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# Identifying Discretionary Fiscal Policy Reactions with Real-Time Data

We propose a method of identifying discretionary fiscal policy reactions using real-time data. Automatic stabilizers should depend on true GDP, while discretionary fiscal policy is contingent on the information that policy makers have in real time. We can compute a real-time measurement error by comparing the first release of GDP data with later revisions. Discretionary fiscal policy is influenced by this measurement error, whereas automatic fiscal policy is not. We use this identification approach to test the central identifying assumption of Blanchard and Perotti's (2002) seminal structural vector autoregression (VAR). According to this assumption, fiscal policy makers do not react to GDP developments contemporaneously in a discretionary fashion. We find that government expenditure is adjusted upward if GDP growth in real time is lower than true GDP. This suggests that fiscal policy makers use short-term funds to buy goods and services in response to their perception of GDP dynamics.

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HOW DOES FISCAL policy respond to economic developments? While the working of automatic fiscal policy as a reaction to the state of the economy is well established, discretionary fiscal policy is usually treated as a residual variable about which we know relatively little. Taylor (2000), for example, specifies a fiscal policy rule in which the actual budget surplus is a function of the output gap. Taylor calls the part of the balance that is explained by the output gap "automatic stabilizers." The residual of this regression reflects, among other things, fiscal policy discretion.

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This distinction contrasts with statements from politicians, who aim to actively use fiscal policy to steer the macroeconomy and to address unwelcome developments. As a reaction to the current financial crisis, governments in all major economies claim to have taken an active role in stabilizing demand and output. If it is true that politicians respond with discretionary measures to the output gap, then the estimated reactions above would reflect not only automatic but also systematic discretionary fiscal policy.[1]

In this paper, we propose a method for identifying discretionary fiscal policy *reactions* to macro-economic fluctuations. We apply our identification scheme to test for contemporaneous fiscal policy discretion. We thereby distinguish systematic discretionary fiscal policy reactions from systematic automatic fiscal policy reactions to macro-economic shocks.

The basic idea behind our identification method relies on the fact that policy makers should react to the state of the economy as they observe it in real time. Discretionary fiscal policy is the result of conscious decisions based on the available information. In contrast, automatic fiscal policy reacts to the true state of the economy as it is defined by unemployment, income dynamics, and profits of corporate and noncorporate enterprises via legislation. GDP measurement errors should be irrelevant for automatic fiscal policy, which is based on micro-economic relationships. We argue that the true state of the economy can be approximated using the final GDP release, while the state of fiscal policy makers' macro-economic knowledge can be gauged using the published real-time GDP data. The difference between final GDP and real-time GDP is a variable that allows us to identify discretionary fiscal policy reactions.

A correct assessment of systematic discretionary fiscal reactions is very important for identifying the effects of fiscal policy on the macroeconomy. In the seminal paper by Blanchard and Perotti (2002), the structural VAR is identified by a crucial assumption: fiscal policy makers cannot react in a discretionary fashion to the state of the economy within the same quarter.[2] This is justified by the argument that fiscal policy decision making is a slow process, involving many agents in parliament, government, and civil society. Consequently, fiscal policy reactions to current developments only result from automatic responses, which are defined by existing laws and regulations. All fiscal policy developments in a given time period that do not reflect automatic responses are considered to be structural fiscal policy shocks exogenous to the macroeconomy. Accordingly, structural, that is, exogenous, fiscal policy shocks can be identified using elasticities computed on the basis of existing legislation, capturing the working of the automatic stabilizers.[3]

1. Blinder (2004) provides a historical overview of the change in public attitudes toward discretionary fiscal stabilization policy. Auerbach (2002) estimates whether the full employment surplus, which is calculated as a residual, reacts to the state of the macroeconomy.

2. A different strand of the literature uses major exogenous events, such as wars, to identify significant changes of fiscal policy stances (Ramey and Shapiro 1998, Edelberg, Eichenbaum, and Fisher 1999, Burnside, Eichenbaum, and Fisher 2004). Here, fiscal policy discretion (going to war) is explicitly identified by news reports.

3. This identification method has subsequently been employed in a large number of papers, for example, Perotti (2005) for OECD countries, de Castro Fernández and de Cos (2006) for Spain, Biau and Girard

We apply our identification approach to test the assumption of no contemporaneous fiscal policy discretion. Our test is based on the fact that only discretionary fiscal policy should react to GDP measurement errors. Accordingly, we estimate a reduced-form VAR, as in the first step of Blanchard and Perotti's (2002) estimation procedure but include the revision error as an exogenous explanatory variable in the expenditure equation. If expenditure reacts within the same quarter to the GDP revision error, policy makers are apparently willing and able to discretionarily change spending without long decision lags.

