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# Did the States Pocket the Obama-Stimulus Money? Lessons from Cross-Section Regression and Interviews with State Officials

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*This paper deploys cross-section regressions and open-ended interviews to assess the impact of grants to the states under the Obama stimulus (ARRA) of 2009-2011. Contrary to John Taylor and John Cogan (Taylor 2011a, b; Cogan and Taylor 2012), who conclude that the states saved rather than spent the grants, we estimate that approximately two-thirds of every ARRA dollar was spent by the states and one-third saved. (JEL E62, H72, H50)*

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Spiegler: University of Massachusetts Boston Economics Department, 100 Morrissey Boulevard, Boston, MA 02125 (email: peter.spiegler@umb.edu). Sam Harland provided excellent research assistance. We are not sure he knew what he was in for when he volunteered for this project, but he put in long hours, responding to many challenges that would have foiled a less resourceful and diligent analyst. Without Sam's help, we would still be sorting the data. Michael Ash provided important econometric advice. Noah Berger helped us to learn the ropes of state budgeting, as did Leslie Kirwan. Many government officials responded to our requests for information and clarification of data. Apart from the state budget officers and their staffs, without whom the interviews reported below would have been impossible, we relied at various points on Federal government officials for clarification of the data. These include officials of the Bureau of Economic Analysis and the Census Bureau of the Department of Commerce, officials of the Department of Health and Human Services, and officials of the Board of Governors of the Federal Reserve System. The authors have no financial interests that relate to this paper. Peter Spiegler gratefully acknowledges financial support from the Institute for New Economic Thinking for a related project during the period over which this paper was researched and written. Material on pp. 28-9, 31 and 32 is drawn from Marglin and Spiegler (2013b).

The 2009 American Recovery and Reinvestment Act (ARRA) was the most far-reaching experiment in fiscal stimulus in the history of the American economy. Like all fiscal stimulus programs, it was designed to raise GDP and employment above what they would have been in the absence of stimulus.

Did it work?

In fact the ARRA has been a Rorschach test of sorts, with assessments falling neatly along the left-right political divide: the left trumpets its success—or did until stimulus became a four-letter word spelled with eight letters—while the right regards it as a failed adventure in big government.<sup>1</sup> Even economists, who pride themselves on being responsive to, indeed, driven by, “the facts,” have not been immune. Many have found in the ARRA confirmation for deep prejudices about the role of government, about conceptions of how the economy works, even about the methodology of economics. As we discuss below, there is not much more consensus among economists on this issue than there is among politicians, and the pros and cons tend to break down along party lines.<sup>2</sup>

Why has consensus been so hard to come by? Certainly, one reason is that the macroeconomy is complicated—there will almost always be *some* measure of controversy over the proper answer to a macroeconomic puzzle. But that has not been the central problem in this case. The more important impediment has been a lack of clarity about the terms of the debate—the framework of assumptions underlying assessments of the ARRA’s effectiveness.

We can usefully distinguish three types of assumptions. *Identifying Assumptions* are those that allow us to interpret regression coefficients in the standard way—for example, the assumption that the residuals are independent and

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<sup>1</sup> The portion of the October 11, 2012 Vice-Presidential debate between Joe Biden and Paul Ryan devoted to the stimulus neatly summarizes the positions of the parties. See Commission on Presidential Debates (2012).

<sup>2</sup> See, for example, the exchange between John Cochrane (2010) and Joseph Stiglitz (2010). We do not mean to suggest that left and right-leaning economists who have cleaved to their party’s line have done so solely for ideological reasons. We merely point out the tendency for the economic positions on ARRA to correlate with the political positions.

identically distributed with mean zero and constant variance. *Behavioral Assumptions* are assumptions about the underlying behavior that generated the data—for example the assumption that agents engage in consumption smoothing. And *Counterfactual Assumptions* are assumptions about the values of the variables of interest in a hypothetical state of the world in which the intervention does not occur. Any valid assessment of the ARRA (or any policy intervention, for that matter) must be built on a foundation of assumptions across these three areas: a properly identified empirical model and a set of plausible counterfactual and behavioral assumptions used in interpreting the results of the analysis. For example, a regression that finds a positive effect of the ARRA on GDP is evidence of the ARRA’s success only if the model used to generate the results is properly identified, one’s counterfactual assumption is that GDP would have increased less than was observed, *and* one can provide sufficient evidence that the behavioral assumptions underlying the counterfactual assumption are plausible.<sup>3</sup> Although these standards may seem obvious, we argue that lack of clarity about them has muddled the debate over the ARRA.

In this paper, we examine one particular aspect of that debate: the question of whether the portion of the ARRA stimulus channeled through state governments (hereinafter “S-ARRA”) achieved its purpose. The S-ARRA is of particular interest for two reasons. First, it comprises a substantial portion of the total stimulus—roughly \$250 of the \$800 million total, around 30%. Second, the fact that there are only 50 states allows us to explore the relevant behavioral and counterfactual conditions of the recipients of S-ARRA in relatively fine-grained detail.

Our contention is that under empirically valid counterfactual and behavioral assumptions and a properly identified empirical model, the S-ARRA gets high marks. We demonstrate this by reviewing the leading argument against

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<sup>3</sup> These are only necessary conditions for the validity of the assessment, not sufficient conditions.

the S-ARRA's effectiveness: that state governments smooth their expenditures and, therefore, that any temporary fiscal stimulus will be largely, if not completely, saved rather than spent. We argue that the premises and conclusions of the expenditure-smoothing argument are highly sensitive to behavioral and counterfactual assumptions that are implausible in the case of the S-ARRA. By paying greater attention to the empirical realities of the execution of the S-ARRA, we formulate more plausible behavioral and counterfactual assumptions and demonstrate that under these assumptions the evidence suggests that the S-ARRA stimulus achieved its stated goals.

The paper proceeds in five sections. In section 1 we provide some context by briefly reviewing the basic terms of the debate over the effectiveness of fiscal stimulus in general and of the total ARRA stimulus in particular. In section 2 we review the leading argument against the effectiveness of the S-ARRA: the striking claim that the S-ARRA failed to stimulate the economy because state governments did not spend any more than they would have in the absence of stimulus; rather, they acted like consumption-smoothing individual agents, using the windfall injections into their coffers to shore up their balance sheets.

The most prominent exponents of this position are John Taylor and John Cogan. In their view the stimulus did nothing more than to provide debt relief—by substituting the debt of the US for the debt of the several states. Cogan and Taylor's conclusions are founded on the position that the proper counterfactual is that states would have maintained spending at pre-recession levels in the absence of S-ARRA, and, therefore, that the S-ARRA can be judged a success only to the extent that the observed spending levels exceeded this counterfactual steady path.

In this paper we present two investigations of the impact of S-ARRA that call into question both Cogan and Taylor's conclusions and the assumptions underlying them. In section 2 we examine the differential impact of the S-ARRA money on spending across the 50 states. For this analysis, we operate under the

working hypothesis that states *could have* borrowed in order to conduct business as usual with respect to spending and so might simply have substituted S-ARRA funds for withdrawals from their bank accounts. Our analysis leads to the conclusion that two-thirds of the money that went to the states was actually spent while only one-third went to shore up balance sheets. In section 3, we test the working hypothesis of Section 2 that, absent the S-ARRA, states could have borrowed enough to sustain pre-recession levels of spending. In this section we report the results of interviews with state budget officers about the impact of the S-ARRA on state government finances. The interviews were designed to elicit answers to the question of whether or not states could have borrowed as Section 2 assumes. These interviews provide a remarkably uniform set of responses that support a clear conclusion: with very few exceptions, the counterfactual claim that the states could have avoided spending cuts in the absence of the S-ARRA is implausible.

We conclude, in section 4, by discussing the general implications of our findings both for the S-ARRA and for the evaluation of fiscal stimulus programs in general. We argue that because of the politically charged nature of these programs and the attendant danger of ideological bias in their evaluation, it is especially important to hold the framing assumptions of any economic evaluation to a high standard of empirical fidelity. As we demonstrate here, direct appeal to the experience of the practitioners on the front lines of such programs can be an invaluable reality check on these assumptions, and a means of resolving disagreements .

## 1. Theory and Data on the Effectiveness of Fiscal Stimulus in General

The official website of the stimulus program, [www.recovery.gov](http://www.recovery.gov), divides the stimulus into three roughly equal parts, as in Table 1.

