Macroeconomic Effects of Debt Relief: Consumer Bankruptcy Protections in the Great Recession*

Adrien Auclert† Will Dobbie‡ Paul Goldsmith-Pinkham§

September 8, 2023

Abstract

This paper argues that the debt forgiveness provided by the U.S. consumer bankruptcy system helped stabilize employment levels during the Great Recession. We document that over this period, states with more generous bankruptcy exemptions had significantly smaller declines in non-tradable employment and larger increases in unsecured debt write-downs compared to states with less generous exemptions. We develop a general equilibrium model consistent with these reduced form estimates. The model suggests that the decline in aggregate employment would have been mitigated by around 7.5% if low bankruptcy protection states had received the same amount of debt relief as high protection states. *JEL codes*: D14, E32, F45, K35.

*First version: November 1, 2014. This version: September 8, 2023. A previous version of this paper circulated under the title "Debtor Protections and the Great Recession." We are extremely grateful to Guido Imbens, David Scharfstein, and Andrei Shleifer for their help and support. We thank David Berger, Jediphi Cabal, John Campbell, Gabriel Chodorow-Reich, Fritz Foley, Sonia Gilbukh, Sam Hanson, Kyle Herkenhoff, David Laibson, Atif Mian, Kurt Mitman, Ben Moll, Matt Rognlie, Isaac Sorkin, Jeremy Stein, Ludwig Straub, Adi Sunderam, Jacob Wallace, Johannes Wieland, Danny Yagan, Crystal Yang, Eric Zwick, and numerous seminar participants for helpful comments and suggestions. We also thank Joanne Hsu, David Matsa, and Brian Melzer for sharing their data on state Unemployment Insurance laws. Daniele Caratelli, Sara Casella, Ashley Litwin, Emma Rackstraw, Rebecca Sachs, and Bailey Palmer provided excellent research assistance. The views expressed are those of the authors and do not necessarily reflect those of the Federal Reserve Bank of New York or the Federal Reserve System. All remaining errors are ours.

†Stanford University, CEPR, and NBER. Email: aauclert@stanford.edu

‡Harvard Kennedy School and NBER. Email: will_dobbie@hks.harvard.edu

§Yale School of Management. Email: paul.goldsmith-pinkham@yale.edu

There is a widespread view that the deterioration of household balance sheets contributed to the sharp decline in U.S. employment between 2007 and 2009. According to this view, the sudden fall in house prices and the simultaneous contraction in credit supply led to a large drop in aggregate consumption as households adjusted towards lower debt levels. Since monetary policy was unable to offset this fall in aggregate demand, employment levels collapsed nationwide (e.g., Eggertsson and Krugman 2012; Mian and Sufi 2014*b*). An important implication of this view is that ex-post debt forgiveness could have prevented such a collapse by mitigating the decline in aggregate demand.[1] To date, however, there is little credible evidence on the macroeconomic effects of debt relief. This type of evidence is critical for understanding the contribution of consumer bankruptcy to the business cycle, as well as evaluating proposals for large-scale debt forgiveness during recessions.

In this paper, we use a combination of quasi-experimental estimates and a general equilibrium model to estimate the effects of ex-post debt forgiveness on aggregate employment during the Great Recession. We focus on the debt forgiveness provided by the U.S. consumer bankruptcy system, the largest debt relief program in the United States. We overcome the endogeneity issues that bias simple time-series comparisons by first estimating the *relative* effect of ex-post debt forgiveness during the Great Recession, leveraging quasi-random variation in the generosity of bankruptcy asset exemptions across states. We then develop a general equilibrium model to recover the *aggregate* effect of this ex-post debt forgiveness that is consistent with these relative cross-state effects.

The U.S. consumer bankruptcy system allows eligible households who file for bankruptcy to discharge most unsecured debts, after relinquishing assets above state-specific exemption limits. Eligible households are thus insured against all financial risk above the level of assets that can be seized in bankruptcy in their state. Bankruptcy exemptions also provide a natural threat point in negotiations with creditors, providing a form of informal debt relief even when these households do not file for bankruptcy (e.g., Mahoney 2015). Households benefiting from the consumer bankruptcy system are, naturally, more financially distressed than the general population (e.g., Dobbie, Goldsmith-Pinkham and Yang 2017), and, as a result, have very high marginal propensities to consume (MPCs, e.g., Gross, Notowidigdo and Wang 2020). The debt forgiveness provided by the bankruptcy system could therefore boost aggregate demand if the households who implicitly finance the bankruptcy system have relatively low MPCs. While this is a natural hypothesis, our paper is the first to test it systematically.

[1] For example, Mian and Sufi (2014*a*) argue that "severe recessions are special circumstances because macroeconomic failures prevent the economy from reacting to a severe drop in demand. [...] When such failures prevent the economy from adjusting to such a large decline in consumption, government policy should do what it can to boost household spending. Debt forgiveness is exactly one such policy, and arguably the most effective."

The debt relief provided by the consumer bankruptcy system during economic downturns—and hence the potential boost to aggregate demand—is economically large, both in absolute terms and compared to other social insurance programs. Figure 1 shows the employment-to-population ratio in the United States between 2000 and 2017, along with estimates of the amount of unsecured credit discharged in Chapter 7 bankruptcy and total net charge-offs on non-real estate consumer credit in each year.[2] By way of comparison, the figure also shows total payments by the Unemployment Insurance (UI) system, an important automatic stabilizer. Both the amount of unsecured debt formally discharged through Chapter 7 bankruptcy and total net charge-offs on consumer credit increased sharply in 2008, just as employment fell across the country. Total net charge-offs on consumer credit reached a peak of almost 1.5 percent of PCE in 2010, about the same magnitude as total UI payments.

Our empirical strategy exploits the economically significant variation in the generosity of the bankruptcy exemptions—and hence the informal and formal debt relief provided by the bankruptcy system—across states. The cross-state variation in bankruptcy exemptions is particularly large for home equity, the largest and most important exemption category. New York, for example, allowed households to protect up to \$100,000 of home equity in bankruptcy prior to the crisis, while Illinois allowed households to protect only \$30,000. The variation in homestead exemptions has also been remarkably stable over time. The initial variation in homestead exemption amounts largely emerged as states formalized their state-level approaches to bankruptcy in the late nineteenth century, in ways that are likely unrelated to current state characteristics (Goodman 1993; Skeel 2001). We also show that a number of key state-level characteristics are uncorrelated with bankruptcy protections in the years just prior to the financial crisis. The combination of substantial cross-state variation in bankruptcy exemptions and economically large transfers during the financial crisis makes the consumer bankruptcy system an ideal setting to study the aggregate effects of ex-post debt forgiveness.

In the first part of the paper, we use this variation in a difference-in-differences design that compares outcomes in states with more generous bankruptcy exemptions (the "treated" states) to states with less generous exemptions (the "control" states). This difference-in-differences specification explicitly controls for any pre-existing differences between states with more and less generous exemptions, including any time-invariant effects of bankruptcy protections on the quantity and price of credit (Gropp, Scholz and White 1997; Mitman 2016; Severino and Brown 2020; Dávila 2020). We also

[2] We report the face value of both net charge-offs and the amount of unsecured credit discharged in Chapter 7. Total charge-offs are net of recoveries. We scale all program payouts by annual personal consumption expenditures (PCE) for comparability over time.

explicitly control for any common time effects of the financial crisis, including any common general equilibrium effects such as changes in monetary policy (Nakamura and Steinsson 2014), aggregate labor supply (Beraja, Hurst and Ospina 2019), or aggregate total factor productivity. The identifying assumption for our difference-in-differences specification is that, in the absence of any variation in bankruptcy exemptions, outcomes in states with more and less generous exemptions would have evolved in parallel during the Great Recession. This "parallel trends" assumption implies that there are no other reasons why the Great Recession would impact high and low exemption states differently, an assumption that we provide extensive support for in our analysis.

Using a rich panel of state-level consumer credit and employment data, we find that states with more and less generous bankruptcy exemptions had statistically indistinguishable outcomes in the years leading up to 2008. Starting in 2008, however, the states with more generous bankruptcy exemptions had significantly larger increases in consumer debt write-downs and smaller declines in local non-tradable employment compared to the states with less generous exemptions. For example, we find that increasing the generosity of bankruptcy protections from the bottom to top tercile leads to a $70 increase in annual debt charge-offs per person and a 1.4 percentage point increase in non-tradable employment from 2008 to 2010. The employment results are also extremely persistent, remaining economically and statistically significant through at least 2015. In contrast, we find a small and statistically insignificant negative effect on local tradable employment over all time periods. In supplementary results, we find that states with more generous bankruptcy exemptions also experienced higher non-tradable employment levels in previous recessions.

In the second part of the paper, we interpret these empirical results through the lens of a structural model of the regional and aggregate effects of ex-post debt relief. We first write down an equation for outcomes in a two-region setting in which debt relief in one region can affect employment in both regions. Using this equation, we show that our difference-in-difference design identifies the *relative debt relief multiplier*, which differs from the *aggregate debt relief multiplier* because of spillover effects in the control region. Using our empirical results, we can reject a non-tradable multiplier below one with 95% confidence, and find tradable multipliers centered around zero. These findings are consistent with a model in which debt relief has a significant effect on spending, nominal rigidities translate effects on spending into effects on employment, and there is limited home bias in tradable spending.

We develop a New Keynesian model consistent with these features in order to recover the aggregate effect of debt relief during the Great Recession. Our model combines elements from the closed-economy literature with borrower-saver heterogeneity (e.g., Eggertsson and Krugman 2012) with elements from the open economy currency union literature (e.g., Nakamura and Steinsson 2014). Specifically, we develop a two-agent, two-region, two-good model, where borrowers and savers are spread across high- and low-protection regions and consume both tradable and non-tradable goods. We model ex-post debt relief as a transfer from savers to borrowers, so that it does not affect interest rates or credit supply ex-ante. We show that a baseline calibration of our model produces relative multipliers for both non-tradable and tradable employment that are consistent with our empirical estimates, with relative debt relief multipliers larger than one for non-tradable employment, and negative but close to zero for tradable employment.

The three main design features of our model are important to this success: (1) heterogeneity in marginal propensities to consume between borrowers and savers, (2) nominal rigidities, and (3) lack of home bias in tradable spending. Because borrowers have higher marginal propensities to consume than savers, debt relief temporarily increases spending on the goods consumed by the high-protection region. Because of nominal rigidities, this temporary increase in aggregate spending leads to an increase in both non-tradable and tradable employment in the high-protection region. Because of the lack of home bias in tradable spending, tradable employment rises in the low-protection region by a similar amount as in the high-protection region. This delivers a relative multiplier that is larger than one for non-tradable employment, and approximately zero for tradable employment. Underlying these are aggregate multipliers that are around 2 for both tradable and non-tradable employment.

We explore extensively the robustness of these results to our calibration choices. Nominal rigidities are important: when prices are flexible, the higher spending in the high-protection region translates into an appreciation of the terms of trade and a reallocation of tradable demand to the low-protection region, pushing the non-tradable employment multiplier towards 0 and the tradable employment multiplier much below 0. This is inconsistent with our empirical estimates. By contrast, conditional on the degree of price rigidity, our results are robust to a large range of alternative parameterizations, with little meaningful change in the model's relative or aggregate multipliers as we vary preferences, the extent of real rigidity, the size of the non-traded goods sector, or the specification of monetary policy.

We use our model to simulate a counterfactual in which low protection states receive the same amount of debt relief as high protection states during the Great Recession. Under the assumption that monetary policy was at the zero lower bound throughout the period, we find that, in 2010, aggregate employment would have been 0.6 percent higher in low protection regions and 0.5 percent higher in high protection regions. In other words, this counterfactual mitigates the overall 7.3 percent decline in the employment-to-population ratio relative to 2007 by around 7.5%. The key to these results is that high protections regions experienced around 0.6% more debt relief as a fraction of total consumption, and the relevant employment multipliers are around 1 in both regions.

We conclude by discussing the limitations of our analysis. We show why the model suggests that high multipliers are only applicable for debt relief of moderate size, we discuss why a model in which those who finance debt relief are highly constrained would generate lower aggregate employment effects, and we speculate on the types of mechanisms that can rationalize the very persistent employment responses we see in the data.

Our results are related to a literature showing how deteriorating household balance sheets can amplify an economic downturn (e.g. Hall 2011; Eggertsson and Krugman 2012; Farhi and Werning 2016*b*; Korinek and Simsek 2016; Guerrieri and Lorenzoni 2017; Jones, Midrigan and Philippon 2018). Mian, Rao and Sufi (2013) and Mian and Sufi (2014*b*) show an empirical link between the fall in house prices, household consumption and non-tradable and tradable employment in the United States during the Great Recession. Verner and Gyöngyösi (2020) show that an increase in debt burdens led to higher default rates and a collapse in spending in Hungary, translating into a worse local recession and depressed house prices. We contribute to this literature by showing that the ex-post debt forgiveness provided by the consumer bankruptcy system helped mitigate the deterioration of household balance sheets and stabilize employment levels during the U.S. Great Recession.[3]

Our results also relate to an emerging literature studying the macroeconomic effects of bankruptcy exemptions. Mitman (2016) studies the long-run effects of exemptions on interest rates and levels of unsecured and secured debt. He finds that households in higher exemption states face higher interest rates and have less unsecured borrowing on average. We abstract away from these ex-ante effects to focus on the response of households to adverse shocks for a given level of debt. In a structural model with both ex-ante and ex-post effects, Auclert and Mitman (2021) show that consumer bankruptcy acts as an automatic stabilizer of the business cycle. On the normative side of this literature, Dávila (2020) solves for optimal bankruptcy exemptions.

Methodologically, our paper builds on the local fiscal multipliers literature. This literature has

[3] Hausman, Rhode and Wieland (2019) show that a related phenomenon took place during the dollar devaluation of the early 1930s. The increase in the prices of farm products lead to a substantial economic increase in auto sales, especially in areas with a high farm debt burden.

convincingly shown that, while cross-state comparisons allow one to use plausibly exogenous variation to analyze the effects of shocks or policies on regional economies, a general equilibrium model is needed to analyze the aggregate effect of these shocks or these policies (e.g., Nakamura and Steinsson 2014; Farhi and Werning 2016*a*; Suárez Serrato and Wingender 2016; Chodorow-Reich 2019; Martin and Philippon 2017; House, Proebsting and Tesar 2020; Dupor et al. 2021; Corbi, Papaioannou and Surico 2018). We follow this literature by calculating a relative multiplier from cross-sectional variation, and then using a structural model to discuss the relationship to the aggregate multiplier. We build on the literature by applying these concepts to the case of debt relief, by showing formally why the relative and aggregate effects differ by the magnitude of the spillover effect in control regions, and by highlighting how separately modeling tradable and non-tradable employment enriches the mapping from the cross-section to the aggregate.

The remainder of the paper is structured as follows. Section I provides a brief overview of the consumer bankruptcy system and describes our data and empirical strategy. Section II presents our difference-in-differences estimates of the effect of bankruptcy protections on employment and credit outcomes during the financial crisis. Section III establishes the relationship between relative and aggregate multipliers and calculates relative debt relief multipliers in our context. Section IV develops our general equilibrium model and performs our main counterfactual. Section V concludes.

I. Background, Data, and Empirical Design

This section summarizes the most relevant aspects of our institutional setting and data, describes our empirical strategy, and provides support for the baseline assumptions required for our difference-in-differences estimator.

A. Background

Under the U.S. bankruptcy code, eligible households can discharge most unsecured debts by filing for Chapter 7 bankruptcy, after relinquishing assets above state-specific exemption limits. Households can choose between Chapter 7 bankruptcy, which requires them to forfeit all assets above their state's exemption limits in exchange for a discharge of debt and protection from collection efforts, and Chapter 13 bankruptcy, which allows them to keep most of their assets in exchange for the partial repayment of their debts. Since Chapter 13 filers are required to repay at least as much as creditors would have received under Chapter 7, the state-specific Chapter 7 exemption limits directly impact the generosity of Chapter 13 in each state. Chapter 7 is also chosen by approximately 75 percent of bankruptcy filers in the United States, as most filers have relatively low levels of non-exempt wealth. For these reasons, we focus on the Chapter 7 bankruptcy exemption limits for the remainder of the paper.[4]

Bankruptcy exemptions can also influence settlements with creditors either just before or after borrowers become delinquent on a debt, but before they formally file for bankruptcy protection. The bankruptcy system provides a natural threat point in these settlements, as households can always threaten to file for Chapter 7 protection as an outside option. Consistent with this idea, Mahoney (2015) shows that uninsured households that have more to lose from declaring bankruptcy make higher out-of-pocket medical payments, conditional on the amount of care received.

The formal and informal debt relief provided by the consumer bankruptcy system during economic downturns is economically large, both in absolute terms and compared to other social insurance programs. The average Chapter 7 bankruptcy filer between 2008 and 2010 discharged more than $94,000 in unsecured debt, with more than 2.8 million households filing for Chapter 7 over this time period (U.S. Courts 2017, see also Figure 1). In other words, Chapter 7 helped bankruptcy filers formally discharge more than $263 billion in unsecured debt during the Great Recession. The informal debt relief provided by the bankruptcy system, as well as the secured debt discharges through Chapter 7 and both the unsecured and secured debt discharges through Chapter 13, only adds to this total. By comparison, UI payouts totaled $319.1 billion over this time period (Whittaker and Isaacs 2016).

Our empirical strategy exploits the considerable variation in the generosity of the bankruptcy exemptions across states. Appendix Table A1 shows Chapter 7 exemptions by state and category in 2007, just prior to the onset of the financial crisis. The variation is particularly large for the home equity exemptions, the largest and most important exemption category. Seven states allow households to protect an unlimited amount of home equity in bankruptcy, for example, while three states do not allow households to protect any home equity. There is also variation in the other exemption categories, albeit at much lower levels, with vehicle exemptions ranging from $0 in 12 states to at least $10,000 in three states, savings exemptions ranging from $0 in 39 states to at least $1,000 in seven states, and wildcard exemptions, which can be applied to any asset, ranging from $0 in 20 states to at least $10,000 in three states.

[4] The 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) changed several relevant features of the U.S. bankruptcy code. First, BAPCPA increased filing fees and imposed mandatory credit counseling for both chapters. Second, BAPCPA reduced the generosity of the bankruptcy code by restricting Chapter 7 to households that passed an income or repayment test, making it more difficult for high-income households to shield their home equity through the bankruptcy system. These changes should not influence our results, however, as they were applied uniformly across states.

The variation in homestead exemptions has also been remarkably stable over time, with the initial variation in homestead exemption amounts largely emerging as states formalized their state-level approaches to bankruptcy in the late nineteenth century, in ways that are likely unrelated to current state characteristics (Goodman 1993; Skeel 2001). The population-weighted correlation between the 2007 and 1991 homestead exemption ranks, for example, is 0.88, while the correlation between the 2007 and 1920 homestead exemption ranks is 0.60 among the 38 states with exemptions in both time periods (see Appendix Figure A1). Consistent with this idea, we show below that both the homestead exemptions and bankruptcy protections more generally have remarkably little correlation with a number of key state-level characteristics in the years just prior to the financial crisis.

B. Data and Summary Statistics

Our empirical analysis uses information from state-level employment and inflation data, and individual-level credit data aggregated to the state level.

State Employment Data. For our main sample, we measure employment outcomes between 2001 and 2015 using state-level data from the Quarterly Census of Employment and Wages (QCEW). The data are derived from individual-level UI tax files submitted by each state to the Bureau of Labor Statistics (BLS). The BLS uses these individual-level files to calculate quarterly employment by state and industry.[5] In our extended analysis, we broaden the sample back to 1991 (the earliest date that NAICS codes are available) and extend forward to 2017 (when the NAICS codes most recently changed again).

There are two important challenges in constructing consistent employment measures over our sample period using the QCEW data. The first is that the industry codes in the QCEW data change from NAICS 2002 to NAICS 2007, and then to NAICS 2012 over this time period. We address this issue by using the publicly-available NAICS code crosswalks and the Mian and Sufi (2014*b*) industry definitions to create a consistent time series of employment in each state and each of their main industry groups.[6]

The second challenge is that the QCEW employment data are suppressed in certain state-industry-year-quarter bins for confidentiality reasons. Following Mian and Sufi (2014*b*), we address this issue

[5] See `www.bls.gov` for additional information on the underlying data.

[6] We construct employment measures in tradable, non-tradable, and other industries in two steps. We first follow Mian and Sufi by assigning each 3-digit 2007 NAICS code to one of non-tradable, tradable, and other. We then use the publicly-available crosswalks to map 2002 and 2012 NAICS into one or several 2007 NAICS and find the corresponding industry, manually resolving any ambiguity.

by imputing employment in suppressed state-industry-year-quarter bins using information from the County Business Patterns (CBP) data.[7]

We use the cleaned and harmonized QCEW employment data to construct a panel dataset for non-tradable employment, tradable and other employment, and total employment. We follow the industry groupings defined in Mian and Sufi (2014*b*), where non-tradable employment is defined as the retail and restaurant sectors, tradable employment is defined as industries with sufficiently large imports and exports from the rest of the United States, and other employment is defined as all other industries excluding the construction sector. In our analysis, we combine tradable and other employment into a single category, which we refer to as "tradable employment" for simplicity, omitting construction. We normalize all employment measures by the number of working-age adults in each state to account for any secular population trends over the years we examine. Information on population totals comes from the U.S. Census.

Credit Bureau Data. We measure credit outcomes between 2001 and 2015 using records from the Equifax / New York Fed Consumer Credit Panel (CCP), a representative five percent random sample of all individuals in the U.S. with credit files.[8] Like other credit report data, the CCP data are derived from public records, collection agencies, and trade lines data from lending institutions. The data include a comprehensive set of consumer credit outcomes, including information on credit scores, unsecured credit lines, auto loans, and mortgages. The data also include year of birth and geographic location at the ZIP-code level. No other demographic information is available at the individual level. We aggregate these data to the state level unless otherwise mentioned. See Avery et al. (2003) and Lee and Van der Klaauw (2010) for additional details on the underlying credit report data.

House Price Data. State-level house price indices between 2001 and 2015 come from CoreLogic. We use quarterly data over the sample period. We also use zip code-level house price estimates from Zillow.com when calculating home equity values.

Inflation Data. Inflation data comes from Hazell et al. (2021), who construct a state-year panel for non-tradable, tradable, and overall annual inflation. The non-tradable goods definition in Hazell et al.

[7]Payroll data is also available from the QCEW and CBP. However, there is no way to impute suppressed cells in the payroll data, and, as a result, there is substantial measurement error in these data that varies by employment density.

[8]The credit bureau data do not include the approximately 22 million adults (nine percent of adults) in the United States without credit files, or information on non-traditional forms of credit such as payday lending, pawnshops, and borrowing from relatives. The data are therefore likely to be less representative of the behaviors and outcomes of very poor populations.

(2021) roughly follows the Bureau of Labor Statistic's definition for services (see Appendix B of Hazell et al. 2021 for details), and tradable goods are defined as all other goods. We construct a price index at the state-year-quarter level for each measure, normalizing each state's index to 100 in 2007q4. These measures are only available for 33 states.

Summary Statistics. Table 1 provides descriptive statistics for key outcomes before, during, and after the financial crisis. We report the change in employment, credit outcomes, consumer price and house price indices for 2001q1 to 2007q4, 2007q4 to 2010q4, and 2010q4 to 2015q4. When reporting percent changes, we annualize to aid with comparison across time periods.

Total employment, scaled by working-age population, fell sharply during the crisis, dropping by an annualized 1.85 percent from 2007q4 to 2010q4, before recovering by 1.72 percent from 2010q4 to 2015q4. Non-tradable and tradable employment both followed similar trends as overall employment in the crisis and afterwards, but had dissimilar trends before the crisis, with tradable employment falling by 0.46 percent from 2001q1 to 2007q4, and non-tradable employment growing by 0.76 percent.

As with employment, annual debt charge-offs rates responded sharply from 2007q4 to 2010q4, increasing by \$126.09, and then declined by \$283.30 from 2010q4 to 2015q4, reflecting both the tighter credit markets and improved economic conditions during this time period.[9]

In contrast to the economically large changes in employment and credit outcomes, consumer prices were relatively stable over this time period. Prices on all consumer goods, for example, increased by 2.38 percent per year from 2001q1 to 2007q4, 2.19 percent per year from 2007q4 to 2010q4, and 1.68 percent per year from 2010q4 to 2015q4. Non-tradable and tradable prices follow a similar trend, although tradable prices increased less from 2001q1 to 2007q4 compared to non-tradable prices. House prices, however, increased by 5.40 percent per year from 2001q1 to 2007q4, before falling by 7.14 percent per year from 2007q4 to 2010q4. House prices then partially recovered after the crisis, increasing by 2.55 percent per year from 2010q4 to 2015q4.

C. Empirical Design

We estimate the relative impact of ex-post debt forgiveness across states using a difference-in-differences design that compares outcomes in states with more generous bankruptcy exemptions (the "treated" states) to states with less generous exemptions (the "control" states). Our difference-in-differences

[9] There is significant noisiness in the charge-off measure from period to period around 2007, and so for our summary statistics, we report the changes in a four quarter rolling average. In the regression analysis, we focus on the unsmoothed measure, but results are qualitatively similar.

design explicitly controls for any pre-existing differences between states with more and less generous exemptions, such as any time-invariant effects of bankruptcy protections on credit demand and credit supply (Gropp, Scholz and White 1997; Mitman 2016; Severino and Brown 2020; Dávila 2020). We also explicitly control for any common time effects of the financial crisis, such as the large negative aggregate demand shock for both tradable and non-tradable goods. The key identifying assumption for our difference-in-differences specification is that, in the absence of any variation in bankruptcy exemptions, outcomes in states with more and less generous exemptions would have evolved in parallel during the Great Recession. This "parallel trends" assumption allows us to interpret the difference between states with more and less generous bankruptcy exemptions during the financial crisis as the causal effect of more generous bankruptcy exemptions in the presence of a large aggregate shock, an assumption we provide extensive support for below.

Bankruptcy Protection Measurement. We measure the generosity of bankruptcy protections in each state s using pre-crisis bankruptcy asset exemptions in that state. Since we use pre-crisis data, our bankruptcy protection measure removes any endogenous changes to the exemptions made during the financial crisis. For transparency and completeness, we present our estimates using three different bankruptcy protection measures.

Our first measure of bankruptcy protection focuses on the homestead exemption since the vast majority of the variation in the bankruptcy protections across states comes from these exemptions (see Appendix A for more details). We split the states in our sample into terciles based on their homestead exemption amounts, and compare bins of the states with the highest and lowest bankruptcy homestead exemptions. This "binned" bankruptcy protection measure keeps the comparison and policy experiment extremely simple, allowing us to transparently present means for each group and construct an easily interpretable counterfactual in Section IV.

Our second measure of bankruptcy protection uses a continuous measure, log(bankruptcy homestead exemptions$_s$ + 2000), to capture the full range of the bankruptcy homestead exemptions. This second measure still keeps the comparison and policy experiment relatively simple, but captures more of the underlying variation in homestead exemptions across states. The downside of both of our first two measures is that the homestead exemptions do not inherently map to the underlying costs facing bankruptcy filers, e.g., an increase of bankruptcy homestead exemptions from $800,000 to $1,000,000 is unlikely to impact most filers, who have home equity well below $800,000. A second issue is that are several other bankruptcy exemptions that are not incorporated in our homestead-based measures, potentially introducing noise to our estimates.

Our third measure of bankruptcy protection addresses these issues by parameterizing the full range of bankruptcy protections within state s using the full set of pre-crisis bankruptcy asset exemptions in state s and the balance sheets of individuals in all other states, $-s$. By using balance sheets of individuals in all other states, this measure isolates the generosity of each state's bankruptcy exemptions, purged of any endogenous variation due to the characteristics of states' actual residents.[10]

Empirical Specification and Identifying Assumptions. For a given outcome, Y, our difference-in-differences regression specification takes the form:

$$Y_{st} = \alpha_s + \lambda_t + \left[\sum_{u \neq 2007} \beta_u \times I_u \times \hat{B}_s \right] + \left[\sum_{u \neq 2007} \gamma_u \times I_u \times X_s \right] + \epsilon_{it} \tag{1}$$

where α_s are state fixed effects, λ_t are year-quarter fixed effects, $\hat{B}_s$ is one of our three measures of bankruptcy protections for state s, and X_s are potential time-invariant characteristics of state s such as the housing supply elasticity measure from Saiz (2010). The estimated effects of $\hat{B}_s$ and X_s are allowed to be time-varying, with β_u denoting the year-by-year impact of the bankruptcy protections each year, and γ_u denoting the time-varying impact of the other controls. We use the summation notation to make explicit that separate coefficients are estimated for each calendar year.

In this specification, the β_u coefficients can be interpreted as the impact of the bankruptcy protections in each year, relative to our baseline year of 2007 (since we omit β_{2007}) and to the effective control group. Our estimates of β_u from 2008 to 2010 will therefore capture the relative short-run impact of the bankruptcy protections during the Great Recession, while the estimates of β_u from 2008 to 2015 will capture the relative overall impact of the protections during the recovery. As discussed in greater detail in Section III, our estimates do not measure the aggregate impact of the bankruptcy protections during the crisis and recovery periods, because they difference out general equilibrium effects.

[10]Our bankruptcy protection measure is closely related to the "simulated instrument" measure developed by Currie and Gruber (1996) to isolate exogenous variation in Medicaid eligibility across states, and adapted by Mahoney (2015) to isolate exogenous variation in the generosity of bankruptcy exemptions across states. Our measure is also closely related to the financial cost of bankruptcy measure developed by Fay, Hurst and White (2002). Our measure differs from the bankruptcy protection measures used in Fay, Hurst and White (2002) and Mahoney (2015) in two ways. First, we are able to directly measure balance sheet values for some assets, rather than relying on assets and liabilities from surveys as in Fay, Hurst and White (2002) and Mahoney (2015). Second, Mahoney (2015) also exploits within-state variation based on average differences in asset holdings across different age, race, and education cells. We ignore this within-state heterogeneity, both because we are interested in the aggregate effect of the bankruptcy laws, and because we are unable to measure across-group heterogeneity in asset holdings using our data.

The identifying assumption for our difference-in-differences specification is that, in the absence of the Great Recession, the outcomes in states with more and less generous protections would have evolved in parallel. This parallel trends assumption implies that there are no other reasons why the Great Recession would impact more and less generous states differently. However, if bankruptcy protections play an important role during recessions, it is plausible that generous protections would have had differential effects in previous recessions as well. We formally examine this hypothesis in our non-parametric analysis of Section II.C, using historical data going back to 1991.

To better understand what our identification assumption entails, Table 2 examines the state-level correlation between our bankruptcy protection measure and other variables that may have caused differential employment responses during the Great Recession. The table reports estimates from OLS regressions of the variable listed in the row on the bankruptcy protection measure listed in the column, weighted by state population, with robust standard errors. Column 2 reports these results using the binned homestead measure for bankruptcy protections, comparing states in the top tercile of our measure to states in the bottom tercile. Column 3 reports results for all states using the log(Homestead) measure. Column 4 uses the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Both the log(Homestead) and simulated bankruptcy measure are normalized to have zero mean and unit standard deviation. All three columns report results for outcomes measured in 2007, with results for outcomes measured in 2001 available in Appendix Table A2. Panel A reports results for baseline employment outcomes, Panel B for baseline credit outcomes, Panel C for time-varying state characteristics, and Panel D for time-invariant state characteristics. Statistically or economically significant estimates from these regressions do not necessarily imply a violation of our identifying assumption, as the state fixed effects α_i in our main specification will control for any time-invariant differences across geographic areas. What is critical is that any characteristics that differ across more and less generous states do not have a confounding time-varying effect on states' outcomes during the Great Recession. For example, our identifying assumption would be violated if states with more generous bankruptcy protections also had more generous UI benefits, and those more generous UI benefits had an independent impact on employment during the financial crisis.

Table 2 reveals no statistically or economically significant differences in the level of employment in 2007 between areas with more and less generous bankruptcy protections using the log(Homestead) and simulated bankruptcy protection measures, although there is a statistically higher level of non-tradable employment in 2007 for states in the top vs. bottom tercile of our binned bankruptcy measure.

There are also no statistically significant differences in credit outcomes between areas with more and less generous protections, with the exception of 2007 bankruptcy filings, which are about 0.04 percentage points lower in areas with a one standard deviation more generous collection of bankruptcy protections, consistent with the idea that these more generous protections raise the average cost of borrowing and induce less unsecured borrowing (Mitman 2016).

We also find few statistically significant differences in the state-level characteristics in more and less generous areas. There are, for example, no differences in Democratic vote shares or the generosity of UI between more and less generous areas, characteristics that partially capture states that are more likely to have responded to the Great Recession through automatic stabilizers or debt forgiveness (e.g., Di Maggio and Kermani 2016; Hsu, Matsa and Melzer 2018). We similarly find no income or demographic differences between more and less generous areas, as well as no differences in the sensitivity of a states' employment with the national employment cycle, the share of individuals who are measured as "hand-to-mouth" measured using the approach from Kaplan, Violante and Weidner (2014), the housing supply elasticities discussed in Saiz (2010), the local house price sensitivity to regional house price cycles discussed in Guren et al. (2021), or the non-recourse mortgage laws discussed in Ghent and Kudlyak (2011). However, we do observe a positive correlation between our bankruptcy measures and predicted exposure to local employment shocks during the Great Recession, measured using a shift-share approach at the three-digit NAICS industry level (e.g., Yagan 2019). We also find that areas with higher bankruptcy exemptions have lower homeownership rates for our simulated bankruptcy protection measure, but not our binned or log(Homestead) measures.