Our main finding is that fiscal policy makers react significantly and with discretion to the perceived state of the economy. When published GDP growth figures are lower than the true *ex post* figures, this will induce a positive contemporaneous deviation of government expenditure. This suggests that fiscal policy makers do indeed look at GDP as it is perceived in real time and are able to react contemporaneously. We thus provide evidence of contemporaneous discretionary fiscal policy reactions employing real-time data.

Following the pioneering work in the area of monetary economics by Orphanides (2001), real-time data have recently also been used to study fiscal policy. Forni and Momigliano (2005) show that, in a sample consisting of countries belonging to the Organisation for Economic Co-operation and Development (OECD), fiscal policy tends to react countercyclically when the information set is restricted to real-time data, while the response to *ex post* data is not statistically significant. Similarly, Cimadomo (2008) shows that fiscal policy makers in real time plan to be countercyclical but in *ex post* data are not. None of the papers, however, exploit the difference between initial and *ex post* data to identify fiscal policy discretion.

The remainder of the paper is organized as follows. The following section outlines our method of testing for contemporaneous fiscal policy discretion in the fiscal VAR context. Section 2 describes the data and how we deal with benchmark revisions. Benchmark revisions may involve major corrections due to redefinitions. As such, these redefinitions do not constitute new information. Section 3 presents the estimation results, and the final section concludes.

## 1. IDENTIFYING DISCRETIONARY FISCAL POLICY USING REAL-TIME DATA

Assume that the policy makers' decision rule regarding government expenditure can be written in the following way:

$$e_t = a_1(L)e_{t-1} + b(L)r_t + c_0 y_t + c_1(L)y_{t-1}, \tag{1}$$

(2005) for France, Giordano et al. (2007) for Italy, and Tenhofen, Wolff, and Heppke-Falk (2010) for Germany.

where $e_t$ is government expenditure, $r_t$ refers to revenue, and $y_t$ is real GDP at time $t$ and coefficients with bracketed lag operators $L$ indicate lag polynomials, for example, $b(L)r_t = b_o r_t + b_1 r_{t-1} + \cdots + b_k r_{t-k}$. Part of the reaction to changes in economic data is codified in existing laws and legal procedures, the *automatic* part of the reaction. Politicians can obtain the desired time profile of fiscal policy by adding or substracting expenditure in a discretionary way. This is systematic fiscal policy discretion. Doing so, they have to rely on the information set accessible to them at the time of decision making. Thus, discretionary fiscal policy is a function of GDP in real time. Automatic fiscal policy, in turn, is a function of the true state of the economy and not the observed state. By definition, automatic fiscal policy is linked to expenditure and revenue laws. These laws state very specific micro-economic links. Corporate income tax revenues are, for example, a function of corporate profits; spending on unemployment is a function of the number of unemployed applying for benefits. For these law-based micro-economic relationships, measurement errors in aggregate GDP should be irrelevant.

Accordingly, we split direct government expenditure at time $t$ into an automatically determined part (A) and a discretionary part (D). This leads to the specification of two different equations of government expenditure:

$$e_t^A = a_1^A(L)e_{t-1} + b^A(L)r_t + c_0^A y_t^* + c_1^A(L)y_{t-1}^* + v_t^A, \tag{2}$$

$$e_t^D = a_1^D(L)e_{t-1} + b^D(L)r_t + c_0^D y_t^t + c_1^D(L)y_{t-1}^t + v_t^D, \tag{3}$$

where $y_t^*$ is the true GDP of the economy, while $y_t^t$ is GDP at time $t$ as observed, or conjectured, at time $t$, where the superscript refers to the vintage of the information set.[4]

Like Croushore and Evans (2006), we use the identity that real-time GDP data reflect the true state of the economy plus a measurement error:

$$y_t^t = y_t^* + \eta_t^t. \tag{4}$$

Collecting terms, the structural equation for total government expenditure is then:

$$\begin{aligned} e_t &= e_t^A + e_t^D \\ &= \left(a_1^A(L) + a_1^D(L)\right) e_{t-1} + \left(b^A(L) + b^D(L)\right) r_t \\ &\quad + \left(c_0^A + c_0^D\right) y_t^* + \left(c_1^A(L) + c_1^D(L)\right) y_{t-1}^* + c_0^D \eta_t^t + c_1^D(L)\eta_{t-1}^t + v_t^A + v_t^D. \end{aligned} \tag{5}$$

Writing the structural equation in this way shows that systematic discretionary policy reaction is identified by the effect of the GDP measurement error on government

4. In the lag polynomial $c_1^D(L)y_{t-1}^t$, the dating of the information set stays constant for each observation. As a robustness check, we also let the information set vary. The differing interpretation is discussed in Section 3.

behavior, $c_0^D$ and $c_1^D(L)$. By separately identifying $c_0^A$, $c_0^D$, $c_1^A(L)$, $c_1^D(L)$, we are able to ascertain whether or not active fiscal policy beyond automatic stabilizers takes place, and we can pin down the time profile of systematic policy reaction to output fluctuations. Obviously, this decomposition can be done with any other (linear) policy functions.

