What Determines Consumer Financial Distress? Place- and Person-Based Factors*

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Abstract

We use credit report data for a representative sample of 35 million individuals over 2000-2016 to examine consumer financial distress in the United States. We show there are large, persistent geographic disparities in consumer financial distress, with low levels in the Upper Midwest and high levels in the Deep South. To better understand these patterns, we conduct a "movers" analysis that examines how financial distress evolves when people move to places with different levels of financial distress. For collections and default, there is only weak convergence following a move, suggesting these types of financial distress are not primarily caused by place-based factors (such as local economic conditions, loan supply, and state laws) but instead reflect person-based characteristics (such as financial literacy and risk preferences). In contrast, for personal bankruptcy, we find a sizable place-based effect, which is consistent with anecdotal evidence on how local legal factors influence the bankruptcy filing decision. Individual characteristics determine whether you get into financial distress, while place-based factors determine whether you use bankruptcy to get out.

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1 Introduction

Consumer financial distress in the United States is high in both absolute and relative terms. In credit report data, one-third of individuals have at least one debt in collections and nearly 5% have declared bankruptcy in the last 7 years.[1] While there is no perfect data for relative comparisons, the available data indicate that financial distress is much higher in the U.S. than Europe.[2]

In this paper, we aim to advance our understanding of consumer financial distress in the U.S. by examining patterns in financial distress across geographic areas. We measure financial distress using a nationally representative panel of TransUnion credit report data that tracks approximately 35 million individuals on a monthly basis over 2000-2016. To the best of our knowledge, this dataset is the largest individual-level credit report dataset made available to researchers.

We focus our analysis on three common metrics of financial distress – debt in collections, credit card non-payment, and personal bankruptcy. We emphasize these three metrics because our aim is to observe financial distress for the broadest possible segment of the population. Other measures – such as home foreclosures or auto repossessions – provide a narrower window on financial distress because these products are held by a smaller and more affluent sample of the population.

The first part of our paper documents large and persistent geographic disparities in financial distress between the Upper Midwest and Deep South regions of the country.[3] In the Deep South, 44% of people with a credit report have an unpaid debt in collections versus 24% in the Upper Midwest. Similarly, measures of credit card non-payment and bankruptcy are 40% to 50% higher in the Deep South as the Upper Midwest.

The main part of the paper aims to better understand what determines these ge-

[1] Authors' calculations using credit bureau data from June 2015 described below.

[2] For the sake of comparability, it is useful to rely on survey data from similar years. According to the 2007 Survey of Consumer Finances, 20.8% of U.S. households are late on their debt payments (`https://www.federalreserve.gov/pubs/bulletin/2014/articles/scf/scf.htm`). In contrast, the EU-SILC (Statistics on Income and Living Conditions) 2008 ad hoc module indicates a median rate of arrears across E.U. countries of 3.2% on mortgage loans and 1.2% on non-mortgage loans (`http://ec.europa.eu/eurostat/web/income-and-living-conditions/data/ad-hoc-modules`).

[3] We define the Upper Midwest as Iowa, Minnesota, North Dakota, South Dakota, Wisconsin, and the Upper Peninsula of Michigan and the Deep South as Alabama, Arkansas, Georgia, Louisiana, Mississippi, and South Carolina.

ographic disparities. Much of the existing research on financial distress can be separated into two categories. One category emphasizes local institutional and economic factors, such as state-level bankruptcy laws (e.g., Fay, Hurst and White, 2002; Agarwal, Liu and Mielnicki, 2003; Livshits, MacGee and Tertilt, 2007; Auclert, Dobbie and Goldsmith-Pinkham, 2019) and shocks to the local economy (e.g., Agarwal and Liu, 2003; Sullivan, 2008; Keys, 2018). A second category emphasizes individual characteristics, such as preference parameters (e.g., discount rates, risk preference, sigma) and behavioral factors (e.g., inattention, financial literacy).[4]

We quantify the relative importance of these categories using a "movers" research design that examines how financial distress evolves when individuals move to places with differential levels of financial distress. Consider an individual who moves from a high financial distress area to a low financial distress area. The analysis asks: To what extent does the mover's outcome transition from the average outcome in their origin to the average outcome in their destination? If local institutional and economic factors are important, we would expect outcomes to converge to those in the new location. If financial distress is determined by individual characteristics, we would not expect any convergence. In keeping with movers design literature, we will sometimes refer to local institutional and economic factors as "place effects" and individual characteristics as "person effects" (e.g., Finkelstein, Gentzkow and Williams, 2016).

We operationalize this movers research design by estimating event-study regressions of a given outcome on the "size of the move," defined as the average difference in that outcome between the origin and destination areas, along with individual and time fixed effects, and other controls. We show robustness to defining the size of the move using narrower and broader levels of geographic aggregation.

The identifying assumption for the movers research design is the standard parallel trends assumption – conditional on controls, the size of the move is uncorrelated with trends in the outcome. A natural concern is that people move to less expensive and potentially more distressed locations in response to persistent negative shocks to their economic

[4]See, for example, Gross and Souleles (2002) on stigma, Agarwal, Chomsisengphet and Liu (2011) on "individual social capital" (as proxied by mobility, rural residency, homeownership, marital status, and borrower age), and Gerardi, Goette and Meier (2013) on financial literacy.

circumstances (e.g., job loss) or conversely move to economically vibrant locations in response to persistent positive shocks. We provide three pieces of evidence in support of our identifying assumption.

First, we show there is no correlation between the size of the move and the pre-move trend in our outcomes. For instance, in our event study plots, there is no evidence of an effect prior to the move, and an effect that occurs fairly precisely, though gradually, after the move takes place.[5]

Second, we show that we get similar results when we exclude potentially problematic variation. To address concerns about bias from origin-specific shocks, we isolate variation from individuals who move "from the same origin" to destinations with different financial distress. To address concerns about bias from destination-specific shocks, we isolate the complementary variation from individuals who move "to the same destination" from origins with differential financial distress.

Third, while we believe our research design is valid, the most likely violation of our identifying assumption would bias upwards our place-based effects. This would occur if moves to more distressed areas were precipitated by negative shocks, such as job loss, that directly cause financial distress. The small place-based effects for debt in collections and credit card non-payment (discussed below) reduce our concerns about bias of this form. Because the place-based effects have a theoretical lower bound of zero, the small estimates impose a fairly tight upper bound on the degree of upward bias, at least for these outcomes.

We find that debt in collections and our credit card non-payment measures converge by less than 10% at 6 years post move. In other words, the place-based component accounts for less than one-tenth of the geographic variation in financial distress, while the person-based component accounts for the remaining nine-tenths.

The small convergence for debt in collections masks a substantially larger place-based component of roughly 20% for medical debt in collections. This is consistent with the heterogeneous collection practices of local medical providers (e.g., doctors offices and hos-

[5]With monthly updates of zip code based on the mailing address of the individual, the depersonalized credit report data allows us to closely track household location.

pitals) having a meaningful impact on overall debt in collections. However, taken together, these results imply that for collections and default, individual characteristics are the dominant force.

In stark contrast to the results discussed above, we find a sizable place-based component for the likelihood of filing for bankruptcy. At 6 years after a move, bankruptcy filing rates converge by roughly one-quarter of the origin-destination difference for Chapter 7 and one-third of the difference for Chapter 13.

Heterogeneity analysis suggests that the Chapter 13 results reflect an underlying informational theory of geographic variation. Under an informational theory, when individuals move to a place with higher Chapter 13 filing rates, they learn about Chapter 13 and there is an increase in the rate of filing. When individuals move to locations with lower Chapter 13 filing rates, they do not unlearn what they previously knew, and so there is not a symmetric decrease in filing. We find that Chapter 13 effects are more than twice as large for moves to places with higher Chapter 13 filing rates than moves to places with lower filing rates. This type of informational theory is supported by previous anecdotal evidence on the importance of lawyer networks and legal traditions in the Chapter 13 filing decision (Sullivan, Warren and Westbrook, 1994; Jacoby, 2014) and parallels a finding in Chetty, Friedman and Saez (2013), who document a similar asymmetry in take-up of the earned income tax credit.[6]

To summarize our findings in one sentence: Individual characteristics determine whether you get into financial distress, while place-based factors determine whether you use bankruptcy to get out.

Taken together, this set of facts helps prioritize competing theories of financial distress, and is thus useful for guiding future research and policy discussions. A large literature in economics and finance – including work by ourselves – has examined the effects of local institutional factors (e.g., state laws, local lending practices) on credit market outcomes.[7] Our finding of statistically significant place-based effects is consistent with the results from

[6]Unlike prior work, we do not find any persistent correlation between our place effects and local or economic factors. We discuss these results and their interpretation in Section 5.

[7]See, for instance, Gropp, Scholz and White (1997); Pence (2006); Dick and Lehnert (2010); Mahoney (2015); Han, Keys and Li (2017).

this literature. However, with the exception of bankruptcy, our finding that place-based factors only account for a small share of the geographic differences suggests that these factors are only of limited quantitative importance for understanding the substantial geographic variation in financial distress we document.

Conversely, the large person-based components for these outcomes suggest an important role for persistent individual characteristics in explaining the observed geographic variation in financial distress. Such characteristics may include financial literacy and human capital; household wealth and intergenerational transfers; and risk preferences, default stigma, or discount rates. These findings are consistent with new evidence on the persistence of financial distress at the individual level (Athreya, Mustre-del Río and Sánchez, forthcoming) and are germane to the broader discussion on the determinants of consumer financial distress (Dynan, 2009; Porter, 2012).

Finally, our research adds a new finance-related dimension to a rapidly growing literature that seeks to separate geographic and institutional factors from individual characteristics using movers designs. This literature includes research on brand preferences (Bronnenberg, Dube and Gentzkow, 2012), health care costs and outcomes (Finkelstein, Gentzkow and Williams, 2016, 2018, 2019), and intergenerational mobility (Chetty and Hendren, 2018*a*,*b*), among other topics.

The remainder of the paper proceeds as follows. Section 2 describes our data and presents summary statistics on our measures of financial distress. Section 3 documents the geographic variation in financial distress. Section 4 provides the econometric methodology of our movers approach. Section 5 conducts the analysis that decomposes the variation in financial distress into person- and place-based components. Section 6 concludes.

2 Data

2.1 Credit Report Data

We measure financial distress using a monthly panel of credit reports over 2000–2016 from TransUnion, one of the three national credit reporting agencies. The panel is based on a random 10% sample of individuals with TransUnion credit records in 2000. In each month,

a small percentage of individuals leave the panel (e.g., due to death). To maintain the representativeness of the data, each month a random 10% sample of individuals with new credit reports is added to the panel.

In the average month, we observe data for 35.6 million individuals. We drop individuals if they have missing age information. Most of these individuals have very little credit utilization. We also drop individuals who are older than 80 or younger than 20. In the average month, the resulting sample has 30.1 million individuals.

For each individual $\times$ month observation, we observe two types of data. First, we observe individual-level information, including zip code, age, credit score, and aggregated data on loans (e.g., aggregate credit card balances).[8] Second, we observe line-item information on trades (e.g., specific credit cards), debts in collection, and public records (e.g, bankruptcies). We use these data to construct our primary measures of financial distress. More detail on variable construction is provided in Appendix Section A.

- **Debt in collections**: We construct indicators for whether an individual has at least one debt currently in collections, at least one medical debt currently in collections, and at least one non-medical debt currently in collections. We also construct measures of the collection balance overall, and separately by medical and non-medical debt.[9]

- **Credit card not current**: We define an indicator for credit card not current as taking a value of 1 if the individual has at least one credit card that is 30 days or more past due (30+ DPD), charged off, or in collections. We view this variable – which includes accounts that have been charged off or are in collections – as measuring financial distress from the perspective of consumers. Chargeoffs and unpaid collections can remain on consumer credit reports for up to 7 years if they are not paid or other-

[8] TransUnion receives updated addresses from data furnishers (e.g., lenders) on a monthly basis. Individuals typically provide their new address to their lenders, who in turn will supply that information to TransUnion. The majority of address updates occur through this channel. Individuals sometimes also contact TransUnion Consumer Relations to update their address information.

[9] Because collections are not automatically removed from credit reports when they are paid, we only include collection accounts that are not yet paid in full. We also drop collections with original balance smaller than $100 to avoid small-dollar nuisance accounts. Accounts with original balances smaller than $100 are similarly ignored by FICO Score 8: `https://www.myfico.com/credit-education/credit-scores/fico-score-versions`

wise removed. We construct this measure for all individuals with a credit report and conditional on those with a credit card.

For some of our analysis, we also consider an alternative credit card delinquency measure, which takes a value of 1 if the individual has at least one credit card that is 30 days or more past due (30+ DPD) but has *not* been classified as charged off or in collections. We view this variable – which excludes chargeoffs and collections – as measuring financial distress from the perspective of credit card issuers (e.g., banks). Issuers typically charge off accounts when they are roughly 180 days past due (180+ DPD). At this point, the account is no longer a liability to the issuer and therefore should not be counted as contributing to an issuer's financial risk.

- **Bankruptcy**: We construct an indicator for whether the individual has declared bankruptcy in the last 3 years, and separate indicators for whether they have filed under Chapter 7 or Chapter 13 of the bankruptcy code in the last 3 years. We use a 3-year window to smooth over noise in more high-frequency measures.

As mentioned in the introduction, we focus on these measures because they provide the broadest possible window into financial behavior. Other financial products – such as home loans and auto loans – are held by a smaller and less representative sample of the general population, and defaults on these products provide a narrower window into financial distress.

2.2 Summary Statistics

Table 1 shows summary statistics for our key measures of financial distress, and other measures we use in robustness analysis, as of June 2015.[10]

Debt in collections (1+ debt in collections) are held by 34.1% of the sample, with 22.1% holding some medical debt and 24.2% holding some non-medical debt in collections. Unconditional collection balances are $1,434 on average, implying that conditional on being positive, collection balances average $4,205.

[10] We use the midpoint of 2015, rather than the start or end of the year, to avoid the unrepresentativeness of the holiday period.

Our estimate of the percentage of individuals with debt in collections compares well to other sources. Using data from an unnamed major credit bureau, the Urban Institute reports that 33% of individuals have at least one debt in collections in 2016 (Urban Institute, 2019). In a nationally-representative survey conducted by the Kaiser Family Foundation / New York Times, 27% percent of 18-to-64 year olds report being contacted by a collection agency in the prior 12 months (Hamel et al., 2016).

Credit card not current (30+ DPD, charge offs, or in collections) is 12.5% for the entire sample, and 17.5% conditional on having a credit card. Delinquency (30+DPD but not charged off, or in collections) rates are much lower, reflecting the fact that accounts spend much less time in delinquency than charged off or in collections.[11]

Over the last 3 years, our data indicate that 14.8 in 1,000 individuals have filed for bankruptcy, with 10.5 in 1,000 filing under Chapter 7 and 4.5 in 1,000 filing under Chapter 13. The overall bankruptcy rate is identical to that reported in New York Fed's Consumer Credit Panel over this time period (NYFed, 2019).[12]

3 Geographic Variation

In this section, we discuss the sharp geographic disparities in financial distress across regions within the United States. Figure 1 presents maps of our key measures. The maps are based on June 2015 data aggregated to the commuting zone (CZ) level.[13] Table 2 shows summary statistics for these CZ-level data, also from June 2015. In this table, we weight the CZ-level data by the number of individual-level observations in each CZ so that the statistics are representative of the underlying individual-level data.

Panel A of Figure 1 shows the percent of individuals with debt in collections (1+ debt in collections). The map shows strikingly high rates of financial distress in the Deep South

[11]For instance, an account that is never paid would spend approximately 5 months in delinquency (30 to 180 days past due) and 84 months charged off or in collections (the 7 years from when the account is charged off until when the flag is removed), a 17 to 1 ratio. We estimate delinquency rates of 2.6% for the entire sample, and 3.6% conditional on having a credit card, which is higher than what is implied by this ratio. This is almost identical to the 3.5% rate reported in TransUnion (2019), albeit for a more recent time period.

