The Aggregate Effects of Budget Stimulus: Evidence From the Large Fiscal Expansions Database

Jérémie Cohen-Setton, Egor Gornostay, Colombe Ladreit

PIIE PIIE Bocconi University

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Abstract

This paper estimates the effects of fiscal stimulus on economic activity using a novel database on large fiscal expansions for 17 OECD countries for the period 1960-2006. The database is constructed by combining the standard statistical approach to identifying large shifts in fiscal policy with narrative evidence from contemporaneous policy documents to classify them according to their motivations. Using episodes that were not motivated by a desire to respond to prospective economic conditions, the paper finds large, persistent, and positive effects of fiscal stimulus on GDP with a decrease in net exports that only partly offsets the increase in private domestic demand. The paper also finds suggestive evidence that fiscal policy is more effective in a slump than in a boom.

Keywords: Fiscal Policy, Public Economics, Public Finance, Tax Elasticities, National Government Expenditure, National Budget, Macroeconomic Policy, Stabilization, Macroeconomic History

JEL: E6, H3, H5, H6, N1
1 Introduction

When the financial crisis hit ten years ago and interest rates fell to zero, policymakers turned to fiscal stimulus to limit the contraction in economic activity and spur a recovery. When trying to predict the effects of the stimulus, they were surprised to learn that there was not only a lack of consensus about the size of the effects of fiscal policy, but sometimes even about its sign (IMF, 2013). A renaissance of fiscal research has since happened (Ramey, 2019) but the range of estimates for the bang for the buck remains "awfully wide" for fiscal expansions (Blanchard et al., 2018) reflecting the difficulty of identifying exogenous shifts in policy from observational data and the absence of a history of fiscal expansions.

This paper introduces a new database of large fiscal expansions for a panel of 17 advanced economies over the last four decades - the Large Fiscal Expansions Database (LFED). The construction of the database follow several steps. First, large declines in cyclically-adjusted budget balances are used to flag "possible" shifts in fiscal policy. Second, original documents by the OECD and the IMF are consulted to check whether the description of fiscal actions fit with the signal provided by the declines in cyclically-adjusted balances. When both the statistical indicator and the description of policy actions match, fiscal expansions are separated between those that were implemented to offset a downturn ("endogenous") and those that occurred for reasons "exogenous" to the state of the economy. Last, OECD and IMF reports are used to obtain estimates of the policy impulse for the "exogenous" fiscal packages.

This paper finds that close to one-third of all episodes of large fiscal expansions identified by a standard statistical approach are, in fact, "incorrect" in the sense that the decline in the fiscal balance happens despite an absence of government fiscal actions and instead reflects mismeasurement. Among the episodes identified by the statistical approach that are confirmed by narrative evidence, we find that around two-thirds are implemented in response to an actual or expected deterioration in economic conditions ("endogenous"). Thus, only one-third are "exogenous" to the state of the economy and can be used for causal inference. Failing to distinguish these "incorrect" and "endogenous" episodes from...
those that are "exogenous" would create a negative bias that is strong enough to make fiscal expansions look contractionary, rather than expansionary.

The paper finds large, persistent, and positive effects of an exogenous fiscal stimulus on GDP, but that decreasing net exports partly offset the increase in private final domestic demand. Following a 1 percent of GDP exogenous fiscal expansion, output increases by around 0.5 percent on impact, rising to 1.5 percent over four years. It also finds evidence that fiscal policy is more effective at stimulating output growth in slumps because monetary policy does not offset higher GDP growth and higher real wages with higher interest rates in that environment.

We make two contributions to the literature on fiscal policy. The first contribution is data. The paper documents in detail why large declines in the cyclically-adjusted balance occurred for more than 152 country-year pairs and - when they are shown to correspond to actual fiscal actions - whether these were motivated by counter-cyclical or by other reasons. To allow researchers to improve our judgment calls on exogeneity and complete our data collection efforts, extensive supplementary material is provided. In particular, the relevant quotes and sources are documented for all episodes. Each "exogenous" episode is, in addition, described in more details in Appendix C.

The second contribution is methodological and relates to the literature on identifying and measuring shifts in fiscal policy. For identification, this paper shows that when the object of research is large shocks in a cross-country context, there is value in using the statistical approach (Alesina and Perotti, 1995; Alesina and Ardagna, 2010) as a first step to reduce the amount of necessary reading. In our case, the authors had to only read narratives on 152 country-year pairs rather than on 799 (17 countries times 47 years) had a pure narrative approach been used. For measurement, the paper confirms that unstable tax and expenditure elasticities (Prîncen et al., 2013; Mertens and Ravn, 2014; Riera-Crichton et al., 2016) due to non-linear effects of output fluctuations (Chalk, 2002) or to fluctuations in asset and commodity prices (OECD, 2006; IMF, 2010; Price and Dang, 2011; Guajardo et al., 2014; Yang et al., 2015) and of revisions in potential output series (Joskows, 2008; Cohen-Setton and Valla, 2011; Darvas, 2013) make ex-post measures of the change in the cyclically-adjusted budget balance a poor indicator of the size of fiscal changes. But it also shows that real-time estimates of the cyclically-adjusted budget balance obtained from OECD and IMF country reports provide a useful alternative.

The remainder of the paper is organized as follows. Section 2 presents our hybrid approach to identifying and measuring large fiscal expansions for a panel of OECD countries. Section 3 decomposes the downward bias generated by a pure statistical approach to identifying and measuring fiscal expansions, presents our benchmark results and robustness checks for both average and state-dependent multipliers. Section 4 concludes.
2 The New Large Fiscal Expansions Database

2.1 Motivation

The key issue in estimating the impact of fiscal policy is identification. Discretionary changes in government taxes and expenditures are likely to affect output. But contemporaneous or expected fluctuations in output are also likely to drive changes in fiscal policy. More precisely, suppose that output growth in country $i$ at time $t$, $\Delta Y_{i,t}$ (where $Y_{i,t}$ is the natural logarithm of real GDP), is related to changes in fiscal policy as follows:

$$\Delta Y_{i,t} = \alpha_i + \lambda_t + \beta \Delta FP_{i,t} + u_{i,t}$$

where $\Delta FP_{i,t}$ is a measure of fiscal policy changes, and $\alpha_i$ and $\lambda_t$ are country and time fixed effects, respectively. Clearly, any measure of fiscal policy changes that is a function of factors also affecting output contemporaneously (i.e. such that $\Delta FP_{i,t} = \Delta FP_{i,t}(u_{i,t})$) cannot be used to identify and consistently estimate the effects of fiscal policy on output growth.

To consistently estimate $\beta$ using the standard fixed effects (FE) estimator, it is generally sufficient that explanatory variables are uncorrelated with the idiosyncratic error in each time period, i.e. $E(\Delta FP_{s,t} u_{i,t}) = 0$ for all $s, t = 1, ..., T$. Strict exogeneity of the policy changes or $E(u_{i,t} | \Delta FP_{i,t}, \lambda_t, \alpha_i) = 0$ for all $t$ is further needed for statistical inference and efficiency. Typical measures of fiscal policy changes such as the change in the budget balance or the change in the cyclically-adjusted balance do not have this property. In fact, even the weaker assumption of contemporaneous exogeneity, $E(u_{i,t} | \Delta FP_{i,t}, \lambda_t, \alpha_i) = 0$ for all $t$, is typically violated.

A useful decomposition of these statistical fiscal policy measures separates them into distinct components as follows:

$$\Delta FP_{i,t} = \underbrace{ENDO(u_{i,t})}_{\text{"Endogenous"}} + \underbrace{EXO_{i,t}}_{\text{"Exogenous"}} + \left[M_0 + M_1(u_{i,t})\right]_{\text{"Measurement Error"}}$$

(2)

The first component ($ENDO(u_{i,t})$) corresponds to movements in the statistical fiscal measure that reflect fiscal policy changes motivated by the state of the business cycle. The second component ($EXO_{i,t}$) corresponds to movements in the statistical fiscal measure that reflect fiscal policy changes motivated by other considerations than the state of the business cycle, such as reducing tax distortions, increasing infrastructures, or redistributing income. The third component ($M = M_0 + M_1(u_{i,t})$) corresponds to movements in the statistical fiscal measure that are not driven by fiscal policy actions, but rather reflect "measurement error." Because even the zero contemporaneous correlation assumption, $E(\Delta FP_{i,t} u_{i,t}) = 0$, $^{3}$ IMF (2010) and Guajardo et al. (2014) show that these measurement errors are often correlated with output growth despite cyclical adjustment. Removing measurement error is thus important not just for efficiency, it is critical for consistency. According to Mertens and Ravn (2013), even measurement errors of the type $M_0$ can create attenuation bias.
is clearly violated, consistent FE estimates of the impact of fiscal policy from Equation (1) cannot be obtained.

In essence, the approach of this paper consists of cleaning $\Delta FP$ of its "endogenous" and "measurement error" components and consider only policy changes not motivated by the state of the business cycle. With a measure of "exogenous" fiscal impulses, $EXO_{i,t}$, the effects of fiscal expansions on output can be estimated by fitting the following distributed lag model with $n$ lags:

$$\Delta Y_{i,t} = \alpha_i + \lambda_t + \sum_{j=0}^{n} \beta_j EXO_{i,t-j} + u_{i,t}$$  (3)

2.2 Construction of the Large Fiscal Expansions Database

Figure 1 shows this paper’s strategy to identify and measure large exogenous fiscal stimulus episodes. First, we start by searching for "possible" episodes of large fiscal expansions by using a statistical approach a la Alesina and Perotti (1995), where large movements in a cyclically-adjusted measure of the fiscal balance are used to identify important changes in fiscal policy. For the years when the decline in this measure is large, we then read IMF Article IV reports and OECD Economic Surveys to see if the description of fiscal policy aligns with the movement in the statistical indicator. Doing so allows us to identify what Alesina (2010) calls "incorrect" episodes that are erroneously captured by the statistical approach. When a fiscal action is confirmed by the policy record, we classify it as "endogenous" if it was taken in response to macroeconomic fluctuations or as "exogenous" if it was the by-product of other considerations. For large "exogenous" fiscal stimulus episodes, we collect real-time narrative measures of the fiscal impulse.

2.2.1 Finding Possible Large Fiscal Expansions

This paper looks for indications of large fiscal changes in an unbalanced panel of 17 countries from 1960 to 2006. The annual data are collected from the OECD Economic Outlook N°100 (June 2016), except when older issues of Economic Outlook provide longer time series. Two approaches are generally used to obtain a cyclically-adjusted measure of the fiscal balance for a panel of countries. The most straightforward approach is to use a measure already calculated by an international organization such as the IMF or the OECD, which in this paper is referred to as structural primary balance (SPB). A change in the latter is denoted as $\Delta SPB^T_i$, where the subscript specifies year $t$, and the superscript points to the vintage of an estimate. In the statistical approach, ex-post data of vintage $T$ are generally used. A disadvantage of this approach is that such measures are only available since the mid-1980s or early 1990s for most countries. Thus, another approach is often used since Alesina

4Our sample covers the same countries as Guajardo et al. (2014).

5This is the case for Australia, Austria, France, Germany and Ireland. See Appendix D for more details on data sources.
Figure 1: Identifying and Measuring Shifts in Fiscal Policy: A Guide

Own-calculated measure of fiscal impulse
(Blanchard Fiscal Impulse)

$BFI_t^F$

$|BFI_t^F| > \text{threshold}$

No

Yes

Endogenous
(66)

Exogenous
(38)

Incorrect
(46)

Ex-post statistical measure of fiscal impulse:
$BFIT_l$ or $\Delta SPB_l^T$

Real-time narrative measures of fiscal impulse

$\Delta SPB_l^T$

(from IMF Article IVs and OECD Surveys)

$FE_l^T$

(Forecast Error in $\Delta SPB_l^T$ from OECD Outlook)

OECD calculated measure of fiscal impulse
(Change in Structural Primary Balance)

$\Delta SPB_t^F$

$|\Delta SPB_t^F| > \text{threshold}$

Yes

No

All possible large fiscal expansions
(152)
and Perotti (1995) and builds on Blanchard (1990)'s suggestion to calculate a cyclically-adjusted measure of the fiscal balance as "the value of the primary surplus which would have prevailed, were the unemployment at the same value as in the previous year, minus the value of the primary surplus in the previous year." In what follows, a change in this measure is referred to as the Blanchard Fiscal Impulse and is denoted as $BFI$ or $BFI^T_t$, where the subscript specifies year $t$, and superscript $T$ points to the vintage of the data to construct the estimates.

To implement the Blanchard (1990)'s idea, we closely follow Alesina and Perotti (1995). For each country, we first estimate the relationship between certain components of government revenues and expenditures (respectively $R_t$ and $G_t$) expressed in percentages of GDP and the unemployment rate ($U_t$)\(^6\). The estimated coefficients together with the previous year’s unemployment rate ($U_{t-1}$) are then used to calculate primary expenditures ($G^*_t$) and revenues ($R^*_t$) adjusted for changes in the unemployment rate.\(^7\) The $BFI$ is then calculated as the difference between the primary balance adjusted for changes in unemployment in period $t$ and the actual primary balance in period $t-1$. A negative $BFI$ means that the government spent more or levied less taxes than what the state of the economy would have normally implied, suggesting a possible expansionary fiscal stance.

\[
G_t = \phi_0 + \phi_1 \text{Trend} + \phi_2 U_t + \epsilon_t \tag{4}
\]
\[
R_t = \gamma_0 + \gamma_1 \text{Trend} + \gamma_2 U_t + \eta_t \tag{5}
\]
\[
G^*_t = \hat{\phi}_0 + \hat{\phi}_1 \text{Trend} + \hat{\phi}_2 U_{t-1} \tag{6}
\]
\[
R^*_t = \hat{\gamma}_0 + \hat{\gamma}_1 \text{Trend} + \hat{\gamma}_2 U_{t-1} \tag{7}
\]
\[
BFI^T_t = [R^*_t - G^*_t] - [R_{t-1} - G_{t-1}] \tag{8}
\]

Figure 2 compares the $BFI$ obtained for France and the US with $\Delta SPB^T_t$ as calculated by the OECD. Clearly, the $BFI$ approach is useful in extending the time coverage as it respectively adds 10 and 20 more years of observations for France and the US. While the two lines move closely together, the exact magnitudes of the two indicators are not always the same.

