Fiscal Policy and the American Nonprofit Sector

by

Nicolas John Duquette

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Doctoral Committee:

Professor James R. Hines, Jr., Chair
Assistant Professor Achyuta R. Adhvaryu
Associate Professor Martha J. Bailey
Professor Paul W. Rhode
Professor Joel B. Slemrod

Former President Lyndon Johnson, President Richard Nixon, and their wives in California with Governor Ronald Reagan for the dedication of Lady Bird Johnson Grove in Redwood National Park. August, 1969. Source: Wikimedia Commons [accessed April 2014].

For Joanna

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While this dissertation has just my name under its byline, many others had a hand in its completion. Any mistakes in this dissertation are the author's only, but anything done well can be credited to my mentors, friends, and colleagues, too.

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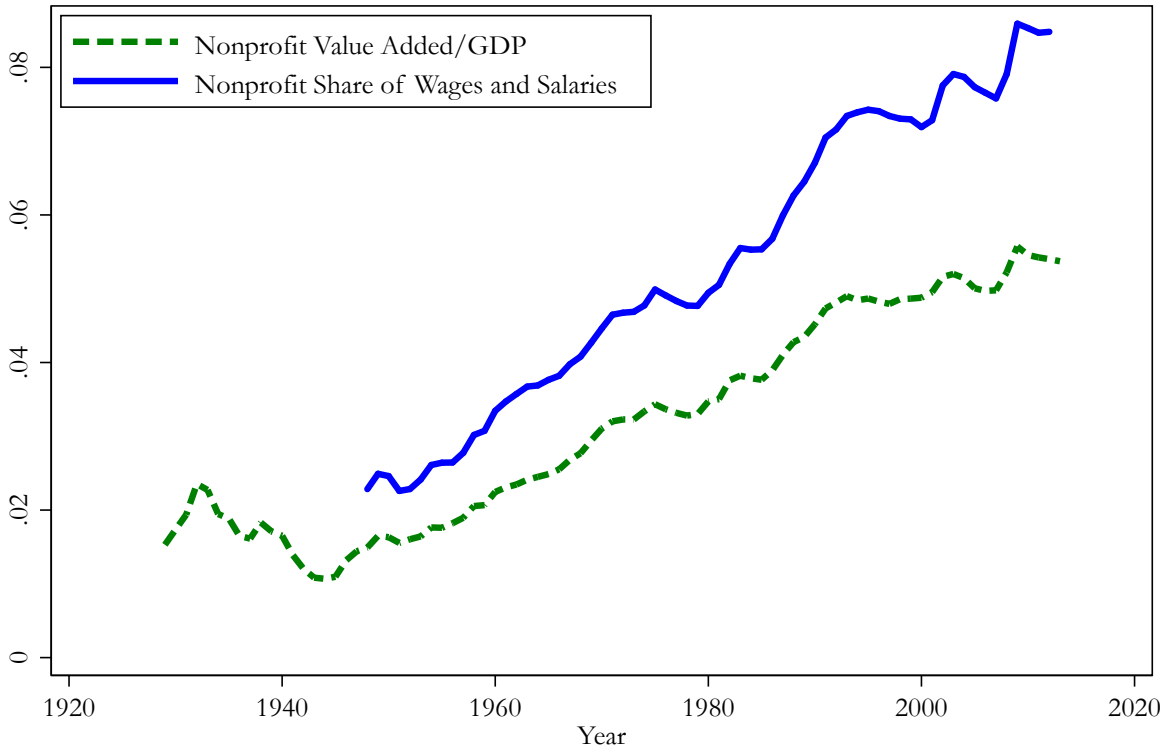
CHAPTER 1

Introduction: Private Nonprofits in the American Economy

A traditional economic model of the economy has at most three sectors: households comprising the consumers who purchase goods, work for wages, and enjoy utility; for-profit firms which make investments and maximize their profits; and a government which sets taxation and spending policies in pursuit of some social welfare function. Yet a fourth type of agent, the private nonprofit organization, is responsible for a large and growing share of the American economy. By definition, private nonprofits are neither profit-maximizing nor public enterprises in the ways usually meant by economists, and economic models of the firm or of government are unlikely to be applicable to these entities.

Figure 1.1 plots the nonprofit sector's share of the American economy over time. The sector has never been small — its lowest level was about one percent of GDP at the end of the Second World War — and it has grown over time, to roughly five percent of GDP and ten percent of wages and salaries in 2012. In spite of the nonprofit sector's high and growing importance, it has received comparatively little attention from economics. A search in the Econlit database for peer reviewed articles in the "top five" economics journals with abstracts containing the words "nonprofit," "non-profit" or "charity" turned up just

Figure 1.1: Nonprofit Sector's Share of the American Economy Over Time



Notes: Series represent ratio of nonprofit value added to total GDP and ratio of nonprofit wages and salaries, plus supplemental benefits, to all wages, salaries, and supplemental benefits.
Sources: FRED, Federal Reserve Economic Data, Federal Reserve Bank of St. Louis: National income: Households and institutions: Nonprofit institutions serving households: Compensation of employees: Wage and salary accruals [W481RC1A027NBEA] ; National income: Households and institutions: Nonprofit institutions serving households: Compensation of employees: Supplements to wages and salaries [W482RC1A027NBEA] ; Compensation of employees: Wages and salaries [A576RC1A027NBEA] ; Compensation of employees: Supplements to wages and salaries [A038RC1A027NBEA] ; Gross value added: GDP: Households and institutions: Nonprofit institutions serving households [B702RC1A027NBEA] ; Gross Domestic Product [GDPA] ; `http://research.stlouisfed.org/fred2/`; accessed March 6, 2014.

twelve items. In contrast, "airline" returned fifteen items; "sports" returned nine. "Hospital" returned sixteen articles, variations on "college" and "university" twenty, and a set of keywords encompassing the arts (such as "symphony," "museum," and "dance") returns sixteen, meaning that more articles have been published in economics' most influential journals on particular nonprofit-heavy sectors than on the nonprofit sector as a whole.[1]

[1]Of course, not every article in the leading economics journals concerns the US economy, and the private nonprofit sector is particularly large in the USA. The "top five" general interest economics journals are *American Economic Review*, *Econometrica*, the *Journal of Political Economy*, the *Review of Economic Studies*, and the *Quarterly Journal of Economics*. Searches in EconLit were conducted using the `ab` and `pub.exact`

This dissertation explores the nonprofit sector generally, focusing on the ways that policies to encourage private nonprofits affect the organizations themselves. Chapter 2 estimates the importance of the charitable contribution tax deduction for charitable giving. The effect of tax incentives on individuals' reported contributions is an old and thoroughly studied question in the empirical tax literature; a meta-analysis by Peloza and Steel (2005) tabulates 70 papers estimating the tax elasticity of charitable giving published by 2004. However, this literature examines the contributions reported by *individuals* on their tax returns or in survey responses. While these studies are useful for understanding how incentives affect altruistic behavior, they only provide indirect evidence on the relationship between tax incentives and the resources available to charities, the entities that actually provide the services tax policy seeks to encourage.

Chapter 2 looks instead at the charitable revenues reported by charities themselves on their annual IRS form 990. Because only total donations are observed at the charity level, I restrict the data set to charities which plausibly received virtually all of their contributions from donors in the state of the filed 990. I then study how reported contributions changes in response to the after-tax cost of a charitable contribution for a representative taxpayer under the laws of a particular state and year. The federal Tax Reform Act of 1986 provides exogenous variation in this measure because of its numerous changes to the tax base, as well as to the federal rate schedule, interact with the details of state income tax laws to create substantial unintended variation in the change in tax incentives across US states. I estimate fairly large effects, finding elasticities of giving with respect to representative tax price of giving of approximately -4; in contrast, the median estimate reported for individual tax return data in Peloza and Steel is about -1.2. The discrepancy appears to be explained by heterogeneity in the sensitivity of contributions among donors and among charities—high-income households give the largest amounts and are more tax-sensitive than other

search operators.

households, making the response of the typical *dollar given* to tax rate changes different from the response of the typical *donor*.

Understanding how tax policy affects contributions received by charities is valuable, but is insufficient to explain how tax subsidies for giving ultimately affect the provision of services like education, health care, arts and human services that the special tax treatment of charities is meant to encourage. For example, if the charitable contribution increases giving to hospitals, but those contributions only serve to increase the salaries or perquisites of staff without improving the quality, quantity, or price of health care provided, then the contribution deduction may not be an effective alternative to direct government provision of health care.

Chapter 3 considers how charities actually use their charitable contributions. I document a tendency of charities to save their donations, rather than spend them immediately on service provision. This behavior is in contrast with the tendency of charities to spend substantial shares of other revenues in the period they are received. If charities save rather than spend their contributions, at the margin contributions (and policies that encourage them) may not actually be substitutes for government expenditures on similar services. I extend the data analysis of chapter 2 to look at the effect of a change in tax incentives on charities' *services provided*. I show that program expenditures do not fall abruptly in response to TRA86 tax cut, but instead decline gradually, taking several years to reach maximum impact. While charities treated with a bigger increase in the after-tax cost of contributions are less likely to engage in paid fundraising than charities facing a smaller increase, the difference across charities by treatment in the book value of net assets widens gradually. Policy makers considering a change in tax incentives for contributions should therefore consider the long-term as well as short-term effects on charities' services, as the magnitudes differ.

Tax policy is not the only way government interacts with private nonprofits. Many charities operate substantially as government contractors, while others pursue their missions

using government grants. Grantmaking makes more resources available for a particular cause, but it can also lead to strategic decisions by donors or charities that undermine the purpose of the grant. For example, Andreoni and Payne (2003) find that charities receiving grants report lower fundraising activity and lower charitable contributions. Unfortunately, the American grantmaking system is difficult to characterize because of its federalist nature; the levels of government either interact simultaneously (such as the mix of state appropriations and federal student loans and research grants made to public universities) or sequentially (such as the routing of federal block grants through state and local governments) in ways that make it difficult to separate the effects of the grants themselves from the effects of shifts in the political relations among private organizations and the layers of government. It would be preferable to study a grantmaking program with fewer institutional constraints on grantmaking and clear political goals.

Such a program once existed. The Community Action Program (CAP) was a centerpiece of Lyndon Johnson's War on Poverty that made grants directly from the federal government to anti-poverty community organizations with only very modest restrictions on the grants' intended uses, their distribution within US states, or the ability of any officials outside the White House itself to divert resources. Indeed, the CAP was explicitly used to *undermine* local elites, dismantle racial segregation, and empower the poor in targeted communities. Where state and local governments opposed poverty relief, the CAP was permitted to work against them; and where existing nonprofit groups were too closely aligned with existing interests, the CAP was empowered to create a new, more suitable organization. As Republican Robert Taft, Jr. of Ohio presciently groused during the House hearings on the legislation that created the CAP, the Director of the Office of Economic Opportunity would be empowerd to "do as he pleased... There's actually no requirement that the Director consult with anyone, other than find some local agency of some sort, public or private, which would be willing to go along. If he did not have one, he could create

one" (*Congressional Record* 1964a).

Chapter 4 contributes a novel quantitative understanding of the political economy underpinning the CAP. Newly compiled county-level data on the distribution of spending over the span of the Johnson administration reveals that the distribution of CAP spending is primarily explained by the distribution of poor and nonwhite Americans, and not by political or electoral considerations. This stands in contrast to Franklin D. Roosevelt's New Deal, which aggressively targeted areas with powerful Democrats or swing voters (Wright 1974; Fishback et al. 2003), and which, ironically, is generally remembered as a less political and more successful anti-poverty agenda than Lyndon Johnson's Great Society programs. Understanding the apparent strategy taken by the Johnson Administration increases our understanding of this era's federal social programs, while providing the first step toward understanding how this large and surprisingly unconstrained federal program may have effected the private nonprofits it made grants to or, in some locations, chose to compete with.

The primary contributions of this dissertation are, first, the development of a new strategy for identifying plausibly exogenous changes in tax incentives for charitable giving by exploiting interactions between state and federal tax laws; second, estimation of a sensitivity of charitable receipts to tax incentives substantially higher than suggested in the individual tax data; third, estimation of short- and long-run responses of the ultimate provision of charities' services to a change in tax incentives for charitable contributions; and lastly, explicating the poverty- and segregation-targeting political economy of the CAP, a federal grantmaking program that disbursed sums of money with few institutional restrictions in a manner unusual in the postwar era. I hope the reader will find this dissertation to be a useful contribution to our understanding of the history and behavior of private charities in relation to government policy.

CHAPTER 2

Do Tax Incentives Affect Charitable Contributions? Evidence from Public Charities' Reported Revenues

Private nonprofit organizations provide many crucial services in the US. They grant 30 percent of bachelor's degrees, make 69 percent of hospital admissions, and supply nearly 100 percent of religious services. Private nonprofits make up 71 percent of museums and 89 percent of emergency shelters and soup kitchens. These organizations are supported in part by donors' gifts; in 2012, charitable giving was equal to 2.0 percent of gross domestic product.[1]

Without the nonprofit sector, many of these goods and services would likely be supplied by the government. Instead of direct government provision, the US indirectly supports nonprofits by exempting them from many income and property taxes that for-profit firms are obliged to pay. Additionally, organizations which serve particular causes can be registered as public charities under section 501(c)3 of the Internal Revenue Code.[2] Donations to

[1]Sources: U.S. Department of Education, National Center for Education Statistics, *The Condition of Education 2013*; American Hospital Association, *AHA Hospital Statistics*, 2012; Institute of Museum and Library Services, *Exhibiting Public Value: Government Funding for Museums in the United States*, 2008; U.S. Bureau of the Census, National Survey of Homeless Assistance Providers and Clients (1999); Giving USA (2013); Bureau of Economic Analysis.

[2]Public charities are a subset of nonprofit organizations. Other types of nonprofit organization enjoy a wide variety of tax subsidies, such as exemption from most income and property taxes paid by for-profit firms, while only public charities and private foundations can receive tax-deductible contributions. Examples of tax-

public charities can be taken as itemized deductions on households' tax returns, reducing the donors' income tax.

This additional tax benefit to donors is meant to increase charitable giving (and avoid the need to supply more services via taxation). However, the effectiveness of this incentive is subject to debate. In the philanthropic world, it has become a stylized fact that charitable giving is fixed at about two percent of gross domestic product, regardless of tax rates.[3] Figure 2.1 plots the ratio of total estimated giving to GDP over time. Even as the top marginal tax rate fell from 91 percent at the close of the Second World War to 28 percent in 1988 (before rising to 39.6 percent today), total contributions have indeed remained steady at roughly two percent of GDP since 1955, and both major political parties have put forward proposals to increase tax revenue from high-income households by limiting the charitable contribution deduction.[4] Yet, a large empirical literature has found a range of behavioral responses to tax incentives in household data. Peloza and Steel (2005) analyze 70 studies of the tax elasticity of charitable giving, and tabulate estimates ranging from 0 to -7 (that is, from no effect to a very large effect), with a median estimate of about -1.2. The importance of the charitable contribution tax deduction to charities therefore remains an unsettled question.

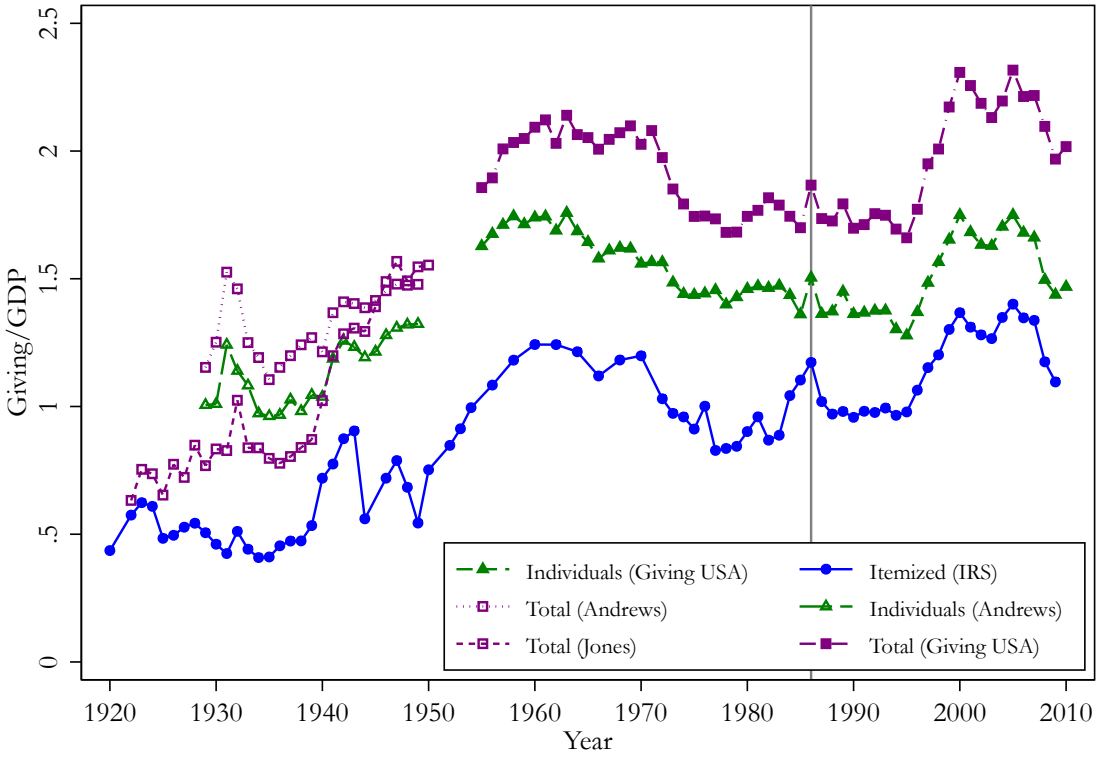
This paper provides new evidence on this question using the Tax Reform Act of 1986 (TRA86), which completely overhauled the federal tax code, including a reduction of the

exempt nonprofit organizations that cannot accept tax-deductible contributions include social welfare groups, political organizations, homeowners' associations, and some professional sports leagues. See Hopkins (2007, §1.2-1.3).

[3]See for example the June 17 2013 *Chronicle of Philanthropy*, "The Stubborn 2% Giving Rate," or Dec. 12 2012 *Wall Street Journal* "A Christmas Wish for Charities."

[4]An Obama administration budget proposal would have limited the rate for the contribution deduction to 28 percent, so that taxpayers in the top 39.5 percent bracket would still owe 11.5 percentage points (39.5-28=11.5) to the federal government on income given to charity (*New York Times*, 11 April 2013, "White House Budget Curbs Some Deductions for the Wealthy"; *Chronicle of Higher Education*, 10 April 2013, "Obama Renews Effort to Limit Charitable Deduction"). A counterproposal by Republican senators would have capped all itemized contributions at two percent of income, including charitable contributions (Martin Feldstein in the *Washington Post*, 12 March 2013, "It's time to cap tax deductions").

Figure 2.1: Charitable Contributions/GDP, 1919-2010



Notes: Total contributions includes charitable giving not out of living persons' income, including bequests, gifts out of foundations, and corporate contributions. Individual giving excludes estates and organizations. Itemized contributions are those claimed on an individual tax return.
Sources: Andrews (1950) estimates personal contributions for itemizers and non itemizers from the Survey of Current Business and Statistics of Income data sets; Jones (1954) totals from Statistics of Income aggregates; Giving USA (2013) from various sources; Itemized contributions from Statistics of Income; nominal GDP from Bureau of Economic Analysis and from Carter et al. (2006, Table Ca9-19).

top marginal income tax rate from 50 percent to 28 percent and the elimination or modification of several deductions. Tax rates determine the "price" of giving to charity, because giving \$1 to a charity costs an itemizing taxpayer only \$1–τ in after-tax personal consumption, where τ is the marginal tax rate.[5] A tax cut is therefore equivalent to a price increase in the cost of charitable giving, and can help to identify the importance of this incentive for

[5]For example, with a tax rate of 36 percent, an itemizing tax payer can give \$1 to a public charity, or could pay the tax authority 36 cents and keep 64 cents for herself. So by reducing the top marginal rate from 50 percent to 28 percent, the TRA86 increased the federal tax cost of giving \$1 to charity among top-bracket itemizing taxpayers from 50 cents to 72 cents, the amount of after-tax income the household could otherwise keep for personal use.

donors.

Federal and state tax laws interact in myriad ways, such as states' reuse of federal tax definitions, or the deductibility of state tax from federal taxable income (and sometimes vice versa). I demonstrate that preexisting differences among state income tax laws resulted in substantial differences across states in the change in overall tax cost of giving following the radical revision of the federal tax code. For example, under 1986 tax law, a representative donor in Kansas and in North Carolina both faced a tax cost of giving of about \$0.67 (\$1 minus a 33 percent marginal tax rate). In 1988, after the TRA86 was fully phased in, the tax cost rose to \$0.82 in Kansas, but to just \$0.78 in North Carolina. The \$0.04 differential is caused by differences in the state income tax systems preceding the federal reform. I show that changes across states in tax cost of charitable giving are uncorrelated with the tax cost of giving before the federal reform.

I exploit these tax cost changes using a panel of reported contributions from charitable organizations' Internal Revenue Service (IRS) filings, the federal form 990. I find that a one percent change in the tax cost of giving following the TRA86 causes about a four percent decline in charitable contribution receipts. Extensions of the analysis demonstrate that pre-trends in charitable giving or intertemporal shifting behavior do not drive these results. Further checks confirm that these results are not driven by sample selection bias, entry and exit of organizations, extensive margin outcomes, endogenous policy changes, or outliers. Such elasticities imply a larger tax-sensitivity of charitable giving than is apparent in the aggregate data or than has been reported by most studies using household data.

This greater tax-sensitivity can be explained by heterogeneous responses of donors and charities alike to tax incentives, and differences in the composition of available data sources. Household tax data only permits analysis of returns with an incentive to report their contributions (generally, higher income households with sufficient deductions to file Schedule A). The IRS does not require some major charitable sectors, particularly

churches, to file a form 990, and my identification strategy requires a focus on local rather than nationally prominent charities. The discrepancy between my estimates and the household literature is consistent with the prior literature on heterogeneous tax sensitivity by household income and by church/non-church charities. Further analysis of the form 990 sample reveals that the effect of the 1986 tax change on charitable contributions is more important for some charities than others, particularly health-related causes, and that the behavior of upper-income households appears to drive variation in giving.

It is the importance of upper-income households for charitable giving that explains the apparent stability of the contributions-to-GDP ratio over the postwar era. Even as tax rates have fallen, the share of national income going to the top earners has risen, offsetting the negative incentive effect with a positive income effect. In no decade were either of these trends as pronounced as in the 1980s. For some local, 990-filing charities, tax incentives to give are very powerful. Understanding the differences in incentive effects across donors and charities is important for interpretation of charitable giving elasticities and for predicting likely outcomes of future tax reforms.

2.1 Charitable Contributions and the US Tax System

The charitable contribution deduction was added to the federal tax code by the War Revenue Act of 1917. The federal government sharply increased the burden of the federal income tax on high-income households as the US prepared to enter the First World War, increasing the top marginal rate from 15 percent to 67 percent. An amendment to the 1917 tax act was introduced by Senator Henry F. Hollis of New Hampshire (who also happened to be a regent of the Smithsonian Institution), allowing up to 15 percent of income to be given without tax to "corporations or associations organized and operated exclusively for religious, charitable, scientific, or educational purposes, or to societies for the prevention

of cruelty to children or animals" (*Congressional Record* v. 55 pt. 7 p. S6741). Charitable giving is a luxury good, Hollis argued, and "usually people contribute to charities and educational objects out of their surplus. After they have done everything else they want to do. . . they will contribute it to a college or to the Red Cross or for some scientific purpose." Therefore, at the margin, high-income households will maintain their own consumption first, and "when war comes and we impose these very heavy taxes on incomes, [charity] will be the first place where the wealthy men will be tempted to economize" (*Congressional Record* v. 55 pt. 7 p. S6729).

Hollis's amendment was accepted quickly and unanimously. The brief Congressional debate on the matter, however, presaged a long scholarly one: is the Senator's fear that charitable contributions respond to taxation empirically relevant? The literature estimating individual donors' response to tax incentives is large and long, but a consensus on the effect the deduction has on charitable giving remains elusive. A meta-analysis by Peloza and Steel (2005) tabulates 70 peer-reviewed studies, most estimating a tax elasticity of charitable contributions between -4 and 0, with a median of about -1.2.

One problem with individual tax return data is correctly parsing observed changes in permanent giving and shifting of giving across years to maximize the tax benefit of anticipated rate changes. Using panel data, Randolph (1995) finds that most of the tax response is temporary shifting, with a permanent giving tax elasticity of about -0.5, while Auten et al. (2002) find a permanent elasticity of -1.2, with a small temporary response. Because a household's income and its tax rate are highly correlated, panel data analysis requires strong assumptions about the comparability of tax changes across time and across income groups; see the discussion of estimation issues in Andreoni (2006) and Bakija and Heim (2011). In addition to shifting of contributions across years, survey data with information on volunteering has found that tax subsidies do not drive away donors, but instead affect the substitution between commitments of time (volunteering) and money (Gruber 2004;

Feldman 2010).

Individual tax filing data also measures actual charitable giving with error. Itemizers overstate their contributions to evade taxation (Slemrod 1989; Fack and Landais 2010), while non-itemizers have no incentive to report contributions at all, underreporting their donations (Dunbar and Phillips 1997; Duquette 1999). Survey datasets avoid the financial incentives to overstate or not report one's contributions, but can be costly to gather and may have their own errors and biases (*e.g.* if people do not recall their contribution amounts accurately, or overstate them to impress the survey-taker).

These problems with identification and measurement have motivated experimental approaches to the study of altruistic giving. Charitable giving experiments vary the cost of making a contribution through matching grants in a randomized fundraising campaign. For example, Karlan and List (2007) solicit donations from potential contributors with a randomized matching grant that will contribute an additional $1, $2, or $3 for every dollar contributed by the solicited donor (making the cost of a $1 contribution $0.50, $0.33, and $0.25, respectively) and find no effect on contributions from varying the match. Other experiments have found that varying a match does affect donations (Huck and Rasul 2011; Karlan et al. 2011), though as in studies of tax data, there is some evidence that lowering the cost of giving through a match may just induce donors to shift their donations across time (Meier 2007) or across charities (Konow 2010) rather than increase total giving. Karlan et al. (2011) and List (2011) provide overviews of this literature.

By demonstrating the importance of factors like charitable solicitation and leading grants, experimental approaches have revealed much about altruistic decisions, but our understanding of the effects of tax rates (or other changes in cost) on charitable giving is still murky. I therefore propose a new approach to this question: how does a change in the tax cost of giving affect contributions reported by the charities themselves?

This paper uses a new approach, using a completely different data source to sidestep

the reporting problems of individual data, combined with a new identification strategy that avoids endogenous or anticipated changes in tax rates. I exploit a federal tax reform that changed the tax cost of giving differentially across US states. By using this plausibly exogenous variation to isolate the causal effect of tax incentives on charitable giving, I avoid problems arising from endogenous fundraising by the organizations, the taxable income response of individuals, and any unobserved changes correlated both with successful passage of tax legislation and with charitable giving, plausibly avoiding endogenous or anticipated tax changes that are the bugbears of empirical tax research. I estimate the effect of this state-level variation on contributions reported by charities on the IRS form 990, not reported by donors, avoiding problems with unreported and overstated contributions in the individual tax return data. I use a difference-in-differences strategy to identify the effect of tax price on charitable contributions, and to combine a natural experiment with charities' reported contribution receipts data to estimate the effects of tax policy.[6] By exploiting exogenous variation across states in the tax cost of giving, I estimate the effects on charities' contribution revenues of changing donors' tax cost.

2.2 The 1986 Tax Reform: Background and Data Sources

The classic problem in identifying the effects of tax rates on behavior is the confounding influence of income on tax rates — after all, marginal tax rate is a nonlinear function of

[6]One paper by Yetman and Yetman (2013) uses form 990 data to estimate partial correlations of direct contributions with organization characteristics and a vector of time series, including last-dollar average tax cost, over the 1991-2007 period for major nonprofit subsectors. For the most part, however, economists have made use of 990 data to examine organizations' strategic behavior, not tax policy *per se*. Okten and Weisbrod (2000) and Andreoni and Payne (2003) use 990 data to show that nonprofits do not choose their fundraising intensity at a revenue-maximizing level, implying that a revenue-maximizing objective function is a poor description of these groups' behavior. Hines (1999) argues that charities pay unrelated income business tax — that is, they report non-tax-exempt income — only when their tax-exempt funding channels are insufficient to meet their needs (i.e. taxable income is sort of an inferior good). Marx (2012) finds that charities will *reduce* their income to avoid a tax compliance notch that requires greater administrative costs tracking their finances.

income and other variables. Separating income and price effects therefore becomes a challenge, and the best available strategies can require, for instance, comparing tax rate changes among high-income groups with low-income groups, or other not-quite-ideal approaches.

This paper will take a different approach by comparing the marginal tax cost of a charitable contribution for a fixed set of taxpayers across states and time. The crucial change is the Tax Reform Act of 1986 (TRA86). This large and complex federal tax reform not only overhauled the federal tax code, but interacted with state income tax codes such that the combined federal and state tax cost of giving changed differently across the states in ways unlikely to have been intended by legislators at either level of government. I use variation arising as an accidental byproduct of federal tax reform as plausibly exogenous variation in the tax cost of giving, and look for a difference in the changes in charitable giving across the states.

The Tax Cost of Giving

I construct a measure of the first-dollar tax cost faced by donors from the IRS Public Use File (PUF) of individual income tax returns and from the TAXSIM calculator (Feenberg and Coutts 1993; Internal Revenue Service). TAXSIM is a tax calculator maintained and hosted by the National Bureau of Economic Research which uses up to 198 different tax return variables to compute household federal tax liability for any year since 1960, or state tax liability for any of the fifty states or the District of Columbia since 1977.

Using a large, national cross-section of individual income tax returns from 1984, I set charitable contributions equal to zero and adjust all other dollar-valued variables for inflation, and calculate the combined federal and state tax liability of each return under the laws of each state and the District of Columbia, for each year. I then perform the same calculation, this time adding a small cash contribution to each return, and use the resulting change in tax liability to compute the first-dollar marginal tax cost of a cash contribution. I

then take the average of the marginal tax costs, across all returns by state and year, weighted by total reported contributions, to obtain a measure of the tax cost of giving.[7] The detailed steps of this calculation and more information on the public use cross-section data are in appendix A.

This approach creates a measure of the state-level effects of tax reform that is not influenced by states' income distributions or economic trends — only by states' legal environments. This approach can be thought of as the reduced form of a common instrumental variables strategy exploiting policy variation across states. For example, Currie and Gruber (1996a,b) apply Medicaid eligibility laws by state and year to a constant, nationally representative sample of 300 children at each age from zero to fourteen to study the health effects of Medicaid reform. Health outcomes and insurance utilization are endogenous, but their state-level measure of insurance eligibility is not. Fishback and Kantor (1995) use changes in workers' compensation laws across states and time to construct a measure of the value of coverage. Feenberg (1987) uses variation across states for a fixed set of returns to identify individuals' tax cost of giving in a cross-section of 1977 returns. However, this is the first paper to use such a strategy to identify variation in the tax cost of a charitable contribution across states and years.

[7] In section 2.1, I noted that contributions reported on individual tax returns are reported with error. The data for 1984 are better than other years in terms of observing the giving of non-itemizers because of the presence of a modest above-the-line contribution deduction introduced by the Economic Recovery Tax Act of 1981 (and abolished by the TRA86), which allowed a limited deduction for the first $300 of charitable giving. This deduction does have limitations: 18.1 percent of non-itemizers claim the maximum allowable amount, and contributions by people not required to file, or who owed no tax against which to deduct their contributions, are still unlikely to be observed. On the other hand, the above-the-line deduction was not aggressively audited, giving both itemizing and non-itemizing taxpayers an incentive to overstate their donations and reduce their taxes in this year. (I prefer the 1984 data to the 1985 cross-section specifically because the $300 limit, which was raised in the following year, censors the dishonest as well as the generous; using 1985 data obtains very similar results, though.)

The 1986 Tax Reform as an Exogenous Shock

I focus on the changes implemented by the Tax Reform Act of 1986 (TRA86). The TRA86 is best known for its steep reduction in marginal rates — the top rate fell from 50 percent to 28 percent — but it also radically altered the tax base. The TRA86 was designed to be revenue-neutral within income deciles, and for each point shaved off a tax rate, somewhere else a deduction, rule, credit or policy had to be altered. It is the scope and complexity of these other changes — combined with the sudden reduction in marginal tax rates — that makes the TRA86 a credible natural experiment.

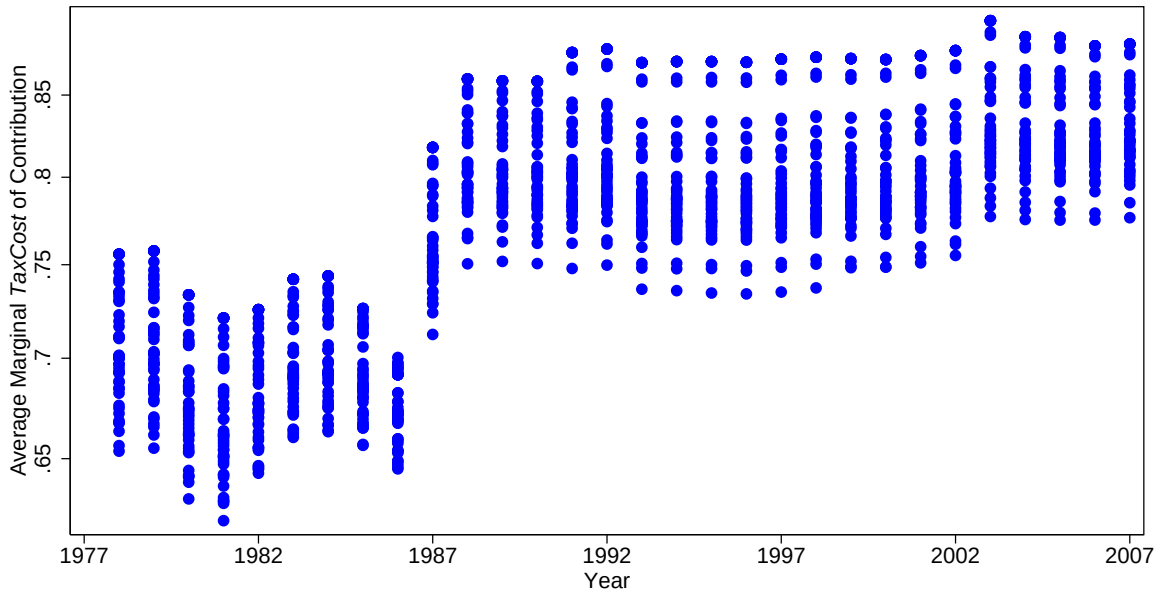
Figure 2.2A plots the tax cost of giving by state and year. The effect of the TRA86 on the tax cost of a cash donation is apparent; no other federal tax reform over the same period comes close to matching its magnitude. Figure 2.2B charts the log difference from year to year by state and demonstrates that the size of this change varied a lot *across* states. The state-level change in the log cost of a contribution from 1986 to 1988 ranged from 14 percent to 22 percent, with a median change of 18 percent. Again, the changes brought about by the TRA86 dwarf any federal or state tax change before or since. There is substantial interstate variation in the year-over-year change in the cost of giving following the TRA86, but only small, isolated changes among the states before and after; the shock of the TRA86 explains most of the change in the state-level tax cost of giving during the time period.

Not only is the change large, but the tax cost before the TRA86 does not predict the state-level change from 1986 to 1988. Figure 2.3 plots marginal tax cost of a contribution in 1986 against the change in tax cost from 1986 to 1988. Each point is one state marked by its postal abbreviation, except for the point labeled "NT" in the upper right region of the scatterplot, which marks the seven states with no state income tax.[8] A linear regression

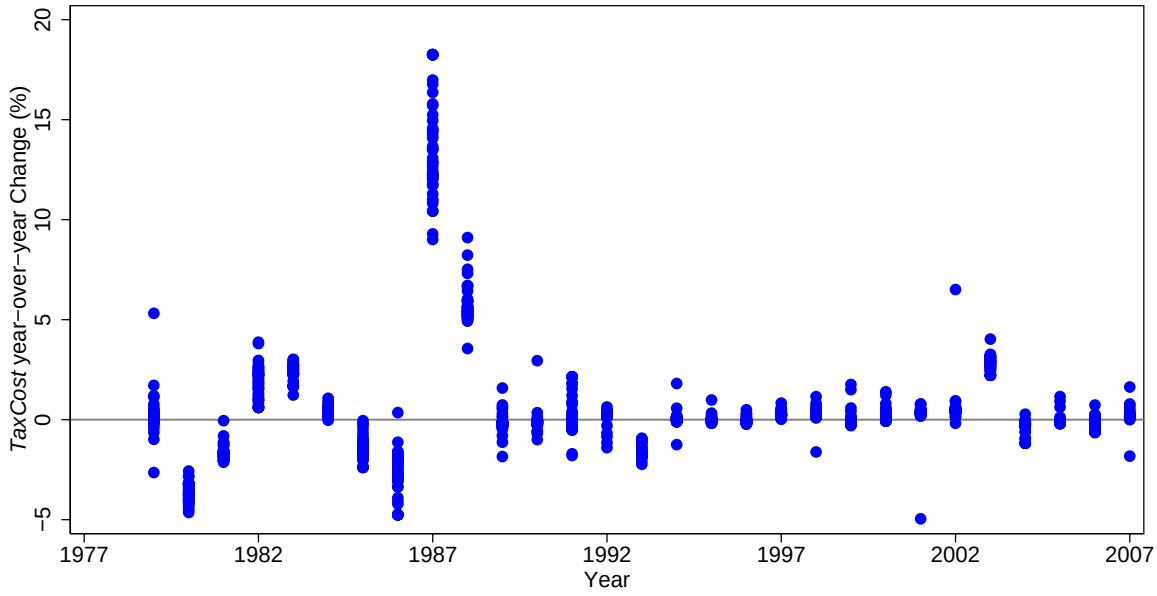
[8]Two states are omitted from this and other figures. West Virginia repeals its state charitable contribution in 1987, and is omitted from every analysis in this paper because of endogeneity concerns. North Dakota has incorrectly high state marginal rates in and is dropped.

Figure 2.2: Tax Cost of Giving by State

A. Tax Price by State and Year



B. Change in Tax Price by State and Year



Notes: Each dot in panel (a) represents the marginal tax cost of giving, averaged over a fixed sample of returns, in one state in one year. Each dot in panel (b) represents the year-over-year percentage change in tax cost. Tax cost of giving is calculated using the NBER TAXSIM calculator for a nationally representative cross-section of 1984 tax returns and weighted by reported contributions. See appendix A for details of the calculation.
Source: Internal Revenue Service, Individual Public Use Tax File.

through this scatterplot yields only a weak relationship between change in average tax cost and the 1986 level:

$$\begin{aligned}\Delta_{`86-`88} \ln(TaxCost_p) = \quad & 0.2115 \quad && +0.0828 * \ln(TaxCost_{p,`86}) + \varepsilon_p \\ & (0.0495) && (0.1247)\end{aligned}$$

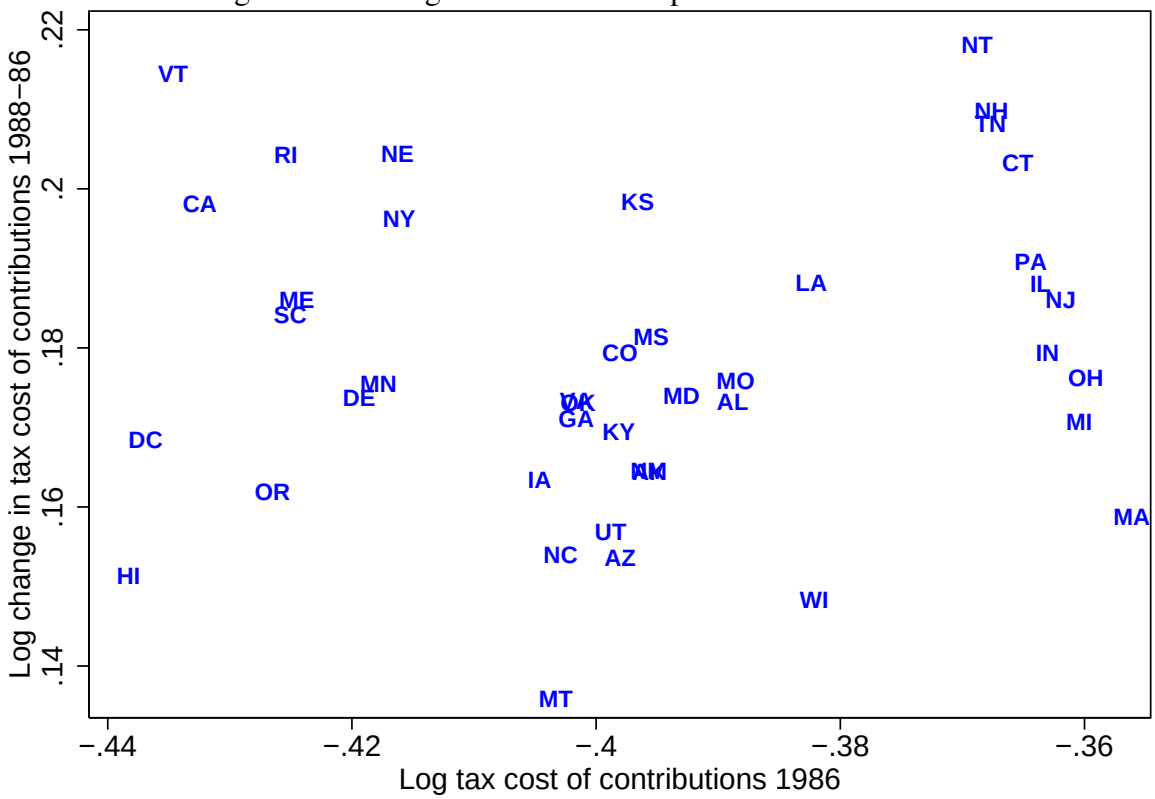
Where the log of the 1986 federal and state tax cost of giving in a state with policy p is denoted $TaxCost_{p,`86}$, and the change in the tax cost from 1986 to 1988 is denoted $\Delta_{`86-`88} \ln(TaxCost_p)$. Regression coefficients are reported directly in the estimated equation, with standard errors in parentheses below. This weak fit is consistent with the explanation that state marginal rates do not monotonically drive the differences in average tax costs following the reform.

It may come as a surprise that the proportional change in the tax cost of contributions is not correlated with the level before 1986. The magnitude of the change is driven not by rates, but by complex interactions between state income tax laws and the changes to the federal code made by the TRA86. These interactions are a function of choices made by state legislators before the federal reform, and the resulting changes in state tax rates are an accidental byproduct of the federal reform. I will provide three examples, though these should not be taken as the only ways in which the TRA86 had differential effects across the states.

First, fourteen states allowed taxpayers to deduct their federal tax liability from their state taxable income. This means that a reduction in federal tax liability increased state taxable income and — to the extent that this increase moved taxpayers into higher-rate tax-brackets at the state level — also increased state marginal rates. In these states, the overall change in the cost of giving was dampened by the state response.[9]

[9]These states are Alabama, Arizona, Colorado, Delaware, Iowa, Kansas, Kentucky, Louisiana, Minnesota, Missouri, North Dakota, Oklahoma, Oregon, and Utah (ACIR 1986, table 54).

Figure 2.3: Change in Tax Price vs. pre-TRA86 Tax Price



Notes: Each point represents total change in a measure of average tax cost of giving for one state income tax policy. The point labeled "NT" represents states with no state income tax. All other points are labeled using state postal abbreviations. The horizontal axis plots the log average cost of giving in 1986 ($\ln(1-\tau)$, where τ is a representative marginal tax rate). The vertical axis plots change in log average cost of giving from 1986 to 1988 ($\ln(1-\tau_{1988}) - \ln(1-\tau_{1986})$) . See appendix A for a precise description of the tax cost variable's calculation.
Source: Internal Revenue Service, Individual Public Use Tax File.

Second, the states varied in the links made between their state systems and federal tax definitions. In the extreme case, four states used "piggyback" tax systems where state tax liability was a function of federal tax liability, meaning that when the federal government reduced its marginal rates, those states' marginal rates fell proportionally, amplifying the total change. Four states used the federal definition of taxable income (without a direct "piggyback" system), which meant that the reductions made by the TRA86 to credits, deductions and exemptions increased state taxable income as well, dampening the federal

change. Nineteen states and the District of Columbia used the federal definition of adjusted gross income and most federal deductions, and seven states used the federal definition of adjusted gross income only, which meant that some federal changes but not others passed through to the state level. Six states had no federal starting point in their state income tax laws.[10]

Third, states were affected to different degrees by the Alternative Minimum Tax (AMT). The AMT is a parallel tax system designed to prevent high-income households from paying "too little" tax through the legitimate use of certain deductions. Affected taxpayers have to calculate their federal tax liability under not just the normal rules, but under the AMT rules, and pay whichever is greater. By reducing the availability of itemized deductions, the TRA86 greatly reduced the number of AMT-eligible returns. In 1986, AMT taxpayers made up 1.3 percent of all federal returns, and 47.9 percent of returns reporting over $1 million dollars adjusted gross income. In 1987, these shares plunged to only 0.3 percent of all returns and just 6.6 percent of returns on over $1 million (Internal Revenue Service 1987). Therefore, many high-income households who paid the AMT in 1986 saw marginal rates leap from the pre-1986 20 percent AMT rate to 28 percent or more, reducing rather than increasing their marginal tax cost of a charitable contribution. Because some state and local tax payments can be taken as itemized deductions, high-income households in states where the burden of these taxes was higher were more likely to have a greater share of AMT taxpayers, dampening the effect of the federal change.

Because its reforms spilled over into state tax incentives in a material and unexpected

[10]See ACIR 1986, table 52. In 1986, the "piggyback" states are Nebraska, North Dakota, Rhode Island and Vermont. The states using federal taxable income without a piggyback system are South Carolina, Idaho, Utah and Oregon. The states using federal AGI and most deductions are Maine, Delaware, Maryland, New York, Iowa, Kansas, Minnesota, Missouri, Georgia, Kentucky, Louisiana, Virginia, West Virginia, New Mexico, Oklahoma, Colorado, Montana, California, and Hawaii. The states using federal AGI only are Massachusetts, Illinois, Indiana, Michigan, Ohio, Wisconsin and Arizona. The states with no federal starting point are New Jersey, Pennsylvania, Alabama, Arkansas, Mississippi, and North Carolina. Connecticut, New Hampshire and Tennessee only tax capital income. Alaska, Florida, Nevada, South Dakota, Texas, Washington, and Wyoming have no state income tax.

way, the TRA86 is a valuable opportunity to examine an exogenous shock to tax incentives. I will use data on charities' reported contribution income together with this exogenous change in tax price to estimate the effects of this incentive on charitable giving.

Associations Between Tax Cost Changes and Charitable Receipts

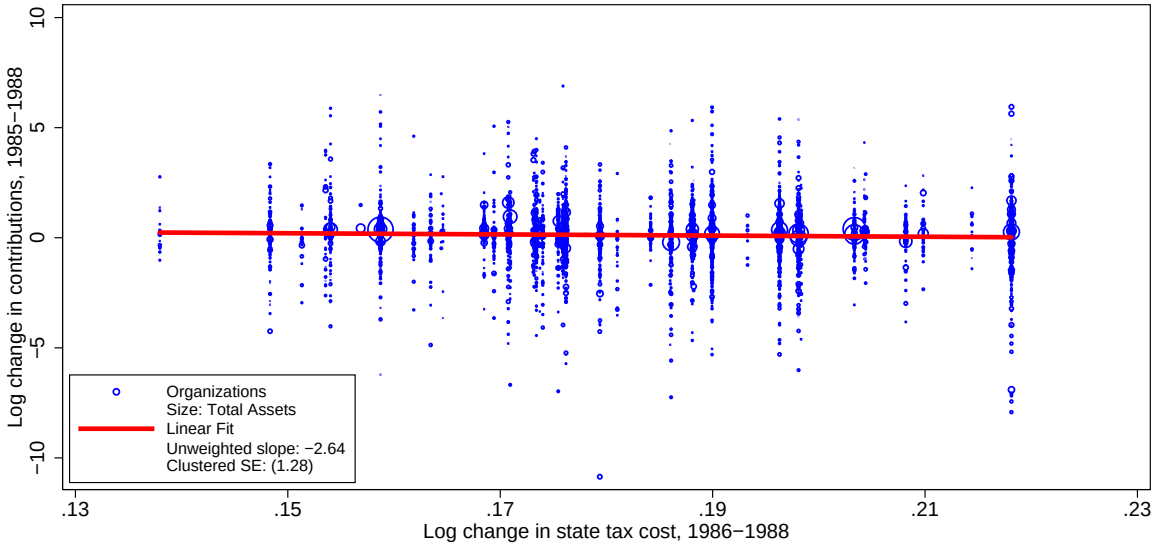
I compile a panel of charities' financial data from full-length IRS forms 990 collected in the Internal Revenue Service's Statistics of Income Data and cleaned and documented by the National Center for Charitable Statistics. The form 990 is a summary of income statements, balance sheets, and other data of interest many charities must file with the IRS each year. The 990 has been a public record since 1950, and the IRS Statistics of Income Division (SOI) has compiled machine-readable data files for a sample of 990s in 1982, 1983, and 1985 to the present. These data files are designed to be stratified cross-sections within a year. They sample all organizations with over $10 million in assets and subsets of smaller organizations. The SOI data also try to follow the same organizations each year — making it feasible to use SOI 990 data files to construct a panel of nonprofit organizations oversampling large organizations.[11] My measure of charitable contributions is "direct public support," the sum of all contributions from taxable entities directly to the organization, which is overwhelmingly composed of individual donations.

My analysis begins by examining the relationship between reported contributions and tax cost in the raw data. Figure 2.4A plots the log change in tax cost from 1985 to 1988 against the 1985 to 1988 change in contributions for individual nonprofits filing in that state. A linear fit through the plot finds that a one percent increase in tax cost is associated with a 2.6 percent decrease in contributions; this slope estimate is statistically different

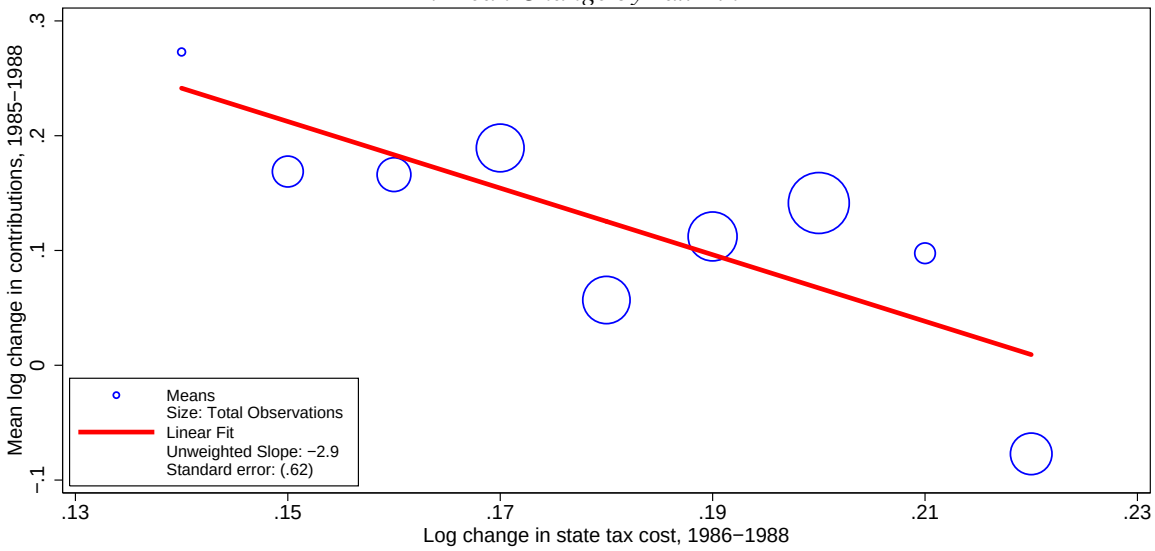
[11]The SOI provides cross-sectional but not sample weights, and the procedure for carrying over some organizations but not others from year to year is not documented; additionally, as described in section 2.5, organizations that ought to be observed every year are sometimes missing without explanation. For these reasons, all regressions in this paper are unweighted.

Figure 2.4: 1985-8 Change in Contributions vs. Change in Tax Price

A. Individual Public Charities



B. Mean Change by Tax Bin



Notes: Figure 2.4A plots the log change direct public support for individual public charities from 1985 to 1988 (on the vertical axis) against the log change in average tax cost of a charitable contribution from 1986 to 1988 (horizontal axis). Blue markers represent individual charities and are scaled by gross assets at the end of fiscal 1988. A red line marks the unweighted linear fit through the plotted charities. Figure 2.4B aggregates the data from panel 2.4A for easier display. Blue circles represent the unweighted mean of log tax changes for all the organizations in states with log tax changes closest to even hundredths. (That is, bins are 0.14 ± 0.05, 0.15 ± 0.05, ..., 0.22 ± 0.05.) Blue marker size represents total observations by tax bin.
Sources: Direct public support data are taken from the 1985 and 1988 Statistics of Income Form 990 data set; all charities reporting strictly positive contributions in both years and filing in states other than North Dakota and West Virginia are plotted. State tax change data are described in notes to figure 2.3 and in appendix A.

from zero at the five percent level (state-clustered standard error = 1.27 on a coefficient of -2.6).[12]

However, the most salient feature of this plot is not the negative slope of the linear fit, but the variance of the changes in contributions. Several organizations report huge swings in contributions across years: 9.7 percent of organizations report log changes from 1985 to 1988 greater than 2 or less than -2. One Colorado organization at the bottom of the chart reports a log change in contributions of -11, receiving $6,260,000 in 1985 but just $1,210 in 1988. It seems like an astonishing plunge until one sees that this is the U.S. Olympic Foundation, which was still riding high on the 1984 summer games in Los Angeles.

