# The Effects of Labor and Product Market Reforms: The Role of Macroeconomic Conditions and Policies

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## Abstract

The paper estimates the dynamic macroeconomic effects of labor and product market reforms on output, employment and productivity, and explores how these vary with prevailing macreconomic conditions and policies. We apply a local projection method to a new dataset of major country- and country-sector-level reform shocks in various areas of labor market institutions and product market regulation covering 26 advanced economies over the past four decades. Product market reforms are found to raise productivity and output, but gains materialize only slowly. The impact of labor market reforms is primarily on employment, but it varies across types of reforms and depends on overall business cycle conditions—unlike that of product market reforms. Reductions in labor tax wedges and increases in public spending on active labor market policies have larger effects during periods of slack, in part because they usually entail some degree of fiscal stimulus. In contrast, reforms to employment protection arrangements and unemployment benefit systems have positive effects in good times, but can become contractionary in periods of slack. The economy's response to such reforms is significantly improved when they are accompanied by fiscal or monetary stimulus.

JEL Classification Numbers: E32; E52; E62; H30; H53;J21; J65; L43; L51; O43; O47

Keywords: Structural reforms, Labor Market, Product Market, Deregulation, Employment Protection; Unemployment Benefits; Labor tax; Active Labor Market Policy; Growth, Productivity, Employment, Monetary Policy, Fiscal Policy.

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## I. INTRODUCTION

Deepening worries over the persistent sluggishness of growth in the advanced economies since the Great Recession have increasingly led policymakers to emphasize structural reforms. The hope is that such reforms could lift potential output over the medium term while also strengthening aggregate demand in the near term by raising consumer and business confidence. In emerging and low-income economies, agricultural, trade and domestic financial and capital account liberalization, as well as strengthening the rule of law, property rights and governance more broadly, often feature among reform priorities although important progress has been achieved in recent decades (Prati et al., 2013). In most advanced economies, which are the focus of this paper, the scope for reform in these areas is much less, and higher on the agenda are reforms designed to strengthen the functioning of product and labor markets (OECD, 2016). Although the specifics vary widely for individual countries, these reforms broadly involve: deregulating retail trade, professional services, and certain segments of network industries, primarily by reducing barriers to entry; easing hiring and dismissal regulations for regular workers; increasing the ability of and incentives for the nonemployed to find jobs by reducing the level or duration of unemployment benefits and/or by increasing the resources for and the efficiency of active labor market policies (ALMPs); cutting labor tax wedges; targeted policies to boost participation of underrepresented groups in the labor market, including youth, women, and older workers. Depending on the type of reform, the goal is to lift productivity, increase employment and/or strengthen resilience to macroeconomic shocks.

The theoretical case for labor and product market reforms has been laid out by an extensive literature that has highlighted the long-term gains from structural reforms (e.g. Blanchard and Giavazzi, 2003), but far less is known about their dynamic impact, and virtually nothing about whether that short-term impact varies depending on the economy's business cycle position or the ability of macroeconomic policies to respond to a reform-driven supply shock. Yet these issues have gained prominence in recent policy debates as many advanced economies in need of reform, including individual euro area countries and Japan, also happened to be experiencing a persistent shortfall in aggregate demand and shrinking monetary and fiscal policy space. Is the impact of reforms smaller, or instead greater, under such circumstances? Does the answer to this question vary across different types of reforms? Theory is limited and so far unsettled (see e.g. Cacciatore et al., 2016; Eggertsson et al., 2014), while comprehensive empirical evidence is still missing.

This paper aims to fill this gap. Building on a new "narrative" database of major reform shocks in a wide range of product and labor market areas for 26 advanced over the past four decades, we estimate the dynamic response of macroeconomic outcomes—real output, but also employment and labor productivity, given that transmission channels may differ—to each type of reform, and how this response varies depending on business cycle conditions and the stance of macroeconomic policies. To this end, we rely on the local projection method (Jordà, 2005), which has been used recently to study the dynamic impact

of macroeconomic shocks such as financial crises (Romer and Romer, 2015) or fiscal shocks (Jordà and Taylor, 2013). The role of macroeconomic conditions is explored using the smooth transition function proposed by Auerbach and Gorodnichenko (2012) to estimate fiscal multipliers in expansions and recessions; here we essentially use this approach to estimate the response to reform shocks instead of fiscal shocks.

The analysis yields four main findings. First, labor and product market reforms generally raise output over the medium term—by boosting employment and/or labor productivity. Past product market deregulation shocks increased GDP by about 1.5 percent on average after five years, similar to that of major unemployment benefit cuts although the latter have statically significant effects only on employment. A one percentage point cut in labor tax wedges and a 10 percent shock to public spending on ALMPs raised GDP by about 0.6 and 0.3 percent, respectively, while no significant average impact of major reforms of employment protection legislation for regular workers could be identified. Second, reforms typically take time to pay off. For example, the positive impact of product market reform becomes statistically significant only after three years, and fully materializes after about seven years. Third, the effects of labor market reforms depend significantly on business cycle conditions—unlike those of product market reforms. Cuts in labor tax wedges and increased public spending on ALMPs have larger effects during periods of economic slack, consistent with the fact that they usually entail some degree of fiscal stimulus and fiscal multipliers are typically larger in recessions (Auerbach and Gorodnichenko 2012; Blanchard and Leigh 2013; Jordà and Taylor 2013; Abiad, Furceri, and Topalova 2015). In contrast, deregulating employment protection legislation—and to a lesser extent reducing the generosity of unemployment benefit systems—can become contractionary in periods of slack, while such measures are expansionary when carried out during expansions. Fourth, and finally, there are complementarities between macroeconomic policies and reforms. Fiscal and monetary policy stimulus improve the short-term response of the economy to job protection and unemployment benefit reforms, over and above its direct impact on aggregate demand.

This paper relates to an extensive empirical literature on the macroeconomic effects of labor and/or product market reforms in advanced economies, which has relied on country- or country-sector-level data (Aghion et al., 2009; Alesina et al., 2005; Barone and Cingano, 2011; Bassanini and Duval, 2009; Bassanini et al., 2009; Blanchard and Wolfers, 2000; Bouis and Duval (2011); Bourlès et al., 2013; Fiori et al., 2012; Inklaar et al., 2008; Nickell et al., 2005; Nicoletti and Scarpetta, 2003). These studies typically focus on the long-term impact of reform, or capture their dynamic effects in a crude way through simple autoregressive-distributed lag specifications. None explores the dynamic response of outcomes to reform shocks, which is the focus of our paper. While their approaches and results vary widely, and are inherently difficult to compare to those implied by our narrative approach, they seem to be reasonably consistent with our medium-term estimates on

average.[2] A handful of recent empirical papers touch on the short-term effects of reforms. Bouis et al. (2012) find the employment response of unemployment benefit reform to materialize only gradually, with medium-term effects close to our estimates. Dabla-Norris et al. (2015) reach a broadly similar conclusion for a wider range of reforms, while Bordon et al. (2016) point out the role of macroeconomic policy support to enhance their effect. Unlike these studies, we rely on a new database that explicitly identifies and dates reform shocks based on actual regulatory and legislative changes, which is key in this context, and we build plausibly exogenous measures of fiscal and monetary policy shocks to explore complementarities between reforms and macroeconomic policies.

Finally, a parallel theoretical literature has been exploring the dynamic effects of labor and product market reforms. Conventional large-scale DSGE model featuring a range of nominal and real frictions typically imply that exogenous wage and price mark-up reductions yield gradual output gains, with those being independent of macroeconomic conditions and policies, partly due to log-linearization around steady state (e.g. Arpaia et al., 2007, Everaert and Schule, 2008; Gomes et al., 2011).[3] Cacciatore and Fiori (2016) and Cacciatore et al. (2016a,b) address these shortcomings by modeling explicitly the primitives of labor and product market regulations in a set-up with endogenous firm entry and search and matching frictions in the labor market. In this set-up, which builds and expands on Blanchard and Giavazzi (2003), cutting regulatory barriers to firm entry increases product variety and shrinks price mark-ups; reducing unemployment benefits weakens workers' outside option and thereby lowers wages and equilibrium unemployment; and reducing firing costs increases the efficiency of resource allocation across firms in the presence of idiosyncratic productivity shocks—but has an ambiguous effect on unemployment *a priori* since it increases both job creation and job destruction. Cacciatore et al. (2016a) simulate the impact of aligning all three policies on US settings for a representative European economy in the mid-2000s. They obtain an overall output gain of about 10 percent, with product market deregulation, unemployment benefit replacement rate cuts and job protection reform contributing about 6.5, 2.5 and 1 percent, respectively. This does not *a priori* differ drastically from what our estimates would imply in light of the policy gaps between both

[2] The estimated effects of labor market reforms at a five-year horizon are roughly in line with those in the literature—including the absence of a significant employment impact of job protection deregulation, see e.g. Bassanini and Duval (2009). Our estimated impact of product market reforms may be somewhat smaller, although comparisons cannot be readily made. For example, Bourlès et al. (2013) find that adopting the lightest regulatory practices observed across advanced economies in each industry would yield a 7.5 percent average GDP gain in advanced economies in the long run. We focus on the much more modest average major historical reform and find a 1.5 percent impact after 5 years (2¼ 2 percent after 7 years).

[3] One exception is when reform is carried out in a situation where monetary policy is constrained by the zero lower bound. Exogenous wage and price mark-up reductions may then have weaker short-term effects than in normal times (Eggertsson et al., 2014).

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sides of the Atlantic—especially in the mid-2000s, i.e. before the recent wave of product market deregulation in Europe (Koske et al., 2015).[4]

Cacciatore et al. (2016b) show that labor market reforms have different effects in recessions compared to normal times. In particular, easing regular job protection has contractionary effects in the short term: since more job matches are unprofitable during recessions, firms respond to reform by destroying these matches, and in turn the larger job destruction further weakens aggregate demand and output. Our paper finds supportive empirical evidence for this prediction, and more broadly for the notion that macroeconomic conditions and policies can shape the response of an economy to reform.

The remainder of this paper is structured as follows. Section II describes the dataset and the methodology used for the construction of reform shocks. Section III describes the empirical setup. Section IV presents the econometric results. Section V concludes.

## II. DATASET ON MAJOR STRUCTURAL REFORMS

### A. Product market regulation, employment protection legislation and unemployment benefits

Our analysis focuses on major policy changes in product market regulation and four key labor market policies and institutions, namely employment protection legislation for regular workers, unemployment benefits, ALMPs, and labor tax wedges.

Major reforms of product market regulation, employment protection legislation, and unemployment benefit systems are identified by Duval et al. (2016), who examine documented legislative and regulatory actions reported in all available *OECD Economic Surveys* for 26 individual advanced economies since 1970, as well as additional country-specific sources.[5] In this respect, the methodology is related to the "narrative approach" used by Romer and Romer (1989, 2004, 2010, and 2015) and Devries and others (2011) to identify monetary and fiscal shocks and periods of high financial distress.

In a first step, Duval et al. (2016) identify all legislative and regulatory actions related to product market regulation, employment protection legislation and unemployment benefits mentioned in any *OECD Economic Survey* for any of the 26 countries over the entire sample. Over 1000 such actions are identified overall. In a second step, for any of these actions to qualify as a major reform or "counter-reform"—namely a major policy change in the

[4] Using a calibrated euro area model with similar theoretical foundations, but using information on more recent (2013) regulatory settings, Cacciatore et al (2016b) find a much smaller steady-state GDP gain of about 2.2 percent for the euro area from converging to US product market regulation.

[5] The 26 countries covered are Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Slovak Republic, Spain, Sweden, Switzerland, United Kingdom and United States.

opposite direction—one of the following three alternative criteria has to be met: (1) the *OECD Economic Survey* uses strong normative language to define the action, suggestive of an important measure (for example, "major reform"); (2) the policy action is mentioned repeatedly across different editions of the *OECD Economic Survey* for the country considered, and/or in the retrospective summaries of key past reforms that are featured in some editions, which is also indicative of a major action; or (3) the existing OECD indicator of the regulatory stance in the area considered displays a very large change (in the 5$^{th}$ percentile of the distribution of the change in the indicator). The OECD indicators used for this purpose are the seven indicators of product market regulation in seven key non-manufacturing industries (telecoms, electricity, gas, post, rail, air passenger transport, and road freight), the employment protection legislation index for regular workers, and the average unemployment benefit replacement rate over a five-year unemployment spell across a set of hypothetical workers with different income and family statuses (see e.g. OECD, 2016). When only the third condition is met, an extensive search through other available domestic and national sources, including through the internet, is performed to identify the precise policy action underpinning the change in the indicator.

Table 1 provides an illustrative example on how these criteria guide the identification of major reforms and "counter-reforms" in the area of product market regulation, employment protection legislation, and unemployment benefits (for details, see Duval and others 2016). In some cases, the major action goes beyond the scope of available indicators (e.g. telecoms deregulation in the United States as a result of the antitrust lawsuit against AT&T, which was not a government measure and as such is not captured by the existing OECD indicator), or pre-dates the period over which the OECD indicator is available (Italy's 1970 "Workers' Statute"), but language used to describe it in the *Country Survey* makes its importance clear. In other cases, the available OECD indicator does not capture the full scope of the measure (1994 overhaul of the unemployment benefit system in Denmark). In other cases, the qualitative information drawn from the *Country Surveys* coincides, and is fully consistent with, the observed change in the value of the corresponding OECD indicator (1995 employment protection legislation reform in Spain).