Focusing on large shocks requires the definition of thresholds. In their seminal paper, Alesina and Perotti (1995) use a fixed threshold of 1.5 percent of GDP for all countries. Given the heterogeneity across countries in the mean ($\mu_i$) and standard deviation ($\sigma_i$) of changes in fiscal policy documented in Appendix Table A.1, the use of country-specific thresholds is preferred. For each country, the $BFI$ (resp. $\Delta SPB$) series is compared to $BFI$ (resp. $\Delta SPB$) thresholds. More specifically, a decline in the cyclically-adjusted fiscal measures is considered large if it is larger in absolute value than $\mu_i - \sigma_i$ in a single year or $\mu_i - 1.5\sigma_i$ over two years (with a size of at least $\mu_i - 0.5\sigma_i$ in each of them). The second criterion helps capture episodes that are large, but happen over several years.

\(^6\)Precisely we adjust transfers. In that respect we follow (Alesina and Perotti, 1995), although it is also the methodology used by the OECD (Girouard and André, 2005).

\(^7\)Other components are then added without any adjustment.
Figure 2: Comparison between OECD’s $\Delta SPB^T_t$ and own calculated $BFI^T_t$

Note: The BFI and OECD lower and upper bounds are for 1-year episodes, meaning that they correspond to the mean +/- 1*s.d. thresholds. Our 2-year thresholds (mean +/- 1.5*s.d.) are not represented here but can be found in Appendix Table A.1
2.2.2 Eliminating False Positives and Classifying by Motivations

Out of our sample of 691 $BFI_t^T$ and 436 $\Delta SPB_t^T$ observations, we obtain 152 country-year pairs of large declines (Appendix Table A.2). For each of these, we read contemporaneous policy documents (typically OECD Economic Surveys and IMF Article IVs for the previous, current, and following years) to assess whether the declines in these statistical measures correspond to fiscal actions and document their rationale.

In our sample, we find that the standard statistical measure is misleading about 30% of the time. For these country-year pairs, we are able to find specific economic or budgetary developments that cause the standard statistical measure to inaccurately identify the size of an episode. Appendix Table A.3 strike through the "incorrect" episodes accordingly from the list of all "possible" episodes. Clearly, the problem is widespread. All countries, except Japan, display years that are wrongly identified as large fiscal expansions by the standard statistical approach. Additional documentation on each of these cases can be found in a companion Appendix provided as supplementary material, in which we provide the citations for each data point that lead us to classify this episode as "incorrect".

In some cases, we can see directly that a decline in a cyclically-adjusted fiscal measure was a misleading indicator of actual fiscal actions from descriptions provided in the policy records, which either emphasize a lack of government intent or a desire to implement a contractionary rather than expansionary fiscal stance. In France in 1993, "the sharp widening in the [...] deficit is described as largely unintended." In Portugal in 1978, "fiscal policy is described as not intended to give impetus" by the OECD. Rather than being expansionary, the fiscal stance in Spain is described as moving towards restriction in the early 1990s. Similarly, the OECD emphasizes the "strongly restrictive stance to monetary and fiscal policy" in Italy in 1981. It also points out that "dictated by balance-of-payments considerations, fiscal policy was tightened" in Sweden in 1980.

In a few cases, declines in the cyclically-adjusted fiscal measures arise because of one-off accounting events. For instance, a "Fund for Railway Infrastructure, which owns the railway infrastructure, was created" in Belgium in 2005. As the general government became "liable for its corresponding debt of 7.4 billion euros [...] government debt increase[d] by 2.5% of GDP". While these one-off transfers onto the balance sheet of the general government are unlikely to be correlated with the state of the economy, such measurement errors do not just create noise and decrease efficiency of statistical estimates, but also bias the estimated impact of fiscal policy toward zero.

More importantly, the bulk of "incorrect" episodes identified by the statistical approach arise because of falls in revenue elasticities and increases in spending elasticities. In Belgium in 1988, it is, for example, argued that the backsliding in fiscal finances since 1987 "result[s] essentially from the unintended fall in the apparent elasticity of revenue with respect to GDP".

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8When needed, we complement these sources with other IMF Staff Reports such as Recent Economic Developments. We also, sometimes, rely on publications by the European Commission and check our findings against narrative histories when available.

946 country-year pairs out of 152.

10It is also available here: https://www.overleaf.com/project/5a12f2b63c2b7170b5572318
observed during this period". Similarly, the "large overshoot" in the budget deficit in Austria in 1993 appears mostly "due to [the] operation of stabilizers" rather than resulting from discretionary fiscal action. That such problem arises is not surprising given the evidence already documented in IMF (2010) and Guajardo et al. (2014) for fiscal consolidation episodes and the well-understood problem with non-constant revenue and expenditure elasticities (Prinzen et al., 2013). But the problem is more widespread than generally understood.

As in Romer and Romer (2010), we then separate "correct" episodes of large fiscal expansions into two broad categories: those that happened in response to factors likely to affect output growth in the near future ("endogenous") and those taken for other reasons ("exogenous"). If policy documents discuss some of the measures contributing to the large fiscal expansion were implemented because of a desire to respond to current or prospective economic conditions, we classify the episode as "endogenous." This is more restrictive than what is usually done in the narrative literature where "exogenous" can happen at the same time than some "endogenous" measures.

Examples of "endogenous" episodes include Belgium in 1972, for which the OECD stated that "the slowdown in economic growth [...] led the Belgian authorities to modify the posture of economic policy in a more expansionary direction"; Canada in 1983 where the government implemented a "Special Recovery Program [...] to stimulate private sector investment, formed the centerpiece of the budget"; or the United States in 1970 where the "policy was eased in the first half of 1970 to limit the downturn of the economy". The online Appendix provides similar quotes for the other episodes that we consider "endogenous."

This leaves us with a set of "exogenous" episodes, which can be used for causal inference. Examples of episodes that are considered exogenous are the United States in 1982-1983, 2002-2003, and France in 2002, where tax cuts were implemented to increase long-term growth; Ireland in 1978 where the government wanted to reduce hysteresis unemployment through excess demand; and the United Kingdom in 2001-2003 where the government wanted to redress past under-investment. Detailed additional documentation on each fiscal policy change can be found in Appendix C, in which we provide detailed citations to show how we determine the motivation.

### 2.2.3 Measuring Large Fiscal Expansions

To use the common metric of fiscal multipliers, the size of the fiscal impulse needs to be documented. Two approaches are usually followed. First, in the statistical literature exemplified by Alesina and Ardagna (2010), $BFI^T_t$ - where subscript $t$ refers to the time period of the fiscal shock and superscript $T$ refers to vintage of the data series - is not only used to identify shifts in fiscal policy, but also to quantify them. As $BFI^T_t$ was revealed to often be a misleading indicator of the direction of a shift in fiscal policy, there is a reason to believe that it might also be a misleading indicator of its magnitude, even when the direction is accurate. Second, in the narrative approach exemplified by Romer and Romer (2010), the fiscal impulse is generally measured as the legislative forecasts of the expected cumulative effect on tax revenues and government expenditures.
In this paper, we propose two other alternative measures. The first one is the change in structural primary balance (denoted $\Delta SPB_t$) as estimated in real time and published in IMF Article IV Reports and OECD Economic Surveys. An advantage of these estimates is that they have "reality checks," as the OECD and the IMF exploit their local presence in the member countries and hold extensive discussions with local government experts and policymakers. A disadvantage of this measure, however, is that the underlying methodology used to obtain these estimates is not uniform across countries, time periods, and sources. We partly address this concern in Section 3.5 when introducing the LP-IV framework.

Another disadvantage is that it ignores that private agents may respond to fiscal policy before the shift in policy is actually implemented (Blanchard and Perotti, 2002; Mertens and Ravn, 2012; Ramey, 2016). To address this issue, we construct forecast error measures ($FE$) for the change of the structural primary balance from multiple editions of the OECD Economic Outlook. Like Auerbach and Gorodnichenko (2013) for government expenditures, IMF (2014) for public investment, and Furceri et al. (2018) for monetary policy, we calculate forecast errors as the difference between the forecast and realized values, thereby purging fiscal variables of their predictable components. Another advantage is that fiscal forecasts from the OECD Economic Outlook are calculated using a uniform methodology for all countries in contrast to the estimates of $\Delta SPB_t$ obtained from IMF Article IVs and OECD Economic Surveys.

OECD fiscal forecasts, however, only start in the 1980s and do not eliminate the impact of one-off operations (Joumard et al., 2008). In fact, it is only since the December 2008 Economic Outlook that the OECD introduced such systematic correction with the production of an "underlying fiscal balance" measure (NLGQU), which removes "net one-offs" (NOOQ) from the "cyclically-adjusted net lending" variable (NLGQA). For both reasons, we use $\Delta SPB_t$ as our baseline measure rather than $FE_t$.

2.2.4 Limitations

A first limitation of our approach is its focus on large fiscal expansions. By construction, small fiscal stimulus cannot be identified with our approach. This could be a problem if the incremental impact of a fiscal expansion on economic activity is related to the size of the package. While we cannot exclude this possibility for fiscal expansions, it has not been documented for fiscal consolidations (IMF, 2010). Both small and large (and even very large defined as a discretionary exogenous decrease in the deficit by more than 3 percent of GDP)
fiscal consolidations are found to reduce output growth by the same amount. The LP-IV approach introduced in Section 3.5 also helps address this issue.

A second limitation has to do with determining the intent of policymakers for some fiscal expansions from narrative evidence. While it is for example relatively clear-cut that an absence of new tax measures can be equated with a neutral tax stance, defining a neutral stance for expenditures (Carnot and Castro, 2015) is harder and sometimes means that one has to judge if an expenditure slippage was intentional or not. Missing non-announced but intentional expenditure slippages could bias our results. Similarly, if policymakers postpone tax cuts and expenditure increases until the economy strengthens (beyond the normal economic dynamic that we control for) then the fiscal expansions might still be associated with business cycle developments. This would bias our results toward associating economic overheating with fiscal expansions and overestimating expansionary effects.

A third limitation concerns the effect of fiscal policy announcements. As argued by Alesina et al. (2015), fiscal policy in year $t$ generally consists of three components: unexpected shifts in fiscal variables (announced upon implementation in year $t$), shifts implemented at time $t$ but announced in previous years, and future announced changes (announced at time $t$ for implementation in some future year). While our approach allows to measure these first two components, it does not capture the third one.

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$^{13}$In IMF (2010), Figure 3.2 shows the effect for all consolidations, while Figure 3.10 shows the effect of large fiscal consolidations, defined as discretionary deficit cuts larger than 1.5 percent of GDP.

$^{14}$From the perspective of getting consistent estimates, missing large intended fiscal expansions does not constitute in itself a problem. If these missing expansions were endogenous, they should not be in the regression. If they were exogenous, missing them is unfortunate, but they end up just adding noise to the regression.
Figure 3: Endogenous and Exogenous Fiscal Expansions

(a) Australia

(b) Austria

(c) Belgium

(d) Canada

(e) Denmark
2.3 Properties of the New Database

Out of 152 country-year pairs of large declines in the cyclically adjusted fiscal measures, 104 are found to reflect government actions. The other 46 correspond to large declines in cyclically-adjusted fiscal balances that happen despite an absence of fiscal stimulus. Among 104 LFE identified, two-thirds (66) are found to be motivated by counter-cyclical reasons and one-third (38) are not. Altogether, this gives 38 country-year pairs that are classified as "exogenous".

The 104 "endogenous" and "exogenous" LFE are shown in the panel of diagrams in Figure 3. An interesting feature is that fiscal policy changes appear synchronized across OECD countries. That this would be the case against the backdrop of the synchronized recessions of the early 1970s, 1980s and 1990s is hardly surprising. But even so-called "exogenous" fiscal changes appear correlated across countries with many countries for example implementing tax cuts to improve potential growth in the early 2000s.

As explained in Section 2.2.1 some episodes are identified and measured in the statistical approach using $BFI_t$ while others are identified and measured using the official measure calculated by the OECD, $\Delta SPB_t$. Since both are similar conceptually and calculated "ex-post," in what follows the "statistical" impulse is referred to as $BFI$ irrespective of whether it is actually $BFI_t$ or $\Delta SPB_t$. This avoids carrying both notations and contrasts the traditional statistical fiscal impulse with "real time" measures such as $\Delta SPB_t$ or $FE_t$ obtained directly from OECD and IMF reports.

Figure 3 displays different measures of the fiscal impulse. For all episodes, the $BFI$ measure is displayed. For "exogenous" episodes, the $\Delta SPB$ and $FE$ measures are also displayed when available. The $BFI$ is generally larger in absolute value than the other measures. For some, this clearly reflects measurement errors. According to the $BFI$ measure, Germany implemented 4 per cent of GDP fiscal stimulus in 2001, which is clearly incorrect. According to $\Delta SPB$, the stimulus only amounted to around 1 per cent of GDP. As described in Appendix C this impulse was widely expected as part of the Tax Relief Act. It is, thus, not surprising that the $FE$ measure is, actually, positive.

The key identifying assumption is that the constructed series of large fiscal expansions is, indeed, exogenous. While the contemporaneous "exogeneity" of the constructed series cannot be tested, it is possible to test whether it is predictable on the basis of past information. To test this hypothesis, a multinomial response model as in Cloyne (2013), Jorda and Taylor (2016), and Mertens and Ravn (2013) as well as Granger causality tests as in Cloyne (2013) are estimated. As in the rest of the literature, lags of the growth rate of real GDP, inflation, short-term interest rate changes, and the government debt to GDP ratio are included as regressors for both tests. Unobserved heterogeneity and common trends are also

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15 Note that Australia 1965 is discarded from the our original list of 39 "exogenous" country-year pairs. Australia 1965 and Australia 1966 are picked up by our identification method, with Australia 1966 identified as an endogenous episode. Once this is taken into account, Australia 1965 no longer meets our 1-year threshold. For that reason, we do not keep it. This is the only country-year pair for which this issue arises. For all other consecutive exogenous-endogenous cases, the exogenous episode still meets the threshold once the endogenous year is dropped.