Figure 2.4B accounts for this by averaging changes in contributions within bins by the state log tax change by hundredths (0.14 ± 0.05, 0.15 ± 0.05, ..., 0.22 ± 0.05). The relationship between the tax rate change and contributions becomes more visibly negative. Our next question is whether these differences are *caused* by the tax change, or just associated with it. The following section develops a difference-in-differences strategy to isolate the causal effects of tax cost shifts.

2.3 Estimates of the Tax Elasticity of Charitable Revenues

Section 2.2 demonstrated that the TRA86 shifted the tax cost of charitable contributions differentially across US states, and that this interstate variation is associated with changes in organizations' donation receipts. Next, I will refine this analysis using a difference-in-differences strategy to determine whether the tax change *caused* the differences in charitable receipts.

Changes in tax policy may be correlated with other conditions that affect charitable

[12] If a linear fit is estimated for between this tax change and three-year contribution growth for 1982 to 1985, the slope is positive and statistically insignificant; estimates and similar scatterplots are available upon request.

giving. Therefore, it is important to examine unintended changes in tax rates to isolate a causal effect. I focus on the change in the combined tax cost of giving from 1986 to 1988 in each state. The large increase in average tax cost in all states — clearly visible in figure 2.2A — is a federal change that affects all organizations equally. But the state-level differences in this measure are particular to that state. Because these are caused by the complex federal tax changes unlikely to have been anticipated by state legislators, the differences in state average tax cost increases from 1986 to 1988 are a plausibly exogenous byproduct of the federal law, not an endogenous policy choice.

My treatment variable is the change in average tax cost of giving by state from 1986 to 1988. I will compare the changes in charitable receipts received by local organizations in the states with smaller tax cost increases, before and after the tax reform, with the charitable receipts received by organizations in the states with the larger tax cost increases, before and after. There are several reasons to think we should see charitable contributions change from 1985 to 1988, such as the federal tax change itself and shifts in economic growth and inflation expectations over that time. However, these national changes should effect all organizations. The gap between the change in this period for high-tax-cost-increase states and low-tax-cost-increase states — the difference in the differences — is therefore plausibly caused by the different tax shifts across states.

It is unlikely that state legislatures could have anticipated many of the changes of the TRA86, or that they would have adapted their tax policy for charitable contributions beforehand. Nor did state legislatures move swiftly to capture money left "on the table" by the federal government. Table 2.1 lists the states that changed their state deduction for charitable contributions or changed marginal tax rates during years 1986 to 1988. If anything, the states moved to reduce their own marginal rates as part of a broader movement of rate-reducing tax reform. Only one state, West Virginia, changes its charitable contribution

deduction during this period (and is therefore dropped from the sample).[13]

Table 2.1: Major State Individual Income Tax Changes Legislated 1986–1988

	1986	1987	1988
Eliminated Deduction for Contributions		WV*	
Reduced Marginal Tax Rates	DE, MI, PA, VT	CA, IA, NY, WI	OK, UT, VT
Increased Marginal Tax Rates	NM, UT	ID, IN, MT, ND†	AZ

Notes: (*) W. Virginia is dropped from all regressions because of this policy change. (†) North Dakota has implausibly high marginal rates in the TAXSIM system until 1987 and is dropped; see discussion in footnote 14.
Sources: ACIR 1987–89, table 49; Feenberg and Coutts (1993).

The 1986 tax reform explains a huge share of variation in state-level tax cost of giving from 1977 to 2007. If I regress the average tax cost by state and year *only* on the change in tax cost after 1986:

$$\ln(TaxCost_{st}) = \alpha + \beta\left[\ln(TaxCost_{s,`88}) - \ln(TaxCost_{s,`86})\right] * Post86_t + \varepsilon_{st}$$

I obtain an R^2 of 0.79. This high explanatory power is consistent with the relatively small and clustered year-over-year changes in average tax cost in other years shown in figure 2.2B.

In short, the TRA86 not only changed federal marginal tax rates significantly, but made many other substantial changes to the federal tax system. Because the US states could not have anticipated the specifics of this reform, and because the states varied in the extent and manner of the links between state income tax systems and the federal tax code, the TRA86 created plausibly exogenous variation in the changes in the tax cost of charitable

[13] In the TAXSIM system, there are five total changes to the deduction policies of states with income taxes in the 1982-2007 period. In addition to West Virginia's repeal of its deduction in 1987, Louisiana repeals theirs in 2003 and then restores it in 2007, and Massachusetts creates one in 2001 but repeals it in 2002.

giving across states. It is this variation I will use to identify the effect of tax incentives on charitable giving.

Sample Selection

My research design exploits variation across states in donors' tax cost of giving. However, because I am examining donors' responses as reported by the recipient organization, I do not directly observe donors' state of residence. I therefore retain an estimation sample only of organizations which plausibly receive almost all of their donations from donors in their state of filing, using multiple filters to exclude charities which might have geographically dispersed donor bases.

I start with the full IRS public charity data, which includes 296,318 observations on 31,779 different organizations in years 1982, 1983, and 1985-2007, altogether accounting for $1,388 billion in direct contributions over the period (in 2012 dollars). I then refine the sample by taking the following steps:

1. Discard all observations except for years 1982, 1983, 1985 and 1988–1990; only keep observations on organizations observed both before and after the 1986 reform. (Remaining: 24,561 Obs, 4,673 Orgs, $157.4 billion direct contributions.)

2. Discard organizations located in West Virginia (which repeals its contribution deduction in 1987) or North Dakota (which reports incorrect marginal tax rates in TAXSIM in 1986).[14] Remaining: 24,326 Obs, 4,632 Orgs, $157.0 billion direct contributions.

[14]For 22.0 percent of sampled returns, TAXSIM computes a state marginal tax rate in North Dakota greater than 50 percent in 1986. This is true for less than 0.0007 percent of observed returns in the other 49 states and DC, and because the same calculations are not observed after the TRA86, these high rates lead to a very large calculated change in North Dakota's cost of giving from 1986 to 1988. In 1986, North Dakota taxpayers could choose between a progressive rate schedule with a top marginal rate of nine percent, or a "piggyback" payment equal to 10.5 percent of federal income tax (ACIR 1987, table 51). Because the top federal rate in 1986 was 50 percent, high earners should not have faced North Dakota marginal rates greater than 5.25 percent (0.105 * 0.50), and certainly nobody should have been subject to marginal rates over nine percent. I do not yet know the reason for these high calculated marginal rates in North Dakota.

3. Discard organizations meeting any of several criteria suggesting they might have donors outside their filing state.

   (a) Organizations that change filing state at any time across all years in the IRS Statistics of Income (SOI) or Core data files files are presumed to provide non-local goods and are dropped. Remaining: 22,927 Obs, 4,356 Org, $140.4 billion direct contributions.

   (b) Organizations which ever file a "group return" on behalf of a network of affiliated organizations are presumed to have branches in other states and are dropped. Remaining: 22,834 Obs, 4,356 Orgs, $138.6 billion direct contributions.

   (c) Organizations whose names or form 990 mission statements match key words implying a non-local orientation (*e.g.* "national" or "global") are dropped; see details in appendix A. Remaining: 20,102 Obs, 3862 Orgs, $106.6 billion direct contributions.

   (d) If an organization is ever among the 25 largest organizations by assets within its major sector (as classified by the National Taxonomy of Exempt Entities), I assume that it is nationally prominent and omit it from the sample. Remaining: 19,120 Obs, 3,684 Orgs, $74.5 billion direct contributions.

4. Finally, since my dependent variable is in logs, I omit organizations that ever report zero direct contributions in the observation period. As I show in section 2.5, the overwhelming majority of charities either always receive contributions, or never do. I omit the few that vary year to year so their patterns of occasional gifts do not introduce observation error. The final sample contains 16,882 observations on 3,273 organizations, comprising $72.1 billion direct contributions in 2012 dollars.

Section 2.5 uses a series of robustness checks to demonstrate that results are not driven

by this sample selection procedure, and obtains results consistent with my preferred sample and empirical approach.

Tax Shock as a Variable Treatment

Given an ideal data set, I would use interstate variation in the effect of the TRA86 on representative donors as an instrument for changes in a charity's donors' tax incentives,

$$\begin{aligned}\ln(\textit{Contributions}_{it}) =& \beta_0^i + \beta_1 \ln(\textit{TaxCost}_{it}) + \mathbf{Controls}'_{st}\boldsymbol{\beta_2} + u_{it} \\ \ln(\textit{TaxCost}_{it}) =& \delta_0^i + \delta_1 \Delta_{86-88} \ln(\textit{TaxCost}_{s(i)}) * \textit{Post86}_t + \mathbf{Controls}'_{st}\boldsymbol{\delta_2} + r_{it} \qquad (2.1)\end{aligned}$$

where $\textit{Contributions}_{it}$ is real direct contributions reported by organization i in year t, and $\textit{TaxCost}_{it}$ is the tax cost $(1 - \tau)$ in year t for the average of organization i's contributors. The instrument for donors' tax cost of a contribution is $\Delta_{86-88} \ln(\textit{TaxCost}_{s(i)})$, the change in the log mean tax price of giving $(1-\tau)$ from 1986 to 1988 in state s, interacted with *Post86*, a dummy equal to 0 before 1986 and 1 after 1986. Organization fixed effects β_0^i and δ_0^i capture time-invariant features of each organization including initial tax cost for donors. **Controls** is a vector of any other control variables or fixed effects, and u and r are residuals.

This ideal estimation is impossible because the tax rate of each charity's donor base, $\textit{TaxCost}_{it}$ is unobserved. Instead, I will estimate the reduced form of this IV system, plugging in treatment effect $\Delta_{86-88} \ln(\textit{TaxCost}_{s(i)}) * \textit{Post86}$ directly as a regressor. Using the data described in the preceding section, I estimate continuous difference-in-differences effects on contributions using the reduced form of this ideal.

$$\ln(\textit{Contributions}_{it}) = \alpha_i + \beta \Delta_{86-88} \ln(\textit{TaxCost}_{s(i)}) * \textit{Post86}_t + \delta_t + \mathbf{X}'_{\mathbf{st}}\boldsymbol{\gamma} + \varepsilon_{it} \qquad (2.2)$$

where $Contributions_{it}$, $Post86$ and $\Delta_{86-88}\ \ln(TaxCost_{s(i)})$ are as defined above; δ_t is a year effect; and α_i is an organization fixed effect. The coefficient of interest is β, which captures the difference in contributions between states with different changes in tax price following the TRA86. Since both the dependent and treatment variables are in logs, we can directly interpret β as an elasticity of contributions with respect to the average tax cost.[15]

Alternative specifications control for differential economic trends by including region-by-year effects $\delta_{r,t}$ that capture unobservable variation across time among the four Census regions, or a row vector of state-level macroeconomic indicators $\mathbf{X'_{st}}$ to capture changes in the local economic environment over time. Regional patterns are also shown in figure 2.5, which maps tax cost changes by state. State-year macroeconomic variables include state population, real gross state product, real per capita income, unemployment rate, and poverty rate, all measured in logs.

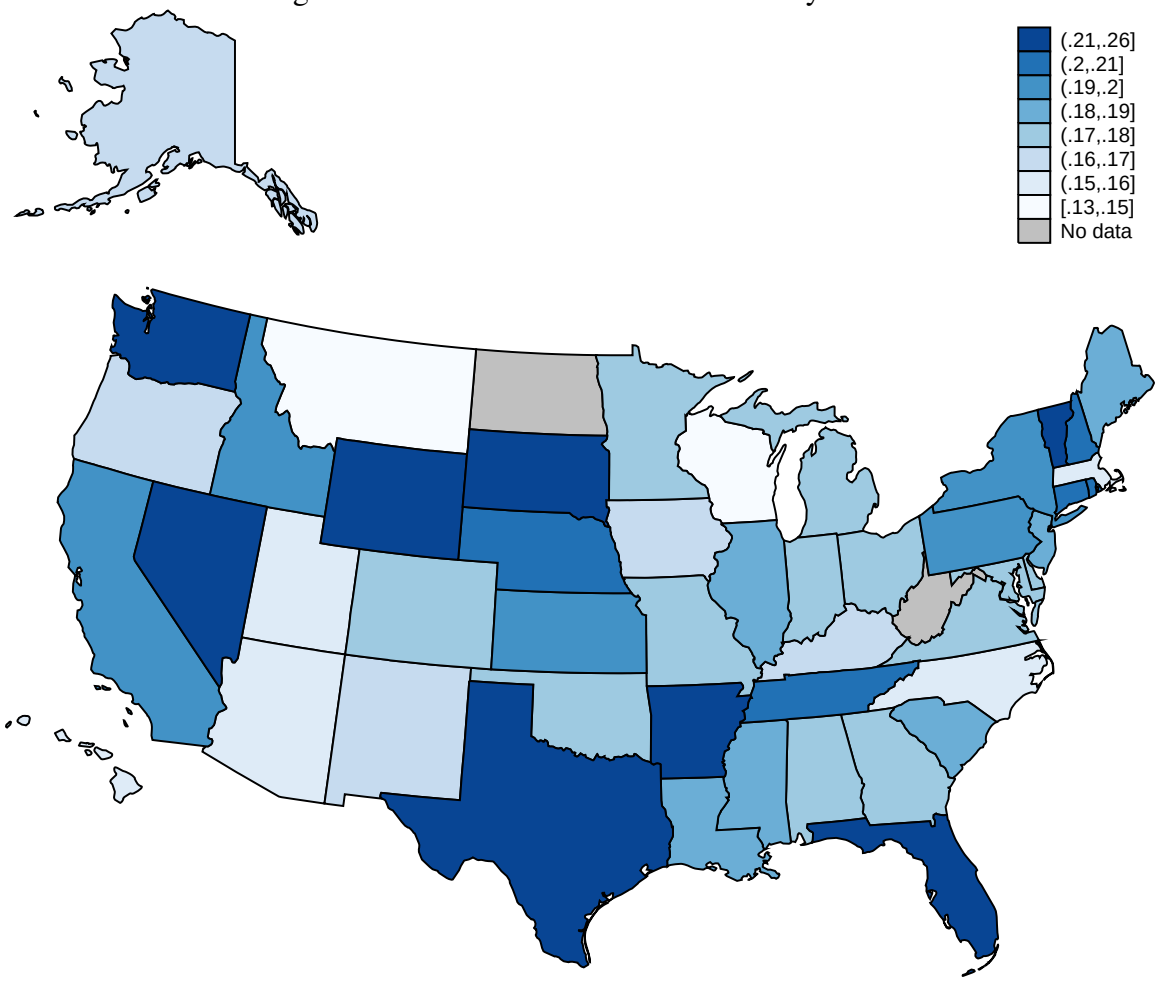
Estimates for this regression are reported in table 2.2. I obtain elasticities ranging from -3.5 to -5. Column 1 reports the basic regression, which finds an elasticity of contributions of -4.5 with respect to the average tax cost of giving. I obtain similar elasticities using region-by-year effects (-5.0), macroeconomic controls (-4.0) or both (-3.5). Standard errors are reported in parentheses and clustered by state. All estimates are statistically different from zero at the five percent level.

These estimates of the tax cost elasticity of charitable receipts for organizations stand in

[15]This approach makes some assumptions about the relationship between the treatment variable and contributions. First, it assumes that the effect of a tax change is log-linear. A more general specification dividing states into "treatment" and "control" groups by whether their tax change was above or below the median is presented in appendix B, and yields similar results.

Second, by interpreting β directly as an elasticity of charitable contributions, this approach assumes that β is a good estimate of the true parameter of interest, β_1 — that is, that the parameter δ_1 in the ideal regression equation 2.1 is approximately one. Because this measure is constructed as a dollar-weighted average of donations, and because the TRA86 drives almost all variation in the state-year level tax measure, this parameter is close to one so long as the composition of donors to sample organizations is close to the composition of all donors. However, to the extent that the composition of donors in the sample is systematically different from the tax returns used to calculate $TaxCost_{st}$, β may be scaled incorrectly; this is discussed further in section 2.6.

Figure 2.5: Tax Cost Treatment Variable by State



Note: Darker states had greater increase in the log average cost of a charitable contribution from 1986 to 1988.
Source: See appendix A for details of the calculation of the average tax cost variable.

striking contrast to the tax elasticities of individuals estimated in the literature. As described in section 2.1, individuals' tax cost elasticity has usually been estimated to be between 0 and -3. In the following sections, I will show that these large estimates are not a fluke. Section 2.4 checks for differential pretrends, a common threat to difference-in-differences strategies, and for transitory effects driven by intertemporal shifting. Section 2.5 checks for threats to identification from other sources, specifically the effects of specific sample

Table 2.2: Difference-in-Differences Estimates of Effect of Tax Incentives on Charities' Received Contributions

	(1)	(2)	(3)	(4)
	Log Direct Contributions			
$\Delta_{86-88} TaxCost_{s(i)} * Post86_t$	-4.450**	-5.016***	-3.990***	-3.503***
	(1.723)	(1.420)	(1.185)	(1.140)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	16882	16882	16882	16882
R-squared	0.861	0.862	0.862	0.862
Number of Orgs	3273	3273	3273	3273

Notes: Dependent variable is log of real direct public support from a panel of IRS form 990 data for 1982–3, 1985, and 1988–90. $\Delta_{86-88} TaxCost$ is the change from 1986 to 1988 in the first-dollar marginal tax cost of a charitable contribution in state s, averaged over a fixed set of individual income tax returns. *Post86* is equal to 1 after 1986 and zero before. "Macro Controls" are a set of macroeconomic variables observed in each state and year: log gross state product, log state population, log unemployment rate, log poverty rate, and log per capita income. See the discussion in section 2.3 for more detail on the construction of these variables. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: Form 990 data taken from the IRS Statistics of Income Division Exempt Organizations Sample, as cleaned and documented by the National Center for Charitable Statistics (Internal Revenue Service 2011b). Marginal tax cost of giving for the representative taxpayer is caluclated using the IRS Individual Public Use Tax File . Macroeconomic variables from Federal Reserve Economic Data (FRED) include population [series code POPx], unemployment rate [URx], and per capita income [PCPIx], where x is the two-letter postal abbreviation of each US state. Gross state product data are from the U.S. Bureau of Economic Analysis [bea.gov]. State-by-year poverty rates are aggregated from March Current Population Survey microdata, as maintained by the Integrated Public Use Microdata Series (King et al. 2010).

selection choices, extensive margin outcomes in receiving or not receiving contributions, organization entry and exit differentials across the states, and influential outliers. In section 2.6, I will argue that the difference between elasticities measured using individual and charity data is a result of looking at receipts rather than donations, which draws on data sets which differ in crucial ways. Because donors and charities are heterogeneous, these questions can have different answers.

2.4 Persistence and Pretrends

The causal effects of a tax cost increase on contribution receipts estimated in section 2.3 suggest that a one percent increase in average tax cost of giving reduces contribution receipts by about four percent. This result is substantially larger than the effects usually estimated using individual donor data. We may therefore fear that an unobserved phenomenon correlated with the change in state average tax cost leads the effects to be overstated.

One possible explanation is that differential pretrends drive the results. If high-tax-cost-increase states happened to have a population of nonprofit organizations experiencing slower growth in contributions than low-tax-cost-increase states before the tax reform, then the difference-in-differences estimates would return a larger estimate of the effect that was actually driven by this pretrend — a common threat to identification with difference-in-differences estimation strategies.

A second concern is that the estimated effect captures intertemporal shifting of contributions, so households can take full advantage of tax incentives. Estimates of the *permanent* elasticity of charitable donations from individual data are often significantly lower because households "bring forward" gifts they plan to make anyway to the year preceding a tax rate cut (Randolph 1995). In section 2.3, I tried to account for this by dropping 1986 and 1987 from the analysis; however, if households brought forward contributions to 1986 from more than one year out, it may be that the differential decline in contributions seen in 1988-1990 is really just a difference in how aggressively donors brought forward several years of planned giving.

I will test for both of these conjectured problems simultaneously by extending the data sample to later years and allowing the estimated effect of the tax change variable to vary by year. I retain the sample described in section 2.3, but also add any observations with positive direct contributions in years 1986, 1987, and 1991-2007 to the panel. I then estimate

an expanded version of equation 2.2

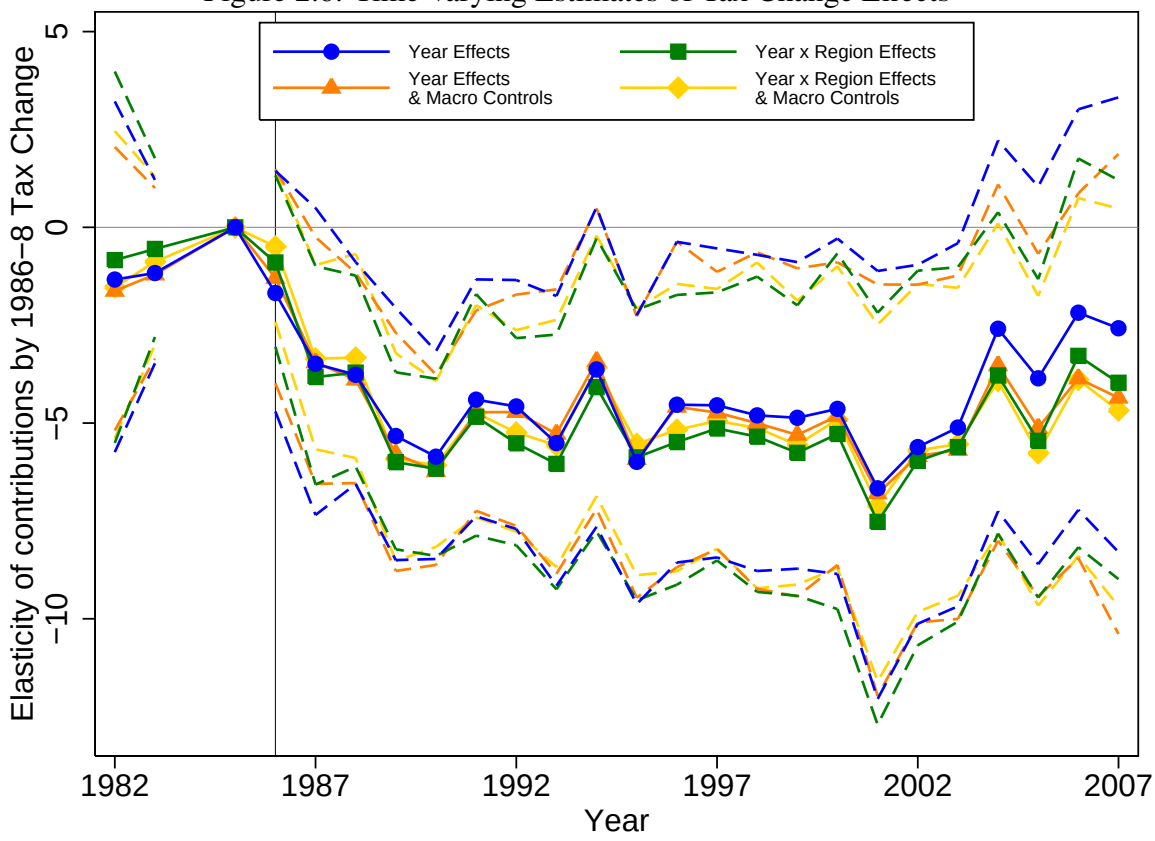
$$Contributions_{it} = \alpha_i + \delta_t + \mathbf{X}'_{\mathbf{st}}\boldsymbol{\gamma} + \sum_{t \in 1982,`83,`86,`87,...,2007} \beta_t(\Delta_{86-88} TaxCost_{s(i)}) * \mathbf{1}\{year = t\} + \varepsilon_{it} \qquad (2.3)$$

The key difference from equation 2.2 is the flexible specification of TRA86 treatment effects, in practice a different effect for the tax cost change β_t in all years. The path over time described by these coefficients shows the size of the gap by treatment dosage each year, relative to the gap in comparison year 1985. Because the treatment does not actually occur until 1986, we expect β_{1982} and β_{1983} to be equal to zero. If instead we observe $\beta_{1982} > \beta_{1983} > 0$, it might mean that the difference-in-differences estimates are describing the continuation of a preexisting trend in contributions. And if the β_t's rapidly revert to zero after the policy change, despite the permanent change in tax cost of giving shown in figure 2.2A, that would be consistent with the estimates describing a short-term shifting of intended contributions, rather than a permanent effect of the policy change on contributions.

Figure 2.6 charts the point estimates of β_t by year, with dashed lines marking pointwise 95 percent confidence intervals for test of $\beta_t = 0$ (that is, statistically indistinguishable from 1985).[16] The implications of these estimates for the two concerns raised above are clear. First, the estimates for 1982 and 1983 are statistically indistinguishable from zero and are, if anything, *increasing* over 1982-5. There is no evidence of a 1982-5 pretrend actually driving the difference-in-differences estimates. Second, the effect of the tax cost change is not only persistent, but if anything the gap between high-increase and low-increase states *expands* over the following 10-15 years. This is consistent with an effect that is not driven by tax-shifting but a permanent fall in the contributions to these organizations. Specifically, it suggests that donors respond with a lag — the instantaneous shift is less than the long-run effect of the policy.

[16] I also tabulate the coefficients in appendix table B.2.

Figure 2.6: Time-varying Estimates of Tax Change Effects



Notes: Y-axis is the coefficient on the 1986–1988 change in log average tax cost by state for year t. Comparison (omitted) year is 1985. Dashed lines plot pointwise 95% confidence intervals using state-clustered standard errors. See discussion of regression equation 2.3 in section 2.4 for more detail.
Sources: See notes to table 2.2.

Overall, these results confirm the validity of the difference-in-differences strategy. Two main threats to my estimates, differential pretrends and intertemporal shifting, are not indicated by the long-run effects or year-specific coefficients estimated. The results also describe an effect that is durable and persists for years.

2.5 Robustness Checks

This section will investigate other possible explanations of the large estimated effects, including robustness to the sample selection decisions described in section 2.3, the possibility that organizations enter and exit differentially across states, and that the estimates are

driven by outliers. None of these checks prompt a reinterpretation of the estimates obtained in prior sections; throughout, I find a robust, negative relationship between average tax cost of giving and charitable receipts.

Extensive Margin of Contributions

My sample selection process discards organizations with zero direct contributions in any of the years of interest. For the full 1982-2007 sample over 78 percent of organizations either *always* receive direct contributions, or *never* do. It appears there is a fundamental difference between organizations that do and do not finance their operations with contribution revenues. However, focusing on organizations that always receive contributions limits our ability to observe important behavior at the extensive margin, reflecting an endogenous decision to start or stop soliciting donations; perhaps changes in contribution receipts would look different if we accounted for organizations deciding endogenously to fire their fundraising staff after the TRA86.

A descriptive multivariate regression allows me to test this claim. Let $ReceivedCont_{it}$ be a binary variable equal to 0 if organization i received zero direct contributions in year t, and equal to 1 if it received strictly positive contributions. I estimate the linear probability model

$$ReceivedCont_{it} = \alpha_i + \mathbf{X}'_{\mathbf{it}}\boldsymbol{\gamma} + \beta \ln(TaxCost_{s(i),t}) + \varepsilon_{it} \tag{2.4}$$

for the entire Statistics of Income sample (to year 2007), where $\mathbf{X}'_{\mathbf{it}}$ is a set of firm financial variables and $TaxCost_{it}$ is the log average tax cost of a contribution. The estimates are reported in table 2.3, column 1. Although revenue from other sources is correlated with contribution revenue, the partial correlation on *TaxCost* is close to zero and not statistically significant.

To confirm that the problem is not at the margin of my sample, I repeat this analysis

Table 2.3: Probability of Positive Contributions (Extensive Margin)

	Pr(Receiving Contributions)	
	(1)	(2)
	All Observations	Main Sample
Log Assets	0.00150	0.00238
	(0.00131)	(0.00637)
Log Gov. Grants	0.00506***	0.00752**
	(0.00133)	(0.00341)
Log Program	0.00617***	0.00164
Service Revenue	(0.00148)	(0.00365)
$TaxCost_{st}$	-0.0745	
	(0.0888)	
$\Delta_{86-88} TaxCost_{s(i)}$		0.366
$*Post86_t$		(0.349)
Org. & Year Effects	✓	✓
Observations	296161	21314
R-squared	0.729	0.512
Number of Orgs.	31772	4125

Notes: Dependent variable is equal to 1 if a charity receives at least one dollar in direct support in year t, zero otherwise. *TaxCost* is the marginal first-dollar tax cost of a charitable contribution, averaged over a fixed set of individual tax returns, in state s and year t. Log of assets is observed at the beginning of the year. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

using data from years 1982, 1983, 1985 and 1988-90 for organizations which meet all other criteria to be included in the main sample, and instead of tax cost I use the continuous treatment variable, $\Delta_{86-88} \ln(TaxCost_{s(i)}) * Post86_t$. These estimates are reported in table 2.3, column 2, and again show no statistically significant relationship between the tax cost measure and the probability of strictly positive contributions.[17]

As an additional check, table 2.4 reports regressions identical to those in table 2.2, column 2, except uses as dependent variable the log of direct contributions plus a constant (since log of zero is undefined). My preferred additive constant is $25,000, which is the minimum income requiring an organization to file a form 990, though columns 1 and 3

[17] Use of a logit instead of a linear probability model does not change either set of results.

report results for an addition of $10,000 and $50,000 as well.[18] Column 4 of the table repeats the regression with the added $25,000, but only for the same sample as in table 2.2. Adding the additional data and changing the dependent variable reduces the magnitude of the estimates somewhat, but qualitatively the result — a large, negative relationship between average tax cost and contribution revenue — is unchanged.

Table 2.4: Difference-in-Differences Estimates (With Reported Zeroes)

	(1)	(2)	(3)	(4)
	Log Real Contributions +$10,000	Log Real Contributions +$25,000	Log Real Contributions +$50,000	Log Real Contributions +$25,000
	Including Zero Contributions			Main Sample
$\Delta_{86-88} TaxCost_{s(i)} * Post86_t$	-3.160*	-3.052**	-2.870**	-3.794***
	(1.664)	(1.431)	(1.262)	(1.385)
Org. Effects	✓	✓	✓	✓
Year Effects	✓	✓	✓	✓
Observations	21318	21318	21318	16882
R-squared	0.835	0.851	0.862	0.890
Number of Orgs	4125	4125	4125	3273

Notes: Dependent variable is log of real charitable contributions plus a constant, in 2012 dollars. Independent variables are described in notes to table 2.2. The sample includes organizations reporting zero direct support in some years. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

In summary, it is uncommon for charities to accept contributions in some years but not in others. Upon further examination, neither the extensive margin of receiving or not receiving contributions, nor the use of observations reporting zero contributions (by adding a constant before taking a log) suggest a different interpretation of the results in section 2.3.

[18] In the literature on individual contributions, it is common to include zeroes in the logged dependent variable by adding $10. This is appropriate because the individual tax return data includes a large number of small, positive contributions: for example, in 1985, 11.9 percent of individual returns deducted a gift between $1 and $100. In contrast, very few charities in the sample ever report contribution receipts below $10,000, and a change in logs from $10 to $525, the first percentile of positive observations in 1985, is greater than the log increase from the median ($467,109) to the 95th percentile ($1,110,000). A larger additive constant than 10 is therefore necessary to use observed zeroes in approximate logs without underweighting the variance among positive observations.

Entry and Exit

Since the sample defined in section 2.3 only uses organizations observed before and after the tax change, another concern is that the effect of the tax cost is partly observed in the form of different rates of organization entry and exit.

There is no data source which observes charities' entry and exit directly. Though the IRS maintains a master file of registered nonprofit organizations, it is rarely updated and inappropriate for this type of quantitative analysis.[19] Nor are the Statistics of Income data appropriate for studying entry and exit, as firms are not observed every year: although charities with at least $10 million in gross assets ought to be observed one hundred percent of the time, major organizations are frequently missing for a year or two. For example, the University of Chicago is missing in year 1997; in 1996 Chicago's total assets were reported to be $3.1 billion, well above the threshold for mandatory sampling.

Those data limitations mean that I cannot answer the question definitively. As an alternative, I demonstrate what the available information hints about entry and exit of organizations by state. The form 990 includes a field for the date of the organization's letter from the IRS recognizing it as a tax-exempt public charity. The date of this letter marks the start of the IRS's recognition of the organization as a charity and defines a minimum age for the organization (which has to be at least as old as its exemption letter date). Therefore, if a state's population of charities tends to have more recent exemption letters on average than other states, that suggests that the turnover rate in the state must be higher (either organizations are being created more quickly than in other states, or old organizations exit more rapidly, or both).

I plot state-level shares of forms 990 filed by organizations with post-1986 exemption letters as of 1989 against post-1986 tax rate (figure 2.7A) and 1986–1988 change in tax rate

[19] See National Center for Charitable Statistics (2013, pp. 4–5)

(figure 2.7B). I derive shares of organizations with recent exemption letters from the 1989 IRS Core Files, a dataset containing a limited set of form 990 variables for the universe of filing organizations (Internal Revenue Service 2011a). Neither tax variable is highly correlated with state shares of recently exempted organizations. Post-1986 tax rate and recent exemption share have state-level correlation 0.061 (p-value = 0.6799), and tax cost change and recent exemption letter share have correlation 0.100 (p-value=0.508).

As an added check, I test whether the change in average tax cost is correlated with exit using prospective data. Let $LastOb_i$ be the last year up to 2007 in which organization i is observed in the Statistics of Income or Core Files data sets; though failure to observe an organization does not mean it has disappeared, a recorded 990 almost certainly means it still exists. Therefore the last year of observation should be highly (negatively) correlated with date of exit. For organizations observed in the 1986 Statistics of Income data, I regress

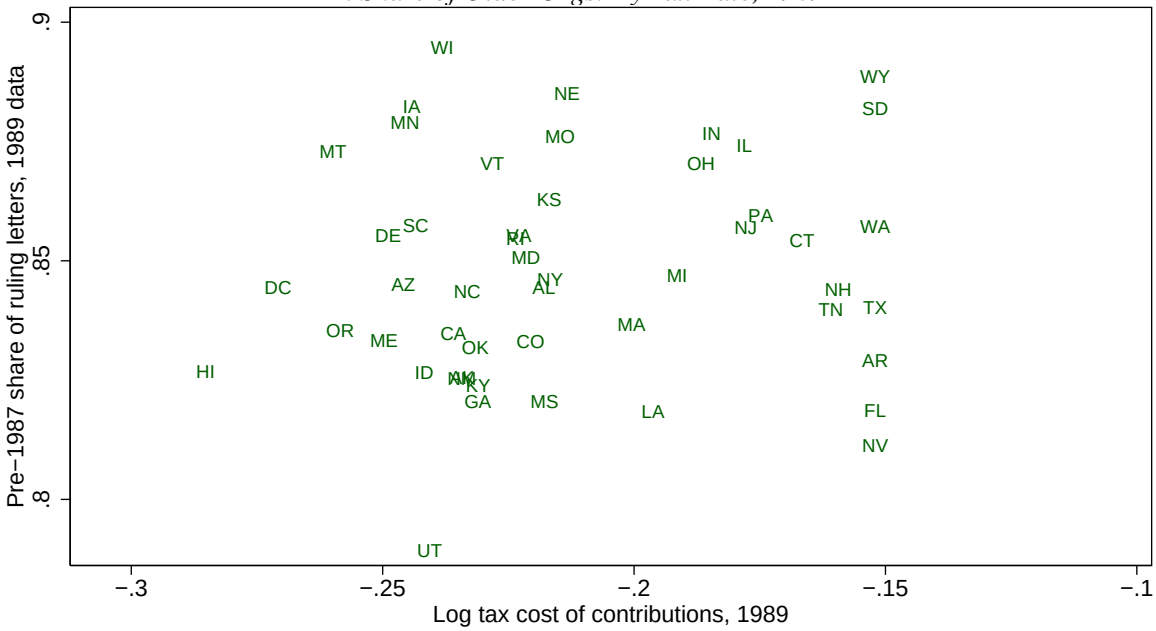
$$LastOb_i = \alpha + \mathbf{X_i'}\boldsymbol{\gamma} + \beta \ln(TaxCost_{s(i),\text{`86}}) + \delta \Delta_{86-88} TaxCost_{s(i)} + \varepsilon_i \tag{2.5}$$

where $\mathbf{X_i'}$ is a vector of organization i's financial variables, and the tax variables capture both the rate before the 1986 tax reform and the TRA86 state level tax change. The results of this regression are presented in table 2.5. Though income and assets are associated with a later end date, there is no significant association between last observation year and tax rates.[20] In summary, there does not seem to be a strong association between the TRA86 tax change and organization entry and exit.

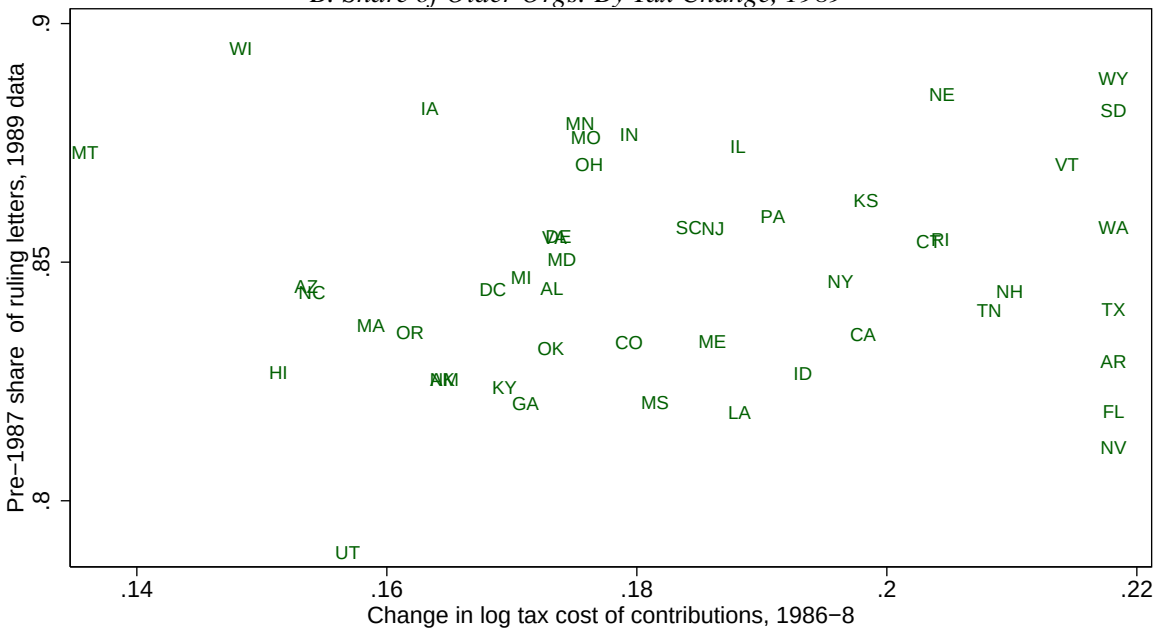
[20]The channel through which income and asset variables are related to last observation year is ambiguous; organizations with more money are presumably less likely to exit for financial reasons, but are also more likely to be required to meet Form 990 filing requirements each year. It is likely that both causes are important.

Figure 2.7: Exemption Letter Share as Test of Entry and Exit

A. Share of Older Orgs. By Tax Rate, 1989



B. Share of Older Orgs. By Tax Change, 1989



Notes: Both Y-axes plot the share of charities in the 1989 IRS Core PC Files with tax exemption letters data 1986 or earlier. In panel (a), the X-axis plots the log of state average tax cost of giving in 1989. In panel (b), the X-axis plots the change in state average tax cost of giving from 1986 to 1988. Points are labeled using state postal abbreviations.
Sources: See notes to table 2.2.

Table 2.5: Last Year Observed Regressed on Financial and Tax Variables

	(1)	(2)
Data Year	1986	1989
Log Assets	0.245***	0.331***
	(0.0453)	(0.00846)
Log Total Income	-0.139**	0.0159
	(0.0610)	(0.0191)
Log Contributions +Grants	0.155***	0.108***
	(0.0154)	(0.00670)
Log Program Service Revenue	0.0228	0.0536***
	(0.0144)	(0.00835)
Log Tax Price	-1.469	-0.912
	(2.390)	(2.177)
$\Delta_{86-88} TaxCost_{s(i)}$	-1.177	-0.202
	(2.198)	(0.801)
Constant	2000***	1998***
	(0.713)	(0.497)
Observations	6644	135808
R-squared	0.043	0.055

Notes: Dependent variable is the last year for which the organization is observed in the Statistics of Income or IRS Core Data files of form 990 filings, up to year 2007 — so column 1 regresses the last year in which organizations present in the 1986 data are observed, while column 2 regresses last year observed for organizations present in the much larger 1989 Core data set. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

State Law Exogeneity

Table 2.1 reports changes to state income tax rates over 1986–1988 reported by ACIR. As described in section 2.3, I drop West Virginia because of changes made to their state charitable contribution deduction during this period, and North Dakota because of implausibly high marginal tax rates. However, my main analysis does retain the tabulated states which changed their marginal tax rates during this period. As table 2.1 demonstrates, there does not seem to be a rush to raise marginal rates at the state level and undo the rate cuts at the federal level, nor is it likely that states would have changed their laws specifically to maintain a constant incentive to give to charitable organizations. Still, we may be concerned that the regression results are driven by states that change their own tax laws, and

that the effect of an exogenous tax change on charitable receipts is therefore not well-identified.

Therefore, table 2.6 replicates the difference-in-differences estimates of equation 2.2, but drops the sixteen states which changed their marginal rates over this period. The reported estimates indicate that, if anything, inclusion of those states attenuated the estimated elasticities toward zero; in the reduced sample, the estimates range from -4.7 to -6.2. State rate changes do not seem to be driving the results.

Table 2.6: Difference-in-Differences Estimates (Excluding Rate-Changing States)

	(1)	(2)	(3)	(4)
	Log Direct Contributions			
$\Delta_{86-88} TaxCost_{s(i)}$	-6.164***	-6.263***	-4.731***	-3.977***
$*Post86_t$	(1.633)	(1.592)	(1.342)	(1.371)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	10422	10422	10422	10422
R-squared	0.860	0.860	0.860	0.861
Number of Orgs	2024	2024	2024	2024
Difference test (p-value)	0.018	0.0019	0.1847	0.0235

Notes: See notes to table 2.2 for description of variables. Sample excludes states in table 2.1 observing a rate change in years 1986–8. "Difference test" reports a p-value for difference between this subsample and the main sample used in table 2.2. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

Sample Selection Checks

Section 2.3 described a series of steps taken to limit the sample to organizations for whom the average tax change in the state of filing plausibly describes average tax change for their pool of possible donors. My estimation strategy requires that a charity's donors be located in the state of the charity's 990 filing, or the attribution of all changes in donation

Table 2.7: Difference-in-Differences Estimates (Nonlocal Organizations Included)

	(1)	(2)	(3)	(4)
	Log Direct Contributions			
$\Delta_{86-88} TaxCost_{s(i)}$	-3.153**	-3.729***	-2.760***	-3.048***
$*Post86_t$	(1.562)	(1.273)	(1.022)	(0.996)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	21653	21653	21653	21653
R-squared	0.880	0.880	0.880	0.880
Number of Orgs	4146	4146	4146	4146
Difference test (p-value)	0.0007***	0.0000***	0.0004***	0.0003***

Notes: See notes to table 2.2 for description of variables. Sample does not exclude non-local organizations. "Difference test" reports a p-value for difference between this larger sample and the nested sample used in table 2.2. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

behavior to particular state policies will tend to attenuate estimates toward zero. (For example, though the American Red Cross is headquartered in Washington, the tax law of the District of Columbia affects only a small share of its donors.) This section demonstrates that the result is robust to the choices made in that sample selection process.

First, I consider whether the sample is too narrow by estimating the difference-in-differences regression described in equation 2.2 without any limitation of organizations by likely "local-ness." Instead, I skip step 3 of the process outlined in section 2.3. I report the results of using this expanded sample in table 2.7. The obtained difference-in-differences estimates are not wildly altered, ranging from -2.8 to -3.8 and remaining statistically significant. The lower magnitudes are consistent with attenuation bias in the measure of average tax cost.

Second, I check whether use of an unbalanced panel distorts the obtained results. Because the Statistics of Income data only try to sample the largest organizations by assets

every year, an unbalanced panel allows use of information about more and smaller organizations than otherwise; however, if the pattern of observation is correlated with the outcome of interest, then estimates from an unbalanced panel may be biased. Table 2.8 reports continuous difference-in-differences estimates using only organizations observed in all six years. The results are consistent with table 2.2, obtaining elasticities from -3.2 to -4.3; none are statistically different from the corresponding estimate in table 2.2. There is no indication that an unbalanced panel is driving the results.

Table 2.8: Difference-in Differences Estimates (Balanced Panel)

	(1)	(2)	(3)	(4)
	Log Direct Contributions			
$\Delta_{86-88} TaxCost_{s(i)}$	-3.686*	-4.716**	-3.673**	-2.962**
$*Post86_t$	(2.074)	(1.778)	(1.507)	(1.453)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	10449	10449	10449	10449
R-squared	0.854	0.855	0.855	0.855
Number of Orgs	1765	1765	1765	1765
Difference test (p-value)	0.3672	0.8352	0.8606	0.6277

Notes: See notes to table 2.2 for description of variables. Sample is restricted to organizations observed in 1982, 1983, 1985, 1988–1990. "Difference test" reports a p-value for difference between this subsample and the main sample used in table 2.2. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

Third, one may be concerned that the filters used to eliminate organizations with broad donor bases are not *strict enough*. If the remaining interstate contributions are randomly distributed, this will tend to attenuate estimates toward zero. But if flows of donations across states are nonrandom, the estimates might be distorted by these cross-border effects. I check for this in two ways. First, I check that the results are not driven by patterns of donation flows within interstate communities by repeating the analysis of table 2.2, but dropping organizations located in Census Metropolitan Statistical Areas that straddle state

borders. For example, the Washington, DC metropolitan statistical area spreads over Maryland, Virginia, West Virginia, and the District of Columbia, each with their own state tax laws, and is therefore dropped. The estimates obtained with this restricted sample, reported in table 2.9, are not qualitatively or statistically different from those presented in table 2.2.

Second, I do a placebo test for the possibility that national changes in giving patterns uncorrelated with donors' tax rates drive the results, by repeating the analysis in table 2.2 for charities that meet other sample selection rules, but are flagged as nationally prominent by sample selection step 3 in section 2.3. Table 2.10 reports these results; the coefficients on the tax treatment variable are positive, consistent with the explanation that it is tax changes, and not some other unobserved change, that drives the results.

Table 2.9: Difference-in-Differences Estimates (Excluding Interstate Metropolitan Areas)

	(1)	(2)	(3)	(4)
	Log Direct Contributions			
$\Delta_{86-88} TaxCost_{s(i)}$	-4.415**	-4.922***	-4.058***	-3.776***
$*Post86_t$	(1.757)	(1.312)	(1.201)	(1.144)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	14297	14297	14297	14297
R-squared	0.863	0.864	0.864	0.864
Number of Orgs	2812	2812	2812	2812
Difference test (p-value)	0.663	0.7835	0.8134	0.8517

Notes: See notes to table 2.2 for description of variables. Data excludes charities sited in Census Metropolitan Statistical Areas that encompass portions of more than one US state. "Difference test" reports a p-value for difference between this subsample and the main sample used in table 2.2. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

Overall, these results suggest that my preferred sampling procedure is not creating a spurious association between tax changes and charitable contributions.

Table 2.10: Difference-in-Differences Estimates (Placebo Test, National Organizations)

	(1)	(2)	(3)	(4)
	Log Direct Contributions			
$\Delta_{86-88}TaxCost_{s(i)}$	1.987	1.654	2.736**	2.277*
$*Post86_t$	(1.732)	(1.418)	(1.294)	(1.202)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	4771	4771	4771	4771
R-squared	0.915	0.915	0.915	0.915
Number of Orgs	908	908	908	908

Notes: See notes to table 2.2 for description of variables. The sample *only* includes charities excluded for violating one of the rules for dropping non-local charities, but otherwise meeting the conditions outlined in section 2.3. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

Outliers

Figure 2.4A not only plots a statistically significant linear relationship between state average tax cost change and log change in individual organizations' contribution revenue, but also makes clear that organizations can experience truly huge swings in their contribution revenue from year to year. One may be concerned that the difference-in-differences results may not be estimating an actual tax effect, but rather just the influence of a few outliers that experience huge changes in their contributions following the tax change, and that just happen to be located in high- or low-tax-change states.

Table 2.11 checks for this by omitting organizations with the largest and smallest volatility of log contribution revenue, measured by individual standard deviations over the observation period. Column 1 omits the most and least volatile one percent of organizations (two percent of organizations, total); column 2 omits the most and least volatile five percent.

The results are qualitatively consistent with table 2.2. Though the obtained point esti-

Table 2.11: Difference-in-Differences Estimates (Excluding Outliers)

	(1) Drop top and bottom most volatile 1 percent	(2) Drop top and bottom most volatile 5 percent
$\Delta_{86-88} TaxCost_{s(i)} * Post86_t$	-2.810**	-2.654*
	(1.262)	(1.374)
Org. Effects	✓	✓
Year Effects	✓	✓
Observations	16508	15162
R-squared	0.874	0.892
Number of Orgs	3169	2901
Difference test (p-value)	0.013**	0.0521*

Notes: See notes to table 2.2 for description of variables. The sample excludes charities with variance in charitable contributions during the sample years above or below percentile thresholds. "Difference test" reports a p-value for difference between this subsample and the main sample used in table 2.2. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

mates are lower in magnitude, they describe a large negative association between tax cost change and contributions received (coefficients of -2.81 and -2.65), and both are statistically different from zero at the ten percent level. At the same time, both estimates are statistically different from the corresponding estimates in table 2.2 at the ten percent level as well (p-values of 0.0130 and 0.0521, respectively), suggesting that outliers may be important to the magnitudes obtained in table 2.2.

2.6 Comparing Donors' and Charities' Tax Elasticities

The preceding sections have demonstrated that organizations with a large increase in tax cost of giving received lower contributions after the TRA86 than organizations in states where the tax cost increased less sharply. Interpreted as an elasticity, contributions fall by about four percent for a one percent increase in tax cost. This stands in striking contrast to the literature on individual donations, which finds a decline of about 1.2 percent in

individual donations for a 1 percent increase in individual tax cost of giving.

The discrepancy between these two estimates is striking, but readily explained by differences in the two data sources and in the methods used to analyze them. First, both data sets are constructed from subsets of all donations in ways that are likely to make them unrepresentative of the whole, and different from each other. Analysis of household tax return data excludes a group of people likely to be more tax-sensitive than the population as a whole — non-itemizers — while the form 990 data excludes one of the least tax-sensitive sectors — churches and houses of worship. In addition to these compositional differences, the composition of observed values differs across the two samples: donors give the most dollars to churches (a low tax-sensitivity sector), while observed charities receive their largest contributions from the very wealthy (the most tax-sensitive group). Finally, individual income tax elasticities may be biased if charities change their fundraising approach heterogeneously by donor income or tax rate; estimates from charity data will necessarily be net of any changes in fundraising.

Heterogeneity by Charity

The sample examined in this paper is not representative of the charitable sector as a whole in two important ways. First, many charitable organizations do not file the form 990, including private foundations (which file the 990-PF), government entities (such as public universities), very small organizations, and churches. In 1985, the charities required to file the form 990 accounted for 41.5 percent of all public charities by contributions; giving to churches and other houses of worship made up over half of charitable giving. Second, the identification strategy used in section 2.3 requires a focus on local charities, whose donors may not respond to tax incentives in the same manner as donors to national charities. The charities observed in the Statistics of Income data represent 24.6 percent of all charitable giving in 1985; the observations retained following the data-cleaning procedure described

in section 2.3 represent 10.7 percent of all charitable contributions.

Analysis of survey data has suggested that giving to churches is less tax-sensitive than other charitable giving (Giving USA 2013; Feldstein 1975). This is consistent with the volatility of aggregates: figure 2.8 plots year-over-year changes in charitable giving. Religious giving is the least volatile of any of the charitable sectors; over the 1982-1990 period, real annual change in aggregate religious giving had a coefficient of variation of 1.04, compared to a coefficient of 2.25 for total charitable contributions.[21] Because the form 990 data does not include churches, we therefore would expect estimates of tax-sensitivity to be of larger magnitude than if churches were included.

While churches are not observed in form 990 data, we can test for differences among the charities that are required to file the form. The NCCS 990 data report National Taxonomy of Exempt Entities (NTEE) sector codes for each organization. Table 2.12 reports the number of organizations and observations in the sample by each NTEE code. Retained organizations are not evenly distributed among the sector codes: health charities, mostly community nonprofit hospitals, comprise a plurality of observations, followed by education and human services, then by arts and culture charities and by grantmaking charities (such as United Ways and community foundations). No other NTEE sectors have more than 100 organizations observed in the sample.

