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

Benjamin J. Keys
The Wharton School and NBER, USA

Neale Mahoney
Stanford University and NBER, USA

Hanbin Yang
Harvard Business School, USA

We use credit report data to study consumer financial distress in America. We report large, persistent disparities in financial distress across regions. To understand these patterns, we conduct a "movers" analysis. For collections and default, there is only weak convergence following a move, suggesting these types of distress are not primarily caused by place-based factors (e.g., local economic conditions and state laws) but instead reflect person-based characteristics (e.g., 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 personal bankruptcy. (*JEL* G5, G51, K35)

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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

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[1] This figure is based on the authors' calculations using credit bureau data from June 2015. See Section 1 for more details.

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there are no perfect data for relative comparisons, the available data indicate that financial distress is much higher in the United States than in Europe.[2]

In this paper, we aim to advance our understanding of consumer financial distress in the United States by examining patterns of 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. We focus our analysis on three common metrics of financial distress: debt in collections, credit card delinquency, and personal bankruptcy. We emphasize these metrics because our aim is to observe financial distress for the broadest possible segment of the population. Other financial products, such as home loans and auto loans, are held by a smaller and less representative sample of the population, and thus provide us with a narrower window on people at risk of default.

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 unpaid debt in collections vs. 24% in the Upper Midwest. Similarly, measures of credit card delinquency and bankruptcy are 40% to 50% higher in the Deep South than in the Upper Midwest.

The main part of the paper aims to better understand what determines these persistent geographic disparities. As we show in a variance decomposition, roughly three-quarters of the variation in financial distress over 2000–2016 occurs across geographic areas, as opposed to within geographic areas over time. Our goal is to understand the sources of these cross-sectional differences. We do not attempt to uncover the drivers of over-time variation in financial distress that is often the focus of macroeconomic analysis.

Much of the existing research on the sources of 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; Auclert, Dobbie, and Goldsmith-Pinkham 2019) and local economic conditions (e.g., Agarwal and Liu 2003). A second category emphasizes individual characteristics, such as preference parameters (e.g., discount rates, risk preference, default stigma, social capital) and behavioral factors (e.g., inattention, financial literacy).[4]

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[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 nonmortgage 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.

[4] See, for example, Gross and Souleles (2002) on default stigma, Agarwal, Chomsisengphet, and Liu (2011) on "individual social capital," and Gerardi, Goette, and Meier (2013) on financial literacy.

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 different levels of financial distress. 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 the movers design literature, we will sometimes refer to local institutional and economic factors as "place effects" and to individual characteristics as "person effects" (e.g., Finkelstein, Gentzkow, and Williams 2016).[5]

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, controlling for individual and time fixed effects. We show robustness to defining the size of the move using narrower and broader levels of geographic aggregation and to estimating two-way fixed effects models that relax assumptions implicit in the event-study design.

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 differential changes in the outcome not caused by the move. 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 economically vibrant locations in response to persistent positive shocks. We provide two sets of evidence in support of our identifying assumption.

First, we show no correlation between the size of the move and the premove 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.[6]

Second, we show that we obtain 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 different financial distress.

[5] As the prior literature has noted, one's local environment may shape their individual characteristics, especially at a young age (e.g., Bronnenberg, Dube, and Gentzkow 2012; Finkelstein, Gentzkow, and Williams 2016). As such, the person effects we estimate may partially embed the place effects of where people grew up. That being said, we think it is appropriate in most contexts to refer to these characteristics as person effects, since they are persistent features of individual behavior that are not affected over the medium run by local institutional or cultural factors.

[6] With monthly updates to an individual's ZIP code based on their mailing address, the depersonalized credit report data allow us to closely track household location.

We address the concern that financial distress experienced shortly after a move may reflect place effects from the prior location by focusing on outcomes 6 years after the move. Bronnenberg, Dube, and Gentzkow (2012) show that in a Becker and Murphy (1988) type model, where behavior today depends on the depreciated sum of past behavior, place effects asymptote to their true value as the stock of "preference capital" depreciates. For our outcomes, place effects reach their steady states within 6 years, indicating that these results no longer embed the effects of the prior location.[7]

We find that credit card delinquency and debt in collections 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 between areas, 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 finding is consistent with local medical providers (e.g., doctors offices and hospitals) having a meaningful impact on overall debt in collections through heterogeneous collection practices. 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.

Chapter 13 results, to some extent, 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. However, 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.[8]

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[7] For our movers analysis, we also focus on "flow" measures of financial distress that reflect recent changes to the credit report, which eliminate any mechanical relationship between current financial distress and financial distress that was incurred at the prior location. However, since virtually all items must be removed from credit reports at 7 years, and in practice most items fall off credit reports in less than 6 years, the results using "stock" measures are very similar.

[8] Unlike the findings of prior work, we do not find any persistent correlation between our place effects and local or economic factors. We will discuss these results and their interpretation in Section 4.

We also find larger place effects for across-state moves than for within-state moves, especially for Chapter 13 filings. Since bankruptcy laws vary at the state level, the larger place effects for across-state moves are consistent with an important role for state-level bankruptcy laws in determining bankruptcy filings.

Our findings can be summarized in one sentence: individual characteristics determine whether you get into financial distress, while place-based factors determine whether you use bankruptcy to get out.

While we believe our research design is valid, the most likely violation of our identifying assumption would bias upward our place-based effects. This violation would occur if negative shocks, such as job loss, which directly cause financial distress, precipitated moves to more distressed areas. To the extent it exists, upward bias works against our finding of small place-based effects for debt in collections and credit card nonpayment, and it would preserve our finding of relatively larger place-based effects for personal bankruptcy.

