The Costs (and Benefits) of Fiscal Consolidation: What Does the Experience of U.S. States Tell Us?*

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Abstract

This paper examines the impact of fiscal consolidation on short-run economic growth of U.S. states. Unlike countries, U.S. states face some form of balanced budget requirement and share a common currency, which leads to a shorter and more recognizable pattern of fiscal actions. We use this pattern along with differences in state budgetary rules to identify episodes of fiscal consolidation. In controlling for the endogeneity of current fiscal policy, we find that fiscal consolidations lower short-run growth. In addition, we find that revenue-based fiscal consolidations are more contractionary than spending-based ones. Our results hold for multiple fiscal data sources, different definitions of fiscal consolidation, and variations to the identification scheme.

Keywords: fiscal consolidation, austerity, state fiscal policy

JEL Classification: E32, E62, H70, R10

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1 Introduction

The euro zone crisis of 2010-11 has sparked a renewed debate into the relationship between fiscal consolidation and short-term economic growth. On the one hand, standard Keynesian theory predicts that cuts in government spending or increases in taxes will reduce real aggregate demand. This decrease in aggregate demand will result in a contraction in output in the short run. In monetary unions recessions are likely to be more severe due to the absence of exchange-rate devaluation and the availability of expansionary monetary policy. On the other hand, small tax increases today eliminate the need for larger and more disruptive adjustments in the future (Blanchard, 1990) and fiscal consolidation today may be seen as a signal that public spending will be lowered in the future (Giavazzi and Pagano, 1990). As a result, households can expect an increase in their permanent income and thus will consume more today. This and other non-Keynesian effects can lead to a less recessionary or even expansionary fiscal consolidation in the short run.

The empirical literature on the short-run impact of fiscal consolidation is equally divided. In one of the earliest analysis, Giavazzi and Pagano (1990) show that sharp fiscal consolidations in Denmark during 1983-86 and Ireland during 1987-88 were accompanied by rapid economic growth. Subsequent cross-country analysis by Alesina and Perotti (1997) and Alesina and Ardagna (2010, 2013) find that fiscal consolidations can stimulate real economic activity, especially when driven by spending cuts. Using an alternative narrative approach to identify fiscal consolidations, IMF (2010) and Guajardo, Leigh and Pescatori (2011, 2014) find that fiscal consolidations are contractionary.

Guajardo et al. (2014) attribute the difference in results to the methods used to identify fiscal consolidations. The conventional approach of Alesina and Ardagna (2010, 2013) use the change in the cyclically-adjusted primary balance relative to GDP (CAPB) to identify fiscal consolidation episodes. Intended to remove the effects of the business cycle from the fiscal balance, the change in the CAPB may include other non-policy factors like asset market booms and busts or alternative policy objectives like aggregate demand management (IMF, 2010). As a result, the traditional approach may suffer from endogeneity bias whose direction supports expansionary fiscal consolidation (Guajardo et al., 2014). The *narrative* approach of IMF (2010) uses stated

deficit reduction objectives in the historical record to identify fiscal consolidations. Although Guajardo et al. (2014) find that these narrative-based fiscal consolidations are more exogenous than those based on CAPB, the use of policy statements is likely to create measurement error resulting from contradictory government statements and unreliable projections of tax and expenditure changes.

In this paper, we use state-level data to examine the impact of fiscal consolidation on short-run economic growth. Like the conventional approach, we define a fiscal consolidation as a minimum increase in the change in CAPB. However, current state fiscal policy and the probability of a fiscal adjustment is endogenous to economic growth. In response, we use the presence and variation of balanced-budget rules to identify the effects of fiscal consolidation. During times of fiscal stress, these budgetary rules force states to reduce their budget deficit, which generates a recognizable pattern of fiscal behavior. Moreover, states with stricter rules will eliminate the deficits more quickly (Alt and Lowry, 1994).

Our state-level analysis affords three potential benefits in estimating the effects of fiscal consolidation. First, state-level fiscal data are available from two independent sources. The *State Government Finances* is collected by the U.S. Census Bureau (Census) and records detailed accounts of all revenue and spending. *The Fiscal Survey of the States* is survey data administered by the National Association of State Budget Officers (NASBO) and focuses on activities of the general fund. Although different in their coverage and focus, the fiscal data provide us with two independent sets of results for comparison purposes.

Second, U.S. states share a common currency and monetary policy. Lambertini and Tavares (2005) and Beetsma, Giuliodori and Hanson (2012) show that the success of a fiscal consolidation depends upon the monetary and exchange rate policy of the country. Although these policies can be included as additional regressors, they are likely to be inextricably linked to the fiscal consolidation decision. For states, however, changes in U.S. monetary policy and the exchange rate are uniform and thus can be controlled for with individual time effects.

Third, unlike most countries, U.S. states face some form of legal balanced budget requirement (BBR). These BBR's are constitutional (and statutory) legal provisions that seek to balance the general operating budget and restrict indebtedness of the state (NCSL, 1999). Regardless of the form, states must respond to an unexpected negative budget shock by reducing spending and/or raising revenue. In addition, the stringency of BBR's varies across states. Weak BBR's

apply to the *enactment* of a state budget, while stricter BBR's apply to its *execution*. The strictest "No Carry" rule prohibits the rolling over of deficits into the next fiscal year.

We use panel instrumental variables (IV) to estimate the effects of fiscal consolidation on short-run growth. Our identification rests on the notion that a state budget deficit should only impact future economic activity through the fiscal response. In our analysis, the fiscal consolidations are observable but the fiscal responses are not. As a result, identification requires that a budget deficit (i) raise the probability of a fiscal adjustment for all states and increases it further for states with strict BBR's (relevance), and (ii) not directly effect future growth (excludability). In our first-stage results, we find support for the first condition in that a negative budget balance by itself and interacted with BBR's are strong determinants of future consolidations. Although the second condition cannot be directly tested, we nevertheless find indirect evidence of excludability through overidentification tests, modifications in the identification scheme, and a detailed examination of fiscal consolidations for the state of Hawaii.

We find that a fiscal consolidation has a contractionary impact on real income. In our baseline estimates, a one percent increase in the CAPB during a fiscal consolidation leads to an immediate drop in real income of 3.3 to 4.1 percent and a long-run cumulated decline of 5.0 to 5.4 percent. We find similar contractionary results using different definitions of a fiscal consolidation and modifications of the identification scheme. In terms of composition, we find that revenue-based fiscal consolidations are much more contractionary than spending-based ones.

The rest of the paper proceeds as follows. Section 2 discusses the related literature on fiscal consolidation. Section 3 describes the state fiscal data sources and the cyclical adjustment of the budget. Section 4 presents our empirical estimates of the effect of fiscal consolidations. Section 5 presents a narrative approach of state fiscal consolidations using Hawaii as an example. Section 6 concludes with some implications of our results on the euro area.

2 Related Literature

There is a large empirical literature on the macroeconomic effects of fiscal adjustments starting with the pioneering work of Giavazzi and Pagano (1990).[1] This literature examines two related questions: what factors contribute to the success of a fiscal adjustment and what are the

[1]Escolano, Mulas-Granados, Terrier and Jaramillo (2014) provides an excellent overview of this literature.

macroeconomic effects of a fiscal consolidation.

Giavazzi and Pagano (1990) raise the possibility that a fiscal consolidation, especially those driven by large spending cuts, can be expansionary due to changes in expectations of future taxes and government spending. This non-Keynesian "expectations" view argues that a credible fiscal austerity plan can increase permanent income by preventing a future costly consolidation and thus raise current output. Using the consolidation experiences of Denmark and Ireland in the 1980s, Giavazzi and Pagano (1990) find evidence to support this expectations view. Giavazzi and Pagano (1996) look at the experiences of 19 OECD countries and find that large and persistent fiscal adjustments are likely to have non-Keynesian effects, while fiscal policy during normal periods is likely to have Keynesian effects.

Subsequent studies focus on whether fiscal adjustments are successful in improving fiscal balances in the medium run. Fiscal adjustment episodes are typically selected according to the size of the improvement in the CAPB relative to GDP where success is defined as a persistent reduction in the debt-to-GDP ratio. Alesina and Perotti (1995a, 1997), Alesina, Perotti and Tavares (1998), and Alesina and Ardagna (1998) argue that fiscal adjustments that rely primarily on the reduction in transfers and government employees' compensation have a higher likelihood to be successful. On the other hand, fiscal adjustments driven by tax increases tend to be unsuccessful and contractionary. Lambertini and Tavares (2005) find that exchange rate depreciations in the two years before a fiscal consolidation significantly increase the probability of success. Hence, fiscal consolidations carried out in monetary unions are less likely to have persistent effects.

Following the euro zone debt crisis in 2010-11, the debate on fiscal adjustments has turned its focus to the economic consequences of austerity. From a methodological point of view, these studies can be divided into two groups: the traditional approach and narrative approach. The traditional approach identifies fiscal episodes by setting a threshold for an increase in the CAPB. Alesina and Ardagna (2010) define a fiscal adjustment as a one-time increase in the CAPB of at least 1.5 percent, while Alesina and Ardagna (2013) define it as an average annual increase of one percent or more for two to three years. Both studies find that cuts in current spending and its wage and non-wage components are associated with expansionary consolidations. Using panel growth regressions, Alesina and Ardagna (2013) show that fiscal consolidations as a whole do not have a statistically significant effect on GDP; however, government spending cuts raise

GDP, while tax increases reduce it.

The narrative approach follows Romer and Romer (2010) and Ramey (2011b) and uses historical records to identify fiscal episodes. Devries, Guajardo, Leigh and Pescatori (2011) examine contemporaneous policy documents for 17 OECD economies for 1978-2009 to identify 173 discretionary changes in taxes and government spending primarily motivated by budget deficit reduction. Using this narrative data, IMF (2010) estimate that a fiscal adjustment of one percent of GDP reduces real GDP by 0.5 percentage point after two years. In terms of composition, spending-based adjustments are typically less contractionary than tax-based ones. Guajardo et al. (2011) argue that selecting fiscal episodes using the traditional approach is likely to bias the results in favor of expansionary effects of austerity due to its procyclical nature. The authors compare the two approach empirically and estimate a positive output effect for the episodes identified by the traditional approach and a negative effect for the narrative-based episodes.

