Debtor Protection, Credit Redistribution, and Income Inequality

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Abstract

A debtor-friendly bankruptcy regime limits the amount of assets creditors can seize from distressed individuals. In response, creditors may redistribute credit towards richer and more able individuals. We show that increasing the amount of asset protection in bankruptcy (exemptions) leads to higher income inequality in the state. Using geographic variation in banking market structure and variation in capital needs across industries, we show that the increase in income inequality is mediated by a credit market channel. We analyze different population groups and find that the increase in inequality is driven by a growing income gap between unskilled and skilled individuals that affects both self-employed and wage workers. We also find a drop in the employment rate and in the relative wage of unskilled workers.

Our results indicate that the redistribution of credit leads to an imbalance in economic opportunities among entrepreneurs that reduces the aggregate demand for unskilled labor.

Keywords: Debtor Protection, Income inequality, Credit Markets.

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1. Introduction

The allocation of credit shapes the distribution of economic opportunities. For instance, credit constraints can prevent individuals with little wealth and income from opening a business or from expanding an existing venture. Consequently, these disadvantaged individuals often miss the opportunity to generate higher incomes from entrepreneurial activity. Moreover, as fewer businesses are created and as existing businesses wane, the number of jobs available to other disadvantaged individuals is also likely to fall, which can also depress the wages of these workers. Thus, credit market imperfections can have profound implications on the distribution of income.

In this paper, we exploit changes across states and across time in personal bankruptcy laws to investigate the effect of credit allocation on income inequality. Specifically, we exploit variation in bankruptcy exemptions, i.e. the maximum asset value that an individual can protect in bankruptcy. The exemptions limit the amount of assets that a bank can seize from defaulting borrowers. To reduce potential credit losses, the bank may in response reallocate credit from riskier to safer borrowers. A safer borrower is one that has either the ability to generate a sufficient stream of income to repay debt (Manove, Padilla, and Pagano 2004) or enough unprotected wealth that the bank can seize upon a default (Lilienfeld-Toal and Mookeherjee, 2008). Consequently, higher exemptions redistribute credit towards the more privileged individuals.[1]

A growing literature finds empirical support for this redistribution channel. For example, Gropp, Scholz, and White (1997) document that the amount of debt held by richer households is positively related to bankruptcy exemptions, while the amount of debt of

[1] Manove, Padilla and Pagano (2004) show that the lack of collateral induces banks to increase their screening effort and to base their credit decisions on the perceived quality of the borrowers rather than on their current wealth. Lilienfeld-Toal and Mookeherjee (2008) show that a debtor-friendly regime redistributes credit from poorer to richer borrowers, since it reduces the debt capacity (i.e., the assets that borrowers can credibly pledge) of low wealth individuals by more than that of high wealth individuals.

poorer households is negatively related to the level of exemptions.[2] More recently, Cerqueiro and Penas (2014) study a panel of U.S. startups and find that bank financing to poorer entrepreneurs falls after an increase in exemptions, while credit card lending increases for richer entrepreneurs.

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 also show that, during our sample period, the timing of the exemption laws is uncorrelated with pre-existing levels of inequality. We obtain income data from the Current Population Surveys (CPS).

We start by showing 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 well-known economic and social determinants of income inequality. 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. We trace the year-by-year effect of the exemptions on income inequality and find that the effect is unanticipated, gradual, and permanent.

To assess whether the mechanisms through which the exemptions affect income inequality are consistent with theory, we conduct a twofold analysis.

[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.

[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).

First, we exploit differences across states in banking market structure and differences across industries in capital needs to show that the exemptions affect income inequality via the credit market. On the credit supply side, we compute market structure measures based on the presence of local banks (i.e., banks headquartered in the state) and of 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). Since single-state banks are very exposed to any statewide shocks, they should react more strongly to an increase in exemptions than banks that have both in-state and out-of-state operations. We find that while the presence of local banks mutes the positive effect of the exemptions on income inequality, the presence of single-state banks amplifies this effect.

On the credit demand side, we exploit variation across industries in start-up capital needs (Adelino et al., 2015) and in external finance dependence (Rajan and Zingales, 1998). If an industry has large start-up capital needs, the entrepreneur is more likely to need external financing in order to set up a new firm. In the same way, firms in industries with a high dependence on external finance rely more heavily on credit to fulfill their investment needs. We find that the effect of the exemptions on income inequality is significantly stronger in industries that either are highly financially dependent or have large start-up capital needs.

Second, we investigate how different population groups are affected by the exemptions. Theory predicts that the exemptions should make credit flow from poorer and less able individuals to the more privileged groups. Lacking information on wealth, we compare college-educated individuals (skilled) with individuals who did not attended college (unskilled).[4] The redistribution of credit from unskilled to skilled individuals can then have a

[4] The level of education is a popular measure of skill that is widely used in the labor economics literature and in studies of income inequality (see, for example, Beck et al., 2010, and Larrain, 2014). In addition, education is also highly correlated with wealth. For instance, the average college wage premium for full-time workers in the US was in 2000 higher than 90% (Autor and Acemoglu, 2011). Highly educated individuals enjoy substantially higher earnings that allows them to accumulate larger amounts of assets, and are more likely to descend from wealthier families (Charles and Hurst, 2003).

direct effect on entrepreneurs and an indirect effect on salaried workers. On the one hand, credit constraints may preclude unskilled individuals from generating an income from entrepreneurial activity and thus create an income gap between unskilled and skilled entrepreneurs. On the other hand, if the unskilled entrepreneurs are forced to reduce employment or shut down their businesses, then the relative decrease in the demand for unskilled labor could also reduce the wages of unskilled wage earners.

