Debtor Protection, Credit Redistribution, and Income Inequality*

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Abstract

A debtor-friendly personal bankruptcy regime reallocates credit by reducing its availability mainly to low-asset individuals. We show that increasing the amount of asset protection in bankruptcy leads to higher income inequality. The increase in inequality is amplified in industries with high credit needs and is triggered by a growing income gap among self-employed individuals. We also find an increase in income inequality among salaried workers, which is explained by a drop in wages and working hours of unskilled workers. Debtor protection thus creates an imbalance in economic opportunities among entrepreneurs that reduces the aggregate demand for unskilled labor. (98 words)

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1. Introduction

It is well known that debtor-friendly personal bankruptcy laws have adverse ex ante incentives effects. If lenders cannot seize assets in the event of default, borrowers cannot credibly commit to repay their loans. This increases the risk of lending and reduces the supply of credit. There is growing evidence that this adverse effect is particularly strong for poorer borrowers, who have less assets to pledge.[1] A debtor-friendly personal bankruptcy regime therefore redistributes credit towards wealthier individuals.

In this paper we analyze the implications of this reallocation of credit for income inequality. There are two channels through which the availability of credit may affect the distribution of income. The first is by affecting directly the incomes generated by entrepreneurs. As documented in Robb and Robinson (2014), entrepreneurs rely heavily on bank financing to operate and expand their businesses. If credit falls more for poorer entrepreneurs than for richer entrepreneurs, this creates an imbalance in economic opportunities that can lead to more income inequality among them. The second channel is by affecting the labor market. If some business owners are forced to reduce employment or shut down their businesses, this may change the relative demand for different types of workers. The reallocation of credit should therefore affect the distribution of income among both entrepreneurs and wage workers.

Specifically, we exploit changes across states and across time in bankruptcy exemptions, which define the maximum asset value individuals can keep under

[1] Lilienfeld-Toal and Mookherjee (2016) show that a debtor-friendly personal bankruptcy regime reduces the debt capacity of borrowers with lower pledgeable wealth (see also Lilienfeld-Toal et al., 2012). Gropp, Scholz, and White (1997) and Cerqueiro and Penas (forthcoming) provide supporting empirical evidence in the context of households and entrepreneurs, respectively. See also Severino and Brown (2016).

Chapter 7 personal bankruptcy. The exemptions limit the amount of assets that a bank can seize from defaulting borrowers and hence provides an objective measure of debtor protection. Our empirical analysis exploits changes in exemption limits between 1994 and 2006, a period during which several states significantly increased their exemptions levels. The staggering of the exemption laws 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.[2]

Using data from the Current Population Surveys (CPS), we start by showing that an increase in state exemptions leads to higher income inequality. The result holds for measures of income inequality based on the Gini coefficient, Theil index, and on income percentile ratios, and after controlling for state fixed effects, year fixed effects, and for several well-known economic and social determinants of income inequality. The estimated effect is also economically meaningful: the exemptions explain about one-fourth of the variation in income inequality relative to state and year averages. We also show that the increase in inequality is driven mainly by a reduction in the incomes of individuals in the lowest income quartile, some of which are pushed below the official poverty line.

We assess the soundness of this finding in several ways. First, we investigate the full dynamic response of income inequality to the exemption laws. We find that income inequality is reacting to changes in exemptions (and not the other way around), and that the increase in inequality is gradual and permanent. Second, we show that our main result holds when we use a different dataset and adopt an alternative identification strategy. In particular, we build county-level measures of income inequality from individual tax return (as reported by the IRS) and assess the

[2] Several studies document an upward trend in income inequality in the United States since the 1980s. See for example Autor, Katz, and Kearney (2006), and Autor and Dorn (2013).

effect of the exemptions by comparing contiguous county-pairs located on opposite sides of a state border. Third, we show in robustness tests that the increase in inequality we find is not driven by other legal reforms, such as changes in state minimum wages, or by migration flows. We discuss several other tests in the paper.

Having established that the exemptions lead to higher income inequality, we turn to the mechanism behind this effect. Previous work shows that the exemptions reduces disproportionally the availability of credit to less affluent entrepreneurs (Cerqueiro and Penas, forthcoming). This may create an income gap among small business owners since credit-starved businesses grow less and generate less income. Moreover, if some business owners are forced to reduce employment or shut down their businesses, the lower demand for labor can also reduce the incomes of the more vulnerable wage workers. We obtain three pieces of evidence that corroborate this mechanism.

First, we show that the exemptions affect income inequality via the credit market. In particular, we show that the effect of the exemptions on income inequality is stronger in industries with high credit needs relative to industries with low credit needs. An industry with high credit needs is one with either high dependence on external financing (Rajan and Zingales, 1998) or high start-up costs (Adelino, Schoar, and Severino, 2015).

Second, we show that income inequality increases both among self-employed individuals and salaried workers. Although the increase in inequality among the self-employed is four times larger than the increase in inequality among salaried workers, it is important to note that the later effect is economically important because salaried workers represent about 90% of the labor force. Using variation across counties in self-employment rates, we also find that the effect of the exemptions on inequality is driven mainly by counties with high self-employment rates. In counties with low self-employment rates the exemptions have no effect on income inequality, showing that business owners are the channel that triggers the increase in inequality.

Third, we analyze the wages and working hours of salaried workers to understand how the exemptions affect the labor market. Building on the notion that unskilled workers are more sensitive to economic conditions than skilled workers, we analyze how the exemptions affect the relative wages and working hours of unskilled (relative to skilled) workers.[3] Since working hours refer only to individuals who are actively working, we also analyze the employment rates of unskilled and skilled individuals. We find that following an increase in exemptions, unskilled workers earn less, work fewer hours, and are more likely to be unemployed, relative to skilled workers. These negative effects are more pronounced in industries with high startup costs, confirming that our findings are driven by changes in credit market conditions. These results show that the exemptions affect the distribution of income among salaried workers by reducing the demand for unskilled labor.

Our study is the first to analyze the direct effect of debtor protection laws on income inequality. We provide evidence that a lenient personal bankruptcy regime, by affecting the allocation of credit, 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 (Beck, Levine, and Levkov, 2010, and Larrain, 2015). These studies analyze episodes of financial liberalization that enhanced economic activity

[3] For evidence that unskilled workers experience much higher risk of becoming unemployed, see for example Mincer (1991) and Topel (1993). Unskilled workers are those without any college education, while skilled workers have some college education.

and thereby affected the relative demand for different types of labor.[4] In contrast, our study emphasizes the role of credit market frictions in directly affecting the business income of self-employed individuals, which in turn affects the demand for labor (Benmelech, Bergman, and Seru, 2015). Our results thus lend empirical support to theories (e.g., Banerjee and Newman, 1993) that argue that income inequality can result from financing frictions hurting business owners with less wealth.

Our study also highlights an unintended consequence in the design of personal bankruptcy law. The most popular arguments in favor of lenient bankruptcy rules are the protection of debtors against unfortunate events, such as illness or job loss, and the preservation of their ex post incentives to work. However, our results indicate that lenient personal bankruptcy laws hurt the economic opportunities of individuals at the lower end of the income distribution and increase poverty.

The paper proceeds as follows. Section 2 details the institutional background of U.S. personal bankruptcy law. Section 3 describes the data and variables used. Section 4 presents our empirical methodology. Section 5 presents the main results and Section 6 provides evidence on the mechanism. Section 7 discusses some robustness tests. Section 8 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).

[4] Beck et al. (2010) show that bank deregulation in the United States decreases income inequality by increasing the wage of unskilled workers. Larrain (2015) 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. There is an extensive literature that analyzes the relationship between financial development and income inequality that provides mixed findings. Demirguc-Kunt and Levine (2009) provide a recent review of the literature that connects finance to income inequality.

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.[5] The bankruptcy trustee uses these nonexempt assets to repay debt. Under Chapter 13 debtors can keep all of their assets, but they must propose to creditors a repayment plan. This plan typically involves using a portion of the debtor's future earnings over a five-year period to repay debt.

Before 2005, debtors were allowed to choose between Chapters 7 and 13. Around 70 percent of all bankruptcy filings were made under Chapter 7 (White, 2007). Debtors with few nonexempt assets had an incentive to choose Chapter 7 over Chapter 13. In this way, debtors maximized their financial benefit from filing for bankruptcy because they were able to preserve both their current assets and future income. This means that the system also allowed individuals with high incomes to benefit from the generous bankruptcy provisions. [6]

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,

[5] 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.

