The “Scarlet E’: Effects of Public Eviction Records on Low-Income Households*

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Abstract

Housing advocates, media outlets, and policymakers have long argued that a public eviction filing record—often referred to as the “Scarlet E”—carries significant consequences, particularly for low-income households. In this paper, we exploit variation induced by a record-sealing policy in Illinois to provide the first causal estimates of the effect of a public eviction filing on residential mobility, neighborhood quality, homelessness, and financial health. Two findings stand out: (i) sealing eviction records at the time of filing prevents tenant screening companies from accessing case information, whereas retroactive record sealing is ineffective in restricting public access; (ii) tenants with public evictions records are more likely to live doubled up with friends or family within the first year of the filing. Our results suggest that housing instability due to public eviction filings manifests primarily through household doubling up rather than through the more extreme forms of homelessness.

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1 Introduction

Landlords file approximately 3.6 million eviction cases annually in the U.S. (Gromis et al. 2022), creating public records that follow tenants long after their housing disputes are resolved. Tenant advocates, media outlets, and policymakers have long argued that a public eviction filing record—often referred to as the "Scarlet E"—carries significant consequences, particularly for low-income and minority households (Goldstein 2021; Navar 2023; Phillips 2023). Individual accounts of this problem suggest that a public eviction court record, regardless of the outcome of the eviction hearing, may be a major barrier to securing future housing (Franzese 2018; Kiviat 2019).[1] Motivated by individual testimony on the "Scarlet E" and the potentially widespread exposure, policymakers in over a dozen state and local jurisdictions have recently passed legislation to seal eviction records.[2]

Despite the attention given to the potential harms of these records, we lack causal evidence on the impacts of public eviction filing records on tenants' ability to secure new housing and their subsequent housing stability. This evidence is necessary to weigh the potential harms to tenants from public records relative to the benefits for landlords of using the records in their risk assessments of prospective tenants. Isolating the causal impact of the public record from the effect of the landlord filing an eviction or being formally evicted is important to understand how the legal framework regulating eviction proceedings may affect access to higher-opportunity neighborhoods and housing stability of low-income renters. Estimating this causal impact requires exogenous variation in the public visibility of eviction filings. While recent state and local legislation mandating the sealing of eviction records generates such variation, these sealing policies inherently restrict access to data, creating a barrier for quantitative research. Moreover, even when data on sealed records is available, testing whether a sealing policy successfully prevents data from being used by tenant-screening companies and landlords presents an additional challenge.

In this paper, we overcome these challenges and provide causal estimates of the effects of a public eviction filing record on residential mobility, neighborhood quality, and financial health in the two years after filing. We leverage quasi-experimental variation from a record-sealing policy change in Illinois using both sealed and public data obtained from the Circuit Court of Cook County via a special order.[3] Effective May 17, 2021, statute 735 ILCS 5/9-122 ordered new eviction cases filed through March 31, 2022 be sealed, and retroactively sealed

[1]In most states, eviction filings remain public even when the case was dismissed, the judge ruled in favor of the tenant, or the landlord and the tenant reached an agreement before the eviction hearing.

[2]See Appendix Table A1 for a description of recent state and local eviction sealing laws.

[3]The statute temporarily allowed researchers to request sealed records for scholarly purposes, conditional on the approval of the Chief Judge.

cases that had already been filed since the beginning of the COVID-19 pandemic (March 9, 2020 – May 16, 2021). Upon the expiration of the statute on April 1, 2022, new eviction filings were again public record by default, while previously filed cases during the sealing period remained sealed. Using an additional dataset from a tenant screening data provider, we show that sealing at the time of filing was highly effective in limiting the spread of case information, in contrast to the retroactive sealing that was applied to a small set of cases filed prior to the implementation of the policy.

Our primary empirical approach is a difference-in-differences (DiD) design that compares the outcomes of tenants with cases filed on or after the end of the sealing policy (April 1, 2022) to those with cases filed during the sealing period (before April 1, 2022). The DiD approach relies on the identifying assumption that, absent the policy change, tenants' post-filing outcomes would have continued to evolve in parallel for individuals with filings during the public or sealed period. Empirically, we observe that both groups exhibit similar trends in the twelve months prior the eviction filing date, consistent with the parallel trends assumption.

To account for seasonal trends in post-filing outcomes that could affect the comparison of pre and post April 1st filings, we use pre-pandemic filings as a placebo check and as an additional source of variation for a triple difference-in-differences specification. We present these as a complementary strategy to our main DiD analysis, discussing the seasonal patterns in residential mobility associated with a winter or spring filing date.

Because our analysis leverages variation tied to a specific date, a regression discontinuity (RD) in time design is an intuitive approach. Yet, the assumptions necessary to recover a causal effect from the RD are not well supported in our context. The RD in time design relies on the assumptions that landlords do not manipulate the timing of filings around the policy end date and that all characteristics of eviction filings besides the public status vary continuously around the policy end date. While we do not find evidence of discontinuities in the density of cases filed around the end of the sealing period—ruling out concerns about landlords or lawyers waiting for the end of the sealing period to file eviction cases—we do observe some discontinuities in the characteristics of cases filed, which we show to be correlated with the monthly rent payment cycle. Due to these imbalances around the cutoff date, the RD in time is not our main specification, and we present it as an alternative approach. Nevertheless, the results from our RD analysis are largely consistent with the DiD results, although much less precise. The similar results across research designs and various specifications strengthen our conclusions about the effects of public eviction filings.

To understand whether the Illinois sealing policy induced sufficient variation in public access to eviction records, we document that the end of the mandated sealing policy generated a strong impact on record visibility. Eviction cases filed after the cutoff date are 89 percentage points more likely to be designated as public records by the court. We interpret this as confirmation of de jure compliance with the previous sealing policy. To examine the de facto effectiveness of the sealing policy in preventing landlords from associating prospective tenants with sealed eviction filings, we link eviction court records to data from a commercial public records company—representing information accessible to landlords when screening prospective tenants. We find virtually no cases that were filed under seal in the public records database. However, cases filed during an earlier policy regime that were initially public records and retroactively sealed are nearly always visible in the public records database. After the cutoff date when eviction cases are no longer filed under seal by law, we find that cases are 67.6 percentage points more likely to appear in the tenant screening database, as compared to the 0.4 percent of cases we match to tenant screening data in the sealed period. Given concerns about authorities' ability to enforce restrictions on what tenant screening companies can disclose, this finding provides important evidence that immediate sealing is an effective mechanism to restrict public access to eviction records, but retroactive sealing is far less effective.

To examine the effect of public records on residential mobility, we link the sealed and public eviction court records to a national database of consumer address histories from Infutor Data Solutions. In addition to tracking moves, we leverage the wide coverage of these data to construct a novel measure of living doubled-up (i.e., moving in with someone due to economic hardship). Living doubled up with friends or family is often discussed as a hidden dimension of homelessness, one that is difficult to measure and not typically included in most counts of people experiencing homelessness.[4] Using our DiD approach, we find that the end of mandated eviction record sealing increased tenants' residential mobility, particularly within the first year of the filing. Tenants with filings from the public period are 5.5 percentage points (18.9%) more likely to move and experience 0.073 more moves (20.3%) within a year of the filing. They are also 3.1 percentage points (16.5%) more likely to be doubled up in the first year after filing. The increases in the doubled-up rate are consistent with the changes we observe in other dimensions of residential mobility. Within a year of the filing, tenants with a public-period filing are 5.7 percentage points (23.9%) more likely to move to a different zip code and more likely to reside in census tracts with slightly lower poverty rates (about 4 percent lower poverty rates relative to the sealed period mean). Accounting for seasonal variation through the triple difference specification modestly reduces the magnitude and precision of our estimates. Nonetheless, we still see that tenants with public-period filings

[4]The US Department of Education considers living doubled up as homeless and provides estimates for K-12 students enrolled in public school. See Section 3.3.1 for more on how doubled-up is measured.

are 2.5 percentage points more likely to live doubled up within 3 and 6 months of their filing date, corresponding to a 42% and 20% increase relative to the doubled up mean among sealed-period filings, respectively. They are also 3.7 percentage points (15.5%) more likely to have moved to a different zip code within a year of filing. Additionally, we rule out that the effects on mobility are driven by evicted tenants or differential rates of eviction orders.

The residential mobility effects are concentrated in the first 12 to 18 months after the filing. By two years after filing, the estimates for the likelihood of moving and the cumulative number of moves converge to the estimates we attribute to annual seasonal trends. However, the increases in doubled-up status and the small improvements in neighborhood quality for those with public-period filing are still present two years after filing, albeit less precise. One model that could be consistent with this pattern relies on differential search patterns in the rental market between the public and sealed groups. Tenants with a public eviction filing record may abandon searching more quickly, so they move to a new address faster than those with a sealed record. The slightly more persistent pattern we observe in doubled-up rates and neighborhood quality suggests that tenants with a public record face an additional challenge in securing their own lease and move in with relatives, friends, or partners in a different neighborhood. Our results are in line with Collinson et al. (2025), which shows that children from evicted households are more likely to later live in multi-generational households.

We examine the impacts on interaction with homelessness services using data from Chicago's Homelessness Management Information System (HMIS). Our results indicate no statistically significant effects from a public eviction filing on engaging with homelessness services. While the 95% confidence intervals include increases in the use of homelessness services of 0.68 percentage points (34%) within one year and 0.8 percentage points (26%) within two years, we note that the coefficients in the event studies are consistently small, suggesting that the effects on the use of homelessnessness services does not exceed 0.1 to 0.2 percentage points (5 to 6 percent of the control group mean). This contrasts with the larger and more precisely estimated effects on doubled-up rates. Taken together, these results suggest that removing eviction record information primarily influences residential mobility and living arrangements rather than more severe forms of homelessness.

We also link defendants in sealed and public eviction filings to Experian credit files and detect no statistically significant changes to credit scores or durable consumption, credit access, and debt collection balances. Considering the lack of precision of these estimates, we cannot reject large negative impacts of public eviction records on access to revolving credit or large increases in collection balances.

This paper builds upon recent work in economics and sociology studying the consequences of the eviction process for renters (Desmond 2012; Desmond et al. 2015; Desmond and Kimbro

2015; Desmond 2016; Desmond and Gershenson 2016; Collinson et al. 2024, 2025). These existing studies focus on outcomes as a result of an eviction order, conditional on case filing. As such, they do not account for the potential effects of a public eviction filing, which may affect tenants regardless of the outcome of the court case. Our estimates shed light on the relative harms of an eviction filing—which generates the public record and impacts a much larger subset of renters.[5] As such, our estimates complement those from Collinson et al. (2024) and Collinson et al. (2025) to provide important evidence on the consequences of an earlier stage in the eviction process.

Our work also contributes to the literature on screening decisions under information asymmetries (Bartik and Nelson 2020; Wozniak 2015). In particular, the policy debate surrounding eviction record-sealing policies parallels that of ban-the-box (BTB) policies that limit the ability of employers to ask job applicants about criminal histories. The literature on BTB studies both direct impacts on individuals with criminal histories (Rose 2021) and spillover effects in the form of statistical discrimination against Black and Hispanic men (Agan and Starr 2018; Doleac and Hansen 2020). While BTB provides policy variation to study the criminal history as information used to screen applicants in the labor market, we focus instead on the rental housing market and a different yet important piece of screening information: public eviction filings.

In our analysis, we estimate the direct impacts of public records on affected tenants, in the spirit of the effects of criminal records in Rose (2021). While eviction record-sealing laws could also induce statistical discrimination in the rental market similar to BTB (Agan and Starr 2018; Doleac and Hansen 2020; Burton and Wasser 2025), we do not address this question as the temporary policy we study is unlikely to generate detectable changes in landlords' screening behavior.[6] It is also important to note that the policy we focus on in this paper restricts the availability of information at the source—similar to the expungement of criminal records in Prescott and Starr (2020)—rather than regulating decision-makers' ability to consider certain information when screening (Agan and Starr 2018; Bartik and Nelson 2020;

[5] Based on eviction filings in Cook County from 2000 to 2016, Collinson et al. (2024) estimate that about two out of three eviction filings result in a court order for eviction and only approximately 25 percent of eviction filings result in evictions enforced by the Sheriff's Office.

[6] In response to eviction record-sealing laws, landlords may increase rent prices and security deposit amounts if they believe background checks are not a reliable tool and require other means to mitigate risk of tenant default. Landlords may also be more reluctant to rent to tenants they perceive to be more likely to have a previous eviction record, resulting in statistical discrimination against racial minorities or low-income renters. The particular policy setting we study in Illinois is poorly suited to test for these potential changes in landlords' screening behavior or rental prices because we focus on the end of a temporary sealing policy. The adoption of a permanent policy to seal all future eviction filings would be a more suitable policy setting to study questions of statistical discrimination since such a policy is much more likely to generate changes to landlord screening or prices.

Doleac and Hansen 2020; Rose 2021; Gorzig and Rho 2023). Another important distinction is that we focus on the effects for records sealed at the time of filing—not retroactively (Agan et al. 2024). This allows us to estimate the impacts of the public record itself, which is a fundamental input in the design of policies that determine what constitutes public record or how decision-makers are allowed to use public information in screening decisions.

The remainder of this paper is organized as follows. Section 2 discusses the use of public eviction records by landlords screening prospective tenants and the details of the Illinois eviction record-sealing policy. Section 3 describes our data sources and how we construct the doubled up measure. Section 4 provides descriptive evidence on the relative effectiveness of automatic and retroactive sealing mechanisms. Section 5 describes the linkage of court data to administrative records. Section 6 details our difference-in-differences and regression discontinuity research designs. Section 7 reports our estimates of effects on residential mobility, homelessness, and financial health. Section 8 concludes.

2 Institutional Background

2.1 Public Eviction Records and Tenant Screening Practices

Eviction cases are typically public records. In most jurisdictions, eviction records remain public even when the case was dismissed, the judge ruled in favor of the tenant, or the landlord and the tenant reached an agreement before the eviction hearing. Eviction filings can be found in online public court databases, in person at the courthouse, or on tenant screening reports. Private companies scrape or purchase eviction court records to compile and sell tenant screening reports to landlords evaluating potential tenants. These reports typically indicate whether an individual is associated with any previous eviction filing, regardless of the outcome of the case. In some cases, the reports may include simply a "thumbs-up" or "thumbs-down" recommendation to the landlord based on limited or ambiguous information about the eviction case (Kirchner and Goldstein 2020). Tenant screening reports are different from credit reports. Tenant screening reports typically include a credit report, but, unlike the credit report from large credit bureaus, tenant screening reports also include information on eviction history, and may include a criminal background check.[7] Federal regulations prohibit screening agencies from reporting judgments more than seven years old, but these

[7]As part of a 2015 multi-state settlement, the three nationwide consumer reporting agencies (Experian, Equifax, and Transunion) took steps to remove civil judgments and tax liens from credit reports. Since 2018, the only type of public record directly reported by the three major credit bureaus is bankruptcy. Defaulted mortgage or rent payments sent to collections are still reported and used to compute credit scores. Other credit reporting agencies could continue to offer lenders access to eviction filings.

laws are difficult to enforce.[8] In tight rental markets, landlords increasingly rely on tenant screening reports as part of their background checks on prospective tenants.[9] Local housing authorities also use background checks, meaning that eviction records can negatively impact an application for a housing voucher or public housing.

Landlords typically use information in the screening reports to evaluate prospective tenants' ability to pay rent and to deny applicants they perceive as high-risk. However, the public information available to landlords about prospective tenants' eviction history is often incomplete or ambiguous (Porton et al. 2021). In the screening process, any link to an eviction case is thought to be a negative signal in the rental market while any positive history of on-time rent payments is not typically reflected in tenant screening reports. As such, having a public eviction record may restrict a tenant's ability to secure future housing. It can prolong the housing search periods and increase instability, potentially forcing tenants to spend significant time and money on application fees, higher security deposits, or other costs, which can exacerbate financial distress and negatively impact financial health. With limited alternatives, tenants with public eviction records may be more likely to rely on family or friends for temporary housing by living "doubled-up," or may face higher risks of relocating to substandard housing or lower-quality neighborhoods. When these options are exhausted, the likelihood of interaction with homeless services may also increase. Importantly, public records may also exacerbate racial discrimination by landlords considering rental applications (Carpusor and Loges 2006; Hanson and Hawley 2011; Ewens et al. 2014; Christensen et al. 2021); over half of all eviction filings are against Black renters despite Black renters comprising less than 20% of the U.S. renter population (Graetz et al. 2023).

The importance of a visible eviction filing record when searching for housing also depends on signals from other tenant characteristics. For example, a public eviction filing record may be more consequential for an individual with stable credit and no criminal history compared to an individual with multiple negative signals or previous eviction cases. Tenants tend to experience declining employment and credit prior to an eviction filing (Collinson et al. 2024), suggesting that for much of the population with visible eviction filing records, the removal of only the eviction-related signal could have minimal impacts. Therefore, predictions of the causal impacts of a visible eviction filing record are theoretically ambiguous and depend on the relative importance of eviction-related signals compared to all other signals considered

[8] The federal Fair Credit Reporting Act limits dissemination of inaccurate information and prohibits reporting of judgments more than seven years old. The Act applies to information collected by consumer reporting agencies such as credit bureaus, medical information companies, and tenant screening services. The federal Fair Housing Act, among other things, prohibits housing screening policies that appear neutral but have a disparate impact based on race or gender.

[9] A 2017 TransUnion survey of 689 landlords across the US found that 90% of landlords relied on online screening companies.

by landlords when screening tenants.

2.2 Illinois Sealing Policy

Citing the potential harms to tenants from public eviction records, several states have recently introduced measures to facilitate the sealing or expungement of eviction records.[10] See Appendix Table A1 for a summary of recent changes to record-sealing laws. In this paper, we focus on an Illinois law that mandated pandemic-era eviction filings be sealed. Given the temporary nature of the pandemic-era sealing mandate, this policy generated quasi-random variation in the status of cases as public records or sealed records around the end date of the sealing mandate in 2022.

On May 17, 2021, the Governor of Illinois signed into law the state's "COVID-19 Emergency Housing Act" (Public Act 102-005), which, among other protections for renters affected by the pandemic, established eviction record-sealing provisions. The law established immediate, automatic, and permanent sealing of residential eviction cases filed beginning with Illinois' COVID-19 emergency declaration (March 9, 2020) through March 31, 2022 (Illinois State Bar Association 2022).[11] Residential eviction cases filed during this sealing period but prior to the passage of the law were to be retroactively sealed, while all new cases filed for the remainder of the sealing period were to be automatically sealed at the time of filing. On April 1, 2022, all new residential eviction cases were to be filed as public records by default and only sealed under a much narrower set of criteria. The cases that were sealed via the state law were to remain sealed.[12] Of critical importance, the law also allowed sealed court files to be made available for scholarly purposes conditional on approval by the court.

Using data obtained via this clause, we exploit the quasi-experimental variation in public records induced by the end date of the sealing period (April 1, 2022). In addition to the

[10] Before the COVID-19 pandemic, sealing laws primarily focused on making it easier to seal an eviction record if the case was dismissed or the judgment was in favor of the tenant. More recent pieces of legislation since the COVID-19 pandemic allow for pandemic-era eviction records to be sealed and outline processes for tenants to retroactively seal older records if they prevailed in court.

[11] See 735 ILCS 5/9-122(a)(b) for details.

[12] As specified in 735 ILCS 5/9-121.5(b)(1-4), cases filed from April 1, 2022, to July 31, 2022 will not be sealed unless a court orders the sealing if (1) the interests of justice in sealing the court file outweighs the public interest in maintaining a public record; (2) the parties to the eviction action agree to seal the court file; (3) there was no material violation of the terms of the tenancy by the tenant; or (4) the case was dismissed with or without prejudice. Residential eviction cases filed beginning on August 1, 2022, are subject to the original legislation, allowing the courts discretionary sealing under very stringent criteria, and mandatory sealing only for mortgage foreclosure cases. As specified in 735 ILCS 5/9-121(b), discretionary sealing only applies to cases filed from August 1, 2022 onward if the court finds that the plaintiff's action is sufficient without the basis of law or that placing the court file under seal is clearly in the interests of justice, and that those interests are not outweighed by the public's interest in knowing about the record. 735 ILCS 5/9-121 (c) establishes that mandatory sealing is reserved for foreclosure cases.

causal analysis comparing cases that were filed under seal and cases filed as public records, we also conduct a descriptive analysis of cases that were retroactively sealed under the earlier period. Figure 1 illustrates the timing of the record-sealing policy changes alongside active state and federal eviction moratoria in Illinois.

The temporary nature of the Illinois sealing policy means that we do not expect it to generate the same general equilibrium effects as permanent record sealing laws. Among the pool of all individuals in Cook County with past eviction filing records, only a small share of those records were sealed under this policy, providing limited scope for landlords to adjust screening practices in response to the removal of this information. This feature of our setting allows us to isolate the direct effects of visible eviction filing records on tenants' future outcomes, absent changes in landlord behavior, which are an important input for welfare analysis of permanent record sealing laws.

3 Data Sources

We analyze Cook County eviction filings linked to a commercial public records database, residential address histories, homeless service records, and credit bureau records. The following sections describe these data sources and linking methodologies.

3.1 Court Records

We obtained public and sealed eviction filings from the Circuit Court of Cook County. The public filings correspond to cases filed between March 11, 2019, and March 23, 2023, and the sealed filings correspond to cases filed between March 9, 2020, through July 31st, 2022. These data include the date of filing, case number, the type of case (single or joint action, due to mortgage foreclosure, or initiated by the Chicago Housing Authority), whether it was referred to the Early Resolution Program (ERP), the judgment (if one was issued), and any eviction order and associated order to the sheriff's office. The court data also identifies whether a case was designated as a sealed or public record by the court. The personal identifiers in eviction court records include tenant names and the property address. We exclude cases from our analysis sample with missing names or addresses since these individuals cannot be linked to outcome datasets. We also exclude filings from commercial properties where the tenant runs a business or occupies office space in a rented property. Our main analysis sample includes tenants named in eviction cases filed between December 1, 2021, and July 31, 2022, including four months before and four months after the end of the sealing policy.[13] In other analyses comparing cases sealed retroactively or immediately, we consider a larger sample of cases filed between 2020 and 2022. Based on the geolocation of tenant addresses, we append neighborhood characteristics from the American Community Survey.

3.2 Tenant Screening Data

To measure the effectiveness of the law in preventing sealed records from being accessed by prospective landlords, we match eviction court records to data from Record Information Services (RIS), a private company that compiles public court records in Illinois. The data from RIS represent what would be easily accessible by a landlord requesting a background check on a prospective tenant. This database allows us to investigate the effectiveness of the policy in restricting public access to eviction filings, an important check because de jure sealing mandates may not always translate to de facto sealing compliance by tenant screening companies. We match the court records to the RIS data using the case filing number.

3.3 Infutor Address Histories

To measure mobility and housing instability, we track address changes using data from Infutor Data Solutions, which aggregates consumer information (e.g. cell phone bills, credit records, voter files, property deeds, magazine subscriptions, change-of-address data, etc.) into an address history that lists exact addresses with start and end dates for most residents in the U.S. Researchers have only recently started to use this data to longitudinally follow residents. For example, Diamond et al. (2019) examines families affected by rent control in San Francisco, Collinson et al. (2024) tracked residential mobility patterns among evicted tenants in New York and Chicago, and Feigenberg and Miller (2022) observe address histories of motorists involved in traffic searches. Phillips (2020) demonstrates the use of consumer reference data to measure housing moves for groups with very low income in situations such as natural disasters and the demolition of public housing.

We link the court records to Infutor address histories using a fuzzy matching algorithm that uses names within latitude and longitude to two decimal places of geolocated addresses and obtain a 25% match rate for individuals in our analysis period.[14] To address concerns

[13] Although the density of filings does not jump precipitously with the end of the eviction moratorium on October 4, 2021, we exclude cases filed in the months right after the end of the moratorium because these cases may include a backlog of pandemic-era cases that differ from typical eviction filings.

[14] Collinson et al. (2024) use names and addresses to match New York City eviction court records to Infutor. While the authors do not explicitly report the match rate, they state that it is much lower than the 40% match they obtain from the name-address linkage to New York City public benefits data. We obtain similar match rates with an 85 Jaccard score threshold or a minimum weight of 0.975 for Jaro-Winkler string

that the eviction filing itself could make someone more likely to be in the match to Infutor data, we require that individuals must have at least one address on file in Infutor before the eviction filing date. Using these data, we determine how frequently individuals move and their neighborhood characteristics. We also construct a measure of "doubling-up" that captures whether an individual moves to housing units with existing residents.