More broadly, in a context where our goal is to identify and trace out economies' responses to major reform shocks, this approach has several strengths compared to indirect methods used in other papers that rely exclusively on changes in OECD policy indicators. Specifically, the reform database: identifies the exact timing of major legislative and regulatory actions; identifies the precise reforms that underpin what otherwise looks like a gradual decline in OECD policy indicators without any obvious break (for example, the series of reforms that took place in the telecommunications industry in many countries in the mid-late 1990s); captures reforms in areas for which OECD indicators exist but do not cover all relevant policy dimensions; covers a longer time period in some policy areas, such as regarding employment protection legislation; documents and describes the precise legislative and regulatory actions that underpin observed large changes in OECD indicators.

Finally, compared with yet other existing databases on policy actions in the area of labor market institutions, such as the European Commission's *Labref*, the Fondazione Rodolfo de Benedetti-IZA database, and the ILO's *EPLex* database, the approach taken here allows identifying a rather limited set of major legislative and regulatory reforms, as opposed to just a long list of actions that in some cases would be expected to have little or no bearing on macroeconomic outcomes.

Overall, an important advantage of this database is to identify the precise nature and timing of *major* legislative and regulatory actions taken by advanced economies since the early 1970s in key labor and product market policy areas. Specifically, compared with existing databases on policy actions in the area of labor market institutions (such as the European commission *Labref*, the Fondazione Rodolfo de Benedetti-IZA, and the ILO-*EPLex* database), the approach allows identifying major legislative and regulatory reforms as opposed to just actions. This is particularly useful for empirical analysis that seeks to identify, and then estimate, the effects or the drivers of reform shocks.

The major strengths of this narrative reform database come with one limitation; because two large reforms in a given area (for example, employment protection legislation) can involve different specific actions (for example, a major simplification of the procedures for individual and collective dismissals, respectively), only the average impact across major historical reforms can be estimated. As a robustness check, we re-run our regressions using instead as reform shocks large changes in OECD indicator values, and show that while our main results are robust to using this methodology, the effects of reforms are weaker and less precisely estimated compared with the narrative approach. This suggests that the latter indeed better identifies major reform events and thereby reduces measurement error. It should also be highlighted that the reform database provides no information regarding the *stance* of current (or past) product and labor market regulations, which is not the purpose of this paper.

Figure 1 provides the number of reforms identified in the sample, and illustrates the heterogeneity of reforms efforts across regulatory areas. Product market reforms have been most frequently implemented, in particular in telecommunications and air transport that underwent major deregulation in many countries between the early 1980s and early 2000s. In general, fewer major reforms have been implemented in the areas of employment protection legislation for regular workers and unemployment benefit systems (replacement rate and/or duration)—about 35 or so in each area.

### B. Labor tax wedges and ALMP spending

For policy changes in the areas of labor tax wedges and public spending on ALMPs, there is no need to follow the approach described above, since these are simple tax and spending—as opposed to harder-to-quantify regulatory—measures that can be computed readily from available OECD data.

Labor tax wedge shocks are identified as the annual change in the measure of tax wedges published in OECD *Taxing Wages*, which is derived from tax models. The measure is defined as the wedge between the labor cost to the employer and the corresponding net take-home pay of the employee for an average single-earner couple with two children, and it expresses the sum of personal income tax and all social security contributions as a percentage of total labor cost.

Data for public spending on active labor market policies are taken from the OECD *Social Expenditure database*. In order to isolate discretionary spending shocks from automatic changes in spending driven by business cycle fluctuations, we follow an approach inspired by Perotti (1999) and Corsetti, Meier, and Müller (2012). Specifically, spending shocks are identified as innovations to past spending and economic activity as well as to expectations about current economic activity, that is as the residuals from the following regression:

$$\Delta s_{it} = \alpha_i + \gamma_t + \beta_i \Delta y_{it} + \delta_i \Delta y^E_{it-1} + \varepsilon_{it},$$

in which $\Delta s$ denotes the growth rate of public spending on active labor market policies; $\Delta y$ is GDP growth; $\Delta y^E$ denotes the OECD forecast for GDP growth at time *t*, made at *t–1*; $\alpha_i$ and $\gamma_t$ are country and time fixed effects, respectively.

### C. Macroeconomic policy shocks

In order to study interactions between reforms and macroeconomic policies, we build plausibly exogenous measures of fiscal and monetary policy shocks.

Fiscal policy shocks are identified as the forecast error of government consumption expenditure to GDP, following the approach used by Auerbach and Gorodnichenko (2012). The forecasts of government consumption used in the analysis are those reported in the fall issue of the OECD's *Economic Outlook* for the same year.[6] This procedure overcomes the problem of fiscal foresight (Forni and Gambetti 2010; Leeper, Richter, and Walker 2012, 2013; Ben Zeev and Pappa 2014), because it aligns the economic agents' and the econometrician's information sets.

The approach we follow to construct monetary policy shocks is similar in spirit. Specifically, monetary policy shocks are computed as the forecast error in monetary policy rates that is orthogonal to unexpected changes in output growth and inflation. The forecasts

[6] Results are robust to using instead the forecasts of the spring issue of the same year or the fall issue of the previous year. These are available upon request.

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of policy rates used in the analysis are those reported in *Consensus* Economics in October of the same year.[7]

These shocks are orthogonal to reform shocks, as well as to macroeconomic conditions (Table 2). In particular, the correlation between fiscal and monetary shocks on the one hand, and reforms of employment protection legislation or unemployment benefits systems—the two types of reform shocks for which we find significant complementarities with macroeconomic policy stimulus below—on the other hand, is found to be close to zero. Likewise, the correlation between fiscal and monetary shocks on the one hand, and the variable capturing macroeconomic conditions—which we define below—or its change on the other hand, is also close to zero.

Finally, the macroeconomic series for GDP, employment and productivity used in the analysis come from the OECD's Statistics and Projections database, which covers an unbalanced sample of 26 OECD economies over the period 1970–2014. The sectoral series used in the sector-level analysis of the impact of product market deregulation (see below) come from the EU KLEMS and World KLEMS databases, which provide annual information on sectoral input, output, and prices over the period 1970–2007.

## III. EMPIRICAL METHODOLOGY

### A. Country-level analysis

In order to estimate the dynamic response of output, employment and labor productivity to reform shocks, we follow the local projection method proposed by Jordà (2005) to estimate impulse-response functions. This approach has been advocated by Stock and Watson (2007) and Auerbach and Gorodnichencko (2012), among others, as a flexible alternative to vector autoregression (autoregressive distributed lag) specifications since it does not impose dynamic restrictions. It is also particularly suited to estimating nonlinearities in the dynamic response—such as, in our context, interactions between reform shocks and macroeconomic conditions and policies. The baseline specification is:

$$y_{t+k,i} - y_{t-1,i} = \alpha_i + \gamma_t + \beta_k R_{i,t} + \theta X_{i,t} + \varepsilon_{i,t} \qquad (1)$$

[7] In particular, we first compute the forecast error of the policy rates ($FE_t^i$)—defined as the difference between the actual policy rates ($ST_t^i$) and the rate expected by analysts as of October of the same year ($CT_{t-1,t}^i$): $FE_{i,t}^i = ST_{i,t}^i - CT_{i,t-1,t}^i$— and then regress for each country the forecast errors of the policy rates ($ST_t^i$) on similarly computed forecast errors of inflation ($FE^{inf}$) and output growth ($FE^g$): $FE_{i,t}^i = \alpha + \beta FE_{i,t}^{inf} + \gamma FE_{i,t}^g + \epsilon_{i,t}$ where the residual series—$\epsilon_{i,t}$— captures exogenous monetary policy shocks (*MP*). We use Consensus Economics forecasts rather than the OECD's *Economic Outlook* because of the greater country and time coverage of the former.

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in which *y* is the variable of interest, namely the log of GDP, employment or labor productivity (defined as the ratio of GDP to employment); $\beta_k$ denotes the (cumulative) response of the variable of interest in each *k* year after the reform; $\alpha_i$ are country fixed effects, included to take account of differences in countries' average growth rates; $\gamma_t$ are time fixed effects, included to take account of global shocks; $R_{it}$ denotes the reform shock in the area considered;[8] and $X_{it}$ is a set a of control variables including two lags of reform shocks, as well as lags of GDP growth and recession dummies—to control for the fact that economic conditions may shape the likelihood of reform, as is the case for example according to the "crisis-induces-reform" hypothesis (Drazen and Easterly, 2001; Velasco, 1996).[9]

Reverse causality and omitted variable bias are two common issues involved in cross-country time-series estimation of the impact of policies and institutions on macroeconomic outcomes. The risk of reverse causality stems from the fact that the probability of reform may depend not only on past growth and the occurrence of recessions—which are controlled for—but also on current and expected future growth. We address this concern by also controlling for the expected values in *t-1* of future GDP growth rates over periods *t* to *t+k*. These are sourced from the fall issue of the OECD's *Economic Outlook* for year *t-1*. Omitted variable bias may arise from the fact that reforms may be implemented across different areas at the same time. In practice, however, as shown by robustness checks further below, including all reform shocks in all areas simultaneously in the estimated equation does not materially affect the magnitude and statistical significance of the results.[10]

This baseline specification is then extended to allow the response to vary with the state of the economy (output or credit growth conditions) or the stance of macroeconomic policy as follows:

$$y_{i,t+k} - y_{i,t-1} = \alpha_i + \gamma_t + \beta_k^L F(z_{i,t}) R_{i,t} + \beta_k^H (1 - F(z_{i,t})) R_{i,t} + \theta Z_{i,t} + \varepsilon_{i,t} \qquad (2)$$

with

[8] All reform shocks featured in our database are country-wide shocks, with the exception of product market reform shocks which are constructed at the country-sector level for seven different network industries. For the country-level analysis, the latter are converted into country-wide product market reform shocks as follows. A major reform is considered to take place in country i in year t when at least two out of the seven network industries experience a reform, which in practice corresponds to the 90th percentile of the distribution of the sum of all seven reform dummy variables. Similar results—not reported below but available upon request—are obtained when using the distribution of the weighted sum of the reform dummies instead, with weights equal to the (country-sector specific time-varying) share of value added of each sector in GDP.

[9] The results are robust to different number of lags.

[10] Estimates could be biased in the event of reform reversals. In practice, however, this bias is negligible, as there are only very few such cases. Furthermore, the results are robust to controlling for future reform shocks.

$$F(z_{it}) = \frac{\exp(-\gamma z_{it})}{1+\exp(-\gamma z_{it})}, \quad \gamma > 0$$

in which $z_{it}$ is an indicator of the state of the economy (or the stance of monetary or fiscal policy) normalized to have zero mean and unit variance, and $Z_{it}$ is the same set of control variables used in the baseline specification but now also including $F(z_{it})$. The indicator of the state of the economy is GDP growth (credit growth), while the indicator of the stance of fiscal (monetary) policy is the fiscal (monetary) shock variable computed above.

$F_{it}$ is a smooth transition function used by Auerbach and Gorodichencko (2012) to estimate the macroeconomic impact of fiscal policy shocks in expansions versus recessions. Similar to them, we use $\gamma = 1.5$ for our analysis of the impact of reform shocks in recessions versus expansions. This approach is equivalent to the smooth transition autoregressive model developed by Granger and Terävistra (1993). The advantage of this approach is twofold. First, compared with a model in which each dependent variable would be interacted with a measure of the business cycle position, it allows to directly test whether the effect of reforms varies across different regimes such as recessions and expansions. Second, compared with estimating structural vector autoregressions for each regime it allows the effect of reforms to change smoothly between recessions and expansions by considering a continuum of states to compute the impulse response functions, thus making the response more stable and precise.

Equations (1) and (2) are estimated for each $k = 0, \ldots, 4$. Impulse response functions are computed using the estimated coefficients $\beta_k$, and the confidence bands associated with the estimated impulse-response functions are obtained using the estimated standard errors of the coefficients $\beta_k$, based on clustered robust standard errors.

**B. Sector-level analysis**

For product market reforms, we supplement the country-level estimates with country sector-level analysis. These enable us further minimize endogeneity issues, and to explore the channels through which reform affects macroeconomic outcomes. There are three potential channels: 1) a direct effect, under which reform can affect output, productivity and/or employment in the deregulated industry itself; 2) a forward spillover, under which reform in "upstream" industries—such as network industries—can reduce the price and improve the quality and variety of the intermediate inputs used by downstream sectors, thereby boosting their productivity (Barone and Cingano, 2011; Bourles et al., 2013); 3) a backward spillover, under which an increase in output in the deregulated industry increases its demand for intermediate inputs from upstream sectors. For example, deregulation in the electricity sector may positively affect other sectors by both reducing their costs of production (backward linkage) and requiring more inputs from these sectors (forward linkage).