We apply this novel method of identifying discretionary fiscal policy to test a central assumption in Blanchard and Perotti (2002). In their benchmark specification, the authors estimate a three-variable structural vector autoregression (SVAR) for U.S. data. In a first step, a reduced-form VAR is estimated,

$$Y_t = C(L)Y_{t-1} + U_t, \quad t = 1, \ldots, T, \tag{6}$$

where $Y_t$ is a vector of endogenous variables (net revenue, government expenditure and GDP) $(r, e, y)$, $C(L)$ is a $3 \times 3$ matrix lag polynomial, and $U_t$ is a $3 \times 1$ vector of reduced-form innovations. $U_t$ is independent and identically distributed with variance–covariance matrix $\Sigma_U = E(U_t U_t')$. The reduced-form innovations $U_t$ and the objects of ultimate interest, the structural shocks $V_t$, are connected by $AU_t = V_t$, where the matrix $A$ describes the instantaneous relationship between the variables.

The central identifying assumption needed to retrieve structural shocks in Blanchard and Perotti (2002) is that there are no "discretionary adjustments made to fiscal policy in response to unexpected events within the quarter" (p. 1333). Therefore, as a second step, the structural shocks to revenue and to expenditure can be retrieved using exogenous elasticities of the automatic response of fiscal policy to the state of the economy, which we label $c_0^A$ for the automatic response of government expenditure to the real economy.

With real-time data, we are able to directly test Blanchard and Perotti's (2002) identifying assumption. To test whether there is a systematic discretionary response of fiscal policy to contemporaneous output, that is, $c_0^D \neq 0$, we estimate the following reduced-form equation:

$$e_t = \alpha(L)e_{t-1} + \beta(L)r_{t-1} + \gamma(L)y^*_{t-1} + \delta_0 \eta_t^t + \delta_1(L)\eta_{t-1}^t + u_t^e. \tag{7}$$

The reduced-form residual, $u_t^e$, is a linear combination of the three structural shocks to expenditure, revenue, and output. Blanchard and Perotti (2002) use $c_0^A = 0$ as an exogenously determined elasticity for the automatic response of government direct expenditure to GDP and they assume that $c_0^D = 0$. In addition, they use the restriction that government expenditure does not contemporaneously react to government revenue.[5] Under these assumptions, the reduced-form shock is equal to the structural shock, that is, $u_t^e = {v_t}^e$.

We may include the GDP revision error $\eta$ contemporaneously in the reduced form, as revisions are statistical measurement errors that are independent of

5. The authors also experiment with restricting the contemporaneous reaction of revenues to expenditure to be zero. For the sake of our argument, we use the restriction on expenditure mentioned in the text.

government expenditure. Government outlays are contemporaneously observed with good precision; statisticians form rational expectations as to the other aggregates given this information. The measurement error of GDP will thus be uncorrelated with the structural shocks of the expenditure equation.

The coefficient on $\eta_t^t$ should consistently be estimated as $\hat{\delta_0} = 0$, if Blanchard and Perotti's assumptions were true. We thus test the null hypothesis $\delta_0 = 0$. If $\delta_0 = 0$, then $c_0^D$ will be zero as well. If we are able to reject this hypothesis, Blanchard and Perotti's (2002) central identifying assumption of no contemporaneous discretionary response is violated.

Blanchard and Perotti (2002) extend their assumptions on decision lags in Section 8 of their paper. To achieve identification of the system if fiscal shocks are anticipated one period ahead, they assume that there is "no discretionary response of fiscal policy to output shocks this quarter (the assumption we made until now) nor to output shocks last quarter (a stronger assumption than before)" (Blanchard and Perotti 2002, p. 1352). In our framework, we can also test whether discretionary fiscal policy reacts to output shocks of last quarter, that is, $\delta_0 + \delta_1(0) = 0$. Again, if Blanchard and Perotti's assumption of no contemporaneous response of government expenditure to GDP and no automatic reaction of direct government expenditure to government revenue is true, then the coefficient on $\eta_{t-1}$ is consistently estimated as the structural shocks to GDP and revenue does not enter the reduced form expenditure equation.