3.3.1 Measuring Doubled-Up Household Rates

Doubling-up, broadly defined as living with others because of economic hardship or housing loss, is difficult to measure and not included in the US Department of Housing and Urban Development's (HUD) census of people experiencing homelessness. The only annual counts of people living doubled-up come from the Department of Education and are limited to school-age children. This type of living arrangement underlies significant material hardship on families and strain on limited living space, and it often precedes episodes of shelter entry or street homelessnes (Koebel and Murray 1999; Wright et al. 1998; Skobba and Goetz 2015). Studies also describe doubled-up situations as overcrowded, which can negatively affect both physical and mental health (Bush and Shinn 2017). Recent efforts to quantify doubled-up rates rely on the American Community Survey (ACS) and focus primarily on the long-term trends of children's living arrangements and differentiating doubled-up rates by household type (multigenerational, extended family, nonkin) (Mykyta and Pilkauskas 2016; Harvey et al. 2021). This type of analysis has the advantage of presenting a detailed picture of households' living arrangements by race and socioeconomic status over several decades. However, these demographic measures of doubled-up status are difficult to link to particular economic shocks, and they cannot be used for individual-level analysis.

In this paper, we leverage the national data on individual-level address histories available through consumer reference data (Infutor) to construct a novel measure of doubled-up status. We define this measure as a move to an address that overlaps with the tenure of an existing resident of that address. To be precise, a defendant in our sample is considered to be doubled-up if he or she moved into the current residence (the address at x months pre or post filing) at least six months after the existing resident(s), and the existing resident(s) does not move out within six months after the defendant moved in.[15] Using this measure, we track changes in doubled-up status for the public and sealed filing group before and after each individual's filing date. In terms of general trends, our measure shows that doubled-up rates increase sharply for anyone with an eviction filing within six months of the filing, and doubled-up rates spiked during the COVID-19 pandemic (See doubled-up trends in Figure

distance matching.

[15]See Appendix E for detailed steps

6 and Appendix Figure E1).

3.4 Data on the Use of Homelessness Services

To measure contact with the homeless service system, we obtain data from Chicago's Homeless Management Information System (HMIS) which allows us to observe shelter entry, as well as other temporary housing and homelessness prevention services. Chicago's HMIS collects client-level data from various public and private homeless service providers in the Chicago Continuum of Care (CoC), allowing us to observe most date-specific service records for those seeking assistance in the City of Chicago. We construct three separate measures that reflect different levels of service coverage to capture interactions with homeless services: (1) Shelter service, which includes entry into emergency or temporary shelters for individuals experiencing homelessness; (2) Homelessness service, which includes all shelter services, as well as transitional housing and street outreach programs; and (3) any CoC service, which includes a comprehensive set of services, such as all homelessness services defined above, rapid re-housing, permanent supportive housing, as well as other services directed toward individuals at risk of homelessness, including coordinated entry and homelessness prevention.

We match defendants in the court records from the city of Chicago to the HMIS data using fuzzy matching based on name and address information provided in the court filings.

3.5 Credit Bureau Records

To measure financial health, we match the names and addresses from court records to Experian credit files. We observe quarterly snapshots of post-filing credit attributes spanning quarter 1 of 2022 through quarter 3 of 2024. We also observe snapshots of pre-pandemic credit attributes from quarters 3 and 4 of 2019. Our key indicators of financial health include the Vantage credit score, unpaid bills (total balance in collections), durable consumption (any auto loans or leases), and access to credit (any open source of revolving credit such as a credit card). We matched 68% of tenants in our analysis sample to at least one post-filing snapshot of Experian data.[16]

4 Retroactive vs. Immediate Sealing

A critical question in the design of eviction-record sealing policies is whether the sealing should happen automatically upon filing or later in the court process (e.g., seal after the

[16] Collinson et al. (2024) use names and addresses to match Cook County eviction court records filed between 2000 and 2016 to Experian credit files and report a similar match rate of 61.3%.

court judgment or seal upon the defendant's request). Because the Illinois statute sealed some records upon filing and others retroactively, we are able to provide evidence—-the first to our knowledge—of the effectiveness of sealing under these two mechanisms. To examine the effectiveness of the sealing policy in preventing landlords from associating prospective tenants with sealed eviction filings, we link eviction court records to data from a commercial public records database—representing the information accessible to landlords when reviewing applications from prospective tenants[17]

In Figure 2, we compare the weekly number of cases matched to the public records database for the retroactive sealing period and the immediate (upon-filing) sealing period. Over 96% of eviction-filings sealed retroactively are found in tenant screening databases, indicating that those records remain in the public domain even after the court ordered them to be sealed and removed the records from the court's own public database. In contrast, less than 6% of eviction records mandated to be sealed upon filing are found in the tenant screening data, confirming that automatic sealing upon filing is effective in preventing the information sealed by the court from reaching the public domain.[18]

Given the effectiveness of immediate sealing at the time of filing, we focus on variation in public access to eviction cases induced by the end of mandated sealing on April 1, 2022. We first document that the change in the sealing rules applied to eviction cases filed around April 1, 2022 had a strong effect on the likelihood of an eviction filing being visible to the public. Figure 3 verifies that cases filed prior to April 1, 2022 were largely not designated as public records by the court. The court labeled only 2.1% of cases during the four months prior to the sealing policy end date as public records. After April 1, 2022, 90.9% of cases filed are designated as public records by the court.[19] The RD estimate visualized in Figure 4a similarly finds that the end of the sealing period led to an 89 percentage point increase in the likelihood of an eviction case designated as a public record by the court. We interpret these findings as evidence of de jure compliance with the sealing rules.

To examine the de facto compliance with the sealing rules, we link eviction cases to the RIS public records database. Figure 3 documents that during the sealing period, few cases

[17]We match the court data to the tenant screening data using case numbers, while a tenant screening company doing the match for a landlord would rely on the personal identifiers and address history provided by the prospective tenant and the names and addresses available in the court records. Given our access to case numbers on both sides of the match, we expect our match rates to be superior to the average match rate of a tenant screening company.

[18]The less than 6% of filings that are found in the public records database are virtually all from the first few weeks of the immediate sealing period where Figure 2 shows that the courts did not appear to have enacted immediate sealing yet in practice. During the later part of the immediate sealing period that we focus on for our main analysis (December 1, 2021 through March 31, 2022), only 0.4% of eviction cases could be matched to the public records database.

[19]These court designations of public records are captured at the time of data acquisition.

(0.4%) can be found in the RIS database, but the end of the sealing policy coincides with a sharp increase in the likelihood of an eviction filing appearing in the RIS database. Among cases filed within four months after the sealing policy ended, 68% can be matched to the RIS database, and the RD estimate in Figure 4b similarly finds that eliminating the requirement that eviction cases be filed under seal led to a 67 percentage point increase in the likelihood of appearing in the RIS database.

This evidence of de facto compliance with the sealing rules is an important contribution to the policy debate around record-sealing laws. Jurisdictions considering similar legislation dedicate considerable attention to the mechanism used to seal records and to regulatory agencies' ability to prevent the distribution of sealed records by tenant screening companies. Given that cases filed during the sealed period of our analysis period in Cook County were automatically, immediately, and permanently sealed at the time of filing, we contribute evidence that this sealing mechanism is effective in preventing tenant screening companies and prospective landlords from accessing information about eviction cases and defendants.

5 Data Linkage and Summary Statistics

Table 1 summarizes the counts of cases and defendants included in our analysis sample that were matched to outcome datasets. Of the 4,144 defendants with a filing from December 1, 2021 through July 31, 2022, 25 percent are matched to Infutor and 68 percent to Experian. We omit the match rates to HMIS and the tenant screening data from this table, as those are presented as outcomes in the results section. We also examine whether the probability of matching eviction filings to Infutor and Experian data is correlated with the public status of an eviction filing in Table 2. In column 1, we regress an indicator for the tenant being matched to Infutor on an indicator for their case being filed during the public filing period, and in column 3, we repeat this exercise with an indicator for having a match in Experian. Columns 2 and 4 report RD estimates of any discontinuous changes in the relevant match rates around the cutoff filing date. We impose that the relationship between a successful match and the filing date is linear on either side of the cutoff, use a triangular kernel weighting function, and allow separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. We detect no imbalance in match rates to Infutor. Individuals with public records are slightly more likely to be matched to Experian, but the magnitude of this imbalance is relatively small.

In Table 3, we report the baseline mobility and neighborhood characteristics of the sample matched to Infutor. Mobility and housing conditions prior to the eviction filing appear similar across both groups. Approximately 20% of defendants with cases in the sealed period had moved in the past year and 6% had doubled up in the past year. To examine neighborhood quality, we merge census tract characteristics from the American Community Survey with the geolocated addresses from Infutor and the court records. For the linked sample, neighborhood characteristics are based on the Infutor address at the time of filing. At the time of filing, defendants from the sealed period, relative to those from the public period, live in neighborhoods with slightly lower poverty rates (14% of households below the federal poverty line) and higher median household income ($63,725) and rent ($1,223). Compared to the average census tract in Cook County, defendants with eviction filings reside in tracts with above-average poverty rates and below-average household incomes and rents.

Table 3 also compares defendants in our analysis period to those from cases filed in Cook County from 2016 through 2018. Defendants in our analysis period tend to live in lower poverty tracts at the time of the eviction filing, and they are more likely to have recently moved or lived doubled up before the filing date. The neighborhood characteristic rates of the 2016 through 2018 sample look similar to those reported in Collinson et al. (2024) for Cook County eviction cases from 2000 through 2016. The average residential mobility and neighborhood differences between pre- and post-pandemic cases may reflect different rental market conditions after the pandemic, which could translate to a different composition of defendants in eviction cases.[20]

6 Empirical Strategy

Our main empirical strategy relies on a difference-in-differences framework. We compare individuals with cases filed within four months before or after the end of the sealing policy, relative to each tenant's filing date. To strengthen our the difference-in-differences analysis we conduct triple-difference estimation using pre-pandemic eviction filings that allow us to account for seasonal variation (winter vs spring) in residential mobility patterns based on the filing date.

Because we are exploiting the variation determined by a particular date, it is natural to consider an RD in time design. In contrast to DiD and DDD estimation, the RD approach relies on filings within much narrower bandwidths around April 1, 2022.[21] We favor the difference-in-differences approach over the RD in time design because we do not find strong support for balance in case and neighborhood characteristics around the threshold date. The RD estimate identifies the causal impact of the reduced form of the public record under the

[20] It remains to be seen if the change in neighborhood composition among eviction cases persists after 2022. In concurrent work, we have requested the Court data through 2023.

[21] Note that the sample period of eviction filings begins approximately two months after the conclusion of the COVID-19 eviction moratorium in Illinois on October 4, 2021.

assumptions of (1) no manipulation or sorting of cases around the cutoff date and (2) that the observed and unobserved characteristics of eviction filings are continuous around the cutoff date. While the density of eviction filings is smooth around the cutoff (see appendix figure C1), appendix table C1 shows statistically significant differences in the types of type of cases filed and census tract characteristics around April 1, 2022. Figure 5 illustrates how these discontinuities around April 1st, occur on the first of every month. Due to these imbalances, we interpret the RD estimates with caution and favor DiD estimates for the remainder of the paper.[22]

6.1 Difference-in-Differences and Triple Difference Model

To estimate the impact of the end of the sealing policy, we use a DiD framework comparing outcomes among individuals with filings during the sealed period (before April 1, 2022) relative to individuals with filings during the public period (on or after April 1, 2022), before and after their individual filing dates. Formally, we adopt the following dynamic two-way fixed effects specification,

$$y_{im} = \alpha_i + \delta_m + \sum_{k \neq -1} \beta_k Public_i \times 1\{m = k\} + \epsilon_{im} \tag{1}$$

where y_{im} is an outcome for tenant i in relative month (or quarter) m since the eviction filing, which ranges from -12 to 24 (-4 through 8 for quarters). $Public_i$ is an indicator that the filing date is on or after April 1, 2022. We include individual fixed effects α_i and relative time fixed effects δ_m. To account for outcomes being correlated across time, we cluster the standard errors at the filing date level. In our preferred specification, we limit the sample to cases filed within four months before and after April 1, 2022. Our event studies take individuals within an 8-month window around April 1, 2022, and follow them for 12 months (4 quarters) before and 24 months (8 quarters) after their eviction filing date. The earliest cases included in our study sample were filed on December 1, 2021, making December 2020 the earliest month in which we observe an outcome. Analogously, the last cases included in our study sample were filed on July 31, 2022, extending the post-period through July 2024. Our results are robust to reducing the study sample window to 45 days before and after April 1, 2022, the inclusion of calendar month fixed effects for when the outcome is observed (e.g, a filing from April 2022 at relative month 2, will have a fixed effect for calendar month

[22]See Appendix C for more on validating the RD assumptions. In appendix D we present donut RD estimates, which attempt to remove the within-month variation in case characteristics. While the donut RD does slightly better on balance, the confidence intervals are larger than those from the conventional RD. Recent work highlights that donut RD can have substantially larger bias and variance than conventional RD estimators (Noack and Rothe 2023)

6), and a fixed effect for when the eviction moratorium was still in effect in the pre-period.

Estimates of β_k identify the causal effect of the public period under the assumption that, absent the end of the sealing policy, post-filing outcomes for the two groups would have continued to evolve in parallel. Although we are not able to directly test the validity of the parallel trends assumption, estimates of β_k for $k < 0$ test for any differences in pre-filing mobility and neighborhood trends.

To test for further violations that could arise from seasonal differences in post-filing outcomes, we compare our estimates of β_k to placebo estimates based on cases filed before and after April 1st of previous years (2016 - 2018).[23] We also present triple difference-in-differences (DDD) estimates that net out any seasonal differences in outcomes observed in placebo years. To achieve a slight improvement in precision, we present triple difference specifications at the quarterly level, instead of the monthly level. Our DDD specification is:

$$y_{im} = \gamma_i + \phi_{c(i),m} + \lambda_{period(i),m} + \sum_{k \neq -1} \beta_k^{DDD} 1\{Public_i\} 1\{m = k\} + \theta Mor_{im} + \upsilon_{im} \tag{2}$$

where $c(i)$ is the calendar quarter of the filing (January - December) in any year, $period(i)$ distinguishes cases filed during our main analysis period from cases filed in placebo periods, and Mor_{im} is an indicator for y_{im} being observed during the eviction moratorium period in Illinois (March 2020 through September 2021). In this specification, $\phi_{c(i),m}$ is a calendar quarter by relative month quarter fixed effect and $\lambda_{period(i),m}$ is a period (real vs. placebo years) by relative time fixed effect. As in the two-way difference-in-differences, we cluster our standard errors by the date of filing. We estimate the DDD specification using eviction cases filed in placebo years (between December 1, 2015 through July 31, 2018) and our main analysis period (December 1, 2021 through July 31, 2022).

6.2 Regression Discontinuity Design

Because we are exploiting variation from the end We complement our DiD approach with an RD in time design around the sealing end date. We estimate the following RD specification:

$$y_i = \beta_0 + \beta_1 \mathbb{1}\{Date_i \geq \tau\} + f(Date_i) + \varepsilon_i \tag{3}$$

where y_i is an outcome for tenant i named in an eviction case filed between December 1, 2021 and July 31, 2022. $Date_i$ is the filing date of tenant i's case, τ is the sealing policy

[23] We focus on placebo years prior to the COVID-19 pandemic to avoid atypical seasonal patterns in post-filing mobility during 2020 and 2021 when few eviction cases were filed due to eviction moratoria.

end date (April 1, 2022), and f is linear on either side of the cutoff filing date with separate slopes. When estimating Equation 3, we impose a triangular kernel weighting function and a data-driven bandwidth selector that allows separate bandwidths on each side of the cutoff and minimizes the mean square error (MSE) of the RD estimate (Calonico et al. 2014).[24]

We focus mainly on the reduced-form estimates, where β_1 corresponds to the causal effect of the cutoff date (i.e, the end of the sealing policy) on the outcome. Because some cases filed during the public period may be sealed for other reasons unrelated to the COVID-19 Emergency Housing Act, the April 1, 2022 cutoff date does not perfectly predict the public status of eviction cases.[25] Therefore, we also present IV estimates where the reduced form estimate of β_1 is rescaled by the first stage estimate of θ_1 from the following specification:

$$Public_i = \theta_0 + \theta_1 \mathbb{1}\{Date_i \geq \tau\} + g(Date_i) + \upsilon_i \tag{4}$$

where $Public_i$ is an indicator for the case i being visible to the public (i.e. found in the RIS public records database).

7 Results

7.1 Residential Mobility Results

We next present results of the effects of a public eviction filing on tenants' residential mobility patterns and housing situation for up to two years following an eviction filing. Along with the DiD model estimates, we plot the raw trends of tenant outcomes before and after an eviction filing for individuals with filings in the sealed period and individuals with filings in the public period.

Prior to an eviction filing, tenants with cases filed in the sealed and public periods exhibit similar trends in doubling up and overall mobility on the left panel of Figure 6, which is consistent with the parallel trends assumption. After the filing, these rates of doubling up and moving diverge, and tenants with public period cases appear more likely to move and double up. The DiD coefficients plotted on the right panel of Figure 6 and reported in Table 4 indicate that the end of mandated sealing increased the rate of doubling up one year post-filing by 3.1 percentage points (16.5%). This evidence of increased doubling-up is consistent

[24]We use the median bandwidth from the following methods: one common MSE-optimal bandwidth selector for the RD treatment-effect estimator, two different MSE-optimal bandwidth selectors (below and above the cutoff), and one common MSE-optimal bandwidth selector for the sum of regression estimates.

[25]Foreclosure-related eviction filings were required to be sealed throughout the entire analysis period. In other rare occurrences, eviction filings may be sealed at the discretion of the court if the court finds the case was filed without sufficient reason. For details, see 735 ILCS 5/9-121.

with tenants with public eviction records facing more difficulty securing a new lease of their own and resorting to doubling up with friends or relatives more often than tenants with sealed eviction records.

Overall mobility also appears to increase; tenants named in public period filings are 5.5 percentage points (18.9%) more likely to have moved at least once and 5.7 percentage points (23.9%) more likely to have moved to a new zip code within one year post-filing. Similar to the doubling-up results, this evidence of more frequent and distant moves may be indicative of less stable housing for tenants with public records.

Except for the doubled-up indicator, all the other mobility measures in Figure 6 show a clear pattern where the effects are concentrated within the first twelve months after the filing, and the differences between the sealed and public group have mostly faded by two years after the filing.[26] The short-run effects can be interpreted as differences in search patterns between tenants with sealed and public records. Tenants with a public filing record may give up searching for a new rental unit faster than those with a sealed record, making them more likely to move and more likely to experience more moves if the quality of the match is deficient. The more persistent effects in the doubled-up rate may be attributed to persistent challenges in securing a lease for a tenant with a public record.

Figure 7 presents event study estimates separately for cases that ended in a court-order eviction and those that did not. The effects of the end of the sealing policy are similar for evicted and not evicted households, providing evidence that our main mobility and doubled-up results are not driven exclusively by evicted households.

The placebo estimates in Figure 8 show persistent seasonal trends in mobility and doubled-up rates when comparing winter (Dec-Mar) filings to spring (April-July) filings. After adjusting our estimates for these seasonal trends, the DDD results in Figure 9 and Table 5 demonstrate that the seasonal trend attenuates the effect we observe in the DiD event studies, but it does not fully account for the effects we observe in our primary specification, especially in the short run. Table 5 presents the DiD estimates in the odd numbered columns and the DDD in the even numbered columns. We highlight the results shown in columns 5 through 8, where the short run effects on moving to a different zipcode and living doubled up are statistically significant across the two specifications. Within the first year of the filing, tenants with public-period filings are more likely to have moved to a different zip code and to live doubled-up, even after accounting for seasonal patterns. The triple difference estimates show that within the first six months of filing, tenants with public period

[26]These findings are robust to using a narrower sample of cases filed within 45 days before or after the end of the sealing policy (Appendix Figure B4) and controlling for seasonal patterns based on the month the outcome variable is observed and the presence of the COVID-19 moratorium in the pre-period (Appendix Figure B5).

filings 3.5 percentage points more likely to have moved to a different zip code (25%) and 2.5 percentage points more likely to live doubled up (20%).

We hypothesize that those seasonal trends may be due to differences in the conditions tenants face post-filing at different times of the year. For example, tenants with filings post April 1st, are more likely to have received their tax refund, which they can use for a new security deposit, and more leases expire in the summer, so they may face a rental market with more vacancies. This is in contrast to the situation for a tenant who is filed against in the winter, when it is generally more difficult to move and households may be more cash-constrained.

Despite concerns about the validity of the RD approach, RD mobility estimates are largely consistent with the DiD estimates. Comparing the RD and DiD estimates in Figure 10, the RD analysis lacks precision, but the results generally suggest that tenants with cases filed during the public period exhibit increased mobility.[27] We also observe residential mobility in the Experian data—in particular, whether a tenant moved to a different zip code than the eviction filing address—and estimate RD mobility coefficients using this Experian measure. Figure 11 demonstrates that we find qualitatively similar mobility results when using the Experian measure of mobility among the larger sample of individuals that can be matched to Experian.[28]

We report results on neighborhood characteristics around the time of eviction filings in Figure 12. Prior to the date of filing, tenants with cases in the public period tend to reside in higher poverty, lower income, and lower rent census tracts, but these characteristics appear to evolve in parallel across the two groups. Post-filing, tenants with cases in the public period appear to exhibit decreases in neighborhood poverty relative to tenants with cases in the sealed period. The DiD coefficients in Figure 12 and Table 6 provide evidence that by one year after the filing, tenants named in public period filings reside in census tracts that have 0.6 percentage points (3.9%) lower poverty rates compared to tenants named in filings during the sealing period.[29] Figure 13 provides supporting evidence that moves to lower-poverty tracts by tenants with public period cases are driven by moves into doubled-up housing. Figure 14 suggests that moves to lower-poverty neighborhoods are driven by both defendants with and without court-ordered evictions.

[27] Figure C2 visualizes the reduced-form RD estimate of mobility changes after two years post-filing. Appendix Tables C2 and C3 report the RD reduced-form and IV estimates, respectively. Appendix Section D reports donut RD estimates.

[28] We are able to match nearly twice as many individuals to Experian compared to Infutor. Since we do not observe Experian characteristics prior to the eviction filing date, we exclusively use an RD approach with Experian outcomes.

[29] These findings are robust to using a narrower sample of cases filed within 45 days before or after the end of the sealing policy (Appendix Figure B6) and controlling for seasonal patterns (Appendix Figure B7).

The placebo check in Figure 15 confirms that the improvements in neighborhood poverty for public-period filings exceed what we can attribute to seasonal trends. The placebo checks for median rent and median household income do not exhibit any seasonal trends, and look fairly similar to the coefficients from our primary DiD specification. In line with the comparison to pre-covid years as a placebo, the triple difference event studies in Figure 16 and Table 7, show some attenuation of the effects for moves to lower-poverty neighborhoods relative to the simple DiD estimates, but largely the same pattern. The largely null effects for changes in census tract median income and median rent are almost the same in the triple difference specification.

RD estimates plotted in Figure 17 suggest qualitatively similar patterns of relative changes to tract poverty around the eviction filing date. However, the RD approach estimates large differences in pre-filing tract poverty rates that shrink after the eviction filing date. These RD results are consistent with the imbalances documented in Appendix Table C1. We find suggestive but largely insignificant estimates of effects on the median household income and rents where tenants reside.

7.2 Homelessness Results

Figure 18 plots the raw trends of homelessness service utilization and event study estimates from linking our study sample in Chicago to the HMIS data. The raw trends show that among those with an eviction filing, engagement with CoC homeless services is relatively low.[30,31] We do not find a statistically significant effect of the end of mandated sealing on CoC engagement within the two years after an eviction filing. This is true for cumulative overall engagement rates, cumulative engagement for homelessness services, and cumulative shelter entries. The coefficients in the first two columns of Table 8 suggest that public records increase the likelihood of experiencing homelessness by 0.1 to 0.2 percentage points (5 to 6 percent of the control group mean). Using 95% confidence intervals, we can rule out increases in homelessness service use greater than 0.68 percentage points (34%) within one year and greater than 0.8 percentage points (26%) within two years.

