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COGNOME | Onorato

NOME | Massimiliano Gaetano

Matr. | 1094713

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# INTRODUCTION

This dissertation consists of three essays which provide an empirical analysis of three research questions in the field of political economy and institutional economics.

The first chapter, which draws from a joint work with Miriam Bruhn and Francisco Gallego, investigates the issue of the institutional persistence with a specific focus on Latin America. Recent literature claims that institutions were shaped by remote historical events, like the colonization period, and tend to persist over time. Countries where "*extractive*" institutions were set in the past by a narrow group of economic and political elite in order to preserve their own interests are likely to be less economically developed today. A relevant issue related to this argument is: if institutions are beneficial only to a narrow group of people in the society (the elite) and are detrimental for economic development, why do they not change when a country experiences a transition to democracy? Recent theoretical models outline that transition to democracy does not necessarily imply a loss of political power by the elite. They can still influence democratic decisions by undertaking several forms of investment such as lobbying, buying votes or patronage. The first chapter of this dissertation contributes to the existing literature, by stressing that a specific feature of democratic regimes - legislative malapportionment - might be used by the pre-democratic elite to preserve their political influence in a democracy. Legislative malapportionment arises when there is a discrepancy between the share of lower house seats and the share of total population held by each electoral district. Building upon the theoretical model provided by Acemoglu and Robinson (2006) in

their book "*Economic Origins of Dictatorship and Democracy"*, we argue that legislative malapportionment provides pre-democracy elite with a way to keep their *de jure power* in a democracy by over-representing specific geographical areas and favoring certain political parties. This skewed political representation survives in equilibrium as long as it makes a reversal to dictatorship less likely and helps democratic consolidation. We provide empirical evidence for this theoretical argument. By using a panel dataset of eleven Latin American countries starting from the late XIX century to the present, we document that higher legislative malapportionment sustains the process of democratic consolidation. We then rely on within country data to study the political tendencies of the districts that are over-represented, in the sense that they have a larger share of representatives in the lower house than their share of the national population. We show that in the first elections after the transition to democracy those districts are more likely to support political parties close to the pre-democracy ruling groups. Over-represented districts are also characterized by a lower degree of political competition and are able to receive larger transfers per capita from the central government. These latter findings might help to explain  the persistence of power of pre-democratic elite.

The second chapter is a joint work with Caterina Gennaioli and is part of a broader research project with Roberto Perotti and Guido Tabellini. It focuses on the Mafia and other forms of organized crime whose persistence over time is a feature of many regions in the South of Italy. We consider the Mafia and other forms of organized crime as a specific kind of entrepreneur ready to move where profitable economic opportunities arise. The chance of making lucrative business induces the

Mafia to expand its activities beyond the geographical areas characterized by its endemic presence and beyond its "traditional" businesses like traffic of arms, drugs and extortions. In specific, we focus on the role that public spending (in particular in the sector of construction) plays in order to explain the spreading of organized crime even in areas whose features (in terms of cultural attitudes and quality of the institutions) are different from those of the Southern regions of Italy. The relevance of public spending for the economic activities of organized crime is, for instance, documented by the official report of the 1988 "*Commissione Parlamentare Antimafia*". The authors of the report stress how organized crime has always paid attention to local municipalities in the South of Italy as providers of public funding in order to increase its economic and political influence. Dossiers from official Committees of the Italian Parliament also report the infiltration of organized crime in certain areas of the South after the big influx of money for reconstruction in the aftermath of the 1980 Irpinia earthquake. Recent judicial investigations point to the attempts made by organized crime to take part in the reconstruction phase that took place after the earthquake that in 1997 hit two regions, Umbria and Marche, not characterized by the endemic presence of organized crime.

To empirically test our research hypothesis that an increase in public spending might favor the spreading of organized crime, we use an instrumental variable approach. We rely on the occurrence of earthquakes as a source of exogenous variation in public spending. In specific, we focus on the earthquake that in 1997 hit the provinces of Marche and Umbria (our "*treated group*") causing damages for an estimated amount of eight billions euro. We choose to use as "*control group*" the provinces belonging to the neighboring regions of Marche and Umbria in order to guarantee a higher degree of homogeneity between treated and control

units in terms of their economic, social and cultural characteristics. To provide a quantitative assessment of the presence of organized crime we rely on its official definition provided by the Italian Interior Department which includes the following crimes: homicides committed by the Mafia, bombing attempts, arson and grand larceny. The relevant data source is ISTAT, "*Statistiche Giudiziarie Penali*". Our findings refer to the years 1993-2003. They suggest that an increase in public spending leads to an increase in activity of criminal organizations. We plan to extend this study in several ways. First, we want to assess whether a similar phenomenon occurred in the aftermath of other major earthquakes that have hit Italy. An interesting and related issue that we plan to explore further concerns the consequences of the spread of organized crime on the quality of politicians.

The third chapter analyzes the association between a broad set of new indicators of structural reforms and economic growth. It is a joint work with Alessandro Prati and Chris Papageorgiou. We address two main research questions. First we ask whether structural reforms are growth enhancing. We then investigate to what extent the effectiveness of structural reforms is influenced by a country's institutional environment or by its distance from the world technological frontier. We take a comprehensive look at these questions by using a novel dataset of structural reform indicators. These new indices are constructed as part of a large project on structural reforms at the International Monetary Fund's Research Department. They provide a quantitative assessment of the process of structural reform both in the real (international trade, agriculture and network industries) and financial (domestic financial, banking, securities and capital account) sectors of the economy. Many of

these indices cover the last thirty years and over ninety countries. The key advantage of these measures over those used in previous literature is that they have a long time series dimension and cover a large number of countries, including advanced and developing economies. Evidence based on the analysis of growth acceleration episodes and on growth regressions suggests that both real and financial sectors' reforms are positively associated with economic growth. We also show that the positive association between reforms and growth is heterogeneous and influenced by a country's constraints on the executive power and by its distance from the technological frontier.

# CHAPTER 1


# LEGISLATIVE MALAPPORTIONMENT AND INSTITUTIONAL PERSISTENCE

*"The rules of the game in a society or, more formally […]*
*the humanely devised constraints that shape human interaction, […]*
*structure incentives in human exchange, whether political, social or economic"*

North (1990, p. 3)

## 1.1    Introduction

A broad and relatively recent literature investigates the effects of legal and political institutions on long-run economic development. The papers in this literature typically claim that institutions were shaped at some time in history, for example during the colonial era. These institutions then persist over time and influence economic outcomes today. A number of papers illustrate that countries where institutions were shaped by economic and political elites in order to promote their own interests tend to be less developed economically today (see for example Acemoglu, Johnson and Robinson, 2001, 2002 and 2006, and Engerman and Sokoloff, 1997).

A fundamental question about this argument is, why and how do these institutions persist? If some institutions benefit only a minority in society (the elite) and hinder economic growth, then why don't they change when the country transitions to democracy? A recent paper by Acemoglu and Robinson (2008) provides an answer to these questions by developing a theoretical model that predicts that transition to democracy does not necessarily lead to a loss of economic and political power of the elite. In this model, the elite can influence democratic decision-making by undertaking several forms of investment, such as lobbying, paramilitary forces, and patronage. This implies that institutions and policies are not

necessarily different in a democracy from what they are in a non-democracy[1].

Our paper contributes to this literature by illustrating that a specific feature of democratic regimes – legislative malapportionment – can also serve as a political tool for pre-democratic elites to preserve their political power and economic interests in a democracy. Legislative malapportionment refers to a discrepancy between the share of lower house seats and the share of the total population held by each electoral district. Many constitutions explicitly state that electoral districts should have the same share of lower house representatives as their respective share of the national population in order to guarantee the legal equality of each citizen's vote. However, this principle does not always hold in practice, and consequently the lower houses in many countries are malapportioned.

This paper first offers a political economy rationale for the emergence and persistence of legislative malapportionment. We base this rationale on Acemoglu and Robinson's (2006) argument that, at the time of transition to democracy, groups holding political power have strong incentives to manipulate the newly established political institutions, in order to protect their political and economic interests. We claim that legislative malapportionment provides these groups with a way of enhancing their *de jure* power by over-representing certain geographic areas and by favoring certain political parties versus others. This skewed political representation survives in equilibrium as long as it makes democratic consolidation more likely. At the same time, it is associated

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[1] Mulligan, Gil, and Sala–I–Martin (2004) show empirically that democracies do not necessarily have very different public policies from authoritarian regimes.

with lower political competition and distorts public policies, which also helps to preserve the power of pre-democratic elites.

The paper then provides empirical evidence for this theoretical argument. In contrast to other institutional arrangements of democracies, such as clientelist policies, corruption or lobbying, malapportionment is clearly defined and measurable, allowing us to test the predictions of our argument empirically. We first use panel data for 11 Latin American countries going from the late XIX century to the present, to show that higher legislative malapportionment makes democratic consolidation more likely, possibly because it helps to safeguard the interests of the groups that held political power before the transition to democracy occurred.

We then rely on within country data for a number of Latin American countries to examine the political tendencies of districts that are overrepresented in the sense that they have a higher share of representatives in the lower house than their population share[2]. Consistent with our theoretical argument, we show that, in the first election after transition to democracy, overrepresented districts are more likely to vote for parties that are close to pre-democracy ruling groups.

We also examine other political and economic policy consequences of malapportionment. In our within country dataset, we find that overrepresented districts have lower levels of political competition. Finally, even though overrepresented districts are not different from underrepresented districts with respect to output per capita and inequality, they receive a larger amount of transfers per capita from the central government. This last finding goes against the insights from

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[2] We chose to limit the analysis to Latin America for two reasons. First, Latin American countries have more time series data available on legislative malapportionment. Second, the relatively higher degree of historical homogeneity of this sample of countries may allow us to better gauge the effect of legislative malapportionment on political and economic outcomes.

traditional models of redistributive politics and confirms that unequal representation can translate into a higher ability to gain monetary benefits.

Although we provide evidence suggesting that malapportioment preserves the political power of pre-democratic elites, we do not show explicitly that this is linked to the persistence of specific institutions. Other papers have identified specific institutions such as property rights or contracting institutions that can affect economic development (see Acemoglu and Johnson, 2005). We do not have a sufficient amount of panel or within country data on these institutions to test whether they are correlated with malapportionment. However, we interpret our findings as indicating that malapportioment helps pre-democratic elites to preserve the "rules of the game in a society" (North 1990, p. 3) that the elite put into place before transition to democracy. We thus view malapportioment as being linked to a broad definition of institutions, following Douglass North's definition of institutions as being the "humanely devised constraints that shape human interaction".

The paper is organized follows. Section 2 provides a short discussion of malapportionment and develops our theoretical argument for the emergence and persistence of legislative malapportionment. It also outlines the possible channels through which legislative malapportionment could affect political and economic policy outcomes. Section 3 describes our data. Section 4 includes the empirical analysis, and Section 5 concludes.

## 1.2    Motivating Theory

This section first discusses several general features of malapportioment and then lays out our theoretical argument for the origins and consequences of malapportionment.

A long standing literature in political science identifies malapportionment as a formal and often deliberate "pathology of electoral systems" (Taagepera and Shugart, 1989; Snyder and Samuels, 2004). Malapportionment – a discrepancy between the share of legislative seats and the share of population held by electoral districts – violates the "one person, one vote" principle that authors like Robert Dahl (1971, 1989) consider to be a basic pillar of fair democratic regimes. Even though this principle is often guaranteed by constitutional charts, in many countries it has been disregarded or implemented only after judicial intervention[3].

Countries with a bicameral system can display a high level of malapportionment in the upper chamber since this chamber usually represents all geographic units more or less equally. While upper chamber malapportionment is normatively justifiable, there is no *a priori* reason for weighing the votes of citizens unequally in the lower chamber. This paper thus focuses only on lower chamber malapportionment.

Lower chamber malapportionment can arise spontaneously over time due to migration or different regional patterns of population growth. Western European and North American democracies typically have low levels of malapportionment since they periodically reapportion the number of seats attributed to electoral districts in response to these demographic changes (Snyder and Samuels, 2004).

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[3] For example, with two verdicts, *Baker v. Carr* in 1962 and *Wesberry v. Sanders* in 1964, the US Supreme Court ruled in favor of redesigning electoral districts since they were characterized by high levels of malapportionment. The Supreme Court's motivation for these sentences was the necessity to safeguard the "one man, one vote" principle (see Casper, 1973).

On the other hand, the data from Samuels and Snyder (2001) and Snyder and Samuels (2004) reported in Tables 1 and 2, suggest that many of the countries with legislative malapportionment are newly established or consolidating democracies, implying that democratic regimes can also start out with high levels of malapportionment. It therefore seems unlikely that malapportionment is only the result of medium to long-run phenomena such as migration and differential patterns of regional demographic change. In addition, the evidence from Latin America reported in Table 2 suggests that malapportionment not only characterizes democracies around the time of their establishment, but that it tends to persist over time in some countries[4].

In this paper, we argue that pre-democratic elites strive to promote malapportionment as a political tool for preserving the political and economic power they had before the transition to democracy. This argument builds on the model of transition to democracy developed by Acemoglu and Robinson (2006). We rely on the main insights of this model to study the origins and the persistence of malapportionment.

Acemoglu and Robinson (2006) highlight how elite groups that hold power in dictatorships can manipulate *de jure* or *de facto* democratic institutions in order to preserve their political and economic interests. Acemoglu and Robinson's model assumes the existence of two groups: (i) an "elite", the (typically) richest fraction of the population that holds political power during a dictatorship, and (ii) the "citizens", the (typically)

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[4] Snyder and Samuels (2004) report that, among the Latin American countries listed in Table 2, only Colombia, Uruguay and Venezuela do not have formal constitutional provisions that guarantee the "one citizen - one vote" principle. The remaining countries (with the exception of Peru) display high levels of malapportionment despite the fact that their constitutions formally prescribe the equality of each citizen's vote.

poorest fraction of the population[5]. In addition, the model assumes that political contracts are incomplete, meaning that the elite promising benefits to the citizens is not incentive compatible ex-post, and that the citizens can threaten the elite with revolution if they do not receive enough income transfers[6]. In this set-up, redistributive policies are only sustainable and credible if the elite transfer  political power, at least in part, to the citizens.

The main insight of this model is that in a Markovian equilibrium democratization acts as a credible commitment to pro-citizen policies. In this equilibrium, the elite has to democratize in order avoid strikes, riots or - in the limit - a revolution. However, it is possible for the elite to build a distorted or "captured" democracy, where the elite holds proportionally more political power than their population share. A transition to a distorted democracy is more likely to occur whenever the elite has vested economic interests that are potentially threatened by the policies preferred by the citizens in the new democratic regime[7]. In addition, if – for ideological or economic reasons – the citizens prefer to live in a democratic regime, they may accept these biased political institutions, thereby committing not to harm the elite's interests.

In a related paper, Acemoglu and Robinson (2008) build a model that predicts that policies remain invariant across authoritarian and democratic regimes in equilibrium. In this model, the elite has incentives

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[5] In general, the elite does not need to be the richest group in the population. They could be any small group that earns political or economic rents during a dictatorship. These rents could be lost as a consequence of the transition to democracy.

[6] An exogenous distribution determines the likelihood that citizens will threaten the elite with revolution.

[7] Acemoglu and Robinson (2006) use the example of the Chilean "*democracia protegida*" after the Pinochet dictatorship as an example of a distorted democracy in which the former dictator and its followers hold a disproportionate quota of *de jure* political power. In general, the model implies that countries where elite groups hold a larger share of national income are more likely to be characterized by distorted democratic institutions.

to manipulate democracy in order to avoid losing their rents to the citizens. The elite can affect democratic decision-making by undertaking several forms of investment, such as lobbying, paramilitary forces, and patronage.

Applying this political economy framework to our paper, we view malapportionment as a device that the elite can employ to keep *de jure* political power after the transition to democracy[8]. That is, malapportionment could allow a democracy to emerge and persist but in a "captured" form with the elite still being able to impose their preferred policies. Malapportionment can act as a substitute for other mechanisms such as coups, lobbying, and vote buying that the elite can use to keep *de facto* power in a democracy but that involve collective action problems. This argument is also related to Dahl (1971), who states that democracies can be defined in terms of (i) institutionalization and (ii) representation. Successful democracies start with (i) and later move to (ii). In contrast, failures start with (ii) and follow with (i). Malapportionment could thus be present in the early stages of successful democracies and could help their consolidation. In Section 4, we test empirically whether malapportionment fosters democratic consolidation.

Our argument also relies on a number of case studies of Latin American countries discussed in Snyder and Samuels (2004). They document that the military dictatorships in Argentina and Brazil

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[8] Several papers investigate the endogenous choice of democratic institutions. Aghion, Alesina, and Trebbi (2004) focus on the political economy of choosing the size of the minority needed to block legislation and the optimal size of the supermajority necessary to govern. Similarly, Trebbi, Aghion, and Alesina (2008) develop a theoretical model to show how the majority of a population can have strong incentives to manipulate electoral rules as the size of the minority changes. Evidence on the electoral rules of southern US cities is consistent with this theoretical argument. Finally, Ticchi and Vindigni (2003) model the determinants of the choice between majoritarian and consensual democracies. They show that more unequal countries are more likely to choose majoritarian democracy since it is more likely to have a political economic equilibrium with lower taxation and a smaller size of government.

redistributed seats in the lower house just before the transition to democracy (1983 in Argentina, 1982 in Brazil) in order to over-represent conservative areas. As a consequence, Argentinean provinces that have only 31% of the national population control 44% of seats in the Chamber of Deputies and Brazilian regions that hold 42% of the population are endowed with 51% of the Lower Chamber's seats. In Chile, the Pinochet regime (1973 - 1990) behaved in a similar manner. Before Chile transitioned to democracy, the electoral system was redesigned to guarantee the over-representation of areas with more conservative political tendencies. Therefore, areas with 35% of the Chilean population control half of the seats in the Lower Chamber. We investigate this argument empirically in a larger set of countries. Section 4 tests whether overrepresented areas in 14 Latin American countries tend to lend political support to conservative parties that are close to pre-democratic political elites.

Overall, we thus argue that malapportionment is a tool that pre-democratic elites can use to achieve a certain degree of institutional persistence. That is, even after a country formally transitions to democracy, policies can still be shaped by the elite's preferences since malapportionment increases the number of lower house votes of parties aligned with the elite. Moreover, if malapportionment is indeed a way of preserving power for the elite, then it is basically self-enforcing, such that it persists over time. Since malapportionment is a legal device, any change to it will require a plurality of votes in parliament. However, such a plurality is unlikely to be achieved since malapportionment distorts the allocation of seats in favor of the groups that stand to gain from this preserving this distortion.

Moreover, malapportionment could lead to the persistence of the elite's political power by affecting the degree of political competition within electoral districts. Cox and Katz (1999) document that the redistricting that took place in US after the Supreme Court's intervention was associated with the disappearance of the long lasting pro-Republican bias in the translation of votes into seats in non-southern congressional elections. Section 4 of this paper tests empirically whether malapportionment is correlated with lower political competition.

Finally, malapportioment could also foster the persistence of the elite's political power by changing the allocation of public funds to areas in which the members of the elites have more political representation. Several empirical papers establish a link between malapportionment and the distribution of public spending. McCubbins and Schwartz (1988) and Ansolabehere, Gerber and Snyder (2002) study the effects of court ordered redistricting in the US. They document that reapportionment did not change the overall level of public spending, but significantly affected its distribution among electoral districts. Similarly, Horiuchi and Saito (2003) analyze the consequences for public spending of the reapportionment that took place in Japan in 1994. They find that this reform was associated with the equalization across municipalities of public transfers per capita. Other studies such as Gibson, Calvo and Falletti (2004) for Latin America and Knight (2004) for the US Senate highlight that overrepresented areas get a larger share of federal funds. Samuels (2002) shows that the composition of several Brazilian budgetary committees (such as the Joint Committee for Planning, Public Budgets and Oversight or CMO) reflects the legislative chambers' patterns of geographical over/under representation. He also documents that this translates into a biased redistribution of public spending among Brazilian

States. Aghion, Boustan, Hoxby, and Vandenbussche (2006) show that members of the appropriation committee in the US legislature are able to channel more resources to electoral districts located in areas they represent. Although these papers document empirically that malapportionment influences that allocation of public resources, we test whether this relationship is also present in our dataset.

## 1.3    Data description

This section describes the data used in the empirical analysis. We first provide a definition of our measures of legislative malapportionment and then turn to the description of the other data. The panel and within country data used in this paper only cover Latin American countries.

### 1.3.1    Measures of legislative malapportionment

This paper uses two main measures of lower chamber legislative malapportionment. The first measure is an index of malapportionment at the country level provided by Samuels and Snyder (2001) and Snyder and Samuels (2004). Their measure is a slight modification of the Loosemore–Hanby index of disproportionality for electoral systems. Country $j$'s overall level of lower house malapportionment is computed as:

$$MAL_j = \frac{1}{2}\sum_{i=1}^{N}\left|s_i - v_i\right| \tag{1}$$

where $s_i$ is the percentage of all seats allocated to district $i$ and $v_i$ is the percentage of the overall population that resides in district $i$. Each district's deviation from perfect apportionment is given by the difference between its share of seats and of the population. The formula sums over

all *N* electoral districts in country *j*. The index thus denotes the share of seats allocated to districts that would not have received those seats if there were no legislative malapportionment.

A score of 0 corresponds to the case of a perfectly apportioned lower chamber where no citizen's vote weighs more than another's. Full malapportionment corresponds to a score of 1 and denotes a situation where a single district with only one voter has the right to choose all the legislators. A value 0.25 of the index means that one fourth of the seats are allocated to districts that would not have them in the absence of legislative malapportionment.

We also use a within country variant on the measure of malapportionment in our empirical analysis. To measure electoral district *i*'s degree of over- or under-representation we follow the existing approach in the literature (see Ansolabehere et al., 2002) and adopt the following measure:

$$rep_i = \frac{s_i}{v_i} \qquad (2)$$

where $s_i$ is the share of seats allocated to the district *i* and $v_i$ is district *i*'s share of the population. Values greater than 1 denote over-representation of district *i*, and the opposite is true for values lower than 1. The data needed to compute (2) are from Samuels and Snyder (2001) and Snyder and Samuels (2004), as well as from national sources.

### 1.3.2 Cross-country panel data

We use historical country–level data from 1870 to 2000 on political institutions for a panel of 11 Latin America countries[9]. Our measure of democracy is the variable *polity 2* from the Polity IV Dataset: it ranges from −10 to +10 with higher values corresponding to better democratic institutions. We normalize the measure so that all values fall between 0 and 1.

Some of our cross-country regressions control for per-capita GDP, which we take from Maddison (2005) for all the countries but Chile. For Chile, we use data from Díaz et al. (2008).

### 1.3.3 Within country data

Our source for Latin American within country data is Bruhn and Gallego (2009). This source provides data on income per capita, the Gini index, temperature, rainfall, and on the type of colonial activities, as well as a landlocked dummy, for different regions within 14 Latin American countries[10]. We collected additional within-country information on political parties, electoral outcomes, and on transfers from the central government from several national sources and documents.

### 1.4 Empirical evidence

This section presents the empirical tests of our theoretical predictions from Section 2. We first study the relationship between

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[9] The countries included in this panel dataset are Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Ecuador, Honduras, Peru, Uruguay and Venezuela.
[10] The countries covered in this within country dataset are Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, El Salvador, Guatemala, Honduras, Mexico, Panama, Paraguay, Uruguay, and Venezuela.

malapportionment and democratic consolidation. Then, we investigate whether malapportionment provides political influence to pre-democracy elites. Finally, we examine the correlation of malapportionment with other economic and political outcomes.

## 1.4.1 Malapportionment and democratic consolidation

In Section 2, we argue that legislative malapportionment can help democratic consolidation. If the groups that held political power pre-democracy are able to strategically engineer democratic institutions in a way that guarantees them more political influence than corresponds to their population share, they have fewer incentives to overthrow democratic regimes. Moreover, as also mentioned in Section 2, Dahl (1971) suggests that democratic consolidation may be more likely if the first step of democratic institutionalization is only later followed by full representation.

