Fiscal Stimulus Through State and Local Governments

by

Laura Feiveson

B.S., Yale University (2002)

ARCHIVES
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
SEP 20 2012
LIBRARIES

Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Economics

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

September 2012

© 2012 Laura Feiveson. All rights reserved.

The author hereby grants to Massachusetts Institute of Technology permission to reproduce and
to distribute copies of this thesis document in whole or in part.

Signature of Author ....
Department of Economics
11 August 2012

Certified by ....
James Poterba
Mitsui Professor of Economics
Thesis Supervisor

Certified by ....
Michael Greenstone
3M Professor of Environmental Economics
Thesis Supervisor

Accepted by ....
Michael Greenstone
3M Professor of Environmental Economics
Chairman, Departmental Committee on Graduate Studies

Fiscal Stimulus Through State and Local Governments

by

Laura Feiveson

Submitted to the Department of Economics
on 11 August 2012, in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy in Economics

Abstract

State and local governments in the United States make up more than half of total government consumption and investment and almost 90 percent of total government employment. Despite these facts, the debates surrounding fiscal policy during business cycles have usually been limited to the actions of the federal government. This is in large part due to two reasons. First, there are 50 state governments and more than 60,000 local governments, making coordinated responses very difficult. Second, because state and local governments are bound by balanced budget rules, their hands are tied, to some degree, in their ability to enact countercyclical spending policies. However, their dramatic expenditure and employment cuts in the recent recession have made it increasingly clear how much their actions affect the economy as a whole and have motivated new research surrounding their budget mechanisms and the broader impacts of their fiscal policy. This dissertation consists of three chapters, each seeking to illuminate a specific issue within this area of research. In the first chapter, I examine how the impact of federal intergovernmental grants on local economies may be mediated by public sector unions. In the second chapter, I explore the impact of revenue structure on city government revenue and expenditure fluctuations. Finally, the third chapter (co-authored with Gabriel Chodorow-Reich, Zachary Liscow, and William Woolston) estimates the fiscal multiplier associated with federal transfers to state governments in the recent recession.

Thesis Supervisor: James Poterba
Title: Mitsui Professor of Economics

Thesis Supervisor: Michael Greenstone
Title: 3M Professor of Environmental Economics

For my parents, Harold and Caroline Feiveson,
and my husband, Nupur Mehta

Acknowledgements

I owe a tremendous debt to my advisors, Jim Poterba and Michael Greenstone. Jim read every piece of writing that I sent to him and never failed to respond quickly and insightfully. My many discussions with him have led me to be both a better thinker and a better writer. He gave criticism in such a way that I was encouraged to propel forward in my research even at times when I would have been discouraged otherwise. Michael's advising to me began at a time when I needed particular guidance, and for that I will always be grateful. As an advisor, he made me a much better empiricist and taught me how to frame concepts in a clear and concise way. I would not have finished my thesis without Jim and Michael's support.

I also would like to thank Olivier Blanchard, who had been my advisor before he left for the International Monetary Fund. He has been and will continue to be a great role model for me. Another faculty member that I feel grateful to is Esther Duflo, who helped me personally during one of my roughest points in graduate school. It is impossible to list all of the other faculty members and students who enriched my life as an economist over the last seven years, but to name a few: Francesco Giavazzi, Guido Lorenzoni, Ivan Werning, Peter Diamond, David Cesarini, Alp Simsek, Jennifer La'O, Jesse Edgerton, Pablo Querubin, and Sahar Parsa.

My year at the Council of Economic Advisors taught me a lot about what it is to be an economist and I am thankful to many of the people there for introducing me to the topics that ultimately motivated this dissertation. In particular, Christina Romer was a kind and inspiring mentor to me throughout that year and beyond. I also met my co-authors, Gabriel Chodorow-Reich, Zachary Liscow, and William Woolston, from whom I learned a lot about how to write a paper.

I would like to thank all my friends and family, who have provided incalculable support to me throughout the writing process. In particular, I thank my grandmother, Virginia Sykes

Dreby, whose optimism and interest in the world is a constant inspiration.

I cannot imagine what my life would have been like without my parents' unfailing support and presence. As I slogged through some of the dreariest parts of the writing process, my dad's poems and math problems kept a smile on my face. And when I needed distraction, my mom's emotional support and patience kept me buoyant. I could write a whole dissertation on their contribution to my life throughout graduate school.

I met my husband, Nupur, just before I entered the hardest time of graduate school. It is because of his presence in my life that this difficult time in school coincided with the one of the happiest times of my life. He has been a tremendous support to me. His presence in my life has been a constant reminder that there is much more to life than a completed dissertation. He also contributed to the recent arrival of Jaya Vivian Mehta, our daughter, who has fully delighted us both in the final months of dissertation-writing. I am extremely lucky to have Nupur and Jaya in my life.

Cambridge, MA
August 11, 2012

Contents

[1]Co-authored with Gabriel Chodorow-Reich, Zachary Liscow, and William Woolston.

Introduction

State and local governments in the United States make up more than half of total government consumption and investment and almost 90 percent of total government employment. Despite these facts, the debates surrounding fiscal policy during business cycles have usually been limited to the actions of the federal government. This is in large part due to two reasons. First, there are 50 state governments and more than 60,000 local governments, making coordinated responses very difficult. Second, because state and local governments are bound by balanced budget rules, their hands are tied, to some degree, in their ability to enact countercyclical spending policies. However, their dramatic expenditure and employment cuts in the recent recession have made it increasingly clear how much their actions affect the economy as a whole and have motivated a new research surrounding their budget mechanisms and the broader impacts of their fiscal policy. This dissertation consists of three chapters, each seeking to illuminate a specific issue within this area of research.

In the first chapter, I examine how the impact of federal intergovernmental grants on local economies may be mediated by local government institutions. The United States federal government implemented a large general revenue sharing program from 1972 to 1986, in which it transferred nearly 300 billion (2009) dollars to over 35,000 state and local governments. I examine whether large city governments spent the funds that they received and how the strength of public sector bargaining affected whether the funds were spent on new employment or increased wages. I find that, on average, city governments spent the transfers completely, in contrast to the findings of some of the recent "flypaper" literature; and that cities in states with pro-union collective bargaining laws spent more than half of the transfers on increased wages while cities in states without such laws spent a greater fraction of the funds on new employment. These findings suggest that local institutions, in this case public sector unions, play an important role in determining the way intergovernmental grants translate into spending outcomes. They highlight the potential heterogeneity in the way such grants may be spent in different jurisdictions. Moreover, if raising the wages of existing workers has a different macroeconomic stimulative effect than hiring new workers, they may also suggest differences across places in the "multiplier" associated with federal transfers to state and local governments. I find suggestive, though weak, evidence that the output multiplier on spending on new employment is larger than the multiplier on increased government wages.

In the second chapter, I explore the sources of municipal government revenue and expenditure fluctuations. Due to a large variation across local governments in their revenue structure, the elasticity of total revenues with respect to the business cycle varies substantially. I document the extent to which revenue structure impacts the revenue responses across city governments in downturns. I then exploit the differences in revenue structure to explore the effects that "revenue shocks" have on expenditures. I find that city governments cut expenditures about one for one in response to a revenue shock, suggesting that the balanced budget rules are binding. These results suggest that the structure of revenue can have important effects on local revenue and spending dynamics, which, in turn, raises questions on the variation in infrastructure spending over the business cycle, on the nature of programs that are cut when county income declines, and on the long-term impacts of cyclical government investment disruptions. Furthermore, to examine the short-term impacts of the local government expenditure fluctuations, I propose a method in which the revenue structure can be used to estimate the size of the multipliers associated with balanced budget shocks.

Finally, the third chapter (co-authored with Gabriel Chodorow-Reich, Zachary Liscow, and William Woolston) explores the fiscal multiplier associated with federal transfers to state governments in the recent recession. The American Recovery and Reinvestment Act (ARRA) of 2009 included \$88 billion of aid to state governments administered through the Medicaid reimbursement process. We examine the effect of these transfers on states' employment. Because state fiscal relief outlays are endogenous to a state's economic environment, OLS results are biased downward. We address this problem by using a state's pre-recession Medicaid spending level to instrument for ARRA state fiscal relief. In our preferred specification, a state's receipt of a marginal \$100,000 in Medicaid outlays results in an additional 3.8 job-years, 3.2 of which are outside the government, health, and education sectors.

Chapter 1

General Revenue Sharing and Public Sector Unions

1.1 Introduction

One of the largest components of the 2009 American Recovery and Reinvestment Act (ARRA) consisted of more than 200 billion dollars in transfers to state and local governments. The passage of the ARRA along with countercyclical measures worldwide have led to renewed interest in the estimation of the government spending multipliers associated with the various components of stimulus packages.[1] Work by Chodorow-Reich, et al (2012) used inter-state heterogeneity to find significant and positive employment effects associated with the ARRA transfers to state governments, at the same time that Cogan and Taylor (2010) used aggregate time series to argue that the transfers to state and local governments had little to no macroeconomic effect. Even as President Obama's new budget plan calls for countercyclical aid to state governments, there remains considerable debate surrounding the macroeconomic effects of intergovernmental transfers. Ultimately, any hope of understanding the stimulative effects of transfers to state and local governments relies on understanding the mechanisms through which the funds affect the state and local government budgeting decisions.

A relatively unexamined possibility is that heterogeneity at the local government level leads

[1] For recent examples of empirical work on fiscal multipliers see Nakamura and Steinssen (2011), Shoag (2010), and Ramey (2011).

to substantial variation in the response of state and local governments to federal transfers.[2] Despite the focus on labor markets in recent years, their interaction with local government budgeting has generally been neglected in the public finance literature. At the same time that policy debates surrounding federal intergovernmental transfers have rippled through Washington, protesters and politicians in Wisconsin, Indiana, and Ohio have brought the role of public sector unions to the forefront of public consciousness. The theoretical underpinnings of research on public sector unions suggest that there may be a good reason to link the two debates.

In this paper, I seek to address this connection by examining how the strength of public sector labor unions affects the response of local governments to intergovernmental transfers. To approach this analysis, I revisit a general revenue sharing program put in place from 1972 to 1986 in which the federal government transferred a total of nearly 300 billion (2009) dollars to state and local governments. Although the stated goal of the law was to move the decisions about government spending "closer to the people", it simultaneously helped to alleviate liquidity crises at the state and local government level during a time period in which many local governments were facing budget deficits. Furthermore, the passage of the general revenue sharing legislation came at the end of a fifteen year period in which a rapid series of new state laws enabled or required local governments to collectively bargain with their employees. I use the diversity of the collective bargaining laws across states to examine how the laws affected city governments' use of the intergovernmental transfers and, in particular, I focus on whether they had influence over whether the transfers were spent on higher wages for existing employees or on new employment. I present an initial empirical estimate of the differential impacts on the private economy generated by these different types of government spending, providing motivation for future research.

The general revenue sharing program is a suitable program with which to study the effect of intergovernmental transfers on local government expenditure decisions for three main reasons. First, it significantly impacted the revenues of local governments. At its peak, general revenue sharing made up to 20 percent of total revenues of the large city governments studied in this paper.

[2]For instance, one well-studied example of a characteristic that has been shown to have significant effects at the state government level is the stringency of the balanced budget rules (Poterba (1994), Clemens and Miran (2011)).

Second, there was substantial and plausibly exogenous variation in the amounts that city governments received. Although the general revenue sharing formula depended on three factors that would be expected to have a separate effect on government expenditure decisions (per capita income, tax-to-income ratio, and population), a "geographic tiering" element to the formula led to variation in the general revenue sharing receipts of city governments that were housed in different counties and states, but were otherwise very similar. Furthermore, the three factors entered the allocation formula in highly nonlinear ways, making it possible to control for them directly. Since the magnitude of transfers to a local government is often correlated to its economic conditions, it can be difficult to disentangle the effects of the transfers from the effects of the economy. Because of the eccentricities in the formula, the general revenue sharing program provides variation that is plausibly immune to this concern.

Finally, the general revenue sharing program led to transfers to over 35,000 state and local governments including all state, county, city, town and township general-purpose governments. It was one of the most comprehensive general purpose transfer programs in the history of the United States and provides a test case for possible future general revenue sharing designs.

Although private sector unions were granted full legislative protection in the first half of the twentieth century, public sector unions did not achieve significant legislative gains until the late-1950s and 1960s. However, starting with Massachusetts in 1958 and Wisconsin in 1959, a series of state laws were passed in rapid succession. By the time that the State and Local Fiscal Assistance Act instituted the first wave of general revenue sharing in 1972, 30 states had passed laws enabling or requiring collecting bargaining by local governments within their state. By the mid-1970s, more than 40% of public sector workers across the country were represented by unions, with substantial variation across the states.[3] Since public sector collective bargaining was pervasive by the time that general revenue sharing was put in place, it is possible to study how the strength of collective bargaining affected the local governments' use of the transfers.

The empirical analysis of this paper is organized into three main parts. First, the general revenue sharing program provides an opportunity to revisit the much-studied research question of whether governments used the transfers to increase expenditures or to reduce taxes. An

[3]These statistics are from the Survey on Labor-Management Relations in State and Local Governments (U.S. Bureau of the Census, Multiple Years) and www.unionstats.com (Hirsch and Macpherson, 2003).

influential paper by Bradford and Oates (1971) theorized that lump-sum intergovernmental transfers should have the same effect on government expenditures as an equivalent increase in personal income of the voting citizens. An extensive empirical literature has emerged since this paper, which has found that intergovernmental transfers, at times, lead to a much higher increase in government expenditures that would plausibly result from an equivalent rise in personal income. This phenomenon has been dubbed the "flypaper effect" because the transfer funds appear to stick where they hit.[4] The flypaper effect remains an important policy issue; as the federal government considers transferring funds to local governments, the question of whether they will spend the funds is crucial to the policy evaluation. I find that the large cities I study in this paper increased expenditures by roughly one dollar for every dollar received in intergovernmental transfers.[5]

Second, I examine how the strength of public sector collective bargaining laws affects the expenditure decisions of the recipient governments. Theories of public sector unions conjecture that union leaders seek to maximize an objective function in which wages and employment are positive inputs. These theories suggest that bargaining in cities in states with pro-union bargaining laws may lead to different uses of transfers than in cities in states with no such laws. I find that the cities with strong collective bargaining laws convert more of the transfers into increased wages than those with no bargaining laws, and, furthermore, that those with no bargaining laws instead spent a significant amount of funds on new or retained employment.

Lastly, I study one way in which the two types of spending by bargaining and no-bargaining cities may have different effects on the private economy. Starting with Wynne (1992), a distinction in the theoretical and empirical literature has been made between the stimulative effects

[4]As summarized by Gramlich (1977) and Hines and Thaler (1995), the empirical literature in the latter half of the century had shown the existence of a strong flypaper effect. Explanations of the flypaper effect range from discussions of a mis-specified model of citizen behavior (see Filimon, Romer and Rosenthal (1982) or Hines and Thaler (1995)) to a repeated game element in the grant process (Chernick (1979)). Inman (2008) provides a comprehensive discussion of other possible explanations. More recent empirical studies have shown more ambiguous results; the flypaper effect seems to at least crucially depend on factors such as the type of democracy (Lutz (2006)) and the strength of collective interest groups (Singhal (2008)). Furthermore, Knight (2002) argued that the possible endogeneity of grant assignment due to differential preferences for government spending may have led to econometric issues in previous studies, and finds a negligible flypaper effect in transportation grants to state governments when appropriately accounting for legislative bargaining power.

[5]Because the general revenue sharing program did include some minor price effects, they were not pure lump sum transfers and thus do not directly address the traditional flypaper effect. Details of the general revenue sharing program and how they relate to the flypaper effect will be discussed in Sections 2 and 6.2.

on the private economy of government consumption of private goods and government compensation of employees.[6] A similar distinction can be made between government spending on increased government wages and increased government employment, due to differential marginal propensities to consume between the two types of recipient employees. Because the strength of collective bargaining laws determines the type of government spending produced, I use this institutional friction to explore the hypothesis that the multipliers on spending on increased wages and on spending on new employment would be different. I find suggestive, though weak, evidence that the multiplier on increased government employment is larger than the multiplier on increased government wages for existing employees. These results are presented as motivation for future research.

The paper is organized as follows: Section 2 discusses the program details of the general revenue sharing transfers, Section 3 reviews the background and literature of public sector unions, Section 4 introduces the empirical strategy, Section 5 explains the data that are used, and Section 6 discusses the main results. Section 7 introduces the macroeconomic hypothesis and empirical analysis. Finally, Section 8 concludes.

1.2 General Revenue Sharing

The policy debates surrounding the growing roles of local governments in the late 1960s and early 1970s ultimately led to the passage of the State and Local Fiscal Assistance Act in October of 1972. This act put in place the largest general revenue sharing scheme in the history of the United States. With this policy, the federal government initially committed to transferring over 30 billion dollars to more than 35,000 general purpose governments–state, county, city, town, and township governments–over a period of 4 years. In 1976, the act was extended for another period of 4 years for state and local governments, and then extended for only local governments from 1980 to 1983 and again from 1983 to 1986 when it finally expired. By the end of the act, over 83 billion dollars (almost 300 billion in 2009 dollars) had been transferred to state and local governments. The motivations for the act were both philosophical and practical; the official goal was to have decisions about government spending "closer to the people", while the act

[6]See Finn (1998), Pappa (2009), and Ramey (2011).

simultaneously served the purpose of providing support to local governments at a time in which many budgets were strained. Although some evidence implies that the Nixon administration had the intention of using the general revenue sharing funds to replace various federal categorical grant programs to local governments, in practice, it acted as a supplement to the programs that already existed.[7] The most binding requirement surrounding the use of the funds was that they were not to be used for operational education expenses; no general revenue sharing funds were transferred to school districts. Otherwise, the governments had almost complete freedom to use the funds as they desired.[8] The governments did have to fill out a "statement of use" in which they described how they used the funds.[9] Furthermore, after the first extension of the funds, the local governments were required to hold public hearings in which the potential uses of the funds were discussed.

Table 1 shows the size of the program throughout the 14 years of its existence. At the peak of the program's impact, in 1974, general revenue sharing (GRS) made up about 15% of total federal intergovernmental transfers to state and local governments, and composed almost 3% of state government budgets and over 3.5% of local government budgets. As Table 1 shows clearly, the size of the program in real dollars decreased substantially over its tenure due to relatively high inflation in the 1970s and the 1980s combined with stagnant nominal amounts. By 1984, the program only amounted to 0.12 percent of GDP and less than 2 percent of local government budgets. Despite the ramp-down, the general revenue sharing program had a substantial effect on the revenues of the 837 cities in my sample. Figure 1 plots

[7] In fact, the Nixon administration promised that the general revenue sharing program would be an "add-on" to existing programs in order to get the support for the passage of the act (Dommel, 1974). However, after the act was passed and Nixon was re-elected in 1972, the administration began to push for the elimination of many block grant programs, claiming that the general revenue sharing funds would make up for the reduced transfers. The Watergate scandal ultimately interfered with the implementation of this policy push, and the grant programs remained largely unscathed, reinforcing the "add-on" nature of the general revenue sharing program (Markusen et al, 1981).

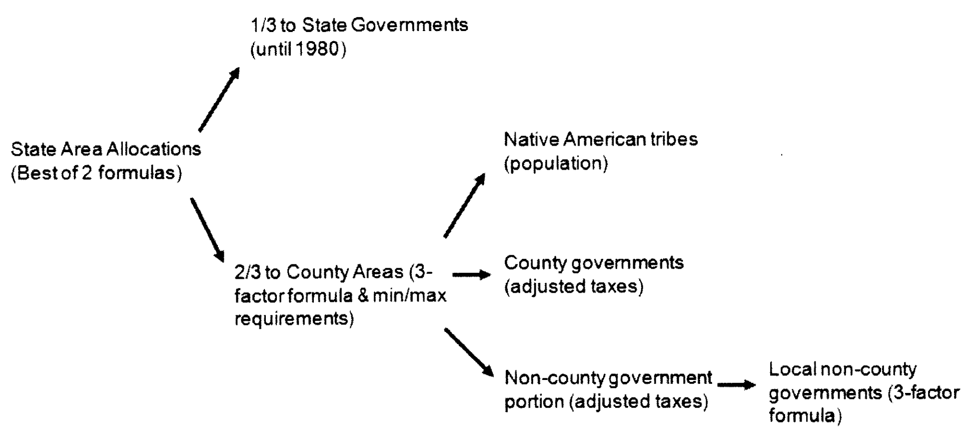
[8] Specifically, the "priority" categories on which the funds could be spent were: all "ordinary and necessary" capital expenditures, and "ordinary and necessary" maintenance and operating expenses for public safety, environmental protection (including sewerage and sanitation), public transportation, health, recreation, libraries, social services for the poor or aged, and financial administration (Joint Committee on Internal Revenue Taxation, 1973). In practice, the only major binding requirement was that the funds were not to be used for education operating expenses.

[9] The specific requirements were the the funds had to be appropriated within 24 months of the entitlement period. Local governments had to fill out planned use and actual use reports and make them available to the public. The planned use reports were to be filled out within each entitlement period while the actual use reports were to be filled out within 60 days of June 30th of each year.

both the total federal intergovernmental funds as well as the total general revenue sharing funds received by the city governments. The figure demonstrates that the movements in total federal intergovernmental transfers appeared to reflect the jump in general revenue sharing funds in the early-1970s as well as the ramp-down in the early- to mid- 1980s.

1.2.1 GRS Allocations

As with any large federal program, there was a significant amount of negotiation involved with the allocation of the general revenue sharing funds. The compromise finally reached between the members of the Congress led to rich variation in the amount that local governments received. One of the key features of the allocation formula is that the funds were allocated with a method of "geographic tiering"; first the funds were allocated to state areas using a federally-mandated formula; then, after removing a portion for state governments, the funds were apportioned to county areas using a federally-mandated formula; then, again after removing funds for county governments and Native American tribes they were divided amongst city, town, and township general purpose governments using the same federally-mandated formula. The diagram below demonstrates this allocation process.



This "geographic tiering" led to a wide dispersion of general revenue sharing funds across cities that were similar, but that were housed in counties and states with different characteristics. The formulas are described in detail in the next three subsections.

Allocations to States The funds were first allocated to the state areas using one of two formulas. The first, called the three-factor formula–or the Senate formula–allocated money to states in proportion to a factor, F_i:

$$F_i = Pop_i * (\frac{Tax_i}{PersonalIncome_i}) * (\frac{PerCapitaIncome_{US}}{PerCapitaIncome_i}) \tag{1.1}$$

The first term of this factor was geared towards equalizing the per capita funds transferred to states. The second component was to address the concern that states may lower taxes in response to the increased federal funds; to try to reduce the incentives to do this, high taxation rates were rewarded. Finally, the third term transferred more funds to states that had lower per capita income. Under this allocation formula, each state i was awarded, S_i^1:

$$S_i^1 = G * (\frac{F_i}{\sum_{States} F_k}) \tag{1.2}$$

where G was the total amount of general revenue sharing funds available for distribution.

In the second, five-factor (House) formula, the allocation was divided into five parts which were each distributed using a different formula as shown in the table below:

Fraction of Funds	Factor used for Allocation
0.25	Pop_i
0.25	$UrbanPop_i$
0.25	$Pop_i * (\frac{PerCapitaIncome_{US}}{PerCapitaIncome_i})$
0.125	$IncomeTax_i$
0.125	$Tax_i * (\frac{Tax_i}{PersonalIncome_i})$

Under this formula, the total GRS allocation was divided into parts and then distributed according to the factors discussed in the table above. Each state was awarded S_i^2:

$$S_i^2 = G * \begin{bmatrix} 0.25 * \frac{Pop_i}{\sum\limits_{States} Pop_k} + \\ 0.25 * \frac{UrbanPop_i}{\sum UrbanPop_k} + \\ 0.25 * \frac{\frac{Pop_i}{PerCapitaIncome_i}}{\sum \frac{Pop_k}{PerCapitaIncome_k}} + \\ 0.125 * \frac{IncomeTax_i}{\sum IncomeTax_k} + \\ 0.125 * \frac{\frac{Tax_i^2}{PerCapitaIncome_i}}{\sum \frac{Tax_k^2}{PerCapitaIncome_k}} \end{bmatrix} \tag{1.3}$$

The final allocation was reached by first calculating the distribution of funds, S_i^1 and S_i^2, for all of the states under each of the three-factor and the five-factor formulas, and then taking the larger of the two amounts for each state. These final amounts were then proportionately adjusted so that the total amount summed to the total GRS funds available. The final allocation for each state area was then:

$$S_i^F = G * \frac{\max(S_i^1, S_i^2)}{\sum \max(S_k^1, S_k^2)} \tag{1.4}$$

Ultimately, 31 states received their revenue-sharing allotment based on the three-factor formula and 19 states and DC received their revenue-sharing allotment based on the five-factor formula. Figure 2 shows the distribution of the 1977 state area allocations.

Allocations to local governments Of the amount allocated to states, one-third was designated for the state government and two-thirds was designated for the local governments within the states. The path for the funds to reach local governments went first through county area allocations. The funds were allocated to counties using the three-factor formula from above so that each county, c, in a state, s, received funds proportional to:

$$F_{cs} = Pop_c * (\frac{NonSchoolTax_c}{PersonalIncome_c}) * (\frac{PerCapitaIncome_s}{PerCapitaIncome_c}) \tag{1.5}$$

where $NonSchoolTax_c$ are the total taxes raised in local governments in the county less the taxes dedicated for educational expenses.[10] After a population-based amount was removed for

[10] The details of the legislation actually gave states some freedom to change the formula with which the state area allocations were divided amongst local governments. However, none of the state governments chose to take

Native American tribes within the county, the county area allocation was then divided into three parts designating funds for the county government, the city and town governments, and the township governments based on their relative non-school tax collection. Finally, the city and town governments and the township governments within a county each split their group's total allocation amongst the individual governments according to proportions once again determined by the three-factor formula. The final allocation formula to a city, i, in county, c, and state, s, was:

$$GRS_{ics}= S_s^F * \frac{2}{3} * \left[\frac{Pop_c^2 * (\frac{Tax_c}{Inc_c^2})}{\sum\limits_{counties \in s} Pop_j^2 * (\frac{Tax_j}{Inc_j^2})} \right] * \frac{\sum\limits_{cities \in c} Tax_k}{\sum\limits_{govts \in c} Tax_j} * \left[\frac{Pop_i^2 * (\frac{Tax_i}{Inc_i^2})}{\sum\limits_{cities \in c} Pop_j^2 * (\frac{Tax_j}{Inc_j^2})} \right] \qquad (1.6)$$

where S_s^F is the state-area allocation.[11]

Minimum and Maximum Requirements Minimum and maximum requirements further distorted the funds received by local governments. There were three main limits. The first requirement was that no local government was allowed to receive a grant that constituted more than 50 percent of its total nonschool taxes and intergovernmental transfers. When the amount allocated to a city or town exceeded this limit, the excess amount was reallocated to the corresponding county government. When the amount allocated to the county government exceeded this amount, the funds went back to the state government. Because of this restriction, more than 10 state governments received more than one-third of their state's allocation, with West Virginia, Kentucky, and Delaware receiving substantially more than the one-third initially allocated (45%, 41%, and 40% respectively).

The second limit was that no county area or local government was permitted to receive more than 145 percent of the state per capita amount, and the third limit was that no county area or local government was permitted to receive less than 20% of the state per capita amount. At the county area level, those funds that were in excess of the 145 percent limit were distributed

up this option and the federal formula was used in all cases.

[11]The allocation amount would be slightly less than implied by the formula in Equation (6) for cities in counties that included a Native American population.

to the non-binding county areas proportionate to the three-factor formula. Similarly, funds were reduced proportionately in non-binding county areas to meet the 20 percent limit for those areas that needed extra funds. Similar adjustments occurred for those local governments that were constrained at the 20 percent or the 145 percent limits.

To be clear, I will map out the steps in which city governments were received funds from their county-wide allocations. Suppose that a county area received GRS_{0c} through the allocation process. A city government, i, within the county would initially receive[12]:

$$
\begin{aligned}
GRS_{0i} &= \left(\frac{Pop_i * \left(\frac{NonSchoolTax_i}{PersonalIncome1969_i}\right) * \left(\frac{PerCapitaIncome_i}{PerCapitaIncome_c}\right)}{\sum\limits_{cities} Pop_j * \left(\frac{NonSchoolTax_j}{PersonalIncome1969_j}\right) * \left(\frac{PerCapitaIncome_j}{PerCapitaIncome_c}\right)}\right) \\
&\quad * GRS_c * \left(\frac{\sum\limits_{Cities} NonSchoolTax_k}{\sum\limits_{GovernmentsInCounty} NonSchoolTax_j}\right)
\end{aligned} \tag{1.7}
$$

The first additional requirement was that the city government did not receive more than fifty percent of its total nonschool taxes and intergovernmental transfers. Thus, in the second step of the allocation, the city government received GRS_{1i}:

$$GRS_{1i} = \min\left(GRS_{0i}, 0.5 * (NonSchoolTax_i + IGR_i)\right) \tag{1.8}$$

Any excess amounts generated in this step, $GRS_{0i} - GRS_{1i}$, were assigned to the county governments within the county. The second and third limits were then applied such that:

$$GRS_{2_i} = \min\left(\max\left(GRS_{1i}, 0.2 * \left(\frac{GRS_s}{Pop_s}\right)\right), 1.45 * \left(\frac{GRS_s}{Pop_s}\right)\right) \tag{1.9}$$

Any excess amounts generated in this step, $GRS_{1i} - GRS_{2i}$, were added to the initial county-wide allocation, GRS_c, and any shortage of funds generated, $GRS_{2i} - GRS_{1i}$ were subtracted from GRS_c. At this point, the steps represented in Equations (7) - (9) would be repeated again until all binding requirements were met.[13] Note that, due to the iteration of these steps, the

[12] If there were Native American Tribes in the county, a portion of GRS_c would be removed before the allocation in Equation (6).

[13] If, at any step, the amount allocated to the county government exceeded 50 percent of the sum of its nonschool taxes and intergovernmental transfers, the excess would be allocated to the state government.

maximum and minimum requirements ended up affecting the allocations of governments that were not at the limits of the requirements. In the first year of the GRS program, 6.6 percent of the GRS funds were redistributed through limits and only 74 out of the 38,000 recipients had allocations which were unaffected by the limit requirements.

1.2.2 Variation in GRS receipts

Any examination of the GRS aggregates will mask the significant variation in the per capita funds received by local governments due to the geographic tiering and other nuances of the allocation process. The variation is especially large at the city level, which is the unit of observation in this paper. To give a sense of the variation, in Figure 3, I plot a histogram of the de-meaned per capita general revenue sharing transfers in 1977. Although the variation in Figures 3 is useful to observe, the variation that I will use in future regressions is the residual left once I control for smooth functions of all of the variables that appear in the allocation formulas in Equations (1) - (9). Figure 4 shows the residuals from a regression of per capita general revenue sharing receipts on cubic polynomials of all of the allocation variables in 1977.[14] Due to the geographic tiering and the non-linearity of the allocation formulas, this figure shows that substantial cross-sectional variation in the general revenue sharing funds remain even after controlling for flexible functions of the allocation variables.

1.3 The Study of Public Sector Unions

In this section, I review the theoretical and empirical underpinnings of the public sector union literature in order to set the framework for the empirical analysis.

Background

The policy push for public sector collective bargaining significantly lagged that of the private sector. The landmark laws supporting the right to unionize, strike, and collectively bargain in the private sector, i.e. the National Labor Relations act in 1935 and the Taft-Hartley Act in 1947, specifically left out public sector workers. It was not until the late 1950s that public

[14]The detailed list of controls will be discussed in Section 5.7.

sector labor gained some legislative traction. In 1959, Wisconsin was the first state to pass a "strong" collective bargaining law requiring that public sector employers collectively bargain with their employees. Around the same time, Massachusetts, Arkansas, and Idaho also passed laws acknowledging the public sector labor in their states. These laws paved the way for the passage of a series of state laws enabling or requiring collective bargaining on the part of state and local governments. The laws covered topics such as the allowed focus of the bargaining (i.e. wages, benefits, firing practices), who the laws would cover (police, fire, teachers, and other), whether strikes were allowed, and whether union dues could be automatically subtracted from employee payrolls. By 1977, 35 states had passed a public sector collective bargaining law, 6 of which explicitly required bargaining and 18 of which implicitly required bargaining.

The laws passed in the 1960s and the 1970s led to a strengthening of public sector unions at the same time that private sector unions declined in importance. Figure 5 plots the number of workers and the percent of workers covered by private and public sector unions from 1977 to 2010. The percent of public sector workers covered by unions has remained roughly constant at 40 percent for the period shown. On the other hand, the percent of private sector workers covered by unions declined precipitously from over 23 percent in 1977 to less than 8 percent in 2010. The number of public sector workers covered by unions exceeded the number of private sector workers for the first time in 2009.

The strong bargaining legislation required that public employers bargain in "good faith". If there was a concern that the employers did not abide by the "good faith" doctrine, organized labor could take them to court after which a judge usually ruled with binding arbitration. In practice, this gave labor a significant amount of power; the laws certainly appeared to have bite. In Section 4.1, I will discuss the evidence that the laws had a causal effect on unionization.

Private versus Public Unions

Private sector unions have gained more research attention than public sector unions. The bargaining table consists of the employer and the labor representation. The scope of the bargaining may include wages (and benefits), and employment. The objective functions of the employer are clear; he wants to choose employment and wages to maximize profit. Outside competition and consumer demand determines the choice of price (and employment, if not in the scope of bargaining) given profit maximization. Although there is debate surrounding the objectives of the labor representation, a simplified assumption is that the union has a well-defined utility function over wages and employment, increasing in both (Dunlop (1944), Farber (1986)).[15] Market discipline plays a large role in the negotiations; because negative profits lead to financial distress and ultimate firm shut-down, the existence of product competition leads to a necessary link between productivity and wage growth.

Although in some ways public sector bargaining mirrors that of the private sector, there are distinct differences. First, the politicians and bureaucrats who participate in the collective bargaining are no longer concerned about profits. Instead, they worry about the prospects of re-election and the perceived welfare of the citizens they represent, both of which can be influenced by public sector unions. By voting in high numbers and staffing campaigns, public sector unions can significantly impact local elections (Bennet and Orzechowski, 1983). Furthermore, public sector unions can actually shift out the demand for public goods by forming special interest groups to advertise their services (Freeman (1986), Marlow and Orzechowski, 1996)). Second, the objective functions of public sector unions may include more than just wages and employment; in particular, because public sector employees have influence over the provision of public goods, they may also include the welfare of citizens in their utility function. For example, one reason that public sector unions may lobby for more public funds is that they have an in-depth understanding of how to supply services in the most effective way (Zax and Ichniowski, 1988).

Finally, the public sector is guided by political discipline rather than the market discipline of the private sector (Gregory and Borland, 1999). At times in which there are large gaps between wages and productivity, public employers are able to resort to a "tax push" to fund the difference; a luxury that private employers do not have. A negative revenue shock in the private sector will either lead to a firm shut-down or a concession by labor to reduce wages or employment. In contrast, a negative revenue shock to a government could be financed by

[15] This is a simplification of a very large literature. Starting with Ross (1948), there has been debate over the value of assigning a well-defined utility function to the labor representation. In particular, political motivations of union leadership and heterogeneous membership make the objectives difficult to summarize. Ashenfelter and Johnson (1969) propose an alternative bargaining model in which there are three parties: the firm management, the union leadership, and the union "rank and file". In this spirit, many recent papers stress the political motivation of the union leadership in their models of union behavior.

increased taxes or debt rather than concessions by labor. Reder (1975) theorized that the tumultuous economic times of the 1970s led to a decrease in the employment and power of private sector unions, initiating their decline. On the other hand, public sector unions, which are less vulnerable to negative revenue shocks, retained and even gained power over the same time period.

