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DEBT RELIEF AND DEBTOR OUTCOMES: MEASURING THE EFFECTS OF CONSUMER BANKRUPTCY PROTECTION

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ABSTRACT

Consumer bankruptcy is one of the largest social insurance programs in the United States, but little is known about its impact on debtors. We use 500,000 bankruptcy filings matched to administrative tax and foreclosure data to estimate the impact of Chapter 13 bankruptcy protection on subsequent outcomes. Exploiting the random assignment of bankruptcy filings to judges, we find that Chapter 13 protection increases annual earnings by $5,562, decreases five-year mortality by 1.2 percentage points, and decreases five-year foreclosure rates by 19.1 percentage points. These results come primarily from the deterioration of outcomes among dismissed filers, not gains by granted filers.

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"The Bankruptcy Act is...of public as well as private interest, in that it gives to the honest but unfortunate debtor...a new opportunity in life and a clear field for future effort, unhampered by the pressure and discouragement of pre-existing debt."

- U.S. Supreme Court, *Local Loan Co.* v. *Hunt*, 292 U.S. 234 (1934)

In 2010, 1.5 million Americans filed for over $450 billion in debt relief through the consumer bankruptcy system.[1] American households receive more resources through the bankruptcy system than through all state unemployment insurance programs combined (Lefgren, McIntyre, and Miller 2010), with nearly one in ten American households having filed for bankruptcy at some point (Stavins 2000). The U.S. bankruptcy system is also among the most generous in the world, allowing debtors to choose between Chapter 7 bankruptcy that provides debt relief and protection from wage garnishment in exchange for a debtor's non-exempt assets, and Chapter 13 bankruptcy that adds the protection of most assets in exchange for a partial repayment of debt.

Despite providing billions of dollars in debt relief each year, it is not clear how bankruptcy protection impacts debtors. In theory, bankruptcy protection increases an individual's incentive to work and prevents any sharp drops in consumption that may have important long-term consequences, such as becoming sick due to the lack of medical care or losing one's home through foreclosure. Yet, in practice, households work about the same number of hours (Han and Li 2007), accumulate less wealth (Han and Li 2011), and have less access to credit (Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2009) after receiving bankruptcy protection, leading some to conclude that the benefits of debt relief have been overstated (Porter and Thorne 2006). The lack of demonstrable benefits, combined with a rapid increase in the number of bankruptcy filings, led Congress to enact new barriers to filing in the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act.

Empirically estimating the impact of bankruptcy protection has been complicated by two important issues. First, there is little information on the long-term outcomes of most bankruptcy filers. Bankruptcy filers are not tracked in a systematic way after filing and datasets such as the PSID and NLSY include only a few hundred bankrupt households. Second, most comparisons are biased due to selection and endogeneity problems. Bankruptcy filers are likely to have had worse outcomes even before filing, biasing cross-sectional comparisons (e.g. Han and Li 2007, 2011), and most proximate causes of bankruptcy such as job loss and health shocks also impact later outcomes, biasing within-individual comparisons (e.g. Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2009). The lack of an unbiased counterfactual control group may help explain why the previous literature has found little evidence that bankruptcy protection benefits debtors.

In this paper, we use a new dataset linking 500,000 bankruptcy filings to administrative tax records from the Social Security Administration (SSA) and administrative foreclosure records to estimate the causal effect of Chapter 13 bankruptcy protection on subsequent earnings, mortality, and home foreclosure. Our empirical strategy exploits the fact that most U.S. bankruptcy courts use

[1] Non-business Chapter 7 and Chapter 13 filing statistics are available at http://www.uscourts.gov/uscourts/Statistics/BankruptcyStatistics/BAPCPA/2010/Table1A.pdf http://www.uscourts.gov/uscourts/Statistics/BankruptcyStatistics/BAPCPA/2010/Table1D.pdf

a blind rotation system to assign cases to judges, effectively randomizing filers to judges within each office. Moreover, while there are uniform criteria by which a judge may dismiss a bankruptcy filing, there is significant variation in the interpretation of these criteria across judges (Sullivan, Warren, and Westbrook 1994, Norberg and Compo 2007, Chang and Schoar 2008). As a result, otherwise identical filers are assigned to judges with substantially different rates of granting bankruptcy protection.

Using these differences in judge leniency as an instrumental variable for bankruptcy protection, we are able to identify the ex-post impact of Chapter 13 on the marginal recipient of protection – filers whose bankruptcy decision is altered by the judge assignment due to disagreement on whether or not they should receive bankruptcy protection. The identified parameter holds fixed any ex-ante impacts of bankruptcy, such as over-borrowing, moral hazard in the workplace (White 2011), entrepreneurial risk-taking (Fan and White 2003, Armour and Cumming 2008), or the crowding out of formal insurance (Mahoney 2012). Our empirical strategy is similar to Kling (2006), which uses the random assignment of judges to estimate the ex-post impact of sentence length on earnings, and subsequent research that estimates the ex-post effects of foster care (Doyle 2007, 2008), juvenile incarceration (Aizer and Doyle 2013), corporate bankruptcy (Chang and Schoar 2008), temporary-help employment (Autor and Houseman 2010), and Disability Insurance (French and Song 2011, Maestas, Mullen, and Strand 2013).

In our empirical analysis, we find compelling evidence that Chapter 13 bankruptcy protection benefits debtors. Over the first five post-filing years, Chapter 13 protection increases the marginal recipient's annual earnings by $5,562, a 25.1 percent increase from the pre-filing mean. Employment increases by 6.8 percentage points over the same time period, an 8.3 percent increase. Five-year mortality decreases by 1.2 percentage points, a 30.0 percent decrease from the dismissed filer mortality rate, and five-year home foreclosure rates decrease by 19.1 percentage points, a more than 100 percent decrease from the dismissed filer foreclosure rate. There is also evidence that Chapter 13 protection deceases the receipt of Supplemental Security Income (SSI), although there is little to no impact on 401k contributions and the receipt of Disability Insurance (DI). Descriptive results suggest that the estimated impacts come from the deterioration of outcomes among dismissed filers, not gains by granted filers. Filers granted bankruptcy protection have similar pre- and post-filing earnings. In contrast, dismissed filers experience large and persistent drops in earnings after filing for bankruptcy.

We find evidence of two mechanisms through which bankruptcy protection benefits filers. First, we find that the impact of Chapter 13 protection is larger when creditors are allowed to garnish a debtor's earnings. Exploiting within- and across-state variation in the marginal garnishment rate, we find that the implied earnings elasticity with respect to potential garnishment is 0.94. These results are consistent with the idea that bankruptcy protection maintains the incentive to work by preventing an increase in the effective marginal tax rate on earnings. Second, we find results suggesting that Chapter 13 protection helps to maintain economic stability by reducing foreclosures and by reducing strategic moves to evade creditors. The marginal recipient of Chapter 13 protection

is 24.6 percentage points more likely to work in his or her pre-filing job, 23.8 percentage points more likely to work in the same industry, and 15.3 percentage points more likely to work in the same state.

There are three important caveats to our analysis. First, our identification strategy measures the effect of Chapter 13 bankruptcy protection for the marginal recipient. It is possible that the impact of Chapter 13 protection is different for filers who are not on the margin of receipt. To partially address this issue, we estimate marginal treatment effects (MTEs) that measure the change in outcomes for the marginal recipient of bankruptcy protection as we move from more strict judges to more lenient judges. The MTE results suggest that the effects of Chapter 13 protection are slightly larger for less deserving filers. Second, we are only able to use our instrumental variables strategy in bankruptcy offices that randomly assign filings to judges. While our instrumental variables sample is broadly similar to the full sample of Chapter 13 filers, it is possible that the effect of bankruptcy may differ in the two samples. To provide some evidence on this issue, we estimate the impact of Chapter 13 in both samples using an event study methodology that does not rely on random assignment. We find that the impact of Chapter 13 protection is similar in our instrumental variables and full samples, though the point estimates are somewhat smaller in our full sample. Third, we are unable to use our instrumental variables strategy to estimate the impact of Chapter 7 bankruptcy protection, which make up approximately 75 percent of all bankruptcy filings. Using our event study design, we find that the effect of Chapter 7 protection is more modest than that of Chapter 13 protection. Filers granted Chapter 7 bankruptcy protection earn $1,639 to $1,936 more than dismissed filers over the first five post-filing years and are 2.7 percentage points more likely to be employed over the same time period. Filers granted Chapter 7 also have five-year foreclosure rates that are 1.7 percentage points lower than dismissed filers, but are neither more nor less likely to be deceased after five years compared to dismissed filers.

The remainder of the paper is structured as follows. Section I provides a brief overview of the consumer bankruptcy system in the United States. Section II describes our data and provides summary statistics. Section III presents a stylized model that motivates our empirical exercise. Section IV describes our empirical strategy. Section V estimates the impact of Chapter 13 bankruptcy protection on labor supply, mortality, and home foreclosure, and Section VI concludes.

I. Consumer Bankruptcy in the U.S.

A. Overview

Bankruptcy is the legal process to resolve unpaid debts. In the United States, individual debtors are allowed to choose between Chapter 7 and Chapter 13 bankruptcy protection.[2]

[2]The most commonly reported cause of bankruptcy is an unexpected income or expense shock. In a hand-collected sample of bankruptcy filers, 67.5 percent of filers report job loss as a precipitating factor, 22.1 percent report family issues such as divorce, and 19.3 report medical expenses (Sullivan, Warren, and Westbrook 2000), with other work suggesting a somewhat larger role for medical expenses (Domowitz and Sartain 1999, Warren, Sullivan, and Jacoby 2000, Himmelstein et al. 2009). Using similar data from PSID, Fay, Hurst, and White (2002) find that households are more likely to file for bankruptcy protection when there are larger financial benefits to doing so, and that there

Under Chapter 7, debtors forfeit all non-exempt assets in exchange for a discharge of eligible debts and protection from future wage garnishment. Nearly all unsecured debts are eligible for discharge under Chapter 7, including credit card debt, installment loans, medical debt, unpaid rent and utility bills, tort judgments, and business debt. Student loans, child support obligations, and debts incurred by fraud cannot be discharged under Chapter 7, and secured debts such as mortgages, home equity loans, and automobile loans can only be discharged if debtors give up the collateral. As most debtors have little non-exempt wealth or can move all non-exempt wealth into exempt assets before filing, the average repayment rate under Chapter 7 is only about one percent (Sullivan et al. 1989). Chapter 7 is chosen by the majority of filers in the United States, with 79.8 percent of debtors in our data filing under Chapter 7.

Chapter 7 cases begin with the debtor filing a bankruptcy petition, a statement of financial affairs, a copy of his or her most recent tax return, executory contracts and unexpired leases, and schedules of current income, expenditures, and assets and liabilities. The assigned bankruptcy trustee then holds a meeting with the debtor to ensure that the debtor is aware of the potential adverse consequences of bankruptcy and to flag any potential issues for the bankruptcy judge. The bankruptcy judge will confirm or dismiss the application using the information from the filing and the trustee's report. If the filing is confirmed, the trustee liquidates the debtor's property and splits it among the creditors. If the filing is dismissed, the debtor must exit the bankruptcy system and may not refile under either chapter for at least six years. In our data, 98.4 percent of Chapter 7 filings ending with a discharge of debt.

Under Chapter 13, filers propose a three- to five-year plan to repay part of their unsecured debt in exchange for a discharge of the remaining unsecured debt, protection from future wage garnishment, and protection of most assets. For example, Chapter 13 allows debtors to retain assets pledged as collateral by including the collateral amount in the repayment plan. Chapter 13 also allows debtors to avoid home foreclosure by including any mortgage arrears in the repayment plan, with the original mortgage reinstated after completion of the plan. Seventy percent of dismissed Chapter 13 filers report that avoiding foreclosure is their principal reason for choosing to file under Chapter 13 (Porter 2011), with 71 percent of filers from a sample of Delaware cases including mortgage arrears in their repayment plans, 41 percent including car loans, and 38 percent including priority debt (White and Zhu 2010).

Chapter 13 cases begin with the debtor filing a repayment plan and the paperwork described above. After a meeting between the debtor and bankruptcy trustee, the bankruptcy judge decides whether the repayment plan is feasible and meets the standards for confirmation set forth in the Bankruptcy Code.[3] If the judge confirms the repayment plan, the debtor makes regular payments to the trustee until the plan is complete. The judge may dismiss or convert the case to Chapter 7 if

is little impact of adverse shocks such as unemployment or divorce of the filing after conditioning on the financial benefits.

[3]There is typically one Chapter 13 bankruptcy trustee that works with all judges in an office. If an office has a particularly high Chapter 13 caseload, judges may have their own Chapter 13 trustee. Chapter 7 trustees are randomly assigned in a process that is independent from the judge assignment.

the debtor fails to make any payments, fails to pay any post-filing domestic support obligations, or fails to make required tax filings during the case. In our data, 48.8 percent of Chapter 13 filings end with a discharge of debt. If a Chapter 13 filing is dismissed, debtors may refile for either Chapter 7 or Chapter 13 after only 180 days. While almost no dismissed Chapter 13 filers in our sample refile under Chapter 13, approximately 20 percent choose to refile under Chapter 7. Thus, we estimate the impact of receiving Chapter 13 protection relative to both no bankruptcy protection and protection via Chapter 7. All of our estimates should be interpreted with this counterfactual in mind.

B. U.S. Bankruptcy Courts

There are 94 Federal bankruptcy courts in the United States, with at least one court in each state, the District of Columbia, and Puerto Rico. Each bankruptcy court hears all cases originating from counties in its jurisdiction. Bankruptcy courts are often further divided into offices that hear all cases originating from a subset of counties in the court's jurisdiction. Appendix Figure 1 displays the 72 courts and 205 offices that we have data for. The median court in our sample is divided into three offices, with little systematic pattern to the number of offices in each office.

Bankruptcy judges are appointed to fourteen-year terms by the court of appeals in their judicial district. Bankruptcy judges only hear cases filed in their court, but often hear cases across multiple offices within their court. Within each bankruptcy office, cases are typically assigned to judges using a random number generator or a blind rotation system.

The assigned bankruptcy judge decides any and all matters connected to a case, including whether or not to dismiss the filing. The most common reason a filing is dismissed is that it constitutes a "substantial abuse" of the bankruptcy process, typically meaning that a debtor should be able to repay his or her debts without bankruptcy protection. Other common reasons for dismissal include a filing missing important information, a Chapter 13 repayment plan being infeasible, or a Chapter 13 repayment plan being too small (Hynes 2004). In Section IV, we discuss how we use systematic differences in the probability that a judge dismisses a filing to estimate the causal impact of bankruptcy protection. These measured differences in judge behavior are likely to be the result of differences in how judges interpret the criteria listed above, implying that more lenient judges may confirm repayment plans that are more generous to debtors or that are less feasible.

Despite the pivotal role of bankruptcy judges, debtors typically have only limited involvement with the assigned judge. Chapter 7 filers do not appear before a judge unless a creditor or trustee raises an objection. Chapter 13 filers appear before the bankruptcy judge at the plan confirmation hearing, but all other administrative aspects of the bankruptcy process are conducted by the bankruptcy trustee, not the bankruptcy judge.

There is considerable variation in the number of bankruptcy judges in each bankruptcy court and office, with courts serving more populous regions tending to have more judges. Appendix Figure 1 displays the number of Chapter 13 bankruptcy judges in each office that we have data for. Of the 205 offices in our sample, 110 have only one Chapter 13 judge, 52 have two Chapter 13 judges, 25 have three Chapter 13 judges, and 18 have four or more Chapter 13 judges.

C. Potential Benefits of Bankruptcy Protection

There are at least two reasons debtors may benefit from bankruptcy protection. First, bankruptcy protection may maintain the incentive to work by protecting future wages from garnishment. Wage garnishments occur when an employer is compelled by a court order to withhold a portion of an employee's earnings to repay a particular debt. Federal law limits the amount that may be garnished in any one week to the lesser of 25 percent of weekly disposable earnings, or the amount by which weekly disposable earnings exceed 30 times the federal minimum wage. Creditors are therefore able to garnish 25 percent of each additional dollar of earnings above 40 times the federal minimum wage, 100 percent of earnings between 30 and 40 times the federal minimum wage, and nothing on earnings below 30 times the federal minimum wage.[4] Bankruptcy protection stops all current garnishment orders and prevents any future garnishment orders on discharged debt, increasing the marginal return to work. Consistent with this mechanism, the U.S. Supreme Court argued in *Local Loan Co.* v. *Hunt* (1934) that eliminating these wage garnishments is "[o]ne of the primary purposes of the Bankruptcy Act," as "[f]rom the viewpoint of the wage earner, there is little difference between not earning at all and earning wholly for a creditor."

Second, bankruptcy protection may prevent economic instability. Creditors have a number of options to collect unpaid debts if a debtor has not filed for bankruptcy protection or after a case is dismissed, including the wage garnishment orders discussed above, collection letters or phone calls, in-person visits at home or work, and the seizing of assets through a court order (Hynes, Dawsey, and Ausubel 2009). Debtors can make these collection efforts more difficult by ignoring collection letters and calls, changing their telephone number, or moving without leaving a forwarding address. Debtors can also leave the formal banking system to hide their assets from seizure, change jobs to force creditors to reinstate a garnishment order, or work less so that their earnings are not subject to garnishment. Thus, bankruptcy protection may prevent economic instability by helping debtors avoid the kinds of sharp drops in consumption that have important long-term consequences, such as losing one's home through foreclosure or becoming sick due to the lack of medical care. Bankruptcy protection may also reduce the incentive to strategically move across state lines or change jobs to avoid creditors.

There are also many reasons to believe that bankruptcy protection will have little impact on debtors. It is possible that financially distressed households have highly inelastic labor supply, or that debt relief will reduce the incentive to work through the income effect. It is also possible that debtors are able to avoid most debt collection efforts at a relatively low cost, or that garnishment amounts are too low to impact labor supply decisions. Finally, it is possible that many bankruptcy filers are in financial distress due to low human capital or poor health that the bankruptcy system is unable to remedy.

[4] Federal law allows garnishments of up to 50 percent of a debtor's disposable earnings for payment related to child support or alimony if the worker is supporting another spouse or child, and up to 60 percent if the worker is not. An additional five percent may be garnished for court order payments more than 12 weeks in arrears.

II. Data

To estimate the impact of bankruptcy protection on debtors, we merge information from individual bankruptcy filings, administrative tax records from the Social Security Administration (SSA), and proprietary real estate records from the data aggregator DataQuick.

Bankruptcy records are available from 1992 to 2009 for the 72 federal bankruptcy courts that allow full electronic access to their dockets. These data represent approximately three quarters of all bankruptcy filings during this period. Each record in our bankruptcy data contains information on the Chapter filed, filing date, court, office, outcome, the judge and trustee assigned to each filing, whether the filing includes any assets, and whether the filing fee was paid immediately or in installments. The data also contain information on each debtor's name, address, and last four digits of each debtor's social security number.

We make five restrictions to our estimation sample. First, we drop 110 offices that only have a single Chapter 13 bankruptcy judge, as there is no variation in judge leniency that allows us to estimate the impact of Chapter 13 protection. Second, we drop all filings originating from counties that assign all cases to a single judge, as these filings are not randomly assigned. Third, we drop office by year bins where a retiring judge's cases were reassigned with no documentation as to the original judge. Fourth, we drop office by year by judge bins with fewer than ten cases. Finally, we restrict the sample to first-time filers between 1992 and 2005 to ensure that we have five or more years of post-filing outcomes for all debtors and that all filings occurred before the 2005 Bankruptcy Reform Act came into effect.

These sample restrictions leave us with 534,980 Chapter 13 filings in 42 offices and 31 bankruptcy courts. These data represent just over 26 percent of the available Chapter 13 filings in the analysis period. The final sample includes 314 office by year observations, 112 office by judge observations, and 758 office-year-judge observations. The number of cases in each office-year-judge bin ranges from 13 to 3,423. The median number of cases in each office-year-judge bin is 1,048. Appendix Table 1 provides additional details on each of the offices in our estimation sample. Appendix Figures 3 and 4 display the distribution of office-year-judge bins and office-year-judge leniency.

To explore the impact of bankruptcy protection on subsequent labor supply and mortality, we matched the bankruptcy records to administrative tax records from the SSA. The SSA data are remarkably complete and include every individual who has ever acquired a SSN, including those who are institutionalized. Illegal immigrants without a valid SSN are not included in these data. Information on earnings and employment comes from annual W-2s. Individuals with no W-2 in any particular year are assumed to have had no earnings in that year. Individuals with zero earnings are included in all regressions throughout the paper. All dollar amounts are in terms of year 2000 dollars.

We measure non-earnings outcomes using data from three sources. Information on annual 401k contributions, job location, and firm characteristics comes from annual W-2s. Information on DI and SSI receipt comes from the Master Beneficiary Record. Information on mortality comes from the Death Master File that is compiled by the SSA, and covers deaths occurring anywhere in the

United States.

We match the bankruptcy data to the SSA records using last name and the last four digits of the filer's social security number. We were able to successfully match 91.6 percent of the bankruptcy records, with nearly all of the unmatched records resulting from a shared name and last four digits of the social security number in the SSA data. The probability of being matched to the SSA data is not significantly related to judge leniency. Our estimation sample consists of the 490,216 filers in the matched dataset.