[30] 2.1% of defendants with a case filed during the sealing period (before April 1, 2022) and 2.5% of defendants with a case filed during the public period (on or after April 1, 2022) accessed homeless services within 12 months after eviction filing. The rates rise to 3.4 percent and 3.9 percent for these respective groups when measuring the outcome 24 months after filing. Our rates of homelessness engagement are similar to those reported among callers to the Homelessness Prevention Call Center in Chicago (Evans et al. 2016)

[31] Since engagement with homeless services is relatively rare, we construct cumulative outcomes at the quarterly rather than monthly level. For example, service interaction in the same quarter as the filing date refers to activity within 0–2 months post-filing, while one quarter post-filing captures service utilization up to 3-5 months after the filing date.

7.3 Financial Health Results

We next present results of the effects of a public eviction filing on tenants' financial health. These outcomes are constructed from linking Cook County court records to snapshots of Experian credit files from quarter 1 of 2022 through quarter 3 of 2024. These outcomes correspond to financial health measured between up to 9 quarters (2.25 years) post-filing for cases filed near the cutoff filing date. Since we do not consistently observe pre-filing measures of financial health, we exclusively rely on the RD approach to estimate financial health effects[32]

Overall, our estimates are not precise enough for us to rule out a large effect on financial health. Figure 19 plots the reduced form effects of the end of the sealing period on four measures of financial health—credit scores, any open revolving account, balance in collections, and any auto lease or loan—for each quarter post-filing.[33] The end of the immediate sealing policy did not lead to statistically significant changes in credit scores or durable consumption (measured auto loans or leases) within 9 quarters post-filing.We detect a statistically significant decrease in access to credit (as measured by having any open revolving account) in the first quarter post-filing, but the reduced-form RD estimates of the impact on credit access shrink and become statistically insignificant as more time passes post-filing. The end of sealing may have led to increases in collection balances, but the confidence intervals of our estimates are too large to be able to say anything conclusive about the effect of the policy on these credit outcomes. Similarly, based on fuzzy RD estimates reported in Appendix Table C5, we cannot reject that public eviction filings cause a 16.1 percentage point (35%) reduction in access to an open revolving account or a $2,589 (70%) increase in collection balances 8 quarters post-filing.

8 Discussion and Conclusion

Landlords file an average of 3.6 million eviction cases annually in the US, amounting to almost 7% of renting household being listed in an eviction filing each year (Gromis et al. 2022; Graetz et al. 2023). Although an eviction filing is undoubtedly a negative shock for tenants, as it increases the risk of displacement and subsequent homelessness, many argue that the public record associated with the filing carries its own burden in the aftermath of the

[32] We have data from quarters 3 and 4 in 2019 and from quarter 3 in 2021 through quarter 1 in 2025. Due to the gap in the pre-period data, the event study approach is not as informative for the analysis of these data.

[33] Appendix Figures C3 and C4 visualize the reduced-form RD estimates of changes to financial health one- and two-years post-filing, and Appendix Table C4 reports the reduced-form RD point estimates for each measure of financial health.

case (Franzese 2018; Kiviat 2019). Leveraging variation from an eviction record sealing law in Illinois, this paper uses a difference-in-differences framework to study the causal effect of public eviction records on residential mobility, living doubled-up, homelessness, and financial health.

Our results imply that the effects of eviction filing records are much more modest and nuanced relative to the predominant narrative among tenant advocates and news outlets, which tends to emphasize large and persistent negative effects on neighborhood quality and housing instability. We find that tenants with an eviction filed after the end of the sealing—the public period—are about 20% more likely to move within a few months of their filing date and are more likely to move to neighborhoods with slightly lower poverty rates. Using a novel measure of doubled-up status, we find that a likely explanation for these moves to lower-poverty neighborhoods involves tenants moving in with friends or relatives. We do not detect statistically significant changes in the interaction with homelessness services within two years of the filing date and the coefficients are consistently small, providing suggestive evidence that the sealing policy did not affect tenants on extreme outcomes such as unsheltered homelessness. While doubling-up may function as a private safety net, it likely remains an unstable housing situation, especially for those who have recently experienced negative shocks such as evictions. For financial health outcomes, we lack precision to rule out large impacts. In ongoing work, we examine longer-run mobility and financial health outcomes.

The interpretation of our results should also take into account the multiple barriers to stable housing and high-opportunity neighborhoods faced by our study sample, and more broadly, faced by the population with eviction filing records. The sealing of an eviction record for someone with a single interaction with housing court and a relatively steady credit score may derive large benefits from a sealing policy. However, as well documented by Collinson et al. (2024), individuals with an eviction filing—those evicted and not evicted—experience significant drops in credit scores before the eviction filing, making the eviction record one of potentially many negative signals in the rental market. In this context, the signals that accumulate during the negative economic shock that precedes the filing may attenuate the effects policymakers and housing advocates expect from a sealing policy. Low-income tenants are also more likely to have older eviction records or gain new ones, which would also attenuate the effects of the policy we study.

Our results are a direct input for any welfare analysis on the effects of permanent record sealing laws. The temporary nature of the sealing law in Illinois allows us to isolate the causal effects of the public record in a way that would not be possible with a permanent policy. Other important inputs to accurately assess the overall welfare effect of sealing policies, which we do not address in this paper, include the potential racial disparities in the effects of eviction records, the behavioral changes of landlords when record-sealing policies are permanent, and the power of eviction records or other pieces of information to predict timely rent payments.

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9 Tables and Figures

Table 1: Observation Counts and Match Rates

	Cases		Defendants	
	N	Match Rate	N	Match Rate
Full Sample				
Total	60,592		141,376	
Attempted Match	49,910	1.00	62,693	1.00
Matched to Infutor	12,477	0.25	13,557	0.22
Matched to Experian	31,144	0.62	35,639	0.57
Analysis Period				
Total	18,171		47,484	
Attempted Match	13,347	1.00	16,735	1.00
Matched to Infutor	3,832	0.29	4,144	0.25
Matched to Experian	9,887	0.74	11,302	0.68

The full sample includes eviction cases filed in Cook County between March 11, 2019 and March 23, 2023. The analysis period includes cases filed between December 1, 2021 and July 31, 2022. Matches were attempted for cases and defendants with non-missing names and addresses that could be geocoded.

Table 2: Probability of Matching

	Infutor		Experian	
	OLS	RD	OLS	RD
	(1)	(2)	(3)	(4)
Public Period	0.0001	-0.031	0.025***	0.053*
	(0.007)	(0.026)	(0.007)	(0.032)
Observations	16735	16735	16735	16735

*p< 0.1, **p< 0.05, ***p< 0.01. Columns 1 and 3 report the results of separately regressing measures data availability in either Infutor or Experian on an indicator for the case being filed during the public period (on or after April 1, 2022). Columns 2 and 4 report the conventional RD estimates of discontinuous changes in the match rate to Infutor or Experian around the cutoff filing date. When generating the RD estimates, we impose that the relationship between a successful match and the filing date is linear on either side of the cutoff, use a triangular kernel weighting function, and allow separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. The sample includes all tenants in eviction cases filed between December 1, 2021 and July 31, 2022.

Table 3: Baseline Infutor Summary Statistics

	Sealed Period	Public Period	Pre-COVID	Cook County Tracts
Mobility				
Any move in past 12 months	0.197	0.208	0.170	
	(0.398)	(0.406)	(0.376)	
Any move in past 24 months	0.321	0.355	0.294	
	(0.467)	(0.479)	(0.456)	
Doubled up in past 12 months	0.064	0.074	0.061	
	(0.245)	(0.262)	(0.240)	
Doubled up in past 24 months	0.123	0.135	0.111	
	(0.329)	(0.342)	(0.314)	
Neighborhood Characteristics				
Tract % below FPL	14.383	15.353	21.668	11.608
	(12.060)	(12.263)	(14.816)	(11.546)
Tract % below 200% FPL	36.400	37.868	46.868	31.753
	(17.634)	(17.897)	(19.294)	(18.719)
Tract median HH income	63,725	61,543	43,873	75,565
	(30,292)	(29,708)	(22,668)	(38,257)
Tract median rent	1,223	1,193	967	1,291
	(386)	(370)	(279)	(412)
Observations	1,813	2,331	15,397	1,332

Columns 1 through 3 report the means and standard deviations of mobility and neighborhood characteristics for defendants with cases filed in the sealed period (December 2021 - March 2022), public period (April 2022 - July 2022), and before the COVID-19 pandemic (January 2016 - July 2018) that could be matched to Infutor. Mobility and neighborhood characteristics are baseline values at the time of the eviction filing. Column 4 reports the means and standard deviations of neighborhood characteristics across all census tracts in Cook County. The top part of column 4 is missing because mobility measures constructed using Infutor are only available for the study sample. Census tract characteristics are 2021 5-year ACS estimates.

Table 4: DiD Mobility Estimates

	Any move	Number of moves	Different zipcode	Doubled up
	(1)	(2)	(3)	(4)
Public period × 6 months post-filing	0.049***	0.056***	0.053***	0.036***
	(0.014)	(0.017)	(0.012)	(0.012)
	[0.186]	[0.210]	[0.140]	[0.122]
Public period × 12 months post-filing	0.055***	0.073***	0.057***	0.031**
	(0.015)	(0.021)	(0.015)	(0.014)
	[0.290]	[0.360]	[0.238]	[0.188]
Public period × 18 months post-filing	0.040**	0.054**	0.039***	0.026*
	(0.015)	(0.023)	(0.015)	(0.015)
	[0.344]	[0.452]	[0.293]	[0.218]
Public period × 24 months post-filing	0.035**	0.036	0.032**	0.026
	(0.015)	(0.024)	(0.015)	(0.015)
	[0.370]	[0.504]	[0.319]	[0.235]
Defendant FE	✓	✓	✓	✓
Relative Month FE	✓	✓	✓	✓
Observations	153,328	153,328	153,328	150,544
R^2	0.512	0.480	0.509	0.510

This table reports DiD results from estimating Equation 1. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Public period is an indicator for the case being filed on or after April 1, 2022. The relative month corresponds to the month relative to the eviction filing and takes a value between -12 and 24. Standard errors are clustered at the filing date level and reported in parentheses. Outcome means among the control (sealed) group are reported in brackets.

Table 5: DiD and DDD Mobility Estimates

	Any move		Number of moves		Different zipcode		Doubled up	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public period × 1 quarter post-filing	0.029**	0.015	0.028*	0.010	0.040***	0.029**	0.032***	0.025**
	(0.012)	(0.014)	(0.015)	(0.017)	(0.010)	(0.012)	(0.010)	(0.011)
	[0.094]	[0.094]	[0.105]	[0.105]	[0.066]	[0.066]	[0.059]	[0.059]
Public period × 2 quarter post-filing	0.036**	0.013	0.042**	0.015	0.055***	0.035**	0.036***	0.025*
	(0.015)	(0.017)	(0.019)	(0.021)	(0.012)	(0.014)	(0.013)	(0.014)
	[0.186]	[0.186]	[0.210]	[0.210]	[0.140]	[0.140]	[0.123]	[0.123]
Public period × 3 quarter post-filing	0.043***	0.016	0.062***	0.033	0.062***	0.041***	0.031**	0.018
	(0.016)	(0.018)	(0.022)	(0.024)	(0.014)	(0.015)	(0.015)	(0.016)
	[0.251]	[0.251]	[0.298]	[0.298]	[0.197]	[0.197]	[0.174]	[0.174]
Public period × 4 quarter post-filing	0.042**	0.017	0.058**	0.031	0.059***	0.037**	0.028*	0.014
	(0.017)	(0.019)	(0.024)	(0.026)	(0.015)	(0.017)	(0.014)	(0.016)
	[0.290]	[0.290]	[0.360]	[0.360]	[0.238]	[0.238]	[0.192]	[0.192]
Public period × 8 quarter post-filing	0.021	0.002	0.021	-0.004	0.034**	0.016	0.023	0.015
	(0.017)	(0.019)	(0.026)	(0.029)	(0.015)	(0.017)	(0.015)	(0.017)
	[0.370]	[0.370]	[0.504]	[0.504]	[0.319]	[0.319]	[0.237]	[0.237]
Observations	253,266	53,872	253,266	53,872	253,266	53,872	249,327	52,810
R^2	0.495	0.509	0.461	0.474	0.520	0.505	0.520	0.505

This table reports DiD and DDD results from estimating Equation 2 at the quarterly level. Odd numbered colums show DiD estimates and even numbered columns show DDD estimates. The sample for DDD estimates includes defendants with cases filed between December 1, 2015 and July 31, 2018 and between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The sample for DiD estimates only includes the defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Every regression includes defendant fixed effects, relative quarter by filing month fixed effects, relative quarter by period fixed effects, and a control for observing the outcomes during the eviction moratorium period in Illinois (March 2020 through September 2021). Public period is an indicator for the case being filed on or after April 1, 2022. The relative quarter corresponds to the quarter relative to the eviction filing and takes a value between -4 and 8. The filing quarter corresponds to the month of the eviction filing and takes a value between 1 and 4. Period fixed effects distinguish eviction cases filed during the main analysis period (December 1, 2021 through July 31, 2022) from eviction cases filed in earlier periods. Standard errors are clustered at the filing date level and reported in parentheses. Outcome means among the control (sealed) group are reported in brackets.

Table 6: DiD Neighborhood Estimates

	% below FPL	% below 200% FPL	Med HH income	Med rent
	(1)	(2)	(3)	(4)
Public period × 6 months post-filing	-0.321	-0.298	119	4.581
	(0.232)	(0.339)	(536)	(6.112)
	[14.670]	[36.648]	[63,228]	[1,219]
Public period × 12 months post-filing	-0.575**	-0.799*	409	3.092
	(0.285)	(0.450)	(694)	(7.538)
	[14.681]	[36.681]	[63,601]	[1,218]
Public period × 18 months post-filing	-0.205	-0.457	52.326	-3.673
	(0.316)	(0.479)	(785)	(8.607)
	[14.436]	[36.479]	[63,830]	[1,221]
Public period × 24 months post-filing	-0.129	-0.461	222	-4.870
	(0.338)	(0.493)	(807)	(9.125)
	[14.477]	[36.562]	[63,666]	[1,222]
Defendant FE	✓	✓	✓	✓
Relative Month FE	✓	✓	✓	✓
Observations	146,202	146,206	145,179	143,303
R^2	0.832	0.840	0.842	0.846

This table reports DiD results from estimating Equation 1. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Public period is an indicator for the case being filed on or after April 1, 2022. The relative month corresponds to the month relative to the eviction filing and takes a value between -12 and 24. Neighborhood characteristics are measured at the census tract level using 5-year ACS estimates. Poverty rate outcomes are measured in percentages and take a value between 0 and 100. Standard errors are clustered at the filing date level and reported in parentheses. Outcome means among the control (sealed) group are reported in brackets.

Table 7: DiD and DDD Neighborhood Estimates

	% below FPL		% below 200% FPL		Med HH income		Med rent	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public period × 1 quarters post-filing	0.247	0.249	0.357	0.303	-622	-417	-2.28	-0.467
	(0.172)	(0.205)	(0.254)	(0.293)	(430)	(457)	(5.52)	(5.93)
	[14.403]	[14.403]	[36.5]	[36.5]	[63,635]	[63,635]	[1,222]	[1,222]
Public period × 2 quarters post-filing	-0.202	-0.064	-0.105	-0.044	-35.8	137.3	5.054	5.971
	(0.240)	(0.270)	(0.345)	(0.383)	(522)	(555)	(7.035)	(7.435)
	[14.670]	[14.670]	[36.7]	[36.648]	[63,229]	[63,229]	[1,219]	[1,219]
Public period × 3 quarters post-filing	-0.447	-0.261	-0.426	-0.307	217	298	7.3	7.7
	(0.274)	(0.303)	(0.394)	(0.430)	(616)	(651)	(7.8)	(8.3)
	[14.759]	[14.759]	[36.7]	[36.7]	[63,325]	[63,325]	[1,214]	[1,214]
Public period × 4 quarters post-filing	-0.456	-0.292	-0.605	-0.507	254	367	3.627	3.405
	(0.295)	(0.324)	(0.443)	(0.477)	(666)	(701)	(8.1)	(8.6)
	[14.681]	[14.681]	[36.7]	[36.681]	[63,601]	[63,601]	[1,218]	[1,218]
Public period × 8 quarters post-filing	-0.010	0.073	-0.268	-0.287	74.3	360	-4.251	-3.297
	(0.350)	(0.377)	(0.493)	(0.530)	(791)	(834)	(9.5)	(10.173)
	[14.477]	[14.477]	[36.562]	[36.562]	[63,666]	[63,666]	[1,222]	[1,222]
Observations	48,598	229,543	48,599	229,544	48,260	229,127	47,630	227,825
R^2	0.830	0.905	0.838	0.905	0.841	0.902	0.846	0.894

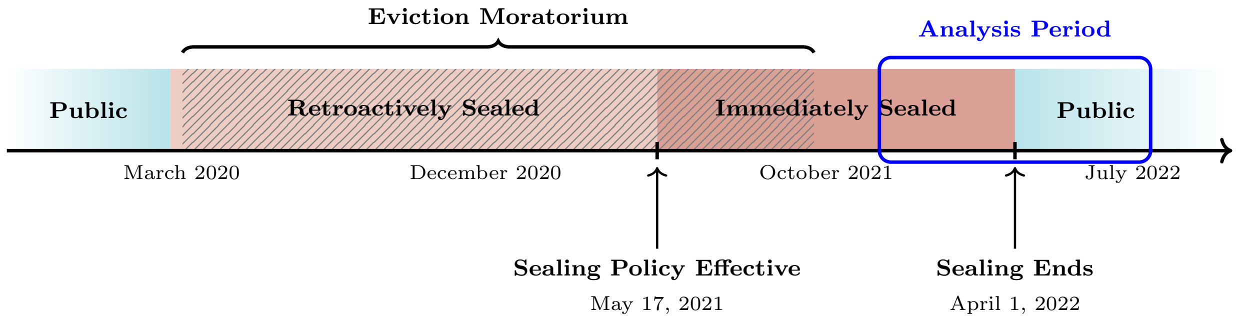
This table reports DiD and DDD results from estimating Equation 2 at the quarterly level. Odd numbered colums show DiD estimates and even numbered columns show DDD estimates. The sample for DDD estimates includes defendants with cases filed between December 1, 2015 and July 31, 2018 and between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The sample for DiD estimates only includes the defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Every regression includes defendant fixed effects, relative quarter by filing month fixed effects, relative quarter by period fixed effects, and a control for observing the outcomes during the eviction moratorium period in Illinois (March 2020 through September 2021). Public period is an indicator for the case being filed on or after April 1, 2022. The relative quarter corresponds to the quarter relative to the eviction filing and takes a value between -4 and 8. The filing quarter corresponds to the month of the eviction filing and takes a value between 1 and 4. Period fixed effects distinguish eviction cases filed during the main analysis period (December 1, 2021 through July 31, 2022) from eviction cases filed in earlier periods. Standard errors are clustered at the filing date level and reported in parentheses. Outcome means among the control (sealed) group are reported in brackets.

Table 8: Homelessness Services Utilization

	Shelter service (cumulative)	Homeless service (cumulative)	Any CoC service (cumulative)
	(1)	(2)	(3)
Public period × 3-5 months post-filing	-0.002	-0.001	-0.005
	(0.002)	(0.002)	(0.004)
	[0.006]	[0.008]	[0.040]
Public period × 9-11 months post-filing	0.000	0.001	-0.004
	(0.003)	(0.003)	(0.005)
	[0.015]	[0.02]	[0.063]
Public period × 15-17 months post-filing	0.002	0.001	-0.004
	(0.003)	(0.003)	(0.006)
	[0.02]	[0.028]	[0.078]
Public period × 21-23 months post-filing	0.003	0.000	-0.002
	(0.003)	(0.004)	(0.006)
	[0.024]	[0.034]	[0.090]
Defendant FE	✓	✓	✓
Relative Month FE	✓	✓	✓
Observations	119,616	119,616	119,616
R^2	0.516	0.536	0.681

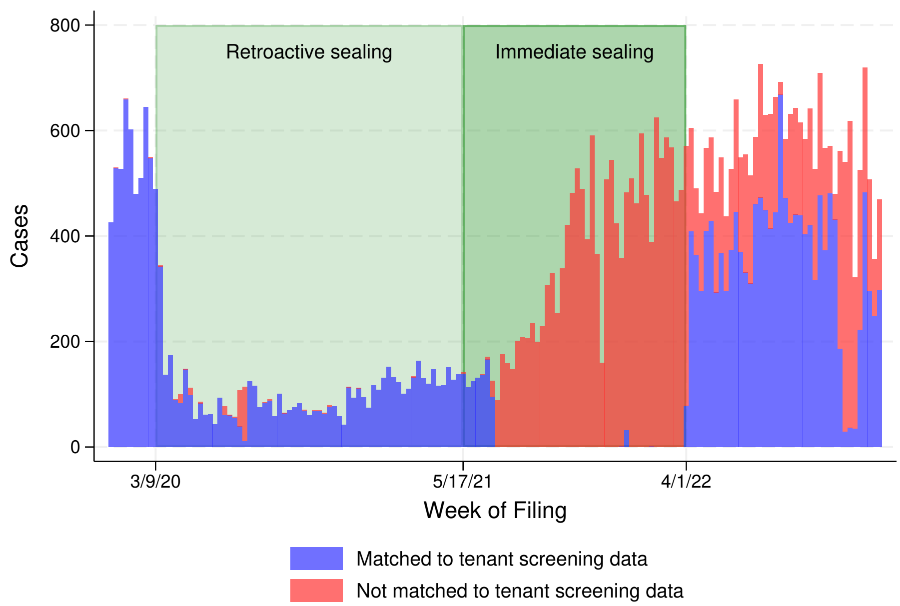
*p< 0.1, **p< 0.05, ***p< 0.01. This table reports DiD estimates using data from the Homelessness Management Information System (HMIS). Each measure represents cumulative entries into HMIS services every quarter relative to the filing date. Shelter services include entries to emergency shelters or safe haven programs. Homeless services include any shelter service, transitional housing, or street outreach. Any Continuum of Care (CoC) services includes enrollment in any homeless services listed above, housing services (such as Rapid Re-Housing, Permanent Housing, and permanent supportive housing), or other services directed towards those at risk of homelessness (such as Coordinated Entry and Homelessness Prevention). Standard errors are clustered at the filing date level and reported in parentheses. Outcome means among the control (sealed) group are reported in brackets.

Figure 1: Illinois Sealing Policy Timeline



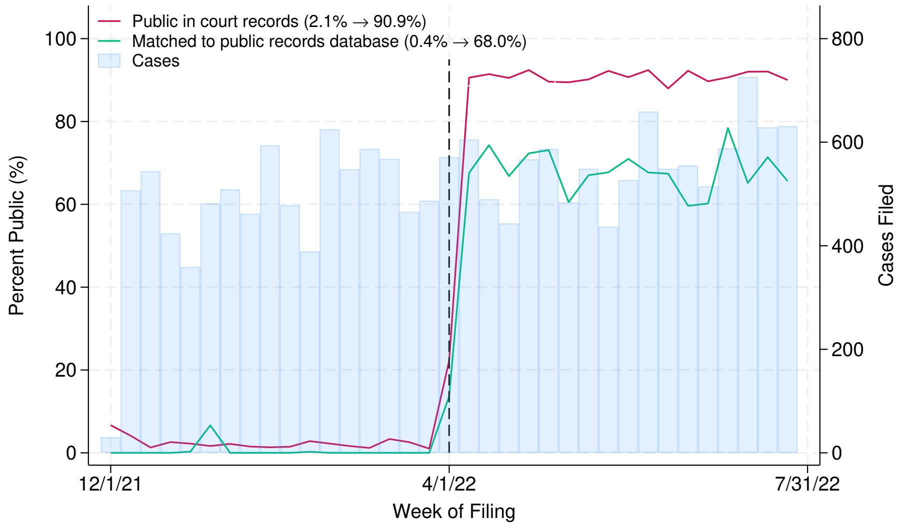
This figure plots the timeline of relevant eviction sealing policy changes in Illinois. Eviction cases filed between March 9, 2020 and May 16, 2021 were to be retroactively sealed as of May 17, 2021. Eviction cases filed between May 17, 2021 and March 31, 2022 were to be filed under seal. The eviction moratorium was in effect between March 14, 2020 and October 3, 2021 in Cook County, Illinois.

