Peer Financial Distress and Individual Leverage: Evidence from 30 Million Individuals

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Abstract

Using a detailed credit and employment dataset covering over 30 million individuals in the U.S., I examine the effect of peer experiences of financial distress on individual leverage and borrowing behavior. I use health shocks to identify financial distress situations that are plausibly exogenous to peer financial conditions and find asymmetry in individual response to such peer shocks. Individuals with low ex-ante leverage increase borrowing while those with high ex-ante leverage decrease borrowing following peer distress. The negative effect dominates on average as individual leverage declines by 4.2% for the entire sample. This response leads to lower delinquency rates. The estimates suggest a social multiplier of -0.16 for defaults, and that these peer effects can explain a decline of up to $213.31 billion in household debt between 2011 and 2015, corresponding to 1.82% of total household debt in 2011. Overall, the results suggest an important role for distress spillovers in determining the demand for individual leverage.

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Introduction

The great recession has heightened interest in understanding the consequences and causes of household leverage. In terms of consequences, recent research shows that household leverage can affect other household decisions such as consumption and investment (Mian, Rao, and Sufi 2013), and macro-economic outcomes such as employment (Mian and Sufi 2014).[1] The research on the causes of household leverage is still evolving and primarily focused on the possible supply-side determinants.[2] I contribute to this literature by highlighting the role of distress spillovers in determining household demand for leverage and debt. Specifically, I examine the effect of peer experiences of financial distress on individual leverage and borrowing behavior.

Understanding if and how peer distress experience influences individual leverage is not only important in informing us about how individuals make borrowing decisions but such peer effects can potentially have macro-economic implications. Consider for instance that in 2015 alone, over six million individuals defaulted on some form of debt. If these distress experiences affect the borrowing behavior of the defaulted individual's peers, then such distress spillovers may aggregate up to exert significant influence on the total household debt in the economy.

Peer experience of financial distress can affect an individual's leverage for multiple non-exclusive reasons. First, on observing such episodes, an individual may update her beliefs about the expected cost of financial distress. If she increases (decreases) her assessment of the cost of financial distress, then that will decrease (increase) her demand for debt. Second, an individual's preferences may change following peer distress, leading to changes in her borrowing behavior. Third, peer distress can influence individual leverage through the peer consumption channel, which states that individual consumption is a function of peer consumption. To the extent distress reduces peer consumption, it can affect an individual's consumption, say, through her need to "keep up with the Joneses'." The relevance of these forces is ultimately an empirical question addressed in this paper.

The empirical analysis in the paper leverages a novel and detailed dataset on individual credit profiles and employment history that comes from Equifax Inc. The credit data includes information

[1] Leverage also has significant effects on entrepreneurial activity (Adelino, Schoar, and Severino 2015), employment opportunities (Bos, Breza, and Liberman 2015), household investment decisions (Foote, Gerardi, and Willen 2008; Bhutta, Dokko, and Shan 2010; Cunningham and Reed 2013; Fuster and Willen 2013; Guiso and Sodini 2013), mortgage defaults (Scharlemann and Shore 2016), labor income (Debbie and Song 2015), labor supply (Bernstein 2016) and labor mobility (Gopalan, Hamilton, Kalda, and Sovich 2017a).

[2] Examples include lax standards of the banking sector, transfer of risks, and the resulting lack of discipline in applying sound banking standards (e.g. Mian and Sufi 2009; Demyanyk and Van Hemert 2012). The demand-side determinants of debt are not well understood and remain under-examined.

on the credit histories of all individuals in the U.S., including historical information on all their credit accounts, credit scores, and zip codes of residence. The employment data covers over 30 million employees across the U.S. from over 5,000 firms and includes information on the employee's wages, job role, and firm-level details. This is one of the first papers to use such detailed credit and employment data on the U.S. population.

Estimation of peer effects poses significant challenges. First, identifying the relevant peer group is difficult. As a result, most existing research takes one of two alternate paths. It either uses small specific samples where peers are well identified or defines peers in a generic manner for more representative samples; for example, individuals living in the same region (e.g. city), individuals sharing similar socio-demographic characteristics, etc. In contrast, the richness of my data allows me to define peers in a very specific manner for a large representative sample. I define peers as individuals residing in the same zip code and employed at the same firm with the same job role. For example, two sales representatives employed at the same firm and residing in the same zip code are peers in my setting.

Second, estimating peer effects is difficult owing to the twin identification challenges of selection and common shocks (Manski 1993). Individuals with similar unobserved characteristics may select into peer groups (i.e. selection). For example individuals employed at the same firm with the same job role may have similar educational backgrounds or similar levels of risk aversion. Such peer group members may also be subject to common unobserved shocks. For instance, peers employed at the same firm may be subject to firm-level shocks.

I address these identification challenges using a two-pronged strategy. First, I study financial distress situations that are triggered by health shocks. Specifically, I identify instances when individuals default on medical bills worth more than $10,000, accrued within a month.[3] I use these instances in a difference-in-differences framework to examine their effects on the peer individuals (i.e. the treated group). To the extent that health shocks are idiosyncratic in nature, they help ensure that financial distress situations are not driven by common shocks affecting the peer group. Second, the choice of the control group further aids in addressing these challenges. The control group comprises individuals employed at the same firm with the same job role as the treated individuals but who reside in a neighboring state and hence work at a different establishment (within the same firm).

[3]Confining the analysis to defaults on amounts greater than $10,000 ensures that I isolate defaults arising from sudden shocks and not regular monthly medical payments as this amount is much greater than the average monthly healthcare expenditure of approximately $1,500 for U.S. individuals. However, the results are not sensitive to using this cut-off.

The similarity in job profiles between treated and control individuals helps control for firm-level correlated shocks, and helps ensure that control individuals are very similar to treated individuals and are likely to have similar unobserved characteristics. This similarity along with the individual fixed effects included in the specification helps overcome the selection problem. Thus, this methodology compares changes in outcome variables, between the periods before and after defaults, for individuals whose peers default on medical bills to similar changes for control individuals.

A remaining concern with this specification is that the treated individuals may be subject to local economic conditions or establishment-level shocks that may be correlated with their peers' health shocks. I test for this by comparing the labor market outcomes of the treated and control individuals, and do not find any significant difference. Specifically, I find that income trends are statistically indistinguishable for the treated and control individuals following peer distress. Further, the likelihood of being employed at the same establishment with the same job role is also not significantly different for the treated individuals. This suggests that common establishment-level shocks are not driving the results. I also find indistinguishable trends in house price indices and median incomes for zip codes where treated individuals reside and zip codes where control individuals reside. This suggests that both treated and control individuals are subject to similar local economic conditions. To further help alleviate this concern, I show that estimates for all tests are robust to controlling for a cubic term in individual-level income, average income at the establishment level and house price indices at the zip code level.

I find that leverage, defined as the debt-to-income ratio, declines on average by 8.3 percentage points more for treated individuals relative to the control group following peer distress. This effect is economically significant as it corresponds to 4.2% of the sample mean. This decline starts almost immediately following peer distress and lasts for at least five years. This effect is driven by a decline in all forms of debt - credit card, auto loans, and home loans. Credit card debt, auto loans and home loans decline by an additional $210 (5.4%), $194 (1.7%), and $5,160 (4.6%) respectively for treated individuals relative to the control group.

The reduction in debt occurs as individuals borrow less on the intensive margin. Thus while they are no less likely to open a new account, conditional on opening an account, they borrow smaller amounts. At the same time, they pay higher fractions of their debt. Treated individuals also save more while their income remains constant, thus suggesting that their consumption declines. As a result, they have lower delinquency rates and better credit scores.[4] The estimates correspond to a social multiplier of -0.16 for defaults.

Peer financial distress may lead to lower borrowing if individuals increase their assessment of expected costs of financial distress. Alternatively, their preferences may change following peer distress. However, the difficulty in observing preferences and the fact that preferences can change through time make it difficult to distinguish the preference channel from the expectations channel. Henceforth, I refer to their combination as the 'learning channel.'[5] If the results are driven by the learning channel, one would expect the effects to be stronger when the costs of peer distress are higher. I test for this differential effect by exploring the heterogeneity in costs of financial defaults across different states, as measured by strictness of wage garnishment laws. I find that the negative effects of peer distress on leverage and debt are magnified in states with higher costs of defaults.

A unique prediction of the learning channel is that the degree and perhaps the direction of updating should depend on ex-ante priors. Individuals with a 'high' prior estimate of the costs of distress may update their beliefs down while those with 'low' prior estimate may update their beliefs up (or less downwards). I use two proxies for individuals' prior beliefs about costs of financial distress to test this prediction. First, I use ex-ante leverage and find results consistent with the learning channel. Individuals with low ex-ante leverage increase borrowing while those with high ex-ante leverage decrease borrowing following peer distress.

Second, I proxy for priors based on individuals' early life experiences. Recent literature argues that individuals who experience a recession during their formative years tend to be more conservative (Malmendier and Nagel 2011; Knupfer, Rantapuska, and Sarvimaki 2017; Schoar and Zuo 2017). Such individuals may have a high prior expectation about the costs of financial distress. Hence, they may update their beliefs to a lesser extent when exposed to peer financial distress. Consistent with this argument, I find weaker effects for individuals who experienced a recession during their formative years as compared to those who did not.

As mentioned before, the peer consumption channel would also predict a reduction in leverage following peer distress. If peer distress reduces peer consumption, it may negatively affect individual consumption and total borrowing. However, the increase in leverage among individuals

[4]This is further inconsistent with establishment-level shocks or local economic conditions driving the results. If the effect is driven by establishment-level shocks, one would expect higher likelihood of default and lower credit scores for treated individuals, but I find opposite results.

[5]In other words, all results that are consistent with the learning channel would be consistent with changes in both beliefs and preferences.

with low ex-ante leverage is inconsistent with the peer consumption channel. Notwithstanding this inconsistency, I test the importance of the peer consumption channel by exploring heterogeneity across individuals who continue to be peers with the distressed individual and those who cease to be peers within three months following distress. The consumption channel would predict a smaller peer effect for the latter group as their social interaction with the distressed individual would be limited. On the other hand, the learning channel would not predict any difference as both groups of individuals would have had the opportunity to learn from the distress episode. I find no statistically distinguishable difference in the treatment effects across the two groups. This is further inconsistent with the peer consumption channel.

The evidence suggests that my findings are driven by peer financial distress rather than individuals updating the probability of experiencing a health shock. Controlling for the size of medical bills, I find significantly stronger effects for individuals whose peers filed for bankruptcy following their health shock relative to individuals whose peers experienced a health shock but did not file for bankruptcy.[6] This suggests that individuals react to financial shocks rather than health shocks. If they were only reacting to the health shocks, one would not expect to observe such heterogeneity. I also estimate the effect of peer financial defaults (not associated with health shocks) on individual leverage. Since financial defaults can be a result of local economic shocks, in this specification, I identify the control group from within the same zip code as the treatment group. Specifically, the treated group comprises individuals whose peers default on their credit card, auto or home loan payments, while the control group comprises individuals who reside in the same zip code and work for the same firm (and establishment) as the treated individuals, have a similar level of income but a different job role.[7] Using this specification, I find that individuals reduce leverage following a peer default on a financial loan.

The peer effects documented here can potentially aggregate up to have significant macro-economic implications. Simple back-of-the-envelope calculations, with some caveats, suggest that these peer effects can explain a decline of up to $213.31 billion in total household debt between 2011 and 2015. This decline corresponds to 1.82% of the total household debt of $11.75 trillion as of January 2011. This effect is economically significant as it corresponds to 8.30% of the average four-year absolute

[6] Though all peers experiencing distress default on their medical bills, not all of them file for bankruptcy.

[7] The restriction on annual income for treated and control individuals is that both incomes belong to the same $500 bucket. For example, if the treated individual earns in the range between [30K,30.5K], the control individual also earns in the same range.

changes in real debt (between 2000 and 2015). These findings also suggest that such peer effects can potentially generate a negative correlation between the time-series patterns in household debt and number of defaults in the economy. Specifically, these results would suggest that in times of no defaults, debt may increase, but when a sufficient number of individuals default, debt will decline for long periods of time until new cohorts appear and start dominating.

This paper makes three primary contributions. First, I document the role of distress spillovers in shaping individual demand for leverage and debt. These findings directly contribute to the literature examining the determinants of household leverage and debt. Second, I show that financial defaults lead to lower delinquency rates among peers through the learning channel. This contrasts with the literature on foreclosures that finds a default contagion behavior owing to the price effects channel. Finally, I introduce a new source of variation to examine default spillovers by using health shocks. This variation can be used to study a number of questions and outcomes relating to default spillovers.

1 Related Literature

This paper is directly related to the literature examining the determinants of household leverage and debt. Most of this literature focuses on supply side factors like lax standards of the banking sector, transfer of risks, and the resulting lack of discipline in applying sound banking standards (e.g. Mian and Sufi 2009; Demyanyk and Van Hemert 2012) to explain both levels and trends in household debt. I contribute to this literature by highlighting the role of distress spillovers in determining the demand for leverage and debt. The results suggest that distress spillovers can aggregate up to have an economically meaningful effect on the total household debt in the U.S.

This paper also relates to the literature on default and foreclosure spillovers. The studies in this literature document the effect of foreclosures on house prices (Campbell, Giglio, and Pathak 2011; Anenberg and Kung 2014; Gerardi, Rosenblatt, Willen, and Yao 2015) and show that through changes in house prices, foreclosures lead to default contagion behavior (Goodstein, Hanouna, Ramirez, and Stahel 2011; Munroe and Wilse-Samson 2013; Towe and Lawley 2013; Agarwal, Ambrose, and Yildirim 2015a; Gupta 2017) and reduction in consumer demand (Mian, Sufi, and Trebbi 2015). In contrast, my paper examines default spillovers for different types of defaults and documents the changes in peers' borrowing behavior owing to the learning channel. As opposed to contagion in foreclosures, I find that other types of defaults lead to lower delinquency rates among peers.