Bouis et al. (2016) find strong evidence for a direct effect on output and productivity—but not on employment—using OECD STAN data. Here we focus on the

backward and forward spillovers from reform. These two indirect effects of product market reforms on sectoral output are estimated using an identification assumption similar in spirit to Rajan and Zingales (1998), that is the fact that spillovers should be larger in industries that are more exposed to the deregulated industry via input-output linkages. Specifically, we estimate the following specification:

$$y_{j,i,t+k} - y_{j,i,t-1} = \alpha_{ij} + \gamma_{it} + trend_j + \beta_k \sum_{s \neq j} \omega_{js,i,t}^{I/O} R_{j,i,t} + \varepsilon_{j,i,t} \qquad (3)$$

in which $\omega_{js,i,t}^{I}$ is the share of intermediate inputs provided by each network industry *s* in country *i* to downstream industry j, and $\omega_{js,i,t}^{O}$ the share of intermediate inputs provided by each industry *j* in country *i* to downstream network industry *s*. Separate regressions are run to estimate backward and forward spillovers. Country-year fixed effects $\alpha_{it}$ allow us to control for any variation that is common to all sectors of a country's economy, such as country-wide macroeconomic shocks and reforms in other areas, including labor market reforms. Country-industry fixed effects $\gamma_{ij}$ control for industry-specific factors, such as cross-country differences in the growth of certain sectors that could arise from differences in comparative advantage. The industry-specific time trend ($trend_j$) is meant to control for the different trend growth rates of different industries at the global level, partly reflecting different rates of technological progress—for example, the boom of the telecommunications industry observed over the sample period.[11] To minimize endogeneity issues and measurement errors, the weights $\omega_{js,i,t}^{I/O}$ are based on input-output data for the year 2000.[12]

## IV. RESULTS

### A. Product market reforms

*Cross-country analysis*

Figure 2 (Panel A) shows the estimated dynamic response of GDP, employment and productivity to product market reform shocks over the five-year period following reform implementation, together with the 90% confidence interval around the point estimate. Major deregulation episodes have a positive and statistically significant output effect over the medium term, of about 1½ percent four years after the reform, which however materializes only gradually. The effect eventually levels off, after seven years, at about 2¼ percent. While

[11] As reforms in some sectors have been clustered around particular years, we use an industry-specific time trend rather than industry-year fixed effects, as the latter would unduly absorb some of the impact of product market deregulation.

[12] Similar results are obtained using 1996 input-output data instead.

the point estimates suggest that both employment and labor productivity rise, none of these effects is *individually* statistically significant.

The GDP impact of product market deregulation does not appear to vary with prevailing business conditions: we find no statistically significant difference in the impact of reform between expansion and recession regimes for any of the four years after the reform. This finding is consistent with recent theoretical work by Cacciatore et al. (2016b). In a two-country DSGE model with endogenous producer entry and labor market frictions, they find that deregulating barriers to entry has broadly similar effects on firm entry, and therefore on the dynamics of the economy, in recessions and normal times. This is because two offsetting effects roughly cancel out. On the one hand, compared with normal times, expected profits among prospective entrants are smaller during recessions, which discourages firm entry. On the other hand, the number of competing firms shrinks during recessions, leading to higher profit margins and stimulating firm entry, all else equal.

In contrast, we do find that the response of output to product market reforms varies significantly with the credit cycle (Figure 2, Panel B). In particular, while the output response is statistically larger and positive during periods of favorable credit conditions (high and positive credit growth), it is negative and not statistically significant during periods of tight credit conditions (highly negative credit growth). Interestingly, this differential effect seems mainly driven by a stronger response of employment in credit booms than in credit downturns. These results are consistent with a role of favorable credit conditions in fostering entry of new firms in the aftermath of deregulation.[13]

*Sectoral analysis*

The sector-level analysis of the backward and forward spillovers from product market reform broadly confirms the results country-level results and highlights that both transmission channels matter. Product market reforms in network sectors have a statistically significant indirect medium-term impact on output in both downstream and upstream industries, of about 0.3 percent on average four years after the reform (Figure 3). In both cases the output effect is mostly driven by an increase in labor productivity, while the response of employment is not statistically significant.

## B. Labor market reforms

*Employment Protection Legislation*

[13] These results are qualitatively robust to several financial indicators such as private credit to GDP, overall credit to GDP, stock market capitalization and money growth.

Major reforms of employment protection legislation for regular workers are not found to have, on average, a statistically significant short to medium-term impact on output, employment or productivity (Figure 4, Panel A), but this masks widely different effects according to overall business conditions (Panel B). In an expansion, reforms have a sizable positive and statistically significant impact on output and employment, whereas they have a negative and statistically significant impact in a recession—the difference in the response across the two economic regimes is statistically significant.

As shown in Cacciatore et al. (2016), the theoretical rationale for this asymmetric effect across different economic regimes stems is that reform affects differently firms' hiring versus firing incentives in good and bad times. In a recession, firms seek to dismiss more and hire less than in a boom, but stringent job protection partly discourages them from laying off; relaxing that constraint triggers a wave of layoffs, increasing unemployment, weakening aggregate demand and delaying the recovery.

*Unemployment benefits*

Major unemployment benefit reforms are found to have statistically significant effects on employment that materialize gradually, increasing employment by over 2 percent four years after the reform (Figure 5, Panel A).[14] These reforms tend to have larger effects during periods of expansion than during periods of slack, even though the difference is not statistically significant (Figure 5, Panel B).[15]

*Labor tax wedges*

Shocks to labor tax wedges have statistically significant short- and medium-term effects on output and employment but, as would be expected, not on productivity (Figure 6). A reduction of 1 percent in labor tax wedges increases the level of output (employment) by about 0.15 (0.2) percent the year of the shock and by about 0.6 (0.7) percent after four years.

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[14] Similar results—albeit somewhat stronger and more statistically significant—are found for the effect of these reforms on unemployment.

[15] There are three possible, non-mutually exclusive explanations for an asymmetric impact of unemployment benefits. First, fiscal multipliers tend to be larger in general during recessions (Auerbach and Gorodnichenko 2012; Blanchard and Leigh 2013; Jordà and Taylor 2013; Abiad, Furceri, and Topalova 2015). This may hold particularly for changes in unemployment benefits, because households also become more credit constrained in downturns (Mian and Sufi 2010). Second, benefit cuts may increase income uncertainty, and therefore precautionary saving, more in recessions than in normal times. Using a heterogeneous-agents model that combines matching frictions in the labor market with incomplete asset markets and nominal rigidities, Ravn and Sterk (2013) show that a reduction in consumption in favor of precautionary saving decreases aggregate demand and firms' hiring, thereby further weakening demand. Third, recessions may be periods when the number of available jobs tends to be rationed (Landais, Michaillat, and Saez, 2015), or periods when hiring is less responsive to benefit policy changes more broadly (Jung and Kuester, 2015), although this remains the subject of an intense theoretical and empirical debate.

The output effect levels off seven years after the tax cut at about 0.8 percent. In addition, cuts in labor tax wedges have statistically significantly larger effects during periods of slack. In such periods, a 1 percent reduction in labor tax wedges increases output (employment) by 0.7 (0.4) percent in the year of the reform and by 1.2 (1.3) percent after four years, whereas in expansions, the impact is not statistically significant. This finding is again consistent with the growing literature that points to larger fiscal multiplier effects during recessions.

*Public spending on active labor market policies*

Discretionary increases in public spending on active labor market policies are also found to raise output and employment gradually (Figure 7). A 10 percent increase in spending increases output and employment by about 0.3 percent four years after the shock, and stabilizes afterward. In addition, the effect on output materializes quickly, reaching 0.2 percent one year after the shock. Given that average spending on active labor market policies across the sample is about 1 percent of GDP, this implies a one-year-ahead multiplier of about 1.2, consistent with other estimates reported in the literature (see Coenen and others 2012 and literature cited therein). Positive shocks to spending on active labor market policies tend to have bigger effects in bad times, even though the difference is not statistically significant.

## C. The role of macroeconomic policies

We now study complementarities between structural and macroeconomic policies by assessing whether the impulse response to a reform shock is shaped by the macroeconomic policy stance—expansionary or contractionary—at the time of reform. This is done by re-estimating equation (2) above but now computing $z_{it}$ as the indicator of fiscal or monetary policy shocks. We focus on job protection and unemployment benefit reforms which the previous section showed could entail short-term costs if carried out under slack.[16]

We find that expansionary fiscal or monetary policy stimulus (contraction) enhances (weakens) the impact of both types of reforms on output and employment, *over and above* its direct impact (Figure 8 and 9).[17] Specifically, while employment protection legislation and

[16] For product market reforms, no statistically significant difference in impulse responses for output, employment or productivity is found across different macroeconomic policy regimes. For labor tax wedge cuts and increases in public spending on active labor market policies, which often involve a fiscal shock in themselves, there is no case for carrying out this exercise.

[17] A potential concern regarding the analysis is that fiscal shocks may respond to output growth surprises. However, as discussed above, these shocks are only weakly correlated with growth surprises. Moreover,

(continued)

unemployment benefit reforms increase output when accompanied by fiscal or monetary stimulus, they have contractionary effects when carried during periods of fiscal or monetary contraction—the difference in the response is statistically significant.[18] These results are consistent with the weaker impact of these reforms in recessions compared with expansions, since fiscal and monetary stimulus, by boosting demand and output, contribute to shift away from recession and toward expansion (Table A1).[19]

### D. Robustness checks

We perform two important robustness checks on the results. First, as discussed in the previous section, a possible concern regarding the analysis is that the results may suffer from omitted variable bias, as reforms may be carried out across different areas at the same time. In order to control for this source of endogeneity, equation (1) is re-estimated by including all reform shocks in all areas simultaneously. The results obtained with this analysis are very similar to, and not statistically different from, those obtained in the baseline specification, suggesting that omitted bias is likely to be negligible (Figure 10).[20]

Second, we noted that, for all its strengths, the narrative dataset cannot be directly used to quantify the intensity of reforms, and as a result only the average impact across major historical reforms can be estimated. As a robustness check, we re-run our regressions using as reform shocks the interaction between the reform dummy identified in our narrative approach and changes in the corresponding OECD policy indicators in areas where the latter exist.[21] The point estimates from this analysis are qualitatively consistent with, but weaker

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purifying our measure of fiscal shocks by purging it from the portion explained by growth surprises delivers results that are similar to those reported here.

[18] Similar results are found for employment, and also when repeating the analysis using public investment shocks—computed similarly to government consumption shocks as the forecast error of government investment to GDP (see Figure A1 in Appendix).

[19] Table A1 reports the response of output and the smooth transition function to fiscal and monetary policy shocks. It shows that an unexpected 1 percent of GDP increase in government consumption raises output by 0.35 percent and decreases the smooth transition function—which is bounded between 0 (extreme expansion) and 1 (extreme recession)—by 0.04 (that is, 4 percentage points). Likewise, an expansionary monetary policy shock of 100 basis points is found to increase output by 0.30 percent and decrease the smooth transition function by 0.03—that is, 3 percentage points.

[20] Similar results are also obtained for productivity and employment.

[21] In order to compare these results with those obtained in the baseline, we scale the estimated effect by the average change in the OECD indicator corresponding to our major reform event. For example, in the case of product market regulation we multiply the estimated effect by 0.42, which is the average change in the OECD indicator (of product market regulation in seven non-manufacturing industries, see Koske et al., 2015) when our reform dummy takes value 1.

and less precisely estimated than those from the narrative approach (Figure 11). Insofar as the true (unknown) output effect of reform is positive, this suggests that the narrative approach may indeed better identify major reform events and thereby reduce measurement error.

## V. CONCLUSION

Labor and product market reforms are high on policy makers' agendas in many advanced economies. An extensive macro and micro-econometric literature has explored empirically their impact, but little is known about their dynamic effects, and even less so about whether these may be shaped by the state of the economy and the stance of macroeconomic policy at the time of reform—issues that recent theoretical papers have started to tackle. This paper fills this gap, using a new reform dataset that identifies and dates carefully the occurrence of major reform in several key labor and product market regulation areas, and estimate the impulse response of aggregate output, employment and labor productivity to these reform shocks by means of a local projection method. We find that the product and labor market reforms considered here generally pay off over the medium term, but these gains typically materialize only gradually. Labor market reforms that often involve short-term fiscal stimulus, such as cutting labor taxes or raising public spending on active labor market policies, have a greater pay-off when economic conditions are weak. By contrast, easing job protection for existing regular workers or reducing the generosity of unemployment benefits has a weaker short-term effect, and indeed can entail costs by lowering aggregate demand, when economic conditions are weak. Macroeconomic policy stimulus improves the response of the economy to these reforms, *over and above* its conventional effect on aggregate demand.

These findings suggest that prioritizing and sequencing reforms can enhance their short to medium-term impact in an environment of persistent slack. Reforms that entail fiscal stimulus will be particularly valuable, including reducing labor tax wedges and increasing public spending on active labor market policies. Productivity-enhancing product market reforms should also be prioritized under such circumstances, because their impact does not hinge significantly on prevailing economic conditions—although it materializes only gradually. Other labor market reforms can be costly in the short term, reducing rather than increasing employment. Other than postponing them until better times, one way to address short-term costs is through accompanying macroeconomic policy stimulus. Other options, that are fruitful avenues for future research, may include credibly announcing today reforms that will come into force only the future, or grandfathering them, that is applying them only to new beneficiaries—a design feature of many of the post-global financial crisis reforms of employment protection legislation in Europe, notably in Italy, Portugal and Spain.

