The Effect of Inequality on Redistribution: An Econometric Analysis [1]

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Abstract

Using data on U.S. state and federal taxes and transfers over a quarter century, we estimate a regression model that yields the marginal effect of any shift of market income share from one quintile to another on the *entire* post tax, post-transfer income distribution. We identify exogenous income distribution changes and account for reverse causality using instruments based on exposure to international trade shocks, international commodity price shocks and national industry demand shocks, as well as lagged endogenous variables, with controls for the level of income, the business cycle and demographics. We find the degree of attenuation of market income shifts initially increases in quintile rank, peaks at the middle quintile and then falls for higher income quintiles, consistent with median voter political economy theory and what Stigler called Director's Law. We also provide evidence of considerable and systematic spillover effects on quintiles neither gaining nor losing in the "experiments," also favoring the middle quintile, what we label the greedy median voter. "Voting" and "income insurance" coalition analyses are presented. We find a strong negative relationship between average real income and redistribution and a modest effect of two year led inequality.

Keywords: Inequality, redistribution, voting, political economy, taxes, transfers

JEL codes: D63, D72, H23

1 Introduction

The last several decades have witnessed a sharp increase in income inequality throughout the developed world.[1] In recent years, this trend has become a major issue of public interest, and the surrounding debate has underscored the influence of distributional concerns on the design of tax codes, transfer programs, and fiscal policies. The nature of this influence is the topic of this paper. We empirically investigate the effect of changes in income inequality on redistributive tax and transfer policy, assessing the role and importance of various political-economic theories in the patterns we observe.

Using Census data from the ASEC supplement to the Current Population Survey, we build and estimate a regression model which maps the distribution of *market* income to the distribution of post-tax, post-transfer *disposable* income in U.S. states over the last quarter century. Each state-year population is first ranked by market income and broken up into five quintiles of equal size. A system of equations then associates quintiles' *market* income shares with a corresponding set of final, disposable income shares (the functional form ensures that the predicted shares always sum to one). To isolate pure changes in market income inequality, we control for the *level* of income, the business cycle, and demographics in the regression. We also account for reverse causality, using proxies for exposure to international commodity price shocks, international trade shocks, and national industry demand shocks as instruments.[2]

Once the model is estimated, it is possible to compute the marginal effect of a shift of market income share from one quintile to another on the *entire* disposable income distribution. Because such shifts are not all equally likely, we examine the 'experiments' which are most representative of recent national and state histories in detail. We then broaden the scope of the analysis by summarizing and distilling the full set of estimates - the marginal effects for each quintile pair and every possible loss-gain scenario.

The first question is whether the so-called 'redistribution hypothesis' holds: as inequality increases, does redistribution increase to compensate?[3] Confirming early studies, we find that, indeed, taxes and transfers *attenuate* shifts in the market income distribution. Quintiles which lose market income share experience a smaller drop in their disposable income share, and quintiles which gain

[1] See Stand and Rising (2011). Autor et al. (2008) highlight the large difference in trends within the distribution, as the 90/50 ratio accounts for most of the rise in inequality, whereas the 50/10 ratio has been fairly stable. They, along with a growing body of literature addressed to the wage premium gap and other issues, highlight the likely role of skill-biased or both capital and human-capital biased technical change (what Boskin and Lau (2000) estimate for the G7 countries and label generalized Solow neutral technical change).

[2] This set of instruments is further supplemented by forecasts of the endogenous variables based on lagged values or national trends, and various extensions consider nonlinear specifications, interaction terms, etc.

[3] This is also usually the result of the benevolent social planner of optimal tax Mirrlees models, although these explore the relationship of inequality in the ability to earn income, not the ex- post realization of market income which also reflects effort. Heathcote et al. (2020) find the utilitarian social planner chooses less progressivity in response to widening skill price dispersion reflecting technical change. This offsets more progressivity in response to larger residual inequality, leaving overall optimal progressivity unchanged for the U.S. data they calibrate for 1980-2016.

market income share experience less growth in their disposable income share. This is unsurprising, but in contrast to the existing literature, the richness of the model allows us to probe further. For the first time, we investigate not only *whether* taxes and transfers stabilize disposable income inequality, but *how* such stabilization occurs and to what degree the responses depend on the way in which the market income distribution is transformed, the quintiles involved, etc. In particular, we show that the degree of attenuation (as described above) is initially increasing in a quintile's percentile rank, peaks at the middle quintile, and then falls for higher income quintiles. In other words, tax and transfer policy is most sensitive to the fortunes of the middle class, consistent with the median voter theorem.[4] We also find that redistribution has significant spillover effects on quintiles which neither gain nor lose market income in the experiments. That is, rather than simply neutralize or reverse shifts in the market income distribution, government intervention also spreads and disperses gains and losses across the *entire* income spectrum. Under a fixed net tax schedule (where 'net taxes' are taxes net of transfers, and 'fixed' means invariant to distribution shocks), such spillovers would be small, so we interpret this as some combination of active policy responses and more complex policy formulas in practice. Finally, the results indicate that these patterns - as well as, to a large extent, the estimates themselves - are quite invariant to the political party in power and the history of market income inequality, expectations of future inequality and other interaction terms we include in the regressions. The only major exception is the level of real disposable income per capita, which has considerable effects (see Section 5.2).

To our knowledge, no other paper has endeavored to predict the disposable income distribution from the market income distribution in a way that respects the adding up constraints involved (shares sum to 1 and share changes sum to 0). This framework opens up various new avenues of inquiry, not all of which are covered in this paper. For instance, in the spirt of Aumann and Kurz (1977), Dixit and Londregan (1996), and Acemoglu et al. (2006), we investigate the possible formation of coalitions across quintiles.

Moreover, we are aware of no other paper which instruments for structural changes in the market income distribution. Because taxes and transfers have obvious implications for work and other incentives and therefore influence the market income distribution, the lack of any correction for reverse causality is an important limitation of previous work. The regional nature of our data makes this issue surmountable. Drawing on methods and insights in Bartik (1991), Autor et al. (2013), Voorheis et al. (2015), Asquith et al. (2017), and Kehrig and Ziebarth (2017), we exploit local heterogeneity in industry composition and state-level exposure to national and international trends to identify exogenous shocks to the market income distribution.

Related Literature. Although a number of important papers have tested the proposition that greater inequality leads to more redistribution, until recently, researchers have lacked the data to construct comprehensive income measures, let alone characterize the distributions of both market

[4]For example, Meltzer and Richard (1981) apply the classic median voter theorem of Hotelling (1929) and Black (1948) to a model economy with income inequality, redistributive taxation, and endogenous labor supply.

and disposable income. As a result, many studies are based on limited proxies for inequality, the level of redistribution, or both. For instance, Meltzer and Richard (1983) uses earnings data, failing to account for capital income and the sorting of wage earners into households, while Alesina and Rodrik (1994) substitutes inequality in land ownership for income inequality. Several papers, such as Perotti (1996) and Bassett et al. (1999), measure redistribution as the ratio of transfer spending to GDP, a statistic which does not capture the distribution of transfers or the effect of progressive taxation. These papers also relate redistribution to *disposable* income inequality, finding, if anything, a negative relationship - an unsurprising result given that the direct effect of the former is to reduce the latter.

More recent studies, beginning with Milanovic (2000) and including Kenworthy and Pontusson (2005), Mahler (2008), Milanovic (2010), and Ostry et al. (2014), address many of these issues. Utilizing national survey microdata - harmonized across countries and over time - these papers disentangle the market and disposable income distributions, measuring redistribution either as the difference in Gini coefficients or the income share gained by a lower quantile. They find that taxes and transfers do, in fact, play a stabilizing role (i.e. greater inequality is associated with more redistribution).

The renewed interest in distributional issues has generated important investments in data development. The World Inequality Database, for example, is increasingly used to describe trends and analyze issues, e.g. Blanchet et al. (2022). Often they combine survey and administrative data, even augmented to try to be consistent with national income accounts data. Of course, each step requires assumptions. While some harmonization is accomplished, it is not complete, given differences in the survey methods and data collection choices of different statistical agencies make. To create annual time series data, considerable imputations and extrapolations are sometimes necessary. The more countries included and the further back in time, the more pronounced these issues become. What's more, in-kind income, a category which includes the imputed rent from owner-occupied housing and publicly financed healthcare (among other income sources received in the form of goods and services rather than cash), either is excluded to maintain consistency or requires extensive interpolation and extrapolation.

In contrast, our state-level data draws upon surveys or administrative data sets from the same statistical agencies, consists of over 1000 observations, includes the same set of years for each state (in the main analysis without gaps), and accounts for in-kind sources of income. Of course, the data, and our income definition, are not perfect. For example, the underreporting or omission of capital gains is especially important at the very top, while Armour et al. (2014) showed that the use of realized capital gains (based on tax unit data) overstate the increase in inequality. A case can also be made that consumption is a better measure of living standards and permanent income for many households (Attanasio et al., 2010) and/or that longer term measures should be used, or at least added, to the discussion (e.g. the classic book by Vickrey (1947) and the important body of work by Auerbach et al. (2023)). However, consumption data are often less comprehensive than income data in household surveys[5], and in any case, the trends from the 1980's to the Great Recession appear to be fairly similar.[6]

Analyses of the distribution of economic (or more general) well-being must make many choices, including: 1) the unit of account, e.g. households, families, individuals, tax units; 2) the time period of measurement, e.g. annual, multi-year, lifetime; and in any trend analysis; 3) the measure of well-being, e.g. market income, disposable income, consumption, wealth; and how comprehensively to measure it, for analytical, policy, statistical or data availability reasons; 4) the ranking (and possibly re-ranking) of households methodology used; 5) closely related to 1) and 3), adjustments for unit size, cost of living and any trends therein.[7] Of course, any (one or more) of these may significantly affect the measured outcome.[8] The literature contains several excellent analyses of the pros and cons of, and inequality trends in, alternative measures, e.g. Deaton and Muellbauer (1986), Fisher et al. (2013), Meyer and Sullivan (2017), Blank and Greenberg (2008), Slesnick (1993), Hutto et al. (2011), and Rose (2020). We make choices that seem sensible to us, for the purposes of our analysis, given the constraints involved, surprisingly including huge gaps in recent years in available data on Medicaid discussed below.

Also relevant are the recent important attempts to assess the usefulness of the widely used median voter model. For example, Acemoglu et al. (2015) examine a series of factors that lead to different or ambiguous outcomes than the median voter model; Mulligan et al. (2004) emphasize the degree of competition rather than the voting processes; Alesina and Giuliano (2011) explore preferences for redistribution, using social and value surveys of individuals and emphasize personal histories and cultural factors; Marechal et al. (2023) compare realized redistribution outcomes in a cross section of countries and conclude the preferences of the lower social economic groups are most decisive. Our results with a quite different methodology, using US data and instrumenting to account for reverse causality of tax and transfer effects on market income and therefore putatively causal, are more consistent with the median voter model. The difference may be explained in the results of Benabou and Tirole (2006), whose model produces two equilibria, labeled "American" and "European." The former is characterized by the belief that effort pays off and has more laissez faire outcomes, whereas the latter is characterized by pessimism that effort pays off and results in a welfare state.

The paper is organized as follows. Section 2 presents the model and introduces notional definitions of the key variables. Section 3 describes the choice of sample and the income definition in

[5]A laudable exception is the more recent waves of the Panel Study of Income Dynamics, but these data are too limited for our analysis.

[6]However, it appears they diverged (consumption inequality decreasing, income inequality continuing to rise) after 2007; see Meyer and Sullivan (2013). Also see Krueger and Perri (2006) for an interesting attempt to analytically relate the two.

[7]Rose (2018) and Early (2018) discuss many of these measurement issues and their implications.

[8]For example, see the debate between Saez and Zucman (2020) and Auten and Splinter (2021) and Splinter (2020), whose different methods generate a top 1% income share growth differing by a factor of almost five.

greater detail and specifies how each variable is constructed from the relevant data sources. Sections 4 and 5 present the results, and Section 7 supplements the analysis with robustness checks and statistical tests. Section 8 discusses directions for further research, and Section 9 concludes. A full set of sensitivity analyses is presented in the Appendix.

2 The Model

2.1 The Mapping from Market Income to Disposable Income

Let the term 'quintile j' refer to the j^{th} quintile, counting from the bottom, of the corresponding (either disposable or market) income distribution. For example, 'quintile 4' refers to the 61^{st} through the 80^{th} percentiles. The share of *disposable* income - that is, market income after taxes and transfers - received by quintile j at year t in state i, denoted D_{it}^{j} , is given by

$$D_{it}^{j} = \frac{\exp\left(\delta_{it}^{j}\right)}{\sum_{k=1}^{5} \exp\left(\delta_{it}^{k}\right)} \tag{1}$$

Where

$$\delta_{it}^{k} = \alpha_{i}^{k} + \gamma_{t}^{k} + \varphi^{k} + \beta^{k} \cdot M_{it} + \theta^{k} \cdot Y_{it} + \epsilon_{it}^{k} \qquad k = 1, 2, \ldots, 5 \tag{2}$$

and where i indexes states, t indexes years, k indexes equations, M_{it} denotes the vector of market income shares in state i at year t, Y_{it} denotes a vector of control variables in state i at year t, and ϵ_{it}^{k} is equation k's error in state i at year t. The first three terms represent the state dummies, time dummies, and intercept (constant) in equation k, respectively. By construction, $\sum_{j=1}^{5} D_{it}^{j} = 1$.[9]

To avoid multicollinearity, a base quintile must be dropped from M_{it}. Thus, if quintile l is excluded, an increase in $M\left(n\right)$ (the market income share of the n^{th} quintile) represents a transfer of market income share from quintile l to quintile n. In addition, identifying the coefficient vectors α, γ, φ, β, and θ requires setting $\delta_{it}^{k} = 0 \ \forall i, t$ for some k. Without loss of generality, assume $\delta^{5} = \mathbf{0}$.

In summary, there are 20 possible "experiments" in which one of the five quintiles loses market income share to one of the other four quintiles and each of these potentially affects the disposable income shares of each of the five quintiles, so there are one hundred β^{k} coefficients to be estimated. However, as discussed below, the effects of such shifts of market income share from quintile i to quintile j is equal, but opposite in sign, to the effects of shifts from j to i, so there are 50 independent effects.

[9] Given administrative costs in the tax/transfer system and government purchases, the population total disposable income is less than the market income total. Of course, there are potentially large compliance and incentive distortion costs as well.

2.2 Identification

The model is meant to capture redistributive responses to *purely* distributional concerns. However, redistributive fiscal policy is also driven by factors which reflect the absolute state of the economy. Indeed, to a large degree, taxes and transfers are dedicated to providing insurance against adverse economic circumstances, such as old age. To the extent that these factors are associated with the market income distribution, they must enter into the controls Y. For instance, inequality is plausibly related to economic growth, and the *level* of income may independently affect taxes and transfers in any number of ways. Likewise, shifts in the market income distribution may be caused by an aging population or the onset of recession, developments which may mechanically trigger social insurance spending. Thus, the following controls are included in the baseline model and all extensions:

1. Real disposable income per capita, i.e. aggregate disposable income deflated by the population and the local price level.
2. The proportion of the population over the age of 65
3. The cyclical component of the unemployment rate
4. The poverty rate

The first measures overall prosperity, while the second, third, and fourth variables correspond to insurance against old age destitution, the business cycle, and privation, respectively, some of which are potentially endogenous. As indicated above, fixed effects and time dummies also enter the model, and are included in all the results reported below. See Section 3 for more detailed definitions and the data sources used to construct each variable.

