

Investment Booms and Institutions:

Implications for the Andean Region

Osmel Manzano
José Luis Saboin

Country Department
Andean Group

TECHNICAL
NOTE N°
IDB-TN-2479

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May 2022



**Cataloging-in-Publication data provided by the
Inter-American Development Bank**

Felipe Herrera Library

Manzano, Osmel, 1971 -

Investment booms and institutions: implications for the Andean Region / Osmel

Manzano, Jose Luis Saboin.

p. cm. — (IDB Technical Note ; 2479)

Includes bibliographic references.

1. Investments-Developing countries-Andes Region-Econometric models. 2.

Investments, Foreign-Andes Region-Econometric models. 3. Economic policy-Andes

Region. I. Saboin, José Luis. II. Inter-American Development Bank. Country

Department Andean Group. III. Title. IV. Series.

IDB-TN-2479

<http://www.iadb.org>

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Investment Booms and Institutions: Implications for the Andean Region*

Osmel Manzano

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Abstract

This paper provides evidence of a positive effect of institutions and reforms in initiating investment booms. We constructed an unbalanced panel of 178 countries for the period 1950–2019 that considered institutions and reforms at different levels and dimensions. We analyzed the effects of these variables on 159 carefully estimated investment boom episodes, controlling for the standard determinants of investment and using a battery of estimation techniques and robustness checks. Overall, market-oriented and democratic institutions favor the advent of investment booms. Structural reforms present mixed effects and in some cases these are nonlinear. While trade and capital account reform have negative effects, domestic finance, product, and labor market reforms have the opposite. Beyond institutions and reforms, we find different effects regarding external, macro, and structural variables.

Keywords: institutions, investment, reforms, economic development, Latin America

JEL classification: E02, E22, F21, H54, O16, O43, O47, O50, P11.

*We would like to thank Emiliano Libman and Wladimir Zanoni for their comments on and suggestions regarding earlier versions of this document. All errors and omissions are strictly ours.

1 Introduction

The Andean Region has had a relatively disappointing growth experience. In spite of a period of high growth registered between 2000 and 2014 led by the commodity boom of the beginning of the century, the average annual per capita growth rate for the region between 1960 and 2020 has been 0.5 percent. There is ample literature that tries to explain this situation, which is summarized by [de la Cruz *et al.* \(2020\)](#).

Among the different factors typically mentioned to explain this low growth is poor institutional performance. [Calvo \(2005\)](#) for Bolivia and [Cueva *et al.* \(2009\)](#) for Ecuador argue that the appropriability of private investment returns is one of the binding constraints in those countries. Similarly, [Hausmann & Klinger \(2009\)](#) discuss the appropriability risks related to the extractive sector in Peru as well as the lack of state capacity. Taking a different approach, [Melendez & Harker \(2008\)](#) study the case of Colombia and find that areas with falling violence showed economic reactivation. Higher violence is also related to another category of institutions. Finally, recently [Obuchi *et al.* \(2016\)](#) find that the business environment and macroeconomic instability in Venezuela are the main reason behind the poor economic performance of the country, even before the recent collapse. Similarly, [Maldonado & Manzano \(forthcoming\)](#), taking a non-parametric approach to investment and income per capita, find that Andean Countries are at a level where institutions are key to achieving a higher income per capita.

The role institutions play with regard to growth has been widely discussed. [Acemoglu *et al.* \(2005\)](#), to cite one example, delve deeply into this topic. Furthermore, there is empirical evidence, as shown in [Acemoglu *et al.* \(2001\)](#). However, the task “improve institutions” could be daunting for a policy maker, because it is a very broad recommendation. As argued concerning the cases presented in [Rodrik \(2003\)](#), many successful experiences in terms of growth start with specific reforms.

This paper sits in the middle of the road of papers that find a role for institutions on growth and papers that look at specific institutional changes that spurred growth for certain countries. We look at different episodes of investment booms. Then we try to disentangle different components of institutional variables to identify specific changes that could trigger growth through the physical capital accumulation channel.

We contribute to the literature of event studies that analyzes turning points in macroeconomic variables, such as [Pritchett \(2000\)](#), [Hausmann *et al.* \(2005\)](#) and [Meneses & Saboin \(2021\)](#) on growth, [Montiel \(2000\)](#) on consumption, [Rodrik \(2000\)](#) on saving, [Freund & Pierola \(2012\)](#) on exports, [Saboin \(2018\)](#) on hyperinflation, and [Libman *et al.* \(2019\)](#) on investment.

Although our results are broadly in line with [Libman *et al.* \(2019\)](#), we extend and differentiate their study in several ways. First, we pay special attention to the roles of institutions and structural reforms. Second, we apply different empirical strategies to test the impact of these variables on investment. Third, we present an expanded and updated data set. Fourth, we present new and different aspects of the determinants of investment booms.

We find that improvements in economic institutions (measured as higher scores in a law and order index) have a positive effect on the probability of initiating an investment boom. This

effect is consistently significant and robust to a series of changes in episode identification parameters, different functional forms, the introduction of control variables, unobserved heterogeneity, sample selection bias, and endogeneity concerns. We find that this effect is also nonlinear, suggesting that after some threshold, economic institutions contribute less to the advent of an investment boom. We attribute this finding to the standard results of the growth and institutions literature. Because countries with higher values in the law and order index are those with higher capital stocks per capita, they have diminishing returns to capital accumulation and therefore present fewer investment booms in our sample. Moreover, this result implies that, as emerging and developing economies reach a certain level in the rule of law, it is more difficult for them to propitiate an investment boom, either because they already had one in past or because they must compete in other dimensions to reach the inframarginal investor (the same logic applies to advanced economies).

Although we do not consistently find statistically significant effects of democracy (measured as increases in a democratic accountability index) on initiating an investment boom, we neither find a negative effect. Rather, we mostly find small unconditional positive effects and parameter sensitivity, particularly to functional form and different episode identification parameters. For instance, when we change from probit to OLS and from probit to a logit model that accounts for rare events, we observe significant effects, particularly in a sample of emerging and low income economies. The same pattern is observed when we look at the effect of democracy on booms where the capital stock per capita grows faster than in our baseline model. When analyzing shorter booms, we observe that the effect becomes positive after a threshold. Moreover, when testing for the possibility of reverse causality (from booms to institutions), we find that democratic accountability has a positive effect on law and order (and vice versa), suggesting the possibility that democratic accountability has an indirect effect on investment booms through its positive impact on law and order.

We also find that while (exogenously determined) economic institutions have an (unconditional) positive and statistically significant effect on initiating an investment booms, a feedback effect model contradicts it, implying that there is a feedback process between boom times and improvements in economic institutions. In contrast, while (exogenously determined) political institutions also have an (unconditional) positive effect, this effect is robust to the possibility of feedback effects as well, that is, we do not find feedback effects between investment booms and political institutions. This suggests the role of political institutions has a more exogenous nature than the role of economic institutions in the advent of an investment boom.

Regarding narrow measures of institutions, we find that trade reforms have a negative effect in initiating an investment boom, a result that is consistently significant and robust to a series of changes in episode identification parameters, functional forms, controls, unobserved heterogeneity, and sample selection and endogeneity concerns. This result suggests the possibility that domestic investors might find their investment prospects slump, because trade reforms not only increase competition in the domestic market due to the arrival of foreign goods (and investors are therefore more cautious with regard to increasing fixed capital) but also incentivize the substitution of domestic production with imports, particularly in the sectors where there are no comparative advantages.

When current account reforms are considered separately from the trade reforms index, some of our models suggest that they show a positive effect after some level of liberalization. This could be the case in the early stages of the accumulation process, particularly when liberalization of current account transactions allows the import of capital goods.

We find that external finance reforms consistently reduce the probability of experiencing an investment boom, but that this effect reverses after some level of liberalization. This result is consistently significant and robust to a series of changes in episode identification parameters, functional forms, controls, unobserved heterogeneity, and sample selection. There is a possibility that a linear effect could occur, because, with more capital account openness, capital might pursue other investment opportunities abroad rather than at home. Since this is a composite index measuring capital inflow and outflow restrictions, it could be that up until a certain mix of both inflow and outflow regulation is achieved, the effect becomes positive.

We find that domestic finance reforms increase the probability of an investment boom and this effect is consistently significant and robust to a series of changes in episode identification parameters, functional forms, controls, unobserved heterogeneity, and sample selection. Moreover, our endogeneity analysis suggests that investment booms and domestic finance reforms are mutually reinforcing. This is an expected result, since the domestic financial market is a key vehicle for the proliferation of investment.

Even though they are not significant in our baseline specification, product market reforms show either negative or nonlinear significant effects, particularly when we use stricter growth parameters to define an investment boom. Specifically, we observe that while they decrease the probability of experiencing an investment boom, after some threshold, the effect reverses. This could happen, for instance, once the sectors analyzed in the index (telecommunications and electricity) reach a certain level of regulation that ensures profitable capital investment opportunities. Interestingly, when we shorten the time window of the episode, the effect changes: after a certain level of product market reform is achieved, the probability of experiencing an investment boom decreases. This might suggest that once a level of liberalization in these sectors is attained, it is harder to initiate a short investment boom.

Labor market reforms mostly show a negative effect across models and tests. However, we observe that while the effect is negative, after a threshold labor market reforms increase the probability of experiencing an investment boom. This nonlinear effect is consistent to the introduction of controls, unobserved heterogeneity, functional form changes, and variation in episode identification parameters. Interestingly, when we reduce the duration of the boom, the nonlinear effect reverses: it starts to reduce the probability of a boom after a certain threshold, suggesting that, at some level of labor market regulation, labor market reforms increase the probability of experiencing a short boom.

Although not consistently significant, agricultural reform seems to have a positive effect, particularly in nonadvanced countries and in shorter boom windows. Since this index relates to the level of regulation in relation to the main commodity of export, it certainly could be the case that the opening of a competitive sector could bring more investment. A nonlinear significant effect suggests that this effect becomes positive after some level is achieved.

From these results, it seems that reaching a certain level in terms of some reforms reduces the probability of experiencing an investment boom. This is an expected result, considering that having more-liberalized sectors is a sign of (a) high levels of capital stock per capita and therefore a lower probability of experiencing an investment boom and (b) the difficulty of propitiating an episode through better regulation or sector liberalization, because the sector might have reached the current frontier and therefore it is harder to promote capital accumulation in this way.

When we consider the strengthening of different institutions and enactment of different reforms as a joint process (using the principal component of our institutional variables) our results are reassuring to our prior that comprehensive improvement in institutions in both a broad and a narrow sense contributes mostly positively (or neutrally) to the start of an investment boom. These results are robust to the introduction of controls, unobserved heterogeneity, sample selection, and changes in functional form and episode identification parameters.

Regarding control variables, we find that increases in real foreign interest rates and capital market volatility reduce the probability of experiencing an investment boom episode. A negative effect occurs with increases in the terms of trade, trade openness, and financial openness, all of them being statistically significant at conventional levels. A negative terms of trade coefficient could imply a validation of the “Dutch Disease” hypothesis, whereas negative trade and financial openness coefficients could imply a *substitution effect*, that is, when the basket of substitute opportunities is increased, capital expenditures might shift not only to current expenditures, but also to *foreign* capital expenditures opportunities as well. This is in line with our results on capital account reforms. The effect of net FDI flows is negative and statistically significant, an expected result, considering that as domestic capital goes abroad, the lower the probability of experiencing an investment episode domestically. The same occurs with increases in net portfolio investment: they lower the probability of an episode taking place.

Some of our models show that the degree of fiscal procyclicality has a positive and statistically significant effect. While high fiscal procyclicality is a sign of macroeconomic volatility, which acts as a deterrent to investment (we call this the *risk aversion effect*), a government that increases (contracts) spending during expansions (downturns) can affect total capital accumulation through the public component of investment. This is typical in emerging markets because capital investment seems to be the “adjustment” variable during the cycle (we call this the *public investment effect*) [Ardanaz & Izquierdo (2017)]. Nonetheless, fiscal procyclicality also shows a negative effective when we restrict the sample to emerging and low income economies, suggesting that the “macroeconomic volatility aversion effect” dominates the “lower public investment effect” with regard to investment booms. Inflation consistently shows a negative and statistically significant effect.

Regarding domestic factors, increases in capital productivity (e.g., a lower level of investment needed to produce a given growth rate in the economy) increase the likelihood of experiencing an investment boom. Increases in per capita GDP by contrast lower the probability of an episode, suggesting the presence of convergence effects. The presence of natural resource rents could go in opposite ways: an increase in the share of natural resource

rents over GDP lowers the probability of experiencing an investment boom, a result that might go in hand with the “Dutch Disease” hypothesis. Nonetheless, in some of our models, an increase in the share of natural resource rents in GDP increases the probability of experiencing an investment boom, suggesting that resource booms can also be investment booms.

Increases in human capital also correlate positively with the probability of an investment boom being initiated. Finally, an interesting result is that the presence of crises (either financial, debt, or currency crises) in the previous five years increases the probability of an investment boom, a result that gives support to some of the findings of the financial and investment literature on low asset prices and investment prospects.

The document continues as follows: section 2 presents our episode identification strategy and some characteristics of the episodes. Section 3 describes our data, discusses the expected signs of our main and control variables, and briefly discusses summary statistics and the dynamics of these during the episodes. Section 4 presents our empirical strategy and summarizes the results. Section 5 describes and analyzes the booms in the Andean region and section 6 presents our concluding remarks.

2 Episode identification

Following Libman *et al.* (2019)¹—who build upon Hausmann *et al.* (2005)—we identify investment boom episodes using the following baseline filter: (a) annual per capita capital stock growth over an 8-year period must be $\geq 3.5\%$, (b) annual per capita capital stock growth must have accelerated by at least 2 percentage points (pp) during the 8-year period, and (c) the level of capital per capita 8 years after the end of the episode must exceed its historical peak.

The first criterion ensures that capital stock per capita grows fast, the second ensures growth deviates significantly from the pre-episode average, and the third ensures that captured events are not pure recoveries from previous contraction episodes such as wars, political conflicts, or natural disasters.

The first step is to obtain the fitted growth rate of capital per capita over each 8-year window within the time frame of the data set. Formally, the following rolling regression was estimated for each country individually:

$$\ln(k_{it}^w) = \alpha_{it}^w + g_{it}^w \cdot t + u_{it} \quad (1)$$

where i and t denote country and year subscripts, respectively; k_{it}^w is the capital to population ratio; t is a time trend; and w denotes the 8-year rolling estimation window. Coefficient estimate \hat{g}^w is the fitted 8-year growth rate of capital per capita and the parameters α^w and u are the intercept and the error term, respectively.

¹We thank Libman *et al.* for kindly sharing their database, code, and suggestions.

An investment acceleration episode is defined as one where (a) the fitted growth rate \hat{g}^w exceeds the $\geq 3.5\%$ a year on average during an 8-year window threshold and (b) the acceleration of the capital stock growth ($\Delta\hat{g}^w$) exceeds the $> 2pp$ threshold over the same period, compared to the previous 8-year window.

The second step is to identify the beginning year of each episode. This is necessary because of the possibility that a number of contiguous years will satisfy the growth and acceleration thresholds. For instance, a country’s capital stock per capita may grow on average more than 3.5 percent a year and growth may accelerate by more than 2 percent over the 8-year windows beginning in 1976, 1977, and 1978. In a case like this, it is important to choose the optimum one as the start year. One way to fulfill this condition is by using Chow tests for each candidate year separately and then comparing the goodness of fit for each one. Formally, we estimate

$$\ln(k_{it}) = \alpha_t + [\beta_{1i}I(t < \tau) + \beta_{2i}I(t > \tau)]t + u_{it} \quad (2)$$

where $I(t > \tau)$ is an indicator function that is equal to 1 for the candidate start year τ and the years afterward and 0 otherwise, and $I(t < \tau)$ is an indicator function that is equal to 1 for the years before the candidate start year τ and 0 otherwise.

We estimate (2) by setting τ equal to each year in our sample and for each country. Then, we calculate the regression F-statistic for each τ year and country. To choose the starting year from among the candidates, we select the τ year that yields the maximum F-statistic. It is important to highlight that we are not testing for a structural break in this step; this was the job of the filter in the previous step. It should be noted that this identification strategy does not allow for overlapping episodes. For example, if the starting year chosen by (2) is 1980, but 1985 also satisfies criteria (a), (b), and (c), we do not consider the latter another separate episode.

2.1 Episode characteristics

To estimate the episodes, we use the log of the capital stock (measured in 2017 U.S. dollars) divided by total population from the Penn World Tables 10.0 [Feenstra et al. (2015)]. This measure is available for an unbalanced panel of 183 countries from 1960 to 2019. Using this identification strategy, we found 159 episodes in 109 countries. This implies that 66 countries had 1 episode, 37 countries had 2 episodes, 5 counties had 3 episodes (Bhutan, China, Kuwait, Malaysia, and Panama), and 1 country had 4 (Sri Lanka)².

²We also test the sensitivity of our results to variation in our episode identification parameters, namely, the episode window growth rate, the acceleration rate between windows, and the time window itself. Modifying the minimum average annual growth rate of capital stock per capita from 3.5 percent to 5 percent and the minimum within-window acceleration threshold from 2 percent to 3 percent, we obtained 92 episodes. We also tested a shorter time window with our baseline growth and acceleration parameters: doing so identified 192 episodes. See the Appendix for details.

Following [Acemoglu *et al.* \(2019\)](#), we group countries into seven geographic regions³. These are Africa (AFR), East Asia and the Pacific (EAP), Eastern Europe and Central Asia (ECA), Western Europe and other developed countries (INL), Latin America and the Caribbean (LAC), the Middle East and the North of Africa (MNA), and South Asia (SSA). After this regional division, there are 42 episodes in AFR, 31 in LAC, 25 in EAP, 21 in MNA, 17 in ECA, 14 in SSA, and 9 in INL. [Figure 1](#) shows this distribution and [Table 1](#) provides a full list of episodes.

[Table 2](#) presents unconditional probabilities by region, income, and decade. Excluding the years in which an episode cannot take place, in general we find that the unconditional probability of occurrence of an investment boom is 1.75 percent, slightly below the figure reported by [Libman *et al.* \(2019\)](#), because there has been fewer occurrences over the last years. The regions with higher probabilities are East Asia and the Pacific and South Asia (EAP and SSA), followed by MNA and LAC, and those with the lower probabilities are Europe and Other Developed Countries and Central Asia (ECA and INL). This is in line with the diminishing returns to capital finding in the growth literature (see the classic works of [Ramsey \(1928\)](#), [Solow \(1956\)](#), [Cass \(1965\)](#), [Diamond \(1965\)](#), and [Koopmans \(1965\)](#)): richer economies (those with higher per capita capital stock) tend to grow slower and therefore might experience fewer investment booms.

In terms of decades, the 1970s and the 2000s show the highest probabilities of occurrence and coincide with higher commodity prices⁴. Finally, the exercise by income quintile (where the first quintile represents the lowest income) shows that the probability of experiencing an investment boom is higher the lower the country’s position in the income distribution. Again, this result is in line with the theoretical and empirical results of the diminishing returns to capital literature.