To test for heterogeneity by charitable sector, I modify equation 2.2. For each nonprofit sector S of interest, I estimate

$$\ln(Contributions_{it}) = \alpha_i + \beta\Delta_{86-88}\ln(TaxCost_{s(i)}) * Post86_t + \delta_t + \mathbf{X}'_{\mathbf{st}}\boldsymbol{\gamma} + \varepsilon_{it}$$
$$+\zeta Post86_t * \mathbf{1}[Sector_i = S] + \eta\Delta_{86-88}\ln(TaxCost_{s(i)}) * Post86_t * \mathbf{1}[Sector_i = S] \quad (2.6)$$

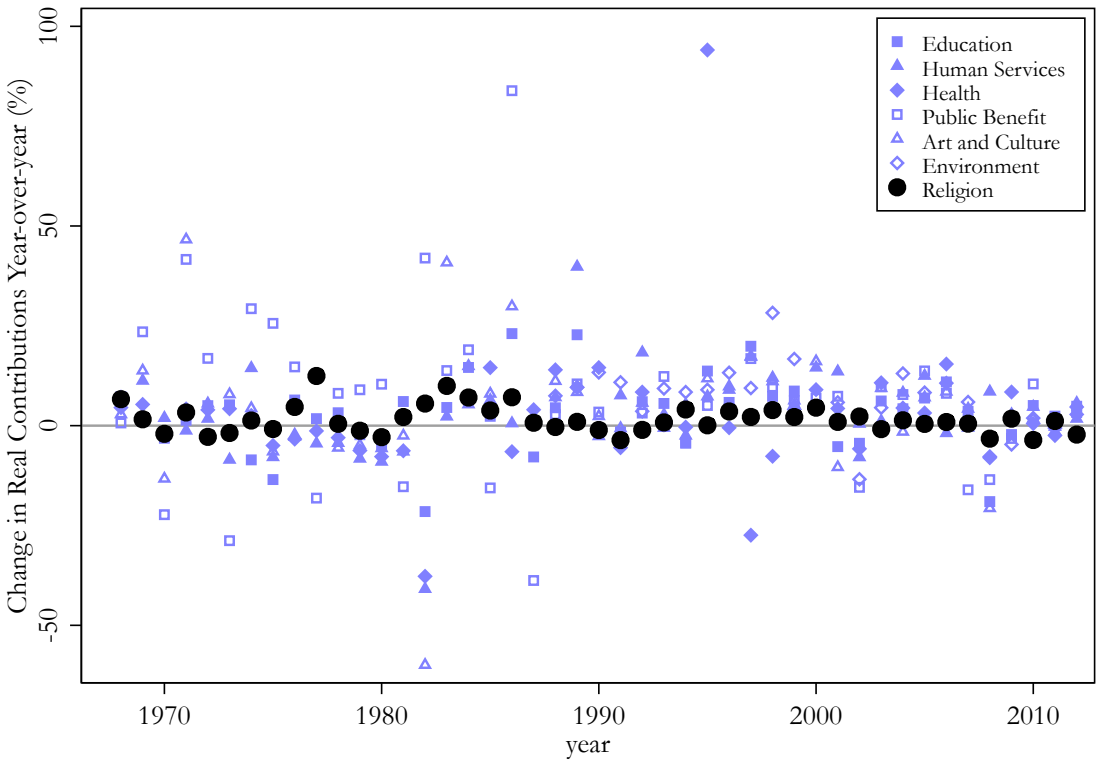
[21] Over this period, religious giving grew by a mean of 3.7 percent a year with a standard deviation of 3.9 percentage points; total giving grew by an average of 2.6 percent each year with a standard deviation of 5.8 percentage points.

Table 2.12: Distribution of Charities by Sector

NTEE Sector	Example	Orgs.	Obs.
A - Arts, Culture, and Humanities	San Diego Museum of Art	182	824
B - Education	Hendrix College	1027	4984
C - Environmental Quality, Protection, and Beautification	Aspetuck Land Trust	18	81
D - Animal-Related	Humane Society of Marin County	15	60
E - Health	Children's Medical Center of Dallas	1390	6860
F - Mental Health, Crisis Intervention	Philadelphia Psychiatric Center	46	197
G - Diseases, Disorders, Medical Disciplines	Dana-Farber Cancer Institute	14	72
H - Medical Research	Hermann Eye Fund	33	157
I - Crime, Legal Related	Mass. Society for Prevention of Cruelty to Children	14	65
J - Employment, Job Related	Blind Industries and Services of Maryland	15	65
K - Food, Agriculture, and Nutrition	Jackson County Meals Service	3	16
L - Housing, Shelter	Presbyterian Retirement Homes of Birmingham	25	116
M - Public Safety	Tacoma Mountain Rescue	5	16
N - Recreation, Sports, Leisure, Athletics	The Fresh Air Fund	18	81
O - Youth Development	Boys and Girls Clubs of Metro Atlanta	26	131
P - Human Services - Multipurpose and Other	YWCA of Walla Walla	428	2047
Q - International, Foreign Affairs, and National Security	Asia Foundation	3	13
R - Civil Rights, Social Action, Advocacy	Anti-Defamation League	1	6
S - Community Improvement, Capacity Building	Junior League of Detroit	12	49
T - Philanthropy, Voluntarism, and Grantmaking Foundations	United Way of Santa Clara County	165	808
U - Science and Technology Research Institutes, Services	University City Science Center	18	87
V - Social Science Research Institutes, Services	Center for Advanced Study in the Behavioral Sciences	2	10
W - Public, Society Benefit - Multipurpose and Other	Hebrew Free Loan Association of San Francisco	7	35
X - Religion Related, Spiritual Development	Upper Peninsula Bible Camp	23	98
Y - Mutual/Membership Benefit Organizations, Other	Lower Marion Township Police Pension Association	1	4
Total		3491	16882

Notes: Tabulation by sector for sample described in section 2.3.
Source: NCCS Statistics of Income.

Figure 2.8: Year-over-Year Growth in Contributions by Sector



Source: Lilly Family School of Philanthropy, Indiana University/IUPUI (2013).

where $\mathbf{1}[Sector_i = S]$ is an indicator equal to 1 if organization i is in sector of interest S. Additional coefficients ζ and η allow for a different effect on organizations S than the rest of the sample. I report these coefficients, as well as β and p-values for the joint significance of the sector-S estimates, for the five most common sectors in table 2.13. The obtained results are consistent with different tax responses by charitable sector: the health and philanthropy sectors are significantly more tax-responsive than the rest of the sample, while the culture and education sectors are less tax-responsive. The human services sector is not statistically different from the rest of the sample. The magnitude of the obtained estimates is particularly striking for the health sector, which not only has a large and highly significant coefficient (-7), but which appears to drive much of the results — the coefficient

for the rest of the sample is much smaller (-0.7) and not statistically different from zero when health charities are allowed to be affected differently.[22]

Table 2.13: Regressions Testing Differential Tax Effects By Sector

Major Sector	$\Delta TaxCost_s$ $*Post86$	$Post86$ $*SectorS$	$\Delta TaxCost_{s,q}$ $*Post86$ $*SectorS$	Sector Joint Test
A- Culture	-3.774***	-0.914	6.173*	0.001***
	(1.411)	(0.627)	(3.342)	
B - Education	-4.493**	-0.379	3.421	0.000***
	(1.809)	(0.406)	(2.199)	
E - Health	-0.715	1.047**	-7.273***	0.000***
	(1.285)	(0.521)	(2.803)	
P - Human Services	-4.093***	-0.996	5.260	0.267
	(1.450)	(0.623)	(3.371)	
T - Philanthropy	-3.496**	0.336	-0.704	0.004***
	(1.416)	(0.646)	(3.523)	

Notes: Sample is identical to the one used in table 2.2. All regressions include controls for organization fixed effects, region-by-year effects, and state-level macroeconomic variables. Additional interaction terms with sectoral indicators are reported; see specification of equation 2.6 in section 2.6. "Joint test" reports p-values for the hypothesis that both sector interaction coefficients equal zero. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

Heterogeneity by Donor Income

In addition to differences in data composition, because donors' behavior is heterogeneous, computing the elasticity of received donations asks a fundamentally different question than the elasticity of donors' contributions. As section 2.2 documents, voluntary contributions to charities can vary greatly from year to year; one important component of this

[22]Tax responses vary by donor as well as by charity. Cross-sectional studies of upper-income households have found that donors' tax-sensitivity is "U-shaped" in income (Feldstein and Taylor 1976; Clotfelter 1985). Because non-itemizers, who tend to have lower incomes than itemizers, have no incentive to report their charitable giving in most years, they are generally excluded from analysis of giving in tax return data. However, from 1983 to 1986, non-itemizers were able to take a limited deduction for their charitable contributions, a provision repealed by the TRA86. Since non-itemizers saw their tax cost of contributions increase after 1986, omitting them from analysis of household data may mean focusing on a less tax-sensitive sample than the population as a whole.

variation is large one-time gifts. A charity might have many small-dollar donors that give regularly and are not particularly tax-sensitive, yet its overall contributions could be profoundly affected by tax rates.

I do a rudimentary test for the possibility that high-income households are driving the tax response by splitting the average tax cost measure into two pieces. Let $\Delta_{86-88} \ln(TaxCost_{s(i),q})$ denote the change in the average tax cost of giving from 1986 to 1988 for returns with adjusted gross income in fractile q of the distribution. (That is, follow the same procedure outlined in section 2.2, but assign zero aggregation weight to returns not in fractile q of the income distribution.) I split the tax cost treatment variable into the portion explained by fractile q, and residual variation:

$$\begin{aligned} \Delta_{86-88} \ln(TaxCost_{s(i)}) =& \alpha_0 + \alpha_1 \Delta_{86-88} \ln(TaxCost_{s(i),q}) + u_{qs} \\ Residual_{qs} \equiv& \hat{u}_{qs} \end{aligned} \tag{2.7}$$

This divided variable is plugged into equation 2.2 to see whether the variation across states is better explained by fractile q or by the unexplained component of $\Delta_{86-88} \ln(TaxCost_{s(i)})$

$$\begin{aligned} \ln(Contributions_{it}) = \alpha_i + \delta_{r,t} + \mathbf{X}'_{\mathbf{st}}\boldsymbol{\gamma} + \beta_1^q Residual_{qs} * Post86_t \\ + \beta_2^q \left(\Delta_{86-88} \ln(TaxCost_{s(i),q}\right) * Post86_t + \varepsilon_{qit} \end{aligned} \tag{2.8}$$

If β_2^q explains most of the variation regardless of income tier, that would be consistent with the interpretation that the change in tax policy common to all levels of the income distribution are most important for explaining changes in charitable receipts. On the other hand, if only the top tiers of the income distribution are associated with differences in contributions across states, that would be consistent with the interpretation that it is the effects of the tax cut on high-income households drives the observed changes.

The estimates for coefficients on the income tier averages and residual pieces are presented in table 2.14. The results are consistent with the interpretation that the tax cut common to all households is not the driving force behind changes in contribution behavior: the residual, and not the income fractile component, is most strongly associated with the tax change for the middle six deciles of the income distribution. On the other hand, for the top two deciles, the component explained by the income decile is highly statistically significant and negative while the residual piece is statistically indistinguishable from zero.[23] Additionally, because the size of the tax change for these deciles is larger, the estimates — negative 2.8 for the top tenth of the income distribution — are somewhat smaller than the corresponding estimate (-3.5) in table 2.2; this suggests that estimates in table 2.2 may be overstated because of scaling. That is, because upper-income households experienced larger tax cuts than the rest of the population on average, if they are the ones driving the results, then the tax change for the population as a whole will be too small in magnitude, overstating estimates.

Endogenous Fundraising

A third possible explanation is that the charities themselves change their behavior following a tax change in a way that confounds — or at least changes the interpretation of — individual charitable elasticities. That is, if charities strategically respond to tax rate changes by targeting the intensity of their fundraising to potential donors in a way that is correlated with changes in tax rates, fundraising could be an omitted variable that affects estimates from individual and organizational data in different ways.

To see how the omission of fundraising could affect tax elasticity estimates of contributions differentially for households and for organizations, consider a hypothetical data

[23] Intriguingly, the same pattern is observed for the two lowest deciles. This may be driven by older taxpayers with high wealth but low income in the current year. However, more investigation is necessary.

Table 2.14: Difference-in-Differences Estimates (Split by Income Quantiles)

Income Group	$\Delta TaxCost_{s,q}$ ∗*Post86*	*Residual* ∗*Post86*	Income Group	$\Delta TaxCost_{s,q}$ ∗*Post86*	*Residual* ∗*Post86*
Bottom 10%	-3.271**	-3.560	6th	-1.084	-6.536***
	(1.416)	(2.495)		(1.904)	(1.985)
2nd	-3.274**	-1.460	7th	-2.097	-5.217**
	(1.273)	(2.796)		(1.316)	(2.080)
3rd	-4.651**	-6.459**	8th	-2.178*	-4.784*
	(2.208)	(2.850)		(1.125)	(2.552)
4th	-0.697	-3.436**	9th	-2.755***	-0.517
	(1.691)	(1.517)		(1.046)	(3.887)
5th	1.514	-6.103***	Top 10%	-2.760***	3.086
	(2.261)	(1.716)		(0.899)	(3.686)
Top 5%	-3.104***	0.652	Top 1%	-2.876*	-4.872*
	(1.049)	(3.217)		(1.528)	(2.498)

Notes: All regressions are on the main sample and include organization effects, region-by-year effects, and state-level time-varying macroeconomic variables (population, unemployment, per capita income, poverty rate, and gross state product). See the description in section 2.6 and appendix A for further explanation. *** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.
Sources: See notes to table 2.2.

set described in table 2.15. In these data, there are three households ($h \in \{1, 2, 3\}$) and two charities ($c \in \{A, B\}$) observed in periods t. In each period, each household has tax rate $1 - \tau_{ht}$, receives a fundraising solicitation F_{hct} from charity c, and makes contribution C_{hct}. However, the investigator only has data on either household-level aggregates such as $C_{ht} = \sum_{ct} C_{hct}$ and $F_{ht} = \sum_{c} F_{hct}$, or organization-level aggregates such as $C_{ct} = \sum_{h} C_{hct}$, $F_{ct} = \sum_{h} F_{hct}$, and average tax cost $\overline{1 - \tau}_{ht} = \sum_{h}(1 - \tau_{hct})\frac{C_{hct}}{C_{ct}}$. The investigator working with these aggregated data can estimate the effect of tax rates on contributions from the household data, likely using a log-linear model with fixed effects for household and period:

$$\ln C_{ht} = \alpha_h + \lambda_t + \beta_1 \ln(1 - \tau_{ht}) + \beta_2 \ln F_{ht} + u_{ht}$$

or from charity-level data

$$\ln C_{ct} = \alpha_c + \omega_t + \delta_c \ln\left(\overline{1-\tau}_{ct}\right) + \delta_2 \ln F_{ct} + \epsilon_{ct}$$

The behavior of each actor following a tax cut is driven by the households' preferences. Households 1 and 2 care strongly about charities A and B, respectively, and only give to their preferred charity. Their contributions are increasing in fundraising F_{hc} for their preferred organization and decreasing in tax cost of giving $1 - \tau_h$. Household 3 is not particularly committed to any cause, but will increase contributions when the tax cost is lowered or fundraising increased. Importantly, household 3's cross-partial is negative — when $1 - \tau_3$ increases, the return on a marginal dollar F spent soliciting household 3 declines.

A tax reform increases the tax cost of giving for households 1 and 3, while leaving household 2's rate unchanged. Because the value of fundraising from household 3 depends on its tax rate, both charities decrease their fundraising from that household, and contributions made by household 3 to either charity fall in response to the higher tax cost and lower fundraising.

Assuming charities set fundraising levels across household so that marginal expected C/F is the same, the falling marginal productivity of fundraising household 3 makes it more appealing for charity A to solicit household 1 more. However, household 1's tax cut makes solicitation less productive at the margin. These countervailing effects cancel each other out, and after the tax change fundraising of household 1 by charity A remains the same, while contributions from household 1 to charity A fall.

Charity B similarly is willing to accept a lower marginal return on fundraising from household 2, but unlike household 1, there is no countervailing change in the tax cost of giving. Charity B increases fundraising of household 2, and contributions from 2 to B

increase following the tax change.

Table 2.15: Hypothetical Data, Contributions with Endogenous Fundraising

	Household 1	Household 2	Household 3	*Organization Data*
Charity A	F_{1A} unch. $C_{1A} \downarrow$	n/a	$F_{3A} \downarrow$ $C_{3A} \downarrow$	$F_A \downarrow, C_A \downarrow\downarrow$ $(\overline{1-\tau_A}) \uparrow\uparrow$
Charity B	n/a	$F_{2B} \uparrow$ $C_{2B} \uparrow$	$F_{3B} \downarrow$ $C_{3B} \downarrow$	F_B unch., C_B unch. $(\overline{1-\tau_B}) \uparrow$
Household Data	$(1-\tau_1) \uparrow$ F_1 unch. $C_1 \downarrow$	$(1-\tau_2)$ unch. $F_2 \uparrow$ $C_2 \uparrow$	$(1-\tau_3) \uparrow$ $F_3 \downarrow\downarrow$ $C_3 \downarrow\downarrow$	

Notes: Arrows denote increases (↑) or decreases (↓) across observation periods; a double arrow denotes a bigger change.

This simple story generates omitted variable bias of opposite signs in the two implicit data sets. In the household data, the covariance between changes in $1-\tau_h$ and F_h is *positive*: household 2 has no tax change but observes increased fundraising, while households 1 and 3 have tax cost increases and unchanged or decrease fundraising. Because the omitted variable bias on the tax cost coefficient β_1 will have the same sign as the covariance between tax cost and the omitted variable, leaving out fundraising implies a positive bias. That is, since we expect tax cost is negatively associated with charitable giving, the negative coefficient should be biased upward, and households should appear less tax-sensitive than they actually are.

Conversely, the covariance between changes in tax cost $\left(\overline{1-\tau_c}\right)$ and fundraising F_c in the charity data set is *negative*: both of charity A's donors experience a tax cost increase, and A's fundraising declines, whereas B leaves total fundraising unchanged and sees a cost increase for just one of its donors. This implies that charity data will tend to *overstate* the tax sensitivity of donors.

Of course, this is example is purely hypothetical — it is *possible* that fundraising creates

omitted variable biases of opposite signs, but it is not necessarily so. Unfortunately, neither individual tax return data nor the survey data collected by the Center on Philanthropy/PSID has information on how aggressively donors were solicited by charities (Survey Research Center 2013). It is doubtful that donors would remember the details of fundraising campaigns even if survey takers did ask. It is not possible to avoid omission of fundraising from analysis of individual data.

Our ability to check whether omitted fundraising behavior biases estimates from charity data is only slightly better. While the form 990 requires charities to report total expenditures on fundraising, we cannot directly observe changes in the allocation of fundraising among donors. The best we can do is see whether charities respond to a tax cut by increasing or decreasing their *total* expenditures on fundraising. Table 2.16A examines whether fundraising changes on the extensive margin, by presenting results for regression of a variable equal to 1 if a charity has strictly positive fundraising and zero otherwise on the same regressors as table 2.2 (see equation 2.2). Similarly, table 2.16B regresses the log of fundraising expenditure (plus $25,000 to avoid dropping zeroes) on the same regressors. Neither set of regressions finds a statistically significant change in fundraising following the TRA86.[24] The average charity in the sample experiences a change in the *TaxCost* treatment variable of 0.185, which means that, interpreted as linear probabilities, the largest point estimates in table 2.16A imply a fall of about six percentage points in the likelihood of engaging in fundraising. The point estimates in table 2.16B imply an elasticity of roughly -1 for fundraising expense in response to a tax change, so a one percent increase in the representative tax cost of giving might cause a one percent decrease in fundraising. These effects are economically significant, but not statistically different from zero.

We can further test to see whether the results in 2.2 are confounded by the omission

[24]This regression only compares 1988–90 with the pre-period data. Evidence presented in section 3.4 of the following chapter will, however, suggest fundraising intensity may more strongly associated with tax rate changes over the longer term.

of changes in total fundraising (although, again, we cannot see whether the allocation of fundraising across donors changes). Table 2.17C modifies the specification in equation 2.2 to add charities' fundraising expenditures.

$$\begin{aligned}\ln(Contributions_{it}) =& \alpha_i + \delta_t + \mathbf{X}'_{\mathbf{st}}\boldsymbol{\gamma} + \beta\Delta_{86-88}\ln(TaxCost_{s(i)}) * Post86_t \\ &+\mu\mathbf{1}[Fundraising > 0] + \theta\ln(Fundraising + \$25{,}000) + \varepsilon_{it} \qquad (2.9)\end{aligned}$$

where $\mathbf{1}[Fundraising > 0]$ is equal to 1 if fundraising expenditure is strictly positive and zero otherwise, and $\ln(Fundraising + \$25{,}000)$ is log of fundraising expenditure plus a constant ($25,000).

Unsurprisingly, choosing to fundraise and the amount spend fundraising are both associated with higher contribution revenues.[25] However, the inclusion of fundraising variables does not change the estimated tax elasticities greatly, nor are the estimates statistically different from those in table 2.2. To the limited extent we can investigate the question, the difference between tax elasticities estimated in this paper and those estimated from individual tax data do not appear to be driven by tax-related shifts in charities' fundraising activities. However, it is possible that total fundraising expenditures just conceals important information about shifts by charities among targeted donors in the manner described in table 2.15.

In conclusion, there are good reasons to expect that public charity data would yield a different tax cost of giving elasticity than household tax returns. The two data sources are composed of particular kinds of charity and particular kinds of household that are not representative of all charities or all household: household tax return data does not observe non-itemizers' giving, while my regression sample omits national charities and churches.

[25]Note that, since we have made no attempt to deal with the endogeneity of fundraising expenditures, these estimates should not be interpreted as causal effects.

Table 2.16: Difference-in-Differences Estimates of Fundraising Activity

A. Decision to Fundraise (Extensive Margin)

	(1)	(2)	(3)	(4)
	Positive Fundraising Expenditure (0/1)			
$\Delta_{86-88} TaxCost_{s(i)}$	-0.185	-0.348	-0.322	-0.354
$*Post86_t$	(0.501)	(0.518)	(0.388)	(0.404)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	16882	16882	16882	16882
R-squared	0.821	0.821	0.821	0.821
Number of Orgs	3273	3273	3273	3273

B. Log of Fundraising Expenditure +$25,000

	(1)	(2)	(3)	(4)
	Log Fundraising Expense + $25,000			
$\Delta_{86-88} TaxCost_{s(i)}$	-0.834	-1.298	-1.212	-1.330
$*Post86_t$	(1.342)	(1.383)	(1.159)	(1.122)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	16882	16882	16882	16882
R-squared	0.899	0.899	0.899	0.899
Number of Orgs	3273	3273	3273	3273

Moreover, because total household donations and total charitable receipts are aggregations of the contributions from particular donors to particular charities, we expect the measured responses to differ to the extent that the two variables aggregate the underlying heterogeneous responses in different ways. A third possibility, that endogenous shifts in fundraising by the charities creates a difference between individual and organizational estimates, is not

Table 2.16 — Continued

C. Direct Contributions With Fundraising Regressors

	(1)	(2)	(3)	(4)
	Log Direct Contributions			
$\Delta_{86-88} TaxCost_{s(i)}$	-4.297**	-4.735***	-3.752***	-3.225***
$*Post86_t$	(1.731)	(1.488)	(1.100)	(1.065)
1[*Fundraising*]	0.192***	0.191***	0.189***	0.189***
	(0.0241)	(0.0243)	(0.0244)	(0.0242)
Log Fundraising	0.130**	0.132**	0.139**	0.139**
+$25,000	(0.0555)	(0.0547)	(0.0533)	(0.0533)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	16882	16882	16882	16882
R-squared	0.865	0.865	0.865	0.865
Number of Orgs	3273	3273	3273	3273
Difference test (p-value)	0.9300	0.8510	0.8294	0.7952

Notes: Dependent variable in panel A is equal to 1 if charity has strictly positive fundraising expenditure, zero otherwise. Dependent variable of panel B is the log of fundraising expenditure plus $25,000 (to retain observed zeroes). Dependent variable in panel C is log of direct contributions; dummy for fundraising at the extensive margin and log of fundraising expenditure plus $25,000 are added as additional control variables to panel C. "Difference test" is p-value for difference of coefficients from corresponding point estimates in figure 2.2.
Sources: See notes to table 2.2

supported by the limited data available.

2.7 Interpreting Trends in Aggregate Charitable Giving

The preceding sections have shown that tax policy can matter for charitable giving, at least for local, non-church organizations, especially health charities. Recall, however, that the share of national income going to going to charitable contributions hardly changed following the TRA86, and is consistently about two percent of GDP over the postwar period even as tax rates changed substantially over this period. Far from witnessing a plunge in

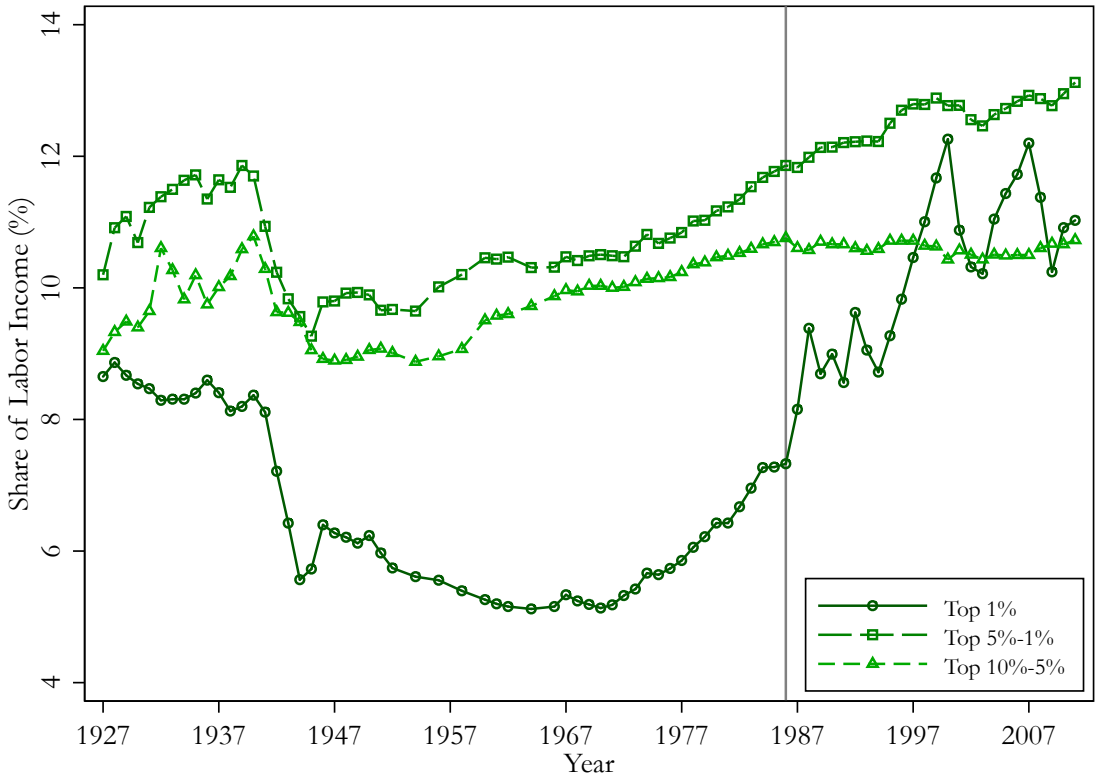
charitable contributions, charitable giving rose 10.1 percent in real terms from 1985 to 1988. A similar modest increase happened for sampled organizations as well: aggregate gifts to organizations in the regression sample in both 1985 and 1988 rose 4.7 percent over that period in real terms. If one believes that the tax cost elasticity of charitable contributions for these charities is really about -3 to -5, then one must also believe that had the TRA86 not reduced tax rates, charitable giving to these groups over this period would have risen sharply.

Such a surge is plausible. The contributions-to-GDP ratio in the postwar period masks two countervailing trends: the steady decline in marginal tax rates (which has decreased charitable donations by raising the tax cost of giving) and rising income of high-income households (which, because philanthropy is a luxury good, has increased charitable giving). The causes and implications of income inequality are a fiercely debated topic, and beyond the scope of this section. Instead, I will briefly provide two pieces of evidence that the constancy of the contributions-to-GDP ratio does not tell the entire story.

First, the TRA86 coincided with a rapid increase in real incomes at the top of the income distribution. Figure 2.9 charts the share of national wage and salary income redounding to households at the top of the income distribution from 1927 to the present. After a long decline, the top one percent of households saw their income share increase gradually beginning in the 1970s, with a particularly sharp increase over 1986 to 1988.[26] Yet this rapid

[26]The rapid increase in observed personal income after 1986 was not necessarily independent of changes in federal tax law. Slemrod (1996) and Gordon and Slemrod (2000) observe that 1988 to 1990 was a brief period when the top tax rate on personal income was lower than the corporate income tax rate, and argue that much of the increase in personal income was really business owners moving the tax base from C-corporations to S-corporations, partnerships, and other forms of personal income. But if the falling share of personal income contributed to charities were explained by shifting of taxable income out of C-corporations, then we should expect corporate charitable contributions to rise following the TRA86 (shifting the tax benefit of charitable contributions from individuals to corporations). Instead, corporate charitable contributions experienced a year-over-year decline every year from 1987 to 1991, both in absolute terms and as a share of corporate profits (Giving USA 2013, §18). Furthermore, the inversion of personal and corporate tax rates ended with the 1990 tax increase, but the rising share of income redounding to the top of the distribution continued unabated (figure 2.9). It is therefore likely that long-run changes at the top of the income distribution are driven by real changes in the economy, and not solely by tax base shifting.

Figure 2.9: Income Share of Top Fractiles

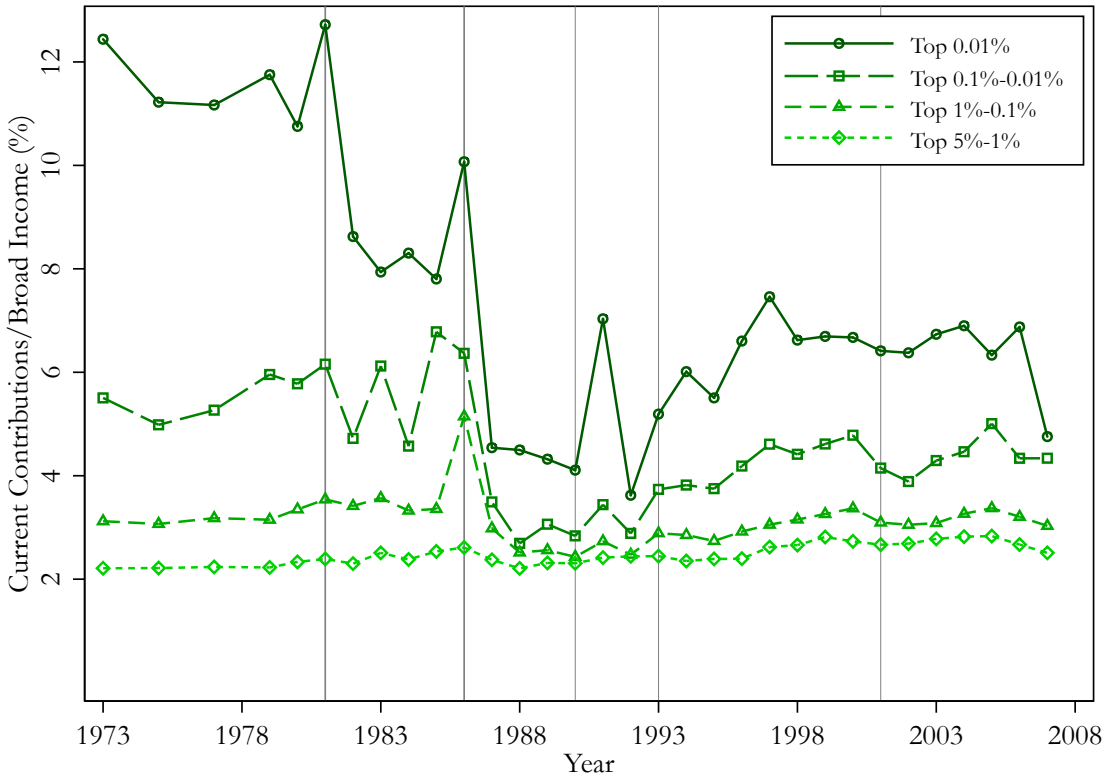


Source: Piketty and Saez (2003), updated data accessed September 2013.

increase in income did not translate into a surge in charitable contributions: figure 2.10 charts the share of pre-tax non-capital gains income contributed by households top income tiers. Vertical lines denote federal tax reforms; giving noticeably spikes in 1981 and 1986, preceding tax cuts, before falling to new rates; it then rises again following the tax hikes in 1990 and 1993.

This income surge explains why my difference-in-differences estimates can imply a large sensitivity of charitable receipts following the TRA86 without an accompanying plunge in aggregate giving: had income spiked as it did without a simultaneous tax cut, then charitable contributions would have surged in this period. Table 2.18 reports giving per household among high-income tiers of itemizing households in 1985 and 1988.

Figure 2.10: Share of Broad Income Contributed by High-Income Fractiles



Notes: "Broad Income" follows Gruber and Saez (2002) by excluding capital gains and social security income.
Sources: Tax return data are from the IRS Public Use File maintained by the National Bureau of Economic Research.

Had itemizing households with more than $100,000 in real (2012) income contributed the same share of income in 1988 as they had in 1985, real personal charitable contributions would have risen by 30 percent over that period instead of 10 percent. Since this back-of-the-envelope estimate does not analyze possible changes in the giving behavior of non-itemizers or lower-income itemizers, who also saw their incentives to contribute reduced after 1986, this is a conservative estimate of the counterfactual increase in charitable contributions. Had the post-1986 surge in incomes happened without a tax cut, and without an accompanying decline in share of income donated, then a counterfactual surge in charitable

contributions looks plausible.

Table 2.18: Calculation of Counterfactual Contributions, 1985-88

Real Income Tier	1985 Real Income ($ bil.)	1985 Real Contrib. ($ bil.)	1985 Contrib. / Income	1988 Real Income ($ bil.)	1988 Real Contrib. ($ bil.)	1988 Contrib. / Income	1988 Income x 1985 Share Contributed
$100-200K	1140	27.70	2.43%	1180	28.00	2.37%	28.67
$200-500K	342	10.40	3.04%	470	12.10	2.57%	14.29
$500-1000K	89	4.51	5.08%	184	4.50	2.45%	9.36
≥$1000K	96	7.71	8.03%	322	10.80	3.35%	25.86
Remainder	3340	72.05	*2.16%*	3600	80.48	*2.24%*	80.48
Total	5007	122.37		5756	135.88		158.66
Change						11.04%	29.66%

Notes: All dollar values are inflated to real 2012 dollars using the Consumer Price Index. Data for high-income households is taken from the IRS public-use cross-sections of individual tax returns. "Contributions" is defined as the sum of current-year cash and non-cash contributions. "Income" is the sum of: wages and salaries, dividends, interest income, alimony, business income, Schedule E income, pensions and annuities, farm income, unemployment insurance, capital gains, and other income. Each income tier excludes tax returns which did not file itemized deductions. "Remainder" is total real contributions estimated by Giving USA, less the sum of itemized contributions from high-income itemized returns, estimated to be $122.37 billion in 1985 and $135.88 billion in 1988 (Giving USA 2013, Table 18).
Sources: Tax return data from the IRS Public Use File. Contribution shares from Giving USA (2013).

Additionally, a comparison with eras with similar income inequality, but differing marginal tax rates, tells a different story. Charitable giving in the interwar period — when marginal tax rates were below twenty percent for almost everybody, and the income share at the top of the distribution comparable to the late 1980's — was significantly lower than two percent of GDP. Figure 2.1 plots four different measures of the charitable contributions-to-GDP ratio for this earlier period from Andrews (1950) and Jones (1954), as well as itemized contributions from tax returns. Estimates of total interwar giving are consistent with a rate of contributions well below the lowest share of GDP observed in the postwar era — but rising rapidly in the 1940s as tax rates rose broadly for another war.[27]

[27]Though marginal rates did rise under the New Deal, before the Second World War they did so more as a populist gesture than a serious tax reform; the Revenue Act of 1935 set a 75 percent top marginal rate on incomes over $5 million dollars, a tax bracket believed to have applied solely to John D. Rockefeller. In

With the benefit of longer historical perspective, there is no reason to believe charitable contributions are permanently anchored to two percent of GDP. Rather, the stability of charitable giving over the postwar period is consistent with a general decline in tax incentives for charitable giving happening concurrently with an increase in the top households' share of income. If the charitable contribution for upper-income households were to be curtailed by a future tax reform, it is entirely possible that charitable giving would fall.

In conclusion, the evidence suggests that reducing upper-income households' charitable contribution tax incentive is not a "free lunch." Charitable contribution receipts are sensitive to the tax subsidy for individual income taxes; this sensitivity is driven by the very high-income households that tax reformers have recently proposed targeting. The size of the effect is remarkable in light of the consensus from the related literature on individual donors that finds a less sensitive elasticity of charitable contributions. However, the discrepancy is explicable because I am estimating a different effect — the response of donations, not donors — and those who give the most tend to be the most tax-sensitive. My estimates imply that the tax cost of a charitable donation matters a great deal, at least for some donors and some charities.

That these charities appear to be more tax-sensitive than household donations overall limits the external validity of these findings to the charitable sector as a whole, but it also implies that a focus on the average donor response ignores heterogeneous effects of changes to charitable tax incentives across donors and across charities. Upper-income households' contributions to particular charitable sectors are quite tax-sensitive. Proposed tax reforms that undermine these incentives could have large effects on provision of these services. As policymakers consider tax reforms, they should consider both the higher responsiveness to these incentives of upper-income households, and whether the charities most likely to

contrast, filers at the 99th percentile of income from 1932 to 1939 (ranging from about $74,000 to $138,000 in 2012 dollars) faced marginal rates of 10 to 15 percent. Marginal rates at the 99th percentile of income ranged from 39 to 62 percent, however, during the war (Piketty and Saez 2003; Tax Foundation 2013).

be harmed by a change in tax incentives — rather than contributions in the aggregate — are worth the costs of the foregone tax revenue. Instead of raising revenue by limiting the existing deduction, legislators might prefer to consider narrowing eligibility for the deduction to sectors where the incentive has the largest effect.

CHAPTER 3

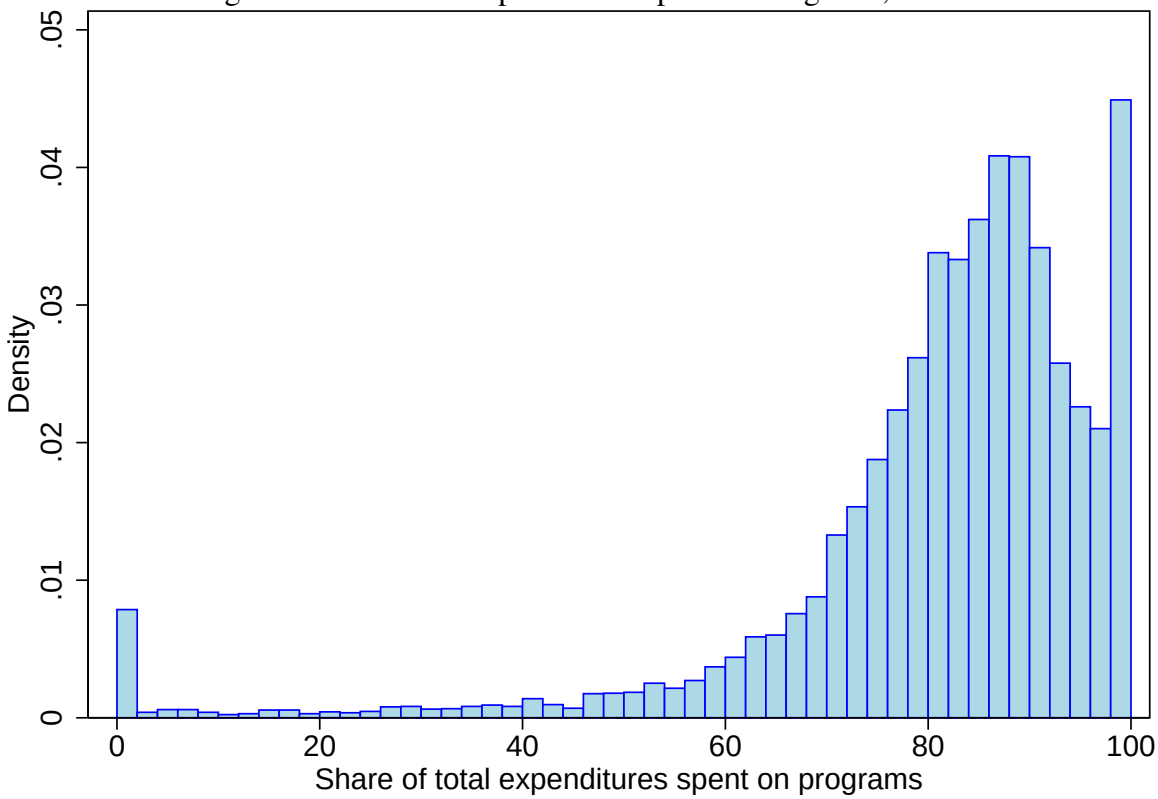
On Nonprofits' Uses of Charitable Contributions

Chapter 2 explored the relationship between the charitable contribution tax deduction and donation revenue. An equally important question, however, is how the flow of donations affects the provision of charitable services. Donors and policymakers should not really care about the after-tax cost of *giving charity money* but the total cost of *creating charitable goods*, including the share of donations the charity spends on ends other than its mission as well as the donor's tax subsidy (see discussion in Weisbrod and Dominguez 1986).

For example, charities vary substantially in the share of revenues spent on overhead. Figure 3.1 plots the share of all expenditures going to a charity's tax exempt purpose for charities observed in the IRS Statistics of Income or Core Files data sets in 2007 (the most recent year available). While program expenditures are by far the largest cost for most charities, a substantial minority spend much of their funds on other things. Charity watchdog groups like Charity Navigator and GiveWell.org rate charities based in part on their average propensity to spend contributions on their missions because such behavior is so prevalent. Charity Navigator automatically assigns groups whose programs account for less than one-third of expenditures its worst rating.[1]

[1]See Charity Navigator, "Financial Ratings Tables": `http://www.charitynavigator.org/index.`

Figure 3.1: Share of Expenditures Spent on Programs, 2007



Notes: Graph plots distribution of charities by share of expenses spent on their tax-exempt purpose in 2007, measured as the ratio of program expenditures to total expenditures.
Source: 343,485 charities observed in year 2007 in the IRS Statistics of Income or Core Files data from form 990 with non-missing, non-negative program expenditures and total expenditures observed.

Such ratings, of course, look at the use of the *average* dollar; it may well be that these high-overhead charities actually spend the *marginal* donation almost entirely on programs. Alternatively, charities may see their charitable goals as largely met beyond some level, and spend any additional gifts on employee perquisites. Charties can also save surplus contributions for future years, or reduce their effort pursuing other revenue sources — Andreoni and Payne (2011) for instance find that charities receiving large government grants exhibit donor "crowd-out" largely due to reduced fundraising by the charity. These charities might respond to a large gift by investing less effort applying for government grants. Because

`cfm?bay=content.view&cpid=48`, accessed March 23 2014.

the relationship between contributions received and program expenditures is unknown, is unclear how much charitable giving increases services actually provided.

This chapter will consider charities' response to changes in contributions to draw inferences about the ultimate effect of the charitable contribution deduction on the provision of charitable services. Form 990 data reveals that charities tend to save their direct contributions — their second-largest revenue source — rather than spend them on programs immediately. This differs substantially from program service revenue — charities' largest revenue source — which is overwhelmingly spent on provision of services.[2] It seems curious that changes in one revenue source would be associated with a completely different pattern of behavior than another, since money is fungible. Yet it is also true that charitable contributions are *less predictable* than service revenues, with occasional large gifts making total receipts change substantially from one year to the next. Charities save to smooth contribution revenues over time.

The implied result of this behavior following a reduction in tax incentives for giving is not an abrupt fall in charity service provision, but rather a gradual decline as savings and expenditures drift toward a lower equilibrium. I exploit variation across states in the change in incentives to give following the 1986 tax reform to explore how charities' program spending and other financial choices respond to a change in the donative environment. I use the same empirical strategy as in chapter 2 to demonstrate that in states where the combined federal and state tax subsidy for charitable contributions fell more sharply following the Tax Reform Act of 1986 (TRA86), there indeed is no immediate difference in program expenditures. Instead, there is a gradual widening in net assets and program expenditures across charities by size of the tax change, peaking five to fifteen years after the tax reform.

[2]Program service revenue is an umbrella term for any income from provision of the charity's tax-exempt purpose. Examples include college tuition, billing for medical expenses, or charges for museum admission. It does not include revenue from provision of services unrelated to the charity's core mission, which is in theory taxable as "unrelated business income," although the boundary between exempt and unrelated income is not well-defined.

3.1 Charitable Contributions as Uncertain Revenues

I begin by establishing an fact about the funding environment for charities: voluntary contributions are charities' second largest source of income, yet they are are more volatile than other income sources. This is not a function of changing fundraising intensity over time, but because of factors beyond charities' control.

Table 3.1 presents summary statistics and simple autocorrelation results for a panel of charities' revenue sources. I estimate a regression of the form

$$R_{i,t} = \alpha_i + \beta R_{i,t-1} + \varepsilon_{i,t} \tag{3.1}$$

where R is a particular revenue source for charity i in period t, α_i is a charity fixed effect, and ε is a residual. Table 3.1 reports demeaned autocorrelation coefficient β and the R-squared for each of these simple regressions. Knowing charitable contributions in a particular year is a weaker predictor of next year's contributions than any of five other important revenue items are for the corresponding item; the R-squareds for indirect contributions (that is, from other charities and foundations), government grants, program service revenue, investment income, and memberships range from 0.75 (for indirect contributions) to 0.99 (for program service revenue), compared to just 0.71 for direct contributions.

Charitable contributions are not exogenous — many charities invest considerable effort and resources into fundraising, and the "power of the ask" is an important determinant of charitable giving (Andreoni and Rao 2011).[3] Table 3.1 does not indicate whether it is fundraising intensity (which charities can control), or giving habits (which they cannot),

[3]The psychological power of a charitable solicitation is such that in field experiments, informed subjects will *avoid* solicitors rather than pay the financial cost of a donation or the mental cost of refusal (DellaVigna et al. 2012; Andreoni et al. 2011). Professional fundraisers, too, place particular emphasis on the act of asking, to the point of teaching that "[s]uccess in fundraising is defined by how many people you ask, not how much money you raise," (Klein 2009, p. 116); at least one book, Axelrod (2004, chap. 16), capitalizes "the Ask."

Table 3.1: Year-to-year Volatility of Charities' Revenues and Fundraising

Revenue Source	Mean	Standard Deviation	Share of Total Income	Coeff.	R^2
Program Service Revenue	49.5	248.6	75.4%	1.06	0.99
Direct Contributions	5.0	27.9	7.6%	0.87	0.71
Government Grants	3.1	31.9	4.7%	1.02	0.96
Investment Income	2.9	58.1	4.4%	1.05	0.98
Indirect Contributions	0.9	25.3	1.4%	1.10	0.75
Dues and Memberships	0.4	9.7	0.6%	0.93	0.89
Other Income	3.8	40.4	5.8%	0.82	0.60
Total Income	65.6	297.8	100%	1.05	0.98
Fundraising Expenditure	0.5	2.9	0.8%	1.02	0.95
Direct Contributions / Lagged Fundraising	88.8	3984.1		0.03	0.00
Direct Contributions / Current Fundraising	73.9	2301.9		0.19	0.04

Notes: Mean and standard deviations are for 248,774 complete observations in 1982, 1983, and 1985–2007. Coefficient and R-squared are for a regression of each variable on its lagged value for the same charity. See specification described in equation 3.1.
Sources: National Center for Charitable Statistics, IRS Statistics of Income and 990 Core Files.

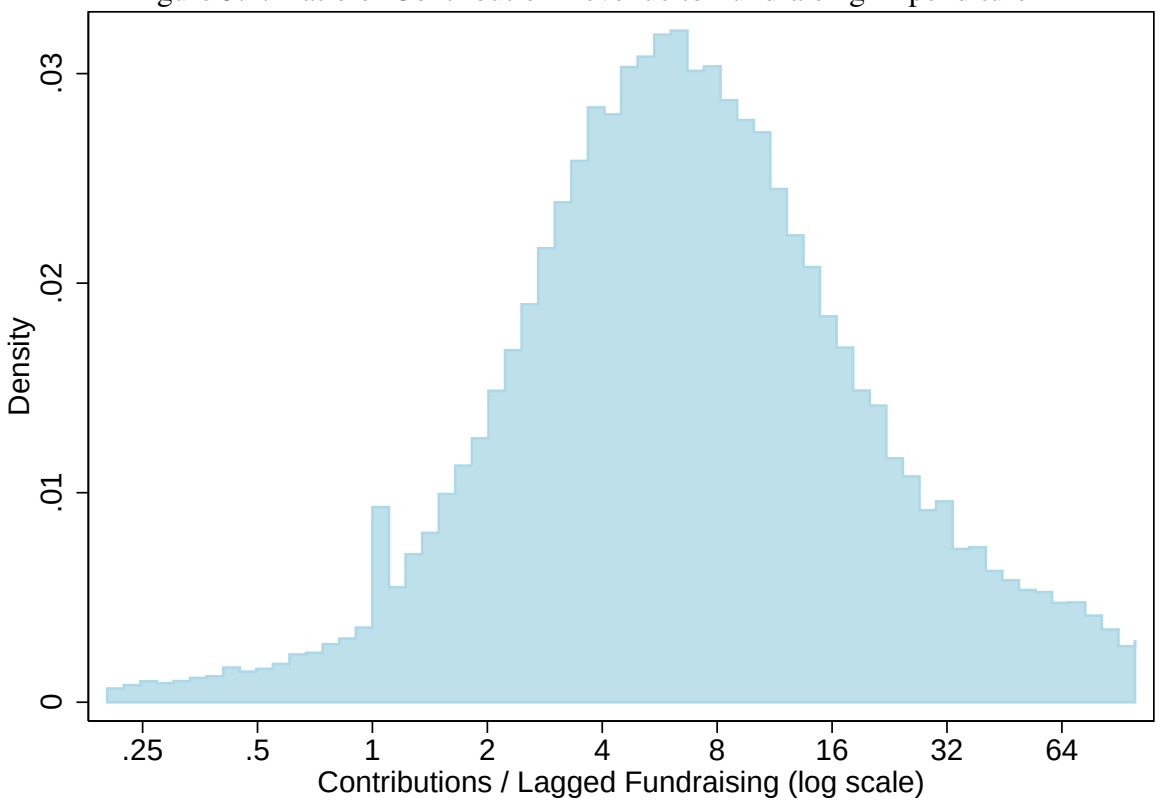
that causes contributions to fluctuate from year to year.

Though fundraising increases expected contributions, it does not follow that contribution revenues are predictable for charities. Figure 3.2 charts the distribution of charities' return on fundraising expenditures. Following Okten and Weisbrod (2000), this is calculated as the ratio of the current year's direct contributions to the previous year's fundraising expenditure.[4] Consistent with Andreoni and Payne's (2011) findings, the modal return on fundraising is over five-to-one. However, many fundraising campaigns do significantly worse: 5.4 percent of observations report less in contributions than was spent on fundrais-

[4] A ratio of current-year contributions to current-year fundraising expenditure obtains a similar distribution.

ing, that is, they lost money. Furthermore, while most of the distribution is log-hump-shaped, there is a spike in the distribution just over a one-to-one ratio, suggesting charities or professional fundraisers may put forth additional effort or manipulate financial reporting in order to disguise a fundraising campaign's failure to cover its own expenses.[5]

Figure 3.2: Ratio of Contribution Revenue to Fundraising Expenditure



Notes: Plot is a histogram of ratios of current year direct charitable contributions to previous year's fundraising expenditure. Values plotted are for 80,570 observations with strictly positive and observed values for contributions and for lagged fundraising, less 982 observations with ratios below 0.2 and 3,420 with ratios above 100, which are suppressed for better visibility.
Sources: National Center for Charitable Statistics, IRS Statistics of Income.

This variability in the return on fundraising is not a function of some charities be-

[5]This distribution also mismeasures the return to fundraising by attributing all contributions to the current year and to paid fundraising. A large share of charities — 33.6 percent of non-missing observations — report strictly positive contributions in years where the current and prior year's fundraising expenditure was zero. A significant share of contributions are likely to come not from formal fundraising campaigns but charity officers soliciting potential large donors without a formal expenditure. Thus 5.4 percent of observations have a contribution/fundraising expense ratio over 100, which is unlikely to be strictly an outcome of a fundraising campaign.

ing naturally better at solicitation than others. In addition to revenue variables, table 3.1 presents autocorrelation regression results for fundraising expenditure and for the ratio of contributions to fundraising. Fundraising expenditure is highly autocorrelated for particular nonprofits, with an R^2 of 0.95, but the ratio of direct contributions to fundraising expenditure is not, with regression of this fundraising ratio on its lag explaining less than half of one percent of variation.

The volatility of contributions is important for interpreting what charities do with resources at the margin. If charities adjust their total expenditures in response to current revenues each period, then we can simply see whether fluctuations in giving shift program expenditures, or mostly affect other expenditures. However, if charities prefer to smooth their expenditures over time — perhaps because adjustments to program service capacity over time are costly, or perhaps for other reasons (*e.g.* risk-averse trustees) — then it is reasonable for charities to smooth unpredictable revenues using their endowments. If charities are expenditure-smoothing, it is possible that changes to the environment for charitable contributions will not affect program expenditures with a lag, as charities draw down their endowments to ensure a "soft landing." The following section will demonstrate that in the raw data, changes in contributions are associated with changes in saving, not changes in program expenditures, in contradistinction with program service revenues and other sources of income. Discussion of charities' propensity to adjust program expenditures will therefore need to pay attention to how any effects change over time.

