WP/16/62

IMF Working Paper

When Do Structural Reforms Work? On the Role of the Business Cycle and Macroeconomic Policies

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INTERNATIONAL MONETARY FUND

WP/16/62

IMF Working Paper

European Department

When Do Structural Reforms Work? On the Role of the Business Cycle and Macroeconomic Policies[1]

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Authorized for distribution by Jörg Decressin

March 2016

Abstract

Structural reforms are expected to lift growth and employment, but their effects are surprisingly difficult to pin down empirically. One reason is their potential endogeneity to the economic environment in which they are conducted. For example, the impact of a reform implemented shortly before a cyclical upswing is difficult to distinguish from the recovery itself. Similarly, macroeconomic policies conducted along a structural reform could affect the estimated impact. Exploring various options, this paper develops robust estimates of the impact of labor and product market reforms by using local projection techniques while controlling for endogeneity of reforms and other biases. The results suggest that labor and product market reforms have a lagged but positive impact on employment creation, and the positive effect remains even after controlling for the endogeneity of the decision to reform. Supportive macroeconomic policies are found to increase the effect of labor and product market reforms, consistent with the view that some structural reforms are best initiated in conjunction with supportive fiscal or monetary policy.

JEL Classification Numbers: E24; E50; E62

Keywords: Structural reforms; employment; monetary policy; fiscal policy

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[1] We are indebted to Helge Berger for his valuable inputs and guidance. We thank Romain Duval, Davide Furceri, participants at the IMF EUR surveillance meetings and SPR-RES Jobs and Growth seminar and participants at the IMF-DNB October 2015 Conference on Structural Reforms in Amsterdam for their comments and suggestions.

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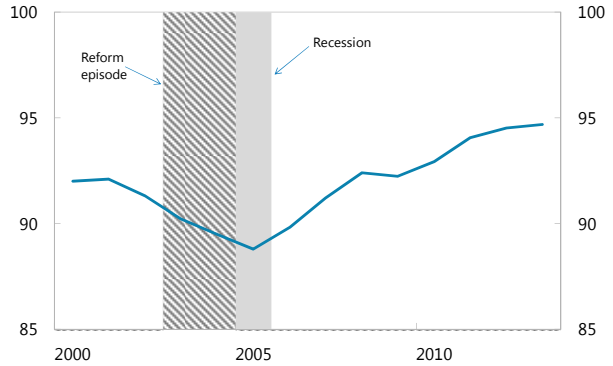
I. INTRODUCTION

The effects of structural reforms are surprisingly difficult to pin down empirically for a variety of reasons. First, the effects of structural reforms may be endogenous to the economic environment in which reforms are conducted. For example, the impact of a reform implemented shortly before a cyclical upswing is difficult to be distinguished from the recovery itself. Figure 1 illustrates this point: the chart plots employment (in percent of labor force) in Germany. Employment appears to have risen after the so-called Hartz reforms implemented during the downturn in the early 2000s (shaded area), but one will need to untangle the impact of the reform on employment from the general improvement in the economic condition after the recession (gray shaded area) in order to correctly capture the impact of the reforms. In this example, the possible endogeneity of the reform effort—that is, the fact that it was implemented when economic circumstances were about to turn—creates a possible upward bias in the estimates. There could also be a downward bias, however. If, for example, countries implement structural reforms early during a downturn, this could create a downward bias in the estimate. In such a case, structural reforms will tend to be observed when employment or growth is low, and a simple regression will underestimate the impact of reforms on employment or growth.

Figure 1. Germany: Employment Rate
(Percent)



Source: IMF WEO; OECD.

Second, the business cycle could affect the magnitude of the impact of structural reforms. For example, structural reforms may free up resources that cannot be absorbed in more efficient sectors because excess capacity exists and aggregate demand is low. A reform launched during recessions could also add further uncertainty, for example, because of required legal clarification. In these cases, some structural reforms may not achieve the desired improvements, having small or even negative effects in depressed economies.

Third, the state of the cycle itself may not only affect the likelihood of a reform or shape the reform's impact on macroeconomic outcomes, but also prompt a macroeconomic policy response. Monetary or fiscal policy conducted with structural reforms could affect the estimated impact of the reform. By reducing the uncertainty and sustaining aggregate demand during the reform episode, supportive macroeconomic policies can increase the positive effects of reforms on macroeconomic outcomes. It is therefore crucial to account for the policy stance when examining the macroeconomic effects of reforms.

The large literature on estimating the impact of structural reforms is only beginning to address these issues (see Section II). Existing studies have shown that the long-run effects of

structural reforms on growth and employment are positive. However, the evidence on the short-run effects of structural reforms is rather mixed and limited. While some studies find that the impact of structural reforms may depend on the business cycle, these effects vary across studies in terms of size as well as direction.

This paper contributes to this literature by presenting robust estimates of the impact of structural reforms by using local projection techniques while controlling for endogeneity and other biases. Local projections allow for estimating the dynamic effects of structural reform by computing the cumulative impact of a reform shock on the change of employment over a 5-year horizon. To address endogeneity, we adopt the augmented inverse probability weighting (AIPW) method which estimates the treatment effects of reforms while controlling for potential selection bias. The paper also examines the role of the business cycle and macroeconomic policies by explicitly controlling for these variables.

The empirical results suggest that structural reforms have a lagged but positive impact on employment. This estimated positive effect remains even after the endogeneity of the decision to reform is taken into account. Both labor and product market reforms increase employment rates by about a little over one percentage point over 5 years. Our analysis also suggests that supportive macroeconomic policy plays an important role in reaping the medium term benefit of labor and product market reforms by enhancing their impact on employment. This supports the view that some structural reforms are best initiated in conjunction with supportive fiscal or monetary policy if policy space is available.

The rest of the paper is organized as follows. The next section provides a brief overview of the exisiting literature. Section III discusses the data, incluing the definitions of various variables. Section IV presents the methodologies used in the empirical analysis. Section V reports the empirical results, and Section VI concludes.

II. LITERATURE REVIEW

Recent macro studies assessing the impact of structural reforms on the economy can be broadly classified into (i) those using simulations (typically based on DSGE models, such as the IMF's *Global Integrated Monetary and Fiscal model* (GIMF) or the EC's *QUarterly ESTimated macroeconomic model* (QUEST)); and (ii) those using empirical methods (typically based on cross-country data).[2] Many studies have found that the long-run effects of structural reforms on output are positive. However, the short-run effects are ambiguous, and some recent studies find that the impact of structural reforms may depend on the business cycle.