Based on these correlations, we can identify two potential confounding factors that may have led to differential employment responses during the Great Recession. First, areas with more generous bankruptcy protections had fewer homeowners, exactly the types of individuals who suffered the largest wealth shock during the crisis. Second, areas with more generous bankruptcy protections were potentially less exposed to local unemployment shocks during the financial crisis, at least as measured by our shift-share variable.

We partially test our identifying assumptions to assess whether the above factors (or others) are potential sources of bias. We do so in two complementary ways. First, we examine whether employment and credit outcomes evolve in parallel in states with more and less generous bankruptcy protections before the financial crisis. As discussed below, our non-parametric specifications show that outcomes for states with more and less generous bankruptcy protections move in close parallel during the pre-crisis period. These results give us confidence that our "control group" of less generous states is valid and provides us with an accurate counterfactual for what would have happened to the "treatment group" of more generous states, despite the differences documented above. In addition, we can identify examine the effect of bankruptcy protections in the 1990 and 2001 recessions as dated by the NBER.

Second, we examine whether our coefficient estimates change when we add controls for potential confounding variables interacted with time fixed effects. The added control variables interacted with time effects estimate the direct impact of these alternative economic channels during the Great Recession. If our point estimates remain consistent across specifications, then we are more confident that these potential confounding variables are not driving our main estimates. We add controls for the average supply elasticity of housing, the share of the pre-crisis share of homeowners, and the pre-crisis state-level debt-to-income interacted with time fixed effects. These set of controls tests for the potential confounding effect of the boom-bust cycle, as well as the differences in homeownership rates found in Table 2. All of these results again give us confidence that our "control group" of less generous states is valid and provides us with an accurate counterfactual for what would have happened to the "treatment group" of more generous states.

II. Results

In this section, we examine the relative effects of consumer bankruptcy protections across states using our difference-in-differences research design. We first analyze the relative effects of bankruptcy protections on employment and debt write-downs. We then present robustness checks and discuss the relative effects of bankruptcy protections on other credit outcomes.

A. Descriptive Trends

Figure 2 plots the mean outcomes of states in the top and bottom tercile of bankruptcy homestead protections, providing a transparent presentation of the effect of bankruptcy exemptions. Panel A presents the mean level of log non-tradable employment, Panel B the mean level of log tradable employment, Panel C the mean level of log total employment, and Panel D the mean level of charge-offs. We measure debt write-downs using the charge-offs that occur when seriously derogatory debts are removed from individuals' credit reports. Charge-offs do not include medical debt held by hospitals (e.g., Mahoney 2015), mortgage debt on second homes with negative equity (e.g., Ganong and Noel 2020), and payday loans (e.g., Dobbie and Skiba 2013), meaning that we will understate the impact of bankruptcy protections to the extent that these debts are also written off due to the more generous protections. We return to this issue below when calculating the relative debt relief multiplier. Finally, we normalize the employment outcomes for both high and low protection areas to 0 in 2007, so that comparisons can be made in percentage points changes relative to 2007, and the charge-off levels to the low protection average in 2007 for both groups.

We find that states with more generous bankruptcy protections had significantly smaller declines in local non-tradable employment (Panel A) and larger increases in charge-offs (Panel D) during the financial crisis compared to states with less generous protections, consistent with the debt forgiveness provided by the U.S. consumer bankruptcy system stabilizing employment levels during the Great Recession. In contrast, we see relatively small differences between high and low protection states for tradable employment (Panel B) and overall employment (Panel C) in the recession period, as well as for all outcomes in the pre-recession period.

B. Reduced Form Effects of Bankruptcy Protections

Table 3 reports formal estimates of the impact of bankruptcy protections on employment from our parametric difference-in-differences specification that pools the effect of increasing the generosity of bankruptcy protections for 2008q1-2010q4 and 2008q1-2015q4 in Panels A and B, respectively. Column 1 reports the cross-state average and standard deviation of the change in the dependent variable. Columns 2-7 report the coefficients of the reduced form effect of bankruptcy protections in pairs of columns using the different measures of bankruptcy protection discussed in Section I.C. First, in columns 2 and 3, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy homestead exemptions. Second, in columns 4 and 5, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$ + 2000). Third, in columns 6 and 7, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. We focus on the estimates using the simulated IV measure in our discussion below, and highlight cases where they disagree. The first column for each bankruptcy protection measure includes state and year-quarter fixed effects, and the second column adds additional controls interacted with year-quarter fixed effects. All regressions are weighted by state population as of 2007 and cluster the standard errors at the state level to account for within-state correlation between the treatment variable and unobservable shocks (Bertrand, Duflo and Mullainathan 2004; Abadie et al. 2017). All regressions also use data from the 2001q1-2015q4 period to mimic identical samples between the employment and credit data.

We find that states with more generous bankruptcy protections had significantly smaller declines in local non-tradable employment during the short-run crisis period compared to states with less generous protections, as expected given the patterns observed in Panel A of Figure 2. Controlling for only state and year-quarter fixed effects in column 6, for example, we estimate that a one standard deviation increase in our summary bankruptcy protection measure increases non-tradable employment by 0.56 percentage points, or 10.2% of the average decline in non-tradable employment over this period. This estimate increases to 0.67 percentage points (12.3% of the average decline) after adding controls. This suggests that a more generous bankruptcy protections had an economically and statistically significant impact on non-tradable employment during the Great Recession, consistent with the consumer bankruptcy system acting as an automatic stabilizer.

In contrast, we find an economically small and negative effect of bankruptcy protections on tradable employment during the short-run crisis period. In our specification that includes time-invariant controls interacted with year-quarter fixed effects, for example, we find that a one standard deviation increase in the simulated bankruptcy protection measure *decreases* tradable employment by 0.15 percentage points during the 2008q1-2010q4 period, or 2 percent of the overall decline in tradable employment during this period (column 5), and is statistically indistriguishable from zero. We find similarly small effects when comparing states in the top and bottom tercile of our bankruptcy homestead exemptions measure or when using our log(Homestead) measure, with none of the estimates suggesting economically significant effects of the bankruptcy protections on tradable employment. We also find an economically and statistically insignificant effect on total employment during the short-run crisis period, consistent with the fact that non-tradable employment makes up only about 23 percent of overall employment.

We also find that states with more generous bankruptcy protections had significantly larger increases in debt write-downs during the short-run crisis period compared to states with less generous bankruptcy protections. In our specification that includes time-invariant housing controls interacted with year-quarter fixed effects, for example, we estimate that a one standard deviation increase in our simulated bankruptcy measure increases annual debt write-downs by $29.66, or 71% of the average change in debt write-downs during this period (column 7). An important feature of these charge-off results is that while the point estimates are consistently similar between our specifications with and without additional time-invariant controls interacted with time fixed effects, the standard errors are significantly smaller once we add the controls. We will emphasize this issue again in our graphical results below. Nonetheless, all of the results suggest that the more generous bankruptcy protections had an economically and statistically significant impact on debt write-downs during the Great Recession.

In Panel B of Table 3, we find large persistent effects in non-tradable employment and debt write-downs and negligible effects in tradable employment when we estimate effects over the full time period from 2008q1-2015q4 that incorporates both the short-run crisis period (2008q1-2010q4) and the subsequent recovery period. These persistent effects, particularly for non-tradable employment, are difficult to justify in standard models of aggregate demand, which imply that the effect of transitory demand shocks should die out over time. We return to this issue in Section IV.H.[11]

C. Graphical Results of Time-Varying Effects

Figure 3 plots the estimated effect of the generous bankruptcy protections from the non-parametric difference-in-differences shown in Equation (1). These non-parametric estimates allow us to observe the dynamics of the bankruptcy protections effects during the Great Recession and the subsequent recovery, as well as assess the pre-trends assumption during the period prior to the Great Recession. We can also examine whether the more generous bankruptcy protections had differential effects in previous recessions, at least for employment outcomes that are available back to 1991. For ease of presentation, we focus on the estimates using the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. We include state and year-quarter fixed effects and time-invariant housing controls interacted with year-quarter fixed effects throughout, and report both pointwise (bar) and uniform (spike) 95% confidence intervals for each point estimate using standard errors clustered at the state level.[12] Non-parametric estimates using the log(Homestead) and binned Homestead protection measures are available in Appendix Figures A5 and A6.

The effects on non-tradable employment are persistent (and growing) from the 2007 to 2015 period, but begin to decline afterwards. The results from Panel A of Figure 2 suggest that the growth in the gap reflects a faster recovery (and smaller initial decline). In contrast, Panel B suggests little impact of the bankruptcy protections on tradable employment during the recovery period until 2015, when relative tradable employment begins to decline in high protection states relative to low protection

[11]The long-lasting effects of the bankruptcy protections on non-tradable employment throughout the recovery period are broadly consistent Verner and Gyöngyösi (2020), who find persistent effects of foreign currency debt exposure on employment in Hungary.

[12]We follow the recommendation from Freyaldenhoven, Hansen and Shapiro (2019) and report uniform confidence intervals for difference-in-difference when considering pre-trends, using the results from Montiel Olea and Plagborg-Møller (2019). We are grateful to Ryan Kessler for posting code to construct these intervals here: `https://github.com/ryanedmundkessler/simultaneous_confidence_bands`.

states. We also find an economically and statistically insignificant effect on total employment during this time period, again because of the small share of non-tradable employment. In Panel D, we see the same pattern in effects for charge-offs, but with a faster reduction back to zero than non-tradable employment.

We see statistically indistinguishable effects from 2003 to 2007 in Figure 3 for all four outcomes, providing strong evidence that our parallel pre-trends assumption is appropriate. Finally, we also see suggestive evidence that bankruptcy protections played a potential role in the previous recessions of 2001 and 1991. First, in the 2001 recession, we see a small temporary increase in non-tradable employment due to more generous protections, followed by a small (but statistically indistinguishable from zero) effect on charge-offs, consistent with the lagged behavior of charge-offs in the 2008 recession. Second, going back to 1991, we see a large effect from more generous bankruptcy protections on non-tradable employment that declines to zero in 1994, consistent with the behavior in the recovery at the end of the recovery after the Great Recession.

D. Additional Credit Outcomes and Robustness Checks

Appendix Table A3 reports estimates of the impact of bankruptcy protections on other credit market outcomes in both the short-run and overall. There are no clear theoretical predictions for other credit outcomes, including bankruptcy filing rates, due to the offsetting effects of the greater availability of debt forgiveness and the improved local economic conditions documented above. In fact, we find little impact of the more generous bankruptcy protections on bankruptcy filing rates in either the crisis period or overall. There is also no discernible impact on credit card limits in either time period, with insignificant effects on credit card debt during both periods. We also find a modest impact of more generous bankruptcy protections on mortgage debt. Taken together, these results suggest there may have been a minor contraction in borrowing limits due to the increase in debt write-downs, but that any contraction was small compared to the nationwide fall in borrowing observed during the financial crisis.

The only credit outcome significantly impacted by the more generous bankruptcy protections during the financial crisis is foreclosure rates. Focusing on Column 7, a one standard deviation in our simulated bankruptcy protection measure increases foreclosures by 0.20 percentage points from 2008q1-2010q4 and 0.23 percentage points from 2008q1-2015q4 in our preferred specification that includes time-invariant housing controls interacted with year-quarter fixed effects. The increase in foreclosure rates may be due to strategic incentives to enter foreclosure on one's secondary home because of the benefit of preserving one's primary assets against creditors.[13]

Appendix Table A5 explores the robustness of our results to using state homestead exemptions in 1991, the earliest year for which information on exemption laws is available for all 50 states and well before either the financial crisis or the pre-crisis increase in house prices and employment. It is not possible to calculate the simulated instrument in this setting due to lack of balance sheet information, and so we report only the binned homestead and log(homestead) results. We find consistently positive results for non-tradable employment across all specifications, but the estimates are smaller in magnitude and statistically insignificant in some specifications, likely because the additional noise in our bankruptcy protection measure attenuates the estimates towards zero. We also find positive and statistically significant results for debt write-downs, although the estimates are again smaller than our main results. We interpret these results as supporting our conclusion that more generous bankruptcy protections increase non-tradable employment and debt write-downs, even when using noisier historical proxies of bankruptcy protection.

III. Recovering Aggregate Effects from Reduced-Form Estimates

Our difference-in-differences results indicate that states with more debt relief experienced significantly smaller declines in non-tradable employment compared to states with less debt relief during the Great Recession. In this section, we discuss what these difference-in-differences results can tell us about the macroeconomic effect of debt relief on aggregate employment. We argue that these results are consistent with a model where debt relief is a transfer from agents with low MPCs to agents with high MPCs, and nominal rigidities translate effects on spending into effects on employment.

A. Regional vs. Aggregate Debt Relief Multipliers

Consider a general equilibrium model with two symmetric regions Home H and Foreign F, and assume they can receive different debt relief, analogous to our empirical approach. The equation for employment in any sector in region $i \in \{H, F\}$ is:

[13] Appendix Table A4 explores the possibility that the increase in foreclosure rates is driven by a confounding variable, such as a differential change in house prices in more and less generous areas. We find that areas with more generous bankruptcy protections experienced a larger fall in house prices in the crisis period, but this fall is only statistically significant once we control for state-level policies and the shift-share shock (columns 3, 5, and 7). These declines in house prices in the presence of higher employment numbers are potentially a consequence of the foreclosure effects documented above, and are possibly due to the strategic incentive to enter foreclosure on one's second home. Given the results from Mian and Sufi (2014*b*), these effects should bias us *against* finding positive employment results.

$$\log N_{i,t} = \alpha_i + \lambda_t + \mathcal{M}_H \Delta_{i,t}/C + \mathcal{M}_F \Delta_{-i,t}/C \tag{2}$$

where α_i represents the time-invariant steady state level of the outcome, λ_t represents aggregate time trends, $\Delta_{i,t}$ is debt relief in region i at time t, and $\Delta_{-i,t}$ debt relief in the other region. The common shocks λ_t move employment across regions equally (the "parallel trends" assumption). Because the regions are interconnected, local debt relief has an effect on employment not just in the home region (via the home multiplier $\mathcal{M}_H$) but also in the foreign region (via the foreign multiplier $\mathcal{M}_F$).

Consider first a situation where both regions experience the same level of debt relief at all times, $\Delta_{i,t} = \Delta_{-i,t}$. Let $t = 0$ be a base period. Then, Equation (2) shows that:

$$\frac{(\log N_{i,t} - \log N_{i,0}) - (\lambda_t - \lambda_0)}{(\Delta_t - \Delta_0)/C} = \mathcal{M}_H + \mathcal{M}_F \equiv \mathcal{M}_{agg} \tag{3}$$

Equation (3) shows that the effect of aggregate debt relief between two time periods, over and above the effect of common shocks $\lambda_t - \lambda_0$, is captured by the *aggregate* multiplier is $\mathcal{M}_{agg} = \mathcal{M}_H + \mathcal{M}_F$. This is a natural causal object of interest from a macroeconomic perspective.

Common shocks are unobserved, so estimating $\mathcal{M}_{agg}$ directly in practice is difficult. However, consider the general case where regions experience different amounts of debt relief. Manipulating Equation (2) again, we find:

$$\frac{(\log N_{H,t} - \log N_{H,0}) - (\log N_{F,t} - \log N_{F,0})}{((\Delta_{H,t} - \Delta_{H,0}) - (\Delta_{F,t} - \Delta_{F,0}))/C} = \mathcal{M}_H - \mathcal{M}_F \equiv \mathcal{M}_{rel} \tag{4}$$

Equation (4) shows that the ratio of the difference-in-differences estimators for employment and debt relief recovers the *difference* in multipliers $\mathcal{M}_H - \mathcal{M}_F$, rather than the aggregate multiplier $\mathcal{M}_{agg} = \mathcal{M}_H + \mathcal{M}_F$. The reason is that in an integrated economy, debt relief $\Delta_{H,t}$ at home simultaneously "treats" all regions, but difference-in-differences estimates take out the effect $\mathcal{M}_F$ of debt relief in the "control" (foreign) region. Following Nakamura and Steinsson (2014), we call $\mathcal{M}_{rel}$ the *relative (employment) multiplier*.

These calculations show that the relative multiplier $\mathcal{M}_{rel}$ differs from the aggregate multiplier $\mathcal{M}_{agg}$ unless $\mathcal{M}_F = 0$. The sign and magnitude of $\mathcal{M}_F$ will vary from model to model depending on the relative strength of general equilibrium effects such as the response of relative prices, incomes, and credit supply. However, there is a presumption that $\mathcal{M}_F$ may be close to zero for non-tradable employment, since increased debt relief in one region does not directly affect demand for non-tradable goods in other regions. Estimating $\mathcal{M}_{rel}$ for non-tradable goods will therefore be informative about the magnitude of $\mathcal{M}_{agg}$. Similarly, estimating $\mathcal{M}_{rel}$ for tradable goods will be informative about the magnitude of the difference between $\mathcal{M}_H$ and $\mathcal{M}_F$ for tradable goods, which in turn will be informative about the extent to which markets for tradable goods are integrated across regions.

B. Multiplier Estimates and Implications for Modeling

We estimate $\mathcal{M}_{rel}$ from Equation (4) using two-stage least squares, with a scaled version of our debt write-downs measure to proxy for debt relief and the effect of state bankruptcy protections in the Great Recession as an instrument for debt write-downs.[14] Table 4 reports estimates for the 2008-2010 period. This approach maps the difference-in-difference estimates from Panel A of Table 3 to the formula in Equation (4) by exploiting just the effect of the bankruptcy protections during the Great Recession (relative to the pre-period) to identify variation in the debt write-downs.[15] Because the estimated effects on debt write-downs are noisily estimated in the regressions without controls, we account for weak instruments by reporting p-values for the Anderson-Rubin test statistic for each estimate, testing against the null hypothesis of the multiplier equal to zero (Andrews, Stock and Sun, 2019).

There is significant variation in the estimated relative debt relief multipliers in Table 4 across our specifications and measures of bankruptcy protections. We focus on our estimates in Column 2, 4, and 6 which include additional time-varying controls, as these are the only specifications with sufficient first-stage power to precisely estimate $\mathcal{M}_{rel}$. In all three cases, we estimate non-tradable relative multipliers that are large and significantly different from zero, and tradable multipliers centered at zero with large standard errors. For example, using the binned Homestead bankruptcy protection measure, we estimate a non-tradable multiplier of 4.73 with a 95% confidence interval from 1.12 to 8.33. In contrast, we estimate a tradable multiplier of 0.02, with a confidence interval from -5.62 to 5.66.

While these multipliers are not precisely estimated, the fact that we can reject a non-tradable multiplier below one with 95% confidence is suggestive of a model in which debt relief has a significant effect on spending because those who receive debt relief have much higher MPCs than those who implicitly finance it, and nominal rigidities translate effects on spending into effects on employment. Since for non-tradable employment, $\mathcal{M}_F$ is likely small, a *relative* multiplier above one also likely

[14] We scale debt write-downs by Personal Consumer Expenditure in 2007 and account for potential missing debt relief by scaling up our debt write-down measure. We discuss the exact approach and specification in Appendix XXX.

[15] For the model, we focus on short-term elasticities because we do not allow for medium-term adjustments and other frictions, but we report the elasticities for the full period in Appendix Table A8, which correspond to Panel B in Table 3.

implies an aggregate non-tradable multiplier above one. Generating such a large multiplier from a transfer such as debt relief requires significant differences in MPCs between givers and receivers. Moreover, the fact that the tradable multiplier is around zero across specifications suggests that $\mathcal{M}_H$ and $\mathcal{M}_F$ are close in magnitude, as in a model in which there is limited home bias in tradable spending. We turn to such a model in the next section. We use the model to study the sensitivity of all multipliers to parameterizations and to run counterfactual experiments.

IV. Model

We build a New Keynesian model of a currency union with incomplete markets, similar to those that have recently been used to study regional fiscal and transfer multipliers (see, for example, Nakamura and Steinsson 2014, Farhi and Werning 2016*a*, and Chodorow-Reich 2019). We add two dimensions to the standard representative-agent model with produced traded goods. First, we consider heterogeneous agents, with borrowers having higher marginal propensities to consume out of liquid wealth than savers. This feature allows ex-post debt relief to have real effects. Second, we assume that regions produce non-traded goods in addition to traded goods. This feature allows us to compare the model's sectoral employment outcomes to our empirical estimates.

A. Model Setup

We model a two-region, two-good, two-agent currency union. A home region H and a foreign region F each produce traded goods T and non-traded goods NT, and are each inhabited by a representative borrower B and a representative saver S. The two regions are symmetric with equal size $\frac{1}{2}$. In each region, borrowers make up the fraction φ^B of the population, and savers the faction $\varphi^S = 1 - \varphi^B$. Households $h \in \{B, S\}$ share the same utility function over consumption and hours worked (up to a factor χ^h that normalizes their total hours), but differ in their discount factor β^h, with $\beta^B < \beta^S$. They also face an exogenous credit constraint, which is binding for borrowers in steady state. Goods are produced under monopolistic competition. We index goods by type $\{T, NT\}$, production location $i \in \{H, F\}$, producer j, and time t, and drop indices when there is no ambiguity.

Households. All households within a region consume traded and non-traded goods, receive employment and dividend income, and can borrow and save in nominal risk-free bonds. Households maximize their intertemporal utility:

$$\mathbb{E}\left[\sum_{t=0}^{\infty}\left(\beta^{h}\right)^{t} u^{h}\left(C_{i,t}^{h}, N_{i,t}^{h}\right)\right]$$

where β^h is the discount factor of household type $h \in \{B, S\}$, $C^h_{i,t}$ is the aggregate consumption of household type h living in region i at time t, and $N^h_{i,t}$ is that household's labor supply. Given the sensitivity of regional multipliers to the specification of preferences between consumption and labor highlighted by Nakamura and Steinsson (2014), we adopt the "GHH-plus" functional form from Auclert, Bardóczy and Rognlie (2021), which nests most commonly-used preferences in the literature:

$$u^{h}(C, N)=\frac{1}{1-\sigma}\left(C-\chi^{h} \xi \frac{N^{1+\psi}}{1+\psi}\right)^{1-\sigma}-\chi^{h}(1-\xi) \frac{N^{1+\psi}}{1+\psi}$$

In our baseline specification, preferences are separable ($\xi = 0$), so that ψ is the inverse Frisch elasticity of labor supply and σ the inverse elasticity of intertemporal substitution in consumption. As ξ increases, income effects on labor supply weaken and the degree of complementarity between consumption and labor supply rises; $\xi = 1$ corresponds to the case of GHH preferences (Greenwood, Hercowitz and Huffman 1988). Consumption C is an aggregate of traded and non-traded goods:

$$C\left(C_{T}, C_{NT}\right)=\left[\alpha^{\frac{1}{\eta}}\left(C_{T}\right)^{\frac{\eta-1}{\eta}}+(1-\alpha)^{\frac{1}{\eta}}\left(C_{NT}\right)^{\frac{\eta-1}{\eta}}\right]^{\frac{\eta}{\eta-1}}$$

where α represents the expenditure share on traded goods at a unit relative price and η is the elasticity of substitution between tradable and non-tradable goods. In turn, traded and non-traded goods are made up of Kimball aggregates of intermediate goods:

$$\int G\left(\frac{C_{T,i,j}^{h}}{C_{T,i}^{h}}\right) dj=1 \qquad \int G\left(\frac{C_{NT,i,j}^{h}}{C_{NT,i}^{h}}\right) dj=1$$

where $C^h_{T,i,j}$ is household h's demand for the traded good produced in location i by firm j (similarly for $C^h_{NT,i,j}$), and $G(\cdot)$ is the Klenow and Willis (2016) aggregator, satisfying $G(1) = 1$ and $G'(x) = \exp\left\{\frac{1-x^{\Upsilon\left(\frac{\epsilon-1}{\epsilon}\right)}}{\Upsilon(\epsilon-1)}\right\}$, with ϵ denoting the elasticity of substitution between goods at unit relative price and Υ the elasticity of firm j's optimal markup to its relative price. When $\Upsilon = 0$, preferences are CES, the price elasticity of demand is the constant ϵ, and the optimal markup is the constant $\epsilon/(\epsilon - 1)$.

We assume that non-traded goods are all produced locally, but traded goods can be imported from the other region. Tradable goods $j \in \left[0, \frac{1}{2}\right]$ are produced in the home region, and goods $j \in \left(\frac{1}{2}, 1\right]$ are produced in the foreign region. Note that, since the home region also has size $\frac{1}{2}$, there is no home bias in tradable spending; overall home bias comes from non-traded goods.

There is free trade across the two regions, so the price of traded goods is the same in both locations. Letting P_T be the union-wide tradable price index and $P_{NT,i}$ the non-tradable price index in region i, the consumer price index in region i is given by:

$$P_i = \left[\alpha P_T^{1-\eta} + (1-\alpha)\left(P_{NT,i}\right)^{1-\eta}\right]^{\frac{1}{1-\eta}}$$

Workers are immobile, so the nominal hourly wage can differ in the two regions. We write W_i for the nominal wage in region i. The budget constraint of consumer h, in region i at time t is given by:

$$P_{i,t}C_{i,t}^h + B_{i,t-1}^h - \Delta_{i,t}^h = \frac{B_{i,t}^h}{1+I_t} + W_{i,t}N_{i,t}^h + D_{i,t}$$

where $C_{i,t}^h = C\left(C_{T,i,t}^h, C_{NT,i,t}^h\right)$ is aggregate consumption for consumer h in region i at time t, $B_{i,t}^h$ is her nominal debt level, I_t the nationwide nominal interest rate, and $D_{i,t}$ are total nominal profits of firms in region i. There are no taxes or transfers, except for those the government uses to impose the consumer debt write-downs $\Delta_{i,t}^h$, as detailed below. Consumers face a borrowing constraint, such that their real debt in units of tradable goods is constrained to stay below a constant level $\overline{b}$ at all times:

$$B_{i,t}^h \leq \overline{b}P_{T,t} \quad \forall h, i$$

Firms. Firms produce goods under monopolistic competition using local labor as their only input. Firm j produces tradable good j using the production function:

$$Y_{T,j,t} = A_{T,t}\left(N_{T,j,t}\right)^{\gamma}$$

where A_T denotes productivity in the traded good sector—which is common across firms and regions—and γ denotes the extent of diminishing returns to production. Similarly, non-tradable firms in both the home and foreign regions produce non-tradable goods using the production function:

$$Y_{NT,i,j,t} = A_{NT,t}\left(N_{NT,i,j,t}\right)^{\gamma} \qquad i \in \{H, F\}$$

where A_{NT} denotes the common productivity in the non-traded good sector.

Prices for both traded and non-traded goods are set in a Calvo fashion, with each firm getting a chance to reset their price with a probability $1 - \theta$ that is identically and independently distributed across firms and over time. When $\theta = 0$, prices are fully flexible. The degree of price rigidity θ is the same for both traded and non-traded goods. Where $\Upsilon > 0$, firms face an effective 'kink' in their demand curves, and they have an incentive to keep their prices close to their competitors' prices. The implied real rigidity flattens the slope of the Phillips curve (see e.g. Gopinath and Itskhoki 2011, Nakamura and Steinsson 2013).

Monetary Policy. For our baseline specification, we assume that monetary policy is such that there is a constant nominal interest rate of I_t:

$$I_t = \bar{I} \tag{5}$$

where $\bar{I}$ is the steady-state nominal interest rate in our model. This baseline specification implies that monetary policy is unresponsive to the effect of debt relief shocks, mimicking a situation where debt relief takes place at the zero lower bound.[16] We refer to this policy as *ZLB policy*.

We will also examine the effect of debt relief in normal times, where we would expect monetary policy to react to the inflationary effects of debt relief shocks. To capture this scenario, we consider *Taylor rule policies* in which the central bank sets the nominal interest rate to respond to consumer price inflation in both regions with an equal weight ϕ_π:

$$1 + I_t = (1 + \bar{I}) \left(\frac{P_{H,t}}{P_{H,t-1}} \right)^{\phi_\pi} \left(\frac{P_{F,t}}{P_{F,t-1}} \right)^{\phi_\pi} \tag{6}$$

where $\frac{P_{H,t}}{P_{H,t-1}}$ is home consumer price inflation at time t, $\frac{P_{F,t}}{P_{F,t-1}}$ is foreign CPI inflation, and ϕ_π is the degree of responsiveness of monetary policy to inflation.

To ensure the determinacy of outcomes in response to transitory debt relief shocks, both in the ZLB policy case given by Equation (5), and in the Taylor rule policy case given by Equation (6) when ϕ_π is sufficiently low, we assume that the central bank ensures that the economy returns to steady state at a time T that is far enough in the future that the effects of these debt relief shocks have died out.

[16] While the steady-state nominal interest rate $\bar{I}$ is always strictly positive in our model, it is easy to conceive of "Great Recession" shocks, such as shocks to productivity or discount factors, that would push the economy to $I_t = 0$. Since the model is approximately linear, these shocks would not interact with the effects of debt relief conditional on ZLB policy. Our assumption helps us isolate the effect of debt relief at the ZLB without explicitly modeling these Great Recession shocks.

Equilibrium. Equilibrium in our model is defined in a standard way.[17] At any point in time, the labor market clears locally, so that aggregate employment in region i is both equal to aggregate supply by all agents and to aggregate demand by all local firms (the sum of traded goods employment $N_{T,i}$ and non-traded goods employment $N_{NT,i}$),

$$N_{i,t} \equiv \varphi^B N_{i,t}^B + \varphi^S N_{i,t}^S = N_{T,i,t} + N_{NT,i,t} \quad i \in \{H, F\}$$

The non-traded goods market also clears locally, so that $C_{NT,i,j,t} = Y_{NT,i,j,t}$ in each region i and for each non-traded firm j. The traded goods and the asset market clear nationwide, implying in particular that all borrowers' debts are also savers' assets:

$$\frac{\varphi^B}{2}(B_{H,t}^B + B_{F,t}^B) + \frac{\varphi^S}{2}(B_{H,t}^S + B_{F,t}^S) = 0.$$

In the zero-inflation steady state of our model, borrowers in both regions are at their credit constraint:

$$B_i^B = \bar{b} P_T \quad \forall i$$

and the nominal and real interest rate are both equal to $\bar{I} = \left(\beta^S\right)^{-1} - 1$.

Debt Relief. We model the debt relief shock as follows. At time 0, the government unexpectedly announces that home borrowers will receive a sequence of transfers $\Delta_{H,t}^B$, and that foreign borrowers will receive a sequence of transfers $\Delta_{F,t}^B$. These transfers are financed by a contemporaneous tax on savers, and are therefore equivalent to a write-down of borrowers' debts. Note that the lump-sum nature of these transfers imply that they do not affect borrowing rates or ex-ante credit supply. Hence, they represent a pure *ex-post debt relief shock*. We come back to this assumption in Section G.

Each period, total transfers $\Delta_t \equiv \frac{\varphi^B}{2}\left(\Delta_{H,t}^B + \Delta_{F,t}^B\right)$ are paid for by a lump-sum tax on savers in one or both regions, such that:

$$\frac{\varphi^S}{2}\Delta_{H,t}^S = -f\Delta_t \quad \frac{\varphi^S}{2}\Delta_{F,t}^S = -(1-f)\Delta_t \tag{7}$$

The parameter $f \in [0,1]$ in (7) indexes the regional incidence of payment for the debt relief in our model. Consider, for instance, the case where $\Delta_{F,t}^B = 0$, so that debt relief only takes place in the home region H. Then, if $f = 1$, savers at home pay entirely for local debt relief, generating the

[17] See Appendix D for the full set of equations describing the equilibrium, as well as a description of our solution method.

largest domestic offset. In that case, the aggregate demand effect of debt relief comes entirely from the difference in marginal propensities to consume between borrowers and savers in the home region. If $f < 1$, debt relief also implies regional redistribution, or a net asset transfer from the foreign region to the home region. The case $f = \frac{1}{2}$ implies that the burden of debt relief in the home region is borne equally by savers in both regions.

B. Model Calibration

Table 5 summarizes our baseline calibration, with all parameters at the quarterly frequency. We set the fraction of borrowers in each region to 0.5, reflecting the approximate share of households carrying a positive credit card balance in our data.

We set $\beta^S = 0.983$ so that the steady-state annual real interest rate is 7 percent per year, corresponding to the average interest rate paid by households in 2007Q4 according to national account estimates.[18] For borrowers, we consider a baseline of $\beta^B = 0.96$, implying that they would be ready to pay up to a 15% annual interest rate on consumer credit.[19] We set $\overline{b}/C = 1$ so that aggregate credit card debt equals 12.5 percent of annual PCE, its U.S. value in 2008.[20] We set the Frisch elasticity of labor supply to a standard macroeconomic value of $\psi^{-1} = 1$, and set $\sigma = 1$ so that, in our baseline of separable preferences, $u^h(C, N) = \frac{C^{1-\sigma}}{1-\sigma} - \chi^h \frac{N^{1+\psi}}{1+\psi}$, the long-run income and substitution effects on labor supply cancel out.