3.2 Do Charitable Contributions Increase Program Expenditures?

The previous section established a pattern in charities' income statements: contribution revenues are volatile, and not because of patterns in fundraising. This section establishes a second fact, this time about patterns in charities' balance sheets: charities save increases in

charitable contributions rather than spend them, whereas increases in revenue from other sources tend to be spent on program expenditures, not saved.

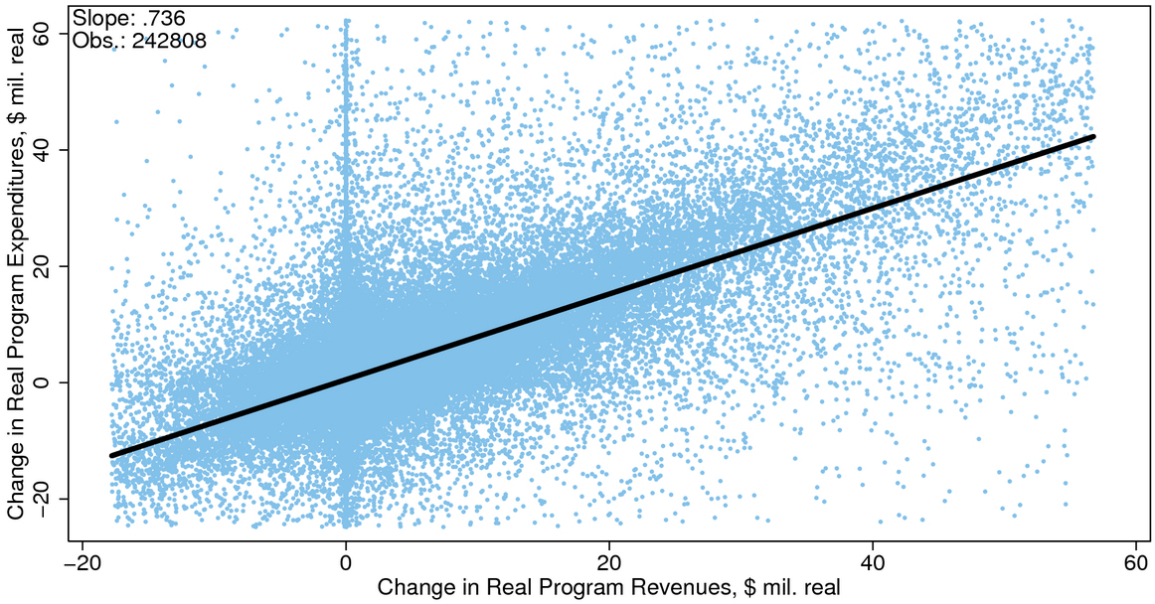
Figure 3.3 plots the change in dollars in two possible uses of charities' revenues — program expenditures and savings (measured by change in net assets) against changes in revenue from program services or from direct charitable contributions.[6] The most extreme one percent at the top and bottom for either variable are not plotted to make the core of the distribution more visible. A black line through each plot shows the least-squares fit through the distribution. Because the scales of the axes and the number of organizations with complete observations change across panels, the slope of the linear fit and the number of observations is printed in the top left corner of each plot. Each observation is the change for one organization in one pair of consecutive periods; if an organization is observed over 20 consecutive years, it appears in the plot 19 times.

The first four panels of figure 3.3 reveal a distinction between the uses of program service revenue and of direct contributions. A one-dollar increase in program service revenue is associated with an increase of $0.74 in program expenditures (figure 3.3A), but only $0.06 in flows to net assets (figure 3.3B). In contrast, an extra dollar in direct contributions is associated with just $0.15 in additional program expenditures (figure 3.3C), but an additional $0.73 in flows to net assets (figure 3.3D). This relationship is not solely a function of expanding program capacity increasing revenues and expenditures at the same time: additional panels show that charities tend to increase program expenditures by $0.74 for a one-dollar increase in government grants (figure 3.3E), by $0.51 for a one-dollar increase in grants from foundations or other public charities, such as United Ways (figure 3.3F), and by $0.54 for a one-dollar increase in revenue from investments (figure 3.3G). Direct charitable contributions are unique in having a weak relationship between program expenditures

[6] "Net assets" or "total fund balances" is the difference between the book value of a nonprofit's assets and its liabilities. This difference on a for-profit balance sheet is called "shareholder equity," but of course nonprofits have no shareholders.

Figure 3.3: Changes in Expenditures vs. Changes in Revenues (Outliers Censored)

A. Change in Program Expenditures vs. Change in Program Service Revenue (Outliers Censored)



B. Change in Flows to Total Fund Balance vs. Change in Program Service Revenue (Outliers Censored)

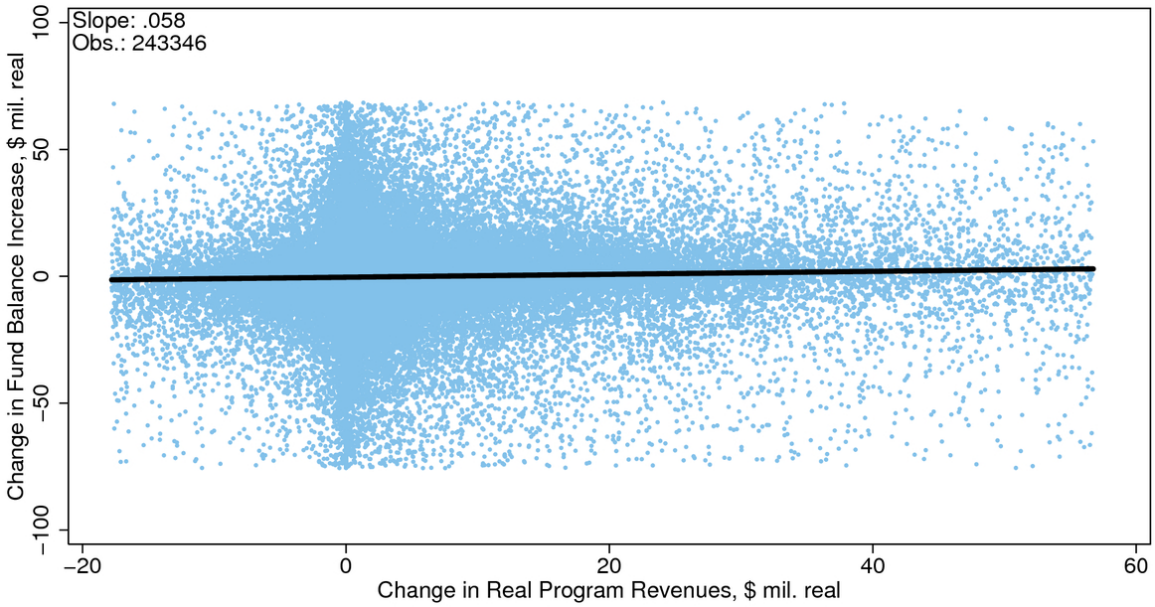
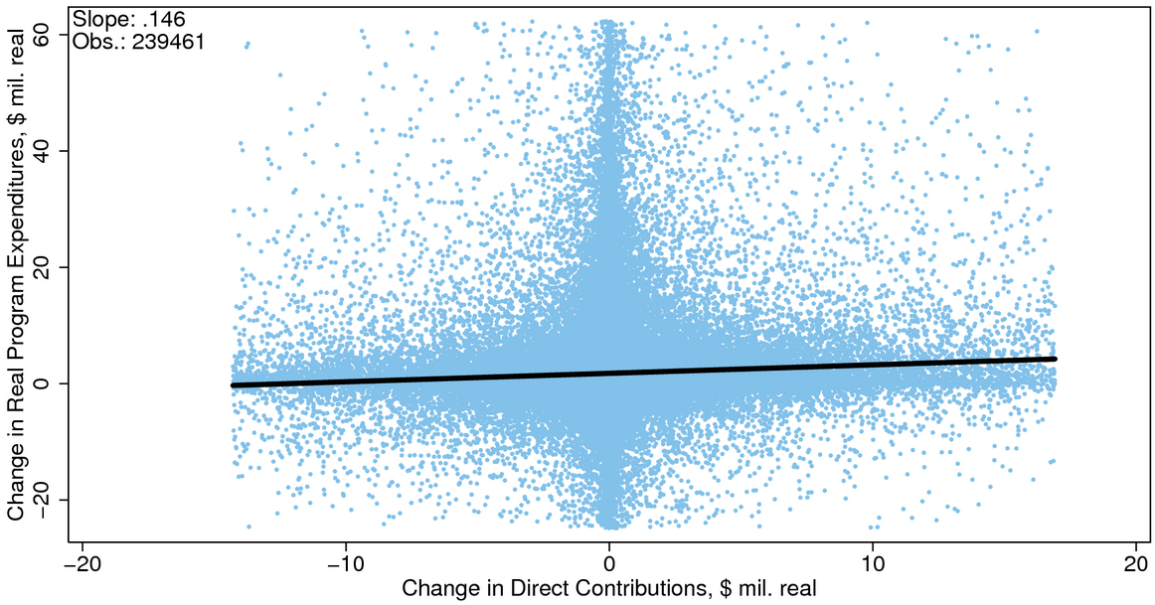


Figure 3.3 — Continued

C. Change in Program Expenditures vs. Change in Direct Contributions (Outliers Censored)



D. Change in Flows to Total Fund Balance vs. Change in Direct Contributions (Outliers Censored)

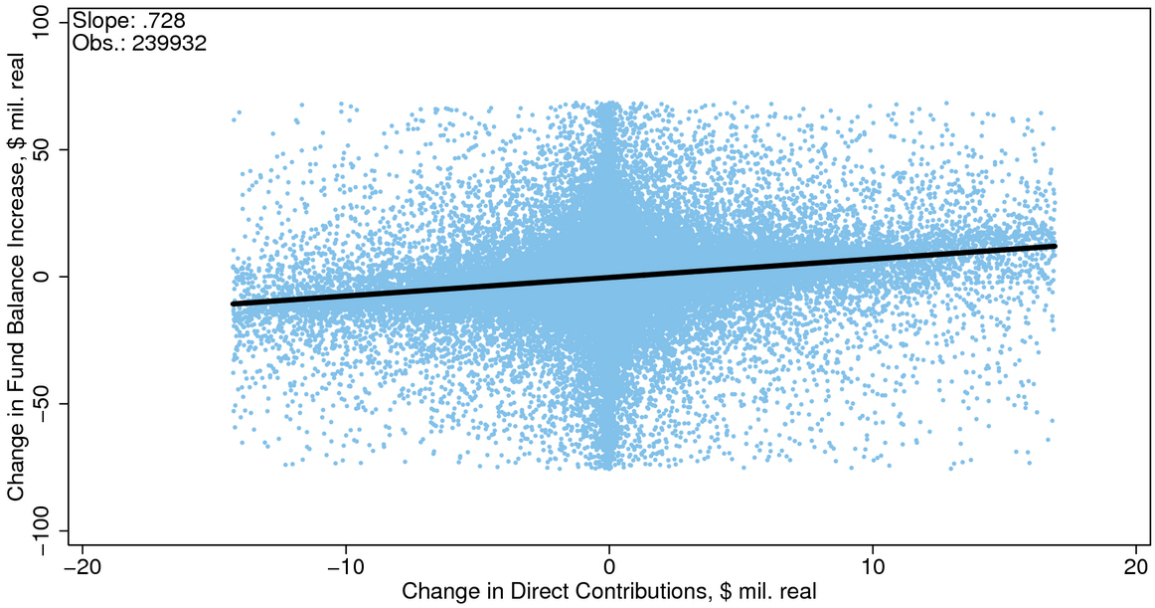
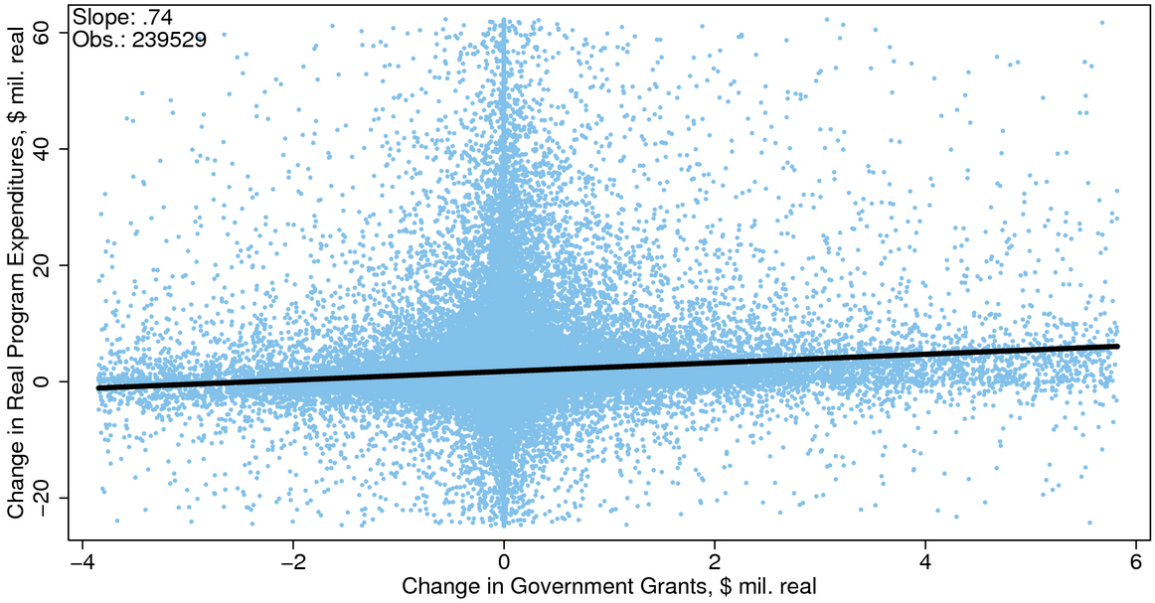


Figure 3.3 — Continued

E. Change in Program Expenditures vs. Change in Government Grants (Outliers Censored)



F. Change in Flows to Program Expenditures vs. Change in Indirect Contributions (Outliers Censored)

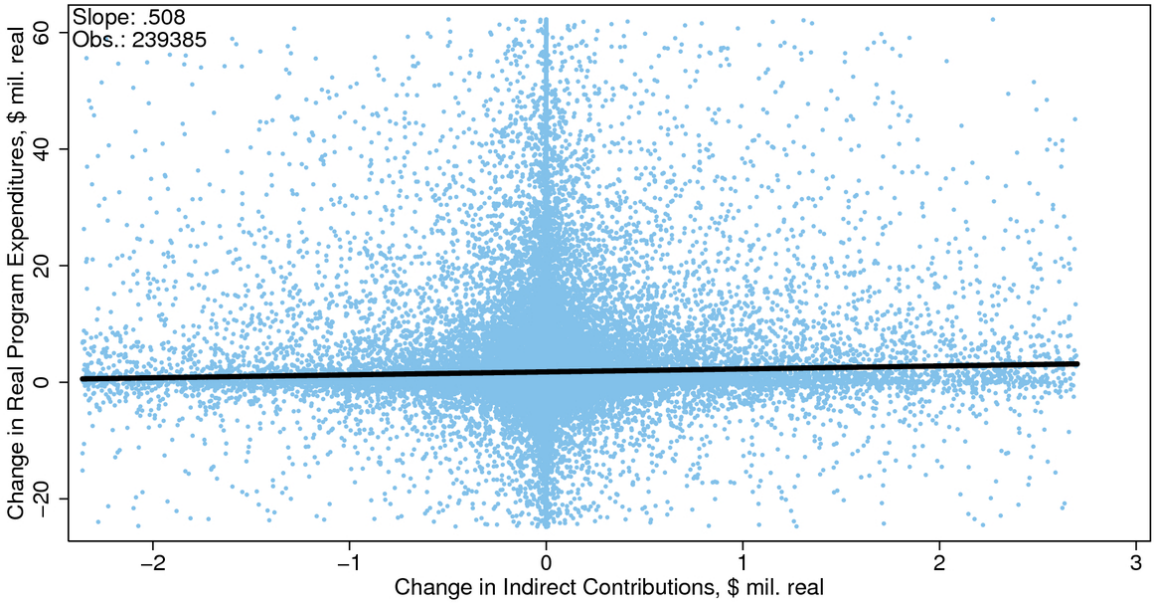
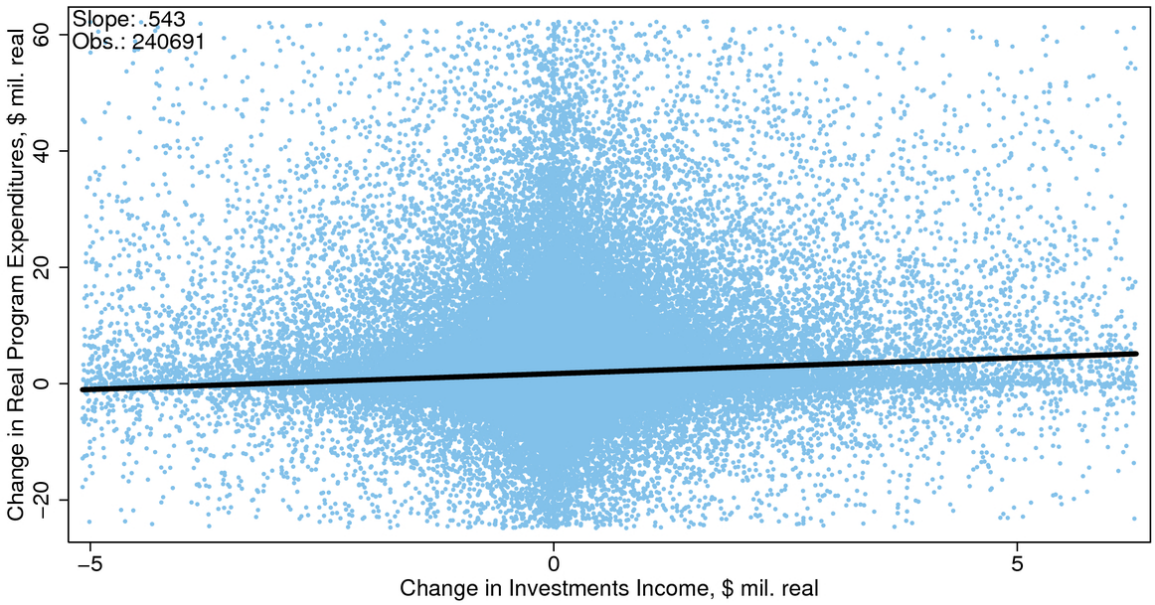


Figure 3.3 — Continued

G. Change in Flows to Program Expenditures vs. Change in Investment Income (Outliers Censored)



Notes: All variables are differenced across consecutive observation periods for one of four financial flows taken from the IRS form 990, adjusted for inflation using the consumer price index. All variables are in year 2012 dollars. Program expenditures, direct contributions, and program service revenue are all as reported. Flows to Fund Balances is calculated by taking the difference between the charity's net assets or total fund balances (akin to shareholder equity of a for-profit firm) at the beginning and the end of the fiscal year; both values are reported on each form 990. The black line represents a least-squares fit through the plotted points. The slope of the fitted line is printed in the top left corner of the plot. Any observation in the top or bottom one percent of observed values for either change in flow variable is omitted. See appendix figure B.1 for the same charts with outliers retained, and appendix figure B.2 for plots normalized to organizations' means.
Sources: National Center for Charitable Statistics, IRS Statistics of Income and 990 Core Files.

and revenue flows.[7]

The relationships between program expenditures, saving, program revenue, and contributions is investigated more carefully in table 3.2, which presents regression results from a

[7]The panels of figure 3.3 trim the top and bottom one percent of both variables for ease of viewing, which does not change the results; appendix figure B.1 leaves extreme values in without changing the observed relationships. One may also be concerned that, by leaving changes denominated in dollars, this result is driven by large organizations that are likeliest to have large changes to their financial variables in dollar terms; appendix figure B.2 plots changes in variables in terms of standard deviations from the mean of each organization, without a qualitatively different finding.

regression of the form

$$\Delta Y_{i,t} = \alpha_i + \boldsymbol{\beta}\Delta\mathbf{R_{i,t}} + \varepsilon_{it} \tag{3.2}$$

where $\Delta Y_{i,t}$ is a the change in flow variable for a use of charitable funds (program expenditures or change in net assets) for charity i from period $t-1$ to t; $\Delta\mathbf{R_{i,t}}$ is the change in vector of revenue sources $\mathbf{R}$ for charity i from period $t-1$ to t, α_i is a fixed effect for organization i, implemented by demeaning all regression variables, and ε_{it} is a residual. Vector of revenues $\mathbf{R}$ includes all the major revenue streams of charities, allowing analysis of the relationship between any one revenue source and program expenditures and savings holding other revenues constant.

Because there are a few extreme values in most change variables, results are computed using both ordinary least squares and "robust regression," which places less weight on very influential observations.[8] Huber-White heteroskedasticity-robust standard errors are presented for the OLS results, bootstrapped standard errors for the robust regrssions. As in figure 3.3, both the dependent variable and independent variable of interest are in real dollars, and the coefficient of interest can be interpreted as the change in dollars for a particular expenditure item associated with a one-dollar change in a particular revenue source.

Consistent with figures 3.3B and 3.3A, a dollar increase in program service revenue is associated with an increase of 0.89 dollars in program expenditures (0.79 dollars if outliers are downweighted), but the change in organizations' balance sheets is tiny: negative and statistically insignificant under OLS, 0.06 dollars saved per dollar of program service revenue with reweighting. Conversely, a dollar change in contribution revenue is associated with an increase of 0.28 dollars of program expenditure and 0.67 dollars of increases

[8]Robust regression is implemented using the `rreg` Stata function, which iteratively downweights observations with residuals beyond a fixed value proportionally to the inverse of the residual; default reweighting parameters are used.

Table 3.2: Changes in Expenditures vs. Changes in Revenues

	Δ Program Expenditures		Δ Flows to Fund Balances	
	OLS	Robust Reg.	OLS	Robust Reg.
	(1)	(2)	(3)	(4)
Δ Program	0.893	0.786	-0.044	0.064
Service Revenue	(0.025)	(0.017)	(0.195)	(0.011)
Δ Direct	0.276	0.003	0.656	0.962
Contrib.	(0.089)	(0.002)	(0.10)	(0.009)
Δ Indirect	0.094	0.007	0.822	0.915
Contrib.	(0.028)	(0.010)	(0.206)	(0.054)
Δ Government	0.729	0.701	0.168	0.168
Grants	(0.046)	(0.030)	(0.144)	(0.036)
Δ Investment	0.067	0.012	-1.366	0.904
Income	(0.120)	(0.007)	(1.694)	(0.070)
Δ Dues and	0.853	0.764	-0.393	0.070
Memberships	(0.047)	(0.049)	(0.470)	(0.019)
Δ Other	0.049	-0.001	1.128	0.883
Income	(0.014)	(0.003)	(0.214)	(0.028)
Constant	0.000	-0.019	0.000	0.081
	(0.033)	(0.002)	(0.204)	(0.004)
Observations	248774	248747	248774	248753
R-squared	0.652	0.998	0.104	0.997
Organization Fixed Effects	Y	Y	Y	Y

Notes: All variables are differenced across consecutive observation periods for one of four financial flows taken from the IRS form 990, adjusted for inflation using the consumer price index. All variables are in year 2012 dollars. Program expenditures, direct contributions, and program service revenue are all as reported. Flows to Fund Balances is calculated by taking the difference between the charity's net assets or total fund balances (akin to shareholder equity of a for-profit firm) at the beginning and the end of the fiscal year; both values are reported on each form 990. All observed values are used for all regressions, and number of observations changes slightly when one variable is not observed or outlier-robust regression assigns zero weight. Outlier-robust regression is implemented using Stata's `rreg` command. Huber-White heteroskedasticity-robust standard errors are presented for OLS regressions. Standard errors for outlier-robust regressions are bootstrapped (100 repetitions).
Sources: National Center for Charitable Statistics, IRS Statistics of Income and 990 Core Files.

in net assets; if influential observations are downweighted, an increase in contributions is associated with just 0.003 dollars of program expenditures and 0.96 dollars of net assets. Consistent with figures 3.3D and 3.3C, changes in contributions are overwhelming associated with increases in flows to net assets, not in expenditures. The uses of these revenue sources are very different.

The tendency of charities to save one type of revenue while spending the other is striking — whatever the source, a dollar of one kind of revenue should be just as appropriate for serving the charities' mission as any other dollar.[9] Not only are contributions more volatile than other revenues, as shown in section 3.1, they are spend differently.

3.3 IV Estimates of Contributions' Effect on Program Expenditures

Chapter 2 documented an abrupt decline in charitable contributions to charities in states where the TRA86 affected tax incentives more intensely. Using the same sample described in section 2.3, I get IV (2SLS) estimates of the effect of a change in contributions on program expenditures using the following.

$$\ln(ProgramExpenditures_{it}) = \beta_0^i + \beta_1 \ln(Contributions_{it}) + \mathbf{Controls}'_{st}\boldsymbol{\beta_2} + u_{it} \tag{3.3}$$

$$\ln(Contributions_{it}) = \delta_0^i + \delta_1 \Delta_{86-88} \ln(TaxCost_{s(i)}) * Post86_t + \mathbf{Controls}'_{st}\boldsymbol{\delta_2} + r_{it} \tag{3.4}$$

[9] One possible exception is if a charity receives a gift restricted by donor wishes to an endowment only, that is, that is not to be drawn down and spent. However, such a constraint should only affect charities that cannot shift unrestricted funds. For instance, a wealthy private college which regularly requests donations from me offers a menu of purposes I would like to designate my contribution toward — *e.g.* financial aid, athletics, or performing arts — but the institution has a large general fund that can be reallocated among these goals. Conti-Brown (2010) notes that over half of large university endowments are comprised of unrestricted funds, giving these institutions wide leeway to shift resources in response to a restricted gift. The ability to designate funds for one goal or another is either an expressive act or possibly a fundraiser's trick, but is not likely to constrain this college's budgetary choices.

where $ProgramExpenditures_{it}$ is the program expenditures of charity i in year t plus \$25,000. Addition of a constant allows the use of the same estimation sample as table 2.2; otherwise, 1.6% of observations with zero program expenditures in a particular year would be dropped after taking logs. $Contributions_{it}$ are direct contributions received by the charity in that period, and instrumental variable $\Delta_{86-88} \ln(TaxCost_{s(i)}) * Post86_t$ is the change in representative tax cost interacted with a post-period dummy. Organization fixed effects are denoted β_0^i and δ_0^i. Additional controls $\mathbf{X}'_{st}$ vary by specification, always including either year fixed effects or region-by-year fixed effects, as well as a vector of time-varying macroeconomic variables by state and year in some specifications (including gross state product, unemployment rate, population, poverty rate, and per capita income). Residuals are denoted u_{it} and r_{it}. OLS standard errors are clustered by state; 2SLS standard errors are bootstrapped, with 100 replications.

Both OLS estimates of equation 3.3 and corresponding two-stage least-squares IV estimates are presented in table 3.3.[10] The OLS estimates vary little with the addition of various sets of control variables, consistently obtaining an elasticity of about 0.016. In the estimation sample, direct contributions has a mean of \$1.08 million (in 2012 dollars) while program expenditures has a mean of \$18.6 million; at the means, an elasticity of 0.016 implies that an additional dollar of contributions is associated with an extra \$0.28 in program expenditures.[11] Even if the true elasticity were at the high end of the 95% confidence intervals (about 0.03), this still suggests only about half of an extra dollar of contributions goes to program expenditures.

The IV point estimates are two to ten times larger than the corresponding OLS estimates, although the much larger (bootstrapped) standard errors mean these estimates are not statistically different from the OLS estimates or from zero. Such wide confidence in-

[10] Table 2.2, which presents regressions of contributions on the tax cost instrument, is equivalent to first-stage results.

[11] $\frac{18.6}{1.08} \times 0.016 \approx 0.276$

tervals require cautious interpretation, although the large difference in magnitude suggests OLS could be biased downward, suggesting an omitted variable (such as financial distress) may be correlated with endogenous decisions to increase contribution revenues and lower program expenditures.

Table 3.3: IV Estimates of Effect of Additional Contributions on Program Expenditures

	(1)	(2)	(3)	(4)
	Log Program Expenditures: Ordinary Least Squares			
Log Direct Contributions	0.0159**	0.0159**	0.0161**	0.0158**
	(0.0067)	(0.0072)	(0.0071)	(0.0073)
R-squared	0.92	0.92	0.92	0.92
	Log Program Expenditures: Instrumental Variables			
Log Direct Contributions	0.0387	0.113	0.112	0.157
	(0.210)	(0.183)	(0.300)	(0.221)
F-statistic	46.54	45.04	45.35	43.83
Observations	16882	16882	16882	16882
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓

Notes: Dependent variable is log of real expenditures on exempt programs plus $25,000. *Contributions* is log of real direct public support. "Macro Controls" are a set of macroeconomic variables observed in each state and year: log gross state product, log state population, log unemployment rate, log poverty rate, and log per capita income. See the discussion in section 2.3 for more detail on the construction of these variables. First-stage results for regression of contributions on instrument for tax cost of giving are equivalent to table 2.2. *** p <0.01, ** p <0.05, * p <0.1. OLS standard errors are clustered by state. IV standard errors are bootstrapped (100 replications).
Sources: See notes to table 2.2.

Unfortunately, it is not obvious that this IV's exclusion restriction is satisfied. If the tax cost of giving affects the productivity of expenditures on fundraising, then the instrument may also affect program expenditures by leading charities to decrease their fundraising expenditures and spend those marginal dollars on additional programs.[12] The "real" equations

[12]Alternatively, if the charity is liquidity constrained or otherwise dependent on contributions, it might

of interest may therefore be something like

$$\ln(ProgramExpenditures_{it}) = \beta_0^i + \beta_1 \ln(Contributions_{it}) + \beta_2 \ln(Fundraising_{it}) + \mathbf{Controls}'_{st}\boldsymbol{\beta_3} + u_{it}$$

$$\ln(Contributions_{it}) = \delta_0^i + \delta_1 \ln(TaxCost_{it}) + \delta_2 \ln(Fundraising_{it}) + \mathbf{Controls}'_{st}\boldsymbol{\delta_3} + r_{it}$$

$$\ln(Fundraising_{it}) = \lambda_0^i + \lambda_1 \ln(TaxCost_{it}) + \mathbf{Controls}'_{st}\boldsymbol{\lambda_3} + \nu_{it}$$

This system is not identified with a single instrument for *TaxCost*, but may be estimable using an additional instrument for fundraising expenditure. Andreoni and Payne (2011) instrument for fundraising expense using measures of charities' "financial security." This approach may be promising, although its validity depends on donor perceptions of charities' financial stability either being unrelated to their giving decisions or varying little over time (and thus absorbed into fixed effects). Further investigation is necessary to determine whether Andreoni and Payne's strategy is appropriate for this estimation, or whether another instrument for fundraising is available.

Instead, I return to the methodology employed in section 2.4, which looks at time-varying associations between the change in tax cost of giving and direct contributions. If we similarly look for such associations between program expenditures and tax cost, we can take such estimates as the total effect of tax reform through all causal channels. This approach will also allow us to see whether charities smooth a post-1986 shortfall in contributions using their endowments, leading to different short- and long-term effects of tax reform on program expenditures.

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fundraise *more* and spend *less* on programs.

3.4 The Long-Run Impact of Tax Reform on Charities' Finances

The previous sections established that there is not a clear link between additional direct contributions and program expenditures. However, to the extent that contributions are endogenous, this failure to find a correlation may be spurious. I will now look for causal effects of the charitable contribution deduction on other elements of nonprofits' finances, using the same time-varying continuous-treatment difference-in-difference framework of section 2.4. Specifically, I use a regression specification similar to equation 2.3:

$$Y_{it} = \alpha_i + \delta_t + \mathbf{X}'_{\mathbf{st}}\boldsymbol{\gamma} + \sum_{t \in 1982, `83, `86, `87, \ldots, 2007} \beta_t (\Delta_{86-88} TaxCost_{s(i)}) * \mathbf{1}\{year = t\} + \varepsilon_{it} \tag{3.5}$$

where Y_{it} is a logged dependent variable from the form 990 of organization i in year t; $\Delta_{86-88} \ln(TaxCost_{s(i)})$ is the change in the log mean tax price of giving $(1-\tau)$ from 1986 to 1988 in state s; $\mathbf{1}\{year = t\}$ is equal to 1 if $y = t$ and 0 if $y \neq t$; δ_t is a year effect; and α_i is an organization fixed effect. All regressions use, to the greatest extent possible, the same sample as the regressions in figure 2.6, described in section 2.3.[13]

The coefficients of interest are β_t, which capture the change in variable Y in year t caused by the change in charitable contribution tax incentives following the TRA86, relative to the difference across states in 1985 (the year before the bill became law and its terms known). As discussed in section 2.4, we should expect β_{1982} and β_{1983} to be zero. However, we should not necessarily expect β_t to be nonzero immediately following the tax reform, if the financial variable of interest responds to tax reform with a lag. Point estimates for each regression are presented graphically in the panels of figure 3.4. Dashed lines represent pointwise 95 percent confidence intervals, clustered by US state. Appendix table B.2 presents these point estimates and clustered standard errors as text.

[13] Appendix B presents results for alternative samples of form 990 data and for additional dependent variables, as well as tabulating point estimates and standard errors for results which are here presented graphically.

Figure 3.4A plots estimates of year-varying treatment effects on the log of program expenditures. As with other variables, and consistent with the finding that stock of assets is the pathway for contributions' effect on program expenditures, organizations' spending on exempt purposes declines slowly but steadily for years, reaching its greatest extent in 1997, ten years after TRA86 passed into law. In that year the time-varying elasticity is estimated to be -4.1 to -4.5, which translates into an interquartile difference in program expenditures of 10 to 11 percent.

This is a substantial effect. In 1997, program expenditures by all public charities filing the form 990 exceeded $571 billion in real (2012) dollars (Internal Revenue Service 2011a). Even if we conclude that elasticity estimates for the sample may not be representative of the nonprofit sector as a whole (see the discussion in section 2.6), even a *one percent* difference in program expenditures in this period is equal to 5.7 billion real dollars, an amount roughly equal in size to the entire 1997 federal appropriation for the State Children's Health Insurance Program (S-CHIP).[14] Furthermore, the time-varying estimates presented in figure 3.4A reveal an increasing pretrend in states with the more intense treatment, suggesting that these estimates may be *too small* if they are confounded by a countervailing trend.

And yet, the *immediate* effect on program expenditures is smaller. Estimates for time-varying elasticities across the four sets of covariates in years 1988 to 1990 range from -0.9 to -1.6, or one-third to one-quarter the magnitudes observed in 1997. Taking the elasticities for 1996 to be the "long run," the estimates shown in figures 2.6 and 3.4 imply a long-run elasticity of program expenditures of approximately 0.6; at the means for this period, this means that for every dollar in contributions unobserved because of the tax change, about seven dollars in program expenditures is unobserved.[15] Obviously, a greater than one-to-

[14] 105th Congress Public Law 33, "Balanced Budget Act of 1997," §2104. The law appropriated $4.275 billion for fiscal 1998, or $6.085 in 2012 dollars, adjusted using December values of the CPI. `http://www.gpo.gov/fdsys/pkg/PLAW-105publ33/html/PLAW-105publ33.htm`.

[15] Calculated using the elasticities presented in appendix table B.2 for year 1996: $\varepsilon_{Programs,Contributions}$ =

one ratio cannot be supported by a *direct* reduction in contribution spending. Instead, this may mean that the saving behavior noted for charitable contributions is used to expand the capacity for future program expenditures. Such an interpretation is consistent with observing a smaller effect in the short run: in 1989, a year after the full phase-in of the TRA86, the equivalent ratio of elasticities is about 0.2, corresponding to a ratio of 2.4 dollars in program expenditures per dollar of contributions. [16] Still, this short-run ratio is greater than 1, implying that more than the direct spending of contributions on programs may be involved.

One complication for such an interpretation of the relative magnitudes is the evolution of the coefficients across states before the tax change. Unlike the time-varying coefficients for contributions presented in figure 2.6, there is a clear positive pretrend in the time-varying estimates of program expenditures. If we interpret this to mean that program expenditures were rising before 1986 for some reason correlated with, but independent of, the tax reform, then this implies that these estimates *underestimate* the effect on program expenditures! On the other hand, if there organizations in more heavily treated states coincidentally happened to increase their expenditures in the years leading up to the change — and would have backed off in the following years anyhow — then the effect of TRA86 on expenditures is *overstated*. In the absence of further evidence these results should be interpreted cautiously.

Next, we look at how the stock of net assets evolves in response to differential changes

$\varepsilon_{Programs,1-\tau}/\varepsilon_{Contributions,1-\tau} = -2.838 \times \frac{1}{-4.530} \approx 0.626$, that is,

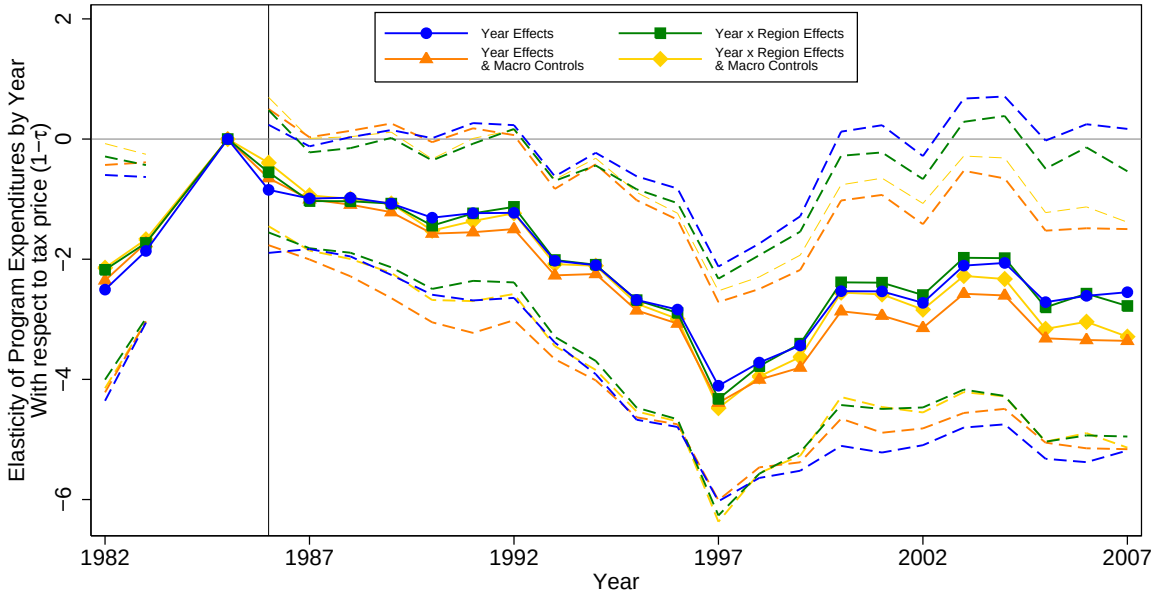
$$\frac{d\ln Programs}{d\ln Contributions} = \frac{\frac{d\ln Programs}{d\ln 1-\tau}}{\frac{d\ln Contributions}{d\ln 1-\tau}}$$

In 1996, mean values within the sample of direct contributions and program expenditures were \$5.3 million and \$64.8 million, respectively, in 2012 dollars, a ratio of 12.2 dollars of program expenditures for every dollar of contributions.

[16] $\frac{-1.074}{-5.331} \approx 0.201$. The within-sample ratio of program expenditures to contributions is also about 12 in 1989.

Figure 3.4: Year-varying Tax Price Elasticities of Nonprofit Financial Variables

A. Tax Elasticities by Year, Log Program Expenditures



B. Tax Elasticities by Year, Net Assets (Log-level)

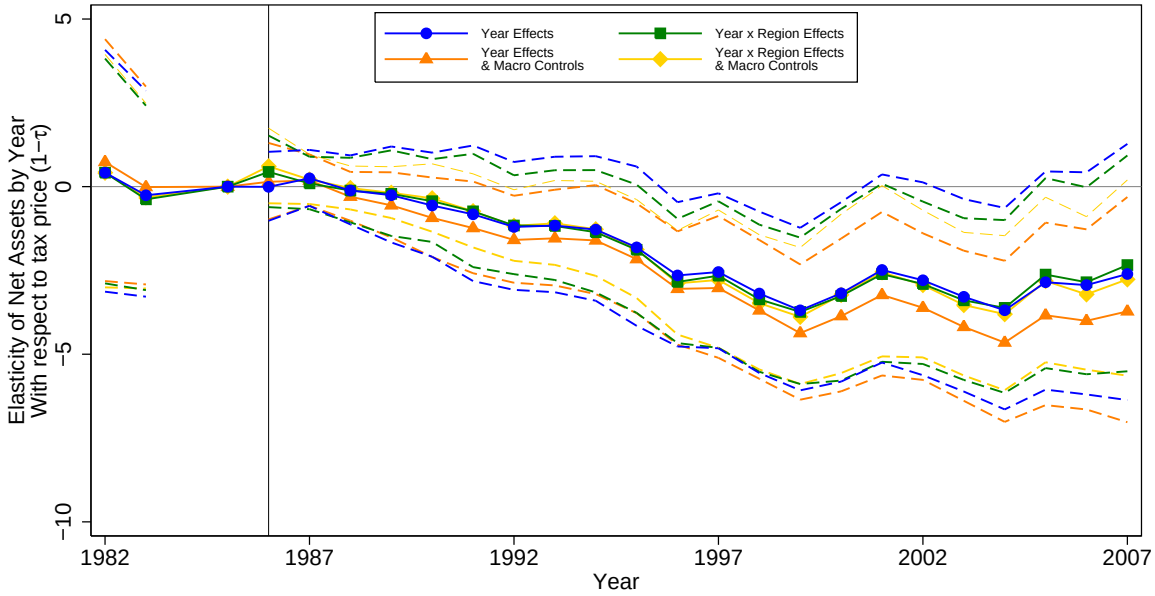
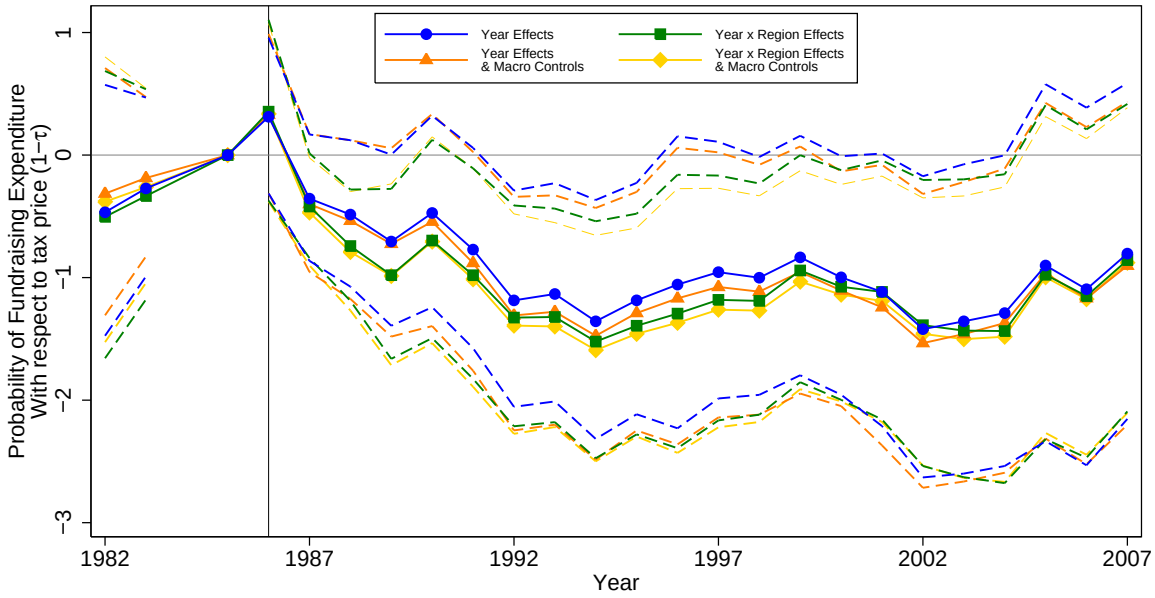
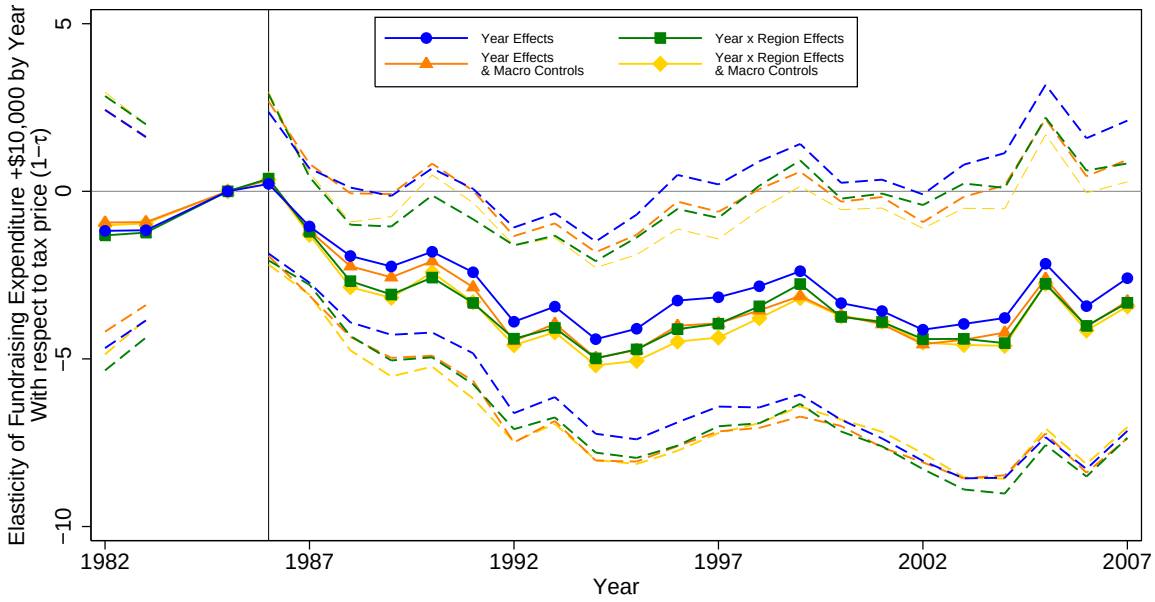


Figure 3.4 — Continued

C. Tax Elasticities by Year, Fundraising (Extensive Margin)



D. Tax Elasticities by Year, Fundraising Expenditure (Log + $10,000)



Notes: Figures present coefficients for year-varying effects of 1986 tax rate changes on nonprofit financial variables: log of net assets or total fund balances (A), a binary variable equal to 1 if a charity had strictly positive fundraising and zero if zero fundraising (B), log of fundraising expenditure plus $10,000 (C), and log of program expenditures (D). Point estimates for four different specifications with and without state-year macroeconomic variables and region-by-year effects are presented. Dashed lines represent 95% pointwise confidence intervals, clustered by US state. See sections 2.3 and 2.4 for a description of the sample selection. *Sources:* NCCS Statistics of Income and Core Files 990 Data.

in the contribution deduction. Graphical evidence in section 3.1 suggested that charities save rather than spend contribution revenues. Following the large and rapid decline in contributions caused by the 1986 tax reform (see section 2.3), we might see charities draw down their savings to dampen the shock. On the other hand, in the short run charities might save more in response to the reduced appeal of fundraising. Figure 3.4B plots the relationship between state-level variation in the TRA86 treatment effect on end-of-year net assets, measured by time-varying treatment coefficients β_t.

Since net assets is a stock, rather than a flow, one would not expect to see a discontinuous drop after 1986, and the estimates do not show one. Instead, the gap among the states by treatment intensity widens each year (β_t declines) for over ten years following the law. Consistent with the observation in the raw data, organizations receiving lower contributions see their financial position grow more slowly (or shrink faster) than less affected charities. The estimated difference reaches its largest magnitude of -3.7 to -4.4, varying by the set of controls, in 1999. To give this number some perspective, the interquartile range in the log tax change is about 0.025, meaning that an elasticity of -3.7 implies a gap of about 9.2 percent in charities' net assets across the interquartile range by 1999.[17]

Lastly, as noted in the preceding section, we may be concerned that charities change their fundraising intensity strategically if changes in tax incentives reduce the return on fundraising. Analysis of fundraising behavior is complicated by the high share of charities which, though they receive direct contributions, do not spend money on fundraising: of the 81,264 observations meeting all other sample selection criteria, 78,201 have nonmissing observations for log net assets, and 68,110 have nonmissing observations for log of direct contributions.[18] In contrast, barely more than half of all observations, 41,152, have values

[17]One-quarter of organizations observed in 1986 have a log tax change in the tax cost of a charitable contribution greater than or equal to California's (0.1981); one-quarter have a change less than or equal to Kentucky's (0.1734). The difference between these percentiles is about 0.025 log-points.

[18]All 3,063 missing observations for log net assets are undefined because net assets was a nonpositive number. For direct contributions, 5,439 reported a nonpositive number, while 7,715 did not report direct

for log of fundraising expenditure.[19] Because the decision whether or not to engage in fundraising at all may be important, I present two more sets of results, one focused on the extensive margin, the second adding a constant before taking logs to avoid composition errors.

Figure 3.4C presents estimates for linear probability model regressions with a dependent variable equal to 1 if the organization *i* reported positive fundraising expenditure in year *t*, and zero if it reported zero expenditure. Relative to the pre-period, the likelihood that an organization chose to spend money fundraising falls in the years after the TRA86, stabilizing about five years later. Over 1992 to 1999, year-varying coefficients across the four models range from -1.6 to -0.8; across the interquartile range of state treatment variables, this suggests a differential of two to four percentage points in the change in likelihood of engaging in paid fundraising.

Figure 3.4D presents the regression results for the entire sample where the dependent variable is the log of $10,000 plus total fundraising expenditure. As discussed in section 2.5, the addition of $1 or $10 before taking logs, as is common in the individual tax literature, is inappropriate for the larger dollar amounts handled by nonprofit organizations, and so a bigger additive constant is used.[20] Consistent with the results in figure 3.4C, the pooled data also show a decrease in fundraising for organizations in states with a greater treatment effect. Point estimates in the years following TRA86 for both fundraising specifications are statistically different from β_{1985} but generally not β1982 or β_{1983}; in the absence of a longer pre-period or other evidence, it is ambiguous whether the fundraising response is understated by a pre-trend associated with the size of the following tax change, or whether 1985–6 is an unusual period and fundraising activity reverts to an earlier, stable level.

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contributions, largely because they were drawn from the IRS Core files (which do not report that variable).

[19] 39,965 reported nonpositive fundraising expenditures, while 147 reported a missing value.

[20] For example, the first percentile of strictly positive fundraising expenditure observed is $2,542; the difference between the log of 2552 and 10 is greater than the log difference between 2552 and the median ($533,367). Regressions with additive constants, and constant samples, are presented in appendix B.

3.5 Discussion

This chapter has established a new stylized fact about the behavior of nonprofits — that contributions are associated with greater saving, while program revenues are associated with greater program expenditures. This observation may signify that charities smooth their program expenditures across time, as contributions are charities' most unpredictable revenue streams. If charities are expenditure-smoothers, it follows that a reduction in charitable contributions following a tax cut may not immediately translate into a reduction in program expenditures. An attempt to measure the effect of the TRA86 on program expenditures via its effect on charitable contributions using two-stage least squares yielded estimates too imprecise to be definitive.

Using the same estimation strategy as section 2.4, I show that the Tax Reform Act of 1986 did not immediately affect program expenditures, but rather diminished them with a ten-year lag, though a surprising pretrend in the data before 1986 suggests these results should be interpreted cautiously. The fundraising of charities in more strongly treated states did decline after 1986, but the effect was primarily observed on the extensive margin as more intensively treated organizations were less likely to engage in paid fundraising following the reform. These lagged effects have implications for future reforms to the charitable contribution deduction and the level of service provision. Because charities respond to changes in the giving environment by at least partially smoothing using their endowments, tax incentives for voluntary contributions are likely to be an ineffective vehicle for changing the level of services provided by a large amount, quickly. Further research on this question should consider not only changes in donation behaviors by the dynamics of service provision implied by policy changes.

CHAPTER 4

How Johnson Fought the War on Poverty: The Politics and Economics of Funding at the Office of Economic Opportunity[1]

In his first State of the Union address in January 1964, President Lyndon B. Johnson asked Congress to declare an "unconditional war on poverty" and to aim "not only to relieve the symptom of poverty, but to cure it and, above all, to prevent it" (1965). Over the next five years, Congress passed legislation that transformed American schools, launched Medicare and Medicaid, and expanded housing subsidies, urban development programs, employment and training programs, food stamps, and Social Security and welfare benefits. These programs more than tripled real federal expenditures on health, education, and welfare, which grew to over 15 percent of the federal budget by 1970 (Ginzberg and Solow 1974).