## 2. GDP REVISIONS AND DATA

We use the real-time data set for the United States, which was compiled at the Federal Reserve Bank of Philadelphia and is described in detail by Croushore and Stark (2001, 2003). The data range from 1965:3 to 2009:1.

Since we do not observe "true" GDP $y^*$, we need to resort to proxies. First, we suppose that the final vintage $T$ best reflects true GDP. Thus, $y_t^T = y_t^*$. The measurement error can then be computed as $\eta_t^t = \log(y^t{}_t/y_t^T)$. When using this measure, we need to deal with benchmark revisions that shift GDP levels substantially. Indeed, benchmark revisions do not constitute an improvement of the information set in the sense that anything new is learnt. Level shift issues can most easily be dealt with by computing growth rates of GDP for all vintages and then rescaling the GDP of each vintage to a common level. To the extent that redefinitions merely cause level shifts, this method is fine.

Figure 1 gives the ratio of the first GDP release to the last release of GDP where GDP has been rescaled to the same initial level using growth rates.

As can be seen, the variable is highly autocorrelated and there are still shifts in the series.[6] Croushore and Stark (2001) indeed show that benchmark revisions have

6. The series has a missing observation in 1996Q1. While in this quarter a new vintage was published, the new vintage did not include an additional quarterly observation. The following vintage included two new quarters.

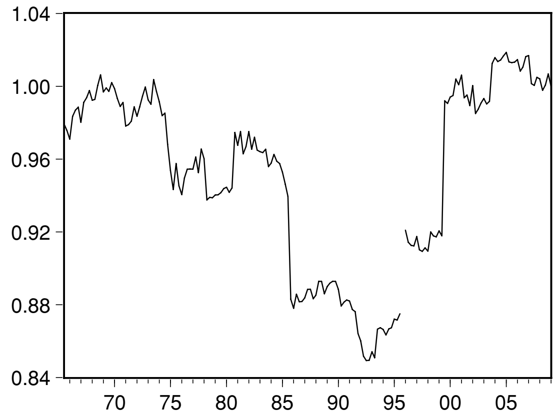


FIG. 1. The Log of the First Release Relative to the Last Release of Real GDP Data.

a very complicated form and do not just shift the entire series up by some ratio. Moreover, we find that the series has a mean smaller than one, indicating that the first release of GDP is often smaller than the last. In addition, the autocorrelation suggests that policy makers in real time might be able to forecast some of the revision. We therefore do not use this measure.

In the empirical estimations, we instead resort to three different measures to capture the measurement error. First, to better capture the actual arrival of new information, we use the first difference of the level revision.[7] More formally, the variable is defined as

$$\Delta\eta_t^t = \eta_t^t - \eta_{t-1}^{t-1} = \log\left(\frac{y_t^t}{y_t^T}\right) - \log\left(\frac{y_{t-1}^{t-1}}{y_{t-1}^T}\right) = \log\left(\frac{y_t^t}{y_{t-1}^{t-1}}\right) - \log\left(\frac{y_t^T}{y_{t-1}^T}\right). \tag{8}$$

The equation shows that this measure essentially captures the revision in the growth rate of GDP. However, the information set used to compute the growth rate in real-time varies. The series could therefore still have odd shifts due to GDP redefinitions in benchmark revision years. Benchmark revisions influencing the real-time growth rate appear particularly large in 1985Q4 and 1999Q3.[8] In the empirical specification using this approach, we take out these two large spikes in 1985Q4 and 1999Q3. Figure 2 plots the adjusted data.

7. We are grateful to one of the referees for having suggested this measure.

8. Major benchmark revisions occur around every 5 years (e.g., November 1975, 1980, etc. according to Croushore and Stark 2001), the detailed description is available on the webpage documenting the data set at http://www.philadelphiafed.org/research-and-data/real-time-center/real-time-data/data-files/ROUTPUT/specific_documentation_ROUTPUT.pdf. We did not take out all these data points, since it would imply a significant loss of data for the estimation of our model with four lags of the variables. Instead, we took out the two largest spikes; both are due to benchmark revisions.

