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Three Essays on Fiscal Policy and Income Dynamics

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Three Essays on Fiscal Policy and Income Dynamics

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Three Essays on Fiscal Policy and Income Dynamics

Abstract

This dissertation is an empirical investigation of the effects of fiscal policies in the U.S. Two fiscal policies are covered. The first two chapters examine the distributional effects of income tax changes. In the third chapter, I investigate the effects of mean-tested transfers on local economic activity.

The literature on the effects of taxes on output has consistently established that an income tax cut has expansionary effects on the GDP. Yet, it is not clear how these gains in GDP are distributed across households. The first chapter looks at the big-picture question of how cuts in the average tax rate affect income and consumption inequality. To provide a causal interpretation of their effect, I build on the narrative tax changes literature to isolate income tax changes in the US that are unrelated to contemporaneous economic conditions. I study the effects of policy-induced changes in federal tax liabilities using Romer and Romer (2010) and Mertens and Ravn (2013)'s narrative-identified shocks for average income taxes in the U.S. I compute quarterly level Gini indexes from the Consumer Expenditure Survey and find that an average tax cut of one percentage point increases inequality in gross and disposable income by twelve percent and consumption inequality by seven percent, as measured by Gini indexes, a year after the cut. I find that increases in the labor supply at intensive margins in top income quintiles can explain the persistence of income inequality. My analysis also provides some policy implications: policies that cut top marginal tax rates do not increase the bottom 60% of incomes in the medium term, and tax cuts and tax increases do not have symmetric effects on income inequality.

Given that oftentimes a fiscal reform modifies the marginal tax for different income brackets,

I look at the spillover effects of the average marginal tax rates across income percentiles in the second chapter. I analyze the spillover effects of top marginal tax cuts on bottom income groups and vice-versa using Mertens and Montiel Olea (2018)'s counterfactual marginal tax rates as instruments. I present new short-run cross elasticities of taxable income and examine income dynamics after five years of a marginal tax cut. I find that "trickle-down" effects are only concentrated on the top incomes: a tax cut in the top 1% only increases income in the short run for the top decile. In contrast, a tax cut in the bottom 90% increases income in the top decile after four years.

Finally, given the theoretical equivalency between income tax cuts and increases in transfer payments, in the third chapter, I examine whether changes in means-tested transfers affect local economic activity. I study the case of a plausible exogenous permanent change in the generosity of housing vouchers in 2005. I leverage adjustments to generosity thresholds that can be attributed to measurement errors from rent estimates. I show these adjustments are unrelated to past local economic trends and use the geographical variation across MSAs in the U.S. to instrument changes in housing transfers. I find that a 1% increase in the MSA average housing transfer decreases the proportion of income that voucher holders spend on rent by 0.05 percentage points and increases the beneficiaries' household income by 1 percent. The effects on beneficiaries, however, do not seem to have meaningful general equilibrium effects, as the effects on GDP and personal income per capita at the MSA level cannot be distinguished from zero.

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Chapter 1

Income tax changes and income and consumption inequality

1.1 Introduction

The consensus of the empirical literature on the effects of income tax shocks on economic aggregates is that a decrease in the average income tax rate increases real output. Early work used different strategies to identify structural tax shocks in SVARs, such as calibrated elasticities (Blanchard and Perotti, 2002) and sign restrictions (Mountford and Uhlig, 2009), pointing towards US output multipliers lower than the unity at impact and over three after several years respectively. A more recent strand of the literature has used historical documents to construct exogenous tax shocks, so that the effect of tax shocks on output can be estimated directly. Romer and Romer (2010) pioneered this narrative approach in the fiscal policy literature and find that a policy-induced increase in federal tax liabilities of 1% as proportion of GDP decreases output by 3% for the US, a finding also replicated in other countries (Cloyne, 2013, Hayo and Uhl, 2014 and Guajardo et al., 2014).[1] How these gains are shared among households is less clear. This paper contributes to the growing literature that examines the relationship between inequality and macroeconomic aggregates;[2] in particular, in the role that income tax policy plays in determining the income distribution (for example, Piketty and Saez, 2007).[3]

I examine the effects of a reduction in the average income tax on inequality by exploiting policy records liabilities forecasts, and the motivation and timing of tax legislations contained in the Romer and Romer (2010) and Mertens and Ravn (2013) narrative income tax series of the US. I find that decreasing the average personal income tax by one basis point increases inequality in gross and disposable income by 12% and consumption inequality by 7%, as measured by Gini indexes.

The main challenge in estimating the causal effects of taxes on income inequality is to address the endogenous nature of the policy, as changes in the tax system typically respond to contemporaneous economic conditions. To address this issue, I build on the macroeconomic literature that examines aggregate effects of fiscal policy through narrative methods. Using information from historical documents to identify plausibly *exogenous* tax shocks, Romer

[1]Limiting the set of taxes to those that were proposed and passed within a quarter to avoid confounding anticipatory effects, Mertens and Ravn (2012) find that a one percent tax cut is associated with a two percent peak increase in GDP per capita and using the shocks as instrument for average tax rates, Mertens and Ravn (2013) find that one percentage point decrease in the average personal income tax leads to an increase in output of 1.4 percent in the first quarter and a peak increase of 1.8 percent which occurs three quarters after the tax cut.

[2]For example, see the review in section 7 of Krueger et al. (2016).

[3]This literature is part of an extensive research agenda that examine changes in diverse type of taxes (capital income, corporate income, consumption and wealth among others) on particular groups that is informative on how inequality changes. Instead, I am interested in the big picture question of the effects of policy-driven changes in income tax liabilities on overall inequality. Other literature has examined the effects on corporate capital taxes and finds that corporate tax cuts increase inequality (e.g. Nallareddy et al., 2018). Another large part of the literature has focused on the top 1% (Piketty et al., 2018) or have relied on cross-country variation to shed light on the positive association between tax system progressivity and economic equality (Martinez-Vazquez et al., 2012, Gupta and Jalles, 2022).

and Romer (2010) -hereafter RR- identified federal taxes that were motivated by long-run concerns and were not accompanied by any discussion of counteracting shocks or a desire to return growth to normal levels. To focus on personal income taxes, I use Mertens and Ravn (2012; 2013)'s subset of individual income tax liabilities and employment taxes (payroll and social security taxes) from RR's tax series. These series also isolate the taxes that were implemented within three months of having been legislated to avoid confounding anticipatory effects.

I compute quarterly measures of income and consumption inequality in the U.S. with data from the Consumer Expenditure Survey (CE). I use a clean version of the survey with data from 1980, the year the survey started, to 2004. This is the lowest frequency survey data available that can measure income and consumption inequality in the US and that also contains labor supply data, allowing me to test an important mechanism of household behavior.

I follow Coibion et al. (2017) and use their approach to investigate the effects of aggregate shocks on income inequality.[4] I recover Impulse Response Functions using local projections Jordà (2005) and I find that a tax reform that decreases the average income tax by 1 percentage point increase gross and after-tax income inequality by 12 percent, as measured by the Gini index, a year after the change. The effect is transitory and subsides after two years and a half. An analysis by income quintiles shows that most of the temporary increase in inequality can be explained by an increase in top income quintiles. The fact that gross income increases as much as the after-tax income suggests the presence of behavioral and general equilibrium effects. I find the response of labor supply at the intensive margin is consistent with these income dynamics.

[4] They examine the effect of narrative monetary tax shocks on income inequality.

An important critique of this approach in analyzing the effect of reforms that had an effect of changing the average tax rate is that the reforms comprise a bundle of different provisions that independently could have different impacts across income percentiles. To address this, I use only the reforms that changed the top marginal tax rates and classify them between those where the marginal tax rates were increased and those where they were cut. The main advantage of this reclassification is that allows to test directly for the presence of for "trickle-down" effects from tax cuts.

There is some evidence that tax cuts in the average marginal tax rate of the top 1% increases the incomes of the bottom 99%. Using constructed instruments for the average marginal tax rate by income groups based on the same reforms as Romer and Romer (2010), Mertens and Montiel Olea (2018) find that a 1% cut in the average marginal tax rate of the top 1% increases the income of the bottom 99% by 0.44% a year after. Since the same marginal tax rate cut leads to an increase in the income of the top 1% by 1.5% in the first two years, they conclude that a targetted cut in the top marginal rate leads to larger inequality in gross income.

I examine instead how cuts in the average tax rate that are mostly due to statutory changes of marginal tax rates affect inequality and incomes across quintiles. I find no evidence supporting the presence of generalized trickle-down effects. Tax reforms that decreased the average tax rate by decreasing top marginal tax rates only increase temporarily the income of top quintiles.

The rest of the paper is structured as follows. The next sections provides an overview of related literature. I describe my empirical strategy in the third section and provide details of the different data sets I use afterwards. I next discuss the results and their implications in the last section.

1.2 Related literature

The usual evaluation approach to examine the effects of the tax system on income inequality is through comparisons of pre-tax and post-tax income distributions (for example, Heathcote et al., 2010 and OECD, 2012). This comparison does not distinguish between the direct effect of the tax schedule on pre-tax incomes and the indirect effects, or behavioral induced effects of the tax system. For example, individuals may decrease the hours worked in response to higher marginal tax rates, delay income payments, or shift their income from personal income to corporate income.

A strand of the literature has focused on estimating how responsive incomes are to increases in tax rates (for a review see Saez et al., 2012) and has given a particular attention to the behavior of the top 1% (Piketty and Saez, 2003). Perhaps more related to overall income inequality are the studies that conduct counterfactual income distributions to quantify the direct effect of specific tax reforms. These counterfactual income distributions are typically constructed applying the tax schemes of different years to previous or current income distributions. For instance, Gramlich et al. (1993) apply the tax reforms of the 1980s in the US to the pretax income distribution of 1990 and attribute 16% of the increase in the Gini coefficient from 1980 to 1990 to the tax policies of the 1980s. Poterba (2007) computes the shares of incomes by quintiles and top decile percentiles that would result from applying the tax rates from 2004 to the pre-tax incomes of 2000 and vice-versa. He compares these with the actual shares of 2000 and 2004 and concludes that the changes in the pre-tax income distribution between 2000 and 2004 have a more important role than the 2003 Tax Act in explaining the reduction in the after-tax income inequality between 2000 and 2004. For example, he finds that the share of after-tax income accruing to the top 1% is reduced by 1.4

percentage points between 2000 and 2004 due to the changes in pre-tax incomes; in contrast, the mechanical effect of the tax act of 2003 was to increase the share of the top 1% by 0.3 percentage points.

A study looking at all the tax reforms in the US between 1979 and 2007 also suggests that the tax system has had small but adverse effects in explaining the overall changes in income distribution. Bargain et al. (2014) examine all the policy reforms in the US between 1979-2007. Using data from the Internal Revenue Service (IRS) and the NBER's TAXSIM program, they compute counterfactual simulations by applying the new tax schedule to the previous year incomes. Their decompositions suggest that tax changes increased the income share of the top 20% of the taxpayers, suggesting that if the tax policies had not occurred, inequality in the mid 2000s would have been lower. They conclude that between 11 to 29% of the change in income shares can be attributed to the effect of the tax reforms.

Another way to estimate how changes in the tax system have affected income inequality is leveraging quasi-experimental events. Troiano (2017) looks at the introduction of some features of the tax system that increased tax revenues: the introduction of the state personal income tax, tax withholding and third-party reporting, and intergovernmental agreements to coordinate tax auditing. He shows that these changes in the tax code increased income inequality in states that implemented the changes earlier.

Finally, in a quantitative study, Bayer et al. (2020) through a HANK model with incomplete markets and portfolio choice between liquid and illiquid assets conclude that shocks to the progressivity of the income tax schedule explain 7% of the increase in the top 10% income share from 1985 to 2019. Although their tax progressivity shocks are not directly comparable to the income tax shocks analyzed here, their model also suggests that had the US enacted more aggressive stabilization through cuts in the average tax rates, income inequality would have been reduced in recessions but would have resulted at the cost of higher inequality during booms.

This paper does not aim to quantify the role that tax reforms have had on income inequality trends, rather, this paper aims to track how a shock in the average tax rate, product of diverse tax reforms, affect income inequality. Unlike previous work that has looked at all the major tax reforms in the US, I only focus on the subset of reforms that were not motivated due to current economic conditions. This is important since some reforms might have been implemented with the specific motivation of reducing income inequality.

1.3 Empirical Strategy

Narrative methods offer a way to address tax policy endogeneity.[5] RR argue that historical records contain sufficient information to select tax changes that do not obey to current economic conditions, and identify tax changes in the US from 1945 to 2007 that were motivated by long-term growth and long-term debt concerns.[6] They show that these shocks are not Granger caused by output growth. To avoid confounding anticipatory effects, I focus on MR's subset of personal income unanticipated taxes, defined as individual income and employment tax liability changes that were implemented within 90 days after the corresponding tax change became law (as forecasted in Congress reports). Appendix Table 1.3 provides a brief description of these reforms and highlights the main changes to the income tax code along with the projected change in income tax liabilities as reported by MR. For the years 1980-2004 (for which my primary household income data are available), this classification leaves a total of seven personal income tax changes.

[5]See Ramey (2016) for an overview of their use in the identification of macroeconomic shocks.

[6]For example, they examine the annual Economic Report of the President that typically discusses the motivation, revenue effects, and nature of tax changes in the previous calendar year.

From these reforms, I use MR's definition of tax shocks, scaling the change in the policy makers' tax liabilities forecasts by the total personal taxable income of the previous time period:[7]

$$\tau_t = \frac{\text{Indiv Income Tax Liab } \Delta_t + \text{Empl Tax Liab } \Delta_t}{\text{Personal Taxable Income}_{t-1}}, \tag{1.1}$$

where Indiv Income Tax Liab Δ_t and Empl Tax Liab Δ_t stand for changes in individual income and employment tax liabilities. The series are plotted in Figure 1.3.1, marked in red are the reforms that included changes in marginal tax rates.

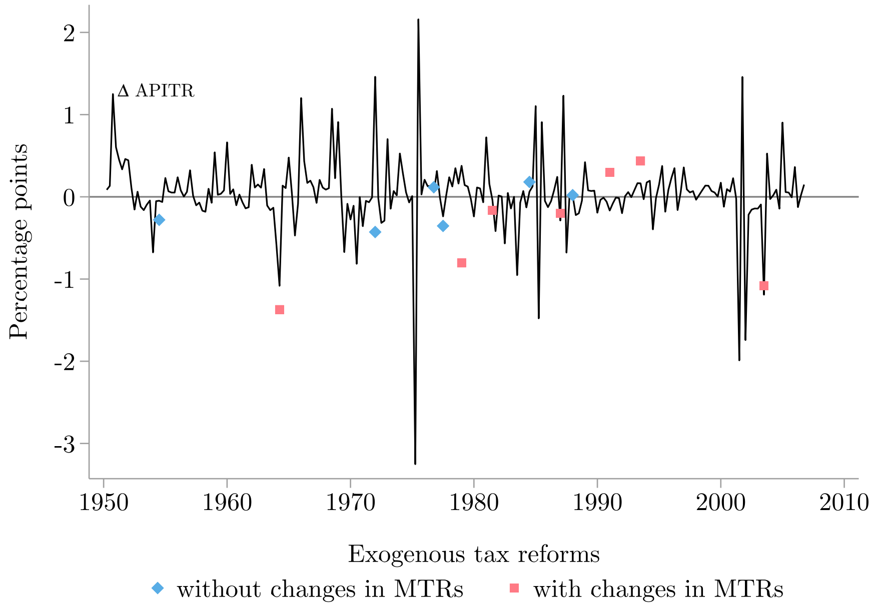
The tax reforms selected were quite heterogeneous. The forecasted changes in income tax liabilities ranged from an increase of 22.8 billions dollars in 1993 to a reduction in liabilities of 94.6 billions in 2003 (all in current dollars). These changes implied big swings in the U.S. average tax rate but not all of the reforms modified statutory marginal tax rates. Several reforms instead or in addition reimposed or repealed tax credits, modified the way to compute the taxable income, either through changes in personal exemptions or by imposing limits to income averaging, or modified minimum tax provisions. Other reforms altogether changed statutory marginal tax rates, or modified the range of income brackets. To investigate the phenomenon of trickle-down effects, I limit the analysis to those reforms that legislated changes in the marginal tax rates. This classification reduces the number of tax reforms analyzed but homologates the reforms since the reforms that increased (decreased) revenues were due mostly to increases (decreases) in the top marginal tax rates.[8]

As a baseline, I analyze the dynamic effects of an unexpected reduction of income tax

[7]Personal taxable income is defined as personal income less government transfers plus contributions for government social insurance.

[8]The reforms that that decreased top marginal tax rates are five, ERTA 1981, TRA 1986 and JGTRRA 2003. The reforms that decreased top marginal tax rates are two, OBRA 1990 and OBRA 1993.

Figure 1.3.1: Change in average personal income tax rate and narratively identified shocks



Change in average personal income tax rate (ΔAPITR) is defined as the change in federal personal income tax revenues including contributions to government social insurance as percentage of lagged taxable personal income. In squares and diamonds, narratively identified shocks to personal income taxes; defined as the change in the projected federal personal income tax liabilities as documented by Romer and Romer (2009) and Mertens and Ravn (2013) as percentage of lagged taxable personal income. Reforms that changed the statutory marginal tax rates in red squares.

liabilities as a proportion of taxable income, computing the impulse response function of τ_t on an inequality measure, y_t, through local projection for horizons $h = 0, ..., H$:[9]

$$y_{t+h} - y_{t-1} \quad = \quad c_h + A(L)X_{t-1} + \beta_h \tau_t + u_{t+h} \tag{1.2}$$

where $A(L)$ is a four-lag polynomial, and X_{t-1} is a vector containing the dependent variable, the average personal income tax rate, and the logarithms of real personal income tax base, government expenditures, GDP and government debt per capita. I interpret the $\{\hat{\beta}_h\}_{h=0}^{H}$ coefficients multiplied by -1 as the reduced form estimates of a tax cut in the average tax

[9] I demean the series τ_t for the non-zero observations before the regressions.

Table 1.1: Summary statistics of narrative identified shocks

	Sample: 1950-2006			Sample: 1980-2006		
	Mean	Range	N	Mean	Range	N
All	-0.279	[-1.37, 0.435]	13	-0.073	[-1.08, 0.435]	7
With changes in MTR						
Cut	-0.724	[-1.37. -0.165]	5	-0.481	[-1.08, -0.165]	3
Increase	0.366	[0.297, 0.435]	2	0.366	[0.297, 0.435]	2

Mean of federal personal income tax liabilities change as share of taxable personal income. Note that the total number of observations do not coincide with the sum of the reforms that changed the Marginal Tax Rates since some reforms included provisions that resulted in changes in the projected revenues but through provisions other than changes in the MTR.

rate.[10]

Since the reforms combine different provisions, I limit the analysis to those reforms that changed the top marginal tax rates. In this way, I can test directly for "trickle-down" effects through the following reduced-form IRF:

$$y_{t+h} - y_{t-1} = A(L)X_{t-1} + \beta_h^{\text{increase}} \cdot \tau_t^{\text{increase}} + \beta_h^{\text{cut}} \cdot \tau_t^{\text{cut}} + \varepsilon_{t+h} \quad (1.3)$$

where the only difference with equation (1.2) is the explicit classification of the narrative taxes between tax cuts (tax reforms that reduced the top marginal tax rates) and tax increases (tax reforms that raised top marginal tax rates). The reforms that did not change the marginal tax rates are omitted in this specification. Since the reforms that reduced the top marginal tax rates resulted in a decrease in projected tax revenues, all the values for τ_t^{cut} are

[10] I prefer this approach to using τ_t as instrument for changes in the average tax rate as the standard errors from those regressions are too large to make meaningful conclusions. Alternatively, if the income tax shocks are not correlated to other current and past unobservable structural shocks that also affect the current change in income inequality, then $\{\hat{\beta}_h\}_{h=0}^{H}$ can be interpreted as the causal effects of income tax shocks on inequality.

negative. Therefore, I interpret the $\{\hat{\beta_h^{cut}}\}_{h=0}^H$ coefficients multiplied by -1 as the effect of a 1 p.p. reduction in the average tax rate from reforms that decrease the marginal tax rates, and the $\{\hat{\beta_h^{increase}}\}_{h=0}^H$ coefficients as the effect of a 1 p.p. increase in the average tax rate from reforms that raise the marginal tax rates.

1.4 Data

I use quarterly data from the Consumer Expenditure Survey (CE), a rotating panel survey that contains household data of income and consumption representative of the U.S. For convenience, I rely on a the Krueger and Perri (2006)'s cleaned dataset that comprehends 1980 to 2004 data. Over 5,000 households are interviewed in each wave and each household is assigned a weight designed to represent the national population, which I use in all computations. Households are interviewed once per quarter, up to five consecutive quarters.

All household income variables are recorded at an annual basis, while household consumption variables are recorded at a quarterly level. The main advantage of using the CE is that its quarterly frequency allows me to pin down a short and medium-term timing of the responses to tax changes. It also provides detailed information on consumption components, so it is possible to track whether expenditure responses are due to durable or non-durable components. Moreover, the survey records data on hours and weeks worked, allowing me to analyze labor supply responses.

Some scholars (e.g. Garner et al., 2006) have raised the concern that CE data may not be appropriate for macroeconomic research on consumption, as the CE microconsumption data do not reflect the growth in real per capita consumption observed in the national accounts (NIPA). However, this is not a concern in my setting. First, this paper focuses on the dispersion of incomes and expenditures across households. Any mismeasurement would need to differentially impact measured consumption across income groups in a way that is correlated with my tax shocks in order to impact my estimates. Moreover, a large component of consumption expenditures captured in the NIPA aggregates but not in the CE are payments made by third parties on behalf of households, specifically the financing of Medicare and Medicaid spending. These expenditures drove much consumption growth in the NIPA, but are not relevant to my setting.

To reduce the likelihood of measurement error, I first drop the observations that record a non-positive labor income, and only keep the records from the fifth interview. Then, I drop those observations that report positive labor income but no hours worked. Finally to reduce noise, I trim the bottom five percent of gross income per quarter and the top 1 percent to eliminate top-coded incomes.

The primary income measures of my analysis are before- and after-tax household labor earnings. Gross income captures all the general equilibrium effects as it includes wages, self-employment (farm and non-farm) income, income from dividends, and welfare receipts from all household members. After-tax income subtracts from the former measure reported federal, state, and local taxes (net of refunds) and Social Security contributions.

I use as proxy for consumption the expenditures in non-durable goods, durable goods, and total expenditures as computed in Krueger and Perri (2006).[11] All income and expenditure variables are normalized by the number of adult equivalents in the household using the Census equivalence scale. Table 1.2 summarizes the sample averages at 1982 constant dollars.

[11]Total expenditures is a weighted average of non-durable expenditures that include food, alcohol, tobacco, personal care, fuels, utilities, public services, public transportation, apparel, tuition, recreational, health services and medical care expenditures, and of durable expenditures, an imputed variable that measure service flows from the value of the stock of durable goods (housing and cars) of a household.All the expenditure components are deflated by expenditure-specific, quarter-specific consumer price indexes.