To explore the impact of bankruptcy protection on home foreclosures, we also matched the bankruptcy records to proprietary real estate data purchased from DataQuick. The DataQuick files include information on the most recent county assessment, pre-foreclosure notices, and all home transactions. The DataQuick files are compiled by county and year, with more complete coverage for more urban areas and more recent years. There are 280,202 Chapter 13 bankruptcy filers matched to the SSA data living in county by year bins covered by the DataQuick records. We matched these filers to the DataQuick records using last name and filing address. We were able to successfully match 48.5 percent of filers to either a past home transaction or a current home assessment. In a random sample of approximately 3,000 Chapter 13 filings between 1999 and 2005, Agarwal et al. (2010) find that 51 percent of Chapter 13 filers are homeowners, suggesting that we are finding most homeowners in our data.[5] Importantly, the probability of being matched to the DataQuick data is not related to judge leniency.

Table 1 presents summary statistics for all first-time filers between 1992 and 2005 and our estimation sample of first-time filers randomly assigned to judges between 1992 and 2005. Consistent with previous research on bankruptcy filers, 98.4 percent of Chapter 7 filers in our data are granted bankruptcy protection, compared to 48.8 percent of Chapter 13 filers. Sixty percent of Chapter 7 filers are male, 74.2 percent are white, and 13.3 percent are black. For Chapter 13, 63.4 percent of filers are male, 55.8 percent are white, and 33.9 percent are black.

The typical bankruptcy filer earns far less than the average American worker. In the five years before filing, 80.6 percent of Chapter 7 filers are employed on average, with average annual earnings of just $21,090. Eighty percent of Chapter 13 filers are employed, earning $22,333 annually in the five years before filing. Over the same five year time period, 4.7 percent of Chapter 7 filers receive DI, and 9.7 percent receive SSI. Just over four percent of Chapter 13 filers receive DI, and 8.4 percent receive SSI.

Column 3 of Table 1 presents summary statistics for filers in our estimation sample. The estimation sample is very similar to the full sample of filers. Forty-four point eight percent of Chapter 13 filers in our sample are granted bankruptcy protection, 4.0 percentage points less than the full sample. Sixty point nine percent of filers in our sample are male, 2.5 percentage points less

[5] Filers are classified as homeowners if they list real property on Schedule A of the bankruptcy form. The Chapter 13 homeownership rate was provided directly by the authors. There is also evidence that the fraction of Chapter 13 filers who are homeowners has increased in recent years. Lawless et al. (2008) find that 69.6 percent of Chapter 13 filers are homeowners in a random sample of filers in 2007, with other, more selected survey samples suggesting homeownership rates of between 70 and 96 percent in the post-2005 period (White and Zhu 2010, Porter 2011).

than the full sample. 401k contributions are also $33 per year higher in the estimation sample, while average firm wages are $3,874 higher. Conversely, filers in our estimation sample are statistically indistinguishable from the full sample with regard to age, ethnic background, employment, earnings, job tenure, the probability of being matched to a home in the DataQuick files, and the probability of receiving DI or SSI.

III. Model

In this section, we motivate our empirical analysis by describing a simple model of how consumer bankruptcy functions as a social insurance mechanism. The model illustrates the trade-off between the consumption smoothing benefits provided by bankruptcy protection with the increased borrowing costs that result from the higher risk of default. We then consider how the estimates of the effect of bankruptcy protection on labor supply can shed light on those benefits and costs.

A. Bankruptcy as Social Insurance

Consider a continuum of risk averse agents who experience an expense shock π distributed $\pi \sim [\underline{\pi}, \overline{\pi}]$. Each agent pays for the expense shock through an existing credit line at interest rate r. We assume that agents borrow exactly π and that there is a single interest rate for all all agents.[6]

Agents who are not eligible for bankruptcy protection repay $(1+r)\pi$ to lenders. Agents who are eligible for bankruptcy protection repay $(1+r)\alpha_b wl$, where $\alpha_b wl < \pi$ for all agents. We assume that there are no other direct or indirect costs of bankruptcy, and that all agents eligible for bankruptcy protection will file for bankruptcy.

Each agent's utility is defined as $U(c) - \psi(l)$, where consumption c equals wage earnings wl minus debt repayment $(1+r)\pi$ or $(1+r)\alpha_b wl$. We assume that U is concave and ψ is convex. We begin with the assumption that l is fixed for all agents to highlight the insurance aspects of bankruptcy protection. In the next section, we allow agents to choose labor supply to show how bankruptcy protection may impact earnings. We assume wages w are fixed throughout.

The lending market is perfectly competitive, leading to no profits in equilibrium. We normalize the shadow cost of bank funds to zero so that the expected cost of lending equals the expected repayment amount:

$$\int_{\underline{\pi}}^{\overline{\pi}} \pi \, d\pi = \int_{\underline{\pi}}^{\pi^*} (1+r)\pi \, d\pi + \int_{\pi^*}^{\overline{\pi}} (1+r)\alpha_b wl \, d\pi \qquad (1)$$

where π^* is the cutoff for bankruptcy eligibility chosen by the social planner. Equation (1) yields the familiar result that a more restrictive bankruptcy system decreases borrowing costs as more

[6] The assumption that agents borrow exactly π simplifies the model without affecting the key results. We further simplify the model by assuming that agents only face expense risk that is unlikely to be covered by existing social insurance mechanisms, such as Unemployment Insurance. Our model is therefore unable to shed light on the interaction of bankruptcy protection and these other forms of social insurance. The derivation of an optimal social insurance system with conceptually distinct types of risk remains an important area for future work. See Jackson (1986) and Posner (1995) for discussion of the relationship between bankruptcy and government safety net programs, and Rea (1984) for discussion of why private consumption insurance does not exist in the United States.

individuals fully repay their debts:

$$\frac{\partial \bar{r}}{\partial \pi^*} = -\frac{(1+r)(\pi^* - \alpha_b wl)}{\int_{\underline{\pi}}^{\pi^*} \pi \, d\pi + \int_{\pi^*}^{\bar{\pi}} \alpha_b wl \, d\pi} < 0 \tag{2}$$

The sensitivity of borrowing costs to the bankruptcy eligibility cutoff is increasing in the difference between what creditors are repaid by the marginal non-filer and marginal filer ($\pi^* - \alpha_b wl$). This is because a larger change in r is needed to compensate creditors for the lost revenue associated with a more lenient bankruptcy cutoff when the difference in the amount repaid is large. Conversely, the sensitivity of borrowing costs to the bankruptcy eligibility cutoff is decreasing in the expected repayment amount $\left(\int_{\underline{\pi}}^{\pi^*} \pi \, d\pi + \int_{\pi^*}^{\bar{\pi}} \alpha_b wl \, d\pi\right)$, as a smaller change in r is needed to compensate creditors for the lost revenue associated with a more lenient bankruptcy cutoff when the total repayment amount is large.

The social planner chooses the bankruptcy eligibility cutoff π^*, where agents are eligible for bankruptcy protection iff $\pi > \pi^*$. Since all agents are identical ex-ante, the social welfare function is the same as the representative consumer's expected utility function:

$$\int_{\underline{\pi}}^{\pi^*} U(wl - (1 + r(\pi^*))\pi) - \psi(l) \, d\pi + \int_{\pi^*}^{\bar{\pi}} U(wl - (1 + r(\pi^*))\alpha_b wl) - \psi(l) \, d\pi \tag{3}$$

The first order condition for equation (3) equates the benefit of bankruptcy protection for the marginal recipient with the marginal cost of providing bankruptcy:

$$\left(U(wl - (1+r)\alpha_b wl) - \psi(l)\right) - \left(U(wl - (1+r)\pi^*) - \psi(l)\right) = -\frac{\partial \bar{r}}{\partial \pi^*}\left(\int_{\underline{\pi}}^{\pi^*} \pi U_c(wl - (1+r)\pi) \, d\pi + \int_{\pi^*}^{\bar{\pi}} \alpha_b wl U_c(wl - (1+r)\alpha_b wl) \, d\pi\right) \tag{4}$$

where U_c indicates the first derivatives with respect to consumption.

Equation (4) highlights the trade-off between the consumption smoothing benefits provided by bankruptcy protection when shocks are large ($\pi > \pi^*$) and the increased costs of self-insurance when shocks are small ($\pi < \pi^*$). This trade-off is at the heart of an important literature measuring the effect of consumer bankruptcy on welfare using quantitative models of the credit market (e.g. Athreya 2002, Li and Sarte 2006, Livshits, MacGee, and Tertilt 2007, Chatterjee and Gordon 2012). An important limitation of this literature has been the lack of empirical evidence on the magnitude of both the benefits provided by bankruptcy protection for the marginal recipient and the costs associated with more expensive borrowing. There is also relatively little information on the mechanisms through which bankruptcy protection benefits debtors, forcing the previous literature to make strong assumptions, such as abstracting away from the production side of the economy (e.g. Athreya 2002, Livshits, MacGee, and Tertilt 2007), assuming that labor supply is only influenced by the aggregate capital stock (e.g. Li and Sarte 2006), or assuming that labor supply is extremely inelastic (e.g. Chatterjee and Gordon 2012).

B. Bankruptcy and Labor Supply

Next, we consider how estimates of the impact of bankruptcy protection on labor supply shed light on the benefits and costs of the consumer bankruptcy system as described by equation (4). In the simple setup described above, bankruptcy protection unambiguously decreases labor supply through both substitution and wealth effects. However, the assumption that agents fully repay their debts outside of the bankruptcy system is unlikely to hold in practice given that the majority of debt write-offs occur outside of the bankruptcy system (Hynes, Dawsey, and Ausubel 2009). We therefore make the more realistic assumption that agents who do not receive bankruptcy protection default on their debt and are subject to wage garnishment at rate α_g. The wage garnishment rate is assumed to be larger than the bankruptcy repayment rate $\alpha_g > \alpha_b$, implying that bankruptcy protection reduces the implicit tax on earnings associated with default. We also assume that the amount repaid through garnishment $(1+r)\alpha_g wl$ is less than the amount borrowed π for all agents.

Each agent's labor supply is described by the following first order condition:

$$w(1-(1+r)\alpha)\cdot U_l(wl(1-(1+r)\alpha)) = \psi_l(l) \tag{5}$$

where U_l and ψ_l indicate first derivatives with respect to labor supply, and α is the implicit tax on earnings associated with either wage garnishment or bankruptcy. Equation (5) yields the familiar result that a reduction in the implicit tax rate α through bankruptcy protection has an ambiguous impact on labor supply due to opposing substitution and wealth effects:

$$\frac{\partial l}{\partial \alpha} \ominus \frac{[(1+r)wU_l]}{[[(1-(1+r)\alpha)w]^2 U_{ll} - \psi_{ll}]} \oplus \frac{[(1+r)(1-(1+r)\alpha)w^2 l U_{la}]}{[[(1-(1+r)\alpha)w]^2 U_{ll} - \psi_{ll}]} \lessgtr 0 \tag{6}$$

Thus, bankruptcy protection increases the earnings of the marginal recipient if the substitution effect associated with the reduced tax on earnings dominates. Conversely, bankruptcy protection decreases the earnings of the marginal recipient if the marginal non-recipient is not subject to wage garnishment or the wealth effect dominates. The effect of bankruptcy protection on labor supply is therefore an empirical question.

Labor supply estimates for the marginal recipient of bankruptcy protection influence the benefits and costs of the consumer bankruptcy system in at least two ways. First, any labor supply response will change the consumption smoothing benefits provided by bankruptcy protection. If bankruptcy protection increases the marginal recipient's earnings, the difference between consumption of the marginal filer and marginal non-filer is increased, as are the consumption smoothing benefits of bankruptcy protection. The reverse holds if bankruptcy has a negative impact on debtor earnings. Second, any labor supply response will change the sensitivity of borrowing costs to bankruptcy policy and the costs associated with bankruptcy protection. If bankruptcy protection increases the marginal recipient's earnings, the difference in the amount repaid to creditors by the marginal filer and marginal non-filer is reduced, decreasing the sensitivity of borrowing costs to bankruptcy policy and lowering the cost of the bankruptcy system. The reverse again holds if bankruptcy has

a negative impact on debtor earnings. Thus, a positive (negative) impact of bankruptcy protection on earnings suggests that the bankruptcy system has larger (smaller) benefits and smaller (larger) costs than implied by models with inelastic labor supply (e.g. Athreya 2002, Livshits, MacGee, and Tertilt 2007, Chatterjee and Gordon 2012).[7]

IV. Research Design

Consider a model that relates post-filing outcomes such as earnings to bankruptcy protection:

$$y_{it} = \alpha + \beta X_i + \gamma Bankruptcy_i + \varepsilon_{it} \quad (7)$$

where i denotes individuals, t is the year of observation, γ is the causal impact of bankruptcy protection, X_i includes controls such as race and gender, and ε_{it} is noise. The problem for inference is that OLS estimates of γ may be biased if bankruptcy protection is correlated with the unobservable determinants of later outcomes: $E[\varepsilon_{it}|Bankruptcy_i] \neq 0$. For example, the most proximate causes of bankruptcy, such as job loss and health shocks, may also impact later outcomes, biasing both cross-sectional (e.g. Han and Li 2007, 2011) and within-individual (e.g. Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2009) estimates of equation (7).

We identify the causal impact of bankruptcy on debtors γ using judge leniency as an instrument for bankruptcy protection. Intuitively, we compare the post-filings outcomes of debtors assigned to bankruptcy judges that have different propensities to grant Chapter 13 protection, interpreting any differences as a causal effect of the change in the probability of receiving bankruptcy protection associated with these propensities.

Formally, we estimate the causal impact of receiving bankruptcy protection through a two-stage least squares regression using judge leniency as an instrumental variable for bankruptcy protection. The second stage estimating equation is:

$$y_{it} = \alpha + \alpha_{ot} + \beta X_i + \gamma Bankruptcy_i + \varepsilon_{it} \quad (8)$$

where α_{ot} are office by month-of-filing fixed effects and X_i includes race, gender, five-year age effects, a five-year average of baseline employment, and a five-year average of baseline earnings. The first stage estimating equation associated with equation (8) is:

$$Bankruptcy_{it} = \alpha + \alpha_{ot} + \beta X_i + \delta \sigma_j + \varepsilon_{it} \quad (9)$$

[7] In a model with endogenous borrowing, the consumer bankruptcy system increases loan demand through lower costs of default and improved risk sharing. Borrowing distortions from the lower costs of default are likely increasing with the benefits of filing for bankruptcy protection. As the benefits of bankruptcy protection are increasing with the magnitude of the labor supply estimates, the distortions to borrowing behavior are also likely to be increasing with the magnitude of the labor supply estimates. Severino, Brown, and Coates (2014) provide more direct evidence on this issue using within-state variation in asset exemptions, finding that increased protection under the Chapter 7 bankruptcy system increases unsecured borrowing without increasing the probability of default. These results are consistent with the consumer bankruptcy system increasing borrowing through improved risk sharing, without significantly increasing the prevalence of adverse selection and moral hazard.

where σ_j is the systematic component of judge behavior and δ represents the impact of that judge behavior on the probability of receiving bankruptcy protection. To account for any serial correlation across filers at the level of randomization, we cluster standard errors at the office level in both the first and second stage regressions. Results are qualitatively similar if we cluster at the office by judge or office by month-of-filing level.

Using an exhaustive set of judge fixed effects as an instrument for bankruptcy protection yields a consistent two-stage least squares estimate of γ as the number of filers $i \to \infty$, but is potentially biased in finite samples. This bias is the result of the mechanical correlation between a filer's own outcomes and the estimation of that filer's judge fixed effects. There are several potential solutions to this own-observation issue. Jackknife IV eliminates the bias by omitting a filer's own observation when forming the instrument (Angrist, Imbens, and Krueger 1999). Split-sample two-stage IV addresses the own-observation issue by randomly splitting the sample into two groups, using judge tendencies in one part of the sample as an instrument for bankruptcy protection in the other part of the sample (Angrist and Krueger 1995). Limited information maximum likelihood (LIML) eliminates the own-observation bias by collapsing the parameter space and using maximum likelihood to obtain a consistent estimate of the effect of bankruptcy protection.

We address the own-observation problem by using a leave-one-out measure of judge leniency as an instrument for bankruptcy protection (Kling 2006, Chang and Schoar 2008, Doyle 2007, 2008, Autor and Houseman 2010, French and Song 2011, Aizer and Doyle 2013, Maestas, Mullen, and Strand 2013). Our measure of judge leniency Z_{icjt} is defined as the leave-one-out fraction of filings granted by judge j in year t minus the leave-one-out fraction granted in his court c in year t:

$$Z_{icjt} = \frac{1}{n_{cjt} - 1}\left(\sum_{k=1}^{n_{cjt}}(B_k) - B_i\right) - \frac{1}{n_{ct} - 1}\left(\sum_{k=1}^{n_{ct}}(B_k) - B_i\right) \quad (10)$$

where i again denotes individuals, c denotes courts, j is the assigned judge, t is the year of observation, B_i is an indicator for receiving bankruptcy protection, n_{cjt} is the number of cases seen by a judge in year t, and n_{ct} is the number of cases seen by a court in year t. This leave-one-out procedure is essentially a reduced-form version of jackknife IV that purges the mechanical correlation between a filer's own outcomes and our measure of judge leniency.[8] Following the literature (Kling 2006, Chang and Schoar 2008, Doyle 2007, 2008, Autor and Houseman 2010, French and Song 2011, Aizer and Doyle 2013, Maestas, Mullen, and Strand 2013), we do not adjust our standard errors to account for the fact that the instrument is estimated.

Consistent with past research (Sullivan, Warren, and Westbrook 1994, Norberg and Compo 2007), we find considerable variation in the treatment of Chapter 13 cases within an office. The standard deviation of Z_{ijct} is 0.030 for Chapter 13 filers in our sample. There is also significant persistence in our measure of judge behavior. Appendix Figure 4 plots current and lagged judge

[8] Appendix Table 2 presents two-stage least squares, LIML, and Jackknife IV results using judge fixed effects as instruments for bankruptcy protection. The results are qualitatively similar to our preferred estimates presented in Table 4.

discharge rates, with each point representing a separate judge by year observation. Discharge rates are highly correlated across time, with an OLS regression relating each judge by year discharge rate to the lagged discharge rate yielding a coefficient of 0.814. These results suggest that we are capturing systematic differences in judge behavior, not random year to year noise.

In contrast to Chapter 13, there is almost no variation in the treatment of Chapter 7 cases across judges within an office. The standard deviation of Z_{ijct} for Chapter 7 filers is only 0.003 in our data, likely because almost all Chapter 7 filings are granted. This lack of variation makes it difficult to measure the effect of Chapter 7 bankruptcy protection using our instrumental variables strategy. In Section VE, we use an event study strategy to estimate the impact of Chapter 7 protection.

Using our reduced form measure of judge leniency Z_{ijct} as an instrument for the receipt of Chapter 13 bankruptcy protection, the identified two-stage least squares parameter from equation (8) measures the local average treatment effect of Chapter 13 for filers whose bankruptcy outcomes are altered by judge assignment. The conditions necessary to interpret these two-stage least squares estimates as the causal impact of Chapter 13 protection are: (1) that judge assignment is associated with bankruptcy protection, (2) that judge assignment only impacts debtor outcomes through the probability of receiving bankruptcy protection, and (3) that the impact of judge assignment on the probability of receiving bankruptcy protection is monotonic across filers.

The first assumption is empirically testable. Figure 1 plots average discharge vs. our leave-one-out measure of judge leniency. The estimation sample includes first-time filers between 1992 and 2005 in the 42 offices in the 31 courts that randomly assign Chapter 13 filings to judges. To construct the plot, we calculate mean residuals from a regression of an indicator for receiving Chapter 13 protection on office by month-of-filing fixed effects. We then add the mean discharge rate to the mean residual in each judge by year bin to aid in the interpretation of the scale. The resulting scatter plot provides a non-parametric representation of the conditional expectation function, but does not show the underlying variance in the individual-level data. The solid line and corresponding coefficient show the best linear fit estimated on the underlying individual-level data, controlling for office by month-of-filing fixed effects and with standard errors clustered at the office level. Table 2 presents analogous individual-level estimates with and without additional controls.

First stage results from Figure 1 and Table 2 show a large and precisely estimated relationship between judge leniency and the probability of receiving bankruptcy protection. With no filer level controls, a one percentage point increase in Z_{ijct} increases the probability that a debtor receives bankruptcy protection by 0.749 percentage points. Controlling for gender, race, age, and baseline earnings, a one percentage point increase in Z_{ijct} increases the probability that a debtor receives bankruptcy protection by 0.731 percentage points. Thus, a one standard deviation (three percentage point) increase in judge leniency increases the likelihood of receiving Chapter 13 bankruptcy protection by about 2.2 percentage points, a 4.9 percent change from the mean. To put these magnitudes in perspective, black filers are 9.0 percentage points less likely to receive bankruptcy protection, a one year younger filer is 0.5 percentage points less likely, and filers with baseline earnings that are $10,000 lower are about 0.05 percentage points less likely.

The probability of receiving Chapter 13 protection does not increase one-for-one with our measure of judge leniency, likely because of measurement error that attenuates the effect toward zero. For instance, judge leniency may drift over the course of the year or fluctuate with case characteristics, reducing the accuracy of our leave-one-out measure. Nevertheless, the results from Figure 1 and Table 2 confirm that judge leniency is highly predictive of case outcomes.