[12]The rate of Chapter 7 and Chapter 13 bankruptcies sum to greater than the combined bankruptcy rate because individuals sometimes file under Chapter 13 and then Chapter 7 in close succession. This is colloquially known as filing under "Chapter 20."

[13]Commuting zones are clusters of counties characterized by strong within-cluster commuting ties. There are 741 CZs in the United States. Unlike metropolitan statistical area (MSA) designations, CZs cover the entire landmass of the United States.

and low rates in the Upper Midwest.[14] Specifically, Table 2 indicates that the percentage of individuals with debt in collections is 83% higher in the Deep South than the Upper Midwest (43.5% vs. 23.8%).

The sharp geographic differences are apparent for medical and non-medical debt in collections (Appendix Figure A1). However, as shown in Table 2, the differences are larger for medical debt (31.6% vs. 14.4%) than for non-medical debt in collections (29.1% vs. 17.4%). The differences between the Deep South and Upper Midwest are proportionally similar when we examine average collection balances, overall and separately for medical and non-medical debt. The maps for these outcomes are somewhat less crisp, partially due to the increased noisiness of these measures (Appendix Figure A2).

Panel B of Figure 1 shows the percentage of individuals with at least one credit card that is not current (30+ DPD, charged off, or in collections). Like debt in collections, there is a sharp geographic disparity, with credit card not current rates 51% higher in the Deep South than in the Upper Midwest (13.4% vs. 8.9%). We focus on credit cards because they are widely held (71% of individuals in our data). However, as shown in Panel A of Appendix Figure A3, credit card holding rates are lower in the Deep South. Thus, if we condition on having a card, the difference in credit card not current grows to 84% (21.4% vs. 11.6%). There are qualitatively similar geographic disparities in credit card delinquencies (30+ DPD but not charged off or in collections), although the magnitudes are much smaller.[15]

We next turn to bankruptcy filings, which, as discussed in Section 2, are measured as filings over the last 3 years per 1,000 people. As shown in Table 2, overall bankruptcy filings are 37% higher in the Deep South than in the Upper Midwest (19.8 vs. 14.5 per 1,000).[16] However, as shown in Panels C and D of Figure 1, these overall numbers mask large differences by chapter. Chapter 13 filing rates are almost 4 times higher in the Deep South than the Upper Midwest (10.8 vs. 2.8 per 1,000). Chapter 7 filings, on the other hand, are concentrated in a region that stretches from Michigan in the north through Indiana and

[14] As we mention in Section 1, we define the Deep South as Alabama, Arkansas, Georgia, Louisiana, Mississippi, and South Carolina, and the Upper Midwest as Iowa, Minnesota, North Dakota, South Dakota, Wisconsin, and the upper peninsula of Michigan.

[15] A map of credit card delinquencies is shown in Panel A of Appendix Figure A4.

[16] A map of overall bankruptcy rates is shown in Panel B of Appendix Figure A4.

Ohio to Kentucky and Tennessee in the south. This alternative pattern means that Chapter 7 rates are 23% lower in the Deep South than in the Upper Midwest (9.2 vs. 11.9 per 1,000). Alternatively put, while Chapter 13 accounts for 30% of bankruptcies nationwide, Chapter 13 accounts for 55% in the Deep South and only 19% in the Upper Midwest. These differences in the chapter of filing, which have been documented in prior studies, are thought to reflect lawyer networks and differences in legal traditions (Foohey et al., 2016).

The measures we construct are based on credit report data, and thus condition on individuals with a credit report. According to Brevoort, Grimm and Kambara (2016), 89% of adults have a credit report, so the averages are roughly representative of the national population. Notably, the geographic disparities we document would be even greater as measured relative to the adult population in each region. Panel B of Appendix Figure A3 shows the number of individuals with a credit report in our data as a percentage of individuals aged 20-80 calculated from the 2015 American Community Survey. As expected, our 10% sample of credit bureau data covers roughly 10% of the adult population. However, our coverage rates are higher in the Deep South than in the Upper Midwest (13.6% vs 12.2%). This implies that if we adjusted for the underlying population, our measures of financial distress would be relatively higher in the Deep South and relatively lower in the Upper Midwest, further increasing the disparities.

A natural question is whether these differences we document using July 2015 data are persistent features of these geographic areas or reflect more transitory or cyclical factors. For our key outcomes, Appendix Figure A5 plots the rank of each CZ in 2015 against the rank in 2001. For debt in collections and credit card not current, the slope coefficients are 0.89 and 0.75 respectively, indicating that a CZ ranked 100 places higher in 2001 is ranked 89 to 75 places higher in 2015. In other words, while there is variation in the absolute level of financial distress over the business cycle, the relative rank of geographic areas in the U.S. is remarkably stable over time.

For Chapter 7 and Chapter 13 bankruptcy, rank stability ranges from 0.55 to 0.74, which is strong but lower than the persistence of the collections and credit card measures. The lower persistence of the bankruptcy measures may reflect the 2005 bankruptcy reform (BAPCPA), which changed the incentives on both the extensive and chapter-of-filing

margins (Mitman, 2016; Gross et al., 2018). The differences may also reflect the fact that bankruptcy filings, and in particular Chapter 13, are more strongly related to negative housing market shocks, which have been less persistent over time.

4 Econometric Framework

In the prior section, we documented large differences in financial distress within the U.S., most notably between the Deep South and Upper Midwest. In this section, we present the econometric framework for a "movers" analysis that decomposes this variation in financial distress to place- and person-based components. The place-based component captures local institutional and economic factors – such as state level bankruptcy laws and shocks to the local economy – that have been emphasized by one branch of the literature. The person-based component captures individual characteristics – such as preference parameters (discount rates, risk preference) and behavioral factors (e.g., inattention, financial literacy) – that have been highlighted by other research.

For this analysis, we restrict our sample to individuals we observe for the entire sample window and who are between 30 and 80 years of age, inclusive, in the last period. Motivated by the CZ-level variation documented above, we also focus our analysis on individuals who move across CZs, rather than considering more local moves. Specifically, our baseline sample restricts to individuals who had exactly one cross-CZ move, with the move occurring between 2004 and 2007, inclusive. For these individuals, we can observe at least 4 years of pre-move data, which is important for examining pre-existing trends, and we can observe at least 8 years of post-move outcomes, which is enough to estimate convergence after moves.[17] The resulting sample consists of 145,805 movers, with a roughly even number of moves across years.

Let y_{it} indicate an outcome for individual i in time period t, where time is measured in quarters. Let r indicate "event time" or quarters relative to the move, with $r = -1$ indicating the last quarter in the origin and with $r = 0$ indicating the first quarter in the destination location. For each outcome and individual i, we construct our measure of the

[17] Restricting the sample to moves that occur in 2007 or earlier also avoids moves that were precipitated by the financial crisis. We discuss robustness to including these movers below.

size of the move, $\widehat{\delta_i}$, as the average difference in the outcome between non-movers in the destination and the origin.[18] For instance, an individual moving from a very low to a very high average collections region would have a large, positive $\widehat{\delta_i}$. Since we restrict to individuals with one move, an individual is associated with a single value of $\widehat{\delta_i}$ for each outcome.

In our baseline specification, we construct $\widehat{\delta_i}$ as the average difference between the outcome for non-movers in the origin and destination zipcodes. Among the moves, the median origin or destination has 3,224 non-movers. Appendix Figure A6 shows a histogram of our baseline measure of $\widehat{\delta_i}$. We discuss robustness to alternative measures of $\widehat{\delta_i}$ below.

Our baseline specification is

$$y_{it} = \alpha_i + \alpha_y + \alpha_q + \alpha_r + \left[\sum_{r \neq -1} \theta_r \cdot \widehat{\delta_i} \right] + x_{it}\beta + \epsilon_{it} \tag{1}$$

where α_i are individual fixed effects, α_y are calendar-year fixed effects, α_q are calendar-quarter fixed effects, α_r are event-time fixed effects, and x_{it} are controls for 10-year age bins.[19]

The coefficients of interest are the θ_r and are normalized to zero in the last quarter in the origin ($\theta_{-1} = 0$). With this normalization, θ_r captures the degree to which outcomes converge to those in the destination. An estimate of $\theta_r = 1$ indicates that outcomes have fully converged to those in the destination location; an estimate of $\theta_r = 0$ indicates no convergence. We calculate robust standard errors clustered by origin $\times$ destination CZ.

The identifying assumption is that, conditional on the controls, the size of the move, $\hat{\delta}_i$, is uncorrelated with any trends in the outcome. A natural concern is that people move to less expensive and potentially more distressed locations in response to persistent negative shocks to their economic circumstances (e.g., job loss) or conversely move to more economically vibrant locations in response to persistent positive shocks.

[18] In parallel to movers, we define non-movers as individuals who we observe for the entire sample window, are between 30 and 80 years of age in the last period, and never move across zipcodes.

[19] We are unable to control for fully interacted calendar-year and calendar-quarter fixed effects because of the collinearity between time fixed effects and the event-time fixed effects. This is a standard feature of these type of specifications (see, e.g., discussion in Dobbie et al. (Forthcoming)).

To provide support for our identifying assumption, Figure 2 shows binned scatter plots of the *pre-move* change in outcomes against the size of the move. Specifically, in each plot, the vertical axis shows average financial distress 1 year pre-move minus average financial distress 3 years pre-move, and the horizontal axis shows $\widehat{\delta_i}$. The data is split by ventiles of $\widehat{\delta_i}$ and each point shows the average in that bin. The plots also show the line of best fit, estimated using the underlying data, and its slope and standard error. The plots indicate that the size and direction of the move is uncorrelated with any trends in outcomes. In particular, the correlations are not statistically distinguishable from zero and are small in magnitude relative to the pre-post differences discussed below.

To provide some initial evidence for the movers effects, Figure 3 shows binned scatter plots of the *pre-post* change in outcomes against the size of the move. The plots are constructed in the same manner as before, except that the vertical axis now shows average financial distress 3 years post-move minus average financial distress 3 years pre-move. Across the measures of financial distress, there is a positive and statistically significant relationship between the size of the move and the outcome; we defer our discussion of magnitudes to the next section. The plots indicate that a linear "dose-response" relationship between the size of the move and our outcomes is a reasonable first approximation, although we will explore sensitivity to this assumption below.

We examine the robustness of our results to a number of modifications of our baseline specification. One set of robustness analysis isolates variation stemming from moves "from the same place" or moves "to the same place." For instance, the concern that effects are driven by a persistent origin-specific shock (e.g., mass layoff) can by addressed by conditioning on individuals who moved from the same origin to destinations with differential financial distress, thus generating different values of $\widehat{\delta_i}$. Similarly, the concern that effects are driven by a shock at the destination (e.g., commodity boom) can be addressed by focusing on individuals who arrived at the same destination from different origins.

Econometrically, we isolate variation stemming from moves "from the same place" by adding a full set of origin CZ $\times$ event-time fixed effects to Equation 1. We similarly estimate effects for individuals who move "to the same place" by adding a full set of destination CZ $\times$ event time fixed effects.

A second set of robustness analysis examines sensitivity to how we construct the size of the move, $\widehat{\delta_i}$. The construction of this variable involves a natural tradeoff. If we define the group of non-movers too broadly, they will not be a good proxy for the mover's experience. If we define the group of non-movers too narrowly, we will not have enough sample to reliably estimate $\widehat{\delta_i}$. As discussed above, for our baseline specification we constructed $\widehat{\delta_i}$ using non-movers who reside in the mover's destination and origin zipcodes. Appendix Table A1 shows that for this definition, the median origin or destination we use to construct $\widehat{\delta_i}$ is based on 3,224 non-movers. However, the 5th percentile has only 353 non-movers, raising concerns about measurement error, especially for low-frequency measures, such as Chapter 13 filings.

Thus, as a robustness check, we construct $\widehat{\delta_i}$ using broader geographic areas. Specifically, we construct versions of $\widehat{\delta_i}$ using non-movers in the mover's origin and destination county and CZ, respectively. For these measures, the 5th percentile of origin or destination locations has 3,664 and 16,941 non-movers, respectively, virtually eliminating concerns about measurement error (see Appendix Table A1). We also examine the sensitivity of our results to defining $\widehat{\delta_i}$ more narrowly than the baseline specification, constructing $\widehat{\delta_i}$ using non-movers in both the same zipcode and same 10-year age group as the mover. Age is the only demographic variable available in our data, which limits our ability to construct even finer measures. For the zipcode $\times$ age group measure, the median origin or destination has 748 non-movers, and the 5th percentile has 90 non-movers (see Appendix Table A1).[20]

5 Results

In this section, we present the event-study estimates from the movers analysis. We then probe the robustness of these estimates to alternative specifications and explore heterogeneity to understand the underlying mechanisms.

5.1 Event Study Estimates

Figure 4 presents event-study plots of the coefficient of interest (θ_r) by event time (r) for our main outcome variables. Table 3 shows parameter estimates and standard errors for θ_r

[20] Appendix Figures A6-A9 show histograms of these measures of $\widehat{\delta_i}$.

at 6 years post-move for our baseline and alternative specifications.

Panels A and B examine effects on debt in collections (1+ debt in collections) and credit card not current (30+ DPD, charged off, or in collections). Prior to the move, there is no evidence of an economically significant trend in θ_r, providing further support for our identifying assumption.[21] After the move, the estimates of θ_r gradually increase and then stabilize at less than 10% at 4 to 8 years.

The results imply that, for these outcomes, place-based factors account for a small fraction of the geographic variation in financial distress. With our baseline specification, shown in column 1 of Table 3, we can reject a place-based component of zero but can also reject a place-based effect of 10% at 6 years post-move.

Figure 5 further examines the effect on debt in collections by estimating effects on medical and non-medical debt in collections (1+ medical debt in collections and 1+ non-medical debt in collections). The results show that medical debt in collections is the primary determinant of the collections effect, with a place effect of approximately 20% versus 4% for non-medical collections. This is consistent with the heterogeneous collection practices of local medical providers (e.g., doctors offices and hospitals) and the debt collectors they contract with having an important impact on overall debt in collections.

Panels C and D of Figure 4 examine effects on Chapter 7 and Chapter 13 bankruptcy filings. As before, the results show no evidence of pre-trends, providing support for our identifying assumption. In contrast to the previously examined outcomes, we find economically large place-based effects for bankruptcy, with a place-based component of 27% for Chapter 7 and 33% for Chapter 13 at 6 years post-move. These results are consistent with state level bankruptcy laws (e.g., Fay, Hurst and White, 2002; Agarwal, Liu and Mielnicki, 2003; Livshits, MacGee and Tertilt, 2007; Auclert, Dobbie and Goldsmith-Pinkham, 2019) and local lawyer networks and legal traditions (Sullivan, Warren and Westbrook, 1994; Jacoby, 2014) playing an important role in bankruptcy filings.

A threat to the validity of our research design is a shock that causes people to move from a given location and has a persistent impact on their probability of financial distress.

[21] For the credit card not current variable, there is a marginally significant θ_r at some pre-period time horizons, but the effect is economically small. Adjusting for it by controlling for a pre-existing trend would make the place-based effect marginally smaller, strengthening our interpretation of the results.

The lack of pre-trends provides some evidence against this concern. To more directly rule out this threat, Column 2 of Table 3 shows estimates from a specification where we add origin CZ $\times$ event-time controls to the baseline specification. With these controls, the estimates are identified off of individuals who move "from the same place" to locations with differential financial distress, eliminating any bias from persistent origin-specific shocks. Column 3 addresses the complementary concern about bias from destination-specific shocks by adding destination CZ $\times$ event-time controls to the baseline specification. While the point estimates are not exactly the same, the patterns are very similar, with small place effects for the collections and credit card not current measures, and larger effects for the bankruptcy outcomes.

Columns 4 to 6 of Table 3 examine sensitivity to constructing the size of the move at different levels of aggregation. Column 4 constructs $\widehat{\delta_i}$ more narrowly, using all non-movers in the same zip code and same 10-year age group as the mover. Columns 5 and 6 construct $\widehat{\delta_i}$ more broadly, using all non-movers in the same county and the same CZ, respectively. The estimates using the zip-by-age-group $\widehat{\delta_i}$ are typically smaller than the baseline estimates, and are substantially smaller for bankruptcy outcomes. This is consistent with the smaller sample size leading to measurement error in $\widehat{\delta_i}$, which attenuates the estimates toward zero, with greater measurement error and attenuation for the bankruptcy outcomes due to the low rate of filing. The county and CZ level estimates tend to be modestly larger. As in the other robustness exercise, the results are qualitatively similar.

