Peer Financial Distress and Individual Leverage

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Abstract

Using health shocks to identify financial distress situations, I document that peer distress leads to a decline in individual leverage and debt on average. Individual leverage declines by 5.7% and remains deflated for at least five years following peer distress. This decline occurs as individuals borrow less on the intensive margin, pay higher fractions of their debt and save more while their income remains unchanged following peer distress. As a result, individuals are less likely to default during the period following peer distress. The heterogeneity in responses highlight the role of changes in beliefs and preferences as the underlying mechanism.

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Introduction

Fluctuations in household leverage play an important role in driving and exacerbating business cycles (Mian, Sufi and Verner (2017)). Understanding the determinants of leverage choices is therefore a question of central economic importance.[1] In this paper, I explore the role of individuals' preferences and beliefs about financial distress costs as one potential determinant of their leverage decisions.

To accomplish this, I use peer experiences of financial distress as a shock to individual's preferences and assessment of expected distress cost, and examine how peer distress affects individual leverage and borrowing behavior.[2] It is difficult for an individual to make an accurate assessment of the cost of financial distress, especially because of the complexities of the financial laws in the U.S. that vary across states and over time.[3] On observing peer experiences of distress, she may learn new information and update her beliefs on the expected costs of financial distress. Alternatively her expectations may change owing to salience of the distress event. If she increases (decreases) her assessment of the expected cost of distress, then that will decrease (increase) her demand for leverage. An individual's preferences may also change on observing peer distress events and may lead to changes in her borrowing behavior.

Understanding how peer distress experience influences individual leverage is not only important in informing us about how preferences and beliefs affect borrowing decisions but such 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.

The empirical analysis in the paper leverages a detailed dataset on individual credit profiles and employment history that comes from Equifax Inc. The credit data includes information on the credit histories of all individuals in the U.S., including historical information on all their credit accounts,

[1]Household leverage also affects macro-economic outcomes such as employment (Mian and Sufi 2014), and other household decisions such as consumption and investment (Mian, Rao and Sufi 2013), 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]Expected distress cost is a function of both the likelihood of experiencing distress and cost conditional on being in distress.

[3]For e.g. bankruptcy protection laws, wage garnishment laws, debt discharge laws etc.

credit scores, and zip codes of residence. The employment data covers over 30 million employees 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 distress on individual leverage poses significant challenges. First, identifying the relevant peer group is difficult. Ideally peers should be identified as individuals who are able to observe each others' distress experiences. However, empiricists cannot observe this information.[4] To overcome this limitation, I define peers in a very specific manner in order to maximize the proximity between them. Peers are defined 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. Even with this close definition, it is an assumption that peers either interact about or observe each others' distress experiences. I conduct a battery of tests to show evidence consistent with this assumption.

Second, peers may be subject to certain common shocks (Manski 1993). This concern is exacerbated owing to the specific peer definition. For instance, peers employed at the same firm may be subject to firm-level shocks. I use a matched sample in a difference-in-differences framework to overcome this concern. I study financial distress situations that are triggered by health shocks as events in this setting. Specifically, I identify instances when individuals default on medical bills worth more than $10,000, accrued within a month, and examine how this affects peer individuals (i.e. the treated group).[5] For each treated individual, I identify matches that comprise 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). The advantage of choosing the control individuals from neighboring states is that it helps ensure that they don't interact with individuals experiencing the health shock.[6] The similarity in job profiles between treated and control individuals helps control for firm-level correlated shocks, and helps ensure that

[4]An exception to this limitation would be to survey individuals and ask them who they talk with about financial distress situations.

[5]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 $862.3 for U.S. individuals as reported by the Centers for Medicare and Medicaid Services. However, the results are not sensitive to using this cut-off.

[6]In a robustness test, I choose control individuals residing in the same zip code and employed at the same firm as individuals whose peers experience distress but with a different job role, and find similar results. However, in this setting the control individual may also interact with individuals experiencing the shock and hence may be affected by their shock. For this reason, I don't use this setting as the main specification.

control individuals are very similar to treated individuals and are likely to have similar unobserved characteristics.

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 9.2 percentage points more for treated individuals relative to the control group following peer distress. This effect is economically significant as it corresponds to 5.7% 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 \$136 (5.4%), \$126 (3%), and \$1,768 (4.8%) 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.[7] 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 ex-

[7] 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.

pected 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 between changes in preferences and expectations. Henceforth, I refer to their combination as 'learning.'[8] If the results are driven by learning, 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.

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). To test this prediction, I use ex-ante leverage as a proxy for priors and find results consistent with learning. Individuals in the lowest decile based on ex-ante leverage increase borrowing while those in the top five deciles decrease borrowing following peer distress. Within individuals with low ex-ante leverage, the increase in leverage is confined to those individuals whose peers reside in states with lower cost of defaults. These results are consistent with approximately 10% of individuals over-estimating and 50% of individuals under-estimating the cost of distress ex-ante.

The degree of updating should also depend on the uncertainty in ex-ante priors. I proxy for this uncertainty in priors based on individuals' early life experiences. Individuals who experience a recession during their formative years are likely to have better information about the costs of distress than individuals who did not experience a recession. 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.

Individuals may update their beliefs on either the expected cost of financial distress or expected cost of experiencing a health shock (or both). I perform a number of tests that provide suggestive evidence supporting the cost of financial distress channel. For instance, I examine the heterogeneity in responses for individuals residing in states with different wage garnishment laws and find stronger results for those residing in states with higher costs of defaults. Under the assumption that

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

observed health shocks are similar across states with different wage garnishment laws, these results are consistent with individuals learning about the cost of financial distress. 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.[9] Using this specification, I find that individuals reduce leverage following a peer default on a financial loan.

Finally, I examine the heterogeneity in response for hourly and salaried workers. Hourly wage workers are less likely to be provided with medical insurance by their employers and hence more likely to have worse health insurance. If the results are driven by individuals learning that health shocks are more costly than they expected, then one would expect that individuals with worse health insurance should react more because they will bear higher costs in the event of a health shock. However, I find similar effects of peer distress on individual borrowing behavior across both hourly and salaried workers which suggests that learning about costs of health shock likely doesn't play an important role in driving the results. To the extent that assumptions associated with the these heterogeneity tests are violated, results may be driven by individuals learning about the cost of experiencing health shocks instead of financial distress.

The effect of peer distress on individual leverage 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 \$194.61 billion in total household debt between 2011 and 2015. This decline is economically significant as it corresponds to 1.65% of the total household debt of \$11.75 trillion as of January 2011.

This paper makes two primary contributions. First, I document the role of preferences and beliefs 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 defaults lead to lower delinquency rates among peers. This contrasts with the extant literature that documents a default contagion behavior for foreclosures and strategic defaults.

[9] 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.

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 preferences and beliefs in determining the demand for leverage and debt. The results also show that the effect of peer distress on individual leverage 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. Guiso, Sapienza and Zingales (2013) document that individuals are more likely to strategically default if they are exposed to other people who strategically default. While this paper finds that strategic defaults lead to more strategic defaults when individuals are already in distress owing to underwater mortgages, I find that defaults lead to lower delinquency rates when individuals are not in distress. Hence, in my setting individuals learn about both the likelihood of distress and costs conditional on distress. The studies on foreclosures document their effect 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, Rantapuska and Sarvimaki 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.

This paper also relates to the literature on health expectations and financial decisions. This literature documents the role of mortality beliefs and expectations about medical expenses in determining individual consumption and saving patterns (De Nardi, French and Jones (2010); Heimer, Myrseth and Schoenle (2015); Balasubramaniam (2016)). To the extent that my results are driven by changes in expectations about cost of health shocks, I contribute to this literature by documenting that individuals borrow less and save more when they expect the likelihood or cost of medical shocks to be higher.

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), stock market participation (Hong, Kubik and Stein 2004; Brown, Ivkovic, Smith and Weisbenner 2008; Kaustia and Knupfer 2012), and stock market investment choices (Hvide and Ostberg (2015)). 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 proprietary 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.

For sample construction, I use job title reported by these firms as a measure of job role. Job titles are assigned within different organizations. These titles are not standardized across firms. For instance, one firm can assign a title of 'analyst' to an employee while another may call the employee doing similar work as 'researcher'. But more importantly for the analysis in this paper, these titles remain consistent within the firm and through time. This consistentcy plays a key role because I use this variable to identify peers and control individuals employed within the same firm.

There are two main limitations with the data. First, while the credit profiles part of the data contains information on all individuals who have some form of credit history, it does not include information for a small fraction of the population that has never had any form of credit (for e.g. young individuals with no credit history). Second, the employment data comprises individuals employed at firms that subscribe to income or employment verification services from Equifax Inc which tend to be larger firms. The median firm in the data employs over 5,000 employees which is larger than the median firm in the US. To the extent that individuals working in larger firms have different characteristics than those employed at smaller firms, the conclusions drawn using this data may not extend over to individuals employed in smaller firms. I discuss the representativeness of my specific sample further in Section 2.4 below.

2.2 Sample Construction

I begin by identifying individuals who experienced 'health shocks' during the period between 2011-2014.[10] 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 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. The advantage of choosing the control individuals from neighboring states is that it helps ensure that they don't interact with individuals experiencing the health shock. 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.

[10] 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.

2.3 Sample Statistics

Table 1 reports summary statistics for the variables used in the analysis. The mean debt-to-income ratio in the sample is 1.61 while the mean total debt is $52,615. The mean credit card debt is $2,497 while the mean home loan debt is $36,726.