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.[9] Our finding of statistically significant place-based effects is consistent with the results from 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.[10]

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 origins and persistence of financial distress at the individual level (Athreya, Mustre del Río, and Sánchez 2019; Brown, Cookson, and Heimer 2019) 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

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

[10] Note that our results do not suggest that place-based effects are not important. Many place-based factors, such as peer effects in Agarwal, Mikhed, and Scholnick (2020), can have a significant influence on individual financial outcomes, but do not explain the geographic variation in financial distress.

research on brand preferences (Bronnenberg, Dube, and Gentzkow 2012), health care costs, behavior, and outcomes (Finkelstein, Gentzkow, and Williams 2016, 2018, 2021; Hinnosaar and Liu 2020), and intergenerational mobility (Chetty and Hendren 2018a, 2018b), among other topics.

1. Data

1.1 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.[11] 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., because of death). To maintain a representative sample, 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 resultant sample has 30.1 million individuals.

For each individual $\times$ month observation, we observe two types of data. First, we observe individual-level information, such as ZIP code, age, credit score, and aggregated data on loans (e.g., aggregate credit card balances).[12] 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. Internet Appendix Section 6 provides more details about the variable construction.

Bankruptcy: Filing for bankruptcy allows individuals to discharge their debts, stop foreclosure or repossession of property, and prevent wage garnishment. Bankruptcy is often considered a "last resort" and thus serves as an indicator for serious financial distress. 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, since bankruptcy is a rare outcome.

Credit card delinquency: Credit card delinquency occurs when an individual is 30 days or more past due (30+ DPD) on their required monthly payments. Compared to bankruptcy, delinquency is an early indicator of

[11] TransUnion approved a proposal for this project and then reviewed the working paper to ensure compliance with the project proposal.

[12] 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 directly contact TransUnion Consumer Relations to update their address information.

financial distress. To align with our bankruptcy measure, we construct an indicator for whether an individual has become delinquent on any credit card during the last 3 years, for all individuals with a credit report and conditional on those with a credit card.

Debt in collections: Debt in collections is debt reported to TransUnion by third-party debt collectors. Third-party collectors acquire debt from primary creditors, such as health care providers, financial institutions, and utilities (Internet Appendix Table 5 provides a breakdown of collections by creditor). For instance, health care providers typically sell accounts to collectors after 180 days and credit card issuers usually write off accounts and sell their debt when borrowers are 180 days past due. Depending on state laws, debt collectors may contact debtors by letter or phone about outstanding debts, and can also attempt to collect through wage garnishment. Debt collectors may cease reporting for a variety of reasons, including when the debtor agrees to a repayment plan or when the account is paid in full. Debts in collection cannot be reported more than 7 years after the initiating event. See CFPB (2016, 2021) for more details.

To align with our bankruptcy and credit card delinquency measures, we construct an indicator for whether an individual has received at least one new collection account during the past 3 years and a measure of total collections balances accrued in the past 3 years.

Debt collections that are more than 3 years old are still a liability for individuals and thus informative of financial distress. Hence, to complement the flow measure of debt accrued during the last 3 years, we also construct stock measures of whether the individual has at least one debt in collections and their total collection balance, regardless of when the collection was incurred. Since collection items are deleted from credit reports after 7 years, the stock variables can reflect collections accrued up to 7 years ago.[13] For the stock measures, we are also able to separately identify medical and nonmedical debt in collections, and we construct indictors for having at least one debt in collections and collections balances in each category.[14]

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, are held by a smaller and more affluent sample of the general population. Because of this, the set of individuals who are "at risk" for financial distress for this product is less broadly representative.

[13] As shown in Internet Appendix Figure 6, most debt in collections reported to credit bureaus is fairly recent; for a given vintage of debt in collections, about 60% of collections items are no longer reported at a time horizon of 3 years.

[14] We are unable to separate flow measures into medical and nonmedical debt because of data limitations. Specifically, prior to 2009, we are unable to separately identify medical debt in the line-item collections data, and therefore cannot observe the origination date separately for medical vs. nonmedical debt in collections for the first part of our sample.

Table 1
Financial distress measures

	Mean	SD	Median	Pct 75	Pct 90	Pct 95	Pct 99
Collections - Flow							
Debt in collections in past 3 years (%)	28.4	45.1	0	100	100	100	100
Balance of collections in past 3 years ($)	818.0	3,192.7	0	237	2,000	4,121	13,386
Collections - Stock							
Debt in collections (%)	34.1	47.4	0	100	100	100	100
Medical collections (%)	22.1	41.5	0	0	100	100	100
Nonmedical collections (%)	24.2	42.8	0	0	100	100	100
Collections balance ($)	1,350.9	4,843.2	0	606	3,401	6,748	21,234
Medical collections balance ($)	697.3	3,448.4	0	0	1,245	3,178	13,770
Nonmedical collections balance ($)	624.4	2,577.0	0	0	1,445	3,218	10,993
Credit Card							
Credit card delinquency in past 3 years (%)	10.1	30.1	0	0	100	100	100
Credit card delinquency in past 3 years (%, cond'l)	14.1	34.8	0	0	100	100	100
Bankruptcy							
Bankruptcy lings in past 3 years (per 1,000)	14.8	120.9	0	0	0	0	1,000
Chapter 7 lings in past 3 years (per 1,000)	10.5	101.8	0	0	0	0	1,000
Chapter 13 lings in past 3 years (per 1,000)	4.5	67.2	0	0	0	0	0
N	29,398,845						

This 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. We include both flow measures of debt in collections over the past 3 years and the stock measures of whether the individual has 1+ debt in collections. Credit card delinquency is an indicator for 1+ credit card that is 30+ DPD over the past 3 years. 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 1 for more details on variable construction.

1.2 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.[15]

The top-two panels show statistics for debt in collections. Over the last 3 years, 28.4% of individuals have incurred new collections. Unconditional on incurring debt, individuals have accrued an average of $818 in balances in the last 3 years, implying average balances of $2,880 among those who have accrued debt. The stock measures are only modestly higher. 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 nonmedical debt in collections. This pattern suggests that roughly four-fifths of those with debt in collections have accrued new collections items in in the last 3 years (28.4% out of 34.1%). The unconditional stock of collection balances is $1,351 on average.