Consistent with these predictions, we find that the exemptions increase the income gap between unskilled and skilled individuals both for the self-employed and salaried workers. Although the increase in inequality we find is four times larger for the self-employed than for the salaried workers, the later effect is also very meaningful because the wage workers represent about 90% of the labor force. For this reason and in order to better understand the spillover effects of the growing inequality among entrepreneurs on labor market outcomes, we also analyze the employment rates and wages of salaried workers. We find that both the employment rate and the relative wage of unskilled workers fall following an increase in exemptions. In contrast, self-employment rates rise significantly for skilled workers.

To the best of our knowledge, our study is the first to analyze the direct effect of credit redistribution induced by debtor protection laws on income inequality. Overall, our paper provides strong evidence that the reallocation of credit from the less privileged to the more privileged individuals makes the distributions of economic opportunities and income more unequal. Our study therefore contributes to a growing literature that shows how regulations that affect financial markets can also have an impact on income inequality (e.g., Beck et al., 2010, and Larrain, 2014).[5] For instance, Beck et al. (2010) show that bank deregulation in the United States decreases income inequality by increasing the wage of

[5] A recent review of this literature is provided in Demirguc-Kunt and Levine (2009).

unskilled workers. Larrain (2014) studies episodes of capital account liberalization in several developed countries and finds that opening the capital account increases wages of skilled workers due to the complementarity between skill and capital.

A large literature documents widening educational income differentials and rising income inequality since the 1980s and relates these patterns to skill-biased technological changes among other factors.[6] Our study focus on a well-identified legal mechanism (i.e., personal bankruptcy) that, to the best of our knowledge, has not been examined in the context of the income inequality literature. Our results indicate that the recent upward trend in income inequality in the U.S. 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.[7] The bankruptcy trustee uses these nonexempt assets to repay debt. Under Chapter 13 debtors can keep all of their assets, but they must

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[6] See, for example, Autor, Katz, and Kearney (2006), Goldin and Katz (2008), and Acemoglu and Autor (2013).

[7] 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.

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. [8]

2.1. Bankruptcy exemptions

Under Chapter 7 debtors are allowed to keep certain assets in bankruptcy up to the 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.[9]

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,

[8] 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.

[9] 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.

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 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.[10] 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.

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

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

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.[11]

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, *Exemptions*, equals the sum of the

[11] 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.

homestead exemption and the personal property exemptions in the state.[12] 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 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. Credit needs

We use two measures of an industry's need for external finance. First, we compute for each industry the Rajan and Zingales (1998) external dependence index as the fraction of

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

capital expenditures that is not financed by internal cash flows. A high value for this index means that a large share of firms' investments in that particular industry are financed by credit markets. The dummy variable *High financial dependence* equals one for industries with external financial dependence above the median, and zero otherwise. Second, we create the dummy variable *High startup capital* that indicates whether the amount of capital needed to set up a firm in a particular industry is above the median, and zero otherwise. We obtain this measure from Adelino et al (2015) (see their Appendix Table A5).

3.1.5. 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.[13]

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 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:

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

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

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.

Finally, we run several regressions using more disaggregated industry-level data to assess the importance of the credit channel. The specification we estimate at the state-industry-year level is:

$$y_{sjt}=\alpha_{st}+\alpha_{jt}+\alpha_{sj}+\beta Exemptions_{st}\times HighCreditNeeds_j+\varepsilon_{sjt},$$

where j indexes industry and could either be high or low credit needs industry. Our dependent variables are measures of income inequality for these two groups of industries at each state in each year. Therefore, we will have twice as many observations in these regressions compared to our previous state-level specifications. The interaction term multiplies the *Exemptions* variable with one of the industry-level variables measuring credit needs (that is, either *High financial dependence or High startup capital*). The specification is saturated with fixed effects at the state-year, industry-year, and state-industry level. [14] One implication of including state-year fixed effects is that they absorb the *Exemptions* variable, as well as all our control variables. Therefore we can identify the differential effect of the exemptions across industries with high versus low credit needs. Although we provide several strong evidence that in our state level regressions we are indeed identifying the effect of exemptions

[14] This is a standard triple difference specification. For examples of papers in the finance literature using this type of specification, see Cetorelli and Strahan (2006), Larrain (2014), and Boustanifar (2014).

on inequality, this industry level identification strategy significantly reduces the concern that our results is driven by either reverse causality (changes in inequality leads to changes in exemptions) or by an omitted variable such as any state-level policy change that drives both changes in exemptions and inequality. We cluster standard errors at the state-industry level.

