

An Empirical Analysis of the Revival of Fiscal Activism in the 2000s

JOHN B. TAYLOR*

An empirical review of the three fiscal stimulus packages of the 2000s shows that they had little if any direct impact on consumption or government purchases. Households largely saved the transfers and tax rebates. The federal government only increased purchases by a small amount. State and local governments saved their stimulus grants and shifted spending away from purchases to transfers. Counterfactual simulations show that the stimulus-induced decrease in state and local government purchases was larger than the increase in federal purchases. Simulations also show that a larger stimulus package with the same design as the 2009 stimulus would not have increased government purchases or consumption by a larger amount. These results raise doubts about the efficacy of such packages adding weight to similar assessments reached more than thirty years ago. (JEL E21, E23, E32, E62, H50)

1. Introduction

Economists agree about the goals of price stability, low unemployment and stable economic growth, but they disagree about the policies to achieve these goals. The disagreement is particularly heated over discretionary countercyclical Keynesian fiscal policy. After the poor macroeconomic performance of the 1970s and critical policy evaluations of the Keynesian approach—ranging from Robert E. Lucas and Thomas J. Sargent's (1978) "After Keynesian Macroeconomics" to Edward M. Gramlich's

(1978, 1979) empirical assessments—discretionary countercyclical policy fell out of favor for more than two decades, only to return again in the past decade.¹ Regardless of one's views about the rationale for the recent revival,² it provides another opportunity to assess discretionary countercyclical fiscal policy. The purpose of this paper is to contribute to such an assessment by reviewing the impact of the stimulus packages enacted in the past decade.

¹ The recent revival includes the tax rebate portion of the Economic Growth and Tax Relief Reconciliation Act of 2001, the Economic Stimulus Act of 2008, and the American Recovery and Reinvestment Act of 2009, as well as many other smaller stimulus programs in 2009 such as cash for clunkers and first-time home buyer credits. They were all temporary and explicitly enacted to counter the 2001 or the 2007–09 recession.

² In John B. Taylor (2009), I argued that there was a lack of a rationale for the revival.

* Stanford University. I am grateful to John Cogan for comments on this paper, which is largely based on our joint research. I also thank Lewis Alexander, Cynthia Liu, Ricardo Reis, and participants at seminars at the Federal Reserve Bank of New York and the International Monetary Fund.

2. Methodological Issues in Policy Evaluation

First consider the basic idea behind Keynesian countercyclical fiscal policy as presented—along with alternative views—in college textbooks. A decline in aggregate demand, caused, say, by a decline in investment (I), can be offset by increasing government purchases (G) or temporarily increasing transfer payments or tax refunds. In terms of the Keynesian cross diagram, a shift down in the aggregate expenditures line due to the fall in investment can be countered by increasing government purchases, which shifts the line back up. Government purchases—augmented by possible multiplier effects—thus fill the “gap” left by the decline in investment. Countercyclical changes in income tax payments and transfers work the same way except that consumption (C) fills the gap.

Estimated macro models used for policy evaluation—whether Keynesian or new Keynesian—have this basic mechanism built into them. However, they differ greatly in their predictions of the policy impact because of different assumptions about expectations, the marginal propensity to consume, the degree of consumption smoothing, the speed of price adjustment, and crowding out of other spending as G is raised. For example, Romer and Bernstein (2009) used Keynesian models without forward looking expectations to predict the effect of the stimulus package of 2009—the American Recovery and Reinvestment Act (ARRA)—before it was implemented. They predicted large effects of the package with multipliers around 1.5. In contrast, John F. Cogan et al. (2010) used a new Keynesian model to predict the effects of ARRA before it was implemented. They predicted a much smaller effect, with multipliers averaging 0.5.

The problem with using these existing macro models for the *evaluation* of actual

packages is that they will simply repeat the same *prediction* story over again. You learn virtually nothing about the efficacy of a stimulus package if you use the same models to evaluate its impact *ex post* that you used to predict its impact *ex ante*. Indeed, this is one reason for the disagreement about the impact of the recent stimulus packages. The same models are frequently being used in policy evaluation studies, which are then referred to in many of the debates about policy.³

To be concrete, consider two models relating the size of the stimulus package S to output Y . Model A is $Y = \alpha S + Z$ and model B is $Y = Z$, where Z is an unobservable shock and α is a coefficient that I set to 1.5. Now, suppose that a stimulus is enacted with $S = 2$, but Y decreases by -1 . Then the shock implied by model A is $Z = -4$ while the shock implied by model B is $Z = -1$.

Now consider policy evaluation of the stimulus based on a counterfactual where there is no stimulus so $S = 0$. Economists using model A would say: “Just as we predicted, the stimulus package worked. Without it, Y would have fallen to -4 rather than -1 . The decline in output would have been four times as deep, a Great Depression 2.0.” Economists using model B would simply say “Just as we predicted the stimulus package did not work.”

One way to tackle this problem is to look at the direct effect of the stimulus packages within the context of the Keynesian paradigm, but without imposing a rigid

³ For example, the quarterly impact reports by the Congressional Budget Office (2011) focus on existing models while alternatives models are discussed in the testimony by Taylor (2010). An example of how these simulation studies are referred to in the media is the news article by Jackie Calmes and Michael Cooper (2009) who wrote “The accumulation of hard data and real-life experience has allowed more dispassionate analysts to reach a consensus that the stimulus package, messy as it is, is working,” offering as evidence simulations from the same models which had predicted large impacts of the stimulus package in advance.

parametric model structure. This approach has been taken, for example, by economists using micro data to evaluate the impact of transfers and tax rebates on consumption expenditures in the 2001 and 2008 stimulus packages. See Matthew D. Shapiro and Joel Slemrod (2003, 2009), David S. Johnson, Jonathan A. Parker, and Nicholas S. Souleles (2006), and Parker et al. (2009).

In this paper, I take this more direct approach, but rather than using micro data I use some informative aggregate data series extracted from the stimulus packages by the Bureau of Economic Analysis (BEA). In addition to looking at the effect of the temporary transfers and tax rebates in the 2001, 2008, and 2009 stimulus packages on consumption, I consider the impact on government purchases which received considerable attention when the 2009 stimulus was passed. The approach differs from, and complements, the general approach reviewed by Valerie A. Ramey (2011), which looks at time series data on output and government purchases over longer periods of time. The approach used here focuses on stimulus-specific timing and compositional effects that help in the policy evaluation.⁴ I use simple graphs and regression techniques to identify and estimate the impacts.

I first consider the tax and transfer components of the 2001, 2008, and 2009 stimulus packages and then the government purchases components of the 2009 package.