This section provides empirical evidence for a positive relationship between legislative malapportionment and democratic consolidation. Our empirical strategy closely follows Acemoglu, Johnson, Robinson, and Yared (2005 and 2007). The estimating equation is:

$$d_{it}^{+} = \alpha d_{it-1} + \beta mal_{it-1} + \gamma y_{it-1} + \delta_i + \mu_t + \varepsilon_{it-1} \qquad (3)$$

where $d_{it}$ is the POLITY IV score of democracy normalized between 0 and 1 and $d_{it-1}^{+} = \max\{d_{it}, d_{it-1}\}$, implying that Equation 3 only focuses on upward trends in the democracy score. Equation 3 includes the lagged dependent variable to capture persistence in democracy scores and the lag of the logarithm of legislative malapportionment $(mal_{it-1})$. Since an extensive literature dating back to Lipset (1959) claims that economic prosperity has

a positive impact on democracy and democratic consolidation, and given that malapportionment is correlated with income[11], we also include the log of lagged income as a control in Equation 3[12]. Finally, we control for country and time fixed effects. The observations, going from 1870 to 2000, are taken over 5 year intervals and standard errors are clustered at the country level.

Column 1 of Table 3 reports OLS estimates of Equation 3. The estimate of the coefficient of interest $\beta$ is positive and statistically significant, suggesting that higher legislative malapportionment promotes democratic consolidation for the Latin American countries included in our sample. Moreover, the democracy score shows high persistence over time and income per capita is positively correlated with the democracy score[13].

As a robustness check, Column 2 of Table 3 presents the estimates of Equation 3 using a GMM procedure, to address the potential biases that can arise when estimating a quasi-dynamic panel with country fixed effects. The results confirm the positive effect of malapportionment on democratic consolidation[14]. However, the size of the coefficient on lagged malapportionment increases in magnitude, suggesting that the results in Column 1 are biased due to the fact that they are estimated in a quasi-dynamic panel with country fixed effects. In the GMM regression,

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[11] Running panel data regressions in our sample of Latin American countries, controlling for countries and time fixed effects, we find that the lagged level of malapportionment Granger – causes income, but not the opposite. These results are available from the authors upon request.

[12] See Acemoglu et al. (2007) for a critical reexamination of the empirical evidence about the modernization hypothesis.

[13] This finding is not in line with Acemoglu et al. (2007). Using a sample much larger than ours, they find no statistically significant relation between income and improvement in the democracy score.

[14] Regressions for downward trends in democracy that use $d_{it-1}^{-} = \min\{d_{it}, d_{it-1}\}$ as the dependent variable show no correlation between malapportionment and democracy. The results are available from the authors upon request.

the lagged value of income is no longer correlated at a statistically significant level with the democracy score, which is in line with Acemoglu et al. (2007).

The effect of malapportionment on democratic consolidation is also economically significant. A one standard deviation increase in log malapportionment implies an increase of 0.73 standard deviations in the democracy index in the short-run. Taking into account that the democracy index is persistent over time, the long-run effect of malapportionment on democratic consolidation is even larger. In the long-run, a one standard deviation increase in log malapportionment is associated with a one standard deviation increase in the democracy index.

Overall, the results in this section are consistent with our hypothesis suggesting that malapportionment has a significant and economically relevant positive effect on democratic consolidation.

## 1.4.2 Malapportionment and political representation of pre-democracy elites

Our theoretical argument and the evidence we have provided so far suggest that legislative malapportionment may guarantee the pre-democracy elite a disproportionate level of political influence after transition to democracy. We test this argument directly using within country data for six Latin America countries. This data allows us to investigate whether regions that are overrepresented are more likely to be represented by political parties that are closest to the most recent authoritarian regime.

We identify these parties based on information from the Economist Intelligence Unit's country reports and other sources. For each state or region, we then compute the vote shares that these political

parties received in the first lower house election after the transition to democracy. We collected data for 118 regions in six Latin America countries that have transitioned to democracy since the 1980s and that have political groups close to the previous non-democratic regime (Bolivia, Brazil, Chile, Mexico, Paraguay, and Uruguay)[15]. The model we estimate is:

$$s_{ij} = \alpha + \beta rep_{ij} + \phi x_{ij} + \delta_j + \varepsilon_{it} \tag{4}$$

where for each country $j$, $s_{ij}$ is the share of lower house votes going to the parties close to the pre-democracy regime in region $i$, $rep_{ij}$ is the log of our measure of district $i$'s over or under-representation, $x_{ij}$ is a set of climate controls (rainfall and temperature) and geography controls (elevation and a landlocked dummy), and the $\delta_j$'s are a full set of country fixed effects.

The results in Columns 1 and 2 of Table 4 document that over–represented electoral districts were more likely to vote for representatives belonging to the political parties that were close to former non–democratic regimes in the first election after transition to democracy. Our estimates imply that a one-standard deviation increase in the log of

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[15] We consider the following years for the low chamber elections and the following parties to be closest to the former non-democratic regime:
- Bolivia, 1989, the A.D.N. party.
- Brazil, 1990, the P.D.S. party.
- Chile, 1989, the Alianza coalition.
- Mexico, 2000, P.R.I.
- Paraguay, 1996, the Colorado party.
- Uruguay, 1984, the Colorado party.

The case of Peru is also interesting, but we do not include it here because, since the 1991 reform, Peru has a lower chamber with only one nationwide electoral district and therefore it is not possible to compute the degree of over-representation for each region. This reform was implemented during the Fujimori dictatorship, and in the 1990 election the degree of over-representation was negatively correlated with the percentage of support for Cambio 90, the political group closest to Fujimori in the 1990 elections. Therefore, the 1991 Fujimori reform can actually also be explained with our theory.

overrepresentation in these regions is associated with between a seven and an 11 percent increase in the vote shares going to parties close to the pre-democracy regime (equivalent to between 29 and 46 percent of a standard deviation of the vote shares going to these parties). We interpret this as evidence that malapportionment provides a disproportionately high share of political power to pre-democracy elites.

Next, we conduct a robustness check in which we run regressions similar to Equation 4, but using information on elections that took place *during* non-democratic periods. This is the case of the 1978 elections in Brazil, the 1988 plebiscite in Chile, and the 1991 parliamentary elections in Mexico. For these elections, we compute the vote shares supporting the regime[16]. The results in Columns 3 and 4 in Table 4 show a positive correlation between both variables. Results using a country dummy are imprecisely estimated, but results using a random effects model imply the effect is positive and significant at a 1.5% level. The economic significance of these results is very similar to the results presented in columns 1 and 2 (equivalent to between 26 and 29% standard deviations of the support of the dictatorships). This exercise gives additional support to our hypothesis of a positive association between the share of seats granted to electoral districts in the democratic period and their past political support to authoritarian regimes.

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[16] For Brazil, we look at support for the ARENA party, for Chile at the SI option in the plebiscite, and for Mexico at support in favor of the PRI party.

### 1.4.3 Malapportionment and political and economic policy outcomes

This section uses within country data for 15 Latin America countries to study the possible effects of malapportionment on political and economic policy outcomes. We need to use *within* country data to study these channels since our theoretical argument suggests that malapportionment shifts the distribution of political power across regions within countries.

We first study whether malapportionment is correlated with political competition. We proxy political competition by using the Herfindahl-Hirschmann index of political concentration (HH index) based on the share of votes going to different political coalitions in different states. An increase in this index implies an increase in the degree of political concentration. The first two columns of Table 5 present the results of running regressions similar to Equation 4 but using the HH index as the dependent variable. Columns 1 and 2 include regression without and with controls, respectively. The results show a positive relationship between malapportionment and the degree of political concentration. However, the results in the regression with controls are only marginally statistically significant (p-value of 0.15). In terms of the economic significance of the results, a one standard deviation increase in over-representation leads to an increase of about 0.09 standard deviations of our measure of concentration.

To further study this mechanism and to relate it to the results in Table 4, we run regressions of the interaction of the pro-government share and the HH index on the same determinants. The idea here is to study whether political concentration in these areas is related to the

political groups that are closer to former non–democratic regimes[17]. Columns 3 and 4 of Table 5 present the results without and with controls. The estimates in both columns are positive, statistically significant and economically relevant. A one standard deviation increase in over-representation increases the dependent variable by between 0.35 and 0.50 standard deviations, accordingly to the results of Columns 3 and 4, respectively. All in all, these results imply that the degree of political concentration, especially in parties that benefit from over-representation after transition to democracy, increases as over-representation increases, as suggested by our motivating theory.

Our theoretical discussion and the previous literature also suggest that malapportionment could affect the distribution of public transfers across regions. We estimate the relationship between malapportionment and transfers per capita with a model similar to Equation 4, where the dependent variable is the transfers per capita from the central government to region $i$. We use two alternative measures of transfers. "Total transfers" includes all transfers that the central government made to a region, including transfers to state and municipal governments, social transfers, direct expenditures and investment by the central government, as well as transfers to public universities, whenever available. The categories included vary from country to country, depending on availability. A more uniform variable is "transfers to sub-national government" which includes only transfers to state and/or municipal governments within a region.

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[17] Notice that the dependent variable can be interpreted as the probability that two random voters vote for the political groups that are closer to former non–democratic regimes because it is the probability that two people vote for the same party (the HH index) times the probability that a person votes for the political groups that are closer to former non–democratic regimes (the share of votes going to these groups).

The results in Columns 1 and 2 of Table 6 confirm the previous findings in the literature that over–representation translates into higher transfers per capita from the central government. In terms of economic significance, our results imply that a one standard deviation increase in malapportionment at the local level increases transfers per-capita by about 10 percent of a standard deviation.

Columns 3 and 4 of Table 6 check whether overrepresented areas are either more unequal or poorer than underrepresented areas. If this were true, then the higher transfers to these regions could be due to a welfare criterion in which poorer regions or poorer people receive more transfers. However, Columns 3 and 4 show that overrepresented areas are neither poorer nor more unequal than underrepresented areas. We thus interpret the results in Table 6 as providing additional evidence that stronger legislative representation translates into more political influence and higher economic benefits for overrepresented regions.

## 1.5    Conclusion

In this paper, we argue that pre-democracy elites can strategically create malapportionment in the electoral system during the transition to democracy in order to safeguard their economic interests in a newly established democracy. Our results show that higher levels of legislative malapportionment foster democratic consolidation, presumably because it makes pre-democratic elites feel less threatened by the policies that might be implemented during the new democratic regimes. Moreover, we find that, within a country, overrepresented electoral districts are more likely to vote for parties that are close to the former non-democratic regime.

In addition, we highlight two consequences of malapportionment for political and economic policy outocomes. First, malapportionment is associated with a decrease in political competition particularly for parties close to former non-democratic regimes. Second, overrepresented districts receive larger transfers per capita from the central government, despite the fact that they are not poorer or more unequal. This contrasts with traditional models of redistributive political economy and highlights that larger legislative representation induces greater political influence.

In future research, we plan to investigate the effects of malapportionment on economic development. The decrease in political representation or miss-allocations of public transfers we document in this paper may have negative effects on regional development. The big challenge with identifying these effects though is to find a source of exogenous variation in malapportionment at the state level.

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**Table 1. Most Malapportioned Countries and Transition to Democracy**

| Country | Malapportionment (Lower Chamber) | Transition to democracy (Year) |
|---|---|---|
| Tanzania | 0.2619 | 2000 |
| South Korea | 0.2075 | 1987 |
| Ecuador | 0.2040 | 1979 |
| Kenya | 0.1946 | 2002 |
| Ghana | 0.1782 | 1996 |
| Zambia | 0.1725 | 1991 |
| Iceland | 0.1684 | 1944 |
| Bolivia | 0.1677 | 1982 |
| Malawi | 0.1659 | 1994 |
| Chile | 0.1622 | 1989 |

SOURCES: Samuels and Snyder (2001) for the measure of malapportionment and POLITY IV database for coding transition to the democracy. Transition to democracy is defined as the first year where the variable POLITY2 assumes a value greater than zero with no subsequent reversal to below zero. The year of transition to democracy in Iceland is the year in which the country became an independent Republic.

**Table 2. Malapportionment and Transition to Democracy in Latin America**

| Country | Transition to democracy | Malapportionment at transition to democracy | Malapportionment today (in 2000) |
|---|---|---|---|
| Argentina | 1983 | 0.15 (1985) | 0.14 |
| Bolivia | 1982 | 0.23 (1985) | 0.17 |
| Brazil | 1985 | 0.10 (1985) | 0.09 |
| Chile | 1989 | 0.15 (1990) | 0.15 |
| Colombia | 1957 | 0.15 (1960) | 0.13 |
| Ecuador | 1979 | 0.15 (1980) | 0.20 |
| Honduras | 1980 | 0.07 (1980) | 0.04 |
| Peru | 1993 | 0 (1995) | 0 |
| Uruguay | 1985 | 0.07 (1985) | 0.03 |
| Venezuela | 1958 | 0.06 (1958) | 0.07 |

SOURCES: Snyder and Samuels (2004) for the measure of malapportionment and POLITY IV database for coding transition to the democracy. Transition to democracy defined as the first year where the variable POLITY2 assumes a value greater than zero with no subsequent reversal to below zero.

## Table 3. Malapportionment and Democratic Consolidation

| | Dependent variable: Polity 2 Measure of Democracy | |
|---|---|---|
| | (1) | (2) |
| Democracy$_{t-1}$ | 0.751*** | 0.281* |
| | (0.104) | (0.172) |
| Log Malapportionment $_{t-1}$ | 0.033* | 0.260*** |
| | (0.016) | (0.089) |
| Log GDP per capita $_{t-1}$ | 0.153*** | 0.145 |
| | (0.020) | (0.152) |
| | | |
| Observations | 103 | 103 |
| R-squared | 0.879 | - |
| Implied Cumulative Effect of Malapportionment | 0.12 [0.15] | 0.36 [0.02] |
| Estimation Method | OLS | GMM |
| AR (2) (p-value) | | 0.770 |
| Sargan test (p-value) | | 0.108 |

NOTES: Data covers 11 Latin American countries from 1870 to 2000, over 5 year intervals. The OLS regression in Column (1) includes country and year fixed effects and has the error term clustered at the country level. Specification in Column (2) includes year fixed effects. The instruments for income and malapportionment in the first differenced equation are the lags of these variables. In both columns, the implied cumulative effect of malapportionment is the coefficient estimate of malapportionment divided by 1 minus the coefficient on lag democracy. The p-value from a non-linear test of the significance of this coefficient is in square brackets. Robust standard errors in parenthesis. Significance levels: * 10%  ** 5%  ***1%

## Table 4. Malapportionment and Political Representation of Pre-Democracy Elites

| | Vote share for parties close to pre-democracy regimes | | Share of votes for pre-democracy regimes | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Log(Seats/Pop) | 0.064** | 0.099** | 0.047* | 0.053** |
| | (0.026) | (0.041) | (0.026) | (0.021) |
| | | | | |
| Country dummies | Yes | Yes | Yes | No |
| Controls | No | Yes | Yes | Yes |
| | | | | |
| Observations | 118 | 118 | 70 | 70 |
| R-squared | 0.47 | 0.56 | 0.60 | 0.45 |

NOTES: The sample in Columns 1 and 2 include regional data for Bolivia, Brazil, Chile, Mexico, Paraguay, and Uruguay. The vote share, as well as the measure of over- or under-presentation, is for the first election after transition to democracy in these countries. Columns 3 and 4 include data from Brazil, Chile, and Mexico. Robust standard errors in parenthesis. Significance levels: * 10%  ** 5%  ***1%.

**Table 5. Overrepresentation and Political Competition**

| | Dependent variable: | | | |
|---|---|---|---|---|
| | Herfindahl-Hirschmann (HH) index of political concentration | | HH index times share of votes for parties close to former dictatorships | |
| | (1) | (2) | (3) | (4) |
| Log(Seats/Pop) | 0.016*** | 0.006 | 0.028** | 0.040** |
| | (0.007) | (0.007) | (0.013) | (0.017) |
| | | | | |
| Controls | No | Yes | No | Yes |
| | | | | |
| Countries | 12 | 12 | 6 | 6 |
| Observations | 236 | 236 | 118 | 118 |
| R-squared | 0.74 | 0.76 | 0.44 | 0.53 |

OLS regressions with country fixed effects and robust standard errors in parenthesis. Regressions include the following region level controls: landlocked dummy, average yearly temperature and temperature squared, total yearly rainfall and rainfall squared, altitude and altitude squared (for sources see Bruhn & Gallego, 2008). Significance levels: * 10% ** 5% ***1%.

**Table 6. Over-Representation and Transfers**

| | Dependent variable: | | | |
|---|---|---|---|---|
| | Total transfers per capita | Transfers per capita to sub-national governments | Log Gini index | Log GDP per capita |
| | (1) | (2) | (3) | (4) |
| Log(Seats/Pop) | 0.482*** | 0.558*** | 0.007 | 0.011 |
| | (0.056) | (0.063) | (0.014) | (0.057) |
| | | | | |
| Countries | 12 | 9 | 9 | 12 |
| Observations | 229 | 176 | 167 | 229 |
| R-squared | 0.99 | 0.99 | 0.75 | 0.50 |

NOTES: OLS regressions with country fixed effects and robust standard errors in parenthesis. Regressions include the following region level controls: landlocked dummy, average yearly temperature and temperature squared, total yearly rainfall and rainfall squared, altitude and altitude squared (for sources see Bruhn & Gallego, 2008). The variable "total transfers" includes all transfers that the central government made to a region, including transfers to state and municipal governments, social transfers, direct expenditures and investment by the central government, as well as transfers to public universities, whenever available. The categories included vary from country to country, depending on availability. A more uniform variable is "transfers to sub-national government" which includes only transfers to state and/or municipal governments within a region. It is missing for Chile, Ecuador, and Honduras because data on transfers to state or municipal governments were unavailable or because total transfers were not available broken down into sub-categories. Significance levels: * 10% ** 5% ***1%.

# CHAPTER 2

## PUBLIC SPENDING AND ORGANIZED CRIME:
## THE CASE OF THE 1997 MARCHE AND UMBRIA EARTHQUAKE

## 2.1    Introduction

The persistence of organized crime is a feature of most southern Italian regions. Criminal organizations such as the Mafia, Camorra and 'Ndragheta have systematically grown over the last fifty years and massively entered the political and economic environment. This, arguably, is one of the reasons for the social and economic backwardness of the South of Italy. In this paper we focus on the role of public spending to explain the level of criminal activity. Transfers from higher levels of government can increase the incentives for politicians, public officials and entrepreneurs to engage in rent seeking activities. Competition for rents can also assume legal forms, but under some circumstances, it can take the form of bribery, corruption, extortion, black markets and, therefore, promote criminal activity. Since public transfers are a key policy instrument in fighting underdevelopment it is crucial to analyze their effect on political and economic outcomes.

It is very difficult, though, to provide a systematic assessment of the impact of public transfers on different measures of criminal performance, since they are endogenous and rarely randomly distributed. To address this issue we use the earthquake that affected two regions, Marche and Umbria, in the Center of Italy in order to identify exogenous variation in public spending. Anecdotal evidence suggests that in southern Italy, public transfers from the central government triggered the collusion

between politicians and organized crime or entrepreneurs. Documents from official Commissions of the Italian Parliament report the infiltration of organized crime in certain areas of the South after the inflow of money for reconstruction in aftermath of the 1980 Irpinia earthquake (see, also, Barbagallo, 1999).

In the case of Marche and Umbria, the question we ask is: did the six billion euro that flooded a region of 820.000 inhabitants increase the scope for rent seeking and foster organized crime' s activities? The earthquake which hit Umbria and Marche on September 26, 1997 was particularly violent, reaching a magnitude of nine degrees on the Mercalli scale. The whole region was, indeed, declared damaged. There is some anecdotal evidence given by the population, reporting observable changes since 1997 in everyday life such as increasing fights, arsons and bombing attempts ("Dossier Umbria", Libera 2008). In fact, in February of 2008 a special unit of Carabinieri, R.O.S., imprisoned, as a precautionary measure, 57 people charged of affiliation to the Mafia, grand larceny, extortions and other offenses. The inquiry documented an association between the "Casalesi", a clan of the Camorra, and the Morabito -- Palmara - Bruzzaniti, a clan of the 'Ndragheta, that operate jointly in several of Umbria' s economic sectors, particularly in housing and construction. These families are suspected to exert their influence in public procurement contracts and public auctions. Additional evidence, directly related to our findings, describes the role that the organized crime played in Umbria's reconstruction phase. Several confessions made by Mafiosi turned into informants and investigations done by the Sicilian and Umbria judiciary, reported the willingness of Mafia bosses, as Provenzano and Lo Piccolo, to establish their illegal activities in Umbria. In 2000 and in 2007 respectively, three entrepreneurs operating in the Umbria building

sector were arrested and incriminated of several offences as among others, affiliation to the Mafia, arsons, extortions, manipulation of public contract and money laundering. According to the inquiry, they established enterprises in Umbria right after the earthquake and took part in the reconstruction on behalf of Brancaccio and Lo Piccolo clan. In fact, this is consistent with a statement of Baldassarre di Maggio, ex mafioso, describing the mafia's role in organizing bid rigging in construction: "we kept 5%; 3% was for the Mafia, and 2% was for a middle man entrepreneur to pay the politicians as part of the overall agreement". In line with this evidence, we view the Mafia as a type of entrepreneur (see, for instance, Arlacchi, 2007) ready to move where profitable economic opportunities arise. Since the Mafia is considered to be specialized in building activity, we expect it to respond more to opportunities in this sector.

Preliminary results we have obtained so far by using a panel data set of the regions of the Center of Italy for the period 1993 – 2003, show a positive association between public spending in construction and the activities of organized crime. This result is robust to the inclusion of time varying covariates such as unemployment, income and educational attainment rates. The same exercises produce no correlation between public spending and violent crime activity. These results are consistent with our *a priori* expectations that the relevant flow of public capital in the earthquake's aftermath have represented a big economic resource that several actors started to compete for. As a further step we intend to use corruption data to address the relationship between the Mafia and the political sector, and to collect data on the efficiency of judicial institutions since the quality of the judiciary affects the extent to which public spending can foster criminal activities.

This paper is related to two strands of literature. The first one explores the link between targeted public funds from the central government and the increase in rent seeking practices. A large debate has focused on the effects of public transfers on several political and economic outcomes. Tanzi and Davoodi (1997) present cross country evidence showing a positive correlation between corruption and public investment. They also find that higher corruption leads to a lower quality of public infrastructure and reduces the productivity of public investments. Public financing can be particularly harmful in countries with high corruption and weak institutional controls. With a more specific focus on the theory of rent seeking, Murphy et al. (1991) show how an increase in the number of rent seekers, lowering returns to both rent seeking and entrepreneurship, can crowd out entrepreneurship and induce more people to engage in rent seeking. Assuming increasing returns for rent seeking, they identify a mechanism which can explain its high self sustaining levels. For this reason some countries are trapped in a bad equilibrium characterized by a high level of rent seeking and low economic growth, despite the large amounts of funds they receive. The authors distinguish between private and public rent seeking activities. The private one takes the form of theft, piracy, litigation and usually consists of transfers among private parties of capital and land output that affects the productive sector of the economy. On the other hand, public rent seeking attacks the innovative sector of the market. This emerges when a bureaucrat is able to affect the fortunes of private parties and can take the form of bribing, corruption and lobbying. Under some circumstances, rent seeking translates into violent activities such as extortion, expropriation, murder, and as long as an offense generates a demand for defense, this creates scope for the emergence of criminal organizations and criminal behavior in general.

In our study we take the same perspective of Murphy et al. (1991) considering organized crime as a violent expression of rent seeking. If organized criminal groups compete for rents with other economic and political actors, we can expect an increase in their criminal activity as the scope for rents becomes higher. In our study we offer suggestive empirical evidence about the link between exogenously variation in public transfers and organized crime. Surprisingly we find a positive correlation in an area with strong institutions and civic values which should fuel the productive use of public funds. A careful comparative analysis needs to be done to understand how these elements affect the appearance of organized crime.