The results documented here also contribute to a research effort analyzing how personal experiences affect individual financial decisions through their influence on beliefs and preferences (Choi, Laibson, Madrian, and Metrick 2009; Malmendier and Nagel 2011; Malmendier, Tate, and Yan 2011; Cameron and Shah 2013; Callen, Isaqzadeh, Long, and Sprenger 2014; Anagol, Balasubramaniam, and Ramadorai 2016; Koudijs and Voth 2016; Malmendier and Nagel 2016; Bernile, Bhagwat, and Rau 2017; Bharath and Cho 2017; Knupfer et al. 2017). A closely related paper in this literature is Bailey, Cao, Kuchler, and Stroebel (2017), where the authors use data for individuals based in Los Angeles and document that house price experiences within the social network of an individual contribute to the formation of individuals' housing market expectations. In contrast, I use data for over 30 million individuals in the U.S. and document asymmetric effects of peer experiences of financial distress on individual leverage and borrowing behavior.

Finally, this paper is related to the literature that examines the role of peers in various types of financial choices like retirement savings (Duflo and Saez 2002; Beshears, Choi, Laibson, Madrian, and Milkman 2015), consumption (Kuhn, Kooreman, Soetevent, and Kapteyn 2011; Agarwal, Qian, and Zou 2017), refinancing (Maturana and Nickerson 2017), loan repayments (Breza 2016; Breza and Chandrasekhar 2016) and stock market participation (Hong, Kubik, and Stein 2004; Brown, Ivkovic, Smith, and Weisbenner 2008; Kaustia and Knupfer 2012). My paper differs from this literature in three important ways. First, most extant papers document a conforming behavior where individuals mimic their peers. In contrast, the results in this paper show non-conforming outcomes. Second, the data allows me to use improved peer definitions for a large representative sample of the U.S. population. Third, the peer effects of financial distress documented in this paper can potentially aggregate up to have macro-economic implications.

2 Data

2.1 Data Sources & Description

The analysis in this paper leverages anonymized data on individual credit profiles and employment information from Equifax Inc., one of the three major credit bureaus, which is involved in the collection and transmission of data on credit histories and employment for individuals in the U.S. This is one of the first papers to use such detailed credit and employment data on the U.S. population.

The anonymized credit data contains information on the credit histories for all individuals (with a credit history) in the U.S. for the period between 2010-2015. This includes anonymous information on historical credit scores along with disaggregated individual credit-account level information such as account type (e.g. credit card, home loan, etc.), borrower location, account age, total borrowing, account balance, any missed or late payments, and defaults. In some cases, information on payment histories for various accounts is also available. The credit data also includes the universe of all bankruptcy filings and accounts under collections.

Accounts are reported under collections when individuals default on their loans or bills, and lenders or other third parties attempt to recover this amount owed. Importantly for this paper, when individuals fail to pay their medical bills or negotiate a payment plan with the hospital, these bills are usually sold to collection agencies/debt buyers (typically six months following the first due date). The collection agencies then report these accounts to the credit bureaus. This collections data includes information on account type, amount owed on the account, date of first missed payment, account status, etc.

The employment data covers over 30 million individuals employed at over 5,000 firms in the U.S. This granular data includes anonymous information on each employee's wages, salary, bonus, average hours worked, job title, job tenure, firm-level details, and whether the employee remains employed at the firm at a given point in time. This information is self-reported by all firms that subscribe to income and employment verification services provided by Equifax Inc. These firms provide this information for all their employees on a payroll-to-payroll basis. However, firms that only subscribe to the employment verification service may not provide income details.

2.2 Sample Construction

I begin by identifying individuals who experienced 'health shocks' during the period between 2011-2014.[8] To this end, I identify accounts in the collections data classified as 'medical bills' with the collections amount of at least $10,000 that was accrued within a month. Confining the sample to accounts with large collection amounts helps ensure that these are defaults on unexpected health shocks and not scheduled monthly payments. I merge this account information to the intersection

[8] I don't include years 2010 and 2015 in order to have at least twelve months of data before the first and after the last shocks respectively.

of the credit and employment data in order to obtain zip codes of residences and employment information for these individuals.

Next, I identify peers (i.e. treated individuals) for every individual in this subset of the collections database by identifying individuals that reside in the same zip code and are employed at the same firm with the same job role as individuals who defaulted on their medical bills. Further, for each peer I find control individuals by identifying those employed at the same firm with the same job role as the peers but living in a neighboring state (and hence employed at a different establishment within the same firm) whose peers did not default during the sample period. Finally, I merge credit and employment information for the peer group and the control group to obtain a panel over the 72-month period between 2010-2015. This results in a sample of 11,317,643 observations with 46,590 and 139,811 individuals belonging to the peer group and control group respectively. These individuals are associated with 6,656 individuals who defaulted on their medical bills.

Following similar steps, I construct the other two samples used in the second specification where the control individuals are defined as those residing in the same zip code and employed at the same firm as the treated individuals but with a different job role. The first of these samples uses financial defaults (i.e. credit card, auto loans, or mortgage defaults not associated with health shocks) as events. In this case, I begin by identifying individuals who defaulted on financial products that led them into bankruptcy. Confining the sample to defaults that led to bankruptcy ensures that these are large financial shocks and I'm not capturing transitory shocks, lapses in payments for non-financial reasons, or data errors. For these individuals, I identify the treated group following the method described above and the control group using the new definition for control individuals. The final sample is used in a robustness test that employs medical defaults as events in the second specification. The only way the sample construction differs in this case is how the control group is selected.

2.3 Sample Statistics

Table 1 reports summary statistics for the variables used in the analysis. Each variable is reported for observations that have non-missing values. The mean debt-to-income ratio in the sample is 1.96 while the mean total debt is $60,493. The mean credit card debt among individuals with an open credit card account is $3,895 while the mean home loan debt among individuals with an open mortgage account is $128,402.

The next few variables describe the characteristics of different components of debt. The mean likelihood of opening a new credit card account in any given month is 1%, while that of opening a new auto and home loan account is 0.1% and 0.01% respectively. The mean credit card monthly payment for the subset of individuals is $578, while mean payments made on auto and home loan accounts are $370 and $1,715 respectively. Note that the monthly payments made by individuals on different types of debt are only available for a fraction of the sample.[9]

The variable *Delinquency* measures the likelihood of delinquency in a given month and is constructed as a dummy variable that takes a value of one during the months when an individual becomes more than 90 days late on any account. The average monthly delinquency rate in the sample is 1%. Finally, the median credit score is 645, which is similar to the median credit score for the U.S. population of 638.

The median monthly income in the sample is $2,276, which is lower than the median income for the U.S. population of $3,450 as reported by the Bureau of Labor Statistics (BLS). This is plausibly driven by the fact that individuals with lower income are more likely to default on their medical bills, and hence their peers with similar income are more likely to be in the sample.

Table 2 compares summary statistics for different characteristics of the treated and control groups in the sample where all variables are calculated for the third month before treatment (i.e. the base month in the analysis). The last column reports the difference in means between the two groups, across different dimensions, along with the statistical significance. The estimates suggest that the treated and control groups are statistically similar along all dimensions, except age. The average treated individual is 149 days younger than the average control individual. To ensure that this difference doesn't bias the analysis, I control for age in various specifications.

2.4 Sample Representativeness

A potential concern is how representative the employment database and the sample are. I address this concern by comparing various characteristics of individuals within these databases to the population.

As detailed in Gopalan, Hamilton, Kalda, and Sovich (2017b), the employment data is geograph-

[9]Lenders are not required to report the exact payment amounts to the credit bureau but only if the individual is current on the account or not. As a result, some lenders choose not to report these amounts.

ically representative of the U.S. population. The industry distribution is also similar to that reported by the BLS, although I'm unable share the exact figures for confidentiality purposes. The income distribution is representative of the U.S. workforce as well. For instance, the median individual in the data is 41 years old with an annual salaried income of $41,015. This is comparable to the U.S. workforce where the median individual with full-time employment is 41.9 years of age, is salaried, and earns an income of $41,392.

Next, I compare the distribution of individual borrowing and credit scores for individuals in this database to those in the population. Figure 1 plots this comparison. The first three rows plot different categories of accounts (i.e. credit card, auto and home loans) while the fourth row plots credit score. The column on the left plots the empirical cumulative density function (CDF) for the number of accounts in different categories (i.e. extensive margin) while the one on the right plots the kernel density function for the amount of debt conditional on having an open account in the given category (i.e. intensive margin). The solid line represents individuals covered in the employment database while the dashed line represents the population.

The first row plots the CDF and density functions for credit card debt. The CDF on the left suggests that individuals in the employment database are likely to have a greater number of credit card accounts than the population. Conditional on having an account, these individuals hold higher credit card balances; however the difference is not significant. Similarly, the second row plots characteristics for auto loans and shows that individuals in the employment database are likely to have a higher number of auto loan accounts than the population. However, conditional on having an account, they have similar balances as the population. The third row represents home loans and shows a similar comparison between the employment database and the population. Finally, the fourth row plots credit scores and reveals that individuals in the employment dataset have slightly worse scores than the population.

The sample comprises peers of individuals who defaulted on their medical bills worth greater than $10,000 between 2011-2014 and are covered in the employment dataset. Hence, it is important to understand the representativeness of the employment database in terms of coverage among individuals who experienced such defaults. To this end, I compare individuals who experienced such defaults and are covered in the database to the population of individuals who experienced such defaults. Figure 2 plots this comparison. As before, the first row plots the CDF and density functions for credit card debt. The plots suggest that both groups are similar in terms of credit card borrowing both at the extensive and intensive margins. They are also similar in terms of borrowing on auto and home loans on both margins. Finally, the groups are indistinguishable in terms of credit scores as well. Overall, this comparison suggests that the sample spans a representative population of defaults.

3 Empirical Challenges & Methodology

Estimation of peer effects poses two significant problems. First, identifying the relevant peer network is difficult given the lack of data. Second, identification is difficult owing to the reflection problem (Manski 1993).

The definition of relevant peer networks is severely limited by data availability. Ideally, one would survey individuals, reconstruct the web of interactions they span (family, friends, co-workers, etc.), and then collect socio-economic information on both ends of each node. In practice, this task is rarely accomplished (exceptions are the Add Health data in the U.S. and the Indian microfinance clients network of Banerjee, Chandrasekhar, Duflo, and Jackson (2013)). Most existing research either uses small specific samples where peers are well identified or defines peers in a generic manner for a large representative sample, for example individuals living in the same region (e.g. city), individuals sharing similar socio-demographic characteristics, etc. In contrast, the rich dataset allows me to define peers in a very specific manner for a large representative sample of the U.S. population. I define peers as individuals residing in the same zip code and employed at the same firm with the same job role.

As discussed in De Giorgi, Frederiksen, and Pistaferri (2017), co-workers are a credible peer group owing to two reasons. First, if peer effects increase with the time spent together, co-workers are obvious candidates for the ideal peer group as they spend most of their day together. Second, evidence from sociology and labor economics shows that owing to job search mechanisms friendships often lead to individuals being co-workers (Holzer 1988). Hence, not only do co-workers become friends; in some cases it is actually friendship that causes co-workership. In my setting, peers are not only co-workers but also neighbors. Hence they form an even more credible peer group. Nonetheless, my network definition may not be perfect as some individuals who are peers in my setting may not actually interact with or influence others in the group. However, this would bias me against finding any effect as these individuals may not react to financial distress experiences of other individuals in their peer group.

Any estimation of peer effects also faces the twin identification challenges of selection and common shocks. I address these challenges by using 'health shocks' to identify financial distress situations that are potentially not correlated to peers' financial conditions. Specifically, I identify instances when individuals default on medical bills worth more than $10,000 that were accrued within a month. I use these events in a difference-in-differences framework where the control group comprises individuals employed at the same firm with the same job role as the treated individuals but who live in a neighboring state and hence work at a different establishment (within the same firm).

To the extent that 'health shocks' are idiosyncratic in nature, they help ensure that financial distress situations are not a result of common shocks affecting the peer group. The choice of the control group further aids in addressing the identification challenges. Specifically, the similarity in job profiles ensures that both treated and control individuals are very similar and hence likely to have similar unobserved characteristics. This helps overcome the selection problem in conjunction with the individual fixed effects included in the specification. Further, this definition of control individuals also helps control for firm-level correlated shocks.

Thus, I estimate variants of the following model:

$$y_{i,t} = \delta_i + \delta_t + \beta \times PeerShock_i \times Post_t + \gamma \times X_{i,t-1} + \epsilon_{i,t} \tag{1}$$

where the dependent variables represented by $y_{i,t}$ include leverage (debt-to-income ratio), various components of debt (credit card, auto and home loans), credit card spending, loan origination amounts, payments, credit score, account openings and delinquencies for individual i as of end of month t.

The main difference-in-differences term is $PeerShock \times Post$, where $PeerShock$ is a dummy variable that takes a value of one for individual i if her peer experiences distress owing to a health shock, and $Post$ is a dummy variable that takes a value of one during the months following such distress situations. The specification controls for a vector of lagged variables represented by $X_{i,t-1}$ including a cubic term in monthly income and average income at the establishment level that control for employment level changes, age that controls for life cycle effects, and zip code level house price indices that control for local economic conditions to an extent. δ_i are individual fixed effects whose inclusion ensures that the coefficient of interest, β, is estimated using within individual variation in the dependent variable and δ_t are year-month fixed effects that control for economy-wide time trends. The standard errors are corrected for heteroskedasticity and autocorrelation, and are double clustered at the individual and year-month level.

The coefficient on $PeerShock \times Post$ compares changes in the dependent variable before and after peer financial distress, for individuals whose peers experienced distress, to changes for similar individuals whose peers did not experience distress. Panel A of Figure 3 illustrates this variation with an example. Consider a sales representative living in zip code Z and employed at firm Y who experiences financial distress induced by health shock. This methodology compares the borrowing behavior of another sales representative that also resides in zip code Z and is employed at firm Y to a third sales representative that is employed at the same firm Y but lives in a neighboring state.