[15] 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), which is almost identical to our stock measure of 34.1%. In a nationally representative survey conducted by the Kaiser Family Foundation and *New York Times*, 27% of 18- to 64-year-olds report being contacted by a collection agency in the prior 12 months (Hamel et al. 2016).

The bottom-two panels of Table 1 show statistics for credit card delinquency and bankruptcy. Our data indicate that 10.1% of individuals become delinquent on a credit card over the past 3 years. Conditional on having a credit card, 14.2% become delinquent over the past 3 years. We estimate that 14.8 in 1,000 individuals have filed for bankruptcy over the last 3 years, 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).[16]

Accurate measurement of when individuals move is important for our movers analysis. To validate the measurement of moves in the TransUnion data, Internet Appendix Table 6 reports the frequency of moves in the TransUnion data, Current Population Survey, and American Community Survey across different geographic units (any move, move across ZIP code, move across county, move across state), different time horizons (1 and 5 years), and at different points in time (2005 and 2015). The table shows that for all the outcomes where there is overlap, the frequency of moves is quite similar in the TransUnion data to the rates reported in the CPS and ACS.

2. Geographic Variation

In this section, we discuss the sharp geographic disparities in financial distress across regions within the United States. To motivate our focus on cross-sectional differences—in contrast with the time-series variation emphasized in macroeconomic analysis—we start by presenting a variance decomposition of financial distress. In particular, for each measure of financial distress, Table 2 shows the decomposition of the pooled variation into the time-series and cross-sectional components, based on a panel data set of average financial distress for each commuting zone (CZ) × year.[17] For nearly all of the outcomes, more

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

[17] Let x_{it} be the average level of financial distress in CZ i and year t. The overall variation can be decomposed according to

$$\sum_{i=1}^{N}\sum_{t=1}^{T}(x_{it}-\bar{\bar{x}})^2=\underbrace{\sum_{i=1}^{N}\Big(\sum_{t=1}^{T}(x_{it}-\bar{x}_i)^2\Big)}_{\text{over-time}}+\underbrace{\sum_{i=1}^{N}T(\bar{x}_i-\bar{\bar{x}})^2}_{\text{cross-sectional}},$$

where $\bar{x}_i$ is average over time within CZ i and $\bar{\bar{x}}$ is the pooled average across CZs and years.

Table 2
Variance decomposition

	Time-series	Cross-section
Debt in collections in past 3 years (%)	35.19	64.81
Debt in collections (%)	27.77	72.23
Medical collections (%)	22.87	77.13
Nonmedical collections (%)	14.04	85.96
Credit card delinquency in past 3 years (%)	57.37	42.63
Chapter 7 lings in past 3 years (per 1,000)	43.00	57.00
Chapter 13 lings in past 3 years (per 1,000)	10.54	89.46

This table presents a decomposition financial distress into its time-series and cross-sectional components, based on a panel data set of average financial distress for each CZ × year from 2000 to 2016.

than half of the variation is cross-sectional, and for half of the outcomes more than three-quarters of the variation occurs across CZs rather than within CZs over time.

Figure 1 presents maps of our key measures of financial distress. The maps are based on June 2015 data aggregated to the CZ level.[18] Table 3 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 percentage of individuals with debt in collections (1+ debt in collections) in the past 3 years. The map shows strikingly high rates of financial distress in the Deep South and low rates in the Upper Midwest.[19] Specifically, Table 3 indicates that the percentage of individuals with debt in collections is 87% higher in the Deep South than the Upper Midwest (37.3% vs. 19.9%). We think this large geographic difference is intrinsically interesting and focus our analysis on the comparison between these two regions in the remaining discussion in this section. We note, however, that Table 3 shows other measures of the variation in distress across CZs.

The sharp geographic differences are similar for the stock measure of debt in collections and also for medical and nonmedical debt in collections (Internet Appendix Figure 7). However, as shown in Table 3, the differences are larger for medical debt (31.6% vs. 14.4%) than for nonmedical debt in collections (29.1% vs. 17.4%). The differences in medical debt may reflect the initial impact of the ACA Medicaid expansions, which most Southern states did not take part in. However, there were large differences prior to the 2014 Medicaid expansions, and most of the impact of the expansions does not materialize in the stock of medical debt in collections until after June 2015.[20] The differences between the

[18] 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.

[19] As we mentioned in footnote 3, 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.

[20] See Kluender et al. (2021) on how the Medicaid expansions reduced medical debt in collections and Argys et al. (2017) and Gallagher, Gopalan, and Grinstein-Weiss (2019) on the broader link between health insurance and financial distress.