In the empirical section we consider also alternative explanations for the increase in organized criminal activities. In particular, we test the classic Becker's (1968) crime equation from theory on criminal behavior. Becker analyzes the determinants of criminal behavior using the logic of individual rational choice. He shows that agents become involved in illegal activities whenever the marginal returns from crime exceed the marginal returns from legal occupations and once the expected level of conviction and the severity of punishment are taken into account. The amount of crime is determined not only by individual preferences but also by the socio economic environment created by public policies, such as spending on police, severity of convictions for different crimes and opportunities for employment. According to Becker's theory, crime should reduce if there is an increase in the probability of being caught or in the cost of punishment. There are several factors which can affect costs and gains of crime. Levitt and Venkatesh (1998) emphasize the role of education, which, by improving labor market outcomes, implies a higher opportunity cost for committing crime. In addition, education positively affects individual

preferences and values. Lochner and Moretti (2001), indeed, find that schooling significantly reduces criminal activity. The crime equation has been tested also in the Italian context. Buonanno and Leonida (2006) use a fifteen year regional panel and consider several types of crime against the property and the person. They include three sets of explanatory variables to account for education, deterrence and socioeconomic situation. They use the percentage of crimes committed by unknown offenders to proxy the probability of apprehension. They find that education is significantly and negatively correlated with total and property crimes. The probability of apprehension is significant and with the expected sign for crimes against the property and the person. Marselli and Vannini (1997) also test the crime equation. Their results are in contrast with the predictions of the standard economic model of crime. Using a panel of Italian regions for the period 1980-1999, they find that probability of punishment is more effective as deterrent for crime than the severity of punishment and the efficiency of police. While the rate of unemployment has a positive effect on crime rates, monthly wage rate and level of education do not play any relevant role. In our analysis we do not find strong evidence of the relevance of socioeconomic variables (such as education level, unemployment rates, and income) on the incidence of criminal activities. A possible rationale for these findings is that if higher levels of education, employment and per capita income can increase the opportunity cost to engage in criminal activities, they, on the other hand, create more wealth and consequently larger expected gains from criminal activities such as robbery, kidnappings and extortions.

The paper will continue as follows. In section 2 we describe the data. Section 3 and 4 describe respectively the methodology and the main

results. In section 5 we present some robustness checks. Section 6 concludes.

## 2.2    Data and Background

The panel we use comprises annual observations on the 32 provinces of the regions of Emilia-Romagna, Toscana, Marche, Umbria, Lazio e Abruzzo for the period 1993-2003.

The data source for the two measures of criminal activity we consider in this paper is ISTAT, "*Statistiche Giudiziarie Penali".* Our measure of organized criminal activity (Index of organized crime) is computed by summing up the charges made by the five sectors of the police force to the judiciary for the following crimes: homicide committed by the Mafia, bombing attempts, arson and grand larceny. We compute also a second index where each crime is weighted by the seriousness of the sentence. Values are reported as the incidence of the listed crimes over 100,000 inhabitants. The second measure of criminal activity we use is the index of violent crime. This is computed on the basis of the charges made by the five sectors of the police force to the judiciary for the following crimes: massacre, homicides, infanticide, lesions, sexual assaults, kidnapping, assassination attempts and theft. Values are reported as the incidence of the above mentioned crimes over 100,000 inhabitants for all our measures of criminal activity.

We take public spending data from ANCI (National Association of Italian Municipalities) and we consider total expenses and expenses in capital accounts. All values are per capita and in real term. Other data we use are unemployment rate, secondary school enrolment from ISTAT and gdp per capita provided by Istituto Tagliacarne. Finally we use data on the

intensity of the Umbria-Marche earthquake for each province that was hit as measured on the Mercalli scale, weighted by the population of each municipality interested by this natural disaster.

As it will be clear in the section that describes the methodology, we collapse the time series dimension into two periods (pre and post earthquake) and, therefore, we consider only the average value of each variable for the pre and post period.

Table 1 reports the descriptive statistics for all the provinces in our sample.

We focus on this sample of regions since our aim is to study whether the big inflow of public money in the Umbria and Marche earthquake aftermath has increased the activity of organized crime.

The Umbria and Marche earthquake was violent and disastrous. It caused extensive damages: 3.687 houses were evacuated, 22.000 private buildings and 1.336 public buildings ruined, 461 infrastructures such as hospitals, schools, roads and universities damaged and 213 hydro geological disorders took place. The homeless amounted to 25.000 and 76 cities were hit. The total amount of estimated damages is 8 billion euro. The relief program was organized in two phases according to the priority of interventions; the first (1998-2001) was aimed at the reconstruction of private housing and involved mainly homeless and the most damaged buildings. The second phase (2002-2008) consisted of interventions toward building less damaged and toward houses where families do not live permanently.

At first glance Figures 1-2 seem to suggest a positive association over time between our two measures of public spending per capita (in 1,000 of Euros) and the level of organized crime in our treated group (the provinces of Umbria and Marche).

Moreover, we can notice the relevant increase of public expenses after the year of the earthquake (1997).

## 2.3    Methodology

In this section we describe the methodological approach that we adopt to test whether an increase in public spending could boost the activity of organized crime.

Consider the following linear model that will be at the basis of our empirical analysis:

$$organiz. \; crime_{it} = a + \beta \; spending_{it} + \mathbf{x}_{it}' \gamma + \delta_i + \mu_t + \varepsilon_{it} \qquad (1)$$

where $organiz. \; crime_{it}$ is the measure of criminal activity in province $i$ at time $t$, $spending_{it}$ denotes public spending per capita, $\mathbf{x}_{it}$ a vector of potential time varying covariates and $\varepsilon_{it}$ is a random error term. The model also includes a full set of province fixed effects and a full set of time fixed effects to capture unobserved shocks (time trend) common to all the provinces in our sample. The coefficient of main interest is $\beta$.

Ordinary least-squares (OLS) estimates of equation (1) will not be consistent if Cov($spending_{it}, \varepsilon_{it}$)≠0. This can be the case in the presence of a reverse effect of organized crime on public spending or of omitted time varying factors that affect both public spending and the intensity of criminal activities. We, therefore, rely on an instrumental variable approach in order to identify the impact of an increase in public spending on the spread of organized crime. We use the strong earthquake that hit the provinces of Umbria and Marche in September 1997 as a source of exogenous variation in public spending per capita.

In specific, we posit the following relationship between public spending per capita  and the occurrence  of the earthquake as the first stage equation of the instrumental variable approach:

$$spending_{it} = a + \zeta\, earthquake_{it} + x_{it}{}'\, \pi + \delta_i + \mu_t + v_{it} \qquad (2)$$

where $spending_{it}$ is public spending per capita in province $i$ at time $t$,. $x_{it}$ a vector of potential time varying covariates, $\delta_i$ and $\mu_t$ denote respectively a full set of province and time fixed effects and $v_{it}$ is a random error term. The covariate $earthquake_{it}$ is our treatment variable. In our baseline specifications, this variable is a dummy denoting the provinces of Umbria and Marche affected by the earthquake for the years after the earthquake (from 1998 onward). We also provide estimates where the variable $earthquake_{it}$ is our province level measure of the earthquake's magnitude for the provinces hit by the earthquake after 1997 e 0 otherwise. The coefficient of main interest is $\zeta$.

The sample is comprised of the six provinces of the regions of Umbria and Marche hit by the 1997 earthquake and the twenty-six provinces of the neighboring regions of Emilia Romagna, Toscana, Lazio and Abruzzo (control group). We choose to use as control group the provinces belonging only to the neighboring regions of Marche and Umbria in order to guarantee a higher degree of homogeneity between the treated and control groups in terms of their economic, cultural and social characteristics. The years to which our analysis refers, span from 1993 to 2003.

The validity of our empirical strategies and the consistency of the instrumental variable estimates rely on two assumptions: a) the instrument $earthquake_{it}$ has to be strongly correlated with the explanatory variable of interest in the first stage equation, which is to say the

coefficient $\zeta$ in the equation (2) must be statistically different from zero or, equivalently, cov($earthquake_{it}$,$spending_{it}$)$\neq$0; b) the instrument $earthquake_{it}$ should have no partial effects on our indicator of organized crime and should not be correlated to unobserved time – varying factors that influence the spreading of organized crime, which is to say cov($earthquake_{it}$,$\varepsilon_{it}$)=0 where $\varepsilon_{it}$ is the error term in the second – stage equation (1).

While the first assumption can be statistically tested and its validity in the context of our analysis is discussed in the subsequent section, the plausibility of the second one cannot be empirically assessed. In some of our estimating equations we include a set of time – varying covariates, like unemployment rate, gdp per capita and secondary school enrolment rate. These variables represent potential relevant omitted factors which might influence the spread of organized crime and, at the same time, be affected by the occurrence of a disastrous earthquake.

Communities hit by major earthquakes might also experience a change in their cultural attitudes and in the quality of the overall institutional framework (see, for instance, Pereira, 2006). This, in turn, may affect the spreading of organized crime. Institutions and civic norms tend, nevertheless, to show a high degree of resilience over time (see, among the others, Acemoglu, Johnson and Robinson, 2001, and Tabellini, 2008). Even if the extent to which institutions and cultural attitudes might adapt to major external shocks is an issue that deserves further empirical analysis, we think that the short length of the post-treatment period that we consider (1998 – 2003) makes it less likely that institutional and cultural evolution is in the short run a relevant omitted phenomenon eventually correlated to the occurrence of the Umbria and Marche' earthquake.

We also expect that our dependent variables are characterized by a high positive serial correlation. To deal further with this issue, we use as a simple remedy a strategy suggested by Bertrand, Duflo and Mullainathan (2004). We take the average of our variables before and after the occurrence of the earthquake and estimate equations (1) and (2) by two stage least-squares in a panel dataset with time dimension equal to two.

## 2.4 Main results

### 2.4.1 OLS estimates

Tables 2 and 3 report OLS estimates of equation (1). Our variable *spending$_{it}$* is measured as province level expenses per capita in capital account in Table 2 and as total expenses (current and in capital account) in Table 3. In both Tables, in columns 1 -3 the dependent variable is the simple incidence over 100,000 inhabitants of our organized crime indicator. In columns 4 – 6 of both Tables the dependent variable is our second index of organized crime in which each included crime receives a weight equal to the maximum number of years you can be sentenced to.

Columns 1 and 4 provide baseline results from models where the only covariate is public spending per capita. In columns 2 and 5 we report estimates from models in which we include as additional covariates the unemployment rate and the log of gdp per capita. The estimating equations of Columns 3 and 6, finally, include also the secondary school enrolment rate as additional control variable.

The OLS estimates of our crime equation (1) show a positive but not statistically significant association between our measures of public

spending and the two indices of organized crime. The results reported in Tables 2 and 3 should, nevertheless, be interpreted with a word of caution. Our measures of public spending are very broad and refer to a large variety of areas of intervention which public spending can be addressed to. If public spending, in particular in the sector of construction, may actually favor the spreading of organized crime, other forms of government intervention, like spending in schooling, in public security or in assistance to socially marginalized people, might reduce the breeding grounds of organized crime. We cannot say which of these effects prevails. By relying on the "natural experiment" of the earthquake, we think to be able to better identify that variation in public spending that involves more the sector of the construction.

Moreover, if citizens expect higher public spending to attract organized crime, they might be more careful in choosing politicians less prone to collusion with organized crime and at monitoring their activity. This mechanism might be of particular relevance in the regions of the center of Italy that we consider in our analysis, since they have not been affected by an endemic presence of organized crime.

Finally, we find no evidence of the relevance of socioeconomic variables (unemployment rate, income per capita and educational attainment) on the incidence of the organized crime activity. A possible rationale is that if higher levels of education, employment and per capita income increase the opportunity cost to engage in criminal activities, they, on the other hand, create more available wealth and larger expected gains from engaging in criminal activities.

As discussed in the previous session, we nevertheless expect that OLS estimates are not consistent. In the following section, we report the results we obtain from our instrumental variable strategy.

## 2.4.2 Reduced forms and instrumental variable estimates

In order to understand whether the treated provinces experienced an increase in organized crime activity after the treatment year we estimate the following specification:

$$organiz.\ crime_{it} = a + \beta\ earthquake_{it} + \textbf{\textit{x}}_{it}'\ \gamma + \delta_i + \mu_t + v_{it} \qquad (3)$$

In Figure 3 we plot the index of organized crime for the treatment and the control group to study whether after the year of the earthquake treated provinces have experienced an increase in organized crime compared to the control group. Looking at the graph one can notice that over the whole period, the level of organized crime in the control group is higher than the one in the treatment. However, the difference substantially shrinks after 1997. This is consistent also with results reported in Table 4. In the table we see that control and treatment groups are similar in all the variables we consider before and after the earthquake, except for the two organized crime measures. Consistent with what we observed in the previous graph, it appears that the difference in organized crime between control and treatment groups, despite being positive and significant in the pre period, becomes smaller and not significant in the post period.

Reduced form results are reported in Tables 5 and 6, where we use both the treatment variables as previously defined. The coefficient of interests $\beta$ is always positive and statistically significant. When we consider the magnitude of the earthquake as treatment variable, it remains statistically significant. The magnitude of the coefficients are stable to the inclusion of additional control variables. These results confirm a positive association between the occurrence of the earthquake

and the increase in organized crime activity. To have an idea of the size of the effect, by looking at column 2 in Table 5, we find that the occurrence of the earthquake is associated to an increase in organized crime activity of about 2.4 offences over 100,000 inhabitants.

Finally we run a placebo regression to be confident that our reduced forms results are not driven by some omitted phenomenon associated with the year of the earthquake. In Table 7 we report the results for the placebo where we change the treatment year to be 1996. The coefficient of interest is always not statistically different from zero. We do not show the results for when we use the magnitude of the earthquake since they are similar to the one displayed.

In Tables 8 and 9 we report the results we obtain by estimating equations (1) and (2) by two stage least-squares. Our variable $spending_{it}$ is measured as province level expenses per capital in capital account in Table 8 and as total expenses (current and in capital account) in Table 9. In columns 1 – 3 and 4 -6 of both Tables, the dependent variable of the second stage equation is, respectively, the simple and weighted indicator of organized crime.

Panels at the bottom of both the Tables report first stage estimates of the coefficient $\zeta$ of the dummy variable $earthquake_{it}$ in equation (2). Results displayed in both Tables show a strong and statistically significant first stage relationship between the occurrence of the earthquake and the increase in public spending per capita. The occurrence of the earthquake increased on average capital spending per capita and total expenditure per capita, respectively, by 271 – 277 Euros and 373 – 381 Euros, depending on the different specifications. Second stage estimates suggest a positive and statistically significant association between public spending per capita (measured as both capital and total

expenses) and our two indices of organized crime. The magnitude of the coefficients and the precision of the estimates are fairly stable to the inclusion of additional control variables. Estimates from Table 8 suggest that on average a positive change of 100 Euros in the capital expenses per capita induced an increase of almost 1 unit in the incidence over 100,000 inhabitants of the number of the crimes related to the Mafia.

In the specifications of Tables 10 and 11 we use the average magnitude of the earthquake at province level as an instrument for public spending. First stage results are again strong and show a positive and statistically significant relationship between our instrument and the two measures of public spending per capita. The estimated effect of public spending per capita on organized criminal activity is also positive and significant at a conventional level. The estimated magnitude of the coefficients of the two measures of public spending is fairly similar to the one we obtain by using the simple dummy variable as instrument.

## 2.5    Robustness

In this section we perform additional econometric exercises as robustness checks for the main results that we have so far discussed. Specifically, we consider whether our estimates might be simply capturing a more general spread of violent criminal activities in the areas affected by the earthquake due, for instance, to the social and economic disruption caused by such a natural disaster.

We report estimates from a reduced form similar to the equation (3) except for considering as dependent variable, the indicator of the incidence of violent criminal activities. The results which are displayed in Table 12 show a negative, albeit not statistically significant, association

between the dummy variable *earthquake$_{it}$* and our index of violent crime. We obtain very similar results when we use the average intensity of the earthquake at province level as an independent variable. These findings alleviate the concern that our results might simply reflect a wider increase of crime in the provinces affected by the earthquake.

In Tables 13 we report two stage least-squares estimates of equation (1) and (2) with the index of violent crime as the dependent variable in the second stage regression. There are no clear reasons to expect that a higher influx of public money for the reconstruction in the aftermath of an earthquake should produce a spreading of other kinds of violent crimes. If this were our finding, this would cast serious doubt on the reliability of our results. The estimates we obtain by our instrumental variable strategy show a negative, albeit not statistically significant, association between increase in public spending (measured by both capital and total expenses) and the incidence of violent crimes. Very similar results are obtained by using the average intensity of the earthquake at province level rather than the simple dummy variable as an instrument.

The robustness results, therefore, support our findings that an increased influx of public spending for reconstruction is conducive to the expansion of Mafia activity.

## 2.6    Conclusion

The Mafia and the other forms of organized crime that affect the Southern regions of Italy can be thought of as criminal entrepreneurs that try to take advantage of profitable business opportunities whenever and wherever available. The presence of lucrative opportunities often induce the Mafia to expand its activities beyond the geographical areas

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characterized by its endemic presence and beyond its "traditional" core business such as traffic of arms, drugs and extortions.

Preliminary evidence that we provide from the reconstruction in the aftermath of the 1997 earthquake that hit two regions in the center of Italy, Marche and Umbria, is in line with this reasoning. Marche and Umbria are two areas of Italy that did not experience an endemic presence of Mafia. Nevertheless the relevant influx of money for their reconstruction seems to have induced an increase in the presence and activity of criminal organizations.

We plan to extend our study in several ways. We also want to assess whether a similar phenomenon occurred in the aftermath of other major earthquakes that have hit Italy. An interesting and related issue that we plan to explore further concerns the consequences of the spread of organized crime on the quality of politicians.

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**Table 1. Descriptive statistics**

| Variables | Mean |
|---|---|
| Organized Crime | 19.231 (6.841) |
| Organized Crime (Weighted) | 2.140 (0.663) |
| Violent Crime | 81.245 (34.869) |
| Capital Exp. pc | 323.975 (121.828) |
| Total Exp. pc | 1244.758 (228.780) |
| Unemployment | 7.395 (2.772) |
| Log gdp pc | 9.677 (0.165) |
| School | 0.888 (0.084) |
| Observations | 64 |

Standard deviations in parentheses.

**Figure 1.**



**Figure 2.**

**Table 2. Organized Crime and Capital Expenses per capita.**

| | DEPENDENT VARIABLES | | | | | |
|---|---|---|---|---|---|---|
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Capital Exp. pc | 0.0038 | 0.0040 | 0.0042 | 0.0004 | 0.0004 | 0.0004 |
| | (0.0033) | (0.0031) | (0.0033) | (0.0004) | (0.0004) | (0.0004) |
| Unemployment | | -0.5265 | -0.4773 | | -0.0477 | -0.0518 |
| | | (0.6480) | (0.6928) | | (0.0625) | (0.0636) |
| Log gdp pc | | 2.0214 | 2.3591 | | 2.1703 | 2.1420 |
| | | (29.8626) | (29.7641) | | (2.9246) | (2.9533) |
| School | | | 12.2835 | | | -1.0294 |
| | | | (17.9426) | | | (1.5102) |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.887 | 0.889 | 0.890 | 0.881 | 0.885 | 0.886 |

OLS regressions with time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

**Table 3. Organized Crime and Total Expenses per capita.**

| | DEPENDENT VARIABLES | | | | | |
|---|---|---|---|---|---|---|
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Total Exp. pc | 0.0017 | 0.0018 | 0.0020 | 0.0001 | 0.0001 | 0.0001 |
| | (0.0021) | (0.0020) | (0.0021) | (0.0002) | (0.0002) | (0.0002) |
| Unemployment | | -0.5022 | -0.4538 | | -0.0449 | -0.0493 |
| | | (0.6713) | (0.7176) | | (0.0648) | (0.0659) |
| Log gdp pc | | 5.3635 | 5.8118 | | 2.4896 | 2.4489 |
| | | (30.2876) | (30.2030) | | (2.9282) | (2.9528) |
| School | | | 11.9320 | | | -1.0857 |
| | | | (17.9955) | | | (1.4479) |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.886 | 0.888 | 0.889 | 0.879 | 0.883 | 0.884 |

OLS regressions with time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

**Figure 3. Organized Crime Index (treated and control groups). 1993 – 2003.**

**Table 4. Treated and control provinces before and after the earthquake**

| | Treated Provinces Mean | Non-Treated Provinces Mean | Mean difference (p-value) |
|---|---|---|---|
| | (1) | (2) | (3) |
| **_Panel A. Province Characteristics Before 1998_** | | | |
| Organized crime | 12.436 | 18.862 | 0.063 |
| | (2.441) | (7.993) | |
| Organized Crime (Weighted) | 1.413 | 2.02 | 0.051 |
| | (0.273) | (0.714) | |
| Violent crime | 56.543 | 69.555 | 0.386 |
| | (20.975) | (34.543) | |
| Capital exp. pc | 270.644 | 272.722 | 0.939 |
| | (65.213) | (59.003) | |
| Total exp. pc | 1081.615 | 1202.725 | 0.223 |
| | (279.932) | (199.712) | |
| Unemployment | 7.419 | 8.448 | 0.412 |
| | (1.167) | (2.947) | |
| Log gdp pc | 9.600 | 9.650 | 0.491 |
| | (0.063) | (0.174) | |
| School | 0.873 | 0.838 | 0.258 |
| | (0.033) | (0.017) | |
| **_Panel B. Province Characteristics After 1997_** | | | |
| Organized crime | 17.558 | 21.552 | 0.114 |
| | (2.283) | (5.855) | |
| Organized Crime (Weighted) | 2.159 | 2.422 | 0.295 |
| | (0.305) | (0.582) | |
| Violent crime | 81.827 | 98.501 | 0.250 |
| | (18.663) | (33.387) | |
| Capital exp. pc | 594.136 | 325.190 | 0.000 |
| | (209.4033) | (58.026) | |
| Total exp. pc | 1514.937 | 1262.091 | 0.010 |
| | (249.101) | (194.609) | |
| Unemployment | 5.644 | 6.739 | 0.358 |
| | (1.138) | (2.793) | |
| Log gdp pc | 9.670 | 9.721 | 0.504 |
| | (0.065) | (0.180) | |
| School | 0.946 | 0.927 | 0.594 |
| | (0.040) | (0.084) | |
| N. Obs. | 6 | 26 | |

Standard deviations in parentheses in columns (1) and (2).

