The Redistributive Effects of Debtor Protection Laws

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Abstract

Recent literature shows that debtor-friendly regimes may redistribute credit from low-asset to high-asset individuals and thereby affect the performance of their businesses. We use state-level changes in the maximum amount of personal wealth individuals can protect under Chapter 7 personal bankruptcy to investigate the causal effect of debtor protection on income inequality. We find that higher debtor protection reduces the income of lower-income individuals and increases the income of higher-income individuals. The increase in income inequality is larger among the self-employed than among wage earners, and it is due mainly to a growing income gap between skilled and unskilled entrepreneurs. We also find an increase in self-employment rates among skilled individuals and a reduction in the employment rate and relative wage of unskilled workers. We exploit differences across states in banking market structure and find that the increase in income inequality is mitigated by the presence of local banks and amplified by the presence of single-state banks, which are more exposed to the legal reform. These results suggest that debtor protection laws affect income inequality via the credit market.

Keywords: Debtor Protection, Income inequality, Credit Markets.

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1. Introduction

An important economic role of personal bankruptcy is to provide individual debtors with partial consumption insurance. Borrowing enables individuals to smooth their consumption over time. However, adverse events, such as unemployment, illness, or divorce, may reduce their incomes and thus their ability to repay debt. Personal bankruptcy prevents affected individuals from suffering a sharp drop in consumption by forgiving some or all of their debts and by allowing them to keep some or all of their assets. Bankruptcy protection should be particularly valuable for lower-income households, for whom a drop in consumption could mean becoming homeless or being unable to pay their medical expenses. Such consumption insurance, however, comes at a cost.

A recent literature argues that debtor-friendly personal bankruptcy laws reduce credit availability mainly to individuals with fewer assets. In particular, Lilienfeld-Toal and Mookeherjee (2008) study the optimal design of personal bankruptcy law in a general equilibrium setting. Their model predicts that debtor-friendly regimes reduce the debt capacity (i.e., the assets that borrowers can credibly pledge) of low wealth individuals by more than that of high wealth individuals, leading to a redistribution of credit from poorer to richer borrowers.

There is growing evidence in support of this redistribution channel.[1] Gropp, Scholz, and White (1997) first document in a cross-sectional study of consumer credit that the amount of debt held by high asset households is positively related to bankruptcy exemptions (i.e., the maximum amount of personal assets borrowers can protect in bankruptcy), while the amount of debt of low asset households is negatively related to the level of exemptions.[2] More recently,

[1] Most of this literature exploits the fact that personal bankruptcy provisions in the U.S. vary widely across states and across time. In particular, these studies exploit variation in bankruptcy exemption limits, which is the maximum asset value that an individuals who files for Chapter 7 bankruptcy can keep. Section 2 describes U.S personal bankruptcy law.

[2] In contrast to the findings in Gropp, Scholz, and White (1997), a recent study by Brown, Coates and Severino (2014) documents that borrowers' holdings of unsecured debt rise following an increase in exemptions.

Cerqueiro and Penas (2014) study a panel of U.S. startups and find that bank financing to low wealth entrepreneurs falls after an increase in exemptions, while credit card lending increases for high wealth entrepreneurs. They also find that while low wealth owners reduce employment and experience a fall in productivity and in survival rates, high wealth entrepreneurs hire more workers.

This empirical evidence suggests that the more vulnerable individuals could be actually worse-off under more debtor-friendly bankruptcy regimes, since credit constraints may decrease their economic opportunities. In particular, credit constraints could prevent some individuals from obtaining the necessary funds to either start a business or expand an existing one. Debtor protection could therefore remove a ladder that would enable lower-income individuals to increase permanently their income and consumption levels.

The aim of this paper is to paint a more complete picture regarding the potential redistributive effects of debtor protection laws. Specifically, we investigate how changes in state exemptions affect income inequality and assess how the exemptions affect the incomes of particular groups of individuals. Our analysis distinguishes between self-employed individuals and wage earners, and between skilled (high education) and unskilled (low education) individuals. In order to pin down the precise mechanisms through which the exemptions affect income inequality, we also analyze labor market outcomes, such as employment rates and relative wages. Furthermore, we exploit differences across states in banking market structure to understand the role of credit markets. To the best of our knowledge, our study is the first attempt to assess empirically the effects of debtor protection on income inequality.

Our empirical analysis exploits the passage of exemption laws between 1994 and 2006, a period during which several states significantly increased their exemptions levels. The staggering of the exemption laws is particularly important because it allows us to identify the effects on income inequality at different points in time, minimizing the possibility that our results might pick aggregate trends in inequality.[3] We perform several tests to rule out alternative explanations for our findings, such as migration flows and individual state policies. We also show that the timing of the exemption laws is uncorrelated with pre-existing levels of inequality, strongly suggesting that our results are not driven by reverse causality.

Our main finding is that an increase in exemptions leads to higher income inequality in the state. This result holds both across different measures of income inequality, such as measures based on the Gini coefficient, Theil index, and the tails of the income distribution, and after controlling for state and year fixed effects and for other important economic and social determinants of income inequality, such as the unemployment rate, proportion of blacks, real growth of per capita GDP, proportion of high-school drop-outs, and proportion of female-headed households. We also show that the increase in inequality occurs at both ends of the income distribution: a higher exemption level reduces significantly the income of lower-income individuals, while it increases slightly the income of higher-income individuals.

One potential explanation for this result is that high exemptions redistribute credit from lower-income individuals, who tend to have few assets, to high-income individuals, who often accumulate large amounts of assets (Lilienfeld-Toal and Mookherjee, 2008). The redistribution of credit between the two groups of individuals could then affect their relative abilities to generate income as entrepreneurs (Cerqueiro and Penas, 2014). We investigate the plausibility of this mechanism with several empirical tests.

First, we exploit differences across states in banking market structure and find that the exemptions affect income inequality via the credit market. We compute market structure measures based on the presence of local banks (i.e., banks headquartered in the state) and of

[3] Several studies document an upward trend in income inequality in the United States since the 1980s and propose skill-biased technological change as its primary cause. See, for instance, Autor, Katz, and Kearney (2006), and Autor and Dorn (2013).

single-state banks (i.e., banks that operate only in the state). Local banks build stronger ties with the local communities and often lend on the basis on soft information (Berger et al., 2005), reducing moral hazard concerns caused by the higher level of protection granted in bankruptcy to debtors. In contrast, single-state banks are very exposed to any statewide shocks and therefore they should react more strongly to an increase in exemptions than banks that operate both in-state and out-of-state. We find that the presence of local banks mutes the positive effect of the exemptions on income inequality, while the presence of single-state banks amplifies this effect.

Second, we compare the changes in the income distribution of self-employed individuals with those of salaried workers. We find that increases in exemptions lead to significantly higher inequality mainly among the self-employed. Therefore the increase in income inequality we report as our main finding is due to low-income entrepreneurs doing worse following an increase in exemptions, while high-income entrepreneurs are actually better off.

Third, we investigate whether these effects differ systematically between skilled and unskilled individuals (the distinction is based on whether the individual has a college degree). Unskilled individuals are presumably less wealthy and more credit constrained than skilled individuals. As expected, we find that the increase in inequality among the self-employed is due to a growing income gap between skilled and unskilled entrepreneurs. However, we also find evidence of a similar, albeit less pronounced, growing income gap between skilled and unskilled wage workers.

Fourth, we analyze employment rates and wages. We find that a higher fraction of the labor force is self-employed following an increase in exemptions. The increase in self-employment rate is driven by skilled individuals, who are presumably less affected by credit constraints. In contrast, the employment rate of unskilled wage workers falls. We also find that the relative wage of unskilled workers (to skilled workers) decreases significantly. These results suggest that the exemptions reduce the demand for unskilled workers. Supporting this claim, Cerqueiro and Penas (2014) show that less wealthy entrepreneurs reduce their demand for labor following an increase in exemptions.

Overall, our paper provides strong evidence that higher levels of debtor protection increase income inequality. The increase in inequality seems to be driven by the redistribution of credit from the less affluent (unskilled) towards the more affluent (skilled) individuals, which creates an imbalance in economic opportunities between them. Skilled individuals are more likely to become entrepreneurs and to generate higher incomes from their businesses. In contrast, unskilled entrepreneurs, who are more credit constrained, face a reduction in their income and in their ability to expand their businesses, which depresses further the demand for unskilled labor.

A large literature documents widening educational income differentials and rising income inequality since 1980s and relates these patterns to skill-biased technological changes among other factors (e.g., Autor, Katz, and Kearney (2006), Goldin and Katz (2008), and Acemoglu and Autor (2013)). Our study focus on a particular – and well-identified – mechanism that to the best of our knowledge has not been examined in the income inequality literature. In particular, debtor protection seems to distort the market for entrepreneurs in favor of the more affluent individuals and reduce economic opportunities to the less affluent ones. Our results indicate that the recent upward trend in income inequality in the U.S. should not be seen merely as an aggregate phenomenon; rather, it can partially result from legal reforms that provide stronger protection to indebted individuals.

The paper proceeds as follows. Section 2 details the institutional background of U.S. personal bankruptcy law. Section 3 describes the data set and presents our empirical methodology. Section 4 presents the results and Section 5 discusses some robustness tests. Section 6 concludes.

2. U.S. personal bankruptcy law

When an individual files for bankruptcy all collection efforts by creditors must terminate. There are two separate personal bankruptcy procedures in the U.S.: Chapter 7 (a liquidation procedure) and Chapter 13 (a reorganization procedure). Under Chapter 7 filers keep all their future income but they must turn over any unsecured assets they own above the exemption limit in their state of residence.[4] The bankruptcy trustee uses these nonexempt assets to repay debt. Under Chapter 13 debtors can keep all of their assets, but they must propose to creditors a repayment plan. This plan typically involves using a portion of the debtor's future earnings over a five-year period to repay debt.

