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Essays in Public Economics

By

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Committee in charge:

Professor Emmanuel Saez, Chair
Professor Alan Auerbach
Professor Steven Raphael

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Abstract

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This dissertation seeks to understand the determinants and effects of public policies and to understand how to use these results to improve policy. The first two parts consider government spending during recessions—the effects of the spending and how to spend most effectively.

In "Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act," my co-authors and I measure the employment effects of spending during recessions. 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.

In "Labor Market Policy for Inefficient Job Rationing During Recessions," my co-author and I consider how to best design government spending during recessions. We consider a simple static model of labor markets during recessions where the allocation of scarce jobs to workers is resolved inefficiently. In our model, some of the unemployed have high surplus from employment (e.g., those with a mortgage, three children, and a non-working spouse), while some of the employed do not. This inefficient rationing increases the welfare costs of recessions. We propose three solutions: (i) subsidizing non-employment, (ii) taxing employees, and (iii) subsidizing employers. These policies make "space" for those who really need jobs.

In "Why Fight Secession? Evidence of Economic Motivations from the American Civil War," I turn to the determinants of government policy. I ask why governments fight secession. This paper is a case study on this question, asking why the North chose to fight the South in the American Civil War. It tests a theoretical prediction that economic motivations were important, using county-level presidential election data. If economic interests like manufacturing wished to keep the Union together, they should have generated votes to do so. That prediction is borne out

by the data, and explanations other than Northern economic concerns about Southern secession appear unable to explain the results, suggesting that economic motivations were important to support for fighting the South.

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Part A: Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act

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 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.[1] 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.[2] 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.[3]

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

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[1] 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).

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

[3] 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).

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 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.[4] 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; 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.

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.[5] 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.[6] 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.

[4] 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.

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

[6] 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%).

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 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.[7] 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.[8] 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 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.[9] The House appropriation

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

[8] 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.

[9] 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

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. Econometric methodology and baseline specification

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:

$$(1)\ \frac{E_1^s - E_0^s}{N^s} = \beta_0 + \beta_1 \frac{Aid^s}{N^s} + \beta_2 Controls^s + \varepsilon^s$$

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 (1) 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 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.

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

Bill." *Reuters*, December 1. Retrieved on August 10, 2010. http://www.reuters.com/article/idUSTRE4B05QP20081201..

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.[10] Thus, employment through the middle of July reflects any changes to 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.[11] 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).[12]

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

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

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

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 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.

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). [13] 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).[14]

Instrument: The instrument is a state's Medicaid spending in fiscal year 2007, normalized by the 16+ population.[15,16] 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

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[13] 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.

[14] 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 spending during our period of study.

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

[16] 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.

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 low Medicaid spending states might have been on different employment trends prior to the ARRA.[17] 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

[17] 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).

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.

5. Baseline results

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.[18] 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) 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.

[18] 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.

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).

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.

6. Robustness checks and extensions

Falsification 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.

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.[19] 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.[20]

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.[21] The second stage coefficient is 2.89, essentially unchanged from 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.[22] In sum, the relationship between FMAP transfers and employment

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

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

[21] 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 most severe recession since World War II, this assumption may not be valid.

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

growth appears very robust to our alternative methods of generating an employment change counterfactual.[23]

7. Discussion

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 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.[24]

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"

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[23] 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.

[24] 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.

projects were often not necessary; in many cases, state and local governments only needed to avoid cuts.

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 to our estimate of cost per job, at $30,000 and $35,000 per year respectively.[25]

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.[26] 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.[27]

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.[28] 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.[29]

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

[26] 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.

[27] 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.

[28] 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.

[29] 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 model

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.[30] 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.[31] Indeed, by December 2008, 22 states had made or announced cuts to their expenditures totaling $12 billion.[32] 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.[33]

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.[34]

We can directly test the necessary condition that FMAP outlays affected spending or tax 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.[35] Furthermore, the models allow us to reject the null that half of

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.

[30] 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.

[31] 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.

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

[33] 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).

[34] 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).

[35] 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

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. [36] 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.[37]

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 yield a coefficient of zero).[38] 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.

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

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.

[36] 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.

[37] 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.

[38] 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).

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.

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.[39] For example, during the 1990s economic boom, states increased spending and cut taxes rather than contributing to their rainy day funds.[40]

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.

[39] 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.

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

FIGURES

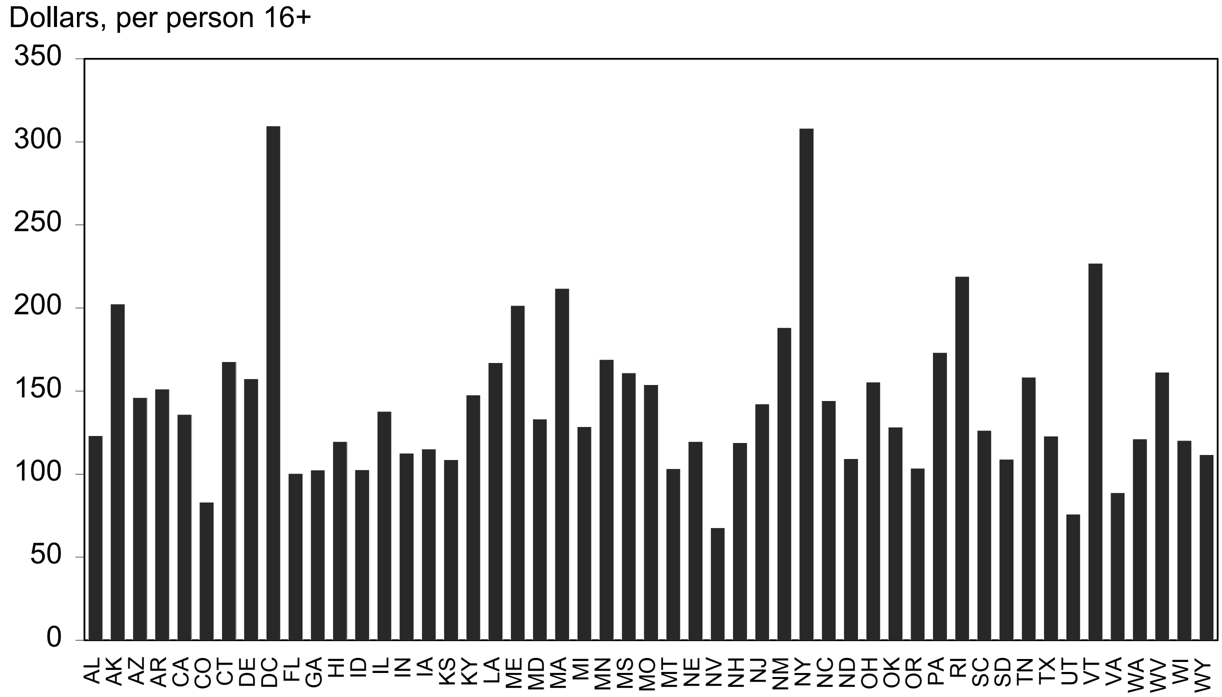

Figure 1. Value of the Scaled Instrument

Notes: 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.

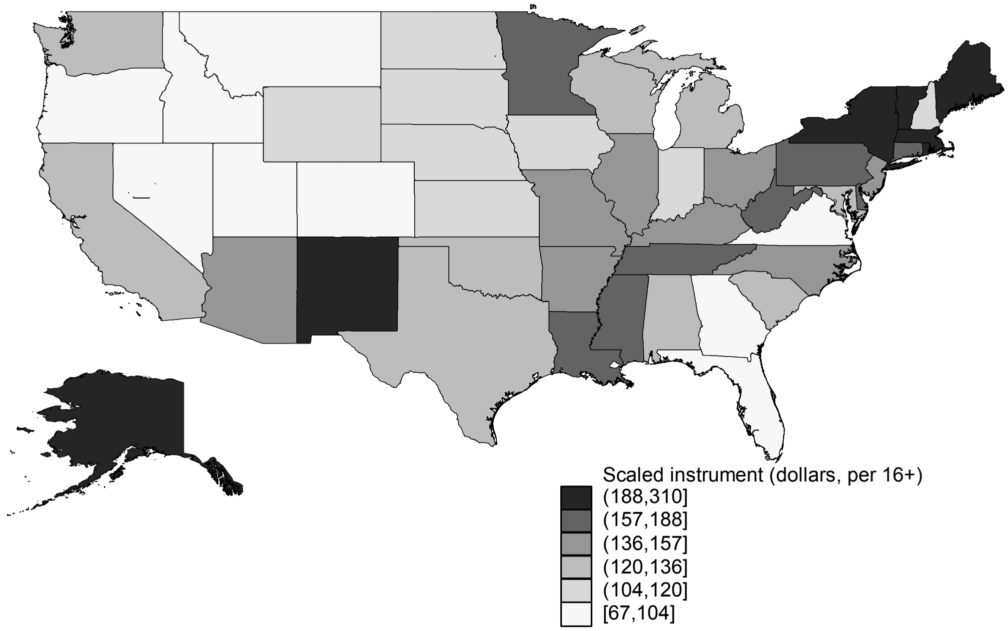

Figure 2. Value of the Scaled Instrument

Notes: 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.