As mentioned earlier, we restrict the sample to moves that occur in 2007 or earlier to avoid including moves that were precipitated by the financial crisis. Appendix Tables A3 and A4 are based on a larger sample of moves between 2004 and 2012, and show parameter estimates and standard errors for θ_r at 4 years post-move. With a shorter post-move period, the convergence effects are slightly weaker, but the general patterns are very similar.

Taken together, this evidence reinforces the view that the general pattern of small place effects for collections and credit card not current and larger place effects for bankruptcy is robust.

5.2 Heterogeneity and Correlates

To shed light on underlying mechanisms, we examine heterogeneity and correlates of the place-based effects.

The first form of heterogeneity we examine is based on the direction of the move. We define a move as "positive" if an individual moves to a destination with higher financial distress ($\widehat{\delta_i} > 0$) and "negative" if an individual moves to a destination with lower financial distress ($\widehat{\delta_i} < 0$). Informational theories predict larger effects for positive. If, for example, individuals learn about the benefits of filing for bankruptcy when they move to places with higher filing rates (e.g., from peers or advertisements) but do not unlearn the benefits if they move to places with lower filing rates, we would expect larger place-based effects for positive moves.[22]

Columns 2 and 3 of Appendix Table A2 show estimates of the place effect for positive and negative moves. The starkest difference, in absolute value, is for Chapter 13 filing, with a place-based effect of 45% for positive moves versus 19% for negative moves. This asymmetry is consistent with the type of informational effects highlighted by previous anecdotal evidence on the importance of lawyer networks and legal traditions in the Chapter 13 filing decision (see, e.g., Sullivan, Warren and Westbrook, 1994; Jacoby, 2014).[23] There are statistically significant asymmetries for some other variables but they tend to be economically smaller and are not always robust to alternative specifications, making us hesitant to strongly interpret them.

Research using movers designs in other contexts has found larger differences by the age of the move. For example, Chetty and Hendren (2018*a*) find that children who moved neighborhoods at younger ages experienced larger place-based effects, likely because they were exposed to local schooling environment for longer. In our context, we would expect larger impacts for younger movers to the extent that younger people are responsive to

[22]Similarly, debt trap models – under which it is easier to get into financial distress than get out of it – suggest larger effects for moves to places with higher financial distress rates (positive moves). Supply driven models, on the other hand, predict larger effects for moves to places with lower financial distress. To the extent the places with lower financial distress have higher credit supply, moves to lower financial distress places raises mover's access to credit, which can lead to higher rates of default for these individuals.

[23]Note that this pattern is inconsistent with large local stigma effects (Gross and Souleles, 2002), as it is unlikely that the potentially stigmatizing aspects of filing for bankruptcy would differ by the chapter of filing.

information, more influenced by peers, or more malleable in the preferences that may contribute to financial distress. To examine the effects by age, columns 4 and 5 estimate the baseline specification separately by whether the mover is less than 40-years-old at the time of the move. Across most of the outcomes, the estimates are statistically indistinguishable for older and younger movers, and the point estimates do not suggest any clear pattern.

A standard practice in movers designs is to examine correlates of place-based effects (e.g., Finkelstein, Gentzkow and Williams, 2016; Chetty and Hendren, 2018*b*). Because a number of the correlates vary at the state level (e.g., bankruptcy exemptions), we conduct analysis at the state level. We recover state-level place-based effects by running two-way fixed effects regressions of our outcomes on individual and state fixed effects. We then project the state-level fixed effects on state-level legal factors (e.g., measures of the generosity of bankruptcy laws, wage garnishment levels) and state-level economic factors (e.g., median income, house values, employment).

In Appendix Section B, we provide more details on the regression specification and the results. To summarize our findings, we do not find any consistent patterns in this correlational analysis. This might arise from the fact that many of our measures have theoretically ambiguous effects on financial distress. For instance, more consumer-friendly bankruptcy laws could increase bankruptcy filings, because they make bankruptcy more beneficial to filers, or reduce them, because lenders endogenously respond by reducing the supply of credit. Similarly, better economic conditions might directly reduce financial distress but also increase borrowing amounts, offsetting the direct effect. Indeed, in the cross section, bankruptcy filing rates are weakly inversely U-shaped in zipcode income.[24] The absence of any correlations may alternatively reflect the more standard critique that there may be unobserved factors that are correlated with these local characteristics, clouding the interpretation of the estimates.

6 Conclusion

In this paper, we use monthly credit report data for a representative 10% panel of individuals over 2000-2016 to examine financial distress in the United States. We document large,

[24]See Appendix Figure A17.

persistent geographic differences in financial distress across regions, with a particularly stark disparity between the Upper Midwest and the Deep South.

To better understand these patterns, we conduct a movers analysis that examines how financial distress evolves when people move to areas with different levels of financial distress. For debt in collection and our credit card outcomes, we find only weak convergence following a move, while for bankruptcy we find fairly large place-based effects.

These findings are helpful in weighing competing theories of financial distress. The small place-based component for debt in collections and credit card not current indicates that supply-side factors (e.g., state laws, local lending practices) are not a primary explanation for the geographic variation in distress, and instead suggests an important role for persistent individual characteristics in explaining the geographic variation. Such characteristics include financial literacy and human capital, household wealth and intergenerational transfers, and variation across individuals in risk preferences, default stigma, or discount rates.

In contrast, the larger place-based estimates for bankruptcy, and in particular the large place-based effects for positive moves for Chapter 13 filings, is consistent with an informational theory whereby individuals learn about Chapter 13 when they move to places with high filing rates but do not unlearn when they move to places with low rates. This evidence supports prior anecdotal evidence on the importance of local lawyer networks and legal traditions in driving Chapter 13 filing decisions.

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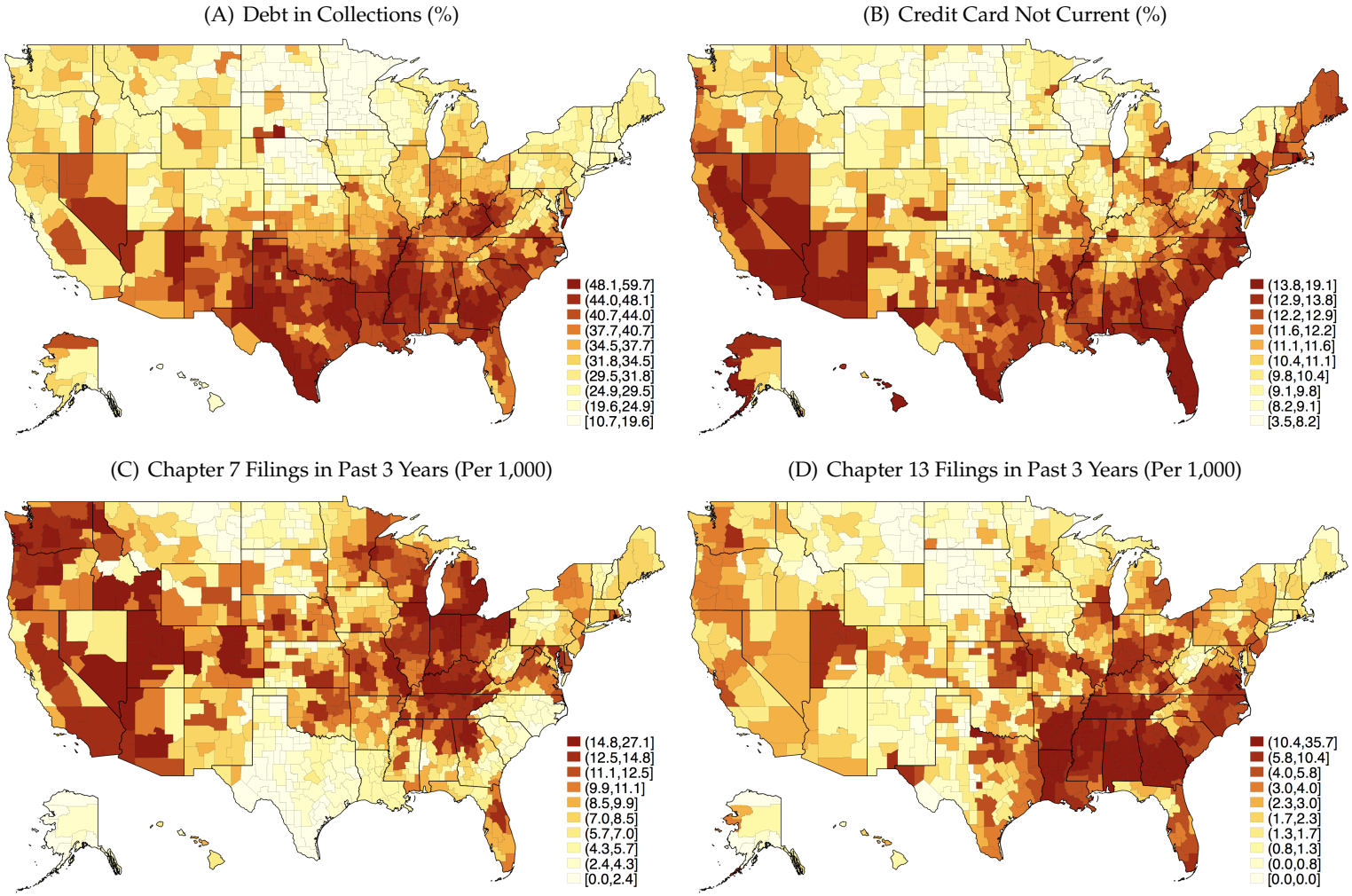
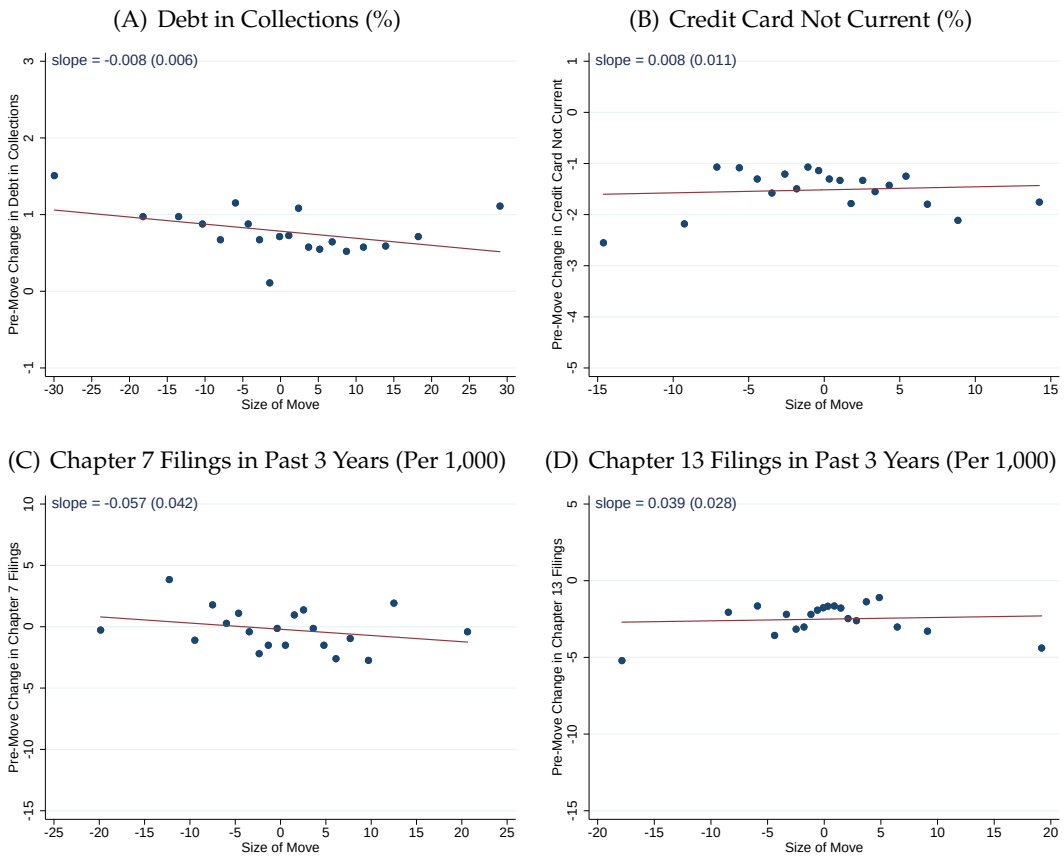
Figure 1: Geographic Variation in Financial Distress



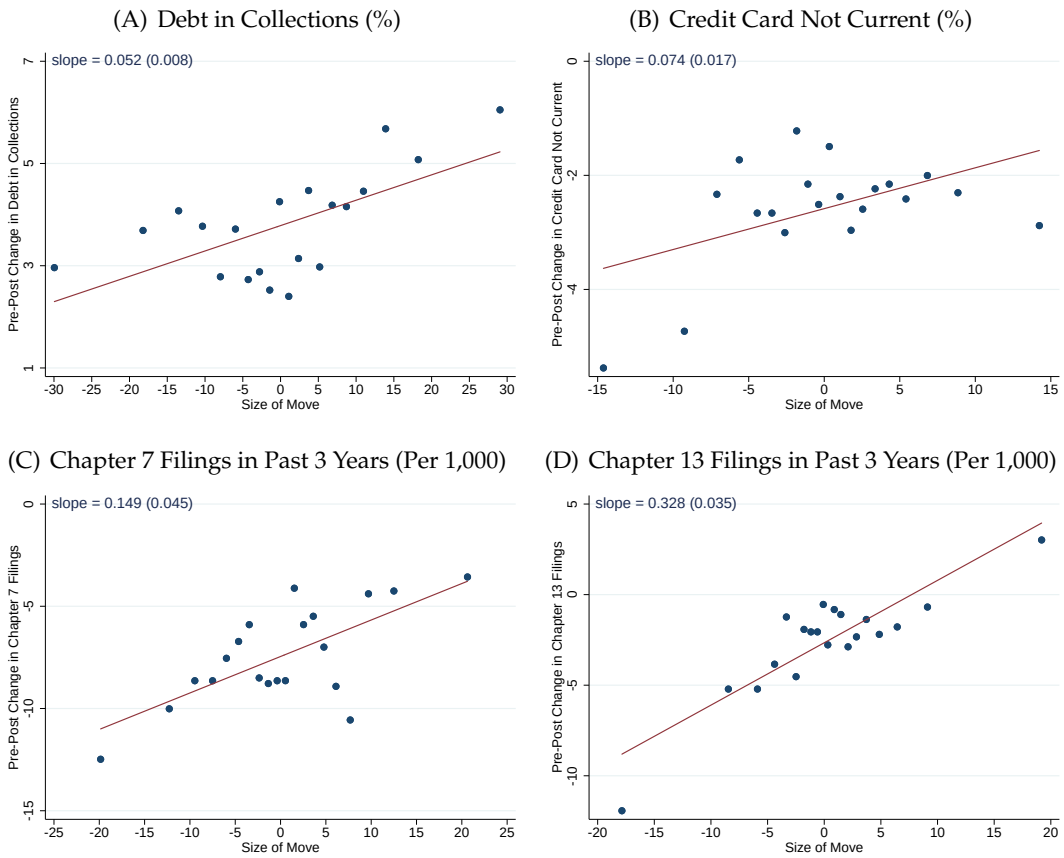
Note: Figure shows CZ-level maps of financial distress. CZ means are constructed using a 10% random sample of TransUnion credit records from June 2015. Debt in collections is an indicator for 1+ debt in collections. Credit card not current is an indictor for 1+ credit card that is 30+ DPD, charged off, or in collections. Bankruptcy filings are the number of individuals out of 1,000 who file for chapter 7 and 13, respectively, in the last 3 years. See Section 2 for more details on variable construction.

