Discussion of Beetsma et al.’s  
“The Confidence Channel of Fiscal Consolidation”

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Fiscal consolidation involves a retrenchment of government expenditures and/or the imposition of higher taxes often with the objective of reducing the share of government spending in GDP and/or of reducing the debt-to-GDP ratio to more acceptable levels. Fiscal consolidation is a recurring theme in policy circles. Although there are historical examples of successful fiscal consolidations such as the reduction in U.S. fiscal debt during the Clinton years, the mere fact that calls for fiscal consolidation resurface time and again suggests that governments find it hard to implement and sustain fiscal consolidations. Often it is only the threat of imminent economic collapse from events such as the Euro crisis that forces governments to tackle the challenge of fiscal reform. Even in the latter case, governments have shown great creativity in delaying or at least minimizing fiscal austerity.

How then did Clinton succeed in turning a sizeable U.S. budget deficit to a budget surplus? Feldstein (2001) suggests that the primary source for eliminating the budget deficits was higher tax revenues resulting from increases in economic growth rather than changes in tax policies or government expenditures. If there was a role for policy, then simply that Republicans in Congress blocked spending increases that would have dissipated the increased tax funds, while the president blocked Republican initiatives to cut taxes.

It is not hard to see why governments would be reluctant to reduce their expenditures, given that much of their power derives from being able to allocate these expenditures. It is also not surprising that there are limits to a government’s willingness to raise taxes. The concern is not only that tax increases are unpopular and may undermine the government’s success at the next election, but it is well understood that either form of fiscal consolidation involves a fiscal multiplier that may bring the economy closer to a recession. This is a particular concern when the economy is already weak at the time when calls for fiscal consolidation mount. How large this fiscal multiplier is and how its magnitude differs depending on the mix of expenditure reductions and tax increases is an active area of research.
From the conventional macroeconomic point of view, there is a trade-off between the economic pain inflicted by fiscal consolidation in the short run and its longer-run benefits. Traditional static textbook models of aggregate demand predict that a cut in government spending (or an increase in taxes) inevitably depresses real output. This view has been questioned by some researchers, however. For example, Bertola and Drazen (1993) show that a fiscal contraction may paradoxically generate an economic expansion when the public expects that a failure to contract now would be followed by an even more severe contraction later. Their point is that a cut in government spending may stimulate current private sector spending, if it causes expectations that future government spending and hence future taxes will be lower. If the increase in private spending more than offsets the reduction in government spending, real output actually increases. Somewhat more pointedly, one might argue that the economy could be stimulated by budget cuts, if these cuts help avoid an even larger fiscal crisis in the future. Indeed, there is an empirical literature dating back to the late 1990 that has suggested that austerity measures can generate expansionary effects on the economy, although there is some disagreement among researchers about whether this result may be believed. In sharp contrast, many EU governments in recent years have made the case that the growth of their economies is held back by fiscal austerity measures, rather than being helped along.

The study by Beetsma et al. reexamines this controversy. Rather than studying the response of private spending (or the response of the economy at large), they focus on the effects of fiscal reform on measures of consumer confidence and business confidence. The central question is whether fiscal reforms undermine confidence in the economy or strengthen it. Increased confidence in the economy may be viewed as a precondition for an increase in private spending without which a positive response of the economy to a budgetary tightening cannot be explained. In order to answer this question we need to identify exogenous variation in government spending and/or government taxes.

In the first part of the analysis, Beetsma et al. rely on a data set developed by De Vries (2011) that includes all measures aimed at reducing the government deficit in 17 OECD countries between 1978 and 2009 that represent a response to past policy decisions and economic conditions. This classification is based on an evaluation of official documents in conjunction with information about the institutions in each country. The approach employed by De Vries et al. (2011), known more generally as the narrative approach to identification, is
intended to make sure that these actions are not correlated with prospective economic conditions and can be used to infer to causal effects of fiscal consolidations on the economy. The fact that the sample ends in 2009 is an advantage in the current context in that otherwise the results may be driven by the unique experience of the Euro crisis.

As their starting point, the authors specify an annual panel regression model for all countries in their sample attempting to explain variation in consumer confidence. Consumer confidence is smoothed using an HP filter. It is not clear why this smoothing is required or how the results would be affected if one focused on changes in consumer confidence instead. The baseline regression model allows for country and year fixed effects, includes lagged dependent variables, and conditions on each country’s current economic growth, inflation, stock returns, long-term nominal interest rates, and changes in unemployment. Interest centers on the coefficients of a measure of unanticipated fiscal policy changes in the current period \((CS_u^t)\), a measure of anticipated fiscal policy changes in the current period \((CS_a^t)\), and \(CS_f^t\), which refers to the present value of all unanticipated future consolidations as of the current period. The baseline specification makes no distinction between expenditure and revenue based fiscal plans. All coefficients but the fixed effects are constrained to be the same across countries. It is clear that this assumption is helpful in making the model more parsimonious and perhaps necessary for obtaining any empirical results. It is less clear how realistic this assumption is.