We set $1 - \alpha = 0.236$, since our measure of non-tradable sectors, following Mian and Sufi (2014*b*), represents 23.6 percent of employment. We follow the standard practice of setting an elasticity of substitution within tradable and non-tradable goods equal to 10 to match a steady-state level of markups of 11 percent, and set the elasticity of substitution between T and NT to a relatively high value of 2. We set $\gamma = 0.66$ to match a steady-state labor share of $\frac{\epsilon}{\epsilon-1}\gamma = 0.6$, as in NIPA data for the period.

Our baseline features sticky prices with $\theta = 0.76$, implying a frequency of price changes of 24% per quarter, corresponding to the frequency of regular price changes for the median sector in the U.S. CPI (Nakamura and Steinsson 2008). In this baseline, we also assume no Kimball kink, $\Upsilon = 0$. We

[18] According to the BEA, mortgage interest payments were \$682bn and other consumer credit payments were \$265bn at an annualized rate in 2007Q3. According to the Flow of Funds, in that quarter there was \$10.5trn outstanding in mortgage credit and \$2.5trn in consumer credit. The average annual interest rate paid by households was therefore 6.9 percent in that quarter.

[19] 15% is the average interest rate charged on credit card plans by commercial banks (source: FRED, TERMCBCCINTNS). Provided that $\beta^B < \beta^S$, our model produces identical outcomes for small shocks. β^B becomes relevant when we study large shocks, as we discuss in Section IV.F.

[20] In our data, the average credit card balance is \$4000 per person. This implies a balance of $\frac{\$4000}{\varphi^B} = \$8000$ per borrower, which is 25 percent of annual PCE, or 100 percent of quarterly PCE.

consider robustness with respect to both of these parameters. When $\theta = 0$, prices are fully flexible.

We normalize the steady-state hours of borrowers and savers to 1, implying that borrowers and savers have the same income before interest payments. In the steady state equilibrium, borrowers are at their borrowing limits and spend 1.7 percent of their income on interest payments. We set average PCE at $C = \$32,000$, its U.S. value in 2007Q4. We solve for a symmetric steady state in which all relative prices are 1. We obtain the values of A_T, A_{NT}, χ^S and χ^B that achieve these steady-state targets, as described in Appendix D.

C. Impulse Responses to Debt Relief Shock and Baseline Multipliers

We study impulse responses to debt relief shocks in our model. Recall from Section III that we are interested in recovering employment multipliers $\mathcal{M}_H$ and $\mathcal{M}_F$ for both the tradable and non-tradable sectors. To calculate these, we assume that the only shock that hits the model is a one-time home debt relief shock of $\Delta_H = 1$ percent of home PCE, with $\Delta_F = 0$. This shock corresponds to a reduction of the level of debt of every borrower in the home region by 2 percent, or \$160.

For our baseline calibration, Appendix Figure A8 presents the impulse responses of macroeconomic aggregates to this one-time debt relief shock in the home region. The shock generates essentially no persistence on aggregate outcomes beyond the first quarter. The intuition is that, given the small magnitude of the shock, constrained agents have a zero marginal propensity to save out of debt relief: their spending and labor supply is affected immediately, and in the next period they are back at their borrowing constraint.[21] Our model therefore fits the assumptions used to derive Equation (2), so that we can focus on "the" debt relief multiplier for a given region-sector pair. Consistent with this equation, we calculate $\mathcal{M}_{s,H}$ (resp. $\mathcal{M}_{s,F}$) as the first-quarter response of log employment in sector s in the home (resp. the foreign) region to this home debt relief shock of 1% of consumption.

Panel A of Table 6 reports these multipliers for our baseline calibration. When the home debt relief shock hits, non-tradable employment rises by 1.56% at home and by 0.33% in the foreign region—a relative multiplier of 1.23. In contrast, tradable employment increases by 0.89% in the home region and 0.99% in the foreign region—a relative multiplier of -0.10. These two relative debt relief multipliers share two key features of our empirical estimates reported in Table 4: the tradable relative multiplier is large (i.e. above 1), and the nontradable multiplier is around 0.

Our model's success in achieving these two features relies on the interaction between its three

[21]See Auclert, Rognlie and Straub (2018) for a richer model of heterogeneity in marginal propensities to consume that would generate more persistence.

main ingredients: heterogeneity in marginal propensities to consume between borrowers and savers, nominal rigidities, and trade linkages across regions. First, debt relief redistributes wealth from savers, who have low MPCs, to borrowers in the home region, who have high MPCs. This redistribution stimulates consumption in the home region, increasing aggregate demand for non-tradable home goods and for tradable goods. Second, given relatively sticky prices, this additional spending stimulates production and employment in the sectors producing these goods. Finally, given trade linkages, the additional home spending also stimulates production of foreign tradable goods, and therefore increases foreign tradable employment. Overall, nontradable employment increases mostly at home, delivering a large $\mathcal{M}_{NT,rel}$ due to a large $\mathcal{M}_{NT,H}$, while tradable employment increases everywhere, delivering an $\mathcal{M}_{T,rel}$ of around 0.

An important take-away from these calculations is that an empirical estimate of zero for relative tradable multipliers can mask a large and positive response of tradable employment in both regions. In our model, $\mathcal{M}_{T,rel}$ is in fact slightly negative, because the increase in home wages makes goods produced in the home region relatively more expensive. These "terms of trade" effects induce some substitution of consumers away from home goods towards foreign goods, so that foreign traded employment increases a little more than home traded employment. Similarly, $\mathcal{M}_{NT,H}$ is even larger than $\mathcal{M}_{NT,rel}$, because the increase in foreign tradable employment increases foreign incomes, which in turn increases foreign spending on non-tradable foreign goods, and therefore non-tradable employment in the foreign region (i.e., $\mathcal{M}_{NT,F}$ is positive).[22] The broader conclusion is that, when equilibrium adjustment mostly takes place through incomes—as is the case in this calibration with nominal rigidities—the effect of an aggregate demand treatment on "control" regions is often positive ($\mathcal{M}_F > 0$), so that the empirically measured difference-in-difference effect $\mathcal{M}_{rel}$ understates the effect of the treatment on the treated region $\mathcal{M}_H$.

Panel B of Table 6 shows why nominal rigidities are essential to simultaneously achieving a large nontradable and a zero tradable relative multiplier by considering an alternative calibration with flexible prices ($\theta = 0$). In this calibration, aggregate home employment goes *down* in the short-run, while aggregate foreign employment goes up. (Appendix Figure A9 shows the full set of impulse responses under flexible prices.) Upon receiving debt relief, home borrowers increase consumption but also immediately reduce hours worked (a wealth effect, large under standard separable preferences). Since

[22] See Chodorow-Reich 2019, House, Proebsting and Tesar (2020), Dupor et al. (2021), Chodorow-Reich et al. (2020) and Chodorow-Reich, Nenov and Simsek (2021) for other currency union models in which tradable demand spillovers are important determinants of the aggregate multiplier.

non-traded goods can only be produced locally while traded goods can be imported, the overall effect is a moderate rise in local non-tradable employment and a large increase in imports. Higher prices for foreign tradable goods stimulates employment in the foreign region overall, as well as reallocation away from non-tradable production in that region. Overall, under flexible prices, $\mathcal{M}_{NT,rel}$ is close to 0 while $\mathcal{M}_{T,rel}$ is very negative (i.e., less than -0.6). Those features are qualitatively very different from our empirical multipliers in Table 4.[23]

D. Sensitivity to Parameter Values

Our baseline calibration used parameters that are standard in the macroeconomics literature, and was able to achieve a fit to the key qualitative features of our empirical multipliers in Table 4. In this section, we demonstrate that these magnitudes have limited sensitivity to our calibrated parameter values. We do this by varying the parameters of our model one by one starting from our baseline and recalculating multipliers.

The three parameters that do have a significant effect on multipliers are the degree of price rigidity θ, the Taylor rule coefficient ϕ_π, and the degree of home bias α. Figure 4 shows the effect of varying these parameters. In each Panel, the x axis varies parameters away from their baseline value in Table 5, indicated by a dashed vertical line. The left Panels report nontradable employment multipliers, and the right Panels reports tradable employment multipliers. The black line denotes the relative multiplier $\mathcal{M}_{rel}$ that we would measure from a differerence-in-difference estimates, while the red and blue line reports the underlying home and foreign multipliers $\mathcal{M}_H$ and $\mathcal{M}_F$.

Panels A and B vary the degree of price rigidity θ. All multipliers are monotone in θ. When $\theta = 0$, we recover the results from Panel B in Table 6. At the other extreme, when prices are fully fixed ($\theta = 1$), the relative employment multiplier is 0 for nontradable employment and 1.3XX for nontradable employment. Hence, increasing price rigidity can increase multipliers somewhat, but cannot increase nontradable multipliers beyond 1.5. The intuition can be understood by considering that, when prices are fully sticky, we can show:

$$\mathcal{M}_{agg} \simeq \epsilon_{N,Y} \cdot M^G \cdot \left(\overline{MPC_R} - \overline{MPC_D}\right) \tag{8}$$

where $\epsilon_{N,Y}$ is the employment-on-output elasticity (which could vary by sector), M^G is the govern-

[23] Another way of validating our assumption of price rigidity is to directly look at inflation as an outcome in our empirical research design. Appendix Table A7 does this using inflation measures from Hazell et al. (2021) and shows that the response of both non-tradable and tradable CPI to the bankruptcy protections was small and insignificant.

ment spending multiplier on GDP, $\overline{MPC_R}$ the average MPC of the receivers, and $\overline{MPC_D}$ is the average MPC of the donors. Here the MPC difference is 1, $\epsilon_{N,Y} = \frac{1}{\gamma} = 1.5$, and the government spending output multiplier M^G is around 1, as follows from a standard model calibration with fully sticky prices at the zero lower bound.

Panels C and D vary the Taylor rule response to inflation ϕ_π. Our base case of the zero lower bound corresponds to $\phi_\pi = 0$. Here, monetary policy does not respond to the increase in inflation from the pressure of aggregate demand of debt relief. The figure shows what happens with $\phi_\pi > 0$, which we would expect from normal times with the central bank countering the inflation created by the shock. The larger the response of monetary policy, the lower the effect on non-tradable and tradable employment in both regions. However, irrespective of the degree of monetary tightening, the relative multipliers $\mathcal{M}_{T,rel}$ and $\mathcal{M}_{NT,rel}$ remain unchanged.[24] This is because, as Nakamura and Steinsson (2014) have pointed out, the relative multiplier differences out the effect of monetary policy across the two regions. Hence, the monetary response cannot help us achieve a closer fit to empirical multipliers. With a sufficiently large ϕ_π, $\mathcal{M}_{NT,F}$ turns negative, but note that $\mathcal{M}_{T,F}$ is positive throughout. Intuitively, the conclusion that a small relative tradable multiplier masks two large positive multipliers in the home and foreign region is robust to the response of monetary policy, because tradable goods always have large regional spillover effects.

Finally, Panels E and F evaluate the effect of changing the tradable share of consumption α. As α is reduced, the home multiplier rises and the foreign multiplier falls, increasing $\mathcal{M}_{NT,rel}$. More home bias reduces the amount of foreign demand "leakage," increasing the local multiplier. Holding the other parameters constant, we obtain a $\mathcal{M}_{NT,rel}$ as large as 2 when α becomes very small. However, in this case, the tradable multiplier $\mathcal{M}_{T,rel}$ is quite negative. Intuitively, the economy is never fully closed, because some "leakage" always occurs via tradable goods even when there aren't any nontradable goods.

While the three parameters θ, ϕ_π and α have a relatively large effect in our model, the other parameters have a comparably much smaller effect. Appendix Figure A7 shows this by reproducing Figure 4 when we instead vary the preference complementarity parameter ξ, the Kimball superelasticity parameter Υ, and the fraction of debt relief f borne at home. These parameters have essentially no effect on either the relative or the absolute debt relief multipliers. The only exception is when ξ is exactly equal to 1. This corresponds to the case where preferences are exactly GHH, for which

[24] Since this is a currency union model where relative prices across regions also matter, the limit with the Taylor rule coefficient $\phi_\pi \to \infty$ is different from the flexible price limit, $\theta \to 0$.

multipliers tend to infinity (Auclert, Bardóczy and Rognlie 2021). However, multipliers only start to increase away from the baseline when ξ is very close to 1, showing that our results are robust to plausible degrees of consumption-labor complementarity.

We conclude that our baseline calibration delivers a set of multipliers that are very reasonable to conduct a counterfactual analysis. Those multipliers are qualitatively consistent with the features of our emprical estimates in Table 4. Confirming the back-of-the envelope calculation in equation 8, reasonable parameter changes can never achieve non-tradable multipliers in the 2-6 range, leading us to conclude that our empirical strategy likely overstates these multipliers.

E. Aggregate Effect of Debt Relief and Main Counterfactual

Our main counterfactual exercise asks the following question: across the United States, what would aggregate employment have been if all states had started off with larger exemptions? Concretely, we take states in the bottom tercile of homestead exemptions in Appendix Table A1 and ask what their counterfactual employment would have been if they had been in the top tercile of homestead exemptions (an increase from a median of $15000 to a median of Unlimited).

It is clear that the relative multiplier $\mathcal{M}_{rel}$ cannot be used to answer this question. If bottom and top tercile states had had the exemption level, they would also have experienced the same path of debt relief. The best we can say is that their counterfactual level of employment would have been the same, but we do not know what that level would have been.

By contrast, we can construct this counterfactual with knowledge of $\mathcal{M}_H$ and $\mathcal{M}_F$. First, we assume that the additional amount of debt relief that would have occurred in the bottom tercile states would have been the same as that observed in the difference-and-difference across states. Second, we use $\mathcal{M}_H$ and $\mathcal{M}_F$ to predict the additional employment in low and high exemption regions from this additional debt relief in low exemption regions.

Figure **??** reports the result from this exercise. Solid lines report the actual paths of employment that occurred in high and low exemption regions. Dotted lines report the counterfactual path, had low exemption regions had a more generous bankruptcy system.

We find that, at the peak of the recession in 2009, nontradable employment would have been about 0.2 percentage points higher in low exemption regions, and midly higher in high exemption regions. We also find that tradable employment would have been 0.2 percentage points higher. The key to these findings is that high exemption states had chargeoffs that were higher by around 0.2%, and that the multipliers $\mathcal{M}_{NT,H}$, $\mathcal{M}_{T,H}$, and $\mathcal{M}_{NT,F}$ are all around 1, while $\mathcal{M}_{NT,F}$ is around 0.

Figure **??** also shows that the trough of employment was around 2%. Our results therefore suggest that the consumer bankruptcy system mitigated employment fluctuations in the Great Recession by about 10%.

F. Size Dependence

An important question is the extent to which the aggregate effects of debt relief depend on its scale. The empirical variation in debt relief is relatively small. How useful are these effects as a guide to the consequences of larger-scale debt relief? Figure 6 plots the debt relief multipliers, both absolute and relative, as a fraction of the size of the debt relief shock. The impact multiplier is largest when the shock Δ is small relative to PCE. This is because small transfers are spent immediately by constrained consumers. When the transfer gets sufficiently large, borrowers save a fraction of it, lowering the impact multiplier and spreading the aggregate effect of debt relief over multiple periods. The lower their discount factor β^B, the larger the transfer needs to be before this happens. In our baseline calibration with an annual $\beta^B = 0.96$, this nonlinearity takes place once transfers reach XX percent of PCE.[25] This nonlinearity result provides an important cautionary tale for debt relief programs as aggregate demand management, since they show that those programs are most effective at boosting current demand when they are relatively small in magnitude.

G. Financing Debt Relief

We now discuss how our results change depending on who finances the debt relief. If the agents that are financing the debt relief are themselves highly constrained, for example, the aggregate effect of debt relief may be smaller or even contractionary.

First, within our model, we can assess what happens as we vary the burden of debt relief financing f between home and foreign savers. If home savers pay for home debt relief, aggregate effectiveness is be more limited than if foreign savers paid for it. Panels E and F of Appendix Figure **??** shows that while this argument is qualitatively right, lowering f towards zero does not significantly reduce either the relative or the home multiplier significantly. The intuition is that in the model, saver MPCs are always low, so the amount of regional offset is low no matter where savers are located.

[25]This result can be understood by considering the shape of the consumption of borrowers, which we derive explicitly for a continuous-time version of the model in Appendix D. As in Achdou et al. (2021), the consumption function of borrowers behaves likes a square-root function of debt relief when debt relief is sufficiently small. This concave consumption function translates into a concave aggregate employment effect as a function of debt relief size, or a diminishing multiplier.

Second, we can speculate on what would happen in alternative models where the agents financing the debt relief are themselves constrained. While we do not have a fully structural model to evaluate this hypothesis, we can go back to equation (8) to intuit quantitative magnitudes. First, consider a model in which banks pass-through the cost of debt relief to their shareholders. This could be because share-holders are wealth hand-to-mouth individuals, or are older without bequest motives. In such a case, the MPC of givers $\overline{MPC}^G$ is likely bounded by 1. Presuming that the receivers also have an $\overline{MPC}^R$ around 1, this suggests the aggregate effect of debt relief might be 0.

Second, consider a model in which banks are themselves constrained and are not getting bailed out. With balance sheet constraints as in e.g. Bernanke, Gertler and Gilchrist (1999), banks could cut lending and therefore affect aggregate investment by more than 1 for 1 in response to a decline in equity. This would translate into an effective marginal propensity to spend of more than 1 for the givers, bringing $\overline{MPC}^R - \overline{MPC}^G$ below 0, potentially much below 0. Since banks are national, this effect would be differenced out from the cross-section.

We conclude that a full assessment of the effects of debt relief depend on government bailouts, and the extent to which banks experience financial constraints that prevent them from passing through the cost of debt relief to their customers. We cannot reject the hypothesis that the aggregate effects of debt relief were very negative during the Great Recession from financial accelerator mechanisms in the banking sector. A more

H. Empirics-Theory Disconnect: Magnitudes and Persistence

We conclude this section by considering the discrepancies between empirical estimates and model predictions. There are two sources of discrepancies: the model does not generate large enough magnitudes for nontradable multipliers relative to the data, and the model predicts much less persistent effects than those we observe in the data.

On the level, we have shown that bringing multipliers to the range of 2-6 requires implausible assumptions. This could in principle occur with very strong consumption-labor complementarity, much larger home bias (such as if tradable goods were largely produced locally), or large intertemporal feedbacks from precautionary-savings type effects. We view these as implausible, but further research is needed to rule them out completely.

Figure 3 shows that the non-tradable employment effects are highly persistent in the data, and, in fact, increase over time. These results are consistent with the highly persistent responses to local unemployment shocks documented in Yagan (2019), but imply that the long-run debt relief multipliers are even larger than the short-run multipliers. Simple models in which debt relief constitutes a transitory demand shock, including our model, are unable to generate these kinds of highly persistent responses. Neoclassical adjustment mechanisms, such as a change in relative prices or movements of capital and labor across regions, tend to make these transitory demand effects disappear in the medium-run, also contrasting with our empirical findings. On the other hand, mechanisms relying on an endogenous response of innovation to demand shocks (Benigno and Fornaro 2018), or on a slow human capital decay following aggregate unemployment (Jarosch 2021) may be able to quantitatively explain our findings. We also leave this question to future research.

V. Conclusion

We use cross-state variation in bankruptcy exemptions and a general equilibrium model to show that the ex-post debt forgiveness provided by the consumer bankruptcy system stabilized macroeconomic activity during the Great Recession. We find that states with more generous bankruptcy exemptions had larger consumer debt write-downs and smaller declines in local non-tradable employment compared to less generous states during this time period. In contrast, we estimate a small and statistically insignificant effect of more generous bankruptcy exemptions on local tradable employment during the Great Recession, and all outcomes prior to the Great Recession. The implied relative debt relief multiplier from our estimates suggests that each additional percent of PCE spent on reducing local borrowers' debt results in a 5.58 percent relative increase in non-tradable employment and a 0.39 percent relative increase in tradable employment. However, we show that these relative multipliers are insufficient to identify the overall level effect of debt relief during this period.

To recover an aggregate debt relief multiplier, we develop a general equilibrium currency union model of consumer debt forgiveness featuring non-tradable and tradable goods. We find that substantial nominal rigidities are required to rationalize our reduced form estimates. With monetary policy at the zero lower bound, spillovers through traded good demand imply a large causal effect of debt relief on employment in both high and low debt relief locations. Our model implies an aggregate debt relief multiplier of 1.88, suggesting that the debt forgiveness provided during the Great Recession increased aggregate employment by almost two percent at the end of 2009. Our multiplier is large in part because debt relief is sufficiently small that consumers spend all of it immediately.

The findings from this paper can help inform ongoing efforts to evaluate the welfare impact of the consumer bankruptcy system. These evaluations typically use quantitative models to weigh the trade-off between the ex-post consumption smoothing benefits provided by bankruptcy protection at the individual level (e.g., Dobbie and Song 2015), with the ex-ante increased borrowing costs suggested by economic theory (e.g., Athreya 2002, Li and Sarte 2006, Livshits, MacGee and Tertilt 2007, Chatterjee and Gordon 2012) and documented empirically by Severino and Brown (2020). Our results suggest that aggregate demand effects are important to evaluate the macroeconomic benefits of the consumer bankruptcy system, so that the overall benefit may be broader than previously realized. We therefore view the incorporation of our empirical estimates into a general equilibrium model of the credit market incorporating ex-ante effects as an important area for future research.

References

Abadie, Alberto, Susan Athey, Guido W. Imbens, and Jeffrey Wooldridge. 2017. "When Should You Adjust Standard Errors for Clustering?" National Bureau of Economic Research Working Paper 24003.

Achdou, Yves, Jiequn Han, Jean-Michel Lasry, Pierre-Louis Lions, and Benjamin Moll. 2021. "Income and Wealth Distribution in Macroeconomics: A Continuous-Time Approach." *Review of Economic Studies*.

Andrews, Isaiah, James H Stock, and Liyang Sun. 2019. "Weak Instruments in Instrumental Variables Regression: Theory and Practice." *Annual Review of Economics*, 11: 727–753.

Athreya, Kartik B. 2002. "Welfare Implications of the Bankruptcy Reform Act of 1999." *Journal of Monetary Economics*, 49(8): 1567–1595.

Auclert, Adrien, and Kurt Mitman. 2021. "Consumer Bankruptcy as Aggregate Demand Management." *Manuscript*.

Auclert, Adrien, Bence Bardóczy, and Matthew Rognlie. 2021. "MPCs, MPEs and Multipliers: A Trilemma for New Keynesian Models." *Review of Economics and Statistics*, Forthcoming.

Auclert, Adrien, Matthew Rognlie, and Ludwig Straub. 2018. "The Intertemporal Keynesian Cross." National Bureau of Economic Research Working Paper 25020.

Avery, Robert B, Paul S Calem, Glenn B Canner, and Raphael W Bostic. 2003. "An Overview of Consumer Data and Credit Reporting." *Federal Reserve Bulletin*, 89: 47.

Benigno, Gianluca, and Luca Fornaro. 2018. "Stagnation Traps." *Review of Economic Studies*, 85(3): 1425–1470.

Beraja, Martin, Erik Hurst, and Juan Ospina. 2019. "The Aggregate Implications of Regional Business Cycles." *Econometrica*, 87(6): 1789–1833.

Bernanke, Ben S., Mark Gertler, and Simon Gilchrist. 1999. "Chapter 21 – The Financial Accelerator in a Quantitative Business Cycle Framework." In *Handbook of Macroeconomics*. Vol. 1, Part C, , ed. John B. Taylor and Michael Woodford, 1341–1393. Elsevier.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-In-Differences Estimates?" *Quarterly Journal of Economics*, 119(1): 249–275.

Board of Governors. 2018*a*. "Charge-Off and Delinquency Rates on Loans and Leases at Commercial Banks." Board of Governors of the Federal Reserve System. `https://www.federalreserve.gov/releases/chargeoff/chgallsa.htm`.

Board of Governors. 2018*b*. "Households and Nonprofit Organizations; Consumer Credit; Liability, Level." Board of Governors of the Federal Reserve System. Retrieved from FRED, Federal Reserve Bank of St. Louis. `https://fred.stlouisfed.org/series/HNOCCIQ027S`.

Chatterjee, Satyajit, and Grey Gordon. 2012. "Dealing with Consumer Default: Bankruptcy vs Garnishment." *Journal of Monetary Economics*, 59: S1–S16.

Chodorow-Reich, Gabriel. 2019. "Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?" *American Economic Journal: Economic Policy*, 11(2): 1–34.

Chodorow-Reich, Gabriel, Gita Gopinath, Prachi Mishra, and Abhinav Narayanan. 2020. "Cash and the Economy: Evidence from India's Demonetization." *Quarterly Journal of Economics*, 135(1): 57–103.

Chodorow-Reich, Gabriel, Plamen T. Nenov, and Alp Simsek. 2021. "Stock Market Wealth and the Real Economy: A Local Labor Market Approach." *American Economic Review*, 111(5): 1613–57.

Corbi, Raphael, Elias Papaioannou, and Paolo Surico. 2018. "Regional Transfer Multipliers." *Review of Economic Studies*, 86(5): 1901–1934.

Currie, Janet, and Jonathan Gruber. 1996. "Saving Babies: The Efficacy and Cost of Recent Changes in the Medicaid Eligibility of Pregnant Women." *Journal of Political Economy*, 104(6): 1263–1296.

Dávila, Eduardo. 2020. "Using Elasticities to Derive Optimal Bankruptcy Exemptions." *Review of Economic Studies*, 87(2): 870–913.

Department of Labor. 2018. "Unemployment Insurance Chartbook." Department of Labor.

Di Maggio, Marco, and Amir Kermani. 2016. "The Importance of Unemployment Insurance as an Automatic Stabilizer." National Bureau of Economic Research Working Paper 22625.

Dobbie, Will, and Jae Song. 2015. "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection." *American Economic Review*, 105(3): 1272–1311.

Dobbie, Will, and Paige Marta Skiba. 2013. "Information Asymmetries in Consumer Credit Markets: Evidence from Payday Lending." *American Economic Journal: Applied Economics*, 5(4): 256–82.

Dobbie, Will, Paul Goldsmith-Pinkham, and Crystal S Yang. 2017. "Consumer Bankruptcy and Financial Health." *Review of Economics and Statistics*, 99(5): 853–869.

Donaldson, Jason Roderick, Giorgia Piacentino, and Anjan Thakor. 2019. "Household Debt Overhang and Unemployment." *Journal of Finance*, 74(3): 1473–1502.

Dupor, Bill, Marios Karabarbounis, Marianna Kudlyak, and M. Saif Mehkari. 2021. "Regional Consumption Responses and the Aggregate Fiscal Multiplier." Federal Reserve Bank of San Francisco Working Paper 2018-04.

Eggertsson, Gauti B., and Paul Krugman. 2012. "Debt, Deleveraging, and the Liquidity Trap: A Fisher-Minsky-Koo Approach." *Quarterly Journal of Economics*, 127(3): 1469–1513.

Fan, Wei, and Michelle J White. 2003. "Personal Bankruptcy and the Level of Entrepreneurial Activity." *Journal of Law and Economics*, 46(2): 543–568.

Farhi, Emmanuel, and Iván Werning. 2016*a*. "Fiscal Multipliers: Liquidity Traps and Currency Unions." In *Handbook of Macroeconomics*. Vol. 2, 2417–2492. Elsevier.

Farhi, Emmanuel, and Iván Werning. 2016*b*. "A Theory of Macroprudential Policies in the Presence of Nominal Rigidities." *Econometrica*, 84(5): 1645–1704.

Fay, Scott, Erik Hurst, and Michelle J White. 2002. "The Household Bankruptcy Decision." *American Economic Review*, 92(3): 706–718.

Freyaldenhoven, Simon, Christian Hansen, and Jesse M Shapiro. 2019. "Pre-Event Trends in the Panel Event-Study Design." *American Economic Review*, 109(9): 3307–38.

Ganong, Peter, and Pascal Noel. 2020. "Liquidity versus Wealth in Household Debt Obligations: Evidence from Housing Policy in the Great Recession." *American Economic Review*, 110(10): 3100–3138.

Ghent, Andra C, and Marianna Kudlyak. 2011. "Recourse and Residential Mortgage Default: Evidence from US States." *Review of Financial Studies*, 24(9): 3139–3186.

Goodman, Paul. 1993. "The Emergence of Homestead Exemption in the United States: Accommodation and Resistance to the Market Revolution, 1840–1880." *Journal of American History*, 80(2): 470–498.

Gopinath, Gita, and Oleg Itskhoki. 2011. "In Search of Real Rigidities." *NBER Macroeconomics Annual*, 25(1): 261–310.

Greenwood, Jeremy, Zvi Hercowitz, and Gregory W. Huffman. 1988. "Investment, Capacity Utilization, and the Real Business Cycle." *American Economic Review*, 78(3): 402–417.

Gropp, Reint, John Karl Scholz, and Michelle J White. 1997. "Personal Bankruptcy and Credit Supply and Demand." *Quarterly Journal of Economics*, 112(1): 217–251.

Gross, Tal, Matthew J Notowidigdo, and Jialan Wang. 2020. "The Marginal Propensity to Consume Over the Business Cycle." *American Economic Journal: Macroeconomics*, 12(2): 351–84.

Guerrieri, Luca, and Matteo Iacoviello. 2015. "Occbin: A Toolkit for Solving Dynamic Models with Occasionally Binding Constraints Easily." *Journal of Monetary Economics*, 70: 22–38.

Guerrieri, Veronica, and Guido Lorenzoni. 2017. "Credit Crises, Precautionary Savings, and the Liquidity Trap." *Quarterly Journal of Economics*, 132(3): 1427–1467.

Guren, Adam M, Alisdair McKay, Emi Nakamura, and Jón Steinsson. 2021. "Housing Wealth Effects: The Long View." *Review of Economic Studies*, 88(2): 669–707.

Hall, Robert E. 2011. "The Long Slump." *American Economic Review*, 101(2): 431–469.

Hausman, Joshua K., Paul W. Rhode, and Johannes F. Wieland. 2019. "Recovery from the Great Depression: The Farm Channel in Spring 1933." *American Economic Review*, 109(2): 427–472.

Hazell, Jonathon, Juan Herreno, Emi Nakamura, and Jón Steinsson. 2021. "The Slope of the Phillips Curve: Evidence from U.S. States." *Quarterly Journal of Economics*, Forthcoming.

House, Christopher L, Christian Proebsting, and Linda L Tesar. 2020. “Austerity in the Aftermath of the Great Recession.” *Journal of Monetary Economics*, 115: 37–63.

Hsu, Joanne W, David A Matsa, and Brian T Melzer. 2018. “Unemployment Insurance as a Housing Market Stabilizer.” *American Economic Review*, 108(1): 49–81.

Jarosch, Gregor. 2021. “Searching for Job Security and the Consequences of Job Loss.” National Bureau of Economic Research Working Paper 28481.

Jones, Callum, Virgiliu Midrigan, and Thomas Philippon. 2018. “Household Leverage and the Recession.” National Bureau of Economic Research Working Paper 16965.

Kaplan, Greg, Giovanni L. Violante, and Justin Weidner. 2014. “The Wealthy Hand-to-Mouth.” *Brookings Papers on Economic Activity*, 2014(1): 77–138.

Klenow, Peter J., and Jonathan L. Willis. 2016. “Real Rigidities and Nominal Price Changes.” *Economica*, 83(331): 443–472.

Korinek, Anton, and Alp Simsek. 2016. “Liquidity Trap and Excessive Leverage.” *American Economic Review*, 106(3): 699–738.

Lee, Donghoon, and Wilbert Van der Klaauw. 2010. “An Introduction to the FRBNY Consumer Credit Panel.” Federal Reserve Bank of New York Staff Report.

Livshits, Igor, James MacGee, and Michele Tertilt. 2007. “Consumer Bankruptcy: A Fresh Start.” *American Economic Review*, 97(1): 402–418.

Li, Wenli, and Pierre-Daniel Sarte. 2006. “US Consumer Bankruptcy Choice: The Importance of General Equilibrium Effects.” *Journal of Monetary Economics*, 53(3): 613–631.

Mahoney, Neale. 2015. “Bankruptcy as Implicit Health Insurance.” *American Economic Review*, 105(2): 710–746.

Martin, Philippe, and Thomas Philippon. 2017. “Inspecting the Mechanism: Leverage and the Great Recession in the Eurozone.” *American Economic Review*, 107(7): 1904–37.

Mian, Atif, and Amir Sufi. 2014*a*. *House of Debt: How They (and You) Caused the Great Recession, and How We Can Prevent It from Happening Again*. Chicago; London: University Of Chicago Press.

Mian, Atif, and Amir Sufi. 2014*b*. “What Explains the 2007–2009 Drop in Employment?” *Econometrica*, 82(6): 2197–2223.