Before 2005 debtors were allowed to choose between Chapters 7 and 13. Around 70 percent of all bankruptcy filings were made under Chapter 7 (White, 2007). Debtors with few nonexempt assets had an incentive to choose Chapter 7 over Chapter 13. In this way debtors maximized their financial benefit from filing for bankruptcy because they were able to preserve both their current assets and future income. This means that the system also allowed individuals with high incomes to benefit from the generous bankruptcy provisions. [5]

2.1. Bankruptcy exemptions

Under Chapter 7 debtors are allowed to keep certain assets in bankruptcy up to the

[4] Most unsecured debt, including credit card and personal loans are discharged in bankruptcy. In contrast, mortgages and other secured loans cannot be discharged. However, filing for bankruptcy often delays creditors from repossessing the collateral, because they must first obtain the bankruptcy trustee's permission to seize the assets. The probability of bankruptcy should thus reduce the value of both unsecured and secured claims.

[5] The Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) of 2005 sought to prevent borrowers from abusing the bankruptcy regime. This legal reform essentially introduced a *means test* that prevents individuals whose income over the *previous six months* is above the median for their state from filing for Chapter 7 bankruptcy. Higher income debtors with sufficient means can file only for Chapter 13 bankruptcy.

state's predefined exemption limits. A higher exemption level provides additional wealth insurance to debtors because it reduces the asset value that creditors can seize in bankruptcy. Although the Bankruptcy Reform Act of 1978 established a uniform national set of exemptions, it allowed states to opt out and set their own exemption levels. About three quarters of the states opted out (Hynes et al., 2004). As a result, exemption limits vary widely across states.[6]

There are several categories of asset exemptions. The most important is the homestead exemption, which provides protection for equity in the debtor's family residence. The homestead exemption varies from a few thousand dollars to unlimited. Lower exemption amounts are also available for various other types of personal property, such as clothing, furniture, cattle, guns, and motor vehicles. Many states offer wildcard exemptions that allow debtors to retain any personal property up to a specified dollar amount. The types of personal assets specified in the law vary considerably across states and many of these assets have unspecified exemption amounts. It is therefore infeasible to include all personal assets specified in these various state laws. Similar to Gropp et al. (1997) and Cerqueiro and Penas (2014), our measure of personal property exemptions includes only assets that have specific dollar amounts in most states: jewelry, motor vehicles, cash and deposits, and the wildcard exemption. In our empirical analysis we use a measure of state exemptions that combines the homestead exemption and the personal property exemptions.

2.2. State laws amending bankruptcy exemptions

Between 1994 and 2006 several states enacted laws that increased their exemption levels. These laws can dictate an increase in either the homestead exemption or the personal property exemptions, or in both. In most cases the same law amends the exemption limits for

[6] Several states allow their residents to choose between the state and the federal exemptions. In these cases we selected the option that grants the claimant the highest exemption level. In some states, married couples are allowed to double the amount of the exemption when filing for bankruptcy together (called "doubling"). We have doubled all amounts except in those cases where bankruptcy law explicitly prohibits doubling.

various assets (e.g., homestead and motor vehicle). Table 1A shows that many states changed their exemption levels during the sample period. Moreover, some states have raised exemptions more than once (e.g., Arizona in 2001 and 2004). Table 1B shows that there is wide variation in the magnitude of the exemption changes.

3. Data and methodology

3.1. Data and variables

In this paper, we use three sets of variables: measures of inequality based on income distribution data, bankruptcy exemptions, and other state-level variables.

3.1.1. Income distribution data

We use the March Supplement of the Current Population Survey (CPS) to obtain data on income distribution. The CPS is a repeated annual survey of more than 60,000 households across the United States. The CPS is a representative sample of the U.S. population, but it does not track individuals over time. We obtain from the CPS data on total income, employment status, years of education, as well as demographic characteristics, such as race and gender. We use the sampling weights reported in the CPS in all our analyses.

Our sample construction follows common practice in the income inequality literature that uses CPS data.[7] The sample focus on civilians in the age range of 25 to 64 years who have non-negative income, and excludes individuals with missing observations on key variables, such as demographics and education, individuals with income below the 1st or above the 99th percentile of the income distribution, individuals who have zero income and live in households with zero or negative income from all sources of income, people living in group quarters, and individuals with zero or missing sampling weights. In robustness checks we show that our results are robust to changing or relaxing these standard practices.

[7] See, for instance, Beck et al. (2010).

We construct four measures of income distribution for each state and year over the period 1994 to 2006. Having different measures of inequality is important for three reasons. First, the measures complement each other, as they encompass alternative definitions of income inequality and thus have different interpretations. Second, only particular measures (see below) allow us to study income inequality across different subgroups. Third, it tests the robustness of our findings. We describe all inequality measures used in Appendix Table 1.

Our first measure of inequality is the Gini coefficient of income distribution, which equals zero when there is perfect equality and equals one when one individual receives all the income. We use both the natural logarithm of the Gini coefficient and its logistic transformation, which we refer to as logistic Gini. The logarithmic transformation of the Gini coefficient removes the floor and makes the measure upper-bounded at zero. The advantage of using the log of Gini is that it allows us to interpret coefficients relative to this variable as percentage changes. The logistic transformation removes both the floor and the ceiling of the original variable, implying that the logistic Gini ranges from minus infinity to plus infinity.

Our second measure of inequality is the Theil index, which equals zero in case of perfect income equality, and equals the natural log of the number of individuals when all income is concentrated in one individual. Although the Theil index is less straightforward to interpret than the other measures, it has the advantage of being a decomposable inequality measure. In particular, overall inequality can be decomposed into the part of inequality from differences in income between groups and the part of inequality from differences in income within each group. We use this decomposition extensively in our analysis to investigate the sources of income inequality.

Our third and fourth measures of inequality capture differences in income between individuals in the upper and bottom tails of the income distribution. Specifically, we use the difference between the natural logarithm of incomes at the 90th percentile and the 10th percentile (Log(90/10)), and we compute the same difference between the 75th percentile and the 25th percentile (Log(75/25)). Although these inequality measures do not exploit the entire distribution of income, they are robust to outliers in the upper tail of the distribution.

We use data from 1994 to 2006 (13 years) and for 50 states plus the District of Columbia, which gives us a total of 663 observation. In Table 2 we present descriptive statistics for all measures of income inequality.[8]

3.1.2. Bankruptcy exemptions

We hand-collect data on personal bankruptcy exemptions for each state and year from individual state legal codes. Our main variable of interest, *State exemptions*, equals the sum of the homestead exemption and the personal property exemptions in the state.[9] In robustness tests we use alternative measures, such as including only the homestead exemptions or log-transforming the exemption variables. Table 1A describes the timing of the exemption laws and Table 1B shows the distribution of the increase in exemption values.

3.1.3. Banking variables

We compute characteristics of the banking market using information from the Summary of Deposits (SOD). The SOD contains data on deposits held by individual bank branches of all FDIC-insured financial institutions. For each bank branch in the sample we retain its parent company (a bank or a bank holding company) and the location of both entities. We use this information to compute two sets of variables. First, we create measures of the proximity between banks and the local communities. We define a bank branch to be *local* when the branch is located in the same state as the parent company. For each state we compute the *% Local Branches* as the fraction of bank branches in the state that are *local*. Following the

[8] In the Appendix Table 2 we also report three types of standard deviations for each measure: cross-state, within state, and within state-year. We use these standard deviations to assess the economic magnitude of our results.

[9] Section 2 describes the different types of bankruptcy exemptions.

same procedure, we also compute for each state the *% Local Deposits* as the fraction of deposits in *local* bank branches.

Second, we create measures of the exposure of the banks' portfolios to state risk. We define a parent company as *single-state* when all of its affiliated branches (and deposits) are located in the same state. For each state we compute the *% Single-state Branches* as the fraction of bank branches owned by *single-state* parent companies. In the same way, we also compute for each state *% Single-state Deposits* as the fraction of deposits in branches affiliated with *single-state* banks or bank holding companies.

3.1.4. Other state variables

In all our regressions we control for time-varying state level variables that could be correlated with our income inequality measures. From the U.S Department of Commerce we obtain the growth rate of the per capita Gross State Product, and from the Bureau of Labor Statistics we collect state unemployment rates. We use these variables to control for changing state economic conditions.

We also control for several time-varying demographic characteristics, which we compute using the CPS data and aggregate for each state. These include the proportion of female-headed households, the proportion of blacks, and the percentage of high-school dropouts. In robustness tests we also control for migration flows between states using data from the Statistics of Income Division (SOI) of the Internal Revenue Service (IRS), which keeps records of all individual income tax forms filed in each year.

3.2. Empirical methodology

Our baseline panel regression model is:

$$y_{st} = \alpha_s + \alpha_t + \beta Exemptions_{st} + \delta Controls_{st} + \varepsilon_{st},$$

where s indexes state, t indexes time, y_{st} is a measure of income distribution in state s at time t, α_s and α_t are state and year fixed effects, *Exemptions* is the exemption amount in state s at time t, *Controls* are state-level control variables, and ε is an error term.

The year fixed effects control for aggregate changes in income inequality. The state fixed effects control for all time-invariant heterogeneity at the state level. Therefore, these fixed effects ensure that our identification of the exemptions effect comes entirely from within state changes in exemption levels.

The coefficient β measures the effect of the exemption laws on income distribution. Two distinctive features of our empirical setting improve the identification of this effect. First, the regression model accounts for the fact that we have several exemption laws staggered during our sample period. Consequently, our "control" group is not restricted to states that never raised exemptions. The regression model above implicitly takes as the control group all states not changing exemptions at time t, even if they changed exemptions before or will change exemptions later on. Second, the regression model exploits variation in the dollar amounts by which exemption limits are amended. The model implicitly assumes that the effect of an exemption law increases proportionally with the size of the limit change. The variation in the intensity of the "treatment effect" provides better identification than the standard binary treatment outcome (i.e., whether a legal change occurred or not). Finally, to account for potential serial correlation in the error term within states, we cluster standard errors at the state level.[10]

We also examine the dynamics of the relationship between changes in exemptions and the distribution of income. To this end, we compute the year-by-year estimates of the effect of

[10] In robustness tests we exploit alternative methods of computing the standard errors of our main estimates.

changing exemptions on our measures of income inequality. We focus on an 8-year window around the passage of the laws. The regression we estimate is:

$$y_{st}=\alpha_s+\alpha_t+\beta_1 L_{st}^{\leq -4}+\beta_2 L_{st}^{-3}+\beta_3 L_{st}^{-2}+\beta_4 L_{st}^{-1}+\beta_5 L_{st}^{+1}+\beta_6 L_{st}^{+2}+\beta_7 L_{st}^{+3}+\beta_8 L_{st}^{\geq +4}+\delta Controls_{st}+\varepsilon_{st},$$

where the dummy variables L^k indicate whether the state will increase its exemption level in *k* years (for negative *k*) or already increased its exemption level *k* years ago (for positive *k*). The indicators $L^{\leq -4}$ and $L^{\geq +4}$ also equal one if the state either will change exemptions in more than four years or changed exemptions more than four years ago, respectively. The omitted category is the year of the exemption change, implying that all coefficients are relative to this reference year. As in the baseline regression model, we include state and year fixed effects as well as several control variables.

