# Household Spatial Effects of Foreclosure Process

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**Abstract**

US homeowning households tend to be less geographically mobile than households that do not own a home. However, it is unknown whether the inducement to move from a legal process that makes foreclosure more likely on the margin can lead households to make beneficial moves even under adverse circumstances. This paper documents how foreclosure process affects the outcomes of US households through location. I link mortgage and foreclosure deeds and property characteristics with residential moves of US households and their corresponding neighborhood characteristics. Variation in the foreclosure process used across US states puts observably similar households at greater risk of foreclosure in those that follow a nonjudicial (faster and cheaper for lender) rather than judicial (slower and more expensive) process. I use the resultant variation in foreclosure rates across borders in a spatial regression discontinuity design (RDD) to estimate local average treatment effects of nonjudicial foreclosure process and, incorporating two-stage least squares, of being foreclosed. Under a nonjudicial foreclosure process, households are more likely to be foreclosed (2 pp), to move out of their home (3 pp), and to undertake more distant moves (2 pp). Moves come not only from completed foreclosures but also from preventative moves by households at risk of foreclosure. Furthermore, households experience relatively greater increases in destination relative to origin tract median income ($1,000) and household income rank at age 29 of children who grew up there (0.5 percentile) under a nonjudicial rather than judicial foreclosure process. Together these results suggest that any potential long-term negative effects of foreclosure on household members are not driven by adverse changes in neighborhood characteristics.

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# 1 Introduction

Studies of home foreclosure have historically focused on its market consequences for market participants: mortgage lenders, borrowers, insurers, servicers, etc. More recently, economists have begun to explore consequences outside of the mortgage market for borrowers. Studies by Currie and Tekin (2015) and Diamond et al. (2020) respectively find adverse health and labor market outcomes for foreclosed-upon borrowers.

However, it is not well understand where households relocate in the wake of foreclosure and whether the characteristics of these destination neighborhoods are systematically different from their origins. On the one hand, foreclosed households could be expected to make suboptimal moves given scarcity of resources like time and income that they may have during the stressful event of a foreclosure. On the other hand, foreclosures come with the opportunity to re-optimize location choice once pushed to move by removal of the default option to stay in place. Despite the value to policymakers of housing and finance, causal estimates of the effect of foreclosure on households remain elusive because of confounding variables like household income and neighborhood quality.

This paper overcomes these identification challenges to answer how foreclosure of parents affects the economic outcomes of children. To do so I combine a source of exogenous variation in the foreclosure rates faced by children with the method of spatial regression discontinuity design (RDD). Together these give a causal interpretation to differences in household outcomes as local average treatment effects.

To identify effects I focus on US households single-family home or condo was foreclosed in 2007–2012, tracing moves outcomes through 2014. I restrict attention to properties with a loan no more than fifteen years old in 2007. These loans were made in a lending environment where foreclosures were comparatively uncommon and unanticipated by borrowers. The financial crisis of 2007–2008 and ensuing Great Recession unleashed declines in both house prices and employment income that together left many US borrowers delinquent, i.e., three months past due on their mortgages. Lenders completed about six million foreclosures on delinquent borrowers in this period, and only a quarter of affected households regained homeownership (Piskorski and Seru (2021)).

To observe which mortgaged homes get foreclosed, I begin with data from CoreLogic parcel-level deed and property tax records. To track moves and thereby assess neighborhood quality, I link this to address histories from Infutor Identity Graph. With this panel I can also track household moves before and after the foreclosure, comparing the destination to the origin, etc. Public data from Opportunity Insights and IRS Statistics of Income provides tract-level and ZIP-level characteristics of neighborhoods, respectively.

Variation in the foreclosure process used across US states places observably similar borrowers at higher risk of foreclosure in some states, and I use this variation to identify effects of foreclosure. Following Demiroglu et al. (2014) and Mian et al. (2015), I categorize states into those following judicial or nonjudicial foreclosure processes. The authors document substantial evidence that judicial foreclosure processes are slower, more costly to a lender, and less likely to complete foreclosure relative to nonjudicial processes. These differences typically emerged before the US Civil War as a result of how state judges interpreted case law (Ghent (2012)).

I identify effects of foreclosure by focusing on residents of mortgaged homes along US state borders where differences in foreclosure law mean that comparable delinquent borrowers face a slower judicial foreclosure process on one side and a faster nonjudicial foreclosure process on the other. This produces sharp changes in foreclosure rates across relevant state borders, namely, lower rates in judicial states. Our identification assumes that other determinants of child outcomes vary smoothly across the borders. I use this variation in foreclosure rates across borders in a spatial RDD to estimate local average treatment effects of foreclosure. This quasi-experimental design gives estimates approaching those of the ideal experiment of random assignment to foreclosure while avoiding its ethical and practical defects. I offer new guidance on how to combine separately-identified estimates from spatial RDDs over multiple borders.

I find statistically and economically significant effects of nonjudicial foreclosure process on households. Under a nonjudicial foreclosure process, households are more likely to be foreclosed (2 pp from a base of 5 pp or 40% increase), to move out of their home (3 pp from a base of 23 pp or 13% increase), and to undertake more distant moves over 80 km, i.e., nearly 50 miles, (2 pp from a base of 8 pp or 25% increase). Moves come not only from completed foreclosures but also from preventative moves by households at risk of foreclosure. Furthermore, households experience relatively greater increases in destination relative to origin tract median income ($1,000) and household income rank at age 29 of children who grew up there (0.5 percentile) under a nonjudicial rather than judicial foreclosure process.

# 2 Literature

The financial crisis of 2007–2008 and ensuing Great Recession unleashed drops in both house prices and employment income that together brought many mortgage borrowers into delinquency. Lenders completed about six million foreclosures in this period, and only a quarter of affected households regained homeownership (Piskorski and Seru (2021)).

A central preoccupation in the foreclosure literature both before and after this crisis is whether

borrowers engage in "strategic" or "ruthless" default, i.e., choose to default on mortgages that they could afford to keep paying. The motivation for strategic default is negative equity, i.e., the debt outstanding on the mortgage is greater than the value of the house. As Vandell (1995) describes, mortgage pricing based on an option-theoretic model assumes strategic default will occur immediately. Nonetheless, this disregards several realities that matter in practice: borrower transaction costs from default, borrower solvency, and lender discretion to foreclose.

An explanation for foreclosure related to strategic default is the "double-trigger" hypothesis, namely that households will only default on their mortgages when negative equity beyond some threshold is combined with a negative income shock. Foote et al. (2008) finds that negative equity is a necessary condition for foreclosure since borrowers with positive equity that would like to move will instead sell the house and repay the mortgage. Nonetheless, the authors also find that most borrowers with negative equity will not be foreclosed on. They interpret this finding as evidence that continued mortgage payments are worthwhile to these borrowers for expected income from remaining in that home. This could make sense if the borrower needs to continue living in the area for a job but could not rent for a lower amount than the monthly payment.

Combining loan-level mortgage data with consumer-level credit bureau data, Elul et al. (2010) find that both negative equity and illiquidity (as measured by high credit card utilization) are significantly associated with mortgage default and have similar magnitude of marginal effects. Their finding suggests that liquidity is an important consideration, consistent with the double-trigger hypothesis. Providing more detailed evidence, Bhutta et al. (2017) identify strategic defaults but find that borrowers far more commonly reach negative equity well below the value of the house before they default. The authors interpret this in light of repeated survey evidence that most US borrowers are morally averse to strategic default and feel obligated to repay, arguing that behavioral factors such as loss aversion, overestimation or transaction costs from default, or attachment to one's house may be at play.