Employment versus Wages

As mentioned above, the objective function of unions is often considered to be a function of wages and employment, and increasing in both. The intuition of why wages are in the utility function is clear; we generally think that an increase in income, given the same amount of work, yields in increase in utility. However, the idea that the union leadership (at the bargaining table) would also value higher levels of employment is less obvious. The theories that advocate for having employment in the utility function mostly rely on the idea that higher levels of employment increase the negotiating power of the public sector union members. In addition, some economists have argued that public sector unions put greater weight on employment than private sector unions because numbers boost the political power of the union itself (Courant, Gramlich, and Rubinfeld (1979), Freeman (1986)). On the other hand, advocates of the "insider-outsider" theory conjecture that the current members care only about their own wages and job security without any direct desire to increase employment (Blanchard and Summers, 1986). The key feature of the utility functions in the insider-outsider literature is their dependence on the wages of the current members rather than the employment of possible future members.[16]

Empirical Findings

Most studies of the effect of public sector unions on wages find a positive effect on the wages of unionized workers. In a comprehensive summary of the literature through 1986, Lewis (1990) reported that the union-nonunion public sector wage gap was in the range of 8 to

[16] By limiting the discussion of the objective function to only employment or wages, I am skimming over other variables that have been included in the utility functions of unions. For instance, in the utility function posited by Blanchard, Summers (1986), the union members valued wages and the probability of retaining their job in the next period.

12 percent. He also reported that the wage gap tended to be negatively correlated with the level of government; local government employees experienced larger gaps than federal employees. Zax and Ichniowski (1988) find a significant union-nonunion wage gap as well as finding that high levels of unionization lead to increased government expenditures. More recently, using the passage of the state-level bargaining laws as exogenous events, Hoxby (1996) finds a positive effect of teacher unionization on teacher salaries and Frandsen (2011) finds a positive effect of collective bargaining laws on the wages of police and firefighters, although a negligible effect on teachers.

The results on the effect of unionization of public sector workers on employment have been more mixed. While Zax and Ichniowski (1988) found a significant effect of unionization on employment supporting the view that public sector unions push out the labor demand curve for public services, others have found that omitted variables may have biased naive regressions of employment on unionization. Trejo (1991) argued that economies of scale led to more union formation in larger municipalities, leading to a natural correlation between unionization and employment that could be deceptively interpreted causally. Valletta (1993) argued that unions may be less likely to form in municipalities with high levels of volunteerism (for example, in the fire departments) or privatization; since these municipalities tend to have lower public employment, this phenomenon would also lead to a deceptive correlation between the union level and employment. Ultimately, the question of whether public sector unions are able to push out the public's demand for public services so as to increase both wages and employment simultaneously remains unresolved.

Public Sector Unions and Intergovernmental Transfers

This paper examines the role of public sector unions in determining how intergovernmental transfers are spent. To my knowledge, there have been no previous papers that deal with this particular question. In a related topic, Allen (1988) addresses the question of how public sector unions affect employment in the presence of negative revenue shocks. He finds that, contrasting with the dynamics in the private sector, union workers in the public sector face lower rates of unemployment than nonunion workers. He theorizes that this occurs because of the union's ability to use their power to prevent budget cuts, while in the private sector, where cuts cannot be avoided, union contracts tend to protect the wages of the most senior workers at the expense of the new hires. Using this logic, when budget cuts cannot be avoided in the public sector in extreme times, it would be expected that the public sector layoffs would look similar to those of the private sector as long as the layoff policies in the contracts were similar.

My research, however, has to do with the union's effect on employment in the presence of positive transfers. If unions were aware of the general revenue sharing transfers during their collective bargaining processes–which is likely, as this was a highly publicized policy–one may expect that the use of the marginal dollars would be included in their bargaining. The theory above gives reason to think that the transfers may be "captured" by the unions in increased wages or increased employment or both.

1.4 Empirical Strategy

I estimate how cities respond to intergovernmental transfers, and how the strength of public sector collective bargaining affects that response. The main estimation equation is:

$$E_{it} = \beta_0 + \beta_1 IGR_{it} + \beta_2 IGR_{it} * U_{it} + \beta_3 U_{it} + \mathbf{X}_{it}\boldsymbol{\eta} + \lambda_i + \omega_t + \varepsilon_{it} \tag{1.10}$$

where E_{it} is a per capita government finance component in government i and year t, IGR_{it} are the per capita intergovernmental transfers, U_{it} is an indicator for the strength of collective bargaining laws, λ_i are city fixed effects, and ω_t are time fixed effects. X_{it} is a set of control variables including cubic polynomials of population, lagged per capita tax revenue, lagged tax effort, lagged own-source revenue, lagged per capita county income, lagged state-level total taxes, lagged state per capita income, and lagged state government individual income taxes. Although I show the results for a number of finance components, the outcome variables that I particularly focus on are total real expenditures, public employee real wages, and the number of public employees. The effect of the transfers on total expenditures will demonstrate whether governments spend the funds transferred to them (i.e. the expenditure effect), while the latter two outcome variables will speak to the quality of the public spending. Later in the paper, I discuss how the quality of public spending might impact the stimulative effectiveness of transfers to city governments, particularly during recessions.

An issue with the estimation of Equation (10) is that the federal and state governments target some of their funds to cities or areas that are in particular need. This could bias β_1 and β_2 either downward or upward if there is a systematic difference in how city governments "in need" and other cities respond to the transfers. For example, if funds are transferred to cities with high unemployment, reversion to the mean may mistakenly attribute an improvement in economic conditions (and thus an increase in government expenditures) to the transfers received. On the other hand, if funds are transferred to cities that are beginning to experience budgetary problems, the continuation of the negative trend (leading to a contraction in government expenditure) may be wrongly attributed to the transfers, biasing the β coefficient downwards.

1.4.1 A 2SLS Approach

To address the problem of potential bias, I use the general revenue sharing transfers as an instrument for total intergovernmental transfers. In particular, I have two instruments, GRS_{it}, per capita general revenue sharing receipts, and $GRS_{it} * U_{it}$, per capita general revenue sharing receipts interacted with the indicator for bargaining strength, for the two endogenous variables, IGR_{it} and $IGR_{it} * U_{it}$. Because I control for the cubic polynomials of all of the GRS-correlated variables, my instruments are essentially the GRS_{it} and the $GRS_{it} * U_{it}$ after conditioning for these variables. I estimate using 2SLS estimation, in which the second stage is represented by Equation (10), and the two first-stage regressions are:

$$IGR_{it} = \varphi_0^A + \varphi_1^A GRS_{it} + \varphi_2^A GRS_{it} * U_{it} + \varphi_3^A U_{it} + \mathbf{X}_{it}\boldsymbol{\alpha}^A + \sigma_i^A + \mu_t^A + \nu_{it}^A \qquad (1.11)$$

$$IGR_{it} * U_{it} = \varphi_0^B + \varphi_1^B GRS_{it} + \varphi_2^B GRS_{it} * U_{it} + \varphi_3^B U_{it} + \mathbf{X}_{it}\boldsymbol{\alpha}^B + \sigma_i^B + \mu_t^B + \nu_{it}^B \qquad (1.12)$$

The exclusion restriction of the IV estimation is that GRS_{it} and $GRS_{it} * U_{it}$ are independent of the error term in Equation (10). As described above, the nonlinearities in the general revenue sharing formula ensured that similar cities received different amounts of funds. However, the three-factor and five-factor formulas imply that the GRS transfers were correlated to per capita income, non-school "tax effort", and population of the city governments, as well as the higher-level variables used for the allocation to their encompassing counties and states. Assuming that the expenditures are independent of all of these variables would be implausible; in particular, the city-level tax effort, taxes and population as well as the county-level per capita income seem likely to have an effect on city government expenditure decisions in perhaps a nonlinear way.[17] To satisfy the exclusion restriction given my assumptions, I include a flexible cubic polynomial of the lags of each of the variables that are used in the general revenue sharing calculation.[18] Details on the sources of the controls are described in the next section.

Given the independence of GRS_{it}, I argue that $GRS_{it} * U_{it}$ is also independent of the error term in Equation (10). Since most of the law changes to U_{it} occurred before the estimation time period, the city fixed effects will largely pick up the city characteristics that may have been correlated to U_{it}. However, there is a concern that the controls included to ensure the independence of the GRS_{it}, i.e. the city-level tax effort, taxes, and population, and county income, may affect "no-bargaining" cities in a different way than "bargaining" cities. To account for this possibility, I also interact each control with the bargaining indicator, U_{it}, in my preferred specification.

Finally, for those cities that were parts of states that instituted bargaining laws within the sample studied, there is the concern that factors present within the cities (and reflected in city government finance decisions) affected the timing of the passage of the laws. If this were the case, than it is possible that U_{it} is correlated with ε_{it} in Equation (10). As is shown in Figure 7, 8 states passed laws during the period studied, changing their value of U_{it}. Extensive research on the collective bargaining laws carried out in the late 1980s found that, given that the change was ultimately going to occur, the timing of the law passages were largely exogenous, having more to do with the superficial political environment of the state legislature than the political or public will towards collective bargaining. Ohio is a good example of this; although Ohio

[17]Because the city-level per capita personal income is only released every 10 years, the (annual) county-level per capita income is the variable in the GRS formula that best proxies fluctuations in local-area personal income.

[18]The general revenue sharing allocations were updated quarterly with the most current data available. In practice, this meant that the data used in the GRS formula were lagged at least 2 to 4 quarters. To best approximate this lag with annual variables, I include all controls with a one-year lag. Furthermore, I do not include county-level controls or the state-level urban population, since these variables were not updated on an annual basis.

had some of the strongest private sector unions in the country, they were one of the last states to pass a public sector bargaining law in 1985. Although the will of labor and the public had been behind the law for many years, haggling over the details of the law lead to a long delay. Saltzman (1988) documents this delay, and also argues convincingly that the passage of the law in Ohio had a significant effect on the strength of its public sector unions. Freeman and Valletta (1988) also provide evidence that the state laws were a major factor in determining whether public sector employees were covered by collective bargaining contracts. Given this research surrounding the timing of the laws, it may be reasonable to assume that the city fixed effects will pick up any political or public will toward collective bargaining so that the timing of the law change and U_{it}, and $GRS_{it} * U_{it}$, remain independent of the error term.

1.4.2 Further Assumptions

To ensure the validity of the empirical strategy outlined above, I must make two more assumptions. First, I assume that the dependent varible in Equation (10) depends only on contemporaneous general revenue sharing funds, and not on lagged or future general revenue sharing funds. Since GRS_{it} can affect future values of itself through macroeconomic effects on I_{it} or any of the other correlates, changes to GRS_{it} may be correlated over time. By only including the contemporaneous change, I introduce an omitted variables problem if the true relationship actually consisted of the dependent variables depending on future or past values of GRS_{it}. I test this by including past and future values of GRS_{it} in the estimation equation. I find that the results are little changed, although the standard errors increase.

Second, I assume that the coefficients on GRS-correlated cubic controls are constant over time. This would not be true, for example, if different governors or mayors weigh personal income differently when determining budgeting policies. If the coefficients are not constant over time, the cubic controls as described in Section 4.1 would not appropriately account for that portion of the general revenue sharing variation that was due to fluctuations in its correlates. To deal with this possibility, in a robustness check, I interacted all of the controls with year dummies and include them in the main specification. I find that the direction of the main results are little changed. In my preferred specification, however, I do not include the control-time interactions.

Further robustness measures are reported in Section 6.

1.5 Data

In this section, I describe the sample used for the analysis, the data sources, and the summary statistics. In the appendix, I include an additional explanation of the adjustment I make to account for the variation in the fiscal years covered by the Annual Survey of Governments.

1.5.1 The Sample

State and local governments are often under-emphasized in analyses of the government spending in the United States. In the 2000s, federal nondefense consumption and investment made up only about 2 percent of GDP in comparison to that of state governments which made up 4 percent of GDP and that of local governments which comprised 8 percent of GDP.[19] Federal defense spending is more volatile, making up more than 10 percent of GDP in the 1960s and less than 4 percent of GDP at its trough in 2000. Figure 6 shows this breakdown of government spending and highlights the particular importance of local governments to GDP.

Despite its limited direct effect on GDP, federal policy does play a significant role in the path of government consumption and investment through its control over intergovernmental grants to state and local governments as well as regulation of their activities. In this paper, I focus on the effect of intergovernmental grants to large city governments over the period 1971 to 1989. This time period comes at the tail end of a fifteen-year period of rapid growth in local governments; as seen in Figure 6, the local government contribution to GDP grew from 5.9 percent in 1959 to 8.2 percent by 1974 after which it roughly leveled off. I specifically focus on city governments that had a population of 25,000 or greater in 1972. Collectively, these 837 city governments accounted for roughly 30 percent of all local government expenditure.[20]

[19] Each of these estimates came from averaging over the years 2000-2007, which are the seven most recent years in which the government GDP data were broken up between state and local governments. During the same time period, federal defense consumption and investment made up 4.4 percent of GDP. All components of government together made up 18.6 percent of GDP.

[20] In 1972, the number of governments (with the percent of the local government expenditure that they made up in parentheses) was: 3044 county governments (20%), 18,517 city and town governments (36%), 16,991 township governments (3%), 23,885 special district governments (7%), and 15,779 school districts (33%).

City governments provide a broad range of services including police and fire protection, highway construction, sewerage, solid waste management, and utility provision. Their revenues come mainly from a combination of property taxes, intergovernmental revenues, charges and fees, and utility payments. Table 2 shows the breakdown of expenditures and revenues for the 837 cities studied in this paper.

It is worth noting that although education expenditures made up 13 percent of the total expenditures of all of the cities in the sample (see Column (2) of Table 2), less than half of the cities have a positive amount of education expenditures (Column (3) of Table 2). In fact, only 131 of the city governments in the sample are responsible for the K-12 school systems within their city. In the other 706 cities, school districts with separate revenue streams are responsible for funding and organizing K-12 education. When including school districts in the universe of all city, town, and township governments, education made up more than 50 percent of total expenditures in 1977.

1.5.2 The Annual Survey of Governments

Since many of the variables used in the estimation are directly from the Annual Survey of Governments (ASG) produced by the Bureau of the Census, it is worth mentioning a few facts about this survey. In years ending in -2 and -5, the Census conducts a complete survey of all state, county, city, town, and township governments and school districts. In the intermediate years, they only survey a random sample in which local governments are assigned a probability depending on the area population and other characteristics. Because most large cities are included in the yearly sample with 100% probability, most of the cities in my sample are represented in every year from 1971-1989.[21] The exact variables used from the survey will be described in the sections below. All of the finance variables used from the ASG are deflated using the state and local GDP deflator (from the Bureau of Economic Analysis) and are normalized by the city population (from the ASG).

[21] The sample consists of 837 cities that had a population of greater than 25,000 in 1972. All of the cities appear in the sample for the following years: 1972, 1974, 1975, 1976, 1977, 1978, and 1979. For the rest of the years, the number of cities in the sample is shown in parentheses: 1971 (806), 1973 (805), 1980 (834), 1981 (835), 1982 (830), 1983 (830), 1984 (830), 1985 (829), 1986 (829), 1987 (830), 1988 (828), 1989 (803).

1.5.3 Outcome Variables

The three outcome variables that I will focus on are total city government expenditures, normalized employment, and normalized government employee annual wages. Total expenditures come directly from the ASG and are deflated using the state and local GDP deflator and normalized by the city population. To calculated normalized annual wages, the annual wages are first computed by dividing deflated salaries and wages by total government employment. Multiplying this annual wage by the number of government employees in 1972 gives the normalized wage. The normalized employment variable is arrived at in a similar manner, from multiplying total government employment by the 1972 annual wage. The purpose of these normalizations is to convert the employment and wage variables into expenditure statistics such that the β_1 and β_2 coefficients in Equation (10) can be used to answer the question: How much of each dollar transferred to local governments goes towards employment, and how much goes toward increased wages? I use these normalized variables for ease of interpretation; the message and significance are unchanged when I use non-normalized employment and annual wages in the estimations.

I show results for other outcome variables. Capital outlays, expenditures on employee retirement programs, and own source revenues are taken directly from the ASG. Net new debt issued is calculated by subtracting retired debt from new debt issues, the change in cash and security holdings is calculated from subtracting the previous year's holdings from the current year holdings, and the change in retirement fund cash and security holdings is also calculated by subtracting a lag of total holdings from the holdings in the current year.

1.5.4 Union Variables

The variables used to represent the collective bargaining strength of the public sector come from a dataset collected by Richard Freeman and Robert Valletta at the National Bureau of Economic Research for the years 1959 to 1986, and then extended by Kim Rueben through 1996. In my preferred specification, I use an indicator variable that is equal to zero if the city resides in a state in which there is no provision for public sector collective bargaining or in which collective bargaining is explicitly prohibited. In the cities that reside in a state with an indicator of one, it is either the case that there is a "weak" bargaining provision in which public sector labor has a right to present proposals or to meet and confer or the employer is authorized but not required to bargain, or a "strong" bargaining provision in which the public sector employers have an implied or explicit duty to bargain "in good faith". Figure 7 shows the timing of the legislation passage for this indicator variable.

Figure 8 shows the geographic variation across states in the collective bargaining laws in the year 1972. Expectedly, there appears to be a high correlation between the existence of collective bargaining laws and the party preference of a state. In the robustness section, I will show that controlling for the party of the state governor does not alter the estimated effects of the collective bargaining indicator term.

The Freeman-Valletta dataset and the Rueben extension distinguish between bargaining laws for state employees, municipal police, municipal fire fighters, noncollege teachers, and other local employees. I use the "other local employees" category for the creation of the union indicator variable. The correlation between the legislation for different employee groups is high.[22]

In the robustness section, I explore the results with two different possible union variables. First, I examine an indicator as to whether union dues are allowed to be subtracted directly from the paychecks of government employees. Second, I create an indicator which represents whether there exists legislation which specifically includes wages in the scope of bargaining.

1.5.5 Endogenous Variables

In all of the instrumental variable regressions, the two endogenous variables are per capita intergovernmental transfers and per capita intergovernmental transfers interacted with the union variable described above.[23] Intergovernmental transfers are from the ASG.

[22] Over the time period 1970 to 1989, the correlation between the indicator representing a weak bargaining provision for "other local employees" and each of the indicators for police employees and fire protection employees is greater than 70 percent. The correlation with the bargaining provisions for noncollege teachers (which is less relevant for the study of general revenue sharing) is 50 percent.

[23] I choose total intergovernmental transfers rather than just federal intergovernmental transfers because of a correlation between general revenue sharing funds and state intergovernmental transfers. The source of this correlation is described in Section 6.1.

1.5.6 Instruments

The two instruments used are the general revenue transfers received by the city, and the general revenue sharing funds interacted with the union variable. Because the ASG included a general revenue sharing variable for the years in which the program was in place, I have the exact amounts that the city governments received through the program (as reported in the Census survey by the city governments).

1.5.7 Controls

In almost all regressions, I include city and year fixed effects. Because of this, any city characteristics that are immutable over time cannot be included in the regressions as they are collinear with the government fixed-effects. The baseline controls that I choose to use are those that validate the instrument, as discussed in Section 4.1. The controls that I use are a flexible cubic polynomial in each of the following variables: population, lagged per capita tax revenue, lagged "tax effort" (non-school taxes divided by 1969 per capita personal income), lagged per capita county income, lagged state-level total taxes, lagged state per capita income, and lagged state government individual income taxes.[24] Tax revenue is from the ASG, 1969 per capita personal income is from the 1970 Dicennial Census, and county personal income is from the Bureau of Economic Analysis (BEA) regional accounts. I also include a cubic polynomial of lagged own source revenue as a baseline control variable. Although the GRS formula relied on own source *taxes,* there is evidence that some of the local governments were able to count other types of revenue (fees, for example) in the tax base when the formula was calculated. To fully capture this, I include total own-source revenues as a control.

1.5.8 Summary Statistics

Table 3 shows the summary statistics for the variables used in the estimation equations for the year 1977. On average, cities employeed over 2,000 workers and paid them almost 37,000 (2005) dollars each, although the variation across cities for both of these statistics was substantial. The

[24]For areas smaller than counties, per capita income is released every ten years as a part of the Decennial Census. The measure of local per capita income used in the general revenue sharing formulas through 1982 was therefore the 1969 per capita income published in the 1970 Decennial Census.

average city population in my sample was about 100,000, again with considerable variation.

1.6 Results

In this section, I present the baseline results. I first show that the IV regressions have a strong first-stage. I then examine the flypaper effect and find that the city governments increased expenditures by one dollar for every dollar of intergovernmental transfers received. In the next subsection, I explore what the city governments spent the funds on; in particular I examine the employment/wage decision and find that the no-bargaining cities used a significant portion of the transfers to fund new employment whereas the bargaining cities spent on increased wages instead. Finally, I explore the robustness of the results.

1.6.1 First-Stage Regressions

Table 4 displays the first-stage regressions in specifications that do not yet include the indicator for bargaining. The first three columns of Table 4 show OLS regressions of per capita intergovernmental transfers against per capita general revenue sharing receipts, as well as a set of controls including city fixed effects, year fixed effects, and state-time trends. In the first column, I include the full set of baseline controls (without the interactions with the bargaining indicator), in the second column, I exclude the quadratic and cubic polynomials of the allocation variables, and in the third column I do not weight by population. Columns (4) and (5) split the effect on total intergovernmental transfers into the effect on federal intergovernmental transfers in Column (4) and state intergovernmental transfers in Column (5).

The first-stage is strong. The coefficient of 1.5 in Column (1) implies that each dollar of per capita general revenue sharing receipts to the city governments led to an increase in per capita intergovernmental transfers of 1.5 dollars. There are two main reasons that the coefficient exceeds one. First, as shown in Column (5), a large part of the excess is due to a positive (albeit insignificant) correlation between the general revenue sharing transfers and state intergovernmental transfers. Because the geographic tiering led to a high correlation between the state government general revenue sharing funds and the local government general revenue sharing funds, one would expect that the state intergovernmental transfers would be correlated with the city general revenue sharing receipts if the state governments "passed on" a certain percentage of revenue that they received from the federal government. Furthermore, it is possible that state governments piggy-backed on the general revenue sharing formula to disperse some of their own intergovernmental transfers, which would also lead to a positive coefficient in Column (5). The positive correlation between the state intergovernmental transfers and the general revenue sharing funds is the primary reason that I chose the endogenous variable to be total intergovernmental transfers rather than just federal intergovernmental transfers.

Second, a reason that the coefficient on federal intergovernmental transfers in Column (4) slightly exceeds one is that a countercyclical revenue sharing program implemented from July 1, 1976 through September 30, 1978 was based on the general revenue sharing formula. Through this program, a total of 3.1 billion dollars was distributed by the federal government to all governments in areas that experienced unemployment rates greater than 4.5 percent.[25] In the ASG 1978, 733 out of the 837 cities studied in this sample had an unemployment rate of less than 4.5 percent, representing more than 90 percent of the total population in the sample cities. Because the countercyclical revenue sharing funds were positively correlated to the GRS allocations, it is expected that a regression of total intergovernmental transfers on general revenue sharing funds would be greater than 1, albeit not substantially so; at its peak in 1977, the countercyclical program made up roughly only one-quarter of the GRS transfers. Although the size of the countercyclical revenue sharing program was too small to have a significant macroeconomic impact–research at the time estimated only very small budgetary responses (Gramlich, 1979 and General Accounting Office, 1977)–the structure could be used as a starting point for future countercyclical revenue sharing designs.

Table 5 shows the first-stage results when the indicator for bargaining power is interacted

[25]The funds were only made available if the national unemployment rate, lagged two quarters, was above 6 percent–a constraint that did not bind for the duration of the program. From July 1, 1976 through September 30, 1977, a baseline allocation of $125 million per quarter was made available for this program, with an additional $62.5 million for each complete one-half percentage point that lagged national unemployment rate was over 6 percent. From July 1, 1977 through September 30, 1978, the baseline allocation continued to be $125 million but with an addition $30 million per quarter for each one-tenth of a percentage point that the lagged national unemployment rate was above 6 percent. The distribution of these funds were as follows: for each government an index was created by mutliplying the amount that the unemployment rate exceeded 4.5 percent by the government's general revenue sharing allocation. Governments that resided in areas with an unemployment rate less than 4.5 percent were assigned an index of zero. The quarter's allocations were then distributed across governments based on their index. For more details of the formulas see the U.S. Budget (1978), the U.S. Budget (1979), and Government Accounting Office (1977).

with the intergovernmental and general revenue sharing transfers, increasing both the number of endogenous variables and the number of instruments to two. Column (1) shows the results with the total intergovernmental transfers as the dependent variable, and Column (3) shows the regression with the dependent variable as the interaction of total intergovernmental transfers with the bargaining indicator variable. The first-stage for both endogenous variables remains strong, with F-statistics above 45 for total intergovernmental transfers, and above 16 for the interacted endogenous variable. For both endogenous variables, the coefficient on its corresponding instrument is roughly 1.5 as in Table 4, Column (1), for the same reasons discussed above.

1.6.2 The Expenditure Response

Table 6 shows the OLS and the IV results for total expenditures with five different specifications. All regressions shown include city fixed effects, year fixed effects, fiscal year interacted with year dummies, and are population weighted. Specification (1) does not include the union variable, the union variable interaction with the endogenous variable, state-time trends, or union interaction terms. Specification (2) adds in the state-time trend to the first specification. Specification (3) includes the bargaining variable and the bargaining interaction term without state-time trends or the controls interacted with the bargaining indicator variable, Specification (4) adds in state-time trends, and finally Specification (5) adds in the bargaining interactions with the baseline controls. The coefficients on the interaction terms in Specifications (3)-(5) are measures of the difference between bargaining and no-bargaining cities. To be clear about the interpretation, the coefficients in the OLS panel of Column (3) suggest that for every dollar of intergovernmental revenues, governments in no-bargaining cities spent 97 cents (from the coefficient on the IGR term), and governments in bargaining cities spent 62 cents (which is achieved by adding the coefficient on IGR, 0.97, and the coefficient on $IGR * Bargaining$, -0.35). The coefficient on the $IGR * Bargaining$ term suggests that the difference between the two city types in Specification (3) was significantly different from zero at the 1% level.

Examination of both panels in Specifications (3) through (5) show that the OLS coefficients are quite similar to the IV coefficients for the no-bargaining cities. However, the coefficients for the bargaining cities are higher in the IV regressions than in the OLS regressions, suggesting that there was a downward bias in the OLS results for bargaining cities. A downward bias is unsurprising if the intergovernmental transfers were targeted toward struggling city governments that were in the process of cutting expenditures. The difference between the OLS and the IV results emphasize the need for instrumental variables in this analysis.

The IV coefficients in all of the specifications above are suggestive of a strong expenditure response to the intergovernmental transfers. In the preferred Specification (5), the results imply that for every one dollar of increased intergovernmental transfers, no-bargaining cities increased their expenditures by 0.96 dollars and bargaining cities increased their expenditures by 0.88 dollars. The difference between the expenditure response in bargaining and no-bargaining cities is not significant.

One concern with a policy of intergovernmental transfers during recessions is that the recipient governments will use the funds to reduce debt or pad their balances rather than to increase expenditures. Furthermore, although a legislated decrease in taxes might have a stimulative effect, it is often argued that the multiplier is much lower than the multiplier attached to government spending. Thus, it is important to understand whether any of the transfers went towards reduced taxes, reduced debt, or increased savings. To fully map the passage of each dollar received by the city governments, I consider the identity describing the possible effect of IGR on four broad government finance components: total expenditures, own-source revenues, net debt issued, and savings. Equation (13) displays this identity.

$$IGR = Expenditure + Savings - (Own\,\mathrm{Re}\,venue + NetDebtIssued) \tag{1.13}$$

Table 7 maps the effects of a dollar of IGR on these components of government finance; from the identity above, one would expect that the coefficients in the first row should sum to one dollar, and that the coefficients in the second row should sum to zero. The first column of Table 7 shows the effect of IGR on total expenditures, the second on own-source revenue, the third on net debt issued, and the last column on the change in cash and security holdings, an imperfect proxy for savings. Even given this imperfect proxy, the identity of Equation (13) roughly holds. For non-bargaining cities, I find that an increase of one dollar per capita intergovernmental transfers leads to an increase of 0.96 dollars in expenditures, an *increase* of 0.20 dollars in own source revenues, a decrease of 0.35 dollars in net debt issued, and no change in savings. For bargaining cities, I find that expenditures increase by 88 cents and that own source revenues only increase by 10 cents for each dollar received of intergovernmental transfers. Furthermore, there appears to be a negligible affect on debt issuance and a slightly positive, but insignificant, effect on savings for bargaining cities.

Although the increase in own-source revenue is not significantly positive, it *is* meaningfully non-negative; i.e. the results comfortably rule out any substantial decrease in taxes in response to the intergovernmental transfers. In fact, it may be puzzling that the response of own-source revenue appears to be positive, albeit with large standard errors. A positive response of own-source revenue would be consistent with an upturn in the economy due to the increase in government spending, a theory that I will touch upon later in the paper. It would also be consistent with any legislated tax increases that occurred coincidentally with the general revenue sharing transfers. For the moment, the most telling aspect of the result in Column (2) is that I find no evidence that any of the transfers were used to alleviate taxes.

In summary, I find that throughout the 1970s and the 1980s, intergovernmental transfers led to a one-for-one increase in city expenditures and did not lead to any decrease in own-source revenues. Although these results agree with much of the earlier flypaper literature (see Hines and Thaler, 1995), they are at odds with some of the more recent work on the flypaper effect (see Lutz (2006) and Knight (2002)).

The setting in which an intergovernmental program is studied is crucial to the examination of the expenditure responses of the local governments. For instance, Lutz (2006) found a negligible expenditure response to a large grant increase to New Hampshire school districts in 1999, a time when the unemployment rate in New Hampshire was 2.8 percent. Certainly, the effect of transfers on government expenditures should depend on the type of local government, the geographic location in which the "experiment" occurred, and the state of the economy. Because of this, any evaluation of a policy of intergovernmental transfers ought to rely on analysis conducted over similar settings to the one in which the policy would be implemented.

The setting of the general revenue sharing program makes it particularly suitable for the evaluation of a broad-based federal transfer stimulus policy. First, with federal transfers to all general-purpose governments, the general revenue sharing program is the most comprehensive transfer program in the history of the United States. Second, it was conducted at a time when state and local government budgets were suffering and over a period in which two large recessions occurred. As Figures 9 and 10 show, the national and local unemployment rates were relatively high throughout the entire duration of general revenue sharing; in fact, from 1972 to 1986 the national unemployment rate never fell below 5.7 percent, its average in the post-war period.

There are other reasons that the expenditure effect may have been larger with the general revenue sharing funds than with other transfer programs. The general revenue sharing amounts did depend on the tax-effort of the recipient government. This measure was put in particularly to mute the incentive for the local governments to use the transfers to reduce taxes. Although analysis at the time did not find a relationship between the strength of these incentives and legislated tax decreases,[26] the prospect that these incentives prevented tax offsets is worth exploring more. The fact that the general revenue sharing funds did have a price effect means that the results in this section do not directly test the Bradford-Oates hypothesis that underlies most flypaper discussions. Furthermore, the general revenue sharing program was highly publicized and governments had to fill out statement of use forms. Starting in 1976, governments also had to hold town meetings to discuss the spending of the funds. Although one might argue that the public awareness should have led to a *greater* tax offset (under the Bradford-Oates paradigm), another possibility is that the public awareness led to newly publicized programs that would not have been funded otherwise.

1.6.3 Employment and Wage Responses

Table 8 shows the results for the key outcome variables of this paper. Columns (1) and (2) are of particular interest. In bargaining cities, a one dollar increase of intergovernmental transfers leads to a 0.77 dollar increase in wages of existing employees (the sum of 0.21 and 0.56), whereas in non-bargaining cities there is only a 0.21 dollar increase in wages (and insignificant from zero). On the other hand, in bargaining cities, only 0.12 dollars go towards increased employment, while in non-bargaining cities, 0.41 dollars goes to increased employment. For both wage and employment expenditures, the difference between the bargaining and the no-bargaining amounts are significantly different from one another. There is mild evidence in Column (3)

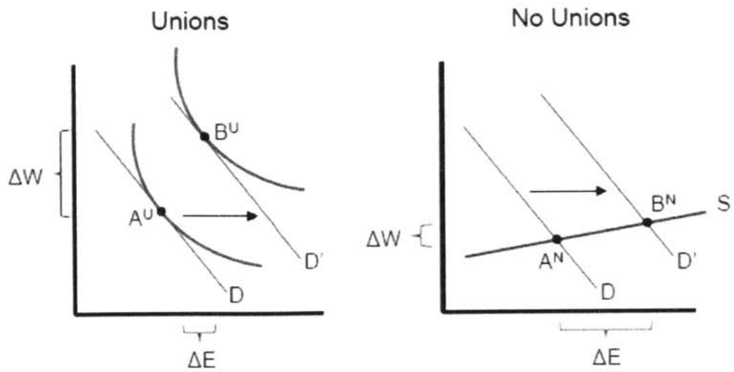
[26] Reischauer, 1975.

that higher transfers lead to more capital outlays in no-bargaining cities. Column (4) shows the effect on retiree expenditures and Column (5) shows the effect on the change in the cash and securities of the retirement funds (a proxy for retirement fund contributions). Neither of these two variables appears to be significantly affected by an increase in intergovernmental transfers in either type of city.

To build a framework in which to think about these results, it is useful to consider a taxpayer-imposed public sector labor demand curve. Taking the expenditure response as given (as discussed in the above section), the demand curve for public employment is shifted out when an intergovernmental transfer is received. Suppose that in cities in states with pro-union laws, the unions have strong bargaining power and are able to choose their position on the labor demand curve.[27] The unions would choose the point on the demand curve that is tangent to their indifference curve. The first plot in Figure 11 shows the point A^U that the unions would initially choose, and the point B^U is the point that the unions would choose once the demand curve had shifted out to D'. In the example shown in the figure, the change in wages would be large, and the change in employment would be small. On the other hand, in the cities in states without bargaining laws, the labor market supply can be thought of as following an elastic labor supply curve; without the unions to impose employment, wage, and hiring restrictions, the market for labor would be more competitive than in their bargaining counterparts. In this case, one sees that a shift in the demand curve would yield a relatively large change in employment and only a small increase in wages.

[27]Note that if unions bargained over wages and employment, they may not end up on a wage-employment point on the demand curve. The question of what unions bargain over is debated in the literature. I assume that the unions bargain over only wages for illustrative reasons.

Figure 11



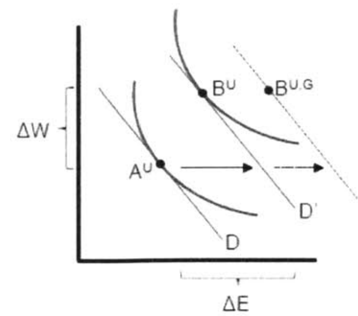
The examples drawn in Figure 11 are illustrative of how the bargaining might lead to a higher wage increase and a smaller employment increase than what would occur in cities without bargaining.

