The Effects of Credit Access on Mobility and Neighborhood Choice*

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Abstract

Using detailed credit data and an empirical strategy based on the removal of Chapter 7 bankruptcy flags, we study the effects of credit access on internal migration and neighborhood choice. We document that increased credit access increases zip-code migration rates but has no positive effect on a person's neighborhood quality. We interpret this result as consistent with a view that unconstrained movers equalize utility across space by their willingness to pay for housing, so constrained agents have no incentive to move to better-on-observable neighborhoods when their constraints are relaxed.

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Access to credit may affect both whether and where people choose to move. Greater credit access directly reduces moving costs by improving households' liquidity and easing financial constraints. It may also be especially helpful for moving to expensive areas because greater credit access could lead to lower mortgage interest rates and a higher chance of being approved for a large mortgage. Therefore, improved credit may be particularly valuable for accessing neighborhoods with better amenities or stronger labor markets. Yet, whether credit access actually leads households to move to areas with higher amenities depends on how they weigh the potential benefits against the higher cost of living there.

Motivated by evidence that neighborhood choice substantially affects individual outcomes—particularly children's opportunities and long-run prospects (Chetty et al., 2018; Wodtke et al., 2011; Finkelstein et al., 2021)—recent research examines whether limited credit access constrains households' ability to relocate to more desirable areas. Increased access to credit might enable moves to high-cost neighborhoods (Bergman et al., 2024), or help families smooth temporary income shocks, allowing them to remain in desirable neighborhoods that would otherwise be unaffordable (Bilal and Rossi-Hansberg, 2021; Giannone et al., 2020). These models suggest that greater credit availability should lead households toward more expensive, higher-amenity locations. However, if households place lower value on the improved amenities relative to increased expenses, they might instead move to neighborhoods that appear "worse" along observable dimensions. Related to this possibility, many families prefer neighborhoods near their original homes, even when given opportunities to relocate to higher-quality areas through government programs (Bergman et al., 2024).

Our paper tests the causal effect of credit access on whether and where people move using a large sample of borrowers who receive an exogenous shock to credit access. We motivate our estimates with a stylized framework where credit access lowers moving costs as well as the cost of living in expensive areas. In the framework, the direction of the moves is unclear and depends on how households weigh better amenities against higher costs. Then, in the data, we show that credit access raises the likelihood that households move. However, households do not move to areas with better labor markets, amenities or opportunities measured in various ways. They stay in neighborhoods that are very similar, and if anything, slightly worse along the dimensions we study.

We use an empirical strategy that has been used in the literature as an exogenous change to credit access, but which has yet to be used to study location choice: the removal of Chapter 7 bankruptcy flags from credit records 10 years after the bankruptcy was filed (Dobbie et al., 2020; Gross et al., 2020; Herkenhoff et al., 2021). The literature has argued that the removal of this flag is a quasi-random change in credit access because the removal leads to an unanticipated credit score change long after the initial shock. In other words, we can reasonably assume that someone seven to nine years post-bankruptcy is on a parallel trend to someone ten to twelve years post-bankruptcy—but for their increased credit access.

Using a panel dataset covering 2 percent of Americans who filed for bankruptcy and modern two-way fixed effects methods, we first confirm that the assumptions for identification from this strategy are valid. Specifically, we replicate the increase in credit scores and credit usage, making sure that the findings are not changed due to the more modern methodologies, and that tests for pre-trends are still satisfied. Turning to migration, we find strong evidence that people change zip codes more after the removal of the bankruptcy flag, compared to the trends they were on pre-flag-removal. In particular, the quarterly zip-migration rate increases by a bit more than 0.1 percentage points in the 10 quarters after flag removal.

Looking into the mechanisms, we think that increased access to mortgages can explain much but not all of the increase. In particular, the rate of acquiring a new mortgage increases by a comparable amount, some of which are temporally associated with a move, but also some that are not. Therefore, we suspect that credit access may also be allowing agents to afford the monetary cost of a move or to pass a credit check for a lease. Consistent with that, we do not see people moving into neighborhoods with higher single-family housing or higher rates of home-ownership. Instead, we see that the main migration results are primarily driven by people that already have a mortgage.

Having established that people move more, the next question concerns where they move. Given the literature on the potential benefits of moving for a wide variety of outcomes, including income, health, and the social mobility of children, we might expect that neighborhoods with better observable characteristics would be desirable for these agents (Card et al., 2025; Finkelstein et al., 2021; Chetty et al., 2018; Nakamura et al., 2022, etc.). In particular, the removal of the bankruptcy flag expands a person's budget set, so we would expect them to consume more of any normal good based on a pure income effect. While it is less clear if there are price effects that would change their neighborhood choice, we suspect that if there were, they would also favor the neighborhoods that are better on observables.

Interestingly, we find no evidence that they move towards neighborhoods with better observable qualities. If anything, households move to neighborhoods with slightly lower incomes, worse schools, and that have a less educated population. Correspondingly, these neighborhoods have lower house prices and rents. Compared to the results on moving, the effect on neighborhood quality is small.

We interpret our results as likely reflecting that the complier population weighs the better amenities against the higher cost of moving to a more expensive area, and on the margin prefer lower costs over better amenities. In other words, it is not the lack of access to credit that keeps people from moving to nicer neighborhoods, but rather the expensive housing in those neighborhoods.

Finally, we look at the racial composition of the neighborhoods to see if that changes post flag removal. While the average results are largely insignificant, we do find sorting post flag removal. People who were already in more-white neighborhoods move to even whiter neighborhoods, and people that were already in less-white neighborhoods move to even less-white neighborhoods. This would be consistent with our previous story that people are moving to places that match their idiosyncratic tastes—but that preferences for universally preferred qualities are already offset by housing costs.

Related Literature

Our work builds on the work of Gross et al. (2020), Dobbie et al. (2020), and Herkenhoff et al. (2021) who used this empirical strategy to study consumption, labor market outcomes, and entrepreneurship, respectively. We contribute to this literature by confirming that the empirical strategy still works with modern two-way fixed effects methods (Borusyak et al., 2021; Wooldridge, 2021, etc.).

We are also building on a literature that has used the credit panel to study migration (Howard and Shao, 2025; DeWaard et al., 2023; Davis et al., 2023). In particular, a recent literature has used the data to document mortgage lock when interest rates are rising (Fonseca and Liu, 2024; Liebersohn and Rothstein, 2025; Aladangady et al., 2024; Batzer et al., 2024). DeWaard et al. (2019) documents that the credit panel has similar properties to many other datasets such as the American Community Survey or the IRS Statistics of Income, but is much richer in that you can follow people over time.

Some recent papers have emphasized the substitution of borrowing and moving because they each are ways to deal with a negative income shock (Bilal and Rossi-Hansberg, 2021; Giannone et al., 2020). Such a channel would suggest that people might move less if they have access to credit because they can borrow instead. On the other hand, moving costs money, and access to credit might facilitate additional moves that would not otherwise be possible. Di Maggio et al. (2019) finds the forgiving student loans facilitates moving in the U.S.

The other question we address is to what extent credit access facilitates moves to "better" locations. Many models predict that access to credit will allow people to access good neighborhoods (Ortalo-Magné and Rady, 2006; Ouazad and Rancière, 2019; Bilal and Rossi-Hansberg, 2021; Dvorkin and Greaney, 2024; Greaney et al., 2025), consistent with the previously mentioned Bryan et al. (2014) which is focused on rural-urban migration. Giannone et al. (2020) estimate a structural model and present calibrated estimates for the effects of increasing credit access. They find that removing borrowing limits would lead to a decrease in equilibrium moving ratesw, and would lead to small increases in neighborhood quality for movers, relative to the counterfactual with less credit access. Bergman et al. (2024) study the effects of a multi-armed intervention focused on helping poor households move to high-opportunity neighborhoods. They find that financial support has a modest effect, as compared to the benefits of personalized search and outreach by landlords.

Evidence from developing countries has found a particularly strong effect of credit access on mobility, particularly on mobility to higher-opportunity areas. Bryan et al. (2014) show that providing small financial grants to households in rural Bangladesh significantly increases seasonal migration to urban areas. Similarly, Angelucci (2015) finds that conditional cash transfers in Mexico allowed households to overcome financial barriers to international migration. Bazzi (2017) provides related evidence from Indonesia, showing that temporary income shocks increase migration by easing immediate financial constraints. In these contexts, the ability to move for greater opportunities is substantially limited by limited credit, but whether similar findings hold in the United State are unclear.

Our work is also related to Golosov et al. (2024), who documents an increase in migration for lottery winners.[1] This paper also finds relatively limited effects on the direction of moves, except perhaps to less urban areas. Of course, given the different setting, the different size of the shock, and the different complier population, it is not *ex ante* obvious that the results would be similar across the two groups.

1 Illustrative Framework

To formalize how credit access can affect mobility, we present a simplified model of location choice. The model allows credit to affect mobility in two ways. First, increased credit access reduces moving costs in general, thereby allowing households to move to areas more aligned with their preferences. Second, the credit reduces the user cost of housing, and more so in more expensive areas. The latter channel could

[1] Larsson (2011) also documents a similar trend on a smaller sample in Sweden.

be because better credit allows households to qualify for cheaper mortgages or access apartments that they otherwise could not, for example.

The framework shows that, even though credit lowers costs more in expensive places, the average household will not necessarily move to a more expensive area if their credit improves. Lower-cost neighborhoods may still offer amenities that align better with the preferences of the average household, causing them to move to areas that align with their preferences but have similar or lower prices.

Formally, consider locations indexed by i, each of which has quality q_i. There is also a continuum of agents that are indexed by n. These agents have different idiosyncratic tastes, moving costs, and may also differ by credit access.

Credit access x affects both the cost of living and *moving costs.* c is the cost of living, which depends on a common component r (i.e. the rent or user cost of housing) and credit access x. We assume:

$$\frac{\partial c}{\partial r} > 0, \quad \frac{\partial c}{\partial x} < 0, \quad \text{and} \quad \frac{\partial^2 c}{\partial r \partial x} < 0.$$

Thus, improved credit reduces living costs and disproportionately benefits higher-rent neighborhoods.

Credit also expands households' options generally, by lowering moving costs. We assume that moving costs mc_{in} enter additively in the model, and have a specific distribution that makes our model extremely tractable.

Agents solve the following maximization problem:

$$u_n = \max_i q_i - c(r_i, x) + mc_{in}(x) + \epsilon_{in}$$

where moving costs have distribution $mc_{in} \sim \frac{\mu}{\lambda(x)} C(\lambda(x))$, where $C(\lambda)$ is an i.i.d. Cardell distribution with parameter $\lambda \in (0, 1]$ and λ is increasing in credit access. When λ is close to 1, moving costs are small. When it is far below 1, they are large. We will assume ϵ_{in} is a Gumbell distribution with shape parameter μ. A Cardell distribution is defined such that the sum $\epsilon_{in} + mc_{in}$ is distributed as a Gumbell distribution with shape parameter $\mu/\lambda(x)$.[2]

Then

$$p_j(x) = \frac{\exp\left(\frac{\lambda(x)}{\mu}(q_i - c(r_i, x))\right)}{\sum_j \exp\left(\frac{\lambda(x)}{\mu}(q_j - c(r_j, x))\right)}$$

Raising x lowers living costs, especially in high-rent areas, so it shifts choice probabilities toward places that feature higher r_i. Simultaneously, by increasing $\lambda(x)$, it amplifies sensitivity to net surplus $q_i - c(r_i, x)$, shifting choices toward locations with the greatest utility net of costs.