Figure 2: Pre-Move Change in Financial Distress by Size of Move



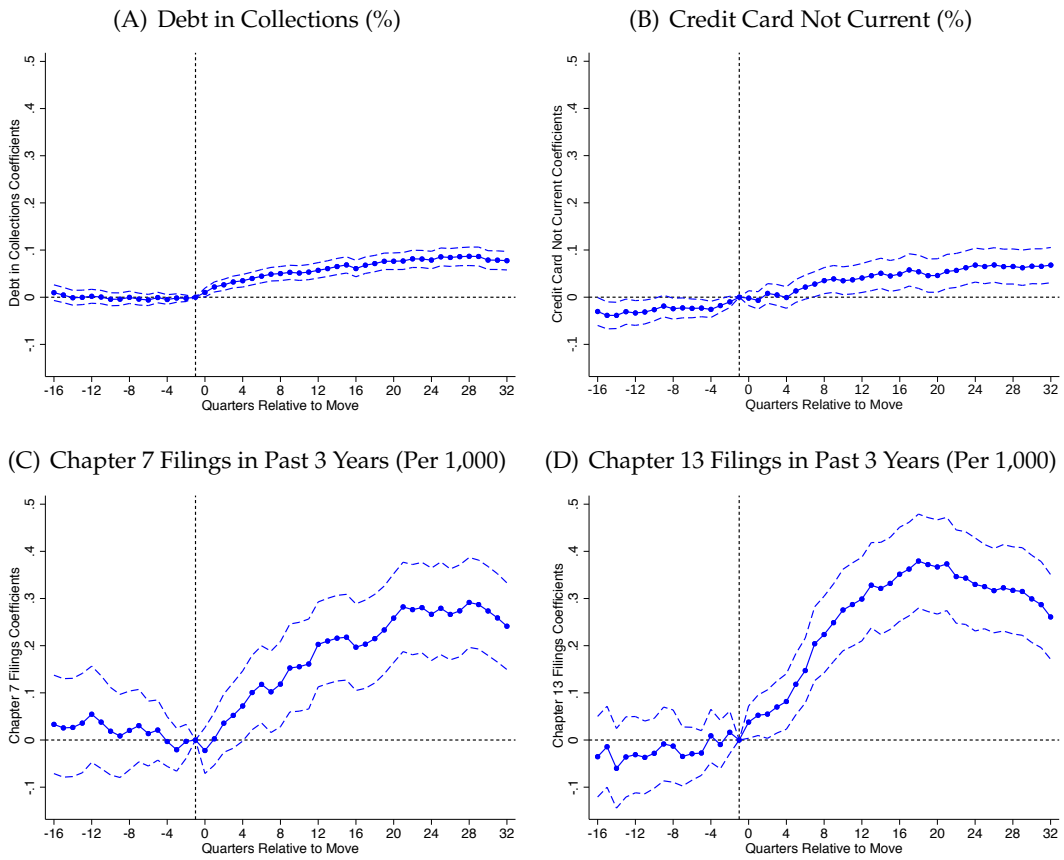
Note: Figure shows binned scatter plots of the *pre*-move change in the outcome against the size of the move $\hat{\delta}_i$. The vertical axis shows average financial distress 1 year pre-move minus average financial distress 3 years pre-move. The horizontal axis shows $\widehat{\delta_i}$, the average financial distress between destination and origin zip code. The data is split by ventiles of $\widehat{\delta_i}$ and each point shows the average in that bin. The plots also show the line of best fit, estimated using the underlying data, and its slope and standard error.

Figure 3: Pre-Post Change in Financial Distress by Size of Move



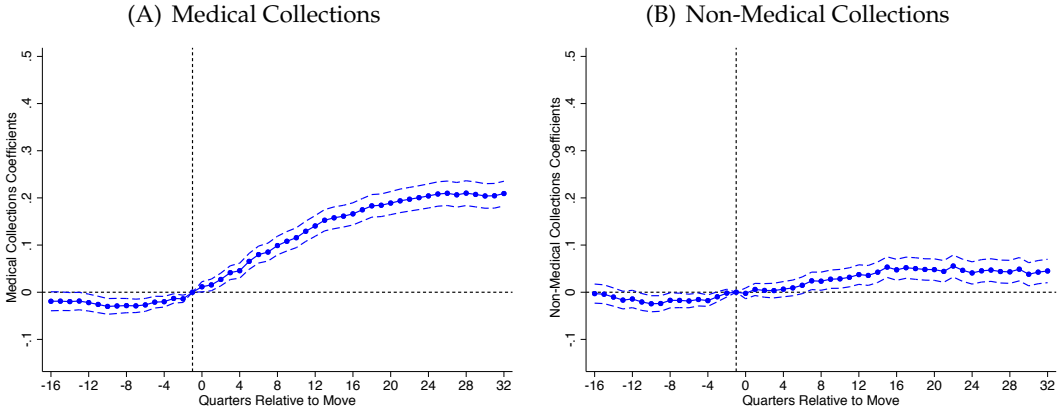
Note: Figure shows binned scatter plots of the *pre-post* change in the outcome against the size of the move $\hat{\delta}_i$. The vertical axis shows average financial distress 3 years post-move minus average financial distress 3 years pre-move. The horizontal axis shows $\widehat{\delta_i}$, the average financial distress between destination and origin zip code. The data is split by ventiles of $\widehat{\delta_i}$ and each point shows the average in that bin. The plots also show the line of best fit, estimated using the underlying data, and its slope and standard error.

Figure 4: Event-Study Plots



Note: Figure shows place-based effects θ_r from event study regressions of financial distress on the size of the move $\hat{\delta}_i$, individual and time fixed effects, and other controls. The dash lines show 95% confidence intervals, based on standard errors clustered by origin $\times$ destination CZ.

Figure 5: Event-Study Plots: Medical and Non-Medical Collections



Note: Figures shows place-based effects θ_r from event study regressions of financial distress on the size of the move $\hat{\delta}_i$, individual and time fixed effects, and other controls. The dash lines show 95% confidence intervals, based on standard errors clustered by origin $\times$ destination CZ.

Table 1: Financial Distress Measures

	Mean	Std. Dev.	Median	Pct 75	Pct 90	Pct 95	Pct 99
Collections							
Debt in collections (%)	34.1	47.4	0	100	100	100	100
Medical collections (%)	22.1	41.5	0	0	100	100	100
Non-medical collections (%)	24.2	42.8	0	0	100	100	100
Collections balance ($)	1,434	126,675	0	606	3,401	6,748	21,234
Medical collections balance ($)	750	6,090	0	0	1,245	3,178	13,770
Non-medical collections balance ($)	684	126,521	0	0	1,445	3,218	10,993
Credit Card							
Credit card not current (%)	12.5	33.1	0	0	100	100	100
Credit card not current (%, cond'l)	17.5	38.0	0	0	100	100	100
Credit card delinquent (%)	2.6	15.9	0	0	0	0	100
Credit card delinquent (%, cond'l)	3.6	18.7	0	0	0	0	100
Bankruptcy							
Bankruptcy filings in past 3 years (per 1,000)	14.8	120.9	0	0	0	0	1,000
Chapter 7 filings in past 3 years (per 1,000)	10.5	101.8	0	0	0	0	1,000
Chapter 13 filings in past 3 years (per 1,000)	4.5	67.2	0	0	0	0	0
N	29,398,845						

Note: Table presents summary statistics for measures of financial distress constructed using a 10% random sample of TransUnion credit records from June 2015. Debt in collections measures are indicators for 1+ debt in collections. Credit card not current is indictor for 1+ credit card that is 30+ DPD, charged off, or in collections. Credit card delinquency is an indicator for 1+ credit card 30+DPD (but not charged off or in collections). Bankruptcy filings are the number of individuals out of 1,000 who file for any bankruptcy, chapter 7, and chapter 13, respectively, in the last 3 years. See Section 2 for more details on variable construction.

Table 2: Geographic Variation in Financial Distress

	(1) Mean	(2) Std.Dev.	(3) 75% - 25%	(4) 90% - 10%	(5) Deep South	(6) Upper Midwest	(7) Deep South - Upper Midwest
Collections							
Debt in collections (%)	34.1	7.9	11.5	20.8	43.5	23.8	19.8
Medical collections (%)	22.1	8.1	11.5	22.5	31.6	14.4	17.2
Non-medical collections (%)	24.2	5.2	7.1	14.1	29.1	17.4	11.7
Collections balance ($)	1,434	570	648	1,233	1,792	1,033	759
Medical collections balance ($)	750	411	523	1,037	1,140	590	549
Non-medical collections balance ($)	684	352	338	539	652	443	209
Credit Card							
Credit card not current (%)	12.5	2.0	2.4	4.8	13.4	8.9	4.5
Credit card not current (%, cond'l)	17.8	3.5	5.3	8.6	21.4	11.6	9.8
Credit card delinquent (%)	2.6	0.3	0.5	0.8	2.7	2.1	0.6
Credit card delinquent (%, cond'l)	3.6	0.5	0.6	1.3	4.3	2.7	1.6
Bankruptcy							
Bankruptcy filings in past 3 years (per 1,000)	14.8	6.4	8.7	17.8	19.8	14.5	5.3
Chapter 7 filings in past 3 years (per 1,000)	10.5	4.6	7.5	12.1	9.2	11.9	-2.6
Chapter 13 filings in past 3 years (per 1,000)	4.5	4.1	2.9	7.7	10.8	2.8	8.1

Note: Table presents statistics on CZ-level measures of financial distress. CZ means are constructed using a 10% random sample of TransUnion credit records from June 2015. Summary statistics are calculated using the CZ level data, with CZs weighted by the number of individual-level observations in each CZ so means are representative of the underlying individual-level data. The Deep South is defined as Alabama, Arkansas, Georgia, Louisiana, Mississippi, and South Carolina and Upper Midwest as Iowa, Minnesota, North Dakota, South Dakota, Wisconsin, and the upper peninsula of Michigan. See 1 note for more details on the financial distress measures.

Table 3: Event-Study Estimates

	Fraction converged to outcome at destination 6 years post move					
Financial distress measures	(1) All movers	(2) Same origin	(3) Same destination	(4) Zip × age level	(5) County level	(6) CZ level
Debt in collections	0.0788	0.0536	0.0611	0.0659	0.1539	0.2231
	(0.0095)	(0.0095)	(0.0097)	(0.0081)	(0.0141)	(0.0174)
Medical collections	0.2042	0.1291	0.1908	0.1575	0.3036	0.3361
	(0.0130)	(0.0131)	(0.0133)	(0.0110)	(0.0164)	(0.0187)
Non-medical collections	0.0409	0.0213	0.0337	0.0490	0.0956	0.1708
	(0.0120)	(0.0122)	(0.0123)	(0.0103)	(0.0187)	(0.0249)
Credit card not current	0.0633	0.0611	0.0482	0.0487	0.1583	0.2244
	(0.0180)	(0.0182)	(0.0183)	(0.0136)	(0.0308)	(0.0409)
Chapter 7 filings (3 years)	0.2665	0.2088	0.2212	0.0863	0.5068	0.6254
	(0.0500)	(0.0501)	(0.0509)	(0.0325)	(0.0897)	(0.1139)
Chapter 13 filings (3 years)	0.3298	0.3578	0.2095	0.1097	0.4940	0.5115
	(0.0503)	(0.0522)	(0.0535)	(0.0422)	(0.0625)	(0.0707)

Note: Table shows place-based effects at 6 years (24 quarters) after post move. Specifically, we report θ_{24} from event study regressions of financial distress on the size of the move $\hat{\delta}_i$, individual and time fixed effects, and other controls. Standard errors, clustered by origin × destination CZ, are shown in parentheses. Column (1) shows the baseline specification. In column (2), we isolate variation from moves "from the same place" by including fully interacted origin CZ × event time fixed effects. In column (3), we isolate variation from moves "to the same place" by including fully interacted destination CZ × event time fixed effects. In columns (4) to (6), we show alternative specifications where we construct $\hat{\delta}_i$ using non-movers in the movers' origin and destination zip code × 10 year age-bin, county and CZ.

What Determines Consumer Financial Distress? Place- and Person-Based Factors

Online Appendix

Benjamin J. Keys Neale Mahoney Hanbin Yang

A Variable Definitions

Debt in collections: We use the line-item information from the collection segment to construct the indicator of whether an individual has at least one debt in collections. We rely on the Legislative Prohibited Code to construct measures for whether an individual has medical and non-medical debts in collections. Because collection accounts are not automatically removed from credit reports when they are paid, we drop accounts that have been charged off or paid in full. We rely on the Manner of Payment (MOP) code to separate accounts that are in collection (9B), charged off (9) or paid in full (9P). Our collection measure only includes 9B. We also drop accounts with original balance smaller than $100 to avoid small-dollar nuisance accounts. Accounts with original balances smaller than $100 are similarly ignored by FICO Score 8.[25] Across unpaid collection accounts, around 2% are disputed by customers with code (AID). Dropping these accounts have negligible impact on the fraction of individuals with debt in collections, and so we do not make this adjustment.

Collections balances: To measure the amount of debt in collections, we compute the total collection balance amount of all collections, medical collections, and non-medical collections owned by each individual. Again, we only include collection accounts not yet paid in full with original balance greater than $100.

Credit card not current: We use the line-item information from the trade segment to construct this indicator of whether an individual has at least one credit card that is not current. The "not current" status includes accounts that are 30 days or more past due (30+ PDP), charged off, or in collections. We identify the status of credit card accounts using their MOP codes. MOP codes of 2, 3, 4, and 5 indicate that the credit card account is 30 DPD, 60 DPD, 90 DPD, and 120+ DPD, respectively. MOP code 7 is for wage earner or similar plan. MOP codes starting with number 8 indicate that the account is currently a repossession or was a repossession but paid in full, although this code is very rare. MOP codes starting with number 9 indicates that the account is currently in collection, charged off, or was a collection but paid in full.

This variable measures financial distress from the perspective of consumers, since chargeoffs and collections can remain on consumer credit reports for up to 7 years. We

[25] See `https://www.myfico.com/credit-education/credit-scores/fico-score-versions`.

construct this measure for all individuals with a credit report and conditional on those with a credit card.

Credit card delinquent: For some of our analysis, we also consider an alternative credit card delinquency measure, which indicates whether the individual has at least one credit card that is 30 days or more past due (30+ DPD) but has *not* been classified as charged off or in collections. Similar to credit card not current, we use MOP code 2, 3, 4, and 5 to select credit cards that are past due but not in collection or charged off.

This variable measures financial distress from the perspective of credit card issuers (e.g., banks). Issuers typically charge off accounts when they are roughly 180 days past due (180+ DPD). At this point, the account is no longer a liability to the issuer and therefore should not be counted as contributing to an issuer's financial risk.

Bankruptcy: We use the line-item information from the public record segments. We use public record type code to identify bankruptcy filings and the chosen chapter (i.e. 7 vs 13). The public record segment also includes the date of filing, so we can restrict to filings in the past 3 years to focus on relatively recent distress conditions. Since bankruptcy filings are not nearly as common as other measures of distress above, we use the 3-year window to smooth over noise in more high-frequency measures.

B Correlates of Place-Based Effects

We examine correlates of our place-based effects following the methodology in Finkelstein, Gentzkow and Williams (2016). As in our baseline movers analysis, we restrict the sample to individuals who we observe for the entire sample window and who are between 30 and 80 years of age, inclusive, in the last period. We further restrict the samples to individuals who did not move at all (non-movers) or moved only once across commuting zones (movers) between 2000 and 2016. The resulting sample contains 571,380 movers.

As before, let i indicate individuals, t indicate time measured in quarters, and r indicate event time. For a given outcome y_{it}, we recover state-level place-based effects with regressions of the form

$$y_{it} = \alpha_i + \alpha_y + \alpha_q + \alpha_r + \gamma_s + x_{it}\beta + \epsilon_{it},$$

where α_i are individual fixed effects, α_y are calendar-year fixed effects, α_q are calendar-quarter fixed effects, α_r are event-time fixed effects, and x_{it} are controls for 10-year age bins.

The coefficients of interest are the state fixed effects, γ_s, and can be interpreted as the causal effect of residing in state s on the given measure of financial distress. The state fixed effects are identified off individuals who move across states in our sample. The identifying assumption is that moves across states are uncorrelated with trends in the outcome.