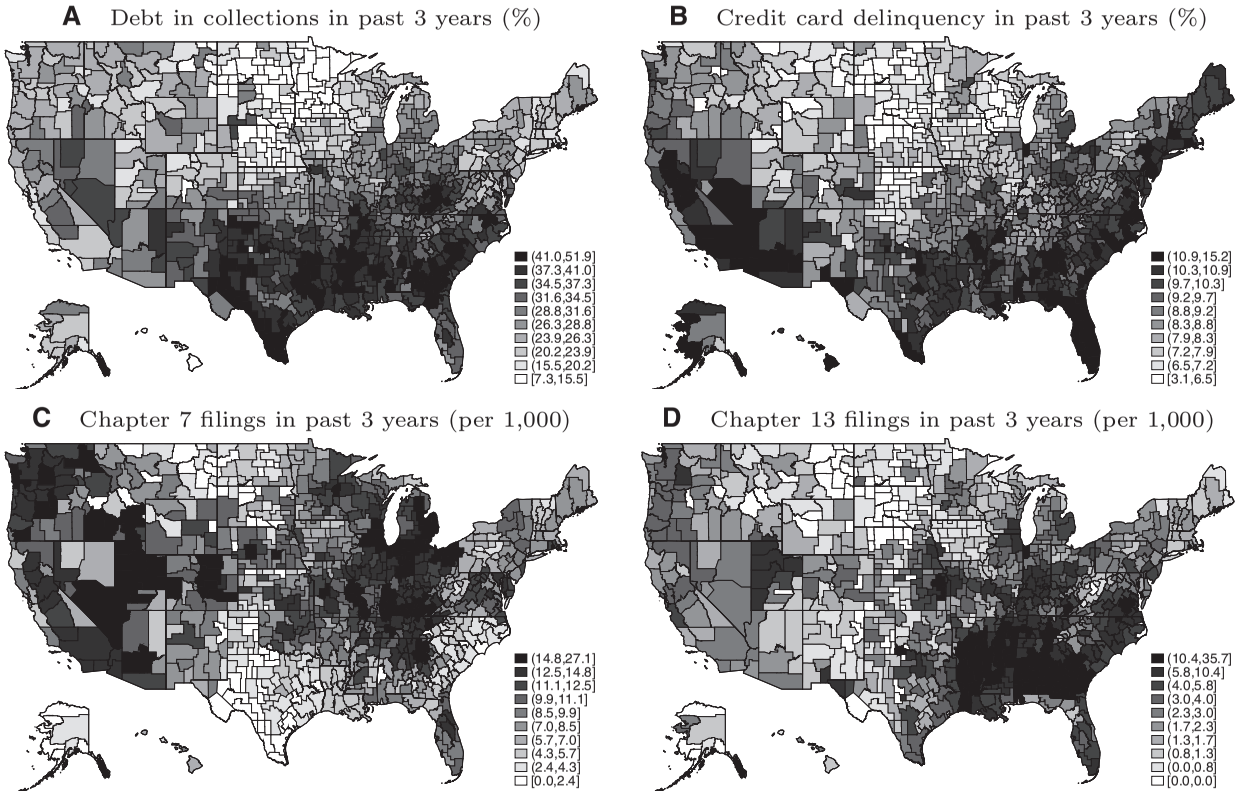

Figure 1
Geographic variation in financial distresses
This figure shows CZ-level maps of financial distress. CZ means are constructed using a 10% random sample of TransUnion credit records from June 2015. Debt in collections is an indicator for 1+ debt in collections in the last 3 years. Credit card delinquency is an indicator for 1+ credit card that is 30+ DPD in the last 3 years. 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 1 for more details on variable construction.

Table 3
Geographic variation in financial distress

	(1) Mean	(2) SD	(3) 75%–25%	(4) 90%–10%	(5) Deep South	(6) Upper Midwest	(7) Deep South-Upper Midwest
Collections - Flow							
Debt in collections in past 3 years (%)	28.4	7.1	11.0	18.7	37.3	19.9	17.4
Balance of collections in past 3 years ($)	818.0	286.4	411.9	719.3	1,117.5	601.3	516.2
Collections - Stock							
Debt in collections (%)	34.1	7.9	11.5	20.8	43.5	23.8	19.8
Medical collections (%)	22.1	8.1	11.5	22.5	31.6	14.4	17.2
Nonmedical collections (%)	24.2	5.2	7.1	14.1	29.1	17.4	11.7
Collections balance ($)	1,350.9	461.4	580.4	1,160.5	1,723.2	987.5	735.7
Medical collections balance ($)	697.3	373.4	500.8	962.0	1,064.8	541.9	522.9
Nonmedical collections balance ($)	624.4	188.7	303.4	483.8	627.0	423.0	204.0
Credit Card							
Credit card delinquency in past 3 years (%)	10.1	1.5	2.0	3.4	10.7	7.2	3.5
Credit card delinquency in past 3 years (%, cond'l)	14.2	2.6	3.5	6.7	17.1	9.3	7.7
Bankruptcy							
Bankruptcy lings in past 3 years (per 1,000)	14.8	6.4	8.7	17.8	19.8	14.5	5.3
Chapter 7 lings in past 3 years (per 1,000)	10.5	4.6	7.5	12.1	9.2	11.9	−2.6
Chapter 13 lings in past 3 years (per 1,000)	4.5	4.1	2.9	7.7	10.8	2.8	8.1

This 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 Table 1 note for more details on the financial distress measures.

Deep South and Upper Midwest are proportionally similar when we examine average collection balances, overall and separately for medical and nonmedical debt. The maps for these outcomes are somewhat less crisp, partially because of the increased noisiness of these measures (Internet Appendix Figure 8).

Panel B of Figure 1 shows the percentage of individuals with at least one credit card delinquency (30+ DPD) over the past 3 years. Like debt in collections, there is a sharp geographic disparity, with credit card delinquency rates 49% higher in the Deep South than in the Upper Midwest (10.7% vs. 7.2%). We focus on credit cards because they are widely held (71% of individuals in our data). However, as shown in panel A of Internet Appendix Figure 9, credit card holding rates are lower in the Deep South. Thus, if we condition on having a card, the difference in credit card delinquency grows to 84% (17.1% vs. 9.3%).

Next, we turn to bankruptcy filings, which, as discussed in Section 1, are measured as filings over the last 3 years per 1,000 people. As shown in Table 3, overall bankruptcy filings are 37% higher in the Deep South than in the Upper

Midwest (19.8 vs. 14.5 per 1,000).[21] 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 in 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 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 are conditional 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 Internet Appendix Figure 9 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%).[22] 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, Internet Appendix Figure 11 plots the rank of each CZ in 2015 against the rank in 2001. For debt in collections and credit card delinquency, the slope coefficients are 0.89 and 0.69 respectively, indicating that a CZ ranked 100 places higher in 2001 is ranked 89 to 69 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 United States is remarkably stable over time.[23]

For Chapter 7 and Chapter 13 bankruptcies, rank stability ranges from 0.55 to 0.74, which is strong but lower than the persistence of the collections and

[21] Panel B of Internet Appendix Figure 10 shows a map of overall bankruptcy rates.