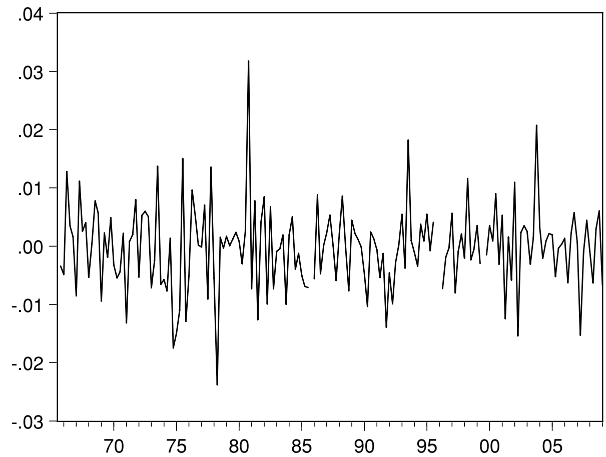


FIG. 2. The Log Difference between the First Release and the Last Release of Real GDP Data as Defined in Equation (8).

However, as one can see, even then other large spikes remain and we cannot be sure whether they constitute benchmark revisions or truly new information on the growth rate.

Second, we also resort to the difference in GDP growth rates as measured by the first vintage and the last vintage. That is, we do not reconstruct levels but use the differences in growth rates directly for identification. The advantage of this measure is that it cannot jump due to benchmark revisions, as the elements constituting the real-time growth rate are defined on the basis of a common information set.

Figure 3 shows our measure derived as the deviation of the growth rate as measured in real time from the final growth rate. More formally, it can be written as

$$gr_t^t = \log\left(\frac{y_t^t}{y_{t-1}^t}\right) - \log\left(\frac{y_t^T}{y_{t-1}^T}\right), \tag{9}$$

where $gr$ stands for growth revision. As can be seen, revisions to the growth rate can be at times quite substantial. This suggests that we might be able to use them successfully for identification. Moreover, the measure looks well behaved in a statistical sense.[9]

The statistical properties of this measure have been investigated in detail by Aruoba (2008). The series has a nonzero mean and is uncorrelated with the initial growth level. Moreover, it has a meaningful signal-to-noise ratio, which is defined as the the standard deviation of final growth revisions divided by the standard deviation of

9. Autocorrelation tests show that it is not autocorrelated. Moreover, it is stationary and not trending.

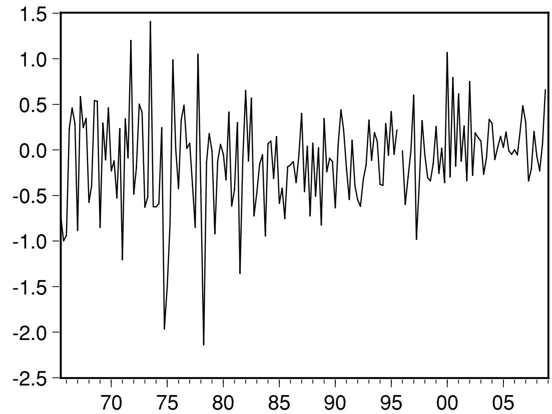


FIG. 3. The Difference between the First and the Last Release of Real GDP Growth.

the final growth rate. Revisions are thus of an economically meaningful size, which makes it possible to find fiscal policy reactions. While many of the other national account variables investigated in Aruoba (2008) are autocorrelated, quarterly real GDP growth rate revisions are not autocorrelated. This suggests that they are not likely to be forecastable. More formal forecast tests presented in Aruoba confirm this. In a real-time forecasting exercise, the $p$-value of the employed test statistics is not in the usual significance range, indicating that a random walk forecast cannot be beaten by an econometrician in real time.

This has an important implication. Fiscal policy makers cannot foresee the correction of the error of the initial GDP release. Thus, while they are aware of the fact that the first GDP release is an imperfect description of reality, they are compelled to rely on this information since better information cannot be obtained in real time.

Third, we define the growth revision to cover the period from the first release to the release 3 years later. This is consistent with the work of Aruoba (2008), who argues that a large percentage of nonbenchmark revisions are incorporated in the vintage 3 years after the first data vintage. Thus, we approximate the measurement error with the first to the 13th vintage revisions, that is, $growth_t^t - growth_t^{t+12}$.

Besides the contemporaneous revision, we include the first four lags of the revision in the estimation, as the policy rule may also include lagged GDP. Thus, fiscal policy makers can react with a lag to the perceived state of the economy. Two different approaches to including the lag suggest themselves. First, we can confine ourselves to the variables described above and lag them. This would imply that the revision is lagged as well as the information set, the first lag would thus be $\Delta\eta_{t-1}^{t-1}$. The economic interpretation of this measure is that policy makers react with a lag to the information they have received a quarter earlier. Second, fiscal policy makers can use all available information and react to what they believe happened last period and this period. The first lag of the revision would then be $\Delta\eta^{t}_{t-1}$. Both ways of including lags of the revision appear to be meaningful, and thus we show the results for both. In most cases, they provide essentially similar results.