The underlying assumption of this framework is that, if not for peer distress, the two sets of individuals would follow parallel trends; that is, the change in outcome y for individuals whose peers default on a medical bill would have been the same as for similar individuals whose peers did not default.

To examine the dynamics of the effect of peer financial distress on individual borrowing and to test for parallel trends before treatment (i.e. pre-trends), I estimate the following dynamic difference-in-differences model:

$$y_{i,t} = \delta_i + \delta_t + \sum_{s=-13}^{-1} \beta_s \times Pre - PeerShock(-s) + \sum_{s=0}^{13} \beta_s \times PeerShock(s) + \gamma \times X_{i,t-1} + \epsilon_{it} \qquad (2)$$

where the subscripts and dependent variables are same as before, but $PeerShock$ has been interacted with event time. Specifically, $Pre - PeerShock(-s)$ ($PeerShock(s)$) is a dummy variable that takes a value of one for individual i, 's' years before (after) her peers experience distress. Since there are individual-month observations more than thirteen months before and after peer distress, there is one dummy variable each for multiple months at the two end points. That is, $Pre - PeerShock(-13)$ ($PeerShock(13)$) equals one for individual i, for all months greater than and equal to thirteen months before (after) peer distress. The model is fully saturated with the third month before peer distress as the excluded category, i.e. $Pre - PeerShock(-3)$ is not included in the specification. Therefore, the coefficients on $Pre - PeerShock(-s)$ ($PeerShock(s)$) compare the change in the dependent variable between 's' months before (after) peer distress and three months before the distress to similar changes for similar individuals whose peers did not experience distress.

A remaining concern with this specification is that the treated individuals may be subject to local economic conditions or establishment-level shocks that may be correlated with their peers' health shocks. The control variables included in the specification account for these omitted variables. However, to the extent that these control variables do not fully capture the local shocks, they still remain a concern. I directly test for these concerns by comparing trends in local economic conditions between treated and control zip codes (i.e. where treated and control individuals reside respectively) using a dynamic difference-in-differences framework similar to Equation 2. Figure 4 plots the coefficients of these regressions where house price index (HPI) and median income at the zip code level are used to proxy for economic conditions.[10] The plots show that the trends in HPI and median income are statistically indistinguishable for treated and control zip codes. This suggests that both treated and control individuals are subject to similar local economic conditions.

I also compare labor market outcomes for treated and control individuals following peer distress using Equation 1. Table 3 reports estimates for this comparison. The estimates in Columns (1) and (3) suggest that income trends are statistically indistinguishable for the treated and control individuals following peer distress. Further, the likelihood of being employed at the same establishment with the same job role is also not significantly different for the treated individuals (Columns (2) and (4)). This suggests that peers are not subject to common establishment-level shocks that may be correlated with health shocks for some individuals.

3.1 Financial Shocks (Specification II)

A potential concern with the above specification is that the results may be specific to health shocks; i.e. peer individuals may react to health shocks instead of financial shocks. To examine if this is indeed the case, I estimate the effect of defaults on financial loans (not associated with health shocks) on peer leverage. Specifically, I identify instances where individuals default on credit card, auto or home loan payments and file for bankruptcy within a year. Confining the analysis to defaults that lead to bankruptcy ensures that these are large financial shocks and that I'm not capturing transitory shocks, lapses in payments for non-financial reasons or data errors.

Since defaults on financial loans can be a result of local economic shocks, in this specification, I identify the control group from within the same zip code as the treatment group. Specifically, the

[10] Median income at the zip code level is only available bi-annually, and hence this analysis is done at the bi-annual frequency.

treated group comprises individuals whose peers default on their credit card, auto or home loan payments, while the control group comprises individuals who reside in the same zip code and work for the same firm (and establishment) as the treated individuals, have a similar level of income but a different job role. The annual income for treated and control individuals belongs to the same $500 range. For instance, if the treated individual earns an annual income between $30,000 and $30,500, the control individual also earns in the same range. This definition for the control group allows me to control for establishment-level employment and zip code level economic shocks.

Panel B of Figure 3 illustrates this variation with an example. Again consider a sales representative living in zip code Z and employed at a firm Y who experiences financial distress (not related to health shocks). This methodology compares the borrowing behavior of another sales representative who resides in zip code Z and is employed at the firm Y to a flight attendant who lives in zip code Z and is employed at the same firm Y. Thus, I estimate regressions similar to Equations 1 and 2 where $PeerShock$ is now associated with financial shocks (that are not related to health shocks).

This specification captures the differential communication between individuals with the same job roles and those with different job roles. To the extent that individuals in the control group communicate with individuals experiencing distress, the estimates will be attenuated. In this sense, the estimates in this specification capture the lower bound of the effect.

4 Empirical Results

In this section, I examine the effect of peer financial distress on individual borrowing behavior using individual leverage, different components of debt, likelihood of originating new debt, savings, loan performance and credit score as the main dependent variables.

4.1 Peer Financial Distress & Individual Level Debt

Table 4 reports estimates for the effect of peer financial distress on individual leverage and total debt estimated using variants of Equation 1. The analysis uses debt-to-income ratio as a measure of individual leverage, calculated as the ratio of total debt at the end of the month and income. Column (1) reports estimates for the effect of peer financial distress on debt-to-income ratio using a specification that does not include control variables. The coefficient shows that debt-to-income ratio declines by 6.1 percentage points (pp) more for treated individuals relative to the control group following peer financial distress. The magnitude of the effect is economically significant as it corresponds to a decline of 3.1% relative to the mean debt-to-income ratio in the sample. Column (3) reports similar estimates for the specification that includes a cubic term in income, age, average income at the establishment level and house price indices as control variables, and finds a stronger result as the estimate shows that debt-to-income ratio declines by 8.3 pp (4.2% of the mean) more for treated individuals relative to the control group.

The estimates reported in columns (2) and (4) show a similar effect of peer financial distress on total debt. The coefficient in column (2) shows that total debt for treated individuals declines by $2,338 more than for control individuals whose peers did not experience distress. As before, this effect is economically significant as it corresponds to 3.9% relative to the mean level of debt. Column (4) finds a similar result for the specification that includes all control variables.

The identifying assumption is that, if not for peer defaults, the two sets of individuals would follow parallel trends; that is, the change in outcome y for individuals whose peers default on a health shock would have been the same as for similar individuals whose peers did not default. Though this assumption cannot be empirically verified for the period post treatment, I test it for the period before treatment by estimating the effect of peer financial distress on individual leverage using Equation 2. Additionally, this estimation allows for a better exploration of the dynamics of the treatment effect.

Figure 5 plots these coefficients for debt-to-income ratio (panel A) and total debt (panel B) for 12 months around peer distress. The horizontal axis represents months relative to peer distress, and the vertical axis represents the magnitude of the coefficient estimates. I omit the month which is three months prior to peer distress as the base month. Hence, each estimate on the plot compares the difference in the outcome variable for the third month before peer distress and a corresponding month for treated individuals, relative to the same difference for control individuals. The vertical bars represent confidence intervals at 95% level.

The plots show that estimates for the period before peer distress are not statistically different from zero, thus confirming that the trends in individual leverage and total debt for treated and control individuals are statistically indistinguishable for the period before peer distress. However, both leverage and debt decline significantly for treated individuals during the months following peer distress, and the effect lasts for at least a year.

I next examine the effect of peer distress on different components of debt to understand which component drives the results. I confine this analysis to individuals who have at least one account in the given category. For instance, when estimating the effect on credit card debt, the sample is confined to individuals who have at least one open credit card account during the sample period.[11] This is because individuals without any credit card accounts can't reduce their debt any further.

Table 5 reports estimates from this analysis. Columns (1) and (4) report estimates for the effect on credit card debt. The coefficient in column (1) shows that credit card debt for treated individuals declines by $210 more than for the control group following peer distress. As before, this effect is economically significant as it corresponds to 5.4% relative to the sample mean. Column (5) finds a similar result for the specification that includes control variables. The remaining columns explore the effects on auto and home loans. Columns (2) and (3) find that auto and home loans decline by $194 (1.7% of the mean) and $5,113 (3.9% of the mean) respectively more for treated individuals relative to the control group. In columns (5) and (6), I include the control variables and find similar estimates.

Figure 6 plots coefficients for credit card and home loans estimated using Equation 2. As before, the horizontal axis represents months relative to peer distress, and the vertical axis represents the magnitude of the coefficient estimates. The estimates for the period before peer distress are not statistically different from zero suggesting that the trends in credit card and home loans are statistically indistinguishable for treated and control individuals for this period. However, both credit card and home loans decline significantly for treated individuals during the months following peer distress, and the effect lasts for at least a year.

4.2 How Do Individuals Reduce Debt?

Individuals whose peers default on their medical bills may have lower debt relative to the control group owing to two different reasons - either they pay down their debt faster or borrow less. To identify the mechanism through which individual debt declines, I examine the characteristics of different components of debt separately.

Panel A of Table 6 reports results for the effect of peer distress on different credit card characteristics. The estimates show that treated individuals are not less likely to open new credit card accounts (Column (1)), but conditional on having an account, they spend less and have lower card utilization. Spending declines by $62 (6.9% of the mean), and utilization declines by 1.9 pp (4.5% of

[11] Note that this allows for individuals who get their first credit card account following treatment.

the mean) more for treated individuals relative to the control group (Columns (2) and (3)).[12] Even though treated individuals spend less on their accounts, their monthly payment does not decline, suggesting that they pay down larger fractions of their debt (Column (4)). Columns (5) through (8) find similar results with a specification that includes control variables.

Panel B reports similar results for auto loan characteristics. The estimates show that treated individuals are not less likely to originate new auto loan accounts (Column (1)). However, conditional on originating an account, treated individuals borrow $414 less than the control group (Column (2)). This effect is economically significant as it corresponds to 1.8% of the mean auto loan origination amount. Note that Columns (2) and (6) use a specification that includes firm fixed effects instead of individual fixed effects because including individual fixed effects would generate variation only among individuals who already had at least one auto account in the period before treatment, and thus would exclude individuals who opened their first auto account post treatment. Columns (3) and (7) show a similar effect for this subset of individuals. Even though treated individuals borrow less, their nominal payments are statistically indistinguishable from those of the control group (Column (4)). This suggests that they pay larger fractions of their debt.

Panel C finds similar results for home loan characteristics where treated individuals borrow less on the intensive margin but their nominal payments do not change relative to the control group. Conditional on originating a new home loan, treated individuals borrow 2.2% less than the control group following treatment.

Figure 7 plots coefficients for the effect on credit card utilization and home loan origination amounts estimated using Equation 2. As before, the estimates for the period before peer default are not statistically different from zero, suggesting that the trends in outcome variables are statistically indistinguishable for treated and control individuals for the period before treatment. However, credit card utilization and home loan origination amounts significantly decline for treated individuals during the months following peer default.

Overall, these results suggest that debt levels for individuals whose peers experience distress decline more than debt levels for the control group because they borrow less on the intensive margin and pay higher fractions of their debt.

[12] As mentioned earlier, payment and hence spending are only available for a fraction of the sample.

4.3 Borrowing vs Consumption

The results discussed so far show that individual debt declines following peer financial distress because individuals borrow less on the intensive margin and pay higher fractions of their debt. This can occur either when individuals consume less or borrow less for the same consumption. I test for this mechanism by examining the effect of peer financial distress on individual savings.

The data on savings comes from an investments dataset maintained by Equifax Inc. The anonymized investments data contains information on more than 45 percent of all U.S. consumer assets and investments ($>$ \$10 trillion in coverage) at the nine-digit zip code level (zip+4) broken down into age buckets. This data is available at the bi-annual frequency and includes information on investments in stocks, bonds, mutual funds, and several other investment vehicles. The data also includes information on household bank deposits and savings broken down by account type (e.g. savings, certificates of deposit, etc.). The data is sourced directly from banks, brokerage firms, and other financial entities.

Using this data, I define savings as the sum of total deposits, certificates of deposit, interest-bearing and non-interest-bearing checking account balances, and savings account balances. Table 7 reports estimates for the effect of peer distress on savings. Column (1) finds that savings for treated individuals increases by \$2,415 more than savings for the control group. Column (2) finds a similar result with the specification that includes control variables.

Taken together, the results show that individual borrowing declines, saving increases while income remains stable, implying that individuals consume less following peer financial distress. For example, in terms of auto (home) loans, individuals are not less likely to purchase a new car (house), but conditional on purchasing, they purchase a cheaper car (house).

4.4 Delinquencies and Credit Scores

Next I examine the consequences of this borrowing response in terms of changes in delinquency rates and credit scores. Table 8 reports estimates for this analysis. Column (1) reports estimates for the effect on the likelihood of delinquency (on any type of loan), and finds that individuals whose peers experience distress are 0.1 pp less likely to become delinquent in a given month relative to the control group. This effect is economically significant as it corresponds to a decline of 10% relative to the mean delinquency rate in the sample. This estimate corresponds to a social multiplier of -

0.16 for defaults. Column (2) reports estimates for the effect on credit score and finds that credit scores for treated individuals increases by 10 points more than scores for the control group following peer distress. Columns (3) and (4) find similar estimates with the specification that includes control variables.

5 Interpretation of Results & Underlying Mechanisms

This section discusses and examines the underlying mechanisms.

5.1 Learning

Peer financial distress may lead to lower borrowing if individuals learn that they are more likely or it is more costly to experience negative economic shocks than they expected. Alternatively, their preferences may change following peer distress. As discussed earlier, the difficulty in observing preferences and the fact that preferences can change through time make it difficult to distinguish the preference channel from the expectations channel. Hence, I refer to the combination of changes in beliefs and preferences as the learning channel. Any results consistent with the learning channel would be consistent with changes in both beliefs and preferences.