The implicit premise is that a correlation between the fiscal regressors and consumer confidence (as reflected in nonzero coefficients of these regressors) implies a causal effect. The reason for focusing on the impact effect only presumably is that the effects occurring within the current year are what a policymaker would care about most. There are six distinct concerns with this approach.

One concern is that fiscal shocks usually refer to unanticipated changes in fiscal policy. If a change is anticipated it is not a shock and should not have causal effects. It is not clear how to interpret the coefficient on \(CS_a^t\) therefore. The authors hint at this point when stating that the implementation of anticipated measures should not affect confidence, because such effects should have occurred already at the moment the measure was first announced. Of course, by this logic it is not clear why this regressor was included at all.
Another concern is that after aggregating the data to annual frequency there is plenty of room for two-way causality in the model, even when there is none at monthly frequency. The only reason for relying on annual data is presumably that the macroeconomic aggregates that the panel regression conditions on are not available at monthly frequency. It may be possible to ameliorate these concerns by switching to quarterly rather than annual data. My suggestion would be more radical. It seems to me that these so-called controls are redundant because the fiscal shocks were explicitly constructed not to reflect current economic conditions and expected future conditions. Thus, there is no need to include any of these macroeconomic aggregates and we could return to monthly specifications of the model without loss of generality.

One could of course make the case that the effects of fiscal consolidation on consumer confidence are nonlinear and depend on the state of the economy. This would provide a rationale for including additional macroeconomic aggregates in the regression model. The problem is that the current panel specification does not allow for this type of nonlinearity. A crude way of allowing for such nonlinearities would have been to interact the coefficients on the fiscal consolidation variables in the panel model with the variables capturing the state of the economy.

A related issue that also could have been avoided by using monthly data is how to aggregate monthly fiscal shocks to annual frequency. There is no explicit discussion of this point in the paper. For example, it is not clear how to convert an unanticipated shock measured at monthly frequency to a shock measured at annual frequency. Intuitively, it matters at what time within the year the unanticipated shock occurs. It also matters how many unanticipated fiscal shocks there are within a given year. There is also the issue of how to define the information set relative to which this shock is defined. A very similar problem arises in measuring monthly monetary policy shocks using changes in daily interest rate futures data at dates of policy shifts (e.g., D’Amico and Farka 2011).

Moreover, the fact that $CS_{it}^a$, $CS_{it}^p$ and $CS_{it}^u$ are correlated means that the individual coefficients for each of these regressors do not measure the marginal causal effect of each type of fiscal shock. Perhaps one could report instead by how much these three regressors jointly lower or raise consumer confidence, which would also provide a measure of the economic significance of these effects? Likewise, it is not clear why the authors report $t$-tests for $CS_{it}^a$, $CS_{it}^p$ and $CS_{it}^u$ rather than testing the joint significance of the regressors, given that these...
regressors are necessarily correlated. One concern is that a Wald test of joint significance is unable to discriminate between positive and negative effects on consumer confidence under the alternative hypothesis. This fact suggests that addressing the question of interest in a panel model setting is inherently difficult. A final concern is that the effects of fiscal consolidations are likely to last longer than one year, but the panel model allows only for impact effects.

For these reasons the estimates of this panel regression model should be taken with a grain of salt. The second part of Beetsma et al.’s study actually addresses some of these concerns. The authors create a new data set for the monthly dates of fiscal consolidations, extending the analysis of fiscal policy shifts in De Vries et al. (2011). This new data set then is used to conduct an event study. The event study approach simply involves comparing the level of consumer confidence for a given country before and after dates at which a fiscal policy shift takes place, possibly dropping some observations near the date of the policy change to control for leakage from policy announcements. This approach allows formal tests of the hypothesis that the change in consumer confidence is zero against the alternative that it is positive. The event study suggests that fiscal consolidations on average tend to be followed by declines in consumer confidence, although the decline is barely statistically significant. This preliminary evidence supports the position of many EU governments that fiscal retrenchment is contractionary and suggests that the countervailing forces discussed in Bertola and Drazen (1993) are not quantitatively important on average. This evidence does not, however, speak to the question of whether fiscal reform should be delayed, as maintained by many politicians.

The authors then proceed by dividing these fiscal events further into tax increases and expenditure reductions and repeating the event study. This additional classification is based on work by Alesina et al. (2014) who quantify the extent to which each fiscal event in De Vries et al. (2011) was predominantly based on reducing expenditures or based on raising government revenue. This exercise suggests that the earlier results were driven by tax increases. If we focus on tax increases only, fiscal consolidations are followed by statistically significant reductions in consumer confidence. If we only focus on reductions in spending, in contrast, fiscal consolidations tend to be followed by increases in consumer confidence, consistent with Bertola and Drazen (1993), although these increases are not distinguishable from zero at conventional significance levels. This result is consistent with the interpretation that only reductions in government spending are viewed as a signal that the government is serious and committed to
sustaining fiscal reforms, whereas tax increases are viewed as a signal that the government intends to conduct business-as-usual. It is important to keep in mind, however, that this analysis does not answer the question of whether the stimulus arising from reductions in government spending is strong enough to lift the entire economy, which is what ultimately matter to policymakers.