⁴ A number of cross section studies, such as Timothy Conley and Bill Dupor (2011), have examined the impact of ARRA by looking at the “reduced form” effect on employment rather than looking at the direct effects on consumption and government purchases as implied by the Keynesian model. Other studies have used cross section data to examine the impact of some of the smaller-scale interventions, such as the cash for clunkers, which used incentive effects to shift spending forward in time.

3. *Temporary Changes in Taxes and Transfer Payments*

The Keynesian argument for temporary tax rebates or transfer payments is that they will increase disposable personal income and thereby stimulate consumption, which will in turn increase GDP and thereby either prevent a recession or accelerate the recovery from a recession already underway. Counterarguments arise from doubts about the reliability and stability of the connection between income and consumption, especially when the increase in income due to the stimulus is temporary.

Figure 1 shows the impact on quarterly disposable personal income of the temporary changes in taxes and transfers due to the three stimulus packages of the 2000s. The impacts of the packages on income were calculated by BEA. For the 2001 and 2008 packages, the data were collected from various monthly BEA press releases of “Personal Income and Output” as described in Taylor (2009). For the 2009 package, the data were collected in a satellite quarterly account on ARRA prepared by BEA, “Effect of the ARRA on Selected Federal Government Sector Transactions” under the categories “personal current taxes” or “current transfer payments to persons.” These changes include one-time \$250 payments, refundable credits, and a “making work pay” tax credit.⁵ Not shown in the figure is the impact of yet another temporary stimulus package passed in December 2010 in which payroll taxes were temporarily reduced for the year 2011. While this change was not part of ARRA, its impacts will undoubtedly be the subject of future research.

⁵ The extension of the Alternative Minimum Tax (AMT) is also included in BEA’s ARRA table because ARRA was used as the legislative vehicle for the annual AMT extension in 2009. However, since that extension occurs regularly every year, it is not considered as part of the stimulus package in this study.

Billions of dollars

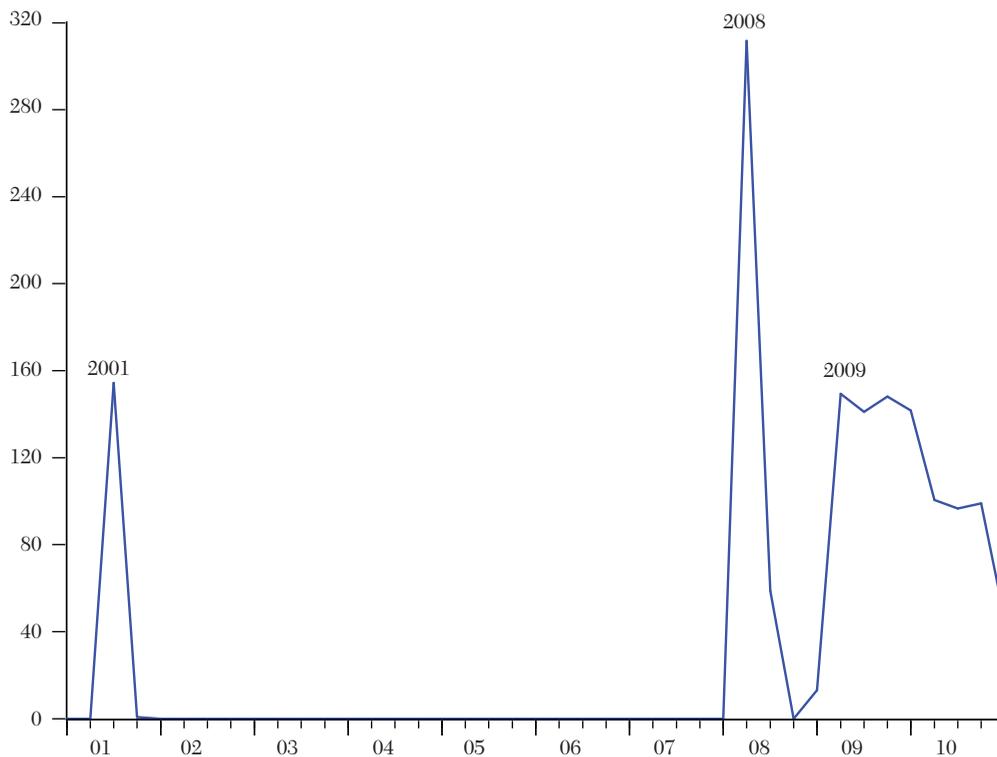


Figure 1. Effects of Three Stimulus Packages on Disposable Personal Income

Though these packages differed in size, duration, and the mechanism for distribution of the stimulus payments,⁶ they were quite similar from the point of view of macroeconomics because they were all widely viewed as temporary and were justified on the grounds of stimulating or jump-starting consumption.⁷ In fact, a major principle underlying the 2008

⁶ The 2001 tax rebates could be viewed as an advanced installment on the more permanent tax cut passed that year; the 2009 stimulus had more refundable credits and was implemented in part by a change in withholding.

⁷ Other rationales are sometimes given for stimulus packages, including that the payments or government purchases are appropriate in their own right. This paper

and 2009 stimulus packages was that they should be temporary, as well as targeted and timely. This temporary feature distinguishes these actions from more permanent changes such as the personal income tax rate cuts in the 1960s and 1980s.

Now consider the direct impact which these temporary changes in disposable personal income may have had on consumption.⁸ It would be too narrow an

focuses on assessing the Keynesian macroeconomic stabilization rationale for these packages.

⁸ In Taylor (2009), I looked at the 2001 and 2008 programs using monthly data from BEA and found that they