3 Data and Descriptive Statistics

3.1 Institutional variables

Following [North \(1991\)](#), in this study we understand institutions as the *humanly devised constraints that structure political, economic and social interaction*. Moreover, following [Acemoglu *et al.* \(2005\)](#), we distinguish between economic and political institutions. We understand the former as those institutions that define the economic regime and shape a country’s economic incentives (e.g., property rights, contract enforcement, the extent of regulation in a sector or industry, etc.). By symmetry, political institutions are those that define the political regime and shape political incentives (e.g., form of government, separation and division of powers, constraints on politicians and elites, etc.).

We see these two classes of institutions at two margins. First, we consider institutions in a *broad* sense, that is, as measured by overall indices that summarize the conundrum and

³These regions are similar to the World Bank classification.

⁴In section 3, we test for the possibility of an effect of terms of trade shocks in the advent of an investment boom episode.

layers of institutions within a country. For instance, an index that measures the rule of law in general necessarily abstracts several aspects and levels of the rule of law. Consider, for example, the case of the property rights regime in the hydrocarbon sector vs. the property rights regime in the food sector—these can be quite different within and between countries. Thus, a general index summarizes the different property rights in a given country. The same applies to broad measures of political institutions. Consider the case of the constraints on power to the executive branch of the national government vis-a-vis those of subnational governments: these can be quite different, particularly across countries.

Second, and as a consequence of the first, we consider institutions in a *narrow* sense. For instance, in terms of narrow economic institutions, in a given country we distinguish the extent to which property rights work in a particular market/industry. In terms of narrow political institutions, for a given country, we observe how constrained the different branches of government are or how free the electoral system is.

Last, although we consider both political and economic institutions at the broad level, we only consider economic institutions at the narrow level. Our main rationale is that we constrained our main interest to the impacts of different economic institutions in the advent of an investment boom. Nonetheless, because we recognize that there could be effects of economic institutions on political ones and vice versa (see Barro (2012), Acemoglu *et al.* (2019), and the papers therein for discussions of the possibilities of these feedback effects), we are also interested in the role of political institutions in fostering an investment boom. Thus, we do consider their role, though only in the broad sense.

Taking all these considerations into account, we use data on institutions from three different data sets, each of which we discuss in what follows.

3.1.1 Broad measures of institutions

Our first set of institutional variables comes from the International Country Risk Guide (ICRG) political risk index, which aggregates several socioeconomic, institutional, and political conditions into a country-level political risk index. Although the ICRG database comprises an unbalanced panel of 19 variables for the period 1984–2017, we focus primarily on 2. As in Barro (2012), to have a proxy of economic institutions we use a measure of the rule of law named law and order. For a proxy on democratic institutions, we use a measure of democracy named democratic accountability.

According to ICRG, the law and order index is comprised of two broad categories: (a) impartiality of the judicial system and (b) effective application of the law. Thus, this index has a lot to do with the protection of basic rights (e.g., economic, social, and cultural rights) that allow individuals to carry out their most basic economic activities—production and exchange—without being affected either by a predatory government or private capture.

The democratic accountability index measures how responsive a government is to its people on the basis of the type of governance enjoyed by the country in question (e.g., whether it is an alternating democracy or a *de jure* one-party state). The remaining components of the index are bureaucracy quality, government stability, corruption, external conflict, internal

conflict, and religious tensions and ethnic tensions. We use some of these components as control variables in the regressions of section 3.

We expect a positive effect of institutions on the probability of experiencing an investment boom. For a recent account of this literature, see [Haggard & Tiede \(2011\)](#). For instance, [Hall & Jones \(1999\)](#) find that differences in capital accumulation, productivity, and therefore output per worker are driven by differences in institutions and government policies, which they call social infrastructure. [Barro \(1996, 2000\)](#) goes one step further and argues that property rights and the rule of law (but not democratic elections) are key determinants of economic growth. Later, he finds feedback effects from economic freedoms and property rights on growth and human capital and, from these, on democracy [[Barro \(2012\)](#)]. In essence, property rights and economic freedoms allow individuals and firms to own resources for production and exchange, appropriately reaping the profits of such activities in a timely and efficient manner [[La Porta *et al.* \(1998\)](#), [De Haam & Sturm \(2000\)](#), [Rodrik *et al.* \(2004\)](#) and [Acemoglu *et al.* \(2005\)](#)].

In their review of studies on the impact of democracy on economic growth, [Gerring *et al.* \(2012\)](#) find a null or negative effect during the 1960–2000 period. However, several authors have found positive effects ([Rodrick & Wacziarg \(2005\)](#), [Persson & Tabellini \(2006\)](#), [Papaioannou and Siourounis \(2008\)](#)). The reasons behind a seemingly predominant pessimistic view of the impact of democracy on economic outcomes can be related to a number of factors, among which are measurement error and identification problems and beyond these technical issues the characteristics of the democratic process, such as the consequences of political defeat and the interaction of political elites ([Alesina & Drazen \(1991\)](#), [Acemoglu & Robinson \(2000\)](#)), which might harm economic outcomes and the development process in general. More recently, [Acemoglu *et al.* \(2019\)](#) has challenged this pessimistic view by addressing some of the problems discussed above. Accounting for typical initial contractions of GDP after a democratic process starts, they find a large and positive effect of democracy on growth over a similar period. At least in our econometric exercises below, we mostly find that more democracy, as measured through democratic accountability, has a positive effect in propitiating an investment boom.

3.1.2 Narrow measures of institutions

Our second set of institutional variables, which mostly focuses on narrow economic institutions, comes from [David *et al.* \(2021\)](#), who build upon a data set created by [Alesina *et al.* \(2020\)](#)⁵. The data set comprises an unbalanced panel of 5 indices of structural reforms for 90 countries over the 1973–2014 period at an annual frequency. Higher values of an index signal more-liberalized and better-regulated markets. The indices cover reforms implemented in five broad areas: (a) domestic finance (credit and interest rate controls, entry barriers, public ownership, quality of supervision in the domestic financial system), (b) external finance (capital account openness, encompassing regulations governing international transactions), (c) trade (tariffs), (d) product market (stringency of regulations and public ownership in two

⁵We thank all of these authors and the RES department of the IMF, who kindly provided the data set to the IDB.

large network industries: electricity and telecommunications), and (e) labor market (stringency of job protection legislation). In the rest of the document, we will refer to this database as the Structural Reforms Indicators as of 2014 or in short, the SRI - 2014 database.

Our third set of institutional variables comes from [Giuliano *et al.* \(2013\)](#). In essence, this data set is a previous version of that of [Alesina *et al.* \(2020\)](#). We use this database for three reasons: first, it incorporates years at the beginning of the sample period that are not covered in the updated database (it constitutes an unbalanced panel of 6 indices of structural reforms for the period 1960–2005). Second, it has one additional index of reform that was not available in the updated database but that we consider is still informative, agriculture market reforms (it should be noted that this database lacks the labor market reform index). Third, we use it to check robustness of the results from the updated database. The index covers reforms in 6 broad areas: (a) financial reform (similar to domestic finance in the updated database), (b) capital account (similar to external finance reform in the updated database), (c) current account (the degree of government intervention over the proceeds from international trade in goods and services),⁶ (d) trade (similar to the updated database), (e) product market (similar to the updated database) and (f) agricultural market reforms (extent of intervention in the market for the main agricultural export commodity). In the rest of this document, we will refer to this database as the SRI - 2005 database.

Structural reforms have been closely studied in recent years. [Alesina *et al.* \(2020\)](#) provide a “survey of surveys” of the economic (and political) effect of reforms. In sum, while several authors have found positive effects for all the indices reported above, some have found that such benefits are not immediate, especially in regard to product and labor market reform. Reforms can also come in waves: [Buera *et al.* \(2011\)](#), in explaining the diffusion of market-oriented reforms across countries in the period after World War II, conclude that the adoption of liberalization policies by neighboring countries influences the beliefs of domestic policy makers about the desirability of reforms. More recently, [David *et al.* \(2021\)](#), using [Alesina *et al.* \(2020\)](#)’s database, find positive effects on most economic outcomes, although the picture was not that satisfactory for societal outcomes, an issue that brings to the fore the need to consider accompanying policies to ensure that reforms promote inclusive growth.

3.2 Control variables

We included in our specifications three sets of control variables: the first encompasses country macroeconomic conditions and policies, the second considers external sector conditions and policies, and the third accounts for domestic factors. In what follows, we will provide an explanation of each variable as they pertain to each set as well as a rationale for its inclusion. Together with institutional variables, the control variables’ summary statistics can be seen in Table 3.

⁶This index was appended to the trade index in the 2014 version.

3.2.1 Macroeconomic conditions and policies

Macroeconomic factors are key drivers of growth [Fischer (1993)]. Thus, as in Libman *et al.* (2019), we consider the degree of fiscal procyclicality, the capital to output ratio, the inflation rate, the degree of exchange rate undervaluation, the degree of capital account openness, and an index of exchange rate stability. To measure fiscal procyclicality, we estimated the correlation coefficient of the cyclical components of real government consumption and real GDP. It is expected that as these variables become more correlated (that is, as a government's fiscal policy is more procyclical), there is more economic volatility [Perotti (1999)] and, because volatility acts as an investment deterrent, there is a lower probability of experiencing an investment boom. That said, fiscal policy procyclicality, often common in emerging economies, can have a positive impact on the probability of an investment boom. This, as the capital investment component in total government spending, is usually the most procyclical one [Ardanaz & Izquierdo (2017)]. In this paper, we advance the following hypothesis: if procyclicality has a negative effect on the probability of an investment boom occurring, we say that the *risk aversion effect* dominates; if the effect is positive, we say that the *public investment effect* dominates.

The capital to output ratio captures a productivity effect, according to which a low ratio reflects the fact that smaller amounts of capital are needed to produce a given level of output). We expect that increases in the capital to output ratio will decrease the likelihood of an episode. The impact of inflation is expected to be nonlinear. With some expected positive effect when inflation accelerates from very low levels, but negative when it surpasses a threshold [Ha *et al.* (2019)].

An undervaluation index was estimated following Rodrik (2008)'s three-step procedure: (a) we construct a real exchange rate index using relative prices from PWT 10.0 [Feenstra *et al.* (2015)], (b) we regress our real exchange rate index on per capita GDP and a set of time fixed effects, and (c) we estimate the residuals from the previous regressions to construct the undervaluation index. A positive residual denotes "undervaluation" while a negative residual denotes "overvaluation." It is expected that the sign of the undervaluation is positive. This as an undervalued exchange rate will work as incentive to attract investment (see Rodrik (1986), Razmi *et al.* (2012), and Rodrik (2008)).

Regarding exchange rate policy, two variables have been included: (a) the type of exchange rate regime (based on the index of Itzenski *et al.* (2017)), with the understanding that changes in the exchange regime have repercussions on other policy variables as well as on investment prospects; and (b) an index of restrictions and controls to the financial account (using the index of Chinn & Ito (2006)), with the understanding that the level of regulation of international financial transactions impacts the flow of capital and therefore economic growth. We would expect exchange rate stability to have a positive effect whereas capital controls to have an ambiguous effect. See Levy-Yeyati & Sturzenegger (2003) and Heathcote & Perri (2016) for a discussion of these issues.

3.2.2 External conditions and policies

External factors matter, especially when we want to try to answer Easterly *et al.* (1993)’s famous question: good policy or good luck? External factors manifest themselves in several ways, among which we identify the three that are most common: (a) terms of trade shocks, which are particularly relevant for developing economies because they are vulnerable to swings in the international price of their exports (see Kose (2002) and Mendoza (1995)); (b) volatility in the international capital markets (such as stock market crashes or bank runs); and (c) barriers to external financing and external trade (such as higher foreign interest rates and external trade barriers such as tariffs). These sources of external disturbance can translate into domestic macroeconomic volatility, reduce access to financing, limit capital inflows, initiate financial crises, etc., all of which separately and together have an impact on investment and growth [Calvo *et al.* (2004)].

To understand the role of terms of trade in investment booms, a terms of trade index (TOT, from now on) has been used as a proxy variable for changes in the terms of trade.⁷ The expected sign of the TOT index is ambiguous; on the one hand, higher terms of trade can increase investment in both beneficiary sectors and the overall economy in the short and long runs (if, for instance, the gains are allocated to other sectors and/or saved and managed well so that they act as buffers in times of distress; see Fornero *et al.* (2016)). On the other hand, higher terms of trade can lower the probability of an investment boom due to “Dutch Disease”⁸ effects (see Lane & Tornell (1998) and Lederman & Maloney (2007)).

Regarding barriers to international finance, the U.S. federal funds rate (corrected using the U.S. GDP deflator) and the standard deviation of the VIX index⁹ were used. Because higher interest rates in safer markets (such as the U.S.) reduce risk appetite from investors, a higher interest rate in the U.S. reduces net capital flows into emerging markets and, at the same time, a higher interest rate in the U.S. increases financial constraints and credit, the expected sign is negative. The same logic applies to capital market volatility.

Regarding barriers to international trade, we considered the “de facto” measure of openness, that is, the sum of exports and import over GDP. Countries that are more open to trade can exploit the gains from allocative efficiency. On the other hand, restrictions to foreign trade (e.g., imposition of tariffs, controls on current account transactions, etc.) can affect investment and growth through several channels, some of them being price distortions and lack of access to foreign knowledge (see, for instance, Rodriguez & Rodrik (2000) and Madsen (2009)). Moreover, in some countries (e.g., Brazil in the 1960s and 1970s) an inward-oriented growth strategy boosted investment, for instance via a protected manufacturing sector. Therefore, the expected sign is ambiguous.

Finally, to observe the outcomes of these sources of external disturbance, we include variables that capture net capital flows. These variables are (a) net foreign direct investment

⁷We use the commodity terms of trade index of Gruss & Kebhaj (2019).

⁸This term is used to describe the “de-industrialization” of a country after a resource boom. The term was coined by the magazine *The Economist* in an article about the Netherlands (“The Dutch Disease”, *The Economist*, November 26, 1977, pp. 82–83).

⁹The Chicago Board Options Exchange (CBOE)’s Volatility Index.

(FDI, from now on) flows, (b) net portfolio investment (FPI, from now on) flows, and (c) the change in international reserves. The expected sign of (a) is positive, as increases in FDI not only increase the capital stock and growth (Borensztein *et al.* (1998)), but also have other spillovers in the economy (Haddad & Harrison (1993), Alfaro & Johnson (1997)) and economic institutions (Bengoa & Sanchez-Robles (2003)). The expected sign of (b) is ambiguous because on the one hand, they can have a positive impact in developing financial markets and promoting economic growth, although these benefits might be contingent on the level of development and the economy’s absorptive capacity [de Vita & Kyaw (2009)]. On the other, its impact can be negative, as increases in net FPI flows crowd out resources that otherwise would go to real productive investment and at the same time cause the real exchange rate to appreciate, reducing the incentives to invest, particularly in manufacturing (see Botta (2018) and the papers therein); also, FPI flows are subject to capital market volatility, which affects investment through sudden stops (Calvo *et al.* (2004)) and capital flight. The expected sign for the change in international reserves could also be ambiguous, as it influences investors’ decisions regarding the location and the type of investment. Qian & Steiner (2014) show that higher reserves, thanks to their ability to lower exchange rate risk, reduce the risk premium of portfolio equity investment and in turn increase the inflow of FPI (relative to FDI). Higher reserves therefore could have a positive effect, although it is possible they might crowd capital investment.

3.2.3 Domestic factors

We included a dummy variable that accounts for the occurrence of crises (e.g., banking, currency, and debt)¹⁰ in the five years prior to the start of the episode. Because the presence of crises may disrupt long-term investment prospects or hurt the financial sector (through balance sheet effects), one would expect the sign to be negative (Rioja *et al.* (2014), Allen *et al.* (2003)). However, as asset prices plummet during a crisis, investors could be dragged into the economy, boosting investment. Moreover, the extent to which investment reacts to a crisis is also tied to financial system management and the degree of capital account openness if, for instance, a banking crisis does not become systemic, sudden stops might fail to have an impact on investment (Joyce & Nabar (2009)).

We also included a variable that accounts for the role of natural resource dependence, the expected sign of which is ambiguous. On the one hand, an increase in the rent from natural resources may enhance investment in natural-resource intensive sectors (and from resource-incentive to others), but on the other hand, it may also generate “Dutch Disease” effects, reducing investment in the manufacturing sector and creating other sources of issues [Lane & Tornell (1998) and Lederman & Maloney (2007)].

A human capital index from the Barro & Lee (2013) data set was included to account for the role of human capital. Specifically, we included the share of the population with completed secondary education. We expect the effect of human capital to be positive, as capital might follow talent and skills (Kremer (1993)). Finally, to account for convergence

¹⁰We use the Reinhart *et al.* (2018) Global Crises data set.

effects, we included the level of per capita GDP. If convergence effects are present, we expect the sign of this variable to be negative (Barro & Sala-i-Martin (1992)).

3.3 Sample characteristics

With all these variables, we constructed an annual panel that comprised 178 countries over the 1960–2019 period, although not all variables are available for all countries and for the entire sample period. Table 3 shows descriptive statistics distinguishing periods within boom years (i.e., our baseline 8-year window) and between boom years (i.e., the previous and next 8-year windows).

From these statistics, we notice interesting patterns. First, we observe that all broad indices of economic institutions are higher (relative to the U.S.)¹¹ within boom years than between boom years. Second, broad measures of political institutions are also higher during boom years, although the Acemoglu *et al.* (2019) index of democracy shows that countries did not become more democratic during boom years. Third, regarding narrow measures of economic institutions, we observe mixed results. Although the SRI - 2005 database indices show an improvement in structural reforms during boom years, there are no clear—and sometimes even contradictory—patterns when it is compared with the SRI - 2014 version¹²; we disentangle this issue further in the document. Fourth, for our set of control variables, within boom years we observe: (q) higher GDP growth, (b) higher rents from natural resources (as percentage of GDP), (c) higher human capital, (d) slightly lower interest rate and capital market volatility, (e) higher trade openness and terms of trade, and (f) undervalued exchange rates, higher FDI outflows, and lower fiscal procyclicality.

Regarding episode dynamics, the first row of charts in Figure 2 shows that, for a representative (average) country in the IMF’s income group classification¹³, broad measures of economic (chart 1) and political (chart 2) institutions tend to improve before and during boom years, and then deteriorate after the end of the boom, particularly in our region of interest, LAC. The bottom row of the figure shows two charts that depict simple averages of the different dimensions of the two indices of reforms discussed in the previous subsection. Data show that, for a representative country in LAC, narrow measures of institutions show mixed signs. Specifically, the SRI - 2014 database shows that investment booms seem to be accompanied by reform reversals, while the SRI - 2005 database shows that reforms tend to reverse before boom years, improve during boom years, and finally stagnate during the following 8-year period. However, these regional averages often entail country heterogeneity.