These results shed light onto the role of actors in the public sector labor markets in affecting the use of intergovernmental transfers. They also emphasize that the study of intergovernmental transfers should not be limited to the expenditure effect (i.e. whether the funds are spent), but also to the *type* of spending induced by the transfers. If increased wages did not lead to greater hours or higher productivity, the results would be suggestive that the quality of the spending (i.e. the "bang for the buck") increased more in the cities that were not subject to collective bargaining laws. However, with these data it is impossible to determine decisively whether or how services were affected unequally in the two types of cities. Because the hours of employees are not measured in these data (or in any available data at the city level over that time period), I cannot test whether the increased wages in bargaining cities funded an equivalent increase in hours to the rise in hours coming from the increased employment in the no-bargaining cities. Furthermore, even if hours were not increased, higher wages may lead to higher productivity or service provision. This is demonstrated in Mas (2006) which shows that New Jersey police officers that won in wage arbitration exhibited higher workplace productivity. Although these caveats make it impossible to determine which type of city increased services more in response to the transfers, the results of this section strongly suggest that there is a difference in the way in which the funds were used.

Interactions with Unemployment Rate Table 9 shows the results when the intergovernmental transfers are interacted with an indicator representing the state of the economy. Specifically, the measure I use in the table below is whether the state unemployment rate is more than 2 percentage points above the period average. Other measures of "bad economic times" yield similar results. The first and second rows represent the response to IGR in no-bargaining cities to "bad times" and "good times", respectively, while the third and the fourth rows show the difference between the response in bargaining cities and no-bargaining cities in "bad times" (Row 3) and "good times" (Row 4). Columns (1) and (3) show that the expenditure and the wage responses do not appear to change substantially in either time frame. However, the difference in the transfer-induced employment increase between bargaining and no-bargaining cities appears to shrink during "good" times (when unemployment is less than two percentage points above the period average). The standard errors in Column (4) are too large to draw any firm conclusions about the effect on capital outlays.

The fact that the difference between the employment response in bargaining and the no-bargaining cities shrinks in times when the unemployment rate is lower than average is particularly interesting. To explain why this might occur, I consider the theory in the public sector union literature that public sector unions can use their political strength to shift out the demand for public employment (Freeman (1986), Marlow and Orzechowski, 1996)), but I assume that their ability to do so depends on the state of the economy. This could occur if the union's political voice is drowned out by the many disgruntled voices of citizens at times when the city government is losing revenues and cutting expenditures. In "good" times, when the union succeeds in shifting out the demand curve (and for simplicity, I assume they achieve this goal after the collective bargaining negotiations are carried out), city governments will see a larger rise in employment than they would otherwise. This is pictured in Figure 12.

Figure 12



1.6.4 Robustness

Table 10 explores the robustness of the total expenditure, normalized wage, and normalized employment results. The first column shows the results of the preferred specification shown in Table 5, Column (5) and Table 8, Columns (1) and (2). Columns (3) and (4) show the results with the different measures of bargaining strength: Column (3) uses an indicator for whether the bargaining laws (if they exist) specifically include wages in the scope of bargaining, and Column (4) uses an indicator for whether union dues can be automatically deducted from employee payrolls. The fourth column drops the five largest cities in 1972.[28] Finally, Column (5) shows the unweighted results. The expenditure results are robust to all of these changes. The normalized wage and normalized employment results are fairly robust to changing the measure of bargaining strength, but are not robust to dropping the five largest cities or running unweighted regressions. Together, the five most populous cities make up 20 percent of the sample population in 1972; it is unsurprising that removing them could have substantial effects on the coefficient estimates or that these would move the results in the direction of the unweighted results. Since I seek to find the effect of the bargaining on large cities, conceptually these cities do belong in the sample. Furthermore, to the extent that there may exist fixed costs to unionization (see Trejo (1991)), there is not an a priori reason that one would expect that the effect of bargaining would be the same in a population-weighted regression as in an

[28]These cities, their populations, and their population percent of the total 1972 sample population are: New York (7.9 million, 8.9%), Chicago (3.4 million, 3.8%), Los Angeles (2.8 million, 3.2%), Philadelphia (2.0 million, 2.2%), and Detroit (1.5 million, 1.7%).

unweighted regression. In fact, Columns (4) and (5) provide suggestive evidence that Trejo's argument of fixed costs in union power is correct; the bargaining laws appear to have less power in smaller cities.

In Table 11, I explore the possibility that the union variable is actually capturing political views rather than collective bargaining strength, as well as the possibility that the dependence of the general revenue sharing on higher order interactions of its correlates may be driving the results. In Column (1), I show the results of the preferred specification. In Column (2), I add as a control an indicator variable for whether a democratic governor is in office, and in Column (3) I also interact this indicator variable with the general revenue sharing funds. Columns (2) and (3) specifically address the concern that the bargaining indicator variable is capturing the politics of a state (and its cities) rather than the strength of public sector unions within the state. The results show that the coefficients on the interaction of general revenue sharing with the bargaining indicator variable are little changed, even when I include the indicator of the party of the governor interacted with the general revenue sharing funds, as in Column (3). Legislation such as that surrounding public sector collective bargaining is often politically hard to change once in place, leading to legislative persistence over time even as the party preferences of the states change.

In Column (4), I include the "three-factor" formula at the city level; i.e. the product of population, tax effort, and inverse per capita income.[29] In Column (5), I also include the "three-factor" formula at the state level. These last two columns deal with the concern that the cubic polynomials of the GRS-correlated variables do not fully capture their direct impact on the outcome variables. Including more interactions of these variables yields almost no difference in the estimation of the coefficients of interest. These results are supportive of the conclusion that the flexible cubic polynomials absorbed an appropriate amount of the general revenue sharing variation.

[29] Because per capita income is not annually available at the city level, I interact with per capita income at a county level.

1.7 Intergovernmental Transfers and Aggregate Economic Activity

My results above show that the general purpose transfers in the 1970s and the 1980s led to approximately one-for-one increases in local government expenditure. At first blush, these findings of a strong expenditure effect are supportive of a stimulative policy of transferring funds to local governments; at least the highly debated question of whether the funds would be spent appears to be resolved for the context I study in this paper. Any complete evaluation of a transfer policy, however, would have to include a consideration of the output multipliers attached to local government spending. Since the results of Section 6.3 find that there exists an institutional friction, public sector bargaining, that determines whether federal transfers are applied to higher wages or new employment in large cities, the question of whether these two types of spending could have different effects on the surrounding private economy is crucial. In this section, I explore the possibility that the types of spending produced by transfers to bargaining and no-bargaining cities are associated with different output multipliers.

A clear connection has been shown between an individual's income and their marginal propensity to consume (Parker, Souleles, Johnson, McClelland, 2011). If government spending on employment gives income to a person who would have otherwise been unemployed as opposed to increasing the wages of an already employed individual, one would expect that the marginal propensity to consume of the former would be greater than the latter. If this is the case, theory would suggest that the multiplier would be higher when the spending is on employment rather than wages.[30] This argument depends on the idea that increasing the number of government jobs would actually reduce unemployment in the local economy. At times of full employment, this is an unreasonable assumption; government spending on increased employment would crowd out employment in the private sector. However, when the unemployment rate is above the natural rate of unemployment and there is excess capacity in the economy and slack demand, government job creation is more likely to have an immediate effect on unemployment. This description is a much closer approximation to the time period in which the general revenue

[30]The connection between the marginal propensity to consume and the output multiplier is discussed extensively in the New-Keynesian literature (see, for example, Gali, Lopez-Salido, Valles, 2007).

sharing was carried out.

1.7.1 Empirical Strategy and Results

In this section, I explore the possibility that the multiplier on the types of expenditures generated in bargaining and no-bargaining cities differ from one another. Note that although the theory above focuses on the distinctions that might arise due to differential spending on new employment versus existing wages, my methodology only allows me to test the difference between bargaining and no-bargaining city spending. To the extent that the spending differs in other ways (such as the suggestive evidence that no-bargaining cities spend more of the transfers on capital outlays), I will not be able to determine which particular differences are affecting any divergence in multipliers.

Because city-level private employment and income data are not available, I use annual county employment and income data from the Bureau of Economic Analysis regional accounts. The data used are from BEA Table CA04. As described above for non-ASG variables, I adjust these data to match the timing of the fiscal years in each city, and I deflate the income data using the GDP deflator.

I examine the effect of intergovernmental transfers on private employment and income. The government finance data are not ideally suited to study the effects on the private economy. Because the macroeconomic data are only available at the county level while the intergovernmental transfer data are at the city level, I cannot simply replace the outcome variables in Equation (10) with the county BEA data.[31] I deal with the city and county disparities in three ways. First, I limit my analysis to cities that make up at least 50 percent of their corresponding county, which dramatically shrinks the sample from 837 cities to 206 cities. Second, I normalize the county variables by county population rather than city population. Finally, to account for city-county differences in per capita intergovernmental transfers, I scale the endogenous variables, IGR_{it} and $IGR_{it} * U_{it}$, by the county-city IGR ratio in the most recent government census year (i.e. 1972, 1977, 1982, or 1987).

Table 12 shows the crosswalk from the city employment results initially reported in Table

[31] County area data are only available in the Annual Survey of Governments in the Census years ending in -2 and -7.

8 to the macroeconomic total employment results. Column (1) shows the results achieved when using the normalized employment variable as shown in Table 8. Column (2) changes the dependent variable to employment per 1000 population, and the results imply that a transfer of 1,000,000 (2005) dollars led to an increase of 10.8 jobs in no-bargaining cities, and 2.6 government jobs in bargaining cities. Column (3) limits the sample to cities that make up at least 50 percent of their county. The findings are similar to that of Column (2), although the standard errors have increased due to the smaller sample size.

Finally, Columns (4) and (5) have per capita total county employment as the dependent variable and scale the endogenous variables as discussed above. Column (4) implies that 1,000,000 dollars of general revenue sharing receipts *to all of the local governments in a county* yield an increase of 9.9 government jobs in no-bargaining cities and a decrease of 2.8 jobs in bargaining cities. If the response of all of the governments mirrors the response of the city government, one would expect that the coefficients in Columns (3) and (4) should match. Indeed, they are within the same range, and a difference of zero cannot be rejected. It is concerning that the county government employment data appears to predict a *decrease* in government employment in response to the intergovernmental transfers. However, wide standard errors make it impossible to reject zero or a positive response. The most significant result in Column (4) is the *difference* between the government employment produced in bargaining and no-bargaining cities.

Column (5) shows the main test of interest, which is the effect of the intergovernmental transfers on total employment within the county. There appears to be a large response in no-bargaining cities, and a slightly negative response (although zero cannot be rejected) in bargaining cities. Again, the most significant result is the *difference* between bargaining and no-bargaining cities, which is significant at the five percent level.

The level of the coefficients in Table 12 Column (5) implies that 1,000,000 of (2005) dollars of transfers led to an increase of 32 total jobs in no-bargaining counties, and a decrease of 13 jobs in bargaining counties. One way to interpret these coefficients is to assume that the employee compensation is equal to the employee's marginal product and to multiply the jobs created by the average employee compensation in 1977 (to take an intermediate year), which was 39,000 dollars.[32] This yields a relative increase in GDP of about 1,200,000 (2005) dollars for every 1,000,000 received in transfers, which would imply a multiplier of roughly 1.2 in no-bargaining cities (and a slightly negative multiplier–insignificant from zero–in bargaining cities). These multiplier estimates are within the bounds calculated in recent empirical work; however, the large standard errors around the point estimates prevent the possibility of firm conclusions about the levels of multipliers in this exercise.[33] Indeed, the key result is the difference between the multipliers in bargaining and no-bargaining cities.

Table 13 explores the robustness of the total employment results in Table 11, Column (5). In Column (2), the sample is expanded to include cities that make up at least 30 percent of the counties in which they reside, and in Column (3), the sample is restricted to only include those cities that make up at least 85 percent of their counties. In Column (4), the state-time trend is removed, and in Column (5), the largest five cities are removed from the preferred specification. The results are not robust to all of these changes. In particular, the standard errors are too large in Columns (3) and Columns (5) to infer anything meaningful from the difference in the response in bargaining and no-bargaining locales, although it is notable that the sign of the difference in the multipliers switches in Column (5). In Columns (2) and (4), the difference between the two types of city remains significantly negative at at least the 10 percent level. Overall, the (mostly) negative point estimates in the second row are suggestive of the idea that the multipliers on intergovernmental transfers to no-bargaining cities are higher than those to bargaining cities, which would be consistent with the hypothesis that spending on new employment stimulates the private economy more successfully than spending on wages. However, due to large standard errors and fluctuations across specifications, these data are not able to fully weigh in on this hypothesis. I present these employment results to motivate further exploration into how the public sector labor markets may affect the stimulative output multipliers associated with federal government transfers to state and local governments.

[32] To arrive at this average compensation number, I used figures from the Bureau of Economic Analysis NIPA accounts. Specifically, I divided the total compensation of employees in 1977 from Table 6.2B by the total number of full-time equivalent employees in Table 6.5B. Lastly, I multiplied by the GDP price index from Table 1.1.4.

[33] I also examined the effect of the general revenue sharing transfers on per capita personal income. However, the standard errors in these calculations were too large to draw any meaningful conclusions; neither the multiplier levels nor the differences between bargaining and no-bargaining cities were significantly different from zero in any specification.

1.8 Conclusion

In this paper, I revisit a large intergovernmental grant program in which the federal government transferred funds to all general-purpose state and local governments. I find, contrary to some recent research, that the recipient city governments spent almost all of the funds that they received. This finding, on the face, is supportive of a stimulative policy of transferring funds to city governments, at least during tumultuous economic times similar to those of the 1970s and the 1980s. However, I discuss the possibility that the "type" of government spending produced by the transfers may be as important as the fact of the spending itself when evaluating a policy of intergovernmental transfers. Motivated by the literature on public sector unions, I explore whether the type of spending is affected by the existence of public sector collective bargaining legislation. I find that the cities subject to state-level pro-union bargaining laws spent a significant portion of the transfers on increased wages of existing employees, while cities without such laws spent a larger fraction of the funds on new employment. Finally, I explore the possibility that these two types of spending have differential effects on the private economy.

This paper brings together a combination of macroeconomics and public finance topics that have rarely been linked in previous research. The public finance literature on intergovernmental grants has tended to focus on the impacts of grants on the hiring and spending behavior of the government bodies without considering the implications that this changed behavior may have on the private economy. This issue becomes particularly important in a recessionary environment when national governments are weighing their countercyclical options at the same time that their subnational governments are responding to their own fiscal pressures. Subnational governments play a substantial role in most advanced and emerging countries; the ratio of their expenditures to total government expenditures hovers between 30 to 50 percent for many countries, and exceeds 50 percent in at least Canada, Denmark, Switzerland, and the United States (Rodden, 2004). Yet, there has been little work done to understand the effectiveness of leveraging these subnational governments to stimulate the economy. More broadly, the general connection between subnational budgets and the business cycle warrants further investigation. While there has been some work using aggregate time series on the extent to which subnational government variables move with or against the business cycle (Hines, 2010, Rodden and Wibbels, 2010), this paper highlights the fact that aggregate time series may mask some interesting heterogeneity at the local government level. Exploring this set of questions using disaggregated data will be fruitful for future research.

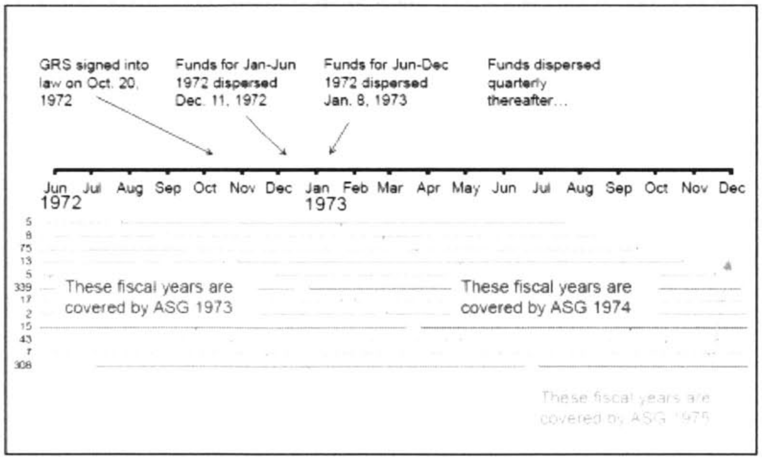
From the macroeconomics side, the literature on the effect of government spending tends to ignore the political economy frictions that influence the "quality" or type of government spending that is feasible. The institutional frictions that influence the direction of spending may have a large impact on the ultimate effectiveness of a temporary expansion of government. The findings of this paper suggest a natural path for future exploration; namely, probing into the possible multiplier differences that may arise from government spending on new employment versus government spending on increased wages. Furthermore, although this paper focuses on the local government sector of the United States, the large size of public sector unions in other advanced economies suggests that the relevance of this line of future research may apply to other countries.

1.9 Appendix

In this appendix, I describe the adjustments that I make to account for the fact that the Annual Survey of Governments covers a variation of fiscal years across cities.

City governments vary substantially in the timing of their fiscal years reported in the ASG. Naively using year fixed effects corresponding to the years in the ASG to pick up macroeconomic events would be incorrect. Local governments filling out the ASG in a particular year are instructed to report on their fiscal year that ended between July of the previous year and June of the survey year. For example, in the 1973 ASG, the finance variables of cities that have fiscal years from July to June will cover the fiscal year July 1972 to June 1973. On the other hand, cities with fiscal years from August to July will cover August 1971 to July 1972 in the same 1973 survey. The ASG year fixed effect will therefore not capture the macroeconomic events corresponding to a particular year. Furthermore, other variables such as the price index used to deflate the nominal finance figures must correspond to the time period of the cities' finance variables.

The figure below shows the timing of the initial general revenue sharing fund disbursements as well as the timing of the fiscal years covered by the 1973 and the 1974 ASGs. The numbers in the left column underneath the timeline represent the number of cities in the sample with the corresponding fiscal year timing to the right of the number. There are 339 cities that have a fiscal year that end in December, and 308 cities that have a fiscal year that end in June. As shown in the figure, the initial funds were disbursed in December 1972. This disbursement should show up in the 1973 ASG for cities that have fiscal years ending in December through June, and will show up in the 1974 ASG for all other cities.



There are two main issues that need to be addressed in this analysis. The first is that the year fixed effects need to be adjusted so as to correctly capture the timing of the nationwide macroeconomic events. The second is that the non-ASG variables must be adjusted to match the fiscal years represented by the cities.

Accomplishing the first task is easy; rather than only including year fixed effects, I also include an indicator for the fiscal year timing interacted with year dummies–which means that, in essence, I allow the year fixed effect to depend on the fiscal year timing of the particular government. For instance, if there was a macroeconomic event that occurred in January 1973 that caused a significant drop in city government revenue, we would expect to observe that drop in the 1973 ASG for all cities that have a fiscal year ending in the months from January to June, and in the 1974 ASG for cities that have a fiscal year ending in a month after June. Allowing the year fixed effect to depend on the fiscal year ending month will allow this drop to be distributed as it should be.

To accomplish the second task, I must adjust all non-ASG variables to match the timing of the specific city to which the variable is "applied". All non-ASG variables are adjusted in the following way:

$$\begin{aligned} Y_{it} &= Y_{it-2} * (1 - \tfrac{fy}{12}) + Y_{it-1} * (\tfrac{fy}{12}) \quad \text{if } fy > 6 \\ Y_{it} &= Y_{it-1} * (1 - \tfrac{fy}{12}) + Y_{it} * (\tfrac{fy}{12}) \quad \text{if } fy \leq 6 \end{aligned} \tag{1.14}$$

where fy is the fiscal year ending month, and Y_{it} is the non-ASG variable to be adjusted. The non-ASG variables described in the data sections above that are adjusted are the GDP deflators and the BEA per capita county income.

Tables

Table 1: Federal Funds Transferred through the GRS program

Year	(1) Millions of current dollars	(2) Millions of 2005 dollars	(3) Percent of GDP	(4) Percent of Gov't Expenditures: State Governments	(5) Percent of Gov't Expenditures: Local Governments	(6) Per Capita, 2005 Dollars
1972	5,300	25,364	0.43%	2.79%	3.56%	120.8
1974	6,125	24,529	0.41%	2.62%	3.25%	114.7
1976	6,500	22,326	0.36%	2.23%	2.80%	102.4
1978	6,850	20,731	0.30%	2.02%	2.48%	93.1
1980	6,279	15,697	0.23%	1.19%	2.06%	69.1
1982	4,567	9,761	0.14%	0.00%	1.79%	42.1
1984	4,567	8,948	0.12%	0.00%	1.55%	37.9
1986	3,425	6,276	0.08%	0.00%	0.95%	26.1

Notes: Government expenditures in Columns (4) and (5) exclude intergovernmental transfers. The GDP state and local price deflator are used to deflate data in Columns (2) and (6). General revenue sharing data are from Maguire (2009). GDP data are from the Bureau of Economic Analysis NIPA Tables 1.1.4, 1.1.5, 3.20, and 3.21.

Table 2: City Finance Statistics

	Total (Millions of $)	Percent of Total	Median Percent of Total
	(1)	(2)	(3)
		Expenditures	
Total Expenditures	57,507	100	100
General Expenditures	47,007	81.7	88.2
Salaries and Wages	23,041	40.1	42.1
Capital Outlays	8,685	15.1	16.1
COMPONENTS			
Education	7,279	12.7	0
Police	5,011	8.7	11.2
Welfare	4,561	7.9	0
Fire Protection	2,903	5	7.5
Sewerage	2,850	5	4.4
Interest Payments	2,885	5	3.7
Highways	2,844	4.9	8.3
Parks	2,012	3.5	4.4
Housing and Community Dev.	1,700	3	0.1
Solid Waste Management	1,526	2.7	3.2
Central Staff Services	930	1.6	2.6
Health	871	1.5	0.3
Financial Administration	767	1.3	1.7
Public Buildings	687	1.2	1.1
Libraries	570	1	1
Air Transportation	559	1	0
Utilities (Non-General)	8,323	14.5	10.1
Insurance Trust (Non-General)	2,144	3.7	0.3
		Revenues	
Total Revenue	59,933	100	100
Property Tax	12,942	21.6	21.6
State IGR	12,614	21.0	13.3
Charges and Misc. Revenue	8,026	13.4	15.5
Federal IGR	7,556	12.6	8.3
Sales and Gross Receipts Tax	4,749	7.9	5.3
Income Tax	2,880	4.8	0.0
Utilities (Non-General)	7,243	12.1	9.0
Insurance Trust (Non-General)	1,135	1.9	0.0

Notes: Data from the 1977 Census of Governments.

Table 3: Summary Statistics - 1977

	Mean	St Dev	Min	Median	Max
Dependent Variables					
Total Expenditure (pc)	1,451	938	203	1,148	8,098
Total Revenue - Own Sources (pc)	1,040	658	119	828	4,629
Net Debt Issued (pc)	89	347	-742	-15	3,540
Change in Cash Securities (pc)	129	377	-1,849	73	3,925
Salaries and Wages (pc)	579	402	35	441	3,728
Annual Wage	36,984	9,694	2,819	36,130	95,243
Total Employees	2,208	12,788	24	664	350,302
Capital Outlays (pc)	270	277	0	179	2,126
Retirement Expenditures (pc)	19	38	0	3	405
Change in Ret Securities (pc)	69	163	-1,339	0	1,781
Endogenous Variables					
IGR (pc)	438	530	28	296	10,191
Fed IGR (pc)	174	216	0	104	3,425
State IGR (pc)	240	429	0	151	9,693
Instrument and Control Variables					
GRS (pc)	53	27	0	48	174
Total Taxes (pc)	516	390	46	396	3,402
Tax Effort (%)	4	2	0	3	19
County Income (pc)	19,326	3,303	9,528	19,152	32,482
Population	105,516	322,489	21,042	48,327	7,481,613
State Income (pc)	18,810	2,335	12,593	18,565	31,779
State Total Taxes (pc)	2,001	469	1,184	1,972	3,152
State Income Taxes (pc)	323	219	0	367	856

Notes: All dollar terms are expressed in 2005 dollars; the government finance terms were deflated using the state and local GDP deflator, while the county income was deflated using the GDP deflator.

Table 4: First Stage Regressions I

	(1)	(2)	(3)	(4)	(5)
		Total IGR		Federal IGR	State IGR
GRS	1.50***	1.36***	1.59***	1.12***	0.39
	(0.24)	(0.24)	(0.16)	(0.08)	(0.25)
Cubic baseline controls	X		X	X	X
Linear baseline controls		X			
Population Weighted	X	X		X	X
Observations	14,378	14,378	14,378	14,378	14,378
Adjusted R2	0.346	0.313	0.197	0.377	0.387
Partial R2	0.021	0.018	0.016	0.037	0.002
F-Stat	38.3	31.9	94.9	185.3	2.4

Notes: Each column represents the results from an OLS regression in which the dependent variable is regressed against the per capita general revenue sharing receipts (GRS), the baseline controls, city and year fixed effects, interactions between the year and the city fiscal year timing, and state-time trends. The dependent variable is total per capita intergovernmental revenues (IGR) in Columns (1)-(3), federal intergovernmental revenues in Column (4), and state intergovernmental revenues in Column (5). The baseline controls are city population, lagged per capita tax revenue, lagged "tax effort", lagged per capita county income, lagged state-level total taxes, lagged state per capita income, and lagged state government individual income taxes. In Column (2), the baseline controls are entered linearly; otherwise, they are entered as cubic polynomials. The population-weighting is shown in the table. All ASG finance variable are deflated using the state and local GDP deflator, and all non-ASG variables are adjusted for the city fiscal years as discussed in the text. Standard errors are clustered at the city level.

* p<0.10, **p<0.5, ***p<0.01

Table 5: First Stage Regressions II

	(1) Total IGR	(2) IGR*Bargain
GRS	1.48***	0.06
	(0.22)	(0.19)
GRS*Bargain	0.01	1.47***
	(0.39)	(0.35)
Union Interactions	X	X
Observations	14,378	14,378
Adjusted R2	0.354	0.439
Partial R2	0.020	0.023
F-Stat	45.7	16.6

Notes: Each column represents an OLS regression in which the dependent variable is regressed against the per capita general revenue sharing receipts (GRS), the interaction of GRS with the bargaining indicator variable (GRS*Bargaining), the baseline controls, the baseline controls interacted with the bargaining indicator, city and year fixed effects, interactions between the year and the city fiscal year timing, and state-time trends. The dependent variable is total per capita intergovernmental transfers (IGR) in Column (1), and IGR interacted with the bargaining indicator variable in Column (2). The baseline controls are cubic polynomials of city population, lagged per capita tax revenue, lagged "tax effort", lagged per capita county income, lagged state-level total taxes, lagged state per capita income, and lagged state government individual income taxes. All regressions are population-weighted. All ASG finance variable are deflated using the state and local GDP deflator, and all non-ASG variables are adjusted for the city fiscal years as discussed in the text. Standard errors are clustered at the city level.
* p<0.10, **p<0.5, ***p<0.01

Table 6: Total Expenditures Regressions

	(1)	(2)	(3)	(4)	(5)
			OLS Results		
Total IGR	0.66***	0.66***	0.97***	0.94***	1.01***
	(0.17)	(0.18)	(0.11)	(0.09)	(0.07)
Total IGR*Bargain			-0.35***	-0.31**	-0.40**
			(0.12)	(0.14)	(0.17)
			IV Results		
Total IGR (per capita)	0.83***	0.89***	1.03***	0.92***	0.96***
	(0.16)	(0.16)	(0.23)	(0.22)	(0.21)
Total IGR*Bargain			-0.22	-0.03	-0.08
			(0.19)	(0.18)	(0.27)
Government FE	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
State*Time Trend		X		X	X
Fiscal Year Dummies	X	X	X	X	X
Union Interactions					X
Population Weighted	X	X	X	X	X
Observations	14,378	14,378	14,378	14,378	14,378

Notes: The dependent variable in all regressions is total per capita city expenditures. The top panel shows OLS regressions and the bottom panel shows 2SLS IV regressions. In Columns (1) and (2) of the bottom panel, total per capita intergovernmental revenues (IGR) are instrumented by per capita general revenue sharing receipts (GRS). In Columns (3)-(5), IGR and IGR interacted with the bargaining indicator (IGR*Bargaining) are instrumented by GRS and GRS interacted with the bargaining indicator. The additional regressors in each column include the baseline controls, city and year fixed effects, and interactions between the year and the city fiscal year timing. Columns (2), (4), and (5) also include state-time trends, and Column (5) includes the baseline controls interacted with the bargaining indicator. The baseline controls are cubic polynomials of each of: city population, lagged per capita tax revenue, lagged "tax effort", lagged per capita county income, lagged state-level total taxes, lagged state per capita income, and lagged state government individual income taxes. All regressions are population-weighted. All ASG finance variable are deflated using the state and local GDP deflator, and all non-ASG variables are adjusted for the city fiscal years as discussed in the text. Standard errors are clustered at the city level.
* p<0.10, **p<0.5, ***p<0.01

Table 7: Finance Components

	(1)	(2)	(3)	(4)
	TotalExp	RevOwnSource	DebtIssue	DCashSec
Total IGR	0.96***	0.20	-0.35	-0.02
	(0.21)	(0.14)	(0.27)	(0.31)
Total IGR*Bargain	-0.08	-0.10	0.32	0.16
	(0.27)	(0.13)	(0.42)	(0.32)
Observations	14,378	14,378	14,378	14,378

Notes: Each column represents the results from a 2SLS IV regression in which total per capita intergovernmental revenues (IGR) and IGR interacted with the bargaining indicator (IGR*Bargaining) are instrumented by per capita general revenue sharing receipts (GRS) and GRS interacted with the bargaining indicator. The dependent variables in Columns (1)-(4) are, respectively, per capita total expenditures, per capita own source revenue, per capita net debt issued, and the per capita annual change in the city's cash and security holdings. In addition to IGR and IGR*Bargaining, the regressors in each column include the baseline controls, the baseline controls interacted with the bargaining indicator, city and year fixed effects, interactions between the year and the city fiscal year timing, and state-time trends. The baseline controls are cubic polynomials of city population, lagged per capita tax revenue, lagged "tax effort", lagged per capita county income, lagged state-level total taxes, lagged state per capita income, and lagged state government individual income taxes. All regressions are population-weighted. All ASG finance variable are deflated using the state and local GDP deflator, and all non-ASG variables are adjusted for the city fiscal years as discussed in the text. Standard errors are clustered at the city level.

* p<0.10, **p<0.5, ***p<0.01

Table 8: Expenditure Components

	(1)	(2)	(3)	(4)	(5)
	Wage_Norm	Emp_Norm	CapOutlays	RetExp	DRetCashSec
Total IGR	0.21	0.41***	0.26*	-0.03	0.00
	(0.13)	(0.11)	(0.16)	(0.02)	(0.14)
Total IGR*Bargain	0.56***	-0.29**	-0.26	0.03	-0.17
	(0.20)	(0.14)	(0.20)	(0.02)	(0.20)
Observations	14,378	14,378	14,378	14,378	14,378

Notes: The dependent variables in Columns (1)-(5) are, respectively, the normalized wage of government employees, the normalized government employment, per capita capital outlays, per capita retirement expenditures, and the per capita annual change in the city's retirement cash and security holdings. For more details on the specifications, see notes to Table 7.

* p<0.10, **p<0.5, ***p<0.01

Table 9: Recession Effects

	(1)	(2)	(3)	(4)
	Total Expenditures	Emp_Norm	Wage_Norm	Capital Outlays
Total IGR*(UR-UR_AVE $\geq$ 2)	0.77**	0.55***	0.17	-0.08
	(0.30)	(0.15)	(0.21)	(0.37)
Total IGR*(UR-UR_AVE < 2)	0.96***	0.41***	0.17	0.23
	(0.22)	(0.11)	(0.15)	(0.15)
Total IGR*Bargain*(UR-UR_AVE $\geq$ 2)	0.16	-0.43**	0.65**	0.11
	(0.35)	(0.18)	(0.31)	(0.40)
Total IGR*Bargain*(UR-UR_AVE < 2)	-0.06	-0.21	0.77***	-0.17
	(0.30)	(0.14)	(0.28)	(0.25)
Observations	14,181	14,181	14,181	14,181

Notes: Each column represents the results from a 2SLS IV regression in which there are four endogenous variables of interactions with total per capita intergovernmental transfers (IGR) as shown in the table, and four corresponding instruments of the equivalent interactions with per capita general revenue sharing receipts (GRS). "UR" refers to the state unemployment rate of the city, and "UR_AVE" refers to the average of the state unemployment rate from 1972 to 1989. All "recession indicators" interacted with IGR are also includes as controls. The dependent variables in Columns (1)-(4) are, respectively, per capita expenditures, normalized employment, normalized wage, and per capita outlays. For more details on the specifications, see notes to Table 7.

* p<0.10, **p<0.5, ***p<0.01

Table 10: Robustness I

	(1)	(2)	(3)	(4)	(5)
			Total Expenditures		
Total IGR	0.96***	1.03***	0.92***	0.87***	0.81***
	(0.21)	(0.19)	(0.21)	(0.22)	(0.20)
Total IGR*Bargain	-0.08	0.02	0.10	0.03	0.04
	(0.27)	(0.27)	(0.26)	(0.25)	(0.20)
			Normalized Wages		
Total IGR	0.21	0.37*	0.36**	0.17	0.18
	(0.13)	(0.20)	(0.16)	(0.10)	(0.15)
Total IGR*Bargain	0.56***	0.40	0.38*	0.14	0.08
	(0.20)	(0.25)	(0.23)	(0.15)	(0.14)
			Normalized Employment		
Total IGR	0.41***	0.47***	0.33***	0.42***	0.16
	(0.11)	(0.18)	(0.10)	(0.12)	(0.17)
Total IGR*Bargain	-0.29**	-0.37**	-0.15	-0.00	0.03
	(0.14)	(0.14)	(0.14)	(0.18)	(0.17)
Population Weighted	X	X	X	X	
Pop Cutoff				No Top 5	
Bargain Measure	Standard	Union Dues	Scope: Wages	Standard	Standard
Observations	14,378	14,378	14,360	14,288	14,378

Notes: Each column represents the results from a 2SLS IV regression in which total per capita intergovernmental revenues (IGR) and IGR interacted with the bargaining indicator (IGR*Bargaining) are instrumented by per capita general revenue sharing receipts (GRS) and GRS interacted with the bargaining indicator. The dependent variables in the top, middle, and bottom panels are, respectively, per capita expenditures, normalized wages, and normalized employment. Alternative bargaining measures are used in Columns (2) and (3) as described in the text. In Column (4), the top 5 largest cities are dropped, and in Column (5), the regression is not population-weighted. For more details on the specifications, see notes to Table 7.