Our second identifying assumption is that judge assignment only impacts debtor outcomes through the probability of receiving bankruptcy protection. This assumption would be violated if judge leniency is correlated with unobservable determinants of future outcomes. Table 3 presents a series of randomization checks to partially assess the validity of this exclusion restriction. Column 2 reports results from an OLS regression of judge leniency on a filer's age, gender, race, an indicator for being matched to a home at baseline, and baseline earnings, employment, self-employment earnings, self-employment, 401k contributions, receipt of Disability Insurance, job tenure, and average employer wages. Each baseline measure is the average over the five years before filing. We control for office by month-of-filing fixed effects, and cluster standard errors at the office level. Job tenure is associated with judge leniency at the ten percent level. None of our other baseline variables are significantly related to judge leniency, and a joint F-test of the hypothesis that all baseline differences are equal to zero has a p-value of 0.233.

Columns 3 adds controls for predicted earnings, employment, and mortality. We predict each outcome over the first five post-filing years using gender, race, five-year age effects, and employment and earnings in the sixth through tenth years before bankruptcy. Job tenure is the only significant measures, and a joint F-test that all the baseline differences listed in column 3 are equal to zero has a p-value of 0.252.[9]

Column 4 presents results from our final test of random assignment. We regress each baseline measure on an exhaustive set of judge fixed effects. Each regression controls for office by month-of-filing fixed effects. We report the p-value from a joint F-test that the judge effects are jointly different than zero, which provides an omnibus test for the null hypothesis that filer covariates do not differ significantly among filers assigned to judges within an office by month-of-filing combination. The joint F-test for age has a p-value of 0.051. None of the other joint F-tests in column 4 suggest that there is systematic non-random assignment of filings to judges.

The exclusion restriction could also be violated if judge leniency impacts future outcomes through channels other than bankruptcy protection. For example, the exclusion restriction would be violated

[9] To put the magnitude of the age and tenure correlations in Table 3 in context, we compare the implied bias in our reduced form earnings results reported in Section VA. Controlling only for office by month fixed effects, an OLS regression of average earnings over the first five post-filing earnings on age suggests that being a year older is associated with earnings that are $176.86 lower. Controlling for all of the covariates listed in Table 3, being a year older is associated with earnings that are $252.70 lower. An additional year of job tenure is associated with earnings that are $1,917.81 higher when controlling only for office by month fixed effects, and $1,265.64 higher when controlling for all other covariates in Table 3. Thus, a one standard deviation (three percentage point) increase in judge leniency is associated with a -$0.000037 to $0.000037 change in earnings due to the differences in age observed in Table 3, and a $0.002885 to $0.004430 change in earnings due to the differences in job tenure observed in Table 3. In contrast, our reduced form results plotted in Figure 3 show that a one percentage point increase in judge leniency is associated with a $128.55 increase in post-filing earnings. Results are similar for employment, mortality, and home foreclosure.

if more lenient judges are also more likely to provide financial counseling to debtors, and that financial counseling has an independent impact on future outcomes. If judge leniency impacts future outcomes through any other channels, then the resulting local average treatment effect would incorporate any additional impacts associated with judge assignment. The assumption that judges only systematically affect debtor outcomes through bankruptcy is fundamentally untestable, and our estimates should be interpreted with this potential caveat in mind. However, we argue that this exclusion assumption is reasonable in our setting. Recall that bankruptcy judges typically interact with debtors only at the confirmation hearing, while the separately assigned court trustee handles all pre- and post-filing issues. Thus, it seems unlikely that judges would confer significant benefits to debtors other than through their ruling on the bankruptcy filing.

Our third identifying assumption is that there is a monotonic impact of judge assignment on the probability of receiving bankruptcy protection. The monotonicity assumption implies that being assigned to a more (less) lenient judge does not result in a decrease (increase) in the likelihood of receiving bankruptcy protection. This monotonicity assumption would be violated if judges differ in the types of filings they treat more leniently. For example, the monotonicity assumption would be invalid if some judges treat women more leniently, while other judges do not treat women more leniently. If the monotonicity assumption is violated, our estimates from equation (8) would still be a weighted average of marginal treatment effects, but the weights would not sum to one (Angrist, Imbens, and Rubin 1996, Heckman and Vytlacil 2005). The monotonicity assumption is therefore necessary to interpret our estimates as a well defined local average treatment effect. The bias away from this local average treatment effect is an increasing function of the number of individuals for whom the monotonicity assumption does not hold and the difference in the marginal treatment effects for those individuals for whom the monotonicity assumption does and does not hold. The amount of bias is also a decreasing function of the first stage relationship described by equation (9) (Angrist, Imbens, and Rubin 1996).[10]

To partially test the monotonicity assumption, Appendix Figure 5 plots judge leniency measures that are calculated separately for each judge by gender, race, baseline income, age, and home ownership. We also report the coefficient and standard error from an OLS regression relating each measure of judge leniency. Consistent with our monotonicity assumption, judges exhibit remarkably similar tendencies across observably different filers. Regressing the judge leniency for male filers on those for female filers yields a point estimate of 0.838. For white and non-white filers, the point estimate is 1.005, for high and low baseline earnings the coefficient is 0.998, for filers older and younger than 40 the coefficient is 1.104, and for those matched to a home and those not matched to a home the coefficient is 0.889. None of the results suggest that the monotonicity assumption is invalid in our setting.

[10] Small and Tan (2007), Klein (2010), de Chaisemartin (2014), and Huber and Mellace (2011) establish conditions under which it is possible to interpret the instrumental variables estimates as a LATE for a different population when the monotonicity assumption is violated.

V. The Impact of Chapter 13 Bankruptcy Protection on Labor Supply, Mortality, and Home Foreclosure

A. Labor Supply

As a benchmark for evaluating the causal effects described below, we begin with a descriptive analysis of granted and dismissed filers. Figure 2 plots average pre- and post-filing earnings and employment for our estimation sample. Earnings include both wage and self-employment earnings including zeros.[11] To aid in the interpretation of the results, we plot expected outcomes calculated using a regression of the outcome five years before filing on controls for gender, race, a quadratic in age, a quadratic in tenure, industry fixed effects, and earnings in the previous five years. We then predict the outcome for the four years before filing and the five years after filing using age and tenure coefficients.

Filers granted Chapter 13 bankruptcy protection earn $5,000 to $5,500 more than dismissed filers in the years leading up to filing. Earnings for both groups fall two to three years before filing, with a larger dip for dismissed filers.[12] The post-filing earnings of dismissed filers dip further, falling about $4,000 below the expected trajectory five years after filing. In contrast to the large and permanent decline in earnings experienced by dismissed filers, individuals granted bankruptcy protection appear to have no long-term earnings losses. These descriptive trends suggest that any causal impacts of bankruptcy protection are likely to be driven by the deterioration of outcomes among dismissed filers, as opposed to gains by granted filers. This interpretation suggests that bankruptcy protection mitigates the long-term consequences of financial shocks that might otherwise harm debtors, but does not confer any benefits in the absence of a financial shock.

Reduced form estimates of the impact of judge leniency on average earnings and employment over the first five post-filing years are presented in Figure 3. Following our first stage results from Figure 1, we construct each plot by regressing each outcome on office by month-of-filing fixed effects, calculating residuals, and then adding the mean outcome to each residual to facilitate interpretation of the scale. The solid line and corresponding coefficient show the best linear fit estimated on the underlying micro data controlling for office by month-of-filing fixed effects, with standard errors clustered at the office level.

Figure 3 shows that being assigned to a more lenient judge significantly increases post-filing earnings and employment. A one standard deviation (three percentage point) increase in judge leniency increases post-filing earnings by $128.55 relative to the baseline mean of $22,115. Employ-

[11] Appendix Table 4 displays the same information in tabular form. Appendix Figure 6 plots mean and median earnings with and without zeros included. Median earnings are lower than mean earnings for both granted and dismissed filers, but the same qualitative trends exist before and after filing as in Figure 2. Mean and median earnings without zeros included also follow the same trend as Figure 2. Two-stage least squares estimates that condition on non-zero earnings are somewhat larger than those reported here (See Appendix Table 5).

[12] The fall in pre-filing earnings is likely related to the "Ashenfelter dip" – the drop in earnings among participants in job training programs – discussed by Ashenfelter (1978), Ashenfelter and Card (1985), and Heckman and Hotz (1989). In our context, the negative earnings shocks before bankruptcy filing may result in OLS estimates with a non-filing control group to overstate the true gains of bankruptcy if there is mean reversion in earnings, and to understate the impact of bankruptcy if shocks have consequences that increase over time.

ment over the same time period increases by 0.2 percentage points relative to the baseline mean of 81.3 percent.

Figure 4 presents corresponding two-stage least squares results measuring the causal impact of Chapter 13 bankruptcy protection on earnings and employment. We use judge leniency Z_{ijct} as an instrumental variable for bankruptcy protection, and control for gender, race, five-year age effects, a five-year average of baseline employment, a five-year average of baseline earnings, and office by month-of-filing fixed effects. For treatment effects in the baseline period, we control for gender, race, five-year age effects, office by month-of-filing fixed effects, and average employment and earnings in the years prior to the baseline year under consideration. Standard errors are clustered at the office level throughout. We estimate the impact of bankruptcy protection for each year separately. Table 4 presents estimates pooling outcomes across the first five post-filing years.

There is a large and precisely estimated impact of Chapter 13 bankruptcy protection on post-filing earnings and employment. In the first full year after filing, the marginal recipient of Chapter 13 protection earns $6,228 more than the marginal dismissed filer. The impact of Chapter 13 protection remains both economically and statistically significant for the first five post-filing years. Pooling outcomes across those five years, the marginal recipient of Chapter 13 earns $5,562 more than the marginal dismissed filer, a 25.1 percent increase from the baseline mean. The earnings estimates reported in Table 4 are similar to those suggested by the descriptive trends in Figure 2, and formal event study estimates presented in Section VE. Conversely, our two-stage least squares estimates are considerably larger than cross-sectional (e.g. Han and Li 2007, 2011) and within-individual estimates (e.g. Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2009) that do not use dismissed filers as a comparison group.

Over the first five post-filing years, Chapter 13 also increases employment by 6.8 percentage points, a 8.3 percent increase from the baseline mean. This point estimate suggests that approximately $(\$22,115 \cdot 6.8) = \$1,503$, or 27 percent, of the earnings estimate can be explained by a decline in labor force attachment.

Our two-stage least squares estimates can only be interpreted as the causal effect of bankruptcy protection if judge leniency is not systematically related to unobserved filer characteristics. One way to evaluate the validity of this assumption is to a estimate the impact of bankruptcy protection in the years before a debtor files. Consistent with our identifying assumptions discussed above, there is no systematic relationship between bankruptcy protection and either earnings or employment in the pre-filing years, with the estimated coefficients being economically and statistically insignificant.

Appendix Table 2 presents estimates using a variety of specifications and instruments to assess the robustness of our main results. Column 2 replicates our preferred estimates from Table 4 using leave-one-out judge leniency as an instrument for Chapter 13 protection. Column 3 uses a leave-month-out measure of judge leniency – where we calculate judge leniency only using cases in the 11 other months of the filing year – as an instrument for Chapter 13 protection. Measuring judge leniency only using cases from the other 11 months purges any remaining correlation between a filer's outcomes and our instrument introduced by the estimation of the office by month-of-filing

fixed effects in our first and second stage regressions. Column 4 uses a leave-one-out measure of judge leniency measured over the first 90 post-filing days as an instrument. Column 5 uses a randomly selected subset of 50 percent of filers to calculate a leave-month-out measure of judge leniency that is used as an instrument in the mutually exclusive subset of filers. Columns 6 through 8 present results that use judge fixed effects as instruments for bankruptcy protection estimated using two-stage least squares, LIML, and jackknife IV, respectively. All regressions control for gender, race, five-year age effects, a five-year average of baseline employment, a five-year average of baseline earnings, and office by month-of-filing fixed effects, with standard errors clustered at the office level.

Our two-stage least squares estimates are robust to the choice of instrument and estimation method. The impact of Chapter 13 protection on post-filing earnings is $5,602 in the leave-month-out specification, $9,126 in the 90-day judge leniency specification, $4,628 in the split sample specification, $4,445 in the two-stage least squares judge fixed effects specification, $4,263 in the LIML specification, and $4,463 in the jackknife IV specification. Due to the relatively large standard errors when using judge fixed effects as instruments, we cannot reject the hypothesis that the estimated treatment effects are identical to our preferred estimate of $5,562 for all of our estimates except the 90-day judge leniency specification. The results follow a similar pattern for employment, while the mortality and foreclosure results are larger when using judge fixed effects. None of the point estimates for employment, mortality, and foreclosure are statistically distinguishable from our preferred estimates in column 2 of Table 4.

Appendix Table 6 presents two-stage least squares results using our preferred specification for the sixth through tenth post-filing years. We restrict the sample to filings originating between 1992 and 2000. Filings originating after 2000 are excluded because we do not observe post-filing outcomes for all ten years for these individuals. In this sample of older filings, Chapter 13 protection increases the marginal recipient's annual earnings by $6,772 in the sixth through tenth post-filing years. These results suggest that the impact of bankruptcy protection is persistent after the completion of the repayment plan. The probability of being employed is also 6.6 percentage points higher in the sixth through tenth post-filing years.

Table 5 presents two-stage least squares results from our preferred specification separately by filer gender, race, age, baseline earnings, and being matched to a home. The effects of Chapter 13 protection on annual earnings are somewhat larger for female and non-white filers, though neither difference is statistically significant. There are significantly larger impacts of Chapter 13 protection on younger filers, likely because older filers in our sample have already left the labor market. Specifically, Chapter 13 increases the annual earnings of filers who are 25 to 40 years old by $7,833 and the annual earnings of filers who are 40 and 60 years old by $6,299, while having no discernible effect on filers who are 60 or older at the time of filing. Chapter 13 protection also has a larger impact on filers with above median earnings. Chapter 13 increases annual earnings by $8,650 for filers with above median earnings, compared to an increase of $1,691 for filers with below median earnings. The impact of Chapter 13 protection on employment is also 6.0 percentage points higher for filers with above median baseline earnings. This may be the result of higher earning filers facing

a higher marginal rate of garnishment, or non-linearities in the elasticity of labor supply. In Section VD, we show that the garnishment channel is at least partially responsible for the pattern of results observed in Table 5.

Panel D of Table 4 presents results for additional labor supply outcomes available in the SSA data. We estimate the impact of Chapter 13 protection on the average of each outcome over the first five post-filing years. Among eligible filers, bankruptcy protection decreases the receipt of SSI by 12.8 percentage points, though the point estimate is only significant at the ten percent level. Bankruptcy protection also appears to have little impact on the receipt of DI or on annual 401k contributions.

B. Mortality

The reduced form estimate of judge leniency on five-year mortality is presented in Figure 3, and the corresponding two-stage least squares estimates are presented in Figure 4. The dependent variable for each regression is an indicator for being deceased in or before the specified year. Trends in the raw data are presented in Figure 2.

In the first full year after filing, only 0.3 percent of filers granted Chapter 13 protection are deceased, compared to 1.15 percent of dismissed filers. Five years after filing, 2.1 percent of filers granted Chapter 13 protection are deceased, compared to 4.0 percent of dismissed filers. These statistics suggest that dismissed bankruptcy filers have extremely high mortality rates. The annual mortality rate for individuals age 25-70 in the United States is about 0.45 percent, one-third to two-fifths the mortality risk of dismissed filers. The annual mortality rate for a national population with a similar age profile as our estimation sample is 0.3 percent, one-quarter to one-third the mortality risk of dismissed filers (CDC 2014).[13] The high mortality rates among dismissed filers are consistent with the high rates of medical debt (Domowitz and Sartain 1999, Sullivan, Warren, and Westbrook 2000, Warren, Sullivan, and Jacoby 2000, Himmelstein et al. 2009) and high levels of self-reported stress (Porter 2011) among bankruptcy filers. The mortality rates among dismissed filers are also consistent with the high mortality rates reported among displaced workers. Sullivan and von Watcher (2009) find average annual mortality rates of 0.52 percent among displaced workers age 28-57, nearly 30 percent higher than the national mortality rate for similarly aged individuals during the same time period.

Turning to our causal estimates, the reduced form results from Figure 4 show that being assigned to a more lenient judge decreases five-year mortality. A one standard deviation increase in judge leniency decreases five-year mortality by 0.0003 percentage points relative to the dismissed filer mean of 4.0 percentage points. Note that we use the dismissed filer mean as a reference point

[13] The CDC reports the annual population, number of deaths, and mortality rates for five year age groups. We calculate the annual mortality rate in 2011 for the 25 to 39 age group by dividing the number of deaths by the total population of people in the combined 25 to 29, 30 to 34, and 35 to 39 age groups. A similar approach is used to calculate the mortality rates for the 40 to 59 and 60 to 70 age groups, using the appropriate age groups from the CDC. We adjust for the age profile of our sample by calculating a weighted average of mortality rates where weights are proportionate to the size of the each age cohort in our sample.

because the deceased cannot file for bankruptcy protection, making the baseline mortality rate zero by construction.

In the two-stage least squares results, Chapter 13 bankruptcy protection lowers five-year mortality by a statistically significant 1.2 percentage points, a 30.0 percent decrease from the control mean. Aggregating the mortality effects across the first five post-filing years, our results imply that the marginal recipient of Chapter 13 protection is alive for 0.053 more years than the marginal non-recipient. In a sample of debtors filing between 1992 and 2000, Chapter 13 decreases ten-year mortality by 1.5 percentage points, though the estimate is not statistically significant. We wish to emphasize that due to the relative imprecision of our mortality estimates, the 95 percent confidence intervals include both very small and implausibly large point estimates.

Panel B of Table 5 reports two-stage least squares results interacted by filer gender, race, age, baseline earnings, and being matched to a home. The effect of Chapter 13 protection on mortality is larger for filers who are white, have above median baseline earnings, and are not matched to a home at baseline. However, the most striking pattern is by age. Chapter 13 protection decreases five-year mortality by 10.9 percentage points for filers 60 and older at the time of filing, despite having little to no impact on earnings for these filers. In contrast, Chapter 13 decreases five-year mortality by only 2.2 percentage points for filers between 25 and 40, and 1.7 percentage points for filers between 40 and 60.

C. Home Foreclosure

The reduced form estimate of judge leniency on five-year foreclosure is presented in Figure 3, and the corresponding two-stage least squares estimates are presented in Figure 4. The sample includes filings in our estimation sample in county by year bins covered by the DataQuick records. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to a REO or a guarantor on or before the indicated year. Note that we are unable to estimate the impact of bankruptcy protection on pre-filing foreclosure as we only have address information for the year of filing, and were unable to match pre-filing transactions to filers. Trends in the raw data are presented in Figure 2.

Chapter 13 bankruptcy protection significantly lowers the probability of home foreclosure. In the raw data plotted in Figure 2, five-year foreclosure rates are less than 2.5 percent for filers receiving bankruptcy protection, compared to 14.9 percent for dismissed filers. This implies that approximately one-third of dismissed filers matched to a home experience a home foreclosure within the first five post-filing years. Note that we use the dismissed filer mean for reference, as we only observe a filer's address at the time of filing. Because the vast majority of foreclosures result in a change of address, we are largely unable to measure the probability of foreclosure before filing.

Our reduced form results from Figure 3 show that being assigned to a more lenient judge has an economically and statistically significant effect on post-filing foreclosure. A one standard deviation increase in judge leniency decreases five-year foreclosure by 0.00459 percentage points relative to the dismissed filer mean of 14.9 percentage points. In the two-stage least squares estimates, Chapter 13

protection decreases foreclosure by 11.9 percentage points in the first full post-filing year and 16.7 percentage points in the second post-filing year. Foreclosure rates are 19.1 percentage points lower five years after filing, a 127.5 percent decrease from the dismissed filer mean. Conditional on being matched to a home, Chapter 13 protection decreases five-year home foreclosure by 35.3 percentage points.

In Appendix Table 5, we show that bankruptcy protection also significantly decreases voluntary and short home sales. Distress sales, which include both foreclosures and short sales, are 22.2 percentage points lower after five years. Home sales, which include all types of housing transactions, are 33.9 percentage points lower after five years.

Panel C of Table 5 reports subsample results. The effect of Chapter 13 protection on foreclosure is larger for filers with low baseline earnings and filers who are 60 or older at the time of filing. These results may be due to these filers being more at risk of foreclosure or having fewer alternatives if a bankruptcy filing is dismissed. However, the precise mechanisms for this pattern of results are unclear.

D. Discussion and Potential Channels

Why are there such large benefits of receiving bankruptcy protection? In this section, we first explore whether protection from wage garnishment and reduced economic instability can explain our labor supply results. We then conclude by considering potential explanations for our mortality result.

One explanation for our results is that Chapter 13 maintains the incentive to work by protecting future earnings from wage garnishment. In this scenario, dismissed filers reduce their labor supply due to the tax on earnings imposed by wage garnishment. Table 6 partially tests this hypothesis by estimating the impact of Chapter 13 protection interacted with proxies of garnishment risk. Columns 1 and 2 of Table 6 present results for filers in the four states that prohibit wage garnishment – Florida, Pennsylvania, South Carolina, and Texas – and filers in states that allow at least some wage garnishment. The impact of Chapter 13 on annual earnings over the first five post-filing years is $5,635 in states that allow garnishment, compared to $3,193 in the four states that prohibit garnishment. The pattern of results is reversed for employment, however, with larger impacts in the four states that prohibit garnishment, though the difference is not statistically significant.

