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Reforms and re-elections in OECD countries

Paper presented at the 49th Panel Meeting of Economic Policy in Brussels

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Economic reform is sometimes seen as damaging for governments' re-election chances, although anecdotal evidence in OECD countries do not seem to strongly support this perception. This paper tests this hypothesis on a sample of 21 OECD countries over the period 1985-2003, controlling for other economic and political factors that affect re-elections. It is found that the chances of re-election for the incumbent government are, controlling for other factors, not significantly affected by its record of pro-market reforms. However, the electoral impact differs strongly depending on which type of reforms are considered. In particular, reform measures more likely to hurt large groups of "insiders" appear electorally more damaging while the reverse is the case for insider-friendly reforms. A series of framework conditions appears to interplay with reforms in affecting re-elections. Reformist governments tend to be voted out of office in countries with rigid product and labour markets, suggesting a "rigidity trap" in those countries. While fiscal stimulus accompanying reforms is neutral with regard to the odds of re-election, the presence of liberal financial markets appears to soften the electoral resistance to structural reform. The latter finding is of particular relevance in the current financial crisis: forward-looking governments should not rush to over-regulate financial markets in order not to compromise the feasibility of reforms in product and labour markets.

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1. INTRODUCTION

Most observers and policy makers agree that structural reform in product and labour market is necessary to raise the growth potential of market economies. In the European Union, this has motivated the pursuit of what has become known as the 'Lisbon Strategy for Growth and Jobs' – an EU wide effort to encourage structural reform across all members. Even so, the progress with structural reform in the EU economies is mixed, especially in the euro area (see e.g. Duval, 2005, Duval and Elmeskov, 2006, and European Commission, 2008).

More generally, the reform experience of different countries looks very diverse. For example, the 'English speaking' OECD countries have been reform frontrunners, while the southern EU countries have only recently started to reform their economies. Why is it the case? Is it mainly culturally determined or are there also economic factors at play? Are slow reformers less equipped to compensate their worst affected special interest groups or are these simply more powerful?

A possible way to examine these questions is to look at the re-election probabilities of reforming governments. There may be regularities in the re-election data that point to conditions that either favour or impede re-election of reforming incumbents, and this could indicate either strong or weak reform incentives. The existing literature on the political business cycle and the determinants of re-election suggests that policy activism under some conditions could favour the incumbent (e.g., Nordhaus, 1975, Rogoff and Sibert, 1988; Rogoff, 1990; Persson and Tabellini, 1990). The argument, originally formulated for macroeconomic policies, may also apply to structural reforms. A series of case studies reported in Munkhammar (2007) indeed find that reformist governments do tend to get re-elected.

A rather different message emerges from the literature dealing with the political economy of reform. Reform-inertia is a rather pervasive phenomenon, stemming from a lack of political capital to confront vocal interest groups opposing reform and from the fact that reform benefits mostly arise in the medium to long run, while there may be cost showing up in the short run (e.g., Drazen, 2000). The corollary is that reform would be punished rather than rewarded by the electorate. This hypothesis has gained popularity among policy makers and political leaders, as illustrated by the famous quote from Jean-Claude Juncker, Prime Minister of Luxembourg and President of the Eurogroup: "We all know what to do, but we don't know how to get re-elected once we have done it." ("The Quest for Prosperity", *The Economist*, March 15th 2007).

While anecdotal evidence and case studies point to a link between reforms and re-elections, this topic has so far received very limited attention in cross-country empirical analysis. The few empirical cross-country studies on the determinants of re-election that we know of (e.g., Powel and Whitten, 1993; Brender, 2003; Brender and Drazen, 2008) do not include structural reform among their explanatory variables.

We aim to fill this gap in this paper, estimating a probit model on a panel of countries akin to Brender and Drazen (2008) – but also including structural reforms as an explanatory variable. For this purpose we use re-election and reform indicators in labour and product markets for a sample of 21 OECD countries over the 1985-2003 period.

We focus on three basic questions. First, are reformist governments more likely to be re-elected? To address this question we construct a synthetic measure of the reform record of governments and test if there is a relationship with their probability of re-election. Second, we investigate whether the electoral impact differs depending on the type of reform considered. Third, we analyse whether there are concomitant factors that matter for the electoral impact of reforms. In this respect, we investigate whether the policy environment where reforms take place plays a role. For instance, the short-run costs of reforms may be lower if there is sufficient scope for income smoothing, either via financial markets or fiscal stabilisers. Moreover, we test if incumbent-specific factors matter, notably whether governments endowed with a strong mandate for political change are more likely to be re-elected.

Overall, our analysis suggests that whether an incumbent is reformist or not has no significant impact on the odds of its re-election. It appears, however, that the electoral impact differs strongly across the specific type of reform considered. In particular, reform measures that are more likely to hurt large groups of "insiders" – such as pension reform and reform of employment protection – appear electorally damaging. Moreover, the policy environment appears to interact significantly with reforms in affecting re-election results. Reformist governments tend to be voted out of office in countries with rigid product and labour markets, suggesting a "rigidity trap" in those countries. By contrast, the presence of liberal financial markets appears to weaken the electoral resistance to structural reform, suggesting that voters perceive income or job protection provided by the government to be substitutes for consumption or income smoothing opportunities offered by financial markets. Finally, we find evidence that newly-

appointed governments who are more likely to be endowed with a strong mandate for change, are more easily re-elected after having carried out reforms.

The remainder of the paper is structured as follows. In the next section we discuss the conceptual framework underpinning the analysis. In section 3 we describe the data and perform preliminary descriptive analysis. In Section 4 we outline our empirical strategy. Section 5 analyses whether reformist governments are more or less likely to be re-elected. Section 6 investigates the electoral impact of different type of reforms. Section 7 is devoted to the analysis of the factors that interact with the electoral impact of reforms. Section 8 concludes.

2. REFORMS AND ELECTORAL OUTCOMES: A CONCEPTUAL FRAMEWORK

To our knowledge there is no encompassing theory predicting a systematic impact of structural reform on the re-election of incumbent governments. However, it is possible to extract relevant arguments from the existing literature. We start by reviewing the main tenets of the political economy of reform inertia. Second, we discuss some key findings from existing theoretical and empirical analyses on the determinants of re-election. Finally, we identify a series of factors that are likely to affect the link between reforms and re-elections and that will be taken into account in the subsequent empirical analysis.

2.1. The political economy of reform inertia

The term reform is generally used for policy interventions which have a long-lasting impact on the functioning of the economy. Reform may aim to modify the institutional setting shaping the interplay among private economic agents. This is typically the case for regulatory reform of product or factor markets. In other instances, reform may be aimed at modifying the interplay between economic agents and the government (e.g., reforms in the tax code or in the rules defining access to the welfare state) or other policy institutions (e.g. the status of authorities enforcing competition policy or regulating public utilities). In our analysis we aim to include reforms in both categories.

The reform process is not always smooth and gradual. Indeed, it seems quite often characterised by jumps and discontinuities: substantial policy changes are clustered in time. The sequencing of reforms also shows regularities. Notably, in most advanced countries, reforms in the banking and financial sector was concentrated in the early eighties, while product and labour market reforms followed from the mid-eighties onward (IMF, 2004; Caselli and Gennaioli, 2008). There is also evidence that reforms in one sector of the economy go hand in hand with reforms in other sectors. For example, in several advanced countries, labour market, product market and tax reforms occurred broadly at the same time (IMF, 2004).[1] A common thesis for the discontinuity of reform

[1] The fact that reforms in different sectors of the economy tend to occur together could be explained by complementarity relations that often characterises reforms (e.g.., Blanchard and Giavazzi, 1993).

processes is that reforms tend to be triggered by crisis, i.e., periods of exceptionally bad economic performance.[2]

The recurrence of protracted periods where the status-quo persists is usually explained by the resistance reforms may encounter in the policy-making process (Drazen, 2000). Several political economy arguments have been offered in the literature to explain why, even when there is a widespread perception that reform would be in the general interest, action could be delayed or blocked altogether.[3]

A common explanation is the role of lobbying in the policy-making process, as first put forward in Olson (1971). According to this explanation, reforms, even when they can potentially benefit a majority of citizens, often produce losses to particular groups in society. These groups, even if a minority, could be highly motivated to organise resistance to reform and may face lower costs to structure themselves into organised pressure groups, thus prevailing in the political arena over large, but dispersed groups that may be in favour of reform.[4] The ability of these pressure groups to mobilise campaign contributions, offer a possible explanation why governments with electoral concerns may prefer not to carry out reforms. Quite often the groups opposing reforms are the "insiders", i.e., the economic actors that, being already "in the market", benefit from existing anti-competitive policies that shelter incumbents from potential competitors (e.g., incumbent firms vis-à-vis potential entrants, employed workers vis-à-vis the unemployed, etc.).

An alternative political economy explanation refers to uncertain reform payoffs at the individual level (Fernandez and Rodrik, 1991). When individuals are uncertain about whether they will benefit from a given reform, they could form a ex-ante majority in favour of blocking the reform, even if ex-post the reform may benefit the majority. Again, electorally-concerned governments may refrain from undertaking reforms if there is a perception that a majority of voters favours the status-quo.

Yet another strand in the literature focuses on the existence of a 'dual equilibrium', predicting that societies encumbered with a lack of 'social capital' and trust will induce individuals to behave uncivil and the government to impose excessive regulation, whereas societies where social capital abounds regulation will be leaner and productivity higher (Aghion *et al*., 2008).

An explanation for reform deadlocks that has received particular attention among both academic economists and policy makers is based on the uneven distribution of reform payoffs over time coupled with the short-sightedness of governments.[5] Some reforms, notably in labour and product markets, could lead to temporary job destruction and reduced wage or profit income in the reformed sectors. In the presence of short-run costs

[2] See, e.g., Drazen (2000) and Drazen and Easterly (2001) for a discussion of this thesis and for a survey on empirical evidence. .On the other hand, the political opposition to, for example, labour market reform may be relatively strong in crisis situations as it increases workers' attachment to job protection (Bean, 1998).

[3] However, as stressed by several academicians and policy-makers, stalemates in the implementation of reforms could simply be related to technical uncertainty. See, e.g., Sachs (1994) for anecdotal evidence on the frequent disagreement within governments on how to proceed with economic reforms in countries facing macroeconomic crises.

[4] See Grossman and Helpman (2002) for theory and empirical evidence on the idea that lobbies can explain the presence and persistence of protection.

[5] In this vein, Alesina and Drazen (1991) show theoretically how governments may be induced to delay reforms aimed at stabilizing public finances. See also Tabellni and Alesina (1990) for a model explaining the origin of a deficit bias by governments on the basis of short-sighted politicians.

of reforms and reform gains materialising only in the long run, politicians with an electoral concern may opt not to carry out welfare-enhancing reforms.[6]

2.2. The determinants of re-elections

There is consensus among both political scientists and economists that economic performance can potentially affect electoral outcomes. "Political business cycle models", where incumbent governments have an incentive to manipulate the state of the economy before elections in order to affect their re-election probabilities go back to Nordhaus (1975). Early political business cycle models failed to rationalise why voters should reward politicians for such manipulations without learning that after elections economic conditions would change for the worse. Recent variants of political business cycle models provide a rationale based on asymmetric information: voters, via a retrospective evaluation of the performance of the economy during the incumbents' administration, could infer their (unobservable) competence from their ability to manipulate macroeconomic variables (e.g., Rogoff and Sibert, 1988; Rogoff, 1990; Persson and Tabellini, 1990).

Although there is a vast literature investigating empirically whether "policy activism" increases when elections approach, there are only scant attempts to assess whether economic performance and the policy record of incumbents affect electoral outcomes.

There is evidence, mostly found in political science literature, to strongly support the view that macroeconomic performance does have an impact on the electoral performance of the incumbents.[7] Meanwhile, there is also evidence that voters have only limited memory, in the sense that economic outcomes close to elections have a stronger impact on voting behaviour than the economic performance at the beginning of the incumbents' mandate (Fair, 1978). Moreover, aggregate economic performance seems to matter more than individual economic conditions for voting behaviour (Lewis-Beck, 1988).

Results in the economics literature are more mixed. Alesina, Perotti and Tavares (1998) analyze the effects of growth, inflation, fiscal adjustments and other macroeconomic policy controls on cabinet changes in a cross section of OECD countries and find that, while fiscal variables and growth have no significant explanatory power, inflation increases the probability of a change in government. Peltzman (1992), Brender (2003), and Drazen and Eslava (2005) examine the direct effect of fiscal performance on re-election probabilities at sub-national level in the United States, Israel, and Colombia. In all these analyses loose fiscal policies appear to be detrimental rather than profitable

[6] Available evidence shows that the timing of economic reforms on growth depends quite crucially on the specific type of reform considered. Simulations based on a small scale econometric model contained in IMF (2004) show that while product and labour market reforms take time to produce positive effects on output, financial market and tax reforms may produce positive effects on output already in the short term.

[7] Early analyses focus on the determinants of incumbents' vote counts in US elections. Kramer (1971) finds that real per capita GDP growth and inflation in the election year significantly affect votes received by the incumbent party in congressional elections. Fair (1978, 1982) and Alesina and Rosenthal (1988) find evidence of significant role for the growth performance looking at long time series of presidential elections. Analogous results have been found for large European countries in Lewis-Beck (1988) and for Scandinavian countries in Madsen (1980). A positive but weak impact of growth on electoral outcomes is also found in Powell and Whitten (1993), who analyse a cross-section of developed countries.

in electoral terms. Brender and Drazen (2008) assess the impact of growth and the fiscal stance on the probability of re-elections in a large panel of advanced and developing economies. Their results indicate that higher growth increases the probability of re-election significantly over the whole sample, but for developing economies only. As for the impact of fiscal expansions, these appear to affect negatively the odds of re-election, and in most cases significantly so.

2.3. Economic reforms and re-elections: in search of possible links

Modern variants of the political business cycle models state that policy activism, by signalling competence, could be electorally beneficial. This argument, formulated originally for macroeconomic policies, can be extended to the case of structural reforms. To implement this type of policies successfully, policy makers' competence and skills play a key role. Additionally, in comparison with macro policies, reforms are likely to affect permanently and positively economic performance and require policy changes to be sustained over several years, so that reforms started by an incumbent government during its first mandate often raise expectations of further reforms after re-election. There are, however, other relevant arguments going in the opposite sense. Reforms may imply relevant short-term costs and benefits coming with a lag: reforms in such a case would not work as a signalling device (Rogoff and Sibert, 1988) and may turn out being electorally costly. Moreover, political economy explanations of reform inertia suggest that potentially beneficial reforms may be blocked because detrimental to strong interest groups.

Hence, whether reforms raise or lower re-election probabilities depends on the balance between expected costs and benefits by the electorate and the size, resources, and mobilisation capacity of relevant interest groups. The assessment of electoral costs and benefits of reforms is often hard to be made ex-ante by governments, which explains why sometimes reforms with largely negative electoral pay-offs could be carried out.

Additionally, the electoral impact of reforms may depend on factors that go beyond the narrow computation of economic costs and benefits associated with the reform. In particular, the electoral feasibility of reforms may depend on the *incumbent's mandate for change*, which may consist of intangibles such as "charisma", but it may also include tangible factors such as the incumbent's mandate for change when a programme of substantial economic reforms is accompanied by long-overdue and widely-endorsed changes in political priorities on other fronts.

In the subsequent empirical analysis, our aim is not only to assess whether reforms are on average beneficial or detrimental in terms of re-election probabilities, but also whether the type of reform considered matter and which other factors play a relevant role in shaping the link between reforms and re-elections.