Table 1.2: CE summary statistics per adult equivalent

		Annual Income		Quarterly Expenditures	
Year	Households	Before-tax	After-tax	Non-durables	Total
1980	2526	19,582	16,866	1895	3294
1981	2607	19,001	16,522	1850	3187
1982	2409	19,784	17,288	1787	3221
1983	2485	19,506	17,227	1778	3325
1984	2968	19,513	17,286	1837	3390
1985	2889	20,251	18,103	1836	3501
1986	3074	19,800	17,775	1810	3609
1987	3271	20,909	18,822	1840	3586
1988	2783	21,088	19,064	1841	3520
1989	2833	21,361	19,186	1851	3639
1990	2940	21,202	19,095	1815	3669
1991	2773	21,401	19,149	1825	3585
1992	2793	21,303	19,212	1782	3542
1993	2758	20,750	18,750	1733	3412
1994	2823	20,924	18,765	1770	3554
1995	2602	20,712	18,683	1714	3544
1996	2611	21,071	19,138	1776	3623
1997	2781	21,940	19,980	1760	3535
1998	2731	22,764	20,700	1820	3780
1999	3613	22,844	20,841	1764	3668
2000	3634	23,116	21,271	1750	3680
2001	3848	23,113	21,598	1723	3595
2002	4050	23,953	22,681	1760	3742
2003	4151	24,030	22,652	1711	3727
2004	1130	24,555	23,243	1780	3805

Description: Total number of households with head of households aged 21-64 in every year. Sample is limited to households that report positive labor income and to records of the fifth interview. The bottom 5% of income before taxes per quarter have been trimmed to reduce noise. Income and consumption measures are in 1982 constant dollars per adult equivalent. Before-tax income includes wages, self-employment (farm and non-farm) income, income from dividends and welfare receipts. After-tax income subtracts from the former measure reported federal, state, and local taxes (net of refunds) and Social Security contributions. Non-durable expenditures include food, alcohol, tobacco, personal care, fuels, utilities, public services, public transportation, apparel, tuition, recreational, health services and medical care expenditures. Total expenditures is a weighted average of non-durable and durable expenditures, an imputed variable that measure service flows from the value of the stock of durable goods (housing and cars) of a household. Averages are weighted using CE population weights.

For each variable, I compute Gini coefficients to measure inequality. The smoothed time series are plotted in Figure 1.4.1.[12] As documented in previous studies, inequality has increased both in income and consumption since 1980. The trimming decisions are evident in the smaller Gini coefficients reported with respect to other datasets. However, as documented in Appendix Figure 1.6.1 the sample mean income by quintiles follow closely those reported in the CPS annual economic supplements.[13] A couple of other points are worth noting. First, expenditure inequality seems to stem from the consumption of durable goods, since inequality for non-durable goods is small (with Gini coefficients below 0.3) and stable across time. Second, although all variables have increased across time, total expenditure inequality grew more than income inequality. From 1980 to 2004, expenditures inequality increased by 24% compared to a 18% increase in gross income inequality and a 21.4% in after-tax income inequality.[14] The fact that after-tax income inequality had grown more than gross-income inequality is consistent with the documented fact that the tax system of the US has become less progressive across time (Piketty and Saez, 2007). This, along with the small difference between before and after-tax income inequality would suggest a limited role of the tax system in the income distribution.

I use the same macroeconomic quarterly variables and definitions from Mertens and Ravn (2013). The average personal income tax rate (APITR), shown in first differences in Figure 1.3.1, is the sum of federal personal current taxes and contributions for government social

[12]Time series smoothed using the median of t, $t+1$ and $t-1$ and a Hanning linear smoother (the simple average between the value in $t+1$, the value in $t-1$ and the double of the value in t.

[13]These averages are rather stable across time and would suggest inequality has not risen substantially. Gini coefficients are measures of inequality more sensitive to the extremes, and Gini coefficients computed from the CEX follow a similar trend to the Gini coefficients from the CPS with a couple of deviations, for the mid-80s, where the CE reports a higher income inequality than the CPS and for 1995, for which the CE reports a lower income inequality than the CPS (see Figure 1.6.2 in the Appendix).

[14]This difference is usually interpreted in policy papers as the role of taxes in reducing income inequality, without differentiating between the mechanical and behavioral effects for instance, OECD (2012). This simple difference neglects the behavioral effects on labor supply and income portfolio allocation among other decisions that affect income, as well as the fact that changes in the tax schedule may have aroused as a consequence of previous income inequality levels.

Figure 1.4.1: Gini coefficients from CE (1980-2004)

Own computations using CE population weights. Sample restricted to households that worked during the interview year. The bottom 5% of income before taxes per quarter have been trimmed to reduce noise. Household variables were scaled by the number of adult equivalents in each household and deflated to reflect 1982 dollars before computations. Before-tax income includes wages, self-employment income, income from dividends and welfare receipts. After-tax income subtracts from the former measure reported federal, state, and local taxes (net of refunds) and Social Security contributions. Non-durable expenditures include food, alcohol, tobacco, personal care, fuels, utilities, public services, public transportation, apparel, tuition, recreational, health services and medical care expenditures. Total expenditures is a weighted average of non-durable and durable expenditures, an imputed variable that measure service flows from the value of the stock of durable goods (housing and cars) of a household. Quarterly time series smoothed using the median of t, $t+1$ and $t-1$ and a Hanning linear smoother.

insurance divided by the personal income tax base (personal income less government transfers plus contributions for government social insurance). These components along with real GDP and government spending come from NIPA tables. Debt is federal debt held by the public from Favero and Giavazzi (2012) divided by the GDP deflator and population.

Additional inequality measures, at the annual level come from Piketty and Saez (2003)'s updated series for the top 1% gross income share, and the World Inequality Database's shares for disposable income for the bottom 90% and 50%. Gross income includes labor earnings, entrepreneurial income, dividends, interest, and rents before deductions and exemptions but excludes realized capital gains and government transfers.

1.5 Results

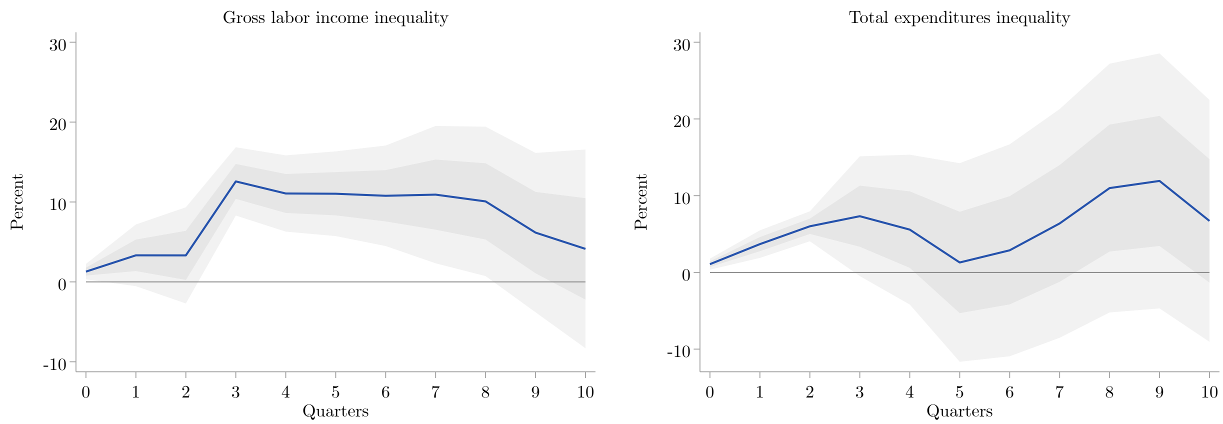
In this section I analyze how a revenue-reducing tax reform impacts income and consumption inequality. To start, I discuss the results of my estimation of the impulse response functions implied by the equations in (1.2). All figures show the response of outcome variables to a 1 p.p. reduction in tax revenues as a proportion of personal income.

Figure 1.5.1 shows the responses of Gini coefficients for gross labor income and total expenditures with respect to a 1 p.p. reduction in tax revenues as a proportion of personal income.[15] Gross labor income inequality increases immediately but reaches a maximum increase of 12% a year after the shock (equivalent to an increase of 4 Gini points). The effect persists for another year before subsiding. Consumption inequality also increases during the first year, but to a smaller extent, by 7% (2 Gini points); the effect is imprecisely estimated after the first two years. After-tax household income displays very similar dynamics, while

[15] For inequality data from the CE, the data spans 1980 to 2004, and therefore I am limited to seven tax reforms. I assess the specification (1.2) on output and other aggregates in Appendix Figure 1.6.3

the majority of the increase in consumption inequality can be attributed to an increase in non-durable goods consumption inequality (see Appendix Figure 1.6.4).[16]

Figure 1.5.1: Inequality responses to a projected decrease in tax revenues of 1 p.p. as proportion of taxable income



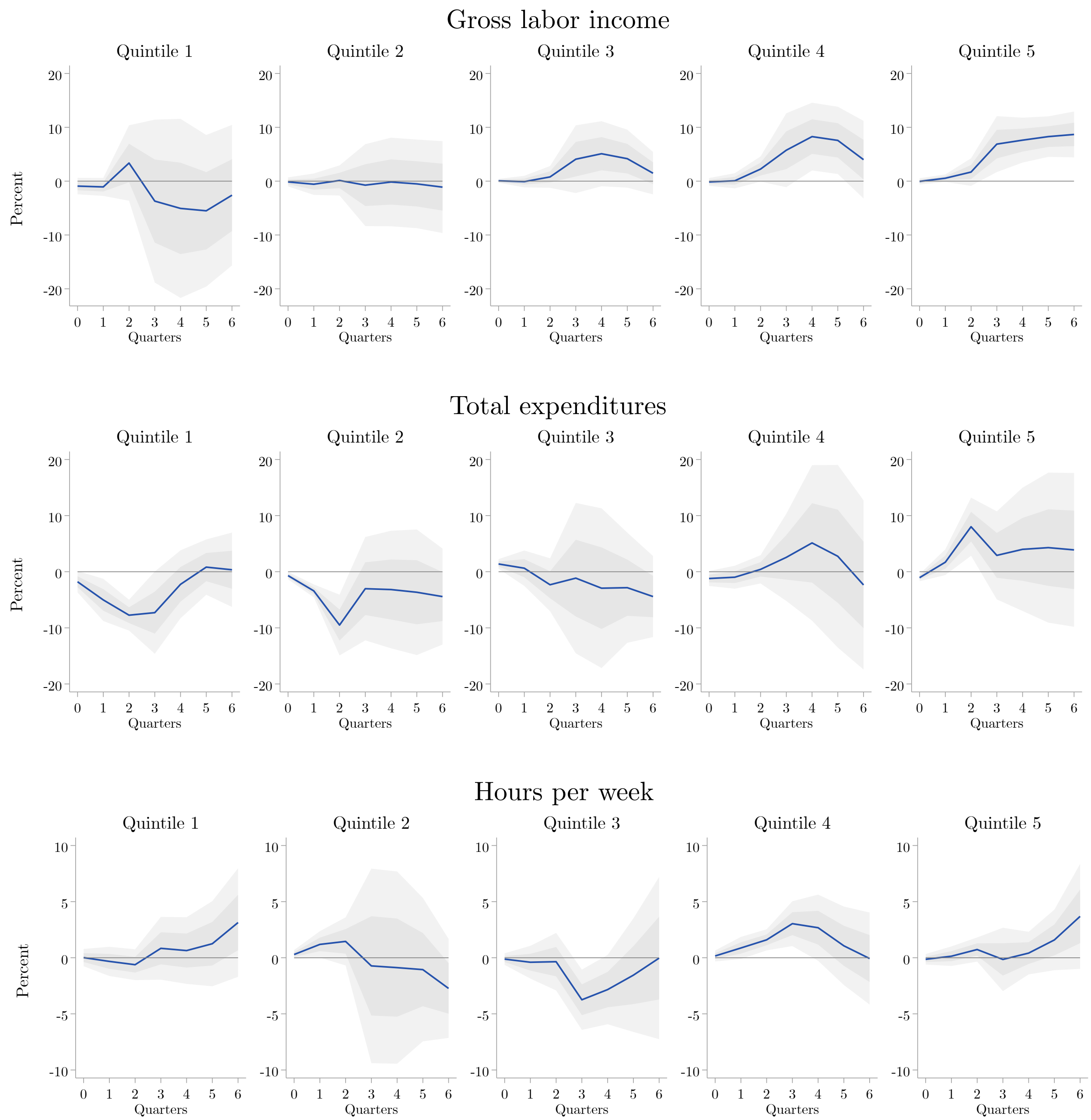
Inequality measured by Gini coefficients. IRFs computed from $y_{t+h} - y_{t-1} = c_h + A(L)X_{t-1} + \beta_h \tau_t + u_{t+h}$. 95% and 68% confidence intervals based on Newey-adjusted standard errors. τ_t are the projected changes in tax revenues as proportion of taxable income. The $\{\hat{\beta}_h\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t.

A more granular analysis shows that the increase in income inequality can be explained by the higher income levels in the top 40%. The upper panel of Figure 1.5.2 shows the effects of a tax cut by gross labor income quintiles. Income increases in the top two quintiles, about 10% a year after the shock with little effect in bottom quintiles. The response of total expenditures by quintiles (middle panel of Figure 1.5.1) is very similar to the changes with an important difference, there is an immediate sharp reduction in consumption in the bottom two quintiles, between 5 and 9 percent. This helps explaining the immediate increase in consumption inequality, and would also suggest that the increase in consumption inequality is driven by different income groups dynamics at different horizons.

There is no marginal tax rate data for income groups at the bottom of the distribution,

[16]The use of other inequality measures, such as the ratio between the 90th and 10th percentiles, are consistent with these results, however significance is lost in most quarters (Appendix Figure 1.6.5).

Figure 1.5.2: Responses to a projected decrease in tax revenues of 1 p.p. as proportion of taxable income by income quintiles



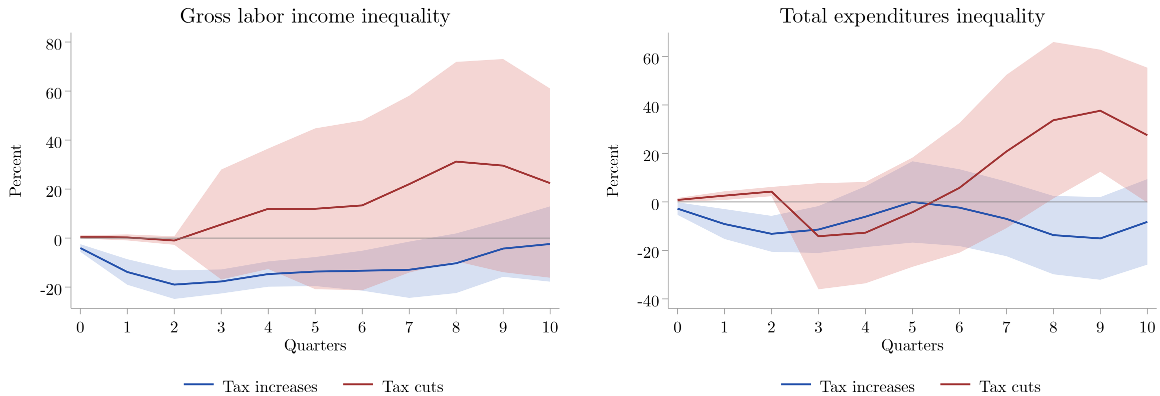
Quintiles by before-tax income. IRFs computed from $y_{t+h} - y_{t-1} = c_h + A(L)X_{t-1} + \beta_h \tau_t + u_{t+h}$. 95% and 68% confidence intervals based on Newey-adjusted standard errors. τ_t are the projected changes in tax revenues as proportion of taxable income. The $\{\hat{\beta}_h\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t.

so it is difficult to assess directly if these results are the consequences of changes in the relative price of leisure. To gain some traction on this issue, the bottom panel of Figure1.5.2 examines the labor supply responses by income quintiles. There is suggestive evidence that the intensive margin channel plays a role in explaining the increase in gross income. Hours worked per week increase by 5 percent three quarters after the shock at top income groups.

An important critique of this approach is that the exogenous reforms used resulted in changes in tax revenues through an array of provisions, each of which could have a different impact across income percentiles. To address this, I limit the analysis to the reforms whose primary provisions were modifications in the top marginal tax rates. I analyze separately the effect of tax reforms that decreased the top marginal tax rates and resulted in a decrease in the average tax rate, and of tax reforms that increased the top marginal tax rates and resulted in an increase in the average tax. The main advantage of this classification is that it allows for a direct test for trickle-down effects of tax cuts.

Figures 1.5.3-1.5.4 show the estimates from equation (1.3). I scale the coefficients by -1 for the tax cut reform coefficients to give an interpretation of a reduction in the tax revenues. Perhaps not surprisingly, income inequality decreases following an increase in tax revenues (left panel of Figure 1.5.3); by 4% at impact and by 19% after two quarters, the decrease persists well after two years. In contrast, tax cuts appear to increase income inequality only 3 quarters after the shock, though these estimates are imprecise and should be interpreted with caution. Overall, consumption inequality (right panel) follows similar dynamics as income inequality. Consumption inequality decreases by 8% two quarters after a tax increase but quickly returns to pre-shock levels; a tax cut, in contrast, has a significant increase in consumption inequality after 2 years, by 40%. The dynamics also suggest a more symmetric response of consumption than income to opposite tax changes.

Figure 1.5.3: Inequality responses to a projected decrease in tax revenues of 1 p.p. as proportion of taxable income



Inequality measured by Gini coefficients. IRFs computed from $y_{t+h} - y_{t-1} = A(L)X_{t-1} + \beta_h^{\text{increase}} \cdot \tau_t^{\text{increase}} + \beta_h^{\text{cut}} \cdot \tau_t^{\text{cut}} + \varepsilon_{t+h}$. 95% confidence intervals based on Newey-adjusted standard errors. $\tau_t^{\text{increase}}(\tau_t^{\text{cut}})$ are the projected increases (decreases) in tax revenues from exogenous reforms with marginal tax rate increases (decreases) as proportion of taxable income. The $\{\hat{\beta_h^{cut}}\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t^{cut}.

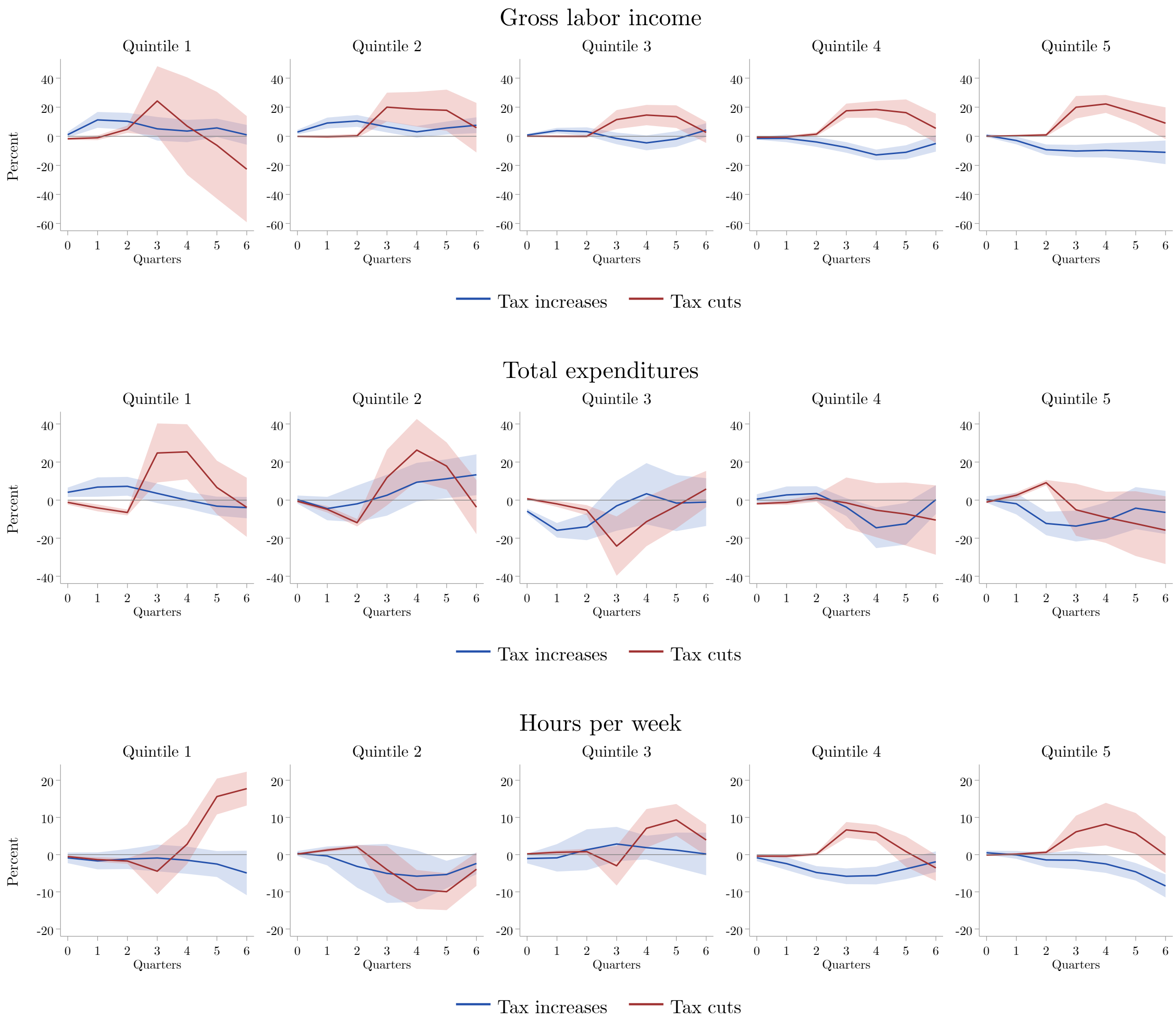
Figure 1.5.4 shows the response of gross income and total expenditure by income quintiles with one standard error confidence bands.[17] There is no strong evidence that a tax cut of top marginal tax rates that decrease income revenues by 1 p.p. trickles down to the bottom of the income of the distribution. Although there appear to be increases across all quintiles, the estimates for the bottom 60% are imprecisely estimated and cannot be distinguished from zero. Tax cuts, however, appear to have significant increases in the top two quintiles by 18 and 22 percent, respectively, during the first year after the shock. In contrast, tax reforms that increased the top marginal tax rates had positive effects on the income of the bottom quintiles and decreased income in the top quintiles. However, the estimates are not significant.

Expenditures mirror the income dynamics for most quintiles; however, the estimates are more prone to large standard errors. With that in mind, it is interesting to note that bottom

[17]This is done for presentation purposes, Appendix Figure1.6.6 shows the results for 95% confidence bands, with some significance loss for specific horizons, but do not change the conclusions laid out here.

quintiles' expenditures seem to fall immediately following a tax cut and increase during the first year.

Figure 1.5.4: Income quintiles responses to a 1 p.p. shock by type of reforms



Quintiles by before-tax income. IRFs computed from $y_{t+h} - y_{t-1} = A(L)X_{t-1} + \beta_h^{\text{increase}} \cdot \tau_t^{\text{increase}} + \beta_h^{\text{cut}} \cdot \tau_t^{\text{cut}} + \varepsilon_{t+h}$. 68% confidence intervals based on Newey-adjusted standard errors. $\tau_t^{\text{increase}}(\tau_t^{\text{cut}})$ are the projected increases (decreases) in tax revenues from exogenous reforms with marginal tax rate increases (decreases) as proportion of taxable income. The $\{\hat{\beta}_h^{cut}\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t^{cut}.

Bottom panel of Figure 1.5.4 suggests some delayed labor responses consistent with the above results, in particular for the top quintiles. The dynamics of the bottom quintile could explain the dynamics on the expenditures following a tax cut. Labor supply is initially flat but increases after a year. Similarly, for the first half year after a tax cut, expenditures decrease, but then increase following the labor supply response. A possible explanation is that workers cut expenditures after top marginal tax cuts, anticipating cuts in benefits or broad-based tax raises in the future. Then, hours and income increase, and with them expenditures.