Columns 3 through 7 exploit both within- and across-state variation in garnishment laws to estimate results for filers who are likely to be subject to different marginal garnishment rates. Within each state, wage garnishments are a non-linear function of earnings. For example, in states that follow the federal guidelines, creditors are allowed to garnish each additional dollar of disposable earnings between 30 and 40 times the minimum wage, but only 25 cents of every additional dollar after that point. There is also across-state variation in garnishment rates. Twelve states have lower marginal garnishment rates, ten have lower caps on the total amount that can be garnished, and four states prohibit wage garnishment altogether. In each of these states, the more stringent state regulation takes precedence over the federal guidelines.

We exploit this variation in garnishment laws by estimating the impact of bankruptcy interacted with the marginal garnishment rate a filer is likely to face under both state and federal law.[14] Our estimating equation is:

$$y_{it} = \alpha + \alpha_{ot} + \beta X_i + \gamma_0^0 B_i G_0^0 + \gamma_{25}^{25} B_i G_{25}^{25} + \gamma_{100}^{100} B_i G_{100}^{100} + \gamma_0^{25} B_i G_0^{25} + \gamma_0^{100} B_i G_0^{100} + \varepsilon_{it} \quad (11)$$

where G_x^y is an indicator for a filer facing a marginal garnishment rate of x in his or her state of filing and a marginal garnishment of y under federal law. We instrument for each $B_i G_x^y$ using the interaction between judge leniency and the garnishment bin $Z_{ijct} G_x^y$. We estimate the impact of bankruptcy separately for filers facing different state and federal garnishment rates to control for any differential effects of bankruptcy correlated with baseline earnings that are unrelated to the marginal garnishment rate. For example, γ_0^{25} measures the impact of Chapter 13 protection on filers with higher earnings who are exempt from garnishment in their state of filing, but who would have been subject to 25 percent garnishment under federal law, while γ_{25}^{25} measures the impact of Chapter 13 on filers with higher earnings in states that do not exempt them from garnishment. Thus, γ_0^{25} measures the effect of bankruptcy without garnishment protections for higher-earning filers, while γ_{25}^{25} measures the effect of Chapter 13 with garnishment protections for higher-earning filers.

The effect of Chapter 13 protection is small and imprecisely estimated for filers unlikely to face wage garnishment. The impact on annual earnings is negative $471 for filers subject to no garnishment under both state and federal law, $5,607 for filers who would have been subject to marginal garnishment of 25 percent had they lived in another state, and negative $1,759 for filers who would have been subject to marginal garnishment of 100 percent had they lived in another state. None of the point estimates are statistically significant at conventional levels.

In contrast, there is a large and precisely estimated impact of Chapter 13 on filers subject to the 25 percent marginal garnishment rate in their state of filing. The impact on annual earnings is $7,892 for filers subject to marginal garnishment of 25 percent under both state and federal law. Due to the imprecision of the point estimates, the estimate is not statistically different than that for filers subject to no garnishment under state law but 25 percent under federal law (p-value = 0.572). For filers subject to the 100 percent marginal garnishment rate, the impact is a statistically insignificant $7,435. The difference between this estimate and the estimate for filers subject to no garnishment under state law but 100 percent under federal law is marginally significant (p-value = 0.10). However, if we take these estimates from columns 4 and 6 at face value, the implied elasticity of earnings with respect to garnishment is about 0.94 for the 25 percent bracket.[15] All of

[14] We estimate disposable earnings using pre-tax earnings in the five most recent pre-filing years and the tax rate implied by the NBER TAXSIM federal and state income tax calculator. We assume that all filers are the only earner in their households, as we do not observe marital status in the bankruptcy or SSA data.

[15] The earnings elasticity with respect to garnishment is equal to the log change in taxable earnings divided by the log change in the net tax rate. We assume that the state and federal earnings tax rate is 20 percent, implying that the net tax and garnishment rate is $1 - (1 - 0.2)(1 - 0.25) = 40$ percent. Finally, we use the impact of bankruptcy on filers facing no garnishment in their state of filing who would have faced a 25 percent garnishment under federal law as a counterfactual impact. These assumptions imply an elasticity of earnings with respect to garnishment of

the results from Table 6 are broadly consistent with bankruptcy protection increasing the incentive to work by lowering the effective marginal tax rate on earnings. Nevertheless, it is unlikely that a lower marginal tax rate explains all of our results. Previous studies suggest that the intensive marginal elasticity of earnings with respect to the net-of-tax rate is approximately 0.25 (e.g. Chetty 2012), implying that a 25 percent marginal tax cut should increase earnings by about 6.25 percent, 24.9 percent of our earnings estimate from Table 4. Our implied elasticity measure is likely to be positively biased by other laws and regulations correlated with wage garnishment policies, such as restrictions on third-party debt collectors. We also note that the standard errors in Table 6 are large enough that the implied elasticity with respect to earnings includes a wide range of estimates.

A second explanation for the estimated effects is that bankruptcy protection maintains economic stability. Bankruptcy protection discharges most debts, allows debtors to repay mortgage arrears, and puts a hold on almost all debt collection efforts. These features of the bankruptcy code may maintain economic stability by allowing debtors to avoid eviction or home foreclosure, reducing the incentive to strategically move across state lines or change jobs to avoid creditors, or preventing sharp drops in consumption that have important long-term consequences. Consistent with this explanation, our earnings result is of similar magnitude (in the opposite direction) to the estimated effect of job loss (e.g. von Wachter, Song, and Machester 2009), suggesting that bankruptcy protection may be able to offset the adverse consequences of a financial shock such as job loss. This interpretation is also consistent with our results from Figure 2, which show a significant deterioration of outcomes among dismissed filers, but few gains for filers granted bankruptcy protection.

We present further evidence on this issue by examining filer mobility as proxied by firm EIN. Table 7 presents estimates of the impact of Chapter 13 protection on the probability of working in the same baseline job, industry, county, and state. We also present results for job tenure and average firm wages. The sample is restricted to filers with at least one year of employment in both the pre- and post-filing period.

Bankruptcy protection increases the probability of working in the same (2-digit NAICS) industry by 23.8 percentage points, a 42.7 percent change from the baseline mean of 55.7 percent, and increases the probability that a filer stays at his or her baseline job by 24.6 percentage points, a 49.0 percent change from the baseline mean of 50.2 percent. There is also a large impact of bankruptcy protection on geographic mobility, with bankruptcy protection increasing the probability that a filer works in his or her baseline county by 22.9 percentage points, a 39.7 percent increase, and his or her baseline state by 15.3 percentage points, a 24.4 percent increase. Job tenure and average firm wages are also higher, suggesting that this increase in economic stability is beneficial for the worker. In Appendix Table 7, we find some evidence that the mobility estimates are larger in states that allow garnishment, though not all of the differences are statistically significant. The estimated impacts also tend to increase with the marginal garnishment bracket, though the differences are not statistically significant and the estimated effects remain relatively large in the no garnishment bracket.

$\frac{log(0.248)-log(0.189)}{log(1-0.2)-log(1-0.4)} = 0.94$ for the 25 percent bracket. Note that we are unable to calculate the elasticity for the 100 percent bracket, as $log(0)$ is undefined.

Taken together with the foreclosure results, we interpret this pattern of results as suggesting that Chapter 13 protection maintains economic stability both by reducing forced moves from evictions and foreclosures and unforced, strategic moves to avoid creditors.

We conclude this section by considering potential explanations for our mortality results. In a sample of Pennsylvania workers, Sullivan and von Wachter (2009) find that job displacement increases five-year mortality by 1.2 percentage points. Thus, one potential interpretation of our estimates is that bankruptcy protection can offset much of the increased mortality risk from financial distress caused by events such as job loss. To partially test the hypothesis that the change in mortality that we estimate is driven by a change in earnings, we follow Sullivan and von Wachter (2009) and compare our two-stage least squares results for five-year mortality to the effect implied by the cross-sectional correlation between five-year mortality and both earnings and employment in our estimation sample. Our two-stage least squares results suggest that bankruptcy protection increases annual earnings by $5,562 and employment by 6.8 percentage points. In a regression with post-filing earnings, employment, and no additional controls, the estimated correlation between five-year mortality and average annual earnings (in $1,000s) is -0.00019, and the correlation between five-year mortality and average employment is -0.00996. Taken together, these results suggest that the change in labor supply associated with Chapter 13 protection can explain a $(0.068 \cdot 0.00996 + 5.562 \cdot 0.00019) \cdot 100 = 0.17$ percentage point decrease in five-year mortality, or about 14.1 percent of our estimated treatment effect of 1.2 percentage points. This result is in stark contrast to Sullivan and von Wachter (2009), who find that approximately 50 to 75 percent of the impact of job displacement on mortality is driven by the reduction in long-term earnings.

Further evidence that the change in earnings associated with bankruptcy protection plays little to no role in explaining the mortality results comes from our subsample results reported in Panel B of Table 5. Recall that Chapter 13 protection decreases five-year mortality by 10.9 percentage points for filers 60 and older at the time of filing, despite having little to no impact on earnings for these filers. In contrast, Chapter 13 decreases five-year mortality by only 2.2 percentage points for filers between 25 and 40, and 1.7 percentage points for filers between 40 and 60.

There are several potentially relevant channels for our mortality result that we cannot examine with our current data. For example, bankruptcy protection may decrease an individual's stress by reducing contact with creditors and allowing greater control over his or her financial future. Consistent with this idea, 84 percent of debtors report being under extreme stress before filing for bankruptcy, while only 35 percent report being under extreme stress after filing for bankruptcy (Porter 2011). Dismissed filers may also lose their health insurance or change family environments in a way that impacts health. Unfortunately, it is not possible to link information on morbidity, health insurance, or family status to our data. The precise mechanisms for our estimated mortality effect therefore remain unclear, and likely include a combination of these factors.

E. External Validity

In this section, we discuss the external validity of our results for Chapter 13 filers in our estimation sample, Chapter 13 filers in our full sample, and Chapter 7 filers.

One potential caveat of our results is that we estimate the effects of Chapter 13 protection for the marginal recipient of bankruptcy protection. Recall that our instrumental variable identification strategy identifies the impact of Chapter 13 for filers whose bankruptcy decision is altered by judge assignment due to disagreement on whether or not they should receive bankruptcy protection. This local average treatment effect may or may not reflect the average treatment effect of bankruptcy for all filers. To investigate heterogeneous treatment effects across unobservable debtor characteristics, we estimate MTEs (Heckman and Vytlacil 2005). In our setting, the MTE estimates illustrate how the outcomes for debtors on the margin of bankruptcy protection change as we move from more strict to more lenient judges. Thus, the MTE estimates shed light on the types of filers who benefit most from bankruptcy protection, and whether our local average treatment effects are likely to apply to filers who are further from the margin.

To calculate the MTE function, we follow Doyle (2007) and predict the probability of bankruptcy protection using a probit model with judge leniency Z_{ijct} as the only explanatory variable. We then predict the relationship between each outcome and the predicted probability of receiving bankruptcy protection using a local quadratic estimator with a bandwidth of 0.069.[16] The first derivative of this relationship is then evaluated at each percentile of the predicted probability of receiving bankruptcy protection using the coefficients from the local quadratic regression. We calculate standard errors using the standard deviation of MTE estimates from a bootstrap procedure with 250 iterations adjusted for clustering at the office by filing month level.

Appendix Figure 11 reports the MTE of Chapter 13 protection for earnings, employment, five-year mortality, and five-year home foreclosure. The MTE function for earnings is increasing in the predicted probability of bankruptcy protection, while the MTE function for mortality and home foreclosure are decreasing in the predicted probability of bankruptcy protection. The upward slope in the earnings MTE and downward slope in the mortality and home foreclosure MTEs suggest that filers on the margin of bankruptcy who are assigned to the most lenient judges experience the largest gains when granted bankruptcy protection. These are likely filers with unobservable characteristics that make them the least likely to be granted bankruptcy in the first place, as the margin for relatively lenient judges should entail relatively less deserving filers. This interpretation of the estimates suggests that the impact of Chapter 13 on earnings is modestly larger for less deserving debtors. Conversely, the MTE function for employment is flat, suggesting that the employment effects do not differ systematically across unobservable characteristics.[17]

[16] Following Doyle (2007), the pilot bandwidth was chosen by minimizing the sum of squared errors between the local quadratic estimator and a fourth-degree polynomial model for each outcome. Larger bandwidths lead to more linear relationships and flatter MTE estimates. We chose the minimum bandwidth across all outcomes to explore variations from linearity. The local quadratic estimator was chosen as it is thought to have better properties when evaluating first derivatives (Fan and Gijbels 1996). Results are nearly identical for higher and lower polynomials and both larger and smaller bandwidths.

[17] Appendix Figure 12 reports the MTE of Chapter 13 protection for 401k contributions, DI, and SSI, and Appendix

A second potential caveat is we are only able to use instrumental variables design in bankruptcy offices that randomly assign filings to judges. Recall from Table 1 that our estimation sample is broadly similar to the full sample of Chapter 13 filings. Yet, it is possible that the two groups of filers differ in some unobservable way. We provide estimates of the impact of Chapter 13 protection in our full sample using an event study methodology that compares the outcomes of granted and dismissed filers. Our event strategy design is in the spirit of Bound's (1989) analysis of accepted and rejected DI applicants, and more recent work estimating the effects of job loss on subsequent outcomes (Jacobson, Lalonde, and Sullivan 1993, von Wachter, Song, and Manchester 2009, Sullivan and von Wachter 2009).

Intuitively, our event study design uses granted filers as a treatment group and dismissed filers as a control group. The key identifying assumption is that dismissed filers would have experienced the same outcomes as granted filers had they been granted bankruptcy protection. However, recall from Figure 3 that dismissed filers start with lower earnings and experience larger earnings shocks before filing than granted filers. To account for the difference in pre-filing earnings between granted and dismissed filers, we include individual fixed effects that control for time-invariant differences across filers. To account for different earnings trends, we also include fixed effects for the decile of predicted propensity score separate by event time. We estimate the propensity score using an OLS regression of an indicator for bankruptcy protection on gender, race, five-year age bins, five-years of baseline employment, five-years of baseline earnings, and office by month fixed effects. We then group filers into deciles of the predicted probability of receiving bankruptcy protection. Thus, our event strategy design compares the outcomes of granted and dismissed filers with similar predicted probabilities of being granted bankruptcy protection.

Formally, we estimate various specifications of the following model:

$$y_{it} = \alpha_i + \alpha_t + \sum_{k=-5}^{5} \beta_k Pscore_i^k + \sum_{k=-5}^{5} \gamma_k Bankruptcy_i^k + \varepsilon_{it} \tag{12}$$

where y_{it} is our outcome of interest, α_i are individual fixed effects that control for time-invariant differences across filers, α_t are year fixed effects that control for time-varying factors that affect all filers similarly, and $Pscore_i^k$ is the decile of predicted propensity score separate by event time k. $Bankruptcy_i^k$ is an indicator for being in the k-th period before or after filing interacted with having been granted bankruptcy protection. The coefficients γ_k therefore measure the evolution of y_{it} for granted bankruptcy filers relative to dismissed filers. We omit effects for the year prior to filing, so that all parameter estimates measure differences relative to this base year. For mortality and home foreclosure where we do not observe differences in the year prior to filing, we omit the year of filing.

Figure 13 reports the MTE of the job mobility outcomes described in Table 7. Consistent with our main results, the MTE functions for DI and SSI are decreasing in the predicted probability of bankruptcy protection and the MTE functions for same state, same industry, same job, and average firm wage are increasing in the predicted probability of bankruptcy protection. These results suggest that filers on the margin of bankruptcy who are assigned to the most lenient judges experience the largest gains in stability when granted bankruptcy protection. The MTE functions for 401k contributions, same county, and job tenure are flat.

As a result, our event study estimates for these outcomes are likely attenuated towards zero as we attribute any treatment effects in the year of filing to the effects of bankruptcy protection.

Appendix Figure 14 presents event study estimates of equation (12) for Chapter 13 filers in our estimation sample and all Chapter 13 filers in our data. The "full" sample is restricted to a ten percent random draw of first-time filers for the non-foreclosure results for computational reasons. The home foreclosure sample includes all first-time filers in counties covered by DataQuick. Standard errors are clustered at the individual level.

There is a large and statistically significant effect of Chapter 13 protection on all of our outcomes in both the estimation sample and the full sample of Chapter 13 filers. Over the first five post-filing years, filers receiving Chapter 13 protection earn $4,034 to $5,581 more than dismissed filers in our estimation sample, and $3,783 to $5,114 more in our full sample. Employment is 2.6 to 8.1 percentage points higher in our estimation sample, and 2.3 to 6.7 percentage points higher in our full sample. Five-year mortality and foreclosure are 2.0 and 10.9 percentage points lower in our estimation sample, and 1.5 and 12.9 percentage points lower in our full sample. Thus, the impact of Chapter 13 protection appears to be similar in our full and estimation samples.

A final concern is that our analysis has focused on Chapter 13 bankruptcy, which makes up about 25 percent of all bankruptcy filings. Recall that there is not enough variation in the treatment of Chapter 7 cases to estimate the effects of Chapter 7 protection using our instrumental variables strategy. Appendix Figure 14 presents results for Chapter 7 protection using our event study methodology described above. We again restrict the non-foreclosure results to a ten percent random draw of all first-time filers. The home foreclosure sample includes all first-time filers in counties covered by DataQuick.

The effect of Chapter 7 protection is much more modest than the effect of Chapter 13 protection. Over the first five post-filing years, filers granted Chapter 7 bankruptcy protection earn $1,639 to $1,936 more than dismissed filers, and are 2.4 to 2.8 percentage points more likely to be employed. Filers granted Chapter 7 protection are 1.7 percentage points less likely to experience a home foreclosure after five years, but are no more or less likely to be deceased. The more modest impact of Chapter 7 is somewhat surprising given the fact that, in principle, all Chapter 7 filers have the option of filing under Chapter 13. The more modest benefits of Chapter 7 therefore seem likely to be the result of the different composition of filers under each Chapter. There may also be important impacts of Chapter 7 on other ex-post outcomes that we are not measuring here, such as credit availability or cost, that compare more favorably with respect to Chapter 13. Finally, it is possible that our event study design is less plausible for Chapter 7 cases. Less than two percent of Chapter 7 cases are dismissed, with these cases typically dismissed due to fraud or an inability to complete the bankruptcy paperwork correctly. Thus, dismissed Chapter 7 filers may not be a suitable control group for granted filers.

VI. Conclusion

In this paper, we estimate the impact of Chapter 13 bankruptcy protection on subsequent labor supply, mortality, and home foreclosure. We find that Chapter 13 increases the marginal recipient's annual earnings in the first five post-filing years by $5,562, a 25.1 percent increase from the baseline mean. Employment increases by 6.8 percentage points over the same time period, an 8.3 percent increase from the baseline mean. Five-year mortality is 1.2 percentage points lower, a 30.0 percent decrease from the dismissed filer mortality rate, with five-year foreclosure rates falling by 19.1 percentage points, a more than one hundred percent decrease from the dismissed filer foreclosure rate. We find evidence consistent with the results being driven by increased incentive to work and increased economic stability following the receipt of bankruptcy protection. A descriptive analysis of granted and dismissed filers further suggests the impacts are driven by the deterioration of outcomes among dismissed filers as opposed to any gains by granted filers.

Our results provide new evidence on the ex-post benefits of debt relief. These results are particularly important in light of the on-going debate surrounding the use of debt relief and mortgage modification to stimulate the economy. Work by Mulligan (2008), Hall (2011), and Eggertsson and Krugman (2012) suggests that household borrowing constraints can help explain the severity of the recession, while Mian and Sufi (2012) show that regional differences in debt overhang can explain differences in unemployment. Our estimates also suggest that the restrictions on bankruptcy filing introduced by the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act may have important adverse consequences on debtors.

The main limitation of our analysis is that we are not able to estimate the impact of bankruptcy laws on ex-ante borrowing costs or behavior. There may also be important ex-post impacts of bankruptcy protection on outcomes such as credit availability that we are unable to measure with our data. Finally, our analysis has focused on Chapter 13 bankruptcy, which makes up about 25 percent of all bankruptcy filings. This paper should therefore be viewed as a first step towards characterizing the impact of consumer bankruptcy protection on debtors.

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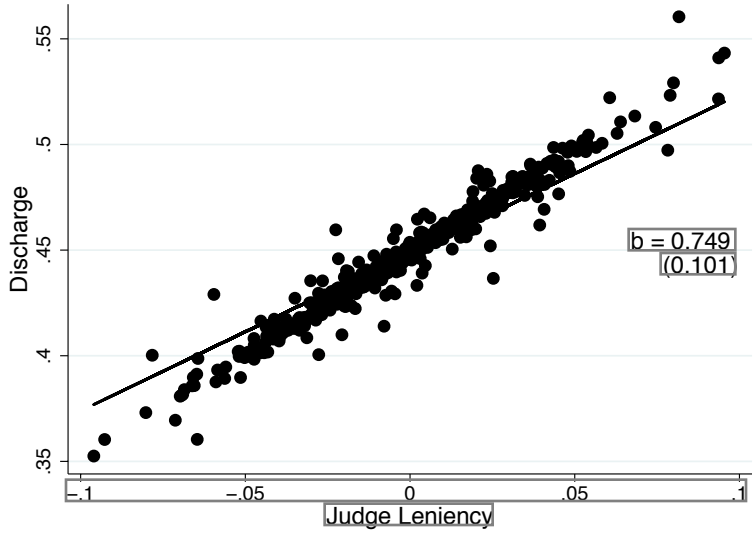
Figure 1

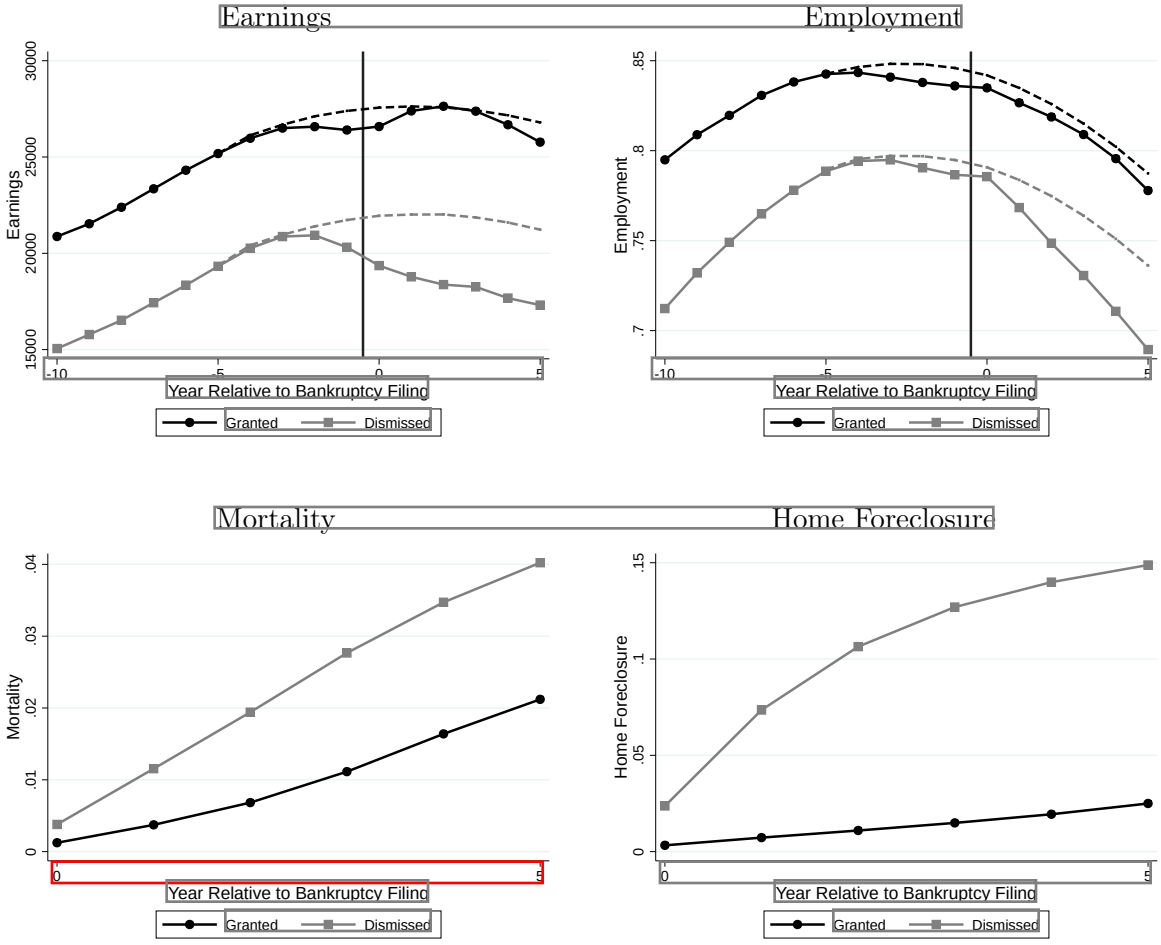
Chapter 13 Judge Leniency and Chapter 13 Bankruptcy Protection



Notes: This figure plots Chapter 13 discharge vs. our leave-one-out measure of judge leniency. The sample consists of all first-time Chapter 13 filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. Judge leniency is the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the court in the same filing year. To construct the binned scatter plot, we first regress an indicator for discharge on office by month-of-filing fixed effects and calculate residuals. We then take the mean residual in each judge by year bin, adding the mean discharge rate to each residual to aid in the interpretation of the plot. The solid line shows the best linear fit estimated on the underlying micro data estimated using OLS. The coefficients show the estimated slope of the best-fit line including office by month-of-filing fixed effects, with standard errors clustered at the office level reported in parentheses.

Figure 2

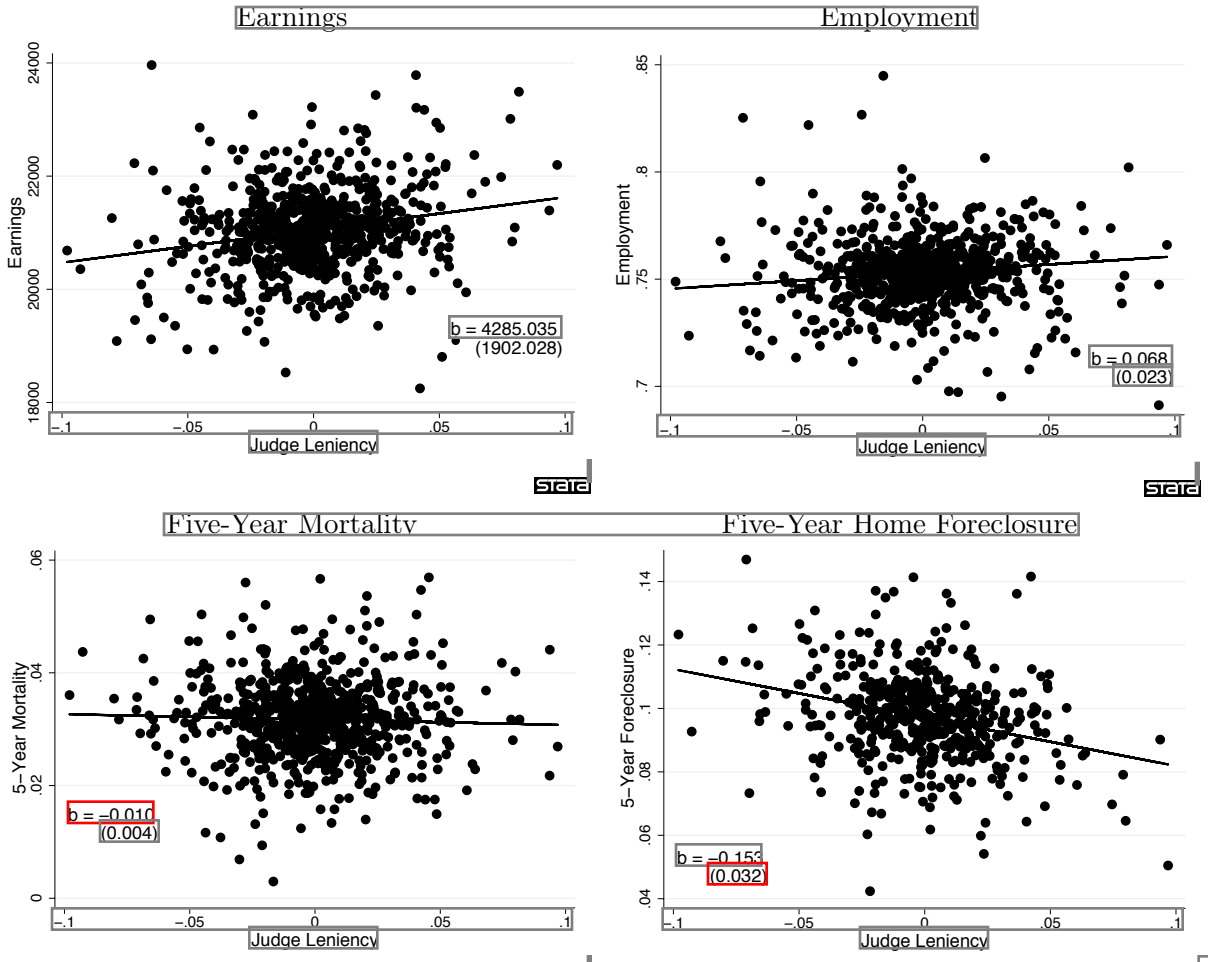
Labor Supply, Mortality, Home Foreclosure for Granted and Dismissed Chapter 13 Filers



Notes: These figures plot average labor supply, mortality, and home foreclosure for granted and dismissed bankruptcy filers. The labor supply and mortality sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. The foreclosure sample includes the subset of those filings originating in county by year bins with foreclosure data coverage. Predicted earnings and employment are based on the fitted values from a regression of earnings on gender, race, a quadratic in tenure, industry fixed effects, and earnings in the sixth through tenth years before filing. Year 0 indicates the year a debtor files for bankruptcy protection. Earnings are winsorized at the top and bottom one percent. Employment is an indicator for non-zero wage earnings on the W-2. All monetary values are expressed in real 2000 dollars. Mortality is an indicator for being deceased in or before the indicated year using information from the Death Master File. Foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or being transferred to a REO or a guarantor in or before the indicated year.

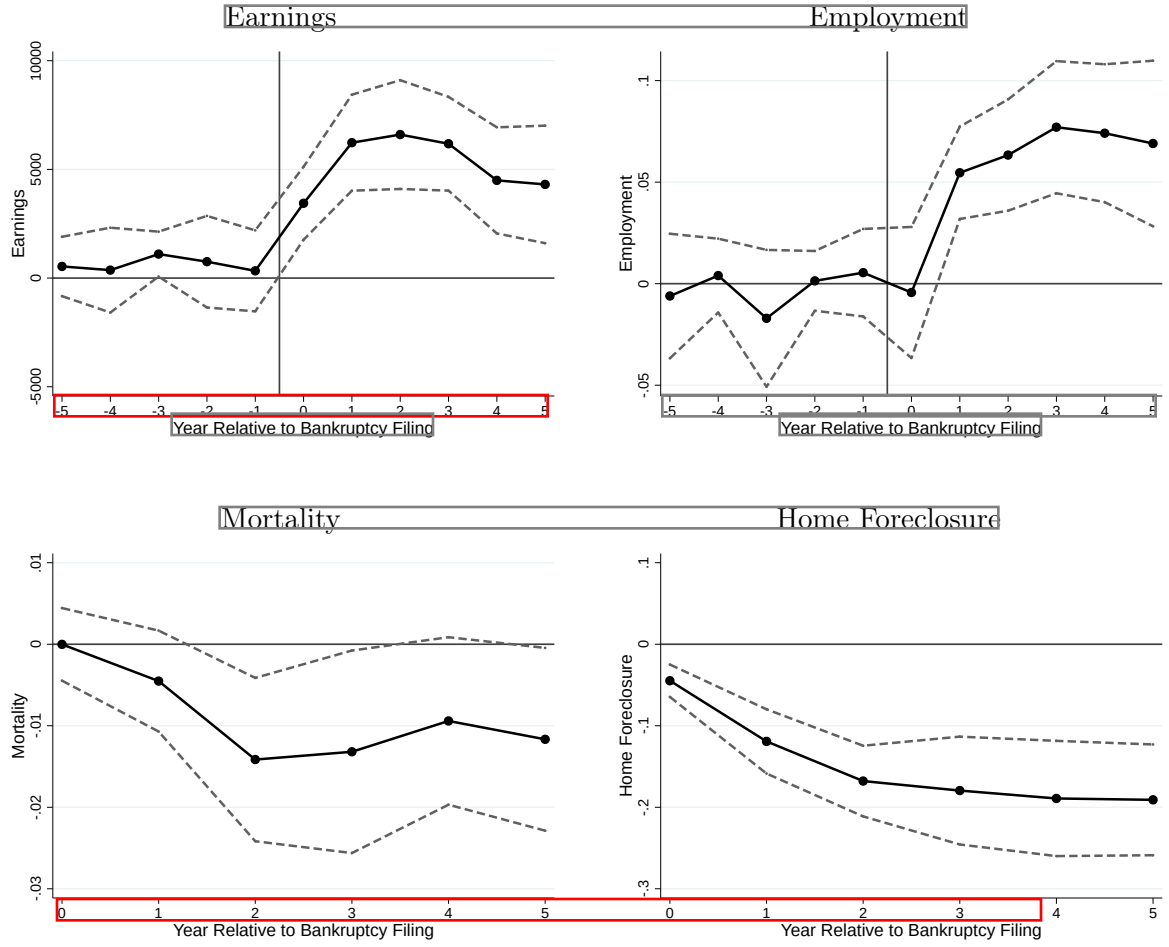
Figure 3

Chapter 13 Judge Leniency and Labor Supply, Mortality, and Home Foreclosure



Notes: These figures plot earnings, employment, five-year mortality, and five-year foreclosure vs. our leave-one-out measure of judge leniency. The earnings and mortality sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. The foreclosure sample includes the subset of those filings originating in county by year bins with foreclosure data coverage. Judge leniency is the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the court in the same filing year. To construct the binned scatter plot, we first regress each outcome on office by month-of-filing fixed effects and calculate residuals. We then take the mean residual in each judge by year bin, adding the mean discharge rate to each residual to aid in the interpretation of the plot. The solid line shows the best linear fit estimated on the underlying micro data estimated using OLS. The coefficients show the estimated slope of the best-fit line including office by month-of-filing fixed effects, with standard errors clustered at the office level reported in parentheses. Earnings are winsorized at the top and bottom one percent. Employment is an indicator for non-zero wage earnings on the W-2. All monetary values are expressed in real 2000 dollars. Mortality is an indicator for being deceased in or before the indicated year using information from the Death Master File. Foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or being transferred to a REO or a guarantor in or before the indicated year.

Figure 4
IV Estimates of the Impact of Chapter 13 Protection on
Labor Supply, Mortality, and Home Foreclosure



Notes: These figures plot two-stage least squares results of the impact of Chapter 13 bankruptcy protection on earnings, employment, cumulative mortality, and home foreclosure. The earnings and mortality sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. The foreclosure sample includes the subset of those filings originating in county by year bins with foreclosure data coverage. We instrument for bankruptcy protection using judge leniency and control for gender, race, five-year age effects, baseline employment, baseline earnings, and office by month-of-filing fixed effects. The dashed lines are 95 percent confidence intervals from standard errors clustered at the office level. Year 0 indicates the year a debtor files for bankruptcy protection. Earnings are winsorized at the top and bottom one percent. Employment is an indicator for non-zero wage earnings on the W-2. All monetary values are expressed in real 2000 dollars. Mortality is an indicator for being deceased in or before the indicated year using information from the Death Master File. Foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or being transferred to a REO or a guarantor in or before the indicated year.

Table 1

Summary Statistics

	Chapter 7	Chapter 13		
	Full Sample	Full Sample	Judge Sample	Difference
Demographics	(1)	(2)	(3)	(4)
Granted Bankruptcy	0.984	0.488	0.448	0.040*
Age	41.813	42.530	42.847	−0.317
Male	0.603	0.634	0.609	0.025*
White	0.742	0.558	0.528	0.030
Black	0.133	0.339	0.361	−0.022
Matched to a Home	0.386	0.455	0.485	−0.030
Baseline Outcomes 5 Years Before Filing				
Employment	0.806	0.802	0.813	−0.011
Self Emp.	0.060	0.061	0.061	0.000
Earnings	21.090	22.333	22.115	0.218
Wages	20.586	21.725	21.529	0.196
Self Earnings	0.504	0.608	0.585	0.023
401k Contributions	0.260	0.283	0.316	−0.033**
Disability Insurance	0.047	0.042	0.041	0.001
Sup. Security Income	0.097	0.084	0.086	−0.002
Job Tenure	3.413	3.612	3.624	−0.012
Firm Wages	22.424	22.983	19.109	3.874**
Observations	7393983	1869772	490216	

Notes: This table reports summary statistics. The full sample consists of first-time filers between 1992 and 2005 in 72 bankruptcy courts. The judge sample consists of all first-time filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. Home match is calculated only for the subset of those filings originating in county by year bins with home data coverage (N = 280202). Granted bankruptcy is an indicator for receiving a discharge of debt. Home match is an indicator for being matched to a home in the DataQuick real estate data. Each baseline outcomes is averaged over five pre-filing years. Earnings and employment outcomes come from 1978 - 2010 W-2s, where employment is an indicator for non-zero wage earnings and self employment is an indicator for non-zero self employment earnings. 401k contributions come from annual W-2s. DI and SSI receipt come from the Master Beneficiary File. Job tenure is calculated using firm EINs. Firm wages are averaged over all employees listing the same EIN in the same calendar year. All monetary values are expressed in real 2000 dollars divided by 1,000.

Table 2

Judge Leniency and Chapter 13 Protection

	(1)	(2)
Judge Leniency	0.749***	0.731***
	(0.101)	(0.095)
Male		−0.015*
		(0.008)
Black		−0.090***
		(0.013)
Age		0.005***
		(0.000)
Earnings		0.005***
		(0.000)
Observations	490216	490216

Notes: This table reports first stage results. The sample consists of all first-time Chapter 13 filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. Judge leniency is the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the court in the same filing year. All regressions control for office by month-of-filing fixed effects and cluster standard errors at the office level. See Table 1 for details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3

Test of Randomization

	Baseline Mean	Judge Leniency		F-test p-value
	(1)	(2)	(3)	(4)
Age	42.847	0.000005	−0.000005	[0.051]
	(16.374)	(0.000007)	(0.000007)	
Male	0.609	−0.000004	0.000028	[0.780]
	(0.491)	(0.000120)	(0.000103)	
White	0.528	0.000045	0.000046	[0.383]
	(0.499)	(0.000065)	(0.000073)	
Employment	0.813	0.000197	0.001479	[0.877]
	(0.343)	(0.000291)	(0.001414)	
Self Emp. Earnings	0.585	−0.000012	−0.000010	[0.489]
	(3.644)	(0.000012)	(0.000012)	
Earnings	22.115	0.000005	0.000009	[0.138]
	(17.176)	(0.000007)	(0.000009)	
Self Employment	0.061	0.000112	0.000093	[0.509]
	(0.183)	(0.000232)	(0.000234)	
Matched to Home	0.485	−0.000352	−0.000351	[0.181]
	(0.448)	(0.000362)	(0.000362)	
401k Contributions	0.316	−0.000119	−0.000117	[0.429]
	(0.855)	(0.000116)	(0.000116)	
Disability Insurance	0.041	0.000578	0.000573	[0.800]
	(0.191)	(0.000561)	(0.000563)	
Job Tenure	3.624	0.000076*	0.000077*	[0.519]
	(3.735)	(0.000042)	(0.000043)	
Firm Wage	19.109	0.000002	0.000003	[0.745]
	(18.387)	(0.000002)	(0.000003)	
Predicted Earnings	0.021		−0.000114	[0.549]
	(0.012)		(0.011114)	
Predicted Employment	0.753		−0.004109	[0.794]
	(0.282)		(0.004583)	
Predicted Mortality	0.032		−0.007110	[0.468]
	(0.045)		(0.009418)	
Joint F-Test		[0.233]	[0.252]	
Observations	490216	490216	490216	

Notes: This table reports reduced form results testing the random assignment of filings to judges. The sample consists of all first-time Chapter 13 filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. Column 1 reports means and standard deviations for dismissed filers. Columns 2 - 3 report estimates from an OLS regression of judge leniency on the variables listed and office by month-of-filing fixed effects, with standard errors clustered at the office level. Judge leniency is the leave-one-out mean rate of granting bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the court in the same filing year. The p-value reported at bottom of columns 2 - 3 is for a F-test of the joint significance of the variables listed in the rows. Each row of column 4 reports a p-value from a separate OLS regression of only the pre-determined variable listed in the corresponding row on judge and office by month-of-filing fixed effects. The p-value is for a F-test of the joint significance of the judge fixed effects. Predicted earnings, employment, and mortality are formed using all other variables listed. See Table 1 for details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 4

IV Estimates of the Impact of Chapter 13 Protection on Labor Supply, Mortality, and Home Foreclosure

	Baseline Mean	2SLS Results	
Panel A: Labor Supply	(1)	(2)	(3)
Earnings	22.115	5.721***	5.562***
	(17.176)	(1.644)	(1.141)
Employment	0.813	0.091***	0.068***
	(0.343)	(0.023)	(0.013)
Panel B: Mortality			
5-year Mortality	0.000	−0.013**	−0.012**
	(0.000)	(0.006)	(0.006)
Panel C: Home Foreclosure			
5-year Foreclosure	0.001	−0.190***	−0.191***
	(0.019)	(0.034)	(0.035)
Panel D: Misc. Outcomes			
401k Contributions	0.316	0.055	−0.008
	(0.855)	(0.048)	(0.044)
Disability Insurance	0.041	0.000	0.012
	(0.191)	(0.012)	(0.012)
Sup. Security Income	0.006	−0.140*	−0.128*
	(0.073)	(0.081)	(0.074)
Controls	–	No	Yes
Observations	490216	490216	490216

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection. The sample consists of all first-time filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. The foreclosure sample includes the subset of those filings originating in county by year bins with foreclosure data coverage (N = 280202). The SSI sample includes the subset of filers who are 65 or older (N = 53594). Column 1 reports the mean and standard deviation for the five years before filing. Columns 2 - 3 instrument for bankruptcy protection using the reduced form measure of judge leniency described in the text. All specifications control for office by month-of-filing fixed effects, and cluster standard errors at the office level. Column 3 adds controls for gender, race, age, and the five year average of baseline employment and baseline earnings. All monetary values are expressed in real 2000 dollars divided by 1,000. See Table 1 for additional details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 7

Chapter 13 Job Mobility Results

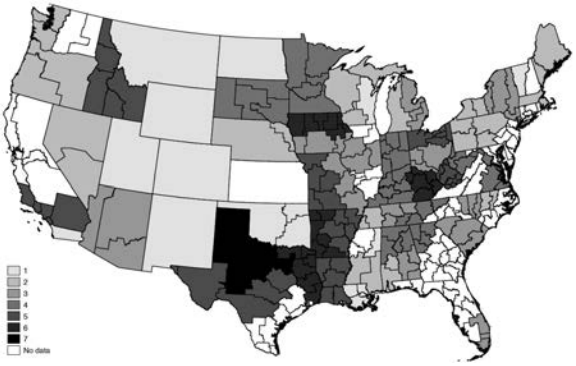
	Baseline Mean	2SLS Results	
	(1)	(2)	(3)
Work in Same County	0.576	0.250***	0.229***
	(0.399)	(0.025)	(0.024)
Work in Same State	0.627	0.170***	0.153***
	(0.405)	(0.024)	(0.024)
Work in Same Job	0.502	0.272***	0.246***
	(0.386)	(0.026)	(0.025)
Work in Same Industry	0.557	0.262***	0.238***
	(0.396)	(0.030)	(0.026)
Job Tenure	3.624	0.987***	0.820***
	(3.735)	(0.196)	(0.175)
Firm Wages	19.109	4.635***	4.102***
	(18.387)	(1.347)	(1.096)
Controls	–	No	Yes
Observations	382385	382385	382385

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection. The sample consists of all first-time filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges that are employed for at least one time period post-bankruptcy. Column 1 reports the mean and standard deviation for the five years before filing. Columns 2 - 3 instrument for bankruptcy protection using the reduced form measure of judge leniency described in the text. All specifications control for office by month-of-filing fixed effects, and cluster standard errors at the office level. Column 3 adds controls for gender, race, age, and the five year average of baseline employment and baseline earnings. Same county, state, job and industry are defined using the filer's job in the year prior to filing. Baseline means for each group are reported in brackets. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

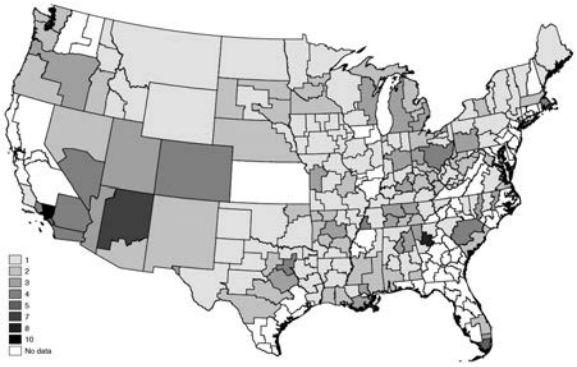
Appendix Figure 1

The Geography of Consumer Bankruptcy

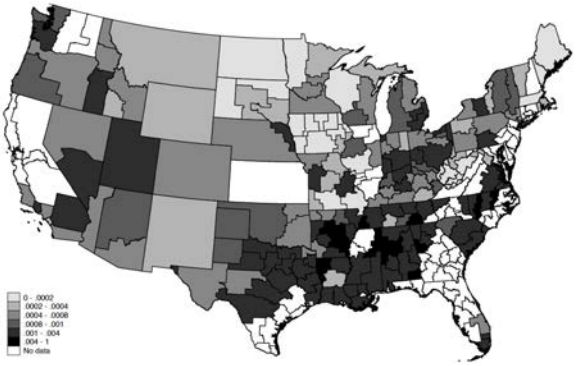
Number of Bankruptcy Offices in Each Court



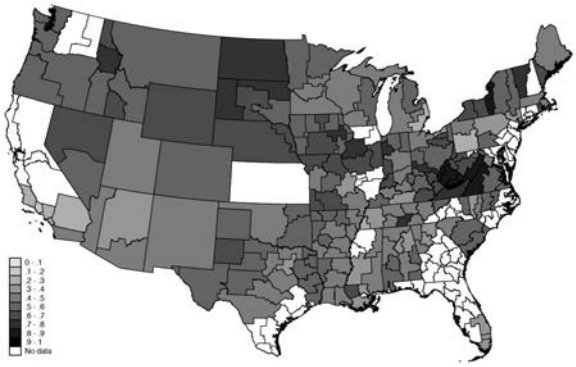
Number of Chapter 13 Judges



Chapter 13 Filings Per Capita



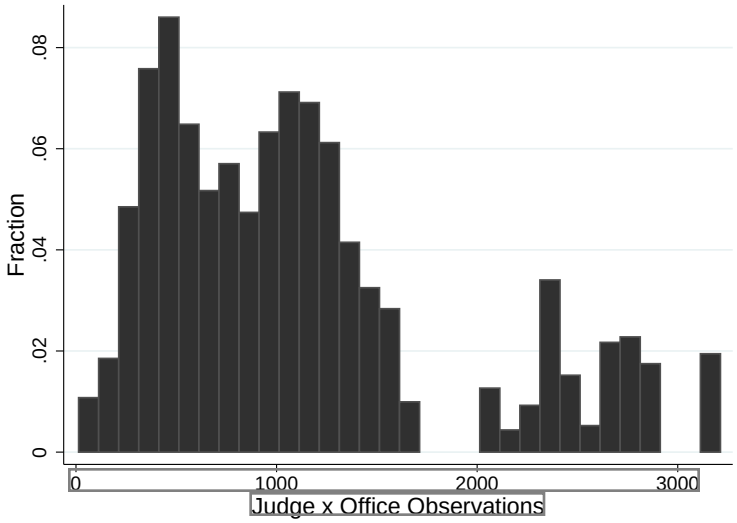
Chapter 13 Discharge Rate



Variation in Chapter 13 Judge Leniency

Notes: These figures display summary statistics by bankruptcy office. The sample includes the 72 bankruptcy courts that allow electronic access to their dockets. The number of Chapter 13 judges is based on 2003 filings. Per capita Chapter 13 filings are calculated using the average annual number of filings between 2000 and 2004 divided by population as reported by the 2000 census. The Chapter 13 discharge rate is calculated across all available years between 1992 and 2005. The Chapter 13 judge leniency sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges.

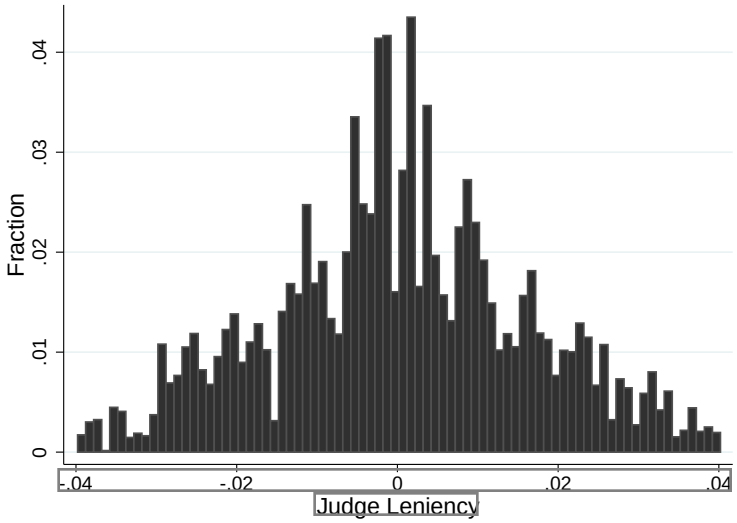
Appendix Figure 2
Distribution of Judge by Office Observations



Notes: This figure displays the distribution of judge by office cell sizes in our estimation sample. The sample consists of all first-time filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. See text for details.

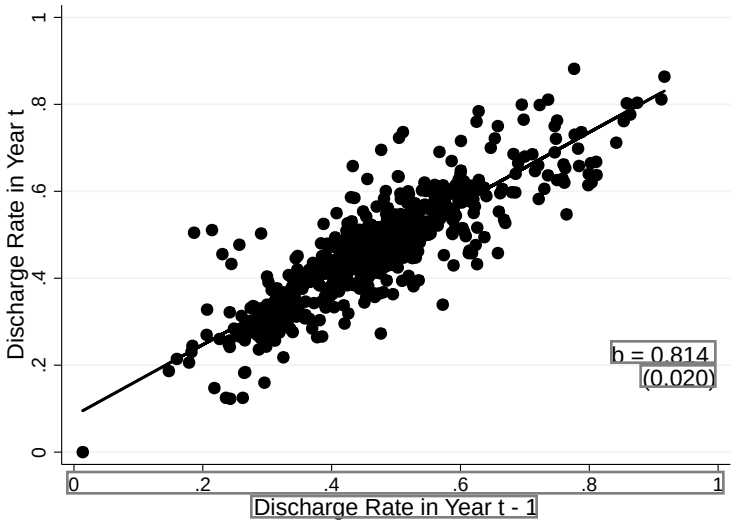
Appendix Figure 3

Distribution of Chapter 13 Judge Leniency



Notes: This figure displays the distribution of Chapter 13 judge leniency for the estimation sample. The sample consists of all first-time filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. Judge leniency is defined as the leave-one-out mean rate of granting bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office in the same filing year.

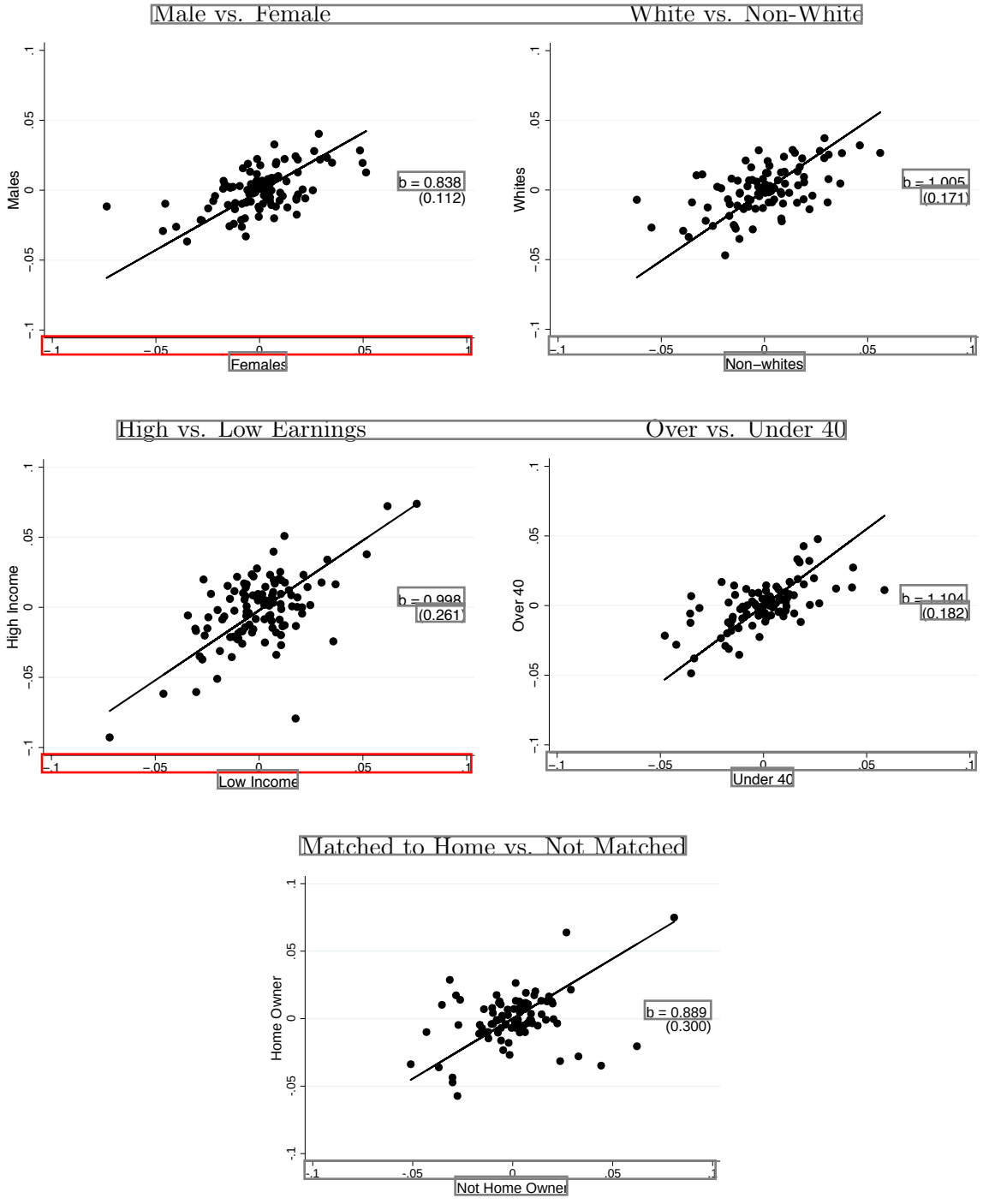
Appendix Figure 4
Persistence of Chapter 13 Judge Discharge Rates



Notes: This figure shows the correlation between judge specific Chapter 13 discharge rates in the current and lagged years. The sample consists of all first-time filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. See text for details.

Appendix Figure 5

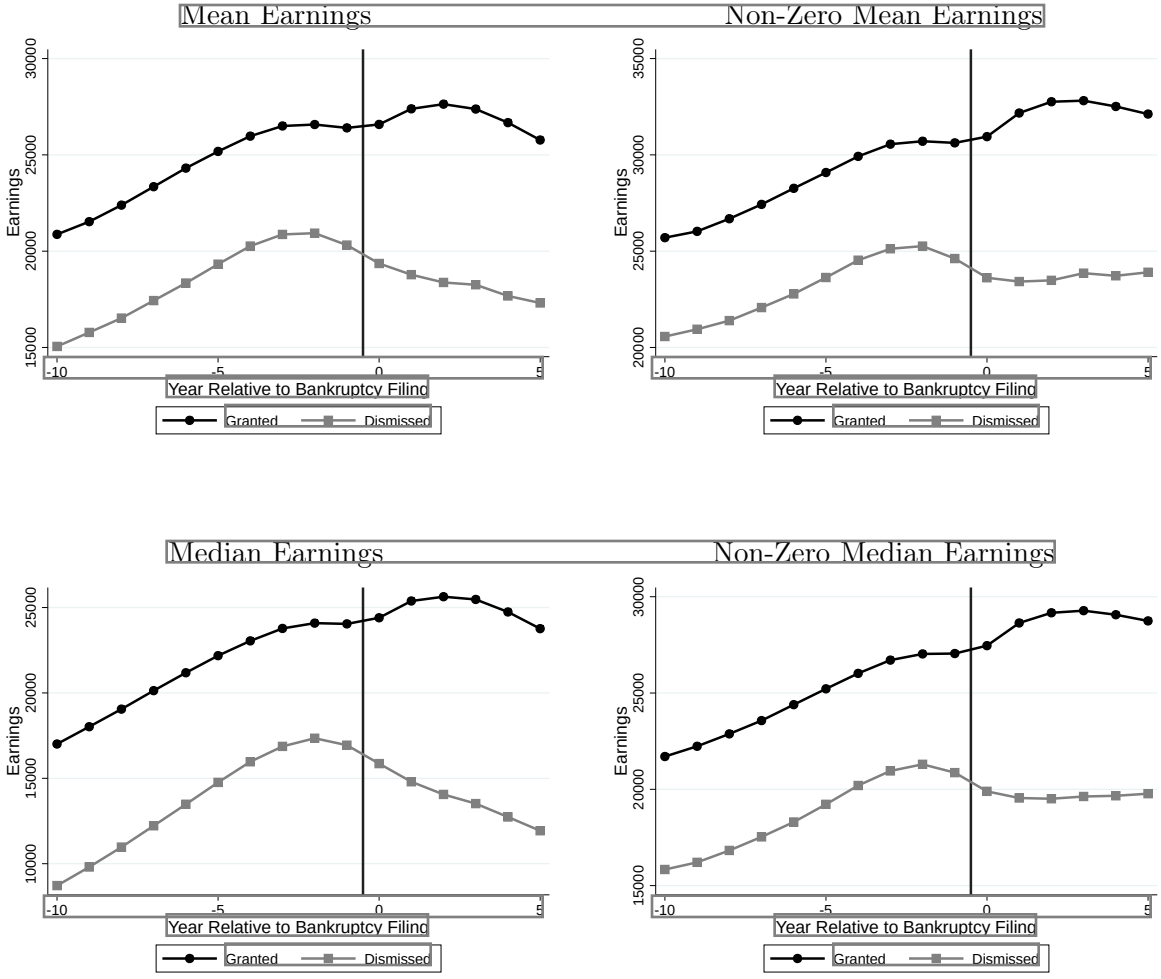
Chapter 13 Judge Leniency by Filer Characteristics



Notes: These figures show the correlation between judge leniency for different groups of filers. The sample consists of all first-time filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. Judge leniency is defined as the leave-one-out mean rate of granting bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office in the same filing year. We take the average leniency for each group over all available years of data. The solid line shows the best linear fit estimated using OLS relating each judge leniency measure.

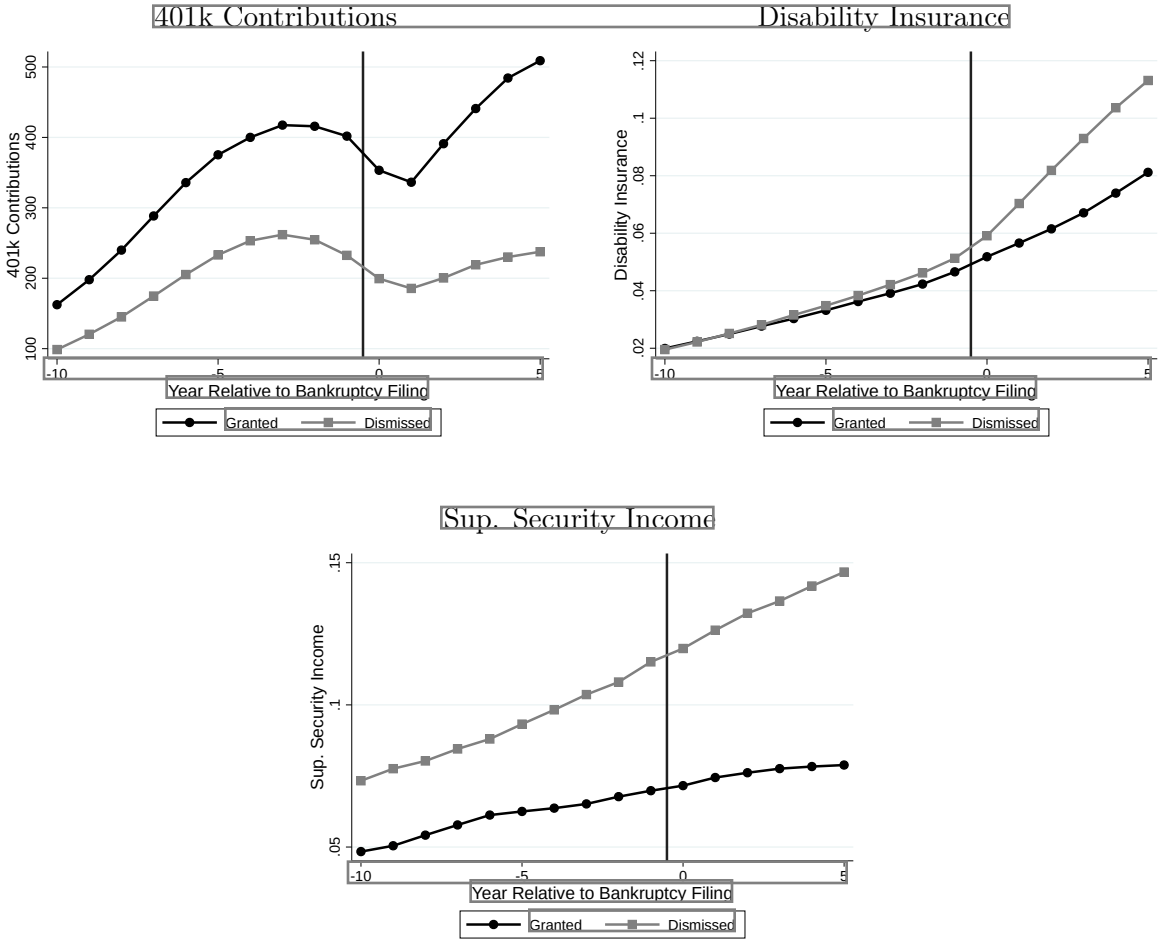
Appendix Figure 6

Mean and Median Earnings for Granted and Dismissed Chapter 13 Filers



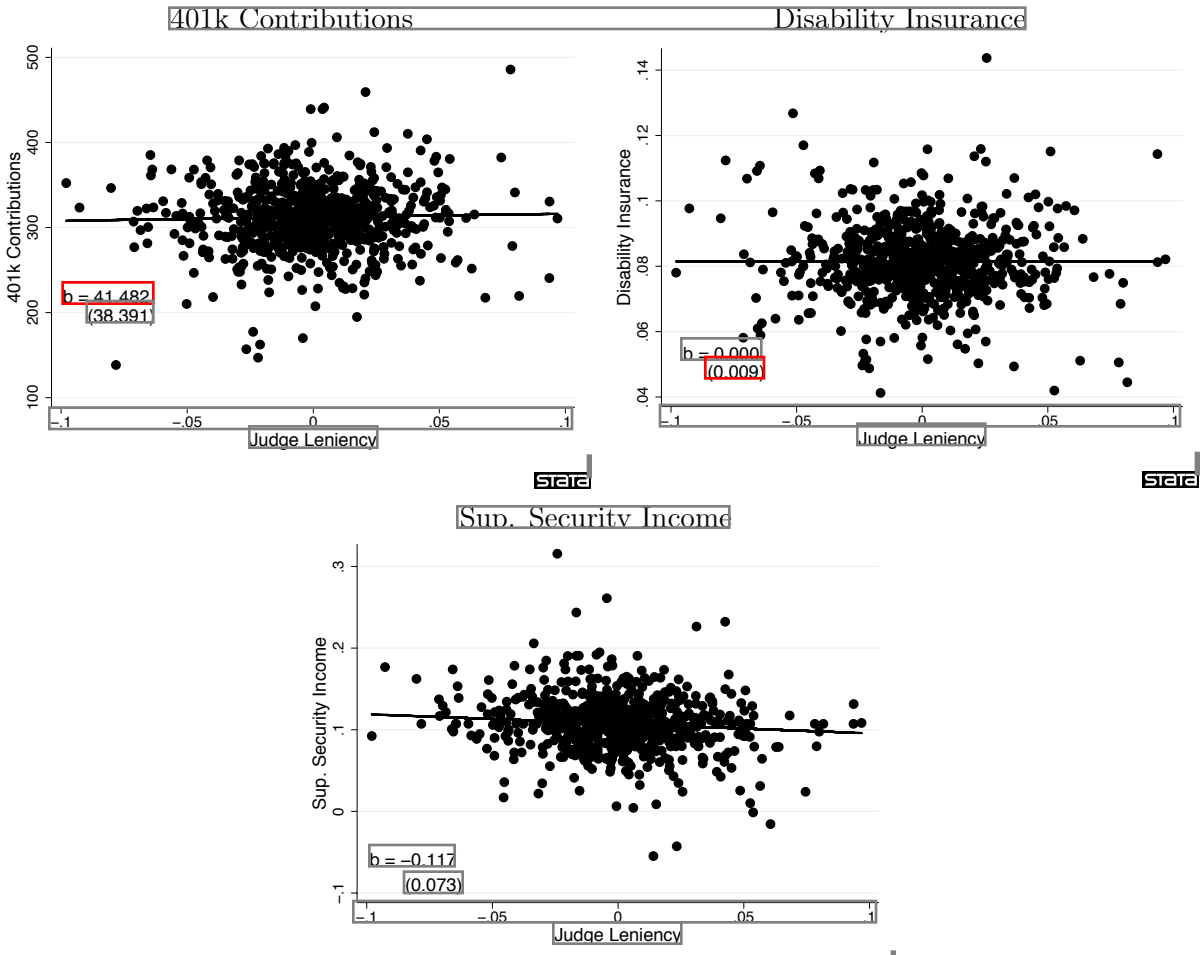
Notes: These figures plot mean and median earnings for granted and dismissed bankruptcy filers. The sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. Year 0 indicates the year a debtor files for bankruptcy protection. See text for additional details.