Obviously, the taxes and transfers that transform $\boldsymbol{M}$ into $\boldsymbol{D}$ shape work incentives and hence influence $\boldsymbol{M}$ itself. Thus, to account for reverse causality and isolate structural, exogenous changes to the market income distribution, we assume that the explanatory variables are endogenous (all except the share of the population over 65) and use instruments to identify the parameters.

Our instruments fall into one of three categories:

1. Proxies for local labor market shocks attributable to national or international variables
2. Four year (or more) lags of the endogenous variables
3. Forecasts of the endogenous variable based on initial values and national-level trends (of the same variables)

The idea behind the first set of instruments is that national or international developments are plausibly independent of the tax and transfer policies of any U.S. state. If exposure to these shocks varies geographically, any measure of the local impacts should yield consistent estimates. We employ four instruments of this type. The first, based on Bartik (1991), is a projection of state employment growth rates (net of working age population growth) based on state industry composition and national industry trends. The second, following Autor et al. (2013) and Asquith et al. (2017), measures import pressure by relating national imports by industry to each state's share of national industry employment. The third and fourth instruments, inspired by Kehrig and Ziebarth (2017), are interactions of the price of a globally traded commodity (or commodity index) with the share of state employment dedicated to its production.

The rationale for the second category is that the omitted variables which make up the error terms in (2) are essentially political in nature. Given the regular election cycle, it follows that any autocorrelation in the errors should fade over four years, and lags of this depth can be included as instruments without biasing the estimates. This intuition is buttressed by formal autocorrelation tests described in Section 7. The last set of instruments is a substitute for the second set for the purpose of robustness exercises. The approach is taken from Voorheis et al. (2015). For details on the variable definitions and data sources, see Section 3.

Of course, the underlying premise of *all* the instruments (and the model itself) is that redistribution is ultimately or principally determined by voters in the states themselves, whether voting for their state officials or federal office holders. Given that federal taxes and transfers are generally much larger than those of individual states, this proposition may seem dubious. However, state fiscal activity is substantial. In principle, states can 'undo' or 'buttress' national fiscal policy to the extent that federal taxes and transfers are fungible. Moreover, state governments are responsible for implementing federal programs such as Medicaid and TANF (cash welfare) and have wide discretion in determining benefit levels, eligibility, and total spending. Importantly, even at the federal level, U.S. senators and representatives are accountable to state and local constituencies, and state delegations in Congress often work across party lines in the interest of their state.[10] In view of the considerable autonomy of the states, the devolved nature of federal programs, and the representative structure of the U.S. government, the assumption of local agency is not implausible.

2.3 Estimation Procedure

The first step is to take the disposable income shares in each state i and year t and solve for $\left\{\delta_{it}^k\right\}_{k=1}^4$ using (1). Once $\boldsymbol{\delta}$ is computed, the model reduces to the system of four linear equations given by (2). We estimate α, γ, φ β, and θ in all equations simultaneously by the Generalized Method of Moments (GMM).

Once the model is estimated, we compute the marginal effect at mean values of a one percentage point transfer of market income from the base (excluded) quintile to another (considering all four included quintiles in turn) on each of the disposable income shares. By construction, these marginal effects must sum to zero, since the disposable income shares always sum to 1. We note that, over

[10] See Weingast et al. (1981) and Clemens and Veuger (2021), who demonstrate the value of federal representation to states' allocation of federal funding.

our sample, all quintiles experience changes in shares of this magnitude.

The last step is to repeat the process for each base quintile. Simply interchanging the excluded quintile may be redundant in the sense that one set of estimates might be perfectly predictable from the other. Indeed, our baseline model imposes a symmetry condition - the marginal effect of a shift of market income share from one quintile to another is equal and opposite to that of the reverse transfer - along with a host of other cross-system restrictions (see Appendix A). The correspondence arises for the same reason that the choice of whether to include a dummy variable or its complement (e.g. a 'male' or 'female' indicator variables) is irrelevant in linear regression. However, all such restrictions may be relaxed by introducing nonlinearities or using different instruments for each system. Thus, quadratic specifications provide a robustness check (see Section 7), and extensions which include interaction terms are also unconstrained.

3 Data Sources, Sample, and Variable Definitions

We have delayed publication of our analysis, hoping to obtain consistent post-Affordable Care Act data on Medicaid by state by risk group, given the potentially significant effect the expansion had on the post-tax and transfer income distribution. That would also have been a natural test of any lagged or cumulative effect of inequality changes on redistribution. But data were at first unavailable, then delayed, then available only for eleven (unnamed states), then finally released for 2017-2018, but not for 2014, 2015 and 2016. Data on fungible values for Medicaid (described below) were discontinued. BLS added some additional state data, and Census corrected errors in its tax imputations, but we were unable to obtain information on which states had certain data imputed by which models. So we first present our analysis based on the most consistent, uninterrupted, data available. We discuss sensitivity and sample expansion, e.g. to additional state inclusions, various alternative measures for the value of government subsidized health insurance, and (in our view less reliable) results based on data through 2017, using the less accurate measure of Medicaid available, in Section 6 below. Beyond that date, unfortunately, the ASEC no longer includes data on the imputed income from owner occupied housing - a major source of income for a majority of households. Nor does it include data for the large and growing employer contributions for health insurance that cover a majority of the population. And finally it no longer includes data on property taxes.

Some states were dropped in our original analysis due to missing data for important instruments, such as BLS employment data by NAICS. Five of those states are added in the sensitivity analysis. Our interaction with BLS reveals that sometimes they use a statistical model to avoid problems with sample size that leads to the inability to produce a data series. But for privacy reasons, BLS has made a policy decision not to provide information to the public on which series are model based and which are sample based. Therefore, we include these five additional states only as a sensitivity check.

CPS stopped including Medicare/Medicaid data after the 2015 survey year. We explored the Kaiser Family Foundation (KFF), which provided information updated to 2014 based on analysis of the data from the 2014 Medicaid Statistical Information System and the Urban Institute's estimates from CMS - 64 reports. Adjustments were made for 21 states because "spending per enrollee may not match the MSIS data or states own reporting systems."

The Centers for Medicare and Medicaid Services (CMS) finally released per capita spending by enrollment group by state for Medicare, but not for Medicaid. The 2019 Medicaid/CHIP scoreboard shows 2017 average Medicaid spending by state by enrollment group, but only for an incomplete list of states. As for the gaps for 2014/2015/2016, CMS stated "there are ... few Medicaid Public Use Files (PUF) and this very well could be due to state data submission."

3.1 Income Distribution Variables

All income distribution variables (market income shares, disposable income shares, market and disposable income Gini coefficients) are based on survey data from the ASEC March supplement of the Current Population Survey (CPS). The principal advantage of the CPS (and other survey data sources) is its scope; it contains information on both taxable *and* non-taxable sources of income, transfers, and the composition of families and households. Of course, researchers interested in measuring income inequality (especially top income shares) have often eschewed the CPS to avoid censored data - namely, income variables capped by a 'top code' for privacy reasons. However, recent releases apply a 'rank proximity swapping method' which reshuffles reported values in a way that obscures the identities of survey respondents while preserving the income distribution. What's more, the Census Bureau has released revisions based on current methods for survey years dating back to 1975. We utilize these corrections to construct a consistent data series.

Market income includes earnings, capital income[11], retirement/survivors'/disability pensions and annuities, private educational assistance, child support/alimony, the return on home equity, and employer contributions to health plans, among other sources.

Disposable income consists of market income plus transfers less taxes. Transfers include social security income, unemployment compensation, workers' compensation, veterans' payments, public educational assistance, cash welfare (TANF, etc.), disability (SSI), the (federal) earned income tax credit, food stamps (SNAP), school lunches, public housing and rental subsidies, low income energy assistance, and alternative measures, fungible and adjusted average spending, of the values

[11] Capital income is composed of interest income, dividends, rents, royalties, estate and trust income.

of Medicaid, Medicare coverage and employer provided health insurance[12], among other sources.[13] Taxes include federal and state income taxes[14], payroll taxes, and home property taxes but exclude corporate, sales, and excise taxes.[15] Although most variables are drawn directly from the survey, many are imputations provided directly by the Census Bureau.[16] These include all tax variables and most sources of in-kind income.

This income definition is fairly comprehensive. The only major omission is capital gains income, which is notoriously difficult to measure. The ASEC supplement actually includes imputations for *realized* capital gains, but only through 2008. As noted in Piketty and Saez (2003), realized capital gains are highly volatile and may reflect years of accrued income. Armour et al. (2014) attempt to capture both realized and unrealized capital gains in each year, but their approach assumes that all assets of the same type yield the same returns. As in Piketty and Saez (2003), we choose to exclude this source of income but would prefer to use a consistent, reliable series, were it available.

The unit of the analysis is the individual. However, given that households share both income and expenses, an individual's *means* and standard of living depend on the income and composition of the household. To account for this, we compute individual income by summing market and disposable income within households and then dividing the total by the 3-parameter 'equivalence scale' of Betson (1996). Equivalence scales account for returns to scale in consumption - expenses do not increase at the same rate as household size, children consume less than adults, etc. Among the many such scales that have been proposed, Betson's scale has been extensively analyzed in the poverty literature, is thought to best conform to the National Academy of Sciences recommendations for improved measurement, and yields inequality estimates in the middle of the pack.

The last step is to rank individuals according to equivalent income. It is then straightforward to compute income distribution statistics, from income shares to percentile ratios and Gini coefficients.

The resulting quintile shares (at the national level) are roughly consistent with the range of estimates reported in the literature. However, other researchers reasonably use different data sets and make different methodological choices, which can produce nontrivial differences in measured inequality. For comparison, Figure 1 depicts the U.S. distributions of market and disposable income

[12] Fungible values are computed by taking the minimum of the market value of the insurance policy and a family's disposable income after taxes and spending for basic needs (e.g. food and rent). We base values for medical insurance on Finkelstein et al. (2019), 30% of average spending for Medicaid; we use 60% of risk group average state spending for Medicare and 80% for employer provided health insurance, but explore alternative values in section 6. The value of employer provided health insurance has additional factors to consider, e.g. the tax subsidy, the fact that for most people marginal cost is a modest copay and (at least until the ACA) the higher cost on the individual private market.

[13] It is sometimes unclear in the data dictionaries whether a particular income source should be attributed to public assistance, public employment, or private employment. Wherever this is the case, we follow the methodology used in the Census Bureau's own P-60 research series.

[14] These tax variables do not account for credits. However, the data set contains a separate variable for the earned income tax credit.

[15] Accounting for the distributional effects of corporate, sales, and excise taxes would require estimates of substitution elasticities (across factors of production and consumer goods), tax incidence, and heterogeneity in consumer preferences across income percentiles. Hence, such measures would be highly speculative.

[16] Meyer et al. (2020) analyze the accuracy of tax imputations.

in 2013 according to both our calculations and those of the CBO[17], and Appendix E provides a table comparing our definitions with those of some other leading studies (see Table 17).[18]

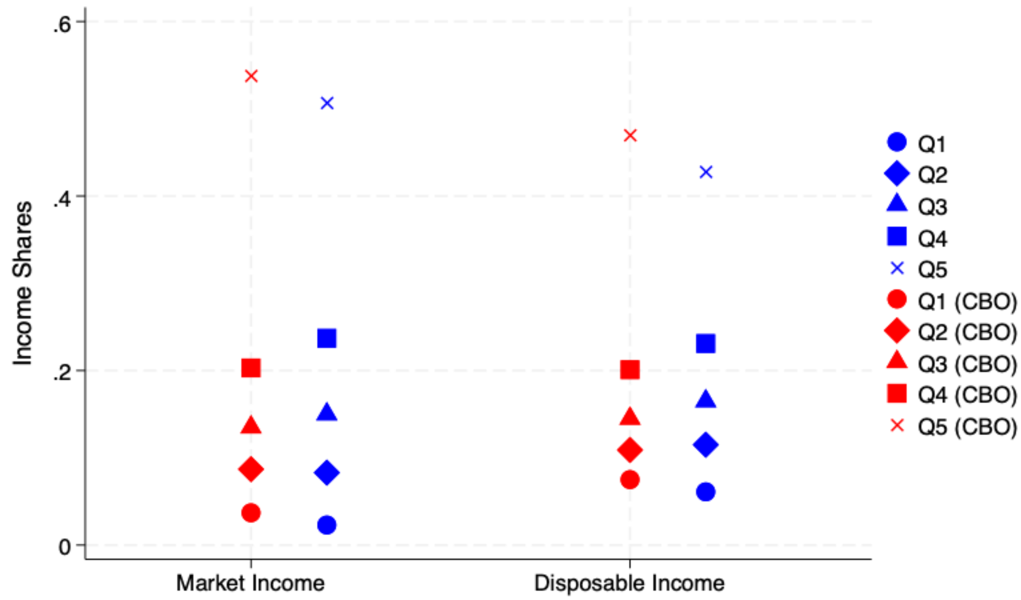


Figure 1: National Market and Disposable Income Shares by Quintile, 2013

3.2 Controls

Real disposable income per capita is computed by deflating a state's aggregate disposable income by its population and the local price level. Aggregate disposable income is obtained from the Bureau of Economic Analysis (BEA), and population statistics are taken from the Census Bureau. Figures are converted from local contemporaneous dollars to 2008-national dollars using the national personal consumption expenditure deflator and state regional price parities (RPP). The BEA provides RPP for all 50 states (and D.C.) from 2008-2013. Imputations of RPP are used for the prior years.

RPP is imputed in two stages. First, we project RPP forward and backward from 2008 using estimates of relative state inflation. One estimate is based on the Federal Housing Finance Agency's (state-level) House Price Index, while another is based on the BEA's Gross State Product (GSP) price deflator series. The projections are scaled so that average state price levels (weighted by GSP) conform to national prices in every year. Next, we regress actual post-2008 RPP on both projections and then use the fitted regression model to impute values prior to 2008. In both the original and extended data sets, RPP ranges from about 0.8 to 1.15.

[17] See Perese (2016).

[18] The CBO column in Table 17 is based on the definition used in Perese (2016) to calculate CBO's income distribution statistics for 2013, which we compare to our own 2013 numbers in Figure 1. Curiously, the CBO reclassifies Medicare and Social Security benefits as market income in a follow-up study for 2014; see Perese (2018).

Our findings are robust to the imputation procedure and the exclusion of RPP entirely, but the results are more pronounced when state pricing data informs the imputation (as above). See footnote 31 in Section 5 for more detail.

Cyclical unemployment is computed by decomposing official (annual) state unemployment rates from the Bureau of Labor Statistics (BLS) into cyclical and structural components using a Hodrick -Prescott filter.[19]

Finally, we compute the share of the population over 65 using the ASEC supplement of the CPS and take the official poverty rate directly from the Census Bureau.[20]

3.3 Instruments

To construct our 'Bartik national demand shock' instrument, we first compute predicted employment growth in each state-year as a weighted average of *national* employment growth by industry, where the weight on each industry is equal to its share of local (state) employment. The difference between these projections and working age population growth is then compounded year-over-year to capture the accrued effects on local labor market tightness. Employment by NAICS industry comes from the BLS, while population data is taken from the Census Bureau. We calculate working age population shares directly using the CPS ASEC supplement.

To construct our 'import shock' instrument, gross national imports in a given industry are allocated to the states according to their share of national industry employment. This 'lost business' is then aggregated across industries and scaled by gross state product (GSP). We use Census Bureau data on national imports by NAICS classification downloaded from trade.gov. As before, we obtain employment by NAICS industry from the BLS and GSP from the BEA.