Annual Inequality

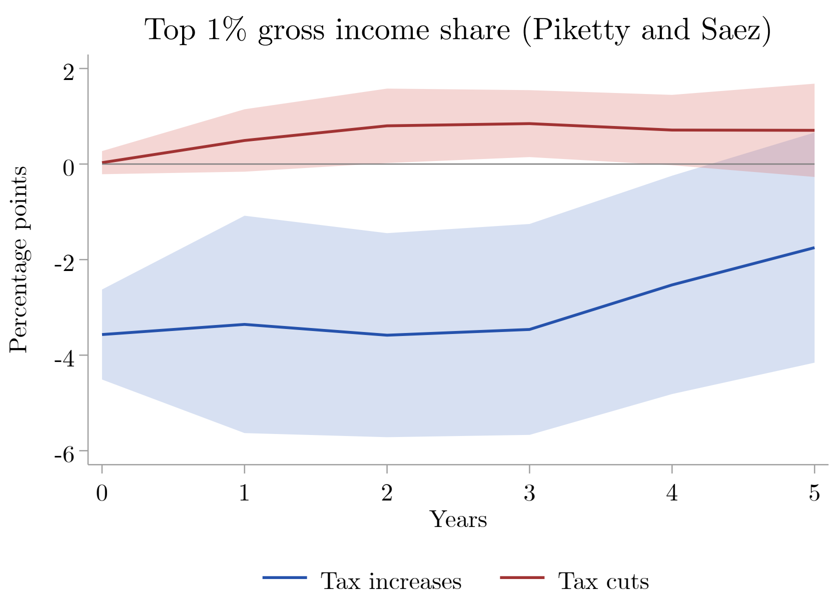
I analyze how robust my results are to extending the time frame to all tax reforms from the post-war period. The drawback is the lack of inequality data at the quarterly level before 1980. As main measure of inequality, I use the top 1% gross income share as computed by Piketty and Saez (2007). I find a close one-to-one response in the increase of income inequality: following an average tax cut of 1 p.p., income inequality increases by 1 p.p. after two years (Figure 1.5.5). The increase in inequality is also robust to the use of other inequality measures that capture inequality in other parts of the distribution, such as the share in disposable income of the bottom 50% and of the bottom 90%. Although not significant, the shares of income in the bottom of the distribution decrease following an average tax cut (Appendix Figure 1.6.7).

Consistent with the findings for quarterly data, an increase in tax revenues by 1 p.p. reduces income inequality by reducing the share of income held by the top 1% by 4 p.p., and the effect persists after four years (Figure 1.5.6). In contrast, a similar sized tax cut increases the share the top 1% by less than 1 p.p. but the increase is not statistically different from zero. Similarly, an ATR increase of 1 p.p. increase the income shares of the bottom 50% and 90% by 1 and 2 p.p., respectively, over 3 years (Appendix Figure 1.6.8).

Figure 1.5.5: Annual inequality response to a 1 p.p average tax rate cut

IRFs computed from $y_{t+h} - y_{t-1} = c_h + A(L)X_{t-1} + \beta_h \tau_t + u_{t+h}$. 95% and 68% confidence intervals based on Newey-adjusted standard errors. τ_t are the projected changes in tax revenues as proportion of taxable income. The $\{\hat{\beta}_h\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t.

Figure 1.5.6: Annual inequality response to 1 p.p average tax rate cut



IRFs computed from $y_{t+h} - y_{t-1} = A(L)X_{t-1} + \beta_h^{\text{increase}} \cdot \tau_t^{\text{increase}} + \beta_h^{\text{cut}} \cdot \tau_t^{\text{cut}} + \varepsilon_{t+h}$. 95% confidence intervals based on Newey-adjusted standard errors. $\tau_t^{\text{increase}}(\tau_t^{\text{cut}})$ are the projected increases (decreases) in tax revenues from exogenous reforms with marginal tax rate increases (decreases) as proportion of taxable income. The $\{\hat{\beta_h^{cut}}\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t^{cut}.

1.6 Conclusion

In this paper, I study the effects of policy-induced changes in federal tax liabilities on income and consumption inequality. To address endogeneity, I rely on the narrative literature that has identified tax changes that did not respond to contemporaneous conditions of the economy (Romer and Romer, 2010; Mertens and Ravn, 2013).

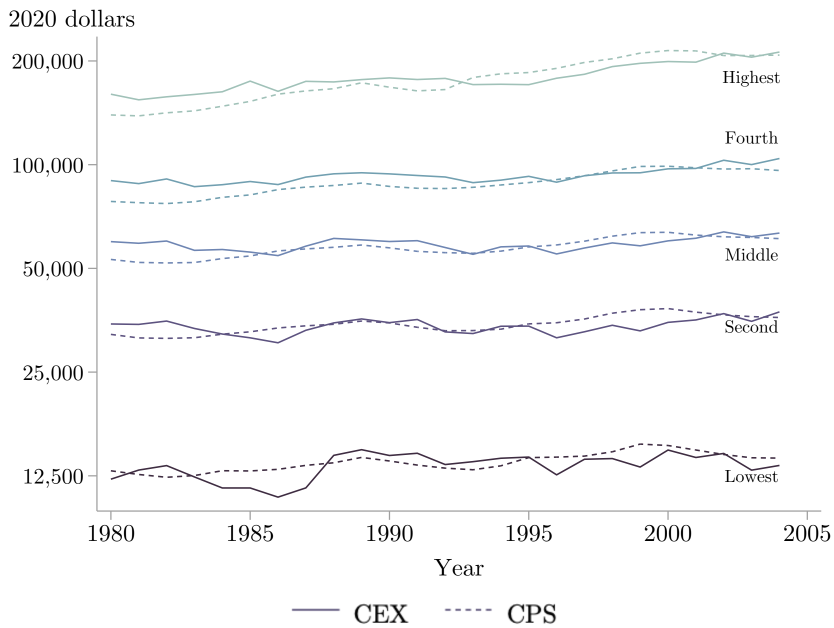
I find that decreasing income tax liabilities as a proportion of personal taxable income by one percentage point increases inequality in gross and disposable income by 12% and consumption inequality by 7%, as measured by Gini indexes, after three quarters. The increase in income inequality is more persistent than consumption inequality. An analysis by income quintiles suggests that different groups' responses across time are behind these patterns. At impact, households at the bottom quintiles reduce their consumption by more than the fall in their incomes, while in longer horizons, income and consumption increase at the top quintiles by increasing the number of hours worked. These patterns are not the result of tax reforms that reduce aggregate liabilities through some regressive provisions. A further analysis of tax legislations with modifications in top marginal tax rates also indicates immediate expenditure reductions in bottom quintiles from tax cuts.

Finally, analyzing tax reforms that included marginal tax changes has two essential policy lessons. First, I do not find evidence of generalized "trickle-down" effects, as opposed to findings in Mertens and Montiel Olea (2018). Reforms that reduced top marginal tax rates only increased the income of the top 40% one year after the tax cut. And second, I only find some evidence of symmetry between tax cuts and tax increases in the dynamics of expenditure inequality.

This paper focused on the labor supply mechanism, but others can rationalize these findings. In particular, these results can be used in a structural model that analyses with more rigor the role of this and other mechanisms including expectations in future government spending, changes in income portfolio, and labor demand.

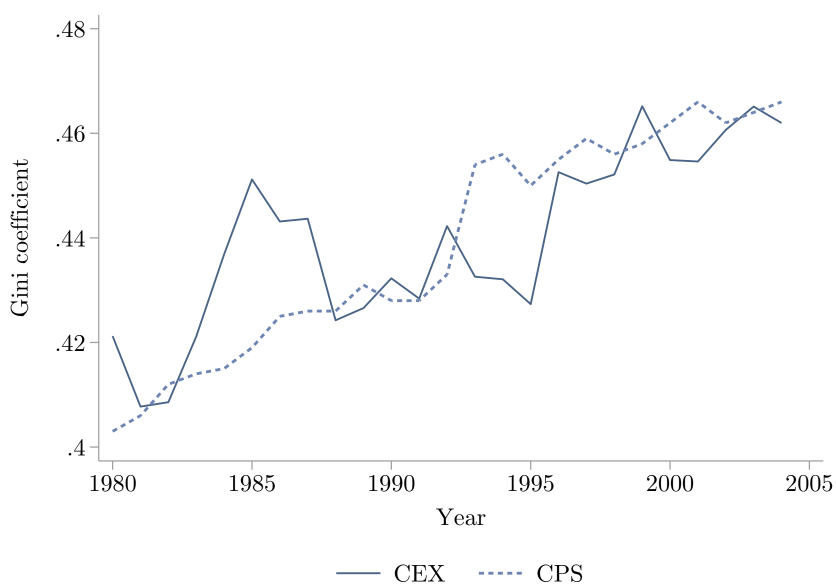
Appendix

Figure 1.6.1: Comparison of CE with CPS by quintiles



Household gross income averages by quintiles from CE (solid lines) and household mean income by quintiles from CPS (dashed lines). 2020 CPI-U-RS adjusted dollars plotted in logarithmic scale. CPS series from Table H-3 of Historical Income Tables.

Figure 1.6.2: CE and CPS Annual Gini Indexes



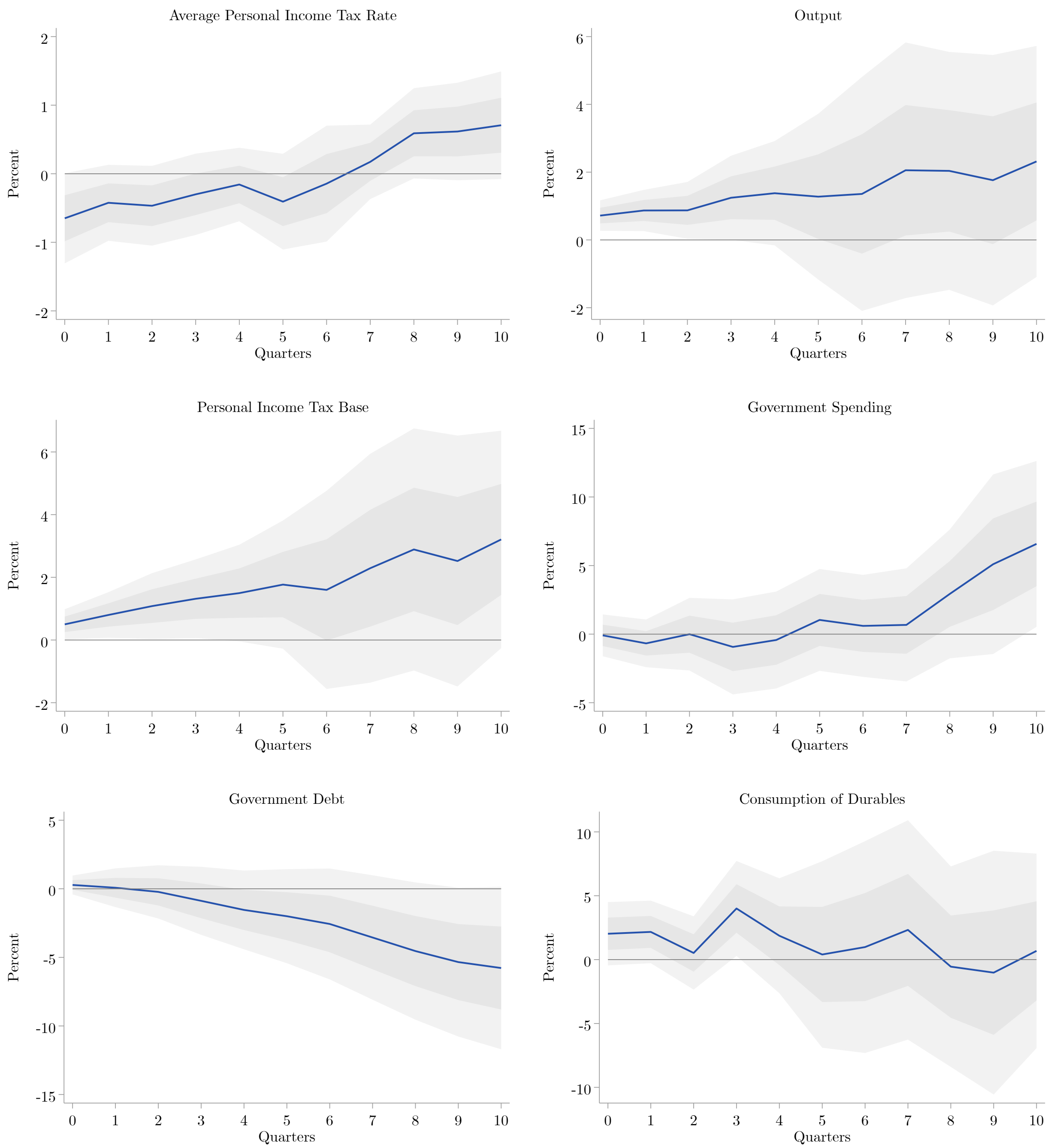
Gini coefficients of CE household gross income by year (own computations) and Gini coefficients for households from CPS (dashed lines). CPS series from Table H-4 of Historical Income Tables.

Table 1.3: Brief description of reforms provisions to individual income taxes

Tax Act	Main Provisions Changed	Effect on nominal liabilities (billions)	Changes in MTR
I.Regressive tax reforms: mechanically increased income inequality			
Revenue Act of 1964	Marginal tax rates decreased across the board. The top marginal tax rate went from 91% to 70%.	-6.7	Yes
Revenue Act of 1971	Reimposition of the investment tax credit, changes to depreciation guidelines and increases in personal excemption.	-3.8	No
Revenue Act of 1978	The act lowered individual tax rates. It widened and reduced the number of brackets, increased the personal exemption and the zero bracket amount and expanded the EITC.	-14.8	Yes
Economic Recovery Tax Act of 1981 (ERTA)	Across-the-board reductions in marginal tax rates. Top marginal tax rate decreased from 70% to 50%	-4	Yes
Deficit Reduction Act of 1984	Limits to income averaging. Repealed tax reductions scheduled to take effect after 1984. Increased income tax credits at bottom of distribution.	5.6	No
Tax Reform Act of 1986 (TRA)	New tax rate scheduled, reduced the 15 income brackets to 5 in 1986. Top marginal tax rate decreased from 50% to 38.5% Increased income taxable base.	-7.2	Yes
Omnibus Budget Reconciliation Act of 1987	Increased employment taxes by expanding the social security wage base	0.8	No
Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA)	Expansion of the lowest tax bracket, reduction of rates on married couples, increase in EITC, and increase in exemptions for the minimum tax.	-94.6	Yes
II. Progressive tax reforms: mechanically decreased income inequality			
Internal Revenue Code of 1954	Increased allowances for medical expenses, child-care expenses, and more liberal rules governing the personal tax treatment of medical insurance and sick benefits.	-0.8	No
Tax Reform Act of 1976	Increase in the minimum tax and tax simplification changes.	1.65	No
Tax Reduction and Simplification Act of 1977	Permanent increase in the standard deduction.	-5.4	No
Omnibus Budget Reconciliation Act of 1990	Increased income tax rates for upper-income taxpayers	14	Yes
Omnibus Budget Reconciliation Act of 1993	Increased income tax rates, mostly for higher earners.	22.8	Yes

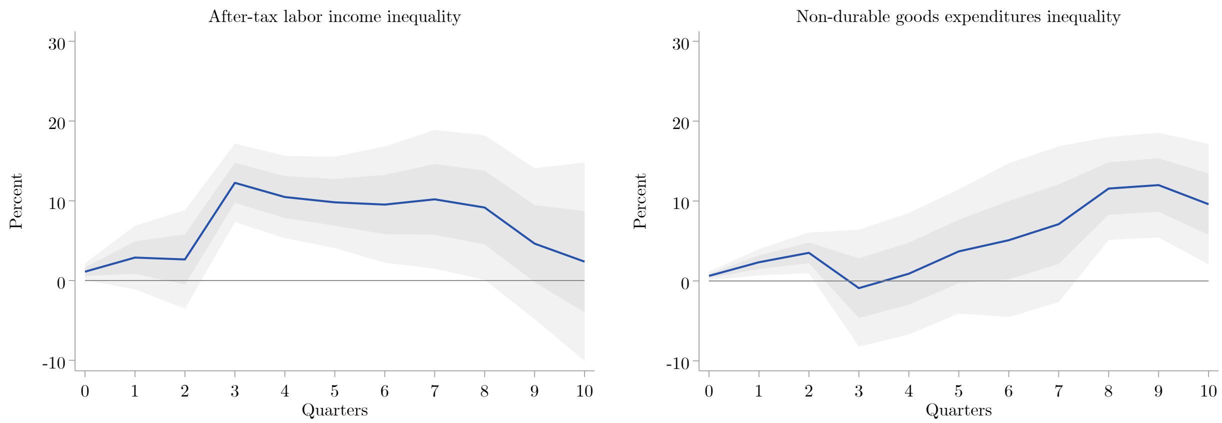
Note: provisions only include those that appartain to individual income taxes. For more details on each of the reforms see Romer and Romer (2009), and Mertens and Ravn (2013) and Mertens and Montiel-Olea (2018) appendixes.

Figure 1.6.3: Response to to a projected decrease in tax revenues of 1 p.p. as proportion of taxable income on aggregate variables



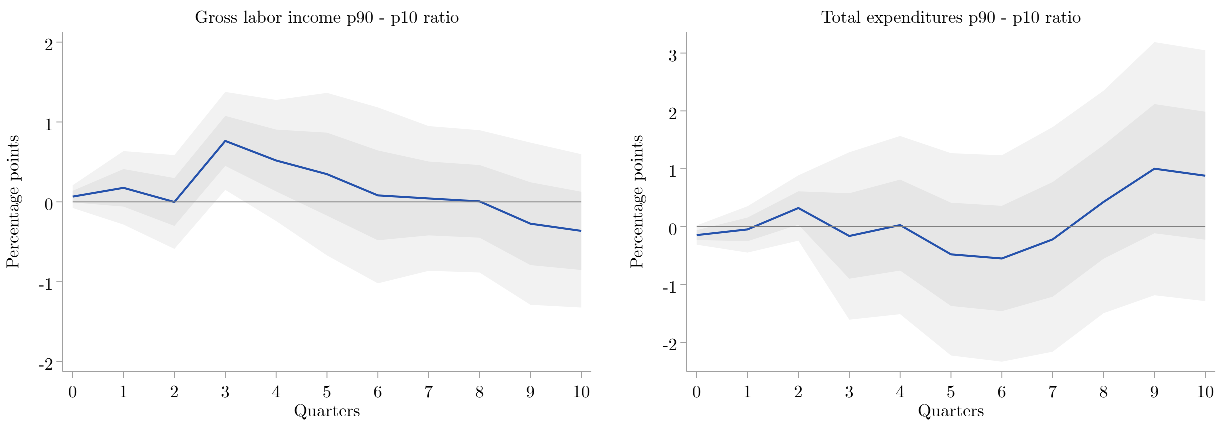
IRFs computed from $y_{t+h} - y_{t-1} = c_h + A(L)X_{t-1} + \beta_h \tau_t + u_{t+h}$. 95% and 68% confidence intervals based on Newey-adjusted standard errors. τ_t are the projected changes in tax revenues as proportion of taxable income. The $\{\hat{\beta}_h\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t.

Figure 1.6.4: Gini indexes responses to a projected decrease in tax revenues of 1 p.p. as proportion of taxable income



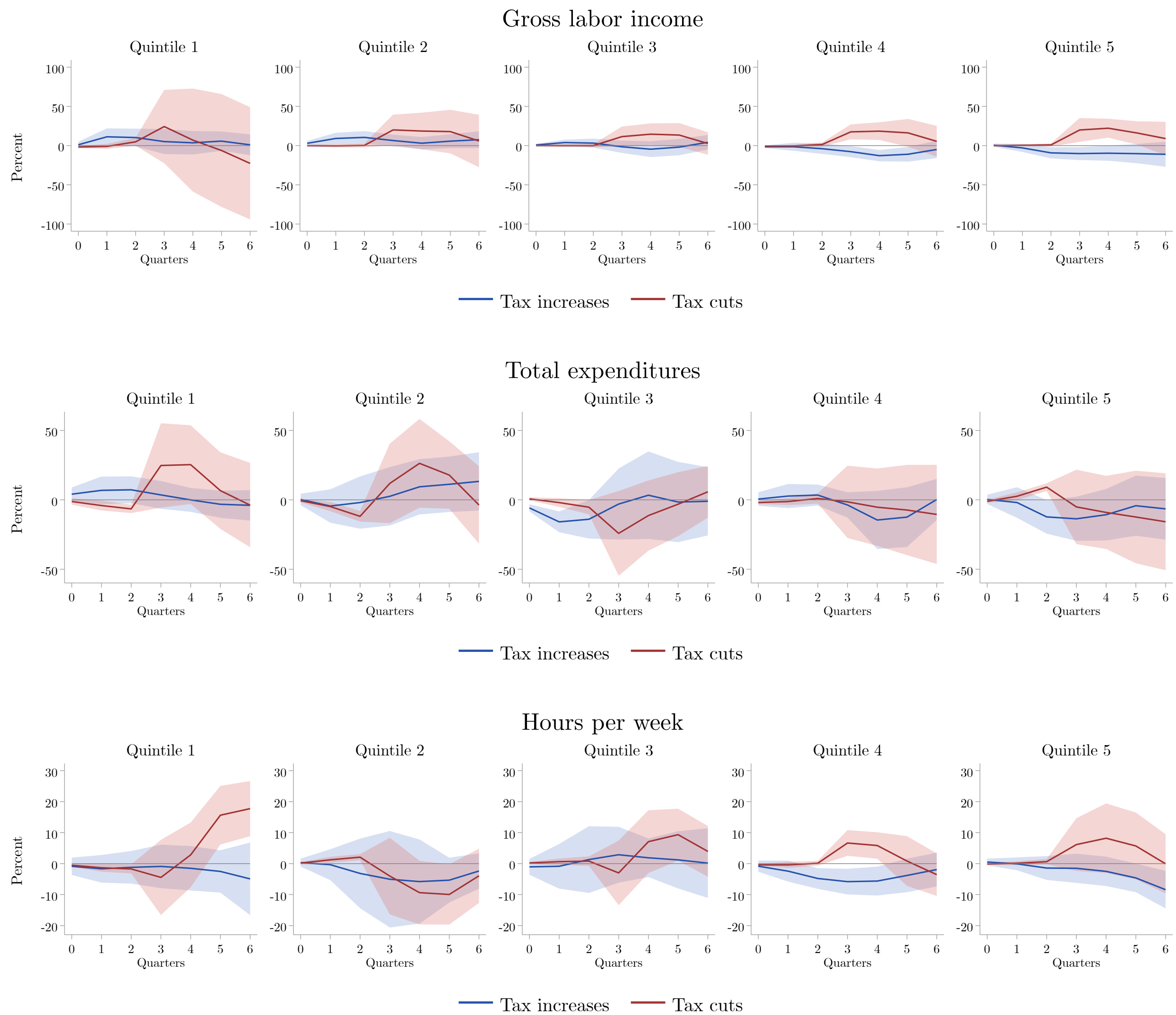
Inequality measured by Gini coefficients. IRFs computed from $y_{t+h} - y_{t-1} = c_h + A(L)X_{t-1} + \beta_h \tau_t + u_{t+h}$. 95% and 68% confidence intervals based on Newey-adjusted standard errors. τ_t are the projected changes in tax revenues as proportion of taxable income. The $\{\hat{\beta}_h\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t.

Figure 1.6.5: Inequality responses to a projected decrease in tax revenues of 1 p.p. as proportion of taxable income



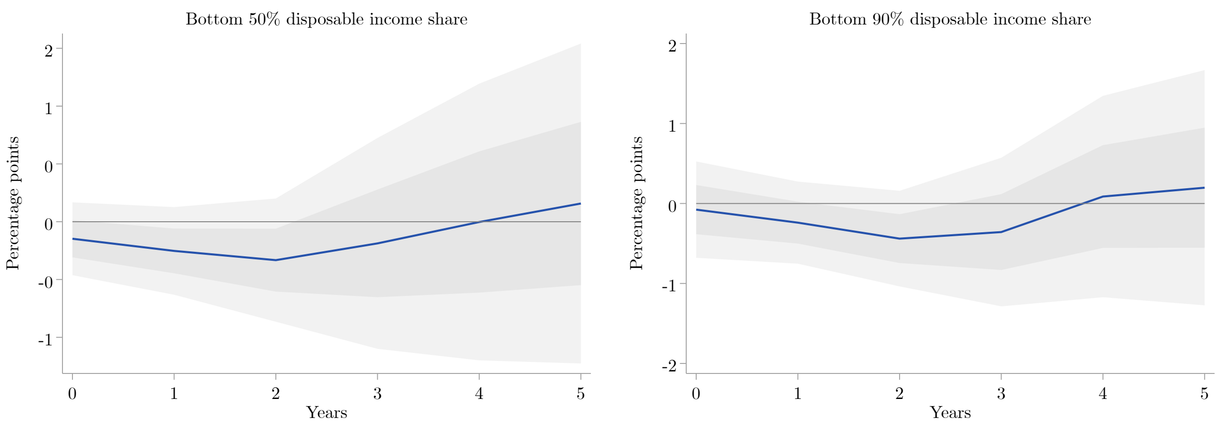
Inequality measured as the ratio between the 90th and 10th percentiles. IRFs computed from $y_{t+h} - y_{t-1} = c_h + A(L)X_{t-1} + \beta_h \tau_t + u_{t+h}$. 95% and 68% confidence intervals based on Newey-adjusted standard errors. τ_t are the projected changes in tax revenues as proportion of taxable income. The $\{\hat{\beta}_h\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t.