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# Table 1. Examples of reforms identified

| Reform (+) or Counter-reform (–) | Implementation Year | Area | Country | Content | Normative Language featured in OECD Country Survey | Mention in later Surveys | Large Change in OECD Indicator |
|---|---|---|---|---|---|---|---|
| + | 1984 | Product market (telecommun-ications) | United States | Antitrust suit against AT&T | The most important deregulatory move in telecommunications came with the antitrust suit against AT&T by the United States. Competition for long-distance voice services entered a new phase in 1984. | 1986, 1989, 2004 | No |
| + | Late-1994 /1995 | Employment protection legislation | Spain | Draft law modifying the current law regulating employment. It introduces dismissals of permanent workers. | … far-reaching labor market reforms aimed at lifting barriers to job creation. A decree was passed at the end of December 1993, and a draft has been presented to Parliament and is expected to become law by the middle of 1994. | No | Yes for 1995 |
| – | 1970 | Employment protection legislation | Italy | Act of 1970, referred as the "workers' statute" | The Act of 1970 referred to as the "workers' statute" laid the basis for employer-employee relations and regulations concerning hiring. The two main sources of rigidity seem to be the regulations governing hiring and firing. The conditions and procedures for hiring workers are extremely stringent, particularly for large firms. | 1986 | |
| + | 1994 | Unemployment benefits | Denmark | Labor market reforms of 1994: activation of the unemployed, limiting the duration of unemployment benefits, enforcing job availability criteria, compulsory full-time activation, stricter eligibility criteria. | The measures taken …are steps in the right direction. Training and education offers are fully operational, a foundation has been established for reducing the duration of unemployment benefits on a sustainable basis. | 2000 | Yes for 1994 (replacement rate); other aspects (duration, eligibility, active policies) not captured |

## Table 2. Matrix of correlations between reform shocks and other key variables

| | Product market reform | EPL reform | Unemployment benefits reform | ALMP shocks | Labor tax wedge shocks | Fiscal policy shocks | Monetary policy shocks | State business cycle | Change of state business cycle |
|---|---|---|---|---|---|---|---|---|---|
| Product market reform | 1 | | | | | | | | |
| EPL reform | -0.011 | 1 | | | | | | | |
| Unemployment benefits reform | -0.045 | -0.023 | 1 | | | | | | |
| ALMP shocks | -0.065 | -0.086 | -0.046 | 1 | | | | | |
| Labor tax wedge shocks | -0.045 | -0.118 | 0.110 | 0.153 | 1 | | | | |
| Fiscal policy shocks | 0.096 | 0.138 | 0.025 | 0.065 | -0.033 | 1 | | | |
| Monetary policy shocks | 0.152 | 0.094 | 0.019 | 0.029 | -0.061 | 0.068 | 1 | | |
| State business cycle | -0.102 | 0.096 | -0.135 | 0.135 | -0.005 | -0.028 | -0.186 | 1 | |
| Change of state business cycle | -0.090 | 0.034 | -0.044 | 0.039 | 0.108 | 0.056 | -0.156 | 0.549 | 1 |

**Figure 1. Number of reforms shocks** (26 advanced economies, 1970-2013)

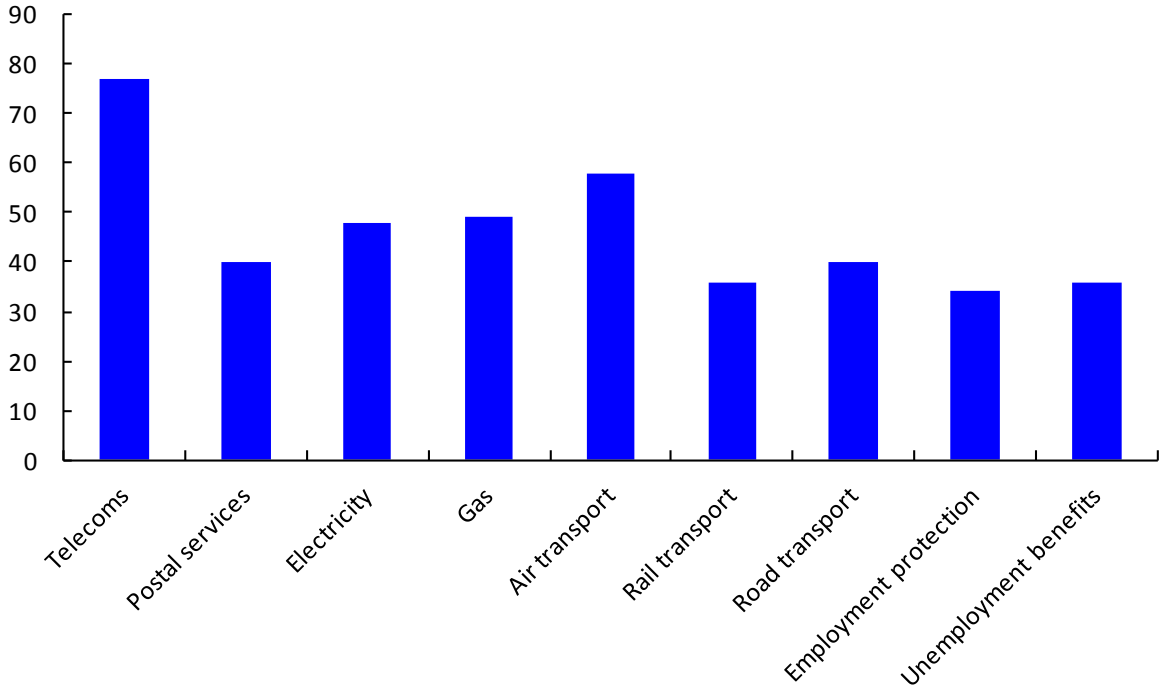
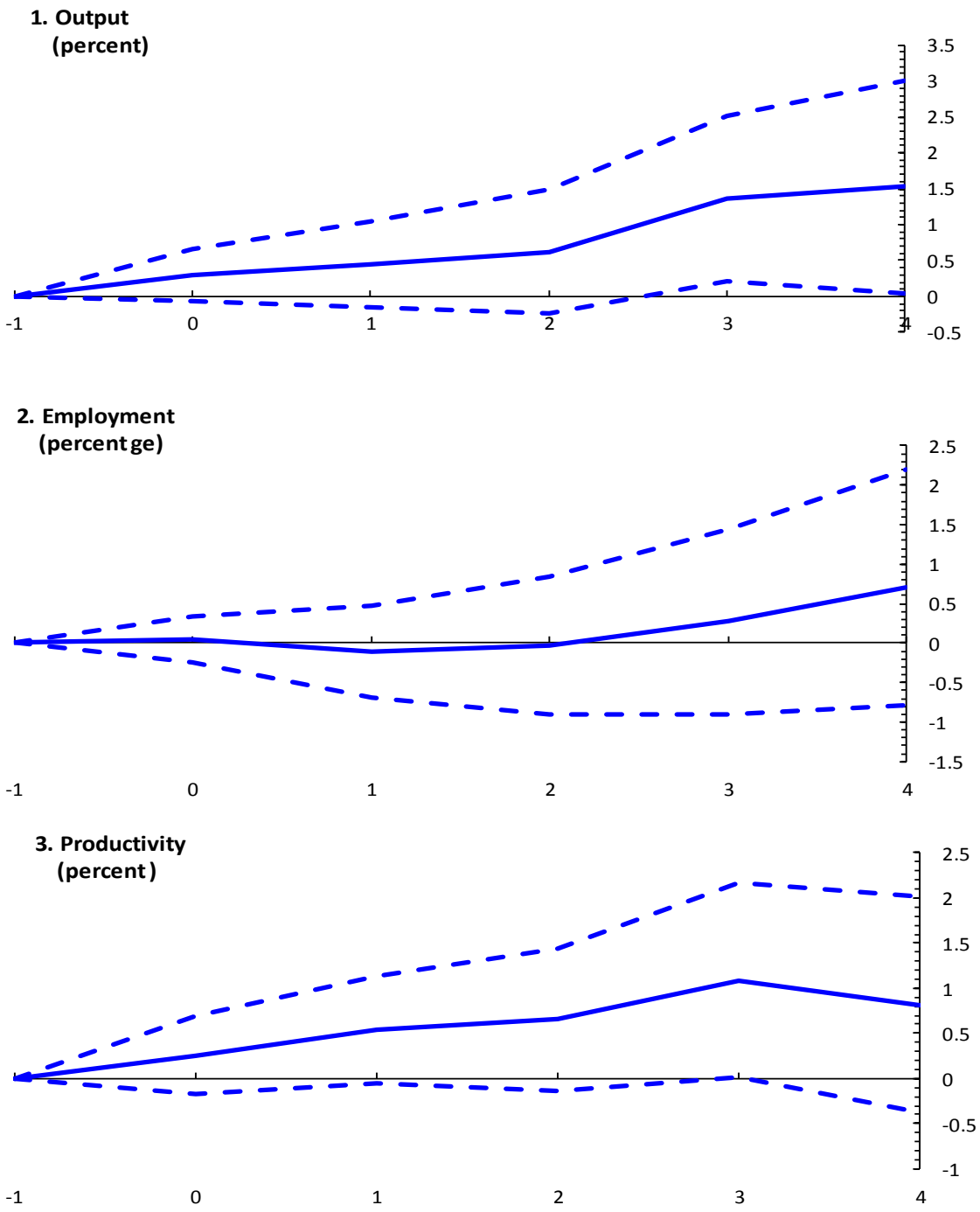


**Figure 2. Impulse responses to product market deregulation: country-level results**

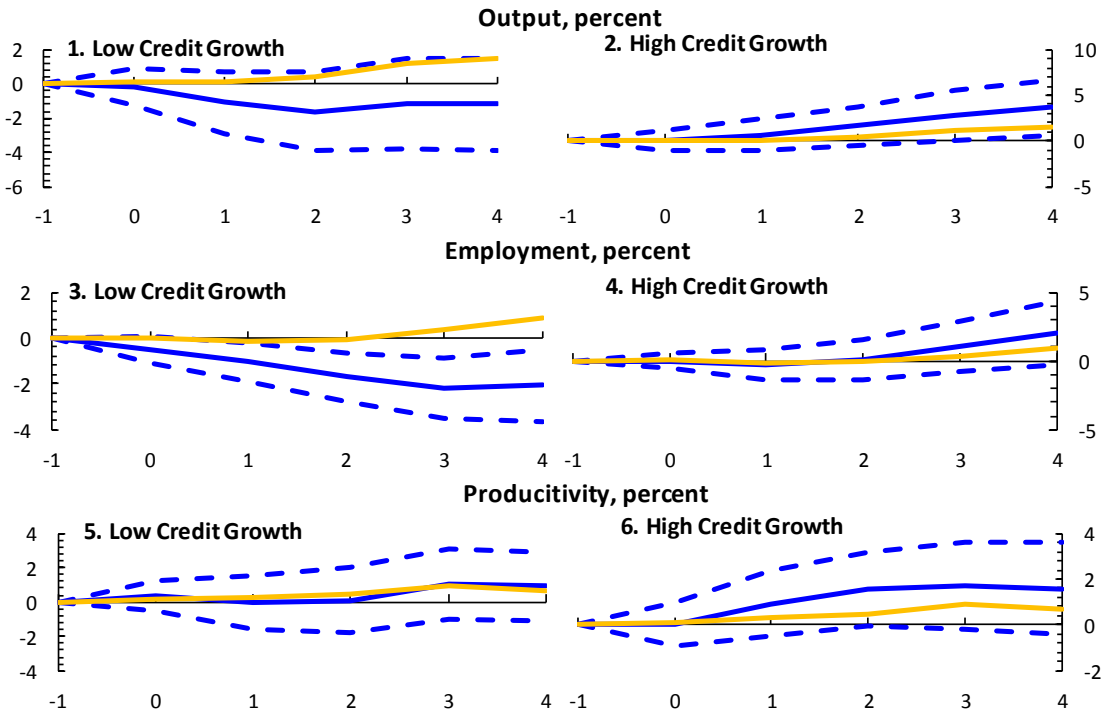
**Panel A. Baseline**




Note: t=0 is the year of the reform; dotted lines denote 90 percent confidence bands. Estimates based on equation (1).