The two commodity interactions are given by (oil&gas PPI)*(mining employment share) and (agricultural sector PPI)*(farming employment share), where PPI stands for producer price index.[21] We obtain producer prices from the Bureau of Labor Statistics and agricultural, mining, and total employment data from the BEA.

The last set of instruments consists of imputations of the endogenous variables based on movements in the corresponding national-level variables. For the poverty rate and cyclical unemployment, we perform a state-specific regression of the panel variable on the national-level variable and use fitted values. For *mean* market income by quintile as well as real disposable income per capita, the procedure is similar, but the regressions are in logs and fitted values are exponentiated for the final result. Final instruments for the market income *shares* are then calculated by dividing the given quintile's imputed mean income by total imputed mean income (where the total is the sum across

[19] The filter has parameter $\lambda = 1 \times 10^7$.

[20] We account for the fact that the poverty rate may be endogenous. See Section 3.3.

[21] In the NAICS system, mining (21) includes oil and gas extraction (211); mining ,except oil and gas (212); and support activities for mining (213). Empirically, mining employment consists predominantly of employment in oil and gas extraction.

quintiles).

3.4 Interaction Variables

In some of the regressions below, we interact the market income shares with the number of branches of state government controlled by the Democratic party. This variable is defined as follows: the governor's office counts as one branch, an independent in the governor's office counts as half a branch, chambers in bicameral legislatures count as half a branch, and ties count as half a chamber.[22] To construct the variable, we use data on the party affiliation of state governors collected from National Governor's Association website.[23]

3.5 Sample Size

Because the income (and health) questions of the ASEC supplement were redesigned in 2015, our sample ends in 2013. In order to utilize the Census Bureau's imputations for in-kind income, we also only use CPS data after 1990. Finally, due to limitations in data availability, some variables are missing for particular states. However, we are able to construct a balanced panel data set containing all variables for 41 states and the years 1991-2013. This truncated data set is the basis of most of our regressions. We do report in Section 7 analogous results using data updated through 2017 with less reliable post-Affordable Care Act Medicaid data mentioned above.

4 Experiments of Interest

The National Picture

Once the model is estimated, it is possible to compute the marginal effect of a one percentage point transfer of market income share from one quintile to another on the disposable income shares of *all* quintiles. Clearly, any transformation of the market income distribution (by quintile) can be constructed as a sequence of such simple 'experiments.' For instance, Figure 2 presents actual vs. predicted national-level market and disposable income share by quintile in 2013. Between 1991 and 2013, the market income shares of quintiles 1 through 4 fell by 0.36, 1.51, 1.56, and 0.85 percentage points, respectively, and the top quintile gained 4.28 percentage points (pp) in market income share. This history can be represented by the scenario

$$.36\,(Q_1 \to Q_5) + 1.51\,(Q_2 \to Q_5) + 1.56\,(Q_3 \to Q_5) + .85\,(Q_4 \to Q_5) \tag{3}$$

[22] In our data set, there is no case in which one party has a plurality but does not have a majority.

[23] See https://www.nga.org/cms/governors. Information on the control of state legislative chambers comes from the Statistical Abstract of the United States and the website of the National Conference of State Legislatures (Bureau, 2012). In some years, the number of seats held by each party in a given state is unavailable. These gaps are filled in by setting missing values in odd (even) years equal to the values in the subsequent (previous) year on the assumption that elections take place in even years. See http://www.ncsl.org/research/about-state-legislatures/legislator-data.aspx.

Of course, there are infinitely many alternative combinations of Q_i to Q_j transfers which net out to the same pattern of gains and losses (in market income share) across quintiles. However, given the nature of the model, the predicted effects are invariant to the particular construction or sequence of events (for a discussion of path dependence, asymmetries, etc., see Section 8).

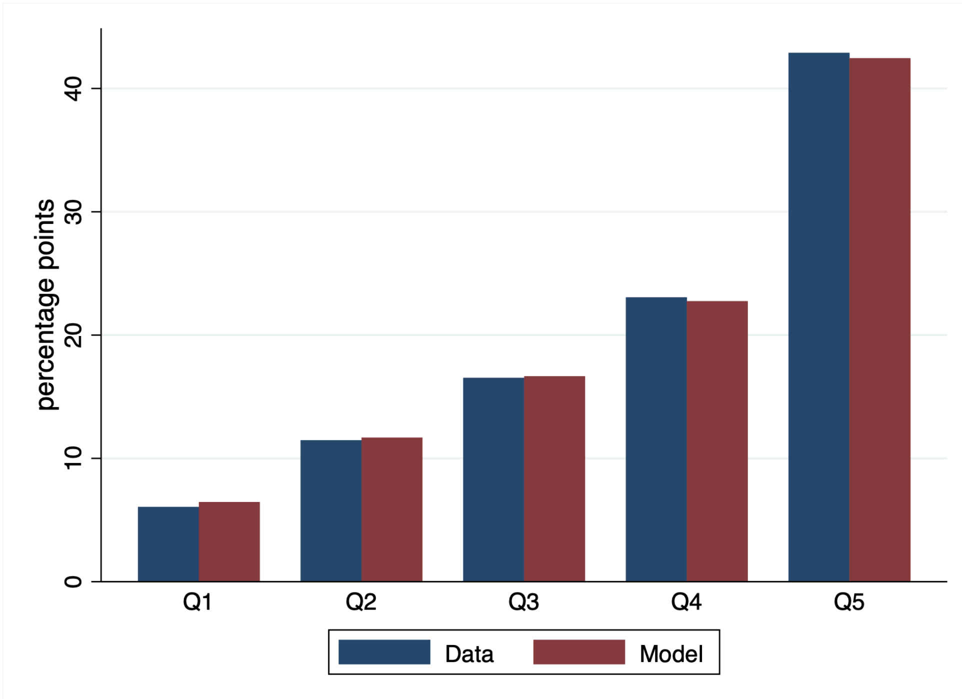


Figure 2: Income Shares by Quintile, 2013 (National Level)

As discussed in Section 2, there are 20 ordered quintile pairs and five effects for each experiment, for a total of 100 marginal effects, although only 50 are independent. Table 1 provides the full set of GMM point estimates in tables organized by the 'giving' quintile (i.e. the base quintile excluded from the regression). Each entry represents the marginal effect, at mean values, of a one percentage point shift of market income share from the base quintile to the row quintile on the disposable income share of the column quintile. For instance, the bottom row in the first table corresponds to the experiment $Q_1 \rightarrow Q_5$, and the -0.908 figure in the bottom left corner indicates that when the top quintile gains 1% of market income share at the expense of the bottom quintile, the disposable income share of Q_1 itself falls by 0.908%. In a sense, the bottom quintile gets 0.092% back after taxes and transfers. Likewise, the 0.727 figure in the bottom right corner indicates that when $Q_1 \rightarrow Q_5$, the disposable income share of Q_5 itself rises by 0.727% . In a sense, the top quintile *gives* 0.273% back after taxes and transfers. We shall refer to this dampening of shifts in market income as *attenuation*.

Plugging the marginal effects of $\{Q_i \rightarrow Q_5\}_{i=1}^{4}$ into (3), the model predicts the 2013 disposable income shares to be 6.46, 11.68, 16.66, 22.75, and 42.45 percentage points for of Q_1 through Q_5,

Quintile	Q1	Q2	Q3	Q4	Q5
Q2	-0.76321***	0.54973***	0.069906	0.51188***	-0.36831***
Q3	-0.82626***	0.34361***	0.27606***	0.13229***	0.074303
Q4	-0.68502***	0.030706	0.085855***	0.93356	-0.3651***
Q5	-0.90803*	0.047941	-0.11299***	0.24646***	0.72662***

Base Quintile = Q1

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.73808***	-0.5562***	-0.065275	-0.4981***	0.38149***
Q3	-0.071919	-0.21191***	0.21398***	-0.37176***	0.44162***
Q4	0.064161	-0.5246***	0.023721	0.42543***	0.011284
Q5	-0.15422***	-0.50693***	-0.17694***	-0.26028***	1.0984*

Base Quintile = Q2

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.81218***	-0.34464***	-0.27824***	-0.128***	-0.061299
Q2	0.070232	0.21248***	-0.21016***	0.37149***	-0.44404***
Q4	0.13818***	-0.31218***	-0.19258***	0.79448***	-0.4279***
Q5	-0.081**	-0.29308***	-0.39***	0.10918***	0.65489***

Base Quintile = Q3

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.68371***	-0.032921	-0.089392***	-0.93147	0.37008***
Q2	-0.078785*	0.52093***	-0.012337	-0.41804***	-0.011772
Q3	-0.15154***	0.31297***	0.1965***	-0.79305***	0.43511***
Q5	-0.22654***	0.016581	-0.19563***	-0.68366***	1.0893***

Base Quintile: Q4

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.89278**	-0.045412	0.11792***	-0.24456***	-0.72072***
Q2	0.15083***	0.505***	0.17741***	0.27804***	-1.1113*
Q3	0.077647**	0.29696***	0.3942***	-0.11566***	-0.65315***
Q4	0.22468***	-0.019924	0.19716***	0.68722***	-1.0891***

Base Quintile: Q5

Table 1: GMM Estimates

respectively, for the country as a whole.[24] Although we use state data to estimate the model, these estimates closely approximate the actual observed values depicted in Figure 2: 6.06, 11.47, 16.53, 23.06, and 42.89 percentage points, respectively. The average absolute deviations between actual and predicted income share is less than one third of a percentage point. Of course, this is a back

[24]These numbers can be reproduced by referring to Appendix C.

of the envelope calculation; whereas the marginal effects above are evaluated at mean values, the full model can account for the level of each regressor and control variable in both 1991 and 2013 (except for the state fixed effects, given the application to national data) and even yields accurate out-of-sample predictions (see Section 7).

A surprising pattern of disposable income share changes over the period arises because Q_5 retains slightly more than transferring quintiles Q_2 or Q_4 ultimately give up. This discrepancy can only be explained by losses for quintiles which neither gain nor lose market income share directly in the experiment. We shall term such effects *spillovers.* For instance, Q_3 is consistently affected when other quintiles transfer market income to Q_5, losing .113, .177, and .196 percentage points when $Q_1 \to Q_5$, $Q_2 \to Q_5$, and $Q_4 \to Q_5$, respectively. Q_1 also shares in losses; its disposable income share falls by .154, .081, and .227 percentage points when $Q_2 \to Q_5$, $Q_3 \to Q_5$, and $Q_4 \to Q_5$, respectively. We believe our study is the first to estimate large, statistically significant spillovers of this kind. Indeed, for the nation as a whole, such spillovers can be larger than the direct effects of tax-transfer adjustments.

As it happens, the shifts in the market income distribution depicted in Figure 2 are largely representative of most state histories. For instance, the market income share of the top quintile increased in all but Minnesota. Likewise, the market income shares of Q_1, Q_2, and Q_3 fell in all but 7 states, 1 state, and 3 states, respectively. A table listing the profile of market income share changes by quintile for all 41 states is provided in Appendix E.

It follows that the set of experiments $\{Q_i \to Q_5\}_{i=1}^4$ are of particular interest. Inspecting the GMM estimates in Table 1, we find that the top quintile keeps most of its gains in these experiments, *except when such gains come at the expense of the middle quintile.* For example, as previously indicated, Q_5 retains .727 pp when it gains 1 pp of market income share at the "expense" of Q_1. Similarly, when $Q_2 \to Q_5$ and $Q_4 \to Q_5$, the increases in Q_5's disposable income share are 1.098 pp and 1.089 pp respectively. In contrast, when $Q_3 \to Q_5$, Q_5 ultimately gains only .655 pp in disposable income share. Among the transferring quintiles (Q_1 through Q_4), *the middle quintile gets the most back* (ultimately losing only .39 pp), the bottom quintile gets the least back (losing .908 pp), and compensation for Q_2 and Q_4 lies in between. As noted earlier, 'spillover' effects (on quintiles which experience no change in their market income shares) are predominantly large and negative, only occasionally positive.

On the whole, the bottom quintile fares far worse, and the top quintile fares far better, than conventional wisdom might suggest. Consistent with the median voter hypothesis and other theories which postulate a special role for the middle class, Q_3 stands apart, both in the extent to which it is compensated as well as its effect on the top quintile. Finally, given Q_5's generally high retention rate and the prevalence of negative spillovers, we may conclude that taxes and transfers stabilize disposable income inequality more by spreading losses rather than redistributing gains.

For the most part, these insights depend critically on the use of instruments to account for endogeneity in the regressors. When the marginal effects are estimated by SUR (presented in Appendix B), Q_5 generally keeps less of its gains (around .75 pp), and the bottom three quintiles all get about .5 pp back when transferring 1 pp of market income to the top quintile. Q_4 is the exception, getting only about a quarter of its loss back, and Q_5 keeping all of its transfer. Thus, relative to GMM, the SUR estimates overstate the degree to which Q_1's losses and Q_5's gains are compensated (perhaps due to reverse causality) and fail to capture the uniqueness of the middle quintile. See Appendix B for the the full set of results. While we would usually from this point on present only our GMM results, we will on occasion below contrast them to analogous SUR results to illustrate the very different interpretation of the political economy mechanism consistent with each.

It is interesting to consider how the marginal effects of $\{Q_i \to Q_5\}_{i=1}^4$ depend on the levels of various interaction terms. Surprisingly, we find that interacting with the current political party in power has virtually no impact. The previous political party in power, taken at a 4-year lag, has a slight skewing towards Q_2.[25] The estimates under full Democratic Party control and full Republican Party control are almost identical. Similarly, past levels of inequality (as measured by the Gini coefficient, at 2 and 4-year lags) have virtually no effect on the marginal effects in any of these experiments.

However, greater inequality two years in the *future* (a proxy for expectations of future inequality) is associated with more positive effects on the disposable income shares of the middle quintiles (Q_2 through Q_4) and more negative effects on the disposable income shares of the tail quintiles (Q_1 and Q_5). For instance, when the bottom quintile transfers 1 pp of market income share to the top quintile, a 4 standard deviation increase (from 2 standard deviations below the mean to 2 standard deviations above the mean) in the market income Gini coefficient 2 years in the future increases the marginal effects on Q_1 through Q_5 by -.05, .03, .03, .04, and -.04 percentage points, respectively. Likewise, when $Q_3 \to Q_5$, the same rise in the future market income Gini increases the marginal effects by -.06, .04, .05, .05, and -.10 percentage points, respectively. Though the impact is modest, expectations of greater inequality in the future seem to shift redistribution toward the middle quintiles.

The *level* of income is a uniquely potent factor. Although the marginal effects of $\{Q_i \to Q_5\}_{i=1}^4$ are largely robust to the other interaction terms, differences in real disposable income per capita can lead to strikingly different model predictions. In particular, higher per capita income tends to increase both the amount that the receiving quintile (Q_5) ultimately keeps as well as the amount that losing quintile ultimately gives up. For instance, when $Q_1 \to Q_5$ and per capita income is 2 standard deviations below the mean, Q_1's disposable income share falls by only .531 percentage points while Q_5's disposable income rises by only .263 percentage points. However, when per capita income is 2 standard deviations *above* the mean, Q_1's disposable income share falls by a full .805

[25] Of course, the sample period is reduced to accomodate the 4-year lag.

while Q_5's disposable income rises by .835.