Figure 1.6.6: Income quintiles responses to a decrease in tax revenues of 1 p.p. as proportion of taxable income by type of reforms 95% CI



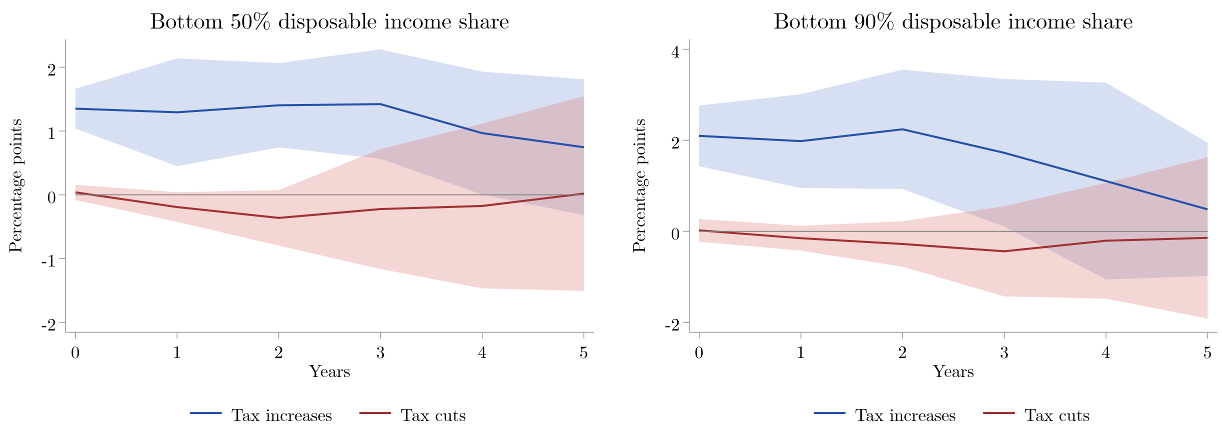
Quintiles by before-tax income. IRFs computed from $y_{t+h} - y_{t-1} = A(L)X_{t-1} + \beta_h^{\text{increase}} \cdot \tau_t^{\text{increase}} + \beta_h^{\text{cut}} \cdot \tau_t^{\text{cut}} + \varepsilon_{t+h}$. 95% confidence intervals based on Newey-adjusted standard errors. $\tau_t^{\text{increase}}(\tau_t^{\text{cut}})$ are the projected increases (decreases) in tax revenues from exogenous reforms with marginal tax rate increases (decreases) as proportion of taxable income. The $\{\hat{\beta_h^{cut}}\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t^{cut}.

Figure 1.6.7: Annual inequality response to a decrease in tax revenues of 1 p.p. as proportion of taxable income



IRFs computed from $y_{t+h} - y_{t-1} = c_h + A(L)X_{t-1} + \beta_h \tau_t + u_{t+h}$. 95% and 68% confidence intervals based on Newey-adjusted standard errors. τ_t are the projected changes in tax revenues as proportion of taxable income. The $\{\hat{\beta}_h\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t.

Figure 1.6.8: Annual inequality response to decrease in tax revenues of 1 p.p. as proportion of taxable income by type of reforms



IRFs computed from $y_{t+h} - y_{t-1} = A(L)X_{t-1} + \beta_h^{\text{increase}} \cdot \tau_t^{\text{increase}} + \beta_h^{\text{cut}} \cdot \tau_t^{\text{cut}} + \varepsilon_{t+h}$. 95% confidence intervals based on Newey-adjusted standard errors. $\tau_t^{\text{increase}}(\tau_t^{\text{cut}})$ are the projected increases (decreases) in tax revenues from exogenous reforms with marginal tax rate increases (decreases) as proportion of taxable income. The $\{\hat{\beta_h^{cut}}\}_{h=0}^{H}$ estimates have been scaled by -1 to reflect the effect of a decrease in the Average Tax Rate implied by τ_t^{cut}. Shares for disposable income from WID (2021).

Chapter 2

Spillover effects of tax changes across income percentiles

2.1 Introduction

The notion that policies favoring the rich lead to benefits for the poor, often times referred to as ”trickle-down theory,” has existed in some form in American politics for over a century.[1] In modern times, discussions of “trickle-down theory” typically center on claims that tax cuts for top earners will result in growth and jobs that benefit the economy more broadly. While there is a considerable related tax literature, there is little direct evidence on the effect of tax cuts for top earners on income for those in lower tax brackets, or conversely the effect of tax cuts for lower earners on income for those in higher tax brackets.

[1]U.S. presidential candidate William Jennings Bryan, in advocating for a bimetallic standard in 1896, described the two contrasting views: “There are two ideas of government. There are those who believe that if you just legislate to make the well-to-do prosperous, that their prosperity will leak through on those below. The Democratic idea has been that if you legislate to make the masses prosperous their prosperity will find its way up and through every class that rests upon it." Democratic National Committee (1896)

In this paper, I analyze the spillover effects of income tax cuts on households in tax brackets not directly impacted by the tax cut. That is, I consider the effect of cuts in the marginal tax rates for low-income brackets on the income of high earners and the effect of cuts in the marginal tax rates for high-income brackets on the income of low earners. I examine whether there is evidence for the "trickle-down" effects and for "trickle-up" effects and discuss where in the income distribution those effects are concentrated. Understanding how responsive income groups are to particular changes in the tax rates of higher or lower income groups is informative about general equilibrium effects of tax policies. This paper contributes to this literature and investigates the spillover effects of top marginal tax cuts on bottom income groups and vice-versa by estimating cross elasticities of taxable income to marginal tax rates with respect to other income groups in the U.S.

In addition to the policy relevance of this topic, there is evidence that targeted changes in income tax rates have different effects in total output. Zidar (2019), for instance, finds that cutting taxes for the bottom 90% of earners by 1 percent of GDP increases real GDP by 3.8% over a 2-year period while a similar-sized tax cut for the top 10% increases GDP by only 1.1%. Mertens and Montiel Olea (2018) find that a tax cut of 1% in the average marginal tax rate (AMTR) of the top 1% increases real GDP by 0.26% while a tax cut of 1% in the AMTR of the bottom 99% increases real GDP by 1.63% after three years.

There are two important challenges in estimating the effect of tax changes across income percentiles. First, many tax changes happen in response to current or expected economic conditions, and second, tax changes for low- and high-income taxpayers often occur simultaneously, so identifying separately the effects of low- and high-income tax cuts is difficult. To address these issues, I use the instruments proposed by Mertens and Montiel Olea (2018), which only use the variation in marginal tax rates from *exogenous* tax reforms (legislations that were motivated on long-run concerns according to Romer and Romer (2010)'s classification). Although the Mertens and Montiel Olea's AMTRs instruments are available for several income percentiles, their spillover analysis is focused on the top 1% and bottom 99% incomes. I examine the spillovers across several other percentiles, the top 1%, the top 5%, the top decile, the bottom 99% and bottom 90% incomes as measured by short-run cross income elasticities and IRFs. In this way, this work can be seen as a natural follow-up of Mertens and Montiel Olea (2018)'s work. The disaggregated analyses in this paper also provide nuances to their results.

Mertens and Montiel Olea results, for instance, indicate that a 1% cut in the top 1%'s AMTR rise the bottom 99%'s average income by 0.44% in the following year. Taken on its own, this would suggest some evidence for "trickle-down" effects. However, I find that most of the increase in the bottom 99% income can be attributed to the raise in income of the top decile. I find that the increase in the bottom 90%'s income is not statistically significant at any horizon. In other words, I find that the "trickle-down" effects are only concentrated on the top incomes. I also document that the lack of evidence for positive spillovers of tax cuts at the top is not exclusive to the top 1%.

On the other side of the income distribution, Mertens and Montiel Olea conclude that a 1% cut in the bottom 99%'s AMTR has a negative effect in the top 1%'s income in the first two years. My findings suggest that while this result is most likely coming from changes in the AMTR of the bottom 90%, there are positive spillovers to top income groups from relatively lower income groups. In particular, the income of the top decile (excluding the top 1%) increases following a tax cut in the AMTR of the bottom 90%, consistent with Zidar (2019) findings that tax cut's in the bottom 90% generate growth.

In the next section, I briefly describe the data used, which I mostly borrow from Mertens and Montiel Olea (henceforth MMO). In the second section, I estimate IV and reduced-form short-run cross elasticities of taxable income (ETI) using MMO's instruments. I next present estimates for spillovers at longer horizons from IRFs estimated through local projections. The last section discusses the implications of my results.

2.2 Data

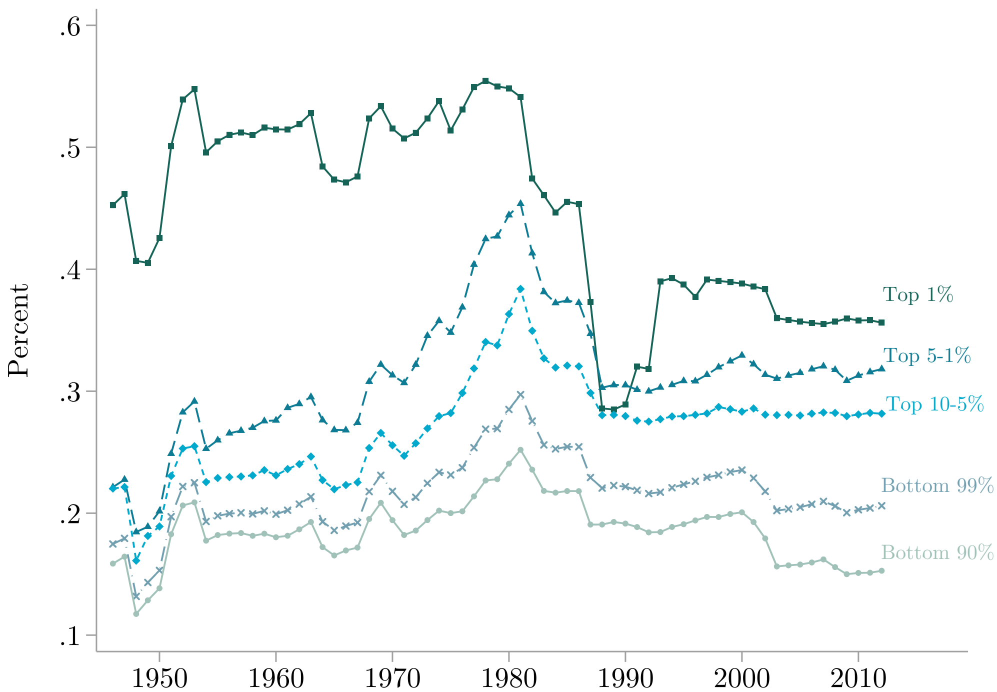
I use data from MMO. I use their income-weighted average marginal federal individual income tax rates for the US, from 1966 onwards computed through the NBER's TAXSIM and prior based on approximations using the annual Statistics of Income from the IRS and probability distributions for adjusted gross income for each return.[2] The TAXSIM system contains a large sample of tax returns and and provides programs to compute the tax liabilities of each return. The tax units used are all males aged 20 years and older. To compute the AMITR, the program first calculate the tax liability of each return for the actual income and then for an hypothetical case where each source of income is increased by 1%. The marginal tax rates are the difference in tax liabilities divided by the difference in total incomes. The annual series are plotted in Figure 2.2.1, span the years from 1946 to 2012 and are disaggregated at a number of percentiles: top 1%, top 5% excluding the top 1%, top 10% excluding the top 5%, bottom 99% and bottom 90%. Percentiles are based on MMO's updated series of income distribution provided in Piketty and Saez (2003).[3] The corresponding income series for each percentiles refer to a measure of taxable income used in the literature Piketty and Saez (2003, 2013) and includes all sources of market income (labor earnings, entrepreneurial income, dividends, interest, and rents) before deductions and exemptions but excludes realized capital gains and government transfers. The series by

[2] For details see Mertens and Montiel Olea (2018)'s Appendix A.1.

[3] In MMO specifications, they look at all income tax rates, that include payroll taxes. In contrast, I use only individual income taxes.

percentiles also comes from MMO, and are the updated series from Piketty and Saez (2003). The annual averages are in constant 2010 dollars per tax unit and are reported in Table 2.2.1 in the Appendix.

Figure 2.2.1: Average marginal income tax rates by income percentiles



Source: Mertens and Montiel Olea (2018).

Other aggregate variables used are the same as in MMO. Real GDP per tax unit (line 1 of NIPA Table 1.1.3) divided by tax units; inflation is the log change in the CPI Research Series Using Current Methods (CPI-U-RS) from the Bureau of Labor Statistics (BLS). The Federal funds rate is the annual average effective Federal funds rate from the Board of Governors. Government debt per tax unit is federal debt held by the public (line 19 of Table L.106 from the U.S. Financial Accounts from the Federal Reserve), divided by the CPI-U-RS and tax units. Government spending per tax unit is the sum of federal government purchases, net interest rate expenditures and net transfers (line 46 minus lines 3, 4, 7, 10, and 11 of NIPA

Table 3.2, plus line 25 of NIPA Table 3.12), divided by the CPI-U-RS and tax units. The real stock price is the S&P composite index from updates of Shiller (2000), divided by the CPI-U-RS. The unemployment rate is the the civilian non-institutional population aged 16 or older from the BLS.

2.3 Short run ETI

I define a cross-elasticity of taxable income (cross-ETI) as the percentage change in the income of group j following an increase of 1% in the net of taxable marginal income tax rate (1-AMITR) of group k, with $k \neq j$. I estimate simultaneously short-run ETI and cross-ETI for each income group j according to:

$$\ln(\text{income}_{t+s}^{j}) - \ln(\text{income}_{t-1}^{j}) = \alpha^{j} \Delta ln(1-\text{AMITR}_{t}^{j}) + \beta^{j} \ \Delta ln(1-\text{AMITR}_{t}^{k}) + \gamma^{j} X_{t-1}^{j} + \varepsilon_{t}^{j} \quad (2.1)$$

for the same year ($s = 0$) and a year after the tax change ($s = 1$). X_{t-1}^{j} are the same controls used in MMO to estimate short-run own-ETI and include two lags of jth's income, two lags of the net of income marginal tax rate, and two lags of macroeconomic indicators.[4] The α^{j} coefficients capture the (own) elasticity of the net of tax marginal income rate on income, and the β^{j} coefficients the cross-ETI or spillover effect of k's net of tax marginal tax rate on j's income. To illustrate, for two income groups loosely referred to as "top" and "bottom," evidence for trickle-down effects would be found if $\beta^{\text{Bottom}} > 0$, and evidence for trickle-up effects if $\beta^{\text{Top}} > 0$.[5]

[4] Specifically, controls include two lags of the following variables: (log) income of main percentile, AMITR, real GDP per tax unit, unemployment rate, (log) government spending per tax unit, debt growth rate, inflation, (log) real estate prices and federal fund rates, as well as an indicator variable for the years 1949 and 2008.

[5] The ETI estimates from equation (2.1) for different income groups j also offer general guidelines to assess the effect of a change in the AMITR of one income group on income inequality. For example, an increase of 1% in the AMITR of the "top" income group would lead to a change in the income gap between the "top" and the "bottom" income groups by $(\alpha^{\text{Top}} - \beta^{\text{Bot}})$% in partial equilibrium.

The main threat to identification in (2.1) is that the inclusion of the macroeconomic variables as controls fails to capture unobservables that correlate with both AMITRs and income growth, and thus leads to biased estimates. To address these concerns, I use as instruments exogenous statutory AMITR changes as computed by MMO.

The statutory AMITR change in year t is calculated as the difference between two counterfactual AMITRs computed using the year $t-1$ income distribution. The first counterfactual computes the AMITR according to the tax scheme of new policies to be implemented in year t deflated by any automatic adjustments between $t-1$ and t. The second AMITR counterfactual uses the tax rates and brackets that would have been effective in year t had the policy changes not occurred (i.e. typically last year's tax scheme and therefore the actual AMITR in $t-1$). I refer to these series as exogenous statutory changes since the policy reforms used are only those classified as *exogenous by* Romer and Romer (2010), i.e. if the reform was motivated by long-run concerns such as reducing debt, rather than concurrent economic conditions.[6]

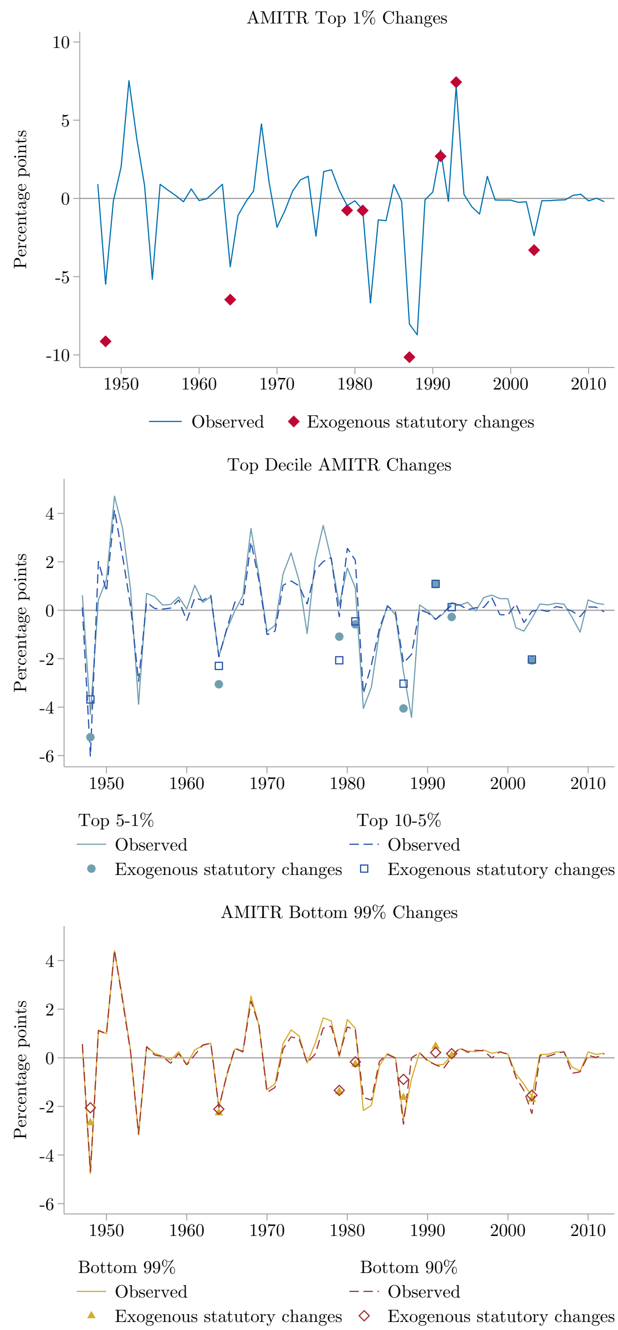
In other words, if we consider the AMITR as a function of the income distribution I_t, and the tax schedule τ_t, AMITR (I_t, τ_t), then the statutory change can be formulated as:

$$\text{Statutory change}_t \quad = \quad \text{AMITR}(I_{t-1}, \tau_t) - \text{AMITR}(I_{t-1}, \tau_{t-1}) \qquad (2.2)$$

The observed annual changes in the AMITR and exogenous statutory changes are displayed in Figure 2.3.1. The top 1% experiences the largest swings in the AMITR. Notable are the tax cuts of the 1980s, clearly seen in the top income groups, but with smaller reductions for the bottom 90%. It is noticeable the collinearity of some AMITR changes, especially for

[6] For a brief description of each of these reforms, see Appendix Chapter 1 Table 1.3.

Figure 2.3.1: Percentage changes in the AMITR and statutory changes by percentiles



Changes in the income-weighted averages of marginal federal individual income tax rates. Most of the AMITR series are based on the NBER's TAXSIM datasets and programs. For exceptions and more details see Mertens and Montiel Olea (2018)'s Appendix A.1. Percentiles are based on the updated series for income distribution of Piketty and Saez (2003). Income includes labor earnings, entrepreneurial income, dividends, interest, and rents and excludes capital gains and government transfers. Tax units are all men aged 20 or over. Exogenous AMITR statutory changes computed as the difference between two counterfactual AMITRs using the year $t-1$ income distribution. The first counterfactual computes the AMITR according to the tax scheme of the Romer and Romer (2010)'s exogenous tax reforms that modified statutory marginal tax rates that would be implemented in year t deflated by any automatic adjustments between $t-1$ and t. The second AMITR counterfactual uses the tax rates and brackets that would have been effective in year t had the policy changes not occurred. Source: Mertens and Montiel Olea (2018).

the top decile excluding the top 1 percent), but the statutory changes are less correlated. It is also interesting to see that some of the observed changes in AMITR are very close to the size of the statutory changes while some are opposite in sign. However, there is no clear systematic pattern.

I examine the evidence for trickle down effects in Table 2.1, where I report the β^j spillover effects of a tax cut in the AMITR of income groups in the top decile on bottom income groups. Panel A shows the 2SLS short-run cross-ETI estimates within 1 and 2 years using the statutory changes described above as instruments for changes in AMITR and Panel B reports the reduced estimates.

Columns (1)-(4) show the spillover estimates of a tax cut in the AMITR of the top 1%. The estimates suggest the presence of trickle-down effects of a tax cut in the top 1% on the rest of the top decile (columns 1-2) but not to the overall income distribution as summarized by either the bottom 99% (column 3) or the bottom 90% (column 4). The largest spillovers appear to be concentrated in the top 5% with a cross-ETI of 0.42 and 0.6 in the top 5% (column 1) in the same year and a year after the tax cut. The positive effects of a tax cut at the very top extend to the rest of the top decile but declines as we descend the income distribution. The average income of the group between the top 10 and the top 5% (column 2) increases by 0.27% at impact and by 0.32% the following year, but the estimate is not statistically significant. Unlike the results from MMO, I do not find significant short-run responses in the income of the bottom 99%. Their estimates imply short-run ETIs of 0.23 at impact and 0.44 in the following year, comparable in magnitude to my own estimates of 0.19 and 0.4, respectively. Consistent with these results, it is not surprising to see a very small and not significant response of the incomes of the bottom 90%, of 0.01% at impact

Table 2.1: Short-run Cross ETI - Trickle down effects

	Income response to a decrease of 1% in the Average Marginal Income Tax Rate of							
	Top 1%				Top 5-1%		Top 10-5%	Top 10%
On the income of the	Top 5-1%	Top 10-5%	Bottom 99%	Bottom 90%	Top 10-5%	Bottom 90%	Bottom 90%	Bottom 90%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. 2SLS Estimates								
Same year	0.416**	0.269*	0.188	0.012	-0.760	13.287	18.181	-0.655
	(0.16)	(0.15)	(0.22)	(0.31)	(2.61)	(74.26)	(110.97)	(2.10)
	[3.6]	[6.8]	[12.2]	[11.8]	[6.8]	[15.9]	[13.3]	[13.1]
Following year	0.594**	0.320	0.403	0.262	-1.158	10.011	18.389	0.391
	(0.26)	(0.24)	(0.35)	(0.47)	(3.10)	(52.81)	(116.15)	(2.84)
	[3.7]	[6.6]	[11.4]	[11.3]	[6.6]	[15.5]	[13.1]	[12.9]
B. Reduced-form estimates								
Same year	0.553***	0.349**	0.229	0.105	-0.247	-0.644**	-0.749	0.054
	(0.17)	(0.14)	(0.27)	(0.29)	(1.04)	(0.32)	(0.57)	(0.61)
Following year	0.775***	0.420**	0.482	0.366	-0.547	-0.190	-0.436	0.559
	(0.19)	(0.21)	(0.33)	(0.36)	(1.56)	(0.64)	(1.00)	(0.83)

Each column contains the percentage change estimate of percentile j's income in the same year and a year after a decrease of 1% in the AMITR of income group k. Each cell is the β^j 2SLS cross-ETI (Panel A) or reduced-form estimate (Panel B) from equation $\ln(\text{income}^j_{t+s}) - \ln(\text{income}^j_{t-1}) = \alpha^j \Delta ln(1-\text{AMITR}^j_t) + \beta^j\ \Delta ln(1-\text{AMITR}^k_t) + \gamma^j X^j_t + \varepsilon^j_t$ using the exogenous statutory changes from Mertens and Montiel Olea (2018) as instrument for the changes in the AMITR of income group k. For example, in column (1) the j group is the top 5-1%'s and k is the top 1%. Robust standard errors in parenthesis and first stage F-statistic in brackets.

and of 0.26% after a year.