I begin by examining the heterogeneity of the effect among treated individuals based on differences in the costs of peer financial defaults. If the results are driven by the learning channel, one would expect the effects to be stronger when the costs of peer distress are higher. I use state-level variation in strictness of wage garnishment laws to generate differences in such costs. Wage garnishment laws allow creditors to garnish income from individuals who default on their loans or bills. Restrictions on such garnishments hinder the creditors' ability to recover the amounts owed and reduce the costs of default for the borrower.[13]

Following Lefgren and McIntyre (2009), I segment states into three categories based on wage garnishment restrictions. The first category is for those states that use the federal wage garnishment standard, allowing up to 25 percent of wages to be garnished as long as wages are above a threshold level of 30 times the federal minimum wage per week. A second group consists of states that allow garnishments but add restrictions beyond those mandated by federal law. Typically, this occurs by raising the threshold of wages that are protected or reducing the percentage of wages that can be

[13] A detailed account of the wage garnishment process can be found in Yannelis (2017).

garnished. The final group consists of states that either explicitly or implicitly eliminate effective wage garnishment.

To examine the heterogeneity in the effect of peer distress across these states, I use triple difference regressions that interact the difference-in-differences variable with a dummy variable corresponding to the type of state where the treated individual resides. Panel A of Table 9 reports results for this analysis. Consistent with the learning channel, the estimates suggest that the negative effects of peer distress on leverage and debt are more pronounced in states with higher costs of defaults, i.e. states that have the least restrictions on wage garnishments.

A unique prediction of the learning channel is that the degree and perhaps the direction of updating should depend on ex-ante priors. Individuals with a 'high' prior estimate of the costs of distress may update their beliefs down while those with 'low' prior estimate may update their beliefs up (or less downwards). I use ex-ante leverage as a proxy for priors about costs of financial distress and examine for differential response among individuals with different priors. Panel B of Table 9 reports estimates for this analysis where the difference-in-differences variable is interacted with dummy variables that correspond to whether the ex-ante leverage was above or below the median level. The specification includes median fixed effects to ensure that treated individuals are compared to control individuals with similar levels of ex-ante leverage. The estimates show that individuals with an above-median level of leverage before peer distress reduce their debt while those with below-median leverage increase debt.

I further analyze this asymmetric effect by dividing the sample into deciles based on ex-ante leverage. Panel A of Figure 8 plots estimates for this analysis. The specification includes decile fixed effects to ensure that treated individuals are compared to control individuals with similar levels of ex-ante leverage. The estimates show that individuals in the lowest three deciles based on ex-ante leverage increase their leverage following peer distress while those in the top five deciles decrease their leverage. Consistent with the learning channel, this effect is monotonic. However, the negative effect for the highest deciles is much stronger than the positive effect for the lowest deciles.

Panel B of Figure 8 reports similar estimates where the outcome variable is the delinquency rate. Individuals in the top five deciles have lower delinquency rates following peer distress. However, the delinquency rate for individuals in the bottom three deciles remains statistically indistinguishable from the control group even though the former individuals increase their leverage.

Panel C of Table 9 examines the interaction of ex-ante priors and costs of distress. Consistent with the results presented in Panel A, I find that negative effects are stronger and positive effects are weaker when the costs of distress are larger. The positive effect becomes statistically insignificant in states that have no restrictions on wage garnishment.

I also use a second proxy for priors based on an individual's early life experiences. Recent literature argues that individuals who experience a recession during their formative years tend to be more conservative (Malmendier and Nagel 2011; Knupfer et al. 2017; Schoar and Zuo 2017). Such individuals may have a high prior expectation about the costs of financial distress. They may also update their beliefs to a lesser extent when exposed to peer financial distress. To examine this heterogeneity, I interact the difference-in-differences variable with the dummy variable $Recession$ ($Non - Recession$) that takes a value of one for individuals who experienced (did not experience) recession between 18 and 23 years of age. Table 10 reports estimates for these regressions where the main statistic of interest is the difference between the coefficients on the two interacted terms. Consistent with the learning channel, I find that leverage and debt decline significantly more for individuals who did not experience a recession during their formative years.

5.2 Salience or Over-reaction

An alternative channel through which individuals may react to peer experiences of financial distress is salience. This channel states that individuals may put higher weight on the information arriving from peers' experiences of distress (even if there is no updating of beliefs) immediately following the event. Alternatively, when individuals update their beliefs, they may over-react to the event. Both these mechanisms would predict that the effect of peer distress on individual leverage would be short-lived. I test for these mechanisms by examining the long-term dynamics of the effect.

To this end, I use regressions similar to Equation 2 where the difference-in-differences variables are defined based on the number of quarters from peer shocks (instead of months). Figure 5 plots these coefficients for debt-to-income ratio (panel A) and total debt (panel B). The horizontal axis represents quarters relative to peer distress, and the vertical axis represents the magnitude of the coefficient estimates. For consistency, I omit the quarter that is three quarters prior to peer distress as the base period. Hence, each estimate on the plot compares the difference in the outcome variable for the third quarter before peer distress and a corresponding quarter for treated individuals, relative to the same difference for control individuals. The vertical bars represent confidence intervals at the

95% level.

As before, the plots show that the trends in individual leverage and total debt for treated and control individuals are statistically indistinguishable for the period before treatment. However, both leverage and debt decline significantly for treated individuals following peer distress, and the effect lasts for at least 20 quarters. This suggests that the effects are plausibly not driven by salience or over-reaction as there is no reversal in the effect for a long time.

5.3 Peer Effects of Consumption

The effects of peer financial distress on individual borrowing may be driven by the 'peer effects of consumption' channel, which states that individual consumption is a function of peers' consumption. For instance, this could work through 'keeping up with the Joneses' where individuals consume more to keep up with their peers' consumption. To the extent that distress reduces peer consumption, it can affect an individual's consumption as her incentives to keep up with peers may decline. However, the consumption channel would not predict an increase in leverage among individuals with low ex-ante leverage, nor would it predict weaker effects for those with early-life experience with recession.

Notwithstanding these findings, I test the importance of the peer consumption channel by exploring heterogeneity across individuals who continue to be peers with the distressed individual and those who cease to be peers within three months following distress. The consumption channel would predict a smaller peer effect for the latter group as their social interaction with the distressed individual would be limited. On the other hand, the learning channel would not predict any difference as both groups of individuals would have had the opportunity to learn from the distress episode.

Table 11 reports results for this heterogeneity, where the specification interacts $PeerShock \times Post$ with dummy variables $Peer$ and $Non-Peer$, that respectively take a value of one for individuals who continue to be peers and those who do not. The estimates show that there exists no significant heterogeneity in treatment effects across these individuals, which is consistent with the learning channel but inconsistent with the peer effects of consumption channel.

6 External Validity & Implications

6.1 Are Results Specific to Health Shocks?

A potential issue with the above specification is that the effects may be specific to health shocks, i.e. peer individuals may directly react to health shocks by updating the probability of facing health shocks rather than financial shocks. I address this concern in two different ways. First, I conduct cross-sectional tests that examine the heterogeneity of treatment effects across individuals whose peers filed for bankruptcy following health shocks and those whose peers did not file for bankruptcy while controlling for size of the medical bill. To the extent that the size of medical bills captures the intensity of health shocks, this generates variation in intensity of financial shocks while keeping the intensity of health shocks constant. I find that the treatment effect is significantly stronger for individuals whose peers filed for bankruptcy following health shocks (as reported in Table IA1 of Appendix IA). This suggests that individuals react (at least partially) to financial shocks. If they were only reacting to the health shocks, one would not expect to observe such heterogeneity.

Second, I estimate the effect of defaults on financial loans (not associated with health shocks) on peer leverage. Since defaults on financial loans can be a result of local economic shocks, I use the second specification discussed in Section 3.1. This specification isolates within firm-zip code variation to estimate the effect of peer financial distress on individual borrowing behavior. Table 12 reports results for the effect on individual leverage and debt estimated using this specification. The estimates suggest that the debt-to-income ratio declines by 9.7 percentage points more for treated individuals relative to the control group (Column (1)). I find similar results with the specification that includes control variables and $Firm \times ZIPCode \times Month$ fixed effects. Further, the estimates reported in Columns (2) and (4) show that individual debt declines by over $4,350 more for treated individuals relative to the control group.

Figure 10 plots the dynamics of this effect. As before, the plots show that the trends in individual leverage and total debt for treated and control individuals are statistically indistinguishable for the period before treatment. However, both leverage and debt decline significantly for treated individuals following peer distress and the effect lasts for over twelve months.

I also find results similar to the baseline specification for different components and characteristics of debt, as reported in Tables IA2 and IA3 of Appendix IA.

Overall, these results suggest that changes in borrowing behavior following peer distress are driven by financial shocks and are not specific to health shocks.

6.2 Is the Aggregate Effect Economically Significant?

To get some assessment of the aggregate effects, I conduct a simple back-of-the-envelope estimation. Specifically, conditional on the number of defaults that occurred in the U.S. between 2011 and 2014, I compute the total effect on household debt using my estimates. For medical defaults with amounts greater than $10,000 I use estimates from the first specification, while for credit card, auto or mortgage defaults that lead to bankruptcy filings I use estimates from the second specification. This calculation involves two caveats. First, I assume the same average effect even for individuals not included in my sample. Since my sample comprises over 30 million individuals and covers a representative sample of defaults in the U.S., this assumption may not be completely implausible. Second, based on my sample, I assume that individuals who default have seven peers on average.

The credit data for the population indicates that 71,596 individuals defaulted on their medical bills worth greater than $10,000 between 2011 and 2014. Under the assumption that the average effect on total debt is $2960.98 and the average number of peers is seven, this yields an aggregate effect of a decline of $1.48 billion in the total household debt. The credit data further indicates that 6,519,065 individuals defaulted on their credit card, auto or mortgage loans and filed for bankruptcy following default during this period. A similar calculation for these defaults yields an aggregate effect of $211.83 billion on total household debt. Thus, these peer effects can explain a decline of up to $213.31 billion in total household debt between 2011 and 2015. This decline corresponds to 1.82% of the total household debt of $11.75 trillion as of January, 2011. This effect is economically significant as it corresponds to 8.30% of the average four-year absolute changes in real debt (between 2000 and 2015).

7 Robustness

7.1 Peer Health Shocks: Within Firm-zip code variation

In this sub-section, I conduct an analysis that uses health shocks in the second specification. This analysis further helps alleviate the concern that results may be driven by local economic conditions because it isolates within firm-zip code variation. As discussed in Section 3.1, this specification captures the differential communication between individuals with the same job roles and those with different job roles. To the extent that individuals in the control group communicate with individuals experiencing distress, the estimates will be attenuated. Hence, one would expect to find smaller estimates with this specification as compared to the first specification.

Table 13 reports estimates for this analysis. The coefficient in Column (1) shows that the debt-to-income ratio declines by 4.6 percentage points more for treated individuals relative to the control group. Consistent with the attenuation bias, this estimate is smaller than the effect of 6.1 percentage points documented in Table 4. In Column (2), I find that total debt declines by $1,753 more for treated individuals relative to the control group. Columns (3) and (4) include a cubic term in monthly income, average income at the establishment level, age and zip code level house price indices as control variables along with the $Firm \times ZIPCode \times Month$ fixed effects. I find larger magnitudes with this more stringent specification.

7.2 Heterogeneity by Number of Peers

The effect of peer distress on individual borrowing should be greater for peers who have stronger relationships. I test this conjecture by examining the heterogeneity in the effect of peer distress for individuals belonging to small versus large peer groups. Individuals who belong to small groups are likely to have stronger relationships with their peers relative to those who belong to large groups.

Table 14 reports results for this heterogeneity, where the specification interacts $PeerShock \times Post$ with dummy variables $Above$ and $Below$, that respectively take a value of one for individuals with the above and below median number of peers. The estimates show that individual leverage and debt decline more for individuals who belong to smaller peer groups.

8 Conclusion

This paper documents that peer financial distress leads to an average decline in individual leverage and debt. This decline occurs as individuals borrow less on the intensive margin, pay higher fractions of their debt, and save more following peer distress. As a result, these individuals have lower delinquency rates and better credit scores. I document a social multiplier effect of -0.16 for defaults. The heterogeneity of treatment effect suggests that individual borrowing declines following peer distress because individuals update their beliefs about the likelihood or cost of experiencing negative economic shocks, or their preferences change. Overall, these results highlight the important role of distress spillovers in shaping the demand for individual leverage and debt.

These findings plausibly have important macro-economic implications. In particular, these results suggest that peer effects can further dampen consumption during times of recession when many individuals experience financial distress, and can potentially exacerbate the recession. These peer effects can also hinder the post-economic recovery as individuals de-lever and consume less for a long period of time.

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Figure 1: Sample Representativeness I

This figure compares the distribution of individual borrowing and credit scores for individuals in the employment database to that of the population. The first three rows plot different categories of accounts (i.e. credit card, auto loans and home loans) while the fourth row plots credit score. The column on the left plots the empirical cumulative density function (CDF) for the number of accounts in different categories (i.e. extensive margin) while that on the right plots the kernel density function for the amount of debt conditional on having an open account in the given category (i.e. intensive margin). The solid line represents individuals covered in the employment database while the dashed line represents the population.