We define government expenditure and net revenue exactly as Blanchard and Perotti (2002) have done. It is important to note that our fiscal data are taken from the final vintage; that is, they are taken from the Bureau of Economic Analysis website and refer to the currently valid definition.[10] Revisions of government expenditure due to redefinitions in the system of national accounts can be significant. However, we want to assess to what extent governments adjust their spending due to the perceived state of the economy. To do so, we need to use an identical definition of government expenditure throughout our sample. Since we also control for real GDP in the VAR equation, we also want to make sure that the definition of GDP corresponds to the definition of the government spending series.

Our estimation equation follows the specification established in Blanchard and Perotti (2002). Equation (5) provides us with a link between the *levels* of $\eta$ and government spending. As we discussed above, however, the level measure $\eta$ suffers from a number of drawbacks and a first difference measure is needed. We therefore start by estimating the entire equation in first difference. Second, to replicate the original estimation equation of Blanchard and Perotti, we rely on the estimation in levels but include our measures for the revision process as discussed in growth rates. While the second specification is not fully consistent with the derived model, the Blanchard and Perotti hypothesis in its extended form would clearly imply a coefficient of zero, which we can test. Our estimation equations include a trend and a quadratic trend as their counterparts in Blanchard and Perotti. Excluding the trend variables does not change our results. The equation includes four lags of the log (difference) of real per capita GDP, real per capita revenue, and real per capita government spending.

## 3. EMPIRICAL RESULTS

Table 1 presents our central estimation results. In column A, we employ the first difference of the total revision, defined in equation (8), as a proxy for the perceived versus the actual state of the economy. Equation (6) is estimated in first differences. Government expenditure clearly responds to the revision error. We find that the coefficient of the contemporaneous GDP measurement error is significant and negative. Thus, if GDP growth is perceived to be smaller than it actually is, this leads to a positive deviation of per capita expenditure growth. Quantitatively, the result suggests quite a sizable response. If the revision amounts to 1% of GDP, per capita government expenditure is estimated to adjust by around 0.3%.

10. www.bea.gov

TABLE 1

RESPONSE OF GOVERNMENT EXPENDITURE GROWTH TO TOTAL GROWTH REVISIONS

| | A | | B | | C |
|---|---|---|---|---|---|
| $\Delta\eta_t^t$ | **−0.29** | $gr_t^t$ | **−0.48** | $gr_t^t$ | **−0.46** |
| | −2.47 | | −3.01 | | −2.85 |
| $\Delta\eta_{t-1}^{t-1}$ | **0.03** | $gr_{t-1}^{t-1}$ | **0.21** | $gr_{t-1}^t$ | **0.33** |
| | 0.21 | | 1.15 | | 1.94 |
| $\Delta\eta_{t-2}^{t-2}$ | **0.17** | $gr_{t-2}^{t-2}$ | **0.00** | $gr_{t-2}^t$ | **−0.13** |
| | 1.34 | | −0.02 | | −0.72 |
| $\Delta\eta_{t-3}^{t-3}$ | **0.00** | $gr_{t-3}^{t-3}$ | **−0.13** | $gr_{t-3}^t$ | **0.02** |
| | −0.03 | | −0.69 | | 0.10 |
| $\Delta\eta_{t-4}^{t-4}$ | **0.04** | $gr_{t-4}^{t-4}$ | **−0.04** | $gr_{t-4}^t$ | **−0.24** |
| | 0.36 | | −0.25 | | −1.36 |

NOTES: The $t$-values are under the coefficients. Dependent variable is the log difference of real per capita government consumption and investment (e). We do not report the coefficient results on the lagged log-difference of real per capita net revenue, real per capita government consumption, or real per capita GDP of the final release, $(r, e, y^T)$ and trend. The sample covers 1965Q3–2009Q1.

Column B documents the estimation results of a regression analysis, which includes the growth revision, as defined in equation (9) and lags 1–4.[11] The advantage of this measure over the first is that it is least influenced by benchmark revisions. Government expenditure clearly responds to the revision error defined thusly. We find that the coefficient of the contemporaneous GDP measurement error is significant and negative. Quantitatively, fiscal policy makers will increase per capita government consumption by around 0.5% if perceived GDP growth is 1 percentage point lower than actual GDP growth. Moreover, the other lags of the regression are not statistically significant. This suggests that fiscal policy makers only react contemporaneously to what they know about the economy. Outdated information on the economy from last quarter does not significantly influence current fiscal policy.