[6] 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.

exemption limits vary widely across states.[7]

There are several categories of asset exemptions. The most important is the homestead exemption, which provides protection for equity in the debtor's family residence. The homestead exemption varies from a few thousand dollars to unlimited. Lower exemption amounts are also available for various other types of personal property, such as clothing, furniture, cattle, guns, and motor vehicles. Many states offer wildcard exemptions that allow debtors to retain any personal property up to a specified dollar amount. The types of personal assets specified in the law vary considerably across states and many of these assets have unspecified exemption amounts. It is therefore infeasible to include all personal assets specified in these various state laws. Similar to Gropp et al. (1997) and Cerqueiro and Penas (forthcoming), 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 1 shows in Panel A that many states changed their exemption levels

[7] 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.

during the sample period. Moreover, some states have raised exemptions more than once (e.g., Arizona in 2001 and 2004). Panel B of the same table shows that there is wide variation in the magnitude of the exemption changes.

3. Data and variables

3.1. Income distribution data from CPS data

We use the March Supplement of the Current Population Survey (CPS) to obtain data on the distribution of income. 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 (e.g., Beck et al, 2010). The sample focus on civilians in the age range of 25 to 64 years who have non-negative income. Excluded from the sample are 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 measure the distribution of income for each state and year over the period 1994 to 2006 in three different ways. First, we create measures of inequality based on the Gini index. The Gini index equals zero when there is perfect equality and equals one when one individual receives all the income.[8] We use both the natural logarithm of the Gini coefficient (*Log Gini*) and its logistic transformation (*Logistic Gini*). The logarithmic transformation of the Gini coefficient removes the floor and makes the measure upper-bounded at zero. The advantage of using the log of Gini is that it allows us to interpret coefficients relative to this variable as percentage changes. The logistic transformation removes both the floor and the ceiling of the original variable, implying that the logistic Gini ranges from minus infinity to plus infinity.

The 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. The Theil index has an interesting decomposability property that we use to investigate the sources of income inequality.
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. In our regressions, we use the natural logarithm of the Theil index (*Log Theil*).

Third, we build measures of inequality based on the 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 90^{th} percentile and the 10^{th} percentile (*Log 90/10*), and we compute the same difference between the 75^{th} percentile and the 25^{th} percentile (*Log 75/25*). Although these inequality measures do not exploit the entire distribution of income, they are robust to outliers in the tails of the distribution.

[8] We provide more information about the inequality measures used in Appendix A.

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 Panel A of Table 2 we present descriptive statistics for the measures of income inequality obtained from CPS data.[9]

3.2. State 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 homestead exemption and the personal property exemptions in the state in a given year (see Section 2 for details on the different types of exemptions). We express state exemptions in $100,000. In robustness tests, we use alternative measures, such as including only the homestead exemptions or log-transforming the exemption variables. Panel A of Table 1 describes the timing of the exemption laws and Panel B shows the distribution of the changes in exemption values.

3.3. State control 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 first compute using CPS data and then aggregate for each state. These variables are the proportion of blacks, the proportion of high-school dropouts, and the proportion of female-headed households.

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

3.4. Credit market

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 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 is 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). We provide additional information about the industry composition of our sample in Appendix B.

3.5. Employment rates

We compute from CPS data state-level employment rates both in aggregate and for groups of workers with different levels of education. We compute the employment rate as the number of wage workers divided by the total labor force. We also compute separate employment rates for unskilled and skilled workers. Unskilled individuals completed at most 12 years of education (i.e., they did not attend college), while skilled individuals have 13 or more years of completed education. Panel A of Table 2 displays descriptive statistics for these three variables.

3.6. County-level data from IRS-SOI

We also compute measures of income inequality based on individual income tax returns. The Internal Revenue Service (IRS) – Statistics of Income (SOI) data reports for each postal zip code the number of tax returns, which approximates the number of households, and total adjusted gross income. The information is reported as a total per zip code, but it is also divided in four households' income groups: under \$25,000, between \$25000 and \$50,000, between \$50,000 and \$100,000, and above \$100,000. We calculate the average household income in each zip code and in each income bracket by dividing the adjusted gross income reported by the number of tax returns. We then use these income averages to compute measures of income inequality at the county level taking the average income of households in a given zip code and income bracket as the income of a representative household.

Finally, we create the dummy variable *High % self-employed* that equals one for counties with above-median self-employment rates, and zero otherwise. The self-employment rate is the number of non-farm self-employed individuals over the entire labor force in the same geographic area. We calculate the median self-employment rate at the state-level and as of 1994 (the beginning of our sample period) to minimize potential endogeneity concerns.[10]

We provide descriptive statistics for all county-level variables obtained from IRS in Panel B of Table 2.

3.7. County-level poverty measures

We obtain county-level measures of poverty from the Census Bureau's Small Area Income Poverty Estimates (SAIPE), which is the source of the official national estimates of poverty levels and rates.[11] The SAIPE provide measures of poverty for each county on an annual basis since 1993. The measures of poverty are based on estimates produced from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey. Poverty is determined by comparing the total income of

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[10] The median self-employment rate is 8.2%, ranging from 1.7% to 49%.

[11] The SAIPE also provide school district poverty estimates that are used by the U.S. Department of Education to allocate Title I ("Financial Assistance To Local Educational Agencies For The Education Of Children Of Low-Income Families") funds. For details on the SAIPE data see Bell et al. (2007).

the family to poverty thresholds for that family size. The thresholds account for annual changes in the Consumer Price Index that are published periodically by the U.S. Census Bureau.

We use two poverty measures in our analysis. *Percentage of poor* is the percentage of households with income below the poverty threshold in a given county and year. *Number of poor* is the number of households with income below the poverty threshold in a given county and year. Descriptive statistics for the poverty measures are presented in Panel C of Table 2.

4. 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. We cluster standard errors at the state level to account for serial correlation within states.[12]

The main coefficient of interest, β, measures the effect of the exemption laws on income distribution. The identification of this parameter rests on two important features of our empirical setting. First, the regression model accounts for the fact that we have several exemption laws staggered during our sample period. Consequently,

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

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. In fact, one of the robustness tests we perform is to estimate the above regression on the subsample of states that changed their exemption limits during our sample period.

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).

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 with the following regression:

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

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 the state-level control variables.

Finally, we run 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 (i.e., high versus low credit needs). The dependent variable is a measure of income inequality in each state and year, and in each of these two types of industries. Therefore, we have twice as many observations in this regression compared to our previous state-level specification. 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 levels.[13] One implication of including state-year fixed effects is that they absorb the *Exemptions* variable, our control variables, but also any aggregate shocks hitting states at any point in time. Therefore, we can identify only the differential effect of the exemptions in industries with high as opposed to low credit needs. We cluster standard errors at the state-industry level.

5. Exemptions and income inequality

In Table 3 we study the effect of exemptions on the distribution of income using CPS data for the period 1994-2006. We analyze five measures of income inequality: two based on the Gini index (specifications 1 and 2), one based on the Theil index (specification 3), and two based on income percentile ratios (specifications 4 and 5). All regressions include state fixed effects, year fixed effects,

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[13] This is a standard triple differences specification. For examples of papers in the finance literature using this type of specification, see Cetorelli and Strahan (2006), Larrain (2015), and Boustanifar (2014).

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. This is our main result. 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 A.2), the exemptions explain 25% of the variation in income inequality relative to state and year averages.[14]

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.

Our main finding of a positive effect of exemptions on income inequality survives to a comprehensive battery of robustness tests, which we discuss in detail below. By way of preview, our result holds when we: (i) control for state minimum wage laws, (ii) control for cross-state migration, (iii) drop unemployed individuals or individuals with outlying income, (iv) control for various house price indices, (v) drop one state at a time, exclude states that did not change their exemption limits during

[14] To put this number into perspective, Beck et al. (2010) find that branching deregulation explains 60% of the variation of income inequality after controlling for state and year effects.

our sample period, or exclude states that increased exemptions by small nominal amounts, (vi) use alternative measures of exemptions, and (vii) analyze separate age groups.

5.1. Reverse causality concerns

Our empirical analysis rests on the assumption that the passage of the exemption laws is unrelated to the distribution of income. It is therefore important to investigate the full dynamic response of income inequality to the exemption laws. Figure 1 plots year-by-year coefficient estimates and 95% confidence intervals of the effect of the exemptions on the log Gini (top panel) and on the log Theil (bottom panel) 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 1 confirms our main result that there is a significant increase in income inequality following the exemption laws. There are, however, three additional features of Figure 1 that merit special attention. First, 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. Second, the graphs show 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.

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

5.2. Evidence from individual income tax return data

We use income data from individual tax returns as reported by the IRS – Statistics of Income in order to validate empirically our main result with a different dataset. The income data are reported at the zip-code level for five households' income groups. We use these data to build income inequality measures at the county level for each year in which data are available (1998-2006).

Although the CPS is the official source of labor force statistics for the population of the United States, there are two advantages of using IRS data. First, it provides full coverage of the US income earning population. Second, the additional geographic granularity of the IRS data allows us to implement an alternative empirical strategy that relies on comparing contiguous county-pairs located on opposite sides of a state border (as in Dube et al., 2010).