* p<0.10, **p<0.5, ***p<0.01

Table 11: Robustness II

	(1)	(2)	(3)	(4)	(5)
			Total Expenditures		
Total IGR	0.96***	0.96***	0.90***	0.98***	1.02***
	(0.21)	(0.21)	(0.29)	(0.21)	(0.20)
Total IGR*Bargain	-0.08	-0.07	-0.07	-0.17	-0.13
	(0.27)	(0.27)	(0.27)	(0.26)	(0.28)
			Normalized Wages		
Total IGR	0.21	0.22	0.21	0.22*	0.22
	(0.13)	(0.13)	(0.22)	(0.13)	(0.14)
Total IGR*Bargain	0.56***	0.55***	0.55***	0.56***	0.56**
	(0.20)	(0.20)	(0.21)	(0.21)	(0.22)
			Normalized Employment		
Total IGR	0.41***	0.41***	0.31**	0.41***	0.42***
	(0.11)	(0.11)	(0.15)	(0.11)	(0.11)
Total IGR*Bargain	-0.29**	-0.29**	-0.29**	-0.32**	-0.27*
	(0.14)	(0.14)	(0.13)	(0.14)	(0.14)
Baseline Controls	X	X	X	X	X
Party of Governor		X	X		
Party of Governor*GRS			X		
City 3-Factor Formula				X	X
State 3-Factor Formula					X
Observations	14,378	14,378	14,378	14,378	14,378

Notes: Each column represents the results from a 2SLS IV regression in which total per capita intergovernmental revenues (IGR) and IGR interacted with the bargaining indicator (IGR*Bargaining) are instrumented by per capita general revenue sharing receipts (GRS) and GRS interacted with the bargaining indicator. The dependent variables in the top, middle, and bottom panels are, respectively, per capita expenditures, normalized wages, and normalized employment. Column (2) includes as a control an indicator for whether the city was residing in a state with a democratic governor. Data were received from Professor Jim Snyder. Column (2) includes the interaction of this indicator with GRS. Column (4) includes the product of city-level population, tax effort, and inverse per capita income. Column (5) includes, in addition, the product of state-level population, tax effort, and inverse per capita income. For more details on the specifications, see notes to Table 7.

* p<0.10, **p<0.5, ***p<0.01

Table 12: Macroeconomics Crosswalk

	(1)	(2)	(3)	(4)	(5)
	Emp_Norm	City Gov't Employees (1000)		BEA Gov't Employees	BEA Private and Gov't Employees
Scaled IGR	0.41***	10.83***	14.52***	9.87*	31.99*
	(0.11)	(2.87)	(5.28)	(5.53)	(17.98)
Scaled IGR*Bargain	-0.29**	-8.27***	-15.54***	-12.65**	-45.04**
	(0.14)	(3.18)	(4.66)	(5.47)	(22.25)
City-County Population Ratio Cutoff	0	0	0.5	0.5	0.5
Observations	14,379	14,379	3,705	3,705	3,705

Notes: Each column represents the results from a 2SLS IV regression in which total per capita intergovernmental revenues (IGR) and IGR interacted with the bargaining indicator (IGR*Bargaining) are instrumented by per capita general revenue sharing receipts (GRS) and GRS interacted with the bargaining indicator. The dependent variables in Columns (1)-(5) are, respectively, per capita normalized city government employment, city government employment per 1000 in the city population, city government employment per 1000 in the city population, total government employment per 1000 in the county population, and total employment per 1000 in the county population. Columns (3)-(5) limit the sample to cities that make up at least 50 percent of their counties. To scale appropriately in Columns (4) and (5), the per capita city intergovernmental transfers are scaled by the county area to city IGR ratio from the most recent government census year. This scaling is discussed and explained in the text. For more details on the specifications, see notes to Table 7.

* p<0.10, **p<0.5, ***p<0.01

Table 13: Macroeconomic Effects

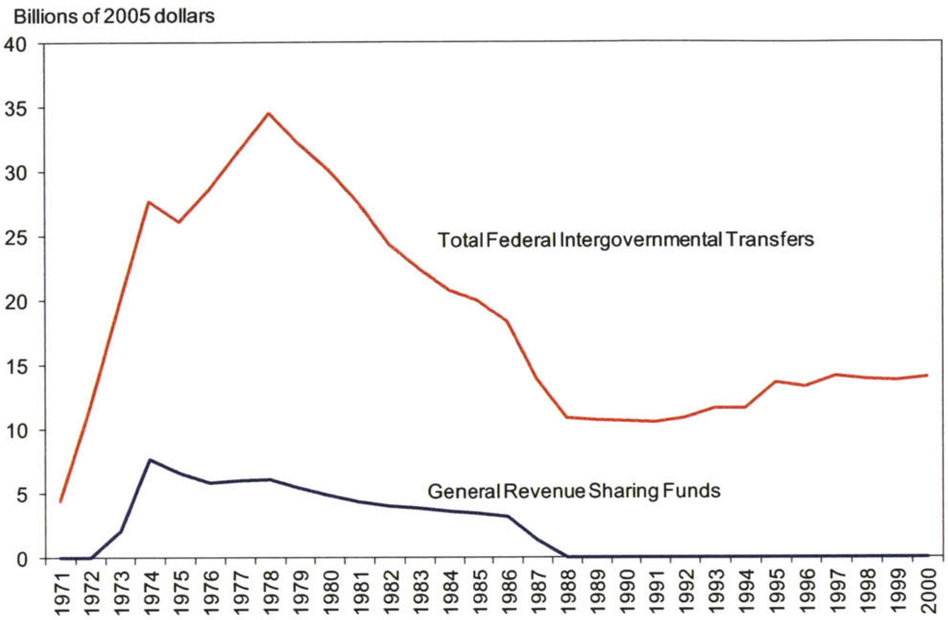
	(1)	(2)	(3)	(4)	(5)
	Total Employment (1000 per capita)				
Scaled IGR	31.99*	14.84	5.50	52.59*	33.76*
	(17.98)	(15.98)	(40.53)	(26.85)	(18.19)
Scaled IGR*Bargain	-45.04*	-29.62*	-15.79	-50.56**	23.51
	(22.25)	(17.47)	(25.12)	(19.70)	(30.03)
State*Time Trend	X	X	X		X
Union Interactions	X	X	X	X	X
Population Cutoff					No Top 5
City-County Population Ratio Cutoff	0.5	0.3	0.85	0.5	0.5
Observations	3,705	6,308	631	3,705	3,515

Notes: Each column represents the results from a 2SLS IV regression in which total per capita intergovernmental revenues (IGR) and IGR interacted with the bargaining indicator (IGR*Bargaining) are instrumented by per capita general revenue sharing receipts (GRS) and GRS interacted with the bargaining indicator. The dependent variable is total employment per 1000 in the county population. In Columns (1), (4), and (5), the sample is limited to cities that make up at least 50 percent of their counties, in Column (2), the sample is limited to cities that make up at least 30 percent of their counties, and in Column (3), the sample is limited to cities that make up at least 85 percent of their counties. In Column (4), state-time trends are not included, and in Column (5), the top 5 largest cities are dropped. For more details on the specifications, see notes to Table 7.

* p<0.10, **p<0.5, ***p<0.01

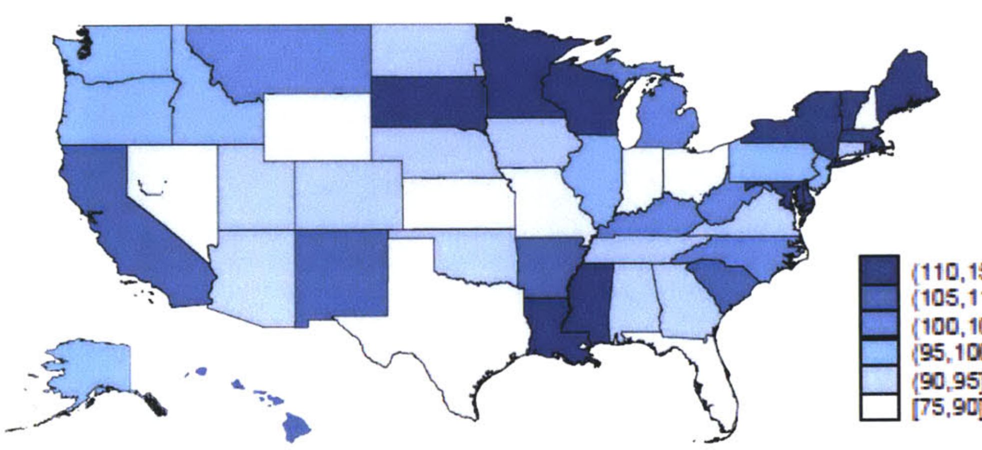
Figures

Figure 1: Federal IGR and General Revenue Sharing Funds



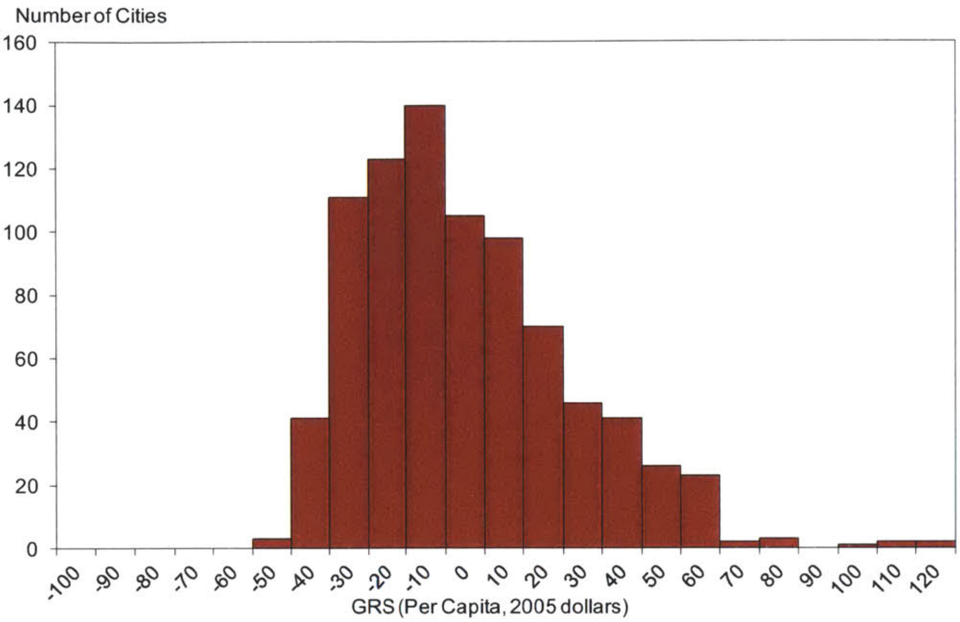
Notes: From the 1971-2000 Annual Surveys of Government Finance.

Figure 2: State Area Allocations



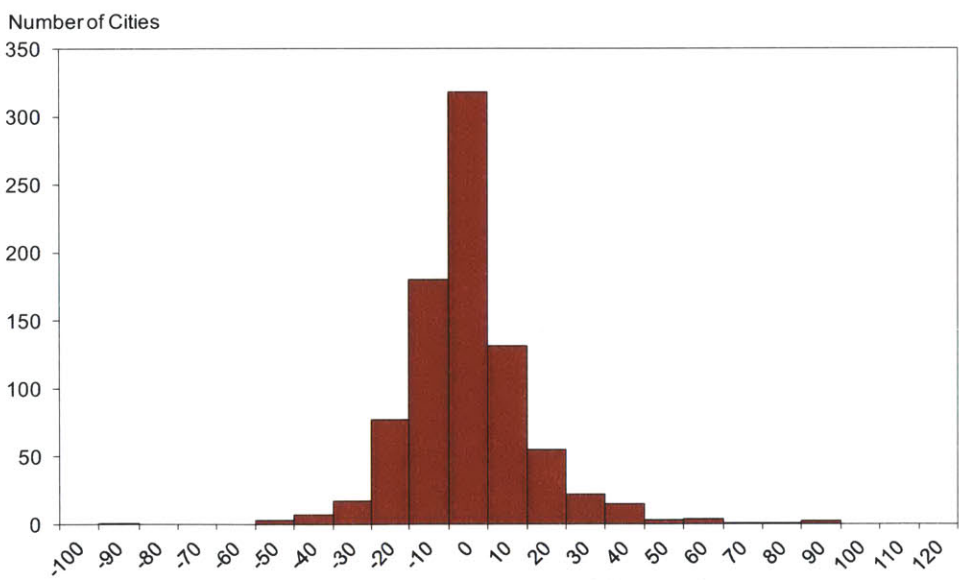
Notes: Data are from the 1977 Annual Survey of Governments. Amounts are per capita amounts in 2005 dollars.

Figure 3: 1977 General Revenue Sharing Transfers



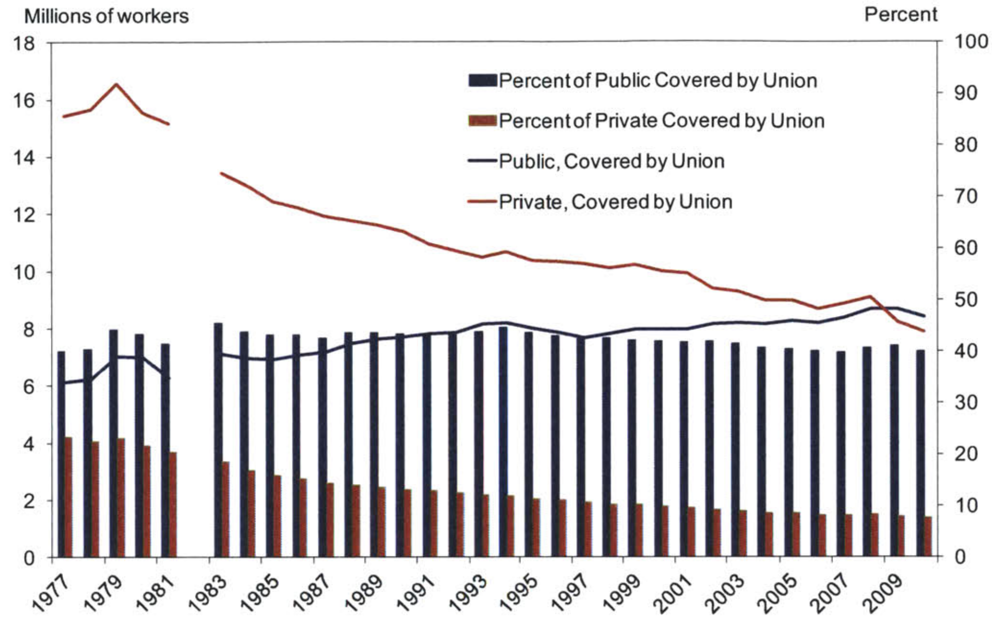
Notes: Data are from the 1977 Census of Governments, and are de-meaned.

Figure 4: 1977 General Revenue Sharing Variation



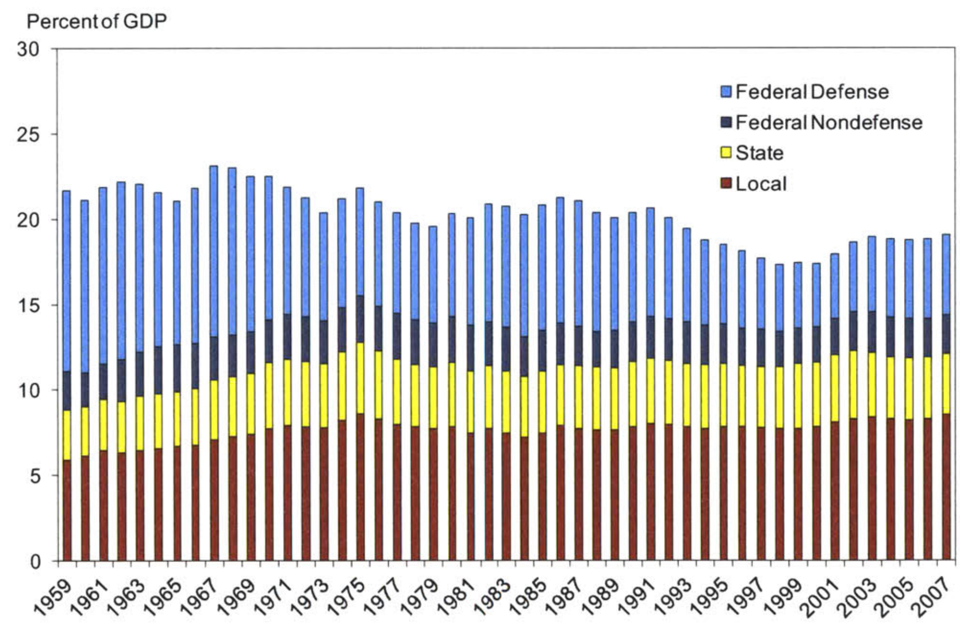
Notes: This histogram shows the residuals produced by the regression of per capita general revenue sharing against cubic polynomials of all of the allocation variables as described in the text.

Figure 5: Public and Private Union Coverage



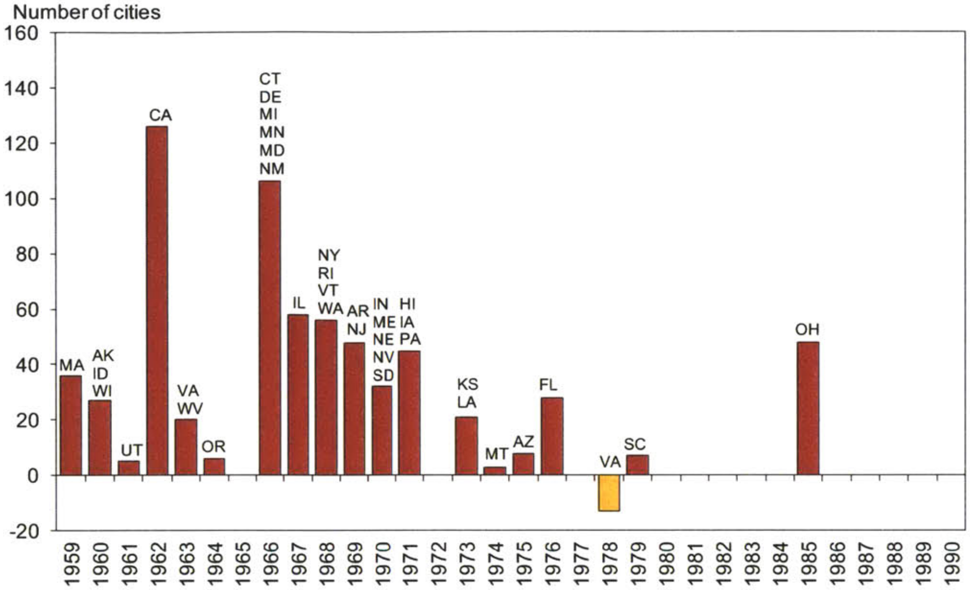
Notes: From Unionstats.com, Hirsch, Macpherson (2011).

Figure 6: Government Consumption and Investment



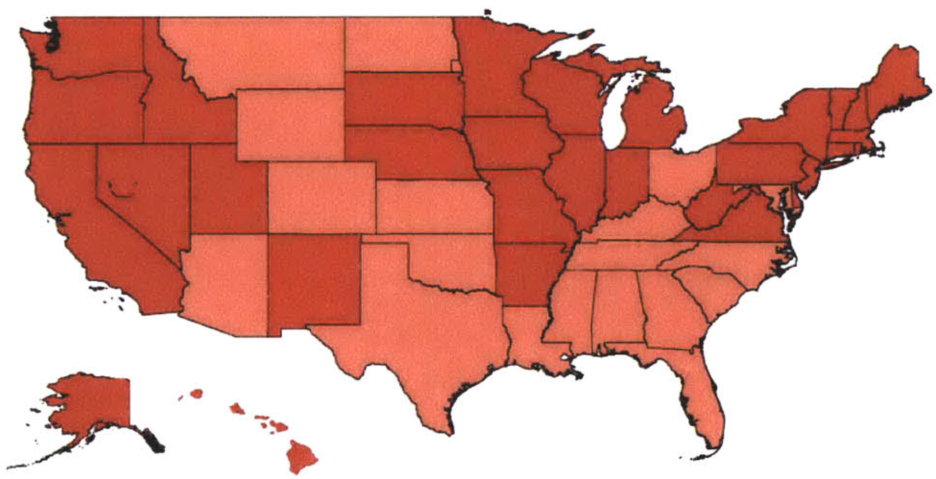
Notes: Bureau of Economic Analysis NIPA Tables 1.15, 3.20, and 3.21.

Figure 7: Timing of Bargaining Law Passage



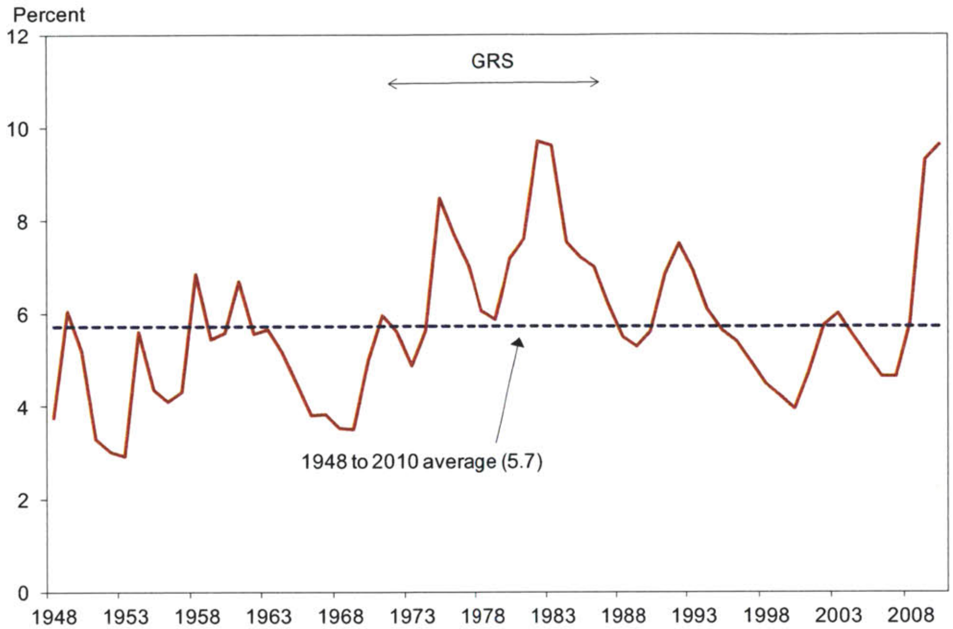
Notes: Bargaining legislation data from Freeman and Valletta, 1987. The year marked represents the first full year after which the law was changed.

Figure 8: 1972 Bargaining Laws



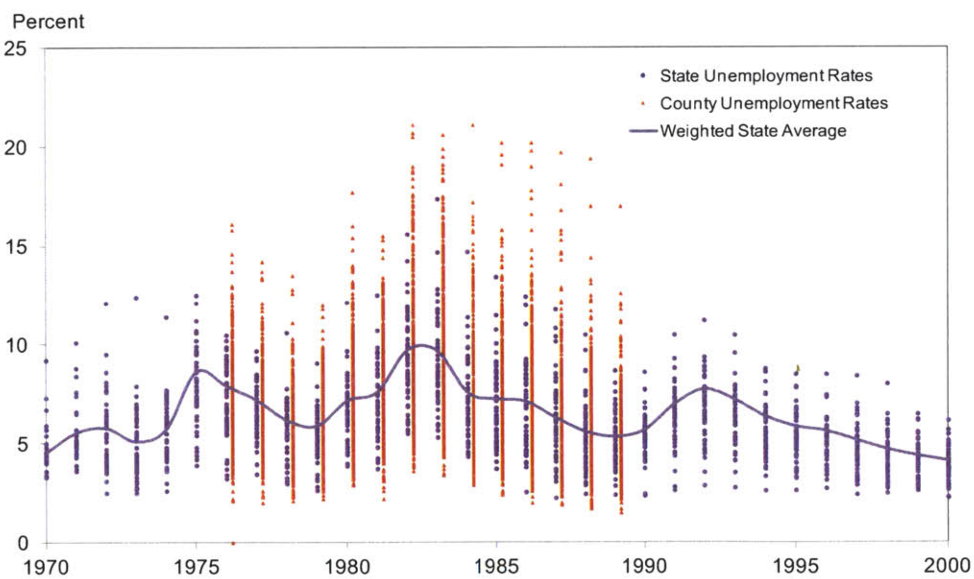
Notes: Dark red represents the existence of a pro-union collective bargaining law in 1977. Light red indicates either the absence of such a law, or the existence of a law specifically prohibiting collective bargaining.

Figure 9: National Unemployment Rate



Source: Bureau of Labor Statistics.

Figure 10: Unemployment Rates



Note: This graph shows a scatterplot of the unemployment rates of the states (1970 to 2000) and the counties (1977 to 1989) in which the cities in the sample studied reside. The solid line shows the population-weighted average of the state unemployment rates across all cities in the sample. The state data for 1970 and 1971 are from the BLS and are only available for 27 of the 50 states. The state data from 1972 to 1976 are from Wayne Vroman. Post-1976 county and state data were received upon special request from the BLS.

Chapter 2

City Government Revenues and Business Cycles

2.1 Introduction

Hartford must cut expenses big and small.[1] *Pittsburgh Public Schools sends 285 layoff notices.*[2] *Montebello may face insolvency if it doesn't close budget deficit.*[3] In the recent recession, headlines such as these have become commonplace in local newspapers around the country. In response to revenue shortfalls, local governments have reduced local services and cut over 500,000 jobs since the employment peak in August 2008.[4] The broader implications of these local budget cuts are not well understood, although it has become increasingly clear that the impacts on the economy are likely to be large. The magnitude of the cuts warrants a close examination of the source of the local government revenue fluctuations and their effects on expenditures in terms of scale and scope.

Due to a large variation across local governments in their revenue structure, the elasticity of total revenues with respect to the business cycle varies substantially. For instance, some cities rely heavily on property taxes which are relatively stable through booms and busts, whereas other cities rely more on the volatile sales tax. In this paper, I document the extent to which

[1] Hartford Courant, February 27, 2011.
[2] Chute, Eleanor. The Pittsburgh Post-Gazette. May 31, 2012.
[3] Garrison, Jessica and Hector Becerra, Los Angeles Times, April 23, 2011.
[4] Figures from the BLS Current Employment Statistics Survey.

revenue structure impacts the revenue responses across city governments in downturns. I then exploit the differences in revenue structure to explore the effects that "revenue shocks" have on expenditures. I find that city governments cut expenditures about one for one in response to a revenue shock, suggesting that the balanced budget rules are binding. Furthermore, I find that these fluctuations disproportionately affect capital expenditures and, specifically, spending on transit (including highways) and hospitals. These results suggest that the structure of revenue can have important effects on local revenue and spending dynamics, which, in turn, raises questions on the variation in infrastructure spending over the business cycle, on the nature of programs that are cut when county income declines, and on the long-term impacts of cyclical government investment disruptions.

The short-term impacts on the private economy of an expenditure cut are captured by the size of an associated "fiscal multiplier". The spending shocks generated by the variation in the revenue structure are "balanced budget" shocks, as distinct from deficit-financed expenditure shocks, which are those typically studied in the literature on fiscal stimulus. Therefore, the estimates of fiscal multipliers that already exist are not typically appropriate to apply to the shocks to spending experienced by state and local governments in the recent recession. Instead, I propose that the revenue structure can be further exploited to estimate the multipliers associated with the balanced budget shocks.

To study these issues, I use a panel data set of city government finance data from 1972 to 2006. By combining these finance data with county-level personal income data, I am able to estimate the elasticity of each large revenue component (property taxes, sales taxes, individual income taxes, state intergovernmental revenues, federal intergovernmental revenues, and charges and fees) with respect to personal income. Armed with these elasticity estimates along with the 1972 revenue structure for each city in the sample (i.e. the fraction of total revenue raised from each component), I create a variable that captures the responsiveness of an individual city's revenue stream to the business cycle *that is due only to the differences in their revenue decomposition.* I argue that this variable can be used to instrument for revenues to determine how revenues affect expenditures, overcoming the econometric problem that revenues and expenditures are jointly determined.

The paper is organized as follows: The second section discusses balanced budget expenditure shocks. The third section lays out the revenue structure of city governments and then focuses on measuring the elasticity of each of the revenue components with respect to changes in personal income. In the fourth section, I use the elasticity estimates to explore the impact of revenue structure on revenues and expenditures. In the fifth section, I propose a method to estimate balanced budget multipliers. In the sixth section, I discuss caveats and concerns, and, in the final section, I conclude.

2.2 Balanced Budget Shocks

A recent surge in the public finance and macroeconomics literature has produced a number of estimates of local fiscal multipliers using regional data in the United States. Each "experiment" has measured slightly different forms of the multipliers. For example, Chodorow-Reich (2012), Wilson (2011), and Feiveson (2012) use the variation in federal transfers to state or local governments to estimate the impact of federal intergovernmental grants on local employment. Shoag (2011) uses state pension "windfalls" to instrument for increases in state spending and Nakamura-Steinsson (2011) exploit differences in direct federal spending across states, both estimating what Nakamura and Steinsson dub an "open economy relative multiplier". What these estimates have in common is that the "shock" to the local economy is not financed by the local economy itself.[5] The estimates of the papers mentioned are designed to shed light on the standard deficit-financed central government fiscal multiplier.

An equally important, but understudied, multiplier is the balanced budget multiplier, which measures the change in output or employment associated with an expenditure shock that is attached to a revenue shock of equal magnitude. This multiplier is important for two reasons. First, as the size of the deficit has become a polarizing issue, there is a political interest in finding a way to stimulate the economy without increasing the deficit. Second, because state and local governments almost all face balanced budget rules, their expenditures are constrained, to some extent, to move with revenues. Therefore, learning about the effects of a balanced

[5]To be more precise, in all of these studies, the expenditure or transfer "shocks" are financed by taxpayers or borrowers across the country, some of which could be in the local economy that experiences the shock. However, the studies are designed so that the variation in expenditures or transfers is independent of any regional variation in financing. Thus, the relative multiplier measured will reflect the variation on the expenditure/transfer side, and not the financing side.

budget shock would help to understand the private sector implications of the balanced budget contractions experienced by state and local governments throughout the recent recession.

In this paper, I am concerned with the second motivation, although the results may shed light onto the first. When a local government loses revenue, balanced budget rules require that they cut expenditures, increase taxes, or do a combination of the two. Any cut to expenditures will, therefore, be roughly matched by an equivalent loss of revenue. Generally, taxes that fall during a recession are a form of automatic stabilizer in that they leave funds in the hands of taxpayers at a time when they need it most. However, the concurrent contraction of expenditures will also have a contractionary effect by directly reducing total output and by reducing the incomes of the recipients of the government spending.

In the early Keynesian framework, the "balanced-budget multiplier theorem" suggests that the multiplier associated with a balanced budget shock should be exactly one; i.e. for every dollar of expenditures and revenues cut, one might expect that total output in the economy will contract by exactly one dollar. This occurs because, under the simple assumptions of the theory, while total pre-tax income falls by the equivalent amount of the expenditure decline, the taxes fall by exactly the same amount. Therefore, consumer behavior stays constant at the same time that the government output declines. However, in a more complex world, the multiplier could be greater or smaller than one. If the consumers who bear the income brunt of the government expenditure cuts have a higher marginal propensity to consume than the ones whose taxes are reduced, the multiplier may exceed one. On the other hand, the multiplier may be less than one if government expenditures crowd out private consumption or investment through price changes, or if consumers expect that the government expenditure changes will persist and adjust their behavior accordingly.

In this paper, I use local government data to examine the size of the balanced budget shocks produced by the variation in revenue structure; i.e. for a shock to revenue, how much do expenditures actually respond? Because the balanced budget multiplier should be expected to vary by the type of expenditure, I also explore the response of individual expenditure components. Finally, I propose that the variation in the revenue structure may be exploited to actually measure the size of the balanced budget multiplier.

2.3 City Revenue Components and their Elasticities

2.3.1 The Revenue Structure of Local Governments

My analysis focuses on the cities that contained more than 25,000 people in 1972. These 837 cities are included in the Annual Survey of Governments annually, almost without exception. The largest sources of revenue for these cities are property taxes, charges and fees, utility fees, and state intergovernmental transfers. The rest of revenues are composed of sales taxes, federal intergovernmental transfers, and, in some cases, individual income taxes. As shown in Table 1, there is substantial variation in the revenue structure across the sample.

Table 2 shows the 20 largest cities in 1972 and their respective revenue structures. As can be seen in the table, the dependence on property taxes varies from 10 percent in Memphis to 54 percent in Boston; 17 percent of New Orleans revenues come from sales tax at the same time that many cities do not have sales taxes at all; and the dependence on state intergovernmental transfers varies from 0 percent to 48 percent. The revenue structure varies quite dramatically even in the largest cities.

The large variation across city governments in their revenue structure leads to the obvious question: what is driving these different structures across city governments? For one, city government revenue structure is highly affected by state-level laws which regulate what taxes city level governments have the authority to levy. Figure 1 shows the taxes that each state authorizes their local governments to levy. Twenty-five states do not allow their local governments to levy sales tax at all. Although the average reliance on sales tax shown in Table 1 is only 7 percent, the average reliance on sales tax in cities in states that allow sales tax to be levied at the municipal level is 15 percent, making clear that the cities in the states that allow municipal sales tax do make use of that tax source.

The other state level laws that can affect the municipal level tax structure are the state-level tax and expenditure limitations (TELs), which often put a limit on the revenue growth associated with property taxes. Figure 2 shows the geographic distribution of TEL laws (in 2006). The states in red are states that have a "binding" property tax limit and a general limit for expenditure increases. In the TEL literature, a "binding" property tax limit means that there are limits on the property tax growth or that there is a combination of a property tax rate limit along with an assessment limit, which together mean that a local government cannot circumvent the law to increase property taxes. Many of these laws were passed post-1972, and therefore could not have affected the revenue structure at the beginning of my sample. However, a few of the states did have early passage of the laws.[6] For example, in Colorado, who passed a property tax levy growth limit in 1913, the average reliance on property taxes in 1972 is 7 percent, while the sample mean is 20 percent. Because the Colorado governments cannot rely on property taxes, they had to find other sources of revenue; in the same cities, the fraction of the revenue that comes from charges and fees is 31 percent, while the overall average is 25 percent.[7]

Beyond these state-level laws, there remains significant variation in the reliance on different taxes at the local government level as well as the discretion the cities are able to use in setting rates. For instance, when in some states, such as California, the sales tax is automatically split between the state government and the municipal governments, the city governments in other states have the authority to choose the sales tax rate.[8] Furthermore, there is quite a bit of discretion in the user fees set on recreation, utility services, public works, planning and economic development, health, and transportation. The income tax rates of large cities are chosen by the city or in negotiation between the city and the state. All of this added variation is decided at the local government level and reflects the preferences of voters, the preferences of the city legislators, and the laws that were already in place.

Ideally, a revenue structure will produce a revenue stream that will both grow with income and population as well as having the property of being relatively stable over time. As Groves and Kahn (1952) first discussed, these two goals can often be at odds; taxes that grow long-term with income may also be more responsive to short term fluctuations that occur with the business cycle. A progressive income tax is a great example of such a tax; as incomes rise in a given population, the per person revenues will also rise. However, when income takes a hit in a recession, the income tax revenues will also fall especially since the incomes at the

[6]Arizona, Colorado, Oregon, Utah, Kansas, Iowa, Minnesota, and Washington all passed TEL laws prior to 1972.

[7]Because both the laws and the reliance on property taxes reflect voter preferences, one needs to be careful in assuming that the law causes the lower reliance on property taxes. Previous estimates using instruments for the TEL laws confirmed that the laws resulted in a lower reliance on property taxes and a higher reliance on charges and fees. See Shadbegian (1999).

[8]see Bland (2005).

top end of the distribution are often particularly affected during a recession.[9] The common linkage between short-term and long-term elasticities in revenue sources is an example of the fact that lawmakers (and citizens) have multiple dimensions over which to "optimize" their revenue structure, many of which may conflict with one another. Since state laws and preferences over different revenue sources vary in many more dimensions than just their short-run elasticities, the revenue structures are generally not designed to minimize their responsiveness to business cycles. The variation in the revenue structure yields a large variation in the elasticities of revenues with respect to the state of the local economy. In the next subsection, I explore the differences in the short-term elasticities of the various revenue sources.