Figure 2: Retroactive vs. Immediate Sealing



This figure plots the weekly volume of eviction filings in Cook County, Illinois between January 1, 2020 and December 31, 2022. The blue bars represent filings that were matched to a public records database and the stacked red bars represent filings that were not found in the public records database.

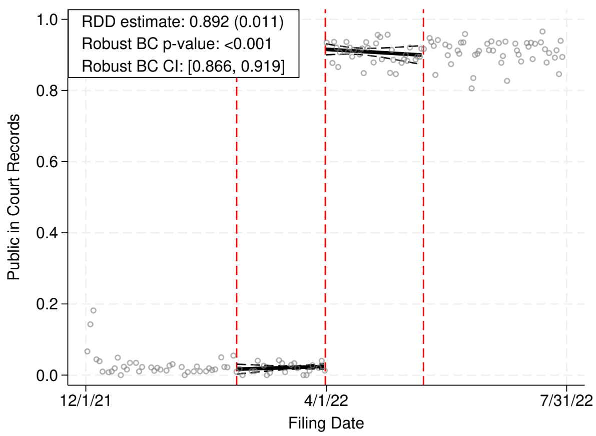
Figure 3: Variation in Public Status of Eviction Filings



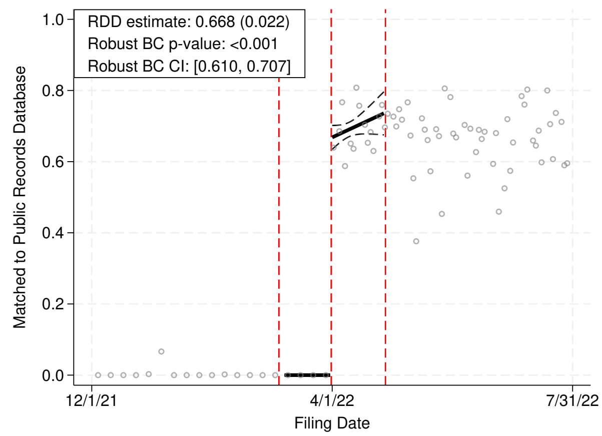
This figure plots the share of weekly filings that were public records around the end of the sealing policy on April 1, 2022. The red line plots the percent of weekly filings designated as public records by the court, and the green line plots the percent of weekly filings that could be matched to a public records database. The blue bars correspond to the volume of cases filed by week.

Figure 4: First Stage Effects

(a) De Jure First Stage Effect

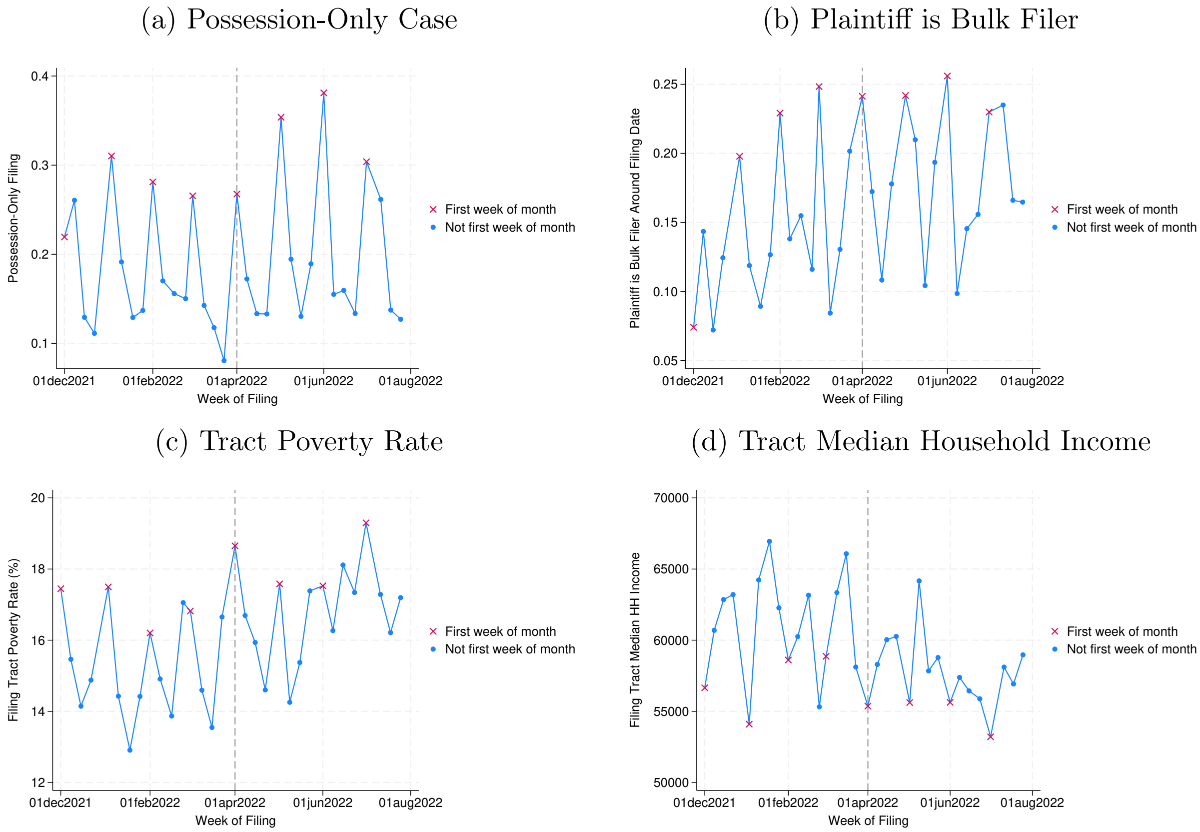


(b) De Facto First Stage Effect



This figure plots the first stage of the RD, corresponding to equation 4, which estimates the effect of the end of the sealing policy on the public status of eviction cases. The sample includes cases filed between December 1, 2021 and July 31, 2022. Subfigure (a) reports the first stage effect on whether the eviction case was designated as public in the records received directly from the court. Subfigure (b) reports the first stage effect on whether the eviction case was found in the RIS public records database. The reported RD estimate and standard error are the conventional estimates. The robust bias-corrected p-value and confidence interval correspond to the bias-corrected RD estimate and the robust variance estimator. When generating these estimates, we impose that the relationship between the case characteristic and filing date is linear on either side of the cutoff, use a triangular kernel weighting function, and allow separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate.

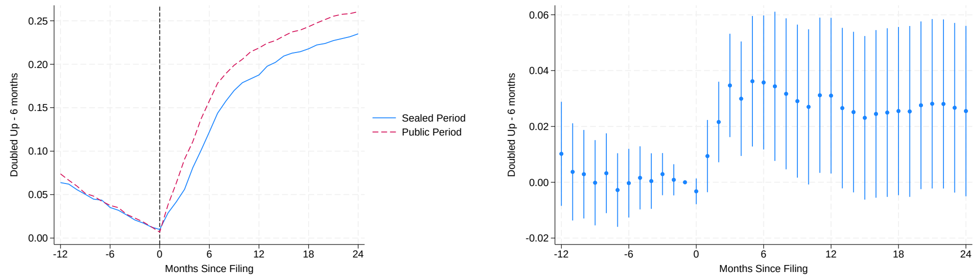
Figure 5: Monthly Cyclicality in Eviction Filing Characteristics



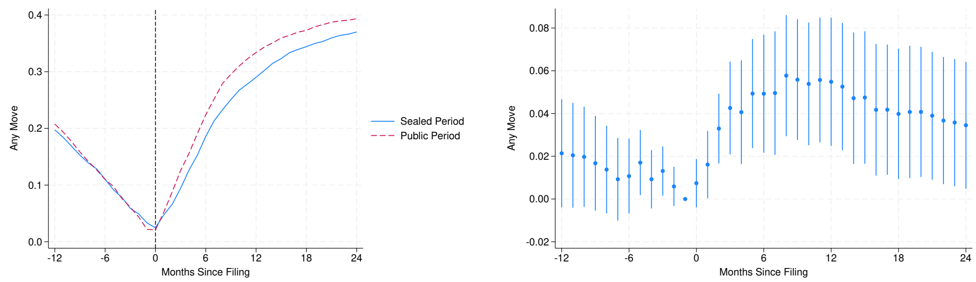
This figure plots average characteristics of eviction filing over the the analysis period. The first week of the month is defined as the first five business days within a given month. A plaintiff is a bulk filer around a given filing date if their filing volume in the period of 30 days on either side of the filing date is in the 95th percentile. Census tract characteristics are 2021 5-year ACS estimates.

Figure 6: Mobility Raw Trends and DiD Estimates

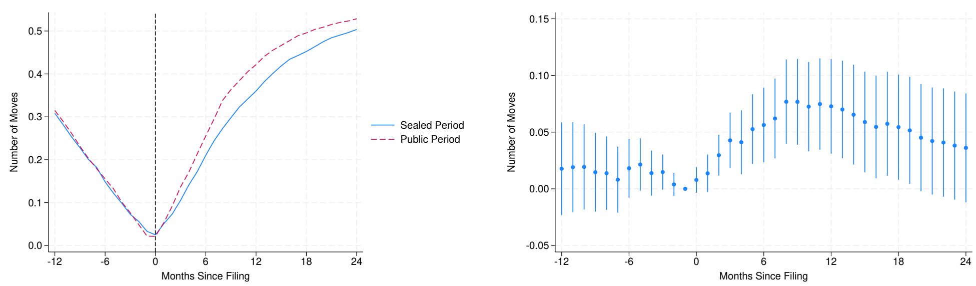
(a) Doubled Up



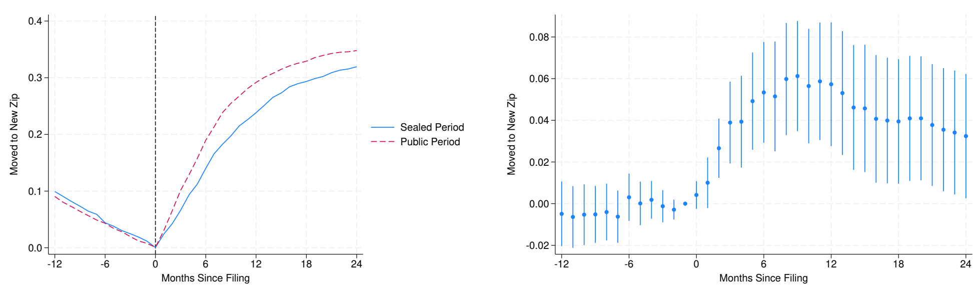
(b) Any Move



(c) Cumulative Number of Moves



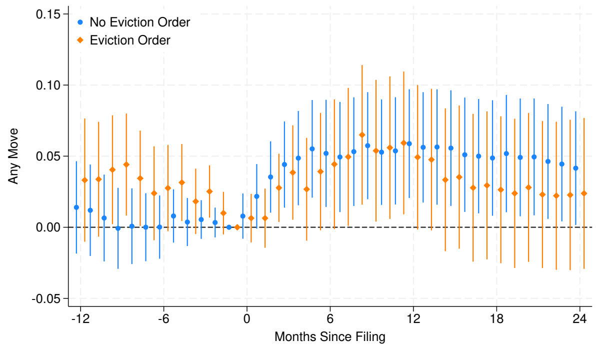
(d) Different Zip



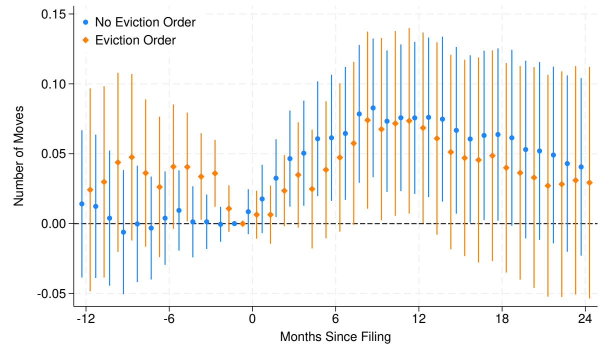
The figures on the left side plot raw means around the time of an eviction filing, separately for defendants with cases filed in the public and sealed period. The figures on the right side plot the event study estimates from Equation 1. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Doubled up indicates that an individual moved into a unit that overlaps with the tenure of other individuals who moved in at least three months before and do not move out within three months after. Any move corresponds to at least one move occurring within the number of months shown on the x-axis before or after the month of filing. Number of moves is the sum of moves that occurred within the number of months shown on the x-axis before or after the month of filing. Different zip code corresponds to moving to an address with a zip code that does not match the zip code from the time of filing. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 7: Mobility DiD Estimates by Case Outcome

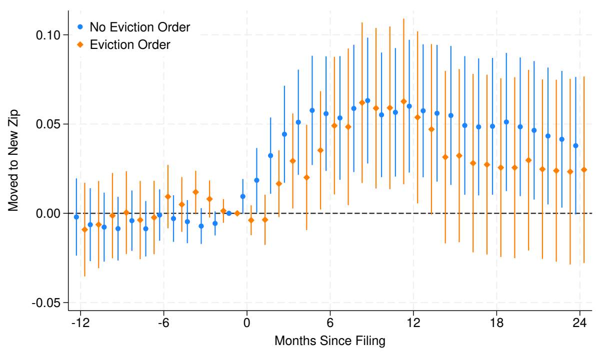
(a) Any Move



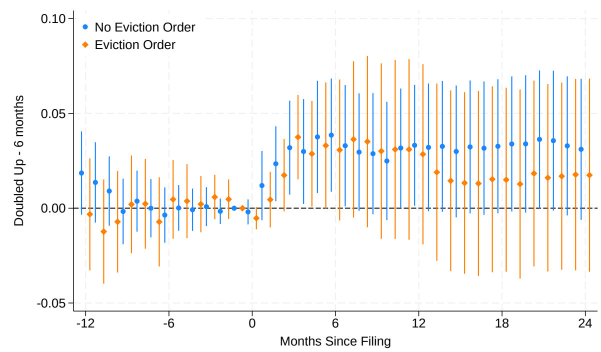
(b) Cumulative Moves



(c) Different Zipcode



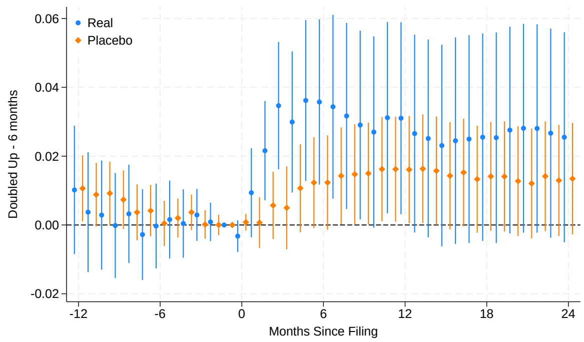
(d) Doubled Up



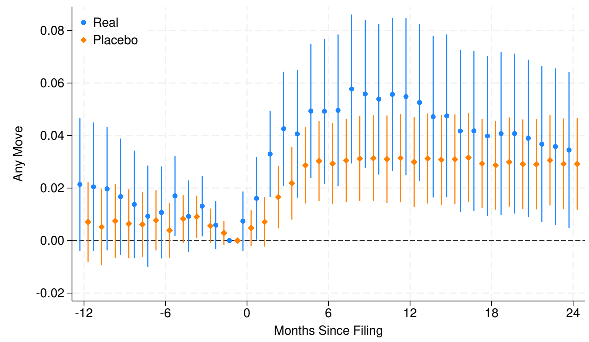
This figure plots the results from estimating Equation 1 separately for cases that resulted in an eviction order within 8 months post-filing and for cases that did not. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 8: Mobility Placebo DiD Estimates

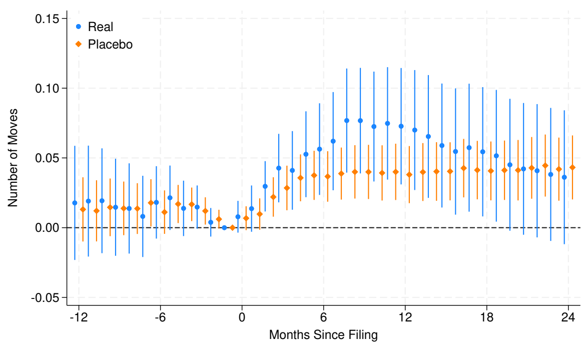
(a) Doubled Up



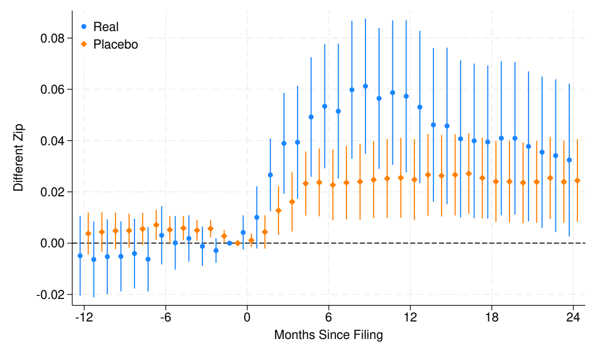
(b) Any Move



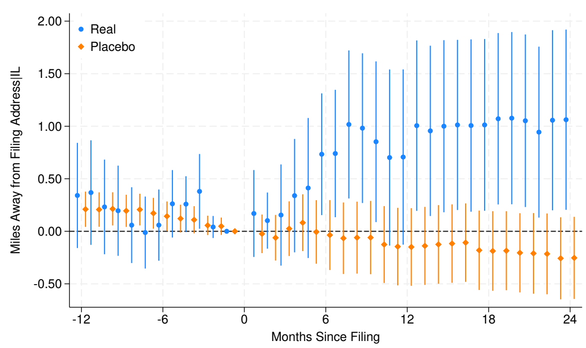
(c) Cumulative Number of Moves



(d) Different Zip Code



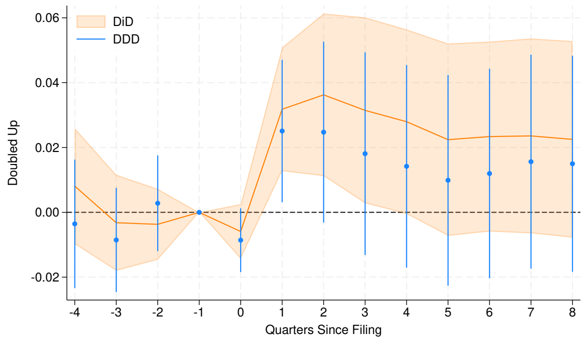
(e) Distance Away From Filing Address (within IL)



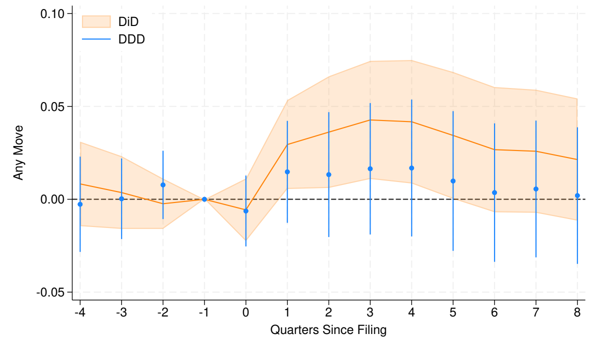
This figure plots estimates of Equation 1 separately for cases from placebo filing years and our main analysis sample. The placebo sample includes defendants with cases filed between January 1, 2016 and July 31, 2018 that could be matched to Infutor. The main analysis sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Doubled up indicates that an individual moved into a unit that overlaps with the tenure of other individuals who moved in at least three months before and do not move out within three months after. Any move corresponds to at least one move occurring within the number of months shown on the x-axis before or after the month of filing. Number of moves is the sum of moves that occurred within the number of months shown on the x-axis before or after the month of filing. Different zip code corresponds to moving to an address with a zip code that does not match the zip code from the time of filing. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 9: Mobility DDD Estimates

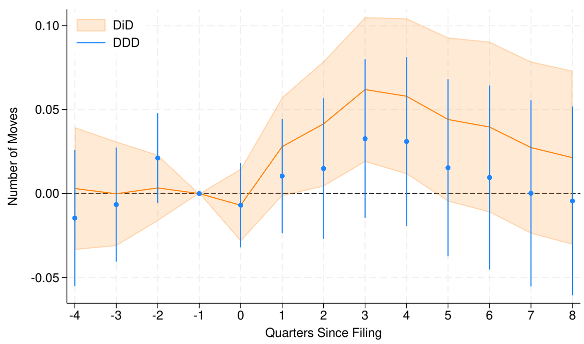
(a) Doubled Up



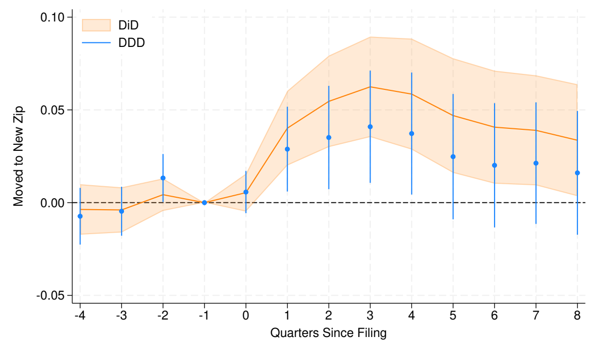
(b) Any Move



(c) Cumulative Number of Moves



(d) Different Zip Code



This figure plots DDD coefficients from estimating Equation 2 alongside DiD coefficients from estimating Equation 1. The DDD sample includes defendants with cases filed between January 1, 2016 and July 31, 2018 and between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Doubled up indicates that an individual moved into a unit that overlaps with the tenure of other individuals who moved in at least three months before and do not move out within three months after. Any move corresponds to at least one move occurring within the number of months shown on the x-axis before or after the month of filing. Number of moves is the sum of moves that occurred within the number of months shown on the x-axis before or after the month of filing. Different zip code corresponds to moving to an address with a zip code that does not match the zip code from the time of filing. The 95% confidence intervals are based on standard errors clustered at the filing date level.

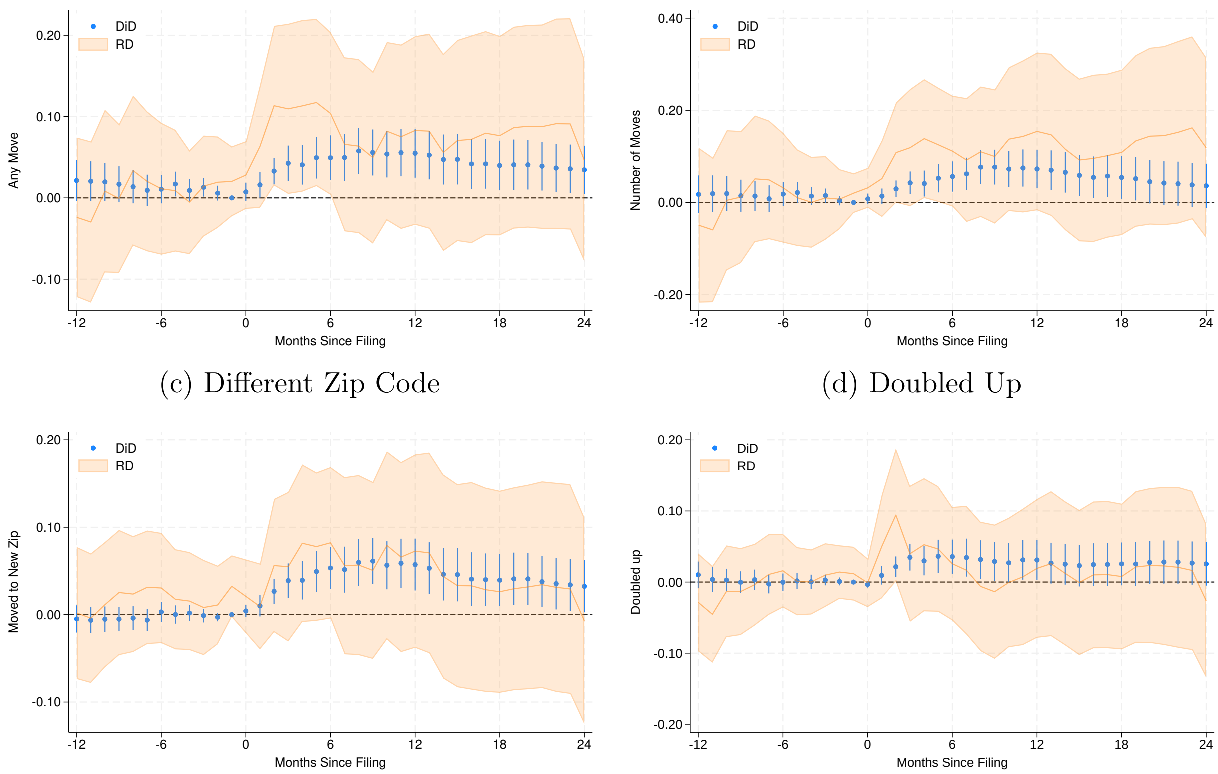
Figure 10: Mobility Estimates: RD vs DiD

(a) Any Move

(b) Cumulative Moves

(c) Different Zip Code

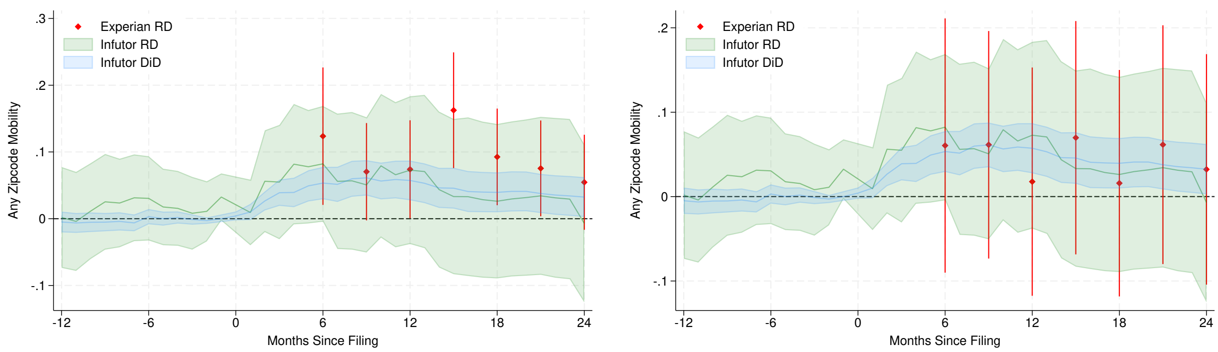
(d) Doubled Up



This figure compares DiD and RD mobility estimates. The sample used for both sets of estimates includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The DiD coefficients correspond to estimates of Equation 1. The RD estimates are derived from estimating Equation 3 separately for outcomes observed in each month relative to the eviction filing date. The RD estimates are conventional reduced-form RD coefficients from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff filing date, uses a triangular kernel weighting function, and allows separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. The 95% confidence intervals for the DiD estimates are based on standard errors clustered at the filing date level. The 95% confidence intervals for the RD estimates are derived from the conventional standard error of the reduced-form RD estimate.