4. Results

4.1. Exemptions and income inequality

Our empirical analysis rests on the assumption that the passage of the exemption laws is unrelated to the distribution of income. To assess the plausibility of this assumption, we investigate the relationship between the timing of the exemption laws and pre-existing income inequality. Figure 1 provides some reassuring evidence. It shows that the pre-law averages of both the log of the Gini coefficient (Panel A) and changes in the Gini coefficient (Panel B) appear to be unrelated to subsequent changes in exemptions.[11] We also use duration analysis to investigate more formally whether income inequality can predict the timing of subsequent exemption changes, holding other factors constant. The results (not shown) corroborate the

[11] The two Gini measures shown are year-demeaned. More specifically, we first regress each of the two measures on a set of year dummies and use the corresponding residuals in the analysis.

graphical evidence in Figure 1 that current income inequality levels do not predict the timing of the exemption laws.[12]

In Table 3 we study the effect of exemptions on income inequality. We find that an increase in exemptions significantly increases income inequality. This result is statistically significant across all five measures of inequality and holds after controlling for state and year fixed effects and for several economic and demographic state characteristics. These characteristics include the proportion of blacks, the real growth rate of per capita GDP, the unemployment rate, the proportion of high-school drop-outs, and the proportion of female-headed households.

The estimated effects are also economically relevant. For instance, an increase of $100,000 in state exemptions leads to a 1.1% increase in the logistic Gini. To assess the economic relevance of this result, we compare the coefficient estimate to the demeaned standard deviation of the logistic Gini, which we compute after accounting for state and year effects. The standard deviation is 4.4% (see Appendix Table 2), indicating that the exemptions explain about 25% of the variation in income inequality relative to state and year averages.

We also find that several of our time-varying control variables are significant predictors of income inequality. As expected, a higher unemployment rate, a higher proportion of blacks, or a higher proportion of high school dropouts all lead to an increase income inequality. In turn, a higher per capita GDP growth reduces inequality, but the effect is only significant in one specification.

Next, we analyze the full dynamic response of income inequality to the exemption laws. Figure 2 plots year-by-year coefficient estimates and 95% confidence intervals of the effect of the exemptions on the logistic Gini using an 8-year window around the passage of the laws. As

[12] We do not present the results of the duration models for brevity, but they are available upon request.

in the previous regressions, we control for state and year fixed effects and for the same set of economic and demographic state variables. Standard errors are clustered at the state level.

Several features of Figure 2 merit attention. First, the graph confirms our main result that there is a significant increase in income inequality following the exemption laws. Second, the graph corroborates our previous findings (see Figure 1) that the exemptions do not seem to be amended in response to changes in income distribution. The coefficient estimates for all years preceding the exemption laws are economically small and statistically insignificant, showing that the increase in income inequality post-dated (and did not precede) the exemption laws. The timing evidence thus corroborates our empirical strategy and speaks to a causal interpretation of our results. Third, the adjustment in income inequality depicted in Figure 2 seems plausible because it is not sudden. Instead, the estimates indicate a small increase in inequality one year after the law, which is only marginally significant. The increase in inequality becomes larger and statistically significant at the 5% level in the second year after the law change, persisting after that.

4.2. The winners and the losers

The fact that increases in exemptions lead to an increase in income inequality raises the question of how the distribution of income is actually changing. Are lower-income individuals becoming poorer or higher-income individuals becoming richer? Or are both happening at the same time? To answer these questions, we slice the distribution of income into 20 percentiles and run separate regressions of the logarithm of total income on exemption levels, controlling for state and year fixed effects and for the time-varying state variables reported in Table 3. Figure 3 depicts the coefficient of the exemptions variable for the different income percentiles (5^{th}, 10^{th}, 15^{th},…, 95^{th}). Dark bars indicate that the estimates are statistically significant at the 5% level. The figure shows that increasing exemptions reduces the incomes of individuals at the bottom of the income distribution and raises the incomes of individuals at the top of the distribution. We note, however, that the drop in income for the lower-income individuals is substantially larger than the modest increase in income experienced by the high-income earners.

One potential explanation for this result is that high exemptions redistribute credit from lower-income individuals, who tend to have few assets, to high-income individuals, who often accumulate large amounts of assets (Gropp et al., 1997; Lilienfeld-Toal and Mookherjee, 2008; Cerqueiro and Penas, 2014). The credit constraints faced by lower-income individuals could deteriorate their economic opportunities, such as the possibility to open a business or operate an existing one. Consistent with this argument, Cerqueiro and Penas (2014) find that increases in exemptions have negative real effects on low-wealth entrepreneurs. These entrepreneurs reduce their labor force, experience a fall in their productivity and also in their survival rates. Consistent with the redistribution effect above, they also find that high wealth entrepreneurs increase their labor force.

Next we investigate the extent to which these mechanisms can explain the increase in income inequality we find. The mechanisms we propose rest on three conjectures that we test below. The first conjecture is that the exemptions affect individuals via the credit market. To test this premise we exploit differences across states in banking market structure. The second conjecture is that if the exemptions affect entrepreneurs' opportunities to generate an income, then the increase in inequality should be driven predominantly by entrepreneurs. We investigate the plausibility of this argument by comparing the changes in the income distribution of self-employed individuals with those of salaried workers. The third conjecture is that the less affluent individuals are those who should face a reduction in their incomes. Since we do not have information about wealth, we compare individuals with high versus low levels of education.

4.3. The credit market channel

In this subsection, we investigate the role of credit supply. A higher exemption level creates moral hazard and adverse selection problems, because it makes individuals more likely to file for personal bankruptcy and because it reduces the amount of assets creditors can seize in bankruptcy. In turn, banks may react by reducing credit availability.

To test whether the mechanism through which the exemptions affect income inequality is the credit market, we exploit differences across states in banking market structure. We analyze two dimensions of banking markets. The first is the existence of close ties between local banks and the community, which we measure with the variables *% Local branches* and *% Local deposits.* Local banks enjoy a local informational advantage that distance erodes. This informational advantage enables local banks to make loans on the basis of customer relationships and other soft information, such as the reputation of a borrower in the community.[13] The presence of local banks should therefore attenuate the response of credit markets to an increase exemptions.

The second dimension is the exposure of local banks to state risk, which we measure with the variables *% Single-state branches* and *% Single-state deposits*. To illustrate these measures, consider a bank that operates only in its local market. This single-state bank is particularly vulnerable to any statewide changes in regulation, because its loan portfolio is geographically concentrated. An increase in bankruptcy exemptions, in particular, reduces the credit quality of loans granted in this state, since it raises the incidence of defaults and lowers recovery rates. In order to limit losses, the bank is likely to redistribute credit from low asset borrowers to high asset borrowers within the state. The other local banks that have out-of-state operations hold more geographically diversified loan portfolios and should therefore react less

[13] A large literature argues that the availability of soft information is particularly valuable in the presence of severe information asymmetries. See, for instance, Petersen and Rajan (2002), Berger et al. (2005), and Agarwal and Hauswald, (2010).

to the increase in exemptions. For this reason, the presence of single-state banks should amplify the response of credit markets to an increase in exemptions.

We investigate the role of banking market structure in Tables 4A and 4B. In Table 4A we extend the baseline model of Table 3 by adding the banking market variables based on branches (% Local branches and % Single-state branches) as well as interactions of these variables with the variable Exemptions. In Table 4B we run similar regressions using the alternative banking market variables based on deposits (% Local deposits and % Single-state deposits). We obtain two main results that are line with our expectations.

We find that the presence of local banks *mutes* the positive effect of the exemptions on income inequality, while the presence of single-state banks *amplifies* the positive effect of the exemptions on income inequality. The estimates displayed in Column 1 of Table 4A indicate that increasing the *% Local banks* by 1 standard deviation (holding the *% Single-state banks* at its mean value) reduces the effect of exemptions on income inequality from 0.011 (the effect reported in Column 1 of Table 3) to 0.007. In contrast, a similar increase in the *% Single-state banks* (holding the *% Local banks* at its mean value) increases the effect of exemptions on inequality from 0.011 to 0.014. This result holds across most measures of income inequality and for the two alternative measures of banking market structure (i.e., branches or deposits). Importantly, this finding provides preliminary evidence that the exemptions seem to be affecting income inequality via the credit market.

4.4. Self-employed and salaried workers

In this subsection we investigate the effect of the exemptions on the incomes of the self-employed and salaried workers. We decompose the effect of exemptions on income inequality into two parts: the part accounted for by an increase in the income gap *between* the self-employed and the wage earners, and the part accounted for by an increase in income inequality *within* the two groups. The Theil index is easily decomposable into subgroups of the population and therefore we select this inequality measure for this decomposition exercise. Using the Theil index (rather than its log), we decompose income inequality into the within and between components for each state and year. Then, we estimate the impact of exemptions on each of these components, controlling for state and year fixed effects and for our time-varying state variables. We report the results in Table 5. Column 1 shows that the effect of the increase in exemptions on income inequality is positive and significant. In Columns 2 and 3 we investigate how much of the increase in total inequality is accounted for by the within and between components, respectively.[14]

The results indicate that the increase in inequality is driven by an increase in inequality only within groups. The answer to the question of which of the groups actually drives the increase in inequality lies in Columns 4 and 5, which report the effects on inequality within the self-employed and within salaried workers, respectively. We find that increases in exemptions lead to significantly higher inequality among both the self-employed and salaried workers. However, the increase in inequality among the self-employed is four times larger than the increase in inequality among salaried workers, indicating that the increase in income inequality we found is driven mainly by the entrepreneurs.[15]

These results support our conjecture that higher exemptions create an imbalance in economic opportunities among entrepreneurs. The question that arises again is who benefits and who loses from the higher exemptions. If the exemptions lead to a redistribution of credit from poorer to richer individuals, as predicted in Lilienfeld-Toal and Mookherjee (2008) and documented in in Gropp et al. (1997) and Cerqueiro and Penas (2014), we should find that the poorer entrepreneurs generate lower incomes while the richer entrepreneurs are able to maintain or even increase their earnings. Unfortunately, we do not have information about

[14] Note that the sum of the estimates in Columns 2 and 3 equals the estimate in Column 1.