[2] There is also a large literature that looks at micro evidence from a particular country on the effectiveness of structural reforms, such as trade liberalization, opening to FDI, and entry of large retail chains on firm productivity or employment. This section focuses on the empirical macro literature.

Model based analysis has an advantage of being able to run various scenarios and quantify the impact of structural reforms under different circumstances. For example, Hobza and Mourre (2010) quantify the impact of various structural reforms envisaged in the Europe 2020 strategy using the macroeconomic model QUEST III. They find that structural reforms could boost real GDP growth from 1.7 percent to 2.2 percent between 2010 and 2020, depending on the depth of the reforms while employment gains range from 1 percent to 4.5 percent. Anderson and others (2014), using the GIMF model, find that structural reforms in the euro area can increase real GDP, but it will take time for the benefits to fully materialize.[3] Gomes and others (2013) use a multi-country general equilibrium model of the euro area to assess the macroeconomic effects of increasing competition in the labor and services markets in Germany and the rest of the euro area. The paper finds that such reforms would increase long-run output.

In empirical studies, the long-run effects on growth, employment, and productivity of product and labor market reforms are also often found to be positive. Bouis and Duval (2011) report on the impact of various structural reforms using estimated models from Bassanini and Duval (2006), Basannini and others (2009), and Bourles and others (2010b).[4] They find that a gradual alignment of product market regulations with best practice in a broad range of non-manufacturing sectors could boost aggregate labor productivity by several percent over 10 years in many OECD countries and by more than five percent across most of continental Europe. They also find that labor market reforms in the areas of unemployment benefit systems, activation policies, labor taxes and pension systems could raise employment rates by several percentage points in many OECD countries over a 10 year horizon if these reforms are phased in faster. They also report that the impact of structural reforms is larger in the long run (10 years) than in the medium run (5 years).

There are varied views on the short run impact of structural reforms, and empirical quantification is relatively limited. OECD (2012b) notes that benefits from structural reforms usually take time to fully materialize, but seldom involve significant losses and often deliver gains already in the short run.[5] However, some argue that short run effects of structural reforms could be negative especially when slack in the economy is large.[6] Structural reforms may free up resources that cannot be absorbed in more efficient sectors, thus not achieving the desired productivity improvement.

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[3] See also Anderson and others (2013) and Lusinyan and Muir (2013) who also use the GIMF model to assess the impact of the structural reforms.

[4] See also Barnes and others (2011) which adopt a somewhat similar approach but cover slightly different reform areas.

[5] See also Cacciatore and others (2012).

[6] See, for example, "Europe's Way Out" by Dani Rodrik available at https://www.project-syndicate.org/commentary/saving-the-long-run-in-the-eurozone-by-dani-rodrik.

In theory, unemployment benefits and activation policies are likely to boost employment rates relatively quickly and reduce employment because they increase the cost of being unemployed. Job protection reforms, however, can have ambiguous near-term effects as layoffs are likely to rise in the short-run if legal or regulatory constraints are relaxed. Product market reforms can also have ambiguous short-run effects as inefficient firms may be forced to exit the market due to more competition. On the other hand, new entrants may invest more and create new jobs (OECD (2012a)).

Some model-based studies show that short-run effects of some structural reforms can be indeed negative. Anderson et al. (2014) find that weak demand conditions could dampen the short-run impact of structural reform, and in some cases, structural reforms initiated in weaker initial demand conditions have very little positive and possibly negative impact on growth and employment even in the medium run.[7]

Empirical studies on the short-term effect of structural reforms are rather scarce. Bouis and others (2012a) find that structural reforms deliver short-run benefits. For example, an increase in spending on ALMP employment incentives will raise employment even in the short run. Unemployment benefit reforms (especially a reduction in unemployment benefit duration) also boost employment relatively quickly. However, they also find tentative evidence that unemployment benefit (a reduction in the initial unemployment benefit replacement rate) and job protection reforms pay off more in good times than in bad times, and can entail short-term losses in severely depressed economies. Bouis and others (2012b) report similar results for the impact of reducing unemployment benefit.

More recently, there has been an emerging view on the role of macroeconomic policies and structural reforms, particularly in the context of the debate on policy options to facilitate recovery from the European debt crisis. Eggertsson and others (2014), using a standard dynamic stochastic genearl equilibrium model, show that structural reform do not increase output during a crisis. Their simulation also show that structural reforms may have negative impact when monetary policy is constrained by the zero lower bound (ZLB). On the other hand, Decressin and others (forthcoming) find that structural reforms have a positive impact under quantitative easening.[8] Empirical studies, however, are rather limited in this area. Building upon the existing studies, particulalry Bouis and others (2012a and 2012b), the following sections estimate the impact of structural reforms on employment. It improves

[7] See also Cacciatore and others (forthcoming) who find that easing job protection implemented in a crisis time deepens and lengthens the recession using a dynamic general equilibrium model.

[8] Policy makers are also acknowledging the link between structural reforms and macro policies in the context of Europe. For example, Mario Draghi stated that QE will bring an "additional benefit" if "complemented by structural reforms" at the hearing of the European Parliament's Economic and Monetary Affairs Committee (https://www.ecb.europa.eu/press/key/date/2015/html/sp150323_1.en.html).

upon previous studies by attempting to correct for selection bias in the estimates and by examining the complementary role of demand policies.

III. Measuring Structural Reform Shocks

The rest of the paper estimates the dynamic impact of structural reforms on changes in the employment rate. This section explains how we derive the structural reform variable in the sample and discusses other variables used in the analysis.

A. Structural Reform Shocks

Structural reform shocks are identified based on standard OECD reform indicators. The OECD indices range from 0 to 6 to capture the restrictiveness of regulation in labor and product markets. The indices are computed as a weighted sum of scores assigned to several underlying criteria.[9] A higher value indicates more restrictive regulation and the introduction of a reform would be represented by a fall of the index. Following Bouis and others (2012a, 2012b), a reform shock in this study is identified as a drop in the OECD index, and the reform variable is defined as a dummy variable which takes a value of one when a reform shock is observed.

More specifically, the reform variable used here has the following characteristics:

- *Large*. A change in an OECD index is considered a reform shock if it exceeds two standard deviations of the change in the indicator over all observations. There are however far fewer labor market reforms than product market reforms in the sample. Given that fewer observations complicate econometric analysis, the floor for labor market reforms was reduced to one standard deviation. The focus on large episodes allows us to treat them as a shock and to estimate impulse responses using a dynamic specification. This implies that a series of small reforms over several years may not be identified as reform shocks in this study.

- *Discrete*. A reform shock is represented by a dummy variable. While this approach neglects the intensity of a reform, it allows for identifying the impact of reform shocks using treatment evaluation techniques where information on the predicted probability to reform can be taken into account to address the endogeneity issues mentioned earlier. We will discuss the degree to which reform intensity might matter for the reform impact later on.