To address some of the unobserved heterogeneity of Figure 2, in Figure 3 we present three charts for the LAC region. The first, presenting correlate variables to our measures of

¹¹Due to the nature of these indices, it is not possible to claim that a country is better in one index than another just by directly comparing the levels of these indices. For instance, it is not possible to claim that, in country X rule of law is higher than democratic accountability by making a comparison of both indices. Therefore, all indices are ratios relative to the U.S.

¹²This might be due to data revisions and measurement error corrections.

¹³The IMF proposes three broad income groupings: Advanced Economies (AE), Emerging and Developing Economies (EMDE), and Low Income Developing Countries (LIDC).

economic and political institutions, provides evidence that (a) bureaucratic quality improves during the boom years (only to then decrease below the level before the boom); (b) the same happens with corruption (the higher the indicator, the lower the corruption); (c) interestingly enough, data show that government stability improves, even relative to the U.S. during the years near the start of the boom; and (d) a variable named investment profile (which measures other components not present in the law and order index)¹⁴ shows a pattern very similar to that of the government stability index.

The middle chart shows each of the dimensions of the SRI - 2014 database. We observe that all but the trade index decrease in the years close to the start of the boom. Indices stabilize during boom years, except, interestingly, the index of labor market reforms, which constantly improves from the start of the boom. After the boom window, all indices resume falling to then increase or stabilize at the end of the analyzed period.

The last chart shows each of the dimensions of the SRI - 2005 database. All indices, except the agricultural markets index, decrease in the years around the start of the boom. During the boom, the pattern reverses: all indices stabilize or improve, except the agricultural reform index. After the boom, the indices stabilize, except the trade index, which continues to improve.

4 Empirical Strategy

4.1 Probit model

Since our ultimate interest is to determine whether a particular variable, institutions, could propitiate an investment boom, the strategy was to use a probit model of the form:

$$Pr(Y_{it} = 1 | \mathbf{X}_{it}, \mathbf{I}_{it}, \beta, \gamma) = F(\beta \mathbf{X}'_{it} + \gamma \mathbf{I}'_{it}) \quad (3)$$

where Y_{it} is a dummy variable that takes the value of 1 at the beginning of an investment boom episode (as defined in Section 1) and 0 otherwise, $F(\cdot)$ is the standard normal density function, and \mathbf{I}_{it} and \mathbf{X}_{it} are $1 \times k$ and $1 \times j$ vectors of the institutional and control variables discussed in the previous section, respectively. β and γ are $k \times 1$ and $j \times 1$ vectors of coefficients, respectively.

Moreover, because there could be uncertainty around the precise starting date of an episode, for our baseline specification, as in Hausmann *et al.* (2005), we expanded our dependent variable to also take the value 1 the immediate year before and the year after the beginning of the episode, and 0 otherwise. Also, the years an episode cannot take place (i.e., the years 2 to 7 after the start of an episode, as well as the first and last seven years of the sample) are excluded.

¹⁴This index is an assessment of three factors affecting the risks to investment that are not covered by the other risk components of the country risk index, which are (a) contract viability/expropriation, (b) profit repatriation, and (c) payment delays.

4.1.1 Results

Because investment booms are triggered by factors other than changes in institutions, we will discuss results for the control variables as well. The results of the model defined in equation (3), are presented in Table 4 for our set of institutional variables in a broad sense, whereas Table 5 and Table 6 show results for our set of narrow measures institutions. Column 1 shows results for our institutional variables only. Column 2 expands the model by adding external factors. Column 3 adds policy variables and Column 4 incorporates domestic factors. We show average partial effects. In what follows, the results for each set of institutional variables will be briefly discussed.

Broad measures of institutions Our full controls specification (column 5 in Table 4) indicates that increases in the law and order index have either a near-zero effect on the probability of initiating an investment boom and the effect is not statistically significant. Column 5 also shows that the democratic accountability index has either a small positive or a near-zero and statistically insignificant effect on the probability of initiating an investment boom.

Narrow measures of institutions Table 5 repeats the procedure of Table 4 for our set of narrow measures of economic institutions. For the reform indicators pertaining to the SRI - 2014 database, it is observed that external finance reforms consistently reduce the probability of experiencing an investment boom, at the 1 percent level, whereas our model with full controls (column 5) shows that domestic finance reforms increase the probability of an investment boom at the 5 percent level. Most of the covariates that reported statistical relevance in the models that consider broad measures of institutions report a similar pattern here, suggesting a good specification performance for a potential “structural” model of investment boom predictability.

Table 6 repeats the process described above but with the indicators pertaining to the SRI - 2005 database. It is observed that trade reforms reduce the probability of experiencing an investment boom, at the 5 percent level. The same happens with capital account reforms, a result that coincides with that of the external finance reform index of the SRI - 2014 database. Although not consistently significant to the inclusion of controls, the indices of current account reform and product market reform report positive and negative signs, respectively. As with the SRI- 2014 database indicators, the control variables that showed statistical relevance before continue to do so.

Control variables Regarding our external sector variables, our model’s results indicate that increases on real foreign interest rates and capital market volatility reduce the probability of experiencing an episode, each at the 5 percent level. As more controls are added to the model, this coefficient remains consistently significant, strongly suggesting that these variables are relevant to the likelihood of an investment boom’s taking place. No significant results are obtained for terms of trade shocks. For trade openness, there is positive but near zero effect on the likelihood of an episode’s taking place at the 5 percent level. The opposite

occurs with a higher degree of financial account openness, which correlates negatively at the 1% level, a result that could be tied to higher constraints on capital to pursue investment opportunities abroad. Both results remain significant as more controls are added. Finally, an increase in portfolio investment lowers the probability of an episode’s taking place at the 5 percent level. Interestingly, the coefficient of FDI flows is not statistically significant in this model.

In terms of macroeconomic policy variables, the degree of fiscal procyclicality has a positive and statistically significant effect, a not-so- unexpected result, because a government that contracts spending during downturns can affect total capital accumulation through the public component of investment. This is typical in emerging markets because capital investment seems to be the “adjustment” variable during bad times (Ardanaz & Izquierdo (2017)), that is, the *public investment effect* dominates. As expected, inflation has a negative and statistically significant effect. The coefficients regarding exchange rate policy, the undervaluation index, and the exchange rate stability index are not statistically significant.

Regarding domestic factors, the (natural log of) capital to output ratio is negative and statistically significant at the 1 percent level. More formally, a 1 percent increase in the capital to output ratio decreases the probability of an episode’s taking place by around 4 percent. This suggests that rises in capital productivity (e.g., a lower level of investment needed to produce a given growth rate in the economy) increase the likelihood of experiencing an investment boom. Increases in per capita GDP lower the probability of an episode at the 1 percent level, suggesting the presence of convergence effects. An increase in the share of natural resource rents over GDP lowers the probability of experiencing an investment boom at the 10 percent level, a result that might go hand in hand with the “Dutch Disease” hypothesis. Increases in human capital (measured by the share of the population with at least secondary education completed) also correlate positively with the probability of initiating an investment boom and the result is significant at the 1 percent level. Finally, an interesting result is that the presence of crises (financial, debt, or currency crises) in the previous 5 years increases the probability of an investment boom episode. The effect is statistically significant at the 5 percent level.

4.2 Fixed effects model

The previous model, however, does not allow the possibility of unobserved individual heterogeneity and therefore could be considered a naive model. To control for the possibility that the coefficients of the explanatory variables in the previous pooled model are driven by either (a) country specific characteristics or (b) a worldwide movement toward investment, our baseline specification is augmented including country and time fixed effects. More concretely, we tried to estimate the following model:

$$Pr(Y_{it} = 1 | \mathbf{X}_{it}, \mathbf{I}_{it}, \alpha, \tau, \beta, \gamma) = F(\beta \mathbf{X}'_{it} + \gamma \mathbf{I}'_{it} + \alpha_i + \tau_t) \quad (4)$$

where, as before, $F(\cdot)$ is the standard normal density; \mathbf{X}_{it} and \mathbf{I}_{it} are $1 \times k$ and $1 \times j$ vectors of institutional and control variables, respectively; β and γ are $k \times 1$ and $j \times 1$

vectors of coefficients, respectively; and α and τ represent unobserved country specific and time specific effects, respectively. The model does not impose restrictions on the relations between the covariate vector and the unobserved effects. In our setting, conditioning on the unobserved effects also serves to control for endogeneity, as the country and time effects capture unobserved heterogeneity that can be related to the covariates¹⁵.

4.2.1 Results

Broad measures of institutions The results of the model are presented in Table 7. Our full controls specification (column 5) indicates that increases in the law and order index have a large positive effect on the probability of initiating an investment boom and that the effect is statistically significant at the 1 percent level. In contrast, the model shows that increases in the democratic accountability index reduce the probability of experiencing an investment boom, also at the 1 percent level.

Narrow measures of institutions Table 8 show results for the SRI - 2014 indices. It is observed that domestic finance reform is again associated with a positive effect on the probability of experiencing an investment boom while external finance loses significance. Trade reform now has a significant negative effect at the 1 percent level. Even though they are not statistically significant, product and labor market reform consistently show negative signs.

Table 9 show results for the SRI - 2005 indices. Domestic finance reform stands out as consistently showing a positive effect on the probability of experiencing an investment boom. As in the model without fixed effects, trade reforms have a negative and significant effect. Product market reform shows a negative effect in our model with full controls (column 4). Even though they are not statistically significant, current account and agriculture reform report positive signs in some of our models.

Control variables Regarding external sector controls, as in the previous model increases in real foreign interest rates and capital market volatility reduce the probability of experiencing an episode, both at the 1 percent level. The same negative effect occurs with increases in the terms of trade, trade openness, and financial openness, all of them being statistically significant at conventional levels. Notice that, compared to the naive model, trade openness switches sign. A negative terms of trade coefficient could imply a validation of the “Dutch Disease” hypothesis, whereas negative trade and financial openness coefficients could imply a *substitution effect*, that is, when the basket of substitute opportunities is increased, capital expenditures might shift not only to current expenditures, but to *foreign* capital expenditures opportunities as well, a hypothesis that has to be tested in future research. The coefficient of FDI flows is negative and statistically significant at the 10 percent level, an

¹⁵Two-way (N and T) fixed-effects panel-data methods can be biased because of the incidental parameter problem (Neyman & Scott (1948), Wooldridge (2010)). However, this can be mitigated by using the corrections proposed by Cruz-Gonzalez *et al.* (2017). These authors show that bias correction is not needed when $T > 30$. Therefore, our parameters are robust to the incidental parameters problem.

expected result, considering that we are using the *net* value of FDI, that is, the higher the amount of domestic capital going abroad (assets increase more than liabilities), the lower the probability of experiencing an investment episode domestically. Finally, as in the naive model, an increase in portfolio investment lowers the probability of an episode's taking place.

Of the macroeconomic policy variables, the degree of fiscal procyclicality has a positive and statistically significant effect, suggesting that the *public investment effect* dominates the *risk aversion effect*. Inflation continues to show a negative and statistically significant effect. The coefficients regarding exchange rate policy—the undervaluation index and the exchange rate stability index—are not statistically significant.

For domestic factors, as in the naive model, the effect of the capital to output ratio is negative and statistically significant at the 1 percent level. Increases in per capita GDP lower the probability of an episode at the 1 percent level. In this model, an increase in the share of natural resource rents share on GDP increases the probability of experiencing an investment boom at the 5 percent level. Increases in human capital increase positively the probability of initiating an investment boom and the result is significant at the 1 percent level. Finally, the presence of crises in the previous 5 years increases the probability of an investment episode and this effect is statistically significant at the 1 percent level.

4.3 Robustness models

4.3.1 Sample selection

A source of bias in the previous results could be sample selection. In this section, we control for it by testing to what extent the inclusion of a group of countries in the sample drives the results. For instance, it could be that episodes happening in AE (as per the IMF definition), which in turn have higher scores in the indices of institutions used in this study, are the ones driving the results of the previous sections. Therefore, an interesting exercise is to restrict the sample by excluding these countries.

The results restricting the sample to only the IMF categories of EMDE and LIDC are reported in Table 10. Column 1 shows the result of the fixed effects model without controls, column 2 shows the result including controls and, because when controls are added the sample size is reduced significantly, column 3 repeats the model of column 1—that is, without the controls—but uses the restricted sample of the model with controls (thus controlling for bias coming from the smaller sample).

As can be seen in Table 10, the results reveal that improvements in the index of economic institutions increase the probability of an investment boom's taking place, which does not seem to be driven by the introduction of controls (column 3). As with the full sample, the coefficient of law and order is consistently positive and statistically significant while the coefficient of democratic accountability is not.

Regarding controls, we will only comment on the relevant changes vis-a-vis the previous models. One is that, in this model, trade openness is statistically significant at the 10 percent level. In a sample that excludes AEs, this is an expected effect, because for countries in

the early stages of accumulation, trade openness enables the importation of capital goods. Reserve accumulation is also negative, suggesting that this crowds out domestic capital investment. Fiscal procyclicality is negative, suggesting that the *risk aversion effect* dominates the *public investment effect*.

We repeat the previous procedure with our two measures of narrow economic institutions. Table 11 shows that, for the reform indicators pertaining to the SRI - 2014 database, no consistent pattern of significance across indices is observed. The two indices that perhaps show a consistent pattern are domestic finance and labor market reforms, which consistently report the same signs (positive and negative, respectively) with statistical significance across different specifications, suggesting they are robust to the inclusion of controls, fixed effects, and sample selection.

Table 12 shows the same models for the indices that comprise the SRI - 2005 database. Here, we observe clearer patterns: for instance, trade reforms correlate negatively at the 5 percent level, finance reforms correlate positively at the 10 percent level, and agricultural sector reforms correlate positively at the 5 percent level. These results seem robust to the introduction of controls, country, and time effects, as well as of sample selection.

4.3.2 Endogeneity

Another source of bias comes from the possibility that an investment boom episode may have an effect on institutions. To try to deal with this issue, we adopt two approaches: (a) use instrumental variables and (b) check if an investment boom has an impact on institutions.

Instrumental variables An ideal source of exogenous variation of institutional variables is always hard to find. Most proposed instruments on studies related to the impact of institutions on an outcome variable (usually economic growth) do not vary over time within countries; therefore, they do not help much when country fixed effects are included.¹⁶ The other approach is to use lagged values of the independent variables as instruments. By using variables that are predetermined (i.e., determined prior to the current period) rather than endogenous, the principal assumption is that they are uncorrelated with the past error term, an assumption that is easily violated when independent variables possess autocorrelation. Moreover, Bellemare & Wang (2019) show that when the “lagged IV” has a direct causal impact on the dependent variable or on unobserved confounders, it violates both the independence assumption and the exclusion restriction [Angrist & Pischke (2009)] and therefore it increases bias relative to OLS. Thus, the lagged dependent variable approach deals more with sequential endogeneity rather than with theoretical endogeneity. In sum, any strategy has its pros and cons and the best strategy is probably a combination of the two.

¹⁶Some of the previously proposed instruments are country size and trade restrictions that influence international trade (Lee (1993)), absolute degrees latitude and primary language (Hall & Jones (1999)), population density and settler mortality at the time of colonial settlement (Acemoglu *et al.* (2001)), and the presence of state religion (Barro & McCleary (2003)).

In this study, we try to address these challenges by building upon the idea of [Persson & Tabellini \(2009\)](#), who exploit variation in geographically proximate neighbors' democracy to control for the impact of a country's historical experience with democracy on that country's output (measured as GDP). The intuition behind their instrument is that democracy (democratic capital, to be precise) in neighboring countries only affects GDP in the domestic economy through its effect on the probability of political regime change. Another study that takes a similar approach is that of [Buera *et al.* \(2011\)](#), who propose and estimate a learning model to explain the diffusion of market-oriented policies across countries in the period after World War II. In their model, the authors instrument policy makers' beliefs with a distance-weighted measure of past market-oriented policy decisions in neighboring countries, with the result that past policy decisions in countries that are geographically closer receive a higher weight in influencing the beliefs of the policy makers of the country that is the focus. The authors conclude that the adoption of liberalization policies by neighboring countries influences the beliefs of policy makers about the desirability of reforms in their domestic economies.

The study of [Giuliano *et al.* \(2013\)](#) uses democracy in neighboring countries as an instrument for democracy in the country of study to determine the effect of democracy on structural reforms. The idea behind this instrument is that democracy in political allies has an influence on domestic democracy but no direct impact on a country's ability to reform. More recently, to assess the effect of democracy on GDP, [Acemoglu *et al.* \(2019\)](#) use regional waves of democratization in countries with common political histories as an instrument for democracy in the original country. The relevance of this instrument is that it distinguishes the diffusion of democracy from the role of regional economic shocks or the spread of economic conditions to nearby countries through trade and other mechanisms, thus ensuring that idiosyncratic changes in a country's political regime that are endogenous to its GDP do not bias their model estimates¹⁷.

[Ponticelli & Voth \(2020\)](#) also use a distance-weighted instrument to study the impact of fiscal austerity on social unrest in Europe during the 1918–2008 period. Specifically, they use a distance-weighted measure of other countries' past fiscal policies as an instrument of fiscal policies in the country of study. [David *et al.* \(2021\)](#) study the impact of structural reforms on different outcome variables. They exploit the timing of liberalization/reform reversals across countries by instrumenting distance-weighted changes in the reform index in the country of study with current and past changes in the reform index in nearby countries. Again, the validity of their strategy is rooted in the assumption that the sources of proximate external variation of reforms are not affected by their own economic conditions and that they affect economic performance only through its impact on a country's own adoption of reforms.

¹⁷Note that the difference between the two instruments is in the weighting: [Persson & Tabellini \(2009\)](#) use a distance-weighted measure of democracy for all countries whereas [Acemoglu *et al.* \(2019\)](#) use a jackknife average for different regions (see [Acemoglu *et al.* \(2019\)](#) for a discussion of this difference and its impact on their model estimates).

Building upon these previous strategies, for each of our institutional measures we estimate the following instrumental variable model:

$$Z_{it}^{-i,W} = \sum_{j \in W_{-i}} \frac{1/\log(d_{i,j})}{\sum_{k \in W_{-i}} (1/\log(d_{i,k}))} I_{jt} \quad (5)$$

where W_{-i} is the set of all countries for which data on the institutional index are available, excluding country i ; I_{it} is the institutional index; and $d_{i,j}$ is the bilateral population-weighted distance between country i and country j , as presented in the CEPII Geodist data set (Mayer & Zignago (2011)).