TABLE 1—THE AMERICAN RECOVERY AND REINVESTMENT ACT WRIT LARGE (\$BILLIONS)

Tax Benefits	297.8
Contracts, Grants and Loans	237.5
Entitlements	229.2
Total	764.5

Each of these categories includes a multitude of programs. Take “Tax Benefits.” The largest single Tax Benefit program was an across-the-board benefit enjoyed by over 116 million taxpayers, Making Work Pay, which provided a \$400 credit for an individual and a \$800 credit for a couple with two working spouses for the 2009 and 2010 tax years, phasing out only at relatively high levels of \$75,000 for an individual taxpayer and \$150,000 for a couple filing joint returns. The total benefit amounted to \$104.1 billion. At the other end of the spectrum were adjustments to the Alternative Minimum Income Tax totaling over \$69 billion, which accrued to 13 million taxpayers. Tax Benefits also included some \$11 billion of credits for improving residential energy efficiency, enjoyed by 6 million taxpayers and \$33 billion of tax breaks for businesses.

“Grants, Contracts, and Loans” consisted chiefly of grants to states and local governments (the vast majority to states) for purposes ranging from education to highway construction and repair—\$180 billion by the count of the Bureau of Economic Analysis, excluding Medicaid funding. But this category also included contracts with private entities totaling almost \$30 billion, contracts which ranged from thousands of dollars to \$1.5 billion awarded to Savannah River Nuclear Solutions to clean up the Savannah River Site, at which production

of nuclear materials for the military's nuclear arsenal took place during the Cold War.

“Entitlements,” the smallest of the three parts, consisted primarily of three programs: Medicaid grants to the states (\$92 billion), extension of unemployment insurance (\$61.8 billion), and family services (\$41 billion, of which the lion's share was food stamps).

#### *A. Controversy over the impact of the stimulus*

In the present political climate it is perhaps not surprising that politicians respond to countercyclical fiscal policy along party lines. Democrats credit the stimulus for keeping the economy going after the financial upheaval that culminated in the fall of Lehman Brothers in September of 2008. Many feared that the recession already under way for the better part of a year would turn into a depression of 1930s proportions, which hit bottom with one-third of the non-agricultural labor force without jobs. In contrast, US unemployment never climbed above 10 percent after Lehman went under, and the economy has been slowly improving since the middle of 2009, when the stimulus kicked in. Republicans argue that things would have got better much faster were it not for the Obama Administration's policies: the stimulus was a colossal waste which added neither jobs nor income, but only increased the national debt. Case in point: Solyndra, the solar panel manufacturer that received \$527 million in ARRA guaranteed loans in 2009, only to go bust in 2011.

It is perhaps more surprising that economists are also divided. The Congressional Budget Office—whose estimates reflect (and perhaps to some extent mold) a consensus view—estimates that, at its peak in the spring and summer of 2010, the stimulus added between 700,000 and 3.5 million jobs, and



between 0.8 and 4.6 percent to Gross Domestic Product (Congressional Budget Office 2012, Table 1). Table 2 summarizes the CBO’s after-the-fact analysis and the estimates of the Obama economic team in advance of the stimulus. These estimates, at least if we confine ourselves to the midpoints reported in column 7, are very close to what President Obama’s advisers estimated the results of the stimulus would be, at least with regard to its incremental impact.<sup>4</sup>

TABLE 2— IMPACT OF ARRA ON GDP, AS ESTIMATED BEFORE THE FACT BY PRESIDENT OBAMA’S ECONOMIC TEAM AND EVALUATED AFTER THE FACT BY THE CBO

(1) Year and Quarter	(2) GDP (\$ bil.SAAR)	Before the Fact		After the Fact (Percentage of GDP)		
		(3) Contribution of Stimulus to GDP (\$ bil. SAAR)	(4) As Percentage of GDP	(5) Low	(6) High	(7) Midpoint
2009 Q1	13,893.7	31.2	<b>0.2</b>	0.0	0.1	<b>0.05</b>
2009 Q2	13,854.1	75.8	<b>0.5</b>	0.4	1.4	<b>0.9</b>
2009 Q3	13,920.5	194.8	<b>1.4</b>	0.6	2.4	<b>1.5</b>
2009 Q4	14,087.4	227.6	<b>1.6</b>	0.7	3.4	<b>2.05</b>
2010 Q1	14,277.9	265.5	<b>1.9</b>	0.9	4.3	<b>2.6</b>
2010 Q2	14,467.8	313.5	<b>2.2</b>	0.8	4.6	<b>2.7</b>
2010 Q3	14,605.5	330.7	<b>2.3</b>	0.7	4.1	<b>2.4</b>
2010 Q4	14,755.0	336.0	<b>2.3</b>	0.6	3.5	<b>2.05</b>
2011 Q1	14,867.8	307.1	<b>2.1</b>	0.6	3.2	<b>1.9</b>
2011 Q2	15,012.8	226.4	<b>1.5</b>	0.4	2.5	<b>1.45</b>
2011 Q3	15,176.1	195.4	<b>1.3</b>	0.3	2.0	<b>1.15</b>
2011 Q4	15,319.4	169.3	<b>1.1</b>	0.2	1.5	<b>0.85</b>

Sources: St. Louis Federal Reserve website for GDP; Bureau of Economic Analysis, Department of Commerce for primary effect of ARRA on components of GDP; Romer and Bernstein (2009) and authors’ calculations for estimates in columns 3 and 4; Congressional Budget Office (2012) for estimates in columns 5, 6, and 7.

The range of its estimates runs from a lackluster impact at the low end (column 5)—never more than a 1 percent boost in GDP—to stellar at the high end (column 6), accounting for all the growth of the economy, and then some, in 2010. In the end, something for everybody. Rorschach wins.

<sup>4</sup> Almost everybody inside and outside the Administration underestimated the severity of the downturn. Obama’s economic team thus missed the mark with regard to *levels* of GDP and employment that the stimulus would achieve, but their predictions for the incremental impact of the stimulus were nonetheless close to the after-the-fact measurements of the CBO.

While even at the low end of the estimates there is some stimulus to employment and output, there is a surprising amount of disagreement within the economics profession regarding the ARRA's contribution. According to a survey of 41 leading economists conducted by the Initiative on Global Markets (a project of the University of Chicago's Booth School of Business), only 80 percent of the respondents—the sum of those economists who “agree” and those who “strongly agree”—concurred with the view that the stimulus added jobs (Initiative on Global Markets 2012).<sup>5</sup>

Not only does 80 percent fall far short of the near-unanimity one might expect if economics lived up to its claims of scientific status, but the dissenters include many distinguished economists. Even before the stimulus was enacted, Robert Barro (2009) pronounced it dead on arrival. In his view the stimulus would crowd out other economic activity rather than unleashing a virtuous circle of spending. John Taylor is perhaps the most widely known and the most vocal naysayer with regard to the stimulus, and Taylor—unlike Barro who was writing before the ink was dry on the stimulus legislation—offers empirical evidence on the effects of the ARRA to make his case. With John Cogan, Taylor has forcefully challenged the consensus on the stimulus. Additionally, Taylor has made his views about the stimulus known not only in academia, but also in more accessible form, with Cogan in the *Wall Street Journal* (Cogan and Taylor 2011), on National Public Radio, and in congressional testimony (Taylor 2011b). As he put it on NPR,

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<sup>5</sup> Regarding the makeup of the IGM panel, the organization's website explains:

Our panel was chosen to include distinguished experts with a keen interest in public policy from the major areas of economics, to be geographically diverse, and to include Democrats, Republicans and Independents as well as older and younger scholars. The panel members are all senior faculty at the most elite research universities in the United States. The panel includes Nobel Laureates, John Bates Clark Medalists, fellows of the Econometric society, past Presidents of both the American Economics Association and American Finance Association, past Democratic and Republican members of the President's Council of Economics, and past and current editors of the leading journals in the profession. This selection process has the advantage of not only providing a set of panelists whose names will be familiar to other economists and the media, but also delivers a group with impeccable qualifications to speak on public policy matters.

I have looked at [the stimulus] with the numbers, looked at what happened, traced the money, and I don't find an impact. The studies that show it had an impact, they just simulate models. When I look at the data, where it went, temporary tax reductions went into people's pockets, they didn't spend it. This money that [was] sent to the states, they didn't spend it. They actually put it in their coffers. You can't see any impact on the infrastructure or the things that were supposed to happen. And those are the facts. (Taylor 2011c)

Taylor is on solid ground in pointing out that the CBO's evaluation presupposes a particular structure, a particular model of the economy, indeed the same model that served as the basis for Obama's advocacy of the stimulus. (Auerbach, Gale, and Harris (2011, 156) and Wilson (2011, 6) make the same point.) In fact, in arriving at its *ex post* assessment of the effectiveness of ARRA, the CBO does little more than to substitute the actual timeline of disbursements for the (*ex ante*) conjectural timeline of Federal disbursements that was used to argue for the stimulus in the first place (Christina Romer and Jared Bernstein, 2009). And though the details were modified as the legislation moved through the various committees of the House and Senate, the overall size of the stimulus did not change very much from the projected figure used by Obama's advisers to the figure actually enacted.