Table 4 presents the estimation results with the IRS data. The three measures of inequality shown are the logistic Gini, log Gini, and log Theil. For each inequality measure, we estimate two specifications. The first one includes county fixed effects, year fixed effects, and the same set of state-level control variables of Table 3. This specification is therefore similar to those shown in Table 3 but with the analysis done at the county level rather than at the state level. The second specification additionally controls for county-pair specific year effects, which uses only variation in exemptions within each contiguous border county-pair. While in the first specification all counties that do not pass exemptions in a given year are in the control group, the second specification reduces the control group to contiguous border counties. The second specification should therefore do a better job at controlling for local economic conditions.

The results in Table 4 confirm that an increase in exemptions leads to higher income inequality. All estimated coefficients are statistically significant. In terms of magnitude, the coefficients are substantially larger in the second specification that compares contiguous counties than in the first, suggesting that differences in economic conditions are – if anything – biasing downwards our coefficients estimates.

5.3. 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 in each percentile on exemption levels, controlling for state and year fixed effects and for the time-varying state variables reported in Table 3. Figure 2 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.[15]

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[15] We find similar results with the alternative dataset based on income tax returns (see section 4.1.2). For example, we find that the effect of the exemptions is -1.2% for the 25^{th} income percentile and 0.3% for the 75^{th} income percentile (both effects are significant at the 5% level).

5.4. Exemptions and poverty

When a family's total income is below an official poverty threshold, then the household is considered to be in poverty. Since the exemptions depress the incomes of individuals with already low incomes, it is important to quantify the percentage of households that are being pushed below the poverty line.

We investigate this question in Table 5 using data from the Census Bureau's Small Area Income Poverty Estimates (SAIPE). We compute two measures of poverty for each county and year, and for the period 1998-2006. The measure in column 1 is the percentage of households with income below the official poverty threshold. The measure in column 2 is the log of the number of households with income below the threshold.[16] Since the poverty data are at the county-level, we estimate the effect of the exemptions on poverty using variation within contiguous border county-pairs (as we did in subsection 5.2). Standard errors are clustered at the state level.

The results in Table 5 show that the exemptions increase poverty both in relative and absolute terms. All estimated coefficients are statistically significant. In terms of magnitude, the point estimate in column 1 indicates an increase of $100,000 in state exemptions increases the fraction of households below the poverty line by 0.74 percentage points (the unconditional poverty rate during our sample period is 14%).

6. Understanding the mechanism

A higher exemption limit reduces the amount of assets that creditors can seize from defaulting borrowers. Therefore, one potential explanation for our results is that higher exemptions affect income inequality via the credit market. As documented in

[16] For example, the poverty threshold in 2006 for a single individual was $10,294.

Robb and Robinson (2014), entrepreneurs rely heavily on external debt finance in order to operate and expand their businesses. If exemptions reduce credit more to some entrepreneurs than to others, this can create an imbalance in economic opportunities among them, which in turn can lead to more inequality.

Theory suggests that the exemptions should reduce credit especially to individuals with fewer assets (Gropp et al., 1997; Lilienfeld-Toal and Mookherjee, 2016). Consistent with this prediction, Cerqueiro and Penas (forthcoming) find that an increase in exemptions reduces credit available to start-ups owned by less wealthy entrepreneurs, forcing them to reduce their labor force and making their ventures more likely to fail.

One dimension through which the exemptions can increase income inequality is thus via a disproportionate reduction in credit availability to the less affluent business owners (i.e., via a "redistribution of credit").[17] Moreover, if some business owners 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 can increase income inequality among both business owners and wage workers.

We investigate the plausibility of this mechanism in three steps. First, we exploit differences across industries in capital needs to test whether the channel through which the exemptions affect income inequality is the credit market. Second, we assess the contribution of each population group (self-employed and wage

[17] We use the term "redistribution of credit" in a relative rather than in an absolute sense. Our proposed mechanism simply requires that the reduction in credit availability is stronger for some individuals than for others. Lilienfeld-Toal and Mookherjee (2016) show that following an increase in exemptions richer individuals can actually have greater access to credit than before due to general equilibrium effects. Although Gropp et al. (1997) and Cerqueiro and Penas (forthcoming) find some evidence consistent with this prediction, it is well documented that the first order effect of the exemptions is a net reduction in the supply of credit to households and small businesses.

workers) to the increase in inequality we find. Third, we investigate separately how each population group is affected by the exemptions.

6.1. The credit market channel

If the exemptions affect income inequality via the credit market, then we should see stronger effects for industries with higher capital needs. To investigate the role of credit markets, 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 6 presents the results from our industry analysis. The three measures of income inequality analyzed are the logistic Gini, log Gini, and log Theil. We calculate the inequality measures for each state, year, and industry type (i.e.. high versus low credit needs).[18] We interact the variable *Exemptions* with *High financial dependence* in columns 1, 3, and 5, and with *High startup capital* in columns 2, 4, and 6. All specifications include 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

[18] This explains why the number of observations in Table 6 is twice the number displayed in Table 3, in which inequality measures vary only at the state-year level.

exemptions on income inequality for industries with high versus low capital needs. We cluster standard errors at the state-industry level.

The results in Table 6 show that the effect of the exemptions on income inequality is amplified in industries with high credit needs. All interaction terms have positive coefficients and are statistically significant. Although the two measures of credit needs yield similar results, they have different economic meaning. *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.[19]

The estimated effects are also economically relevant. For instance, consider the first column in Table 6 and an increase in state exemptions of $100,000. The point estimate indicates that the logistic Gini increases by 0.6% 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.[20]

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.

6.2. Self-employed and wage workers

We use the decomposition properties of the Theil index to assess how much of the increase in inequality is driven by self-employed individuals and how much is driven by wage workers. The Theil index equals the sum of the weighted average of

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[19] 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. See Appendix B for details.

[20] 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 A).

inequality within subgroups plus inequality between those subgroups. Accordingly, we decompose the effect of exemptions on income inequality into 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* these two population groups. 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 (similar to Table 3).

We report the results in Table 7. 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.[21] The results indicate that the increase in inequality is driven by an increase in inequality only within groups.

The next question is which of the groups drives the increase in inequality. Columns 4 and 5 provide the answer by reporting respectively the effects on inequality within the self-employed and within salaried workers. We find that increases in exemptions lead to significantly higher inequality among both the self-employed and salaried workers. However, the increase in inequality among the self-employed is four times larger than the increase in inequality among salaried workers. The stronger effect we find for the self-employed is consistent with our proposed mechanism, in which the exemptions have a direct effect on income inequality via a disproportionate reduction in credit to the less wealthy business owners. It is also important to note that the increase in inequality among wage workers, albeit smaller

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

in magnitude, is economically important because salaried workers represent about 90% of the labor force.

6.2.1. Are the self-employed the trigger?

We provide additional evidence that the direct channel through which the exemptions affect income inequality is via the self-employed. In particular, we use variation across counties in self-employment rates to investigate whether the effect of the exemptions on inequality depends on the incidence of self-employment.

Table 8 presents the results we obtain with the IRS data for the period 1998-2006. The three measures of inequality shown are the logistic Gini, log Gini, and log Theil. All regressions include county fixed effects and year fixed effects. For each inequality measure, we estimate two specifications. The first specification in columns 1, 3, and 5 presents baseline estimates of the effect of the exemptions on inequality. The results we obtain with this specification confirm our earlier findings in Table 4 of a positive effect of the exemptions on inequality.

The second specification in columns 2, 4, and 6 adds an interaction of the exemptions with a dummy that equals one for counties with above-median self-employment rates.[22] Both estimated coefficients in the second specification merit our attention. On the one hand, the coefficients on the *Exemptions* are economically small and statistically insignificant for all three inequality measures. This is an important result because it shows that there is virtually no statistically significant effect of the exemptions on inequality when we mute our proposed channel (i.e., when self-employment rates are low). On the other hand, the estimates obtained for the interaction terms are positive and highly significant for all three measures of inequality, indicating that the effect of the exemptions on inequality is driven mainly

[22] The median is calculated across all states in 1994. See Section 3.6. for details on this measure.

by counties with high self-employment rates. In sum, both estimates corroborate the view that the self-employed are the primary channel through which the exemptions affect inequality.

6.2.2. Wage workers and the labor market

Table 7 shows that the exemptions increase inequality also among wage workers. Wage workers represent the majority of the labor force and therefore it is important to understand what adjustments are taking place in the labor market.