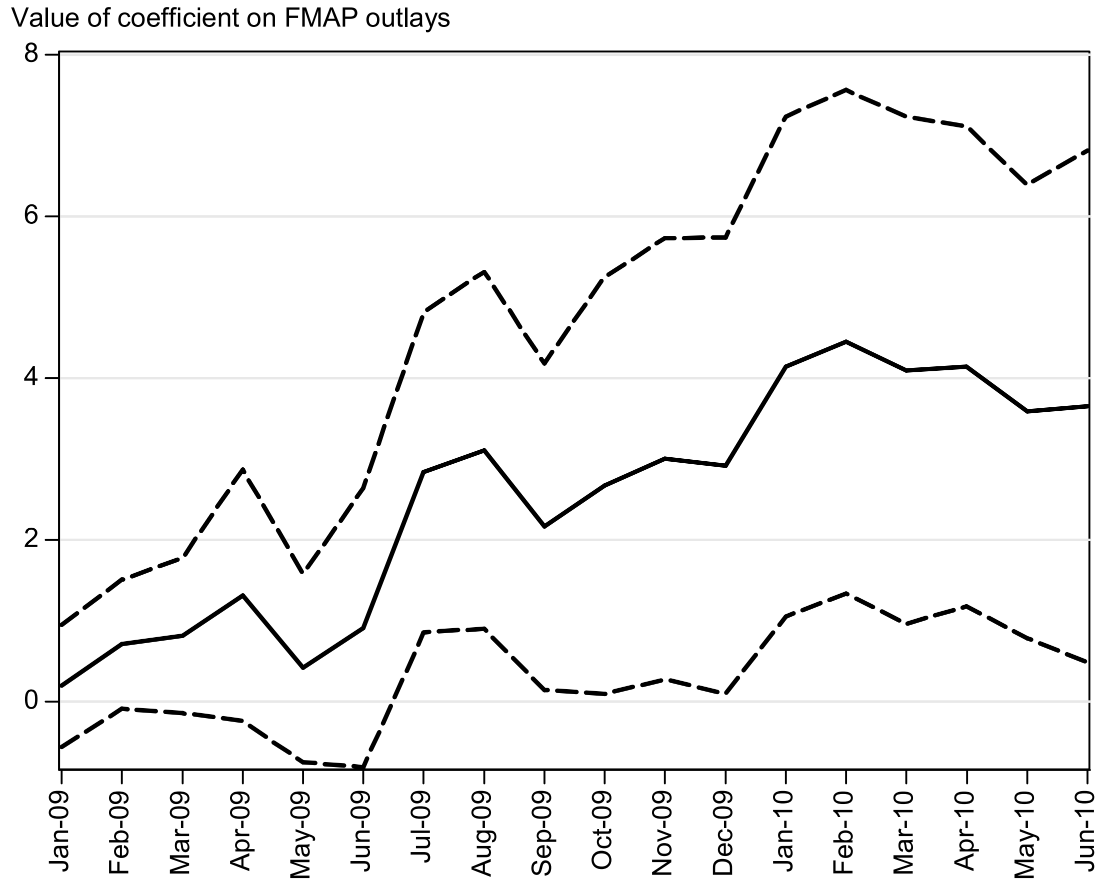

Figure 3. Total Nonfarm Second Stage Coefficients

Notes: 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.

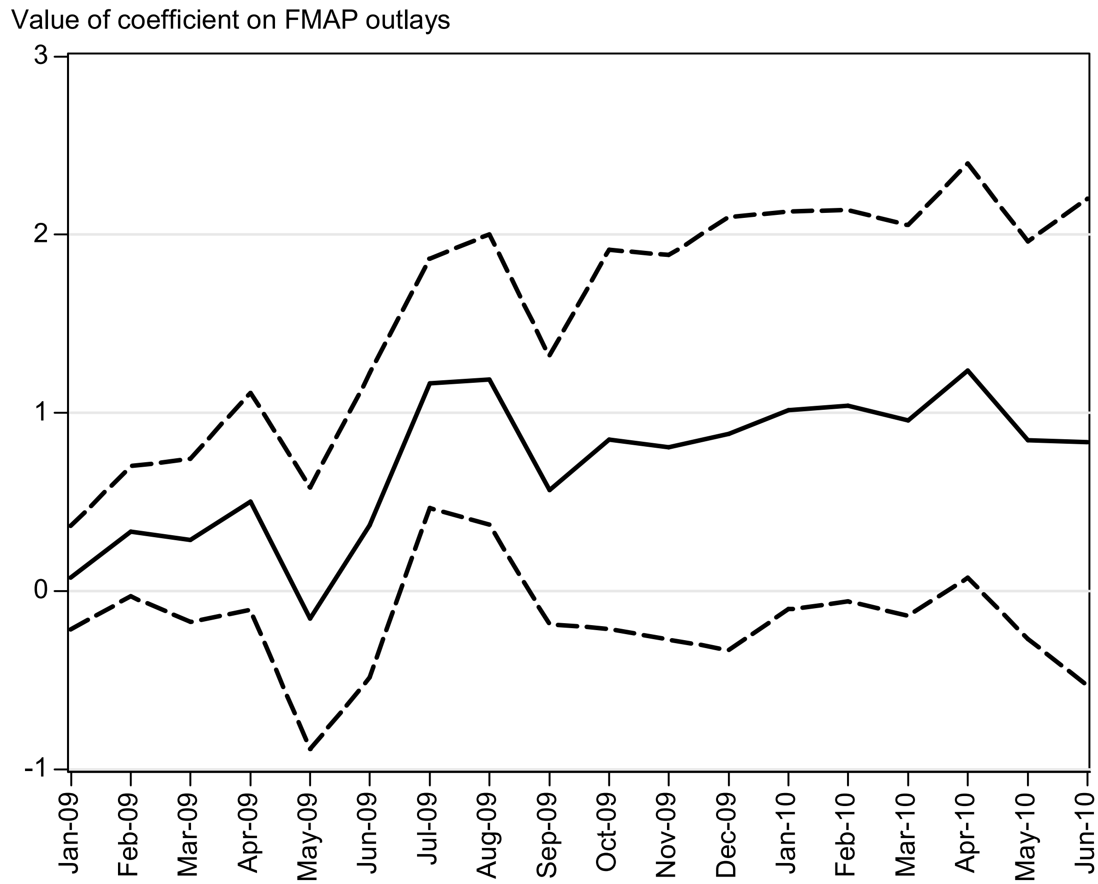

Figure 4. Government, Health, and Education Second Stage Coefficients

Notes: 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.

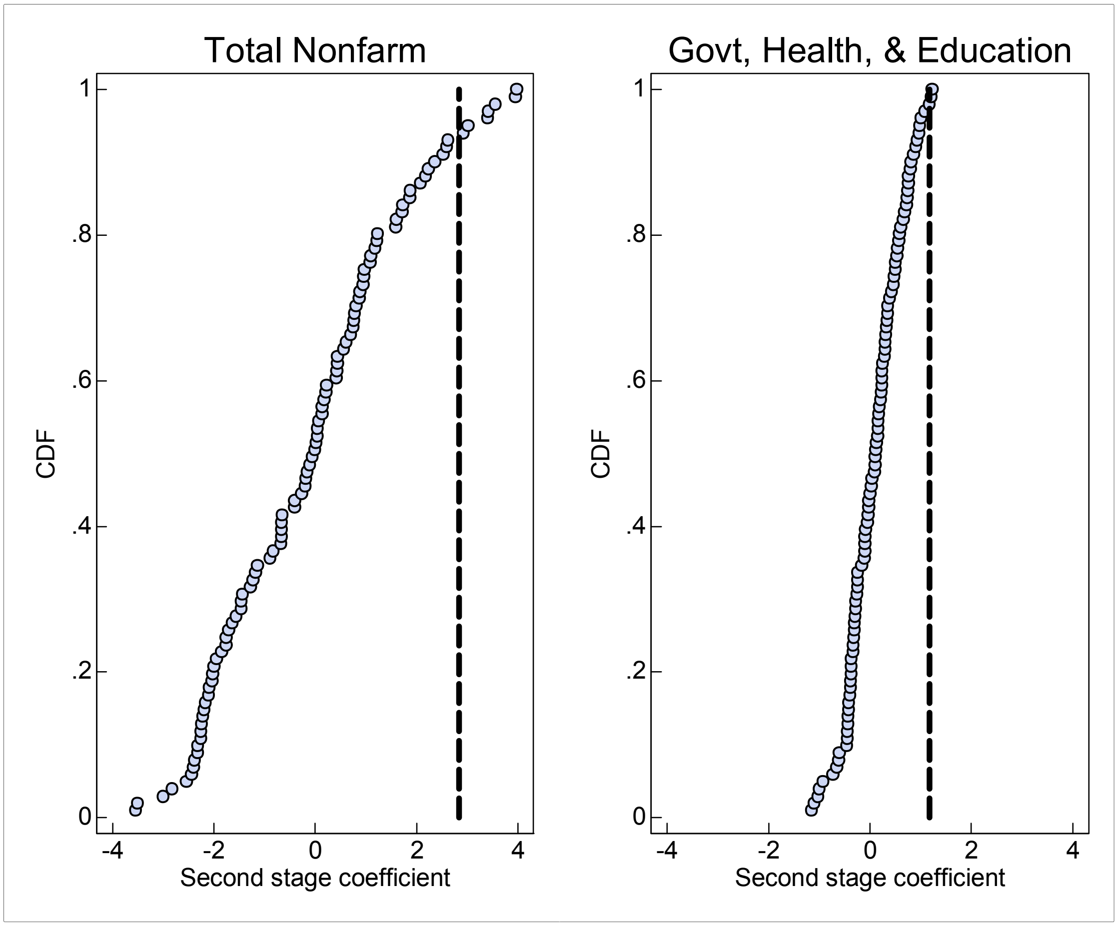

Figure 5. Placebo Results

Notes: Plots results of second stage regressions, where the outcome variable is seasonally adjusted change in employment for each overlapping seven 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.