**Figure 2. Impulse responses to product market deregulation: country-level results**

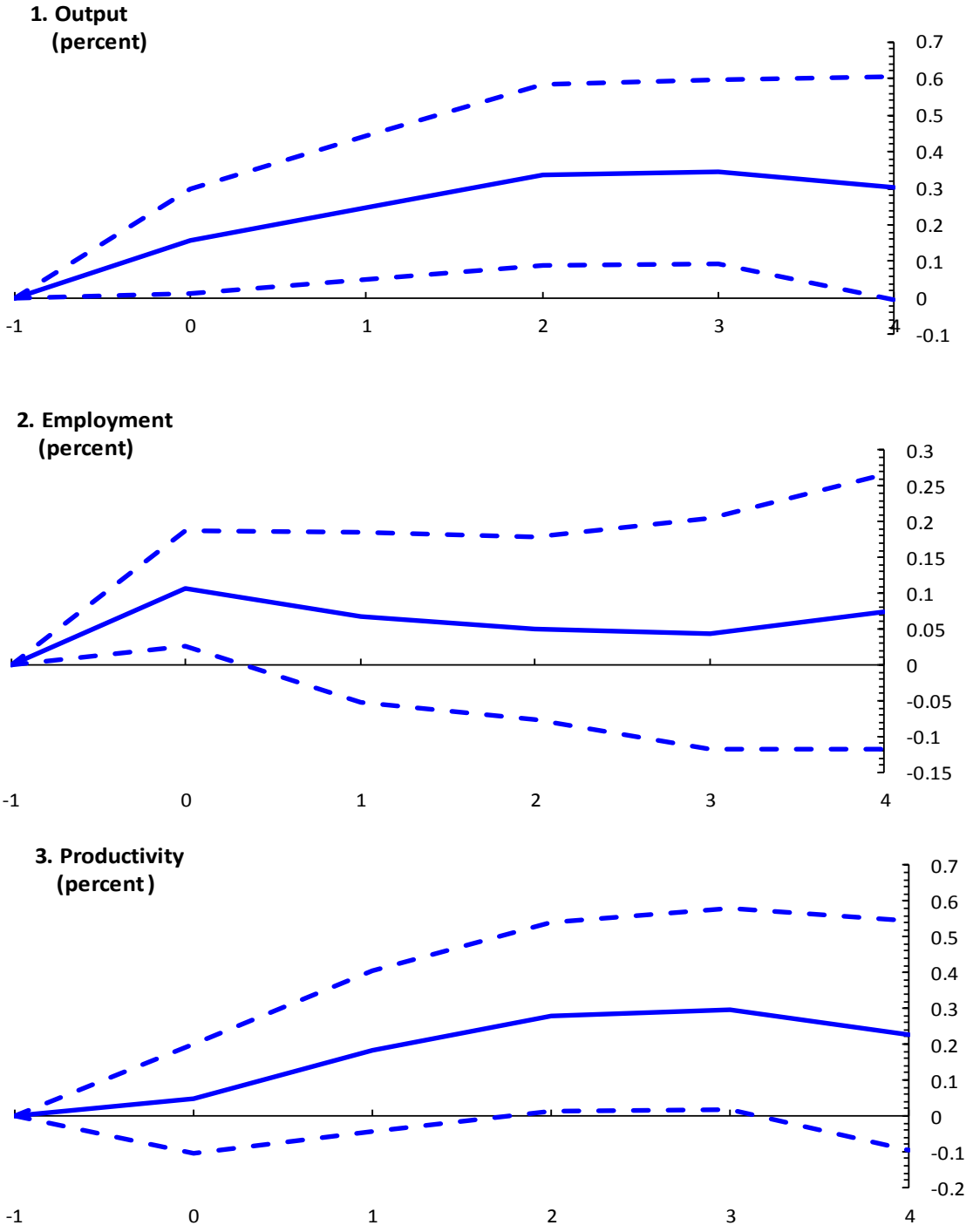
**Panel B. Low vs. high credit growth**




Note: t=0 is the year of the reform Solid blue lines denote the response to reform and dashed lines denote 90 percent confidence bands. Solid yellow lines denote the unconditional (baseline) response presented in Panel A. Estimates based on equation (2).

**Figure 3. Impulse responses to product market deregulation: sector-level results**

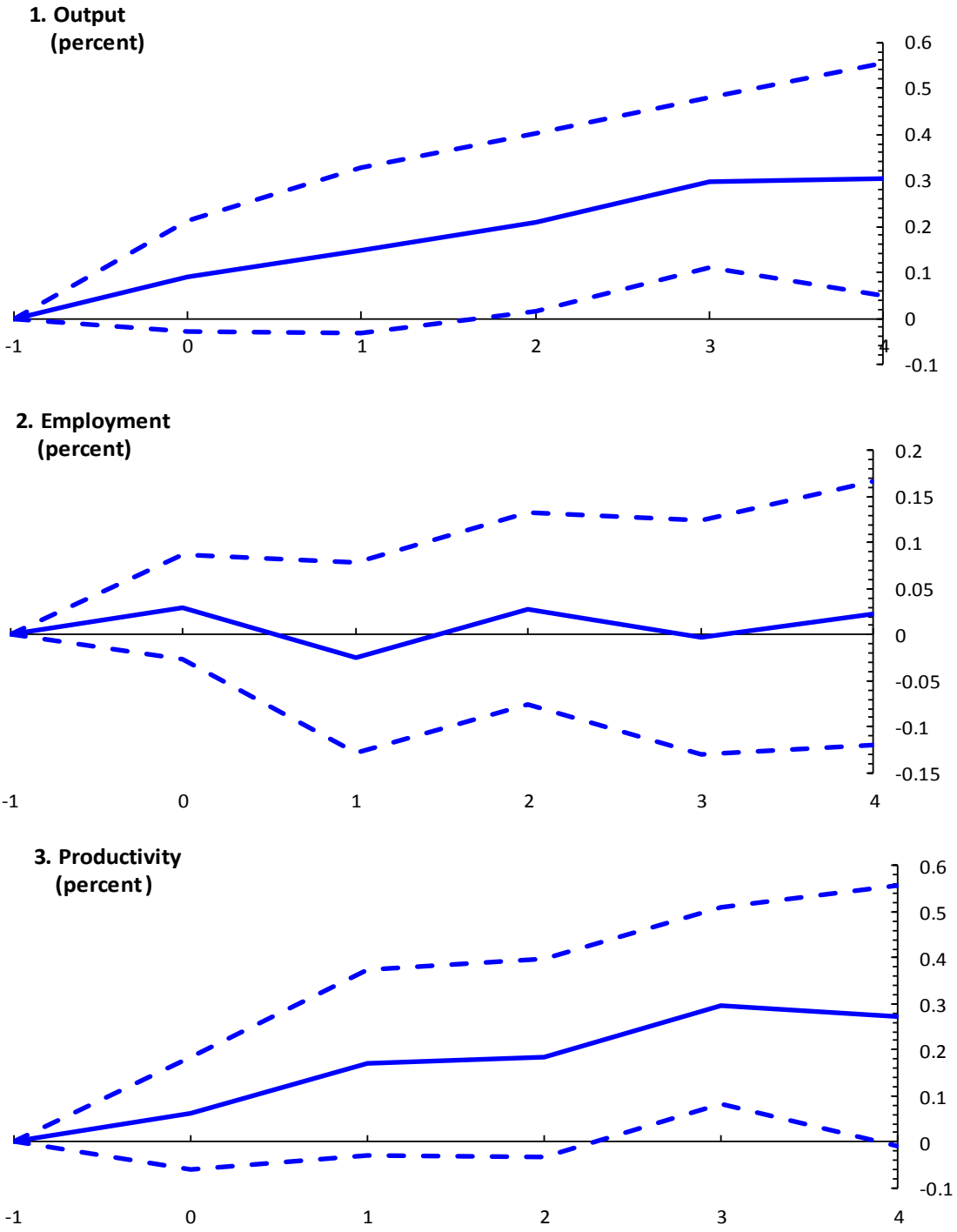
**Panel A. Backward linkages**




Note: t=0 is the year of the reform; dotted lines denote 90 percent confidence bands. Estimates based on equation (1).

**Figure 3. Impulse responses to product market deregulation: sector-level results**

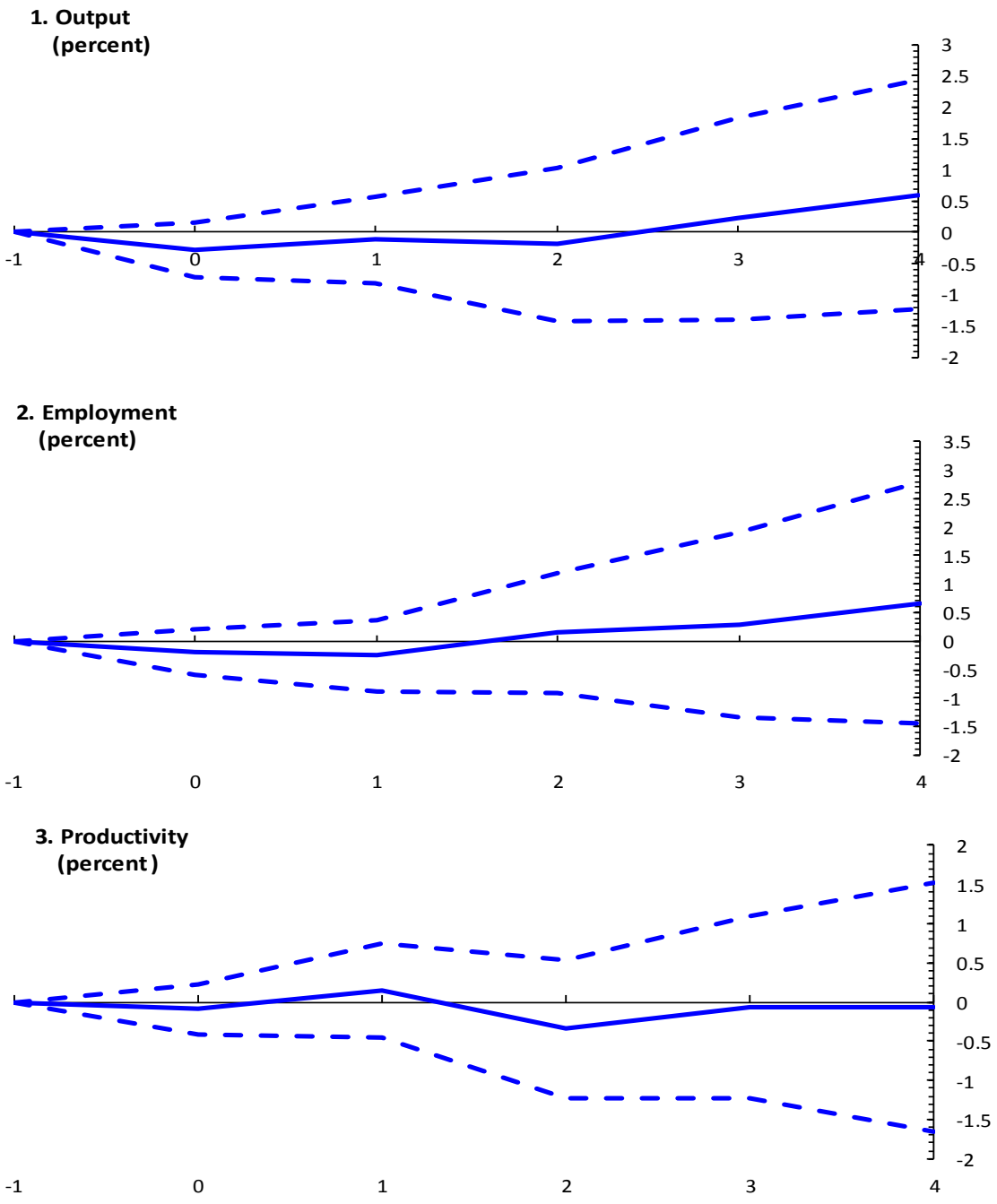
**Panel B. Forward linkages**




Note: t=0 is the year of the reform; dotted lines denote 90 percent confidence bands. Estimates based on equation (1).

**Figure 4. Impulse responses to regular employment protection legislation reform**

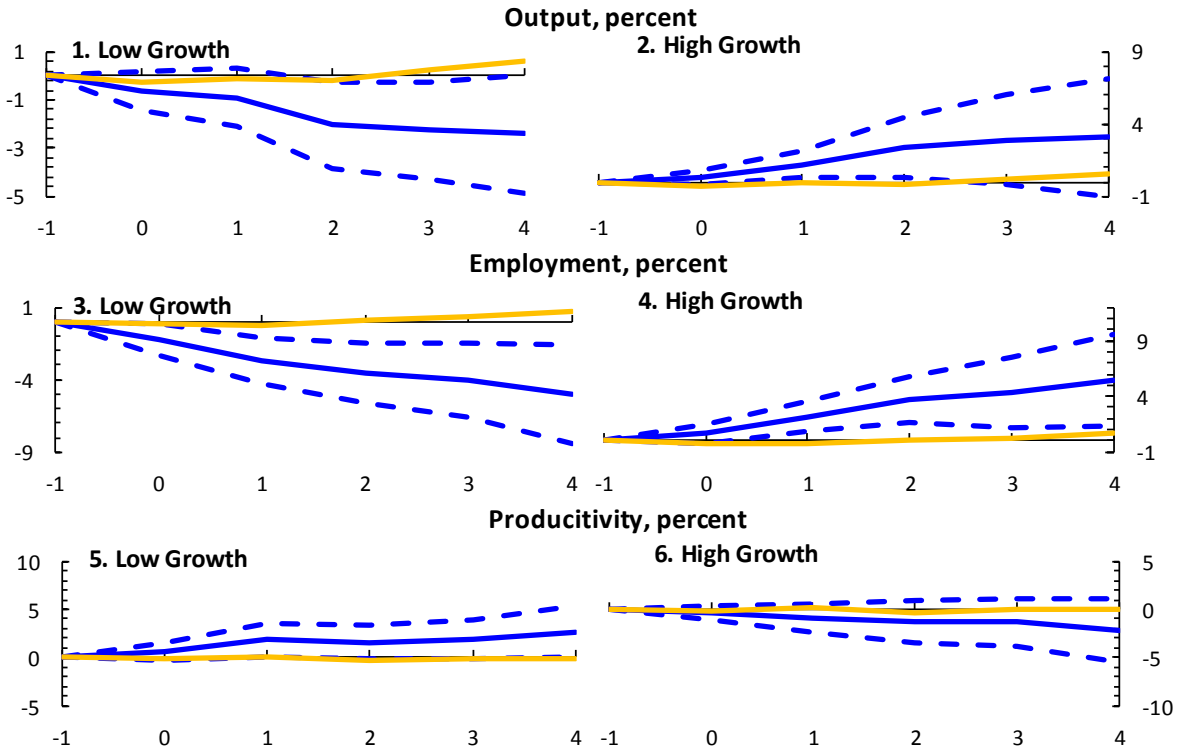
**Panel A. Baseline**




Note: t=0 is the year of the reform Solid blue lines denote the response to reform and dashed lines denote 90 percent confidence bands. Estimates based on equation (1).