Borrower creditworthiness is another key dimension of prior work on foreclosure. Ferreira and Gyourko (2015) use predecessor data to CoreLogic (DataQuick) to understand factors leading borrowers to default on their loans in the foreclosure crisis. They have rich panel data and many controls but lack strong identification, so they cast their results as associations. They find that while subprime borrowers tended to experience foreclosures earlier in the crisis period, many more houses were lost by prime borrowers as the composition of foreclosures changed over time. This has implications for my project in assessing different motivations for lenders to foreclose or borrowers to default in the first place, as I may wish to exclude borrowers who strategically default (and are more likely to be prime).

Mian et al. (2015) examine the effect of state laws defining a judicial (e.g., New York) or nonjudicial (e.g., California) as an instrument for probability of foreclose on a home. The authors use Fiserv Case Shiller Weiss (FCSW) and Zillow.com data on house prices and Equifax data on foreclosures, all at the quarter-by-ZIP-code level. They document substantial evidence that judicial foreclosure processes are slower, more costly to a lender, and less likely to complete foreclosure relative to nonjudicial processes. Below I consider using state foreclosure law as a secondary part of my instrumental variables strategy.

Finally, some borrowers may have avoided foreclosure with assistance from two federal programs begun in 2009: the Home Affordable Modification Program (HAMP) and the Home Affordable Refinance Program (HARP). While HARP involved new financing for a home mortgage, HARP modified the existing financing in one or more of several possible ways. Recent evidence from Ganong and Noel (2020) suggests that HAMP modifications that reduced short-term payments to improve liquidity were more effective in avoiding default than reducing long-term obligations without changing liquidity.

Recent work in economics on effects of place demonstrates that neighborhoods have a strong and cumulative effect throughout childhood on later-life outcomes. Chetty et al. (2016), Chetty and Hendren (2018a), and Chetty and Hendren (2018b) show using the IRSStatistics of Income Databank that children who move to higher- (or lower-) opportunity neighborhoods accrue better (or worse) chances of attending college and higher (or lower) expected income in adulthood. One might therefore be concerned that foreclosure creates or enlarges childhood exposure to disadvantage, potentially through leaving or entering particular neighborhoods. Among other identification strategies, the authors compare siblings who move at different ages from the same origin to the same destination.

There is also a rich history in sociology of studying childhood exposure to disadvantage. Work examining duration of exposure to disadvantage through the channel of neighborhood generally find that this correlates negatively with education outcomes, as in Wodtke et al. (2011). Given the wide range of studies and very different methods used outside economics, I do not detail these here, but data sources include surveys, semi-structured interviewing, and field observation. These suggest the importance of avoiding disadvantage.

In the context of foreclosure, it is ambiguous whether a foreclosed household relocates into a more or less disadvantaged neighborhood given that it is more likely than not to be disadvantaged itself, though foreclosures can happen throughout the household income distribution. It is also ambiguous whether the household's financial position is improved in losing both an asset whose value likely dropped in this period as well as a debt that may be fully discharged regardless of the

foreclosure sale price in many states. Thus we need data to understand how households respond to conditions that make foreclosure possible and the event of foreclosure itself.

# 3 Data

I use CoreLogic proprietary aggregation of public records from US municipalities, including deeds, tax assessor, and pre-foreclosure records. Significantly, I have access to transaction history, property characteristics, and legal claims that I can link at the property level from 2007–2019 for most of the US and even decades earlier in some counties. The relevant variables in each are:

- Transaction-level deeds: transaction date, buyer, seller, lender, loan type, sale type, foreclosure flag, etc.
- Assessor-level tax records: property characteristics, e.g., age, size, number of bedrooms and bathrooms, assessed value, etc.
- Pre-foreclosure records: *lis pendens* notification of delinquency, foreclosure, trustee sale, etc. with dates and contemporaneous parties (lenders, owners, trustees, etc.).

I document coverage of CoreLogic deed data covers my preferred foreclosure window of 2007–2012. Counties representing 88.9% of housing units and 89.5% of population have foreclosure data for all six focal years. Counties representing 92.6% of housing units and 93.1% of population have foreclosure data for at least five of six focal years. Given the small improvement in coverage from allowing partial coverage of the window, I restrict to counties reporting in the full six-year period. Counties with partial coverage tend to be rural, though some states have more or less complete coverage due to historic interest from participants in the mortgage industry who purchase CoreLogic's data. I combine this with Infutor Identity Graph data that gives the address histories of 125 million households and 260 million adults matched to CoreLogic. Infutor uses a host of sources in its aggregation "including sources such as phone books, voter files, property deeds, magazine subscriptions, credit header files, and others" as described in Diamond et al. (2019). I use location outcomes for all adults whose surnames match to a name on the most recent deed. For all moving variables that I construct, a household is only considered to have moved if all matched adults move. This prevents cases where one adult leaves the house, e.g., to go to college or to marry, from being flagged as household moves if at least one other adult stays behind. This concept of household move is appropriate to foreclosure because completed foreclosures ultimately require the entire foreclosed household to move except in the relatively infrequent case of being able to remain as new renters.

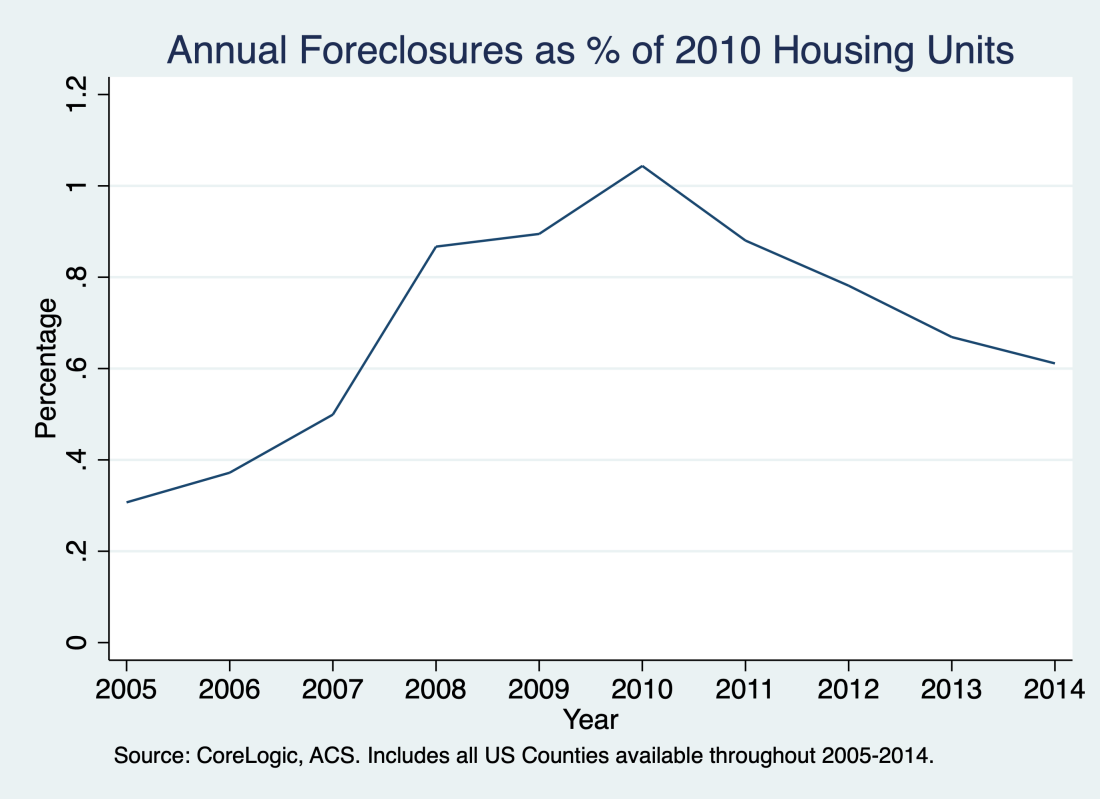
To identify moves across school districts, I compare children's addresses to the School District Boundaries shapefiles from the National Center for Education Statistics (NCES). To identify

performance across school districts, I am exploring data for each district, grade, and year from the National Assessment of Educational Progress (NAEP) or state-level subject-matter achievement scores, e.g., through the Stanford Education Data Archive. Following Ho (2007), I take care to compare scores across states and begin by assessing the prevalence of interstate moves for families in my sample.