Likewise, when $Q_3 \to Q_5$ and per capita income is 2 standard deviations below the mean, Q_3's disposable income share falls by only .344 while Q_5's disposable income rises by .796. However, when per capita income is 2 standard deviations *above* the mean, Q_3's disposable income share falls by much more, .455, while Q_5's disposable income rises by roughly the same amount,.761. Likewise, when $Q_4 \to Q_5$ and per capita income is 2 standard deviations below the mean, Q_4's disposable income share falls by only .455 while Q_5's disposable income rises by .765. However, when per capita income is 2 standard deviations *above* the mean, Q_4's disposable income share falls by .865 while Q_5's disposable income rises by 1.33.

At the same time, the effects on quintiles which neither gain nor lose market income share tend to diminish in magnitude as per capita income rises. Diminishing spillovers as per capita income rises suggests *more precisely targeted* redistribution. For instance, when per capita income is 2 standard deviations below the mean, the absolute values of these effects are .120, .156, and .305 when $Q_1 \to Q_5$ and .501, .273, and .389 when $Q_2 \to Q_5$. In contrast, when per capita income is 2 standard deviations *above* the mean, the absolute values of these effects are .094, .102, and .022 when $Q_1 \to Q_5$ and .243, .034, and .287 when $Q_2 \to Q_5$.

Taken together, the implications of the estimates are clear. It appears that, *in our sample*, the conventional wisdom that higher income societies redistribute more does not hold. In fact, the opposite may well occur. For an explanation, see Section 5.

A Broader Set of Experiments

The discussion so far has focused exclusively on the four experiments in which the market income share of the top quintile increases. Widening the scope of the analysis to the broader, but still historically relevant, set of experiments in which any quintile gains market income share at the expense of another quintile, $Q_i \to Q_j$, it is possible to explore more general questions. For instance, do higher quintiles fare worse, as might be expected under a progressive system of taxes and transfers; and more stringently, are these effects monotonic? We discuss transfers to higher quintiles, because as discussed below, the results are symmetric for transfers in either direction between any pair of quintiles.

To maintain comparability, this question must be subdivided into a series of more specific questions. First, fixing the giving (i.e. losing) quintile, do higher receiving quintiles fare worse, and does the giving/losing quintile do better as the receiving quintile's (percentile) rank increases? Examining the GMM estimates in Table 1, we see that for $\{Q_1 \to Q_i\}_{i>1}$ and $\{Q_2 \to Q_i\}_{i>2}$, there is no obvious monotonic pattern as hypothesized when Q_1 is losing. For instance, transfers of market income share from Q_1 to Q_2, Q_3, Q_4, and Q_5 lead to increases of .550, .276, .934, and .727 in the disposable income shares of the receiving quintiles, respectively. However, when Q_3 is the losing/giving quintile, Q_4 does in fact do better than Q_5, retaining .794 as opposed to .655 for the top quintile. As for the effect on the giving/losing quintile, there is no trend for Q_1; transfers of market income share from Q_1 to Q_2, Q_3, Q_4, and Q_5 reduce Q_1's share of disposable income by .763, .826, .685, and .908, respectively. For Q_2, the trend is not monotonic either. For example, Q_2's losses (in disposable income share) are, .212, .525, and .507 when transferring market income to Q_3, Q_4, and Q_5, respectively. Q_3's losses are monotonic but in the reverse direction, increasing from .193 to .39 when transferring to Q_4 and Q_5.

The second subquestion is: fixing the receiving quintile, do higher (percentile) giving quintiles get less back, and does the receiving quintile keep more as the giving/losing quintile's (percentile) rank increases? Again, there is either no trend, or the estimates are monotonic in *reverse.* As indicated above, when Q_5 is the receiving quintile, Q_1 actually gets the *least* back (losing .908), while Q_3 gets the most back (ultimately losing only .39). For $\{Q_i \rightarrow Q_4\}$, Q_3 also does best, losing only .193, whereas Q_1 and Q_2 each lose around .7 and .5 respectively. When Q_3 is the receiving quintile, Q_2 does significantly better than Q_1, losing only .211 as compared to .826 for Q_1. As for the effect on the receiving quintile, Q_5 and Q_4 give back the most by far when receiving market income from Q_3 and Q_2, respectively (not when receiving from Q_1, as might be expected under a redistributionist mechanism), and Q_3's disposable income share rises by roughly the same amount whether gaining market income at the expense of Q_1 or Q_2. This suggests great sensitivity to fortunes of the middle quintiles.

The last subquestion is: do higher quintiles fare worse among those which neither gain nor lose market income share in the experiment? When market income shifts from Q_1 to another quintile, the effects are not monotonic, but Q_5 does poorly among quintiles experiencing no change in their market income share. For instance, when $Q_1 \rightarrow Q_2$, Q_5 loses .368, and when $Q_1 \rightarrow Q_4$, Q_5 loses .365.

In sum, any hypothesized monotonicity does not hold. Otherwise, either *higher* quintiles fare better, or the middle quintile dominates.

5 A Political Economy Perspective

This section takes a more systematic, if more speculative, approach to the analysis of our model. Rather than identify especially salient or relevant experiments and explore them in great detail (as in Section 4), we instead summarize, distill, and aggregate the full set of estimates in various ways in order to make broad inferences about the apparent political economy of responses to income distribution shocks. Hence, generalizations based on unweighted averages of the marginal effects or joint hypothesis tests, etc. may not be an accurate representation of any likely change in the inequality of market income. Nevertheless, the results are interesting and potentially informative, both themselves and for what they imply about political mechanisms. Our estimated marginal effects can be adapted to estimate the effect of *any* alternative hypothesized, or actual, changes in the distribution of market income by simply applying probability weights to any specific set or subset of changes, so long as the weights are non-negative and sum to one. We provide one such example in Section 6.

5.1 Hypothesis Tests

In Section 4 as well as the appendices, p-values for the point estimates (and statistical significance, as indicated by the star-markings) are calculated under the following null hypothesis: for any quintiles l and k,

$$\frac{\partial D^l}{\partial m(k)} = \begin{cases} 1 & l = k \\ -1 & l = \text{base} \\ 0 & \text{otherwise} \end{cases} \tag{4}$$

According to equation (4), the disposable income shares of receiving quintiles rise by one percentage point, the disposable income shares of transferring quintiles fall by one percentage point, and the disposable income shares of all other quintiles remain the same. In other words, shifts in the market income distribution pass through perfectly to the disposable income distribution. Since perhaps the simplest test of the model is whether it can distinguish disposable income share changes from market income share changes, this is a sensible benchmark.

More substantive tests are also possible. For instance, since redistributive responses are largely embedded in the design of fiscal systems, it may be interesting to consider whether and how closely the estimates adhere to some particular fixed policy formula. This can be tested by computing the automatic feedback effects of the given policy formula for each experiment and then setting the null hypotheses equal to these automatic effects. Statistically significant deviations from the null would then reflect either discretionary policy responses (adjustments to policy induced by shocks to inequality or other factors) or a misspecified benchmark (i.e. current law embeds a different policy formula).

For example, such a policy formula might simply take the form of a (possibly progressive) fixed net tax rate schedule, where "net" means taxes net of transfers.[26] This may be one way citizens, politicians, and scholars conceptualize our current system or preferred alternatives. Of course, the automatic feedback effects of such a policy would depend on the particular schedule enacted and the way in which the market income distribution evolves. Whatever the details, however, the 'cross-effects' should be approximately zero. That is,

$$l \notin \{k, \text{base}\} \Rightarrow \frac{\partial D^l}{\partial m(k)} \approxeq 0 \tag{5}$$

In words, under a fixed net tax schedule, if a quintile's market income share neither rises nor falls in the experiment, its disposable income share should be roughly unchanged. This can be

[26] Net tax rates are calculated by subtracting transfers received from taxes paid and dividing by total income.

shown formally, but the intuition is simple. Disposable income is market income after net taxes.[27] Therefore, if an experiment involves neither a change in a quintile's market income share nor its net tax rate, its disposable income share will be unchanged, ignoring small differences related to fluctuations in the average net tax rate across all households (i.e. the ratio of total disposable income to total market income, reflecting the overall burden of the tax/transfer system).

As for the rest of the effects, a full set of null hypotheses is possible with an additional assumption. As shown in Appendix F, if tax rates are not only constant over time but uniform within quintiles, the automatic effect of the policy corresponds to

$$e_k^l = \begin{cases} 1 & l = k \\ \frac{-D^k}{D^l} & l = \text{base} \\ 0 & \text{otherwise} \end{cases} \tag{6}$$

where e_k^l is the elasticity of quintile l's disposable income share D^l with respect to quintile k's market income share $m(k)$, defined as $\frac{m(k)}{D^l}\frac{\partial D^l}{\partial m(k)}$.

Appendix C.2 provides estimates of the elasticities for all 20 possible experiments and reports p-values, for all 100 resulting estimates with respect to (6). Nearly all are statistically significant at the 1% level, and the null hypothesis is jointly rejected. The results provide clear evidence that a more complex process is at work. Either taxes and transfers do not conform to a simple fixed net tax schedule in practice, or market income inequality changes induce discredtionary redistributionist policy changes over time, which seems more than plausible.

5.2 Attenuation, Cancellation, and Spillovers

Definitions

Each set of regressions produces 100 point estimates (5 alternative losing quintiles, times 4 different potentially gaining quintiles, times 5 affected quintiles). To help distill the results, we combine and synthesize the marginal effects in various ways and supplement each set of estimates with tables of summary statistics. Again, because the 'experiments' are not all equally likely, these summary statistics should be taken with a grain of salt. Nevertheless, the results are noteworthy.

[27] Let d_i, m_i, and τ_i be quintile i's *ex ante* disposable income share, market income share, and average net tax rate, respectively, so that

$$d_i = \frac{(1-\tau_i)m_i}{1-\bar{\tau}}.$$

Let d_i', m_i', and τ_i' be the corresponding values *ex post*. If quintile i neither gains nor loses market income share in the experiment and $\tau_i = \tau_i'$, then

$$d_i' = \frac{1-\bar{\tau}}{1-\bar{\tau}'}d_i$$

where $\bar{\tau} = \sum_i \tau_i m_i$ and $\bar{\tau}' = \sum_i \tau_i' m_i'$ are the weighted average tax rates ex ante and ex post, respectively. In most applications, $\frac{1-\bar{\tau}}{1-\bar{\tau}'}$ is approximately equal to 1.

The first step is to record the *benefits, wins, losses, attenuations, cancellations,* and *spillovers* associated with each experiment. We then average these values over all 20 possible gain/loss pairs and report the results in summary tables.

The terms are most easily defined by considering an example. Suppose quintile 4 gains 1% of market income share at the expense of quintile 2. We can represent this in the following way:

	Q1	Q2	Q3	Q4	Q5
Mkt		-1		1	

Table 2: Market Income Share Changes, %

Suppose further that, according to the model, the effect on the disposable income shares is:

	Q1	Q2	Q3	Q4	Q5
Disp	-0.2	-0.5	0.1	0.4	0.2

Table 3: Disposable Income Share Changes, %

Comparing disposable income share changes to market income share changes, it is clear that quintiles 2, 3, and 5 gain from the tax-transfer system, while quintiles 1 and 4 lose out. The *benefits* reaped by quintiles 1 through 5, measured by subtracting the 'Mkt' vector from the 'Disp' vector above, are -0.2, 0.7, 0.1, -0.6, and 0.2, respectively. If a quintile's benefit is positive and statistically significant, it is said to have *won.* Likewise, if a quintile's benefit is negative and statistically significant, it is said to have *lost.*

To the extent that redistribution is the outcome of a political contest, *benefits, wins,* and *losses* provide an easy way to score the results. However, the figures provide little insight into the underlying process. To understand the mechanics and uncover any patterns, we use *attenuation, cancellation,* ancd *spillovers* to deconstruct the transformation of market income share changes into disposable income share changes at a more generic level.

Attenuation denotes the degree to which initial (market) gains and losses are ultimately compensated. For instance, in the example above, Q_2 gets 0.5 pp of its initial 1 pp loss back, while Q_4 gives 0.6 pp of its initial 1 pp gain back. Thus, attenuation is 0.5 pp for Q_2 and 0.6 pp for Q_4. Because the estimates are highly symmetric - in the sense that the marginal effects $Q_i \to Q_j$ are approximately equal and opposite to that $Q_j \to Q_i$ - the greater a quintile's attenuation on average, the more stable its share of disposable income. Thus, attenuation may be interpreted as a measure of the level of insurance provided by the tax-transfer system - the sensitivity of redistributive fiscal policy to a particular quintile's income share.

Spillovers are effects on quintiles whose market income shares are unchanged in the experiment. We attribute positive spillovers to receiving quintiles and negative spillovers to transferring/losing quintiles in the accounting. For example, in Table 3, .3 percentage points of Q_4's gains spill over to Q_3 and Q_5, while .2 percentage points of Q_2's losses spill over to Q_1. As emphasized in equation (5), spillovers are difficult to reconcile with static or fixed net tax schedules, so large spillovers provide evidence of more complex policy formulas or 'active' policy responses.

Cancellation is defined as the difference between attenuation and spillovers. The idea is that attenuation can be achieved in two ways: by spreading gains and losses to other quintiles (spillovers), or by reversing/blunting the original shift in the market income distribution (cancellation). For instance, the transition from Table 2 to Table 3 can be decomposed as follows:

	Q1	Q2	Q3	Q4	Q5
Mkt		-1		1	
Disp_0		-0.7		0.7	
Disp	-0.2	-0.5	0.1	0.4	0.2

Table 4: Two Steps from Market to Disposable Income

The second row of Table 4 is formed by scaling down the first row (market income share changes) so that the total income share transferred matches that of the third and final row (disposable income share changes). Taking 'Disp_0' as an intermediate stage on the path from 'Mkt' to 'Disp', the table represents the following sequence. First, the initial shift in market income is offset by .3 percentage points. This is cancellation. Next, the remaining .7 percentage points of transfers is chopped up and reallocated among all the quintiles, producing spillover effects. Clearly, attenuation is the sum of cancellation and spillovers. However, as the breakdown above makes clear, the two sources of attenuation have very different implications. Cancellation simply truncates the initial shift in market income and hence has an unambiguously stabilizing effect. In contrast, spillovers scramble gains and losses, transforming the disposable income distribution in the process.

Results

We begin our analyses with the SUR estimates, again given the stark contrast in potential political economy mechanisms with our GMM estimates. Figure 3 presents average attenuation by quintile under SUR estimation, including the breakdown between cancellation and spillovers. Throughout, we refer to this diagram as the *attenuation structure*. Inspecting the figure, attenuation is greatest and roughly constant (at .6 pp) for the bottom three quintiles, before falling off dramatically for quintiles 4 and 5. In a sense, quintiles 1-3 are the most 'protected' by redistributive fiscal policy. As for how this attenuation occurs, the breakdown is roughly split between spillovers and cancellation. Thus, shifts in market income share are directly offset as much as they are scrambled and dispersed and (indeed, gains and losses to Q_5 are barely shared).

The results under GMM estimation (where we use the instruments to identify shocks) are quite different. The attenuation structure depicted in Figure 4 reveals a striking parabolic shape in which

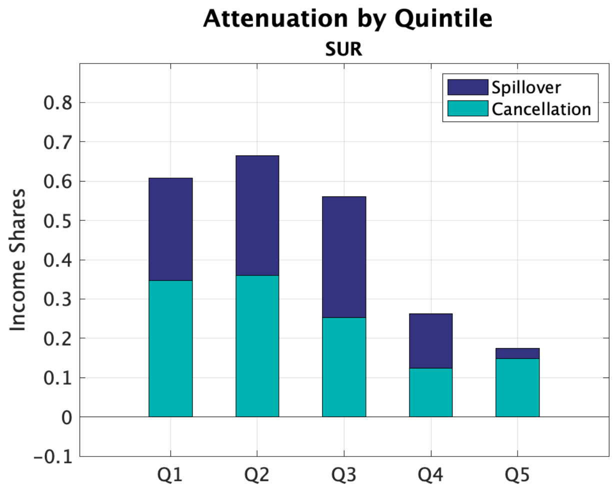


Figure 3: The Attenuation Structure

attenuation peaks for Q_3. Thus, redistributive fiscal policy is most sensitive to the fortunes of the middle quintile in particular (as opposed to the poorest quintiles more broadly). The sources of attenuation also differ; in contrast to the SUR results, spillovers dominate cancellation. In other words, the government moderates market income share changes primarily by spreading gains and losses across the income spectrum. Marked disparities in the treatment of different quintiles are mostly due almost to spillover effects. We label this result the *greedy median voter* effect. This basic attenuation structure and the other qualitative results are robust to the inclusion of quadratic and interaction terms, estimation with clustered standard errors[28], and replacement of lagged values with imputations of the endogenous variables in the set of instruments.[29]

In essence, the contrast between Figures 3 and 4 illustrates the following: while the SUR estimates are roughly consistent with the conventional redistribution hypothesis, the GMM estimates are more in line with political economy median voter theory. Based on the SUR estimates, the government provides insurance to the poorest quintiles, and taxes and transfers counteract trends in the market income distribution. This accords with how redistribution policy is often understood or conceived. Using the GMM estimates, however, the middle quintile is more protected than any other; changes in the income share of the middle quintile induce the greatest (compensatory) policy response. This is more consistent with the median voter hypothesis and what Stigler (1970) calls Director's Law: redistributive politics favors the middle class at the expense of the rich and poor. At the same time, large spillovers indicate a high degree of flexibility in the political system/process, a broad feature of the voting and bargaining models in which inequality concerns shape legislation.

[28]Because estimation of the system is carried out in two steps and makes use of an optimal weighting matrix, the point estimates depend on the choice of standard errors.

[29]The attenuation structure is also robust when the five additional states are added, although very slightly skewed towards the bottom with quadratic terms.

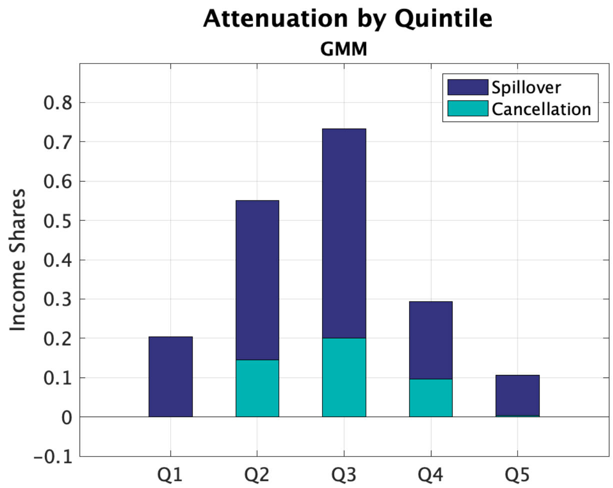


Figure 4: The Attenuation Structure

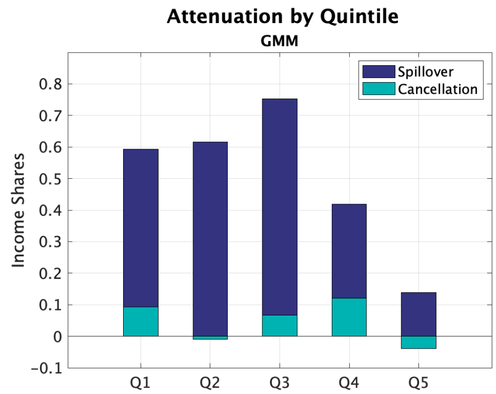
Either policy is highly multidimensional and complex in design, or shifts in the market income distribution trigger 'active' policy changes over time.

It is interesting to consider how these results vary with economic conditions. To supplement the analysis, we separately interact lagged market income Gini coefficients, led market income Gini coefficients, real disposable income per capita, and the number of branches of state, and federal, government under Democratic party control with the market income shares and estimate the marginal effects at different points of evaluation.[30]

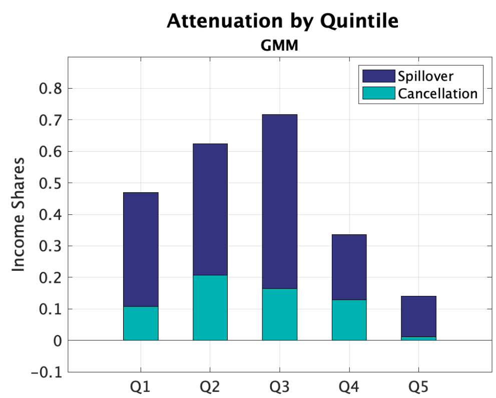
Throughout, the attenuation structure is remarkably stable. Although cancellation can exhibit substantial dependence on (levels of) the interaction terms, the effects are largely offset by countervailing changes in spillovers. The only major exception is real disposable income per capita, which has dramatic effects on attenuation. Figure 5 illustrates the evolution of the attenuation structure. When average income is low, attenuation is high, especially for the first quintile - the parabolic shape breaks down. As average income increases, attenuation falls across the board, plummeting for Q_1. In our sample, it appears that richer societies demand less insurance, especially for the bottom quintile.[31] This is consistent with the notion that the absolute, rather than relative, level of income is the primary source of voter concern. We conjecture that this considerable negative relation between redistribution and the level of real income may be due to the political pressure

[30]Like most of the literature, we use the Gini coefficient as our measure of spread. There are alternatives, such as the Atkinson and Theil indices, but any discrepancies in measured *levels* of inequality typically do not carry over to measured changes (more relevant to this analysis). This is evident in data provided by the Census Bureau's annual income and poverty (P-60) reports, which contain a comprehensive set of inequality measures for the United States going back to 1967. See Semega et al. (2017), Table A-2.

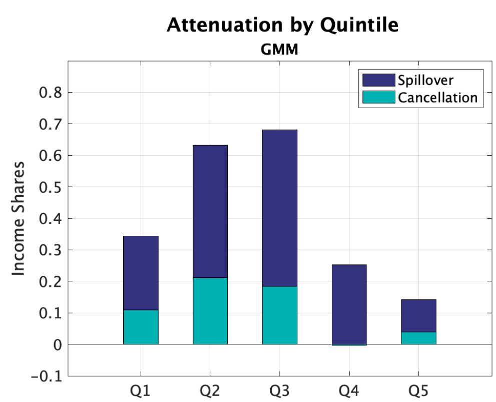
[31] This result, and the basic shape of the attenuation structure, is fairly robust to how real income is defined. Using a simple imputation procedure which substitutes the 2008 BEA relative price parities for all prior years, or dropping RPP entirely, both Q_1 and Q_2 see declines in attenuation as real income increases.



(a) Attenuation Structure: Low Income



(b) Attenuation Structure: Moderate Income



(c) Attenuation Structure: High Income

Figure 5: The Effect of Per Capita Income on Redistribution

reflecting voters' greater concern with the level of low income than inequality per se. This result may seem surprising because, according to conventional wisdom, higher income states like New York and California tend to tax their own residents more and provide more state funding to safety net programs. However, this pattern is not apparent in the data. For instance, rankings of U.S. states based on real disposable income per capita, on the one hand, and state and local tax burdens, on the other, have a correlation of only 0.1. Misconceptions may stem from a failure to recognize that nominal incomes are a poor guide to *real* incomes. After adjusting for the cost of living, household size, and higher effective federal tax rates - federal income tax rates are applied to income that is not spatially indexed - California and New York actually lie on the bottom half of the income distribution. Indeed, California's high housing costs make it the state with by far the largest fraction of the population – one in five – living in poverty.[32]

In any case, our results are not inconsistent with this hypothesis. Even if higher income states do, in fact, redistribute more, to the extent that this reflects different preferences for redistribution (due to culture, the red-state/blue-state divide, etc.), this cross-sectional variation is already captured by state fixed-effects. Moreover, our results concern overall or net redistribution, not state policy alone. As noted above, federal income taxes are applied to tax bases that do not reflect geographic cost of living differences; and the same is true for the bulk of transfer income, e.g. social security benefits. To a large degree, local taxes and transfers may be a response to the effect (or lack thereof) of federal programs. Indeed, the impact of federal policy varies considerably across states. Table 5 reports the results of a regression of the federal transfer-income ratio (total federal transfers over total disposable income in the given state-year) on relative state income, where relative state income is defined as real disposable income per capita, scaled by the average (across states) in the given year.[33] Since federal transfers are largely directed to senior citizens (through Social Security and Medicare), we control for the share of the population over 65. Still, the effect is about -0.12 and significant at the 1% level. This means that for every 10% increase in per capita income (relative to the mean), the share of federal transfers in state income falls by 1.2%. Given that relative state income ranges from .80 to 1.50 in the sample and the mean federal transfer-income ratio is .171, this is quite a large effect. Thus, to the extent that higher income states do, in fact, have more generous transfer programs or more progressive tax codes, this may only partially compensate for less federal redistribution.

This negative effect of real income growth is precisely estimated for our sample of U.S. states over recent decades. While the real income range is substantial, the low end – the poorest states at the beginning of the sample period – is still a quite high real income by international and historical standards. It is, of course, quite possible that the relationship is different for low and middle income ranges, as for example in the famous inverted u-shape of the Kuznets curve relating inequality and income.[34]

Turning to the remaining interaction terms, we use both 2 and 4-year lags and leads of the market

[32] See Fox (2017).

[33] Data on total transfers and total disposable income by state come from the BEA, while variables for state government spending on social insurance and "public assistance and subsidies" are taken from the Census Bureau.

[34] See Kuznets (1955).

VARIABLES	(1) Federal Transfers (Share of Income)	(2) Federal Transfers (Share of Income)	(3) Federal Transfers (Share of Income)
Relative State Income	-0.131***	-0.108***	-0.119***
	(.0105874)	(.009007)	(.0080281)
Population over 65		0.992***	0.811***
		(.0505931)	(.0473966)
Constant	0.305***	0.167***	0.199***
	(.010747)	(.0114679)	(.0104867)
Observations	943	943	943
R^2	0.141	0.360	0.532
Year FE	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Relative State Income v. Federal Share of Transfer Spending

income Gini coefficient as proxies for inequality trends. As *past* inequality increases, cancellation rises for the bottom two quintiles and falls for the top three quintiles. However, the attenuation structure is mostly unchanged.[35] One possible explanation is that greater inequality has a lasting impact in the form of 'automatic stabilizers' more sensitive to lower quintiles (increased cancellation for Q_1 through Q_3), but in the event of actual shocks, the effect is almost completely undone by ad hoc policy changes (as evidence by spillovers). Interacting instead with the led Gini coefficient, attenuation does in fact rise for Q1 and Q2, but the effect is modest.Thus, expectations of future inequality make policy (somewhat) more sensitive to the extremes of the income distribution.[36]

Finally, we interact the market income shares with both 2-year and 4-year lags of political party in control. Looking back two years, attenuation is mostly unchanged as power shifts to the Democratic party. However, cancellation increases across the board. As above, one possible interpretation is that policy sensitivity increases uniformly as Democratic party control increases, but the effect is small, and offset by spillovers. Redistribution does becomes more skewed to Q_2, possibly reflecting a lower income median voter for Democrats.[37]

Looking back four years, there is no obvious pattern moving from Republican party control through divided government. However, any effect is largely offset by the adjustment of spillovers;

[35] However, with the change in sample period resulting from the 4-year lag, the attenuation structure is slightly skewed towards Q2.

[36] This perhaps suggests as voters expect future inequality to increase, pressure builds for redistributionist attenuation. This is one reason we are disappointed that consistent Medicaid data beyond 2014, reflecting the expansion under the ACA, has not become available for us to test for this effect.

[37] Dropping 2 more years resulting from the 4-year lag, the attenuation structure is also slightly skewed towards Q2.

attenuation rises for Q_1 and falls for Q_5, but only slightly from small levels. Thus, as Democratic party control increases, redistribution becomes more sensitive to the lower tail and less sensitive to the upper tail, but the net effect is small.

Overall, the state political party in power matters far less than conventional wisdom might suggest. When we incorporate alignment with the political party in power in Washington, the result is slightly more redistribution when Democrats are in full control. This is consistent with the findings of Clemens and Veuger (2021) on the distribution of federal funds in the March 2021 "American Rescue Plan," when Democrats controlled the executive and both legislative branches.

5.3 Coalitions

In the classic models of the political economy of redistribution (e.g. Meltzer and Richard (1981)), voters differ on a single characteristic - productivity in atemporal models, age in the social security and public debt models focusing on intergenerational redistribution. Other sources of population heterogeneity are ignored. As a result, individuals can be straightforwardly ranked along a single axis, and equilibrium is generally determined by the preferences of the median voter. However, this assumption precludes the possibility of specially targeted taxes, benefits and net transfers capable of building (or, conversely, blocking) a winning coalition. The classic papers of Dixit and Londregan (1995, 1996) analyze precisely these incentives in detailing when political parties (in our terms, voting coalitions) will target groups and which groups will be targeted. An elaboration of the simple median voter theorem points to the population where it is easy, i.e. not very costly, to pick up votes, even while potentially decreasing the net benefit surplus to some members of the winning coalition. We discuss coalition formation in this spirit below, understanding that it is conjectural, as 1) while the numbers are equal across the five quintiles, they may have different abilities or propensities to vote; and 2) voters may have distributional preferences that affect their voting behavior.[38]

A quintile is said to be a member of a *voting coalition* if it has won and the number of wins is three or more. The idea is that quintiles (representing distinct income groups) may band together to expropriate the other quintiles which are not in the majority.

If the marginal effects on two quintiles share the same sign (with estimates statistically significantly different from 0), they are said to be part of an *insurance coalition*. This notion of cooperation is based on the assumption that reciprocal transfers of market income produce equal and opposite marginal effects. In that case, if the disposable income shares of two quintiles increase when, say, $Q_i \rightarrow Q_j$, then the disposable income shares of the same two quintiles *decrease* when

[38] We use the term more loosely than in more formal leading game theoretic political power models as in Acemoglu et al. (2006). And we do not begin to claim any elegant properties of weakly dominating equilibria or core properties of winning coalitions, e.g. satisfying a power constraint and an enforcement constraint that rules out any sub-coalition of the winning coalition from being self-enforcing. We do share a belief that framing these issues in game theoretic terms is potentially insightful, which is why we include it here.

$Q_j \rightarrow Q_i$. Thus, members of insurance coalitions share both gains and losses, stabilizing their disposable income shares. In the example of Section 5.2, the two insurance coalitions are $\{Q_1, Q_2\}$ and $\{Q_3, Q_4, Q_5\}$.

The summary tables which accompany each set of estimates include average coalition membership rates by quintile. We also compute the frequency with which each pair of quintiles appears in a coalition together and report the data in frequency matrices. Of course, the experiments are unweighted and not equally likely in historical experience, but the results are interesting and informative nonetheless. We view them as shedding some modest light on the question of ex-ante potential coalition formation if market income shifts are considered approximately equally likely.

Appendix B contains the full set of summary statistics under SUR estimation. Q_5 has a low membership rate in insurance coalitions, while the remaining quintiles 1-4 all appear in the most frequent pairs. As for voting coalitions, Q4 has the highest membership rate, and the most frequent pairings are $\{Q_1,Q_4\}$ and $\{Q_4,Q_5\}$. Using the GMM estimates in Table 1, however, Q3 has the highest membership rate in insurance coalitions, and the most frequent pairings are $\{Q_1,Q_3\}$, $\{Q_2,Q_3\}$, and $\{Q_2,Q_4\}$. The result re-enforces median voter type of reasoning. Different from the SUR estimates, the highest membership rate in voting coalitions are shared among multiple quintiles, and multiple pairings are tied for the most frequent pairing. Because these patterns are fairly diffused and the analysis speculative, we do not draw any firm conclusions, but rather relay the analysis as potentially an important avenue for future research.

In general, the results reinforce the impressions drawn from the analysis of attenuation, cancellation, and spillovers in Section 5.2. In particular, the SUR estimates are more consistent with the redistribution hypothesis. Redistribution only provides insurance to the lowest quintiles, and the highest quintiles are more reliant on broad 'voting coalitions' to extract gains from the tax-transfer system (they only benefit when other quintiles do). As before, the GMM estimates accord more with political economy theory. The middle quintile is the most protected, consistent with the median voter hypothesis. Moreover, it is the bottom quintile which seems to lack political power under GMM estimation - the quintile most dependent on voting coalitions is Q_1, not Q_5.

6 Sensitivity Analyses

As we noted above, there are a series of sensitivity analyses and sample extensions, each with its pros and cons, that we believe merit discussion. In particular, we focus on an alternative measure of the value of in-kind health insurance and extending the sample for several more years and to a few additional states (despite data comparability issues). We emphasize the robustness of our major basic results: the uniqueness of the greedy median voter, the parabolic (in quintile) attenuation structure, the effect of real income on redistribution, the effect of expected future inequality and the striking differences in results and implications for different political economy mechanisms between the instrumented (GMM) and uninstrumented (SUR) estimates. We also point out a few of the large number of sample and measurement combinations that result in qualitatively nontrivial differences, which usually occur in extreme cases.

First, alternative measures of medical insurance value were tried. For example, the Census produced "fungible" values, defined as the amount of budgetary resources freed up beyond food and housing costs. Researchers e.g. Burkhauser et al. (2013), explore how such a valuation of Medicaid and Medicare affects the distribution of income. An excellent survey of the pros and cons can be found in Kaestner and Lubotsky (2016). Using fungible values, our basic results held; most importantly, the parabolic shape of attenuation, peaking in the middle quintile, and the sizable negative real income effects, but of course, there were minor differences. We also tried different quantitative adjustments to average spending for Medicaid, Medicare and employer provided health insurance from our base case discussed in Section 3.1. Again, the *qualitative* results were unchanged, although some detail moved around depending on which measure was employed; the larger the imputed value of health insurance, the lower the base degree of inequality, consistent with Burkhauser et al. (2013).

The most important sample extension was to extend the period through 2017, when data on Medicaid became available, but only for total spending by state, *unadjusted* by risk group. Given considerable differences across states of both the Medicaid base group and, of course, between states that expanded Medicaid post-Affordable Care Act adoption and those that did not, we summarize the qualitative results here and include the baseline SUR and GMM attenuation by quintile in Figure 6 below. Again, the model estimates closely approximate the actual observed values. Likewise, the middle quintile gets the most back of any of the transferring quintiles Q1-Q4. Again, the top quintile keeps most of its gains but now Q1 does slightly better. Spillover effects remain predominately large and negative. The bottom quintile fares worse, and the top quintile better, with the instrumented GMM results compared to the SUR results. Importantly, consistent with the median voter hypothesis, Q3 stands apart, both in the extent to which it is compensated as well as its effect on the top quintile. Compared to the baseline results using data through 2013 with risk-adjusted Medicaid data, median voter effects are slightly stronger, and Q1 is also slightly better protected. For Q3, cancellation increases, but the main story, the greedy median voter, is still true. Taxes and transfers stabilize disposable income inequality more by spreading losses rather than redistributing gains. For the most part, these insights depend critically on the use of instruments to account for endogeneity in the regressors.

While interacting with the current political party in power has surprisingly little impact, there is a slight shift toward Q2 with lags. The effects of led inequality continue to be positive on Q2, Q3 and Q4. Higher per capita income generally increases the amount Q5 keeps as well as the amount the losing quintile gives up, except for Q3 transfers to Q5, another example of the median voter effect. The effects on quintiles which neither gain nor lose market income share also tend to diminish as per capita income rises. The conventional wisdom that higher income societies redistribute more also does not hold in the results expanded to 2017, although obviously our data are for the rich

United States, where those in even relatively poor states are well off by historical and international standards.

Turning to our political economy framework discussion, the hypothesis tests reveal that nearly all marginal effects are statistically significant at the 1% level, and the null hypothesis that direct effects are 1 and -1 for the disposable income shares of the receiving quintiles and transferring quintiles, respectively, and zero for the disposable income shares of all other quintiles, is jointly rejected. The results also hold for the more relaxed "fixed" net tax schedule, based on elasticity estimates as described in equation (6) above.

Interacting with the LED GINI coefficient increases attenuation for Q1 through Q4. With the four-year lag, there still is no pattern of Republican party control through divided government.

For the SUR estimates, the attenuation structure is roughly constant for the bottom three quintiles but falls off dramatically for Q4 and Q5, and is roughly spilt between spillovers and cancellations and is robust to a quadratic extension and the use of clustered standard errors. Ditto for the estimates adding the five states.

Most importantly, extending the GMM estimates through 2017 continues to reveal the striking parabolic shape in which attenuation peaks markedly at the middle quintile, Q3, and spillovers dominate cancellations overall, but are concentrated at Q2 and Q3. This basic attenuation structure and other qualitative results are robust to interaction terms, estimation with clustered standard errors and replacement of lagged values with imputations of the endogenous variables in the set of instruments. Ditto when the five additional states are added.

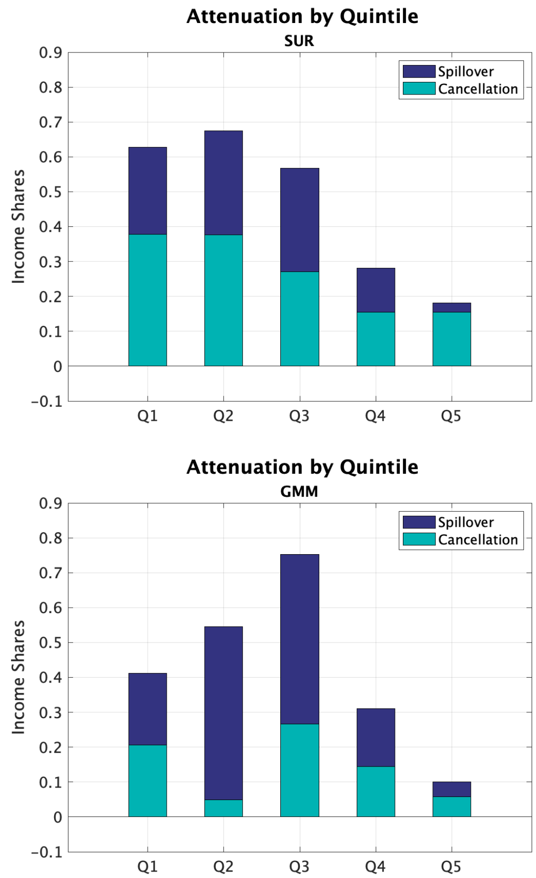


Figure 6: The Attenuation Structure, 1991-2017

In summary, adding the four additional years (which required compromises in the measure of the value of medical insurance)[39], either alone or when adding the five additional states, still reveals that the un-instrumented estimates are roughly consistent with the conventional redistribution hypothesis, the government provides insurance for the poorest quintiles, perhaps how redistribution policy is often understood or conceived.

However, instrumental variable (GMM) estimates, accounting for endogeneity and reverse causality, are putatively causal and provide a strikingly different result: the middle quintile is the most protected; changes in its income share induce the largest policy response, consistent with Director's Law that redistribution always favors the middle class.

Whether just extending through 2017 or also adding the five states, the attenuation structure is quite stable. The effects of varying cancellation depend on the levels of the interaction terms, but these are largely offset by spillovers. Again, the only exception is real disposable income per capita,

[39]In addition, there is the separate issue of whether the ACA expansion of Medicaid to the "near poor" might involve a population with a different marginal willingness to pay than estimated for the traditional Medicaid population by Finkelstein et al. (2019).

which has the qualitatively the same sizable effects on attenuation.

Finally, overall the state political party in power, whether with a two or four year lag, seems to matter less than conventional wisdom might suggest. In the insurance coalitions, Q3 and Q4 have the highest membership rate.

Last but not least, the marginal effect estimates provided in this paper are synergistic with future works that study either realized or hypothetical shifts in the market income distribution. Although in our discussion of the overall attenuation structure we have focused on the case where all possible "experiments" of redistributing market income shares from one quintile to another are equally likely, heterogeneous weights could be given to each of the 100 experiments to study the consequences of any specific event associated with particular changes in the market income distribution. As discussed in Section 5, we can use the (symmetric, but opposite in sign) matrix of marginal effects of all potential shifts in market income from quintile i to quintile j on each of the five quintiles to explore other potential and actual historical scenarios.[40] Applying a "weighting matrix" of non-negative weights summing to 1 will yield the mapping to the changes in disposable income. For example, if we use the relative weights from the example in section 4 on "the national picture," from the shifts of each of the bottom four quintiles to the top quintile, with all other potential shifts assigned a weight of zero, and aggregate the resulting effects on the disposable income distribution, Figure 7 below still yields the striking parabolic shape with GMM estimation we emphasized in Sections 5.

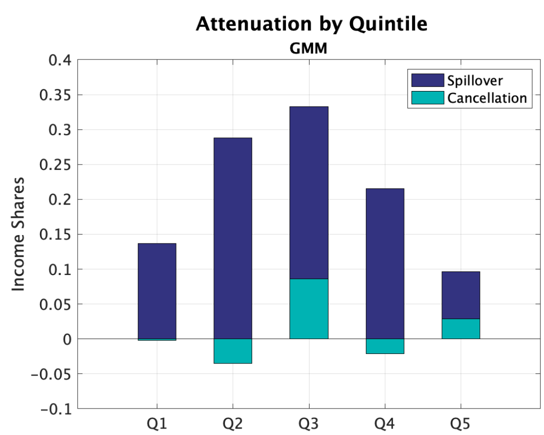


Figure 7: The Attenuation Structure, National Picture

[40]The 100 marginal effects reduce to 50 independent effects, given symmetry; the effects of any shift in market income from quintile i to quintile j are equal but opposite in sign to those from j to i.

7 Measures of Fit, Statistical Validation, and Robustness Checks

Because the disposable income distribution is highly correlated with the market income distribution, the model produces very accurate predictions. As illustrated in Figure 8, using quintile 4 in Alabama as an example, fitted values closely track actual values. Indeed, the R^2 of the regression is greater than 99%.[41]

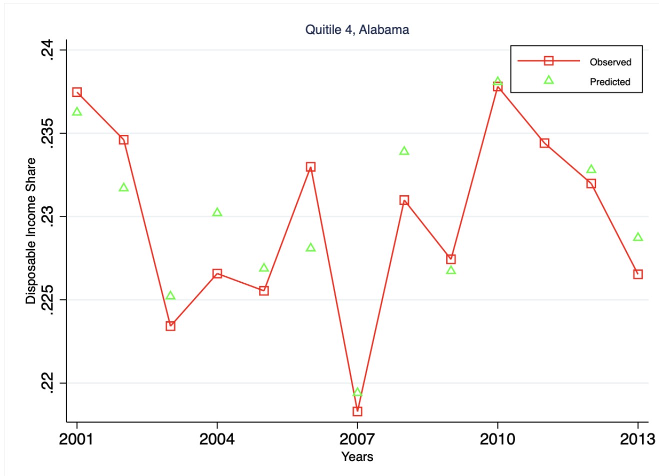


Figure 8: Actual v. Model-Predicted Disposable Income Shares

This may seem to indicate over-fitting, but in fact the model performs nearly as well out of sample. For instance, removing state fixed effects and applying the model estimated using 1991-2012 to *national* data, the forecasts for 2013 are almost an exact match of the actual disposable income shares (see Table 6). Similarly, when we run the model on the observations dropped from the regressions (in order to include lags and instruments which are not available for the entire data set), the fit is excellent, and the R^2 remains above 0.99. In addition, to test whether the model can produce a good fit for the more recent trend of income inequality at the bottom, we re-estimate the model using the updated 1991-2016 sample and compare its out-of-sample predictions for 2017 disposable income distribution to the data. The model predicts 6.56%, 11.64%, 16.30%, 22.23%, and 43.27% for Q1-Q5 which are consistent with the actual observed distribution of 6.40%, 11.50%, 16.30%, 22.82%, and 42.98%.

	Q1	**Q2**	**Q3**	**Q4**	**Q5**
Actual	6.06	11.47	16.53	23.06	42.89
Predicted	6.17	11.76	16.76	23.11	42.20

Table 6: National Disposable Income Shares, 2013

[41] Here, we use the broader definition of the statistic, in which R^2 is equal to the square of the (Pearson) correlation between the model predictions and the actual values.

This predictive power does not come at the expense of the statistical precision of the parameter estimates. For the baseline model, 74 of the 100 estimated marginal effects are significant at the 1% level, 3 are significant at the 5% level, and 4 are significant at the 10% level. Unsurprisingly, the null hypothesis (4) is also jointly rejected. Of course, regression estimates are only as trustworthy as the underlying model, so it's important to validate the key assumptions. With respect to identification, the Hansen test (based on the J-statistic) fails to reject the null hypothesis of joint exogeneity of the instruments; in fact, the p-value is nearly 1.[42] As for the assumption that errors are only correlated within a 4-year election cycle (our justification for Newey-West standard errors as well as the inclusion of lags in the instrument set), a Cumby-Huizinga test does indeed show that autocorrelation fades over time. By the 5th lag, p-values for the presence of serial correlation range from .4373 to .8584 depending on the base variable.

While tests of the model are useful and informative, perhaps the important question is whether the results are robust to the model itself. In the appendix, we reproduce the estimates under the assumption that the error terms are fully serially correlated. This requires dropping any lags in the instrument set and using clustered standard errors (where the cluster groups are states). Of course, it is not computationally feasible to estimate a model with more parameters than clusters. In order to reduce the number of regressors, we 'partialed out' the fixed effects using a Frisch-Waugh-Lovell procedure. Each of the key variables - the income shares as well as the key controls - were first regressed on the state and year dummies. We then substituted the residuals into a truncated version of equation 2 with the fixed effects dropped. As for the instruments set, we replaced lagged market income shares with imputations based on national trends, an approach used in Voorheis et al. (2015).

As the tables in Appendix 7 make clear, the estimates are qualitatively similar. In particular, the attenuation structure is little changed. Cancellation is a bit larger (both giving and receiving) in the bottom three quintiles and smaller for the top two, but the pictures are nearly identical.

Though the tables are omitted for brevity, we also experimented with quadratic specifications and different income definitions. As before, the results are qualitatively similar.