More recent papers by Guajardo et al. (2014), Yang, Fidrmuc and Ghosh (2015), Jordà and Taylor (2016), and Banerjee and Zampolli (2016) examine the possibility that the fiscal consolidation decision and its size are endogenous to the current state of the economy. One source of endogeneity is that the cyclical correction of the traditional approach does not remove all the automatic changes in the fiscal variable. In response, these authors use the more exogenous narrative fiscal shocks (and other lagged indicators) to instrument for the change in the CAPB at all time and also during fiscal consolidations. Using different estimation methods (2SLS, SVAR, local projections), these authors typically find that a one percent increase in the CAPB ratio, identified by a narrative fiscal shock, lowers real GDP from 0.5 to 1.0 percentage point after two years.

The second related literature is the multiplier literature. Although a subject of research since the time of Keynes, the multiplier has received renewed attention in the policy debates during and after the Great Recession. For our purposes, we focus on the recent estimates of the U.S. fiscal multiplier using aggregate and state-level data.[2] Romer and Romer (2010) use a narrative approach to identify U.S. tax policy changes driven by political ideology or deficit reduction and estimate a tax multiplier of around three. Ramey (2011b) and Barro and Redlick (2011) estimate a spending multiplier of 0.5 to 1.0 for defense spending shocks and a tax multiplier of 1.1 for average marginal tax rates. Subsequent research by Auerbach and Gorodnichenko (2012)

[2]See Ramey (2011a) for a concise review of the estimates of the income multiplier using state-level data.

find that the fiscal multiplier is larger in recessions and for investment spending in particular.

Cohen, Coval and Malloy (2011), Chodorow-Reich, Feiveson, Liscow and Woolston (2012), Clemens and Miran (2012), and Nakamura and Steinsson (2014) use exogenous variation in subnational government outlays to identify the fiscal multiplier. Our paper is closest to Clemens and Miran (2012), who estimate fiscal multipliers on state government spending. Using mid-year adjustments relative to final outlays and revenue realizations, they recover deficit shocks which are then used to estimate the different spending responses of weak vs. strong BBR states. The government multiplier is then estimated by exploiting such variation in spending response. The on-impact multiplier is estimated at around 0.40 percent, which suggests a contractionary impact of subnational fiscal adjustments on subnational output.

We examine state fiscal policy and its effect on economic activity. Our focus however is on large budgetary changes – whether they stem from spending cuts or tax increases or a combination of them. Like the contributions in the first group, we adopt the traditional approach and identify fiscal episodes by setting a threshold for the size of the CAPB. Like Clemens and Miran (2012), we exploit exogenous variation in U.S. state budgetary requirements to study the effect of fiscal adjustment on output.

3 Data

We use data for 49 states from 1973 to 2017. We exclude Alaska because its fiscal stance is heavily dependent on natural resource prices. Our dependent variable is the growth rate of real per capita state personal income net of transfers between calendar year $t-1$ and t. We deflate nominal income using the regional CPI of the Bureau of Labor Statistics. We use personal income rather than GDP because the former is available from 1929 to the present, while the latter is only consistently available for ten years. We exclude transfer receipts such as Social Security, Medicare, Medicaid, and unemployment insurance payments in an effort to eliminate the effect of redistributive policies. Appendix A provides details of our data and sources.

We use state-level fiscal data from two independent sources: *State Government Finances* of the U.S. Census Bureau (Census) and *The Fiscal Survey of the States* of the National Association of State Budget Officers (NASBO). The Census data provides a summary of annual survey findings for state governments. The data is organized in revenue, by source; expenditure, by

object and function; and indebtedness, by short- or long-term debt. Revenues and expenditures are presented within four broad activity sectors: general government, utilities, liquor stores, and insurance trust sectors. We use the general revenue and general expenditure accounts since they are under the control of legislators. Utility and liquor store revenues and expenditures are negligible amounts (less than 0.1% of total revenues and 0.3% of total expenditures). We do not consider insurance trust items; which include unemployment insurance, state pension and workers' compensation; since these items are statutory benefit payments and contributions and therefore are not discretionary. The Census data are reported by fiscal year and are available by source and function from $FY1969$ to $FY2016$.

The Fiscal Survey of the States of NASBO presents data on states' general fund receipts, expenditures and balances. This biannual survey reports enacted budgets for the following year, preliminary actual budgets for the current year, and actual budget figures for the previous year. We use actual budget figures. An advantage of NASBO data is the reporting of stabilization (rainy day) funds and their end-of-year balance. The NASBO data are reported by fiscal year and are available from $FY1979$ to $FY2017$.

There are important differences between Census and NASBO data. The Census data are more comprehensive and longer running than the NASBO data. Census includes practically all expenditures in its general expenditure concept, including outlays over which the state government has no discretion such as transfers to local governments and Medicaid payments. NASBO, on the other hand, focuses on general fund spending and revenues. General fund spending represents the primary component of discretionary expenditures from sources that have not been earmarked for specific purposes. On average, this accounts for 40 percent of total state spending. General fund revenues include most tax revenues but exclude funds received from the federal government and the proceeds from the sale of bonds. Another strength of the NASBO data is that it provides information on enacted policy changes and revenue implications. Expenditures and revenues for NASBO are reported as aggregate and not broken down by function. For this reason spending, revenues and budget balances data from Census and NASBO are not comparable and thus we use them separately.

We aim to estimate the effect of fiscal adjustments on economic conditions with real per capita income growth on the left-hand side and measures of fiscal consolidations on the right-hand side. Current economic conditions, however, affect current fiscal outcomes due to the presence of

automatic stabilizers. During economic expansions tax revenues are high and certain categories of public outlays are low and vice versa during downturns. To account for the role of economic conditions on the budget balance, we cyclically adjust our fiscal measures.

We use the cyclically-adjusted primary budget balance approach of the European Community (1995) to measure discretionary fiscal policy. The cyclically-adjusted primary balance ($CAPB$) is obtained by adding up cyclically-adjusted revenue categories, subtracting out cyclically-adjusted expenditure categories and then dividing by trend income; further details are reported in Appendix B. Our measure of interest is the change relative to the previous period in the cyclically-adjusted primary budget balance ratio, $\Delta CAPB$. By adjusting for the economic cycle, the $CAPB$ removes the impact of automatic stabilizers from actual budget balances. The main alternative is the narrative approach of Romer and Romer (2010) and Devries et al. (2011) that relies on announced fiscal plans drawn from budget documents. Relative to the narrative approach, the CAPB approach has the advantages of (i) possessing uniform methodology, (ii) recording *actual* fiscal adjustments, and (iii) capturing *all* policy changes (Escolano et al., 2014).

Figures 1 and 2 plot the distribution of $\Delta CAPB$ for the Census and NASBO samples, respectively. Both distributions are fairly symmetric and centered around zero with mean values of 0.001 and -0.001 percent.[3] The range of values for the Census data (-4.64 to 3.26 percent) is wider than for the NASBO data (-1.86 to 2.53 percent). This may be due to the fact that NASBO covers only the discretionary budgetary items or reflects a state government's desire to smooth budget balances, which will be explained in detail later. There is positive but moderate correlation ($\rho = 0.34$) between the two measures of $\Delta CAPB$. This moderate correlation suggests the two data sets are not replicating each other, but rather are capturing independent information about the stance of state fiscal policy.

In the cross-country literature, most authors define a fiscal consolidation as a minimum increase in $\Delta CAPB$ for one or more years.[4] U.S. states however face much tighter budget constraints due to BBR's. As a result, we define a state-level fiscal consolidation as a one-year increase in $\Delta CAPB$ above a certain threshold. We begin with a threshold of 1.0 percent or

[3]In contrast, the mean values for the *level* of $CAPB$ are 0.633 and 0.012 percent for Census and NASBO, indicating a slight surplus for the average budget stance.

[4]Escolano et al. (2014), Table 1 shows that the definition of a fiscal consolidation varies considerably across the variables used to measure adjustment and the threshold set. However, out of the 32 papers reviewed, 18 of them use a criteria of a minimum increase in $\Delta CAPB$ to define a consolidation.