**Figure 4. Impulse responses to regular employment protection legislation reform**

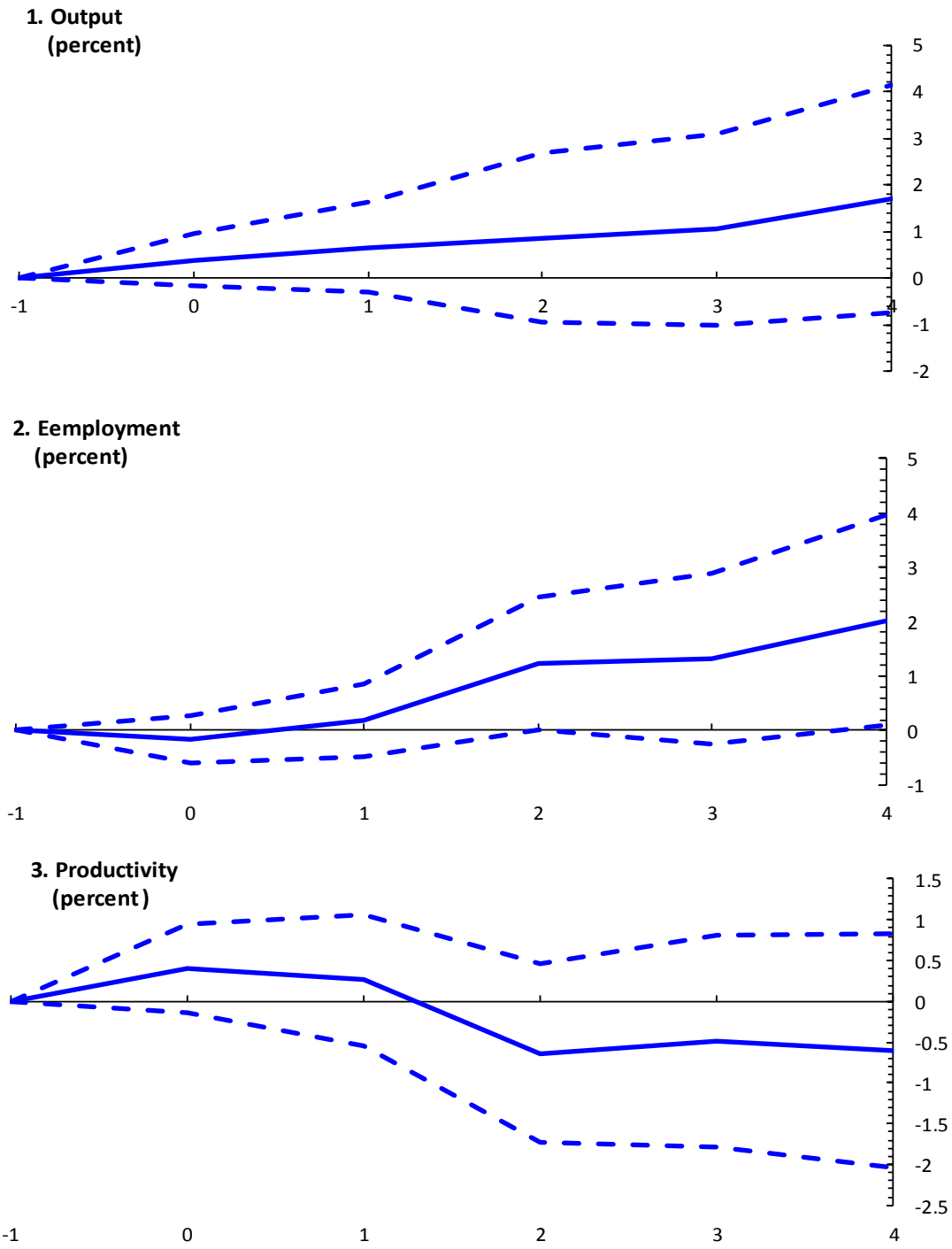
**Panel B. Low vs. High Growth**




Note: t=0 is the year of the reform Solid blue lines denote the response to reform and dashed lines denote 90 percent confidence bands. Solid yellow lines denote the unconditional (baseline) response presented in Panel A. Estimates based on equation (2).

**Figure 5. Impulse responses to unemployment benefit reform**

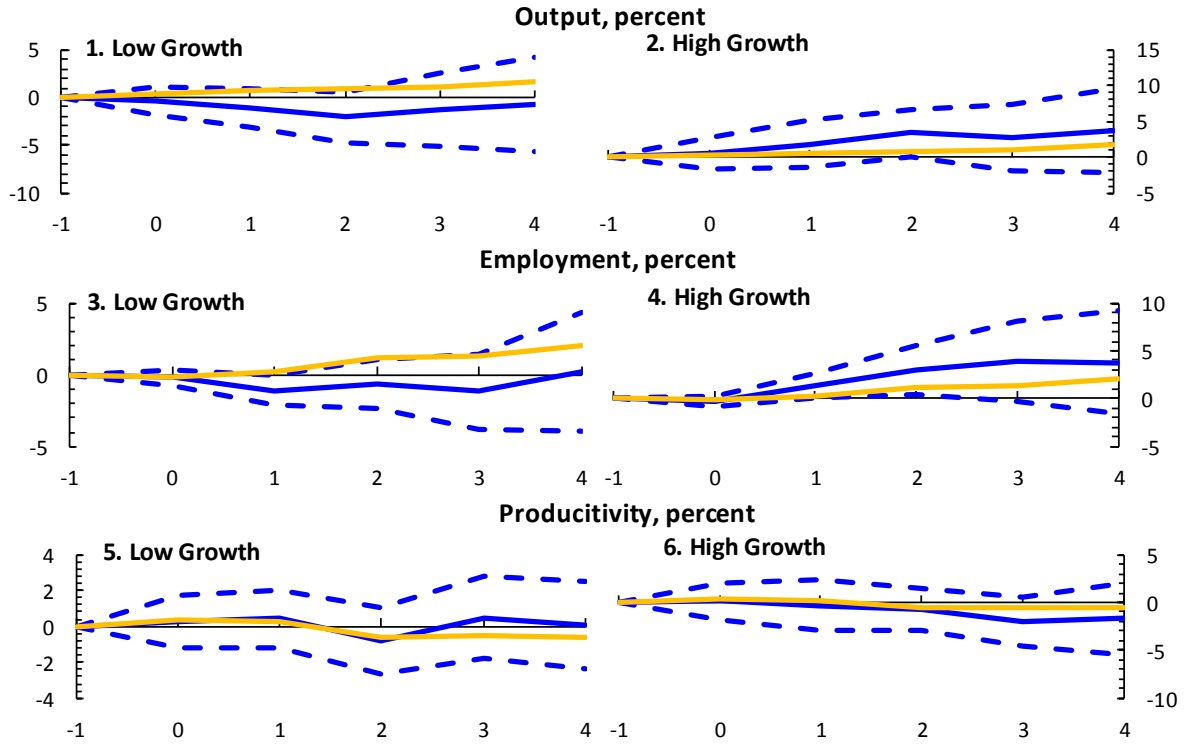
**Panel A. Baseline**




Note: t= is the year of the reform Solid blue lines denote the response to a reform and dashed lines denote 90 percent confidence bands. Estimates based on equation (1).

**Figure 5. Impulse responses to unemployment benefit reform**

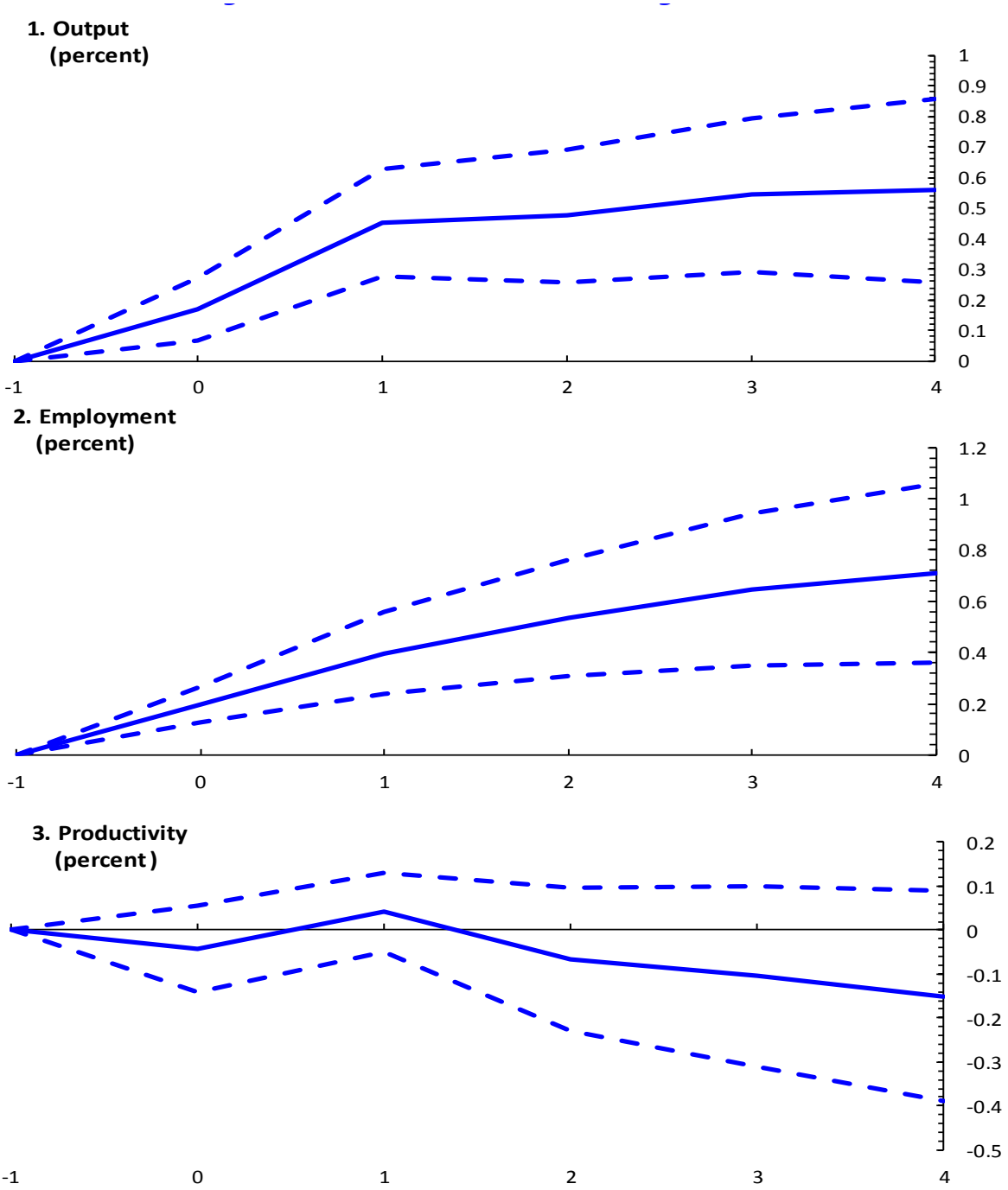
**Panel B. Low vs. High Growth**




Note: t=0 is the year of the reform Solid blue lines denote the response to a reform and dashed lines denote 90 percent confidence bands. Solid yellow lines denote the unconditional (baseline) response presented in Panel A. Estimates based on equation (2).