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Table 1: Predictability tests for "exogenous" episodes

<table>
<thead>
<tr>
<th>Test</th>
<th>Test statistic</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Logit (LR test)</td>
<td>4.3</td>
<td>0.231</td>
</tr>
<tr>
<td>Granger causality (F-test) for $\Delta SPB$</td>
<td>1.45</td>
<td>0.242</td>
</tr>
<tr>
<td>Granger causality (F-test) for $FE$</td>
<td>0.91</td>
<td>0.443</td>
</tr>
</tbody>
</table>

Note: In both models, regressors comprise one lag of each of the macroeconomic variables as well as one lag one the dependent variable. These results are robust when using two or three lags instead. Both models include country and year fixed effects. To correct for possible misbehavior of standard errors, statistical inference from the Granger causality test is based on Driscoll and Kraay (1998) standard errors.

controlled by including country and year fixed effects. Since a lag of the growth rate of real GDP is controlled for in our baseline model specification for output impulse responses (see Section 3.1), it is not included in the joint tests in Table 1.

The top line of Table 1 displays the test statistic and p-value associated with the null-hypothesis that these variables have no explanatory power in a conditional logit model, where the dependent variable takes a value of 1 when a large fiscal stimulus happens. With a p-value of 0.231, the hypothesis that the "exogenous" episodes are not predictable cannot be rejected. The next two lines of Table 1 show the result of the single-equation version of the Granger causality test, which exploits information about both the timing and the size of an exogenous fiscal stimulus, with either $\Delta SPB$ or $FE$ used to measure the size of the fiscal impulse. For both measures, the non-predictability hypothesis cannot be rejected. Thus, these tests also suggest that the exogenous fiscal stimulus episodes in our sample cannot be forecast based on past information.

\[16\] Jorda and Taylor (2016) and Mertens and Ravn (2013) used the unconditional probit estimator, which in panel data with fixed effects suffers from "the incidental parameters problem," leading to inconsistent parameter estimates. We thus use the conditional logit estimator instead.

\[17\] The difference in results between the two measures is mostly explained by the fact $FE$ has fewer observations than $\Delta SPB$. 

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3 Effects of Fiscal Expansion on Economic Activity

3.1 Baseline Specification

The specification we consider takes the following single-equation form:

\[ Y_{i,t+h} - Y_{i,t-1} = \alpha_i^h + \lambda_t^h + \phi^h \Delta Y_{i,t-1} + \sum_{j=0}^{n} \beta_j^h \Delta FP_{i,t-j}^{a,b,c,d,e} + u_{i,t+h} \]  

(9)

where subscript \( i \) indexes countries, subscript \( t \) indexes years, \( Y \) is the logarithm of real GDP (or hours, real wages, ...) or the level the unemployment rate (or interest rates, ...), \( \alpha_i \) denotes country-fixed effects, \( \lambda_t \) denotes year-fixed effects, and \( u_{i,t} \) is an error term. The \( \beta \) coefficients are the contemporaneous and lagged effects of fiscal expansions. A lag order of \( n = 1 \) is selected based on. The variable of interest \( \Delta FP \) has a number of different upper-scripts because we estimate Equation 9 based on several measures of the fiscal impulse.

As the most obvious concern when regressing a shock onto output growth is that the result does not distinguish between the effect of the shock and that of normal output dynamics (Romer and Romer, 2010), our benchmark specification includes one lag of output growth as a regressor. Including lagged values of the dependent variable as controls, however, makes the fixed effects (FE) estimator inconsistent due to the violation of the strict exogeneity assumption (Nickell, 1981) and is likely to generate an upward bias. The inconsistency of the estimated coefficients is, however, unlikely to be sizable as it is of order \( 1/T \). In fact, we verify this by re-estimating Equation (9) using Arellano and Bond (1991) (AB) GMM estimator that is designed to address this problem. Like Guajardo et al. (2014) for fiscal consolidations, we find that a minor difference in results between the FE and AB estimators and so use the FE estimator in the rest of the paper.

Following Jorda (2005), we use the method of local projections (LP) to estimate impulse response functions (IRFs). In contrast with the traditional vector autoregression (VAR) method, which uses the one-period ahead expectation to form the two-period ahead expectation, local projections use a separate regression for each forecast horizon. Hence, Equation (9) is re-estimated for each IRF horizon \( h = 1, 2, 3, 4 \) (with \( h = 1 \) indicating the initial year of a fiscal expansion episode), and \( \beta_h^1 \) is stored after each such regression.

Despite the inclusion of time-fixed effects, the standard Pesaran (2004) and Frees (1995) statistical tests reject the null hypothesis that the residuals from the FE estimation are uncorrelated across countries. Using commonly applied robust standard errors (e.g. White, clustered (Rogers), or Newey-West) would thus be inappropriate for statistical inference as those techniques only correct for heteroscedasticity and serial correlation within countries (or clusters of countries). Instead, we use Driscoll and Kraay (1998) standard errors, which are designed to address the problem of cross-sectional correlation in addition to within-country heteroscedasticity and serial correlation.\(^{19}\)

\(^{18}\)With positive coefficients on the lagged dependent variable and on (the negative of) the fiscal impulse, we expect the FE estimator to overestimate the impact of fiscal expansions.

\(^{19}\)The asymptotic properties of this variance-covariance matrix estimator do not rely on the number
3.2 Biases From "Incorrect" and "Endogenous" Fiscal Expansions

When all country-year pairs with a large decline in $BFI^T_{i,t}$ are used in the regression, the variable of interest is called $\Delta FP_{i,t}^a$. When attention is restricted to large declines in $BFI^T_{i,t}$ that also reflect actual fiscal actions according to IMF and OECD reports (the so-called "correct" episodes in the Alesina (2010) terminology), the variable of interest is called $\Delta FP_{i,t}^b$. Thus, $\Delta FP_{i,t}^b = \Delta FP_{i,t}^a = BFI^T_{i,t}$ for "correct" episodes, while $\Delta FP_{i,t}^b = 0$ and $\Delta FP_{i,t}^a = BFI^T_{i,t}$ for "incorrect" episodes. When attention is further restricted to "correct" episodes that were not motivated by counter-cyclical reasons ("exogenous" episodes in the Romer and Romer (2010) terminology), the variable of interest is called $\Delta FP_{i,t}^c$. It takes the value $BFI^T_{i,t}$ for "exogenous" episodes, and zero otherwise. To assess whether the $BFI^T_{i,t}$ measure might itself generate a bias, two alternative narrative fiscal impulse measures obtained from OECD and IMF reports are used. More precisely, we let $\Delta FP_{i,t}^d = \Delta SPB_{t,t}$ for "exogenous" episodes and zero otherwise, where $SPB$ stands for structural primary balance and the upper-script $t$ points out that this is a real-time rather than ex-post measure. We let $\Delta FP_{i,t}^e = FE_{t,t}$ for the subset of "exogenous" episodes for which this variable is available and zero otherwise.

Figure 4a compares the relative path of the log of real GDP in response to a 1 percent of GDP fiscal expansion based on the different definitions and measurements of the fiscal shock. The difference between $\Delta FP_{i,t}^a$ and $\Delta FP_{i,t}^b$ shows how the inclusion of episodes that are incorrectly identified, i.e. that do not reflect a fiscal action by the government, can bias the results. The difference between $\Delta FP_{i,t}^b$ and $\Delta FP_{i,t}^c$ reflects the bias that can arise from mixing counter-cyclical and "exogenous" episodes. The difference between $\Delta FP_{i,t}^c$ and $\Delta FP_{i,t}^d$ measures the bias due to an incorrect measurement of the fiscal impulse associated with a correctly-identified "exogenous" fiscal expansion.

The results are striking. First, they illustrate how relying only on the statistical approach to both identify and measure large fiscal expansions generates the puzzling finding that fiscal expansions are contractionary. In other words, the puzzling result of Alesina and Ardagna (2010) is not only driven by expansionary fiscal consolidations, but also by contractionary fiscal expansions. As in IMF (2010) and Guajardo et al. (2014), it arises because of measurement errors - due mostly to movements in revenue and expenditure elasticities - that are correlated with movements in economic output. Second, they emphasize the importance of classifying fiscal expansions by motivations. The blue dotted line shows the path of real GDP following large fiscal expansions "correctly" identified and primarily motivated by "exogenous" reasons, while the black dashed line shows the effects of all "correctly" identified episodes (i.e. both "exogenous" and "endogenous" fiscal expansions). It reveals that the downward bias created by mixing these two kinds of fiscal expansions is particularly large.

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of countries, $N$, but rather on the number of time periods, $T$. As with the Newey-West standard errors, the number of lags up to which the residuals may be auto-correlated needs to be specified. We follow the simple rule of thumb of the \textit{xtscc} STATA procedure for selecting $m(T)$ as $\text{floor}(\frac{4(T/100)^2}{9})$. In our setup, this leads to $m(T) = 3$.
Figure 4: Identification and Measurement Pitfalls

(a) Identification: All-identified by statistical approach, correctly-identified (endogenous and exogenous), and exogenous episodes

(b) Measurement: Exogenous episodes with different fiscal impulse measures

Note: The lines indicate the cumulative percentage change in real GDP at years 1 to 4 relative to year 0 in response to a fiscal shock of 1 percentage point of GDP in year 1. In the bottom panel, only exogenous episodes for which the FE measure exists are used. This explains the difference between the solid green lines and the dotted blue lines in the two panels.
3.3 Biases From Mismeasurement of Fiscal Impulse

The top two lines of Figure 4a illustrate the difference in results that arises from using either BFI or ΔSPB as a policy impulse. The fact that the IRF associated with ΔSPB is higher than that associated with BFI illustrates that the fiscal impulse obtained from IMF and OECD reports (ΔSPBf) is typically smaller than that estimated with ex-post data (BFIf and ΔSPBT). This difference is either due to measurement errors in the "statistical impulse" or measurement errors in the "policy records impulse." In other words, either the policy records impulse on average underestimates the actual size of fiscal expansions or the statistical fiscal impulse on average overestimates the size of fiscal expansions.

Differences in follow-up could explain measurement errors in the "policy records impulse." If fiscal plans rather than fiscal actions are recovered from the policy records, and if governments tend to deviate from their fiscal plans (i.e. spend more or tax less than planned), then the "policy records impulse" would understate the actual size of fiscal expansions. This explanation is, however, not very convincing here. In fact, rather than collecting fiscal plans as in IMF (2010), Guajardo et al. (2014), and Alesina et al. (2017), we collect fiscal actions as documented in the policy record in years following the fiscal impulse. For instance, the fiscal impulse for the fiscal expansion that happened in Australia in 2000 is obtained from documents published in 2003 rather than from documents published before the shock implementation (Appendix C).

To compare the impact of different measure of the fiscal impulse, Figure 4b reproduces the results of our baseline specification for the sub-sample for country-year pairs where the Forecast Errors fiscal impulse measure is also available. Two things are worth noting. First, IRFs for both BFI and ΔSPB are slightly higher in Figure 4b than in Figure 4a. This suggests that fiscal expansions in the late 1970s in Austria, Ireland and Spain, and in Portugal in the late 1980s and early 1990s were less effective at stimulating output than the other exogenous episodes in our sample. Second, and in line with the findings of the literature on fiscal foresight Mertens and Ravn (2014) and Ramey (2019), the unanticipated parts of fiscal policy changes appear to have stronger effects than the part that is anticipated.

3.4 Benchmark Effects

We start by reporting in Figure 4 the IRF to a 1 percent of GDP fiscal expansion for (the natural logarithm of) real GDP and the unemployment rate. For all IRFs, the shock corresponds to fiscal impulse measure ΔSPB that happens in year 1, and the forecast horizon is 4 years. A large fiscal stimulus is found to set off a major and persistent expansion in the economy.

According to our estimates, a 1 percent of GDP fiscal expansion is associated with a 1.5

\footnote{Beetsma et al. (2017) investigate this hypothesis for fiscal consolidations and whether differences in follow-up between Expenditure-Based and Tax-Based fiscal plans can explain the difference in tax and spending multipliers.}

\footnote{Forecast Errors are not available for Austria (1976), Ireland (1978), Ireland (1979), Portugal (1987), Portugal (1990), Portugal (1991), and Spain (1978).}
percent peak cumulative increase in GDP. The increase in output is statistically significant in the first year of the shock and builds up over time to reach its peak after two years. The size of the effect is the same (in absolute value) than that obtained by Jorda and Taylor (2016) for fiscal consolidations who report that real GDP is pushed down on average by over 0.57 percent each year for every 1 percent in fiscal consolidation. In this sense, our results do not support the hypothesis that the contractionary multiplier is bigger (in absolute value) than the expansionary multiplier (Barnichon and Matthes, 2016).

The time profile of the decrease in the unemployment rate is similar to that of the increase in output. A 1 percent of GDP fiscal stimulus is associated with a maximum cumulative decrease in the unemployment rate of 0.3 percentage points after three years. The effect is the same than that obtained by Guajardo et al. (2014) who find a 0.3 cumulative increase in the unemployment rate two years after the start of a 1 percent of GDP fiscal consolidation. The fact that we obtain the same results on unemployment but not on GDP than Guajardo et al. (2014) for fiscal consolidations suggest either that their results are smaller due to specification choices or that the link between output growth and unemployment is different during fiscal stimulus and fiscal consolidations. As Jorda and Taylor (2016) do not report results for unemployment, further work is needed to separate between these hypotheses.