8 Directions for Further Research

It is natural to conceive of the scenarios $\{Q_i \to Q_j\}$ as independent in the sense that their outcomes are not co-determined. For instance, suppose that a transfer of market income share from the middle quintile to the top quintile is followed by an equal and opposite transfer of market income share from the top quintile to the middle quintile. In a perfectly passive environment, with all else equal, the disposable income distribution would revert to its original position, implying that the marginal effects of the initial and final market income shifts perfectly cancel. In reality, however, these events (and their effects) may be completely disconnected. Indeed, if disturbances to the market income

[42] For the baseline model, the J-statistic is 145.134 with 289 degrees of freedom.

distribution can set off a fresh renegotiation of the political economy, anything (consistent with rational political coalitions and respecting economic constraints) is possible.

The existence of asymmetries would thus provides strong evidence of ‘active’, i.e. discretionary, policy responses. Indeed, since any differentiable function mapping the market income distribution to the disposable income distribution satisfies symmetry by construction, a statistical rejection of symmetry might be interpreted as a rejection of static policy rules in general.[43] Formally, symmetry holds if, for all l, k, and j

$$\frac{\partial D^l}{\partial m\left(k\right)} = v \text{ when } m\left(j\right) \text{ is excluded } \iff \frac{\partial D^l}{\partial m\left(j\right)} = -v \text{ when } m\left(k\right) \text{ is excluded} \tag{7}$$

where $m\left(k\right)$ and D^k are quintile k's share of market and disposable income, respectively. In words, the effect on the disposable income distribution of a transfer of market income from quintile k to quintile j is equal and opposite to the effect of the reverse transfer from quintile j to quintile k.

The possibility of asymmetric effects, and hence independent experiments, path dependent outcomes, etc., gives rise to many other conjectures. For instance, we might say that *monotonicity*, as discussed in the empirical results Section 4, is satisfied if

1. $\frac{\partial D^l}{\partial m(l)}$ is decreasing in l (holding base fixed) and increasing in the base quintile
2. $\frac{\partial D^l}{\partial m(k)}$, $l = base$, is decreasing in l and increasing in k
3. $\frac{\partial D^l}{\partial m(k)}$, $k \neq l$ and $l \neq$ base, is decreasing in l (holding k and base fixed)

where $m\left(k\right)$ and D^k are quintile k's share of market and disposable income, respectively. In words, if market income shifts from one quintile to another, the post-tax, post-transfer *gain* to the the *receiving* quintile is decreasing in the receiving quintile's rank (holding the giving quintile fixed) and increasing in the rank of the giving quintile, the post-tax, post-transfer *loss* to the *giving* quintile is increasing in the giving quintile's rank (holding the receiving quintile fixed) and decreasing in the rank of the receiving quintile, and the spillover effect on the post-tax, post-transfer income share of an *unrelated* quintile is decreasing in that quintile's rank. The basic idea is that the system is always less generous to higher income individuals, whatever the circumstances. This is intuitive, and in fact, we examined this hypothesis in Section 4 for a subset of the experiments, $\{Q_i \rightarrow Q_5\}_{i=1}^{4}$. However, the monotonicity property cannot be jointly satisfied by *all* the experiments, unless the marginal effects are also asymmetric. Quite simply, if, say, experiments$\{Q_i \rightarrow Q_5\}_{i=1}^{4}$ satisfy monotonicity, then symmetry implies that $\{Q_5 \rightarrow Q_i\}_{i=1}^{4}$ satisfy *backward* monotonicity (because all the signs are switched).

[43]Here, we use the term 'static policy rules' to refer to fully prescriptive policy functions of (non-dynamic) state variables. In practice, policy proposals are often judged by 'rules' of a more dynamic flavor and which are better described as constraints, such as the requirement that any change be progressive (i.e. effecting lower post-tax, post-transfer inequality).

Thus, symmetry has many implications, and the question of whether the estimates exhibit the property is clearly of great interest. In principle, the conditions (7) form a joint hypothesis which can be statistically tested against the data, for example with a Wolak (1989) test. Unfortunately, any standard differentiable functional form regression - indeed any regression model based on a fixed mapping from the market to the disposable income distribution - implicitly subsumes symmetry by construction, making such a test infeasible. In our setting, this takes the form of cross-system restrictions (see Appendix A). The problem is mitigated somewhat by including higher order terms in the regression. In that case, the equation is nonlinear, so the choice of which base variable to drop affects the estimates. However, the differences are minor; in essence, the same information is being used to estimate the same relationships. Alternatively, we could use a unique set of instruments for each quintile/base variable (i.e. for each system of equations). As with higher order terms, this would relax symmetry in a mechanical sense. Conceptually, however, the instruments would not only have to be associated with the corresponding quintile, but 'correlated' in only one direction (i.e. increases induce increases but decreases do not induce decreases, or vice versa). This is of course a tall order. Indeed, a fully unconstrained estimation requires a wholly different paradigm, e.g. a dynamic setup with possible path dependence.

9 Conclusion

This paper breaks new ground in the empirics of income inequality and redistribution. Methodologically, we make two principal contributions. First, we construct richer, larger, and more comprehensive data sets at the local level over a quarter century. This enhances the accuracy and precision of the estimates, allowing us to include more control variables in the regression. What's more, the regional nature of the data makes it possible to instrument for exogenous shocks to the market income distribution (by exploiting local heterogeneity in exposure to national/international factors) to account for reverse causality.

Second, rather than use summary statistics (e.g. Gini coefficients) to measure both redistribution and inequality, we use quintile shares to represent whole distributions and a system of equations to map the market income distribution to the disposable income distribution. This approach allows us to explore a larger and more refined set of hypotheticals, distinguish between income groups, and ask questions beyond the scope of earlier studies.

By showing that taxes and transfers are uniquely responsive to the income share of the middle quintile, we provide strong evidence in support of the median voter hypothesis at work in the United States in our sample period. Similarly, we document both a large income effect as well as the negligible influence of state political party control, inequality trends, and other seemingly relevant factors. We also find large, statistically significant spillover effects on quintiles whose income shares remain unchanged, a novel result which suggests that any tax and transfer system attempting to stabilize the disposable income distribution provides an incomplete model of future redistribution (either 'active' policy responses play an important role, or passive feedback effects are more complicated). Finally, our examination of coalitions across income quintiles would not be possible without the framework introduced in the paper.

As demonstrated in Section 7, the assumptions satisfy all relevant statistical tests, e.g. for exogeneity of the instruments and lack of serial correlation beyond our four years lag structure. What's more, the marginal effects are quite precisely estimated, the model performs well both in and out of sample, and our main findings are robust to the choice of controls, instruments, standard errors, etc. Nevertheless, it remains to be seen whether similar patterns hold across countries, regions, time periods, political systems, as discussed in Section 8, or with even more complex regression designs.

Appendices

A Cross-System Restrictions

Suppose that the first market share is excluded from the regression (i.e. the lowest quintile is the 'base' quintile). Then, for the k^{th} equation, we have

$$\delta_{it}^{k} = \varphi^{k} + \beta_{2}^{k} m\left(2\right)_{it} + \beta_{3}^{k} m\left(3\right)_{it} + \beta_{4}^{k} m\left(4\right)_{it} + \beta_{5}^{k} m\left(5\right)_{it} + e_{it}^{k} \tag{8}$$

where $m\left(i\right)$ is the market share of quintile i,, and coefficient vectors φ^{k} and β^{k} are chosen so that e minimizes the GMM objective function.

Because the market income shares sum to 1, $m\left(2\right)$ may be eliminated in the following way:

$$\delta_{it}^{k} = \varphi^{k} + \beta_{2}^{k}\left[1 - m\left(1\right)_{it} - m\left(3\right)_{it} - m\left(4\right)_{it} - m\left(5\right)_{it}\right] + \beta_{3}^{k} m\left(3\right)_{it} + \beta_{4}^{k} m\left(4\right)_{it} + \beta_{5}^{k} m\left(5\right)_{it} + e_{it}^{k}$$

or

$$\delta_{it}^{k} = \varphi^{k} + \beta_{2}^{k} - \beta_{2}^{k} m\left(1\right)_{it} + \left[\beta_{3}^{k} - \beta_{2}^{k}\right] m\left(3\right)_{it} + \left[\beta_{4}^{k} - \beta_{2}^{k}\right] m\left(4\right)_{it} + \left[\beta_{5}^{k} - \beta_{2}^{k}\right] m\left(5\right)_{it} + e_{it}^{k} \tag{9}$$

Notice that (9) is the same regression equation as (8), except that $m\left(2\right)$ is dropped rather than $m(1)$ - the second quintile, rather than the first, is the base. Moreover, it is clear that any set of coefficients in (9) corresponds to some selection of $\left\{\alpha^{k}, \beta_{2}^{k}, \beta_{3}^{k}, \beta_{4}^{k}, \beta_{5}^{k}\right\}$. That is, the space of coefficients of the transformed equation is fully spanned by the coefficients of the original equation. Of course, there is nothing special about $m\left(2\right)$; market shares $m\left(3\right)$ - $m\left(5\right)$ can be eliminated in the same way. Since the same instrument set and control variables are used for all equations , it follows that the same e^{k} minimizes the GMM objective function regardless of which quintile share is excluded. Because in each case the parameters are chosen to reproduce the same e, the estimates for one system of equations are completely determined by the estimates of any other.

For example, if we denote with a 'prime' the estimated coefficients when the second quintile is excluded, it must be that

$$\begin{gathered}
\varphi^{k\prime} = \varphi^{k} + \beta_{2}^{k} \\
\beta_{1}^{k\prime} = -\beta_{2}^{k} \\
\beta_{i}^{k\prime} = \beta_{i}^{k} - \beta_{2}^{k},\ i \geq 3
\end{gathered}$$

The restriction $\beta_{1}^{k\prime} = -\beta_{2}^{k}$ is perhaps intuitive; it means that the effect on the disposable income distribution of a transfer of market income from quintile 1 to quintile 2 is equal and opposite to the effect of a transfer of market income from quintile 2 to quintile 1. However, none of the constraints are formally imposed. Rather, they arise as a by-product of the functional form and the possibility of rearrangement as above.

B Baseline Model - SUR Estimates

In this section, estimates are computed under the assumption that the regressors are exogenous (no instruments are used). Standard errors are robust to heteroskedasticity as well as autocorrelation up to three lags.[44]

Marginal Effects

In the tables below, values represent the estimated marginal effect at mean values of a 1-percentage point transfer of market income from the base quintile to the row quintile on the disposable income share of the column quintile. P-values are calculated under the null hypothesis (4), i.e. that "own" effects are either 1 (receiving) or -1 (losing) and all other effects are zero. Symbol * indicates significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level.

Quintile	Q1	Q2	Q3	Q4	Q5
Q2	-0.29147***	0.40331***	-0.13285***	0.094411	-0.073393
Q3	-0.43234***	0.29684***	0.37722***	-0.16475***	-0.076967
Q4	-0.37719***	-0.0089098	0.038898	0.70435***	-0.35715***
Q5	-0.46681***	-0.014616	-0.15599***	-0.061981	0.69939***

Base Quintile = Q1

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.29147***	-0.40331***	0.13285***	-0.094411	0.073393
Q3	-0.14087**	-0.10647***	0.51007***	-0.25916***	-0.0035737
Q4	-0.085721*	-0.41222***	0.17175***	0.60994***	-0.28375***
Q5	-0.17533***	-0.41793***	-0.023134	-0.15639***	0.77279***

Base Quintile = Q2

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.43234***	-0.29684***	-0.37722***	0.16475***	0.076966
Q2	0.14087**	0.10647***	-0.51007***	0.25916***	0.0035734
Q4	0.055149	-0.30575***	-0.33832***	0.8691***	-0.28018***
Q5	-0.034464	-0.31146***	-0.5332***	0.10277***	0.77636***

Base Quintile = Q3

[44] We use an adaptation of the Newey-West formula for systems and panel data.

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.37719***	0.0089099	-0.038898	-0.70435***	0.35715***
Q2	0.085721*	0.41222***	-0.17175***	-0.60994***	0.28375***
Q3	-0.055149	0.30575***	0.33832***	-0.8691***	0.28018***
Q5	-0.089613***	-0.0057063	-0.19489***	-0.76633***	1.0565*

Base Quintile: Q4

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.46681***	0.014616	0.15599***	0.061982	-0.69939***
Q2	0.17533***	0.41793***	0.023134	0.15639***	-0.77279***
Q3	0.034464	0.31146***	0.5332***	-0.10277***	-0.77636***
Q4	0.089613***	0.0057063	0.19489***	0.76633***	-1.0565*

Base Quintile: Q5

Summary Tables

See Section 5.1 for definitions.

Statistic	**Q1**	**Q2**	**Q3**	**Q4**	**Q5**
Average Benefit	-1.4913e-07	5.7357e-08	2.2938e-08	1.3889e-07	-7.0053e-08
Average Positive Benefit	0.36546	0.51059	0.36208	0.21667	0.24701
Average Negative Benefit	0.36546	0.51059	0.36208	0.21667	0.24701
Average Benefit, Receiving	-0.60805	-0.66502	-0.5603	-0.26257	-0.17373
Average Benefit, Giving	0.60805	0.66502	0.5603	0.26257	0.17373
Attenuation, Receiving	0.34639	0.35943	0.25312	0.12357	0.14804
Attenuation, Giving	0.34639	0.35943	0.25312	0.12357	0.14804
Win Share	0.4	0.35	0.4	0.4	0.35
Loss Share	0.4	0.35	0.4	0.4	0.35
Voting Coalition Share	0.75	0.25	0.25	1	0.75
Insurance Coalition Share	0.61538	0.46154	0.53846	0.46154	0.30769

Table 7: Summary Statistics (of Point Estimates)

Quintiles	Q1	Q2	Q3	Q4	Q5
Q1	NaN	0.3	0.3	0.4	0.2
Q2	0.3	NaN	0.3	0.2	0.2
Q3	0.3	0.3	NaN	0.2	0.1
Q4	0.4	0.2	0.2	NaN	0.1
Q5	0.2	0.2	0.1	0.1	NaN

Table 8: Insurance Coalition Frequency Matrix

Quintiles	Q1	Q2	Q3	Q4	Q5
Q1	NaN	0	0.05	0.15	0.1
Q2	0	NaN	0	0.05	0.05
Q3	0.05	0	NaN	0.05	0
Q4	0.15	0.05	0.05	NaN	0.15
Q5	0.1	0.05	0	0.15	NaN

Table 9: Voting Coalition Frequency Matrix

C Baseline Model - GMM

In this section, the market income shares as well as the control variables for real disposable income per capita, the cyclical unemployment rate, and the poverty rate are assumed endogenous, and the variables defined in Section 3.3 (Bartik cumulative shock, import exposure, the commodity interactions) as well as lags of the endogenous variables are used as instruments. Standard errors are robust to heteroskedasticity as well as autocorrelation (in the error terms) up to three lags.[45]

C.1 Summary Tables

See Section 5.1 for definitions.

Statistic	**Q1**	**Q2**	**Q3**	**Q4**	**Q5**
Average Benefit	-0.0047032	-0.00054363	0.001458	0.002273	0.0015158
Average Positive Benefit	0.1761	0.45056	0.43842	0.30607	0.30526
Average Negative Benefit	0.17393	0.45168	0.43678	0.30125	0.30303
Average Benefit, Receiving	-0.21831	-0.55296	-0.72981	-0.28982	-0.10771
Average Benefit, Giving	0.20437	0.55009	0.73225	0.29345	0.10643
Attenuation, Receiving	0.00094122	0.14551	0.20119	0.095889	0.0026707
Attenuation, Giving	-0.0072317	0.15047	0.20922	0.093713	3.6926e-05
Win Share	0.4	0.35	0.4	0.45	0.4
Loss Share	0.45	0.35	0.4	0.45	0.4
Voting Coalition Share	0.75	0.75	0.25	0.75	0.5
Insurance Coalition Share	0.51724	0.41379	0.48276	0.58621	0.41379

Table 10: Summary Statistics (of Point Estimates)

[45] We use an adaptation of the Newey-West formula for systems and panel data.