1.1 Equilibrium

The previous result begs the question of whether $q_i - c(r_i, x)$ is increasing or decreasing in q_i, and for which x. In other words, if credit access allows you to move to places that have higher $q_i - c(r_i, x)$ is that towards higher q_i or lower q_i places? To answer the question, we have to endogenize r_i and understand its behavior in equilibrium.

To define the equilibrium we assume housing costs $r(q_i)$ adjust endogenously, such that $c = c(r(q_i), x))$, and that each location has fixed supply. Further assume that total population of i is given by

$$\int_x p_i(x) dF(x) = 1$$

Our first result is that for the average person, $q_i - c(r(q_i), x)$ is decreasing in q_i. The intuition is that people with high x are more mobile, and so rents will have to reflect their preferences disproportionately. People with low x will not be mobile enough to change prices to

[2]The Cardell distribution is used purely to give closed-form solutions. Note that the mean of the distribution is meaningless, so to make it feel more like a moving cost, we could normalize the distribution person-by-person by subtracting the largest draw across locations, and calling that location the original location.

their preferences, but based on our assumption that credit access is more helpful for high-cost places, this means that for most people the high priced places will be even more expensive.

To see this, assume that each place is small relative to the rest of the options. The population increase from a small change in quality is:

$$\frac{d\log p_i}{dq_i} \approx \int \frac{\lambda(x)}{\mu} p_i(x) dF(x)$$

But if rents change, then population changes by

$$\frac{d\log p_i}{dr_i} \approx -\int \frac{\lambda(x)}{\mu} p_i(x) \frac{\partial c}{\partial r}(r(q_i), x) dF(x)$$

Since these have to offset to keep total population fixed,

$$\frac{dr_i}{dq_i} \approx \frac{\int_x \lambda(x) p_i(x) dF(x)}{\int_x \lambda(x) p_i(x) \frac{\partial c}{\partial r}(r(q_i), x) dF(x)}$$

This derivative defines the slope of the rent-quality gradient in equilibrium.

Now, consider the tradeoff of moving to a slightly better neighborhood. A person gets slightly higher quality, but pays higher costs, which depend on their level of credit access:

$$\frac{d(q_i - c(r(q_i), x)}{dq_i} = 1 - \frac{\partial c}{\partial r}(r(q_i, x)) \frac{dr_i}{dq_i}$$

If we take an average, weighted by the distribution of credit access living in that neighborhood, then this is given by:

$$1 - \frac{\int \frac{\partial c}{\partial r}(r(q_i), x) p_i(x) dF(x)}{\int p_i(x) dF(x)} \times \frac{dr_i}{dq_i}$$

Note that dr_i/dq_i is the inverse of the *weighted* average of the change in costs for the average person living in the city, with weights λ. Recall that λ is increasing in x.

Therefore, this term is bigger than one, since $\frac{\partial^2 c}{\partial r \partial x} < 0$. So for the average person living in i, moving to a slightly nicer place involves a cost-of-living increase that more than offsets the gain in quality.

2 Data

In this section, we give a brief overview of the data used in our analysis. More detail on the sources and descriptions of the data can be found in Appendix A.1.

Consumer Credit Panel

The primary dataset for this study is sourced from the University of California Consumer Credit Panel (hereafter UC-CCP) and is maintained by the California Policy Lab (CPL) at the University of California. The UC-CCP consists of a longitudinal and nationally representative sample of 2% of individuals with information on credit reports from a major consumer credit bureau. Individuals are randomly assigned based on the last two digits of their permanent identifier number. The complete data include snapshots at the end of each calendar quarter from the first quarter of 2004 to the second quarter of 2023. Individuals that are part of the 2% representative sample are known as primary respondents. In addition to credit records from primary respondents, the UC-CCP also includes credit records from household members and associated borrowers.[3]

The UC-CCP includes three different files. First, an individual-level database that contains individual-specific information, including credit scores, credit inquiries, and information on the location of the main residence of the individual, as well as estimates of income and demographic characteristics as determined by the credit bureau. Second, linked tradeline-level data includes payment and account information for all open and closed forms of credit

[3] Household members are defined as consumers who share an address with the primary respondent. This might be a misleading measure of household size for regions with a higher share of multi-family housing units. Associated borrowers are consumers who share a tradeline with the primary respondent.

from individuals, including mortgages and credit cards. Third, a database of public records, including bankruptcies, civil judgments, and tax liens, with information on the filing date of the public record. Each database contains individual specific identifiers that allow us to link the databases in each quarter longitudinally.

Sample

Our sample consists of a panel of individuals who are sampled as primary respondents and who have filed a Chapter 7 bankruptcy between 1994 and 2021. If a household has more than one bankruptcy, we consider only the first. Given our interest in internal migration, we keep only individuals whose information is available for consecutive periods. In addition, we consider only consumers living in one of the 50 states of the United States, the District of Columbia, and Puerto Rico. We also exclude consumers who have missing information on either credit scores or zip code of residence in any period in the sample. Because consumers who filed for bankruptcy are fundamentally different than those who didn't, our identification strategy relies on using individuals whose bankruptcy flag is not yet removed as a control group for individuals whose bankruptcy flag is just removed. Because when we look more than three years before the bankruptcy flag is removed, we are confounded by the dynamic effects of the initial bankruptcy, it is impossible to construct a control group to estimate effects more than two or three years beyond the flag removal. For that reason, we restrict our sample to a ten-quarter balanced window around the removal of the bankruptcy flag, which gives us a sample with 314,093 episodes of bankruptcy flag removal from a consumer credit history.

Mobility

The UC-CCP data contains information on the location of each consumer's main residence, including state and zip code. For each consumer, we measure mobility by creating an indicator variable that takes the value of one if the individual is located in a different zip code compared to the previous quarter. We also compute measures of 1-year migration rates by computing an indicator variable that takes the value of one if the individual reside in a different zip code compared to the same quarter in the previous year. Previous research has established that internal migration statistics are similar to estimates from other data sources, such as the American Community Survey (ACS) and Internal Revenue Service (IRS) (DeWaard et al., 2019). Some of the advantages of the UC-CCP are that it includes finer geographic coverage, is sampled at higher frequencies, and allows for multi-move tracking, making it better relative to other sources of data to study migration. A caveat of using the UC-CCP to study migration is that it only includes individuals with a credit history. Since poorer people and younger people are less likely to have any credit, they are also less likely to have a credit report and, thus, are less likely to be in our sample.

Regional Characteristics

We construct measures of demographic and economic characteristics at the local level using data from various sources. We use the 2010 Demographic Census (U.S. Census Bureau, 2010) to build measures of demographic factors such as population density, defined as population per square kilometer, and racial shares. For the latter, more specifically, we compute the share of whites, blacks, Asians, and Hispanics for each zip code. For whites, blacks, and Asians, we only consider individuals who are of this race alone and not Hispanic. We also use the 5-year estimates from the ACS 2006-2010 to compute proxies of economic characteristics at the zip code level, which includes median household income and share of homeowners. We also extracted from ACS measures of average household size, share of single-family housing units, and share of multi-family housing units. Lastly, we use data from Zillow, available online by Zillow Research (2022), to calculate measures of housing characteristics. Housing prices are sourced from the Zillow Home Value Index (ZHVI), and local market rent is based on the Zillow Observed Rent Index (ZORI). For both variables, we use the median value in 2022. The choice of 2022 is to minimize the number of missing observations in the sample.

Summary Statistics

Table 1 presents summary statistics for the bankruptcy sample. Panel (A) indicates that the average credit score of consumers in the sample is 645. On average, consumers hold 3.1 credit cards, with balances of approximately $3,400 and credit limits of $9,600 across open credit cards. Not surprisingly, the bankruptcy sample before the flag is removed has a lower credit score than the average person by about 40 points.[4]

Panel (B) reveals that around 5 percent of individuals reside in a different zip code compared to the previous quarter, while 15 percent have moved to a new zip code within the past year. Regarding state migration, 1.1 percent of consumers relocate to a new state compared to the previous quarter, and 3.6 percent are in a different state compared to the previous year. For all these migration variables, the bankruptcy sample has moving rates that are slightly higher than the full sample, consistent with Giannone et al. (2020). However, this should not be interpreted as a causal effect of credit on moving, which is what we are after.

Lastly, Panel (C) summarizes the average characteristics of the zip codes where consumers are located. On average, consumers reside in zip codes with a 67 percent white population, 69 percent homeownership rate, and median household characteristics including an annual income of approximately $56,000 and house prices around $386,000. Notably, the average consumer in the sample resides in a zip code with demographic and economic characteristics similar to those of the average individual in the UC-CCP. The differences compared to the full sample are small, although the bankruptcy sample tends to live in lower-quality, cheaper-housing places, as measured by median income, population growth, bachelor's degree, test scores, social mobility, house prices, and rents.

Please see Appendix A.1 for additional information on the source of each data series.

[4]For variables that are missing summary statistics for the full sample, this is due to the fact that we have to construct a dataset from the full underlying data. We do not construct the 1 percent sample, so the variables available in each are slightly different.

Table 1. Summary Statistics

	Bankruptcy Sample		UC-CCP Sample	
	Mean (Std. Dev.)	N	Mean (Std. Dev.)	N
Panel A. Summary of Credit				
Credit Score	645.898 (89.693)	314,093	686.284 (103.177)	87,393
Number of Credit Cards	3.183 (3.621)	314,093	-	-
Balance on Credit Cards	3,431.532 (5,856.017)	314,093	-	-
Limit on Credit Cards	9,603.143 (14,101.81)	314,093	-	-
Household Mortgages	0.723 (1.033)	314,093	-	-
Landlord Credit Checks	0.056 (0.285)	314,093	-	-
Panel B. Migration Rates				
Zip Code Migration (1-Quarter)	0.049 (0.217)	291,010	0.039 (0.193)	87,393
Zip Code Migration (1-Year)	0.152 (0.359)	279,847	0.121 (0.326)	87,393
State Migration (1-Quarter)	0.011 (0.106)	291,010	0.009 (0.097)	87,393
State Migration (1-Year)	0.036 (0.185)	279,847	0.030 (0.171)	87,393
Panel C. Regional Characteristics				
Share of White	0.667 (0.265)	301,293	0.654 (0.268)	82,778
Homeowner Share	0.691 (0.150)	301,288	0.679 (0.166)	82,771
Single-Family Share	0.774 (0.179)	301,288	0.749 (0.203)	82,771
Population Density	1,598.813 (3,342.474)	301,293	1,992.334 (4,356.918)	82,778
Median Income	56,155.05 (19,289.24)	301,285	59,240.41 (23,142.85)	82,766
Population Growth	16.413 (37.046)	288,446	15.257 (35.159)	79,624
Share with a Bachelor's Degree	0.252 (0.135)	301,288	0.288 (0.162)	82,772
3rd Grade Math Scores	3.213 (0.818)	300,788	3.296 (0.857)	82,573
Social Mobility (Chetty et al., 2018)	0.442 (0.042)	301,260	0.451 (0.046)	82,743
House Prices	386,096.2 (279.332.8)	269,459	436,920.1 (354,234.8)	80,973
Market Rent	1,954.051 (706.360)	120,399	2,042.432 (876.496)	35,314

Notes: The table reports the summary statistics from the bankruptcy sample alongside the summary for a 1% random sample of the UC-CCP data. For the bankruptcy sample, the values correspond to the quarter before the bankruptcy flag is removed. N indicates the number of consumers in the sample.