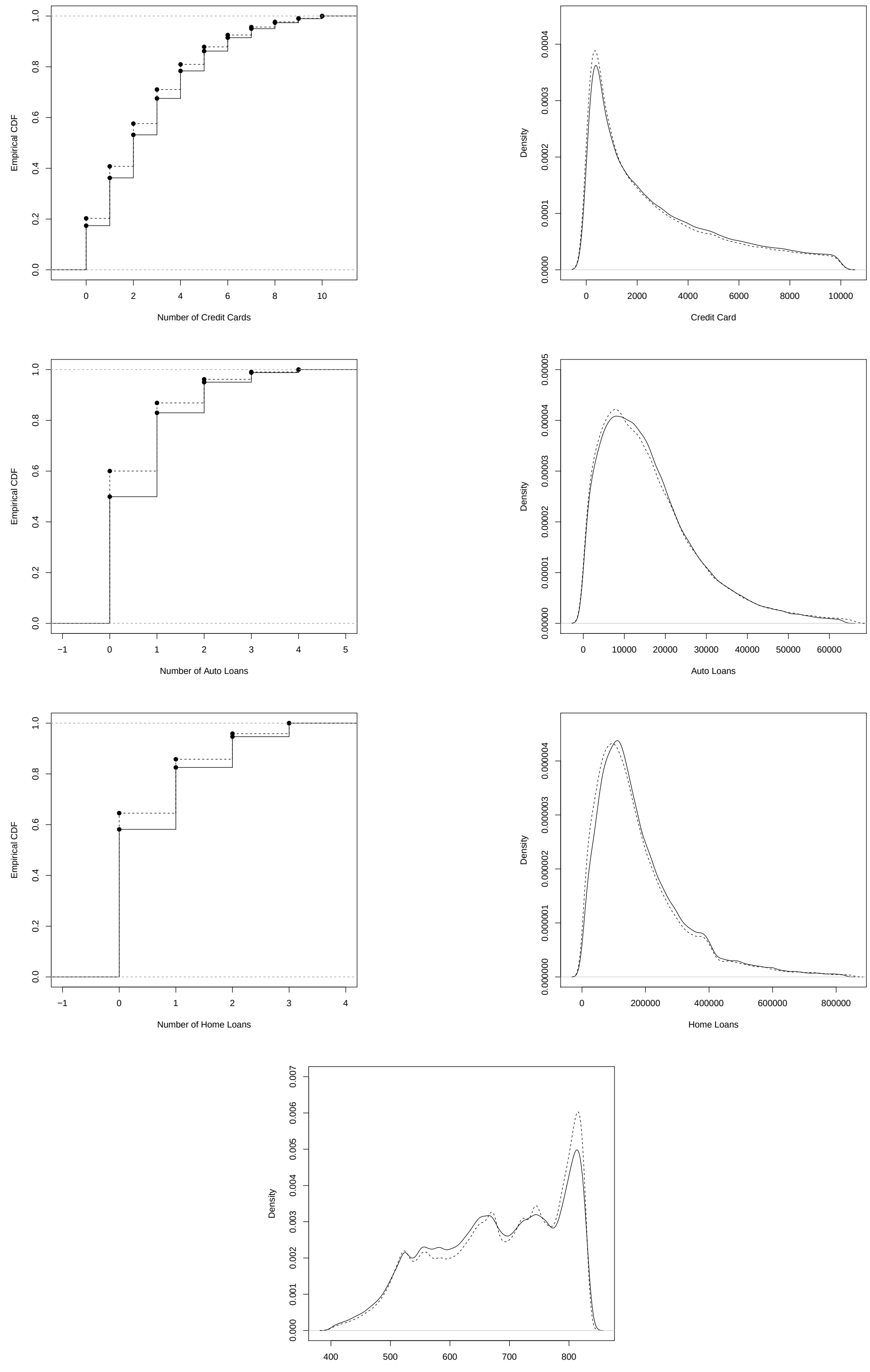


Figure 2: Sample Representativeness II

This figure compares the distribution of individual borrowing and credit scores for individuals who defaulted on their medical bills worth greater than ten thousand dollars between 2011-2014 and are covered in the employment database (hence their peers are in the sample) to those with similar defaults who are not covered in the employment database. The first three rows plot different categories of accounts (i.e. credit card, auto loans and home loans) while the fourth row plots credit score. The column on the left plots the empirical cumulative density function (CDF) for the number of accounts in different categories (i.e. extensive margin) while that on the right plots the kernel density function for the amount of debt conditional on having an open account in the given category (i.e. intensive margin). The solid line represents individuals covered in the employment database while the dashed line represents the population.

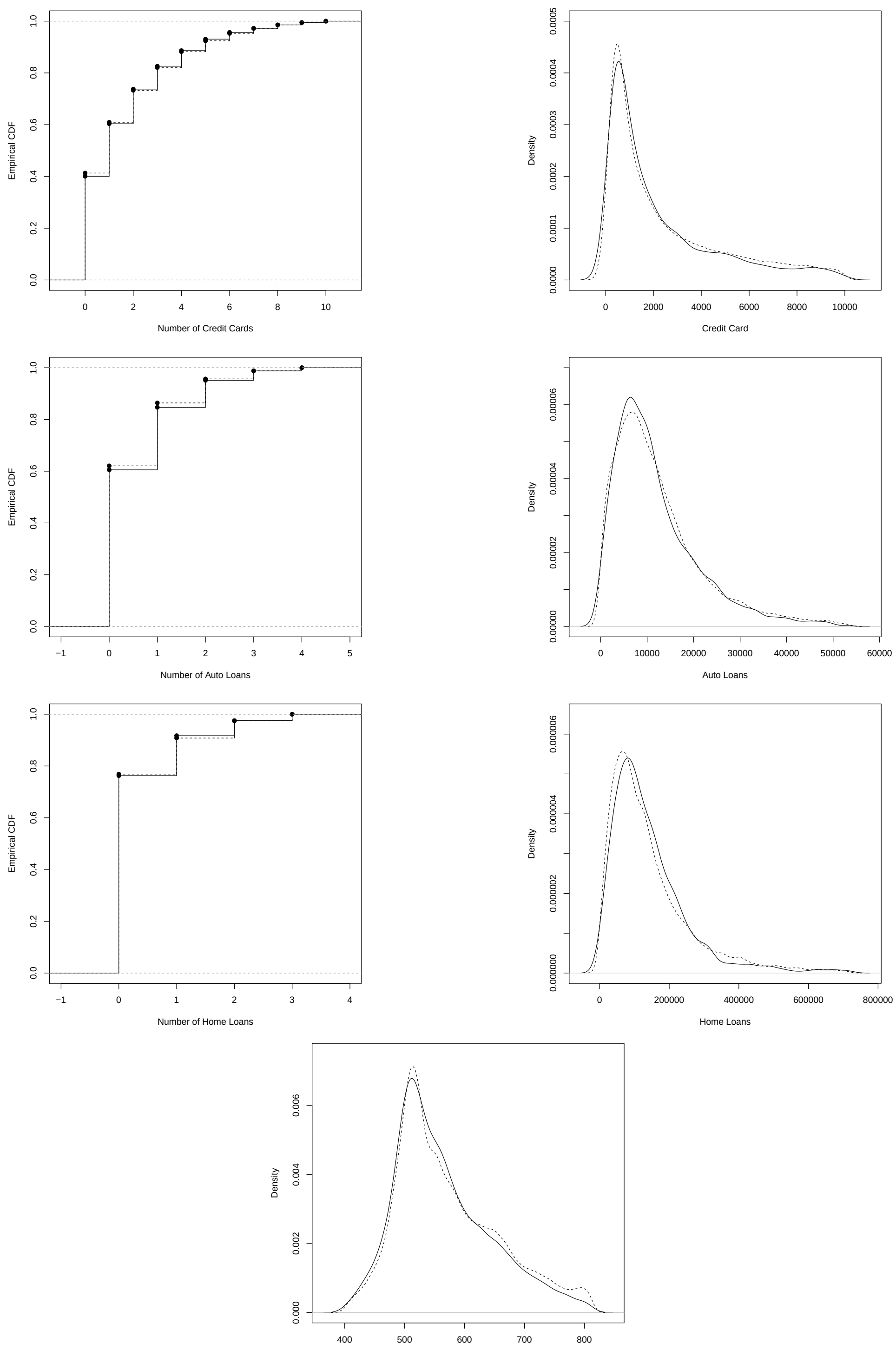


Figure 3: Description of Variation

This figure describes the variation used in different specifications arising from the two dimensions in the difference-in-differences analysis.

Example : Sales Rep A experiences financial distress (living in zip code Z and employed at firm Y)

First Difference: (Post-Distress) - (Pre-Distress)	Pre-Distress	Post-Distress
Second Difference: Treated-Control	Sales Rep B living in zip code Z and employed at firm Y (Treated)	Sales Rep C living in zip code Z1 (in a neighboring state) and employed at firm Y (Control)

Panel A : **Specification I : Comparing Individuals with Same Job Roles but Living in Neighboring States**

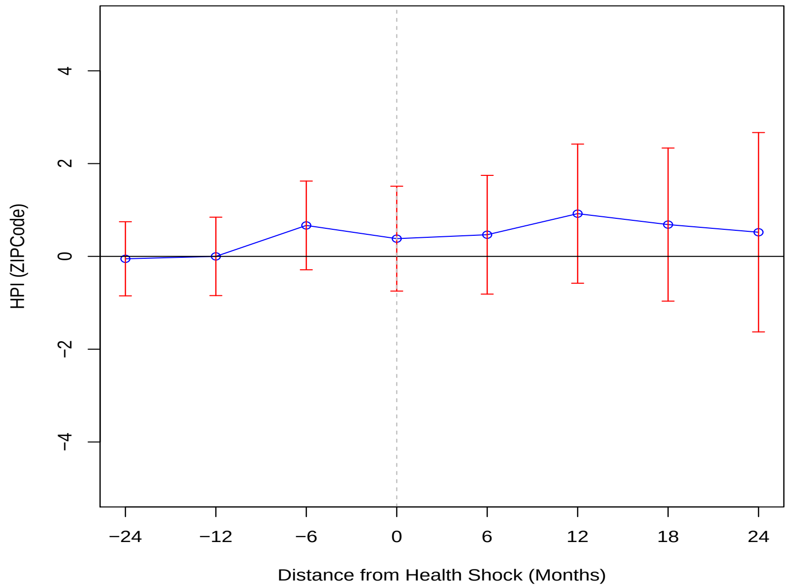
Example : Sales Rep A experiences financial distress (living in zip code Z and employed at firm Y)

First Difference: (Post-Distress) - (Pre-Distress)	Pre-Distress	Post-Distress
Second Difference: Treated-Control	Sales Rep B living in zip code Z and employed at firm Y (Treated)	Flight Attendant D living in zip code Z and employed at firm Y with similar income (Control)

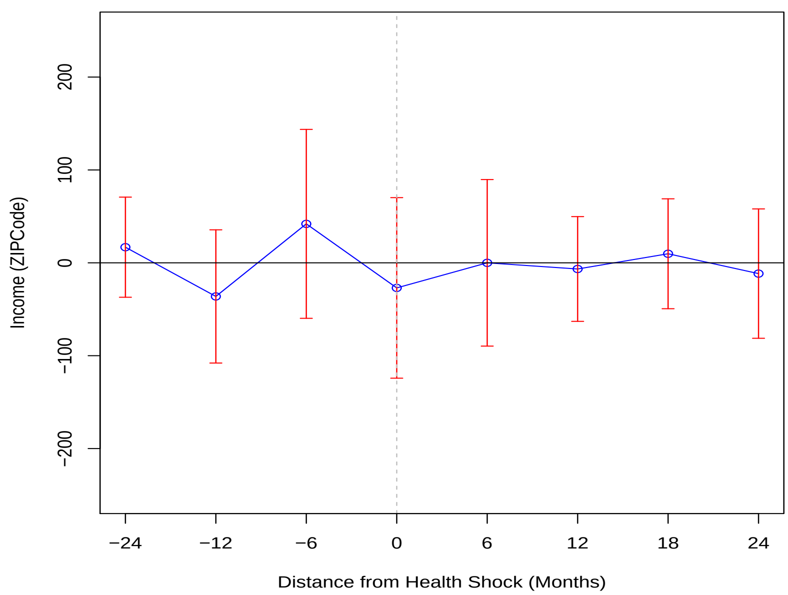
Panel B : **Specification II : Comparing Individuals with Different Job Roles but Living in the Same zip code**

Figure 4: Economic Conditions Across Treated and Control zip Codes

This figure plots estimates for the dynamic difference-in-differences regressions that compare economic conditions across treated and control zip codes (i.e. zip codes where treated and control individuals reside), where house price index (HPI) and median income at the zip code level proxy for economic conditions. These regressions are estimated at the bi-annual frequency. The vertical bars represent confidence intervals at the 5% level while standard errors are clustered at the zip code level.



Panel A : House Price Index (HPI)



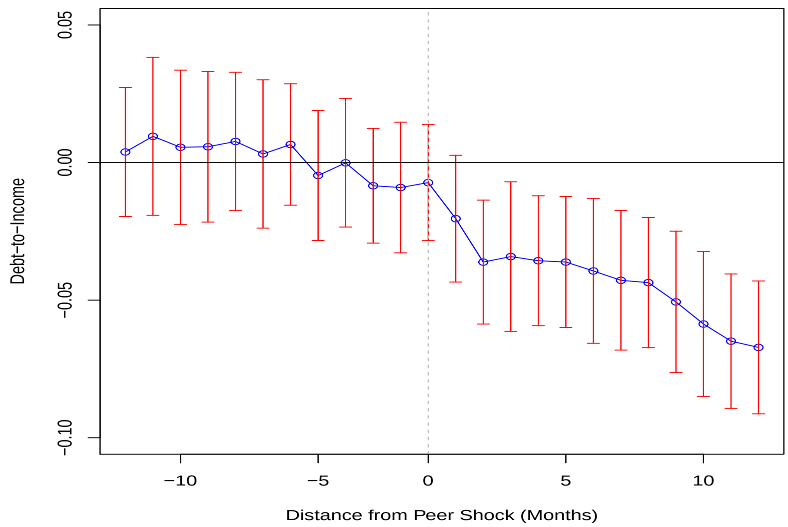
Panel B : Income

Figure 5: Peer Financial Distress and Individual Leverage

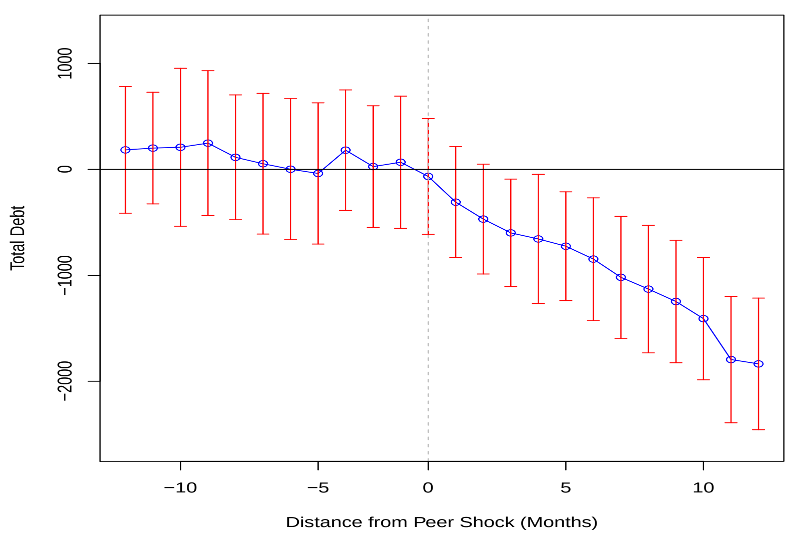
This figure plots the estimates for the dynamic difference-in-differences regressions of the following form that estimate the effect of peer distress on individual leverage:

$$y_{i,t} = \sum_{\substack{k=-13 \\ k \neq -3}}^{-1} \beta_k Pre-PeerShock(-k) + \sum_{k=0}^{13} \beta_k PeerShock(k) + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $Pre-PeerShock(-k)$ ($PeerShock(k)$) is an indicator variable that takes a value of one for individual i, k months before (after) her peer experiences distress, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $EstablishmentIncome$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents individual leverage and total debt. Standard errors are double clustered at the individual and month level. Vertical bars represent confidence intervals at the 5% level.