We focus our analysis on the state fixed effects based on the medical debt in collections and bankruptcy outcome variables. These are the outcomes where we find economically significant place effects, and where there is the potential to detect meaningful correlations. For these outcomes, we project the state-level place-based effects, $\widehat{\gamma}_s$, on state-level legal and economic factors.

For legal factors, we consider median seizable assets, wage garnishment levels, Chapter 7 fees, and Chapter 13 fees. Median seizable assets are a state-level measure of the assets above the Chapter 7 bankruptcy exemptions in that state for the median household from a nationally representative distribution, and are taken from Mahoney (2015). Wage garnishment is the percentage of a borrower's disposable income that can be withheld to pay back creditors, which we obtain from `nolo.com`. [26] Chapter 7 and Chapter 13 bankruptcy fees are taken from Lupica (2012). To construct state-level measures, we aggregate the judicial district level fees to the state level, weighting by number of observations in our sample, and then we average fees before and after the Bankruptcy Abuse Prevention and Consumer Production Act (BAPCPA), weighting by number of years that our sample falls into the pre- and post-BAPCPA periods.

For economic factors, we use state-level measures of median income, the percentage who own a house, median house value, percent who own a vehicle, the percentage with a bachelors degree or more, the percentage employed, and the percentage with health insurance. All of these variables are constructed from the 2011-2015 5-year sample of the American Community Survey (ACS). We also use a state-level measure of for-profit hospitals, defined as the share of for-profit hospitals weighted by bed count, from the 2014 Provider of Services (POS) file provided by the Centers for Medicare & Medicaid Services (CMS).

To conduct the analysis, we standardize all variables to have a mean of zero and a standard deviation of one. Then, following Finkelstein, Gentzkow and Williams (2016), we examine the correlation between the normalized place-effects and covariates using two approaches. First, for a given outcome, we estimate separate bivariate regressions of the place-effects on each covariate. The coefficient from these regressions is the Pearson correlation coefficient. It takes on values between -1 and $+1$ and its square is the r^2 of the regression. Second, we report coefficients from a post-Lasso multivariate regression. For a given outcome, we run a Lasso regression of the place effects on the full set of covariates, with the penalty level chosen by a 4-fold cross-validation to minimize the mean squared error. Then, for that outcome, we run an OLS regression on the set of covariates chosen by the Lasso regression.

Figures A10 to A12 examine correlates with the medical debt, Chapter 7 bankruptcy and Chapter 13 bankruptcy place effects. In each figure, the left panel shows the coeffi-

[26] The federal limit for wage garnishment is 25%, with some states imposing a lower limit. These limits usually apply for debts other than child support, taxes, and student loans.

cients from the separate bivariate regressions and the right panel shows the coefficients from the multivariate regression with the covariates chosen by a Lasso regression. The results are hard to interpret. While there are correlations that are consistent with existing theories, there are also patterns that suggest important omitted variables. For instance, the strong correlation between wage garnishment and Chapter 7 bankruptcy place effects is consistent with theories that wage garnishment "pushes" people into bankruptcy. On the other hand, the fact that there is a correlation between for-profit hospitals and the Chapter 13 filing place effects, but not the Chapter 7 filings place effects, strikes us as spurious. In our view, the results are due to the fact that places with more for-profit hospitals, which are typically in the Deep South, also have distinctive legal cultures which drive the correlations.

To potentially improve interpretation, we conduct additional analysis of the bankruptcy place effects that allows us to separately examine the extensive margin and the chapter of filing margin. Figure A13 shows results for a combined bankruptcy outcome, which captures the extensive margin decision. In the bivariate regressions, wage garnishment is associated with high bankruptcy place effects, while Chapter 7 filing fees are associated with lower bankruptcy place effects. The local economic factors have mixed effects. This is consistent with the fairly weak, inverse-U-shaped cross sectional relationship between bankruptcy rates and zip code income.[27] Figure A14 and A15 show results for the Chapter 7 and Chapter 13 place effects, including the combined bankruptcy place effect as an additional covariate, so that the resulting partial correlations can be interpreted as the effects on the chapter of filing. As in the prior analysis, the results are hard to interpret.

C Flag Removal Event Study

Our credit card not current variable includes chargeoffs and unpaid debt in collections that are up to 7 years old. To assess whether a 7-year-old flag still matters for consumer access to credit, we conduct event study analysis where we examine the effect of the legislatively required removal of these flags at a 7-year time horizon on credit scores.

We use the 10% sample from TransUnion at quarterly frequency from Q3 2000 to Q3 2016. We restrict the sample to individuals with valid credit scores in all quarters. We also require individuals to be between 30- and 80-years-old at the end of the panel.

We study the removal of flags for whether the account is charged off, in collections, or other related category.[28] For these categories, we define flag removal as occurring when the number of flagged credit card accounts decreases from a positive number in the previous quarter to zero in the current quarter.

We include six quarters before and after the flag removal to capture any pre-trends and to measure the persistence of the flag removal effect. To avoid potential confounds,

[27] See Appendix Figure A17.

[28] Specifically, we include MOP code 8, 8A, 8D, 8P, 8R, 9, 9B, 9P.

we only consider events when there is no other change in the number of flagged credit card accounts during the six quarters before and after the removal.

To facilitate the analysis, we aggregate together individuals who have their flags removed in the same calendar quarter. Let i denote these flag-removal cohorts, t calendar time measured in quarters, and s event time measured in quarters, which we normalize to 0 in the quarter of flag removal. Letting y_{it} denote credit scores, we estimate event study regressions of the form

$$y_{it} = \gamma_i + \gamma_t + \theta \cdot s + \left[\sum_{s \geq 0} \delta_s \right] + \epsilon_{it} \tag{2}$$

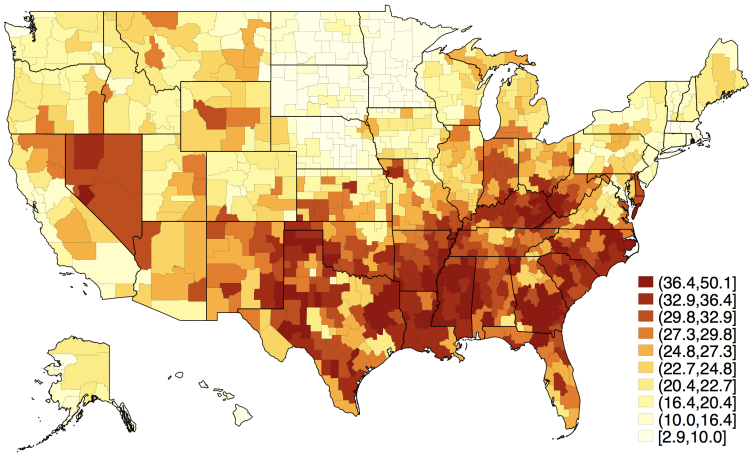
where γ_i are cohort fixed effects, γ_t are calendar time fixed effects, and θ is a linear trend in event-time s. The coefficients of interest δ_s capture the incremental effect of flag removal on credit scores, net of the linear trend. Standard errors are clustered at individual level.

Figure A16 shows the results of this analysis. The dashed line corresponds to the event time trend θ. The solid line is constructed by adding the coefficients δ_s to this trend. The dashed blue lines represent 95% confidence intervals. In the top left corner, we display the immediate removal effect on credit score (δ_0).

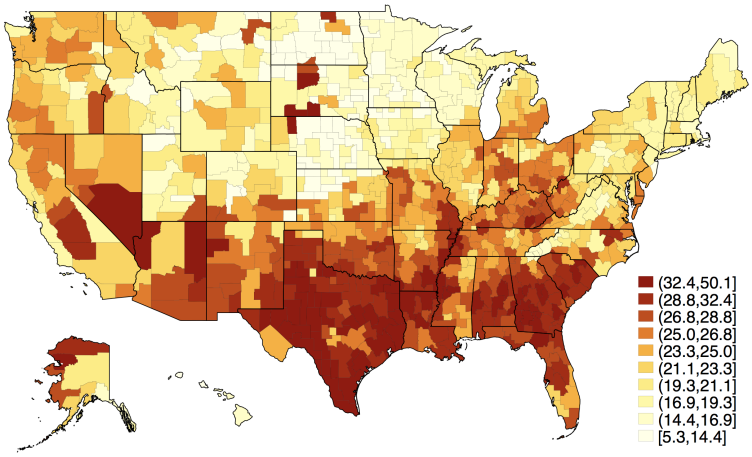
The plot shows a sharp, highly significant 5-point increase in credit scores. Relative to the linear counterfactual, the increase is fairly persistent over the 6 quarters after flag removal. This analysis indicates that the chargeoffs and unpaid debt in collections included in our credit card not current measure of financial distress are important for access to credit, as the removal of these flags increases credit scores and thus access to loans.

Figure A1: Geographic Variation in Medical and Non-Medical Debt in Collections

(A) Medical Collections (%)



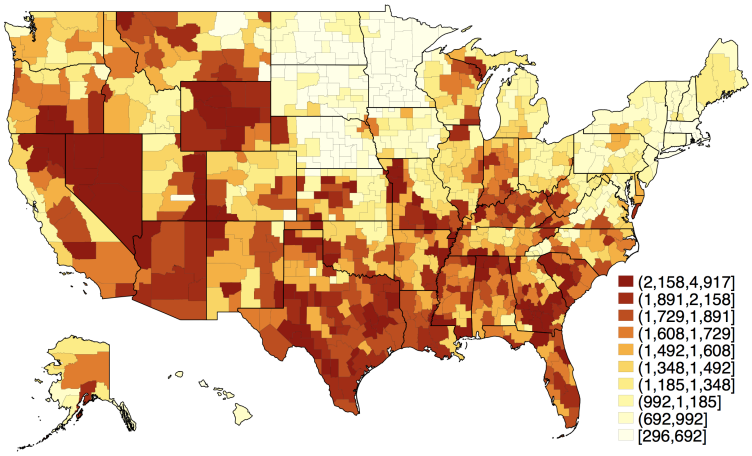
(B) Non-Medical Collections (%)



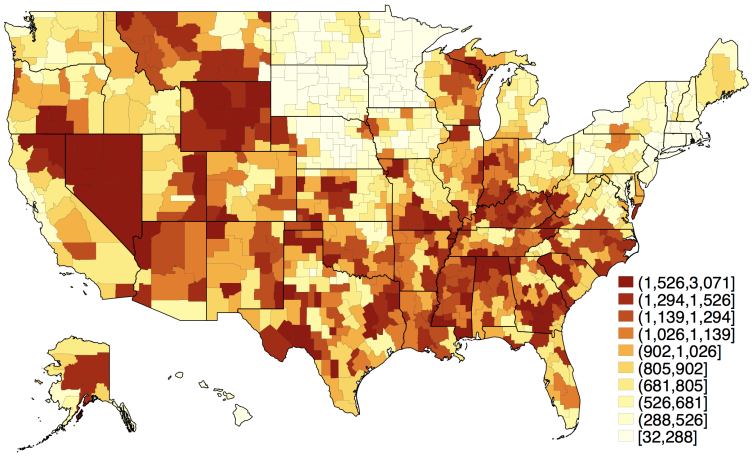
Note: Figure shows maps based on data aggregated to the commuting zone (CZ) level. CZ means are constructed using a 10% random sample of TransUnion credit records from June 2015. Medical collections is an indicator for 1+ medical debt in collections. Non-medical collections is an indicator for 1+ non-medical debt in collections. See Section 2 for more details on variable construction.

Figure A2: Geographic Variation in Collections Balances

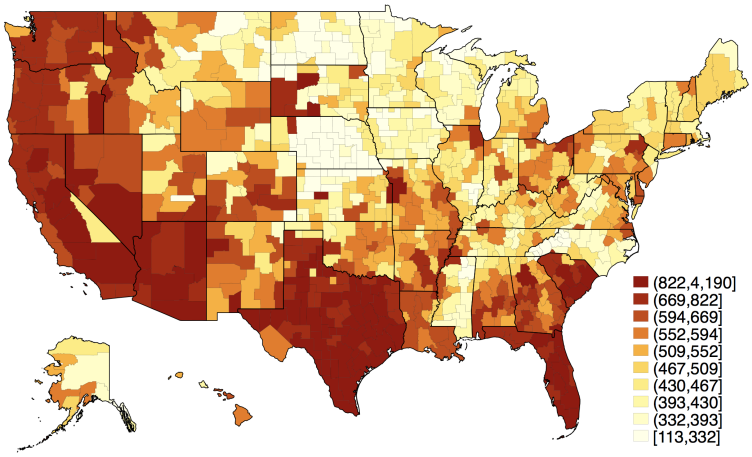
(A) Collection Balance ($)



(B) Medical Collection Balance ($)



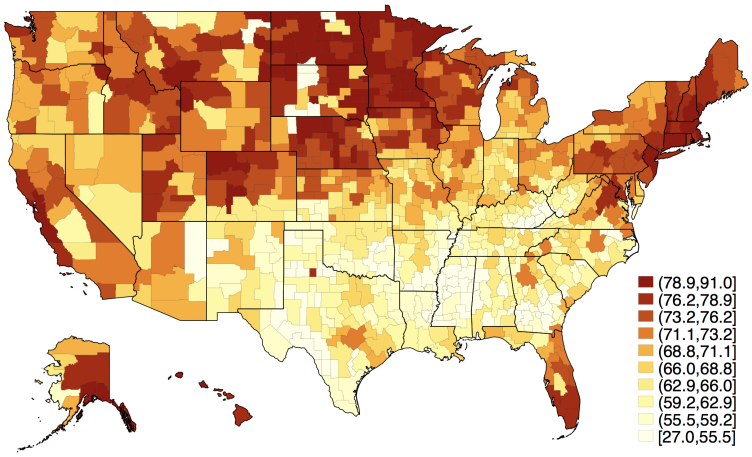
(C) Non-Medical Collection Balance ($)



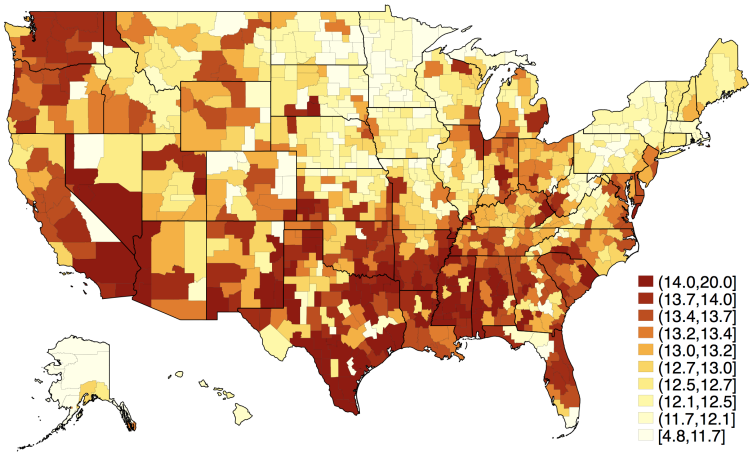
Note: Figure shows maps based on data aggregated to the commuting zone (CZ) level. CZ means are constructed using a 10% random sample of TransUnion credit records from June 2015. Collections balance is the average balance of debt in collections. Medical and non-medical collections balances are the average balances of medical and non-medical debt in collections. See Section 2 for more details on variable construction.