Cerqueiro and Penas (forthcoming) show that the exemptions reduce credit availability to many small business owners, who in response reduce employment or even shut down their businesses. The reduction in employment, however, should not affect all types of workers equally. There is ample evidence that unskilled workers are more sensitive to labor market conditions than skilled workers. For instance, Mincer (1991) documents that unskilled workers face substantially higher risk of becoming unemployed, especially during recessions. Topel (1993) shows that the unemployment rate of unskilled workers is not only higher, but also much more volatile.[23] It is therefore reasonable to expect unskilled wageworkers to be more negatively affected by the exemptions than skilled workers.[24]

6.2.2.1. Relative wages and working hours

Building on this literature, we analyze how the exemptions affect the relative wage gap between unskilled and skilled workers as well as their relative working

[23] There are several theories that explain why skilled individuals have more employment stability. One explanation is that the cost of hiring (and firing) skilled labor is substantially higher (Oi, 1962; see Manning (2011) for a recent review of this topic). Mincer (1991) argues that skilled workers are more likely to get on the-job-training, which gives incentives to employers to keep these workers.

[24] Consistent with this view, Beck et al. (2010) and Larrain (2015) show in different settings that changes in credit market conditions have differential effects on the wages of unskilled and skilled workers.

hours. Unskilled workers are those without any college education, while skilled workers have some college education. We also analyze differential labor market responses to the exemptions across industries with high versus low credit needs to test whether the effects found are consistent with our proposed mechanism. In particular, if the exemptions force some credit-constrained business owners to reduce their relative demand for unskilled labor, the decline in relative wages and working hours should be more pronounced in industries with high credit needs.

We investigate in Table 9 the effect of exemptions on labor market outcomes using individual-level data from the Outgoing Rotation Groups CPS files.[25] The dependent variables are the log of real wages (columns 1 and 2) and the log of weekly working hours (columns 3 and 4) of unskilled relative to skilled workers. Following Beck et al. (2010), we calculate the wage and working hours of each unskilled worker relative to the expected wage and working hours of a skilled worker with identical demographic characteristics (we explain the methodology in detail in Appendix C). Unemployed individuals are excluded from the analysis. All specifications include state fixed effects, year fixed effects, and the time-varying state variables reported in Table 3. We cluster standard errors at the state level.

The first specification in columns 1 and 3 assesses the average effect of the exemptions on labor outcomes across all industries. The second specification in columns 2 and 4 adds an interaction of the exemptions variable with a dummy that indicates whether the worker is in an industry with high start-up costs. The estimate in column 1 shows that following a €100,000 increase in exemptions, the relative wage of unskilled workers falls by 1.3% (the effect is statistically significant at the 1%

[25] We use Outgoing Rotation Group waves of CPS for this analysis because they include statistics on individuals' weekly working hours and wages.

level). Column 2 shows that although the relative wage of unskilled workers falls in all industries, the drop is more pronounced in industries with high start-up costs.[26]

For the relative working hours we obtain in column 3 a negative and insignificant effect of the exemptions. Column 4 shows that there is substantial heterogeneity in this effect across industries. While relative working hours do not change in industries with low start-up costs, they drop significantly for industries with high start-up costs. In particular, the estimated coefficients indicate that relative working hours of unskilled workers drop 6% following an increase in exemptions.

In sum, unskilled workers earn less and work less relative to skilled workers when the exemptions increase. This suggests that the exemptions lead to a reduction in the demand for unskilled labor. This is an important result because it shows that the exemptions affect negatively a group of individuals who ex ante should benefit more from generous debtor protection laws. One legitimate concern, however, is reverse causality. For instance, it could be that the exemptions laws are passed in response to the political pressure of lower-income groups (Gala, Kirshner, and Volpin, 2009).

As before, we address this concern by tracing the year-by-year changes in the labor outcome variables around the passage of the exemption laws. In particular, we analyze the dynamic behavior of relative wages and relative working hours of unskilled workers in industries with high capital needs, because our results are mostly driven by those industries. Figure 3 plots the year-by-year coefficient estimates and 95% confidence intervals of the effect of the exemptions on the relative wage (top graph) and on the relative working hours (bottom graph) of unskilled workers. As in

[26] A reduction in the demand for unskilled labor in industries with high start-up costs can create downward pressure on the wages paid in all industries because unskilled workers typically have higher mobility across industries (say, from retail, which has high capital requirements, to construction, which has low capital requirements) than workers who have more specific skills (Mincer, 1991).

the baseline regressions in Table 9, we include state fixed effects, year fixed effects, and the same set of control variables. Standard errors are clustered at the state level.

The figure confirms the drop in relative wage and working hours that follow an increase in exemptions. More importantly, there is no evidence of pre-trends in these labor market outcomes. The coefficient estimates for all years preceding the exemption laws are economically small and statistically insignificant. We thus obtain strong evidence that the labor market results are not driven by reverse causality.

6.2.2.2. Employment rates

The drop in working hours does not take into account individuals who are unemployed. For this reason we also test in Table 10 how the exemptions affect the employment rates of unskilled and skilled workers. We compute employment rates as the number of wageworkers in each group (i.e., all workers in columns 1 and 2, unskilled workers in columns 3 and 4, and skilled workers in columns 5 and 6) divided by the total labor force. In columns 1, 3, and 5 we analyze employment rates at the state-year level, controlling for state fixed effects, year fixed effects and the same set of state control variables shown in Table 3. In columns 2, 4 , and 6 we analyze employment rates at the state-industry-level to assess differential effects across industries with high versus low high startup capital. This second specification is saturated with fixed effects at the state-year, industry-year, and state-industry level (as in Table 6).

The results in column 1 show that individuals are less likely to be employed following an increase in exemptions. The estimated effect is quantitatively small, but nevertheless relevant. The point estimate indicates that a $100,000 increase in state exemptions decreased the employment rate by 0.11 percentage points, which corresponds to roughly 150,000 individuals.[27] Column 2 shows that this effect is amplified in industries with high (relative to low) startup costs, once again confirming that our findings are driven by a change in credit market conditions.

The subsequent columns speak to the question of which of the groups is most affected. The answer is unambiguous. Column 3 shows that following an increase in exemptions unskilled individuals experience substantially higher unemployment rates. The estimated effect indicates a 0.43 percentage points drop in the employment rate of unskilled individuals following a $100,000 increase in exemptions. Column 4 confirms that the drop in employment of unskilled workers is amplified in industries with high startup costs. For the skilled individuals we find no significant change in employment in any of the specifications.

The exemptions thus reduce working hours of unskilled workers both along the intensive and extensive margins.

7. Robustness tests

7.1. 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 Appendix Table D.1 we report results from our baseline regressions (i.e., similar to Table 3) when we control for state minimum

[27] Based on the authors' calculations using 1994 as the reference year and CPS data.

wages. We find that the effect of the exemptions on income inequality remains virtually unchanged.

7.2. Migration flows

One important concern is that the increase in inequality may be due to migration flows. For instance, Brinig and Buckley (1996) find that generous personal bankruptcy laws attract high human capital debtors who seek a fresh start from out-of-state creditors. One could therefore argue that an increase in exemptions attracts high-income migrants to the state, leading to an increase in income inequality. 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 Appendix Table D.2. The coefficients on the exemptions are similar to our baseline specifications, confirming that migration flows are not a confounding factor in our analysis.

7.3. Unemployed individuals

We investigate if our results are driven by the unemployed. We reconstruct 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 Appendix Table D.3 and show that most of the effect of the exemptions on income inequality is due to changes in the incomes of employed individuals, corroborating our labor market results (see Section 6.2.2.).

7.4. Controlling for house prices

Cerqueiro and Penas (forthcoming) 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 Appendix Table D.4 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.

7.5. 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 Appendix Table D.5 we test alternative measures of the exemptions, using as dependent variables the logistic Gini and the log of the Gini coefficient. In Columns 1 and 5 we replicate the baseline results of Table 3, which uses total exemptions. In Columns 2 and 6 we replace total exemptions by its natural logarithm. In Columns 3 and 7 we replace total exemptions by the homestead exemptions, which in most states is the most important type of exemption. In Columns 4 and 8 we consider the natural logarithm of homestead exemptions. Our results are robust to alternative definitions of the exemption variable.

7.6. Individuals with outlying income

In our analysis we excluded individuals with incomes in the bottom or top 1% of the income distribution. To test the sensitivity of our results to these exclusions, 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 1st and the 99th percentile (this is our baseline specification). We report the results for the logistic Gini and for the log of Gini in Appendix Table D.6. The results we obtain are similar across all specifications shown.

7.7. Age groups

Our CPS sample contains individuals aged 25-64. In this section, we repeat the main analysis for the three following 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 separate panels in Appendix Table 7. The results are similar in terms of statistical and economic significance across the different age groups.

7.8. Standard errors

In our baseline specification, we cluster standard errors at the state level. We replicate the results in Table 3 using alternative methods for computing the standard errors. In particular, we compare our baseline clustered standard errors with bootstrapped standard errors and SUR standard errors. The results shown in Appendix Table D.8 indicate that clustering the standard errors at the state level leads to conservative statistical inference.

7.9. Dropping small exemption changes

Some states made very small changes to their exemption limits (see Panel B of Table 1), which typically reflect statutory increases in the nominal value of exemptions based on inflation (Cerqueiro and Penas, forthcoming). Our empirical methodology exploits variation in the intensity of treatment, implying that the larger changes in exemptions should matter more than the smaller ones. Therefore, removing the smaller exemption changes should not drastically affect our findings.