Based on the above arguments, we expect that the policy environment in which the incumbent operates will shape the electoral impact of reforms in several respects:

- *The initial structural conditions in labour and product markets.* A-priori it is not clear whether more rigid labour and product markets entail a more positive or

more negative effect of reforms on re-election probabilities. On the one hand, the net perceived benefits of reforms may be higher if markets are more rigid as there are still 'easy' reforms to be picked. On the other hand, rigid markets lower the resilience to economic shocks, including temporary income or job loss arising from "reform shocks" and therefore may be more costly. Moreover, reform-adverse interest groups are likely to be stronger the more rigid are markets as anti-competitive distortions create rents for the insiders: the bigger the potential for these rents, the larger the pool of insiders opposing reforms and their stake in the political game. Saint Paul (1993, 1996) demonstrates that labour market reforms can be less politically feasible when large distortions in labour markets lead to high unemployment. Moreover, barriers in one sector of the economy may create resistance to reforms in other sectors. Blanchard and Giavazzi (2003) show that product market regulations sustaining high firms' market power also hamper labour market reforms by raising union rents associated with the status-quo.

- *Financial market regulations*. Liberal financial markets may help contain the net cost of structural reforms via various channels (Buti, Turrini, Van den Noord, and Biroli, 2008): they provide means for borrowing against future income prospects; permit to "bring forward" reform-related gains materialising in the medium-to-long term; and provide insurance against idiosyncratic risk. Additionally, financial market reforms, by improving the efficiency of stock markets where the ownership of existing firms is traded, can help to reduce incumbents' resistance to product market reforms (Caselli and Gennaioli, 2008).
- *Fiscal policy*. Large fiscal automatic stabilisers permit income smoothing via the operation of progressive taxation and automatic transfers, thus reducing the size of short-term reform-related losses. Moreover, an expansionary fiscal policy may be a way to compensate possible short-term losers of structural reforms, thereby limiting the possible negative electoral impact of reforms. A widely held view is that structural reforms are hardly feasible if carried out together with fiscal consolidation and due to the limited political capital of governments, although complementarity cannot be excluded (see, e.g., Buti, Röger, and Turrini, 2008).

Moreover, there are incumbent-specific elements that need to be taken into account among the factors that shape the link between reforms and re-elections. In particular, the mandate to reform and political capital can be comparatively strong for a 'fresh' incumbent who has been voted in office on the basis of a different ideological orientation or a re-definition of political priorities compared with its predecessors.

Whether positive or negative electoral effects of reforms prevail will depend on how the above factors combine. The net impact will depend on the particular combination of policies carried out by the incumbent, since factors weighing positively and negatively will in general differ across specific structural policies. The impact will also depend on country-specific and incumbent-specific characteristics. In the empirical analysis, we will analyse separately the impact of different type of reforms. We will also analyse how the above mentioned factors relating to the policy environment and incumbent-specific factors interact with the different types of reforms in determining electoral outcomes.

3. DATA AND DESCRIPTIVE EVIDENCE

3.1. Variables and sample choice

3.1.1. Elections

The sample includes 21 OECD countries (US, Japan, Canada, Australia, Switzerland, New Zealand, Norway and the EU-15 countries excluding Luxemburg) over the 1985-2003 period. For this sample it was possible to obtain information on both electoral outcomes and reform activity.

Our (re-)elections data are based on the World Bank Database on Political Institutions (Beck et al., 2001).[8] Both legislative and executive elections are considered. Legislative elections concern the election of the legislature (parliament or congress), while executive elections concern the direct election of the head of state, who is also the head of the executive in presidential systems (the US) or formally appoints the chief executive (Finland, France, Portugal). The chief executive in the Beck *et al.* (2001) database is the person formally (*de jure*) in charge of the executive branch. In our sample this is the prime minister in all cases except the US.

In line with Brender and Drazen (2008), the re-election variable is designed to indicate whether, following an election, the incumbent chief executive is still in power. Hence if the chief executive is not reconfirmed but his successor is a member of the same party, this would not qualify as a re-election. The re-election variable defined in this way permits to capture also cases in which the electoral cost of reform is not paid by the main party in power, but is high enough to prevent the re-appointment of the government leader.

Formally, our baseline re-election variable is a dichotomous variable defined as follows:

- 1 if an election (either of the legislative or the executive) takes place in year *t* and country *i* and the same government's chief executive that was in office in year *t* is also in office in year *t*+1.
- 0 if election takes place in year *t* and country *i* and the government's chief executive that was in office in year t is not anymore in office in year *t*+1.
- Missing if no election takes place.

In most cases, re-election corresponds to the confirmation of the chief executive after legislative elections; in other cases, the incumbent is either directly re-elected (US) or re-appointed (Finland, France, Portugal) after executive elections. Cases where re-appointment of the government chief executive are prevented by law are dropped from the sample (US elections in 1988 and in 2000, when the President terminated a second

[8] The information in Beck et al. (2001) was counterchecked from alternative sources. See Appendix.

mandate). To assess the robustness of our results, we construct alternative re-election dummies as well. One alternative considers the confirmation of a chief executive of the same party even if the identity of the chief executive is changed. This allows a different handling of those countries where chief executives alternate with high frequency (e.g., Italy and France). Another alternative, in line with Alesina, Perotti, and Tavares (1998), considers whether there is a change in the identity of the chief executive irrespective of elections being held.

3.1.2. Reforms

Concerning the reform variable, two broad approaches are possible (see Box 2). One possibility (the *ex-ante* approach) is to construct indexes for the occurrence of legislative changes. An alternative approach (*ex-post* approach) is to use indexes measuring the impact of reforms on the functioning of the economy. In our baseline analysis we follow the second route for two main reasons. First, the available databases on legislative action cover relatively small samples. Second, dummies simply reporting the occurrence of legislative changes are silent on the impact of policy measures, both for what concerns the timing and the intensity of such effects.

The reform database in our analysis is that used in Duval (2008), and includes *indexes of market "rigidity"* from various OECD sources measuring the anti-competitive / distortionary effects of policies for the 21 OECD countries included in our sample over the period 1985-2003 in five policy areas: labour taxes, unemployment benefit system, employment protection legislation (EPL), retirement schemes and product market regulations (see Appendix for details). In addition an *overall index of rigidity* is constructed as the sum of the five indicators for each area. We standardised all indexes so as to have a zero mean and unit standard deviation.

There is no obvious single way to assess the reform stance on the basis of market rigidity indexes, and different routes have been followed. In analogy with a series of existing analyses (e.g., Duval and Elmeskov, 2005; Duval, 2008), we adopt *asymmetric* and *discrete* baseline *reform indicators*. In our approach a reform in a given policy field is identified by a dummy taking value 1 whenever the corresponding index of market rigidity falls sufficiently. As a benchmark, we require the change in the index to be below the 20^{th} percentile of its distribution across the whole sample.

For our analysis there are advantages with symmetric and discrete indexes. Since we are interested in the electoral consequences of reforms reducing market rigidity, the use of a symmetric index would be justified only if the electoral impact of policies introducing more stringent regulations mirrors that of liberalisation and deregulation policies.

However, there are instances where the impact of reform raising or lowering the extent of regulation is likely to be highly asymmetric on the behaviour of interest groups and re-election probabilities. This is the case whenever stricter regulations only have indirect effects on the groups directly concerned, while leaner regulations may have strong and

visible direct consequences (think of EPL or regulations limiting competition in product markets). As for the choice of a discrete index, this has the advantage of capturing only relevant changes in policy, which are those that the electorate perceive.

However, aware of the inevitable risk or measurement errors inherent to any reform index, we complement the analysis on the basis of our baseline reform dummies with alternative indicators. In particular, we will also adopt, as an alternative benchmark, a symmetric and continuous indicator of reform, namely the simple change in the market rigidity index. We will also consider indexes identifying large increases in market rigidity ("counter-reforms").

To assess whether an incumbent government in a given electoral year is a reformist or a conservative one, reform indexes in single policy areas need to be aggregated into a *synthetic reform index*. The need for aggregating comes from the fact that the electorate is potentially affected by the whole series of policy actions carried out by the incumbent and that reforms undertaken in one area may be correlated with those in other areas. Table A1, reporting correlation coefficients across market rigidity indexes and reform dummies shows indeed that countries that are more rigid in one area tend to be more rigid in other areas as well, and that the pair-wise rank correlation between reform dummies is often positive and significant. Additionally, a synthetic index for our analysis needs to take into account the fact that re-elections may be affected not only by current reforms but also by past, recent ones. Hence, the baseline synthetic reform index used in the following analysis is as follows:

- A dummy taking value 1 if at least one reform (a change in the market rigidity index below the 20^{th} percentile) took place in country *i* either in year *t* or year *t*-1 in one policy field and if in neither year, and in no field, "counter-reforms" (a change in the market rigidity index above the 80^{th} percentile) took place.

Such an index is aimed at conveying information on whether the *incumbent government has been keeping a "market-friendly" reformist attitude* close to elections. Indeed, what the index requires is that some areas of the economy are substantially reformed while no areas are made "more rigid". An advantage of an index constructed this way is that no assumption is necessary on the weight to be assigned to reforms in different areas. Regarding the time horizon considered relevant for voters, the assumption embedded in our baseline reform index is that electoral outturns are affected by current or very recent events (reforms taking place in the current or preceding year), an assumption in line with existing empirical evidence on voters' horizons (Fair, 1978) and that we maintain throughout most of the empirical analysis that follows.

A limitation of our baseline synthetic index is that it may not capture reform episodes where a substantial reduction of rigidity in one policy field is coupled with substantially increased regulations and interventions in the market mechanism in a different field. The notable case is a reform seeking "flexicurity", combining lean EPL with relatively generous unemployment benefits. Table A1 shows that, although market rigidity indexes are strongly positively correlated across countries, their time variation covariates

negatively in some cases. Even so, reform dummies covariate positively in most cases (only exceptions being tax wedge reform with reforms in EPL and PMR), which may indicate that negative correlations are more often observed for small variations in the indexes, which are not picked up by the reform dummies.

As a robustness check, in the following analysis we will also make use of indexes constructed with alternative aggregation criteria, notably on the basis of the change in the overall index of market rigidity, which identify reforms when market rigidity falls on average, without excluding cases of substantial increase in rigidity in some policy area.

Box 1. **Measuring economic reforms**

The measurement of economic reforms involves the major difficulty of having to quantify the degree of intensity of policies of very different types. Several attempts have been made in recent times by the academia, policy institutions, and independent research centres to collect data on economic reforms and to develop indicators for the measurement of the effectiveness of such reforms.

A first approach to measure reforms consists of constructing indicators based on information on actual policies that have been implemented in given sectors, periods, and countries. Information is generally provided on the number of policy measures of certain types, possibly accompanied by an evaluation of such policies according to pre-defined criteria. This approach permits to obtain information on the action taken by governments with the purpose of reforming the functioning of markets or state institutions. Databases on policy measures of different types are constructed and maintained by national and international policy institutions and by independent research centres. Rodolfo de Benedetti Foundation (fRDB) maintains a database of reforms in the labour market domain (including retirement schemes) in EU countries starting from 1985 providing date and broad typology and characteristics of legislation changes approved by Parliaments. The European Commission maintains a database on labour market reforms enacted in all EU countries since 2000 (LABREF). Information is provided on a large set of reform features. A similar database developed by the European Commission focuses on product market reforms (MICREF). A database on labour market reforms in both developed and developing countries is constructed and maintained by the ILO.

An alternative approach consists of constructing indicators measuring the extent of existing distortions associated with government policies (e.g., the distortions associated with taxation or with the presence regulations in particular markets). The impact of reforms is measured in this case by the change in the level of the indicator measuring the degree of distortions. This second approach does not account directly for government reform initiatives, but permits to gauge the impact of such initiatives on the structural conditions of the different sectors considered. This approach also permits to assess the extent to which reforms are needed. Whenever the indicator reveals a high degree of distortions in particular sectors (as compared with other countries or periods) there is indication of a stronger need to carry out reforms. Abundant work in this area has been done by the OECD, both in the field of labour market (e.g., Employment Outlook, various issues) and product market reforms (e.g., Going for Growth, various issues). Indexes have been built also with the purpose of measuring, through questionnaires to businesses or consultant firms, the perceived impact of policy frameworks, thus providing information not only on legislated

policies but also on their enforcement. Examples of this approach are the World Bank "Doing Business" indicators and the World Economic Forum "World Competitiveness Index" available for both advanced and developing countries. The Fraser Institute assembles information on economic conditions, legislated policies, and experts' opinions to constructs indexes of economic freedom measuring inversely the degree of anti-competitive regulations and policies in several domains in a large set of advanced and developing countries since 1970.

3.2. Stylised description of the data

Over a total sample of 399 observations in our database, 123 elections are recorded, 117 of which were legislative elections and 12 were executive elections (held in Finland, France, Portugal, and the US). Incumbent chief executives were re-elected or re-appointed after 67 elections. The average rate of re-election does not exhibit any clear trend over time (Figure 1): re-elections became on average less frequent between 1985 and 1997, but rebounded since then. The frequency of elections (either legislative or executive) exhibits a limited variation across countries (Figure 2), but differences are substantial regarding re-election rates. While in Australia, Germany, the UK, and the Netherlands the chief executive was re-elected in more than 70 per cent of elections, re-elections occurred in less than a third of the cases in Greece, Japan, France, and Italy, with no re-elections recorded at all in the latter two countries.[9]

(Figures 1 and 2)

Regarding reforms, over the sample period a reduction in the extent of anti-competitive distortions in the majority of policy fields and countries is observed (Figure 3), but the differences across countries in terms of the levels of distortions have remained sizeable. Figure 4 displays the average values of the indexes of market rigidity in the different policy fields in 1985 and 2003 for various country groups, selected to match similarities in their "social models" (Sapir, 2006): EU, Southern EU (Italy, Greece, Portugal, Spain), Continental EU (Austria, France, Germany, Belgium), Northern EU (Denmark, Finland, the Netherlands, Sweden), and Anglo-Saxon countries (Australia, Canada, Ireland, New Zealand, UK, US).[10] Each of these country groupings portrays rather distinct reform histories. In all areas the Anglo-Saxon countries are those characterised by the highest market flexibility, and they have clearly preserved this position over time. The Southern EU countries have made major progress in the areas of

[9] These results are sensitive to the definition of re-election adopted. For instance, re-election frequencies in France and Italy are, respectively, 0.166 and 0.6, when reference is made to the confirmation of a chief executive belonging to the same party rather than to the identity of the chief executive.

[10] Two countries, Japan and Switzerland, are not included among any of the country groups in Figure 4. Japan exhibits falling rigidity indexes in the first years of the sample followed by rising indexes for tax wedge on labour and unemployment benefits and (de-trended) product market regulation. Rigidity in EPL and retirement schemes fall throughout the whole sample. Switzerland is characterised by stable (tax wedge, EPL) or rising (unemployment benefits, retirement schemes, product market regulation) rigidity indexes throughout most of the period. The product market regulation index in Figures 3 and 4 is de-trended, since in most countries product market regulation has been significantly falling over the sample period.

early retirement and employment protection legislation (EPL), but appear to have traded this against an increase in the generosity of unemployment insurance. As a result their constellation of structural indicators has converged to that of the Continental EU countries' model. The Northern EU countries, in contrast, drifted away from the Continental EU model, combining strong declines in the tax wedge on labour income, employment protection legislation (EPL) and product market regulation with virtually no change in the generosity of unemployment and early retirement benefits – reminiscent of their 'flexicurity' approach. The Continental EU countries portray, on balance, rather modest progress with structural reform, combining a policy easing in the areas of EPL, PMR and early retirement incentives with increases in the tax wedge on labour. As a result, the European Union as a whole has seen a modest easing in all market rigidity indexes with the exception of an increase in unemployment benefits – which is driven mostly by the Southern EU countries.

(Figures 3 and 4)

Figure 5 reports the frequency of reforms in each policy field as defined by our baseline threshold -- i.e., reductions in the index of market rigidity below the 20th percentile -- by country. By construction, in most countries reforms are recorded in 10 to 60 per cent of the sample years, with some countries recording zero reforms in some areas.[11] Those countries that exhibit a more intense reformist stance are in general those characterised by more rigid initial conditions as far as unemployment benefits, EPL and product market regulation are concerned (rank correlation between reform dummy across the whole sample equal, respectively, to 0.05, 0.12, 0.003), pointing to convergence, while for the tax wedge on labour and retirement schemes more frequent reforms are observed on average in already flexible environments (correlation between reform dummy and rigidity index equal, respectively to -0.08 and -0.01).

