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**Joint Estimates of Automatic  
and Discretionary Fiscal Policy :  
The OECD 1981–2003**

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## **Joint estimates of automatic and discretionary fiscal policy: the OECD 1981-2003**

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### **Abstract**

Official calculations of automatic stabilizers are seriously flawed since they rest on the assumption that the only element of social spending that reacts automatically to the cycle is unemployment compensation. This puts into question many estimates of discretionary fiscal policy. In response, we propose a simultaneous estimate of automatic and discretionary fiscal policy. This leads us, quite naturally, to a tripartite decomposition of the budget balance between revenues, social spending and other spending as a bare minimum. Our headline results for a panel of 20 OECD countries in 1981-2003 are .59 automatic stabilization in percentage-points of primary surplus balances. All of this stabilization remains following discretionary responses during contractions, but arguably only about 3/5 of it remains so in expansions while discretionary behavior cancels the rest. We pay a lot of attention to the impact of the Maastricht Treaty and the SGP on the EU members of our sample and to real time data.

AEA classification: E62, E63

Keywords: automatic stabilization, discretionary fiscal policy, government social and health spending, Maastricht Treaty, Stability and Growth Pact, real time reaction functions.

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## **1. Introduction**

Official figures for cyclically adjusted government budget balances rest on the assumption that automatic fiscal policy essentially works through taxes and unemployment compensation. The major international organizations, including the OECD, the IMF and the EU, and many national governments within the OECD proceed on this assumption. In a review article, Golinelli and Momigliano (2009) provide a long list of econometric studies that employ the resulting official data for cyclically-adjusted budget balances. Yet there is little theoretical and empirical support for this approach. Various other components of government spending besides unemployment compensation may, in principle, respond automatically to the cycle. Workers may retire earlier in recessions and later in expansions. The evidence for the OECD indicates that they do. People facing job loss or temporary lay-off during a recession may be eligible for invalidity benefits or sick pay as an alternative to unemployment benefits. There has been a trend toward greater spending on invalidity benefits in many OECD countries in recent decades. Therefore, invalidity benefits and sick pay might now move in sync with unemployment compensation over the cycle. There is evidence that invalidity payments do. Finally, according to the indications, personal health care varies counter-cyclically or, in other words, inversely with the opportunity cost of time, which is higher in expansions than recessions. Consequently, social spending on health care moves in a stabilizing manner over the cycle.<sup>1</sup> In Darby and Melitz (2008), we discussed this reasoning and the evidence at some length and we shall confirm our earlier findings below. As a result, official figures for cyclically adjusted government deficits should not serve in studying discretionary fiscal policy. Unreliable inferences about discretionary policy will follow since the officially adjusted figures fail to remove all the automatic responses of the fiscal aggregates.

Admittedly, not all studies proceed in this way, even when they do not question the usual view. Many studies of discretionary fiscal policy apply various filters to construct their own cyclically-adjusted budget balances. See, for example, Alesina et al. (2002), Lane (2003), Aghion and Marinescu (2007), Beetsma and Giuliadori (2010) and Alesina and Ardagna

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<sup>1</sup> This spending refers to payments for insured health services provided to individuals or spending on entitlements, not to the wage bill or capital expenditures in a nationalized health sector. The latter are not included in the social spending data.

(2010). In these cases, the cyclically adjusted balances will reflect all responses of government social spending to the cycle and our previous complaint does not hold. The only objection we still retain concerns the attempt to construct cyclically adjusted data as a preliminary prior to the study of discretionary fiscal policy responses. This two-step procedure makes some untested assumptions about independence in the data. Joint estimation of automatic and discretionary fiscal policy action over the cycle is better. This then defines the primary objective of our study: to study both automatic and discretionary fiscal policy jointly. However, this objective leads to another. If automatic and discretionary fiscal policy are to be estimated jointly, it becomes difficult to justify dealing with the government surplus as a single aggregate, rather than separating out some of its parts. The automatic responses of fiscal policy to the cycle differ too widely on the revenue and expenditure sides. Social spending might also need to be treated separately rather than lumped together with the rest of government spending, or alternatively deducted from revenues after trimming social spending down to transfer payments in order to construct *net* taxes. So the minimum decomposition of the government budget therefore becomes a central issue as well. We shall attempt to study automatic and discretionary fiscal policy jointly with the minimal decomposition of the government budget for a sample of 20 OECD countries over the period 1981-2003.

We are not the first to propose estimating automatic and discretionary fiscal policy jointly: Celasun et al. (2007) and Bernoth et al. (2008) do so too.<sup>2</sup> Both of them simply use a different identification rule than ours for distinguishing between automatic and discretionary fiscal policy. Celasun et al. suppose that all common effects on primary surpluses across countries are automatic while the idiosyncratic national ones are discretionary. Bernoth et al. separate

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<sup>2</sup>There are also many studies of fiscal policy that simply make no distinction between automatic and discretionary responses and use unadjusted budget data as the dependent variable (though they may sometimes draw inferences about one sort of response or the other based on the results, for example, the source of influence on the budget). See for example Arreaza et al (1999) and Balassone and Francese (2004) or Balassone et al. (2008). Celasun et al. (2007) and Bernoth et al. (2008) are simply the only previous studies, to our knowledge, that explicitly try to estimate automatic and discretionary fiscal policy simultaneously from the start. In addition, Romer and Romer have recently proposed a new approach to discretionary fiscal policy responses to the cycle, their so-called “narrative” approach that, in principle, permits studying this one aspect of discretionary fiscal policy independently of automatic fiscal policy. Thus far, they have applied their approach only to tax policy in the US (Romer and Romer (2009, 2010)). But the IMF has already extended their approach to government spending and a wide international sample of countries (IMF (2010)).

automatic and discretionary responses to the cycle based on the differences between real time and final data. More precisely they use the differences between real time and final data about the output gap to identify discretionary responses while employing the final data to capture the automatic responses. We base our identification strategy on the timing of responses to the business cycle. By and large, automatic fiscal policy occurs more quickly than discretionary fiscal policy. Accordingly, our basic procedure is to try to capture the difference between the current calendar year response of different budgetary items to the cycle and the lagged one-year response. No simple decomposition of the automatic and the discretionary responses follows, since some current fiscal responses may be discretionary and some lagged responses may be automatic. Indeed, there are grounds to think that some effects of the business cycle on revenues and unemployment compensation occur only after the current year based on existing legal rules, and this is perhaps especially true for unemployment compensation if, as in our case, the relevant definition of the cycle is the OECD one, which rests on the output gap rather than the Blanchard (1990) one, which rests on unemployment. However, only the results can tell how serious the ambiguities are. We shall reason, based on the results, that there are few ambiguities. A fairly clear distinction between automatic and discretionary fiscal policy responses will emerge.

There is now also an important movement afoot to use real time data to identify discretionary fiscal policy responses to the cycle, to which Bernoth et al. belong. Except for them and Kalckreuth and Wolff (2007), the other authors in this movement begin with the official figures for cyclically adjusted budget balances. See Forni and Momigliano (2005), Golinelli and Momigliano (2006, 2009), Cimadomo (2007), Giuliadori and Beetsma (2008), and Beetsma and Giuliadori (2008). Yet it is not clear to us that the real time data is necessarily superior to the final data. Observers and officials differ in their perceptions of current output gaps, and decision-makers know that current output gap data are subject to large revisions. Therefore, discretionary fiscal policy may rest on a broader assessment of the evidence than the real time figures published by the OECD for the current output gap provide. As a result, the lagged values of the final data that eventually emerge could provide as good or better

grounds for analyzing official intentions than the real time data does. This is an open issue, in our eyes. A further consideration is that real time data are not available for our entire study period but only for a sub-section, since 1993, so its exclusive use would have limited our study greatly. We will use the available post-1993 sub-sample to check on our conclusions about automatic and discretionary fiscal policy in our full sample.

Contrary to many studies of fiscal policy, though not all (see Golinelli and Momigliano (2009)), we estimate our fiscal reaction function in first differences. One reason is the evident non-stationarity of the data in levels. Another is our primary concern with the temporary fiscal responses to the cycle. Many structural factors will affect the budget balance in levels, but these structural influences should matter less in first differences, while the cyclical influences should remain as important. We also avoid the introduction of the lagged dependent variable, which figures in many other works. Apart from the well-known objections to this variable in panel estimation based on statistical bias, the variable would greatly complicate our distinction between the automatic and discretionary policy responses.

Several earlier studies of fiscal policy behavior employed a dynamic panel estimator, Blundell-Bond. However, ours is an unbalanced panel of only 23 annual observations at most for 20 countries; so such estimators are not attractive.<sup>3</sup> For panels such as ours, we believe a simpler version of GMM is superior, namely, an IV-GMM procedure providing standard errors that are robust to serial correlation and that make no assumptions about heteroskedasticity (see Baum et al. (2003) on this point). In the end, we also find that simple IV estimates with correction for heteroskedasticity yield results that are virtually

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<sup>3</sup> These studies may have been motivated by a desire to avoid large sample bias in panel estimation resulting from the correlation of the lagged dependent variable with the error term (see Nickell (1981)) since the studies tend to introduce the lagged dependent variable. It is however important to remember that dynamic panel data (DPD) techniques, including Blundell-Bond's system GMM estimator, were designed for panels with many cross-sectional units  $N$ , whether countries, firms or people. The consistency of the DPD estimators was first established under the assumption of a fixed time dimension  $T$  but  $N$  tending to infinity. More recently, however, Bun and Kiviet (2006) examined the performance of a number of dynamic panel techniques in samples where both  $T$  and  $N$  are only moderate or small, as is true in our tests, and reported that in these circumstances "all dynamic panel techniques show substantial bias... so standard first-order asymptotic theory is of little use in ranking the qualities". Furthermore Cameron and Trivedi (2005) warn that the application of Blundell Bond style estimators to panels with small  $N$  can, in practice, lead to a large loss of efficiency while masking problems associated with weak instruments. This last warning concerns us since we instrument.

indistinguishable from IV-GMM estimates.<sup>4</sup> Since there is therefore only negligible gain in efficiency from IV-GMM, simple panel IV results are the ones that we report below. Following Galí and Perotti (2002), virtually all studies of fiscal policy behavior tend to instrument the output gap in order to avoid simultaneity bias. We do the same.

As a final introductory note, we may return to the issue of the proper degree of aggregation. It is generally recognized that taxes move up with output regardless of cyclical upswings or secular growth whereas government consumption and investment may not go up with cyclical upswings but only with economic growth. Moreover, some parts of social spending can be expected to go down during a boom while going up with secular growth. This is true of unemployment compensation, and based on our opening paragraph, might be true as well of other parts of social spending. On these grounds, any single-aggregate approach to discretionary and automatic fiscal policy cannot be simply taken for granted and could well be overly restrictive and possibly misleading.

The next section will lay out our fundamental econometric model. Following, we shall offer separate estimates of automatic stabilization for unemployment compensation and other elements of social expenditure. These estimates will proceed from our fundamental approach, but they have no purpose other than to underline our opening message of the need to extend the analysis of automatic stabilization beyond unemployment compensation to many other parts of social spending. Our preferred estimates of automatic stabilization follow in the next section, IV, where we consider automatic and discretionary fiscal policy jointly. In that section we also treat social expenditures as a separate aggregate. Section V introduces asymmetric responses to expansions and recessions over the cycle. The results of this section notably qualify those in the preceding section IV. Section VI next considers the effects of the Maastricht Treaty and the Stability and Growth Pact in the EMU. Section VII discusses the impact of real time data. Section VIII concludes.

### *A summary of the results*

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<sup>4</sup> We carried out both sets of IV estimates with Stata 11, using `ivreg2` routines to generate the IV-GMM ones; see Baum et al. (2010), and Roodman (2003, 2009).

Our baseline estimate of automatic stabilization is .59. Specifically, a one percent increase in output relative to potential output raises the net primary surplus by .59 of one percent of output. Given our choice of specification in terms of ratios of output, the results imply that any contribution of government receipts must hinge on changes in the progressivity of taxation over the cycle, for which we find inadequate evidence. Thus, the estimated stabilization shows up essentially on the spending side (compare Arreaza et al. (1999)). According to our estimate, a one per cent rise in output relative to potential output raises government spending on goods and services exclusive of health by .24 of a percentage-point of output and raises social expenditures by .35 of a percentage-point of output. However, in the following year, discretionary policy offsets the entire automatic response stemming from government consumption in an expansion, possibly entirely, whereas no similar offset takes place in a contraction. As a result, the .59 automatic stabilization remains standing following discretionary action in a contraction whereas as little as .35 may remain in an expansion. In addition, though the total stabilization coming from both automatic and discretionary policy combined is divided between social spending and other government spending roughly in a ratio of 3:2 in a contraction, during an expansion still more of the stabilization and possibly all of it comes from social spending (compare Hercowitz and Strawczynski (2004)). This asymmetry in the response of fiscal policy must induce deficit spending over the cycle, in accordance with the widespread thesis of a deficit bias over the cycle (see Roubini and Sachs (1989), Grilli et al. (1991), Alesina and Perotti (1995) von Hagen and Harden (1995), de Haan et al (1999) Kontopoulos and Perotti (1999), and Velasco (1999)). Our finding also supports the frequent view that EU members tend to neutralize automatic stabilization through discretionary fiscal policy during expansions (see Buti and Sapir (1998), Fatas and Mihov (2003a, b), Balassone and Francese (2004), and Balassone et al (2008)). Note, however, that we do not support the extreme conclusion, occasionally found, of a complete neutralization of automatic stabilization during expansions.

To continue with the discussion of the results for stabilization policy, we find no “fiscal drag” resulting from inflation over the cycle. This “drag” refers to the idea that current inflation



automatically leads to net government surpluses on a secular basis, as taxes move up in step with inflation – or even faster through “bracket creep” – whereas spending does not. According to our results, there is indeed a “fiscal drag” on the spending side as neither government social payments nor government consumption keep up with inflation. But there is a comparable “drag” on the revenue side as taxes do not keep up with inflation either. It all comes out even in the wash.

Besides the destabilizing behavior of discretionary fiscal policy during expansions, two other discretionary influences of fiscal policy appear. One is a stabilizing response of the net primary surplus to government debt. A one-percentage-point rise in debt relative to output raises the primary surplus (conservatively) by .022 of one percentage-point of output. This accords with much recent work (see Melitz (1997), Bohn (1998), Ballabriga and Martinez-Mongay (2002), Galí and Perotti (2003), Annett (2006), Mendoza and Ostry (2007), Balassone et al. (2008), Candelon et al (2010)).

The second discretionary influence that we found is the impact of an election year. In line with a great deal of earlier research, in an election year, tax revenues tend to fall (see Annett (2006), Golinelli and Momigliano (2006), Candelon et al (2010)). If anything, the effects on government consumption and social spending are positive but insignificant at conventional levels. No other political influence emerges, despite the fact that many other such influences appear in the rich literature on the political influences on government deficit spending (for references, see Annett (2006), and, for example, Hallerberg and von Hagen (1999)). Perhaps the failure of these variables to enter is related to our move to first differences which eliminates all country-specific fixed effects on levels of government receipts and expenditures.

For similar reasons, which may be magnified by our instrumentation of output gaps, we have had no success finding any impact of the Maastricht Treaty and the SGP on the fiscal behavior of EMU members, even in the 1993-1998 period, when efforts to qualify for entry into EMU are widely believed to have reduced fiscal deficits.

Finally, our key results survive when we introduce real time data despite the consequent sharp reduction in time-span. We cannot say anything about asymmetry in this case, since regardless of final or real time data, the results are too weak for the shorter period. But otherwise our estimates of automatic fiscal policy and our conclusions about discretionary fiscal policy responses to the cycle are little affected.

## **II. The test specification**

### *Preliminaries*

Official estimates of automatic stabilization proceed in levels and provide the number of cents of net government surplus resulting from a dollar of output gap (GDP minus potential GDP). By contrast, estimates of discretionary fiscal policy responses to the cycle usually proceed in ratios. They provide the fraction of a percentage point of net government surplus relative to (divided by) output or potential output resulting from a percentage point of output gap relative to (divided by) output or potential output. All the studies we cited earlier that construct their own cyclically-adjusted data for the net primary surplus proceed in ratios. In the numerous instances where the choice is simply to import the official series for cyclically adjusted budget balances (which are in levels), the tendency is to divide by output or potential output as a separate step prior to turning to the analysis of discretionary fiscal policy (for example, Galí and Perotti (2003)). Since we estimate automatic and discretionary policy simultaneously, we must make a choice. Ours is to use the percentage formulation. While this changes nothing fundamental, it does dwarf the contribution of receipts to automatic stabilization relative to the contribution of spending. Simple as it is, this point deserves major attention.

Take the simple classroom example of proportional taxation and total independence of spending from the cycle. If we proceed in levels, taxes respond to the output gap in a stabilizing manner and government spending does not. Taxes rise while government spending stands still in an expansion. If we proceed in ratios, spending responds to the output gap in a stabilizing manner whereas taxes do not. The ratio of government spending to output falls while the ratio of government receipts stands still in an expansion. The fundamental truth is

that the stabilization depends entirely on the combination of proportional taxation and cyclical inertia of spending in both examples. Without both together – and in particular if spending automatically followed output over the cycle like taxes – then there would be no automatic stabilization regardless of estimate in levels or ratios in the case of a balanced budget and with minor qualification otherwise.

We will therefore attach little significance, if any, to our results about the lack of contribution of taxes to automatic stabilization relative to government expenditures. On the other hand, our results for the behavior of different sources of government revenue relative to one another will deserve emphasis. So will our results about the relative contributions of different components of spending relative to one another. This last point, concerning spending, deserves particular emphasis. We see reason to expect some *automatic* counter-cyclical movement of social spending in *level* form and we see no reason to expect any *automatic* counter-cyclical movement of government consumption in *level* form. Therefore, we expect a higher contribution of social spending to automatic stabilization than government consumption in level form. But if so, we must also expect the same in ratios or after we divide both spending and output gaps by output.

### *The test specification*

With these points in mind, our basic estimating equations for the fiscal policy reaction functions can be expressed as follows:

$$\Delta \left( \frac{X_{ijt}}{Y_{jt}} \right) = \alpha_i + \alpha_{it} + \beta_{i1} \Delta \left( \frac{Y_{jt}}{Y_{jt}^*} \right) + \beta_{i2} \Delta \left( \frac{Y_{jt-1}}{Y_{jt-1}^*} \right) + \beta_{i3} \Delta \pi_{jt} + \beta_{i4} \left( \frac{D_{jt-1}}{Y_{jt-1}} \right) + \beta_{i5} elec_{jt} + \varepsilon_{ijt} \quad (1)$$

$X$  refers either to the primary budget surplus or the disaggregated components of the primary budget surplus. In case of disaggregation, there are as many equations (1) as components  $i$ . Subscript  $j$  refers to the country and  $t$  to time.  $\alpha_i$  refers to time fixed effects.  $Y$  refers to GDP,  $Y^*$  to potential GDP,  $\pi$  to inflation (calculated using the GDP deflator),  $D$  to total public debt, and  $elec$  is a dummy variable equal 1 in the year of a national election in country  $j$  and zero

otherwise. As indicated previously, we expect  $\beta_{i1}$  to be positive for the primary surplus and for government spending, if only because of automatic effects, but we are agnostic about it for revenues and we are agnostic about  $\beta_{i2}$  in general. The reason for using the primary budget surplus rather than the observed budget surplus as the dependent variable (either as a single aggregate or as the sum of the relevant parts) is our interest in  $\beta_{i4}$ , that is, the response of the budget to the previous year's debt to GDP ratio. Given this dependent variable, Bohn (1998, 2005) has shown that if the equation for the primary surplus is properly specified or contains all the proper explanatory variables on the right hand side, a positive value of  $\beta_4$ , however low, is a sufficient condition (not a necessary one) for the solvency of the government. We experimented with a number of other political and economic variables besides *elec* that move over time, including openness, but the dummy variable for the years of a national election, *elec*, is the only one that we retain. As we noted earlier, many studies have provided strong political reasons to expect  $\beta_{i5}$  to be negative (in its effect on the primary surplus). Earlier empirical work would also lead one to expect negative values for this coefficient as well as positive ones for  $\beta_{i4}$  (in their respective effects on the primary surplus) for OECD countries.

We have already explained our decision to exclude the lagged dependent variable from the regressions, both for econometric reasons and because of our intended interpretation of  $\beta_{i1}$  and  $\beta_{i2}$ .<sup>5</sup> In addition, as a result of our decision to proceed in first differences, there is no need for country-specific influences. Such influences would merely use up degrees of freedom but otherwise have no effect, at least in principle (see Cameron and Trivedi (2005, pp. 781ff.) and Roodman (2009)).<sup>6</sup>

We initially instrumented all the first four variables in equations (1) in the estimates. However, test statistics consistently failed to reject the weak exogeneity of the inflation variable. Thus, in the end, we only retained instruments for the two output gap variables and the debt/GDP ratio. These instruments are 2, 3 and 4-period lags of  $(Y/Y^*)_t$  and 2 and 3-

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<sup>5</sup> We did experiment with the second lag in the dependent variable, since at this lag length there would be no clear interference with our intended interpretation of  $\beta_{i1}$  and  $\beta_{i2}$  and the variable could reflect omitted influences in the rest of the equation. At this lag length, the lagged dependent variable is insignificant, whether we instrument it or not.

<sup>6</sup> We added country-specific effects in Darby and Melitz (2008) but they indeed made no difference.

period lags of  $(D/Y)_{t-1}$ . This economy of instruments proved valuable in estimation. Our basic data sources are the OECD, the *Economic Outlook* and *Social Expenditure* databases, except for *elec* which comes from Armingeon et al. (2008).

The twenty countries in the sample are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, Japan, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, the UK and the US. Ours is an unbalanced panel for 1981-2003 at best. We could have extended the study prior to 1981 and beyond 2003 had we not been keen on using a series for social spending inclusive of health expenditure.<sup>7</sup> Our measures of  $Y^*$  and the output gap  $Y/Y^*$  rely upon the OECD's measure of potential output. But we have experimented with HP filtered data and the use of cubic spline functions to represent the long run secular trend in output or potential output instead.

### III. The automatic responses of social spending

We begin discussion of the results with a simple confirmation of our starting message that automatic stabilization issues from many parts of social spending and not only unemployment compensation. Consider equations (1) for the spending items  $X_i$  consisting of unemployment compensation, pensions, health spending, incapacity benefits and sick pay after dropping all explanatory variables reflecting strictly discretionary action, and also dropping the lagged output gap. Dropping the lagged output gap could well remove some automatic stabilization. Yet even if it does, the resulting estimates should still reflect automatic responses alone, since the responses of social spending to the current output gap clearly depend on the application of standing legal rules and we know of little evidence of short-term adjustment of these rules to the cycle. The resulting equations are:

$$\Delta \left( \frac{X_{ijt}}{Y_{jt}} \right) = \alpha_i + \alpha_{it} + \beta_{i1} \Delta \left( \frac{Y_{jt}}{Y_{jt}^*} \right) + \beta_{i2} \Delta \pi_{jt} + \varepsilon_{ijt} \quad (2)$$

Table 1 shows the separate IV estimates of equations (2) for the spending items in question.

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<sup>7</sup> Specifically, the series we use for government spending on health benefits comes from the OECD *Social Expenditure* database. This database is less frequently published than the *Economic Outlook* one and we accessed the 2007 release that only provides data going up to 2003.

The table only shows the estimates of  $\beta_{il}$  or the automatic responses to the cycle. Standard errors are robust to heteroskedasticity and autocorrelation, and  $\Delta(Y/Y^*)$  is instrumented in the manner previously described and repeated at the bottom of the table.

Of course, these are not our preferred estimates of automatic stabilization, which only follow from equations (1) where discretionary fiscal policy enters simultaneously. However, the estimates of equations (2) are relevant in discussing the popular view that unemployment compensation is the only major category of social spending that responds automatically to the cycle since this view generally rests on estimates of automatic stabilization that exclude any discretionary policy. We must also emphasize that the conclusions in table 1 do not depend on our detailed estimation procedure. They would also follow if we used alternative measures of potential output depending on HP-filtered data or after approximating long term trend output via a spline function. They would similarly follow just as well if we introduced the lagged dependent variable or if we used 3SLS. Our previous work (Darby and Melitz (2008)) already cast much light on this point.

As seen from table 1, based on equations (2), the contribution of social spending to automatic stabilization is .34 of a percentage-point relative to output. Consider instead the contribution of unemployment compensation by itself. We get a value of only .08. This last contribution thus makes up only about a quarter of the response of total social spending in the current period. Separate estimates for the other elements of social spending follow. They show a contribution of .12 of a percentage-point for pensions and one of around .06 of a percentage-point for health spending. Given the confidence intervals, both figures are of a similar order of magnitude to the one for unemployment compensation. The contribution of incapacity benefits is also clear, though much smaller than any of the preceding. Sick pay is the one category of social spending in Table 1 whose contribution to automatic stabilization does not emerge. The sum of the individual contributions in the table does not add up to the total for social spending partly because our disaggregated estimates omit subsidies to firms and other miscellanea.

#### IV. Automatic and discretionary fiscal policy combined

We turn next to the central issue in this study of the joint estimation of automatic and discretionary fiscal policy. Let us begin with the case of symmetric responses to cyclical expansions and contractions, meaning equations (1). The relevant estimates are summarized in Table 2. There will be some important modifications once we allow for asymmetry, but these will strictly concern government consumption spending net of health. Table 2 offers estimates of five selected equations. The first one (Part A) relates to the net primary surplus and the next four pertain to our basic decomposition, which is between government receipts (Part B), social expenditures including health (Part C), government consumption exclusive of social health spending (Part D), and government investment (Part E). The equation for investment is extremely unstable. None of its coefficients are significantly different from zero apart from the  $-0.01$  for the debt ratio; and we shall center the discussion on the other four equations. We shall also refer henceforth to government consumption exclusive of social health spending simply as government consumption.

Let us focus first on the general test statistics. The performance of the instruments is satisfactory in all four equations. There is no problem of under-identification. Furthermore, the Hansen test of the joint null hypothesis that the instruments are uncorrelated with the error term and that the excluded instruments are correctly excluded from the estimated equation, is highly satisfactory. Interestingly enough, the tests of endogeneity reveal less need for instruments than the use we make of them. The tests unambiguously fail to require rejecting the null hypothesis of weak exogeneity of the contemporaneous output gap in all of the equations except social spending. In the case of the debt/GDP ratio too, these tests reject the null hypothesis of weak exogeneity in all the equations (with any ambiguity at all only in the consumption one).

As regards the individual coefficients, let us consider first those reflecting the responses to the business cycle,  $\beta_{i1}$  and  $\beta_{i2}$ . We shall also look carefully at the sum of these two coefficients,  $\beta_{i1} + \beta_{i2}$ , which concerns the total fiscal policy response to the cycle without any regard for the time profile or the distinction between automatic and discretionary action. This sum and

its standard error both appear in the table right below all the individual coefficients. In the case of the first four equations (Parts A through D),  $\beta_{i1} + \beta_{i2}$  is highly significant except for revenues. In the case of the primary surplus, Part A, this sum is also extremely high, .73. However, in Part A neither  $\beta_{i1}$  nor  $\beta_{i2}$  is individually significant at conventional levels. This might be related to the result for revenue, in Part B, which shows opposite signs for the current and lagged responses. There is a stabilizing lagged response  $\beta_{i2}$  offsetting a (less well defined) destabilizing current one  $\beta_{i1}$ . If accepted, this see-saw movement could be interpreted at least in two ways: a discretionary offset of an initial destabilizing automatic effect or a lagged automatic offset of an initial destabilizing automatic effect. We lean toward the latter interpretation. As mentioned earlier, some tax revenues can be expected to respond to the cycle with a one year lag based on existing tax legislation. This could cause revenues not to keep up with cyclical movements in output at first but to catch up a year later. On this interpretation, the results for revenues correspond to proportional taxation following a year (since  $\beta_{i1} + \beta_{i2}$  is insignificant). We have studied direct taxes on households, direct taxes on firms, indirect taxes and social security revenues separately in order to see if this sheds any more light on the issue but it does not. The same basic time profile and aggregate outcome emerges for each separate category of revenues.

The results for government social spending and government consumption are the central ones. In both cases, there is a well-defined response to the business cycle, which is entirely concentrated in the current period. We see little reason to question in either case that the response is entirely automatic. As mentioned before, in the case of social payments, the rules of eligibility do not alter in a cyclical manner. As for government consumption, there is a priori ground to expect an automatic counter-cyclical movement of the ratio to output and if discretionary fiscal policy either amplified or attenuated this movement, we would have expected the discretionary action not only to show up within the current year but also a year later. On these grounds, we shall interpret the results of Parts C and D of table 2 to signify strictly automatic stabilization, .35 coming from social spending and .24 more coming from consumption spending.



There is no evidence of automatic effects of current inflation based on the estimates of the primary surplus (Part A). This fits in well with the disaggregated results, showing significant offsetting effects of inflation on the revenue and spending sides of roughly equal size. Ratios of government revenues to output do not keep up with inflation but nor do ratios of taxes to output.

With respect to debt, a stabilizing discretionary response of .022 shows up based on the 3-part decomposition of receipts, social spending and consumption (which would be higher if we admitted the single significant coefficient in the equation for investment), .015 of it coming from taxes and less, only .007, from social spending. This is below the significant value of .032 stabilization that we obtain in the single-equation estimate for the government primary surplus (which may incorporate the figure for investment). In the light of Mendoza and Ostry (2007) in particular, we also looked for non-linear effects of debt but found nothing.

As regards elections, the tendency of an election year to lead to looser fiscal policy emerges plainly. A reduction in taxes is particularly clear. While any rise in government spending is not, the estimate for *elec* in the net primary surplus equation closely resembles the one we get by summing up the separate tax and spending coefficients and this value is also significant at the 95 percent confidence level. We are therefore prone to accept the conclusion that a national election raises the primary deficit relative to GDP by approximately one-half of one percentage-point of output, that is, roughly the amount shown in the primary surplus equation.

## **V. Asymmetric responses to the cycle**

Political considerations offer strong reasons to suspect there might be asymmetric responses to the business cycle, and more specifically, that fiscal discipline may relax substantially during expansions. We can allow for such responses by controlling separately for ‘good times’ (when  $Y > Y^*$ ) and ‘bad times’ when ( $Y < Y^*$ ). We shall do so by introducing three additional variables in equation (1). The first is a dummy variable  $\alpha_i^+$  which is equal 1 when  $Y > Y^*$  and equal 0 otherwise. This allows for a separate intercept in ‘good times’ ( $\alpha_i + \alpha_i^+$ ) and in ‘bad times’ ( $\alpha_i$ ). We also add two interaction terms resulting by multiplying this dummy by

the output gap terms. Those generate  $\Delta(Y/Y^*)_t^+$  and  $\Delta(Y/Y^*)_{t-1}^+$  values, which are simply the values of  $\Delta(Y/Y^*)_t$  and  $\Delta(Y/Y^*)_{t-1}$  when  $Y > Y^*$  and equal 0 otherwise. The new test equation is hence:

$$\begin{aligned} \Delta\left(\frac{X_{ijt}}{Y_{jt}}\right) = & \alpha_i + \alpha_i^+ + \alpha_{it} + \beta_{i1} \Delta\left(\frac{Y_{jt}}{Y_{jt}^*}\right) + \beta_{i2} \Delta\left(\frac{Y_{jt-1}}{Y_{jt-1}^*}\right) + \beta_{i3} \Delta\left(\frac{Y_{jt}}{Y_{jt}^*}\right)^+ + \beta_{i4} \Delta\left(\frac{Y_{jt-1}}{Y_{jt-1}^*}\right)^+ \\ & + \beta_{i5} \Delta\pi_{jt} + \beta_{i6} \left(\frac{D_{jt-1}}{Y_{jt-1}}\right) + \beta_{i7} elec_{jt} + \varepsilon_{ijt} \end{aligned} \quad (3)$$

An alternative approach to modeling asymmetric adjustment in some studies is to include separate output gap terms for ‘good’ and ‘bad’ times instead a term applying at all times and an additional term for in the output gap in ‘good’ times only, as we do. The implied responses in ‘good’ and ‘bad’ times are identical in both cases, but our approach has the advantage that the estimates of  $\beta_{i3}$  and  $\beta_{i4}$ , and the associated tests of significance, show directly whether there is a significant asymmetry while a separate test is necessary (for the significance of the difference between the coefficients in ‘good’ and ‘bad’ times) in the alternative approach. We also report an F-test of the joint restriction  $\alpha_i^+ = \beta_{i3} = \beta_{i4} = 0$  to test the null hypothesis of symmetry against the alternative of asymmetry.

In the estimates of equation (3), the results for the primary surplus are unacceptable based on the Hansen J statistic, whose probability value falls close to zero. The overidentifying conditions concerning the instruments can no longer be upheld. The results for government revenues, for their part, are similar to the previous ones. Allowing for asymmetry in this equation changes little. The results for social spending and consumption are the interesting ones. We present them in the first two columns of table 3 (A and B).

In both columns (A) and (B) it is clear that the joint F-tests for  $\alpha_i^+ = \beta_{i3} = \beta_{i4} = 0$  fail to reject the null hypothesis of symmetry. Notwithstanding, it is helpful to look more closely at the implied responses in ‘good times’ and in ‘bad times’, since this is our primary concern. As regards social spending, the automatic stabilization in ‘good times’  $\beta_1 + \beta_3$  appears to be just as significant as in ‘bad times’ (for  $\beta_1$  alone). In addition, the combined automatic and

discretionary action in ‘good times’  $\beta_1 + \beta_2 + \beta_3 + \beta_4$  differs little from the combined automatic and discretionary action in ‘bad times’  $\beta_1 + \beta_2$ . Therefore the hypothesis of symmetry seems sound. However, in the case of government consumption, things are different. The high probability value of .38 associated with the combined automatic and discretionary action in ‘good times’ would indicate insignificance, while the total effect in ‘bad times’ is highly significant, with a probability value of .00. This clearly suggests an important asymmetry. The next two columns of Table 3 investigate this possibility further.

Neither  $\beta_2$  nor  $\beta_3$  is significant in the case of government consumption in column B. Accordingly suppose we set both coefficients equal to zero. In column C, we set  $\beta_3$  equal zero, while in column D we additionally drop  $\beta_2$ . Thus, column C simply imposes symmetry in the current response to the cycle while column D additionally admits no lagged response to the cycle in ‘bad times’. As can be seen, the estimates in these next 2 columns hardly differ at all from column B. To be quite specific, the fiscal response in the current period, or the automatic one, varies only between .23 and .26, regardless of ‘good’ or ‘bad times,’ in all 3 columns, and the estimates for the total effect in ‘good times’ are similarly uniform and insignificant. Furthermore direct testing of the joint restrictions that are imposed by the successive deletions of the two terms starting from the column B specification shows that the restrictions cannot be rejected, as indicated by the F test. The key difference is that the relevant hypothesis of zero asymmetry becomes less and less probable as we move from column B to column C to column D. The probability-value for the F test of the corresponding symmetry null F test goes down from .51 to .32 to .13 from B to C to D. Symmetry becomes less plausible.

Based on the estimates of the simplified specification reported in column D (and the previous two columns as well for that matter), there would seem to be a significant asymmetry in the implied behavior of government consumption: we cannot even reject the hypothesis that discretionary action totally offsets automatic stabilization. It also makes sense to attribute such reversal to discretionary action. The alternative of supposing an *automatic* tendency for government consumption to go into reverse and cancel its own earlier stabilizing movement

from one year to the next does not seem plausible. We are also not necessarily justified, however, in going to the extreme conclusion that the discretionary offset of the automatic response of government consumption is complete. Indeed, the standard confidence interval indicates that a wide range of estimates of the combined automatic and discretionary effect centered on the reported point estimate, cannot be rejected by the data. On this ground, the most likely degree of reversal of the automatic effect is about half.

Based on the estimate for government consumption in column D of table 3, the earlier estimates of the other influences on government consumption (table 2, column D), or those for inflation, the debt ratio and the election years, are hardly affected.

If we combine the estimates for government revenues and social spending in table 2 in the previous section with those for government consumption in table 3 in this section, we get the basic results of our study that we reported in the introduction. There is .59 automatic stabilization, .35 coming from social spending and the other .24 from government consumption. The full .59 stabilization remains following the response of discretionary policy in a contraction, but arguably, only .35 (out of 1) remains after this response in an expansion.

## **VI. The Maastricht Treaty and the Stability and Growth Pact**

The Maastricht Treaty came into effect in 1993 and the Stability and Growth Pact (SGP) did so late in 1997 or basically in 1998. Starting with Ballabriga and Martinez-Mongay (2002) and Galí and Perotti (2003), a sizeable literature analyzes the possible effect of both on the fiscal policy behavior of the EU members, the EMU members in particular. Galí and Perotti found little difference for the EMU members to speak of. More recent results are mixed (Balassone and Francese (2004), Forni and Momigliano (2005), Golinelli and Momigliano (2006), Balassone et al (2008), Beetsma and Giuliodori (2008), Candelon et al (2010)). One conclusion seems to stand out if any: namely, that the effort to meet the entry conditions of the Maastricht Treaty led the candidate countries to rein in their budget deficits in 1993-1998, whereas following entry, the Treaty and the SGP ceased to exert any disciplinary pressure (see von Hagen et al (2000), IMF (2001), OECD (2005), Annett (2006), and Poplawski

Ribeiro (2009)). Even on this seeming point of agreement, there is no unanimity: Hercowitz and Strawczynski (2005) rally to Galí and Perotti's view (at least for government spending if not the net primary surplus as a whole) in their statement: "We found that government spending adjustment began in 1994, and that it can be characterized as an OECD phenomenon rather than as a phenomenon specific to countries participating in the Maastricht Treaty or the Stability and Growth Pact" (p.822).

In view of this literature, we made a strenuous effort to test for some differences in the behavior of EMU members following Maastricht. We constructed a dummy variable for EMU members that began with the Maastricht Treaty or candidacy for membership and covers 1993-2003 and interacted the variable with current and lagged changes in output gaps and, in addition, we included the dummy separately. This mimics Galí and Perotti's (2003) procedure, except that they use variables for before and after Maastricht whereas we use variables for the full-sample period and post-Maastricht (for the same reasons that we mentioned in connection with asymmetry: namely, to facilitate the interpretation of the statistical significance of the differences before and after Maastricht).<sup>8</sup> In some cases, rather than defining the Maastricht variables for the EMU members for the entire study period, we defined them only for 1993-1998. This was meant to test whether Maastricht had an impact during candidacy but ceased to have any thereafter. In addition, we borrowed the suggestion of Forni and Momigliano (2005) (subsequently adopted by Beetsma and Giuliodori (2008) and Poplawski Ribeiro (2009)) of including a variable for deficit to GDP ratios in excess of the Maastricht limit of 3% in order to test whether trespassing the limit fostered fiscal discipline in subsequent periods. We then included the relevant variable with a one-year lag together with the previous Maastricht variables (alternatively, those for 1993-2003 and only for 1993-1998) or by itself alone. None of these experiments was successful.

In no case could we find a difference in behavior before and after Maastricht, regardless

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<sup>8</sup>Of course, there are other differences between our estimates and those of Galí and Perotti: they use the cyclically adjusted net primary balance (as a ratio of potential output) in levels as the dependent variable, employ a different dynamic structure and do not attempt to distinguish between current and lagged influences of the output gap.

whether we defined after-Maastricht as 1993-1999 only or the whole post-1993 period. In the process, though, we did find a significant difference between the behavior of the (eventual) EMU members and the rest for social spending, which we discuss in the appendix.

Our fundamental conclusion is that, whatever may be true about Maastricht and its effects, it is not possible to find any influence of the Treaty following the introduction of first differences and instruments for the output gap, at least thus far or until longer time series become available.

## **VII. The real time output gap**

Finally, we turn to results incorporating real time data. The relevant data are taken from the OECD's forecast of output gaps that have been published annually in *Economic Outlook* since 1993. Our measure of the change in the real time output gap,  $\Delta(Y_r/Y_r^*)_t$ , is the value of the year  $t+1$  forecast of  $Y/Y^*$  published in the OECD's December year  $t$  edition of *Economic Outlook*, minus the year  $t$  forecast of  $Y/Y^*$  published in the same edition. Since we use data in first differences, the time period available for estimation is restricted to 1994 through to 2003, leaving 177 observations, approximately half as many as before. This drop in the span of data is unfortunate, not least because it means that we are left trying to discriminate between automatic and discretionary policy responses to the cycle in a period that encompasses most of "the Great Moderation" or a period of low cyclical movement. We can immediately infer that this will make the exercise more difficult. In order to draw any inference, it is necessary first to reproduce the estimates in table 2, and those for government consumption in table 3, for this reduced time span, for comparison. The effort to replicate the earlier tests proves unsuccessful in the case of the asymmetric behavior of government consumption in table 3. The conditions of under- and over-identification which relate to the adequacy of the instruments are not sufficiently satisfied (the probability value of the Kleibergen-Paap test statistic rises too much and that of the Hansen J statistics drops too low). The revised estimates are uninformative. This should not be surprising. A loss of power is bound to follow from the shorter sample, even apart from the cyclical calm in the part of the sample that remains. Forging ahead with real time data does not help. We therefore center our

attention on table 2 concerning symmetry in all of the equations.

Estimates of the table 2 specification over the shorter 1994-2003 sample are set out in the leftmost columns of each section of table 4 (in this case we ignore the investment equation, which makes no difference for the subsequent comparisons). We shall alter the usual order of discussion by beginning with social spending and consumption. As regards social spending, the results correspond closely to the earlier ones in table 2. For government consumption, matters are more complicated. The combined current and lagged response is similar in size to the earlier one in the corresponding equation in table 2 and strongly significant, as it was before. However, whilst in table 2 the entire impact was contemporaneous (automatic), in the shorter sample the point estimates for the current and lagged effects are similar. Nonetheless the considerable drop in the probability value for the Hansen J statistic conditions to .07 raises doubt about the over-identifying restrictions and the validity of these estimates. On the other hand and to complicate matters further, the case for instrumenting the output gaps becomes particularly fragile based on the endogeneity test. Therefore, we experimented with instrumenting only the debt ratio. In this case, none of the same difficulties arise. The results for the decomposition between the current and lagged responses are similar to the previous ones in table 2 and those for the aggregate of both responses continue to resemble the common ones in tables 2 and 4. Switching to the new consumption equation therefore seems unimportant. It also makes little difference for the subsequent comparison with the real time results. In the end, we stick to the consumption equation in the table.

In the case of government revenues, shown in section (B) of table 4, it becomes possible to accept a positive contribution to stabilization at the 90% confidence level for the shorter sample. This may explain why the estimate of the total response of the primary surplus, section (A), is now higher than before at 1.08. However, the impact of the cycle on the primary surplus remains as difficult to decompose between a current and lagged response as it was before. As for the other influences on government fiscal behavior, all in all, the only notable difference is that the significance of the election year disappears.

In the second set of results in Table 4, all four parts, we show what happens when we substitute the real time value of the output gap (in first differences),  $\Delta(Y_r/Y_r^*)_t$ , for the lagged final value of the output gap (in first differences),  $\Delta(Y/Y^*)_{t-1}$ , to capture the discretionary response of fiscal policy to the cycle. Our measure of  $\Delta(Y_r/Y_r^*)_t$  is the value of the year t+1 forecast of  $Y/Y^*$  published in the OECD's December year t edition of *Economic Outlook*, minus year t forecast published in the same edition. We do not instrument  $\Delta(Y_r/Y_r^*)_t$  since government primary surpluses during a year cannot affect the real time value of the gap at the beginning of the year. The equation is:

$$\Delta\left(\frac{X_{ijt}}{Y_{jt}}\right) = \alpha_i + \alpha_{it} + \beta_{i1} \Delta\left(\frac{Y_{jt}}{Y_{jt}^*}\right) + \beta_{i2} \Delta\left(\frac{Y_{rjt}}{Y_{rjt}^*}\right) + \beta_{i3} \Delta\pi_{jt} + \beta_{i4} \left(\frac{D_{jt-1}}{Y_{jt-1}}\right) + \beta_{i5} elec_{jt} + \varepsilon_{ijt} \quad (4)$$

There is remarkably little difference. Some improvement occurs in relation to our estimate on the left hand column in the case of the government consumption equation, where a stabilizing response to the gap emerges in the current period. On the other hand, the government revenue equation is more challenging: discretionary policy seems partly to offset a stabilizing automatic response to the cycle (at the 90% confidence level). This result for revenues may explain why the total response to the cycle, based on real time, is lower in the primary surplus equation than in our specification, on the left hand side. However, the main impression from the results of table 4 is that the use of real time data to capture discretionary fiscal policy responses to the cycle leads to no fundamental reassessment. At least in the case of symmetry, which is the only one that we can usefully study, discretionary fiscal policy responses to the cycle are about as small and subordinate relative to automatic stabilization with real time data as with final data.

As an alternative to the previous approach, we also took a nested approach to the question whether real time data adds anything to our basic specification and added an extra term for the difference between the real time output gap and the lagged output gap to the final data specifications in Table 4. Doing so also has a negligible impact on the parameter estimates and test statistics.



However, our verdict does not agree with the earlier literature introducing real time data in analyzing fiscal policy, where we encounter frequent suggestions that fiscal authorities try to move in a stabilizing direction on the basis of real time data. In our view, though, the conflict is smaller than it appears. It is plain only in the case of two studies: Kalckreuth and Wolff (2007) and Bernoth et al. (2008).

In their lead-off article on fiscal policy based on real time data, Forni and Momigliano (2004) only support the stabilizing movement of discretionary fiscal policy for contractions, not expansions. Subsequent support of this stabilizing movement by Cimadomo (2007) and Golinelli and Momigliano (2009) strictly concerns intentions rather than outcomes. Both papers argue for stabilizing fiscal policy based on *predicted* values of primary government net surpluses. Indeed, in the case of Cimadomo, the actual movement in the primary government surpluses is even perverse: the surpluses move in the opposite direction to the intended. In the case of Golinelli and Momigliano, things are not as bad: the authorities merely fail to obtain the movement they desire in primary surpluses.<sup>9</sup> Both papers effectively therefore contradict Forni and Momigliano's (2004) positive assessment of stabilizing discretionary policy outcomes in contractions. Over and above this, all three studies and the rest concerning real time responses of fiscal authorities to the cycle, with the exception of Kalckreuth and Wolff (2007) and Bernoth et al. (2008), use official data for cyclically adjusted primary government surpluses (whether final or in real time) as the dependent variable. Yet we have shown that this data does not properly correct for the cycle. The studies also use real time data for the output gap as the sole representation of the gap on the right hand sides. Yet the real time data for the output gap, however distinct it may be from the final data, is still positively correlated with it (anywhere from .40 to .62 in our sample). Thus, even so far as the results of these studies intersect ours, the influence of the real time variable could well partly reflect

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<sup>9</sup> Both results are severely damning for discretionary fiscal policy responses to the cycle, especially those of Cimadomo, as he recognizes. It would be bad enough if the fiscal authorities did not succeed in moving the net primary surplus instrument in the intended direction. But if they even tend to move the surplus in the opposite way, they should clearly desist from any short run action. Indeed, even if the fiscal authorities control their instrument but merely significantly misinterpret the output gap in real time, questions already arise about discretionary fiscal policy in the short run. See, for example, Jonung and Larch (2006). We do not deal with any of these issues.

automatic rather than discretionary fiscal policy. Still another difference, of course, is that these studies adopt a single-equation approach to fiscal behavior.

While the conflict between our results and the rest of the literature on real time is not clear in the majority of cases, it is so for Kalckreuth and Wolff (2007) and Bernoth et al. (2008). Kalckreuth and Wolff (2007) obtain discretionary responses of fiscal policy to real time errors in the data about output (not the output gap) in the U.S.<sup>10</sup> On their part, Bernoth et al. (2008) report support for the stabilizing movement of fiscal policy in response to real time errors about the output gap in a sample covering 14 European countries and most of our time span in Table 4. For the moment, our only explanation for these differences is that Kalckreuth and Wolff deal strictly with the U.S. and rely heavily on a different estimation method (SVAR) whereas Bernoth et al. not only employ a different estimation method (Blundell-Bond GMM), but a different specification (and their country/year sample also does not coincide with ours).

## **VIII. Conclusion**

We have argued that official calculations of automatic stabilizers are seriously flawed. This puts doubt on many estimates of discretionary fiscal policy responses to the cycle. One approach is to avoid the official estimates but still retain the conventional two-step approach to discretionary fiscal policy that consists of constructing cyclically adjusted data on the basis of various filters for the cyclical adjustment in a first step. However, we believe that a better solution is to estimate automatic and discretionary fiscal policy jointly from the start. One consequence of our approach is to put into question any single-equation treatment of fiscal policy since the revenues and spending sides respond widely differently to the cycle and since the automatic responses of spending to the cycle depend heavily on most social spending. This leads to a minimal three-part decomposition of the budget balance between revenues, social spending, and spending on goods and services. However, we prefer a four-part decomposition since the behavior of capital spending is sufficiently different. There is also an argument for combining social spending on health with other social spending rather than

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<sup>10</sup> They also decompose government budget balances in two parts, net taxes (revenues minus transfer payments) and government spending on goods and services, as did Blanchard and Perotti (2002), in the study that serves them as a guide.

government consumption, where it is usually found. Social spending on health responds automatically to the cycle much like other social spending, consisting of transfer payments, and not like government consumption. It is also a large and increasingly significant spending item (around 13 percent of total government spending in the recent part of our OECD sample). Unfortunately, though, OECD data series for social health spending (which strictly relates to spending in response to claims by insured individuals and not any wage or capital expenditures in a nationalized health sector, as explained in note 1) is only available from 1980 forward and published with far greater delays than the rest of the series in our study. Therefore our insistence on reclassifying social health spending cuts down our estimation period both at the beginning and the end.

Our headline results are .59 automatic stabilization in percentage-points of primary surplus balances.<sup>11</sup> In addition, discretionary policy cancels part of automatic stabilization through destabilizing government consumption behavior (net of health) in expansions, leaving only perhaps as little as .35 stabilization standing. On the other hand, the full influence of automatic stabilization remains standing with no subsequent discretionary policy action in contractions. These asymmetric results concerning government consumption fit in well with many earlier theoretical and empirical findings. We also corroborate many earlier reports of stabilizing responses of government budget balances to government debt and destabilizing responses to national election dates, especially on the tax side. On the other hand, we fail to confirm the occasional successes of earlier researchers in uncovering effects of the Maastricht Treaty and the SGP on fiscal policy behavior in the eurozone and uncovering major differences in responses of discretionary fiscal policy based on real time series. The reason could lie in the greater demands that we make on the data via the use of first differences. The shortness of our time series could also be a factor. Perhaps our use of instruments for the separate influences of the output gap has something to do with the differences as well, especially in light of the two previous factors. For these reasons, we only claim to be unable

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<sup>11</sup> The profiles of the cyclically adjusted budget balances for the individual countries based on our estimates are available upon request. They show far fewer long cyclical swings and many more reversals than the official series of the OECD though the two series are still positively correlated. We are grateful to David Cobham for suggesting that we examine this issue.

to confirm earlier conclusions about Maastricht influences or real time, not to refute them.

Let us note, in closing, that the recent upsurge of interest in fiscal policy following the 2007-2009 financial crisis has increased the significance of a proper assessment of fiscal policy, a large subject, to which we contribute. Without such assessment, efforts to detect the impact of discretionary fiscal policy action on the economy cannot go far.

## APPENDIX

### Are EMU countries different? – Social and health spending

In the effort to uncover some difference in the behavior of (eventual) EMU members before and after Maastricht, we introduced general controls for a difference in the behavior of the EMU members from the rest for the period as a whole. While these controls made no difference otherwise, they revealed a significant difference in behavior in the EMU for social spending. The current response of social spending to the output gap in the EMU countries is substantially more stabilizing than in the rest of the OECD, while the lagged response to the gap in the EMU is somewhat destabilizing whereas outside the EMU it is not. In fact, there is no significant lagged response to the gap at all outside EMU. Because of the destabilizing lagged response in EMU, the combined current and lagged responses are only moderately more stabilizing there than elsewhere. The result is in table A1 (column ii), where the estimating equation is at the top.

As can be seen, for the current period, we get .55 stabilization for the EMU members ( $\beta_1+\beta_3$ ) and .24 ( $\beta_1$ ) for the rest. Once the lagged effects come into view, the sum stabilizing response for the EMU members is .42 ( $\beta_1+\beta_2+\beta_3+\beta_4$ ) while that for the rest is .34 ( $\beta_1+\beta_2$ ): thus the difference narrows. All of these figures are estimated with fair accuracy. The figures are also fairly consistent with the earlier estimate of .35 stabilization for the OECD members as a group in the current period and .36 when the lagged effect is added in table 2 (as is shown once again in table A1). It is easy to accept the hypothesis of a higher immediate impact of the output gap on social spending in the EMU than the rest of the OECD. Social spending programs in the EMU are known to be larger than elsewhere. On this ground, social spending could yield more automatic stabilization there as well. On the other hand, the narrowing of the difference between EMU and non-EMU responses to the cycle once the lagged effects are included is rather uncomfortable. The easiest interpretation, in our reasoning, would be some tendency for the EMU sub-group to engage in destabilizing discretionary social spending in recessions and expansions alike. However, this is an unfamiliar result that would need further support.

TABLE 1 – AUTOMATIC STABILIZATION

$$\Delta \left( \frac{X_{ijt}}{Y_{jt}} \right) = \alpha_i + \alpha_{it} + \beta_{i1} \Delta \left( \frac{Y_{jt}}{Y_{jt}^*} \right) + \beta_{i2} \Delta \pi_{jt} + \varepsilon_{ijt}$$

20 countries, 1980-2003, 368 obs	IV
Social spending	-0.336(.053)[.00]
Unemployment compensation	-0.079(.018)[.00]
Pensions	-0.118(.023)[.00]
Health	-0.057(.017)[.00]
Incapacity	-0.014(.008)[.06]
Sick Pay	-0.001(.007)[.92]

Notes: The equations include a full set of time dummies. The instruments used for the change in the output gap are lags 2 through 4 period lags of  $(Y/Y^*)_t$ . The standard errors reported in (.) are robust to heteroscedasticity and autocorrelation while p-values are given in [.]

TABLE 2: AUTOMATIC AND DISCRETIONARY RESPONSES TO THE CYCLE

$$\Delta \left( \frac{X_{ijt}}{Y_{jt}} \right) = \alpha_i + \alpha_{it} + \beta_{i1} \Delta \left( \frac{Y_{jt}}{Y_{jt}^*} \right) + \beta_{i2} \Delta \left( \frac{Y_{jt-1}}{Y_{jt-1}^*} \right) + \beta_{i3} \Delta \pi_{jt} + \beta_{i4} \left( \frac{D_{jt-1}}{Y_{jt-1}} \right) + \beta_{i5} elec_{jt} + \varepsilon_{ijt}$$

IV Estimates 20 countries, 1982-2003, 350obs	(A) Primary Surplus	(B) Government Receipts	(C) Government Social Spending	(D) Gov. Consumption Spending	(E) Gov. Investment Spending
$\Delta(Y/Y^*)_t$	0.301(.281)[.29]	-0.252(.178)[.16]	-0.353(.073)[.00]	-0.242(.069)[.00]	0.041(.085)[.63]
$\Delta(Y/Y^*)_{t-1}$	0.431(.271)[.11]	0.354(.161)[.03]	-0.009(.067)[.90]	0.029(.069)[.67]	-0.097(.091)[.28]
$\Delta \pi_t$	0.010(.009)[.92]	-0.114(.040)[.01]	-0.067(.026)[.02]	-0.081(.028)[.00]	0.024(.038)[.52]
$(D/Y)_{t-1}$	0.032(.009)[.00]	0.015(.005)[.01]	-0.007(.003)[.02]	-0.003(.003)[.21]	-0.008(.003)[.02]
<i>Elec</i>	-0.480(.195)[.02]	-0.283(.122)[.02]	0.084(.061)[.17]	0.072(.057)[.21]	0.041(.088)[.64]
Combined automatic and discretionary response ( $\beta_1 + \beta_2$ )	0.733(.195)[.00]	0.101(.106)[.34]	-0.361(.070)[.00]	-0.213(.054)[.00]	-0.057(.069)[.61]
Endogeneity Tests $\chi^2(1)$ : $(Y/Y^*)_t$	0.055[.82]	0.519[.47]	2.564[.11]	0.996[.32]	0.006[.94]
$(D/Y)_{t-1}$	21.01[.00]	7.869[.01]	4.571[.03]	2.092[.15]	5.798[.02]
Joint signif. of $\alpha_{it}$ F(22,321):	1.50 [.07]	2.42[.00]	1.42[.10]	2.38[.00]	1.25[.20]
Underidentification test:					
Kleibergen-Paap rK LM test	16.13[.00]	16.13[.00]	16.13[.00]	16.13[.00]	16.13[.00]
Over identification Restrictions:					
Hansen J-test	0.169[.92]	0.448[.80]	0.183[.91]	1.265[.53]	0.109[.95]

Notes: The equations include a full set of time dummies. Instruments used are lags 2-4 of the output gap and lags 2-3 of the change in the debt to GDP ratio. The standard errors reported in (.) are robust to heteroscedasticity and autocorrelation while P-values are given in [.]

TABLE 3: ASYMMETRIC RESPONSES TO THE CYCLE

$$\Delta \left( \frac{X_{ijt}}{Y_{jt}} \right) = \alpha_i + \alpha_i^+ + \alpha_{it} + \beta_{i1} \Delta \left( \frac{Y_{jt}}{Y_{jt}^*} \right) + \beta_{i2} \Delta \left( \frac{Y_{jt-1}}{Y_{jt-1}^*} \right) + \beta_{i3} \Delta \left( \frac{Y_{jt}}{Y_{jt}^*} \right)^+ + \beta_{i4} \Delta \left( \frac{Y_{jt-1}}{Y_{jt-1}^*} \right)^+ + \beta_{i5} \Delta \pi_{jt} + \beta_{i6} \left( \frac{D_{jt-1}}{Y_{jt-1}} \right) + \beta_{i7} elec_{jt} + \varepsilon_{ijt}$$

IV, 350 obs.	Gov. Social Spending (A)	(B)	Government Consumption (C)	(D)
$\Delta(Y/Y^*)_t$	0.310(.081)[.00]	-0.263(.069)[.00]	-0.259(.070)[.00]	-0.238(.058)[.00]
$\Delta(Y/Y^*)_{t-1}$	-0.013(.067)[.84]	0.036(.068)[.59]	0.033(.068)[.63]	---
$\Delta(Y/Y^*)_t^+$	-0.182(.170)[.29]	0.021(.157)[.13]	---	---
$\Delta(Y/Y^*)_{t-1}^+$	0.124(.190)[.51]	0.010(.152)[.66]	0.114(.129)[.38]	0.120(.126)[.34]
$\Delta \pi_t$	-0.077(.023)[.00]	-0.088(.025)[.00]	-0.088(.025)[.00]	-0.086(.025)[.00]
$(D/Y)_{t-1}$	-0.003(.003)[.25]	-0.001(.002)[.55]	-0.001(.002)[.55]	-0.001(.002)[.54]
<i>Elec</i>	0.077(.061)[.20]	0.078(.057)[.17]	0.077(.055)[.17]	0.078(.055)[.16]
Automatic policy in 'bad' times: $\beta_1$	-0.310(.081)[.00]	-0.263(.069)[.00]	-0.259(.070)[.00]	-0.238(.058)[.00]
Automatic policy in 'good' times: $\beta_1 + \beta_3$	-0.492(.159)[.00]	-0.242(.162)[.14]	-0.259(.070)[.00]	-0.238(.058)[.00]
Combined automatic & discretionary policy in 'bad' times: $\beta_1 + \beta_2$	-0.324(.089)[.00]	-0.226(.064)[.00]	-0.226(.064)[.00]	-0.238(.058)[.00]
in 'good' times: $\beta_1 + \beta_2 + \beta_3 + \beta_4$	-0.381(.124)[.00]	-0.105(.120)[.38]	-0.112(.095)[.24]	-0.118(.097)[.22]
Endogeneity Tests $\chi^2(1)$ :				
$(Y/Y^*)_t$	1.002[.32]	0.663[.42]		
$(Y/Y^*)_t^+$	0.761[.38]	0.152[.70]		
$(D/Y)_{t-1}$	0.519[.47]	0.550[.46]		
Joint signif. of $\alpha_{it}$ :	F(22,318)=1.34[.14]	F(22,318)=1.93[.01]	F(22,319)=1.96[.01]	F(20,320)=2.03[.00]
Kleibergen-Paap rK LM Statistic	12.09[.02]	12.09[.02]	16.99[.00]	17.28[.01]
Hansen J statistic	3.757[.29]	3.970[.26]	4.098[.39]	4.687[.46]
Tests of joint restrictions:				
$H_0: \alpha^+ = \beta_3 = \beta_4 = 0$	F(3,318) = 0.59[.62]	$H_0: \alpha^+ = \beta_3 = \beta_4 = 0$ F(3,318) = 0.76[.51]	$H_0: \alpha^+ = \beta_4 = 0$ F(2,319) = 1.14[.32]	$H_0: \alpha^+ = \beta_4 = 0$ F(2,320) = 2.09[.13]
Test of reduction column (B) to (D):		$H_0: \beta_2 = \beta_3 = 0$ F(2,318) = 0.14[.87]		

Notes: see Table 2.

TABLE 4: AUTOMATIC AND DISCRETIONARY RESPONSES - INTRODUCING REAL TIME DATA



$$\Delta\left(\frac{X_{ijt}}{Y_{jt}}\right) = \alpha_i + \alpha_{it} + \beta_{i1} \Delta\left(\frac{Y_{jt}}{Y_{jt}^*}\right) + \beta_{i2} \Delta\left(\frac{Y_{rjt}}{Y_{rjt}^*}\right) + \beta_{i3} \Delta\pi_{jt} + \beta_{i4} \left(\frac{D_{jt-1}}{Y_{jt-1}}\right) + \beta_{i5} elec_{jt} + \varepsilon_{ijt}$$

IV, 177 obs. 1994-2003	(A) Primary Surplus		(B) Government Receipts		(C) Government Social Spending		(D) Government Consumption	
	Final Data	Real Time Data	Final Data	Real Time Data	Final Data	Real Time Data	Final Data	Real Time Data
$\Delta(Y/Y^*)_t$	0.568(.354)[.11]	1.081(.271)[.00]	0.023(.296)[.94]	0.297(.185)[.11]	-0.451(.101)[.00]	-0.477(.113)[.00]	-0.120(.087)[.17]	-0.217(.083)[.01]
$\Delta(Y/Y^*)_{t-1}$	0.512(.288)[.08]		0.318(.235)[.18]		-0.014(.125)[.11]		-0.107(.097)[.27]	
$\Delta(Y_r/Y_r^*)_t$		-0.317(.113)[.01]		-0.119(.070)[.09]		0.034(.061)[.57]		0.048(.042)[.26]
$\Delta\pi_t$	-0.002(.131)[.99]	-0.016(.123)[.90]	-0.125(.066)[.06]	-0.132(.059)[.03]	-0.079(.033)[.02]	-0.077(.034)[.03]	-0.123(.024)[.00]	-0.120(.022)[.00]
$(D/Y)_{t-1}$	0.024(.012)[.04]	0.019(.011)[.08]	0.016(.008)[.03]	0.012(.006)[.06]	-0.004(.005)[.36]	-0.004(.004)[.31]	-0.001(.034)[.73]	-0.002(.003)[.40]
<i>Elec</i>	-0.277(.273)[.31]	-0.534(.235)[.02]	-0.041(.219)[.85]	-0.188(.159)[.24]	0.129(.107)[.23]	-0.140(.097)[.15]	-0.001(.079)[.99]	0.050(.064)[.78]
Combined response $\beta_1 + \beta_2$	1.080(.294)[.00]	0.764(.244)[.00]	0.341(.195)[.08]	0.178(.171)[.30]	-0.495(.072)[.00]	-0.443(.083)[.00]	-0.326(.064)[.00]	-0.169(.067)[.01]
Endog. tests: $\Delta(Y/Y^*)_t$	1.584[.21]	7.854[.01]	0.277[.60]	1.518[.22]	6.193[.01]	4.131[.04]	0.001[.98]	0.627[.10]
$(D/Y)_{t-1}$	6.827[.01]	4.371[.04]	4.703[.03]	2.980[.08]	0.876[.35]	1.502[.22]	0.183[.67]	0.008[.93]
Joint significance of $\alpha_{it}$ F(9,162)	3.12[.00]	3.92[.00]	3.41[.00]	3.67[.00]	1.14[.33]	1.33[.22]	3.38[.00]	3.50[.00]
Kleibergen-Paap LM	7.315[.06]	7.624[.11]	7.315[.06]	7.624[.11]	7.315[.06]	7.624[.11]	7.315[.06]	7.624[.11]
Hansen J-test	0.668[.71]	2.420[.49]	0.495[.78]	3.317[.35]	2.898[.25]	3.006[.39]	5.323[.07]	6.266[.10]

Notes: See Table 2. Real time output gaps are not instrumented.

**TABLE A1: ARE EMU COUNTRIES DIFFERENT? – SOCIAL SPENDING**

$$\Delta\left(\frac{X_{ijt}}{Y_{jt}}\right) = \alpha_i + \alpha_{it} + \mu_i D_{EMU_{it}} + \beta_{i1} \Delta\left(\frac{Y_{jt}}{Y_{jt}^*}\right) + \beta_{i2} \Delta\left(\frac{Y_{jt-1}}{Y_{jt-1}^*}\right) + \beta_{i3} \Delta\left(\frac{Y_{jt}}{Y_{jt}^*} D_{EMU}\right) + \beta_{i4} \Delta\left(\frac{Y_{jt-1}}{Y_{jt-1}^*} D_{EMU}\right) + \beta_{i5} \Delta\pi_{jt} + \beta_{i6} \left(\frac{D_{jt-1}}{Y_{jt-1}}\right) + \beta_{i7} elec_{jt} + \varepsilon_{ijt}$$

IV estimates 20 countries, 1982-2003, n=350	Government Social Spending (i) (As Table 2)	Government Social Spending (ii)
$\Delta(Y/Y^*)_t$	-0.353(.073)[.00]	-0.243(.073)[.00]
$\Delta(Y/Y^*)_{t-1}$	-0.009(.067)[.90]	-0.097(.075)[.20]
$\Delta(Y/Y^*)_t \cdot D_{EMU}$	--	-0.302(.105)[.00]
$\Delta(Y/Y^*)_{t-1} \cdot D_{EMU}$	--	0.227(.107)[.04]
$\Delta \pi_t$	-0.067(.026)[.02]	-0.080(.025)[.00]
$(D/Y)_{t-1}$	-0.007(.003)[.01]	-0.007(.004)[.06]
<i>Elec</i>	0.084(.061)[.17]	0.037(.064)[.56]
Combined response all countries $\beta_1 + \beta_2$	-0.361(.070)[.00]	--
Combined response non EMU countries $\beta_1 + \beta_2$	--	-0.341(.074)[.00]
Contemporaneous response EMU countries $\beta_1 + \beta_3$	--	-0.546(.098)[.00]
Combined response EMU countries $\beta_1 + \beta_2 + \beta_3 + \beta_4$	--	-0.416(.086)[.00]
Joint significance of $D_{EMU}$ terms $\gamma, \beta_3$ and $\beta_4$	--	F(3,318)=2.81[.04]
Joint significance of $\alpha_{it}$	F(22,321)=1.42[.10]	F(22,318)=1.53[.04]
Underidentification test: Kleibergen-Paap LM test	16.13[.00]	17.36[.00]
Over identification Restrictions: Hansen J-test	0.183[.91]	0.704[.87]

Notes: See Table 2.

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