[15] Below we also discuss possible explanations for the increase in income inequality among wage workers.

wealth. For this reason, we compare individuals with high versus low levels of education, which we call *skilled* and *unskilled* workers. Skilled workers have more than 12 years of education (that is, they attended college), while unskilled workers have 12 years of education or less. We presume that skilled workers are less financially constrained than unskilled workers.[16]

In Table 6 we decompose the Theil index of income inequality using the same procedure as before. In Panel A we divide the group of self-employed individuals into skilled and unskilled workers. In Panel B we perform the same division for the group of salaried workers. As before, we report in the first column the total estimates of exemptions on inequality. Columns 2 and 3 indicate how much of the increase in total inequality is accounted for by changes within and between education groups. Columns 4 and 5 present, respectively, estimates of the impact on income inequality within unskilled and skilled workers.

Column 3 in Panel A shows that about 95% (0.0078/0.008) of the increase in inequality within the self-employed is accounted for by an increase in inequality between skilled and unskilled entrepreneurs. This finding corroborates our claim that increasing exemptions makes entrepreneurs who face less severe financing constraints better off, at the same time that it makes less privileged entrepreneurs struggle. The remaining 5% comes from an increase in inequality among skilled entrepreneurs (Column 5).

Panel B of Table 6 shows that most of the increase in inequality within salaried workers is also due to the increase inequality between skilled and unskilled salaried workers, albeit the estimated magnitude is smaller than in Panel A. More specifically, Column 3 shows that about

[16] Several studies document substantial returns to college education in the United States (for a recent review of this literature, see Autor and Acemoglu, 2011). For instance, the average college wage premium for full-time workers in the US was in 2000 higher than 90%. Highly educated individuals enjoy substantially higher earnings that allows them to accumulate larger amounts of assets. But even if we control for the individual's level of income, education could be still correlated with wealth due to the intergenerational persistence of wealth (Charles and Hurst, 2003). That is, highly educated individuals are more likely to have wealthier parents who can invest in their education.

two-thirds (0.0012/0.002) of the increase inequality is due to differences between education groups, while one-third of the increase is due to a higher inequality among unskilled salaried workers (Column 4).

One possible interpretation for the results in Panel B is that unskilled wage workers are worse off simply because the demand for unskilled labor is lower. This mechanism is consistent with the evidence in Cerqueiro and Penas (2014), who show that less wealthy entrepreneurs reduce their demand for labor following an increase in exemptions. In the next section we evaluate the plausibility of this argument by studying the effect of exemptions on labor market outcomes.

4.5. Exemptions and the labor market

4.5.1 Exemptions and employment rates

Table 7 shows the results of the effect of exemptions on employment rates. Our dependent variables are expressed in logs and all regressions include state and year fixed effects and the time-varying state controls shown in Table 3. Column 1 shows that higher exemptions lead to an increase in total employment rates. The estimated effect indicates that a $100,000 increase in exemption increases the employment rate by 0.2%.

The subsequent columns break down this effect by employment type (self-employed versus salaried workers) and by education level (skilled versus unskilled workers). The results show that the positive effect of the exemptions on employment is driven by an increase in the employment rate of skilled self-employed workers (Column 3), which more than compensates for a decrease in the employment rate of unskilled salaried workers (Column 7).

These results are consistent with the redistribution effects documented Cerqueiro and Penas (2014). Skilled individuals, who presumably are less financially constrained, benefit from the increase in exemptions and become disproportionally more likely to be self-employed (see also Fan and White, 2003, and Armour and Cumming, 2008). In contrast, unskilled individuals suffer a significant reduction in employment. If the reduction in employment reflects lower demand for unskilled workers, we should see also a decline in wages and working hours for this group. We investigate these hypotheses below.

4.5.2. Exemptions and the demand for unskilled labor

We use data from the Outgoing Rotation Groups CPS files to investigate whether the exemptions affect the relative wages and relative working hours of unskilled workers vis-à-vis skilled workers. Following Beck et al. (2010), we compute the relative wage and relative working hours of unskilled workers after controlling for several well-known determinants of wages, such as gender, race, and experience, and after allowing the returns to these characteristics to vary over time. [17]

We test whether the exemptions affect the relative wages and relative working hours of unskilled workers with the following regression:

$$r(w)_{ist}=\alpha+\beta Exemption_{st}+X_{st}+\gamma_t+\delta_s+\varepsilon_{ist},$$

where $r(w)_{ist}$ is the log of either real relative wages or weekly working hours of unskilled worker i in state s in year t. As before, we include state and year fixed effects, as well as our time-varying state variables reported in Table 3.

Table 8 reports the estimated effect of exemption changes on the relative wages and relative working hours of unskilled workers. The table shows that the relative wage of unskilled workers falls after an increase in exemptions. The effect is statistically significant at the 1% level. For the relative working hours we obtain a negative, albeit insignificant effect of the exemptions.

Overall, the evidence is consistent with an increase in exemptions affecting not only the entrepreneurs but also the salaried workers. A potential explanation is that tighter credit

[17] We explain the methodology in detail in the Appendix.

constraints put less wealthy (unskilled) entrepreneurs at a disadvantage (vis-à-vis skilled individuals). Furthermore, if unskilled workers are more likely to be hired by unskilled entrepreneurs, then their wages could decrease as a consequence of a fall in entrepreneurs' demand for their labor.

5. Robustness tests

5.1. Controlling for migration flows

One important concern is that the increase in inequality may be due to migration flows. For instance, Brinig and Buckley (1996) find that generous personal bankruptcy laws attract high human capital debtors who seek a fresh start from out-of-state creditors. One could therefore argue that an increase in exemptions attracts high-income migrants to the state, leading to an increase in income inequality. To address this concern, we collect information on state migration flows from the Internal Revenue Service (IRS) and create two measures of immigration: the number of returns filed by movers and the number of tax exemptions claimed by movers.

We perform two separate tests using these alternative measures of immigration. First, we run similar regressions as in Table 3, but controlling for immigration flows. The results are shown in Columns 1 to 5 of Table 9. In Panel A we control for the log of the number of returns filed by movers, and in Panel B we control for the log of the number of tax exemptions filed by movers. The coefficients on the immigration variables are always statistically insignificant and, more crucially, the effect of the exemptions on income inequality remains virtually unchanged across all specifications. Second, we run regressions with these immigration proxies on the left hand side to investigate whether an increase in exemptions leads to higher migration flows to that state. Similar to the regressions in Table 3, we include state and year fixed effects, and the same set of state time-varying controls. The results are shown in Column 6 of Table 9.

The coefficients on the exemptions variable are economically small and statistically insignificant, confirming that migration flows are not a confounding factor in our analysis.

5.2. Influential states

Table 1A shows that some states raised exemptions more than once, while Table 1B shows that some states experienced very large changes in exemption limits. We therefore worry that our results might be driven by a few states. To investigate this issue, we run 51 regressions (similar to those displayed in Table 3) excluding one state at a time. Figure 4 plots the coefficient estimates and 95-percent confidence intervals of the effect of exemptions on the logistic Gini. If a handful of states were driving our results, dropping any of these influential states should substantially affect our findings. As the figure shows, all of the estimates are statistically significant and the magnitudes are quite stable no matter which state is dropped, indicating that the results are not driven by one state.

5.3. Alternative exemption measures

The main explanatory variable of interest in all our regressions is *Exemptions*, which equals the sum of the homestead and the personal property exemptions. In addition, the functional form used imposes a linear effect of this variable on income inequality. In Table 10 we test alternative measures of the exemptions, using as dependent variables the logistic Gini and the log of Gini. In Columns 1 and 5 we replicate the baseline results of Table 3, which uses total exemptions. In Columns 2 and 6 we use the log of total exemptions. In Columns 3 and 7 we use the homestead exemptions, which in most states is the most important type of exemption. In Columns 4 and 8 we consider the log of homestead exemptions. As shown in Table 10, our results are robust to alternative definitions of the exemption variable.

5.4. Other robustness tests

5.4.1. Other robustness tests

In our analysis we excluded individuals with incomes in the bottom or top 1% of the income distribution. However, one might wonder to what extent our result depend on these exclusions. Therefore, we construct our inequality measures in four different ways: (1) including the entire income distribution (2) excluding the 1st percentile (3) excluding the 99th percentile (4) excluding the first and the 99th percentile (this is our baseline specification). We report the results for the logistic Gini and log Gini in Table 11. The results we obtain are similar across all specifications shown.

5.4.2. Age groups

We have used the sample of individuals between 25 and 64 years old to construct our inequality measures. In this section, we do the same analysis with different age groups. Specifically, we use three additional age groups: 18-64, 18-54, and 25-54. For each case, we compute our measures of inequality and then run similar regressions as in Table 3. The results for each age group are reported in Table 12 in separate panels. Again, the results stay statistically significant and the magnitudes remain similar as before.

5.4.3. Standard errors

One might be concerned about the robustness of our results with respect to the way we estimate standard errors. In our baseline specification we cluster standard errors at the state level. In addition to the baseline method, we compute standard errors in two alternative ways: bootstrapped standard errors and SUR standard errors. Again we run our baseline regressions of Table 3, but report all three standard errors for the coefficients. The results are shown in Table 13 and indicate that clustering the standard errors at the state level provides conservative estimates.

5.4.4. Excluding the unemployed

In this section, we investigate if our results are driven by the unemployed. We construct our measures of inequality dropping all unemployed individuals from the sample. Then, we run similar regressions as in Table 3 using these new measures of income inequality. The results are displayed in Table 14 and show that most of the effect of the exemptions on income inequality is due to changes in the incomes of employed individuals.

6. Conclusion

We study the effect on the income distribution of changes in state bankruptcy exemptions. We find that an increase in exemptions leads to a significant increase in income inequality. The increase in inequality occurs at both ends of the income distribution: a higher exemption level reduces significantly the income of lower-income individuals, while it increases the income of higher-income individuals.