I use data from Opportunity Insights for tract-level characteristics and estimates of economic mobility as well as United States Census Bureau (2010) Gazetteer Files for county-level counts of housing units and population. I link HMDA loan applications that were approved to obtain race of the borrowers. I use the Global Subnational River Borders from Popelka and Smith (2020) to determine which state borders are rivers.

The US experienced a substantial increase in foreclosures per year starting in 2007 and peaking in 2008–2009, nearly tripling in this period, as Mian et al. (2015) report. I replicate this finding in Fig. 1, which confirms that my 2008–2012 sample period focuses on the five years of highest foreclosure rates nationally.

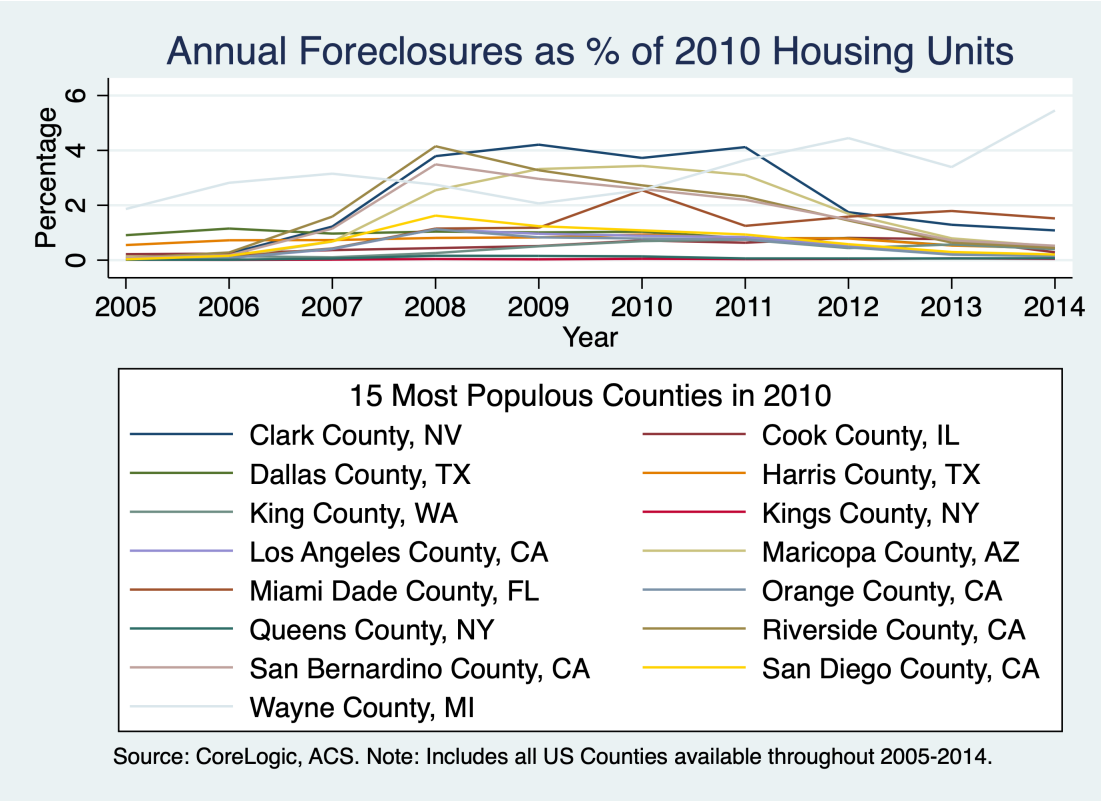
Figure 1: Annual Foreclosures in US, 2005–2014




Nonetheless, some geographies had markedly different patterns. To illustrate this I focus on the

fifteen largest counties by Census 2010, as shown in Fig. 2. Of these, six reach foreclosure rates above 2% per year and mostly follow a similar time trend to the rest of the county with amplified levels. Wayne County, Michigan, illustrates a different trend, however, as the automotive industry centered in county seat Detroit had already been suffering joblessness and outmigration several years before the national foreclosure crisis began. The other nine counties have low base rates of foreclosure and vary much less over time, suggesting that they weathered the Great Recession relatively unaffected in terms of foreclosure.

Figure 2: Annual Foreclosures in 15 Largest Counties, 2005–2014




# 4 Research Design

Comprehensive nationwide statistics on comparative outcomes for those who do or do not experience foreclosure as a child would be a substantial advance in current knowledge. To further address whether foreclosure causes these outcomes, I pursue a regression discontinuity design using variation across states in lenders' access to nonjudicial foreclosure.

## 4.1 Judicial vs. nonjudicial foreclosure process

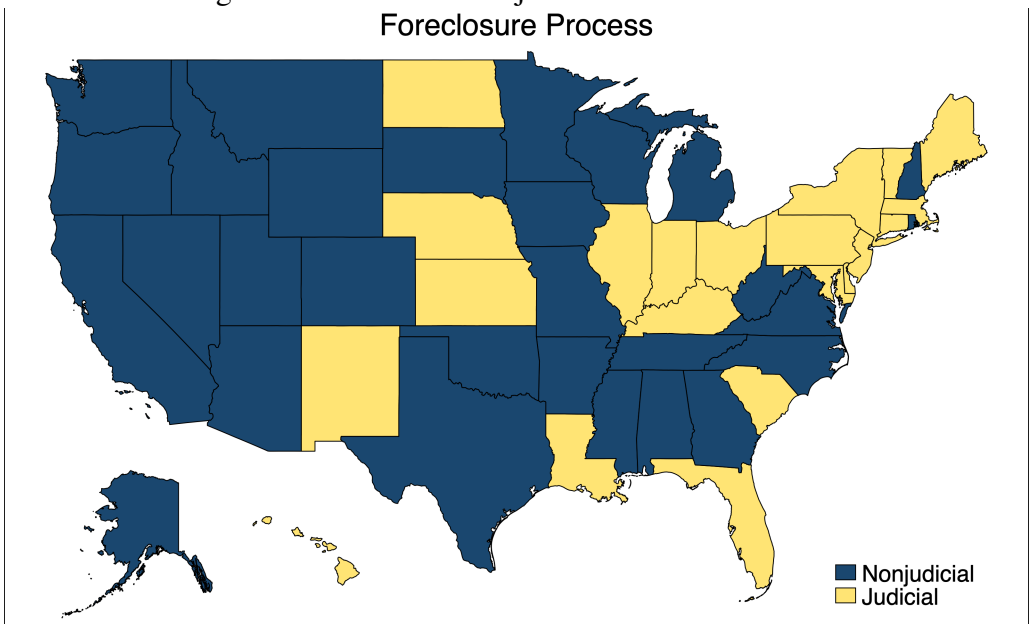
Variation in the process of foreclosure may serve as an additional instrument for completed foreclosure. Following Demiroglu et al. (2014) and Mian et al. (2015), I categorize states into those following judicial or nonjudicial foreclosure processes.