In the third column of the table, we essentially replicate the regression of the second column of the table but do not allow the information set to vary any further. The regression should thus capture how policy makers react in real time to what they know about GDP in this quarter and the preceding four quarters. In this regression, we see confirmation that fiscal policy makers react in real time to what they perceive to be the state of the economy. Moreover, policy makers appear to slightly counteract the stimulus they have provided in the following quarter, even though the effect is significant only at the 10% level. A significance test of the sum of the contemporaneous and the first lag of the variable indicates that the stimulus is still highly significant after the first quarter, albeit quantitatively smaller. A potential reason for the countering of the initial stimulus is that the limited appropriated budget forces the executive to revert some of the stimulus unless congress increases appropriations during the next quarters.

The estimation results are robust to shortening the estimation sample. The employed sample covers the period up to 2009Q1. Arguably, the currently still ongoing

11. The information set therefore varies with the lag of the variable.

TABLE 2

Response of Government Expenditure in Levels to Total Growth Revisions

| | A | | B | | C |
|---|---|---|---|---|---|
| $\Delta\eta_t^t$ | **−0.33** | $gr_t^t$ | **−0.57** | $gr_t^t$ | **−0.56** |
| | −2.68 | | −3.47 | | −3.39 |
| $\Delta\eta_{t-1}^{t-1}$ | **0.09** | $gr_{t-1}^{t-1}$ | **0.29** | $gr_{t-1}^t$ | **0.37** |
| | 0.75 | | 1.61 | | 2.19 |
| $\Delta\eta_{t-2}^{t-2}$ | **0.13** | $gr_{t-2}^{t-2}$ | **−0.04** | $gr_{t-2}^t$ | **−0.12** |
| | 0.98 | | −0.20 | | −0.69 |
| $\Delta\eta_{t-3}^{t-3}$ | **0.04** | $gr_{t-3}^{t-3}$ | **0.02** | $gr_{t-3}^t$ | **0.08** |
| | 0.35 | | 0.11 | | 0.44 |
| $\Delta\eta_{t-4}^{t-4}$ | **−0.04** | $gr_{t-4}^{t-4}$ | **−0.29** | $gr_{t-4}^t$ | **−0.37** |
| | −0.39 | | −1.89 | | −2.31 |

Notes: The *t*-values are under the coefficients. Dependent variable is the log of real per capita government consumption and investment (e). We do not report the coefficient results on the lagged difference of real per capita net revenue, real per capita government consumption, or real per capita GDP of the final release, $(r, e, y^T)$ and trend. The sample covers 1965Q3–2009Q1.

TABLE 3

Response of Government Expenditure Growth to 3-year Growth Revisions

| | A | | B | | C |
|---|---|---|---|---|---|
| $\Delta\eta_t^t$ | **−0.12** | $gr_t^t$ | **−0.05** | $gr_t^t$ | **0.02** |
| | −0.79 | | −0.25 | | 0.10 |
| $\Delta\eta_{t-1}^{t-1}$ | **−0.32** | $gr_{t-1}^{t-1}$ | **−0.46** | $gr_{t-1}^t$ | **−0.53** |
| | −2.00 | | −2.24 | | −2.39 |
| $\Delta\eta_{t-2}^{t-2}$ | **0.12** | $gr_{t-2}^{t-2}$ | **0.18** | $gr_{t-2}^t$ | **0.06** |
| | 0.70 | | 0.88 | | 0.26 |
| $\Delta\eta_{t-3}^{t-3}$ | **0.12** | $gr_{t-3}^{t-3}$ | **−0.20** | $gr_{t-3}^t$ | **−0.12** |
| | 0.73 | | −0.98 | | −0.51 |
| $\Delta\eta_{t-4}^{t-4}$ | **0.16** | $gr_{t-4}^{t-4}$ | **−0.06** | $gr_{t-4}^t$ | **−0.27** |
| | 1.08 | | −0.28 | | −1.13 |

Notes: The *t*-values are under the coefficients. Dependent variable is the log difference of real per capita government consumption and investment (e). The revision is measured as the difference between the quarterly growth rate of the first data release and that of the 13 data release (regression B and C). In regression A, the measure as described in equation (7) is used but instead of the final release *T*, the release at $t + 12$ is used. As above, this measure is also cleaned for the largest spikes. We do not report the coefficient results on the lagged log-difference of real per capita net revenue, real per capita government consumption, or real per capita GDP of the final release, $(r, e, y^T)$ and trend. The sample covers 1965Q3–2009Q1.

crisis is an exceptional downturn, which has triggered an exceptional fiscal policy response. However, shortening the sample to cover only the period up to 2007Q1 yields very similar results, which are available upon request.