2.3.2 Elasticity Estimates

My goal is to understand how the business cycle affects the percent change in each revenue component. I approximate "business cycles" by fluctuations in personal income in the county in which the city resides.[10] Because counties and cities may have very different levels and growth rates of revenues and personal income, I include both city fixed effects and a city-specific time trend in the estimation equations.[11] The regression that I run is:

$$\ln R_{it}^{j} = \beta_j \ln I_{it} + \eta_i t + \varphi_i + \varepsilon_{it} \tag{2.1}$$

where R_{it}^{j} is the real amount of funds from revenue source j in city i and time t, and I_{it} is real personal income in the county containing city i in time t.[12] The β_j estimates represent the elasticity of the revenue component j with respect to personal income. For every one percent that personal income is above its trend, the revenue component will be above trend by β_j percent. Table 3 shows the results of this exercise.

I find that all revenue components other than federal intergovernmental transfers are highly

[9] The trade-off may not always be in effect. In particular, Sobel and Holcombe (1996) explore the fact that the short-run and long-run elasticities are not necessarily linked.

[10] My data sources are detailed in the data appendix.

[11] The city-specific time trend allows for the time trends to vary in the components included. (For instance, personal income and revenues are allowed to grow at different rates with this specification.)

[12] Note that I only have data from BEA at the county-level, which is why I use county level income. Finance variables are deflated using the state and local GDP deflator, and personal income is deflated using the GDP deflator.

cyclical. However, property taxes, utility revenues, and the "other" category respond to changes in personal income less than sales taxes, income taxes, state intergovernmental transfers, and charges and fees. For every one percent that personal income is above trend, property taxes will increase by 0.53 percent whereas sales taxes will increase by 0.78 percent. This difference is substantial; if a ten percent income shock hit two cities, one of which depended only on property taxes and the other of which depended only on sales tax, the difference in the revenue shocks to the two cities would be 2.5 percent of total revenue. Charges and fees are the most responsive revenue component, probably because the demand for costly local services such as parking facilities, air transportation, and parks and recreation is procyclical. There is a surprising amount of variation in the amount that local governments depend on charges and fees (see Table 1), and this ends up playing a large role in determining the differences in the elasticity of total revenues with respect to personal income.

It is important to note that the intergovernmental transfers deserve special attention when considering their elasticities with respect to changes in personal income. While the other sources of revenue are affected either directly by changes in personal income (like the income tax) or by the resulting changes to individual behavior (like the sales tax or the charges and fees), the intergovernmental transfers are decided by state legislatures. While the individual responses to losses in personal income are likely to be consistent across the country, the response of legislatures need not be; some states may tend to decrease grants during downturns, while others might increase grants during downturns. Indeed, when I estimate the per-state state intergovernmental grant elasticities, I get a large variation across states in the measurement, with some not responsive at all, and some very responsive to the business cycle. Figure 3 shows a bar chart of the elasticity estimates for state intergovernmental transfers for the 26 states that contain at least 10 cities in my sample.[13] The state elasticities do vary substantially across state governments (although the standard errors of the estimates are quite large due to small sample size for each state). In Section 4.1, I will discuss what this variation means for my empirical strategy.

The assumption that elasticities are constant over time may be misguided. In particular,

[13]For each state, I estimated the equation: $\ln IGR_{it} = \beta^s_{IGR} \ln I_{it} + \eta_{it} + \phi_i + \varepsilon_{it}$, where IGR_{it} represents the state intergovernmental transfers for city i within the state. I used the data from all of the cities in my sample within the state, and weighted by population.

one possibility, explored in Bruce et al (2006), is that is that the revenue components may respond in an asymmetric way to movements in the business cycle leading to different elasticity measurements in "booms" and "downturns". In Table 4, I explore this possibility by interacting the income variable with a dummy for whether there is a local downturn.[14] The regression is:

$$\ln R_{it}^{j} = \beta_{j}^{boom} \ln I_{it} * (1 - I(downturn)) + \beta_{j}^{downturn} \ln I_{it} * I(downturn) + \eta_{i}t + \varphi_{i} + \varepsilon_{it} \quad (2.2)$$

In property taxes, sales taxes, income taxes, and state intergovernmental transfers, there is no obvious asymmetry in the elasticity measurements. The main components that are substantially different over the business cycle are charges and fees and utilities, which are significantly more responsive to income during booms. These findings are consistent with the existence of luxury government services and utilities that exhibit a high income elasticity of demand, but are only demanded in "good" times.

I also explore whether the elasticities have changed over the time period that my sample covers. In particular, I compare the elasticity coefficient in the pre-1990 years to the one estimated post-1990. I find that property taxes have gotten significantly less responsive, while sales taxes have become significantly more responsive over time.[15] The property tax result is consistent with the gradual adoption of TEL laws; with these laws, property taxes are limited in their ability to respond positively to increases in income. The sales tax result may reflect the greater proportion of luxury goods in the tax base spurred by the overall increase in real income per capita.

The results of these exercises show that the elasticity estimates shown in Table 3 are average estimates; actual elasticities may vary from city to city and over time. While keeping this fact in mind, I purposefully will be using the average elasticity estimates later in the analysis to construct a "total elasticity" measure that is designed to be independent of city and time variables.

[14]Measured by whether the ln(real income) is below a linear trend.

[15]The property tax estimate in the pre-1990 and post-1990 periods are 0.59 (0.06) and 0.38 (0.07), respectively. The sales tax estimates are 0.63 (0.10) and 1.12 (0.13). Standard deviations are in parentheses.

2.3.3 Comparison to Previous Empirical Work

Most of the previous literature on tax elasticities uses state level rather than local level data. While my use of city government data provides a unique window into the responsiveness of revenues at that level of government, my estimates necessarily lack some of the nuance of the estimates possible at a more macro level. In particular, because of data limitations at the city level, I am unable to estimate the effect of changes in personal income or sales tax *bases* rather than total tax revenues. This means that my estimates will incorporate any legislated changes to the tax rates that occurred concurrently with the change in personal income. In other words, if city governments tend to respond to a drop in revenues by increasing tax rates, the elasticity estimates of the changeable components (such as property and sales taxes) might be underestimates of the true elasticities.

One way to get a sense of the size of this problem is to compare my estimates to the existing estimates in the literature. In one of the earliest estimates of short-run tax elasticities of state government revenue components, Williams et al (1973) find that sales taxes had an income elasticity of 0.81, while income tax had a higher elasticity of 1.08. While, for many years, this empirical study fixed expectations about the relative elasticities of sales versus income taxes, a later study on tax bases by Dye and McGuire (1991), found that a sales tax might actually be as or more volatile than an income tax. Holcombe and Sobel (1996) confirm this finding using national proxies for state tax bases, and estimate short-term tax base elasticities around 1 for both personal taxable income and retail sales. More recently, Bruce et al (2006) measured state-specific tax elasticities for income and sales taxes and found that the relative elasticities largely depended on the specific tax structures within the state government. All in all, my finding that local government sales and income taxes have roughly the same elasticities appear to be consistent with the literature on state government taxes, although their levels are slightly lower than previous estimates.

The literature on property taxes has generally found that they are a stable source of revenue (Giertz, 2006). In an analysis that measured the elasticity of property tax with respect to house prices, Lutz (2008) finds that only 40 percent of the fluctuations in house prices are reflected in changes to property tax revenue and, even that, with a three year lag. Lutz et al (2011) found that in the recent recession when house prices declined dramatically, property taxes nevertheless remained relatively stable. My estimate of 0.53 for the income elasticity of property taxes seems to be slightly larger than the estimates in the literature would suggest. However, because I measure the *income* elasticity of property taxes rather than the housing elasticity, these estimates are not directly comparable. Certainly, my finding that the property tax income elasticities fall below those of the sales and income taxes is consistent with the literature.

Most of the studies on the cyclicality of intergovernmental transfers examine federal intergovernmental transfers to state and local governments rather than state intergovernmental transfers to local governments. Rodden and Wibbels (2010) find that federal grant policy in the U.S. tends to be acyclical, which is consistent with my income elasticity estimate for federal transfers. My finding that state intergovernmental transfers are highly procyclical is more controversial in the literature. Hines (2010), for cxample, actually finds that state expenditures are mildly countercyclical. However, as shown in Figure 1, my estimate is limited to be the average across states; I find that there is significant variation across states in how they handle their grant policy in recessions and booms.

2.4 The Effect of Revenues on Expenditures

As discussed in the last section, the factors that determined the revenue structure at the local government level are multifold. It is certainly not the case that local governments chose based solely on minimizing the amount that their revenues respond to the business cycle even within the constraints of the state laws. Nevertheless, the elasticities of each revenue component vary substantially (as seen in Table 3), amounting to the fact that there must be a large variation in the elasticity of total revenues with respect to personal income due to the variation in the revenue structure.

My goal is to understand how expenditures respond to the revenue "shocks" generated by the variation in revenue structure. The equation that I want to estimate is:

$$\ln E_{it} = \beta \ln R_{it-1} + \alpha X_{it} + \eta_i t + \varphi_i + \kappa_t + \mu_{it} \quad (2.3)$$

where E_{it} is the real expenditures in city i and time t, R_{it-1} are real revenues in city i and time $t-1$, and X_{it} are city-level control variables that vary over time. I lag revenues by one period in order to allow time for a legislated expenditure response.[16] The econometric problem in estimating this equation is that expenditures and revenues are jointly determined. Demand for government services, which may correlate with fluctuations in the business cycle (and revenue), will have an independent impact on expenditures. Furthermore, legislatures will change revenues and expenditures together for philosophical rather than economic reasons.

To overcome these econometric issues, I turn to the revenue structure to create a variable that can instrument for $\ln R_{it-1}$ in the above regression. In the next subsection, I will describe the construction of the variable in which I rely only on the revenue structure of the city governments (i.e. the fraction of total revenues attributed to each revenue component). Furthermore, I restrict to using the revenue structure from the first year of the sample, 1972, in order to avoid the problem that revenue structure itself is somewhat related to the state of the business cycle: i.e. those parts of the revenue that are highly elastic will disproportionately shrink in bad times.

2.4.1 A "Total Elasticity" Measure

In this section, I use the elasticity measurements that I found above to construct a "total elasticity" measurement for each city.

Consider that total revenues are the sum of all the revenue components.

$$R_{it} = \sum_j R_{it}^j \tag{2.4}$$

The natural logarithm of total revenues is therefore the weighted sum of the natural logarithms of the components, where the sum is their share of total revenues. In equation form:

$$\ln R_{it} = \sum_j s_{it}^j \ln R_{it}^j \tag{2.5}$$

where s_{it}^j is the share that revenue source j makes up of total revenue in city i in time t. In

[16]Furthermore, estimates in (Holtz-Eakin, 1989) find that expenditures respond to lagged revenue rather than concurrent revenue.

the previous section, we found that the natural logarithm of each revenue source is related to the natural logarithm of personal income by the estimated elasticities. Thus, an estimate for the growth rate of total revenue using the average estimated elasticities, ε^j, is:

$$\ln R_{it} = \left(\sum_j s_{it}^j \varepsilon^j \right) \ln I_{it} \tag{2.6}$$

Intuitively, the elasticity of total revenue with respect to personal income is the weighted sum of the elasticities of the revenue sources. It is the variation of the term $\epsilon_{it} = \sum_j s_{it}^j \varepsilon^j$ across cities that causes variation in the amount that their revenues respond to income shocks. It is important to note, however, that the ϵ_{it} terms are not exogenous to the state of the business cycle since the shares of the revenue components, s_{it}^j, will respond to fluctuations in the business cycle. In order to capture the component of the variation that is not due to changes in the business cycle, I use the elasticity term from 1972 in all of the analysis. This is possible because ϵ_{it} is persistent across time; the basic structure of revenues is not easy for a city legislature to change.[17]

By using the average elasticity estimates, ε^j, rather than city-specific elasticity estimates, I avoid the issue of that city-specific elasticities are endogenous to the cities' individual responses to revenue shocks. In particular, we saw that the elasticities of state intergovernmental revenues vary from state to state. I use the average elasticity in constructing the total elasticity measure. Of course, this sacrifices some accuracy in the extent to which Equation (6) holds, but is needed to skirt endogeneity problems. As long is the total elasticity variable still has predictive power for revenues (which I will show it does), I am able to use it to complete the analysis.

Figure 4 shows a histogram of the distribution of ϵ_{i1972} for the cities in my sample. There is a significant amount of variation; the total elasticity measure varies from 18 percent to 98 percent. What does this variation mean? Consider City A with an elasticity of 60 percent, and City B with an elasticity of 80 percent. If both of these cities are hit with a negative income shock that leaves income 10 percent below trend, City A's revenues will decrease by 6 percent and City B's revenues will decrease by 8 percent; a 2 percent difference. When we consider that many of the cities were in counties that experienced personal income shocks of

[17]The correlation coefficient of ϵ_{i1972} and ϵ_{i2002} is 0.32 with a p-value less than 0.01%.

10 percent or greater throughout this period, this example demonstrates how the difference in elasticities can cause a substantial difference in the revenue shock faced by the city government. The scatterplot in Figure 5 plots the deviations of personal income from trend against the same variable multiplied by the total elasticity variable, ϵ_{i1972}. As one can see from the scatterplot, for the same drop in personal income, the difference in the revenue response caused by the elasticity differences can be as large as 5 percent of revenues (i.e. the thickness of the "line" in the figure is roughly 0.05).

2.4.2 Revenue Shocks

The total elasticity variable is only useful to the extent that it actually predicts changes in revenue. The reason that it may not is that it is possible that the elasticities of the tax rates depend on how much a city government relies on the tax source. For example, suppose that sales taxes become more elastic as the tax base broadens. Then cities that have a high reliance on sales taxes will actually have sales tax revenues that are more responsive to fluctuations in personal income than the elasticity variable would predict.

In order to check the predictive power of the constructed elasticity variable, I regress $\ln R_{it}$ against the $\epsilon_{i1972} \ln I_{it}$, which I will from now on refer to as the "elasticity-income measure". The baseline regression is:

$$\ln R_{it} = \alpha + \beta\epsilon_{i1972} \ln I_{it} + \eta_1 \ln I_{it} + \eta_2(\ln I_{it})^2 + \eta_i t + \varphi_i + \kappa_t + \mu_{it} \tag{2.7}$$

As before, I include city and city-specific time trends. In the baseline, I also include year-fixed effects. It is important to note that in the above regression, the inclusion of a flexible polynomial of $\ln I_{it}$ is essential. My goal is to use the differential revenue structures as an "exogenous" reason that there are different government revenue shocks in cities that *face similar personal income shocks.* My claim is that $\epsilon_{i1972} \ln I_{it}$ is plausibly exogenous only after controlling for $\ln I_{it}$. Table 5 shows the results of this regression.

In the first column of Table 5, I show the results from running the baseline regression in Equation (7) without year fixed effects. I show the baseline regression in Column (2), and in Column (3) I interact the elasticity-income measure with an indicator for whether there is a downturn in time t, as measured by whether real personal income is below trend.[18] I find that the elasticity-income measure is a good predictor of the percent deviation of total revenues from trend; the coefficient in the baseline regression with year fixed effects (Column (2)) is 1.17, with a standard deviation of 0.34 using robust standard errors. This implies that for a one percent increase in the elasticity-income measure, revenues increase by 1.17 percent. The expected coefficient of 1 (see Equation (6)) is well within any reasonable confidence bounds of the coefficient estimation. In Column (3), I interact the elasticity-income measure with an indicator for whether there is a downturn, measured by whether real personal income is below trend. I find that there may be an asymmetry as to how much revenues respond to the elasticity-income measure. During downturns, the coefficient is 0.26 below the coefficient during upturns, although the difference is only significant at the 10 percent level. The difference may derive from the fact that thc highest elasticity revenue component, charges and fees, tends to have a lower elasticity during downturns (see Table 3).

To get a sense of how large these revenue shocks can be, consider Boise City, ID which has a low total elasticity variable of 49 percent due to a high reliance on property taxes, and Spokane City, WA which has a total elasticity variable of 73 percent, due to a higher reliance on sales taxes and state intergovernmental transfers. Both cities suffered income shocks of roughly 10 percent of income in the 1980s. Given a 10 percent income shock, had the two cities swapped revenue structures, Boise's revenue shock would have been 2.4 percent greater than it was and Spokane's would have been 2.4 percent less than it was. In both cities, this amount corresponds to roughly 20 to 30 (2005) dollars per capita and 3,000 dollars per government employee.[19] This is certainly a large enough difference to have a substantial effect on government budgets.

2.4.3 The Effect on Expenditures

Now armed with an instrument for total revenues, I can turn to the question of how revenues affect expenditures. I want to understand how revenue shocks affect expenditure decisions in the following year, and the estimation equation is:

[18] The "trend" is defined as a trend line fitted to $\ln I_{it}$ for each city.

[19] These are 2005 dollars and are calculated by using the revenue, population, and employment figures from 1985.

$$\ln E_{it} = \beta \ln R_{it-1} + \eta_1 \ln I_{it-1} + \eta_2 (\ln I_{it-1})^2 + \eta_i t + \varphi_i + \kappa_t + \mu_{it} \tag{2.8}$$

As detailed above, I will use $\epsilon_{i1972} \ln I_{it-1}$ as an instrument for $\ln R_{it-1}$. The validity of the instrument relies on including $\ln I_{it-1}$ and $(\ln I_{it-1})^2$ as controls. The previous subsection was devoted to exploring the first-stage of the IV regression, and in this section, I present the second-stage results.

In Table 6, I show the results of running both the OLS regressions and the IV regressions. Column (1) does not include year fixed effects, Column (2) represents the baseline regressions with year fixed effects, and Column (3) includes the log of total revenues interacted with an indicator for whether the time period is a downturn. Columns (4)-(6) mirror the specifications of the OLS regressions, but represent IV regressions in which $\epsilon_{i1972} \ln I_{it-1}$ instruments for $\ln R_{it-1}$ in Columns (4) and (5), and in which there is an additional instrument of $\epsilon_{i1972} \ln I_{it-1} * I(downturn)$ and an additional endogenous regressor of $\ln R_{it-1} * I(downturn)$ in Column (6).

The OLS baseline regression produces a coefficient of 0.58, i.e. for a one percent increase of revenues, expenditures increase by 0.58 percent. However, as seen in the baseline IV regression in Column (5), a one percent increase in revenues produces a 1.14 percent increase in expenditures in the following period. The OLS coefficient does appear to be biased downward. There are two reasons that could contribute to the bias. First, in the time series dimension, there may be an omitted variable of people's demand for government services. If demand rises in economic slumps as revenues fall, the β coefficient would be biased downward. Second, in the cross-sectional dimension, the existence of both "profligate" legislatures (that tend to increase spending and decrease taxes at the same time), and "thrifty" legislatures (that do the opposite) could also produce a downwards bias in β. The results from the IV regression suggest that the balanced budget rules appear to be binding. Certainly, the regressions in Table 6 show that the revenue structure not only has a large impact on revenues, but also a large impact on expenditures.

In Table 7, I explore the impact of a revenue shock on the components of expenditures. In Columns (2)-(5), I show the response of the basic components of expenditures:

$$
\begin{aligned}
Expenditures &= CapitalOutlays + CurrentOperations \\
&+ InsuranceTrustFunds + Other
\end{aligned} \tag{2.9}
$$

where *Other* includes intergovernmental transfers, interest payments, and total assistance and subsidies. In Column (6), I show the impact of a revenue shock on salaries and wages which may be distributed throughout the other components (but are mostly concentrated in current operations). The bottom rows of the table show the percent that each of these components contributed to expenditures in both 1972 and 2002. Current operations made up the largest part of expenditures at a little under seventy percent in both years shown. Capital outlays made up 18 percent of expenditures in 1972, and 16.5 percent in 2002. Salaries and wages made up between 30-45 percent of expenditures, depending on the year.

The coefficients shown in Table 7 are the results of IV regressions of the expenditure components against the logarithm of revenues as in Equation (8). To interpret the coefficients, I consider that if a revenue shock were spread across all expenditures in a proportional way, the coefficient should be the same across components. A coefficient higher than 1.14 signals that that the component is extra affected by the change to revenue, and a coefficient less than 1.14 signals that the component is less affected than average. Capital outlays, contributions to insurance trust funds, and the "other" category are the components that are the most (relatively) affected by the shifts to revenue. Current operations are the least relatively affected. While the standard errors are large, the hypothesis that all of the coefficients are the same can be rejected at the five percent level.

I further probe the impact of revenues on expenditures by looking at expenditure functions in Table 8, in which I split expenditures into ten functions: protection, education, transit, sanitation, buildings, health and hospitals, welfare, administration, parks and recreation, and other. Again, the coefficients shown are the estimates from IV regressions of the natural logarithm of the expenditure function against lagged revenues. As in Table 7, Table 8 shows the percentages that each function contributed to expenditures in 1972 and 2002.

Again, a coefficient above 1.14 in Table 8 signals an expenditure function that is especially sensitive to changes in revenue. The most sensitive components are spending on transit (most of which is on highways) and spending on health and hospitals (most of which is hospitals). For a one percent increase in revenue, spending on transit will increase by 3.97 percent, and spending on health and hospitals will increase by 3.70 percent. Also significant are the large responses of buildings and administration to fluctuations in revenues. The responsiveness of transit, hospitals, and buildings is consistent with my finding that capital expenditures are especially responsive to movements in city revenue.

2.5 Estimating Local Fiscal Multipliers

In this section, I attempt to use the variation in the revenue structure of the city governments as an instrument to estimate the impacts of a balanced budget shock. Imagine the following: A negative shock occurs to both City A and City B. City A has very responsive revenues, and is forced to cut expenditures substantially in response to the initial shock. City B, on the other hand, does not have very responsive revenues, and does not have to cut expenditures to the same degree. In the following period, the economies of City A and City B may respond differently because of the difference in local government expenditures and revenues. If these differences are somewhat randomly assigned, we can learn about the effects of a balanced budget change to local government expenditures on employment. Given the analysis that I did in Section 4, I argue that I can use the elasticity-income variable to instrument for a balanced budget expenditure shock in a regression of government expenditures on total private employment:

$$\ln Emp_{it} = \alpha + \beta \ln Exp_{it} + \eta_1 \ln I_{it-1} + \eta_2 (\ln I_{it-1})^2 + \eta_i t + \varphi_i + \kappa_t + \mu_{it} \qquad (2.10)$$

where Emp_{it} is total employment in the county that contains the city government. Because the employment data are at the county level rather than the city level, I need to limit to cities that make up a substantial part of their counties so that it is plausible that their actions would have an effect on total county employment. Also, because there is evidence that the multiplier might be larger in recessions (Christiano et al, 2011), it is useful to interact expenditures with an indicator for whether there is a downturn. As usual, in these fiscal multiplier analyses, the key of estimating Equation (10) is to find an instrument for expenditures. In the analysis laid out above in the paper, we have found one (for balanced budget government spending shocks) in the elasticity-income variable. In Table 9, I show the results from the first-stage for the two endogenous variables, Exp_{it} and $Exp_{it} * I(downturn)$.

Since the regressions shown in Table 9 are the reduced form of the analysis done in Section 4.3, it should come as no surprise that the elasticity-income variable predicts expenditures. In the first column, I show the baseline regression for all city governments. In columns (2) - (4), I limit the sample to cities that make up more than 80 percent of their counties. Even in the limited sample, it appears that the income-elasticity variable appropriately predicts expenditures (albeit with wider standard errors).

Table 10 shows both the OLS and the IV results in estimating Equation (10). The dependent variable in all of these regressions is total county level employment, as measured by the BEA. In Columns (2), (3), (5), and (6), I limit to the cities that make up more than 80 percent of their counties so that their governmental actions could plausibly have an effect on the aggregate county employment. I present Columns (1) and (4) to show the mapping from the previous analysis.

In the OLS baseline regression in Column (2), I find a very small (although significant) positive response of total employment to expenditures. When expenditures are increased by one percent, I find that total county employment is increased by 0.02 percent. In the IV results, the point estimates jump substantially (for the regressions that limit to those cities that make up a large percent of their counties). In the baseline regression I find that a one percent increase in expenditures leads to a 0.35 percent increase in employment. Since the average employment to expenditure ratio is about 0.0002, 0.35 corresponds to an average measurement of 14,000 dollars per job–which is a smaller measurement than the estimates in Chodorow-Reich (2012), Wilson (2012), and Shoag (2011).[20] However, the standard errors are too large to draw conclusions about this point estimate. Furthermore, the standard errors in Column (6) are too large to distinguish between the multiplier difference in downturns versus upturns.

Because of the imprecision of the results, my contribution in this section is not a precise

[20]To convert the elasticity coefficient to a level coefficient, consider that $\beta = \frac{d \ln Emp}{d \ln Exp} = \frac{dEmp}{dExp}(\frac{Exp}{Emp})$, so that $\frac{dExp}{dEmp} = \frac{Exp}{\beta * Emp}$.

estimate of the balanced budget multiplier, but rather a new approach to measuring a multiplier that deserves more attention in the literature. The analysis of this paper suggests that to understand the private sector implications of the recent state and local government budget cuts, the multiplier of interest is the balanced budget multiplier rather than the deficit-financed fiscal multiplier. In future analysis, I believe that the variation in revenue structure of local governments could be used to achieve a more precise estimate of this variable. In particular, given the magnitude of the cuts in the recent recession, there may be success in applying this methodology to recent years when the local government data becomes available.[21]

2.6 Caveats and Concerns

In Sections 4 and 5, I have argued that $\epsilon_{i1972} \ln I_{it}$ can be used as an instrument for $\ln R_{it}$ and balanced budget shocks, and showed that the first stage holds. However, to be a successful instrument, it also has to be independent of μ_{it} in Equation (3) and Equation (10). The primary concern with this instrument is that some of the determinants of the revenue structure may also affect the short-term movements of personal income. Recall that the determinants of the revenue structure are related to state-level laws, the preferences of voters, the preferences of legislators, and the previous legislation. By fixing the year in which the revenue structure is taken (to be 1972), I avoid the problem that the revenue structure also responds to the state of the business cycle (in which the more elastic components move more). Because the state laws are not exogenous, it could be that the voter preferences that lead to the state laws will also be correlated with cyclical polices that affect personal income. In order to test this possibility, I look to see whether the ϵ_{i1972} is correlated with the political beliefs of a state as measured by whether there is a democratic or republican governor elected to office. Specifically, I run the regression:

$$\epsilon_{i1972} = \beta I(dem\_gov_{i1972}) + v_i \tag{2.11}$$

I find a coefficient tightly estimated around zero. When I use the share that voted for a democratic governor, or the share of the legislature that is democratic, I find a similarly negligible

[21] Currently, the individual government finance data are available only through 2006.

correlation.

Perhaps the most troublesome concern is that the cyclicality of state intergovernmental transfers may be correlated to the revenue structure of the local government. This would occur if cities were able to choose a more elastic revenue structure precisely because their state government increased transfers during recessions. This might happen, for example, if the revenue structure of the state government counterbalanced the revenue structure of the local government in terms of short-term elasticity. I test this possibility using two methods. First, I regress ϵ_{i1972} against the equivalent "total elasticity" variable of the state government. I find zero correlation. Second, as shown in Figure 1, on a state-by-state basis, I estimate the elasticity of the state intergovernmental transfers for each state. I then regress ϵ_{i1972} against that elasticity and, again, find a zero correlation. These tests suggest that the concern that state intergovernmental transfers are correlated to the revenue structure of the local government is unfounded.

Another concern is that the revenue structure choice may be correlated to elements of the economic environment which are also related to the business cycle. This is especially concerning for cities that rely on income taxes. If cities that have a financial sector and a lot of commuters tend to rely more on individual income taxes in order to capture some of the income of their wealthy workers in order to subsidize their less wealthy residents, there is a definite reason to think that this could affect the response of the city to recessions. Because this is particularly concerning for individual income taxes, I re-run the regressions excluding those cities that have positive individual income taxes. I find that the results hold.

2.7 Conclusion

When a government can borrow countercyclically, having revenues that automatically fall during recessions may be a good thing; while keeping expenditures constant, this automatic reduction will leave more cash in the hands of the taxpayers at a time when money is tight. However, when the government faces a balanced budget rule, the benefits of procyclical revenues can be overshadowed by their impacts on expenditures. In the analysis of this paper, I find that in city governments in the United States fluctuations in revenues lead to fluctuations in expenditures of roughly the same size leading to balanced budget spending shocks to the local economies.

Public sector expenditure volatility may have large impacts on both the short-term and long-term outlook for private local economies. In the short-term, the balanced budget spending cuts are likely to have negative effects on the economy through balanced budget multipliers. In Section 5, I argued that the revenue structure of local governments may be used as an instrument for balanced budget shocks. While my sample did not produce precise results, the strong first-stage suggests that future research along these lines has the potential of producing illuminating estimates. The balanced budget multiplier is distinct from other multipliers recently measured in the literature, and estimating its size would not only aid in understanding the impacts of the recent cuts at the local government level, but could also shed light on whether a deficit-neutral fiscal stimulus might be possible at the federal level.

In Section 4, I showed some evidence that capital outlays might be disproportionately affected by the revenue shocks at the city government level. Highways, other transit, and hospitals are the components of expenditures that are the most responsive to fluctuations in revenue. There is an existing literature that suggests that these components of public investment may be integrally connected to aggregate productivity. Aschauer(1989) found that, of all infrastructure spending, the "core" infrastructure of "streets, highways, airports, mass transit, sewers, water systems, etc." had the most power in explaining the connection between aggregate productivity and public investment. These are exactly the components of public infrastructure that are implemented at the local government level. The Congressional Budget Office did an analysis in the 1980s on highway spending and found a surprisingly high 35 percent rate of return to projects that maintain current highway conditions (Congressional Budget Office, 1988). Other estimates of the rates of return associated with public investment are reported in Gramlich (1994). Although these studies focus on the level of public investment rather than its responsiveness to the business cycle, their findings suggest that cyclical disruptions to infrastructure spending may have negative effects on productivity during vulnerable times. The long-term effects that expenditure fluctuations have on productivity and growth through this channel are well-worth exploring in future research.

Finally, the results in this paper highlight the potential importance of automatic stabilizers. While at the federal level automatic stabilizers can exist for both revenue and expenditure components of the budget (where in recessions revenues automatically fall and expenditures automatically increase), the balanced budget rules at the local government levels suggest that a better approach would be to find a way to automatically stabilize revenues in order to avoid expenditure fluctuations. The federal government can have a role in smoothing local government revenues by implementing a policy of countercyclical intergovernmental grants. In the late 1970s, the federal government passed a temporary countercyclical revenue sharing program in which all local governments were awarded grants when their local unemployment rates reached high enough levels. Although that program was too small to have significant macroeconomic effects at the time, a similar program might successfully help to soften the impact of negative revenue shocks at the municipal level. Furthermore, grants that were automatically distributed in response to local economic conditions would help to alleviate any revenue declines in a timely fashion. Local governments could also work to stabilizc their own revenue sources by adjusting their revenue structures within the constraints of state level laws. In addition, they could legislate revenue rate increases that are implemented automatically when revenues fall or local economies deteriorate.

2.8 Appendix

The sources of the data used are described below:

Local government finance and population data - These data are all from the Annual Survey of Government Finance (ASG) as collected by the Census Bureau, in which all governments are surveyed every five years (in years ending in -2 and -5), and a sample of governments are surveyed each year. The probability of being sampled is related to the population of the city. By limiting my sample to cities with more than 25,000 in 1972, I construct a sample that is close to being balanced. The ASG data are reported by the fiscal year of the local government. Because my analysis relies on merging these data with macroeconomic variables, I adjust the ASG data to match the calendar years. Details on how I do this can be found in Feiveson (2012).

GDP deflators - I deflate all the ASG finance data using the GDP state and local deflator, and the macroeconomic level data using the GDP deflators. Both deflators come from the BEA National and Income Product Accounts, Table 1.1.4.

County level income, employment, and population data - These data are from the BEA regional accounts. In particular, the income and population data are from the series CA05, and the employment data are from the series CA25.

Tables

Table 1: City Revenue Decomposition: Percent of Total Revenues

Revenue Source	1972				2002			
	Mean	Standard Deviation	Minimum	Maximum	Mean	Standard Deviation	Minimum	Maximum
Property Taxes	29	18	0	96	20	13	0	81
Sales Taxes	6	9	0	56	8	10	0	45
Individual Income Taxes	2	8	0	63	2	8	0	47
State Transfers	14	9	0	57	16	14	0	73
Federal Transfers	7	7	0	63	4	4	0	30
Charges and Fees	18	12	1	79	25	11	4	74
Utilities	14	15	0	73	14	15	0	68
Other	9	7	0	38	10	9	-52	56

Table 2: Top 20 City Revenue Decomposition

City	State	Population	Percent of Revenues Coming From:							
			Property Taxes	Sales Taxes	Individual Income Taxes	State Transfers	Federal Transfers	Charges and Fees	Utilities	Other
NEW YORK CITY	NY	7,895,563	21	5	8	39	3	9	7	9
CHICAGO CITY	IL	3,369,359	31	6	0	9	11	11	6	25
LOS ANGELES CITY	CA	2,809,596	17	7	0	7	5	14	31	18
PHILADELPHIA CITY	PA	1,950,098	15	0	32	12	14	16	3	9
DETROIT CITY	MI	1,512,893	21	0	13	11	22	13	11	10
HOUSTON CITY	TX	1,232,802	34	14	0	1	11	17	12	11
BALTIMORE CITY	MD	905,759	21	0	4	48	9	8	2	7
DALLAS CITY	TX	844,401	37	10	0	1	5	15	20	11
WASHINGTON DC	DC	756,510	13	9	13	0	43	8	1	14
CLEVELAND CITY	OH	750,879	16	0	19	9	12	19	24	1
INDIANAPOLIS CITY	IN	748,056	47	0	0	23	12	17	0	2
MILWAUKEE CITY	WI	717,372	32	0	0	28	7	13	8	12
SAN FRANCISCO CITY	CA	715,674	24	4	0	23	11	16	9	13
SAN DIEGO CITY	CA	697,027	18	10	0	12	15	20	15	11
SAN ANTONIO CITY	TX	654,153	13	6	0	0	10	14	54	3
BOSTON CITY	MA	641,071	54	0	0	17	9	12	2	6
HONOLULU CITY	HI	630,528	45	0	0	8	13	13	9	12
MEMPHIS CITY	TN	623,530	10	0	0	18	3	10	36	22
ST LOUIS CITY	MO	622,236	16	7	16	8	13	14	4	22
NEW ORLEANS CITY	LA	593,471	15	17	0	13	18	21	6	10

Notes: Revenue decomposition from 1972.