The authors document substantial evidence that judicial foreclosure processes is slower, more costly to a lender, and less likely to complete foreclosure relative to nonjudicial processes. Barring systematic differences between the characteristics of states with each type of law, I consider nonjudicial foreclosure process as an instrument for foreclosure completion.

In the 21 judicial states shown in Fig. 3, the lender generally must first sue the borrower in court before the lender may schedule an auction to sell the property. In the nonjudicial context this auction is often called a trustee's sale because it is based on a deed of trust with a power-of-sale clause. The lender avoids this multistage legal process and may undertake an auction once sufficient notice is given to the delinquent borrower, lowering the cost to foreclosing on a typical borrower.

A distinct but dimension of state law besides foreclosure process is called recourse. This allows lenders to collect any remaining mortgage debt in the event that the foreclosure sale does not pay off the entire debt. While potentially operating to change foreclosure patterns in general, this is present in all but one of the states in my sample, so I confine discussion to the Appendix.

Figure 3: Judicial vs. nonjudicial foreclosure states




One potential concern is that the classification of states into judicial and nonjudicial by Mian et al.

(2015) is too coarse, weakening relevance of the instrument. Some states that allow nonjudicial foreclosure to the lender may impose other borrower protections such as redemption rights that in practice slow down the process, Massachusetts being the most clear example of this. This may include equitable redemption rights, which add time between first delinquency and the earliest allowable foreclosure sale, as well as statutory redemption rights, which add time after the foreclosure sale to allow the original borrower to repay [Ghent (2012)]. This could weaken the instrument since costs to lenders would be less starkly greater in the judicial states than presumed.

A second potential concern is systematic differences between borrowers in judicial and nonjudicial foreclosure states that affect household outcomes through other channels than foreclosure, violating the exclusion restriction. Ideally I could study large-scale reforms such as the introduction of nonjudicial foreclosure, but these are not present in the relevant period when most mortgages were originated or refinanced that would became delinquent in 2007–2012. Mian et al. (2015) show that these correlations are not present for a host of aspects of state law that affect foreclosure, as compiled by Rao and Walsh (2009). Before a foreclosure sale these include access to court review, loss mitigation requirement before foreclosure, right to cure before acceleration, right to reinstate before sale, personal service requirement for complaint or sale notice, and housing emergency assistance fund. After a foreclosure sale these include the right to redeem (purchase the house back with appropriate fees and interest), deficiency judgments (which may be banned or controlled in how they are calculated), accounting of sale proceeds, and prompt return of surplus from sale to the foreclosed borrower.

## 4.2 Spatial regression discontinuity design

I construct running variable $X$ as geodesic distance from border into nonjudicial state of property, using 0 as the border cutoff. Households with negative values are on the judicial side, and those with positive values on the nonjudicial side, so crossing the border from left to right means a faster foreclosure process across all borders. Let $D$ be indicator for nonjudicial foreclosure process, i.e., $D = 1[X > 0]$. Let $F_g = 1[g_i = g]$ be indicators used as fixed effects.

I use local linear regression to explain outcome $Y$ for household $i$ in geography $g$.

$$Y_{ig} = \tau D_i + \beta X_i + \gamma D_i X_i + \alpha_g + \varepsilon_{ig}$$

For this setting I operationalize geography with border county pair segments, much as Black (1999) uses school district boundary pair segments in a forerunner of the contemporary spatial RDD. I take the set of properties whose closest state border boundary is part of the set of border points

shared by a given pair of counties, and I give these the same indicator for a border county pair fixed effect. While the properties themselves may be located in some other county, typically with no state border points of its own, they are always being compared to properties in the vicinity defined by the bandwidth of the same border segment.

RDD requires that the conditional expectation functions of the potential outcomes are continuous at the cutoff. In my setting this means that $E[Y_{ig}(0)|X_i = 0]$ and $E[Y_{ig}(1)|X_i = 0]$ must be continuous. After describing sample construction below I show tests of these assumptions in the next subsection.

## 4.3 Sample construction

My sample construction proceeds as follows. I identify the full set of US properties in CoreLogic Tax data built by 2016. I create an annual panel for each property over the years 2000–2016 and merge on any completed foreclosures and their dates within a given year as well as the most recent loan origination or refinance at the start of each year from CoreLogic Deed, preferring the largest loan when there are multiple from the same year. I restrict to single-family homes and condos to avoid properties that are likely to be at least partial rental regardless of whether owners live there, e.g., apartment buildings. I also restrict to counties that report both mortgage and foreclosure data in all six focal years 2007–2012 and are not sparse relative to counts in HMDA.

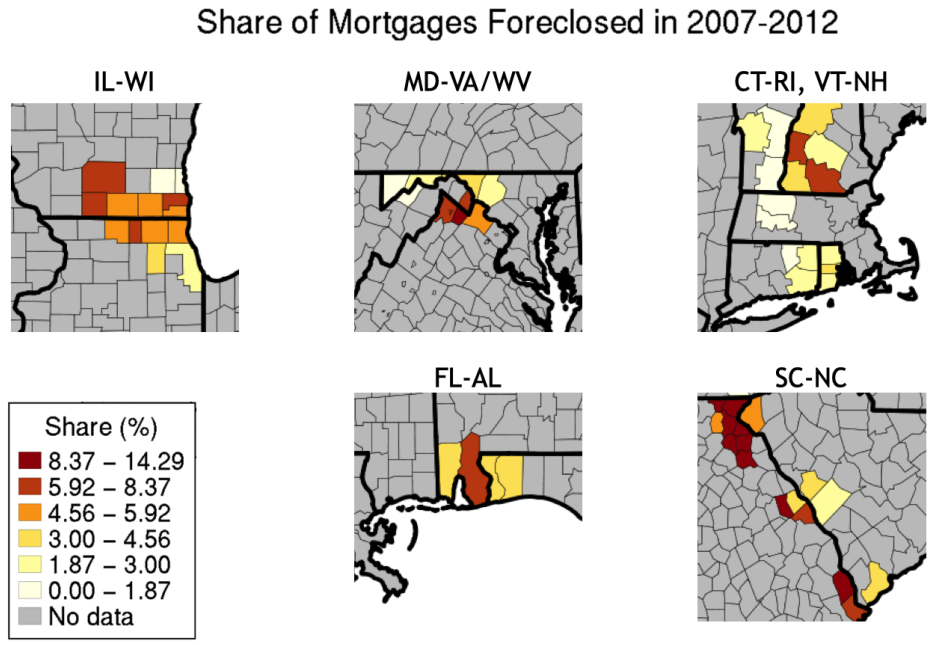
To focus attention on relevant borders for the spatial RDD, I restrict to bordering states with different foreclosure process. I then restrict data to properties within a buffer of 40 km (25 mi) in geodesic distance to the nearest state border. In practice this is more than four times the greatest distance that automated bandwidth selection will use for my spatial RDD.

Finally I return to check that borders between each state pair pass a manipulation test developed in Cattaneo et al. (2018) that is a refinement to the approach used by McCrary (2008) to check for smooth density of observations around the cutoff. For example, the New Mexico and Texas border is eliminated because it has uneven mass as reflected in border cities like El Paso without comparable levels of housing on the New Mexico side. This step helps address potential concerns about marginal residents sorting onto one side of a border over the other, which would be concerning if the reason were to access a preferred foreclosure process or for any reason that for another factor related to outcomes of interest like median household income.