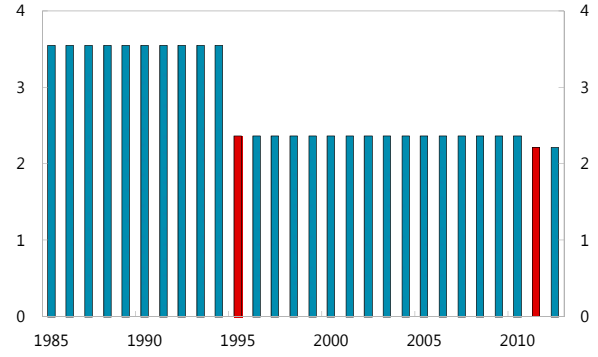
[9] See http://www.oecd.org/els/emp/EPL-Methodology.pdf and http://www.oecd.org/eco/reform/Schemata_PMR.xlsx on the derivation of labor and product market indices, respectively.

- *Unsequenced*. The paper does not address the issue of reform sequencing. This implies that it also does not capture reform reversals. Focusing on drops in the OECD indicators implies that our analysis will ignore episodes where the OECD indicator increases significantly (i.e. tightening of regulations), even after an initial decline in the indicator. Ignoring the presence of possible reform reversals down the road could be misleading and could create substantial biases, given that the approach traces the dynamic effects of reform over time. However, a careful examination of the sample suggests that there are very few cases of reform reversals in practice.

- *Non-sectoral*. The paper deals with aggregate macroeconomic indicators and outcomes, and does not examine sectoral effects of reform shocks which would require measuring reform shocks at the sectoral levels and measuring outcomes at that level of disaggregation.

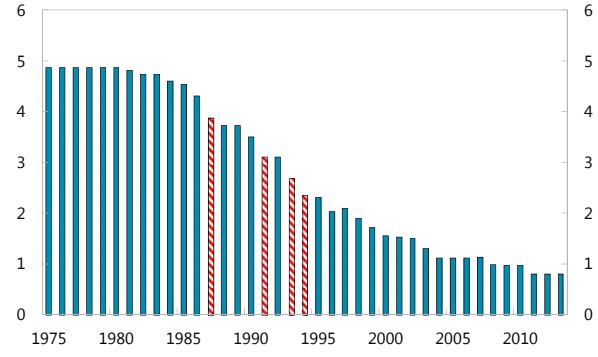
Figures 2 and 3 illustrate some of these characteristics. Figure 2 shows the OECD indicator on employment protection legislation on regular workers (EPLR) for Spain, and Figure 3 shows the OECD indicator on product market reform (PMR) for the UK. The red bars indicate the years when the drop in the indicators exceed our threshold of two standard deviations and therefore appear with a dummy variable value of 1 in the data. The Spain EPLR indicator captures labor market reforms applying to regular workers in 1995 and 2011. The changes in the indicator were large enough such that they were captured as reform episodes. The UK PMR indicator, on the other hand, shows the gradual implementation of privatization and liberalization in the UK economy. For almost every year from the early 80s to early 2000s, the indicator declined but in small steps. As a result, only 3 reform episodes are picked up.

Figure 2. Spain: OECD Indicator of EPLR
(Index)



Source: OECD.

Figure 3. United Kingdom: OECD Indicator of PMR
(Index)



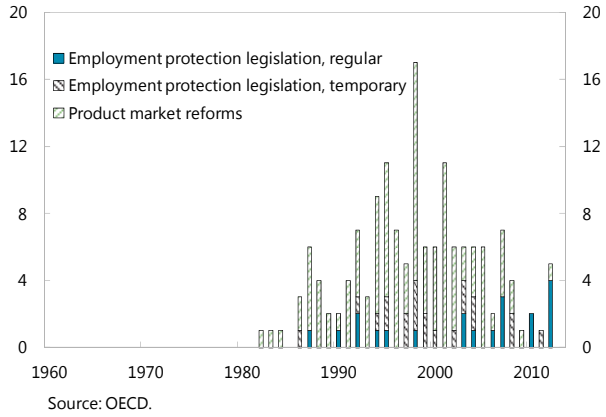
Source: OECD.

B. Caveats

The OECD indicators are among the few quantitative and comprehensive measures of reform effort but have a number of well-known limitations. These include the fact that the complexity of employment protection legislation can be difficult to summarize in an index.

Also, interactions across reforms may not be well captured in these indicators. For example, several labor market reforms in 80s and 90s that made it more difficult to dismiss regular workers but easier to hire temporary workers are captured as reforms in employment protection legislation on temporary workers. The product market indicator summarizes reforms in seven industries in the energy, communication, and transportation sectors. However, the OECD indicator does not capture any reform that occurs outside these industries.

Figure 4. Number of Reforms



Source: OECD.

With these caveats in mind, this paper analyzes the impact of individual reforms at the aggregate macro level. All in all, we have data for 36 countries from 1960 to 2013, picking up 28 reforms in employment protection legislation for regular workers (EPLR), and 102 reforms in PMRs. The rest of the paper will focus reforms of employment protection on regular workers (EPLR) and product market reforms (PMR).

IV. Methodological Issues and Results

Estimating the impact of reforms on any outcome is a difficult task. As discussed, several considerations should be taken into account. First, tracing the impact of a reform shock on a macro variable over time makes it sensitive to the internal dynamics of the business cycle itself. For instance, if labor market reforms tend to be implemented or launched during periods of substantial slack in the economy but close to a business cycle's turnaround, there is a risk of confounding the positive impact of the reform shocks on job creation with the recovery that follows a recession.

Second, even after cyclical conditions are controlled for, there could still be reasons why the effects of structural reforms could be biased. Reforms and macroeconomic outcomes could be jointly determined by third factors such as the economic and political cycle, the competencies of the leaders in charge of economic affairs, globalization or regional integration, etc. The risk of finding or not finding an impact of reforms on a given outcome will depend on the direction of the bias and its severity.

Third, there could be heterogenous effects in the sense that the marginal and dynamic effects of structural reforms are shaped by the macroeconomic environment and policy responses undertaken by decision makers, which are also strongly correlated.

The next subsection starts by discussing a baseline model. Then, several extensions are considered, including by reducing endogeneity issues and allowing for non-linearities in the effects of reforms.

