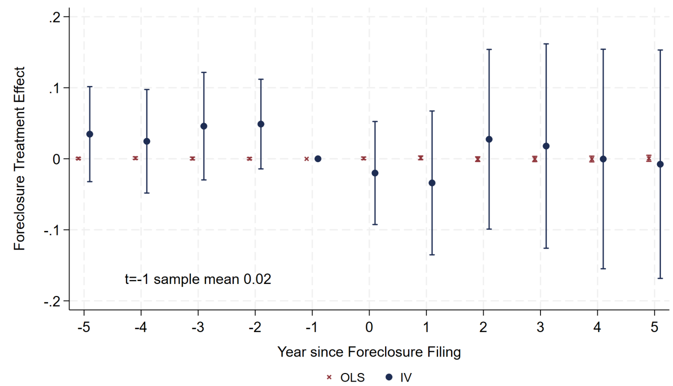
(e) Share of Credit Cards in Default

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.

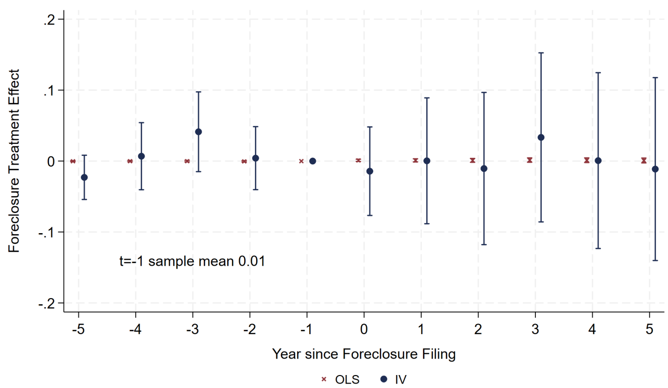
Figure A.14: Renters: Additional Financial Outcomes



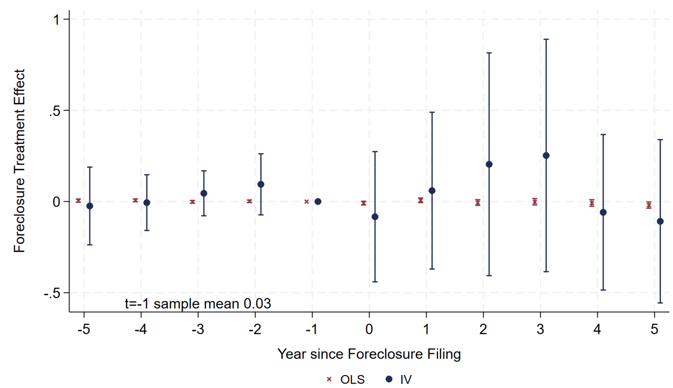
(a) Number of Active First Mortgages



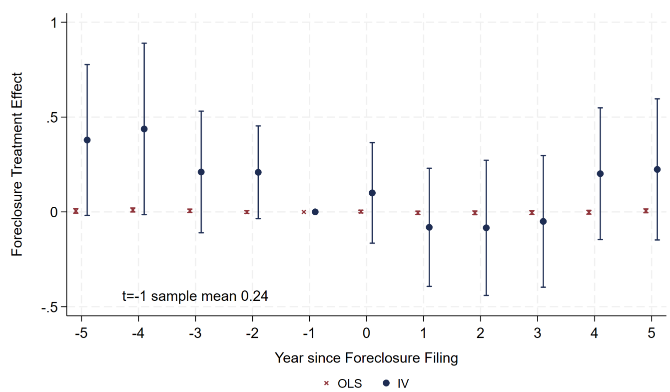
(b) Ever Defaulted on a First Mortgage



(c) Ever Defaulted on a Home Equity Loan



(d) Share of First Mortgages in Default



(e) Share of Non-Mortgage Accounts in Default

Notes: Each panel shows IV and OLS 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 OLS estimate for β_s estimated using equation (1). OLS confidence intervals are small enough that they are not shown. Standard errors are clustered by case, and regressions are weighted by the inverse number of people in each case.