4. Results

4.1. Exemptions and income inequality

In Table 3 we study the effect of exemptions on the distribution of income using our five measures of income inequality (see Appendix Table 1 for a detailed description of each measure). All regressions include state fixed effects, year fixed effects, and several state-level control variables, including 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. We cluster standard errors at the state level.

We find that an increase in exemptions significantly increases income inequality during our sample period from 1994 to 2006. The estimated coefficients are statistically significant across all specifications and 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 that we obtain after accounting for state and year effects. Since the standard deviation is 4.4% (see Appendix Table 2), the exemptions explain 25% of the variation in income inequality relative to state and year averages.

We also find that several of our economic and demographic state controls are significant predictors of income inequality. As expected, a higher unemployment rate, a higher proportion of blacks, and 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. In robustness tests that we present below, we show that controlling for various house price indices does not alter our main result.

4.1.1 Reverse causality concerns

Our empirical analysis rests on the assumption that the passage of the exemption laws is unrelated to the distribution of income. However, higher inequality could potentially lead to increases in exemptions (Gala, Kirshner, and Volpin, 2009). We assess the plausibility of our identification strategy in two ways. First, we investigate the relationship between the timing of the exemption laws and pre-existing income inequality. Figure 1 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.[15] 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 confirm that during our sample period current income inequality levels do not predict the timing of the exemption laws.[16]

Second, 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 in the previous regressions, we control for state and year fixed effects and for the same set of control variables shown in Table 3. Standard errors are clustered at the state level.

Figure 2 provides additional evidence that the exemption laws do not appear to respond to changes in the 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.

[15] 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.

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

The timing evidence thus corroborates our empirical strategy and speaks to a causal interpretation of our results.

Three other 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 shows that the increase in income inequality is permanent. Third, the adjustment in income inequality depicted seems plausible because it is not sudden. 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 and persists after that.

4.1.2 Who wins and who loses?

The fact that increases in exemption levels lead to higher 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 higher exemptions redistribute credit from lower-income to higher-income individuals. The exemptions impose a limited liability constraint on borrowers that reduces the amount of assets they can credibly pledge to creditors. Since banks can seize fewer assets in bankruptcy, they may reallocate credit towards wealthier individuals who still have unprotected assets (Lilienfeld-Toal and Mookherjee, 2008) or higher ability individuals who are more likely to generate sufficient income to pay back their loans (Manove, Padilla and Pagano, 2004). Henceforth, we refer for simplicity to these higher quality individuals as *skilled* as opposed to *unskilled*.

The redistribution of credit may then create an imbalance in economic opportunities between skilled and unskilled individuals. The availability of financing is an important condition for some individuals to be able to generate income. For example, credit constraints may reduce the amount of income that unskilled entrepreneurs generate from their businesses. Consistent with this view, Cerqueiro and Penas (2014) find that an increase in exemptions reduces credit available and harms the performance of businesses owned by poorer entrepreneurs. In particular, these entrepreneurs reduce their labor force and become more likely to fail. Moreover, if these entrepreneurs are forced to reduce employment or close their businesses, the decline in labor demand could also reduce the income of affected wage workers. Consequently, the exemptions may increase income inequality among both entrepreneurs and wage workers.

We investigate the plausibility of these mechanisms in two steps. In Section 4.2 we exploit differences across states in banking market structure and differences across industries in capital needs to test whether the channel through which the exemptions affect income inequality is the credit market. Then, we investigate in Section 4.3 how the different population groups are affected by the exemptions.

4.2. The credit market channel

We investigate the role of credit markets in two ways. First, we exploit differences across states in banking market structure as source of variation in the supply of credit.

Second, we exploit differences across industries in capital needs as a source of variation in the demand for credit.

4.2.1 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.[17] The presence of local banks should therefore dampen 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 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

[17] 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).

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).

4.2.2 Capital needs

On the credit demand side, we exploit variation across industries in external finance dependence (Rajan and Zingales, 1998) and in start-up capital needs (Adelino et al., 2015). Firms in industries with a high dependence on external finance rely more heavily on credit to fund their investment needs. We exploit the variation in financial dependence across industries using the variable *High financial dependence*, which equals one for above-median dependence industries, and zero otherwise. In the same way, an industry with large start-up capital needs is one in which the entrepreneur is more likely to need external financing in order to set up a new firm. *High startup capital* is an indicator variable for industries with above-median start-up capital needs.

Table 5 presents the results from our industry analysis. In columns 1-3 we interact the variable *Exemptions* with *High financial dependence*, while in columns 4-6 we interact *Exemptions* with *High startup capital*. The three measures of income inequality analyzed are the Logistic Gini (columns 1 and 4), Log Gini (columns 2 and 5), and Log Theil (columns 3 and 6). All specifications includes fixed effects at the state-year, industry-year, and state-industry level. Identification thus comes from comparing within a given state the effect of the exemptions on income inequality for industries with high versus low capital needs. We cluster standard errors at the state-industry level.