The corresponding IV model we estimate is given by

$$\begin{aligned} Y_{it} &= \mathbf{X}'_{it}\beta + \mathbf{I}'_{it}\gamma + \alpha_i + \tau_t + \varepsilon_{it}, Y_{it} = 1[Y_{it}^* > 0] \\ \mathbf{I}'_{it} &= \mathbf{Z}'_{it}\pi + \mathbf{X}'_{it}\phi + \theta_i + \eta_t + v_{it} \end{aligned} \quad (6)$$

where \mathbf{I}_{it} is a $1 \times p$ vector of endogenous variables, \mathbf{X}_{it} is a $1 \times k$ vector of control variables, \mathbf{Z}_{it} is a $1 \times k$ vector of instruments, and the equation for \mathbf{I}_{it} is written in reduced form. By assumption, $(\varepsilon_{it}; v_{it}) \sim N(0; \Sigma)$. β and γ are vectors of structural parameters, and π and ϕ are matrices of reduced-form parameters.

The main assumption for the exclusion restriction is that it requires that geographically proximate changes in institutions are significant determinants of local institutions but are not themselves caused by geographically proximate investment booms. Thus, to verify that our results are not driven by a potential correlation between neighbor-country institutions and neighbor-country investment booms (that are not occurring through the impact of a country's own institutional changes), we present our estimates both with and without controlling for different economic factors that may occur in neighboring countries (e.g., our control variables above).

Results Table 13 shows the second stage results for our first set of institutional variables while Table 14 shows the first stage results. Because the addition of controls captures time-variant effects for all countries and, at the same time, reduces models' degrees of freedom, we run our specifications using country fixed-effects only. Table 14 confirms the validity of the instruments.

Broad measures of institutions. In Table 13, columns 1 and 2 show the results for the variable law and order (instrumented by the geographically weighted measure of law and order in other countries). Column 1 shows results without our set of controls while column 2 includes them. Overall, for this variable, we observe an unconditional significant and positive effect. When controls are added, however, the effect switches sign, is reduced in magnitude, and loses statistical relevance. Columns 3 and 4 repeat the process for the democratic accountability variable. A similar pattern is observed, though after conditioning on controls, the variable keeps its positive, although insignificant, sign.

Narrow measures of institutions – SRI - 2014 database. For our narrow measures of economic institutions, Tables 15 and 16 show second and first stage results, respectively, for the indicators pertaining to the SRI - 2014 database. We present the results in three panels: panel A shows the fixed-effect model without the inclusion of our set of controls, panel B includes our set of controls, and panel C uses the model of panel A—that is, without the controls—but with the restricted sample of the model with controls. As before, our model only adds country fixed effects.

Table 16 shows the validity of the instruments—namely, that each SRI - 2014 index of reforms is determined by its correspondent geographically weighted equivalent in other countries. Interestingly, for labor market reform, the coefficient is consistently negative across specifications, suggesting that when neighbors free the labor market, countries next door regulate them. Table 15 shows that, although the instruments are valid, most of the instrumented SRI - 2014 indicators are not statistically significant. Essentially, the only consistent result is for trade reforms, which reduce the probability of experiencing an investment boom. This result continues to be consistent across models and now to the possibility of endogeneity as well. Beyond trade reform, the other index that shows statistical relevance is that for product market reform, which has a negative effect, again an effect consistent to different robustness checks.

Taken together, these results suggest the possibility for a channel through which these reforms might reduce the probability of experiencing an investment boom: domestic investors might find their investment prospects slump due to these type of reforms, such as trade and product market reforms, that might increase competition not only from foreign capital (due to trade reforms) but from domestic capital (due to product market reforms), too.

Narrow measures of institutions – SRI - 2005 database. Tables 17 and 18 show second and first stage results, respectively, for the indicators pertaining to the SRI - 2005 database. As before, we show the results in three panels. Table 18 shows that all the instruments are valid. Table 17 shows that although the instruments are valid, most of the impacts of reforms in terms of increasing the probability of experiencing an investment boom are not significant. Only in the case of trade reforms do we observe a significant and negative effect, in line with the results of the previous index. Although the product market and agriculture indices seem to have positive effects in the model without controls, they reverse their signs when controls are included. However, these results could be driven by sample selection. This is in fact the case: panel C confirms that, as variables lose significance. Although not statistically significant across models, finance reform seem to have a negative effect, but this effect seems driven by sample bias.

In sum, it seems that the negative effect of trade reforms on the probability of experiencing an investment boom is robust to the inclusion of controls, fixed effects, sample selection, and endogeneity.

Reverse causality We now move on to checking whether the presence of an investment boom has an effect on institutions. Specifically, for each index in our three sets of institutional variables we estimate the following model:

$$\mathbf{I}'_{it} = \gamma B_{it} + \beta \mathbf{X}'_{it} + \alpha_i + \tau_t + \varepsilon_{it} \quad (7)$$

where \mathbf{I}_{it} is a $1 \times p$ vector of endogenous variables; B_{it} is a dummy variable that takes the value of 1 at the beginning of the investment boom episode, 1 the year before, 1 the year after, and 0 otherwise; and \mathbf{X}_{it} is a $1 \times k$ vector of control variables. β and γ are vectors of parameters; α and τ are country and year effects, respectively; and ε_{it} is the error term. Here, the parameter of interest is γ .

Results Tables 19 to 21 show the results for our three sets of institutional variables.

Broad measures of institutions. Table 19 shows the results for our set of broad institutions. Columns 1 to 3 show the effect of the start of a boom on the law and order index while columns 4 to 6 do so for the democratic accountability index. As before, columns 1 and 4 show the unconditional model, columns 2 and 5 add controls, and columns 3 and 6 repeat the model of columns 1 and 3—that is, without the controls—but using the restricted sample of the model with controls.

As shown in Table 19, the start of an investment boom correlates positively with the law and order index, the results being statistically significant. Although the effect is also positive for the democratic accountability index, it is not statistically significant. We observe, however, that the law and order index has a positive effect on the democratic accountability index and that law and order does the same on democratic accountability, that is, there is a mutually reinforcing effect result. This result is in line with Barro (2012), who finds an indirect connection between law and order and democracy through the income and education channels (although he does not find that the connection flows in the reverse direction). Concretely, improvements in the law and order index boost economic growth, leading to higher levels of per capita income and human capital in the future. These higher levels of income and human capital would then tend to improve democracy, which in turns improves growth further at later stages. In our case, controlling for investment booms, the results suggest that improvements in the rule of law foster democratic accountability and vice versa.

When comparing to the results of our IV model in Table 13, the picture is clearer: while (exogenously determined) economic institutions have an (unconditional) positive and statistically significant effect on investment booms, a feedback effect model contradicts this finding. In contrast, while (extraneously determined) political institutions also have an (unconditional) positive effect, this effect is robust to the possibility of feedback effects as well. This suggests a more exogenous nature for the role of political institutions than for that of economic institutions in relation to the advent of an investment boom.

Narrow measures of institutions Table 20 show the results for the SRI - 2014 indices, which show that investment booms don't have any statistically significant effect on reforms. Only in the case of domestic finance reforms is it observed that investment booms have a

positive and statistically significant effect. Table 21 shows the same results for the SRI - 2005 indices. As with the 2014 indicators, investment booms only have a positive and statically significant effect on finance reforms. Taking the results of Tables 20 and 21 together, it seems that investment booms do improve the conditions of the domestic financial market. This is an expected result, because the domestic financial market is a key vehicle for the proliferation of investment [Levine (2005)] during the years of the next boom.

4.3.3 Nonlinearities

Institutions can have nonlinear effects on the fostering of an investment boom. In particular, this could happen if an investment boom episode occurs after a certain level of institutions is reached. To investigate this hypothesis, we replicate our baseline model with fixed effects and add a squared reform index term for each of the variables in our three sets of institutional variables.

Results Tables 22 to 24 present the results of these regressions on our three sets of institutional variables.

Broad measures of institutions In Table 22 we present the results for our set of broad economic and political institutions. As in the previous sections, we first estimate our model without controls, then with controls, and finally controlling for sample selection. Columns 1 to 3 report the results for the law and order variable. It is observed that both coefficients, linear and squared, are statistically significant, the first being positive and second being negative, suggesting that law and order has a diminishing effect on the probability of experiencing an investment boom. While the same happens in the model without controls for democratic accountability, the effect is reversed in sign when controls are added.

These results seem to go hand in hand with the growth and institutions literature. Since countries with higher values in the law and order index are those with higher capital stocks per capita, they have diminishing returns to capital accumulation and therefore are associated with fewer investment booms in our sample. Moreover, this result also implies that as EMDEs reach a certain level in terms of the rule of law, it is more difficult for them to propitiate an investment boom, either because they already had one in the past or because they have to compete in other dimensions (and with AEs) to reach the inframarginal investor (the same can happen in AEs).

Narrow measures of institutions Table 23 shows the results for the SRI - 2014 indices. We observe a diminishing effect on external finance reforms (panel B, column 2) and an increasing effect on labor market reforms (panels B and C, column 5), the latter being robust to sample selection. Table 24 shows the results for the SRI - 2005 index; we consistently observe diminishing effects for capital account (panels A and B, column 3) and finance reforms (panels A and B, column 4). In our models we observe—although not consistently—an increasing effect of trade reforms (panel B, column 1) and decreasing effects on current

account (panel B, column 2), product market (panel A, column 5), and agriculture (panel B, column 6) reforms.

Although the picture is not entirely consistent across the narrow indices of economic institutions, it seems that reaching a certain level in some instances reduces the probability of experiencing an investment boom, an expected result considering that having more-liberalized sectors could at least either be a sign of (a) high levels of capital stock and (b) difficulties in propitiating an episode through better regulation or sector liberalization (due to a higher level of competition). An interesting case is that of labor market reforms, which suggests there are increasing probabilities of experiencing a boom after the market reaches a high level of regularization, and reducing it will decrease the probability.

4.3.4 Factor analysis

Finally, to take into account the possibility that the institutional process is one unique process common to all institutions, we conduct a factor analysis on our three sets of indices of institutions. Specifically, we extract the first principal component¹⁸ from all variables pertaining to each set. More formally, we estimate the following model:

$$Pr(Y_{it} = 1 | \mathbf{X}_{it}, PCI_{it}, \alpha, \tau, \beta, \gamma) = F(\beta \mathbf{X}'_{it} + \gamma PCI_{it} + \alpha_i + \tau_t) \quad (8)$$

where, as before, $F(\cdot)$ is the standard normal density, PCI_{it} is the first principal component in each set of institutional variables, \mathbf{X}_{it} is a $1 \times k$ vector of control variables, β is a $k \times 1$ vectors of coefficients, α represents unobserved country specific effects, τ time specific effects, and γ is the parameter of interest.

Results The results are reported in Tables 25 to 27.

Broad measures of institutions Table 25 shows that the first principal component of our two measures of economic and politic institutions, together with indices of bureaucratic quality and corruption, have a positive effect on the probability of experiencing an investment boom and these results are robust to the introduction of controls and fixed effects.

Narrow measures of institutions Table 26 applies the same procedure to the first principal component of all the indicators pertaining to the SRI - 2014 database. The results, however, are not statistically significant. Table 27 shows the effect for the first principal component of the indicators of the SRI - 2005 database; in this case, the effect is positive and statistically significant at the 1 percent level.

Overall, these results are reassuring to our prior that a comprehensive improvement in institutions in both a broad and in a narrow sense contributes positively to the starting of an investment boom.

¹⁸For a formal compendium of these methods, see Afifi *et al.* (2012).

5 Institutions and Booms in the Andean Region

What can we say about the role of institutions in the Andean Region? In this section we will discuss developments in the Andean countries during their boom episodes, compare them with other boom episodes in Latin America and provide some observations regarding these experiences.

5.1 Andean countries

The first important fact is that there has been only three booms in the region. In chronological order, there was one in Venezuela in 1973, another in Peru in 2005, and the last one was in Bolivia in 2010.

Looking at Figure 4, these booms happened at the time of important resource booms. However, as seen in Figure 5, these booms do not necessarily imply higher terms of trade. They do imply higher production of commodities as well as more fiscal revenue from the sector.

Looking more broadly and comparing investment booms in these three countries, most external variables behaved in a relatively similar fashion before, during, and after the booms. Only the terms of trade and total trade indices seemed to vary more in the booms of the Andean countries. Performance in terms of policy variables (Figure 6) was different across these countries, as well as across other Latin American countries and other emerging markets. Therefore, there is not one distinct policy instrument that characterized these booms. In terms of internal factors (Figure 4), an interesting fact is that the three Andean booms were not preceded by crises.

Does this mean that institutions were not important? In 1973, Venezuela had trade, current account, capital account, and finance reform indices higher than other Latin American countries, as well as other emerging markets. Both capital account and finance reform indices were not only higher than for other emerging markets, but they kept or increased their levels during the boom. In 2005, Peru had higher indices than Latin American and other emerging market countries in all areas, with the exception of labor market reform. Furthermore, these indices kept these levels. Finally, in 2010 Bolivia only had higher indices in external finance and domestic finance reforms, with the latter maintaining its high level for the duration of the boom. Therefore, a common element in all three cases is that finance reform indices were higher.

However, was this behavior different than in booms in other countries? In general, institutional indices were at a higher level before the boom in the Andean Countries than in other Latin American countries and other emerging markets before their booms. The main exceptions were indices related to labor markets and product markets, which were higher than for other emerging markets but similar in level to other Latin American countries (see Figures 7 to 9).

5.2 Other LAC countries

We considered booms in other Latin American countries that were not related to resource booms: Chile in 1962 and 1992 and the Dominican Republic in 1971 and in 2006 (see Figures 10 to 15). For the booms in 1962 and 1971 we have less data, but the main similarity is that the index for trade reform in both countries was either higher than the average for Latin American and other emerging market countries or increased during the boom. For the booms in 1992 and 2006, the index for trade reforms also performed at the level seen in the previous booms. In addition, in both cases the indices for product market reform and finance reform were also higher than in similar countries. Again, therefore, finance reform indices were important.

Nevertheless, as we asked above, how do these countries compare to other similar countries in terms of institutional variables before their respective booms? The evidence here is muddled. In contrast to the Andean countries, depending on the set of institutional variables used the results change. Like the Andean countries, they tend to consistently outperform other emerging markets before their booms. However, when they are compared against other Latin American countries, in some data sets they outperform the latter and in others they do not. The only exception is in domestic finance reform, where they outperform other emerging markets, and even though they do not outperform other Latin American countries initially, their trend is positive as opposed to that for the region. When looking at the alternative data set, they do outperform other similar countries in general finance reform.

5.3 Section remarks

In summary, the three Andean countries that have experienced a boom do show better institutional performance at the start of the boom. A common element is finance reform, which is in line with the results of the models estimated here and with booms observed in other Latin American countries that were not related to commodity booms. Furthermore, they tend to have better indices in many institutional aspects. Consequently, in spite of enjoying commodity booms, these countries seemed to have benefited from better institutions.

These results are interesting for Colombia and Ecuador, which present contrasting pictures. The economic performance of Colombia has been one of stability. Therefore, the fact that there has not been an investment boom may not be a relevant issue, though capital accumulation is still a challenge for the country¹⁹. However, Ecuador has experienced economic volatility.

Both countries have experienced commodity booms. However, in contrast to the other Andean countries, only in capital account and current account indices did they experience periods with better performance than other Latin American and emerging markets. Ecuador did have a high external finance reform index at the same time the country experienced its commodity boom at the beginning of this century. Currently, their indices are similar or

¹⁹See: Melendez & Harker (2008).

below the average for Latin America and other emerging markets. Therefore, an area of further research is to understand how to foster investment in these countries.

6 Concluding Remarks

In this study we asked the question: can institutions foster an investment boom? To answer it, we constructed an unbalanced panel for the last 70 years that considered reforms at different levels and dimensions. In particular, we considered “broad” (that is, aggregate measures of institutions) economic and political and “narrow” economic (that is, at some particular level) institutions. We analyzed the effects of these on carefully designed investment boom episodes, controlling for the standard determinants of investment and using a battery of estimation techniques and robustness checks.

In this section we will discuss our takeaways from the study, highlighting potential areas for future research, most of them related through the specific channels through which they might work, notably the further study of the nonlinear nature present in some of the indices.

The answer to our main question is that improvements in economic institutions (measured as increases in a law and order index) have a positive effect on the probability of initiating an investment boom. This effect is consistently significant and robust to a series of changes in episode identification parameters, different functional forms, the introduction of control variables, unobserved heterogeneity, sample selection bias, and endogeneity concerns.

We also find that this effect is nonlinear, suggesting that after some threshold, economic institutions contribute less to the advent of an investment boom. This is an expected result, given the high correlation between countries with high capital stock per capita and higher institutional development; we could think of these economies as being at the steady state. This result also implies that, as emerging and developing economies reach a certain level in the rule of law, it is more difficult for them to propitiate an investment boom, either because they already are in the steady state or because they must compete in other dimensions in order to reach the inframarginal investor and initiate a boom (the same logic applies to advanced economies).

With regard to political institutions, we mostly find small unconditional positive effects and parameter sensitivity, particularly to functional form and different episode identification parameters in a sample of emerging and low income economies. The same pattern is observed when we look at the effect of democracy on booms where the capital stock per capita grows faster than in our baseline model. When analyzing shorter booms, we observe that the effect becomes positive after a threshold. We also find a more exogenous nature for the role of political institutions than for economic institutions in fostering an investment boom.

Moreover, when testing for the possibility of reverse causality (from booms to institutions), we find that democratic accountability has a positive effect on law and order (and vice versa), suggesting the possibility that democratic accountability might have an indirect effect on investment booms through its positive impact on law and order.

Regarding narrow measures of institutions, we find that trade reforms have a negative effect in initiating an investment boom. This result invites the study of the channels through which this might occur. A possibility is that domestic investors might find their investment prospects slump, as trade reforms increase not only competition in the domestic market due to the arrival of foreign goods (and investors are therefore more cautious about increasing fixed capital), but also the incentive to substitute for domestic production with imports, particularly in the sectors where there are no comparative advantages.

External finance reforms consistently reduce the probability of experiencing an investment boom, but this effect reverses after some level of liberalization. A possibility for a linear effect to occur is that, as there is more capital account openness, capital might pursue other investment opportunities abroad rather than at home. Since this is a composite index of capital inflow and outflow restrictions, it could be that up until a certain mix of both inflow and outflow regulation is achieved, the effect becomes more and more positive. Gaining an idea of where this threshold is would be an interesting avenue for future research.

Interestingly, when we instrumented labor market reforms, the first stage yielded consistently negative coefficients across specifications, suggesting that when neighbors free the labor market, countries next door regulate them. This might be due to the social unrest that typically accompanies labor market liberalization. This is a great avenue for research.

Another possibility to explore is the positive effect of agricultural reforms. Since the index relates to the level of regulation in the main commodity of export, it certainly could be the case that opening a competitive sector could bring more investment. A nonlinear significant effect suggests that the impact occurs after some level of regulation is achieved.

Overall, the presence of nonlinearities in reforms could be a sign of (a) high levels of capital stock per capita and therefore a lower probability of experiencing an investment boom once a certain level of capital is reached or (b) the difficulty of igniting an episode through better regulation and/or sector liberalization, since the sector might have reached the current frontier and therefore it is harder to promote capital accumulation this way.