A. The Local Projection Method

The key outcome variable of this paper is the change in the employment rate, following Bouis and others (2012a, 2012b). Labor market reforms are likely to impact employment more directly while the transmission mechanism of product market reforms on employment may be more complex. The treatment variable is the reform shock and we are interested in estimating the impact of a reform shock in the following years. The employment rate is a more direct consequence of employment protection legislation reforms. The data on employment and real GDP growth are taken from the IMF World Economic Outlook (WEO) database.

The core econometric approach relies on local projection (LP) estimates (Jorda, 2005). This follows recent work on estimating fiscal multipliers (Auerback and Gorodnichecko, 2013; Owyang, Ramey, and Zubairy, 2013; Jorda and Taylor, 2013) where fiscal consolidation is treated as a shock whose impact on growth over several years is estimated via local projections. A key advantage of the LP technique is its flexibility. LP accommodates possibly nonlinear or state-dependent impacts easily, which allows for investigating whether the effects of structural reforms can vary during booms or slumps, and in periods of supportive or non-supportive fiscal and monetary policies.

The LP technique is also flexible enough to robustly control for endogeneity issues, especially when the shock variable is not necessarily exogeneous. Later, we will be amending the LP framework to allow for an identification strategy which uses treatment effect methods as in Jorda and Taylor (2013) to reduce risks of endogeneity bias.

B. Baseline Specification

The first set of estimations aims at measuring the time-varying association between reform shocks and changes in employment rates while controlling for basic determinants, cyclical conditions, and time-invariant factors. More formally, the LP specification is as follows:

$$e_{i,t+h} = \theta_h R_{i,t} + \psi_h(L) e_{i,t+h-1} + X'_{i,t-1}\Gamma_h + u_i + \lambda_t + \epsilon_{i,t+h}$$

where $e_{i,t+h} = E_{i,t+h} - E_{i,t-1}$, and E_{it} is the employment rate in country i observed at year t. We estimate the model at each horizon $h = 0, 1, \ldots, 5$.[10]

[10] The employment rate here is defined as the ratio of total employment to the labor force.

R is the reform variable and X is a matrix of control variables. Control variables include the lagged change in the employment rate, the output gap, a banking crisis dummy variable, and country and year fixed effects to account for time-invariant country specific factors and common time effects across countries (e.g., the global business cycle), respectively.[11] The output gap is included to control for cyclical conditions while the output loss during financial crises aims at capturing possible structural breaks at the country level arising from large financial instability. Except for the output loss variable, which is taken from Laeven and Valencia (2013), all variables are from the IMF WEO database.[12] The coefficients of interest are θ_h which measure the impact of reforms on the cumulative change in the employment rate at each horizon starting in year $h = 0$ (the year of the reform) up to 5 years after the reform is identified. Driscoll and Kraay (1998) standard errors are computed to account for correlations in the error terms. The models are estimated using a sample of about 30 OECD countries (see Appendix I) observed over the period 1980–2013.

Table 1 reflects the cumulative impact of the reform on the employment rate on years 0 to 5. For labor market reforms, proxied by changes in the EPLR indicator, the impact is not significantly different from zero in years 0 to 3. It becomes significantly positive in year 4 to 5. By year 5, the reform increases the employment rate by 1.5 percentage points. This is the same order of magnitude that Bouis and others find. For product market reforms, the impact is positive and statistially significant in years 1 to 5, with the employment rate rising by about 1 percentage point by year 5.

Table 1. Effect of Reform on the Employment Rate (Baseline)

Dependent variable: Deviation in employment rate relative to pre-reform year

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
Labor market reforms	-0.185	-0.311	0.103	0.705	1.233**	1.468**
	(-1.15)	(-0.97)	(0.19)	(1.55)	(2.59)	(2.53)
Observations	555	555	555	526	497	468
Number of countries	29	29	29	29	29	24
Product market reforms	0.134	0.261*	0.444*	0.645***	0.781***	0.964***
	(1.37)	(1.70)	(1.93)	(2.75)	(3.14)	(3.60)
Observations	709	709	709	683	657	631
Number of countries	26	26	26	26	26	26

Note: t-statistics from Driscoll-Kraay standard errors in parentheses.
Additional controls: Lagged annual change in employment rates (3 lags), lagged output gap, output loss during crisis (Laeven and Valencia, 2013).
*** p<0.01, ** p<0.05, * p<0.1.

[11] There may be other factors that affect employment (e.g. social benefits and pensions, labor market income tax). Country fixed effects capture these institutional differences to the extent that these policies are time-invariant.

[12] WEO data is as of February 2014. Alternative specifications that we tried include the labor force participation rate and its various lags as additional control variables.

These results are consistent with other empirical work: in the near term, the impact of reforms on the change in employment is not significant. The positive impact is felt in the medium term.

C. State-Dependent Reform Multipliers: Initial Conditions Could Matter

Initial conditions could matter for structural reforms, and the effects of these reforms may be affected by the state of the business cycle. Assume for example that two countries with otherwise similar characteristics launch structural reforms in good and bad times, as measured by the size of the output gap, respectively. The interesting question is whether the resulting employment rate paths will be the same.

Analytical results on this question are not clear cut as discussed in Section II. In this part of the paper, we propose a formal test of the interaction between reforms and employment rate, conditional on business cycle conditions at the time of the inception of structural reforms.

To measure the effects, we amend the baseline LP model to include an interaction of the reform variable crossed with a measure of the business cycle at the time of the introduction of the reform. The cyclical variable is a dummy variable taking the value 1 in each year in which the output gap as percentage of potential output (extracted from the IMF WEO database and based on desk estimates) is lower than –1 percent ("bad times"), and 0 otherwise ("good times").[13]

To ensure that we correctly identifies the contribution of the state of the the business cycle to the marginal effect of structural reforms on employment, one needs to take into account the fact that the state of the business cycle is likely to prompt a policy response in the form of demand-supporting policies in the short-term. The risk is therefore to confound the effect of the interaction of reforms with the business cycle variable, with the effect of policy changes put in place to respond to the economic cycle. To gauge this possible effect, we define and control in the model for indicators of fiscal and monetary policy stances (P).[14] Following the work by Alesina and Perotti (1995), we define a fiscal consolidation event within countries as a period where the annual change in the cyclically-adjusted primary balance-to-potential GDP exceeds 1 percentage point.[15] Monetary stance (here a restrictive monetary stance) is

[13] The results are unchanged when we use more or less restrictive thresholds for the output gap (-1.5 percent or 0 percent). Results are available from the authors upon request.

[14] As for fiscal policy, this paper does not control for the composition of government policies (expenditure vs. revenue) beside the overall stance. Admittedly, tax increases or spending cuts may have differential impacts on the cycle.