Mian, Atif, Kamalesh Rao, and Amir Sufi. 2013. “Household Balance Sheets, Consumption, and the Economic Slump.” *Quarterly Journal of Economics*, 128(4): 1687–1726.

Mitman, Kurt. 2016. “Macroeconomic Effects of Bankruptcy and Foreclosure Policies.” *American Economic Review*, 106(8): 2219–2255.

Montiel Olea, José Luis, and Mikkel Plagborg-Møller. 2019. "Simultaneous Confidence Bands: Theory, Implementation, and an Application to SVARs." *Journal of Applied Econometrics*, 34(1): 1–17.

Nakamura, Emi, and Jón Steinsson. 2008. "Five Facts about Prices: A Reevaluation of Menu Cost Models." *Quarterly Journal of Economics*, 123(4): 1415–1464.

Nakamura, Emi, and Jón Steinsson. 2013. "Price Rigidity: Microeconomic Evidence and Macroeconomic Implications." *Annual Review of Economics*, 5(1): 133–163.

Nakamura, Emi, and Jón Steinsson. 2014. "Fiscal Stimulus in a Monetary Union: Evidence from US Regions." *American Economic Review*, 104(3): 753–792.

Saiz, Albert. 2010. "The Geographic Determinants of Housing Supply." *Quarterly Journal of Economics*, 125(3): 1253–1296.

Severino, Felipe, and Meta Brown. 2020. "Personal Bankruptcy Protection and Household Debt." *Manuscript*.

Skeel, David A. 2001. *Debt's Dominion: A History of Bankruptcy Law in America.* Princeton University Press.

Suárez Serrato, Juan Carlos, and Philippe Wingender. 2016. "Estimating Local Fiscal Multipliers." National Bureau of Economic Research Working Paper 22425.

U.S. Courts. 2017. "Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) Report." http://www.uscourts.gov/statistics-reports/bapcpa-report-2017.

Verner, Emil, and Győző Gyöngyösi. 2020. "Household Debt Revaluation and the Real Economy: Evidence from a Foreign Currency Debt Crisis." *American Economic Review*, 110(9): 2667–2702.

Whittaker, Julie M., and Katelin P. Isaacs. 2016. "Unemployment Insurance: Programs and Benefits." Congressional Research Service CRS Report RL33362. https://fas.org/sgp/crs/misc/RL33362.pdf.

Yagan, Danny. 2019. "Employment Hysteresis from the Great Recession." *Journal of Political Economy*, 127(5): 2505–2558.

Table 1: Descriptive Statistics

	Avg Change from 2001q1-2007q4	Avg Change from 2007q4-2010q4	Avg Change from 2010q4-2015q4
Panel A: Employment Outcomes	(1)	(2)	(3)
Non-Tradable Emp. (Δ p.p.)	0.76	-1.85	1.72
Tradable + Other Emp. (Δ p.p.)	-0.46	-2.43	1.35
Total Emp. (Δ p.p.)	-0.25	-2.33	1.42
Panel B: Credit Outcomes			
Annual Charge-offs (Δ $)	-0.81	126.09	-283.31
Bankrupty Rate (Δ p.p.)	0.46	0.10	-0.19
Foreclosure Rate (Δ p.p.)	0.18	0.92	-0.66
Credit Card Limits (Δ $000)	2.87	-3.05	2.24
Credit Card Debt (Δ $000)	0.32	-0.21	-0.11
Mortgage Debt (Δ $000)	26.14	0.44	-1.44
Panel C: Inflation Outcomes			
Log Non-Tradable Price Index (Δ p.p.)	2.93	2.71	2.27
Log Tradable Price Index (Δ p.p.)	1.29	1.19	0.51
Log Total Price Index (Δ p.p.)	2.38	2.19	1.68
Panel D: Housing Outcomes			
Log House Price (Δ p.p.)	5.40	-7.14	2.55

Note: This table reports summary statistics. Each column reports the difference in the outcome over the period, averaged over all counties in the sample using population weights. Panel A reports the annualized difference in log employment per working-age person multiplied by 100 (the approximate annualized percent change in the share employed of the working-age population). Employment data is from the Quarterly Census of Employment and Wages. Non-tradable industries are defined as the retail and restaurant sectors following Mian and Sufi (2014*b*). Tradable and Other industries are also defined following Mian and Sufi (2014*b*). Panel B reports the average change across counties for various credit outcomes using data from Equifax. Annual charge-offs reports the average difference in dollars in the amount of derogatory debt discharged by credit card and auto lenders per person. Bankruptcy and foreclosure rates report the difference in the percent of people who file for foreclosure and bankruptcy per county. Credit card limits and debt report the difference in dollars per person and mortgage debt reports the difference in mortgage debt in thousands of dollars per person. Panel C reports the annualized difference in log consumer price index over the period multiplied by 100, or the annualized percent change in inflation. Price index data are from Hazell et al. (2021). Panel D reports the annualized difference in log house prices multiplied by 100, or the annualized percent change in house prices. House price data comes from CoreLogic.

Table 2: Correlates with State Bankruptcy Protection Measure in 2007

		Bankruptcy Protections		
	Average Value	Binned Homestead	Log Homestead	Simulated IV
Panel A: Employment Outcomes	(1)	(2)	(3)	(4)
Log Non-Tradable Emp. (p.p.)	228.41	5.71**	0.75	0.59
	(7.48)	(2.83)	(1.21)	(1.19)
Log Tradable + Other Emp. (p.p.)	383.73	1.52	−0.04	−0.38
	(7.35)	(2.92)	(1.21)	(1.26)
Log Total Emp. (p.p.)	402.98	2.27	0.11	−0.19
	(6.49)	(2.58)	(1.02)	(1.07)
Panel B: Credit Outcomes				
Bankruptcy Rate (p.p.)	0.44	−0.08**	−0.04**	−0.04**
	(0.12)	(0.04)	(0.02)	(0.02)
Foreclosure Rate (p.p.)	0.89	−0.06	−0.07	−0.10
	(0.37)	(0.13)	(0.06)	(0.06)
Credit Card Limits ($000)	15.47	−0.56	−0.16	−0.07
	(2.41)	(0.96)	(0.45)	(0.48)
Credit Card Debt ($000)	3.66	−0.02	0.02	0.04
	(0.37)	(0.10)	(0.04)	(0.05)
Mortgage Debt ($000)	54.11	−8.91	−3.21	−0.59
	(22.61)	(10.06)	(4.00)	(4.48)
Debt-to-Income Ratio	1.97	−0.09	−0.02	0.04
	(0.62)	(0.34)	(0.13)	(0.13)
Panel C: State Characteristics				
Democratic Vote Share (p.p.)	51.89	−3.66	−0.89	−0.20
	(8.76)	(3.97)	(1.82)	(2.06)
Max. Unemp. Benefit ($)	10.63	−0.01	0.19	0.24
	(3.21)	(1.60)	(0.69)	(0.79)
Population <45 (p.p.)	62.02	−0.13	0.11	0.27
	(2.94)	(1.95)	(0.81)	(0.80)
College Educated (p.p.)	27.40	−0.52	−0.23	0.09
	(4.26)	(1.46)	(0.77)	(0.81)
Employment Sensitivity to Business Cycle	1.04	−0.00	−0.04	−0.04
	(0.24)	(0.09)	(0.04)	(0.04)
Log Average House Price	12.32	−0.14	−0.02	0.03
	(0.47)	(0.21)	(0.09)	(0.10)
Homeownership Share	0.68	0.01	−0.01	−0.01*
	(0.05)	(0.02)	(0.01)	(0.01)
Panel D: Invariant State Characteristics				
Elasticity of Housing Supply	1.26	0.27	0.04	0.01
	(0.56)	(0.29)	(0.13)	(0.14)
Local House Price Sensitivity	0.89	0.15	0.08	0.11
	(0.38)	(0.24)	(0.11)	(0.10)
Recourse Indicator	0.23	−0.08	0.00	0.03
	(0.42)	(0.20)	(0.06)	(0.07)
Predicted Employment Shock	−7.79	0.75	0.51**	0.64***
	(1.79)	(0.59)	(0.24)	(0.22)
Average Hand-to-Mouth	0.51	−0.03	−0.01	−0.01
	(0.08)	(0.04)	(0.01)	(0.02)

Note: This table reports results of OLS regressions of various outcomes on various cross-state bankruptcy protections, $\hat{B}_s$. Binned Homestead compares the top tercile to the bottom tercile. Both measures of Log Homestead and Simulated IV are normalized to be mean zero and standard deviation of one. Each row and column refer to a separate regression. The employment outcomes and credit outcomes are defined in Section I. We report the log employment scaled by working population multiplied by 100. Debt-to-Income Ratio is from Mian and Sufi (2014*b*), and measures the state-level total debt divided by income. Democratic Vote Share is the House of Representatives vote counts by state in 2006, excluding special elections. Maximum Unemployment Benefit is the maximum UI benefit in 2007 by state, as measured in Hsu, Matsa and Melzer (2018). Population <45 measures the population share below the age of 45 in 2007 and College Educated measure to the percent of population over 25 with a bachelor's degree or higher in 2007. Both measures come from the ACS. The Employment Sensitivity to Business Cycle is the time series correlation between each state's employment growth and national employment growth from 1961 to 2007. Annual Income per Capita measures the average state-level income per capita from the Current Population Survey in 2007. Log Average House Price is the log of the Corelogic House Price index. Homeownership Share is the share of individuals in an area that are homeowners as measured in the ACS. Elasticity of Housing Supply is from Saiz (2010). Local House Price Sensitivity, from Guren et al. (2021), measures systematic differences in exposure to regional house price cycles. The Recourse Indicator, from Ghent and Kudlyak (2011), measures whether state law allow lenders to seek recourse on defaulted underwater mortgages. Predicted Employment Shock is the predicted exposure to local unemployment shocks during the Great Recession from Yagan (2019). Each area's shift-share shock is equal to the predicted 2006-2010 percentage change in that area's employment based on local industry composition and leave-one-out nationwide changes in employment by three-digit NAICS industry categories. The Average Hand-to-Mouth variable follows Kaplan, Violante and Weidner (2014). We compute hand-to-mouth status at the individual level in the PSID, then aggregate to the state-level to obtain the average hand-to-mouth share. All regressions are run at the state-level using robust standard errors, and weighted by state population as of 2007.

Table 3: Reduced Form Effects of Bankruptcy Protections on Employment and Debt Write-Downs

		Effect of Bankruptcy Protections					
	Avg Change	Binned Homestead		Log(Homestead)		Simulated IV	
Panel A: 2008q1-2010q4	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Non-Tradable Emp.	-5.46	1.37^{***}	1.41^{***}	0.65^{***}	0.62^{***}	0.56^{**}	0.67^{***}
	(2.80)	(0.51)	(0.42)	(0.19)	(0.19)	(0.23)	(0.18)
Log Tradable + Other Emp.	-7.12	0.18	0.43	−0.11	−0.09	−0.15	0.02
	(2.93)	(1.14)	(0.75)	(0.52)	(0.39)	(0.52)	(0.39)
Log Total Emp.	-6.82	0.41	0.63	0.04	0.05	−0.01	0.15
	(2.66)	(0.97)	(0.60)	(0.44)	(0.31)	(0.45)	(0.31)
Per-Capita Debt Write-Downs	41.78	104.55	70.05^{***}	33.27	27.95^{***}	49.83	29.66^{***}
	(143.72)	(72.14)	(19.30)	(32.84)	(6.69)	(35.26)	(7.76)
Panel B: 2008q1-2015q4							
Log Non-Tradable Emp.	9.60	2.14^{**}	1.87^{**}	1.12^{***}	0.93^{**}	1.07^{***}	0.98^{***}
	(3.12)	(0.83)	(0.76)	(0.20)	(0.37)	(0.23)	(0.36)
Log Tradable + Other Emp.	7.51	−0.01	0.38	−0.15	−0.08	−0.26	−0.02
	(2.77)	(1.36)	(0.80)	(0.59)	(0.42)	(0.59)	(0.44)
Log Total Emp.	7.91	0.42	0.69	0.10	0.13	−0.00	0.19
	(2.43)	(1.09)	(0.57)	(0.46)	(0.28)	(0.47)	(0.30)
Per-Capita Debt Write-Downs	-223.60	67.87	44.51^{**}	22.82	20.31^{**}	32.74	21.02^{**}
	(60.58)	(55.42)	(17.50)	(24.96)	(7.75)	(25.73)	(8.13)
Number of Observations		2,950	2,950	2,950	2,950	2,950	2,950
County F.E.		Y	Y	Y	Y	Y	Y
Year-Quarter F.E.		Y	Y	Y	Y	Y	Y
Controls $\times$ YQ F.E.		N	Y	N	Y	N	Y

Note: This table reports estimates of the reduced form effect of cross-state bankruptcy protections on log employment outcomes scaled by working-age population (age 18-65) and average per-capita debt write-downs. The first row of each panel reports results for log non-tradable employment, defined as the retail and restaurant sectors. The second row reports results for log tradable and other employment, where tradable employment is defined as industries with sufficiently large imports and exports from the United States, and other employment is defined as all other industries excluding the construction sector (Mian and Sufi, 2014*b*). The third row reports results for log total employment. The fourth row reports results for per-capita consumer debt charge-offs. Coefficients for employment are scaled by 100 to correspond to percentage point effects. Column 1 reports the cross-county average and standard deviation of the change in the dependent variable from 2007q4-2010q4 or 2007q4-2015q4. Columns 2-7 report the coefficients of the reduced form effect of bankruptcy protections in pairs of columns using the different measures of bankruptcy protection discussed in Section I.C, pooled over the periods 2008q1-2010q4 in Panel A and 2008q1-2015q4 in Panel B. All estimates should be interpreted as the relative effect compared to 2007q4. First, in columns 2 and 3, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy home-stead exemptions. Second, in columns 4 and 5, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). Third, in columns 6 and 7, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Both the log homestead exemptions and simulated bankruptcy protection are normalized to be mean zero and standard deviation of one. The first column for each bankruptcy protection measure includes state and year-quarter fixed effects, and the second column adds the average state Saiz supply elasticity, the share of the population who own a home in 2007q1, the predicted employment shock measure, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For parts of the state without the Saiz supply elasticity measure, the measure is set to zero, and a control for the share of a state missing supply elasticity is interacted with year-quarter fixed effects. All regressions are weighted by state population as of 2007 and cluster the standard errors at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table 4: Relative Multiplier ($\mathcal{M}_{rel}$) Estimates

	Binned Homestead		Log(Homestead)		Simulated IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Tradable	2.15	4.73	4.87	5.48	2.80	5.58
	(2.66)	(1.84)	(5.83)	(1.76)	(2.92)	(1.86)
	[0.17]	[0.03]	[0.03]	[0.02]	[0.08]	[0.01]
Tradable + Other	1.11	0.02	-0.82	-0.79	-0.73	0.17
	(2.28)	(2.88)	(3.23)	(3.32)	(2.18)	(3.32)
	[0.70]	[0.30]	[0.83]	[0.81]	[0.78]	[0.96]
Total	1.30	0.97	0.29	0.46	-0.04	1.26
	(2.27)	(2.39)	(3.56)	(2.81)	(2.19)	(2.82)
	[0.71]	[0.26]	[0.93]	[0.85]	[0.99]	[0.59]
State F.E.	Y	Y	Y	Y	Y	Y
Year-Quarter F.E.	Y	Y	Y	Y	Y	Y
Controls $\times$ YQ F.E.	N	Y	N	Y	N	Y

Note: This table reports estimates of the effect of adjusted debt write-downs scaled by PCE on log employment outcomes scaled by working-age population (age 18-65) over the periods 2008q1-2010q4. Columns 1-6 report the coefficients of instrumental variables regression of log employment outcomes regressed on scaled debt write-downs in pairs of columns, instrumented using the different measures of bankruptcy protection discussed in Section I.C, and corresponding to the estimates from columns 2-7 in Table 3. First, in columns 1 and 2, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy homestead exemptions. Second, in columns 3 and 4, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). Third, in columns 5 and 6, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Standard errors are clustered at the state level. Weak-IV-robust p-values are reported in square brackets, testing whether the elasticity is equal to zero. See the text for additional details on the specification.

Table 5: Model Parameters and Baseline Calibration

	Parameter (Quarterly Frequency)	Value	Target
φ^B	Fraction of borrowers in each region	0.5	Share with credit card balance
β^S	Saver discount factor	0.983	7% annual interest rate
β^B	Borrower discount factor	0.96	15% annual credit card rate
$\bar{b}/C$	Debt limit (% of annual PCE)	16.67%	Avg Debt/PCE=8.33%
f	Incidence of debt relief	0.5	Equal incidence across regions
ξ	Consumption-labor complementarity	0	Separable preferences
σ^{-1}	Elasticity of intertemporal substitution	1	Standard value
ψ^{-1}	Frisch elasticity of labor supply	1	Standard value
η	Elasticity of subst. between T and NT	2	Standard value
ϵ	Elasticity of subst. within T and NT	10	Standard value
$1-\alpha$	Non-tradable share	0.236	Data (Mian and Sufi 2014*b*)
γ	Exponent on labor in production	0.66	Data (NIPA)
θ	Fraction of firms with fixed price	0.76	Data (Nakamura and Steinsson 2008)
Υ	Elasticity of markup to relative price	0	No Kimball real rigidity

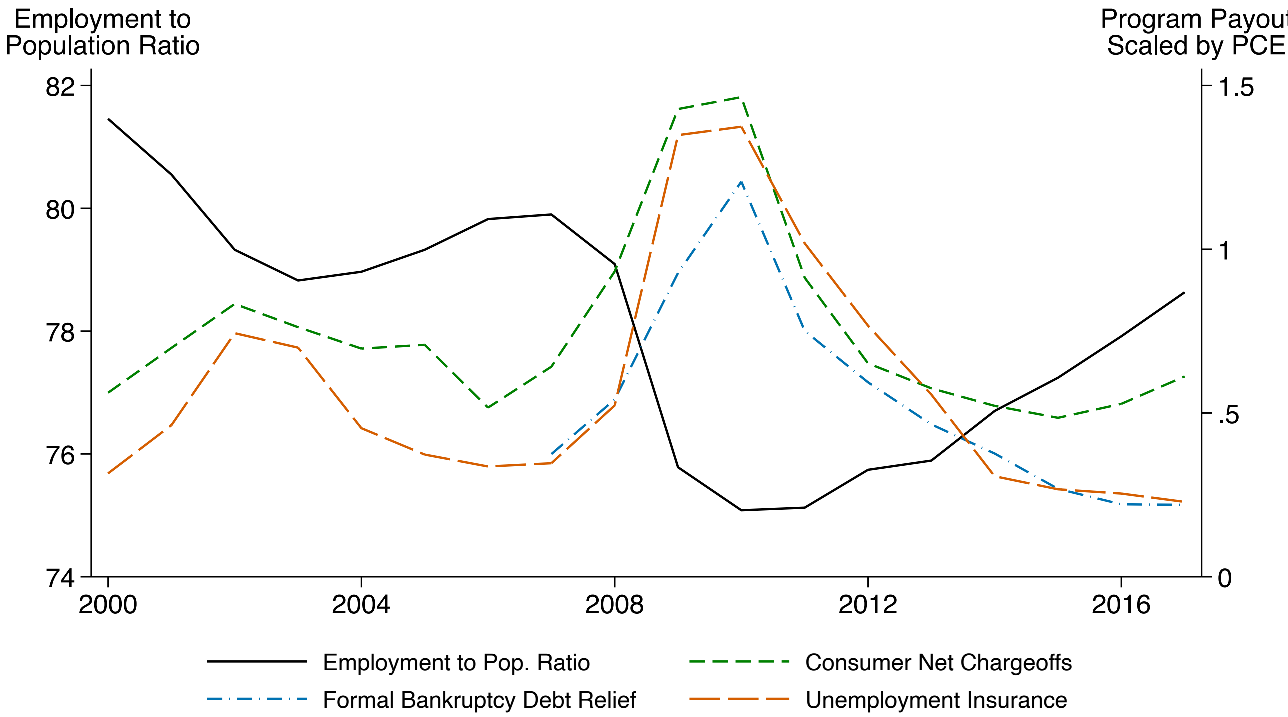
Note: This table displays baseline parameter calibration values for the model in Section IV. See the text for additional details on the definition of each parameter and target values.

Table 6: Calibrated Debt Relief Multipliers

	Home ($\mathcal{M}_H$)	Foreign ($\mathcal{M}_F$)	Relative ($\mathcal{M}_{rel}$)	Aggregate ($\mathcal{M}_{agg}$)
	(1)	(2)	(3)	(4)
Panel A: Baseline Calibration ($\theta = 0.76$, ZLB)				
Non-Tradable	1.55	0.34	1.21	1.89
Tradable	0.88	1.01	-0.14	1.89
Total	1.03	0.85	0.18	1.89
Panel B: Flexible Prices ($\theta = 0$)				
Non-Tradable	0.09	-0.10	0.19	-0.01
Tradable	-0.33	0.32	-0.64	-0.01
Total	-0.23	0.22	-0.45	-0.01
Panel C: Taylor Rule ($\theta = 0.76$, $\phi_\pi = 0.8$)				
Non-Tradable	1.46	0.25	1.21	1.71
Tradable	0.79	0.92	-0.14	1.71
Total	0.95	0.77	0.18	1.71

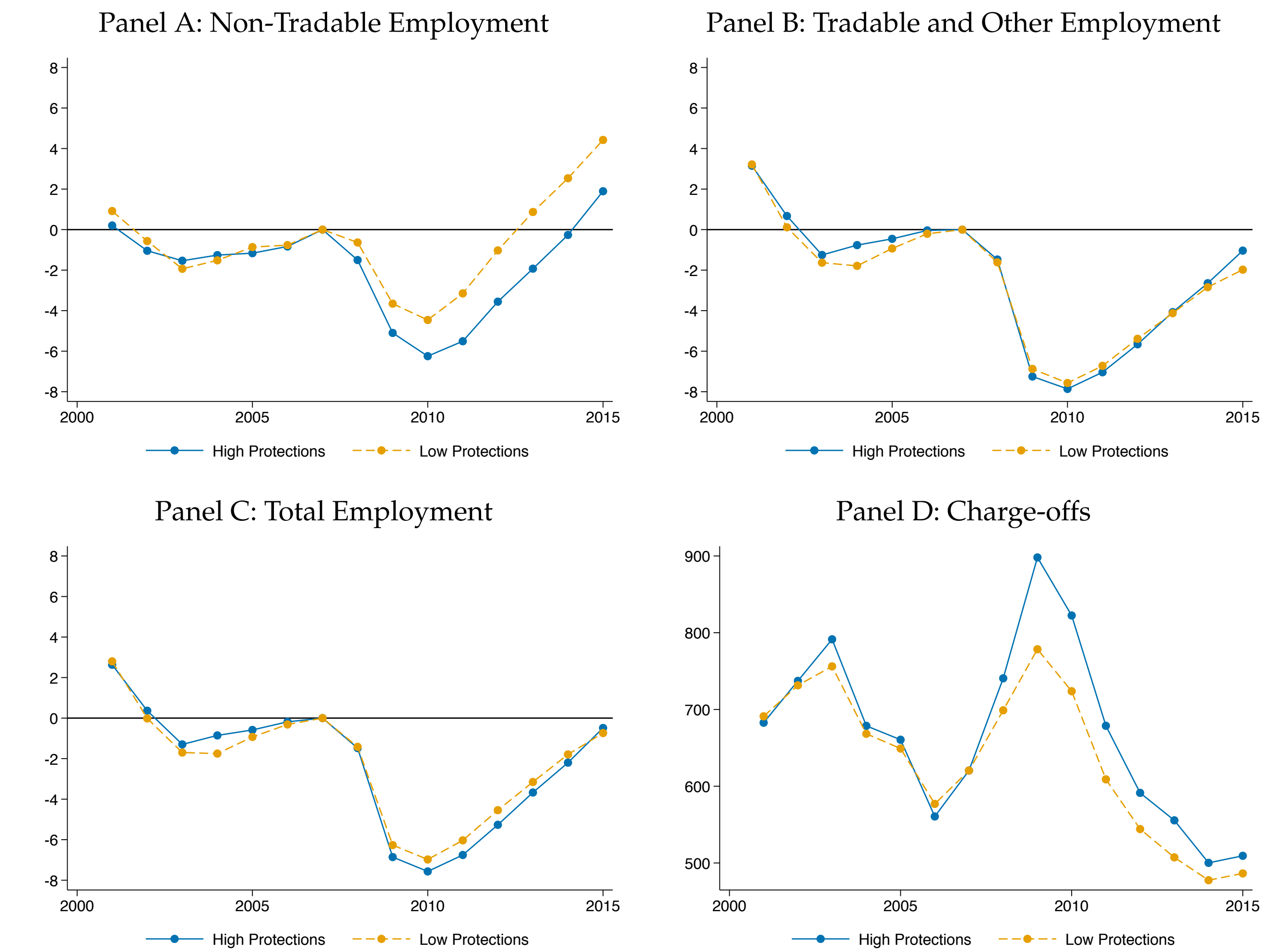
Note: This table reports the elasticities from the model under different calibrations discussed in Section IV. The baseline calibration parameters are given in Table 5. See the text for additional details.

Figure 1: Employment and Debt Relief during the Great Recession



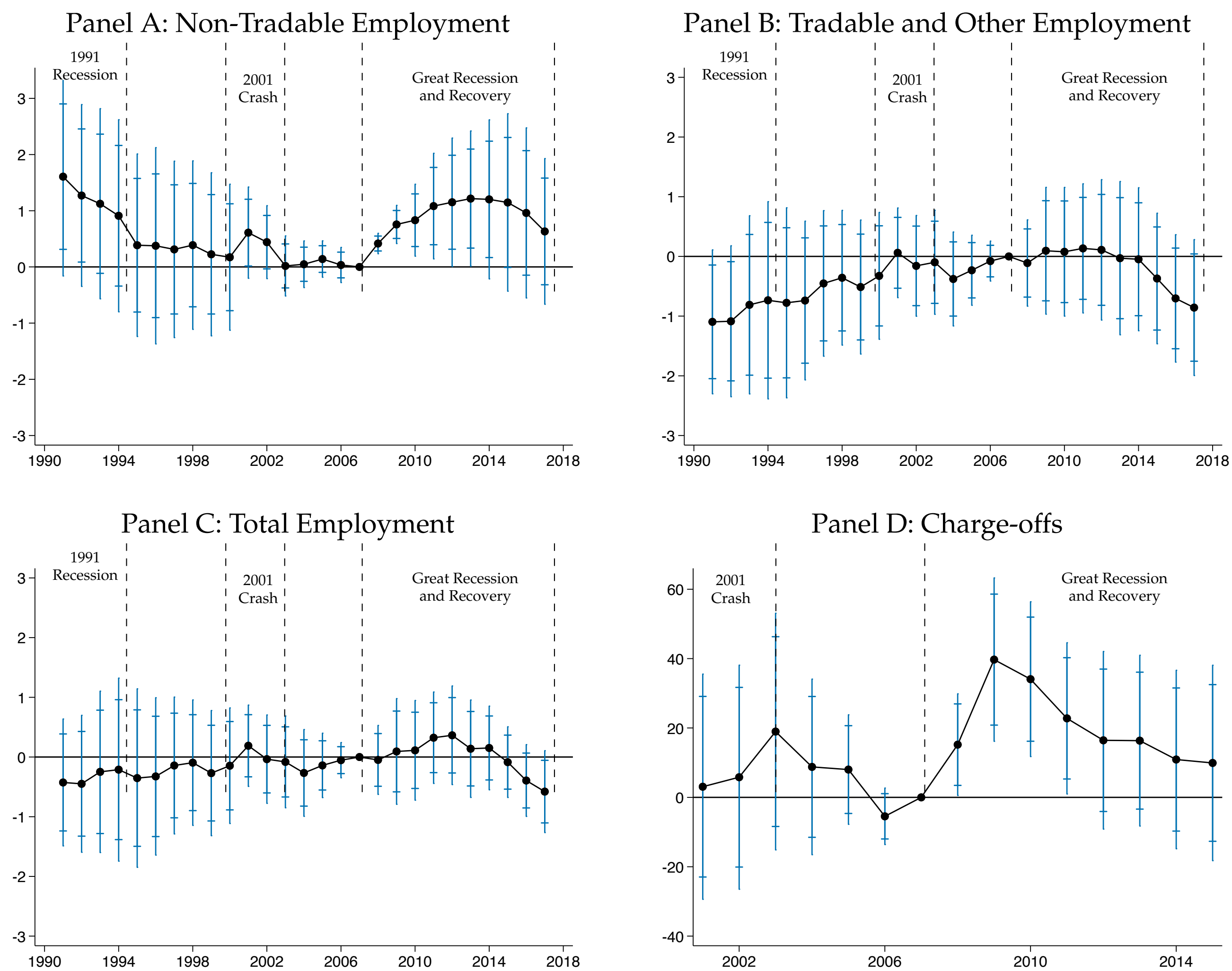
Note: This figure plots the employment-to-population ratio over time on the left y-axis, and payout measures as a percentage of PCE for four different debt relief programs on the right y-axis. Employment to population ratio is the 12-month average employment-to-population ratio for the working age population of individuals aged 25-54, as measured by the Current Population Survey in December of each year. Consumer net charge-offs is net consumer charge-offs on non-housing debt, as measured by the product of net charge-off rates from commercial banks on all non-real estate consumer loans (Board of Governors, 2018*a*) and consumer loans liabilities measured using the flow of funds (Board of Governors, 2018*b*). Formal bankruptcy debt relief is the annual level of unsecured debt scheduled for discharge for Chapter 7 bankruptcy filers reported in the BAPCPA reports (U.S. Courts, 2017). We report the face value of both net charge-offs and the amount of unsecured credit discharged in Chapter 7. Unemployment insurance is the annual program outlays from unemployment insurance reported by the Department of Labor (Department of Labor, 2018). We thank Kurt Mitman for pointing out the time series relationship with UI payouts.