**Table 5. Reduced form. Organized Crime and Earthquake.**

| | DEPENDENT VARIABLES | | | | | |
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Earthquake Dummy | 2.431* | 2.402* | 2.667* | 0.343** | 0.341** | 0.335* |
| | (1.303) | (1.306) | (1.454) | (0.157) | (0.161) | (0.173) |
| Unemployment | | -0.472 | -0.400 | | -0.042 | -0.043 |
| | | (0.658) | (0.708) | | (0.061) | (0.063) |
| Log gdp pc | | 5.762 | 6.437 | | 2.547 | 2.529 |
| | | (30.829) | (30.646) | | (2.917) | (2.954) |
| School | | | 16.795 | | | -0.425 |
| | | | (19.434) | | | (1.638) |
| | | | | | | |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.890 | 0.892 | 0.894 | 0.889 | 0.893 | 0.893 |

OLS regressions with time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

**Table 6. Reduced form. Organized Crime and Earthquake.**

| | DEPENDENT VARIABLES | | | | | |
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Earthquake Magnitude | 0.465* | 0.460* | 0.515* | 0.065** | 0.065** | 0.064* |
| | (0.238) | (0.241) | (0.269) | (0.029) | (0.030) | (0.032) |
| Unemployment | | -0.463 | -0.389 | | -0.041 | -0.042 |
| | | (0.659) | (0.710) | | (0.061) | (0.063) |
| Log gdp pc | | 6.694 | 7.504 | | 2.678 | 2.661 |
| | | (30.740) | (30.504) | | (2.893) | (2.931) |
| School | | | 17.288 | | | -0.372 |
| | | | (19.526) | | | (1.647) |
| | | | | | | |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.890 | 0.892 | 0.894 | 0.889 | 0.893 | 0.893 |

OLS regressions with time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

**Table 7. Placebo Reduced Forms**

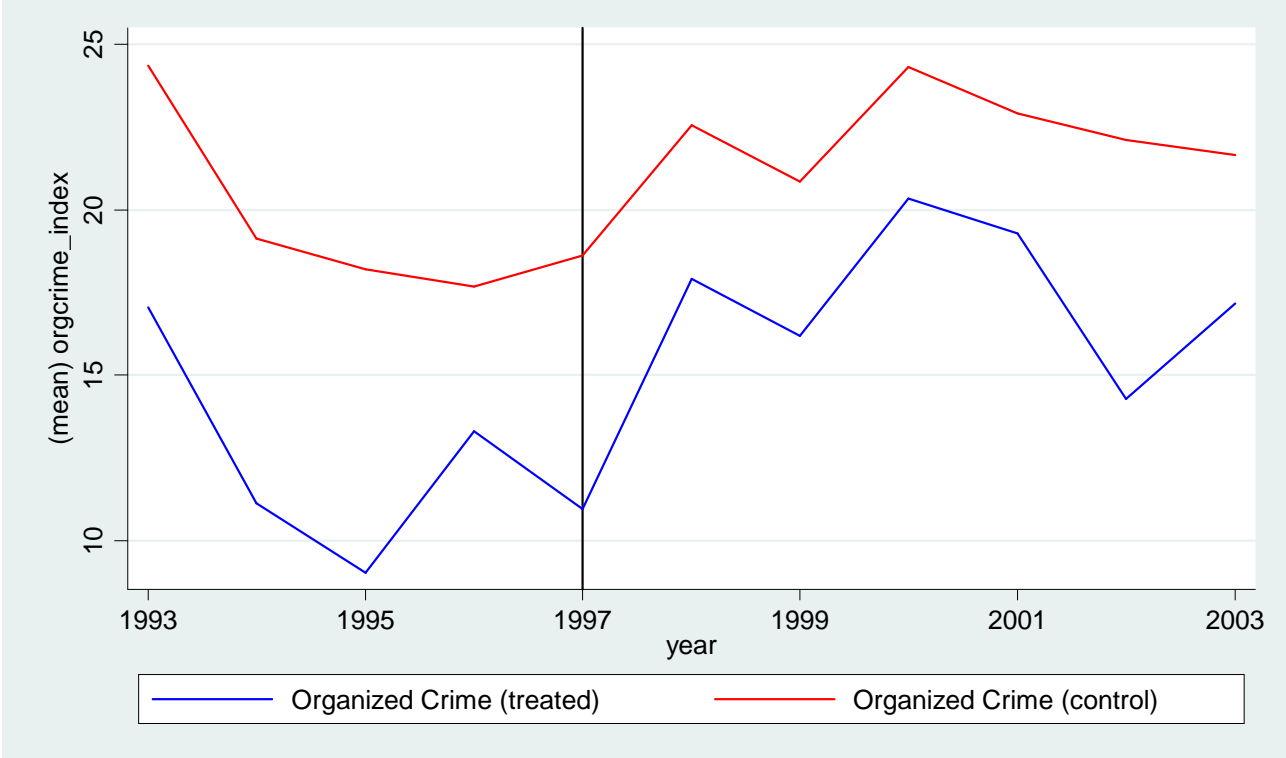| | DEPENDENT VARIABLES | | | | | |
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Earthquake Dummy | 1.585 | 1.568 | 1.634 | 0.216 | 0.212 | 0.216 |
| | (1.594) | (1.607) | (1.656) | (0.200) | (0.201) | (0.209) |
| Unemployment | | -0.440 | -0.423 | | -0.063 | -0.061 |
| | | (0.695) | (0.731) | | (0.062) | (0.065) |
| Log gdp pc | | 9.772 | 9.403 | | 2.069 | 2.046 |
| | | (30.108) | (30.714) | | (2.516) | (2.602) |
| School | | | 4.314 | | | 0.267 |
| | | | (21.392) | | | (1.599) |
| | | | | | | |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.880 | 0.882 | 0.882 | 0.873 | 0.878 | 0.878 |

OLS regressions with time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

**Table 8. Capital expenses per capita and Organized Crime. Two stage least-squares estimates.**

| | DEPENDENT VARIABLES | | | | | |
| --- | --- | --- | --- | --- | --- | --- |
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Capital Exp. pc | 0.009* | 0.009** | 0.010** | 0.001** | 0.001** | 0.001** |
| | (0.005) | (0.004) | (0.005) | (0.001) | (0.001) | (0.001) |
| Unemployment | | -0.572 | -0.524 | | -0.056 | -0.059 |
| | | (0.414) | (0.434) | | (0.040) | (0.040) |
| Log gdp pc | | -1.955 | -2.099 | | 1.449 | 1.458 |
| | | (20.290) | (19.998) | | (2.046) | (2.032) |
| School | | | 13.652 | | | -0.819 |
| | | | (11.844) | | | (1.108) |
| | First Stage | | | | | |
| Earthquake Dummy | 271.023** | 272.094** | 277.257** | 271.023** | 272.094** | 277.257** |
| | (99.821) | (103.782) | (103.744) | (99.821) | (103.782) | (103.744) |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.883 | 0.886 | 0.886 | 0.870 | 0.874 | 0.876 |

Two-stage least squares estimates. All the specifications include time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

**Table 9. Total expenses per capita and Organized Crime. Two stage least-squares estimates.**

| | DEPENDENT VARIABLES | | | | | |
| --- | --- | --- | --- | --- | --- | --- |
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Total Exp. pc | 0.007** | 0.006** | 0.007** | 0.001** | 0.001** | 0.001** |
| | (0.003) | (0.003) | (0.003) | (0.000) | (0.000) | (0.000) |
| Unemployment | | -0.537 | -0.485 | | -0.051 | -0.054 |
| | | (0.434) | (0.459) | | (0.043) | (0.043) |
| Log gdp pc | | 5.371 | 5.887 | | 2.491 | 2.460 |
| | | (20.042) | (19.837) | | (2.071) | (2.033) |
| School | | | 13.732 | | | -0.809 |
| | | | (11.737) | | | (1.016) |
| | First Stage | | | | | |
| Earthquake Dummy | 373.955*** | 374.656*** | 381.58*** | 373.955*** | 374.656*** | 381.58*** |
| | (128.581) | (131.714) | (131.982) | (128.581) | (131.714) | (131.982) |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.880 | 0.883 | 0.883 | 0.861 | 0.867 | 0.869 |

Two-stage least squares estimates. All the specifications include time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

**Table 10. Capital expenses per capita and Organized Crime. Two stage least-squares estimates. Average earthquake magnitude as instrument.**

| | DEPENDENT VARIABLES | | | | | |
| --- | --- | --- | --- | --- | --- | --- |
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Capital Exp. pc | 0.009** | 0.008** | 0.009** | 0.001** | 0.001** | 0.001** |
| | (0.004) | (0.004) | (0.004) | (0.001) | (0.001) | (0.001) |
| Unemployment | | -0.568 | -0.520 | | -0.055 | -0.058 |
| | | (0.415) | (0.435) | | (0.040) | (0.040) |
| Log gdp pc | | -1.561 | -1.709 | | 1.508 | 1.518 |
| | | (20.206) | (19.923) | | (2.038) | (2.023) |
| School | | | 13.532 | | | -0.838 |
| | | | (11.806) | | | (1.090) |
| | First Stage | | | | | |
| Earthquake Magnitude | 54.505*** | 55.021*** | 56.343*** | 54.505*** | 55.021*** | 56.343*** |
| | (16.867) | (17.658) | (17.781) | (16.867) | (17.658) | (17.781) |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.884 | 0.887 | 0.887 | 0.872 | 0.876 | 0.878 |

Two-stage least squares estimates. All the specifications include time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

**Table 11. Capital expenses per capita and Organized Crime. Two stage least-squares estimates. Average earthquake magnitude as instrument.**

| | DEPENDENT VARIABLES | | | | | |
| --- | --- | --- | --- | --- | --- | --- |
| | Organized Crime | | | Organized Crime (weighted) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Total Exp. pc | 0.006** | 0.006** | 0.007** | 0.001** | 0.001** | 0.001** |
| | (0.003) | (0.003) | (0.003) | (0.000) | (0.000) | (0.000) |
| Unemployment | | -0.535 | -0.483 | | -0.051 | -0.054 |
| | | (0.434) | (0.459) | | (0.043) | (0.043) |
| Log gdp pc | | 5.370 | 5.884 | | 2.491 | 2.460 |
| | | (20.022) | (19.803) | | (2.060) | (2.021) |
| School | | | 13.645 | | | -0.824 |
| | | | (11.723) | | | (1.006) |
| | First Stage | | | | | |
| Earthquake Magnitude | 74.372*** | 74.633*** | 76.371*** | 74.372*** | 74.633*** | 76.371*** |
| | (21.158) | (21.859) | (22.036) | (21.158) | (21.859) | (22.036) |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.881 | 0.883 | 0.883 | 0.863 | 0.868 | 0.870 |

Two-stage least squares estimates. All the specifications include time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

**Table 12. Reduced form. Violent Crime and Earthquake.**

| | DEPENDENT VARIABLE Violent Crime | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Earthquake Dummy | -3.662 | -3.505 | -5.461 |
| | (5.450) | (5.351) | (6.316) |
| Unemployment | | 1.257 | 0.733 |
| | | (2.434) | (2.237) |
| Log gdp pc | | 203.798 | 198.823 |
| | | (174.986) | (160.930) |
| School | | | -123.780 |
| | | | (142.110) |
| Observations | 64 | 64 | 64 |
| R-squared | 0.914 | 0.919 | 0.922 |

OLS regressions with time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

**Table 13. Public spending per capita and Violent Crime. Two stage least-squares estimates.**

| | DEPENDENT VARIABLE Violent Crime | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Capital Exp. pc | -0.014 | -0.013 | -0.020 | | | |
| | (0.013) | (0.012) | (0.014) | | | |
| Total Exp. pc | | | | -0.010 | -0.009 | -0.014 |
| | | | | (0.009) | (0.009) | (0.010) |
| Unemployment | | 1.404 | 0.985 | | 1.353 | 0.906 |
| | | (1.549) | (1.385) | | (1.538) | (1.370) |
| Log gdp pc | | 215.061** | 216.301** | | 204.370* | 199.949** |
| | | (109.094) | (98.689) | | (113.010) | (101.612) |
| School | | | -117.345 | | | -117.509 |
| | | | (88.428) | | | (89.145) |
| | First Stage | | | | | |
| Earthquake Dummy | 271.023** | 272.094** | 277.257** | 373.955*** | 374.656*** | 381.58*** |
| | (99.821) | (103.782) | (103.744) | (128.581) | (131.714) | (131.982) |
| Observations | 64 | 64 | 64 | 64 | 64 | 64 |
| R-squared | 0.915 | 0.920 | 0.924 | 0.916 | 0.921 | 0.925 |

Two-stage least squares estimates. All the specifications include time and province fixed effects. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

# CHAPTER 3

# WHICH REFORMS WORK AND UNDER WHAT INSTITUTIONAL ENVIRONMENT: EVIDENCE FROM A NEW DATASET ON STRUCTURAL REFORMS

## 3.1    Introduction

Over the last few decades many countries have experienced remarkable, and in many cases surprising, progress in their economic performance. This coincided with an unprecedented wave of structural reforms including trade and financial liberalization. Although there are many possible driving forces underlying the recent global economic upsurge[1], the apparent co-movement between growth and a broad range of structural reforms deserves renewed attention.

Do reforms promote growth? Which reforms really work? Do institutions supersede policies to explain economic performance? Or does the institutional environment play a role on how effective reforms may be? These questions have been fiercely debated among academics and policy makers for a long time with no much progress in arriving to a satisfactory answer[2]. Testimony to how contentious and divisive discussions have been about the success and failure of certain reforms or packages of reforms is a voluminous literature that emerged by advocates and

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[1] A reasonable concern is that the observed recent growth in resource-rich developing countries is primarily due to hikes in oil and other commodities prices. However, it is still remarkable that growth has been so spectacular in most regions – including sub-Saharan Africa - and countries of the world including non resource-rich countries.

[2] Easterly (2005), for instance, focuses on the association between a large set of economic policies (price distortions, financial development, trade openness and macroeconomic policies) and growth. His baseline growth regression suggests that improvements in these policy dimensions lead to a substantial increase in income per capita growth. However, once the sample is restricted to exclude big outliers any association between policy variables and growth disappears. Rodrik (2005) discusses how policies aimed at promoting economic growth can be highly context specific. Recent literature investigates whether institutions are more relevant than policies to explain country-wide differences in economic performance. Easterly and Levine (2003) ask whether policies such as openness to international trade, inflation and impediments to international transactions matter to explain current differences in income levels. The evidence they provide suggests that macroeconomic policies do not have a big relevance to explain current level of economic development once the impact of the institutions on economic development is taken into account. They argue that bad policies might be "symptoms" of deeper institutional failures. Acemoglu, Johnson, Robinson, and Thaicharoen (2003) reach a similar conclusion. Once the historically determined component of institutions is controlled for, economic policies play a small role to explain economic volatility, crises, and growth. Distortionary policies are likely to be mirroring the existence of weak institutions.

critiques of the "Washington Consensus", a list of policies originally proposed by John Williamson in 1990[3].

In this paper we take a comprehensive look at the debate on the reform – growth nexus. We first ask and empirically test whether structural reforms matter for economic growth. We then investigate to what extent "*key variables"* like a country institutional environment or its distance from the technological frontier influence the association between structural reforms  and economic growth. In carrying out our empirical analysis we employ a newly constructed dataset that includes information about several kinds of structural reforms in both the real and financial sectors of the economy for both industrialized and developing countries over roughly the past thirty years. Three indices of structural reforms in the real sector of the economy measure, respectively, the reduction of public intervention in the *agricultural market*; the degree of liberalization in the *telecommunication and electricity markets* and the extent of openness to the *international trade*. The indicators of structural reforms in the financial sector encompass a broader measure of liberalization in the *domestic financial sector* and two more specific sub-indices that refer to the *banking* and *security* markets. A last set of indicators captures the extent of *external capital account* liberalization. The richness of our data - in terms of the sectors of the economy they refer to and of their time and country coverage - is essential to empirically investigate the different hypothesis about the relationship between reforms and growth that have been suggested by recent literature.

Economic theory suggests that structural reforms should remove obstacles to an efficient allocation of resources, thereby increasing

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[3] Williamson originally coined the phrase in 1990 "to refer to the lowest common denominator of policy advice being addressed by the Washington-based institutions to Latin American countries as of 1989" (Williamson, 2000).

average income levels. At the same time, a perennial challenge for policymakers is finding ways to improve economic performance. This is a difficult and complex task, but there is a general agreement that structural reforms - such as reducing rigidities in product and factor markets, liberalizing capital flows, and freeing international trade - are an important part of an overall strategy for raising incomes and sustaining economic growth. Despite the importance of the issue, the analysis of the effects of reforms has been limited because of the lack of consistent historical data on reforms in many non-OECD economies.

While much is still to be learned about the connection between structural reforms and economic performance, several insights emerge from existing research. First, a wide body of empirical evidence documents that trade liberalization raises the level of real income in an economy, as a result of improvements in efficiency[4]. Furthermore, there seems to be a presumption that trade liberalization also raises an economy's long-run growth rate. Sachs and Warner (1995), for instance, construct a composite index of openness to international trade and find that in the period from 1970 to 1989 open economies experienced an average growth 2.45 percentage points higher than closed economies[5]. Dollar and Kraay (2004) use decade – over – decade variations in volume of trade as a proxy for change in trade policy. Openness to international trade appears to sustain higher income growth rates. Nevertheless, empirical  research has not established a conclusive relationship between

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[4] Frankel and Romer (1999) use country geographical characteristics as instruments for trade shares. Their results suggest that a rise of one percentage point in the ratio of trade to GDP increases per capita income by one-half percent.

[5] Sachs and Warner (1995) define a country open to international trade if none of the following conditions hold: a) nontariff barriers covering 40% or more of trade; b) average tariff rates of 40% or more; c) a black market exchange rate depreciated by 20% or more relative to the official exchange rate; d) a socialist economic system) and e) a state monopoly on major exports. Rodriguez and Rodrik (2000) discuss in depth whether the Sach and Warner' index provide an appropriate measure of openness to international trade.

trade liberalization and economic performance (see Berg and Krueger, 2003, for a survey)[6].

A large literature suggests that a well-developed financial sector promotes economic growth (Levine, 1997, 2005). However, relatively few studies try to assess the impact of financial sector reforms on economic growth. Bekaert, Harvey, and Lundblad's (2005) main measure of financial liberalization is a dummy variable equal to one for the years when foreign investors can own equities of a particular market. Equity market liberalization increases annual real per capita GDP growth by almost 1 percent. Quinn and Toyoda (2008) provide detailed *de jure* measures of capital account and financial current account openness and document that capital account liberalization is positively associated with growth. Finally, recent empirical work provides evidence that structural reforms improve economic performance in advanced economies. Nicoletti and Scarpetta (2003) use an original dataset on product market regulation in eighteen OECD countries. They find that product market reforms that promote private corporate governance, competition, and privatization raise productivity growth. Evidence on the impact of these kinds of structural reforms in emerging market economies does not exist.

Motivated by this literature, we take a broad look at the association between a wide range of structural reforms and economic growth by relying on two empirical approaches. First, we investigate the relationship between reforms and growth spells, meant as long periods of remarkably sustained or slow growth. Second, we discuss first – pass

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[6] Rodrik, Subramanian, and Trebbi (2004), for instance, follow Frankel and Romer (1999) and instrument actual trade/GDP shares by using estimated trade/GDP shares constructed on the basis of gravity equations for bilateral trade flows. Settlers' mortality rates from Acemoglu, Johnson and Robinson (2001) are used as an instrument for the quality of institutions. Their core results suggest that institutions are the main determinants of current differences in economic development, while geography and trade do not have relevant explicative power.

associations between these novel indicators of structural reforms and economic growth based on panel data analysis that controls for country and time fixed effects.

We then move to an empirical assessment of the extent to which country – wide " *key variables*" - like the broad institutional environment or the distance from the technological frontier –influence the impact of structural reforms on economic performance. Acemoglu, Johnson, Querubin, and Robinson (2008) suggest that the degree politicians are accountable for and constrained in their behaviour might affect the impact of reforms on economic outcomes and might help to explain the heterogeneity of their effects. More specifically, their theoretical analysis predicts that reforms have a less relevant impact on economic outcomes in countries with a high or a low level of constraints on the executive power. In countries that place high constraints on the executive power politicians are highly accountable for and constrained in their action, therefore *de facto* distortionary policies are less likely to be implemented to start with. In such environments *de jure* reforms should have a less relevant impact on economic outcomes. On the other hand, in contexts characterized by weak mechanisms to check politicians' behaviour, reforms can be easily *de facto* undermined. Reforms consequently could have a limited and poor impact on economic performance. It is in countries with intermediate levels of constraints on politicians that they expect more room for reforms to be effective. The institutional environment is not sound enough to make bad economic policies rare, but at the same time is not so weak that *de jure* reforms can be easily undermined by groups detaining political power.

A second line of research builds upon the Schumpeterian growth paradigm (see, for instance, Aghion and Howitt, 2006). It emphasizes that

the design and effectiveness of structural policies aimed at fostering economic growth are context – specific and depend on a country's distance to the technological frontier. To properly assess the impact of structural policies on growth is, therefore, necessary to take into account the existence of non – linear effects that derive from the interactions between policies and a country's distance from the technological frontier.

The broad coverage of our dataset allows us to shed new light on the hypotheses recently developed in the literature that the relevance of structural reforms for growth might be context – specific and highly heterogeneous.

Our main findings are as follows: First-pass estimation shows evidence that both real and financial sector reforms are positively associated with growth. Among real sector reforms, openness to international trade has a significant positive effect on growth, while the impact of agriculture and networks reforms is not significant. Among financial sector reforms, both domestic financial reform and capital account liberalization significantly raise growth with the effect of the former being larger than the latter. We also show that the positive association we find is heterogeneous across different kinds of reforms and is influenced by the level of a country's constraint on the executive power and by its distance from the technological frontier.

The paper is organized as follows. Section 2 provides a description of the data used in the empirical analysis with special emphasis on the newly constructed structural reform indices. This section also presents some preliminary evidence on the relationships between the key variables of interest. Section 3 reports and discusses our  main results, whereas Section 4 presents extensive robustness checks. Section 5 summarizes our main findings and concludes.

## 3.2    A First Look at the Data

This section describes the new measures of structural reforms that we use throughout the paper. Section 2.1 provides an introduction to the indicators of the large variety of structural reforms we consider in our analysis and describes their time patterns both at aggregate and regional level. Section 2.2 takes a first look at the relationship between reforms and different levels of constraints on the executive index as from the POLITY IV dataset.

### 3.2.1   New data on structural reforms

In our empirical analysis we use novel indicators of structural reforms regarding both the real and the financial sector of the economy. Many indices cover about 30 years and over 90 countries[7]. The key advantage of these measures over those used in previous work is that they have a long time series dimension and comprise a large number of countries, including advanced and developing economies. The Appendix 1 and 2 provide, respectively, the complete list of the countries included in the sample and detailed information about the methodology used to construct the different indicators and about their time and country coverage.

The indicators of reforms in the real sector of the economy regard *product market* liberalization and openness to the *international trade*. Two different indices capture the degree of structural reforms undertaken in the product markets. The first refers to the *agricultural sector.* It

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[7] For a few reforms (i.e. trade, agriculture, current account and capital account) our data goes back to 1960 and covers more than 100 countries.

measures the extent of public intervention in the market of each country main agricultural export commodity. It includes the presence of export marketing boards and the incidence of administered prices. The second measures the degree of liberalization in the *telecommunications and electricity* markets, including the extent of competition in the provision of these services and the existence of an independent regulator. Openness to *international trade* is captured along two dimensions. The first measures average tariff rates; the second covers restrictions on current account transactions, including payments and receipts on exports and imports of goods and services[8].

Among the indicators of financial-sector reforms, the index of *domestic financial liberalization* is derived from the database of Abiad, Detragiache and Tressel (2008). It is an average of six sub – indices. Five of them refer to the *banking system* and cover: (i) credit controls, such as subsidized lending and directed credit; (ii) interest rate controls, such as floors or ceilings; (iii) competition restrictions, such as entry barriers and limits on branches; (iv) the degree of state ownership; and (v) the quality of banking supervision and regulation. The sixth dimension relates to the *security markets* and captures the degree of legal restrictions on the development of domestic bonds and equity markets as well as the existence of independent regulators. When investigating the association between reforms in the domestic financial sector and economic growth, we use both the overall index of domestic financial liberalization and the two different sub-indices that relate to the banking and securities sectors.

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[8] This index—measuring how compliant a country is with its obligations under the IMF's Article VIII to free from restrictions the proceeds from international trade in goods and services—captures only some non-tariff barriers to trade. For other non-tariff barriers—such as quotas and subsidies—there is not a sufficiently long time series to be used in the analysis.

The extent of *external capital account liberalization* measures a broad set of restrictions on financial transactions for residents and non residents, as well as the use of multiple exchange rates. In the empirical analysis we use both this aggregate measure and the two separate indicators of external capital account openness for *resident* and *non resident*. The last two sub indices measure, respectively, the intensity of legal restrictions on residents' versus non residents' ability to move capital in and out the country.

All indicators are rescaled to range between zero and one, with higher values corresponding to greater degree of liberalization. Differences in values of each index across countries and over time provide useful information on the variation in the absolute degree of economic liberalization within each sector. Instead, differences in the value of the indices across sectors do not provide a precise quantitative measure of whether one sector is more liberalized than another because of the different methodology used to construct each index. For instance, a positive difference between the trade index and the financial index does not necessarily mean that trade is more liberalized than the financial sector.

All indices trend upwards over time towards a high degree of liberalization (Figure 1). At a sectorial level, the global liberalization of international trade, capital movements, and the domestic financial sector has been fairly steady and gradual over the last three decades, whereas product market liberalization started only around 1990. There have been no global setbacks in the average degree of liberalization in any sector. Structural reform indicators display significant differences across regions (Figure 2), pointing to imitation and "catch-up" effects.