The dearth of evidence for trickle down effects of tax cuts to lower income groups is not exclusive to tax cuts in the top 1%. In the next columns, I investigate the effects of tax cuts in the AMTR of the income group in for the top 5% of earners excluding the top 1% (5-1%), for the top 10% of earners excluding the top 5% (10-5%), and for earners in the top decile. Although none of the 2SLS estimates are significant at usual significance levels, it's worth discussing some points. First, the top 10-5%'s short-run cross ETI of a tax cut in the top 5-1% (column 5) are negative, suggesting the presence of income effects. Second, the response of the average income of the bottom 90% to tax cuts in the top 5-1% (column 6) and in the top 10-5% (column 7) are very imprecisely estimated.[7] And third, the cross ETIs of the bottom 90% to a tax cut at the top 10% is -0.65 in the same year and 0.39 the following year, but the standard errors are well above the point estimates. For all these estimates, the F-statistic of the first stage regressions (in brackets) are above usual levels, except for the cross-ETI of the top 5-1% to the AMITR of the top 1%.

Table 2.2 examines the inverse phenomenon: the response of top incomes following a tax cut in the AMITR of lower income brackets, or trickle up effects. I start discussing the presence of spillover effects on the top 1%. The same year cross-ETI with respect to a tax cut in the AMITR of the bottom 99% is negative and significant, suggesting that the top percentile's income decreases almost 3% a year after a decrease of 1% in the AMITR of the bottom 99% (column 1). The effect is larger a year after (a decrease of 4.2%) but is not significant. MMO's findings are in line with these estimates. They find that the top 1%'s income decreases almost by 5% following a 1% tax cut in the bottom 99%'s AMITR. The top 1% cross-ETI with respect to the AMITR of other lower income groups, the top 5-1%

[7]The responses of the averages of the incomes of the bottom 90% oscillate between the 13.2% for a tax cut in the top 5-1% (column 6) to 18.2% for a tax cut in the top 10-5% (column 7).

Table 2.2: Short-run Cross ETI - Trickle up effects

	Trickle up effects of a decrease of 1% in the Average Marginal Income Tax Rate of							
	Bottom 99%	Top 5-1%	Top 10-5%		Bottom 90%			
On the income of the	Top 1%	Top 1%	Top 1%	Top 5-1%	Top 1%	Top 5-1%	Top 10-5%	Top 10%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. 2SLS Estimates								
Same year	-2.909*	-1.602	-1.730	1.030	-3.867*	2.367	2.408	-5.196
	(1.57)	(1.04)	(1.32)	(2.08)	(2.21)	(13.35)	(7.54)	(6.28)
	[13.7]	[13.9]	[13.4]	[3.4]	[13.6]	[3.5]	[6.7]	[5.7]
Following year	-4.273	-1.433	-2.545	1.181	-6.238	1.089	4.588	-8.253
	(2.55)	(1.35)	(1.94)	(2.71)	(4.09)	(13.63)	(12.82)	(10.89)
	[13.7]	[13.9]	[13.4]	[3.4]	[13.6]	[3.5]	[6.7]	[5.7]
B. Reduced-form estimates								
Same year	-2.692**	-1.357	-1.492	0.680	-2.908*	-0.076	-0.350	-1.895
	(1.33)	(0.91)	(1.02)	(1.55)	(1.51)	(0.72)	(0.63)	(1.47)
Following year	-3.393	-0.168	-1.822	0.603	-4.335**	0.301	-0.950	-2.987**
	(2.29)	(1.46)	(1.53)	(1.90)	(2.05)	(0.95)	(1.00)	(1.25)

Each column contains the percentage change estimate of percentile j's income in the same year and a year after a decrease of 1% in the AMITR of income group k. Each cell is the β^j 2SLS cross-ETI (Panel A) or reduced-form estimate (Panel B) from equation $\ln(\text{income}^j_{t+s}) - \ln(\text{income}^j_{t-1}) = \alpha^j \Delta ln(1-\text{AMITR}^j_t) + \beta^j\ \Delta ln(1-\text{AMITR}^k_t) + \gamma^j X^j_t + \varepsilon^j_t$ using the exogenous statutory changes from Mertens and Montiel Olea (2018) as instrument for the changes in the AMITR of income group k. For example, in column (1) the j group is the top 1%'s and k is the bottom 99%. Robust standard errors in parenthesis and first stage F-statistic in brackets.

(column 2) and the top 10-5% (column 3) are also negative, ranging from -1.4 to -2.5, though they are imprecisely estimated and statistically indistinguishable from 0 at conventional levels. Not surprisingly, the effect of a tax cut in the AMITR of the bottom 90% has a larger negative effect on the income of the top 1%, nearly -3.9% the same year (column 5) . The analysis of other income groups would suggest the presence of positive trickle-up effects, at least for the incomes of the top 5-1% (columns 4 and 6) and of the top 10-5% (column 7). However, none of the estimates are significant. Finally, consistent with the results for the top 1%, the average income of the top decile decreases following a tax cut in the AMTR of the bottom 90% (column 8).

For completion, Table 2.4 in the Appendix shows the corresponding own-ETI for each income percentile, controlling for different income groups' AMITRs. It is worth noting that including other income groups' AMITRs brings closer the OLS estimates to the IV estimates, suggesting that OLS ETI estimates were suffering form omitted variable biases in previous specificiations, for example around the unity for the top 1%'s ETI (compared to the ETI of 0.48 in MMO using OLS). The 2SLS-IV own-ETI estimates are only significant at conventions levels for the top 1%, they range from 1.18 to 1.7 for the same-year, and from 1.8 to 3 for the next year, depending on which secondary income group is used as control. In general, the own-ETI are larger the more separated is the secondary group from the top 1%, in other words there is evidence for some monotonicity. The implication of these results on income inequality are in line with MMO's. In partial equilibrium, a cut in the AMTR of the top 1% unambiguously leads to an increase in income inequality since the increase in the top 1% income (of 1.46%) is higher than the increase in the income fo the bottom 99% (0.18%, although not significant).[8] Meanwhile a tax cut in the bottom 90% or in the bottom 99% AMITR would lead to a decrease in inequality mainly through a reduction in the incomes

[8]Consistently, the estimates suggest the income of the rest of the top decile would increase between 0.27 and 0.4% with no meaningful increases in the income of the bottom 90%.

of the top percentile.

Given the large standard errors associated with the 2SLS estimates, to look at dynamics at longer horizons, I will be focusing in the reduced-form estimates associated with equation (2.1). The counterpart of the short-run ETIs using the exogenous statutory changes as main regressors are shown in panels B of Tables 2.1 and 2.2. It is reassuring to see that the reduced form are very similar to the 2SLS estimates. One main difference is that the coefficients will be reflecting income responses to a tax cut of 1 percentage point in the AMITR, and thus would be interpreted as semi-elasticities.

2.4 Dynamic Spillovers

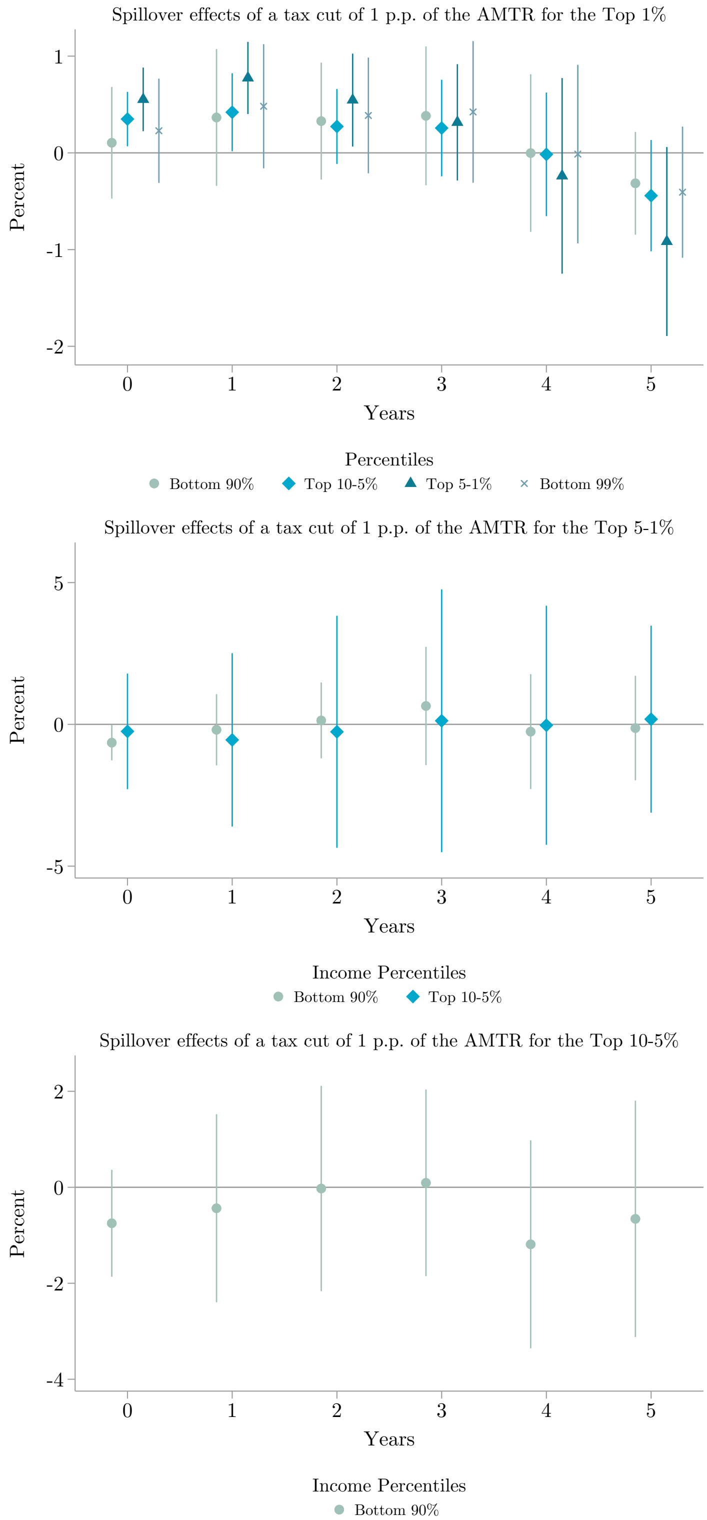
The cross-ETIs estimated in the previous section provide short-run responses to AMITR changes. Given the large standard errors associated with the IV estimates, in particular for income groups outside the top 1 percent, I use the following reduced-form specification to examine the dynamics beyond the two-year horizon:

$$\ln(\text{income}^j_{t+h}) - \ln(\text{income}^j_{t+h-1}) = a^j_h z^j_t + b^j_h z^k_t + c_h X^j_{t-1} + e^j_{t+h} \tag{2.1}$$

where z^j_t is the exogenous statutory change as defined in (2.2) for $h = 0, 1, ...5$. The b^j_h estimates can then be interpreted as group j's income response to a 1 p.p. policy-induced increase in k's AMITR after h periods, or simply put, as the spillover effects of group k's AMITR on j's income. Figures 2.4.1-2.4.2 show the spillovers effects from a tax cut of 1 p.p. of the AMITR of income group k on the rest of the income distribution.

I first examine how income respond to tax cuts at the top of the income distribution. The upper panel of Figure 2.4.1 shows the responses to cutting the AMITR of the top 1% by 1

Figure 2.4.1: Trickle down effects



Reduced form estimates of b_h^j from $\ln(\text{income}_{t+h}^j) - \ln(\text{income}_{t+h-1}^j) = a_h^j z_t^j + b_h^j z_t^k + c_h X_t^j + e_{t+h}^j$ for horizon $h = 0, 1, ..., 5$. All estimates include 95% confidence bands from Newey-adjusted standard errors.

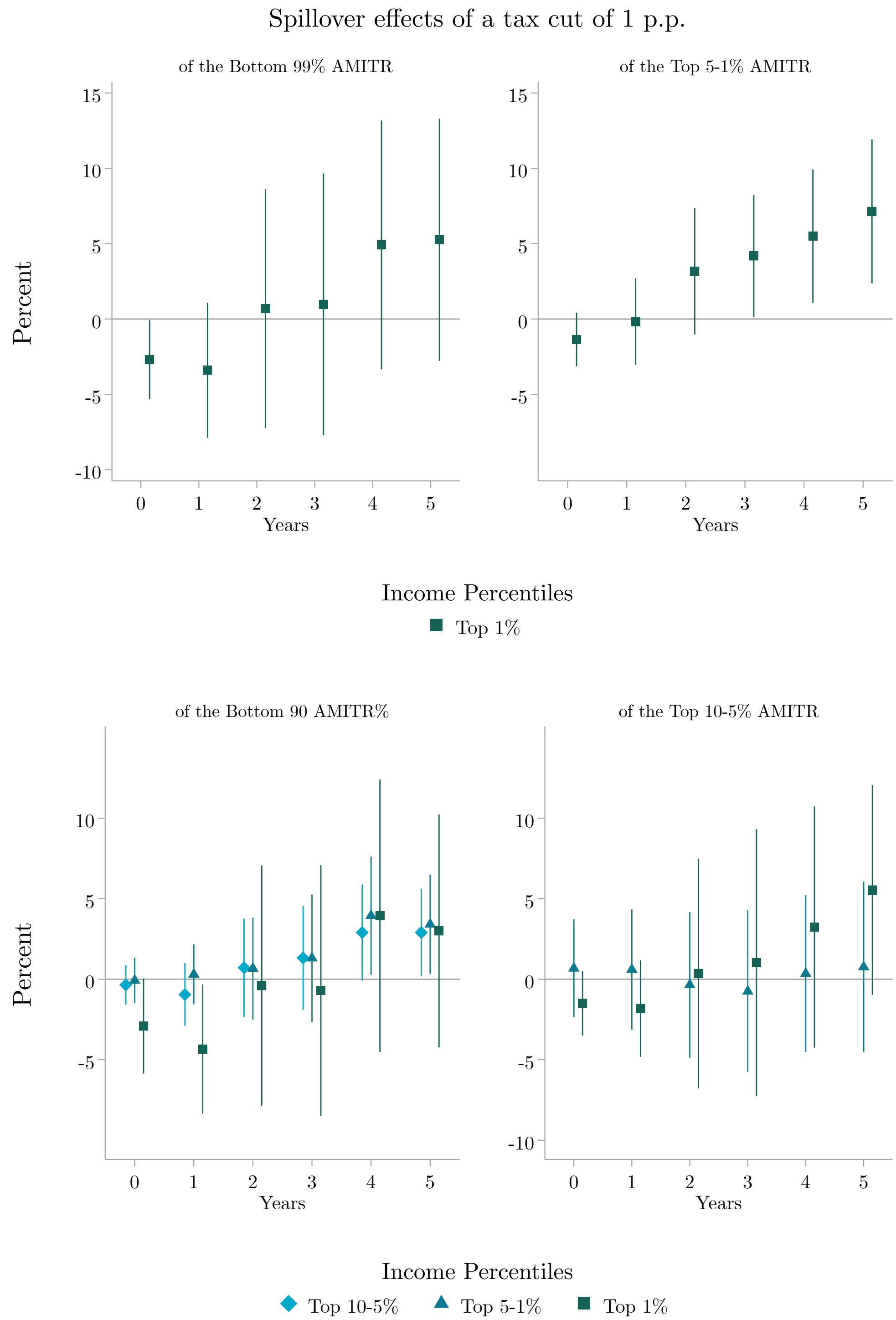
p.p. The increase in the average income for groups outside the top 1% in the first two years is consistent with the short-run cross-ETI estimated in the previous section.

The incomes of the top 10-5% and of the top 5-1% groups increase in the first four years, but are not significant after the third year. The response of the bottom 90% and the bottom 99% is not significant at any horizon, suggesting that the trickle-down effects concentrate on the top decile. Moreover, there is suggestive evidence that spillovers are monotonic, highest for the richest income group outside the top 1% in the first three years. However, the positive spillovers in the top decile are short-lived and indicate a reversal after four years. There is suggestive evidence that in the medium term a tax cut in the top 1% leads to decreases in income across all percentiles, in particular for the top 5% (although the estimates are not significant).

The reduced-form estimates also suggest that there is no evidence of positive spillover effects from tax cuts in other top income groups. A 1 p.p. tax cut in the AMITR of the top 5-1% decreases income on the bottom 90% by 0.64% in the same year, but has no effect on further years, nor on the income of the top 10-5% (central panel of Figure 2.4.1). The responses of the bottom 90% to tax cuts in the top 10-5% are also very imprecisely estimated (bottom panel of Figure 2.4.1).

Figure 2.4.2 summarizes the dynamics of top incomes following a tax cut in lower income groups. The response of the top 1%'s income to a tax cut of 1 p.p. in the AMITR of the bottom 99% is similar to MMO's, a decrease in the first years followed by an increase in later years, with wide confidence intervals (upper left panel). The top 1%'s income responds in a similar pattern to a 1 p.p. cut in the AMITR of the top 5-1% (upper right panel), with large increases after 2 years, up to 7.1% after five years and significant.

Figure 2.4.2: Trickle up effects



Reduced form estimates of b_h^j from $\ln(\text{income}_{t+h}^j) - \ln(\text{income}_{t+h-1}^j) = a_h^j z_t^j + b_h^j z_t^k + c_h X_t^j + e_{t+h}^j$ for horizon $h = 0, 1, ..., 5$. All estimates include 95% confidence bands from Newey-adjusted standard errors.

The responses to tax cuts in the rest of the income distribution help to clarify the top 1% income dynamics to a bottom 99% AMITR cut. In particular, it is noticeable that at impact and a year after a tax cut in the bottom 90% (bottom left panel), the top 1%'s income decreases by 3% and 4.3% respectively. Meanwhile, the medium term positive spillover in the top 1% is largely driven by cuts at the top 5%.

For the income of the top 5-1%, the positive spillovers of a tax cut in the AMITR of the bottom 90% are stronger after 4-5 years, as seen in the bottom right panel of Figure 2.4.2. A 1 p.p. tax cut for example, leads to a 4% income increase after four years. In contrast, the standard errors for the spillovers from the top 10-5%'s AMITR are wide and do not allow to make meaningful conclusions at longer horizons (bottom left panel).

Finally, the reduced-form estimates also suggest the presence of positive trickle-up effects from a tax cut in the AMITR of the bottom 90% onto the income of the top 10-5% (bottom left panel) very similar to the effects on the top 5-1% income.

As a robustness check, Appendix Figures 2.5.1-2.5.2 show the semi-own-elasiticities estimated with different control groups, showing the robustness for top 1% and bottom 90% to different specifications, but weak evidence of robustness for the top 5-1% and the top 10-5%. The dynamics for the top 1%'s income are remarkably similar to the ones estimated by MMO. Although point estimates for the top decile income (excluding the top 1%) would suggest that the semi elasticities are sensitive to the choice of the AMITR of the secondary group, the differences of the estimates are small relative to their standard errors.

2.5 Conclusion

In this paper, I use Mertens and Montiel Olea (2018)'s instruments for the AMTR of specific income percentiles to estimate responses to a tax cut in the AMTR of income groups in the highest decile onto lower income groups and vice-versa. In this way, my analysis extends previous work that examined the feedback between the AMTR of the top 1% and the bottom 99% to broader income categories.

I present new short-run cross elasticities of taxable income (cross-ETI) to the net of income tax rates to assess the effect on the income of a given percentile following a tax cut in the AMITR in a different income group. Although not significant, the magnitude of the 2SLS cross-ETIs for a tax cut in the AMITR of the top 1% on the income of the bottom 99% are 0.19 and 0.4 for 1 and 2 years, consistent with Mertens and Montiel Olea (2018)'s estimates of 0.23 and 0.44 based on an SVAR-IV.

My approach, however, highlights the importance of analyzing narrower income percentiles. I find that most of the increase in the bottom 99%'s income can be attributed to the raise in the income of the top decile. I estimate a cross-ETI of a tax cut in the top 1% of 0.42 for the top 5% (excluding the top 1%), and of 0.2 for the next 5 percentiles (between the top 10 and the top 5%). Meanwhile, the response of the income of the bottom 90% to a tax cut in the highest percentile is not significant at any horizon. I do not find evidence of positive spillovers from tax cuts on other income percentiles to lower income groups. In other words, I find that the "trickle-down" effects are only concentrated on the top incomes.

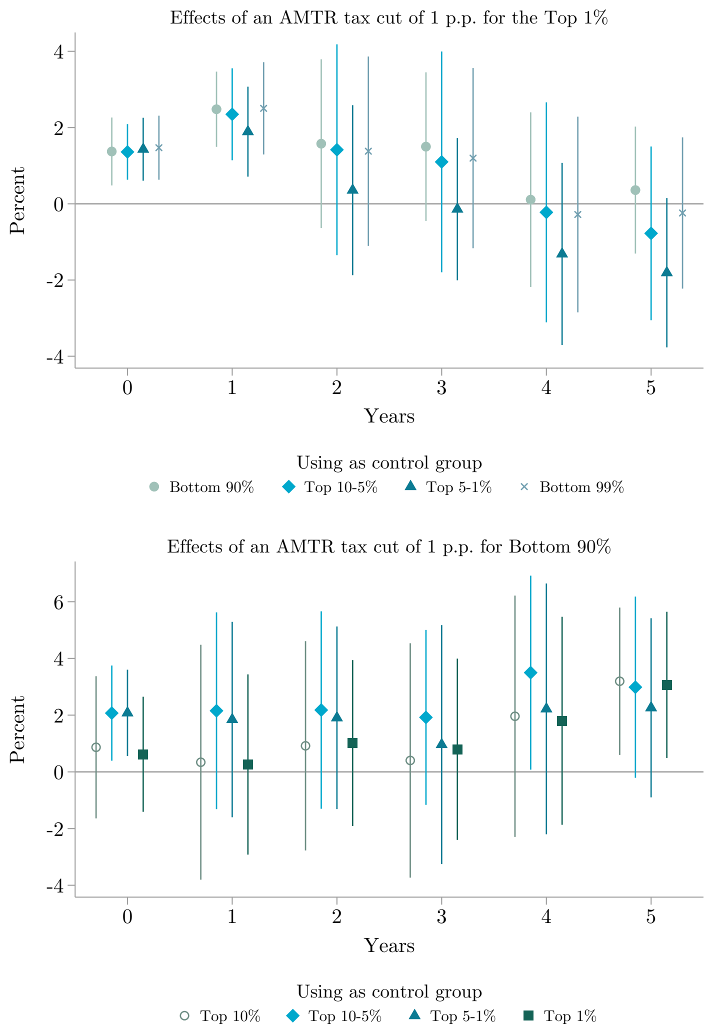
I provide further nuances to the previous literature's finding of a negative spillover effect on the income of the top 1 percent following a tax cut in the bottom of the 99%. My estimates suggest that this negative effect is most likely coming from changes in the AMTR of the bottom 90% and that if the tax cut is set on the AMTR of the top 5-1%, the income of the top 1% increases by more than 5% after four years. Moreover, I find positive spillovers at longer horizons from tax cuts in the bottom 90% on the incomes of the top decile excluding the top 1%.