Using the volumes of oral histories, taped conversations, and archival documents, historians have pieced together competing (but not mutually exclusive) narratives of this

[1]This chapter is adapted from Martha Bailey and Nicolas Duquette, "How Johnson Fought the War on Poverty: The Politics and Economics of Funding at the Office of Economic Opportunity," *Journal of Economic History* 74(1):351–88, June 2014. A preprint version of this paper is also available as NBER Working Paper #19860. Appendices C and D are taken from the corresponding online appendices of Bailey and Duquette (2014).

decade's political economy (Levitan 1969; Ginzberg and Solow 1974; Gettleman and Mermelstein 1966; Caro 2012, 2002, 1982; Gillette 1996; Davies 1996; O'Connor 2001; Germany 2007; Orleck and Hazirjian 2011). Economic historians attribute the policy shift in the 1960s to a long-term decline in Southern planters' demand for cheap agricultural workers and accompanying decline in plantation paternalism (Alston and Ferrie 1999, 1993). Relative to the large literature that examines the political economy of the New Deal, little quantitative research has considered the political economy of the War on Poverty: how and why it evolved from the small-scale, academic brainchild of the Council of Economic Advisors to a controversial and enduring legacy of the Johnson presidency.[2]

This paper contributes a novel quantitative description to the vast narrative, documentary, and oral history of the 1960s political economy. We analyze how the War on Poverty was fought through the lens of the legislation that came to define it: the 1964 Economic Opportunity Act (EOA).[3] This centerpiece legislation created the Office of Economic Opportunity (OEO) to coordinate federal antipoverty initiatives and empower the poor to transform their own communities. The EOA also contained two radical provisions that facilitate our analysis. First, the EOA apportioned funding across states according to an index, but it imposed no requirements on how and where to spend money *within* states. Second, the EOA enabled the federal government to fund local private and nonprofit organizations directly, rather than funneling money through state or local governments. This provision

[2]Many reasons recommend a separate treatment of the two periods. The New Deal was developed in response to high unemployment and the economic crisis of the Great Depression; Johnson launched the War on Poverty during a period of widely shared economic prosperity. The New Deal significantly expanded programs cooperatively administered between the federal and state governments (Fishback and Wallis 2012, p. 291), whereas the federal government retained the purse strings and discretionary power for many of the War on Poverty programs. Finally, the New Deal built on and expanded many existing national programs (public infrastructure, benefits to veterans, agricultural assistance, and emergency loans for farmers) and significantly expanded unemployment relief—programs that benefited the average American and median voter. In contrast, the War on Poverty made longer-term investments with less tangible effects for smaller subgroups. See Fishback et al. (2003) and Fleck (2008).

[3]Public Law 88-452, 78 Stat. 2642 and amendments. Public Law 89-794, 80 Stat. and Public Law 90-222, 81 Stat.

encouraged the development of customized programs to combat the root causes of local poverty and also allowed the federal government to work around widespread *de jure* racial segregation, which had restricted the political participation of African Americans, and *de facto* exclusion of the poor from the policy making process. These provisions relaxed many of the usual constraints on federal funding choices (for example, cooperation with state and local government officials). Observed funding choices, therefore, provide a great deal of information about the objectives of the Johnson administration during this transformative period of U.S. history.

Our analysis uses data on OEO grants from the National Archives and Records Administration (NARA), which we link to a variety of other data sources to describe the decade's complex political economy. These county-level data include measures of local demographic characteristics, political importance, local government expenditures and tax revenue per capita (U.S. Bureau of the Census 1964), riot intensity (Collins and Margo 2007), the escalation of the Vietnam war (casualty rates from U.S. Department of Defense 2008), and the intensity of sharecropping to proxy for Alston and Ferrie's (1993) paternalism hypothesis. Our quantitative findings show that a modest share of the within-state spending is explained by this rich set of covariates (5 to 9 percent versus roughly 37 percent during the New Deal). The variation explained by the model reflects systematic spending in higher poverty areas. After accounting for a variety of other county-level covariates, the extent and intensity of poverty significantly predict within-state, county-level CAP spending from 1964 to 1968 in both the South and the non-South.

An important and often forgotten component of the War on Poverty is its "assault on [racial] discrimination" (Council of Economic Advisers 1964, p. 56). The OEO monitored compliance with the Civil Rights Act and threatened violators with the withholding of federal funds. Interestingly, counties' share of population that was nonwhite in 1960 accounts for the bulk of explained, within-state variation in OEO funding nationally—a

pattern driven by the *non-South*. Together, poverty rates and the share nonwhite explain more of the within-state variation in OEO spending (roughly 30 percent) than do the more than twenty other covariates combined. In the non-South, these two variables alone account for over 60 percent of the explained, within-state variation in OEO spending.

Consistent with New Deal funding patterns, political considerations also influenced where OEO money was spent. We find that the Johnson administration invested in Democratic strongholds and rewarded areas with bigger swings in favor of the Democrats in the 1964 presidential election. Swing counties received slightly less funding overall, but swing counties *won* by Johnson in 1964 received slightly more OEO money *ceteris paribus*. But, although politics mattered, political considerations together explain surprisingly little of the county-level variation in OEO funding. Measures of political considerations explain no more than 1 percent of the variation in county-level spending at the national level. In short, the Johnson administration appears to have invested in nonwhite and poor areas rather than in Democratic strongholds, newly won districts, or the districts of powerful congresspersons. Our analysis of voting reinforces the notion that Johnson's War on Poverty failed to build a Democratic constituency in the short run. Although turnout increased in areas with higher spending, a greater share of votes in nonwhite areas went to Republicans. The paper concludes with a discussion of how these findings inform a better understanding of why the War on Poverty is remembered as a failure.

4.1 The Enactment and Provisions of the Economic Opportunity Act

Poverty emerged as a new and pressing social issue in the United States in the late 1950s and early 1960s (O'Connor 2001). Bestselling books on the topic, including *The Affluent Society* by Galbraith (1958) and *The Other America* by Michael Harrington (1962), as well as popular journal articles, catapulted the issue into the national consciousness.

Yet Johnson's motivations for championing the War on Poverty as the centerpiece of his domestic agenda have been subject to disagreement among contemporaries and historians.

The facts are straightforward. Johnson inherited a large legislative backlog from the John F. Kennedy administration. Arthur Schlesinger (1965) argues that Johnson continued what would have been Kennedy's poverty agenda. Yet Walter Heller, the chairman of Kennedy's Council of Economic Advisors (CEA), notes that only days before his Dallas assassination, Kennedy's thinking on the matter "had not gone beyond the vague concept of doing something that would focus specifically on the roots of poverty" (1970, pp. 19-20). In contrast, Heller recalls Johnson's unequivocal affirmation of the poverty program in his first briefing: "That's my kind of program. I'll find money for it one way or another. If I have to, I'll take away money from things to get money for people....Give it the highest priority. Push ahead full tilt" (p. 21). In the seven weeks between Kennedy's assassination and Johnson's State of the Union debut, the "War on Poverty" grew from a small, academic pilot program of the CEA to a core agenda of Johnson's presidency. In the next seven months, the EOA morphed from a draft bill into one of the most controversial pieces of legislation passed during Johnson's administration.

The Conception and Promotion of the Economic Opportunity Act

The EOA was the centerpiece of Johnson's War on Poverty and has been remembered as "the most dramatic and highly publicized of the Great Society's programs" (Levitan 1969, p. 3). It established the OEO, a new agency within the executive branch charged with initiating and coordinating government-wide antipoverty initiatives.

Shortly after Johnson's State of the Union address, he appointed Sargent Shriver, Kennedy's brother-in-law, to head the antipoverty task force. Within six weeks of his appointment, Shriver claimed that he had consulted with over one hundred different leaders in agriculture, business, labor, and civil rights groups; officials from various levels of gov-

ernment; and academics, administrators, and foundation representatives (Levitan 1969, pp. 30–31). Johnson's insistence that there be "no doles" and Shriver's commitment to doing things his way meant that social workers and welfare administrators—the embodiment of the old school of thought on reducing poverty—were omitted from this list or given little attention (p. 31). Shriver's task force drafted the final bill (with no input from Congress) and sent the draft EOA to Congress on 16 March 1964.

To promote the EOA, President Johnson embarked on a public relations tour. In April he visited the family of Tom Fletcher, an unemployed coal miner with a wife and eight children who lived in the hollows of Appalachia outside of Inez, Kentucky. The Fletchers had been chosen by the White House to become the face of American poverty—the faces of the 35 million Americans (roughly 20 percent of the population) who lived on less than $3,000 per year, roughly the poverty threshold for a family of four. Johnson is said to have remarked to a reporter, "I don't know if I'll pass a single law or get a single dollar appropriated, but before I'm through, no community in America will be able to ignore poverty in its midst" (Jordan and Rostow 1986, p. 16). Indeed, Walter Bennett's iconic Time Magazine photo that captured Johnson chatting with the Fletchers on their front porch achieved just that.

The Enactment of the Economic Opportunity Act

The passage of the EOA was swift and decisive. The Senate approved the bill on 23 July 1964 by a vote of 61 to 34 after only two full days of debate, in which a conservative coalition of Southern Democrats and Republicans succeeded in modestly reducing the authority of the OEO director.[4] On the House side, though final passage took only a few days,

[4] *The Congressional Quarterly's Weekly Report* states, "In a series of tight roll calls, the Democratic leadership turned back crippling 'states' rights' amendments by Sens. Winston L. Prouty (R. Vt.) and Spessard L. Holland (D. Fla.)...the final bill included two compromise states' rights amendments, offered by George A. Smathers (D. Fla.)...the first, adopted July 22 by voice vote, permitted the Governor of a state to veto the establishment of a Job Corps camp in his state, within 30 days after being notified of the project. The second, adopted July 23 by a 80-7 roll call, gave the Governor an identical veto power over all anti-poverty projects contracted between the Federal Government and a private agency. Contracts with public bodies, such as city

the process was more contentious. Levitan (1969, p. 40) notes that Republicans found the EOA hearings "frustrating" because only nine of the 69 primary witnesses opposed the bill. Moreover, Representative Adam Clayton Powell, Jr. (D-NY), chairman of the House Education and Labor Committee that received the bill, excluded Republicans from raising their objections in the hearings and from subsequent participation in the EOA's amendment.

Many congressmen objected to the concentrated power of the OEO director. On 17 March 1964, the first day of hearings before the House's War on Poverty subcommittee, Representative Robert Griffin (R-MI) asked Shriver,

> As much as we all admire your work and believe in your competence... I think we must... look at this legislation from the point of view that you may not always be the chief of staff...
>
> In every title of this bill, it provides that the Director shall establish criteria to achieve an equitable distribution of funds among the States. I see this as handing to the Director a blank check in terms of deciding how much money the various states are going to get...
>
> Do you have any idea at this time how you are going to distribute the money among the States? (U.S. House of Representatives 1964a, pp. 70–71).

Shriver sardonically replied that the concentration of power in his office conveniently solved the problem of distribution across states by making it easy for Congress to determine whom to fire if things went badly. Unsatisfied with Shriver's answer, Representative Peter Frelinghuysen (R-NJ) announced an alternative antipoverty bill on April 28, the last day of the hearings. His bill appropriated funds to antipoverty programs created by states (rather than the OEO) and apportioned funds across the states using an index based on total population, unemployment, and average income (Congressional Record 1964a).[5] Democrats ultimately compromised to include an apportionment index, but not the one Frelinghuysen proposed.

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councils and county committees, were not subject to the Governor's disapproval" (*Congressional Quarterly*, "Senate Passes Johnson's Anti-Poverty Bill, 61-34," 23 July 1964, pp. 1533-34).

[5] *Wall Street Journal*, "GOP Critic of Johnson's Drive on Poverty Offers Plan with Lower Federal Outlays," 29 April 1964, p. 5.

This compromise was enough to pass the EOA in the House (Gillette 1996, pp.121–23) On 5 August 1964, the Economic Opportunity Act (H.R. 11377) was introduced to the House floor with six hours for debate. Northern House Republicans spent much of their time decrying the power the EOA gave to the OEO director. One such remark in the Congressional Record was by Republican Robert Taft, Jr., of Ohio, who complained,

> [T]his attack which we are supposed to be launching upon poverty would enable the Director to do as he pleased....There's actually no requirement that the Director consult with anyone, other than to find some local agency of some sort, public or private, which would be willing to go along. If he did not have one available, he could create one (*Congressional Record* 1964a).

This was a prescient criticism of the provision that ultimately would give significant power to Shriver and the Johnson administration to exercise as they saw fit.

Southern Democrats occupied important posts in the House and Senate and had the power to block legislation—a power they had long exercised to protect the interests of the Southern elites (Katznelson 2013). They succeeded only in securing modest amendments—most notably the inclusion of a gubernatorial veto for key programs (§209[c]). Shriver recalled that Senator Herman Talmadge (D-GA), a former governor, suggested the veto as a way to let Southerners support the bill while neither alienating states' rights supporters nor "allow[ing] all this money to become bogged down in the state and local government apparatus, and...frustrated totally by the clique that might be hanging around a particular governor" (Gillette 1996, pp.129–30).[6]

[6]Southern Democrats ultimately voted for passage 60-40 in the House and 11-11 in the Senate (*Congressional Quarterly*. "Congress Clears Johnson's Anti-Poverty Bill," 14 August 1964, p. 1729-30; *Congressional Quarterly*. "CQ Senate Votes 218 through 223," 23 July 1964, p. 1567). In practice, the gubernatorial veto was rarely exercised and, when exercised, was so blatantly political that the gubernatorial veto was effectively removed only one year later. In the first year of the OEO's existence, the governors' veto was exercised just five times, including one widely publicized case in May 1965 when George Wallace made a point of blocking a grant to a racially integrated antipoverty program in Birmingham (*New York Times*, "Wallace Vetoes a Poverty Grant." 13 May 1965, p. 23; Levitan 1969, p. 62). The 1965 amendments defanged the gubernatorial veto by allowing the OEO director to override a veto if a grant was "reconsidered by the Director and found by him to be fully consistent with the provisions and in furtherance of the purposes of [the relevant portions of the EOA]." See Economic Opportunity Amendments of 1965, Public Law 89-253,

In contrast to state governors, local government had no power in the original EOA. Even though the U.S. Conference of Mayors and National Association of Counties had endorsed the original EOA with the reservation that funds be channeled through an official local poverty agency, the 1964 bill was never revised in this manner (Levitan 1969, p. 65). Local government had no direct, statutory power to block EOA spending or designate community groups until the EOA was later amended. The President's deftness at influencing the media—liberal and conservative—ensured that those opposed to the EOA looked like they were for poverty and against helping the poor. With the election looming in the fall of 1964 and the President's support surging, the amended 27-page EOA passed the House on 8 August 1964 with a vote of 226 to 185, just three days after it was introduced.[7]

The Radical Provisions and Financial Stakes of the Economic Opportunity Act

The EOA was an experiment on a grand scale. About half of the EOA's funding went to programs with a direct chain of command linking local organizations to Washington, such as Job Corps, Work-Study, and Volunteers in Service to America (VISTA); the other half went to the Community Action Program (CAP), which funded ideas put forth by local organizations that were to be customized to the needs of different communities.[8] The OEO designated over 1,000 Community Action Agencies (CAAs) between 1965 and 1968 to coordinate these locally customized antipoverty initiatives.

The CAP was the most novel and idealistic part of the War on Poverty and, unsur-

79 Stat. §16. In short, the OEO director could largely do as the administration pleased after 1965.

[7]Using newly assembled data on individual roll-call votes in the House and Senate (ICPSR 2010), measures of Democratic electoral strength (Clubb et al. 2006), and economic and demographic characteristics (Adler, undated) we find that party identity is the most important determinant of a favorable vote on the EOA. Southern Democrats were less likely to vote for the EOA than Democrats of other census regions, but were more likely to cast a favorable vote than Republicans from any region. We find a negative relationship between a positive EOA vote and share of black population—perhaps a prescient resistance to the imminent sea change in race politics encouraged by the EOA that would negatively affect Democrats in subsequent elections. In the House, we also find that unemployment rates were a strong predictor of a positive vote.

[8]Haddad, William F. "Mr. Shriver and the Politics of Poverty." *Harper's Magazine*, December 1965, pp. 43-50.

prisingly, the most controversial. "Community action" was vaguely defined as a program "which provides services, assistance, and other activities to give promise of progress toward elimination of poverty or a cause or causes of poverty" (§202 a[2], emphasis added). The EOA also contained three radical provisions about how CAP grants could be made. First, CAP funds were to be allocated across states according to an apportionment index in the legislation. States with more of the nation's poor were supposed to get more funding. But within states, the OEO had complete discretion to spend its money in "any...geographical area" (§202 a[1]). Second, funds need not flow through or to state or local governments. Instead, the EOA authorized the federal government to fund programs "conducted, administered or coordinated by a public or private nonprofit agency (not a political party)" (§202 a[4]). A final provision noted that CAP programs should be "developed, conducted, and administered with maximum feasible participation of residents of areas and members of groups served" (§202 a[3]).[9]

The combined effect of the EOA's provisions was to allow Shriver to circumvent state and local governments, which many believed had failed to alleviate poverty or, worse, been complicit or instrumental in its persistence. This direct funding mechanism allowed the federal government to work around de facto exclusion of the poor from designing programs to address their own poverty and de jure racial segregation that had restricted the political participation of African Americans.[10] CAPs aimed to empower the poor themselves to

[9]Levitan (1969, pp. 110-11) writes that most of the Johnson administration officials who testified to Congress regarding the EOA were naïve about the implications of this clause. Only Robert Kennedy, the chairman of the Cabinet Committee on Juvenile Delinquency, which oversaw an earlier, localized federal community action program, mentioned the provision of "maximum feasible participation" in his congressional testimony. Kennedy argued that "there certainly should be an opening to deal with local agencies, private and public, who could get together and come up with a plan or an organization which could handle a particular function."

[10]Strom Thurmond (D-SC) railed against this provision during the debate over the EOA: "Under the innocent sounding title of 'Community Action Programs,' the poverty czar would not only have the power to finance the activities of such organizations as the National Council of Churches, the NAACP, SNCC, and CORE, but also a SNOOP and a SNORE which are sure to be organized to get their part of the green gravy." Thurmond also accused Shriver of having promised the NAACP that he'd use the OEO to promote desegregation (*Congressional Quarterly*. "CQ Senate Votes 218 through 223," 23 July 1964, p. 1567). Thurmond

change their communities—to fight poverty while reforming local social institutions and undermining entrenched racial segregation (Forget 2011).

These radical provisions would have mattered little had the demands of the OEO been modest or the financial stakes small. But Johnson's choice of Sargent Shriver to head the poverty task force and the EOA's funding for local organizations made the EOA matter.

Shriver had an impressive record of political effectiveness. According to Murray Kempton, Shriver could weather the political attacks of Congress by day but "then at night he will call up some power figure from [the Representative's] district and the next morning [the Representative] is unexpectedly slapped on the back of the head."[11] Shriver maintained independent staffing and funding criteria. He also linked OEO funding to compliance with the 1964 Civil Rights Act and interpreted "maximum feasible participation" for CAA boards more strictly than was popular.

The federal funding at stake was large. From 1965 to 1968, the CAP funds amounted to a cumulative $2.64 billion (in real 1968 dollars). While small in relation to other federal expenditures, these funds were large relative to local government spending on related programs. Average, annual real CAP funding from 1965 to 1968 amounted to over a 25 percent increase relative to the sum of local government welfare expenditures in 1962. Furthermore, public welfare spending was often lower in the poor counties where CAP funds went. In 791 funded counties, average real CAP grants from 1965 to 1968 more than doubled 1962 public welfare spending. In addition, CAP grants were made to 83 counties which in 1962 spent nothing on public welfare (OEO 1965-1968, U.S. Bureau of the Census 1964).[12] Roughly 38 percent of CAP dollars went to Head Start and another 39

would change his party affiliation to Republican the following September.

[11] Kempton, Murray. "The Essential Sargent Shriver." *New Republic*, 28 March 1964, p. 13.

[12] County-level data are created by aggregating data on individual CAP grant actions from NACAP files to the county-year level. These data contain 162,795 individual grant actions to 4,818 grantee organizations from 1965 to 1981 (figures for 1969 are lost). They also include information on funding amounts from the OEO, cost sharing by local governments and other sources, the name and address of the grantee (county, city, street address, and name of the grantee), and brief descriptions of grants' intended uses. See appendix C for

percent went to local initiative programs (Levitan 1969, p. 123). CAP money, therefore, represented a tremendous increase in funding available for local anti-poverty programs.

The direct financial stakes also understate the broader implications of federal dollars for communities. In addition to over 18 million people who participated in CAP programs (equivalent to half of America's poor), Shriver told Congress in 1965 that "the most important and exciting thing about the War on Poverty" was "that all America is joining in...religious groups, professional groups, labor groups, civic and patriot groups are all rallying to the call" (Gettleman and Mermelstein 1966, p. 207).[13] Similarly, the *New York Times* featured the "group of leaders" in "every city and community" who "believe this job can be done and who are helping."[14] Importantly, CAP dollars may have crowded in local resources from public, private, and nonprofit sources, making the financial stakes even higher.[15]

In summary, the EOA allowed tremendous federal discretionary power over a meaningful amount of resources. Our analysis adds to the historical literature on this topic by quantifying how the administration used this discretion in two steps. First, we investigate whether the OEO complied with the EOA's apportionment index, which was added as a check on the director's power. Second, we investigate how the OEO spent money within states, which was not prescribed by legislation. Both shed light on the how the War on Poverty was fought.

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more detail.

[13]The 18 million figure relies upon summing over all participants recorded in administrative records.

[14]Reston, James. "The Problem of Pessimism in the Poverty Program." *New York Times*, 10 January 1965, p. E12.

[15]Much has been written about the controversy surrounding the CAP program, but the vast majority of CAAs functioned with the support of their communities. When the 1967 EOA amendments required that the CAAs be designated by state or local governments (and that the OEO director could designate CAAs only in the event that local governments failed to exercise their authority), 792 of the affected 1,018 state, county, or city governments exercised their new authority to designate CAAs within the year. Moreover, 97 percent of these governments elected to continue the existing CAA without change (Levitan 1969, p. 67)—a testament to the program's widespread local approval.

4.2 Was the Index Binding? OEO Funding Decisions at the State Level

The EOA nominally imposed a constraint on Shriver's discretion by requiring that the OEO allocate 78.4 percent of federal CAP funds (what we call "index eligible funds") across the 50 states and the District of Columbia using the apportionment index. The index assigned a share of eligible funds to each state, s, in a fiscal year (FY; July 1 to June 30 in this period), t, using the following formula,

$$\text{EOA}_{s,t} = \frac{1}{3}\text{UE}_{s,t} + \frac{1}{3}\text{PA}_{s,t} + \frac{1}{3}\text{PK}_{s,t} \tag{4.1}$$

where *UE* is the state's share of the national number of unemployed, *PA* is the state's share of the national public assistance recipients, and *PK* is the state's share of poor children (defined as the number of children in families with household incomes below $1,000).[16] For instance, in 1964 Michigan was estimated to have 3.89 percent of the nation's public assistance recipients (295,278 out of 7,581,084), 4.04 percent of the nation's unemployed workers (154,700 out of 3,832,500), and 2.72 percent of the nation's children in families earning less than $1,000 (92,000 out of 3,382,000). Therefore, the index gave Michigan $7,124,240 or 3.55 percent of the 1964 index eligible CAP funding (U.S. House 1964b).

In practice, the EOA gave Shriver considerable discretionary power to withhold grants.[17]

[16]The rationale for this poverty threshold is unclear. It is below Orshansky's poverty line, which estimated that a family of four would spend $1,033 per year on food alone (Oregon Center for Public Policy 2000). Most of the details of how the index was developed are not in the historical record. Asked whether the poverty task force ever kept records, member Eric Tolmach replied, "Not formally, no…there were no recorders…no secretaries…at the meetings. No minutes were kept. There are memos based on people's interpretations of what took place at the meeting. There are no word-for-word accounts" (Gillette, 1996, p. 45). Oral histories provide incomplete accounts.

[17]The EOA also included requirements that the OEO "establish procedures which will facilitate effective participation of the States in community action programs" (§209[a]) and develop guidelines for "equitable distribution of assistance under [the Community Action Program] within the States between urban and rural areas" (§210). Like many of its other provisions, the details of these regulations were left to the OEO itself and, ultimately, imposed insignificant constraints. Although the EOA was amended in each year from 1965 to

For instance, EOA §203(c) left it within Shriver's authority to reallocate CAP money designated for one state to the others. In addition, the EOA imposed no restrictions on Shriver's use of the remaining non-index eligible CAP funds, which made up 20 percent of the total. Even if the EOA permitted deviations in practice, the index apportionment provided a transparent benchmark by which politicians could evaluate whether their state had been shortchanged.

The historical record is rife with examples of Shriver's use of discretionary power. One highly publicized showdown in early 1965 featured Louisiana's governor, John J. McKeithen. McKeithen announced the appointees to run the EOA-funded antipoverty program, but many objected (and wrote to Washington) that the appointees were "rabid segregationists" (Germany 2007, p. 49).[18] Although Shriver could not pick the appointees, he had the authority to withhold funding (which he did) if he disapproved. McKeithen appealed to Congress, the Vice President, and the President, to no avail. Ultimately, McKeithen selected a new set of appointees, and OEO money began to flow into Louisiana.

But the OEO was not always successful. The Child Development Group (CDG), which obtained a grant to set up a Head Start program in rural Mississippi, shows how Congress could check the OEO's power. After a media bonanza at launch, the CDG infuriated politicians and other Mississippians because it offered a blueprint for desegregation in Mississippi's public schools. The administration ultimately backed down and reduced the CDG's funding when Senator John Stennis (D-MS), chair of the Senate Appropriations Committee, threatened to hold the President's other legislation hostage, including funding for the

1967, the poverty index remained unchanged except for using updated information for the index components (U.S. House 1967). The funding formula set aside 2 percent of the eligible index funds for U.S. territories, which means that 78.4 percent (rather than the printed 80 percent) went to states. In addition, the funding formula only applied to funds for general program development and administration, not technical assistance or training costs, or programs funded in other sections of the EOA. Programs for migrant workers and job retraining were funded in separate sections of the EOA, although the migrant programs were administered under the CAP.

[18]Haddad, William F. "Mr. Shriver and the Politics of Poverty." *Harper's Magazine*, December 1965, p. 48.

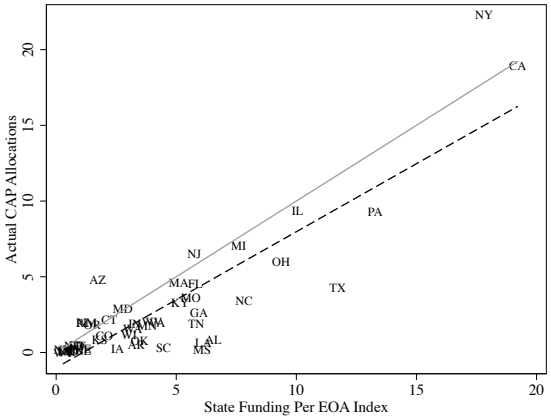
Vietnam War (Carter 2009).

Figure 4.1 describes the OEO's use of discretion by examining deviations from the apportionment index. The horizontal axis plots each state's apportionment under the EOA and the vertical axis plots each state's receipt of funding. A state will fall along the 45-degree line (plotted as a solid line) if it received the minimum apportioned in the EOA and above/below it if the state received more/less than its index apportionment. Positive and negative deviations are interesting because they represent, respectively, the OEO's topping up of a state's apportionment with discretionary funding or its failure to reach an agreement with a particular local organization. The dashed line represents the least squares regression fit, shown in detail in table 4.1.

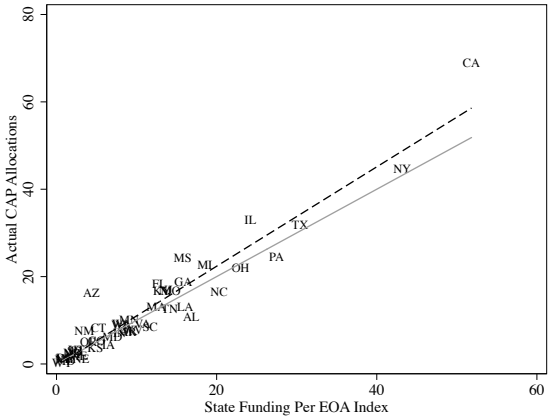
Between FY 1965 and 1968, federal EOA appropriations grew from $237 million (nominal dollars) to $867 million in FY 1968 (U.S. House of Representatives 1964b, §2; 1967, §2). The OEO's difficulties spending its allocation in its first FY—due to Shriver asserting his authority and challenges setting up programs—is captured in Figure 4.1A. By the end of the first FY on 30 June 1965, the CAP had only existed for nine months and spending reached only $143 million of the budgeted $199 million in nondiscretionary funds. Forty-one states lie below the 45-degree line because they received less than their EOA minimum apportionment in federal funds. Many states fell very far below their apportionment: Mississippi received about 3 percent of its apportionment; South Carolina, 7 percent; and Nebraska, 11 percent. These surpluses at the OEO resolved within several years as local organizations developed more applications and as OEO administrators succeeded in making grants. By FY 1966, most states had moved closer to or exceeded the 45-degree line; many had crossed it (Figure 4.1B). In this year, each apportionment dollar translated into $1.14 in actual funding (table 4.1A, column 2). By FYs 1967 and 1968, almost all states lie near or above the 45-degree line and most exceeded it (figures 1C and 1D). In these two years, each apportionment dollar translated into $1.40 and $1.19 in actual funding, re-

Figure 4.1: The Relationship of 1965-1968 CAP Spending to the EOA Apportionment Index, by Fiscal Year

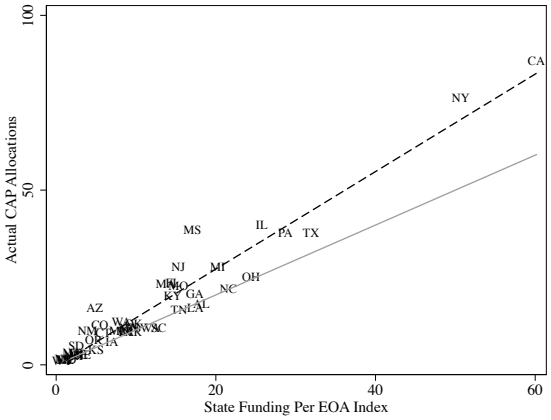
A. Fiscal Year 1965



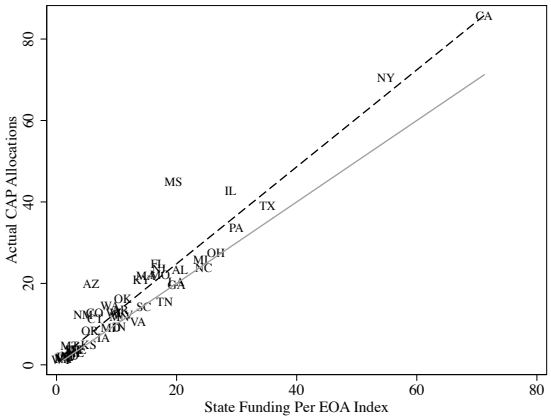
B. Fiscal Year 1966



C. Fiscal Year 1967



D. Fiscal Year 1968



Notes: The x-axis indicates the total minimum funding according to the EOA poverty index for each state for the relevant fiscal year in millions of 1968 dollars. See equation (1) and the text for details. The y-axis indicates actual federal allocations in millions of 1968 dollars for each state for the relevant fiscal year, including index allocations and the remaining 20 percent of CAP funds at Shriver's discretion. The solid line is the 45-degree line, and the dashed line is fit by least squares regression. A state will fall along the 45-degree line if it received the minimum apportioned in the EOA. Points above the 45-degree line indicate that a state received more than its minimum apportionment, whereas points below the line indicate the reverse.
Sources: EOA index is developed from U.S. House of Representatives (1964a), U.S. Department of Labor (1968), and U.S. Department of Health, Education and Welfare (1968). Actual allocations are aggregated using the NACAP grant data.

Table 4.1: The Relationship between EOA Poverty Index and CAP Spending, 1965 to 1968

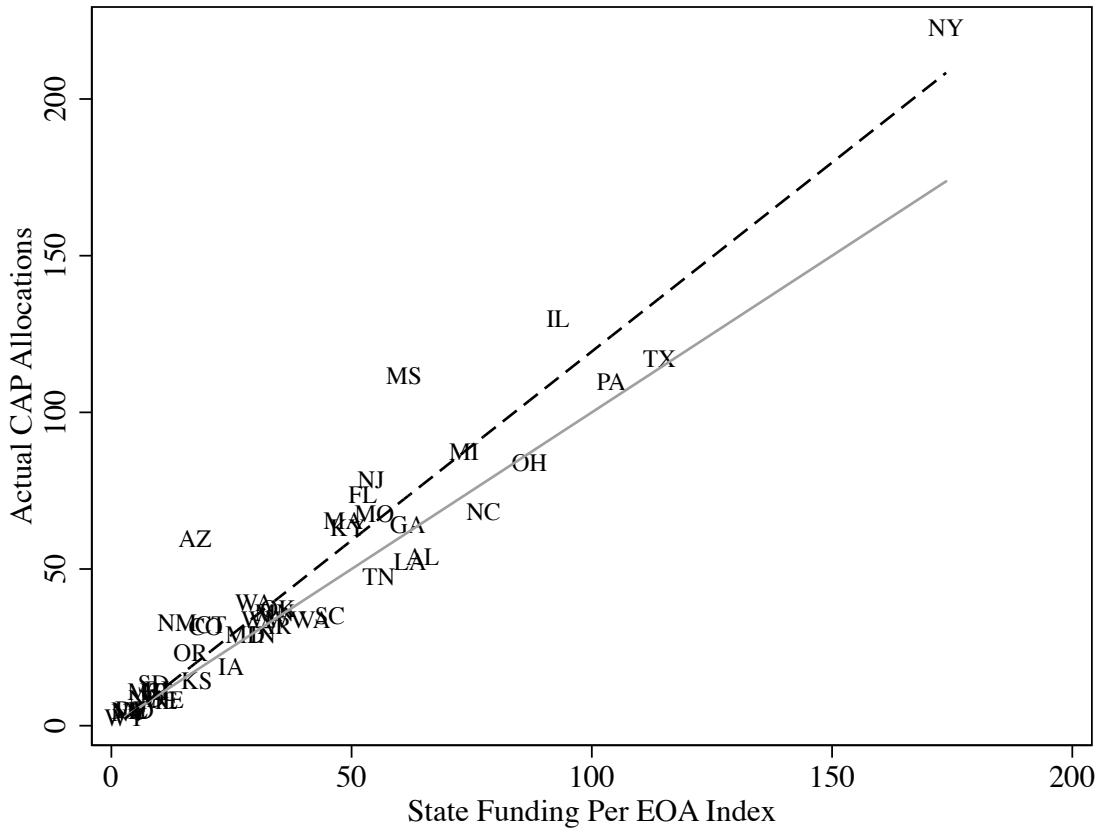
	(1)	(2)	(3)	(4)	(5)
A. Dependent Variable: Real Federal CAP Funding (No Census Region Fixed Effects)					
	1965	1966	1967	1968	1965-1968
Index apportioned funds in FY	0.898***	1.137***	1.395***	1.190***	
	[0.137]	[0.0915]	[0.0563]	[0.0362]	
Cumulative apportioned funds, 1965-1968					1.206***
					[0.0549]
Constant	-1.002**	-0.373	-0.495	1.015	-1.16
	[0.476]	[0.876]	[0.708]	[0.680]	[2.408]
Observations	48	48	48	48	48
R^2	0.763	0.914	0.93	0.92	0.932
Coefficient >1 *p*-value	0.230	0.070	0.000	0.000	0.000
Coefficient=1.25 *p*-value	0.033	0.497	0.001	0.790	0.914
B. Dependent Variable: Real Federal CAP Funding (With Census Region Fixed Effects)					
	1965	1966	1967	1968	1965-1968
Index apportioned funds in FY	0.938***	1.168***	1.418***	1.201***	
	[0.103]	[0.0724]	[0.0366]	[0.0372]	
Cumulative apportioned funds, 1965-1968					1.230***
					[0.0340]
Midwest=1	-1.470*	0.654	-2.756*	-1.841	-5.591
	[0.734]	[1.268]	[1.397]	[1.247]	[4.028]
South=1	-3.339***	-0.999	-4.486**	-1.53	-10.90**
	[0.925]	[1.464]	[1.800]	[1.888]	[5.183]
West=1	-0.201	3.297*	0.572	1.548	5.699
	[0.695]	[1.683]	[1.322]	[1.454]	[4.493]
Constant	0.346	-1.295	1.285	1.479*	1.542
	[0.448]	[1.150]	[0.988]	[0.861]	[2.738]
Observations	48	48	48	48	48
R^2	0.862	0.93	0.945	0.927	0.946
Coefficient >1 *p*-value	0.274	0.013	0.000	0.000	0.000
Coefficient=1.25 *p*-value	0.014	0.659	0.000	0.970	0.386

Notes: Stars denote statistical significance as follows: * = Significant at the 5 percent level. ** = Significant at the 1 percent level. *** = Significant at the .05 percent level. Huber-White standard errors are in brackets. Sample excludes Alaska, Hawaii, and the District of Columbia.
Sources: See Figure 4.1.

spectively (table 4.1A, columns 3 and 4). Aggregating over 1965 to 1968, the OEO hit the apportionment requirements for almost all states and, although not mandated by the EOA, tended to spend CAP discretionary funding in proportion to each state's index (Figure 4.2). For the overall period, the regression line slope is greater than the 45-degree line; every apportionment dollar translated into $1.21 in actual spending, almost the $1.25 one would obtain if the OEO, on average, allocated its discretionary 20 percent in proportion to the 80 allocated by apportionment index (table 4.1A, column 5).

Three main conclusions follow. First, the EOA apportionment index was not *absolutely*

Figure 4.2: The Relationship of Cumulative 1965-1968 CAP Spending to the EOA Apportionment Index



Notes: The x-axis indicates the total minimum funding according to the EOA poverty index for each state for fiscal years 1965-1968 in millions of 1968 dollars. See equation (1) and the text for details. The y-axis indicates actual federal allocations in millions of 1968 dollars for each state, including index allocations and the remaining 20 percent of CAP funds at Shriver's discretion.
Sources: See Figure 4.1.

binding at the state level. Consistent with Shriver's reputation and the historical literature on funding controversies, this analysis suggests that Shriver had a good deal of leeway and that he exercised it (at least in the program's early years). Over time, however, compliance with the poverty index increased as Congress and the public pressured the Johnson administration.

Second, the South was an important outlier (table 4.1B), which is consistent with Alston and Ferrie's (1999) hypothesis about Southern politicians trying to dissuade Shriver from

putting EOA money into their states. Contrary to the common perception that the South received *more* War on Poverty spending, Southern states averaged almost $11 million *less* in actual CAP funding than Northeastern states over the 1965 to 1968 period, after accounting for their shares of the nation's poor (table 4.1B, column 5). The Southern disadvantage persists even in specifications including multiple covariates.

Third, the OEO largely allocated CAP funds across states in proportion to the apportionment index. This was the case both for the nondiscretionary index-apportioned spending and the discretionary spending. The apportionment index explains 76 to 93 percent of the variation in state-level CAP spending across the four fiscal years (table 1A, columns 1-4) and exceeds 93 percent for the entire period (column 5). The inclusion of region fixed effects has almost no effect on this relationship (table 4.1B). Considering the tremendous power given to the OEO director, President Johnson's reputation for playing politics, and the empirical literature showing Franklin D. Roosevelt's use of New Deal funds to build a longer-term political coalition, this compliance may be surprising. Yet compliance is consistent with Democrats having chosen the apportionment index and hints that fighting poverty reflected the Johnson administration's unconstrained choice.

4.3 How the War on Poverty was Fought: A County-Level Analysis of OEO Spending

Within states, the EOA imposed no constraints on the distribution of funds, location of CAP project (such as a congressional district or city), or type of project funded. The rationale for this flexibility was that it allowed antipoverty programs to be customized to community needs and would create pressure for the reform of social institutions that perpetuated an economic underclass. In practice, this flexibility allowed the Johnson administration to pursue its broader agenda. How these funds were spent, therefore, reveals much

about the Johnson administration's objectives.

The Competing Objectives of Politics and Poverty

Fighting poverty and building political coalition were potentially competing objectives determining the Johnson administration's funding choices. One hypothesis is that Johnson used the War on Poverty to forge a new electoral consensus, much as Roosevelt had used the New Deal (Wright 1974; Fleck 2001; Couch and Shugart 1998; Wallis 2001, 1998, 1987, 1984; Fishback and Wallis 2012; Fishback et al. 2003). Johnson cut his political teeth as a New Dealer and may have learned from Roosevelt's and Harry Hopkins' alleged claim to "tax and tax and spend and spend and elect and elect" (Fishback 2007). Congressional testimony following the EOA's first year claimed as much, saying that its funding had degenerated into "giant fiestas of political patronage."[19]

Building political consensus should result in measurable spending patterns. As Gavin Wright (1974) and Fleck (2008) argue regarding the New Deal, a reelection-seeking president should spend more on swing districts. Because congressional races are determined by winning a plurality, Democrats would seek to convince the pivotal voter to put them in office (or keep them there) through greater spending when a particular race is almost even. OEO funding could also be used to repay favors to prominent congressional committee members or chairs or to encourage favorable future votes for items on the Democrats' agenda. This would result in more OEO funds being spent in areas served by powerful congressional committee members or chairpersons (Anderson and Tollison 1991).

Our analysis tests the importance of political considerations by examining the relationship of OEO funding to the share of the county population voting for Democrats in the 1964

[19] *Congressional Quarterly Almanac*. "Antipoverty Program Funds Doubled," 1965, pp. 405-20. Multiple accounts reflect this thinking in the months leading up to the passage of the EOA. For instance, Johnson promised a reluctant congressman that despite CAP's direct grant mechanism, no money would be spent in his district "that hasn't got your initial on it, or mine" (McKee 2011, p. 50).

presidential election, the change in the Democratic vote share between 1960 and 1964 in the presidential election, whether Johnson won the county in the 1964 election, whether the 1964 presidential election was close (the margin of victory or loss was within 10 percentage points), and the interaction of a Democratic win with a close election. These political variables allow us to test whether CAP funds disproportionately flowed to those counties with stronger Democratic constituencies, those with newer (young) Democratic constituencies, or those districts Democrats narrowly lost but hoped to regain in the 1966 midterm elections. The analysis also examines the relationship between OEO funding and whether the county was represented by a member or chair of a major House committee during the 89th Congress (January 1965-December 1966).[20] These variables allow us to test whether major committee members or chairs brought more funding to their home districts (or kept it out).[21]

A second (and potentially complementary) hypothesis is that the Johnson administration prioritized fighting poverty and racial discrimination, two pillars of his domestic campaign platform. This agenda may have been chosen because Johnson saw the platform's potential political benefits or because it reflected Johnson's long-suppressed humanitarian

[20]See appendix C for more detail on sources and data construction.

[21]"Keeping funding out" relates closely to Alston and Ferrie's hypothesis about the political power and interests of the Southern Democrats. They argue that, after slavery was abolished, Southern plantations owned by the elites developed a system of plantation paternalism to attract and retain labor. Compliant laborers were rewarded with economic support and protection, and this system allowed Southern plantations to retain a supply of cheap workers to keep the plantation system functioning (Alston and Ferrie 1993; 1999). To protect this paternalist system, the South had opposed federal antipoverty programs during Reconstruction and the New Deal, which threatened to give agricultural laborers better outside options. (Thus, for example, agricultural workers were excluded from the original Social Security program. See Alston and Ferrie 1999, pp. 67-70; Newman and O'Brien 2011, pp. 7–20). As the invention of a mechanical cotton harvester made low-wage labor less important and the obligations of paternalism thus became a burden to the landed elite, the South's incentives for blocking federal welfare legislation fell. Mechanization, in effect, made a federal antipoverty program more appealing to Southern elites—as long as the programs were implemented in other areas and would encourage the outmigration of black and poor white farm laborers. We examine whether Johnson earned the support, or at least the neutrality, of Southern power brokers by not making CAP grants in areas dominated by Southern paternalism. Such a pattern would suggest that members of Congress had compromised with Johnson about the EOA's implementation in their districts instead of blocking the EOA's passage.

agenda. Robert Caro's biographies of Johnson document episodes during Johnson's rise to power that revealed "hints of compassion for the downtrodden, and of a passion to raise them up; hints that he might use power not only to manipulate others but to help others—to help, moreover, those who most needed help" (2002, p. xxi). Johnson also may have believed his domestic legacy would be the antipoverty agenda. Speaking with Senator Joseph Clark (D-PA), Johnson commented, "Lincoln abolished slavery, and we're going to abolish poverty" (Miller Center 1964).

Our analysis tests the importance of fighting poverty and racial discrimination by examining the relationship of EOA funding with measures of poverty and the nonwhite share of the population. Greater spending of EOA money in areas with higher poverty rates is broadly consistent with the Johnson administration targeting funds in accordance with the War on Poverty platform. We also examine the extent of this commitment. Whereas politically expedient adherence to the platform might result in the targeting of funds to areas with more citizens just under the poverty line, a more sincere (and less expedient) approach might target the most disadvantaged areas. These areas could show less measureable improvement by official poverty rates, even if livelihoods and individual welfare improved significantly (albeit not enough to cross the poverty threshold). We measure the intensity of disadvantage by the share of individuals in households with incomes below $2,000 and $1,000.

Spending OEO money in areas with more nonwhites is consistent with the Johnson administration's battle against racial discrimination and Johnson's antipoverty agenda. Not only did African Americans have twice the national poverty rate of whites, but *de jure* and *de facto* institutions limited their opportunities to escape poverty. The distribution of more OEO funding directly to communities with more racial minorities could empower these minorities to develop their own antipoverty programs. It also could diffuse civil unrest and rioting (Gillezeau 2012) or reduce crime rates by allowing minorities greater access

to formal institutions (Cunningham 2013). Moreover, even if money was not flowing to minorities directly, the OEO could threaten to withhold funding from whites to ensure greater cooperation. Thus OEO funding could help the federal government buy compliance with the 1964 Civil Rights Act and catalyze racial integration *while* fighting poverty.[22]

Determinants of County-Level OEO Spending

Figure 4.3 maps cumulative real per capita federal grants from 1965 to 1968 by county, with the more darkly shaded areas receiving more funding per person. Fewer obvious patterns emerge than might be expected. The counties of Appalachia are well funded, as are poor rural areas like the desert Southwest, eastern Oklahoma, and the Mississippi Delta. Large cities and metropolitan areas received grants but not typically amounts out of proportion to their populations. Yet clusters of unfunded counties appear in eastern Mississippi, rural Georgia, and the eastern Carolinas: all high-poverty regions.

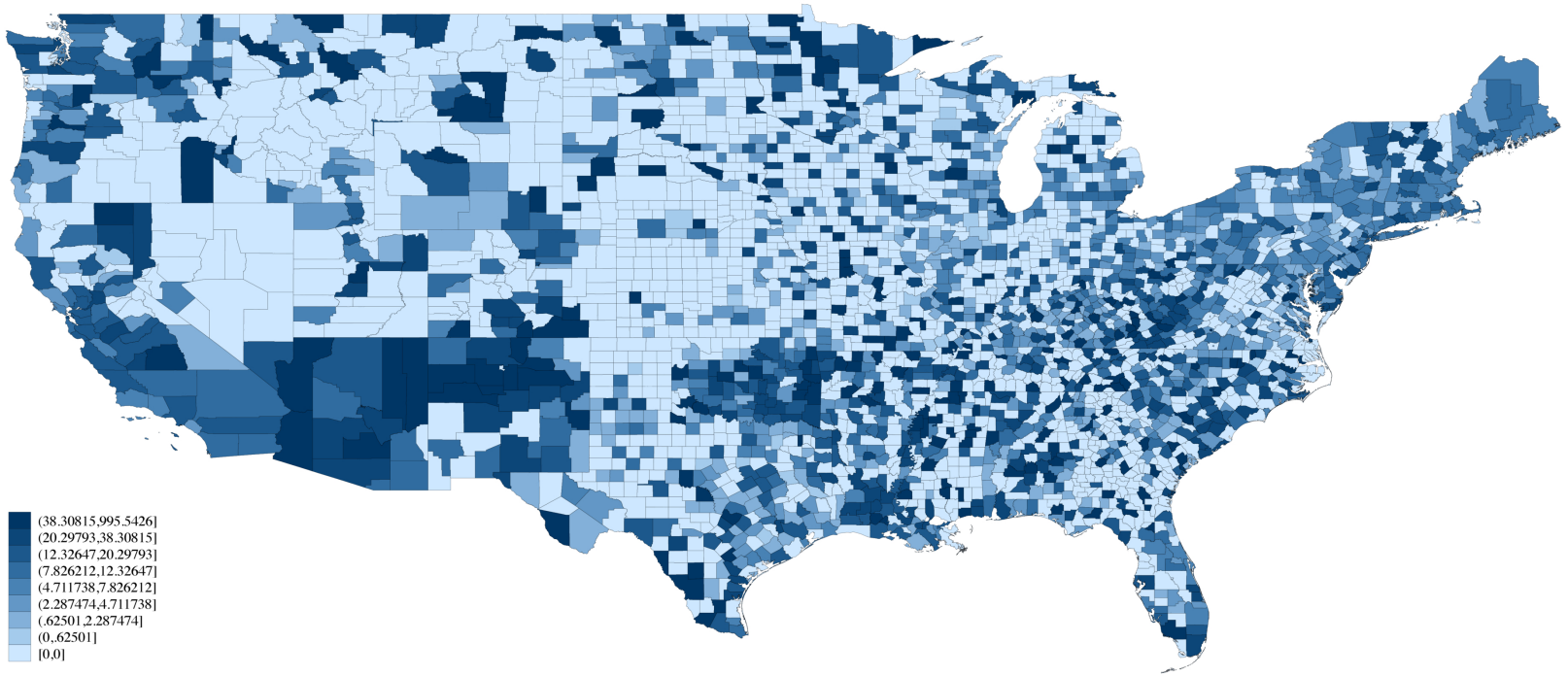
Our regression analysis describes the role of politics and poverty in determining EOA funding using the following linear specification,

$$Y_i = \mathbf{H}_i'\boldsymbol{\theta} + \mathbf{P}_i'\boldsymbol{\gamma} + \mathbf{X}_i'\boldsymbol{\beta} + \varepsilon_i \tag{4.2}$$

Y is real cumulative CAP funding for years 1965 to 1968 in county i, expressed as real 1968 dollars per 1960 county residents, purged of state fixed effects. (Y can be thought of as the within-state variation in per capita EOA funding: the component of EOA funding that was not determined by the apportionment index.) The first two sets of covariates correspond to our two hypotheses: $\mathbf{P}$ is a row vector of covariates to measure political considerations, including the inverse population ratio (1,000/total county population), the 1964 Democratic

[22]Previous versions of this paper included explicit tests of the role of race riots, the escalation of Vietnam, and Alston and Ferrie's (1993) hypothesis about the erosion of paternalism. This version of the paper suppresses discussion of these hypotheses, because our analysis revealed that they played little role in the distribution of OEO funding decisions.

Figure 4.3: County-Level, Per-Capita Community Action Program Spending, 1965-1968



Notes: Shades of gray indicate the cumulative amount of CAP per capita spending from 1965 to 1968 in real 1968 dollars in each county. Zero dollars are indicated in the lightest shade and the highest per capita spending is indicated the darkest shade. Bracketed ranges in the legend indicate the ranges represented by each shade of gray.
Source: Authors' calculations using the NACAP grant data. See appendix C for more details.

presidential vote share, whether Johnson won the county in 1964, whether the presidential election was close, the interaction of whether the election was close and Johnson won, and the difference in Democratic vote shares between the 1964 and 1960 presidential elections.[23] In some specifications we include variables for the power of the county's delegation in the 89th Congress. These variables include the proportion of representatives that are Democrats; indicator variables for whether any of the county's representatives were members of a major committee, chairs or minority leaders of a major committee, or part of the House leadership; and indicator variables for whether any of the county's representatives were both Democrats and held seats or powerful positions on major committees or in House leadership. **H** is a row vector containing covariates related to fighting poverty and racial discrimination. These include measures of the share of the county population in households with annual incomes of less than $3,000 in 1960, measures of the intensity of poverty (share of individuals in households earning less than $2,000 and the share earning less than $1,000 in 1960), and the share of the population that is nonwhite.

In addition, all specifications include other covariates, **X**, to account for other cross-sectional differences between counties that may have influenced the distribution of OEO funding: the shares of the county's population in urban areas, in rural farm areas, under age 5, over age 64, and of the very affluent (in households with annual incomes of $10,000 or more in 1960). **X** also includes the size of local government in terms of total local government expenditures per capita, local government welfare expenditures per capita, and total local government tax revenue per capita in 1962. We also include a riot intensity measure for the fiscal year of funding (Collins and Margo 2007); a measure of the escalation of the Vietnam war (casualty rates), which many claim robbed the War on Poverty of funding and Johnson of political and public support; and a proxy of Alston and Ferrie's "paternal-

[23]The inverse of population is often included in the New Deal literature to capture a fixed dollar amount per state or county. It is also an approximation of the importance of a given voter in a jurisdiction (Fleck 2001; Wallis 2001; Fishback et al. 2003).

ism" using the share of sharecroppers in the total number of farm operators from the 1930 Census of Agriculture (U.S. Bureau of the Census 1930; Depew et al. 2012).[24]

Table 4.2 presents the regression results. For brevity, tables 4.2 and 4.3 suppress the point estimates for the covariates in **X** (the full set of estimates is reported in appendix D). Huber-White standard errors are presented in brackets beneath each estimate. The first three columns present the point estimates of different metrics of county-level poverty rates and column 4 uses the share of the county population that is nonwhite. Column 5 adds the political variables.[25] Columns 6 to 8 present the estimates with all of the variables combined using each of three poverty rates (the three measures are highly collinear, so they are not included together).