Fig. 4 shows detail of foreclosure rates in counties that provide the most sample units. These cover the 40-km buffer around the borders of US states in the Midwest, Mid-Atlantic, New England, and South. Note that the border between North Carolina and South Carolina is a good example of how absence of complete county data in CoreLogic on one side leads me to also remove units on

the other side of the same border county pair.

Figure 4: Borders with the Most Sample Units



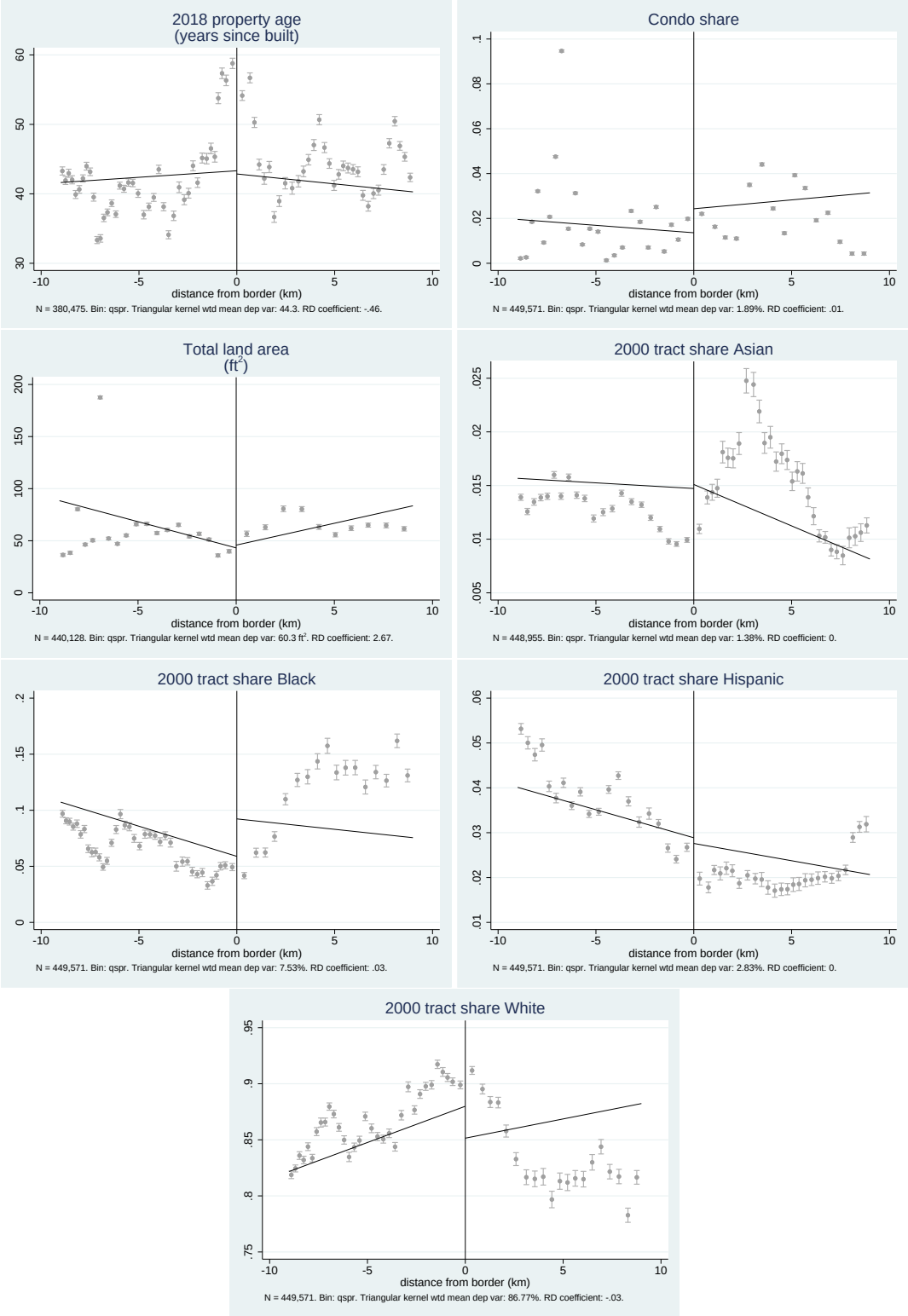

Sample: Single-family residences and condos within 40 km of border discontinuity.
Sources: CoreLogic, Infutor, IRS SOI, Opportunity Insights.

## 4.4 Tests of model assumptions

RDDs assume that observations are comparable on either side of the border, leading to a notion of quasi-random assignment for the purposes of foreclosure process that gives rise to an interpretation of causal inference. One indispensable way to check this comparability is to look for continuity of covariates across the discontinuity. I do this in two ways in Fig. 5. First, I show that characteristics of the origin properties are comparable; age of the property as of 2018, indicator for condo (as opposed to single-family residence), and total land area all show differences over the border that are neither statistically nor economically significant. Second, I look to Census tract-level measures to check that neighbors are comparable in terms of racial composition. This is important to check in light of historical patterns of segregation and of lending discrimination in favor of White borrowers for much of US history. Although I do not find significant difference for Asian, Hispanic, or White (non-Hispanic) borrowers, I do find a significant difference for Black borrowers. However, this disappears when examining plots with their individual bandwidths selected automatically (Fig. 9 in Appendix). These tend to choose smaller bandwidths than my default one for standard presentation of results. In turn the polynomials fit by local linear regression over these bandwidths tend to be less influenced by nonlinear trends evident in some variables like share of Black resident and property

age. Such trends are no concern from the perspective of satisfying continuity, which is defined immediately on either side of the cutoff, but they can lead to poor polynomial fit and thus estimation of the jump at the border. Thus there is a tension for users of RDD in presenting multiple outcomes of interest in a way that is accessible and useful on the one hand and in making the best estimates of the treatment effects at the discontinuity on the other hand.

Figure 5: Property and tract characteristics are smooth over the border



# 5 Results

Table 1 presents the main findings of separate RDD regressions on each row for distinct outcome variables. These use a triangular kernel, meaning that weight in the regression of an observation decreases linearly from full weight of one at the border discontinuity to zero weight at the edge of the selected bandwidth. For ease of interpretation across many specifications I fix a common bandwidth of 9 km that corresponds to upward rounding of the largest bandwidth automatically selected for any single regression. I verify identical signs and comparable statistically significant magnitudes in the same regression specifications with automatic bandwidths selected, as show in the Appendix in Table 2.

I find that nonjudicial foreclosure process increases the six-year foreclosure rate in the focal period by 2 pp, i.e., a 50% increase from a triangular kernel-weighted mean of 4% of households foreclosed. Fig. 6 depicts this visually with a plot. Such plots are helpful to understand how the local linear polynomial does relative to binned means across distances on either side of the border. The gap in intercepts of the cutoff between the polynomials corresponds exactly to the regression coefficient $\tau$. However, the binning of observations to produce means is entirely separate. If the polynomials are misunderstood as fitting the dots representing the binned means rather than the underlying data, this can obscure the use of the triangular kernel, covariates like my border county pair fixed effects, and the different numbers of observations used in each bin. For this reason as well as reasons of space I do not report these plots in the body of the paper for all outcomes, as I do with the table.