3 Empirical Strategy

Our empirical strategy uses the removal of a bankruptcy flag from a person's credit record, which happens mechanically approximately 10 years after the bankruptcy, and for our purposes serves as an exogenous shock to credit access. In this section, we give the institutional details and detail how this shock allows us to estimate a causal effect.

Institutional Background

The Fair Credit Reporting Act (FCRA), officially designated as Public Law No. 91-508 and implemented in 1970, was established to foster precision, equity, and safeguarding the privacy of personal information collected by Credit Reporting Agencies (CRAs). The FCRA governs how individuals may dispute inaccurate information and limit the use of credit reports to certain purposes. For example, a CRA cannot release a credit report for credit, insurance, or pre-employment background check purposes without the consent of the consumer, and they also must notify the consumer when an adverse action occurs on the basis of such report. The FCRA also establishes the length of time some information can appear in the credit report. For instance, civil judgments, tax liens, and accounts placed for collection can only appear for 7 years.

As outlined by the 15 U.S.C. §1681c in the FCRA, "Cases under title 11 or under the Bankruptcy Act that, from the date of entry of the order for relief or the date of adjudication, as the case may be, antedate the report by more than ten years." The rule states that bankruptcies—both Chapter 7 and Chapter 13—must be removed from credit reports after 10 years. Here, we focus on Chapter 7 bankruptcy cases over Chapter 13 for two reasons. First, more Chapter 7 bankruptcies are discharged, providing a larger sample size and greater statistical power. The higher discharge rate in Chapter 7 bankruptcy is because Chapter 13 bankruptcy involves payment plans that individuals often struggle to fulfill successfully. Second, although the FCRA sets a ten-year limit for removing the bankruptcy flag, consumer credit bureaus frequently remove Chapter 13 bankruptcies voluntarily after seven years. However, this is complicated by the FCRA's seven-year limit on various other records associated with bankruptcy filings, such as civil judgments and credit delinquencies, introducing potential confounds in the changes in consumer credit reports associated with the removal of Chapter 13 flags (Gross et al., 2020).

Gross et al. (2020) argues that the removal of the credit flag is not anticipated by agents, even though it happens at a deterministic time period. If consumers anticipated the removal, they would strategically delay applying for credit to get better terms afterward. Instead, the data show no increase or dip in credit inquiries or borrowing in the months leading up to removal, indicating that consumers do not anticipate the change.

By focusing on households that previously declared bankruptcy, we identify effects for a segment of the population that differs from the rest of the sample in various dimensions. One reason that the sample population could be different than the rest of the population is that the propensity for bankruptcy is correlated to other household characteristics. However, as discussed in Section 2, households in the sample have slightly lower credit scores than other households but are otherwise quite similar. Arguably, households with below-average credit are most interesting on both policy and theoretical grounds, since they are most affected by changes in credit standards. We look at the effects on different subsamples to see how the results vary by population.

Another reason that the study sample might be different is that bankruptcy has direct effects on the households in question. Beyond the negative effect on credit scores, bankruptcy wipes out unsecured debts. Importantly, it does not wipe out mortgages, so our sample includes both renters and owners as well as households with and without mortgages. Under U.S. law, a Chapter 7 bankruptcy discharge eliminates a debtor's personal liability on a mortgage note, but it does not eliminate the mortgage lien itself. Therefore, lenders may still foreclose on the property if payments are not made, though they cannot pursue the borrower for any unpaid balance following a foreclosure sale. Most Chapter 7 filers who wish to keep their home simply continue making payments and remain in the property (they may also "reaffirm" the debt with the lender, but this isn't necessary). If the debtor is current and has limited home equity (within the homestead exemption), they can generally keep their home through and after bankruptcy, while those with substantial equity may be required to sell as part of the estate. In practice, most Chapter 7 debtors who are homeowners retain their homes, while those who cannot afford their mortgage, or have negative equity, typically surrender the property. Pang (2024) shows that approximately 74% of Chapter 7 debtors with mortgages indicate an intent to keep their home, and of these, fewer than 10% are foreclosed upon within three years.

Identification Strategy

We use changes in credit scores around the timing that a bankruptcy flag is discharged as a source of exogenous variation in credit access to study its effects on mobility and neighborhood choice. The removal of bankruptcy flags may affect the availability of credit in two ways. First, bankruptcy flags are a direct input into credit scores. Individuals who file for bankruptcy have, on average, a lower credit score, which makes it harder to access credit in good conditions. Second, bankruptcy flags might directly reduce credit access since lenders are unlikely to extend credit to households that have experienced bankruptcy. Previous research has documented that the removal of a bankruptcy flag leads to a sudden and unanticipated increase in household access to credit (Gross et al., 2020; Dobbie et al., 2020).

To assess the effects of bankruptcy flag removals on credit access, we use the following dynamic differences-in-differences design that evaluates what happens to individuals when bankruptcy filings leave their credit records. The methodology we propose studies changes in consumer behavior around the time that their credit record improves. More specifically, we consider the following specification

$$Y_{it} = \alpha_i + \delta_t + \tau F_{it} + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome of interest, α_i and δ_t are individual and time (quarter) fixed effects, and ε_{it} is the error term. The variable F_{it} is an indicator that takes the value of one once the bankruptcy flag is removed. Our research design compares households during the time that the bankruptcy flag disappears from the credit record to households whose bankruptcy flag has not yet disappeared. The parameter of interest in equation (1) is τ. Of course, as a recent literature on two-way fixed effects has emphasized, estimating equation (1) using ordinary least squares leads to bias if there are dynamic effects of flag removal. For this reason, we will be using more modern methods that we detail below.

The identifying assumption is that the counterfactual trend if households' bankruptcy flags had not been removed is the same as the actual trend for households whose flags have not yet been removed. In essence, households whose flag is yet to be removed are a good control group for households whose flag has already been removed. As established by previous research (Gross et al., 2020), these two groups of households have very similar characteristics but are likely to have differential credit access because of the effect of the bankruptcy flag.

Our parallel trends assumption is equivalent to assuming that the time since flag removal has a linear effect on our outcomes of interest, but for the flag removal. This is because for a line to be parallel to itself shifted on the x-axis, it must be linear. This linearity assumption is more believable the shorter the window we have around the flag removal, which is why we (and the literature) focus on only a narrow time window.

Recent advances in econometric literature have raised concerns regarding the interpretation of conventional difference-in-differences estimates using two-way fixed effects (TWFE), particularly in settings involving staggered treatment rollouts and heterogeneous treatment effects (Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Wooldridge, 2021). See Roth et al. (2023) for a recent survey of the literature. These papers demonstrate that, in the presence of heterogeneous treatment effects, TWFE models estimated via ordinary least squares (OLS) may lead to biased results. Because bankruptcy flags are removed in a staggered setting, and we believe that the effects of a higher credit score are dynamic and time-varying, it is essential for us to use the newer empirical methods rather than the standard TWFE design.

We primarily use the imputation estimator proposed by Borusyak et al. (2021) that takes into account arbitrary heterogeneity and dynamics of causal effects.[5] The imputation approach runs a TWFE regression using only observations that are not yet treated to compute never-treated potential outcomes for each treated unit using the predicted value from this regression. This provides an estimate of the treatment effect for each treated unit that is aggregated to form estimates of the average treatment effect. The identification assumptions required for the Borusyak et al. (2021) imputation estimator is that the parallel trends assumption holds.

Effects Based on Pre-Treatment Characteristics

We are also interested in estimating the effects of bankruptcy flag removals for different groups of individuals based on characteristics in the quarter preceding the bankruptcy flag removal. However, because the timing of when to measure the pre-treatment characteristics is different for each cohort, and since each cohort is also being used as a control group for future cohorts, we cannot estimate these conditional effects in one step.

To address this, we adopt a sequence of common-timing differences-in-differences approach to estimate the effects conditional on pre-treatment characteristics. Specifically, let g represent the treatment group (individuals whose bankruptcy flag is removed in a given quarter), and h denote the number of periods after the treatment begins (i.e., quarters since the bankruptcy flag is removed). Using this framework, one can estimate the following extended two-way fixed effects (TWFE) model

[5]One might wonder why we choose the imputation approach of Borusyak et al. (2021) over other estimators in the literature. First, our setting does not permit the use of methods that require a never-treated group, such as Sun and Abraham (2021). Second, the data is stored on a server with limited computational power, making it impractical to use certain statistical packages, such as Callaway and Sant'Anna (2021). Our main results remain robust to the extended TWFE estimator proposed by Wooldridge (2021), as well as the sequence of common-timing differences-in-differences approach we detail in the next subsection.

$$Y_{it} = \alpha_i + \delta_t + \sum_{g \in G} \sum_{h=0}^{T-t_g} \tau_{gh} F_{it}^{gh} + \varepsilon_{it} \tag{2}$$

where F_{it}^{gh} is an indicator variable for group g at treatment horizon h, and t_g is the quarter when bankruptcy flag is removed for group g. The parameter of interest is τ_{gh}, which measures the effect of bankruptcy flag removal for group g at horizon h. These group-specific coefficients can then be aggregated to estimate the average treatment effect at each horizon h. It is noteworthy that the extended TWFE estimator in equation (2) jointly estimates all τ_{gh} parameters within a single equation.

Alternatively, the coefficient τ_{gh} can be estimated sequentially through separate regressions. For each group g at treatment horizon h, one can estimate the following specification

$$Y_{it} = \alpha_i + \delta_t + \tilde{\tau}_{gh} F_{it}^{gh} + \varepsilon_{it} \tag{3}$$

In this approach, we restrict the sample for each regression to include only the relevant observations for estimating the treatment effect for group g at horizon h. Using the estimated coefficients $\tilde{\tau}_{gh}$, the effects of bankruptcy flag removals can be calculated as a weighted average of common-timing differences-in-differences estimators

$$\tau_h = \frac{\sum_{g \in G} w_g \tilde{\tau}_{gh}}{\sum_{g \in G} w_g} \tag{4}$$

where w_g represents the number of observations used to estimate $\tilde{\tau}_{gh}$ in equation (3).[6] Notably, since the coefficients are estimated through separate regressions, this method allows for the evaluation of bankruptcy flag removal effects across different groups of individuals, categorized by their pre-treatment characteristics in the quarter prior to flag removal.

[6] Appendix A.2 shows that this approach produces very similar results to the imputation approach proposed by Borusyak et al. (2021) when we do not split by g.

4 Results

This section presents the main empirical results of the paper. First, we estimate the effects of bankruptcy flag removal on credit access. We then proceed to study the effects of improvements in credit access on mobility and location choice.

Throughout this section, we present the results using event study figures from either the Borusyak et al. (2021) imputation method or the sequence of common-timing difference-in-difference estimator outlined in the previous setting. While for the Borusyak et al. (2021) we present both the pre-trends and the dynamic effects on the same graph, we would like to note that these come from different specifications. So while the pre-trends are a test of whether the identification assumption is likely to hold, the dynamic effects are estimated assuming the pre-trends are zero, and should be interpreted as such.

4.1 The Effect of Flag Removal on Credit Access

As mentioned before, public records—bankruptcy included—are an important input in credit score models. Thus, the removal of a bankruptcy flag from credit records may improve credit access through an increase in credit scores. Panel (A) in Figure 1 confirm this hypothesis. Following the discharge of the bankruptcy flag in the credit report, there is a sudden and sharp increase in consumer's credit score, with the effects lasting for more than 2 years after the bankruptcy flag removal.

Corresponding to this increase in credit scores, we observe a large increase in credit usage across a variety of measures. In particular, we see an increase in the number of new credit cards by about 0.02 cards per quarter per capita, which corresponds to a 20 percent increase with respect to the baseline mean (Panel B). While an average increase of 0.02 new credit cards per quarter per person may not seem significant, over two years, this translates to one new card for every six individuals—a notable increase in credit availability. In addition, we also observe a sizable increase in the limit of new credit cards by about $150 dollars per quarter per capita, as depicted in Panel (D). Lastly, Panel (C) shows that consumers run an increased balance on these new credit cards of about $30 dollars per quarter per person, corresponding to a 40 percent increase in the baseline mean.[7]

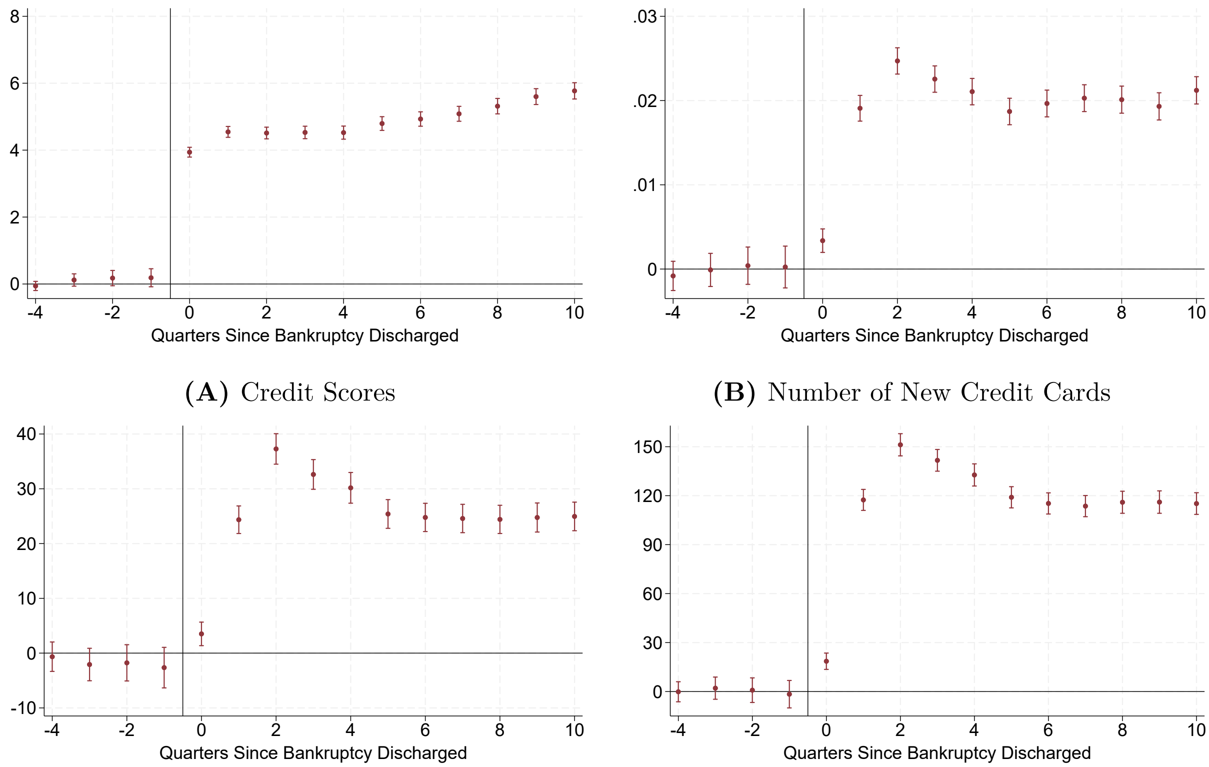
These findings closely align with those of Dobbie et al. (2020) and Gross et al. (2020), who found that the removal of the bankruptcy flag from consumer's credit report was plausibly exogenous. Although we are using the imputation approach proposed by Borusyak et al. (2021), our results similarly indicate that the pre-trends are not statistically significant. This support the parallel trends assumption between individuals who have the bankruptcy flag removed with those individuals whose flag have not yet been removed, aside from the increase in credit access.

4.2 The Effect of Credit Access on Migration

Next, we evaluate whether individuals move in response to relaxed credit conditions. Panel (A) of Figure 2 depicts an increase of approximately 0.001 moves per quarter per capita following the removal of a bankruptcy flag from credit reports. This corresponds to an increase of 2 percent in quarterly zip-code migration compared to the baseline mean of about 0.05. While 0.001 moves per person might seem small, the effect is quarterly, so over the ten quarter post-period that we look at, the effect is an additional 0.014 moves per person. One important thing to note is that the 0.014 moves can include one person moving multiple times. The pre-trends are insignificant, and after the flag removal, the effect is consistently positive and mostly statistically significant.

Since quarterly migration rates are not often measures in the literature, we also present the effects on yearly migration rates on Panel (B). Not surprisingly, these effects take several quarters to materialize because the measure at $t = 0$ includes three quarters with the

[7] These variables are coded as zero if the person does not open new credit cards in that quarter, and otherwise is the total of all new credit cards, but does not include limit changes or balance changes on existing cards.



(A) Credit Scores

(B) Number of New Credit Cards

(C) Balance on New Credit Cards

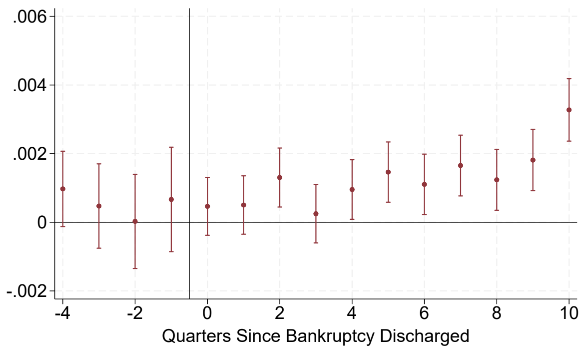
(D) Limit on New Credit Cards

Figure 1. Effect of Flag Removal on Credit Access. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel (A) shows the estimated results for credit scores. Panel (B) displays the results for the number of new credit cards, with a baseline mean of 0.105. Panel (C) depicts the results for balance on new credit cards, with baseline mean of $67.722. Lastly, Panel (D) shows the results for limits on new credit cards, with baseline mean of $235.989.

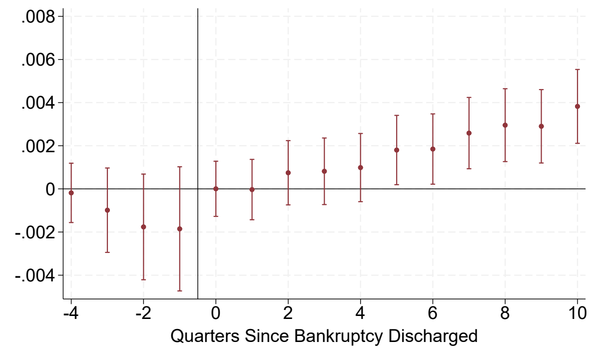
bankruptcy flag and only one quarter without. As we might expect the magnitudes are somewhat larger in magnitude than the effect on quarterly migration.

In comparison, we do not find effects on interstate migration at the quarterly or yearly levels (Panels C and D), suggesting that the effect of migration is due to an increase in local moves. Intuitively, the size of the shock is small compared to people's preferences over which region of the country to live in, but might be reasonably large compared to which specific neighborhood to choose.

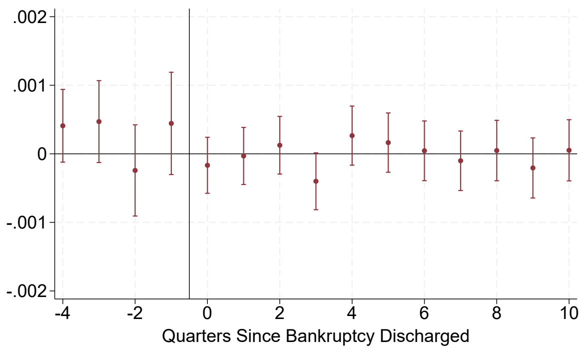
One thing to note about these results is that they indicate that a lack of credit access is not why people were moving more before flag removal. The summary statistics (Ta-



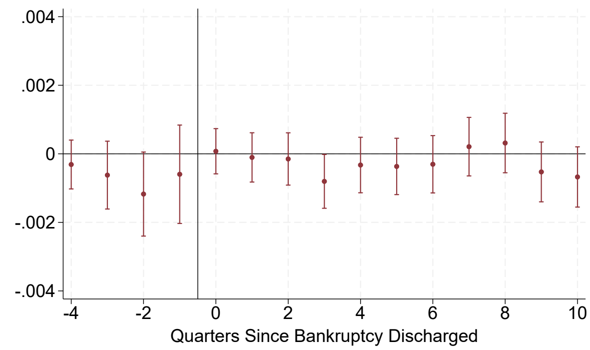
(A) 1-Quarter Zip Code Migration



(B) 1-Year Zip Code Migration



(C) 1-Quarter State Migration



(D) 1-Year State Migration

Figure 2. Effect of Flag Removal on Migration. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel (A) shows the estimated results for migration to a new zip-code compared to the previous quarter, while Panel (B) displays the results for zip-code migration compared to the same quarter in the previous year. Similarly, Panel (C) depicts the estimated results for migration to a new state compared to the previous quarter, whereas Panel (D) displays the results for state migration compared to the same quarter in the previous year. The baseline means are reported in Table 1.

ble 1) showed that people in the bankruptcy sample moved about 50 percent more than the typical person. These results suggest that result is due to selection on who is in the bankruptcy sample, not because credit access is a substitute for migration as in Bilal and Rossi-Hansberg (2021) or Giannone et al. (2020).

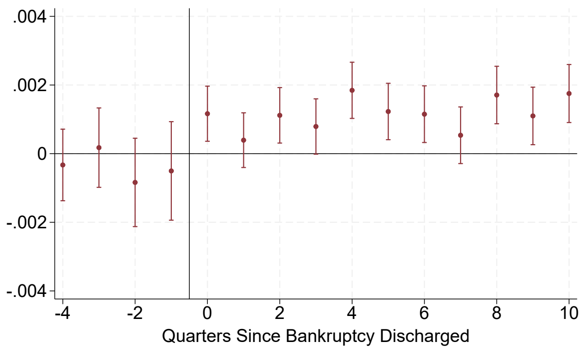
4.3 Potential Channels

In Figure 3, we try to better understand the mechanisms through which improvements in credit access can have effects on migration. In Panel (A), we observe a significant increase in the number of new mortgages for the households of individuals whose bankruptcy flag was removed. This result suggests that improved credit access makes it easier to secure a mortgage for the household, and since many mortgages are probably linked to a relocation, this could be a driver of the increased migration. Furthermore, the magnitude of this increase is comparable to the overall effect on zip-code migration rates, indicating that new mortgages could account for a sizable portion of the observed migration effect.

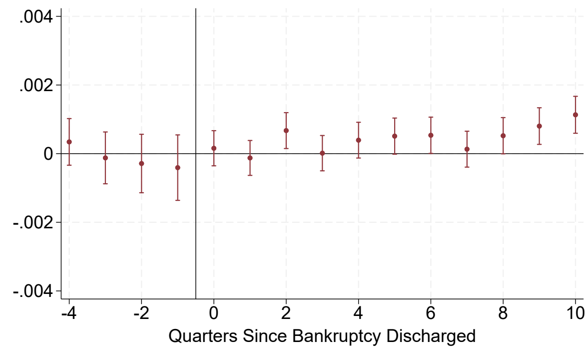
However, we can also look for the effect on new household mortgages that coincide with migration. Because the new mortgage and the migration might not happen exactly at the same time, we interact new household mortgage with a dummy for moving anytime within a 2-quarter window of the new mortgage. If all the new mortgages induced by bankruptcy flag removal involved moving, then we would expect that the effect on this new variable would be the same magnitude as the overall effect on new mortgages. Instead, what we find is a still-positive but somewhat smaller effect, suggesting that mortgage access can explain a significant increase in migration but is not the whole story. Likely, some of the mortgages induced by the flag removal are for refinancing an existing mortgage, taking out a mortgage on a house they already live in, or for migration within a zip code.

Interestingly, we also do not find that people move into neighborhoods with higher homeowner shares (Panel C) or higher shares of single-family units (Panel D), which we might expect if the story is primarily about people who are currently renting getting access to mortgage credit. While we cannot rule out tiny increases in these neighborhood characteristics, our point estimates are negative, and even small increases relative to the number of additional moves are outside of our confidence intervals. Furthermore, in Panel (E), we split our main result based on whether the person's household already had a mortgage or not.[8] Indeed, the effect on quarterly zip migration is larger for people who already had a mortgage and is not statistically different from zero for those without one. Based on these exercises, we conclude

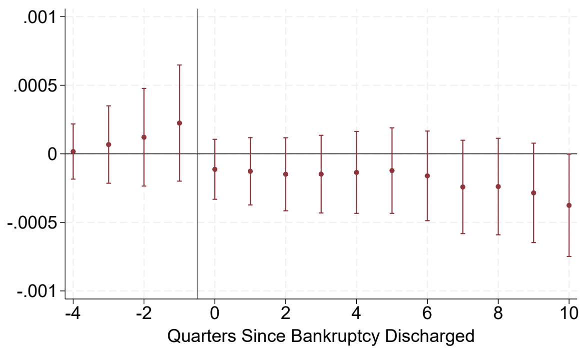
[8] In Appendix Figure A.5, we split the sample based on credit score and whether the individual (rather than the household) had a mortgage or not. We find a larger effect on higher credit score people and on individuals who already had a mortgage, consistent with our results in Panel (E).



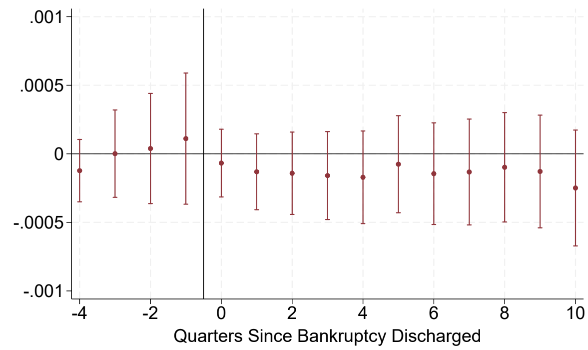
(A) New Household Mortgage



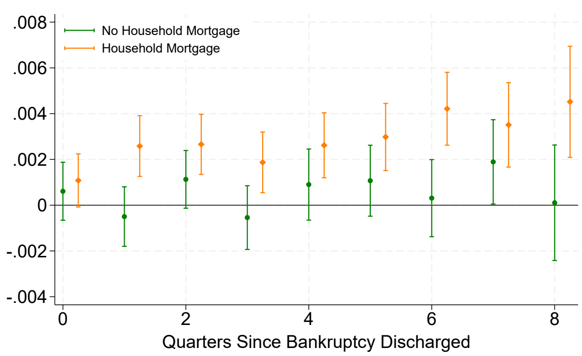
(B) New Household Mortgage × Migration



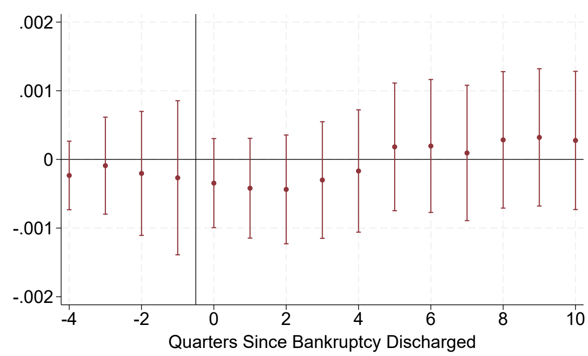
(C) Homeowner Share



(D) Share of Single-Family Units



(E) Migration, Conditional on Household Mortgage



(F) Landlord Credit Checks

Figure 3. Effect of Flag Removal on Migration (Mechanisms). Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel (A) shows the estimated effect on household mortgages open after the bankruptcy flag removal, while Panel (B) shows the effect of new household mortgages interacted with a dummy for migration within a 2 quarters window. Panel (C) and (D) depicts the estimated results using the share of homeowners and the share of single-family housing units as the dependent variable. Panel (E) shows the results on zip-code migration using the sequence of common-timing differences-in-differences approach describe in section 3. The samples are divided based on having or not an open household mortgage in the period before the bankruptcy flag is removed. Lastly, Panel (F) shows the effect of bankruptcy flag removals on landlord credit checks.

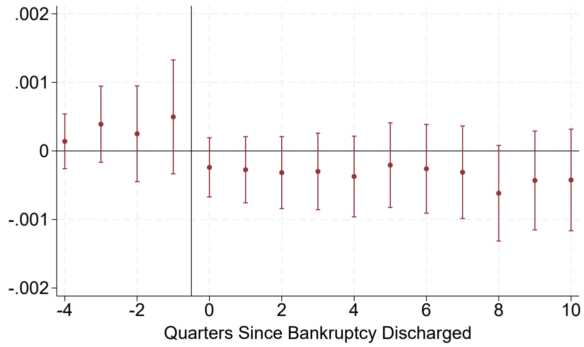
that the part of the total effect that is driven by increased mortgage access is mostly about existing homeowners access to credit, rather than inducing renters into homeownership.

In particular, there are at least two reasons why migration might be influenced by improved credit access outside of mortgages. First, the act of moving—even for non-homeowners—often involves significant expenses, such as hiring movers, renting temporary storage space, or putting down security deposits. Access to credit can help cover these costs, and we do observe an increase in credit card balances (Panel C in Figure 1), which could be used to finance moving. Second, landlords often review credit records when deciding whether to approve rental applications. In fact, in our sample, the number of landlord credit checks per quarter is a bit larger than the quarterly migration rate (see Table 1). Although we do not find a significant increase in landlord credit checks, as depicted in Panel (F), it might be the case that an increased credit score or the removal of the bankruptcy flag from credit reports may increase the likelihood of a landlord approving a tenant after the credit check. While we cannot give conclusive evidence of these channels, we think they are the most likely alternative explanations, given that mortgage access is not the complete mechanism.

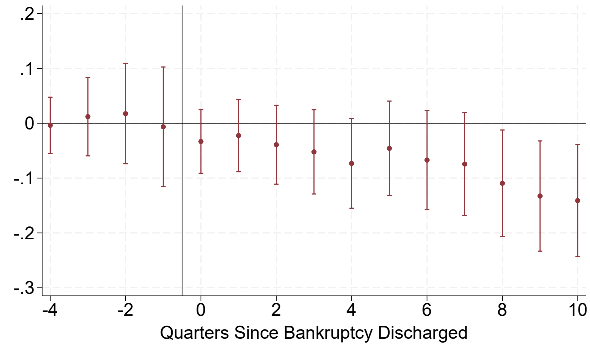
4.4 The Effect of Credit Access on Neighborhood Choice

The results in Section 4.2 suggest that credit frictions can be a limitation on an individual's mobility. However, once these barriers are overcome, an important question is where people choose to relocate. To answer this question, we examine how neighborhood characteristics of an individual change after the bankruptcy flag is removed from the credit report. We look across a range of characteristics that one might expect to be associated with neighborhood quality, and we find very few significant effects. In fact, we can rule out even very small positive effects, suggesting that people whose budget sets suddenly increase do not move to places that are more appealing based on observable characteristics.

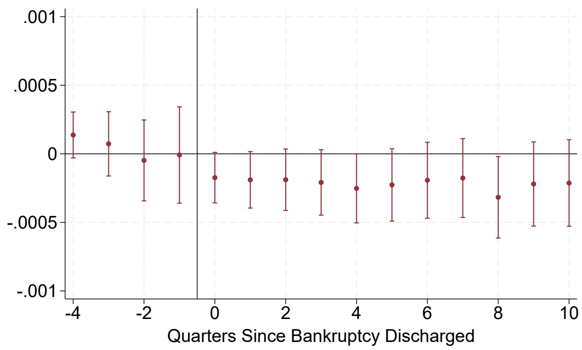
In Figure 4, we find a slight decrease in median income, share of neighborhood with a bachelor's degree, 3rd grade math scores, and zip-code population growth, indicating that



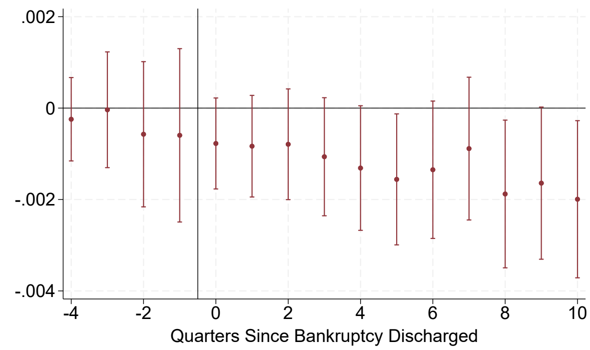
(A) Median Income



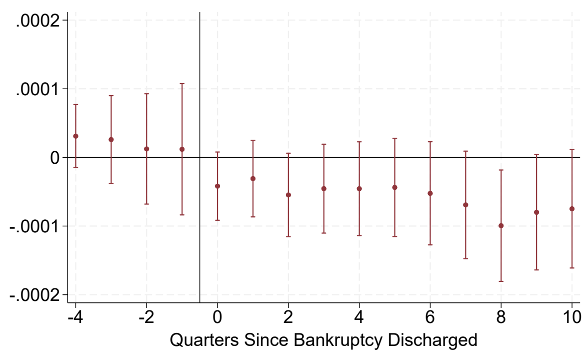
(B) Zip-Code Population Growth



(C) Share with a Bachelor's Degree



(D) 3rd Grade Math Scores



(E) Social Mobility (Chetty et al., 2018)

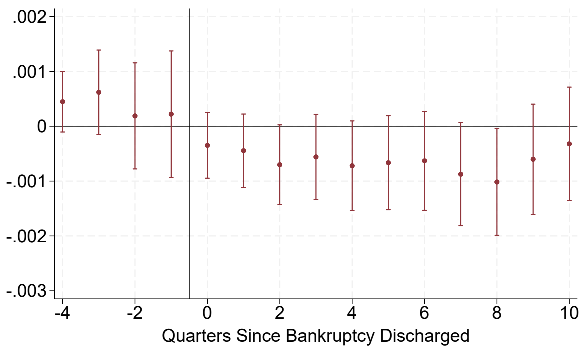
Figure 4. Effect of Flag Removal on Neighborhood Choice. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel (A) shows the estimated results for household median income (log) in the zip-code. Panel (B) displays the results for the population growth rate in the zip-code between 2000 and 2020. Panel (C) depicts the results for the fraction of people aged 25 or older who have at least a bachelor's degree. Panel (D) shows the results for the Mean 3rd grade math test scores in 2013, obtained from the Stanford Education Data Archive. Panel (E) shows the effects on the mean individual income percentile rank for children with parents in the percentile 25 of the national household distribution of income.

there is little evidence that people are moving to "better" neighborhoods. Typically, we would associate higher incomes, a more educated neighborhood, growing neighborhoods, or a neighborhood with better test scores to be "goods." In contrast, we can rule out even small increases in these measures, and the point estimates for all of the regressions would suggest that people move to less "good" neighborhoods after bankruptcy flag removal.

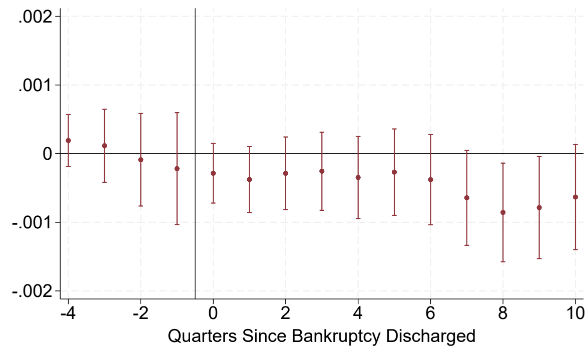
We also look at measures of social mobility by Chetty et al. (2018). This measure track children that grow up in the zip code with parents at the 25th percentile of income, and measures their income percentile once they grow to adulthood. Again, we can rule out even small increases in neighborhood quality based on this measure.

We look at three additional measures in Appendix Figure A.2: social mobility as measured using family income from Chetty et al. (2018), adult incarceration of children growing up in that neighborhood also from Chetty et al. (2018), and life expectancy. On all three dimensions, we find similar results, where the effect is not statistically different from zero, and we can rule out even small increases.

One reason neighborhood quality might decline is if the people that move in response to the bankruptcy flag expiration were already living in better neighborhoods and the decline reflects a reversion to the mean. Our summary statistics suggest this is not the case because the neighborhoods of people in our bankruptcy sample are a little worse than average. Nonetheless, a more rigorous test would be to condition our results on initial neighborhood quality. In Appendix Figure A.6, we split the sample by the zip code median income in the year before flag removal, and analyze whether people from each group move to better neighborhoods. Initially, there is little heterogeneity—again, if anything, both groups move to slightly worse neighborhoods. Over time, it seems that people that start in better neighborhoods move to even better neighborhoods, and people in worse neighborhoods move to even worse neighborhoods, which is the opposite of the reversion to the mean story we might have thought.



(A) House Prices



(B) Rents

Figure 5. **Effect of Flag Removal on Neighborhood Choice.** Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel (A) shows the estimated results for house prices (log) measured as the median Zillow Home Value Index (ZHVI) in 2022. Panel (B) depicts the results for local market rents (log), measured as the median Zillow Observed Rent Index (ZORI) in 2022.

In Figure 5, we look at measures of housing costs. Consistent with the declines in neighborhood quality, we also find (mostly) insignificant declines in house prices and rents, and we can rule out even fairly small increases in either price.[9]

Discussion

Flag removal has no positive effect on neighborhood quality, and may have a negative effect. These negative effects are offset by a decrease in housing costs. What should we make of this result? On the one hand, bankruptcy flag removal clearly alleviates some budget constraint for the agent, e.g. access to lower interest rates, higher credit limits, or reducing the price

[9]In Figure A.3, we look at other measures related to housing supply. We see perhaps a small increase in population density and a slight decrease in the distance to the central business district (CBD), consistent with our previous findings on lower home-ownership and single-family housing from Figure 3. Note that to make the distance to CBD comparable across cities, this is measured as a share of the radius of the city (Baum-Snow and Han, 2024).

This is a bit surprising given that house prices and rents are both lower after flag removal, and we might expect a negative relationship between the distance to CBD and prices. Interestingly, we do find a small insignificant increase in the housing supply elasticity as measured by Baum-Snow and Han (2024). We were independently interested in housing supply elasticity is to see if credit access has an effect on housing prices due to people using the credit to raise relative demand in housing supply inelastic areas. In Howard and Liebersohn (2021), we argued that this was an important channel to understand the rise in rents nationally from 2000-2018. Here because we find that credit access is leading people to move to more elastic places, this is one channel through which increased credit access would have a negative effect on aggregate house prices and rents, although the magnitudes would be small.

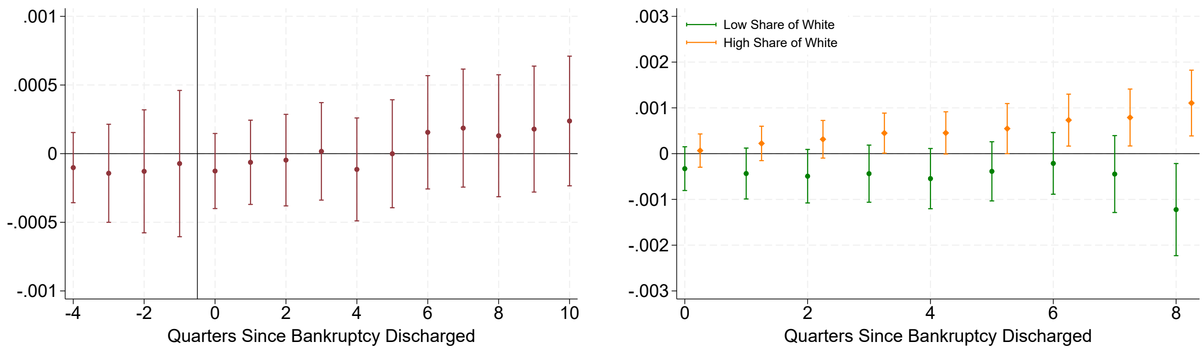
of a rental from infinity to the market price if the landlord has a threshold credit score. Holding prices constant, we would expect households to typically consume more neighborhood "quality" if "quality" were a normal good (as opposed to an inferior good). Of course, there could also be price effects as credit access changes the relative price of neighborhoods in addition to changing the budget constraint. However, we might expect that credit access reduces the relative price of home-ownership to renting, and reduces the relative price of higher "quality" housing. In particular, it also reduces the relative price of all other housing compared to the person's current housing, in that credit access helps facilitate moving.

One possible conclusion from this set of results is that neighborhoods of higher quality are not a normal good for the population that is treated by the bankruptcy flag removal. Of course, we see that as people move to lower quality neighborhoods, they are also getting cheaper housing, in terms of both house prices and rents. If, in equilibrium, other people value neighborhood quality more than the the treated group, and if those people are sufficiently mobile such that prices and rents reflect their preferences, and if the treated groups' preferences value these goods less, then we might expect the treated group to move to lower quality neighborhoods when the flag is removed. For them, the increased prices and rents more than offset the increased quality, so their net utility is higher in places with lower quality.

One of the big assumptions from the last paragraph is that there exists some set of people that are mobile enough so that rents and prices reflect their preferences, i.e. the main assumption in the literature following Rosen (1979) and Roback (1982). So one might take our results as saying that even though we have shown that lack of credit access is a migration friction, we are nonetheless finding evidence in favor of the Rosen (1979)-Roback (1982) assumptions.

4.5 The Effect of Credit Access on Local Racial Composition

Figure 6 displays the effects on the racial composition of the neighborhood a person lives in post-flag-removal. While any particular racial composition is not likely to be universally viewed as a measure of quality, people do likely have idiosyncratic preferences over it. In



(A) Share of White

(B) Share of White, Conditional on Initial Share

Figure 6. Effect of Flag Removal on Neighborhood Racial Composition. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel (A) shows the estimated results for share of white. Panel (B) shows the results on the share of white in the neighborhood using the sequence of common-timing differences-in-differences approach described in section 3. The samples are divided based on whether you live in a zip-code with a high (low) share of white people (based on the median) before the bankruptcy flag is removed.

fact there is a large literature on racial homophily; a very incomplete list would start with Schelling (1971) and include recent papers such as Boustan (2010), Bayer et al. (2017), Shertzer and Walsh (2019), Bayer et al. (2022), Davis et al. (2023), and Almagro et al. (2024).

We find very little evidence that people move to more or less White neighborhoods. In Appendix Figure A.4, the point estimates on the share of other groups suggest people might move to more Hispanic and less Black neighborhoods, but the estimates are mostly insignificant. Similarly, we do see an increase in the share of foreign-born, but the point estimates are quite small. However, one interesting finding, in Panel (B) of Figure 6, is that when we split the sample based on the share of white people in the neighborhood you live in before the flag is removed, we see that people in already white neighborhoods move to even more white neighborhoods, while people in less white neighborhoods move to even less white neighborhoods. Unfortunately, we do not directly observe the race of the person, but this would be consistent with people having preferences for homophily, and those preferences not being reflected in house prices and rents because different people have different choices over racial composition.

5 Conclusion

In this paper, we document that credit flag removal increases the across-zip and across-state migration rates, establishing a link between credit access and migration. Interestingly, we do not find that people move toward higher quality neighborhoods, which we interpret as being consistent with a Roback (1982)-Roback (1982) model of the world.

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A Appendix

A.1 Data

Table A.1 provides a detailed description of the data used in the analysis, and its sources.

Table A.1. Description of the Variables

Variable	Description
Credit Score	Snapshot of the credit score of the primary respondent in the of the calendar quarter.
New Credit Cards	Number of newly open credit cards in a given calendar quarter. It is coded as zero if the person does not open new credit cards in that quarter.
Balance on New Credit Cards	Balance on newly open credit cards in a given calendar quarter. It is coded as zero if the person does not open new credit cards in that quarter.
Limit on New Credit Cards	Credit limit on newly open credit cards in a given calendar quarter. It is coded as zero if the person does not open new credit cards in that quarter.
New Household Mortgages	Indicator variable that takes the value of 1 if the household of the primary respondent has a new open mortgage in a given quarter, and 0 otherwise.
Landlord Credit Checks	Total number of credit checks by landlords in a given calendar quarter.
Migration	Indicator variable that takes the value of 1 if an individual reside in a different location compared to the previous quarter, and 0 otherwise. For yearly migration, we consider the same quarter in the previous year.
Share of Homeowners	Share of homeowners in the zip code, as measured in the U.S. Census Bureau (2010).
Share of Single-Family Units	Share of single-family housing units in the zip code, as measured in the U.S. Census Bureau (2010).
Population Density	Number of residents in the zip code per square kilometers, as measured in the U.S. Census Bureau (2010)
Median Income	Median household income in the zip code, as measured in the U.S. Census Bureau (2010).
House Prices	Zillow Home Value Index (ZHVI) at the zip code. Obtained from Zillow Research (2022). We use the median house price in 2022.