[15] Data on cyclically-adjusted primary balance ratio are drawn from the IMF WEO database. We also explore the same question but focusing on the narrow measure of fiscal consolidation episodes identified using the IMF narrative approach and borrowed from Guajardo and others (2011). The results do not differ substantially.

defined as country-year observations corresponding to positive annual changes in countries' specific short term nominal interest rates. Data on short-term nominal interest rate are from the IMF WEO database.

The model takes the following form:

$$e_{i,t+h} = (\theta_{1h} + \theta_{2h} I_{i,t}) R_{i,t} + \sigma_h P_{i,t} + \phi_h I_{i,t} + \psi_h(L) e_{i,t+h-1} + X'_{i,t-1}\Gamma_h + u_i + \lambda_t + \epsilon_{i,t+h}$$

where I is a dummy variable capturing periods of economic slack and defined as:

$$I_{i,t} = \mathbf{1}[Output\ gap < -1]$$

and P is a dummy variable capturing fiscal or monetary policy stances:

$P_{i,t} = \mathbf{1}[\Delta\, CAPB \geq 1]$ or $P_{i,t} = \mathbf{1}[\Delta\, INT \geq 0]$.

The coefficients of interest are θ_{1h} and $(\theta_{1h} + \theta_{2h})$. They measure the association of reforms with cumulative changes in employment rates at each horizon in good and bad times, respectively. The model is estimated using the LP method with corrected standard errors.

Table 2. Effect of Reform on the Employment Rate, Accounting for the Economic Cycle

Dependent Variable: Deviation in employment rate relative to pre-reform year

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
Labor market reform, no slack	-0.0373	0.529**	1.069**	1.243**	1.344**	1.886***
	(-0.133)	(2.194)	(2.148)	(2.276)	(2.095)	(3.287)
Labor market reform, slack	-0.436**	-1.563***	-1.853***	-1.027**	-0.024	0.193
	(-2.23)	(-3.92)	(-3.19)	(-2.33)	(-0.03)	(0.24)
Observations	442	442	442	421	400	379
Number of countries	21	21	21	21	21	20
Product market reform, no slack	0.180*	0.245*	0.365**	0.696**	0.755**	0.953**
	(1.780)	(1.826)	(2.166)	(2.744)	(2.526)	(2.702)
Product market reform, slack	0.143	0.051	-0.143	-0.145	-0.034	-0.032
	(0.80)	(0.15)	(-0.27)	(-0.23)	(-0.07)	(-0.12)
Observations	430	430	404	379	354	329
Number of countries	26	26	25	25	25	25

Note: t-statistics from Driscoll-Kraay standard errors in parentheses.
Additional controls: Lagged annual change in employment rates (3 lags), lagged output gap, output loss in crisis (Laeven and Valencia, 2013), fiscal and monetary stance dummy variables.
*** p<0.01, ** p<0.05, * p<0.1.

Table 2 shows that the estimated impact of labor and product market reforms is affected by the cyclical position of the economy, but the effect differs depending on the type of reforms. More specifically, the effect of labor market reforms (here a significant decline in the employment protection of regular workers) launched during bad times is negative and statistically significant, and these effects are felt almost immediately after the reform is

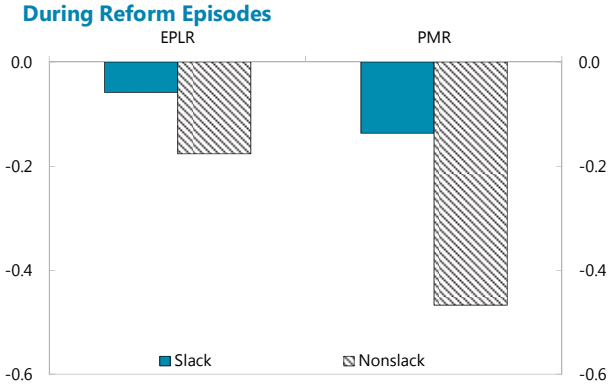
implemented. The negative and statistically significant coefficients found from year 0 to year 3 are in line with the view that a reduction in the protection of regular workers during periods of slack in the economy leads to more job destruction, and the effect is almost immediate and sizable. This result arises as removing excessive job protection makes it easier for firms to lay off labor in periods of significant slack in the economy. In contrast, our econometric results show that product market reforms launched during bad times do not necessarily lead to negative and significant employment losses—a somewhat surprising result as it is often thought that the additional supply capacity created by structural reforms will not be absorbed when aggregate demand is weak, leading to deflationary pressures. In fact, the traditional argument would suggest that the additional capacity created by the reforms will initially remain unmatched by demand and could further only worsen the unemployment rate—the opposite of what our results for product market reforms suggest. This is an interesting result as this suggests that different mechanisms may be at play when differnet types of structural reforms are implemented. The next subsection explores this possibility by examining the intensity of reforms.

D. The Intensity of Reforms

The seriousness of reform implementation, which is not controlled for in the model above, may differ depending on when the reform is implemneted. Reforms initiated during periods of slack may be more or less ambitious depending on a number of considerations including political economy ones.

On one hand, in periods of slack, with their backs against the wall, leaders and policymakers may be more determined to adopt bigger—and likely more difficult—reforms that also could have a stronger impact on growth and employment. This could then offset a possible negative impact of additional excess capacity freed up by structural reforms. On the other hand, leaders can also on the contrary pass weaker reform bills in bad times because of fear of uncertainty and political backclash following an unpopular reform in the public opinion. In that context, bold, ambitious and signifcant reforms could be less likely during downturns.

Figure 5. Average Change in the OECD Indicator During Reform Episodes



Sources: OECD; IMF staff calculations.

To investigate these aspects, we compare the size of reforms, as captured by the average decline in the OECD indicator, implemented during good and bad times as defined before using the output gap threshold. With labor market reforms, the average decline in the OECD indicator is larger when the reform is initiated during good times. The same is also found for product market reforms, suggesting that more serious product market reforms tend to be implemented

during non-slack periods. These results are also consistent with the econometric regressions of Table 2 which show that both labor and product market reforms have a positive medium-term effect on job creation in a relatively less demand-constrained environment. At the same time, these results suggest that various factors may play a role in determining the timing of reform implmentation. The next section proposes a more robust approach to assess the effect of structural reforms, taking into account the underlying economic and political forces behind the reform adoption.

V. A More Robust Approach to Measuring the Impact of Reforms

A. Baseline Specification

The results so far suggest that the occurrence of structural reforms may not be random and the non-exogeneity of the reform shock could potentially bias the results obtained from the LP technique. The LP approach in Section IV tried to control for the impact of business cycle movements and the associated macroeconomic policies and added several time-varying and time-invariant factors including country-fixed effects. However, this may not be enough. Countries that do (or do not) reform could share other characteristics beyond their cyclical position that also determine employment changes. For example, some might be more or less inclined or able to accompany structural reforms during crises periods with macroeconomic demand support measures. In addition, political factors might play a role and impact, among other things, the effectiveness of a given reform approach.