Panel A : Debt-to-Income Ratio



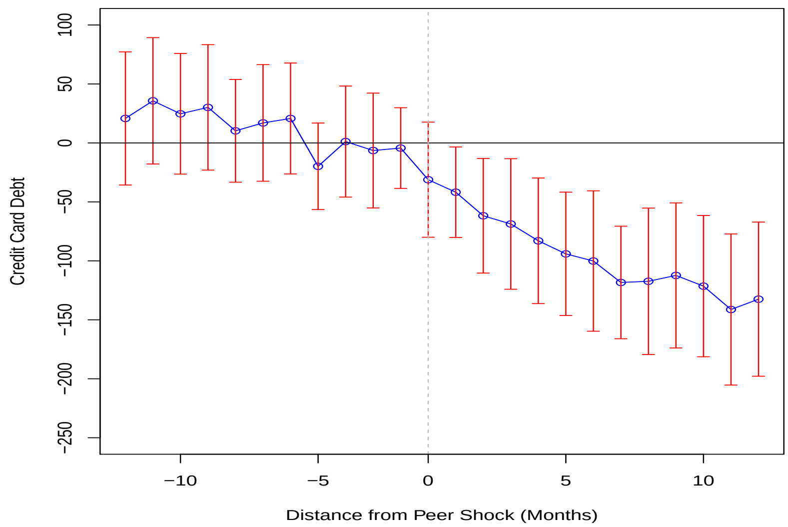
Panel B : Debt

Figure 6: Peer Financial Distress and Components of Debt

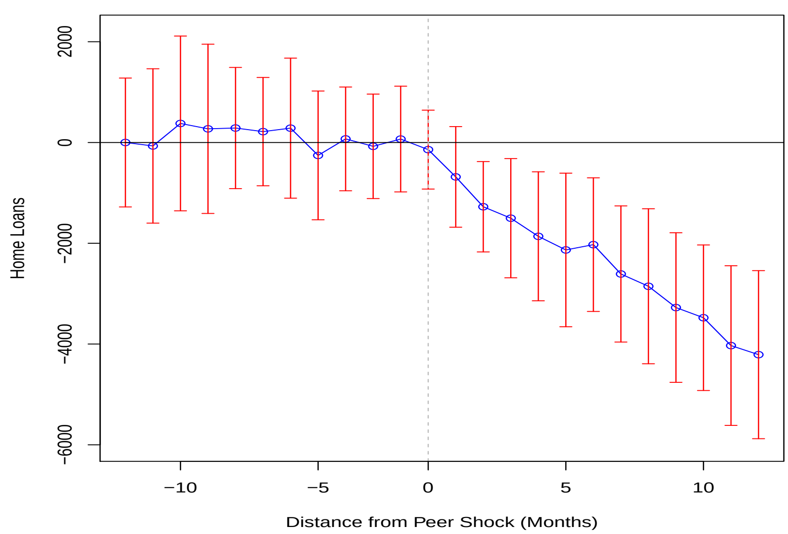
This figure plots the estimates for the dynamic difference-in-differences regressions of the following form that estimate the effect of peer distress on individual debt:

$$y_{i,t} = \sum_{\substack{k=-13 \\ k\neq -3}}^{-1} \beta_k Pre-PeerShock(-k) + \sum_{k=0}^{13} \beta_k PeerShock(k) + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $Pre-PeerShock(-k)$ ($PeerShock(k)$) is an indicator variable that takes a value of one for individual i, k months before (after) her peer experiences distress, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents different components of debt. Standard errors are double clustered at the individual and month level. Vertical bars represent confidence intervals at the 5% level.



Panel A : Credit Card



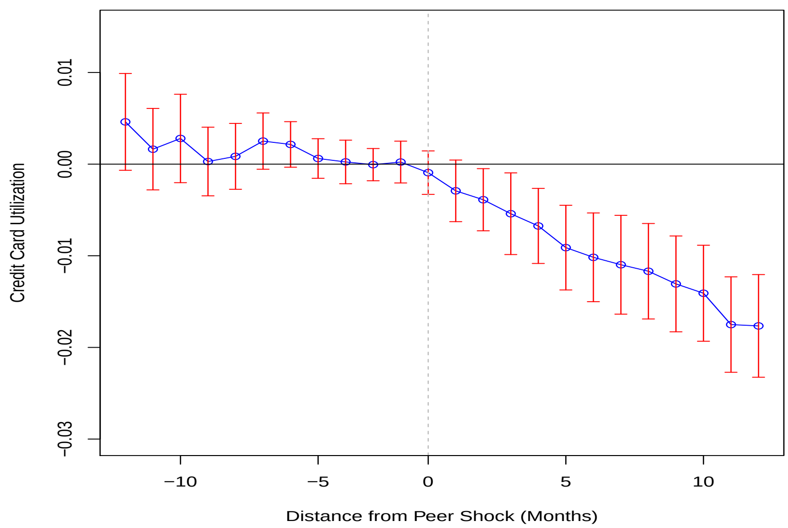
Panel B : Home Loans

Figure 7: How Do Individuals Reduce Debt?

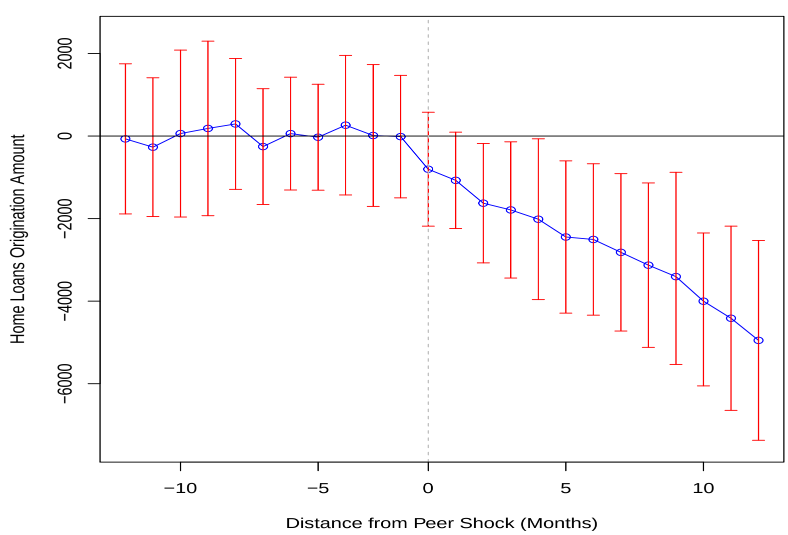
This figure plots the estimates for the dynamic difference-in-differences regressions of the following form that estimate the effect of peer distress on individual level debt characteristics:

$$y_{i,t} = \sum_{\substack{k=-13 \\ k\neq-3}}^{-1} \beta_k Pre - PeerShock(-k) + \sum_{k=0}^{13} \beta_k PeerShock(k) + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $Pre-PeerShock(-k)$ ($PeerShock(k)$) is an indicator variable that takes a value of one for individual i, k months before (after) her peer experiences distress, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents different characteristics for various components of debt. Standard errors are double clustered at the individual and month level. Vertical bars represent confidence intervals at the 5% level.



Panel A : Credit Card



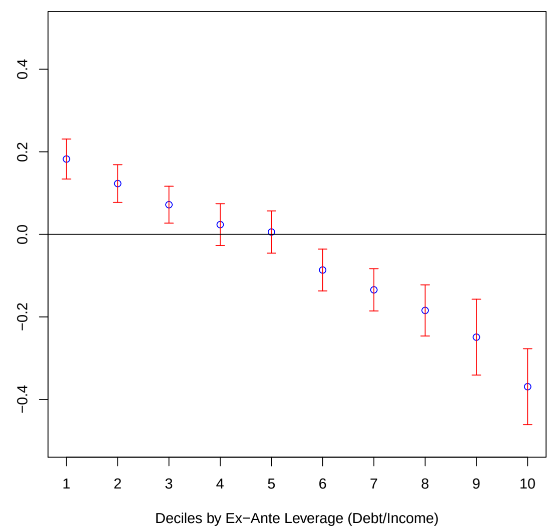
Panel B : Home Loans

Figure 8: Heterogeneity by Ex-Ante Leverage

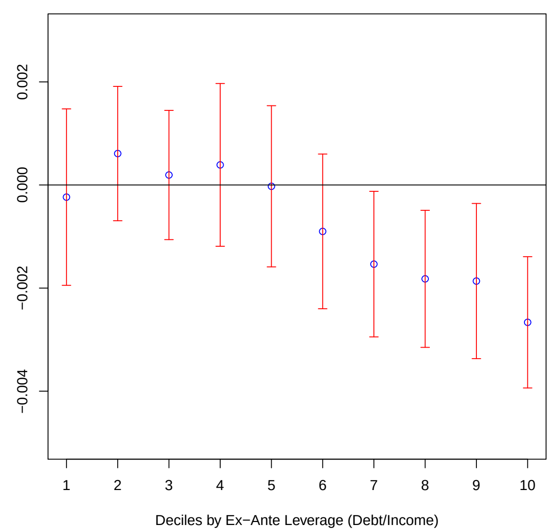
This figure plots the estimates for the triple difference regressions of the following form that interact the difference-in-differences term with dummy variables representing different deciles of ex-ante leverage:

$$y_{i,t} = \sum_{k=1}^{10} \beta PeerShock_i \times Post_t \times Decile_k + \delta_i + \delta_t + \delta_k + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial distress owing to health shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $Decile_k$ is a dummy variable that takes a value of one if individual i belongs to the k^{th} decile in terms of ex-ante leverage; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; δ_k are decile fixed effects that ensure that the comparison in trends occurs between treated and control individuals with similar ex-ante leverage; and $y_{i,t}$ represents individual leverage and delinquency rate. Standard errors are double clustered at the individual and month level. Vertical bars represent confidence intervals at the 5% level.



Panel A : Debt-to-Income Ratio



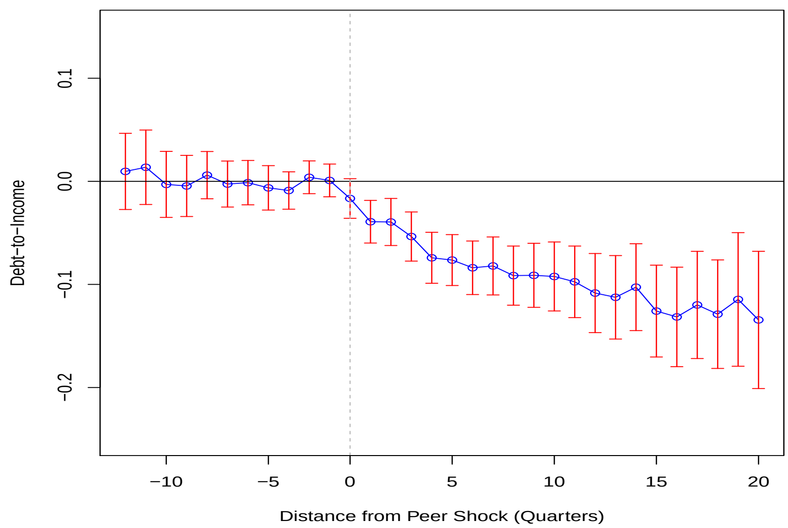
Panel B : Delinquency Rate

Figure 9: Long Term Dynamics

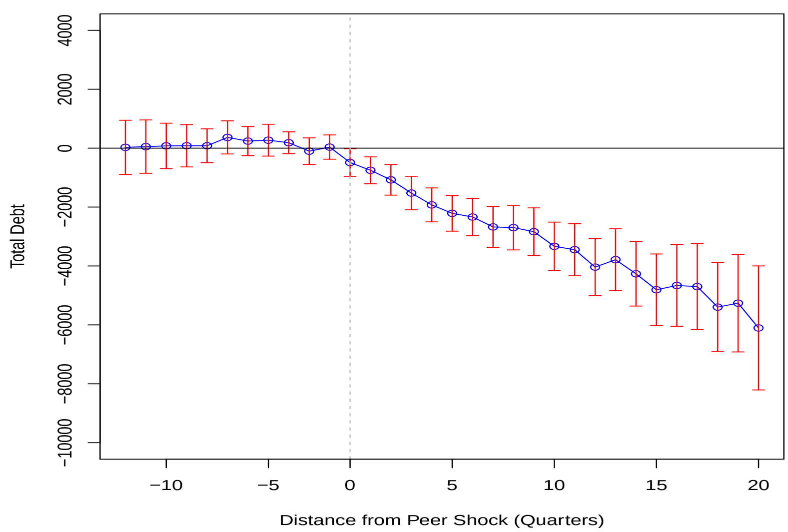
This figure plots the estimates for the dynamic difference-in-differences regressions of the following form that estimate the effect of peer distress on individual leverage:

$$y_{i,t} = \sum_{\substack{k=-13 \\ k\neq-3}}^{-1} \beta_k Pre-PeerShock(-k) + \sum_{k=0}^{20} \beta_k PeerShock(k) + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $Pre-PeerShock(-k)$ ($PeerShock(k)$) is an indicator variable that takes a value of one for individual i, k quarters before (after) her peer experiences distress, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents individual leverage and total debt. Standard errors are double clustered at the individual and year-quarter level. Vertical bars represent confidence intervals at the 5% level.