These results provide strong evidence that the Johnson administration used OEO funding to fight poverty. Within states where the poverty index did not bind, the population share in poverty (by three measures) and share nonwhite are individually statistically significant at the 1 percent level and robust to the inclusion of other covariates in columns 6 through 8. But the effects are not large in an economic sense. A one standard-deviation higher share of the 1960 population in households with incomes less than $3,000 (0.16) implies a $7.86 increase in real, cumulative per capita federal CAP funding—about one-fifth of a standard deviation in funding (column 6). The implied elasticities are similar for measures of the

[24] A further source was Price Fishback, Michael Haines, and Paul Rhode, "Data Entered from the Agricultural Censuses of 1930, 1935, and 1940," personal communication, 20 June 2012. Paternalism is difficult to measure with existing data, so we experiment with multiple measures of Southern plantation agriculture. Qualitatively similar results were derived by using an indicator variable based either on counties considered plantation areas, according to a special U.S. census report on 1910 cotton farming (U.S. Bureau of the Census 1916; Whatley 1987), or on the devolution of the plantation system, using sharecropping rates in 1959 (U.S. Bureau of the Census 1959; Haines 2010) to determine the percentage of sharecroppers among farm operators. Alston and Ferrie's hypothesis also bears on the interpretation of variables for whether Democratic power on key committees in the House of Representatives is negatively correlated with CAP spending in the South, although we described these variables as part of **P**.

[25] Note that the partial R^2 statistics are calculated by leaving each individual or set of regressors out of the respective model. This approach may understate the explanatory power of the excluded regressor to the degree that it is correlated with the included regressors. We choose this approach because it is a more conservative approach to attribution.

Table 4.2: County-Level Correlates of CAP Spending, 1965 to 1968

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable: Real Federal CAP Expenditures per Capita							
Poverty Variables								
Population share in with incomes								
≤ $3K	72.34***					49.14***		
	[14.290]					[11.649]		
≤ $1K		144.86***					80.92***	
		[36.345]					[25.957]	
≤ $2K			93.40***					51.75***
			[19.661]					[13.186]
Share nonwhite				85.59***		85.14***	82.04***	82.09***
				[21.242]		[21.777]	[20.671]	[21.554]
Political Variables								
1,000/population					7.63	9.75*	6.69	6.19
					[5.850]	[5.579]	[5.353]	[5.291]
1964 Presidential Election								
Change in share for					26.97***	56.62***	54.68***	55.03***
Democrat, 1960-1964					[10.404]	[14.875]	[14.743]	[14.832]
Share for Democrat					29.70**	32.30**	33.72**	32.79**
					[13.337]	[13.084]	[13.190]	[13.130]
1= Democrat won					-5.08	-4.88	-5.05	-4.92
					[3.511]	[3.424]	[3.436]	[3.428]
1=Election close					-6.07***	-4.29**	-4.36**	-4.26**
(+/-10 points)					[2.116]	[1.997]	[1.992]	[1.995]
1= Election close					7.66**	6.67*	6.94*	6.78*
x 1=Democrat won					[3.695]	[3.569]	[3.547]	[3.578]
89th Congress House Representative(s)								
1=Democrat					0.20	-0.87	-0.90	-0.79
					[2.311]	[2.262]	[2.264]	[2.256]
1=Major committee					-2.02	-0.78	-0.75	-0.69
member/leader					[3.067]	[2.832]	[2.813]	[2.821]
1=Major committee					0.32	0.75	0.69	0.59
member x 1=Democrat					[3.598]	[3.450]	[3.418]	[3.429]
1=Major committee chair/					-0.17	0.10	-0.21	-0.16
leader					[2.368]	[2.242]	[2.281]	[2.262]
1=Major committee chair/					-0.40	-1.27	-0.30	-0.81
leader x 1=Democrat					[3.223]	[3.222]	[3.199]	[3.223]
Observations	3,091	3,091	3,091	3,091	3,091	3,091	3,091	3,091
R^2	0.045	0.050	0.050	0.070	0.038	0.090	0.089	0.089
Partial R^2								
Poverty variables	0.009	0.015	0.014	0.022		0.031	0.027	0.028
Political variables					0.004	0.010	0.009	0.009

Table 4.2 — Continued

Notes: The unit of observation is a county or "super-county," whichever is the lowest unit of geographic aggregation observed in all of our data sources. Estimates are obtained by ordinary least squares regression of the specification described in equation 4.2. The dependent variable is the residual of a regression of real federal expenditures through the Community Action Program from 1965 to 1968 on state fixed effects. Suppressed covariates include percent of a county's population that is rural or farm and percent that is urban, under the age of 5, over the age of 64, or lives in a household with more than $10,000 in annual income. They also include the Vietnam casualty rate, a measure of riot intensity (Collins and Margo 2007), the number of sharecroppers per total farm operators, local government's direct expenditures per capita (1962), direct expenditures on welfare (1962), and total tax receipts per capita (1962). Estimates are unweighted. Stars denote statistical significance as follows: * = 5 percent level. ** = 1 percent level. *** = .5 percent level. Huber-White standard errors are in brackets. The county sample includes the coterminous United States, excluding the District of Columbia.
Sources: See appendix C for more details and appendix D for coefficients on suppressed covariates.

intensity of poverty. For a one standard deviation higher share of the 1960 population in households with incomes less than $1,000 and $2,000, columns 6 and 7 imply a similar increase in cumulative per capita federal CAP funding—0.14 and 0.17 of a standard deviation, respectively. By this metric, the population share of nonwhites, however, is about twice as economically important. A one standard-deviation higher share of nonwhites (0.16) implies a $14 increase in real, cumulative per capita federal CAP funding—over one-third of a standard deviation in the dependent variable (columns 6 to 8).

Political considerations also shaped decisions at the OEO. A one standard-deviation increase in Democratic share in the 1964 presidential election has roughly the same effect as a one standard-deviation increase in poverty—about 0.16 of a standard deviation increase in funding. Not only did more Democratic counties receive more money per capita, funds also rewarded districts with larger increases in Democratic share in the presidential elections between 1960 and 1964, with a one standard-deviation increase in this variable leading to 0.20 of a standard deviation increase in funding. Whereas swing counties received less funding, swing counties won by Democrats received slightly more ceteris paribus. These findings are robust to the inclusion of identical covariates for the 1960 presidential election as well as to alternative definitions of "swing" areas. In short, the Johnson administration invested in its new Democratic constituency by directing OEO funds to Democrat-trending areas

as well as to Democratic strongholds. In contrast, we find no evidence that congressional committee membership during the 89th Congress mattered at all (the results for the 88th and 90th Congress do not alter this conclusion). Counties with representatives on major House committees, chairing major committees, or in positions of leadership (Democratic or Republican) have no predictive power in any of our regressions.

Presidential politics, however, explains very little of county-level OEO's funding decisions. To make the relative contributions of poverty and politics more explicit, we summarize the partial R^2 values of these sets of variables at the bottom of table 4.2—a simple metric for determining how much of the variation in within-state per capita CAP funding is explained by the set of poverty variables or the set of political variables. The R^2 value for the political variables is 0.004 (column 5), significantly lower than for the poverty variables. Across specifications, approximately 3 percent of the variation in funding is explained by poverty, or 30 percent of the variation explained by the model. Politics explains less than 1 percent in all cases.

Much of the history of the War on Poverty focuses on the South and its interaction with civil rights. The majority of nonwhites lived in the South, and table 4.2 shows that the nonwhite population share played an important role in shaping OEO funding. Moreover, the paternalism hypothesis (Alston and Ferrie 1999, 1993) suggests that local economic, demographic, and political considerations may have had different effects in the South. For both reasons, table 4.3 splits our sample into non-Southern and Southern counties, roughly partitioning the country into halves (46 percent of all counties are in the South).

The results highlight some similarities and differences between the regions: poverty rates and share nonwhite have a strong and robust relationship to OEO spending. Similarly, changes in the share of a county voting for Democrats also matters in both regions, although Democratic strongholds do not appear to be rewarded in the South (where most places were Democratic strongholds). The economic significance of each of these variables, however,

Table 4.3: County-Level Correlates of CAP Spending in the South and Non-South, 1965 to 1968

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Real Federal CAP Expenditures per Capita					
	Non-South Only			*South Only*		
Poverty Variables						
Population share with incomes						
≤ $3K	83.45***			31.93**		
	[26.025]			[12.451]		
≤ $1K		211.44***			45.54**	
		[70.400]			[20.150]	
≤ $2K			127.09***			34.39***
			[37.551]			[12.256]
Share nonwhite	411.66***	396.67***	396.04***	21.65*	20.18*	19.45
	[88.228]	[81.937]	[85.550]	[12.375]	[11.710]	[11.972]
Political Variables						
1,000/population	7.75	2.69	2.84	1.39	0.36	-0.85
	[8.332]	[8.355]	[8.818]	[2.974]	[3.030]	[3.195]
1964 Presidential Election						
Change in share for	45.86**	45.01**	42.77**	33.04**	31.83**	31.93**
Democrat, 1960-1964	[20.882]	[21.264]	[21.223]	[13.342]	[13.390]	[13.405]
Share for Democrat	51.78**	53.59**	53.19**	1.69	2.27	1.96
	[22.044]	[22.018]	[22.090]	[14.422]	[14.373]	[14.384]
1= Democrat won	-9.11*	-9.08*	-9.00*	1.26	1.17	1.25
	[5.073]	[5.085]	[5.064]	[4.337]	[4.337]	[4.334]
1=Election close	-3.48	-3.83	-3.33	-2.37	-2.40	-2.37
(+/-10 points)	[2.965]	[3.049]	[3.012]	[2.317]	[2.343]	[2.314]
1= Election close	9.23*	10.05**	9.30*	2.72	2.66	2.76
x 1=Democrat won	[4.735]	[4.764]	[4.746]	[4.480]	[4.500]	[4.487]
89th Congress House Representative(s)						
1=Democrat	-1.97	-2.24	-2.02	2.54	2.60	2.82
	[2.251]	[2.229]	[2.243]	[3.519]	[3.561]	[3.554]
1= Major committee	3.26	3.41	3.48	-6.09	-6.27	-6.08
member/leader	[2.791]	[2.770]	[2.766]	[3.897]	[3.876]	[3.892]
1= Major committee member	-3.68	-4.26	-4.16	4.39	4.54	4.31
x 1=Democrat	[3.529]	[3.489]	[3.507]	[3.920]	[3.929]	[3.930]
1=Major committee chair/	-2.68	-2.75	-2.58	-0.12	0.41	0.13
leader	[3.038]	[3.030]	[3.045]	[2.813]	[2.785]	[2.796]
1=Major committee chair/	20.37*	19.49*	20.35*			
leader x 1=Democrat	[10.742]	[10.478]	[10.481]			
Observations	1,677	1,677	1,677	1,414	1,414	1,414
R^2	0.413	0.419	0.417	0.028	0.027	0.028
Partial R^2						
Poverty variables	0.279	0.253	0.252	0.006	0.005	0.006
Political variables	0.022	0.024	0.023	0.005	0.004	0.004

Notes: See table 4.2 notes for specification and other details. In columns 4-6, interactions of 1=Represented by a House Democrat with committee membership and chairmanship/leadership are omitted due to collinearity.
Sources: See appendix C for more details.

is weaker in the South. A one standard-deviation increase in Southern poverty rates or share nonwhite implies a 0.12 and 0.11 standard deviation increase in EOA spending, respectively (column 4). This is one-half (for poverty) to one-fifth (for share nonwhite) the magnitude of these effects in the rest of the United States (column 1).

The results also reveal striking differences in funding patterns between the two regions. First, roughly 40 percent of the variation in per capita OEO funding is explained by the model in non-Southern regions (partial R^2, columns 1 to 3). In the South, less than 3 percent is explained by the model (columns 4 to 6). Second, the lion's share of the explained variation outside the South is accounted for by the poverty variables (25 to 27 percent), whereas in the South very little of the variation is explained by poverty measures (less than 1 percent in all cases). Third, presidential politics seem to matter very little in general and even less in the South. Putting aside the fact that these estimates are statistically insignificant, their magnitude in the South is half the size of the implied effects elsewhere.

Correlations of OEO Spending with Electoral Outcomes

Our final analysis investigates whether EOA money was successful in building the Democratic coalition. Our analysis draws upon estimates by Clubb et al. (2006) of House election voter turnout and share of votes for Democrats by county using a panel version of equation (2) which omits the political variables and includes state-by-year fixed effects,

$$Y_{it} = \sum_{y} \left[\mathbf{H}_i'\boldsymbol{\theta} + \mathbf{X}_i'\boldsymbol{\beta}\right] \mathbf{1}\{t = y\} + \rho_{s(i),t} + \varphi_i + \varepsilon_{i,t} \tag{4.3}$$

where Y_{it} is now voter turnout or share of votes cast for Democratic candidates in county i, in year t=1950, 1952,...,1958,1962,..., 1972 (note: 1960 is omitted); φ_{it} are county fixed effects; $\rho_{s(i),t}$ are state-by-year fixed effects; and other variables remain as previously defined. The specifications describe changes in voter turnout and support of Democrats in

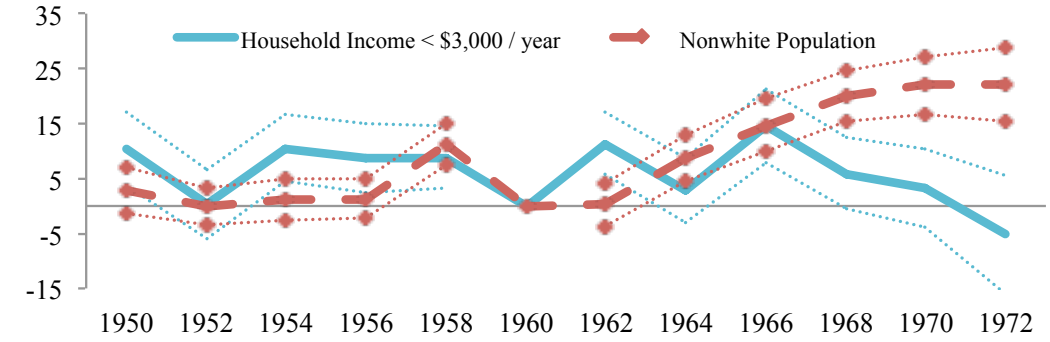
poorer and more nonwhite counties after accounting for secular changes in state politics, redistricting in the state-by-year fixed effects, and the time-varying effects of other covariates.

Figure 4.4 supports the claim that OEO spending boosted voter turnout (panel A) and support of the Democrats (panel D) in poor areas. Panel A plots the point estimates for each year on the interaction between year and either (1) the share of the population in households earning less than $3,000 per year (solid line) or (2) the share nonwhite (dashed line). Except for a short-term increase in 1958, the relationship of share nonwhite (dashed line) to voter turnout is stable from 1950-1962. In 1964, however, the relationship increases rapidly. Counties with a one percentage point higher share nonwhite in 1960 experienced a 0.20 percentage point increase in voter turnout during the 1968 election—a pattern driven by higher turnout in nonwhite counties of the South. A more modest and short-run relationship is seen for poorer counties, which experienced low turnout in 1960 and 1964 relative to the Eisenhower era, only to surge to their highest level in 1966 before falling steadily from 1968 to 1972.

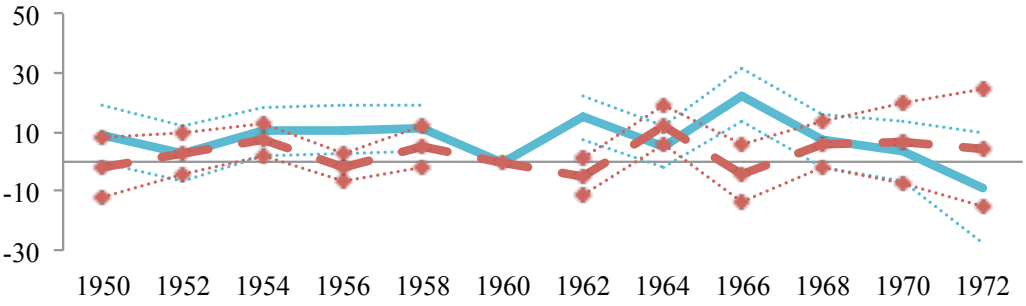
One interpretation of this result is that the groups the OEO intended to empower, especially nonwhites, became more politically engaged during the 1960s. If this were the case, the share of voters supporting Democrats should also grow in poorer and more nonwhite areas where turnout increased after 1964. But panel D shows this is only part of the story. After steadily decreasing during the 1950s and early 1960s, support for Democrats in poorer areas increased from 1964 to 1968 (solid line). A one standard-deviation higher poverty rate in 1960 (0.16) implies a 1.8 percentage point (11.3 x 0.16) higher Democratic vote share in the 1966 election, or an increase of 3.5 percent. Because turnout was not trending this way, the War on Poverty may have won over existing voters or changed the composition of who went to the polls. The reverse appears to be true for more nonwhite areas. Again, after the 1950s and early 1960s, when the relationship between nonwhite pop-

Figure 4.4: County-Level Election Outcomes in the House of Representatives by Year and County Characteristics

A. Voter Turnout, All Counties



B. Voter Turnout, Non-South Only



C. Voter Turnout, South Only

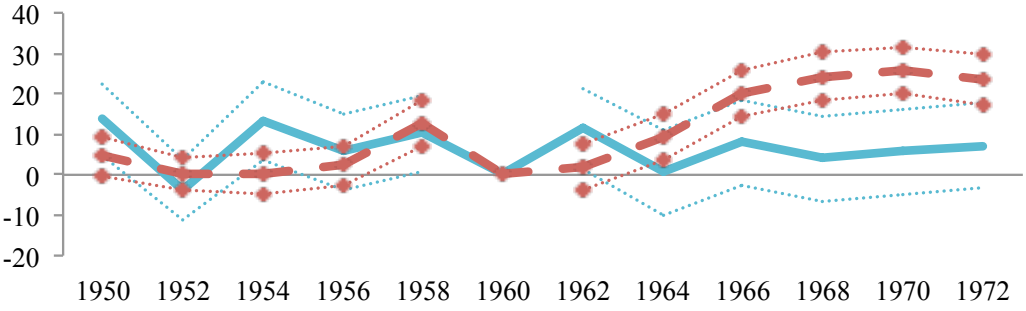
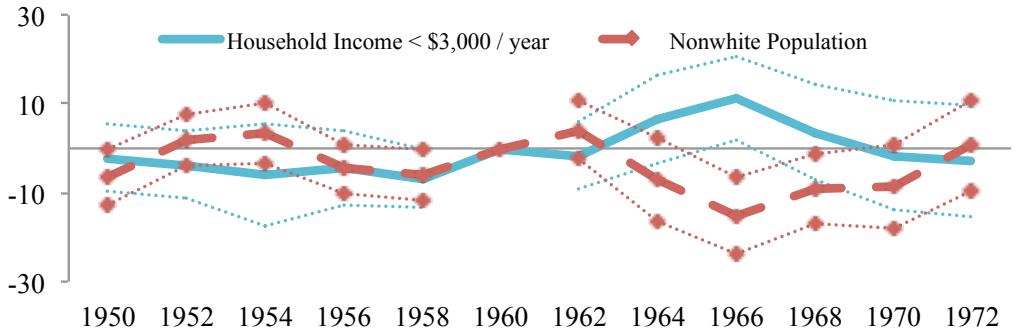
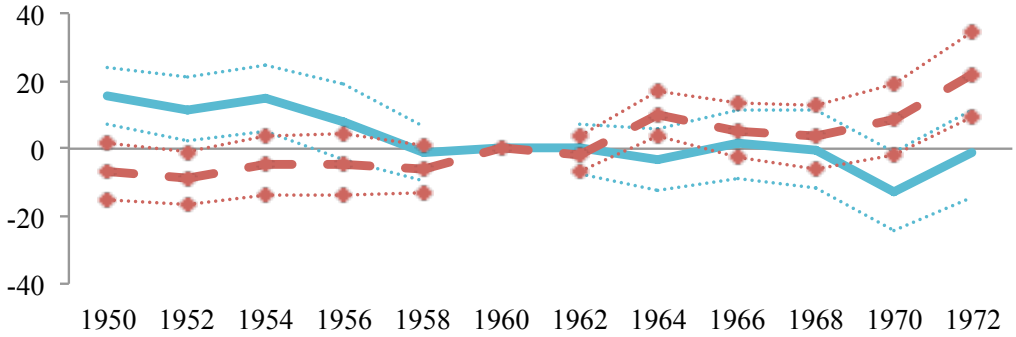


Figure 4.4 — Continued

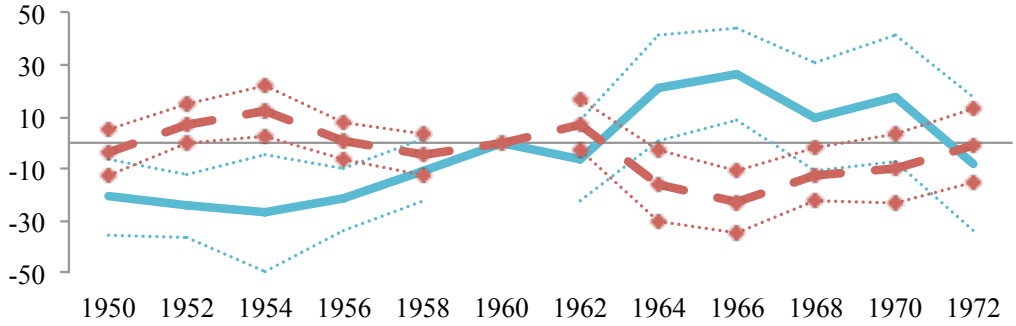
D. Democratic Vote Share, All Counties



E. Democratic Vote Share, Non-South Only



F. Democratic Vote Share, South Only



Notes: Estimates plot the changing relationship of share of population in poverty and share of population nonwhite to the outcome variable of interest. The outcome variable for panels A, B, and C is voter turnout. For panels D, E, and F, the outcome variable is Democratic vote share. See text for more details.
Sources: Estimated voter turnout and Democratic vote share from Clubb et al. (2006). Household income < $3,000 and nonwhite shares from Haines and Inter-university Consortium for Political and Social Research (2010). See appendix C for more details.

ulation share and share voting for Democrats changed little, the Democratic vote share in areas that were more nonwhite decreased rapidly between 1964 and 1968. A one standard-deviation higher share nonwhite in 1960 (0.16) implies a 1.4 percentage point (8.7 x 0.16) or 2.8 percent lower vote share in the 1968 election. The rising turnout in more nonwhite counties, therefore, reflected increases in votes for Republicans. This is consistent with backlash voting or to growing disillusionment with the War on Poverty.

Notably, these patterns differ across regions. Voter turnout in poor areas increased in non-Southern regions (figure 4.4B) but not in the South (figure 4.4C), whereas voter turnout in nonwhite areas increased in the South (figure 4.4C) but not in non-Southern areas (figure 4.4B). Interestingly, the share of the vote for Democrats changed little in poorer areas of the non-South (figure 4.4E), whereas Democratic vote share rose sharply in poorer areas of the South (figure 4.4F). For more nonwhite areas, Democratic vote share increased in the non-South (figure 4.4E) but fell sharply in the South (figure 4.4F). On the whole, these results are consistent with the Democrats' strategy backfiring, but only somewhat. Although turnout increased after 1964 in areas getting greater EOA funding, Democrats gained fewer of these votes than the Republicans did, and in the region where they gained the greatest share among the poor—the South—they appear to have lost more to the politics of race (see also Cascio and Washington 2014 on the role of the Voting Rights Act).

4.4 Remembering the War on Poverty: The Importance of Politics

Just a few years after Johnson's bold 1964 State of the Union declaration, the OEO's demise began. As early as 1967, Congress transferred control of some of its programs to local governments. Largely due to discontent over the Vietnam War, Johnson dropped out of the 1968 presidential election. Under President Richard Nixon, more OEO programs

were transferred to other federal agencies and, ultimately, the office was disbanded. The conventional wisdom about the War on Poverty and the OEO has become that they failed.

How this conventional wisdom came to be is not precisely known. It emerged in the late 1960s and early 1970s as the official poverty rate plummeted to reach its historic low of around 11 percent in 1973, down from 19 percent in 1964. Even though presidents are generally held accountable for changes in economic outcomes occurring during their administration (regardless of whether they caused them), Johnson did not get credit for the *30 percent drop* in poverty (6.2 percentage points, over 19.1 official poverty rate in 1964) from 1964 to 1968.

One hypothesis for the conventional wisdom that the War on Poverty failed is that its programs did not work. Recent work, however, suggests that the War on Poverty had a large impact on poverty (Bailey and Danziger 2013). Some of this effect was immediate. Recent work extending the supplemental poverty measure (which takes a fuller accounting of changes in non-cash transfers and those through the tax code) backwards in time shows that poverty rates fell from almost 26 percent in 1967 to 16 percent today, a fall greatly aided by programs begun under the War on Poverty (Wimer et al. 2013). A complementary, consumption-based measure of poverty registers a 26 percentage point decline from 1960 to 2010, with just over two-thirds of this decline occurring before 1980 (Meyer and Sullivan 2012). Many benefits were also longer-run in nature: a growing literature argues that many War on Poverty programs were fairly successful at increasing human capital, improving health, and reducing racial inequality over the longer term (Gillezeau 2012; Almond et al. 2011; Ludwig and Miller 2007; Almond et al. forthcoming; Alston and Ferrie 1999, 1993; Bailey 2012; Bailey and Goodman-Bacon 2013; Chay et al. 2010; Bailey 2013; Cascio et al. 2010; Cunningham 2013). In short, it seems puzzling that the Johnson administration did not get credit for some of these successes.

A second hypothesis is that the "failure" narrative reflects the success of critics in

rewriting history. But this claim forgets the fact that President Ronald Reagan's quip in his 1988 State of the Union that "the federal government fought the war on poverty, and poverty won" was not new. Allegations of the War on Poverty's failure dates to critics in both political parties. Accounts from the late 1960s argued that the CAP programs were born of conflicting ideas and administrative chaos—the programs of professors, not practitioners (Levine 1970; Forget 2011).[26] Prominent scholars agreed saying that the War on Poverty's "promises were extreme; the specific remedial actions were untried and untested; [and] the finances were grossly inadequate" (Ginzberg and Solow 1974, p. 219).

The difference in historical memory of the War on Poverty and the New Deal is striking. Although the New Deal's effectiveness as a set of policies has been contested in scholarship, its policies—in spite of a large or immediate rebound in private sector employment—were regarded as successful at the time and today are remembered as successes. This collective memory of the New Deal's success transcends party lines. When criticized for dismantling New Deal programs, Reagan corrected reporters noting he had voted for Roosevelt four times and remarked, "I'm trying to undo the Great Society...it was LBJ's war on poverty that led us to our present mess" (Berkowitz 2001, p. 98). Although the Great Depression lasted, Roosevelt's New Deal programs are not remembered as failures.

This paper's analysis provides hints regarding a third hypothesis for the belief that the War on Poverty failed: implementation. Our finding that the Johnson administration distributed CAP funds to poorer areas—much more than those with greater political importance—shows how differently the War on Poverty was waged than the New Deal. Rather than including and empowering state and local politicians and community leaders in the allocation process as in the New Deal, OEO funds were used to circumvent and challenge these interests. OEO funds flowed to poor and nonwhite areas, which empowered

[26]Moynihan, Daniel Patrick. "The Professors and the Poor." *Commentary*, August 1968. Other critics argued that not enough money was spent on the poor and that the Johnson administration did too little to effect change (King 1968; Katznelson 1989).

new constituencies of poor and African Americans. Our quantitative analysis underscores these differences. Measured by partial R^2, Fishback *et al.*'s (2003) political variables explain 25 percent of the county-level distribution of New Deal spending. The equivalent variables during the War on Poverty explain roughly *one percent* of the variation in OEO county-level spending (see appendix table D.5). Unlike New Deal funding, OEO grants did not flow to (or away from) areas with powerful congresspersons or meaningfully reward swing voters that helped Democrats win the most liberal Congress since the New Deal.

In line with many contemporary accounts and retrospectives, our analysis suggests that OEO funding generated backlash and appeared to hurt Democrats in the late 1960s and early 1970s—especially relating to the politics of race in the South. Unlike the New Deal, which engendered good will for decades, the War on Poverty generated resentments—and, in the shorter term, votes for Republicans, especially in areas with more African American voters.

Given Johnson's ambition to recreate Roosevelt's style and political reputation, the differences between the political economy of the New Deal and War and Poverty are striking. Just days after Kennedy's assassination, a staffer advised Johnson to focus on small, popular policy proposals instead of bold—and divisive—goals like civil rights. Johnson responded, "What the hell's the presidency for?" (Caro 2012, p. 428). The OEO's focus on fighting poverty and racial discrimination—over politics as usual—is consistent with this humanitarian vision. The quantitative picture that emerges from our analysis of the War on Poverty is that of a sincere attempt, albeit an underfunded one, to champion change.

APPENDICES

APPENDIX A

Charitable Giving Data Appendix

IRS/NCCS 990 Data

My sample of IRS form 990 data is taken from the IRS Statistics of Income micro data, as cleaned and documented by the Urban Institute's National Center for Charitable Statistics (NCCS). This is a detailed data set including most data items on the form 990. The data are tiered by asset classes, including 100% of the largest organizations and decreasing shares of smaller organizations by total assets, with thresholds for asset size varying a little bit each year; however, the same small organizations tend to be observed each year of the panel, indicating that these are *not* cross-sections selected by stratified random sampling but that the IRS has tried to make a somewhat balanced panel with extra weight on the largest organizations.

The NCCS variable names of 990 data used are tabulated in table A.1.

Table A.1: Form 990 data items by NCCS variable code

	Statistics of Income			Core Files
	Form 990EZ 1982-1999	All Forms 990 2000-2007	Form 990EZ 1992-1999	1989-2007
Employer Identification Number	ein		ein	ein
Organization Name	name		name	name
State	state		state	state
Primary Metropolitan Area	pmsa		pmsa	pmsa
Major Subsector (NTEE)	ntee1		ntee1	ntee1
Major Subsector (12 groups)	ntmaj12		ntmaj12	ntmaj12
Total Revenue	e047	r270	ez31	totrev2
Total Contributions and Grants	e024	r040	ez17	cont
Direct Contributions	e021	r010	--	--
Indirect Contributions	e022	r020	--	--
Government Grants	e023	r030	--	--
Total Investment Income	--	rz100*	ez20	invinc
Interest Income	e027		--	savint
Dividend Income	e028		--	secinc
Net Rental Income	e031		--	netrent
Other Investment Income	e032		--	othinvst
Memberships and Dues	e026	r060	ez19	dues
Program Service Revenue	e025	r050	ez18	progrev
Total Assets, Beginning of Year	e177	a030	ez47	ass_boy
Total Assets, End of Year	e178	a180	ez48	ass_eoy
Net Assets or Total Fund Balances, Beginning of Year	e194		ez41	neta_boy
Net Assets or Total Fund Balances, End of Year	e195	n040	ez43	fundbal
Fundraising Expenditure	e050	x030	--	solicit
Program Service Expenditure	e048	x010	--	--
Filed Group Return	e012	cond	--	frcd
Compensation of officers, total	e060	f825	--	compens
Other salaries and wages, total	e064	f830	--	othsal
Total payroll tax	e076	f845	--	paytax
IRS exemption letter date	--	--	--	ruledate

Notes: *rz100 for 990EZ only.

TAXSIM Cost of Giving

The tax cost of giving measure used in this paper is created by estimating a first-dollar marginal cost of giving cash for a constant set of individual tax returns, indexed for inflation and calculated for each state in each year. The only change in the measure is therefore in state and federal laws.

I start with the IRS Public Use File for 1984, a cross-sectional sample of 79,556 individual income tax returns for that year. For each state s and year t from 1979 to 2007, I (1) replace the year variable (`data103`) with year value t; (2) replace the state variable (`data6`) with numeric state code s; (3) replace variables for cash contributions (`data58`), gifts of appreciated assets (`data59`) and carryover contributions (`data60`) with zero values; and (4) use the Consumer Price Index to adjust all other money variables from year-1984 dollars to year-t dollars. This modified data set is fed into the `taxpuf9` FORTRAN program, which calculates the federal and state tax income tax for each return i — call them $Federal^0_{i,s,t}$ and $State^0_{i,s,t}$. I then repeat the calculation, changing only the value of cash contribution to \$10. The individual's tax cost of giving is calculated as the change in total income tax liability:

$$TaxCost_{ist} \equiv \frac{\left(Federal^0_{i,s,t} + State^0_{i,s,t}\right) - \left(Federal^{10}_{i,s,t} + State^{10}_{i,s,t}\right)}{10} \tag{A.1}$$

For a small number of observations, the implied marginal rate can be very large. I censor $TaxPrice_{ist}$ above at a marginal rate of 100% and below at 0% before aggregating.

The state-year-level tax cost is then calculated by taking a mean weighted by sampling weight (`data1`) and reported contributions (`data58+data59+data60`).[1]

[1]Because there was an above-the-line contribution in 1984, non-itemizers had an incentive to report their contributions in the 1984 tax return data. Weighting by reported contributions will be incorrect to the extent that contributions are misreported.

$$TaxCost_{st} = \frac{\sum_i \texttt{data1}_i * (\texttt{data58}_i + \texttt{data59}_i + \texttt{data60}_i) * TaxCost_{ist}}{\sum_i \texttt{data1}_i * (\texttt{data58}_i + \texttt{data59}_i + \texttt{data60}_i)} \tag{A.2}$$

Guidestar Mission Statement Data

Form 990 mission statements are looked up by searching the Guidestar database (`www.guidestar.org`) for each employer identification number (EIN) of organizations in the sample. Of the 4,356 EINs attempted, 3,984 had retrievable mission statements; 363 were in the Guidestar database but had no recorded statement; 315 were not in the Guidestar database. Organizations were encoded as nonlocal if the mission statement or organization's name included any of:

- "Global"
- "National" or "International"
- "World"

Macroeconomic Data

The nonprofit regressions in chapters 2 and 3 include in some specifications a set of logged macroeconomic variables observed at the state-year level, intended to capture time-varying changes in the resources of possible donors and the demands on charity services perhaps not captured by fixed effects.

Macroeconomic variables from Federal Reserve Economic Data (FRED) include population [series code POP*x*], unemployment rate [UR*x*], and per capita income [PCPI*x*], where *x* is the two-letter postal abbreviation of each US state.

Gross state product data are from the U.S. Bureau of Economic Analysis's U.S. Economic Accounts:Regional:GDP by State and Metropolitan Area:Gross Domestic Product by State:Gross Domestic Product:All Industries. The data were extracted as two separate files, one for 1997 following the SIC breakdown of industries, and one from 1997 onward following the NAICS system, and the two merged together. Because only total product was of interest, the change in classification schemes was not relevant.

State-by-year poverty rates are aggregated from March Current Population Survey microdata, as maintained by the Integrated Public Use Microdata Series (King et al. 2010). State-year poverty rates were calculated using weighted shares of households in each state below 100% of the poverty line, as observed in the `POVERTY` variable.

Table A.2: Change in Average Tax Cost of Giving by State, 1986 to 1988

State	Tax Rate		Log Diff.	State	Tax Rate		Log Diff.
	1986	1988	1986–8		1986	1988	1986–8
Alabama	32.2%	19.4%	0.173	Montana	33.2%	23.3%	0.138
Alaska	32.7%	20.6%	0.164	Nebraska	34%	19.1%	0.204
Arizona	32.8%	21.7%	0.154	Nevada	30.8%	14%	0.218
Arkansas	30.8%	14%	0.218	New Hampshire	30.8%	14.6%	0.21
California	35.1%	20.9%	0.198	New Jersey	30.4%	16.1%	0.186
Colorado	32.8%	19.6%	0.179	New Mexico	32.7%	20.6%	0.165
Connecticut	30.6%	15%	0.203	New York	34%	19.7%	0.196
Delaware	34.3%	21.8%	0.174	North Carolina	33.2%	22%	0.154
District of Columbia	35.4%	23.5%	0.168	North Dakota*	37.6%	19%	0.261
Florida	30.8%	14%	0.218	Ohio	30.2%	16.8%	0.176
Georgia	33.1%	20.6%	0.171	Oklahoma	33.1%	20.4%	0.173
Hawaii	35.5%	24.9%	0.151	Oregon	34.7%	23.3%	0.162
Idaho	35.1%	21.3%	0.193	Pennsylvania	30.5%	16%	0.19
Illinois	30.5%	16.1%	0.188	Rhode Island	34.6%	19.8%	0.204
Indiana	30.4%	16.8%	0.179	South Carolina	34.6%	21.4%	0.184
Iowa	33.3%	21.4%	0.163	South Dakota	30.8%	14%	0.218
Kansas	32.7%	18%	0.198	Tennessee	30.8%	14.7%	0.208
Kentucky	32.8%	20.4%	0.169	Texas	30.8%	14%	0.218
Louisiana	31.8%	17.6%	0.188	Utah	32.9%	21.5%	0.157
Maine	34.6%	21.2%	0.186	Vermont	35.2%	19.8%	0.214
Maryland	32.5%	19.7%	0.174	Virginia	33.1%	20.4%	0.173
Massachusetts	30%	17.9%	0.159	Washington	30.8%	14%	0.218
Michigan	30.3%	17.3%	0.171	West Virginia†	34.2%	17.2%	0.23
Minnesota	34.2%	21.5%	0.175	Wisconsin	31.8%	20.8%	0.148
Mississippi	32.7%	19.3%	0.181	Wyoming	30.8%	14%	0.218
Missouri	32.2%	19.2%	0.176				

Notes: * North Dakota calculates impossibly high marginal rates pre-1986 and is dropped. † West Virginia repeals its contribution deduction in 1987 and is dropped.

APPENDIX B

Charitable Giving Additional Results

Median State Change as Treatment/Control Divide

The continuous difference-in-differences specification used in section 2.3 of this paper implicitly assumes a log-linear relationship between change in the tax cost and change in contributions. This assumption can be relaxed somewhat by splitting states by tax change into "treatment" and "control" groups and comparing across the two. The downside of this alternative specification is the loss of information within the state groups.

I use above- and below-median state treatment groups to conduct a difference-in-difference analysis of individual nonprofits. I estimate

$$\ln(Contributions_{it}) = \alpha_i + \beta D86_{s(i)} * Post86_t + \delta_t + \mathbf{X}'_{\mathbf{st}}\boldsymbol{\gamma} + \varepsilon_{it} \tag{B.1}$$

where $Contributions_{it}$ is real direct contributions reported by organization i in year t; δ is a year or region-by-year effect; $D86$ is equal to 1 (0) if state $s(i)$ has tax price change above (below) median for 1986-8; *Post86* is equal to 0 for years 1982-3 and 1985 and equal 1 for

years 1988-2007; α_i is an organization fixed effect. The coefficient of interest is β, which captures the difference in contributions between states with above- versus below-median changes in tax price following the TRA86.

The results of this regression are reported in table B.1. For the basic version of the regression, reported in column 1, organizations located in state with an above-median tax cost increase receive about 14% lower direct contributions than organizations in states with below-median increases. The size of this gap is even larger when we add region-by-year effects (-20%, column 2), state-year macro variables (-15%, column 3) or both (-16%, column 4). All of these estimates are statistically different from zero at the 5% level using state-clustered standard errors. The mean log difference in tax cost across states is 0.03, which means these estimates translate into an elasticity of contribution receipts with respect to average tax cost of -4 to -5, consistent with the magnitudes in table 2.2. (That is, a one percent increase in the average tax cost is associated with about a four percent decline in contribution receipts.)

Table B.1: Difference-in-Difference (at Median)

	(1)	(2)	(3)	(4)
	Log Direct Contributions			
$D86_{s(i)} * Post86_t$	-0.141**	-0.202***	-0.147***	-0.155***
	(0.0632)	(0.0593)	(0.0449)	(0.0463)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓
Observations	16882	16882	16882	16882
R-squared	0.861	0.862	0.862	0.862
Number of Orgs	3273	3273	3273	3273

*** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by state.

Scatterplots of Changes in Revenues vs. Changes in Expenditures

The scatterplots in section 3.2 omitted the most extreme one percent of changes in either plotted variable for ease of viewing. This trimming does not change the results substantially. Figure B.1 repeats the first four panels of figure 3.3 with extreme values included. Though a few extreme values make it much harder to see what's going on in the rest of the distribution, the slopes of the fitted lines printed in the top left corner of each panel are not particularly different: figure B.1A fits a slope of 0.91 dollars in additional program expenditures for a dollar in program revenues (compared to 0.74 in figure 3.3A); figure B.1B estimates a slope of 0.28 dollars in program expenditures for a dollar of direct contributions (0.15 in figure 3.3C); on the other hand, a dollar of program revenues is associated with -0.06 dollars in additional saving according to the slope plotted in figure B.1C (0.15 in figure 3.3B) and an additional 0.61 dollars for an extra dollar of contributions in figure B.1D (0.73 in figure 3.3D). While extreme values influence the slope of the line, they do not qualitatively change the observed relationship.

Plotting these data in terms of dollar changes makes the slopes very easy to interpret, but make it possible that the relationships for the *largest* organizations, which are likelier than small groups to have big changes in dollar terms in their financial inputs and outputs, dominate the figures and the observed plots. I therefore present one more set of scatterplots with changes normalized to individual organization's size. For each variable X_{it}, including flows to net assets, direct contributions, program revenues, or program expenditures for organization i in year t, I calculate z-scores for the size of the observed change in terms of organization-level standard deviations from the organization's mean change.

$$Z_i(\Delta X_{it}) = \frac{\Delta X_{it} - \overline{\Delta X_i}}{\sqrt{\frac{1}{N_i}\sum\left(\Delta X_{it} - \overline{\Delta X_i}\right)^2}}$$

where $\overline{\Delta X_i}$ is the average of all first differences in variable X observed for organization i in the data, and Z is the organization-level standard deviations from the mean observed in period t for charity i. These renormalized variables allow examination of the relationship between large changes in charities' finances *relative to the norm* for that charity. The downside of this approach is that the slope of a fitted line will now be more difficult to interpret, since changes in terms of standard deviations have different dollar meanings across charities and across variables.

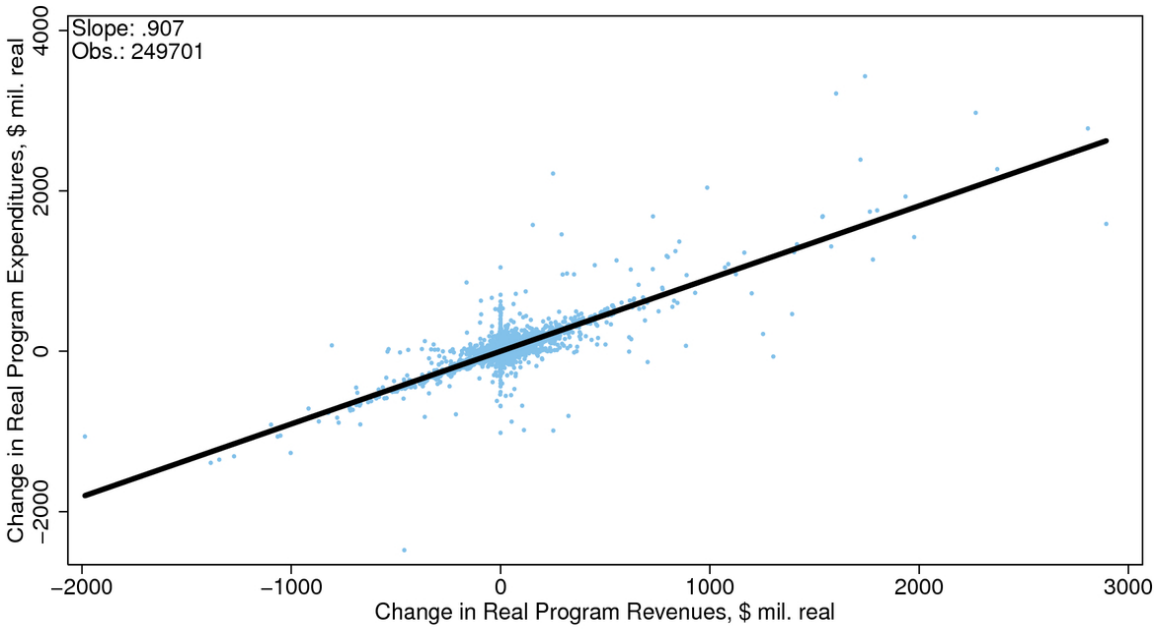
Figure B.2 presents scatterplots in terms of normalized variables. Consistent with figures 3.3 and B.1, the panels of figure B.2 reveal a strong positive relationship between program revenues and expenditures and between contributions and savings. Figure B.2A finds a one standard deviation increase in the growth of program revenues is associated with an additional 0.371 deviations in program expenditures; figure B.2B finds a one standard deviation increase in contributions is associated with just 0.036 standard deviations in program expenditures. On the other hand, a one standard deviation increase in program revenues is associated with an increase of 0.11 deviations in flows to net assets (figure B.2C), compared with an increase of 0.30 standard deviations for direct contributions (figure B.2D).

Estimates with Added Constants

Figure 3.4D plotted estimates of a time-varying treatment effect of a tax reform on fundraising expenditure plus an added constant. The charts in this section similarly add $10,000 to the dependent variable, maintaining a consistent sample while still using an (approximately) log-log regression specification.

Figure B.1: Changes in Expenditures vs. Changes in Revenues (All Observations)

A. Change in Program Expenditures vs. Change in Program Service Revenue (All Observations)



B. Change in Program Expenditures vs. Change in Direct Contributions (All Observations)

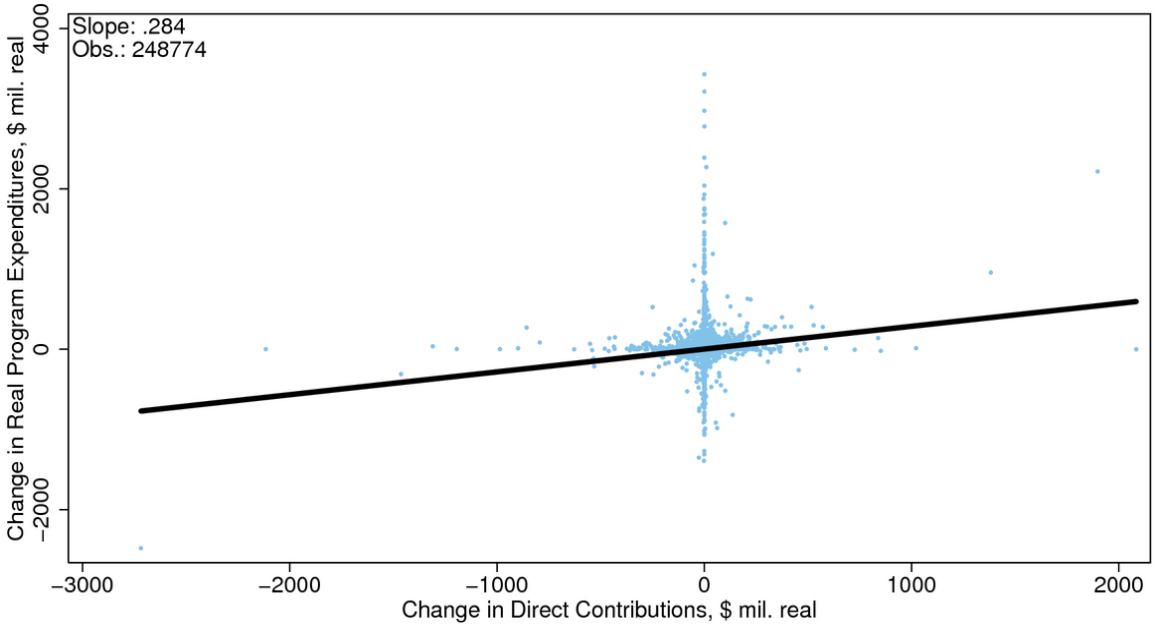
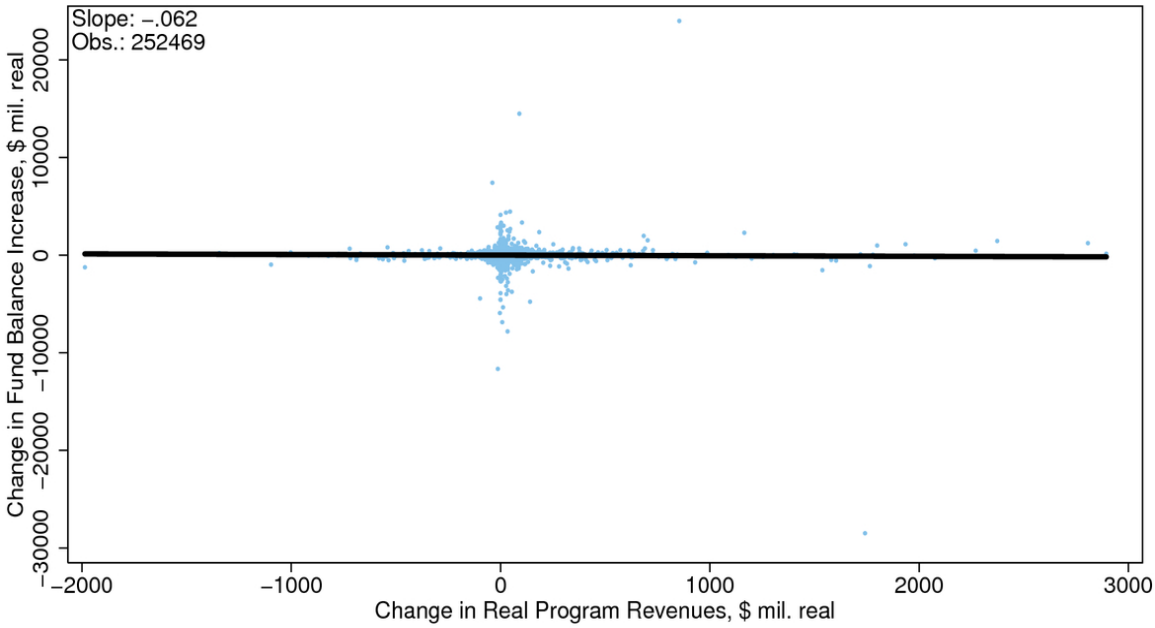
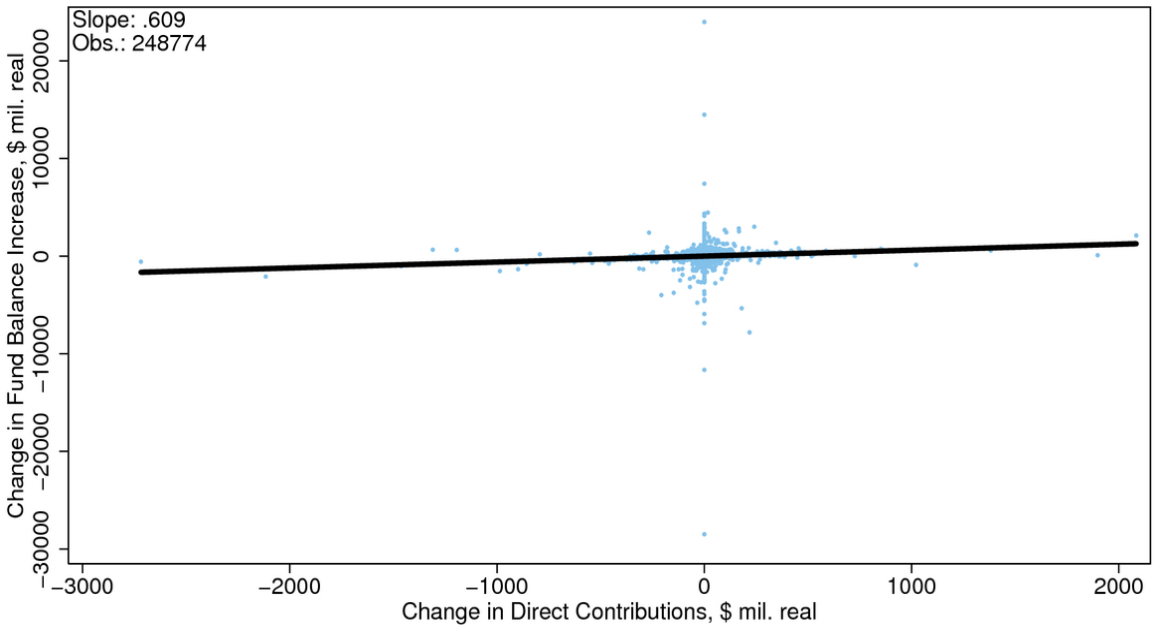


Figure B.1 — Continued

C. Change in Flows to Total Fund Balance vs. Change in Program Service Revenue (All Observations)



D. Change in Flows to Total Fund Balance vs. Change in Direct Contributions (All Observations)



Notes: See notes to figure 3.3.