To more robustly assess the link between reforms and outcomes, the rest of the paper adopts treatment effect techniques, which have been used extensively in micro and medical studies. An alternative approach to address endogeneity is to use an instrumental variable (an exogeneous source of variation) for the reform variable. However, finding such an instrument is not easy and particularly difficult with macro data. Instead, the treatment effect approach allows us to implement doubly robust matching estimates (Imbens 2004; Lunceford and Davidian 2004; and Kreif et al. 2011; Jorda and Taylor, 2013) where the treatment group (in this case, countries engaged in reforms) is compared to the counterfactual group.

These methods proceed in several steps. First, policy propensity scores are derived from a latent model which, in our context, explains the probability of implementing a structural reform based on a number of possible factors, including cyclical, structural and political variables. Any predictor of policy should be included, regardless of whether that predictor is a fundamental variable in a macroeconomic model. These propensity scores are then used in the next step to correct for selection bias and to achieve a quasi-random distribution of

treatment and control observations via reweighting.[16] Second, a regression model—the LP model in our context—is used to fit or project the outcome variables (at each horizon in our case) in the treatment group and in the control group (countries which did not reform) on a number of determinants to obtain conditional means. Finally, differences in weighted conditional means (where weights are represented by the inverse propensity scores of each observation) at each horizon between the treatment and control groups are computed and give an approximation of average treatment effects (ATEs).

Specifically, we use the the (locally) semi-parametric efficient estimator (Lunceford and Davidian, 2004), the Augmented Inverse Propensity-Score Weighting (AIPW) which adds an adjustment factor to the ATE to stabilize the estimator when the propensity scores get close to zero or one. Jorda and Taylor (2013) use the methodology to estimate the fiscal multiplier, given that consolidation (the treatment) is determined by many factors that also impact growth (the outcome).

The first stage regression is presented in Appendix II. It employs a probit regression to estimate the probability of implementing structural reform. The second stage follows the baseline LP model discussed in Section IV B. The average treatment effect is computed as the difference of the estimated weighted mean change in employment rate between the reformers and non-reformers where the weight of each observation is the inverse of the propensity to reform as estimated in the first stage logit regressions.

As a benchmark, Table 3 first reports the baseline model estimated using the AIPW approach. Similar to the results of the baseline LP model reported in Table 1, the positive

Table 3. Effect of Reform on the Employment Rate (AIPW)

Dependent Variable: Deviation in employment rate relative to pre-reform year

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
Labor market reform	0.014	-0.162	-0.149	0.296	1.123**	1.276**
	(0.09)	(-0.77)	(-0.36)	(0.53)	(2.20)	(2.15)
Observations	555	555	555	526	497	468
Product market reform	-0.019	0.115	0.457*	0.750**	0.866***	1.188***
	(-0.20)	(0.66)	(1.85)	(2.42)	(2.66)	(3.27)
Observations	709	709	709	683	657	631

Note: t-statistics in parentheses.
Conditional mean controls: Lagged annual change in employment rates (3 lags), lagged output gap, output loss during crisis (Laeven and Valencia, 2013).
Propensity score based on the *probit* model as described in the text and includes: lagged GDP growth, legislative election dummy, forward EU accession dummy, age dependency ratio, and political leader's education background. *AIPW* estimates do not impose restrictions on the weights of the propensity score.
*** p<0.01, ** p<0.05, * p<0.1.

[16] Weighting by the inverse of the propensity score shifts weight away from the oversampled toward the undersampled region of the distribution. This shift of probability mass reconstructs the appropriate frequency weights of the underlying true distribution of outcomes under treatment and control.

impact of reforms emerges only in the medium term. However, the impact of PMR identified by the more robust AIPW approach is now esimated to be larger than the estimate in Table 1. Labor and product market reforms increase the employment rate by 1.3 and 1.2 percentage points, respectively, by the fifth year. These results confirm that structural reforms have a lagged but positive impact on employment, in line with earlier studies.

B. State-Dependent Reform Multipliers: The Role of the Business Cycle

Do structural reforms deliver different results if implemented in periods of slack or non-slack? We revisit this question by re-estimating the state-dependent reform parameters using the AIPW approach outlined in the previous section. In the AIPW approach, the first stage estimates the joint probability of implementing structural reforms and a specific business cycle position (e.g. slack versus non-slack). The second stage then estimates the effect of state-dependent structural reforms on changes to the employment rate. As the model allows for reducing the selection bias that possibly affect the estimates, we would interpret the results as robustness checks of the effect of reforms depending on the state of the business cycle. However, unlike in Table 2, the AIPW approach does not allow us to control for macroeconomic policy variables in this specific context: If we control for macroeconomic policies, the sample size reduces, and this makes it difficult to find convergence in the first-stage probit models. Thus the results below will not be directly comparable with the results in Table 2, but still give us some sense about the possible impact of selection bias. On the role of the macroeconomic policies, we will examine it further in the next subsection.

Tables 4 presents the results of the effects of labor market and product market reforms dummies interacted with the slack or non-slack dummy variables. The impact of labor and product market reforms on employment is positive and statistically significant when these reforms are launched in a growth-friendly environment, where there limited slack in the economy. However, when labor market reforms are launched in periods of slack, our results indicate a negative effect on employment The esimated coefficients are large, but these results need to be interpreted with caution because the number of episodes of reform shocks during slack (12 labor market reforms-slack occurences versus 16 labor market reforms-non slack occurrences) is relatively limited, and this is likely to affect the first stage probit regressions in the AIPW approach.[17] Despite these limitations, the results suggest that structural reforms (both labor and product market reforms) are most effective when implemented in period of limited slack. A natural question, however, is to what extent supportive macroeconomic policies in place can also enhance the effectiveness of the reform implementation and impact. The next section specifically addresses this question using the AIPW framework.

[17] For product market reforms, we have maximum of 34 product market-slack occurrences versus 63 product market-non slack occurrences.