We dig deeper and investigate the effect of exemptions on the income of different population groups. The increase in inequality we found is driven mostly by the self-employed. We find that the increase in inequality among the self-employed is due a growing income gap between skilled and unskilled entrepreneurs. It thus appears that the exemptions create an imbalance in economic opportunities among entrepreneurs, consistent with the redistribution effects proposed in Lilienfeld-Toal (2008) and found in Cerqueiro and Penas (2014).

We also find evidence of a growing income gap between skilled and unskilled wage workers, an effect that seems to result from a reduction in the labor demand for unskilled workers. The fact that the relative wage of unskilled workers (to skilled workers) falls following an increase in exemptions supports this view.

Finally, we provide evidence on the credit channel as an important mechanism through which the increase in exemptions affects income inequality. We find that the effect of exemptions on inequality is amplified by a strong presence of banks that operate only in the affected state and are therefore very exposed to the state shock.

Overall, our paper provides strong evidence that a higher level of debtor protection increases income inequality by increasing the income of skilled entrepreneurs and by reducing the demand for unskilled labor and the wages of this group. This evidence indicates that more debtor-friendly bankruptcy regimes may redistribute welfare towards the most privileged individuals.

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Table 1A. States changing bankruptcy exemption levels, 1995-2006.

Year	States
1995	CA, ME, NH, NV
1996	MN, VT, WV, WY
1997	MT, NE, NH, NV, UT
1998	HI, MI, MN, NJ, PA, RI, SD, WA
1999	AK, DC, ID, MT, RI, UT, WA
2000	CO, DC, LA, MA
2001	AZ, GA, HI, ME, MI, MT, NJ, PA, RI
2002	NH, WA, WV
2003	CA, ME, MO, NV
2004	AK, AZ, HI, MA, MD, MI, MN, MO, NH, NJ, PA, RI
2005	DE, IN, KY, NV, NY, OK
2006	IA, ID, IL, MN, NC, OR, RI, SC

Table 1B. Distribution of changes in state exemption levels, 1995-2006.

Exemption change	States
< $5,000	MN, WY, HI, MI, MN, NJ, PA, RI, SD, WA, DC, MT, HI, MI, NJ, PA, ME, HI, MI, MN, MO, NJ, PA, MN, CA
[$5,000-$20,000)	CA, ME, NH, WV, NE, NH, NV, UT, AK, ID, WA, LA, AZ, GA, MO, AK, MD, OK, IA, OR
[$20,000-$50,000)	NV, MT, UT, CO, ME, NH, IN, KY, IL, NC, ME
[$50,000-$100,000)	VT, RI, MT, RI, NV, AZ, RI, NY, ID, SC, NV
>= $100,000	DC, MA, MA, NH, DE, NV, RI

Table 2. Descriptive statistics

The table shows descriptive statistics of the variables used in the paper. We use five measures of income inequality: a logistic transformation of the Gini coefficient, the log of the Gini coefficient, the log of the Theil index, the log ratio of the 90th and 10th percentiles of the income distribution, and the log ratio of the 75th and 25th percentiles of the income distribution. We use total personal income and sampling weights in the Current Population Survey (CPS) to calculate each inequality measure for each state in each year. The sample includes 51 states and the sample period is 1994 to 2006. Data on proportion blacks and female-headed households are calculated from the CPS. Data on real per capital GDP are obtained from the Bureau of Economic Analysis. Data on unemployment rate are from Bureau of Labor Statistics. The proportions of different employment types are calculated from CPS data. Data on immigration flows between states is from the Statistics of Income Division (SOI) and data on banking market structure is from the Summary of Deposits (SOD).

Variable	Mean	St. Dev.	Min	Perc. 10	Perc. 25	Perc.50	Perc.75	Perc. 90	Max
Logistic Gini	-0.30	0.07	-0.52	-0.40	-0.35	-0.30	-0.25	-0.21	-0.08
Log Gini	-0.86	0.04	-0.99	-0.91	-0.88	-0.85	-0.83	-0.80	-0.73
Log Theil	-1.18	0.08	-1.45	-1.29	-1.23	-1.17	-1.11	-1.07	-0.94
Log 90/10	2.47	0.19	1.86	2.23	2.33	2.46	2.57	2.72	3.25
Log 75/25	1.16	0.11	0.80	1.03	1.09	1.16	1.24	1.30	1.53
Unemployment rate	4.85	1.20	2.30	3.30	4.00	4.80	5.60	6.50	8.70
Proportion blacks	0.10	0.11	0.00	0.01	0.02	0.06	0.14	0.26	0.65
The real growth rate of GDP per capita	0.02	0.03	-0.10	-0.01	0.00	0.02	0.03	0.05	0.18
Proportion drop-outs	0.10	0.03	0.03	0.06	0.07	0.09	0.12	0.15	0.21
Proportion female-headed households	0.40	0.07	0.20	0.31	0.36	0.41	0.46	0.49	0.59
Proportion employed	0.96	0.01	0.92	0.95	0.95	0.96	0.97	0.98	0.99
Proportion self-employed	0.11	0.03	0.05	0.08	0.09	0.11	0.13	0.15	0.21
Proportion skilled self-employed	0.07	0.02	0.02	0.04	0.05	0.07	0.08	0.10	0.14
Proportion unskilled self-employed	0.04	0.02	0.01	0.03	0.03	0.04	0.05	0.06	0.10
Proportion salaried workers	0.88	0.03	0.77	0.84	0.86	0.88	0.90	0.91	0.94
Proportion skilled salaried workers	0.53	0.07	0.38	0.46	0.49	0.53	0.57	0.60	0.84
Proportion unskilled salaried worker	0.34	0.07	0.04	0.26	0.30	0.35	0.39	0.43	0.51
Log (# returns filed by movers)	10.63	0.83	8.98	9.48	9.98	10.64	11.26	11.66	12.59
Log (# exemptions filed by movers)	11.28	0.84	9.57	10.12	10.62	11.34	11.91	12.30	13.23
Proportion local branches	0.64	0.21	0.08	0.32	0.49	0.68	0.79	0.89	1.00
Proportion local deposits	0.62	0.23	0.04	0.28	0.47	0.64	0.79	0.90	1.00
Proportion single-state branches	0.44	0.18	0.02	0.21	0.31	0.44	0.58	0.66	0.98
Proportion single-state deposits	0.38	0.17	0.01	0.17	0.25	0.37	0.50	0.59	0.99

Table 3. The impact of bankruptcy exemptions on income inequality

The table shows estimates of the impact of state exemption laws on income inequality. State exemptions include the homestead exemption and the personal property exemptions. The measures of income inequality are: (1) the logistic transformation of the Gini coefficient, (2) the natural logarithm of the Gini coefficient, (3) the natural logarithm of the Theil index, (4) the natural logarithm of the ratio of the 90th and 10th percentiles, and (5) the natural logarithm of the ratio of the 75th and 25th percentiles. Income inequality data are from the Current Population Survey (CPS). We use total personal income and the CPS sampling weights to calculate each inequality measure for each state and year. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable:	Logistic Gini	Log Gini	Log Theil	Log 90/10	Log 75/25
	(1)	(2)	(3)	(4)	(5)
Exemptions ($100,000)	0.011***	0.006***	0.012***	0.031***	0.013**
	(0.003)	(0.002)	(0.003)	(0.008)	(0.005)
Proportion blacks	0.545***	0.321***	0.665***	1.067**	0.614**
	(0.194)	(0.112)	(0.224)	(0.485)	(0.285)
Real growth rate of per capita GDP	-0.103	-0.059	-0.095	-0.178	-0.204*
	(0.086)	(0.049)	(0.104)	(0.281)	(0.121)
Unemployment rate	0.011***	0.006***	0.013***	0.041***	0.014***
	(0.003)	(0.002)	(0.004)	(0.011)	(0.005)
Proportion high-school dropouts	0.664***	0.381***	0.781***	0.852*	0.448**
	(0.153)	(0.088)	(0.191)	(0.457)	(0.198)
Proportion female-headed households	0.042	0.024	0.055	-0.050	0.007
	(0.085)	(0.049)	(0.105)	(0.240)	(0.087)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663
R-squared	0.133	0.132	0.170	0.354	0.285

Table 4A. The impact of bankruptcy exemptions on income inequality: The role of banking market structure (branch-level variables).

The table investigates the role of banking market structure using measures based on bank branches. State exemptions include the homestead exemption and the personal property exemptions. The banking market structure variables are from the Summary of Deposits. The measures of income inequality are: (1) the logistic transformation of the Gini coefficient, (2) the natural logarithm of the Gini coefficient, (3) the natural logarithm of the Theil index, (4) the natural logarithm of the ratio of the 90th and 10th percentiles, and (5) the natural logarithm of the ratio of the 75th and 25th percentiles. Income inequality data are from the Current Population Survey (CPS). We use total personal income and the CPS sampling weights to calculate each inequality measure for each state and year. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable:	Logistic Gini	Log Gini	Log Theil	Log 90/10	Log 75/25
	(1)	(2)	(3)	(4)	(5)
Exemptions ($100,000)	0.013***	0.008***	0.014***	0.030***	0.013***
	(0.003)	(0.002)	(0.003)	(0.008)	(0.003)
Exemptions × % Local branches	-0.016**	-0.009**	-0.016**	-0.009	-0.012
	(0.007)	(0.004)	(0.008)	(0.026)	(0.009)
Exemptions × % Single-state branches	0.018**	0.010**	0.017*	0.023	0.023*
	(0.007)	(0.004)	(0.009)	(0.030)	(0.011)
% Local branches	-0.047	-0.026	-0.050	-0.062	-0.060
	(0.032)	(0.019)	(0.039)	(0.088)	(0.038)
% Single-state branches	0.012	0.005	-0.001	0.017	0.028
	(0.043)	(0.025)	(0.053)	(0.122)	(0.039)
State controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663
R-squared	0.147	0.146	0.180	0.357	0.295