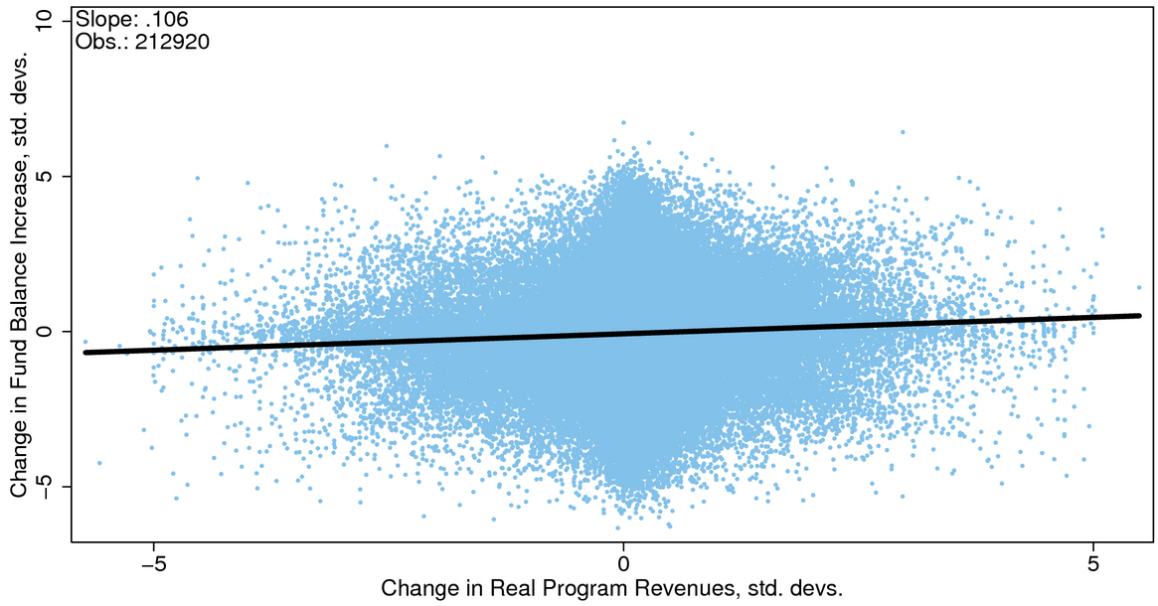
Figure B.2: Changes in Expenditures vs. Changes in Revenues (Standard Deviations)

A. Change in Program Expenditures vs. Change in Program Service Revenue (Standard Deviations)

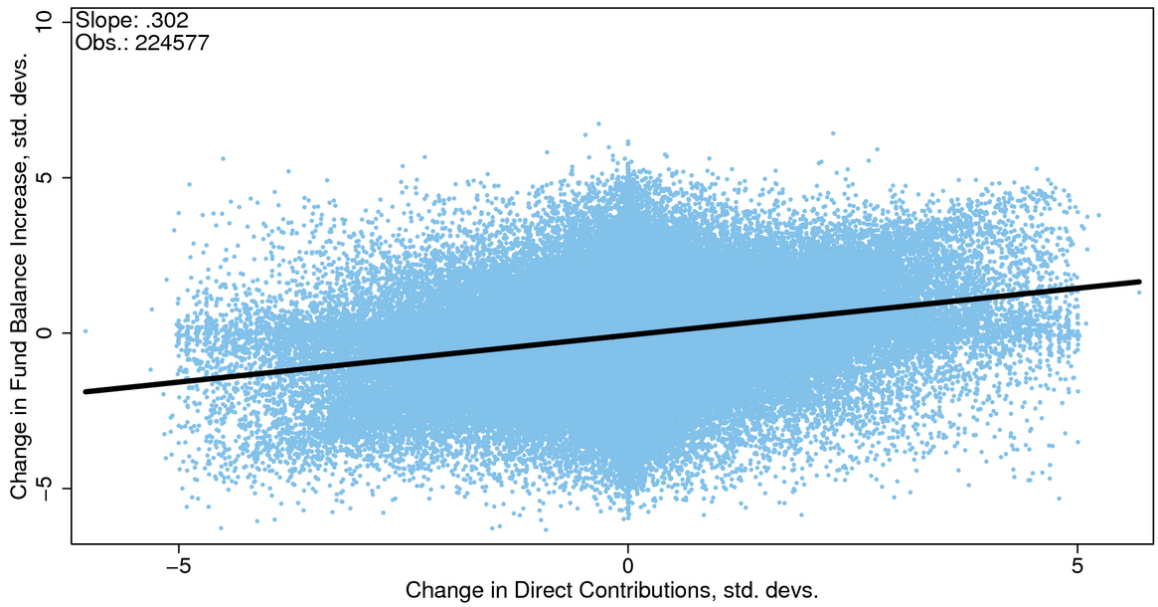
B. Change in Program Expenditures vs. Change in Direct Contributions (Standard Deviations)

Figure B.2 — Continued

C. Change in Flows to Total Fund Balance vs. Change in Program Service Revenue (Standard Deviations)



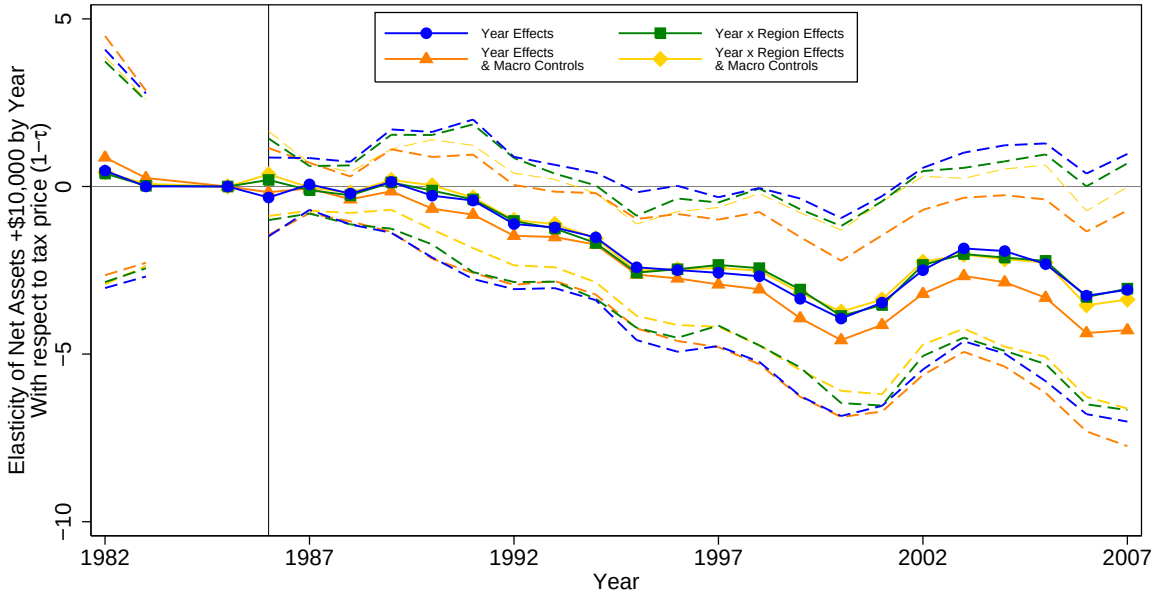
D. Change in Flows to Total Fund Balance vs. Change in Direct Contributions (Standard Deviations)



Notes: See notes to figure 3.3.

Figure B.3: Tax Elasticities by Year with Added Constants in Dependent Variables

A. Tax Elasticities by Year, Log of Net Assets + $10,000



B. Tax Elasticities by Year, Log of Program Expenditures + $10,000

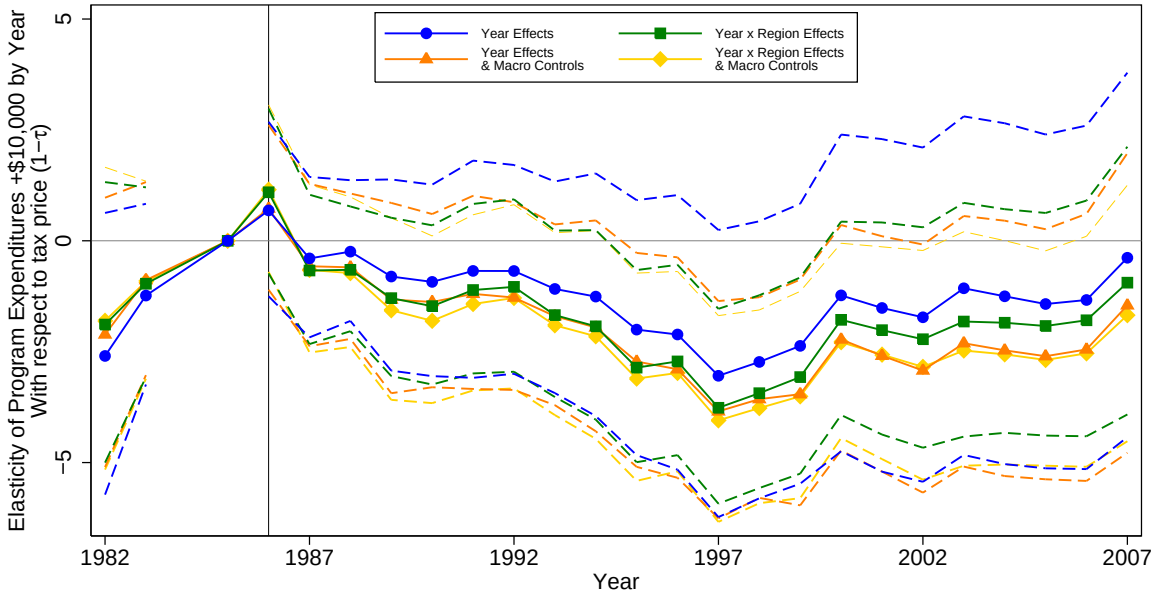
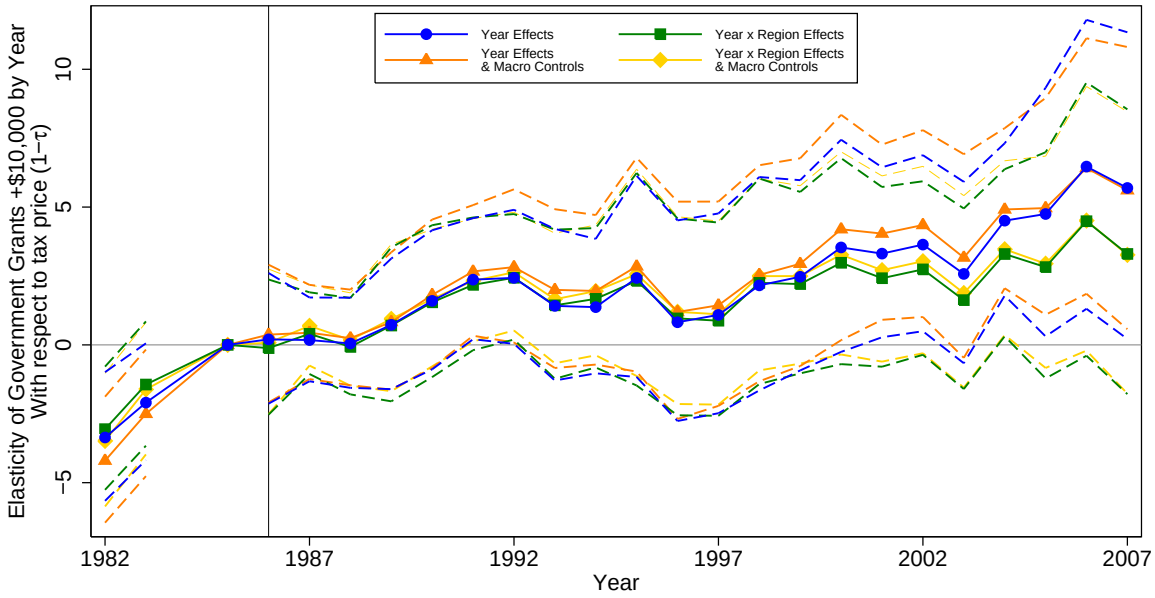
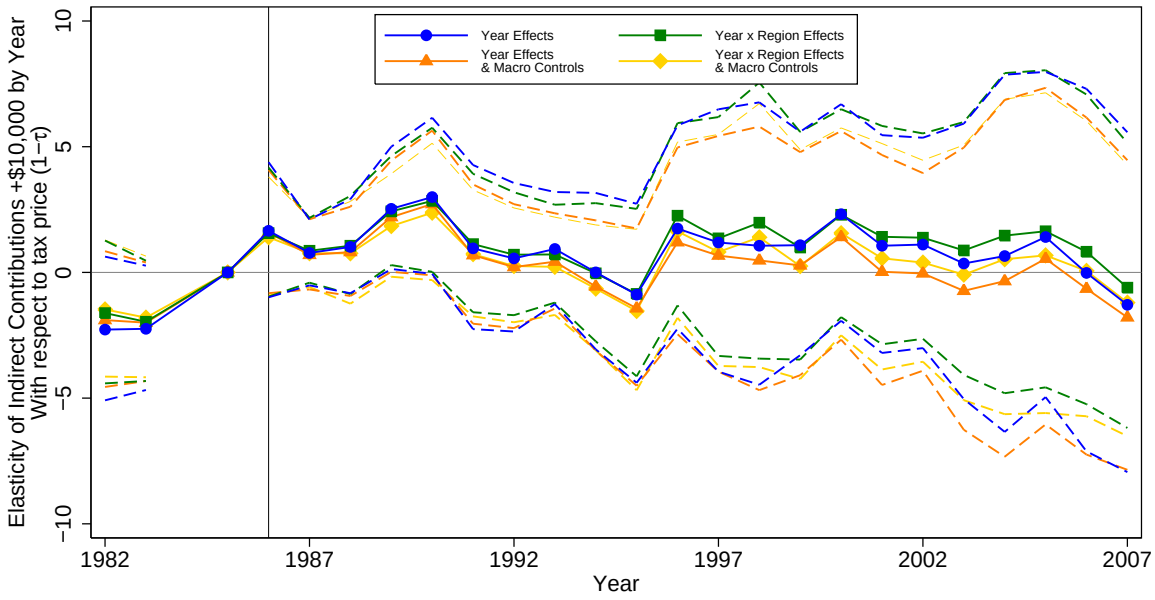


Figure B.3 — Continued

C. Tax Elasticities by Year, Log of Government Grants + $10,000



D. Tax Elasticities by Year, Log of Indirect Contributions + $10,000



Sources: See notes to table 2.2.

Additional Dependent Variables

For brevity, section 3.4 does not consider the effect of the 1986 tax reform on all financial variables of possible interest. Those additional results are presented in the panels of figure B.4. All are consistent with the behavior described in chapter 3, and arguably shed further light on charities' behavior.

As in section 3.4, all panels plot time-varying coefficients of the state treatment effect on some dependent variable, as specified in equation 3.5. One possible strategic response of charities to a shortfall in contributions would be to strive to increase program service revenue. Figure B.4A plots time-varying treatment effects of the tax treatment on program service revenue. Any effect is not visible before the late 1990s, when there is a modest decline in the estimates which is statistically significant in some specifications. Overall, figure B.4A is not strong evidence for an effect on endogenous program revenue decisions, but should not be taken as strong evidence against changes, either. Since figure 3.4A documents a negative effect on program expenditures, flat program service revenue may mean charities extract higher per-unit service charges for a lower level of provision. Unfortunately, form 990 data do not allow us to observe price increases directly.

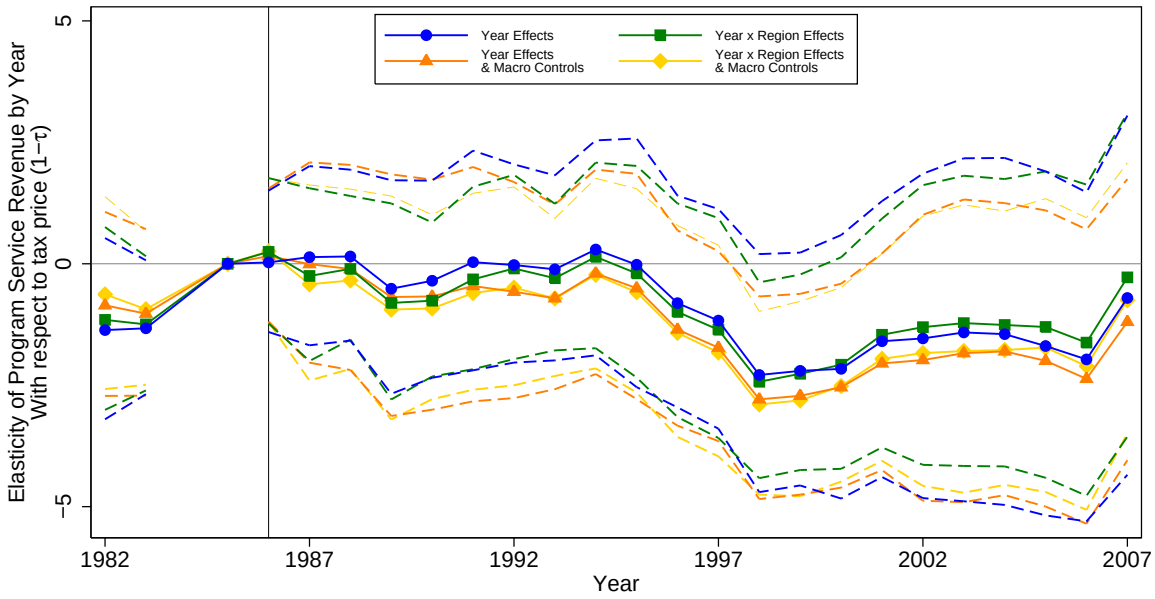
Since much of the existing literature on nonprofit behavior has debated the motivations of charity managers, we may also be interested in the effect of tax reform on managers' salaries. Figure B.4B plots time-varying estimates of the treatment effect on executive officers' compensation. The obtained estimates are very small and not consistently signed across time, suggesting that there is not an observable difference in executive salaries across states by treatment intensity.

This does not mean there is no effect on compensation, however. Figure B.4C plots estimates for the effect on *total* employee compensation, and consistent with the main results, differences in compensation expenditure increase steadily following the TRA86, with the

estimated effect at its most negative in 1997. Figure B.4D repeats this analysis for expenditure on payroll taxes, which can be thought of as a proxy for spending on lower-income employees (since above some maximum employees no longer owe payroll taxes). Taken as a whole, these results offer suggestive evidence that charitable contributions do not have a direct effect on trends in executive compensation in the nonprofit sector, but do affect demand for non-executive labor.

Figure B.4: Year-Varying Tax Elasticities by Year for Additional Dependent Variables

A. Tax Elasticities by Year, Log of Program Service Revenue



B. Tax Elasticities by Year, Log of Executive Compensation

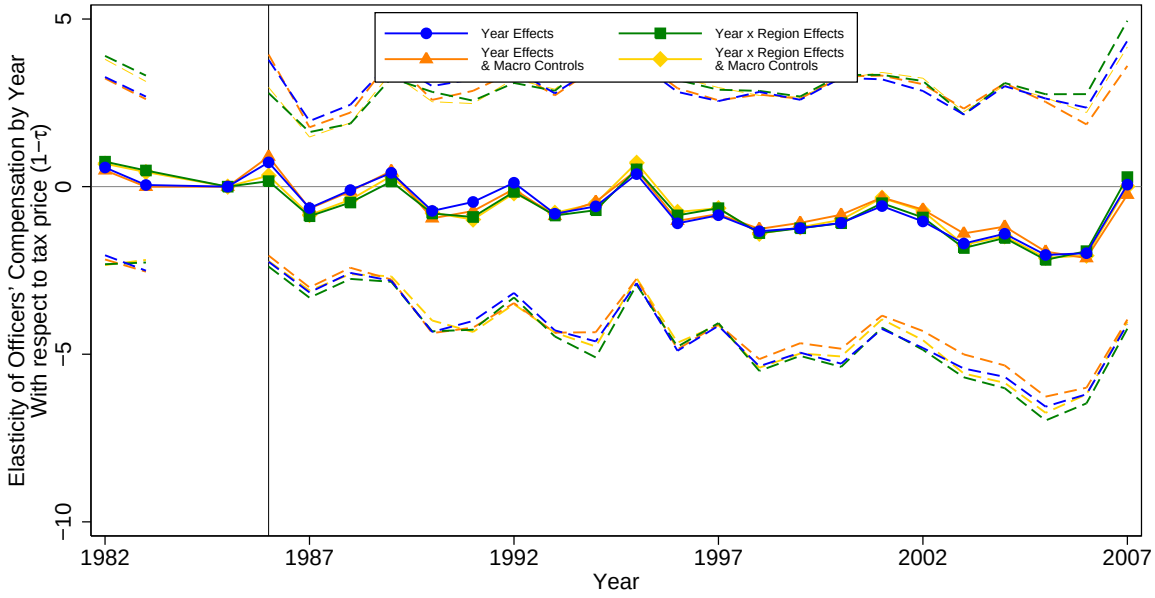
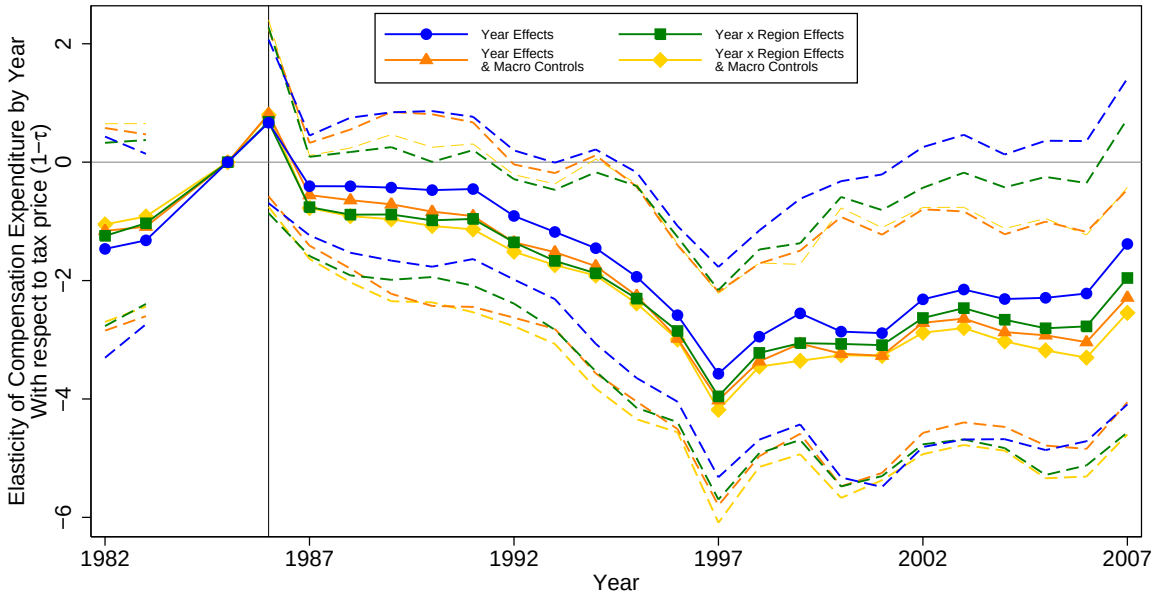
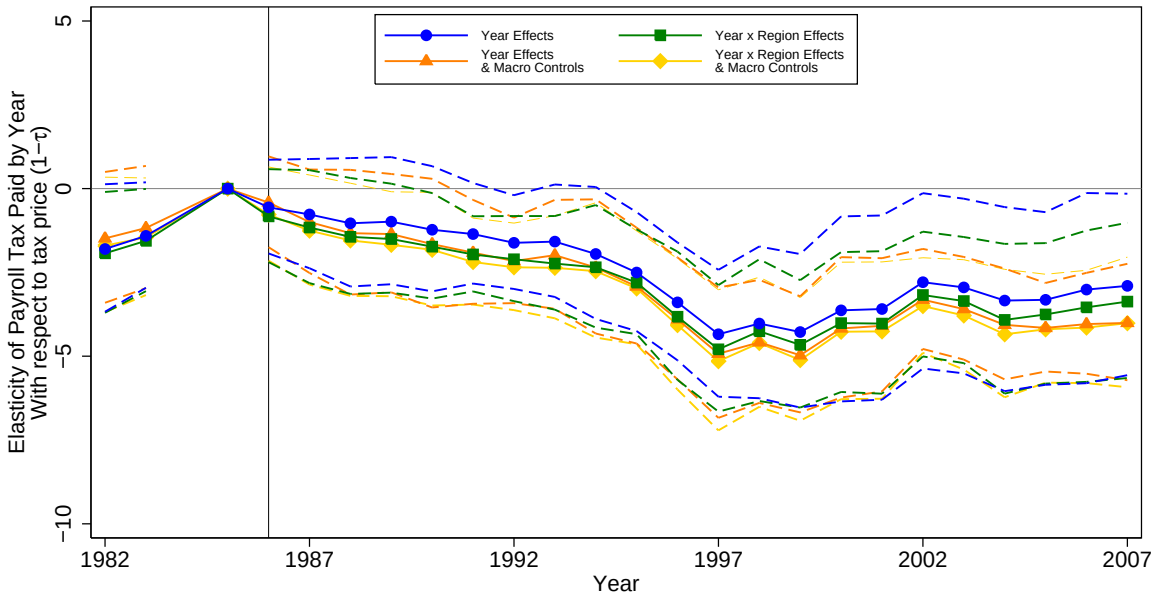


Figure B.4 — Continued

C. Tax Elasticities by Year, Log of Total Compensation Expense



D. Tax Elasticities by Year, Log of Payroll Tax Expense



Sources: See notes to table 2.2.

Tabulated Estimates of Time-Varying TRA86 Treatment Effects

This section tabulates the time-varying estimates plotted in figures 2.6 and 3.4.

Table B.2: Tabulated Estimates of Time-Varying Treatment Effects

A. Time-Varying Treatment Effects, Log of Direct Contributions

1982	-1.333	-0.837	-1.624	-1.528
	(2.321)	(2.458)	(1.876)	(2.035)
1983	-1.167	-0.552	-1.207	-0.882
	(1.214)	(1.184)	(1.129)	(1.110)
1986	-1.679	-0.898	-1.293	-0.492
	(1.593)	(1.135)	(1.416)	(1.011)
1987	-3.487*	-3.824**	-3.450**	-3.354***
	(2.031)	(1.445)	(1.633)	(1.219)
1988	-3.766**	-3.711***	-3.892***	-3.327**
	(1.470)	(1.265)	(1.391)	(1.351)
1989	-5.331***	-5.999***	-5.783***	-5.911***
	(1.668)	(1.171)	(1.576)	(1.375)
1990	-5.858***	-6.166***	-6.230***	-6.077***
	(1.376)	(1.173)	(1.260)	(1.097)
1991	-4.402***	-4.839***	-4.722***	-4.740***
	(1.567)	(1.599)	(1.325)	(1.405)
1992	-4.575***	-5.517***	-4.719***	-5.241***
	(1.647)	(1.371)	(1.531)	(1.333)
1993	-5.513***	-6.043***	-5.274***	-5.570***
	(1.918)	(1.686)	(1.882)	(1.636)
1994	-3.630*	-4.082**	-3.414*	-3.591**
	(2.116)	(1.940)	(1.990)	(1.724)
1995	-5.991***	-5.874***	-5.921***	-5.533***
	(1.912)	(1.923)	(1.864)	(1.765)
1996	-4.530**	-5.486***	-4.586**	-5.172***
	(2.122)	(1.918)	(2.154)	(1.902)
1997	-4.547**	-5.138***	-4.732**	-4.944***
	(2.045)	(1.774)	(1.834)	(1.720)
1998	-4.803**	-5.348**	-5.003**	-5.125**
	(2.092)	(2.086)	(2.227)	(2.156)
1999	-4.865**	-5.760***	-5.302**	-5.557***
	(2.029)	(1.923)	(2.171)	(1.878)
2000	-4.637**	-5.279**	-4.819**	-4.906**
	(2.220)	(2.354)	(2.003)	(1.995)
2001	-6.667**	-7.524***	-6.796**	-7.111***
	(2.833)	(2.726)	(2.722)	(2.361)
2002	-5.613**	-5.966**	-5.844**	-5.698**
	(2.376)	(2.479)	(2.235)	(2.170)
2003	-5.115**	-5.614**	-5.684**	-5.539***
	(2.403)	(2.346)	(2.273)	(2.038)
2004	-2.590	-3.783*	-3.525	-3.922*
	(2.453)	(2.127)	(2.358)	(2.052)
2005	-3.857	-5.454**	-5.122**	-5.768***
	(2.499)	(2.107)	(2.268)	(2.049)
2006	-2.180	-3.283	-3.861	-3.894
	(2.651)	(2.573)	(2.414)	(2.369)
2007	-2.578	-3.970	-4.353	-4.678*
	(3.009)	(2.640)	(3.177)	(2.631)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓

Table B.2 — Continued

B. Time-Varying Treatment Effects, Log of Program Expenditures

1982	-2.506**	-2.176**	-2.353**	-2.143**
	(0.973)	(0.962)	(0.980)	(1.055)
1983	-1.862***	-1.729**	-1.731**	-1.670**
	(0.628)	(0.661)	(0.686)	(0.723)
1986	-0.845	-0.552	-0.650	-0.395
	(0.552)	(0.527)	(0.587)	(0.556)
1987	-0.989**	-1.032**	-1.003*	-0.937*
	(0.443)	(0.413)	(0.527)	(0.484)
1988	-0.976*	-1.035**	-1.092*	-0.994*
	(0.514)	(0.451)	(0.627)	(0.527)
1989	-1.074*	-1.075*	-1.216	-1.074*
	(0.625)	(0.558)	(0.753)	(0.607)
1990	-1.308*	-1.441**	-1.574**	-1.524**
	(0.675)	(0.554)	(0.777)	(0.608)
1991	-1.233	-1.236**	-1.550*	-1.364*
	(0.765)	(0.591)	(0.882)	(0.701)
1992	-1.228	-1.129*	-1.498*	-1.235*
	(0.745)	(0.662)	(0.798)	(0.708)
1993	-2.026***	-2.014***	-2.269***	-2.082***
	(0.717)	(0.673)	(0.734)	(0.718)
1994	-2.099**	-2.091**	-2.246**	-2.108**
	(0.955)	(0.843)	(0.932)	(0.913)
1995	-2.677**	-2.681***	-2.852***	-2.739***
	(1.050)	(0.941)	(0.936)	(0.945)
1996	-2.838***	-2.893***	-3.071***	-2.993***
	(1.028)	(0.932)	(0.882)	(0.898)
1997	-4.105***	-4.325***	-4.388***	-4.477***
	(1.012)	(1.021)	(0.854)	(0.996)
1998	-3.720***	-3.779***	-4.003***	-3.951***
	(1.011)	(0.942)	(0.769)	(0.845)
1999	-3.436***	-3.405***	-3.803***	-3.627***
	(1.097)	(0.951)	(0.829)	(0.865)
2000	-2.532*	-2.385**	-2.866***	-2.555***
	(1.355)	(1.075)	(0.941)	(0.915)
2001	-2.536*	-2.390**	-2.939***	-2.585**
	(1.412)	(1.106)	(1.025)	(0.987)
2002	-2.726**	-2.596**	-3.143***	-2.838***
	(1.247)	(0.985)	(0.881)	(0.901)
2003	-2.106	-1.976*	-2.574**	-2.277**
	(1.419)	(1.155)	(1.044)	(1.018)
2004	-2.062	-1.982	-2.603**	-2.329**
	(1.414)	(1.207)	(0.994)	(1.028)
2005	-2.714*	-2.801**	-3.316***	-3.160***
	(1.372)	(1.177)	(0.914)	(0.988)
2006	-2.609*	-2.573**	-3.344***	-3.041***
	(1.457)	(1.242)	(0.949)	(0.976)
2007	-2.549*	-2.778**	-3.358***	-3.289***
	(1.387)	(1.144)	(0.949)	(0.972)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓

Table B.2 — Continued

C. Time-Varying Treatment Effects, Log of Net Assets

1982	0.418	0.416	0.736	0.416
	(1.869)	(1.738)	(1.870)	(1.800)
1983	-0.258	-0.378	-0.0122	-0.319
	(1.592)	(1.425)	(1.529)	(1.431)
1986	-0.00538	0.439	0.145	0.607
	(0.533)	(0.553)	(0.591)	(0.581)
1987	0.249	0.0978	0.182	0.211
	(0.432)	(0.406)	(0.396)	(0.383)
1988	-0.113	-0.117	-0.300	-0.0422
	(0.533)	(0.499)	(0.377)	(0.336)
1989	-0.256	-0.214	-0.558	-0.183
	(0.741)	(0.661)	(0.503)	(0.396)
1990	-0.562	-0.433	-0.932	-0.349
	(0.805)	(0.640)	(0.614)	(0.525)
1991	-0.826	-0.736	-1.236*	-0.733
	(1.048)	(0.876)	(0.708)	(0.568)
1992	-1.201	-1.158	-1.589**	-1.169**
	(0.988)	(0.764)	(0.672)	(0.549)
1993	-1.160	-1.172	-1.542**	-1.094
	(1.047)	(0.848)	(0.740)	(0.652)
1994	-1.281	-1.359	-1.605*	-1.272*
	(1.117)	(0.945)	(0.842)	(0.731)
1995	-1.812	-1.882*	-2.162**	-1.876**
	(1.231)	(0.991)	(0.848)	(0.761)
1996	-2.649**	-2.842***	-3.049***	-2.884***
	(1.115)	(0.960)	(0.875)	(0.804)
1997	-2.546**	-2.656**	-3.023***	-2.783**
	(1.198)	(1.133)	(1.096)	(1.065)
1998	-3.188**	-3.362***	-3.693***	-3.475***
	(1.247)	(1.137)	(1.072)	(1.041)
1999	-3.690***	-3.738***	-4.369***	-3.884***
	(1.256)	(1.131)	(1.046)	(1.055)
2000	-3.181**	-3.264**	-3.863***	-3.229**
	(1.387)	(1.327)	(1.181)	(1.228)
2001	-2.485*	-2.609*	-3.231**	-2.556*
	(1.454)	(1.376)	(1.264)	(1.320)
2002	-2.793*	-2.906**	-3.612***	-2.934**
	(1.491)	(1.255)	(1.133)	(1.137)
2003	-3.286**	-3.391***	-4.185***	-3.531***
	(1.487)	(1.250)	(1.160)	(1.106)
2004	-3.687**	-3.616***	-4.652***	-3.803***
	(1.557)	(1.336)	(1.245)	(1.194)
2005	-2.851*	-2.623*	-3.838***	-2.816**
	(1.686)	(1.469)	(1.410)	(1.275)
2006	-2.937*	-2.851*	-4.003***	-3.213***
	(1.717)	(1.445)	(1.392)	(1.183)
2007	-2.606	-2.339	-3.721**	-2.766*
	(1.980)	(1.668)	(1.740)	(1.513)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓

Table B.2 — Continued

D. Time-Varying Treatment Effects, Fundraising (Extensive Margin)

1982	-0.467	-0.504	-0.314	-0.381
	(0.530)	(0.607)	(0.523)	(0.603)
1983	-0.272	-0.334	-0.187	-0.261
	(0.378)	(0.445)	(0.337)	(0.411)
1986	0.312	0.354	0.328	0.336
	(0.329)	(0.382)	(0.337)	(0.376)
1987	-0.355	-0.420*	-0.400	-0.469**
	(0.267)	(0.221)	(0.291)	(0.227)
1988	-0.485	-0.742***	-0.536	-0.790***
	(0.310)	(0.235)	(0.335)	(0.251)
1989	-0.705*	-0.980***	-0.725*	-0.986**
	(0.362)	(0.359)	(0.398)	(0.383)
1990	-0.472	-0.697	-0.544	-0.706
	(0.405)	(0.419)	(0.448)	(0.436)
1991	-0.771*	-0.980**	-0.881*	-1.013**
	(0.423)	(0.445)	(0.462)	(0.462)
1992	-1.186**	-1.326***	-1.309**	-1.391***
	(0.457)	(0.467)	(0.494)	(0.465)
1993	-1.134**	-1.321***	-1.279**	-1.398***
	(0.461)	(0.451)	(0.485)	(0.432)
1994	-1.357***	-1.522***	-1.475***	-1.592***
	(0.505)	(0.501)	(0.532)	(0.478)
1995	-1.186**	-1.393***	-1.289**	-1.459***
	(0.489)	(0.467)	(0.504)	(0.441)
1996	-1.057*	-1.294**	-1.169*	-1.369**
	(0.617)	(0.578)	(0.627)	(0.559)
1997	-0.955*	-1.182**	-1.076*	-1.261**
	(0.543)	(0.518)	(0.560)	(0.505)
1998	-1.001*	-1.190**	-1.115**	-1.270**
	(0.503)	(0.489)	(0.528)	(0.478)
1999	-0.835	-0.942*	-0.954*	-1.033**
	(0.506)	(0.480)	(0.522)	(0.462)
2000	-0.998*	-1.075**	-1.106**	-1.139**
	(0.504)	(0.486)	(0.496)	(0.459)
2001	-1.117*	-1.117**	-1.242**	-1.190**
	(0.576)	(0.548)	(0.593)	(0.520)
2002	-1.421**	-1.388**	-1.535**	-1.459**
	(0.637)	(0.605)	(0.621)	(0.566)
2003	-1.357**	-1.432**	-1.461**	-1.502**
	(0.654)	(0.630)	(0.633)	(0.596)
2004	-1.289*	-1.436**	-1.371**	-1.482**
	(0.657)	(0.653)	(0.643)	(0.624)
2005	-0.901	-0.977	-0.965	-0.997
	(0.755)	(0.706)	(0.710)	(0.669)
2006	-1.095	-1.151	-1.169	-1.176*
	(0.756)	(0.694)	(0.713)	(0.668)
2007	-0.804	-0.857	-0.903	-0.878
	(0.710)	(0.650)	(0.683)	(0.645)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓

Table B.2 — Continued

E. Time-Varying Treatment Effects, Log of Fundraising Expenditure + $10,000

1982	-1.179	-1.314	-0.929	-1.016
	(1.841)	(2.121)	(1.715)	(2.025)
1983	-1.161	-1.231	-0.919	-0.957
	(1.416)	(1.649)	(1.302)	(1.509)
1986	0.222	0.379	0.341	0.350
	(1.094)	(1.284)	(1.195)	(1.335)
1987	-1.048	-1.206	-1.176	-1.296
	(0.883)	(0.834)	(1.019)	(0.940)
1988	-1.929*	-2.680***	-2.235**	-2.860***
	(1.042)	(0.860)	(1.110)	(0.992)
1989	-2.241**	-3.078***	-2.563**	-3.176**
	(1.073)	(1.035)	(1.266)	(1.235)
1990	-1.802	-2.574**	-2.084	-2.416
	(1.269)	(1.251)	(1.484)	(1.481)
1991	-2.415*	-3.331**	-2.859*	-3.307**
	(1.272)	(1.275)	(1.470)	(1.514)
1992	-3.890***	-4.397***	-4.462***	-4.590***
	(1.435)	(1.421)	(1.595)	(1.516)
1993	-3.439**	-4.075***	-3.950**	-4.204***
	(1.421)	(1.405)	(1.527)	(1.439)
1994	-4.410***	-4.983***	-4.972***	-5.191***
	(1.486)	(1.480)	(1.611)	(1.487)
1995	-4.100**	-4.715***	-4.731***	-5.060***
	(1.737)	(1.704)	(1.753)	(1.618)
1996	-3.259*	-4.112**	-4.011**	-4.484**
	(1.912)	(1.829)	(1.889)	(1.714)
1997	-3.160*	-3.949**	-3.938**	-4.362***
	(1.717)	(1.610)	(1.697)	(1.500)
1998	-2.833	-3.431*	-3.551*	-3.784**
	(1.902)	(1.836)	(1.845)	(1.649)
1999	-2.383	-2.766	-3.124	-3.185*
	(1.936)	(1.882)	(1.892)	(1.701)
2000	-3.335*	-3.747**	-3.707**	-3.725**
	(1.831)	(1.800)	(1.733)	(1.623)
2001	-3.572*	-3.895*	-3.962**	-3.887**
	(2.000)	(1.953)	(1.935)	(1.729)
2002	-4.129*	-4.408**	-4.559**	-4.512**
	(2.060)	(2.041)	(1.856)	(1.738)
2003	-3.956	-4.402*	-4.429**	-4.581**
	(2.424)	(2.365)	(2.177)	(2.079)
2004	-3.778	-4.528*	-4.214*	-4.608**
	(2.509)	(2.363)	(2.241)	(2.087)
2005	-2.161	-2.760	-2.606	-2.758
	(2.724)	(2.530)	(2.431)	(2.271)
2006	-3.425	-4.015*	-4.041*	-4.153*
	(2.556)	(2.365)	(2.291)	(2.096)
2007	-2.591	-3.326	-3.279	-3.433*
	(2.398)	(2.121)	(2.152)	(1.895)
Org. Effects	✓	✓	✓	✓
Year Effects	✓		✓	
Year*Region Effects		✓		✓
Macro Controls			✓	✓

Notes and Sources: See notes to figure 3.4.

APPENDIX C

War on Poverty Data Appendix

Our final dataset is compiled from multiple sources: some hand-entered, some shared by other researchers, and some publicly available at Inter-University Consortium for Political and Social Research (ICPSR), Integrated Public Use Microdata Series (IPUMS), or through the National Archives and Records Administration (NARA). We describe each of these data sources and our variables here.

State-Level Data

Information on CAP Grants

State-level information on federal CAP grants are derived from the NARA microdata (Community Services Administration 1981). These records are structured as two data files spanning 1965 to 1981. One dataset is observed at the level of 163,483 individual grant actions; the other dataset records data on the 4,769 organizations receiving grants. The combined data include information on any "action" on a grant (when it is recorded, extended, renewed or terminated), dates associated with these actions, and some information about the funded project. We use addresses from the grantee data and grant-action file,

which include the name and address of the designated grantee as well as the county where the services are provided in most cases. Because a larger urban organization may be funded to provide services in a rural area, we use information on the area of service delivery in all of our analysis, data permitting. We aggregate these amounts by the fiscal year of disbursement and state of service delivery[1].

We have verified these amounts by state against information printed in OEO annual reports for FY 1965, 1966, 1967, and 1968 (Office of Economic Opportunity 1965–8). The NARA data have systematically fewer CAP grants than the OEO reports in 1965, which can be attributed to missing Head Start grants in this first year. We are unsure about why these data are missing from the electronic files: the 1965 report tabulates Head Start grants separately from other CAP program grants, and includes all "approved" grants. Since Head Start began as a summer program in 1965, it may be that these grants are charged to the new fiscal year that began in July 1965 and counted in both 1965 and 1966 reports. The other FYs are very close in the NARA and OEO annual reports.

CAP grant-action data include data on the target population of grant proposals; taken together, grants from 1965-8 total 18 million people targeted by approved CAP grants. The OEO deemed these data unreliable in 1971 and ceased collecting it; available documentation does not indicate how grantees made their estimates or even whether the OEO believed the estimates were too high, too low, or simply too inaccurate.

Reconstruction of the EOA Apportionment Index, FY 1965-1968

Our reconstruction of the components of the EOA apportionment index in equation 4.1 requires three state-level components in each year from 1965 to 1967: (A) unemployment

[1]The grant-action file includes separate variables for the action's fiscal year, "signing date," and "obligation date," as well as a "termination date" in some cases. The documentation does not make clear the meaning of these variables or the distinction between them. The number of grant-actions where the fiscal year recorded and the fiscal year implied by signing and obligation dates accounts for a small share of federal dollars, and using either of these as an alternative fiscal year identifier does not change our results.

counts, (B) public assistance recipients, and (C) counts of children in families with household incomes below $1,000. The House Conference Report on the 1964 EOA (H.R. Rep. 88-1458 1964: 13-14) tabulates the current state-level data on these three measures that was used for FY 1965, and estimates the share of federal funds apportioned to each state based on the data. We hand entered these data for FY 1965.

The three components of the EOA apportionment index for the subsequent years were entered using the following sources.

(A) Unemployment counts come from the annual *Manpower Report of the President* for FY 1966 to 1968 (U.S. Department of Labor 1966-8). These totals are transformed into state shares of the national total.

(B) Public assistance recipients are derived from the *Social Security Bulletin*'s annual statistical supplements for FY 1966 to 1968 and transformed into state shares of the national total (Department of Health, Education and Welfare 1966, 1967, 1968). The annual supplement tabulates data for five public assistance programs: old age assistance, aid to the blind, aid to the permanently and totally disabled, aid to families with dependent children (AFDC), and general assistance. For each of these five programs, the *Bulletin* tabulates total expenditures and expenditure per recipient by state and program; total recipients are derived by state and program by dividing total expenditures ($) by expenditures per recipient ($/person), and then summing across the five categories. This risks double-counting recipients of more than one form of public assistance, but the number of such people are likely to be small relative to the total number of recipients, as old-age assistance and AFDC recipients exceed recipients of all the other three forms of assistance by orders of magnitude, and since relatively few elderly people have dependent children. Furthermore, to the extent that such double-counting occurs proportionally in every state, transforming total recipients into state share of the national total eliminates any problem.

(C) We found no state-level information on child poverty counts beyond the 1964 infor-

mation printed in the House Conference Report (H.R. Rep. 88-1458 1964: 13), nor do we know how subsequent figures would have been computed at the OEO. Consequently, our primary analysis uses the published 1964 child poverty rates for *subsequent* fiscal years. Because our state-level analysis relies on the index being what the OEO actually used, the lack of child poverty data could introduce measurement error. Therefore, we also examine the robustness of our results to a measure of child poverty that is linearly interpolated between 1964 and the 1970 census, which we compute using the 1970 IPUMS Census 1% sample.

County-Level Data

Except as noted, all data are reported at, aggregated to, or mapped onto consistent geographic units. The final data set contains 3,091 counties or super-counties.[2] Super-counties are the aggregations of individual counties into a single unit for analysis, either because some data are reported at a super-county level, or because county boundaries change over the period of study. These super-counties include:

1. the five boroughs comprising New York City;
2. Menominee, Oconto, and Shawano counties, Wisconsin;
3. La Paz and Yuma, Arizona;
4. Cibola and Valencia, New Mexico;
5. Washabaugh and Jackson, South Dakota;
6. Armstrong and Dewey, South Dakota;

[2]Throughout this paper, we use "county" to refer to both proper counties and county equivalents, including independent cities and Louisiana parishes.

7. Yellowstone National Park designated area and Yellowstone county, Montana;
8. Yellowstone National Park designated area and Park county, Wyoming;
9. Lexington city and Rockbridge county, Virginia;
10. Elizabeth city and Hampton city, Virginia;
11. Emporia city and Greensville county, Virginia;
12. Norfolk county and South Norfolk city become Chesapeake city, Virginia;
13. Princess Anne county and Virginia Beach city, Virginia;
14. Franklin city and Southampton county, Virginia;
15. Warwick county and city and Newport News city, Virginia;
16. Fairfax city and Fairfax county, Virginia;
17. Salem city and Roanoke county, Virginia;
18. Bedford city and Bedford county, Virginia;
19. Manassas city, Manassas Park city, and Prince William county, Virginia; and
20. Poquonson city and York county, Virginia.

For each of the datasets described below, we aggregated county information (count information is summed and share information is population weighted) to our set of consistently identified counties and supercounties and then linked these datasets together. Alaska, Hawaii and the District of Columbia are excluded from analysis. Additionally, Bradley and Independence counties, Arkansas, are missing 1960 presidential election data from all

sources, and are excluded from analysis. Necessarily, all are reported at the county level or lower.

Figure 4.3 maps per-capita CAP spending at the county level using GIS shape files obtained from the Newberry Library (Siczewicz 2011). We extracted county borders as of December 31, 1959 from their panel of US historical county boundaries. In a small number of cases where map counties were more disaggregated than our super-counties, the super-county value is mapped in each constituent county.

Demographic Information

Demographic information are data entered from the U.S. Census publications and posted at ICPSR by Michael Haines (2010; ICPSR dataset 2896). Variables from this dataset include shares of population living in urban, farm, and rural non-farm areas; shares of the population that are white and nonwhite; shares of population younger than five years old and older than sixty-four; share of population in households earning less than $3000 per year or more than $10,000; shares of population with less than or equal to 4 years educational attainment and with greater than or equal to twelve years.

Haines (2010) is also the source of several additional demographic and political controls included in table D.5, used to replicate as closely as possible for the 1960s the variables used in Fishback, Kantor and Wallis's (2003) study of the New Deal. These additional variables include the civilian unemployment rate, inverse population density, voter turnout, total population, change in per capita retail sales 1958 to 1963, share of population with less than four years of education, share of land in farms, average farm size in 1964, and change in average farm size 1959 to 1964.

County-level Measures of Local Government Spending

Data on county-level local government spending are collected from Volume VII, table 28 of the 1962 Census of Governments (U.S. Bureau of the Census 1964). We collect data on direct general expenditure, which includes all outlays other than outgoing intergovernmental transfers, direct public welfare expenditure, and total local tax revenue. Local government spending is an aggregate of all local governments in a county, including the towns, cities, townships, villages, school districts, special districts and county government itself. We divide these aggregats by total population to obtain per-capita figures.

County-level Information on Vietnam Casualties

Unfortunately, information on the number of men serving by county is not available from the National Archives or Selective Service, so casualty records are the best information we have on Vietnam mobilization. Data on Vietnam casualties is obtained from the NARA (U.S. Department of Defense 2008). The level of observation is the individual decedent. Records for 58,220 decedents contain date of death, state, and home city; most records also have data on home county.

We aggregate individual decedents and compute the cumulative number of deaths in Vietnam in a county per draft-eligible man ($\times$ 100) at t+2 for, because tours were two years long. The number of draft-eligible men by county are calculated using the number of males in the 1960 Census who would have been draft-eligible by year t+2. So, for the share of men in years 1960-70, we total males aged 8 to 20 in the 1960 Census. (The youngest would have been 18 by the start of 1970; the oldest 26 in 1966.) To confirm that this variable is not driven by outmigration of males in heavily drafted areas, we also construct a backward-looking denominator of *females* aged 21-34 in the 1970 Census. Using this alternative measure of Vietnam mobilization does not affect our results. In practice, deaths

appear to be a good proxy for mobilization. The association between two-year rolling windows of Vietnam casualties and lagged mobilization rates at the state level is 0.66.

In 1,709 cases where the decedent's city could refer to areas in more than one county, the county identifier is missing. When a decedent's city is uniquely named within a state, he is assigned to the county containing that city. When a decedent's city of origin is in multiple counties, we assign the casualty to the county containing the larger area or population mass of that city. For example, the incorporated area of Columbus, Ohio includes parts of three different counties, and Columbus decedents are coded as being from multiple counties. However, the largest, oldest, and most populous portion of Columbus is located in Franklin County, and we therefore assign Columbus decedents to Franklin County. When a decedent's city could refer to more than one populated area within a state, we assign the casualty to the county containing the largest populated area. For example, there are two places named Liberty in Illinois. One is a small town of a few hundred people in Adams County; the other is an even smaller, unincorporated place in Saline County. Two decedents from Liberty, Illinois are assigned to Adams County.

For 13 observations lacking county identifiers, a county cannot be assigned, and these decedents are dropped from the analysis. Once county assignments have been made, casualties are aggregated to the county-year level.

County-year casualty totals are then transformed into a proxy measure of mobilization at year t by dividing casualties from 1960 to year $t + 2$ by the number of males in the 1960 census eligible for service by year $t + 2$. For instance, for year 1966, we total all casualties in a particular county recorded from 1960 to 1968 and divide by the number of males in the 1960 census aged 10 to 20 (since all would have been greater than 18 and draft eligible by 1968). We look forward two years from year t to capture men who may have been drafted in year t and killed during their two-year tour of duty. We confirm that this measure is not driven by migration by calculating an alternative version using the 1970 census and women

of appropriate age from Haines (2010).

Information on Riot Intensity

Data on rioting was generously shared by Collins and Margo (2007). These data were originally collected by Carter (1986) and encompass 752 urban riots spanning from 1964 to 1971, observed at the level of individual riots.

We match city names for these riots to counties by hand and then aggregate to the county-year level. Where a city is located in more than one county (e.g. Columbus, Ohio) we match it to its principal county.

Following Collins and Margo, we measure riot intensity using an equally-weighted index of five measures of riot intensity (deaths, injuries, arrests, days of rioting, and number of arsons) as a proportion of the sum of these measures for all urban riots from 1964 to 1971 in a specific county-year, measured as shares of the total count of all of these measures in all years and multiplied by a scaling factor of 10,000. The county-year index value is the sum of all riot index values in that county and year. For instance, the riot data set in cumulatively counts 15,835 arsons, 69,099 arrests, 1,802 days of rioting, 12,741 injuries, and 228 deaths. The notorious July, 1967 Detroit riot had 7,231 arrests, 1,682 arsons, 491 injuries, 43 deaths, and lasted 9 days. Therefore the 1967 Detroit riot's index value is

$$\left(\frac{1682}{15835} + \frac{7231}{69099} + \frac{9}{1802} + \frac{491}{12741} + \frac{43}{228} \times 10,000 = 4430\right)$$

Counties with no riots observed in a particular year are assigned a zero value. The Detroit riots were much more extensive than several small riots more typical of the time. During 1968, the worst year of the rioting, there are 167 individual riots, with a mean index value of 126. Of the 330 individual riots observed from 1964 to 1968, the mean index value is 140 and the mean index value is 30.

County-level Poverty Counts

In addition to income measures from the Haines Census files, we obtain counts and shares of families living below $1,000, $2,000, $3,000 and below the poverty line from the 1960 Putnam file (Community Services Administration 1975). The documentation included with the National Archives' copy of the Putnam file explains,

> OEO needed a definition of poverty and identification of the extent and pattern of poverty in the United States. The definition did not exist previously, and no documentation was available on what constituted poverty. No one knew exactly how the picture of poverty in the United States looked or what programs would act to eliminate it in the nation... To establish this definition and pattern of poverty, the Bureau of the Census took the first step and created the PUTNAM FILE using data from the Census of 1960. The file acquired the name from its originator in the OEO, Israel Putnam... OEO and other agencies used their files recurrently... OEO also used these files to justify the allocation of funds, the Department of Agriculture used them to estimate the cost burden of the Food Stamp Program, and the Center for Infectious Disease in Atlanta used them to identify disease vectors.

The documentation suggest the Putnam file once included data by city and congressional district as well, but the National Archives holds a copy of county-level data exclusively. The documentation also indicates that the Putnam file is stored as EBCDIC plain text, but we found no software that was able to successfully convert the Putnam file to ASCII. To translate the data into a modern format, we did a "hex dump" of the underlying binary code into a text file recording each byte in hexadecimal (base-16) digits. We then compared individual bytes to a sample page of bytes and corresponding meanings in the National Archives documentation to "decode" the data format of the underlying file. We then translated the hexadecimal output directly into Stata format. We confirmed that this method correctly obtained the underlying data by aggregating poverty counts from the Putnam file and comparing the results to those published in a 1970 Census special report on changes in local poverty rates (U.S. Bureau of the Census 1975).