The top two panels of Figure 6 summarize the results of re-estimating our baseline specification for the contributions to real GDP of final domestic expenditures and net exports, respectively. The contribution of real final private domestic demand is defined as 

\[
\frac{FDPV_{t-1}}{GDPV_{t-1}} \times gFDPV_{t},
\]

where \(FDPV\) and \(GDPV\) respectively denote real final private domestic expenditures and real GDP, and \(gFDPV_{t}\) denotes the growth rate of \(FDPV\). Similarly, the contribution of net exports is defined as 

\[
\frac{NXV_{t-1}}{GDPV_{t-1}} \times gNXV_{t},
\]

where \(NXV\) and \(GDPV\) respectively denote real net exports and real GDP, and \(gNXV_{t}\) denotes the growth rate of real net exports. We find that a decrease in net exports partly offsets the expansionary effects on total domestic demand (Figure 6b). According to our estimates, net exports reduces GDP growth by up to a cumulative 0.6 percentage points. The striking similarity in the magnitude of the effects for total GDP and private final demand (Figure 6a) suggests, however, that the negative contribution to growth of net exports is fully compensated by higher government investment and consumption expenditures.

For fiscal consolidations, Guajardo et al. (2014) find that the improvement in net exports is associated with declines in interest rates (Figure 6f) and a depreciation of the currency.

Guajardo et al. (2014) obtain smaller effects on real GDP (between 0.5 and 0.8 percent of GDP after two years depending on the specification) than Jorda and Taylor (2016) despite using the same IMF narrative database of fiscal consolidations. If true, our results together with those of Guajardo et al. (2014) would suggest asymmetric effects of fiscal policy, but not in the direction argued by Barnichon and Matthes (2016).

We compute real final private domestic (\(FDPV\)) expenditure as \(FFDV - CGV - IGV\) where code \(FFDV\) is real final domestic expenditures, code \(CGV\) is real government final consumption expenditure, and code \(IGV\) is real government investment expenditures in the OECD Economic Outlook Database. For Italy, Germany, Portugal, and Spain, code \(IGV\) is not consistently available. \(FDPV\) is thus simply equal to \(FFDV - CGV\).

We compute real net exports as code \(GDPV\) minus code \(FFDV\) as in Alesina et al. (2019).
Our results are less conclusive, but Section 3.6 suggests that this average behavior hides an important heterogeneity in the behavior of inflation and interest rates for fiscal stimulus that take place during slumps and those that happen during booms. Despite the standard prediction of theoretical DSGE models that total hours should increase under both higher government spending and lower taxes, we find no response (Figure 6c). Interestingly, this lack of response results from both a positive response in total employment and a negative response in the number of hours worked per employee (not shown for brevity). For real wages, we find a gradual increase to a cumulative peak of around 2 percent compared to an otherwise similar economy that would not undergo any fiscal expansion. This suggests that the increase in demand effects dominates any increase in supply associated with large fiscal expansions.
Figure 5: Impact of 1 Percent of GDP Fiscal Expansion: GDP and Unemployment Rate

(a) GDP
(b) Unemployment rate

Note for panel (a): The solid line indicates the cumulative percentage change in real GDP at years 1 to 4 relative to year 0 in response to a fiscal shock of 1 percentage point of GDP in year 1. Note for panel (b): The solid line indicates the cumulative percentage point change in the unemployment rate. Note for panels (a) and (b): The shaded areas represent one standard-error bands.
Figure 6: Impact of 1 Percent of GDP Fiscal Expansion: GDP Components, Labor Market and Interest Rates

(a) Domestic Private Contribution

(b) Net Exports Contribution

(c) Hours

(d) Real wages

(e) Short-Term Interest Rates

(f) Inflation
3.5 Robustness checks

If our series is truly exogenous there should be no need to control for other structural shocks. Yet, fiscal expansions like fiscal consolidations often come as a package (Alesina et al., 2019). The 1982 Reagan tax cuts was, for instance, accompanied by a deregulation push that, if anything, should also have a positive impact on short-term growth. Similarly, the 2003 income tax cuts in Finland were implemented in the context of a broader agreement to moderate wages that may decrease domestic demand, but help increase foreign demand. Our results could thus be affected by other structural shocks or accompanying policies, especially given the limited size of our sample. To help address this issue, we augment the local projection (LP) specification with changes in short-term interest rates to control for monetary policy as in Romer and Romer (2010), Mertens and Ravn (2012), and Cloyne (2013). Doing this does not, however, change results significantly (not shown for brevity).

More generally, the small size of our sample could make our results sensitive to the presence of outliers. To address this issue, we re-estimate Equation (3) but drop one episode at a time from our database. The red thick line in Figure 7a show our baseline estimate. The black dotted lines show the results one would obtain by dropping one episode at a time. Clearly, one episode seems to matter for the effect to converge to 1.5 in year 2 rather than in year 3, but none affects the shape of the IRF and its end point rests within a 0.5 percentage point window from the baseline specification.

One problem recently discussed in the literature is that narrative shocks, here $\Delta SPB_t$, are measured with error because (i) different estimates of the size of the impulse from historical records (IMF, OECD) require judgment for choosing a single measure; (ii) small fiscal expansions are neglected and censored to zero; and (iii) estimates of the fiscal impulse are not always specified using the same metrics (e.g. as a percentage of GDP or potential GDP).

Mertens and Ravn (2013) show that these measurement errors can create an attenuation bias, but that the narrative measure can be used as a “proxy” for the latent fiscal shock. For the narrative measure to be a good “proxy”, it needs to be (i) correlated with the structural fiscal shock and (ii) uncorrelated contemporaneously with all other structural shocks. Under these conditions, unbiased impulse responses can be obtained by estimating a proxy SVAR (Mertens and Ravn, 2013) where a naive measure of the change in fiscal policy (e.g. the change in tax revenues, government expenditures or the deficit) is instrumented by the narrative measure. In our set-up, this means using $\Delta SPB_t$ as a proxy of the latent fiscal shock, and as an instrument for $BFI_t^T$. When using LP (see our Equation (9)), the same proxy approach can be applied, providing that the narrative measure is uncorrelated with all other structural shocks at all leads and lags (Ramey, 2016; Stock and Watson, 2018). Under these assumptions, the causal effect of the fiscal expansions can be estimated via a standard two-stage least squares (2SLS) approach. The LP-IV methodology has the

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25We follow an equivalent timing convention than used by these authors within a VAR framework.

26In the context of Equation (2), these measurement errors are of $M_0$ type as they are not systematically related to output movements.
Figure 7: Robustness Checks

(a) Dropping one episode at a time

(b) $\Delta SPB_1$ as the fiscal shock and as a proxy of the shock

Note for panels (a) and (b): The solid lines indicate the cumulative percentage change in real GDP at years 1 to 4 relative to year 0 in response to a fiscal shock of 1 percentage point of GDP in year 1. Note for panel (b): The dashed lines and shaded area represent one standard-error bands.
advantage of providing consistent estimates even in the presence of measurement error in explanatory variables, so long as the instrument (and its measurement error) is uncorrelated with any measurement error in the explanatory variables. In our set-up, this translates into the condition that $\Delta SPB_i^t$ should be uncorrelated with measurement error in $BFI_i^t$.

We implement LP-IV with 2SLS. More specifically, we regress $BFI_i^t$ on the narrative measure $\Delta SPB_i^t$ (and controls) in a first stage and then regress GDP growth on the fitted values of $BFI_i^t$ (and controls) in a second stage.

\[
\text{Stage 1: } BFI_{i,t}^T = c_1^i + \tau_1^i + \varphi^1 Y_{i,t-1} + \sum_{j=0}^1 \delta_1^j \Delta SPB_{i,t-j} + \epsilon_{i,t}^1
\]

\[
BFI_{i,t-1}^T = c_2^i + \tau_2^i + \varphi^2 Y_{i,t-1} + \sum_{j=0}^1 \delta_2^j \Delta SPB_{i,t-j} + \epsilon_{i,t}^2
\]

\[
\text{Stage 2: } Y_{i,t+h} - Y_{i,t-1} = c_h^i + \lambda_h^i + \phi_h \Delta Y_{i,t-1} + \sum_{j=0}^1 \beta_h^j BFI_{i,t-j} + u_{i,t+h}
\]

Figure 7b compares the baseline IRF (in black) with that obtained (in red) when using the narrative measure as a proxy of the true underlying structural shock. As expected, the new point estimates are higher at all horizons but the difference between the two lines is economically small and not statistically significant.

Another concern is that the multipliers we calculate are impact rather than cumulative multipliers. While the cumulative response of output over several years is compared to the initial shift in fiscal policy for impact multipliers, it is compared to the cumulative change in fiscal policy that follows from the initial shift for cumulative multipliers (Ramey, 2016; Ramey, 2019; Stock and Watson, 2018). To obtain cumulative multipliers, one only has to replace $BFI_i^T$ by the sum of $BFI_i^T$ over the relevant horizon. More specifically, we estimate the following equations:

\[
\text{Stage 1: } \sum_{k=0}^h BFI_{i,t+k}^T = c_1^h + \tau_1^h + \varphi^1 Y_{i,t-1} + \sum_{j=0}^1 \delta_1^h \Delta SPB_{i,t-j} + \epsilon_{i,t+h}^1
\]

\[
\sum_{k=0}^h BFI_{i,t-1+k}^T = c_2^h + \tau_2^h + \varphi^2 Y_{i,t-1} + \sum_{j=0}^1 \delta_2^h \Delta SPB_{i,t-j} + \epsilon_{i,t+h}^2
\]

\[
\text{Stage 2: } \sum_{k=0}^h \Delta Y_{i,t+k} = \alpha^h + \lambda^h + \phi^h \Delta Y_{i,t-1} + \sum_{j=0}^1 \beta^h \sum_{k=0}^h BFI_{i,t-j+k} + u_{i,t+h}
\]

Figure 8 shows the difference between impact and cumulative multipliers. An important gap opens between the two types of multipliers in year 2 suggesting that the fiscal policy continues to be stimulative after the first year of the policy shift. As a result, the cumulative
multiplier converges to around 1 after three years, a value one-third lower than that of the impact multiplier.

Figure 8: Impact and Cumulative multipliers

Note: The lines indicate the cumulative percentage change in real GDP at years 1 to 4 relative to year 0 in response to a fiscal shock in year 1.
3.6 State Dependence: Fiscal Multipliers in Good and Bad Times

There are various reasons why the macroeconomic effects of fiscal policy may vary depending on the state of the economy. Most often cited is the view that fiscal policy may be more effective when the economy is in a slump and operating below capacity\textsuperscript{27}. In this view, an increase in the budget deficit would not crowd-out and might even crowd-in private domestic spending when there is slack. This could arise because prices are less responsive in this environment (higher labor elasticity and lower markups (Hall, 2009)) or because an economy with idle resources does not hit bottlenecks and capacity constraints (Gordon and Krenn, 2010). It may also arise because credit constraints and financial frictions are counter-cyclical (Tagkalakis, 2008; Canzoneri et al., 2016) or because higher public sector employment leads to a milder increase in labor market tightness (Michaillat, 2014). Finally, it could arise because of the lack of inflationary concerns and the muted response of monetary policy (Hall, 2009)\textsuperscript{28}.

The evidence is, however, mixed. Using a panel of OECD countries, Auerbach and Gorodnichenko (2013) and Holden and Sparrman (2018) find that higher government expenditures lead to a larger reduction in unemployment when the output gap is negative. Fazzari et al. (2015) also find that the government spending multiplier is larger and more persistent whenever there is considerable economic slack. On the other hand, Owyang et al. (2013) and Ramey and Zubairy (2018) find no evidence of higher spending multipliers during periods of high unemployment in the US and attribute in subsequent research the higher spending multiplier found for Canada to exceptional circumstances. For US taxes, Eskandari (2015) and Demirel (2016) find that multipliers are actually smaller when unemployment is high than when it is low. Alesina et al. (2019) argue against the case of state-dependence for fiscal consolidations but do not consider measures of slack to define the state. In contrast, Jorda and Taylor (2016) find strong state-dependence effects with a 1 percent of GDP fiscal consolidation associated with a reduction of real GDP by around 4 percent after five years when the economy is in a slump and no negative effect when it is in a boom.

We contribute to this literature by investigating how the effects of fiscal stimulus vary depending on the level of slack in the economy. Methodologically, we follow Auerbach and Gorodnichenko (2012; 2013) and create a continuous measure of the state of the economy, expressed by the logistic transition function $\Lambda(z_t) = \frac{\exp(-\rho z_t)}{1 + \exp(-\rho z_t)}$, which assigns to each state $z_t$ a value between 0 and 1 that can be interpreted as the probability of being in the bad state. An alternative approach would be to use the discrete threshold specification adopted by Owyang et al. (2013), Ramey (2019), and Eskandari (2015), which replace the transition function $\Lambda(.)$ with an indicator function that takes the value 1 if $z$ is above a certain threshold and zero otherwise. The advantage of the Auerbach and Gorodnichenko

\textsuperscript{27}Also cited is whether the economy is moving from its peak to its trough (recession) or moving from its trough to its peak (expansion).

\textsuperscript{28}This is, particularly, the case when the economy is at the zero lower bound (ZLB) (Christiano et al., 2011; Woodford, 2011), but the point is more general.
(2012; 2013) approach is that the statistical inference for each regime is effectively based on a larger set of observations than when using a discrete threshold specification and is thus particularly appropriate in our case. In addition, Auerbach and Gorodnichenko (2013) argue and Eskandari (2015) confirm empirically that the former approach generates a conservative bias against finding state-dependence effects.

We define the state variable $z_t$ by estimating an output gap as the deviation of the logarithm of real GDP from a trend obtained using an HP filter with the smoothing parameter of 100, which is typical for annual data. Before entering the equation, the gap series is standardized and transformed by the transition function, $\Lambda(\cdot)$. The transition parameter $\rho$, which stretches the function around the 0.5-level along the $Y$-axis, is set to 1.5 as in Auerbach and Gorodnichenko (2013). Figure 9 illustrates the dynamics of $\Lambda(z_t)$ for the US and France against that obtained if the OECD measure of the output gap was instead used. It also displays as shaded areas recessions dates obtained from the NBER for the US and from the Economic Cycle Research Institute (ECRI) for France. The first thing to note is that our simple HP filtering generates a measure of economic slack that is very close to that obtained by using the output gap as calculated by the OECD when the two series overlap. In fact, the within correlation between the two measures is 0.87 (with $p$-value of 0.0000). The second thing to note is that the probability of the bad state typically spikes following a recession. For recessions followed by strong recoveries, the measure goes down fairly quickly. For other recessions, the increase in slack is persistent.