Figure 11: Experian vs. Infutor Mobility Estimates

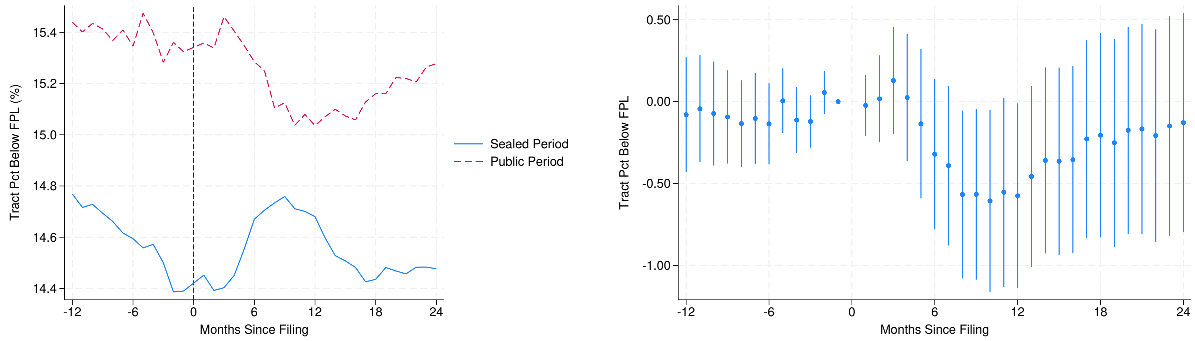
(a) Full Experian Sample (b) Infutor-Matched Sample



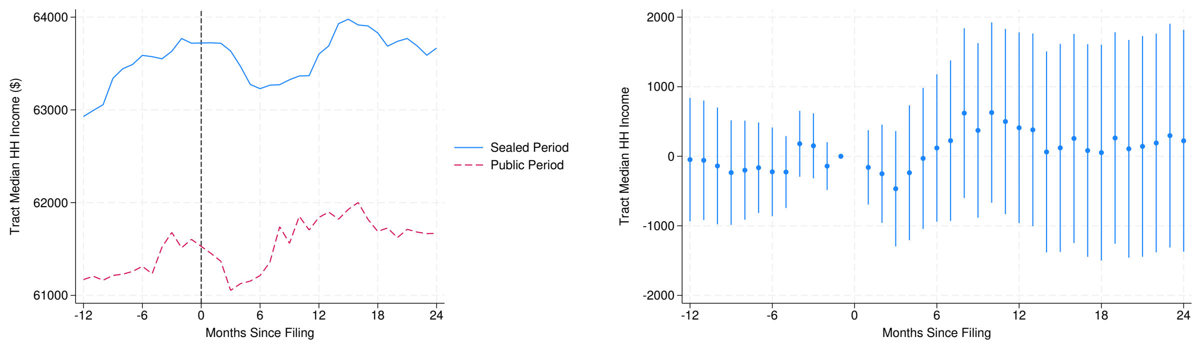
This figure compares mobility estimates based on Infutor-matched data and Experian-matched data. The sample used for both sets of estimates includes defendants with cases filed between December 1, 2021 and July 31, 2022. Zip code mobility measured in Experian is defined as residing in a different zip code than the zip code associated with the eviction filing address. Zip code mobility measured in Infutor is defined as residing in a different zip code than the time of the filing. The DiD coefficients correspond to estimates of Equation 1. The RD estimates are conventional reduced-form RD coefficients from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff filing date, uses a triangular kernel weighting function, and allows separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. The 95% confidence intervals for the DiD estimates are based on standard errors clustered at the filing date level. The 95% confidence intervals for the RD estimates are derived from the conventional standard error of the reduced-form RD estimate.

Figure 12: Neighborhood Quality Raw Trends and DiD Estimates

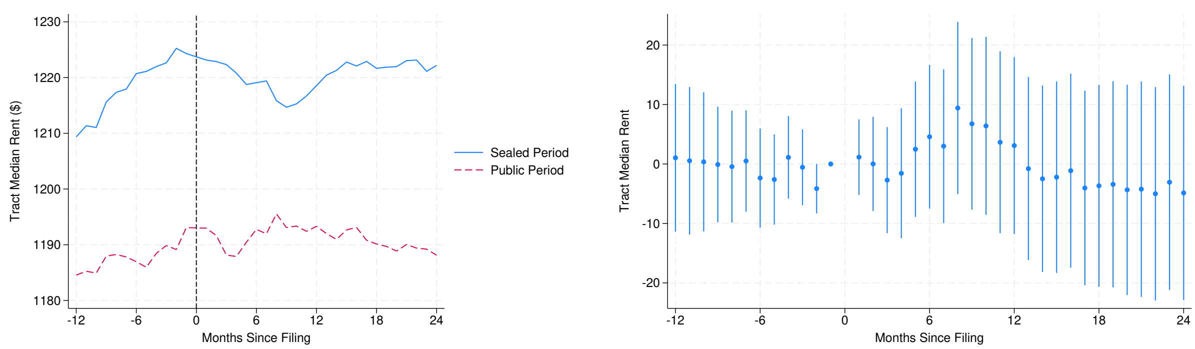
(a) Tract Pct Below FPL



(b) Tract Median HH Income



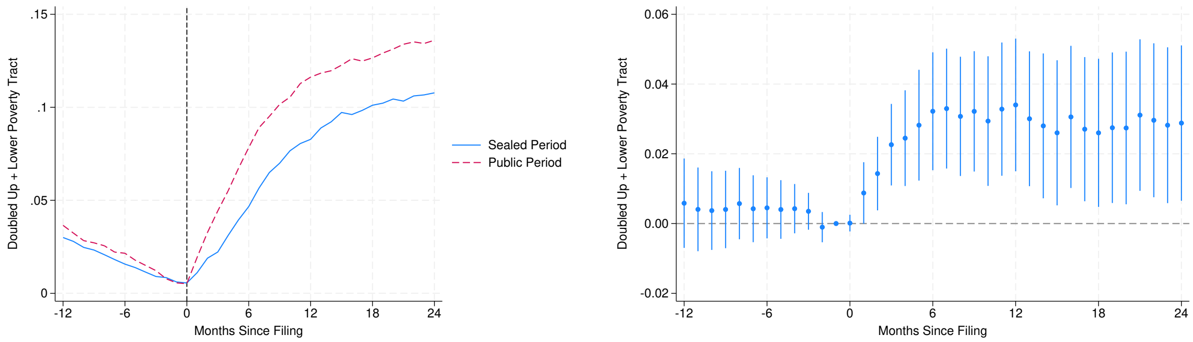
(c) Tract Median Rent



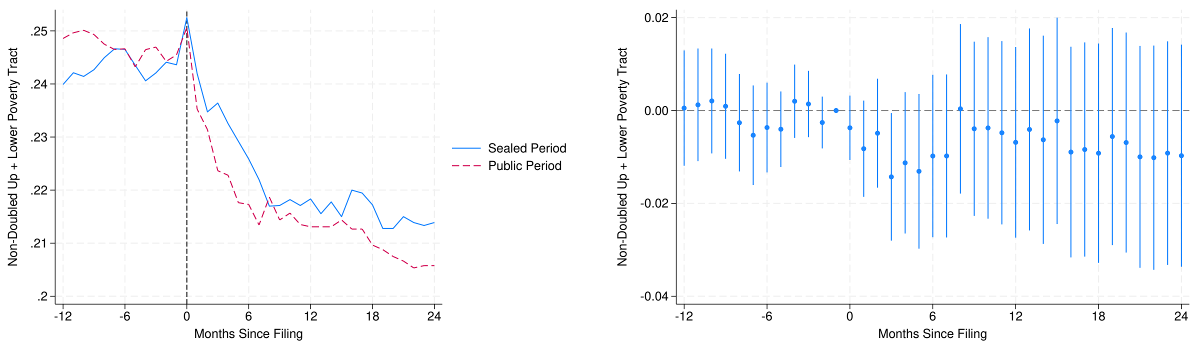
The figures on the left side plot raw means around the time of an eviction filing, separately for defendants with cases filed in the public and sealed period. The figures on the right side plot the event study estimates from the DiD model in Equation 1. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Tract characteristics are 2021 5-year ACS estimates. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 13: Doubled Up × Neighborhood Poverty DiD Results

(a) Doubled Up, Lower Poverty Tract



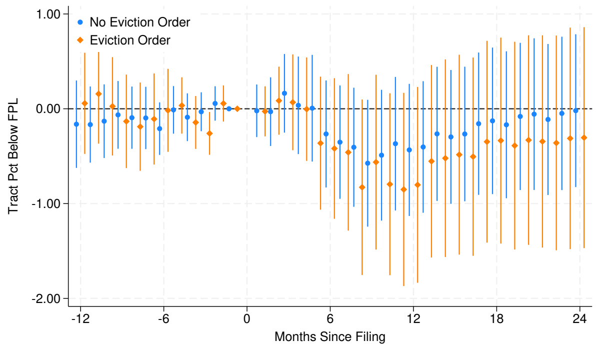
(b) Non-Doubled Up, Lower Poverty Tract



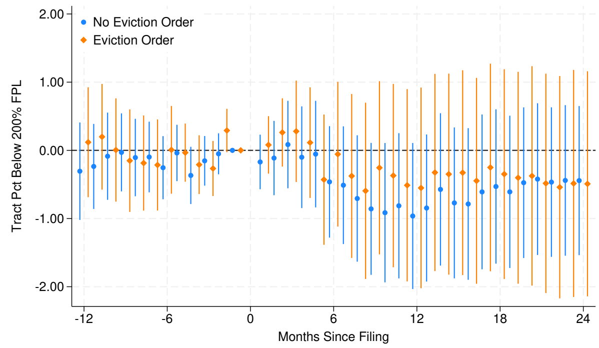
The figures on the left side plot raw means around the time of an eviction filing, separately for defendants with cases filed in the public and sealed period. The figures on the right side plot the event study estimates from Equation 1. The outcome value is an indicator that equals 1 if an individual's current tract has lower poverty rate than tract of the filing address and is living in a given doubled-up status. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Tract poverty rates are 2021 5-year ACS estimates. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 14: Neighborhood DiD Estimates by Case Outcome

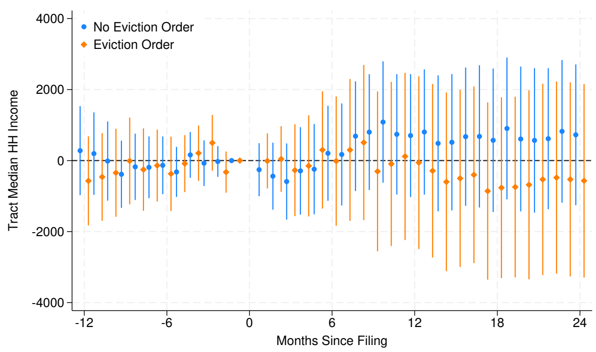
(a) Tract Pct Below FPL



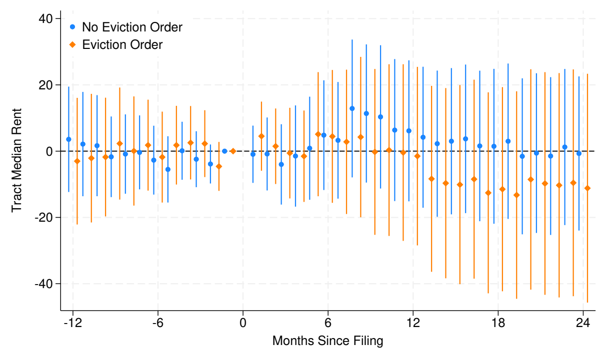
(b) Tract Pct Below 200% FPL



(c) Tract Median HH Income



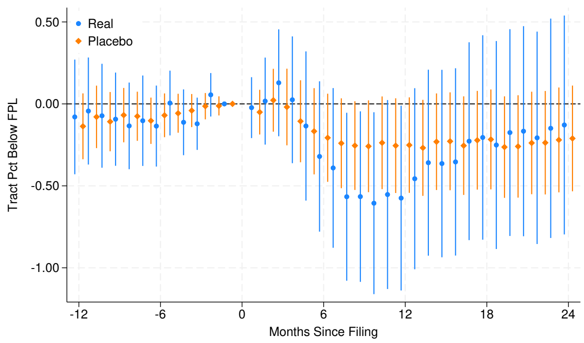
(d) Tract Median Rent



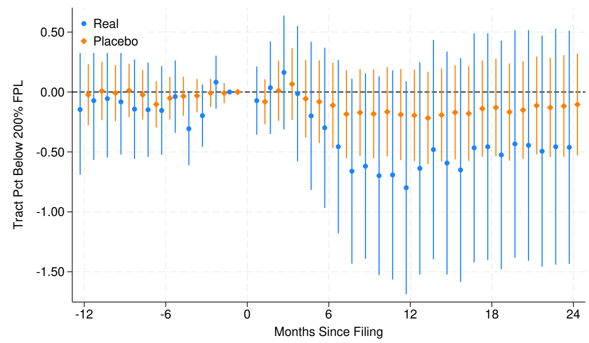
This figure plots the results from estimating Equation 1 separately for cases that resulted in an eviction order within 8 months post-filing and for cases that did not. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Standard errors are clustered at the filing date level. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 15: Neighborhood Placebo DiD Estimates

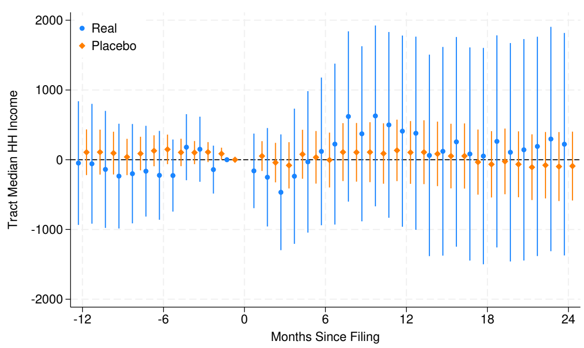
(a) Tract Pct Below FPL



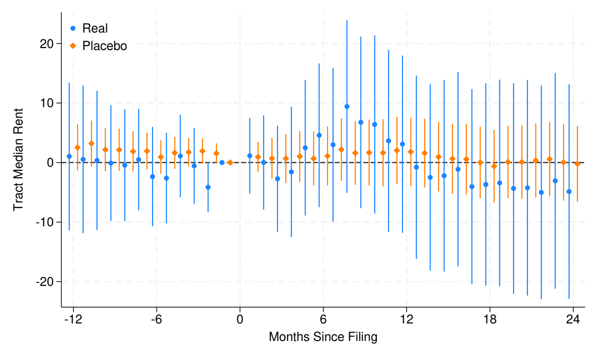
(b) Tract Pct Below 200% Below FPL



(c) Tract Median HH Income



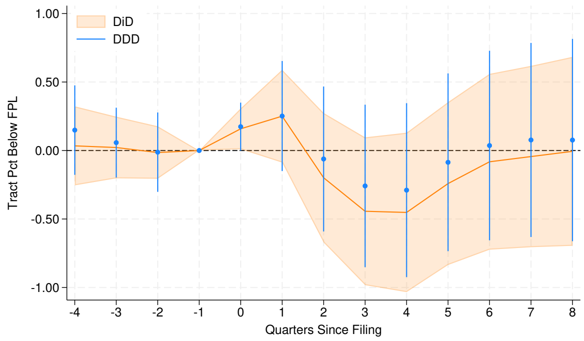
(d) Tract Median Rent



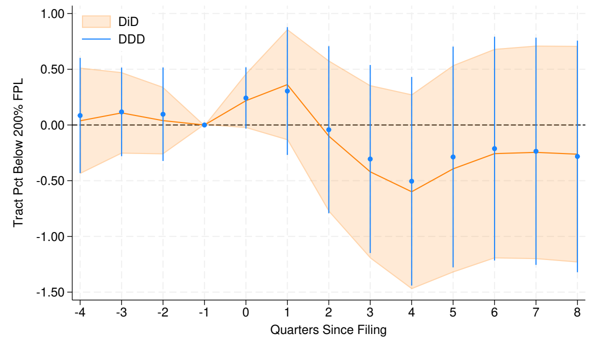
This figure plots estimates of Equation 1 separately for cases from placebo filing years and our main analysis sample. The placebo sample includes defendants with cases filed between January 1, 2016 and July 31, 2018 that could be matched to Infutor. The main analysis sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Census tract characteristics are 2021 5-year ACS estimates. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 16: Neighborhood DDD Estimates

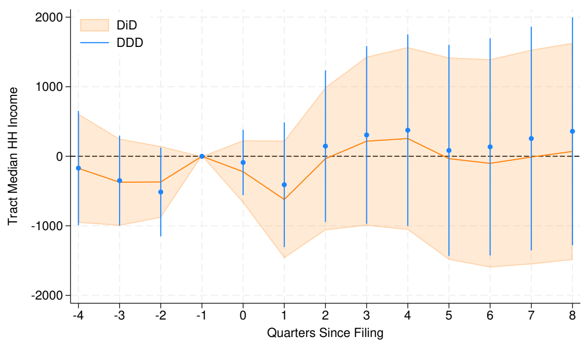
(a) Tract Pct Below FPL



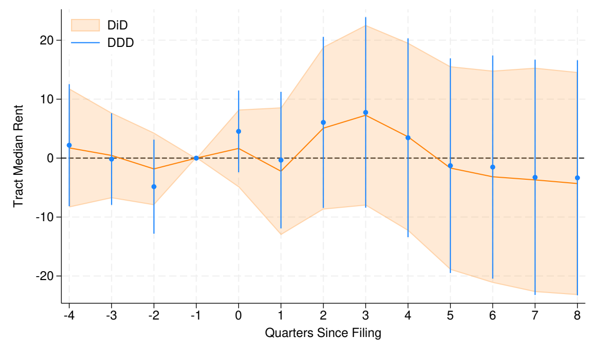
(b) Tract Pct Below 200% Below FPL



(c) Tract Median HH Income



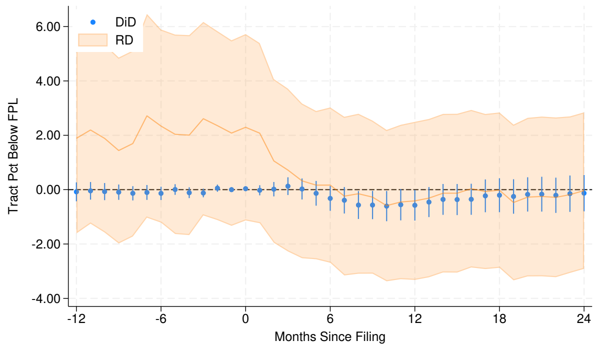
(d) Tract Median Rent



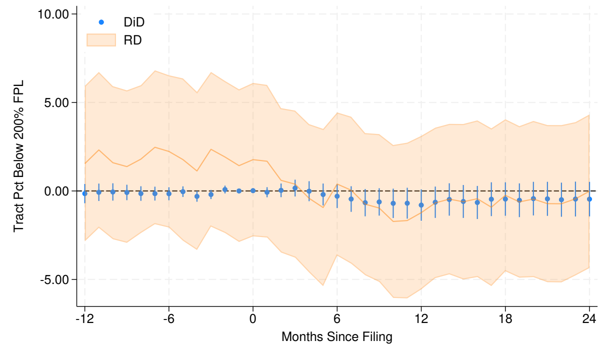
This figure plots DDD coefficients from estimating Equation 2 alongside DiD coefficients from estimating Equation 1. The DDD sample includes defendants with cases filed between January 1, 2016 and July 31, 2018 and between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Census tract characteristics are 2021 5-year ACS estimates. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 17: Neighborhood Estimates: RD vs DiD

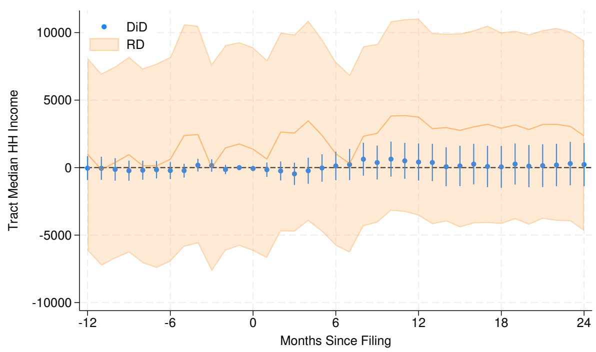
(a) Tract Pct Below FPL



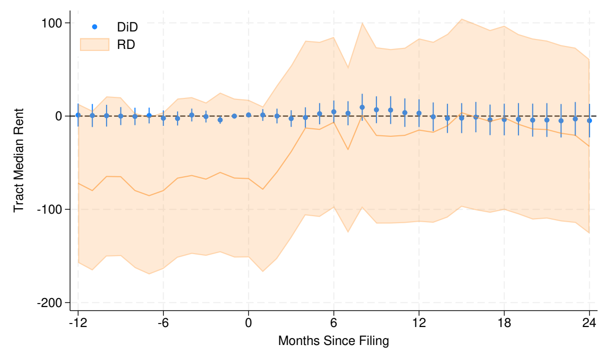
(b) Tract Pct Below 200% FPL



(c) Tract Median HH Income



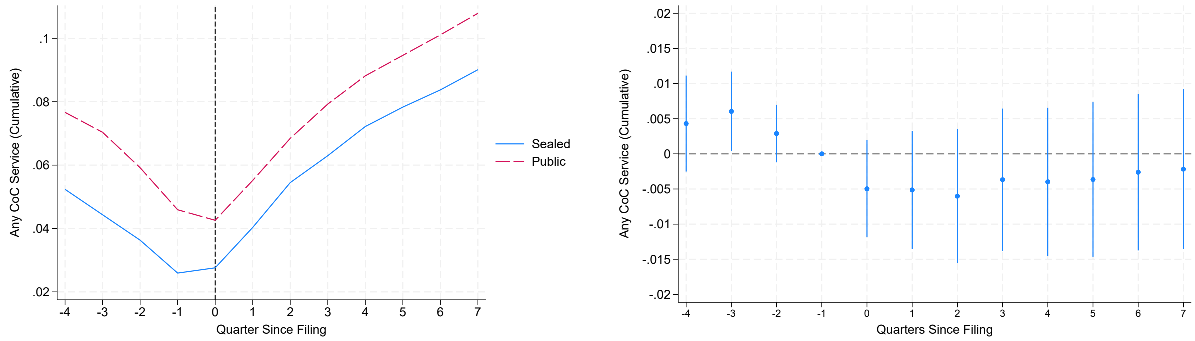
(d) Tract Median Rent



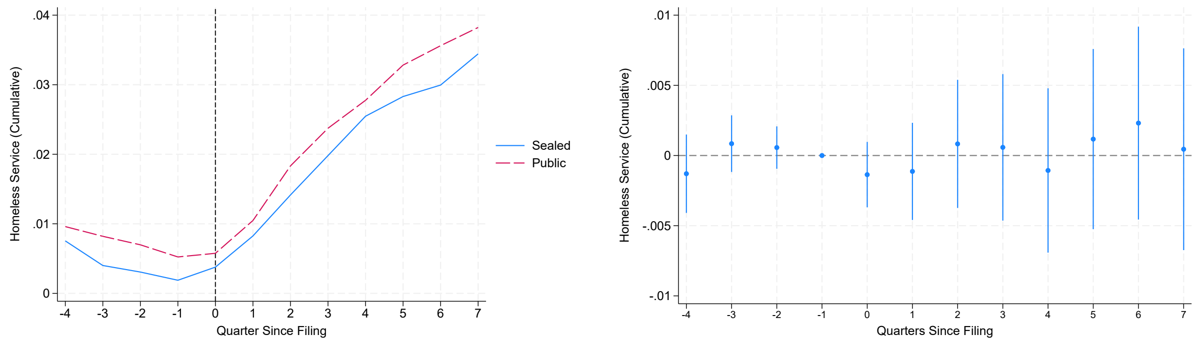
This figure compares DiD and RD neighborhood estimates. The sample used for both sets of estimates includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Census tract characteristics are 2021 5-year ACS estimates. The DiD coefficients correspond to estimates of Equation 1. The RD estimates are conventional reduced-form RD coefficients from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff filing date, uses a triangular kernel weighting function, and allows separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. The 95% confidence intervals for the DiD estimates are based on standard errors clustered at the filing date level. The 95% confidence intervals for the RD estimates are derived from the conventional standard error of the reduced-form RD estimate.