**Figure 6. Impulse responses to a one percentage point reduction in the labor tax wedge**

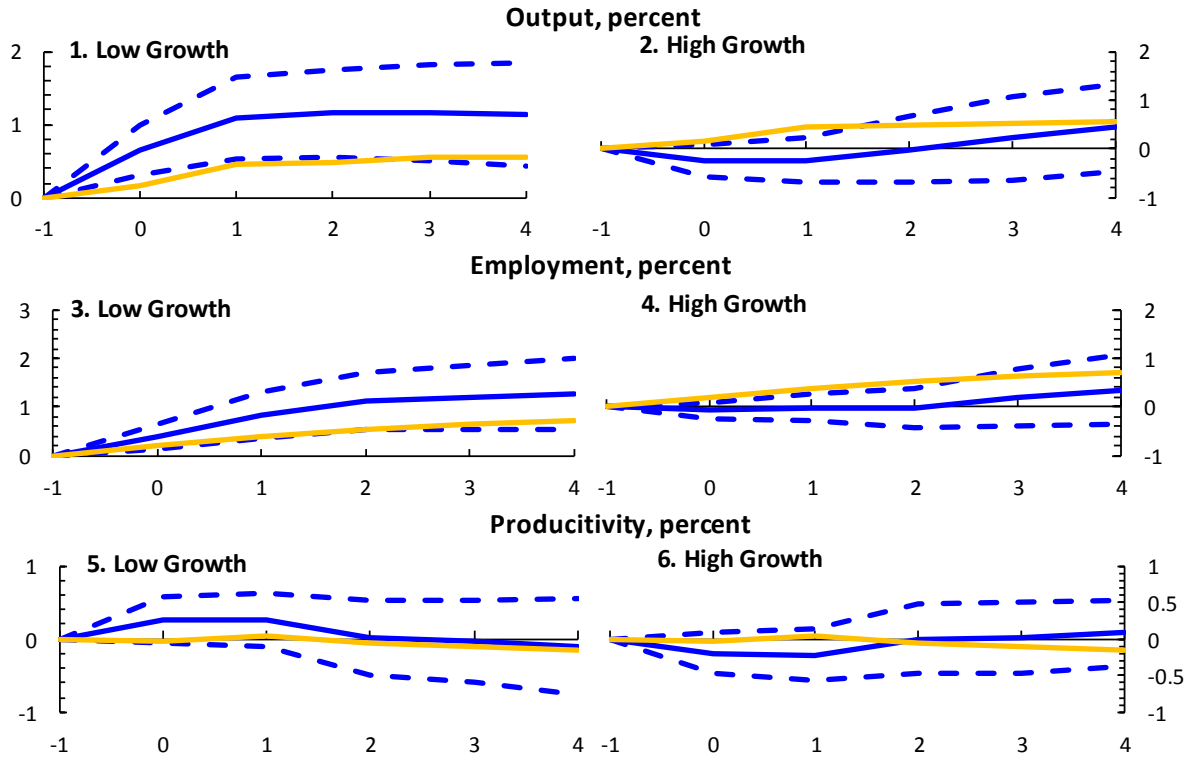
**Panel A. Baseline**




Note: t=0 is the year of the reform Solid blue lines denote the response to a 1 percentage point reduction in labor tax wedges and dashed lines denote 90 percent confidence bands. Estimates based on equation (1).

**Figure 6. Impulse responses to a one percentage point reduction in the labor tax wedge**

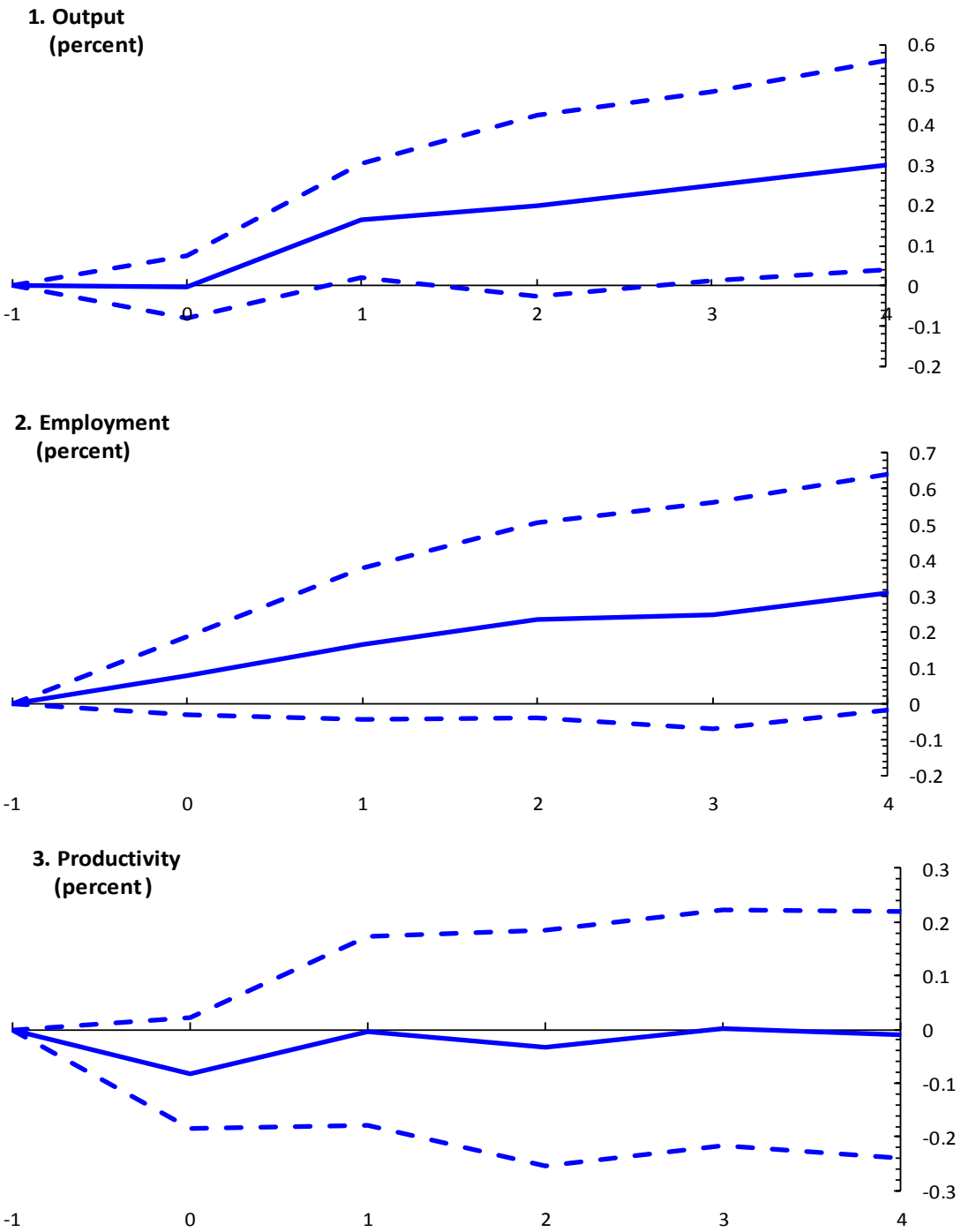
**Panel B. Low vs. High Growth**




Note: t=0 is the year of the reform Solid blue lines denote the response to a 1 percentage point reduction in labor tax wedges and dashed lines denote 90 percent confidence bands. Solid yellow lines denote the unconditional (baseline) response presented in Panel A. Estimates based on equation (2).

**Figure 7. Impulse responses to a 10 percent increase in ALMP spending**

**Panel A. Baseline**




Note: t=0 is the year of the reform Solid blue lines denote the response to a 1o percent increase in ALMP spending and dashed lines denote 90 percent confidence bands. Estimates based on equation (1).

**Figure 7. Impulse responses to a 10 percent increase in ALMP spending**

**Panel B. Low vs. High Growth**

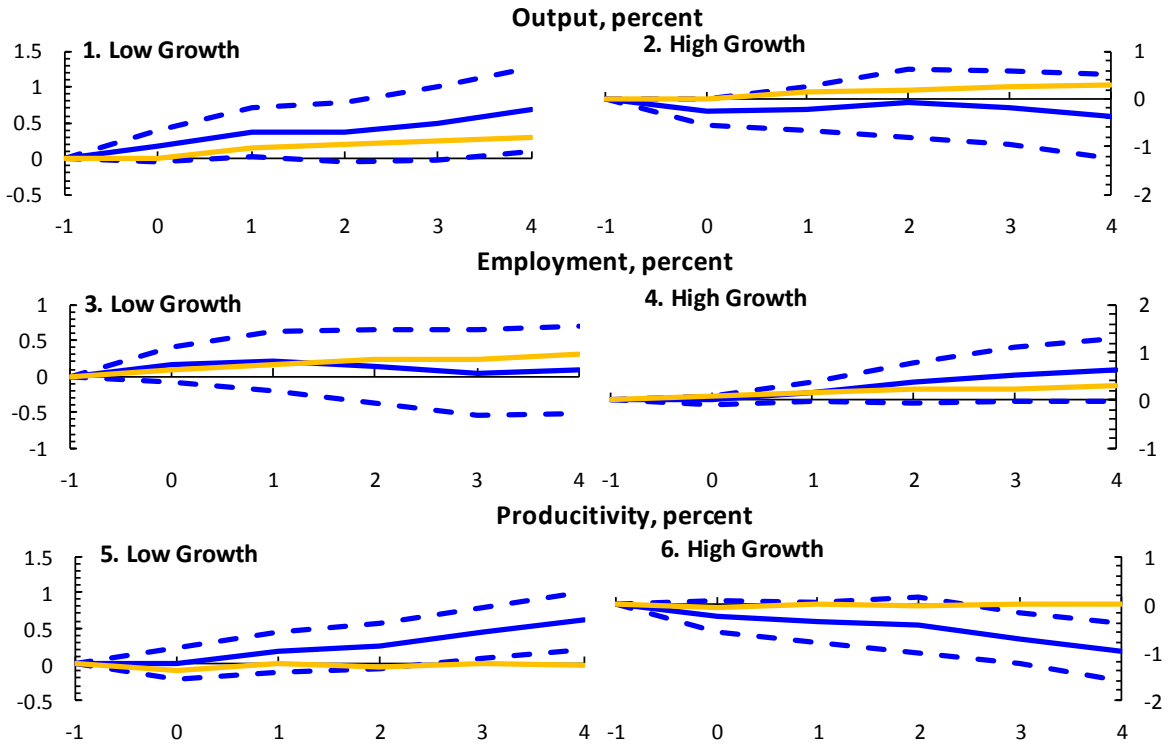
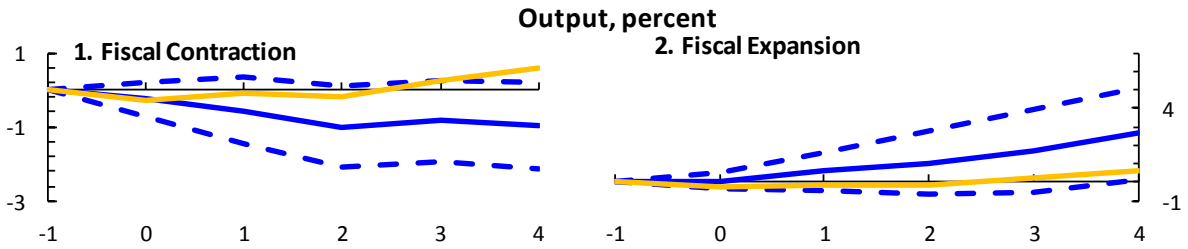
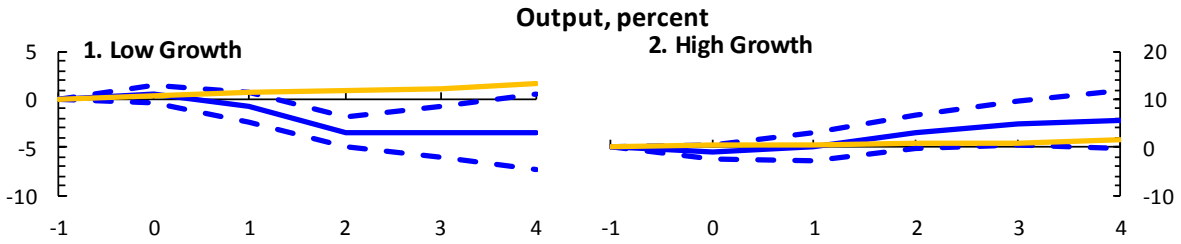


**Figure 8. Impulse responses to reforms under alternative fiscal policy stances**

**Panel A. Employment protection legislation reforms**



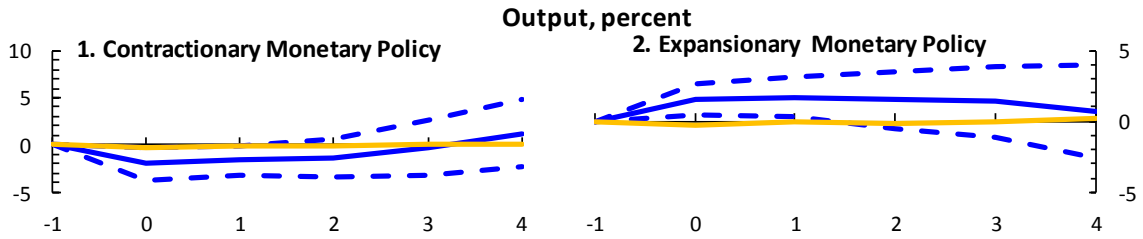

**Panel B. Unemployment benefit reforms**



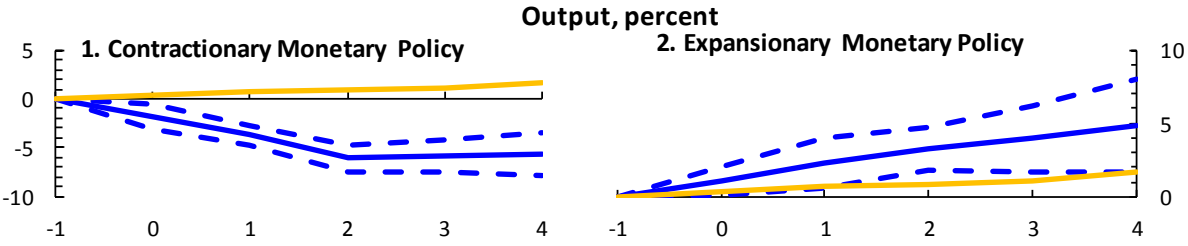

Note: t=0 is the year of the EPL (UB) reform. Solid blue lines denote the response to reform in periods of fiscal expansion (contraction) and dashed lines denote 90 percent confidence bands. Solid yellow lines denote the unconditional (baseline) response presented in Figure 3 (4). Estimates based on equation (2).