(Figure 5)

The baseline synthetic reform dummy used in the following analysis of re-election probabilities identifies reforms in about 24 per cent of the sample observations. Figure 4 shows that reform periods identified by our synthetic dummy were frequent especially in Austria, Belgium, Japan, and the US. These were the countries maintaining a consistent reform stance towards reduced market rigidity. Conversely, the frequency of reforms appears to be particularly low in Canada and Switzerland.[12]

[11] The histogram refers to the whole period, without restricting to years before or during elections. Note that data on retirement scheme policies for Greece are missing, so the histogram bar in this case needs to be interpreted as a missing rather than a zero.
[12] Note that in Figure 4 reform dummies in each area separately record reforms whenever a large fall in the market rigidity index takes place in the current year, while the synthetic reform dummy considers information spanning a 2-year period (see Appendix).

3.3. Reforms and re-elections: prima-facie evidence

From a simple comparison of re-election frequencies, there appears to be no significant difference between the re-election probabilities of governments no matter whether they carry out reforms or not, as gauged by our baseline synthetic reform dummy (Table 1, first line). Either way, the re-election probability is just over 50%.

(Table 1)

However, splitting the sample according to the occurrence of reforms in the each of the five individual reform areas seems to matter somehow for re-election probabilities (Table 1, lines 2-6). Tightening of early retirement benefits appears to be associated with significantly more negative electoral outcomes, and the same holds true for EPL. By contrast, tightening of unemployment compensation appears to be associated with an electoral gain. Finally, reforms regarding the tax wedge on labour or product market regulation (PMR) do not appear to matter much for re-election probabilities.

Based on the discussion in Section 2.3, we expect the re-election probability after major reforms to be affected by the policy environment in which the reforms are carried out, notably the initial degree of labour and product market rigidity, the degree of financial freedom, the size of government (a gauge of the size of the automatic fiscal stabilisers) and the fiscal policy stance. The first four lines of Table 2 reports the re-election probabilities of reformist governments (according to our synthetic reform dummy) in countries which either score high or low on these policy environment factors. The evidence seems to favour the hypothesis that reforms are more rewarding in electoral terms if carried out in economies which have already relatively flexible product and labour markets, a high degree of financial freedom (as measured by the Fraser Index of Financial Freedom; see Gwartney et al., 2007), a large government sector and a restrictive fiscal stance. However, also in view of the relatively small sample, for none of these environment factors are the differences statistically significant.

(Table 2)

Following the previous discussion, re-election could also be related to the strength of the incumbent chief executive's mandate for political change. One way to control for this is checking whether the government's chief executive is a newly appointed one. The prima facie evidence indicates that chiefs of government after having carried out reforms have a significantly bigger change of being re-elected if they are new in the job (70%) than if they are in their first term in office (40%).

4. EMPIRICAL STRATEGY

The next step is to formally test the prima facie findings using econometric methods. In this section we set up a baseline regression equation to test whether reforms affect electoral outcomes, controlling for other explanatory factors, which will then be estimated in Section 5. But first we need to discuss a series of econometric difficulties that may have an impact on our estimates.

4.1. Econometric issues

Before moving to the description of our estimation approach it is worthwhile recalling a series of thorny and intertwined econometric issues that are inherent to any analysis which aim at investigating the link between reforms and re-elections:

- *Measurement error*. As discussed previously, measuring reforms involves major difficulties. The implication is that econometric estimates could be biased by measurement error.
- *Reverse causation*. It cannot be excluded that incumbent governments form expectations on their re-election probability and take them into account when shaping their reform programme. If so, there may be causation running from the re-election probability to the reform effort. This endogeneity issue will result in biased and inconsistent estimates. A-priori is not evident whether such possible bias would be positive or negative. On the one hand, governments expecting to be re-elected could become "bolder" and carry out reforms that could have otherwise been considered excessively risky in electoral terms. On the other hand, a government which has little to lose electorally could pursue a programme of ambitious reforms to "shake the electorate" and overcome a situation where re-election prospects look grim anyway.
- *Selection bias*. A related endogeneity issue arises from the fact that observed policy changes do not permit to take into account the electoral impact of potential reforms that have *not* been carried out due to an expected bad electoral impact. This selection bias is likely to tilt upward the estimate impact of reforms on re-election probabilities.
- *Omitted variables*. If omitted variables are relevant and correlated with the reform variable, there is a risk of estimating with a bias the electoral impact of reforms. Such an issue cannot be easily fixed by adding regressors if the omitted relevant variables are unobservable. This eventuality cannot be easily ruled out in our case. Incumbent-specific characteristics observable to the electorate but not easily observable to the researcher (e.g., political ability, charisma,…) may at the same time be correlated with reform activism and have a significant impact on re-elections. Alternatively, market rigidities may be co-determined with social capital and trust (Aghion, Algan, Cahuc, and Shleifer, 2008), which could in turn have explanatory power on the odds of re-election.

Tackling the above issues raises substantial difficulties and an attempt to satisfactorily dealing with all of them is beyond the scope of this paper. Regarding measurement errors, we will perform robustness checks with respect to the way the reform variable is constructed. Additionally, we will check robustness with respect to alternative assumptions for the construction of the re-election variable and with respect to the time horizon assumed for the electorate. As regards omitted variables, we will consider, in addition to reforms, a relatively large set of potentially relevant economic and political explanatory variables and will experiment with the inclusion of country and time dummies as a way to account for a possible omitted variable bias.

Ideally, dealing with reverse causation, selection bias, and the omission of relevant unobservable variables would require instrumenting the reform variable. However, the usual difficulty of selecting valid and strong instruments appears particularly severe for our analysis because most of the macroeconomic factors that may be expected to exert an impact on reforms (economic conditions, other government policies,…) could in principle affect also re-elections. Additionally, since the available sample is small (the sample is limited to election years only), and statistical properties of instrumental variable estimators hold only asymptotically, instrumenting will lead not only to imprecise estimates, but relevant small sample biases may remain. As an alternative to deal with endogeneity issues, and building on existing literature, we repeat our baseline estimates on a sample including only EU countries after 1992 which is likely to include observations where reforms are more likely to be exogenous because of the external constraints of the Single Market and the Maastricht Treaty.

4.2. Baseline specification

Econometric investigation of the determinants of re-elections require limited dependent variable techniques, where the dependent variable is a dichotomous index and the estimation method is non linear and obtained by maximum likelihood. In our baseline regressions, we rely on Probit estimations. Our baseline econometric specification expressed in latent-variable formulation is as follows:

$$R_{i,t} = 1 \text{ if } R^*_{i,t} > 0; \qquad R_{i,t} = 0 \text{ if } R^*_{i,t} \leq 0, \tag{1}$$

where $t = 1,...,T$ denotes time, $i = 1,...,N$ denotes countries, $R_{i,t}$ is our dichotomous re-election variable, while $R^*_{i,t}$ is a latent variable with $\text{Prob}(R^*_{i,t} > 0) = F(\beta' X_{i,t})$ given by the standardised normal distribution function $F(.)$, and

$$\beta' X_{i,t} = \beta_0 + \beta_1 D^{reform}_{i,h<=t} + \sum_{k \in K} \beta_k X^k_{i,h<=t} \tag{2}$$

In (2), D^{reform} is a reform variable and X^k are economic, political, and institutional controls that vary across countries and possibly over time. In the following analysis, we will consider both our baseline synthetic reform dummy, reform dummies for each

policy fields and alternative reform indicators, primarily the change in the overall or field-specific market rigidity index.

It is important to note that the value of the explanatory variables that *potentially* affect re-election are those of the election year and preceding the election year up to the initial year of mandate of the incumbent. In line with the existing evidence indicating that voters are likely to be more concerned by recent events and outcomes (e.g., Fair, 1978), and following the lead of previous papers analysing re-election determinants (e.g., Brender and Drazen, 2008), we assume in our baseline specification that voters are mostly affected by recent events. Hence, in our baseline specification the reform variables, as well as the other explanatory variables incorporate information spanning two years (the values for the macroeconomic controls are averages of the current and preceding year).[13]

In addition to reform variables, measures of cyclical conditions and of the fiscal stance are included among the macroeconomic factors that are expected to affect the probability of re-elections. Cyclical conditions are captured by both the level and the change in the output gap. While the former captures whether the level of economic activity is above or below potential, the latter permits to take into account whether the economy was in an upturn or in a slowdown during and just before elections. We expect the first variable to display stronger explanatory power if voters are more likely to judge the performance of the incumbent on the basis of how macroeconomic conditions have changed, rather than on their level. The fiscal stance is measured by the change in the cyclically-adjusted primary balance (primary CAB), which is a standard measure of the discretionary fiscal impulse. In line with previous studies, we also include a measure of inflation (year-on-year changes in CPI-based inflation) among the macroeconomic factors that may affect electoral outcomes.

To control for country-specific political conditions that may affect the probability of re-election, at given macroeconomic performance, we included in our baseline regression a series of variables capturing the characteristics of the political system and of the political juncture. All variables are from the Database of Political Institutions (Beck et al., 2001) and are described in the Statistical Appendix. Political system variables are dummies indicating whether the political system of each country in each year is parliamentary (vs. presidential) and whether the voting system is proportional (vs. majoritarian). The variables that capture the particular political juncture in each country, and notably whether the incumbent government is "strong" vis-à-vis the opposition, are the margin of majority in the Parliament by the ruling party, a variable capturing the degree of political polarization between the ruling party and the other main parties. Moreover we control for the composition and the history of the ruling coalition and the possible erosion of its power using the percentage of veto players who drop the government as a control (see Appendix). Finally, we include the years of democratic history of each country as an explanatory variable, on the basis of the finding in previous

[13] Only contemporaneous values are used instead for time-varying political controls since those variables are assumed not to depend on the incumbents' actions during their mandate.

analyses that young democracies have a higher re-election rate and are more subject to political budget cycles (Brender and Drazen, 2005). [14]

5. ARE REFORMIST GOVERNMENTS MORE LIKELY TO BE RE-ELECTED?

5.1. Baseline results

Table 3 reports estimation results. Column (1) and (2) refer to pooled Probit regressions (standard errors robust with respect to heteroschedasticity and possible non-independence within countries). Hence, variation is allowed to take place along both the cross-country dimension (do more reformist countries exhibit higher re-election frequencies?) and the time series dimension (do more reformist incumbents see their re-election probabilities increased?). Column 1 in Table 1 reports the results for a comprehensive specification. Column (2) restricts the specification to significant macroeconomic explanatory variables.

Results indicate that cyclical conditions, as measured by both the level and the change in the output gap, have a significant effect on the re-election probability. This result complements that obtained in Brender and Drazen (2008), where, in a larger panel of developed and developing countries, it is shown that growth increases the probability of re-election, although significantly so only among developing countries. Conversely, the change in inflation and the fiscal stance, as measured by the change in the primary cyclically-adjusted budget balance. do not reach statistical significance at the 10 per cent level. While inflation has the expected negative sign, the fiscal policy variable has a positive effect, indicating that a tightening of the fiscal stance increases the probability of re-election. This result is at odds with the common belief that incumbents may have an incentive to loosen strategically the budget in order to expand the economy and enhance this way their probability of re-elections, but is consistent with a series of recent studies linking fiscal performance to political outcomes in single countries (Peltzman, 1992; Brender, 2003; Drazen and Eslava, 2007) and across large country panels (Brender and Drazen, 2008).

Regarding the performance of political controls, it clearly appears that re-elections are less likely in parliamentary systems, which is in line with the argument in Persson and Tabellini (2003) that in a parliamentary system the equilibrium rents of politicians are usually higher, therefore inducing the voters' disapproval. The probability of re-election is instead significantly higher with proportional voting rules. In line with expectations it appears that a stronger incumbent has a higher probability of re-election: a greater margin of majority, a lower number of verto playes leaving the government, and a less polarised political position compared to the other main parties of the legislature help re-

[14] A dummy for Parliamentary elections taking place under plurality rule (vs. other types of majority), a variable quantifying the fragmentation of the opposition, a variable measuring the longest tenure of a veto player a dummy taking value 1 if the ruling party has a majority in all Parliamentary houses were also tested but not included in the selected specification because insignificant.

election. Not all political are statistically significant. However, a Wald test for the hypothesis of political controls being jointly equal to zero suggests keeping them in the baseline equation.

As for the re-election variable, it appears not to be a powerful explanatory factor for re-elections: the regression coefficient is positive (a marginal effect on the re-election probability of about 11 per cent) but not statistically significant.[15] Hence, also controlling for other factors that affect the probability of re-election, the prima-facie evidence presented in the previous section is confirmed: being reformist does not have a strong impact on the re-election of incumbents per-se.

(Table 3)

The issue arises whether the macroeconomic and political controls in columns (1) and (2) of Table 3 are sufficient to capture the heterogeneity of re-election probabilities across countries. Although the inclusion of fixed effects to be estimated in a probit regression would lead to inconsistent estimates (see, e.g., Woolridge, 2006, page 484), cross-panel heterogeneity in the logit model can be accounted for by the maximisation of a likelyhood function conditional on the frequency of successes within each panel (the re-election history of each country in our case), an estimator commonly referred to as fixed-effect logit or conditional logit (Chamberlain, 1980). Columns (3) repeats the regression in column (2) but using logit estimates, column (4) reports the marginal effects on probability, column (5) displays the results for the conditional logit. It appears that the results obtained in the logit and in the conditional logit estimates are fairly close.[16] As suggested in Greene (2000), the choice between an unconditional versus a conditional logit fixed effect estimator could be made on the basis of a Hausman specification test on the hypothesis that the coefficients obtained with the more efficient estimator (unconditional logit) are jointly non-significantly different than those obtained with the consistent estimator under the alternative hypothesis of non homogeneity (conditional logit). The Hausman test does not reject at the 10 per cent level the null hypothesis of homogeneity neither for the regression coefficients nor for marginal effects. Based on this result, one should opt for the unconditional probit estimator.

Additionally, we directly test estimated fixed effects but using a linear probability model -- where the re-election variable is regressed on the explanatory variables in column (2) -- to overcome the issue of inconsistent estimates arising in Probit regressions. Both country and year effects are considered in the specification in column (6). Including dummies both for years and countries non-surprisingly kills the significance of most explanatory variables. Wald tests for the joint significance of fixed effects reject both year and country effects. By excluding time effects (column 7) results hold qualitatively as with pooled Probit regressions, and the Wald test rejects country fixed effects. To check robustness of the findings in column (2) for the inclusion of

[15] The marginal effect on probability for dummy explanatory variables is reported as the discrete change in probability for the dependent when the explanatory is set equal to one.

[16] Note that in the conditional logit the parliamentary system dummy is dropped due to absence of within group variation and in all estimations including estimated fixed effects (columns (6)-(8)) it is dropped due to collinearity.

estimated fixed effects in the Probit, we repeat Probit regression with inclusion of estimated country effects in column (8), aware that this would lead to inconsistent estimates. Results appear fairly robust. Hence, it seems that, also when controlling for the distinct re-election history in each country, results are not significantly affected. Based on this evidence, we keep the pooled Probit regression in column (2) as our baseline. However, in the following analysis, we will also continue checking whether robustness is kept when the inclusion of fixed effects in Probit estimates is allowed.

Finally, our baseline results appear to be fairly robust also with respect to the composition of the sample along the country dimension. As shown in Figure 6, the marginal effect on probability associated with the reform dummy is never largely affected by the exclusion of any of the country in the sample.