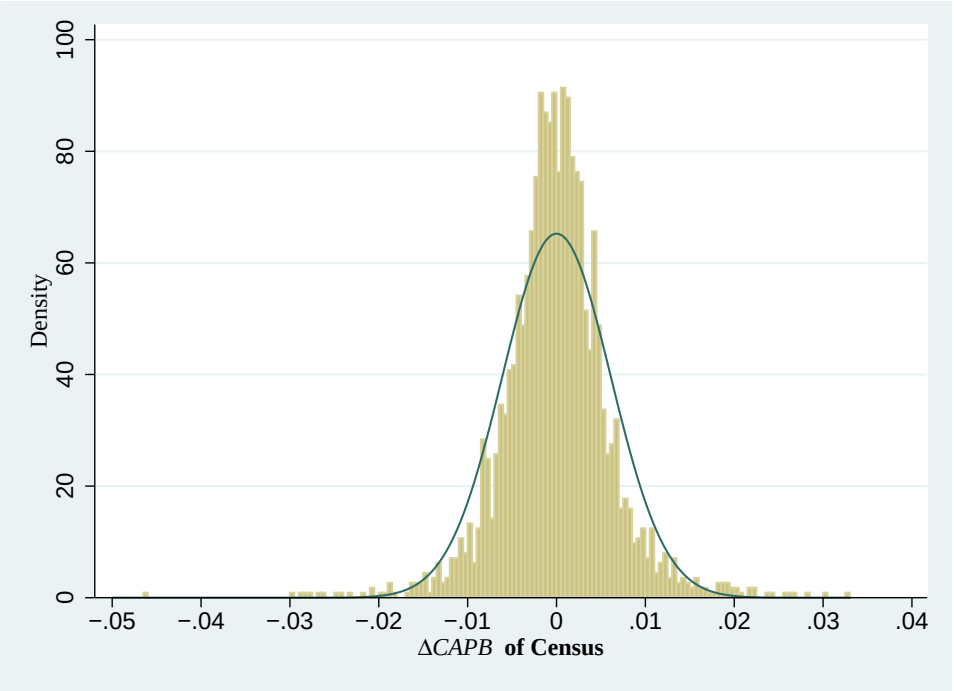

Figure 1: Distribution of $\Delta CAPB$ of Census

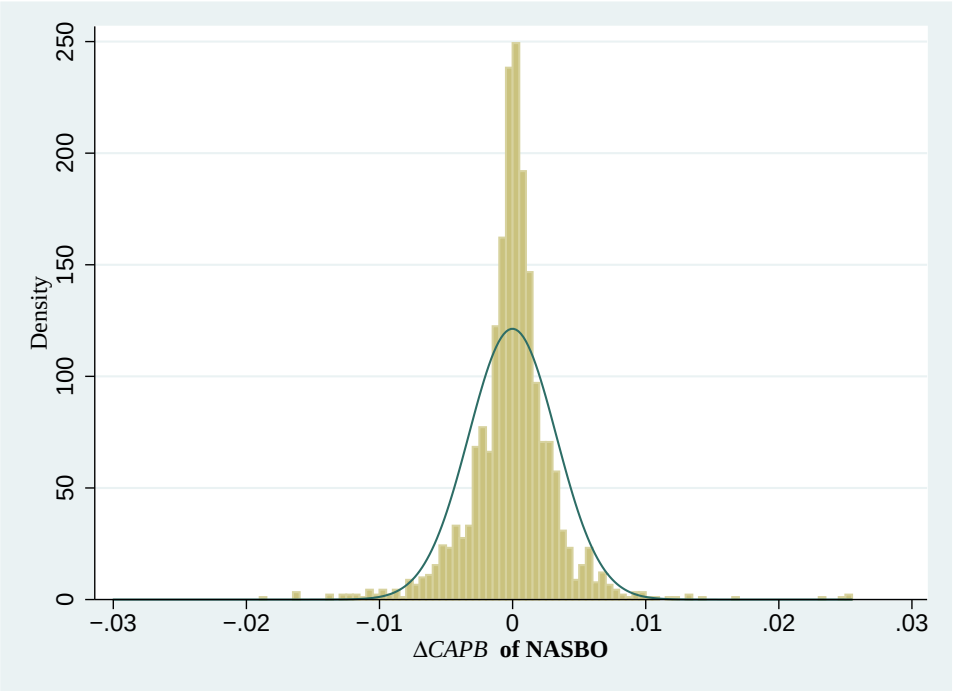

Figure 2: Distribution of $\Delta CAPB$ of NASBO

greater for Census and 0.6 percent or greater for NASBO. These fiscal consolidation thresholds generate 103 episodes for the Census sample and 49 episodes for the NASBO sample – 12 of which are common to both. The resulting incident rates of 4.6 and 2.7 percent are lower than the past estimates of Alesina and Perotti (1995b) and Alesina and Ardagna (1998, 2010), but are in-line with the more recent estimates of Devries et al. (2011), Escolano et al. (2014) and Eichengreen and Panizza (2014).[5] In later analysis, we alter the threshold to examine the robustness of our results.

Table 1 presents the summary statistics of our fiscal consolidation measures of each state budget source. The first two entries are the $\Delta CAPB$ and fiscal consolidation incidence rate. For the Census sample, the average $\Delta CAPB$ is 1.50 percent during fiscal consolidations and -0.07 percent during non-consolidations. Likewise, for the NASBO data, the average $\Delta CAPB$ is 0.97 percent during fiscal consolidation episodes and -0.03 percent otherwise. The last four entries in each panel are the composition measures, which will be discussed in section 4.6.

4 The Effect of Fiscal Consolidations

4.1 OLS Results

We follow IMF (2010), Alesina and Ardagna (2013), Guajardo et al. (2014), and Yang et al. (2015) and estimate the following equation

$$\Delta y_{s,t} = \sum_{j=1}^{2} \delta_j \Delta y_{s,t-j} + \sum_{j=0}^{2} \beta_j \Delta CAPB^{FC}_{s,t-j} + \alpha_s + \lambda_t + \epsilon_{s,t}, \tag{1}$$

where s indexes the state and t denotes time. The dependent variable Δy is the growth rate of real income (without transfers) per person measured in the calender year. The $\Delta CAPB^{FC}$ is our variable of interest measured as the change in the cyclically-adjusted budget balance relative to trend income during a fiscal consolidation and zero otherwise. Fiscal outcomes are measured for the fiscal year, which ends on June 30th for 46 of the 49 states we consider. As a result, fiscal policy lags income growth by 6 months in our regressions, which helps attenuating endogeneity concerns. The term α_s are state fixed effects, λ_t are year fixed effects, and $\epsilon_{s,t}$ is a mean-zero

[5]The incident rates are 10.0, 7.3 and 13.4 percent for the former set of papers; but 6.4, 3.8 and 1.7 percent for the latter (Escolano et al., 2014).

error term. The δ's are the autoregressive coefficients capturing the dynamic adjustment of economic activity and the β's are the direct effects (contemporaneous and lagged) of a fiscal consolidation.[6] We choose two lags ($j = 2$) based on specification tests and for comparability with past research.

We estimate equation (1) by OLS with fixed state and time effects for each fiscal data set. We then cumulate the estimated coefficients for $\Delta CAPB^{FC}$ at times t, $t+1$ and $t+2$ to measure the 3-year response of the log of real income per person to a one percentage point increase in the $CAPB$ during a fiscal consolidation. We also report the long-run effect of fiscal consolidation on real income which we measure as follows

$$\text{Long-run effect} = \frac{\sum_{j=0}^{2} \beta_j}{1 - \sum_{j=1}^{2} \delta_j}.$$

The standard errors of the impulse responses are computed using the delta method.

The OLS results for the Census are presented in the first column in Table 2 and those for NASBO are shown in the first column of Table 3. In both data sets, a change in the $CAPB$ during a fiscal consolidation has no statistically significant impact, immediate nor accumulated, on real personal income across U.S. states.

4.2 IV Strategy

The consistency of the OLS results rests upon the assumption that all variables, including contemporaneous fiscal consolidation, are exogenous. However, as discussed before, current fiscal policy and thus the probability of a fiscal consolidation is likely to be endogenous. For instance, current economic growth may increase (or decrease) the possibility of a fiscal adjustment. Moreover, the cyclical adjustment may not remove all linkages between current economic conditions and the cyclically-adjusted budget balance. Therefore, current real income could impact the measured $\Delta CAPB^{FC}$ and thus alter our measurement of a fiscal adjustment.

All U.S. states except Vermont face a legal balanced budget requirement (BBR). For state policymakers, the requirement applies to the operating budget, which is subject to annual or biennial appropriations. There are two sorts of enforcement mechanisms: prohibitions against

[6] The β's are interpreted as the change in the log of real income resulting from a one percentage point increase in $CAPB^{FC}$, namely to a fiscal consolidation of one percentage point of trend income.

carrying deficits into the next fiscal year and limits on state indebtedness, spending and revenues (NCSL, 1999). However, the nature and thus the strictness of BBR's vary across states. Some BBR's apply to the *enactment* of a state budget so that the governor and/or legislature pass a balanced budget in expectation. Other stricter BBR's apply to the *execution* of budget, with the strictest rule (called the No-Carry) prohibiting deficits to be carried over to the next fiscal year. Work by Poterba (1994), Bohn and Inman (1996) and others have shown that states with weak BBR's will respond less to budget shortfalls than those with strong rules. As a result, the probability of creating a fiscal consolidation should be lower in states with weak rules.

We exploit the requirement that all states close fiscal deficits and their institutional differences in their responses to identify contemporaneous fiscal consolidation. Realized state budget deficits are met by expenditure reductions and revenue increases in a relatively short horizon. This fiscal response to a deficit raises the probability of a fiscal consolidation. In contrast, when experiencing a budget surplus, state governments are free (though not required) to pursue expansionary policies that could lower the probability of a consolidation. We thus use the twice-lagged budget balance (as a share of personal income) and its interaction with "weak-rule" states as instruments to estimate the following fixed effects-instrumental variable (FE-IV) model:

$$\Delta y_{s,t} = \sum_{j=1}^{2} \delta_j \Delta y_{s,t-j} + \sum_{j=0}^{2} \beta_j \Delta CAPB^{FC}_{s,t-j} + \alpha_s + \lambda_t + \epsilon_{s,t}, \tag{2}$$

$$\Delta CAPB^{FC}_{s,t} = \eta\, budget_{s,t-2} + \gamma\, weakBBR_s \times budget_{s,t-2} + \sum_{j=1}^{2} \rho_j \Delta y_{s,t-j} + \sum_{j=1}^{2} \phi_j \Delta CAPB^{FC}_{s,t-j} + \theta_s + \mu_t + \nu_{s,t}, \tag{3}$$

where $budget_{s,t-2}$ is the budget balance as percentage of personal income for state s in fiscal year $t-2$ and *weakBBR* is a dummy variable that takes the value of 1 for all U.S. states for which the Advisory Commission on Intergovernmental Relations (1987) score, which measures the stringency of budgetary rules, is less than five.[7] We use a within-group FE estimator rather

[7] As with Clemens and Miran (2012), we use the Advisory Commission on Intergovernmental Relations (1987) $1-10$ score to measure strictness of BBR's. Unlike them, we include all states in our analysis regardless of whether the budgetary cycle is annual or biennial. Appendix C lists the *weakBBR* states.

than a system GMM of Arellano and Bover (1995) due the large T of our sample.[8]

Our exclusion restrictions are that twice-lagged budget deficits (and surpluses) and their interaction with BBR's have no impact on growth except through its influence on state fiscal policy, conditional on the lags of growth and fiscal policy. Formally, they are

$$E\left[budget_{s,v} \times \epsilon_{s,t}\right] = 0, \qquad \forall v \leq t-2 \tag{4}$$

$$E\left[budget_{s,v} \times weakBBR_s \times \epsilon_{s,t}\right] = 0, \qquad \forall v \leq t-2. \tag{5}$$

The economic reasoning for the first assumption is that the presence of a deficit (or surplus) should have no direct effect on the growth of real per capita income two years in the future and afterwards. Instead, a deficit should trigger a fiscal response by state policymakers, which in turn will raise the possibility of a fiscal consolidation. The economic rationale for the second assumption is that the institutional constraints governing that response in the form of BBR's should have no direct effect on future growth. Following Clemens and Miran (2012), this assumption can be rewritten as $E\left[budget_{s,v} \times \epsilon_{s,t}\right] = E\left[budget_{s,v} \times \epsilon_{s,t} | weakBBR = 1\right] = 0$, which is interpreted as budget deficits should contain similar economic content for weak- and strong-BBR states and we estimate only the effects of the different fiscal response to budget balances.