If the exemptions affect income inequality via the credit market, then one would expect to see relatively stronger effects for industries with high capital needs. This is precisely what we find in Table 5. All interaction terms have positive coefficients and are statistically significant. It is important to note that the two measures of credit needs yield similar results. This similarity is remarkable, since the two variables measure credit needs at different stages of a firm's lifecycle: *High financial dependence* measures the need for credit to finance the firm's ongoing investment, while *High startup capital* measures the need for credit to set up the firm.[18] The estimated effects are also economically relevant. For instance, consider the first column in Table 5 and an increase in state exemptions of $100,000. The point estimate indicates that the logistic Gini increases by 0.5% more for individuals working in industries with high (rather than low) dependence on external financing. This differential effect explains more than 11% of the variation in income inequality relative to state and year averages.[19]

[18] The two measures overlap for 60% of the industries. One third of the remaining industries are characterized by high set up costs but low dependence on external financing.

[19] To calculate this figure we divide the point estimate by the standard deviation of the logistic Gini that we obtain after accounting for state and year effects, which equals 4.4% (see Appendix Table 2).

All results in this section corroborate the view that the exemptions affect income inequality via the credit market. Next, we investigate how different population groups are affected by the exemptions.

4.3. Self-employed and salaried workers

We start by investigating 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 6. 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.[20]

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. Although the increase in inequality among the self-employed is four times larger than the increase in inequality among salaried workers, we argue that the later

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

effect is economically important because salaried workers comprise about 90% of the labor force.

We argued above that the increase in income inequality we find may be due to the redistribution of credit from unskilled to skilled borrowers that affects directly entrepreneurs and indirectly wage workers. On the one hand, credit constraints may preclude unskilled individuals from generating an income from entrepreneurial activity and thus create an income gap between unskilled and skilled entrepreneurs. On the other hand, if the unskilled entrepreneurs are forced to reduce employment or shut down their businesses, then the relative decrease in the demand for unskilled labor could also reduce the wages of unskilled wage earners.[21]

We test for potential redistribution effects among entrepreneurs and wage workers by comparing within each employment group the outcomes of college-educated individuals (skilled) and individuals who did not attend college (unskilled).[22] Specifically, we analyze in Section 4.4.1 the effects of the exemptions on the income distributions of unskilled versus skilled self-employed individuals. We then investigate in Section 4.4.2 the effects on the wage earners, in two steps. First, we analyze the effects of the exemptions on the income distribution of unskilled versus skilled wage earners. Second, we analyze several labor market outcomes, including the employment rates and relative wages of these individuals.

4.3.1 Inequality among the self-employed

In Table 7 we decompose the Theil index to assess the contribution of different education groups to overall inequality among the self-employed. As before, we report in the first column the estimate of exemptions on total income inequality. Columns 2 and 3 indicate

[21] There is ample evidence that labor market outcomes (such as unemployment) of less skilled individuals are more sensitive to economic conditions than higher skill groups (see, for example, Topel, 1993 and Hoynes, 2000).

[22] The level of education is widely used as a proxy for individual skill in the labor economics literature and in studies of income inequality (see, for example, Beck et al., 2010, and Larrain, 2014). The CPS does not provide information about wealth.

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. All specifications shown control for state and year fixed effects and for our time-varying state variables. We cluster standard errors at the state level.

Column 3 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 is the main effect. The remaining 5% comes from an increase in inequality among skilled entrepreneurs (Column 5). These findings, coupled with the evidence that a credit market channel is at work, corroborates our view that the exemptions lead to the redistribution of credit towards higher quality borrowers. Unskilled entrepreneurs, who are relatively more credit constrained, struggle to generate an income from their ventures, explaining the increase in income inequality we find.

4.3.2 Inequality among salaried workers

In Table 8 we decompose the Theil index of income inequality for the group of salaried workers using the same procedure as before. The first column reports the estimated effect of the exemptions on income inequality for this group. 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.

The results show that most of the increase in inequality within salaried workers is also due to the increase inequality between skilled and unskilled salaried workers. More specifically, Column 3 shows that about 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 these results 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) that less wealthy entrepreneurs reduce their demand for labor following an increase in exemptions. Next, we evaluate the plausibility of this argument by studying the effect of exemptions on labor market outcomes.

4.3.3 Employment rates and real wages

Table 9 shows the results of the effect of exemptions on employment rates. Our dependent variables are expressed in logs and all regressions include state fixed effects, 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). The results also show a decline in in the rate of unskilled self-employed workers although not statistically significant. This suggest that the reduction in unskilled salaried workers is *not* because these workers switch from salaried to self-employed workers in which case we would have seen an increase in unskilled self employed workers.

These results are consistent with the redistribution effects documented Cerqueiro and Penas (2014). Skilled individuals, who 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 in Table 10 whether the exemptions affect the relative wages and working hours of unskilled workers (relative to skilled workers). Data are from the Outgoing Rotation Groups CPS files. 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.[23] The regression we estimate with individual-level data is:

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

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.