(Figure 6)

5.2. Robustness checks

5.2.1. Alternative ways to measure reform

As stressed above, measuring reforms involves a series of conventions and assumptions whose validity is uncertain. This raises a measurement error issue. Moreover, depending on the way the reform variable is constructed, the interpretation of the empirical results of the electoral impact of reforms may change, adding potentially useful information to the analysis. In this section, we test the robustness of results compared with different ways of constructing the synthetic reform variable. In doing that, we repeat regressions for specification (2) in Table 3 (with and without country fixed effects) using different definitions for the reform dummy.

We consider first an asymmetric and discrete synthetic indicator which however combines in a different way information in different reform areas. To this end, we construct a dummy that takes a value 1 if the overall index of market rigidity falls by more than the cut-off point of its lowest two deciles, and 0 otherwise. Hence, it this variable is equal to 1 the reform stance is on net pointing to substantially lower market rigidity, even if this may allow that some areas are become more rigid. This variable assumes that market rigidity indexes for different areas can meaningfully summed up, an assumption which is not necessary in our baseline dummy, but accommodates somehow for cases in which reforms in one sector of the economy need to be accompanied by reforms in a different sector (e.g., moves towards flexicurity).

Second, still building on the change in the overall index of market rigidity, we compute a symmetric reform dummy which equals -1 if the synthetic index increases by more than the cut-off of the upper two deciles but which is otherwise identical to the above version.

Third, we consider a continuous reform index constructed on the basis of the level or change in the overall market rigidity index (columns (3) and (4) of Table 3). This

permits to check whether it matters that the reform selection is restricted to bold reforms only or also includes minor policy changes. To allow for non-linearity in the electoral impact of reform we also experiment with the inclusion of the square of the overall market rigidity index (or its change) in the specification to be estimated.

Fourth, a reform index is constructed as the arithmetic sum of the reform dummies (based on the second percentile cut-off points) for each of the five policy areas. Hence, this index (which ranges from 0 to 5) provides information not only on whether a reform has taken place but also on the number of areas affected by reforms.

Finally, we investigate the impact of a "counter reform" dummy capturing whether the incumbent has increased market rigidity in at least one policy area beyond the cut-off point of the upper two deciles of the sample.

Table 4 reports results on the basis of these alternative variants for the reform index. It confirms the baseline result that on average reforms have no significant impact on re-elections. There is no significant effect of the squares of the continuous indexes. "Counter reforms" do not seem to deliver electoral gains: assuming everything else equal, 'populist' governments have no higher chance of getting re-elected. Although it is never statistically significant, the impact of the continuous reform indexes switches from negative (signalling that reforms are good for re-elections) to positive when fixed effects are introduced, suggesting that reforms and re-election are more negatively correlated over time than across countries. This difference arises only if the continuous index is used, indicating that while controlling for country effects is immaterial for big reforms, it could matter for marginal policy changes.

(Table 4)

5.2.2. Alternative re-election variables and voters' time horizons

Our baseline specification calls a re-election if the chief executive is reconfirmed in office, but not necessarily if his party wins the elections. It also assumes that voters base their judgement on the incumbent's performance in the election year and the preceding year. Both assumptions are relaxed in a set of alternative specifications. Table 5 reports the results for the following cases: (i) re-election is called if the identity of the chief executive or someone from his party is reconfirmed in office, (ii) the chief executive is reconfirmed in any year irrespective of whether there has been an election or not, (iii) voters base their judgement only on the election year ("myopic voters"), (iii) voters base their judgement on the three years preceding the elections or (iv) the whole term in office of the incumbent ("long-memory voters"). In all these cases the predictive power of the cyclical position of the economy of the election result is broadly reconfirmed, although it is slightly weaker for the alternative election definitions (Table 5). It is also confirmed the absence of statistical significance on re-election of reforms, irrespective of the assumed memory of voters or the definition of the re-election variable. However, results do provide some evidence that voters are more sympathetic towards reforms implemented at the beginning of the incumbent's term than to those implemented close

to elections. This may reflect that it takes several years for voters to perceive the positive effects of reform or for the initial "pain" to fade.

(Table 5)

5.2.3. Are reforms exogenous?

The results so far suggest that the re-election probabilities of incumbent governments are largely unaffected by their overall reform effort, although there may be differences depending on the reform orientation. However, reformers are likely to form expectations about their re-election probability and take this into account when shaping their reform programme. If so, the reform effort is endogenous, with the (expected) re-election probability being one of its determinants. Moreover, observed policy changes may not be exogenous because the prospects of election may induce incumbents to select only those reforms that are less electorally costly. For those reasons, a priori it cannot be ruled out that our regression result is biased by this potential source of endogeneity.

It is beyond the scope of this paper to pursue a fully-fledged account of endogeneity of the explanatory variables, which calls for instrumentation techniques with strong limits in our small sample. However, we attempt to address somehow a possible endogeneity problem by limiting the sample to cases where reforms are less likely to be highly endogenous with a view to compare results with the big sample.

Specifically, we partition our sample is such a way to keep only countries and years for which to a greater extent reforms are the result of an inevitable adaptation to structural changes triggered by developments in the international policy environment, and therefore more likely to be exogenous (i.e., less affected by electoral considerations). On the basis of the findings from existing work on the determinants of elections (Høj, Galasso, Nicoletti, and Dang, 2006), we limit the sample to EU countries after 1992 since the Maastricht Treaty and the Single Market, both in force in the European Union since 1992, had an impact on EU countries' structural reform effort since then. [17]

Repeating our baseline specification for a sample so defined, it appears that the synthetic reform dummy turns from positive to negative but still has no significant effect on re-election, irrespective of the inclusion of country-fixed effects (column (1) and (2), Table 6). However, when using the continuous reform variable based on the change in the overall market rigidity index (columns (3) and (4)), reforms do appear to have a significant negative impact on re-election, but this effect disappears again when country-fixed effects are included.

We interpret this evidence as follows. After the completion of the Single Market and the signing of the Maastricht Treaty, some countries found themselves with less freedom to select their reform projects and therefore felt the pinch of reform-related electoral backlashes, unlike their non-EU peers. However, this phenomenon seemed to have been

[17] Joj, Galasso, Nicoletti, et al. (2006) investigate the determinant of reforms as measured by OECD indicators and find a strong explanatory power of a dummy variable equal to 1 for EU countries after 1992 and which is interpreted as capturing the effect arising from the completion of the Single Market.

relevant mostly in the case of marginal reforms, as evidenced by the non-significant coefficient for the baseline reform dummy (which only considers significant policy changes reducing market rigidity). This could be an indication that major reforms are in general relatively more driven by the structural needs of the economy, and therefore less prone to reverse causation and selection bias, while cosmetic policy manipulation for electoral purposes is more likely in the case of marginal policy changes.

All considered, while the endogeneity of our reform variables is a distinct theoretical possibility, it remains an unsettled issue as to what extent this affects our results. Based on our tentative checks we are inclined to give it the benefit of the doubt. However, we feel somehow relieved by the indication that discrete reform dummies capturing only big policy changes may be less subject to endogeneity issues.

(Table 6)

6. WHICH REFORMS HELP RE-ELECTION?

The prima-facie evidence in section 3.3 suggests marked differences in the reform record across policy areas. Tightening of retirement benefits and easing of employment protection legislation (EPL) appeared to be associated with negative electoral outcomes, thus providing support for the hypothesis that such reforms adversely affect 'insiders'. By contrast, tightening of unemployment compensation would be associated with an electoral gain as it would be more detrimental for outsiders and therefore less electorally damaging. In contrast, electorates seem to be indifferent with regard to tax reform and reform of product market regulation (PMR). In this section our objective is to check if these prima facie impressions can be confirmed in a fully-fledged probit exercise in which relevant controls are included.

The results are reported in Table 7. Columns (1)-(5) reports our baseline specification using as reform variable reform dummies for each of the five policy areas separately. Column (6) reports results when using simultaneously all reform dummies in the same specification. The main message is that the electoral impact of reforms is indeed strongly dependent on the specific reform considered. As expected, the sign of the impact differs across policy areas, with some reforms significantly helping re-election while others making it more difficult. All this suggests that the ambiguous and non significant sign of the overall reform index in our baseline specification can be seen as the outcome of offsetting positive and negative effects of different types of reforms.

(Table 7)

Specifically, reforms reducing the tax wedge are electorally profitable, which appears in line with the idea that, other things equal, lower tax wedges benefit the economy via lower unemployment and improved work incentives, while directly favouring workers, undoubtedly a large constituency. Interestingly, the inclusion of proper controls in the

analysis of re-election determinants permit to identify a statistically significant impact of tax-wedge reforms, while this was not the case from the simple comparison of re-election frequencies (see Table 2).

As for the remaining labour market policies, the lowering of unemployment compensation appears to be electorally rewarding while policies to make employment protection less generous appears to be detrimental for re-election probabilities. This seems to corroborate the relevance of the insider-outsider hypothesis, with cuts in unemployment insurance mostly hitting outsiders and cuts in EPL colliding with the interests of insiders. By the same token, tightening of retirement benefits turns out to be clearly detrimental for the re-election probability of chief executives. This is not surprising, given the large electoral constituency that is directly affected by these reforms.

Product market reforms, as captured by our indicator, do not seem instead to exert a significant impact on re-election, possibly due to the fact that the groups concerned (those employed in the non-manufacturing sectors covered by the indicator) are not sufficiently quantitatively relevant.

When including all reforms dummies in each area in a single specification the results hold in a qualitative sense, the only substantial difference being a drop in the significance of the tax wedge variable, possibly due to multi-collinearity problem.

The issue arises whether the results above are robust with respect to the inclusion of country effects and whether it matters how the reform variable is constructed. To shed light in this direction, Table 8 presents results allowing for the inclusion of fixed effects and considering as an alternative the reform variable defined in term of changes in market rigidity indexes. Results hold broadly in qualitative terms, although statistical significance drops somewhat when country-fixed effects are included, as may be expected since countries' distinct re-election histories are now picked up by the country dummies.

The results are also less significant if the continuous reform index is used. In this case, in particular, the evidence on the electoral impact of tax wedge and umemployment benefits reforms becomes shaky, with non-significant coefficients and signs changing after the inclusion of fixed effects. Conversely, the evidence regarding the negative electoral impact of reforms affecting retirement schemes appears robust and further strengthens if country fixed effects are included.

(Table 8)

7. WHICH FACTORS MATTER FOR THE ELECTORAL IMPACT OF REFORMS?

The results for our baseline specification suggest that the re-election probabilities of incumbent governments are probably unaffected by their reform record, except for very specific combinations of reform. However, in light of the arguments discussed in

section 2.3., there may well be factors which can raise or reduce the reformist incumbent's probability of re-election. We distinguish two groups of factors: (i) those related to the policy environment in which the incumbent is operating, such as the degree of financial freedom or the size of the public sector, and (ii) features of the incumbent itself, such as the strength of his mandate or more generally his political capital. In this section we will examine both sets of factors.

7.1. The policy environment

As discussed in Section 2.3, and as partly confirmed by the prima facie evidence in Section 3.3, the probability of re-election of a reformist government may be affected by the initial degree of market rigidity, financial market efficiency, the size of the fiscal automatic stabilisers and the stance of fiscal policy. We gauge these factors by, respectively, our baseline overall index of market rigidity, the Fraser index of financial freedom, the ratio of current cyclically-adjusted government expenditure on GDP and the change in the primary cyclically-adjusted fiscal balance, respectively. We enter these factors as interactions with the reform dummy, with the interaction terms standardised (zero mean and unit standard deviation) so as to ease interpretation: the non-interacted term is the impact of reforms keeping the interacted term at sample mean; the interacted term is the change in the electoral impact of reforms when raising the interacted variable by one standard deviation.

Results are reported in Table 9. The specification is our baseline (column (2) in Table 3). Columns (1) – (5) consider separately each of the interactions with the policy environment variables. Column (6) only maintains the interactions that turn out being significant. Column (7) repeats the analysis in column (6) but using the change in the overall index of market rigidity as a reform indicator. Results can be summarised as follows:

- The interaction effect of the reform dummy and the market rigidity of the economy is always negative, meaning that the more rigid product and labour markets are, the less likely it is that a reformist government will be re-elected. This seems a relevant finding. It suggests that economies with poor structural policy conditions may be trapped in this situation, at least in the absence of major exogenous shocks. It also suggests that, once the reform process has started, it tends to reinforce itself because further reforms get politically more acceptable. This evidence is consistent with the widely investigated phenomenon of reform inertia discussed in Section 2.3.
- The interaction effect of the financial freedom index with the reform dummy is always positive. This confirms our prior that liberal financial markets will facilitate voters' acceptance of structural reform and hence raise the re-election probability of reformist governments. This result is relevant mostly across countries, i.e. as an explanation of their different electoral histories, as suggested by the fact that the interaction becomes statistically insignificant when country-fixed effects are included. Financial freedom, quite intuitively, seems also relevant especially for big

reforms reducing market rigidity, an interpretation that arises from the statistically insignificant coefficient for the interaction when using the change in the overall market rigidity index as reform variable (which incorporates information on reforms increasing market rigidity and marginal reforms as well). Overall, it appears that, other things being equal, in countries with less intrusive financial market regulations there is better electoral acceptance of most type of substantial reforms reducing market rigidity.

- The interaction with government size (as measured by the share of current cyclically-adjusted government expenditure) has the expected sign: more redistribution taking place via the government raises acceptance of reforms. However, the significance level does not reach 10 per cent. Hence, the prima-facie descriptive evidence in Table 2 is not confirmed once proper controls are added in explaining re-election probabilities. As for the fiscal stance variable, not only is its impact largely insignificant, but the sign goes against common expectations: the more restrictive discretionary fiscal policy, the more likely is it that a reformist government is re-elected. This suggests that there is no point in the government "bribing" the electorate into a structural reform programme through fiscal handouts.[18]

(Table 9)

The above results refer to what factors matter for a reformist government to be re-elected. A related but different question is what factors matter for the electoral impact of the various types of reforms. We address this question in the analysis displayed in Table 10. The variables describing the policy environment used as interaction terms are the same as those that were significant in Table 9, i.e., market rigidity and financial markets, but the reform indicator corresponds this time to reform dummies in each field separately.

We opt to keep the overall market rigidity index (rather than that relating to each policy under consideration) as an interaction variable for two main reasons. First, market rigidity is likely to matter not only for the own policy domain but also in other related domain (a notable example being product market rigidity affecting the incentives to reform labour markets). Second, this permits a comparison with the results obtained for the synthetic reform variable in Table 9. On the basis of the findings in Table 8, showing that using reforms dummies or the simple change in the rigidity indicator matters for estimating the electoral impact of tax wedge and unemployment benefits policies, both reform variables are used and robustness is checked with respect to the inclusion of country effects. Several results stand out.

[18] The main message from this analysis of the impact of economic framework conditions appear robust with respect to alternative ways for constructing the reform and the re-election variable and alternative assumptions for the memory of voters (unreported results, available upon request. As alternative measures of initial market rigidity we have also considered performance-based variables, rather than policy indexes. In this respect, both the unemployment rate and potential growth are non-significant when interacted with the reform dummy. Moreover, as an alternative measures of government size, total government revenues on GDP exhibit a positive a almost significant interaction coefficient with the reform dummy (unreported, available upon request). Unreported results also show that, among the sub-indexes forming the Fraser index of financial freedom , those referring to interest rate control and bank competition have the strongest explanatory power in driving the coefficient of the interaction term.