Quintiles	Q1	Q2	Q3	Q4	Q5
Q1	NaN	0.2	0.4	0.35	0.2
Q2	0.2	NaN	0.4	0.4	0.1
Q3	0.4	0.4	NaN	0.3	0.2
Q4	0.35	0.4	0.3	NaN	0.2
Q5	0.2	0.1	0.2	0.2	NaN

Table 11: Insurance Coalition Frequency Matrix

Quintiles	Q1	Q2	Q3	Q4	Q5
Q1	NaN	0.1	0.05	0.1	0.05
Q2	0.1	NaN	0	0.1	0.1
Q3	0.05	0	NaN	0.05	0
Q4	0.1	0.1	0.05	NaN	0.05
Q5	0.05	0.1	0	0.05	NaN

Table 12: Voting Coalition Frequency Matrix

C.2 Elasticities

In the tables below, values represent the estimated *elasticities* at mean values of a 1-percentage point transfer of market income from the base quintile to the row quintile on the disposable income share of the column quintile. P-values are calculated under the null hypothesis (6) described in Section 5.1. Symbol * indicates significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level.

Quintile	Q1	Q2	Q3	Q4	Q5
Q2	-1.0468***	0.41482***	0.03796	0.20644***	-0.082068***
Q3	-1.9293***	0.44138***	0.2552***	0.090821***	0.028185
Q4	-2.4225***	0.05974	0.1202***	0.97073	-0.20975***
Q5	-6.5883	0.19136	-0.32457***	0.52579***	0.85647**

Base Quintile = Q1

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.2965***	-0.12289***	-0.010378	-0.058818***	0.024891***
Q3	-0.16796	-0.2722***	0.19779***	-0.25522***	0.16752***
Q4	0.22695	-1.0206***	0.03321	0.44236***	0.0064828
Q5	-1.1192***	-2.0234***	-0.50823***	-0.55526***	1.2947***

Base Quintile = Q2

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.3263***	-0.076146***	-0.044239***	-0.015115***	-0.0039994
Q2	0.096363	0.16033***	-0.11412***	0.14981***	-0.098942***
Q4	0.48881***	-0.60735***	-0.26961***	0.82608***	-0.24583***
Q5	-0.5879**	-1.1698***	-1.1202***	0.23292***	0.77192***

Base Quintile = Q3

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.27455***	-0.0072733	-0.014214***	-0.11002***	0.024144***
Q2	-0.10804*	0.39305***	-0.0066993	-0.16863***	-0.0026229
Q3	-0.35379***	0.402***	0.18165***	-0.54458***	0.16504***
Q5	-1.6434***	0.066178	-0.56196***	-1.4588***	1.2838***

Base Quintile: Q4

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.3585***	-0.010033	0.01875***	-0.028882***	-0.047024***
Q2	0.20685***	0.38103***	0.096342***	0.11214***	-0.24762***
Q3	0.18127**	0.38142***	0.36441***	-0.079411***	-0.24775***
Q4	0.79443***	-0.038758	0.27605***	0.71464***	-0.62571***

Base Quintile: Q5

D Robust Estimates

Quintile	Q1	Q2	Q3	Q4	Q5
Q2	-0.81928***	0.43733***	0.002992	0.38384***	-0.0048928
Q3	-0.58834***	0.3805***	0.34742***	0.014301	-0.15388***
Q4	-0.71547***	-0.036334**	0.12776***	0.8929***	-0.26885***
Q5	-0.84836***	0.0077915	-0.10444***	0.15225***	0.79276***

Base Quintile = Q1

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.81981***	-0.43764***	-0.0029316	-0.38371***	0.0044724
Q3	0.23103***	-0.056915***	0.34447***	-0.36957***	-0.14901***
Q4	0.10389***	-0.47375***	0.12482***	0.50903***	-0.26398***
Q5	-0.029	-0.42965***	-0.10737***	-0.23162***	0.79763***

Base Quintile = Q2

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.58884***	-0.38067***	-0.34731***	-0.014077	0.15321***
Q2	-0.23096***	0.057033***	-0.34419***	0.36946***	0.14866***
Q4	-0.12705***	-0.41671***	-0.21945***	0.87854***	-0.11534***
Q5	-0.25997***	-0.37265***	-0.4517***	0.13793***	0.94639*

Base Quintile = Q3

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.73049***	0.041847**	-0.12706***	-0.89407***	0.24879***
Q2	-0.10436***	0.47502***	-0.12537***	-0.51319***	0.2679***
Q3	0.12839***	0.42169***	0.21939***	-0.87995***	0.11048**
Q5	-0.13143***	0.04639***	-0.23255***	-0.74197***	1.0596***

Base Quintile: Q4

Quintile	Q1	Q2	Q3	Q4	Q5
Q1	0.8618***	-0.0037087	0.10487***	-0.15319***	-0.80976***
Q2	0.028556	0.4301***	0.10719***	0.22828***	-0.79413***
Q3	0.25742***	0.37246***	0.45241***	-0.13641***	-0.94587*
Q4	0.13302***	-0.04417***	0.23202***	0.74007***	-1.0609***

Base Quintile: Q5

Statistic	**Q1**	**Q2**	**Q3**	**Q4**	**Q5**
Average Benefit	0.0014519	0.00089847	4.8452e-05	-0.00055722	-0.0018416
Average Positive Benefit	0.20914	0.42944	0.37045	0.24918	0.15987
Average Negative Benefit	0.20587	0.42789	0.37035	0.25044	0.16391
Average Benefit, Receiving	-0.24976	-0.65013	-0.65908	-0.24486	-0.10092
Average Benefit, Giving	0.25714	0.65051	0.65934	0.24271	0.09732
Attenuation, Receiving	0.11147	0.26591	0.18001	0.064488	0.014826
Attenuation, Giving	0.11504	0.26621	0.17998	0.062515	0.012951
Win Share	0.45	0.45	0.45	0.45	0.45
Loss Share	0.45	0.45	0.45	0.45	0.45
Voting Coalition Share	0.42857	0.71429	0.42857	0.71429	0.85714
Insurance Coalition Share	0.47059	0.52941	0.52941	0.47059	0.47059

Table 13: Summary Statistics (of Point Estimates)

Quintiles	Q1	Q2	Q3	Q4	Q5
Q1	NaN	0.3	0.6	0.2	0.3
Q2	0.3	NaN	0.4	0.3	0.5
Q3	0.6	0.4	NaN	0.4	0.1
Q4	0.2	0.3	0.4	NaN	0.3
Q5	0.3	0.5	0.1	0.3	NaN

Table 14: Insurance Coalition Frequency Matrix

Quintiles	Q1	Q2	Q3	Q4	Q5
Q1	NaN	0.15	0.05	0.05	0.1
Q2	0.15	NaN	0.05	0.15	0.2
Q3	0.05	0.05	NaN	0.1	0.1
Q4	0.05	0.15	0.1	NaN	0.25
Q5	0.1	0.2	0.1	0.25	NaN

Table 15: Voting Coalition Frequency Matrix

E Charts and Tables

Table 16: Market Income Share Changes by Quintile, 1991-2013, %

State	**Q1**	**Q2**	**Q3**	**Q4**	**Q5**
Alabama	-0.534	-1.267	-2.061	-1.897	5.779
Alaska	-0.430	0.105	-0.138	-0.425	0.880
Arizona	-1.532	-2.827	-2.873	-1.078	8.288
Arkansas	-0.358	-1.982	-0.875	-0.263	3.463
California	0.197	-1.255	-2.169	-2.318	5.558
Colorado	-0.122	-0.775	-1.034	-1.662	3.585
Connecticut	-1.449	-2.757	-2.016	-0.096	6.331
Georgia	-0.107	-1.060	-1.872	-1.229	4.281
Idaho	-0.767	-1.723	-0.548	0.646	2.331
Illinois	-0.101	-1.163	-0.997	-0.463	2.706
Indiana	-0.806	-0.751	-0.426	-0.468	2.445
Iowa	-1.173	-1.710	-0.809	0.142	3.481
Kansas	-1.272	-1.730	-0.578	1.423	2.116
Kentucky	0.368	-1.089	-2.494	-1.335	4.549
Louisiana	-0.055	-1.797	-1.904	-0.670	4.433
Maine	-0.647	-1.736	-1.721	0.616	3.460

Massachusetts	-1.143	-1.810	-0.674	-0.681	4.295
Michigan	0.268	-1.426	-1.778	0.353	2.581
Minnesota	0.347	-0.051	0.290	0.190	-0.776
Mississippi	-0.720	-1.680	-1.564	-1.914	5.780
Missouri	-0.650	-1.301	-1.307	-0.126	3.394
Montana	-0.408	-0.981	-1.059	-0.551	2.946
Nevada	-1.261	-2.289	-2.345	-0.383	6.309
New Hampshire	-1.188	-1.861	-1.251	0.214	4.035
New Jersey	-0.764	-2.221	-1.850	-0.990	5.827
New York	0.073	-1.441	-2.066	-1.271	4.708
North Carolina	-1.090	-2.258	-1.631	-0.709	5.683
North Dakota	-0.286	-1.244	-0.978	-1.015	3.409
Ohio	-0.517	-1.813	-1.413	-0.168	3.919
Oklahoma	0.146	-0.010	0.016	-1.640	1.427
Oregon	-1.095	-2.553	-2.412	-0.938	6.999
Pennsylvania	-0.888	-1.574	-1.036	0.301	3.202
Rhode Island	-0.810	-2.838	-1.798	0.242	5.213
Texas	-0.043	-0.597	-1.173	-1.203	3.001
Utah	0.165	-0.716	-0.979	-0.150	1.650
Vermont	-0.753	-1.989	-2.030	-1.049	5.852
Virginia	-0.849	-0.680	0.413	0.611	0.509
Washington	-1.083	-2.775	-2.993	-1.956	8.816
West Virginia	-0.552	-1.730	-2.442	-2.229	6.928
Wisconsin	-1.221	-1.551	-1.621	-0.744	5.089
Wyoming	-0.668	-1.465	-1.077	0.834	2.367

F Derivation of (6) in Section 5.1

A value of 1 in the first case follows from first degree homogeneity of a quintile's disposable income share in its own market income share. Letting D and M denote aggregate disposable and market income, respectively, and letting ϕ_i denote the net tax rate applied uniformly to quintile i, we have $P(l) = (1-\phi_l) X_l \frac{M}{D}$, which is homogenous in X_l (holding M and D fixed). Even if we suppose that tax rates are idiosyncratic within quintiles, $P(l) = \frac{\sum_i m_{li}(1-\phi_{li})}{D}$, where m_{li} and ϕ_{li} are the market income and net tax rate, respectively, of individual i in quintile l. This is linearly homogenous in $\overrightarrow{m_l}$, and hence linearly homogenous in X_l if $\overrightarrow{m_l}$ is linearly homogenous in X_l (i.e. proportional changes in market income are uniform within quintiles).

A value of $\frac{-P(k)}{P(l)}$ in the second case follows from the fact that if we assume that the disposable income shares of quintiles not involved in the transfer are unaffected (i.e. $e_k^l = 0$ for $l \notin \{k, base\}$, as in the null hypothesis above), then the sum of the disposable income shares of the receiving and losing quintiles is fixed, i.e. $P(k) + P(base) = C$, C a constant. Thus,

$$\frac{X_k}{P(k)\,P(base)}\left[\frac{\partial P(k)}{\partial X_k} + \frac{\partial P(base)}{\partial X_k}\right] = 0$$

$$\frac{1}{P(base)} + \frac{e_k^{base}}{P(k)} = 0$$

$$e_k^{base} = -\frac{P(k)}{P(base)}$$

where the second line uses $e_k^k = 1$, established in the previous paragraph.

			CBO	Census P-60	Census Research Series (2005)	Our Work	Piketty-Saez	Armour et al.
Data Sources			CPS,SOI (full), CES	CPS	CPS,SOI,AHS,SIPP	CPS+Census Imputations	IRS tables	CPS+Census Imputations, SCF, HPI
Income Unit			Household	Household	Household	Household/Family	Tax Unit	Household
Equivalence Scale			Square root	Betson	Betson	Betson/Eurostat	None	Square root
Unit of Analysis			Individual	Household	Household	Individual	Tax Unit	Individual
Price Level Adjusment	Temporal		PCE deflator	CPI-U-RS	CPI-U-RS	PCE Deflator	CPI-U	CPI-U-RS
	Geographic		None	None	None	BEA RPP (and HPI Imputations)	None	None
Market Income	Labor Earnings	Wages and salaries, self-employment	✓	✓	✓	✓	✓	✓
		Employer-paid health insurance	✓			✓		✓
		Employer's share of payroll taxes	✓					
		Corporate Tax Borne by Workers	✓					
		Work expenses (deduction)			✓			
	Capital Income	(Privately Held) Business Income	✓	✓	✓	✓	✓	✓
		Interest	✓	✓	✓	✓	✓	✓
		Dividends	✓	✓	✓	✓	✓	✓
		Rental income	✓	✓	✓	✓	✓	✓
		Imputed Rent for Owner-Occupied Housing			✓	✓		
		Corporate Tax Borne by Capital Owners	✓					
	Retirement Income	Pensions	✓	✓	✓	✓	✓	✓
		401(k) and IRA withdrawals	✓	✓	✓	✓	✓	✓
		Survivors' Income	✓	✓	✓	✓	✓	✓
		Disability		✓	✓	✓	✓	✓
	Capital Gains	(Taxable) Realized	✓		✓			✓
		Accrued						✓
	Private Transfers	Remittances				✓		
		Alimony		✓	✓	✓	✓	✓
		Child Support		✓	✓	✓		✓
		Scholarships, grants, etc.		✓	✓	✓		✓
Transfers	Cash	Social Secuirty	✓	✓	✓	✓		✓
		Unemployment Insurance	✓	✓	✓	✓	✓	✓
		SSI	✓	✓	✓	✓		✓
		Welfare	✓	✓	✓	✓		✓
		Veterans' benefits	✓	✓	✓	✓		✓
		Workers' comp	✓	✓	✓	✓		✓
		Educational Assistance		✓	✓	✓		✓
	In-kind	SNAP (food stamps)	✓		✓	✓		✓
		School meals	✓		✓	✓		✓
		Housing assistance	✓		✓	✓		✓
		Energy Assistance	✓			✓		
		Medicare	Cost			Fungible Value		Cost
		Medicaid	Cost			Fungible Value		Cost
		CHIP	Cost					
Taxes	Federal	Income Tax Before Credits	✓		✓	✓		✓
		Earned Income Tax Credit	✓		✓	✓		✓
		Other Tax Credits	✓		✓			✓
		Employee's Share of Payroll	✓		✓	✓		✓
		Employer's Share of Payroll	✓	✓	✓	✓	✓	✓
		Corporate Tax Borne by Workers	✓	✓	✓	✓	✓	✓
		Corporate Tax Borne by Capital	✓	✓	✓	✓	✓	✓
		Excise Taxes	✓					
	State	Before Credits				✓		
		After Credits			✓			✓
	Local	Property			✓	✓		

Table 17: Comparing Income Definitions

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