The result in Column 1 of Table 10 shows that following an increase in exemptions the relative wage of unskilled workers falls by 1.3%. The effect is statistically significant at the 1% level. For the relative working hours we obtain a negative and insignificant effect of the exemptions (see Column 2). Our results therefore show that the exemptions affect not only unskilled entrepreneurs, but also unskilled salaried workers. In particular, credit constraints harm the performance of companies owned by unskilled entrepreneurs and forces them to reduce employment. In turn, the lower demand for unskilled labor depresses the incomes of these workers.

5. Robustness tests

5.1. Influential states

[23] We explain the methodology in detail in Appendix 3.

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 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 reasonable stable no matter which state is dropped, indicating that the results are not driven by one state.[24]

5.2. State minimum wages

One of the potentially important factors affecting the distribution of income is the minimum wage level (see, for example, Acemoglu and Autor, 2011). Since several states changed their minimum wage level during our sample period, we worry that our exemption laws might be correlated with these minimum wage laws. To address this concern, we collected from the Bureau of Labor Statistics state minimum wages for our sample period. In Table 11 we report results from our baseline regressions (i.e., similar to Table 3) when we control for state minimum wages. We find that the effect of the exemptions on income inequality remains virtually unchanged.

5.3. 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

[24] Two states that appear to be somewhat influential are the District of Columbia and Massachusetts. Unlike the other states, in the District of Columbia the exemptions are set by Congress. For this reason, some studies drop DC (e.g., Hynes et al., 2004). We prefer to report the more conservative results that we obtain with the full sample. Massachusetts was severely by the 2001 recession, just one year after the state increased its exemption level. This can help explain why the point estimate of the effect of the exemptions on income inequality becomes smaller when we drop MA.

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.

We recalculate our measures of inequality dropping from the sample all individuals who moved to the state during the previous year. Then, we run similar regressions as in Table 3 using these new measures of income inequality. We present the results in Table 12. The coefficients on the exemptions are similar to our baseline specifications, confirming that migration flows are not a confounding factor in our analysis.

5.4. Unemployed individuals

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 13 and show that most of the effect of the exemptions on income inequality is due to changes in the incomes of employed individuals.

5.5. Controlling for house prices

Cerqueiro and Penas (2014) document that the main motive behind the increase in exemption limits is the level of house prices. Since rising house prices can be themselves a determinant of inequality, we test whether this potentially confounding factor can explain our results. We collect for each state and year the house price index from the FHFA (based on all transactions) and use it to control for changes in house prices. The results shown in Table 14 confirm that higher house prices are themselves an important determinant of income inequality. Although our coefficients become marginally smaller, they remain statistically and economically significant.

5.6. 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 15 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. Our results are robust to alternative definitions of the exemption variable.

5.7. Individuals with outlying income

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 16. The results we obtain are similar across all specifications shown.

5.8. 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 17 in separate panels. Again, the results stay statistically significant and the magnitudes remain similar as before.

5.9. 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 18 and indicate that clustering the standard errors at the state level provides conservative estimates.

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 provide evidence that a credit channel is the mechanism through which the increase in exemptions affects income inequality. On the credit supply side, 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. On the credit demand side, we find that the effect of the exemptions on income inequality is significantly stronger in industries that either are highly financially dependent or have large start-up capital needs.

We also investigate the effect of exemptions on different population groups. The increase in inequality we found is driven by both self-employed and wage workers. 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 a growing income gap between skilled and unskilled wage workers that seems to result from a reduction in the labor demand for unskilled workers. The fact that the employment rate and the wages of unskilled workers (relative to skilled workers) fall following an increase in exemptions support this view.

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. 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. The Impact of Bankruptcy Exemptions on Income Inequality: The Role of Credit Demand.

The table investigates the differential impact of exemption laws on income inequality for industries with high versus low credit needs. Our dependent variables are measures of income inequality in two groups of industries (high and low credit needs) in each state and each year. State exemptions are expressed in 100,000 dollars and include the homestead exemption plus the personal property exemptions. *High financial dependence* equals one for industries with above-median external financial dependence (based on the Rajan and Zingales (1998) index), and zero otherwise. *High startup capital* is from Adelino et al. (2015) and equals one for industries with above-median startup capital needs, and zero otherwise. The measures of income inequality are: the logistic transformation of the Gini coefficient (columns 1 and 4), the natural logarithm of the Gini coefficient (columns 2 and 5), and the natural logarithm of the Theil index (columns 3 and 6). Income inequality data are from the Current Population Survey (CPS). The sample contains 51 states and the sample period is from 1994 to 2006. Standard errors are clustered at the state-industry 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	Logistic Gini	Log Gini	Log Theil
	(1)	(2)	(3)	(4)	(5)	(6)
Exemptions × High financial dependence	0.006***	0.004***	0.007***			
	(0.002)	(0.001)	(0.002)			
Exemptions × High startup capital				0.006**	0.004**	0.008**
				(0.002)	(0.001)	(0.003)
State-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State-industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1326	1326	1326	1326	1326	1326
R-squared	0.603	0.604	0.621	0.611	0.612	0.627

Table 6. Income Inequality and 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 7. Income Inequality Among Self-employed Workers