- Regarding market rigidity, its interaction is mostly significant with tax wedge reforms and reforms in retirement schemes. The finding relating to tax wedge reforms is robust to the introduction of country-fixed effects, so it works in both dimensions, over time and across countries. It becomes weaker instead when tax wedge reforms are measured via the change in the market rigidity indicator. So, apparently, there are mechanisms at work pushing the electorate to vote even more in favour of lower income taxes for regular wage earners when markets are more flexible. Our favourite reading of this finding builds on an argument akin to Blanchard and Giavazzi (2003): the marginal gain from tax wedge cuts to the largest constituency in favour to such policy (i.e., workers) is higher when markets are less rigid, because it is in that case that the tax wedge is less easily shifted onto employers. The effective distribution of the tax wedge burden between workers and employers depends – in addition to fundamentals affecting labour supply and demand elasticities - upon the relative bargaining power. Hence, the less the policy environment favours workers' and unions' bargaining power (e.g., low unemployment benefits, low EPL, low product market regulation, tight retirement treatment), the larger the marginal gain from a lowering of the tax wedge to workers, and then the stronger the electoral support in favour of tax wedge reductions.[19]
- Regarding retirement schemes, in line with insider-outsider arguments, the evidence indicates that the opposition to tightening retirement benefits is stronger when markets are more rigid.[20] This result is not significant but close to borderline using the reform dummy indicator, becomes largely insignificant introducing country effects, and turn highly significant using the change in the overall rigidity index. Hence, it appears that market rigidity matters especially for the consensus towards marginal reforms and in raising support in favour of reforms increasing the generosity of pensions (which induce a positive change in the indicator of market rigidity).
- Market rigidity plays in line with the insider-outsider argument also for product market reforms, although with non significant coefficients of the interacted variable, while the evidence is not consistent with this explanation for the case of unemployment benefits (non-significant interaction term) and employment protection legislation (fragile estimates for the interaction terms, with the cross-section variation playing against the time series variation when the reform dummy is measured by the change in market rigidity).
- Turning to the performance of financial freedom as an interaction variable, in line with the findings in Table 10, whether reforms are measured with a discrete dummy

[19] The interaction of the tax-wege dummy is negative with each of the market rigidity indexes, borderline for unemployment benefits EPL, and and significant for the other policy areas. This evidence could provide, inter-alia, a rationalisation to the finding in Joj, Galasso, Nicoletti, and Dang (2006) that tax wedge reforms are less likely the more generous are unemployment benefits.

[20] In particular, unreported results experimenting with alternative interaction terms show that reforms in retirement schemes are more strongly opposed electorally if the market rigidity index for implicit tax on continued labour is high, suggesting that when the pension system is more generous, the rents accruing to the group of insiders are higher and the opposition to its reform is more vocal.

or with the change in the rigidity indicator matters for the interaction with the financial freedom index. Financial freedom generally appears to significantly facilitate consensus towards reforms when those are measured by discrete dummies (the only exception being EPL). The positive interactions of financial freedom with labour market and retirement scheme reforms can be interpreted in light of better room for absorbing temporary costs of reforms. In case of product market reforms, the evidence is also consistent with the view that more efficient financial markets reduce the opposition of insiders to product market reforms (Caselli and Gennaioli, 2008). However, interaction coefficients are normally non-significant when reforms correspond to changes in the market rigidity index. Additionally, financial freedom appears to matter especially along the cross-section dimension, since the inclusion of country dummies generally reduces significance.

(Table 10)

7.2. Incumbent-specific factors

The analysis so far does not explicitly assess whether re-election probability after reforms also depends on the role of the incumbent, notably to the strength of its mandate to promote change. Economic reforms may indeed be just one part of an overall package for political change supported by voters. The electorate could therefore be inclined to reward reformist incumbents if their reform effort conforms to their original mandate for change. As discussed in Section 2.3, such a commitment for change is likely to be stronger when government chief executives are new in the job and when governments have different ideological orientation compared with predecessors. Moreover, reforms are more likely to be included in the government's initial mandate if among voters they are perceived as a priority, irrespective of their expected or actual economic impact. This is the case for instance if reforms are widely seen as a necessary step to "modernise" the country or increase international prestige and reputation.

In a first regression (column 1 of Table 11) we consider whether the chief executive is in his first term in office. If so, this may signal appetite for change among the electorate and could affect his re-election probability after reform is implemented. This is indeed supported by the results: the re-election probability of a reformist government increases if the incumbent is in his first term in office, at least if country-fixed are included. Note that the very strong prima-facie results in Table 2 appear weaker and qualified once other explanatory factors of re-elections are taken into account. In particular, it appears that the extent to which new chief executives are more likely to be re-elected if reformists is largely country-specific, i.e., needs to control for the electoral history of different countries, as suggested by the improved significance once including fixed effects in the specification.

A different way to capture to what extent reforms could be embedded in the incumbents' mandate is to look at the record of the predecessor. If the defeated

predecessor was a non-reformist government, then this could be an indication of a general appetite for reform. With such a measure no significant results are obtained (column 2 of Table 11), but the sign is as expected: the odds of re-election tend to be higher for a reformist government whose predecessor was non-reformist.

In alternative specifications (columns 3 and 4 of Table 11) we interact measures of the ideological orientation of the incumbent. First, the political colour (an indicator taking values -1, 0, 1, according as the incumbent is judged to be left-wing, center, or right-wing) with the left/right dimension being largely identified by less or more pro-market orientation. Second, the political distance of the incumbent from its predecessor (the difference in the political colour index). The result is not statistically significant, but again the sign is as expected: relatively pro-market governments are more likely to be rewarded for structural reform.

(Table 11)

8. CONCLUSIONS

Very few attempts have so far been made to assess empirically the impact of structural reforms on re-elections, even if this is widely recognised to be a very relevant piece of the puzzle in understanding the determinants and dynamics of structural policy change. There are many unsettled issues, most prominently the choice of the best gauge of the intensity and direction of reform and the degree to which reform can be considered as exogenous vis-à-vis the election histories of countries. As well, while there is a plethora of theories regarding reform inertia and elections available in the literature, a unifying paradigm is far from being in the bag. Although the results reported in this paper are still to some extent tentative, our analysis uncovers a series of findings of potential interest.

First of all, the fact that an incumbent government has taken a pro-market reformist stance does not appear to have any discernable impact on the re-election of incumbents *per se*, although voters appear more sympathetic to reform activism at the beginning of the incumbent's term than close to elections. This may reflect that it takes time for the favourable effects of reform to reveal themselves or for possible reform costs to fade away. It also appears that “counter reforms” that introduce more market rigidity, are not helping governments to be re-elected: “populist” economic policies are not necessarily popular.

While the electoral impact of overall reform activity seems to be negligible, this is not necessarily the case across specific types of reform. Specifically, pension reform and reform of employment protection legislation turn out to be detrimental for re-election, whereas the reverse would be the case for reform of unemployment insurance. Such divergent electoral effects may point to a predominance of insider-outsider mechanisms. Pension and employment protection reforms may hit large constituencies of insiders in the labour market, and therefore would be electorally damaging, whereas the reverse

may be the case for unemployment insurance reform which is possibly more directed towards outsiders.

We find no evidence that expansionary fiscal policy would be of any help for the electorate to digest structural reform in product and labour markets, nor do we find a highly significant impact of automatic fiscal stabilisers on the probability of reformist governments to be re-elected. Apparently, there is no point for the government in bribing the electorate into a structural reform programme through fiscal handouts – which is again contrary to popular perceptions.

There are framework conditions which, however, do seem to have a role in shaping the electoral chances of reformist governments. First, the more rigid product and labour markets are initially, the less likely is it that a government will be re-elected after having pursued structural reform. This negative impact of market rigidity on the electoral acceptance of reform appears to be dominated by the impact of cuts in the tax wedge on labour . Apparently, in rigid economies voters are less pre-occupied by labour income tax, possibly because they can more easily shift the burden forward. Second, in countries where financial markets are less heavily regulated voters' acceptance of structural reform appears higher, the most likely reason being that financial markets help to absorb possible short-term losses associated with major reforms reducing market rigidity. Third, we find evidence that a reformist government is more likely to be re-elected if it is endowed with a strong mandate for market-friendly policies: newly-appointed and pro-market reformist governments are more likely to be re-elected.

A major difficulty in our analysis is that there may be an issue of reverse causality and selection bias when assessing the electoral impact of reforms. Governments are aware of the electoral costs and benefits of reform and may be inclined to carry out those which are least costly, and be more active reformers if they can be sure to be re-elected for reasons that are beyond their economic policy agendas. Satisfactorily addressing such issues would require instrumenting the reform variable, which however requires the sample to be of a sufficiently large size, which is most probably not the case in our analysis. Nevertheless, by restricting our sample to a sample where reforms are less likely to be affected by endogeneity issues, we find no strong evidence that our baseline estimates suffer from these biases. We find some evidence that selection bias may affect policy changes of small scale, while less frequent bold reforms may be least shaped by electoral expectations.

The above results have a number of policy implications. First, there is support to the view that governments aiming at reforming the economy could in principle increase their re-election chances by an appropriate selection of reforms, by accompanying unpopular policy changes with reforms that appeal the electorate. Second, the evidence that reforms appear to be more costly politically when markets are more rigid suggests that there is a role for other countries' peer-pressure and support and for international organisations in overcoming a possible "rigidity trap". A supportive international environment could help governments with reformist intentions to break resistance from interest groups. Third, countries would gain from reduced electoral cost of reforms by adopting a balanced combination of fiscal and financial-based mechanism of insurance and income-

smoothing. These considerations may enter the assessment of the design of welfare state and financial sector reforms. Concerning the latter, the current financial crisis has been prompting strong calls for tighter regulation of financial markets. Our analysis suggests that forward-looking governments should not rush to over-regulate financial markets in order not to compromise the feasibility of reforms in product and labour markets and the associated long-term growth benefits.

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Figures

Figure 1. Re-election frequency over time
(frequency of legislative or executive elections resulting into re-election of the chief executive)

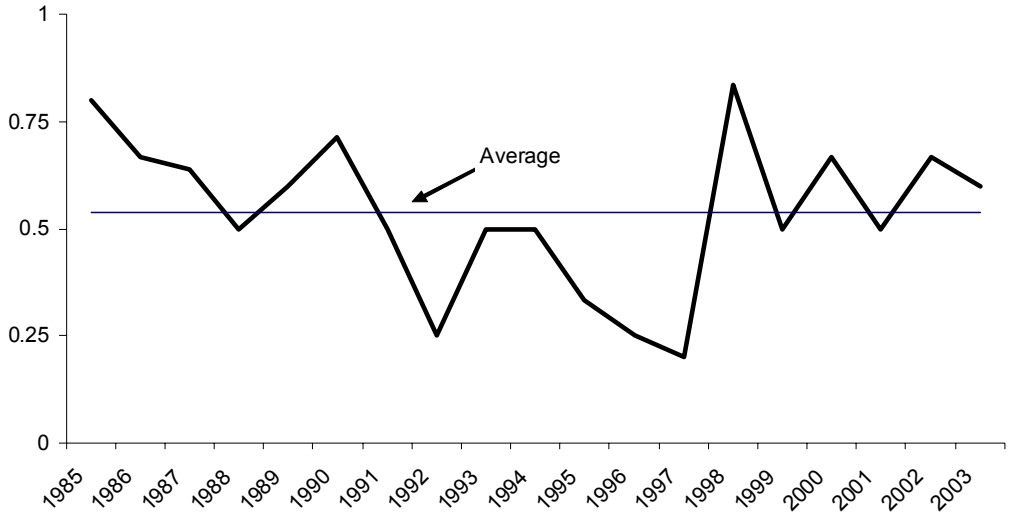


Figure 2. Election and re-election frequency across countries

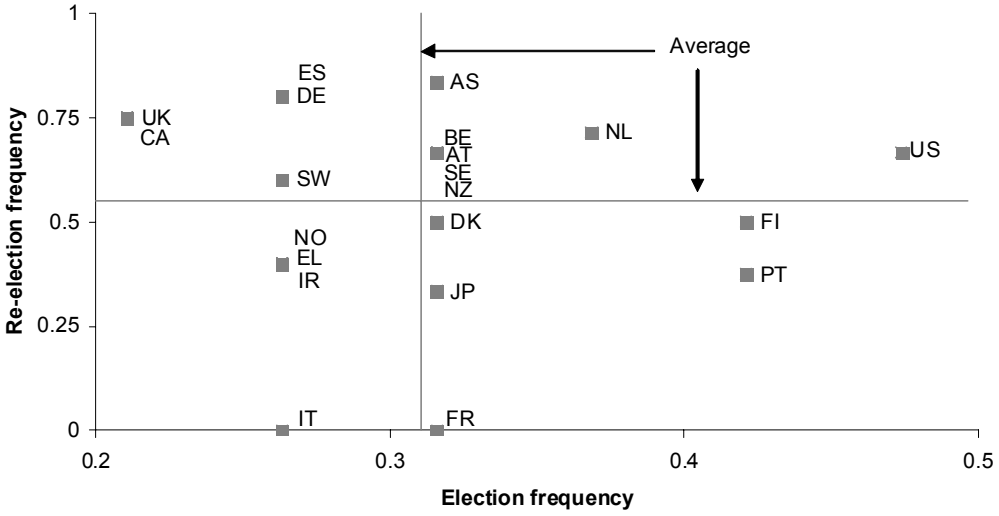


Figure 3. Market rigidity indexes
(standardised values)

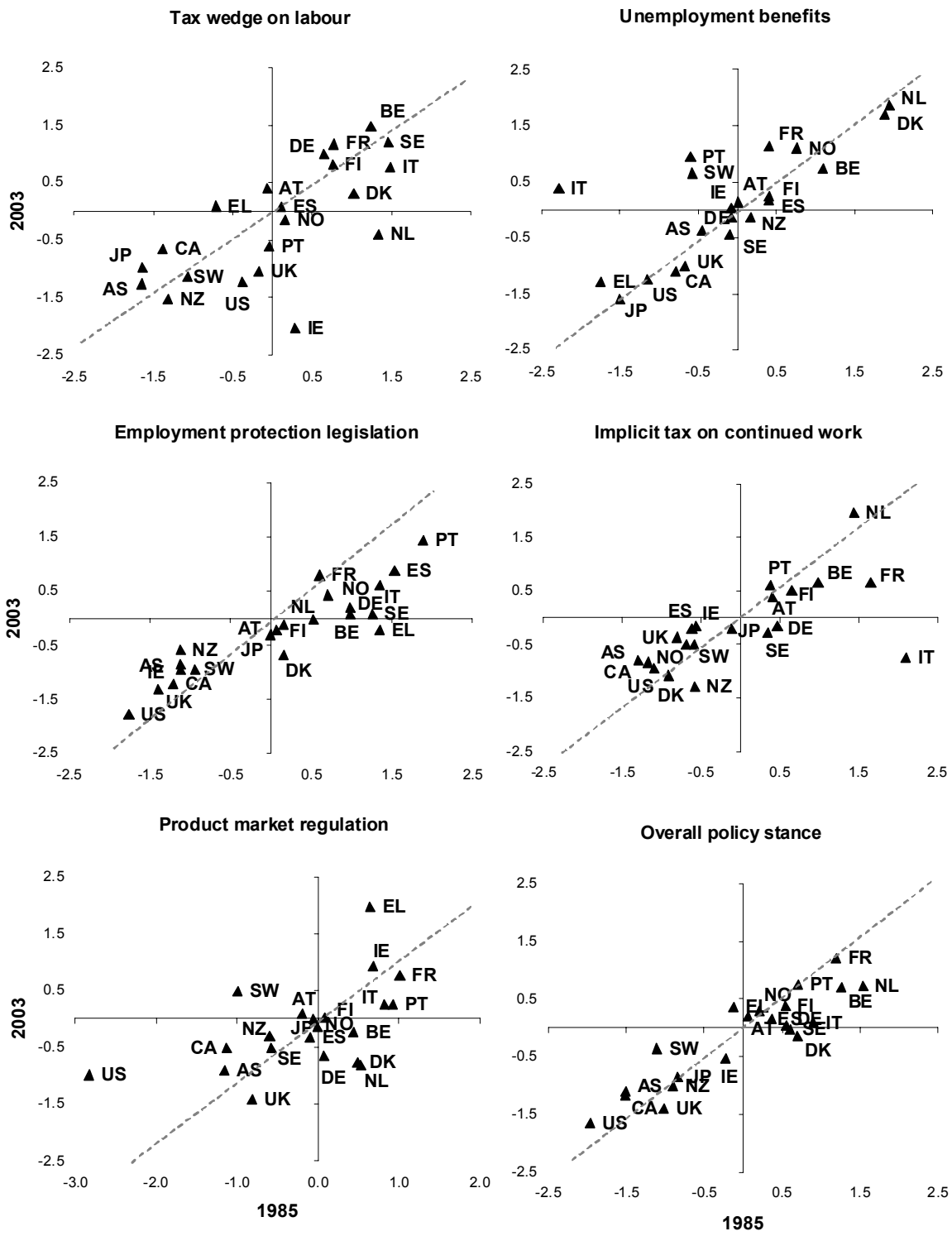


Figure 4. Market rigidity indexes
(standardised values, simple averages across country groups)