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 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.[11]

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 635, which is similar to the median credit score for the U.S. population of 638.

The median monthly income in the sample is $2,201, 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. An average individual in the sample is 41 years of age and has 7 peers.

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 124 days younger than the average control individual. To ensure that this difference doesn't bias the analysis, I control for age in various specifications.

[11] 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.

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 geographically 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. 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. To the extent individuals don't react to distress experiences of their peers, the estimates reported in this paper reflect the lower bound of the effect.

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.

I estimate variants of the following model:

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

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 $PeerShock_i \times Post_t$, where $PeerShock_i$ is a dummy variable that takes a value of one for individual i if her peer experiences distress owing to a health shock, and $Post_t$ is a dummy variable that takes a value of one during the months following such distress situations. The specification includes a separate set of individual and time fixed effects for each group of treated and control individuals that are associated with a specific health shock (i.e., the event). Since individuals are allowed to be chosen as matches for multiple events, I include event-by-individual fixed effects ($\delta_{e\times i}$) that ensures that the estimation uses within individual variation in the dependent variable. The event-by-month fixed effects ($\delta_{e\times t}$) ensure that the time trends in dependent variables for the treated individuals are compared to the those of their matched control counterparts who are all associated with a particular health shock. Hence, the identifying variation comes from the comparison between treated individuals and their matched control counterparts at each point in time.

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. 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 distress, for individuals whose peers experienced distress, to changes for their matched control individuals whose peers did not experience distress. 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 matched 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 their matched 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:

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

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 matched control individuals whose peers did not experience distress.

A potential limitation of this specification is that the peer shocks induced by a health shock include the decision to default on the medical bill along with the unexpected health event itself. Whether an individual defaults on unexpected medical bills depends on their ex-ante financial conditions and capacity to absorb such shocks (Morrison, Gupta, Olson, Cook and Keenan (2013); Gupta, Morrison, Fedorenko and Ramsey (2017); Dobkin, Finkelstein, Kluender and Notowidigdo (2018b)). This decision also allows for the possibility that individuals may strategically choose to default on medical bills if they're already in distress. This limitation could bias the results in two ways. First, individuals being observed as experiencing health shocks in the data may have limited assets and savings, and to the extent these variables are correlated among peers, the treated individuals in the sample may also have low levels of assets and savings. This may potentially bias the estimates if the dynamics of leverage is correlated with ex-ante balance sheet variables like assets. Second, common unobservable shocks may have pushed all peers into financial distress but only those experiencing an unexpected health event get identified as experiencing the health shock.

The baseline specification and the choice of the sample help mitigate these concerns in a number of ways. First, while constructing the sample I confine the health shocks to those medical defaults where the individual did not default on any other liability for at least two years prior to medical default. This ensures that individuals experiencing health shocks were not in severe observable distress prior to experiencing the health event. Second, I compare the financial characteristics of the treated and control individuals during the third month before treatment. As discussed earlier, I find that both groups of individuals are observationally similar across many different dimensions. This suggests that any influence that ex-ante characteristics may have on leverage dynamics would likely be netted out in the specification. Third, the similarity in job profiles across treated and control individuals help control for employment related common shocks across peers. Finally, I conduct a number of placebo and robustness tests to ensure that health shocks are not correlated in my setting and results are not driven by common unobservable shocks.

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 shocks. 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.[12] 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)).

Figure 5 compares the trends in labor market conditions as indicated by income. Panel A plots unconditional means for both the treated and control groups for twelve months around peer distress. The blue color represents the treated group while the black color represents the control group. The vertical bars represent confidence intervals at 95% level. The plot suggests that both treated and

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

control groups follow parralel trends across all 24 months around peer distress. Though treated individuals have slightly lower income, the difference is not significant for most of the months.

I verify this by empirically estimating the differential trends between treated and control groups using Equation 2. 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. Panel B plots the results for this estimation which shows that none of the coefficients for 24 months around peer distress are statistically different from zero, thus confirming that the trends in income for treated and control individuals are statistically indistinguishable for at least two years around peer distress. Overall, these results suggest that peers are not subject to common establishment-level shocks that may be correlated with health shocks for some individuals.

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 11.6 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 7.2% relative to the mean debt-to-income ratio in the sample. Column (4) 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 similar result that debt-to-income ratio declines by 9.2 pp (5.7% of the mean) more for treated individuals relative to the control group.

In columns (2) and (4) I examine the effect of peer distress on growth in leverage as measured by log changes in debt-to-income ratio. Using leverage growth as an outcome variable helps control for certain time trends in leverage that may be correlated with personal characteristics not captured in the specification. I find that leverage grows at 2.1pp (column (4)) slower rate for treated individuals relative to their control counterparts. The estimates reported in columns (3) and (6) show a similar effect of peer distress on total debt. The coefficient in column (3) shows that total debt for treated individuals declines by $2,541 more than for control individuals whose peers did not experience distress. As before, this effect is economically significant as it corresponds to 4.8% relative to the mean level of debt. Column (6) finds a similar result for the specification that includes all control variables.[13]

These results can potentially be surprising, especially given that the individuals experiencing health shocks in the data are employed workers and hence are likely to be covered by medical insurance. However, as documented in Dobkin, Finkelstein, Kluender and Notowidigdo (2018a), hospital admissions and medical expenses can lead to significant financial distress including bankruptcy for both insured and uninsured individuals. Hence, it is plausible that even if individuals experiencing health shocks in this paper are likely to be covered with medical insurance, they may still experience significant financial costs owing to unexpected medical bills.

The identifying assumption 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 health shock would have been the same as for similar individuals whose peers did not default. To provide empirical support to this assumption, I test for pre-trends in leverage between treated and control individuals in Figure 6. Panel A plots unconditional means for both the treated and control groups for twelve months around peer distress. The blue color represents the treated group while the black color represents the control group. The vertical bars represent confidence intervals at 95% level. The plot suggests that both treated and control groups follow parralel trends in the pre-period. Though treated individuals have slightly lower debt-to-income ratios, the difference is not significant for most of the months before treatment. However, following peer distress, the trends diverge as leverage for treated individuals declines signficantly more than for the control group.

I verify this by empirically estimating the differential trends between treated and control groups

[13] Table IA1 reports results of the same estimation as in Columns (4) through (6) of Table 4 but also reports coefficients on the control variables. In Table IA2, I conduct a robustness test where I control for different personal characteristics in a non-parametric manner.

using Equation 2. This estimation also allows for a better exploration of the dynamics of the treatment effect. 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. Panel B plots the coefficients for this estimation which shows that the effects for the period before peer distress are not statistically different from zero, thus confirming that the trends in individual leverage for treated and control individuals are statistically indistinguishable for the period before peer distress. However, leverage declines significantly more 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 assume the balance for a particular category of account to be zero if the individual doesn't have an account in that category. For instance, when estimating the effect on credit card balance, I assume a balance of zero for those individuals who do not have an open credit card account.[14]

Table 5 reports estimates from this analysis. Columns (1) and (4) report estimates for the effect on credit card balance. The coefficient in column (1) shows that credit card balance for treated individuals declines by $136 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 (4) 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 $126 (3% of the mean) and $1,768 (4.8% 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 7 plots the unconditional means around peer shocks (Panel A) and coefficients estimated using Equation 2 (Panel B) for home loans. As before, the horizontal axis represents months relative to peer distress, and the vertical axis represents the average magnitude of the home loans or coefficient estimates. Panel A shows that home loan balances follow a parallel trend during the months before peer distress while they significantly decline more for the treated individuals relative to the control group after peer distress. Panel B confirms this finding where the estimates for the period before peer distress are not statistically different from zero but decline significantly during the months

[14] The results are robust to confining the sample to individuals with at least one open account in the given category.

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 $28 (4.6% of the mean), and utilization declines by 1.9 pp (4.5% of the mean) more for treated individuals relative to the control group (Columns (2) and (3)).[15] 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 $661 less than the control group (Column (2)). This effect is economically significant as it corresponds to 2.9% of the mean auto loan origination amount. Note that Columns (2) and (6) use a specification that includes event-by-firm fixed effects instead of event-by-individual fixed effects because including event-by-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 (8)). 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 4.7% less than the control

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

group following treatment.

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 moving forward.

4.3 Borrowing vs Consumption

The results discussed so far show that individual debt declines following peer 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. For instance, individuals could be paying higher downpayments and borrowing less which would result in a decline in debt but not consumption. I test for this mechanism by examining the effect of peer 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 \$1,690 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 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. Columns (1) and (4) report results for the effect on the likelihood of delinquency (on any type of loan). The estimate in column (3) shows 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 estimate corresponds to a social multiplier of -0.16 for defaults. In columns (2) and (5), I focus on medical related defaults and find that individuals whose peers experience distress owing to health shocks are 0.07 pp less likely to default on their own medical bills relative to the control group. These findings suggest that peers are likely not subject to common unobservable shocks that drive health shocks across peer groups. Columns (3) and (6) report results for the effect on credit score and the estimate in column (6) finds that credit scores for treated individuals increases by 10 points more than scores for the control group following peer distress.

5 Learning & Alternative 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 between changes in preferences and expectations. Hence, I refer to the combination of changes in beliefs and preferences as learning. Any results consistent with learning 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 learning, 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.[16]

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 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.