Billions of dollars

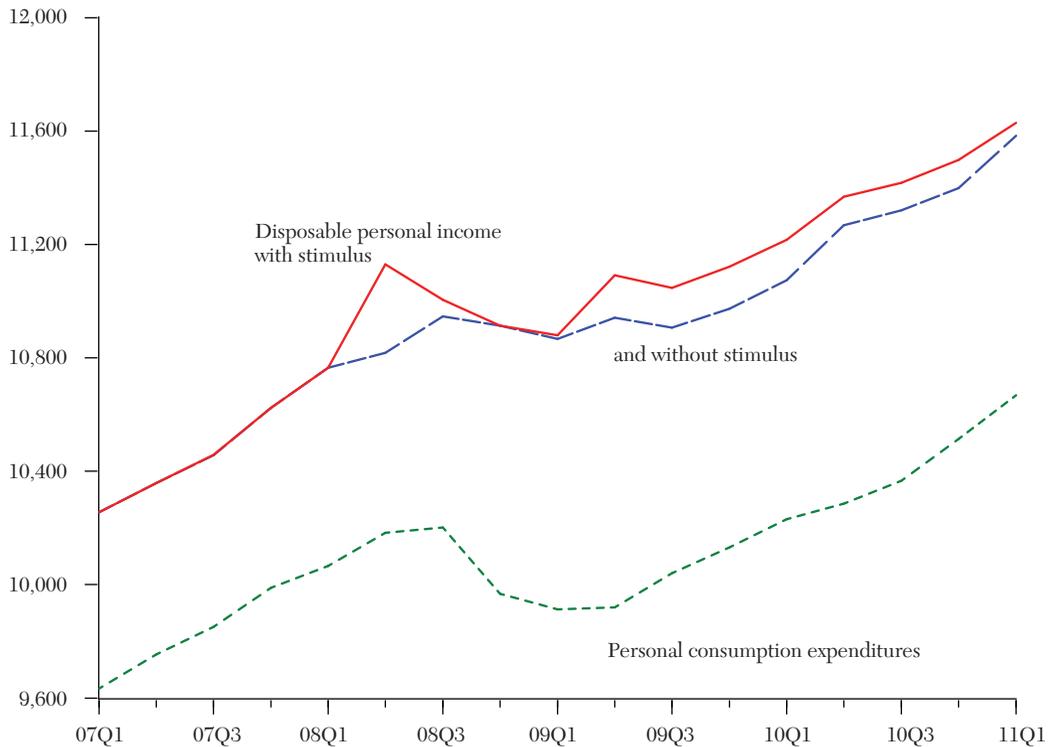


Figure 2. Quarterly Disposable Personal Income, with and without Stimulus, and Personal Consumption

interpretation of the Keynesian consumption model to say that consumption would adjust in synch with the ups and downs in income due to the payments, but if the stimulus payments worked to stimulate the economy as envisioned in this model, one would have to see some associated movements in consumption. To look for such direct effects, I subtracted the payment amounts in figure 1 from actual disposable personal

income to get an adjusted income series as shown in figure 2 for the 2008 and 2009 stimulus packages. The actual and adjusted series can then be compared with personal consumption expenditures also shown in figure 2.

Figure 2 does not reveal any noticeable effects of the temporary payments on consumption. The sharp increases in personal disposable income in the second quarter of 2008 and the second quarter of 2009 do not show up in corresponding movements in consumption. The lack of a relationship is even more striking in 2008 with monthly data as shown in Taylor (2009).

had little effect on personal consumption expenditures. Soon after ARRA was enacted, BEA stopped reporting monthly data on its impact, so in extending the evaluation to include the 2009 package I use quarterly data.

TABLE 1
QUARTERLY PCE REGRESSIONS WITH AND WITHOUT STIMULUS PAYMENTS

	(1)	(2)	(3)
Constant	104.5 (0.78)	-47.1 (-0.48)	-27.1 (-0.25)
Disposable personal income	0.817 (40.9)	—	—
Disposable personal income—without stimulus	—	0.857 (73.0)	0.851 (60.4)
Stimulus payments	—	—	0.127 (0.81)
Oil price (\$/bbl lagged 2 quarters)	-2.41 (-4.71)	-2.55 (-4.14)	-2.55 (-4.61)
Net worth (lagged 2 quarters)	0.021 (8.53)	0.017 (7.32)	0.018 (7.97)
Standard error of regression	76.9	65.9	66.3

Notes: The dependent variable is personal consumption expenditures. The *t*-statistics in parentheses are based on Newey–West standard errors. The consumption and income variables are from the Bureau of Economic Analysis with the stimulus payments calculated as in the text. The oil price variable is West Texas Intermediate from the U.S. Energy Information Association and net worth variable is Household Net Worth from the Flow of Funds, table B-100, line 42. Sample period is 2000Q1–2011Q1.

More precise information about the direct impact of the stimulus payments on consumption can be obtained from regression estimates. Table 1 reports the results from three regressions in which personal consumption expenditures is the left-hand side variable. In regression equation 1, displayed in column 1, the income variable is simply disposable personal income (which includes the stimulus). In the second regression equation, the income variable is disposable

personal income without the stimulus. In the third equation, the stimulus payments for 2001, 2008, and 2009 are added as a separate variable. In all three regressions, oil prices and household net worth are included as control variables. Higher oil prices would be expected to depress consumption while higher net worth should have a positive effect, both with some lag, and this is what the regressions show with the lag equal to two quarters.

By choosing to put consumption on the left-hand side, we are looking for effects of the stimulus payments on consumption, which is where the Keynesian model says we should find them. By splitting disposable personal income into two parts—a temporary part due to the stimulus and the remaining more permanent part—we are allowing for a distinction predicted by the permanent income theory, though we are not prejudging the size of the temporary versus permanent effect. The regressions are estimated over the sample period 2000Q1–2011Q1, which includes the effects of all three stimulus packages.

First note that the standard error in regression equation 2 is less than the standard error in regression equation 1. In other words, including the stimulus payments in disposable personal income worsens the fit of the equation, suggesting that the impact of the temporary changes on consumption is less than the more permanent changes. This idea is borne out by comparing equation 2 with equation 3, where the stimulus payment is separated from other sources of income. Regression equation 3 indicates that the temporary stimulus payments had a very small effect on consumption and that this effect is not statistically significantly different from zero. In contrast, the adjusted disposable personal income variable—a more permanent measure of income—has a much larger and statistically significant effect in regression equation 3.

This is the kind of regression result that one would expect from the Friedman permanent income hypothesis, the Modigliani life cycle hypothesis, or from consumption smoothing in an intertemporal utility maximization model. Experimenting with different regression specifications gives similar results, so effectively the data are speaking for themselves without the constraint of particular parameter values or functional form. The results imply that the Keynesian

multiplier for transfer payments or temporary tax rebates was not significantly different from zero for the kind of stimulus programs enacted in the 2000s.

4. *Government Purchases and the 2009 American Recovery and Reinvestment Act*

Now consider the impact of changes in government purchases. The 2001 and 2008 stimulus programs did not have a government purchases component, so I focus on the 2009 ARRA package.

Figure 3 summarizes the impact of ARRA on federal government sector transactions from 2009.1 to 2011.1.⁹ Three components of ARRA are shown: (1) the temporary transfers and tax rebates or credits which increase the disposable personal income of individuals and families; (2) federal government purchases of goods and services (government consumption and government investment); and (3) federal grants to states and local governments

The first category, payments to persons, has already been considered. The second category, federal government purchases of goods and services, is part of GDP and thereby contributes directly to changes in GDP. The amount by which an increase in government purchases in a stimulus package raises GDP is of course the government purchases *multiplier*, which has been a subject of much disagreement among economists. From a Keynesian stimulus perspective, the purpose of the third category—sending grants to state and local governments—is to get these governments to increase purchases.

⁹ The data are from the BEA table “The Effect of the ARRA on Selected Federal Government Sector Transactions.” A small part of ARRA—not shown in the bar chart—was classified as going to the business sector in the form of subsidies and tax benefits, for example for renewable energy or first time home buyers credits, which I do not explicitly consider in this paper. Also, as stated in footnote 5, the extension of the AMT is not included.