There are three important things to note about our instruments. First, we split the budget balance into two separate variables for positive (surplus) and negative (deficit) to allow for different responses. Similarly, we split the interaction term into two separate variables to allow for the impact of BBR's to vary across surpluses and deficits. Second, the budget rule is time-invariant so its effect can only occur through its interaction with the twice-lagged budget share. Third, we lag the budget share by two periods to allow for formation of a policy response and also to help ensure exogeneity.

[8] A large T has two important implications for the choice of panel FE-IV vs. GMM. First, the Nickell bias of lagged growth occurring under FE decreases in magnitude as T becomes larger relative to N and also with the strength of the autocorrelation coefficient (Judson and Owen, 1999). Second, the number of instruments grows quadratically in T and GMM becomes inconsistent as the number of instruments diverge. Even if one collapses the instruments as recommended by Roodman (2009), one endogenous variable under system GMM requires 80 instruments when $T = 45$.

4.3 2SLS Results

Tables 2 and 3 present our FE-IV estimates for Census and NASBO, respectively. The coefficients for the first-stage instruments are shown in Panel B and those for the second-stage cumulative effects are shown in Panel A. The robust standard errors clustered on each state are in parentheses. We consider alternative combinations of our four potential instruments to arrive at our preferred specification in column 5.

We begin by discussing the performance of our instruments in Panel B. In column 2, we use the twice-lagged interaction terms as instruments, which corresponds to equation (4). We find that weak-rule states have a $0.23-0.56$ percent lower $\Delta CAPB_{s,t}^{FC}$ (during a fiscal consolidation) for each one percent budget deficit at time $t-2$. At the same time, there is no statistical difference in the response to a budget surplus. In column 3, we use the twice-lagged budget levels as instruments. Our first-stage results show that a budget deficit increases fiscal consolidation two years later. The interpretation is that a one percent budget deficit leads to a $0.12-0.23$ increase in $\Delta CAPB_{s,t}^{FC}$. At the same time, a fiscal surplus has no significant effect. In column 4, we include the twice-lagged budget levels along with their interactions as instruments and confirm that budget deficits lead to greater fiscal consolidation with weak-rule states responding less than medium- and strong-rule states.

In column 5, we use our preferred set of instruments of twice-lagged budget deficit and its interaction with $weakBBR$. These instruments in Panel B have their correct sign and strong predictive power. In addition, our preferred instruments result in the highest first-stage F-statistic, while maintaining their exogeneity according to the Hansen overidentification and second-order autocorrelation AR(2) tests.

Our second-stage 2SLS estimates in panel A show that state fiscal consolidation are *contractionary*. By controlling for the simultaneity of current fiscal actions and growth, we estimate a negative effect of a fiscal consolidation on real income at time t to $t+2$ and also in the long run. In the Census data, a one percent increase in the CAPB during a fiscal consolidation leads to an immediate drop in real income of $4.1-4.2$ percent at time t and a cumulated $5.0-5.1$ percent decline in the long run. In the NASBO data, the effects are very similar although more varied: a drop in real income of $2.2-6.0$ percent at time t to $3.97-9.1$ in the long run. However, for our preferred instrument set in column 5, the estimated effects of a fiscal consolidation are

remarkable similar with an immediate and long-run effects of -4.1 and -5.0 percent for Census and -3.2 and -5.4 percent for NASBO.[9]

4.4 Robustness to Threshold

Tables 4 and 5 present the results using alternative thresholds of a fiscal consolidation. Panel A shows the cumulated long-run effect using 2SLS, while panel B displays the corresponding long-run effect using OLS. In the first four columns, we move from no threshold ($\Delta CAPB > 0$) in column 1 to a lower threshold in 2 to our baseline threshold in 3 to a higher threshold in 4. For 2SLS, we use the twice-lagged budget deficit and its interaction with *weakBBR* as instruments in the first four columns.

In panel B, we find evidence of positive correlation between a fiscal consolidation and real income. In the Census data, the OLS estimate for the long-run effect is positive in each instance and significant in column 1. The point estimates of $0.35 - 0.76$ cumulated effect are in line with those found by Alesina and Ardagna (2010, 2013) for OECD countries.

When we control for endogeneity in panel A, the 2SLS results continue to find that fiscal consolidations are *contractionary*. For all thresholds, a fiscal consolidation leads to a statistically significant decrease in real income. Not surprisingly, the magnitude of the decrease in real income rises in absolute value as the threshold value (along with the criteria for a consolidation) increases. Under no threshold in column 1, a one percent increase in the $\Delta CAPB$ leads to a 3.1 to 5.4 percent decrease in real income in the Census and NASBO data. The estimated effects of a fiscal consolidation increase as the threshold rises and thus the consolidations become more severe. Under the highest threshold in column 4, a one percent increase in the $\Delta CAPB$ leads to a 7.2 and 5.9 percent decrease in real income.

Alesina and Ardagna (2013) argue that the estimate for fiscal consolidation can suffer from omitted variables bias if $\Delta CAPB$ for non-consolidation periods is not included. In response, we estimate our model with both fiscal consolidation (FC) and non-fiscal consolidation (NFC)

[9]We also used the criteria of a fiscal consolidation in *both* fiscal data sets to record $\Delta CAPB^{FC}$. Using either the Census or NASBO data to measure $CAPB$, we estimate a negative long-run impact of a fiscal consolidation although the magnitude is quite high and imprecise. This is not surprising given that these 12 observations represent 0.7 percent of the sample.

variables

$$\Delta y_{s,t} = \sum_{j=1}^{2} \delta_j \Delta y_{s,t-j} + \sum_{j=0}^{2} \beta_j \Delta CAPB^{FC}_{s,t-j} + \sum_{j=0}^{2} \gamma_j \Delta CAPB^{NFC}_{s,t-j} + \alpha_s + \lambda_t + \epsilon_{s,t}, \qquad (6)$$

where $\Delta CAPB^{NFC}$ is equal to the change in the cyclically-adjusted balance in "normal" times (non-consolidation periods) and zero during periods of fiscal consolidation. For our instruments, we use the twice-lagged deficit and surplus and their interactions under 2SLS.

The results for equation (6) are presented in the last three columns of Tables 4 and 5. As before, we move the threshold from lower in column 5 to our baseline in 6 to higher in 7. Under OLS, there is evidence of a positive correlation between $\Delta CAPB$ and real income growth during fiscal consolidations and even stronger evidence during non-consolidation periods. Under 2SLS, however, we find that a fiscal consolidation reduces real income growth, while a non-consolidation has no significant impact. The point estimates imply that a one percent increase in $\Delta CAPB$ during a fiscal consolidation decreases real income in the long run by $4.1 - 7.0$ percent under Census and $3.4 - 4.2$ percent under NASBO.

4.5 Robustness to Instrument Choice

Table 6 examines the robustness of our results to instrument selection. The odd columns report the results for the Census data, while the even columns show those for the NASBO data. We start in columns 1 and 2 by using *only* the interaction condition (4) to achieve identification and find that a fiscal consolidation reduces real income by $4.0 - 5.0$ percent. In columns 3 and 4, we use two alternative fiscal institutions – budgetary line-item veto and supermajority rules for tax increases – interacted with the twice-lagged deficit as instruments. Knight and Levinson (2000) show that these fiscal institutions influence the fiscal responses to budget shocks. Although the instrument sets are weaker, we nevertheless continue to find that a fiscal consolidation is contractionary.

Several threats to our exclusion restrictions still remain. First, the tax structure and/or spending commitments of states could vary across weak and strong BBR states. As a result, an economic shock of a given size could result in systematically different deficits for the two group of states (Clemens and Miran, 2012). We examine this possibility by comparing various fiscal

measures across BBR's and find no statistical difference in the means. The second threat is that the lagged deficit itself may directly influence growth through expectations of a future fiscal adjustment. With two instruments available, we can enter the twice-lagged deficit variable as an additional regressor and test its significance. We find in columns 5 and 6 that the lagged deficit has no significant effect while the fiscal consolidation variables continue to exert a negative effect. The third threat is that the policies used to achieve a consolidation may be biasing our results. In column 7 and 8, we include the growth rates of tax revenue and expenditures from time $t-1$ to t as additional regressors. Although the fiscal policy variables are significant, the estimated effects of a fiscal consolidation continue to be contractionary. However, these fiscal policy changes are endogenous which we address in the next section.

4.6 Compositional Effects of Fiscal Consolidations

We next examine the impact of the composition of a fiscal consolidation on real state income. In the cross-country literature, Alesina and Ardagna (2010, 2013), Alesina, Favero and Giavazzi (2015a) and Alesina, Barbiero, Favero, Giavazzi and Paradisi (2015b) find that fiscal consolidations resulting from spending cuts are associated with slightly higher real GDP, while those resulting from revenue increases are associated with decreases in real GDP. At the same time, IMF (2010) and Guajardo et al. (2014) find that both spending- and revenue-based fiscal consolidations are associated with decreases in growth, although more contained for spending-based consolidations.

To estimate the compositional effect of fiscal consolidation, we repeat our estimation procedure using separate revenue-based and spending-based fiscal consolidation measures

$$\Delta y_{s,t} = \alpha + \sum_{j=1}^{2} \delta_j \Delta y_{s,t-j} + \sum_{j=0}^{2} \alpha_j \Delta RBFC_{s,t-j} + \sum_{j=0}^{2} \gamma_j \Delta SBFC_{s,t-j} + \mu_s + \lambda_t + \nu_{s,t}, \qquad (7)$$

where $\Delta RBFC$ and $\Delta SBFC$ are the changes in the revenue-based and spending-based portions of the CAPB during periods of fiscal consolidation. We demarcate the revenue vs̀spending portions of the fiscal consolidation in two separate ways. Following Alesina and Ardagna (2010, 2013) (AA), we split the $\Delta CAPB^{FC}$ between $\Delta RBFC$ and $\Delta SBFC$ using the percentage attributed to each component. We also follow Guajardo et al. (2011) (IMF) and have one

compositional measure equal $\Delta CAPB^{FC}$ and the other compositional measure equal 0 depending upon which component contributed the majority of $\Delta CAPB^{FC}$. Appendix D details the construction of the composition variables.