County-level Information on US Election Outcomes and Voter Turnout

We obtain votes cast by party by county for the 1960, 1964, and 1968 presidential elections from ICPSR (1995), "General Election Data for the United States, 1950-1990." From these data, we create variables for each election measuring the share of votes cast for the Democrat relative to the decisive vote, indicator variables for close vote shares within the county or for Democratic victory, and interactions of these variables. Because county-level 1960, 1964 and 1968 presidential election vote totals for Mississippi and Alabama have missing observations in ICPSR (1995), we hand-entered this information using official state publications: the *Alabama Official and Statistical Register* (1963) and *Mississippi Election Statistics* (1968). Bradley and Independence counties, Arkansas have missing data as well, but we could not find an alternative data source. These two counties are dropped from our analysis.

Specifically, we construct the *relative* vote share as half the difference between the Democratic vote share and the winner (in the case of a loss) or the second-place candidate (if the Democrat is the winner). In a two-party race, a 50-50 split sets this variable equal to 0; a 75-25 win is a relative share of +25 percent; a 60-40 loss is a relative share of -10 percent; etc. For the 1964 election, the share of ballots cast for third parties is small in all states, and the variable is extremely close to this ideal.

We obtain county-level estimates of voter turnout and measures of Democratic vote share in elections to the House of Representatives from 1950 to 1972 from (Clubb et al. 2006, ICPSR 8611). This dataset estimates the number of eligible voters using interpolation between decennial censuses and laws governing voter eligibility. As such, turnout can exceed one hundred percent when county vote totals exceed their estimated number of eligible voters; see their documentation for more details. Some counties are missing election data in this source in each election cycle; as such we use balanced panel of 2,585 counties

in our analysis of turnout and Democratic vote share that are observed in each election from 1950 to 1972.

Clubb et al. (2006) is also the basis of several additional political variables included in table D.5 to replicate as closely as possible for the 1960s the variables used in Fishback, Kantor and Wallis's (2003) study of the New Deal. These additional variables are the Democratic presidential vote share over 9 election cycles, the volatility (measured by standard deviation) of Democratic share over 10 election cycles, and the swing in Democratic share since the previous election.

County-level Information in Agriculture

An indicator variable for the Census "plantation county" designation is derived from a special report on 1910 cotton production in 337 counties characterized by plantation agriculture (U.S. Bureau of the Census 1916). The list of these counties was generously provided in machine-readable form by Paul Rhode.

Sharecroppers as a share of farm operators are derived from counts of total operators and croppers from the U.S. Censuses of Agriculture for 1930 and 1959. Data for 1930 and 1935 were kindly provided by Price Fishback, Michael Haines and Paul Rhode. Total operators for 1959 was obtained from Haines (2010). Sharecropper counts for 1954 and 1959 were hand-entered (U.S. Bureau of the Census 1959; Depew et al. 2013).

Measures of Congressional Power and Committee Chairmanship

Party identification and committee membership and rank data for members of Congress is observed at the level of the individual, with one observation per committee assignment, per congress (Nelson 1993). We matched these data to counties using a crosswalk constructed from the district number variable in Clubb, Flanigan and Zingale (2006) and maps in the *Atlas of Congressional Districts* (1964, 1966, 1968). Following Alston and Ferrie

(1999, p.45), we define the powerful committees of the House of Representatives as Ways and Means, Appropriations, Rules, Education/Labor, Judiciary, and Agriculture.

House representatives in leadership positions — that is, Speaker of the House or the leader or whip of either party — who do not also serve on committees are missing from the data set. We add information about these people and their leadership roles to the data manually, using the printed edition of Nelson's data (1993).

We use our crosswalk to turn these variables and their interactions at the level of the congress-representative-assignment level into variables at the county level. Some counties have more than one representative in the House. We create indicator variables equal to one if *any* county representative is a member of a powerful committee or in a House leadership position; if *any* representative is in chair or senior minority member of a powerful committee or in a House leadership position; if *any* representative is both a Democrat *and* a committee member or a leader; and if any representative is both a Democrat *and* a committee chair or member of the House leadership; otherwise, each variable is equal to zero.

We also create a variable equal to the share of a county's representatives that were Democrats. When counties have more than one representative, the variable for a Democratic representative is equal to (Number of county Democratic representatives) / (Total number of county representatives). Since most counties have a single House representative, in the 88th congress there are only 150 of 3,093 counties where this variable is not equal to zero or one.

Information on CAP Grants

See discussion on CAP grant data under state-level discussion. Note that the address of the provision of services may differ from the grantee's primary address, so we use information on the delivery of services to aggregate to the county level when available. For 55

grant actions where service delivery county codes are missing, grant actions are assigned to a local area based on the location of the grantee organization. For 24 grant actions missing both grantee and grant-action county codes, geographic codes are assigned by inferring a project's probable location from the project description string variable which contains this information. Then, federal CAP funding is aggregated to the county-FY level using these geographic codes and the dates of likely disbursement.

New Deal Spending and Covariates

Column 4 of Appendix Table D.5 adds a vector of correlates of New Deal spending to a county-level analysis of War on Poverty grants. These variables include demographic, political, and structural measures of the American economy up to year 1932, and are described in detail in the Data Appendix to the working paper version of Fishback, Kantor and Wallis (2003) (see NBER working paper No. 8309). The county-level data were obtained from Price Fishback's web site in June 2013.

Miscellaneous Data

County Boundaries

Figure 4.3 maps real 1965-8 per capita War on Poverty spending at the county level. County boundaries were obtained from the Newberry Library's Atlas of Historical County Boundaries and downloaded in August 2013. Shape files for counties as of December 31, 1959 were extracted from the full panel of US county shape data using ARCMap software. The map was created by merging our county-level data set with the extracted shape files using the `shp2dta` and `spmap` Stata .ado extensions. As described above, our dataset is observed at the level of "super-counties"; when a super-county corresponded to more than one county shape, the same super-county value is mapped to all component counties.

Roll Call Votes

Roll Call votes in the House and Senate for passage of the Economic Opportunity Act are taken from ICPSR dataset No. 4, "United States Congressional Roll Call Voting Records, 1789-1998." Roll call votes are coded as a 1 for a YES and 0 for a NO (including paired votes), and coded as missing for members not voting or voting present.

Congressional District Demographic Data

The roll call vote analysis in table D.6 uses demographic and economic data from the *Congressional District Data Book* for the 88th Congress obtained from E. Scott Adler's homepage.

Congressional Tenure

The comparative analysis with New Deal spending uses ICPSR and McKibben (1997)'s data on individual congresspersons to construct a measure of tenure. We define tenure as the total number of Congresses served by a Representative of Senator in their respective chamber at the time of the vote for passage of the Economic Opportunity Act. Partial terms (*e.g.* those who took office in a special election) are counted as equivalent to full terms. Non-consecutive terms are included, but service in the other chamber of Congress or in other political office is not.

Table C.1: Summary Statistics for County Dataset

		Mean	Median	SD
Federal CAP funds. (Cumulative in thousands 1968$, others current thousands $)	1965-68	783576	10528	4633000
	1965	46111	0	413798
	1966	188728	0	1101000
	1967	253857	0	1568000
	1968	265428	0	1445000
Local government direct general expenditures per capita	1962	183.1	171.1	77.78
Local government public welfare expenditures per capita	1962	9.743	3.001	15.48
Local government tax revenue per capita	1962	87.23	79.62	55.48
Share of Population ≤ 5 years old	1960	0.111	0.111	0.018
Share of Population > 64 years old	1960	0.106	0.104	0.033
Share of Nonwhite population	1960	0.107	0.024	0.165
Share of Population living in urban areas	1960	0.321	0.311	0.284
Share of Population living in rural farm areas	1960	0.226	0.204	0.162
Share of Population with income <$3K/year	1960	0.079	0.068	0.051
Share of Population with income ≥ $10K / year	1960	0.355	0.335	0.162
Vietnam deaths 1960-65 per Men aged 11-20 in 1960 x 100		0.129	0.117	0.12
Vietnam deaths 1960-68 per Men aged 10-20 in 1960 x 100		0.21	0.202	0.149
Vietnam deaths 1960-69 per Men aged 9-20 in 1960 x 100		0.256	0.243	0.161
Vietnam deaths 1960-70 per Men aged 8-20 in 1960 x 100		0.267	0.257	0.16
Collins-Margo Riot Intensity Index	1964	0.90	0	29
	1965	1.63	0	87.5
	1966	1.09	0	25.7
	1967	4.48	0	93.1
	1968	5.62	0	64.8
Democratic vote share for President	1964	0.0534	0.0765	0.151
Democrats win electoral votes	1964 (0/1)	0.731	1	0.443
Presidential vote margin within 10 pct. points	1964 (0/1)	0.233	0	0.423
Dem win * close election	1964 (0/1)	0.138	0	0.344
Democratic vote share for President	1960	-0.0208	-0.0388	0.134
Democrats win electoral votes	1960 (0/1)	0.38	0	0.486
Presidential vote margin within 10 pct. points	1960 (0/1)	0.275	0	0.446
Dem win * close election	1960 (0/1)	0.118	0	0.322
Croppers as share of farm operators	1930	0.0837	0	0.14
Percent Change in cropper share of operators	1930-1959	-33.64	0	42.56
Census plantation county designation	1910 (0/1)	0.109	0	0.312
Northeast	(0/1)	0.0689	0	0.253
Midwest	(0/1)	0.341	0	0.474
South	(0/1)	0.457	0	0.498
West	(0/1)	0.133	0	0.34

Table C.1 — Continued

		Mean	Median	SD
Democratic Share of US House Reps., 88th Congress	1964	0.589	1	0.48
House Reps. include major committee member/leadership, 88th Congress	1964	0.455	0	0.498
House Reps. include Democrat major committee member, 88th Congress	1964	0.247	0	0.431
House Reps. include major committee chair or leader, 88th Congress	1964	0.0456	0	0.209
House Reps. include Dem major comm. member/leadership, 88th Congress	1964	0.0197	0	0.139

Notes: Summary statistics for 3,091 counties in the 48 contiguous states (excludes Alaska, Hawaii, and District of Columbia; see data appendix). Sources: Riot data, Collins and Margo (2007); Vietnam data, Defense Casualty Analysis System; 1960 Census data, Haines (1995); Election outcomes, ICPSR (1997); Local government budgetary data, Census of Government (1962); Plantation data, U.S. Census Bureau (1910, 1930, 1959), Haines (2010) and Fishback, Haines and Rhode (2011).

Table C.2: Summary Statistics for Dataset on Voter Turnout and Share Democrat

	Year	Mean	Median	SD
Estimated Voter Turnout	1950	43.98	50.6	22.68
	1952	58.61	67.3	22.07
	1954	44.87	50.2	20.90
	1956	57.46	65.7	21.33
	1958	45.36	52.5	23.06
	1960	61.19	69.9	22.15
	1962	48.01	52.9	21.27
	1964	60.28	65.7	18.46
	1966	49.68	50.8	15.41
	1968	59.65	62.3	14.66
	1970	49.07	50.1	18.56
	1972	56.30	58.9	16.78
Democratic House Election Vote Share	1950	56.55	47.86	27.29
	1952	52.84	44	28.31
	1954	56.87	49.19	25.89
	1956	57.18	50.23	25.23
	1958	63.54	56.54	23.78
	1960	59.07	52.4	24.38
	1962	57.10	50.55	23.24
	1964	56.43	52.48	22.14
	1966	51.93	48.19	21.87
	1968	49.31	45.5	22.97
	1970	55.31	51	23.17
	1972	52.43	48.5	24.38

Notes: Summary statistics for a balanced panel of 2,585 counties in the 48 contiguous states (excludes Alaska, Hawaii, and District of Columbia). Source: Clubb, Flanigan and Zingale (2006).

APPENDIX D

War on Poverty Additional Results

Comparison of the New Deal with the War on Poverty

This article's political economy of the EOA can also be compared to research on the political economy of the New Deal.[1] Like the New Deal literature, we find that EOA spending was influenced by need and by presidential politics. Unlike the New Deal, we do not find evidence that the OEO attempted to allocate grants to areas with powerful congressmen. This is consistent with historical accounts of the lack of influence by local elites and Congress, or, perhaps, with Alston and Ferrie's hypothesis that some powerful Congressmen did not want EOA spending in their districts.

In his seminal analysis of reelection-seeking behavior of the Roosevelt administration, Gavin Wright (1974) constructs a measure of "political productivity" for each state that captured its electoral votes per capita and proximity to the 50% vote threshold to create a measure of expected electoral votes for FDR per New Deal dollar spent per capita.[2] His

[1] See Table 2 of Fishback et al. (2003) for a thorough overview of the literature and Fleck (2008).

[2] Wright assumes the cost of buying one vote is the same everywhere, so the formula is Index = Electoral votes * (probability of winning with 1% votes "bought" - probability of winning with no spending) / number of votes needed to buy 1% of electorate.

state-level analysis finds that New Deal spending is correlated with his measure of political productivity and that spending increased Democratic share in 1936 and 1938 elections, but not in the 1940 election. In contrast, he reports that federal work-relief job allocation predicts vote share in all three elections. Wallis (1987) adds annual data on state unemployment rates to the analysis and finds that Wright overstates the importance of politics relative to economic conditions. The series of articles that followed have investigated these findings further. Wallis (1998) found his own results were driven by a single outlier, Nevada, which had extremely high electoral votes per capita (over triple the second-highest state's) and which was represented the powerful Senator, Key Pittman, throughout the New Deal. Fleck (2001) argues that Wallis (1998) should control using land area rather than 1/population.

Because it is difficult to disentangle these competing hypotheses with state-level data, a new literature examines the correlates of New Deal spending at the county-level. Fleck (1999) shows higher voter turnout increased spending under the Federal Emergency Relief Administration in Southern counties. Fishback, Kantor and Wallis (2003) extend this analysis to all counties. Their baseline specification is

$$SpendingPerCap_i = \frac{\alpha_0}{Pop_i} + \beta_0 + \sum_i^n \beta_k X_{ik} + \sum_{s=1}^{48} \beta_s b_{si} + \varepsilon_i \tag{D.1}$$

where *SpendingPerCap* is per-capita New Deal spending from 1933-9 in county i, *Pop* is county population, β_0 is a constant (baseline spending per person), α_0 is also a constant (baseline spending per county), X_{ik} is one of k controls at the county or state level, b is a state fixed effect. Fishback, Kantor and Wallis exploit the greater number of observations available in a county-level analysis to include a variety of measures of political productivity and both 1/population and square mileage/population terms, encompassing both approaches of the state-level analysis.

To examine the robustness of our findings to Fishback, Kantor and Wallis's (2003) county-level model, our table D.5 replicates their elasticities (column 1) and then uses the same specification for our dependent variable of interest: real, cumulative CAP spending from 1965 to 1968 (column 2). Their regressors are changed to be the closest available analogues in more recent data. In particular, we add land area and some additional economic and political variables from the 1960 Census and 1962 and 1967 County Data Books (Haines 2010). Instead of tax returns, which were mandated for a much larger share of the population following the Second World War, we use share of households earning more than $10,000 to measure high-income households.[3] In addition, we use share of population with less than four years of education in lieu of literacy rate. Average tenure of congressional representatives for a county is measured by averaging the number of Congresses served by all representatives of a county for representatives serving at the time of the August 1964 vote on the EOA, using the Congressional data from ICPSR and McKibbin (1997).

Consistent with our preferred specification and with Fishback, Kantor, and Wallis's analysis, high per capita CAP spending is associated with measures of poverty (unemployment rate) and with presidential politics (mean Democratic vote over recent elections, Democratic swing from 1960 to 1964). As in our main table, the inverse of population and inverse of population density are not statistically significant predictors of CAP spending. Interestingly, we find a negative (though not statistically significant) relationship between a Representative's tenure and total CAP funding, where Fishback *et al.* find a positive relationship. Consistent with the Alston and Ferrie hypothesis, this seems to be driven by lower funding in the South, where Representatives generally had served longer. (The median county in the South had an average tenure of 6 terms across its Representatives; the North, Midwest and West all had medians of 3 terms.) Column 3 adds variables for rioting,

[3]The $5,000 filing requirement on the 1932 income tax return equals $8,663 in 1960 dollars, making $10,000 household income the closest equivalent income level in the available data.

Vietnam mobilization, and Southern paternalism, which do not meaningfully change the estimates.

These estimates confirm the robustness of our primary findings: that the OEO directed funds toward poorer areas, as well as those most valuable for presidential politics, while actually spending *less* in congressional districts held by powerful Southern congressmen, consistent with Alston and Ferrie's hypothesis. Overall, however, politics mattered far less for CAP spending relative to New Deal spending. Together, the political variables directly analagous to Fishback, Kantor and Wallis's have a partial R^2 of just 0.014 for the Community Action Program, compared to 0.206 for the New Deal.

Roll Call Voting Analysis of the Economic Opportunity Act of 1964

Footnote 6 of chapter 4 describes patterns of roll call voting on the Economic Opportunity Act; this section provides more detail on the roll call voting analysis. For a set of votes on the Economic Opportunity Act, we estimate a linear probability model

$$Y_i = \alpha + \mathbf{P_i'}\boldsymbol{\gamma} + \mathbf{X_i'}\boldsymbol{\beta} + \sum_{p=0}^{1}\sum_{j}(D_i = p) \times R_j(i) \times \delta_{pj} + \varepsilon_i \tag{D.2}$$

where Y_i is equal to 1 for a yea vote and 0 for a no vote on a particular roll call (including paired and announced votes), and missing if members voted present or did not vote (ICPSR 2010). $\mathbf{P_i'}$ is a column vector of political controls from election outcomes, including Democratic vote share and a close election dummy. In the House, Democratic vote share and the close election dummy are calculated with respect to the 1962 election to that House seat; since only one-third of Senate seats are up for election in a given cycle, Democratic vote share and close election dummy in the Senate regressions are for the 1960 Presidential election (Clubb *et al* 2006). $\mathbf{X_i'}$ is a column vector of socioeconomic controls including

black, urban, and farm shares of the population and the median income taken from Census estimates for congressional districts (Adler undated; U.S. Bureau of the Census 1963); δ_{pj} is a coefficient on a vector of interacted dummies for membership p in the Democratic party (D_i) and a district in each of j census regions (R_j). The residual is denoted ε_i and α is a constant. Standard errors are corrected for heteroskedasticity.

The results are reported in table D.6. In both chambers, the most important determinant of a positive vote for the Economic Opportunity is partisan identity: Southern Democrats were less likely to vote for the EOA than Democrats of any other region, but much more likely to vote for passage than Northeastern Republicans (who were themselves more favorable than any other regional block in the GOP). In addition to partisan and regional patterns, legislators from states or districts with high shares of black population were less likely to vote for the bill, though this effect seems to be driven by the inclusion of Southern legislators (columns 2-3 and 6-7). House members were significantly more likely to vote for EOA passage if unemployment in their districts was high.

Estimated Relationships between Election Outcomes and Demographics Over Time

Table D.1: County-Level Correlates of CAP Spending, by Urban Status

A. Urban Counties Only

	Dependent Variable: Real Federal CAP Expenditures per Capita, 1965–1968							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poverty Variables								
Population share in HH with incomes								
≤ $3K	37.01***					37.16**		
	(13.04)					(15.28)		
≤ $1K		108.97***					113.92***	
		(38.92)					(43.12)	
≤ $2K			53.67***					53.71***
			(16.10)					(17.68)
Share nonwhite				23.60*		22.03	12.05	16.62
				(13.02)		(15.63)	(12.64)	(13.75)
Political Variables								
1,000/Population					–16.83	–19.48	–12.90	–16.67
					(20.19)	(21.92)	(21.35)	(21.25)
For 1964 Presidential election:								
Change in share for Democrat,					36.72*	47.50**	49.46**	48.04**
1960–1964					(20.89)	(22.95)	(24.11)	(23.64)
Share for Democrat					–1.33	0.61	0.38	0.21
					(23.94)	(22.26)	(22.98)	(22.74)
1= Democratic won					2.18	1.10	1.06	1.14
					(5.24)	(4.90)	(4.94)	(4.93)
1= Election close (+/–10 points)					–1.01	–1.31	–1.07	–1.09
					(2.67)	(2.51)	(2.65)	(2.58)
1= Presidential election close					1.64	2.47	2.50	2.42
x 1=Democrat won					(4.65)	(4.33)	(4.44)	(4.41)
89th Congress House Representative(s)								
1= Democrat					2.18	1.92	2.02	2.05
					(2.22)	(2.07)	(2.12)	(2.10)
1= Major committee member/					0.93	0.88	0.95	0.95
Leader					(1.74)	(1.72)	(1.73)	(1.73)
1= Major committee member					–2.92	–2.88	–2.91	–3.02
x 1=Democrat					(3.30)	(3.13)	(3.19)	(3.18)
1= Major committee chair/					–2.21	–1.31	–1.08	–1.20
Leader					(1.67)	(1.61)	(1.63)	(1.58)
1= Major committee chair/					1.23	1.50	2.37	1.70
leader x 1= Democrat					(3.59)	(3.58)	(3.64)	(3.60)
Observations	1,545	1,545	1,545	1,545	1,545	1,545	1,545	1,545
R-squared	0.089	0.075	0.079	0.077	0.074	0.078	0.085	0.088
Partial R-squared								
Poverty variables	0.002	0.006	0.004	0.003		0.003	0.004	0.004
Political variables					0.007	0.008	0.008	0.008

Table D.1 — Continued

B. Rural Counties Only

	Dependent Variable: Real Federal CAP Expenditures per Capita, 1965–1968							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poverty Variables								
Population share in HH with incomes								
≤ $3K	76.60***					55.68***		
	(17.32)					(15.95)		
≤ $1K		136.29***					69.87**	
		(38.27)					(29.19)	
≤ $2K			105.03***					59.20***
			(25.11)					(17.93)
Share nonwhite				98.25***		98.31***	98.30***	94.60***
				(25.54)		(25.82)	(25.64)	(25.34)
Political Variables								
1,000/Population					9.27	12.28*	7.96	8.29
					(7.26)	(6.93)	(6.46)	(6.42)
For 1964 Presidential election:								
Change in share for Democrat, 1960–1964					12.99	45.91***	44.72***	44.30***
					(11.57)	(17.14)	(16.90)	(17.03)
Share for Democrat					47.35**	41.22**	41.42**	41.38**
					(19.84)	(18.51)	(18.51)	(18.51)
1= Democratic won					–8.12	–3.81	–3.86	–3.71
					(5.55)	(5.20)	(5.21)	(5.21)
1= Election close (+/–10 points)					–7.30**	–2.82	–2.92	–2.79
					(3.25)	(3.01)	(3.00)	(3.00)
1= Presidential election close x 1= Democrat won					6.25	1.25	1.40	1.21
					(5.09)	(4.78)	(4.78)	(4.79)
89th Congress House Representative(s)								
1= Democrat					–8.08*	–9.29**	–9.27**	–9.02**
					(4.70)	(4.59)	(4.59)	(4.58)
1= Major committee member/ leader					–8.54	–4.50	–4.68	–4.28
					(6.50)	(5.94)	(5.85)	(5.90)
1= Major committee member x 1=Democrat					13.46*	13.94**	13.57**	13.27*
					(7.24)	(6.93)	(6.81)	(6.88)
1= Major committee chair/ leader					5.75	3.77	2.95	3.38
					(6.87)	(5.40)	(5.67)	(5.59)
1= Major committee chair/ leader x 1= Democrat					–9.58	–10.09	–8.53	–9.44
					(7.91)	(6.72)	(6.93)	(6.90)
Observations	1,546	1,546	1,546	1,546	1,546	1,546	1,546	1,546
R-squared	0.242	0.244	0.250	0.275	0.238	0.301	0.298	0.300
Partial R-squared								
Poverty variables	0.013	0.020	0.021	0.035		0.054	0.047	0.048
Political variables					0.014	0.021	0.019	0.019

Table D.1 — Continued

C. Farming Counties Only

	Dependent Variable: Real Federal CAP Expenditures per Capita, 1965–1968							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poverty Variables								
Population share in HH with incomes								
≤ $3K	60.83***					35.58*		
	(22.64)					(21.07)		
≤ $1K		105.53**					21.02	
		(45.47)					(34.11)	
≤ $2K			84.81***					26.77
			(31.07)					(21.64)
Share nonwhite				113.58***		117.85***	121.15***	117.83***
				(34.17)		(33.96)	(34.27)	(33.76)
Political Variables								
1,000/Population					12.38	12.87	9.42	9.68
					(8.80)	(8.53)	(7.56)	(7.61)
For 1964 Presidential election:								
Change in share for Democrat,					22.67	48.45**	48.91**	48.08**
1960–1964					(17.52)	(21.72)	(21.67)	(21.73)
Share for Democrat					21.04	27.59	26.24	26.99
					(20.09)	(20.74)	(20.74)	(20.73)
1= Democratic won					–1.47	–0.36	–0.54	–0.46
					(5.31)	(5.21)	(5.21)	(5.21)
1= Election Close (+/–10 points)					–3.96	–1.20	–1.56	–1.38
					(2.94)	(2.82)	(2.80)	(2.81)
1= Presidential election close					–0.25	–2.32	–1.90	–2.12
x 1= Democrat won					(4.75)	(4.69)	(4.68)	(4.68)
89th Congress House Representative(s)								
1= Democrat					–3.03	–6.09	–6.11	–5.97
					(4.79)	(4.92)	(4.91)	(4.87)
1= Major committee member/					–3.42	–1.84	–2.16	–1.94
leader					(5.40)	(5.01)	(4.95)	(4.98)
1= Major committee member					5.52	8.18	8.36	8.11
x 1= Democrat					(6.77)	(6.69)	(6.62)	(6.63)
1= Major committee chair/					–0.24	–1.05	–1.48	–1.32
leader					(4.43)	(4.12)	(4.25)	(4.21)
1= Major committee chair/					–5.81	–6.12	–5.40	–5.63
leader x 1= Democrat					(6.29)	(6.13)	(6.20)	(6.19)
Observations	1,545	1,545	1,545	1,545	1,545	1,545	1,545	1,545
R-squared	0.050	0.051	0.054	0.086	0.050	0.101	0.099	0.100
Partial R-squared								
Poverty variables	0.005	0.006	0.009	0.024		0.030	0.026	0.026
Political variables					0.004	0.010	0.009	0.009

Table D.1 — Continued

D. Industrial Counties Only

	Dependent Variable: Real Federal CAP Expenditures per Capita, 1965–1968							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poverty Variables								
Population share in HH with incomes								
≤ $3K	89.39***					66.21***		
	(19.83)					(14.88)		
≤ $1K		244.17***					189.24***	
		(82.46)					(62.26)	
≤ $2K			115.35***					81.79***
			(28.98)					(18.77)
Share nonwhite				68.61**		63.60**	54.54**	59.56*
				(30.91)		(32.01)	(26.80)	(31.18)
Political Variables								
1,000/Population					6.16	9.88**	8.41	6.88
					(5.39)	(4.88)	(5.30)	(4.99)
For 1964 Presidential election:								
Change in share for Democrat,					24.51**	55.12***	51.54***	53.38***
1960–1964					(11.07)	(19.83)	(19.62)	(19.79)
Share for Democrat					57.44***	50.06***	48.54***	50.21***
					(18.07)	(17.00)	(17.42)	(17.13)
1= Democratic won					–11.71**	–10.45**	–9.43**	–10.08**
					(4.80)	(4.57)	(4.65)	(4.58)
1= Election close (+/–10 points)					–9.25***	–8.20**	–7.24**	–7.92**
					(3.42)	(3.24)	(3.18)	(3.25)
1= Presidential election close					18.33***	16.85***	15.70***	16.68***
x 1= Democrat won					(6.37)	(6.03)	(5.65)	(6.00)
89th Congress House Representative(s)								
1= Democrat					–0.39	–0.70	–0.12	–0.41
					(2.64)	(2.46)	(2.56)	(2.48)
1= Major committee					–3.43	–2.87	–2.51	–2.73
member/leader					(2.70)	(2.43)	(2.44)	(2.44)
1= Major committee member					–0.26	0.47	–0.06	0.20
x 1= Democrat					(3.71)	(3.40)	(3.49)	(3.41)
1= Major committee chair/					1.26	2.44	2.32	2.09
leader					(2.35)	(2.38)	(2.26)	(2.30)
1= Major committee chair/					0.34	–0.68	1.59	0.13
leader x 1= Democrat					(4.05)	(4.23)	(4.24)	(4.18)
Observations	1,546	1,546	1,546	1,546	1,546	1,546	1,546	1,546
R-squared	0.079	0.103	0.086	0.088	0.065	0.131	0.145	0.132
Partial R-squared								
Poverty variables	0.022	0.046	0.031	0.020		0.039	0.046	0.038
Political variables					0.028	0.032	0.029	0.030

Table D.1 — Continued

Notes: Urban counties are defined as those with an urban share of population in 1960 above the median (31.3%) and rural counties are defined as those at or below the urban share median. Farming counties are those above the median share of population living on farms in 1960 (20.4%) and industrial counties are those at or below the farming median. See table 2 notes for information on specification and sources.

Table D.2: County-Level Correlates of CAP Spending, 1965 to 1968, Using Alternative Cutoffs for "Close" Election

	Dependent Variable: Real Federal CAP Expenditures per Capita, 1965–1968							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Closeness cutoff	+/– 3%		+/– 5%		+/– 7%		+/– 15%	
Poverty variables								
Population share in HH with incomes								
≤ $3K		49.53***		48.89***		48.71***		49.36***
		(11.65)		(11.62)		(11.63)		(11.65)
Share nonwhite		85.58***		85.78***		86.15***		85.56***
		(21.59)		(21.67)		(21.67)		(21.80)
Political Variables								
1,000/Population	7.81	9.99*	7.82	9.94*	7.67	9.82*	7.75	9.99*
	(5.95)	(5.66)	(5.87)	(5.61)	(5.87)	(5.62)	(5.93)	(5.68)
For 1964 Presidential election:								
Change in share for Democrat,	26.35**	56.09***	27.06**	56.53***	27.39***	57.10***	26.53**	55.34***
1960–1964	(10.57)	(14.92)	(10.55)	(14.90)	(10.46)	(14.88)	(10.47)	(14.61)
Share for Democrat	28.91**	30.37***	29.60**	32.02**	28.71**	34.90**	14.21	21.80
	(11.96)	(11.67)	(13.42)	(13.13)	(14.25)	(14.10)	(17.13)	(17.03)
1= Democratic won	–3.65	–3.48	–5.15	–4.99	–5.70	–6.79	0.23	–0.72
	(2.69)	(2.61)	(3.53)	(3.44)	(4.27)	(4.24)	(6.83)	(6.73)
1= election close	–4.70**	–3.07*	–6.15***	–4.41**	–6.24***	–4.98**	–3.41	–1.03
	(1.92)	(1.84)	(2.14)	(2.02)	(2.30)	(2.31)	(3.31)	(3.33)
1= Presidential election close	8.35*	6.97	7.71**	6.77*	6.88*	7.40**	0.75	0.53
x 1= Democrat won	(4.61)	(4.33)	(3.71)	(3.58)	(3.64)	(3.68)	(5.16)	(5.09)
89th Congress House Representative(s)								
1= Democrat	–0.97	–2.33	–1.08	–2.40	–1.02	–2.31	–1.09	–2.37
	(2.51)	(2.43)	(2.52)	(2.44)	(2.51)	(2.43)	(2.50)	(2.42)
1= Major committee member/leader	–3.58	–2.24	–3.66	–2.29	–3.63	–2.22	–3.68	–2.23
	(3.07)	(2.81)	(3.08)	(2.82)	(3.08)	(2.82)	(3.09)	(2.82)
1= Major committee member	2.90	3.98	3.09	4.13	3.04	4.06	2.99	4.02
x 1=Democrat	(3.83)	(3.65)	(3.83)	(3.65)	(3.83)	(3.65)	(3.81)	(3.64)
1= Major committee chair/leader	1.67	2.25	1.77	2.30	1.69	2.10	1.91	2.42
	(2.32)	(2.20)	(2.33)	(2.21)	(2.33)	(2.20)	(2.33)	(2.22)
1= Major committee chair/leader	–2.53	–3.37	–2.73	–3.51	–2.76	–3.42	–3.03	–3.71
x 1= Democrat	(3.33)	(3.28)	(3.33)	(3.28)	(3.32)	(3.28)	(3.34)	(3.30)
Observations	3,091	3,091	3,091	3,091	3,091	3,091	3,091	3,091
R-squared	0.008	0.090	0.008	0.090	0.008	0.090	0.008	0.089

Notes: "Close elections" are defined using margins of +/- 3 percentage points from the pivotal vote (columns 1 and 2), +/- 5 (3 and 4), +/- 7 (5 and 6), and +/- 15 (7 and 8). Specifications are otherwise identical to columns 5 and 6 of table 2. See table 2 notes for information on specification and sources.

Table D.3: County-Level Correlates of CAP Spending, 1965 to 1968, Omitting Local Government Variables

	Dependent Variable: Real Federal CAP Expenditures per Capita, 1965–1968							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poverty variables								
Population share in HH with incomes								
≤ $3K	83.09***					59.62***		
	(16.44)					(12.99)		
≤ $1K		156.98***					90.68***	
		(38.81)					(27.42)	
≤ $2K			103.63***					62.06***
			(21.60)					(14.46)
Share nonwhite				87.77***		85.13***	82.82***	81.68***
				(21.77)		(21.74)	(20.70)	(21.40)
Political Variables								
1,000/Population					–1.28	5.59	0.87	0.98
					(3.80)	(4.36)	(3.73)	(3.90)
For 1964 Presidential election:								
Change in share for Democrat,					23.63**	54.98***	52.36***	53.00***
1960–1964					(10.30)	(14.86)	(14.65)	(14.77)
Share for Democrat					34.45**	35.43***	37.74***	36.31***
					(14.16)	(13.64)	(13.90)	(13.76)
1= Democratic won					–5.56	–5.25	–5.58	–5.34
					(3.62)	(3.51)	(3.54)	(3.52)
1= Election close (+/–10 points)					–6.75***	–4.69**	–4.92**	–4.70**
					(2.20)	(2.04)	(2.05)	(2.04)
1= Presidential election close					7.32**	6.37*	6.75*	6.52*
x 1=Democrat won					(3.70)	(3.57)	(3.57)	(3.59)
89th Congress House Representative(s)								
1= Democrat					–1.52	–2.66	–2.67	–2.50
					(2.51)	(2.41)	(2.42)	(2.40)
1= Major committee member/					–4.42	–2.66	–2.65	–2.54
leader					(3.22)	(2.88)	(2.86)	(2.87)
1= Major committee member					3.41	4.24	4.07	3.93
x 1= Democrat					(3.85)	(3.64)	(3.62)	(3.61)
1= Major committee chair/					0.38	1.35	0.49	0.82
leader					(2.29)	(2.10)	(2.10)	(2.10)
1= Major committee chair/					–2.15	–2.95	–1.52	–2.23
leader x 1= Democrat					(3.29)	(3.23)	(3.17)	(3.21)
Observations	3,091	3,091	3,091	3,091	3,091	3,091	3,091	3,091
R-squared	0.085	0.041	0.043	0.045	0.061	0.027	0.084	0.082

Notes: Local government variables are omitted from the set of suppressed controls. Specifications are otherwise identical to columns 1 to 8 of table 2. See table 2 notes for information on specification and sources.

Table D.4: County-Level Correlates of CAP Spending, 1965 to 1968, Regression Coefficients Suppressed in Tables 2 and 3

A. Regression Coefficients Suppressed in Table 2

	Dependent Variable: Real Federal CAP Expenditures per Capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1960 Census Variables (Share of Population)								
< 5 years old	202.51*	166.90	165.85	–62.14	286.32**	–74.53	–78.69	–79.09
	(116.67)	(105.92)	(111.61)	(87.66)	(130.33)	(90.41)	(90.40)	(90.76)
> 64 years old	–98.72**	–84.22*	–97.31**	–158.71***	–32.17	–219.18***	–200.42***	–208.12***
	(50.15)	(47.23)	(49.51)	(52.35)	(46.94)	(54.49)	(52.45)	(53.35)
Urban	1.64	–0.72	0.03	–1.25	–1.80	2.66	0.36	0.70
	(3.99)	(4.15)	(4.11)	(4.11)	(4.38)	(3.88)	(3.94)	(3.97)
Rural nonfarm	–41.17***	–39.91***	–43.36***	–17.56***	–17.94**	–25.62***	–22.40***	–24.14***
	(9.79)	(10.34)	(10.38)	(6.72)	(7.39)	(7.68)	(7.80)	(7.64)
Income ≥ $10,000	15.03	–2.88	11.07	–40.31***	–37.28**	6.98	–12.86	–5.94
	(17.62)	(16.11)	(16.64)	(15.16)	(15.36)	(17.58)	(15.62)	(16.09)
Census of Government (1962) Local Government Finance								
Direct total expenditures	–0.01	–0.00	–0.00	–0.01	–0.01	–0.01	–0.01	–0.01
per capita	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
Total tax revenue per capita	–0.05	–0.07*	–0.06	–0.09**	–0.11**	–0.07	–0.08*	–0.07*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
Welfare expenditures	0.31**	0.36**	0.31**	0.35**	0.35**	0.30**	0.34**	0.32**
per capita	(0.14)	(0.14)	(0.14)	(0.15)	(0.15)	(0.14)	(0.15)	(0.14)
Miscellaneous Social Factors								
Sharecroppers / total operators	1.44	–7.33	–7.15	–33.89**	17.16*	–20.87*	–24.12*	–23.95*
1930 (Southern paternalism)	(7.93)	(9.51)	(8.83)	(14.18)	(8.76)	(12.56)	(13.00)	(12.80)
Collins-Margo riot intensity	35.00***	35.27***	31.22**	10.32	60.53***	–2.57	1.75	0.04
Index, 1964–1968	(13.44)	(11.97)	(12.15)	(11.01)	(19.25)	(13.31)	(12.99)	(13.28)
Vietnam deaths 1960–1970 /	4.05	4.81	4.56	6.89	4.35	6.16	6.40	6.24
Males aged 8–20 in 1960	(4.85)	(4.83)	(4.83)	(4.70)	(4.95)	(4.73)	(4.77)	(4.76)
Observations	3,091	3,091	3,091	3,091	3,091	3,091	3,091	3,091
R-squared	0.045	0.050	0.050	0.070	0.038	0.090	0.089	0.089

Table D.4 — Continued

C. Regression Coefficients Suppressed in Table 3

	Dependent Variable: Real Federal CAP Expenditures per Capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1960 Census Variables (Share of Population)								
< 5 years old	–13.83	–20.99	–34.97	–78.06	44.99	–131.19	–125.54	–135.66
	(106.04)	(104.18)	(106.03)	(118.80)	(100.97)	(127.23)	(126.90)	(127.73)
> 64 years old	–72.47	–58.67	–69.99	–89.12	–43.87	–143.94**	–125.91**	–134.58**
	(55.86)	(52.54)	(53.81)	(57.94)	(46.63)	(61.62)	(59.42)	(60.40)
Urban	12.76***	11.12**	11.61**	10.75**	10.50*	12.23**	10.71*	10.94*
	(4.94)	(5.16)	(5.10)	(5.20)	(5.73)	(5.43)	(5.67)	(5.64)
Rural nonfarm	–15.93**	–15.62*	–18.08**	–5.65	–3.78	–11.58	–10.66	–12.97
	(8.10)	(8.32)	(8.58)	(7.25)	(7.46)	(8.38)	(8.36)	(8.63)
Income ≥ $10,000	11.49	–11.90	1.65	–37.97	–43.45	5.61	–16.98	–5.64
	(32.53)	(28.29)	(30.46)	(26.15)	(28.79)	(36.22)	(31.14)	(32.74)
Census of Government (1962) Local Government Finance								
Direct total expenditures	–0.05	–0.05	–0.05	–0.05	–0.05	–0.05	–0.05	–0.05
per capita	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Total tax revenue per capita	0.05	0.03	0.04	0.01	0.02	0.04	0.02	0.03
	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Welfare expenditures	0.26	0.30	0.28	0.41	0.29	0.24	0.27	0.24
per capita	(0.31)	(0.31)	(0.31)	(0.31)	(0.32)	(0.32)	(0.32)	(0.32)
Miscellaneous Social Factors								
Sharecroppers / total operators	7.39	5.05	4.81	0.68	15.29*	5.73	4.38	4.13
1930 (Southern paternalism)	(7.56)	(8.01)	(7.70)	(9.08)	(9.06)	(9.53)	(9.72)	(9.58)
Collins-Margo riot intensity	6.63	2.24	–0.30	–28.07	8.79	–39.56	–40.62	–41.56
Index, 1964–1968	(59.89)	(59.37)	(58.93)	(63.11)	(61.69)	(61.78)	(61.00)	(60.98)
Vietnam deaths 1960–1970 /	7.41	7.49	7.45	9.16*	7.69	8.27	8.30	8.13
Males aged 8–20 in 1960	(5.44)	(5.46)	(5.50)	(5.32)	(5.59)	(5.60)	(5.57)	(5.62)
Observations	1,414	1,414	1,414	1,414	1,414	1,414	1,414	1,414
R-squared	0.017	0.017	0.017	0.016	0.018	0.028	0.028	0.028

Notes: Specifications are identical to the specifications reported in columns 1 to 8 of table 2 (panel A) and table 3 (panel B). Estimates reported in tables 2 and 3 are suppressed here for brevity. See table 2 and 3 notes for information on specification and sources.

Table D.5: Comparision of Total Federal Grants Per Capita by County, New Deal and Community Action Program

	Fishback, Kantor, and Wallis	*Community Action Program, 1965–1968*		
	(1)	(2)	(3)	(4)
Relief and Recovery				
Growth retail sales per cap.	0.001	0.311*	0.311*	0.351*
Unemployment rate	0.058*	1.117*	1.106*	1.142*
% Farm failures[a]	–0.021*	–0.127	–0.117	–0.136
Redistribution and Reform				
Tax returns / % High income[b]	–0.06*	0.0621	0.419*	0.484*
Retail sales per capita	0.12*	–0.585*	–0.588*	–0.809*
% Black	0.02	0.109	0.0238	0.116
% Illiterate / % Low education[c]	–0.027	0.388*	0.00170	0.240
Average farm size	0.303*	0.0513*	0.0610*	0.0650*
Political Variables				
9-cycle Dem. pres. vote mean	0.14*	0.610	0.845*	
Presidential election swing	0.226*	0.0534*	0.0569*	
10-cycle Dem. pres. vote std. dev.	0.016	–0.0565	0.0758	
Pres. votes per population	0.58*	–1.725*	–2.118*	
Avg. tenure in House[d]	0.009	–0.0158	–0.009	
Structural Variables				
Inverse population	0.024*	0.0904	0.0764	–0.00215
Square miles per capita	0.067*	–0.0507	–0.0449	–0.0427
% Population urban	–0.004	0.114	0.245*	0.286*
% Land on farms	–0.278*	0.386*	0.358*	0.374*
Additional Controls				
% Families <= $3K Income, 1960			0.0904	0.0764
Sharecroppers, 1930			–0.0507	–0.0449
Vietnam deaths, 1960–1970 p.c.			0.114	0.245*
Riot intensity index			0.386*	0.358*
1964 Presidential Election Variables				
Dem Swing from previous election				0.175*
Democratic vote share				0.128*
Win county (0/1)				–0.177
Close election (< 10% margin)				–0.0665
Win * close				0.0540
State-fixed effects	Yes	Yes	Yes	Yes
Committee indicators	Yes	Yes	Yes	Yes
R-squared	0.426	0.107	0.114	0.108
Observations	3,060	3,067	3,067	3,067
Partial *R*-squared				
All variables except state-fixed effects	0.367	0.040	0.051	0.047
Political variables	0.206	0.011	0.014	0.005

Table D.5 — Continued

Notes: Elasticities for New Deal spending taken from the working paper version of Fishback, Kantor and Wallis (2003), Table 4, "Elasticities of relief, recovery, and reform variables: Total Grants." An asterisk ("*") denotes an elasticity that is significant at the 10% level in a two-tailed test. Congressional standing committees varied over time; our set of committee effects comprises indicator variables equal to 1 if the district was represented in the 88th Congress by a representative on one of the following committees: Appropriations, Agriculture, Banking, Education and Labor, Judiciary, Foreign Commerce, Merchant Marine, Public Works, Rules, and Ways and Means. Fishback, Kantor and Wallace use: Agriculture, Appropriations, Banking and Currency, Exports, Flood Control, Irrigation Control, Labor, Labor, Public Buildings, Public Lands, Rivers and Harbors, Roads, Ways and Means. Independent variables for OEO spending are the contemporary equivalents of the New Deal variables, with some substitutions: (a) Because farm failures are not presented at the county level in the 1963 census of agriculture, we use negative of the percent change in number of farms from 1958 to 1963; (b) Instead of tax returns per capita, we use 1960 share of population in households with income above $10,000 from the Putnam file; the filing cutoff for an income tax return in 1932, $5,000, equals $8,663 in 1960 dollars; (c) Instead of percent illiterate, we use 1960 share of population with less than four years of education. (d) We measure tenure in the House using the number of Congresses served as of the 88th Congress, for representatives as of the vote on the EOA. Partial R-Squared is calculated by taking the sum of the partial R-squareds for variables of interest from the Stata ado-file `pcorr2`.

Table D.6: Multivariate Regression of Affirmative Vote on EOA

	Senate Vote For EOA Passage July 23, 1964				*House Vote for EOA Passage August 8, 1964*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat	0.654***	0.672***	0.795***		0.887***	0.875***	0.765***	
	(0.0964)	(0.118)	(0.208)		(0.0515)	(0.0440)	(0.115)	
Electoral Outcomes								
Democratic vote	–2.079*	–1.718	0.982	–2.134**	–0.536***	–0.286	–0.518*	–0.480**
share	(1.077)	(1.299)	(2.676)	(1.056)	(0.194)	(0.174)	(0.283)	(0.189)
Close election	0.0652	0.0605	0.0623	0.00981	–0.0209	0.00443	–0.137	–0.0408
	(0.0911)	(0.107)	(0.202)	(0.0863)	(0.0352)	(0.0342)	(0.118)	(0.0356)
JFK Win 1960	0.193	0.0895	0.186	0.232**				
	(0.130)	(0.168)	(0.319)	(0.115)				
Region Dummies								
Midwest	–0.421***	–0.507**			–0.177***	–0.180***		
	(0.152)	(0.194)			(0.0434)	(0.0387)		
South	–0.268*				–0.163**			
	(0.148)				(0.0654)			
West	–0.357**	–0.328**			–0.195***	–0.166***		
	(0.142)	(0.141)			(0.0410)	(0.0397)		
Regions by Party								
Democrat * Northeast				0.205				0.691***
				(0.194)				(0.0817)
Democrat * Midwest				0.0762				0.704***
				(0.178)				(0.0784)
Democrat * South				0.227				0.557***
				(0.186)				(0.105)
Democrat * West				0.114				0.666***
				(0.160)				(0.0793)
Not Dem. * Midwest				–0.899***				–0.344***
				(0.157)				(0.0710)
Not Dem. * South				–0.494**				–0.141
				(0.207)				(0.116)
Not Dem. * West				–0.623***				–0.376***
				(0.221)				(0.0763)
Demographic Controls								
Black pop.	–1.753***	1.311	–3.665***	–1.975***	–0.495**	–0.152	–0.592	–0.498**
	(0.528)	(2.526)	(1.020)	(0.490)	(0.202)	(0.151)	(0.367)	(0.201)
Urban pop.	0.429	0.219	0.310	0.588	0.231*	0.151	0.0502	0.293**
	(0.610)	(0.946)	(1.156)	(0.487)	(0.124)	(0.107)	(0.280)	(0.131)
Rural farm pop.	0.662	0.809	3.049	1.248	0.348	0.0234	1.005	0.444
	(1.013)	(1.303)	(2.685)	(0.865)	(0.386)	(0.321)	(0.766)	(0.383)
Unemployment	21.57	13.73	–9.954	18.56	12.64***	4.429	32.96***	10.40***
	(14.17)	(17.66)	(41.71)	(12.90)	(2.866)	(2.723)	(8.611)	(2.706)
Median Income	–0.00246	0.00488	0.0315	0.0214	0.0142	–0.00745	0.107	–0.00206
	(0.0683)	(0.0796)	(0.171)	(0.0709)	(0.0302)	(0.0303)	(0.0695)	(0.0300)
Constant	–1.753***	1.311	–3.665***	–1.975***	–0.495**	–0.152	–0.592	–0.498**
	(0.528)	(2.526)	(1.020)	(0.490)	(0.202)	(0.151)	(0.367)	(0.201)
Observations	99	67	32	99	422	292	130	422
R–squared	0.541	0.639	0.410	0.602	0.594	0.776	0.245	0.617
Region	All	Non-South	South	All	All	Non-South	South	All

Table D.6 — Continued

Notes: standard errors are adjusted for clustering at the state level (*** p<0.01, ** p<0.05, * p<0.1).
Sources: Demographic data from Adler (undated) and U.S. Bureau of the Census (1963); Voting data from ICPSR (2010).

Table D.7: Congressional Election Outcomes by Demographic Variable and Year

		Turnout			*Democratic Vote Share*		
	Year Dummy	All	Non-South	South	All	Non-South	South
Share of population with income ≤ $3,000 x	1950	10.516***	9.138*	13.762***	–2.261	15.675***	–20.779***
		(3.386)	(4.960)	(4.560)	(3.886)	(4.247)	(7.458)
	1952	0.437	2.992	–3.791	–3.652	11.604**	–24.060***
		(3.225)	(4.813)	(3.862)	(3.870)	(4.751)	(6.075)
	1954	10.565***	10.225**	13.371***	–6.196	14.792***	–26.986**
		(3.181)	(4.165)	(5.046)	(5.881)	(5.086)	(11.442)
	1956	8.753***	10.870***	5.730	–4.632	8.145	–21.447***
		(3.136)	(4.192)	(4.792)	(4.268)	(5.786)	(6.111)
	1958	8.876***	11.199***	10.284**	–6.903**	–1.402	–10.696*
		(2.966)	(3.981)	(4.730)	(3.348)	(4.227)	(6.094)
	1962	11.423***	14.924***	11.437**	–1.624	0.028	–6.742
		(2.954)	(3.641)	(5.184)	(3.818)	(3.663)	(7.734)
	1964	2.774	5.288	0.572	6.562	–3.335	21.215**
		(3.009)	(3.471)	(5.367)	(5.093)	(4.649)	(10.236)
	1966	14.613***	22.430***	7.954	11.290**	1.445	26.282***
		(3.398)	(4.525)	(5.482)	(4.863)	(5.093)	(9.186)
	1968	5.964*	7.065	4.025	3.498	–0.105	9.932
		(3.312)	(4.442)	(5.380)	(5.485)	(5.785)	(10.417)
	1970	3.354	3.602	5.704	–1.702	–12.780**	17.085
		(3.587)	(5.181)	(5.334)	(6.287)	(6.010)	(12.543)
	1972	–5.237	–8.941	7.327	–2.742	–1.172	–7.879
		(5.508)	(9.439)	(5.276)	(6.422)	(6.597)	(13.079)
Share nonwhite x	1950	2.784	–1.728	4.573*	–6.401**	–6.709	–3.272
		(2.126)	(5.099)	(2.431)	(3.164)	(4.204)	(4.517)
	1952	–0.195	2.770	0.314	2.049	–8.727**	7.352**
		(1.715)	(3.684)	(2.018)	(2.952)	(4.002)	(3.728)
	1954	1.200	7.407***	0.437	3.348	–4.930	11.896**
		(1.910)	(2.872)	(2.640)	(3.523)	(4.499)	(4.962)
	1956	1.354	–1.944	2.333	–4.496	–4.677	0.392
		(1.760)	(2.436)	(2.417)	(2.799)	(4.595)	(3.613)
	1958	11.259***	5.189	12.683***	–5.981**	–6.009*	–4.739
		(2.000)	(3.490)	(2.813)	(2.914)	(3.419)	(4.179)
	1962	0.184	–5.034	1.942	4.092	–1.541	7.153
		(1.980)	(3.209)	(2.808)	(3.341)	(2.624)	(4.911)
	1964	8.721***	12.481***	9.156***	–7.105	10.373***	–16.237**
		(2.138)	(3.238)	(2.941)	(4.847)	(3.552)	(6.926)
	1966	14.703***	–4.044	19.971***	–15.216***	5.458	–22.718***
		(2.428)	(4.894)	(2.888)	(4.374)	(4.236)	(6.074)
	1968	20.088***	5.936	24.326***	–9.245**	3.443	–12.120**
		(2.359)	(3.919)	(3.082)	(3.973)	(4.716)	(5.354)
	1970	22.017***	6.438	25.789***	–8.505*	8.590	–9.737
		(2.711)	(6.973)	(2.871)	(4.711)	(5.385)	(6.595)
	1972	22.150***	4.615	23.553***	0.739	21.951***	–1.410
		(3.377)	(10.033)	(3.201)	(5.190)	(6.518)	(7.179)

Table notes: standard errors are adjusted for clustering at the state level (*** p<0.01, ** p<0.05, * p<0.1). Regression specification is described in equation 3 of the main text. 1960 is the comparison election and is dropped. Omitted controls include interactions of election year dummies interacted with the set of suppressed control variables tabulated in Table D.4 and with state dummies.

Sources: Estimates of voter turnout from Clubb et al. (2006). Other sources are described in the note to Table 2.

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