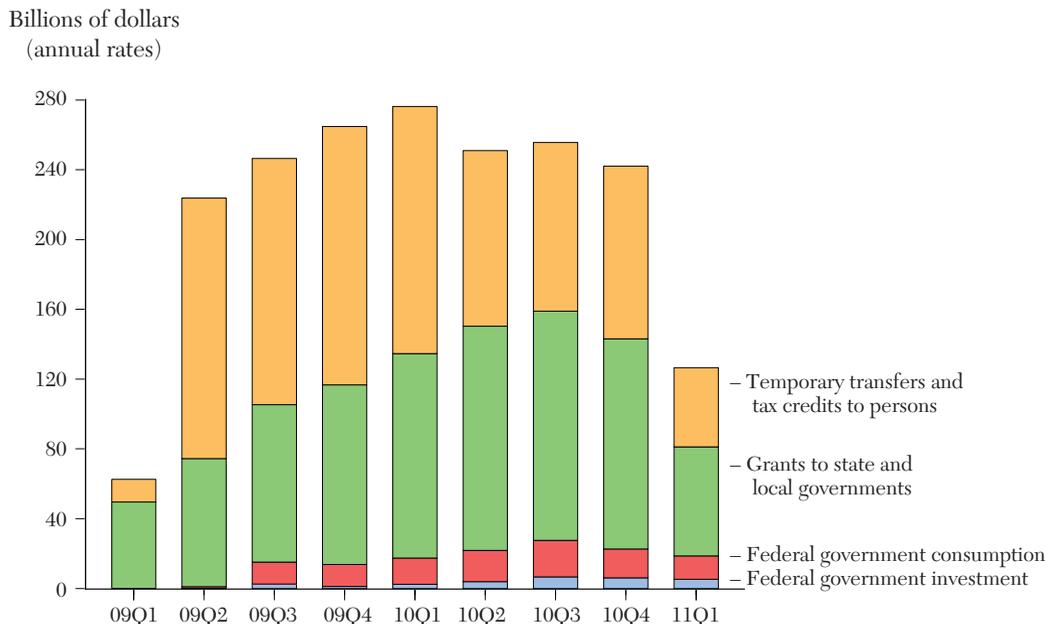


Figure 3. Major Federal Budget Categories of ARRA

4.1 Federal Government Purchases

The most striking finding in figure 3 is that only a small part of ARRA went to purchases of goods and services by the federal government. Measured as a percentage of GDP the amounts were immaterial: At the maximum effect, which occurred in the third quarter of 2010, federal government purchases due to ARRA reached only 0.21 percent of GDP and federal infrastructure only 0.05 percent of GDP.

These amounts are too small for the stimulus package to have had a significant effect on the overall economy. In this case, the debate over the size of the government purchases multiplier is largely moot because the government purchases multiplier had virtually nothing to multiply at the federal level.

4.2 State and Local Government Purchases

State and local governments received substantial grants under ARRA as shown in the bar chart. The purpose of sending these grants to the states was to encourage them to start infrastructure projects and purchase other goods and services. But this is not what happened.

Consider figure 4, which shows the ARRA grants along with the change in state and local government purchases, borrowing, and expenditures other than government purchases relative to the fourth quarter of 2008 as published in the BEA's National Income and Product Accounts. ARRA grants increased steadily from the first quarter of 2009 through the third quarter of 2010 before tapering off. But state and local government purchases hardly changed at all during this period. The

biggest change during the period of the ARRA grants was a large decrease in state and local government net borrowing, or, equivalently, an increase in net lending. Expenditures other than the purchases of goods and services rose by a smaller amount than net lending. Net borrowing by the state and local government sector is defined as the difference between the net increase in financial liabilities and the net acquisition of financial assets or equivalently by total expenditures less total revenues.¹⁰

4.3 Regression Estimates of the Impact of ARRA on State and Local Government Purchases

To get a better estimate of the direct impact of the ARRA grants on government purchases, Cogan and Taylor (2010) used regression methods to control for other state and local government revenues (excluding ARRA grants) and also take account of the state and local government budget constraint. By imposing the budget constraint on the regression coefficients we can let the data determine what component of the budget the ARRA grants affected and by how much.

Consider the following three equation system that was estimated over the period from 1969Q1 through 2011Q1 using National Income and Product Account data:

$$(1) \quad G_t = 3.70 + 0.864G_{t-1} + 0.123R_t - 0.115A_t,$$

$$(2) \quad E_t = -4.24 + 0.809E_{t-1} + 0.0418R_t + 0.115A_t,$$

$$(3) \quad L_t = 0.54 - 0.864G_{t-1} - 0.809E_{t-1} + 0.835R_t + 1.000A_t,$$

where

G = Government purchases of goods and services

E = Expenditures other than for the purchase of goods and services

L = Lending or borrowing (-), net

A = ARRA grants

R = Revenues excluding ARRA grants

and where the budget constraint is

$$(4) \quad G_t + E_t + L_t = R_t + A_t.$$

The ARRA grants (A) and the other revenues (R) are treated as exogenous, while G , E , and L are endogenous, changing as the states and local governments react to changes in income. The budget constraint places cross-equation restrictions on the system. The coefficients on the ARRA grants and the other revenues variable are constrained to sum to one, and the coefficients on the lagged dependent variables in the purchases and other expenditures equations are constrained to sum to zero. These lagged dependent variables allow for a slow adjustment due to a variety of adjustment costs as the ARRA grants and other revenues are allocated into these two categories of expenditures. The cross-equation constraints were imposed in the estimation of the equations. All the estimated coefficients are statistically significant at the 5 percent level.

Note that the coefficient on the ARRA grant variable in the net lending equation is very close to 1, meaning that the direct effect affect of ARRA grants was to lower net borrowing by the same amount as these ARRA grants. Second, note that the coefficient on the ARRA grant variable in the purchases equation is negative while the

¹⁰ Net borrowing is computed from the changes in financial assets and liabilities in the Federal Reserve's Flow of Funds accounts and from state and local expenditures and receipts in BEA's National Income and Product Accounts. Because of the different data sources the two measures of net borrowing are not exactly the same, differing by a statistical discrepancy.