Rents	Zillow Observed Rent Index (ZORI) at the zip code. Obtained from Zillow Research (2022). We use the median rent value in 2022.
House Price Index	Federal Housing Finance Agenty (FHFA) House Price Index (HPI) at the zip code. We compute the growth of the HPI between 1998 and 2022.
Mean of Commute Time	Mean commute time for workers over 16 years old in the county, as measured in the 2000 Decennial Census. Obtained from the Opportunity Insights data library from Chetty et al. (2018). We create a crosswalk to zip code by using a population-weighted average.
Distance to CBD	Fraction of the way from the region's CBD to the region edge. Obtained from Baum-Snow and Han (2024). We create a crosswalk to zip code by using a population-weighted average.
Housing Supply Elasticity	Measure of housing supply elasticity from Baum-Snow and Han (2024). We create a crosswalk to zip code by using a population-weighted average.
Zip-Code Population Growth	Changes in the natural log of total population in the zip code between 2000 and 2020, times 100, as measured in the Decennial Census.
Share with a Bachelor's Degree	Number of people aged 25 or older who have at least a bachelor's degree in 2010. Obtained from the Opportunity Insights data library from Chetty et al. (2018). We create a crosswalk to zip code by using a population-weighted average.
3rd Grade Math Scores	Mean 3rd grade math test scores in 2013, from the Stanford Education Data Archive and measured at the district level. Obtained from the Opportunity Insights data library from Chetty et al. (2018). We create a crosswalk to zip code by using a population-weighted average.
Share of Race	Racial shares as measured in the U.S. Census Bureau (2010). All races − except Hispanics − exclude Hispanics and Latinos.
Share of Foreign-Born	Share of foreign-born residents in the 2010 Decennial Census. Obtained from the Opportunity Insights data library. We create a crosswalk to zip code by using a population-weighted average.
Life Expectancy	Life expectancy estimates at birth by census tract for the period between 2010 and 2015. Obtained from the U.S. Small-area Life Expectancy Estimates Project (USALEEP). We create a crosswalk to zip code by using a population-weighted average.

Mean Individual Income Rank	Mean percentile rank in the national distribution of individual income for children with parents in the percentile 25 in the national household income distribution, measured in 2014-2015. Obtained from the Opportunity Insights data library from Chetty et al. (2018). We create a crosswalk to zip code by using a population-weighted average.
Mean Household Income Rank	Mean percentile rank in the national distribution of household income for children with parents in the percentile 25 in the national household income distribution, measured in 2014-2015. Obtained from the Opportunity Insights data library from Chetty et al. (2018). We create a crosswalk to zip code by using a population-weighted average.
Jail	Fraction incarcerated children on April 1st, 2010 with parents in the percentile 25 in the national household income distribution. Obtained from the Opportunity Insights data library from Chetty et al. (2018). We create a crosswalk to zip code by using a population-weighted average.

Table A.2. Summary Statistics Based on Credit Score (UC-CCP Sample)

	Low Credit Score (550-600)	High Credit Score (700-750)
Zip Code Migration (1-Quarter)	0.044	0.038
	(0.205)	(0.193)
Share of White	0.582	0.672
	(0.292)	(0.259)
Homeowner Share	0.642	0.686
	(0.172)	(0.166)
Single-Family Share	0.732	0.749
	(0.210)	(0.207)
Population Density	2,164.9	2,086.1
	(4,461.8)	(4,666.5)
Median Income	51,804.6	61,074.1
	(19,277.9)	(23,128.9)
Population Growth	12.316	16.700
	(33.965)	(36.551)
Share with a Bachelor's Degree	0.239	0.300
	(0.139)	(0.164)
3rd Grade Math Scores	3.079	3.345
	(0.043)	(0.840)
Social Mobility	0.437	0.454
	(0.043)	(0.046)
House Prices	373,071.2	457,507.5
	(291,333.9)	(368,248.0)
Market Rent	1,903.5	2,101.8
	(913.863)	(859.546)

Table A.3. Summary Statistics Based on Credit Score (Bankruptcy Sample)

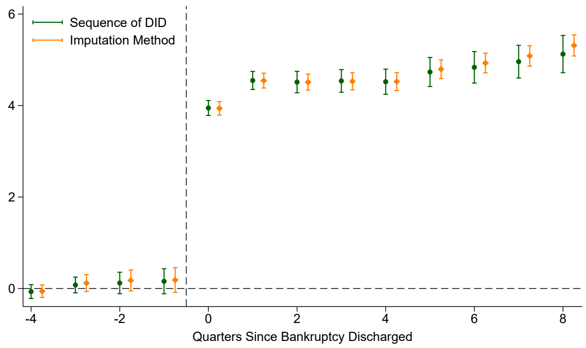
	Low Credit Score (550-600)	High Credit Score (700-750)
Zip Code Migration (1-Quarter)	0.062 (0.241)	0.035 (0.184)
Share of White	0.656 (0.273)	0.681 (0.255)
Homeowner Share	0.683 (0.152)	0.701 (0.147)
Single-Family Share	0.770 (0.183)	0.779 (0.177)
Population Density	1,623.6 (3,455.1)	1,578.4 (3,339.8)
Median Income	53,719.6 (18,181.6)	58,171.5 (19,516.9)
Population Growth	14.291 (35.316)	18.272 (37.887)
Share with a Bachelor's Degree	0.238 (0.129)	0.262 (0.136)
3rd Grade Math Scores	3.163 (0.819)	3.259 (0.809)
Social Mobility	0.438 (0.041)	0.447 (0.042)
House Prices	358,107.1 (260.997.8)	411,059.3 (288.592.2)
Market Rent	1,885.3 (662.3)	2,007.5 (747.5)

Table A.4. Regression of Migration Between Samples

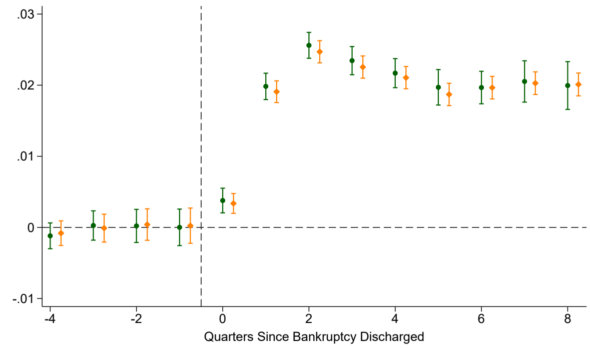
	Zip Code Migration			
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Bankruptcy Sample)	0.0113	0.0133	0.0112	0.0009
	(45.98)	(53.58)	(42.32)	(3.85)
N	9,011,934	9,011,934	8,194,367	9,011,927
R^2	0.0007	0.0047	0.0063	0.0086
State FE		✓		
Time FE		✓		
Age FE			✓	
Credit Score FE				✓

A.2 Robustness Check

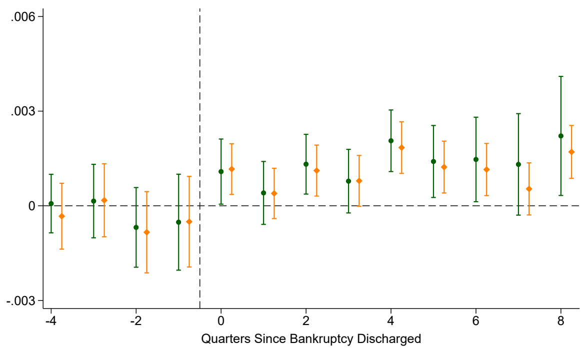
Figure A.1 shows the effects of bankruptcy flag removal on credit scores, new credit cards, new mortgages and migration using the Borusyak et al. (2021) imputation approach alongside the estimates using the sequence of common-timing differences-in-differences approach described in equation (3).



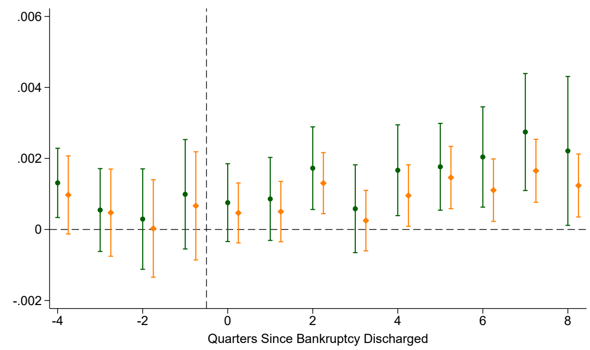
(A) Credit Score



(B) New Credit Cards



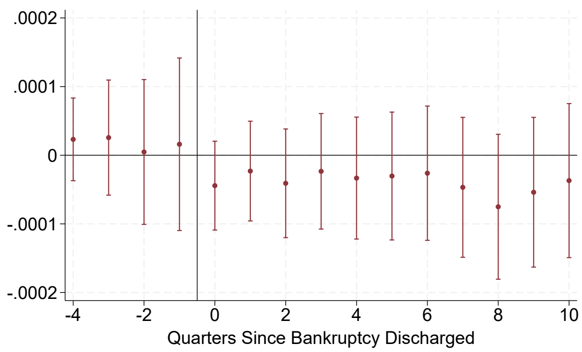
(C) New Household Mortgage



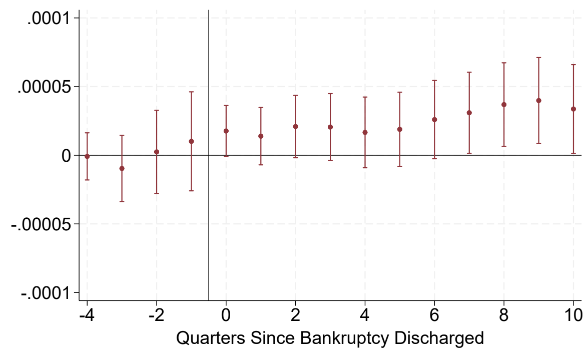
(D) 1-Quarter Zip Code Migration

Figure A.1. Effect of Flag Removal on Credit Access and Mobility. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. The orange lines depicts the estimates from the Borusyak et al. (2021) imputation approach. The green lines displays the estimates using the sequence of common-timing differences-in-differences approach described in equation (3). The confidence intervals for the green lines are based on 100 bootstrap replications.

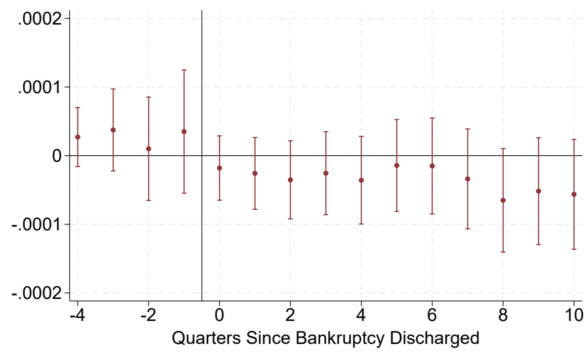
A.3 Additional Results



(A) Social Mobility (Chetty et al., 2018)

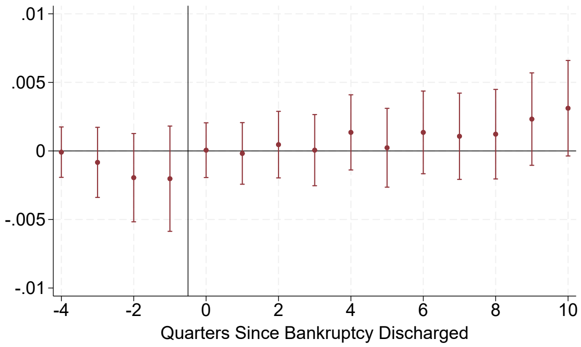


(B) Fraction of Children Incarcerated

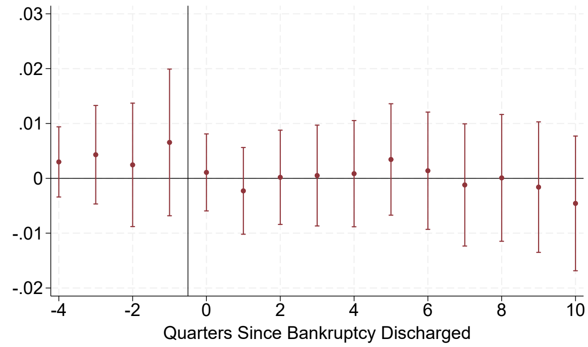


(C) Life Expectancy

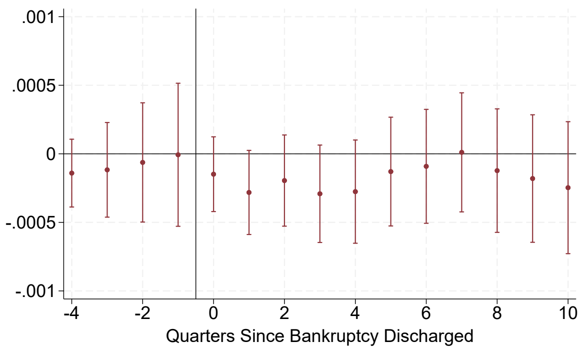
Figure A.2. Effect of Flag Removal on Neighborhood Choice. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel A shows the effects on social mobility, as measured by the mean adult (family) income rank of children growing up in the zip code with parents at the 25th percentile of national income, as measured by Chetty et al. (2018). Panel (B) shows the fraction of the children that grow up to be incarcerated, also from Chetty et al. (2018). Panel (C) shows the life expectancy in the neighborhood.