[22] The high coverage rates in the Deep South are driven by people who do not have credit activity, such as people who only have a credit report because they have medical debt in collections. To see this, panel C of Figure 9 displays the percentage of the population with a credit report but no trade accounts (i.e., no loans). The higher rate in the Deep South vs. the Upper Midwest (2.3% vs. 1.2%) explains virtually the entire gap in coverage rates.

[23] Internet Appendix Figure 12 shows that the results are very similar when we examine the correlation in outcomes over time.

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. 2021). 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.

3. Econometric Framework

In the prior section, we documented large, persistent differences in financial distress within the United States. In this section, we present an 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 local economic conditions, 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 have exactly one across-CZ move, with the move occurring between 2004 and 2007, inclusive. For these individuals, we can observe at least 4 years of premove data, which is important for examining preexisting trends, and we can observe at least 8 years of post-move outcomes.

The longer time horizon allows us to examine the dynamics of the place effects. For instance, in a Becker and Murphy (1988) type model, where behavior today depends on the depreciated sum of past behavior, place effects asymptote to their true value as the stock of "preference capital" acquired at the prior location depreciates. The longer time horizon also addresses concerns that short-run effects may be attenuated toward zero by measurement error in the exact timing of the move. Restricting the sample to moves that occur in 2007 or earlier also avoids moves that were precipitated by the financial crisis. Below, we will discuss the robustness of including these movers. The resultant sample consists of 145,805 movers, with a roughly even number of moves across years.[24]

[24] Internet Appendix Figure 13 shows heat maps of origin and destination CZs for these movers. The locations where people move from and to are broadly representative of the underlying geographic distribution of people with credit reports, also shown in this figure.

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 size of the move, $\widehat{\delta_i}$, as the average difference in the outcome between nonmovers in the destination and the origin.[25] 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 nonmovers in the origin and destination ZIP codes. Among the moves, the median origin or destination has 3,224 nonmovers. Internet Appendix Figure 14 shows a histogram of our baseline measure of $\widehat{\delta_i}$. We discuss robustness to alternative measures of $\widehat{\delta_i}$ below.

Our baseline event-study 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.[26]

The coefficients of interest are the θ_r and are normalized to zero in the last quarter in the origin ($\theta_{-1}=0$). Movers with $\widehat{\delta_i}=0$, who move across CZs with equivalent financial distress, can be thought of as the control group. The coefficient of interest θ_r captures the degree to which the change in outcomes for movers reflect the average difference between the origin and the destination, relative to this control group and the control variables. 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 the size of the move, $\hat{\delta}_i$, is uncorrelated with any differential changes in the outcome not caused by the move, conditional on the controls. 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.

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[25] In parallel with movers, we define nonmovers 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 ZIP codes.

[26] 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. (2020)).

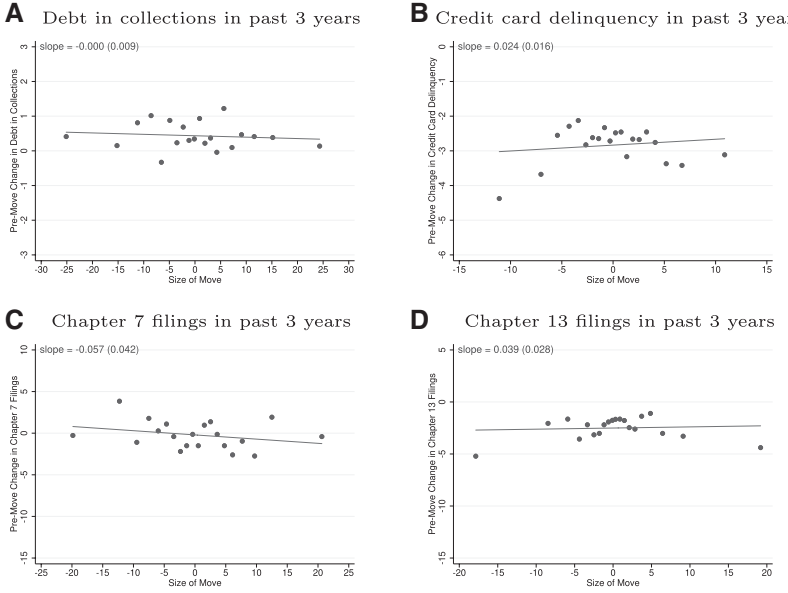

Figure 2
Premove change in financial distress by size of move
This figure shows binned scatter plots of the *pre*-move change in the outcome against the size of the move $\hat{\delta}_i$. The vertical axis shows the average financial distress 1 year premove minus the average financial distress 3 years premove. The horizontal axis shows $\widehat{\delta_i}$, the average financial distress between the destination and origin ZIP codes. The data are split by ventiles of $\widehat{\delta_i}$, and each point represents 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.

To support our identifying assumption, Figure 2 shows binned scatter plots of the *premove* change in the flow measures of financial distress against the size of the move. Specifically, in each plot the vertical axis shows the average financial distress 1 year premove minus the average financial distress 3 years premove, and the horizontal axis shows $\widehat{\delta_i}$. The data are split by ventiles of $\widehat{\delta_i}$, and each point represents 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 are 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- vs. post- differences discussed below.

To provide some initial evidence for the movers effects, Figure 3 shows binned scatter plots of the *pre- vs. post-* change in the same flow measures of financial distress against the size of the move. The plots are constructed in the same manner as before, except that the vertical axis now shows the average financial distress 3 years post-move minus the average financial distress 3 years premove. 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

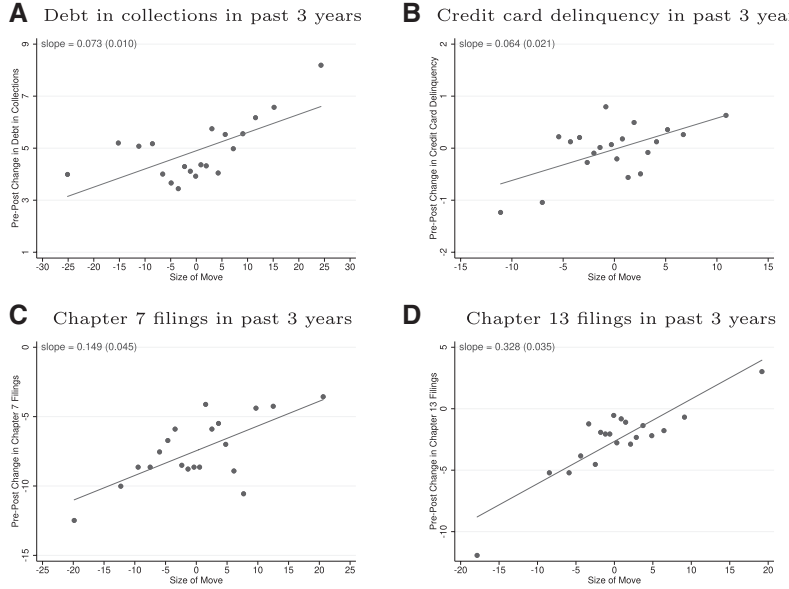

Figure 3
Pre- vs. post-change in financial distress by size of move
This figure shows binned scatter plots of the *pre- vs. post-* change in the outcome against the size of the move $\hat{\delta}_i$. The vertical axis shows the average financial distress 3 years post-move minus the average financial distress 3 years premove. The horizontal axis shows $\widehat{\delta_i}$, the average financial distress between the destination and origin ZIP codes. The data are split by ventiles of $\hat{\delta}_i$, and each point represents 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.

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 be 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 nonmovers too broadly, they will not be a good proxy for the mover's experience in the origin and destination

locations. If we define the group of nonmovers 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 nonmovers who reside in the mover's destination and origin ZIP codes. Internet Appendix Table 7 shows that for this definition, the median origin or destination we use to construct $\widehat{\delta_i}$ is based on 3,224 nonmovers. However, the fifth percentile has only 353 nonmovers, 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, which reduces concerns about statistical noise at the cost of having a more geographically disperse measure of place.[27] Specifically, we construct a version of $\widehat{\delta_i}$ using nonmovers in the mover's origin and destination county. For this measure, the fifth percentile of origin or destination locations has 3,664 nonmovers (see Internet Appendix Table 7). We also examine the sensitivity of our results to defining $\widehat{\delta_i}$ more narrowly than the baseline specification, constructing $\widehat{\delta_i}$ using nonmovers in both the same ZIP code 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 ZIP code $\times$ age group measure, the median origin or destination has 748 nonmovers, and the fifth percentile has 90 nonmovers (see Internet Appendix Table 7).[28]

Our third robustness exercise examines sensitivity to a two-way fixed effects specification, where we replace the $\sum_{r \neq -1} \theta_r \cdot \widehat{\delta_i}$ terms in Equation (1) with fixed effects for each CZ. Under the assumption that there is minimal noise in $\widehat{\delta_i}$ and any heterogeneity in the place effects is orthogonal to the size of the move, the baseline specification will provide identical estimates of the place and person components to a two-way fixed effects specification (Finkelstein, Gentzkow, and Williams 2016). By allowing for fully nonparametric place effects, the two-way fixed effect model relaxes these assumptions.

As we will discuss below, the two-way fixed effects model produces very similar estimates of the relative importance of place vs. person effects, indicating that more restrictive assumptions of the baseline specification are not quantitively important. We prefer our baseline specification because it allows us to visually display the pre-trends and the timing of the response to the moves.

4. 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.

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[27] For consistency, we continue to define nonmovers 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 ZIP codes.

[28] Internet Appendix Figures 15 and 16 show histograms of these measures of $\widehat{\delta_i}$.

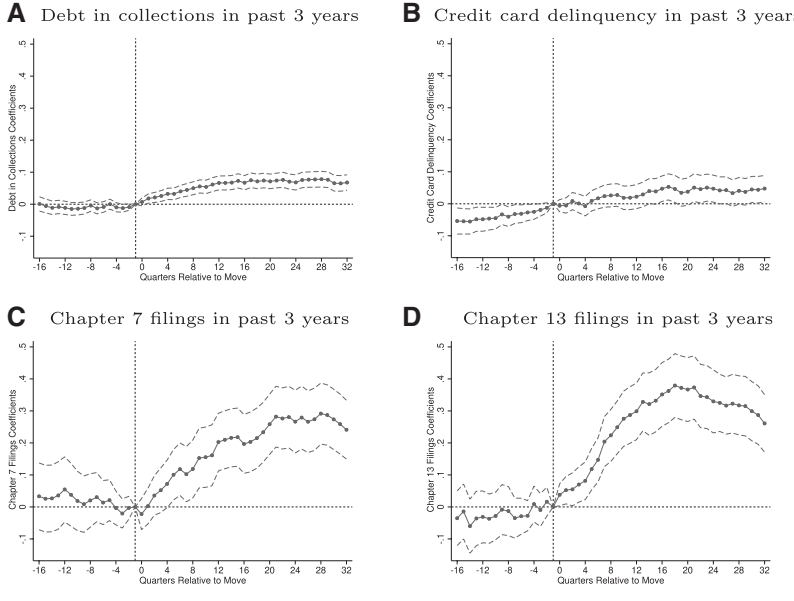

Figure 4
Event-study plots
The figure shows place-based effects, θ_r, from event study regressions of financial distress on the size of the move $\hat{\delta}_i$, individual and time fixed effects, and other controls. The dashed lines represent 95% confidence intervals, based on standard errors clustered by origin × destination CZ.

4.1 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 4 shows parameter estimates and standard errors for θ_r at 6 years post-move for our baseline and alternative specifications.

Panels A and B of Figure 4 examine effects on debt in collections (1+ debt in collections) and credit card delinquency (30+ DPD) in the past 3 years. Prior to the move, there is no evidence of an economically significant trend in θ_r, providing further support for our identifying assumption.[29] There is an inflection point at $r=0$, which is consistent with limited measurement error in the timing of the move. 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 between areas. With our baseline specification, shown in column 1 of Table 4, we can reject a

[29] For credit card delinquency, 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 preexisting trend would make the place-based effect marginally smaller, strengthening our interpretation of the results.

Table 4
Event-study estimates

	Fraction converged to outcome at destination 4 years post-move				
Financial distress measures	(1) All movers	(2) Same origin	(3) Same destination	(4) ZIP × age level	(5) County level
Debt in collections 3 years	0.0681	0.0358	0.0487	0.0622	0.1615
	(0.0122)	(0.0123)	(0.0125)	(0.0105)	(0.0173)
Debt in collections	0.0788	0.0536	0.0611	0.0659	0.1539
	(0.0095)	(0.0095)	(0.0097)	(0.0081)	(0.0141)
Medical collections	0.2042	0.1291	0.1908	0.1575	0.3036
	(0.0130)	(0.0131)	(0.0133)	(0.0110)	(0.0164)
Nonmedical collections	0.0409	0.0213	0.0337	0.0490	0.0956
	(0.0120)	(0.0122)	(0.0123)	(0.0103)	(0.0187)
Credit card delinquency 3 years	0.0473	0.0681	0.0149	0.0326	0.0710
	(0.0215)	(0.0215)	(0.0213)	(0.0157)	(0.0376)
Chapter 7 lings 3 years	0.2665	0.2088	0.2212	0.0863	0.5068
	(0.0500)	(0.0501)	(0.0509)	(0.0325)	(0.0897)
Chapter 13 lings 3 years	0.3298	0.3578	0.2095	0.1097	0.4940
	(0.0503)	(0.0522)	(0.0535)	(0.0422)	(0.0625)

This table shows the 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 × 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 and 5, we show alternative specifications where we construct $\hat{\delta}_i$ using nonmovers in the movers' origin and destination ZIP code × 10-year age bin, and using nonmovers in the movers' origin and destination county.

place-based component of zero but can also reject a place-based effect larger than 10% at 6 years post-move.

Figure 5 further probes these results with event study plots on the stock of debt in collections, medical debt in collections, and nonmedical debt in collections. The effects for stocks are almost identical to those for flows, which as discussed above is because financial distress indicators decay rather quickly. The results in the bottom row show that medical debt in collections is the primary determinant of the effect on collections, with a place effect of approximately 20% vs. 4% for nonmedical collections. This pattern is consistent with the heterogeneous collection practices of local medical providers, 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, and an inflection point at $r=0$. 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.

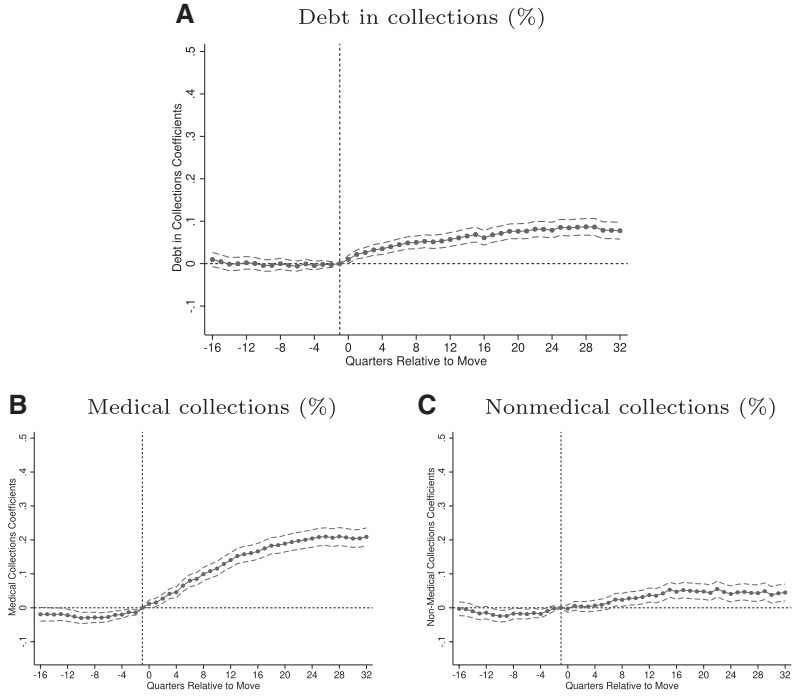

Figure 5
Event-study plots: Debt in collections stock measures
The panels show place-based effects θ_r from event study regressions of financial distress on the size of the move $\hat{\delta}_i$, individual and time fixed effects, and other controls. The dashed lines represent 95% confidence intervals, based on standard errors clustered by origin × destination CZ.

Internet Appendix Figure 17 shows bankruptcy event study plots separately for across- vs. within-state moves (among the baseline sample of individuals who move across CZs). We find consistently larger place effects for across-state moves than for within-state moves, especially for Chapter 13. At 6 years, the Chapter 13 place effect is 43% for across-state movers vs. 14% for within-state moves. The Chapter 7 place effect is 34% for across-state movers vs. 17% for within-state movers. The larger effects for across-state moves are consistent with an important role for state-level bankruptcy laws in determining the place effects.