We estimate Equation (10) for the same variables as previously.

$$Y_{i,t+h} - Y_{i,t-1} = \alpha_i^h + \lambda_t^h + \phi^h \Delta Y_{i,t-1} + \sum_{j=0}^{1} \beta_{j,B}^h [(1 - \Lambda(z_{i,t-j})) \Delta SPB_{i,t-j}]$$

$$+ \sum_{j=0}^{1} \beta_{j,S}^h \Lambda(z_{i,t-j}) \Delta SPB_{i,t-j} + \delta^h \Lambda(z_{i,t}) + u_{i,t+h}$$

(10)

The dynamics are constructed by varying the horizon $h$ of the dependent variable so that we can directly read the impulse responses off estimated $\{\hat{\beta}_{0,B}^h\}_{h=0}^H$ for booms and $\{\hat{\beta}_{0,S}^h\}_{h=0}^H$ for slumps. We start by reporting in Figure 10 the IRFs to a 1 percent of GDP fiscal expansion for (the natural logarithm of) real GDP and the unemployment rate for the slump and boom regime. For all IRFs, the shock corresponds to fiscal impulse measure $\Delta SPB$ that happens in year 1 and the forecast horizon is 4 years.

According to our estimates, a 1 percent of GDP fiscal expansion is associated with an over 2 percent peak cumulative increase in GDP during a slump. The increase in output is statistically significant in the first year of the shock and builds up overtime. The size of the effect is consistent (in absolute value) with that obtained for fiscal consolidations by Jorda and Taylor (2016) who obtain that a 1 percent of GDP fiscal consolidation is associated with a reduction of real GDP by around 4 percent after five years when the economy is in a slump. Like them, we also find that the effects are not statistically different from zero when the economy is in a boom. The difference in results between slump and boom is less
Figure 9: Estimated weight on the slump regime: own calculated vs. OECD output gap
clear for the unemployment rate. In a slump, the decrease in the unemployment rate is only statistically significant in the second year of the shock. The point estimate in a boom is smaller (in absolute value) than that in a slump and is not statistically different from zero in the first two years of the shock, but the ordering reverses in the third year.

The top two panels of Figure 11 show the contribution of private final demand and net exports to real GDP growth. It reveals that the difference for GDP between slump and boom is not so much due to a higher response of private consumption and private investment during a slump. Instead, the difference appears to arise because the offset from net exports is stronger during booms. Another important difference revealed by the state-dependent results is the behavior of inflation and interest rates. Fiscal expansion in a slump does not appear to generate inflationary concerns. Rather than counteracting the fiscal expansions, monetary policy accommodates or even supports it by also providing stimulus in the form of lower rates. The opposite happens during a boom thereby dampening the impact of fiscal stimulus.

A drawback from this method is that the state of the economy is assumed to not change following a shift in fiscal policy for $h$ years. Had we defined the state of the economy based on output growth or on whether monetary policy is constrained by the ZLB, this would be problematic because of the short duration of recessions (less than four quarters according to Ramey and Zubairy (2018) and Alesina et al. (2019)) and ZLB episodes. This assumption is, however, less problematic when the state of the economy is based on a measure of slack as it typically takes long periods of high growth (resp. low growth) to get an economy back to (away from) its potential.

A first robustness check consists of modifying the timing convention to define the state of the economy to avoid contemporaneous feedback from policy action into whether the economy is in a slack state or a non-slack state. Alloza (2017) and Ramey and Zubairy (2018), for instance, show that modifying this timing convention reverse the results of Auerbach and Gorodnichenko (2012) when looking at recessions versus expansions. To check for this possibility, we use the period $t-1$ output gap instead of that of period $t$ to define the state when the fiscal stimulus happens. Appendix Figure B.1 shows the results. Modifying the timing convention does lower the point estimates for slumps and brings them closer to those found for booms. But, and in contrast to the effects for booms, the effects remain statistically different from zero at all horizons for slumps.

A second robustness check consists of changing the way we constructed the output gap. In particular, we can check whether our results depend on the specific value of the smoothing parameter $\lambda$ choosing to extract a trend with the HP filter. Auerbach and Gorodnichenko (2013) and Eskandari (2015), for example, use larger values of the smoothing parameter to get trend that is only slowly moving. In their sample, such choice is crucial for the procedure not to consider most of the Great Recession as a reduction in potential output rather than as a reduction in cyclical output. Reflecting the fact that our sample ends before the Great Recession starts, the particular choice of $\lambda$ is not crucial with our sample with values of $\lambda$ between typical values such as 6.25 or 100 (Ravn and Uhlig, 2002) for annual data and more extreme values like 5000 giving essentially the same results. A third robustness check consists
Figure 10: Impact of 1 Percent of GDP Fiscal Expansion: GDP and Unemployment Rate

(a) GDP

(b) Unemployment rate

Note for panel (a): The solid lines indicate the cumulative percentage changes in real GDP at years 1 to 4 relative to year 0 in response to a fiscal shock of 1 percentage point of GDP in year 1. Note for panel (b): The solid lines indicate the cumulative percentage point changes in the unemployment rate. Note for panels (a) and (b): The dashed lines and shaded areas represent one standard-error bands.
Figure 11: Impact of 1 Percent of GDP Fiscal Expansion: GDP Contributions, Labor Markets and Interest Rates

(a) Domestic Demand Contribution

(b) Net Exports Contribution

(c) Hours

(d) Real wages

(e) Short-Term Interest Rates

(f) Inflation
of changing the parameter $\rho$ of the transition function that is set to 1.5 as in Auerbach and Gorodnichenko (2013) in our baseline specification. With a smoothing parameter $\lambda$ of 100 for the HP filter, we doubled the value of $\rho$ to 3 and found that it did not substantially affect our state-dependent results for real GDP.

4 Conclusion

This paper constructs a new dataset of large fiscal expansions. To do this, a hybrid methodology that complements the standard statistical approach to identifying episodes of large fiscal changes with narrative evidence is proposed. Results highlight the downward bias induced by "incorrect" and "endogenous" episodes of large fiscal expansions identified by the simple statistical approach. Exploiting large fiscal expansions that were not motivated by counter-cyclical reasons, this paper finds evidence that multipliers that are on average above 1 and state-dependent.

Several improvements and extensions can be envisaged. First and foremost, country experts and those with better knowledge of primary historical sources could document the size of fiscal stimulus for some of the episodes in the 1960s and 1970s for which we could not find estimates of the fiscal impulse. Second, the dataset could be extended by successively decreasing the level of the threshold for what a large fiscal expansions is. Third, the analysis could be refined by documenting the exact composition of each episode between tax cuts and expenditure increases and see if this distinction is as important for fiscal expansions than for fiscal consolidations (Alesina et al., 2019). Fourth, it would eventually be important to combine our database with those on fiscal consolidations to investigate whether the effects of fiscal policies are sign-dependent.
References


### Table A.1: Mean and Standard Deviation of Changes in Cyclically-Adjusted Fiscal Measures (in % of GDP)

<table>
<thead>
<tr>
<th>Country</th>
<th>BFI measured as in Alesina and Perotti [1995]</th>
<th>ΔSPBᵢᵀ measured by OECD</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td># of obs. µᵢ σᵢ 1-yr thld 2-yr thld</td>
<td># of obs. µᵢ σᵢ 1-yr thld 2-yr thld</td>
</tr>
<tr>
<td>Australia</td>
<td>42 .04 1.08 -1.05 -1.59</td>
<td>35 .07 .93 -.85 -1.32</td>
</tr>
<tr>
<td>Austria</td>
<td>46 .02 1.28 -1.25 -1.89</td>
<td>36 -.07 1.22 -1.29 -1.9</td>
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<tr>
<td>Belgium</td>
<td>36 .15 1.81 -1.66 -2.56</td>
<td>21 .11 1.22 -1.11 -1.72</td>
</tr>
<tr>
<td>Canada</td>
<td>36 .07 1.32 -1.24 -1.9</td>
<td>34 0 1.25 -1.26 -1.88</td>
</tr>
<tr>
<td>Denmark</td>
<td>35 .16 1.59 -1.43 -2.23</td>
<td>21 .06 1.22 -1.17 -1.78</td>
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<tr>
<td>Finland</td>
<td>46 .1 1.79 -1.69 -2.59</td>
<td>21 .01 1.65 -1.64 -2.47</td>
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<tr>
<td>France</td>
<td>43 -.03 .88 -.9 -1.34</td>
<td>34 -.08 .82 -.89 -1.3</td>
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<tr>
<td>Germany</td>
<td>46 -.02 1.36 -1.38 -2.06</td>
<td>35 -.02 1.09 -1.1 -1.64</td>
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<td>Ireland</td>
<td>46 .11 1.66 -1.55 -2.38</td>
<td>26 .59 1.53 -.94 -1.7</td>
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<td>Italy</td>
<td>46 -.03 1.38 -1.41 -2.1</td>
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</tr>
<tr>
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<tr>
<td>US</td>
<td>46 -.03 .94 -.97 -1.44</td>
<td>21 -.01 1.03 -1.04 -1.55</td>
</tr>
</tbody>
</table>
Table A.2: List of large declines in cyclically-adjusted fiscal measures

<table>
<thead>
<tr>
<th>Country (start of sample)</th>
<th>Year</th>
</tr>
</thead>
</table>

Note: starred episodes (*) were identified using the methodology described in the text, but the with the structural primary balance (\(\Delta SPB_t^R\)) as calculated by the OECD.
Table A.3: List of large declines in cyclically-adjusted fiscal measures due to policy changes or other reasons (struck through)

<table>
<thead>
<tr>
<th>Country (start of sample)</th>
<th>Year</th>
</tr>
</thead>
</table>

Note: date means that the decline in a cyclically-adjusted fiscal measure is not mostly due to a change in fiscal policy. Starred episodes (*) were not identified using the methodology described in the text, but with $\Delta SPB_t^T$ as calculated by the OECD.
Table A.4: List of large declines in cyclically-adjusted fiscal measures due to endogenous motivations, exogenous motivations (bold) or other reasons (struck through)

<table>
<thead>
<tr>
<th>Country (start of sample)</th>
<th>Year</th>
</tr>
</thead>
</table>

Note: date means that the decline in a cyclically-adjusted fiscal measure is not mostly due to a change in fiscal policy. date means that it is due to a policy change motivated by endogenous reasons (i.e. counter-cyclical reasons). date means that it is due to a change in fiscal policy motivated by exogenous reasons. Starred episodes (*) were not identified using the methodology described in the text, but with $\Delta SPBT_t$ as calculated by the OECD.
Table A.5: Economic conditions when fiscal stimulus happens: percentage point changes relative to a year preceding stimulus

<table>
<thead>
<tr>
<th></th>
<th>Real GDP growth</th>
<th>Unemployment rate</th>
<th>Output gap</th>
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<tr>
<td>Endogenous</td>
<td>-0.43</td>
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<td>-0.93</td>
</tr>
<tr>
<td>Exogenous</td>
<td>-0.06</td>
<td>-0.09</td>
<td>0.04</td>
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</table>

Note: See Section 3.6. Sources: IMF, OECD, authors’ calculations.
Table A.6: Probability $\Lambda(z_t)$ of an episode occurring in a bad state

<table>
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<th>Country</th>
<th>Year</th>
<th>$z_t$</th>
<th>$z_{t-1}$</th>
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<td>.21</td>
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</tr>
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<td>2004</td>
<td>.48</td>
<td>.43</td>
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<tr>
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<td>1990</td>
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<td>2002</td>
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</tr>
<tr>
<td>United States</td>
<td>2003</td>
<td>.69</td>
<td>.63</td>
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</tbody>
</table>

Note: A value of $\rho$ needs to be chosen to evaluate the $\Lambda(z_t) = \frac{\exp(-\rho z_t)}{1+\exp(-\rho z_t)}$ function, which estimates the probability of a bad state. Following Auerbach and Gorodnichenko, 2013 we use $\rho = 1.5$. 

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B Figures

Figure B.1: State-dependent multipliers and monetary policy response: using $t-1$ gap to define the state.

(a) GDP

(b) Short-Term Interest Rates
C Description of Exogenous Episodes

**Australia, 2000** The easing in FY 2000-2001 mainly reflected the introduction of income tax cuts under The New Tax System. According to the OECD Economic Outlook (06/2000, p.84), the package "provide[d] substantial income tax cuts and increases in welfare benefits in fiscal year 2000-01, the effects on government finances being partly offset by higher indirect tax revenues." In FY 2001-2002, the stimulus was largely related to the impact of unanticipated non-cyclical expenditure, in defense, domestic security and border protection (OECD, 2003, p. 58).

Overall, these have resulted in an estimated economic stimulus of 1% in both 2000-2001 and 2001-2002 according to estimates included in the 2002-2003 budget (OECD, 2003, p. 58). The IMF finds similar estimates of the fiscal impulse (IMF IV, 2003, p.). For FY 2002/2003, the OECD estimates that most of the impulse was removed (OECD, 2004).

Contemporary observers felt that the "output gap [was] almost closed" (OECD, 2001) when the tax package of 2000 was implemented. The OECD Economic Outlook of June 2000 describes the Australian economy as having entered 2000 with "substantial momentum." Later estimates (e.g. OECD, 2006, p.22), in fact, consider that the Australian economy was operating above potential in 2000. According to the IMF, the significant reduction in the surplus was seen as necessary by the authorities the "price" to be paid for implementing an important tax reform (IMF IV, 2000, p.10-12, p.17).