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 event-by-median fixed effects to ensure that treated individuals are compared to the matched 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 event-by-

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

decile-by-month fixed effects, in addition to the fixed effects included in the baseline, which ensure that estimates are computed by comparing treated individuals to the matched control individuals with similar levels of ex-ante leverage. The estimates show that individuals in the lowest decile based on ex-ante leverage increase their leverage following peer distress while those in the top five deciles decrease their leverage. However, the negative effect for the highest deciles is much stronger than the positive effect for the lowest decile. These results are consistent with over half of the individuals in the treated sample under-estimating and around 10% of them over-estimating the cost of distress ex-ante.

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

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.

The degree of updating should also depend on the uncertainty in ex-ante priors. I proxy for this uncertainty in priors based on individuals' early life experiences. Individuals who experience a recession during their formative years are likely to have better information about the costs of distress than individuals who did not experience a recession. 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 learning, I find that leverage and debt decline significantly more for individuals who did not experience a recession during their formative years.

The results presented in this section may also be driven by individuals learning about the cost of experiencing health shocks instead of cost of experiencing financial distress. To distinguish between the two, I examine the heterogeneity in responses for hourly and salaried workers. Hourly wage workers are less likely to be provided with medical insurance by their employers and hence more likely to have worse health insurance. If the results are driven by individuals learning that health shocks are more costly than they expected, then one would expect that individuals with worse health insurance should react more because they will bear higher costs in the event of a health shock.

Table 11 reports estimates for this analysis. The specification used in the analysis includes event-by-job type-by-month fixed effects, in addition to the fixed effects included in the baseline specification, which ensure that coefficients are estimated using a comparison in trends between treated and control individuals employed at the same type of job (hourly or salaried). Across the three outcome variables, I don't find any difference in response between individuals employed at hourly jobs and those employed at salaried jobs suggests that learning about costs of health shock likely doesn't play an important role in driving the results.

However, to the extent that hourly and salaried workers have similar health insurance, these results may be driven by individuals learning about the cost of experiencing a health shock instead of learning about the cost of financial distress.

5.2 Salience

Individual beliefs and preferences may also change owing to the salience of the peer distress event. However, salience would predict that the effect of peer distress on individual leverage would be short-lived. I test this prediction 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 6 plots these coefficients for debt-to-income ratio (panel A) and home loans (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 home loans for treated and control individuals are statistically indistinguishable for the period before treatment. However, both leverage and home loans decline significantly for treated individuals following peer distress, and the effect lasts for at least twenty quarters. To the extent that twenty quarters is a long-term effect, this suggests that the effects are plausibly not driven by salience as there is no reversal in the effect for a long time.

5.3 Peer Effects of Consumption

An alternative channel through which peer distress may affect individual borrowing is 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 12 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 Robustness

6.1 Peer Proximity

An assumption in the analysis is that peers either discuss or observe each others' distress experiences. Though the results presented so far support this assumption, I further test it by conducting robustness checks. If this assumption holds and the results are driven through social interactions, one would expect stronger results when peers have closer relationships. I conduct a couple of tests to evaluate this conjecture.

First, I examine 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 13 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.

Second, I estimate the effects for different samples where I define peers differently and this allows me to proxy for closeness among peers. Table 14 reports results for this estimation. In Columns (1)-(3), I use the baseline definition of peers where they are defined as individuals residing in the same zipcode and employed at the same firm with the same job role, while in Columns (4)-(6), peers are defined as individuals residing in the same zipcode but employed at a different firm, and in Columns (7)-(9), peers are defined as individuals residing in different zipcodes but employed at the same firm with same job role.[17] Individuals residing in the same zipcode and employed at the same firm with the same job title are likely to interact more amongst themselves relative to individuals who either don't work for the same firm or don't reside in the same zipcode. Consistent with this conjecture, I find weaker results for both these alternate peer definitions. The results for instances where peers are co-workers but not neighors are larger than instances where peers are neighbors but not co-workers, thus suggesting that co-workers are more likely to interact amongst themselves than neighbors (as documented in De Giorgi et al. (2017)).

6.2 Alternate Sample

In this sub-section, I repeat my baseline analysis with a new sample where the control individuals are defined as those residing in the same zip code and employed at the same firm with similar income as the individuals whose peers experience health shocks but with 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

[17] In order to limit the number of observations, I randomly choose upto ten peers for each health shock in the sample.

also earns in the same range. This specification helps control for zipcode level economic conditions because the control individuals also reside in the same zipcode and hence are subject to similar economic conditions. However, these control individuals may also be exposed to treatment as they also reside in the same zipcode and are employed at the same firm as individuals experiencing health shocks and may interact with them. To the extent that control individuals interact with individuals experiencing health shocks, the estimates from this analysis would be attenuated.[18]

Table IA3 reports estimates for this analysis. The coefficient in Column (1) shows that the debt-to-income ratio declines by 7.1 percentage points more for treated individuals relative to the control group. Consistent with the attenuation bias, this estimate is smaller than the effect of 11.6 percentage points documented in Table 4. In Column (2), I find that the change in leverage is lower for treated individuals relative to control individuals following peer distress. Column (3) shows that total debt declines by $1,988 more for treated individuals relative to the control group. Finally, in columns (4) through (6), I find similar results with a specification that includes control variables.

7 External Validity & Implications

7.1 Are Results Specific to Health Shocks?

If the results are driven by individuals updating their beliefs on costs associated with experiencing health shocks, the results may not generalize to other types of financial distress situations. I check for this external validity by examining the effect of peer financial distress not triggered by health shocks on individual leverage and borrowing behavior. For this analysis, I confine myself to instances where peers default on credit card, auto or home loan payments and file for bankruptcy within a year. Confining 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 are more likely to be driven by local economic shocks, in this specification, I identify the control group from within the same zip code as in the previous section.

Table IA4 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 8.2 percentage points more for treated individuals relative to the control group (Column (1)). I find similar results with the specification that includes control variables. The estimate reported in column (6) shows that individual

[18]Figure IA1 illustrates the variation used in this sample with an example.

debt declines by over $4,238 more for treated individuals relative to the control group.

I also find results similar to the baseline specification for different components and characteristics of debt, as reported in Tables IA5 and IA6 of Appendix IA.

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

7.2 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 main specification, while for credit card, auto or mortgage defaults that lead to bankruptcy filings I use estimates from the second robustness specification described in Section 7.1. This calculation involves two caveats. First, I assume the same average effect even for individuals not included in my sample. Since my sample is selected from over 30 million individuals and covers a representative sample of defaults in the U.S., this assumption may not be completely implausible. Second, this calculation assumes that individuals who default are employed and the spillover effect occurs only for their co-workers. Third, 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 $2357.05 and the average number of peers is seven, this yields an aggregate effect of a decline of $1.18 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 $193.43 billion on total household debt. Thus, these peer effects can explain a decline of up to $194.61 billion in total household debt between 2011 and 2015. This decline is economically significant as it corresponds to 1.65% of the total household debt of $11.75 trillion as of January, 2011.

8 Conclusion

This paper documents that peer 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 beliefs and preferences 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.

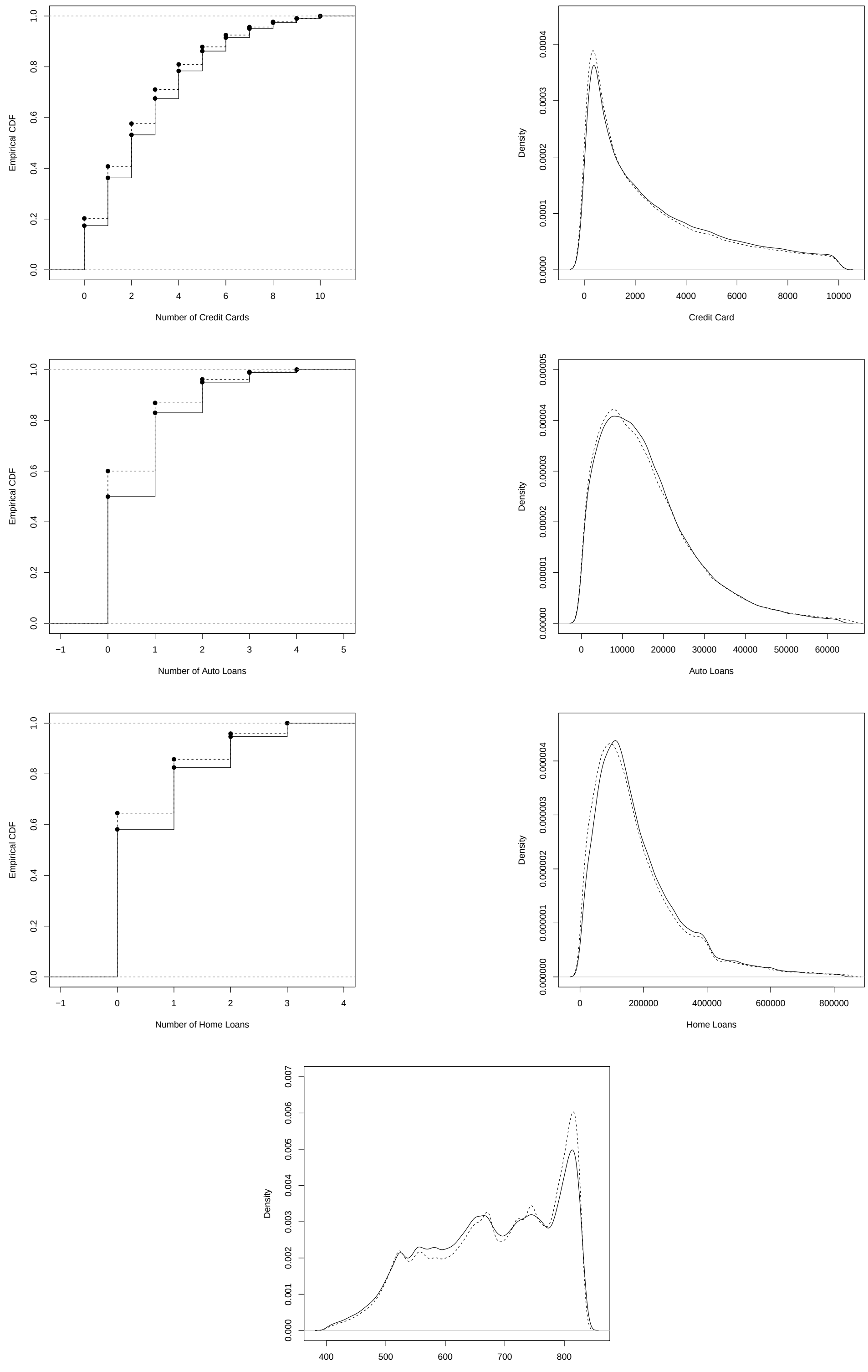


Figure 2: Sample Representativeness II

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.