One departure from Mertens and Montiel Olea (2018)'s analysis is the estimation method. Their use of an SVAR-IV allows feedback between the income groups' AMITRs, however, the restriction used for identification more closely resembles a policy reform that changes one group's statutory marginal tax rate at the time. My reduced-form specification controls for the simultaneous changes in AMITRs that tend to characterize the reforms. In turn, my estimates are less precise, but the 2SLS short-run cross-ETIs give some reassurance about their size.

The opposite dynamics that incomes follow depending on whether a tax cut is coming from the top percentile or outside, suggests the presence of different transmission mechanisms and highlights the intertemporal trade-off policymakers face. The taxable income measure used here, total market income excluding realized capital gains, is broad. While an advantage is to make comparable results with previous studies, a further analysis of narrower income measures may offer some insights. For example, if the ETIs presented here differ from ETIs based on wages, that could be indicative of some tax avoidance effects. Alternatively, similar results could point towards a primary role of labor supply channels. A structural model could help to clarify the importance of different channels at play: labor and investment incentives, tax avoidance and job creation.

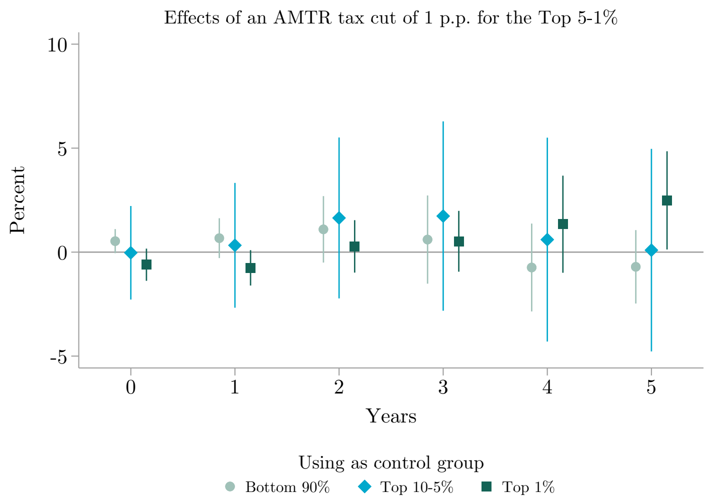
Appendix

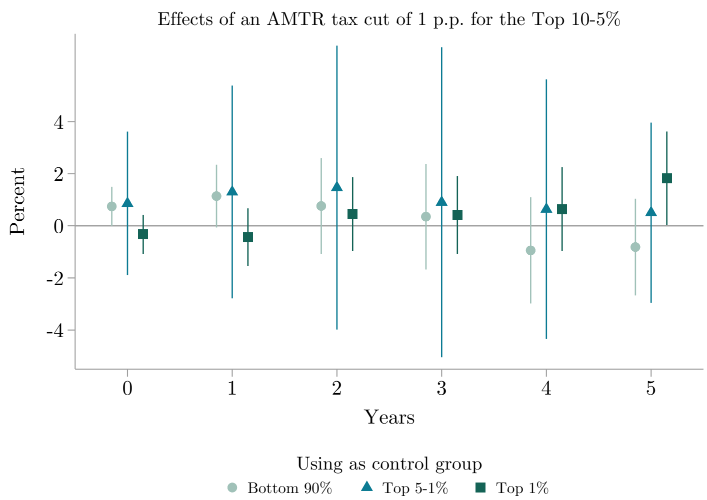
Figure 2.5.1: Semi Elasticity of Taxable Income controlling for different income groups



Reduced form estimates of a_h^j from $\ln(\text{income}_{t+h}^j) - \ln(\text{income}_{t+h-1}^j) = a_h^j z_t^j + b_h^j z_t^k + c_h X_t^j + e_{t+h}^j$ for horizon $h = 0, 1, ..., 5$. All estimates include 95% confidence bands from Newey-adjusted standard errors.

Figure 2.5.2: Semi Elasticity of Taxable Income controlling for different income groups





Reduced form estimates of a_h^j from $\ln(\text{income}_{t+h}^j) - \ln(\text{income}_{t+h-1}^j) = a_h^j z_t^j + b_h^j z_t^k + c_h X_t^j + e_{t+h}^j$ for horizon $h = 0, 1, ..., 5$. All estimates include 95% confidence bands from Newey-adjusted standard errors.

Table 2.3: Taxable income averages by percentiles

Year	Bottom 90%	Bottom 99%	Top 10-5%	Top 5-1%	Top 1%
1960	23,757	28,961	69,745	95,074	261,457
1961	23,951	29,306	69,569	99,465	263,913
1962	24,775	30,400	72,842	103,915	271,450
1963	25,407	31,200	74,761	107,043	274,570
1964	26,777	32,753	77,671	111,067	282,755
1965	27,923	34,077	79,402	115,918	295,971
1966	28,963	35,469	84,228	120,944	320,686
1967	29,636	36,307	86,142	124,113	330,717
1968	30,639	37,529	89,237	127,945	338,580
1969	31,160	38,215	91,648	130,208	329,770
1970	31,473	38,517	92,028	130,108	322,745
1971	31,277	38,415	92,865	131,054	321,133
1972	32,636	40,026	96,683	135,471	333,077
1973	33,202	40,862	98,945	140,625	339,465
1974	31,967	39,475	96,323	137,310	345,529
1975	30,165	37,438	93,442	131,141	322,566
1976	30,982	38,388	95,490	133,647	325,507
1977	31,284	38,765	96,684	134,737	329,150
1978	31,865	39,469	98,295	136,996	337,575
1979	31,875	39,394	97,666	135,674	340,622
1980	30,783	38,277	96,538	134,052	337,452
1981	30,585	38,008	96,133	132,385	328,375
1982	29,749	37,101	94,800	130,362	336,384
1983	29,248	36,654	94,519	130,937	341,117
1984	30,114	37,763	97,253	135,517	364,638
1985	30,508	38,347	99,176	138,671	379,789
1986	30,766	38,843	101,397	142,395	386,344
1987	30,708	39,224	104,367	149,488	467,562
1988	31,139	40,052	106,659	157,356	601,220
1989	31,124	40,186	107,504	159,928	574,135
1990	30,719	39,734	106,528	159,033	586,829
1991	30,064	38,956	104,958	156,568	534,283
1992	29,608	38,697	105,715	159,412	596,866
1993	29,336	38,418	105,313	159,131	559,342
1994	29,794	39,081	107,381	162,605	570,567
1995	30,172	39,889	110,345	170,494	617,812
1996	30,550	40,540	112,101	175,851	659,201
1997	31,452	41,821	115,354	183,154	717,554
1998	32,761	43,588	119,874	191,843	779,087
1999	33,725	44,992	123,888	199,797	840,376
2000	34,001	45,374	124,762	201,943	887,220
2001	33,717	44,903	124,281	197,346	807,402
2002	32,358	43,384	121,279	194,179	757,313
2003	31,693	42,679	120,662	192,362	758,136
2004	32,051	43,251	122,170	196,676	836,146
2005	31,995	43,487	123,592	201,899	924,692
2006	32,254	44,085	126,863	206,796	961,888
2007	33,065	45,185	129,505	212,478	1,003,825
2008	31,170	43,055	126,520	206,156	928,807
2009	29,620	41,143	123,779	197,120	815,367
2010	29,292	40,974	124,027	200,007	857,523
2011	28,928	40,667	123,450	201,353	852,094
2012	29,162	41,167	124,707	206,902	948,285

Gross market data includes labor earnings, entrepreneurial income, dividends, interest, and rents, before government transfer payments and before any adjustments or deductions. Averages per tax unit in 2010 constant dollars. Source: Mertens and Montiel Olea (2018).

Table 2.4: Own ETI

	Top 1%				Top 5-1%			Top 10-5%			Bottom 90%			Bottom 99%
Spillover from:	Bottom 99	Top 5-1	Top 10-5	Bottom 90	Top 1	Top 10-5	Bottom 90	Top 1	Top 5-1	Bottom 90	Top 1	Top 5-1	Top 10-5	Top 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
I. OLS														
Same year	1.103***	0.778**	0.672**	1.096***	-0.656***	-0.446	0.052	-0.410	-0.091	0.300	-0.742**	-0.516	-0.939*	-0.761**
	(0.25)	(0.33)	(0.28)	(0.22)	(0.21)	(0.54)	(0.21)	(0.25)	(0.34)	(0.25)	(0.36)	(0.41)	(0.49)	(0.35)
Following	0.904**	1.122***	0.712**	0.902**	-0.765***	-0.122	0.104	-0.546	0.118	0.253	-0.926	-0.127	-0.283	-1.095*
year	(0.40)	(0.34)	(0.31)	(0.36)	(0.23)	(0.53)	(0.25)	(0.35)	(0.58)	(0.37)	(0.71)	(0.56)	(0.60)	(0.63)
II. IV-2SLS														
Same year	1.460**	1.179**	1.196**	1.698**	-0.587	-0.443	-1.692	-0.232	1.214	-1.801	0.539	-16.182	-21.458	-0.062
	(0.54)	(0.47)	(0.52)	(0.68)	(0.46)	(2.04)	(12.64)	(0.47)	(2.62)	(7.07)	(1.21)	(91.99)	(133.02)	(0.75)
$F_{\text{1st-stage}}$	13.7	13.9	13.4	13.6	3.6	3.4	3.5	6.8	6.8	6.7	11.8	15.9	13.3	12.2
Following	2.501***	1.831***	2.116***	2.989**	-0.734	-0.262	-0.138	-0.306	1.669	-3.796	0.111	-11.761	-21.475	-0.347
year	(0.84)	(0.62)	(0.76)	(1.18)	(0.67)	(2.66)	(12.68)	(0.65)	(3.05)	(11.98)	(1.72)	(65.43)	(139.70)	(1.12)
$F_{\text{1st-stage}}$	13.4	13.6	13.1	13.3	3.7	3.5	3.5	6.6	6.6	6.5	11.3	15.5	13.1	11.4

Each cell is the α^j OLS (Panel I) or 2SLS ETI estimates (Panel II) from equation $\ln(\text{income}^j_{t+s}) - \ln(\text{income}^j_{t-1}) = \alpha^j \Delta ln(1-\text{AMITR}^j_t) + \beta^j \ \Delta ln(1-\text{AMITR}^k_t) + \gamma^j X^j_t + \varepsilon^j_t$. Panel II uses the exogenous statutory changes from Mertens and Montiel Olea (2018) as instrument for the changes in the AMITR. Robust standard errors in parenthesis and first stage F-statistic in brackets. Controls include two lags of the following variables: (log) income of main percentile, AMITR, real GDP per tax unit, unemployment rate, (log) government spending per tax unit, debt growth rate, inflation, (log) real estate prices and federal fund rates, as well as an indicator variable for the years 1949 and 2008.

Chapter 3

Housing vouchers and local economic activity

3.1 Introduction

US federal transfer payments to individuals are large and vary geographically. In pre-pandemic years they accounted for about 10.8% of GDP and raised to 15.8% in 2021.[1] Transfer payments have also become increasingly larger than government consumption. Despite these recent trends, the work estimating the effect of transfers on economic activity is limited since large swings are typically consequences of current economic conditions. This paper examines the case of housing vouchers, a transfer that since 1980 has quadrupled the federal outlays of cash assistance programs. I study the effect of a plausible exogenous one-time, permanent change in generosity across geographic areas on local economic activity.

[1]These figures come from the ratio of current transfer payments to persons (line 28 of NIPA Table 3.2) over GDP (line 1 of NIPA Table 1.1.5). The pre-pandemic years figures is the 2018 and 2019 average. The corresponding figures including transfer payments as grants-in-aid to state and local governments (line 31 of Table 3.2) and federal housing subsidies (line 4 of Table 3.13) are 13.8% and 20.7% respectively.

The maximum amount a household can receive in housing vouchers is based on the local median rent as estimated by the latest decennial Census. Following Collinson and Ganong (2018), I rely on the incorporation of the 2000 Census in the computation of the 2005 maximum housing voucher at the metro area level, which led to some metro areas having a larger than usual increase in the maximum benefit and to other areas a decrease in the maximum transfer. I show how the resulting thresholds were unanticipated given their previous year's level and unrelated to past local economic trends. I then use this variation to instrument the change in housing transfers.

In general, the effects of transfers on economic activity are not obvious. Being designed within a social insurance framework, transfer payments are negatively dependent on individuals' income level, which disincentivizes work, leading to lower levels of employment and output. Further negative effects on the economy can arise if transfer payments disincentivize precautionary savings (Aiyagari and McGrattan, 1998). However, consumption and investment can raise if transfers relax liquidity constraints (Woodford, 1990). Moreover, if transfers are well targeted and are limited to lower-income groups characterized by high MPC, higher transfers can increase the consumption of these groups and raise aggregate demand. If the marginal worker pays more in taxes than what he receives in transfers, an increase in transfer would result in a negative wealth effect, incentivizing the marginal worker to work more, leading to higher levels of employment and output (Oh and Reis, 2012).

The US housing voucher program is an interesting case study for studying the effects of transfers in the economy for a couple of reasons. First, the beneficiary households' heads are working-age (as opposed to beneficiaries of Social Security checks) allowing us to capture labor supply responses. Second, housing vouchers are a good example of means-tested transfers since beneficiaries have to be approved by local agencies that ensure that only low-income individuals enter the program, thus ensuring targeting. Moreover, the fact that eligible households can spend several years on a waiting list before becoming beneficiaries implies that an increase in generosity level in a given year is unlikely to change the pool of beneficiaries. Finally, the policy change I examine has two features worth noting. First, it can be thought of as an example of a policy where the generosity is increased for an already existent transfer; in other words, it is an example of a change at the intensive margin. Second, the inclusion of the 2000 Census data led to changes in the generosity level in different directions, so I am able to test for asymmetry in transfer changes.

Several papers have studied how transfer payments affect the economy. Romer and Romer (2016) document the increases in Social Security benefits in the US during the twentieth century that were not motivated by contemporaneous economic conditions and find that an increase in SS benefits of 1 percent of personal income increases aggregate consumption by 1.2 percent in the next 5 months, but do not find significant effects on other indicators of economic activity. Moreover, these results are driven by permanent changes in SS benefits, casting doubt on the efficacy of temporary transfers to serve as countercyclical mechanisms. Oh and Reis (2012), however, estimate that the 3.4% increase in social transfers in the US during the Great Recession had positive, albeit limited, effects on output and employment. Their model, featuring high MPC for beneficiaries, suggests the increase in transfers increased GDP by no more than 0.06%.

The policy change I examine is a one-time permanent change in 2005. Unlike the SS payments, the housing vouchers are not necessarily directed to the elderly,[2] however, the number of beneficiaries is considerably lower compared to the SS recipients.

[2]Beneficiaries with head of households older than 65 constitute less than 2% of total beneficiaries.

More generally, this paper is related to the work examining the capability of fiscal policies in a federal union to smooth regional business cycles. Most of the empirical work has aimed to estimate cross-region multipliers, i.e the additional income generated in an area when an additional dollar is received or spent.[3] Pennings (2019) estimates a cross-region transfer for the U.S. combining on the one hand the federal SS increases of the twentieth century and the 2001 and 2008 stimulus payments, and on the other hand the distribution of recipients across the US according to prior tax returns to argue that the federal policies were not designed to favor specific states. Pennings (2019) finds that states receiving larger transfers have faster short-run economic growth; states that received an extra dollar increase their GDP by 0.3 dollars in the case of temporary transfers and 1.5 dollars in the case of permanent transfers. This paper also exploits the geographical variation to estimate the effect of transfers to individuals in the local economy, in this case at the metropolitan area (MSA) level. Although a federal transfer, the dollar amount changed differed across MSAs. Due to data limitations, I cannot use the same approach used by Pennings (2019) to determine which areas were more likely to receive larger transfers based on the number of eligible residents. However, I show that areas that received higher transfers were not systematically related to prior economic trends.

The effects of larger housing vouchers are not straightforward. On the one hand, they can be pass-through onto higher rents, offsetting any benefit to the voucher holders and possibly becoming a transfer to landlords. If landlords do not change their rents, higher vouchers would increase beneficiaries' available income which may in turn have effects in the aggregate. I find that an increase in the average transfer improves the conditions of beneficiaries by making more resources available to them, not only through decreasing the total payments households allocate to rent but also by increasing the gross household income, suggesting

[3] For a recent review see Chodorow-Reich (2019) and Guren et al. (2020).

the presence of behavioral effects. I find that a 1% increase in the average housing transfer decreases the proportion of income spent on rent by half a percentage point and increases the household income of voucher holders by 1 percent. However, I find that the effects on the MSA-level GDP and personal income per capita are less clear-cut, suggesting that the general effects of a small increase in the average housing transfers are close to zero.

I provide more context on the housing vouchers program in the next section. In particular, I emphasize how the maximum level of generosity is determined, since understanding how the formula works is fundamental to clarify where the source of variation is generated and the possible challenges it conveys. I then describe the data I use and the limitations I face. In the fourth and fifth sections I detail the empirical strategy I follow and discuss the results. I conclude with some remarks on how to continue this research project in the last section.

3.2 Housing vouchers: background

At the program's inception in the late 1970s, vouchers were allocated across areas according to demographic variables, so that MSAs that had a higher proportion of households living under the federal poverty rate relative to other MSAs received a higher number of vouchers.

Local public housing authorities request funding to the US Department of Housing and Urban Development (HUD) and Congress then approves funding through discretionary appropriation acts.[4] This funding comprises subsidies for existing and new vouchers and administrative expenses. Once an eligible household has been granted the transfer, they are responsible to pay 30% of their income towards rent. The federal government covers the difference between this amount and the total rent up to a maximum estimated to represent the 40th percentile of the median rent. This monthly voucher ceiling, called the fair market rent (FMR), is set by HUD at the MSA level. It is common that the rents of the beneficiaries exceed the FMR, so the monthly average housing transfer is highly correlated with the MSA FMR (figure 3.2.1).

The FMR for the fiscal year t is published in year $t-1$ and can be broken into three components: a base rent representing the 40th percentile gross rent, an inflation-based update factor, and a trend factor. The base rent comes from the latest Census survey[5] that is updated by local housing inflation factors through $t-1$, and a trend factor to account for the fact that the FMR for fiscal year t is calculated using data at $t-1$.[6]

[4]Appropriations not mandated by existing law and therefore made available annually in appropriation bills in amounts chosen by Congress.

[5]The decennial Census until the year 2005, the annual American Community Survey (ACS) from 2006 to 2009 for areas with more than 65,000 people, and the 5-year ACS for all areas from 2009 onwards.

[6]The inflation factors are based on MSA level CPI available for the largest 25 metro areas, and for the rest, the HUD carries out regional surveys. Before 2011, the expected growth rate in rents was forecasted using the annualized national growth rate of rents between the last two decennial Census; afterward the HUD used the annual growth rate of the past 5 years.

Figure 3.2.1: FMR and Housing Transfer Payments

Variables demeaned by MSA FE.
Plot over 100 quantiles of FMR.
287 MSAs, 2004-2018, current dollars.

3.3 Data

I rely on two main sources of data. The first one is from the federal agency that administers the housing voucher program, the US Department of Housing and Urban Development (HUD) and contains the monthly MSA-level maximum benefit, the Fair Market Rent (FMR), as well as several household beneficiaries averages at the MSA level including the average transfer (the main endogenous variable), gross household income (all income and regular payments received from any family member, including welfare payments, unemployment insurance, and Social Security payments, before deductions), and household payments towards rent (utilities and rent paid in addition to the housing voucher). I use the FMR adjusted for 2 rooms, available starting in 1983. The rest of the transfer variables are only publicly available for the years 2000 and 2004 onwards.

The measures of economic activity at the MSA level come from the US Bureau of Economic Activity (BEA). Personal income is available annually from 2000, and real GDP from 2001. Both measures are converted in per capita terms using the population estimates from the Census Bureau. I limit the sample to the mainland U.S. From those 380 MSAs, only 287 MSAs are matched with information from the HUD to form a panel starting in 2004.

I complement these datasets with labor data from the Current Population Survey, Consumer Price Indexes from the Bureau of Labor Statistics and an MSA-level housing price index from Freddie-Mac. I compute MSA-level labor data aggregates using information from working-age individuals (25-54 years old). I use CPI series for all urban consumers for the 25 largest metro areas, and for four Census regions. I also rely on housing market variables characterizing MSAs in the early 2000s from Eriksen and Ross (2015) including vacancy rate, and Housing Supply Elasticities estimated by Saiz (2010). Unless indicated, all dollar variables are 2012 constant dollars deflated by national CPI-U.

3.4 Identification

The main concern in estimating the effect of transfer payments on the economic activity of metropolitan areas comes from possible reverse causality. In the case of housing vouchers, two possible motives behind the increases in transfers could bias the estimates in opposite directions. On the one hand, more generous transfers could be assigned to depressed areas to smooth regional shocks resulting in negative-biased estimates. On the other hand, higher transfers could be the response to rising living costs associated with increasingly booming areas, resulting in positive-biased estimates. This is particularly concerning in this context since housing transfers are aimed to help with rent expenditures.

The introduction of new decennial Census data to determine the maximum levels of housing transfers in 2005 generated unusually large swings in these thresholds. In this section, I first show that the 2005 changes in thresholds can be understood as the result of measurement errors between Census surveys. Nevertheless, these measurement error terms could still be the result of past economic trends and be subject to the biases described above. I provide empirical evidence to argue this is not the case and that therefore this policy change can be used to isolate the variation of changes in transfers in 2005 unrelated to concurrent and past economic conditions.

3.4.1 Housing voucher thresholds

To see how the variation from updates on the thresholds from the 2000 Census can be used as an instrument it is helpful to express the maximum subsidy as computed by the HUD. Following the nomenclature and intuition in Collinson and Ganong (2018), the Fair Market Rent in logarithms can be expressed as:

$$\log \text{FMR}_{m,t} = \rho_{m,s} + \phi_{m,s} + \sum_{j=s+1}^{t} \sigma_{m,j} \tag{3.1}$$

where the true 40th percentile rent, $\rho_{m,s}$, is observed up to a measurement error, $\phi_{m,s}$, with information from the Census year s. To account for annual growth in rents the base rent is updated each year by a housing-specific CPI $\sigma_{m,j}$, from $j = s + 1$ to $j = t$.

For years where the median rent is estimated from the same Census survey, the change in FMR is equal to the local housing inflation, $\sigma_{m,t}$. This is easily seen expressing (3.1) as:

$$\log \text{FMR}_{m,t} \quad = \quad \log \text{FMR}_{m,t-1} + \sigma_{m,t} \qquad \text{for } t \text{ using Census } s \text{ data.} \tag{3.2}$$

In contrast, when new Census data is available, there is an additional change whenever the inflation updates from the last Census rent estimate are off from the new rent estimate. To illustrate this, let's look at the case of the FMRs in 2004 and 2005. From 1996 to 2004, the base rent was estimated from the 1990 Census, while the 2000 Census was firstly used to compute the FMR of 2005. Thus, according to (3.1) the 2004 and 2005 FMR were given by

$$\log \text{FMR}_{m,2004} \quad = \quad \rho_{m,1990} + \phi_{m,1990} + \sum_{j=1991}^{2004} \sigma_{m,j} \tag{3.3}$$

and

$$\log \text{FMR}_{m,2005} = \rho_{m,2000} + \phi_{m,2000} + \sum_{j=2001}^{2005} \sigma_{m,j} \tag{3.4}$$

respectively. The FMR annual change between 2004 and 2005 then can be expressed as:

$$\log \text{FMR}_{m,2005} - \log \text{FMR}_{m,2004} = \{\rho_{m,2000} + \phi_{m,2000}\} - \{\rho_{m,1990} + \phi_{m,1990} + \sum_{j=1991}^{2000} \sigma_{m,j}\} + \sigma_{m,2005},$$

or, equivalently:

$$\log \text{FMR}_{m,2005} - \log \text{FMR}_{m,2004} = \{\rho_{m,2000} - [\rho_{m,1990} + \sum_{j=1991}^{2000} \sigma_{m,j}]\} + \{\phi_{m,2000} - \phi_{m,1990}\} + \sigma_{m,2005}$$

This is, the change in FMR in 2005 is given by the difference in the *true* 2000 rent and an estimate based on the 1990 Census, the difference in the measurement errors between the decennial Census and the CPI-based adjusted factor.