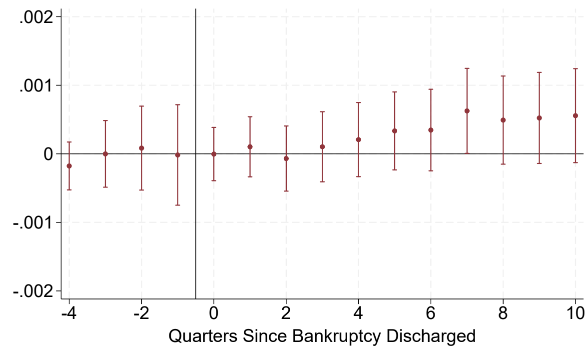
(A) Population Density



(B) Mean Commute Time

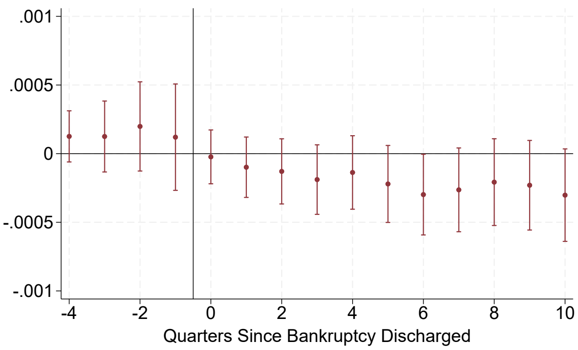


(C) Distance to CBD, as a share of city radius

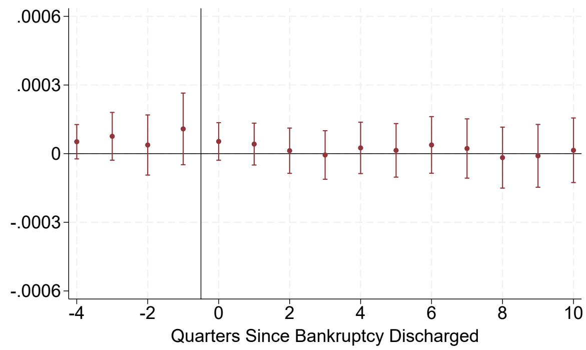


(D) Housing Supply Elasticity

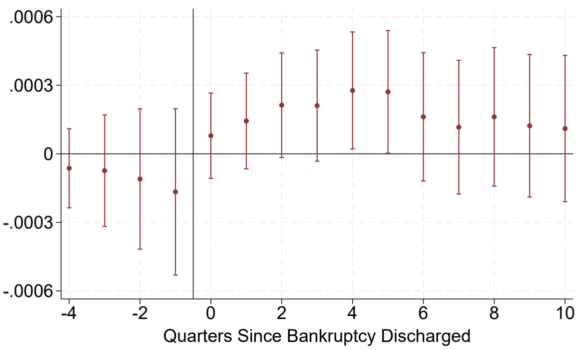
Figure A.3. Effect of Flag Removal on Neighborhood Choice. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel (A) shows the estimated results for log population density in the zip code. Panel (B) displays the results for the mean commute time in the zip code. Panel (C) depicts the distance to the central business district, as a share of the city radius (Baum-Snow and Han, 2024) Panel (D) shows the effects on housing supply elasticity (Baum-Snow and Han, 2024).



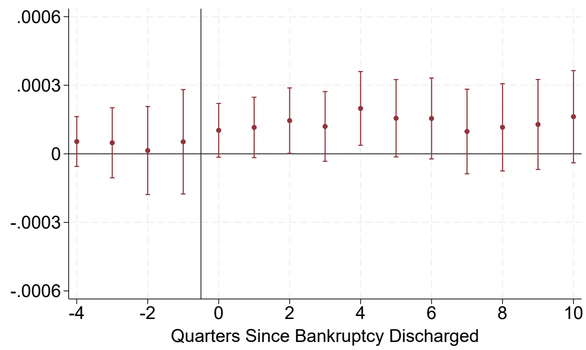
(A) Share of Black



(B) Share of Asian

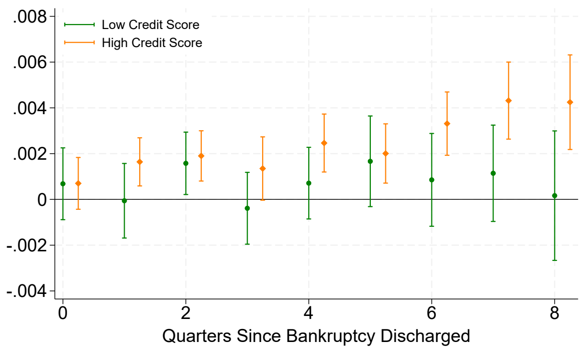


(C) Share of Hispanic

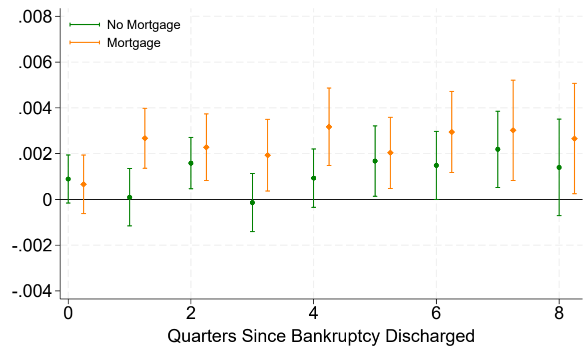


(D) Share of Foreign-Born

Figure A.4. Effect of Flag Removal on Neighborhood Racial Composition. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The bar lines are 95% confidence intervals, with standard errors clustered at the individual level. Panel (A) displays the results for the share of blacks. Panel (B) depicts the results for the share of Asians. Panel (C) shows the results for the share of Hispanics. For the share of white, blacks and Asians, we consider only individuals that are not Hispanics as well. Panel (D) displays the results for the share of foreign-born residents in the 2010 Census.

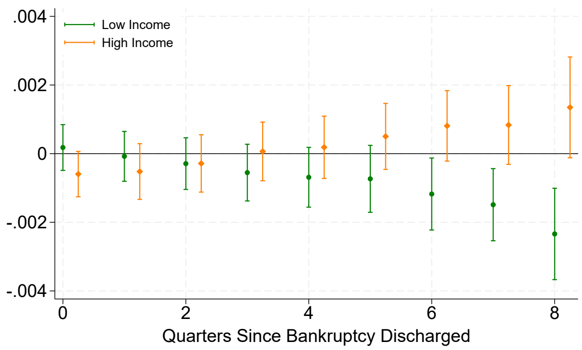


(A) Migration, Conditional on Credit Score

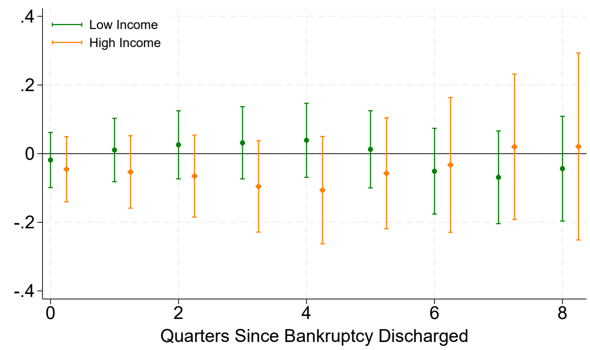


(B) Migration, Conditional on Mortgage

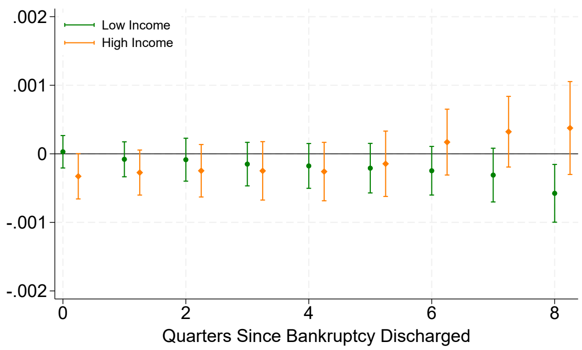
Figure A.5. Effect of Flag Removal on Migration, Conditional on Characteristics. Each dot represents the estimated effect of a removal of the bankruptcy flag on zip-level migration, conditional on individual and time fixed effects. The coefficients are estimated using the sequence of common-timing differences-in-differences approach described in section 3. The bar lines are 95% confidence intervals based on 100 bootstrap replications. Panel (A) splits the sample by the median credit score. Panel (B) splits the sample by whether a person had a mortgage.



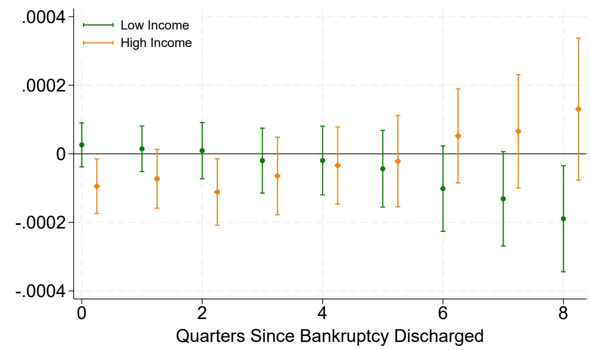
(A) Median Income



(B) Zip-Code Population Growth



(C) Share with a Bachelor's Degree



(D) Social Mobility (Chetty et al., 2018)

Figure A.6. Effect of Flag Removal on Neighborhood Choice, by Initial Neighborhood Median Income. Each dot represents the estimated effect of a removal of the bankruptcy flag on the outcome of interest, conditional on individual and time fixed effects. The coefficients are estimated using the sequence of common-timing differences-in-differences approach described in section 3. The bar lines are 95% confidence intervals based on 100 bootstrap replications. The samples are divided based on whether an individual live in a zip-code with a high (low) income (based on the median) before the bankruptcy flag is removed. Panel (A) shows the estimated results for household median income (log) in the zip-code. Panel (B) displays the results for the population growth rate in the zip-code between 2000 and 2020. Panel (C) depicts the results for the fraction of people aged 25 or older who have at least a bachelor's degree. Panel (D) shows the effects on the mean individual income percentile rank for children with parents in the percentile 25 of the national household distribution of income.