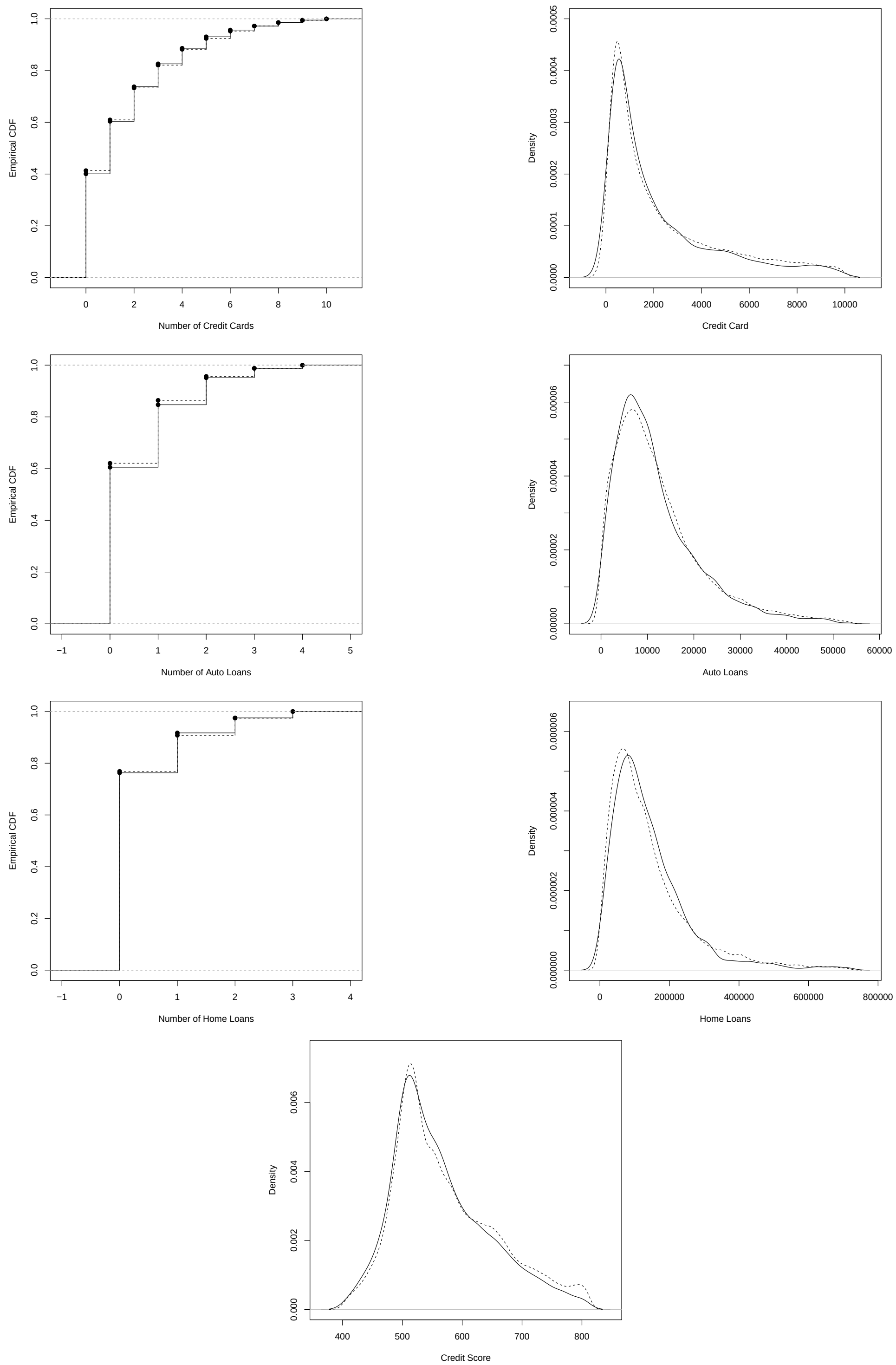


Figure 3: Description of Variation: Comparing Individuals with Same Job Roles but Living in Neighboring States

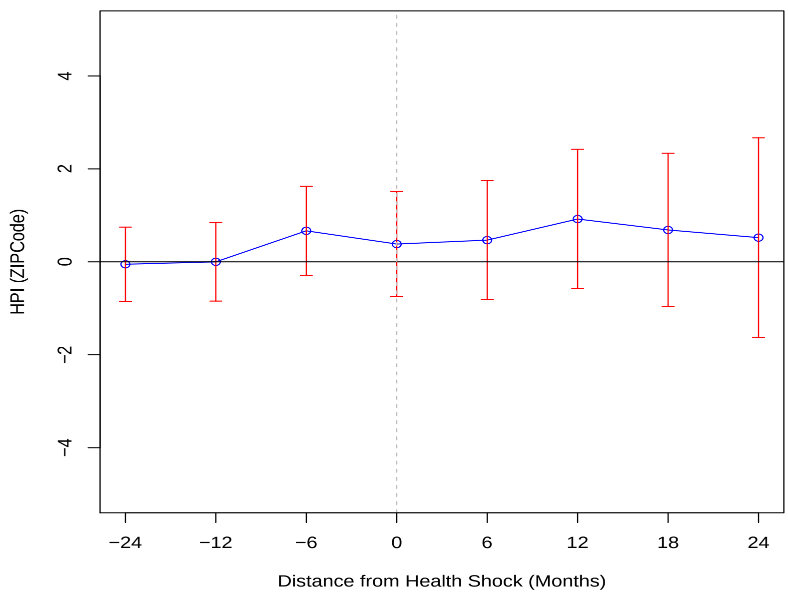
This figure describes the variation used in main specification where the treated individuals are those whose peer experiences distress while the control individuals are those who are employed at the same firm with the same job title but reside in a neighboring state.

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

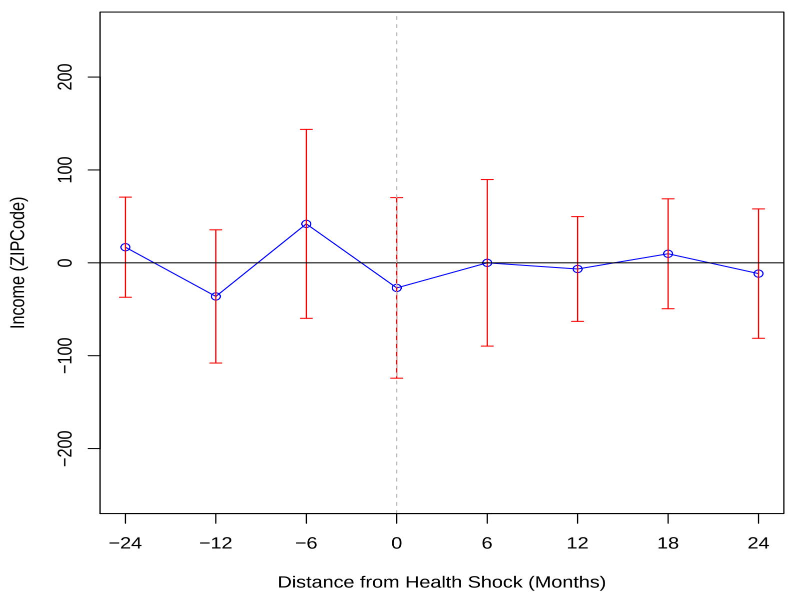
First Difference: (Post-Distress) - (Pre-Distress)	Pre-Distress	Post-Distress
Second Difference: Treated-Control	Sales Rep B living in zip code Z and employed at firm Y (Treated)	Sales Rep C living in zip code Z1 (in a neighboring state) and employed at firm Y (Control)

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.