Figure 2: Comparison of Employment and Charge-offs across High and Low Bankruptcy Protection States



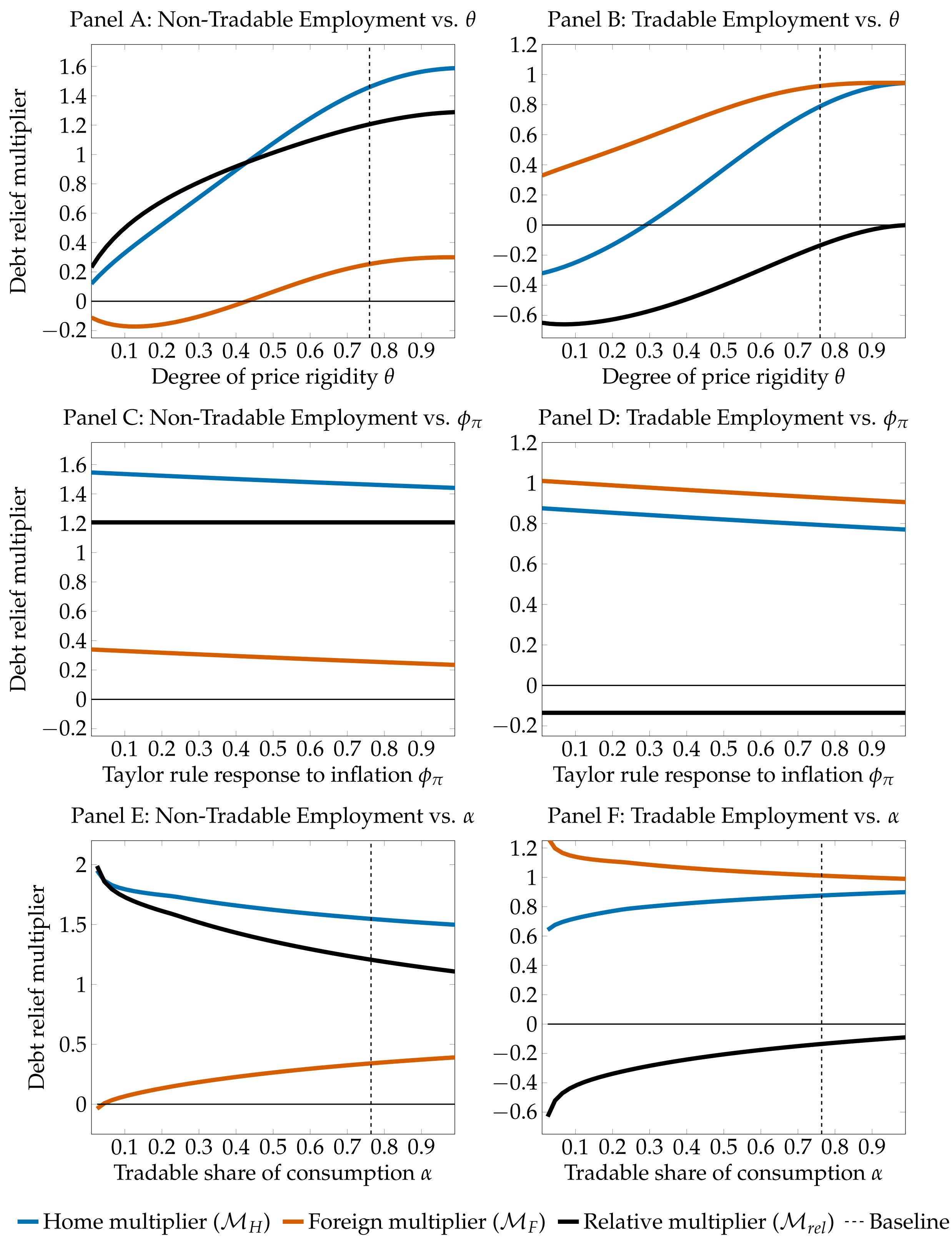
Note: This figure plots binned average employment outcomes split by cross-state bankruptcy protections. The solid line plots the average outcome for states in the top tercile of bankruptcy protections. The dashed line plots the average outcome for states in the bottom tercile of bankruptcy protections. Panel A reports results for log non-tradable employment, defined as the retail and restaurant sectors. Panel B reports results for log tradable and other employment, where tradable employment is defined as industries with sufficiently large imports and exports from the United States, and other employment is defined as all other industries excluding the construction sector Mian and Sufi (2014*b*). Panel C reports results for log total employment. Panel D reports results for per-capita debt write-downs. For Panels A, B and C, outcomes are normalized equal 100 in 2007 by dividing each group's outcomes by the outcome value in 2007, and multiplying by 100. All figures are weighted by state population as of 2007. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure 3: Reduced Form Effects of Bankruptcy Protections on Employment



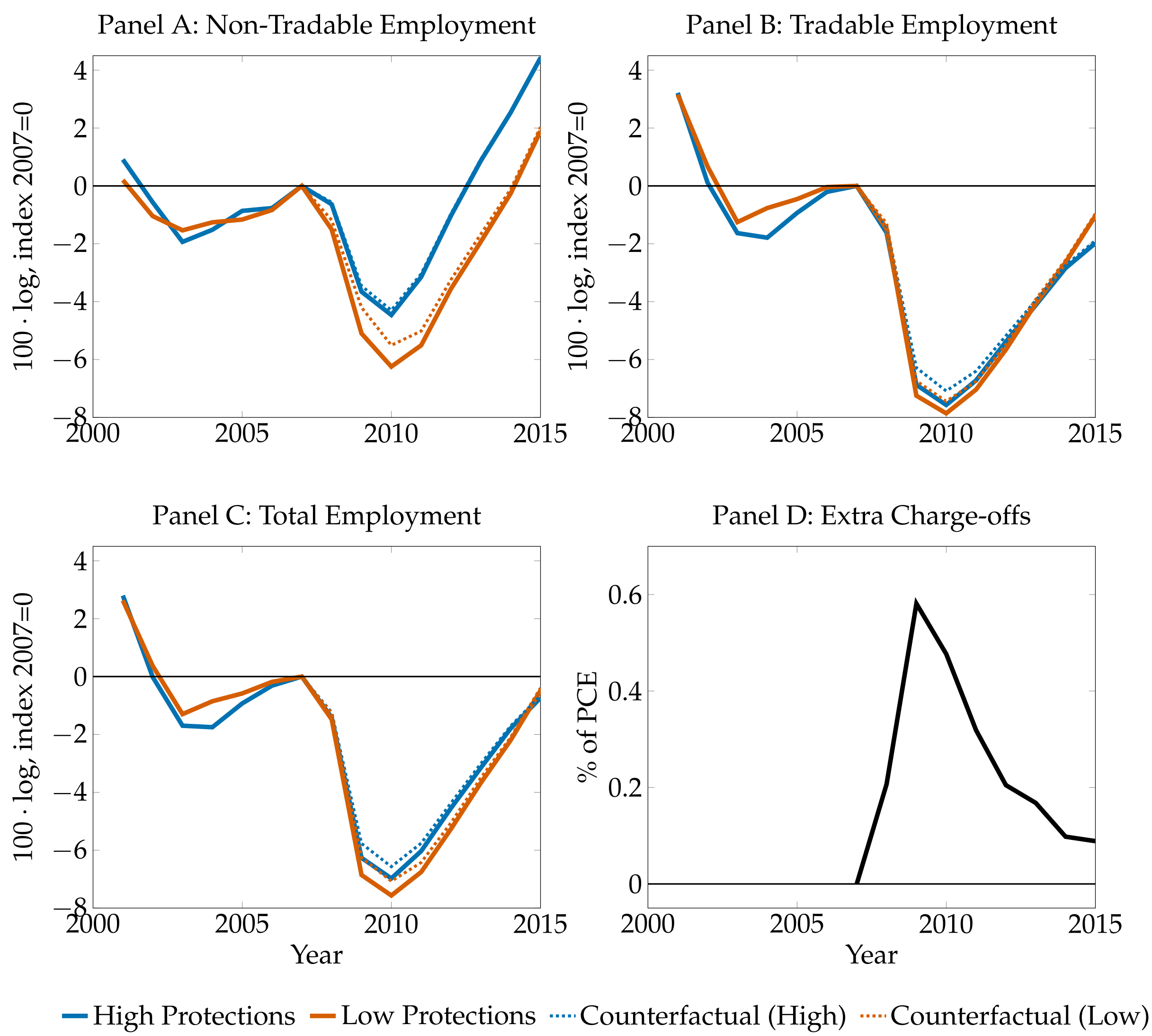
Note: This figure plots reduced form estimates of the effect of the simulated bankruptcy protection measure on employment outcomes. Panel A reports results for log non-tradable employment, defined as the retail and restaurant sectors. Panel B reports results for log tradable and other employment, where tradable employment is defined as industries with sufficiently large imports and exports from the United States, and other employment is defined as all other industries excluding the construction sector Mian and Sufi (2014*b*). Panel C reports results for log total employment. Panel D reports results for consumer debt charge-offs. We report the coefficients from a panel regression of each log employment measure scaled by working-age population on the negative of log financial cost of filing for bankruptcy protection interacted with year fixed effects. All specifications are weighted by state population as of 2007 and include state and year-quarter fixed effects, as well as the average state Saiz supply elasticity, the share of the population who own a home in 2007q1, the predicted employment shock measure, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For parts of the state without the Saiz supply elasticity measure, the measure is set to zero, and a control for the share of a state missing supply elasticity is interacted with year-quarter fixed effects. The estimated effect is normalized to zero in 2007, meaning all estimates are relative to 2007. The dashed lines are 95 percent confidence intervals from standard errors clustered at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure 4: Debt Relief Multiplier as a Function of Parameters θ, ϕ_π, and α



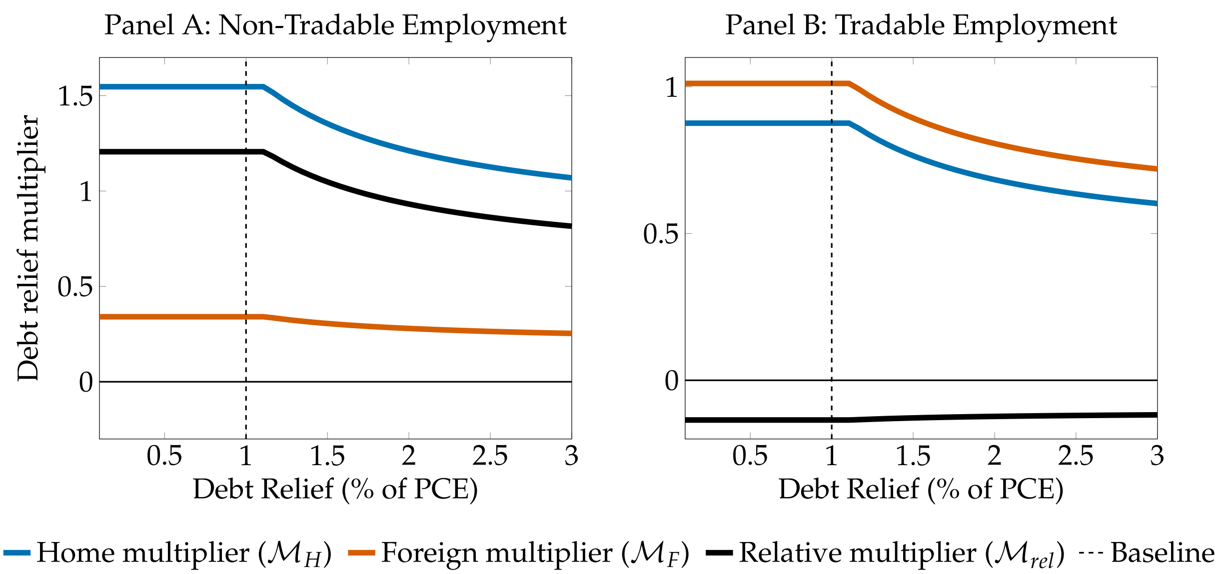
Note: This figure plots the home, foreign and relative debt relief multipliers in our model for non-tradable and tradable employment as a function of the degree of price rigidity θ, the Taylor rule response to inflation ϕ_π, and the degree of openness α. The blue line is the response of log employment in the home region ("high-exemption") to a reduction in home borrowers' debts of 1 percent of PCE. The red line is the response of log employment in the foreign region ("low-exemption") to the same shock. The black line is the difference between the responses in the home and the foreign region ("relative multiplier"), which corresponds to the debt relief multiplier identified by our difference-in-differences research design.

Figure 5: Main Counterfactual



Note: This figure plots...

Figure 6: Debt Relief Multiplier as a Function of the Size of Debt Relief



Note: This figure plots the debt relief multiplier in our model for non-tradable and tradable employment in high- and low-exemption regions as a function of the size of the debt relief shock. The blue line is the debt relief multiplier in the home region ("high-exemption"), the red line is the debt relief multiplier in the foreign region ("low-exemption"), and the black line represents the difference between the multipliers in the home and the foreign regions. The effect of debt relief on employment is linear initially, but later declines with the size of the debt relief due to borrowers becoming less constrained and spreading their spending out over additional periods.

Appendix A. Additional Discussion of Bankruptcy Protections

We calculate our summary bankruptcy protection measure in three steps. First, we calculate the financial cost of filing Chapter 7 bankruptcy, B_i^s, for individual i in state s using individual-level data on both asset values and unsecured debts. Following Fay, Hurst and White (2002) and Mahoney (2015), we define the financial cost of bankruptcy for individual i in state s as the value of i's assets that can be seized under Chapter 7 bankruptcy in state s minus the value of any debts that can be discharged under Chapter 7 and plus a common filing fee:

$$B_i^s = \text{Seizable Assets}_{is} - \text{Dischargeable Debt}_i + \text{Filing Cost}$$

where an individual's seizable assets Seizable Assets$_{is}$ is the sum of individual i's seizable home equity, vehicle equity, retirement savings, financial assets, and other assets:

$$\begin{aligned}\text{Seizable Assets}_{is} &= \max(\text{Home Equity}_i - \text{Homestead Exemption}_s, 0)\\ &+ \max(\text{Auto Equity}_i - \text{Auto Exemption}_s, 0)\\ &+ \max(\text{Retirement Assets}_i - \text{Retirement Exemption}_s, 0)\\ &+ \max(\text{Financial Assets}_i - \text{Financial Assets Exemption}_s, 0)\\ &+ \max(\text{Other Assets}_i - \text{Wildcard Exemption}_s, 0),\end{aligned}$$

We measure dischargeable debt using the level of unsecured debt from credit cards in the CCP credit bureau data described above; home equity using mortgage balances from the CCP credit bureau data and zip code-level housing values from Zillow; and vehicle equity, retirement assets, and other financial assets using the 2005 and 2007 PSID Wealth Files datasets, excluding households with at least one individual who is enrolled in public insurance or is over the age of 65 in the survey year. We use state bankruptcy exemption laws from 2007 for all asset categories to remove any endogenous changes to the exemptions made during the financial crisis.[26]

Second, we calculate the average cost of filing for bankruptcy in state s, $\bar{B}_s$, using the household characteristics of individuals in all other states $-s$:

$$\bar{B}_s = |I_{-s}|^{-1} \sum_{j \in I_{-s}} B_j^s$$

where I_{-s} is the entire sample of individuals in all states excluding state s, and B_j^s is the financial cost of bankruptcy measure for individual j if they were subject to the laws of state s. By using household characteristics of individuals in all other states, $-s$, we make sure that the variation in $\bar{B}_s$ is only driven by the differences in exemption laws across states, and therefore remove any potential effects of the bankruptcy exemptions on household characteristics such as asset holdings or income.

Finally, we rescale our bankruptcy protection measure to have a mean of zero and standard deviation of one for simplicity in all our regression specifications. We also multiply our rescaled measure, $\hat{B}_s$, by negative one so that we can interpret positive numbers as a higher level of bankruptcy protection:

$$\hat{B}_s = -\frac{\bar{B}_s - \mathbb{E}_s[\bar{B}_s]}{\sqrt{\text{Var}_s(\bar{B}_s)}}$$

Bankruptcy Protection Variation. Appendix Figure A2 plots the financial cost of bankruptcy by state $\bar{B}_s$, or our bankruptcy protection measure before we multiply by negative one and rescale to have a mean of zero and a standard deviation of one. There is considerable geographic variation in

[26] In robustness checks below, we show that we obtain similar estimates when using state exemption laws in 1991, the earliest year for which information on exemption laws is available for all 50 states, and well before either the financial crisis or the pre-crisis increase in house prices and employment.

the average financial cost of bankruptcy across states, with a population-weighted standard deviation of \$60,000 on a base of \$254,000. The state with the lowest financial cost of bankruptcy is Kansas, at \$154,000, and the highest is Virginia, at \$337,000. The difference between the 10th and 90th percentile of the state-level distribution is similarly large, at \$163,000. Moreover, high- and low-cost regions are not exclusively located in any area of the United States.

Appendix Figure A3 plots the financial cost of bankruptcy in each state against the relative generosity of the homestead, vehicle, savings, and wildcard exemptions in each state. We also plot the line of best fit weighted by state population. The variation in the financial cost of bankruptcy across states is almost entirely explained by the state homestead exemptions laws, with a correlation of -0.94 between the overall financial cost of bankruptcy, and homestead exemption ranks. In contrast, the correlation between the financial cost of bankruptcy and vehicle exemptions is only -0.10, while the correlations with savings and wildcard exemptions are only -0.07 and 0.04, respectively.

We observe a similar pattern in Appendix Table A1, which lists the financial cost of bankruptcy separately for seizable home equity and other seizable financial assets for each state. The variation in the financial cost of bankruptcy for home equity is approximately ten times larger than the variation in the financial costs for any other asset, with a population-weighted standard deviation of \$ 58,999 on a base of \$97,388 for seizable home equity, but only \$6,473 on a base of \$159,841 for other seizable financial assets. The difference between the 10th and 90th percentile of the state-level distribution is similarly large for seizable home equity at \$160,172, compared to only \$11,149 for other seizable financial assets.[27]

Appendix B. Estimation of $\mathcal{M}_{rel}$ from the data

We now use our difference-in-differences approach to calculate the relative debt relief multipliers $\mathcal{M}_{rel}$ for both non-tradable and tradable employment. As defined in equation (4), the relative debt relief multipliers are the ratios of our difference-in-differences estimates for each employment sector and debt write-downs, scaled by consumption.[28] In order to account for uncertainty in these estimates, we formally estimate the multiplier using two stage least squares (2SLS) with the relative effect of bankruptcy protections during the Great Recession as our excluded instrument, debt write-downs as our endogeneous variable, and employment as our outcome.

There are two potential problems with this 2SLS approach to construct the debt relief multiplier. The first is that it assumes that *all* of the debt forgiveness created by the consumer bankruptcy system is captured by our debt write-down measure. Yet, there are many important types of debt forgiveness that are not included in our debt write-down measure, including medical debt charged off by hospitals (e.g., Mahoney 2015), the debt on second homes with negative equity charged off by mortgage lenders (e.g., Ganong and Noel 2020), and the debt charged off by most payday lenders (e.g., Dobbie and Skiba 2013). We therefore inflate our difference-in-differences estimate for debt write-downs to account for the incomplete nature of our write-down measure.[29] The second potential problem with the 2SLS approach is that this assumes that the effect of bankruptcy protections acts solely through the channel of debt relief, and not through some alternative channel. Our debt relief multiplier will therefore be biased if, for example, bankruptcy protections have an additional independent effect on employment

[27]Retirement savings and financial assets (e.g., stock holdings) make up the vast majority of the other financial assets category. The median level of vehicle equity, for example, is less than \$10,000 in most states.

[28]In the model below, we assume that debt write-downs are a one-time transfer to borrowers. In practice, however, the economic benefits of debt write-downs may occur over multiple time periods. We therefore calculate the multipliers over a three-year time horizon, rather than a shorter time horizon such as only one year.

[29]We calculate the fraction of forgiven debt included in our debt write-down measure using a sample of Chapter 7 bankruptcy filings, where we find that our write-down measure captures approximately 77.5 percent of the debt forgiven in the typical Chapter 7 filing. We estimate the fraction of forgiven debt included in our debt write-down using the random sample of Chapter 7 bankruptcy filings described in Dobbie, Goldsmith-Pinkham and Yang (2017). To do this, we first identify the composition of different debts that were discharged in Chapter 7. We then calculate the share of that total debt that is accounted for by credit card and auto debt: 77.5 percent.

(e.g., Fan and White 2003; Dobbie and Song 2015; Donaldson, Piacentino and Thakor 2019). We are unable to estimate the magnitude of these independent effects using our data, and, as a result, unable to account for this issue when calculating our debt relief multiplier.

Appendix C. Additional Results

Appendix Table A1: Bankruptcy Exemptions by State

	Chapter 7 Bankruptcy Exemptions in 2007				Financial Cost of Bankruptcy	
State	Homestead	Vehicle	Savings	Wildcard	Home Equity	Other Assets
Alaska	67,500	3,750	1,750	0	113,742	159,444
Alabama	10,000	0	0	3,000	160,173	162,586
Arkansas	Unlimited	1,200	0	500	0	182,904
Arizona	150,000	5,000	150	0	66,855	152,309
California	75,000	2,550	4,050	0	86,855	152,515
Colorado	90,000	3,000	0	0	99,353	160,923
Connecticut	150,000	1,500	0	1,000	66,356	161,572
Delaware	50,000	0	0	500	126,679	163,453
Florida	Unlimited	1,000	0	1,000	0	158,622
Georgia	20,000	3,500	0	600	153,329	160,939
Hawaii	30,000	2,575	0	0	133,183	161,519
Iowa	Unlimited	7,000	0	100	0	157,972
Idaho	50,000	3,000	0	800	126,750	160,401
Illinois	30,000	2,400	0	4,000	143,346	160,583
Indiana	30,000	0	300	8,000	145,012	161,844
Kansas	Unlimited	20,000	0	0	0	153,636
Kentucky	10,000	2,500	0	1,000	160,056	160,170
Louisiana	25,000	0	0	0	147,256	164,026
Massachusetts	500,000	700	1,200	0	4,323	160,463
Maryland	0	0	0	11,000	166,668	159,593
Maine	70,000	5,000	0	400	111,979	159,852
Michigan	31,900	2,950	0	0	137,110	161,934
Minnesota	200,000	3,800	0	0	48,263	159,709
Missouri	15,000	3,000	0	1,250	156,118	160,760
Mississippi	75,000	0	0	10,000	108,814	162,834
Montana	200,000	2,500	0	0	47,869	161,567
North Carolina	37,000	3,500	0	500	139,270	161,600
North Dakota	80,000	1,200	0	5,000	105,199	170,776
Nebraska	12,500	0	0	2,500	157,593	162,728
New Hampshire	200,000	4,000	0	1,000	47,904	160,220
New Jersey	0	0	0	1,000	131,025	162,298
New Mexico	60,000	4,000	0	500	119,369	160,310
Nevada	350,000	15,000	0	0	16,683	155,892
New York	100,000	2,400	2,500	0	88,370	156,339
Ohio	10,000	1,000	400	400	163,915	162,217
Oklahoma	Unlimited	3,000	0	0	0	161,414
Oregon	39,600	2,150	7,500	400	134,560	182,680
Pennsylvania	0	0	0	300	136,446	163,664
Rhode Island	300,000	10,000	0	0	24,032	156,892
South Carolina	10,000	1,200	1,000	0	160,519	162,895
South Dakota	Unlimited	0	0	6,000	0	165,850
Tennessee	7,500	0	0	4,000	164,097	162,346
Texas	Unlimited	0	0	30,000	0	153,806
Utah	40,000	2,500	0	0	134,532	161,927
Virginia	10,000	2,000	0	0	158,357	183,046
Vermont	150,000	2,500	700	400	66,713	160,856
Washington	40,000	2,500	0	2,000	132,896	151,869
Wisconsin	40,000	1,200	1,000	0	135,426	162,018
West Virginia	50,000	2,400	0	800	126,737	161,485
Wyoming	20,000	2,400	0	0	150,847	161,709

Note: This table reports Chapter 7 asset exemptions and average financial cost of bankruptcy by state in 2007. The homestead and vehicle exemptions refer to the maximum equity in a residential home or vehicle exempt in bankruptcy. Savings refers to the amount of cash and bank account assets exempt, while wildcard exemptions can generally be applied to any other assets, or increase the exemption amount of other categories. The last two columns report the average financial cost of bankruptcy by state based on state-specific exemptions and a national sample of households' wealth. The first of the last two columns reports the average home equity at risk in each state, using home equity amounts from a national sample of 2007 home equity estimates from Equifax and Zillow. The second of the last two columns reports the average amount of assets at risk for all other assets except housing in each state, using 2005 and 2007 PSID surveys excluding the state at hand. Both columns exclude individuals in the random sample from that state.

Appendix Table A2: Correlates with State Bankruptcy Protection Measure in 2001

		Bankruptcy Protections		
	Average Value	Binned Homestead	Log Homestead	Simulated IV
Panel A: Employment Outcomes	(1)	(2)	(3)	(4)
Log Non-Tradable Emp. (p.p.)	228.65	1.96	0.90	0.97
	(9.27)	(2.51)	(1.09)	(1.11)
Log Tradable + Other Emp. (p.p.)	388.27	0.40	0.13	0.02
	(7.70)	(3.28)	(1.37)	(1.42)
Log Total Emp. (p.p.)	406.80	0.71	0.28	0.20
	(6.92)	(2.85)	(1.21)	(1.25)
Panel B: Credit Outcomes				
Bankruptcy Rate (p.p.)	0.29	-0.05^{*}	-0.02^{*}	-0.02^{**}
	(0.08)	(0.03)	(0.01)	(0.01)
Foreclosure Rate (p.p.)	0.92	0.03	−0.02	−0.01
	(0.31)	(0.08)	(0.03)	(0.03)
Credit Card Limits ($000)	15.40	0.64	0.03	0.13
	(2.16)	(0.78)	(0.33)	(0.34)
Credit Card Debt ($000)	3.82	0.11	0.01	0.03
	(0.39)	(0.14)	(0.06)	(0.05)
Mortgage Debt ($000)	33.66	−1.20	−1.82	−1.41
	(12.14)	(3.24)	(1.34)	(1.42)
Debt-to-Income Ratio	1.24	−0.02	−0.02	−0.02
	(0.27)	(0.10)	(0.04)	(0.04)
Panel C: State Characteristics				
Democratic Vote Share (p.p.)	48.03	−0.12	−0.87	−0.52
	(7.21)	(3.11)	(1.29)	(1.41)
Max. Unemp. Benefit ($)	8.23	1.07	0.50	0.55
	(2.52)	(1.14)	(0.45)	(0.53)
Population <45 (p.p.)	65.59	−0.70	−0.19	−0.22
	(2.88)	(1.95)	(0.80)	(0.83)
College Educated (p.p.)	24.34	0.97	−0.06	0.17
	(3.85)	(1.43)	(0.69)	(0.69)
Employment Sensitivity to Business Cycle	1.04	−0.10	−0.05	−0.06
	(0.25)	(0.11)	(0.04)	(0.04)
Log Average House Price	11.88	0.00	−0.02	−0.01
	(0.29)	(0.09)	(0.04)	(0.04)
Homeownership Share	0.70	-0.03^{*}	-0.01^{***}	-0.01^{**}
	(0.05)	(0.02)	(0.01)	(0.01)
Panel D: Invariant State Characteristics				
Elasticity of Housing Supply	1.26	−0.08	0.01	−0.00
	(0.56)	(0.29)	(0.12)	(0.13)
Local House Price Sensitivity	0.89	0.25	0.08	0.09
	(0.38)	(0.22)	(0.10)	(0.10)
Recourse Indicator	0.23	0.15	0.03	0.03
	(0.42)	(0.12)	(0.03)	(0.04)
Predicted Employment Shock	−7.79	1.65^{***}	0.51^{*}	0.67^{***}
	(1.79)	(0.52)	(0.26)	(0.21)
Average Hand-to-Mouth	0.51	-0.07^{*}	−0.02	−0.02
	(0.08)	(0.04)	(0.01)	(0.02)

Note: This table reports results of OLS regressions of various outcomes on cross-state bankruptcy protections, $\hat{B}_s$. All county-level outcomes are aggregated to the state level using population weights. Each row and column refer to a separate regression. Outcomes in Panel A and B are described in the notes of Table 1 and Section I. The remaining variables are pulled from additional sources. Debt-to-Income Ratio is from Mian and Sufi (2014*b*), and measures the state-level total debt divided by income. Democratic Vote Share is the presidential vote counts in 2000, excluding special elections. Maximum Unemployment Benefit is the maximum UI benefit in 2001 by state, as measured in Hsu, Matsa and Melzer (2018). Population < 45 measures the population share below the age of 45 in 2001 and College Educated measure to the percent of population over 25 with a bachelor's degree or higher in 2001. Both measures come from the ACS. The Employment Sensitivity to Business Cycle is the coefficient relating national employment growth to state employment growth from 1961-2001. We calculate it using the time series correlation between each state's employment growth and national employment growth from 1961 to 2001. Annual Income per Capita measures the average state-level income per capita and is from the Current Population Survey in 2001. Log Average House Price is the log of the Corelogic House Price index. Homeownership Share is the share of individuals in an area that are measured as homeowners, using the Equifax data. We mark mortgage holders are homeowners. Elasticity of Housing Supply is time-invariant and comes from Saiz (2010). Local House Price Sensitivity comes from Guren et al. (2021) and measures systematic differences in exposure to regional house price cycles. The Recourse Indicator, from Ghent and Kudlyak (2011), refers to whether or not state law allows for lenders to seek recourse in the case of defaulted underwater mortgages and is treated as time invariant in the sample. Predicted Employment Shock is the predicted exposure to local unemployment shocks during the Great Recession from Yagan (2019). Each area's shift-share shock is equal to the predicted 2006-2010 percentage change in that area's employment based on a combination of local industry composition and leave-one-out nationwide changes in employment by three-digit NAICS industry categories. The Average Hand-to-Mouth variable follows Kaplan, Violante and Weidner (2014). We compute hand-to-mouth status at the individual level in the PSID, then aggregate by state to obtain the average hand to mouth share. All regressions are run at the state-level using robust standard errors, and weighted by state population as of 2001.

Appendix Table A3: Reduced Form Effects of Bankruptcy Protections on Additional Credit Outcomes

	Avg Change	Effect of Bankruptcy Protections					
		Binned Homestead		Log(Homestead)		Simulated IV	
Panel A: 2008q1-2010q4	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bankruptcy Rates	0.10	−0.01	−0.02	−0.01	−0.01	−0.00	−0.01
	(0.07)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Credit Card Limits	-3.05	−0.13	−0.15	−0.02	−0.03	−0.01	−0.04
	(0.72)	(0.09)	(0.11)	(0.05)	(0.04)	(0.05)	(0.04)
Credit Card Balance	-0.21	0.05	0.00	0.01	0.00	0.03	0.00
	(0.11)	(0.07)	(0.03)	(0.03)	(0.01)	(0.04)	(0.01)
Mortgage Balance	0.44	−0.37	−0.63	−0.24	−0.32	−0.10	−0.29
	(4.15)	(0.46)	(0.64)	(0.28)	(0.29)	(0.27)	(0.27)
Foreclosure Rates	0.92	0.61**	0.47***	0.20	0.19***	0.27*	0.20***
	(0.93)	(0.29)	(0.08)	(0.14)	(0.03)	(0.15)	(0.03)
Panel B: 2008q1-2015q4							
Bankruptcy Rates	-0.17	0.00	−0.01	−0.00	−0.00	0.00	−0.00
	(0.06)	(0.03)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)
Credit Card Limits	1.10	−0.13	−0.18	0.01	−0.04	0.02	−0.04
	(0.66)	(0.28)	(0.21)	(0.12)	(0.08)	(0.12)	(0.08)
Credit Card Balance	-0.41	−0.02	−0.05	−0.01	−0.02	−0.00	−0.02
	(0.30)	(0.05)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)
Mortgage Balance	-3.41	−1.74	−1.33	−0.62	−0.61	−0.80	−0.56
	(6.32)	(1.91)	(1.12)	(0.83)	(0.48)	(0.87)	(0.45)
Foreclosure Rates	-0.38	0.74*	0.57***	0.24	0.22***	0.32*	0.23***
	(0.46)	(0.40)	(0.12)	(0.18)	(0.03)	(0.19)	(0.04)
Number of Observations		3,000	3,000	3,000	3,000	3,000	3,000
County F.E.		Y	Y	Y	Y	Y	Y
Year-Quarter F.E.		Y	Y	Y	Y	Y	Y
Controls × YQ F.E.		N	Y	N	Y	N	Y

Note: This table reports estimates of the reduced form effect of cross-state bankruptcy protections on additional credit outcomes. The first row of each panel reports results for per capita bankruptcy filing rates in percentage points. The second row reports results for per capita credit card limits in thousands of dollars. The third row reports results for per capita credit card balances in thousands of dollars. The fourth row reports results for per capita mortgage balances in thousands of dollars. The fifth row reports per capita foreclosure rates in percentage points. Column 1 reports the cross-county average and standard deviation of the change in the dependent variable from 2007q4-2010q4 or 2007q4-2015q4. Columns 2-7 report the coefficients of the reduced form effect of bankruptcy protections in pairs of columns using the different measures of bankruptcy protection discussed in Section I.C, pooled over the periods 2008q1-2010q4 in Panel A and 2008q1-2015q4 in Panel B. All estimates should be interpreted as the relative effect compared to 2007q4. First, in columns 2 and 3, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy home-stead exemptions. Second, in columns 4 and 5, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). Third, in columns 6 and 7, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Both the log homestead exemptions and simulated bankruptcy protection are normalized to be mean zero and standard deviation of one. The first column for each bankruptcy protection measure includes state and year-quarter fixed effects, and the second column adds the average state Saiz supply elasticity, the share of the population who own a home in 2007q1, the predicted employment shock measure, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For parts of the state without the Saiz supply elasticity measure, the measure is set to zero, and a control for the share of a state missing supply elasticity is interacted with year-quarter fixed effects. All regressions are weighted by state population as of 2007 and cluster the standard errors at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Appendix Table A4: Reduced Form Effects of Bankruptcy Protections on Log House Prices

		Effect of Bankruptcy Protections					
	Avg Change	Binned Homestead		Log(Homestead)		Simulated IV	
Panel A: 2008q1-2010q4	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log House Price	-19.94	-7.27	-4.77^{***}	-1.78	-1.82^{***}	-3.04	-2.00^{***}
	(15.40)	(6.11)	(1.66)	(3.15)	(0.61)	(3.29)	(0.67)
Panel B: 2008q1-2015q4							
Log House Price	10.44	-4.61	-2.05	-0.57	-0.92	-1.64	-1.00
	(12.22)	(8.82)	(2.67)	(3.94)	(1.13)	(3.99)	(1.21)
Number of Observations		2,984	2,984	2,984	2,984	2,984	2,984
County F.E.		Y	Y	Y	Y	Y	Y
Year-Quarter F.E.		Y	Y	Y	Y	Y	Y
Controls $\times$ YQ F.E.		N	Y	N	Y	N	Y

Note: This table reports estimates of the reduced form effect of cross-state bankruptcy protections on house prices. Each panel reports results for log house prices in percentage points Column 1 reports the cross-county average and standard deviation of the change in the dependent variable from 2007q4-2010q4 or 2007q4-2015q4. Columns 2-7 report the coefficients of the reduced form effect of bankruptcy protections in pairs of columns using the different measures of bankruptcy protection discussed in Section I.C, pooled over the periods 2008q1-2010q4 in Panel A and 2008q1-2015q4 in Panel B. All estimates should be interpreted as the relative effect compared to 2007q4. First, in columns 2 and 3, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy home-stead exemptions. Second, in columns 4 and 5, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). Third, in columns 6 and 7, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Both the log homestead exemptions and simulated bankruptcy protection are normalized to be mean zero and standard deviation of one. The first column for each bankruptcy protection measure includes state and year-quarter fixed effects, and the second column adds the average state Saiz supply elasticity, the share of the population who own a home in 2007q1, the predicted employment shock measure, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For parts of the state without the Saiz supply elasticity measure, the measure is set to zero, and a control for the share of a state missing supply elasticity is interacted with year-quarter fixed effects. All regressions are weighted by state population as of 2007 and cluster the standard errors at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Appendix Table A5: Reduced Form Effects of 1991 Homestead Exemptions on Employment and Debt Write-Downs