Figure 18: Homelessness Service Utilization Raw Trends and DiD Estimates

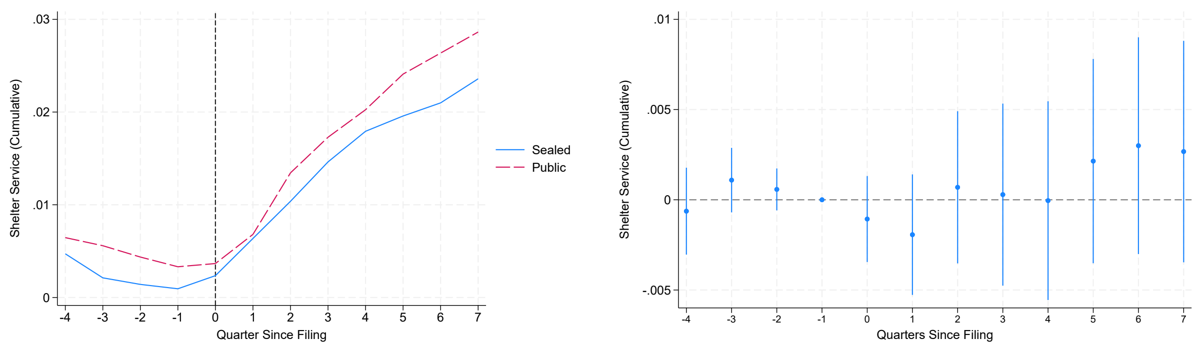
(a) Any CoC Service



(b) Any Homelessness Service

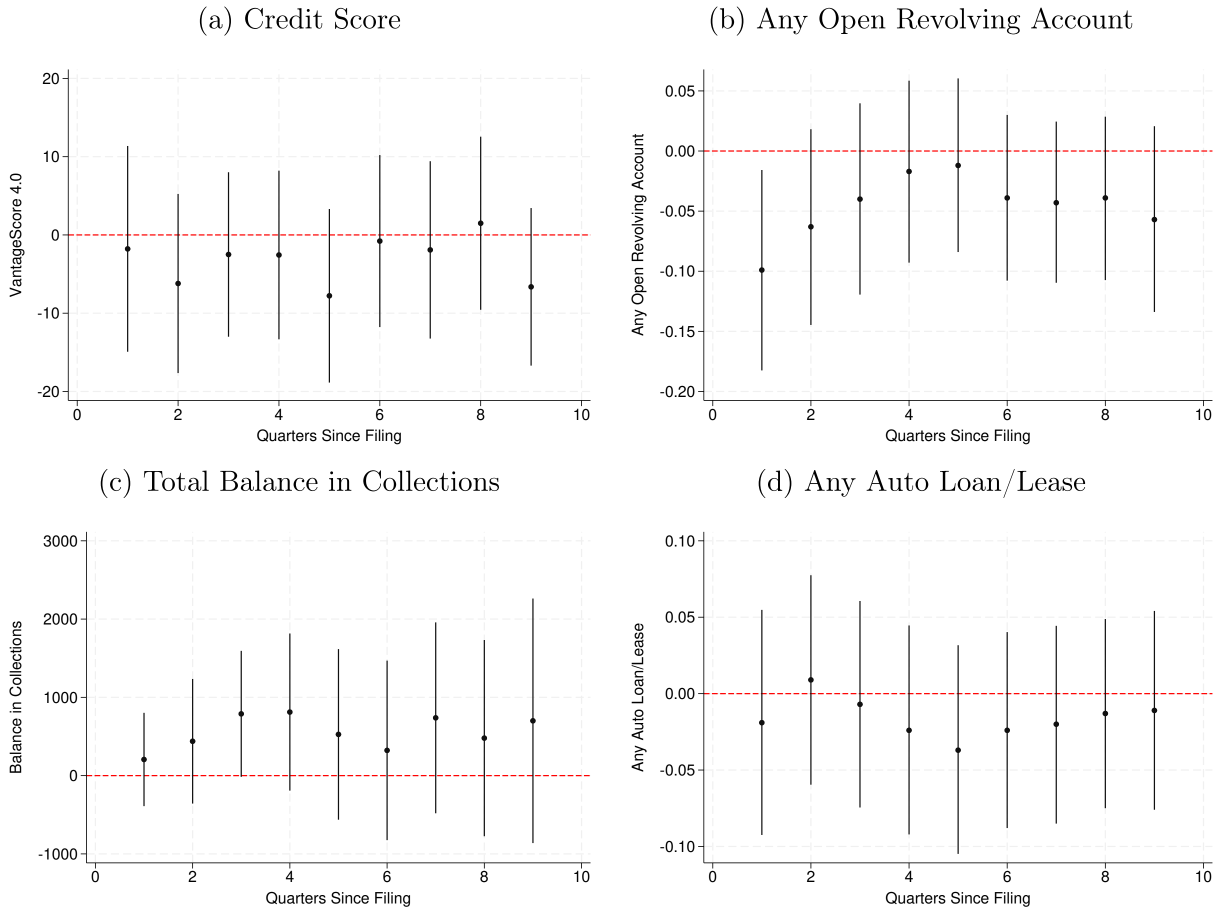


(c) Any Shelter Service



The figures on the left side plot raw means around the time of an eviction filing, separately for defendants with cases filed in the public and sealed period. The figures on the right side plot the event studies from the DiD in Equation 1. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 in Chicago. All outcomes are constructed as cumulative, capturing service engagement as recorded in the Homelessness Management Information System (HMIS) in Chicago from the point of eviction filing up to the specified period. Shelter services include entries to emergency shelters or safe haven programs. Homeless services include any shelter service, transitional housing, or street outreach. Any Continuum of Care (CoC) services includes enrollment in any homeless services listed above, housing services (such as Rapid Re-Housing, Permanent Housing, and Permanent Supportive Housing), or other services directed towards those at risk of homelessness (such as Coordinated Entry and Homelessness Prevention).The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure 19: Financial Health RD Estimates Over Time



This figure plots reduced-form RD estimates of the effect of the end of the sealing policy on financial health over time following the eviction filing. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022 that could be matched to Experian records. The outcome variable in subfigure (a) is the tenant's credit score in a given quarter relative to the eviction filing. The outcome variable in subfigure (b) is an indicator for whether the tenant had an open revolving account. The outcome variable in subfigure (c) is the tenant's total balance in collections. The outcome variable in subfigure (d) is an indicator for whether the tenant had an auto loan or lease. The error bars represent the 95% confidence intervals derived from the conventional standard error of the reduced-form RD estimate.

A Appendix: Recent eviction record sealing reforms in US cities and states

Table A1: Other Recent Changes to Eviction Record-Sealing Laws

Location	Year	Summary
Arizona	2022	The court must enter an order for sealing eviction case records if the case is dismissed pre-judgment or the court enters a judgment in favor of the tenant. See Arizona House Bill 2485 for details.
California	2017	Eviction case records are automatically and permanently sealed from the time of filing unless the landlord prevails at a trial within 60 days of filing the complaint. See California Code of Civil Procedure §1161.2 for details.
Cleveland, OH	2018	Eviction case records may be sealed upon request if the case was dismissed or the court ruled in favor of the tenant. If the landlord won an eviction judgment against the tenant, the tenant must wait at least five years to ask for the record to be sealed.
Colorado	2020	Eviction case records are sealed from the time of filing and are made public if the landlord wins possession of the property. See Colorado House Bill 20-1009 for details.
Connecticut	2024	Eviction case records are to be sealed within 30 days of an eviction case being withdrawn, a judgment of dismissal, or a judgment in favor of the tenant. See Connecticut Public Act No. 23-207 Sec. 23 for details.
Indiana	2022	Tenants can request eviction court records be sealed if case was dismissed, the case resulted in a judgment in favor of the tenant, or a judgment against the tenant was overturned or vacated. See Indiana House Enrolled Act 1214 for details.
Minnesota	2024	Eviction records are sealed until the court enters a final judgment. Eviction cases qualify for mandatory expungement if the case was related to a deed cancellation or mortgage foreclosure, the case was settled, the tenant prevailed, the case was dismissed, the parties agreed to an expungement, or three years have passed since the eviction order. The law also allows for discretionary expungement in some cases. See Minn. Stat. § 484.014 and § 504B.321, Subd. 6 for details.

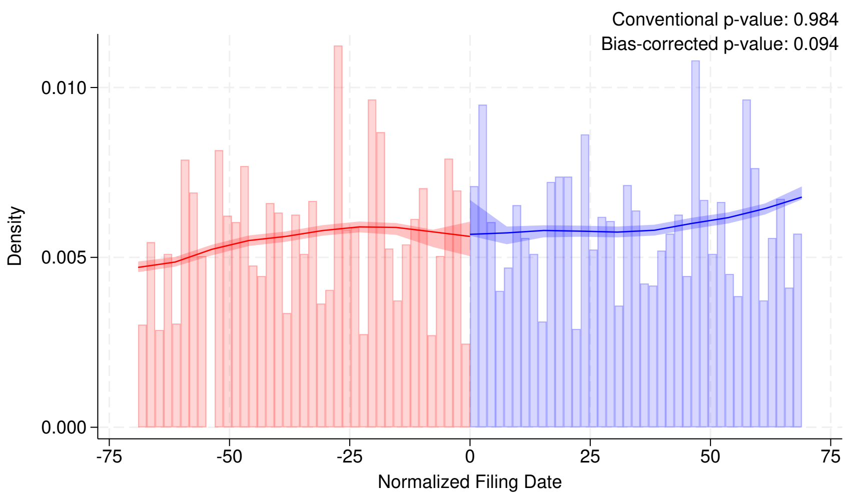
Table A1 – Continued

Location	Year	Summary
Nevada	2019, 2021	Eviction case records are automatically sealed if the case was dismissed, 10 days after the eviction was denied, or 31 days after the tenant files the tenant's affidavit if landlord does not respond. COVID-19 era cases filed over non-payment of rent are also to be automatically sealed. Cases may be sealed upon request under other conditions. See Nev. Rev. Stat. § 40.2545 for details.
New Jersey	2021	Seals court records of non-payment eviction actions initiated during the COVID-19 pandemic. See New Jersey P.L. 2021, Chapter 189 for details.
Oregon	2020	Allows for eviction records to be expunged if the case was dismissed, the tenant prevailed, the tenant completed agreements made in court, or the case is five years old and no money is owed. Pandemic-era cases can also be expunged. For details, see Oregon Senate Bill 873, 80th Leg. Assemb., 2019 Reg. Sess. (Or. 2020).
Rhode Island	2024	Eviction court records may be sealed at least 30 days after the conclusion of the case if the case was dismissed, settled, or any monetary judgment has been satisfied. See R.I. Gen. Laws § 34-18-60 for details.
Texas	2021	If a landlord and tenant enter mediation through the Texas Eviction Diversion Program, the case records are sealed. See Executive Order No. 27 by the Supreme Court of Texas for details.
Utah	2022	For eviction cases filed after July 1, 2022, case records are automatically expunged if the case was dismissed, no appeal is pending, and at least three years have passed since filing. Eviction cases filed before July 1, 2022may be expunged upon request if the case was caused by remaining after the end of the lease or non-payment of rent, and any judgment has been satisfied. See Utah State Statute Title 78B, Chapter 6, Part 8a for details.
Washington, DC	2022	Eviction case records are sealed 30 days after resolution if the case did not result in a judgement for the landlord and 3 years after resolution if the case resulted in a judgement for the landlord. See the D.C. Law 24-115 for details.

B Appendix: Robustness Checks

Figure B1: Density Test Using Business Days

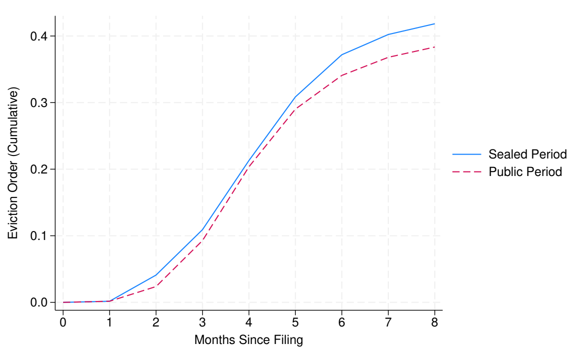
Figure B2: Business Days



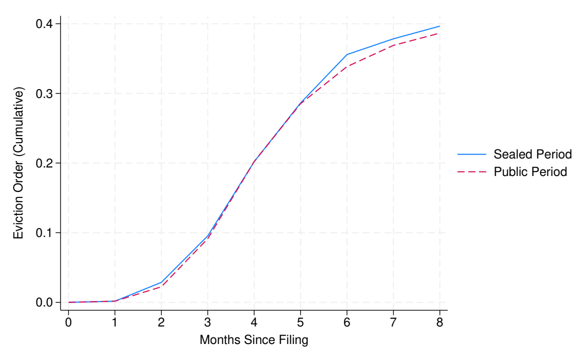
This figure plots the density of eviction filings between December 1, 2021 and July 31, 2022. The x-axis in subfigure (a) is the normalized filing date relative to April 1, 2022. The x-axis in subfigure (b) is the normalized filing date is the number of business days relative to April 1, 2022.

Figure B3: Eviction Order

(a) Full Sample



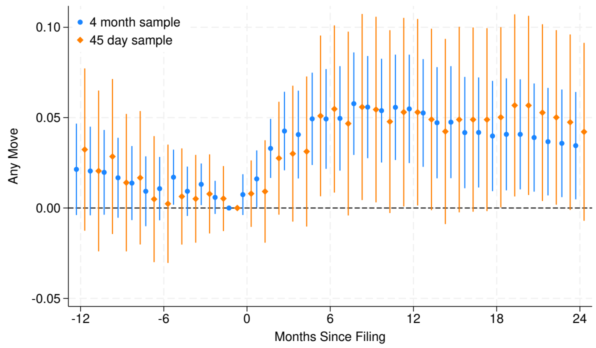
(b) Infutor-Matched Sample



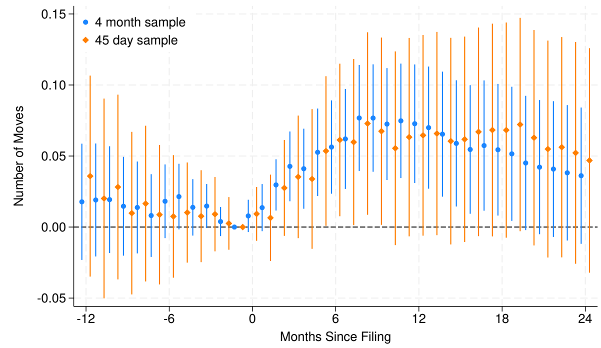
Eviction is measured using case outcomes data from the court records. Eviction is defined to include eviction orders and sheriff's eviction affidavits. Based on the time of the data pull, we only observe case outcomes that occurred within 8 months post-filing.

Figure B4: Mobility Estimates using Alternate Sample Period

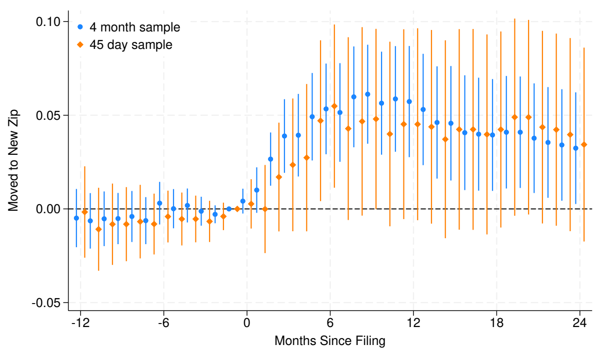
(a) Any Move



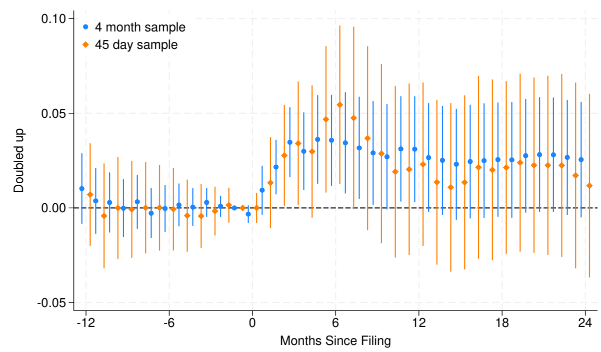
(b) Cumulative Moves



(c) Different Zip Code

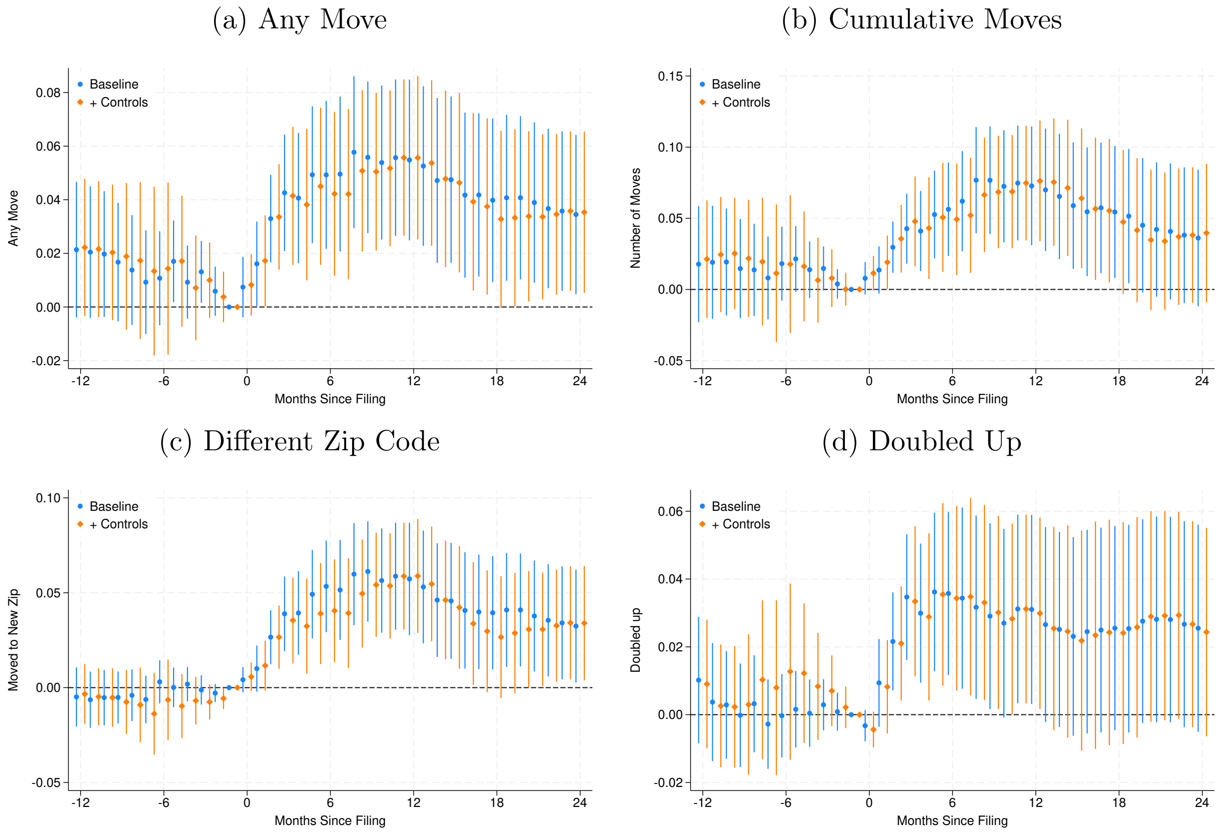


(d) Doubled Up



This figure plots the results from estimating Equation 1 using different sample periods. The estimates plotted in blue are based on the baseline sample that includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The estimates plotted in orange are based on an alternate sample that includes defendants with cases filed within 45 days before or after April 1, 2022 that could be matched to Infutor. The 95% confidence intervals are based on standard errors clustered at the filing date level.