### 3.2.2 Structural reforms at different levels of constraints to executive power

Acemoglu, Johnson, Querubin, and Robinson (2008) develop a political economy model where the effectiveness of *de jure* policy reforms crucially depends on the state of existing political institutions. The main theoretical insight is that policy reforms can be expected to have the most relevant effect on distortionary policies in societies characterized by intermediate levels of constraints on the executive. Actually, *de facto* distortionary policies are more likely to be implemented in places with weak institutions where politicians are less constrained in their action and less accountable to voters. On the contrary, whenever checks on politicians' activity are sufficiently strong, there is less room for *de facto* distortionary policies to be produced. Therefore, *de jure* reforms can be expected to have little effect on policies in places where constraints on politicians are so weak that they can be easily undermined or where constraints on politicians are already so strong that policies are unlikely to be distortionary to start with. The evidence relative to the central bank independence they provide in the paper is consistent with this theoretical framework. Central Bank independence appears to be more effective in reducing inflation in countries with an intermediate level of constraints on the executive, while it generates no or smaller effect in countries with low or high constraints.

In line with this framework, in the empirical section of this paper we investigate whether other kinds of reforms have heterogeneous impacts on economic outcomes - namely per capita income growth - depending on country-wide differences in institutional environments. Specifically, we check whether the positive relationship between structural

reforms and growth is stronger in countries with intermediate levels of institutional quality.

As in Acemoglu, Johnson, Querubin, and Robinson (2008), we use the index of constraints on the executive from the POLITY IV dataset to measure the degree of political accountability in different countries. This variable measures to what extent institutional constraints limit the executive's decision-making power. It takes values between 1 - to denote contexts whit no regular limitations on the executive's power – and 7 – to describe countries where political bodies such as legislatures have equal or even larger authority than the executive. We then compute the sample mean of our variable of interest. Countries above, within and below one-standard deviation from the sample mean are assigned, respectively, to the categories of high, middle, and low constraints on the executive.

Panels in Figure 3 depict the evolution over time of our main reform indices for countries grouped according to the three categories of high, medium, and low constraints on the executive. The six measures of liberalization we consider (trade, current account, network, agriculture, capital account, and domestic finance) show an overall upward trend over time in countries characterized by high, medium, and low constraints on executive. This suggests a general tendency toward greater liberalization, the only less clear-cut cases being current and capital account liberalization in countries with low constraints on the executive. Moreover, countries with better institutions generally have more liberalized economies than countries with low and intermediate constraints on the executive. These figures, nevertheless, suggest a sort of catching-up or imitation effect toward greater liberalization for countries with poorer institutions.

## 3.3   Estimation and results

Our empirical analysis is organized around three related parts. First, we present a graphical and econometric analysis that investigates correlations between our reform indexes and growth spurts, motivated by the recent literature on growth accelerations (e.g., Hausmann, Pritchett, and Rodrik, 2005). Second, we present first-pass associations between reforms and growth obtained from OLS estimations that control for country and time fixed effects. Third, we examine the hypotheses that the reforms-growth relationship is influenced – albeit in a heterogeneous way – by a country's institutional environment and by its distance from the technological frontier. To test the relevance of the institutional framework for the relationship between structural policies and economic performance we include in the growth regressions an interaction term between the real and financial reform variables and the constraints on the executive index from the Polity IV dataset. To assess the presence of non – linearity in the association between reforms and growth that depends on a country's distance from the technological frontier, we estimate econometric specifications where the structural reforms indicators are interacted with the ratio of each country's GDP per capita with the USA's GDP per capita.

In the next section, we provide extensive robustness checks of our results to different estimation methods, to the inclusion of additional control variables and to the use of lower frequency data.

### 3.3.1   Reforms and growth breaks

In this subsection we present preliminary evidence from graphical and econometric analysis about the association between different types of real and financial structural reforms and growth spells. Growth spells,

broadly defined as extended periods of very rapid or markedly slow growth, are a striking feature of the development process in many countries. Recent work by Hausmann, Pritchett, and Rodrik (2005), Berg, Ostry, and Zettelmeyer (2008), and Jones and Olken (2008) use this new approach to the data analysis to understand the differential growth experiences of rich and poor countries[9]. This subsection uses a set of growth spells—identified by using the econometric methodology of Antoshin, Berg, and Souto (2008)[10]—to examine whether structural reforms accompany growth accelerations and whether reform reversals or absence of reforms are associated with growth decelerations. In the Appendix 3 we list all the episodes of up and down breaks in economic growth since 1960 that we are able to detect by using the aforementioned statistical methodology.

Figures 4-5 plot the average level of the residuals of a panel regression of each index on country and year fixed effects for a period starting five years before the break (0 on the horizontal axis) and ending five years after the break. All plotted averages are based on the set of countries for which the index is available three years before the break so that each line shows how the average index has evolved around the break for the same group of countries. Given that panel regressions remove country and year specific averages of each index, a movement of the plotted average residual from below to above the zero reference line on the vertical axis prior to an up-break (for example, in the case of the current account index, solid line of the middle panel in Figure 4) indicates

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[9] Two early precursors of the current work on growth spells are Ben-David and Papell (1998), and Pritchett (2000), both of which employed novel econometric methods to identify shifts in growth performance.
[10] These authors identify "growth spells" by modifying the procedure pioneered by Bai and Perron (1998) to determine sample-specific critical values as it is appropriate when the time dimension is 30 years or less.

that the reform index has gone from below the country-average to above the country-average prior to an up-break.

The year-specific fixed effects effectively remove also the global trend in each index so that, in practice, the country-specific averages relative to which the plotted residuals are measured are trend-corrected. This means that the decline of the plotted residual lines around down-breaks (for example, in the case of the agriculture index, dashed line of bottom panel in Figure 4) can indicate either reform reversals or lack of reform in a period where most other countries are reforming[11].

Among real-sector reforms (Figure 4), liberalization of the current account and of the agriculture sector are clearly associated with growth accelerations with the indices improving about three years before the up-break and continuing on an upward trend afterwards. Conversely, growth decelerations are associated with a tariff-based trade liberalization index below the country average (the zero reference line for the vertical axis) and with deteriorating indices of current account liberalization and agriculture.

Among financial-sector reforms (Figure 5), liberalization of the domestic financial sector and of the capital account are both associated with growth accelerations. The banking component of the domestic financial sector index starts improving about two years before an up-break. As in the case of most real-sector indices, growth decelerations are associated with a downward trend of all financial indices, which tends to begin before the down-break and markedly continues afterwards.

We then turn to simple econometric analysis to test whether improvements (reversals) in the structural reform indicators help to predict growth accelerations (decelerations). The dependent variable UP3

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[11] In Figure 4, there is no chart for networks reform because of the few spells available after 1990.

is meant to capture the timing of growth accelerations. It takes value equal to 1 for the 3 years centered on the first year of the growth acceleration episode and 0 otherwise. The dependent variable DOWN3 relates to growth decelerations and is a dummy equal to 1 for the 3 years centered on the first year of the growth deceleration episode and 0 otherwise. We choose to use a 3 – year time window around a growth acceleration (deceleration) episode as dependent variable for two main reasons. First, it makes less likely that we are missing a growth acceleration (deceleration) episode because of measurement error in the GDP data. Second, we prefer to use a quite narrow time window around the year of the break in the GDP per capita growth to reduce the possibility that we are capturing the effect of unobserved medium – term economic or institutional changes that might affect the realization of a growth acceleration (deceleration) episode.

The explanatory variable of main interest is the change over the previous year in each of our structural reforms indicators. Taking the first difference of each of the indices of structural reforms helps to capture movement toward a larger (lower) degree of liberalization in the different real and financial sectors of the economy. In all the specifications we include as additional controls the first lag of the polity2 index (POLITY21) from the POLITY IV database to measure the quality of each country democratic institutions and the yearly growth rate of the country specific terms of trade[12] (TOT_Gr). The basic and most parsimonious specification includes also a full set of year fixed effects.

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[12] We include as control the lagged value of each country index of democracy since a recent work by Giuliano, Mishra and Spilimbergo (2009) documents a positive association between democracy and the adoption of structural reforms. Moreover, we could expect that improvement in democratic institutions might lead to a better economic performance. The inclusion of the yearly growth rate of the terms of trade is in line with the analysis of growth accelerations by Hausmann, Pritchett, and Rodrik (2005).

Given the binary nature of the dependent variable our main estimation method is maximum likelihood probit. To check the robustness of the results to different estimation methods we also report estimates from a linear probability model.

Results reported in Table 1A and 1B refer to specifications – estimated respectively by maximum likelihood probit and linear probability model - where UP3 is the dependent variable. The estimates from the two models tell a consistent story. With the only exception of *Networks*, the coefficients of the variables of main interest (the change over the previous year in each of our structural reform indicators) have the same (expected) positive sign across the two models. Moreover, the same variables, namely the change over the previous year in the indices of structural reforms for the *domestic financial sector* (DF_ch), for the *banking system* (BK_ch), for the *external capital account,* as measured by both its aggregate indicator (CAP100_ch) and by the sub-indices related to resident (CAPRES_ch) and non resident (CAPNONRES_ch), are statistically significant at conventional levels in both the models.

Consistently with the graphical analysis of Figure 5, these results suggest that reforms in the domestic and external financial sectors are associated to an increase in probability of occurrence of growth accelerations. According to our probit estimates, at the average values of the independent variables, a one-time jump from the minimum of 0 to the maximum of 1 in the yearly change of the *domestic financial* index increases the probability of growth acceleration by 0.138; of the *banking system* index by 0.125; of the *external capital account* index by 0.105; of the *external capital account index* for *resident* by 0.086 and of the external capital account index for *non resident* by 0.069. These results are in line with those reported by Hausmann, Pritchett, and Rodrik (2005).

They find that their measure of financial liberalization is a strong predictor of the probability of the occurrence of growth accelerations. Depending on the different specifications they use, financial liberalization increases the probability of experiencing growth accelerations by 7 to almost 11 percentage points.

Tables 2A and 2B report respectively estimates from probit and linear probability models for the specifications where DOWN3 is the dependent variable. Though almost all the coefficients of interest have the expected negative sign in both the models, only the decrease in the intensity of the legal restrictions on the residents' ability to move capital in and out the country (as captured by the variable CAPRES_ch) is associated at statistical significant level to a decrease in the probability of growth deceleration across the two models. According to our probit estimates, at the average values of the independent variables, a sudden and full liberalization in the external capital account for resident decreases the probability of growth deceleration by 0.080[13].

The specifications so far discussed pool together all the countries and years. We, therefore, check whether our results still hold once we control for country specific unobserved heterogeneity. A simple and natural starting point is a linear probability model with additive unobserved country effects. Table 3A and 4A report estimates from this model respectively for growth acceleration and deceleration episodes. Linear probability model imposes, nevertheless, strong restrictions on the unobserved effects[14]. We, therefore, also report estimates from a random effects probit model which treats the omitted time - invariant

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[13] We also estimate the same specifications both for growth accelerations and decelerations by using maximum likelihood logit models. The results are very consistent with those reported in the paper and are available upon request from the authors.
[14] See Wooldridge (2002) chapter 15.

unobserved effects as a random variable orthogonal to the other regressors. Estimates are reported in Tables 3B and 4B respectively for growth acceleration and deceleration episodes. The estimates we obtain from both the linear probability model and the random effects probit model are consistent with those discussed above. In particular, they confirm a positive and statistical significant association between reforms in the domestic financial sector, in the banking system and in the external capital account and the probability of experiencing growth acceleration.

In summary, the graphical and econometric analysis of the reform process around growth spells provides suggestive evidence of a positive association between a broad range of structural reforms – in particular in the domestic and external financial sectors - and economic growth.

### 3.3.2 Reforms and growth – simple associations

In this section we provide additional econometric evidence based on more conventional growth regressions about the relationship between structural reforms and economic growth. We estimate through ordinary least-squares (OLS) the following specification:

$$lnGDP_{i,t} - lnGDP_{i,t-1} = a_0 + a_1\,lnGDP_{i,t-1} + a_2\,Reform_{i,t-1} + \eta_i + \delta_t + \varepsilon_{it} \tag{1}$$

where the dependent variable is the growth rate of the GDP per capita in country $i$ at period $t$, regressed on one year lag of (the log of) GDP per capita and on year lag of each of our reform indicators. The terms $\eta_i$'s and $\delta_t$'s represent respectively a full set of country and year fixed effects, and $\varepsilon_{it}$ captures all the omitted factors. By including country fixed effects, we control for any country time – invariant characteristic (such as colonial legacies, legal origins, ethnic fragmentation, etc.) that could affect both

the adoption of structural reforms and per capita income growth. While this specification does not rule out all possible sources of bias in our estimations, like omitted time-varying factors or reverse causation, it, nevertheless, allows to deal with a potentially relevant case of omitted variable bias.

In our baseline specifications we use annual data to better date the effects of reforms and capture growth accelerations immediately after reforms take place. Acknowledging the warning in Johnson et al. (2009) of the large measurement error associated with high frequency Penn World Tables PPP-adjusted GDP data, we re-estimate all the regressions by using 3 and 5 year interval data. The structural reform indicators are lagged one period to mitigate the most obvious form of reverse causation. Robust standard errors are clustered at country level.

Table 5 presents results from these regressions. Standard growth regressions confirm the positive association between structural reforms and growth suggested by the analysis of growth spells. With the exception of the liberalization in the product markets (agriculture, electricity and telecommunications), all the real and financial sector structural reform indices show a positive and statistically significant association with growth[15].

The magnitude of the estimated effect of structural reforms on per capita income growth is substantial. To gauge it, it is better to focus on long-term multipliers which take into account the different dynamics of each reform and make the growth effects comparable across reforms. For example, complete trade liberalization with the average tariff falling from the maximum to the minimum level in the sample (the trade index going

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[15] Our baseline regressions do not include other control variables. Results including other typical growth covariates, such as education, terms of trade, and democracy are presented in the Section 4.

from zero to one) is estimated to increase output per capita in the long run by 44 percent. The largest estimated effect is that of domestic financial liberalization, which would approximately double output per capita in the long run[16].

### 3.3.3. Reforms and growth. The role of constraints on the executive power

In this section we examine whether the institutional environment - as measured by a country's constraints on the power of the executive authority - affects the way in which reforms promote growth. Following Acemoglu, Johnson, Querubin, and Robinson (2008), we classify countries into three categories according to their level of constraints on executive authority as measured by the Constraints on the Executive index from the Polity IV dataset. In specific, countries above, within, and below one-standard deviation from the sample mean of the Constraints on the Executive index are respectively assigned to high, middle, and low-constraints on the executive categories.

Table 6 provides an unconditional analysis of how a country's level of constraints on the executive power affects the impact of reforms on growth. For example, countries with low constraints on the executive which have implemented aggressive trade and current account reforms have benefited substantially more in terms of growth than countries with middle and high levels of constraints. The same also seems to be the case for the capital account index and for its residents and nonresidents sub indices. Alternatively, as a consequence of improvement in the domestic

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[16] The inverse of the coefficient on lagged GDP per capita is the factor by which each impact coefficient needs to be multiplied to obtain the long-run growth effect of a change of an index from zero to one (full liberalization). When reforms are compared on the basis of one standard deviation shock or an improvement from the 25th to the 75th percentile of each index, the ranking of their long-run growth effects remains broadly unchanged.

finance index and in its banking and securities sub indices, growth increased significantly regardless of a country's constraints on the executive power.

Moving on to the regression analysis, our estimating equation is:

$$lnGDP_{i,t} - lnGDP_{i,t-1} = a_0 + a_1 lnGDP_{i,t-1} + a_2(LowCE_i * Reform_{i,t-1}) + a_3(MiddleCE_i * Reform_{i,t-1}) + a_4(HighCE_i * Reform_{i,t-1}) + \eta_i + \delta_t + \varepsilon_{it}$$
(2)

where $LowCE_i$, $MiddleCE_i$, and $HighCE_i$ represent three sets of dummies respectively for low, middle, and high constraints on the executive categories.

We report in Tables 7 and 8 OLS estimates of the specification (2), respectively, for the real and financial sectors' structural reform indicators. The most interesting insight from these results is the heterogeneity of the reforms-growth relationship that depends on a country's constraints on the authority of the executive power. A closer look at the result reveals a series of intriguing results.

The broader index of reform of the capital account and of its sub index for *non resident* display a statistically significant positive association with economic growth only for countries with medium or medium and low level of constraints on the executive. On the other hand, structural reforms in the domestic financial sector show a significant and positive association with economic growth regardless of the level of constraints on the executive. This result suggests that financial and more specifically banking reforms may have a broader use for the economy as they can positively affect many sectors. In Acemoglu et al. (2008) model this result could be generated if financial reforms exert a very high cost to the authorities to impose distortionary policies – even under an authoritarian regime.

### 3.3.4 Structural reforms and distance from the technological frontier

An alternative to the neoclassical specification considered in Table 5 is the Schumpeterian specification reported in Table 9. This specification derives from the Schumpeterian growth theory which is based on the process of creative destruction[17]. A key implication of the Schumpeterian theory is that economic development can be evaluated by the distance of a country's gdp per capita to that of frontier countries. The most relevant question in the Schumpeterian growth literature is, therefore, how fast low-income countries can close their income gap with countries at the technological frontier.

Key economic state variables, such as a country's distance from the technological frontier, should, therefore, affect the design of appropriate policies aimed at fostering economic growth. As stressed in Acemoglu, Aghion and Zilibotti (2006), when a country is far from the world technology frontier, the most relevant source of growth is the adoption of already well established technologies. The closer a country gets to the technological frontier, the more innovation matters for economic growth. In other words, the closer a country is to the world technological frontier, the higher is the relative importance of innovation versus imitation to sustain productivity growth. Consequently, the set of possible policies aimed at sustaining growth, what the authors define as "*appropriate institutions*", can vary for countries at different stages of economic development.

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[17] The process of creative destruction was pioneered in the writings of Joseph Schumpeter (1928, 1942) and refers to the endogenous introduction of new products and processes that inevitably eliminates some of the existing products and processes. Schumpeterian growth theory has been revived and formally modeled by Aghion and Howitt (1992).

Building on these theoretical insights, Aghion and Howitt (2006) analyze in depth the case of education[18]. The authors argue that primary and secondary education matters more for a country's ability to imitate the frontier technology, while tertiary education has a larger impact on a country's possibility of innovating. As a country catches up with the technology frontier, tertiary education should be more relevant for growth than primary/secondary education[19]. Vandenbussche, Aghion and Meghir (2006) provide evidence relative to a panel of 19 OECD countries for the period 1960-2000 consistent with the idea that higher education matters more as a country catches up with the technological frontier.

The Schumpeterian approach calls for an econometric specification as follows:

$$lnGDP_{i,t} - lnGDP_{i,t-1} = a_0 + a_1(GDP_{i,t-1}/GDP_{US,t-1}) + a_2 Reform_{i,t-1} + a_3[Reform_{i,t-1} * (GDP_{i,t-1}/GDP_{US,t-1})] + \eta_i + \delta_t + \varepsilon_{it} \qquad (3)$$

There are two main differences between this econometric model and the neoclassical specification of the equation (1). First, convergence is now captured by the "income-gap" usually proxied by the ratio of GDP per capita in country *i* to the GDP per capita of the United States (the country with the higher per capita GDP in our sample). Second, the interaction

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[18] The analysis of education policies in Aghion and Howitt (2006) is based also on works by Vandenbussche, Aghion and Meghir (2006) and Aghion, Boustan, Hoxby and Vandenbussche (2005).

[19] Aghion and Howitt (2006) combine insights from Nelson and Phelps (1966) and Acemoglu, Aghion, Zilibotti (2006). Nelson and Phelps model an economy where the productivity growth can be expressed according to the equation: $\dot{A} = f(h)(\bar{A} - A)$ where *h* is the current stock of human capital in a country and $\bar{A}$ is the frontier technology growing over time at same exogenous rate. A higher stock of human capital fosters growth by facilitating the catching up with the technological frontier. Similarly to Acemoglu, Aghion, Zilibotti (2006), in Aghion and Howitt (2006) productivity growth can be generated or by imitating the frontier technology or by innovating on past technologies. The relative importance of innovation increases as a country gets closer to the technological frontier. Moreover, higher education investment should produce a bigger effect on a country's ability of making leading-edge innovation, while primary and secondary education should exert a larger impact on a country's ability of implementing frontier technology.

term between reform and "income-gap" is included in the model to assess the effect of reforms in closing the gap to the level of output in the United States. If the interaction term is negative and statistically significant, the growth returns from reforming that sector will be larger the further a country is from the technological frontier.

Results based on OLS estimations of equation (3) are reported in the Table 9. Openness to the international trade and liberalization in the agricultural and financial sectors (with the only exception of the external capital account for non – resident) show a positive association with economic growth. Moreover – as suggested by the negative and statistical significant sign of the interaction term - structural reforms in the international trade and in the domestic financial sector (with the exception of the security markets) are more relevant for economic growth for countries further from the technological frontier. In particular, the finding that reforming the domestic financial sector sustains the convergence to the world technological frontier is in line with the relevance of financial development for convergence stressed by Aghion, Howitt and Mayer – Foulkes (2005).

## 3.4 Robustness

In this section we examine the robustness of our findings to alternative estimation methods and to the inclusion of additional control variables. We also check whether changing the frequency of the data from annual to three - year intervals alters our results.

### 3.4.1 Arellano Bond GMM estimations

Through a simple manipulation we can write equation (1) as:

$$lnGDP_{i,t} = a_0 + \bar{a}_1 \, lnGDP_{i,t-1} + a_2 \, Reform_{i,t-1} + \eta_i + \delta_t + \varepsilon_{it} \qquad (4)$$

where $\bar{a}_1 = 1 + a_1$. From this expression we can see that estimating equation (1) is equivalent to estimate a dynamic model with lagged – dependent variable on the right side[20]. The presence of the lagged – dependent variable makes the fixed effect OLS estimator no longer consistent due to the correlation of the lagged regressor with the error term. For fixed $N$, OLS estimates are consistent only for $T \to \infty$ (see Cameron and Trivedi, 2005, chapter 22). To deal with this issue, we use the GMM estimator proposed by Arellano and Bond (1991). This procedure amounts to first differencing equation (5) in order to remove country fixed effects and to use two and more lags of the dependent variable as instruments. Consistency of the estimators requires, moreover, lack of second order serial correlation in the first differenced error term which can be tested.

In the Table 10 we report the results we obtain by estimating equation (4) using yearly data. Overall they confirm the positive association between reforms in the real[21] and domestic financial sectors and economic growth. The coefficients of the variables *Capital Account* and *Capital Account (non resident)* are instead no longer statistically significant. Finally, the *p-values* reported in the bottom row of the Table show that there is no statistically significant second order autocorrelation among the first-differenced error terms.

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[20] See for instance Caselli, Esquivel and Lefort (1996).
[21] Among the reforms in the real sector of the economy only the liberalization of electricity and telecommunication market shows no association with economic growth.

In Tables 11 and 12 we report the results we obtain by repeating the GMM estimation exercise for equation (2). Therefore, we investigate whether reforms have an heterogeneous impact on income growth depending on different levels of constraints on the executive. Liberalization of real sectors shows a more heterogeneous effect on growth which is influenced by the different levels of constraints on the executive. It is worth noticing that the evidence about the effect of reforms in the financial sector is in line with the results we obtain from OLS regressions. The effectiveness of reforms in the domestic finance, banking and security sectors does not seem to depend on the broader institutional framework. Reforms in these sectors have a significant and positive impact on per capita income growth for the three levels of constraints on the executive we consider. Among the remaining indicators of structural reforms in the financial sector of the economy, only liberalization of capital account for residents in countries with medium level of constraints on the executive has a significant and positive association with growth. The *p-values* reported in the bottom row suggest that we cannot reject the null hypothesis of no second order serial correlation among the differenced error terms.

Results discussed in this section should, nevertheless, be taken with a word of caution given that in all the specification reported in Tables 10 – 12 the Sargan test rejects the null hypothesis that the over-identifying restrictions are valid.

## 3.4.2  Additional control variables and lower frequency data

We check the robustness of our baseline results to the inclusion in the growth regressions of a set of control variables. Tables 13-16 reproduce results of Tables 5, 7, 8 and 9 by adding three additional

control variables: political institutions, tertiary educational attainment, and terms of trade. Our results are fairly robust to the inclusion of this and alternative sets of control variables[22].