Appendix Figure 7
401k Contributions, Disability Insurance, and Supplemental Security Income for Granted and Dismissed Chapter 13 Filers



Notes: These figures plot average 401k contributions, Disability Insurance, and Sup. Security Income for granted and dismissed bankruptcy filers. The sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. The Sup. Security Income sample includes the subset of filers 65 or older. Year 0 indicates the year a debtor files for bankruptcy protection.

Appendix Figure 8
Chapter 13 Judge Leniency and 401k Contributions,
Disability Insurance, and Supplemental Security Income

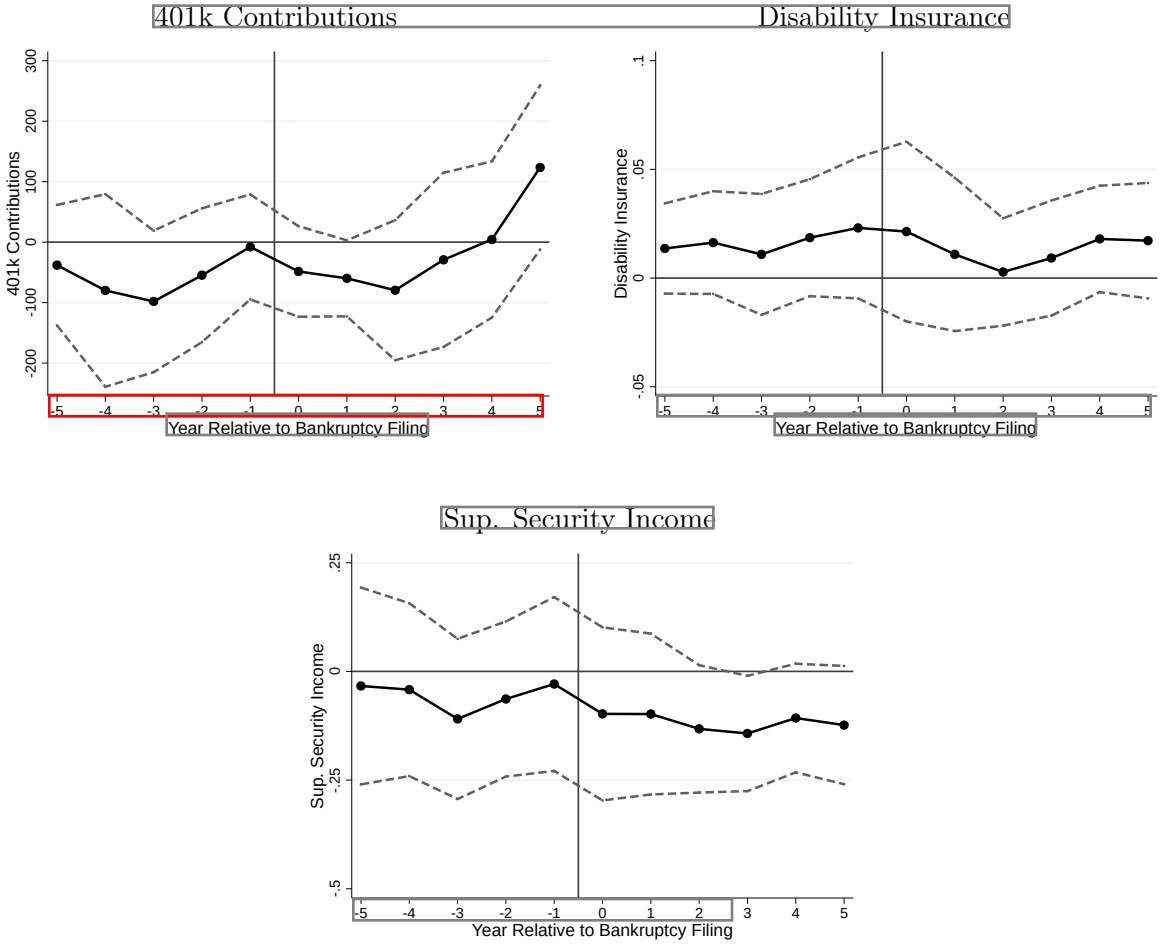


Notes: These figures plot 401k contributions, Disability Insurance, and Sup. Security Income vs. our leave-one-out measure of judge leniency. The sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. The Sup. Security Income sample includes the subset of filers 65 or older. Judge leniency is the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the court in the same filing year. To construct the binned scatter plot, we first regress each outcome on office by month-of-filing fixed effects and calculate residuals. We then take the mean residual in each judge by year bin, adding the mean discharge rate to each residual to aid in the interpretation of the plot. The solid line shows the best linear fit estimated on the underlying micro data estimated using OLS. The coefficients show the estimated slope of the best-fit line including office by month-of-filing fixed effects, with standard errors clustered at the office level reported in parentheses.

Appendix Figure 9

IV Estimates of the Impact of Chapter 13 Protection on

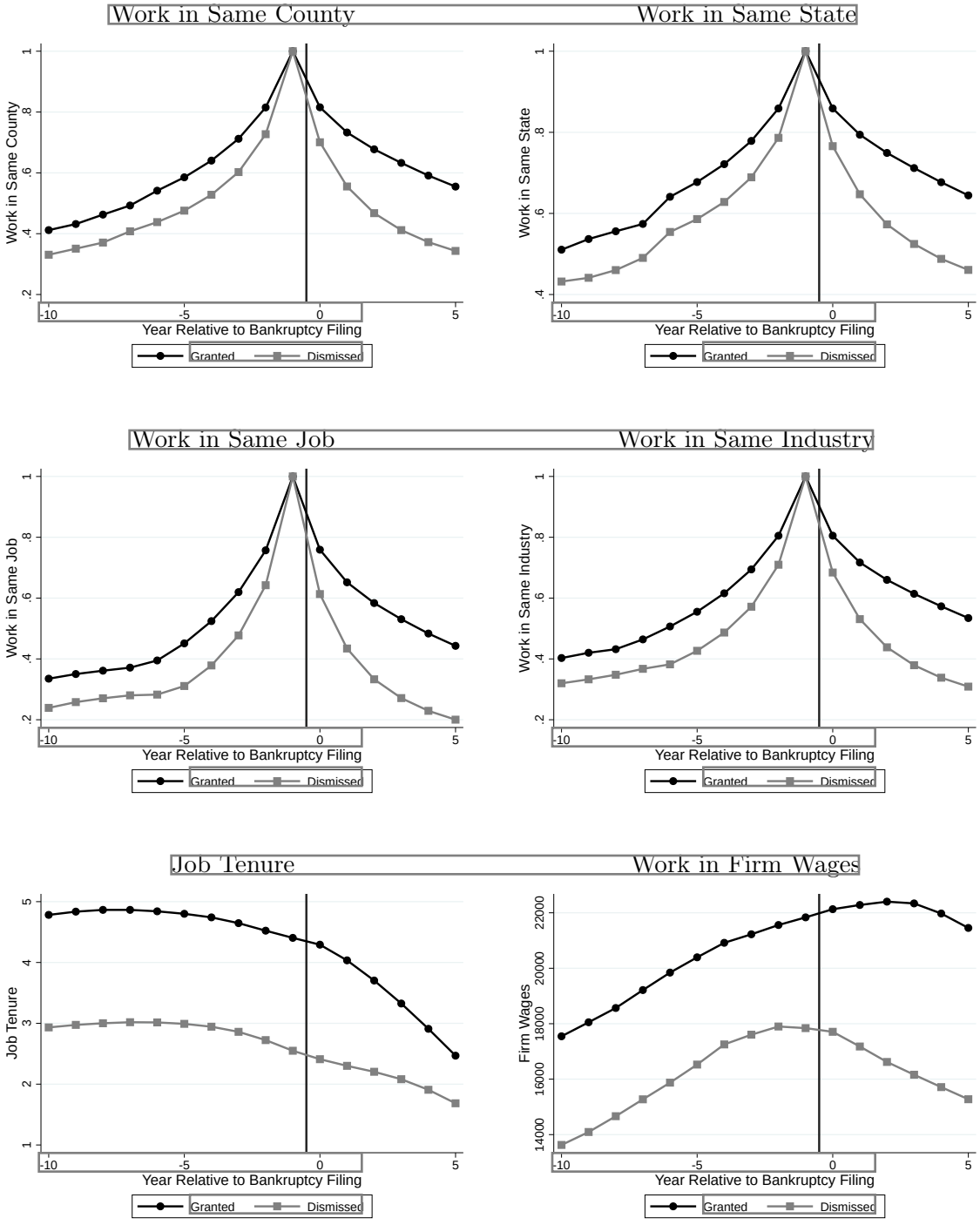
401k Contributions, Disability Insurance, and Supplemental Security Income



Notes: These figures plot two-stage least squares results of the impact of Chapter 13 bankruptcy protection. The earnings and mortality sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. The Sup. Security Income sample includes the subset of filers 65 or older. We instrument for bankruptcy protection using judge leniency and control for gender, race, five-year age effects, baseline employment, baseline earnings, and office by month-of-filing fixed effects. The dashed lines are 95 percent confidence intervals from standard errors clustered at the office level. Year 0 indicates the year a debtor files for bankruptcy protection.

Appendix Figure 10

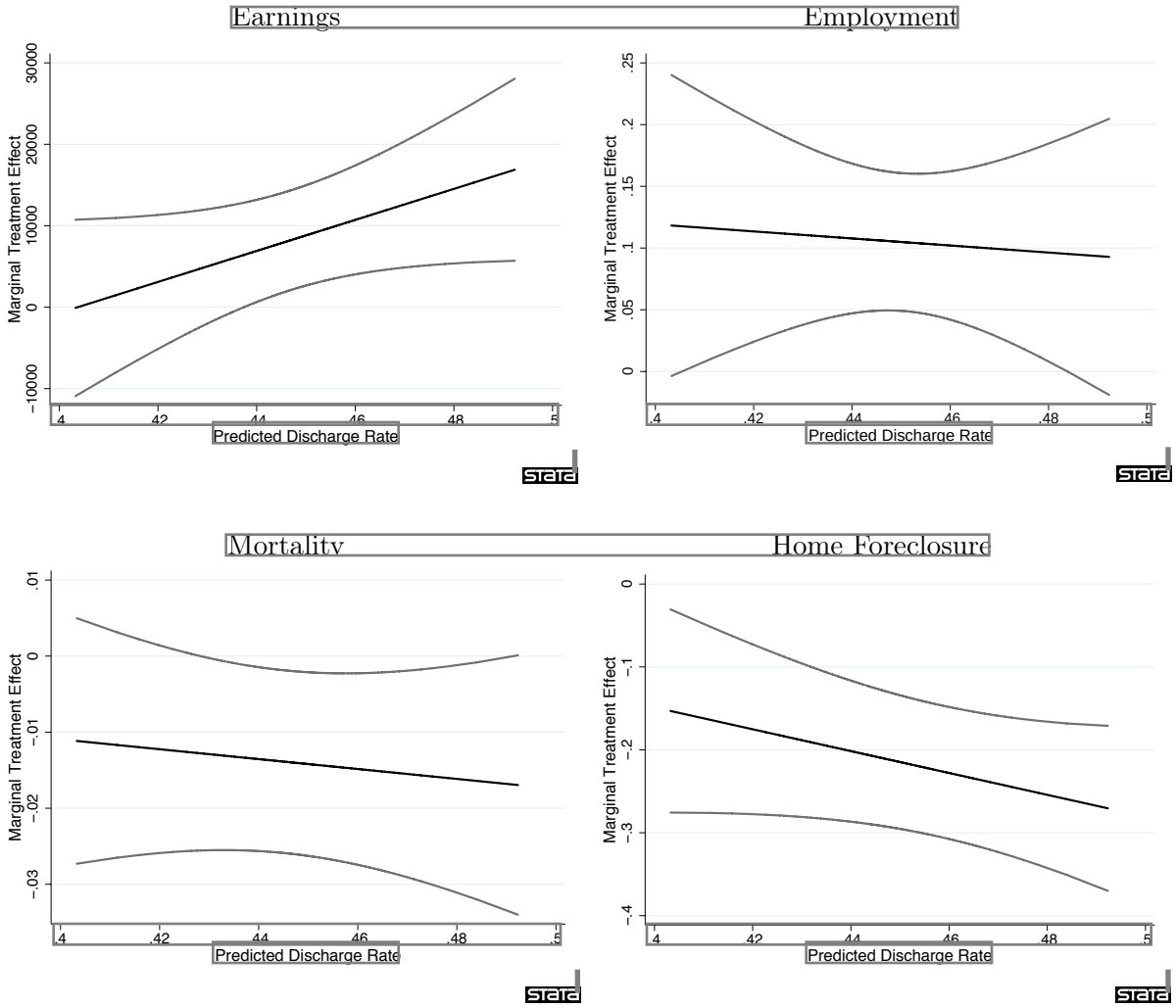
Job Mobility Outcomes for Granted and Dismissed Chapter 13 Filers



Notes: These figures plot average job mobility outcomes for granted and dismissed bankruptcy filers. The sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges that are employed for at least one time period post-bankruptcy. Year 0 indicates the year a debtor files for bankruptcy protection. Same county, state, job, and industry variables are defined relative to the year before filing. Each variable is therefore mechanically equal to one in year -1. See Table 7 for additional details.

Appendix Figure 11

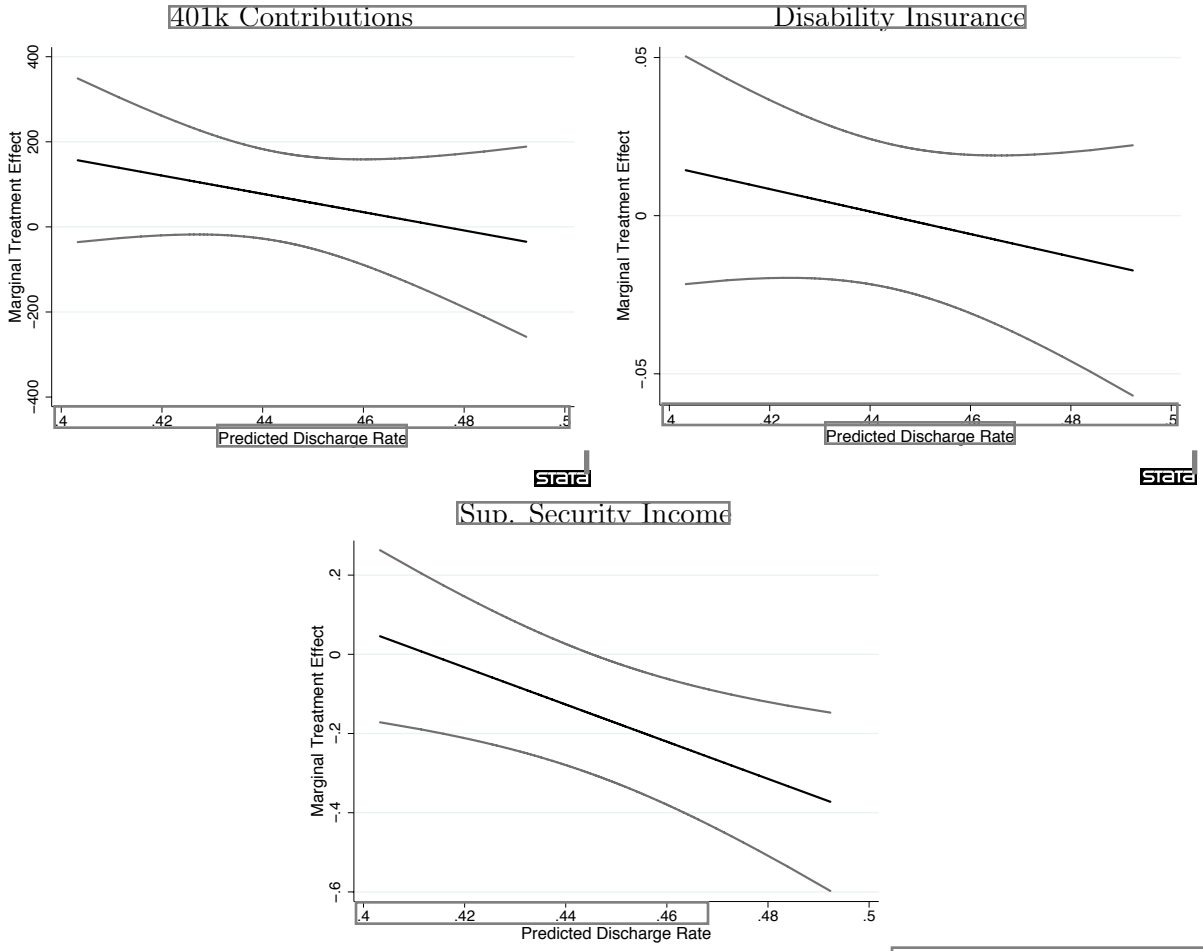
Marginal Treatment Effects for Labor Supply, Mortality, and Home Foreclosure



Notes: These figures plot marginal treatment effects and associated 95 percent confidence intervals. The sample includes first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. The foreclosure sample includes the subset of those filings originating in county by year bins with foreclosure data coverage. We predict the probability of bankruptcy protection using our leave-one-out measure of judge leniency. We then predict the relationship between each outcome and the predicted probability of receiving bankruptcy protection using a local quadratic estimator with bandwidth 0.069. The estimates of the first derivative of this relationship are then evaluated at each percentile of the predicted probability. Standard errors are calculated using a bootstrap procedure with 250 iterations. All monetary values are expressed in real 2000 dollars divided by 1,000. Earnings information comes from the W-2, and is averaged over the first five post-filing years. Employment is an indicator for non-zero wage earnings on the W-2, and is averaged over the first five post-filing years. Five-year mortality is an indicator for being deceased within the first five post-filing years using information from the Death Master File. Five-year foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or being transferred to a REO or a guarantor within the first five post-filing years using real estate records from DataQuick.