This is precisely what we find in Appendix Table D.9. This table shows that our results in Table 3 hold when we ignore increases in exemptions of less than $10,000.

7.10. Dropping control states

In Appendix Table D.10 we re-estimate the regressions displayed in Table 3 after dropping states that never changed their exemption limits during our sample period (1994-2006). Removing these control states may provide a better counterfactual, since these states might be fundamentally different from the treated states. We are able to identify the effect of the exemptions using only the treated states due to the staggering of the exemption laws. Our main results hold when we use the subsample of treated states.

7.11. Influential states

Table 1 shows that some states raised exemptions more than once (in Panel A) and that some states experienced very large changes in exemption limits (in Panel B). 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. In Appendix Figure D.11 we plot 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.[28]

[28] 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.

8. 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 is mainly due to a drop in the incomes of individuals in lowest quartile of the income distribution and is also accompanied by an increase in poverty.

We also obtain several findings that collectively point to financing frictions affecting small business owners as the likely mechanism behind these results. In particular, we show that the increase in inequality is mediated by the credit market and by the self-employed individuals. We also find evidence of spillovers to the labor market. Following an increase in exemptions, the unskilled workers earn less, work fewer hours, and are more likely to be unemployed than skilled workers. These results show that the exemptions affect the distribution of income among salaried workers by reducing the demand for unskilled labor.

Our study highlights an unintended consequence of bankruptcy protection. In particular, a debtor-friendly personal bankruptcy regime reduces disproportionally credit available to individuals with fewer assets, thereby hurting their economic opportunities.

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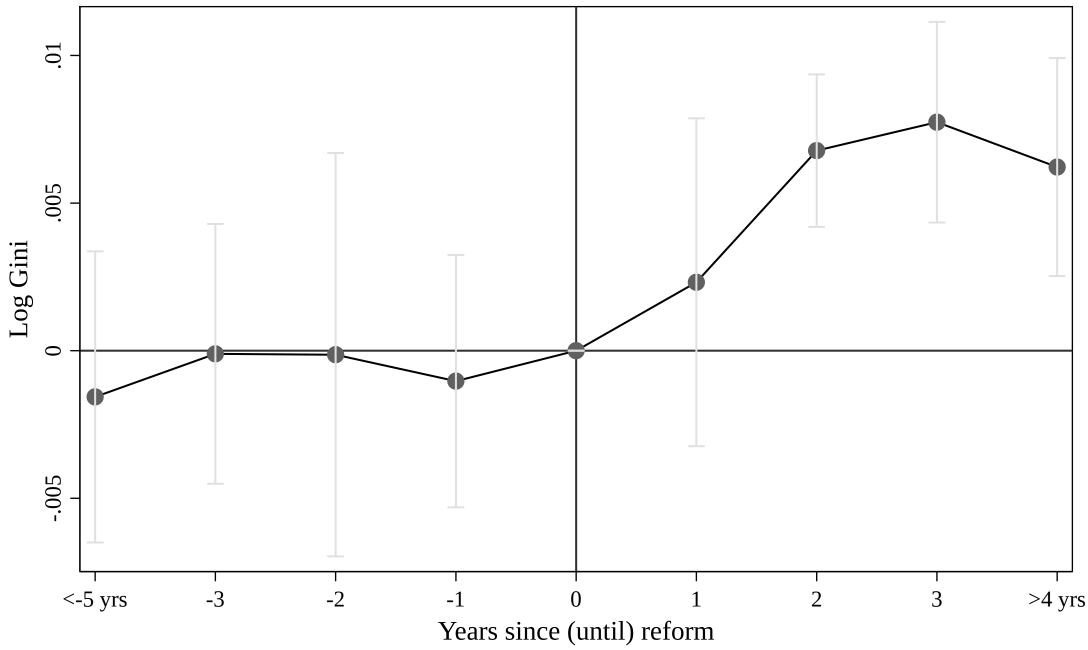

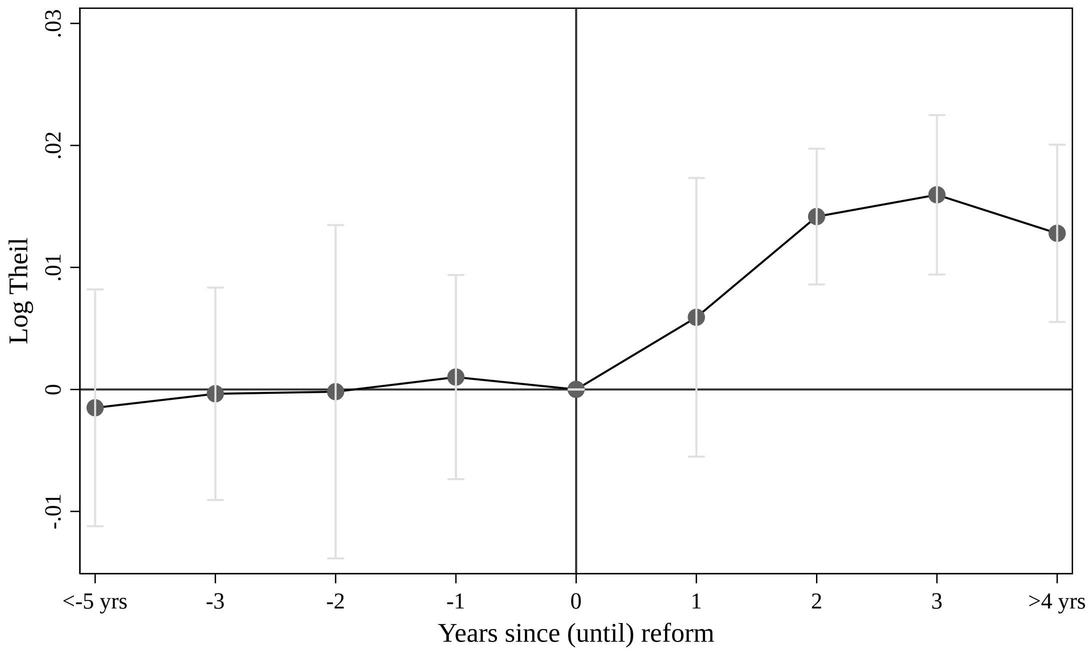

Figure 1. The dynamic effect of bankruptcy exemptions on income inequality. Data are from the CPS for the period 1994-2006. The figure displays point estimates and 95% confidence intervals of the estimated effect of the exemption laws on income inequality for each year around the law change. The dependent variables are the logarithm of the Gini index (top graph) and the logarithm of the Theil index (bottom graph). The regressions include state fixed effects, year fixed effect, and the state level controls displayed in Table 3. Standard errors are clustered at the state level.

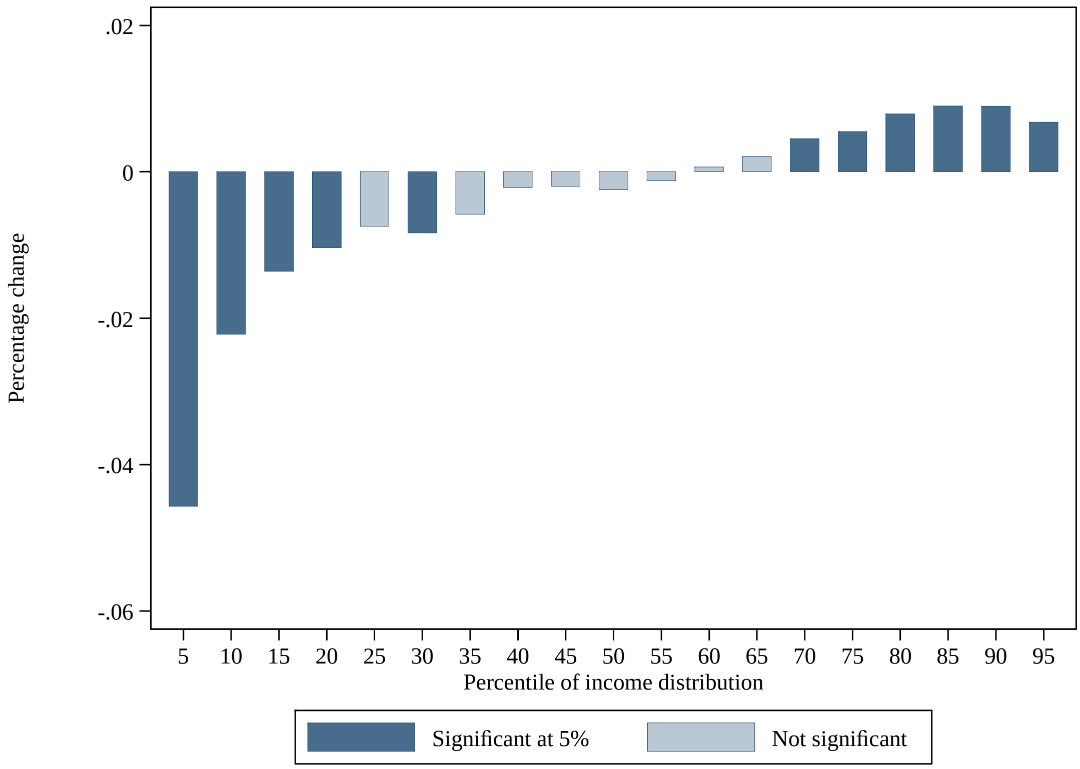

Figure 2. The impact of bankruptcy exemptions on different income groups.
Data are from the CPS for the period 1994-2006. The figure shows the impact of the exemption laws on different percentiles of the income distribution. Each bar in the figure represents the estimated impact of the exemptions on the natural logarithm of a given percentile of the income distribution, after controlling for state fixed effects, year fixed effects, and the state controls displayed in Table 3. Standard errors are clustered at the state level. Dark bars indicate significant estimates at the 5% level.

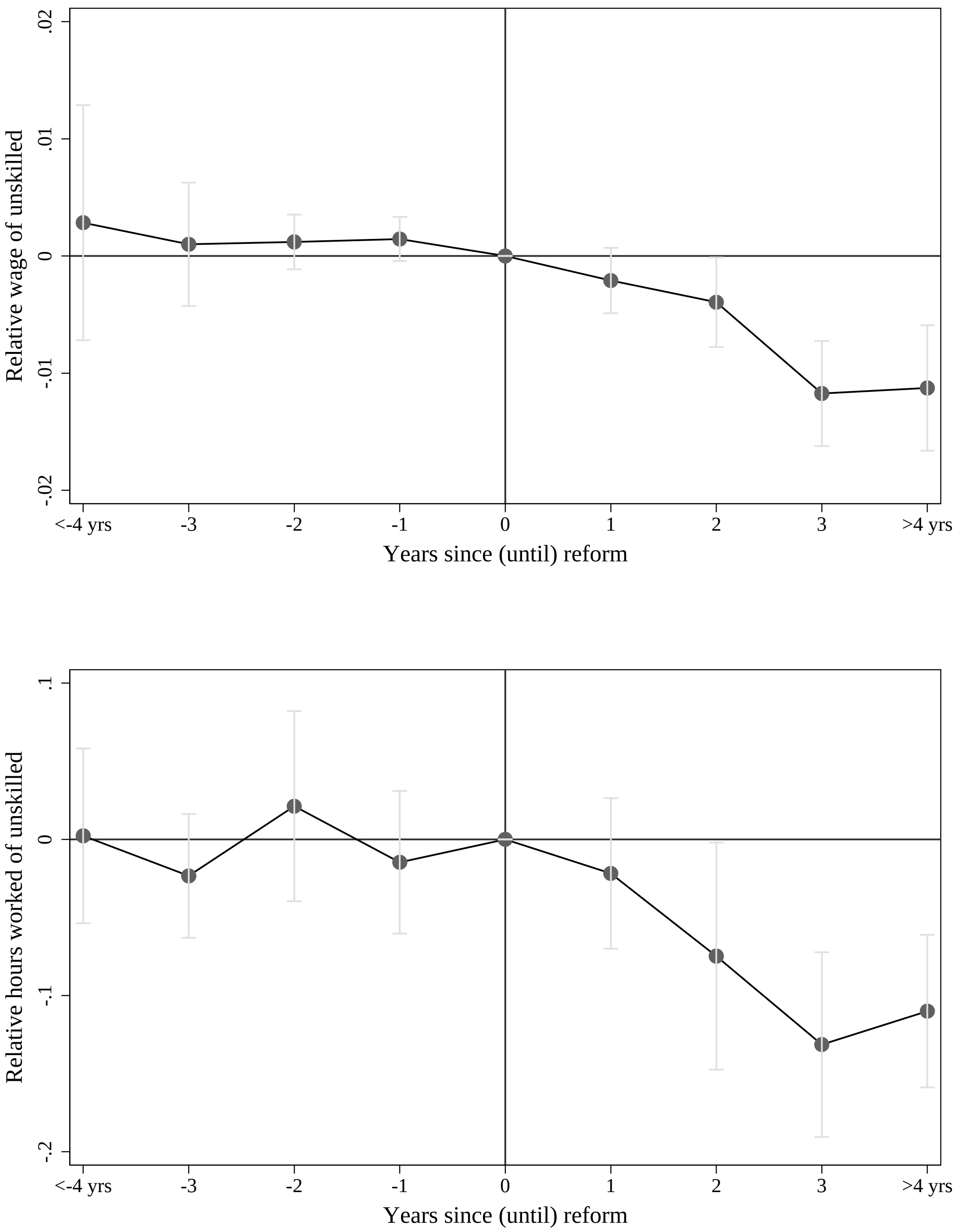

Figure 3. The dynamic effect of bankruptcy exemptions on the labor market

Data are from the CPS-ORG for the period 1994-2006. The figure displays for each year around the law change point estimates and 95% confidence intervals of the effect of the exemption laws on the log of real hourly wages of unskilled workers relative to skilled workers (top panel) and on the number of weekly working hours of unskilled workers relative to skilled workers (bottom panel). Skilled workers are those with 13 or more years of completed education, while unskilled workers completed at most 12 years of education. Relative wages are calculated after controlling for experience, race, and gender, and after allowing for time-varying returns to these characteristics (see the Appendix for details). Standard errors are clustered at the state level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 1. State exemption laws

State exemptions include the homestead exemption and the personal property exemptions. The exemption laws are hand-collected from individual state codes.

Panel A: States Changing Bankruptcy Exemption Levels, 1994-2006

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

Panel B: Distribution of Changes in State Exemption Levels, 1994-2006

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

Panel A: CPS data

This panel shows descriptive statistics of the variables used in the main analysis (see Section 3 for details). The unit of analysis is state-year. The sample period is 1994-2006. We use total personal income and sampling weights in the CPS to calculate each inequality measure for each state and year. We calculate the proportion blacks, the proportion of high school drop-outs, and and the proportion of female-headed households 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 groups are calculated from CPS data. Skilled workers are those with 13 or more years of completed education, while unskilled workers completed at most 12 years of education.

	Mean	St. dev.	Min	Perc. 10	Perc. 25	Perc.50	Perc.75	Perc. 90	Max
Measures of inequality									
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
State-level control variables									
Proportion of blacks	0.10	0.11	0.00	0.01	0.02	0.06	0.14	0.26	0.65
Real growth rate of per capita GDP	0.02	0.03	-0.10	-0.01	0.00	0.02	0.03	0.05	0.18
Unemployment rate (%)	4.85	1.20	2.30	3.30	4.00	4.80	5.60	6.50	8.70
Proportion of high school drop-outs	0.10	0.03	0.03	0.06	0.07	0.09	0.12	0.15	0.21
Proportion of female-headed households	0.40	0.07	0.20	0.31	0.36	0.41	0.46	0.49	0.59
Employment rates									
Wage workers / Labor force (%)	87.71	2.76	77.46	83.96	86.09	88.34	89.58	90.79	93.74
Unskilled wage workers / Labor force (%)	53.39	6.59	37.89	46.16	49.41	52.88	56.80	60.13	83.89
Skilled wage workers / Labor force (%)	34.32	7.08	4.10	26.32	30.32	34.64	38.99	42.80	50.62

Table 2. (continued)

Panel B: IRS data

This table shows descriptive statistics for the IRS-SOI sample for the period 1998-2006. The unit of analysis is county-year. The measures of income inequality are the logistic transformation of the Gini coefficient, the natural logarithm of the Gini coefficient, and the natural logarithm of the Theil index. *High % self-employed* equals one for counties with self-employment rates higher than the sample median of their state, and equals zero otherwise. We calculate the self-employment rate from CPS data as the number of non-farm self-employed individuals over the entire labor force in the state in 1994.

	Mean	St. dev.	Min	Median	Max
Measures of inequality					
Logistic Gini	-0.04	0.22	-1.37	-0.06	0.54
Log Gini	-0.72	0.11	-1.60	-0.72	-0.16
Log Theil	-0.82	0.27	-2.47	-0.84	-0.12
Other variables					
High % self-employed	0.5	0.5	0	1	1

Panel C: Poverty data

Poverty data are from the Census Bureau's Small Area Income Poverty Estimates for the period 1998-2006. The unit of analysis is county-year. The measures of poverty are the percentage of households with income below the poverty threshold in a given county and year (column 1) and the log of the number of households with income below the poverty threshold in a given county and year (column 2). The poverty thresholds are published annually by the Census Bureau.

	Mean	St. dev.	Min	Median	Max
Measures of poverty					
Percent of poor (%)	14.05	5.73	0	13.1	51
Number of poor	8.24	1.50	1.95	8.15	15.41

Table 3. The impact of bankruptcy exemptions on income inequality (CPS data)

Income data are from the CPS for the period 1994-2006. The unit of analysis is state-year. 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. 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)
State controls					
Proportion of 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 of high-school dropouts	0.664***	0.381***	0.781***	0.852*	0.448**
	(0.153)	(0.088)	(0.191)	(0.457)	(0.198)
Proportion of 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
Number of observations	663	663	663	663	663
R-squared	0.133	0.132	0.170	0.354	0.285

Table 4. The impact of bankruptcy exemptions on income inequality (IRS data)

Income data are from the IRS-SOI for the period 1998-2006. The unit of analysis is county-year (columns 1, 3, and 5) and county-county pair-year (columns 2, 4, and 6). The measures of income inequality are the logistic transformation of the Gini coefficient (columns 1 and 2), the natural logarithm of the Gini coefficient (column 3 and 4), and the natural logarithm of the Theil index (columns 5 and 6). State controls include the proportion of blacks, the real growth rate of per capita GDP, the unemployment rate, the proportion of high-school dropouts, the proportion of high-school dropouts, and the proportion of female-headed households. Income data are from the IRS Statistics of Income. Standard errors are clustered at the county 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	
	(1)	(2)	(3)	(4)	(5)	(6)
Exemptions ($100,000)	0.017**	0.026**	0.008**	0.012**	0.016**	0.032**
	(0.007)	(0.10)	(0.003)	(0.005)	(0.008)	(0.013)
State controls	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No	Yes	No
County pair × Year fixed effects	No	Yes	No	Yes	No	Yes
Number of observations	18,221	6,862	18,221	6,862	18,221	6,862
R-squared	0.454	0.750	0.431	0.739	0.526	0.721

Table 5. The impact of bankruptcy exemptions on poverty (SAIPE data)

Poverty data are from the Census Bureau's Small Area Income Poverty Estimates for the period 1998-2006. The unit of analysis is and county-county pair-year. The dependent variables are the percentage of households with income below the poverty threshold in a given county and year (column 1) and the log of the number of households with income below the poverty threshold in a given county and year (column 2). 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:	Percent of poor	Log(Number of poor)
	(1)	(2)
Exemptions ($100,000)	0.074**	0.025***
	(0.30)	(0.004)
State controls	Yes	Yes
County fixed effects	Yes	Yes
County pair × Year fixed effects	Yes	Yes
Number of observations	6,862	6,862
R-squared	0.848	0.896

Table 6. Credit market channel (CPS data)

Income data are from the CPS for the period 1994-2006. The unit of analysis is state-industry-year. The measures of income inequality are: the logistic transformation of the Gini coefficient (columns 1 and 2), the natural logarithm of the Gini coefficient (columns 3 and 4), and the natural logarithm of the Theil index (columns 5 and 6). We calculate for each state and year separate income inequality measures for industries with high and low credit needs. *High financial dependence* is based on the Rajan and Zingales (1998) index and it equals one for industries with above-median external financial dependence, 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. State exemptions are expressed in 100,000 dollars. 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	
	(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
Number of observations	1326	1326	1326	1326	1326	1326
R-squared	0.603	0.611	0.604	0.612	0.621	0.627

Table 7. Self-employed and salaried workers (CPS data)

Income data are from the CPS for the period 1994-2006. The unit of analysis is state-year. 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 controls include the proportion of blacks, the real growth rate of per capita GDP, the unemployment rate, the proportion of high-school dropouts, the proportion of high-school dropouts, and the proportion of female-headed households. 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 effect	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 8. Exploring heterogeneity in self-employment rates (IRS data)

Income data are from the IRS-SOI for the period 1998-2006. The unit of analysis is county-year. The measures of income inequality are the logistic transformation of the Gini coefficient (columns 1 and 2), the natural logarithm of the Gini coefficient (columns 3 and 4), and the natural logarithm of the Theil index (columns 5 and 6). *High % self-employed* equals one for counties with self-employment rates higher than the sample median, and equals zero otherwise. We calculate the self-employment rate from CPS data as the number of non-farm self-employed individuals over the entire labor force in the state in 1994. State controls include the proportion of blacks, the real growth rate of per capita GDP, the unemployment rate, the proportion of high-school dropouts, the proportion of high-school dropouts, and the proportion of female-headed households. Standard errors are clustered at the county 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	
	(1)	(2)	(3)	(4)	(5)	(6)
Exemptions ($100,000)	0.016**	0.003	0.007**	0.002	0.015*	0.001
	(0.007)	(0.006)	(0.003)	(0.003)	(0.008)	(0.008)
Exemptions × High % self-employed		0.032***		0.014***		0.033***
		(0.006)		(0.003)		(0.008)
State controls	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	18,217	18,217	18,217	18,217	18,217	18,217
R-squared	0.453	0.454	0.430	0.430	0.524	0.524

Table 9. Relative wage gaps between unskilled and skilled workers (CPS data)
Data are from the CPS-ORG for the period 1994-2006. The unit of analysis is worker-state-year. The dependent variables are the log of real hourly wages of unskilled workers relative to skilled workers (columns 1 and 2) and the number of weekly working hours of unskilled workers relative to skilled workers (columns 3 and 4). Skilled workers are those with 13 or more years of completed education, while unskilled workers completed at most 12 years of education. *High startup capital* is from Adelino et al. (2015) and equals one for industries with above-median startup capital needs, and zero otherwise. 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 (see the Appendix for details). State controls include the proportion of blacks, the real growth rate of per capita GDP, the unemployment rate, the proportion of high-school dropouts, the proportion of high-school dropouts, and the proportion of female-headed households. 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 worked by unskilled workers	
	(1)	(2)	(3)	(4)
Exemptions ($100,000)	-0.0126***	-0.00912***	-0.0239	-0.00926
	(0.00334)	(0.00307)	(0.0275)	(0.0280)
Exemptions × High startup capital		-0.00479***		-0.0511***
		(0.00184)		(0.00807)
State controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Number of observations	842,194	842,194	842,194	842,194
R-squared	0.022	0.035	0.006	0.006

Table 10. Employment rates of unskilled and skilled individuals (CPS data)

Income data are from the CPS for the period 1994-2006. The unit of analysis is state-year (columns 1, 3, and 5) and state-industry-year (columns 2, 4, and 6). The employment rate equals the number of workers in each group over the total labor force, in percent. In columns 1, 3, and 5 we calculate employment rates for each state and year. In columns 2, 4, and 6 we calculate employment rates for each state and year, and for industries with high and low credit needs. *High startup capital* is from Adelino et al. (2015) and equals one for industries with above-median startup capital needs, and zero otherwise. Skilled workers are those with 13 or more years of completed education, while unskilled workers completed at most 12 years of education. 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:	Employment rate in %					
	All wage workers		Unskilled wage workers		Skilled wage workers	
	(1)	(2)	(3)	(4)	(5)	(6)
Exemptions ($100,000)	-0.108**		-0.198**		0.0898	
	(0.0409)		(0.0901)		(0.0854)	
Exemptions × High startup capital		-0.146***		-0.216***		0.070
		(0.051)		(0.072)		(0.097)
State controls	Yes	No	Yes	No	Yes	No
State fixed effects	Yes	No	Yes	No	Yes	No
Year fixed effects	Yes	No	Yes	No	Yes	No
State × Year fixed effects	No	Yes	No	Yes	No	Yes
Industry × Year fixed effects	No	Yes	No	Yes	No	Yes
State × Industry fixed effects	No	Yes	No	Yes	No	Yes
Number of observations	663	1326	663	1326	663	1326
R-squared	0.189	0.105	0.467	0.205	0.534	0.285

Appendix for

Debtor Protection, Credit Redistribution, and Income Inequality

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Appendix A. Measures of income inequality

A.1. Additional information

Gini index

The Gini index is given by:

$$Gini = 1 - 2\int L(x)dx,$$

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 ranges between 0 and 1. It is equal to 0 in the case of perfect equality among individuals and it equals 1 if all the income is held by one individual. The Gini index makes use of all information about the income distribution, which makes it sensitive to changes in the middle of the distribution.

Theil index

The Theil index is given by:

$$T_T = \frac{\sum_{i=1}^{N}\left\{\left(\frac{y_i}{\bar{y}}\right) ln\left(\frac{y_i}{\bar{y}}\right)\right\}}{N},$$

where *N* is the number of individuals, y_i is the personal income of individual i, and $\bar{y}$ is the mean value of personal income. The Theil index ranges between 0 (in the case of perfect equality) and *Ln(N)* (when all income is concentrated in one individual). The Theil index can be decomposed into the between-group and between-group components as follows:

$$T_T = \sum_{i=1}^{m} s_i T_{T_i} + \sum_{i=1}^{m} s_i ln\frac{\bar{y_i}}{\bar{y}},$$

where m denotes the 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 of group i.

A.2. Additional statistics

The table displays additional 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) for the period 1994-2006. We use total personal income and sampling weights in the CPS to calculate each inequality measure for each state and year. 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 deviation:	Cross-states	Within-states	Within state-years
Logistic Gini	0.073	0.045	0.044
Log Gini	0.042	0.026	0.025
Log Theil	0.084	0.056	0.053
Log 90/10	0.193	0.135	0.113
Log 75/25	0.105	0.065	0.057

Appendix B. Industry-level measures of credit demand

B.1. Descriptive statistics

The table shows industry information for the CPS sample. *High financial dependence* is based on the Rajan and Zingales (1998) index and it equals one for industries with above-median external financial dependence, 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.

Industry	High financial dependence	High startup capital	% of working population
Agriculture, forestry, fishing, hunting		0	2.4
Mining		1	0.7
Utilities		1	2.6
Construction		0	7.0
Manufacturing		1	17.3
Wholesale		0	3.9
Retail		1	10.0
Transportation		0	3.9
Information		1	1.8
Finance and Insurance		0	4.8
Real estate		1	1.8
Professional services		0	7.7
Administrative services		0	6.8
Educational services		0	9.8
Health care		0	8.8
Art and recreation		1	1.3
Accommodation and food services		1	5.1
Other services		0	4.4

Appendix C. Worker-level income data from CPS-ORG

C.1. Methodology

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 the set of individual characteristics mentioned above (experience, race, and gender). We include not only the level, but also the square, cubic, and quartic terms of these explanatory variables, as well as cross-interaction terms. We obtain the time-varying returns to the personal characteristics, $\beta_t^{skilled}$, by estimating the above equation for each years of our sample. We also include a constant term in X_{ist}, so that we obtain an estimate of the conditional mean skilled wage rate in each year.

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 rationale behind this approach is to control for differences in returns to personal characteristics between unskilled and skilled workers. Note that when we compute the relative unskilled wage rates from the above equation, the conditional mean skilled wage rate in each year is part of the second term (and hence it is subtracted from the first term).

We compute the relative working hours of unskilled workers following the same two-step procedure as above.

C.2. Descriptive statistics

Worker-level data are from the CPS-ORG for the period 1994-2006. The variables are the log of real hourly wages of unskilled workers relative to skilled workers and the number of weekly working hours of unskilled workers relative to skilled workers. Skilled workers are those with 13 or more years of completed education, while unskilled workers completed at most 12 years of education. 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 (see the previous subsection). The total number of observations is 842,194.

	Mean	St. dev.	Min	Median	Max
Relative wage of unskilled workers	-0.27	0.41	-2.47	-0.30	0.70
Relative hours worked by unskilled workers	1.92	8.61	-14.86	2.04	14.95

Appendix D. Robustness tests

D.1. Controlling for state minimum wage laws

This table replicates Table 3 in the paper after controlling for state-level minimum wages laws. *, **, 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 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	0.135	0.134	0.171	0.358	0.286

D.2. Dropping recent immigrants

This table replicates Table 3 in the paper after dropping individuals who immigrated to the state during the previous year. *, **, 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 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	0.125	0.124	0.170	0.355	0.295

D.3. Dropping unemployed individuals

This table replicates Table 3 in the paper after dropping unemployed individuals. *, **, 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 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	0.127	0.126	0.170	0.338	0.270

D.4. Controlling for house prices

This table replicates Table 3 in the paper after controlling for house prices. House price data are based on the all-transactions house price index (HPI) from the FHFA. *, **, 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 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	0.149	0.148	0.184	0.377	0.288

D.5. Alternative measures of exemptions

This table replicates Table 3 in the paper using different 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 (see Section 2 in the paper for details). *, **, 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)
State controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	663	663	663	663	663
R-squared	0.133	0.139	0.142	0.138	0.132	0.139	0.141	0.138

D.6. Individuals with outlying income

This table replicates Table 3 in the paper after dropping individuals with outlying income. In columns 1 and 5 we include all individuals. 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 (this is our baseline specification in Table 3). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable:	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)
State controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	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

D.7. Results across different age groups

This table replicates Table 3 in the paper for different age groups. *, **, 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
			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)
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

D.8. Standard errors

This table replicates Table 3 in the paper under alternative standard error estimates. 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.

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
Clustered errors	(0.003)***	(0.002)***	(0.003)***	(0.008)***	(0.005)**
Bootstrapped errors	(0.002)***	(0.001)***	(0.003)***	(0.007)***	(0.005)**
SUR errors	(0.003)***	(0.001)***	(0.003)***	(0.007)***	(0.003)***
State controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared	0.133	0.132	0.170	0.354	0.285
Observations	663	663	663	663	663

D.9. Dropping small exemption changes

This table replicates Table 3 in the paper after ignoring exemption laws that increased exemptions by less than $10,000. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

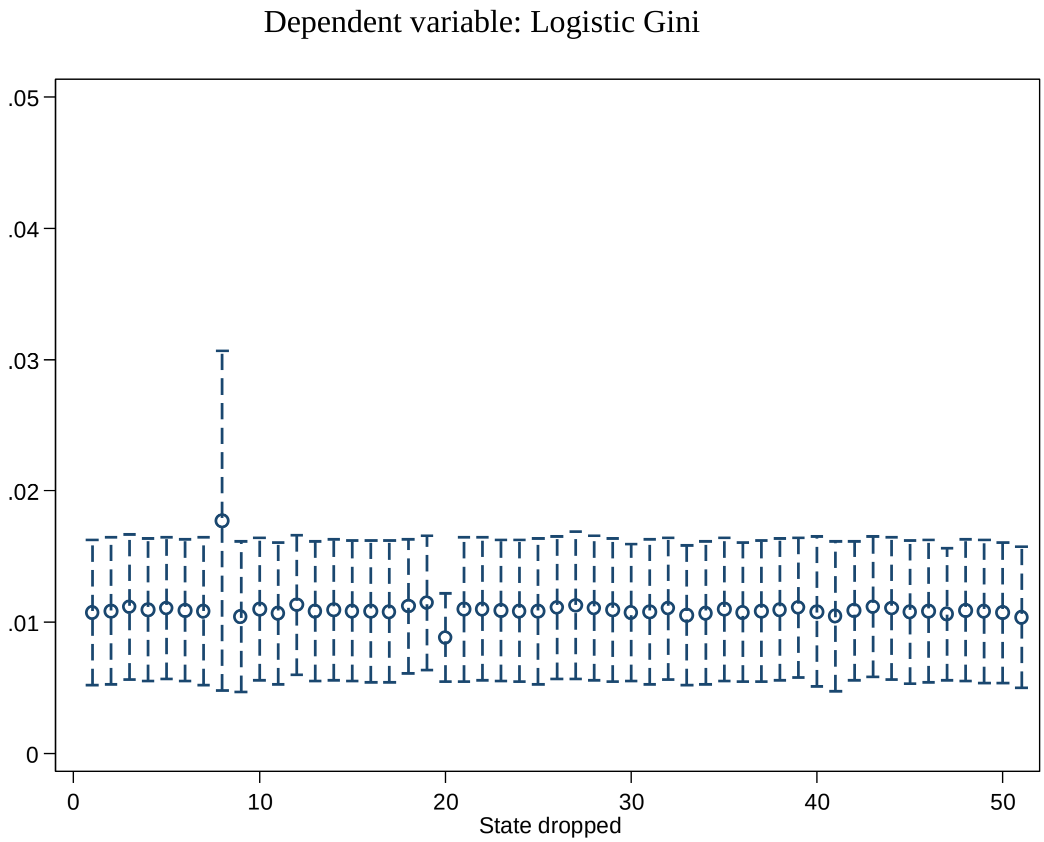
Dependent variable:	Logistic Gini	Log Gini	Log Theil	Log 90/10	Log 75/25
	(1)	(2)	(3)	(4)	(5)
Exemptions ($100,000)	0.011***	0.006***	0.011***	0.031***	0.012***
	(0.003)	(0.001)	(0.003)	(0.008)	(0.005)
State controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared	0.133	0.132	0.170	0.355	0.285
Observations	663	663	663	663	663

D.10. Dropping states that do not change exemptions

This table replicates Table 3 in the paper after dropping states never changed their exemption limits during our sample period from 1994 to 2006. *, **, 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***	0.007***	0.013***	0.030***	0.012**
	(0.003)	(0.002)	(0.004)	(0.009)	(0.006)
State controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared	0.139	0.139	0.180	0.321	0.255
Observations	494	494	494	494	494

D.11. Excluding one state at the time



This figure shows estimates of the impact of exemption laws on the logistic Gini 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. Data are from the CPS for the sample period 1994-2006. The dashed bars show the 95% confidence intervals. Standard errors are clustered at the state level.