The table estimates the impact of state exemption laws on the Theil index of income inequality for self-employed workers. Column 1 displays the total effect. 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)
Exemptions ($100,000)	0.008***	0.0004*	0.0078***	0.0005	0.0007***
	(0.002)	(0.0002)	(0.0020)	(0.005)	(0.0002)
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 8. Income Inequality Among Salaried Workers

The table estimates the impact of state exemption laws on the Theil index of income inequality for salaried workers. Column 1 displays the total effect. 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)
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 9. 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 10. 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 11. Robustness Test: Controlling for Minimum Wage Laws

The table shows estimates of the impact of state exemption laws on income inequality after after controlling for state-level minimum wages laws. 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). State minimum wages are from the Bureau of Labor Statistics. 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)	(0.001)	(0.003)	(0.007)	(0.005)
Minimum wage	0.006	0.003	0.007	0.028**	0.007
	(0.005)	(0.003)	(0.006)	(0.014)	(0.007)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663
R-squared	0.135	0.134	0.171	0.358	0.286

Table 12. Robustness Test: Dropping Immigrants

The table shows estimates of the impact of state exemption laws on income inequality after droppping individuals who immigrated to the state during the previous year. 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 90^{th} and 10^{th} percentiles, and (5) the natural logarithm of the ratio of the 75^{th} and 25^{th} 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)	0.009***	0.005***	0.010***	0.026***	0.011**
	(0.003)	(0.002)	(0.003)	(0.009)	(0.005)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663
R-squared	0.125	0.124	0.170	0.355	0.295

Table 13. Robustness Test: Dropping Unemployed Individuals

The table shows estimates of the impact of state exemption laws on income inequality after droppping unemployed individuals. 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 90^{th} and 10^{th} percentiles, and (5) the natural logarithm of the ratio of the 75^{th} and 25^{th} 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)	0.012***	0.007***	0.013***	0.032***	0.014***
	(0.003)	(0.001)	(0.003)	(0.008)	(0.005)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663
R-squared	0.127	0.126	0.170	0.338	0.270

Table 14. Robustness Test: Controlling for House Prices

The table shows estimates of the impact of state exemption laws on income inequality after after controlling for changes in house prices. 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). House price data are based on the all-transactions house price index (HPI) from the FHFA. 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)	0.009***	0.005***	0.010***	0.025***	0.012**
	(0.003)	(0.002)	(0.003)	(0.007)	(0.005)
% Change in the HPI	0.200***	0.114***	0.224***	0.719***	0.109
	(0.064)	(0.036)	(0.071)	(0.181)	(0.103)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663
R-squared	0.149	0.148	0.184	0.377	0.288

Table 15. Robustness Test: 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. Income inequality is 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). 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. All regressions include state fixed effects, year fixed effects, 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 16. Robustness Test: Individuals 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. 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. 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 17. Robustness Test: 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. 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.

	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 18. Robustness Test: 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 ($100,000)	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