Table 4B. The impact of bankruptcy exemptions on income inequality: The role of banking market structure (deposit-level variables).
The table investigates the role of banking market structure using measures based on bank deposits. State exemptions include the homestead exemption and the personal property exemptions. The banking market structure variables are from the Summary of Deposits. The measures of income inequality are: (1) the logistic transformation of the Gini coefficient, (2) the natural logarithm of the Gini coefficient, (3) the natural logarithm of the Theil index, (4) the natural logarithm of the ratio of the 90th and 10th percentiles, and (5) the natural logarithm of the ratio of the 75th and 25th percentiles. Income inequality data are from the Current Population Survey (CPS). We use total personal income and the CPS sampling weights to calculate each inequality measure for each state and year. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable:	Logistic Gini	Log Gini	Log Theil	Log 90/10	Log 75/25
	(1)	(2)	(3)	(4)	(5)
Exemptions ($100,000)	0.013***	0.007***	0.014***	0.034***	0.013***
	(0.003)	(0.002)	(0.003)	(0.007)	(0.003)
Exemptions × % Local deposits	-0.015**	-0.009**	-0.017**	-0.021*	-0.015*
	(0.006)	(0.003)	(0.006)	(0.011)	(0.008)
Exemptions × % Single-state deposits	0.020***	0.012***	0.020***	0.023	0.028***
	(0.007)	(0.004)	(0.007)	(0.018)	(0.010)
% Local deposits	-0.016	-0.009	-0.021	-0.087	-0.042
	(0.026)	(0.015)	(0.030)	(0.066)	(0.034)
% Single-state deposits	0.002	-0.000	-0.004	0.057	0.015
	(0.035)	(0.020)	(0.044)	(0.092)	(0.030)
State controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663
R-squared	0.148	0.147	0.181	0.361	0.301

Table 5. Income Inequality Between- and Within-Employment Groups

This table estimates the impact of state exemption laws on the Theil index of income inequality for the entire sample (Column 1), and separately for the self-employed (Column 4) and salaried workers (Column 5). Columns 2 and 3 decompose the aggregate income inequality index in Column 1 into the within-group and between-group components, respectively. State exemptions include the homestead exemption and the personal property exemptions. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

		Decomposition by employment group		Employment group:	
	Total	Within-Group	Between-Groups	Self-Employed	Salaried workers
	(1)	(2)	(3)	(4)	(5)
Exemptions ($100,000)	0.003**	0.003***	0.000	0.008***	0.002**
	(0.001)	(0.001)	0.000	(0.002)	(0.001)
State controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	663	663	663	663	663
R-squared	0.133	0.170	0.132	0.354	0.285

Table 6. Income Inequality Between- and Within-Employment Groups for Skilled versus Unskilled Workers

The table estimates the impact of state exemption laws on the Theil index of income inequality separately for self-employed workers (Panel A) and salaried workers (Panel B). Column 1 displays the total effects for each employment group. Columns 2 and 3 decompose the aggregate income inequality index in Column 1 into the within-education group and between-education group components, respectively. Columns 4 and 5 compute the effects on income inequality for the unskilled workers and skilled workers, respectively. Unskilled workers are those who have completed at most 12 years of education. Skilled workers are those with 13 or more years of completed education. State exemptions include the homestead exemption and the personal property exemptions. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

		Decomposition by education group		Education group	
	Total	Within-Group	Between-Groups	Unskilled workers	Skilled workers
	(1)	(2)	(3)	(4)	(5)
Panel A: Self-employed workers					
Exemptions ($100,000)	0.008***	0.0004*	0.0078***	0.0005	0.0007***
	(0.002)	(0.0002)	(0.0020)	(0.005)	(0.0002)
Panel B: Salaried workers					
Exemptions ($100,000)	0.002**	0.0006*	0.0012***	0.0004**	0.0006
	(0.001)	(0.0003)	(0.0006)	(0.0002)	(0.0005)
State controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	663	663	663	663	663

Table 7. The Impact of Exemptions on Employment Types

The table estimates the impact of state exemption laws on employment types. All dependent variables are measured in logs. The proportion of self-employed and the proportion of salaried workers are computed as the number of workers in each group over the total labor force. State exemptions include the homestead exemption and the personal property exemptions. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	% Employed	% Self-employed			% Salaried workers		
Sample:	Total	Total	Skilled	Unskilled	Total	Skilled	Unskilled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exemptions ($100,000)	0.002***	0.019***	0.018***	-0.003	-0.001***	-0.0003	-0.008**
	(0.0004)	(0.003)	(0.004)	(0.005)	(0.0005)	(0.002)	(0.003)
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	663	663	663	663	663	663	663
R-squared	0.300	0.147	0.088	0.236	0.193	0.520	0.405

Table 8. The Relative Impact of Bankruptcy Exemptions on Unskilled Workers.

The table estimates the impact of state exemption laws on the log of real hourly wages of unskilled workers relative to skilled workers (Column 1) and on the number of weekly working hours of unskilled workers relative to skilled workers (Column 2). The unit of observation is worker-state-year. Relative wages and relative working hours are calculated after controlling for experience, race, and gender, and after allowing for time-varying returns to these characteristics. Data are from the Outgoing Rotation Groups CPS files. The methodology used in this analysis is explained in detail in the Appendix. The methodological details of this analysis are provided in the Appendix. Unskilled workers are those who have completed at most 12 years of education. Skilled workers are those with 13 or more years of completed education. State exemptions include the homestead exemption and the personal property exemptions. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable:	Relative wage of unskilled workers	Relative hours by unskilled workers
	(1)	(2)
Exemptions ($100,000)	-0.013***	-0.024
	(0.003)	(0.027)
State controls	Yes	Yes
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	842,194	842,194
R-squared	0.022	0.006

Table 9. Bankruptcy Exemption Laws, Immigration, and Inequality

The table estimates the impact of state exemption laws on income inequality after controlling for immigration flows (Columns 1 to 5) and the impact of exemption laws on immigration flows (Column 6). The measures of immigration are the number of tax returns filed by movers (in Panel A) and the number of tax exemptions claimed by movers (in Panel B). Immigration data are from the Internal Revenue Service (IRS). State exemptions include the homestead exemption and the personal property exemptions. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Logistic Gini	Log Gini	Log Theil	Log 90/10	Log 75/25	Log(Immigration)
Panel A: Immigration measured as the number of returns filled by movers						
Exemptions ($100,000)	0.011***	0.006***	0.011***	0.030***	0.013**	0.007
	(0.003)	(0.002)	(0.003)	(0.008)	(0.005)	(0.004)
Log(Number returns by movers)	0.032	0.017	0.031	0.192	0.001	
	(0.039)	(0.022)	(0.046)	(0.118)	(0.055)	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663	663
R-squared	0.134	0.133	0.171	0.359	0.285	0.390
Panel B: Immigration measured as the number of exemptions claimed by movers						
Exemptions ($100,000)	0.011***	0.006***	0.012***	0.030***	0.013**	0.004
	(0.003)	(0.002)	(0.003)	(0.008)	(0.005)	(0.004)
Log(Tax exemptions by movers)	0.030	0.017	0.027	0.154	-0.002	
	(0.037)	(0.021)	(0.045)	(0.107)	(0.048)	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663	663
R-squared	0.135	0.134	0.171	0.358	0.285	0.378

Table 10. The Impact of Bankruptcy Exemptions on Income Inequality: Alternative Exemption Variables

The table shows estimates of the impact of state exemption laws on income inequality using alternative measures of exemptions. State exemptions include the homestead exemption and the personal property exemptions. Homestead is the amount of home equity that is exempt in bankruptcy. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable:	Logistic Gini				Log Gini			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exemptions ($100,000)	0.011***				0.006***			
	(0.003)				(0.002)			
Log (Exemptions)		0.016***				0.009***		
		(0.006)				(0.003)		
Homestead ($100,000)			0.011***				0.006***	
			(0.003)				(0.002)	
Log(Homestead)				0.015**				0.009**
				(0.006)				(0.003)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663	663	663	663
R-squared	0.142	0.139	0.142	0.138	0.141	0.139	0.141	0.138

Table 11. Robustness of the Results to Inclusion of Observations with Outlying Income

The table estimates the impact of state exemption laws on income inequality, measured with the logistic transformation of the Gini coefficient (Columns 1 to 4) and with the natural log of the Gini coefficient (Columns 5 to 8). State exemptions include the homestead exemption and the personal property exemptions. In Columns 1 and 5 we use the entire income distribution to calculate our inequality measures. In Columns 2 and 6 we exclude individuals with real income below the 1st percentile of the income distribution. In Columns 3 and 7 we exclude individuals with real income above the 99th percentile of the income distribution. In Columns 4 and 8 we exclude individuals with real incomes below the 1st percentile or above the 99th percentile of the income distribution. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Logistic Gini				Log Gini			
		Excluding percentiles:				Excluding percentiles:		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	With outliers	1st	99th	1st and 99th	With outliers	1st	99th	1st and 99th
Exemptions ($100,000)	0.009**	0.009**	0.008***	0.011***	0.005**	0.005**	0.005***	0.006***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.18	0.18	0.19	0.13	0.18	0.18	0.2	0.13
Observations	663	663	663	663	663	663	663	663

Table 12. Robustness of the Results to Using Different Age Groups

The table estimates the impact of state exemption laws on income inequality for different age groups. State exemptions include the homestead exemption and the personal property exemptions. Homestead is the amount of home equity that is exempt in bankruptcy. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Logistic Gini	Log Gini	Log Theil	Log 90/10	Log 75/25
Panel A: Ages 25-64					
Exemptions ($100,000)	0.011***	0.006***	0.012***	0.031***	0.013**
	(0.003)	(0.002)	(0.003)	(0.008)	(0.005)
Panel B: Ages 18-64					
Exemptions ($100,000)	0.009***	0.005***	0.009**	0.027***	0.011*
	(0.003)	(0.002)	(0.004)	(0.009)	(0.006)
Panel C: Ages 18-54					
Exemptions ($100,000)	0.010**	0.006**	0.011**	0.032***	0.013**
	(0.004)	(0.002)	(0.004)	(0.012)	(0.005)
Panel D: Ages 25-54					
Exemptions ($100,000)	0.011***	0.006***	0.011**	0.031***	0.014***
	(0.004)	(0.002)	(0.004)	(0.011)	(0.005)