TABLES

Table 1—Summary Statistics

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Outcome Variables, Per 1,000 People 16+					
Δ total nonfarm employment, Dec. 2008 → July 2009	18.76	.15	38.84	18.23	.11
Δ govt, health, & education, Dec. 2008 → July 2009	.97	.06	2.13	.53	.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	1.03	.28	.40	1.00	0.30
Vote share Kerry (2004), percent	6.52	0.38	6.00	7.02	9.18
Union share, percent	1.16	.49	.00	0.40	5.20
GDP per person 16+ ($1,000)	9.20	7.20	1.91	6.28	54.89
Population 16 and older (millions)	.60	.13	.41	.32	7.85
Δ total nonfarm employment, May 2008 → Dec. 2008	11.04	.91	33.42	11.25	.60
Δ govt, health, & education, May 2008 → Dec. 2008	.73	.27	1.44	.75	.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 (2004)		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 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 3— Total Employment Baseline Results

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

Notes: 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 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

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

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

Notes: 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 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent 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
Forecasted 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 2008, squared, July 2008 to Dec, 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

Notes: 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 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent 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 person 16+ ($100,000)	-0.26	0.01	-0.14	-0.04	0.08	0.04
	(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 dep. var. (*100,000)	-29.22	-29.22	-29.22	-17.84	-17.84	-17.84

Notes: 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 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Part B: Labor Market Policy for Inefficient Job Rationing During Recessions

1. Introduction

Understanding the right policy response to recessions is a central question in public finance. We present a very simple static model where wages are sticky, resulting in job rationing (e.g., Bewley 1999 and Taylor 1979), and workers have homogeneous ability but heterogeneous surplus from work. This excess supply of labor can be resolved efficiently (those with the most surplus get the job) or uniformly (all those willing to work have an equal chance). Uniform rationing substantially increases the welfare loss from recessions. We then show that, in the case of uniform rationing, policies that remove marginal works from the workforce or increase the number of available jobs lead to first-order welfare gains.

2. Model

To make the insights most transparent, we keep the math to a minimum and relegate the details to an online appendix.[41] We consider a standard model with linear labor demand and supply. Also assume that labor supply decisions are binary. Labor's productivity is uniform, but its reservation wage is heterogeneous.

Suppose that, because a recession has just begun and wages are sticky, the wage level is some amount θ greater than the efficient market-clearing price (P^*). Effectively, labor demand has shifted down but wages have not adjusted. As shown in Figure 1, Q^S workers are willing to queue for work, but only Q^D are employed, resulting in an excess supply of workers.

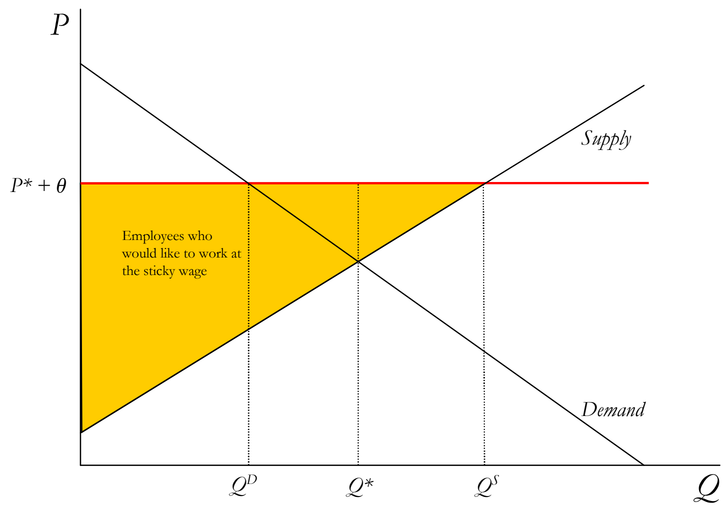

Figure 1: Employees who Queue

Workers can be rationed into jobs either efficiently or uniformly. While the assumption of uniform rationing may seem extreme, it strikes us as a reasonable case to consider because firms presumably care primarily about productivity. In the absence of any price-based incentives

[41] See https://sites.google.com/site/liscow for the appendix. Our model mirrors that of Glaeser and Luttmer (2003) in their application to rent control. Lee and Saez (2010) study the optimal minimum wage if the social planner values redistribution.

(or ability) for firms to distinguish between those with higher or lower reservation wages, there may be little reason that they would. Moreover, it is difficult to imagine a Coasian bargain being struck between high surplus unemployed individuals and low surplus employed ones, as such a bargain would have to involve the potential employer.

Finally, suppose that welfare can be measured by taking the area between the labor demand and supply curves, as would be the case with quasi-linear utility.

3. Deadweight Loss from Rationing

This set-up yields the following propositions:

P1: The DWL (deadweight loss) from efficient rationing is second-order in θ.

P2: The DWL from uniform rationing is first-order in θ.

P3: The DWL from uniform rationing is strictly higher than that the DWL from efficient rationing.

To gain insight into these results, we turn to graphical representations of the DWL in the two rationing regimes. Figure 2 shows the DWL in the case of efficient rationing. The deadweight loss is a Harberger triangle represented by ABC, the area between the supply and demand curves for workers who would work at quantity Q^* at the market-clearing wage, but not at the sticky wage. We call this triangle the "quantity DWL."

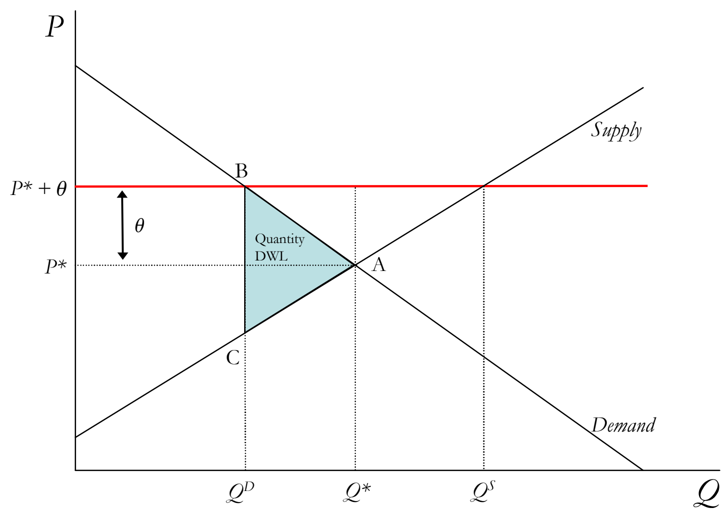

Figure 2: Deadweight Loss from Sticky Wages and Efficient Rationing

Uniform rationing results in the same quantity DWL, plus an additional DWL resulting from the misallocation of workers to jobs, the "allocative DWL" represented by area CDEF in Figure 3. In the efficient rationing case, the worker surplus is the area from the wage to the supply curve. In the uniform case, the worker surplus only constitutes the area from the wage down to the *average* reservation wage of workers who queue, which is substantially higher if all willing workers have an equal chance of working. (Note that a *lower* reservation wage means *higher* worker surplus.) The DWL is first-order because the "first" individual rationed out of a job is the *average* individual willing to work at the sticky wage, who is not indifferent between working and not working.

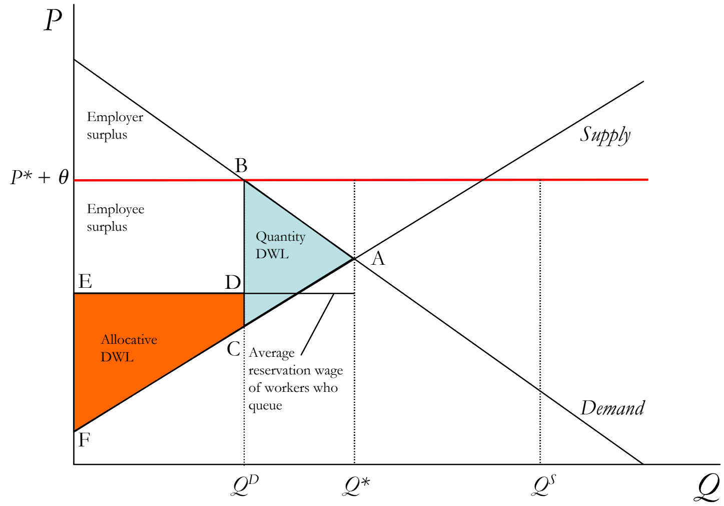

Figure 3: Deadweight Loss from Sticky Wages and Uniform Rationing

This analysis builds on earlier critiques of Lucas's (1987) use of a representative agent—and implicitly perfect insurance—in finding that recessions result in little welfare loss. In particular, the combination of heterogeneous surplus from work and inefficient rationing is a reason that imperfect insurance would yield particularly large welfare losses: some of those losing jobs—and consumption—during recessions are those most in need.

4. Optimal government response

This framework yields several results for optimal labor policy during recessions. Here, we consider three policies that can help alleviate the DWL: a subsidy for non-employment, a tax on employment, and a subsidy for employers. For simplicity, we assume that all government policy is financed by non-distortionary lump-sum taxes.

P4: A small subsidy for non-employment, a tax on employees, or a subsidy for employers each has first-order welfare gains in the case of uniform rationing.

First, consider a small subsidy to non-employment, like an increase in unemployment insurance or a welfare program like Temporary Assistance for Needy Families. As shown in Figure 4, labor supply shifts up since the value of having a job is no longer as large.

Under efficient rationing, this policy would have no impact on DWL. Regardless of the subsidy, the individuals gaining the most from the job are employed. However, with uniform rationing and no subsidy, many employed workers receive little surplus from their employment. A subsidy for non-employment induces those with the least surplus to exit the labor force, creating "space" for workers with a higher surplus from work. Figure 4 shows the reduced allocative DWL (CD'E'F) if labor supply changes to *Supply*$_2$, decreasing the average reservation wage of those employed by removing *marginal* workers from the labor force. Indeed, the allocative DWL can be eliminated if the subsidy is raised enough that workers are only willing to supply Q^D of labor.

With uniform rationing, there is an obvious welfare-enhancing trade: people who value working highly but did not get hired should pay those who value working only a small amount but who were hired to give up their jobs. A subsidy for non-employment replicates this payment system.