That the introduction of the New Tax System would be associated a fiscal stimulus became news in the first half of 2000. In its December 1999 edition (p.72), the OECD Economic Outlook reports that "the general government budget surplus should remain at around 0.5 percentage point of GDP over the projection period [as the] reform, which includes the introduction of a Goods and Services Tax (GST), offsets the consolidation that would otherwise have occurred." In its June 2000 edition, the OECD, however, writes that "on balance, the fiscal stance is likely to be mildly supportive." This is reflected in forecast for the the change structural balance, which went from 0 in December 1999 to -0.9 in June 2000.

**Australia, 2004** In its December 2004 edition of the Economic Outlook, the OECD writes that "the May 2004 Budget contained further income tax cuts and introduced significant targeted spending, with families and low income earners the main beneficiaries. The centrepiece of the budget is the More Help for Families package [...] The package increases assistance to families by raising the rate of Family Tax Benefits and reducing withdrawal rates. It also introduces, from July 2004, a new lump-sum maternity payment granted to all families [...] The new initiatives improve rewards to work through lower effective marginal tax rates (EMTRs) and cuts in income taxes, encouraging labour force participation and boosting productivity."

We use the December 2007 edition of the OECD Economic Outlook (p.250) to get an estimate of the fiscal impulse because no estimates are provided in the December 2016 edition (p.196). It gives an impulse of -1% in 2004, 0.2% in 2005, and 0% in 2006.
The news of a turnaround in fiscal policy became known during 2003. While fiscal policy remains described as "firming" in the June 2003 edition of the OECD Economic Outlook, the December 2003 edition writes that "lower budget surpluses are expected for 2004 and 2005 [following] personal income tax reductions worth a cumulative 1.5 per cent of GDP over the next four years and increased spending for defence, domestic security, health and education." Accordingly, the forecast for the change in the 2004 cyclically-adjusted general government primary balance is forecast in December 2003 to be -0.4 per cent against 0.3 in June 2003.

**Austria, 1963** Consistent with the idea "the primary role assigned to government at the time [in the 1960s] was that of allocating resources to secure the provision of services of general economic interest and make sure that the essential needs of society would be met" (Katterl and Kohler-Toglhofer, 2005), "a substantial part of the deficit [in 1963] was associated with the operation of some State enterprises, especially the Federal railroads" (IMF Art. VIII Part. 2 p.37, 1964).

The 1963 budget, which "was twice as high as that of 1962" (OECD, 1964, p.15, p.8, p.28), "reflected an agreement on the solution of certain social security problems involving [...] the financing of the deficit of the workers’ insurance scheme and some pension institutions" (IMF Art. VIII, 1963, p.7).

We do not have estimates of the fiscal impulse for this episode from the policy record. It is, thus, not included in our calculations.

**Austria, 1976** In its 1978 Recent Developments publication, the IMF notes that that "the most important factors securing a net expansionary effect of fiscal policy have been the continuing rapid growth of federal public consumption and of transfer payments to private households." In its 1976 Economic Survey, the OECD (p.24) notes that "the thrust of the January measures aims at a further expansion and advancement of public infrastructure investment [...] and temporary incentives for private investment (temporary suspension of the investment tax and higher depreciation allowances for private construction)."

In 1978, the IMF estimated that although reduced by half, Austria was still suffering from a 1% negative output gap in 1976 (Recent Developments, 1978, Table 19).

The IMF estimates that the fiscal impulse was equal to 0.5% of GDP in 1976 (Recent Developments, 1978, Table 18: Computation of the Effects of Fiscal Policy). This is consistent with estimates provided by the OECD (Economic Survey, 1977, p.39), which writes that "after a record negative swing in the federal budget in 1975 (equivalent to 3 per cent of GNP) fiscal policy has lent further support to economic activity as witnessed by the continued, albeit diminishing, increase in the demand effective deficit."

**Canada, 2001** "After nearly seven years of restraint, fiscal policy is becoming expansionary" writes the June 2000 edition of the OECD Economic Outlook. In its Staff Report for the Article IV 2002 Consultation (2002, p.21), the IMF notes that "the tax reductions and other measures announced by the federal government in 2000 and 2001 are providing..."
substantial stimulus to the economy.

"There are several sources of fiscal stimulus. In its February 2000 Budget and October 2000 Economic Statement and Budget Update, the federal government announced substantial reductions in personal and corporate income taxes during the period ahead. The December 2001 Budget included further modest expenditure measures and some tax deferrals from 2001-02 to 2002-03. The employment insurance premium rate also is being steadily reduced, from $2.40 in 2000 to $2.10 in 2003, providing a small stimulus."

According to the IMF, "the authorities saw these reforms as a major step to boost incentives to work and invest" (Art. IV, 2001). When it became clear that the economy was deteriorating following the 9/11 terrorist attacks, the authorities declined to change their fiscal paths to further accommodate the downturn. "Although there have been some calls for more aggressive action, [the authorities] felt that a substantial shift to stimulus in the budget would be interpreted as stepping away from a hard-won commitment to fiscal discipline and would drive up long-term interest rates" (IMF, Art. IV, 2002).

We estimate that the fiscal impulse was -1.6% in 2001 (IMF Art. IV 2002 p.21. Box 5: Sources of Fiscal Stimulus). This was not an unexpected fiscal stimulus. Instead, the change in fiscal policy forecast happened between the December 1999 and the June 2000 edition of the OECD Economic Outlook, with respective changes in the structural balance of -0.1 and -0.4 per cent of GDP.

**Finland, 2003** In its 2004 Article IV Consultation (p.10), the IMF writes that "the strong fiscal expansion - second only to that observed in the United States and the United Kingdom in the OECD area - was largely brought about by increased public spending in 2002 and tax cuts in 2003."

"Although no longer necessary from a cyclical perspective, the authorities viewed the income tax cuts as appropriate in the context of a moderate wage agreement and to achieve a critically needed increase in employment" (IMF Art. IV, 2004, p.4).

Using IMF Article IV 2005, we estimate a fiscal impulse of -1.2% in 2003. The change in fiscal policy was largely unexpected. In its December 2002 edition, the OECD Economic Outlook estimates a change in the cyclically-adjusted general government primary balance of -0.5 per cent of GDP (Annex Table 31). In its June 2003 edition, the forecast is revised to the level of -1.4 per cent of GDP.

**France, 1990** According to the 1990 edition of the IMF Recent Developments, “the 1990 budget is a continuation of the 1989 budget in that it combines conservative thinking about the need for a reduction in the fiscal deficit and in taxes with a renewed emphasis on social equity and appeals to solidarity." “Specific expenditure and revenue decisions within the budget reflect the three broad priorities that the authorities have adopted for 1990. These are: (1) preparation for the future and leading role of France in the construction of Europe; (2) reduction of social inequities and support to developing countries; and (3) rehabilitation of the role of government."

The 1993 edition of the IMF Recent Developments notes that the “deterioration in the
structural component of the deficit from 1990 to 1993 is principally explained by a dramatic increase in expenditures". The 1991 edition also emphasizes increases in military spending. According to recent evaluation, the structural effort is estimated to have moved from a slightly positive number in 1989 to almost -1% in 1990 (Ecalle, 2018, https://www.fipeco.fr/pdf/0.97011900%201527854994.pdf).

We use PPAA/94/13, "Recent Fiscal Developments in the European Countries of the G-7" to get estimates of the fiscal impulse. It gives -0.3% in 1990, 0.2% in 1991, and -1.1% in 1992. The pattern is similar if 1993 IMF Article IV (Chart 4) or 1993 IMF Recent Developments (Chart 7) is used. Note that IMF (PPAA/94/13) considers that the bulk of the increase in the late years is in reaction to the downturn.

The change in the French fiscal stance appears to have largely unexpected. In fact, even in December 1990, the OECD still expected a positive change of 0.3 per cent of GDP in cyclically adjusted overall balance. But "higher-than-anticipated expenditure induced by the Gulf war and social solidarity measures" (OECD EO, June 1991) resulted in fiscal slippage.

France, 2002 In its Staff Reports for the 2002 and 2003 Article IV Consultations with France, the IMF notes that "fiscal policy relaxed substantially in 2002." As noted by the OECD in its June 2002 Economic Outlook, the easing of fiscal policy was largely unexpected as fiscal policy turned sharply expansionary despite a "budget that called for a stable government deficit"

In its 2003 Economic Survey (p.35), the OECD writes that the measures included "increases in the employment tax credit (prime pour l’emploi, PPE), corporate income tax reductions for small business, the suppression of the part of the "professional tax" applied to salaries and reductions in social security charges linked to the 35 hour workweek legislation were of particular importance."

According to the IMF, "central to the new government’s policies are the desire to refocus the role of the state, by adjusting spending priorities and decentralization, and to boost the growth potential of the economy through tax reductions and the creation of a business-friendly environment."

Using IMF Article IV 2005 (p.40), we estimate that the fiscal impulse was -1.1% in 2002 and -0.5% in 2003.

Germany, 1990-91 Already in 1989, the stance of fiscal policy was expected to be easier "because of the implementation of the last (and largest) instalment of the Income Tax Reform" (OECD EO, December 1989). Uhl (2013) also points that the revenue effect of the Steuerreformgesetz 1990 took place in 1990 with a decrease of almost 15 billion euros in revenues. Uhl (2013) writes that this tax legislation was the "core part of an attempt to reform and modernize the German tax system [...] Based on the statements on the introduction of the bill, there is little doubt that the law was driven by structural considerations."

But the largest influence on the 1990 fiscal stance was unexpected and resulted from
the reunification process that started during 1990. In its 1991 Survey, the OECD (p.62-65) writes that "since unification, fiscal policy has become very stimulatory as a consequence of developments in the eastern Länder." A 1994 IMF study (PPAA/94/13) also notes that an important increase in transfers took place "to cushion the east German population against the dislocations brought about by the rapid transformation of east Germany’s centrally planned economy into a "social market economy" on the west German model."

"General government spending rose almost 10 per cent, within which current transfers, including transfers to the former GDR area, almost doubled. Large increases were also recorded for government consumption and interest payments, while cyclically sensitive social outlays and subsidies rose at a more moderate pace. As the integration process unfolded during 1990, three supplementary federal budgets - in February, DM 7 billion; in May, DM 5 billion; and October DM 84 billion - were required to provide bridging finance for the health insurance, pensions and unemployment benefits, and for transfers to the eastern German authorities. The last supplementary budget integrally included the ex-GDR public accounts for the second half of 1990."

In November 1990, the Federal Government announced its budgetary benchmarks for 1991, which required budgetary savings. In its 1992 Economic Developments and Issues, the IMF (p.16-17) notes that "action was taken in the first half of 1991 in order to achieve this target. In January, measures are announced to reduce the federal deficit by DM 37 billion, which included a reduction in the federal government transfer to the Federal Labor Office and an increase in contribution rates, higher transfers from the Post Office, and cuts in defense expenditure and subsidies. By February, it became evident that the financing needs of east Germany would be greater than anticipated. In addition, Germany had made a commitment to provide financial support for the war in the Middle East."

"Additional spending pressure emerged in the early months of 1991: the financial situation of the new Länder and communes was more critical than previously estimated. The commitment to the reconstruction of the eastern Länder was thus further strengthened by the announcement in March 1991 of the "Upswing East" package - worth DM 24 billion over two years - aiming mainly at accelerating infrastructure investment at the regional and municipal level."

An IMF study (PPAA/94/13, "Recent Fiscal Developments in the European Countries of the G-7") estimates that the change in the structural primary balance was -3.2% in 1990 and -1.7% in 1991.

**Germany, 2001**  In its 2002 Survey, the OECD (p.58) writes that the "fiscal policy was expansionary in 2001, with new business and income tax reductions having been phased in."

The fiscal impulse was largely expected. In fact, the June 2000 edition of the OECD Economic Outlook notes that "for 2001 the government has decided to bring forward reforms of income and company taxation, originally scheduled for 2002. The reforms include inter alia significant reductions in statutory tax rates for both personal and corporate income, and an increase in the basic income tax allowance. The tax base will be broadened, notably in terms of more restrictive depreciation rules. These new measures could lead to tax reductions
of around one per cent of GDP. Mainly on this ground, the government has revised its deficit targets presented to the EU (Stability Programme).

In a chapter on the German tax reform, the IMF writes that "tax reform has been a long time coming to Germany. [...] Now, however, Germany has embarked on fundamental reform. This process began last year with the Tax Relief Act of 1999/2000/2002, focused on the rate and allowance structure of the personal income tax (PIT), and culminated in a reform of business taxation that was approved by the Bundesrat on July 14, 2000. The reform package is a bold and sweeping attempt to deal with weaknesses of the pre-existing system that were being increasingly exposed by tax reforms both elsewhere in Europe and more generally" (IMF, Selected Issues, 2000, p.57).

Based on Finanzbericht, an annual budgetary report of the Federal Ministry of Finance, Uhl (2013) and Hayo and Uhl (2014) also classify the reduction in tax liabilities enacted from December 1999 to June 2001 as exogenous. The legislation with the largest fiscal impulse was published in October 2000 (Steuersenkungsgeset - StSenkG) and mostly implemented in 2001. Uhl (2013) describes it as "one of the most extensive tax reforms in the history of the Federal Republic of Germany" with a reduction in the corporate tax rate from 40% on retained profits and 30% on distributed profits to a uniform rate of 20%.

To estimate the change in the cyclically-adjusted fiscal balance from the policy record, we use the 2003 (p.6) and 2004 (p.41) Staff Reports for the Article IV Consultations with Germany. To remove the impact of changes in Interest rate payments, we use the 2002 OECD Survey 2002 (p.63). Overall, we estimate that the fiscal impulse was -1.25% in 2001 and +0.1% in 2002. This is consistent with other estimates of from the IMF where "the package is [considered] sizeable. When fully implemented, the net effect is estimated to be a revenue loss-consolidated across all levels of government-of DM 62.5 billion, equivalent to around 1.5 % of current GDP" (IMF, Selected Issues, 2000, p.61). The OECD EO of June 2000 also forecast that "in structural terms, the deficit may rise from about 0.25 per cent of GDP in 1999 to just below 1 per cent in 2000 and to 2 per cent in 2001."