Table 13. The Impact of Exemptions on Income Inequality: Robustness to Standard Errors
The table provides alternative standard error estimates of the effect of state exemptions on income inequality. State exemptions include the homestead exemption and the personal property exemptions. Homestead is the amount of home equity that is exempt in bankruptcy. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. The sample contains 51 states and the sample period is from 1994 to 2006. We provide three standard error estimates: Clustered at the state level (our baseline estimate), bootstrapped, and SUR. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Logistic Gini	Log Gini	Log Theil	Log 90/10	Log 75/25
Exemptions	0.011	0.006	0.012	0.031	0.013
Clustered s.e.	(0.003)***	(0.002)***	(0.003)***	(0.008)***	(0.005)**
Bootstrapped s.e.	(0.002)***	(0.001)***	(0.003)***	(0.007)***	(0.005)**
SUR s.e.	(0.003)***	(0.001)***	(0.003)***	(0.007)***	(0.003)***
Controls	Yes	Yes	Yes	Yes	Yes
R-squared	0.133	0.132	0.170	0.354	0.285
Observations	663	663	663	663	663

Table 14. The Impact of Exemptions on Income Inequality: Excluding the Unemployed

The table estimates the impact of state exemption laws on income inequality excluding unemployed individuals. State exemptions include the homestead exemption and the personal property exemptions. Homestead is the amount of home equity that is exempt in bankruptcy. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Logistic Gini	Log Gini	Log Theil	Log 90/10	Log 75/25
Exemptions	0.0117***	0.00675***	0.0127***	0.0321***	0.0139***
	(0.00257)	(0.00145)	(0.00274)	(0.00798)	(0.00455)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663
R-squared	0.127	0.126	0.170	0.338	0.270

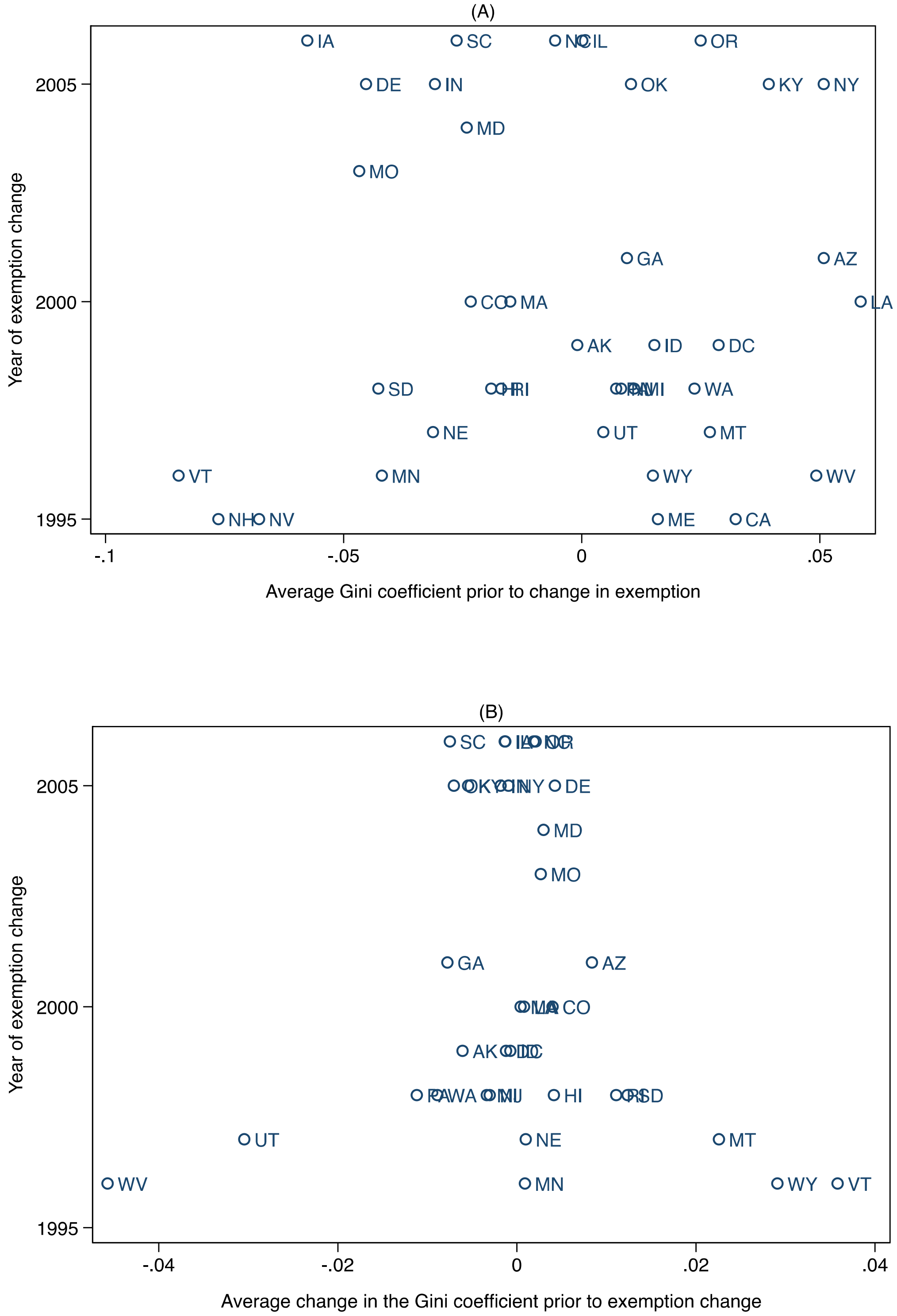

Figure 1. Does Pre-existing Income Inequality Predict Exemption Laws?
Figure (A) plots the average of the log Gini coefficient prior to the change in exemptions against the year of the change. Figure (B) plots the average change in the Gini coefficient prior to the change in exemptions against the year of the change. We computed these averages after year-demeaning the two Gini-related measures. For states that changed exemption levels multiple times, we consider only the first change. The t-statistics for the correlations in Figures (A) and (B) are 0.34 and -0.36, respectively.

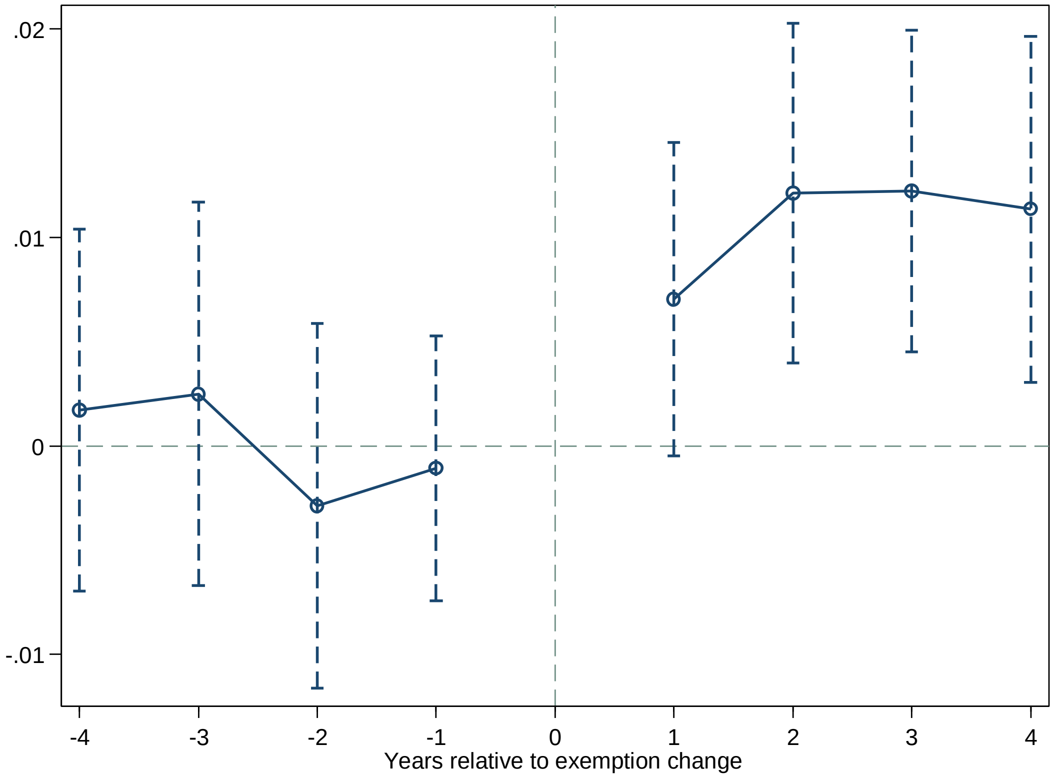

Figure 2. The Dynamic Effect of Bankruptcy Exemptions on Income Inequality.
The dependent variable is the logistic transformation of the Gini coefficient. The figure shows the estimated effect of exemption laws on income inequality for each year around the law change. We consider a 8-year window. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. The sample contains 51 states and the sample period is from 1994 to 2006. The dashed lines indicate the 95% confidence interval. Standard errors are clustered at the state level.

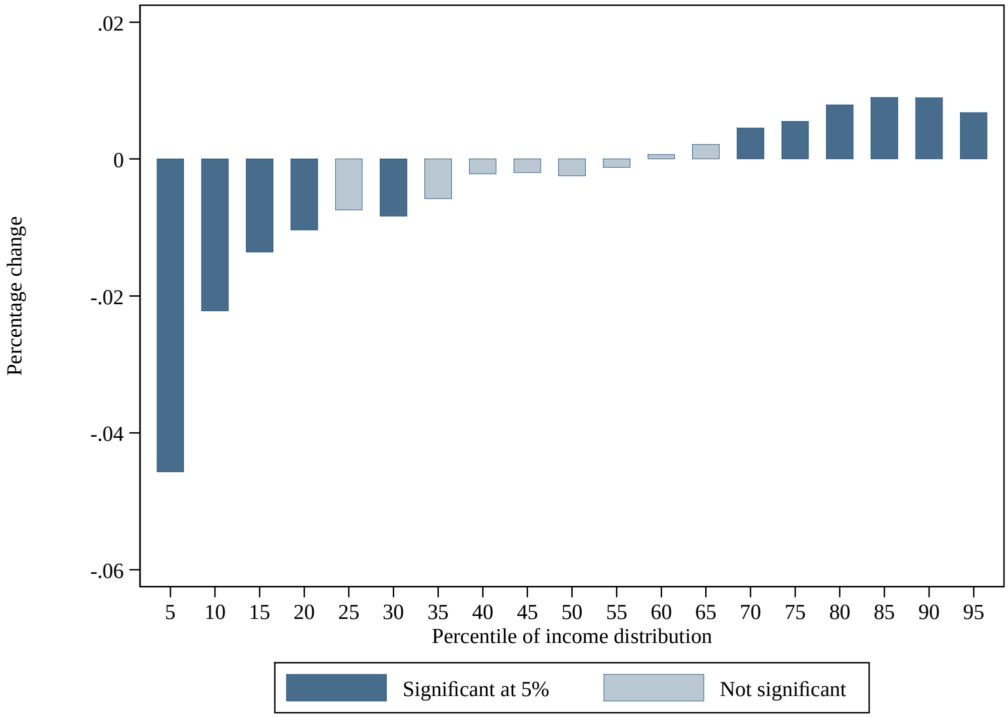

Figure 3. The Impact of Bankruptcy Exemptions Across Different Income Groups.
This figure shows the impact of bankruptcy exemption laws on different percentiles of the income distribution. Specifically, we run 19 regressions where the dependent variables are the natural logarithm of different percentiles of income distribution in each state and year. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state level. The dark bars indicate significant estimates at the 5% level.