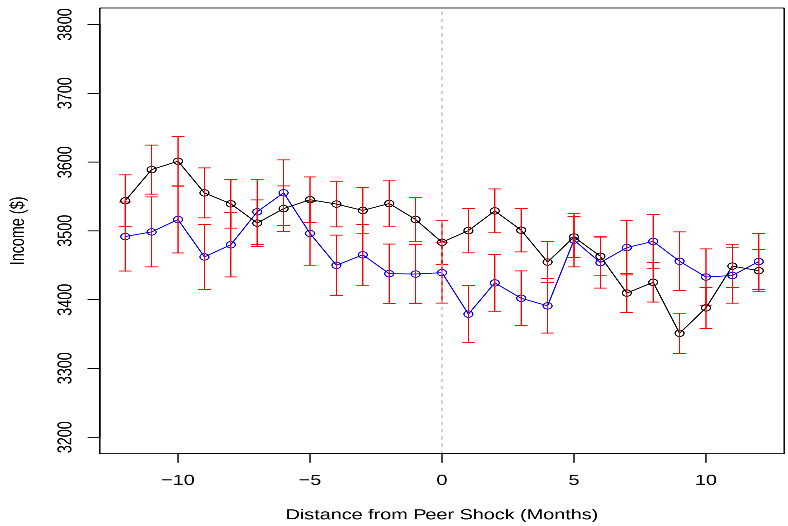
Panel A : House Price Index (HPI)



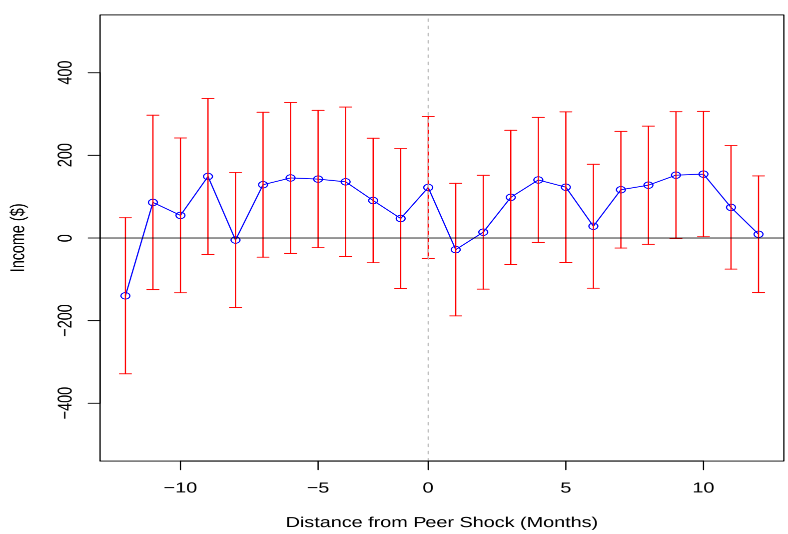
Panel B : Income

Figure 5: Peer Financial Distress and Labor Market Outcomes

This figure compares the trends in income for individuals whose peers experienced distress owing to health shocks to the matched control group of individuals for months around peer distress. Panel A plots unconditional means for the treated and control groups based on event-time. The blue color represents the treated group while the black color represents the control group. Panel B plots the estimates for the dynamic difference-in-differences regression similar to Equation 2 where the outcome variable is income. Vertical bars represent confidence intervals at 5% level.



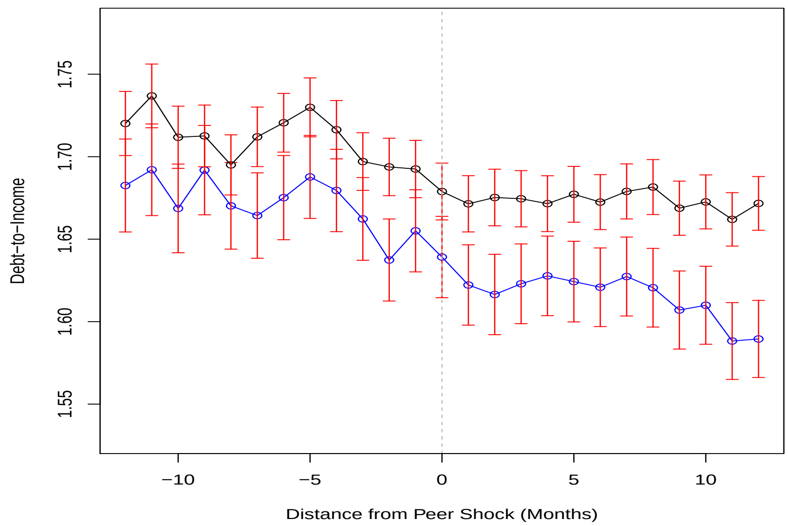
Panel A : Unconditional Means



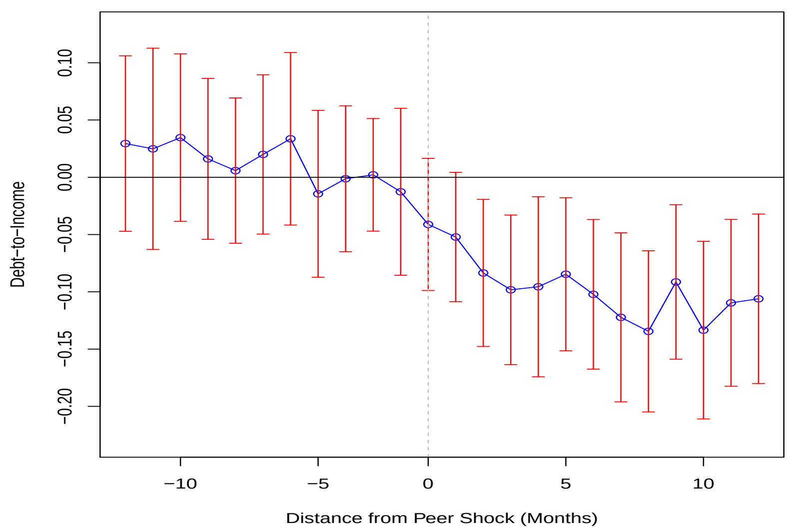
Panel B : Coefficient Estimates

Figure 6: Peer Financial Distress and Individual Leverage

This figure compares the trends in leverage for individuals whose peers experienced distress owing to health shocks to the matched control group of individuals for months around peer distress. Panel A plots unconditional means for the treated and control groups based on event-time. The blue color represents the treated group while the black color represents the control group. Panel B plots the estimates for the dynamic difference-in-differences regression similar to Equation 2 where the outcome variable is the debt-to-income ratio. Vertical bars represent confidence intervals at 5% level.



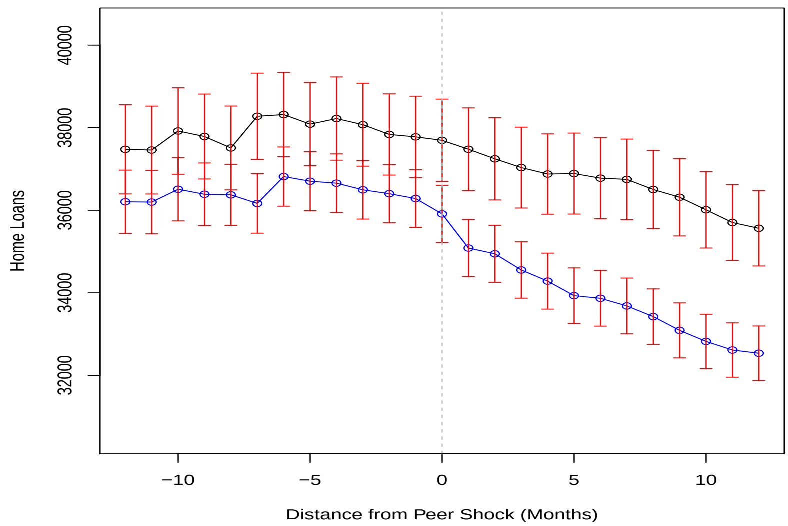
Panel A : Unconditional Means



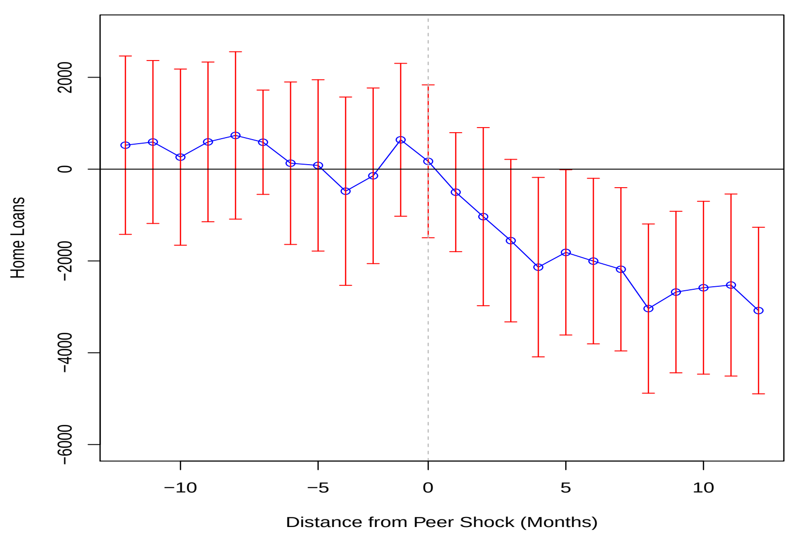
Panel B : Coefficient Estimates

Figure 7: Peer Financial Distress and Components of Debt

This figure compares the trends in home loans for individuals whose peers experienced distress owing to health shocks to the matched control group of individuals for months around peer distress. Panel A plots unconditional means for the treated and control groups based on event-time. The blue color represents the treated group while the black color represents the control group. Panel B plots the estimates for the dynamic difference-in-differences regression similar to Equation 2 where the outcome variable is home loan balance. Vertical bars represent confidence intervals at 5% level.



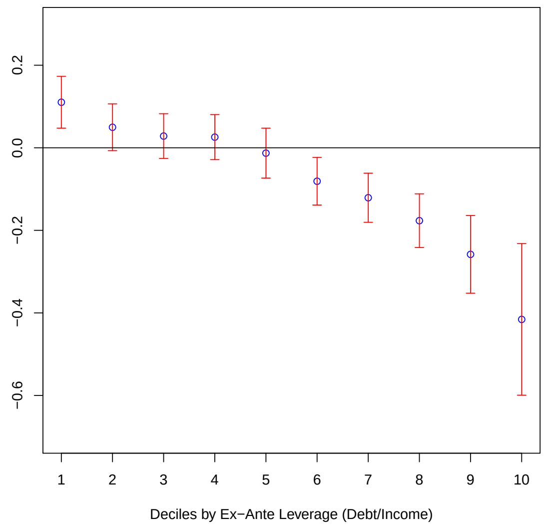
Panel A : Unconditional Means



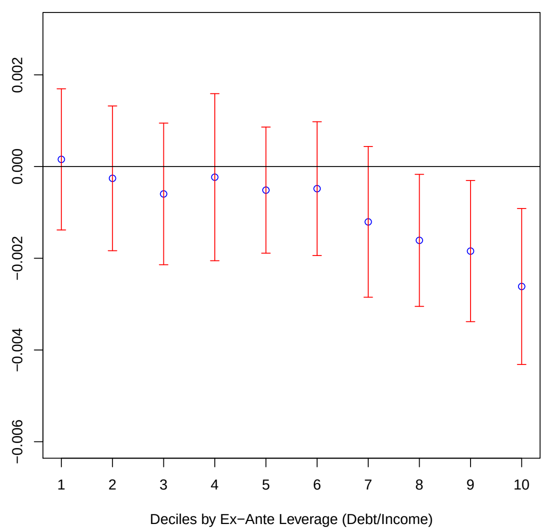
Panel B : Coefficient Estimates

Figure 8: Heterogeneity by Ex-Ante Leverage

This figure plots the estimates for the triple difference regressions that examine the differential effect of peer distress owing to health shocks on individuals with different levels of ex-ante leverage. In this estimation, the difference-in-differences term is interacted with dummy variables that identify the decile that treated individuals belong to based on ex-ante leverage. The specification includes event-by-decile-by-month fixed effects, in addition to the fixed effects included in the baseline, which ensures that the coefficients are estimated by comparing treated individuals to matched control individuals who have similar levels of ex-ante leverage. Panel A plots the estimates for Debt-to-Income ratio while Panel B plots them for delinquency. Vertical bars represent confidence intervals at the 5% level.



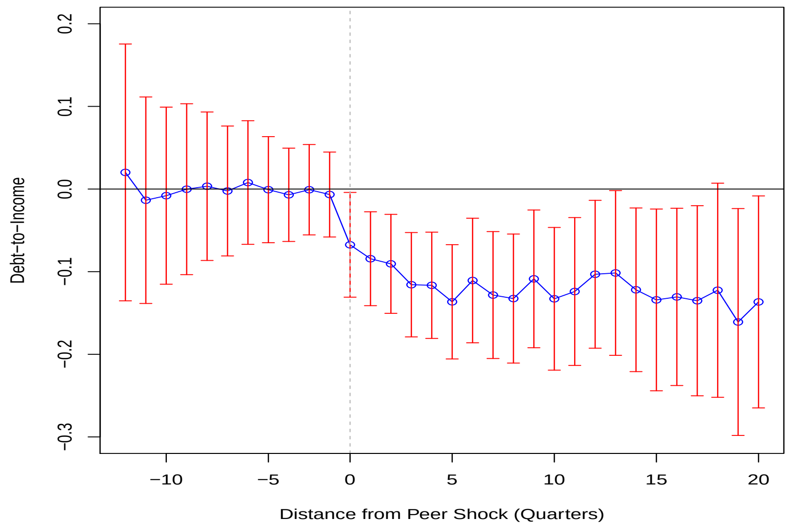
Panel A : Debt-to-Income Ratio



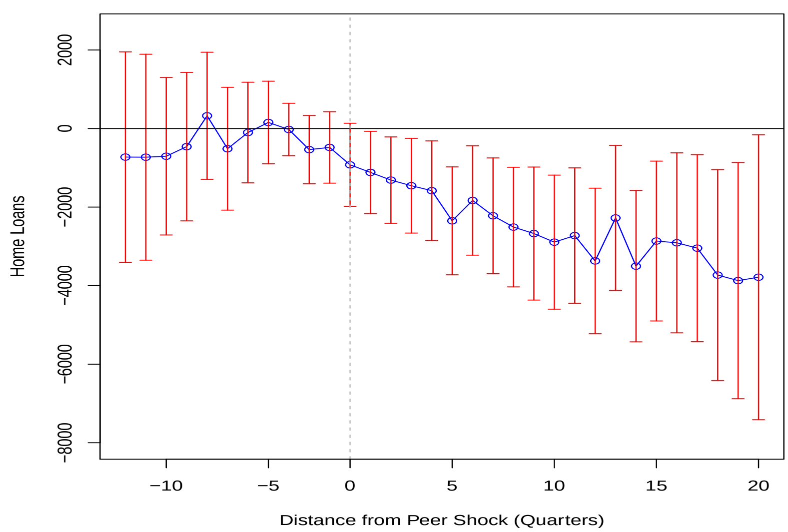
Panel B : Delinquency Rate

Figure 9: Long Term Dynamics

This figure compares longer-term trends in leverage and mortgage loans for individuals whose peers experienced distress owing to health shocks to the matched control group of individuals for quarters around peer distress. Panel A plots the estimates for the dynamic difference-in-differences regression similar to Equation 2 where the independent difference-in-differences variable is interacted with event-time in quarters instead of months, and the outcome variable is the debt-to-income ratio. Panel B plots similar estimates for home loan balances. Vertical bars represent confidence intervals at 5% level.



Panel A : Debt-to-Income Ratio



Panel B : Home Loans

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.

	Mean	SD	Median	Min	Max
Leverage					
$\frac{Debt}{Income}$	1.61	2.45	0.58	0	9.46
Debt Balances					
Debt	52,615.18	81,832.11	15,519	0	478,218
Credit Card	2,497.61	4,654.44	429	0	32,435
Auto Loans	4,111.71	7,402.89	0	0	45,509
Home Loans	36,726.03	70,564.98	0	0	438,224
Credit Card Characteristics					
Openings	0.01	0.08	0	0	1
Payments	578.46	1,067.87	260	1	6,762
Utilization	0.42	0.45	0.22	0	1.16
Spending	597.36	1435.19	295	-4,913	7,781
Auto Loan Characteristics					
Openings	0.001	0.03	0	0	1
Payments	369.83	666.97	263	0	3,072
Loan Origination Amounts	22,550.21	16,108.26	18,180	0	84,113
Home Loan Characteristics					
Openings	0.0001	0.01	0	0	1
Payments	1715.43	3232.75	780	0	15,872
Loan Origination Amounts	263,171.89	224,658.75	178,703	0	1,263,250
Savings & Employment					
Savings	9,711.23	16,625.98	2,974	0	78,874
Employment	0.46	0.49	0	0	1
Loan Performance					
Delinquency	0.01	0.09	0	0	1
Credit Score	634.71	131.77	641	0	839
Income & Age					
Income (Monthly $)	3,386.47	3,594.87	2,201.71	802.71	33,801.08
Age	41.12	15.08	34	18	99
Peer Size					
Number of Peers	7.13	19.55	5	1	165

Table 2: Summary Comparison - Treated vs Control Individuals

This table reports descriptive statistics that compare treated and control individuals. 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)
$\frac{Debt}{Income}$	1.67	0.61	1.70	0.66	-0.03
Debt	54,035.01	17,568	54,660.51	17,750	-625.5
Credit Card	2,580.41	423	2,618.67	469	-38.26
Auto Loans	4,115.43	0	4,209.51	0	-94.08
Home Loans	36,495.86	0	38,073.08	0	-1,577.22
Credit Card Utilization	0.436	0.24	0.43	0.21	0.006
Income (Monthly $)	3,463.69	2,187	3,531.27	2,217	-67.58
Age	40.61	38	40.95	38	-0.34**

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 distress induced by health-shocks and labor market outcomes:

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

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 distress owing to health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents income and likelihood of being employed at the same firm with the same job role while residing in the same zipcode. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Income	Employment	Income	Employment
	(1)	(2)	(3)	(4)
$PeerShock \times Post$	34.85	-0.003	34.16	-0.003
	(31.98)	(0.005)	(31.61)	(0.005)
Controls	No	No	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes
Observations	6,496,191	11,496,255	6,128,418	6,128,418
R2	0.647	0.821	0.652	0.832

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 distress induced by health-shocks on individual leverage and debt:

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

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 distress owing to health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total debt. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)	(4)	(5)	(6)
$PeerShock \times Post$	-0.116***	-0.026***	-2,541.28***	-0.092***	-0.021***	-2,357.05***
	(0.024)	(0.003)	(593.36)	(0.026)	(0.004)	(684.01)
Controls	No	No	No	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,496,191	6,128,418	11,496,255	6,128,418	6,128,418	6,128,418
R2	0.709	0.195	0.841	0.763	0.206	0.850

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 distress induced by health-shocks on different components of debt:

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

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 distress owing to health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents different components of individual debt. Standard errors are double clustered at the individual and month level, and 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)
$PeerShock \times Post$	-136.61***	-126.49***	-1,768.95***	-127.51***	-97.49**	-1,770.65***
	(41.82)	(37.28)	(487.51)	(46.48)	(49.21)	(573.64)
Controls	No	No	No	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,496,255	11,496,255	11,496,255	6,128,418	6,128,418	6,128,418
R2	0.756	0.599	0.827	0.772	0.619	0.837

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 distress induced by health-shocks on individual level debt characteristics:

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

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 distress owing to health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents different characteristics of various components of debt. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

Panel A : Credit Card

	Openings (1)	Spending (2)	Utilization (3)	Payment (4)	Openings (5)	Spending (6)	Utilization (7)	Payment (8)
$PeerShock \times Post$	-0.0002	-27.86***	-0.019***	-7.78	-0.00002	-22.10**	-0.014***	-9.32
	(0.0002)	(7.42)	(0.006)	(11.73)	(0.0003)	(9.62)	(0.005)	(12.94)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,496,255	5,139,155	9,352,950	5,139,155	6,128,418	3,204,803	5,709,753	3,204,803
R2	0.057	0.528	0.714	0.592	0.070	0.547	0.732	0.607

Panel B : Auto Loans

	Openings (1)	Origination Amt (2)	Origination Amt (3)	Payment (4)	Openings (5)	Origination Amt (6)	Origination Amt (7)	Payment (8)
$PeerShock \times Post$	0.00004	-661.39***	-287.98**	-22.09*	-0.0001	-561.94**	-161.22	-20.07
	(0.0001)	(247.40)	(147.43)	(12.28)	(0.0001)	(267.11)	(159.82)	(16.41)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Event×Individual FE	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Firm FE	No	Yes	No	No	No	Yes	No	No
Observations	11,496,255	4,731,560	4,731,560	4,664,183	6,128,418	2,998,990	2,998,990	2,968,954
R2	0.056	0.215	0.782	0.711	0.076	0.252	0.796	0.727

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*	-0.00001	-12,472***	-4,782**	-1,179*	0.00000	-14,799***	-6,481**	-840.66
	(0.00002)	(4,959.31)	(2,450.22)	(635.72)	(0.00003)	(5,251.97)	(2,981.11)	(640.71)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Event×Individual FE	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Firm FE	No	Yes	No	No	No	Yes	No	No
Observations	11,496,255	2,452,908	3,463,416	3,450,086	6,128,418	1,785,411	2,281,109	2,270,083
R2	0.041	0.263	0.901	0.697	0.060	0.295	0.908	0.717

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 distress induced by health-shocks on savings:

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

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 distress owing to health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents different characteristics of various components of debt. 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, and is only available at bi-annual frequency. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Savings (1)	Savings (2)
$PeerShock \times Post$	1,690.69*	1,727.61*
	(913.57)	(1024.78)
Controls	No	Yes
Event×Individual FE	Yes	Yes
Event×Month FE	Yes	Yes
Observations	1,762,754	1,050,185
R2	0.608	0.690

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 distress induced by health-shocks on delinquency rate and credit score:

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

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 distress owing to health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents delinquency rate and credit score. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Delinquency	Medical Default	Credit Score	Delinquency	Medical Default	Credit Score
	(1)	(2)	(3)	(4)	(5)	(6)
$PeerShock \times Post$	-0.003***	-0.001***	10.45*	-0.001**	-0.0007***	9.81*
	(0.001)	(0.0004)	(6.05)	(0.0005)	(0.0002)	(5.91)
Controls	No	No	No	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,496,255	11,496,255	11,496,255	6,128,418	6,128,418	6,128,418
R2	0.735	0.613	0.851	0.747	0.615	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 lagged values of $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, 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 (as measured by debt-to-income ratio), log-changes in leverage and total debt. All specifications include matched group-by-individual and matched group-by-month fixed effects. Panel A includes marched group-by-restriction-by-month 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 marched group-by-median-by-month 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. 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.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)
$PeerShock \times Post \times NoRestriction$	-0.122***	-0.029***	-3,226.12***
	(0.031)	(0.006)	(809.76)
$PeerShock \times Post \times MediumRestriction$	-0.087**	-0.020**	-1,594.88*
	(0.039)	(0.009)	(883.66)
$PeerShock \times Post \times SevereRestriction$	-0.027	-0.013	-983.17
	(0.034)	(0.010)	(252.62)
NoRestriction-SevereRestriction	-0.095**	-0.016	-2242.95***
Controls	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes
Event×Restriction×Month FE	Yes	Yes	Yes
Observations	6,128,418	6,128,418	6,128,418
R2	0.767	0.211	0.853

Panel A: Wage Garnishment Laws

Table 9 (contd)

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)
$PeerShock \times Post \times Above$	-0.298***	-0.045***	-6,155.38
	(0.032)	(0.004)	(807.56)
$PeerShock \times Post \times Below$	0.055*	0.005	1,169.29*
	(0.029)	(0.004)	(636.02)
Above-Below	-0.353***	-0.05***	-7325.09***
Controls	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes
Event×Median×Month FE	Yes	Yes	Yes
Observations	6,128,418	6,128,418	6,128,418
R2	0.765	0.209	0.852

Panel B: Ex-Ante Leverage

Table 9 (contd)

	Leverage	Leverage	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt	Debt	Debt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$PeerShock \times Post \times Above$	-0.360***	-0.298***	-0.257***	-0.063***	-0.034***	-0.029**	-8,383.57***	-5,373.67***	-3,480.30**
	(0.050)	(0.073)	(0.068)	(0.014)	(0.013)	(0.014)	(1279.67)	(1615.73)	(1611.31)
$PeerShock \times Post \times Below$	0.059	0.076	0.133**	-0.020***	0.001	0.005	192.84	486.08	2670.38*
	(0.043)	(0.061)	(0.061)	(0.008)	(0.013)	(0.013)	(1079.76)	(1351.38)	(1447.44)
Sample	No Restriction	Medium Restriction	Severe Restriction	No Restriction	Medium Restriction	Severe Restriction	No Restriction	Medium Restriction	Severe Restriction
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Median×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,847,876	2,220,326	1,060,216	2,847,876	2,220,326	1,060,216	2,847,876	2,220,326	1,060,216
R2	0.777	0.787	0.818	0.248	0.243	0.231	0.858	0.867	0.879

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 distress induced by health-shocks on individual leverage and debt based on individual exposures to recession and non-recession periods during their formative years:

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

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 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; $Recession$ ($Non-Recession$) 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; $\delta_{c\times i}$ are event-by-individual fixed effects, $\delta_{c\times t}$ are event-by-month fixed effects and $\delta_{c\times exp\times t}$ are event-by-experience-by-time fixed effects that ensure that comparison in trends occurs between treated and control individuals with similar formative experiences; event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total 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.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)
$PeerShock \times Post \times Recession$	-0.070***	-0.010**	-1,821.36***
	(0.025)	(0.005)	(634.79)
$PeerShock \times Post \times Non-Recession$	-0.194***	-0.026***	-5,378.61***
	(0.036)	(0.006)	(964.82)
Recession-(Non-Recession)	0.124***	0.016**	-7199.97***
Controls	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes
Event×Experience×Month FE	Yes	Yes	Yes
Observations	6,128,418	6,128,418	6,128,418
R2	0.765	0.210	0.852

Table 11: Heterogeneity by Hourly vs Salaried Workers

This table reports estimates for the triple difference regressions of the following form that estimate the heterogeneous effect of peer distress induced by health-shocks on individual leverage and debt based on whether individuals are employed at hourly or salaried jobs:

$$y_{i,t} = \beta_1 PeerShock_i \times Post_t \times Hourly + \beta_2 PeerShock_i \times Post_t \times Salaried + \delta_{c\times i} + \delta_{c\times t} + \delta_{c\times job\times t} + \gamma X_{i,t-1} + \epsilon_{i,t}$$

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 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; $Hourly$ ($Salaried$) is an indicator variable that takes a value of one for individuals employed at hourly (salaried) jobs; $\delta_{c\times i}$ are event-by-individual fixed effects, $\delta_{c\times t}$ are event-by-month fixed effects and $\delta_{c\times job\times t}$ are event-by-job type-by-month fixed effects that ensure that comparison in trends occurs between treated and control individuals employed at the same type of job (hourly or salaried); event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total 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.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)
$PeerShock \times Post \times Hourly$	-0.089***	-0.017***	-2,044.56***
	(0.033)	(0.004)	(793.32)
$PeerShock \times Post \times Salaried$	-0.091***	-0.016***	-2,092.13***
	(0.041)	(0.005)	(859.42)
Hourly-Salaried	0.002	-0.001	47.57
Controls	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes
Event×Job Type×Month FE	Yes	Yes	Yes
Observations	4,135,258	3,915,702	4,135,258
R2	0.814	0.241	0.866

Table 12: 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 distress induced by health-shocks on individual leverage and debt based on whether they continue to be peers or not following distress:

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

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 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; $Peer$ ($Non - Peer$) 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; $\delta_{c\times i}$ are event-by-individual fixed effects and $\delta_{c\times t}$ are event-by-month fixed effects where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total 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.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)
$PeerShock \times Post \times Peer$	-0.110***	-0.016***	-2,402.96***
	(0.025)	(0.005)	(882.11)
$PeerShock \times Post \times Non - Peer$	-0.104**	-0.023***	-2,319.02***
	(0.032)	(0.004)	(647.93)
Peer-(Non-Peer)	-0.006	0.007	-83.94
Controls	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes
Observations	6,128,418	6,128,418	6,128,418
R2	0.765	0.209	0.852