Ireland, 1978  
After stagnating in 1975, the Irish economy experienced one of the strongest recoveries among OECD countries in 1976 and 1977 (IMF Article IV, 1978). The new government that came to power in mid-1977, however, viewed the high structural unemployment rate as unacceptable (IMF Article IV, 1979) and considered that an increase in aggregate demand could help permanently decrease it. The government was also motivated by a desire to compensate for what it considered years of ‘massive under-spending’ by the previous government (OECD Survey 1979, p.19).

Following the publication in January 1978 of a White Paper on National Development setting out the Government’s views on the Irish economy’s medium term-term growth potential and in June 1978 of a Green Paper detailing the measures to attain these employment targets, fiscal policy in 1978 shifted in a sharply expansionary direction. (IMF Article IV, 1978). The development strategy called for a string initial fiscal stimulus designed to launch the economy into an accelerated growth path. It was assumed that the private sector would respond favorably to this stimulus, leading to increased government revenues and a gradual
reduction in the budget deficit (IMF, IV, 1979). "In addition to the job-creating measures, the budget provided for some increases in public expenditure and a number of taxation measures" (OECD 1978, p.21).

Based on OECD (1979, p.20), we estimate that the fiscal impulse was 2% of GDP in 1978.

**Ireland, 1995** The paper providing background information to the staff report on the 1996 Article IV consultation discussions (IMF, Recent Economic Developments, 1996) notes that "fiscal policy was expansionary, as reflected in an increase in the fiscal deficit in a period of above-trend growth (staff estimates indicate an increase in the structural deficit from 0.5 percent of potential output in 1993 to 2.2 percent in 1995)."

"In addition to the one-off factors that boosted revenue growth in 1994, the sluggish pace of budgeted revenue growth in 1995 reflected a number of discretionary revenue measures aimed at: increasing disposable income to preserve the consensus on promoting competitiveness through wage moderation; reducing the cost of employment creation, particularly at low income levels; and encouraging enterprise, particularly in the services sector, through the reduction in the standard corporation tax rate (from 40 percent to 38 percent) [...] The major discretionary expenditure measures adopted in the 1995 budget–increased social welfare benefits and the abolition of university fees–were expected to increase spending by 0.6 percent of GNP in 1995 and by 0.9 percent over a full year."

In Table 1, p.25, the change in the structural primary balance is estimated to be -1.7 percent of potential GDP for 1995.

**Ireland, 2001** "The budget documentation suggested that, ex ante, the fiscal stance was set to loosen this year [in 2001] and would thus be pro-cyclical. The EU also concluded in its review of the Stability Programme in January that the budgetary plans for 2001 are expansionary and pro-cyclical" (OECD Survey, 2001, p.54).

To explain the reasons behind this easing in fiscal policy, the 2001 OECD Survey (p.54) notes that the authorities felt "under pressure to cut the surplus [...] there are pressures to raise public sector wages, to increase expenditures on health and on education (at least to levels in other countries which are regarded as appropriate examples) and to invest at a more rapid rate, in particular on infrastructure. At the same time, the government has promised further steps to individualize the tax system, thereby lowering the number of people taxed at the highest rate, which would also be expensive."

Based on IMF Article IVs (2002, p.31; 2005, p.28), we estimate that the fiscal impulse was -3.4% in 2001 and -.9% in 2002.

**Italy, 1984-85** Fiscal policy was not initially intended to be stimulative. In its June 1984 edition of the Economic Outlook, the OECD still writes that "one of the goals of fiscal policy in 1984 is to hold the Treasury cash deficit in nominal terms [...] at its 1983 level." Accordingly, a consolidation of 0.3 percent of GDP was still forecast in June 1984. As it became clear that the government would not be able to attain this objective, the forecast
was revised to a decrease of 0.8 per cent of GDP in the structural balance in the December 1984 edition of the OECD Economic Outlook.

The same pattern happened in 1985. In the December 1985 edition of its Economic Outlook, the OECD writes that "the initial objective of the 1985 budget was to stabilize real growth of expenditure (excluding interest payments) and not to increase tax pressure in relation to GDP. The result was to be a deficit [...] equivalent in nominal terms to that of the previous year. This forecast deficit was scaled upwards in the course of the year [...] Some of the tax measures intended to augment revenue were not defined in time, while others, notably the amnesty for illegal construction, were introduced belatedly. Moreover, expenditure overshot the targets, most importantly for local authority spending following the May elections, and for pensions, following the increase in the minimum." Accordingly, fiscal forecast were revised from a positive change in the structural balance of 0.4 points of GDP in June 1985 (and 0.1 in December 1985) to a decrease of 0.9 points of GDP in June 1986.

In its 1985 Survey, the OECD writes that "the [budget] situation took a further turn for the worse in 1984". It was noted that direct taxes were decreased together with an important increase in expenditure. Wage expenditures, in particular, rose substantially, owing to a readjustment resulting from earlier wage agreements and the expansion in public sector employment (OECD 1985, p.42).

In its 1985 edition of Recent Economic Developments, the IMF writes that the increase in the fiscal deficit was due to "the pursuit of welfare objectives, irrespective of financial constraints" (p.34-35).

Based on the 1986 OECD Economic Survey (p.63), we estimate that the fiscal impulse was -2.1% in 1984 and -1.6% in 1985.

**Italy, 1998** In its 1999 *Article IV* Consultation with Italy, the IMF writes that “fiscal policy in 1998 is estimated to have had a slightly expansionary impact on economic activity." The December 1997 edition of the OECD Economic Outlook emphasizes, in particular, "revenue losses associated with the expiration of the temporary Europa tax and other one off measures introduced in 1997."

The December 1998 edition of the OECD *Economic Outlook* notes that "a slowing in the pace of fiscal consolidation [that] reflects the commitments by the government to increase capital spending in the south, to raise welfare spending for families in need, and to introduce a once-and-for-all tax bonus (the partial reimbursement of the 1997 Eurotax)."

Based on Table 5 of the 2003 OECD *Survey*, we estimate that the fiscal impulse was -1.4% in 1998, -0.3% in 1999, and -0.5% in 2000. Table 1 of the 2000 issue of the IMF *Article IV* gives a similar fiscal impulse (-1.3%, -0.1%, and -0.2%). The 2000 edition of the IMF *Special Issues* estimates that the impulse is -1.2% for 1998. The 2000 edition of the OECD *Survey* has an impulse of -1.1% for 1998, -0.1% for 1999, -0.4% for 2000. Among those, the impulses of -1.4% in 1998 and -0.3% in 1999 are selected.
Italy, 2000-2001  In both 2000 and 2001, the decline in the structural primary surplus reflected the combination of policy choices and of an inability to control the growth of expenditures.

In its 2002 Survey, the OECD writes that “the structural primary surplus worsened slightly [as] higher than expected revenues were offset by tax cuts implemented towards the end of the year.” Similarly, the 2003 IMF Staff Report for Article IV Consultations (p.10) points that "the structural primary surplus net of one-off measures had weakened [in 2001] reflecting a reduction in the tax burden not matched by primary spending cuts."

In its 2002 Survey, the OECD (p.43-46) notes that "confident that a deficit out-turn lower than targeted was in reach [...] the government announced its intention of implementing a number of tax rebates before the end of the year [...] Such rebates were justified by the tax reforms of the previous years, which had led to a widening of the tax base and some expansion of the formal economy."

The "planned concession of a fiscal bonus" (OECD EO, 12/2000) was not supposed to result in a loosening of fiscal policy according to the government. While noting "the possibility of a slight slippage from target in the absence of new structural measures to keep expenditures under better control," the December edition of the OECD Economic Outlook forecast an increase in the structural balance of 0.1 point of GDP.

This view was revised in the June edition of the OECD Economic Outlook. "For 2001, the OECD projects some further slippage from the official target [...] This reflects a less optimistic view as to the ability to control current expenditures (in particular for health) as well as the projection of slower real GDP growth than officially assumed. Taking into account the fiscal stimulus provided by the 2001 budget (tax cuts for households and business, increased benefits for lower-income groups and a revival in public work projects), OECD calculations point to a decline in the structural primary surplus of close to half a percentage point of GDP."

The 2003 OECD Survey estimates that the fiscal impulse was -0.5% in 2000, -0.6% in 2001, and +0.1% in 2002. The 2003 IMF Article IV (p.36) estimates the change of structural primary balance, net of asset sales to be -0.8% in 2000, -0.8% in 2001.

The 2004 IMF Article IV (p.33) finds that the fiscal impulse was -1% of GDP in 2001. These estimates are more likely to be accurate as they are "the structural primary surplus net of one-off measures." Indeed, the structural primary balance which does not remove one-off factors is close to the 2003 OECD Survey estimate. Therefore we choose a fiscal impulse of -1% in 2001. The 2004 IMF Article IV does not provide us with an estimate for 2000, hence we take the estimate of -0.8%, the structural primary balance net of asset sales, from the 2003 IMF Article IV.

Netherlands, 2001  In its Staff Report for the 2002 Article IV consultation, the IMF writes that "the sustained process of fiscal consolidation paused in 2001." In its June 2002 edition of the Economic Outlook, the OECD writes that the "substantial easing in fiscal policy in 2001 reflect[ed] the costs of personal income tax reform and expenditure of previous budget windfalls."
According to the 2000 OECD Economic Survey, "in the 1998 Coalition Agreement, the government agreed on an outline of the tax reform plans, and draft legislation was submitted to Parliament in September 1999. By creating a robust tax system, with a broader basis of assessment and lower tax rates, the reform aims at strengthening work incentives, and promoting environmentally sustainable development. The tax reform is partly financed by removing or limiting some of the existing tax deductions, replacing allowances and deductions by tax credits, and increasing indirect taxes such as VAT and environmental levies. In addition, the overall tax burden will be reduced by â€”2.3 billion."

In its 2004 Economic Survey (p.70), the OECD also notes that "windfalls on the revenue side were used to finance tax cuts in addition to the income tax reform." About "25 per cent of revenue windfalls were used to finance tax cuts [that] came on top of an income tax reform (estimated to have cut taxes by 0.5 per cent of GDP)." Windfalls on the expenditure side, which arise when expenditure falls short of that in the medium-term projections, were also used to finance new structural policies. "Hence, lower than expected unemployment outlays and interest payments during the boom were compensated for by higher growth in non-cyclical outlays for education, health care, safety and infrastructure."

In its 2004 Economic Survey (p.77), the OECD writes that that the structural balance deteriorated by roughly 0.8 per cent of GDP in 2001 and 2002 because of the spending of expenditure (0.5 percent of GDP) and revenue windfalls (0.3 per cent of GDP) and by 0.5 per cent of GDP because of the tax reform of 2001. It also notes that one-off factors that were unrelated to discretionary fiscal policy have reduced tax receipts between 2000 and 2002 by approximately 1 per cent of GDP. Altogether and assuming that the budget windfalls were spent uniformly over the 2001 and 2002, this gives a fiscal impulse of -0.9 per cent of GDP for 2001.

Portugal, 1987  Portugal grew strongly in the years following its accession to the European Community in 1986. According to the 1988 IMF Staff Report for Article IV Consultation (p.3), "the decline in tax revenues [in 1987] reflected primarily adjustments in the rates of some taxes and the impact of generous fiscal incentives to investment. The budget also continued to provide a stimulus to consumption through a significant growth of the civil servants’ wage bill and through an increase in social benefits."

The IMF argued that "the [1987 budget] was based on the same policy priorities set by the Government in the 1986 budget, namely the promotion of a sustained investment effort; a positive growth of private consumption" (IMF, RD, 1987, p.52).

Based on the 1992 OECD Economic Survey (p.50), we estimate that the fiscal impulse was -2.8% in 1987 and +0.6% in 1988.

Portugal, 1990-1991  In its 1992 Survey, the OECD notes that "fiscal policy switched to an expansionary stance in 1990 and 1991." It was only in 1992 that, in accord with Maastricht accession criteria, a progressive move towards fiscal restraint was started.

During 1990-1991, there was a sharp rise in public expenditure, largely reflecting rapid increases in real public sector wages following a 1989 salary reform (IMF, IV, 1989). Rapid
wage and price inflation combined with the stable nominal exchange rate (after July 1990, the authorities shifted to a policy of managed floating) led a sharp real appreciation of the escudo.

"Current expenditure accelerated, reflecting in part the civil-servants’ salary scale reform of October 1989, which aimed at making their wages more competitive with those in the private sector and at improving the transparency of the salary grid. This reform is estimated to have added 120 billion escudos (1.3 per cent of GDP) to the general-government wage bill in 1990. Furthermore, social security transfers were boosted by the payment of a fourteenth month of pensions - a measure adopted in June 1990 and estimated to have amounted to about 1/2 per cent of GDP. [...] personal income tax revenues were affected by an ad hoc adjustment of the tax brackets and deductions exceeding the rate of inflation, both measures introduced in February 1990.

In its 1990 issue of Recent Economic Developments (p.28), the IMF writes that the government wanted to provide domestic counterparts to European transfers to maximize absorption. There was a desire to expand of education and health expenditures and to significantly increases in wages of civil servants.

Based on the 1992 and 1993 editions of the OECD Economic Survey (p.50 and p.45 respectively), we estimate that the fiscal impulse was -1.4% in 1990 and -0.9% in 1991.

Spain, 1978 According to the 1980 IMF Recent Economic Developments (p.19-21), "a sharp increase in the budget deficit" took place in 1978. In fact, the OECD (1984, p.17) notes that "the general government financial position [...] shifted from a small surplus in the mid-1970s to a deficit of almost 6 per cent of GDP in 1982. The cyclical component largely associated with rising economic slack and growing outlays for unemployment compensation is estimated to have amounted to 2 percentage points, the structural component accounting for the rest."