**Figure 9. Impulse responses to reforms under alternative monetary policy stances**

**Panel A. Employment protection legislation reforms**

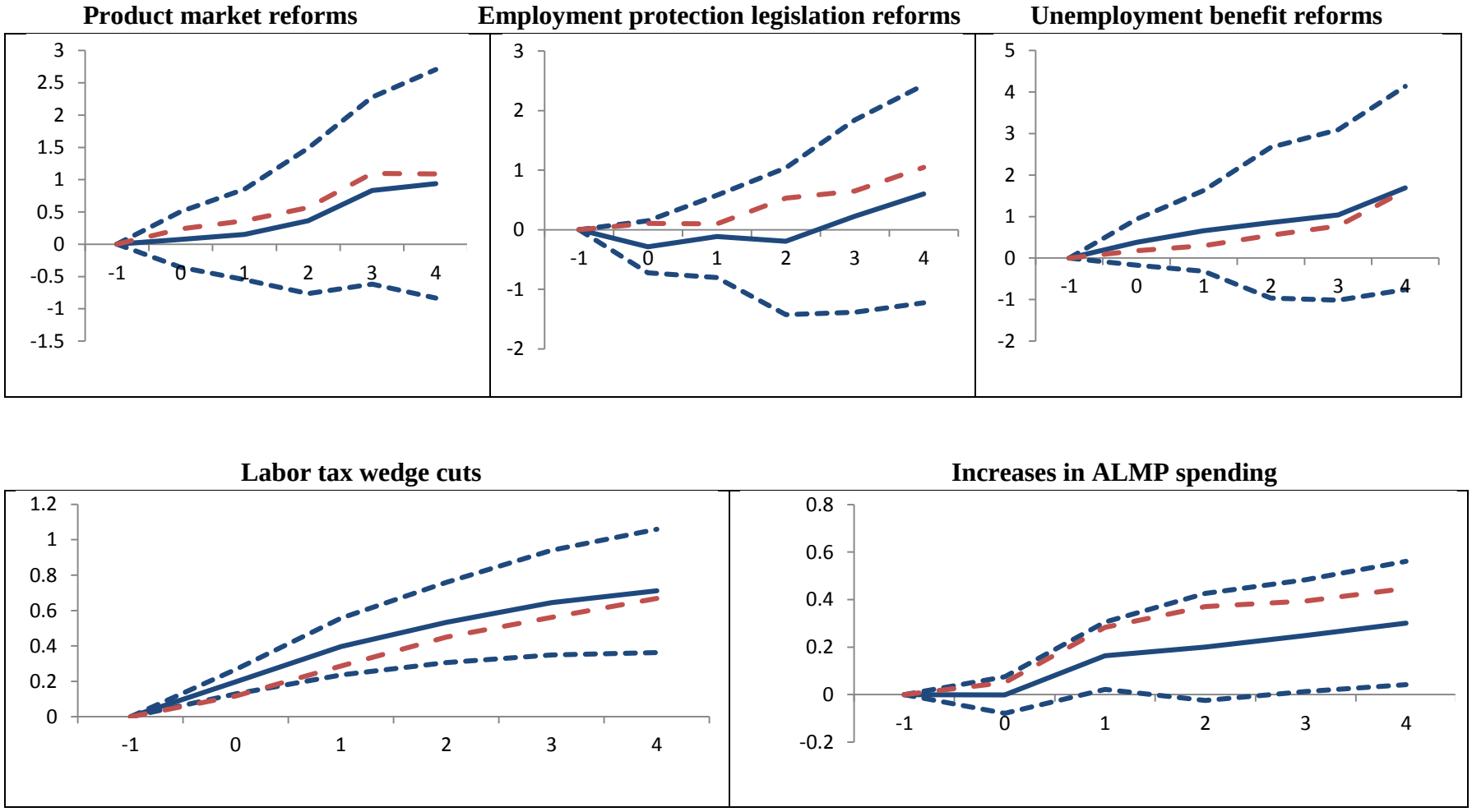



**Panel A. Unemployment benefit reforms**




Note: t=0 is the year of the EPL (UB) reform. Solid blue lines denote the response to reform in periods of fiscal expansion (contraction) and dashed lines denote 90 percent confidence bands. Solid yellow lines denote the unconditional (baseline) response presented in Figure 3 (4). Estimates based on equation (2).

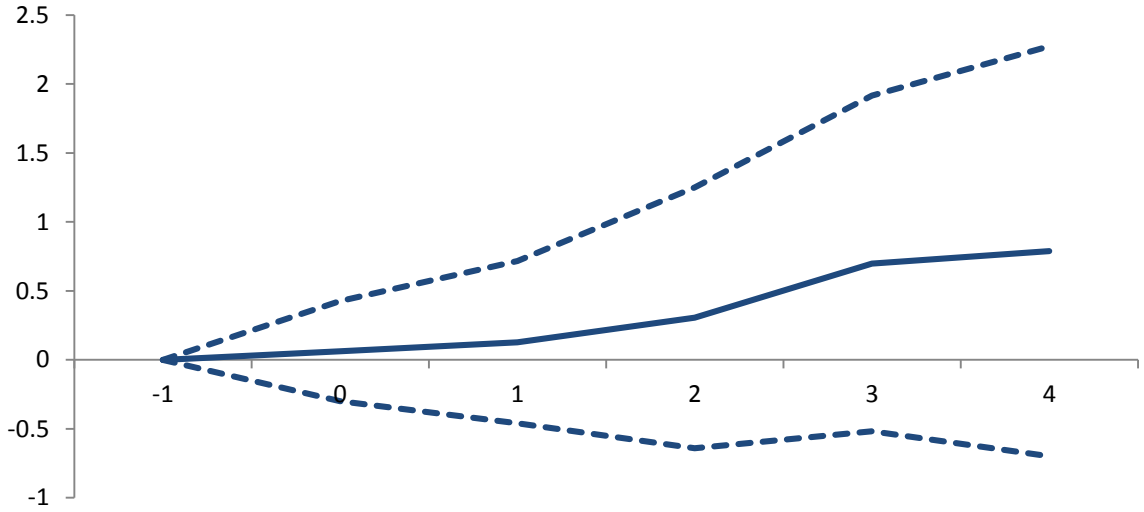
**Figure 10. Robustness check—Estimated effect on output when controlling for all types of reforms**




Note: t=0 is the year of the reform Solid blue lines denote the response to reform and dashed lines denote 90 percent confidence bands. Dashed red lines denote the response to reform when all reforms are considered simultaneously in the estimated equation. Estimates based on equation (1).

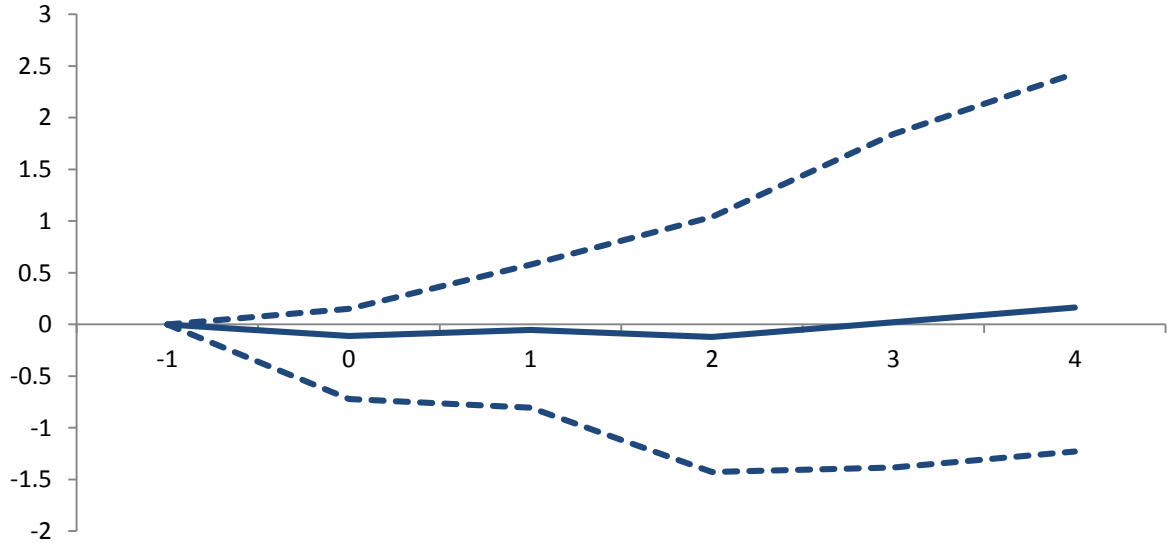
**Figure 11. Robustness check—Estimated effect of reforms on output when accounting for the intensity of reforms**

**Panel A. Product market reforms**




Note: t=0 is the year of the reform. Solid blue lines denote the output response to a reform with a magnitude equivalent to a change of 0.41 in the OECD indicator, and dashed lines denote 90 percent confidence bands. Estimates based on equation (1).

**Panel B. Employment protection legislation reforms**

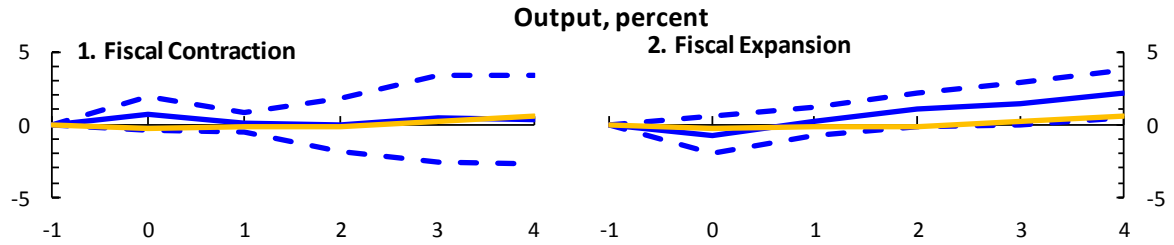



Note: t=0 is the year of the reform. Solid blue lines denote the output response to a reform with a magnitude equivalent to a change of 0.12 in the OECD indicator, and dashed lines denote 90 percent confidence bands. Estimates based on equation (1).

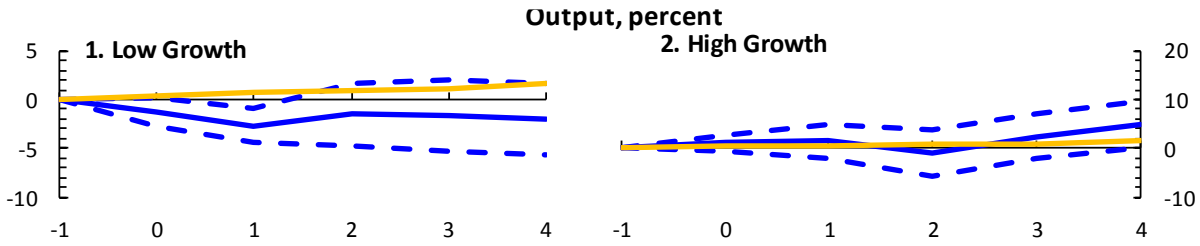
# Appendix

## Figure A1. Impulse responses to reforms under alternative fiscal policy stances—with fiscal stance measured using public investment shocks

### Panel A. Employment protection legislation reforms




### Panel A. Unemployment benefit reforms




Note: t=0 is the year of the EPL (UB) reform. Solid blue lines denote the response to reform in periods of fiscal expansion (contraction) and dashed lines denote 90 percent confidence bands. Solid yellow lines denote the unconditional (baseline) response presented in Figure 3 (4). Estimates based on equation (2), using public investment shocks to compute the fiscal policy regime variable.

**Table A1. The effect of fiscal and monetary shocks on output and state of the economy**

| | Output (%) | | $F(z_{it})$ | |
|---|---|---|---|---|
| Fiscal shock (t) | 0.352*** | | -0.043*** | |
| | (3.37) | | (-3.58) | |
| Monetary shock (t) | | 0.302** | | -0.029** |
| | | (2.08) | | (2.06) |
| Country FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| N | 434 | 350 | 434 | 350 |
| $R^2$ | 0.54 | 0.67 | 0.52 | 0.66 |

Note: fiscal (monetary) policy shocks are identified as the forecast error of the ratio of government consumption expenditure to GDP (of the policy rate, orthogonal to news to output and inflation). The table reports the response of output and the state of the economy—identified by the smooth transition function $F(z_{it})$—to an unexpected increase (decrease) in government spending (in the policy rate) of 1 percent of GDP (100 basis points). T-statistics based on clustered-robust standard errors in parenthesis. *** and ** denote significance at 1 and 5 percent, respectively.