A negative effect occurs with increases in the terms of trade, trade openness, and financial openness, all of them being statistically significant at conventional levels. A negative terms of trade coefficient could imply validation of the “Dutch Disease” hypothesis, whereas negative trade and financial openness coefficients could imply a *substitution effect*, that is, when the basket of substitute opportunities is increased, capital expenditures might shift not only to current expenditures, but to *foreign* capital expenditures opportunities as well. This is also in line with our results on capital account reforms. This is a hypothesis that has to be tested in future research.

The effect of net FDI flows is negative and statistically significant, an expected result, considering that as domestic capital goes abroad, the lower the probability of experiencing an investment episode domestically. The same occurs with increases in net portfolio investment: they lower the probability of an episode taking place. Reserve accumulation is also negative, suggesting that this crowds out domestic capital investment. Research on the channels through which this effect acts, whether crowding out, real exchange rate appreciation, or something else, is certainly pertinent; it was beyond the scope of this paper.

While some of our models show that the degree of fiscal procyclicality has a positive and statistically significant effect, suggesting that macroeconomic volatility acts as a deterrent to investment, other models show a negative effect when we restrict the sample to emerging and low income economies, suggesting that the “macroeconomic volatility aversion effect” dominates the “lower public investment effect” in relation to investment booms. While this might be plausible, it seems counteractive for emerging and low income economies. Therefore, its validation is another avenue for future research.

Consistent with the literature on natural resources, the presence of natural resource rents goes in opposite ways. In some models, an increase in the share of natural resource rents over GDP lowers the probability of experiencing an investment boom. The result might go hand in hand with the “Dutch Disease” hypothesis. Nonetheless, in some of our models, an increase in the share of natural resource rents over GDP increases the probability of experiencing an investment boom, suggesting that resource booms can also be investment booms and that natural resources could be either a blessing or a curse.