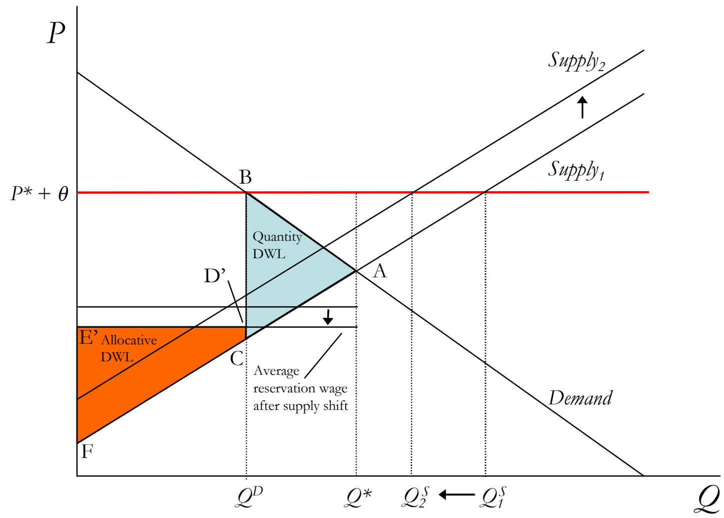

Figure 4: Subsidizing Non-employment or Taxing Employees

Next consider the effect of taxing employees. When workers decide to queue, they compare the utility from entering the labor market with the utility from exiting the labor market. Hence taxing employees has an identical effect to subsidizing non-employment. Both policies encourage marginal workers with the least surplus from work to leave the labor force.

Note that during recessions, governments frequently decrease net taxes on employees. In the U.S., cuts to the payroll tax as were enacted for 2011, and the Making Work Pay Tax Credit enacted in the 2009 stimulus bill was essentially a lump-sum subsidy for employees. In our model, this policy draws more marginal workers into the labor force without increasing the number of jobs. Therefore, these tax cuts are *counter-productive* with uniform rationing, as they aggravate the allocative DWL without reducing the quantity DWL.

Finally, consider an employer subsidy. As shown in Figure 5, suppose that the subsidy increases labor demand to *Demand*$_2$. Since more workers are employed, the quantity DWL (area AB"C") shrinks under either type of rationing, a first-order welfare gain. With efficient rationing, there is no allocative DWL before or after the subsidy.

However, with uniform rationing, the allocative inefficiency (area C"D"EF) *grows* with the subsidy in some cases, as shown in the figure. In particular, when the average reservation wage of workers who queue is greater than the worker who would be rationed in under efficient rationing (the employee at Q_2^D along the supply curve), a marginal increase in the subsidy increases allocative DWL. In other words, when the individual rationed into the new job values the job relatively little, the value of increasing employment is reduced. Indeed, for large employer subsidies, in which the marginal dollar in employer subsidy causes little decrease in quantity DWL, a marginal increase in the subsidy can *increase* total DWL. Thus, an employer subsidy is more effective at reducing DWL with efficient rationing than with uniform rationing. Unlike the other two policies, which remove marginal individuals from the labor force, the employer subsidy is a blunt instrument with uniform rationing, since some jobs are filled by low-surplus individuals

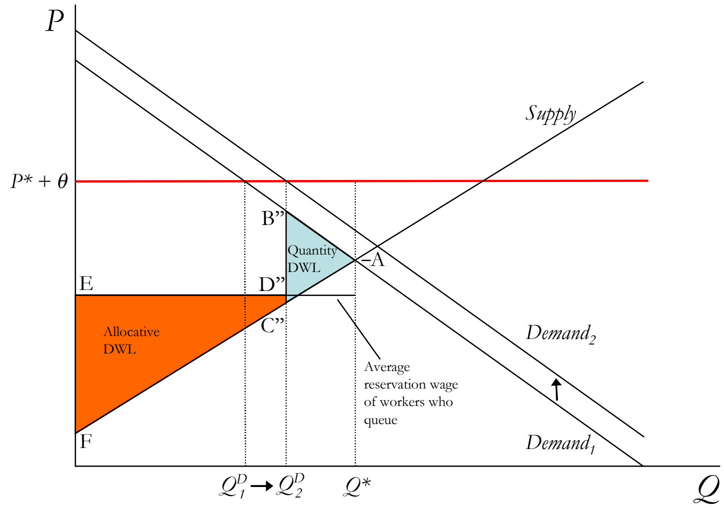

Figure 5: Employer Subsidy with Uniform Rationing

Note that the above policies can complement each other. For example, taxing employees and subsidizing employers effectively reduces the post-tax wage, removing the underlying source of inefficiency and therefore reducing both allocative and quantity DWLs. How does this policy work? Although the market cannot get the wage down, the government can effectively decrease it. Inflation would do the same thing, but may have other undesirable consequences and be harder to target.

To summarize, subsidizing non-employment and taxing employment have first-order welfare gains with uniform rationing, but are neutral with efficient rationing.[42] A small subsidy to employers has first-order welfare gains with either form of rationing, but is of *less* value with uniform rationing. Subsidizing *employees* leads to first-order welfare losses. This result resonates with the current political debate on the importance of "creating" more jobs, rather than subsidizing existing employees.

Of course, this partial equilibrium model leaves out potentially important considerations, including the marginal propensity to consume, hiring and firing costs, heterogeneity across sectors of the economy, the cost of raising funds, the social costs of queuing, targeting based on observables (Neumark 2011), and dynamics (in many recent macroeconomic models). We assume a fixed wage; to the extent that the wage does respond to the reservation wage, fall in a recession, or change with subsidies, our results will be less relevant.

5. Conclusion

[42] Similarly, Landais, Michaillat, and Saez (2011) find that optimal unemployment insurance is higher during recessions, but –lacking heterogeneity in worker surplus—do not make similar conclusions about taxing employees or subsidizing employers; they also do not discuss other forms of subsidizing non-employment.

A key task for future research is measuring rationing efficiency. This will be particularly challenging, given the unobservability of surplus from work and the difficulty of finding variation in surplus from work that is exogenous to other reasons for firms' employment decisions, such as productivity. Some approaches to measure the surplus from work might be using changes or discontinuities in eligibility for government programs, proxies (e.g., Luttmer 2007), or stated values (e.g., Feldstein and Poterba 1984). With a measure of the reservation wage, one could then measure layoff or hiring behavior during recessions.

A model with sticky wages and heterogeneous workers can generate large welfare losses from recessions, if jobs are rationed inefficiently. In this case, the welfare gains are large from either removing the stickiness through taxing workers and subsidizing firms or through removing marginal workers from the labor force, through increased unemployment insurance or welfare benefits.

Part C: Why Fight Secession?
Evidence of Economic Motivations from the American Civil War

1. Introduction

Even after Abraham Lincoln was elected president of the United States in 1860, powerful Northern interests opposed war with a seceded South. For example, three days after Lincoln's election, the influential New York *Tribune* wrote, "If the cotton states shall decide that they can do better out of the Union than in it, we insist on letting them go in peace."[43] The public did not consider war inevitable, even after the secession of most of the Confederacy before the Fort Sumter attack (Stampp 1970). The South was poorer than the North. Were the South to secede, it would not pose an immediate threat to the North. Yet, in Lincoln's immortal words from his Second Inaugural Address, "the war came." By testing an economic explanation, I seek to help answer the question of why—given that the South had seceded—the North chose to fight.

This paper is a case study addressing a more general question: why fight secessions? A variety of explanations have been offered. Bolton and Roland (1997) suggest that the efficiency gains from having one unified market may justify fighting secessions. Walter (2006) finds that nations may seek to build a reputation for opposing secession so that other regions do not secede. Others have suggested that the seceding regions may be important for defense, for psychological and historical reasons, or for providing economic value to the rest of the country (e.g., Bartkus 1999; Coakley 1993; Huth 1996; Touval 1972). This paper tests the proposition that economic considerations may have been important in the American Civil War.

Many important studies of the Civil War barely discuss why the North decided to fight the South after it seceded, instead focusing almost entirely on the decision-making of the South (e.g., Potter 1942; McPherson 1988). History textbooks often note that the North seemed to oppose war; then the South attacked the Union at Fort Sumter in April 1861. After that, Northern support for war built rapidly. To simplify somewhat, one standard textbook says that Lincoln—for complex and somewhat unclear reasons—decided to hold onto Fort Sumter and send it foodstuffs; after the South attacked it, war was a foregone conclusion (Bailey et al. 1998). The federal government was merely enforcing the laws, Lincoln and the Republicans said (McPherson 1988).

The economics literature has discussed the question of the North's motivation. Gunderson (1974) argues that the North may have wanted to prevent the spread of slavery. Ransom (2001) notes several economic tensions between the North and the South, but says that most historians think that the North's decision was non-economic. Wright (1978) argues that a desire to maintain the Union among the Northern leadership was decisive.

Another explanation, described in the work of DiLorenzo (2003, 2006) and Stampp (1970), is that manufacturing interests in the North benefited from keeping the South in the Union. DiLorenzo (2003) provides narrative evidence for his argument. This study is the first attempt to formally test an economic explanation of why the North chose to fight the South and

[43] New York *Tribune*, November 9, 1860, from Bailey et al. (1998).

quantify its importance. [44] To do this, the study uses controlled regressions with large and relatively disaggregated datasets on the county level.

Specifically, using voting patterns as representations of the Northern population's preferences, this paper tests empirically whether the economic motivations of its manufacturing interests might have been important components of Northerners' support of the decision to fight. The hypothesis that the North had economic motivations for keeping the South in the Union yields a specific prediction: counties with relatively large amounts of these manufacturing interests should shift their votes from Democrats to Republicans between 1860 and 1864. The reason is the following: the best way to keep the South in the Union *before the Civil War* was to vote for the Democrats, reducing the likelihood of secession by voting for the party more accommodating to Southern slavery interests. However, the best way to keep the South in the Union *during the War* was to vote for the Republicans, who were more likely to pursue the war until victory was achieved.

Using county-level census data and voting data from the 1860 and 1864 presidential elections, I find that there is a significant shift toward the Republicans associated with manufacturing employment. This shift toward the Republicans associated with manufacturing together amounts to 2.25% of voters in Northern states; that is, taking the results literally suggests that 2.25% of Northern voters shifted their votes to the Republicans out of a desire to protect their manufacturing interests by keeping the South in the Union.

The remainder of the paper proceeds as follows. I first review the possible economic causes of the North's decision. Then I explain the empirical methodology, followed by the paper's data. After presenting the paper's main results, I present robustness checks and argue that other explanations are unlikely to account for the reported shifts in voting patterns. Finally, after interpreting the results, I conclude.

2. Theory and anecdotal evidence: explanations of the North's decision to fight

The debate on whether economic considerations were important causes of the Civil War is decades old. In the 1920s, Charles and Mary Beard (1927) made a classic argument for the importance of economic motivations, saying that structural economic differences were the Civil War's primary cause. The North wanted a strong national government which could promote economic development; the South preferred strong states and did not want to spend the money for that expansionism. Indeed, the idea that a main effect of the Civil War was the triumph of industrial capitalism in the United States has come to be known as the "Beard-Hacker" thesis. While the Beards' work does not go far in explaining why the North did not allow the South to secede, these arguments helped spawn a literature that focused on the economic differences between the North and the South.

This paper tests whether manufacturing interests may have been important to the North's decision to fight the South. Stampp (1970) provides anecdotal evidence that manufacturing interests anticipated being harmed by secession of the South. For example, although there could be multiple explanations for this, he notes that "between January 1860 and January 1861, the

[44] Egnal (2001) used a somewhat similar methodology with county-level voting data to discern shifting interests before the Civil War.

average price of a share of stock in forty-four cotton and woolen mills declined from $518.34 to $304.22."[45] Likewise, John Parsons Foote, the abolitionist uncle of Harriet Beecher Stowe and an astute observer of the economic atmosphere before the war, wrote the following to his industrialist brother Samuel Edmond Foote: "You Connecticut folks support the slave powers because you can send your clocks and buttons among the slave drivers" (Foote 1854).

More specifically, there are three economic reasons why Northern manufacturers might have wanted to keep the South as part of the Union. First, they may have wanted to maintain the Southern market for tariff-protected manufactured goods priced above the foreign competition. If the South had seceded, Northern manufacturers would presumably have faced the same import tariff in the South that Europeans faced, putting the two on equal footing. Domestic manufacturers—mostly Northern—had substantial advantages over the foreign competition in access to domestic markets: in 1860, a low-point of 19th century tariff rates, import taxes amounted to 15.67% of the value of all imports and 19.67% of the value of dutiable imports; in 1864, the taxes amounted to 32.04% of all imports and 36.69% of dutiable imports.[46] By comparison, import taxes amounted to 7.40% of all imports and 12.22% of dutiable imports in 1960.

Second, Northern manufacturers feared a long, porous southern border, through which goods unprotected by tariffs could flow northward, undercutting the prices of their more expensive goods (Foner 1941; Stampp 1970). For example, the London *Times* said in March 1861, after some Southern states seceded, that European businessmen had perceived this opportunity to invade the Northern market claiming, "The smuggler will redress the errors of the statesman."[47] The New York *Evening Post* described the horrifying prospect of all imports coming through the South and "the whole country…given up to an immense system of smuggling."[48] Some at the time even suggested that the agrarian West might choose to join the free-trade South to avoid large tariffs, further harming both industrial and trading prospects.[49] Indeed, Cincinnati began receiving goods tariff-free from New Orleans in 1861 (Stampp 1970). The high rate of tariffs makes such claims more plausible.

Third, manufacturers may have wanted continued reliable access to Southern primary goods, particularly "King Cotton." The South punitively could have refused to sell its agricultural products to the North—or threatened to do so, in order to gain clout in "international" negotiations. Though the Transportation Revolution was in full swing, bulky commodities were still somewhat costly to ship from far away, so primary goods from outside the South would have carried higher prices owing to greater transportation costs.[50] Also, given the state of mid-19th century communication technology, obtaining primary products from outside the South would have increased transaction costs other than those for transportation. Even if the South did not ever threaten to disrupt a steady supply of agricultural products, the need to trade between two countries could have increased manufacturers' transaction costs.

[45] Stampp (1970: 124). Note that this was not a time of deflation.
[46] U.S. Census Bureau (1970). These numbers understate the true impact of the tariff, since highly-protected goods will represent a smaller portion of total imports than they otherwise would, while still protecting domestic producers.
[47] London *Times*, quoted in New York *Herald*, March 17, 1961, from Stampp (1970).
[48] New York *Evening Post*, March 12, 1961, from Stampp (1970).
[49] Westerners were a minority, so they might have been unable to block the imposition of tariffs.
[50] There may have been smuggling from the South, but cotton is so bulky that this is unlikely to have helped Northern manufacturers very much.

Presumably partly for these reasons, McPherson (1988) notes that the price of a pound of cotton in the North increased from ten cents to a dollar during the War.

Northern editorials raised these concerns about manufacturing. For example, on December 10, 1860, the *Daily Chicago Times* noted that the tariff

> "protects our manufacturers … and enables [the North] to consume large quantities of Southern cotton. This operates to compel the South to pay an indirect bounty to our skilled labor, of millions annually. … Let the South adopt the free-trade system … [and the North's] commerce must be reduced to less than half what it now is … [and] our labor could not compete … with the labor of Europe."[51]

The passage raises all three of the above concerns: the access to Southern primary goods like cotton, the "indirect bounty" from selling tariff-protected goods to the South, and the need to compete with European labor should a free trade South secede, presumably due to a porous border. Similarly, on April 2, 1861, the Newark *Daily Advertiser* warned that Southerners had apparently "taken to their bosoms the liberal and popular doctrine of free trade" and that they "might be willing to go . . . toward free trade with the European powers," which "must operate to the serious disadvantage of the North."[52] This editorial emphasizes that the presence of a free trade South was a credible threat that would damage the North.

Similarly, some critics in the North emphasized the economic interests at stake in the War. As emphasized by DiLorenzo (2006), abolitionist and legal scholar Lysander Spooner argued in 1870 that the war "erupted for a purely pecuniary consideration," largely the value of having "control of [Southern] markets" to be able to enforce tariff "extortion." The war would allow "such tariffs on imports as will enable our home manufacturers to realize enormous prices for their commodities" (Smith 1992: 117-118).[53]

3. Empirical method

I evaluate the hypothesis that maintaining manufacturing employment was an important motivation of the North's decision to fight the South by testing a prediction of the hypothesis using presidential election voting data. The prediction is based on the assumption widely-accepted in public choice that people vote their economic interests, suggesting that voters with an economic interest in keeping the South in the Union should vote as such (Persson and Tabellini 2000). Particularly, counties with a relatively large manufacturing sector should shift their votes from Democrats to Republicans between 1860 and 1864 relative to those with a smaller manufacturing sector. The reasoning is the following: the best way to keep the South in

[51] Daily *Chicago Times*, December 10, 1860, from DiLorenzo (2003).

[52] Newark *Daily Advertiser* April 2, 1861, from DiLorenzo (2003).

[53] Historians have also offered a variety of non-economic explanations for the North's decision to fight the South. Since they are not tested in this paper, I will mention the explanations only briefly here. In addition to the claim that the attack on Fort Sumter spurred the North to fight, explanations include: a nationalistic commitment to preserving the Union (Nagel 1964; Ransom 1989), the leadership's commitment to the Union (Wright 1978), a moral commitment to containing or eliminating slavery (Gunderson 1974), sectional animosity (Owsley 1941; Ransom 1989), and the Republicans' desire not to lose the Union under their watch (Stampp 1970). Also see Fogel (1989) and North (1961). Other economic interest groups include those concerned about disruption of Mississippi River shipping and or those involved in international trade, since trade could have been rerouted to a free-trade South or the South could have blocked trade from the rest of the Americas (Foner 1941; Hummel 1996; Stampp 1970).

the Union *before the Civil War* was to vote for the Democrats, reducing the likelihood of secession by voting for the party more accommodating to Southern slavery interests. However, the best way to keep the South in the Union *during the War* was to vote for the Republicans, who were more likely to pursue the war until victory.[54]

In the 1860 presidential election, before the Civil War, the best way to keep the South in the Union was to vote for the Democrats. Southerners were clear that a victory by the Republican candidate Abraham Lincoln over Democrats Stephen Douglas and John Breckinridge and Constitutional Union candidate John Bell meant disunion. McPherson writes that even border state Unionists, like John Crittenden, "Kentucky's elder statesman of unionism," warned Northerners that a Lincoln victory would lead to disunion (McPherson 1988: 229-30). Warnings were often very explicit. For example, Georgia's leading Douglas-supporting newspaper wrote, "Let the consequences be what they may—whether the Potomac is crimsoned in human gore, and Pennsylvania Avenue is paved ten fathoms deep with mangled bodies . . . the South will never submit to such humiliation and degradation as the inauguration of Abraham Lincoln" (McPherson 1988: 230). Southerners meant what they threatened: within three months of Lincoln's election, the Confederate States of America had drafted a constitution and established a capital at Montgomery, Alabama.[55]

In contrast, the best way to keep the South in the Union in 1864, *during* the Civil War, was to vote for the Republicans. In the 1864 election between Union/Republican[56] candidate Abraham Lincoln and Democratic candidate George McClellan, the parties differed substantially on their opinion of the war, since the platform of the Democrats officially declared the war a failure and the Union Party strongly supported continued fighting.[57] Although Democratic candidate McClellan largely ignored the platform and ran on a platform of supporting the war, it was clear that the South interpreted the Lincoln victory as a strike for the Union. For example, Southern soldiers shouted, "Hurrah for McClellan!" (Bailey et al. 1998). Additionally, after Lincoln's victory, desertions from the Southern army sharply rose, suggesting that they viewed Lincoln's victory as a sign of the Union's commitment to the war (Bailey et al. 1998: 479).

Thus, economic interests yield a clear prediction: in counties with a large manufacturing sector, *ceteris paribus*, there should be an increase in the Republican vote share between 1860 and 1864. Using county-level data, I test this hypothesis by running a controlled regression of the difference between the 1864 and 1860 Republican vote shares on the manufacturing employment share and covariates, using state dummies.