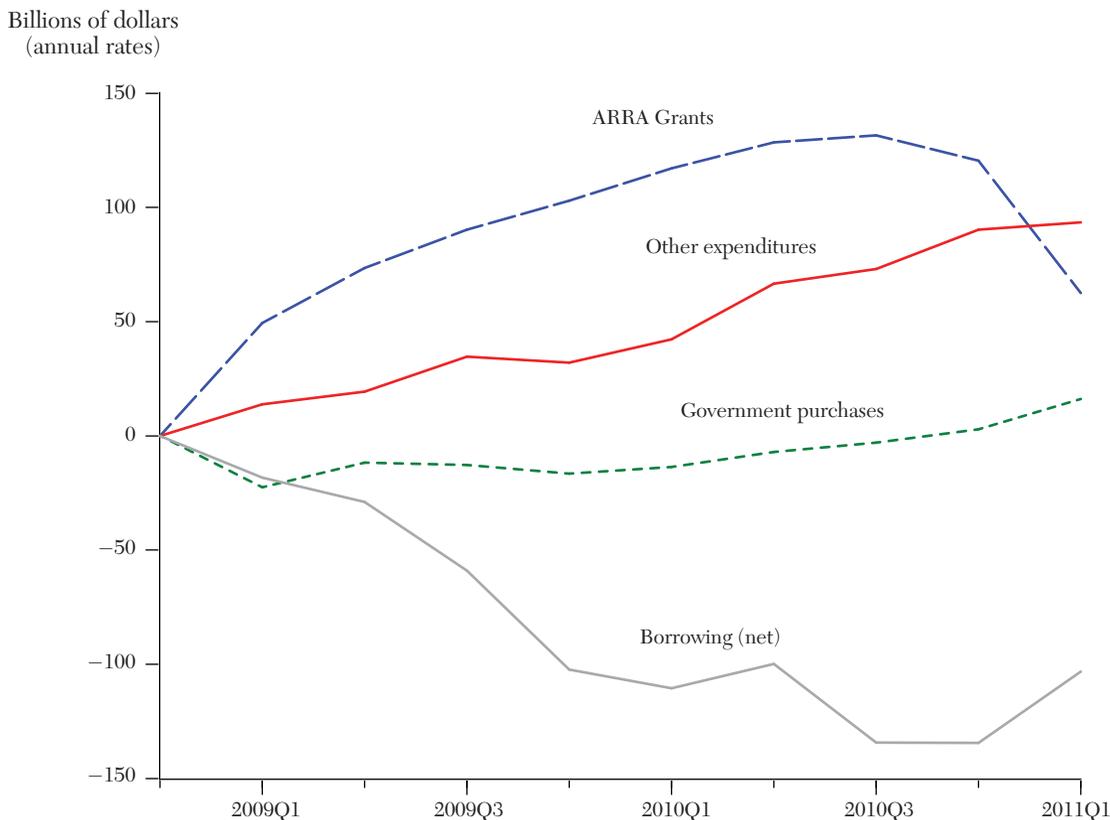


Figure 4. ARRA Grants and State and Local Budgets
(Change from 2008.4 when ARRA Grants were Zero)

coefficient on the ARRA grant variable in the other expenditures equation is positive; since the sum of these coefficients must be approximately zero, they are nearly equal but of opposite signs, meaning ARRA had no effect on the sum of purchases and other expenditures.

4.4 Counterfactual Simulations with the Estimated Model

To investigate the counterfactual hypothesis of no ARRA program, and thereby illustrate the impact of ARRA, one can simulate the three-equation system for the case where

$A = 0$. The counterfactual path is compared with the actual path of ARRA grants in figure 5 and the results of the simulation are shown in figures 6–9.

In each of figures 6 through 9, the *historical data* are shown along with the *counterfactual simulation*. Also shown is the *dynamic simulation* of the three-equation system; this simulation sets the variable A equal to the actual ARRA grants, but sets the residuals to zero rather than to the estimated residuals. In all cases, the dynamic simulations closely track the historical data indicating that the model fits the data well.

Billions of dollars

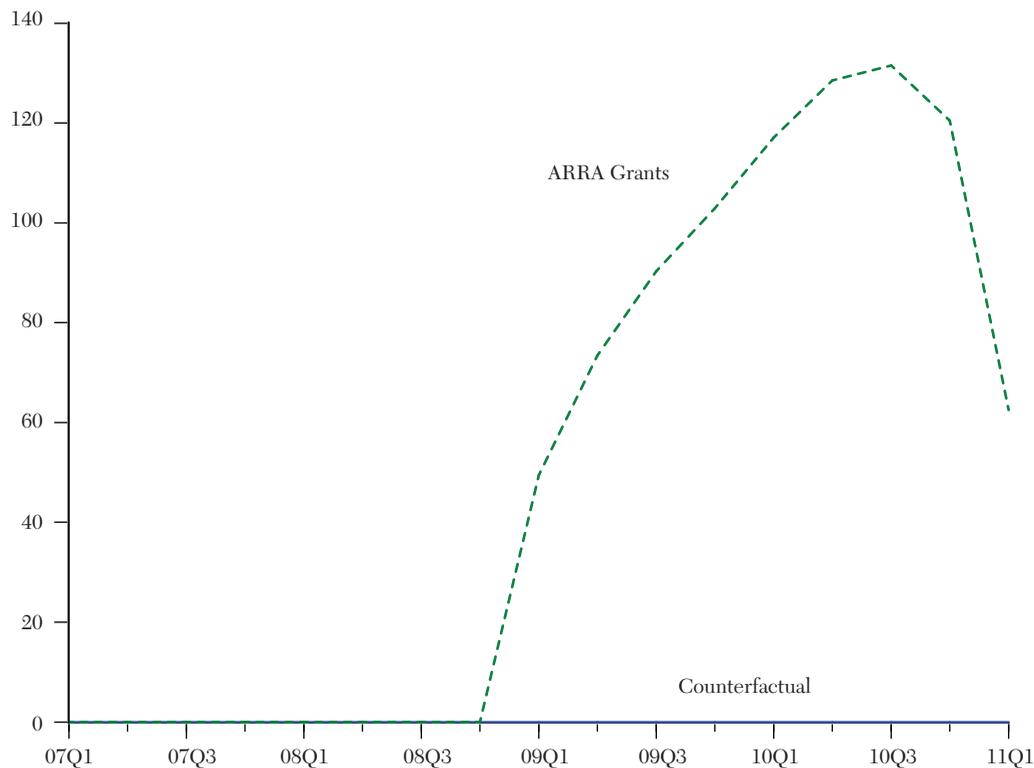


Figure 5. Actual and Counterfactual ARRA Grants to State and Local Governments

Figure 6 shows that in the absence of the 2009 stimulus grants, net borrowing by state and local governments would have been greater than it was with the grants. This is consistent with the view that state and local governments tried to smooth their expenditures in the face of temporary changes in income, much as households without borrowing constraints did.

4.5 *The Plausibility of the Counterfactual*

One might question the plausibility of these simulations, arguing that many state

governments were liquidity or borrowing constrained following the financial crisis and they simply could not have borrowed more if ARRA had not existed. At the least increased borrowing spreads would have reduced the incentives to borrow.