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Tables and Figures

Figure 1: Investment booms in the world

World Distribution of Episodes

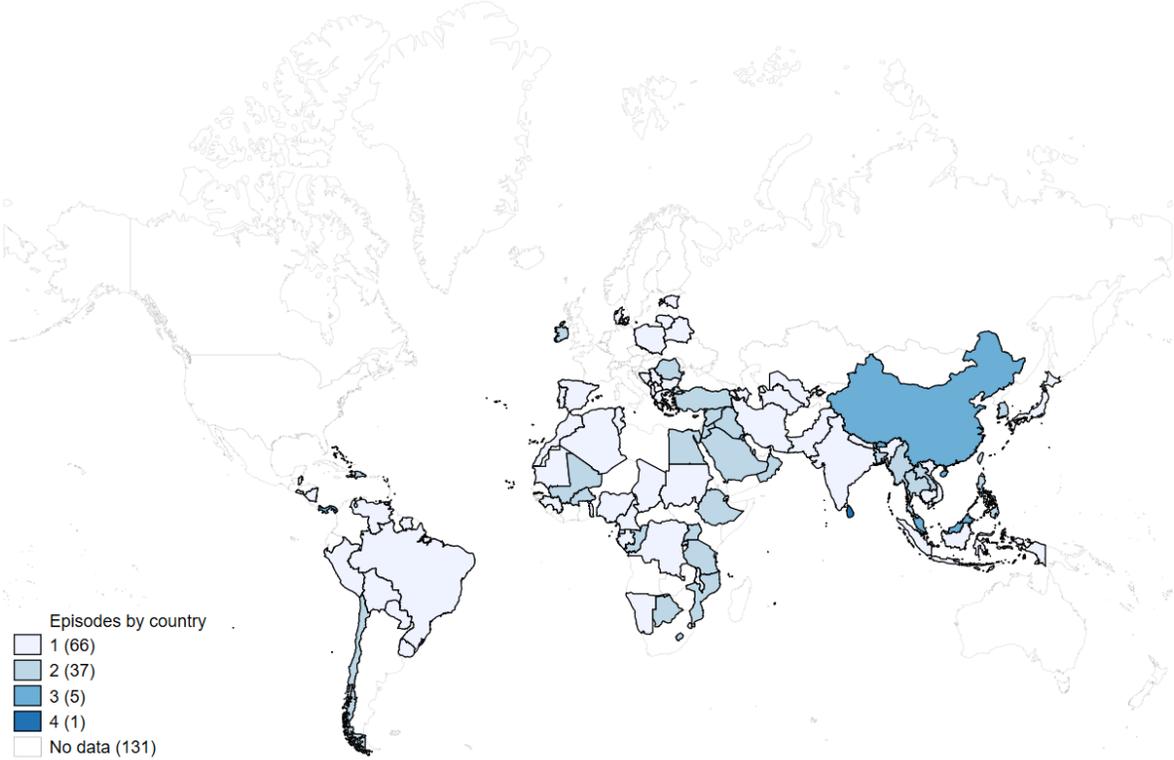


Figure 2: Institutional variables during investment booms

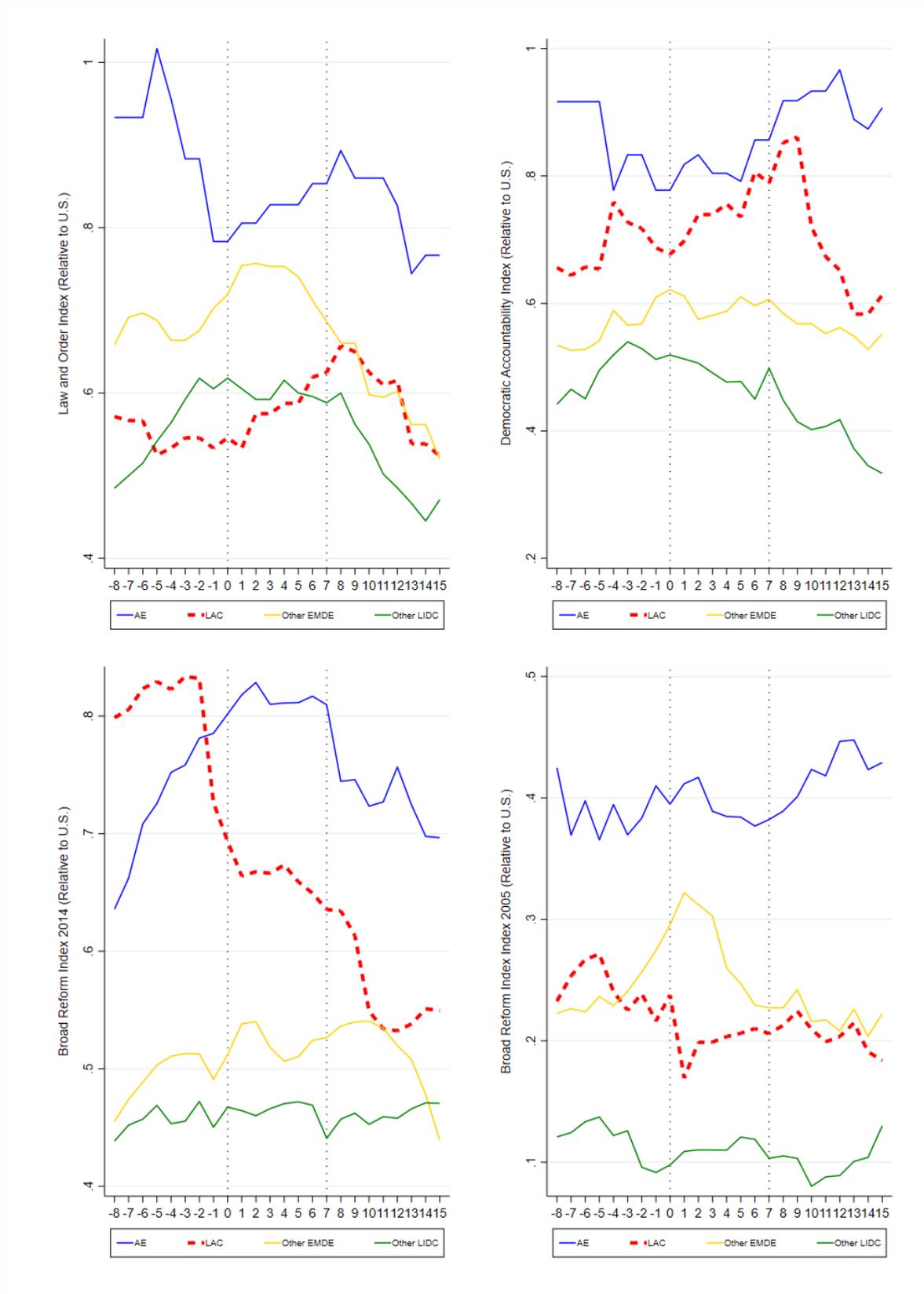


Figure 3: Institutional variables during investment booms, LAC region

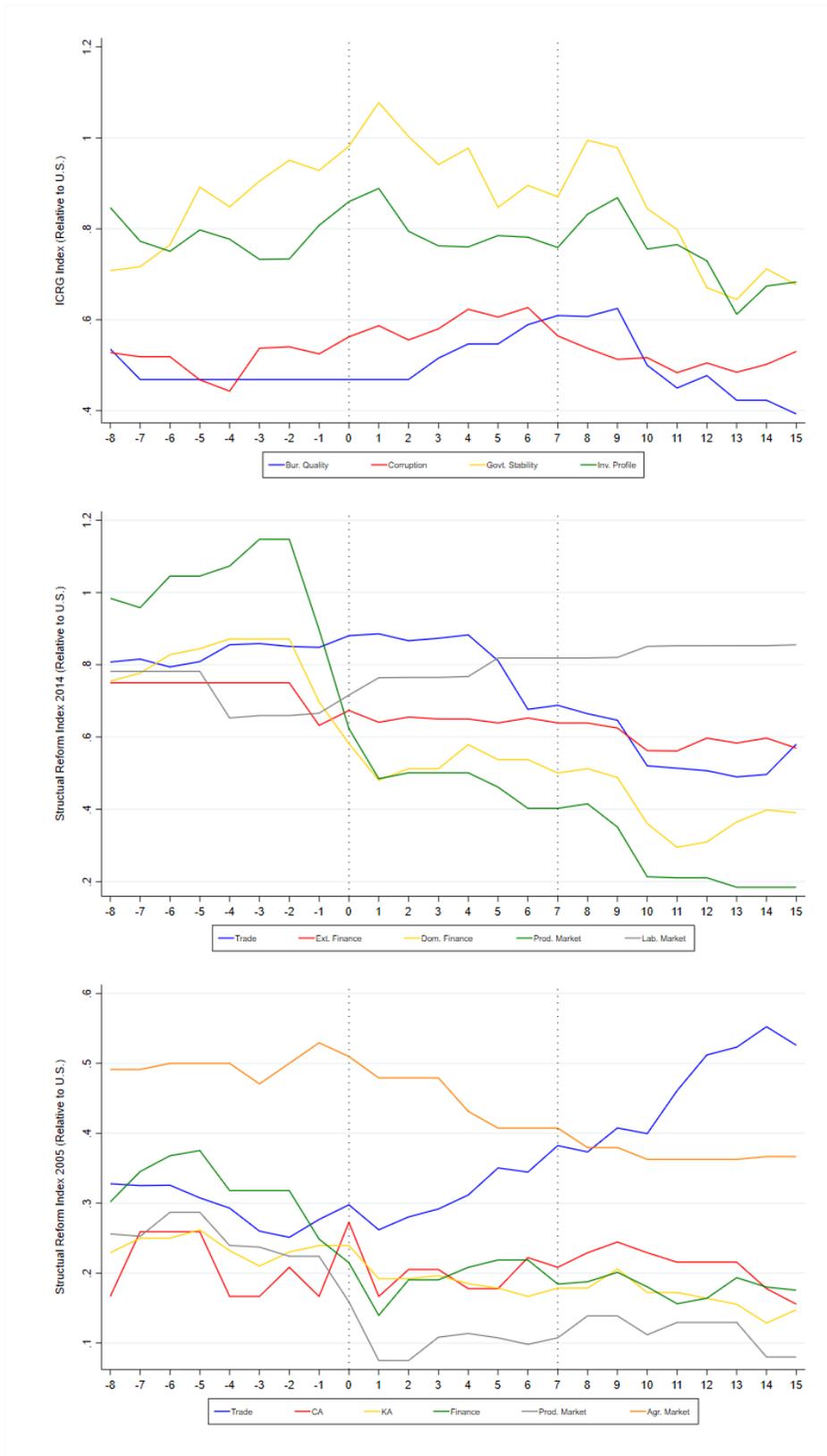


Figure 4: Internal variables during investment booms, Andean countries

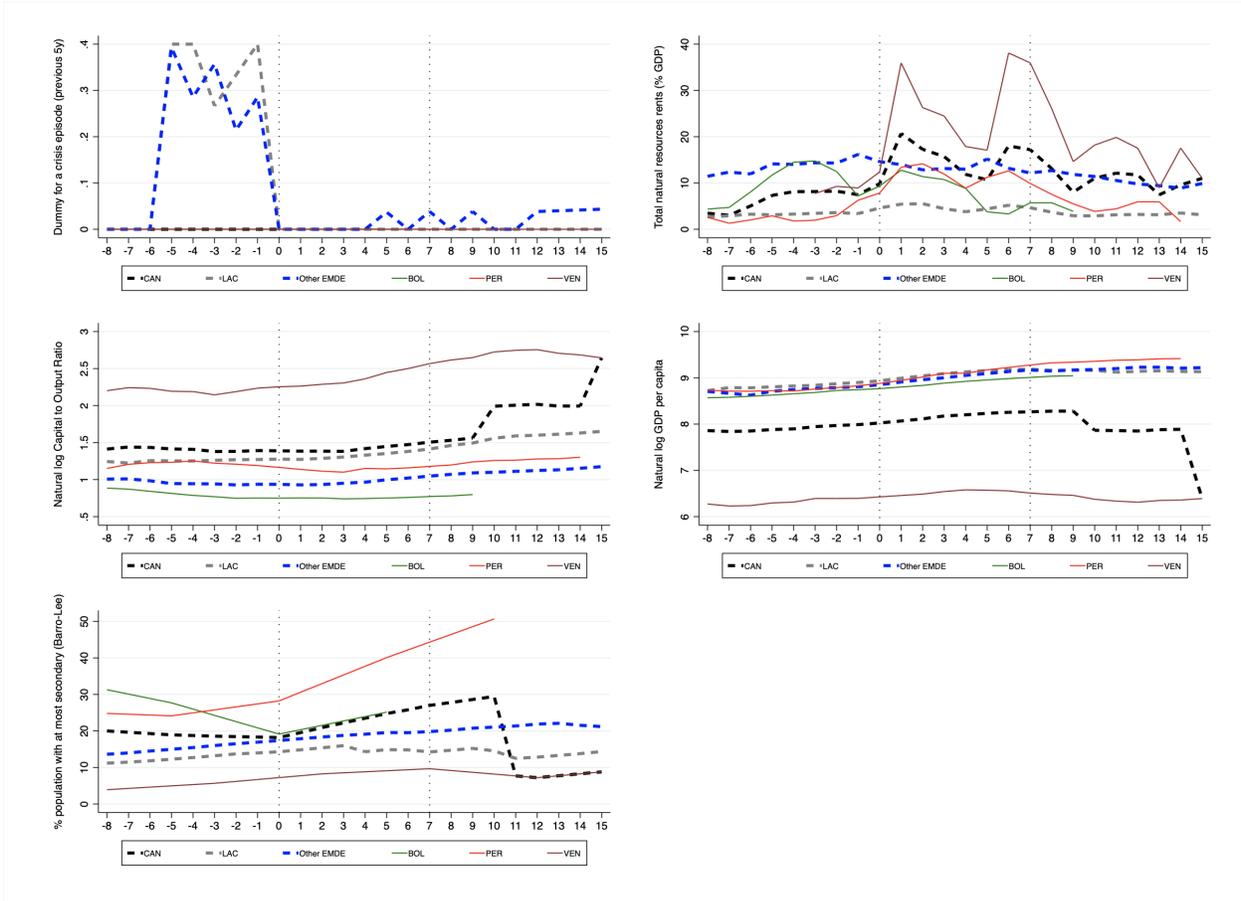


Figure 5: External variables during investment booms, Andean countries

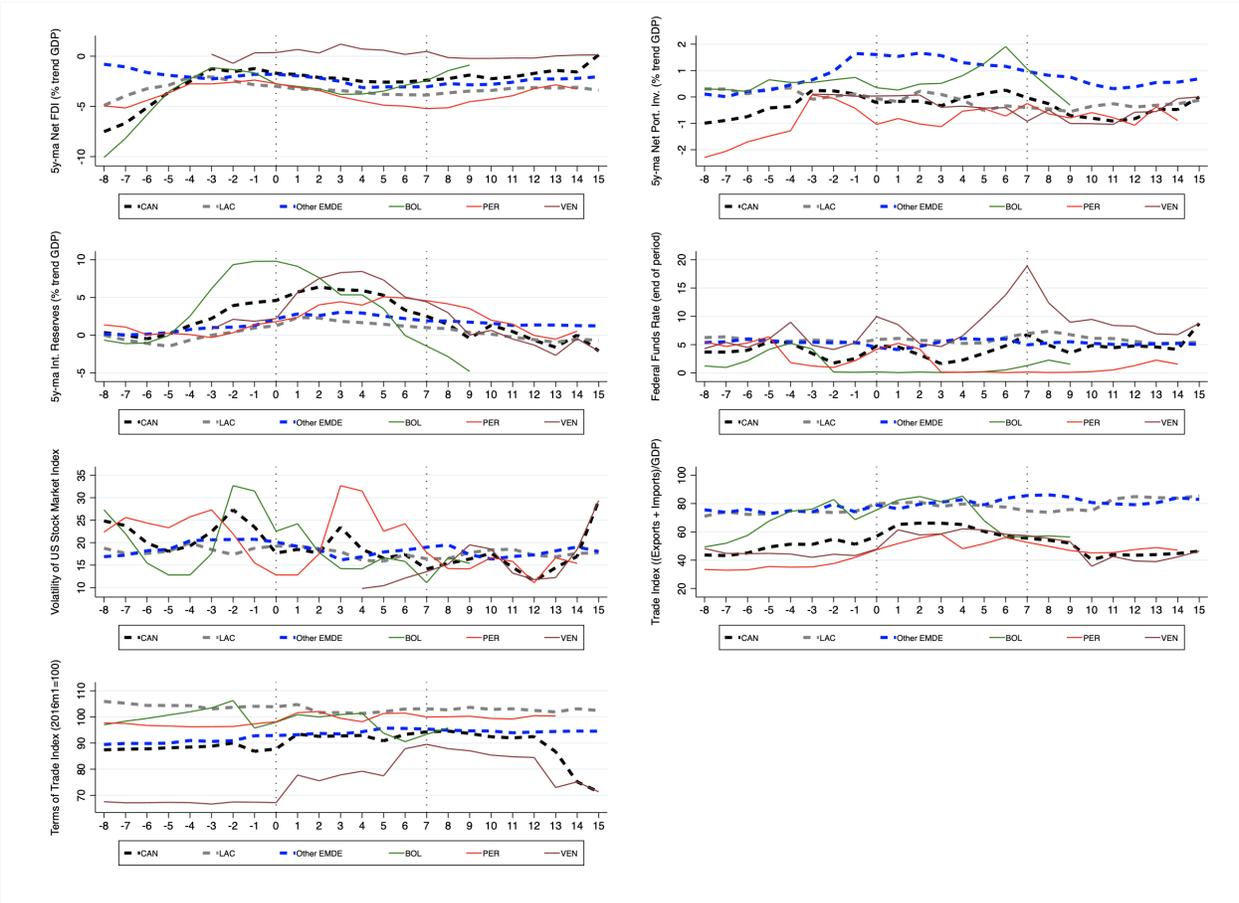


Figure 6: Policy variables during investment booms, Andean countries

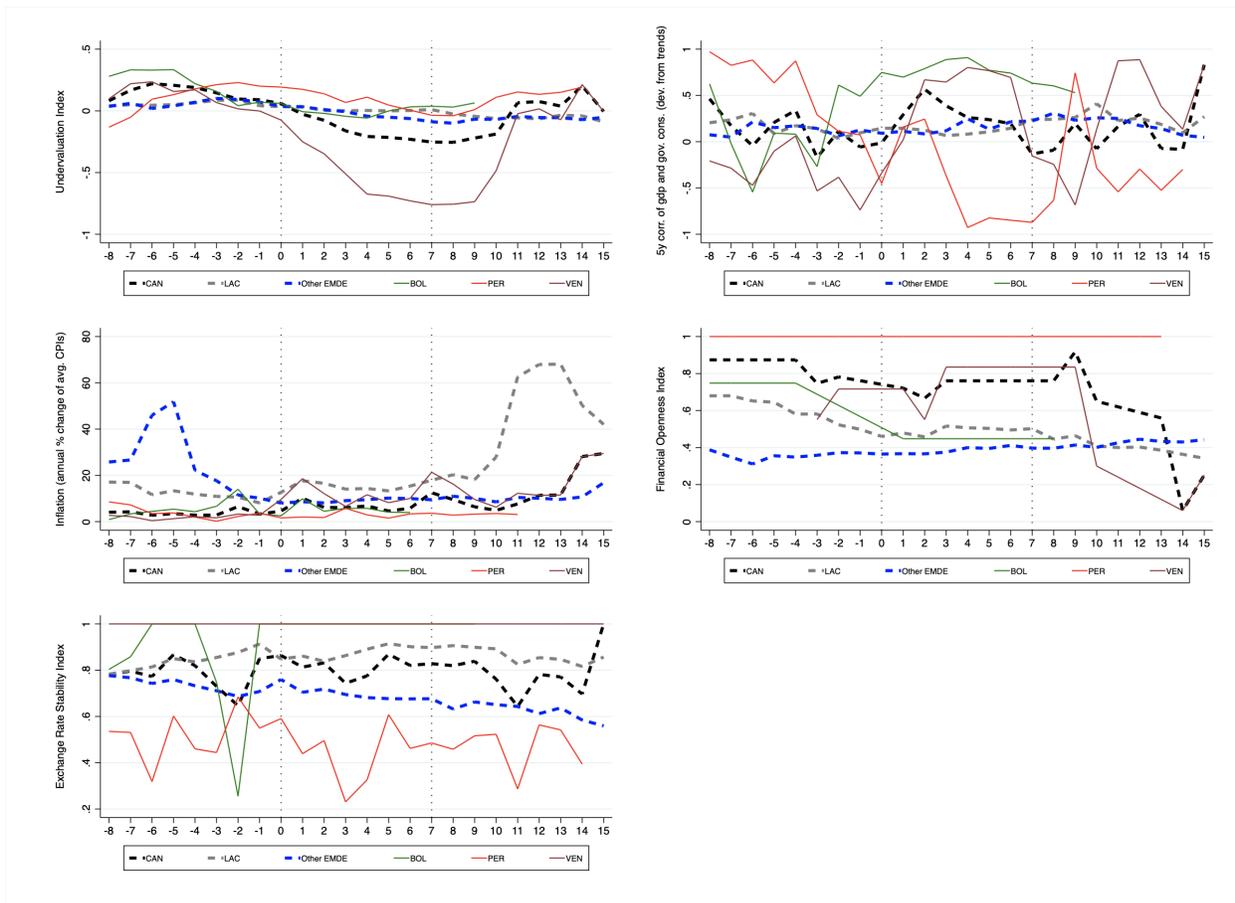


Figure 7: Institutional variables during investment booms, Andean countries

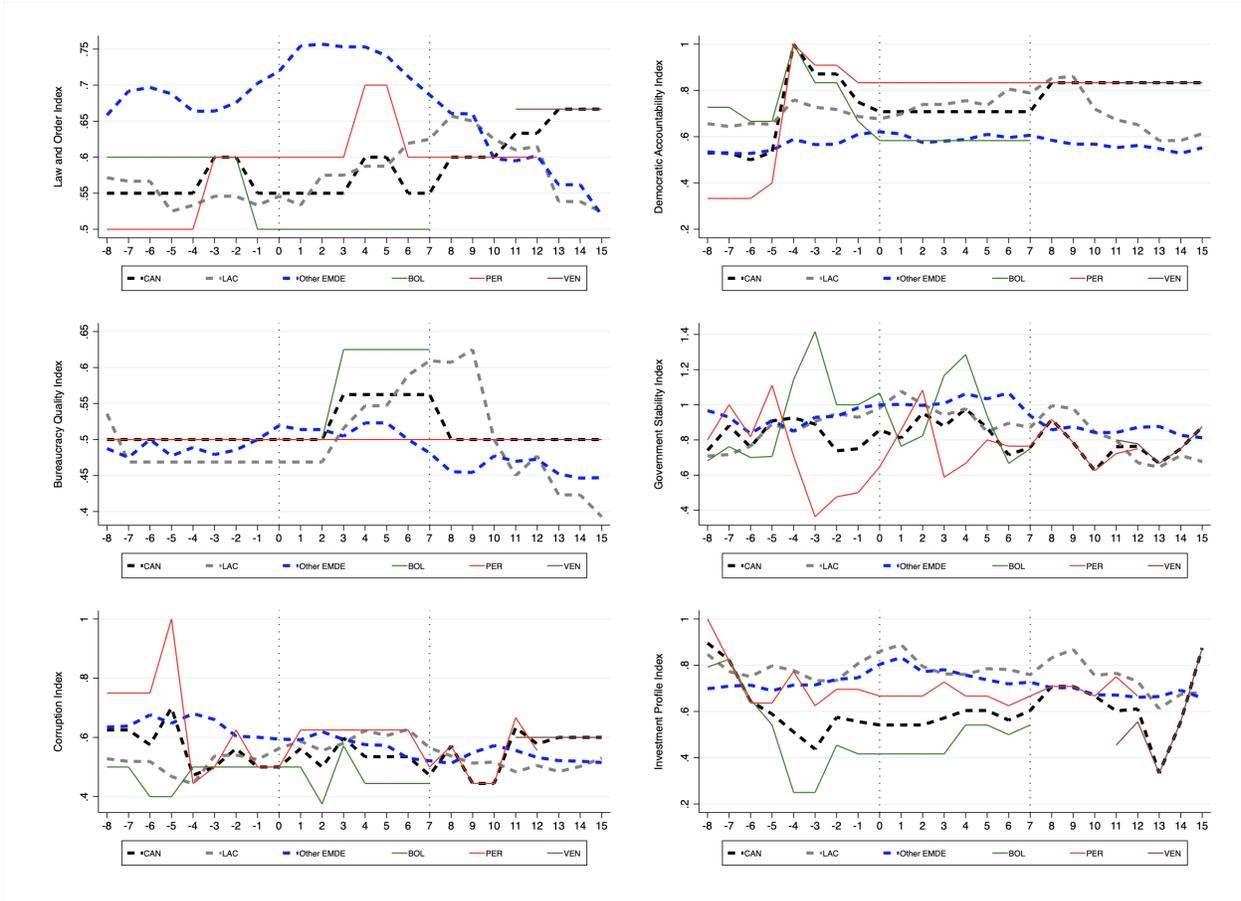


Figure 8: Institutional variables during investment booms - SR - 2014 database, Andean countries

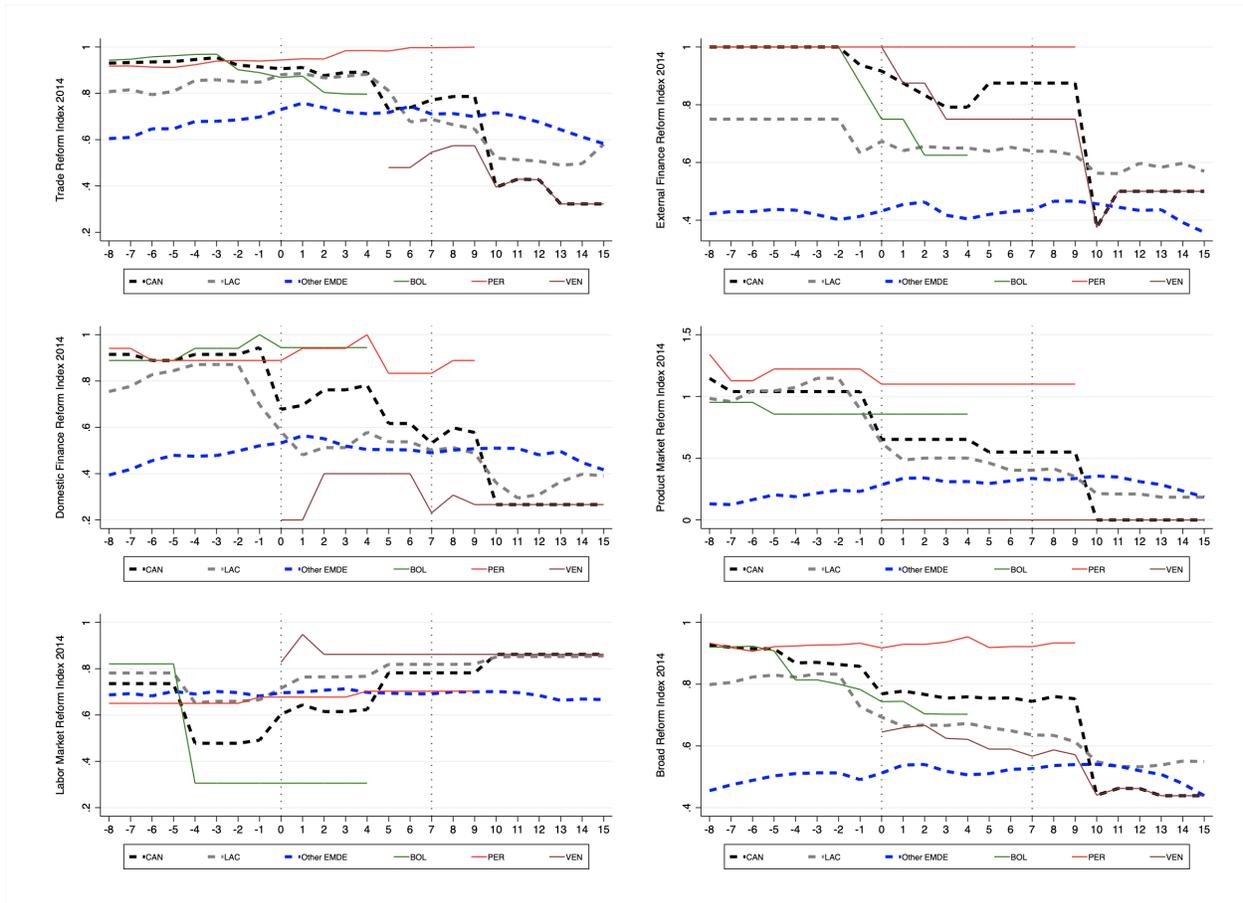


Figure 9: Institutional variables during investment booms - SR - 2005 database, Andean countries

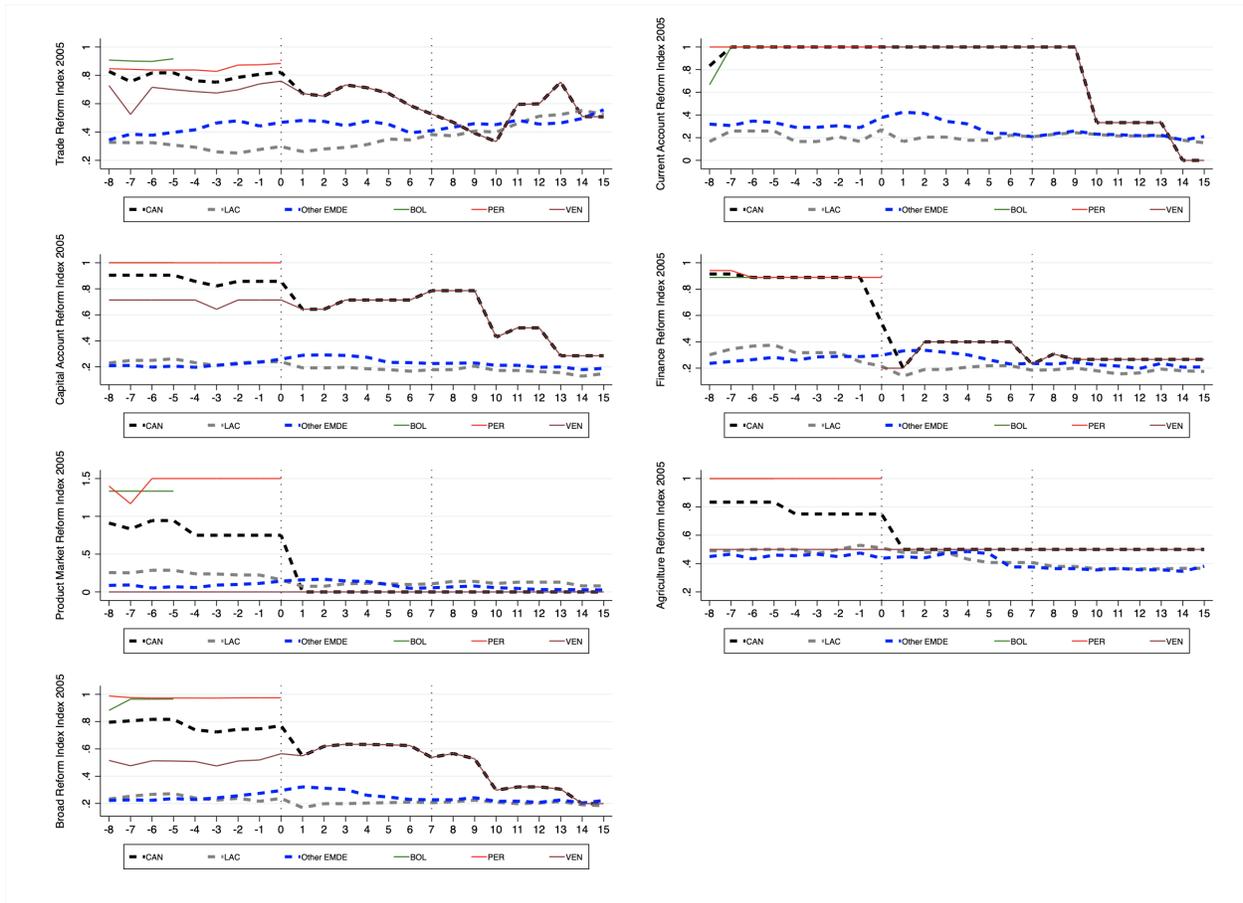


Figure 10: Internal variables during investment booms, other LAC countries

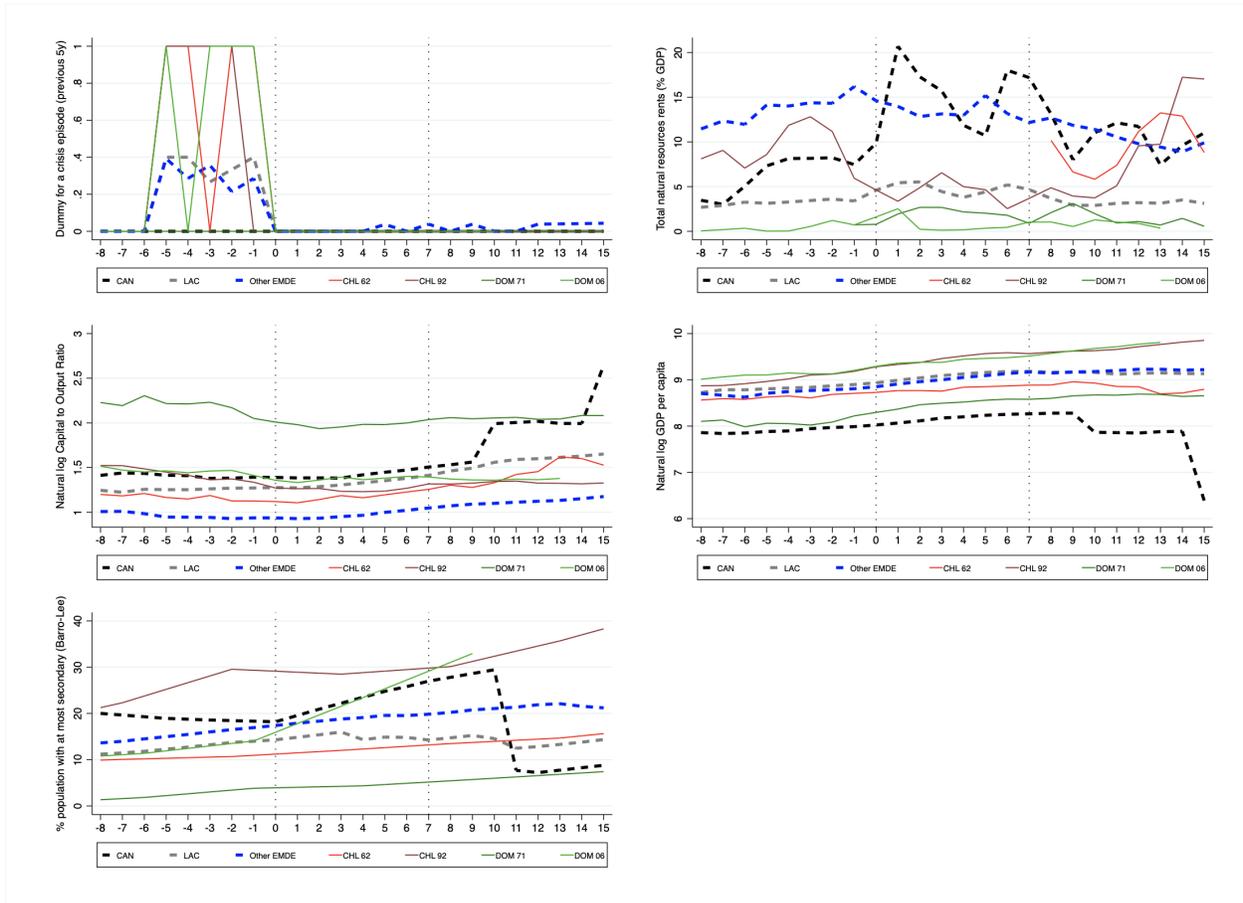


Figure 11: External variables during investment booms, other LAC countries

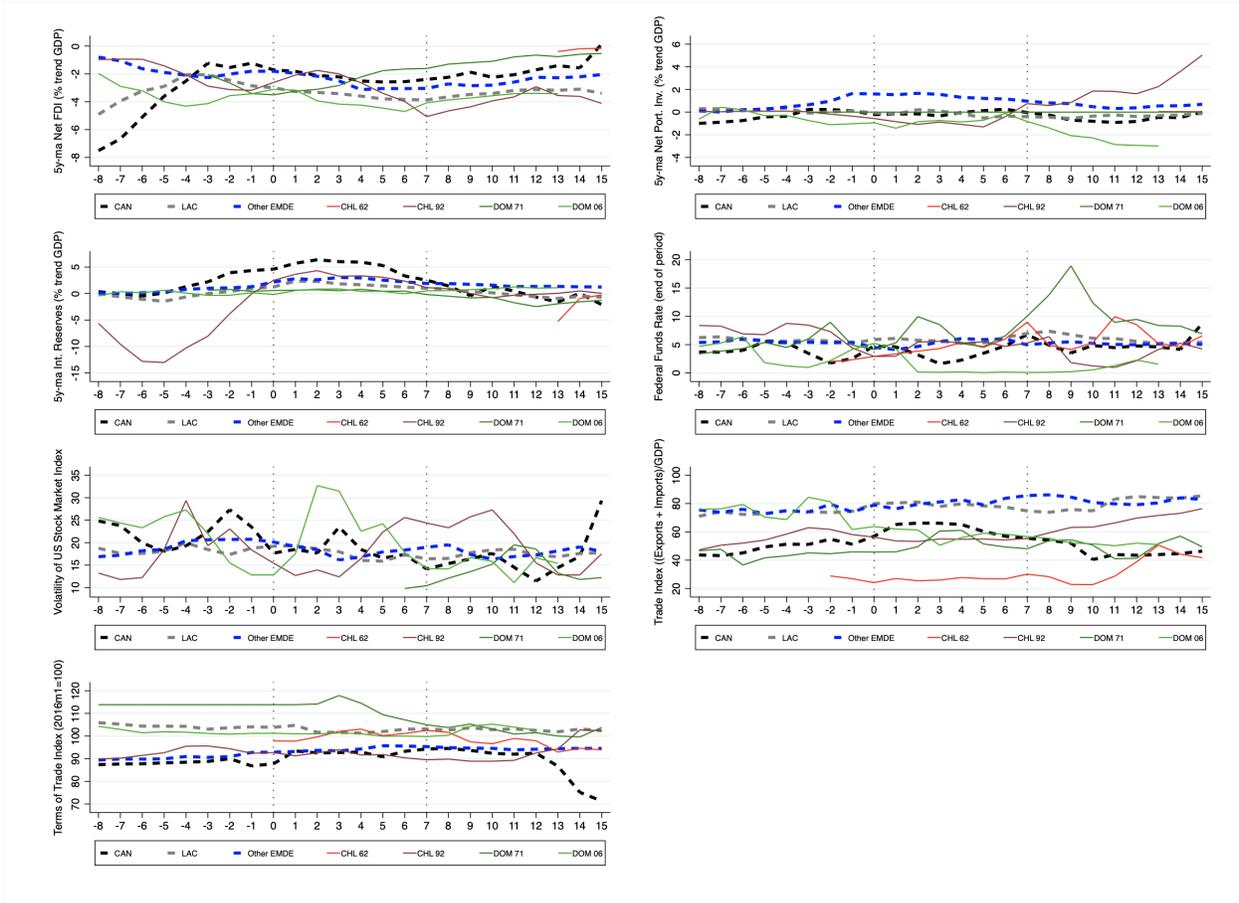


Figure 12: Policy variables during investment booms, other LAC countries

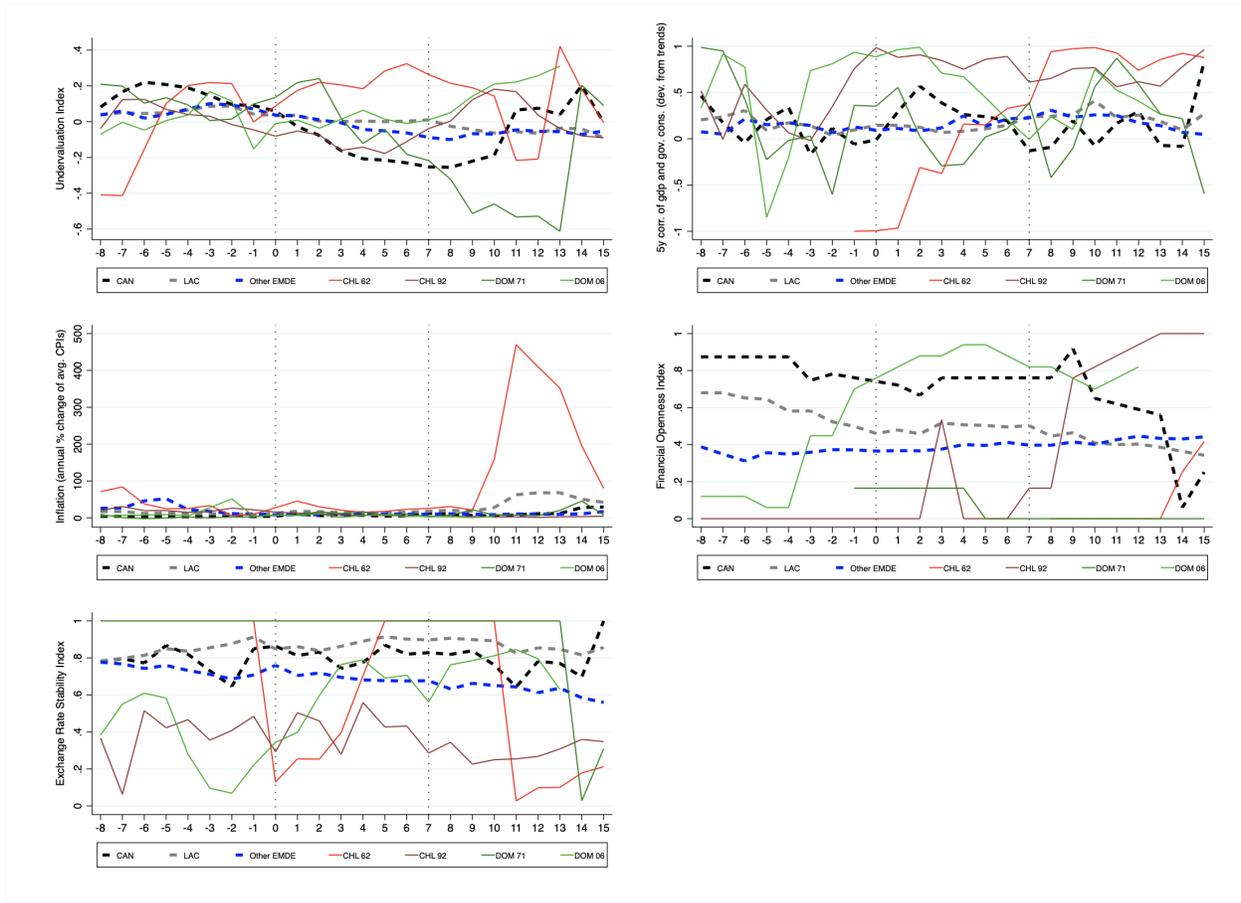


Figure 13: Institutional variables during investment booms, other LAC countries

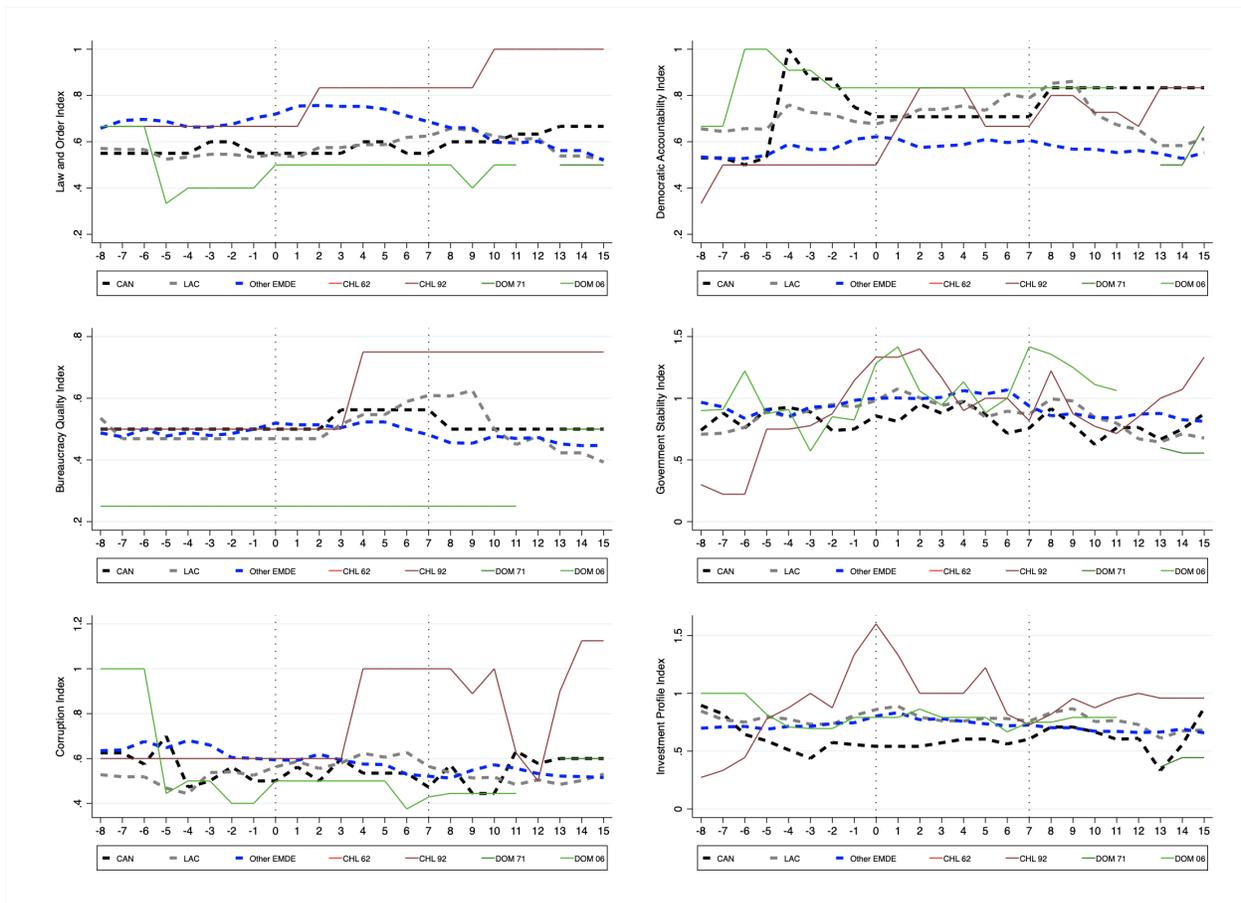


Figure 14: Institutional variables during investment booms - SR - 2014 database, other LAC countries

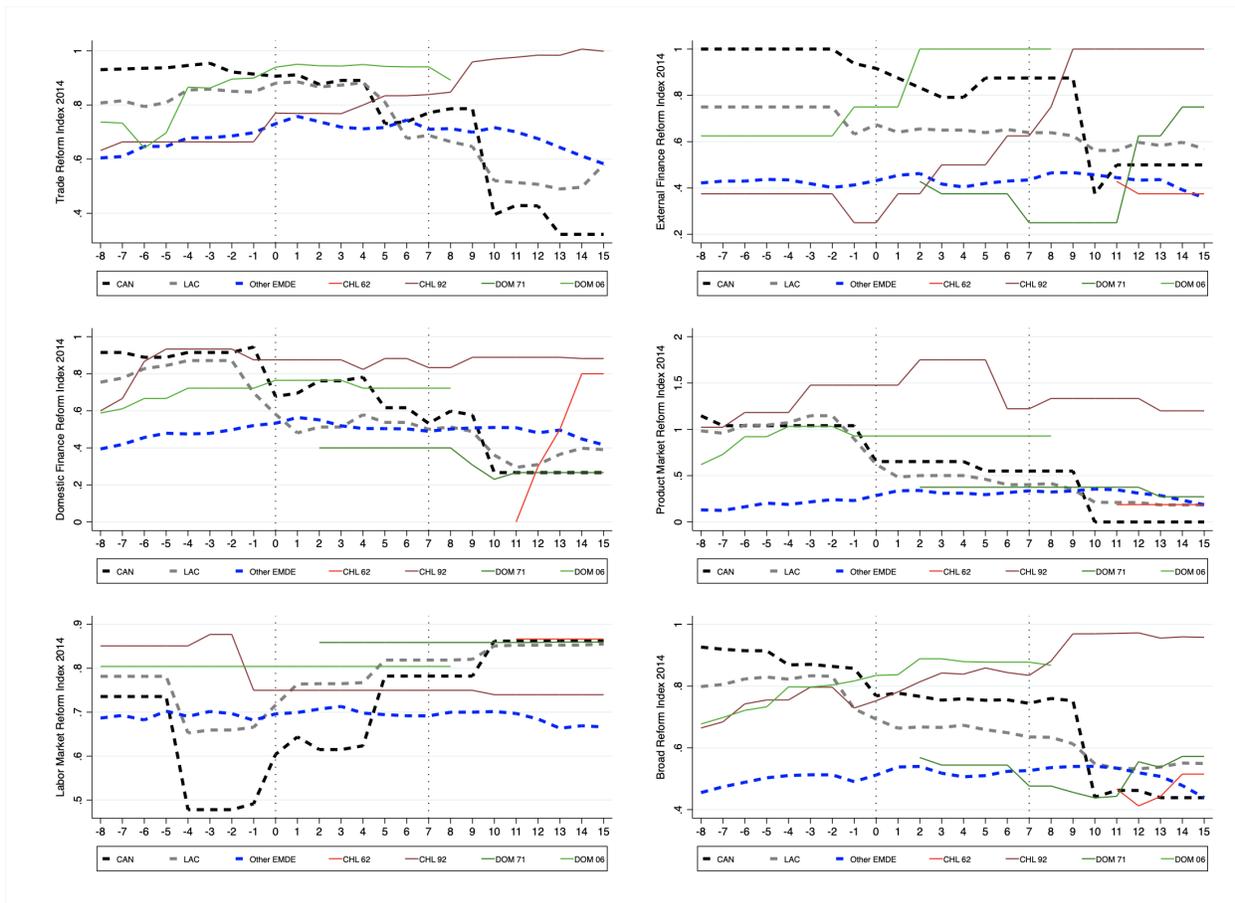


Figure 15: Institutional variables during investment booms - SR - 2005 database, other LAC countries

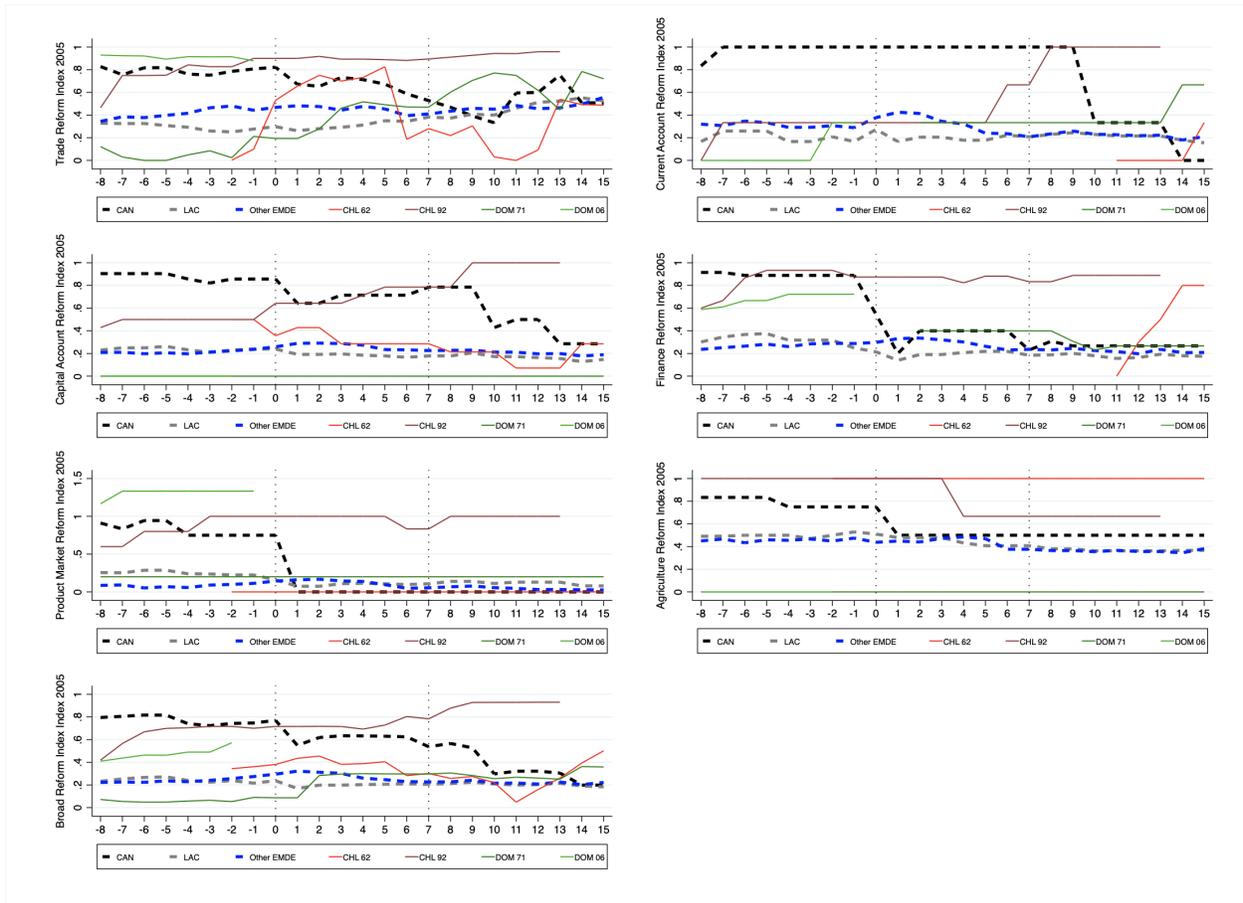


Table 1: Summary of investment boom episodes

Region	Country	Start year			
		Episode 1	Episode 2	Episode 3	Episode 4
SAS	Sri Lanka	1969	1977	1994	2002
EAP	China	1958	1974	1995	
EAP	Malaysia	1965	1992	2010	
LAC	Panama	1969	1991	2009	
MNA	Kuwait	1975	1988	2001	
SAS	Bhutan	1981	1990	2000	
AFR	Botswana	1967	1986		
AFR	Burkina Faso	1970	2005		
AFR	Cape Verde	1965	2004		
AFR	Congo, Republic of	1980	2010		
AFR	Equatorial Guinea	1971	1991		
AFR	Ethiopia	1958	2009		
AFR	Lesotho	1971	1986		
AFR	Mali	1992	2004		
AFR	Mauritius	1974	1984		
AFR	Mozambique	1968	2010		
AFR	Rwanda	1969	2005		
AFR	Tanzania	1967	2008		
AFR	Uganda	1962	1996		
EAP	Korea	1965	1985		
EAP	Laos	1988	2000		
EAP	Macao	1986	2000		
EAP	Myanmar	1978	1995		
EAP	Philippines	1976	2012		
EAP	Taiwan	1957	1988		
EAP	Thailand	1957	1990		
ECA	Romania	1975	2001		
ECA	Turkey	1983	2004		
INL	Ireland	1994	2012		
LAC	Antigua and Barbuda	1982	2002		
LAC	Bahamas	1977	1996		
LAC	Chile	1962	1992		
LAC	Dominican Republic	1971	2006		
LAC	Montserrat	1987	1997		
LAC	Trinidad & Tobago	1974	2012		
MNA	Egypt	1961	1970		
MNA	Iraq	1977	2009		
MNA	Jordan	1960	1974		
MNA	Oman	1974	2003		