		Effect of Bankruptcy Protections					
	Avg Change	Binned Homestead		Log(Homestead)		Simulated IV	
Panel A: 2008q1-2010q4	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Non-Tradable Emp.	-5.46	0.92	1.23^{***}	0.30	0.52^{**}	-	-
	(2.80)	(0.63)	(0.41)	(0.27)	(0.20)		
Log Tradable + Other Emp.	-7.12	−0.54	0.13	−0.30	−0.05	-	-
	(2.93)	(1.25)	(0.79)	(0.55)	(0.40)		
Log Total Emp.	-6.82	−0.25	0.36	−0.17	0.07	-	-
	(2.66)	(1.08)	(0.62)	(0.47)	(0.31)		
Per-Period Debt Write-Downs	41.78	113.35	63.74^{***}	42.57	25.13^{***}	-	-
	(143.72)	(79.20)	(21.78)	(36.27)	(7.66)		
Panel B: 2008q1-2015q4							
Log Non-Tradable Emp.	9.60	0.92	1.23^{***}	0.30	0.52^{**}	-	-
	(3.12)	(0.63)	(0.41)	(0.27)	(0.20)		
Log Tradable + Other Emp.	7.51	−0.54	0.13	−0.30	−0.05	-	-
	(2.77)	(1.25)	(0.79)	(0.55)	(0.40)		
Log Total Emp.	7.91	−0.25	0.36	−0.17	0.07	-	-
	(2.43)	(1.08)	(0.62)	(0.47)	(0.31)		
Per-Period Debt Write-Downs	-223.60	113.35	63.74^{***}	42.57	25.13^{***}	-	-
	(60.58)	(79.20)	(21.78)	(36.27)	(7.66)		
Number of Observations		2,950	2,950	2,950	2,950	-	-
County F.E.		Y	Y	Y	Y	Y	Y
Year-Quarter F.E.		Y	Y	Y	Y	Y	Y
Controls × YQ F.E.		N	Y	N	Y	N	Y

Note: This table reports estimates of the reduced form effect of historical bankruptcy protections measured in 1991 on log employment outcomes scaled by working-age population (age 18-65) and average per-capita debt write-downs. The first row of each panel reports results for log non-tradable employment, defined as the retail and restaurant sectors. The second row reports results for log tradable and other employment, where tradable employment is defined as industries with sufficiently large imports and exports from the United States, and other employment is defined as all other industries excluding the construction sector (Mian and Sufi, 2014*b*). The third row reports results for log total employment. The fourth row reports results for per-capita consumer debt charge-offs. Coefficients for employment are scaled by 100 to correspond to percentage point effects. Column 1 reports the cross-county average and standard deviation of the change in the dependent variable from 2007q4-2010q4 or 2007q4-2015q4. Columns 2-7 report the coefficients of the reduced form effect of bankruptcy protections in pairs of columns using the different measures of bankruptcy protection discussed in Section I.C, pooled over the periods 2008q1-2010q4 in Panel A and 2008q1-2015q4 in Panel B. All estimates should be interpreted as the relative effect compared to 2007q4. First, in columns 2 and 3, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy home-stead exemptions. Second, in columns 4 and 5, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). The log homestead exemptions are normalized to be mean zero and standard deviation of one. The first column for each bankruptcy protection measure includes state and year-quarter fixed effects, and the second column adds the average state Saiz supply elasticity, the share of the population who own a home in 2007q1, the predicted employment shock measure, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For parts of the state without the Saiz supply elasticity measure, the measure is set to zero, and a control for the share of a state missing supply elasticity is interacted with year-quarter fixed effects. All regressions are weighted by state population as of 2007 and cluster the standard errors at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Appendix Table A6: Reduced Form Effects of Bankruptcy Protections on Log In-Migration

		Effect of Bankruptcy Protections					
	Avg Change	Binned Homestead		Log(Homestead)		Simulated IV	
Panel A: 2008q1-2010q4	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log In-Migration	-0.11	0.04	0.04	0.01	0.01	0.01	0.02
	(0.09)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Panel B: 2008q1-2013q4							
Log In-Migration	-0.06	0.05	0.04	0.01	0.01	0.01	0.02
	(0.10)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Number of Observations		1,800	1,800	1,800	1,800	1,800	1,800
County F.E.		Y	Y	Y	Y	Y	Y
Year-Quarter F.E.		Y	Y	Y	Y	Y	Y
Controls $\times$ YQ F.E.		N	Y	N	Y	N	Y

Note: This table reports estimates of the reduced form effect of cross-state bankruptcy protections on thee log of the share of state population who moved from outside the state in the last year. Migration data comes from the American Community Survey (ACS). In-migration is defined as the share of current state residents who lived in a different state or country one year ago. We use data from 2004-2013 due to availability constraints. Column 1 reports the cross-county average and standard deviation of the change in the dependent variable from 2007q4-2010q4 or 2007q4-2015q4. Columns 2-7 report the coefficients of the reduced form effect of bankruptcy protections in pairs of columns using the different measures of bankruptcy protection discussed in Section I.C, pooled over the periods 2008q1-2010q4 in Panel A and 2008q1-2015q4 in Panel B. All estimates should be interpreted as the relative effect compared to 2007q4. First, in columns 2 and 3, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy home-stead exemptions. Second, in columns 4 and 5, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). Third, in columns 6 and 7, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Both the log homestead exemptions and simulated bankruptcy protection are normalized to be mean zero and standard deviation of one. The first column for each bankruptcy protection measure includes state and year-quarter fixed effects, and the second column adds the average state Saiz supply elasticity, the share of the population who own a home in 2007q1, the predicted employment shock measure, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For parts of the state without the Saiz supply elasticity measure, the measure is set to zero, and a control for the share of a state missing supply elasticity is interacted with year-quarter fixed effects. All regressions are weighted by state population as of 2007 and cluster the standard errors at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Appendix Table A7: Reduced Form Effects of Bankruptcy Protections on Inflation

	Avg Change	Effect of Bankruptcy Protections					
		Binned Homestead		Log(Homestead)		Simulated IV	
Panel A: 2008q1-2010q4	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-Tradables	8.36	−0.23	−0.19	−0.21	−0.19	−0.17	−0.10
	(1.26)	(0.35)	(0.42)	(0.21)	(0.22)	(0.18)	(0.16)
Tradables	3.61	−0.22	−0.28	−0.06	−0.09	−0.02	−0.09
	(1.69)	(0.37)	(0.47)	(0.14)	(0.16)	(0.15)	(0.18)
All Goods	6.73	−0.20	−0.20	−0.16	−0.16	−0.13	−0.10
	(1.14)	(0.30)	(0.36)	(0.15)	(0.16)	(0.13)	(0.13)
Panel B: 2008q1-2015q4							
Non-Tradables	14.82	−0.55	−0.42	−0.37	−0.32	−0.32	−0.23
	(2.77)	(0.56)	(0.60)	(0.24)	(0.25)	(0.21)	(0.21)
Tradables	3.80	−0.46	−0.53	−0.15	−0.16	−0.13	−0.14
	(1.76)	(0.67)	(0.76)	(0.22)	(0.26)	(0.24)	(0.29)
All Goods	10.97	−0.43	−0.37	−0.27	−0.24	−0.24	−0.18
	(2.05)	(0.50)	(0.56)	(0.18)	(0.20)	(0.18)	(0.19)
Number of Observations		1,980	1,980	1,980	1,980	1,980	1,980
County F.E.		Y	Y	Y	Y	Y	Y
Year-Quarter F.E.		Y	Y	Y	Y	Y	Y
Controls × YQ F.E.		N	Y	N	Y	N	Y

Note: This table reports estimates of the reduced form effect of bankruptcy protections on log inflation indices. The first row of each panel reports results for non-tradable goods, following the definition in Hazell et al. (2021). The second row reports results for tradable, defined as the remaining goods that are not non-tradable. The third row is reports results for all good. Coefficients are scaled by 100 to correspond to percentage point effects. These measures are only available for 33 states. Column 1 reports the cross-county average and standard deviation of the change in the dependent variable from 2007q4-2010q4 or 2007q4-2015q4. Columns 2-7 report the coefficients of the reduced form effect of bankruptcy protections in pairs of columns using the different measures of bankruptcy protection discussed in Section I.C, pooled over the periods 2008q1-2010q4 in Panel A and 2008q1-2015q4 in Panel B. All estimates should be interpreted as the relative effect compared to 2007q4. First, in columns 2 and 3, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy home-stead exemptions. Second, in columns 4 and 5, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). The log homestead exemptions are normalized to be mean zero and standard deviation of one. The first column for each bankruptcy protection measure includes state and year-quarter fixed effects, and the second column adds the average state Saiz supply elasticity, the share of the population who own a home in 2007q1, the predicted employment shock measure, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For parts of the state without the Saiz supply elasticity measure, the measure is set to zero, and a control for the share of a state missing supply elasticity is interacted with year-quarter fixed effects. All regressions are weighted by state population as of 2007 and cluster the standard errors at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Appendix Table A8: Relative Multiplier $\mathcal{M}_{rel}$ estimates over full period

	Binned Homestead		Log(Homestead)		Simulated IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Tradable	7.30	8.37	12.04	11.33	7.96	11.46
	(6.30)	(3.55)	(13.00)	(2.68)	(6.63)	(3.05)
	[0.09]	[0.11]	[0.01]	[0.06]	[0.02]	[0.05]
Tradable + Other	1.80	-3.51	-1.53	-0.85	-1.96	-0.10
	(4.53)	(3.09)	(5.04)	(4.93)	(3.22)	(5.15)
	[0.55]	[0.04]	[0.81]	[0.86]	[0.66]	[0.98]
Total	2.81	-1.11	1.13	1.66	-0.03	2.28
	(4.59)	(2.42)	(6.26)	(4.04)	(3.59)	(4.27)
	[0.52]	[0.04]	[0.82]	[0.60]	[0.99]	[0.47]
County F.E.	Y	Y	Y	Y	Y	Y
Year-Quarter F.E.	Y	Y	Y	Y	Y	Y
Controls $\times$ YQ F.E.	N	Y	N	Y	N	Y

Note: This table reports estimates of the effect of adjusted debt write-downs scaled by PCE on log employment outcomes scaled by working-age population (age 18-65) over the periods 2008q1-2015q4. Columns 1-6 report the coefficients of instrumental variables regression of log employment outcomes regressed on scaled debt write-downs in pairs of columns, instrumented using the different measures of bankruptcy protection discussed in Section I.C, and corresponding to the estimates from columns 2-7 in Table 3, Panel B. First, in columns 1 and 2, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy homestead exemptions. Second, in columns 3 and 4, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). Third, in columns 5 and 6, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Standard errors are clustered at the state level. Weak-IV-robust p-values are reported in square brackets, testing whether the elasticity is equal to zero. See the text for additional details on the specification.

Appendix Table A9: Elasticities with 1991 Homestead Exemptions

	Binned Homestead		Log(Homestead)		Simulated IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Tradable	0.05	5.08	1.77	5.13	-	-
	(1.70)	(2.03)	(2.81)	(1.60)		
	[0.46]	[0.06]	[0.35]	[0.05]		
Tradable + Other	-0.33	-1.84	-1.72	-0.48	-	-
	(1.27)	(2.42)	(2.06)	(3.81)		
	[0.91]	[0.14]	[0.59]	[0.90]		
Total	-0.26	-0.46	-0.99	0.70	-	-
	(1.28)	(2.01)	(2.03)	(3.18)		
	[0.97]	[0.22]	[0.71]	[0.80]		
County F.E.	Y	Y	Y	Y	Y	Y
Year-Quarter F.E.	Y	Y	Y	Y	Y	Y
Controls $\times$ YQ F.E.	N	Y	N	Y	N	Y

Note: This table reports estimates of the effect of adjusted debt write-downs scaled by PCE on log employment outcomes scaled by working-age population (age 18-65) over the periods 2008q1-2010q4. Columns 1-6 report the coefficients of instrumental variables regression of log employment outcomes regressed on scaled debt write-downs in pairs of columns, instrumented using the different historical measures of bankruptcy protection discussed in Section I.C, and corresponding to the estimates from columns 2-5 in Table A5. First, in columns 1 and 2, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy homestead exemptions. Second, in columns 3 and 4, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). Third, in columns 5 and 6, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Standard errors are clustered at the state level. Weak-IV-robust p-values are reported in square brackets, testing whether the elasticity is equal to zero. See the text for additional details on the specification.

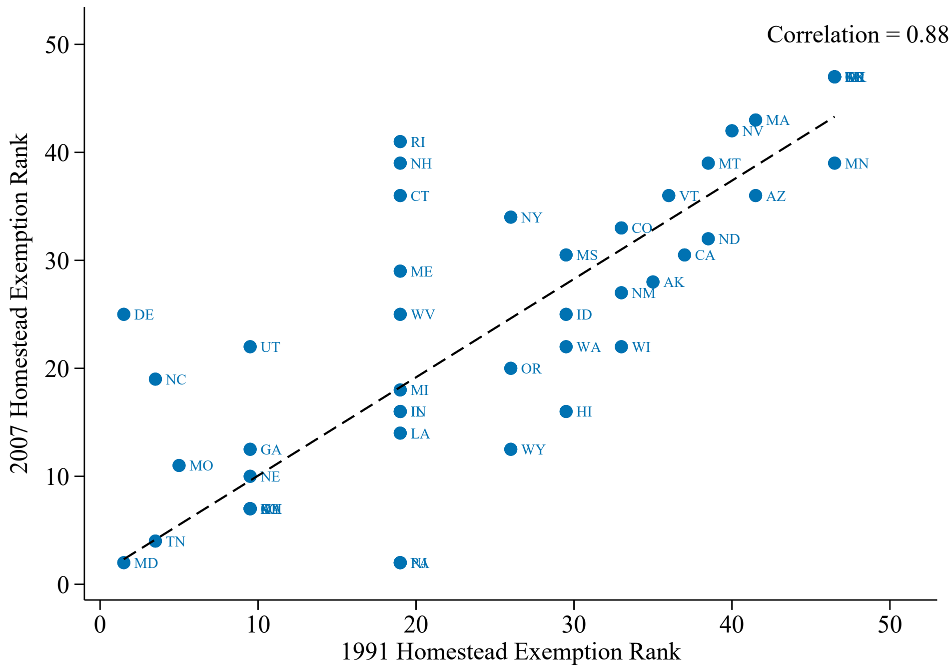
Appendix Table A10: Elasticities over full period with 1991 Homestead Exemptions

	Binned Homestead		Log(Homestead)		Simulated IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Tradable	3.16	9.79	4.81	10.00	-	-
	(4.60)	(3.77)	(5.83)	(2.42)		
	[0.28]	[0.14]	[0.26]	[0.13]		
Tradable + Other	-0.93	-4.04	-3.20	-0.41	-	-
	(2.07)	(2.87)	(3.09)	(5.83)		
	[0.85]	[0.06]	[0.57]	[0.94]		
Total	-0.20	-1.24	-1.47	1.88	-	-
	(2.32)	(2.28)	(3.27)	(4.70)		
	[0.96]	[0.11]	[0.74]	[0.60]		
County F.E.	Y	Y	Y	Y	Y	Y
Year-Quarter F.E.	Y	Y	Y	Y	Y	Y
Controls × YQ F.E.	N	Y	N	Y	N	Y

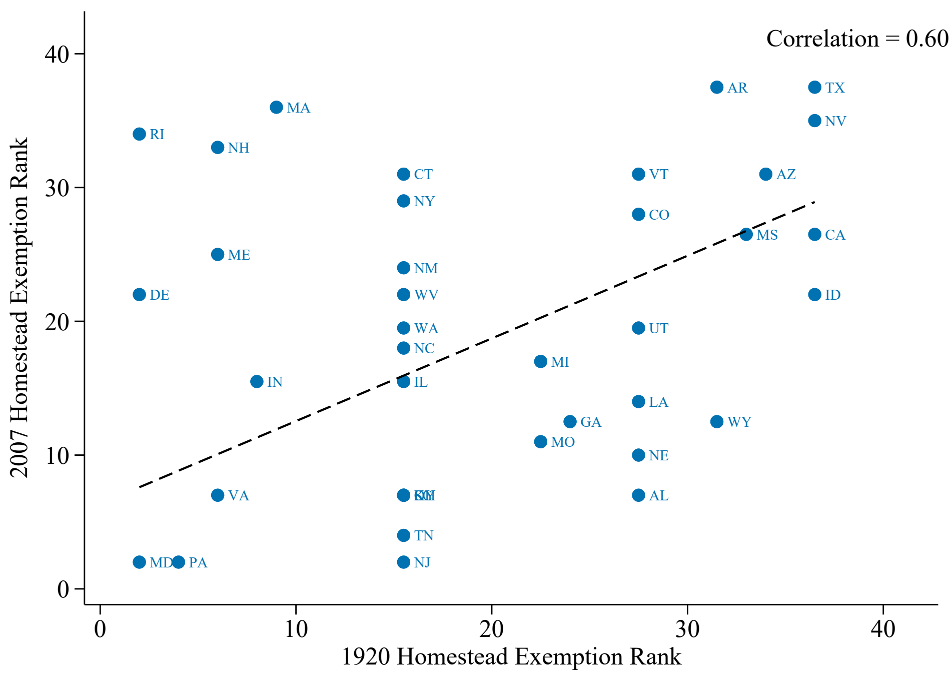
Note: This table reports estimates of the effect of adjusted debt write-downs scaled by PCE on log employment outcomes scaled by working-age population (age 18-65) over the periods 2008q1-2015q4. Columns 1-6 report the coefficients of instrumental variables regression of log employment outcomes regressed on scaled debt write-downs in pairs of columns, instrumented using the different historical measures of bankruptcy protection discussed in Section I.C, and corresponding to the estimates from columns 2-5 in Table A5. First, in columns 1 and 2, we split our sample into terciles and compare bins of the states with the highest and lowest bankruptcy homestead exemptions. Second, in columns 3 and 4, we use the continuous measure of the log(bankruptcy homestead exemptions$_s$+ 2000). Third, in columns 5 and 6, we use the simulated bankruptcy protection measure that parameterizes the full range of bankruptcy protections. Standard errors are clustered at the state level. Weak-IV-robust p-values are reported in square brackets, testing whether the elasticity is equal to zero. See the text for additional details on the specification.

Appendix Figure A1: Stability of Exemptions Over Time

Panel A: 2007 Homestead Exemption vs. 1991 Homestead Exemption

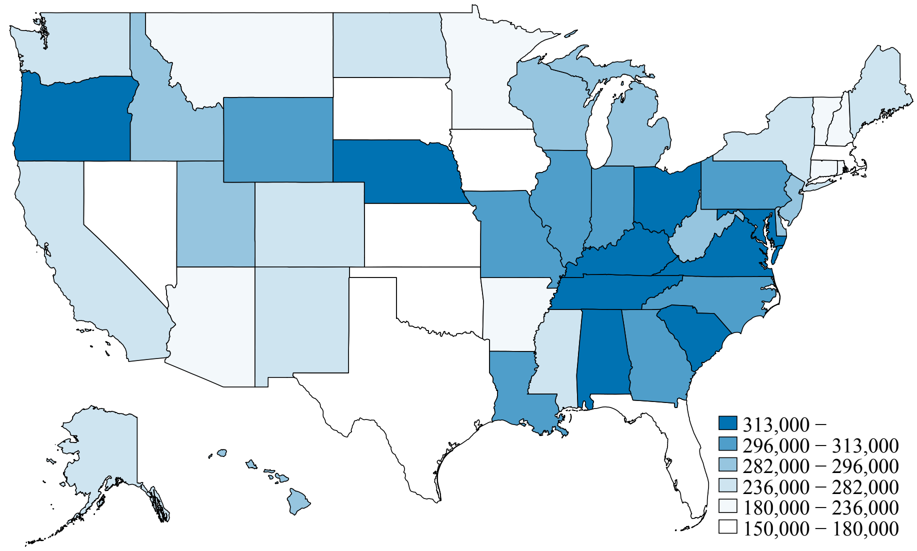


Panel B: 2007 Homestead Exemption vs. 1920 Homestead Exemption



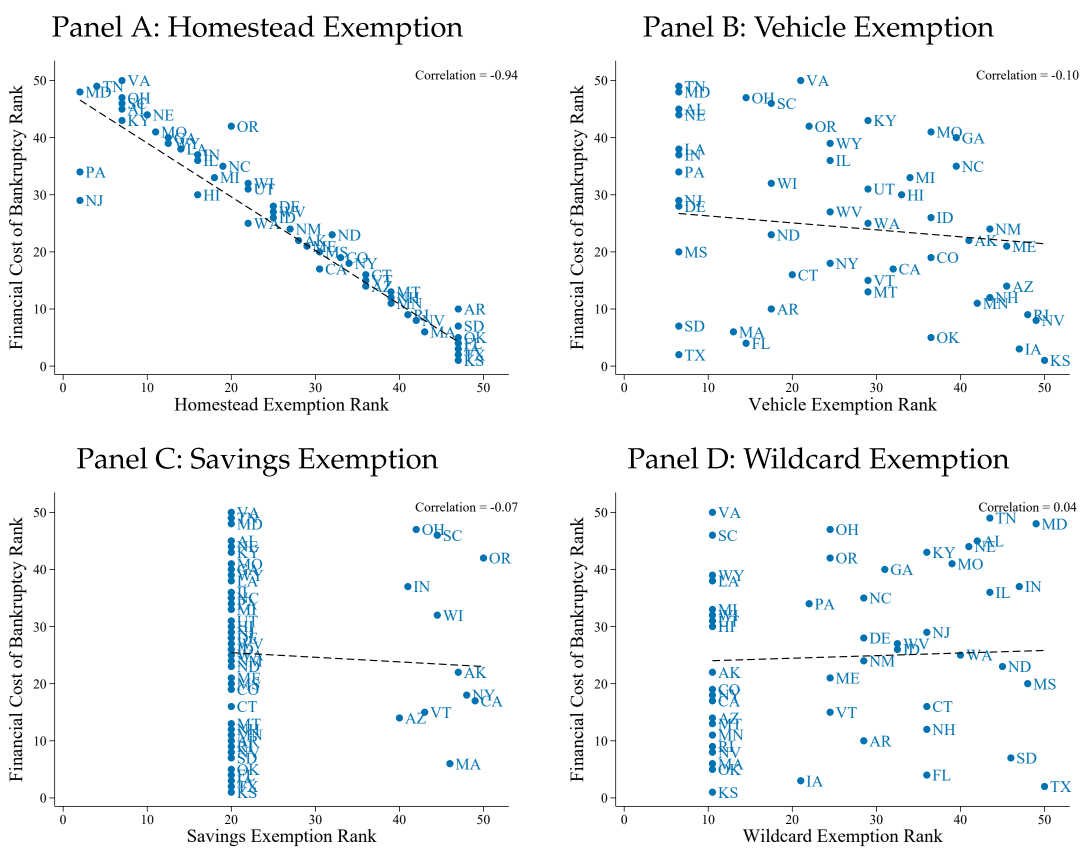
Note: This figure shows the stability of the Chapter 7 bankruptcy exemptions over time. Panel A plots 2007 homestead exemption ranks against 1991 homestead exemption ranks for all 50 states. Panel B plots 2007 homestead exemption ranks against 1920 homestead exemption ranks, excluding states that did not exist in 1920 and states with acre-based homestead exemptions. We use state populations in 2007 as weights and assign a value of $1,000,000 to states with unlimited 2007 homestead exemptions.

Appendix Figure A2: Geography of the Financial Cost of Bankruptcy Filing



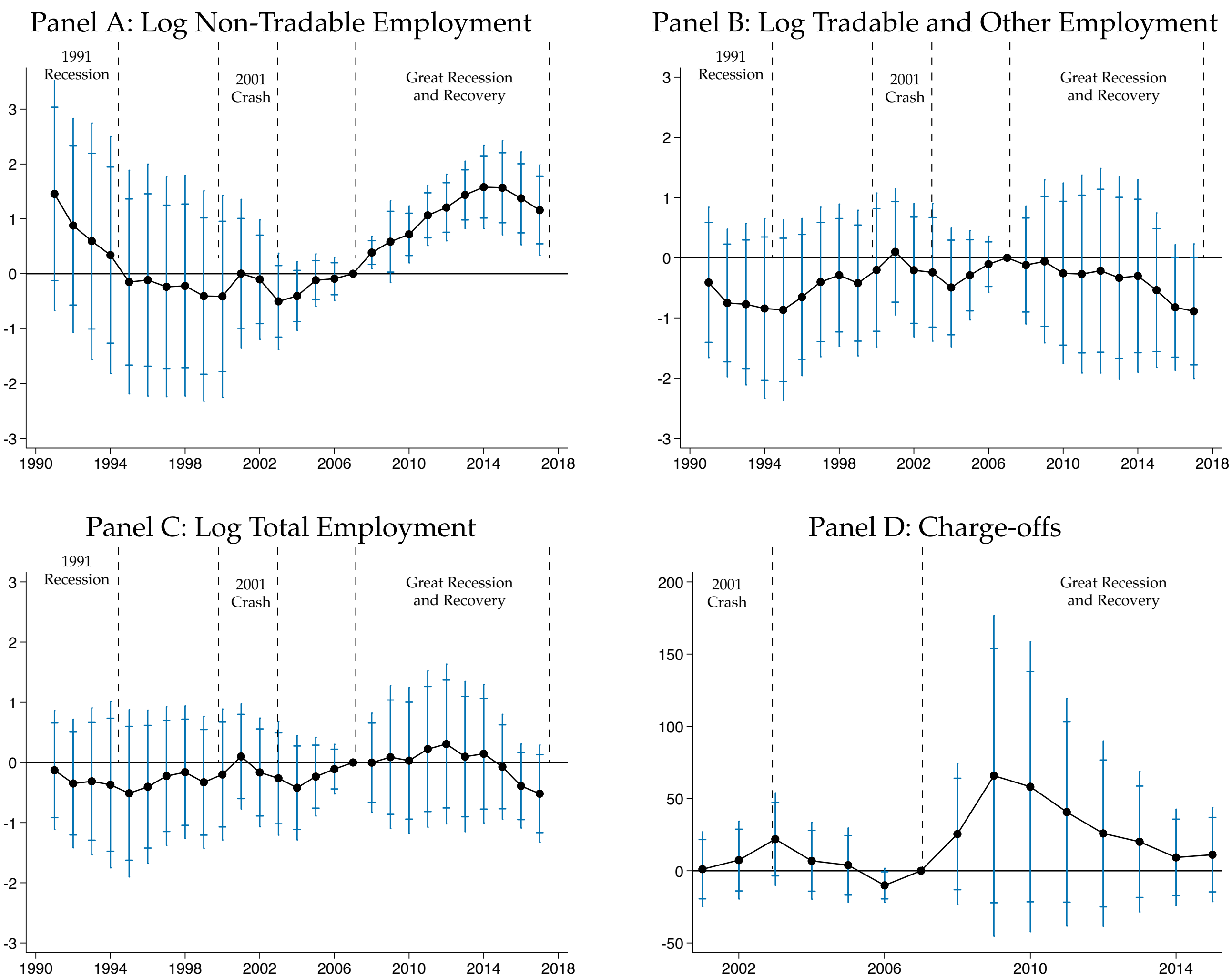
Note: This map shows the geographic distribution of financial cost of filing for Chapter 7 bankruptcy in 2007. See the text for additional details on the construction of this measure.

Appendix Figure A3: Correlation of Exemptions and Financial Cost of Bankruptcy



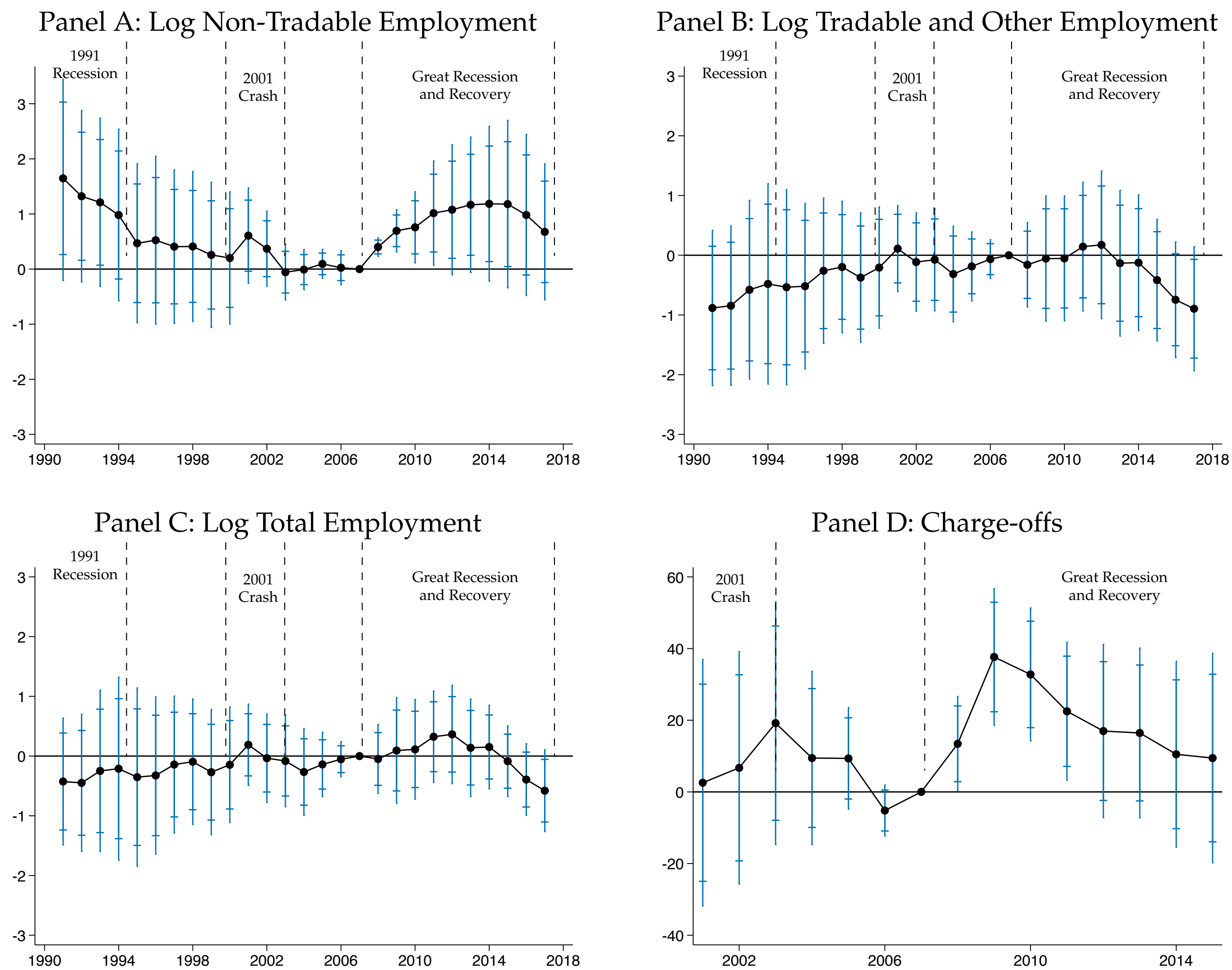
Note: This figure shows the correlation of the financial cost of bankruptcy measure described in the text and the different Chapter 7 bankruptcy exemptions. Each panel plots the state-specific rank for the financial cost of bankruptcy against the state-specific rank for each Chapter 7 asset exemption category. We also plot the line of best fit, weighted by states' population. See the text for additional details.