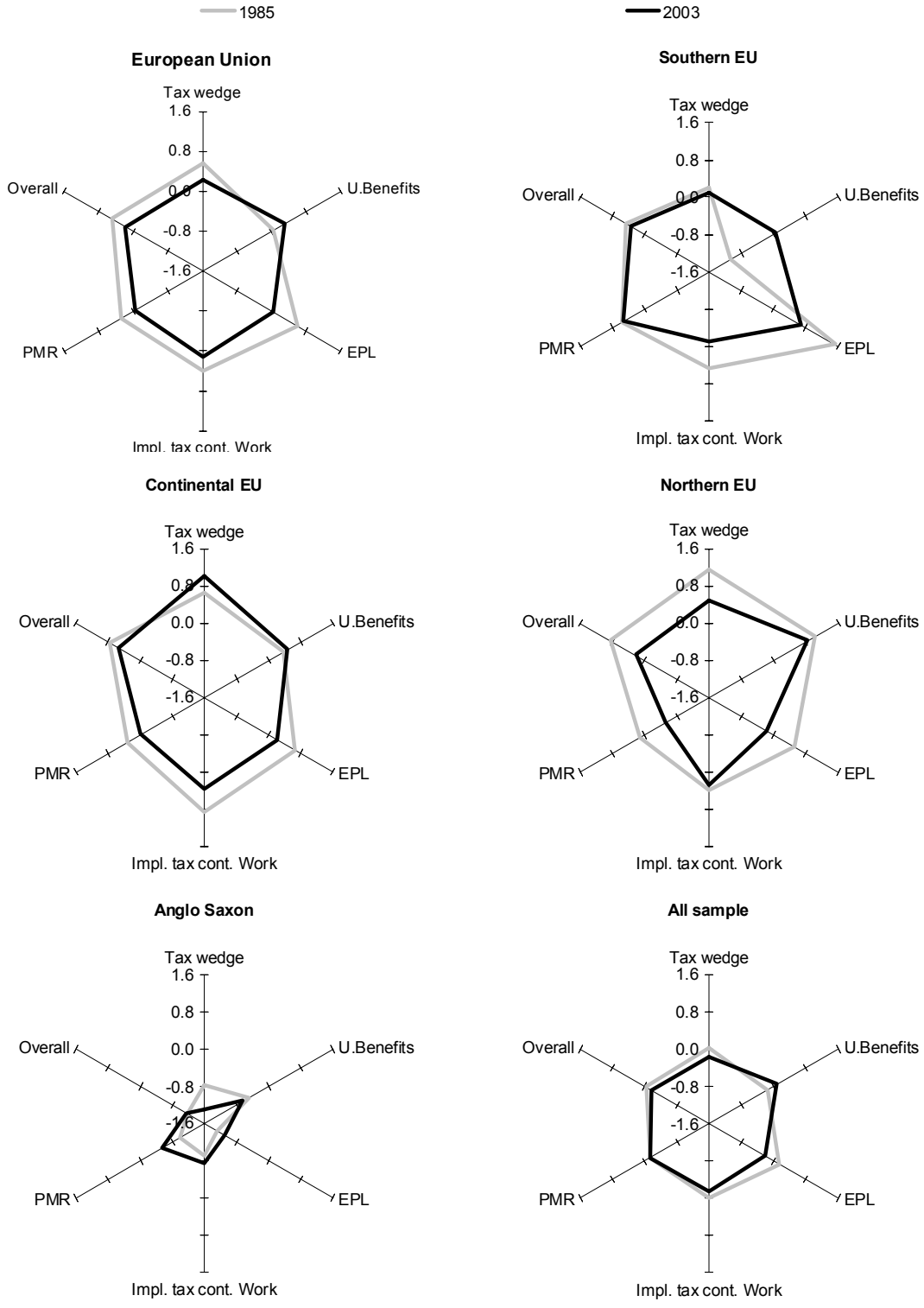


Figure 5. Frequency of reforms by country in various policy fields

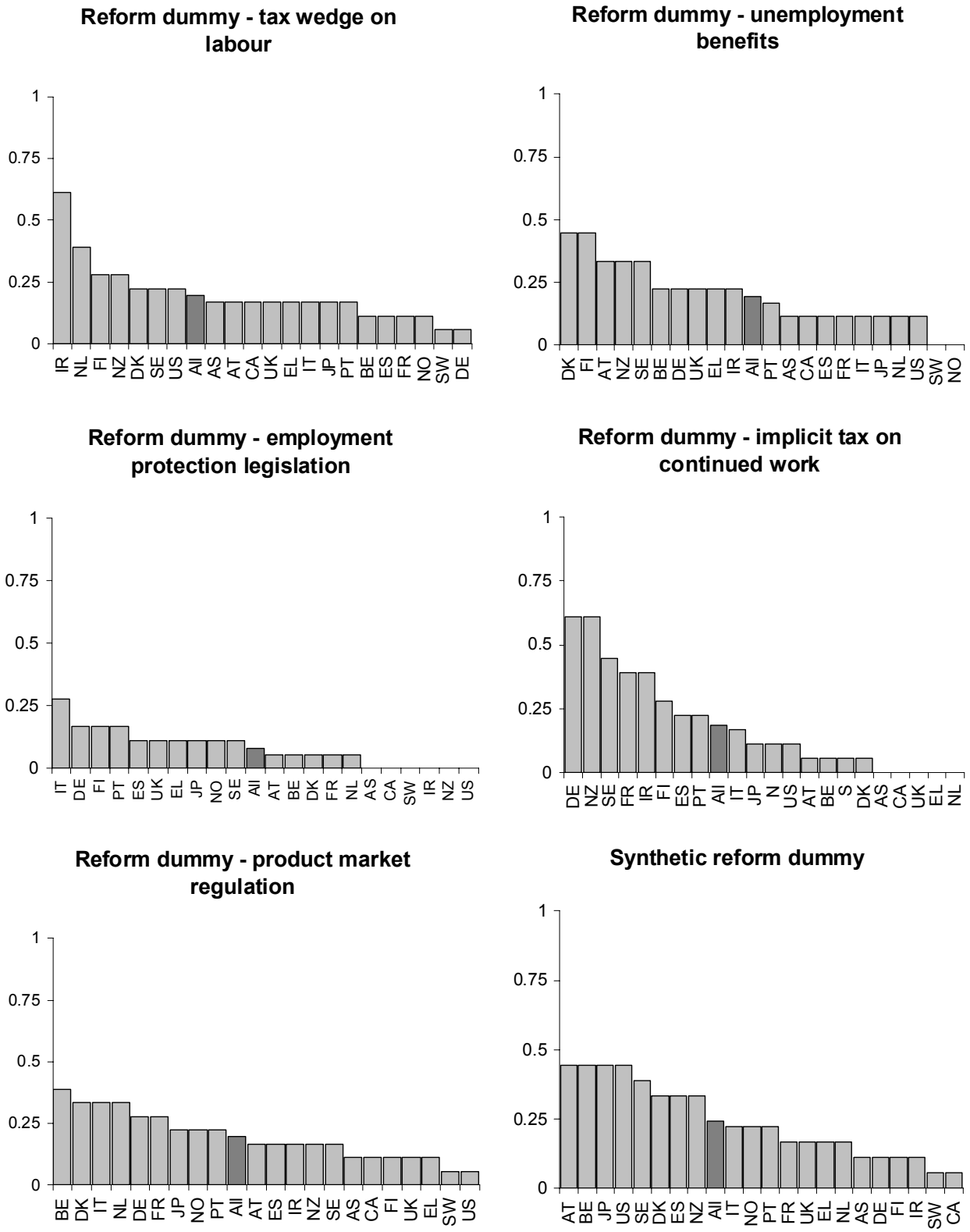
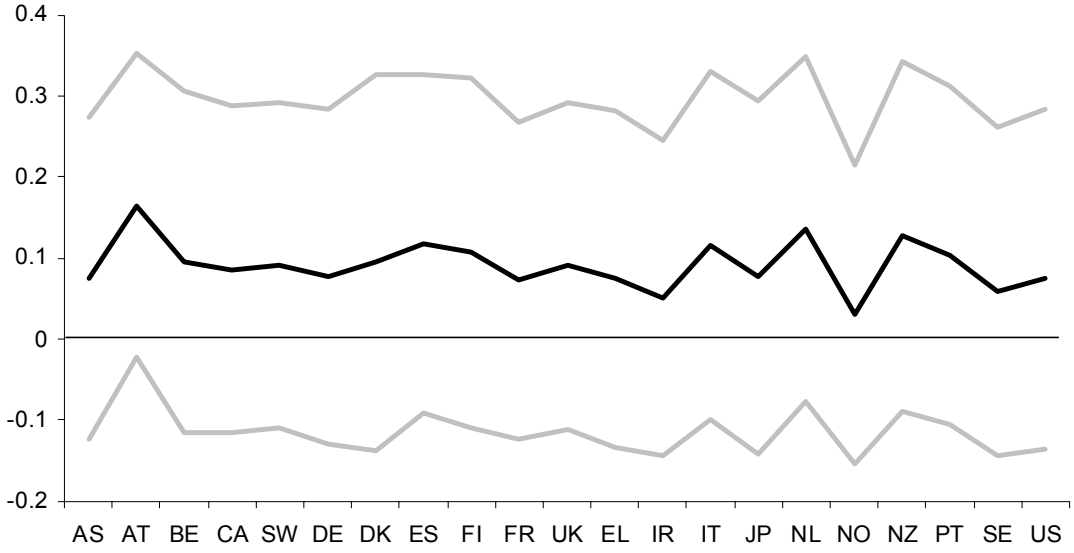


Figure 6. Marginal impact on re-election probability of reform dummy (recursive estimation excluding each country in turn; coefficient and 90% confidence band)



Tables

Table 1. Frequency of re-elections in the aftermath of reforms

All election sample with non missing reform data (118 obs.)	(1)	(2)	(3)
	Frequency not following reforms	Frequency following reforms	T test (1)≠(2), P value
Synthetic reform dummy	0.52	0.54	0.85
Tax wedge reform dummy	0.51	0.57	0.522
Unemployment benefits reform dummy	0.49	0.63	0.16
EPL reform dummy	0.56	0.30	0.08
Retirement schemes reform dummy	0.59	0.39	0.06
PMR reform dummy	0.55	0.5	0.62

Notes. Synthetic reform dummy defined as cases where in the current or previous year the change in the index of market rigidity is below the 20th percentile in at least one of five policy areas (unemployment benefit, labour taxes, EPL, product market regulations, retirement schemes), and neither in the current nor in the previous year a change above the 80th percentile takes place in any indicator. Source: Duval (2008). Reforms dummies in each policy area defined as 1 if year the change in the corresponding index of market rigidity is below the 20th percentile See Appendix.

Table 2. Reforms and re-elections under different conditions, frequency comparisons

(1)	(2)	(3)
Frequency of re-elections following reforms (31 obs.)		T test (1)≠(2), P value
Countries with flexible markets	Countries with rigid markets	
0.61	0.46	0.42
Countries with high financial freedom index	Countries with low financial freedom index	
0.61	0.5	0.54
Countries with low share of current primary government expenditure	Countries with high share of current primary government expenditure	
0.47	0.64	0.35
Expansionary fiscal stance	Restrictive fiscal stance	
0.5	0.63	0.48
Established chief executive	New chief executive	
0.41	0.71	0.09

Definition synthetic reform dummy: see Table 1 and Appendix.

Countries with flexible (rigid) markets: the time average of the overall index of market rigidity is below (above) the median value across countries. Source: Duval (2008). See Appendix.

Countries with high (low) markets: the time average of the index of financial freedom overall indicator of market rigidity is above (below) the median value across countries. Source: Fraser Institute (Gwartney et al., 2007). See Appendix.

Countries with low (high) shares of current primary government expenditure: the time average of the index of the share of current primary government expenditure on GDP is below (above) the median value across countries. Source: European Commission DG ECFIN AMECO database.

Expansionary vs. restrictive fiscal stance: the change in the primary cyclically-adjusted budget balance is negative (positive).

Established vs. new chief executive: the chief executive was (was not) confirmed following a previous election. Source: World Bank Database of Political Institutions (Beck et al., 2001). See Appendix.

Table 3. Are reformist governments likely to be re-elected? Baseline specification

Dependent variable: 1 if chief executive is re-elected	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probit (Marginal effects)		Logit	Logit (marginal effects)	Conditional Logit	OLS fixed effects		Probit fixed effects
Reform dummy	0.117	0.091	0.375	0.093	0.377	0.044	0.037	0.110
	[0.90]	[0.75]	[0.71]	[0.72]	[0.62]	[0.28]	[0.30]	[0.61]
Cyclical Conditions	0.043	0.034	0.141	0.035	0.122	0.034	0.027	0.036
	[2.13]**	[1.76]*	[1.70]*	[1.71]*	[1.24]	[1.23]	[1.39]	[1.39]
Change in cyclical conditions	0.078	0.075	0.317	0.079	0.369	0.079	0.069	0.108
	[2.49]**	[2.13]**	[2.03]**	[2.04]**	[1.86]*	[1.62]	[1.90]*	[2.14]**
Change in Inflation	-0.008							
	[0.78]							
Change in primary CAB	0.087							
	[1.45]							
Parliamentary system dummy	-0.400	-0.385	-1.552	-0.387				-0.299
	[3.49]***	[3.52]***	[3.57]***	[3.53]***				[1.88]*
Proportional representation dummy	0.382	0.327	1.406	0.330	0.910	0.156	0.206	0.280
	[1.76]*	[1.56]	[1.60]	[1.84]*	[0.41]	[0.32]	[0.48]	[1.18]
Margin of majority	0.827	0.896	3.668	0.915	4.880	1.127	0.814	1.263
	[1.55]	[1.66]*	[1.65]*	[1.65]*	[1.18]	[1.49]	[1.19]	[1.04]
Maximum polarisation among parties in the legislature between executive party and the four principal parties of the legislature.	-0.134	-0.118	-0.480	-0.120	-0.752	-0.037	-0.106	-0.210
	[1.74]*	[1.47]	[1.42]	[1.43]	[1.62]	[0.43]	[1.41]	[1.68]*
Percentage of veto players leaving the government	-1.283	-1.188	-4.910	-1.225	-3.673	-0.712	-0.595	-1.065
	[3.36]***	[3.32]***	[3.11]***	[3.08]***	[1.89]*	[2.02]**	[2.05]**	[2.62]***
Years of democratic history	0.005	0.004	0.015	0.004	0.017	0.017	0.002	0.002
	[1.54]	[1.21]	[1.17]	[1.16]	[0.32]	[0.79]	[0.20]	[0.20]
Constant			0.084			-0.648	0.010	
			[0.05]			[0.52]	[0.01]	
Country fixed effects						x	x	x
Year fixed effects						x		
Observations	105	106	106	106	95	106	106	106
Pseudo R-Squared	0.19	0.17	0.17	0.17	0.20	0.27	0.16	0.26
Number of cluster							20	20
Wald test for joint significance political controls (P-value)	0.000							
Wlad test for joint significance of country effects (P-value)						0.89		
Wald test for joint significance of year effects (P-value)						0.45	0.33	
Hausman test[1]					0.2528			
Generalised Hausman test for marginal effects [2]					0.9871			

Notes: coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets (absolute values, standard errors are robust for heteroschedasticity and residual correlation within countries). *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level.
Dependent variable: See Appendix. Source: "Database of Political Institutions", Beck et al. (2001).
Reform dummy: 1 if in the current or previous year the change in the structural indicator of market rigidity (source Duval, 2008) is below the 20th percentile in at least one of five policy areas (unemployment benefit, labour taxes, EPL, product market regulations, retirement schemes) and neither in the current or previous year the change in the market rigidity indicator is above the 80th percentile in any area. See Appendix. Macroeconomic explanatory variables: averages over current and previous year. Source: OECD Economic Outlook. Political controls. Source: World Bank Database of Political Institution, Beck et al. (2001). See Appendix.
[1]Hausman test. Ho: difference in the logit and the fixed effects logit coefficients is not systematic. The sample is kept equal in the specification with and without fixed effects. [2]Generalised Hausman test for marginal effects. Non linear test performed on estimates of coefficients and their robust covariance matrix. Ho: difference in the ratio of any pair of logit and fixed effects logit coefficients is not systematic. The sample is kept equal in the specification with and without fixed effects.