However, Taylor is on less solid ground in claiming to be different from the analysts he criticizes—in effect claiming that he offers objective analysis while they offer ideology. According to Taylor, he and Cogan impose no model on the data but instead let the facts speak for themselves. This is true in the sense that the multiplier is estimated from data that include the ARRA rather than extrapolating, as the CBO does, the results of models based on pre-ARRA data. But Taylor's facts are interpretation, no different in this respect from Christina

Romer's or Robert Barro's—or ours. He, like everybody else, is arguing against the background of counterfactual assumptions—about what the states (or individuals) would have done without the stimulus—which in turn rest on a particular model of the economy; to justify this model he needs a plausible argument about behavior. There are no (counter)facts without a structure of interpretation.

As we turn to the assessments of the S-ARRA's performance, it is important to keep the centrality of interpretive frameworks in mind, as ultimately it is these frameworks—rather than the data themselves—that are at the core of the disagreement between supporters and detractors. We begin here with an examination of one of the central points of disagreement over the ARRA: the controversy over the proper value of the multiplier. As we argue below, the differing positions on this issue stem mostly from disagreement about the underlying behavioral model. To an unfortunate extent, behavioral assumptions are kept in the background, even presented as self-evident features of the world. As a result the debate often devolves into two sides talking past each other and the issue seems to remain implacably partisan in nature.

### *B. Behind the controversy: the size of the multiplier*

There are two points of contention: how much government spending crowds out private spending, and how much income goes into shoring up balance sheets rather than being spent. There is something approaching consensus about the first question, namely, that in slack times there will be less crowding out. Both theory and data lead to this unsurprising conclusion, but the theory is far from uniform. The standard textbook view supposes the economy is at full employment, and employs a loanable funds model to argue that the real interest

rate will rise in response to the greater demand coming from the government (e.g., Mankiw 2007, 588-590). A more recent literature (e.g., Woodford 2011) models the interest rate, and hence the response of economic activity to fiscal initiatives, in terms of the central bank's reaction function. At one limit, when the central bank is able to keep the economy at its preferred combination of inflation and unemployment, crowding out will be complete because any fiscal action will be offset by monetary policy; the monetary authority may fail to achieve its goal, but if it reacts to offset fiscal policy, crowding out will take place. Both the standard textbook, loanable funds, argument and the more sophisticated reaction-function argument suggest that in the conditions that faced the US in early 2009, with the policy rate at its zero lower bound and unemployment approaching 10 percent, crowding out would be minimal.

The data appear to confirm the consensus view. Valerie Ramey (2011) surveys a large literature focused on empirical estimates of the multiplier and concludes

The range of plausible estimates for the multiplier in the case of a temporary increase in government spending that is deficit financed is probably 0.8 to 1.5... If the increase is undertaken during a severe recession, the estimates are likely to be at the upper bound of this range. It should be understood, however, that there is significant uncertainty involved in these estimates. Reasonable people could argue that the multiplier is 0.5 or 2.0 without being contradicted by the data. (Ramey 2011, 680-681)

Despite using very different identification methods, many of these cross-state studies find multipliers on purchases or transfers of about 1.5 to 1.8 for income and an implied cost of around \$35,000 per job created. Several studies also find that the multiplier is significantly higher during

times of higher slack. (Ramey 2011, 683)

Auerbach and Gorodnichenko (2012) explicitly take into account differences between economic conditions by adding the possibility of regime change to the standard structural vector autoregression model of Olivier Blanchard and Roberto Perotti (2002). Their results exhibit substantially higher multipliers in slack conditions. But, once again, consensus does not mean unanimity. Ramey herself (Owyang, Ramey, and Zubairy 2013), using an estimation method introduced by Jordà (2005), finds no evidence that the unemployment rate influences the size of the government-expenditure multiplier.

The second issue is whether or not income recipients spend or save. The logic of both permanent-income and life-cycle hypotheses (Friedman 1957; Modigliani and Brumberg 1954; Ando and Modigliani 1963) is that rational agents smooth consumption in the face of variations in income; the result is that consumption is not sensitive to temporary increases in income. The multiplier is thus sensitive both to the fraction of the population assumed to follow the logic of consumption smoothing, as well as to whether tax reductions or transfers are (in fact or perception) temporary in nature. Models that assume a large fraction of the population are liquidity constrained typically suggest larger multipliers than multipliers that assume agents rationally smooth expenditures.

The literature on the multiplier also takes note of the effects of the composition of government spending. Auerbach and Gorodnichenko (2012) find that multipliers associated with military spending are much higher than other kinds of spending. This observation is especially relevant to fiscal activism that takes the form of transfers or tax reductions since the fraction of liquidity-constrained agents will be higher among lower income groups and so will the multiplier associated with transfers or tax reductions to these groups. On this logic

it makes sense for the CBO to have assigned different multipliers to different portions of the tax and transfer provisions of the ARRA.

All this literature bases multiplier estimates on pre-ARRA historical data. But direct estimates based on the ARRA itself quickly followed its implementation. These studies focused on the effects of grants to states by using cross-section data relating grants to changes in employment. Three of them (Wilson 2011; Chodorow-Reich et al. 2012; Feyrer and Sacerdote 2011) find substantial employment multipliers. There is, however, considerable variation in the estimates because each of these papers uses a different set of instruments to take account of the problem of endogeneity (variations in unemployment and other measures of economic conditions presumably influenced the size of the grant received by a state). One cross-sectional study (Conley and Dupor 2011) found that crowding out meant that the expansion of jobs due to ARRA transfers to the states were offset by the loss of private jobs.

In the context of this generally favorable view of the stimulus, the work of Taylor and his collaborator, John Cogan, is striking. Taylor (2011a) uses aggregate time series on consumption and income to estimate the impact of the portions of the ARRA that involved tax reduction and transfers to individuals. Cogan and Taylor (2012) also use aggregate time series to investigate the impact of grants to states. Again, they find no impact on spending. Both results are attributed to consumption smoothing.

A notable feature of Cogan and Taylor's argument is the idea that the same logic that applies to households also applies to state and local governments. Expenditure smoothing is standard fare in economics when it comes to households, but it is relatively novel to apply it to state and local government. Their results, then, constitute a new addition to the general consumption-smoothing-based argument against temporary fiscal stimulus. And this new addition has been taken up in the literature, with Cogan and Taylor's result now

regularly cited in discussions of the proper value of the fiscal spending multiplier and of the wisdom of fiscal stimulus policy.<sup>6</sup>

Cogan and Taylor's analysis is straightforward. First they estimate the impact of the S-ARRA by regressing aggregate purchases of goods and services (G) and transfers (E) by states on non-ARRA revenues (R) and ARRA grants (A), along with lagged values of the dependent variables:

$$G = a_0 + a_1G_{-1} + a_2R + a_3A + \mu$$

$$E = b_0 + b_1E_{-1} + b_2R + b_3A + \xi$$

From these two equations, Cogan and Taylor estimate aggregate saving on the part of the states (L) as

$$L = -(a_0 + b_0) - a_1G_{-1} - b_1E_{-1} + (1 - a_2 - b_2)R + (1 - a_3 - b_3)A - \mu - \xi$$

This last equation is obtained from the first two by means of the identity

$$L = R + A - G - E.$$

Many questions can be raised about how Cogan and Taylor handle the data for these regressions. First there is the problem of the trend in the data. To address this problem, we normalized the raw data by expressing revenues and expenditures as ratios to potential GDP. To reflect state budgeting law and practice, not to mention the earmarking of ARRA grants, we separated purchases of goods and services on current account from purchases on capital account. And we excluded the imputed value of capital services in NIPA calculations of state government purchases (which reduces G and increases E).

It turns out that these modifications have a big impact on individual regression coefficients, but do not affect the qualitative results that Cogan and Taylor obtain. The key result is that the coefficients on S-ARRA grants in the two estimated equations imply a negative impact of the S-ARRA on G which is not offset by a positive impact on E; the overall impact of the S-ARRA on L is

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<sup>6</sup> See, for example, Feyrer and Sacerdote (2011), Ramey (2011), Auerbach (2012), Jonas (2012), Conley and Dupor (2013), and Leduc and Wilson (2013).



*positive*. Indeed, our own estimates suggest that one dollar of S-ARRA money leads to an increase of net saving of more than a dollar—whereas Cogan and Taylor’s estimate is that just over \$0.90 of every dollar of S-ARRA grant money is saved.

A further modification of the regression procedure has more impact. In our judgment it makes little sense to distinguish between a *state’s* purchases of goods and services on the one hand and its transfer payments on the other. Transfers payments made by states differ in important ways from transfers from the Federal government. Most direct Federal transfers to individuals come with few or no strings attached—think social security—and it is reasonable to consider such transfers simply as putting more money in the pockets of recipients. However, the bulk of transfer payments made by states and localities are not really payments to the nominal recipients except by NIPA convention. Medicaid, the largest single transfer program, appears in the national income accounts as a transfer payment to individuals, but the individual never sees any cash. The payments are actually made to vendors of medical goods and services—for visits to doctors, surgical procedures, prescription drugs—and are purchases of goods and services every bit as much as direct purchases by state governments. From what Taylor terms a Keynesian stimulus perspective, or from any other perspective, it makes little difference as to whether states purchase goods and services directly or purchase goods and services by making payments to vendors of medical services, pharmaceuticals, and medical devices.