Appendix Figure A4: Reduced Form Effects of Bankruptcy Protections on Employment without Controls



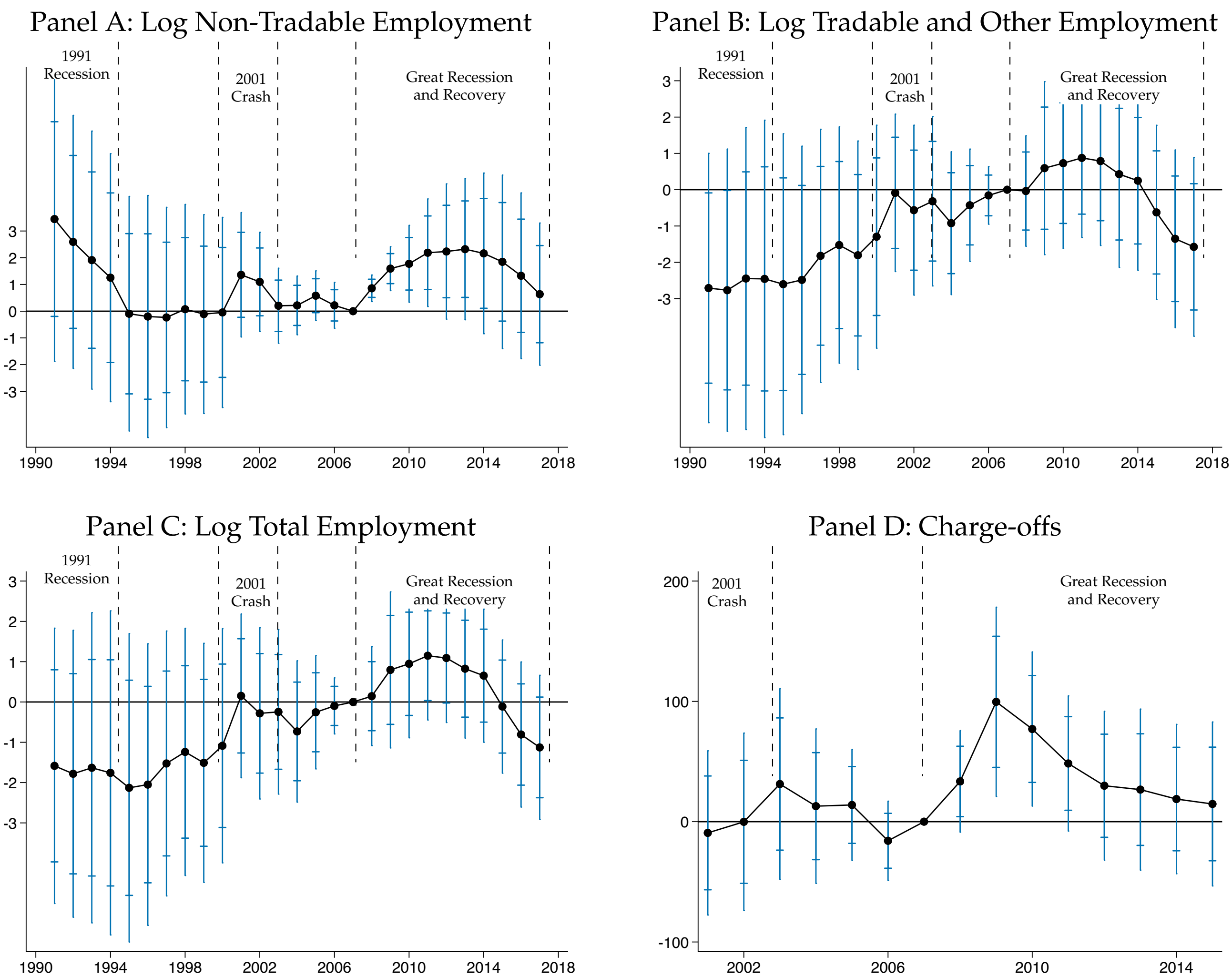
Note: This figure plots reduced form estimates of the effect of cross-state bankruptcy protections on employment outcomes. Panel A reports results for log non-tradable employment, defined as the retail and restaurant sectors. Panel B reports results for log tradable and other employment, where tradable employment is defined as industries with sufficiently large imports and exports from the United States, and other employment is defined as all other industries excluding the construction sector Mian and Sufi (2014*b*). Panel C reports results for log total employment. Panel D reports results for consumer debt charge-offs. We report the coefficients from a panel regression of each log employment measure scaled by working-age population on the log financial cost of filing for bankruptcy protection interacted with year fixed effects. All specifications are weighted by county population as of 2007 and include county and year-quarter fixed effects. The estimated effect is normalized to zero in 2007, meaning all estimates are relative to 2007. The dashed lines are 95 percent confidence intervals from standard errors clustered at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Appendix Figure A5: Reduced Form Effects of Log(Homestead) on Employment



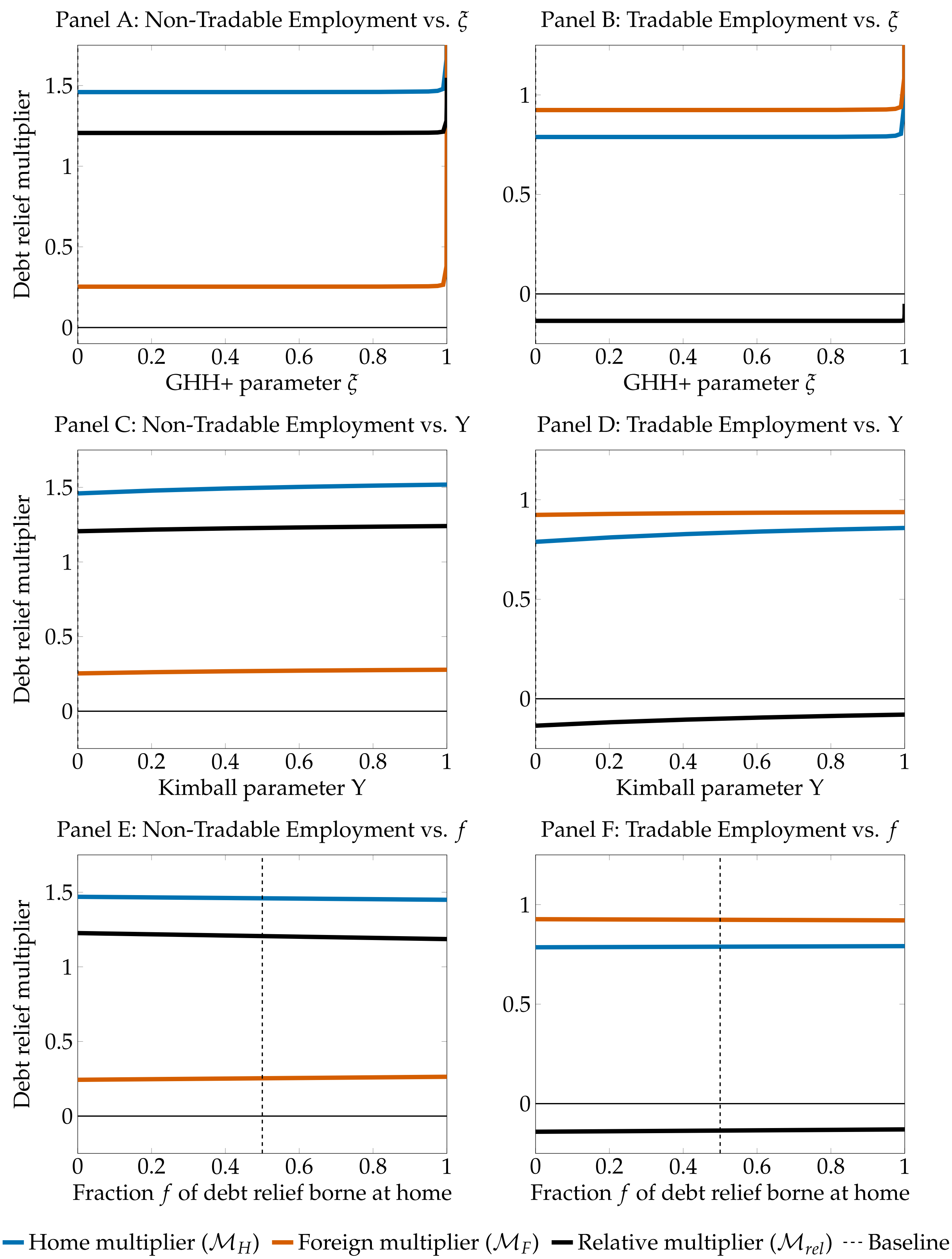
Note: This figure plots reduced form estimates of the effect of cross-state bankruptcy protections on employment outcomes. Panel A reports results for log non-tradable employment, defined as the retail and restaurant sectors. Panel B reports results for log tradable and other employment, where tradable employment is defined as industries with sufficiently large imports and exports from the United States, and other employment is defined as all other industries excluding the construction sector Mian and Sufi (2014*b*). Panel C reports results for log total employment. Panel D reports results for consumer debt charge-offs. We report the coefficients from a panel regression of each log employment measure scaled by working-age population on the log financial cost of filing for bankruptcy protection interacted with year fixed effects. All specifications are weighted by county population as of 2007 and include county and year-quarter fixed effects, as well as the Saiz supply elasticity, the share of the population who own a home in 2007q1 for each county, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For counties without the Saiz supply elasticity measure, the measure is set to zero, and an additional dummy for missing supply elasticity is interacted with year-quarter fixed effects. The estimated effect is normalized to zero in 2007, meaning all estimates are relative to 2007. The dashed lines are 95 percent confidence intervals from standard errors clustered at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Appendix Figure A6: Reduced Form Effects of Binned Homestead on Employment



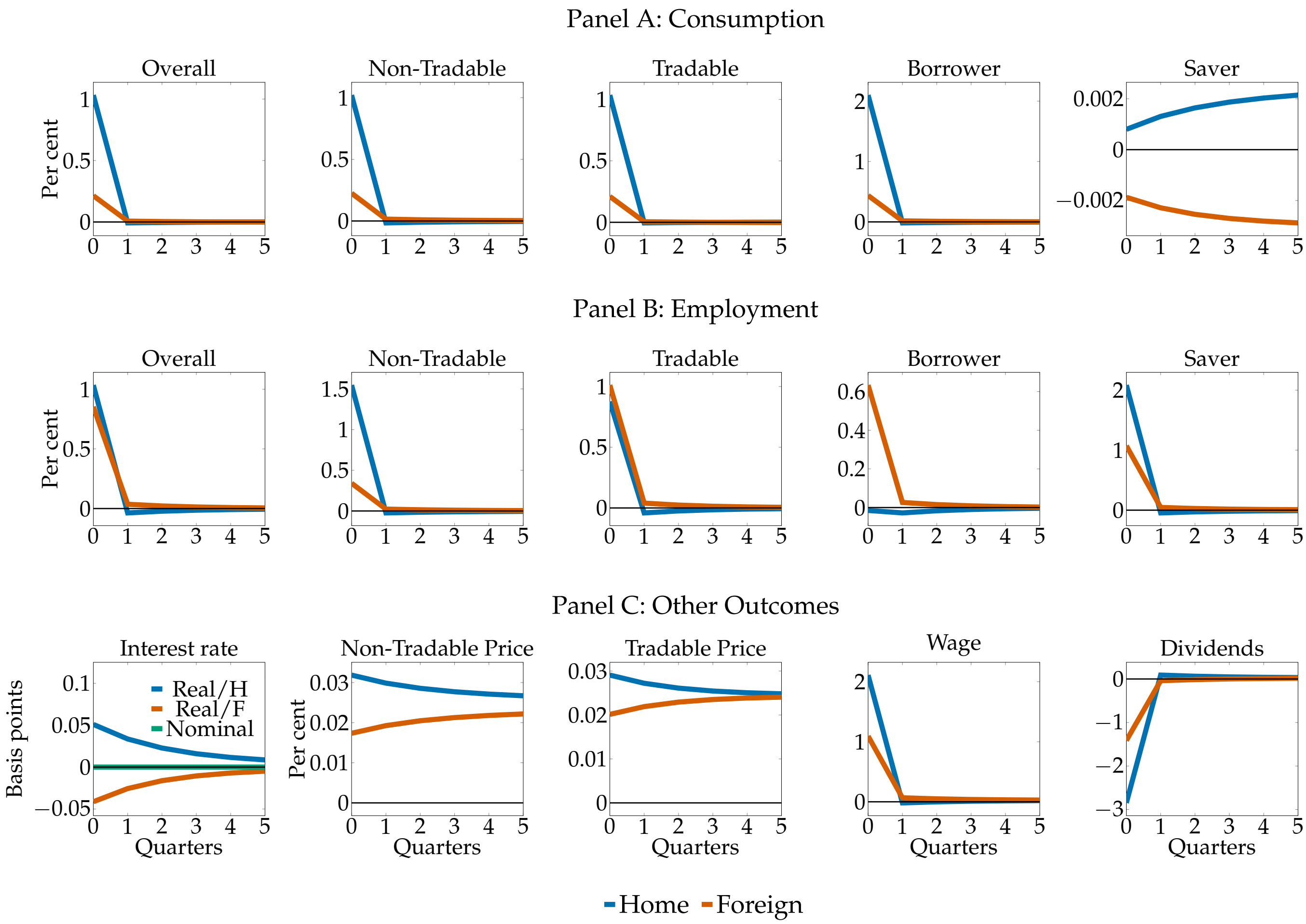
Note: This figure plots reduced form estimates of the effect of cross-state bankruptcy protections on employment outcomes. Panel A reports results for log non-tradable employment, defined as the retail and restaurant sectors. Panel B reports results for log tradable and other employment, where tradable employment is defined as industries with sufficiently large imports and exports from the United States, and other employment is defined as all other industries excluding the construction sector Mian and Sufi (2014*b*). Panel C reports results for log total employment. Panel D reports results for consumer debt charge-offs. We report the coefficients from a panel regression of each log employment measure scaled by working-age population on the log financial cost of filing for bankruptcy protection interacted with year fixed effects. All specifications are weighted by county population as of 2007 and include county and year-quarter fixed effects, as well as the Saiz supply elasticity, the share of the population who own a home in 2007q1 for each county, and the 2007 debt-to-income ratio, interacted with year-quarter fixed effects. For counties without the Saiz supply elasticity measure, the measure is set to zero, and an additional dummy for missing supply elasticity is interacted with year-quarter fixed effects. The estimated effect is normalized to zero in 2007, meaning all estimates are relative to 2007. The dashed lines are 95 percent confidence intervals from standard errors clustered at the state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Appendix Figure A7: Debt Relief Multiplier as a Function of Parameters ζ, Y, and f



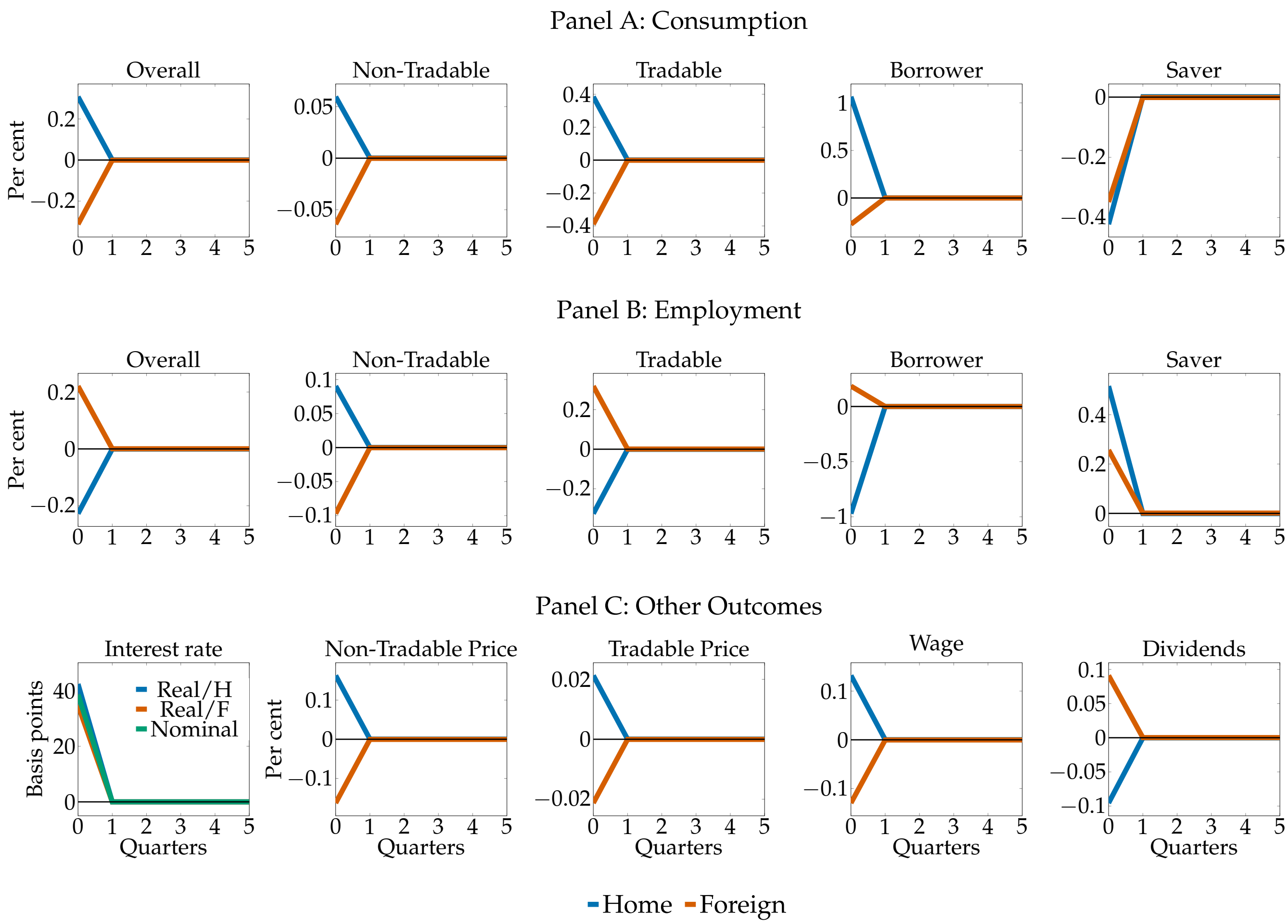
Note: This figure plots the debt relief multiplier in our model for non-tradable and tradable employment in high- and low-exemption regions as a function of the GHH+ parameter ζ, the Kimball parameter Y, and the fraction of debt relief borne at home f. The blue line is the debt relief multiplier in the home region ("high-exemption"), the red line is the debt relief multiplier in the foreign region ("low-exemption"), and the black line represents the difference between the home and the foreign regions.

Appendix Figure A8: Impulse Response to a Debt Relief Shock under Sticky Prices



Note: This figure plots the effect in our model of a debt relief shock that reduces borrower debts in the high exemptions regions by 1 percent of PCE on aggregate outcomes under the assumption that prices are sticky ($\theta = 0.76$). The blue line represents outcomes in the home region ("high-exemption"), and the red line is outcomes in the foreign region ("low-exemption"). Panel A plots consumption outcomes, where the first column plots aggregate consumption, the second and third columns plot non-tradable and tradable consumption, and the fourth and fifth columns plot borrower and saver consumption. Panel B plots employment outcomes, where the first column plots aggregate employment, the second and third columns plot non-tradable and tradable employment, and the fourth and fifth columns plot borrower and saver employment. Panel C plots price outcomes, where the first column plots nominal and real interest rates, the second and third columns plot non-tradable and tradable prices, the fourth column plots nominal wages, and the fifth column plots aggregate dividends from firms.

Appendix Figure A9: Impulse Response to a Debt Relief Shock under Flexible Prices



Note: This figure plots the effect in our model of a debt relief shock that reduces borrower debts in the high exemptions regions by 1 percent of PCE on aggregate outcomes under the assumption that prices are flexible ($\theta = 0$) instead of sticky ($\theta = 0.8$) as in our baseline parameterization shown in Figure A8. The blue line represents outcomes in the home region ("high-exemption"), and the red line is outcomes in the foreign region ("low-exemption"). Panel A plots consumption outcomes, where the first column plots aggregate consumption, the second and third columns plot non-tradable and tradable consumption, and the fourth and fifth columns plot borrower and saver consumption. Panel B plots employment outcomes, where the first column again plots aggregate employment, the second and third columns plot non-tradable and tradable employment, and the fourth and fifth columns plot borrower and saver employment. Panel C plots price outcomes, where the first column plots nominal and real interest rates, the second and third columns plot non-tradable and tradable prices, the fourth column plots nominal wages, and the fifth column plots aggregate dividends from firms.

Appendix D. Model Details

This appendix derives our model, details our calibration strategy and explains our solution method. Here, we allow for a slightly more general version of the model in which:

1. The size of the home region can be an arbitrary number $n \in (0,1)$ (with $n = \frac{1}{2}$ in the main text).
2. Borrowers and savers may own different shares in firms; we denote these shares by v^B, v^S (with $v^B = v^S = 1$ in the main text).
3. Tradable and non-tradable goods firms may have different degrees of diminishing returns to production γ_T, γ_{NT} (with $\gamma_T = \gamma_{NT} = \gamma$ in the main text).

A. Derivation of Equations

Households. As described in Section IV, our general household problem is:

$$\begin{aligned} \max \quad & \mathbb{E} \sum_{t=0}^{\infty} (\beta^h)^t u^h \left(C_{i,t}^h, N_{i,t}^h \right) \\ \text{s.t.} \quad & P_{i,t} C_{i,t}^h + B_{i,t-1}^h - \Delta_{i,t}^h \le \frac{B_{i,t}^h}{1+I_t} + W_{i,t} N_{i,t}^h + v^h D_{i,t}, \\ & B_{i,t}^h \le \bar{b} P_{T,t} \end{aligned}$$

This yields standard first-order conditions: a labor supply decision:

$$\frac{W_{i,t}}{P_{i,t}} = \frac{u_N^h \left(C_{i,t}^h, N_{i,t}^h \right)}{u_C^h \left(C_{i,t}^h, N_{i,t}^h \right)} \tag{D.1}$$

and an intertemporal Euler equation:

$$\beta \frac{u_C^h \left(C_{i,t+1}^h, N_{i,t+1}^h \right)}{u_C^h \left(C_{i,t}^h, N_{i,t}^h \right)} = \frac{P_{i,t+1}}{P_{i,t}} \frac{1}{1+I_t} \left(1 - (1+I_t) \mu_{i,t}^h \right) \tag{D.2}$$

where $\mu_{i,t}$ is the Lagrange multiplier on the borrowing constraint; when $\mu_{i,t}^h > 0$ this constraint binds and:

$$B_{i,t}^h = \bar{b} P_{T,t} \tag{D.3}$$

Static maximization of the allocation of consumption across goods also yields standard first-order conditions: for both $h \in \{S, B\}$,

$$\frac{C_{T,H,j,t}^h}{C_{H,t}^h} = \alpha \left(\frac{P_{T,j,t}}{P_{T,t}} \right)^{-\epsilon} \left(\frac{P_{T,t}}{P_{H,t}} \right)^{-\eta} \tag{D.4}$$

$$\frac{C_{NT,H,j,t}^h}{C_{H,t}^h} = (1-\alpha) \left(\frac{P_{NT,H,j,t}}{P_{NT,H,t}} \right)^{-\epsilon} \left(\frac{P_{NT,H,t}}{P_{H,t}} \right)^{-\eta} \tag{D.5}$$

$$\frac{C_{T,F,j,t}^h}{C_{F,t}^h} = \alpha \left(\frac{P_{T,j,t}}{P_{T,t}} \right)^{-\epsilon} \left(\frac{P_{T,t}}{P_{F,t}} \right)^{-\eta} \tag{D.6}$$

$$\frac{C_{NT,F,j,t}^h}{C_{F,t}^h} = (1-\alpha) \left(\frac{P_{NT,F,j,t}}{P_{NT,F,t}} \right)^{-\epsilon} \left(\frac{P_{NT,F,t}}{P_{F,t}} \right)^{-\eta} \tag{D.7}$$

Separable preferences. Under separable preferences, $u^h(C,N) = \frac{C^{1-\sigma}}{1-\sigma} - \chi^h \frac{N^{1+\psi}}{1+\psi}$, (D.1) reads:

$$\frac{\chi^h (N_{i,t}^h)^\psi}{(C_{i,t}^h)^{-\sigma}} = \frac{W_{i,t}}{P_{i,t}} \tag{D.8}$$

and (D.2) reads:

$$\beta \frac{\left(C_{i,t+1}^h\right)^{-\sigma}}{\left(C_{i,t}^h\right)^{-\sigma}} = \frac{P_{i,t+1}}{P_{i,t}} \frac{1}{1+I_t} \left(1 - (1+I_t)\mu_{i,t}^h\right) \tag{D.9}$$

GHH preferences. Under GHH preferences, $u^h(C,N) = \frac{1}{1-\sigma}\left(C - \chi^h \frac{N^{1+\psi}}{1+\psi}\right)^{1-\sigma}$, instead, (D.1) reads:

$$\chi^h (N_{i,t}^h)^\psi = \frac{W_{i,t}}{P_{i,t}} \tag{D.10}$$

and (D.2) reads:

$$\beta \frac{\left(C_{i,t+1} - \chi \frac{N_{it+1}^{1+\psi}}{1+\psi}\right)^{-\sigma}}{\left(C_{i,t} - \chi \frac{N_{it}^{1+\varphi}}{1+\varphi}\right)^{-\sigma}} = \frac{P_{i,t+1}}{P_{i,t}} \frac{1}{1+I_t} \left(1 - (1+I_t)\mu_{i,t}^h\right) \tag{D.11}$$

Generic firm problem. A generic firm has monopoly power; its maximization problem is:

$$\max_{p(i)} D(i) \equiv p(i)y(i) - Wn(i)$$

where W is the local wage it faces, $p(i)$ is its price, $n(i)$ is its employment, $y(i) = An(i)^\gamma$ is its production function. The firm faces a demand constraint of the form:

$$p(i) = PY^{\frac{1}{\epsilon}} y(i)^{-\frac{1}{\epsilon}}$$

Under fully flexible prices each period, the firm sets $n(i)$ optimally to maximize static profits, this yields:

$$PY^{\frac{1}{\epsilon}} \gamma \frac{\epsilon - 1}{\epsilon} \left[An(i)^\gamma\right]^{\frac{\epsilon-1}{\epsilon}} \frac{1}{n(i)} - W = 0$$

where $n(i) = \left(\frac{y(i)}{A}\right)^{\frac{1}{\gamma}}$; hence:

$$\frac{W}{P} = \gamma \frac{\epsilon - 1}{\epsilon} A^{\frac{1}{\gamma}} Y^{\frac{1}{\epsilon}} y(i)^{1 - \frac{1}{\epsilon} - \frac{1}{\gamma}}$$

All firms in the same industry have the same price $p(i) = P$ and therefore produce the same amount, so $y(i) = Y$ and every period:

$$\frac{W_t}{P_t} = \gamma \frac{\epsilon - 1}{\epsilon} A_t^{\frac{1}{\gamma}} Y_t^{1 - \frac{1}{\gamma}} \tag{D.12}$$

Under sticky Calvo prices, a standard derivation yields a firm's optimal reset price P_t^* at time t, relative to its industry's price index P_t, as:

$$\left(\frac{P_t^*}{P_t}\right)^{1+\frac{\varepsilon(1-\gamma)}{\gamma}} = \frac{\varepsilon}{\varepsilon-1}\frac{\sum_{k=0}^{\infty}\theta^k E\left[\left(\beta^S\right)^k u_{C,t+k}^S Y_{t+k}\left(\frac{P_{t+k}}{P_t}\right)^{\varepsilon+\frac{\varepsilon(1-\gamma)}{\gamma}}\overline{MC}_t^r\right]}{\sum_{k=0}^{\infty}\theta^k E\left[\left(\beta^S\right)^k u_{C,t+k}^S Y_{t+k}\left(\frac{P_{t+k}}{P_t}\right)^{\varepsilon-1}\right]} \tag{D.13}$$

where the firm discounts future real flows using the saver's stochastic discount factor $\left(\beta^S\right)^k u_{C,t+k}^S$ since the saver is always on his Euler equation, Y_t is industry demand, and $\overline{MC}_t^r$ are average real marginal costs at time t, defined as:

$$\overline{MC}_t^r \equiv \frac{W_t}{P_t}\frac{1}{\gamma A_t^{\frac{1}{\gamma}} Y_t^{1-\frac{1}{\gamma}}} \tag{D.14}$$

note that in the limit of flexible prices, $\theta = 0$, $P_t = P_t^*$ and we recover (D.12). Linearization for inflation $\Pi_t = \frac{P_t}{P_{t-1}}$ around a steady-state with zero inflation results in the standard New Keynesian Phillips curve:

$$\hat{\pi}_t = \beta^S E\left[\hat{\pi}_{t+1}\right] + \frac{(1-\theta)\left(1-\theta\beta^S\right)}{\theta}\frac{1}{1+\frac{\varepsilon(1-\gamma)}{\gamma}}\hat{mc}_t^r$$

where:

$$\hat{mc}_t^r = \hat{w}_t - \hat{p}_t + \frac{1-\gamma}{\gamma}\hat{y}_t$$

We now specify this generic problem to non-traded and traded goods firms in the home and the foreign region.

Non-tradable goods firm problem. A firm j in the non-tradable sector of region $i \in \{H, F\}$ operates its technology subject to a demand constraint:

$$\left(\frac{p_{NT,i,j,t}}{P_{NT,i,t}}\right)^{-\epsilon} C_{NT,i,t} = y_{NT,i,j,t} = A_{NT,t}\left(n_{NT,i,j,t}\right)^{\gamma_{NT}} \tag{D.15}$$

where

$$C_{NT,i,t} = (1-\alpha)\left(\frac{P_{NT,i,t}}{P_{i,t}}\right)^{-\eta} C_{i,t}^h$$

is non-traded goods demand in region i, taken as exogenous by the firm. Write $N_{NT,i,t}$ for aggregate employment in the non-tradable sector of region i at time t. Market clearing for each input $N_{NT,i,t} = \int_{j=0}^1 n_{NT,i,j,t}dj$ implies:

$$N_{NT,i,t} = \int_j \left(\frac{p_{NT,i,j,t}}{P_{NT,i,t}}\right)^{-\frac{\epsilon}{\gamma_{NT}}} dj \cdot \left(\frac{C_{NT,i,t}}{A_{NT,t}}\right)^{\frac{1}{\gamma_{NT}}}$$

Hence, at the aggregate level, consumption and employment are related via:

$$C_{NT,i,t} = \frac{A_{NT,t}}{\triangle_{NT,i,t}}\left(N_{NT,i,t}\right)^{\gamma_{NT}}$$

where $\Delta_{NT,i,t} = \left[\int_{j=0}^1 \left(\frac{p_{NT,i,j,t}}{P_{NT,i,t}}\right)^{-\frac{\epsilon}{\gamma_{NT}}} dj\right]^{\gamma_{NT}} \geq 1$.

This has the form of the generic problem described above. Under flexible prices $\Delta_{NT,i,t} = 1$ and we obtain:

$$\frac{W_{i,t}}{P_{NT,i,t}} = \gamma_{NT}\frac{\epsilon-1}{\epsilon}A_{NT,t}^{\frac{1}{\gamma_{NT}}}Y_{NT,i,t}^{1-\frac{1}{\gamma_{NT}}} = \gamma_{NT}\frac{\epsilon-1}{\epsilon}A_{NT,t}N_{NT,i,t}^{\gamma_{NT}-1} \tag{D.16}$$

Under sticky prices, the Phillips curves are:

$$\hat{\pi}_{NT,i,t} = \beta^S E\left[\hat{\pi}_{NT,i,t+1}\right] + \kappa_{NT}\left(\hat{w}_{i,t} - \hat{p}_{NT,i,t} + \frac{1-\gamma}{\gamma}\hat{y}_{NT,i,t}\right)$$

where $\kappa_{NT} \equiv \frac{(1-\theta)\left(1-\theta\beta^S\right)}{\theta}\frac{1}{\left(1+\frac{\epsilon(1-\gamma_{NT})}{\gamma_{NT}}\right)}$.