Figure A3: Geographic Variation in Percent with Credit Cards and Credit Reports

(A) Percent with Credit Card



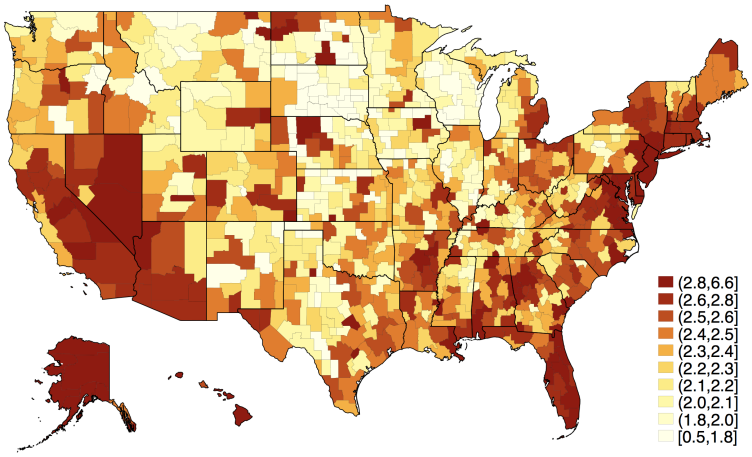
(B) Percent of Population with Credit Report



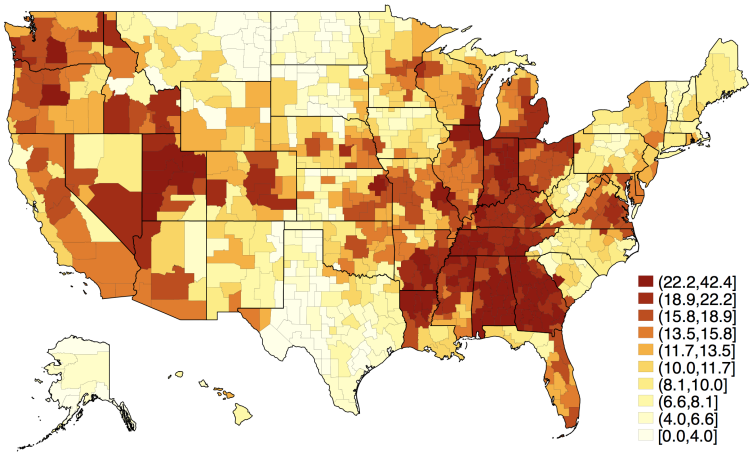
Note: Figure shows maps based on data aggregated to the commuting zone (CZ) level. Panel A shows the percent of individuals with a credit card in our credit report data. Panel B shows the ratio of observations in our credit report data to the the total population between 20 and 80 reported in the 2015 American Community Survey. The credit report data are constructed using a 10% random sample of TransUnion credit records from June 2015, aggregated to the Commuting Zone level.

Figure A4: Geographic Variation in Delinquency and Bankruptcy Filings

(A) Credit Card Delinquency (%)

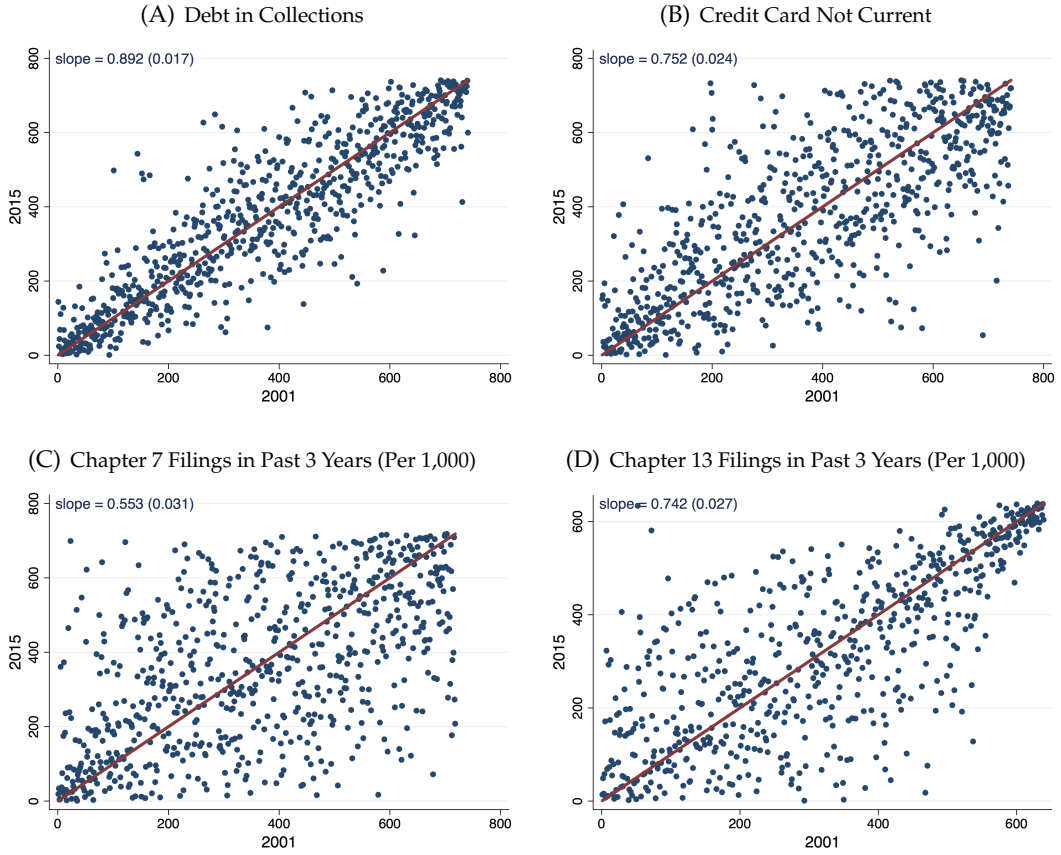


(B) Bankruptcy Filings in Past 3 Years (per 1,000)



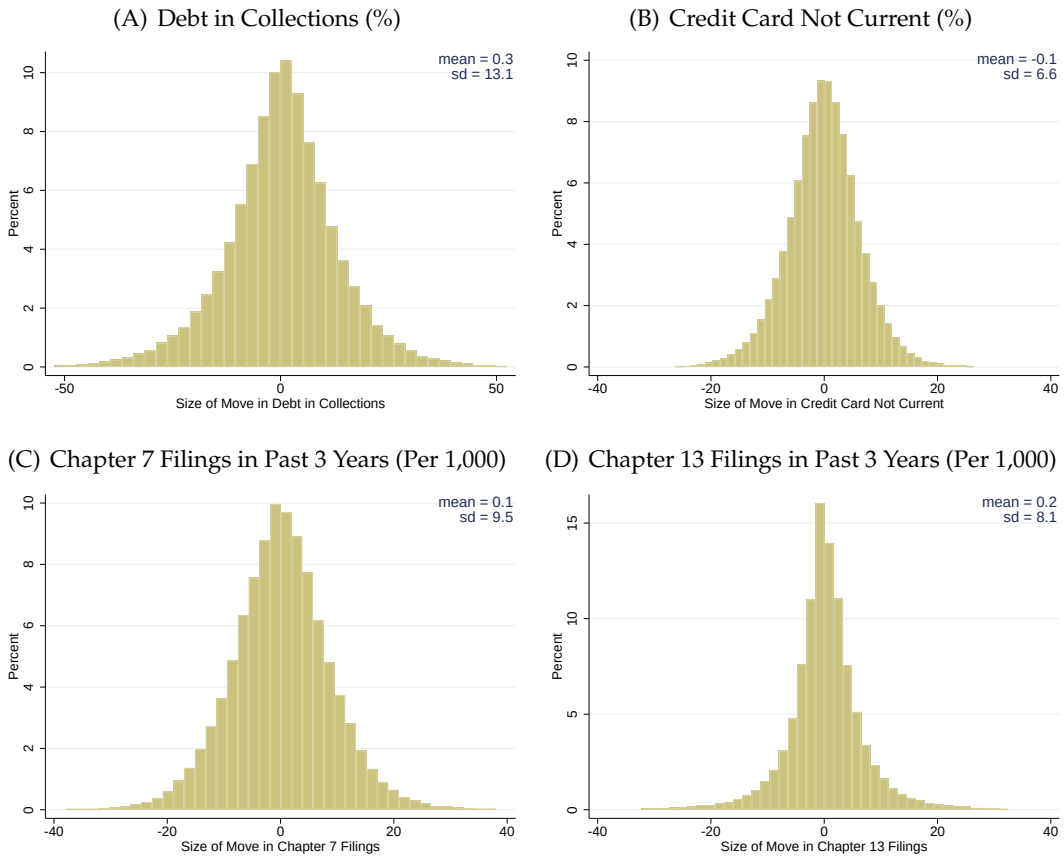
Note: Figure shows maps based on data aggregated to the commuting zone (CZ) level. CZ means are constructed using a 10% random sample of TransUnion credit records from June 2015. Credit card delinquency is an indicator for 1+ credit card (30+DPD but not charged off or in collections).

Figure A5: Rank Stability in Financial Distress



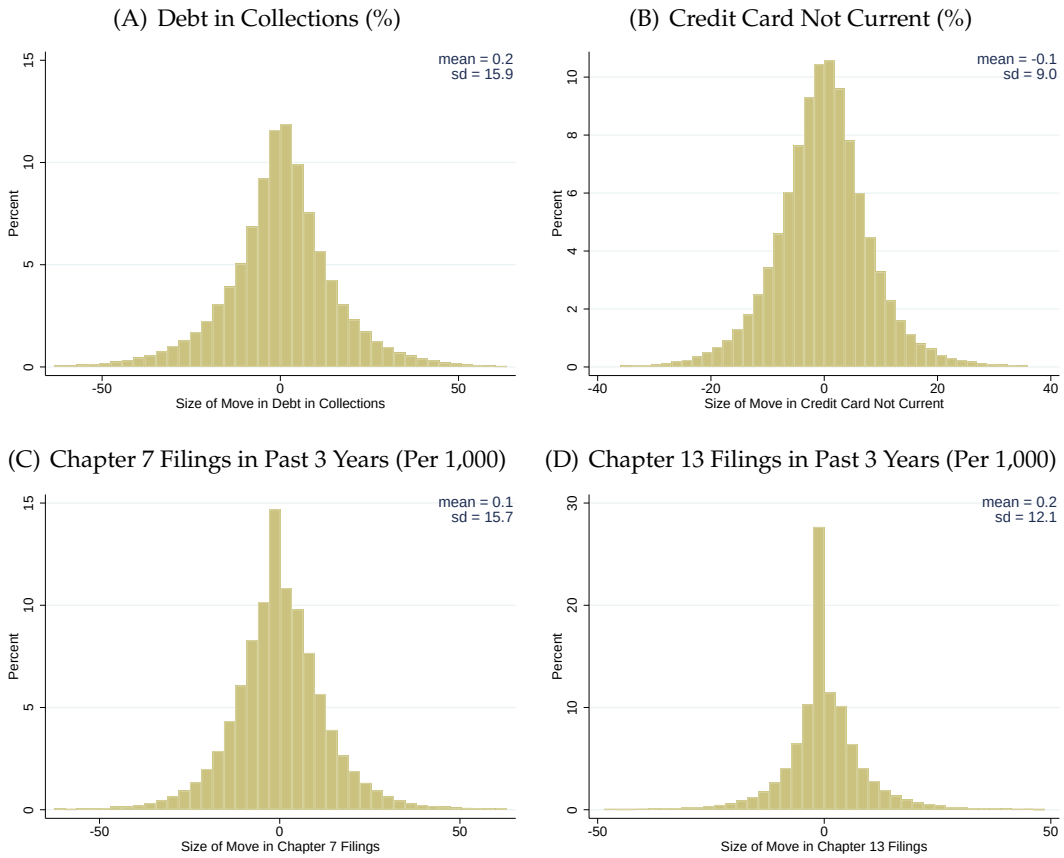
Note: Figures show scatter plots of the ordinal ranking of commuting zones in 2015 versus 2001 for our financial distress measures. The 45 degree line is shown in red.

Figure A6: Histogram of Size of Move $\widehat{\delta_i}$ at Zip Code Level



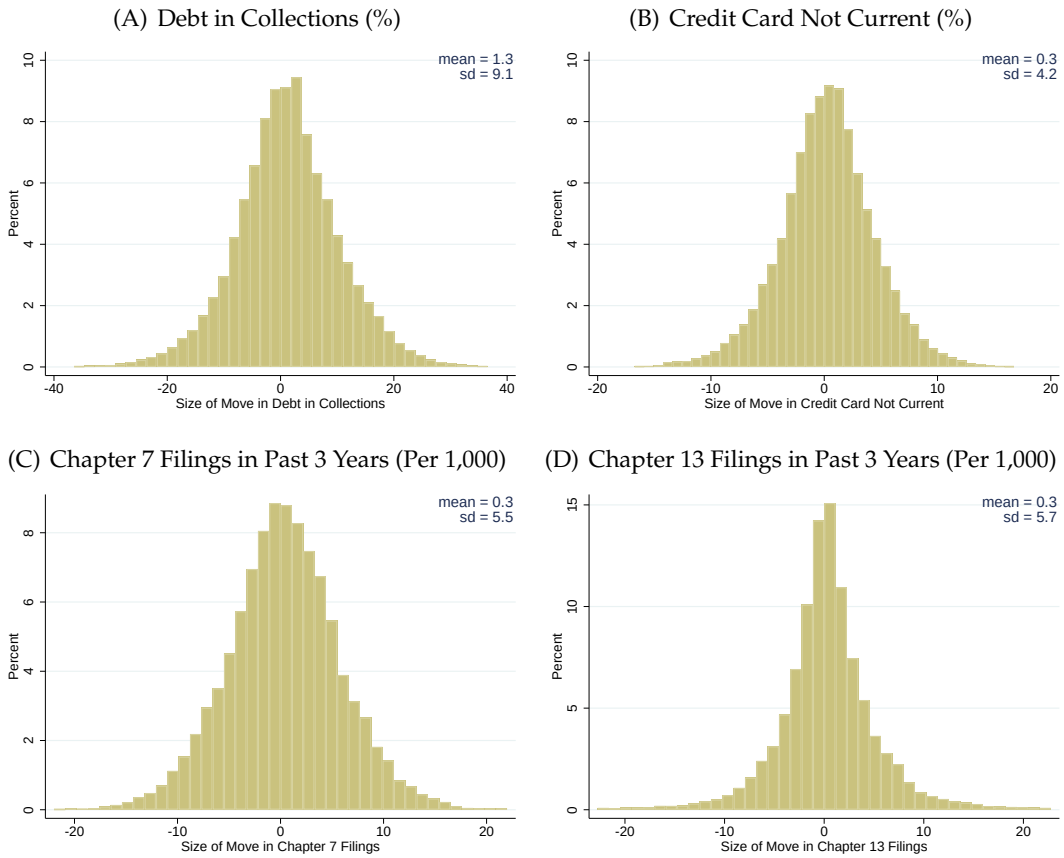
Note: Figure shows histogram of size of move $\widehat{\delta_i}$ computed at zip code level. We keep $\widehat{\delta_i}$ that are within $\pm$ 4 standard deviations from the mean.

Figure A7: Histogram of Size of Move $\widehat{\delta_i}$ at Zip Code $\times$ 10 Year Age Bin Level



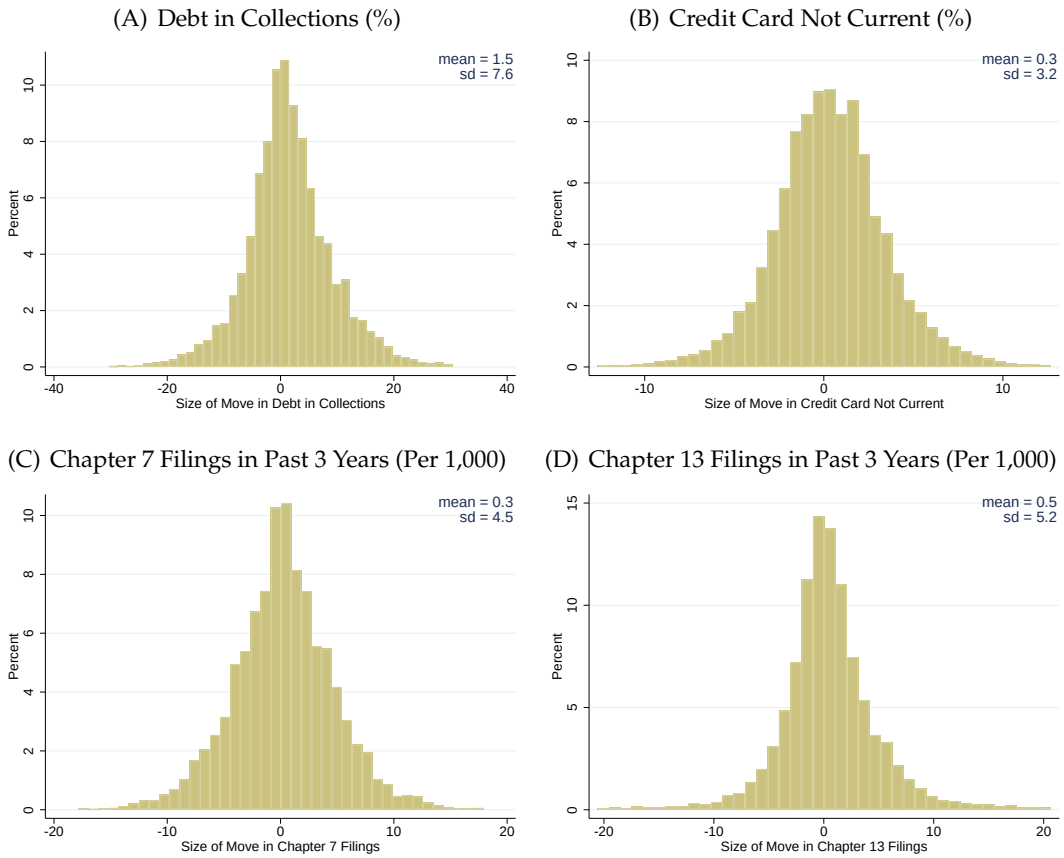
Note: Figure shows histogram of size of move $\widehat{\delta_i}$ computed at zip code $\times$ 10 year age bin level. We keep $\widehat{\delta_i}$ that are within $\pm$ 4 standard deviations from the mean.