Table 3: Elasticity Estimates

	Property Taxes	Sales Taxes	Individual Income Taxes	State Transfers	Federal Transfers	Charges and Fees	Utilities	Other
ln (real income)	0.53***	0.78***	0.70**	0.71***	-0.13	1.11***	0.59***	0.59***
	(0.05)	(0.09)	(0.30)	(0.08)	(0.15)	(0.06)	(0.07)	(0.10)

Notes: Robust standard errors are shown in parentheses. Regressions are the natural logarithm of the revenue source against the natural logarithm of county-level personal income and include city fixed effects and a city-specific time trend. All variables are deflated by the gdp price deflator.
* p<0.10, **p<0.5, ***p<0.01

Table 4: Elasticity Estimates in the Business Cycle

	Property Taxes	Sales Taxes	Individual Income Taxes	State Transfers	Federal Transfers	Charges and Fees	Utilities	Other
ln(income)*	0.58***	0.69***	0.45	0.59***	-0.45*	1.37***	0.83***	0.55***
(1-I(downturn))	(0.08)	(0.16)	(0.36)	(0.14)	(0.27)	(0.12)	(0.11)	(0.18)
ln(income)*I(downturn)	0.47***	0.90***	1.00	0.86***	0.28	0.78***	0.30**	0.64***
	(0.11)	(0.15)	(0.74)	(0.17)	(0.25)	(0.11)	(0.14)	(0.19)
Difference	-0.12	0.21	0.55	0.27	0.73*	-0.59***	-0.53**	0.09
	(0.17)	(0.27)	(0.96)	(0.26)	(0.43)	(0.20)	(0.22)	(0.31)

Notes: Robust standard errors are shown in parentheses. Regressions are the natural logarithm of the revenue source against the natural logarithm of county-level personal income and include city fixed effects and a city-specific time trend. All variables are deflated by the GDP price deflator. A "downturn" is defined by whether the log of real income is below its linear trend.
* p<0.10, **p<0.5, ***p<0.01

Table 5: Total Revenue Regressions

	(1)	(2)	(3)
Elasticity * ln (real income)	1.10***	1.17***	1.28***
	(0.35)	(0.34)	(0.33)
Elasticity * ln (real income) * I(downturn)			-0.26*
			(0.14)
ln (real income)	1.54***	1.08***	1.11***
	(0.41)	(0.36)	(0.38)
ln (real income) ^ 2	-0.04***	-0.03***	-0.03***
	(0.01)	(0.01)	(0.01)
Year Fixed Effects		X	X
Observations	23,996	23,996	23,996

Notes: Robust standard errors are shown in parentheses. The dependent variable of the regressions is the natural logarithm of total city government revenues. All regressions include city fixed effects and a city-specific time trend. Specification (2) and (3) includes year fixed effects. Specification (3) also includes the elasticity variable interacted by an indicator for whether the ln(income) is below its linear trend value. Variables are deflated by the gdp price deflator.
* p<0.10, **p<0.5, ***p<0.01

Table 6: Total Expenditure Regressions

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged ln (revenues)	0.57***	0.58***	0.57***	0.86***	1.14***	1.26***
	(0.02)	(0.02)	(0.03)	(0.31)	(0.33)	(0.29)
Lagged ln (revenues) * I(downturn)			0.03			-0.26
			(0.03)			(0.16)
Lagged ln (real income)	-0.44*	0.30	0.30	-1.14	-0.76	-0.81
	(0.25)	(0.28)	(0.28)	(0.78)	(0.70)	(0.69)
Lagged ln (real income) ^ 2	0.03***	0.01	0.01	0.04**	0.02	0.02*
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Year Fixed Effects		X	X		X	X
Observations	23,625	23,625	23,625	22,916	22,916	22,916

Notes: Robust standard errors are shown in parentheses. The first three regressions are OLS regressions with the natural logarithm of total city government expenditures as the dependent variable. In the IV regressions in columns (4)-(6), the elasticity variable multiplied by the lagged ln (real income) instruments for the lagged ln (total revenues). Finally, in column (6), there is an additional endogenous variable of the lagged revenues times an indicator as to whether there is a downturn, with an additional instrument of the total elasticity multiplied by lagged ln (income) and interacted with the indicator for whether there is a downturn. A downturn is indicated when the ln (real income) lies below its linear trend. All regressions include city fixed effects and a city-specific time trend. Variables are deflated by the GDP price deflator.
* p<0.10, **p<0.5, ***p<0.01

Table 7: Expenditure Components

	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Capital Outlays	Current Operations	Insurance Trust Funds	Other	Salaries and Wages
Lagged ln (revenues)	1.14***	2.39**	0.64**	3.27**	3.20***	0.83**
	(0.33)	(1.14)	(0.26)	(1.64)	(1.18)	(0.37)
% of Expenditures, 1972	100.0	18.1	67.9	3.6	10.4	44.0
% of Expenditures, 2002	100.0	16.5	68.3	6.1	9.1	31.3
Observations	22,916	22,797	22,915	11,629	22,879	22,868

Notes: Robust standard errors are shown in parentheses. Regressions are IV regressions where the elasticity variable multiplied by the lagged ln (real income) instruments for lagged ln (revenues). The dependent variables are the natural logarithms of the expenditure component. All regressions include city fixed effects, city-specific time trends, year fixed effects, and control for lagged ln(income) and lagged ln(income) square. Variables are deflated by the gdp price deflator.
* p<0.10, **p<0.5, ***p<0.01

Table 8: Expenditure Functions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Protection (Police, Fire, Correction)	Education	Transit (Highways, Subways, Air, Water)	Sanitation (Sewerage and Solid Waste)	Buildings (Housing, Public Buildings, Libraries)	Health and Hospitals	Welfare	Admin-istration	Parks and Recreation	Other
Lagged ln (revenues)	1.35***	-1.10	3.97***	1.16	2.61***	3.70**	-0.26	2.44***	0.54	1.02*
	(0.39)	(1.95)	(1.22)	(0.76)	(0.99)	(1.72)	(1.45)	(0.73)	(0.71)	(0.56)
% of Expenditures,	13.9	14.2	6.7	6.5	5.8	6.5	7.8	3.0	3.5	38.9
% of Expenditures,	16.1	13.0	8.6	7.3	6.0	5.2	4.9	4.1	4.0	45.2
Observations	22916	23,958	4,896	23,968	23,475	23,776	17,543	8,851	23,966	23,427

Notes: Robust standard errors are shown in parentheses. Regressions are IV regressions where the elasticity variable multiplied by the lagged ln (real income) instruments for lagged ln (revenues). The dependent variables are the natural logarithms of the expenditure component. All regressions include city fixed effects, city-specific time trends, year fixed effects, and control for lagged ln(income) and lagged ln(income) square. Variables are deflated by the gdp price deflator.
* p<0.10, **p<0.5, ***p<0.01

Table 9: First Stage

	(1)	(2)	(3)	(4)
		ln (expenditures)		ln (exp) * I (downturn)
Elasticity * ln (real income)	1.19***	1.15**	1.36**	0.08
	(0.34)	(0.59)	(0.59)	(0.33)
Elasticity * ln (real income) * I(downturn)			-0.43	1.29***
			(0.40)	(0.35)
City/County Cutoff	None	0.80	0.80	0.80
Observations	22,916	1,525	1,525	1,525

Notes: Robust standard errors are shown in parentheses. All regressions include city fixed effects, city-specific time trends, year fixed effects, and control for lagged ln(income) and lagged ln(income) squared. In Specifications (2)-(4), the observations are limited to city's that make up more than 80 percent of their containing counties. Variables are deflated by the gdp price deflator.
* p<0.10, **p<0.5, ***p<0.01

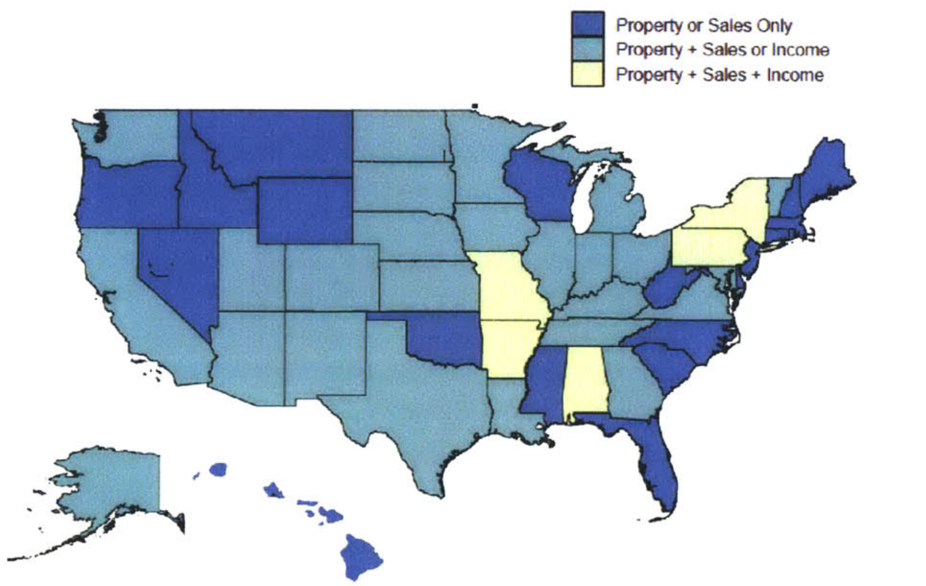
Table 10: Total Expenditure Regressions

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
ln (expenditures)	0.01***	0.02**	0.02	-0.00	0.35	0.40*
	(0.00)	(0.01)	(0.02)	(0.10)	(0.25)	(0.22)
ln (expenditures) * I(downturn)			0.01			-0.08
			(0.02)			(0.12)
City/County Cutoff	None	0.80	0.80	None	0.80	0.80
Observations	23,625	1,525	1,525	22,916	1,525	1,525

Notes: Robust standard errors are shown in parentheses. All regressions include city fixed effects, city-specific time trends, year fixed effects, and control for lagged ln(income) and lagged ln(income) squared. Specifications (1)-(3) are OLS regressions with the natural logarithm of total county employment as the dependent variable. Specifications (4) - (6) are IV regressions where the elasticity multiplied by lagged ln(real income) instruments for ln (expenditures). Variables are deflated by the gdp price deflator.
* p<0.10, **p<0.5, ***p<0.01

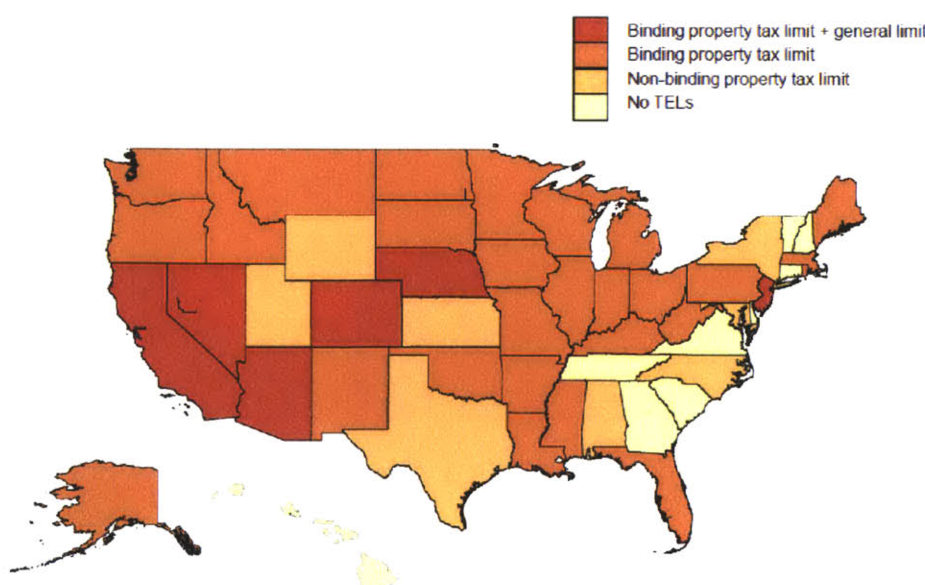
Figures

Figure 1: Municipal Tax Authority



Notes: Data are from Hoene and Pagano (2008).

Figure 2: Tax and Expenditure Limits



Notes: Data are from Hoene and Pagano (2008).

Figure 3: State-Specific Elasticity of Intergovernmental Revenue

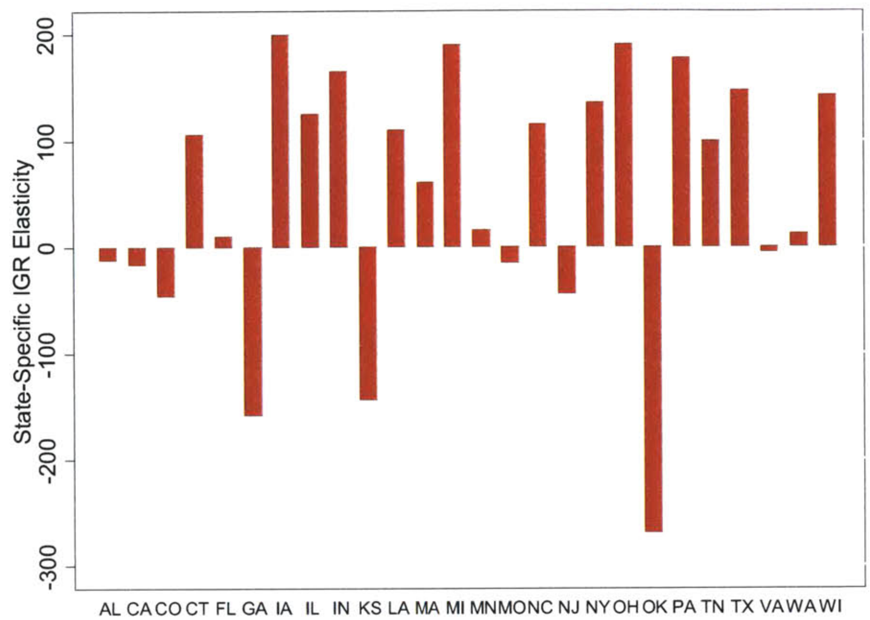


Figure 4: Total Elasticity Variable

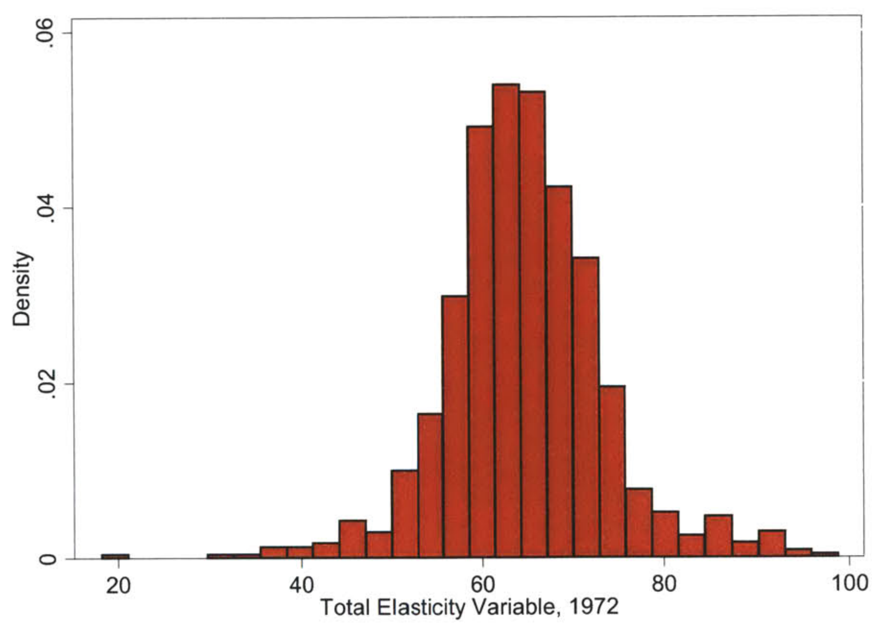
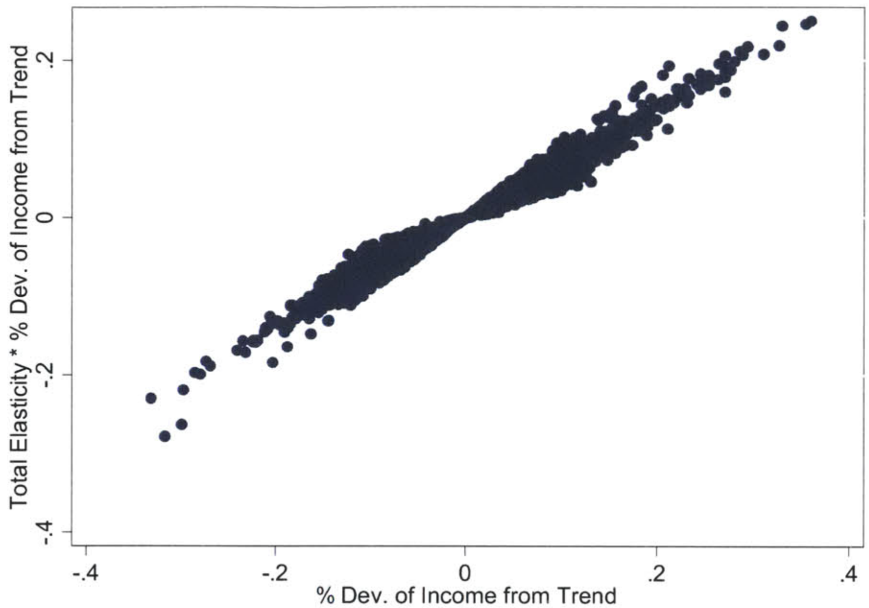


Figure 5: The Size of the Elasticity-Income Shocks



Chapter 3

Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act[1]

3.1 Introduction

The federal government enacted the approximately $800 billion American Recovery and Reinvestment Act (ARRA) in February 2009 to provide a countercyclical impulse during the worst economic downturn in the United States in at least sixty years. At the same time, state governments, almost all of which have balanced budget requirements that restrict borrowing across fiscal years, had already begun to lay off employees, cut spending and transfer programs, and raise taxes. Rather than concentrate the stimulus in direct federal government purchases of output, the ARRA's authors chose to mitigate this sub-national contractionary fiscal impulse by routing roughly a third of the total through state and local governments. The largest of these programs was the increase in the federal match component of state Medicaid expenditures.

Countercyclical intergovernmental transfers to support sub-national budgets have occurred

[1] **Co-authored with Gabriel Chodorow-Reich, Zachary Liscow, and William Woolston.**

previously in the U.S. and in other countries around the world. Yet, this form of stimulus has received little attention in the academic literature, compared with the large number of studies of direct government purchases or tax reductions.[2] A priori, transfers could have a small or zero immediate impact on economic outcomes if states simply use them to bolster their rainy day funds, effectively shifting money between government accounts without affecting the overall stance of the general government sector. On the other hand, states may use the money to reduce tax increases or avert budget cuts, allowing the money to enter the economy more quickly than direct federal purchases that require project selection and approval. Reflecting this theoretical uncertainty, views on the effectiveness of state aid prior to the ARRA's passage ranged from then-House Minority Leader John Boehner, who predicted that "direct aid to the states is not going to do anything to stimulate our economy," to the Obama Administration, which predicted that the state relief would save or create more than 800,000 jobs in the fourth quarter of 2010.[3] Even well after the ARRA's passage, disagreement continued, with many Republicans and some economists claiming that no jobs had been created, while the White House continued claiming large job gains.[4]

This paper aims to fill the gap in our understanding of intergovernmental transfers by empirically assessing the impact of the ARRA's Medicaid match program. The program has a number of features that make it attractive for study. First, the total amount of money distributed through this program is large enough to plausibly generate a detectable effect on employment. Out of a total of $88 billion dedicated to an increase in the Medicaid matching funds, states had received $61.2 billion by June 30, 2010, the end of our period of study. Second, because state Medicaid programs operate on a mandatory basis, increasing the federal share of costs effectively transfers money into state budgets that states can then use for any purpose they choose – the money is fungible. Indeed, many states reported that they had allocated the money quickly to areas that otherwise would have undergone deeper budget cuts (Government

[2] There is a large literature on the extent to which federal grants crowd out local government spending which was spearheaded and summarized by Gramlich (1977).

[3] See http://www.msnbc.msn.com/id/28841300/ns/meet_the_press/t/meet-press-transcript-jan/ and Romer and Bernstein (2009).

[4] See http://www.factcheck.org/2010/09/did-the-stimulus-create-jobs/ for a list of quotes from Republicans claiming that the ARRA created no jobs. Also, a survey by the National Association for Business Economics showed that 69% of business economists they surveyed reported that the ARRA had no impact on employment (http://www.jsonline.com/business/82657582.html).

Accountability Office 2009; National Association of State Budget Officers 2009b). Third, the level of additional money received by states as of June 2010 per person aged 16 or older (16+) varied greatly, from a low of \$103 in Utah to a high of \$507 in DC, with an interquartile range of \$114. This variation makes possible a cross-sectional econometric strategy. We focus our analysis on the effect on employment because the public debate on the effectiveness of the ARRA has centered largely on this outcome. Furthermore, high-quality monthly state level employment data makes it possible to obtain more precise estimates of fiscal multipliers than what is possible with the existing state-level income data.

The primary challenge to a cross-sectional study is that the amount of aid a state receives is endogenous to the state's economic conditions. Because states that were in worse economic shape received more aid, the OLS relationship between the level of state fiscal transfers and changes in employment understates the true effect of state fiscal relief. We address this concern by using an instrument that isolates the component of the Medicaid transfers unrelated to changes in economic circumstances. The ARRA increased the percentage of Medicaid expenditures that the federal government pays for all states by 6.2 percentage points and increased the match rate by more for states that experienced especially large increases in unemployment. Thus, the level of ARRA Medicaid transfers to each state is the result of four factors: the amount of Medicaid spending in the state prior to the recession; the change in the number of beneficiaries during the recession; the change in the average spending per beneficiary; and whether the state qualified for an additional match increase based on the change in the state's unemployment rate. The heart of our identification strategy lies in exploiting only the cross-sectional variation from the first of these factors, that is, the variation in ARRA Medicaid transfers that results from variation in Medicaid programs from before the recession.

Another set of reasons why a state may have both received more Medicaid funding and had different employment outcomes—omitted factors related to both state Medicaid program rules and economic changes—is not solved by the instrument. For example, more liberal coastal and Midwestern states both had larger downturns and have more generous Medicaid programs. We present several pieces of evidence that suggest that our results are not driven by underlying differences between high and low spending Medicaid states. First, to ensure that time-invariant differences between high and low Medicaid spending states are not driving our relationship, our empirical strategy considers changes, rather than levels, of employment. Second, in our baseline specification we exploit only differences in Medicaid spending within census divisions rather than between them, and include a number of variables that help predict how a state's employment would have changed absent the ARRA. Finally, we present falsification tests by running our baseline specification on pre-ARRA data and show that in the decade before the ARRA passed, states with high and low Medicaid spending experienced similar employment outcomes.

An important caveat to our analysis is that a cross-state approach forces us to ignore general equilibrium effects, which could alter our interpretation of the overall effect of stimulus spending on jobs and prevents us from tying down the aggregate fiscal policy multiplier. For example, spending in one state may increase demand in other states, which would lead us to under-state overall job increases.[5] On the other hand, investment could decrease across the country in response to increased government borrowing, though this effect is likely to have been especially muted during the low policy interest rate environment of 2009-10. Likewise, to the extent that people believe that their taxes will be raised in the future due to the increased government borrowing, spending may decrease throughout the country.

With this caveat in mind, we find that the ARRA transfers to states had an economically large and statistically robust positive effect on employment. Assuming that employment does not persist beyond the time during which it is funded, our preferred specification suggests that a marginal $100,000 in Medicaid transfers resulted in 3.8 net job-years (i.e., one job that lasts for one year) of total employment through June 2010, of which 3.2 are outside the government, health, and education sectors. The effect is precisely estimated, and we can reject the null hypothesis that the spending had no effect on employment with a high degree of confidence. For this result to be economically plausible, states must have used the funds to avoid spending cuts or tax increases. Hence we also provide evidence that the transfers do not appear to have increased the states' end of year balances. In connecting our estimates to the implicit changes in government spending or taxes, our paper also adds to the recent literature on the employment effects of state spending (e.g., Shoag 2011; Wilson 2011; Suarez-Serrato and Wingender 2011;

[5] Moretti (2010) notes that, through labor mobility, cross-state spillovers can also be negative. However, labor mobility is likely small over a period of time as short as that considered here.

Clemens and Miran 2011), as well as the fiscal effects of government spending generally (e.g., Nakamura and Steinsson 2011).

The paper proceeds as follows. In Section 2, we describe the institutional details of Medicaid grants and the ARRA stimulus package. Section 3 contains our econometric methodology and describes our baseline specification. In Section 4, we describe our data. Sections 5 and 6 present our main results and robustness checks, respectively. Section 7 provides an interpretation of our results and relates them to the existing literature. Section 8 discusses evidence of a budgetary transmission mechanism, and Section 9 concludes.

3.2 Institutional Details of the ARRA and Medicaid Grants

The ARRA became law in February 2009 at an estimated 10-year cost of $787 billion. Through December 2010, it had distributed $609 billion.[6] As Cogan and Taylor (2010) point out, only $30 billion of this total got recorded in the national income accounts as federal government consumption or investment. A little more than half ($350 billion) went to individuals or business in the form of tax reductions or transfer payments. The rest, more than $200 billion, went through state and local governments, including $88 billion through the Medicaid match program designed especially to alleviate the strain on state budgets.[7] State fiscal relief had the added advantage of getting out the door quickly: in the first quarter of 2009, more than three-fourths of total ARRA outlays and tax expenditures took the form of Medicaid outlays.

Medicaid is a state-run program that provides health insurance for certain individuals and families with low incomes and resources. Both the eligibility requirements and the scope of the insurance coverage vary across states. The federal government reimburses states for between 50 and 83 percent of their Medicaid expenditures, as determined by the Federal Medical Assistance Percentages (FMAP). Many states require that local governments share in financing the non-federal portion of the program. Each federal fiscal year, states' FMAPs are recalculated based on the three-year average of each state's per capita personal income relative to the national

[6] Data in this paragraph come from the Bureau of Economic Analysis Recovery Act data program at www.bea.gov/recovery.

[7] Another $38 billion went through the State Fiscal Stabilization Fund (SFSF), part of a $48.6 billion appropriation that apportioned the money according to a mix of population of persons aged 5-24 (61%) and total population (39%).

average, with poorer states receiving higher reimbursement rates. Thus, states that have lower average incomes, more recipients of Medicaid per capita, or more generous benefits receive larger per capita matching funds from the federal government.

The ARRA made three changes to the baseline FMAP calculation for October 2008 through December 2010. First, the baseline FMAP could not decrease. Second, the FMAP was increased by 6.2 percentage points above the baseline for every state.[8] The additional match applied retroactively from passage in mid-February back to October 2008, making part of the transfer purely lump-sum. Finally, through December 2010, each state received a further increase in its FMAP based on the largest increase in its unemployment rate experienced between the trough three-month average since January 2006 and the most recently available 3-month average.[9] To qualify for the ARRA changes, states had to, at a minimum, maintain the eligibility standards, methodologies, and procedures of their Medicaid programs that existed on July 1, 2008. Program benefits could, however, change. The law also forbade states from increasing the share of the non-federally financed portion of Medicaid spending borne by local governments, in effect extending the fiscal relief to local governments as well.

There appear to have been two main rationales for the FMAP increases. First, unlike direct federal spending, state fiscal relief through changes to the FMAP could be implemented almost immediately; the first ARRA Medicaid reimbursements recorded by the Department of Health and Human Services occurred during the week ending on March 13, 2009, only a few weeks after the ARRA was signed into law. Second, the changes to FMAP were intended to boost the level of discretionary funds available to states, and not only to relieve Medicaid burdens. Because an increase in the FMAP reduces the state portion of mandatory payments, the additional funds are completely fungible – states can use them however they wish. Congress recognized the fungibility of the funds during the legislative debate. Indeed, the legislative text of the ARRA says that the first purpose of the section containing the FMAP increases is to "provide fiscal

[8]Under the ARRA, the 0.83 cap on FMAP was also removed.

[9]In the fourth quarter of 2008 and the first quarter of 2009, the extra amount was actually based on the largest increase between the trough 3-month average unemployment rate since January 2006 and the average unemployment rate from October 2008 to December 2008. In the third and fourth quarters of 2010, the calculation was based on the difference between the same trough average rate and the larger average of the two 3-consecutive month periods beginning with December 2009 and January 2010, respectively. Furthermore, there was a maintenance of status clause which legislated that any increase in FMAP made for a quarter on or after January 1, 2009, would be maintained through the second quarter of 2010.

relief to States in a period of economic downturn." Section 8 discusses the empirical evidence on how states used the extra FMAP funds.

Congress began discussions with state governors on a stimulus bill that would include significant aid to state governments as early as December 2008.[10] The House appropriation committee draft released on January 15, 2009 included an increase in the FMAP of 4.8 percentage points, and both the original House and Senate versions, passed on January 28 and February 10, respectively, had the same $88 billion allocated to Medicaid as the final bill. Hence our analysis should begin no later than December 2008 if state governments incorporated the likelihood of additional federal relief into their budget plans.

3.3 Econometric Methodology and Baseline Specification

3.3.1 Instrumental Variables Motivation

We begin with a simple framework that relates state fiscal relief to total employment. The change in the ratio of employment to potential workers in a state, s, depends on the state fiscal relief that the state receives, a series of controls that capture differential trends, and a state-specific shock:

$$\frac{E_1^s - E_0^s}{N^s} = \beta_0 + \beta_1 \frac{Aid^s}{N^s} + \beta_2 Controls^s + \varepsilon^s \tag{3.1}$$

where E_i^s is the seasonally-adjusted employment in state s in period i, N^s is the 16+ population in state s, β_0 is a national-level shock, Aid^s is the state fiscal relief received by state s, $Controls^s$ are state level controls in state s, and ε^s is a state-level mean-zero shock.

If the state fiscal relief per potential worker, $\frac{Aid^s}{N^s}$, were uncorrelated with the error term, ε^s, then (xxx) could be estimated with bivariate OLS. However, this assumption is almost certainly not valid. The ARRA Medicaid transfers to each state reflect four factors: the amount of Medicaid spending in the state prior to the recession; the change in the number of beneficiaries during the recession; the change in the average spending per beneficiary; and whether the state

[10]For example, House Speaker Nancy Pelosi met with a group of governors on December 1st to discuss the contours of a stimulus bill that would include state aid. See: Cowan, Richard. 2008. "House to Push $500 Billion Stimulus Bill." Reuters, December 1. Retrieved on August 10, 2010. http://www.reuters.com/article/idUSTRE4B05QP20081201.

qualified for the additional match increase based on the change in the state's unemployment rate. These last three factors, and especially the fourth, share the concern of reverse causality with respect to the outcome variable. Hence we use an instrument that restricts the cross-state variation to only that part of Medicaid transfers related to pre-recession Medicaid spending. Specifically, we implement a two-stage least squares estimation strategy, using 2007 Medicaid spending as an instrument for the FMAP transfers. We normalize all relevant variables by the number of individuals age 16+ in a state in 2008.

We also include a number of state-level controls that are potentially correlated with both 2007 Medicaid spending and changes in employment. These controls are detailed in Section 4 and include the lagged change in employment to capture pre-existing trends between high and low Medicaid spending states.

3.3.2 Other Aspects of the Baseline Specification

We focus on two primary outcome variables: change in seasonally adjusted total nonfarm employment and change in seasonally adjusted employment in the state and local government, health, and education sectors. We focus on total nonfarm employment because it is the most comprehensive measure of employment available in our primary data. We also consider government, health, and education workers since the direct effects of state spending are likely to be in these sectors, which contain state government employees, employees of local governments which may have received direct fiscal relief from lower required Medicaid payments and which depend heavily on state transfers for revenue, and employees of many of the private establishments that receive transfers or grants from state and local governments. To ensure that changes in federal employment are not driving our results, we exclude federal workers from this measure.

Although we show how our estimates evolve over time in Section 5, we focus on employment changes from December 2008 to July 2009 for our robustness checks and our summary statistics. We begin our period in December 2008 because, as described above, it is the last month before which the details of the ARRA, including the FMAP extension, became clear to the public. We end in July 2009 for three reasons. First, almost all states have fiscal years that run from July 1 to June 30.[11] Thus, employment through the middle of July reflects any changes to

[11] All states other than Alabama, Michigan, New York, and Texas have fiscal years that begin on July 1.

government employment that occurred at the beginning of the first full fiscal year after the ARRA was passed. Second, employees in education tend to remain on the payroll through the end of the school year, so July is the first month that would fully reflect changes in the number of jobs in education. This is important because of the large fraction of state and local government spending that goes to education.

Historic aggregate time series confirm that employment changes are especially large in July. In regressions reported in an online appendix, we compared the historical mean of the absolute value and square of state and local government employment changes for each month.[12] For both measures, the average July change was larger than that of every other month, and the difference was statistically significant for every month but September and October.

The third reason to end in July 2009 stems from efficiency considerations. For example, if the component of state employment orthogonal to our regressors is i.i.d. with variance σ^2 at a monthly frequency, then the residual variance in a regression with employment change taken over k months will equal $k\sigma^2$. That is, standard errors may increase with the duration of the employment change. This is confirmed in Section 5 where we explore how the effect evolves over time. To generate precise estimates for the baseline specification, it is therefore preferable to restrict the time-window to be as short as possible.

The endogenous variable in our baseline specification is total FMAP outlays to a state through June 30, 2010, normalized by a state's 16+ population. This choice of endogenous variable is crucial to the interpretation of our results. If the state distribution of non-FMAP ARRA spending were correlated with the instrument, we would misestimate the true value of the coefficient on spending if we did not include the correlated component of spending in the endogenous variable. However, a regression of all non-FMAP ARRA outlays to states against the instrument (both normalized by 16+ population) and our baseline controls cannot reject the null that the instrument is uncorrelated with other spending (p-value = 0.413).[13]

Our final decision concerns the time covered by the endogenous variable. Since states tend to budget in yearly cycles, Medicaid transfers from the federal government received during

Alabama and Michigan's start on October 1 (as does the federal fiscal year), New York's fiscal year begins on April 1, and Texas's fiscal year begins on September 1. See National Association of State Budget Officers (2008a).

[12]Note that the employment data are seasonally adjusted, but only for levels, not higher-order moments.

[13]The ARRA state outlays are from Recovery.gov and exclude tax reductions.

a fiscal year could have an effect on employment at any point within that year. Borrowing restrictions make transferring funds across fiscal years difficult. With these facts in mind, we set the endogenous variable equal to the total FMAP transfers through June 2010, which corresponds to the end of fiscal year 2010 for nearly all states. We use this endogenous variable in all of our timing regressions which cover employment changes between December 2008 and each month through June 2010. Because the amount of Medicaid spending in a state exhibits a high degree of serial correlation, the precise end date barely affects the statistical significance of our results.

3.4 Data and Summary Statistics

Outcome variables: Our primary outcome variables are derived from the seasonally adjusted state-level employment series available at a monthly frequency from the Current Employment Statistics (CES).[14] For each state for which the CES has data, we obtained monthly data from January 2000 to June 2010 on employment in total nonfarm, government, health, education, and education and health (a series that is reported separately and is available for a wider group of states than either the health or education series). The latest available vintage of CES data contains benchmarks to unemployment insurance (UI) records through September 2010, meaning that employment for each month is based on data from the UI program (adjusted for coverage using other CES sources) and therefore contains minimal sampling error. We normalize employment by a state's 16+ civilian non-institutional population as estimated by the Bureau of Labor Statistics from Census data.

Endogenous variables: Our primary endogenous variable is a state's total ARRA FMAP outlays as of June 30, 2010, normalized by a state's 16+ population. These data are available from recovery.gov (U.S. Recovery Accountability and Transparency Board 2009-2010).[15]

[14] Because seasonal adjustment differs significantly across states, our baseline specification focuses on seasonally adjusted data. However, in Table 5, we present year-over-year changes in employment using non-seasonally adjusted employment changes from the QCEW.