Beetsma et al. refine their classification of fiscal events even further in the remainder of the event study. While there is no important difference between spending-based and tax-based fiscal consolidations during an economic boom (both being associated with declining confidence in the economy), during recessions only tax increases are associated with lower confidence, while reductions in government spending are associated with increased confidence in the economy. This result is interesting as far as it seems to remove the rationale for delaying fiscal reform until the economic recovers. In fact, it suggests that there is advantage to such a delay and a possible disadvantage. It also would be useful to understand at least intuitively why this difference arises.

It also seems to matter whether the country in question is European or not. There is no evidence of fiscal consolidations being followed by declines in consumer confidence in other countries. In fact, outside of Europe both revenue-based and tax-based reforms tend to be followed by statistically insignificant increases in consumer confidence. Within Europe only revenue-based reforms have statistically insignificant positive effects on consumer confidence, while tax-based reforms have statistically significant negative effects. No results are provided that control for the state of the economy in addition.

With regard to the European countries specifically, there is evidence that without fiscal transparency and without tight fiscal rules, fiscal consolidation tends to be followed by declines in consumer confidence. In contrast, there is no evidence of a decline and sometimes there is evidence of an increase in confidence in other countries, especially countries with credible policy regimes. I found it hard to interpret these results because they do not make allowance for the state of the economy, which was earlier shown to be important in assessing the effects of fiscal reform.

The question is what these increasingly disaggregated results add to the paper. The authors suggest that examining the effectiveness of fiscal consolidations for consumer confidence, while conditioning on the state of the economy, on institutions and on political
conditions is helpful to policymakers in deciding when to undertake reforms. Even ignoring the fact that it may be hard to separate competing explanations if there is not enough variation across countries, I am not persuaded by this argument because most governments which pursue fiscal reform do so because they feel that they have no choice. For example, the timing of fiscal reform commonly is not a choice variable. For example, governments in countries affected by the Euro crisis cannot very well postpone fiscal reform until the economy expands again, although they may be able to drag their feet. Likewise, credibility and good institutions may be helpful in implementing fiscal reforms, but are hardly a choice variable for policymakers. A more interesting use of these disaggregate results might be to think about what they tell us about the design of theoretical models of the transmission of fiscal policy shocks.

Leaving aside these quibbles, does this event study resolve the controversy in the literature about the effects of fiscal consolidations? The answer is not quite. The reason is that event studies in general suffer from the limitation that they treat all events as being essentially the same. This premise is unrealistic in the current context, first, because one fiscal event may involve a small spending reduction and another event a large spending reduction, and, second, because the mix of spending reductions and tax increases will differ from one event to the next. In addition, each fiscal event involves a different mix of unanticipated and anticipated fiscal measures. As emphasized by Alesina et al. (2014), every fiscal consolidation involves a plan that combines different elements that are necessarily correlated and should not be thought of as mutually independent tax shocks or spending shocks. Plans consist of the unanticipated announcement of a sequence of fiscal actions, some to be implemented within the same year as the announcement and some to be implemented in the following years. There also are anticipated fiscal measures that were already announced previously. In fact, a fiscal plan may involve twice as many distinct components, once we differentiate between government expenditures and revenues.

If there were just one fiscal shock, the standard approach would be to trace out the causal effects of fiscal policy shocks by regressing monthly changes in consumer confidence on current and lagged values of the fiscal shock. The regression coefficients of this distributed lag model will capture the causal effects of unanticipated changes in policy, provided these changes are not systematically correlated with other variables over the sample in question. A natural generalization of this approach would be to simulate the path of consumer confidence first with
the vector of all the components of the fiscal shock and then again after setting this vector to zero. The difference between the implied paths of consumer confidence would represent the response to the fiscal plan. A closely related approach is also discussed in Alesina et al. (2014). Ideally, this approach should be applied to each country one at a time to allow for differences in the responses across country, although a case could be made for pooling the data in the interest of greater parsimony. Pooling indeed may be necessary, given the small number of fiscal consolidations in each country. Although such cross-country restrictions could be tested, an obvious concern in this context would be that such regressions do not allow for possible nonlinearities in the response of consumer confidence, depending on the state of the economy.

Where does this leave us? The authors clearly have made some progress in quantifying the response of consumer confidence to fiscal consolidations. The main substantive conclusions of this paper relate to the findings of an event study. This event study makes no allowance for differences in the composition of fiscal reforms across events, but it provides some intriguing tentative evidence about the relationship between fiscal austerity and consumer confidence. For now this may be all that we can learn from these data, given that the number of fiscal consolidations in the historical data is small, and the differences across countries and over time are numerous.

Additional references: