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Recessions and Instable Estimates of Potential Output

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Recessions and Instable Estimates of Potential Output*

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Abstract

This paper analyzes how the OECD revises potential output (PO) estimates after recessions. We show that downward revisions are substantial and mostly driven by supply shocks while PO estimates do not significantly react to demand shocks. In addition, revisions are partly caused by avoidable mismeasurement of PO before recessions. In particular, we show that the length of the preceding boom and pre-recession values of the current account balance and credit volumes are predictors of post-recession PO revisions. Our results call for improved methods for estimating PO and provide evidence against the existence of substantial hysteresis effects of demand shocks.

JEL Classification: E32

Keywords: potential output, trend, output gap, hysteresis, OECD

1 Introduction

Estimates of potential output (PO) are important for decisions about monetary and fiscal policy. Although PO estimates are meant to proxy the level of economic output that is sustainable in the long-run and independent of cyclical (demand-driven) fluctuations, they have been revised downwards in response to the Great Recession for many countries (see, e. g., Benati, 2012; Ball, 2014). This has renewed interest in the question of how sensitive PO estimates are to severe economic downturns and which factors might help to anticipate and avoid post-recession revisions of such estimates.

Against this background, this paper addresses the following research questions: Do recessions have permanent effects on PO estimates? What are the reasons behind downward revisions of

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PO estimates in the aftermath of recessions? To empirically investigate these questions, we use a newly compiled data set with real-time vintages of the Economic Outlook (EO) provided by the Organisation for Economic Co-operation and Development (OECD) that allows us to trace the development of revisions to the *level* of PO after recessions.

We distinguish between four main explanations why PO estimates are revised downwards following a recession. If recessions are caused by permanent supply shocks, the revisions are a reflection of the lower-than-previously-expected long-run output path of the economy (explanation 1). If recessions are caused by demand shocks, revisions of PO estimates would indicate that the analysts believe that hysteresis effects lead to permanent output effects (explanation 2). If PO is overestimated before a recession, subsequent revisions of PO estimates are "merely" a correction of previous measurement errors (explanation 3). Finally, causation could run in the opposite direction, as suggested in Blanchard et al. (2015) and Blanchard et al. (2017). In this case, declining income expectations induced by the PO revision lead to a fall in aggregate demand and a recession (explanation 4). Note that in this case, PO revisions should precede recessions.

Our paper presents two main findings. First, PO is, on average, revised downwards substantially following recessions. These revisions are partly predictable and, thus, the correction of avoidable mismeasurement of PO before recessions. Second, most of the downward revisions are caused by supply shocks while PO estimates do not significantly react to demand shocks.

Our paper is related to a number of strands in the literature. First, it adds to the literature that investigates the reliability of PO estimates. Starting with Orphanides and van Norden (2002) and Orphanides (2003), the unreliability of such estimates in real-time is documented in many studies (see, e. g., Orphanides et al., 2000; Camba-Mendez and Rodriguez-Palenzuela, 2003; Marcellino and Musso, 2011; Jacobs and van Norden, 2016), with Edge and Rudd (2016) presenting somewhat more optimistic findings for output gap estimates published by the Federal Reserve Board since the 1990s. A number of contributions suggest broadening the information set that is used to estimate PO to obtain more stable estimates. Garratt et al. (2008) show how one can use information from different data vintages and model data revisions explicitly to obtain more reliable PO estimates. More recently, Borio et al. (2014) and Borio et al. (2017) suggest using information about the financial cycle to improve estimates. Our paper expands on this literature by being the first that documents in detail the (un-)reliability of PO estimates after recessions and the reasons behind their instability during such periods.

Second, our paper relates to other studies that analyze whether recessions or financial crises affect *potential* output (estimates) or the corresponding growth rates. Based on OECD real-time data for 23 countries, Ball (2014) shows that PO estimates remain permanently below pre-recession trends after the Great Recession of 2008/09.[1] Haltmaier (2012) (using the Hodrick-Prescott (HP) filter) and Martin et al. (2015) (using exponential trends) use PO estimates obtained ex post by filtering the most recent data vintage. Both papers find that PO growth decreases permanently following recessions. Using the production function approach to estimate PO, Furceri and Mourougane (2012) document a similar effect for the times after financial crises for a sample of 30 OECD countries. Finally, Benati (2012) uses a structural vectorautoregressive (VAR) model to provide evidence that PO growth slowed down after the Great Recession in the United States (US), the Euro area, and the United Kingdom. So far, none of these papers have used comprehensive real-time data on actual estimates of PO levels.

The lack of use of real-time data is also a shortcoming of the third strand of literature that our paper relates to. This literature analyzes whether recessions or financial crises affect *actual* output or its growth rate. The most notable study in this context is by Cerra and Saxena (2008), who show that output losses following financial or currency crises are very persistent in the period between 1960 and 2001. Other studies, such as Papell and Prodan (2012) and Abiad et al. (2009), confirm these findings. Based on data for 100 financial crises over the last 150 years, Reinhart and Rogoff (2009, 2014) broaden the view and show that financial crises have negative impacts on a wide range of variables, such as asset prices, employment or government debt. Finally, a number of studies (see, e. g., Hosseinkouchack and Wolters, 2013; Blanchard et al., 2015) provide evidence that also regular recessions tend to permanently reduce the level of output.[2]

The remainder of this paper is structured as follows. Section 2 explains our data and how we make PO estimates from different data vintages and for different countries comparable. Section 3

[1] Using PO estimates from real-time vintages of the IMF World Economic Outlook, Fatás and Summers (2016) provide evidence that fiscal consolidations contributed to the decline of PO during this period.

[2] In a wider context, our paper is related to the literature on macroeconomic hysteresis effects, i. e., the notion that temporary shocks, such as monetary shocks or demand shocks, might have long-lasting or even permanent effects on output (Blanchard and Summers, 1986, 1987; Lindbeck and Snower, 1986; Stadler, 1986, 1990). We do not contribute to the discussion of hysteresis mechanisms. But the latter are one potential explanation for the downward revisions of PO estimates after recessions that we find in our empirical analysis.

contains the empirical results of our study. It first presents non-parametric statistics that show when and by how much PO estimates are revised following a recession. It then provides evidence about potential causes behind the observed revisions; in particular, it contains evidence that supply and demand shocks trigger different patterns of PO revisions. Section 4 concludes.

2 Data

2.1 Identification of Recessions

To identify recessions, we rely on the simple and transparent method proposed by Bry and Boschan (1971), as adapted for quarterly time series by Harding and Pagan (2002).[3] We apply this algorithm to data on real gross domestic product (GDP) from the most recent vintage of the OECD Main Economic Indicator (MEI) database. In total, we identify 95 recessions between 1990 and 2017, for which we have corresponding data on PO estimates by the OECD.[4] The algorithm is accurate and yields plausible recession dates. For the US, for example, our recession dating coincides with the business cycle dates provided by the National Bureau of Economic Research (NBER) with respect to both start and end of the recessions.[5] The mean duration of the identified recessions is 4.4 quarters and the maximum loss in output (relative to the pre-recession peak) is $-3.4\,\%$ on average.

2.2 Real-Time Data from Economic Outlook

Our main data are different vintages of the EO published by the OECD in spring and autumn of each year. This source contains macroeconomic data for member states of the OECD along with forecasts (one and two years ahead) and estimates of unobservables (such as PO) made by the OECD. We use the OECD data for three main reasons. First, the OECD covers a large sample of countries for which it produces consistent PO estimates. Second, the OCED uses the production function approach to estimate PO (see Beffy et al., 2006) which is also widely used elsewhere. Finally, the availability of real-time data allows us to look at actual revisions of PO estimates and their timing.

[3] We require each business cycle phase to last for at least two quarters and each complete cycle (trough to trough and peak to peak) for at least five quarters.

[4] We exclude two "recessions" which are identified by the algorithm (Denmark 1990Q3–1991Q4, Iceland 2003Q1–2005Q1) because they are misclassified. For a full list of the recessions, see Table A.1 in the Appendix.

[5] One exception is the NBER call of a recession in 2001, which is not identified by our algorithm (because it did not involve two consecutive quarters of negative GDP growth).

Table 1: Sample overview of PO estimates

Country	1^{st} vintage	# Vintages	Max. sample	# Recessions
Australia	1989-1	57	1961	1
Austria	1989-1	57	1961	4
Belgium	1989-1	57	1970	4
Canada	1989-1	57	1962	3
Czech Republic	2005-2	24	1992	2
Denmark	1989-1	57	1960	7
Finland	1989-1	57	1961	4
France	1989-1	57	1963	2
Germany	1994-1	47	1963	5
Greece	1989-2	56	1961	6
Hungary	2008-2	18	1992	2
Iceland	2000-1	35	1964	4
Ireland	1989-1	57	1961	3
Italy	1989-1	57	1960	7
Japan	1989-1	57	1962	7
Luxembourg	2005-2	24	1976	2
Netherlands	1989-1	57	1970	2
New Zealand	1989-1	52	1963	5
Norway†	1989-1	52	1965	4
Portugal	1994-2	46	1960	3
Slovenia	2011-2	12	1999	1
Spain	1989-1	57	1965	3
Sweden	1989-1	57	1964	3
Switzerland	1989-1	57	1961	7
United Kingdom	1989-1	57	1963	2
United States	1989-1	57	1960	2

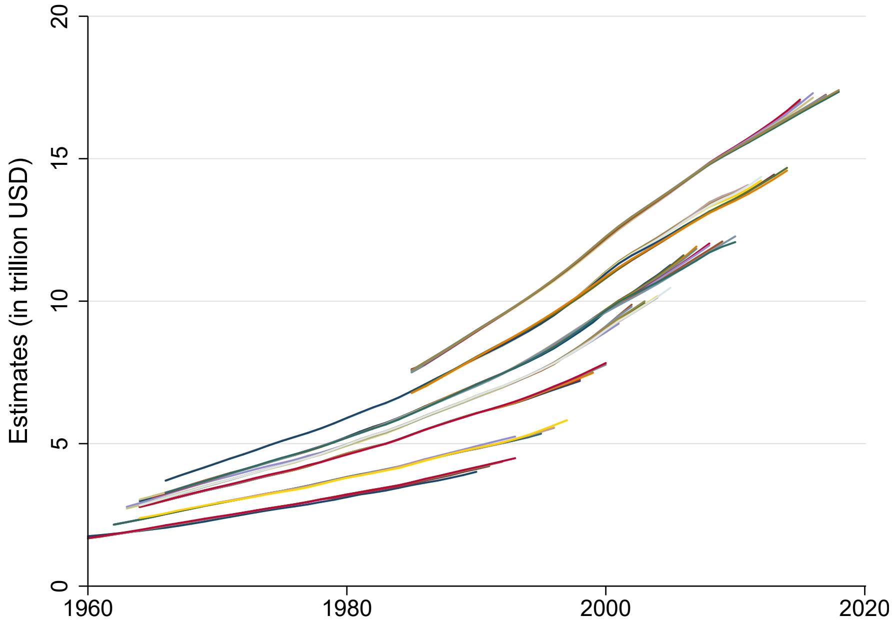
Notes: "1^{st} vintage" refers to the first vintage from which PO estimates are available. "Max. sample" notes the first year for which PO estimates are available in at least one vintage. We use data from the previous vintage to proxy missing vintages in the following cases: Greece (1991–2), Ireland (1991–1/1991–2), and Switzerland (1994–2). Five vintages of PO estimates are missing for New Zealand (1994–2 to 1996–2) and for Norway (1991–1 to 1993–1). PO estimates for unified Germany are not available in vintages before 1994–1. † Data on GDP and PO for Norway refer to domestic production excluding exploration of crude oil and natural gas, transport via pipelines and ocean transport.

Our sample of data vintages that contain information about PO estimates ranges from spring 1989 (EO No. 45) until spring 2017 (EO No. 101). It covers the 26 countries which are listed in Table 1. We have a full set of 57 vintages for 16 of those countries. For Greece, New Zealand, and Norway, only a small number of vintages is missing.

Our main variable of interest are the OECD's estimates of PO levels which we denote by $\bar{y}$.[6] In addition, we use information on the level of GDP (y), the current account balance (in % of GDP), imports and exports (to construct a measure of trade openness), the level of public debt (in % of GDP), and the public primary balance (in % of GDP) from the EO vintages.

[6]Note that PO estimates for Norway refer to the level of domestic production excluding exploration of crude oil and natural gas, transport via pipelines and ocean transport.

Figure 1: Raw data vintages of PO estimates for the US



Notes: The plot shows OECD estimates for (real) PO in the US from different EO vintages. Values are in trillions of real USD (with different base years).

Because we use real-time data vintages, our data have a multi-dimensional structure. This allows us to track how the OECD's PO estimate for any particular year changes across vintages following the start of a recession. Consequently, we are able to analyze how large revisions are and when they occur. We denote a variable x for country i and year t from vintage v by $x_{i,t}^{v}$.

A snapshot of the raw data is plotted in Figure 1 which shows all vintages of PO estimates for the US. The plot shows that revisions can be substantial. It is also evident from the figure that we need to normalize the data due to changes in national accounting standards and, most importantly, base years. This applies to all countries and we explain how we do this in Section 2.4.

2.3 Extrapolation of Potential Output Estimates

The OECD data contain PO estimates that reach two years ahead of the publication time, i.e., a vintage v' from a certain year t' contains information up to $\bar{y}_{i,t'+2}^{v'}$. Because we are also interested in medium-term revisions following a recession and would like to compare how the PO estimates for, say, the fifth year after a recession change during the recession and the following years, we need to extrapolate the raw OECD estimates.

We do so by expanding the OECD estimates for additional 10 years using the implied average potential growth rate of the last 10 observations (which include the OECD's forecast for the next two years). That is, we compute $\gamma = 1/10 \sum_{k=-7}^{2} \Delta \ln \bar{y}^{v'}_{i,t'+k}$, and obtain additional (log) PO estimates as $\ln \bar{y}^{v'}_{i,t'+k} = \ln \bar{y}^{v'}_{i,t'+2} + (k-2)\gamma$ for $k > 2$. Since these additional data points depend on our calculations and are no raw OECD estimates, we indicate below which results depend on the additional data and which results do not.

Given the high degree of smoothness of PO, our approach is adequate. The fit of a linear trend through the last 10 observations of the raw PO estimates is very good on average. Looking at the distribution of the corresponding R^2 across all vintages and countries in our sample reveals a median goodness of fit of above 0.99. In fact, even the 25^{th} percentile of the distribution of R^2s is 0.99 and the 1^{st} percentile is still 0.68. So there is little evidence that our extrapolations lead to large approximation errors (with respect to the unpublished long-run PO forecasts by the OECD). In addition, there is little reason to believe that those approximation errors, which presumably are mainly due to the fact that the OECD could have anticipated demographic changes in real-time, are systematically related to the occurrence of recessions. Thus, overall, we are confident that the extrapolation does not systematically affect our results although it might induces some noise.

2.4 Data Normalization

When comparing PO estimates from different vintages, we have to take into account potential changes in national accounting standards, base year, and/or unit of measurement (e. g., with the introduction of the euro). Since for a number of countries there are some vintages with samples that do not overlap, we cannot use a global base year to normalize all data. Instead, we use a different normalization for each identified recession, making comparable all vintages that are relevant for tracing revisions around this particular recession.

Denoting the first year of a recession by t_0 and the first vintage following the start of a recession by v_0, we construct normalized PO estimates using the following formula:

$$\tilde{y}_{i,t_0+s}^{v_0+k} = \bar{y}_{i,t_0+s}^{v_0+k} \times \frac{y_{i,t_0-s^*}^{v_0}}{y_{i,t_0-s^*}^{v_0+k}}, \tag{1}$$

where k ranges from $k_{min} < 0$ to $k_{max} > 0$ (determining the range of vintages around a particular recession that we consider), s ranges from $s_{min} < 0$ to $s_{max} > 0$ (determining the range of years around a particular recession that we consider), and $s^* > 0$ determines which year we use as the base year for the normalization. s^* needs to be sufficiently large to ensure that already the earliest vintage considered ($v_0 + k_{min}$) contains reliable data about GDP in year $t_0 - s^*$; otherwise, forecast errors made by the OECD or data revisions would influence our results. In practice, we set s^* equal to 5.

We apply a second normalization step to make sequences of PO estimates comparable across recessions and countries. More specifically, we normalize $\tilde{y}_{i,t_0+s}^{v_0+k}$ such that $\tilde{y}_{i,t_0}^{v_0} = 100$, i. e., the estimate of PO in the first year of a recession as reported in the first vintage following the start of the same recession is set to 100.

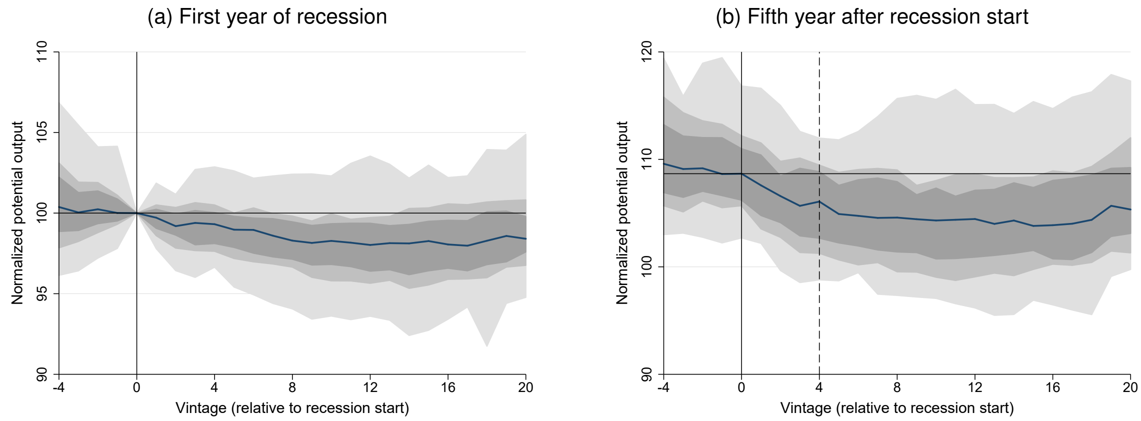
3 Empirical Results

3.1 Timing and Size of Revisions to Potential Output Estimates

We start by tracking how estimates for PO in a certain year evolve across successive vintages of the EO. Figure 2 shows the distribution of the evolution of PO estimates for the first and the fifth year following the start of a recession. The solid vertical line indicates the first data vintage after the start of a recession. It is evident that, on average, PO is revised downwards in the aftermath of a recession.[7] The estimate for the first year after the start of a recession, for instance, is reduced by roughly 1.7 %, on average, from v_0 to v_{10} (Table 2). Looking at later years shows that the gap between pre-recession PO estimates and the post-recession estimates increases with the distance to the recession start. This confirms evidence in Blanchard et al.

[7] One problematic issue could be the secular decline in trend growth rates in most of the countries in our sample. If persistently unanticipated by analysts, this might lead to a pattern of downward revisions of PO across vintages irrespectively of recessions (or any other cyclical phenomena). To check that our results are indeed driven by the occurrence of recessions and not an "artefact" of this secular trend, we re-produced our results based on a set of "randomly distributed recessions" (see Appendix B). These results do not exhibit the same patterns (the interquartile range easily covers the median level from v_0 for all years and vintages).

Figure 2: Revisions to PO estimates after recessions



Notes: The plots show the revisions to OECD PO estimates for the first/fifth year after a recession start across different vintages. Values to the left of the dashed line depend on our extrapolation of the PO estimates. The data are normalized such that the value in the first recession year as estimated in the first vintage following the start of the recession is equal to 100. The sample includes 95 recessions. Grey shaded areas represent the 5^{th} to 95^{th} percentile range, the 17^{th} to 83^{th} percentile range, and the interquartile range.

(2015), who call this phenomenon "super hysteresis". Revisions to PO estimates are made gradually over roughly five years following the recession; about 2/3 of the revisions are made until the end of the recession. The pattern of revisions seems to be non-monotonic in the sense that the initial downward revisions are partly reversed later on: the estimates from v_{20} are between roughly 0.9 percentage points (Year 5) and 3.4 percentage points (Year 10) higher than those from v_{10}.

Interestingly, PO estimates are not revised at all, on average, before the start of a recession. This can be seen from the stable medians at the left side of both plots (for vintages up to v_0). Thus, the OECD does not anticipate lower long-run PO levels already before recessions. In essence, this provides evidence against the hypothesis that the causation could run from downward revisions of PO to recessions (explanation 4).

Another prominent feature of our results is the large variation across countries and recessions. The band spanned by the 5^{th} and the 95^{th} percentile is large in all cases and ranges from solid positive numbers to substantially negative ones. After all, the huge variation is not too surprising given the very different situations of countries when hit by a recession.[8] Focusing on the 83^{th} percentile line shows that it plunges below the median level in vintage v_0 between roughly v_8

[8] We come back to this issue in Section 3.2 and Section 3.3 where we analyze which factors can be used to explain differences in the size of PO revisions.

Table 2: Change of PO estimates around recessions

	$\tilde{y}_t^{v_0}$	Change vs. $\tilde{y}_t^{v_0}$ (in %) $\tilde{y}_t^{v_{-4}}$	$\tilde{y}_t^{v_{-2}}$	$\tilde{y}_t^{v_2}$	$\tilde{y}_t^{v_4}$	$\tilde{y}_t^{v_{10}}$	$\tilde{y}_t^{v_{20}}$
Year 1	100.00	0.38	0.24	−0.81	−0.69	−1.72	−1.59
Year 2	101.99	0.67†	0.33	−1.04	−1.29	−2.35	−1.81
Year 3	104.02†	1.15†	0.38†	−1.69†	−1.83†	−2.78†	−2.02†
Year 5	108.67†	0.85†	0.46†	−1.92†	−2.39†	−4.01†	−3.07†
Year 10	121.08†	0.87†	0.14†	−2.89†	−4.01†	−6.90†	−3.47†

Notes: Full sample median is based on a total of 95 recessions. $\tilde{y}_t^{v_j}$ refers to the normalized PO estimate in a particular year (indicated by the row label) after the start of a recession as reported in the vintage j relative to recession start. $j = 0$ refers to the first vintage following the start of a recession. Estimates for each recession are rescaled such that $\tilde{y}_1^{v_0} = 100$. † indicates that the computation of the result involves our extrapolation of PO estimates.

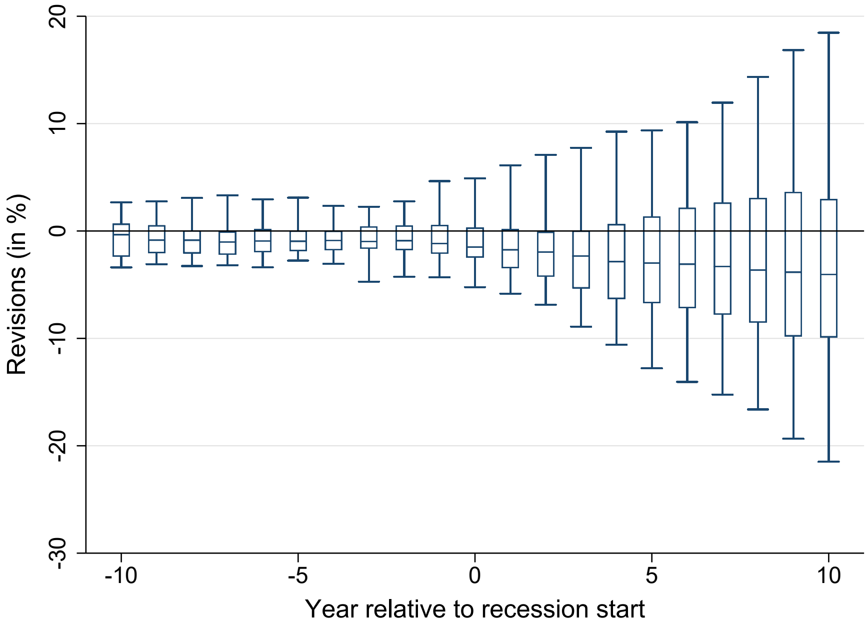
and v_{15} in both sub-plots. Thus, although the variation is large, we see a decline of PO estimates in the vast majority of cases.

To see whether results are heavily influenced by the Great Recession, we also split our sample into one group of recessions that start during the years 2007–09 and one group consisting of all other recessions. The results in Appendix C show that—as expected—the median revision of PO estimates after the Great Recession has been large relative to the historical norm. At the same time, however, the average downward revisions for the remaining sample is almost as large as for the full sample, indicating that the identified phenomenon is not a special feature of the Great Recession and its aftermath.

Another way of presenting the data on PO revisions is to look at the long-run revision made to estimates for several years around the start of a recession. Figure 3 shows that the revisions made to PO (over a span of ten years) for years far after the start of a recession are indeed much larger than for the first recession year. In addition, the data show that post-recession revisions are also made to PO in the years before recessions. PO for the fifth year before a recession, for instance, is revised downwards roughly 1 % on average. Thus, because the OECD uses an estimation approach which is implicitly two-sided, it revises its PO estimates even for the decade before a recession.

What is a benchmark against which to compare the timing and size of the revisions? We compare how the revisions made by the OECD (using its preferred production function approach) relate to results one would have obtained in real-time if one had applied a simple statistical filter to GDP data (incl. the OECD growth forecasts) to estimate PO for each data vintage. We use the one-sided Hodrick-Prescott (HP) filter (see, e. g., Stock and Watson, 1999) for such a purely

Figure 3: Post-recession revisions to PO estimates for different years around recession start



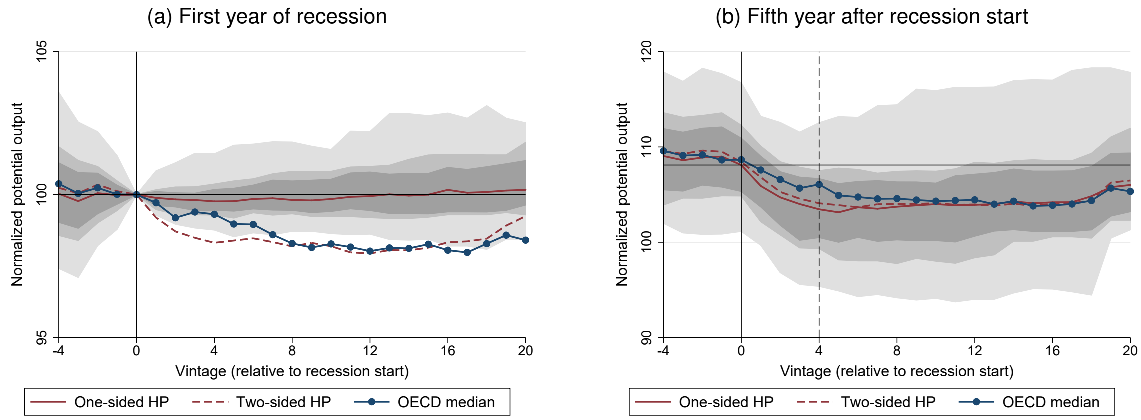
Notes: We compare PO revisions between the first and the 20th vintage after the recession start. In the Box-Whisker plot, the Whiskers include the 5th to 95th percentiles. The boxes correspond to the interquartile range and the medians are indicated by the lines.

statistical approach. We then extrapolate and normalize these alternative PO estimates in the same way as described in Sections 2.3 and 2.4.

It turns out that both size and timing of the OECD revisions are very similar to those which would have resulted from an application of the one-sided HP filter in real time—with the exception of estimates for the first year of a recession (Figure 4). The left plot indicates that five years after a recession started, the OECD, on average, revised downwards their estimate of PO in the first recession year by about 2 percentage points more than the application of the HP filter would suggest. The difference can be explained as follows: As seen above, the OECD implicitly applies a two-sided filtering method that causes the weak economic development during and after a recession to lower the trend estimate also for previous years. In contrast, the one-sided HP filter is only backward looking and estimates of PO in past years do only change due to data revisions. This becomes also evident if we compare the OECD revisions to those based on the two-sided HP filter which, on average, are more or less identical.

For the subsequent years, we do not observe remarkable differences between the size of the OECD's revisions and those based on the HP filters (results for the fifth year after a recession

Figure 4: Comparison of OECD revisions to those based on the HP filter



Notes: The plots show the median revisions to PO estimates for the first/fifth year after a recession start across different vintages based on the one-sided HP filter. Values to the left of the dashed line depend on our extrapolation of the PO estimates. The data are normalized such that the value in the first recession year as estimated in the first vintage following the start of the recession is equal to 100. The sample includes 95 recessions. Grey shaded areas represent the 5^{th} to 95^{th} percentile range, the 17^{th} to 83^{th} percentile range, and the interquartile range. For comparison, we show the median based on the two-sided HP filter and the OECD median revisions (from Figure 2).

start are shown in the right plot of Figure 4).[9] The only observable difference is that the revision process at the OECD seems to be slightly more sluggish than that based on the filtering techniques, which can be explained by a smoothing tendency known from the macroeconomic forecasting literature (see, e.g., Nordhaus, 1987; Dovern and Weisser, 2011; Dovern et al., 2015). Overall, this suggests that a naive application of simple filtering techniques would speed up the revision process but would not change the size of revisions in the long-run.

3.2 Predictors of Potential Output Revisions

Now that we have established that PO estimates are, on average, substantially revised downwards following the occurrence of a recession, it is of interest whether we can identify predictors of the size of these revisions. In the long term, this might help to improve the estimation of PO. We analyze the issue by regressing the size of the PO revision for a particular year (e.g., the first year of a recession) on a number of macroeconomic variables that might potentially correlate with our dependent variable. Thus, we reduce our data to a cross-sectional structure, in which every recession constitutes one observation.

[9] Results for other years look similar (in terms of the similarity of OECD and HP revisions) to those shown for the fifth year following the start of a recession.

More formally, we run regressions of the form

$$\Delta \tilde{y}_{i,t_0+s}^{v' \to v''} = \beta X_{i,t_0} + \varepsilon_{i,t_0+s}, \tag{1}$$

where $\Delta \tilde{y}_{i,t}^{v' \to v''} = \ln \tilde{y}_{i,t}^{v''} - \ln \tilde{y}_{i,t}^{v'}$ denotes the revision of PO in country i for year t from vintage v' to vintage v'', X_{i,t_0} is a vector of covariates, β is a parameter vector of suitable dimension, and $\varepsilon_{i,t_0+s} \overset{iid}{\sim} N(0, \sigma^2)$ is an error term. We consider the following variables as elements of X_{i,t_0}: To characterize the nature of the recession and the previous boom, we include the length of the recession (in quarters), the depth of the recession (in % of the peak level of GDP), the length of the previous boom (in quarters), and the revision of PO in the first year of the recession made during the two years before the recession (in %). To measure a country's connectedness to the global economy, we include a pre-recession measure of trade openness (defined as the sum of exports and imports over GDP), the pre-recession value of an index of financial openness (Chinn and Ito, 2006), the pre-recession values of measures of exchange rate stability and monetary independence (Aizenman et al., 2013), and the change of the current account balance (relative to GDP) over the two years before the recession (in percentage points). To measure the financial situation of the public sector and the private credit cycle, we include (net) public debt in the year before the recession in relation to GDP (in %), the public primary balance in the year before the recession in relation to GDP (in %), and the change in the ratio of credit to the non-financial sector to GDP over the two years before the recession (in percentage points).[10] Finally, we include a number of indices measuring different aspects of economic flexibility and institutional quality (the economic freedom score from heritage.com, the World Bank's ease-of-doing-business index, and two OECD measures of the strictness of employment protection laws).

We present results for $s = 0$ and $s = 4$, i.e., we look at revisions of estimates for the first and the fifth year after a recession has started. In both cases, we look at revisions between the first vintage after the recession start and five years later ($v' = v_0$ and $v'' = v_0 + 10$).

The estimates in the left part of Table 3 indicate that the size of the post-recession revision to PO in the first recession year is significantly correlated with a number of factors. While we test the significance of parameters in single blocks of variables in the first five specifications, we focus here on a discussion of correlations that remain significant in the final specification (5),

[10] We obtain credit data from the Bank for International Settlements (BIS).

Table 3: Determinants of PO revisions

Dependent variable:			$\tilde{y}_1^{v_{10}} - \tilde{y}_1^{v_0}$					$\tilde{y}_5^{v_{10}} - \tilde{y}_5^{v_0}$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Recession length	−0.089					−0.159				
	(−0.65)					(−0.65)				
Recession depth	0.201*				0.222***	0.681***				0.672***
	(1.77)				(3.92)	(3.29)				(5.94)
Length of prev. boom	−0.031**				−0.015	−0.077***				−0.048**
	(−2.30)				(−1.53)	(−3.17)				(−2.40)
$\bar{y}_1^{v_0} - \bar{y}_1^{v-3}$	−0.043					−0.058				
	(−0.39)					(−0.29)				
Trade openness (t-1)		−0.006					−0.016			
		(−0.83)					(−0.93)			
Chinn-Ito index (t-1)		0.145					0.645			
		(0.30)					(0.57)			
Exchange Rate Stability (t-1)		−0.244					−0.956			
		(−0.21)					(−0.36)			
Monetary Independence (t-1)		−0.215					1.044			
		(−0.13)					(0.26)			
Δ CA (t-1)		0.292***			0.256**		0.746***			0.616***
		(2.75)			(2.61)		(3.03)			(3.16)
Primary balance (t-1)			0.084					0.180		
			(1.12)					(1.03)		
(Net) public debt (t-1)			0.001					0.004		
			(0.22)					(0.31)		
dIRS			0.146					0.304		
			(0.94)					(0.84)		
Δ Credit/GDP (t-1)			−0.035***		−0.023***			−0.086***		−0.051***
			(−3.59)		(−2.78)			(−3.78)		(−3.15)
Economic Freedom (t-1)				−0.057					0.066	
				(−0.70)					(0.37)	
EoDB index (t-1)				−0.029					−0.050	
				(−1.03)					(−0.82)	
EP(dis) (t-1)				−0.576					−0.872	
				(−1.00)					(−0.70)	
EP(temp) (t-1)				0.626*	0.322*				1.566**	0.408
				(1.76)	(1.83)				(2.04)	(1.16)
Constant	−0.053	−1.716	−1.593***	3.016	−1.290***	0.413	−4.714	−3.787***	−9.063	−1.513
	(−0.09)	(−0.96)	(−3.89)	(0.42)	(−2.72)	(0.38)	(−1.14)	(−3.95)	(−0.59)	(−1.60)
N	78	76	70	77	76	78	76	70	77	76
R^2	0.25	0.10	0.19	0.06	0.44	0.46	0.13	0.20	0.06	0.59

Notes: Numbers in parenthesis are t-statistics. ***, **, and * correspond to significance levels of 1 %, 5 %, and 10 %, respectively. We indicate by $t-1$ that we use values of the variables from the year before the first recession year. The effective sample of recessions that we can use for the regressions is smaller than stated in Section 2.1 since relevant vintages are not available for recessions at the margins of our sample: We cannot use eleven recessions that start in 2012 or later. Six additional recessions drop from the sample because the availability of vintages starts later (GRC–1990, HUN-2008, ISL-2001, SVN–2011) or due to gaps in the available vintages (NZL–1991, NZL–1997). In two cases (CAN–1990 and FIN–1990), we use information from v_{-2} instead of v_{-3} to calculate the pre-recession revision used as an explanatory variable.

in which we include only those variables that are significant in the group-wise regressions. The depth of a recession seems to be a good predictor of subsequent revisions of PO, the elasticity being 0.22 and highly significant. We find also a linkage between pre-recession credit booms and current account changes to subsequent PO revisions. In the latter case, a 1 percentage point deterioration of the ratio of the current account to GDP goes along with an increase of PO revisions by 0.26 percentage points. One interpretation of the latter result is that current approaches for estimating PO attribute substantial fractions of booms fueled by foreign credit to the structural ability of a country to sustain a certain output level—making later downward revisions of PO necessary.[11] A credit boom during the run-up to a recession is associated, on average, with a larger downward revision of PO. In line with results presented by Gadea Rivas and Perez-Quiros (2015), the linkage between credit booms and post-recession PO revisions disappears if we control for the Great Recession (see Appendix C), suggesting that this factor is only relevant if recessions fall together with turning points of pronounced financial cycles. Finally, we find a weakly significant correlation between the degree of employment protection for workers with temporary contracts and PO revisions, suggesting that stricter employment protection is associated with smaller downward revisions of PO.[12] Overall, the explanatory variables explain 44 % of the variation of the size of PO revisions for the first year of a recession.

Turning to the estimates for the post-recession PO revisions for the fifth year after the start of a recession shown in the right part of Table 3 reveals that we are able to explain a slightly higher share of the variance in this case. Interestingly, the set of significant predictors is very similar. The only differences are, first, that the length of the previous boom is a significant predictor of post-recession revisions of PO which suggests that undue optimism spreads the longer a boom lasts (the longer the boom the stronger the following downward revisions) and, second, that the OECD employment protection indicator does not significantly predict post-recession revisions of PO. In addition, we observe that the estimated elasticities are stronger in all cases. In particular, the elasticity with respect to the depth of a recession is roughly 2/3, indicating that, on average, most of the fall in output during a recession is deemed as permanent

[11]The recent experience with Greece would be a typical example of this kind. Continuous current account deficits fueled a boom with a sectoral composition that could not be sustained once capital flows reversed after 2008.

[12]Note that this coefficient is no longer significantly different from zero once we exclude data from the Great Recession.

by the OECD. This high persistence of output losses is in line with findings in Blanchard et al. (2015).

Overall, the macroeconomic factors that are considered in the regressions are able to explain a surprisingly high share of the huge variation in the size of revisions across recessions and countries. The fact that the length of the previous boom, the change in the current account balance, and the change in the credit volume—all of which are commonly neglected in the estimation of PO—are correlated with post-recession revisions of PO estimates and together explain around 1/3 of their variation suggests that the latter are partly due to previous mismeasurement (explanation 3).

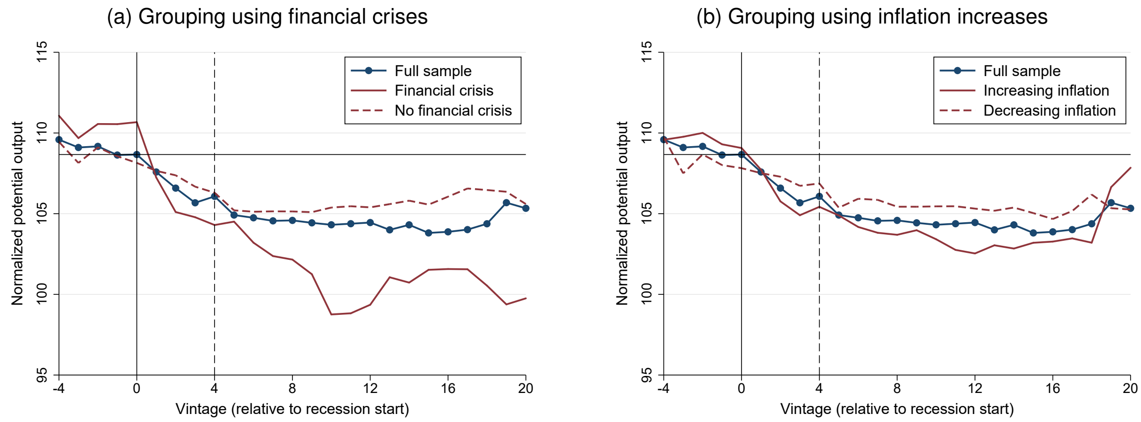
3.3 Demand- vs. Supply-driven Recessions

So far, we have not distinguished between different types of recessions. In general, however, we expect the need to revise downward the long-run potential capacity of an economy to be larger after a (permanent) negative supply shock than after a (temporary) negative demand shock. To see whether we can confirm this conjecture based on our data, we first follow Blanchard et al. (2015) and split our sample of recessions into a group of recessions that are more likely to be driven by supply shocks and one group of recessions that are more likely to be driven by demand shocks. Of course, any classification is imperfect. Still, we present results for two different approaches (both of which we take from Blanchard et al. (2015)) because we think that they provide interesting tentative insights.

First, we label all recessions as supply-driven that are associated with financial crises as identified by Laeven and Valencia (2013). Second, we label all recessions as supply-driven that are associated with an increase of inflation in the first year of the recession (relative to the previous year). The idea here is that only supply side shocks can lead to a decrease in production while at the same time increasing the inflation rate.

We focus on the revisions of PO estimates for the fifth year after the recession start. In Figure 5, we show the baseline results for the full sample together with the medians for the two sub-samples. We refrain from plotting percentile ranges for the sub-samples because, unfortu-

Figure 5: Effects of supply-driven and demand-driven recessions



Notes: The plots show the revisions to OECD PO estimates for the fifth year after a recession start across different vintages. Financial crises correspond to the 21 crises identified by Laeven and Valencia (2013) which coincide with our sample. Increasing inflation means that annual inflation in the first recession year is higher than in the previous year (for 53 recessions). Values to the left of the dashed line depend on our extrapolation of the PO estimates. The data are normalized such that the value in the first recession year as estimated in the first vintage following the start of the recession is equal to 100. The full sample includes 95 recessions.

nately, the number of observations is too small to yield meaningful results.[13] Instead, we focus on discussing the general tendency of the results.

The results based on both classification schemes are similar, even though the differences between the median revisions for the two recession groups differ depending on which classification we look at. We observe the following set of consistent results. First, we find strong and persistent differences in the average size of revisions when contrasting recessions that are more likely to be driven by supply side shocks to those driven mainly by demand shocks. Recessions driven by supply shocks, on average, are followed by (much) larger revisions to PO relative to demand-driven recessions. Second, also after recessions that are likely to be driven by demand shocks the PO revisions are not equal to zero but substantially negative, suggesting that also these recessions leave permanent scars on estimates of the long-run growth paths. At this point, we cannot definitely say whether this is due to hysteresis effects and/or supply shocks that occur also during those recessions. Finally, the fact that the two averages are very similar for vintages from before the start of the recessions is reassuring in the sense that it indicates that the differences are truly due to the different types of recessions.

[13]In general, the variation across recessions is so high that, given our sample size, we do not expect results for the two sub-samples to be statistically significantly different from each other.

The fact that PO is also revised downwards following recessions that we label as predominantly demand-driven does not necessarily imply that PO is revised in response to demand shocks; it could as well be a response to supply shocks that occur simultaneously. To investigate how PO estimates react to both types of structural shocks, we identify three types of such shocks and make inference about the impulse responses of PO estimates through local projections (Jordà, 2005) of cumulative revisions of PO estimates onto those structural shocks.

We compute labor productivity shocks as in Coibion et al. (2017).[14] We identify fiscal shocks as suggested by Auerbach and Gorodnichenko (2012).[15] Finally, we identify monetary policy shocks based on recursive structural VAR models.[16] While the first shock constitutes a supply shock, the two latter ones are demand shocks. We aggregate all shocks to a semi-annual frequency to match the frequency of our vintage data. We denote the semi-annual observations of the three shocks as $\varepsilon_{i,t}^{lp}$, $\varepsilon_{i,t}^{fp}$, and $\varepsilon_{i,t}^{mp}$, respectively. Note that we "flip" the monetary policy shock so that positive values indicate an expansionary shock.

We compute cumulative revisions of PO estimates as $\Delta^h \tilde{y}_{i,y(t)}^t = \ln \tilde{y}_{i,y(t+h)}^{t+h} - \ln \tilde{y}_{i,y(t+h)}^t$, where $\tilde{y}_{i,y(t+h)}^t$ denotes, for instance, the PO estimate for country i from the vintage corresponding to the half-year t for the year that includes the half-year $t+h$.[17] $\Delta^h \tilde{y}_{i,y(t)}^t$ measures how much the estimate of PO for the year that lies (approximately) $h/2$ years ahead is revised between period t and h vintages later. We look at cumulative revisions because, as shown in Section 3.1, the revision process seems to be a gradual one. The local projections are given by

$$\Delta^h \tilde{y}_{i,y(t)}^t = \alpha_i^h + \gamma_t^h + \beta^h \varepsilon_{i,t}^{\bullet} + e_{i,t}^h, \tag{2}$$

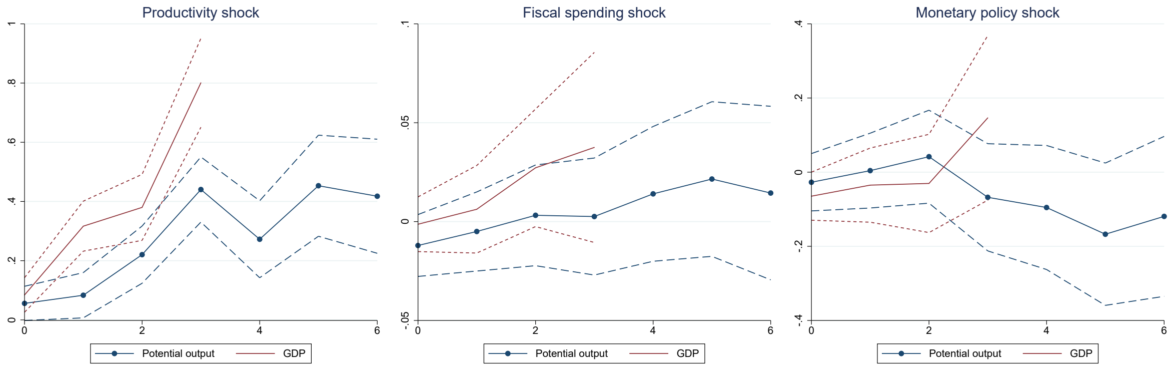
[14] We compute the shocks as the residuals of autoregressive models (of order 4) for the change in labor productivity (that we obtain from the OECD).

[15] Using information from the EO data vintages, we compute the shocks as the difference between observed government spending (consumption plus—when available—investment) and the level that was expected half a year before.

[16] As in Coibion et al. (2017), we include real GDP growth, inflation, the unemployment rate and the short-term interest rate at a quarterly frequency (that we obtain from the OECD), using data from 1980Q1 until 2017Q1 or as available and assuming a VAR(4) specification.

[17] Note that the "target year" changes only every second time as we increase the horizon in steps that are determined by the semi-annual frequency of our vintage data.

Figure 6: Response of PO estimates and GDP forecasts to selected structural shocks



Notes: The plots show the IRFs of (cumulative) revisions to OECD PO estimates and GDP forecasts to selected structural shocks. The dashed lines indicate 90% confidence intervals and are computed based on $+/-$ 1.645 standard errors.

where we include country and time fixed effects. The sequence of β^hs provides the impulse response functions (IRF) of cumulative PO revisions to the different shocks.[18] For comparison, we run the same regressions for revisions of GDP forecasts.[19]

Figure 6 shows the results for horizons up to $h = 6$. As expected, PO estimates seem to respond positively to productivity shocks. In contrast, we do not find any significant responses to the two demand shocks; neither fiscal nor monetary policy shocks seem to systematically move PO estimates during subsequent years. Hence, in terms of PO *level* estimates we cannot confirm the "over-cyclicality" in response to demand shocks that Coibion et al. (2017) report for PO *growth* estimates. We find almost unchanged results if we exclude data from the years 2007–09 to check if our results are driven by the Great Recession (see Appendix C).

Comparing the IRFs for PO and GDP forecasts, we see that GDP forecasts tend to react stronger to productivity shocks relative to PO estimates. For the two demand shocks we do not find any differences. Here, also GDP forecasts do not significantly react to the shocks. The insignificant responses of GDP forecasts to both fiscal policy shocks and monetary policy shocks suggest that either the OECD does not believe that they have any effect on GDP in the short-term or the OECD is not able to observe those shocks in real time.[20] Given how difficult it is

[18]We also run the same regressions including additional lags of the structural shocks. The corresponding IRFs are almost unchanged and the conclusions do not change at all.

[19]Because we do not extrapolate these forecasts, the maximum h that we can consider in this case is rather low.

[20]A third explanation is, of course, that our approaches for estimating these shocks yield estimates that are subject to high measurement errors that make it difficult to identify the IRFs.

to estimate policy impulses in real time, we think that the second explanation is probably the most likely one.

4 Conclusion

In this paper, we have analyzed how OECD estimates for the *level* of PO are revised in the aftermath of recessions. We view these estimates as representative for a wide range of PO estimates published and used by policy institutions. We document that they tend to be revised downwards substantially and broadly in line with results obtained by simple statistical filters. The revisions occur gradually over a period of approximately five years following the start of a recession. In addition, we find that revisions after supply-driven recessions are larger, on average, than those after recessions that are likely to be mainly triggered by adverse demand shocks. Since we do not find a significant response of PO estimates to structural demand shocks, we conclude that the PO revisions following demand side recessions are most likely due to simultaneous adverse supply shocks. Overall, the results suggest that permanent supply shocks (explanation 1) are more relevant for the PO revisions than demand shocks that lead to hysteresis (explanation 2).[21]

We identify a number of variables that have predictive power for the size of post-recession PO revisions. The correlation of revisions with pre-recession credit booms, the current account development and the length of the preceding boom suggest that a substantial part of the observed post-recession revisions is due to a previous underestimation of the boom/overestimation of PO (explanation 3); in fact, these variables explain around 1/3 of the variation of post-recession PO revisions. For our sample of PO estimates, we find no evidence that revisions to PO Granger cause recessions (explanation 4), rejecting the hypothesis of Blanchard et al. (2017) that "the anticipation of a less bright future is leading to temporarily weaker demand" (p. 639).

Our results have important policy implications. On the one hand, in the light of post-recession revisions to PO estimates, monetary and fiscal policy have to consider that the need for stimulative action after economic crises is smaller than indicated by pre-recession estimates. On the other hand, if very deep economic crises lead to substantial permanent output losses (as suggested by the large explanatory power of the recession depth for subsequent PO revisions),

[21] Please note the possibility that other types of demand shocks, which we do not consider here and which might lead to hysteresis effects, could be relevant during some recessions.

there would be a strong case for more aggressive stabilization policy during economic crises to mitigate the detrimental long-run effects (see also Erceg and Levin, 2014; Blanchard et al., 2015; Galí, 2016). In fact, Romer and Romer (2017) show that stabilization policy can help to mitigate the long-term effects of financial crises.

Our findings suggest a number of directions for future research. First, it would be interesting to look at the size and timing of post-recession revisions to estimates of the components of PO, i. e., potential labor input, the capital stock, and the trend of total factor productivity.[22] This would be informative about the mechanisms through which macroeconomic shocks lead to permanent output effects and could help developing future macroeconomic models. Second, the effect of macroeconomic stabilization policy during recessions on their long-term PO effects—similar in spirit to the analysis for financial crises in Romer and Romer (2017)—requires a thorough investigation that is beyond the scope of this paper. Finally, there seems to be room for improving the approaches that are used to estimate PO; taking a broader set of macroeconomic indicators into account could potentially lead to more stable estimates that are less prone to revisions. A first attempt into this direction has been made by a number of papers that try to identify unsustainable growth episodes by taking financial data into account when estimating PO (see, e. g., Borio et al., 2014, 2017). Our results support this idea. In particular, they suggest that taking data on international capital/trade flows into account can help to improve PO estimates.

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[22]Note that this is (not yet) possible based on the EO data because the number of vintages that contain such information is relatively small since the OECD started to include these variables not before the mid 2000s.

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Appendix A List of Identified Recessions

Table A.1: Identified recessions

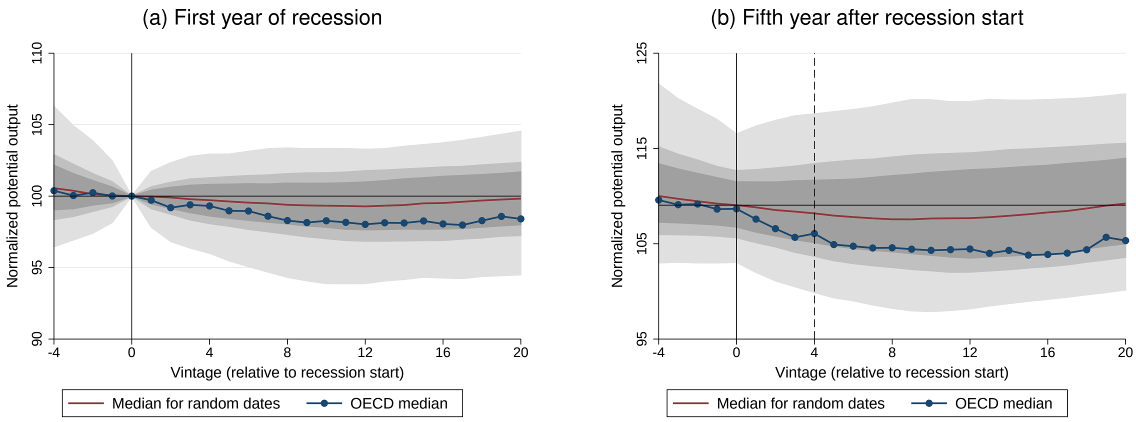
#	Country	Start	Length	Depth	#	Country	Start	Length	Depth
1	AUS	1991q1	2	−1.46	49	GRC	2007q3	26	−27.46
2	AUT	1992q4	2	−0.54	50	GRC	2014q4	4	−2.37
3	AUT	2001q1	2	−0.37	51	HUN	2008q3	7	−7.68
4	AUT	2008q2	5	−5.09	52	HUN	2012q1	2	−2.42
5	AUT	2012q2	4	−0.97	53	IRL	2008q1	8	−10.66
6	BEL	1992q2	4	−2.88	54	IRL	2012q3	3	−1.73
7	BEL	2001q1	4	−0.38	55	IRL	2017q1	2	−2.15†
8	BEL	2008q3	4	−3.83	56	ISL	2000q4	2	−1.47
9	BEL	2012q2	4	−0.80	57	ISL	2008q1	9	−15.29
10	CAN	1990q2	4	−3.43	58	ISL	2012q1	2	−2.25
11	CAN	2008q4	3	−4.48	59	ISL	2017q1	2	−1.82†
12	CAN	2015q1	2	−0.36	60	ITA	1992q2	6	−1.50
13	CHE	1990q3	4	−1.22	61	ITA	1998q1	4	−0.60
14	CHE	1992q2	3	−1.70	62	ITA	2001q2	4	−0.73
15	CHE	1995q1	2	−0.44	63	ITA	2003q1	2	−0.61
16	CHE	1996q2	2	−0.50	64	ITA	2008q2	5	−7.95
17	CHE	1998q4	2	−0.27	65	ITA	2011q3	7	−5.20
18	CHE	2002q2	4	−0.87	66	ITA	2013q4	3	−0.19
19	CHE	2008q4	3	−3.37	67	JPN	1993q2	2	−1.35
20	CZE	2008q4	3	−5.80	68	JPN	1997q2	8	−2.40
21	CZE	2012q1	5	−1.92	69	JPN	2001q2	3	−1.91
22	DEU	1995q4	2	−1.05	70	JPN	2008q2	4	−8.67
23	DEU	2001q3	3	−0.48	71	JPN	2010q4	3	−2.65
24	DEU	2002q4	10	−0.51	72	JPN	2012q2	2	−0.86
25	DEU	2008q2	4	−6.93	73	JPN	2014q2	2	−2.03
26	DEU	2012q4	2	−0.67	74	LUX	2008q2	5	−8.40
27	DNK	1992q4	3	−2.04	75	LUX	2011q4	2	−1.87
28	DNK	1997q3	2	−0.30	76	NLD	2008q3	4	−4.52
29	DNK	2001q4	3	−0.24	77	NLD	2011q2	7	−2.04
30	DNK	2006q3	4	−1.05	78	NOR	2002q3	4	−0.81
31	DNK	2008q1	6	−7.07	79	NOR	2008q1	6	−2.70
32	DNK	2011q3	6	−0.50	80	NOR	2010q2	2	−3.52
33	DNK	2015q3	2	−0.55	81	NOR	2015q4	4	−0.80
34	ESP	1992q2	5	−2.81	82	NZL	1991q1	2	−4.27
35	ESP	2008q3	6	−4.62	83	NZL	1997q4	3	−2.18
36	ESP	2010q4	12	−5.72	84	NZL	2000q2	4	−0.90
37	FIN	1990q2	13	−11.90	85	NZL	2008q1	5	−2.61
38	FIN	2008q1	6	−9.97	86	NZL	2010q3	2	−2.38
39	FIN	2012q2	4	−2.63	87	PRT	2002q2	5	−2.41
40	FIN	2013q4	6	−1.69	88	PRT	2008q2	4	−4.33
41	FRA	1992q2	4	−1.14	89	PRT	2010q4	9	−8.06
42	FRA	2008q2	5	−4.00	90	SVN	2011q3	7	−4.64
43	GBR	1990q3	5	−1.99	91	SWE	1991q1	9	−5.55
44	GBR	2008q2	5	−6.13	92	SWE	2008q1	5	−7.43
45	GRC	1990q2	2	−9.44	93	SWE	2011q4	5	−1.17
46	GRC	1992q2	4	−4.69	94	USA	1990q4	2	−1.32
47	GRC	1994q4	2	−0.87	95	USA	2008q1	6	−4.24
48	GRC	2004q4	2	−0.96					

Notes: "Length" states the duration of a recession in quarters. "Depth" refers to the deviation from the pre-recession peak level of output to the trough (in %). † indicates that a recession is ongoing at our sample end.

Appendix B Simulation based on Randomly Distributed "Recessions"

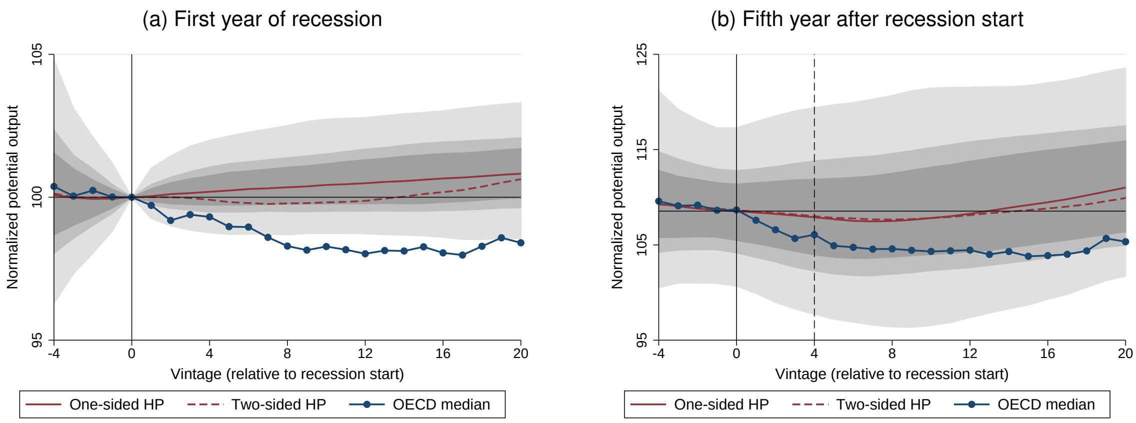
We randomly select 95 periods across countries and time for which we redo our analysis as if recessions had occurred in these periods. By repeating this, we create 1,000 data sets of randomly timed recessions. First, we calculate the median revision and percentile ranges for each data set. Then, we take the average of all 1,000 median revisions and percentile ranges to produce the figures below.

Figure B.1: Revisions to PO during random periods



Notes: The plots show the revisions to OECD PO estimates for the first/fifth year after randomly selected periods across different vintages. Values to the left of the dashed line depend on our extrapolation of the PO estimates. Grey shaded areas represent the 5^{th} to 95^{th} percentile range, the 17^{th} to 83^{th} percentile range, and the interquartile range, calculated as means across 1,000 random simulations. For comparison, we show the OECD median based on actual recessions (from Figure 2).

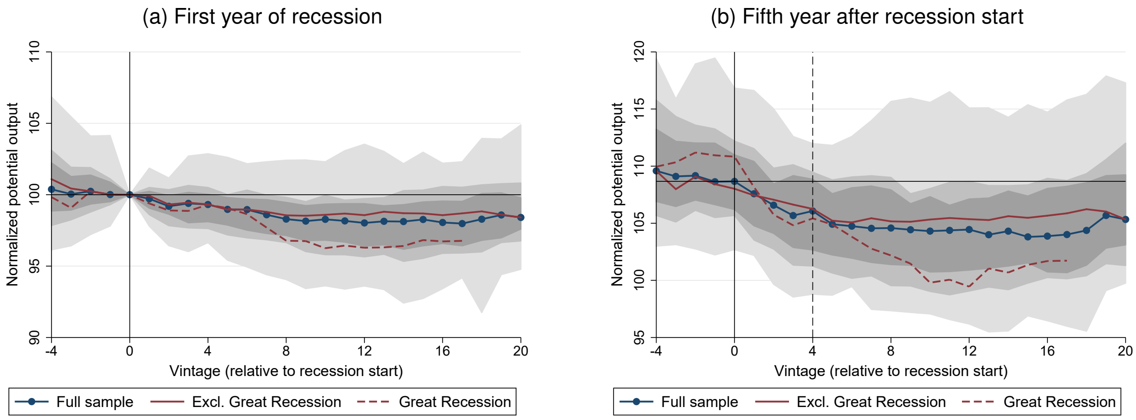
Figure B.2: Revisions based on the HP filter during random periods



Notes: The plots show the median revisions to PO estimates for the first/fifth year after randomly selected periods across different vintages based on the one-sided HP filter. Grey shaded areas represent the 5^{th} to 95^{th} percentile range, the 17^{th} to 83^{th} percentile range, and the interquartile range, calculated as means across 1,000 random simulations. For comparison, we show the median based on the two-sided HP filter for randomly selected periods and the OECD median based on actual recessions (from Figure 2); note that values to the left of the dashed line depend on our extrapolation of the OECD PO estimates.

Appendix C Analyses Excluding the Great Recession

Figure C.1: Revisions to PO estimates after recessions excluding the Great Recession



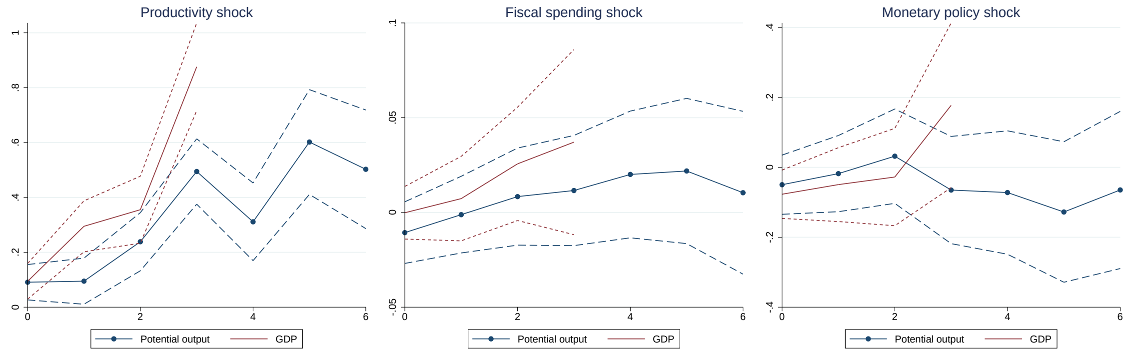
Notes: The plots show the revisions to OECD PO estimates for the first/fifth year after a recession start across different vintages. We separate the sample into 71 recessions which begin before 2007 or after 2009 and 24 recessions which start in 2007–09. Values to the left of the dashed line depend on our extrapolation of the PO estimates. The data are normalized such that the value in the first recession year as estimated in the first vintage following the start of the recession is equal to 100. For comparison, we show the median based on the full sample. Grey shaded areas represent the 5^{th} to 95^{th} percentile range, the 17^{th} to 83^{th} percentile range, and the interquartile range for the full sample.

Table C.1: Determinants of PO revisions excluding the Great Recession

Dependent variable:	$\tilde{\mathbf{y}}_\mathbf{1}^{\mathbf{v10}} - \tilde{\mathbf{y}}_\mathbf{1}^{\mathbf{v0}}$ (5)	$\tilde{\mathbf{y}}_\mathbf{5}^{\mathbf{v10}} - \tilde{\mathbf{y}}_\mathbf{5}^{\mathbf{v0}}$ (10)
Recession depth	0.250*	0.660**
	(1.91)	(2.65)
Length of prev. boom	−0.015	−0.041
	(−0.73)	(−1.08)
Δ CA (t-1)	0.324***	0.649***
	(2.79)	(2.95)
Δ Credit/GDP (t-1)	0.013	−0.012
	(0.44)	(−0.20)
EP(temp) (t-1)	0.332	0.368
	(1.51)	(0.89)
Constant	−1.483**	−1.710
	(−2.54)	(−1.55)
N	53	53
R^2	0.26	0.33

Notes: We report regressions (5) and (10) from Table 3 for a sample without the Great Recession. Specifically, we exclude 24 recessions which start in 2007–09. Numbers in parenthesis are t-statistics. ***, **, and * correspond to significance levels of 1 %, 5 %, and 10 %, respectively.

Figure C.2: Response of PO estimates and GDP forecasts to structural shocks excluding the Great Recession



Notes: The plots show the IRFs of (cumulative) revisions to OECD PO estimates and GDP forecasts to selected structural shocks estimated by excluding any data from the years 2007–09. The dashed lines indicate 90% confidence intervals and are computed based on +/− 1.645 standard errors.