Panel A : Debt-to-Income Ratio



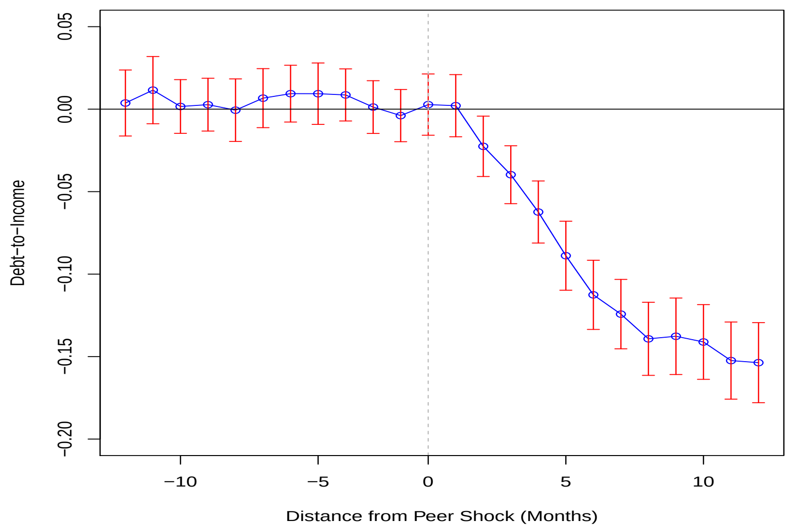
Panel B : Debt

Figure 10: Peer Financial Shock (Specification II)

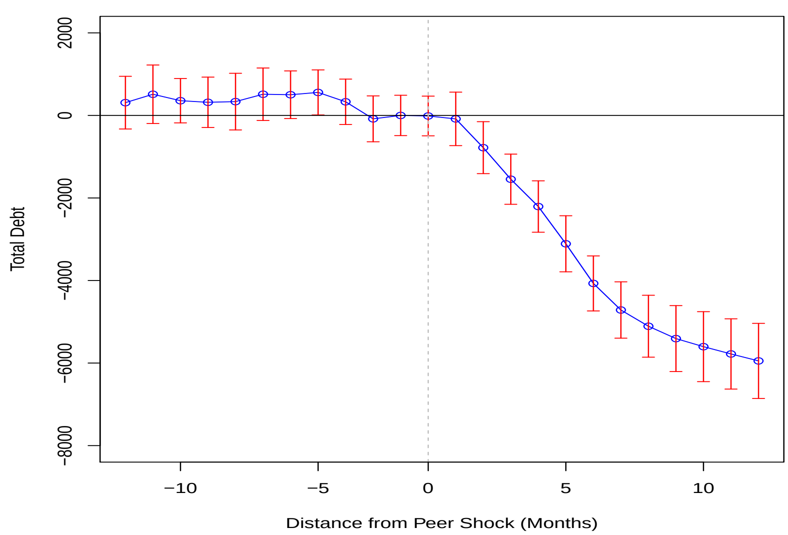
This figure plots the estimates for the dynamic difference-in-differences regressions of the following form that estimate the effect of peer distress on individual leverage and debt:

$$y_{i,t} = \sum_{\substack{k=-13\\k\neq-3}}^{-1} \beta_k Pre-PeerShock(-k) + \sum_{k=0}^{13} \beta_k PeerShock(k) + \delta_i + \delta_{fzt} + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $Pre-PeerShock(-k)$ ($PeerShock(k)$) is an indicator variable that takes a value of one for individual i, k months before (after) her peer experiences distress (not associated with health shock), where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_{fzt} are firm×zip code×month fixed effects; and $y_{i,t}$ represents individual leverage and total debt. Standard errors are double clustered at the individual and month level. Vertical bars represent confidence intervals at the 5% level.



Panel A : Debt-to-Income Ratio



Panel B : Debt

Table 1: Summary Statistics

This table reports the sample statistics of the variables used in this analysis. Each variable is reported for observations that have non-missing values.

	N	Mean	SD	Median	Min	Max
Leverage						
$\frac{Debt}{Income}$	6,331,210	1.96	2.19	0.69	0	9.46
Debt Balances						
Debt	11,317,643	60,493.01	84,293.23	18,009	0	478,218
Credit Card	9,352,962	3,895.75	5,575.74	1,214	0	32,435
Auto Loans	4,731,566	11,215.12	9,782.57	9,070	0	45,509
Home Loans	3,462,370	128,402.91	116,397.41	102,996.5	0	438,224
Credit Card Characteristics						
Openings	11,317,643	0.01	0.08	0	0	1
Payments	5,139,161	578.46	1,067.87	260	1	6,762
Utilization	9,352,962	0.42	0.45	0.22	0	1.16
Spending	5,041,076	697.36	1435.19	295	-7,913	8,781
Auto Loan Characteristics						
Openings	11,317,643	0.001	0.03	0	0	1
Payments	4,664,189	369.83	666.97	263	0	3,072
Loan Origination Amounts	4,731,566	22,550.21	16,108.26	18,180	0	84,113
Home Loan Characteristics						
Openings	11,317,643	0.0001	0.01	0	0	1
Payments	3,450,095	1715.43	3232.75	780	0	15,872
Loan Origination Amounts	3,462,370	263,171.89	224,658.75	178,703	0	1,263,250
Savings & Employment						
Savings	1,762,754	9,757.53	16,135.38	3,063	0	118,874
Employment	11,317,643	0.45	0.5	0	0	1
Loan Performance						
Delinquency	11,317,643	0.01	0.07	0	0	1
Credit Score	11,317,643	638.54	123.64	645	1	839
Income & Age						
Income (Monthly $)	6,739,368	3,431.67	3,644.78	2,276.52	833.33	30,685.08
Age	11,317,643	40.78	17.07	38	18	99

Table 2: Summary Comparison - Treated vs Control Individuals

This table reports descriptive statistics that compare treated and control individuals. The sample consists of 46,590 treated individuals, and 139,811 control individuals employed at the same firm with the same job role (job title) as the treated individuals (but located in a different location), and residing in a neighboring state. All variables are calculated for the third month before treatment, i.e. base month in this analysis. The last column reports the difference in means between treated and control groups. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Treated		Control		Treated-Control
	Mean	Median	Mean	Median	(Mean)
$\frac{Debt}{Income}$	1.95	0.67	1.98	0.69	-0.03
Debt	61,890.36	18,535.5	62,498.51	18,373	-608.15
Credit Card	3,814.55	1,181	3,893.2	1,240	-78.65
Auto Loans	10,659.02	8,970	10,734.03	9,010	-75.01
Home Loans	12,7490.4	10,3172	129,136.02	104,842	-1,645.62
Credit Card Utilization	0.436	0.24	0.43	0.21	0.006
Income (Monthly $)	3,845.19	2,292.12	3,895.71	2,439.56	-50.52
Age	40.57	38	40.98	38	-0.41***

Table 3: Peer Financial Distress and Labor Market Outcomes

This table reports estimates for the difference-in-differences regressions of the following form that estimate the relation between peer financial distress and labor market outcomes:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age (the cubic term in income is not included in Column (3)); δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents income and likelihood of being employed at the same establishment with the same job role. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Income	Employment	Income	Employment
	(1)	(2)	(3)	(4)
$PeerShock \times Post$	83.226	0.009	91.39	0.012
	(55.33)	(0.01)	(67.33)	(0.01)
Controls	No	No	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	6,739,368	11,317,643	6,739,368	6,739,368
R2	0.335	0.648	0.412	0.674

Table 4: Peer Financial Distress and Individual Leverage

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on individual leverage and debt:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial distress owing to health shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents individual leverage and total debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$ (1)	Debt (2)	$\frac{Debt}{Income}$ (3)	Debt (4)
$PeerShock \times Post$	-0.061***	-2,338.30***	-0.083***	-2,960.99***
	(0.012)	(278.991)	(0.011)	(308.925)
Controls	No	No	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	6,328,418	11,317,643	6,328,418	6,328,418
R2	0.686	0.833	0.721	0.839

Table 5: Peer Financial Distress and Components of Debt

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on different components of debt:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents different components of individual debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Credit Card	Auto Loans	Home Loans	Credit Card	Auto Loans	Home Loans
	(1)	(2)	(3)	(4)	(5)	(6)
$PeerShock \times Post$	-210.166***	-194.556***	-5,113.108***	-212.919***	-182.549***	-5,953.585***
	(30.143)	(67.654)	(660.491)	(32.49)	(76.182)	(686.977)
Controls	No	No	No	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,352,950	4,731,560	3,462,361	5,709,753	3,006,487	2,280,321
R2	0.738	0.606	0.82	0.748	0.614	0.827

Table 6: How Do Individuals Reduce Debt?

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on individual level debt characteristics:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents different characteristics for various components of debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

Panel A : Credit Card

	Openings	Spending	Utilization	Payment	Openings	Spending	Utilization	Payment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$PeerShock \times Post$	-0.0001	-62.421***	-0.019***	-9.009	-0.0001	-50.914***	-0.016***	-10.432
	(0.0001)	(5.237)	(0.003)	(7.209)	(0.0001)	(6.434)	(0.003)	(7.56)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,317,643	5,139,155	9,352,950	5,139,155	6,328,418	3,204,803	5,709,753	3,204,803
R2	0.025	0.528	0.694	0.571	0.035	0.547	0.702	0.579

Panel B : Auto Loans

	Openings	Origination Amt	Origination Amt	Payment	Openings	Origination Amt	Origination Amt	Payment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$PeerShock \times Post$	-0.00001	-414.54***	-367.65***	-2.921	-0.00005	-398.54***	-358.08***	-10.002
	(0.00004)	(148.836)	(105.71)	(8.146)	(0.0001)	(149.59)	(111.159)	(8.474)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	No	Yes	No	No
Observations	11,317,643	4,731,560	4,731,560	4,664,183	6,328,418	2,998,990	2,998,990	2,968,954
R2	0.021	0.084	0.775	0.695	0.03	0.105	0.781	0.711

Panel C : Home Loans

	Openings	Origination Amt	Origination Amt	Payment	Openings	Origination Amt	Origination Amt	Payment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PeerShock × *Post*	0.00002*	-7,034***	-5,746***	-94.123**	0.00001	-6,794***	-5,802***	-86.243*
	(0.00001)	(2,651.67)	(1,186.09)	(36.689)	(0.00001)	(2,737.49)	(1,209.85)	(50.79)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	No	Yes	No	No
Observations	11,317,643	2,452,908	3,463,416	3,450,086	6,328,418	1,785,411	2,281,109	2,270,083
R2	0.016	0.154	0.898	0.677	0.023	0.181	0.902	0.682

Table 7: Borrowing vs Consumption

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on savings:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents savings. The savings data comes from the wealth dataset that is available (as median values) at the 9-digit zip code level segmented by age buckets. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Savings	Savings
	(1)	(2)
$PeerShock \times Post$	2,415.899***	2,824.895***
	(462.455)	(520.04)
Controls	No	Yes
Individual FE	Yes	Yes
Month FE	Yes	Yes
Observations	1,762,754	1,050,185
R2	0.523	0.595

Table 8: Peer Financial Distress, Loan Performance & Credit Score

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on delinquency rate and credit score:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents delinquency rate and credit score. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Delinquency (1)	Credit Score (2)	Delinquency (3)	Credit Score (4)
$PeerShock \times Post$	-0.001***	13.94***	-0.001***	11.77**
	(0.002)	(5.31)	(0.002)	(5.73)
Controls	No	No	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	11,317,643	11,317,643	6,328,418	6,328,418
R2	0.735	0.851	0.747	0.867

Table 9: Heterogeneity by Wage Garnishment Laws & Ex-Ante Leverage

This table reports estimates for the triple difference regressions where the difference-in-differences variable is interacted with different dummy variables that capture strictness in wage garnishment laws and level of ex-ante leverage. Each observation corresponds to an individual-month combination. *PeerShock* is an indicator variable that takes a value of one for individuals whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); *Post* is a dummy variable that takes a value of one during the months following distress; *NoRestriction*, *MediumRestriction* and *SevereRestriction* are indicator variables that take a value of one for individuals living in states that impose no restrictions, medium restrictions and severe restrictions on wage garnishment respectively; control variables include $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; *Above* (*Below*) is an indicator variable that takes a value of one for the individual with an above (below) median level of ex-ante leverage; and the outcome variables include individual leverage and debt. All specifications include individual and month fixed effects. Panel A includes state fixed effects to ensure that the comparison in trends occurs between treated and control individuals exposed to similar wage garnishment laws. Panels B and C include median fixed effects (indicating whether an individual is above or below the median level of ex-ante leverage) to ensure that the comparison in trends occurs between treated and control individuals with similar ex-ante leverage. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$	Debt
	(1)	(2)
$PeerShock \times Post \times NoRestriction$	-0.105***	-3,417.22***
	(0.015)	(424.81)
$PeerShock \times Post \times MediumRestriction$	-0.073***	-2,074.27***
	(0.021)	(513.66)
$PeerShock \times Post \times SevereRestriction$	-0.025***	-717.51**
	(0.007)	(252.62)
NoRestriction-SevereRestriction	-0.08***	-2699.71***
Controls	Yes	Yes
Individual FE	Yes	Yes
Month FE	Yes	Yes
State FE	Yes	Yes
Observations	6,328,418	6,328,418
R2	0.723	0.84