Internet Appendix Figure 18 shows bankruptcy event studies separately by credit score tercile, where the tercile is based on the credit score in the quarter prior to the move. We observe very little effect for the highest credit score tercile (765 or higher). In contrast, the largest effects are found for the lowest credit score tercile. In our view, the results are consistent with a "double trigger" model (e.g., Bhutta, Dokko, and Shan 2017), where place affects bankruptcy but

only for people who have enough debt for bankruptcy to have a nonnegligible probability of occurring.

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. The lack of pre-trends provides some evidence against this concern. To more directly rule out this threat, column 2 of Table 4 shows estimates from a specification where we add origin CZ × 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 × 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 debt in collections and credit card delinquency measures, and larger effects for the bankruptcy outcomes.

Columns 4 and 5 of Table 4 examine sensitivity to constructing the size of the move at different levels of aggregation. Column 4 constructs $\widehat{\delta_i}$ more narrowly, using all nonmovers in the same ZIP code and same 10-year age group as the mover. Column 5 constructs $\widehat{\delta_i}$ more broadly, using all nonmovers in the same county. 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-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, which provides us with a long enough post-period to observe convergence in the outcome, and also avoids including moves that were precipitated by the financial crisis. Internet Appendix Table 8 is 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 time horizon, there is somewhat less convergence, but the general patterns are similar.

As discussed in Section 3, we also examine the sensitivity of our results to a two-way fixed effects specification, which relaxes the assumptions that $\widehat{\delta_i}$ has limited measurement error and that any heterogeneity in the place effects is orthogonal to the size of the move. Internet Appendix Table 9 presents estimates of the share of differences in outcomes across regions (e.g., above- vs. below-median financial distress, Deep South vs. Upper Midwest) explained by the place effects. For each outcome, we report the average difference, the average amount attributable to place, and the share of the difference attributable to place. With few exceptions, the share of variation explained by place is very similar across sets of regions. As in the event studies, the share of variation explained

by place is much larger for bankruptcy than for the nonbankruptcy outcomes. The precise estimates for the share of variation explained by place are slightly larger, on average, than those from the baseline event study model but smaller than those from the event study model where $\widehat{\delta_i}$ is constructed at the CZ level. Internet Appendix Section 7 provides more details.

Finally, we examine the sensitivity of the bankruptcy results to the 2005 bankruptcy reform (Bankruptcy Abuse Prevention and Consumer Protection Act, BAPCPA) that occurred during our time period. To do so, we construct an alternative measure of the size of the move, $\widehat{\delta_{it}}$, that is allowed to vary before and after the reform. Specifically, we define the pre-BAPCPA size of the move using average outcomes for nonmovers in the pre-BAPCA period and the post-move size of the move using average outcomes for nonmovers in the post-BAPCA period. Internet Appendix Figure 19 shows event study plots, which use this time-varying measure but are otherwise identical to our baseline specification. The place-based effects for Chapter 7 and Chapter 13 are similar to the baseline estimates. Thus, while BAPCPA had sharp short-run and more modest medium-run effects on bankruptcy filings (Gross et al. 2021), our estimates of the relative importance of place- and person-based factors are robust to this more nuanced treatment of bankruptcy law.

Taken together, this evidence reinforces the view that the general pattern of small place effects for collections and credit card delinquency and larger place effects for bankruptcy is robust.

4.2 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 $\widehat{\delta_i}$. 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.[30]

Columns 2 and 3 of Internet Appendix Table 10 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

[30] 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 that places with lower financial distress have higher credit supply, moves to lower financial distress places raise mover's access to credit, which can lead to higher rates of default for these individuals.

moves vs. 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).[31] 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 mover. For example, Chetty and Hendren (2018a) find that children who moved between neighborhoods at younger ages experienced larger place-based effects, likely because they were exposed to the local schooling environment for longer. In our context, we would expect larger impacts for younger movers if younger people are more responsive to 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 of Internet Appendix Table 10 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 2018b). Because a number of the correlates vary at the state level (e.g., bankruptcy exemptions), we conduct this 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), measures of state-level credit supply (e.g., credit-score-adjusted credit limits, bank branches), and state-level economic factors (e.g., median income, house values, employment).

In Internet Appendix Section 8, 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 inverse U shaped in ZIP code income (see Internet Appendix Figure 26). The absence of

[31] 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.

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.

5. 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, 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 first decompose the variance in financial distress measures into cross-sectional and time-series components. We find that for nearly all of the outcomes, more than half of the variation is cross-sectional, and for half of the outcomes more than three-quarters of the variation occurs across CZs rather than within CZs over time. We then conduct a movers analysis to examine 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 delinquency indicates that supply-side factors (e.g., state laws, local lending practices) are not a primary explanation for geographic variation in distress, and instead suggests an important role for persistent individual characteristics in explaining the geographic variation across regions. 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.[32]

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.

Our movers analysis quantifies the determinants of persistent cross-sectional differences in financial distress between areas. Further decomposing the

[32] These persistent individual characteristics may be affected by the places where people grow up and thus may embed place-based effects. However, because these effects are persistent when people move, they are similarly unlikely to respond to changes in local institutional or cultural factors, at least over the medium run.

mechanisms underlying the person-based and place-based components, along with understanding the impact of over-time variation in financial distress (e.g., because of the business cycle or local labor markets shocks) remain fertile ground for future research. While there is a natural theoretical connection between these types of persistent individual characteristics and financial distress, there is less evidence on the quantitative importance of these factors. A salient example is the literature on financial education, which has largely failed to find effects on financial distress when using credible research designs (**?**). This literature, however, should be interpreted as a joint test of whether financial literacy matters and whether it can be improved via financial education. Our estimates, combined with studies on the limited impacts of financial education, are consistent with a model where persistent individual characteristics like financial literacy matter, but may be difficult to change.

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