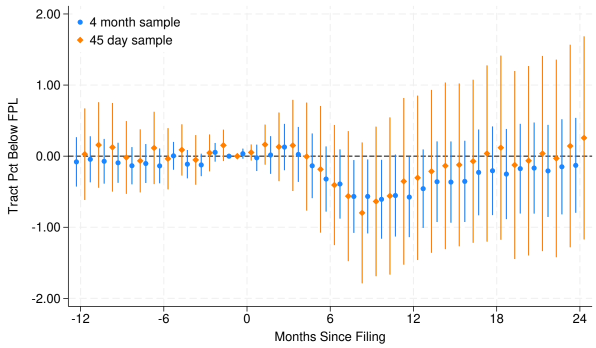
Figure B5: Mobility Estimates with Controls



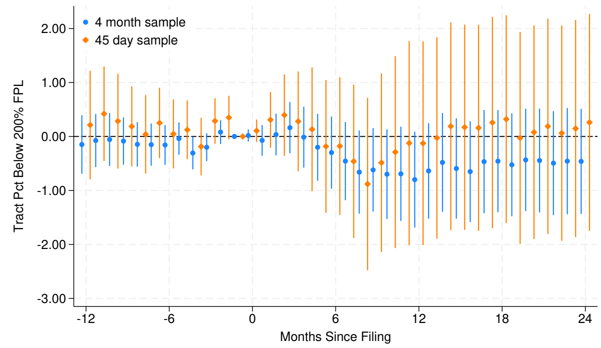
This figure plots the results from estimating Equation 1 with controls. Control variables include month fixed effects and an indicator for whether an eviction moratorium was active in Illinois. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure B6: Neighborhood Estimates using Alternate Sample Period

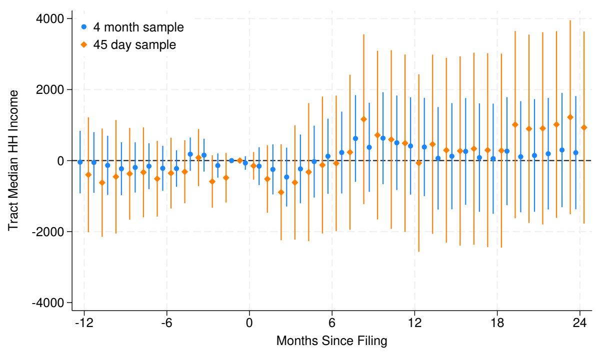
(a) Tract Pct Below FPL



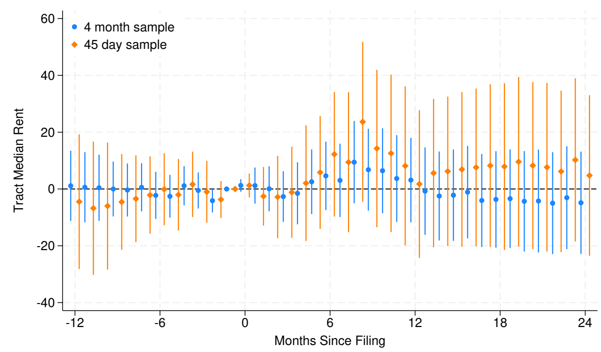
(b) Tract Pct Below 200% FPL



(c) Tract Median HH Income



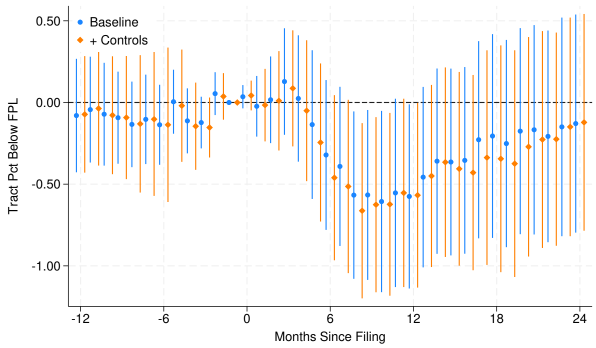
(d) Tract Median Rent



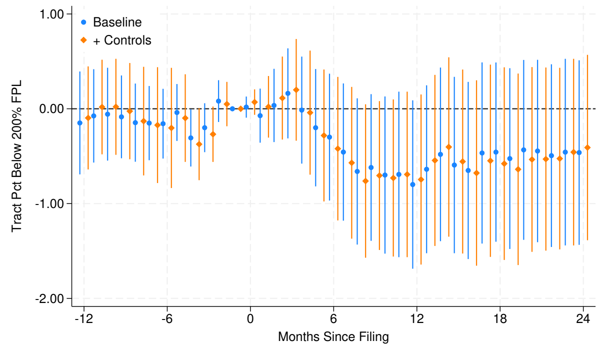
This figure plots the results from estimating Equation 1 using different sample periods. The estimates plotted in blue are based on the baseline sample that includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The estimates plotted in orange are based on an alternate sample that includes defendants with cases filed within 45 days before or after April 1, 2022 that could be matched to Infutor. Census tract characteristics are 2021 5-year ACS estimates. The 95% confidence intervals are based on standard errors clustered at the filing date level.

Figure B7: Neighborhood Estimates with Controls

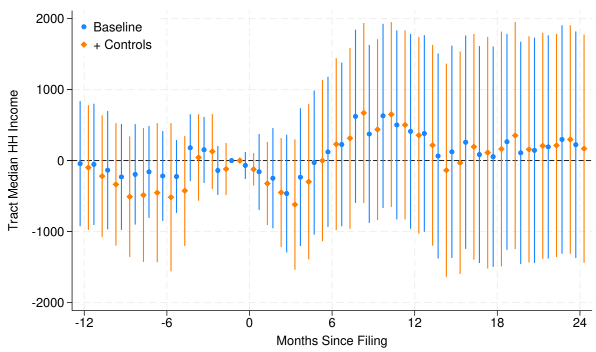
(a) Tract Pct Below FPL



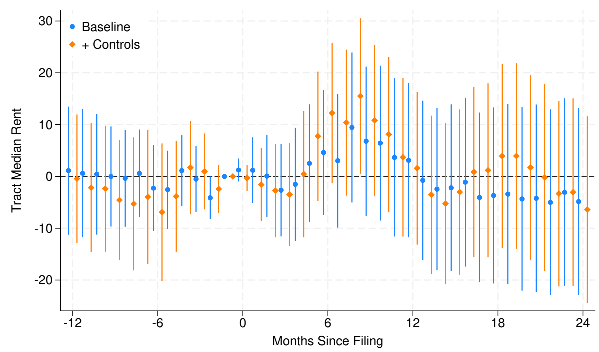
(b) Tract Pct Below 200% FPL



(c) Tract Median HH Income



(d) Tract Median Rent

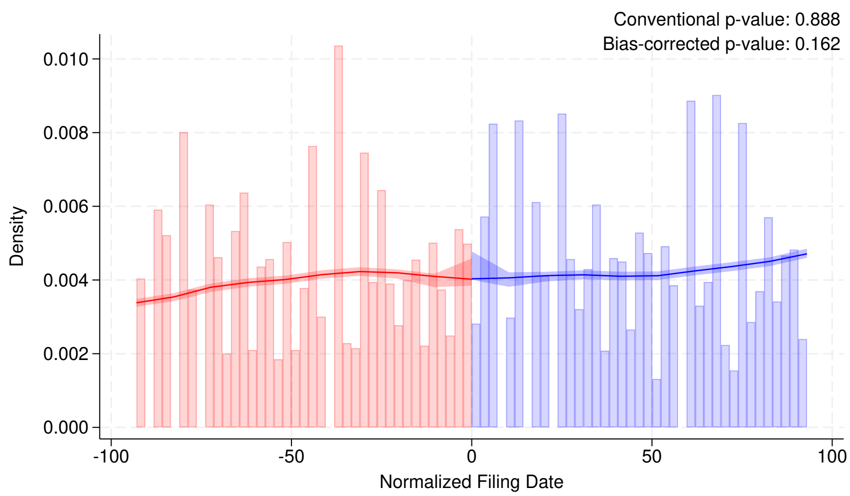


This figure plots the results from estimating Equation 1 with controls. Control variables include month fixed effects and an indicator for whether an eviction moratorium was active in Illinois. The sample includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Census tract characteristics are 2021 5-year ACS estimates. The 95% confidence intervals are based on standard errors clustered at the filing date level.

C Appendix: RDD analysis

The RD estimate identifies the causal impact of the reduced form of the public record under the assumptions of (1) no manipulation or sorting of cases around the cutoff date and (2) that the observed and unobserved characteristics of eviction filings are continuous around the cutoff date. To examine the validity of the no manipulation assumption, we test whether the density of eviction cases filed changes discontinuously at the cutoff in Figure C1. The density of eviction filings visually appears smooth around the cutoff, and we cannot reject equal case densities using the data-driven manipulation test by Cattaneo et al. (2020).[34]

Figure C1: Density Test



This figure plots the density of eviction filings between December 1, 2021 and July 31, 2022. The x-axis in subfigure (a) is the normalized filing date relative to April 1, 2022. The x-axis in subfigure (b) is the normalized filing date is the number of business days relative to April 1, 2022.

To visually evaluate the second assumption, we plot average eviction filing characteristics over filing dates in Figure 5. We find that cases filed on the first week of a month are much more likely to feature the landlord seeking only possession of the property (and not also money) and tend to be filed by plaintiffs that are bulk filers.[35] These cases filed in the first week of a month also correspond to properties in census tracts with higher poverty rates and lower median household incomes. This cyclicality may be explained by large landlords automatically filing cases in bulk near the start of each month against tenants that have not yet paid rent. Since the cutoff date in our setting occurs on the first day of April 2022, this evidence of monthly cyclicality in filing characteristics violates the second RD assumption.

[34] Figure B2 tests for discontinuous changes in filing density using only business days around the cutoff date. This approach finds marginally significant evidence of increased density to the right of the cutoff (p-value = 0.094).

[35] The first week of the month is defined as the first five business days within a given month. A plaintiff is a bulk filer around a given filing date if their filing volume in the period of 30 days on either side of the filing date is in the 95th percentile.

We test this violation formally in Table C1 by estimating reduced-form RD estimates of changes in pre-determined characteristics of tenants and cases, finding similar evidence of imbalances in the type of cases filed and census tract characteristics around April 1, 2022. Due to these imbalances, we interpret the RD estimates with caution and favor DiD estimates for the remainder of the paper.

Table C1: Balance of Case Characteristics

	Sealed Period Intercept	Public Period Intercept	Difference	Robust BC p-value
Filing Characteristics				
Case Type: Joint	0.858	0.750	-0.108	0.014
Case Type: Possession Only	0.114	0.231	0.117	0.004
Case Type: Foreclosure	0.034	0.005	-0.029	0.002
Referral to ERP	0.447	0.386	-0.061	0.170
Chicago Property	0.668	0.636	-0.032	0.111
Census Tract Characteristics				
Rental Vacancy Rate (%)	6.529	7.119	0.590	0.056
Median Rent (2022 $)	1143.597	1142.479	-1.118	0.861
Median Household Income (2022 $)	56617.639	55598.491	-1019.148	0.916
Poverty Rate (%)	15.742	18.701	2.959	0.001
Pct Black (%)	43.169	50.030	6.861	0.017
Pct Hispanic (%)	23.767	19.258	-4.509	0.034
Infutor Characteristics				
Moved Within 12 Mth Before Filing	0.238	0.214	-0.024	0.626
Moved Within 24 Mth Before Filing	0.369	0.414	0.045	0.443
Experian Characteristics				
Credit Score in 2019	548.108	548.876	0.768	0.805
Credit Score at Filing	555.252	554.366	-0.886	0.845
Any Open Rev. Account in 2019	0.468	0.348	-0.120	0.007
Any Open Rev. Account at Filing	0.429	0.423	-0.006	0.886
Balance in Collections in 2019	1642.436	1607.368	-35.068	0.669
Balance in Collections at Filing	2214.540	1840.635	-373.905	0.385
Any Auto Loan/Lease in 2019	0.310	0.274	-0.036	0.364
Any Auto Loan/Lease at Filing	0.271	0.257	-0.014	0.977

This table reports reduced form RD estimates of the change in case characteristics around the cutoff date. The "Public Period Intercept" and "Sealing Period Intercept" columns report the intercept estimates from each side of the cutoff date, the "Difference" column reports the conventional RD estimate, and the final column provides the robust bias-corrected p-value corresponding to the bias-corrected RD estimate and robust variance estimator. When generating these estimates, we impose that the relationship between the case characteristic and filing date is linear on either side of the cutoff, use a triangular kernel weighting function, and allow separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022. Census tract characteristics are 2021 5-year ACS estimates. Experian characteristics of tenants in 2019 reflect credit file attributes measured in quarters 3 and 4 of 2019.

Table C2: Residential Mobility Reduced-Form RD Estimates from Infutor

	Months since filing			
	6 months	1 year	1.5 years	2 years
	(1)	(2)	(3)	(4)
Any Move	0.104**	0.083	0.077	0.046
	(0.051)	(0.059)	(0.063)	(0.064)
Observations	1214	1165	1136	1136
Bias-corrected p-value	0.039	0.161	0.192	0.375
Bandwidths (left, right)	36, 39	31, 40	30, 37	31, 36
Sealed mean	0.168	0.259	0.314	0.368
Cumulative Moves	0.111*	0.154*	0.109	0.119
	(0.061)	(0.087)	(0.092)	(0.100)
Observations	1373	987	1119	907
Bias-corrected p-value	0.070	0.080	0.228	0.178
Bandwidths (left, right)	41, 42	28, 34	29, 35	25, 30
Sealed mean	0.202	0.304	0.395	0.438
Any New Zip	0.143**	0.192***	0.114*	0.056
	(0.062)	(0.068)	(0.069)	(0.068)
Observations	959	904	945	976
Bias-corrected p-value	0.019	0.004	0.073	0.313
Bandwidths (left, right)	31, 32	26, 33	29, 33	31, 34
Sealed mean	0.214	0.258	0.346	0.417
Doubled Up	0.026	0.019	0.008	-0.026
	(0.041)	(0.050)	(0.052)	(0.055)
Observations	1654	1248	1274	1200
Bias-corrected p-value	0.614	0.666	0.857	0.783
Bandwidths (left, right)	49, 50	34, 42	34, 42	35, 40
Sealed mean	0.151	0.192	0.220	0.275

*p< 0.1, **p< 0.05, ***p< 0.01. This table reports RD estimates of changes in residential mobility at different points in time post eviction filing. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Each reported coefficient is the conventional reduced-form RD estimate generated from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff, uses a triangular kernel weighting function, and allows separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. Conventional standard errors are reported in parentheses. The bias-corrected p-value corresponds to the bias-corrected RD estimate and robust variance estimator. The sealed mean reports the intercept of the linear relationship between the outcome and the filing date with the left side of the cutoff filing date.

Table C3: Residential Mobility Fuzzy RD Estimates from Infutor

	Months since filing			
	6 months	1 year	1.5 years	2 years
	(1)	(2)	(3)	(4)
Any Move	0.165**	0.132	0.122	0.074
	(0.082)	(0.094)	(0.100)	(0.101)
Observations	1214	1165	1136	1136
Bias-corrected p-value	0.037	0.153	0.187	0.369
Bandwidths (left, right)	35.504, 38.576	30.755, 39.735	30.326, 36.565	30.529, 36.434
Sealed mean	0.168	0.259	0.314	0.368
Cumulative Moves	0.178*	0.244*	0.173	0.188
	(0.099)	(0.139)	(0.146)	(0.159)
Observations	1373	987	1119	907
Bias-corrected p-value	0.071	0.078	0.224	0.176
Bandwidths (left, right)	40.549, 41.74	27.753, 33.799	29.38, 35.233	25.264, 30.266
Sealed mean	0.202	0.304	0.395	0.438
Any New Zip	0.232**	0.313***	0.185*	0.091
	(0.101)	(0.110)	(0.110)	(0.110)
Observations	959	904	945	976
Bias-corrected p-value	0.018	0.003	0.067	0.304
Bandwidths (left, right)	30.784, 32.107	26.396, 33.13	29.25, 32.963	30.662, 33.872
Sealed mean	0.214	0.258	0.346	0.417
Doubled Up	0.040	0.030	0.012	-0.042
	(0.064)	(0.079)	(0.083)	(0.088)
Observations	1654	1248	1274	1200
Bias-corrected p-value	0.617	0.662	0.856	0.776
Bandwidths (left, right)	48.574, 50.465	34.476, 41.881	34.297, 42.012	34.822, 39.505
Sealed mean	0.151	0.192	0.220	0.275

*p< 0.1, **p< 0.05, ***p< 0.01. This table reports RD estimates of changes in residential mobility at different points in time post eviction filing. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Each reported coefficient is the conventional RD estimate generated from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff, uses a triangular kernel weighting function, and allows separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. The first stage measure of treatment for the fuzzy RD estimation is an indicator for the case appearing in the RIS public records database. Conventional standard errors are reported in parentheses. The bias-corrected p-value corresponds to the bias-corrected RD estimate and robust variance estimator. The sealed mean reports the intercept of the linear relationship between the outcome and the filing date with the left side of the cutoff filing date.

Table C4: Financial Health Reduced-Form RD Estimates

	Time since filing			
	2 quarters	4 quarters	6 quarters	8 quarters
	(1)	(2)	(3)	(4)
Credit score (VantageScore 4.0)	-6.202	-2.561	-0.784	1.494
	(5.840)	(5.498)	(5.606)	(5.641)
Observations	2837	3193	2754	2870
Bias-corrected p-value	0.344	0.736	0.973	0.728
Bandwidths (left, right)	34, 35	39, 36	30, 31	31, 32
Sealed mean	562.732	564.230	566.508	568.449
Any open revolving account	-0.063	-0.017	-0.039	-0.039
	(0.042)	(0.039)	(0.035)	(0.035)
Observations	2606	3251	3939	3939
Bias-corrected p-value	0.127	0.679	0.292	0.253
Bandwidths (left, right)	35, 32	37, 42	44, 42	48, 39
Sealed mean	0.468	0.453	0.475	0.460
Total balance in collections	438.899	812.030	322.378	479.047
	(406.174)	(512.229)	(585.172)	(639.586)
Observations	2602	3193	3263	3529
Bias-corrected p-value	0.310	0.090	0.449	0.503
Bandwidths (left, right)	32, 32	39, 36	37, 36	36, 44
Sealed mean	1898.263	2607.402	3271.854	3753.040
Any auto loan/lease	0.009	-0.024	-0.024	-0.013
	(0.035)	(0.035)	(0.033)	(0.032)
Observations	3097	2910	3432	3549
Bias-corrected p-value	0.836	0.483	0.522	0.724
Bandwidths (left, right)	39, 37	40, 31	36, 41	42, 36
Sealed mean	0.259	0.268	0.265	0.244

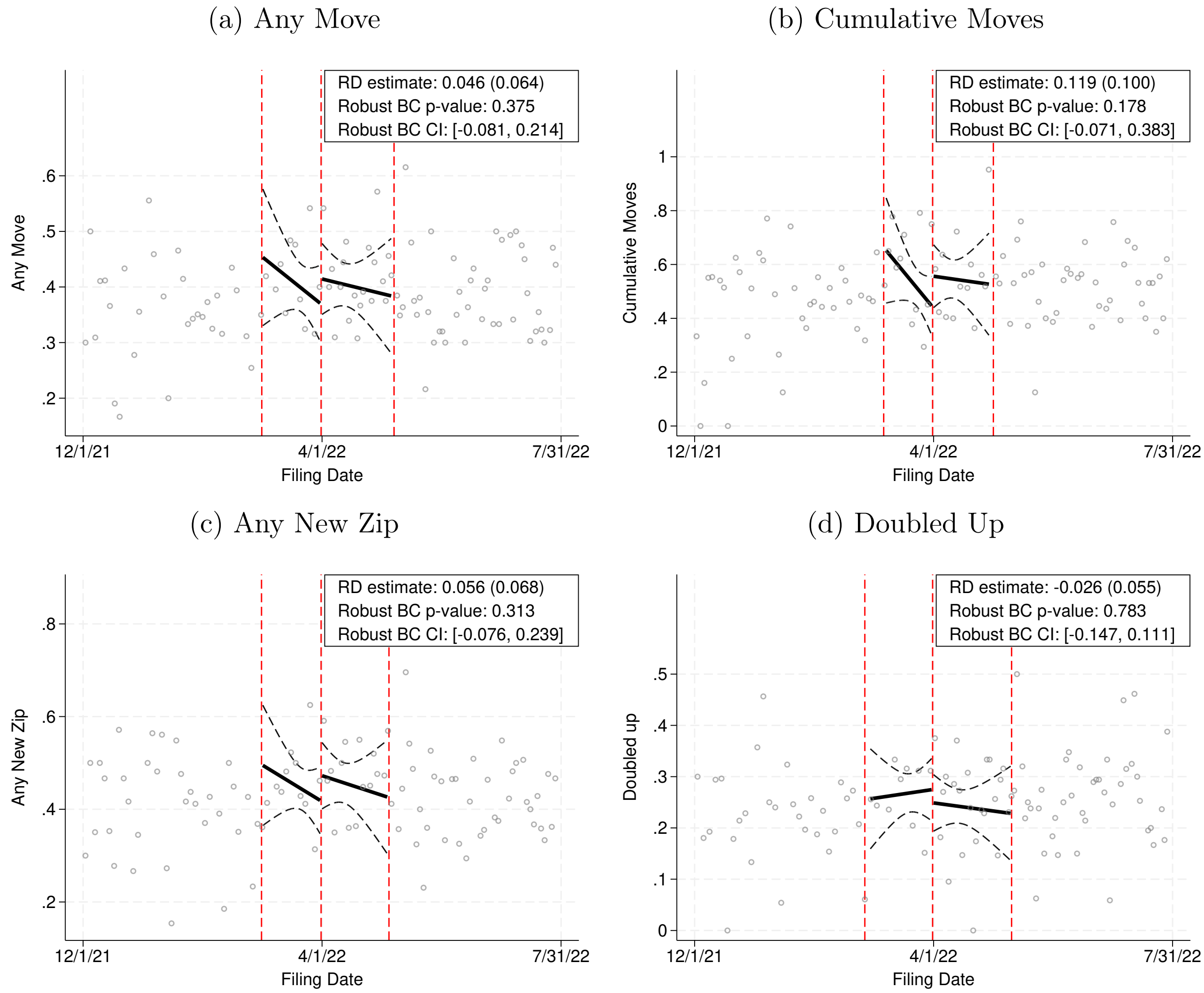
*p< 0.1, **p< 0.05, ***p< 0.01. This table reports RD estimates of changes in residential mobility at different points in time post eviction filing. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022 that could be matched to Experian. Each reported coefficient is the conventional reduced-form RD estimate generated from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff, uses a triangular kernel weighting function, and allows separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. Conventional standard errors are reported in parentheses. The bias-corrected p-value corresponds to the bias-corrected RD estimate and robust variance estimator. The sealed mean reports the intercept of the linear relationship between the outcome and the filing date with the left side of the cutoff filing date.

Table C5: Financial Health Fuzzy RD Estimates

	Time since filing			
	2 quarters	4 quarters	6 quarters	8 quarters
	(1)	(2)	(3)	(4)
Credit score (VantageScore 4.0)	-9.129	-3.773	-1.178	2.223
	(8.591)	(8.095)	(8.418)	(8.402)
Observations	2837	3193	2754	2870
Bias-corrected p-value	0.333	0.731	0.972	0.726
Bandwidths (left, right)	34, 35	39, 36	30, 31	31, 32
Sealed mean	562.732	564.230	566.508	568.449
Any open revolving account	-0.093	-0.025	-0.059	-0.059
	(0.061)	(0.057)	(0.053)	(0.052)
Observations	2606	3251	3939	3939
Bias-corrected p-value	0.120	0.675	0.287	0.246
Bandwidths (left, right)	35, 32	37, 42	44, 42	48, 39
Sealed mean	0.468	0.453	0.475	0.460
Total balance in collections	646.499	1195.686	485.069	714.700
	(599.542)	(758.727)	(881.961)	(956.158)
Observations	2602	3193	3263	3529
Bias-corrected p-value	0.305	0.087	0.444	0.499
Bandwidths (left, right)	32, 32	39, 36	37, 36	36, 44
Sealed mean	1898.263	2607.402	3271.854	3753.040
Any auto loan/lease	0.013	-0.035	-0.036	-0.020
	(0.052)	(0.051)	(0.049)	(0.047)
Observations	3097	2910	3432	3549
Bias-corrected p-value	0.833	0.479	0.515	0.719
Bandwidths (left, right)	39, 37	40, 31	36, 41	42, 36
Sealed mean	0.259	0.268	0.265	0.244

*p< 0.1, **p< 0.05, ***p< 0.01. This table reports RD estimates of changes in residential mobility at different points in time post eviction filing. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. Each reported coefficient is the conventional RD estimate generated from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff, uses a triangular kernel weighting function, and allows separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate. The first stage measure of treatment for the fuzzy RD estimation is an indicator for the case appearing in the RIS public records database. Conventional standard errors are reported in parentheses. The bias-corrected p-value corresponds to the bias-corrected RD estimate and robust variance estimator. The sealed mean reports the intercept of the linear relationship between the outcome and the filing date with the left side of the cutoff filing date. Tract-level differences in poverty, median household income, and median gross rent are differences between the tenant's current tract of residence and the tract at the time of the eviction filing.