The last four rows of Table 1 show the summary statistics of our budget composition measures. As with $\Delta CAPB^{FC}$, there are 103 non-zero values of $\Delta RBFC$ and $\Delta SBFC$ under Census and 49 under NASBO. Of these, the majority of the fiscal consolidations are attributed to revenue increases. For the AA compositional measures, the mean value of $\Delta RBFC^{AA}$ is greater than $\Delta SBFC^{AA}$ in both samples. Likewise, the number of $\Delta RBFC^{IMF}$ exceeds the number of $\Delta SBFC^{IMF}$ in both samples.

Table 7 presents the 2SLS results for the compositional effects of a fiscal consolidation. To help explain the choice of revenue increases and spending cuts in a fiscal consolidation, we expand our instrument set to include the interaction of the twice-lagged budget deficit with dummies for line-item veto, supermajority tax rules, no income or sales tax, plus the interaction of the twice-lagged budget deficit and surplus with Tax and Expenditure Limits (TEL) laws (Knight and Levinson, 2000). The specification test results indicate that our instruments are exogenous but relatively weak.

The results show that revenue-based fiscal consolidations are contractionary. In each column, the contemporaneous and long-run coefficients for $\Delta RBFC$ are negative and statistically significant. The point estimates indicate that a one percent increase in $\Delta RBFC$ leads to a $5.0 - 6.1$ percent decrease in long-run real income. The evidence on spending-based fiscal consolidations is mixed. For the Census data, we estimate a negative impact with smaller (in absolute value) coefficients relative to revenue-based fiscal consolidation. Our estimates using NASBO are insignificant. Taken together, our results support the findings of Alberto Alesina and his co-authors that fiscal consolidations driven by revenue increases are more harmful than those led by spending cuts.

5 The Case of Fiscal Adjustments in Hawaii

In this section, we take a step toward the narrative approach by examining the case of Hawaii using state government documentation and media reports to see whether their fiscal consolidation episodes were intentional fiscal adjustment policy actions. We consider the case of Hawaii

because it is the state with the most consolidations, each occurring in the second half of our sample where supporting documentation is easily available. Using the original thresholds of 1.0 and 0.6 percent, Hawaii experienced fiscal consolidations in both data sets in fiscal years (FY) 1996, 2010, 2013. Fiscal years 2005 and 2016 also emerge as consolidation episodes using the Census data but fail to do so for NASBO.

The state of Hawaii has a biennial budget process. The governor submits an Executive Biennium Budget to the legislature in December of an odd-numbered fiscal year[10] where proposed expenditures and anticipated revenues for the ensuing fiscal biennium are detailed. An Executive Supplemental Budget is then submitted in December of the following even-numbered year. For example, the Executive Biennium Budget of 2015-2017 was submitted in December 2014 during $FY2015$ and laid out expenditure and revenue projections for $FY2016$ and $FY2017$. The Executive Supplemental Budget of 2017 was then submitted in December 2015 with proposed tax and expenditure additions for $FY2017$. The constitution of Hawaii establishes a general fund expenditure limit where the ceiling is adjusted by the average annual percentage change in state personal income for the three calendar years immediately preceding. The expenditure ceiling is thus pro-cyclical – higher following periods with growth but lower following recessions. The constitution also sets a debt limit equal to 18.5% of the average net general fund revenues of the three preceding years. General obligation bonds may be issued providing that such bonds do not cause the total amount of principal and interest payable in the current or any future fiscal year, whichever is higher, to exceed the debt limit.

5.1 The Consolidation of 1996

Starting in 1993, Hawaii suffered a severe economic downturn due to dwindling tourism, decreasing construction and contractions in major industries such as cane sugar. The $FY1995$ budget worsened substantially relative to its appropriated counterpart and a large deficit materialized. The expenditure ceiling and the debt limit were reduced in $FY1996$ as a result of three consecutive years of recession. The supplemental budget for $FY1997$ submitted in December 1996 enacted a large fiscal adjustment primarily through cuts in government spending, as documented in the *The Fiscal Survey of States, Fall 1996*. The supplemental budget called for

[10]The fiscal year runs from July 1 to June 30.

layoffs and furloughs of state employees and other cuts across the board for several programs. The state budget improved significantly from $FY1995$ to $FY1996$, moving from a large deficit to a sizable surplus.[11] Hence, the state fiscal response to the economic downturn of 1993 to 1996 was pro-cyclical in nature.

Figure 3 shows the fiscal measure $\Delta CAPB$ for Census and NASBO on the left axis and the growth of real personal income net of transfers for Hawaii on the right axis; fiscal variables are plotted for fiscal years while income growth is for calendar years. The solid black vertical lines indicate the fiscal adjustment episodes and the dot-dash horizontal lines indicate the threshold for adjustment for Census (1.0%) and NASBO (0.6%). The fiscal consolidation of 1996 came after three consecutive years of falling real personal income: -1.1% in 1993, -2.0% in 1994 and -2.5% in 1995. The 3.5% drop in 2016 was the biggest decrease in personal income for Hawaii since the 1981-82 recession.

Figure 4 displays the growth rates of real general expenditures and real general revenues for both Census and NASBO data. General revenues in Hawaii strongly co-move with income; expenditures are also pro-cyclical to a lesser degree.[12] More importantly, the fiscal adjustment described above is reflected in both data sources. In $FY1996$, general revenues increase by 3.0% in Census and rise by 5.0% in NASBO, while general expenditures fall by 5.0% in Census and by 3.8% in NASBO.

5.2 The Consolidation of 2010

The $FY2010$ budget of Hawaii was in deficit before it began on July 2009. From March 2008 through August 2009, the Council on Revenues – the state agency charged with forecasting tax revenues – projected that Hawaii would have nearly $3 billion less revenue than anticipated through the end of June 2011. In September 2009, Republican Governor Lingle announced that spending would be reduced by $2 billion through several measures. Nevertheless, the state still faced a $496 million shortfall in the following nine months and an additional $529 million in $FY2011$. The loss in tax revenues during the fiscal biennium of the Great Recession would eventually exceed $1 billion. In August and September 2009, the state of Hawaii laid off approx-

[11] The budget (not cyclically-adjusted) improved by 135% under NASBO and 156% under Census.

[12] The contemporaneous correlation of revenue growth and income growth is 0.30 and 0.34 and that of expenditure growth and income growth is 0.19 and 0.33 under Census and NASBO, respectively. The correlation with lagged income growth is 0.54 and 0.51 for revenues and 0.2 and 0.45 for expenditures.

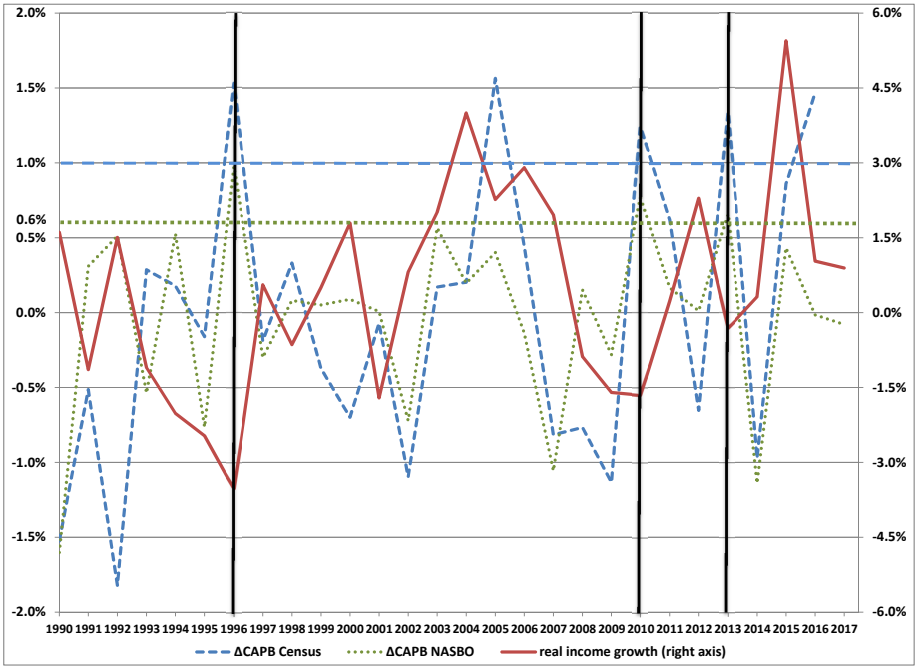

Figure 3: Fiscal Consolidations and Personal Income in Hawaii

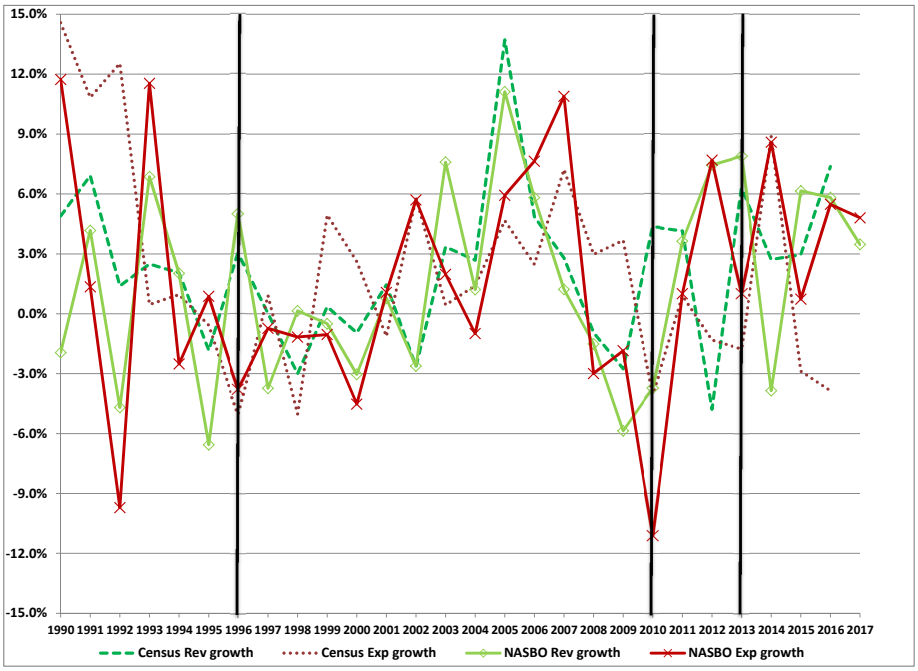

Figure 4: Government Expenditure and Revenue Growth in Hawaii

imately 1,100 state employees and furloughed an additional 900 state employees (Ballotpedia, 2010). In October 2009, the largest public employee union – the Hawaii Government Employees Association – ratified a new contract with 42 furlough days over the next two years, cutting salaries by approximately 8%. In addition, the state also enacted an increase in personal income and cigarettes/tobacco tax.

Hawaii's fiscal response to the Great Recession was typical of U.S. states. Jonas (2012) documents that several states tightened their policies during the Great Recession due to the institutional constraints of state budgeting. Hawaii experienced a milder downturn relative to the rest of the country with real personal income falling by a cumulated 3% in 2008 and 2009 compared to 8% for the nation. However, Hawaii cut government spending sharply (4% in Census and 11.1% in NASBO for $FY2010$) and attempted to increase revenue (8.5% in Census for $FY2010-11$ and 11.1% in NASBO for $FY2011-12$). In response, income growth in Hawaii stalled in 2010-13 while that of the United States rebounded quickly.

5.3 The Consolidation of 2013

In early 2012, tax revenue projections for $FY2013$ were lowered and a budget shortfall of $19 billion was anticipated. The original $FY2011$-13 state budget was amended. Democratic Governor Abercrombie implemented cost-cutting measures of $20 million, including savings from contract talks between the public sector labor union and the adminstration amounting to a 5 percent pay cut. While the Governor did not raise taxes in his proposed supplemental budget, state legislators passed a final version that relied heavily on tax increases on businesses, vehicles and large incomes, which raised more than $600 million over the next two years (Ballotpedia, 2012).

In Figure 3, real income growth went from a positive 2.3% in 2012 to slightly negative in 2013. Due to the tax increases, revenue growth rose sharply by 6.3% for Census and 8.0% in NASBO. As a result, a revenue-based fiscal consolidation occurred and the state budget balance improved significantly during the $FY2013$ relative to the previous fiscal year.

5.4 The Other Census Episodes

There are two additional fiscal consolidations according to the Census data: $FY2005$ and 2016. The 2001 recession hit the state of Hawaii particularly hard. The state Council on Revenues forecasted low revenue growth in the following fiscal years and, against this background, the state took actions to hold expenditures under control. The Hawaiian economy however rebounded in $FY2002$ and showed continuing improvements in $FY2003$-04 with income growth reaching 4%, its highest value since the late 1980s. As personal income grew, revenues kept increasing and official revenue estimates were revised upward. Moreover, the state enacted a number of revenue measures such as increases in fees and charges and a change in income tax withholding remittance date that further improved the state budget – see *The Fiscal Survey of States, Fall 2004*. As a result, revenue growth rose dramatically in $FY2005$ (see Figure 3) and the budget balance improved: Census CAPB went from -0.1% in $FY2004$ to 1.45% in $FY2005$. The $CAPB$ recorded by NASBO improved but only by 0.6%. Figure 4 reveals that both revenues and expenditures rose strongly in $FY2005$; however revenues grew faster in Census relative to NASBO and vice versa for expenditures. On the whole, $FY2005$ does not look like an intended fiscal consolidation. The evidence points to an unanticipated improvement in revenues in the context of an already-approved biennial budget. We could not find reference to an explicit discretionary effort to undertake an adjustment. Interestingly, real income growth was nearly halved, going from 4.0% in 2004 to 2.2% in 2005.

The episode of $FY2013$ is again characterized by strong revenue growth (8%) and negative expenditure growth (-4%). The Council on Revenues underestimated fiscal revenues; expecting low revenues, the state government placed 10% restrictions on discretionary general fund spending across all state departments for $FY2016$. Later on some of these funds were released and given back to departments that demonstrated the need for resources. NASBO does not record an episode for 2016; revenue dynamics are similar but government expenditures increase for NASBO but fall for Census.

Our methodology appears to better identify fiscal consolidations in the NASBO data than in the Census. Census includes outlays and revenues over which the state has no control; depending on how these budget items are correlated with income, Census budget balance will over- or under-estimate the intended discretionary budget balance. The larger variance of

$\Delta CAPB$ under Census relative to NASBO further confirms this intuition. These differences between the fiscal data source confirms our approach of doing the analysis separately.

6 Conclusions

This paper used the experience of U.S. states to estimate the impact of fiscal consolidation on short-run economic growth. We use the presence and variation of balanced-budget rules to identify the effects of fiscal consolidation. We find that fiscal consolidations reduce real income per person by a cumulative 4.5 to 5.5 percent. Our estimates are robust to two fiscal data sources, the value of the fiscal consolidation threshold and the choice of instruments. With regards to the composition, we find that revenue-based fiscal consolidations lower real income by around 5.0 percent and are more contractionary than spending-based consolidations.

Our estimated contractionary effects of a fiscal consolidation are greater than those obtained in the related cross-country literature;[13] and also larger than the income multipliers found in cross-state analysis.[14] This increase in magnitude should not be surprising for several reasons. First, open-economy relative multipliers can differ substantially from closed-economy aggregate multipliers (Nakamura and Steinsson, 2014). In particular, the cross-country literature has found that fiscal consolidations are almost three times more contractionary if carried out under a fixed exchange rate regime (see Guajardo et al., 2011) and government spending multipliers are larger for fixed exchange rate economies (see Ilzetzki, Mendoza and Végh, 2013). Second, state income multipliers are estimates of positive and negative government spending (or tax) shocks during all time periods, while those for fiscal consolidation are estimates of large negative spending (and positive tax) shocks. In addition, income multipliers for revenue-based consolidations are larger than spending-based ones, which raises the impact of our revenue-based consolidations. Third, our state income measure is before federal transfers and taxes, which are significant for U.S. states. Sachs and Sala-i Martin (1992) estimate that a one dollar reduction in a state's per capita income leads to an increase in federal transfers and a decrees in federal

[13]The cumulative effects of fiscal adjustment on real GDP in cross-country studies range from a positive 0.37 (Alesina and Ardagna, 2010) to a negative 1.57% within two years (Guajardo et al., 2014). For revenue-based fiscal adjustments, the cumulative effects range from negative 2% (Alesina et al., 2015b) after three years (for Canada), to negative 3% after 10 quarters (Romer and Romer, 2010) and negative 3.1% (Guajardo et al., 2014) after two years. For spending-based fiscal adjustments, estimates range between positive (Alesina and Ardagna, 2010) to negative 1% (Guajardo et al., 2014).

[14]Ramey (2011a) reports that income multipliers are 1.5 to 3.0 using state expenditures.

taxes of about 40 cents. Therefore our estimated impact of fiscal consolidation on state per capita income is higher than its disposable income counterpart. Fourth, income multipliers are asymmetric. Barnichon and Matthes (2016) find that the multiplier associated with a negative spending is much larger than one, but far below one for a positive spending shock. Similarly, Auerbach and Gorodnichenko (2012) find that multipliers are larger in recessions. In our sample, a fiscal consolidation recorded in a FY is associated with lower than average growth in the first calender year and higher than average growth than the second calender year. However, these growth differences are significant in the Census data but not for the NASBO data.

Although our point estimates may not be directly comparable to the cross-country estimates, the sign of the relationship does have important implications for countries in general and those in monetary unions in particular. U.S. states are often viewed as a well-functioning monetary union where adjustments occurs through internal migration, wage adjustments and federal transfers (see Blanchard and Katz, 1992). Our results suggest that these internal adjustments are far from sufficient when faced with a fiscal consolidation. The implications therefore are that euro zone members with less internal adjustment mechanisms available are likely to contract from a consolidation.

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A Data Definitions and Sources

Variable	Definition	Data Source
Δy	change in log of real personal income minus transfer payments per person	see note
$\Delta CAPB$	change in the cyclically-adjusted (primary) budget balance as share of income	Census/NASBO
$FC\ dummy$	(0,1) indicator of a fiscal consolidation where $\Delta CAPB$ is greater than threshold value	Census/NASBO
$\Delta CAPB^{FC}$	change in the cyclically-adjusted (primary) budget balance as share of income in periods of fiscal consolidation	Census/NASBO
$\Delta CAPB^{NFC}$	change in the cyclically-adjusted (primary) budget balance as share of income in normal periods of no fiscal consolidation	Census/NASBO
$\Delta SBFC^{IMF}$	change in the spending-based cyclically-adjusted (primary) budget balance using IMF criteria	Census/NASBO
$\Delta RBFC^{IMF}$	change in the revenue-based cyclically-adjusted (primary) budget balance using IMF criteria	Census/NASBO
$\Delta SBFC^{AA}$	change in the spending-based cyclically-adjusted (primary) budget balance using	Census/NASBO
$\Delta RBFC^{AA}$	change in the revenue-based cyclically-adjusted (primary) budget balance using AA criteria	Census/NASBO
$budgetdeficit$	budget deficit as share of income (positive value) and zero otherwise	Census
$budgetsurplus$	budget surplus as share of income and zero otherwise	Census
$weakBBR$	$(0,1)$ indicator for a weak balanced budget requirement	Clemens and Miran
$budgetdeficit \times weakBBR$	interaction of budget deficit as share of income and weak BBR	Census
$lineitem$	$(0,1)$ indicator for a budgetary line-item veto	Knight and Levinson
$supermajority$	$(0,1)$ indicator for a supermajority tax increase law	Knight and Levinson
$notax$	$(0,1)$ indicator for a state with no income tax or sales tax	Census
$budgetdeficit \times lineitem$	interaction of budget deficit as share of income and budgetary line-item veto	Census
$budgetdeficit \times supermajority$	interaction of budget deficit as share of income and supermajority tax increase law	Census
$budgetdeficit \times notax$	interaction of budget deficit as share of income and no income or sales tax state indicator	Census
$budgetdeficit \times TELtax$	interaction of budget deficit as share of income and TEL tax rules	Knight and Levinson
$budgetsurplus \times TELtax$	interaction of budget surplus as share of income and TEL tax rules	Knight and Levinson
$budgetdeficit \times TELspend$	interaction of budget deficit as share of income and TEL expenditure rules	Knight and Levinson
$budgetsurplus \times TELspend$	interaction of budget surplus as share of income and TEL expenditure rules	Knight and Levinson

Note: The dependent variable is computed as nominal personal income minus transfers deflated by the regional CPI and then divided by state population. These data were taken from the BEA *Regional Accounts*, BLS *Consumer Price Indices*, and the Census *Population Estimates*. The Census data is from *State Government Finances* and the NASBO data is from *The Fiscal Survey of the States.* The IMF and AA criteria are described in Appendix D.. The data from Clemens and Miran (2012) and Knight and Levinson (2000) are from the papers listed in the References.