Johnson et al. (2009) warn about the serious implications from using in growth regressions annual PPP-corrected GDP data from Penn World Tables due to the presence of measurement error. We take this warning seriously and, although we believe that for the question at hand using reform data at the annual frequency is conceptually the preferred choice, we have re-estimated our baseline results using 3-year and 5-year data intervals[23]. As shown in Tables 17 - 20 our main results are fairly robust to lower frequency data.

In summary, we show that the positive and heterogeneous association between a large variety of structural reforms and growth holds fairly when we include in our specifications additional control variables and when we use data at lower frequencies.


## 3.5    Conclusion

This paper examines whether real and financial reforms over the last three decades have been associated with higher growth, and whether there has been a differential growth response due to a country's institutional environment. Underpinning the empirical analysis is a significant data collection effort, involving the compilation of indicators of structural reform for a large sample developing and developed countries

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[22] Results with alternative sets of control variables have not been reported to save space but they are available upon request from the authors. These controls include, among the others, macro policy variables (i.e. inflation), alternative measures of educational attainment (primary and secondary education), alternative measures of political institutions (from the Polity IV database), and alternative definitions of terms of trade (growth of terms of trade).
[23] To save space we do not report the results with 5-year intervals but are available upon request from the authors.

over the past three decades. Not only is the resulting dataset unique in its country and time coverage, it is also much broader in terms of the sectoral coverage of reforms—including indicators of liberalization in domestic product markets; international trade; several indicators of liberalization of the domestic financial sector; and measures of the capital account liberalization.

Our findings are as follows:

- There exists evidence of a broad positive association between several types of reforms and growth.

- This association, however, is remarkably heterogeneous and crucially depends on the level of a country's constraints on the executive power and on its distance from the technological frontier. A notable exception to the strong heterogeneity found for the rest of structural reforms is domestic financial liberalization and in particular the sub-index on banking liberalization. These reforms are shown to consistently be positively associated with growth regardless of the institutional environment.

A word of caution is in place here. Our results should be taken as evidence of strong associations rather than causation. It is certainly the case that reforms are at least partially determined by the political process, but appropriate instrumental variables that could resolve this and other sources of endogeneity are particularly difficult to find in our context. Having said this, we do not view this as a drawback of this paper but rather a constraint inherently embedded in the complex composition of structural reforms. The heterogeneous effects that "*key variables*" - like the broad institutional environment or the distance from the technological frontier - have on the reform-growth relationship are remarkable and we

hope that together with the novel dataset on reforms this will stimulate further research on this important issue.

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**Figure 1. Structural Reforms Indices**
(All countries)



Source: IMF estimates.

Notes: Higher values of the reform indices represent greater liberalization. Each index is standardized to lie between zero and unity. The average is measured as the mean of the index across countries for each year. "Domestic Finance" captures restrictions on interest rate determination and competition, credit controls, and the quality of supervision in the banking sector, as well as the degree of liberalization of securities markets. "Capital Flows" measures restrictions on international financial transactions between residents and nonresidents. "Trade" denotes average tariff rates. "CA Restrictions" denotes current account restrictions on the proceeds from international trade in goods and services. "Agriculture" captures intervention in the market for each country's main agricultural export commodity. "Networks" captures the degree of competition and liberalization, and the quality of regulation, in these sectors. See Appendix 2 for more details.

## Figure 2. Structural Reforms Indices by Region



**Panel A. Trade Index**

**Panel B. Current Account Restrictions Index**

**Panel C. Networks Index**

**Panel D. Agriculture Index**

**Panel E. Capital Account Index**

**Panel F. Domestic Finance Index**

Legend for each panel: African, Asian, East_EU, Industrial, Latin, M_East

Source: IMF estimates.

Notes: Higher values of the reform indices represent greater liberalization. Each index is standardized to lie between zero and unity. The average is measured as the mean of the index across countries for each year. "Domestic Finance" captures restrictions on interest rate determination and competition, credit controls, and the quality of supervision in the banking sector, as well as the degree of liberalization of securities markets. "Capital Flows" measures restrictions on international financial transactions between residents and nonresidents. "Trade" denotes average tariff rates. "CA Restrictions" denotes current account restrictions on the proceeds from international trade in goods and services. "Agriculture" captures intervention in the market for each country's main agricultural export commodity. "Networks" captures the degree of competition and liberalization, and the quality of regulation, in these sectors. See Appendix Table A2 for more details.

# Figure 3. Structural Reforms Indices by Constraint to the Executive Level

**Panel A: Trade Index**



**Panel B: Current Account Restrictions Index**



**Panel C. Networks Index**



**Panel D. Agriculture Index**



**Panel E. Capital Account Index**



**Panel F. Domestic Finance Index**



Source: IMF estimates.

Notes: Higher values of the reform indices represent greater liberalization. Each index is standardized to lie between zero and unity. The average is measured as the mean of the index across countries for each year. "Domestic Finance" captures restrictions on interest rate determination and competition, credit controls, and the quality of supervision in the banking sector, as well as the degree of liberalization of securities markets. "Capital Flows" measures restrictions on international financial transactions between residents and nonresidents. "Trade" denotes average tariff rates. "CA Restrictions" denotes current account restrictions on the proceeds from international trade in goods and services. "Agriculture" captures intervention in the market for each country's main agricultural export commodity. "Networks" captures the degree of competition and liberalization, and the quality of regulation, in these sectors. See Appendix Table A2 for more details. High, middle, and low constraints on executives categories represent countries above, within and blow one-standard deviation from our sample mean, respectively.

**Figure 4. Growth Breaks and Real Sector Reforms**

**Panel A. Trade Index**



Trade liberalization (tariff)

More liberalization

Less liberalization

**Years before and after growth breaks**

**Panel B. Current Account Restrictions Index**



Trade liberalization (current account)

More liberalization

Less liberalization

**Years before and after growth breaks**

**Panel C. Agriculture Index**



Agriculture liberalization

More liberalization

Less liberalization

**Years before and after growth breaks**

—— Average liberalization index around upbreaks    - - - Average liberalization index around downbreaks

Source: Authors estimates based on Penn World Tables version 6.2.

Notes: The figures plot average liberalization indices for the period beginning five years before a growth break (year 0 on the horizontal axis) and ending five years after the growth break. The plots capture the within-country evolution of the liberalization indices obtained from a panel regression of each index on country fixed effects (to remove country averages) and year fixed effects (to remove global trends). As a result, the zero value on the vertical axis corresponds to the sample average of the liberalization indices for the countries considered. The number of countries used to compute each average varies across indices in line with data availability.

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**Figure 5. Growth Breaks and Financial Sector Reforms**

**Panel A. Domestic Finance Index**



**Panel B. Banking Subindex**



**Panel C. Capital Account Index**



——— Average liberalization index around upbreaks    - - - - Average liberalization index around downbreaks

Source: Authors estimates based on Penn World Tables version 6.2.

Notes: The figures plot average liberalization indices for the period beginning five years before a growth break (year 0 on the horizontal axis) and ending five years after the growth break. The plots capture the within-country evolution of the liberalization indices obtained from a panel regression of each index on country fixed effects (to remove country averages) and year fixed effects (to remove global trends). As a result, the zero value on the vertical axis corresponds to the sample average of the liberalization indices for the countries considered. The number of countries used to compute each average varies across indices in line with data availability.

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## Table 1 A. Structural reforms and Up-breaks in Growth

|  | (1) up3 | (2) up3 | (3) up3 | (4) up3 | (5) up3 | (6) up3 | (7) up3 | (8) up3 | (9) up3 | (10) up3 |
|---|---|---|---|---|---|---|---|---|---|---|
| TR_ch | 0.296 (0.529) | | | | | | | | | |
| CUR100_ch | | 0.813 (0.599) | | | | | | | | |
| AG_ch | | | 0.368 (0.352) | | | | | | | |
| NW_ch | | | | -0.245 (0.927) | | | | | | |
| DF_ch | | | | | 1.738** (0.874) | | | | | |
| BK_ch | | | | | | 1.575** (0.799) | | | | |
| SM_ch | | | | | | | 0.201 (0.450) | | | |
| CAP100_ch | | | | | | | | 1.474*** (0.478) | | |
| CAPRES_ch | | | | | | | | | 1.212*** (0.340) | |
| CAPNONRES_ch | | | | | | | | | | 0.957* (0.491) |
| polity21 | -0.025*** (0.006) | -0.019*** (0.005) | -0.002 (0.005) | -0.005 (0.005) | -0.023*** (0.007) | -0.023*** (0.007) | -0.023*** (0.007) | -0.020*** (0.005) | -0.020*** (0.005) | -0.020*** (0.005) |
| TOT_Gr | -0.460 (0.324) | -0.250 (0.288) | -0.267 (0.292) | -0.235 (0.285) | -0.727* (0.392) | -0.718* (0.392) | -0.712* (0.391) | -0.253 (0.291) | -0.248 (0.291) | -0.260 (0.292) |
| Observations | 2728 | 3472 | 3964 | 4200 | 1980 | 1980 | 1980 | 3472 | 3472 | 3472 |
| Pseudo R-square | 0.0644 | 0.0524 | 0.0416 | 0.0414 | 0.069 | 0.0692 | 0.0645 | 0.0589 | 0.0593 | 0.0541 |

Estimation method: Maximum Likelihood Probit. Robust standard errors in parentheses. All the specifications include year fixed effects. Annual data over 1960-2005 when available. Significance levels: *** 0.01, ** 0.05, * 0.1.

**Table 1 B. Structural reforms and Up-breaks in Growth**

|  | (1) up3 | (2) up3 | (3) up3 | (4) up3 | (5) up3 | (6) up3 | (7) up3 | (8) up3 | (9) up3 | (10) up3 |
|---|---|---|---|---|---|---|---|---|---|---|
| TR_ch | 0.034 (0.057) | | | | | | | | | |
| CUR100_ch | | 0.075 (0.059) | | | | | | | | |
| AG_ch | | | 0.038 (0.045) | | | | | | | |
| NW_ch | | | | -0.019 (0.057) | | | | | | |
| DF_ch | | | | | 0.157* (0.091) | | | | | |
| BK_ch | | | | | | 0.139* (0.084) | | | | |
| SM_ch | | | | | | | 0.025 (0.044) | | | |
| CAP100_ch | | | | | | | | 0.126*** (0.048) | | |
| CAPRES_ch | | | | | | | | | 0.101*** (0.036) | |
| CAPNONRES_ch | | | | | | | | | | 0.076* (0.042) |
| polity21 | -0.002*** (0.000) | -0.001*** (0.000) | -0.000 (0.000) | -0.000 (0.000) | -0.002*** (0.001) | -0.002*** (0.001) | -0.002*** (0.001) | -0.001*** (0.000) | -0.001*** (0.000) | -0.001*** (0.000) |
| TOT_Gr | -0.038 (0.028) | -0.025 (0.026) | -0.022 (0.022) | -0.019 (0.022) | -0.071* (0.036) | -0.070* (0.036) | -0.070* (0.036) | -0.026 (0.026) | -0.026 (0.026) | -0.026 (0.026) |
| Observations | 3474 | 3996 | 4507 | 4758 | 2424 | 2424 | 2424 | 3996 | 4001 | 3996 |
| R-squared | 0.033 | 0.023 | 0.017 | 0.017 | 0.034 | 0.034 | 0.033 | 0.025 | 0.025 | 0.023 |

Estimation method: Linear Probability Model – OLS. All the specifications include year fixed effects.  Robust standard errors in parentheses.
Annual data over 1960-2005 when available. Significance levels: *** 0.01, ** 0.05, * 0.1

**Table 2 A. Structural reforms and Down-breaks in Growth**

| | (1) down3 | (2) down3 | (3) down3 | (4) down3 | (5) down3 | (6) down3 | (7) down3 | (8) down3 | (9) down3 | (10) down3 |
|---|---|---|---|---|---|---|---|---|---|---|
| TR_ch | 0.631 (0.492) | | | | | | | | | |
| CUR100_ch | | -0.776* (0.461) | | | | | | | | |
| AG_ch | | | -0.585 (0.485) | | | | | | | |
| NW_ch | | | | -1.480 (1.321) | | | | | | |
| DF_ch | | | | | -1.092 (1.141) | | | | | |
| BK_ch | | | | | | -0.913 (1.016) | | | | |
| SM_ch | | | | | | | -0.341 (0.649) | | | |
| CAP100_ch | | | | | | | | -0.832* (0.490) | | |
| CAPRES_ch | | | | | | | | | -0.899** (0.433) | |
| CAPNONRES_ch | | | | | | | | | | -0.006 (0.310) |
| polity21 | -0.031*** (0.005) | -0.022*** (0.005) | -0.002 (0.005) | -0.007 (0.005) | -0.038*** (0.007) | -0.038*** (0.007) | -0.038*** (0.007) | -0.022*** (0.005) | -0.022*** (0.005) | -0.022*** (0.005) |
| TOT_Gr | -0.094 (0.247) | -0.188 (0.203) | -0.165 (0.212) | -0.180 (0.206) | -0.262 (0.269) | -0.265 (0.269) | -0.267 (0.268) | -0.185 (0.204) | -0.188 (0.205) | -0.183 (0.204) |
| Observations | 2610 | 3141 | 3358 | 3794 | 1714 | 1714 | 1714 | 3141 | 3141 | 3141 |
| Pseudo R-square | 0.1012 | 0.085 | 0.0569 | 0.0644 | 0.1064 | 0.1062 | 0.1054 | 0.0853 | 0.0872 | 0.0836 |

Estimation method: Maximum Likelihood Probit.  Robust standard errors in parentheses. All the specifications include year fixed effects.
Annual data over 1960-2005 when available. Significance levels: *** 0.01, ** 0.05, * 0.1

## Table 2 B. Structural reforms and Down-breaks in Growth

| | (1) down3 | (2) down3 | (3) down3 | (4) down3 | (5) down3 | (6) down3 | (7) down3 | (8) down3 | (9) down3 | (10) down3 |
|---|---|---|---|---|---|---|---|---|---|---|
| TR_ch | 0.063 (0.058) | | | | | | | | | |
| CUR100_ch | | -0.058 (0.039) | | | | | | | | |
| AG_ch | | | -0.026 (0.021) | | | | | | | |
| NW_ch | | | | -0.030 (0.022) | | | | | | |
| DF_ch | | | | | -0.080 (0.081) | | | | | |
| BK_ch | | | | | | -0.066 (0.072) | | | | |
| SM_ch | | | | | | | -0.022 (0.036) | | | |
| CAP100_ch | | | | | | | | -0.056 (0.037) | | |
| CAPRES_ch | | | | | | | | | -0.072* (0.038) | |
| CAPNONRES_ch | | | | | | | | | | 0.003 (0.022) |
| polity21 | -0.003*** (0.001) | -0.002*** (0.000) | -0.000 (0.000) | -0.000 (0.000) | -0.003*** (0.001) | -0.003*** (0.001) | -0.003*** (0.001) | -0.002*** (0.000) | -0.002*** (0.000) | -0.002*** (0.000) |
| TOT_Gr | -0.017 (0.030) | -0.022 (0.025) | -0.019 (0.022) | -0.021 (0.022) | -0.038 (0.033) | -0.039 (0.033) | -0.038 (0.033) | -0.022 (0.025) | -0.022 (0.025) | -0.022 (0.025) |
| Observations | 3474 | 3996 | 4507 | 4758 | 2424 | 2424 | 2424 | 3996 | 4001 | 3996 |
| R-squared | 0.057 | 0.048 | 0.036 | 0.034 | 0.060 | 0.060 | 0.059 | 0.048 | 0.049 | 0.047 |

Estimation method: Linear Probability Model – OLS. All the specifications include year fixed effects. Robust standard errors in parentheses. Annual data over 1960-2005 when available. Significance levels: *** 0.01, ** 0.05, * 0.1

**Table 3 A. Structural reforms and Up-breaks in Growth**

| | (1) up3 | (2) up3 | (3) up3 | (4) up3 | (5) up3 | (6) up3 | (7) up3 | (8) up3 | (9) up3 | (10) up3 |
|---|---|---|---|---|---|---|---|---|---|---|
| TR_ch | 0.023 (0.054) | | | | | | | | | |
| CUR100_ch | | 0.071 (0.057) | | | | | | | | |
| AG_ch | | | 0.050 (0.041) | | | | | | | |
| NW_ch | | | | -0.000 (0.053) | | | | | | |
| DF_ch | | | | | 0.177** (0.089) | | | | | |
| BK_ch | | | | | | 0.155* (0.082) | | | | |
| SM_ch | | | | | | | 0.029 (0.042) | | | |
| CAP100_ch | | | | | | | | 0.124** (0.048) | | |
| CAPRES_ch | | | | | | | | | 0.098*** (0.036) | |
| CAPNONRES_ch | | | | | | | | | | 0.076* (0.042) |
| polity21 | 0.000 (0.001) | 0.000 (0.001) | 0.000 (0.001) | 0.000 (0.001) | 0.001 (0.001) | 0.001 (0.001) | 0.001 (0.001) | 0.000 (0.001) | 0.000 (0.001) | 0.000 (0.001) |
| TOT_Gr | -0.036 (0.028) | -0.021 (0.025) | -0.019 (0.021) | -0.017 (0.021) | -0.061* (0.037) | -0.060 (0.037) | -0.060 (0.037) | -0.022 (0.025) | -0.022 (0.025) | -0.022 (0.025) |
| Observations | 3474 | 3996 | 4507 | 4758 | 2424 | 2424 | 2424 | 3996 | 4001 | 3996 |
| R-squared | 0.105 | 0.088 | 0.093 | 0.089 | 0.109 | 0.109 | 0.107 | 0.089 | 0.089 | 0.088 |

Estimation method: Linear Probability Model – OLS. All the specifications include country and year fixed effects.  Robust standard errors in parentheses. Annual data over 1960-2005 when available. Significance levels: *** 0.01, ** 0.05, * 0.1

**Table 3 B. Structural reforms and Up-breaks in Growth**

| | (1) up3 | (2) up3 | (3) up3 | (4) up3 | (5) up3 | (6) up3 | (7) up3 | (8) up3 | (9) up3 | (10) up3 |
|---|---|---|---|---|---|---|---|---|---|---|
| TR_ch | 0.284 (0.545) | | | | | | | | | |
| CUR100_ch | | 0.939* (0.549) | | | | | | | | |
| AG_ch | | | 0.674 (0.460) | | | | | | | |
| NW_ch | | | | -0.365 (1.049) | | | | | | |
| DF_ch | | | | | 2.222** (1.100) | | | | | |
| BK_ch | | | | | | 2.070** (0.991) | | | | |
| SM_ch | | | | | | | 0.200 (0.612) | | | |
| CAP100_ch | | | | | | | | 1.515*** (0.493) | | |
| CAPRES_ch | | | | | | | | | 1.240*** (0.394) | |
| CAPNONRES_ch | | | | | | | | | | 1.067** (0.497) |
| polity21 | -0.018* (0.011) | -0.015 (0.010) | 0.000 (0.011) | -0.003 (0.010) | -0.014 (0.013) | -0.014 (0.013) | -0.014 (0.013) | -0.016 (0.010) | -0.015 (0.010) | -0.015 (0.010) |
| TOT_Gr | -0.531 (0.367) | -0.308 (0.314) | -0.308 (0.333) | -0.302 (0.316) | -0.839** (0.394) | -0.831** (0.394) | -0.808** (0.393) | -0.310 (0.315) | -0.310 (0.315) | -0.320 (0.315) |
| Observations | 3474 | 3996 | 4507 | 4758 | 2424 | 2424 | 2424 | 3996 | 4001 | 3996 |

Estimation method: Random Effects Probit. All the specifications include year fixed effects. Standard errors in parentheses.
Annual data over 1960-2005 when available. Significance levels:  *** 0.01, ** 0.05, * 0.1.

| | (1) down3 | (2) down3 | (3) down3 | (4) down3 | (5) down3 | (6) down3 | (7) down3 | (8) down3 | (9) down3 | (10) down3 |
|---|---|---|---|---|---|---|---|---|---|---|
| TR_ch | 0.063 (0.058) | | | | | | | | | |
| CUR100_ch | | -0.063 (0.039) | | | | | | | | |
| AG_ch | | | -0.017 (0.022) | | | | | | | |
| NW_ch | | | | -0.026 (0.027) | | | | | | |
| DF_ch | | | | | -0.073 (0.081) | | | | | |
| BK_ch | | | | | | -0.059 (0.072) | | | | |
| SM_ch | | | | | | | -0.021 (0.037) | | | |
| CAP100_ch | | | | | | | | -0.060 (0.038) | | |
| CAPRES_ch | | | | | | | | | -0.075** (0.038) | |
| CAPNONRES_ch | | | | | | | | | | -0.001 (0.024) |
| polity21 | -0.004*** (0.001) | -0.004*** (0.001) | -0.001 (0.001) | -0.001* (0.001) | -0.006*** (0.002) | -0.006*** (0.002) | -0.006*** (0.002) | -0.004*** (0.001) | -0.004*** (0.001) | -0.004*** (0.001) |
| TOT_Gr | -0.016 (0.030) | -0.023 (0.025) | -0.019 (0.021) | -0.022 (0.021) | -0.037 (0.032) | -0.038 (0.032) | -0.037 (0.033) | -0.023 (0.025) | -0.023 (0.025) | -0.023 (0.025) |
| Observations | 3474 | 3996 | 4507 | 4758 | 2424 | 2424 | 2424 | 3996 | 4001 | 3996 |
| R-squared | 0.110 | 0.097 | 0.097 | 0.094 | 0.116 | 0.116 | 0.116 | 0.097 | 0.098 | 0.097 |

Estimation method: Linear Probability Model – OLS. All the specifications include country and year fixed effects.  Robust standard errors in parentheses. Annual data over 1960-2005 when available. Significance levels: ***  0.01, **  0.05, * 0.1

## Table 4 B. Structural reforms and Down-breaks in Growth

| | (1) down3 | (2) down3 | (3) down3 | (4) down3 | (5) down3 | (6) down3 | (7) down3 | (8) down3 | (9) down3 | (10) down3 |
|---|---|---|---|---|---|---|---|---|---|---|
| TR_ch | 0.743 (0.490) | | | | | | | | | |
| CUR100_ch | | -0.921 (0.620) | | | | | | | | |
| AG_ch | | | -0.563 (0.672) | | | | | | | |
| NW_ch | | | | -1.719 (1.799) | | | | | | |
| DF_ch | | | | | -1.087 (1.203) | | | | | |
| BK_ch | | | | | | -0.893 (1.091) | | | | |
| SM_ch | | | | | | | -0.379 (0.712) | | | |
| CAP100_ch | | | | | | | | -0.884 (0.583) | | |
| CAPRES_ch | | | | | | | | | -0.993** (0.442) | |
| CAPNONRES_ch | | | | | | | | | | 0.050 (0.560) |
| polity21 | -0.037*** (0.009) | -0.031*** (0.008) | -0.008 (0.010) | -0.014 (0.009) | -0.047*** (0.011) | -0.047*** (0.011) | -0.047*** (0.011) | -0.031*** (0.008) | -0.031*** (0.008) | -0.031*** (0.008) |
| TOT_Gr | -0.142 (0.310) | -0.228 (0.269) | -0.189 (0.291) | -0.229 (0.278) | -0.327 (0.366) | -0.330 (0.366) | -0.336 (0.366) | -0.223 (0.269) | -0.228 (0.270) | -0.218 (0.268) |
| Observations | 3474 | 3996 | 4507 | 4758 | 2424 | 2424 | 2424 | 3996 | 4001 | 3996 |

Estimation method: Random Effects Probit. All the specifications include year fixed effects. Standard errors in parentheses.
Annual data over 1960-2005 when available. Significance levels:  *** 0.01, ** 0.05, * 0.1.