However, an examination of the changes in state and local government financial assets and liabilities using the Flow of Funds data from the Federal Reserve shows that the counterfactual increase in net borrowing would have been quite likely even if there were such borrowing constraints. As a matter of accounting, an increase in net borrowing

Billions of dollars

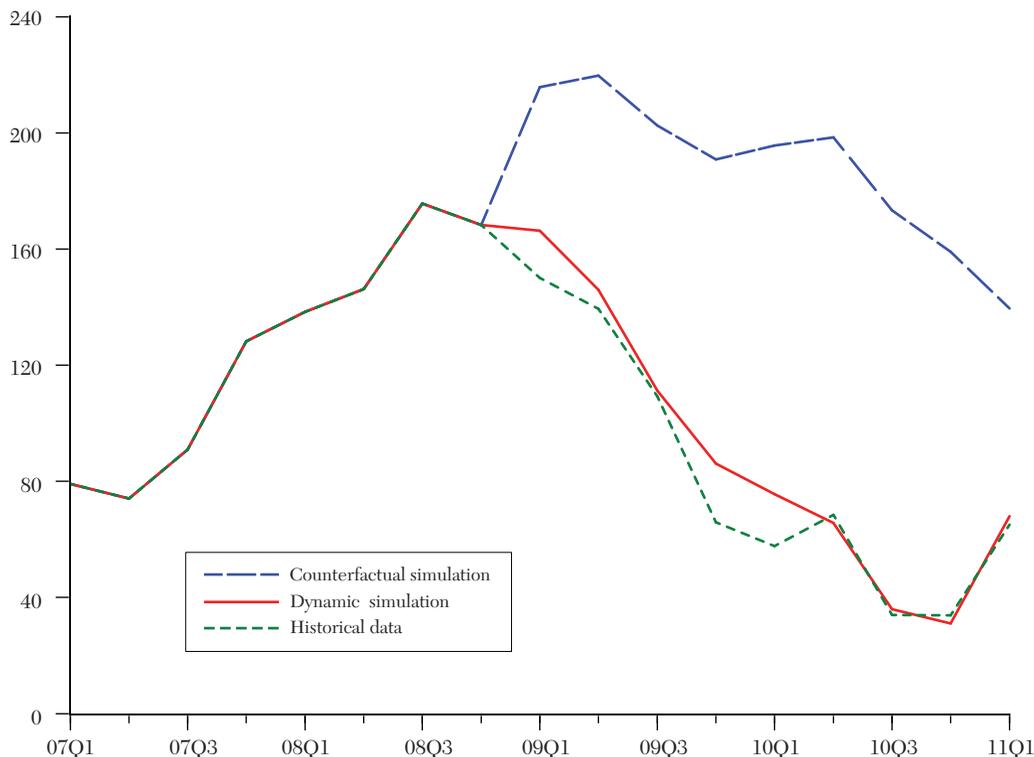


Figure 6. Borrowing (net) by State and Local Governments: Historical and Counterfactual without ARRA

occurs when the net acquisition of financial assets is smaller than the net increase in liabilities. So net borrowing can decrease when there is a decrease in the acquisition of financial assets.

According to annual Flow of Funds data, net borrowing fell by about \$118 billion from 2008 to 2010, or during the first two years of ARRA. This is consistent with quarterly data from the BEA shown in figure 4. During this same period, there was a net increase in liabilities of \$53 billion and net acquisition of financial assets of \$171 billion (which gives the \$118 billion

decrease in net borrowing during the first two years of ARRA). Thus state and local governments were adding significantly to their financial assets as ARRA grants came in. Indeed, it appears that they were saving the grant money rather than using it to increase expenditures.

These data suggest, therefore, that the counterfactual is quite plausible: For net borrowing to have increased in the counterfactual compared with history, it would have been enough for the states simply to have not increased their acquisition of financial assets by as much as they did. So even

Billions of dollars

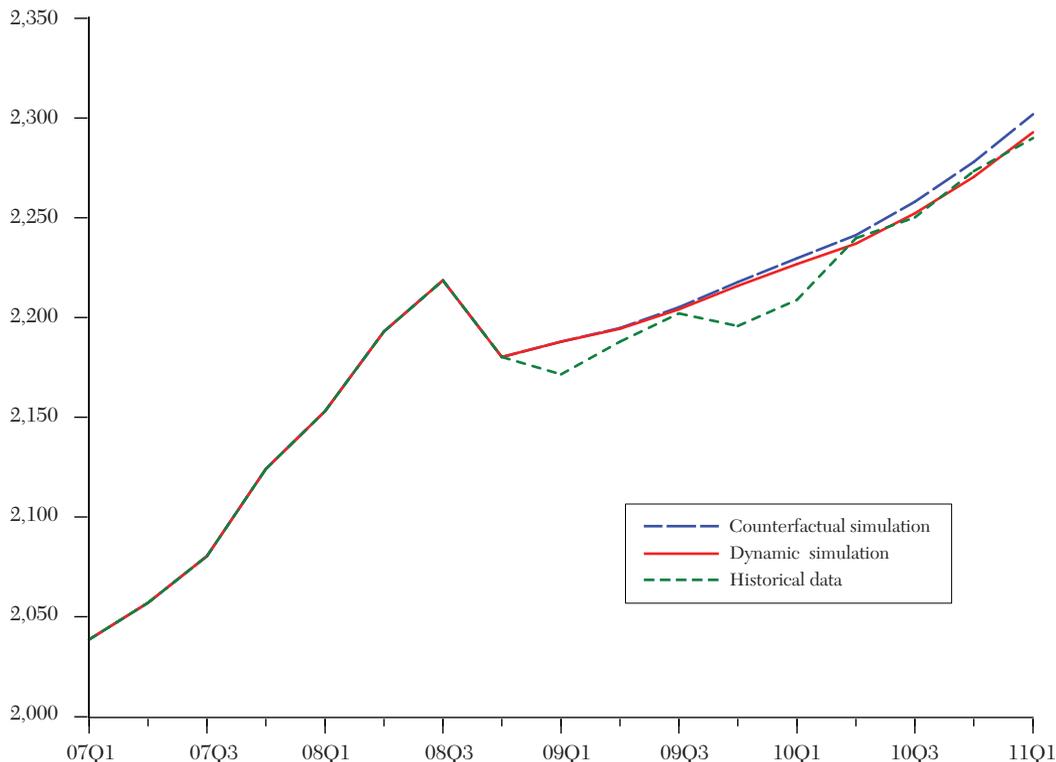


Figure 7. Total Expenditures by State and Local Governments: Historical and Counterfactual without ARRA

without increasing their liabilities, the state governments could have increased their net borrowing by reducing their acquisition of financial assets. Given that state and local governments increased their financial assets by such a large amount during 2009 and 2010, the counterfactual net borrowing path in figure 6 seems quite plausible.