Following this logic, the relevant dependent variable is total current outlays, the sum of current purchases of goods and services and transfer payments. With this variable normalized by expressing it as a ratio to potential GDP, the coefficient on S-ARRA grants becomes statistically indistinguishable from the coefficient on ordinary revenues, contrary to our results with un-normalized data and separate estimation of the impact of S-ARRA money on

purchases and transfers. However, regressions of (normalized) outlays on revenues and lagged outlays still suggest that the lion's share of S-ARRA grants went to shoring up state balance sheets: the impact of a dollar of S-ARRA money on total current outlays is just over \$.25 with the other \$.75 going to increase financial assets or reduce financial liabilities. The heavy lifting in all the estimating equations is done by the lagged dependent variables, a result certainly consistent with expenditure smoothing.

But, as with any econometric result, this interpretation is valid only if the relevant identifying assumptions hold. In particular, the presence of serial correlation can lead to spurious results. Serial correlation in the data can generate the observed results—that lagged outlay matters a lot and current revenue not much—even in the case where lagged outlay is actually irrelevant to current outlay. Suppose that in fact—a messenger of God told us so—it is the other variables (both ordinary and S-ARRA revenues) that are driving expenditures. Nonetheless, lagged dependent variables will still show up with high t-values and bias the estimates of the true drivers downwards, provided that in the correct specification (the one that God's messenger vouched for) the independent variables (revenue) and the error term are serially correlated (Achen 2001). In the event, revenues ( $R$ ) and the error term ( $\mu$ ) in the equation

$$O_t = a_0 + a_1 R_t + \mu_t$$

in which  $O$  represents current outlay, exhibit high serial correlation, with respective coefficients of 1.006 and 0.663 (using annual data over the period 1969 to 2008). This does not disprove Cogan and Taylor's interpretation of the data, but it does suggest that their econometric evidence ought not to be taken as support for their claims that expenditure smoothing undoes stimulus.

It is important, however, to recognize that the application of expenditure smoothing to state budgeting is not simply a logical implication of permanent-income / life-cycle hypothesis reasoning. It is, rather, an extension of this

reasoning beyond the realm of individual agents to a distinct set of agents within a particular institutional setting. As such, it would be inappropriate to build into the analysis of fiscal stimulus the assumption that state governments can and do engage in expenditure smoothing without direct investigation of the plausibility of this assumption. This is especially important in the analysis of the effectiveness of ARRA given the quite substantial portion of the stimulus channeled through the states. In the next two sections we undertake this investigation, first examining whether the states *did* engage in expenditure smoothing (on the assumption that they *could have*), and then examining the extent to which this option was actually available to them.

## **2. Cross Sectional Analysis of the S-ARRA and State Spending**

In this section we deploy cross-sectional evidence to test the hypothesis that states spent the bulk of S-ARRA monies they received against the hypothesis that these monies had little or no effect on spending, instead going to shore up their balance sheets. This exercise provisionally commits us to the stipulation that the states had considerable latitude in this regard, that they could have, if they wished, banked the money, which is to say that they could have managed their actual expenditures if no ARRA monies had been forthcoming. Our conclusion is that even if they could have continued to spend, they didn't; ARRA grants had a considerable impact on spending. But the stipulation must be understood as provisional: in the next section we argue, on the basis of the testimony of state budget officers, that most states could not have maintained their actual spending without the ARRA.

Before turning to the analysis, we need to say a few words about the data. First, in contrast with the time-series analysis of Cogan and Taylor (2012)—

which we discuss in detail in Marglin and Spiegler (2013a)—the data here are restricted to state governments, leaving out the portion of S-ARRA grants channeled directly through local governments and other agencies at one remove from state governments. There are several reasons for this. The most important is that when we performed the analysis in the summer of 2012, the Census Bureau had not yet released state-by-state data that includes local governments beyond FY2009, and the ARRA had not disbursed much money when FY2009 ended (June 30, 2009 for all but four states). By contrast, the Census Bureau had published comprehensive data on state finances through FY2010. And the data are of better quality for the states than for the consolidated accounts of state and local governments; state government data are assembled from a survey of state governments and are not subject to sampling error, whereas local government data is collected through a sampling procedure. Another reason for focusing on the states is that the bulk of the ARRA monies paid out as grants to government entities, plus contracts and loans to non-government entities, in fact went to the states. Substantial amounts were in turn transferred to localities, as well as to higher educational institutions and other non-profits, by the states, but for reasons exemplified by Medicaid grants we regard these transfers as essentially equivalent to purchases of goods and services.<sup>7</sup>

For all the information on the recovery.gov website, no breakdown of ARRA grants is provided between states, localities, universities and other non-profits, and businesses. For the portion of grants covered by the recipient reporting requirement (Section 1512 of the ARRA), we separated the state grants by using a set of keywords like “department,” “education,” “executive office,”

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<sup>7</sup> Although the supplement published by the Bureau of Economic Analysis on the impact of the ARRA only provides aggregate data for state and local governments, NIPA data breaks down Federal grants between states and localities. These data show only a very modest increase in *total* Federal grants to localities over the period of the ARRA. It follows that ARRA grants to the localities could not have been very large. This is confirmed by our analysis of the detailed ARRA data available on the recovery.gov website. Our calculation is that of the total grants, contracts, and loans reported through the end of calendar 2011 (plus Medicaid), 85 percent went to state governments, the rest going to private nonprofit entities (like universities), private businesses, as well as localities.

“human services.” For the programs not subject to Section 1512 reporting, the largest of which was Medicaid, we used the figures of the relevant Federal departments. Because the quarterly listing of recipient reports lumped together disbursements through September 30, 2009, we also relied on Federal agency reports of the Department of Education and the Department of Transportation to separate grants received by the states during FY2009 from grants received during FY2010.

The regressions summarized in Table 1 test the impact of the ARRA by cross-sectional analysis of variations in spending among the several states. The general idea of our regressions was that if the ARRA had an impact, it should show up in greater expenditures by states receiving more ARRA money. If the ARRA had an impact on state balance sheets, it should show up in larger additions to net financial assets for states receiving more ARRA money. We also test whether or not greater ARRA funding was associated with smaller changes in taxes and charges.

The general structure of the estimating equations is

$$(1) \quad \Delta O = a_0 + a_1 A + a_2 N_{-1} + a_3 \Delta N_{-1} + \varepsilon$$

$$(2) \quad \Delta N = b_0 + b_1 A + b_2 N_{-1} + b_3 \Delta N_{-1} + \mu$$

$$(3) \quad \Delta T = c_0 + c_1 A + c_2 N_{-1} + c_3 \Delta N_{-1} + \xi$$

where

$\Delta O$  = Change in expenditure per capita, FY 2010 – FY2009 (expenditure = the sum of purchases of goods and services plus transfer payments)

$A$  = ARRA grants to states per capita of state population as of April, 2010

$N_{-1}$  = Net financial assets per capita, beginning of FY2010

$\Delta N_{-1}$  = Change in net financial assets per capita during FY2009,  $N_{-1} - N_{-2}$

$\Delta N$  = Change in net financial assets per capita during FY2010,  $N - N_{-1}$

$\Delta T$  = Change in non-Federal revenues, FY2010 – FY2009 (taxes, charges, and miscellaneous revenues)

We also estimate versions of equations (1) – (3) substituting current expenditure ( $O^C$ ) for total expenditure, and short term financial assets ( $N^{ST}$ ) for total financial assets.

The null hypothesis, deriving from the work of Cogan and Taylor (2012), is

$$H_0: a_1 = 0; b_1 = 1, c_1 = 0$$

TABLE 1—REGRESSIONS OF YEAR-ON-YEAR CHANGES IN EXPENDITURES, REVENUES, AND ASSETS ON ARRA GRANTS, FY2010

	(1) Eqn 1 $\Delta O$	(2) Eqn 2 $\Delta N$	(3) Eqn 1 $\Delta O^C$	(4) Eqn 2 $\Delta N^{ST}$	(5) Eqn 3 $\Delta T$	(6) Eqn 1 $\Delta O$
Total ARRA Grants Received in FY2010	0.656 (0.071)	0.337 (0.147)			-0.124 (0.053)	0.659 (0.072)
Current ARRA Grants Expended			0.686 (0.080)	0.287 (0.167)		
Change in Non-Federal Revenues						0.094 (0.148)
Net Financial Assets, Beginning of FY2010	0.006 (0.003)	0.087 (0.007)			-0.025 (0.009)	0.007 (0.036)
Change in Net Financial Assets During FY2009	0.236 (0.044)	0.429 (0.090)			-0.074 (0.041)	0.232 (0.044)
Net Short Term Financial Assets, Beginning of FY2010				0.079 (0.006)		
Change in Net Short Term Financial Assets During FY2009			0.180 (0.033)	0.462 (0.096)		
$R^2$	0.656	0.831	0.617	0.836	0.231	0.659
Adjusted $R^2$	0.634	0.820	0.601	0.826	0.175	0.630
Coverage (States)	All	All	All	All	Non-Fossil Fuel	All
N	50	50	50	50	44	50

Notes: Figures in parentheses are Newey West standard errors.