## Table 5. Baseline Growth Regressions

| Dependent Variable: lnGDP (t)- lnGDP (t-1) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| **Real Reforms** | | | | | | | | | | |
| Trade (t-1) | 0.022** (0.009) | | | | | | | | | |
| Current Account (t-1) | | 0.028*** (0.007) | | | | | | | | |
| Agriculture (t-1) | | | 0.010 (0.007) | | | | | | | |
| Networks (t-1) | | | | -0.001 (0.009) | | | | | | |
| **Financial Reforms** | | | | | | | | | | |
| Domestic Finance (t-1) | | | | | 0.064*** (0.014) | | | | | |
| Banking (t-1) | | | | | | 0.050*** (0.012) | | | | |
| Securities (t-1) | | | | | | | 0.037*** (0.008) | | | |
| Capital (t-1) | | | | | | | | 0.019*** (0.007) | | |
| Capital (residents) (t-1) | | | | | | | | | 0.015** (0.006) | |
| Capital (non-residents) (t-1) | | | | | | | | | | 0.013** (0.006) |
| lnGDP (t-1) | -0.040*** (0.009) | -0.038*** (0.009) | -0.026*** (0.008) | -0.032*** (0.008) | -0.042*** (0.008) | -0.041*** (0.008) | -0.047*** (0.008) | -0.037*** (0.008) | -0.036*** (0.009) | -0.037*** (0.008) |
| Observations | 4049 | 4573 | 4402 | 4918 | 2653 | 2653 | 2653 | 4573 | 4599 | 4573 |
| Adjusted R-squared | 0.130 | 0.093 | 0.098 | 0.091 | 0.160 | 0.155 | 0.156 | 0.091 | 0.090 | 0.091 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses.   ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted.

**Table 6. Growth Residuals under Different Levels of Reform and Constraints on Executive Power**

| Constraints on Executive | Reforms | | |
|---|---|---|---|
| | *High* | *Low* | *Difference* |
| **Trade Index** | | | |
| *High* | 0.09435 | -0.08597 | 0.18032 |
| *Middle* | 0.07771 | -0.08166 | 0.15937 |
| *Low* | 0.48323 | -0.51286 | 0.99608 |
| **Current Account Restrictions Index** | | | |
| *High* | 0.09331 | -0.08832 | 0.18163 |
| *Middle* | 0.46251 | -0.45081 | 0.91332 |
| *Low* | 0.67101 | -0.89676 | 1.56777 |
| **Agriculture Index** | | | |
| *High* | -0.05648 | 0.06476 | -0.12124 |
| *Middle* | 0.20025 | -0.18901 | 0.38926 |
| *Low* | -0.47480 | 0.43955 | -0.91435 |
| **Networks Index** | | | |
| *High* | -0.18681 | 0.11574 | -0.30256 |
| *Middle* | -0.15167 | 0.17352 | -0.32519 |
| *Low* | -0.02821 | 0.06062 | -0.08883 |
| **Domestic Finance Index** | | | |
| *High* | 0.21161 | -0.22013 | 0.43174 |
| *Middle* | 0.90357 | -0.87318 | 1.77676 |
| *Low* | 0.78748 | -0.86073 | 1.64821 |
| **Banking Index** | | | |
| *High* | 0.10388 | -0.10118 | 0.20506 |
| *Middle* | 0.73217 | -0.72641 | 1.45858 |
| *Low* | 0.65072 | -0.79532 | 1.44604 |
| **Securities Index** | | | |
| *High* | 0.45956 | -0.44959 | 0.90916 |
| *Middle* | 0.44347 | -0.45053 | 0.89401 |
| *Low* | 0.51990 | -0.50847 | 1.02838 |
| **Capital Acount Index** | | | |
| *High* | 0.09012 | -0.09048 | 0.18061 |
| *Middle* | 0.08838 | -0.08191 | 0.17029 |
| *Low* | 0.79224 | -1.14005 | 1.93228 |
| **Capital Acount (Residents) Index** | | | |
| *High* | 0.09036 | -0.09000 | 0.18035 |
| *Middle* | 0.03323 | -0.03145 | 0.06468 |
| *Low* | 0.50517 | -0.66674 | 1.17191 |
| **Capital Acount (Nonresidents) Index** | | | |
| *High* | 0.09409 | -0.09422 | 0.18831 |
| *Middle* | 0.05540 | -0.05347 | 0.10887 |
| *Low* | 0.16962 | -0.20073 | 0.37036 |

Source: International Financial Statistics, Penn World Tables version 6.2, Polity VI, World Development Indicators, and IMF staff estimates.

Notes: The table shows residual growth under different levels of reforms (high and low) and different levels of constraints on the executive (high, middle, low). High and low levels of reform are defined as above and below the median, respectively. High, middle, and low constraints on executives categories represent countries above, within, and below one-standard deviation from our sample mean, respectively. All growth residuals are calculated with country and year fixed effects, and annual data over 1960-2005 when available. GDP in real terms and PPP adjusted.

**Table 7. Growth Regressions with Constraint to the Executive Interactions (Real Sector Reforms)**

| Dependent variable: lnGDP (t) – lnGDP (t-1) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Real Reforms** | | | | |
| Trade (t-1)*CE-High | 0.017* | | | |
| | (0.009) | | | |
| *CE-Middle | 0.027** | | | |
| | (0.011) | | | |
| *CE-Low | 0.005 | | | |
| | (0.035) | | | |
| Current Account (t-1)*CE-High | | 0.024** | | |
| | | (0.011) | | |
| *CE-Middle | | 0.031*** | | |
| | | (0.009) | | |
| *CE-Low | | 0.038* | | |
| | | (0.022) | | |
| Agriculture (t-1)*CE-High | | | 0.020* | |
| | | | (0.012) | |
| *CE-Middle | | | 0.014 | |
| | | | (0.009) | |
| *CE-Low | | | -0.030 | |
| | | | (0.024) | |
| Networks (t-1)*CE-High | | | | -0.009 |
| | | | | (0.009) |
| *CE-Middle | | | | -0.004 |
| | | | | (0.012) |
| *CE-Low | | | | -0.068** |
| | | | | (0.029) |
| lnGDP (t-1) | -0.040*** | -0.036*** | -0.026*** | -0.032*** |
| | (0.009) | (0.009) | (0.009) | (0.008) |
| Observations | 3956 | 4390 | 4230 | 4663 |
| Adjusted R-squared | 0.129 | 0.090 | 0.104 | 0.099 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted. High, middle, and low constraints on executives categories represent countries above, within and below one-standard deviation from our sample mean, respectively.

## Table 8. Growth Regressions with Constraint to the Executive Interactions (Financial Sector Reforms)

| Dependent Variable: lnGDP (t) − lnGDP(t-1) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Financial Reforms** | | | | | | |
| Domestic Finance (t-1)*CE-High | 0.062*** | | | | | |
| | (0.015) | | | | | |
| *CE-Middle | 0.064*** | | | | | |
| | (0.014) | | | | | |
| *CE-Low | 0.056*** | | | | | |
| | (0.018) | | | | | |
| Banking (t-1)*CE-High | | 0.046*** | | | | |
| | | (0.014) | | | | |
| *CE-Middle | | 0.051*** | | | | |
| | | (0.013) | | | | |
| *CE-Low | | 0.040** | | | | |
| | | (0.017) | | | | |
| Securities (t-1)*CE-High | | | 0.040*** | | | |
| | | | (0.008) | | | |
| *CE-Middle | | | 0.035*** | | | |
| | | | (0.009) | | | |
| *CE-Low | | | 0.033* | | | |
| | | | (0.017) | | | |
| Capital (t-1)*CE-High | | | | 0.017 | | |
| | | | | (0.011) | | |
| *CE-Middle | | | | 0.020** | | |
| | | | | (0.008) | | |
| *CE-Low | | | | 0.041 | | |
| | | | | (0.042) | | |
| Capital (resident) (t-1)*CE-High | | | | | 0.012 | |
| | | | | | (0.009) | |
| *CE-Middle | | | | | 0.014* | |
| | | | | | (0.007) | |
| *CE-Low | | | | | 0.072* | |
| | | | | | (0.037) | |
| Capital (non-resident) (t-1)*CE-High | | | | | | 0.014 |
| | | | | | | (0.010) |
| *CE-Middle | | | | | | 0.015* |
| | | | | | | (0.008) |
| *CE-Low | | | | | | -0.009 |
| | | | | | | (0.022) |
| lGDP1 | -0.042*** | -0.041*** | -0.048*** | -0.035*** | -0.034*** | -0.035*** |
| | (0.008) | (0.008) | (0.008) | (0.009) | (0.009) | (0.009) |
| Observations | 2620 | 2620 | 2620 | 4390 | 4416 | 4390 |
| Adjusted R-squared | 0.156 | 0.151 | 0.153 | 0.088 | 0.088 | 0.087 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted. High, middle, and low constraints on executives categories represent countries above, within and blow one-standard deviation from our sample mean, respectively.

# Table 9. Schumpeterian Growth Regressions

| Dependent variable: lnGDP (t) – lnGDP (t-1) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| **Real Reforms** | | | | | | | | | | |
| Trade (t-1) | 0.029*** (0.011) | | | | | | | | | |
| Current Account (t-1) | | 0.028*** (0.009) | | | | | | | | |
| Agriculture (t-1) | | | 0.016* (0.009) | | | | | | | |
| Networks (t-1) | | | | 0.012 (0.012) | | | | | | |
| **Financial Reforms** | | | | | | | | | | |
| Domestic Finance (t-1) | | | | | 0.075*** (0.014) | | | | | |
| Banking (t-1) | | | | | | 0.064*** (0.013) | | | | |
| Securities (t-1) | | | | | | | 0.036*** (0.010) | | | |
| Capital (t-1) | | | | | | | | 0.017** (0.008) | | |
| Capital (residents) (t-1) | | | | | | | | | 0.017** (0.007) | |
| Capital (non-residents) (t-1) | | | | | | | | | | 0.008 (0.009) |
| [Reformj * (GDPj / GDPus) ] (t-1) | -0.049** (0.023) | -0.010 (0.023) | -0.010 (0.023) | -0.033** (0.014) | -0.038** (0.015) | -0.042*** (0.014) | -0.013 (0.015) | -0.004 (0.018) | -0.010 (0.015) | 0.005 (0.018) |
| [GDPj / GDPus](t-1) | -0.065*** (0.023) | -0.074*** (0.021) | -0.057*** (0.019) | -0.064*** (0.018) | -0.080*** (0.021) | -0.079*** (0.020) | -0.103*** (0.021) | -0.076*** (0.022) | -0.071*** (0.020) | -0.081*** (0.023) |
| Observations | 4049 | 4573 | 4402 | 4918 | 2653 | 2653 | 2653 | 4573 | 4599 | 4573 |
| Adjusted R-squared | 0.119 | 0.082 | 0.093 | 0.082 | 0.151 | 0.148 | 0.142 | 0.080 | 0.080 | 0.080 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted.

## Table 10. Baseline Growth Regressions
## GMM Arellano Bond

| Dependent variable: lnGDP (t) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| **Real Reforms** | | | | | | | | | | |
| Trade (t-1) | 0.048*** | | | | | | | | | |
| | (0.018) | | | | | | | | | |
| Current Account (t-1) | | 0.035** | | | | | | | | |
| | | (0.016) | | | | | | | | |
| Agriculture (t-1) | | | 0.044** | | | | | | | |
| | | | (0.018) | | | | | | | |
| Networks (t-1) | | | | -0.008 | | | | | | |
| | | | | (0.022) | | | | | | |
| | | | | | | | | | | |
| **Financial Reforms** | | | | | | | | | | |
| Domestic Finance (t-1) | | | | | 0.102*** | | | | | |
| | | | | | (0.021) | | | | | |
| Banking (t-1) | | | | | | 0.084*** | | | | |
| | | | | | | (0.019) | | | | |
| Securities (t-1) | | | | | | | 0.059*** | | | |
| | | | | | | | (0.013) | | | |
| Capital (t-1) | | | | | | | | 0.020 | | |
| | | | | | | | | (0.016) | | |
| Capital (residents) (t-1) | | | | | | | | | 0.021* | |
| | | | | | | | | | (0.012) | |
| Capital (non-residents) (t-1) | | | | | | | | | | 0.009 |
| | | | | | | | | | | (0.015) |
| lnGDP (t-1) | 0.9*** | 0.914*** | 0.931*** | 0.912*** | 0.958*** | 0.959*** | 0.944*** | 0.915*** | 0.917*** | 0.915*** |
| | (0.027) | (0.019) | (0.021) | (0.020) | (0.012) | (0.011) | (0.013) | (0.019) | (0.019) | (0.019) |
| Test 2nd order ser. corr.(p-value) | 0.4882 | 0.5843 | 0.3501 | 0.4178 | 0.7945 | 0.8109 | 0.7577 | 0.5801 | 0.5805 | 0.5832 |
| Sargan Test | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| Observations | 3895 | 4454 | 4279 | 4787 | 2562 | 2562 | 2562 | 4454 | 4479 | 4454 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by GMM – Arellano Bond with year fixed effects. GMM – Arellano Bond estimator uses two lags and more of income as instrument. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted.

## Table 11. Growth Regressions with Constraint to the Executive Interactions GMM Arellano Bond (Real Sector Reforms)

| Dependent variable: lnGDP (t) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Real Reforms** | | | | |
| Trade (t-1) * CE-High | 0.061* | | | |
| | (0.035) | | | |
| * CE-Middle | 0.061*** | | | |
| | (0.020) | | | |
| * CE-Low | -0.013 | | | |
| | (0.076) | | | |
| Current Account (t-1) * CE-High | | 0.017 | | |
| | | (0.038) | | |
| * CE-Middle | | 0.038* | | |
| | | (0.022) | | |
| * CE-Low | | 0.009 | | |
| | | (0.054) | | |
| Agriculture (t-1) * CE-High | | | 0.059** | |
| | | | (0.027) | |
| * CE-Middle | | | 0.053** | |
| | | | (0.021) | |
| * CE-Low | | | -0.052 | |
| | | | (0.040) | |
| Networks (t-1) * CE-High | | | | -0.035 |
| | | | | (0.029) |
| * CE-Middle | | | | -0.006 |
| | | | | (0.024) |
| * CE-Low | | | | -0.196** |
| | | | | (0.083) |
| lnGDP (t-1) | 0.899*** | 0.914*** | 0.927*** | 0.907*** |
| | (0.028) | (0.021) | (0.023) | (0.022) |
| Test 2nd order ser. corr.(p-value) | 0.4922 | 0.6564 | 0.3521 | 0.3905 |
| Sargan Test | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| Observations | 3806 | 4276 | 4112 | 4540 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors in brackets. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by GMM – Arellano Bond with year fixed effects. GMM – Arellano Bond estimator uses two lags and more of income as instrument. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted.

**Table 12. Baseline Growth Regressions with Constraint to the Executive Interactions**
**GMM Arellano Bond**
**(Financial Sector Reforms)**

| Dependent variable:<br>lnGDP (t) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Financial Reforms** | | | | | | |
| Domestic Finance (t-1) * CE-High | 0.086*** | | | | | |
| | (0.025) | | | | | |
| * CE-Middle | 0.106*** | | | | | |
| | (0.023) | | | | | |
| * CE-Low | 0.099*** | | | | | |
| | (0.025) | | | | | |
| Banking (t-1) * CE-High | | 0.062*** | | | | |
| | | (0.022) | | | | |
| * CE-Middle | | 0.089*** | | | | |
| | | (0.02) | | | | |
| * CE-Low | | 0.077*** | | | | |
| | | (0.022) | | | | |
| Securities (t-1) * CE-High | | | 0.063*** | | | |
| | | | (0.019) | | | |
| * CE-Middle | | | 0.059*** | | | |
| | | | (0.014) | | | |
| * CE-Low | | | 0.079** | | | |
| | | | (0.031) | | | |
| Capital (t-1) * CE-High | | | | 0.011 | | |
| | | | | (0.041) | | |
| * CE-Middle | | | | 0.028 | | |
| | | | | (0.019) | | |
| * CE-Low | | | | -0.060 | | |
| | | | | (0.069) | | |
| Capital (residents) (t-1) * CE-High | | | | | 0.006 | |
| | | | | | (0.032) | |
| * CE-Middle | | | | | 0.027** | |
| | | | | | (0.013) | |
| * CE-Low | | | | | -0.006 | |
| | | | | | (0.057) | |
| Capital (non-residents) (t-1) * CE-High | | | | | | 0.022 |
| | | | | | | (0.039) |
| * CE-Middle | | | | | | 0.016 |
| | | | | | | (0.019) |
| * CE-Low | | | | | | -0.080* |
| | | | | | | (0.0474 |
| lnGDP (t-1) | 0.958*** | 0.961*** | 0.942*** | 0.912*** | 0.916*** | 0.912*** |
| | (0.012) | (0.012) | (0.014) | (0.022) | (0.021) | (0.021) |
| Test 2nd order ser. corr.(p-value) | 0.837 | 0.8472 | 0.7903 | 0.6594 | 0.6555 | 0.6557 |
| Sargan Test | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| Observations | 2530 | 2530 | 2530 | 4276 | 4301 | 4276 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors in brackets. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by GMM – Arellano Bond with year fixed effects. Income instrumented by using two lags and more. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted.

## Table 13. Baseline Growth Regressions with Controls

| Dependent Variable: lnGDP (t)- lnGDP (t-1) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| **Real Reforms** | | | | | | | | | | |
| Trade (t-1) | 0.022* (0.012) | | | | | | | | | |
| Current Account (t-1) | | 0.033*** (0.008) | | | | | | | | |
| Agriculture (t-1) | | | 0.021** (0.009) | | | | | | | |
| Networks (t-1) | | | | -0.008 (0.012) | | | | | | |
| **Financial Reforms** | | | | | | | | | | |
| Domestic Finance (t-1) | | | | | 0.060*** (0.015) | | | | | |
| Banking (t-1) | | | | | | 0.046*** (0.014) | | | | |
| Securities (t-1) | | | | | | | 0.035*** (0.009) | | | |
| Capital (t-1) | | | | | | | | 0.023** (0.009) | | |
| Capital (residents) (t-1) | | | | | | | | | 0.016** (0.007) | |
| Capital (non-residents) (t-1) | | | | | | | | | | 0.019** (0.009) |
| lnGDP (t-1) | -0.049*** (0.010) | -0.045*** (0.013) | -0.042*** (0.013) | -0.051*** (0.015) | -0.060*** (0.012) | -0.059*** (0.012) | -0.067*** (0.013) | -0.046*** (0.012) | -0.045*** (0.013) | -0.046*** (0.012) |
| Democracy (t-1) | -0.001 (0.000) | -0.001* (0.000) | -0.001 (0.000) | -0.001 (0.000) | -0.000 (0.000) | -0.000 (0.000) | -0.000 (0.001) | -0.001* (0.000) | -0.001 (0.000) | -0.001* (0.000) |
| Terms of trade (t-1) | 0.000 (0.000) | -0.000 (0.000) | -0.000 (0.000) | -0.000 (0.000) | 0.000 (0.000) | 0.000 (0.000) | -0.000 (0.000) | -0.000 (0.000) | -0.000 (0.000) | -0.000 (0.000) |
| Tertiary enroll (t-1) | 0.029* (0.016) | 0.018 (0.019) | 0.034 (0.023) | 0.030 (0.022) | 0.026 (0.018) | 0.027 (0.018) | 0.028 (0.019) | 0.012 (0.019) | 0.016 (0.020) | 0.013 (0.020) |
| Observations | 2616 | 2807 | 2606 | 2807 | 2114 | 2114 | 2114 | 2807 | 2810 | 2807 |
| Adjusted R-squared | 0.128 | 0.117 | 0.139 | 0.129 | 0.135 | 0.130 | 0.133 | 0.114 | 0.112 | 0.113 |

Source: International Financial Statistics, Penn World Tables version 6.2, Polity IV, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted.

**Table 14. Growth Regressions with Constraint to the Executive Interactions and Controls (Real Sector Reforms)**

| Dependent Variable: lnGDP (t)- lnGDP (t-1) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Real Reforms** | | | | |
| Trade (t-1)*CE-High | 0.016 | | | |
| | (0.012) | | | |
| *CE-Middle | 0.025* | | | |
| | (0.013) | | | |
| *CE-Low | 0.018 | | | |
| | (0.046) | | | |
| Current Account (t-1)*CE-High | | 0.033*** | | |
| | | (0.011) | | |
| *CE-Middle | | 0.033*** | | |
| | | (0.010) | | |
| *CE-Low | | 0.027 | | |
| | | (0.017) | | |
| Agriculture (t-1)*CE-High | | | 0.025 | |
| | | | (0.018) | |
| *CE-Middle | | | 0.022** | |
| | | | (0.010) | |
| *CE-Low | | | 0.003 | |
| | | | (0.007) | |
| Networks (t-1)*CE-High | | | | -0.010 |
| | | | | (0.011) |
| *CE-Middle | | | | -0.007 |
| | | | | (0.016) |
| *CE-Low | | | | -0.015 |
| | | | | (0.036) |
| lnGDP (t-1) | -0.050*** | -0.045*** | -0.042*** | -0.051*** |
| | (0.010) | (0.013) | (0.013) | (0.015) |
| Democracy (t-1) | -0.001* | -0.001* | -0.001 | -0.001 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Terms of trade (t-1) | 0.000 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Tertiary enroll (t-1) | 0.030* | 0.018 | 0.032 | 0.032 |
| | (0.017) | (0.018) | (0.023) | (0.024) |
| Observations | 2616 | 2807 | 2606 | 2807 |
| Adjusted R-squared | 0.128 | 0.117 | 0.139 | 0.128 |

Source: International Financial Statistics, Penn World Tables version 6.2, Polity IV, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted. High, middle, and low constraints on executives categories represent countries above, within and blow one-standard deviation from our sample mean, respectively.

**Table 15. Growth Regressions with Constraint to the Executive Interactions and Controls (Financial Sector Reforms)**

| Dependent Variable: lnGDP (t) – lnGDP(t-1) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Financial Reforms** | | | | | | |
| Domestic Finance (t-1)*CE-High | 0.057*** | | | | | |
| | (0.017) | | | | | |
| *CE-Middle | 0.061*** | | | | | |
| | (0.017) | | | | | |
| *CE-Low | 0.049** | | | | | |
| | (0.019) | | | | | |
| Banking (t-1)*CE-High | | 0.040*** | | | | |
| | | (0.015) | | | | |
| *CE-Middle | | 0.048*** | | | | |
| | | (0.016) | | | | |
| *CE-Low | | 0.033* | | | | |
| | | (0.018) | | | | |
| Securities (t-1)*CE-High | | | 0.038*** | | | |
| | | | (0.009) | | | |
| *CE-Middle | | | 0.034*** | | | |
| | | | (0.012) | | | |
| *CE-Low | | | 0.025 | | | |
| | | | (0.023) | | | |
| Capital (t-1)*CE-High | | | | 0.024* | | |
| | | | | (0.014) | | |
| *CE-Middle | | | | 0.022** | | |
| | | | | (0.011) | | |
| *CE-Low | | | | 0.030 | | |
| | | | | (0.018) | | |
| Capital (resident) (t-1)*CE-High | | | | | 0.016 | |
| | | | | | (0.011) | |
| *CE-Middle | | | | | 0.016* | |
| | | | | | (0.008) | |
| *CE-Low | | | | | 0.022 | |
| | | | | | (0.016) | |
| Capital (non-resident) (t-1)*CE-High | | | | | | 0.023* |
| | | | | | | (0.013) |
| *CE-Middle | | | | | | 0.017 |
| | | | | | | (0.012) |
| *CE-Low | | | | | | 0.020 |
| | | | | | | (0.025) |
| lnGDP (t-1) | -0.060*** | -0.059*** | -0.067*** | -0.046*** | -0.045*** | -0.046*** |
| | (0.012) | (0.012) | (0.013) | (0.013) | (0.013) | (0.013) |
| Democracy (t-1) | -0.000 | -0.000 | -0.000 | -0.001 | -0.001 | -0.001 |
| | (0.001) | (0.000) | (0.001) | (0.000) | (0.000) | (0.000) |
| Terms of trade (t-1) | 0.000 | 0.000 | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Tertiary enroll (t-1) | 0.027 | 0.030 | 0.026 | 0.011 | 0.016 | 0.011 |
| | (0.021) | (0.021) | (0.020) | (0.020) | (0.020) | (0.019) |
| Observations | 2114 | 2114 | 2114 | 2807 | 2810 | 2807 |
| Adjusted R-squared | 0.134 | 0.130 | 0.132 | 0.113 | 0.112 | 0.113 |

Source: International Financial Statistics, Penn World Tables version 6.2, Polity IV, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses.   ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted. High, middle, and low constraints on executives categories represent countries above, within and blow one-standard deviation from our sample mean, respectively.