B Cyclical Adjustment

We follow the EU, IMF and OECD approach, detailed in European Community (1995), of estimating the elasticities of selected categories of revenue and expenditure with respect to output. We first apply a Hodrick-Prescott (HP) filter to each revenue, $R_{s,t}$, and expenditure, $X_{s,t}$, series and also to real personal income, $Y_{s,t}$, to generate a trend and cyclical component for each state. We then estimate the following model to generate the cyclical elasticity of each category of revenue and expenditure:

$$r^c_{s,t} = e\, y^c_{s,t}, \tag{8}$$

where $r^c_{s,t} = \ln R_{s,t} - \ln R^*_{s,t}$, $y^c_t = \ln Y_{s,t} - \ln Y^*_{s,t}$, e is the elasticity measure, and starred variables are HP trends. The estimated elasticities, $\widehat{e}$, are then used to adjust each fiscal category

$$R^A_{s,t} = R_{s,t} \exp(-\widehat{e}\, y^c_{s,t}).$$

We compute the cyclically-adjusted primary budget balance by adding up the cyclically-adjusted revenue categories, subtracting out the cyclically-adjusted expenditure categories (excluding interest payments for Census). Lastly, we divide the cyclically-adjusted primary budget balance by trend income in the same period and then take the difference relative to the previous period to obtain the change in the cyclically-adjusted primary budget balance ratio, $\Delta CAPB$.

C States Classification

We classify U.S. states using the Advisory Commission on Intergovernmental Relations (1987) 1-10 index. Weak rule states have an index less than 5; medium rule states have an index between 5 and (less than) 6; strong rule states have an index greater or equal to 6. Unlike Clemens and Miran (2012), we include all 49 states independently of whether the budget cycle is annual or biennial. Table **??** reports the classification.

State Classification

Weak rules	Medium rules	Strong rules			
Connecticut	California	Alabama	Indiana	Nebraska	South Carolina
Illinois	Maryland	Arizona	Iowa	New Jersey	South Dakota
Louisiana	Michigan	Arkansas	Kansas	New Mexico	Tennessee
Massachusetts	Pennsylvania	Colorado	Kentucky	North Caroline	Texas
Nevada	Wisconsin	Delaware	Maine	North Dakota	Utah
New Hampshire		Florida	Minnesota	Ohio	Virginia
New York		Georgia	Mississippi	Oklahoma	Washington
Vermont		Hawaii	Missouri	Oregon	West Virginia
		Idaho	Montana	Rhode Island	Wyoming

D Spending- and Revenue-based Variables

The AA compositional measures are

$$\Delta RBFC = \frac{\Delta REV}{\Delta CAPB^{FC}} \times \Delta CAPB^{FC}$$

$$\Delta SBFC = -\frac{\Delta SPEND}{\Delta CAPB^{FC}} \times \Delta CAPB^{FC},$$

where ΔREV is the change in tax revenues to GDP ratio, $\Delta SPEND$ is the change in current spending to GDP ratio and $\Delta REV - \Delta SPEND = \Delta CAPB$.

The IMF compositional measures are

$$\Delta RBFC = \Delta CAPB^{FC} \text{ if } \frac{\Delta REV}{\Delta CAPB^{FC}} > 0.50 \text{ and } 0 \text{ otherwise}$$

$$\Delta SBFC = \Delta CAPB^{FC} \text{ if } \frac{\Delta SPEND}{\Delta CAPB^{FC}} > 0.50 \text{ and } 0 \text{ otherwise}$$

where $\Delta REV - \Delta SPEND = \Delta CAPB^{FC}$.

Tables

Table 1: Summary Statistics of Fiscal Consolidation Measures

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
		Census			
$\Delta CAPB$	0.00001	0.00611	-0.04638	0.03258	2254
$FC\ dummy$	0.04570	0.20877	0	1	2254
$\Delta CAPB^{FC}$	0.01504	0.00491	0.01000	0.03258	103
$\Delta CAPB^{NFC}$	-0.00071	0.00517	-0.04638	0.00997	2151
$\Delta RBFC^{AA}$	0.00833	0.00784	-0.00836	0.03942	103
$\Delta SBFC^{AA}$	0.00657	0.00763	-0.01091	0.02691	103
$\Delta RBFC^{IMF}$	0.01458	0.00489	0.01000	0.03258	59
$\Delta SBFC^{IMF}$	0.01567	0.00492	0.01011	0.02827	44
		NASBO			
$\Delta CAPB$	-0.00001	0.00328	-0.01856	0.02534	1813
$FC\ dummy$	0.04633	0.21026	0	1	1813
$\Delta CAPB^{FC}$	0.00971	0.00510	0.00601	0.02534	49
$\Delta CAPB^{NFC}$	-0.00028	0.00277	-0.01856	0.00596	1764
$\Delta RBFC^{AA}$	0.00531	0.00678	-0.00969	0.02213	49
$\Delta SBFC^{AA}$	0.00434	0.00638	-0.01141	0.02131	49
$\Delta RBFC^{IMF}$	0.00987	0.00483	0.00648	0.02419	27
$\Delta SBFC^{IMF}$	0.00953	0.00551	0.00601	0.02534	22

Table 2: Estimated Effects of Fiscal Consolidation using Census Data

	OLS	*Panel A: 2SLS estimates*			
ESTIMATED IMPACT	(1)	(2)	(3)	(4)	(5)
Cumulative effect of FC at t	0.143	-4.147	-4.098	-4.168	-4.067
	(0.179)	(2.173)	(1.779)	(1.735)	(1.752)
Cumulative effect of FC at $t+1$	0.202	-4.722	-4.667	-4.747	-4.631
	(0.224)	(2.465)	(1.930)	(1.919)	(1.945)
Cumulative effect of FC at $t+2$	0.243	-5.078	-5.020	-5.105	-4.982
	(0.305)	(2.685)	(2.030)	(2.067)	(2.099)
LR Cumulative effect of FC	0.264	-5.117	-5.060	-5.142	-5.022
	(0.350)	(2.628)	(2.080)	(2.070)	(2.100)
		Panel B: First-stage estimates			
$budget\,deficit_{s,t-2} \times weakBBR_s$		-0.225		-0.152	-0.167
		(0.074)		(0.079)	(0.079)
$budget\,surplus_{s,t-2} \times weakBBR_s$		-0.026		-0.034	
		(0.029)		(0.039)	
$budget\,deficit_{s,t-2}$			0.118	0.087	0.086
			(0.034)	(0.037)	(0.033)
$budget\,surplus_{s,t-2}$			0.001	0.001	
			(0.022)	(0.024)	
Cragg-Donald F-statistic		11.794	12.265	8.766	17.341
Overidentification p-value		0.323	0.764	0.690	0.977
AR(2) p-value	0.710	0.776	0.807	0.611	0.749
Observations	2,156	2,156	2,156	2,156	2,156
Number of states	49	49	49	49	49
Time period	1973-2016	1973-2016	1973-2016	1973-2016	1973-2016
Fixed state and year effects	yes	yes	yes	yes	yes

Note: The dependent variable is the annual growth rate of real per capita income with transfers removed. Each column includes two lagged growth rates and state and time effects whose coefficients are not shown. The robust standard errors clustered on each state are in parentheses. The OLS are the least square estimates of the accumulated impact on real income calculating using the delta method. Panel A are the 2SLS estimates of the accumulated impact on real income instrumenting contemporaneous fiscal consolidation with the instruments shown immediately below. Panel B presents the first-stage estimates of the excluded instruments used in the regression above. The Cragg-Donald F-statistic is a test of the strength of the excluded instruments. The Hansen overidentification test is the p-value of the null of exogeneity of the instruments. The Cumby-Huizinga AR(2) test is the p-value of the null of second-order autocorrelation.

Table 3: Estimated Effects of Fiscal Consolidation using NASBO Data

	OLS	*Panel A: 2SLS estimates*			
ESTIMATED IMPACT	(1)	(2)	(3)	(4)	(5)
Cumulative effect of FC at t	-0.062	-2.170	-5.991	-3.222	-3.265
	(0.284)	(0.772)	(3.254)	(1.511)	(1.521)
Cumulative effect of FC at $t+1$	0.349	-2.279	-7.032	-3.590	-3.643
	(0.758)	(0.882)	(3.700)	(1.635)	(1.635)
Cumulative effect of FC at $t+2$	-0.487	-3.552	-8.966	-5.047	-5.107
	(1.004)	(1.135)	(4.181)	(2.025)	(2.026)
LR Cumulative effect of FC	-0.669	-3.888	-9.089	-5.369	-5.428
	(1.139)	(1.257)	(4.318)	(2.251)	(2.259)
		Panel B: First-stage estimates			
$budget\,deficit_{s,t-2} \times weakBBR_s$		-0.562		-0.471	-0.466
		(0.226)		(0.224)	(0.224)
$budget\,surplus_{s,t-2} \times weakBBR_s$		0.026		0.017	
		(0.048)		(0.059)	
$budget\,deficit_{s,t-2}$			0.225	0.120	0.122
			(0.105)	(0.077)	(0.085)
$budget\,surplus_{s,t-2}$			0.017	-0.004	
			(0.030)	(0.032)	
Cragg-Donald F-statistic		37.470	20.217	21.485	42.994
Overidentification p-value		0.319	0.711	0.268	0.139
AR(2) p-value	0.675	0.602	0.534	0.593	0.587
Observations	1,715	1,715	1,715	1,715	1,715
Number of states	49	49	49	49	49
Time period	1983-2017	1983-2017	1983-2017	1983-2017	1983-2017
Fixed state and year effects	yes	yes	yes	yes	yes

Note: The dependent variable is the annual growth rate of real per capita income with transfers removed. Each column includes two lagged growth rates and state and time effects whose coefficients are not shown. The robust standard errors clustered on each state are in parentheses. The OLS are the least square estimates of the accumulated impact on real income calculating using the delta method. Panel A are the 2SLS estimates of the accumulated impact on real income instrumenting contemporaneous fiscal consolidation with the instruments shown immediately below. Panel B presents the first-stage estimates of the excluded instruments used in the regression above. The Cragg-Donald F-statistic is a test of the strength of the excluded instruments. The Hansen overidentification test is the p-value of the null of exogeneity of the instruments. The Cumby-Huizinga AR(2) test is the p-value of the null of second-order autocorrelation.