Figure 6: Any foreclosure in 2007–2012

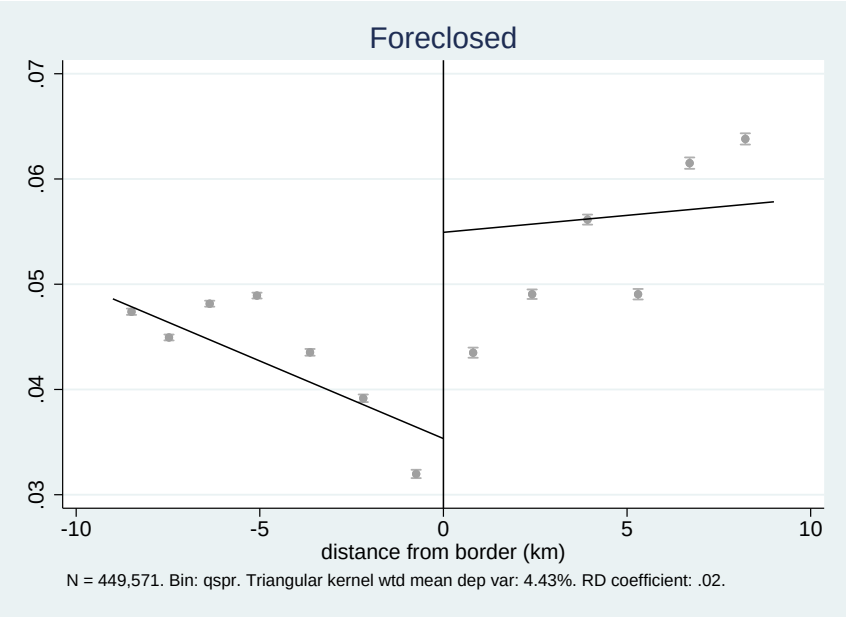


Table 1: RDD: Effects of Nonjudicial Foreclosure Process on Spatial Outcomes

| | Coefficient [Std. Error] | Weighted mean of dependent variable |
|---|---|---|
| Any foreclosure in 2007–2012 | 0.0196*** | 0.04 |
| | [0.00457] | |
| Moved (all matched adults) 2007–2014 | 0.0290*** | 0.22 |
| | [0.0107] | |
| Move distance over 15 km (all matched adults moved) 2007–2014 | 0.0181** | 0.13 |
| | [0.00761] | |
| Move distance over 80 km (all matched adults moved) 2007–2014 | 0.0151*** | 0.08 |
| | [0.00523] | |
| $\Delta$ 2016 tract median HH AGI 2007–2014 ($1k) | 0.964*** | 0.21 |
| | [0.313] | |
| $\Delta$ tract mean pctile HH income rank at age 29 (mean parent) 2007–2014 | 0.452*** | 0.00 |
| | [0.157] | |
| $\Delta$ tract mean pctile HH income rank at age 29 (p25 parent) 2007–2014 | 0.389*** | 0.11 |
| | [0.0941] | |
| $\Delta$ tract mean pctile HH income rank at age 29 (p50 parent) 2007–2014 | 0.304*** | 0.09 |
| | [0.0912] | |
| Observations | 449571 | |
| Effective observations | 85829 | |
| Local polynomial bandwidth (km) | 9.00 | |
| Border county pair fixed effects | yes | |
| State clustered SE | yes | |

Limited to properties with a most recent refinance or resale or new construction (as of 2007) from 2002–2006.
Symmetric bandwidth of 9 km imposed for the RDD treatment effect estimator from 40-km buffer.
Heteroskedasticity-robust standard errors (nearest neighbor minimum 3 for RDD),
triangular kernel, local polynomial order 1 for point estimation and 2 for bias correction.
Sample: Single-family residences and condos within 40 km of border discontinuity.
Sources: CoreLogic, Infutor, IRS SOI, Opportunity Insights.
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

For moving outcomes I look over an additional two years to allow time for moves that lag the recording of the foreclosure in deed records. This accounts for the possibility that foreclosed borrowers may legally stay in their home until given notice to vacate by the new owner. It also allows me to look at location after the household has completed short-term stays in the wake of foreclosure.

For all moving variables I use the location histories of only adults whose surnames match to CoreLogic deeds at the same property location (address). Although these adults begin in the same location at the start of 2007 by construction, they may move to different locations. Thus I take an equally-weighted mean across adults when determining average characteristics of the destination. It is from this average that corresponding characteristics of the origin are subtracted to form differences.

Nonjudicial foreclosure process increases move rates in the focal period by 3 pp, i.e. a 14% increase in household move rates relative to the weighted mean of 22% of households moving over this time. Although these coefficients are not statistically distinguishable from each other, an extra 2 pp in foreclosures and 3 pp are indicative of the fact verifiable in descriptive statistics that virtually all foreclosures move but not all moves come through foreclosure. One likely reason is because marginal households at greater risk of foreclosure under the nonjudicial process can take steps to avoid foreclosure that still require giving up the house. A key avenue to do this is through a short sale, whereby the lender approves the borrower to receive offers and reviews these to choose one to ultimately accept. This typically has a much smaller negative impact on a borrower's credit score relative to timely payment than foreclosure does, as that involves multiple missed payments.

Moreover, nonjudicial foreclosure process leads people to more distant moves, a little under 2 pp increases for both moves of average distance over 15 km (about 9 miles) and over 80 km (about 50 miles), i.e., increases of 14% and 19% relative to weighted mean household moving rates of 13% and 8% over these distances. This shows that a substantial number of people are induced to move not just out of their neighborhood or school district but out of a commutable distance.

Perhaps the most striking set of findings is how the characteristics of the destination relative to the origin locations improve on observable measures as a result of nonjudicial foreclosure process. Tract-level median household income rises by about $1000, a 360% increase relative to the weighted mean just over $200. Tract-level economic mobility based on national rank (0–100) in their cohort's distribution of household adjusted gross income at age 29 also increases for children, not only taking a mean over parent income level but also for children at the 25th percentile and the median. These effects of nonjudicial foreclosure process range from 0.3–0.5 percentiles.

Note that households that did not move or that moved within the same Census tract are included

as zeros in all of these regressions. Differences are driven entirely by moves but not always through foreclosures, as previously noted. Households may be taking advantage of historically low housing prices in this period to move to neighborhoods that would not previously have been accessible. While most of the foreclosed households are likely to have become renters (a topic I am pursuing in subsequent research), that is not necessarily the case for non-foreclosed households.