Figure A8: Histogram of Size of Move $\widehat{\delta_i}$ at County Level



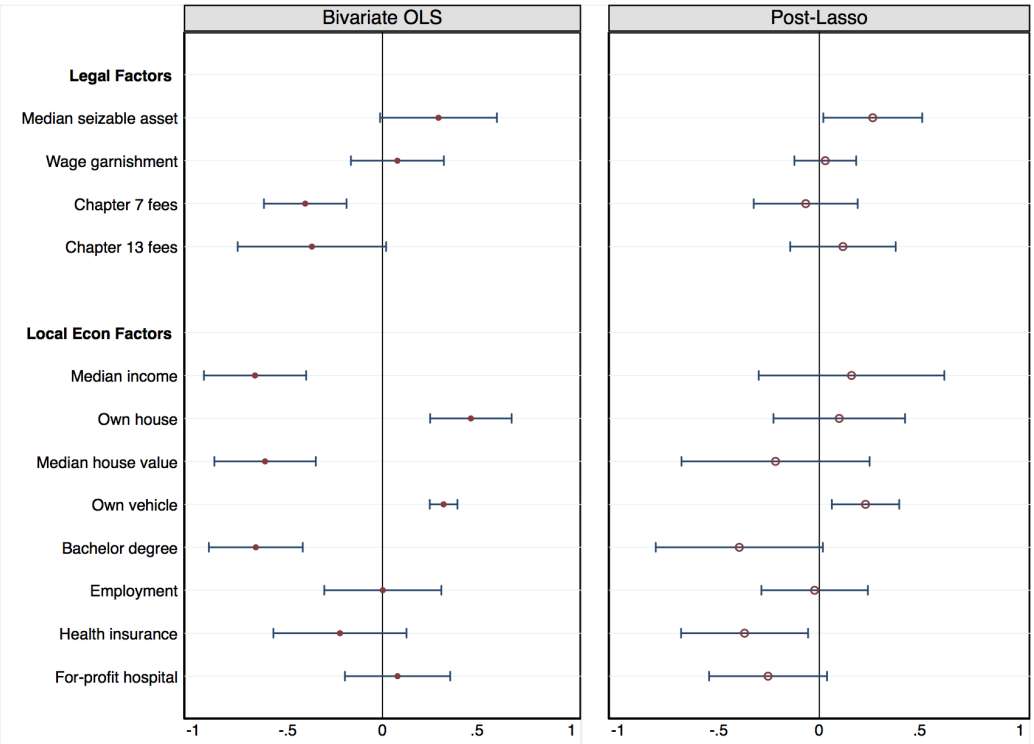
Note: Figure shows histogram of size of move $\widehat{\delta_i}$ computed at county level. We keep $\widehat{\delta_i}$ that are within $\pm$ 4 standard deviations from the mean.

Figure A9: Histogram of Size of Move $\widehat{\delta_i}$ at Commuting Zone Level



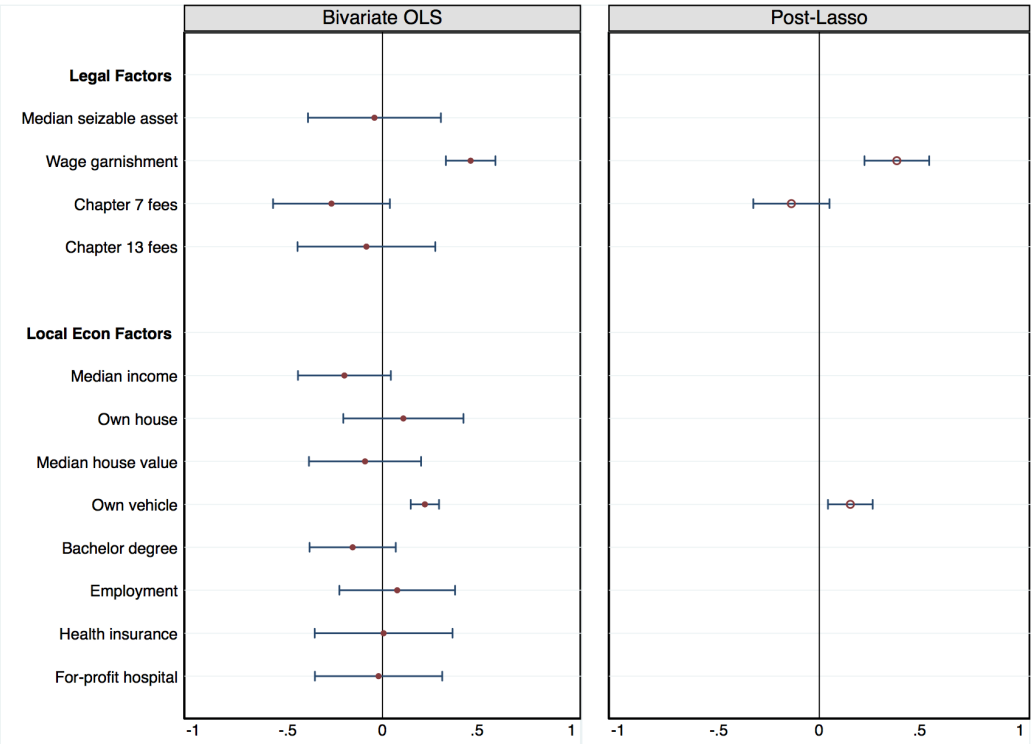
Note: Figure shows histogram of size of move $\widehat{\delta_i}$ computed at commuting zone level. We keep $\widehat{\delta_i}$ that are within $\pm$ 4 standard deviations from the mean.

Figure A10: Correlates of Place Effects: Medical Debt in Collections



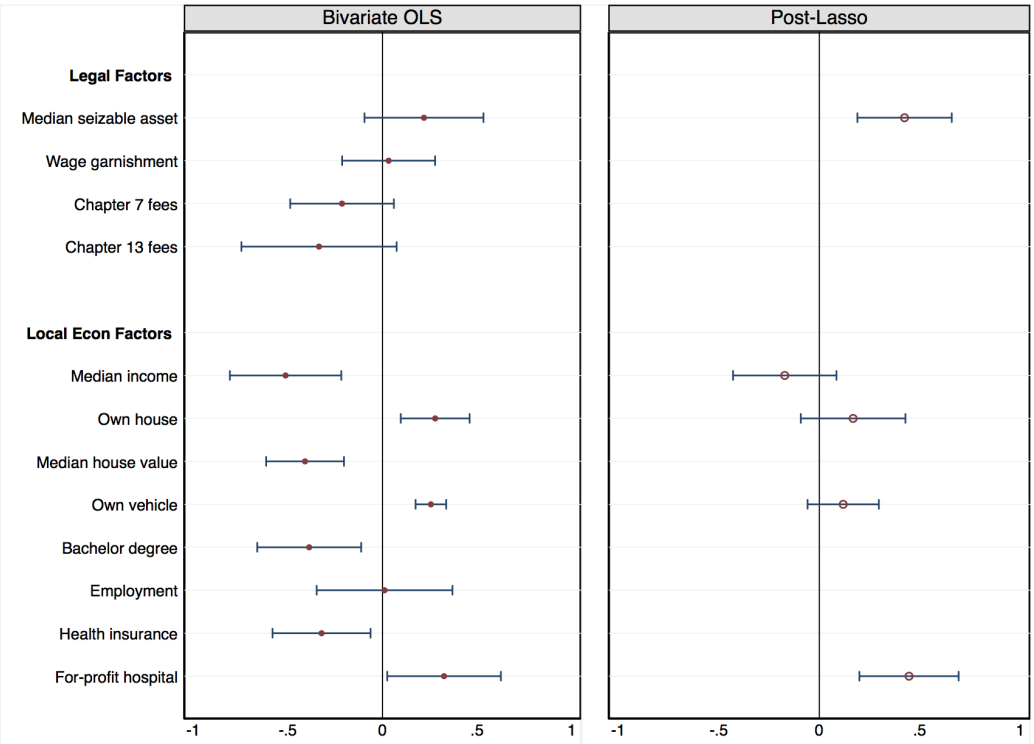
Note: Figure shows bivariate OLS regression coefficients (left panel) and post-Lasso multivariate regression coefficients (right panel) of the place effect of medical debt in collections. The dependent variable and all covariates have been standardized to have mean zero and standard deviation one. To obtain the post- Lasso estimates, we first run a Lasso regression on the full set of covariates, with the penalty level chosen by a 4-fold cross-validation to minimize mean squared error. We then run an OLS regression on the set of covariates chosen by the Lasso regression.

Figure A11: Correlates of Place Effects: Chapter 7 Filings



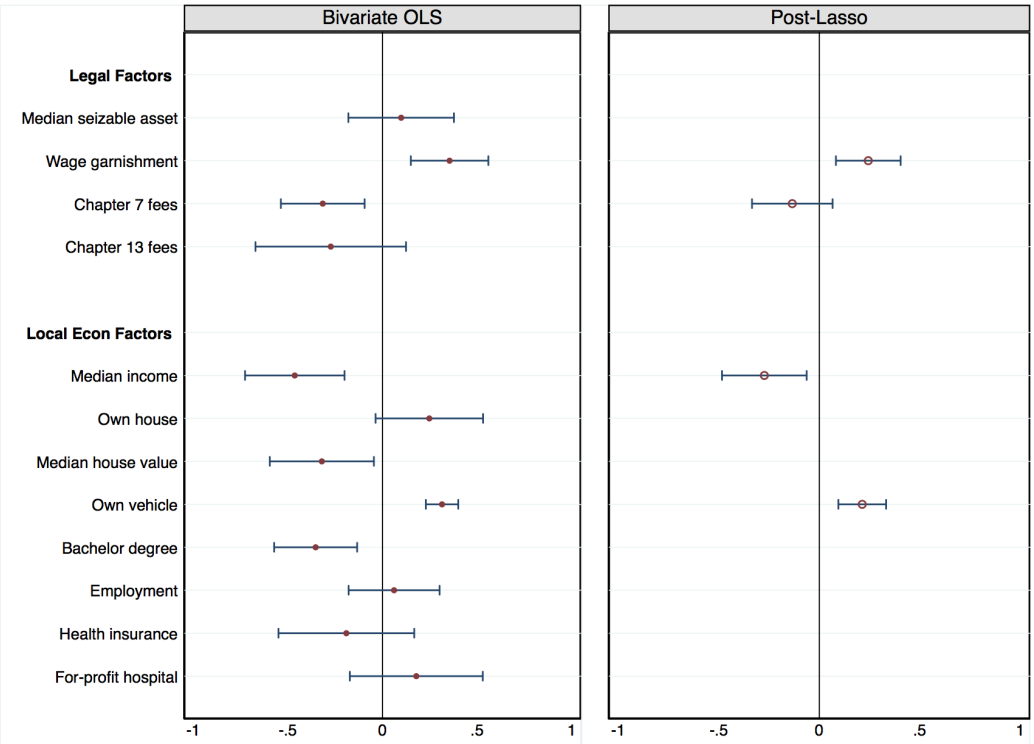
Note: Figure shows bivariate OLS regression coefficients (left panel) and post-Lasso multivariate regression coefficients (right panel) of the place effect of chapter 7 bankruptcy filings in the past three years. The dependent variable and all covariates have been standardized to have mean zero and standard deviation one. To obtain the post- Lasso estimates, we first run a Lasso regression on the full set of covariates, with the penalty level chosen by a 4-fold cross-validation to minimize mean squared error. We then run an OLS regression on the set of covariates chosen by the Lasso regression.

Figure A12: Correlates of Place Effects: Chapter 13 Filings



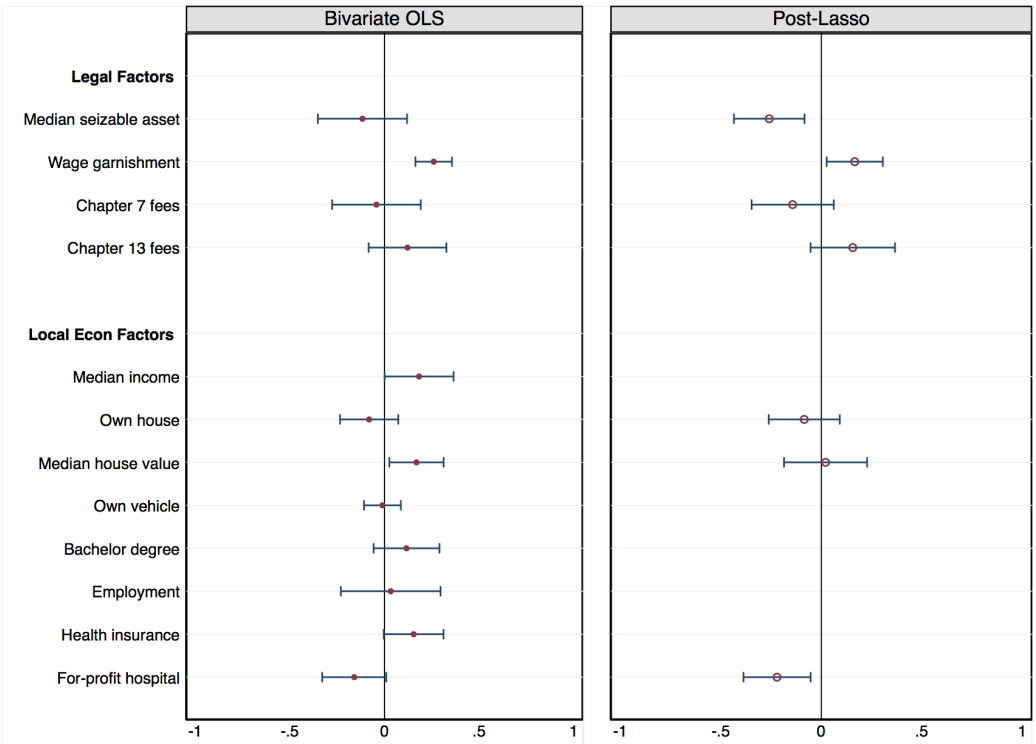
Note: Figure shows bivariate OLS regression coefficients (left panel) and post-Lasso multivariate regression coefficients (right panel) of the place effect of chapter 13 bankruptcy filings in the past three years. The dependent variable and all covariates have been standardized to have mean zero and standard deviation one. To obtain the post- Lasso estimates, we first run a Lasso regression on the full set of covariates, with the penalty level chosen by a 4-fold cross-validation to minimize mean squared error. We then run an OLS regression on the set of covariates chosen by the Lasso regression.