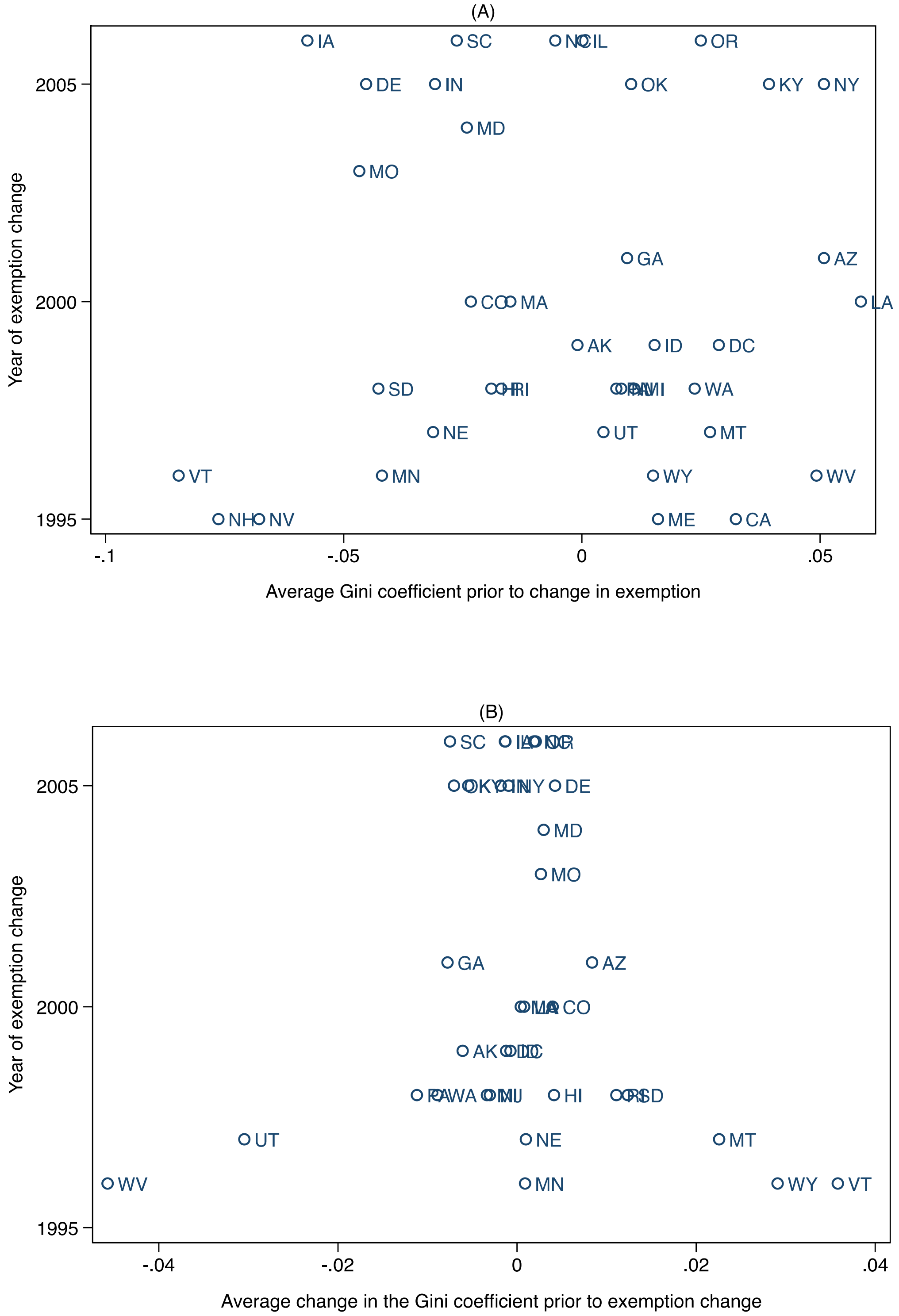

Figure 1. Does Pre-existing Income Inequality Predict Exemption Laws?
Figure (A) plots the average of the log Gini coefficient prior to the change in exemptions against the year of the change. Figure (B) plots the average change in the Gini coefficient prior to the change in exemptions against the year 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.

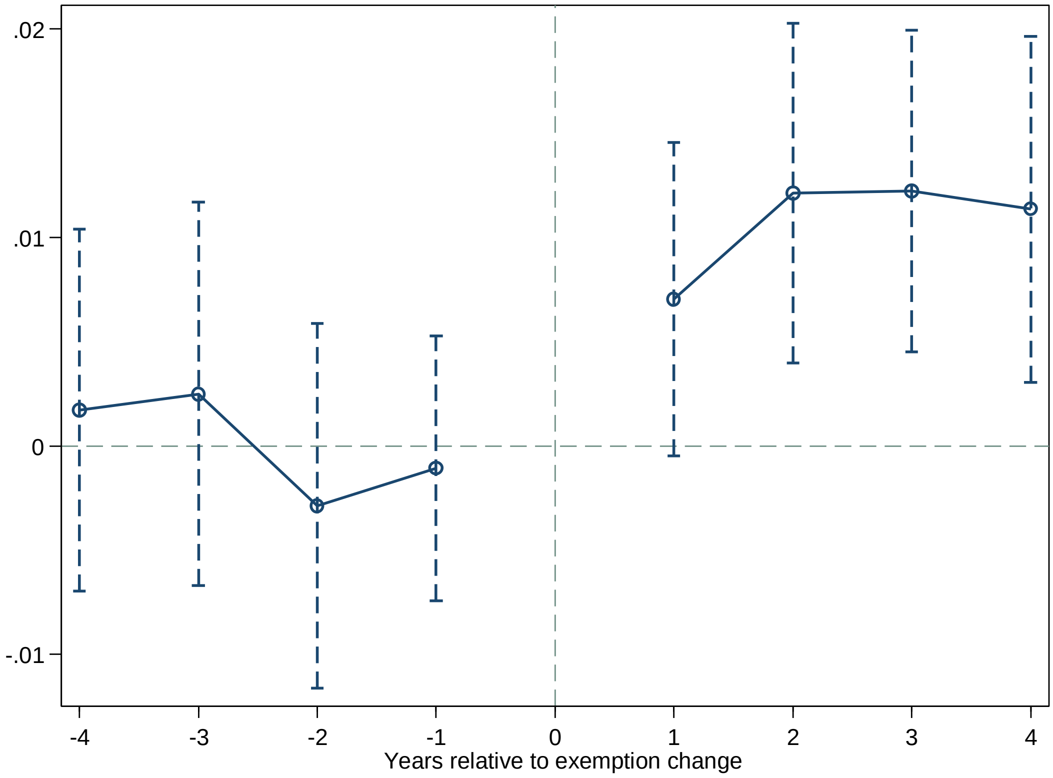

Figure 2. The Dynamic Effect of Bankruptcy Exemptions on Income Inequality.
The dependent variable is the logistic transformation of the Gini coefficient. The figure shows the estimated effect of exemption laws on income inequality for each year around the law change. We consider a 8-year window. All 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.

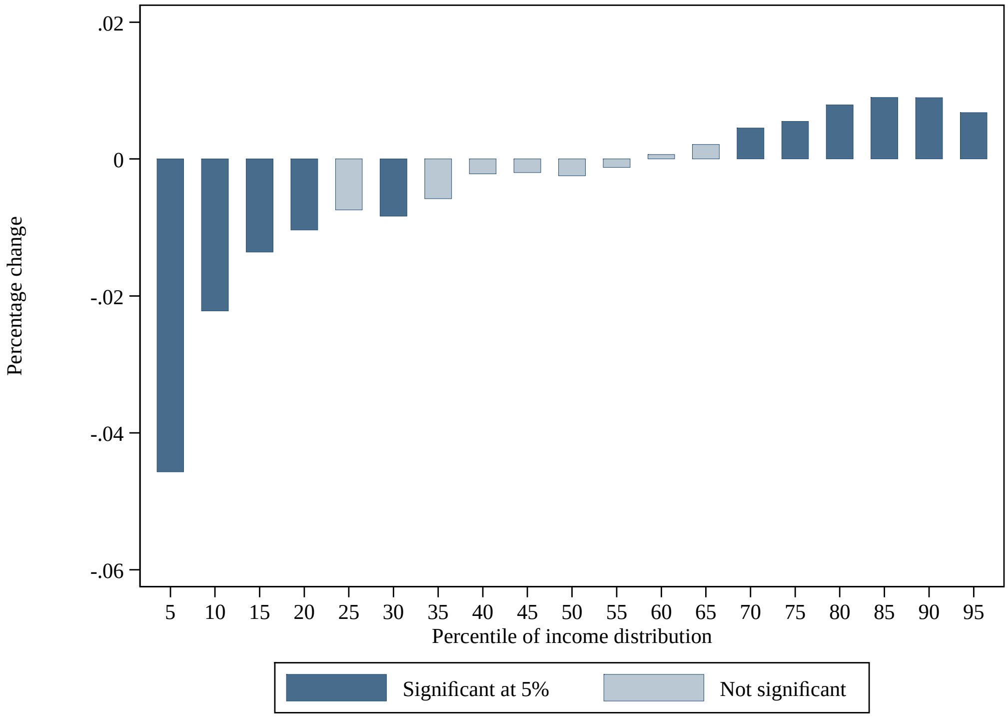

Figure 3. The Impact of Bankruptcy Exemptions Across Different Income Groups.
This figure shows the impact of bankruptcy exemption laws on different percentiles of the income distribution. Specifically, we run 19 regressions where the dependent variables are the natural logarithm of different percentiles 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

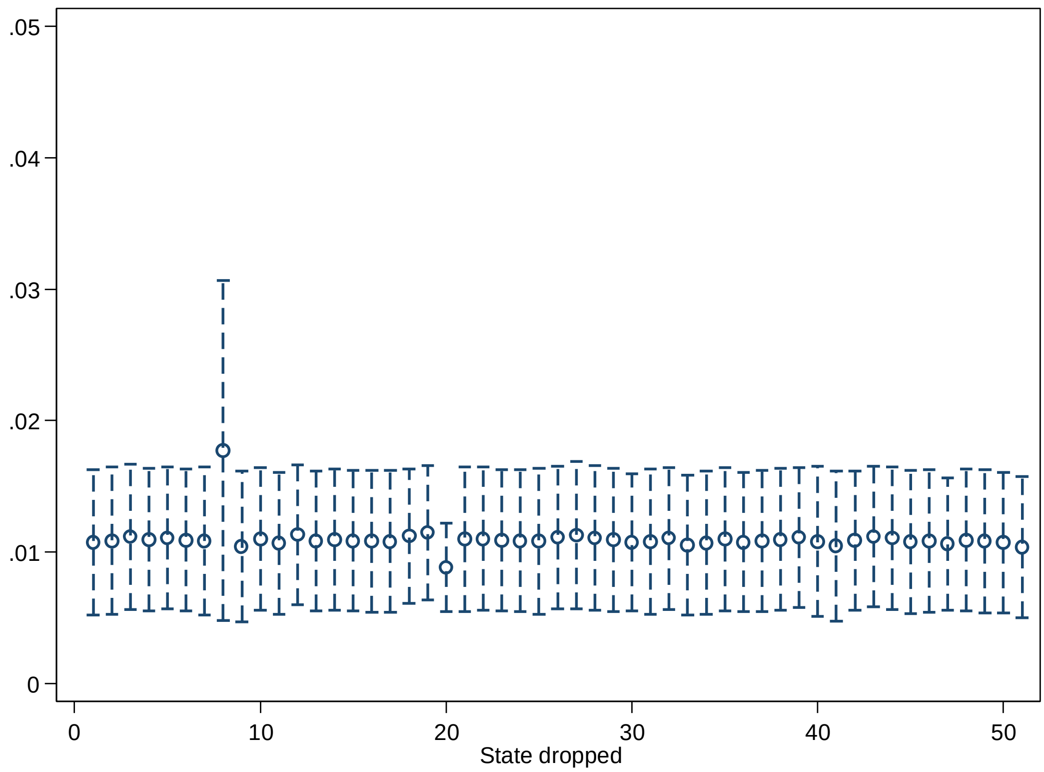

Figure 4. Excluding one state at the time

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.