4.6 Expenditure Switching: Hypothesis and Test

Figure 7 shows that total state and local expenditures would have been about the

same in the absence of the ARRA grants as they were with the ARRA grants. In this sense, ARRA had no impact on *total* state expenditures. But figures 8 and 9 also illustrate the striking divergence of the components of total expenditures—purchases and other expenditures—that ARRA caused.

Why did ARRA cause states to shift funds away from government purchases toward these other expenditures, which consist largely of transfer programs such as Medicaid and Temporary Assistance to Needy Families (TANF)? Medicaid is the public health insurance for poor women

Billions of dollars

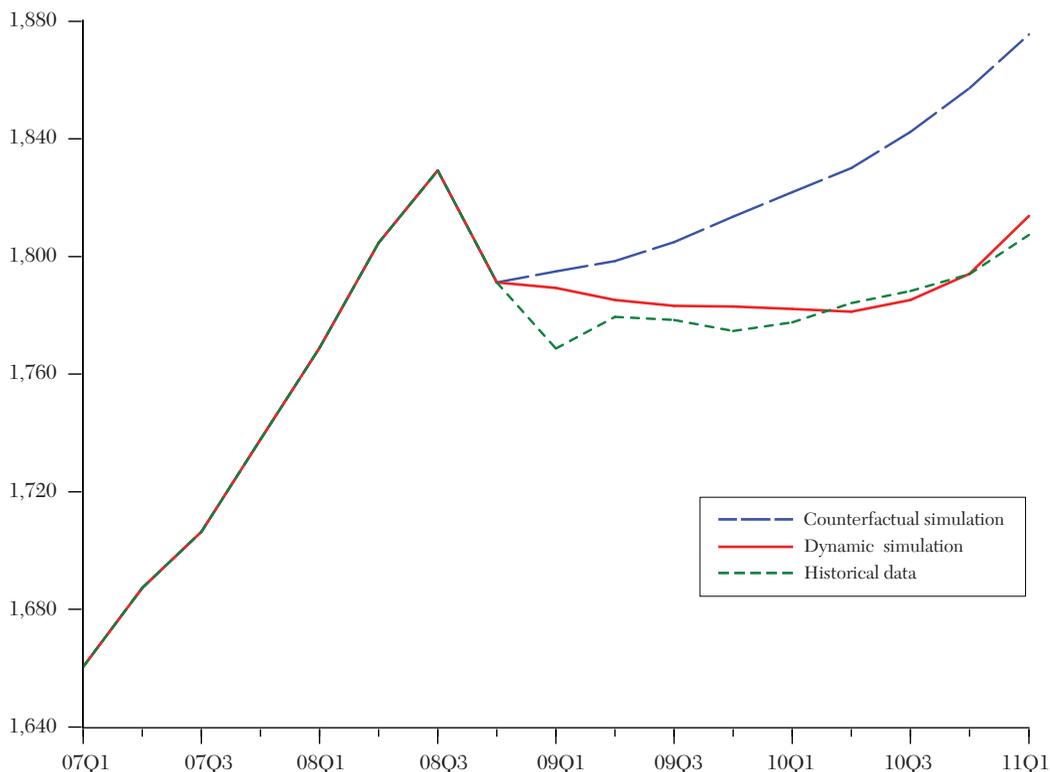


Figure 8. Purchases of Goods and Services by State and Local Governments: Historical and Counterfactual without ARRA

and children, the disabled, and elderly in nursing homes, while TANF is cash welfare for the poor. One hypothesis is that ARRA stipulated that states receiving additional Medicaid grants could not reduce benefits or restrict eligibility rules relative to what they were on July 1, 2008. In some states, this meant reversing benefit reductions or eligibility restrictions that were implemented in the previous seven months before ARRA was passed in February 2009. This “hold-harmless” provision could have forced states to shift funds away from purchases to transfers.

To test this hypothesis, Cogan and Taylor (2010) split ARRA grants (A) into Medicaid (M) and non-Medicaid (N), using the BEA satellite account for ARRA, and ran the regressions in equations (1), (2), and (3), with A replaced by M and N . The Medicaid grant variable in the government purchases equation was negative and significant; the estimated coefficient was -0.318 with a t -statistic of -2.3 . In contrast, the coefficient of the non-Medicaid coefficient was insignificantly different from zero. Hence the statistical results confirm the hold harmless hypothesis.

Billions of dollars

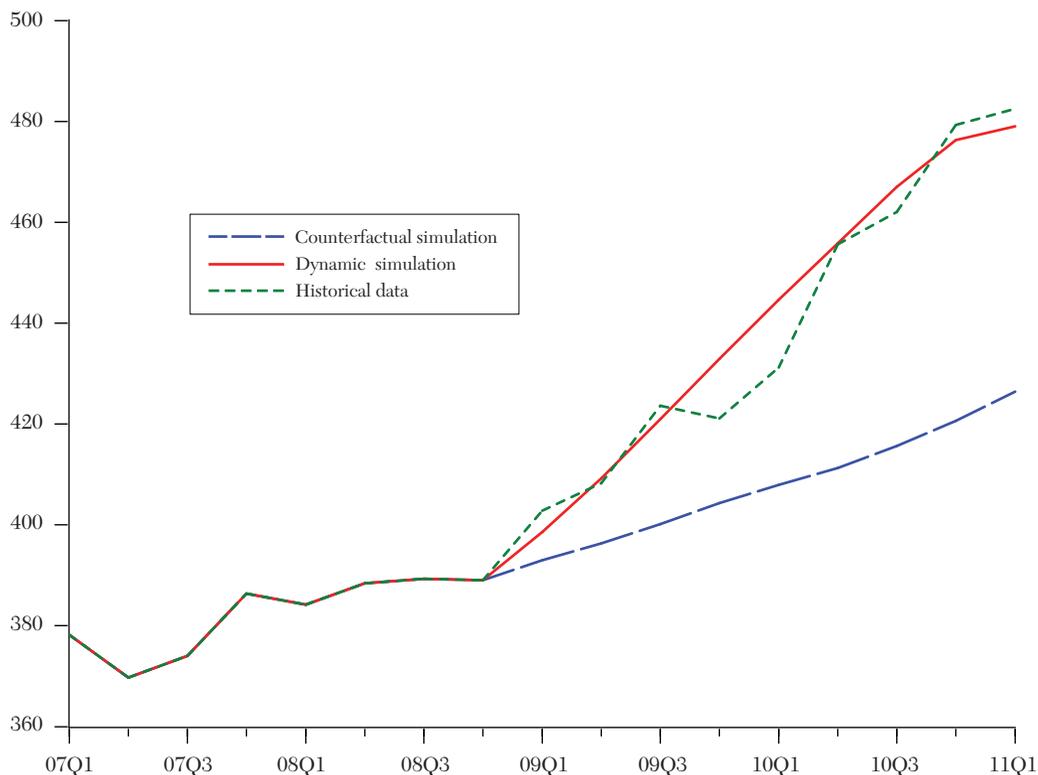


Figure 9. Other Expenditures by State and Local Governments: Historical and Counterfactual without ARRA

To the extent that government purchases had a greater impact on GDP than temporary transfers—which the permanent income theory predicts—then ARRA could have had a negative effect on the economic recovery by reducing purchases and increasing transfers by the same amount. Moreover, according to the simulations in figure 8, the cumulative negative effect on state and local government purchases was \$85 billion. This was nearly three times as large as the \$30 billion cumulative positive effect of ARRA on federal government purchases. The results indicate that government purchases were

less than they would have been without ARRA.