Table 13: 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 distress induced by health-shocks on individual leverage and debt based on the number of peers:

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

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 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; $Above$ ($Below$) is an indicator variable that takes a value of one for treated individuals with the above (below) median number of peers; $\delta_{c\times i}$ are event-by-individual fixed effects and $\delta_{c\times t}$ are event-by-month fixed effects where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables that includes $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total 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.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)
$PeerShock \times Post \times Above$	-0.042	-0.010	-1,081.99
	(0.032)	(0.007)	(807.43)
$PeerShock \times Post \times Below$	-0.110***	-0.027***	-3,102.36***
	(0.010)	(0.004)	(468.85)
Above-Below	0.068**	0.017**	2020.37***
Controls	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes
Observations	6,128,418	6,128,418	6,128,418
R2	0.765	0.209	0.852

Table 14: Alternative Peer Definitions

This table reports estimates for the difference-in-differences regressions of the following form that estimate the effect of peer distress induced by health-shocks on individual leverage and debt for alternative peer definitions:

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

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 distress owing to health shocks; $Post_t$ is a dummy variable that takes a value of one during the months following distress; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total debt. In Columns (1)-(3), peers are defined as individuals residing in the same zipcode and employed at the same firm with the same job role; while in Columns (4)-(6), peers are defined as individuals residing in different zipcodes but employed at the same firm with same job role; and in Columns (7)-(9), peers are defined as individuals residing in the same zipcode but employed at a different firm. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$PeerShock \times Post$	-0.092***	-0.021***	-2,357.05***	-0.035***	-0.01***	-890.83***	-0.013***	-0.004**	-387.32***
	(0.026)	(0.004)	(684.01)	(0.015)	(0.003)	(291.66)	(0.004)	(0.002)	(138.67)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,128,418	6,128,418	6,128,418	9,688,234	9,688,234	9,688,234	10,389,626	10,389,626	10,389,626
R2	0.763	0.206	0.850	0.781	0.194	0.869	0.748	0.173	0.806

IA Internet Appendix

Figure IA1: Description of Variation: Comparing Individuals with Different Job Roles but Living in the Same zip code (Specification II)

This figure describes the variation used in the robustness specification where the treated individuals are those whose peer experience distress while the control individuals are those who reside in the same zipcode and are employed at the same firm but with different job roles.

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)

Table IA1: Peer Financial Distress and Individual Leverage: Estimates for Control Variables

This table reports estimates for the difference-in-differences regressions that estimate the effect of peer distress induced by health-shocks on individual leverage and debt. Each observation corresponds to an individual-month combination for individual i during month t; *PeerShock* is an indicator variable that takes a value of one for individual i whose peers experience distress owing to health shocks, where peers are defined as individuals residing 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. The specification includes event-by-individual fixed effects and event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; and control variables. and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total debt. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)
$PeerShock \times Post$	-0.092***	-0.021***	-2,357.05***
	(0.026)	(0.004)	(684.01)
$Income$	-0.0002***	0.003***	0.557***
	(0.00000)	(0.00000)	(0.068)
$Income^2$	0.00000***	-0.00000***	-0.00002***
	(0.00000)	(0.00000)	(0.00000)
$Income^3$	-0.00000***	0.00000***	0.00000***
	(0.00000)	(0.00000)	(0.00000)
Age	-0.002***	-0.0002**	-37.72***
	(0.0003)	(0.0001)	(7.82)
HPI	-0.00001***	0.0002**	0.0678***
	(0.00000)	(0.0001)	(0.004)
$\overline{EstablishmentIncome}$	-0.00003	0.00000	0.239
	(0.00002)	(0.0002)	(0.26)
Event×Individual FE	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes
Observations	6,128,418	6,128,418	6,128,418
R2	0.763	0.206	0.850

Table IA2: Robustness: Controlling for Time Varying Personal Characteristics

This table reports estimates for the difference-in-differences regressions that estimate the effect of peer distress induced by health-shocks on individual leverage and debt. Each observation corresponds to an individual-month combination for individual i during month t; *PeerShock* is an indicator variable that takes a value of one for individual i whose peers experience distress owing to health shocks, where peers are defined as individuals residing 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. The specification includes event-by-individual fixed effects, event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; and month fixed effects interacted with age, ex-ante leverage, income and tenure at the job deciles. and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total debt. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)
PeerShock × *Post*	-0.084***	-0.024***	-2,465.09***
	(0.020)	(0.004)	(541.51)
Age Decile × Month FE	Yes	Yes	Yes
Ex-ante Leverage Decile × Month FE	Yes	Yes	Yes
Income Decile × Month FE	Yes	Yes	Yes
Tenure Decile × Month FE	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes
Observations	6,128,418	6,128,418	6,128,418
R2	0.760	0.279	0.846

Table IA3: Robustness: Alternate Sample

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

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

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 distress owing to health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total debt. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)	(4)	(5)	(6)
$PeerShock \times Post$	-0.071***	-0.014***	-1,988.64***	-0.078***	-0.015***	-2,071.66***
	(0.016)	(0.002)	(423.92)	(0.018)	(0.005)	(489.17)
Controls	No	No	No	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,712,688	5,622,418	6,107,232	5,622,418	5,622,418	5,622,418
R2	0.778	0.188	0.859	0.791	0.194	0.877

Table IA4: Are Results Specific to Health Shocks? Peer Financial Distress and Individual Leverage (Specification II)

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

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

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 default on their financial loans not induced by health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents individual leverage (as measured by debt-to-income ratio), log-changes in leverage and total debt. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt	Leverage	$Log(\frac{Leverage_t}{Leverage_{t-1}})$	Debt
	(1)	(2)	(3)	(4)	(5)	(6)
$PeerShock \times Post$	-0.082***	-0.002**	-3,538.38***	-0.091***	-0.002**	-4,238.79***
	(0.009)	(0.001)	(330.02)	(0.008)	(0.001)	(297.46)
Controls	No	No	No	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,215,957	11,168,043	15,348,458	11,168,043	11,168,043	11,168,043
R2	0.842	0.116	0.901	0.852	0.133	0.922

Table IA5: Are Results Specific to Health Shocks? Peer Financial Distress and Components of Debt (Specification II)

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

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

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 default on their financial loans not induced by health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents different components of individual debt. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

	Credit Card	Auto	Home Loans	Credit Card	Auto	Home Loans
	(1)	(2)	(3)	(4)	(5)	(6)
$PeerShock \times Post$	-397.83***	-241.03***	-2,984.72***	-387.19***	-252.10***	-2,947.97***
	(25.02)	(38.32)	(259.33)	(24.92)	(38.83)	(262.74)
Controls	No	No	No	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,348,458	15,348,458	15,348,458	11,168,043	11,168,043	11,168,043
R2	0.735	0.567	0.807	0.741	0.574	0.811

Table IA6: How Do Individuals Reduce Debt? (Specification II)

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

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

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 default on their financial loans not induced by health shocks, where peers are defined as individuals residing 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; $\delta_{e\times i}$ are event-by-individual fixed effects and $\delta_{e\times t}$ are event-by-month fixed effects, where event refers to health shocks and identifies the group of treated and control individuals associated with the same health shock; $X_{i,t-1}$ is a vector of control variables including $Income$, $Income^2$, $Income^3$, $\overline{EstablishmentIncome}$, HPI and Age; and $y_{i,t}$ represents different characteristics of various components of debt. Standard errors are double clustered at the individual and month level, and reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level respectively.

Panel A : Credit Card

	Openings (1)	Spending (2)	Utilization (3)	Payment (4)	Openings (5)	Spending (6)	Utilization (7)	Payment (8)
$PeerShock \times Post$	-0.00003	-31.82***	-0.019***	-11.11	-0.00003	-27.10***	-0.018***	-10.24
	(0.0001)	(4.67)	(0.002)	(13.30)	(0.0001)	(4.62)	(0.002)	(13.28)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Event×Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,348,458	7,121,483	11,128,985	7,121,483	11,168,043	6,364,803	9,686,926	6,252,460
R2	0.024	0.466	0.684	0.564	0.025	0.469	0.732	0.567

Panel B : Auto Loans

	Openings (1)	Origination Amt (2)	Origination Amt (3)	Payment (4)	Openings (5)	Origination Amt (6)	Origination Amt (7)	Payment (8)
$PeerShock \times Post$	-0.00001	-861.39***	-699.06**	10.84	-0.00001	-847.66***	-681.36***	9.85
	(0.00004)	(107.38)	(91.61)	(8.04)	(0.00004)	(103.89)	(91.18)	(7.95)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Event×Individual FE	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Firm FE	No	Yes	No	No	No	Yes	No	No
Observations	15,348,458	7,501,091	7,501,091	7,419,954	11,168,043	6,558,337	6,558,337	6,492,993
R2	0.020	0.083	0.748	0.697	0.020	0.088	0.752	0.702

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	-7,846.52***	-4,913.73***	-98.99	-0.00002	-7,405.82***	-5,890.54***	-72.23
	(0.0002)	(886.92)	(1,507.88)	(178.50)	(0.0001)	(878.05)	(1,518.01)	(176.26)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Event×Individual FE	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Event×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event×Firm FE	No	Yes	No	No	No	Yes	No	No
Observations	15,348,458	6,086,783	6,086,783	6,053,792	11,168,043	5,429,416	5,429,416	5,400,139
R2	0.017	0.277	0.898	0.684	0.018	0.284	0.900	0.692