Source: ARRA Grants: [www.recovery.gov](http://www.recovery.gov); financial asset, revenue and expenditure data: Bureau of the Census Annual Survey of State Government Finances.

The expenditure regressions, columns 1 and 3, imply that for each dollar of ARRA funding, between \$0.66 and \$0.69 was spent, depending on the inclusiveness of the concept of expenditure and the associated measure of ARRA

grants, with the estimated value of the coefficient approximately 8 standard errors away from its null hypothesis value of 0. (The lower figure in column 1 is associated with the more inclusive measures, which include spending on capital account in expenditures and include spending of ARRA monies on infrastructure in the measure of ARRA grants. In arriving at the higher figure in column 3, expenditure is limited to current account spending and ARRA grants are correspondingly limited to current expenditure.) The corresponding regressions of changes in net financial assets imply that between \$0.34 and \$0.29 of each dollar of ARRA money was added to the state's balance sheet. These estimates are approximately 4 standard errors from the null hypothesis value,  $b_1 = 1$ . (For the more inclusive definition of expenditure and ARRA funding, the measure of net financial assets is total assets less total liabilities. When expenditure and ARRA funding are limited to current account, the measure of net financial assets is limited to short-term financial assets.)<sup>8</sup>

To test the proposition that the ARRA affected taxation as well as expenditure and saving, we ran the regression reported in column 5. We limited the regression to the 44 “non-fossil-fuel” states—excluding Alaska, Louisiana, North Dakota, Texas, West Virginia, and Wyoming. It seemed to us inappropriate to include these six states, for which energy production provides a very different tax base, with the rest of the country. For instance, in FY2010 the 44 non-fossil-fuel states obtained almost 50 percent of their non-Federal revenues from a combination of sales taxes and individual and corporate income taxes, whereas the six energy states relied on this combination for only 30 percent of their non-Federal revenues. Fossil-fuel states also differ from the rest of the country with respect to their balance sheets: at the beginning of FY2010 the combined financial

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<sup>8</sup> Table 1 omits variables for which preliminary regressions, not reported here, gave insignificant results: constant terms in all regressions were omitted for this reason, as was short term financial assets in column 3. The regression-through-the-origin specification is preferred on the basis of its superior fit in terms of the standard error of the regression. See Eisenhauer (2003).

assets of the six were 266 percent of liabilities; for the other 44 states assets were only 87 percent of liabilities. (Source: Census Bureau 2010 *Annual Survey of the States*; the combined balance sheet of all the states masks the great disparities: for all 50 states together assets were 99.8 percent of liabilities.)<sup>9</sup>

For the 44 non-fossil-fuel states, the ARRA appears to have diminished the need for additional revenue from non-Federal sources. The coefficient on the ARRA is  $-0.124$ , and the standard error allows us to reject the null hypothesis of  $c_1 = 0$ . But the limitation of coverage of this regression makes it difficult to integrate the result with the estimates of  $a_1$  and  $b_1$ . Observe that the coefficients on the two control variables are also of the sign we would anticipate in a world in which state budget policy is partly driven by balance-sheet considerations.

The regression reported in column 6 is intended to test the hypothesis that non-Federal revenue influenced expenditure decisions in FY2010. The coefficient is of the right sign, but it is numerically small,  $0.09$ , as compared with coefficients on ARRA money of  $0.66$  for total grants and  $0.69$  for current grants. Moreover the coefficient on changes in non-Federal revenue differs insignificantly from  $0$ .

Finally, Table 2 reports the mixed results of including political party as a determinant of budget behavior. These regressions add a dummy variable for the states whose governors were Republicans at the beginning of FY2010.

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<sup>9</sup> Why don't we, by the same logic, limit the expenditure and saving regressions to the 44 non-fossil-fuel states? We ran regressions on the 44-state subsample (not reported here), but the results for the total expenditure regression differed very little from the same regression on the full data, while the coefficient on current ARRA funds was substantially reduced in the regression we ran on the restricted set of data. In the saving regression run with total expenditures and total ARRA grants, the coefficient on ARRA funds is no longer precise enough to shed much light on the null hypothesis,  $b_1 = 0$ . In the regression relating non-Federal revenues to ARRA grants, the sign of the coefficient on total ARRA funds has the wrong sign and is insignificant, whereas the in the current-account version of the regression (reported in column 6 of Table 11) the coefficient on the ARRA variable has the expected negative sign, and although small in magnitude is significantly different from zero. The lack of variation among the non-fossil-fuel states is presumably the source of the loss of precision.



TABLE 2—REGRESSIONS OF YEAR-ON-YEAR CHANGES IN EXPENDITURES, REVENUES, AND ASSETS ON ARRA GRANTS AND POLITICAL PARTY, FY2010

	(1) <b>Eqn 1</b> $\Delta O$	(2) <b>Eqn 2</b> $\Delta N$	(3) <b>Eqn 1</b> $\Delta O^c$	(4) <b>Eqn 2</b> $\Delta N^{ST}$
Total ARRA Grants Received in FY2010	0.765 (0.088)	0.396 (0.189)		
Current ARRA Grants Expended			0.770 (0.102)	0.379 (0.213)
Net Financial Assets, Beginning of FY2010	0.007 (0.003)	0.088 (0.007)		
Change in Net Financial Assets During FY2009	0.238 (0.042)	0.430 (0.091)		
Net Short Term Financial Assets, Beginning of FY2010				0.079 (0.006)
Change in Net Short Term Financial Assets During FY2009			0.181 (0.033)	0.472 (0.098)
Republican Governor (as of 1/1/2011)	-80.652 (38.879)	-43.544 (85.736)	-49.604 (37.455)	-55.635 (79.423)
$R^2$	0.684	0.832	0.630	0.838
Adjusted $R^2$	0.657	0.817	0.607	0.824
Coverage (States)	All	All	All	All
N	50	50	50	50

Notes: Figures in parentheses are Newey West standard errors.

Source: Grants: [www.recovery.gov](http://www.recovery.gov); financial assets, revenue and expenditure data: Bureau of the Census *Annual Survey of State Government Finances*; Governor's political party: *Statistical Abstract of the United States 2011*.

The regression reported in column 1, for total expenditures, suggests that political partisanship played a role in spending behavior, but not in how much of the ARRA monies were spent: a Republican in the governor's chair reduced total spending by \$81 per capita, while an interaction variable equal to the product of the dummy variable for a Republican governor and the ARRA grant in an unreported regression had a positive (but insignificant) coefficient. The effect on current spending, reported in column 3, was \$50 per capita, but the t-statistic was only 1.3. The regressions on the change in net assets, reported in columns 2 and 4, suggest that Republican Governors were not more likely to improve their states' balance sheets, but the standard errors are too high to put very much credence in these estimates.

There remains the possibility that the causality in the equations represented in columns 1 and 3 run from higher spending to higher grants, rather than the other way around. But in the present case reverse causality would mean simply that the states spent more in the expectation of reimbursement than they would have otherwise; indeed, this is how the Medicaid program works, Federal reimbursement depending in part on how much states commit to the program. So, even if the formalities go in the opposite direction, ARRA grants still drive state spending.

In any case, there is no argument for reverse causality in the relationship between ARRA grants and state balance sheets: states could not lay claim to ARRA funds on the basis of having added to financial assets or having reduced financial liabilities. Hence reverse causality cannot be the explanation for the statistical rejection of the second part of the null hypothesis, namely  $b_1 = 1$ .

At this point, it seems fair to conclude that the econometric evidence does not support Cogan and Taylor. When we control for differences in financial solvency of the various states, ARRA grants explain a surprising amount of the cross-state variation in the changes in spending and the variation in the amounts added to state bank accounts between FY2009 and FY2010. The  $R^2$ 's are of the order of 0.60 for the expenditure equations and 0.80 for the balance-sheet equations. The regressions suggest that some 2/3 of ARRA monies were spent by the states, the remaining 1/3 going to shore up state finances. As far as states are concerned, expenditure smoothing is either a bright idea whose time has not yet come or a misplaced faith in the rationality and liquidity of economic agents.

Moreover, the results reported in Table 1 are reassuring (or surprising, depending on one's prior beliefs) in that the coefficients on the ARRA grants in the two equations add up to \$0.97 or \$0.99 in expenditure per dollar received, depending on the specification of expenditures and financial assets. In contrast with the procedure followed by Cogan and Taylor (2012), this result is *not*

because of a constraint that forces the coefficient on the net change in financial assets to unity (see Marglin and Spiegler 2013a, Tables 6 and 7) but the outcome of independent estimation of the determinants of expenditures and changes in financial balances. Finally, the results for expenditures are remarkably close to the numbers Christina Romer and Jared Bernstein employed in their prospective evaluation of the ARRA.<sup>10</sup> However, our analysis gives little support for their idea that the ARRA would have a large effect on non-Federal revenues—that is, that the ARRA would dissuade states from raising taxes. Compared to the Romer-Bernstein assumptions, a much larger portion of the ARRA appears to have gone into increasing net financial assets *à la* Cogan and Taylor.

### 3. What State Budget Officers Say About the ARRA

There are limits to the inferences one can make from econometric results. The interpretation of regression coefficients, standard errors, and the rest requires a framing theory—including, crucially, a set of counterfactual assumptions. Cogan and Taylor interpret their results against the counterfactual assumption that states behave like the consumption-smoothing individuals theorized by Friedman and Modigliani in the permanent-income and life-cycle hypotheses. It is against this assumption that the observed lack of significant increase in aggregate state government purchases from FY2009 to FY2011 is interpreted as evidence that the ARRA was a failure.

In the absence of this assumption the econometric results could be interpreted very differently. For example, the *prima facie* equally plausible assumption that states would have sharply cut spending in response to the

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<sup>10</sup> “For transfers to the states, we assumed that 60% is used to prevent spending reductions, 30% is used to avoid tax increases, and the remainder is used to reduce the amount that states dip into rainy day funds.” (Romer and Bernstein 2009, 13)

recession, below the levels actually observed, would support precisely the opposite interpretation.

We have already seen that the time-series evidence for Cogan and Taylor's interpretation of the data is wanting in that whether or not expenditure is driven by revenues (including ARRA revenues), time-series regressions would indicate a large and significant coefficient on lagged outlays. And if indeed the correlation between lagged and current outlays is spurious, the coefficient on revenues would be biased downward. But it is one thing to argue that the time-series data do not support Cogan and Taylor and another to argue that these data reject their hypothesis. They do not. And examination of the aggregate data during previous downturns provides evidence for and against their view. For this reason, the previous section turned to cross-sectional evidence. In our judgment the cross-sectional data provide strong evidence against Cogan and Taylor. Specifically, cross-section regressions indicate that 2/3 of the ARRA money was spent by the states. But we would be the last to claim that our analysis is conclusive.

We therefore supplement these regressions with a direct examination of the plausibility of their counterfactual assumption: a set of open-ended interviews with state budget officers. We sent a questionnaire to all fifty state budget officers that gave the respondents substantial latitude in their answers. The questions were framed to provide a foundation for conversation without being so restrictive that they would prevent us from learning things about state budgeting practice that we had not anticipated.

We recognize the unorthodox aspects of this approach. Economists generally resist asking agents for information about why they do what they do or what they would have done if the circumstances had been different.<sup>11</sup> Often, there

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<sup>11</sup> There are a few notable exceptions. For example, Henderson (1938) and Meade and Andrews' (1938) use of interviews with businessmen to explore the impact of the interest rate in the determination of investment; and Blinder et al. (1998) and

is good reason for this reluctance: there are too many agents, it is hard to get a representative sample, and agents may have trouble reconstructing the circumstances of their decisions well enough to answer, especially when the questions involve a counterfactual. Fortunately, none of these reasons apply to the case at hand. There are only 50 states, and state budget officers are a highly professional group of men and women. *A priori*, then, it seemed sensible to us to ask these officers what they would have done had there been no ARRA funds to offset lost revenues and increased demands for expenditure that were the twin results of the Great Recession. From the information we gathered, we conclude that Cogan and Taylor's assumption that in the absence of ARRA states could have and would have increased net borrowing to fund spending at roughly the levels observed *with* the ARRA is highly implausible, and that it is much more plausible that the great majority of the states would have cut spending significantly without the ARRA. In the remainder of this section, we present the evidence in support of this conclusion.

#### *A. Study Design*

Our goal was to elicit responses from all fifty state budget directors to eight questions designed to allow us to assess the plausibility of Cogan and Taylor's counterfactual assumption. The questions were as follows:

1. What would have been the consequences for current and capital spending had no ARRA money come to [your state]?
2. Again, assume that no ARRA money had come to [your state]. In this case, would your capital budgeting process have required you to reduce capital expenditures in response to worsening economic conditions?

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Bewley's (1999) discussions with relevant economic actors to explore the reasons behind the stickiness of prices and wages, respectively.

3. Outside of the general fund (and stabilization funds) were there other options for funding current budget deficits that might have arisen without ARRA? (*For example: special funds from other public or quasi-public agencies not included in the general fund, but that can be drawn on by the state? Revenue anticipation notes or similar instruments?*)
4. Is it possible for [your state] to borrow to finance operating-budget deficits?
5. In your experience, did the maintenance of effort provisions (MOE) attached to ARRA funding significantly restrict [your state]'s flexibility regarding how to use the funding?
6. In [your state], is there any flexibility with regard to classifying expenses as “current” or “capital”?
7. In [your state], is any portion of the capital budget typically funded from the operating budget (i.e., using current revenues as opposed to bonds)?
8. With regard to ARRA funding for capital projects: to the extent you received such funding, did it fund new incremental capital spending, or did it just act as a replacement funding source?

As explained above, we did not intend the questions to be, in themselves, comprehensive and complete. Rather, we intended them to be a foundation for a less structured provision of information. We specifically wanted to avoid biasing the answers by rigidly steering the budget directors to respond only to those issues that *we* felt were relevant and important and thereby closing off issues that we had not anticipated. At the same time, we wanted the questions to articulate the specific issues relevant to assessing the Cogan-Taylor counterfactual. We chose the questions with the aim of balancing these two imperatives. In order to make participation as convenient as possible we offered the budget officials the option of answering the questions by e-mail or through a brief phone interview.

### *B. Composition of respondents*

Of the 50 state budget directors we contacted, we received responses or had phone interviews with 29. Obviously, our aim was to collect information from all of the states and we made efforts over a 5 month period to collect a comprehensive set of responses. Despite these efforts, however, we received no response from 21 states. Nonetheless, we feel that our group of respondents is large and comprehensive enough and similar enough to the non-response set in many important demographic aspects to give us some confidence that the responses are not tainted with selection bias.<sup>12</sup> Table 3 contains demographic and economic summary statistics of the two groups.

TABLE 3—DEMOGRAPHIC AND ECONOMIC COMPARISON OF RESPONSE AND NON-RESPONSE GROUPS

	Responding States	Non-Responding States	All States
GDP (\$bil.)	8,907	5,021	13,928
Population (July 2009, mil.)	187.1	119.3	306.4
GDP per capita	\$47,603	\$42,092	\$45,457
Total Expenditures FY 2009 (\$mil.)	1,187	644	1,832
Percentage with Republican Governor (July 2009)	41.4	47.6	44.0

*Sources:* GDP: Bureau of Economic Analysis; population and total state government expenditures: U.S. Census Bureau; Governor's political party: *Statistical Abstract of the United States 2011*.

### *C. Findings and interpretation*

The main thrust of our first question was simply to ask budget officers directly if it struck them as plausible for their state that they could have maintained expenditures in the absence of the ARRA. There was consensus among the respondents that this was *not* plausible. Since the evidence supporting the implausibility of business as usual in the face of the Great Recession is

<sup>12</sup> Of course, due to the qualitative and relatively open-ended nature of the information being gathered, we cannot formally quantify the extent of bias or confidence in our conclusion.

slightly different for operating and capital expenditures, we will discuss the two separately, beginning with operating expenditures.

Of all of the respondents, only those states with significant fossil-fuel related revenues indicated that it either might have been possible or definitely would have been possible for them to maintain operating expenditures in the absence of ARRA. Alaska and North Dakota's oil and gas revenues, respectively, shielded them more or less entirely from the budgetary woes of the recession. The response from North Dakota, where the unemployment rate never went above 4.2 percent and was under 4 percent for most of the period in question, was essentially "Recession? What recession?" West Virginia was also shielded, though not quite as thoroughly due to very high Medicaid costs that they might not have been able to cover without ARRA's enhancement of the Federal portion of Medicaid costs (that is, the Federal Medical Assistance Percentage or FMAP). Wyoming avoided the worst of the recession both through fossil-fuel related revenues and two rounds of expenditure cuts in the lead-up to the recession that remained in place throughout.

The fossil-fuel states, however, were the exceptions. All of the other respondents indicated that it would not have been possible to maintain expenditures at the observed levels in the absence of ARRA without additional revenue-raising measures (increased taxes and/or fees). Michigan's State Office of the Budget, for example, reported that "[h]ad no ARRA funding come to Michigan, general fund reductions of approximately 18% would have been required each fiscal year and would have been in addition to measures taken to close a \$1.4 billion budget gap for fiscal 2009, and \$1.8 billion in general fund reductions enacted for fiscal 2010." Moreover, many of the respondents commented that it was likely that the balance of the adjustment to lower revenues would likely have been weighted heavily toward spending cuts rather than tax or fee increases due to political considerations.



The sentiment that lower operating expenditures would have been necessary without ARRA was not sensitive to political party—it was voiced equally by those states with Democratic and those with Republican governors. There was, however, some difference along political lines with respect to the attitude toward the maintenance of spending that was enabled by ARRA. Several officials from Republican states told us that while their states would likely have enacted more spending cuts in the absence of ARRA, this would have been a positive rather than a negative for economic health.<sup>13</sup> We heard this comment both with respect to spending in general, and specifically with respect to Medicaid and education—two areas where ARRA money came with maintenance of effort (“MOE”) provisions. In general, the theme of these comments was that ARRA allowed the state government to put off dealing with budgetary problems, some of which were structural and would still have to be dealt with once the ARRA funds dried up. Many of the budget officials commented that they were wary of creating a “fiscal cliff” by using ARRA money to continue to fund programs at levels that would likely be unsustainable post-ARRA.

The responses of the budget officials regarding operating expenditures takes into account the possible impact of budget stabilization funds (BSFs)—i.e. that even with the aid of internal reserves it would not have been possible to maintain expenditures in the absence of ARRA. Again, with the exception of the fossil-fuel states, all of the respondents commented that their BSFs would not have been sufficient to have undertaken spending at the observed levels.<sup>14</sup> During fiscal 2009, for example, Minnesota drew its budget reserves down to zero and was projecting revenue shortfalls several years in to the future. Similarly, Arkansas, which did not create a budget stabilization fund until 2010, faced

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<sup>13</sup> This sentiment was expressed to us by state budget officers whose current administration is Republican—in particular, those from Ohio, Wyoming and Kansas—or whose state was under a Republican administration during the years in question—in particular, Minnesota.

<sup>14</sup> The one exception to this was Oklahoma, which has significant fossil-fuel related revenues, but indicated that they drew their reserve funds down steadily to zero over the course of fiscal 2010 and 2011.

significant revenue shortfalls in FY 2010 and would not have been able to support the operating budget actually executed in 2010 without ARRA. In this connection, it is important to note that the vast majority of the respondents—including those from the fossil fuel states—indicated that they would have made significant efforts to avoid drawing their BSFs down to zero. This was important to them for two reasons. First, these funds are an important bulwark against all kinds of fiscal emergencies, and not just recessions. Iowa, for example, faced significant unexpected expenses from a major flooding episode in the summer of 2008. Second, as a former Massachusetts state budget officer indicated, the level of these reserve funds affects a state’s credit rating. This adds an additional potential cost to drawing them down too far.

While drawdowns from BSFs are the most obvious form of covering revenue shortfalls, there are other possibilities as well, at least in principle. For example, states may have special funds (for example, from lottery revenues or transportation-related fees) that are a part neither of the general fund nor the BSF that could in principle be tapped to fill general revenue shortfalls. These would simply be another form of “reserve drawdown” and would therefore qualify as additional net borrowing by Cogan and Taylor’s definition. The responses to Question 3 provided direct evidence about this possibility. Nineteen of the respondents indicated either that no such funds would have been available to cover general revenue shortfalls or that such funds could have been tapped but the amounts would have been insignificant. Five states indicated that such funds exist, are substantial and can be tapped, but that even with these contributions the revenue shortfall would have been too great to meet without additional measures (absent ARRA). A Maryland official commented that although “reprioritizing special funds is a significant tool in budget balancing...it would not have been sufficient to prevent significant reductions to key state services in the absence of ARRA funds.” Three additional states indicated that such funds were already

exhausted or being used to the greatest extent possible during the period of ARRA funding. A Connecticut official commented that during the recession, special funds “were significant in offsetting the State’s large shortfalls”, but that “by 2011 all fund sweeps had been exhausted. [And] ARRA filled part of the gap.” And two states, both fossil fuel states, indicated that the point was moot because they would not have needed to explore such possibilities.

Another possibility open to states, in principle, to maintain operating costs through increased net borrowing would be to shift operating costs onto the capital budget. Question 6 asked the budget officials if such a strategy was open to them. With the exception of three states—Hawaii, Kansas and Utah—all respondents indicated that there is very little such flexibility and, that to the limited extent such shifts could be made, their impact would not be significant.<sup>15</sup> In the case of the three exceptions, officials from Kansas and Hawaii indicated that there was some flexibility to make such reclassifications during the recessionary period but did not indicate how significant the flexibility was or the extent to which it was used. And the Utah respondent indicated that some building projects that had been on the operating budget were shifted to the capital budget.

These responses, then, directly undermine the plausibility of the Cogan-Taylor counterfactual with respect to operating expenditures. The responses show that the possible borrowing sources—internal reserve funds and capital market borrowing—either would not have been sufficient, ex-ARRA, to fund operating expenditures at the observed level or were not available for that purpose.

For purposes of assessing the plausibility of the Cogan-Taylor counterfactual, the effect of ARRA on capital expenditures is more complicated than its effect on operating expenditures. The primary reason for this is that direct capital grants constituted a relatively small portion of total ARRA outlays to the

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<sup>15</sup> Many respondents indicated that some operating expenses directly related to capital projects (for example, the salaries of personnel dedicated to the project in question) are routinely included in the cost of the capital project.

states, and that the other portion of the funds—that is, the vast majority of ARRA outlays to states—affected capital spending in less direct and more complex ways. It may be useful to begin by working out what would count as evidence for and against the Cogan-Taylor counterfactual before turning to the responses.

In order for their counterfactual to be plausible with respect to capital spending, it must be the case that the states would have had the wherewithal to fund capital expenditures at the observed levels in the absence of ARRA. For the vast majority of the states capital expenditure is funded with debt, mostly in the form of bonds (for example, general obligation bonds and revenue bonds). For those that fund capital expenditures largely or entirely from the operating budget, our assessment of the effect of ARRA on operating expenditures carries over to capital expenditures: capital expenditures funded out of the operating budget are equivalent to operating expenditures for our purposes.<sup>16</sup> In light of this, our assessment of the plausibility of the Cogan-Taylor counterfactual hinges on whether or not the debt-financing states could have borrowed enough, absent the ARRA, to support the capital expenditure actually observed.

To determine whether or not this is the case, we need to consider all of the various paths through which the ARRA might have affected both borrowing capacity and observed capital expenditure. In all—again, for those states that fund their capital expenditures with debt—there are three: (a) ARRA funds designated specifically as capital grants could have been used directly (i.e. without borrowing) to fund capital projects that otherwise would not have been funded; (b) states could have taken advantage of the ARRA’s “Build America Bonds” program—which provided a partial subsidy for states’ interest payments on newly

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<sup>16</sup> These states are Alaska, North Dakota, West Virginia, Wyoming and Arkansas. The first four are fossil fuel states for whom it is plausible that they would have been able to undertake the observed level of capital expenditure absent ARRA (for the same reason this was deemed plausible for operating expenses). The Arkansas respondent, however, indicated that ARRA funding allowed them to undertake certain critical infrastructure projects that would not have been possible otherwise. In addition, several other states—Delaware, Florida, Iowa and Rhode Island—allocate some amount of surplus general revenue funds, when available, to capital projects funds various kinds. During the recession, these funds were largely exhausted.

issued taxable bonds eligible for the program—to access the capital markets to a greater extent than otherwise might have been possible; and (c) the increased revenue from the ARRA could have increased the state’s borrowing capacity above what it would have been without those revenues.

With respect to direct capital grants, virtually all of our respondents indicated that the ARRA had allowed them to undertake incremental capital spending—either in the form of new projects or the acceleration of existing planned projects. In Florida, for example, ARRA capital grants were used by the Florida Department of Transportation to fund “more complex projects which would result in higher job creation.” They further indicated that this funding “resulted in projects in addition to state funded projects planned for expenditure during this period.” Hawaii and Ohio were partial exceptions, with the respondents of these states indicating that ARRA funds were at least in part used as replacement funding, though the precise extent of replacement was unclear.

With respect to the impact of the Build America Bonds (BAB) program, the responses did not provide enough information to form a clear conclusion. Only four of the states explicitly mentioned the BAB program as having had a significant impact. Of these, two—California and Colorado—indicated that the program had allowed them to undertake more capital spending than would have been possible in the absence of the program. BAB was especially important to California, which was having difficulty accessing the credit markets through standard channels. And an official from the Colorado budget office indicated that, due to the BAB program, the Colorado Bridge Enterprise “actually issued \$300 million in bonds that it otherwise may not have issued.” With respect to the other two states, Rhode Island indicated that Build America Bonds were used for refinancing purposes, and Ohio indicated that the BAB program was probably not used for any capital spending that would not have been undertaken in any event.

With respect to the effect of the ARRA on states' borrowing capacity, we found that the ARRA generally did not affect borrowing capacity. For most of the states, annual capital borrowing is capped by statute, with the cap generally being related in some way to projected revenues.<sup>17</sup> In all of our responding states where this is the case, ARRA revenues were not included in "projected revenues" for the purposes of the debt limit calculation. So the amount these states were willing and able to borrow each year would not have been affected by ARRA grants.

Putting all of these pieces of the story together, it is reasonable to conclude that—at least for the responding states—those states that fund capital borrowing with debt could not have undertaken capital spending at the observed levels in the absence of the ARRA. Since the ARRA led to incremental capital spending in almost all of these states, actual capital expenditures were greater than what they would have been in the absence of the ARRA. Since states' ability to increase capital borrowing was limited by statute—with no responding states indicating that they were borrowing at a rate below that limit—it would not have been possible for them simply to fund such incremental expenditure with additional capital market borrowing.

The evidence gleaned from our questionnaire to state budget officials, then, supports the conclusion that, contrary to the claims of Cogan and Taylor, it is *not* plausible to claim that in the absence of ARRA states would have undertaken expenditures at the level actually observed and would have funded this with additional net borrowing. The evidence indicates that states not only *would not* have done so, but that in almost all cases they *could not* have done so. The borrowing sources available to states are the (financial) capital markets and their internal reserves, which include budget stabilization funds (including rainy

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<sup>17</sup> In Massachusetts, for example, the debt cap was set by statute in 1990 to be \$6.8 billion, and to grow by 5% each year. The same legislation limited total annual debt service (interest and principal) on state general obligation debt to no more than 10% of budgeted appropriations (Massachusetts General Law Part I, Title III, Chapter 29, Sections 60A, B). See Commonwealth of Massachusetts (2008, Appendix A) for an analysis for FY 2009-2013 that utilizes these guidelines.

day funds) and any other special funds that are available for filling shortfalls. The responses to our questionnaire indicate that, aside from the fossil fuel states, internal funds would have been insufficient to support operating expenditure at the level actually observed and that maintaining that expenditure by shifting it to the capital budget was not an option available to a large enough extent to have made a difference. On the capital expenditure side, the responses indicated that (in the absence of changes to statutes) states are very limited in their ability to increase capital borrowing during recessions as their capital borrowing limit is tied to projected revenues.<sup>18</sup> In light of this, it is not plausible to interpret the observed data on expenditures and net borrowing as evidence that ARRA grants to states failed to stimulate additional spending relative to the state of the world without those grants.

### **3. Conclusions**

Did the stimulus work? Our short answer is yes. But to answer the question of whether or not the ARRA stimulus—or any fiscal stimulus for that matter—“worked” one needs to be clear about the assumptions that provide the framework within which the results are interpreted. Based on the analysis of this paper and Marglin and Spiegler (2013a), the proper counterfactual assumption about state government expenditure during the recession is that in the absence of ARRA states would have been unable to maintain expenditures at (or close to) pre-recession levels. The proper measure of success, therefore, is not an observed rise in the expenditure trend, but instead evidence that ARRA funds were used for incremental spending relative to that counterfactual. On that basis, our analyses support the conclusion that the stimulus worked, and that the “rational”

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<sup>18</sup> Again, the fossil fuel states are an exception to this, as they generally fund their capital expenditures from the operating budget. The question of whether they can engage in counter-cyclical capital borrowing, then, is not pertinent.

expenditure smoothing arguments to the contrary are invalid. The evidence suggests that state government expenditure was significantly increased by ARRA relative to what would have been possible without it. For the economy more broadly, the evidence suggests over the period from mid-2009 to mid-2011 it added some 2 percent to GDP. If the Obama Administration can be faulted, it is for failing to appreciate the gravity of the situation it inherited in January 2009, for lacking the courage or foresight to ask for more stimulus over a longer time period, for failing to argue forcefully enough that more of the stimulus should be directed to lower income beneficiaries who would have been more likely to spend than to save, or for all three of these reasons.

Detailed examination of the evidence with respect to grants to the states reinforces our more casual evaluation of the stimulus as a whole: contrary to the conclusions of Taylor (2011a) and Cogan and Taylor (2012), both econometric analysis of cross-sectional state data and interviews with state budget officers suggest that the ARRA allowed the states to maintain spending programs that would have been drastically cut if the stimulus had not been enacted. A portion of the ARRA monies did go to shore up state balance sheets—as indeed was the intention of the ARRA legislation—but far less than Cogan and Taylor contend. Our estimate, based on cross-sectional data, is that during FY2010 approximately 1/3 of grants to states made under the ARRA were added to their balance sheets, whereas 2/3 were spent.

Beyond the stimulus, an important lesson of this paper is the need for methodological pluralism. Aggregate time series is one source of evidence, but as we have seen, it is unnecessarily limiting to focus exclusively on this particular evidence. Bringing cross-sectional and interview evidence to bear adds considerably to our understanding of the impact of the ARRA on state finances.

But the chief methodological lesson is the absolute necessity of grounding empirical assessments firmly in empirical reality, subjecting one's assumptions to



rigorous scrutiny *by whatever investigative means are required*. This is necessary to avoid misinterpreting econometric results—even those arrived at through impeccable econometric analysis. Cogan and Taylor hypothesize that state governments and individual agents engage in expenditure smoothing. This is a reasonable hypothesis for many purposes, one with at least two Nobel Prizes on its side. But the *scope* of its legitimacy is circumscribed by the assumptions on which the underlying theory is based. Before one can legitimately deploy it as a counterfactual assumption, one must know enough about the empirical reality of the target population to ensure that it is really plausible.

The importance of this methodological caveat is especially clear in the case at hand, where the discourse over the success or failure of ARRA has been rife with self-fulfilling analyses on both sides of the debate. Taylor rightly argues that most of the post-hoc vindication of the ARRA could have been—and in fact was—written before one dime of ARRA monies had been spent. But this a case of the pot calling the kettle black: whatever the truth of the Cogan-Taylor hypothesis, their methodology guaranteed that the data on state and local governments would “confirm” expenditure smoothing because of a high correlation between current and lagged expenditure.

Although all empirical analysis presupposes a theoretical framework, not all frameworks are created equal with respect to how much room there is for the empirical results to contradict a preferred hypothesis. Our analyses of the state government channel of the ARRA were designed to minimize the extent to which the assumptions drove the results. In contrast with the Cogan-Taylor assumptions that guaranteed the appearance of expenditure smoothing, our cross-sectional regressions left the answer open: the regressions might have turned out very differently. Certainly there was no *a priori* guarantee of favorable results—the high R-Squares and associated t-values of the coefficients in Table 1, and more particularly that the independently estimated coefficients on outlays and asset

accumulation sum to unity. Of course, as with all econometric analyses, our results must also be interpreted in light of our assumptions. Absent an interpretive framework, it is a cliché that correlation does not imply causality.

Given these limits to the efficacy of regression analysis, we sought to shed additional light on the question of causality by eliciting information directly from SBOs—the very agents who would have been the vehicle of cause and effect. And here too we made every attempt to frame our questions in a way that would have permitted answers on both sides. The open-ended questions we posed allowed SBOs to range freely in their answers. There was a uniformity of responses—but *not* unanimity—with respect to how the ARRA actually affected expenditures, even when the respondents obviously differed in their evaluation of the ARRA as a policy. So, while it is true that the framework of analysis affects the results, it is not the case that all frameworks are created equal. We would claim that our own framework is less restrictive, more open to alternative outcomes, and more sensitive to empirical reality than the framework invoked by the leading critics of the stimulus.

The final lesson is skepticism about the conventional distinction between positive and normative economics. Taylor is very much in the mainstream in believing that description can be separated from values, the first representing science the second ideology. But just as there are no facts without theory, there is no separate realm for description that does not embody values. Ideology ought not to be, as it is glossed in the *Cambridge Dictionary of Philosophy* (Audi 1999, 406) “a disparaging term used to describe someone else’s political views which one regards as unsound.” Acting on ideology is not a failing or disease of the Other against which Taylor (or we for that matter) can claim immunity. Ideology is not the coin of the realm of true and false. We all operate on the basis of assumptions that cannot be proved or disproved, and ideology is the coin of the vast realm of what is beyond our powers to confirm or deny. This does not mean

there is nothing to discuss, nothing to learn. To the contrary. We may seek to transcend ideology, but we will never do so until we admit that it is the necessary starting point of any serious discussion about policy.

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