[15] The agency Financial and Activity Reports available on Recovery.gov report outlays at the Treasury Account Financing Symbol (TAFS) level. The TAFS for FMAP is 750518. A payment to a state is recorded as an outlay when money is transferred from the U.S. Treasury to the state as reimbursement for a Medicaid payment the state has already made. Our data exclude about $3 billion provided through application of the ARRA FMAP increase to state contributions for prescription drug costs for full-benefit dual eligible individuals enrolled in Medicare Part D because the Financial and Activity Reports do not show a state-by-state breakdown of this

Instrument: The instrument is a state's Medicaid spending in fiscal year 2007, normalized by the 16+ population.[16,17] Figure 1 demonstrates the considerable cross-state variation in the instrument. To ease interpretation, the figure shows the instrument scaled by 6.2% because ARRA increased the FMAP by 6.2 percentage points, and inflated by 21/12 because from October 2008 (the month after which the FMAP increase was retroactively increased) through the end of June 2010 (the end of our sample), states received a cumulative 21 months of Medicaid reimbursements. Note that some states that are similar across many other dimensions have very different values; Medicaid spending is roughly twice as high in New York as in California, in Vermont as in New Hampshire, and in New Mexico as in Colorado.

Control variables: Our choice of control variables is motivated primarily by the threat to identification that states that received different amounts of Medicaid funding in 2007 were on different employment trends during the time period studied. Figure 2 shows on a map the value of the instrument, scaled as described above; states are grouped into six groups of spending per capita. One potential concern is there is substantial regional variation in Medicaid spending. For example, the map shows that New England has high Medicaid spending. Because the employment effects of the recession were distributed unevenly across regions, differences in employment between high and low Medicaid spending states could reflect regional differences in underlying economic conditions rather than the effect of state fiscal relief. To address this concern, in our preferred specification, we include categorical variables for the nine census divisions, isolating the variation in the instrument that comes from within regions rather than between them.

In our preferred specification, we also control for pre-existing economic conditions using lagged employment change (from May to December 2008, the seven months prior to the beginning of our sample period). Adding this control is potentially important because empirically, employment changes are highly persistent. Moreover, while we cannot reject the null that our instrument is uncorrelated with employment changes from May to December 2008, the point estimate for this correlation is non-trivial in magnitude, raising the possibility that high and

spending during our period of study.

[16] Data on 2007 Medicaid spending by state are available from the Centers for Medicare and Medicaid Services (2008).

[17] Per capita Medicaid spending is highly correlated over time. For example, the correlation between our instrument using 2007 Medicaid spending per capita and 2001 Medicaid spending per capita is 0.95.

low Medicaid spending states might have been on different employment trends prior to the ARRA.[18] In Section 6, we explore the robustness of our results to controlling for alternative measures of past economic conditions. In our baseline regression, we also control for GDP per potential worker and the employment manufacturing share.

To help address concerns about differential cyclicality of state spending related to the instrument through common political factors, we control for the 2007 share of workers in a union and the vote share for Senator Kerry in the 2004 presidential election. If cyclicality differs between states with different amounts of Medicaid spending (in ways not captured by a lag) because more liberal or unionized states have more Medicaid spending, as well as stronger safety nets and weaker balanced budget requirements, these controls would alleviate that concern. Finally, we control for the 2008 state population. Further details are in the appendix.

Table 1 presents summary statistics for the main variables used in the paper. All relevant variables are normalized by a state's 16+ population. The average total ARRA outlay through June 2010 was approximately $1,000 per person age 16+ (excluding tax benefits and spending not tracked at the state level). Of this, approximately one-quarter came through FMAP outlays, and more than one-third came through FMAP outlays plus the other large state fiscal relief program, the State Fiscal Stabilization Fund. There is considerable variation in both total ARRA and FMAP outlays across states, with the coefficients of variation at 0.32 and 0.36 respectively. During the period considered, average total nonfarm employment changes were sharply negative. However, there is also considerable cross-state variation in this pattern. For example, normalized employment changes were more than 5 times more negative for the state at the fifth percentile of the total employment change distribution (Indiana) than the state at the 95th percentile (Alaska). There is broadly similar variation in the change in employment in the government, health, and education sectors.

[18]The correlation between the change in per capita total nonfarm employment during the seven months prior to the beginning of our sample period (May and December 2008) and the instrument is 0.23 (p-value = 0.10). During this period, the correlation between the change in per capita government, health, and education employment and the instrument is -0.20 (p-value = 0.17). In contrast, during the main period of interest (December 2008 to July 2009), the correlation between the instrument and these outcome variables is larger and precisely estimated. For the change in employment, the correlation is 0.55 (p-value < 0.01), and for total nonfarm, the correlation is 0.40 for government, health, and education (p-value < 0.01).

3.5 Baseline Results

3.5.1 First Stage

In Table 2, we present results from several first-stage regressions. The outcome variable is total FMAP outlays as of June 30, 2010, normalized by a states' 16+ population and measured in \$100,000 increments.

To interpret Table 2, it is useful to divide the instrument coefficient by 0.062 to reflect the ARRA FMAP increase of 6.2 percentage points, and to further divide by 21/12 to adjust for the cumulative 21 months of Medicaid reimbursements through the end of June 2010 (the end of our sample), yielding a cumulative multiplicative scaling factor of 9.2. This scaled first stage coefficient would be 1 if the FMAP outlays simply represented 6.2% of Medicaid spending at 2007 rates. However, there are two reasons why we would expect the scaled coefficient to be larger than 1. First, FMAP ARRA outlays are based on current Medicaid spending, not 2007 spending. Due to the rapid growth in nominal Medicaid expenditures since 2007, if all states' Medicaid expenditures simply increased at the nominal national rate, we would expect a scaled coefficient substantially above 1.[19] Second, as described above, FMAP outlays also include FMAP increases for states that experienced sufficiently large changes in their unemployment rate. If high and low Medicaid spending states experienced identical changes in their unemployment rates, these FMAP expansions would mean that a larger number of dollars would flow to high Medicaid spending states, as a given FMAP increase translates into more dollars for these states. As a consequence, the average difference in Medicaid matching outlay for a high and low Medicaid spending state would be larger.

Model (1) presents a simple bivariate regression. The coefficient on our instrument is 0.18, and it is precisely estimated, with an F-statistic above 260. The instrument alone explains more than 80% of the variation in FMAP outlays. In Model (1), we can strongly reject the hypothesis that the scaled coefficient (0.18 divided by 0.062 and 21/12 = 1.68) is 1. Specifications (2) – (4) show that this positive and precisely estimated relationship between the instrument and our main endogenous variable is robust to including a large number of covariates. Model (2)

[19] The Centers for Medicare and Medicaid Services (CMS) reports that in 2008, Medicaid spending increased 4.7%. CMS projected that Medicaid spending would increase 9.9% in 2009. See http://www.cms.gov/NationalHealthExpendData/25_NHE_Fact_Sheet.asp.

includes our basic set of controls, including region fixed effects. Model (3) adds a control for lagged total employment change from May – December 2008, while Model (4) augments (2) with lagged change in government, health, and education employment over the same period. Overall, the first stage is very strong.

3.5.2 Baseline Results through July 2009

In this section, we present baseline results where the outcome variable is change in employment in a sector from December 2008 to July 2009. Table 3 presents baseline results for total employment. Models (1) – (3) report OLS regressions. The OLS regressions with controls (Models (2) and (3)) indicate a small positive correlation between a state's FMAP outlays and its change in total employment, although the effect is not statistically significant.

Models (4) – (6) present the baseline IV results. There is a precisely estimated positive relationship between instrumented FMAP outlays and a state's change in total employment. In the bivariate IV regression [Model (4)], the coefficient on total FMAP outlays per person 16+ is 4.72. While the large difference between the IV and OLS estimates may appear surprising given the strength of the first stage, recall that the first-stage residual should be strongly negatively correlated with employment growth due to the unemployment triggers in the FMAP increase, biasing the OLS results downward.

Adding a wide variety of control variables [Model (5)] changes the estimate little. Including the lagged employment control [Model (6)] reduces the point estimate by approximately 40% but has little effect on the statistical significance of the result, as the standard error also shrinks. The fact that adding a control for lagged employment influences the point estimate suggests that high and low Medicaid spending states were on different employment trends prior to the ARRA, a hypothesis that we explore in the robustness section.

The coefficient in (6), the preferred specification, suggests that for every $100,000 in FMAP outlays per individual 16+ that a state received by June 30, 2010, that states' total employment increased by 2.83 per individual 16+ from December 2008 to July 2009. Section 7 provides further discussion of how to interpret this magnitude.

Table 4 parallels the results from Table 3, using the change in government, health, and education employment as the outcome variable. The OLS coefficients [Models (1) – (3)] are positive, relatively small in magnitude, and not statistically significant. The IV results [Models (4) - (6)], in contrast, suggest a positive relationship between FMAP transfers and change in employment in these sectors. For the IV specifications, the control variables have very little influence on the point estimates, but they do substantially reduce the standard errors. The coefficient on (6) suggests that for every $100,000 in FMAP outlays per individual 16+ that a state received by June 30, 2010, that states' employment in the government, health, and education sectors increased by 1.17 per individual 16+ over the period considered.

The coefficients in Table 4 are less than half of the magnitude of those in Table 3, suggesting that the "indirect" employment gains in the non-government-related sectors were substantial. To see this more explicitly, we re-estimate our preferred specification, changing the dependent variable to be the change in total employment excluding the change in employment in the government, health, and education sectors. This regression yields a coefficient of 1.86 (95% CI: 0.32, 3.41).

3.5.3 Timing Results

The previous section presented results where the outcome variable was the change in employment from December 2008 until July 2009. This section explores how our estimates evolve as we change the month that marks the end of our sample. Specifically, we re-run the cross-sectional regression for changes in employment from December 2008 until every month from January 2009 to June 2010 and report the second stage coefficients on total FMAP outlays from our preferred specification with the full set of control variables. That is, we re-run the estimate from December 2008 to January 2009, December 2008 to February 2009, December 2008 to March 2009, etc. and report each of these 18 coefficients.

Figure 3 presents these results for total nonfarm employment. The solid line represents the point estimate, and the dashed lines indicate the 95% confidence interval. These timing results suggest three main patterns. First, while there appears to be a positive relationship between FMAP outlays and change in employment before July 2009, the relationship is small and not precisely measured. Second, starting in July 2009, the coefficient jumps in magnitude, varying from a low of 2.16 (September 2009) to a high of 4.44 (February 2010). Finally, as expected, the standard errors tend to widen over time, although all of the coefficients remain statistically significant at the 95% level.

Figure 4 parallels the results from Figure 3, using employment in the government, health, and education sectors. The broad patterns present in Figure 3 are also present in Figure 4. Again, the coefficient increases for July 2009, and the standard errors increase over time. However, the ratio of the standard errors to the point estimate is larger than for total employment. Comparing the magnitudes between the two timing figures shows that in all months, the estimates for total employment are larger than those for government employment, with the gap increasing through 2009 and peaking in early 2010. This pattern is consistent with the government employment results reflecting the relatively immediate direct effect of states and state-funded establishments not having to lay off workers, while the total employment results include the lagging induced effects of households responding to higher disposable income.

3.6 Robustness Checks and Extensions

3.6.1 Falsifications Tests

Our identifying assumption is that, conditional on our control variables, states that had higher pre-recession Medicaid spending would not have experienced different employment outcomes from states that were lower spenders in the absence of the increase in FMAP. One way of assessing this assumption is to consider if the effects we estimate are larger than the relationship between Medicaid spending and employment growth that existed prior to the period of interest.

Figure 5 reports the second stage coefficients for placebo tests using data that begin in January 2000 and end in December 2008. To parallel our baseline specification, we consider seven-month changes in both total nonfarm employment and employment in government, health, and education. We then run our IV estimates on each overlapping seven-month period, for a total of 101 regressions. We rank the coefficients based on their magnitude and report the empirical CDF. For comparison, we also show the second stage estimate run on the baseline period, December 2008 to July 2009, with a vertical line.

The results show two key patterns. First, the estimates are centered around 0; the empirical median of the estimate is 0.00 for total nonfarm and 0.11 for government, health and education. That is, in the years before the ARRA was passed, there is little evidence to suggest that high and low Medicaid spending states experienced systematically different employment trends. Second, our baseline estimates of both total nonfarm and government, health, and education employment are large relative to the coefficients in the period before the ARRA. For total employment, our result is larger than all but seven of the 101 pre-ARRA estimates. For government, health, and education, our estimate is larger than all but three of the pre-ARRA estimates. Both pieces of evidence increase our confidence that the estimates reported above are capturing the effect of the ARRA rather than underlying differences between high and low Medicaid spending states.

3.6.2 Other Robustness Checks

Our baseline specification allows for the possibility that high and low Medicaid spending states were on different pre-existing employment trends by controlling for a linear lag of the change in employment. This subsection addresses the concern that a linear lag may not be a sufficient statistic for pre-existing employment trends. Specifically, we report results allowing for a more flexible pre-existing trend and using a state's pre-treatment industry composition and the change in employment by industry in other states to impute employment change during the treatment period, following Bartik (1991) and Blanchard and Katz (1992). The latter require detailed industry data from the QCEW, a dataset that is not available on a seasonally adjusted basis and that does not have representative coverage of the government sector.[20] We therefore present results for the change in total nonfarm employment, and for December 2008 to December 2009 in the specifications that use the imputed employment predictor.[21]

Model (1) of Table 5 shows the second stage coefficient when we re-run our baseline specification, replacing the linear lag of employment change with an autoregressive model estimated using 18 years of data prior to the sample period to forecast a state's employment change from December 2008 to July 2009.[22] The second stage coefficient is 2.89, essentially unchanged from

[20] According to Bureau of Labor Statistics (2008), 5% of total state and local government workers are not covered by the QCEW.

[21] We perform the imputed employment calculation at the four-digit level because of disclosure limitations that eliminate observations at higher levels of detail.

[22] Specifically, the logarithm of total employment was regressed against a time variable and nine monthly lags of itself. The coefficients were then used on data through December 2008 to forecast employment from January 2009 through July 2009. Note that this control variable is helpful if the patterns of employment changes over the 18 years prior to our sample period remained unchanged during our sample period. Because our period involves

the value of 2.83 in the specification with the linear lag presented in table 3. Models (2)-(3) add a quadratic and cube of the lagged employment change to account for the fact that the serial correlation in changes in per capita employment may be non-linear, again with essentially no effect on the coefficient of interest. The next three columns shift the end-month to December 2009 in order to accommodate our measure of imputed QCEW employment change based on pre-ARRA industrial composition. The appendix contains further details of this variable's construction. As a benchmark, column (4) gives the baseline result that appears in Figure 3. Column (5) adds the imputed employment change, with very little effect on the FMAP coefficient. Column (6) replaces the outcome variable with QCEW data and again finds essentially the same result.[23] In sum, the relationship between FMAP transfers and employment growth appears very robust to our alternative methods of generating an employment change counterfactual.[24]

3.7 Discussion

3.7.1 Job-years

Our results indicate a positive and robust relationship between receiving FMAP transfers and relative employment outcomes. To interpret the magnitude of the estimates, we can translate the regression coefficients into the increase in job-years from \$100,000 of marginal state fiscal relief. This requires two assumptions. First, we assume that FMAP outlays received through June 2010 have no employment effects beyond June 2010. If instead the employment effects linger beyond June 2010, then our estimate of job-years is a lower bound. Second, we assume that transfers to states after June 2010 do not influence employment changes before June 2010. This assumption is likely to be valid (at least for state employment) if states are unable to shift

the most severe recession since World War II, this assumption may not be valid.

[23]The closeness of the coefficients in columns (5) and (6) reflects the benchmarking of the CES to the QCEW.

[24]In results reported in an online appendix, we also experimented with other possible control variables that might capture channels similar to those discussed in the text. These include the generosity of states' unemployment insurance systems and the presence of a Democratic governor in February 2009 as proxies for political factors, an index of budget restrictiveness from the Advisory Committee on Intergovernmental Relations to address the concern that the 2007 Medicaid spending levels might be correlated with state budget rules, and the degree of house price appreciation during the mid-2000s as a proxy for economic conditions. The results reported in Tables 3 and 4 are robust to the inclusion of these additional controls.

money across fiscal years.

Under these assumptions, the increase in job-years from \$100,000 of FMAP outlays can be calculated by taking the integral under the timing charts (Figures 3 and 4) and dividing by 12 to convert job-months to job-years. Our point estimates suggest that \$100,000 of marginal state fiscal relief increases state employment by 3.8 job-years, 3.2 of which are outside the government, health, and education sectors. The associated p-value for this calculation is 0.018 for total employment, while the p-value for total employment excluding the government, health, and education sectors is 0.010. Dividing \$100,000 by 3.8 job-years yields a cost per job of \$26,000.

When considering the generalizability of the results, it is important to consider both the intended and apparently realized fungibility of the funds. As noted above, the text of the bill made clear that the funds were for general obligations, and states reported using them for this purpose. Indeed, results disaggregating the government, health, and education employment results suggest that only about a quarter of the increase in employment was in the health sector, with another quarter in education and the other half in state and local government.[25]

In the context of our broader understanding of the costs and benefits of fiscal stimulus, state fiscal relief, in particular, may be a particularly low-cost means of supporting employment during a recession. Furthermore, the jobs increases were rapid, perhaps because "shovel-ready" projects were often not necessary; in many cases, state and local governments only needed to avoid cuts.

3.7.2 Comparison to the Literature

This paper contributes to a literature which uses cross-state variation to estimate fiscal multipliers. We do this using the most recently-available evidence in a context in which the parameter being estimated has direct relevance to a policy question: how much is employment increased by state fiscal relief during a recession? Although estimated in quite different settings, Suarez-Serrato and Wingender (2011) and Shoag (2011) find estimates which are remarkably similar

[25] When using the change from December 2008 to July 2009 in state and local government employment as the dependent variable in our baseline regression, we estimate a coefficient of 0.65 (SE = 0.26) on the FMAP transfers, while changes in health and education employment yield coefficients of 0.21 (SE = 0.10) and 0.29 (SE = 0.11) respectively.

to our estimate of cost per job, at $30,000 and $35,000 per year respectively.[26]

While the political debate has focused on the effect of fiscal stimulus on employment, the academic literature more commonly estimates the government purchases multiplier for output. Also using cross-state variation, Nakamura and Steinsson (2011) find an open-economy government purchases multiplier of 1.5, and Shoag (2011) finds an output multiplier of 2.1. Our findings are consistent with this range. We roughly map our results to an output multiplier as follows: in 2008 average compensation in both the total economy and state and local government was $56,000 per employee. If total compensation equals the marginal product of labor and workers affected by state fiscal relief have this same average compensation, this result would imply an output multiplier for a dollar of transfers of about 2.[27] Given that the results from this cross-state approach do not incorporate general equilibrium effects, cross-state multipliers, or the response of a monetary authority, we interpret this multiplier as only suggestive of the national multiplier of policy interest.[28]

A few other papers have also studied parts of the ARRA. Wilson (2011) and Feyrer and Sacerdote (2011) report costs per job of $114,312 and $170,000, respectively, but their numbers are not directly comparable to the 3.8 jobs per $100,000 reported above because they do not account for the timing of job creation, and they cover other portions of the stimulus.[29] Sahm et al. (2010) find a relatively modest impact from the Making Work Pay tax cut. Mian and Sufi (2010) find that the relatively small ($3 billion) "Cash for Clunkers" program (which was separate from the ARRA but implemented concurrently during the summer of 2009) had little net effect on purchases.[30]

[26]See also Neumann et al. (2010) and Fishback and Kachanovskaya (2010) for studies using cross-sectional variation during the Great Depression.

[27]This calculation assumes that capital stays fixed. Data on average compensation per employee come from the Bureau of Economic Analysis GDP-by-Industry accounts. The output multiplier equals the jobs multiplier multiplied by value-added per job (equivalent to a worker's marginal product), or (3.8/$100,000)*$56,000=2.13.

[28]Ramey (2011) surveys the literature on national output multipliers. Our estimate is at the upper end of her preferred range, consistent with recent empirical work on state-dependent output multipliers that finds higher multipliers occur during depressed demand conditions such as prevailed during our period of study (Auerbach and Gorodnichenko forthcoming). Nakamura and Steinsson (2011) and Shoag (2011) explore the theoretical mapping from these estimates of local fiscal multipliers to the national multiplier in an open economy setting.

[29]Wilson's results for total job creation are closest to ours. This is not surprising, since his paper adopts our instrument, along with using simulated instruments for highway and education spending. The Feyrer-Sacerdote number corresponds only to "direct jobs" funded by the ARRA. Conley and Dupor (2011) find a positive effect of ARRA transfers on government employment, but no positive effect on employment outside of government.

[30]The Obama Administration (Council of Economic Advisers 2010), Congressional Budget Office (2010) and private forecasters and academics (Blinder and Zandi 2010) have all evaluated the ARRA using a multiplier

3.8 Mechanism

The ARRA transfers reached states in dire fiscal condition. During the 2009 fiscal year, 43 states faced budget gaps totaling more than $60 billion (National Conference of State Legislatures 2009). Almost all states have balanced budget requirements.[31] Thus, the large budget gaps necessitated that they take action by cutting expenditures, raising revenues, or drawing from their "rainy day" funds or end of year balances, which are used to smooth revenue across years.[32] Indeed, by December 2008, 22 states had made or announced cuts to their expenditures totaling $12 billion.[33] By July 2009, 42 states had made cuts to their expenditures totaling more than $30 billion, and 30 states had increased taxes or fees to boost their revenues.[34]

There are essentially only three ways in which states could use the ARRA state fiscal relief funds: to alleviate program cuts, to prevent or lower tax and fee increases, or to contribute to their end of year balances (which include their rainy day funds). As long as the states did not respond to the federal transfers by completely siphoning them to their end of year balances, the observed employment responses could come from multipliers on the states' spending or tax actions. The results in Section 5 suggest that the ARRA funds were at least partially used to avoid program cuts, since a concentration of the employment effects appears to have occurred in sectors (government, health, and education) which are reliant on state funds. That total employment beyond those sectors is also affected positively by the federal fiscal relief suggests that there is a source of spillovers, arising from higher disposable income due to either the wages of the direct hires or lower net taxes because of fewer tax or fee increases.[35]

We can directly test the necessary condition that FMAP outlays affected spending or tax

---

model based on historical relationships between government spending, output and employment. These studies tend to find effects similar to or slightly smaller in magnitude than those in the current study for state fiscal relief. However, they are all calibrated models, whereas the current study uses empirical estimation. Council of Economic Advisers (2009) reported preliminary results of those in the current paper.

[31] All states, except for Vermont, have some version of balanced budget requirements as reported by the National Association of State Budget Officers (2008a). Poterba (1994) gives an overview of the varying requirements.

[32] From National Association of State Budget Officers (2008a). Kansas and Montana do not have budget stabilization (or "rainy day") funds. However, they, like other states, may use surpluses from the prior fiscal year to cushion any fiscal difficulty in the next.

[33] From National Association of State Budget Officers (2008b).

[34] Budget cuts from the National Association of State Budget Officers (2009a). The $32 billion figure refers to the expenditure cuts in fiscal year 2009 alone. Tax increases from Johnson, Nicholas, and Pennington (2009).

[35] Several recent empirical studies have found a positive effect of lower taxes or higher transfers on economic outcomes (Johnson, Parker and Souleles 2006; Sahm, Shapiro and Slemrod 2009; Romer and Romer 2010).

actions by regressing the change in end of year balances from 2008 to 2009 on instrumented FMAP outlays and controls. Models (1) – (3) of Table 6 summarize the results of these regressions. All else equal, if states that received more FMAP money decreased their balances less, we would expect a positive and significant coefficient on FMAP outlays, with the extreme case that if all of the money were saved we would expect a coefficient of 1. Instead, the estimates in (1) – (3) are small in magnitude, negative in all three of the specifications, and never significantly different from 0.[36] Furthermore, the models allow us to reject the null that half of transfers were saved by states at the 99% confidence level for two regressions and at the 95% confidence level for the third, confirming that at least some of the funds were used to slow either budget cuts or tax increases. Models (4) – (6) of Table 6 repeat the same exercise, using the change in end of the year balances from 2009 to 2010 as the dependent variable, and yield similar results.[37] In summary, although the regressions have wide standard errors, the point estimates provide no evidence to suggest that states are retaining the transferred money in the form of end of year balances or rainy day funds.[38]

To determine if states that received more transfers cut their budgets less, we ran specifications that parallel those in Table 6 where the outcome variable was the change in expenditure (normalized by a state's 16+ population) between 2008 and 2009 and between 2009 and 2010. Unfortunately, the results from this regression are quite noisy, and we can neither reject the null that all of the money was spent on reducing budget cuts (which would imply a coefficient of one) nor the null that none of the money was spent on reducing budget cuts (which would

[36]We exclude Alaska, a state that experienced a per 16+ population decline in its end of year funds that was more than ten times larger than that of the next largest states. When we include Alaska, we also cannot reject the null that the coefficient on total FMAP outlays per person is equal to 0 (p-value for the bivariate IV regression is 0.435 for changes from 2008 to 2009 and is 0.311 for changes from 2009 to 2010). In addition, because the National Association of State Budget Officers does not provide data on DC, we exclude it from our regressions.

[37]Poterba (1994) and Alt and Lowry (1994) examine how the states' balanced budget rules affect their responses to deficits and find that, in response to a positive deficit shock, states cut expenditures or raise taxes within either the current or the following fiscal year. This is consistent with the findings that a federal transfer (a negative deficit shock) would impact expenditures or taxes.

[38]These results contradict those of Cogan and Taylor (2010), who find using aggregate time-series data that ARRA Medicaid spending increased aggregate state net lending as measured in the National Income and Product Accounts. Given the unusual nature and length of the 2007-09 recession and its effect on state budgets, it is possible that aggregate time-series regressions misattribute the effect of the worsening recession and the eventual binding of state balanced budget requirements on net lending to the introduction of the FMAP expansion. Alternatively, it is possible that all states increased their saving in response to the FMAP transfers by the same dollar amount per capita, regardless of the amount of FMAP transfers actually received.

yield a coefficient of zero).[39] Results using changes in a state's revenue are similarly noisy, and thus do not provide conclusive evidence about the use of funds to reduce tax or fee increases. Further research into how states optimize over the margins of tax and spending when faced with an altered budget constraint would be a worthwhile area of future study.

3.9 Conclusion

This paper estimates the employment effects of a relatively unstudied form of government macroeconomic intervention that took center stage in the recent ARRA: fiscal relief to states during a downturn. We exploit cross-state variation in transfer receipts that comes from pre-recession differences in Medicaid spending. All else equal, states that spent more money on Medicaid before the recession received more money from the federal government. We confront the major threat to identification—that states that spent more money on Medicaid may be on differential employment trends from states that spent less—in several ways, including adding regional fixed effects and other control variables as well as conducting placebo tests. Our baseline specifications suggest that $100,000 of marginal spending increased employment by 3.8 job-years, 3.2 of which are outside the government, health, and education sectors.

The fact that state fiscal relief may be an effective tool to cushion employment losses in recessions raises two questions. First, if the employment effects of state fiscal relief are substantial, should the federal government play a larger role in providing revenue to states during recessions? When designing state fiscal relief, federal planners face a tradeoff between providing relief to states experiencing critical budget situations and minimizing perverse incentives for state policy makers. If states expect to receive federal aid during recessions, they may not save sufficiently during boom times. This moral hazard is compounded if federal aid targets states with larger budget shortfalls, which might be desirable because aid distributed using a non-need-based formula would likely produce smaller employment effects. An important area of future research is to determine the extent to which these tradeoffs limit the potential for state fiscal relief to be an effective tool for cushioning job losses during recessions.

[39]Fiscal year 2008 expenditure data and the enacted tax and fee data are from the National Association of State Budget Officers (2009b). Fiscal year 2009 and 2010 expenditure data are from the National Association of State Budget Officers (2010).

Second, why are states unable to save money during economic booms and use this savings during recessions? Because most states have adopted balanced budget legislation, states cannot borrow money during recessions to smooth fluctuations. As a substitute, most states have a "rainy day" fund that allows them to avoid the requirement of literally balancing their budget every fiscal year. However, political economy considerations make saving difficult for democratic governments (Alesina and Tabellini 1990; Amador 2003), and most states have essentially no restrictions on when they must contribute to their rainy day fund.[40] For example, during the 1990s economic boom, states increased spending and cut taxes rather than contributing to their rainy day funds.[41]

To help solve these political economy problems, some states have considered adopting rules that would require the state to contribute to their rainy day fund during healthy economic times. For example, a state could be required to contribute to its fund when the unemployment rate in the state falls below a given threshold, and be permitted to tap into its fund when the unemployment rate rises to a sufficiently high level. These regulations have the advantage of constraining politicians, while helping to alleviate some of the fiscal strain induced by a recession. The evidence presented in this paper, though it concerns funding from the federal government, also informs the impact of additional state resources on state-level employment, and suggests that these and other rules may help states boost employment during recessions. Future research could focus on additional benefits, as well as costs, from state fiscal relief and state budgetary rules.

[40]The majority of states have requirements that they contribute to their rainy day funds only if the budget has a surplus. However, because states determine when they have a surplus by setting the level of taxes and spending, in practice such requirements impose few restrictions on states' contributions to their rainy day funds.

[41]See Zahradnik and Ribeiro (2003) and the National Conference of State Legislatures (2004).

3.10 Appendix

3.10.1 Sources and Descriptions of Baseline Control Variables

- **Region effects**: We include dummy variables for each of the 9 census defined divisions. Definitions are given at http://www.census.gov/geo/www/us_regdiv.pdf.

- **Lagged change in employment:** We control for the lagged value of the outcome variable. Specifically, if the outcome variable is change in total (government, health, and education) employment from month m to month m', the lagged change in employment will be the change in total (government, health, and education) employment from month $m - 7$ to month m. In our baseline specification where the outcome variable is change in employment from December 2008 to July 2009, the lagged change in employment is the change in employment from May 2008 to December 2008. This 7 month lag was chosen to follow the 7 month period used for the outcome variable in our baseline specification.

- **GDP per potential worker**: We use a state's 2008 GDP, normalized by a state's 16+ population, from the Bureau of Economic Analysis.[42]

- **Employment manufacturing share**: From the Census Bureau, we also control for the share of the civilian employed population 16 years and older that is in the manufacturing sector. Data are from the American Community Survey and are averaged over 2005 – 2007 to reduce measurement error.

- **State 2008 population and 2008 16+ population**: Data are from the US Census.

- **2004 Kerry share:** The state's share of voters who cast ballots for Senator Kerry in the 2004 United States presidential election.

- **Union share**: Share of workers in a union, from the Bureau of Labor Statistics.[43]

3.10.2 Description of Imputed Employment

As a robustness check, we control for a measure of imputed employment that uses information on the change in employment in each industry in the rest of the country and the initial industry distribution in each state to impute an expected employment change. Specifically, for state j

[42] See http://www.bea.gov/newsreleases/regional/gdp_state/2009/pdf/gsp0609.pdf.
[43] From http://www.bls.gov/news.release/pdf/union2.pdf.

and industry k define the percent change in employment in industry k in all other states as:

$$\%\Delta E^{-j,k} = \frac{\sum_{s\neq j} E_t^{s,k}}{\sum_{s\neq j} E_{t-1}^{s,k}} - 1 \tag{3.2}$$

The imputed employment change for industry k in state j is then:

$$\%\Delta \hat{E}^{j,k} = E_{t-1}^{j,k} \cdot \%\Delta E^{-j,k} \tag{3.3}$$

The total imputed employment change for state j is the sum over all industries:

$$\Delta \hat{E}^{j} = \sum\nolimits_k \Delta \hat{E}^{j,k} \tag{3.4}$$

We implement (3.2)-(3.4) using the QCEW December 2008 and December 2009 state-level flat files.[44] The QCEW provides employment by NAICS code and ownership status (private, federal government, state government, and local government). The QCEW suppresses output for state-industry-ownership rows where the number or concentration of firms does not surpass a minimum disclosure threshold. Letting $o \in \{private, federal, state, local\}$ define ownership status, we set $E_{t-1}^{j,k,o} = 0$ for any state-industry-ownership row with suppressed output. In practice, missing the disclosure threshold correlates well with small size, so this assumption is quite mild. Nonetheless, we define industries using four digit rather than six digit NAICS codes in order to minimize disclosure limitations. Using six digit industries yields very similar results. Since we are interested in industry variation, we collapse the QCEW data on ownership status before implementing (3.2)-(3.4).

[44]The files can be downloaded from ftp://ftp.bls.gov/pub/special.requests/cew/yyyy/state for yyyy={2008,2009}.

Tables

Table 1: Summary Statistics

	Mean	Std. Dev.	Minimum	Median	Max
Outcome Variables, Per 1,000 People 16+					
Change in total nonfarm employment, Dec. 2008 → July 2009	-18.76	7.15	-38.84	-18.23	3.11
Change in govt, health, & education, Dec. 2008 → July 2009	0.97	2.06	-2.13	0.53	9.11
Payout Variables and Instrument, Per Person 16+					
Total ARRA outlays through June 2010	$1,002	$323	$586	$960	$2,940
Total FMAP outlays through June 2010	$250	$90	$103	$235	$507
Total FMAP and SFSF outlays through June 2010	$373	$88	$176	$358	$583
2007 Medicaid spending (instrument)	$1,328	$454	$624	$1,227	$2,854
Control Variables					
Employment in manufacturing, percent	11.03	4.28	1.40	11.00	20.30
Vote share Kerry (2004), percent	46.52	10.38	26.00	47.02	89.18
Union share, percent	11.16	5.49	3.00	10.40	25.20
GDP per person 16+ ($1,000)	49.20	17.20	31.91	46.28	154.89
Population 16 and older (millions)	4.60	5.13	0.41	3.32	27.85
Change in total nonfarm employment, May 2008 → Dec. 2008	-11.04	6.91	-33.42	-11.25	2.60
Change in govt, health, & education, May 2008 → Dec. 2008	1.73	1.27	-1.44	1.75	6.30

Notes: See text and data appendix for sources. Note that "government" excludes federal government employees. All employment data are seasonally adjusted and reported per 1,000 people 16+.

Table 2: First Stage Regressions

	(1)	(2)	(3)	(4)
2007 Medicaid spending (instrument)	0.18***	0.15***	0.16***	0.15***
	(0.01)	(0.01)	(0.01)	(0.01)
Region fixed effects?		X	X	X
Vote share Kerry		X	X	X
Union share		X	X	X
GDP per person 16+		X	X	X
Employment in manufacturing		X	X	X
State population		X	X	X
Lagged total employment change May 2008 to Dec 2008			X	
Lagged government, health, and education employment change May 2008 to Dec 2008				X
Observations	51	51	51	51
R-squared	0.84	0.93	0.93	0.93
Mean of dependent variable	250.23	250.23	250.23	250.23

Notes: The outcome variable for each regression is total FMAP outlays per individual 16+ in a state, through June 30, 2010. The variable is measured in $100,000 per person 16+. See text and data appendix for sources. Note that "government" excludes federal government employees. Robust standard errors are in parentheses.

* significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table 3: Total Employment Baseline Results

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Total FMAP payout per person 16+ ($100,000)	2.94**	1.88	0.82	4.72***	4.61***	2.83***
	(1.35)	(1.83)	(1.06)	(1.31)	(1.57)	(1.01)
Vote share Kerry (2004), percent/10,000		0.28	2.1		-0.79	1.14
		(2.02)	(1.57)		(1.59)	(1.14)
Union share, percent/10,000		-4.26	-2.93		-6.00**	-4.29**
		(3.60)	(2.17)		(2.91)	(2.01)
GDP per person 16+ ($1,000,000)		0.01	-0.03		-0.01	-0.04
		(0.07)	(0.06)		(0.06)	(0.05)
Employment in manufacturing, percent/10,000		-10.05***	-6.61***		-9.75***	-6.83***
		(3.05)	(2.39)		(2.82)	(2.12)
State population 16+, billions		-0.43***	-0.33***		-0.46***	-0.36***
		(0.12)	(0.08)		(0.10)	(0.08)
Lagged total employment change May 2008 to Dec 2008			0.42*			0.37**
			(0.21)			(0.17)
Region fixed effects?		X	X		X	X
Observations	51	51	51	51	51	51
Mean of dependent variable * 1,000	-18.76	-18.76	-18.76	-18.76	-18.76	-18.76

Note: The outcome variable for each regression is the seasonally adjusted change in total non-farm employment per individual 16+ in a state, from December 2008 to July 2009. The main variable of interest is total ARRA FMAP payouts through June 30, 2010. Specifications (4) - (6) instrument total ARRA FMAP payouts with pre-recession Medicaid spending as described in the text. See text and data appendix for sources. Robust standard errors are in parentheses.

* significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table 4: State and Local Government, Health, and Education

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Total FMAP payout per person 16+ ($100,000)	0.43	0.34	0.30	0.99*	1.19***	1.17***
	(0.53)	(0.44)	(0.40)	(0.54)	(0.37)	(0.36)
Vote share Kerry (2004), percent/10,000		-0.76*	-0.64		-1.10***	-1.01***
		(0.39)	(0.39)		(0.30)	(0.32)
Union share, percent/10,000		0.16	0.33		-0.38	-0.26
		(0.95)	(0.96)		0.76	0.8
GDP per person 16+ ($1,000,000)		0.07***	0.07***		0.06***	0.06***
		(0.02)	(0.02)		(0.02)	(0.02)
Employment in manufacturing, percent/10,000		-1.93**	-1.84*		-1.84**	-1.77**
		(0.89)	(0.96)		(0.84)	(0.88)
State population 16+, billions		-0.11***	-0.10**		-0.12***	-0.11***
		(0.03)	(0.04)		(0.03)	(0.03)
Lagged government, health, and education employment change May 2008 to Dec 2008			0.18			0.14
			(0.18)			(0.17)
Region fixed effects?		X	X		X	X
Observations	51	51	51	51	51	51
Mean of dependent variable * 1,000	0.97	0.97	0.97	0.97	0.97	0.97

Note: The outcome variable for each regression is the seasonally adjusted change in total employment in state and local government, health, and education per individual 16+ in a state, from December 2008 to July 2009. The main variable of interest is total ARRA FMAP payouts through June 30, 2010. Specifications (4) - (6) instrument total ARRA FMAP payouts with pre-recession Medicaid spending as described in the text. See text and data appendix for sources. Note that "government" excludes federal government employees. Robust standard errors are in parentheses.
* significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table 5: Total Employment Robustness Checks

	Dec 2008 to July 2009			Dec 2008 to Dec 2009		
	CES			CES		QCEW
	(1)	(2)	(3)	(4)	(5)	(6)
Total FMAP payout per person 16+ ($100,000)	2.89**	2.80***	2.79**	2.92**	2.74**	2.81**
	(1.23)	(0.95)	(1.29)	(1.44)	(1.34)	(1.27)
Baseline controls	X	X	X	X	X	X
Forecased emp ch, Dec 2008 to July 2009, CES	X					
Lagged total emp ch, July 2008 to Dec 2008, CES		X	X	X	X	X
Lagged total emp ch squared, July 2008 to Dec 2008, CES		X	X			
Lagged total emp ch cubed, July 2008 to Dec 2008, CES			X			
Imputed emp ch, Dec 2008 to Dec 2009, QCEW					X	X
Observations	51	51	51	51	51	51
Mean of dependent variable * 1,000	-18.76	-18.76	-18.76	-21.81	-21.81	-22.17

Note: In (1) - (3), the outcome variable is the change in total employment from December 2008 to July 2009 from the CES. In (4) and (5), the outcome variable is the change in total employment from December 2008 to December 2009 from the CES. For (6), the the outcome variable is the change in total employment from December 2008 to December 2009 from the QCEW. The main variable of interest is total ARRA FMAP payouts through June 30, 2010. The construction of the instrument is described in the text. "Baseline controls" are vote share Kerry, union share, GDP per person 16+, employment in manufacturing, state population, and region fixed effects. Sources of control variables are detailed in the data appendix. See the text for the construction of forecasted and imputed employment change. Robust standard errors are in parentheses.
* significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

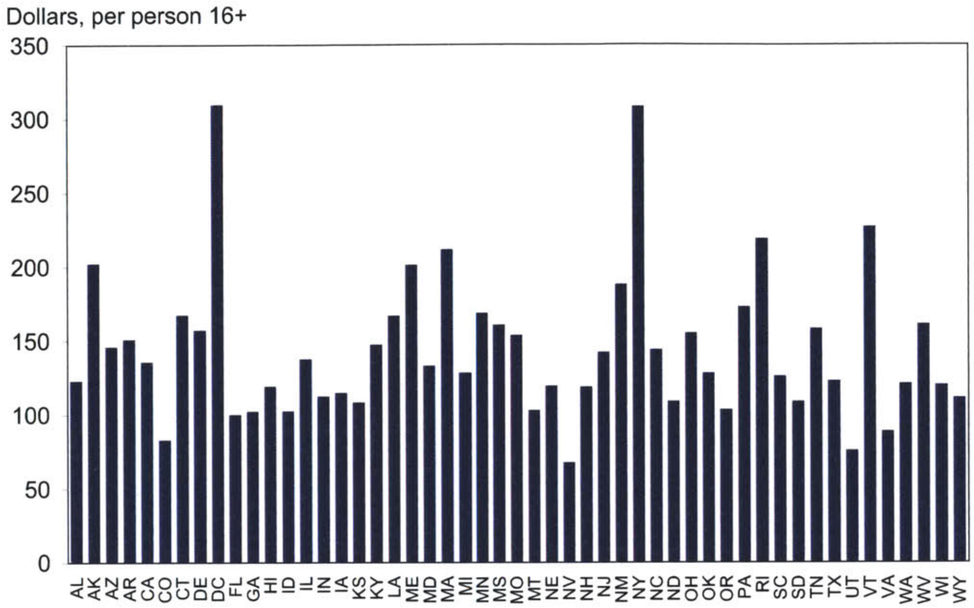
Table 6: Transmission Mechanism

	Rainy Day Fund, change 2008 to 2009 (IV)			Rainy Day Fund, change 2009 to 2010 (IV)		
	(1)	(2)	(3)	(4)	(5)	(6)
Total FMAP payout per	-0.26	0.01	-0.14	-0.04	0.08	0.04
person 16+ ($100,000)	(0.18)	(0.23)	(0.21)	(0.09)	(0.18)	(0.17)
Region fixed effects?		X	X		X	X
Includes lagged employment?			X			X
Excludes Alaska?	X	X	X	X	X	X
Missing DC?	X	X	X	X	X	X
Observations	49	49	49	49	49	-17.84
Mean of dependent variable (*100,000)	-29.22	-29.22	-29.22	-17.84	-17.84	-17.84

Note: The outcome variable for (1) - (3) is change in a state's rainy day fund, in $100,000, per person 16+, from fiscal year 2008 to fiscal year 2009. The outcome variable for (4) - (6) is the change in a state's rainy day fund, in $100,000, per person 16+, from fiscal year 2009 to fiscal year 2010. Data are from the National Association of State Budget Officers (NASBO) Fiscal Survey of the States. The fiscal 2008 rainy day fund data come from the Fall 2009 Fiscal Survey, and the fiscal 2009 and 2010 rainy day fund data come from the Spring 2010 Fiscal Survey. All specifications exclude DC due to missing data. They also drop Alaska, an outlier in terms of the change in the state rainy day fund. Robust standard errors are in parentheses.
* significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

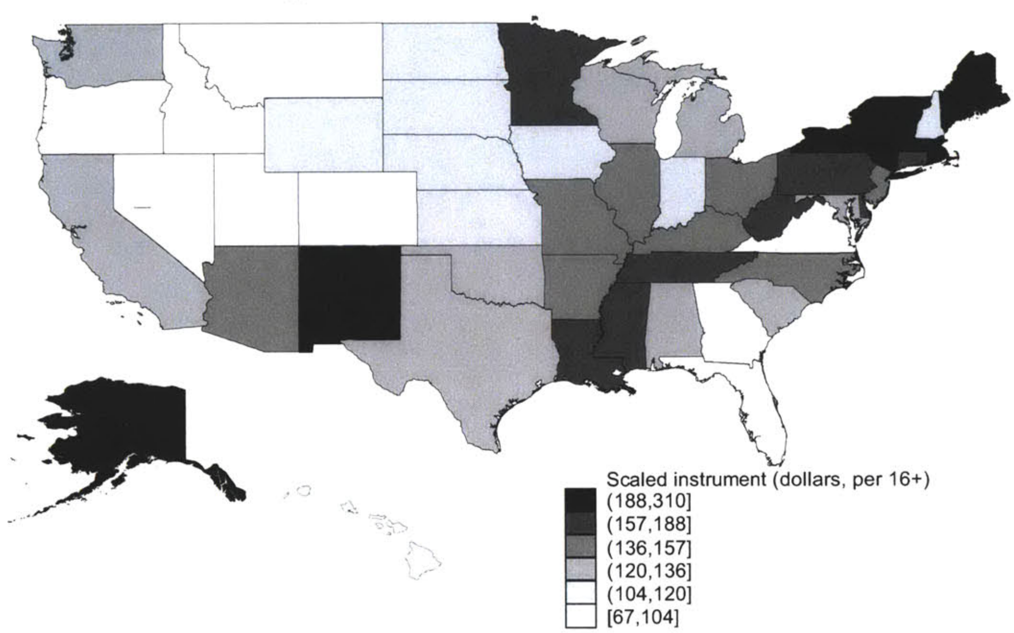
Figures

Figure 1: Value of Scaled Instrument



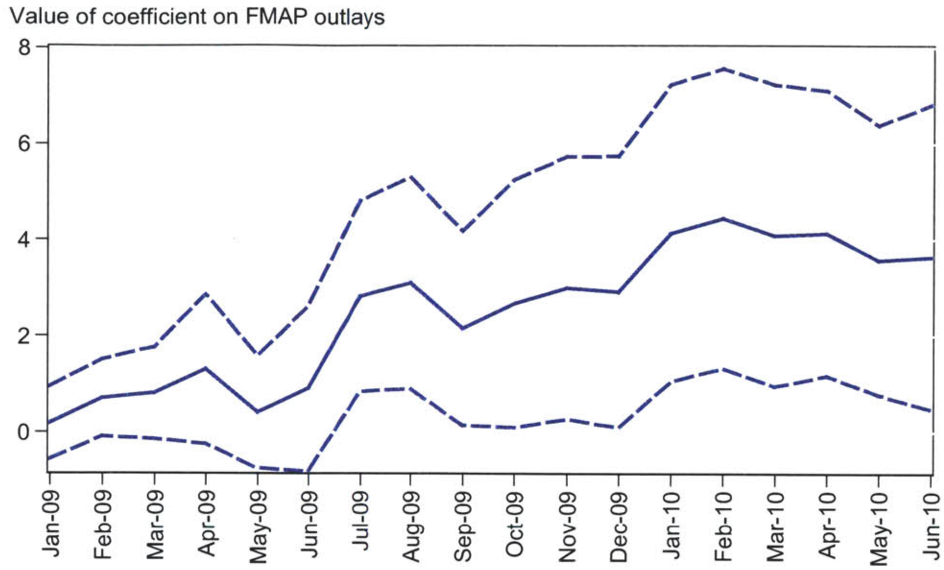
Note: The value of the scaled instrument is 0.062 * state's fiscal year 2007 Medicaid spending * 21/12. See text for full details. Data are from the Center for Medicaid Services, Data Compendium, Table VII.1.

Figure 2: Value of Scaled Instrument



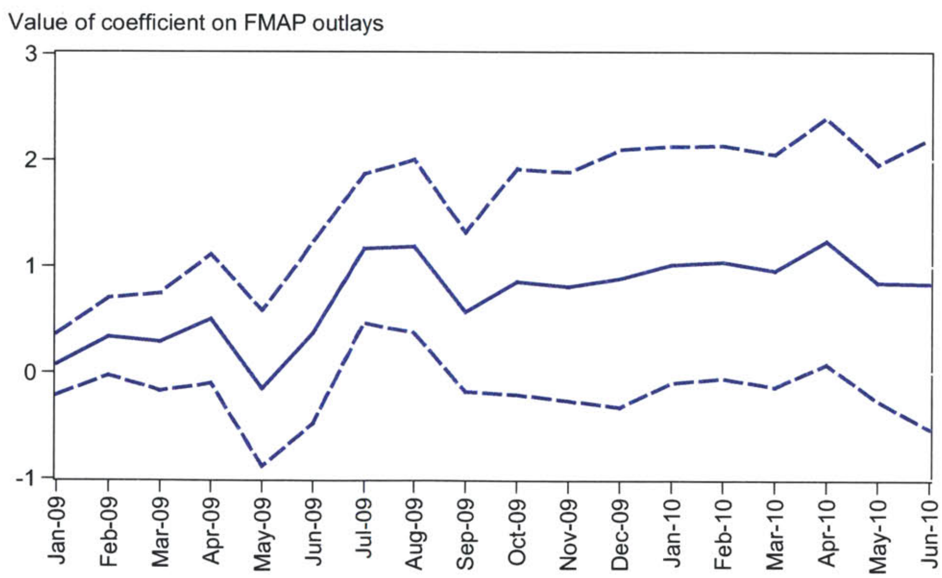
Note: The value of the scaled instrument is 0.062 * state's fiscal year 2007 Medicaid spending (per person 16+) * 21/12. See full text for details. Data are from the Center for Medicaid Services, Data Compendium, Table VII.1.

Figure 3: Total Nonfarm Second Stage Coefficients



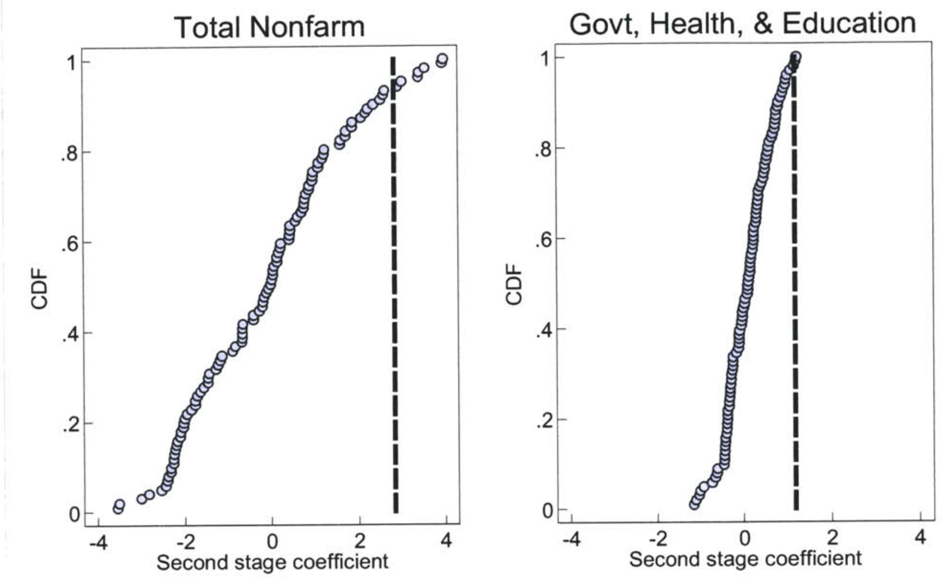
Note: This chart displays the second stage coefficient for regressions where the outcome variable is the change in seasonally adjusted employment between December 2008 and the month indicated on the x-axis. The variable of interest is total FMAP outlays. Regressions include the full set of controls. The 95% confidence interval, derived from robust standard errors, is plotted in dashed lines.

Figure 4: Government, Health and Education Second Stage Coefficients



Note: This chart displays the second stage coefficient for regressions where the outcome variable is the change in seasonally adjusted employment between December 2008 and the month indicated on the x-axis. The variable of interest is total FMAP outlays. Regressions include the full set of controls. The 95% confidence interval, derived from robust standard errors, is plotted in dashed lines.

Figure 5: Placebo Results



Note: Plots results of second stage regressions, where the outcome variable is seasonally adjusted change in employment for each overlapping 7 month period, starting in Jan 2000 and ending in Dec 2008. All regressions include the full set of control variables. Coefficient from Dec 2008 to July 2009 is indicated with the vertical line. Note that government excludes federal government employment.

Bibliography

[1] Alesina, Alberto and Guido Tabellini. 1990. "A Positive Theory of Fiscal Deficits and Government Debt in Democracy." *Review of Economics Studies* 57 (3): 403-414.

[2] Alt, James and Robert Lowry. 1994. "Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States." *American Political Science Review*, 88 (4): 811-828.

[3] Amador, Manuel. 2003. "Political Compromise and Savings." http://www.stanford.com/~amador/savings.pdf.

[4] Aschauer, David A. 1989. "Is Public Expenditure Productive?" *Journal of Monetary Economics* 23 (2): 177-200.

[5] Ashenfelter, Orley and George E. Johnson. 1969. "Bargaining Theory, Trade Unions, and Industrial Strike Activity," *American Economic Review* 59 (1): 35-49.

[6] Allen, Steven G. 1988. "Unions and Job Security in the Public Sector," in *When Public Sectors Unionize*, pp. 271-304.

[7] Auerbach, Alan and Yuriy Gorodnichenko. Forthcoming. "Measuring the Output Responses to Fiscal Policy." *American Economic Journal: Economic Policy.*

[8] Bartik, Timothy. 1991. Who Benefits from State and Local Economic Development Policies? Michigan: W.E. Upjohn Institute for Employment Research.

[9] Bennett, James T. and William P. Orzechowski. 1983. "The Voting Behavior of Bureaucrats: Some Empirical Evidence," *Public Choice* 41 (2): 271-283.

[10] Blanchard, Olivier and Lawrence Katz. 1992. "Regional Evolutions." *Brookings Paper on Economic Activity* 23 (1): 1-75.

[11] Blanchard, Olivier and Lawrence Summers. 1986. "Hysteresis and the European Unemployment Problem," in *NBER Macroeconomics Annual 1986, Volume 1*, ed. Stanley Fischer, MIT Press, pp. 15-90.

[12] Bland, Robert L. 2005. *A Revenue Guide for Local Government*, 2nd Edition, International City/County Management Association.

[13] Blinder, Alan and Mark Zandi. 2010. "How the Great Recession was Brought to an End." http://www.economy.com/mark-zandi/documents/End-of-Great-Recession.pdf.

[14] Bradford, David and Wallace E. Oates. 1971. "The Analysis of Revenue Sharing in a New Approach to Collective Fiscal Decisions", *Quarterly Journal of Economics* 85 (3).

[15] Bruce, Donald, William F. Fox and M. H. Tuttle. 2006. "Tax Base Elasticities: A Multi-State Analysis of Long-Run and Short-Run Dynamics," *Southern Economic Journal* 73 (2): 315-341.

[16] Bureau of Labor Statistics. 1990-2010. "Current Employment Statistics: State Employment and Unemployment." United States Department of Labor. ftp://ftp.bls.gov/pub/time.series/sm/ (accessed June 8, 2011).

[17] Bureau of Labor Statistics. 2008. Employment and Wages, Annual Averages 2008. Washington, DC: Government Printing Office. http://www.bls.gov/cew/cewbultn08.htm.

[18] Bureau of Labor Statistics. 2007-2009. "Quarterly Census of Employment and Wages." United States Department of Labor. ftp://ftp.bls.gov/pub/special.requests/cew/ ftp://ftp.bls.gov/pub/time.series/sm/ (accessed June 8, 2011).

[19] Centers for Medicare and Medicaid Services. 2008. Data Compendium 2008 Edition. https://www.cms.gov/DataCompendium/16_2008DataCompendium.asp.

[20] Chernick, Howard. 1979. "An Economic Model of the Distribution of Project Grants" in *Fiscal Federalism and Grants-in-Aid*, Peter Mieszkowski and William Oakland eds. Washington D.C.: Urban Institute.

[21] Chodorow-Reich, Gabriel, Laura Feiveson, Zachary Liscow, and William Woolston. 2012. "Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act," Working Paper.

[22] Christiano, Lawrence, Martin Eichenbaum, and Sergio Rebelo. 2011. "When is the Government Spending Multiplier Large?," *Journal of Political Economy* 119 (1): 78-121.

[23] Clemens, Jeffrey and Stephen Miran. 2011. "The Effects of State Budget Cuts on Employment and Income," Working Paper.

[24] Cogan, John and John Taylor. 2010. "What the Government Purchases Multiplier Actually Multiplied in the 2009 Stimulus Package," NBER Working Paper 16505.

[25] Congressional Budget Office. 1988. *New Directions for the Nation's Public Works*, Washington, DC: U.S. GPO, September.

[26] Congressional Budget Office. 2010. "Estimated Impact of the American Recovery and Reinvestment Act on Employment and Economic Output from January 2010 Through March 2010." (Washington, D.C.: Congressional Budget Office, May 2010).

[27] Conley, Timothy and Bill Dupor. 2011. "The American Recovery and Reinvestment Act: Public Jobs Saved, Private Sector Jobs Forestalled." http://web.econ.ohio-state.edu/dupor/arra10_may11.pdf.

[28] Council of Economic Advisers. 2009. "The Effect of State Fiscal Relief." (Washington, D.C.: Executive Office of the President, September 2009).

[29] Council of Economic Advisers. 2010. "The Economic Impact of the American Recovery and Reinvestment Act of 2009: Fourth Quarterly Report." (Washington, D.C.: Executive Office of the President, July 2010).

[30] Courant, Paul N., Edward M. Gramlich, and Daniel L. Rubinfeld. 1979. "Public Employee Market Power and the Level of Government Spending," *American Economic Review* 69 (5): 806-17.

[31] Dommel, Paul. 1974. *The Politics of Revenue Sharing*, Indiana University Press, Bloomington.

[32] Dunlop, John T. 1944. *Wage Determination Under Trade Unions*, Macmillan Col, New York.

[33] Dye, Richard and Therese McGuire. 1991. "Growth and Variability of State Individual Income and General Sales Taxes," *National Tax Journal* 44 (1): 55-66.

[34] Farber, Henry S. 1986. "The Analysis of Union Behavior," in *Handbook of Labor Economics, Volume II*, ed. O. Ashenfelter and R. Layard, Elsevier Science Publishers, Chapter 18.

[35] Feiveson, Laura. 2012. "General Revenue Sharing and Public Sector Unions," Working Paper.

[36] Filimon, Radu, Thomas Romer, and Howard Rosenthal. 1982. "Asymmetric Information and Agenda Control," *Journal of Public Economics* 17 (February): 51-70.

[37] Finn, Mary G. 1998. "Cyclical Effects of Government's Employment and Goods Purchases." *International Economic Review* 39 (3): 635–657.

[38] Fishback, Price and Valentina Kachanovskaya. 2010. "In Search of the Multiplier for Net Federal Spending in the States During the New Deal: A Preliminary Report." NBER Working Paper 16561.

[39] Frandsen, Brigham R. 2011. "The Effects of Public Sector Collective Bargaining Rights," Unpublished manuscript.

[40] Freeman, Richard B. 1986. "Unionism Comes to the Public Sector," *Journal of Economic Literature* 24 (1): 41-86.

[41] Freeman, Richard B. and Robert G. Valletta. 1988. "The Effects of Public Sector Labor Laws on Labor Market Institutions and Outcomes," in *When Public Sector Workers Unionize*, Ed. Richard Freeman and Casey Ichniowski, University of Chicago Press.

[42] Gali, Jordi, J. David Lopez-Salido, and Javier Valles. 2007. "Understanding the Effects of Government Spending on Consumption," *Journal of European Economic Association* 5 (1): 227-270, 2007.

[43] General Accounting Office. 1977. United States, "Report to Congress: Antirecession Assistance–An Evaluation." PAD-78-20, November 29.

[44] Government Accountability Office. 2009. "Recovery Act: States' and Localities' Current and Planned Uses of Funds While Facing Fiscal Stresses." Report to Congressional Committees, July 8. http://www.gao.gov/new.items/d09829.pdf.

[45] Giertz, J. Fred. 2006. "The Property Tax Bound," *National Tax Journal* 59 (3): 695-706.

[46] Gramlich, Edward. 1977. "Intergovernmental Grants: A Review of the Empirical Literature," in W.E. Oates (ed.), *The Political Economy of Federalism*, Lexington, MA: Lexington Books, pp. 219-240.

[47] Gramlich, Edward M. 1994. "Infrastructure Investment: A Review Essay," *Journal of Economic Literature* 32 (3): 1176-1196.

[48] Gregory, Robert G. and Jeff Borland. 1999. "Recent Developments in Public Sector Labor Markets," in *Handbook of Labor Economics, Volume 3*, ed. O. Ashenfelter and David Card, pp. 3573-3630.

[49] Groves, Harold M. and C. Harry Kahn. 1952. "The Stability of State and Local Tax Yields," *American Economic Review* 42 (1): 87-102.

[50] Hines, James R. and Richard H. Thaler. 1995. "Anomalies: The Flypaper Effect," *Journal of Economic Perspectives* 9 (4).

[51] Hines, James R. 2010. "State Fiscal Policies and Transitory Income Fluctuations," *Brookings Papers on Economic Activity*, The Brookings Institution, 41 (2): 313-350.

[52] Hirsch, Barry T. and David A. Macpherson. 2003. "Union Membership and Coverage Database from the Current Population Survey: Note," *Industrial and Labor Relations Review* 56 (2): 349-54.

[53] Hoene, Christopher and Michael Pagano. 2008. "Cities & State Fiscal Structure," *Research Report on America's Cities*, The National League of Cities.

[54] Holcombe, Randall G. and Russel S. Sobel. 1996. "Measuring the Growth and Variability of Tax Bases over the Business Cycle," *National Tax Journal* 49 (4): 535-553.

[55] Holtz-Eakin, Douglas, Whitney Newey, and Harvey S. Rosen. 1989. "The Revenues-Expendiures Nexus: Evidence from Local Government Data," *International Economic Review* 30 (2): 415-429.

[56] Hoxby, Caroline M. 1996. "How Teachers' Unions Affect Education Production," *The Quarterly Journal of Economics* 111: 671-718.

[57] Inman, Robert. 2008. "The Flypaper Effect," NBER Working Paper 14579, December.

[58] Johnson, Nicholas, Andrew Nicholas, and Steven Pennington. 2009. “Tax Measures Help Balance State Budgets; A Common and Reasonable Response to Shortfalls.” Center on Budget and Policy Priorities. http://www.cbpp.org/files/5-13-09sfp.pdf.

[59] Johnson, David S., Jonathan A. Parker, and Nicholas S. Souleles. 2006. “Household Expenditure and the Income Tax Rebates of 2001.” *American Economic Review* 96 (5): 1589-1610.

[60] Joint Committee on Internal Revenue Taxation. 1973. "General Explanation of the State and Local Fiscal Assistance Act and the Federal-State Tax Collection Act of 1972: H.R. 14370, 92D Congress, Public Law 92-512," U.S. Government Printing Office, Washington, D.C.

[61] Knight, Brian. 2002. "Endogenous Federal Grants and Crowd-out of State Government Spending : Theory and Evidence from the Federal Highway Program", *American Economic Review* 92 (1): 71-92.

[62] Lewis, Gregg. 1990. "Union/Nonunion Wage Gaps in the Public Sector," *Journal of Labor Economics* 8 (1): S260-S328.

[63] Lutz, Byron. 2006. "Taxation with Representation: Intergovernmental Grants in a Plebiscite Democracy," Finance and Economics Discussion Series, Divisions of Research & Statistics and Monetary Affairs, Federal Reserve Board.

[64] Lutz, Byron. 2008. "The Connection Between House Price Appreciation and Property Tax Revenues," *National Tax Journal* 61 (3): 555-573.

[65] Lutz, Byron, Raven Molloy and Hui Shan. 2011. "The Housing Crisis and State and Local Government Tax Revenue: Five Channels," *Regional Science and Urban Economics* 41: 306-319.

[66] Maguire, Steven. 2003. "General Revenue Sharing: Background and Analysis," CRS Report for Congress, The Library of Congress, May 23.

[67] Markusen, Ann R., Annalee Saxenian, and Marc A. Weiss. 1981. "Who Benefits from Intergovernmental Transfers?" *Publius* 11 (1): 5-35.

[68] Marlow, Michael L. and William Orzechowski. 1996. "Public Sector Unions and Public Spending," *Public Choice* 89: 1-16.

[69] Mas, Alexandre. 2006. "Pay, Reference Points, and Police Performance," *The Quarterly Journal of Economics* 121 (3): 783-821.

[70] Mian, Atif and Amir Sufi. 2010. "The Effects of Fiscal Stimulus: Evidence from the 2009 'Cash for Clunkers' Program." NBER Working Paper 16351.

[71] Moretti, Enrico. 2010. "Local Multipliers." *American Economic Review* 100 (2): 1-7.

[72] Nakamura, Emi and Jon Steinsson. 2011. "Fiscal Stimulus in a Monetary Union: Evidence from U.S. Regions," NBER Working Paper 17391.

[73] National Association of State Budget Officers. 2008a. "Budget Processes in the States." http://nasbo.org/.

[74] National Association of State Budget Officers. 2008b. "The Fiscal Survey of States December 2008." http://nasbo.org/.

[75] National Association of State Budget Officers. 2009a. "The Fiscal Survey of States June 2009." http://nasbo.org/.

[76] National Association of State Budget Officers. 2009b. "The Fiscal Survey of States December 2009." http://nasbo.org/.

[77] National Association of State Budget Officers. 2010. "The Fiscal Survey of States June 2010." http://nasbo.org/.

[78] National Conference of State Legislatures. 2004. "Appendix A. State Budget Stabilization Funds" in Rainy Day Funds. Retrieved on September 10, 2010. http://www.ncsl.org/?TabID=12652.

[79] National Conference of State Legislatures. 2009. "State Budget Update: April 2009." Retrieved on December 6, 2011. http://www.ncsl.org/?TabId=17080.

[80] Neumann, Todd C., Price V. Fishback and Shawn Kantor. 2010. "The Dynamics of Relief Spending and the Private Urban Labor Market during the New Deal." The Journal of Economic History 70 (1): 195-220.

[81] Pappa, Evi. 2009. "The Effects of Fiscal Shocks on Employment and the Real Wage." *International Economic Review* 50: 400–421.

[82] Parker, Jonathan, Nicholas Souleles, David Johnson, and Robert McClelland. 2011. "Consumer Spending and the Economic Stimulus Payments of 2008," Working Paper.

[83] Poterba, Jim. 1994. "State Responses to Fiscal Crises: the Effects of Budgetary Institutions and Politics," *Journal of Political Economy* 102 (4).

[84] Ramey, Valerie. 2011. "Can Government Purchases Stimulate the Economy?" *Journal of Economic Literature* 49 (3): 673-685.

[85] Reder, M.W. 1975. "The Theory of Employment and Wages in the Public Sector," in *Labor in the Public and Nonprofit Sectors*, Ed. D. Hamermesh, Princeton University Press, Princeton, NJ, pp. 1-48.

[86] Rodden, Jonathan. 2004. "Comparative Federalism and Decentralization: On Meaning and Measurement," *Comparative Politics* 36 (4): 481-500.

[87] Rodden, Jonathan and Erik Wibbels. 2010. "Fiscal Decentralization and the Business Cycle: An Empirical Study of Seven Federations," *Economics & Politics* 22 (1): 37-67.

[88] Romer, Christina and Jared Bernstein. 2009. "The Job Impact of the American Recovery and Reinvestment Plan." Obama Transition Document, January 10.

[89] Romer, Christina and David Romer. 2010. "The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks." *American Economic Review* 100 (3): 763-801.

[90] Ross, Arthur M. 1948. *Trade Union Wage Policy.* Berkeley: University of California Press.

[91] Sahm, Claudia, Matthew Shapiro and Joel Slemrod. 2010. "Check in the Mail or More in the Paycheck: Does the Effectiveness of Fiscal Stimulus Depend on How It Is Delivered? Federal Reserve Board Finance and Economic Discussion Series Working Paper 2010-40.

[92] Saltzman, Gregory M. 1988. "Public Sector Bargaining Laws Really Matter: Evidence from Ohio and Illinois," in *When Public Sector Workers Unionize*, Ed. Richard Freeman and Casey Ichniowski, University of Chicago Press.

[93] Shadbegian, Ronald J. 1999. "The Effect of Tax and Expenditure Limitations on the Revenue Structure of Local Government, 1962-87," *National Tax Journal* 52 (2): 221-238.

[94] Shoag, Daniel. 2011. "The Impact of Government Spending Shocks: Evidence on the Multiplier from State Pension Plan Returns," Working Paper.

[95] Singhal, Monica. 2008. "Special Interest Groups and the Allocation of Public Funds", *Journal of Public Economics* 92 (3-4).

[96] Suarez-Serrato, Juan Carlos and Wingender, Philippe. 2011. "Estimating Local Fiscal Multipliers." http://www.jcsuarez.com/Files/Suarez_Serrato-JMP2.pdf.

[97] Trejo, Stephen. 1991. "Public Sector Unions and Muncipal Employment," *Industrial and Labor Relations Review* 45 (1): 166-180.

[98] United States Bureau of the Census. 1972-1987. "Labor-Management Relations in State and Local Governments," Washington: GPO.

[99] United States. Office of Management and Budget. 1978. *Budget of the United States Government: Fiscal Year 1978.* Washington: GPO.

[100] United States. Office of Management and Budget. 1979. *Budget of the United States Government: Fiscal Year 1979.* Washington: GPO.

[101] U.S. Recovery Accountability and Transparency Board. 2009-2010. "Agency Financial and Activity Reports." http://www.recovery.gov/.

[102] Valletta, Robert G. 1993. "Union Effects on Municipal Employment and Wages: A Longitudinal Approach," *The Journal of Labor Economics* 11 (3): 545-574.

[103] Williams, William V., Robert M. Anderson, David O. Froehle, and Kaye L. Lamb. 1973. "The Stability, Growth and Stabilizing Influence of State Taxes," *National Tax Journal* 26 (2): 267-274.

[104] Wilson, Daniel. 2011. "Fiscal Spending Multipliers: Evidence from the 2009 American Recovery and Reinvestment Act." Federal Reserve Bank of San Francisco Working Paper 2010-17.

[105] Wynne, Mark. 1992. "The Analysis of Fiscal Policy in Neoclassical Models." Working paper 9212, Federal Reserve Bank of Dallas.

[106] Zahradnik, Bob and Rose Ribeiro. 2003. "Heavy Weather: Are State Rainy Day Funds Working?" Center on Budget and Policy Priorities. http://www.cbpp.org/archiveSite/5-12-03sfp.pdf.

[107] Zax, Jeffrey S. and Casey Ichniowski. 1988. "The Effect of Public Sector Unions on Payroll, Employment, and Municipal Budgets," in *When Public Sector Workers Unionize*, Richard B. Freeman and Casey Ichniowski, eds., National Bureau of Economic Research and the University of Chicago Press, 232-361.