Table 4: Robustness of Fiscal Consolidation Effects to Threshold Values using Census Data

THRESHOLD	none	0.50 %	1.00 %	1.40 %	0.50 %	1.00 %	1.40 %
			Panel A: 2SLS estimates				
LR Cumulative effect of FC	-3.097	-3.383	-5.022	-7.203	-4.117	-5.426	-6.994
	(1.248)	(1.398)	(2.100)	(2.568)	(1.683)	(2.471)	(2.474)
LR Cumulative effect of NFC					1.246	0.244	-0.041
					(1.013)	(0.853)	(0.892)
Cragg-Donald F-statistic	44.731	36.18	17.341	11.149	6.861	5.343	4.445
Overidentification p-value	0.403	0.577	0.977	0.545	0.901	0.558	0.784
AR(2) p-value	0.784	0.722	0.749	0.851	0.865	0.848	0.592
			Panel B: OLS estimates				
LR Cumulative effect of FC	0.756	0.357	0.264	0.346	0.639	0.693	0.802
	(0.239)	(0.223)	(0.350)	(0.328)	(0.214)	(0.293)	(0.343)
LR Cumulative effect of NFC					1.372	1.176	1.102
					(0.356)	(0.291)	(0.240)
Observations	2,156	2,156	2,156	2,156	2,156	2,156	2,156
Number of states	49	49	49	49	49	49	49
Time period	1973-2016	1973-2016	1973-2016	1973-2016	1973-2016	1973-2016	1973-2016
Fixed state and year effects	yes	yes	yes	yes	yes	yes	yes

Note: The LR Cumulative effect of FC (NFC) reports the long-run cumulative estimated response of a one percentage point increase in the cyclically-adjusted primary budget during a fiscal consolidation (non-consolidation) on the level of real income per person. The robust standard errors computed via the delta method are in parentheses. Panel A presents the second-stage 2SLS results where twice-lagged budget deficit and its interaction with a weak rule dummy variable are used as instruments in columns 1-4 and twice-lagged budget deficit and surplus and their interactions with a weak rule dummy are used as instruments in columns 5-7. Panel B presents the OLS results.

Table 5: Robustness of Fiscal Consolidation Effects to Threshold Values using NASBO Data

THRESHOLD	none	0.30 %	0.60 %	0.80 %	0.30 %	0.60 %	0.80 %
			Panel A: 2SLS estimates				
LR Cumulative effect of FC	-5.354	-5.661	-5.428	-5.901	-3.391	-3.624	-4.206
	(2.334)	(2.505)	(2.400)	(2.421)	(2.827)	(1.290)	(1.773)
LR Cumulative effect of NFC					1.060	0.403	0.451
					(2.721)	(2.032)	(2.223)
Cragg-Donald F-statistic	79.991	54.101	42.994	37.355	7.869	12.830	10.806
Overidentification p-value	0.378	0.230	0.139	0.164	0.644	0.639	0.652
AR(2) p-value	0.882	0.594	0.587	0.558	0.705	0.676	0.659
			Panel B: OLS estimates				
LR Cumulative effect of FC	0.199	-0.189	-0.807	-0.750	1.3397	0.891	0.870
	(1.090)	(0.984)	(0.874)	(0.769)	(1.377)	(1.373)	(1.278)
LR Cumulative effect of NFC					3.256	3.051	2.835
					(1.254)	(1.244)	(1.209)
Observations	1,715	1,715	1,715	1,715	1,715	1,715	1,715
Number of states	49	49	49	49	49	49	49
Time period	1983-2017	1983-2017	1983-2017	1983-2017	1983-2017	1983-2017	1983-2017
Fixed state and year effects	yes	yes	yes	yes	yes	yes	yes

Note: The LR Cumulative effect of FC (NFC) reports the long-run cumulative estimated response of a one percentage point increase in the cyclically-adjusted primary budget during a fiscal consolidation (non-consolidation) on the level of real income per person. The robust standard errors computed via the delta method are in parentheses . Panel A presents the second-stage 2SLS results where twice-lagged budget deficit and its interaction with a weak rule dummy variable are used as instruments in columns 1-4 and twice-lagged budget deficit and surplus and their interactions with weak-BBR are used as instruments in columns 5-7. Panel B presents the OLS results.

Table 6: Robustness of Fiscal Consolidation Effects to Instrument Selection

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: 2SLS estimates							
LR Cumulative effect of FC	-4.989	-3.997	-3.876	-7.999	-2.937	-5.462	-6.932	-6.123
	(2.679)	(1.233)	(1.687)	(4.329)	(1.827)	(2.680)	(3.012)	(2.473)
$budget\,deficit_{s,t-2}$					-0.199	0.341		
					(0.212)	(1.354)		
$tax\,growth_{s,t}$							2.406	1.484
							(1.221)	(0.672)
$spend\,growth_{s,t}$							-1.995	-1.461
							(0.791)	(0.707)
Cragg-Donald F-statistic	23.341	74.843	14.450	26.622	7.494	26.746	6.773	17.321
Overidentification p-value			0.270	0.420	0.700	0.406	0.241	0.300
	Panel B: First-stage estimates							
$budget\,deficit_{s,t-2}\times$	-0.236	-0.556			-0.233	-0.519	-0.142	-0.302
$weakBBR_s$	(0.074)	(0.224)			(0.072)	(0.168)	(0.059)	(0.118)
$budget\,deficit_{s,t-2}\times$			-0.149	-0.210	-0.186	-0.431	-0.068	-0.019
$lineitem_s$			(0.036)	(0.077)	(0.082)	(0.140)	(0.033)	(0.068)
$budget\,deficit_{s,t-2}\times$			0.104	-0.160	0.136	-0.079	0.108	-0.150
$supermajority_s$			(0.067)	(0.246)	(0.043)	(0.132)	(0.038)	(0.093)
$budget\,deficit_{s,t-2}$					-0.059	-0.294		
					(0.082)	(0.137)		
State budget source	Census	NASBO	Census	NASBO	Census	NASBO	Census	NASBO
Observations	2,156	1,715	2,156	1,715	2,156	1,715	2,156	1,715
Time period	1973-2016	1983-2017	1973-2016	1983-2017	1973-2016	1983-2017	1973-2016	1983-2017
Fixed state and year effects	yes	yes	yes	yes	yes	yes	yes	yes

Note: The LR Cumulative effect of FC reports the long-run cumulative estimated response of a one percentage point increase in the cyclically-adjusted primary budget during a fiscal consolidation on the level of real income per person. The robust standard errors computed via the delta method are in parentheses. Panel A are the 2SLS estimates of the accumulated impact on real income instrumenting contemporaneous fiscal consolidation with the instruments shown immediately below. Panel B presents the first-stage estimates of the excluded instruments used in the regression above.

Table 7: Compositional Effects of Fiscal Consolidation

VARIABLES	(1)	(2)	(3)	(4)
Cumulative effect of revenue-based FC at t	-4.084	-4.045	-2.971	-3.616
	(2.520)	(2.416)	(1.410)	(1.006)
Cumulative effect of revenue-based FC at $t+1$	-4.639	-4.579	-2.696	-3.587
	(2.859)	(2.6462	(1.156)	(0.801)
Cumulative effect of revenue-based FC at $t+2$	-5.338	-5.042	-4.464	-5.569
	(3.055)	(2.7678	(1.712)	(1.221)
LR Cumulative effect of revenue-based FC	-5.447	-5.146	-4.971	-6.141
	(3.078)	(2.812)	(2.129)	(1.541)
Cumulative effect of spend-based FC at t	-2.859	-2.924	4.144	3.902
	(1.501)	(1.594)	(4.331)	(4.672)
Cumulative effect of spend-based FC at $t+1$	-3.311	-3.330	4.867	4.793
	(1.634)	(1.751)	(5.320)	(5.819)
Cumulative effect of spend-based FC at $t+2$	-3.055	-3.407	4.869	4.590
	(1.770)	(1.975)	(6.127)	(6.542)
LR Cumulative effect of spend-based FC	-3.044	-3.454	4.870	4.514
	(1.826)	(2.045)	(6.335)	(6.658)
Cragg-Donald F-statistic	2.132	2.158	2.357	3.149
Overidentification p-value	0.093	0.117	0.512	0.754
Specification of composition	AA	IMF	AA	IMF
State budget source	Census	Census	NASBO	NASBO
Observations	2,156	2,156	1,715	1,715
Fixed state and year effects	YES	YES	YES	YES

Note: The revenue-based (spend-based) FC reports the cumulative estimated response of a one percentage point increase in the cyclically-adjusted primary budget during a revenue-based (spending-based) fiscal consolidation at time t, $t+1$, $t+2$ on the level of real income per person. The robust standard errors computed via the delta method are in parentheses. Each regression is run by 2SLS with fixed state and time effects. The instruments used are the twice-lagged values of the budget deficit and its interaction with weak-BBR, line-item veto, supermajority tax rules, no income or sales tax plus the interaction of the twice-lagged budget deficit and surplus with TEL tax and spending rules. The specification of composition are the Alesina and Ardagna (AA) and (IMF) methodology to apportion each fiscal consolidation to revenue increases and to spending cuts described in Appendix D.