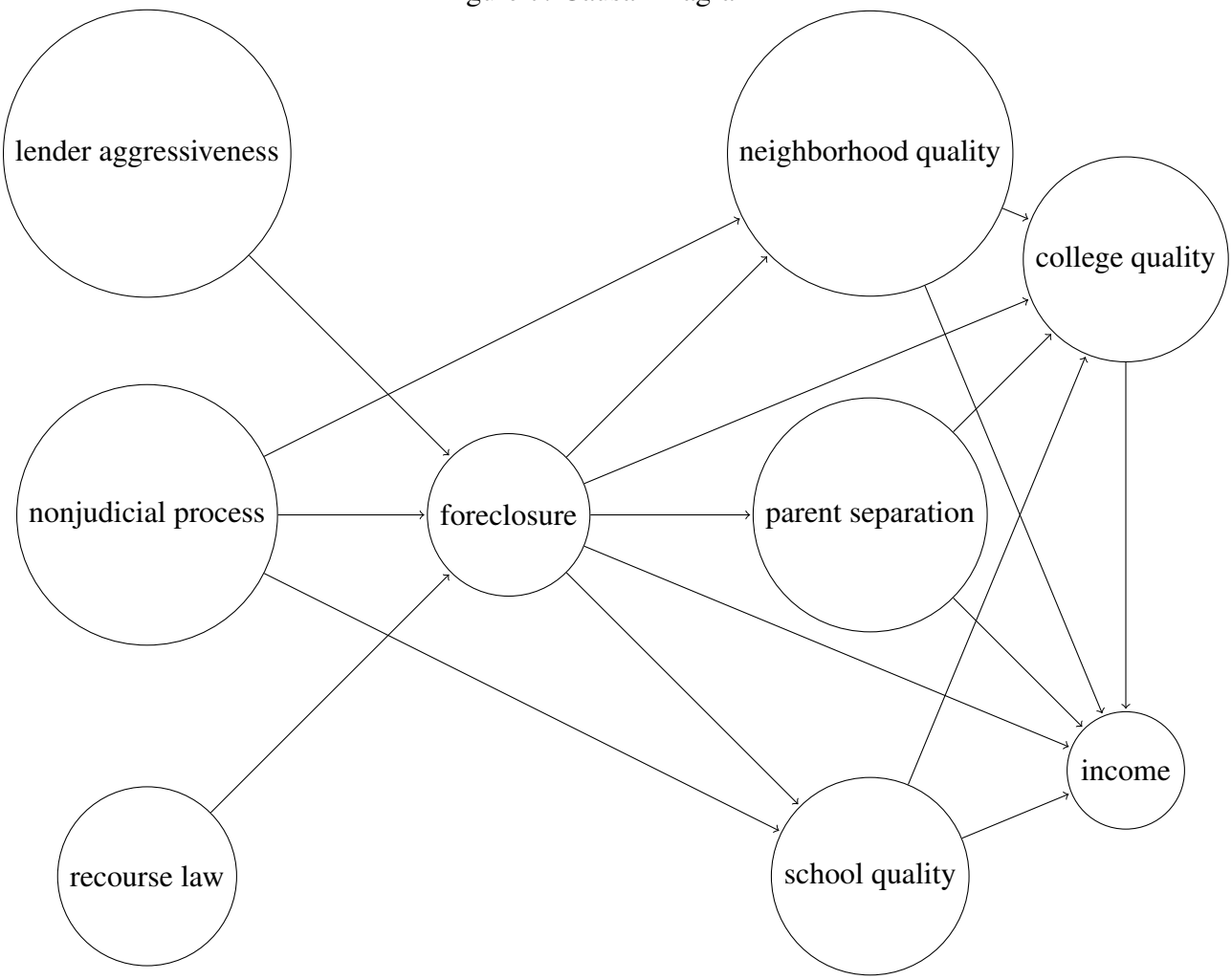
# 6 Discussion

From these results both a nonjudicial foreclosure system and foreclosure itself are seen to cause change in where households live. These changes in neighborhood quality appear beneficial in terms of median income and upward mobility for children growing up in the destination neighborhoods relative to the origins. However, it is uncertain whether these changes are enough to overcome the possible stresses of experiencing foreclosure and the potential disruption to parents' employment and children's schooling.

In "Intergenerational Effects of Foreclosure," a work in progress that builds on the empirical strategy in this paper, I link the IRS Statistics of Income Databank to the CoreLogic Deed and Tax data used here. This panel of US adults and their dependents allows me to analyze individual outcomes of higher education, pregnancy, employment, disability, earnings, and more for the children in households at risk of foreclosure. With these outcomes I can assess whether or not foreclosure process-induced moves to neighborhoods of apparently greater opportunity ultimately translate into long-term individual benefits.

Below I motivate several near-term outcomes that may be related to longer-term outcomes like college attendance and income in early adulthood. I show nonjudicial process along with proposed instruments for lender aggressiveness in foreclosure and recourse law, which directly affects both types of outcomes as illustrated in Fig. 7. If exclusion restrictions hold for either of these proposed instruments, we could learn about the effect of foreclosure per se on both intermediate and long-term outcomes. In either case we can understand longer-term individual effects of foreclosure process on children.

Figure 7: Causal Diagram




# 7 Conclusion

Fine data sensitive to heterogeneity of effects across US neighborhoods and other demographic characteristics can help parents or guardians dealing with the aftermath of a foreclosure make better-informed choices about whether and where to relocate by considering factors related to their children's education and future life outcomes. It can also inform the work of consumer advocacy charities like the Center for Responsible Lending or local resource charities like the Center for New York City Neighborhoods.

As policymakers look to prevent future foreclosure crises, my research allows a quantification of effects on households that is essential to its inclusion in benefit-cost analysis, e.g., by federal agencies like CFPB and HUD on mortgage lending and debt collection, whether in the wake of COVID-19 or future financial crises. This would bring potential harm done to children by

foreclosure directly into calculations used by agencies and policymakers of how best to spend finite public resources.

Finally, I envision the findings of this project may lead me into future partnerships to test whether intergenerational transmission of disadvantage can be disrupted. In a field experiment for households that experience foreclosure, treated households could be given timely information relevant to their geography to inform where to move based on outcomes for neighborhoods or school districts they are considering, then compared against control households without this information intervention.

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# Part

# Appendix

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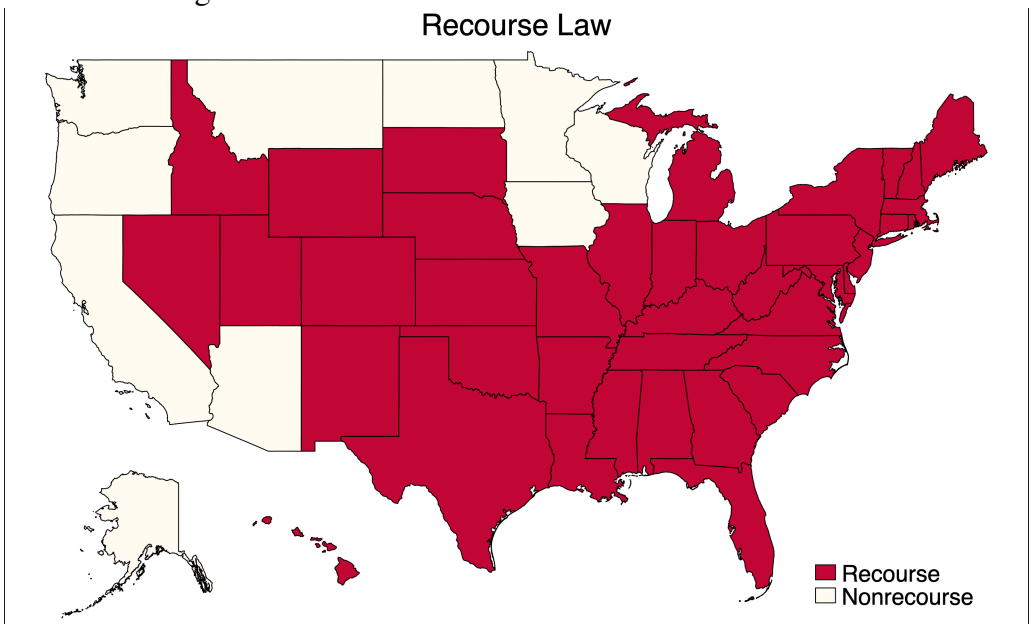
## A Recourse vs. non-recourse in foreclosure

In some states, lenders have recourse, or the ability to seek a deficiency judgement against the borrower in the event that the foreclosure sale recovers less than the borrower's outstanding debt. Orlando, J. (2011) documents substantial heterogeneity across states but broadly classifies only 10 states as non-recourse. These are concentrated in the West and northern Midwest, as shown in Fig. 8. Although courts have generally considered issuance of IRS Form 1099-C to be reporting rather than legal cancellation of debt, borrowers have successfully used 1099-C as evidence to defend against lenders who subsequently attempt to collect on debt reported as cancelled.