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[54] Note that universal white male suffrage for U.S. citizens 21 and older was essentially achieved before 1860 (Porter 1918; Rusk 2001; Williamson 1960). There were small variations across states in this sample in 1860 and 1864, with a few requiring payment of a state or county tax, some allowing blacks to vote, some requiring up to two years of state residency and others requiring none, and some allowing non-citizens to vote. However, the analysis of Rusk (2001) shows that approximately 50% of adults outside the South were eligible to vote in the 1850s and 1860s, suggesting nearly complete adult male suffrage. Additionally, there was not significant movement between 1860 and 1864, which could confound the results (Rusk 2001, Chapter 2 tables).

[55] Those interested in maintaining the Union could also vote for the Constitutional Union Party, which only 2.4% of voters did.

[56] The Republicans cleverly joined with pro-War Democrats to temporarily form the Union party.

[57] The war was going poorly for the Union until shortly before the election, when the North scored several victories, like Admiral Farragut's capture of Mobile, Alabama, and General Sherman's seizure of Atlanta, Georgia (Bailey et al. 1998).

To understand the econometric specification used in this paper, first consider the two levels specifications:

(1) $$y_{1i} = \alpha_1 + x_i\beta_1 + c_i\gamma_1 + d_i\delta + \phi_{1s} + \varepsilon_{1i}$$

(2) $$y_{2i} = \alpha_2 + x_i\beta_2 + c_i\gamma_2 + d_i\delta + \phi_{2s} + \varepsilon_{2i}$$

where equations (1) and (2) represent the equations for 1860 and 1864, respectively. In these equations, i indexes counties, s indexes states, y_{ti} is the Republican vote share in year t, x_i is a vector of variables with time-invariant values but potentially time-varying effects (β_t) on voting, c_i is a vector of control variables which also have time-invariant values but potentially time-varying effects (γ_t) and which are potentially correlated with both x_i and y_{ti}, d_i is a vector of unobserved variables which have time-invariant values and effects and which are potentially correlated with both x_i and y_{ti}, ϕ_{ts} is a vector of state dummies with potentially time-varying values, and ε_{ti} is a time-varying error term. In the preferred specification below, x_i consists of per capita manufacturing employment. Another specification adds to x_i per capita employment by manufacturers which process Southern agricultural commodities, like cotton and tobacco. Note that, since d_i is unobserved, it is not possible to calculate the coefficients in equations (1) and (2), and running the regressions without d_i would yield inconsistent estimators.

Instead, taking the difference between equations (1) and (2) yields:

(3) $$\Delta y_i = \Delta\alpha + x_i\Delta\beta + c_i\Delta\gamma + \Delta\phi_s + \Delta\varepsilon_i$$

where Δ represents the change between 1860 and 1864. Since neither d_i nor its effect, δ, is time-varying, both fall out of the equation. Note that, throughout the paper, coefficient estimates refer to the change in the effect of the given variable on the Republican vote share. The presence of economic motivations predicts a positive coefficient on each element of the $\Delta\beta$ vector.[58]

The identifying assumption of equation (3), which follows from the validity of equations (1) and (2), is that x_i is uncorrelated with $\Delta\varepsilon_i$; that is, the main explanatory variables are uncorrelated with the error term. Conditional on changes in state dummies and changing effects of the control vector, the main explanatory variables must be uncorrelated with other causes of the change in Republican vote share. The key is that these equations measure a *shift* in votes so that any unobservable which does not change its effect on the likelihood of voting for the Republicans between 1860 and 1864 is differenced out and does not threaten the consistency of the estimators.

Also, since I add state dummies to control for any characteristic common to states which might shift its influence between 1860 and 1864, results are identified off of intra-state differences. This is valuable since the states from which candidates come influence electoral outcomes, and the importance of manufacturing varies by state.

In my main specifications, I use two controls. First, I control for the number of foreign-born people. Immigrant voters may live disproportionately in areas with more manufacturing. Also, given the Republican Party's strong anti-immigrant sentiment (Silbey 1985), it is possible that the foreign-born, even those working in manufacturing, would be less susceptible to shifting

[58] Following the guidance of Stock and Watson (2003), all regressions use heteroskedastic-robust standard errors, since there are not compelling reasons to believe that errors would be homoskedastic,.

to the Republican Party than the native-born. Thus, a greater fraction of foreign-born voters might lead to a smaller shift in Republican votes, correlated with a larger amount of manufacturing.

Second, I control for the logarithm of population density, since factors associated with cities (like cultural attitudes) are correlated with manufacturing activity and thus could be an important omitted variable. These cultural attitudes could potentially change their effect over time as ideas changed disproportionately fast in urban areas.

I weight the regression by population size, since what really matters to the question here is the total population, not an average of county-level attitudes among counties which have very different populations. Thus, weighting by population gives a more accurate sense of the opinion in the North at the time.[59]

Note that this methodology assumes that popular opinion mattered for the decision to fight the South. For example, if Lincoln made the decision to fight the South without any regard for Northern opinion, then that opinion would be meaningless. For example, Wright (1978: 156) argues that the decisions regarding whether to fight the South "must be studied at the level of small-group behavior, albeit behavior influenced by perceptions of a social background." However, others argue that Lincoln was quite "sensitive to the drift of public opinion" (Stampp 1970: 189). Thus, there is agreement that public opinion played a role in the decision to go to war; the question is how much. That will not be answered here, but the question of what drove public opinion remains a useful one.

The interpretation of the results using this methodology depends upon whether one assumes that the workers employed in manufacturing enterprises had interests aligned with the owners, insofar as Southern secession was concerned. This assumption is quite reasonable, since harm to manufacturing would lower wages for laborers.[60] Indeed, it was a longstanding Whig and Republican belief that the interests of owners and workers were aligned, as demonstrated particularly by the notion "that the protective tariff was designed primarily to advance the interests of labor" (Foner 1970: 20).[61] Even without this assumption, the per capita sectoral employment variables could be seen as proxies for the manufacturing interests in an area. However, given the reasonableness of the assumption, I will interpret the results as representing the changing preferences of those who worked in these industries.

4. Data

The data for this study come from several sources. First, the explanatory variables on male manufacturing employment, total population, and foreign population come from the 1860 U.S. Census of Population and Housing, from the Geospatial and Statistical Data Center at the University of Virginia Library (2004). Additional control variables on church seats per capita,

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[59] A second reason is that, since the observations represent averages within counties, smaller counties will have larger variances, assuming that voting data represent a sample, not the whole population. Weighting by population yields a more efficient estimator, since observations from counties with larger populations are more precisely estimated. Note that weighting by number of voters may be more ideal; however, this variable is not available.

[60] This assumes the absence of costless migration of labor and capital to the South, where workers could again be employed in the un-tariff-protected trade there. Without costless migration of capital, it may have taken some time for industry to develop in the South.

[61] Similarly, manufacturing unions today often support tariffs.

per capita cash value of farms, and per capita capital invested in manufacturing also come from this source. Disaggregated data on manufacturing employment, used as a measure of manufacturing using Southern agricultural goods were not available electronically, so I coded this manually from the 1860 Census of Manufactures. For the explanatory variable, per capita employment in manufacturing with Southern agricultural goods, I add together employment in firms processing cotton, sugar, and tobacco, as well as producing paint and chemicals. [62]

The dependant variable, the difference between the percent of the presidential vote received by the Republican candidate at the county level in 1860 and 1864 from counties in Union states, is from the Interuniversity Consortium for Political and Social Research (Clubb et al. 2006). Votes from soldiers are not included, since they are tabulated at the state level. The number of soldier votes missing from the analysis is difficult to determine, since only the fraction voting for each party—not the number of votes—is provided in the data. However, with approximately 2,100,000 men participating in the Union army over the course of the war and a Union population of approximately 20,000,000, if half of the total was serving in 1864 and all of them voted, then approximately 10% of voters would be removed from the analysis (McPherson 1988). [63]

The dataset for the baseline regressions is limited to 716 counties for several reasons. First, only non-border state members of the Union during the Civil War are included. Border states (Missouri, Delaware, Kentucky, and Maryland) cannot be included, since many counties had no votes recorded for the Republican candidate in 1860, presumably since there were no local Republican organizations to distribute ballots, making shifts in voting impossible to discern. A further factor is that many counties in Missouri had extremely high vote shares—some as high as 100%—for the Republican in 1864, perhaps a result of the Union military presence or boycotts by non-Republican voters. Heterogeneous voter boycotts, military presence, and absence of the Republican Party in 1860 would add a great deal of noise to the results, at best, so these states were excluded.

Second, in the main specifications, I drop counties which had border changes between 1860 and 1870 to limit measurement error arising from those changes. [64] This results in 33 counties being dropped. Including the variable on manufacturing with Southern agricultural goods results in additional dropped counties, leaving 674.

Summary statistics, weighted by population size, are tabulated in Table 1. The table shows that, among the states studied, Republicans gained virtually no overall vote share between 1860 and 1864. However, there is a substantial amount of variation, with a standard deviation of 7.51 percentage points. Manufacturing employed 5.68% of the population.

I use presidential election results, rather than votes on important pre-War bills in Congress, for several reasons. First, studying presidential elections allows the use of data closer to the time of Southern secession and the Northern declaration of war. Many of the important votes in Congress were in the early 1850s, whereas the relevant presidential elections happened right before and after these events. Second, since there are more counties than congressional

[62] Paint and chemicals are included due to Southern production of turpentine. Employment in these sectors is small and affects the results little.

[63] County areas from the 1950 City and County Data Book, also accessed from the Interuniversity Consortium for Political and Social Research, were used to compute population densities, along with historical research to determine the size of the half-dozen counties which changed sizes or no longer existed in 1950.