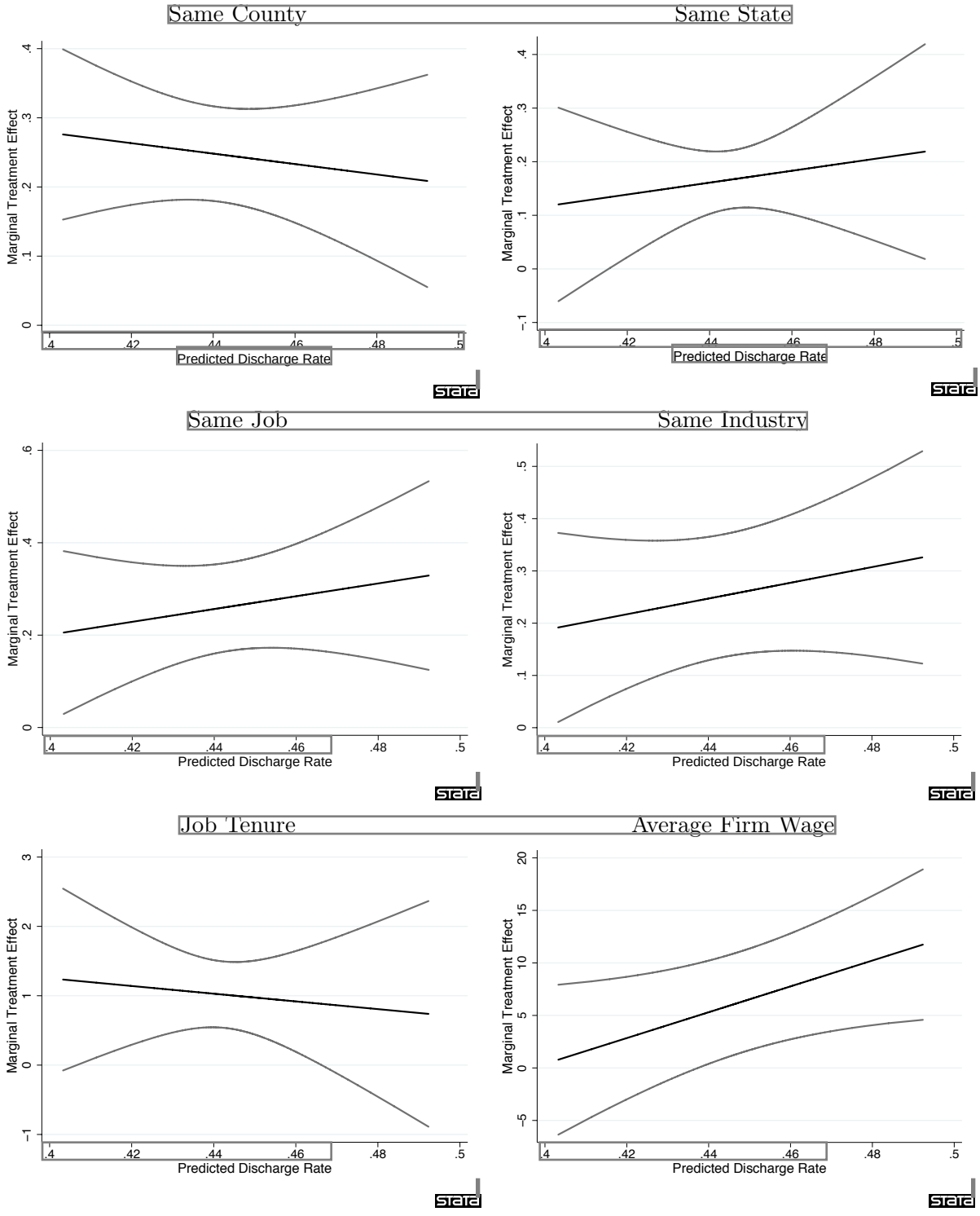
Appendix Figure 12

Marginal Treatment Effects for 401k Contributions, Disability Insurance, and Sup. Security Income



Notes: These figures plot marginal treatment effects and associated 95 percent confidence intervals. The sample includes first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. See Appendix Figure X notes for details on estimation procedure and Table 1 notes for variable definitions.

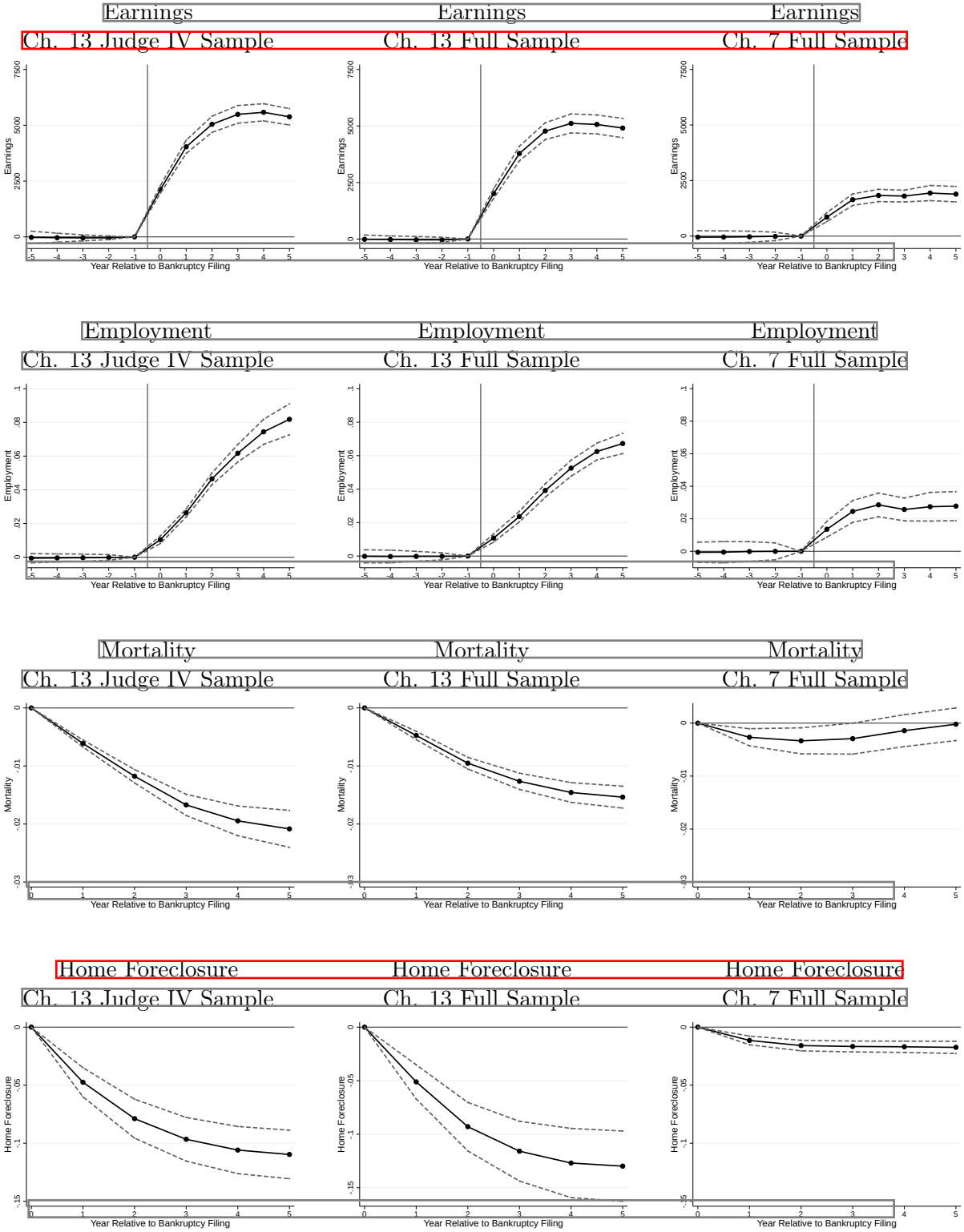
Appendix Figure 13
Job Mobility Marginal Treatment Effects



Notes: These figures plot marginal treatment effects and associated 95 percent confidence intervals. The sample includes first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. See Appendix Figure X notes for details on estimation procedure and Table 7 notes for variable definitions.

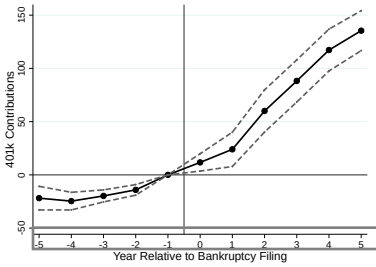
Appendix Figure 14

Event Study Estimates of Chapter 13 and Chapter 7 Protection

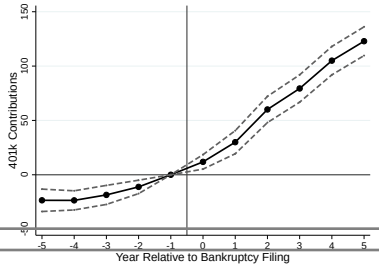


Appendix Figure 14 Continued
Event Study Estimates of Chapter 13 and Chapter 7 Protection

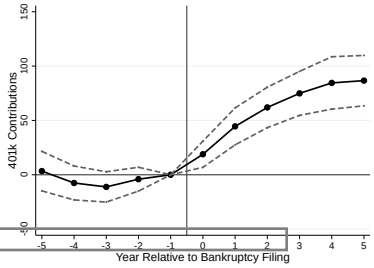
401k Contributions
Ch. 13 Judge IV Sample



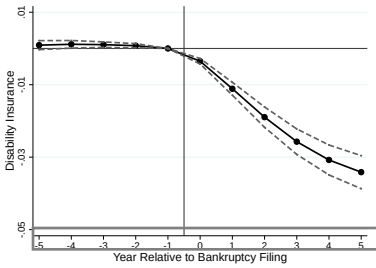
401k Contributions
Ch. 13 Full Sample



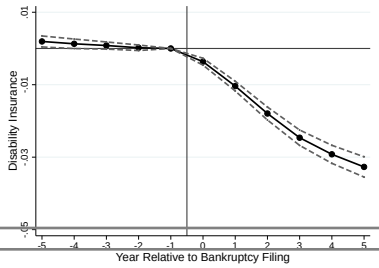
401k Contributions
Ch. 7 Full Sample



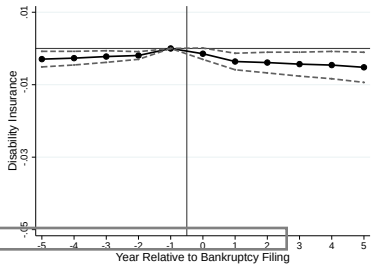
Disability Insurance
Ch. 13 Judge IV Sample



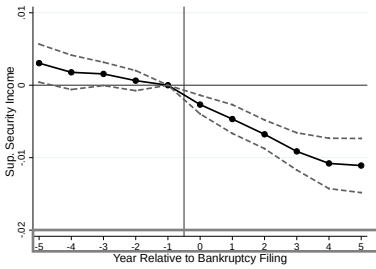
Disability Insurance
Ch. 13 Full Sample



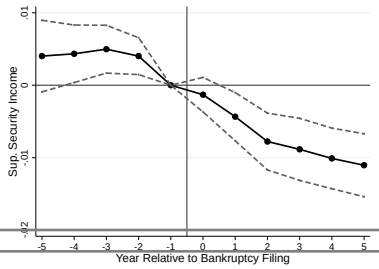
Disability Insurance
Ch. 7 Full Sample



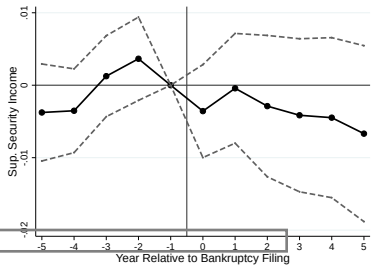
Sup. Security Income
Ch. 13 Judge IV Sample



Sup. Security Income
Ch. 13 Full Sample



Sup. Security Income
Ch. 7 Full Sample



Notes: These figures plot event study results of the impact of Chapter 13 and Chapter 7 bankruptcy protection. The Judge IV sample includes all first-time filings between 1992 and 2005 in the 42 offices that randomly assign cases to judges. The full sample includes a 10 percent random sample of all first-time filings between 1992 and 2005 in the 72 bankruptcy courts that allow electronic access to their dockets. The foreclosure sample includes all filings originating in county by year bins with foreclosure data coverage. We report the coefficient on an indicator for receiving bankruptcy protection interacted with the specified year. We control for individual, year, and propensity score decile by year fixed effects. The propensity score is using a probit regression of an indicator for discharge on gender, race, five-year age bins, five years of baseline employment entered individually, five years of baseline earnings entered individually, office fixed effects, and filing year fixed effects. We then split the sample into deciles and interact each decile with year indicators for the five years before and after filing, omitting the year prior to filing. Standard errors are clustered at the individual level. Year 0 indicates the year a debtor files for bankruptcy protection. All monetary values are expressed in real 2000 dollars. Earnings information comes from the W-2. Employment is an indicator for non-zero wage earnings on the W-2. Mortality is an indicator for being deceased in the indicated year using information from the Death Master File. Foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or being transferred to a REO or a guarantor. See text for additional details.

Appendix Table 1

Bankruptcy Offices in Chapter 13 IV Sample

Court	Office	Years	Judges	Discharge	σ_Z
Northern District of Alabama	Birmingham	1999-2005	3	0.312	0.032
Southern District of Alabama	Mobile	2000-2005	2	0.447	0.006
Eastern District of Arkansas	Little Rock	1996-2001	2	0.467	0.012
Southern District of California	San Diego	1997-2005	4	0.411	0.022
Southern District of Florida	Fort Lauderdale	1994-2005	2	0.335	0.019
Southern District of Florida	Miami	1994-2005	2	0.437	0.012
Northern District of Georgia	Atlanta	2004-2005	8	0.296	0.035
Northern District of Georgia	Rome	2004-2005	2	0.401	0.011
Southern District of Iowa	Davenport	1992-2001	2	0.552	0.019
Southern District of Iowa	Des Moines	1992-2001	2	0.608	0.042
District of Idaho	Boise	1999-2005	2	0.519	0.022
Southern District of Indiana	Indianapolis	2001-2005	3	0.495	0.015
Eastern District of Kentucky	Lexington	1999-2005	2	0.585	0.033
District of Massachusetts	Boston	1995-2003	3	0.344	0.028
Eastern District of Michigan	Detroit	2003-2005	3	0.291	0.007
Western District of Michigan	Grand Rapids	1993-2005	3	0.518	0.023
District of Minnesota	Minneapolis	1992-2005	2	0.462	0.009
District of Minnesota	St. Paul	1992-2005	2	0.485	0.030
Eastern District of Missouri	St. Louis	2003-2005	2	0.405	0.015
Western District of Missouri	Kansas City	2000-2005	3	0.478	0.021
Middle District of North Carolina	Durham	2005	2	0.558	0.017
District of New Mexico	Albuquerque	1999-2005	2	0.315	0.024
District of Nevada	Las Vegas	1996-2005	2	0.396	0.010
Southern District of Ohio	Cincinnati	1997-2005	2	0.598	0.014
Southern District of Ohio	Columbus	1992-2005	3	0.573	0.049
Southern District of Ohio	Dayton	1992-2005	2	0.592	0.030
Northern District of Oklahoma	Tulsa	1997-2005	2	0.486	0.030
District of Oregon	Eugene	1995-2005	2	0.503	0.027
District of Oregon	Portland	2000-2005	3	0.495	0.158
District of South Carolina	Columbia	2003-2005	2	0.751	0.021
Eastern District of Tennessee	Chattanooga	2002-2005	2	0.424	0.017
Middle District of Tennessee	Columbia	2000-2005	3	0.437	0.031
Middle District of Tennessee	Cookeville	2000-2005	3	0.437	0.047
Middle District of Tennessee	Nashville	2000-2005	3	0.467	0.017
Western District of Tennessee	Memphis	1997-2005	3	0.277	0.007
Western District of Texas	San Antonio	1997-2005	2	0.452	0.010
District of Utah	Salt Lake City	2003-2005	3	0.334	0.019
Eastern District of Virginia	Alexandria	2000-2005	2	0.507	0.013
Eastern District of Virginia	Newport News	1997-2005	2	0.523	0.035
Eastern District of Virginia	Norfolk	1997-2005	2	0.551	0.021
Western District of Washington	Tacoma	2000-2005	2	0.503	0.017
Eastern District of Wisconsin	Milwaukee	2003-2005	3	0.456	0.020

Notes: This table presents descriptive statistics for the 42 offices in the 31 bankruptcy courts that randomly assign filings to judges in our instrumental variables sample. σ_Z is the standard deviation of leave-one-out measure of judge leniency described in the text.

Appendix Table 3

Additional Tests of Randomization

	Baseline			F-test
	Mean	Judge Leniency		p-value
	(1)	(2)	(3)	(4)
Number of cases	1118.637	0.000012		[0.000]
	(758.759)	(0.000016)		
Earnings Decile 2	0.100		−0.000444	[0.557]
	(0.300)		(0.000287)	
Earnings Decile 3	0.100		−0.000193	[0.474]
	(0.300)		(0.000147)	
Earnings Decile 4	0.100		−0.000221*	[0.992]
	(0.300)		(0.000129)	
Earnings Decile 5	0.100		−0.000136	[0.733]
	(0.300)		(0.000137)	
Earnings Decile 6	0.100		0.000084	[0.285]
	(0.300)		(0.000259)	
Earnings Decile 7	0.100		0.000044	[0.965]
	(0.300)		(0.000143)	
Earnings Decile 8	0.100		0.000078	[0.813]
	(0.300)		(0.000250)	
Earnings Decile 9	0.100		0.000355	[0.039]
	(0.300)		(0.000350)	
Earnings Decile 10	0.100		0.000147	[0.312]
	(0.300)		(0.000207)	
Joint F-Test		[0.468]	[0.288]	
Observations	490216	490216	490216	

Notes: This table reports reduced form results testing the random assignment of filings to judges. The sample consists of all first-time Chapter 13 filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. Column 1 reports means and standard deviations for dismissed filers. Columns 2 - 3 report estimates from an OLS regression of judge leniency on the variables listed and office by month-of-filing fixed effects, with standard errors clustered at the office level. Judge leniency is the leave-one-out mean rate of granting bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the court in the same filing year. The p-value reported at bottom of columns 2 - 3 is for a F-test of the joint significance of the variables listed in the rows. Each row of column 4 reports a p-value from a separate OLS regression of only the pre-determined variable listed in the corresponding row on judge and office by month-of-filing fixed effects. The p-value is for a F-test of the joint significance of the judge fixed effects. See Table 1 for details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 5

Chapter 13 Results for Additional Outcomes

	Baseline Mean	2SLS Results	
Panel A: Labor Supply	(1)	(2)	(3)
Self Emp. Earnings	0.585	0.290	0.367
	(3.644)	(0.271)	(0.299)
Self Employment	0.061	−0.005	0.001
	(0.183)	(0.016)	(0.018)
Earnings with no zeros	23.729	7.109***	4.416***
	(24.307)	(2.001)	(1.636)
Panel B: Mortality			
Years Alive (out of 5)	5.000	0.057**	0.053**
	(0.000)	(0.023)	(0.021)
Panel C: Home Sales			
5-year Distress Sale	0.001	−0.221***	−0.222***
	(0.021)	(0.035)	(0.036)
5-year Any Home Sale	0.001	−0.335***	−0.339***
	(0.031)	(0.046)	(0.048)
Controls	–	No	Yes
Observations	490216	490216	490216

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection. The sample consists of all first-time filers between 1992 and 2005 in the 42 offices that randomly assign filings to judges. The home sample includes the subset of those filings originating in county by year bins with home sales data coverage (N = 280202). Column 1 reports the mean and standard deviation for the five years before filing. Columns 2 - 3 instrument for bankruptcy protection using the reduced form measure of judge leniency described in the text. All specifications control for office by month-of-filing fixed effects, and cluster standard errors at the office level. Column 3 adds controls for gender, race, age, and the five year average of baseline employment and baseline earnings. All monetary values are expressed in real 2000 dollars divided by 1,000. See Table 1 for additional details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 6

Chapter 13 Results for Years 6 - 10

	Baseline Mean	2SLS Results	
Panel A:Labor Supply	(1)	(2)	(3)
Earnings	20.517	8.127***	6.772***
	(16.147)	(1.949)	(1.603)
Employment	0.812	0.096***	0.066*
	(0.340)	(0.029)	(0.035)
Panel B: Mortality			
10-year Mortality	0.000	−0.026	−0.015
	(0.000)	(0.024)	(0.028)
Panel C: Home Foreclosure			
10-year Foreclosure	0.000	−0.272***	−0.277***
	(0.010)	(0.040)	(0.042)
Panel D: Misc. Outcomes			
401k Contributions	0.221	0.405***	0.298**
	(0.686)	(0.147)	(0.140)
Disability Insurance	0.033	−0.054***	−0.034*
	(0.172)	(0.020)	(0.020)
Sup. Security Income	0.004	−0.209**	−0.156*
	(0.065)	(0.089)	(0.088)
Controls	–	No	Yes
Observations	189100	189100	189100

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection over the sixth through tenth post-filing years. The sample consists of all first-time filers between 1992 and 2000 in the 42 offices that randomly assign filings to judges. The foreclosure sample includes the subset of those filings originating in county by year bins with foreclosure data coverage (N = 84181). Column 1 reports the mean and standard deviation for dismissed filers. Columns 2 - 3 instrument for bankruptcy protection using the reduced form measure of judge leniency described in the text. All specifications control for office by month-of-filing fixed effects, and cluster standard errors at the office level. Column 3 also includes controls for gender, race, age, and the five year average of baseline employment and baseline earnings. All monetary values are expressed in real 2000 dollars divided by 1,000. See Table 1 for additional details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.