Figure A13: Correlates of Place Effects: Combined Bankruptcy Filings



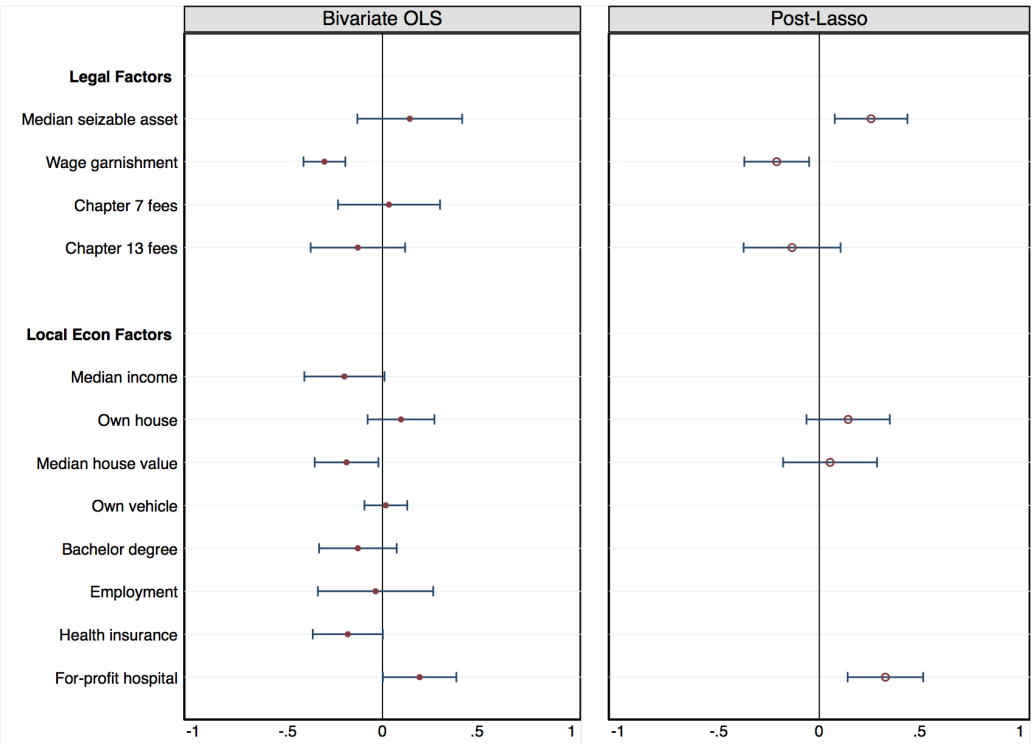
Note: Figure shows bivariate OLS regression coefficients (left panel) and post-Lasso multivariate regression coefficients (right panel) of the place effect of any bankruptcy filings in the past three years. The dependent variable and all covariates have been standardized to have mean zero and standard deviation one. To obtain the post- Lasso estimates, we first run a Lasso regression on the full set of covariates, with the penalty level chosen by a 4-fold cross-validation to minimize mean squared error. We then run an OLS regression on the set of covariates chosen by the Lasso regression.

Figure A14: Correlates of Place Effects: Chapter 7 Filings Controlling for Combined Filings



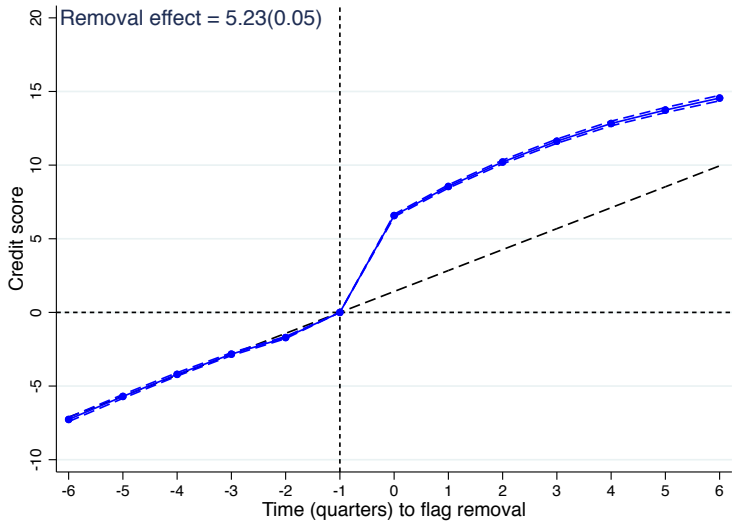
Note: Figure shows bivariate OLS regression coefficients after partialling out combined bankruptcy filings (left panel) and post-Lasso multivariate regression coefficients (right panel) of the place effect of chapter 7 bankruptcy filings in the past three years. The dependent variable and all covariates have been standardized to have mean zero and standard deviation one. To obtain the post- Lasso estimates, we first run a Lasso regression on the full set of covariates including combined bankruptcy filings, with the penalty level chosen by a 4-fold cross-validation to minimize mean squared error. We then run an OLS regression on the set of covariates chosen by the Lasso regression.

Figure A15: Correlates of Place Effects: Chapter 13 Filings Controlling for Combined Filings



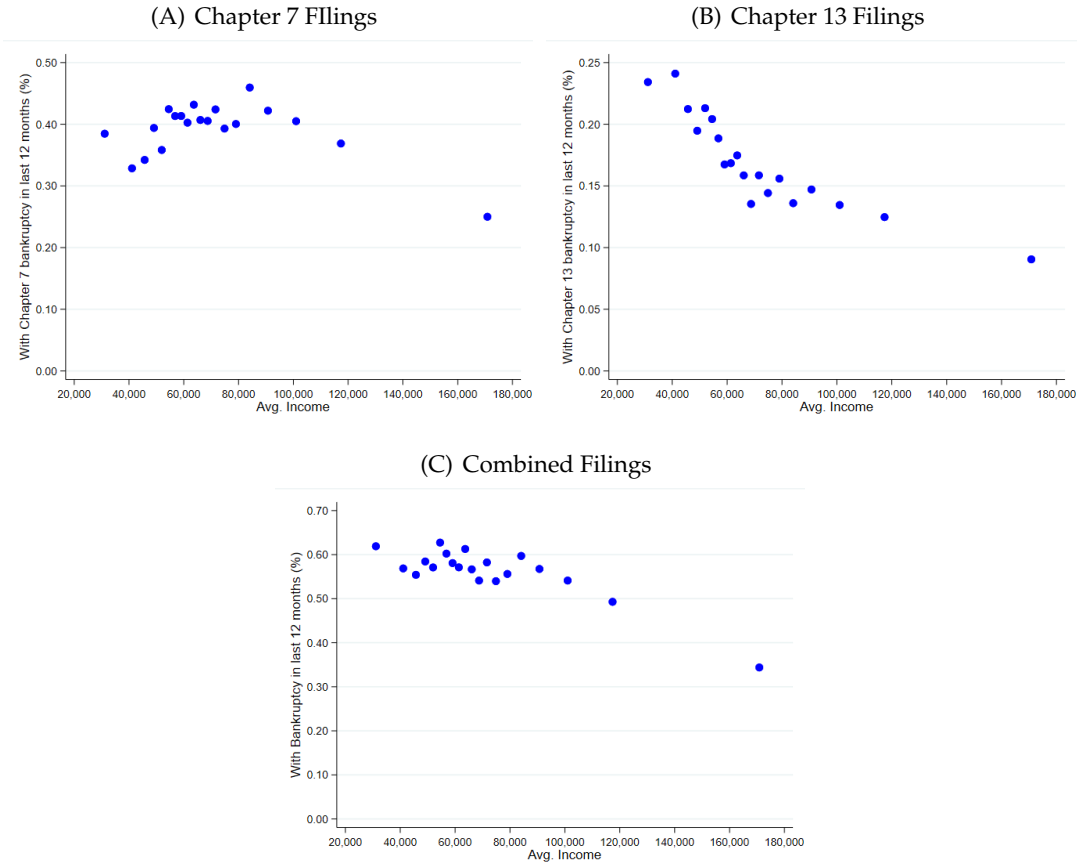
Note: Figure shows bivariate OLS regression coefficients after partialling out combined bankruptcy filings (left panel) and post-Lasso multivariate regression coefficients (right panel) of the place effect of chapter 13 bankruptcy filings in the past three years. The dependent variable and all covariates have been standardized to have mean zero and standard deviation one. To obtain the post- Lasso estimates, we first run a Lasso regression on the full set of covariates including combined bankruptcy filings, with the penalty level chosen by a 4-fold cross-validation to minimize mean squared error. We then run an OLS regression on the set of covariates chosen by the Lasso regression.

Figure A16: Flag Removal Event Study



Note: The dashed line corresponds to the event time trend θ. The solid line is constructed by adding the coefficients δ_s to this trend. The dashed blue lines represent 95% confidence intervals. In the top left corner, we display the immediate removal effect on credit score (δ_0).

Figure A17: Bankruptcy Filings across Income



Note: Binned scatter plots show the relationship between bankruptcy filing and average income at zip code level. The y-axis corresponds to the fraction of individuals who filed for chapter 7, 13, or any bankruptcy in the past 12 months, constructed using pooled 10% random samples of TransUnion credit records from June 2013, 2015 and 2016. The x-axis corresponds to average income at zip code level from 2013-2017 5-year ACS.

Table A1: Number of Non-Movers Used to Construct $\hat{\delta}_i$

No. Non-Movers	Mean	Pct 1	Pct 5	Pct 10	Pct 25	Pct 50	Pct 75	Pct 90	Pct 95	Pct 99
Zip	3,795	100	353	671	1,631	3,224	5,320	7,528	9,185	12,989
Zip $\times$ age bin	956	34	90	156	357	748	1,308	1,969	2,501	3,842
County	71,162	1,601	3,664	5,799	12,762	37,224	91,388	172,316	238,733	549,968
CZ	264,449	6,154	16,941	26,771	55,679	145,633	366,119	650,512	1,068,960	1,144,226

Note: Table shows summary statistics on the number of non-movers used to construct different versions of $\hat{\delta}_i$. The statistics pool across origins and destinations and are weighed by the number of movers so that they are representative for our sample. For example, the mean should be interpreted as the mean, taken across all movers, of the number of non-movers in an origin or destination.

Table A2: Event-Study Estimates

	Fraction converged to outcome at destination 6 years post move				
	(1)	(2)	(3)	(4)	(5)
Financial distress measures	All movers	Positive moves	Negative moves	Move <40	Move $\geq$ 40
Debt in collections	0.0788	0.0645	0.0574	0.0759	0.0810
	(0.0095)	(0.0191)	(0.0200)	(0.0190)	(0.0109)
Medical collections	0.2042	0.2099	0.1173	0.2336	0.1955
	(0.0130)	(0.0248)	(0.0269)	(0.0260)	(0.0140)
Non-medical collections	0.0409	-0.0846	0.1405	0.0173	0.0485
	(0.0120)	(0.0260)	(0.0240)	(0.0250)	(0.0136)
Credit card not current	0.0633	0.0296	0.1239	0.0697	0.0674
	(0.0180)	(0.0386)	(0.0405)	(0.0372)	(0.0204)
Chapter 7 filings (3 years)	0.2665	0.2276	0.2423	0.2383	0.2777
	(0.0500)	(0.0934)	(0.1004)	(0.0982)	(0.0577)
Chapter 13 filings (3 years)	0.3298	0.4518	0.1944	0.3924	0.3050
	(0.0503)	(0.0756)	(0.1198)	(0.0903)	(0.0624)

Note: Table shows place-based effects θ_r at 6 years (24 quarters) after the move from event study regressions of financial distress on the size of the move $\hat{\delta}_i$, individual and time fixed effects, and other controls. Standard errors, clustered by origin $\times$ destination CZ, are shown in parentheses. Column (1) shows the baseline specification. In columns (2) and (3), we report estimates from regressions estimated separately on positive moves to places with higher financial distress ($\hat{\delta}_i > 0$) and negative moves to places with lower financial distress ($\hat{\delta}_i < 0$). In columns (4) and (5), we report estimates from regressions separately on individuals who moved when they are less than 40-year-olds and individuals who moved at age greater or equal to 40-year-old.

Table A3: Event-Study Estimates with Larger Sample

	Fraction converged to outcome at destination 4 years post move					
	(1)	(2)	(3)	(4)	(5)	(6)
Financial distress measures	All movers	Same origin	Same destination	Zip × age level	County level	CZ level
Debt in collections	0.0605	0.0458	0.0491	0.0517	0.1059	0.1500
	(0.0068)	(0.0069)	(0.0070)	(0.0059)	(0.0091)	(0.0116)
Medical collections	0.1510	0.1114	0.1426	0.1144	0.1993	0.2179
	(0.0090)	(0.0092)	(0.0091)	(0.0076)	(0.0113)	(0.0131)
Non-medical collections	0.0467	0.0372	0.0414	0.0432	0.0730	0.1191
	(0.0086)	(0.0087)	(0.0089)	(0.0074)	(0.0120)	(0.0159)
Credit card not current	0.0325	0.0271	0.0243	0.0331	0.0897	0.1562
	(0.0127)	(0.0126)	(0.0128)	(0.0097)	(0.0210)	(0.0265)
Chapter 7 filings	0.1735	0.1137	0.1745	0.0441	0.3845	0.4305
	(0.0352)	(0.0360)	(0.0363)	(0.0234)	(0.0602)	(0.0727)
Chapter 13 filings	0.2611	0.3171	0.1581	0.1433	0.3382	0.3656
	(0.0361)	(0.0372)	(0.0391)	(0.0261)	(0.0489)	(0.0522)

Note: Table shows place-based effects θ_r at 4 years (16 quarters) after the move from event study regressions of financial distress on the size of the move $\hat{\delta}_i$, individual and time fixed effects, and other controls. We include a larger sample of individuals who moved across CZs between 2004 and 2012. Standard errors, clustered by origin × destination CZ, are shown in parentheses. Column (1) shows the baseline specification. In columns (2) and (3), we report estimates from regressions estimated separately on positive moves to places with higher financial distress ($\hat{\delta}_i > 0$) and negative moves to places with lower financial distress ($\hat{\delta}_i < 0$). In columns (4) and (5), we report estimates from regressions separately on individuals who moved when they are less than 40-year-olds and individuals who moved at age greater or equal to 40-year-old.

Table A4: Event-Study Estimates with Larger Sample

	Fraction converged to outcome at destination 4 years post move				
	(1)	(2)	(3)	(4)	(5)
Financial distress measures	All movers	Positive moves	Negative moves	Move <40	Move $\geq$ 40
Debt in collections	0.0605	0.0884	0.0194	0.0758	0.0568
	(0.0068)	(0.0132)	(0.0140)	(0.0153)	(0.0074)
Medical collections	0.1510	0.1783	0.1029	0.2215	0.1323
	(0.0090)	(0.0173)	(0.0186)	(0.0209)	(0.0094)
Non-medical collections	0.0467	0.0270	0.0683	0.0641	0.0411
	(0.0086)	(0.0178)	(0.0169)	(0.0203)	(0.0093)
Credit card not current	0.0325	0.0412	0.0443	0.0407	0.0365
	(0.0127)	(0.0256)	(0.0280)	(0.0310)	(0.0135)
Chapter 7 filings	0.1735	0.0755	0.2369	0.0936	0.1955
	(0.0352)	(0.0673)	(0.0734)	(0.0831)	(0.0387)
Chapter 13 filings	0.2611	0.4272	0.0878	0.3509	0.2331
	(0.0361)	(0.0555)	(0.0810)	(0.0764)	(0.0408)

Note: Table shows place-based effects θ_r at 4 years (16 quarters) after the move from event study regressions of financial distress on the size of the move $\hat{\delta}_i$, individual and time fixed effects, and other controls. We include a larger sample of individuals who moved across CZs between 2004 and 2012. Standard errors, clustered by origin $\times$ destination CZ, are shown in parentheses. Column (1) shows the baseline specification. In columns (2) and (3), we report estimates from regressions estimated separately on positive moves to places with higher financial distress ($\hat{\delta}_i > 0$) and negative moves to places with lower financial distress ($\hat{\delta}_i < 0$). In columns (4) and (5), we report estimates from regressions separately on individuals who moved when they are less than 40-year-olds and individuals who moved at age greater or equal to 40-year-old.