[64] I use Thorndale and Dollarhide (1987) to determine which counties had border changes between 1860 and 1870.

districts, using counties yields a greater number of observations. Third, all of the data used here are at the county level; constructing congressional district-level data would require somewhat arbitrarily combining data from counties within multi-county congressional districts, adding measurement error. Similarly, two rather different congressional districts within the same county would be assigned the same data. Likewise, congressional districts change over time, further complicating measurement. Finally, Members of Congress may vote strategically—for example voting in favor of pro-slavery policies in return for votes for pro-development economic policy (Weingast 1998). It is likely that, due to uncertainties over electoral outcomes and coordination problems, voters are less likely to be able to vote strategically, making their votes a more accurate representation of their preferences.[65]

5. Main results

Table 2 presents the main results of the paper. The bivariate regression in column (1) is "right"-signed but not significant. Adding state dummies in column (2) nearly triples the coefficient, emphasizing the importance of state as an omitted variable. Column (3), the paper's preferred specification, further adds controls on the foreign-born population and log population density, finding that the fraction of the population in manufacturing is strongly related to a shift the Republican vote share and is significant at the 0.1% level.

Column (4) disaggregates manufacturing employment into that using Southern agricultural goods and that which does not. The coefficient on manufacturing with Southern agricultural goods is significantly larger than that on manufacturing with non-Southern manufacturing goods, though it is not significant. Given the large standard error, this analysis does not provide evidence either in favor of or against the hypothesis that access to Southern primary products was an important part of the North's motivation for keeping the South in the Union.

For interpretation, I focus on column (3). Conducting the thought experiment of what happens to the shift in the Republican vote share as the population share of each employment group goes from zero to its population-weighted average value provides one interpretation of the results. One can calculate this by multiplying each coefficient by the weighted average of its associated regressor and adding these results together. A one percentage point increase in the fraction of the population in manufacturing increases the voting share for Republicans by 0.396 percentage points, and the average employment rate in manufacturing is 5.68%. Multiplying the two together yields a shift in the Republican vote share of 2.25 percentage points associated with manufacturing employment.

Given that ten percentage points separated the two candidates in the 1864 election and a swing of a little over five percentage points would have changed the popular vote outcome, 2.25 percentage points is substantial. To better understand the meaning of the coefficient on manufacturing, given Electoral College concerns and the unequal distribution of manufacturing, I look at states whose voting outcome would have changed without this effect of manufacturing employment. With Lincoln's victory of only 0.92 percentage points in New York and 3.51 percentage points in Pennsylvania, the country's two largest states at the time, as well as a

[65] I also do not analyze votes for congressional candidates because candidate-specific characteristics would add noise to the results.

difference of 2.76 percentage points in Connecticut, these three states would have shifted their votes to the Democrats in the 1864 election without this effect of manufacturing.[66] While not enough to swing the election, this outcome would have shifted over a quarter of the electoral votes to the Democrats (65 out of a total of 233), yielding a 147-86 result in the Electoral College.

6. Robustness checks

Several robustness checks confirm the general results shown above. A first set of robustness checks, shown in Table 3, adds various controls to the preferred specification, column (3) in Table 2. In all cases, the results are little-changed. One concern might be a reversion to the mean in vote share between 1860 and 1864, so column (1) adds the Republican vote share in 1860. Another concern might be that manufacturers switch their votes to the Republicans because they are making armaments for a war which lasted surprisingly long. Column (2) adds a control for those employed in weapons manufacturing; this does not change the results, although the coefficient on weapons manufacturing itself is not precisely estimated. Controlling for church accommodation per capita (which might matter because of the religious connection to abolitionism) in column (3) and the cash value of farms per capita (which would matter if there were trends in farm areas) in column (4), as well as including all five of these controls together in column (5), does not substantively change the results.

Table 4 shows several robustness checks involving changed specifications. Column (1) runs the preferred specification without weights, and column (2) includes the 33 counties removed due to changing county boundaries. In both cases, the coefficient stays highly significant, though it is somewhat smaller. Since counties with changing boundaries may add noise, attenuating the results, this is expected.

Column (3) adds per capita capital invested in manufacturing as a kind of falsification test. If what is really driving the association between manufacturing and shift in the Republican vote share is workers concerned about their jobs, manufacturing *employment* should be a better predictor of the shift in the vote share than manufacturing *capital*. Thus, the fact that adding manufacturing capital per capita does not significantly change the results offers further evidence that the proposed mechanism drives the results.

Finally, I conduct two falsification tests in Table 5, seeing if the variables that have explanatory power for the shift in Republican vote share between 1860 and 1864 also have predictive power before and after this period. Significant coefficients with the sign seen above would lessen the legitimacy of the results during the time period under study by suggesting that the results could be due to another trend. The 1856 election was similar to that of 1860 in that both threatened to break apart the Union if the Republicans won.[67] Thus, absent trends existing

[66] Manufacturing constituted 5.93% of employment in New York, 7.73% of employment in Pennsylvania, and 14.01% of employment in Connecticut.

[67] The loss of the Republican candidate John C. Fremont to the Democratic candidate James Buchanan in the 1856 election between was widely seen as a result of a desire to keep the Union together. Bailey et al. (1998) note that, 'Perhaps more damaging [to Fremont than even his personal foibles] were the violent threats of the southern "fire-eaters" that the election of a sectional "Black Republican" would be a declaration of war on them, forcing them to secede. Many northerners, anxious to save both the Union and their profitable business connections with the South, were thus intimidated into voting for Buchanan" (Bailey et al. 1998; see also O'Connor 1968: 122). Similarly, the New York *Times* wrote, "The Southern press, of every political shade of opinion, with hardly an exception, threatens

before the 1860 election which might be problematic for the main regressions above, we would expect little shift in Republican vote share between 1856 and 1860. The regressions in these columns are very reassuring.[68] Columns (1) – (3) are falsification tests for the change in vote share between 1856 and 1860.[69] Manufacturing employment is insignificant, close to 0, and fairly precisely estimated in all three specifications.

Columns (4) – (6) show falsification tests for the change for 1864 to 1868. While the Civil War was over in 1868, the vote was largely a matter of the ability of the North to maintain its hegemony over the South, somewhat similar to the goals in 1864. Republicans in 1868 threatened that voting Democratic would be akin to reopening the Civil War. Since the goal of keeping the South in the Union had been accomplished, we might expect some decline in the Republican vote share in areas with manufacturing, but perhaps not so much as to mask trends in the other direction which would call into question the above results. The results in these columns are close to 0, insignificant, and precisely estimated. The absence of any positive and statistically significant results provides reassurance that the results in the main regressions are not the result of existing trends.

Overall, several features of the regressions here provide strong evidence that the relationships shown are not the result of omitted variable bias. The results are robust to the inclusion of state dummies, a variety of controls, and not having weights. Importantly, there is no evidence of pre-trends or post-trends in any of the relevant economic variables which could undermine the main results.

7. Interpretation

Even having addressed many questions of omitted variable bias, to interpret the results as coming from a concern for keeping the South in the Union, I must argue that there are not other omitted variable bias problems or other concerns of the manufacturing sector that could have caused this shift. As described above, there are good economic reasons to believe that manufacturing industries would be concerned about the South leaving the Union. Here, I argue that there do not seem to be other explanations of the results above.

Although tariff politics were important during this time period, they imply a shift in the direction *opposite* to that which we see. With the much higher rates of the Morrill Tariff, passed in early 1861, tariffs became a less important issue for Northern manufacturing interests (Taussig 1910). Since the Democratic Party supported low tariffs while the Republican Party supported high ones, manufacturers assigning less importance to lower tariffs would predict shifts from 1860 to 1864 *toward the Democrats*, not the Republicans. Thus, the effect of concerns for the tariff would, if anything, decrease the magnitude of the results.

A second issue could be that it was not concern for keeping the South in the Union, but rather profiting from the war itself that drove manufacturing interests. However, I controlled for the armament industry and the results did not change. More importantly, if profiting from the

disunion in the event of defeat in the present contest for the presidency" (New York *Times*, August 29, 1856, quoted in Potter 1942).

[68] The regressions exclude counties which changed boundaries between 1850 and 1860 according to Thorndale and Dollarhide (1987).

[69] To calculate the Republican vote share, I include the vote shares for the Republican Party and for the nativist American Party, most of whose voters migrated to the Republican Party.

war were an important motivation, one would predict that manufacturers would anticipate the benefits from the war in 1860 and vote Republican in both 1860 and 1864, leading to little shift.

A third question might be what was happening to interests other than those I test for, like agriculture, over the time between 1860 and 1864: perhaps they were trending away from the Republicans in ways that make it appear that the economic interests whose effects I *am* testing are increasing their support for the Republicans. Indeed, as Weingast (1998) argues, coalitions were shifting around this time: partly because of increased immigration, Northern business interests became more willing to accept Western expansion through the provision of free land. So perhaps farmers in the east or west may have been more willing to vote for Republicans, who were increasingly favorable to land distribution, instead of the pro-agrarian Democrats, across time. However, this factor merely strengthens the results above, since the effects of the agricultural interest are trending *in the same direction* as the economic interests for which I am testing. In any case, though, as noted above, controlling for per capita agricultural interests did not affect the results.

Fourth, one might be concerned that manufacturers switched toward the Republicans because the Republican platform became more favorable to them from 1860 to 1864 relative to that of the Democrats. There are several reasons why this is unlikely to be the case. First, the same Republican candidate was running in both elections. More importantly, if anything, the Republicans were *less sympathetic* to the concerns of manufacturers in 1864 than 1860 as the War consumed most of the parties' attention. For example, among manufacturing interests, the 1860 Republican Party platform mentions the tariff, "development of the industrial interest of the whole country," appropriations for river and harbor improvement, and a railroad to the Pacific, while the 1864 platform mentions only the railroad to the Pacific. The Democrats mention industrial policy in neither document. War and the Union were dominant issues in these elections, even more so in 1864, since the country was engulfed in the conflict. To the extent that shifts in platform may have also led to shifts in votes, it is likely that they would lead to shifts *away* from Republicans in the manufacturing sector from 1860 to 1864.