## Table 16. Schumpeterian Growth Regressions with Controls

| Dependent variable:<br>lnGDP (t) - lnGDP (t-1) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Real Reforms** | | | | | | | | | | | |
| Trade (t-1) | 0.022 | | | | | | | | | | |
| | (0.0186) | | | | | | | | | | |
| Current Account (t-1) | | 0.049*** | | | | | | | | | |
| | | (0.0151) | | | | | | | | | |
| Agriculture (t-1) | | | 0.043*** | | | | | | | | |
| | | | (0.0134) | | | | | | | | |
| Networks (t-1) | | | | 0.011 | | | | | | | |
| | | | | (0.0156) | | | | | | | |
| **Financial Reforms** | | | | | | | | | | | |
| Domestic Finance (t-1) | | | | | 0.084*** | | | | | | |
| | | | | | (0.0180) | | | | | | |
| Banking (t-1) | | | | | | 0.073*** | | | | | |
| | | | | | | (0.0170) | | | | | |
| Securities (t-1) | | | | | | | 0.034*** | | | | |
| | | | | | | | (0.0124) | | | | |
| Capital Flows (t-1) | | | | | | | | 0.017* | | | |
| | | | | | | | | (0.0098) | | | |
| Capital (t-1) | | | | | | | | | 0.025 | | |
| | | | | | | | | | (0.0165) | | |
| Capital (residents) (t-1) | | | | | | | | | | 0.020** | |
| | | | | | | | | | | (0.0103) | |
| Capital (non-residents) (t-1) | | | | | | | | | | | 0.018 |
| | | | | | | | | | | | (0.0188) |
| [Reformj * (GDPj / GDPus)] (t-1) | -0.028 | -0.054* | -0.045 | -0.130 | -0.059*** | -0.064*** | -0.009 | -0.017 | -0.0840 | -0.027 | -0.017 |
| | (0.0513) | (0.0307) | (0.0298) | (0.0193) | (0.0237) | (0.0225) | (0.0178) | (0.0149) | (0.0299) | (0.0242) | (0.0310) |
| [GDPj / GDPus] (t-1) | -0.097** | -0.063** | -0.027 | -0.056* | -0.103*** | -0.103*** | -0.134*** | -0.130*** | -0.0268*** | -0.086*** | -0.089*** |
| | (0.0416) | (0.0283) | (0.0348) | (0.0343) | (0.0375) | (0.0369) | (0.0382) | (0.0358) | (0.0248) | (0.0229) | (0.0268) |
| Democracy (t-1) | -0.0003 | -0.0003 | -0.0002 | -0.0003 | -0.0003 | -0.0003 | -0.0002 | -0.0001 | -0.0003 | -0.0003 | -0.0003 |
| | (0.0002) | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) |
| Terms of trade (t-1) | 0.0000 | -0.0000 | -0.0001 | -0.0001 | 0.0000 | -0.0000 | -0.0000 | -0.0000 | -0.0000 | -0.0001 | -0.0001 |
| | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0002) | (0.0002) |
| Tertiary enroll (t-1) | 0.0012 | 0.0061 | 0.0047 | -0.0179 | 0.027 | 0.0321 | -0.0012 | 0.0041 | -0.0113 | -0.009 | -0.015 |
| | (0.0152) | (0.0156) | (0.0183) | (0.0188) | (0.0197) | (0.0201) | (0.0178) | (0.0186) | (0.0213) | (0.0204) | (0.0207) |
| Observations | 2616 | 1719 | 2310 | 2411 | 2114 | 2114 | 2114 | 2114 | 1719 | 1719 | 1719 |
| Adj. R-squared | 0.1154 | 0.1517 | 0.1255 | 0.1161 | 0.1181 | 0.1155 | 0.1089 | 0.1024 | 0.1390 | 0.1386 | 0.1378 |

Source: International Financial Statistics, Penn World Tables version 6.2, Polity IV, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Annual data over 1960-2005 when available. GDP in real terms and PPP adjusted.

## Table 17. Baseline Growth Regressions: 3-year intervals

| Dependent Variable:<br>lnGDP (t)- lnGDP (t-1) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| **Real Reforms** | | | | | | | | | | |
| Trade (t-1) | 0.033<br>(0.024) | | | | | | | | | |
| Current Account (t-1) | | 0.070***<br>(0.020) | | | | | | | | |
| Agriculture (t-1) | | | 0.014<br>(0.018) | | | | | | | |
| Networks (t-1) | | | | 0.004<br>(0.026) | | | | | | |
| **Financial Reforms** | | | | | | | | | | |
| Domestic Finance (t-1) | | | | | 0.137***<br>(0.038) | | | | | |
| Banking (t-1) | | | | | | 0.110***<br>(0.038) | | | | |
| Securities (t-1) | | | | | | | 0.074***<br>(0.021) | | | |
| Capital (t-1) | | | | | | | | 0.060***<br>(0.021) | | |
| Capital (residents) (t-1) | | | | | | | | | 0.041**<br>(0.021) | |
| Capital (non-residents) (t-1) | | | | | | | | | | 0.045**<br>(0.019) |
| lnGDP (t-1) | -0.115***<br>(0.020) | -0.119***<br>(0.022) | -0.095***<br>(0.024) | -0.107***<br>(0.023) | -0.183***<br>(0.027) | -0.182***<br>(0.027) | -0.195***<br>(0.027) | -0.120***<br>(0.022) | -0.117***<br>(0.022) | -0.118***<br>(0.021) |
| Observations | 1271 | 1466 | 1411 | 1581 | 802 | 802 | 802 | 1466 | 1472 | 1466 |
| Adjusted R-squared | 0.292 | 0.231 | 0.213 | 0.207 | 0.366 | 0.361 | 0.361 | 0.229 | 0.225 | 0.227 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses.   ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Three year interval data over 1960-2005. GDP in real terms and PPP adjusted.

**Table 18. Growth Regressions with Constraint to the Executive Interactions: 3-year intervals (Real Sector Reforms)**

| Dependent variable:<br>lnGDP (t) – lnGDP (t-1) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Real Reforms** | | | | |
| Trade (t-1)*CE-High | 0.037 | | | |
| | (0.026) | | | |
| *CE-Middle | 0.046 | | | |
| | (0.031) | | | |
| *CE-Low | -0.060 | | | |
| | (0.069) | | | |
| Current Account (t-1)*CE-High | | 0.062* | | |
| | | (0.032) | | |
| *CE-Middle | | 0.073*** | | |
| | | (0.026) | | |
| *CE-Low | | 0.133** | | |
| | | (0.053) | | |
| Agriculture (t-1)*CE-High | | | 0.075 | |
| | | | (0.051) | |
| *CE-Middle | | | 0.012 | |
| | | | (0.020) | |
| *CE-Low | | | -0.079 | |
| | | | (0.070) | |
| Networks (t-1)*CE-High | | | | -0.019 |
| | | | | (0.027) |
| *CE-Middle | | | | -0.001 |
| | | | | (0.035) |
| *CE-Low | | | | -0.161 |
| | | | | (0.111) |
| lnGDP (t-1) | -0.115*** | -0.113*** | -0.094*** | -0.105*** |
| | (0.020) | (0.024) | (0.024) | (0.024) |
| Observations | 1242 | 1409 | 1357 | 1501 |
| Adjusted R-squared | 0.293 | 0.221 | 0.226 | 0.221 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Three year interval data over 1960-2005. GDP in real terms and PPP adjusted. High, middle, and low constraints on executives categories represent countries above, within and blow one-standard deviation from our sample mean, respectively.

**Table 19. Growth Regressions with Constraint to the Executive Interactions: 3-year intervals (Financial Sector Reforms)**

| Dependent Variable: lnGDP (t) – lnGDP(t-1) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Financial Reforms** | | | | | | |
| Domestic Finance (t-1)*CE-High | 0.134*** | | | | | |
| | (0.042) | | | | | |
| *CE-Middle | 0.134*** | | | | | |
| | (0.041) | | | | | |
| *CE-Low | 0.110* | | | | | |
| | (0.063) | | | | | |
| Banking (t-1)*CE-High | | 0.104** | | | | |
| | | (0.041) | | | | |
| *CE-Middle | | 0.110*** | | | | |
| | | (0.041) | | | | |
| *CE-Low | | 0.078 | | | | |
| | | (0.061) | | | | |
| Securities (t-1)*CE-High | | | 0.082*** | | | |
| | | | (0.021) | | | |
| *CE-Middle | | | 0.066** | | | |
| | | | (0.028) | | | |
| *CE-Low | | | 0.070 | | | |
| | | | (0.078) | | | |
| Capital (t-1)*CE-High | | | | 0.047 | | |
| | | | | (0.033) | | |
| *CE-Middle | | | | 0.057** | | |
| | | | | (0.027) | | |
| *CE-Low | | | | 0.278*** | | |
| | | | | (0.105) | | |
| Capital (resident) (t-1)*CE-High | | | | | 0.032 | |
| | | | | | (0.027) | |
| *CE-Middle | | | | | 0.036 | |
| | | | | | (0.025) | |
| *CE-Low | | | | | 0.255** | |
| | | | | | (0.112) | |
| Capital (non-resident) (t-1)*CE-High | | | | | | 0.037 |
| | | | | | | (0.028) |
| *CE-Middle | | | | | | 0.043 |
| | | | | | | (0.026) |
| *CE-Low | | | | | | 0.142** |
| | | | | | | (0.059) |
| lnGDP (t-1) | -0.181*** | -0.179*** | -0.194*** | -0.111*** | -0.109*** | -0.111*** |
| | (0.028) | (0.028) | (0.028) | (0.023) | (0.023) | (0.023) |
| Observations | 792 | 792 | 792 | 1409 | 1415 | 1409 |
| Adjusted R-squared | 0.357 | 0.353 | 0.353 | 0.221 | 0.219 | 0.217 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Three year interval data over 1960-2005. GDP in real terms and PPP adjusted. High, middle, and low constraints on executives categories represent countries above, within and blow one-standard deviation from our sample mean, respectively.

# Table 20. Schumpeterian Growth Regressions: 3-year intervals

| Dependent variable: lnGDP (t) - lnGDP (t-1) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Real Reforms** | | | | | | | | | | | |
| Trade (t-1) | 0.041 | | | | | | | | | | |
| | (0.0317) | | | | | | | | | | |
| Current Account (t-1) | | 0.092*** | | | | | | | | | |
| | | (0.0337) | | | | | | | | | |
| Agriculture (t-1) | | | 0.038*** | | | | | | | | |
| | | | (0.0277) | | | | | | | | |
| Networks (t-1) | | | | 0.035 | | | | | | | |
| | | | | (0.0367) | | | | | | | |
| **Financial Reforms** | | | | | | | | | | | |
| Domestic Finance (t-1) | | | | | 0.174*** | | | | | | |
| | | | | | (0.0437) | | | | | | |
| Banking (t-1) | | | | | | 0.159*** | | | | | |
| | | | | | | (0.0431) | | | | | |
| Securities (t-1) | | | | | | | 0.061** | | | | |
| | | | | | | | (0.0283) | | | | |
| Capital Flows (t-1) | | | | | | | | 0.022** | | | |
| | | | | | | | | (0.0223) | | | |
| Capital (t-1) | | | | | | | | | 0.048** | | |
| | | | | | | | | | (0.0322) | | |
| Capital (residents) (t-1) | | | | | | | | | | 0.046* | |
| | | | | | | | | | | (0.0239) | |
| Capital (non-residents) (t-1) | | | | | | | | | | | 0.026 |
| | | | | | | | | | | | (0.0348) |
| [Reformj * (GDPj / GDPus)] (t-1) | -0.077 | -0.106 | -0.012 | -0.088** | -0.113** | -0.125*** | -0.008 | -0.022 | -0.055 | -0.070 | -0.014 |
| | (0.0772) | (0.0697) | (0.0881) | (0.0444) | (0.0527) | (0.0485) | (0.0516) | (0.0381) | (0.0582) | (0.0514) | (0.0544) |
| [GDPj / GDPus] (t-1) | -0.303*** | -0.232*** | -0.251*** | -0.261** | -0.447*** | -0.443*** | -0.532*** | -0.532*** | -0.0268*** | -0.256*** | -0.297*** |
| | (0.0841) | (0.0736) | (0.0795) | (0.0718) | (0.0899) | (0.090) | (0.0882) | (0.0875) | (0.0639) | (0.0605) | (0.0262) |
| Observations | 1271 | 857 | 1213 | 1325 | 802 | 802 | 802 | 802 | 857 | 857 | 857 |
| Number of countries | 118 | 58 | 94 | 92 | 88 | 88 | 88 | 88 | 58 | 58 | 58 |
| Adj. R-squared | 0.2760 | 0.2918 | 0.1958 | 0.1921 | 0.3341 | 0.3328 | 0.3173 | 0.3093 | 0.2794 | 0.2803 | 0.2774 |

Source: International Financial Statistics, Penn World Tables version 6.2, World Development Indicators, and IMF staff estimates.

Notes: Robust standard errors, clustered at country level, in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively. All specifications are estimated by OLS and include country and year fixed effects. Three year interval data over 1960-2005. GDP in real terms and PPP adjusted.

## Appendix 1. List of Countries in the Sample

| Low Income | Middle Income | High Income |
|---|---|---|
| Bangladesh | Albania | Australia |
| Burkina Faso | Algeria | Austria |
| Côte d'Ivoire | Argentina | Belgium |
| Ethiopia | Azerbaijan | Canada |
| Ghana | Belarus | Czech Republic |
| India | Bolivia | Denmark |
| Kenya | Brazil | Estonia |
| Madagascar | Bulgaria | Finland |
| Mozambique | Cameroon | France |
| Nepal | Chile | Germany |
| Nigeria | China | Greece |
| Pakistan | Colombia | Hong Kong SAR |
| Senegal | Costa Rica | Ireland |
| Tanzania | Dominican Republic | Israel |
| Uganda | Ecuador | Italy |
| Uzbekistan | Egypt, Arab Rep. | Japan |
| Vietnam | El Salvador | Korea, Rep. |
| Zimbabwe | Georgia | Netherlands |
| | Guatemala | New Zealand |
| | Hungary | Norway |
| | Indonesia | Portugal |
| | Jamaica | Singapore |
| | Jordan | Spain |
| | Kazakhstan | Sweden |
| | Latvia | Switzerland |
| | Lithuania | Taiwan Province of China |
| | Malaysia | United Kingdom |
| | Mexico | United States |
| | Morocco | |
| | Nicaragua | |
| | Paraguay | |
| | Peru | |
| | Philippines | |
| | Poland | |
| | Romania | |
| | Russian Federation | |
| | South Africa | |
| | Sri Lanka | |
| | Thailand | |
| | Tunisia | |
| | Turkey | |
| | Ukraine | |
| | Uruguay | |
| | Venezuela, Rep. Bolivariana | |

Source: World Bank

# Appendix 2. Description of Reform Indices

| Reform Indices | Description | Source | Coverage | | | |
|---|---|---|---|---|---|---|
| | | | Start Year | End Year | MIN # of Countries in any Year | MAX # of Countries in any Year |
| **Real Indices** | | | | | | |
| Trade Openness | | | | | | |
| Tariff Rates | Average tariff rates, with missing values extrapolated using implicit weighted tariff rates. Index normalized to be between zero and unity: zero means the tariff rates are 60 percent or higher, while unity means the tariff rates are zero. | Various sources, including IMF, World Bank, WTO, UN, and the academic literature (particularly Clemens and Willamson, 2004) | 1960 | 2005 | 47 | 142 |
| Current-Account Restrictions | An indicator of how compliant a government is with its obligations under the IMF's Article VIII to free from government restriction the proceeds from international trade in goods and services. The index represents the sum of two sub-components, dealing with restrictions on trade in visibles, as well as in invisibles (financial and other services). It distinguishes between restrictions on residents (receipts for exports) and on non-residents (payments for imports). Although the index measures restrictions on the proceeds from transactions, rather than on the underlying transactions, many countries in practice use restrictions on trade proceeds as a type of trade restriction. The index is scored between zero and 8 in half-integer units, with 8 indicating full compliance. | Quinn (1997), and Quinn and Toyoda (2007; forthcoming). | 1960 | 2005 | 50 | 65 |
| Product Markets | | | | | | |
| Telecom and Electricity Industries | Simple average of the electricity and telecom markets sub-indices, which are constructed, in turn, from scores along three dimensions. For electricity, they capture: (i) the degree of unbundling of generation, transmission, and distribution; (ii) whether a regulator other than government has been established; and (iii) whether the wholesale market has been liberalized.  For telecom, they capture: (i) the degree of competition in local services; (ii) whether a regulator other than government has been established; and (iii) the degree of liberalization of interconnection changes.  Indices are coded with values ranging from zero (not liberalized) to two (completely liberalized). | Based on legislation and other official documents. | 1960 | 2003 | 106 | 108 |

## Appendix 2. Description of Reform Indices

| Reform Indices | Description | Source | Coverage | | | |
|---|---|---|---|---|---|---|
| | | | Start Year | End Year | MIN # of Countries in any Year | MAX # of Countries in any Year |
| **Real Indices** | | | | | | |
| Agriculture | Given that developing countries constitute most of our sample, the degree of regulation in agriculture, which continues to account for a large part of many of these economies, is an essential aspect of product market competition. Index aims to capture intervention in the market for the main agricultural export commodity in each country. As data limitations preclude coding separate dimensions of intervention, the index provides a summary measure of intervention. Each country-year pair is assigned one of four degrees of intervention: (i) maximum (public monopoly or monopsony in production, transportation, or marketing); (ii) high (administered prices); (iii) moderate (public ownership in relevant producers, concession requirements); and (iv) no intervention. | Based on legislation and other official documents. | 1960 | 2003 | 96 | 104 |
| **Financial Indices** | | | | | | |
| Capital Account Openness: Aggregate | Qualitative indicators of restrictions on financial credits and personal capital transactions of residents and financial credits to nonresidents, as well as the use of multiple exchange rates. Index coded from zero (fully repressed) to three (fully liberalized). | Abiad and others (2008), which follows the methodology in Abiad and Mody (2005). The original sources are mostly various IMF reports and working papers, but also central bank websites, etc. Resident/nonresident-specific indices are based on Quinn (1997), and Quinn and Toyoda (2007). | 1973 | 2005 | 72 | 91 |
| Capital Account Openness: Residents (nonresidents) only | Measures the extent to which residents (nonresidents) are free from legal restrictions to move capital into and out of a country. | | | | | |
| Domestic Financial Liberalization | The index of domestic financial liberalization is an average of six sub-indices. Five of them relate to *banking* : (i) interest rate controls, such as floors or ceilings; (ii) credit controls, such as directed credit, and subsidized lending; (iii) competition restrictions, such as limits on branches and entry barriers in the banking sector, including licensing requirements or limits on foreign banks; (iv) the degree of state ownership; and (v) the quality of banking supervision and regulation, including power of independence of bank supervisors, adoption of a Basel I capital adequacy ratio, and framework for bank inspections. The sixth sub index refers to the regulation of *securities markets* , including policies to encourage the development of bond and equity markets, and to permit access of the domestic stock market to foreigners. The sub-indices are aggregated with equal weights. Each sub-index is coded from zero (fully repressed) to three (fully liberalized). | | | | | |

# Appendix 3. Down and Up Breaks in Economic Growth

| Down Breaks | | Down Breaks | | Up Breaks | | Up Breaks | |
|---|---|---|---|---|---|---|---|
| country | year | country | year | country | year | country | year |
| Antigua Barb | 1988 | Liberia | 1986 | Antigua Barb | 1978 | Kuwait | 1990 |
| Austria | 1973 | Luxembourg | 1991 | Bangladesh | 1995 | Lao | 1978 |
| Barbados | 1971 | Madagascar | 1971 | Bolivia | 1986 | Liberia | 1994 |
| Belgium | 1974 | Malaysia | 1972 | Burkina Faso | 1998 | Luxembourg | 1983 |
| Belize | 1980 | Malaysia | 1980 | Cambodia | 1987 | Malaysia | 1964 |
| Bhutan | 1987 | Malaysia | 1996 | Cameroon | 1978 | Malaysia | 1988 |
| Bolivia | 1977 | Maldives | 1990 | Cameroon | 1994 | Mali | 1969 |
| Botswana | 1989 | Malta | 1979 | Chile | 1983 | Mauritius | 1960 |
| Brazil | 1980 | Mexico | 1981 | China | 1977 | Morocco | 1960 |
| Burundi | 1992 | Morocco | 1968 | Costa Rica | 1991 | Mozambique | 1995 |
| Cameroon | 1986 | Morocco | 1982 | Cuba | 1980 | Namibia | 1998 |
| Chile | 1971 | Nicaragua | 1976 | Cuba | 1996 | Nepal | 1980 |
| Comoros | 1968 | Niger | 1981 | Djibouti | 1998 | Nicaragua | 1993 |
| Comoros | 1988 | Nigeria | 1960 | Dominica | 1980 | Nigeria | 1968 |
| Congo | 1982 | Nigeria | 1977 | Ecuador | 1971 | Nigeria | 1987 |
| Costa Rica | 1979 | Pakistan | 1970 | Egypt | 1975 | Pakistan | 1962 |
| Cote D'Ivoire | 1979 | Pakistan | 1988 | Equat Guinea | 1994 | Pakistan | 1978 |
| Cuba | 1988 | Panama | 1981 | Ethiopia | 1987 | Paraguay | 1973 |
| Denmark | 1969 | Papua New G. | 1978 | Ghana | 1965 | Philippines | 1998 |
| Djibouti | 1987 | Paraguay | 1981 | Ghana | 1981 | Qatar | 1996 |
| Dominica | 1988 | Philippines | 1981 | Ghana | 1997 | Rwanda | 1994 |
| Ecuador | 1979 | Portugal | 1973 | Greece | 1962 | Samoa | 1994 |
| Egypt | 1983 | Romania | 1979 | Greece | 1996 | Sao Tome Pr | 1987 |
| El Salvador | 1978 | Rwanda | 1986 | Guatemala | 1988 | South Africa | 1995 |
| Equat Guinea | 1977 | Samoa | 1978 | Guinea | 1994 | St Lucia | 1981 |
| Ethiopia | 1979 | Samoa | 1986 | Haiti | 1989 | Suriname | 1995 |
| France | 1973 | Sao Tome Pr | 1979 | Haiti | 1997 | Syria | 1990 |
| Gabon | 1976 | Sierra Leone | 1985 | Hungary | 1996 | Taiwan | 1962 |
| Ghana | 1973 | South Africa | 1983 | India | 1994 | Tanzania | 1996 |
| Ghana | 1989 | Spain | 1974 | Indonesia | 1968 | Thailand | 1987 |
| Greece | 1973 | St Lucia | 1989 | Iran | 1989 | Togo | 1987 |
| Guatemala | 1980 | Swaziland | 1979 | Ireland | 1993 | Tonga | 1979 |
| Haiti | 1980 | Sweden | 1970 | Jamaica | 1980 | Trinidad Tob | 1974 |
| Hong Kong | 1988 | Switzerland | 1973 | Jordan | 1975 | Trinidad Tob | 1990 |
| Hungary | 1979 | Syria | 1982 | Korea | 1962 | Tunisia | 1995 |
| Hungary | 1988 | Syria | 1998 | | | Uganda | 1988 |
| Indonesia | 1977 | Taiwan | 1996 | | | | |
| Iran | 1976 | Tanzania | 1968 | | | | |
| Italy | 1974 | Thailand | 1995 | | | | |
| Jamaica | 1972 | Togo | 1969 | | | | |
| Japan | 1973 | Togo | 1979 | | | | |
| Jordan | 1967 | Tonga | 1987 | | | | |
| Jordan | 1986 | Trinidad Tob | 1966 | | | | |
| Korea | 1996 | Trinidad Tob | 1982 | | | | |
| | | Tunisia | 1972 | | | | |
| | | Zimbabwe | 1991 | | | | |