Identifying assumption. Assuming that the inflation factors correctly capture the growth in rents:

$$\rho_{m,2000} \quad = \quad \rho_{m,1990} + \sum_{j=1991}^{2000} \sigma_{m,j}, \tag{3.5}$$

we have that

$$\log \text{FMR}_{m,2005} - \log \text{FMR}_{m,2004} = \{\phi_{m,2000} - \phi_{m,1990}\} + \sigma_{m,2005} \tag{3.6}$$

Under (3.5), the 2005 FMR change is given by the usual inflation factor and a measurement error component. In principle, the measurement error component constitutes a potential candidate as an exogenous change in the transfer generosity given that (i) is an unexpected change, and (ii) is an object with information from 1990 and 2000.

Figure 3.4.1 shows the median annual FMR percentage change from 1997 to 2005. The annual dispersion across MSAs is small between 1997 and 2004 compared with the large dispersion seen in 2005. It is also notable that for most MSAs the FMRs tended to increase from year to year up to 2004, while many of the MSAs saw for the first time a reduction in the housing subsidy generosity. This is consistent with changes in FMR given by an inflation update as in (3.2) through 2004, and the unusual increase in FMR in 2005 due to new Census data used.

3.4.2 Shock construction

To isolate the measurement error component in (3.6), $\phi_{m,2000} - \phi_{m,1990}$, from the 2005 update factor I first define a 2005 FMR counterfactual using equation (3.2), the resulting 2005 FMR had it been computed with the 1990 Census:[7]

$$\log \widehat{\text{FMR}}_{m,2005} = \log \text{FMR}_{m,2004} + \sigma_{m,2005} \tag{3.7}$$

[7] An alternative way to isolate the measurement error in (3.6) would be to subtract the inflation fractor from the observed 2005 FMR change, however, I do not have access to the local inflation factors used for annual updates.

Subtracting the counterfactual from the observed 2005 FMR and using (3.6) it is easy to see that the measurement error can be interpreted as a prediction error of the 2005 FMR:[8]

$$\log \text{FMR}_{m,2005} - \log \widehat{\text{FMR}}_{m,2005} = \{\phi_{m,2000} - \phi_{m,1990}\} \tag{3.8}$$

In practice, I estimate the counterfactual $\log \widehat{\text{FMR}}_{m,2005}$ through an out-of-sample prediction for year 2005 from MSA level regressions:

$$\log \text{FMR}_{m,t} = a_0^m + a_1^m \log \text{FMR}_{m,t-1} + a_2^m \log \text{CPI}_{m,t-1} + a_3^m \log \text{CPI}_{m,t-2} + u_{m,t} \tag{3.9}$$

for every MSA m and for years $t \in [1995, 2004]$ where the inflation factor σ_t, is being approximated by the previous year difference in the most local housing CPI available.[9] I approximate the measurement error component as the prediction error from (3.9) for 2005 and define it as a one-time MSA-specific housing transfer shock.

Figure 3.4.1 shows the resulting prediction errors for 1997 to 2005. It is remarkable the similarity between the prediction errors and the observed annual FMR changes. The prediction errors are concentrated around zero from 1997 to 2004, suggesting that equation (3.9) does a good job of approximating the observed FMR levels. The large dispersion seen in 2005 is consistent with the idea that the FMR in 2005 was not easily predicted as in previous years. Moreover, the similarity in dispersion from the prediction errors of 2005 and the actual per-

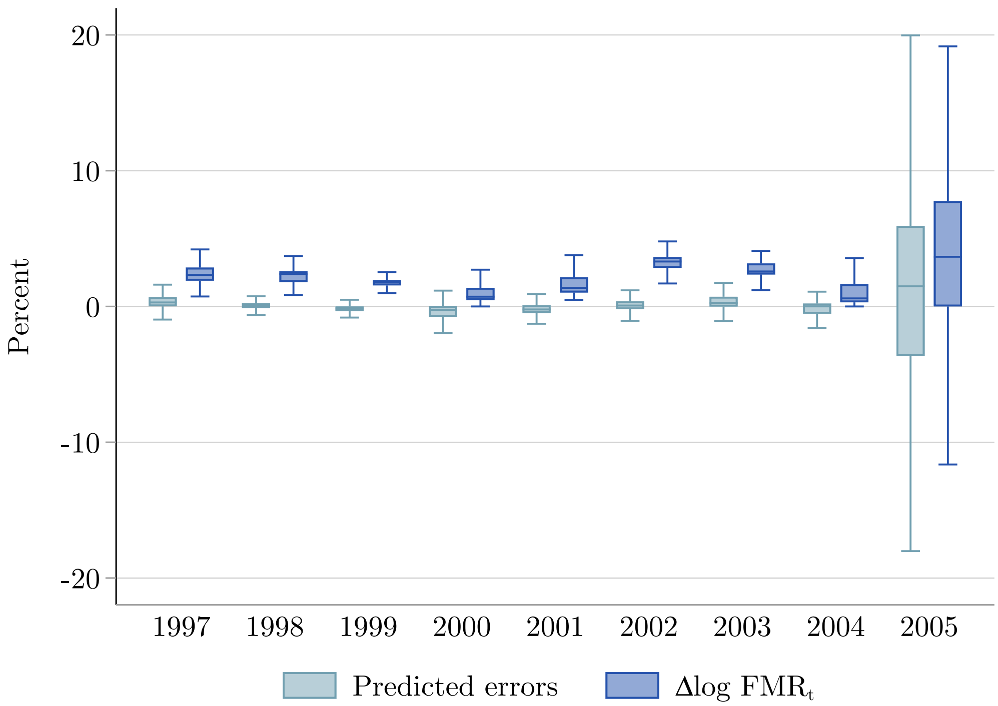
[8]As by construction $\log \text{FMR}_{m,2005} - \log \widehat{\text{FMR}}_{m,2005} = \log \text{FMR}_{m,2005} - \log \text{FMR}_{m,2004} - \sigma_{m,2005}$, using (3.6) in the RHS we have that $\log \text{FMR}_{m,2005} - \log \widehat{\text{FMR}}_{m,2005} = \{\phi_{m,2000} - \phi_{m,1990}\} + \sigma_{m,2005} - \sigma_{m,2005}$. Moreover, note that (3.8) can be equivalently expressed in annual changes by adding and subtracting the FMR level in 2004:

$$\Delta\log \text{FMR}_{m,2005} - \Delta\log \widehat{\text{FMR}}_{m,2005} = \{\phi_{m,2000} - \phi_{m,1990}\}$$

[9]To extract more information I include $\log \text{CPI}_{m,t-1}$ and $\log \text{CPI}_{m,t-2}$ rather than the difference $\Delta\log \text{CPI}_{m,t-1}$. I also refer the previous year difference rather than the current one to be consistent with the fact that the FMR_t for fiscal year t is published in $t-1$, and therefore is based on data up to $t-1$.

centage change in FMR in 2005 confirms the hypothesis that the change seen in 2005 was due to unpredicted factors, and according to the identifying assumption, to a measurement error component, rather than to potential unusual housing inflation factors in 2005.

Figure 3.4.1: Prediction errors and annual percentage change in Fair Market Rent box plots by year



Annual MSA averages Prediction errors from $\log \text{FMR}_{m,t} = a_0 + a_1 \log \text{FMR}_{m,t-1} + a_2 \log\text{CPI}_{m,t-1} + a_3 \log\text{CPI}_{m,t-2} + u_{m,t}$. Annual percentage change in FMR in current dollars. According to the FMR formula computation, the changes in FMR are equal to a housing-specific inflation factor $\Delta\log \text{FMR}_{m,t} = \sigma_{m,t}$ for $t = 1997, ..., 2004$, and $\Delta\log \text{FMR}_{m,2005} = \{\phi_{m,2000} - \phi_{m,1990}\} + \sigma_{m,2005}$ for 2005, where $\phi_{m,s}$ are Census-specific measurement errors.

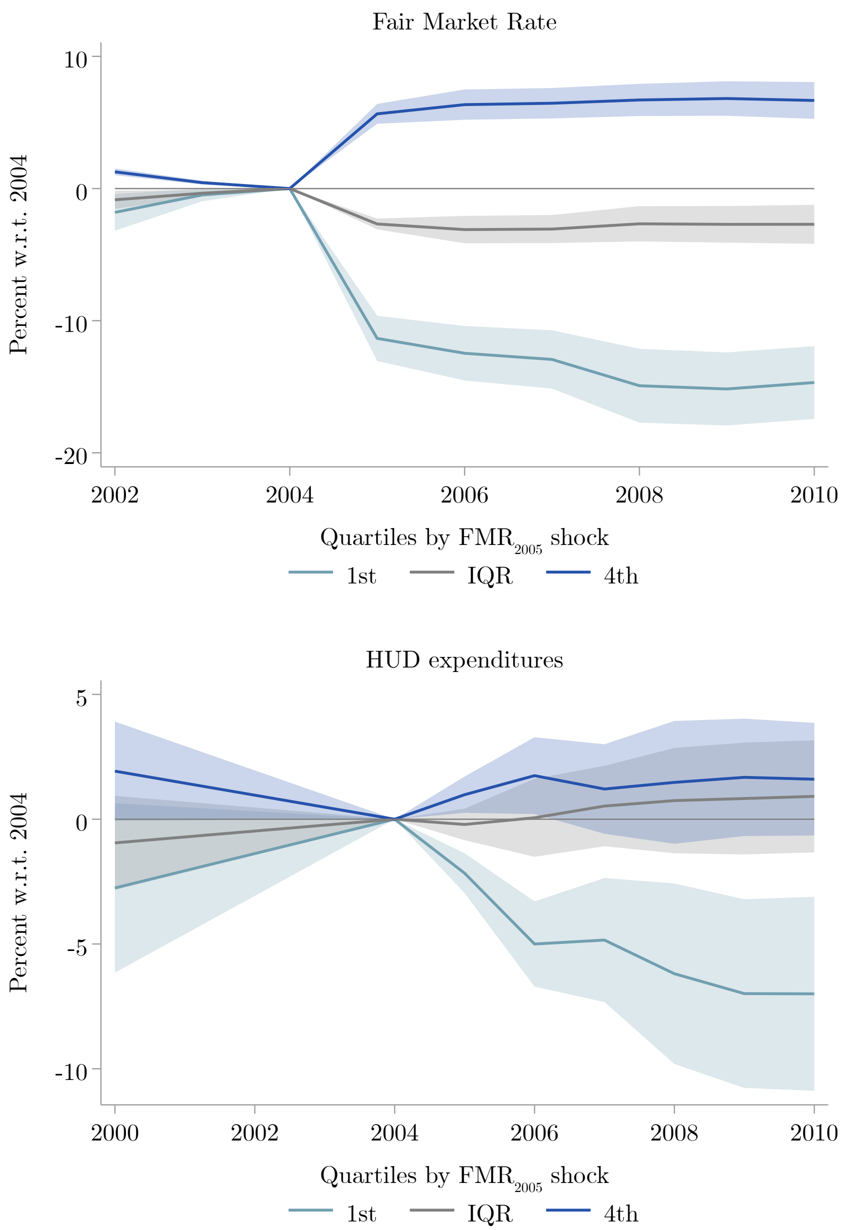
3.5 The effect of housing transfers in the local economy

Figure 3.5.1 shows the policy variation in FMR (upper panel) and in the average vouchers expenditures from HUD (bottom panel) by classifying the metro areas according to the 2005 shock: MSAs in the bottom 25% of revisions (1st quartile), MSAs in the top 25% of revisions (4th quartile) , and MSAs in the second and third quartiles of revisions (IQR). There is some evidence that areas that received an unusual increase in FMR in 2005 had been drifting downwards and vice-versa as seen by the first and fourth quartiles pre-trends. However, it is reassuring that after 2005 the FMRs remained stable with respect to their 2004 values, indicating the persistence of the shock. Moreover, these patterns are also reflected in the average HUD transfers. The validity of using the measurement error term in (3.8) as an instrument for transfer generosity relies on the identifying assumption in (3.5) and on the error term not being systematically related to shocks that affect the outcomes in 2005. I cannot test directly for (3.5) since the MSA base rents from the decennial Census are not publicly available. However, analyzing whether the shock is related to past economic variables offers an indirect test. For instance, one possibility of upward revisions in increasingly dynamic areas could happen if local housing CPI factors were failing to capture the growth in rents (i.e. $\rho_{m,2000} > \rho_{m,1990} + \sigma_{m,1991} + \cdots + \sigma_{m,2000}$), challenging the identifying assumption.[10] Assuming the identifying assumption holds, the measurement error term could still be related to the same economic factors driving local output, for example, if the Census measurement error in small areas was larger in 2000 than in 1990, resulting in upward revisions in less populated areas.

Panel A from Table 3.1 summarizes the results from regressing the 2005 shock on economic variables measured between 2000 and 2004, depending on data availability: columns 1-3 for the 287 MSAs where HUD transfers data is available, columns 4-5 for the 232 MSAs with labor data, column 6 for the 110 MSAs with vacancy rate data, and column 7 for the 76 MSAs with housing supply elasticity. Panel B provides an overview of how averages change across these samples. The first two samples are not statistically different, however, MSAs where more housing market characteristics are available, are on average richer, larger, and with higher housing voucher transfers.

[10]Vice versa, if depressed economic areas had been subject to downward revisions due to local CPI factors overestimating the true rent in 2000.

Figure 3.5.1: Policy Variation by 2005 shocks quartiles



Average monthly fair market rent and average HUD voucher expenditures per household percentage changes with respect to 2004. FMR in current dollars and HUD expenditures in 2012 constant dollars. Quartiles according to the 2005 shock defined as the 2005 prediction error from $\log \text{FMR}_{m,t} = a_0 + a_1 \log \text{FMR}_{m,t-1} + a_2 \log\text{CPI}_{m,t-1} + a_3 \log\text{CPI}_{m,t-2} + u_{m,t}$. IQR stands for interquartile range, the second and third quartiles. 95% confidence bands obtained from regressions of demeaned FMR with respect to 2004 on indicators for quartiles 1, IQR, and 4 by year. Standard errors clustered by MSA.

Table 3.1: Exogeneity tests for 2005 shock

Panel A. Dependent variable: FMR 2005 predicted error

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average growth rates (2000-2004)							
Fair market rent	-2.62***	-2.70***	-2.48***	-2.53***	-2.75***	-2.44***	-2.64***
	(0.33)	(0.34)	(0.36)	(0.39)	(0.43)	(0.47)	(0.50)
Real GDP	-0.09	-0.08	-0.10	-0.11	-0.11	-0.33	0.02
	(0.17)	(0.17)	(0.17)	(0.18)	(0.18)	(0.35)	(0.35)
Population	-0.32	-0.29	-0.18	-0.01	0.02	0.75	0.83
	(0.64)	(0.65)	(0.68)	(0.69)	(0.67)	(0.94)	(1.03)
Housing price index	0.29	0.30	0.41	0.50*	0.52*	1.18**	1.32**
	(0.17)	(0.17)	(0.23)	(0.23)	(0.23)	(0.35)	(0.43)
Housing price index (1990-2000)		0.42	0.51	0.19	0.33	0.89	0.35
		(0.49)	(0.48)	(0.53)	(0.54)	(0.66)	(0.96)
Variables in levels							
(log) Monthly rent			-3.02	-2.81	-6.57	-15.68	-8.22
			(4.57)	(5.19)	(5.60)	(8.79)	(10.99)
(log) Household income			-2.16	-0.66	2.83	-0.33	-12.20
			(6.89)	(7.83)	(7.49)	(10.87)	(12.57)
Unemployment rate (2000-2004 average)				-0.06	-0.03	-1.16	-1.70*
				(0.20)	(0.21)	(0.60)	(0.66)
Labor force rate (2000-2004 average)				-0.13	-0.13	-0.02	-0.50
				(0.10)	(0.10)	(0.28)	(0.46)
(log) Number of vouchers (2004)					0.93	-0.86	-0.18
					(0.55)	(1.62)	(2.69)
Vacancy rate (2001-2003 average)						24.39	6.74
						(29.68)	(33.67)
(log) Occupied rental units (2000)						-3.46	-0.59
						(4.72)	(5.06)
(log) Poverty rate (1999)						5.38	2.04
						(4.26)	(5.11)
Housing supply elasticity (Saiz)							1.49
							(1.18)
HUD Regions FE	x	x	x	x	x	x	x
N	287	287	287	232	232	110	76
Adj. R-squared	0.361	0.361	0.360	0.378	0.385	0.560	0.599

Panel B. Averages by samples (2004)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FMR shock 2005	0.2			-0.2		-1.1	-2.4
	(0.5)			(0.5)		(0.8)	(1.0)
FMR	767			796		854	888
	(14)			(16)		(26)	(33)
HUD voucher expenditures	567			591		639	669
	(10)			(12)		(18)	(23)
Rent paid by household	312			317		322	320
	(3)			(4)		(6)	(7)
Household income (thousands)	1.07			1.09		1.11	1.12
	(0.01)			(0.01)		(0.02)	(0.02)
Per capita GDP (thousands)	44.5			45.5		48.3	50.0
	(0.6)			(0.7)		(0.9)	(1.1)
Population (thousands)	807.7			965.3		1,634.6	2,161.0
	(100.1)			(121.6)		(236.3)	(324.1)
FMR average growth 00-04	2.7			2.9		3.4	3.9
	(0.1)			(0.1)		(0.2)	(0.3)
Housing price growth 00-04	6.2			6.6		7.0	6.8
	(0.2)			(0.3)		(0.4)	(0.5)

Panel A: each column refers to a regression of the predicted error on past economic indicators. Average growth rates between 2000-2004 except for GDP (2002-2004) and population (2001-2004). Monthly rent and household income pertains to beneficiaries and are in 2012 constant dollars. Monthly rent is a proxy computed by adding the average of household monthly payments towards rent and the average of HUD monthly voucher expenditures. Robust standard errors in parentheses (*: p<.05, **: p<.01, ***: p<.001). Panel B: averages for column samples and standard errors in parentheses. FMR, HUD voucher expenditures, rent paid and household income in 2012 constant dollars. Per capita real GDP in 2012 chained dollars.

Two points are worth noting from the regressions in the Table 3.1. First, the mean reversion observed in Figure 3.5.1 is confirmed, MSAs with generosity thresholds that were trending upwards were revised downwards in 2005 and vice-versa. On average, an increase of 1 percentage point in the average growth of FMR between 2000 to 2004 is associated with an unexpected decrease in the maximum subsidy of 2.6%. However, there is no evidence that higher subsidies were systematically assigned to thriving areas with higher GDP or population growth or with higher labor force participation rates, as coefficients for these variables are negative and not significant.[11] There is some indication that areas with rising housing prices between 1990-2000 received upward revisions, but the coefficients are not significantly different from zero. In contrast, there is some indication that areas characterized by faster housing price growth between 2000 and 2004 received more generous transfers when the sample is more limited (and as seen in Panel B, these areas tend to be richer and more populated but the difference is not significant). Moreover, it does not appear that beneficiaries facing higher rents in 2004 would have led to more generous transfers.[12] Finally, there is no strong indication that higher transfers were allocated to depressed areas. On the one hand, MSAs with a larger presence of beneficiaries experienced a decrease in the transfer thresholds, while MSAs with a higher poverty rate in 1999 are associated with increases in thresholds. In both cases, the coefficients are imprecisely estimated and not significant.

Taken together, the results of Table 3.1 do not offer evidence of a systemic association between the unexpected change in the maximum transfers in 2005 and the areas' previous economic activity, labor dynamics, and demographics. Table 3.2 summarizes the main coef-

[11]There is some evidence that areas with lower unemployment rates received an upward revision (column 7); however this result holds when the sample is limited to areas with housing supply elasticity. As seen in Panel B, MSAs with more data availability tend to be larger and and more dynamic, with higher FMRs, GDP per capita, and population.

[12]In contrast, coefficients associated to other housing market characteristics would suggest that more dynamic areas (with lower vacancy rate, higher number of occupied rental units, and lower housing supply elasticity) received downward revisions, but the coefficients are not significant.

ficients from regressing the shock on the average HUD transfer change with respect to 2004. On average, a 1 percentage point increase in the 2005 shock increases the average transfer by 0.12% the same year and 0.11% after two years. Although the significance in only the first two years, it is encouraging that the F-statistics are above conventional levels. A possible explanation on the small effects from the shock on the average transfers is that the effects are not linear. I examine this possibility in the next section.

Table 3.2: First stage regressions by year

Dep. variable: Annual Average Voucher Transfer, change relative to 2004

	(1)	(2)	(3)	(4)	(5)	(6)
	2005	2006	2007	2008	2009	2010
2005 Shock	0.121***	0.102*	0.091	0.014	0.042	-0.039
	(0.028)	(0.052)	(0.055)	(0.078)	(0.061)	(0.041)
N	287	287	287	287	287	287
F-stat	9.53	29.07	21.41	40.16	117.43	608.87
R-squared	0.104	0.323	0.441	0.469	0.778	0.929

* p<0.10, ** p<0.05, *** p<0.01

Regressions include a lag dependent variable and 2000-2004 FMR, GDP, population and housing price index averages growth rates. Standard errors clustered by MSAs in parentheses.

3.5.1 Event studies

I start addressing how receiving a larger or smaller transfer affected areas differently through a quartiles specification and summarize the propagation of the one-time shock from the following set of regressions:

$$y_{m,t} = b_o + b_1\mathbb{I}\{q_m = 1\} + b_2\mathbb{I}\{q_m = 4\} + \Lambda X_{m,2004} + e_{m,t} \tag{3.1}$$

for each $t = [2000, 2010]$, where b_1 and b_2 reflect the average effects of having received a larger than usual and lower than usual FMR associated with the bottom 25% and top 25%

revisions (or first and fourth quartiles).[13] In this way, equation (3.1) mimics an event-study specification of the effect on the MSAs that had the lowest 25% revision (on average, a downward revision of 4.6%) and the highest 25% revision (on average, an upward revision of 12%). b_0 would be capturing all constant shocks in the macroeconomy at time t along with the average effect of MSAs whose FMR change lies in the middle quartiles. Following the intuition from figure 3.5.1, I examine the effects of the outcome variables with respect to 2004 and include the FMR, GDP per capita, and house prices averages growth rates from 2000-2004 in $X_{m,2004}$ to control for pre-trends.

The averages of the main variables of interest by the shock quartiles are shown in Table 3.3. The first quartile (the bottom 25% changes in unexpected FMR) corresponds to an unpredicted decrease in the maximum generosity level of 10%, the fourth quartile (the top 25%) to an increase of 9.8%, and the middle quartiles to an average increase of 0.6%. Not surprisingly, MSAs with downward revisions had a maximum transfer of $157 larger than MSAs in the middle quartiles, and MSAs with upward revisions had a maximum transfer of $86 lower than MSAs in the middle quartiles. Besides these two natural differences, the quartiles do not differ significantly across other variables of interest (the average housing transfer to households, the household rent payments, income, and population).

In figure 3.5.2, I examine the effects on the MSA average transfer to beneficiaries (left panel) and on the fraction of income that households spend on rent (right panel) according to the specification in (3.1). How transfers changed in MSAs that received the largest unexpected increase or decrease would be akin to a discrete version of a first-stage regression; although the dynamics are similar to the raw data in figure 3.5.1, an increase in the maximum generosity transfer appears to have a small and not significant impact in the average transfer,

[13] The omitted category corresponds to the interquartile range, the 2nd and 3rd quartiles.