In a second specification, we estimate the equation in levels but leave our explanatory variable in growth rates as defined in equations (8) and (9). While this model is slightly different from the one above, it can still be used to test the hypothesis of no discretionary response. Table 2 presents the results. The estimation results are virtually identical to the ones presented in Table 1. For all three measures, we again find a significant contemporaneous policy response to the perceived state of the economy.

In a third step, we carry out the first exercise again but define the revision process to cover only the revisions of the first 3 years (see Table 3). This covers a large percentage of the revisions due to the arrival of new information as is discussed in Aruoba (2008). However, the measure also misses some of the fundamental revisions, which do provide new information. The regression results broadly confirm our initial finding. There is a significant and negative discretionary response. However, with this measure, we find the response to be significant only with the first lag, while for the contemporaneous growth revision the coefficient is negative but insignificant. Quantitatively, the response is meaningful and similar in size to that in the previous estimation table. Overall, this again indicates a relatively quick discretionary fiscal policy response to the state of the economy.[12]

The estimation results therefore show that the U.S. government can, and indeed has, mobilized short-term funding to buy goods and services. It is important to consider how this is done in practical terms. As far as central government spending is concerned, once discretionary funds have been appropriated by Congress, the executive branch can spend these funds with some degree of freedom, subject to an extensive body of appropriation laws that governs, among other things, the time limit or spend-out rate for various programs.[13] Apart from this law, the executive branch is relatively free to spend these funds as it sees fit. In practical terms, a president, working with his cabinet officers,[14] can increase the rate of incurring obligations from existing budget authority through the Office of Management and Budget (OMB), which is part of the Executive Office of the President. For example, agencies can speed up the rate at which they enter into obligations, or contracts, for goods and services. This can be done—subject to restrictions—by accelerating agencies' spending (apportionment) plans, consistent with direction from OMB.[15]

To sum up, our findings show that fiscal policy makers react to GDP as measured in real time. The reaction is discretionary, as automatic fiscal policy should not react to measurement errors. The reaction is fast on the expenditure side, with a significant within-quarter reaction according to two measures of the error and a significant reaction with the other measure after one quarter.

These results call into question the identifying assumption of Blanchard and Perotti (2002) that there is no fiscal policy discretion within the quarter or within this and the following quarter. Quantitatively, our results suggest that the values for the automatic

12. The difference in the timing regarding the first and the second lag indicates that after the 12th lag new information that alters the structure of the two series is still arriving. For our purposes, however, both indicate a significant reaction at the latest after one quarter, which is a sign of purposeful and sizable policy discretion.

13. These limits say that, for example, certain types of capital spending must not be obligated and disbursed over more than 5 fiscal years and that personnel expenditures must take place within the fiscal year.

14. To a non-U.S. reader, it might be worthwhile to mention that the cabinet officers usually head the different ministries, such as the U.S. Treasury.

15. The rules for agency spending plans are spelled out in, for example, http://www.whitehouse.gov/omb/circulars/a11/current_year/s120.pdf. According to the rules, agencies can accelerate their spending (apportionment) plans, consistent with direction from OMB. An apportionment is a plan, approved by OMB, to spend resources provided by law. Of course, discretionary policy also occurs at the state and local levels. The practical terms of implementing short-term fiscal action vary across states and cities. The most recent example for a significant degree of executive discretion in the timing and size of spending is, of course, the TARP program.

stabilizers in Blanchard and Perotti's paper could be adjusted upward. This would reflect the average systematic fiscal policy discretion as they show in our estimates with constant coefficients.

## 4. CONCLUSIONS

We have proposed a method for identifying discretionary fiscal policy reactions using real-time data. We started from the observation that automatic stabilizers react to true GDP, while fiscal policy makers react to GDP as observed in real time. We approximate the difference between true and real-time GDP with the difference between the last and the first release of GDP growth figures. As a second approximation of the measurement error, we use the difference between the first and 13th vintage. These measurement errors are exogenous to fiscal expenditure actions and can therefore be used to test for contemporaneous discretionary fiscal policy actions. We applied this method in order to test the identifying assumptions in Blanchard and Perotti (2002) that there is no contemporaneous discretionary response of fiscal policy.

Our main finding is that fiscal policy makers can resort to fiscal policy discretion within the same or following quarter to actively counteract perceived macro-economic developments. Indeed, government expenditure is found to react significantly to the measurement error. Our results therefore cast doubt on the identifying assumption in Blanchard and Perotti's (2002) seminal paper. Expenditure is increased if GDP growth is perceived to be lower than it is in the last release. Overall, our results show that fiscal policy makers do have short-term discretion and use this discretion to try to address seemingly unwelcome macro-economic developments. They do so in response to the pattern of GDP in real time.

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