Tradable goods firm problem. A firm j in the tradable sector of region $i \in \{H, F\}$ operates its technology subject to a demand constraint:

$$\left(\frac{p_{T,i,j,t}}{P_{T,i,t}}\right)^{-\epsilon}\left(\frac{P_{T,i,t}}{P_{T,t}}\right)^{-\epsilon} C_{T,t} = y_{T,i,j,t} = A_{T,t}\left(n_{T,i,j,t}\right)^{\gamma_{NT}} \tag{D.17}$$

The difference with (D.15) is that goods demand is global, namely:

$$C_{T,t} = n \cdot \alpha \left(\frac{P_{T,t}}{P_{H,t}}\right)^{-\eta} C_{H,t} + (1-n)\cdot\alpha\left(\frac{P_{T,t}}{P_{F,t}}\right)^{-\eta} C_{F,t}$$

Write $N_{T,i,t}$ for aggregate employment in the tradable sector of region i at time t. Market clearing at home and abroad, $N_{T,H,t} = \int_{j=0}^{n} n_{T,i,j} dj$ and $N_{T,F,t} = \int_{j=n}^{1} n_{T,i,j} dj$, implies:

$$N_{T,H,t} = \int_{j=0}^{n}\left(\frac{p_{T,i,j,t}}{P_{T,H,t}}\right)^{-\frac{\epsilon}{\gamma_T}} dj \left(\frac{\left(\frac{P_{T,H,t}}{P_{T,t}}\right)^{-\epsilon} C_{T,t}}{A_{T,t}}\right)^{\frac{1}{\gamma_T}}$$

$$\begin{aligned}
\left(\frac{P_{T,H,t}}{P_{T,t}}\right)^{-\epsilon} C_{T,t} &= \frac{A_{T,t}}{\triangle_{T,H,t}}\left(N_{T,H,t}\right)^{\gamma_T} \\
\left(\frac{P_{T,F,t}}{P_{T,t}}\right)^{-\epsilon} C_{T,t} &= \frac{A_{T,t}}{\triangle_{T,F,t}}\left(N_{T,F,t}\right)^{\gamma_T}
\end{aligned}$$

where $\Delta_{T,H,t} = \left[\int_{j=0}^{n}\left(\frac{p_{T,H,j,t}}{P_{T,H,t}}\right)^{-\frac{\epsilon}{\gamma_T}} dj\right]^{\gamma_T} \geq 1$ and $\Delta_{T,H,t} = \left[\int_{j=n}^{1}\left(\frac{p_{T,F,j,t}}{P^F_{T,F,t}}\right)^{-\frac{\epsilon}{\gamma_T}} dj\right]^{\gamma_T} \geq 1$.[30]

This problem also has the form of the generic problem described above. Under flexible prices $\Delta_{T,i,t} = 1$ and we obtain:

$$\frac{W_{i,t}}{P_{T,i,t}} = \gamma_T \frac{\epsilon-1}{\epsilon} A_{T,t}^{\frac{1}{\gamma_T}} Y_{T,i,t}^{1-\frac{1}{\gamma_T}} = \gamma_T\frac{\epsilon-1}{\epsilon} A_{T,t} N_{T,i,t}^{\gamma_T - 1} \tag{D.18}$$

in particular, combining with (D.16), we obtain relative labor demand as:

$$\frac{P_{NT,i,t}}{P_{T,i,t}} = \frac{\gamma_T}{\gamma_{NT}}\frac{A_{T,t}}{A_{NT,t}}\frac{\left(N_{NT,i,t}\right)^{1-\gamma_{NT}}}{\left(N_{T,i,t}\right)^{1-\gamma_T}}$$

Under sticky prices, the Phillips curves are:

$$\hat{\pi}_{T,i,t} = \beta^S E\left[\hat{\pi}_{T,i,t+1}\right] + \kappa_T\left(\hat{w}_{i,t} - \hat{p}_{T,i,t} + \frac{1-\gamma}{\gamma}\hat{y}_{T,i,t}\right)$$

[30] We have defined the local tradable price indices as $\left(P_{T,H}\right)^{1-\epsilon} = \frac{1}{n}\int_{j=0}^{n}\left(p_{j,T}\right)^{1-\epsilon} dj$ and $\left(P_{T,F}\right)^{1-\epsilon} = \frac{1}{1-n}\int_{j=n}^{1}\left(p_{j,T}\right)^{1-\epsilon} dj$ so that the overall traded goods price index is $P_T^{1-\epsilon} = n\left(P_{T,H}\right)^{1-\epsilon} + (1-n)\left(P_{T,F}\right)^{1-\epsilon}$.

where $\kappa_T \equiv \frac{(1-\theta)(1-\theta\beta^S)}{\theta} \frac{1}{\left(1+\frac{\epsilon(1-\gamma_{NT})}{\gamma_{NT}}\right)}$.

B. Steady state

Our flexible-price steady state has 29 equations, as detailed below:

1) Our numeraire is the price index for tradables, which we normalize to $P_T = 1$

2) The definition of the tradable price index P_T implies:

$$\frac{P_{TH}}{P_T} = \left[n + (1-n)\left(\frac{P_{TF}}{P_{TH}}\right)^{1-\epsilon}\right]^{\frac{-1}{1-\epsilon}}$$

where recall that n denotes the size of the home region; in the main text $n = \frac{1}{2}$.

3-4) Consumer price indices at home and foreign:

$$\begin{aligned} P_H &= \left[\alpha (P_T)^{1-\eta} + (1-\alpha)(P_{NT,H})^{1-\eta}\right]^{\frac{1}{1-\eta}} \\ P_F &= \left[\alpha (P_T)^{1-\eta} + (1-\alpha)(P_{NT,F})^{1-\eta}\right]^{\frac{1}{1-\eta}} \end{aligned}$$

5-8) Household intratemporal first-order conditions: in the case of separable preferences this is given by (D.8):

$$\begin{aligned} \chi^B \left(C_H^B\right)^\sigma \left(N_H^B\right)^\psi &= \frac{W_H}{P_H} \\ \chi^B \left(C_F^B\right)^\sigma \left(N_F^B\right)^\psi &= \frac{W_F}{P_F} \end{aligned}$$

$$\begin{aligned} \chi^S \left(C_H^S\right)^\sigma \left(N_H^S\right)^\psi &= \frac{W_H}{P_H} \\ \chi^S \left(C_F^S\right)^\sigma \left(N_F^S\right)^\psi &= \frac{W_F}{P_F} \end{aligned}$$

in the case of GHH preferences, replace with (D.10) instead.

9-10) Labor market clearing conditions at Home and in Foreign, exploiting $\Delta_{T,i} = \Delta_{NT,i} = 1$ at steady state:

$$\begin{aligned} \varphi^B N_H^B + \varphi^S N_H^S &= N_{T,H} + N_{NT,H} = \left(\frac{Y_{T,H}}{A_T}\right)^{\frac{1}{\gamma_T}} + \left(\frac{Y_{NT,H}}{A_{NT}}\right)^{\frac{1}{\gamma_{NT}}} \\ \varphi^B N_F^B + \varphi^S N_F^S &= N_{T,F} + N_{NT,F} = \left(\frac{Y_{T,F}}{A_T}\right)^{\frac{1}{\gamma_T}} + \left(\frac{Y_{NT,F}}{A_{NT}}\right)^{\frac{1}{\gamma_{NT}}} \end{aligned}$$

11-14) Goods market clearing conditions for tradables and non-tradables at Home and in Foreign:

$$Y_{T,H} = \left(\frac{P_{T,H}}{P_T}\right)^{-\epsilon} (nC_{T,H} + (1-n)\,C_{T,F}) \tag{D.19}$$

$$Y_{T,F} = \left(\frac{P_{T,F}}{P_T}\right)^{-\epsilon} (nC_{T,H} + (1-n)\,C_{T,F}) \tag{D.20}$$

$$\begin{aligned} Y_{NT,H} &= C_{NT,H} \\ Y_{NT,F} &= C_{NT,F} \end{aligned}$$

15-18) Consumer demand at Home and in Foreign, (D.4)–(D.7):

$$\begin{aligned}
\frac{C_{T,H}}{C_H} &= \alpha \left(\frac{P_T}{P_H}\right)^{-\eta} \\
\frac{C_{NT,H}}{C_H} &= (1-\alpha)\left(\frac{P_{NT,H}}{P_H}\right)^{-\eta}
\end{aligned}$$

$$\begin{aligned}
\frac{C_{T,F}}{C_F} &= \alpha \left(\frac{P_T}{P_F}\right)^{-\eta} \\
\frac{C_{NT,F}}{C_F} &= (1-\alpha)\left(\frac{P_{NT,F}}{P_F}\right)^{-\eta}
\end{aligned}$$

19-20) Aggregate demand at Home and in Foreign:

$$\begin{aligned}
C_H &= \varphi_B C_H^B + (1-\varphi_B) C_H^S \\
C_F &= \varphi_B C_F^B + (1-\varphi_B) C_F^S
\end{aligned}$$

21-24) Tradable and non-tradable goods firms FOCs, at Home and in Foreign, (D.16) and (D.18):

$$\begin{aligned}
\frac{W_H}{P_{T,H}} &= \frac{\epsilon-1}{\epsilon}\gamma_T A_T^{\frac{1}{\gamma_T}} \left(Y_{T,H}\right)^{-\frac{(1-\gamma)}{\gamma}} \\
\frac{W_F}{P_{T,F}} &= \frac{\epsilon-1}{\epsilon}\gamma_T A_T^{\frac{1}{\gamma_T}} \left(Y_{T,F}\right)^{-\frac{(1-\gamma)}{\gamma}} \\
\frac{W_H}{P_{NT,H}} &= \frac{\epsilon-1}{\epsilon}\gamma_{NT} A_{NT}^{\frac{1}{\gamma_{NT}}} \left(Y_{NT,H}\right)^{-\frac{(1-\gamma)}{\gamma}} \\
\frac{W_F}{P_{NT,F}} &= \frac{\epsilon-1}{\epsilon}\gamma_{NT} A_{NT}^{\frac{1}{\gamma_{NT}}} \left(Y_{NT,F}\right)^{-\frac{(1-\gamma)}{\gamma}}
\end{aligned}$$

25-26) Total firm dividends, at Home and in Foreign:

$$\begin{aligned}
D_H &= \left(P_{T,H}Y_{T,H} - W_H N_{T,H}\right) + \left(P_{NT,H}Y_{NT,H} - W_H N_{NT,H}\right) \\
D_F &= \left(P_{T,F}Y_{T,F} - W_F N_{T,F}\right) + \left(P_{NT,F}Y_{NT,F} - W_F N_{NT,F}\right)
\end{aligned}$$

27-29) Three budget constraints for Home and Foreign borrower and Home saver:

$$\begin{aligned}
P_H C_H^B &= W_H N_H^B + v^B D_H - \frac{I}{1+I}\bar{b} P_H \\
P_H C_H^S &= W_H N_H^S + v^S D_H - \frac{I}{1+I} B_H^S \\
+P_F C_F^B &= W_F N_F^B + v^B D_F - \frac{I}{1+I}\bar{b} P_F
\end{aligned}$$

The Foreign saver's budget constraint follows by Walras's law.

Calibration. Our primitives are n (country size), φ^B (fraction of borrowers), v^B (number of shares per borrower), the preference parameters α, σ, ψ, ϵ, η, β^S and β^B, the production elasticities γ_T and γ_{NT}, as well as a normalization for total PCE C, and the debt-to-PCE ratio $\frac{B}{PC}$. Given these, we now solve for A_T, A_{NT}, χ^S and χ^B to reach a symmetric steady state with zero inflation, so $\Pi = 1$ and $R = 1 + I$, where all relative prices equal to 1, and where hours worked for savers and borrowers in both regions are also both equal to 1.

Given our normalization of $P_T = 1$, our restriction of unit relative prices implies:

$$P_{NT,H} = P_{NT,F} = P_{T,H} = P_{T,F} = P_H = P_F = 1$$

In the symmetric steady state, per capita consumption in each region is the same:

$$C_H = C_F = C$$

given (D.4)–(D.7), this implies:

$$C_T = \alpha C \quad C_{NT} = (1-\alpha)\,C$$

as well as:

$$Y_T = \alpha C \quad Y_{NT} = (1-\alpha)\,C \tag{D.21}$$

Our normalization for hours implies:

$$N_H^B = N_H^S = N_F^B = N_F^S = 1$$

Given (D.16) and (D.18), this implies equalization of the value of the marginal product of labor within regions:

$$P_T \gamma_T A_T N_T^{\gamma_T - 1} = P_{NT} \gamma_{NT} A_{NT} N_{NT}^{\gamma_{NT}-1} \tag{D.22}$$

Together with:

$$N_{NT} + N_T = 1 \tag{D.23}$$

this equation pins down relative employment in each sector as a function of the degree of decreasing returns and technology.

Next, production implies:

$$Y_T = A_T N_T^{\gamma_T} \quad Y_{NT} = A_{NT} N_{NT}^{\gamma_{NT}}$$

Combining with (D.21) and (D.22), we obtain:

$$\frac{\alpha}{1-\alpha} = \frac{A_T}{A_{NT}} \frac{N_T^{\gamma_T}}{N_{NT}^{\gamma_{NT}}} = \frac{\gamma_{NT}}{\gamma_T} \frac{N_T^{1-\gamma_T}}{N_{NT}^{1-\gamma_{NT}}} \frac{N_T^{\gamma_T}}{N_{NT}^{\gamma_{NT}}} = \frac{\gamma_{NT}}{\gamma_T} \frac{N_T}{N_{NT}}$$

hence, given (D.23), tradable goods employment N_T in either region solves:

$$\frac{N_T}{N_{NT}} = \frac{\alpha}{1-\alpha} \frac{\gamma_T}{\gamma_{NT}} = \frac{N_T}{1-N_T}$$

Solving for N_T and N_{NT}, we obtain:

$$N_T = \frac{\alpha \gamma_T}{(1-\alpha)\,\gamma_{NT} + \alpha \gamma_T}$$

$$N_{NT} = \frac{(1-\alpha)\,\gamma_{NT}}{(1-\alpha)\,\gamma_{NT} + \alpha \gamma_T}$$

This delivers A_T and A_{NT}, as desired:

$$A_T = \frac{\alpha C}{N_T^{\gamma_T}} \qquad A_{NT} = \frac{(1-\alpha)\,C}{N_{NT}^{\gamma_{NT}}} \tag{D.24}$$

Symmetry also implies that relative wages are equated across regions:

$$W_H = W_F = W$$

The labor share in the tradable sector and non-tradable sectors are, respectively:

$$\frac{WN_T}{PC} = \frac{P_T}{P}\frac{\epsilon-1}{\epsilon}\gamma_T\alpha = \frac{\epsilon-1}{\epsilon}\gamma_T\alpha$$

$$\frac{WN_{NT}}{PC} = \frac{\epsilon-1}{\epsilon}\gamma_{NT}(1-\alpha)$$

This implies that the level of the wage must be:

$$W = \frac{\epsilon-1}{\epsilon}\left(\gamma_T\alpha + \gamma_{NT}(1-\alpha)\right)C = \omega C$$

where $\omega \equiv \frac{\epsilon-1}{\epsilon}\left(\gamma_T\alpha + \gamma_{NT}(1-\alpha)\right)$, and that dividends are:

$$\begin{aligned}\frac{D}{P} &= Y_T + Y_{NT} - WN \\ &= C - WN \\ &= C(1-\omega)\end{aligned}$$

Finally, budget constraints together with a binding borrowing constraint for borrowers, $B^B = \bar{b}$, imply:

$$\begin{aligned}C_H^B &= W + v^B D - \frac{I}{1+I}\bar{b} \\ C_H^S &= W + v^S D - \frac{I}{1+I}B^S \\ C_F^S &= W + v^S D - \frac{I}{1+I}B^S\end{aligned}$$

Market clearing for domestic debt implies:

$$\varphi^B\frac{\bar{b}}{C} + \left(1-\varphi^B\right)\frac{B^S}{C} = 0$$

so

$$\frac{B^S}{C} = -\frac{\varphi^B}{1-\varphi^B}\frac{\bar{b}}{C}$$

We finally obtain χ^B and χ^S to our normalization for hours worked by setting:

$$\chi^B = \frac{W}{(C^B)^\sigma} = \frac{\omega C}{\left[\left(\omega + v^B(1-\omega) - (I-1)\frac{\bar{b}}{C}\right)C\right]^\sigma} \tag{D.25}$$

$$\chi^S = \frac{W}{(C^S)^\sigma} = \frac{\omega C}{\left[\left(\omega + v^S(1-\omega) + (I-1)\frac{\varphi^B}{1-\varphi^B}\frac{\bar{b}}{C}\right)C\right]^\sigma} \tag{D.26}$$

Note that aggregate household debt, as a share of aggregate consumption, is $\frac{\varphi^B\bar{b}}{C}$.

C. Impulse responses

Our model admits two regimes depending on the set of constraints that binds. When all agents are unconstrained:

$$u^B_{C,H,t} = \beta^B(1+I_t)\frac{P_{H,t}}{P_{H,t+1}}u^B_{C,H,t+1} \tag{D.27}$$

$$u^S_{C,H,t} = \beta^S \left(1 + I_t\right) \frac{P_{H,t}}{P_{H,t+1}} u^S_{C,H,t+1} \tag{D.28}$$

$$u^B_{C,F,t} = \beta^B \left(1 + I_t\right) \frac{P_{F,t}}{P_{F,t+1}} u^B_{C,F,t+1} \tag{D.29}$$

$$u^S_{C,F,t} = \beta^S \left(1 + I_t\right) \frac{P_{F,t}}{P_{F,t+1}} u^S_{C,F,t+1} \tag{D.30}$$

and all agents are on their budget constraints:

$$P_{i,t}C^h_{i,t} + B^h_{i,t-1} - \Delta^h_{i,t} = \frac{B^h_{i,t}}{1 + I_t} + W_{i,t}N^h_{i,t} + v^h D_{i,t}$$

When home borrowers are constrained, (D.27) is replaced by:

$$B^B_{H,t} = \bar{b}P_{T,t}$$

and similarly when foreign borrowers are constrained (D.29) is replaced by:

$$B^B_{F,t} = \bar{b}P_{T,t}$$

D. Loglinearization

Assuming separable preferences, loglinearization of our model around the steady state defined above results in the following equations holding at every time t:

$$\begin{aligned}
p_{TH} - p_T &= (1-n)(p_{TH} - p_{TF}) \\
p_H &= \alpha p_T + (1-\alpha) p_{NT,H} \\
p_F &= \alpha p_T + (1-\alpha) p_{NT,F} \\
\sigma c^B_H + \psi n^B_H &= w_H - p_H \\
\sigma c^B_F + \psi n^B_F &= w_F - p_F \\
\sigma c^S_H + \psi n^S_H &= w_H - p_H \\
\sigma c^S_F + \psi n^S_F &= w_F - p_F \\
n_H &= \frac{N_{T,H}}{\gamma_T} y_{T,H} + \frac{N_{NT,H}}{\gamma_{NT}} y_{NT,H} \\
n_F &= \frac{N_{T,F}}{\gamma_T} y_{T,F} + \frac{N_{NT,F}}{\gamma_{NT}} y_{NT,F} \\
n_H &= \varphi^B n^B_H + \varphi^S n^S_H \\
n_F &= \varphi^B n^B_F + \varphi^S n^S_F \\
y_{T,H} &= -\epsilon(p_{T,H} - p_T) + n c_{T,H} + (1-n) c_{T,F} \\
y_{T,F} &= -\epsilon(p_{T,F} - p_T) + n c_{T,H} + (1-n) c_{T,F} \\
y_{NT,H} &= c_{NT,H} \\
y_{NT,F} &= c_{NT,F} \\
c_{T,H} - c_H &= -\eta(p_T - p_H) \\
c_{T,F} - c_F &= -\eta(p_T - p_F) \\
c_{NT,H} - c_H &= -\eta(p_{NT,H} - p_H) \\
c_{NT,F} - c_F &= -\eta(p_{NT,F} - p_F)
\end{aligned}$$

$$
\begin{aligned}
c_H &= \varphi^B \frac{C^B}{C} c_H^B + \varphi^S \frac{C^S}{C} c_H^S \\
c_F &= \varphi^B \frac{C^B}{C} c_F^B + \varphi^S \frac{C^S}{C} c_F^S \\
d_H &= \frac{P_T Y_T}{D} \left(p_{TH} + y_{TH}\right) + \frac{P_{NT} Y_{NT}}{D} \left(p_{NTH} + y_{NTH}\right) - \frac{WN}{D} \left(w_H + n_H\right) \\
d_F &= \frac{P_T Y_T}{D} \left(p_{TF} + y_{TF}\right) + \frac{P_{NT} Y_{NT}}{D} \left(p_{NTF} + y_{NTF}\right) - \frac{WN}{D} \left(w_F + n_F\right)
\end{aligned}
$$

This completes the static equations. In addition we have, when both borrowers are constrained (reference regime):

$$
\begin{aligned}
c_{Ht}^S &= c_{Ht+1}^S - \frac{1}{\sigma} \left(i_t - \pi_{H,t+1}\right) \\
c_{Ft}^S &= c_{Ft+1}^S - \frac{1}{\sigma} \left(i_t - \pi_{F,t+1}\right)
\end{aligned}
$$

together with the budget constraints:

$$
\begin{aligned}
p_{Ht} + c_{Ht}^S &= \frac{B^S}{RPC^S} \left(b_{Ht}^S - i_t\right) - \frac{B^S}{PC^S} b_{Ht-1}^S + \frac{WN}{PC^S} \left(w_{Ht} + n_{Ht}^S\right) + v^S \frac{D}{PC^S} d_{Ht} \\
p_{Ft} + c_{Ft}^S &= \frac{B^S}{RPC^S} \left(b_{Ft}^S - i_t\right) - \frac{B^S}{PC^S} b_{Ft-1}^S + \frac{WN}{PC^S} \left(w_{Ft} + n_{Ft}^S\right) + v^S \frac{D}{PC^S} d_{Ft}
\end{aligned}
$$

and

$$
\begin{aligned}
p_{Ht} + c_{Ht}^B &= \frac{B^B}{RPC^B} \left(b_{Ht}^B - i_t\right) - \frac{B^B}{PC^S} b_{Ht-1}^B + \frac{WN}{PC^B} \left(w_{Ht} + n_{Ht}^B\right) + v^B \frac{D}{PC^B} d_{Ht} \\
p_{Ft} + c_{Ft}^B &= \frac{B^B}{RPC^B} \left(b_{Ht}^B - i_t\right) - \frac{B^B}{PC^S} b_{Ft-1}^B + \frac{WN}{PC^B} \left(w_{Ft} + n_{Ft}^B\right) + v^B \frac{D}{PC^B} d_{Ft}
\end{aligned}
$$

When both borrowers are unconstrained, instead we have:

$$
\begin{aligned}
c_{Ht}^B &= c_{Ht+1}^B - \frac{1}{\sigma} \left(i_t - \pi_{H,t+1}\right) \\
c_{Ft}^B &= c_{Ft+1}^B - \frac{1}{\sigma} \left(i_t - \pi_{F,t+1}\right)
\end{aligned}
$$

The dynamics are completed by a description of the evolution of inflation in both regions:

$$
\hat{\pi}_{HT,t} = \beta^S E\left[\hat{\pi}_{HT,t+1}\right] + \frac{(1-\theta)\left(1-\theta\beta^S\right)}{\theta} \frac{1}{\left(1+\frac{\varepsilon(1-\gamma)}{\gamma}\right)} \left(\hat{w}_{HT,t} - \hat{p}_{HT,t} + \frac{1-\gamma}{\gamma} \hat{y}_{HT,t}\right)
$$

$$
\hat{\pi}_{HNT,t} = \beta^S E\left[\hat{\pi}_{HNT,t+1}\right] + \frac{(1-\theta)\left(1-\theta\beta^S\right)}{\theta} \frac{1}{\left(1+\frac{\varepsilon(1-\gamma)}{\gamma}\right)} \left(\hat{w}_{HNT,t} - \hat{p}_{HNT,t} + \frac{1-\gamma}{\gamma} \hat{y}_{HNT,t}\right)
$$

$$
\hat{\pi}_{FT,t} = \beta^S E\left[\hat{\pi}_{FT,t+1}\right] + \frac{(1-\theta)\left(1-\theta\beta^S\right)}{\theta} \frac{1}{\left(1+\frac{\varepsilon(1-\gamma)}{\gamma}\right)} \left(\hat{w}_{FT,t} - \hat{p}_{FT,t} + \frac{1-\gamma}{\gamma} \hat{y}_{FT,t}\right)
$$

$$
\hat{\pi}_{FNT,t} = \beta^S E\left[\hat{\pi}_{FNT,t+1}\right] + \frac{(1-\theta)\left(1-\theta\beta^S\right)}{\theta} \frac{1}{\left(1+\frac{\varepsilon(1-\gamma)}{\gamma}\right)} \left(\hat{w}_{FNT,t} - \hat{p}_{FNT,t} + \frac{1-\gamma}{\gamma} \hat{y}_{FNT,t}\right)
$$

together with the monetary policy rule: either

$$i_t = 0$$

at the ZLB, or:

$$i_t = \phi_\pi \left(\pi_{H,t} + \pi_{F,t}\right)$$

when monetary policy follows at Taylor rule.

E. Solution Method

Since our model has occasionally binding borrowing constraints, we solve it using the Dynare "OccBin" package developed by Matteo Iacoviello and Luca Guerrieri (Guerrieri and Iacoviello 2015).

Whenever the model is in the indeterminacy region (with low ϕ_π, as in our main case of the zero lower bound), we resolve this indeterminacy by assuming that the economy returns to steady state by the end of horizon τ of our impulse responses. Practically, since there is a single dimension of indeterminacy, this involves parametrizing the expectation error in one of the Phillips curves with one "sunspot" shock. We set

$$\hat{\pi}_{HT,t+1} = \mathbb{E}_t\left[\hat{\pi}_{HT,t+1}\right] + \epsilon_{t+1}$$

and simulate the model twice. We first obtain the full set of impulse responses to a debt relief shock assuming that the initial expectational error in inflation is $\epsilon_0 = 0$ (call this $\mathbb{I}^0$, where $\mathbb{I}^0(X,t)$ is the impulse response of variable X at time t). We then obtain the full set of impulse responses to a zero debt relief shock, assuming that the expectational error in inflation is $\epsilon_0 = 1$ (call this $\mathbb{I}^1$). We then find the scalar λ such that the impulse response of inflation is exactly zero at date τ, ie $\mathbb{I}^0(\pi_{HT},\tau) + \lambda\mathbb{I}^1(\pi_{HT},\tau) = 0$. Our impulse responses to all shocks are then simply $\mathbb{I}^0 + \lambda\mathbb{I}^1$. We verify that this delivers continuity at the boundary of indeterminacy (see for example figure 4, Panels C and D).

F. MPCs Out of Debt Relief

Here we show why the consumption function out of debt relief is concave and highlight the determinants of MPCs out of debt relief. The analysis is complementary to Achdou et al. (2021), who provide an alternative and very elegant derivation, and extend it to a case where borrowers face idiosyncratic income risk.

For this section, we work in continuous time. An impatient agent faces the real interest rate r and has discount rate $\rho < r$

$$\begin{aligned} \max \quad & \int e^{-\rho t}\left\{u\left(c_t\right) - v\left(n_t\right)\right\} dt \\ \text{s.t.} \quad & \dot{b_t} = rb_t + wn_t + y - c_t \\ & b_t \geq -d \end{aligned} \tag{D.31}$$

where $u(c) = \frac{c^{1-\sigma}-1}{1-\sigma}$ and $v(n) = \chi\frac{n^{1+\psi}}{1+\psi}$. Her wage w, nonlabor income y and the real interest rate r are all constants.

Steady state. In a steady-state the constraint binds: the agent is at his borrowing limit $b = -d$. Consumption is equal to income from labor and nonlabor income net of interest payments:

$$c = wn + y - rd \tag{D.32}$$

The steady-state levels of consumption c and labor supply n solve (D.32) jointly with the optimality condition for labor vs. consumption:

$$c^\sigma n^\psi = \frac{w}{\chi} \tag{D.33}$$

write this solution (c^*, n^*).

Debt relief. Debt relief for this agent means restarting her at level $b_0 = -d_0$, in other words, forgiving the amount $D = d_0 - d$. The agent will plan to return to the constraint at time T. In the meantime, her consumption solves the Euler equation:

$$\frac{\dot{c}_t}{c_t} = \frac{1}{\sigma}(r - \rho)$$

implying

$$c_t = c_0 e^{-\frac{1}{\sigma}(\rho - r)t} \tag{D.34}$$

Along the way, the intratemporal condition:

$$c_t^\sigma n_t^\psi = w \tag{D.35}$$

holds at all times, implying in particular:

$$n_t = n_0 e^{\frac{1}{\psi}(\rho - r)t} \tag{D.36}$$

Hence consumption decays exponentially and labor supply increases exponentially back towards their respective steady-state levels. Moreover, by continuity

$$c_T = c_0 e^{-\frac{1}{\sigma}(\rho - r)T} = c^*$$

which relates the time it takes to hit the constraint to the initial level of consumption c_0:

$$\frac{1}{\sigma}(\rho - r)T = \ln\left(\frac{c_0}{c^*}\right) \tag{D.37}$$

and similarly

$$-\frac{1}{\psi}(\rho - r)T = \ln\left(\frac{n_0}{n^*}\right)$$

The integrated version of the budget constraint (D.31) between $t = 0$ and $t = T$ (where their asset level is back at the constraint $b_T = -d$) reads:

$$d_0 + \int_0^T c_t e^{-rt} dt = \int_0^T y e^{-rt} dt + \int_0^T w n_t e^{-rt} dt + d e^{-rT}$$

Define

$$a \equiv \frac{1}{\sigma}(\rho - r) \quad b \equiv \frac{1}{\psi}(\rho - r)$$

Using (D.34) and (D.36)

$$d_0 + \frac{c_0}{r + a}\left(1 - e^{-(r+a)T}\right) = \frac{y}{r}\left(1 - e^{-rT}\right) + \frac{w n_0}{r - b}\left(1 - e^{-(r-b)T}\right) + d e^{-rT} \tag{D.38}$$

recognizing that $de^{-rT} = d - \frac{rd}{r}\left(1 - e^{-rT}\right)$ we obtain

$$\frac{c_0}{r + a}\left(1 - e^{-(r+a)T}\right) - \frac{y - rd}{r}\left(1 - e^{-rT}\right) - \frac{w n_0}{r - b}\left(1 - e^{-(r-b)T}\right) = d - d_0$$

noting that $c_0^\sigma n_0^\psi = w$, this gives us a second relation between c_0 and T. Solving for both gives us the full solution to our dynamic optimization problem.

Approximation. We first solve out for c_0 and n_0 as functions of T,

$$\frac{c^* e^{aT}}{r+a}\left(1-e^{-(r+a)T}\right)-\frac{y-rd}{r}\left(1-e^{-rT}\right)-\frac{wn^* e^{-bT}}{r-b}\left(1-e^{-(r-b)T}\right)=d-d_0$$

and use the steady state budget constraint $y-rd=c^*-wn^*$ to rewrite this as:

$$c^*\left[\frac{e^{aT}}{r+a}\left(1-e^{-(r+a)T}\right)-\frac{1}{r}\left(1-e^{-rT}\right)\right]+wn^*\left[\frac{1}{r}\left(1-e^{-rT}\right)-\frac{e^{-bT}}{r-b}\left(1-e^{-(r-b)T}\right)\right]=d-d_0$$

Note that, to a second-order approximation,

$$\begin{aligned}\frac{e^{aT}}{r+a}\left(1-e^{-(r+a)T}\right)-\frac{1}{r}\left(1-e^{-rT}\right) &= \left(1+aT+\frac{a^2T^2}{2}+O\left(T^3\right)\right)\left(T-\frac{(a+r)T^2}{2}+O\left(T^3\right)\right)-\left(T-\frac{rT^2}{2}\right)\\ &= \frac{aT^2}{2}+O\left(T^3\right)\end{aligned}$$

and symmetrically for labor supply by substituting $a=-b$. We obtain:

$$\left(ac^*+bwn^*\right)\frac{T^2}{2}+O\left(T^3\right)=d-d_0\equiv D$$

In other words

$$\frac{1}{2}\left(\frac{1}{\sigma}(\rho-r)+\frac{1}{\psi}(\rho-r)\frac{wn^*}{c^*}\right)T^2\simeq\frac{D}{c^*}$$

so that the solution for T is:

$$T\simeq\sqrt{\frac{2}{\rho-r}\left(\frac{1}{\sigma}+\frac{1}{\psi}\frac{wn^*}{c^*}\right)^{-1}\frac{D}{c^*}} \tag{D.39}$$

Hence, we obtain two approximations for the consumption effect of a given amount of debt relief $D=d-d_0$. Our first approximation is in logs:

$$\ln\left(\frac{c_0}{c^*}\right)=\frac{1}{\sigma}(\rho-r)T\simeq\sqrt{2\frac{\rho-r}{\sigma^*}\frac{D}{c^*}} \tag{D.40}$$

with effective risk aversion

$$\sigma^*\equiv\sigma\left(1+\frac{\sigma}{\psi}\frac{wn^*}{c^*}\right)$$

We can also obtain a (less accurate) expression in percentage terms:

$$\frac{c_0-c^*}{c^*}\simeq\sqrt{2\frac{\rho-r}{\sigma^*}\frac{D}{c^*}} \tag{D.41}$$

Equations (D.40) and (D.41) show that the log change in borrower consumption (or its approximation as a percent change $\frac{c_0-c^*}{c^*}$) is proportional to the square root of the ratio of debt relief to consumption $\frac{D}{c^*}$. As is intuitive, the more impatient the borrower is relative to the risk-free rate and the higher the effective elasticity of intertemporal substitution $\frac{1}{\sigma^*}$, the higher the slope of this relationship.

In addition, the labor supply response is negative and proportional to square root of the ratio of debt relief to labor income:

$$\frac{n_0-n^*}{n^*}\simeq\ln\left(\frac{n_0}{n^*}\right)\simeq-\sqrt{2\frac{\rho-r}{\psi^*}\frac{D}{wn^*}} \tag{D.42}$$

where we similarly have defined effective ψ as

$$\psi^* \equiv \psi \left(1 + \frac{\psi}{\sigma} \frac{c^*}{wn^*}\right)$$

which is larger, the larger the Frisch elasticity ψ^{-1}.

MPCs out of debt relief in discrete time. In discrete time, debt relief D leads borrowers to go back to their borrowing constraints in a number of discrete periods T, instead of a continuous T approximated by (D.39). When D is low enough, the continuous-time T is small and the discrete T equals 1. In this case, the discrete time MPC and the marginal propensity to earn MPE solve

$$MPC = MPE + 1$$

as well as (differentiating D.35)

$$MPE = -\frac{wn}{c} \frac{\psi}{\sigma} MPC$$

this the formula derived Auclert, Bardóczy and Rognlie (2021) applied to the case of separable preferences. Hence, for D small enough, the MPC out of debt relief is simply

$$MPC = \frac{1}{1 + \frac{wn}{c} \frac{\psi}{\sigma}}$$

This number is independent of D provided that the equivalent continuous-time T remains below 1, explaining the flat part of figure 6. Equation (D.39) shows that the length of this flat part, as a function of D, depends on several of our parameter values, chiefly the borrower discount factor ρ relative to the interest rate r.