Figure C2: Reduced-form RD Estimates of 2-Year Mobility Effects

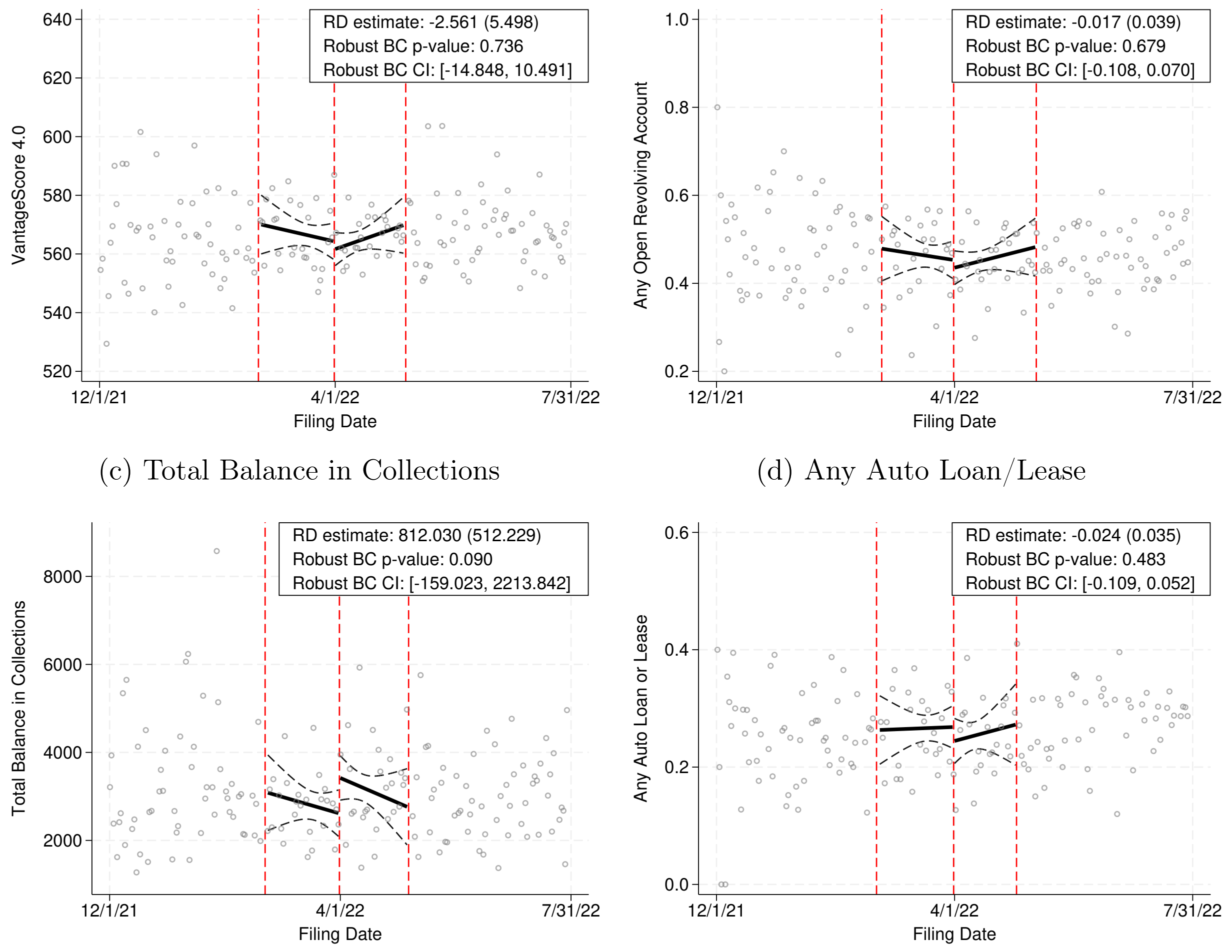


This figure plots reduced-form RD estimates of the effect of the end of the sealing policy on residential mobility measured in Infutor 24 months after the eviction filing. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The outcome variable in subfigure (a) is whether the tenant moved to a new address by a given month post-filing. The outcome variable in subfigure (b) is the cumulative number of moves by a given month post-filing. The outcome variable in subfigure (c) is whether there is a move to a new zip code by a given month post-filing, (d) doubled up is measure that indicates moving into a unit that overlaps with the tenure of other individuals who moved in at least three months before and do not move out within three months after. The error bars represent the 95% confidence intervals derived from the conventional standard error of the reduced-form RD estimator.

Figure C3: Financial Health Effects (1-Year Outcomes)

(a) Credit Score (b) Any Open Revolving Account

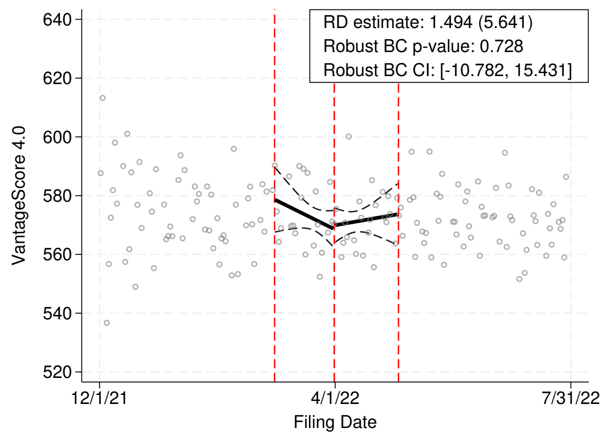
(c) Total Balance in Collections (d) Any Auto Loan/Lease



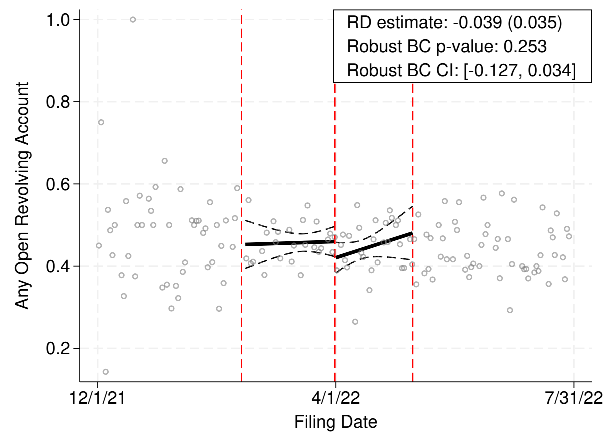
This figure plots reduced-form RD estimates of the effect of the end of the sealing policy on financial health measured one year post-filing. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022 that could be matched to Experian. The outcome variable in subfigure (a) is the tenant's credit score in a given quarter relative to the eviction filing. The outcome variable in subfigure (b) is an indicator for whether the tenant had an open revolving account. The outcome variable in subfigure (c) is the tenant's total balance in collections. The outcome variable in subfigure (d) is an indicator for whether the tenant had an auto loan or lease. The reported RD estimate and standard error are the conventional estimates. The robust bias-corrected p-value and confidence interval correspond to the bias-corrected RD estimate and the robust variance estimator. When generating these estimates, we impose that the relationship between the case characteristic and filing date is linear on either side of the cutoff, use a triangular kernel weighting function, and allow separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate.

Figure C4: Financial Health Effects (2-Year Outcomes)

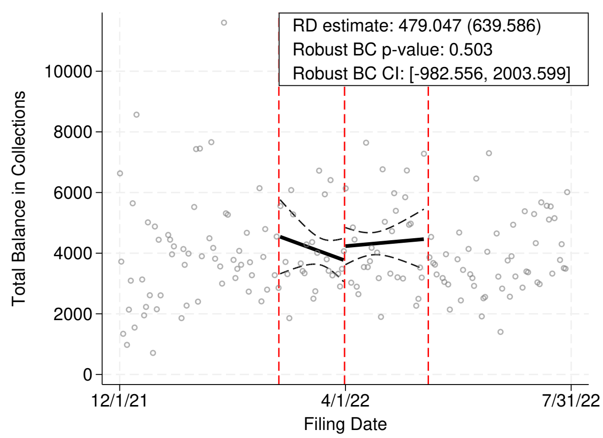
(a) Credit Score



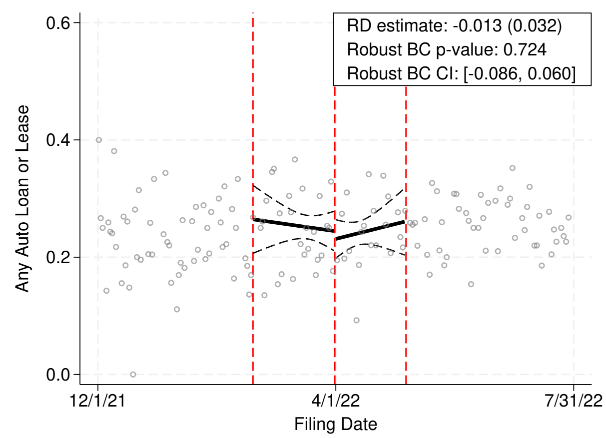
(b) Any Open Revolving Account



(c) Total Balance in Collections



(d) Any Auto Loan/Lease



This figure plots reduced-form RD estimates of the effect of the end of the sealing policy on financial health measured two years post-filing. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022 that could be matched to Experian. The outcome variable in subfigure (a) is the tenant's credit score in a given quarter relative to the eviction filing. The outcome variable in subfigure (b) is an indicator for whether the tenant had an open revolving account. The outcome variable in subfigure (c) is the tenant's total balance in collections. The outcome variable in subfigure (d) is an indicator for whether the tenant had an auto loan or lease. The reported RD estimate and standard error are the conventional estimates. The robust bias-corrected p-value and confidence interval correspond to the bias-corrected RD estimate and the robust variance estimator. When generating these estimates, we impose that the relationship between the case characteristic and filing date is linear on either side of the cutoff, use a triangular kernel weighting function, and allow separate bandwidths on each side of the cutoff that minimize the mean square error (MSE) of the RD estimate.

D Appendix: Donut RD analysis

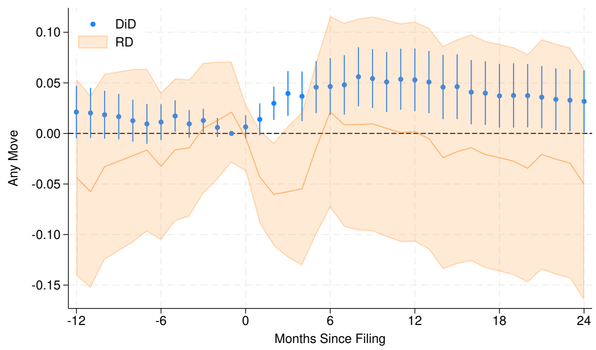
Table D1: Balance of Case Characteristics

	Sealed Period Intercept	Public Period Intercept	Difference	Robust BC p-value
Filing Characteristics				
Case Type: Joint	0.862	0.856	-0.006	0.172
Case Type: Possession Only	0.104	0.142	0.038	0.015
Case Type: Foreclosure	0.033	0.002	-0.031	$<$0.001
Referral to ERP	0.419	0.327	-0.092	0.034
Chicago Property	0.639	0.696	0.057	0.116
Census Tract Characteristics				
Rental Vacancy Rate (%)	6.711	7.956	1.245	$<$0.001
Median Rent (2022 $)	1216.395	1175.685	-40.710	0.124
Median Household Income (2022 $)	60908.615	57849.801	-3058.814	0.712
Poverty Rate (%)	15.598	16.798	1.200	0.136
Pct Black (%)	43.301	51.604	8.303	0.027
Pct Hispanic (%)	21.609	18.960	-2.649	0.507
Infutor Characteristics				
Moved Within 12 Mth Before Filing	0.223	0.179	-0.044	0.546
Moved Within 24 Mth Before Filing	0.361	0.315	-0.046	0.248
Experian Characteristics				
Credit Score in 2019	557.089	556.853	-0.236	0.535
Credit Score at Filing	560.423	568.317	7.894	0.153
Any Open Rev. Account in 2019	0.461	0.438	-0.023	0.523
Any Open Rev. Account at Filing	0.501	0.508	0.007	0.875
Balance in Collections in 2019	1581.996	1887.874	305.878	0.573
Balance in Collections at Filing	2052.213	1739.620	-312.593	0.214
Any Auto Loan/Lease in 2019	0.310	0.300	-0.010	0.794
Any Auto Loan/Lease at Filing	0.303	0.257	-0.046	0.186

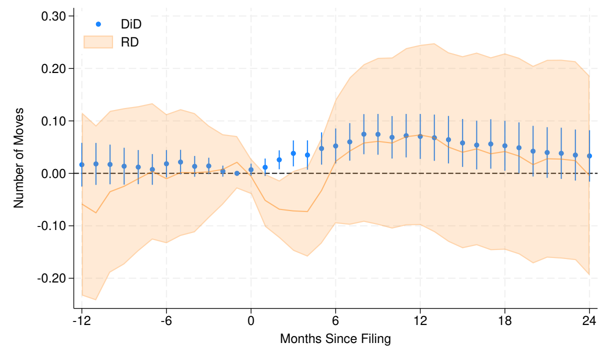
This table reports reduced form RD estimates of the change in case characteristics around the cutoff date. The "Public Period Intercept" and "Sealing Period Intercept" columns report the intercept estimates from each side of the cutoff date, the "Difference" column reports the conventional RD estimate, and the final column provides the robust bias-corrected p-value corresponding to the bias-corrected RD estimate and robust variance estimator. When generating these estimates, we impose that the relationship between the case characteristic and filing date is linear on either side of the cutoff, use a triangular kernel weighting function, and use 60 day bandwidths on each side of the cutoff. The sample includes tenants in eviction cases filed between December 1, 2021 and July 31, 2022. We exclude filings from the first five business days of April 2022. We also control for the first week of other months in the sample by including an indicator for filings occurring within the first five business days of any month. Census tract characteristics reflect 5-year estimates from the 2021 ACS. Experian characteristics of tenants in 2019 reflect credit file attributes measured in quarters 3 and 4 of 2019.

Figure D1: Mobility Estimates: Donut RD vs DiD

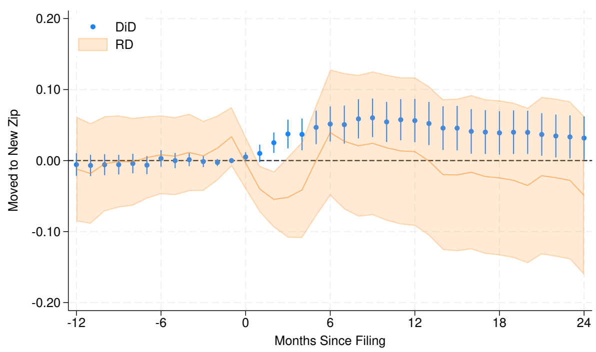
(a) Any Move



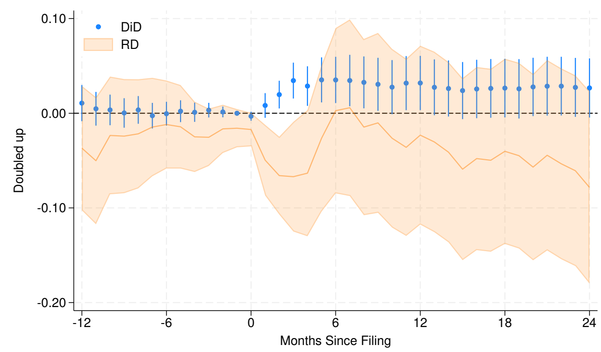
(b) Cumulative Moves



(c) Different Zip Code



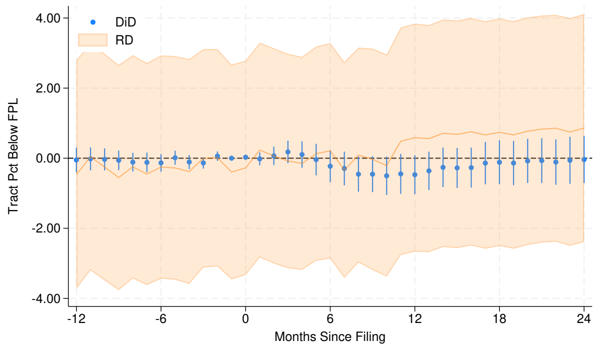
(d) Doubled Up



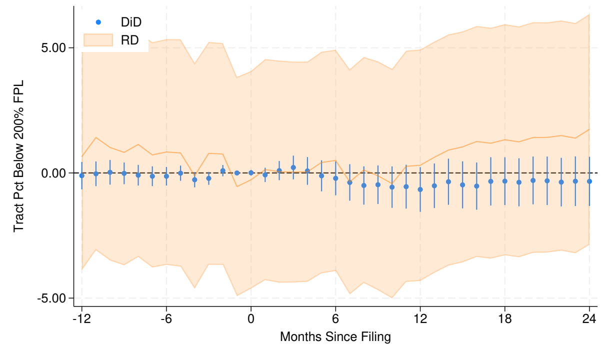
This figure compares DiD and RD mobility estimates. The sample used for both sets of estimates includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The DiD coefficients correspond to estimates of Equation 1. The RD estimates are derived from estimating Equation 3 separately for outcomes observed in each month relative to the eviction filing date. The RD estimates are conventional reduced-form RD coefficients from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff filing date, uses a triangular kernel weighting function, and uses 60 day bandwidths on each side of the cutoff. We exclude filings from the first five business days of April 2022. We also control for the first week of other months in the sample by including an indicator for filings occurring within the first five business days of any month. The 95% confidence intervals for the DiD estimates are based on standard errors clustered at the filing date level. The 95% confidence intervals for the RD estimates are derived from the conventional standard error of the reduced-form RD estimate.

Figure D2: Neighborhood Estimates: Donut RD vs DiD

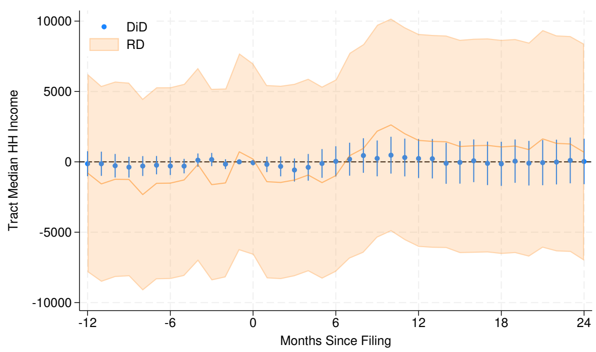
(a) Tract Pct Below FPL



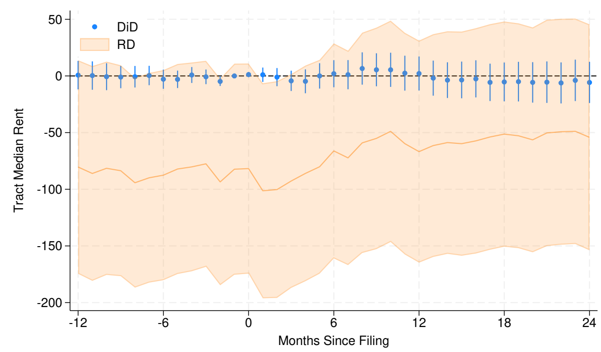
(b) Tract Pct Below 200% FPL



(c) Tract Median HH Income



(d) Tract Median Rent



This figure compares DiD and RD neighborhood estimates. The sample used for both sets of estimates includes defendants with cases filed between December 1, 2021 and July 31, 2022 that could be matched to Infutor. The DiD coefficients correspond to estimates of Equation 1. The RD estimates are conventional reduced-form RD coefficients from a specification that imposes that the relationship between the outcome and the filing date is linear on either side of the cutoff filing date, uses a triangular kernel weighting function, and use 60 day bandwidths on each side of the cutoff. We exclude filings from the first five business days of April 2022. We also control for the first week of other months in the sample by including an indicator for filings occurring within the first five business days of any month. The 95% confidence intervals for the DiD estimates are based on standard errors clustered at the filing date level. The 95% confidence intervals for the RD estimates are derived from the conventional standard error of the reduced-form RD estimate.

E Appendix: Constructing the Doubled Up Measure in Infutor

1. Identify the set of addresses before and after the eviction filing date

   (a) Start with the sample of individuals matched to Infutor

   (b) Generate an address_id to identify unique addresses at the address-city-state-zip level. Notice that, unlike the fuzzy matching between court records and Infutor, here we rely on string to identify unique addresses, not the geocoded coordinates. We do this because the geocoded coordinates do not allow-us to differentiate units withing buildings.

   (c) Using the address_id and the effective dates provided by Infutor for each address, identify the address where someone lived x months before and after filing. For pre-filing addresses, the end date has to be before or equal to the filing month and it cannot be filing address.

2. Identify defendants by address. For every month pre and post filing:

   (a) List all the addresses

   (b) For each address identify who lives there, and their respective start and end date

   (c) Create a wide file, where for every row there is an address_id, and the columns have the person and dates associated with that address (person1 and their dates, person 2 and their dates, etc). Note that the individual in each column are defendants matched to Infutor. There are n persons associated with each address id.

3. Create an extract of all the addresses for anyone who ever lived in Illinois. This is a long file: PID – address_sequence –address_id. Remove the defendants previously matched to Infutor from the file with all the addresses for anyone who has ever lived in Illinois

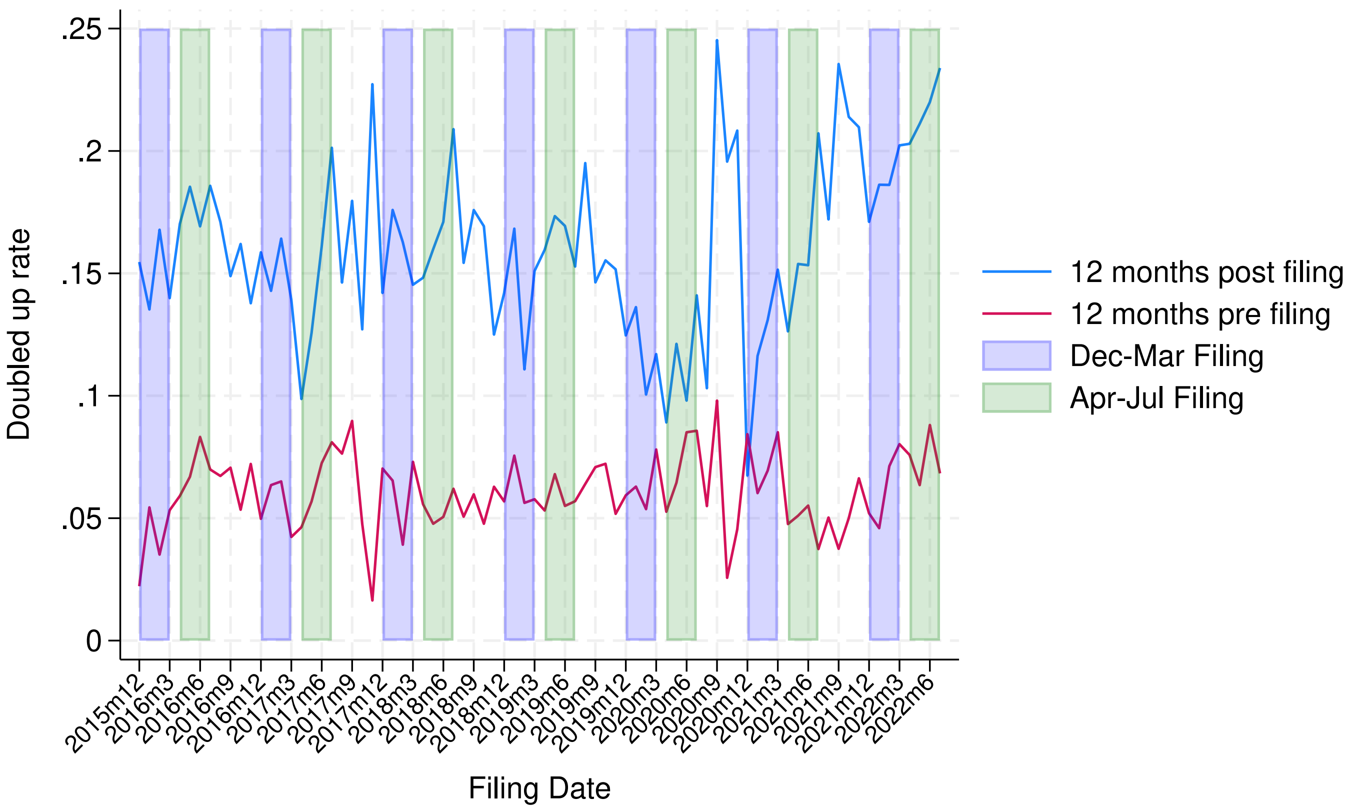
4. Identify situation where a defendant moves to an address that overlaps with the tenure of someone else.

   (a) Use the ever-lived in IL extract and identify addresses that list a unit by searching for "APT" "#" or "UNIT" in the string. Use Infutor's dwell_type variable to exlude addresses that belong to highrise building and do not list a unit number.

   (b) Use the ever-lived in IL extract to do a m:1 merge on address_id for the addresses associated to the matched sample in each pre and post month.

   (c) Create a flag for having a roommate when the nth defendant PID does not match the PID from the long-IL-address file their dates overlaps the roommate has a begin date at least 3/6/9/12 months older than the nth defendants begin date the roommate's end date is at least three months after the nth defendants begin date

Figure E1: Doubled Up Time Series



This figure plots the Infutor doubled-up rate 12 months before and 12 months after filing. The purple areas identify filings from Dec 1 through March 31st, which matches the 4-months caliper we used to identify sealed filings in 2022. The green areas identify filings from April 1 through July 31st, which matches the 4-months caliper we use to identify public filings in 2022.