Fifth, another potential concern might be that soldiers' absence from the data in 1864 but not 1860 could lead to omitted variable bias. However, for the absence of soldiers in 1864 to explain these results, soldiers would have to be either disproportionately Democrats from areas with lots of manufacturing or disproportionately Republicans from areas with little manufacturing. I am unaware of anything that supports either of these two scenarios.[70]

Sixth, a belief in 1860 that a potential war with the South would be a cheap, easy win cannot explain the results.[71] If a war would both be low-cost and have a very high likelihood of regaining the South, then there would be no tendency for voters with manufacturing interests to vote for Democrats in 1860. A war would essentially not matter, neither costing them taxes nor risking the loss of the South, so they could vote for the Republicans without concern. However, this factor would *reduce* the shift toward Republicans, not increase it, since there would be fewer Democratic votes in areas with large amounts of manufacturing in 1860.

[70] One could also imagine more complicated stories about soldiers affecting voters at home or changed preferences of soldiers returning from war, but these seem very unlikely to be related to manufacturing employment share.
[71] The plausibility of this belief is difficult to ascertain. Gunderson notes that "the Treasury and War departments and Congress all readily agreed that the war should last less than one year, require 300,000 troops and cost about $265,000,000," but this analysis was not widely known (Gunderson 1974: 930). See also and Goldin and Lewis (1975) and Goldin (1973).

Finally, Anderson and Tollison (1991) find that states with more electoral votes per capita had lower casualty rates. This would bias down the estimates if high-population states (which therefore had more electoral votes per capita) both had a lot of manufacturing and received unfavorable treatment during the War, leading to more discontent at home and a greater shift away from the Republicans. However, controlling for state removes the concern.

Note also how well these economic explanations fit with the standard story of there being resistance to war until the Fort Sumter attack and then support afterwards (Foner 1941). The Fort Sumter attack was precisely the point at which the best way to keep the South in the Union flipped from accommodation to war. The typical argument is that Northern preferences changed: the North was 'galvanized' by the attack on Fort Sumter and was then willing to be led into war. However, this galvanization also coincided with new information: the South was intent on seceding; the first-best option of peaceful re-unionization was no longer an option, so it was time to go to the second-best option. Thus, the standard story might be mistaking a change in *information* only for a change in *preferences.*

Stampp notes that, "because of their commanding political and economic positions the compromise drive was always spearheaded by eastern merchants and manufacturers" (Stampp 1970: 126). For example, "practically every prominent New York capitalist" met in December, 1860, to at least superficially appeal to the South for compromise. Bostonian businessmen acted similarly. However, once it was clear that it would take war to keep the South in the Union, areas that had been in favor of reconciliation turned quickly toward war. "Union rallies were most numerous in the large cities of the East and in the border areas of the West where the people favored conciliation overwhelmingly. There the specter of financial ruin made other issues shrink into insignificance" (Stampp 1970: 126). Thus, these economic hypotheses also agree with anecdotal evidence on the timing of changes in support for the war.

8. Conclusion

I have argued that an approximately 2.25 percentage point shift in voting for Republicans between 1860 and 1864 is attributable to the economic interests of manufacturing, a factor pivotal in states accounting for over a quarter of electoral votes. Causes other than a desire to keep the South in the Union seem unable to explain this shift, suggesting that a concern for keeping the South as part of the Union motivated this 2.25% of voters. If 2.25% of voters were on the margin between voting for the Democrats and Republicans and changed their votes to keep the South in the Union, then the actual fraction of the population with these economic interests was, no doubt, much larger, since many with this interest either kept voting Republican or were motivated enough by other concerns to keep voting Democratic. Thus, given that Lincoln could not have taken any action that did not reflect the will of at least a substantial part of the population, this paper offers strong evidence that economic motivations were one important component in the North's decision to fight the South after it seceded.

More broadly, this paper is a case study on why states tend to try to fight secessionist movements, even in democracies where leaders are less able to extract rents from seceding regions. These results suggest that powerful groups may benefit economically from having a union and that their influence may encourage countries to resist secession, even of relatively poor regions like the U.S. South.

TABLES

Table 1: Summary statistics, weighted by population size

Variable	Mean	Standard	Minimum	Maximum
Change in fraction Republican vote share between 1860 and 1864	0.0006	0.0751	-0.3800	0.7390
Employment in manufacturing	0.0568	0.0615	0.0005	0.4841
Employment in manufacturing with Southern agricultural goods	0.0030	0.0070	0.0000	0.0750
Fraction of population of foreign origin	0.1855	0.1361	0.0047	0.6828
Log population density (persons / sq. mi.)	4.5307	1.8210	-2.7389	10.5183

Note : There are 716 observations, except for employment in manufacturing with Southern agricultural goods, which has 674. The sample includes all counties from non-border Union states with data for manufacturing employment and the control variables. The employment variables are measured as a fraction of total county population.

Source : Clubb et al. (2006), Geospatial and Statistical Data Center (2004), and 1860 Census of Manufactures.

Table 2: Change in fraction Republican vote share between 1860 and 1864

	(1)	(2)	(3)	(4)
Employment in manufacturing	0.127	0.330***	0.396***	
	[0.0876]	[0.123]	[0.118]	
Employment in manufacturing without				0.382***
Southern agricultural goods				[0.126]
Employment in manufacturing with Southern				0.608
agricultural goods				[0.436]
Fraction of population of foreign origin			-0.192***	-0.192***
			[0.0379]	[0.0380]
Log population density (persons / sq. mi.)			0.0117***	0.0117***
			[0.00388]	[0.00390]
Constant	-0.00661	-0.116***	-0.146***	-0.146***
	[0.00409]	[0.0241]	[0.0251]	[0.0250]
State dummies		X	X	X
Observations	716	716	716	674
R^2	0.011	0.474	0.516	0.513

* = Significant at the 10 percent level.
** = Significant at the 5 percent level.
*** = Significant at the 1 percent level.

Note : Robust standard errors are in brackets. The regressions are population-weighted. The employment variables are measured as a fraction of total county population.

Source : Clubb et al. (2006), Geospatial and Statistical Data Center (2004), and 1860 Census of Manufactures.

Table 3: Change in fraction Republican vote share between 1860 and 1864: other controls

	(1)	(2)	(3)	(4)	(5)
Employment in manufacturing	0.400***		0.384***	0.362***	
	[0.119]		[0.116]	[0.116]	
Employment in non-military manufacturing		0.393***			0.364***
		[0.117]			[0.116]
Fraction of population of foreign origin	-0.186***	-0.193***	-0.201***	-0.219***	-0.211***
	[0.0369]	[0.0385]	[0.0394]	[0.0427]	[0.0426]
Log population density (persons / sq. mi.)	0.00878**	0.0117***	0.0115***	0.0107***	0.00825*
	[0.00420]	[0.00390]	[0.00399]	[0.00385]	[0.00432]
Fraction of vote won by Republicans in 1860	-0.0911**				-0.0852**
	[0.0370]				[0.0365]
Employment manufacturing weapons and gunpowder		2.759			2.774
		[2.851]			[2.693]
Church seats per capita			-0.0184*		-0.0135
			[0.0109]		[0.0117]
Cash value of farms per capita				-0.0683**	-0.0448
				[0.0293]	[0.0300]
Constant	-0.0807**	-0.150***	-0.127***	-0.118***	-0.0576
	[0.0354]	[0.0284]	[0.0245]	[0.0262]	[0.0382]
State dummies	X	X	X	X	X
Observations	716	674	678	713	640
R^2	0.528	0.513	0.52	0.521	0.532

* = Significant at the 10 percent level.
** = Significant at the 5 percent level.
*** = Significant at the 1 percent level.
Note : Robust standard errors are in brackets. The regressions are population-weighted. The employment variables are measured as a fraction of total county population.
Source : Clubb et al. (2006), Geospatial and Statistical Data Center (2004), and 1860 Census of Manufactures.

Table 4: Change in fraction Republican vote share between 1860 and 1864: other robustness checks

	(1)	(2)	(3)
Employment in manufacturing	0.321***	0.280**	0.354**
	[0.0858]	[0.122]	[0.176]
Fraction of population of foreign origin	-0.185***	-0.170***	-0.193***
	[0.0459]	[0.0390]	[0.0385]
Log population density (persons / sq. mi.)	-0.0035	0.0134***	0.0117***
	[0.00487]	[0.00430]	[0.00388]
Manufacturing capital invested per capita			0.0675
($1000)			[0.169]
Constant	-0.0636**	-0.141***	-0.146***
	[0.0263]	[0.0273]	[0.0250]
State dummies	X	X	X
Weights		X	X
Includes boundary-changing counties		X	
Observations	749	716	731
R^2	0.556	0.516	0.521

* = Significant at the 10 percent level.
** = Significant at the 5 percent level.
*** = Significant at the 1 percent level.
Note: Robust standard errors are in brackets. The employment variables are measured as a fraction of total county population.
Source: Clubb et al. (2006) and Geospatial and Statistical Data Center (2004).

Table 5: Change in fraction Republican vote share: falsification tests

	(1)	(2)	(3)	(4)	(5)	(6)
		1856 to 1860			1864 to 1868	
Employment in manufacturing	-0.113	0.0156	0.0506	-0.0573	-0.0708	0.0563
	[0.142]	[0.0847]	[0.122]	[0.0437]	[0.0755]	[0.0625]
Fraction of population of foreign			0.0600*			0.0993***
origin			[0.0363]			[0.0367]
Log population density (persons / sq.			-0.00620*			-0.0147***
mi.)			[0.00342]			[0.00426]
Constant	-0.0321***	0.018	0.0313	0.00541**	0.0135	0.0462***
	[0.00604]	[0.0221]	[0.0224]	[0.00231]	[0.0116]	[0.0107]
State dummies		X	X		X	X
Observations	586	586	586	749	749	749
R^2	0.007	0.719	0.725	0.001	0.054	0.081

* = Significant at the 10 percent level.
** = Significant at the 5 percent level.
*** = Significant at the 1 percent level.
Note: Robust standard errors are in brackets. The regressions are population-weighted. The employment variables are measured as a fraction of total county population.
Source: Clubb et al. (2006) and Geospatial and Statistical Data Center (2004).

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