The OECD (1984, p.17) also notes that "the structural shortfall on the revenue side was essentially related to tax evasion and avoidance. Most of the increase in the structural deficit was therefore accounted for by higher spending. Improvements in the coverage of pensions and higher benefits, together with the fast growth in current subsidies and capital transfers were largely responsible for the structural expenditure overrun." According to the IMF, this reflected the a desire to improve and expand the coverage of social benefits (RD, 1980, p.19-21).

Based on the 1984 OECD Economic Survey (p.16), we estimate that the fiscal impulse was -1.2% in 1978.

Sweden, 1977-78 An important break in economic policy happened in 1977. In August 1977 the government withdrew from the European "currency snake," leading to a 10% effective depreciation. Additional measures included a switch from direct taxation to indirect taxation with a reduction in the payroll tax and an increase in the VAT tax from 17 to 21% (Caprio, 1982).

According to 1979 IMF Staff Report for the Article IV Consultation (p2-3), "the de-
terioration of the Central Government accounts had its roots in the commitments to full employment and improvements in social benefits and services, as well as in tax concessions designed to obtain more moderate wage cost increases, and a slow growth in the tax base."

We do not have estimates of the fiscal impulse from the policy records for this episode. Both the OECD (1980, p.30; 1981, p.26) and IMF (Art. IV, 1980, p.15; RD, 1980, p.26) refer to official Swedish estimates of "fiscal impact measures" or "short term impact of budgetary measures", but these measures include what is referred to as "static multiplier effects." According to these same documents, these estimates were taken from the National Institute of Economic Research (NIER) 1980 report "The Swedish Economy." We reached out to NIER and were able to obtain the report but could not find in it the relevant tables.

Sweden, 2001-02 In its 2002 Survey, the OECD writes that "fiscal policy was strongly expansionary last year and is set to be this year as well."

In its June 2000 edition of its Economic Outlook, the OECD still expected "the cyclically-adjusted surplus to rise by nearly 0.5 percentage point to 2 per cent of GDP" based on the announced spring budget for 2001. It, however, noted that "a decision on whether to cut taxes is to be taken in the autumn."

After the autumn and the decision to implement tax cuts amounting to 0.7 per cent of GDP, the OECD revised its projection in the December 2000 edition of the Economic Outlook and forecast the "the cyclically-adjusted surplus [...] to fall from 2.75 per cent of GDP this year to just over 2 per cent in 2001."

"Tax cuts accounted for the lion’s share of the fiscal easing in both 2001 and 2002, continuing the multi-year tax-cut programme initiated in 2000 [...] Property tax rates have been reduced in both 2001 and 2002 [...] These tax cuts, which cost approximately SEK 8 billion (0.4 per cent of GDP) per year, were implemented mainly to solve a political problem of the government’s own creation" (OECD Survey, 2002).

"It is less straightforward to account accurately for discretionary policy changes on the expenditure side of the budget, [...] However, the National Institute of Economic Research calculates that public expenditure decisions contributed 0.5 per cent of GDP to the fiscal easing in 2001 and a further 0.8 per cent in 2002. The single most important contributor to the overall easing this year [2002] is the newly instituted cap on childcare charges" (OECD Survey, 2002).

Despite "substantial discretionary easing of fiscal policies," some estimates of the change in the structural primary balance for 2001 as reported in the OECD Economic Outlook after 2001 show an improvement of around 2 percentage points of GDP. This, however, results from "exceptionally large, and lagged, payments of corporate and capital gains taxes [...] which may have been as large as 2 per cent of GDP" (OECD EO, 2002.1 p.104). For this reason, we correct these ex-post estimates by 2 points of GDP when necessary. The 2002 OECD Economic Survey also notes that "Ministry of Finance estimates suggest that [these exceptional factors] boosted revenues by 2 percentage points of GDP in 2001."

The document is available here https://www.konj.se/download/18.1f95a67a15ee31ea1c498b04/1507277612002/Konjunkturlaget-OKT-1980.pdf.
Removing these effects, "fiscal policy was [found to be] strongly expansionary [in 2001] and is set to be [in 2002] as well. According to estimates by the Ministry of Finance, calculated as the total value of all discretionary decisions, "fiscal policy stimulus amounted to approximately 1.75 percentage points of GDP in both 2001 and 2002" (OECD 2002, p.55).

**United Kingdom, 1983** Cloyne (2012) writes that "familiar themes were restated in the [March 1982] Budget speech: public borrowing “cannot make excessive demands on the funds available without putting upward pressure on interest rates." However, there was to be no large rise in taxation, in fact “I shall be proposing substantial reductions in taxation." According to Cloyne (2012), “this was not, however, a demand stimulus. In fact, the Chancellor opened his statement [on the March 1983 budget] by announcing that “these [tax cut] proposals will be consistent with our medium term strategy for effective control of the money supply, for lower public borrowing, and for further progress on inflation." Cloyne (2012) writes that “there was a specific section on unemployment and a series of microeconomic measures were announced to help combat it. But there was no return to the old view that the economy required a short term stimulus."

In its 1985 edition of *Recent Developments*, the IMF makes a similar assessment: “the stance of fiscal policy in 1983/84, which was originally projected to be contractionary, with withdrawal of stimulus estimated to be about 1/4 percent of GDP, turned out to be providing a stimulus in the order of 1.5 percent of GDP. The stimulatory stance of fiscal policy in calendar year 1983 is apparent regardless of the method used to estimate the cyclical impact (Table 22). The stance of fiscal policy in 1984, as measured on the basis of the revised estimate of the general government financial balance, is estimated to have continued to be expansionary, rather than shifting to a slightly contractionary stance as envisaged in the original budget." The OECD in its 1985 *Survey* also notes that “fiscal policy in 1983/84 [...] proved to be more expansionary than intended in the March 1983 Budget."

According to the 1985 edition of the IMF *Recent Developments* (Table 22, calendar years), the fiscal impulse was -1.3% in 1983, -0.7% in 1984, and 0.2% in 1985.

**United Kingdom, 2001** In its 2004 Economic Survey (p.31), the OECD notes that the UK "a more supportive fiscal policy [...] with the cyclically-adjusted balance, as conventionally measured, decreasing by around 3 per cent of GDP between 2000 and 2003." According to the IMF (Article IV, 2003), "over two-thirds of the five-percentage point deterioration in the fiscal position between 2000/01 and 2003/4 is estimated by staff as structural. It reflects primarily deliberate increases in spending on public services and unexpected shortfalls in tax receipts attributable to the bursting of the global equity bubble."

According to both the IMF and the OECD, the British authorities stressed that the fiscal relaxation was not motivated by counter-cyclical goals, but by the need to redress past under-investment (OECD 2002, p.41; IMF IV 2001, p.10). "The authorities were raising [expenditures] from levels they regarded as structurally low" as "attitudes to public spending seem now to have shifted markedly - demand for better public services in health,
education and transport was a central issue in the 2001 election, backed by an apparent greater willingness to pay higher taxes, if needed" (OECD Survey, 2002, p.43; IMF IV 2002, p.22). "The United Kingdom has endeavored to increase the quality of public services in three strategic areas where it lags behind countries with a similar degree of economic development: public infrastructure, education and health" (OECD Survey 2004, p.31, p.35, p.65).

Based on the 2005 OECD Economic Survey (pp. 31-33), we estimate that the fiscal impulse was -0.5% in 2001 and -1.8% in 2002.

**United States, 1982** The program was presented as a medium-term strategy designed to reverse the drift toward stagnation that had marked the performance of the economy over the previous one and a half decades. In the Administration’s view, such a deterioration in economic activity could be traced to: (i) a diminution of incentives for risk-taking, saving, investment, and work effort [...] (ii) the rising share of Federal outlays in total economic activity; (iii) an ever-mounting regulatory burden. (OECD 1981, p.9; IMF IV 1983, p.4. See also Romer and Romer, 2010).

According to the IMF (RED, 1984 p.39), "revenue declined from 20 percent in 1982 to 19.5 percent in 1983 largely as a result of the 10 percent personal tax cut that became effective in July of that year [...] The main measures that affected personal tax and non-tax payments included across-the-board reductions in individual income tax rates of 5 percent on October 1, 1981, 10 percent on July 1, 1982 and 10 percent on July 1, 1983."

Based on IMF estimates of the structural balance (RED 1986, p.42) and of net interest payments (RED, 1983, p.34; RED, 1985, p.43), we estimate that the fiscal impulse was -0.6% in 1982 and -0.9% in 1983.

**United States, 2002** According to Romer and Romer, 2010 the budget "bill embodied a temporary, endogenous tax cut in 2001, and a permanent, exogenous tax cut in 2002."

In its 2003 Article IV Staff Report (p.6), the IMF notes "substantial reductions in income tax rates were legislated in June 2001; in March 2002 the legislation increased investment incentives and extended unemployment benefits; defense and security-related spending was also increased significantly in 2002 and 2003; while substantial additional tax cuts were legislated in May 2003." Altogether "these measures contributed to a massive shift in the federal government’s unified budget from a surplus of 2% percent of GDP in FY 2000 (October-September) to a deficit likely to reach almost 2.5 percent of GDP in FY 2003, a 5 percentage point turnaround in structural terms."

In its 2004 Economic Survey, the OECD (pp. 36-37) also notes that "following the two major rounds of tax cuts and investment incentives of June 2001 and March 2002, the revenue legislation passed in May 2003 brought forward many of the personal income tax reductions originally legislated in 2001, expanded the previously implemented investment incentives and reduced the taxation of corporate dividends and longer-term capital gains. The reductions in marginal tax rates on personal income in particular not only provided timely support for household incomes but should also have beneficial effects on labour supply in the long run."
However, the combination of sharp increases in government spending, income tax cuts and plunging tax revenues due to sharply reduced capital gains realizations have led to ballooning deficits at the federal level as well as to severe budget shortfalls at the state level."

Based on the 2004 IMF Article IV (p.4), we estimate that the fiscal impulse was -2.8% in 2002 and -2% in 2003.

D Implementation of the Blanchard Fiscal Impulse

We mostly use data on general government accounts from the OECD Economic Outlook No.100 (June 2016). For some countries, this release does not go far enough back. We therefore use the Economic Outlook No.84 (June 2008) for Australia, Austria, France, and Ireland and the Economic Outlook No.76 (June 2004) for Germany. As we choose to identify episodes up to 2008 it does not shorten our sample much.

Our calculation of the Blanchard Fiscal Impulse follows closely the implementation approach of Alesina and Perotti (1995) and Alesina and Ardagna (2010). In particular, we first isolate the components of government revenues and expenditures that need to be adjusted for cyclical variations. Once these components are adjusted according to Blanchard (1990)’s idea, cyclically-adjusted revenues and expenditures net of capital outlays are obtained. From this, we obtain a measure of the cyclically-adjusted primary balance that we call $BFI_T^P$.

The following variables are used:

- CAPOG: Net capital outlays of the government, value
- CGAA: Government final consumption expenditure, value, appropriation account
- GGINTP: Gross government interest payments, value
- GGINTR: Gross government interest receipts, value
- SSPG: Social security benefits paid by general government, value
- SSRG: Social security contribution received by general government, value
- TIND: Taxes on production and imports, value
- TOCR: Other current receipts, general government, value
- TY: Total direct taxes, value
- YPEPG: Property income paid by government, value
- YPERG: Property income received by government, value
- YPG: Current disbursements, general government, value (CGAA + SSPG + YPEPG + YPOTG)
- YPOTG: Other current outlays, general government, value
- YRG: Current receipts, general government value (TIND + TY + YPERG + SSRG + TOCR)

**Step 1:** Calculate expenditures and revenues that need to be cyclically-adjusted. For each country, define $R^1_t$ the portion of government revenues that needs to be cyclically adjusted as $R^1_t = TY_t + TIND_t + SSRG_t$. It is the sum of total direct taxes, taxes on production and imports and received social security contributions. For government expenditures, only paid social security benefits (SSPG) are included as expenditures that need to be adjusted for cyclical variations.

**Step 2:** Cyclically adjust the revenue and expenditure sides. For each country we regress $R^1_t$ and SSPG on the previous year unemployment and a time trend. From the regression we calculate the adjusted revenue and adjusted expenditure variables $R^U_t$ and $SSPG^U_t$.

**Step 3:** Construct adjusted total revenue and adjusted total expenditure. Adjusted total revenue ($R^*_t$) are obtained by adding property income received by the government net of interest rate receipts and other receipts to $R^U_t$. Adjusted total expenditure ($G^*_t$) are obtained by adding final consumption of expenditure, property income paid by the government net of interest payments, other outlays and net capital outlays to $SSPG^U_t$.

- $R^*_t = R^U_t + (YPERG_t - GGINTR_t) + TOCR_t$
- $G^*_t = SSPG^U_t + CGAA_t + (YPEPG_t - GGINTP_t) + YPOTG_t + CAPOG_t$

**Step 4:** Calculate $BFI^T_t$ as a measure of the change of the cyclically-adjusted primary balance.

$$BFI^T_t = [R^*_t - G^*_t] - [R_{t-1} - G_{t-1}]$$

where the (unadjusted) total revenue and total expenditure of the government are defined as follow:

- $R_t = TY_t + TIND_t + SSRG_t + (YPERG_t - GGINTR_t) + TOCR_t$
- $G_t = SSPG_t + CGAA_t + (YPEPG_t - GGINTP_t) + YPOTG_t + CAPOG_t$

When our own calculated $BFI$ measure is compared to the change in the CAPB as calculated directly by the OECD, the variable NLGXA from the OECD Economic Outlook is used.

**Special cases:** Editions No.84 and No.76 of the OECD Economic Outlook, which we use respectively to obtain data for Australia, Austria, France, Ireland and Germany do not contain data on interest payments/receipts (GGINTP/ GGINTR). For these countries, interest payments are thus not removed from adjusted revenues and expenditures. This data limitation is, however, unlikely to affect our findings because of our focus on particularly large shift in the $BFI$ measure. Data on social security contributions received by the government are also missing for Australia. In this case, adjustable revenue is thus $R_t = TY_t + TIND_t$.