As early as the summer of 2009, it was becoming apparent that the recovery of the U.S. economy from the recession of 2007–09 had little to do with government purchases related to the stimulus. For example, Cogan, Taylor, and Volker Wieland (2009) reported that nondefense government purchases contributed less than 1 percentage points to the 5.4 percentage point real GDP growth improvement from the first to the second quarter of 2009. A comprehensive international comparison by Hyunseung Oh and

Ricardo Reis (2011) shows that government purchases did not increase by very much in many other countries during the recovery from the recent recession. The review here of empirical work on how ARRA worked provides an explanation in the case of the United States. Despite the stated intention to increase infrastructure and other government purchases through large grants to the states, ARRA did not deliver the intended increase.

5. Conclusion

In sum, this empirical examination of the direct effects of the three countercyclical stimulus packages of the 2000s indicates that they did not have a positive effect on consumption and government purchases, and thus did not counter the decline in investment during the recessions as the basic Keynesian textbook model would suggest. Individuals and families largely saved the transfers and tax rebates. The federal government increased purchases, but by only an immaterial amount. State and local governments used the stimulus grants to reduce their net borrowing (largely by acquiring more financial assets) rather than to increase expenditures, and they shifted expenditures away from purchases toward transfers.

Some argue that the economy would have been worse off without these stimulus packages, but the results do not support that view. According to the empirical estimates of the impact of ARRA, if there had been no temporary stimulus payments to individuals or families, their total consumption would have been about the same. And if there had been no ARRA grants to states and localities, their total expenditures would have been about the same. The counterfactual simulations show that the ARRA-induced decline in state and local government purchases was larger than the increase in federal government purchases due to ARRA. In terms of the

simple example of model A versus model B presented above, these results are evidence against the views represented by model A, and thus against using such models to show that things would have been worse.

Others argue that the stimulus was too small, but the results do not lend support to that view either. Using the estimated equations, a counterfactual simulation of a larger stimulus package—with the proportions going to state and local grants, federal purchases, and transfers to individuals the same as in ARRA—would show little change in government purchases or consumption, as the temporary funds would be largely saved. Of course, the story would be different for a stimulus program designed more effectively to increase purchases, but it is not clear that such a program would be politically or operationally feasible.

More generally, the results from the 2000s experience raise considerable doubts about the efficacy of temporary discretionary countercyclical fiscal policy in practice. In this regard, the experience with the stimulus packages of the 2000s adds more weight to the position reached more than thirty years ago by Lucas and Sargent (1978) and Gramlich (1978, 1979).

REFERENCES

- Calmes, Jackie, and Michael Cooper. 2009. "New Consensus Sees Stimulus Package as Worthy Step." *New York Times*, November 21, p. A1.
- Cogan, John F., Tobias Cwik, John B. Taylor, and Volker Wieland. 2010. "New Keynesian versus Old Keynesian Government Spending Multipliers." *Journal of Economic Dynamics and Control*, 34(3): 281–95.
- Cogan, John F., and John B. Taylor. 2010. "What the Government Purchases Multiplier Actually Multiplied in the 2009 Stimulus Package." National Bureau of Economic Research Working Paper 16505.
- Cogan, John F., John B. Taylor, and Volker Wieland. 2009. "The Stimulus Didn't Work." *Wall Street Journal*, September 17.
- Congressional Budget Office. 2011. "Estimated Impact of the American Recovery and Reinvestment Act on Employment and Economic Output from January 2011 through March 2011." Washington, D.C.: Congressional Budget Office.

- Conley, Timothy, and Bill Dupor. 2011. "The American Recovery and Reinvestment Act: Public Sector Jobs Saved, Private Sector Jobs Forestalled." Ohio State University Working Paper.
- Gramlich, Edward M. 1978. "State and Local Budgets the Day After It Rained: Why Is the Surplus So High?" *Brookings Papers on Economic Activity*, 1: 191–214.
- Gramlich, Edward M. 1979. "Stimulating the Macro Economy through State and Local Governments." *American Economic Review*, 69(2): 180–85.
- Johnson, David S., Jonathan A. Parker, and Nicholas S. Souleles. 2006. "Household Expenditure and the Income Tax Rebates of 2001." *American Economic Review*, 96(5): 1589–1610.
- Lucas, Robert E., and Thomas J. Sargent. 1978. "After Keynesian Macroeconomics." In *After the Phillips Curve: Persistence of High Inflation and High Unemployment*, 49–72. Boston: Federal Reserve Bank of Boston.
- Oh, Hyunseung, and Ricardo Reis. 2011. "Targeted Transfers and the Fiscal Response to the Great Recession." National Bureau of Economic Research Working Paper 16775.
- Parker, Jonathan A., Nicholas S. Souleles, David S. Johnson, and Robert McClelland. 2011. "Consumer Spending and the Economic Stimulus Payments of 2008." National Bureau of Economic Research Working Paper 16684.
- Ramey, Valerie A. 2011. "Can Government Purchases Stimulate the Economy?" *Journal of Economic Literature*, 49(3): 673–85.
- Shapiro, Matthew D., and Joel Slemrod. 2003. "Consumer Response to Tax Rebates." *American Economic Review*, 93(1): 381–96.
- Shapiro, Matthew D., and Joel Slemrod. 2009. "Did the 2008 Tax Rebates Stimulate Spending?" *American Economic Review*, 99(2): 374–79.
- Taylor, John B. 2000. "Reassessing Discretionary Fiscal Policy." *Journal of Economic Perspectives*, 14(3): 21–36.
- Taylor, John B. 2009. "The Lack of an Empirical Rationale for a Revival of Discretionary Fiscal Policy." *American Economic Review*, 99(2): 550–55.
- Taylor, John B. 2010. "Perspectives on the U.S. Economy: Fiscal Policy Issues." Testimony Before the Committee on the Budget, U.S. House of Representatives.

This article has been cited by:

1. Jonathan A. Parker. 2011. On Measuring the Effects of Fiscal Policy in Recessions. *Journal of Economic Literature* **49**:3, 703-718. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]