Dependent variable: Logistic Gini

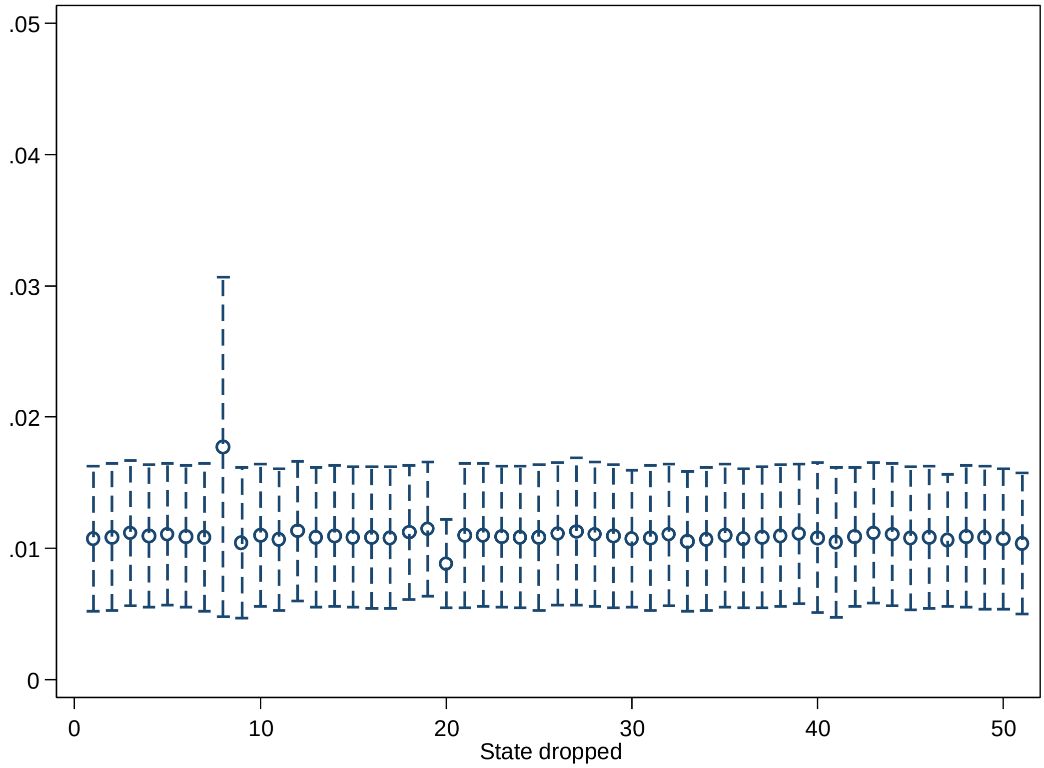

Figure 4. Excluding one state at the time

This figure shows estimates of the impact of exemption laws on the log of the Gini coefficient of income inequality from subsamples that exclude one state at a time. All regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. Each estimation subsample contains 50 states and the sample period is from 1994 to 2006. The dashed bars show the 95% confidence intervals. Standard errors are clustered at the state level.

Appendix

Appendix Table 1. Measures of Income Inequality

Measure	Expression	Interpretation	Advantage	Disadvantage
Gini coefficient	$Gini = 1 - 2\int L(x)dx$ where $L(x)$ is the Lorenz curve showing the relation between the percentage of income recipients and the percentage of income they earn.	The Gini coefficient is between 0 and 1. It is equal to 0 in the case of perfect equality when exactly s percent of total income is held by bottom s individuals (s=1,…100). The Gini coefficient is equal to 1 if all the income is held by one individual.	• Very intuitive and widely used. • Makes use of all information about the distribution.	• Sensitive to changes in the middle of the distribution. • Not easily decomposable to between- and within-group inequality.
Logistic Gini	$Logistic\ Gini = Log\ (\frac{Gini}{1 - Gini})$	Logistic transformation of the Gini coefficient. It maps the Gini coefficient, which is between 0 and 1, to a variable on the real line.	• Same advantages as Gini coefficient and also it is not bound to be between 0 and 1.	• Similar disadvantages as the Gini coefficient.
Theil index	$T_T = \frac{\sum_{i=1}^{N}\left\{\left(\frac{y_i}{\bar{y}}\right) ln\left(\frac{y_i}{\bar{y}}\right)\right\}}{N}$ where i stands for individuals, y is personal income, and $\bar{y}$ is the mean value of personal income. The first term inside the sum is individual's share of total income and the second term is the individual's income relative to the mean.	In case of perfect equality (when all individuals have the same income), the Theil index is 0. If one individual has all the income, then the index is $Ln(N)$.	• Easily decomposable to between- and within-group inequality: $T_T = \sum_{i=1}^{m} s_i T_{T_i} + \sum_{i=1}^{m} s_i ln\frac{\bar{y_i}}{\bar{y}}$ where m represents certain subgroups, s_i is the income share of group i, $T_{T_i}$is the Theil index for that subgroup, and $\bar{y_i}$ is the average income in group i.	• Not easy to interpret.
Log(75/25)	$\ln(y_{75}) - \ln(y_{25})$ where y_{75} and y_{25} are the 75th and 25th percentile of personal income distribution, respectively.	The ratio is equal to 0 if the 75th and the 25th percentiles of distribution are equal. There is no upper bound to the ratio.	• Intuitive measure of percentage difference between the third and the first quartile of a distribution. • Robust to extreme values.	• Does not use all information about income distribution.
Log(90/10)	$\ln(y_{90}) - \ln(y_{10})$ where y_{90} and y_{10} are the 90th and 10th percentile of personal income distribution, respectively.	The ratio is equal to 0 if the 90th and the 10th percentiles of distribution are equal. There is no upper bound to the ratio.	• Intuitive measure of percentage difference between the top and the bottom deciles of a distribution. • Robust to extreme values.	• Does not use all information about income distribution.

Appendix Table 2. Descriptive Statistics for Inequality Measures

The table displays descriptive statistics for the five measures of income inequality used in the paper. The measures of income inequality are: the logistic transformation of the Gini coefficient, the natural logarithm of the Gini coefficient, the natural logarithm of the Theil index, the natural logarithm of the ratio of the 90th and 10th percentiles, and the natural logarithm of the ratio of the 75th and 25th percentiles. Income inequality data are from the Current Population Survey (CPS). We use total personal income and the CPS sampling weights to calculate each inequality measure for each state and year. The sample contains 51 states and the sample period is from 1994 to 2006. We calculate three standard deviations for each measure of income inequality: Cross-states, Within-states, and Within-state-years. Cross-states is the baseline standard deviation of the variable. Within-states is the standard deviation calculated after de-meaning the variable by state. Within-state-years is the standard deviation calculated after de-meaning the variable by state and year.

					Standard deviations		
	Mean	Min	Median	Max	Cross-states	Within-states	Within state-years
Logistic Gini	-0.302	-0.520	-0.296	-0.078	0.073	0.045	0.044
Log Gini	-0.856	-0.987	-0.852	-0.732	0.042	0.026	0.025
Log Theil index	-1.175	-1.446	-1.170	-0.941	0.084	0.056	0.053
Log 90/10 ratio	2.468	1.859	2.459	3.251	0.193	0.135	0.113
Log 75/25 ratio	1.164	0.802	1.163	1.527	0.105	0.065	0.057

Appendix 3. Computing the relative wages and working hours of unskilled workers.

We use the same two-step procedure as in Beck et al. (2010) to construct the relative wages and working hours of unskilled workers. In this analysis, we focus on individuals with positive weekly working hours. In the first step we estimate the time-varying returns to experience, race, and gender characteristics using the following regression with the sample of skilled workers:

$$Log(w)_{ist}^{skilled} = X_{ist}\beta_t^{skilled} + \varepsilon_{ist}.$$

The dependent variable is the log real hourly wage of skilled worker i in state s at time t. X_{ist} is a set of individual characteristics mentioned above. We not only include the level, but also square, cubic, and quartic of potential experience, gender, and race, as well as including the interaction terms between potential experience and gender and race. Estimating the above equation for all years of sample gives time-varying return to the personal characteristics, $\beta_t^{skilled}$. We also have constant term in X_{ist}, so that we obtain an estimate of the conditional mean skilled wage rate in each year by estimating $\beta_t^{skilled}$.

In the second step, we construct the relative wage rate of each unskilled worker as follows:

$$r(w)_{ist}^{unskilled} = w_{ist}^{unskilled} - X_{ist}^{unskilled}\beta_t^{skilled} ,$$

where $w_{ist}^{unskilled}$ is the unskilled worker's actual log real wage rate and $X_{ist}^{unskilled}\beta_t^{skilled}$ is the estimated wage rate that a skilled worker with the same characteristics would earn. The idea is that there might be differences in returns to personal characteristics across unskilled and skilled workers, but we would like to abstract from those and instead focus on relative wage rates controlling for the personal characteristics. It should be noted that in computing relative unskilled wage rates from the above equation, the conditional mean skilled wage rate in each year is part of the second term and is subtracted. The relative working hours of unskilled worker i in state s at time t is computed based on a similar procedure as above, but we use weekly working hours instead of wages in the computation.