Table 4. Effect of Reforms on the Employment Rate, Accounting for the Business Cycle (AIPW)

Dependent Variable: Deviation in employment rate relative to pre-reform year

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
Labor market reform, slack	-0.831	-2.196	-4.674*	-4.951	-7.957*	-11.94*
	(-0.640)	(-0.968)	(-1.838)	(-1.406)	(-1.939)	(-1.907)
Observations	557	557	557	528	499	470
Labor market reform, no slack	-0.00324	0.00188	0.584	1.637*	2.391**	2.778**
	(-0.0224)	(0.00635)	(0.848)	(1.686)	(2.112)	(1.961)
Observations	557	557	557	528	499	470
Product market reform, slack	-0.448	-0.374	0.208	0.594	0.730	1.330
	(-1.467)	(-0.742)	(0.312)	(0.736)	(0.876)	(1.535)
Observations	709	709	709	683	657	631
Product market reform, no slack	0.105	0.306	0.590	0.826*	0.758*	1.020**
	(0.794)	(1.096)	(1.426)	(1.678)	(1.660)	(2.014)
Observations	709	709	709	683	657	631

Note: t-statistics in parentheses.
Conditional mean controls: Lagged annual change in employment rates (3 lags), lagged output gap, output loss during crisis (Laeven and Valencia, 2013).
Propensity score based on the *probit* models as described in the text and include: lagged GDP growth, legislative election dummy, forward EU accession dummy, age dependency ratio, and political leader's education background. *AIPW* estimates do not impose restrictions on the weights of the propensity score.
*** p<0.01, ** p<0.05, * p<0.1.

C. State-Dependent Reform Multipliers: The Role of Macroeconomic Policies

Do structural reforms deliver better results if implemented in periods of supportive macroeconomic policies? This is an important question as several countries had to implement reforms in conjunction with fiscal consolidations. Not all reforms have been implemented in periods of neutral or expansionary fiscal or monetary policies. As noted earlier, understanding the role of macroeconomic policy for structural reforms may require a more robust approach than the simple LP technique as different factors seem to affect the occurrence of different reforms.

We employ the AIPW technique which focuses the empirical analysis on exogenous changes in the structural reform variable to revisit the role of macroeconomic policy for structural reforms. In this approach, the first stage estimates the joint probability of implementing structural reforms and a specific policy stance (e.g. restrictive/non-restrictive fiscal/monetary policy). The second stage then estimates the effect of structural reforms on employment.

Ideally, one would estimate the structural reform impact conditional on the state of the business cycle and, separetly, the stance of monetary and fiscal policy for both labor and product market reforms. However, since the number of structural reform shocks interaacted with the fiscal consolidation dummy is limited, a complete approach quickly runs into data constraints. What is feasible, however, is estimating a model asking whether the impact of

exogenous structural reforms varies with the fiscal and monetary stance, given the availability of sufficient non-zeroes observations to run first-stage probit models for the occurrence of a structural reform interacted with fiscal and monetary stances. For pruduct market reforms, we were able to esimate the impact of reform shocks under various fiscal and monetary policy stances. However, on labor market reforms, we were able to robustly estimate the effect of labor market reforms conditional only on monetary policy stances (we have enough positive observations to run the associated first-stage probit models in this particular case, but not for fiscal poliy stance).

Tables 5, 6, and 7 present the results of the interaction between product market reforms and the fiscal stance and between labor and product market reforms and the monetary stance, respectively. Note that Table 5 shows the results of two regressions: one where the first stage regression outcome variable is non-restrictive fiscal policy interacted with the probability of product market reform and the other where the outcome variable is restrictive fiscal policy interacted with the probability of product market reform. A similar structure is used in Tables 6 and 7 but with monetary policy.

The impact of product market reforms on employment when initiated with non-restrictive fiscal policy is positive and significant in the medium term (starting in the third year after the reform is launched). The employment rate rises starting in year 3 and by more than 1 percentage point in year 5 after the reform. On the other hand, when the fiscal policy stance is restrictive, the impact of product market reforms on the employment rate is negative and statistically significant 4 and 5 years after the reform is launched.

The results for the interaction of labor and product market reforms and supportive monetary policy are similar in nature (Tables 6 and 7). The employment rate rises significantly for reforms occuring along with non-restrictive monetary policy, after the reform. In the case of non-accomodative monetary policies, the positive effect of reforms disappears fully.

Table 5. Effect of Reform on the Employment Rate, Accounting for the Fiscal Policy Stance (AIPW)

Dependent Variable: Deviation in employment rate relative to pre-reform year

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
PMR, non-restrictive fiscal policy	-0.0962	-0.108	0.258	0.633**	0.668**	1.113***
	(-0.800)	(-0.483)	(0.919)	(1.981)	(1.995)	(2.764)
Observations	429	429	429	404	379	354
PMR, restrictive fiscal policy	0.325	-0.104	-0.524	-1.097	-1.667**	-2.271***
	(0.791)	(-0.206)	(-0.761)	(-1.300)	(-1.970)	(-2.818)
Observations	429	429	429	404	379	354

Note: t-statistics in parentheses.
Conditional mean controls: Lagged annual change in employment rates (3 lags), lagged output gap, output loss during crisis (Laeven and Valencia, 2013).
Propensity score based on the *probit* model as described in the text and includes: lagged GDP growth, legislative election dummy, forward EU accession dummy, age dependency ratio, and political leader's education background. *AIPW* estimates do not impose restrictions on the weights of the propensity score.
*** p<0.01, ** p<0.05, * p<0.1.

Table 6. Effect of Reform on the Employment Rate, Accounting for the Monetary Policy Stance (AIPW)

Dependent Variable: Deviation in employment rate relative to pre-reform year

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
LMR, non-restrictive monetary policy	-0.471**	-0.723***	-0.294	0.513*	1.540***	2.897***
	(-2.214)	(-3.735)	(-1.233)	(1.770)	(3.682)	(6.026)
Observations	551	551	551	522	493	464
LMR, restrictive monetary policy	0.163	0.204	-0.0234	-0.887**	-0.543	-0.666
	(0.672)	(0.475)	(-0.0299)	(-2.013)	(-1.089)	(-0.977)
Observations	551	551	551	522	493	464

Note: t-statistics in parentheses.
Conditional mean controls: Lagged annual change in employment rates (3 lags), lagged output gap, output loss during crisis (Laeven and Valencia, 2013).
Propensity score based on the *probit* model as described in the text and includes: lagged GDP growth, legislative election dummy, forward EU accession dummy, age dependency ratio, and political leader's education background. *AIPW* estimates do not impose restrictions on the weights of the propensity score.
*** p<0.01, ** p<0.05, * p<0.1.