Panel A: Wage Garnishment Laws

Table 9 (contd)

	$\frac{Debt}{Income}$ (1)	Debt (2)
$PeerShock \times Post \times Above$	-0.240***	-7,145.05***
	(0.025)	(613.40)
$PeerShock \times Post \times Below$	0.071***	1,167.53**
	(0.011)	(576.95)
Above-Below	-0.311***	-8,312.58***
Controls	Yes	Yes
Individual FE	Yes	Yes
Month FE	Yes	Yes
Median FE	Yes	Yes
Observations	6,328,418	6,328,418
R2	0.723	0.84

Panel B: Ex-Ante Leverage

Table 9 (contd)

	$\frac{Debt}{Income}$ (1)	$\frac{Debt}{Income}$ (2)	$\frac{Debt}{Income}$ (3)	Debt (4)	Debt (5)	Debt (6)
$PeerShock \times Post \times Above$	-0.279***	-0.241***	-0.187***	-8,900.375***	-7,128.055***	-5,463.015***
	(0.033)	(0.046)	(0.036)	(874.95)	(1,122.00)	(928.97)
$PeerShock \times Post \times Below$	0.048	0.079***	0.123***	1071.11*	2270.551**	3680.551***
	(0.031)	(0.021)	(0.020)	(681.38)	(546.99)	(538.22)
Sample	No Restriction	Medium Restriction	Severe Restriction	No Restriction	Medium Restriction	Severe Restriction
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Median FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,918,978	2,275,950	1,086,505	2,918,736	2,274,663	1,085,683
R2	0.724	0.733	0.738	0.841	0.851	0.852

Panel C: Wage Garnishment Laws & Ex-Ante Leverage

Table 10: Heterogeneity by Recession vs Non-Recession

This table reports estimates for the triple difference regressions of the following form that estimate the heterogeneous effect of peer financial distress on individual leverage and debt based on individual exposures to recession and non-recession periods during their formative years:

$$y_{i,t} = \beta_1 PeerShock_i \times Post_t \times Recession + \beta_2 PeerShock_i \times Post_t \times Non-Recession + \delta_i + \delta_t + \delta_{Exp} + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $Recession$ ($Non-Recession$) is an indicator variable that takes a value of one for individuals who experienced (did not experience) a recession during 18 to 23 years of age; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; δ_{Exp} are experience fixed effects that ensure that comparison in trends occurs between treated and control individuals with similar formative experiences; and $y_{i,t}$ represents individual leverage and debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$ (1)	Debt (2)
$PeerShock \times Post \times Recession$	-0.067***	-2,072.232***
	(0.011)	(302.16)
$PeerShock \times Post \times Non-Recession$	-0.148***	-5,772.964***
	(0.027)	(812.01)
Recession-(Non-Recession)	0.081***	3700.73***
Controls	Yes	Yes
Individual FE	Yes	Yes
Month FE	Yes	Yes
Experience FE	Yes	Yes
Observations	6,328,418	6,328,418
R2	0.723	0.84

Table 11: Heterogeneity by Peer vs Non-Peer

This table reports estimates for the triple difference regressions of the following form that estimate the heterogeneous effect of peer financial distress on individual leverage and debt based on whether they continue to be peers or not following distress:

$$y_{i,t} = \beta_1 PeerShock_i \times Post_t \times Peer + \beta_2 PeerShock_i \times Post_t \times Non-Peer + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $Peer$ ($Non - Peer$) is an indicator variable that takes a value of one for treated individuals if their peers who experienced distress (don't) continue to be peers after three months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents individual leverage and debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$ (1)	Debt (2)
$PeerShock \times Post \times Peer$	-0.084***	-2,641.74***
	(0.011)	(319.03)
$PeerShock \times Post \times Non - Peer$	-0.077**	-2,893.35***
	(0.027)	(660.64)
Peer-(Non-Peer)	-0.007	251.61
Controls	Yes	Yes
Individual FE	Yes	Yes
Month FE	Yes	Yes
Observations	6,328,418	6,328,418
R2	0.722	0.84

Table 12: Are Results Specific to Health Shocks? Within Firm-zip Code Variation (Specification II)

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on individual leverage and debt by exploiting within firm-zip code variation:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t(\delta_{fzt}) + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks not induced by health shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; δ_{fzt} are firm×zip code×month fixed effects and $y_{i,t}$ represents individual leverage and debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$ (1)	Debt (2)	$\frac{Debt}{Income}$ (3)	Debt (4)
$PeerShock \times Post$	-0.097***	-4,359.351***	-0.114***	-4,641.916***
	(0.009)	(313.67)	(0.009)	(333.25)
Controls	No	No	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	No	No
Firm×zip Code×Month FE	No	No	Yes	Yes
Observations	14,480,080	14,480,080	8,326,289	8,326,289
R2	0.815	0.833	0.839	0.839

Table 13: Robustness: Within Firm-zip Code Variation (Specification II)

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on individual leverage and debt by exploiting within firm-zip code variation:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t(\delta_{fzt}) + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; δ_{fzt} are firm×zip code×month fixed effects and $y_{i,t}$ represents individual leverage and debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$	Debt	$\frac{Debt}{Income}$	Debt
	(1)	(2)	(3)	(4)
$PeerShock \times Post$	-0.046***	-1,753.73***	-0.061***	-2,220.74***
	(0.010)	(249.66)	(0.013)	(332.79)
Controls	No	No	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	No	No
Firm×zip Code×Month FE	No	No	Yes	Yes
Observations	5,400,796	5,400,796	5,400,796	5,400,796
R2	0.741	0.821	0.816	0.864

Table 14: Heterogeneity by Number of Peers

This table reports estimates for the triple difference regressions of the following form that estimate the heterogeneous effect of peer financial distress on individual leverage and debt based on the number of peers:

$$y_{i,t} = \beta_1 PeerShock_i \times Post_t \times Above + \beta_2 PeerShock_i \times Post_t \times Below + \delta_i + \delta_t + \delta_M + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $Above$ ($Below$) is an indicator variable that takes a value of one for treated individuals with the above (below) median number of peers; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; δ_M are median fixed effects (indicating whether an individual has above or below median number of peers) to ensure that the comparison in trends occurs between treated and control individuals with similar number of peers; and $y_{i,t}$ represents individual leverage and debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$ (1)	Debt (2)
$PeerShock \times Post \times Above$	-0.052***	-2,519.57***
	(0.011)	(327.12)
$PeerShock \times Post \times Below$	-0.11***	-3,677.82***
	(0.019)	(485.37)
Above-Below	0.058***	1158.25**
Controls	Yes	Yes
Individual FE	Yes	Yes
Month FE	Yes	Yes
Median FE	Yes	Yes
Observations	6,328,418	6,328,418
R2	0.723	0.84

IA Internet Appendix

Table IA1: Heterogeneity by Bankruptcy vs Non-Bankruptcy

This table reports estimates for the triple difference regressions of the following form that estimate the heterogeneous effect of peer financial distress on individual leverage and debt based on whether or not peers' financial distress leads to bankruptcy:

$$y_{i,t} = \beta_1 PeerShock_i \times Post_t \times Bankruptcy + \beta_2 PeerShock_i \times Post_t \times Non-Bankruptcy + \delta_i + \delta_t + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $Bankruptcy$ ($Non-Bankruptcy$) is an indicator variable that takes a value of one for treated individuals whose peers (do not) file for bankruptcy following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; and $y_{i,t}$ represents individual leverage and debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$ (1)	Debt (2)
$PeerShock \times Post \times Bankruptcy$	-0.205*	-6,470.762***
	(0.12)	(2,216.87)
$PeerShock \times Post \times Non-Bankruptcy$	-0.085***	-2,624.148***
	(0.011)	(313.53)
Above-Below	-0.12	-3846.61*
Controls	Yes	Yes
Individual FE	Yes	Yes
Month FE	Yes	Yes
Observations	6,328,418	6,328,418
R2	0.723	0.84

Table IA2: Peer Financial Distress and Components of Debt: Within Firm-zip Code Variation

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on different components of debt by exploiting within firm-zip code variation:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t(\delta_{fzt}) + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks not induced by health shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; δ_{fzt} are firm$\times$zip code$\times$month fixed effects and $y_{i,t}$ represents different components of debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Credit Card (1)	Auto (2)	Home Loans (3)	Credit Card (4)	Auto (5)	Home Loans (6)
$PeerShock \times Post$	-557.995***	-511.219***	-7,126.992***	-514.515***	-500.255***	-5,959.961***
	(32.81)	(62.46)	(607.42)	(32.88)	(69.99)	(578.36)
Controls	No	No	No	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,752,588	7,521,203	6,085,846	10,466,443	6,728,367	5,562,597
R2	0.724	0.591	0.815	0.756	0.65	0.85

Table IA3: How Do Individuals Reduce Debt? Within Firm-zip Code Variation

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer financial distress on individual level debt characteristics by exploiting within firm-zip code variation:

$$y_{i,t} = \beta PeerShock_i \times Post_t + \delta_i + \delta_t(\delta_{fzt}) + \gamma X_{i,t-1} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks not induced by health shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; δ_{fzt} are firm×zip code×month fixed effects and $y_{i,t}$ represents different characteristics for various components of debt. The regressions are confined to observations with non-missing values. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

Panel A : Credit Card

	Payment (1)	Openings (2)	Spending (3)	Utilization (4)	Payment (5)	Openings (6)	Spending (7)	Utilization (8)
$PeerShock \times Post$	-10.573***	-0.00001	-72.37***	-0.017***	-10.053***	-0.00003	-70.64***	-0.015***
	(3.60)	(0.0001)	(9.89)	(0.002)	(3.76)	(0.0001	(10.21)	(0.002)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Month FE	Yes	Yes	No	Yes	No	No	No	No
Firm×zip Code× FE	No	No	Yes	No	Yes	Yes	Yes	Yes
Observations	7,193,379	15,503,493	6,827,054	11,141,158	6,477,178	13,628,182	2,510,949	9,935,986
R2	0.562	0.023	0.528	0.687	0.615	0.088	0.547	0.728

Panel B : Auto Loans

	Payment (1)	Openings (2)	Origination Amt (3)	Origination Amt (4)	Payment (5)	Openings (6)	Origination Amt (7)	Origination Amt (8)
$PeerShock \times Post$	2.582	-0.0001***	-753.82***	-678.32***	2.267	-0.0001***	-679.55***	-609.55***
	(3.56)	(0.00004	(102.33)	(78.28)	(3.75)	(0.00004)	(98.37)	(87.11)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Month FE	Yes	Yes	No	Yes	No	No	No	No
Firm×zip Code× FE	No	No	Yes	No	Yes	Yes	Yes	Yes
Observations	7,345,762	15,503,493	8,126,361	7,426,125	6,578,966	13,628,182	7,394,224	6,643,657
R2	0.677	0.019	0.736	0.736	0.728	0.094	0.776	0.776

Panel C : Home Loans

	Payment	Openings	Origination Amt	Origination Amt	Payment	Openings	Origination Amt	Origination Amt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$PeerShock \times Post$	-97.467	-0.00001	-19,049***	-8,413***	-100.882	-0.00002	-14,824***	-6,700***
	(104.52)	(0.00001)	(4,651.67)	(886.67)	(122.445)	(0.00001)	(3,638.79)	(863.26)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Month FE	Yes	Yes	No	Yes	No	No	No	No
Firm×zip Code×Month FE	No	No	Yes	No	Yes	Yes	Yes	Yes
Observations	3,450,086	11,317,631	2,452,908	3,463,416	2,270,083	6,737,064	1,785,411	2,281,109
R2	0.677	0.016	0.154	0.898	0.682	0.023	0.181	0.902

Table IA4: Heterogeneity by Income & Access to Liquidity

This table reports estimates for the triple difference regressions of the following form that estimate the heterogeneous effect of peer financial distress on individual leverage and debt based on income and access-to-credit:

$$y_{i,t} = \beta_1 PeerShock_i \times Post_t \times Above + \beta_2 PeerShock_i \times Post_t \times Below + \delta_i + \delta_t + \delta_M + \gamma X_{i,t} + \epsilon_{i,t}$$

where each observation corresponds to an individual-month combination for individual i during month t; $PeerShock_i$ is an indicator variable that takes a value of one for individual i whose peers experience financial shocks, where peers are defined as individuals living in the same zip code and employed at the same firm with the same job role (job title); $Post_t$ is a dummy variable that takes a value of one during the months following distress; $Above$ ($Below$) is an indicator variable that takes a value of one for treated individuals if their income or credit score is greater (smaller) than the median income or credit score during the third month prior to treatment; $X_{i,t}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; δ_i are individual fixed effects; δ_t are month fixed effects; δ_M are median fixed effects (indicating whether an individual has above or below median level of income and credit score) to ensure that the comparison in trends occurs between treated and control individuals with similar levels of income and credit score; and $y_{i,t}$ represents individual leverage and debt. Standard errors are double clustered at the individual and month level, and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	$\frac{Debt}{Income}$	Debt	$\frac{Debt}{Income}$	Debt
	(1)	(2)	(3)	(4)
$PeerShock \times Post \times Above$	-0.045***	-2,272.462***	0.014	422.59
	(0.017)	(586.07)	(0.017)	(458.74)
$PeerShock \times Post \times Below$	-0.115***	-3,916.512***	-0.151***	-4,716.738***
	(0.017)	(385.68)	(0.013)	(377.96)
Above-Below	0.07***	1644.05**	0.165***	5139.33***
Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Median FE	Yes	Yes	Yes	Yes
Cross-Sectional Variable	Income	Income	Credit Score	Credit Score
Observations	4,320,481	4,309,890	5,901,648	6,247,508
R2	0.733	0.836	0.722	0.048