Figure 8: Recourse vs. non-recourse foreclosure states



I can instrument for likelihood of foreclosure completion, conditional on delinquency, using variation in recourse across states. In a recourse state, the lender may face a greater incentive to complete a foreclosure, sell the home, and pursue any difference between sale price and outstanding loan balance. In a non-recourse state, the lender may face greater incentive to negotiate and perhaps receive a lower stream of payments from the borrower. We may be concerned that recourse could come into play if there are systematic differences between borrowers in recourse and non-recourse states.

The two dimensions of state foreclosure I have described above are formally distinct from one another, but nearly all nonrecourse states are nonjudicial. However, recourse states are a roughly even mix of both judicial and nonjudicial. All but one of the borders in my sample are between recourse states, the exception being nonrecourse in Wisconsin vs. recourse in Illinois.

# B Additional tests of model assumptions

Here I reproduce all figures testing continuity and tables of spatial outcome coefficients using automatically-selected bandwidths. This is the standard in the RDD literature but requires care in comparing outcomes since effects are estimated on different samples.

Figure 9: Property and tract characteristics are smooth over the border

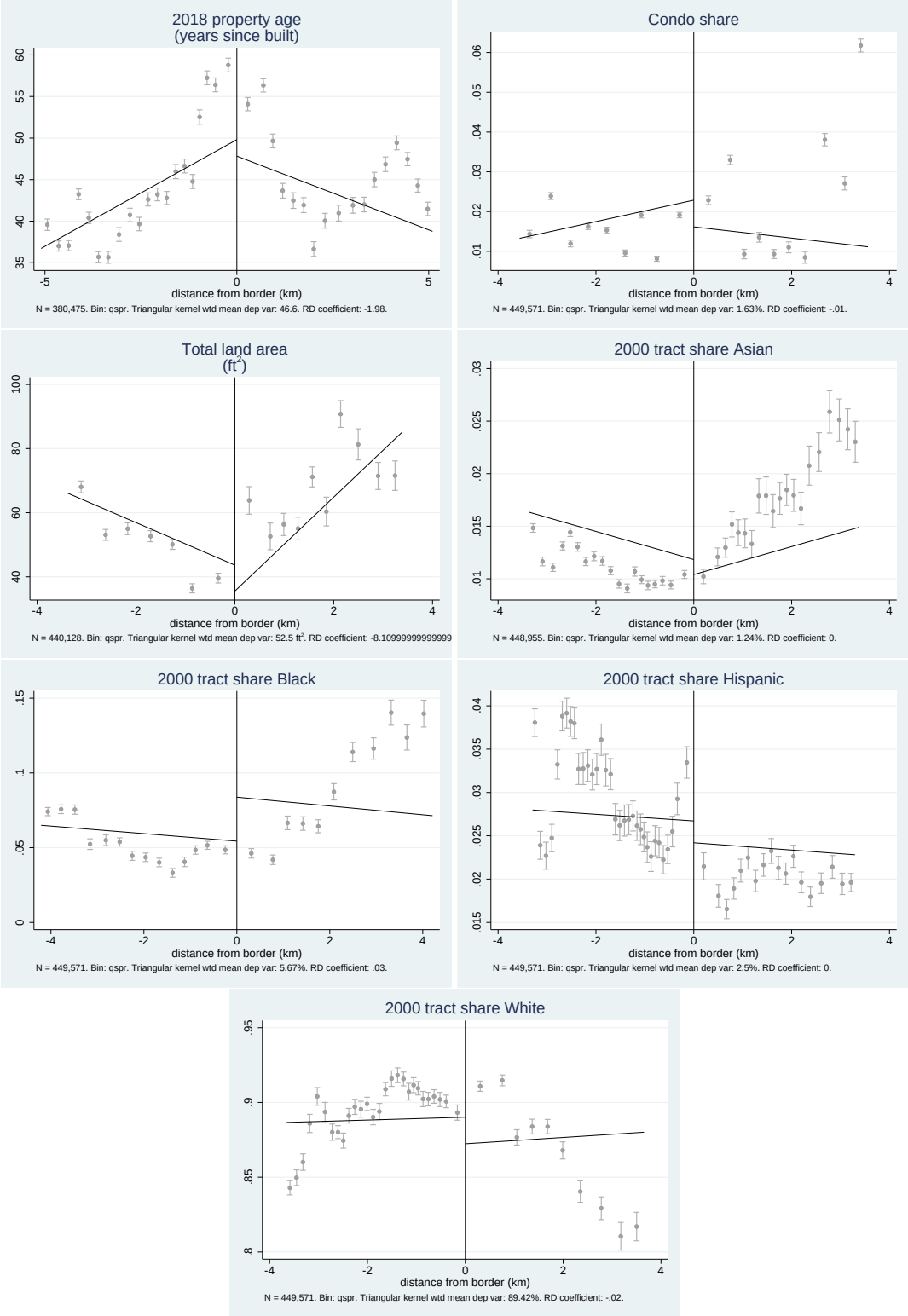


Table 2: RDD: Effects of Nonjudicial Foreclosure Process on Spatial Outcomes

| | Coefficient [Std. Error] | Weighted mean of dependent variable | Bandwidth (km) | Effective observations |
|---|---|---|---|---|
| Any foreclosure in 2007–2012 | 0.0220*** | 0.04 | 4.42 | 38416 |
| | [0.00468] | | | |
| Moved (all matched adults) 2007–2014 | 0.0291** | 0.22 | 7.66 | 70974 |
| | [0.0131] | | | |
| Move distance over 15 km (all matched adults moved) 2007–2014 | 0.0187* | 0.12 | 5.65 | 50089 |
| | [0.00958] | | | |
| Move distance over 80 km (all matched adults moved) 2007–2014 | 0.0121* | 0.08 | 4.96 | 43468 |
| | [0.00640] | | | |
| $\Delta$ 2016 tract median HH AGI 2007–2014 ($1k) | 1.170*** | 0.17 | 5.91 | 52658 |
| | [0.288] | | | |
| $\Delta$ tract mean pctile HH income rank at age 29 (mean parent) 2007–2014 | 0.522*** | -0.03 | 4.15 | 35911 |
| | [0.180] | | | |
| $\Delta$ tract mean pctile HH income rank at age 29 (p25 parent) 2007–2014 | 0.507*** | 0.03 | 4.36 | 37896 |
| | [0.134] | | | |
| $\Delta$ tract mean pctile HH income rank at age 29 (p50 parent) 2007–2014 | 0.307*** | 0.09 | 8.80 | 83611 |
| | [0.0922] | | | |
| Observations | 449571 | | | |
| Border county pair fixed effects | yes | | | |
| State clustered SE | yes | | | |

Limited to properties with a most recent refinance or resale or new construction (as of 2007) from 2002–2006.
Symmetric MSE-optimal bandwidth selected for the RDD treatment effect estimator from 40-km buffer.
Heteroskedasticity-robust standard errors (nearest neighbor minimum 3 for RDD),
triangular kernel, local polynomial order 1 for point estimation and 2 for bias correction.
Sample: Single-family residences and condos within 40 km of border discontinuity.
Sources: CoreLogic, Infutor, IRS SOI, Opportunity Insights.
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$