Table 4: Are reformist governments likely to be re-elected?
Alternative construction methods of the reform variable

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Non linearity in reform variable			
Dependent variable: 1 if chief executive is re-elected	Overall index of market rigidity (reform dummy)	Overall index of market rigidity (symmetric reform dummy)	Overall index of market rigidity (level)	Overall index of market rigidity (change)	Overall index of market rigidity (level)	Overall index of market rigidity (change)	Sum of reform dummies in single areas	Counter-reform dummy
Reform variable	-0.052	-0.008	-0.085	-0.132	-0.087	-1.102	0.034	-0.036
	[0.51]	[0.10]	[1.09]	[0.15]	[1.14]	[0.86]	[0.72]	[0.28]
Reform variable squared					0.014	-18.757		
					[0.17]	[1.40]		
Country fixed effects	**No**	**No**	**No**	**No**	**No**	**No**	**No**	**No**
Observations	106	106	106	100	106	100	106	106
Pseudo R-Squared	0.17	0.17	0.18	0.18	0.18	0.2	0.17	0.17
Reform variable	-0.095	-0.093	0.483	1.629	0.672	0.915	0.050	-0.083
	[0.68]	[0.95]	[1.38]	[1.18]	[1.17]	[0.59]	[0.72]	[0.44]
Reform variable squared					-0.178	-12.758		
					[0.64]	[0.68]		
Country fixed effects	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Observations	101	101	101	95	101	95	101	101
Pseudo R-Squared	0.32	0.32	0.33	0.36	0.33	0.37	0.32	0.32

Notes: Specification: column (2) in Table 3, except reform variable. Probit estimation, standard errors robust for heteroschedasticity and residual correlation within countries. Coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets. *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level.
Variable definitions: See Table 4, except reform dummies which are defined as follows:
Overall index of market rigidity (reform dummy): 1 if in the current or previous year the change in the overall structural indicator is below the 20th percentile.
Overall index of market rigidity (symmetric reform dummy): 1 if in the current or previous year the change in the overall structural indicator is below the 20th percentile; -1 if if in the current or previous year the change in the overall structural indicator is above the 80th percentile; 0 otherwise.
Overall index of market rigidity (level): average of current and preceding level of overall index of market rigidity.
Overall index of market rigidity (change): average of current and preceding year-on-year change in overall index of market rigidity.
Sum of reforms in single areas: sum of reform dummies in each policy area based on 20th percentile.
Counter-reform dummy: 1 if in the current or previous year in at least one policy area the change in the structural indicator is bigger than the 80th percentile.

Table 5: Are reformist governments likely to be re-elected? Alternative definitions of re-election variable and assumptions on voters' memory

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	1 if chief executive or his party is re-elected	1 if chief executives is same as previous year (irrespective of elections)	1 if chief executive is re-eelected; Myopic voters (1 year horizon)	1 if chief executive is re-eelected; longer memory voters (3 years horizon)	1 if chief executive is re-eelected; Long-memory voters (up to whole term, provided that chief executive is still in office)
Reform dummy	-0.037	0.016	-0.084	0.148	0.197
	[0.33]	[0.22]	[0.66]	[1.16]	[1.15]
Change in cyclical conditions	0.060	0.017	0.049	0.016	0.003
	[1.62]	[1.65]*	[2.27]**	[0.88]	[0.18]
Cyclical Conditions	0.023	0.015	0.029	0.128	0.155
	[1.52]	[1.59]	[1.23]	[2.92]***	[2.74]***
Political controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	No	No	No
Observations	106	341	107	106	101
Pseudo R-Squared	0.11	0.04	0.17	0.18	0.18
Reform dummy	-0.07	-0.003	0.022	0.171	0.175
	[0.56]	[0.03]	[0.10]	[0.86]	[0.56]
Change in cyclical conditions	0.063	0.023	0.038	0.178	0.2
	[1.59]	[2.39]**	[1.15]	[2.39]**	[2.06]**
Cyclical Conditions	0.019	0.013	0.052	0.013	-0.001
	[0.85]	[1.53]	[1.72]*	[0.61]	[0.06]
Political controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	106	341	102	101	96
Pseudo R-Squared	0.2	0.2	0.3	0.33	0.33

Notes: Probit estimation, standard errors robust for heteroschedasticity and residual correlation within countries. Coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets. *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level. Variable definitions: see Table 3, except

Cyclical conditions and change in cyclical conditions: value in election year (column (3)); average of value in election year and previous 2 years (column (4)); average over the term in office (column (5)).

Reform dummy: 1 if in the current year the change in the structural indicator of market rigidity (source Duval, 2008) is below the 20th percentile in at least one of five policy areas (unemployment benefit, labour taxes, EPL, product market regulations, retirement schemes) and the change in the market rigidity indicator is above the 80th percentile in any area (column (3)), 1 if in the current or previous 2 years the change in the structural indicator of market rigidity (source Duval, 2008) is below the 20th percentile in at least one of five policy areas (unemployment benefit, labour taxes, EPL, product market regulations, retirement schemes) and in no year the change in the market rigidity indicator is above the 80th percentile in any area (column (4)); 1 if in year during the term in office the change in the structural indicator of market rigidity (source Duval, 2008) is below the 20th percentile in at least one of five policy areas (unemployment benefit, labour taxes, EPL, product market regulations, retirement schemes) and neither in no year the change in the market rigidity indicator is above the 80th percentile in any area.

Table 6: Are reformist governments likely to be re-elected? EU countries after 1992

Dependent variable: 1 if chief executive is re-elected	(1)	(2)	(3)	(4)
	"Maastricht / Single Market sample"			
	Reform dummy (baseline)	Reform dummy (baseline)	Overall index of market rigidity (change)	Overall index of market rigidity (change)
Reform dummy	-0.220	-0.305	3.092	5.031
	[1.16]	[1.08]	[2.01]**	[1.36]
Cyclical Conditions	0.110	0.141	0.137	0.204
	[2.43]**	[2.53]**	[4.19]***	[2.60]***
Change in cyclical conditions	-0.017	0.011	-0.007	0.015
	[0.26]	[0.10]	[0.10]	[0.16]
Country fixed effects		Yes		Yes
Observations	43	38	43	38
Pseudo R-Squared	0.23	0.40	0.27	0.48
Wald test of exogeneity (P value)				

Notes: Specification: column (2) in Table 3, except reform variable. Probit estimation, standard errors robust for heteroschedasticity and residual correlation within countries. Coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets. *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level. The estimate in column (5) is Instrumental Variable Probit. The non-excluded instruments for the reform variable are those in Table 6; fixed effects included.

Table 7: Impact of reforms in different policy areas

Dependent variable: 1 if chief executive is re-elected	(1)	(2)	(3)	(4)	(5)	(6)
	Dummy Variable					
	Tax wedge	Unemployment Benefits	EPL	Implicit tax on continued work	PMR	Encompassing Specification
Reform in the tax wedge	0.171					0.138
	[2.08]**					[1.49]
Reform in unemployment benefits		0.207				0.295
		[2.28]**				[2.58]***
Reform in the EPL			-0.257			-0.24
			[1.52]			[1.09]
Reform in the implicit tax on continued work				-0.193		-0.184
				[2.50]**		[2.85]***
Reform in the PMR					0.004	-0.034
					[0.03]	[0.27]
Cyclical Conditions	0.072	0.069	0.072	0.092	0.072	0.098
	[2.10]**	[2.03]**	[2.04]**	[2.76]***	[2.17]**	[2.36]**
Change in cyclical conditions	0.031	0.039	0.039	0.027	0.036	0.033
	[1.54]	[2.02]**	[2.23]**	[1.24]	[1.83]*	[1.59]
Observations	106	106	106	102	106	102
Pseudo R-Squared	0.18	0.19	0.19	0.2	0.17	0.27

Notes: Specification: column (2) in Table 3, except reform variable. Probit estimation, standard errors robust for heteroschedasticity and residual correlation within countries. Coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets. *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level.
Variable definitions: See Table 3, except reform dummies which are defined as 1 if in the current or previous year the change in the market rigidity index (source Duval (2008)) is below the 20th percentile.

Table 8: Impact of reforms in different policy areas: alternative variable definitions and specifications

Dependent variable: 1 if chief executive is re-elected		(1) Excluding country fixed effects	(2) Including country fixed effects
Reform dummy (different areas)	Tax wedge	0.171	0.187
		[2.08]**	[1.28]
	Unemployment Benefits	0.207	0.268
		[2.28]**	[1.90]*
	EPL	-0.257	-0.505
		[1.52]	[1.57]
	Implicit tax on continued work	-0.193	-0.452
		[2.50]**	[1.95]*
	PMR	0.004	0.066
		[0.03]	[0.43]
Change in the index of market rigidity (different areas)	Tax wedge	-0.343	0.353
		[0.99]	[0.53]
	Unemployment Benefits	-0.430	0.370
		[0.68]	[0.58]
	EPL	1.475	2.841
		[1.69]*	[1.40]
	Implicit tax on continued work	2.463	4.412
		[1.66]*	[1.92]*
	PMR	-0.098	0.042
		[0.24]	[0.07]

Notes: Specification: column (2) in Table 3, except reform variable. Probit estimation, standard errors robust for heteroschedasticity and residual correlation within countries. Coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets. *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level. Variable definition: see Table 3 and 8.

Table 9: The role of the policy environment

Dependent variable: 1 if chief executive is re-elected	(1)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline synthetic reform dummy						Change in overall market rigidity index
Reform variable	0.137	0.168	0.139	0.124	0.176	0.171	-1.182
	[1.13]	[1.40]	[0.94]	[1.02]	[1.24]	[1.07]	[1.02]
Change in cyclical conditions	0.070	0.073	0.092	0.074	0.088	0.088	0.054
	[2.04]**	[2.06]**	[2.28]**	[2.08]**	[2.27]**	[1.83]*	[1.67]*
Cyclical Conditions	0.032	0.034	0.031	0.029	0.033	0.033	0.033
	[1.60]	[1.92]*	[1.62]	[1.58]	[1.64]	[1.35]	[1.58]
Market rigidity *reform variable	-0.245				-0.347	-0.651	2.998
	[1.42]				[2.03]**	[2.02]**	[2.77]***
Change in primary CAB*reform variable		0.100					
		[0.99]					
Financial freedom index*reform variable			0.391		0.440	0.295	1.320
			[2.57]**		[2.08]**	[1.17]	[1.41]
(Current Primary Government Expenditure / GDP)*reform variable				0.018			
				[0.21]			
Country Fixed effects	No	No	No	No	No	Yes	No
Political Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	103	103	103	103	103	92	97
Pseudo R-squared	0.19	0.18	0.22	0.18	0.23	0.32	0.21
Joint significance of fixed effects (P value)						0.43	

Notes: Probit estimation, standard errors robust for heteroschedasticity and residual correlation within countries. Coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets. *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level. Variable definitions, see Table 3. Interaction variables: market rigidity, overall index of market rigidity (see Appendix); cyclical conditions, source OECD Economic Outlook; Financial freedom index, source Fraser Institute (Gwartney et al., 2007); change in primary cyclically-adjusted balance (CAB) and current primary government expenditure/GDP, source European Commission DG ECFIN AMECO database. All interaction variables are standardised in such a way to have zero mean and unit standard deviation over the sample of non-missing observations for the baseline specification with interactions (column 6, Table 10).

Table 10: The role of the policy environment: impact of reforms in different policy areas

Dependent variable: 1 if chief executive is re-elected	(1)	(2)	(3)	(4)	(5)
	Tax wedge	Unemployment benefits	EPL	Implicit tax on continued work	PMR
Reform dummy (different areas)	0.217	0.234	-0.225	-0.135	0.013
	[2.65]***	[1.84]*	[1.35]	[1.46]	[0.11]
Market rigidity *reform variable	-0.313	0.009	0.114	-0.082	-0.081
	[3.80]***	[0.09]	[0.46]	[1.12]	[0.79]
Financial freedom index*reform variable	0.373	0.272	0.087	0.386	0.173
	[3.92]***	[2.80]***	[0.65]	[3.08]***	[1.76]*
Observations	103	103	103	99	103
Pseudo R-squared	0.26	0.23	0.19	0.23	0.19
Reform dummy (different areas), country fixed effects	0.385	0.288	-0.574	0.085	0.006
	[3.64]***	[1.67]*	[3.40]***	[0.49]	[0.04]
Market rigidity *reform variable	-0.680	0.234	0.924	-0.080	-0.305
	[3.79]***	[1.78]*	[2.87]***	[0.35]	[1.12]
Financial freedom index*reform variable	0.328	0.179	-0.260	-0.091	-0.180
	[1.77]*	[1.28]	[1.80]*	[0.56]	[1.02]
Observations	98	98	98	94	98
Pseudo R-squared	0.38	0.37	0.36	0.38	0.32
Change in the index of market rigidity (different areas)	-0.812	-0.309	0.765	1.024	-0.398
	[2.06]**	[0.37]	[1.76]*	[0.62]	[0.80]
Market rigidity *reform variable	0.640	-0.213	-0.086	4.524	0.695
	[1.46]	[0.20]	[0.04]	[2.21]**	[1.49]
Financial freedom index*reform variable	0.439	0.069	-1.662	0.399	0.531
	[0.74]	[0.12]	[1.21]	[0.33]	[1.04]
Observations	97	97	97	97	93
Pseudo R-squared	0.2	0.18	0.21	0.21	0.23
Change in the index of market rigidity (different areas), country fixed effects	-0.458	0.344	4.017	2.943	-0.512
	[0.37]	[0.37]	[2.25]**	[1.17]	[0.66]
Market rigidity *reform variable	1.563	0.001	4.854	9.307	0.512
	[1.20]	[0.00]	[1.41]	[1.89]*	[0.84]
Financial freedom index*reform variable	0.596	0.164	0.576	-0.341	0.807
	[0.56]	[0.24]	[0.28]	[0.18]	[1.05]
Observations	92	92	92	88	92
Pseudo R-squared	0.38	0.36	0.4	0.43	0.37

Notes: Specification: column (6) in Table 10, except for reform variable. Probit estimation, standard errors robust for heteroschedasticity and residual correlation within countries. Coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets. *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level.
Variable definitions: see Table 7. The sample split dummy takes value 1 only for the observations for the countries and/or time period considered. All interaction variables are standardised in such a way to have zero mean and unit standard deviation over the sample of non-missing observations for the baseline specification with interactions (column 7, Table 7).