Table 7. Effect of Reform on the Employment Rate, Accounting for the Monetary Policy Stance (AIPW)

Dependent Variable: Deviation in employment rate relative to pre-reform year

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
PMR, non-restrictive monetary policy	-0.017	0.199	0.563**	0.956***	0.999***	1.191***
	(-0.16)	(1.05)	(2.25)	(2.96)	(2.88)	(2.81)
Observations	689	689	689	663	637	611
PMR, restrictive monetary policy	-0.084	-0.049	-0.006	-0.132	0.012	0.390
	(-0.59)	(-0.26)	(-0.03)	(-0.53)	(0.04)	(1.06)
Observations	689	689	689	663	637	611

Note: t-statistics in parentheses.
Conditional mean controls: Lagged annual change in employment rates (3 lags), lagged output gap, output loss during crisis (Laeven and Valencia, 2013).
Propensity score based on the *probit* model as described in the text and includes: lagged GDP growth, legislative election dummy, forward EU accession dummy, age dependency ratio, and political leader's education background. *AIPW* estimates do not impose restrictions on the weights of the propensity score.
*** p<0.01, ** p<0.05, * p<0.1.

The results suggest that structural reforms are most effective when supported by non-restrictive macroeconomic policy. Similar to the robust baseline model in Section V.A, the reform impact gathers strength in the medium term. While the short-term impact is limited, the benefit of structural reforms materializes in the second or third year. This time pattern is consistent with the simulation studies: when monetary policy is restrictive, which is effectively the case in a binding zero lower bound (ZLB) environment absent effective non-conventional measures, simulation results predict that the impact of structural reforms on output will be small or even negative (see, e.g., Eggertson, and others, 2014). When monetary policy is nonrestrictive, however, for example in the presence of active asset

purchasing programs in a ZLB environment, theoretical results tend to predict that the impact of structural reforms on output will be positive (Decressin and others, 2014).

VI. Conclusion

This paper has investigated the impact of structural reforms on employment, using more robust methods to control for potential biases including endogeneity. The empirical results suggest that structural reforms have a lagged but positive impact on employment, which confirm existing results reported elsewhere. This positive effect, however, tends to be larger once the endogeneity of the decision to reform is taken into account. Using the local projection (LP) approach with treatment effect techniques, our estimates suggest that labor and both product market reforms tend to increase employment rates by about one percentage point over a five-year horizon.

The paper also examined the interplay between structural reforms, the macroeconomic environment, and fiscal and monetary policies. Data limitations can make a full discussion of these interactions difficult. However, we find suggestive evidence that the effects of labor market reforms tend to be negative if they are implemented in the period of economic distress. We also find that supportive macroeconomic policy enhance the positive impact of structural reforms in the medium term, suggesting that structural reforms are best initiated in conjunction with supportive fiscal or monetary policy if policy space is available.

While our findings contribute the current debate on the role of structural reforms and growth, they are far from the final word on this important topic. One important caveat is that a fuller investigation would require a richer data set of reforms—something likely to be more available at the sectoral level than at the macro level focused on here. The present paper also does not address the important questions which reform should be implemented in what order, how will structural reforms impact different sectors, and how will different structural reforms interact with each other.

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APPENDIX I. COUNTRY SAMPLE

The sample covers OECD countries with population of more than 5 million, with large episodes of labor market reforms for regular workers (EPLR) and product market reforms (PMR). Additional restrictions, such as non-missing output gap and employment rate variables with at least 3 lags, are also imposed, given the specification used in estimation. Finally, the first stage AIPW regression required some political variables, the unavailability of which constricts the sample further. All in all, the baseline EPLR regressions, reflected in Tables 1 and 3, include 555 observations from 29 countries (see table for the list of countries). The baseline PMR regression, reflected in Tables 1 and 3, include 709 observations from 26 countries. Regressions in Tables 2, 4-6, that include policy variables, restrict the sample further.

Australia	France	Korea	South Africa
Austria	Germany	Mexico	Spain
Belgium	Hungary	Netherlands	Sweden
Canada	Indonesia*	Poland	Switzerland
Chile	Israel	Portugal	Turkey
Czech Republic	Italy	Russia*	United Kingdom
Denmark	Japan	Slovak Republic	United States*
Finland			

* Dropped in the PMR regression sample owing to the absence of any large reform episode.

APPENDIX II. DETERMINANTS OF LABOR AND PRODUCT MARKET REFORMS

In the first stage, propensity scores to implement reforms are estimated using a probit regression of the probability of implementing either a labor or product market reform on lagged GDP growth (to account for cyclical conditions), a legislative election dummy (political variable), an EU accession dummy (economic integration factor), the age dependency ratio (demographic developments), and the political leader's educational background (either captures the competency of the leaders or their proximity with liberal views).[1]

Lagged GDP growth is included to capture the notion that countries in a recession may be more likely to implement reforms.

A legislative election dummy is 1 when there is a legislative election in that year. The variable is drawn from the database on political institutions (DPI). It helps capture previous findings that reforms are often times implemented early in the political cycle.

The EU accession dummy is 1 if the country is in the EU. External pressures from the EU or from other member countries can induce countries to implement reforms.

The reform implementation literature also includes the age dependency ratio—capturing the notion that the population aged 65 and older will tend to push for more pro-competitive labor market reforms to beef up pensions and social security contributions.

The political leader's educational background variable is drawn from Hallerberg and Wehner (2014) and takes the value 1 whenever the Prime Minister or the Minister of Finance has a degree in economics. It represents the idea that more technocratic leaders with a background in economics are more likely to initiate reforms. For instance, one of the main finding of Hallerberg and Wehner (2014) is that leaders that take office during a banking crisis in advanced economies are more likely to have an economics background.

The table below shows the regression results of the probit model. Results are based on the estimation of a pooled panel or random effect probit model where key determinants of the probability of observing labor and product market reforms are identified, respectively. The results for labor market reforms show that structural reforms are more likely to be implemented following periods of lower growth, off election cycles, and in countries with EU accession commitments. Product market reforms tend to occur in good times, in countries with a high dependency ratio and where key political leaders (Prime Minister or Minister of Finance) have an economic background.

[1] More recently, Duval and others (forthcoming) has examined the drivers of structural reforms in OECD countries where structural reforms are also defined as a large change in the OECD indicators. They also consider the variables used in our probit regression in addition to many other potential determinants.

Table A.1. Determinants of Structural Reforms

Pooled Probit Regression
Dependent variable: Probability of adopting a reform

	Labor market reform	Product market reform
Lagged GDP growth	-0.0493*	0.0653***
	(-1.660)	(4.053)
Legislative election dummy	-0.450*	-0.0226
	(-1.738)	(-0.189)
EU accession dummy, 1 year ahead	0.644**	0.00966
	(2.155)	(0.0780)
Aged 65 up	-0.0382	0.0396**
	(-0.947)	(2.539)
Technocratic leader dummy	-0.0309	0.254**
	(-0.168)	(2.366)
Observations	688	1105