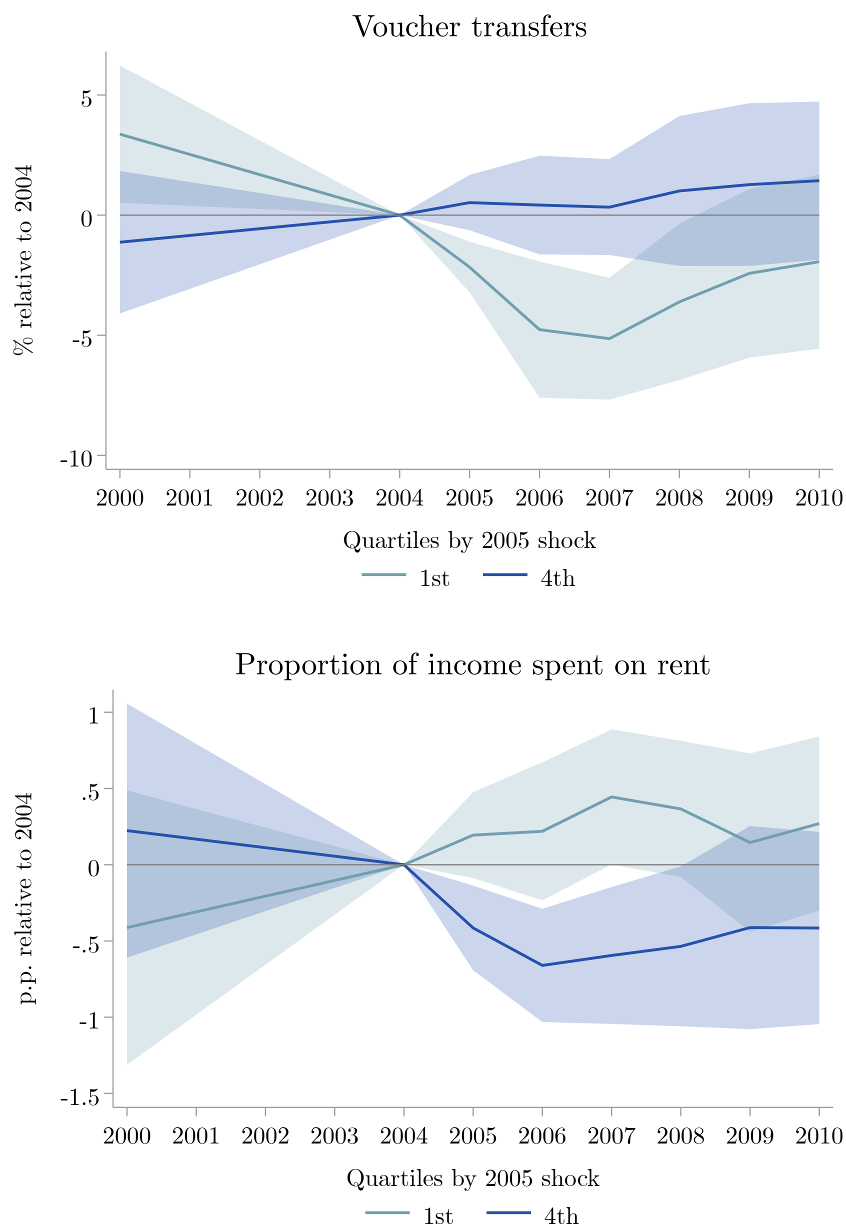
Table 3.3: Balance test - 2004 averages by quartiles.

Variable	(1) Bottom 25%	(2) IQR	(3) Top 25%	Difference (2)-(1)	(2)-(3)
Shock (2005)	-10.16	0.62	9.79	10.78***	-9.17***
	(0.71)	(0.22)	(0.49)		
% Change in FMR 2005	-4.66	3.23	12.06	7.89***	-8.83***
	[0.65]	[0.22]	[0.57]		
Fair Market Rent	770	613	527	-157*	86***
	(31)	(12)	(13)		
Voucher transfers	8,058	6,616	5,936	-1,442	680
	(326)	(132)	(152)		
Rent paid by households	3,946	3,725	3,558	-221	167
	(106)	(51)	(66)		
Household income	13,728	12,766	12,141	-961	626
	(308)	(156)	(200)		
Rent/Income	0.286	0.291	0.293	0.005	-0.001
	(0.002)	(0.001)	(0.002)		
Personal income per capita	40,786	37,767	35,883	-3,019	1,884
	(1,084)	(510)	(551)		
GDP per capita	47,077	43,773	43,224	-3,303	549
	(1,449)	(742)	(1,250)		
Population (thousands)	1,277	756	437	-521	319
	(213)	(161)	(82)		
MSA	72	144	71		

Column (1): MSAs in the bottom 25% of revisions (1st quartile). Column (3): MSAs in the top 25% of revisions (4th quartile). Column (2): MSAs in the interquartile range (IQR) or second and third quartiles of revisions. Standard errors clustered by MSAs in parentheses.

while MSAs with the 25% highest reductions in the thresholds experienced on average a 5% reduction in transfers. This was not necessarily reflected in the fraction of the income that households allocated to rent payments. Households in MSAs where the average transfer decreased (1st quartile) did not have a significant increase in the proportion of income allocated to rent. In contrast, households in the MSAs with the largest increase in thresholds decreased the fraction of their income spent on rent by half a percentage point well after 3 years of the shock.[14]

Figure 3.5.2: Quartiles event studies



95% Confidence Intervals. Standard errors clusted by MSAs.

[14]This result is not dependent on an increase in income, figure 7 in the Appendix shows the case of normalizing constant dollar rents on 2004 income.

3.5.2 Panel Data specification

To control for time-invariant metro area characteristics, I examine a fully interacted version of (3.1) through the following panel data specification:

$$y_{m,t} = a + by_{m,t-1} + \sum_{j=2001}^{2010} c_j \mathbb{I}\{q_m = 1\} * \mathbb{I}\{t = j\} + \sum_{j=2001}^{2010} d_j \mathbb{I}\{q_m = 4\} * \mathbb{I}\{t = j\} + \alpha_m + \delta_t + v_{m,t} \tag{3.2}$$

which includes a lag dependent variable, and MSA and year fixed effects as controls. The c_j and d_j coefficients track the average effect of MSAs with shocks in the first (bottom 25%) and fourth (top 25%) quartiles respectively. Coefficients for $j \leq 2004$ serve as falsification tests for any effects before the 2005 shock whenever data is available.

Table 3.4 summarizes the c_j and d_j coefficients for MSA-level variables: personal income per capita and average housing transfer; and for household-level variables: beneficiaries' rent payments and beneficiaries' proportion of income spent in rent. There is no strong evidence that economic activity decreased in MSAs that on average had a downward revision (column 1); the cumulative effect between 2005 and 2010 indicates that MSAs in the lowest quartile had 1.3 percent less income per capita than MSAs in the middle quartiles but the sum of coefficients is not significant and though the individual coefficients for 2009 and 2010 are significant, there is some concern that these MSAs were already in a declining pattern as seen in the negative coefficients before 2005 (significant for 2002). Similarly, there is no strong evidence that MSAs in the top quartile (column 2) had larger income per capita than the middle quartiles.

The panel data specification confirms the results from the earlier event study for the policy variation. The average transfer decreases almost 2 percent in the same year and 5 percent the following year for areas where FMR was revised downwards compared to the rest of the MSAs (column 3); while the increase in the average transfer following an upward FMR revision is not significant (column 4). This explains why MSAs in the bottom 25% increase their rent payments by 3 percent in 2006 and 4.5% in 2007 (column 5) and by 1 percentage point as a proportion of income (column 8). Though the increases in transfers do not seem to have a significant decrease in payments towards rent (column 6), rent payments, as a proportion of income, decrease by half and one percentage point the same year and the following year of the 2005 revision, suggesting an increase in household income.[15] Finally, across outcome variables, I cannot reject that the effects of receiving a downward or upward revision are symmetric, similar in magnitude but in opposite directions (the last coefficient in the table).

[15] Appendix Table 3.5 shows the effects on beneficiaries household income along with other outcome variables.

Table 3.4: Panel Data

	Personal income per capita		Average transfer		Household rent payments		Rent payment/income	
	Bottom 25%	Top 25%	Bottom 25%	Top 25%	Bottom 25%	Top 25%	Bottom 25%	Top 25%
	(1)	(2)	(3)	(4)	(5)	(6)	(8)	(9)
1{t = 2001}	-0.64	0.07						
	(0.58)	(0.55)						
1{t = 2002}	-0.84*	0.34						
	(0.49)	(0.51)						
1{t = 2003}	-0.54	0.21						
	(0.35)	(0.60)						
1{t = 2004}	0.00	0.00						
	(.)	(.)						
1{t = 2005}	0.20	0.30	-1.96***	0.09	0.23	-1.01	0.13	-0.49**
	(0.40)	(0.40)	(0.67)	(0.77)	(0.86)	(0.86)	(0.23)	(0.23)
1{t = 2006}	0.13	-0.42	-5.01***	0.18	3.01**	-1.19	0.75**	-0.98***
	(0.62)	(0.59)	(1.22)	(1.55)	(1.43)	(1.46)	(0.35)	(0.35)
1{t = 2007}	0.01	-0.30	-5.38***	0.31	4.52**	-0.80	1.37***	-0.81**
	(0.77)	(0.73)	(1.83)	(2.32)	(1.77)	(2.02)	(0.46)	(0.39)
1{t = 2008}	0.53	0.91	-8.97***	1.13	3.56*	-1.80	0.79**	-0.78
	(0.73)	(1.12)	(3.14)	(3.49)	(2.01)	(2.66)	(0.31)	(0.47)
1{t = 2009}	-1.26**	0.06	-10.31***	2.83	2.62	-0.11	1.01***	-0.54
	(0.57)	(1.37)	(3.04)	(4.02)	(2.06)	(2.27)	(0.37)	(0.42)
1{t = 2010}	-0.92*	0.64	-9.77***	2.06	1.54	-0.49	1.00***	-0.63
	(0.54)	(0.40)	(3.00)	(3.51)	(2.05)	(2.39)	(0.34)	(0.43)
Sum t=2005,...,t=2010	-1.33	1.20	-41.41***	6.61	15.48**	-5.41	5.04***	-4.23**
	(2.62)	(2.96)	(11.20)	(14.31)	(7.56)	(10.66)	(1.65)	(1.98)
$\Sigma c_j + \Sigma d_j$	-0.14		-34.80		10.07		0.81	
	(4.59)		(21.16)		(14.44)		(2.90)	
Observations	2,870		2,296		2,296		2,296	
Adj. R-squared	0.984		0.913		0.860		0.911	

MSA-level variables: personal income per capita and average transfer. Household-level variables: beneficiaries' rent payments and proportion of income spent in rent. Effects on MSAs in the bottom 25% and top 25% of the 2005 FMR revisions. Left column of each variable reflects the c_j coefficients and right column the d_j coefficients from the regression $y_{m,t} = a + by_{m,t-1} + \sum_{j=2001}^{2010} c_j \mathbb{I}\{q_m = 1\} * \mathbb{I}\{t = j\} + \sum_{j=2001}^{2010} d_j \mathbb{I}\{q_m = 4\} * \mathbb{I}\{t = j\} + \alpha_m + \delta_t + v_{m,t}$ .Regressions are weighted by population. Standard errors clustered by MSA.

3.5.3 IV estimates

I next use the transfer shock to instrument the average housing voucher transfers in 2005 and later years implementing the following set of regressions:

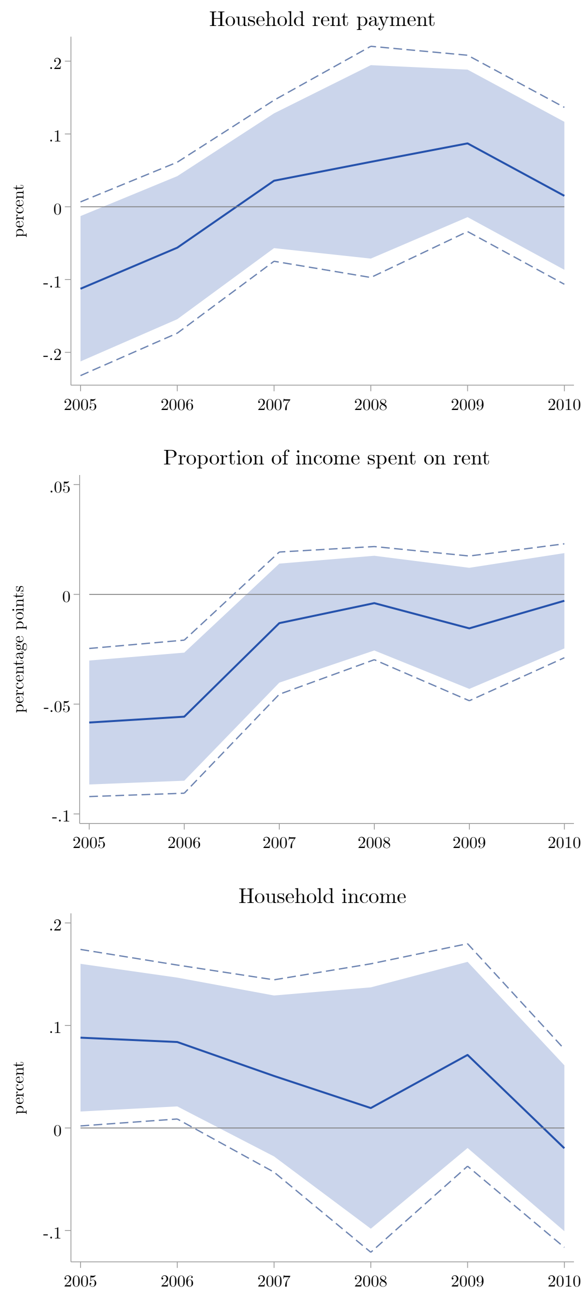
$$y_{m,t} = \beta_o + \beta_1 \text{log transfer}_{m,t} + \Phi X_{t-1} + v_{m,t} \tag{3.3}$$

for $t = 2005, .., 2010$. The controls in X_{t-1} include a lagged dependent variable and the level of FMR in 2004 for HUD variables, and is limited to a lag-dependent variable for BEA variables. Figure 3.5.3 shows the effects on three household voucher holders' variables: rent payment, income share spent on rent, and gross income; and Figure 3.5.4 the effects on MSA-level variables, GDP and personal income per capita.

The IV-estimates are larger in magnitude and less precise than the previous specification, but overall paint the same picture: a 1 percent increase in housing transfers has modest positive effects on the beneficiary households, as it reduces out-of-pocket rent expenditures (upper panel of Figure 3.5.3) and increases household income (bottom panel) by similar small magnitudes (by 0.1 percent in the same year of the revision), leading to more resources available as evidenced by the decrease in the share of income spent on rent payments (middle pannel). The effects are short-lived and cannot be statistically differentiated from zero from 2007 onwards.

The three figures point towards reinforcing factors: raising the subsidies thresholds decreases the average payment rent made by the household, raises available income even a year after the shock, and subsequently reduces the proportion of income spent on rent, suggesting that other factors such as consumption and savings could be raised following the policy change. Does this have an effect on the local economy? Not if measured by the MSA GDP per

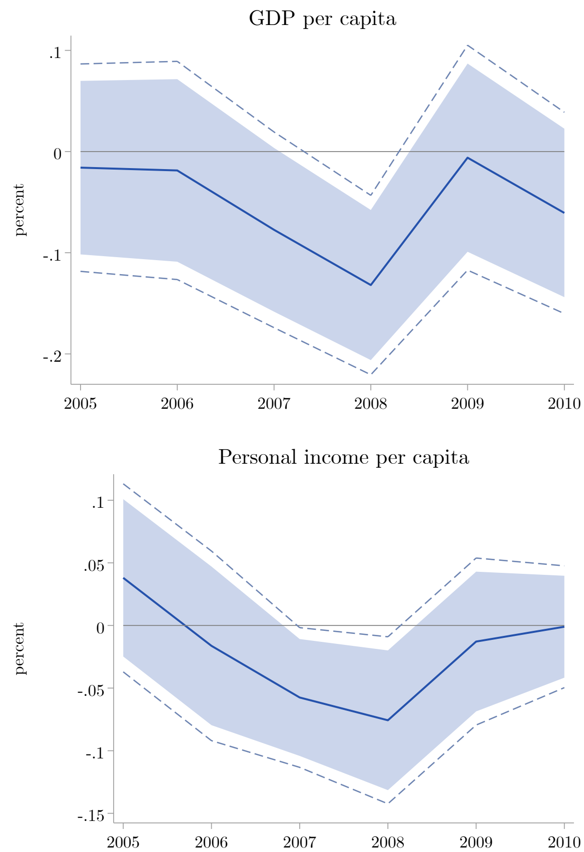
Figure 3.5.3: IV estimates - voucher holders variables



90% and 95% Confidence Intervals shaded area and dashed lines respectively. Standard errors clusted by MSAs.

capita, which would suggest a decrease in the local activity (upper panel of Figure 3.5.4). However, there is suggesting evidence that activity measured in a narrower way, such as the MSA personal income per capita (bottom panel of Figure 3.5.4) increased in the first year of the policy. However, these estimates are not statistically significant.

Figure 3.5.4: IV estimates - effects on local economy



90% and 95% Confidence Intervals shaded area and dashed lines respectively. Standard errors clusted by MSAs.

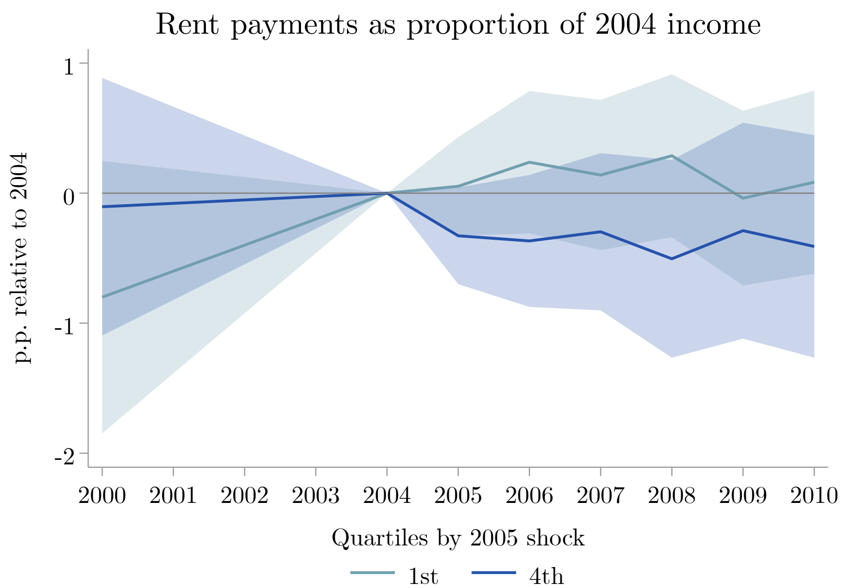
3.6 Conclusion

In this paper, I examine the effects of changes in housing voucher transfers on the local economy. The inclusion of the 2000 Census in the computation of the 2005 maximum transfer level resulted in corrections that could arguably be attributed to measurement errors. I show that these changes were not systematically related to previous economic trends and use the geographic dispersion across metropolitan areas (MSAs) to instrument the change of transfers in 2005 and subsequent years. I find that MSAs where the maximum generosity level was revised downward (on average by 10%) decrease the average household transfer by 5% a year and two years after the shock, leading to beneficiaries to spend 3 and 4.5 percent more in payments towards rent and spending up to 1.4 percentage points more of their income in rent. Although I do not find significant effects for MSAs where the maximum generosity level was increased, I cannot reject the hypothesis of cumulative symmetric effects. The IV estimates suggest that an increase of 1% in the average transfer, increases the beneficiaries' household income by 0.1% and decreases by 0.05 percentage points the proportion of income spent on rent. This suggests that housing vouchers relax household liquidity constraints for voucher holders. Whether these behavioral effects have effects on the aggregate economy is less clear. The small and imprecise estimates for GDP and personal income per capita at the MSA-level suggest that the general equilibrium effects of increasing housing voucher transfers by one percent are close to zero.

Natural next steps are to assess the labor supply channel as an explanation of the increase in household income, preferably with a sample that includes all MSAs for which transfer data is available. Second, I would like to confirm that the evidence suggesting the relaxation of liquidity constraints indeed increases household consumption. Most of the previous literature has found positive effects on consumption coming from increases in transfers. This would help explain the suggestive evidence pointing towards an immediate increase in personal income per capita following the housing voucher shock.

Appendix

Figure 3.6.1: Quartiles event studies



95% Confidence Intervals. Standard errors clusted by MSAs.

Table 3.5: Panel Data Estimates

	Fair Market Rent		GDP per capita		Household income		Rent payment/2004 income	
	Bottom 25%	Top 25%	Bottom 25%	Top 25%	Bottom 25%	Top 25%	Bottom 25%	Top 25%
	(1)	(2)	(3)	(4)	(5)	(6)	(8)	(9)
1{t = 2001}	1.85	0.09						
	(1.13)	(0.72)						
1{t = 2002}	1.79	0.71	-1.28**	-0.53				
	(1.21)	(1.37)	(0.54)	(0.64)				
1{t = 2003}	0.36	-1.71**	-0.67	-0.39				
	(0.79)	(0.84)	(0.52)	(0.44)				
1{t = 2004}	0.00	0.00	0.00	0.00				
	(.)	(.)	(.)	(.)				
1{t = 2005}	-7.85***	8.28***	-0.48	-0.83	-0.29	0.65	0.05	-0.29
	(1.19)	(1.28)	(0.47)	(0.64)	(0.45)	(0.47)	(0.25)	(0.26)
1{t = 2006}	-5.71***	1.85	-1.02*	-0.47	0.45	2.13***	0.90**	-0.33
	(1.58)	(2.58)	(0.59)	(0.64)	(0.67)	(0.75)	(0.44)	(0.45)
1{t = 2007}	-3.97***	3.59***	0.55	-0.22	0.05	2.02**	1.40**	-0.20
	(0.74)	(0.62)	(0.52)	(0.59)	(0.82)	(0.99)	(0.56)	(0.62)
1{t = 2008}	-5.22***	3.56***	0.78	-0.35	0.90	0.92	1.04*	-0.51
	(1.35)	(1.10)	(0.72)	(0.91)	(1.81)	(1.60)	(0.60)	(0.79)
1{t = 2009}	-3.35***	4.21***	-1.03	-0.78	-0.77	1.81	0.79	0.02
	(1.15)	(0.72)	(1.22)	(1.45)	(2.10)	(1.93)	(0.62)	(0.68)
1{t = 2010}	-1.92	3.29***	-0.73	-0.74	-1.81	1.73	0.45	-0.10
	(1.43)	(0.71)	(0.71)	(0.76)	(2.14)	(1.97)	(0.61)	(0.70)
Sum t=2005,...,t=2010	-28.02***	24.79***	-1.93	-3.39	-1.48	9.24	4.63**	-1.41
	(5.58)	(5.09)	(3.01)	(3.62)	(6.35)	(6.54)	(2.26)	(3.21)
$\Sigma c_j + \Sigma d_j$	-3.23		-5.31		7.76		3.22	
	(8.83)		(5.65)		(11.09)		(4.34)	
Observations	2,870		2,583		2,296		2,296	
Adj. R-squared	0.985		0.989		0.934		0.560	

MSA-level variables: FMR and GDP per capita. Household-level variables: beneficiaries income and proportion of 2004 income spent in rent. Effects on MSAs in the bottom 25% and top 25% of the 2005 FMR revisions. Left column of each variable reflects the c_j coefficients and right column the d_j coefficients from the regression $y_{m,t} = a + by_{m,t-1} + \sum_{j=2001}^{2010} c_j \mathbb{I}\{q_m = 1\} * \mathbb{I}\{t = j\} + \sum_{j=2001}^{2010} d_j \mathbb{I}\{q_m = 4\} * \mathbb{I}\{t = j\} + \alpha_m + \delta_t + v_{m,t}$ .Regressions are weighted by population. Standard errors clustered by MSA.

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