Table 11: The role of incumbent-specific factors

Dependent variable: 1 if chief executive is re-elected		(1) New chief executive	(2) Learning from previous incumbents	(3) Colour of chief Executive	(4) Distance from previous chief
Reform dummy		-0.072	0.024	0.121	0.112
		[0.35]	[0.15]	[1.00]	[0.83]
Reform dummy *	New Chief Executive	0.308			
		[1.20]			
	Previous chief executive not re-elected and not reformist		0.193		
			[0.65]		
	Colour of chief Executive			0.08	
				[0.58]	
	Political Distance from the previous Chief executive				0.108
					[0.97]
Political controls		Yes	Yes	Yes	Yes
Country fixed effects		No	No	No	No
Observations		106	106	106	106
Pseudo R-squared		0.19	0.18	0.17	0.18
Reform dummy		-0.155	-0.059	0.231	0.136
		[0.54]	[0.24]	[1.03]	[0.68]
Reform dummy *	New Chief Executive	0.528			
		[1.65]*			
	Previous chief executive not re-elected and not reformist		0.501		
			[1.34]		
	Colour of chief Executive			0.29	
				[1.37]	
	Political Distance from the previous Chief executive				0.081
					[0.67]
Political controls		Yes	Yes	Yes	Yes
Country fixed effects		Yes	Yes	Yes	Yes
Observations		103	103	103	103
Pseudo R-squared		0.27	0.25	0.24	0.23

Notes: Probit estimation, standard errors robust for heteroschedasticity and residual correlation within countries. Coefficients are marginal probability effects computed at sample mean. Robust z statistics in brackets. *, **, and *** denote, respectively, significant at 10; 5, and 1 per cent level. Variable definitions in Table 3 and 10, except: "new chief executive" (1 if current incumbent is serving his/her first term) and "political distance from previous chief executive" (difference between current incumbent's colour and previous chief executive's colour, Right=1, Centre=0, Left=-1), source World Bank Database of Political Institution, Beck et al. (2001).

APPENDIX. VARIABLE AND DATA SOURCES

Re-election variables

All re-election variables have been constructed using data on elections and on information on the chief executive, its party and the main party in the parliament obtained from the World Bank Database on Political Institutions (DPI, Beck et al., 2001), version 2004, taking into account of subsequent checks and corrections provided in the DPI website. They are dichotomic variables focusing on the tenure of a specific political force and its ability to be reconfirmed after an election. The baseline re-election variable is defined as follows:

- 1 if an election (either Parliamentary or presidential) takes place in year *t* and country *i* and the same government's chief executive that was in power in year *t* is also in power in year *t*+1.
- 0 if election takes place in year *t* and country *i* and the government's chief executive that was in power in year *t* is not anymore in power in year *t*+1.
- Missing if no election takes place.

Cases where re-appointment of the government chief executive are prevented by law are dropped. In our sample, these are US elections in 1988 and in 2000, where the President already terminated the second mandate.

An alternative re-election variable, including also the cases in which the identity of the government chief executive changes, is defined as follows:

- 1 if an election (either Parliamentary or presidential) takes place in year *t* and country *i* and the same government's chief executive that was in power in year *t* is also in power in year *t*+1 or if the new chief executive officer is different but belonging to the same party as that of the government's chief executive that was in power in year *t*.
- 0 if election takes place in year *t* and country *i* and the government's chief executive in power in year *t*+1 belongs to a party that is different than that of the government's chief executive that was in power in year *t*.
- Missing if no election takes place

Finally, we analyse the determinants of governments' chief executives' tenure irrespective of the occurrence of election. In this case, the variable is defined as follows:

- 1 if in year *t* and country *i* the same government's chief executive that was in power in year *t* is also in power in year *t*+1.
- 0 if in year *t* and country *i* the government's chief executive that was in power in year *t* is not anymore in power in year *t*+1.

Reform variables

The data source for our baseline reform variable is Duval (2008). It considers 5 policy areas: labour taxes, unemployment benefit system, employment protection legislation, retirement schemes, product market regulations. For each policy area, an *index of market "rigidity"* measuring the anti-competitive / distortionary effects of policies / regulations was constructed in the following way:

- Tax wedge on labour: average of wedge between labour cost and take-home pay across two situations (a single worker and a couple with a dependent spouse and two children, at average earnings levels in both cases). Source: Taxing Wages, OECD, Paris, various issues;
- Unemployment benefits: summary measure of benefit replacement rates (an average of replacement rates across various earnings levels, family situations and durations of unemployment). Source: Benefits and Wages, OECD, Paris, various issues;
- Employment protection legislation (EPL): summary index of EPL. Source: Employment Outlook, OECD, Paris, various issues;
- Retirement schemes: average of implicit tax rates on continued work – which sum up deviations from actuarial neutrality – in old-age pension systems and early retirement schemes across thee situations (at ages 55 and 60 in early retirement schemes, and at age 60 in old-age pension schemes, for a single worker with average earnings in all three cases). Source: Duval (2003).
- Product market regulation: summary index of product market regulation in seven non-manufacturing industries. Source: Conway and Nicoletti (2006).

Information on the above market rigidity indexes is available for all OECD countries included in the dataset for the period 1985-2003, with the exception of data for retirement schemes in Greece (not available). All indexes were standardized in such a way to have zero mean and unit standard deviation across the whole available sample. Moreover, since the PMR index exhibits a trend in most countries and sample sub-periods, it was de-trended before being standardized.

In addition to the indexes of market rigidity in each policy area, an *overall index of rigidity* was composed as the sum of the five indicators for each area.

A reform is interpreted as a policy change that produces a significant reduction in the degree of anti-competitive / distortionary policies / regulations. The baseline reform index adopts the 20th percentile as the reference threshold. Formally,

$$R_{i,t}^{p} = 1 \quad \text{if} \quad \Delta I_{i,t}^{p} < pc_{i,t}^{p,20}$$
$$R_{i,t}^{p} = 0 \quad \text{otherwise}$$

where $R_{i,t}^{p}$ is a dummy variable denoting the presence of reforms in policy area p, in country i, in year t, $\Delta I_{i,t}^{p}$ is the year-on-year change in the index of market rigidity in policy area p, and $pc_{i,t}^{p,20}$ is the 20th percentile of the distribution of the year-on-year change in the index of market rigidity in policy area p. The actual frequency of reforms in each policy area is 20 per cent in all cases, except EPL, where reforms take place in only in 7.7 per cent of the observations, due to the fact that the distribution of the change in the corresponding index of market rigidity is

concentrated over zero, so that the 20th percentile is zero as well, while only 7.7 of the observations are strictly negative (i.e., any reduction in EPL corresponds to a reform). For all other policy areas, the 20th percentile is a negative number, so that reforms select only sufficiently big reductions in the degree of market rigidity.

Table A1 reports the correlation coefficient among market rigidity indexes and the baseline reform dummies.

Indexes capturing the occurrence of "counter-reforms", i.e., policy changes rising the extent of market rigidity are also constructed in a symmetrical fashion. In this case, the index $\widetilde{R}_{i,t}^{p}$ equals to 1 when the year on year change in the market rigidity index in any of the 5 policy areas is above the 80th percentile.

The baseline synthetic reform variable to analyse the impact of reforms on re-elections is constructed as a dummy taking value 1 whenever a reform takes place, in the current or in the preceding year, in at least one of the 5 policy areas and no counter-reform takes place neither in the current nor in the preceding year. Formally, $R_{i,t}=1$ if there exists at least one p such that either $R_{i,t}^{p}=1$ or $R_{i,t-1}^{p}=1$ and $\widetilde{R}_{i,t}^{p}=0$, $\widetilde{R}_{i,t-1}^{p}=0$ for any p. According to this baseline synthetic reform index, in about 24 per cent of the cases, governments have been taking a reformist stance, i.e, have engaged with reform efforts in at least one policy area without relaxing the structural stance in any field.

Alternative reform dummies were constructed as follows:

- in analogy with the baseline index, but using the 10th and the 30th percentile to identify reforms and anti-reforms. The objective is to obtain indexes isolating only "bolder" reforms or also less "ambitious" reforms, respectively;
- based on a sufficiently big positive change in the overall index of rigidity in the current or preceding year. This alternative permits to assess the average reform stance in all policy areas, since it takes into account the possibility that pro-competitive policy changes in one area are accompanied by developments in the opposite sense in other areas. The index was constructed by referring to the 20th percentile as a threshold;
- the average of the year-on-year change in the overall index of market rigidity. The indicator is in this case symmetric, so it permits to assess the impact not only of policies reducing market rigidity but also those raising it.
- A dummy taking value 1, 0, -1 if the change in the overall market rigidity index is, either in the current or preceding year, respectively below the 20th percentile, in between the 20th and the 80th percentile, above the 80th percentile. The index so constructed is symmetric but discrete, it disregard small changes in the market rigidity index and focuses in "bold" reforms and counter-reforms.
- a reform index consisting of the sum of the reform dummies in each of the 5 policy areas. It ranges from 0 to 5 and permits to discriminate between cases in which reforms take place simultaneously in more than one policy domain.
- a synthetic counter-reform dummy, constructed in a symmetrical fashion compared with the baseline reform dummy. It is aimed at capturing whether an overall "counter-reformist" stance by the government has an impact on the re-election probability.

Table A.1 reports the correlation coefficient among market rigidity indexes and reform dummies. Table A.2 reports the data on re-elections and reform events, as defined by our baseline synthetic reform index, in election years.

Macroeconomic Variables

Cyclical conditions are represented by the output gap while the fiscal stance net of the economic slack is captured by the cyclically adjusted primary balance, both estimated by the OECD Economic Outlook of June 2007. From the same source is the inflation, calculated as the percentage change of CPI.

Political Variables

All the variables used in the regression as political controls are taken from the World Bank Database of Political Institution (Beck et al., 2001).

System Dummy

Parliamentary (1), Presidential (0)

Systems with presidents who are elected directly or by an electoral college, in cases where there is no prime minister, receive a 0. In systems with both a prime minister and a president, the following factors are used to categorize the system:

a) president can veto legislation and the parliament needs a supermajority to override the veto.

b) president can appoint and dismiss prime minister and / or other ministers.

c) president can dissolve parliament and call for new elections.

Countries in which the legislature elects the chief executive are parliamentary, except in the case in which it cannot easily recall him (if they need a 2/3 vote to impeach, or must dissolve themselves while forcing him out).

Proportional Representation dummy

"1" if candidates are elected on the basis of the percent of votes received by their party. "0" otherwise.

Margin of Majority

Fraction of seats held by the government. It is calculated by dividing the number of government seats by total (government plus opposition plus non-aligned) seats.

Maximum polarization between the executive party and the four principle parties of the legislature

It is zero if the chief executive's party has an absolute majority in the legislature. Otherwise it is the maximum difference between the chief executive's party's value in a left-right political scale and the values of the three largest government parties and the largest opposition party.

Percentage of veto players leaving the government

This counts the percent of veto players who drop from the government in any given year. In presidential systems, veto players are defined as the president and the largest party in the legislature. In parliamentary systems, the veto players are defined as the prime minister and the three largest government parties. The larger the number of veto players leaving the government in a given year and country, the lower the degree of control exercised by the government on the legislative or the executive power.

Years of democratic history

This variable records how long parties and prime ministers have been competitively elected.

Colour of the chief Executive

Party orientation with respect to the extent of state control of the economy (right=in favour of less control), identified primarily on the basis of their name, and counterchecked on the basis of information from various sources. In case of evidence that the chief executive deviated considerably from the party orientation, the orientation of the ruling party is replaced by the orientation of the chief executive.

Financial Variables

To capture the degree of anti-competitive regulations in financial markets we use the indicators of financial freedom made available by the Fraser Institute.[21] The indicator measures, on an inverse scale, with scores ranging from 0 to 10, the degree of anti-competitive regulations in four areas:

- bank ownership (the score raises the larger the share of privately held deposits);
- foreign bank competition (the score rises with the rate of approval of foreign bank applications for licenses to open subsidiaries and branches with the share of the banking sector assets owned by foreigners);
- extension of private sector credit (percentage of domestic credit consumed by the private sector),
- interest rate controls (the score rises with interest rates determined by the market, stable monetary policy, and positive real deposit and lending rates received).

As alternative measures of financial development we use the "Financial Structure dataset" constructed by the World Bank (Beck et al 2000). In particular we focus on the private credit given by deposit money banks and other financial institutions as a share of GDP; on the ratio of the value of total stock market shares traded as a percentage of the average real market capitalization; on the value of the shares listed in the stock market as percentage of GDP; and finally on the non-life insurance premium volume as a share of GDP.

Finally we also used the information on the balance sheets of households and non financial corporations provided by the OECD and the DG ECFIN AMECO database. In particular we looked into the share of GDP represented by loans, liabilities and net financial asset.

[21] See Appendix 1, Area 5 in http://www.freetheworld.com/release.html

Table A1. Rank correlation among market rigidity indexes and reform dummies

	(1)		(2)		(3)		(4)		(5)	
	Tax wedge on labour		Unemployment benefits		EPL		Implicit tax on continued work (retirement schemes)		Product market regulation	
	Time average, across countries	Country average, over time	Time average, across countries	Country average, over time	Time average, across countries	Country average, over time	Time average, across countries	Country average, over time	Time average, across countries	Country average, over time
Level of index of market regidity										
Time average, across countries										
Tax wedge on labour	1	1								
Unemployment benefits	0.45*	0.24	1	1						
EPL	0.66*	-0.21	0.42*	-0.14	1	1				
Implicit tax on continued work (retirement schemes)	0.66*	-0.35	0.31	-0.12	0.68*	0.37	1	1		
Product market regulation	0.53*	-0.05	0.49*	-0.01	0.72*	0.24	0.74*	0.06	1	1
Reform dummy (across whole sample)										
Tax wedge on labour	1									
Unemployment benefits	0.09*		1							
EPL	-0.07		0.02		1					
Implicit tax on continued work (retirement schemes)	0.02		0.18*		0.17*		1			
Product market regulation	-0.03		0.03		0.05		0.01		1	

Notes: Spearman rank correlation. * denotes statistical significance at 10 per cent level.

Table A3. Reforms and re-elections
(missing if no elections; 1 if reform or re-election; 0 if no reform or no re-election)

		1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
AUS	Reform		0			0			0			0		0			1		
	Re-election		1			1			1			0		1			1		
AUT	Reform	1				0				0	0				1			1	
	Re-election	0				1				1	1				0			1	
BEL	Reform		0				0				0				1				1
	Re-election		1				0				1				0				1
CAN	Reform			0					0				0			0			
	Re-election			1					0				1			1			
CHE	Reform		0				0				0				0				1
	Re-election		1				1				0				1				0
DEU	Reform		0			0				0				0				1	
	Re-election		1			1				1				0				1	
DNK	Reform		0	0		0				0				1			1		
	Re-election		1	0		1				0				1			0		
ESP	Reform	0			1				0			1				0			
	Re-election	1			1				1			0				1			
FIN	Reform		0	0			0			0	0				0	0			0
	Re-election		0	1			0			1	0				1	1			0
FRA	Reform	0		0					0		0		0					0	
	Re-election	0		0					0		0		0					0	
GBR	Reform		1					0					0				0		
	Re-election		1					1					0				1		
GRC	Reform				0				0			0				0			
	Re-election				0				0			0				1			
IRL	Reform		0		0			0					0					1	
	Re-election		0		1			0					0					1	
ITA	Reform		0					0		0		0					1		
	Re-election		0					0		0		0					0		
JPN	Reform	1				0			0			1				0			0
	Re-election	1				0			0			0				0			1
NLD	Reform	0			0		0			0				0				1	1
	Re-election	1			1		1			0				1				0	1
NOR	Reform				0				1				0				0		
	Re-election				0				1				0				0		
NZL	Reform		0			0			1			0			1			0	
	Re-election		1			0			1			1			0			1	
PRT	Reform		0				0				0	1			0		1	1	
	Re-election		0				1				0	0			1		1	0	
SWE	Reform			0			0			0				1				1	
	Re-election			1			0			0				1				1	
USA	Reform	1				0		0		0		0		0				1	
	Re-election	1				1		0		1		1		1				1	

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