Table 1: Summary of investment boom episodes

Region	Country	Start year			
		Episode 1	Episode 2	Episode 3	Episode 4
MNA	Saudi Arabia	1975	2006		
MNA	Syria	1976	2008		
SAS	Bangladesh	1977	1998		
SAS	Maldives	1974	2001		
AFR	Cameroon	1978			
AFR	Chad	2002			
AFR	Comoros	1963			
AFR	Congo, Dem. Rep.	1970			
AFR	Eswatini	1972			
AFR	Gabon	1974			
AFR	Gambia, The	1989			
AFR	Guinea	2004			
AFR	Malawi	1968			
AFR	Mauritania	1971			
AFR	Namibia	2008			
AFR	Nigeria	1973			
AFR	Sao Tome and Principe	1992			
AFR	Seychelles	1970			
AFR	Sudan	1973			
AFR	Togo	1975			
EAP	Cambodia	2000			
EAP	Hong Kong	1961			
EAP	Indonesia	1971			
EAP	Singapore	1967			
EAP	Vietnam	1989			
ECA	Albania	2001			
ECA	Armenia	2000			
ECA	Azerbaijan	2001			
ECA	Belarus	2003			
ECA	Bosnia and Herzegovina	1996			
ECA	Bulgaria	2000			
ECA	Estonia	1999			
ECA	Lithuania	2001			
ECA	Montenegro	2003			
ECA	Poland	1996			
ECA	Serbia	1994			
ECA	Turkmenistan	2007			
ECA	Uzbekistan	2008			
INL	Cyprus	1971			
INL	Denmark	1960			

Table 1: Summary of investment boom episodes

Region	Country	Start year			
		Episode 1	Episode 2	Episode 3	Episode 4
INL	Greece	1965			
INL	Japan	1963			
INL	Malta	1966			
INL	Portugal	1960			
INL	Spain	1963			
LAC	Anguilla	2002			
LAC	Barbados	1966			
LAC	Belize	1988			
LAC	Bolivia	2010			
LAC	Brazil	1972			
LAC	Dominica	1980			
LAC	El Salvador	1974			
LAC	Grenada	1979			
LAC	Haiti	1971			
LAC	Nicaragua	1963			
LAC	Paraguay	1973			
LAC	Peru	2005			
LAC	St. Kitts & Nevis	1987			
LAC	Suriname	1972			
LAC	Uruguay	1972			
LAC	Venezuela	1973			
MNA	Algeria	1971			
MNA	Djibouti	2000			
MNA	Iran	1965			
MNA	Morocco	1970			
MNA	Qatar	2012			
MNA	Tunisia	1974			
SAS	India	2003			
SAS	Nepal	1974			
SAS	Pakistan	1961			

Table 2: Unconditional probabilities by region, income, and decade

	1950s	1960s	1970s	1980s	1990s	2000s	2010s	Total
Africa	0.0074	0.0187	0.0366	0.0119	0.0097	0.0213	0.0177	0.0184
East Asia and Pacific	0.0612	0.0284	0.0272	0.0327	0.0315	0.0205	0.0400	0.0308
Eastern Europe and Central Asia	0.0000	0.0000	0.0035	0.0035	0.0142	0.0493	0.0000	0.0111
Western Europe and Other Developed	0.0000	0.0258	0.0041	0.0000	0.0039	0.0000	0.0128	0.0064
Latin America and the Caribbean	0.0000	0.0112	0.0355	0.0147	0.0118	0.0145	0.0196	0.0163
Middle East and North Africa	0.0000	0.0194	0.0781	0.0067	0.0000	0.0397	0.0238	0.0250
South Asia	0.0000	0.0313	0.0741	0.0200	0.0500	0.1026	0.0000	0.0455
1st quintile	0.0851	0.0342	0.0372	0.0127	0.0126	0.0364	0.0192	0.0274
2nd quintile	0.0000	0.0129	0.0449	0.0144	0.0204	0.0215	0.0135	0.0221
3rd quintile	0.0000	0.0462	0.0481	0.0195	0.0053	0.0260	0.0317	0.0252
4rd quintile	0.0000	0.0220	0.0206	0.0095	0.0210	0.0342	0.0075	0.0206
5th quintile	0.0000	0.0000	0.0091	0.0066	0.0054	0.0090	0.0187	0.0085
Total	0.0075	0.0162	0.0288	0.0109	0.0122	0.0240	0.0166	0.0175

Table 3: Summary statistics

	Between boom years			Within boom years		
	N	Mean	SD	N	Mean	SD
<i>Economic institutions</i>						
Law and Order Index (relative to U.S.)	870	0.61	0.25	434	0.69	0.22
Bureaucracy Quality Index (relative to U.S.)	870	0.44	0.25	434	0.49	0.23
Corruption Index (relative to U.S.)	870	0.56	0.21	434	0.57	0.20
Investment Profile Index (relative to U.S.)	870	0.70	0.21	434	0.76	0.22
<i>Political Institutions</i>						
Democratic Accountability Index (relative to U.S.)	870	0.57	0.27	434	0.61	0.26
Government Stability Index (relative to U.S.)	870	0.86	0.30	434	1.00	0.27
Democracy measure by ANRR	1857	0.37	0.48	1048	0.37	0.48
<i>First Set of Reforms</i>						
Trade Reform Index 2014 (relative to U.S.)	524	0.67	0.26	257	0.75	0.20
External Finance Reform Index 2014 (relative to U.S.)	710	0.50	0.28	400	0.50	0.29
Domestic Finance Reform Index 2014 (relative to U.S.)	697	0.50	0.31	385	0.52	0.31
Product Market Reform Index 2014 (relative to U.S.)	718	0.28	0.39	401	0.34	0.41
Labor Market Reform Index 2014 (relative to U.S.)	685	0.74	0.14	377	0.73	0.13
<i>Second Set of Reforms</i>						
Trade Reform Index 2005 (relative to U.S.)	1457	0.43	0.39	756	0.41	0.41
Current Account Reform Index 2005 (relative to U.S.)	1086	0.23	0.34	549	0.27	0.37
Capital Account Reform Index 2005 (relative to U.S.)	1443	0.18	0.28	745	0.21	0.29
Finance Reform Index 2005 (relative to U.S.)	1086	0.23	0.30	549	0.24	0.31
Product Market Reform Index 2005 (relative to U.S.)	1459	0.08	0.25	754	0.11	0.29
Agriculture Reform Index 2005 (relative to U.S.)	1454	0.33	0.49	751	0.35	0.49
<i>Control Variables</i>						
Real GDP growth (y/y % change)	2357	4.65	0.07	1272	4.67	0.07
Total natural resources rents (% GDP)	1741	8.43	12.89	991	9.87	14.49
Human Capital Index	1889	1.85	0.61	1014	1.87	0.62
Federal Funds Rate (%)	2316	5.49	3.75	1262	5.31	3.94
Volatility of U.S. Stock Market Index	1679	18.19	5.76	865	18.05	6.20
Trade Openness Index	1795	76.24	48.23	981	78.77	45.53
Terms of Trade Index	2038	97.68	16.96	1144	98.52	15.02
Undervaluation Index	2381	0.00	0.26	1272	0.01	0.25
FDI (% Trend GDP)	1292	-2.35	3.95	711	-3.24	5.57
Fiscal Procyclicality Index	1297	0.15	0.58	705	0.12	0.56

Table 4: Pooled probit

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)
Law and order	0.003 (0.002)	0.000 (0.003)	-0.007** (0.004)	-0.000 (0.003)
Democratic accountability	-0.005** (0.002)	-0.006** (0.003)	0.005 (0.004)	0.003 (0.004)
Federal funds rate		-0.003* (0.002)	-0.005** (0.002)	-0.004** (0.002)
Volatility of U.S. stock market		0.000 (0.001)	-0.002** (0.001)	-0.002** (0.001)
Terms of Trade Index		-0.000 (0.000)	0.001* (0.001)	-0.000 (0.001)
Trade openness		0.000 (0.000)	0.000* (0.000)	0.000** (0.000)
Financial openness		-0.018 (0.014)	-0.041** (0.018)	-0.035** (0.017)
Net FDI (% trend GDP)		-0.000 (0.001)	-0.003 (0.002)	-0.001 (0.002)
Net port. inv. (% trend GDP)		-0.000 (0.001)	-0.002** (0.001)	-0.003*** (0.001)
Int. reserves flows (% trend GDP)		0.004*** (0.001)	0.003* (0.002)	0.005** (0.002)
Fiscal procyclicality			0.017* (0.009)	0.024*** (0.008)
Inflation (y/y avg.)			-0.001 (0.000)	-0.001* (0.000)
Undervaluation Index			0.014 (0.020)	-0.005 (0.022)
Exchange Rate Stability Index			0.017 (0.013)	0.019 (0.014)
Natural log capital to output ratio				-0.044*** (0.010)
Natural log GDP per capita				-0.026*** (0.007)
Total natural resources rents (% of GDP)				-0.002* (0.001)
Percentage of population with secondary (Barro-Lee)				0.002*** (0.000)
Dummy for a crisis episode (previous 5 years)				0.052** (0.024)
Observations	3449	2620	1434	1395
pseudo R-sq	0.004	0.031	0.104	0.207
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)				

Table 5: Pooled probit - Structural reforms, 2014

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)
Trade Reform Index 2014	0.047 (0.033)	0.042 (0.035)	0.014 (0.038)	0.011 (0.041)
External Finance Reform Index 2014	-0.111*** (0.024)	-0.102*** (0.024)	-0.095*** (0.028)	-0.076*** (0.028)
Domestic Finance Reform Index 2014	0.035 (0.026)	0.044 (0.028)	0.050 (0.031)	0.086** (0.035)
Product Market Reform Index 2014	0.002 (0.017)	-0.027 (0.018)	0.001 (0.019)	-0.010 (0.018)
Labor Market Reform Index 2014	-0.023 (0.026)	-0.009 (0.026)	0.008 (0.027)	0.051 (0.031)
Federal funds rate		-0.006*** (0.002)	-0.006*** (0.002)	-0.005** (0.002)
Volatility of US stock market		-0.001 (0.001)	-0.003*** (0.001)	-0.003*** (0.001)
Terms of trade		0.001 (0.001)	0.002** (0.001)	0.001 (0.001)
Fiscal procyclicality			0.011 (0.007)	0.013* (0.008)
Inflation (y/y avg.)			-0.000 (0.000)	-0.000 (0.000)
Undervaluation Index			0.012 (0.023)	-0.002 (0.024)
Exchange Rate Stability Index			-0.001 (0.015)	0.005 (0.016)
Natural log capital to output ratio				-0.038*** (0.012)
Natural log GDP per capita				-0.021*** (0.007)
Total natural resources rents (% of GDP)				-0.000 (0.001)
Percentage of pop. with at most secondary educ. (Barro-Lee)				0.001*** (0.000)
Dummy for a crisis episode (previous 5 years)				0.029 (0.022)
Observations	2485	2377	1641	1616
pseudo R-sq	0.040	0.070	0.113	0.190
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)				

Table 6: Pooled probit - Structural reforms, 2005

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)
Trade Reform Index 2005	-0.020** (0.009)	-0.047*** (0.012)	-0.081*** (0.025)	-0.076** (0.031)
Current Account Reform Index 2005	0.037** (0.018)	0.039** (0.018)	0.007 (0.019)	0.016 (0.020)
Capital Account Reform Index 2005	-0.014 (0.011)	-0.033*** (0.014)	-0.030* (0.016)	-0.044*** (0.016)
Finance Reform Index 2005	-0.045* (0.024)	-0.014 (0.024)	0.018 (0.027)	0.012 (0.028)
Product Market Reform Index 2005	-0.012 (0.023)	-0.021 (0.021)	-0.015 (0.022)	-0.018 (0.022)
Agriculture Reform Index 2005	0.010 (0.008)	0.005 (0.010)	0.009 (0.012)	0.019 (0.012)
Federal funds rate		-0.004*** (0.001)	-0.007*** (0.002)	-0.006*** (0.002)
Volatility of US stock market		0.001 (0.001)	-0.002** (0.001)	-0.002** (0.001)
Terms of trade		0.000 (0.000)	0.001 (0.001)	-0.000 (0.001)
Fiscal procyclicality			-0.004 (0.008)	0.004 (0.008)
Inflation (y/y avg.)			-0.001 (0.000)	-0.001 (0.000)
Undervaluation Index			0.035 (0.024)	-0.003 (0.026)
Exchange Rate Stability Index			-0.016 (0.016)	-0.018 (0.016)
Natural log capital to output ratio				-0.025** (0.011)
Natural log GDP per capita				-0.024*** (0.007)
Total natural resources rents (% of GDP)				-0.001 (0.001)
Percentage of population with at most secondary (Barro-Lee)				0.002*** (0.000)
Dummy for a crisis episode (previous 5 years)				0.015 (0.019)
Observations	5835	3381	1456	1430
pseudo R-sq	0.009	0.036	0.130	0.278
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)				

Table 7: Probit with fixed effects

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)
Law and order	0.016 (0.016)	0.027 (0.026)	0.045 (0.039)	0.531*** (0.088)
Democratic accountability	0.018 (0.012)	-0.015 (0.016)	-0.031 (0.023)	-0.168*** (0.049)
Federal funds rate		-0.121 (0.950)	-1.249 (1.046)	-2.631*** (0.678)
Volatility of US stock market		0.016 (0.021)	-0.000 (0.024)	-0.037*** (0.009)
Terms of trade		-0.001 (0.003)	-0.006 (0.008)	-0.037*** (0.012)
Trade openness		-0.001 (0.001)	-0.005*** (0.002)	-0.016*** (0.004)
Financial openness		-0.206*** (0.078)	-0.318*** (0.112)	-0.892*** (0.151)
Net FDI (% trend GDP)		-0.014** (0.006)	-0.011 (0.010)	-0.042*** (0.011)
Net port. inv. (% trend GDP)		0.003 (0.007)	-0.008 (0.010)	0.025* (0.014)
Int. reserves (% trend GDP)		0.030*** (0.006)	0.044*** (0.010)	0.008 (0.011)
Fiscal procyclicality			0.102** (0.043)	0.208*** (0.053)
Inflation (y/y avg.)			-0.007*** (0.002)	-0.011*** (0.002)
Undervaluation Index			0.330* (0.184)	0.021 (0.173)
Exchange Rate Stability Index			0.129 (0.120)	0.269 (0.202)
Natural log capital to output ratio				-7.489*** (1.308)
Natural log GDP per capita				-5.242*** (0.824)
Total natural resources rents (% of GDP)				0.023** (0.010)
Percentage of population with at most secondary (Barro-Lee)				0.074*** (0.017)
Dummy for a crisis episode (previous 5 years)				0.293*** (0.090)
Country FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	1018	636	267	267
pseudo R-sq	0.758	0.184	0.273	0.758
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)				

Table 8: Probit with fixed effects - Structural reforms, 2014

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)
Trade reform Index 2014	0.066 (0.181)	-0.290 (0.273)	-0.785** (0.391)	-0.959*** (0.317)
External Finance Reform Index 2014	-0.170 (0.117)	-0.135 (0.148)	0.012 (0.208)	0.206 (0.164)
Domestic Finance Reform Index 2014	0.424** (0.177)	0.434** (0.221)	1.494*** (0.376)	0.960*** (0.283)
Product Market Reform Index 2014	-0.130 (0.127)	-0.199 (0.158)	0.036 (0.270)	-0.169 (0.131)
Labor Market Reform Index 2014	-0.638*** (0.243)	-0.462 (0.290)	-1.188** (0.532)	-1.466 (0.981)
Federal funds rate		0.009 (1.173)	-1.548 (1.483)	-0.027*** (0.010)
Volatility of US stock market		0.021 (0.023)	0.002 (0.028)	-0.011*** (0.003)
Terms of trade		-0.018** (0.009)	-0.002 (0.011)	-0.003 (0.008)
Fiscal procyclicality			-0.002 (0.049)	0.022 (0.036)
Inflation (y/y avg.)			-0.006** (0.003)	-0.004 (0.002)
Undervaluation Index			0.353* (0.208)	0.339*** (0.127)
Exchange Rate Stability Index			0.176 (0.135)	0.016 (0.102)
Natural log capital to output ratio				-1.139*** (0.240)
Natural log GDP per capita				-0.532*** (0.127)
Total natural resources rents (% of GDP)				-0.007 (0.007)
Percentage of population with at most secondary (Barro-Lee)				0.020*** (0.007)
Dummy for a crisis episode (previous 5 years)				-0.118 (0.080)
Country FE	Y	Y	Y	Y
Year FE	Y	Y	Y	N
Observations	630	490	250	334
pseudo R-sq	0.125	0.093	0.233	0.404
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)				

Table 9: Probit with fixed effects - Structural reforms, 2005

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)
Trade Reform Index 2005	-0.003 (0.029)	0.101 (0.069)	0.248 (0.278)	-0.682*** (0.185)
Current Account Reform Index 2005	0.112*** (0.039)	0.111 (0.088)	0.299* (0.169)	0.070 (0.140)
Capital Account Reform Index 2005	0.080* (0.041)	0.013 (0.139)	-0.297 (0.264)	-0.174 (0.226)
Finance Reform Index 2005	-0.258*** (0.058)	0.201 (0.137)	1.006*** (0.320)	1.489*** (0.329)
Product Market Reform Index 2005	-0.027 (0.058)	0.001 (0.132)	0.044 (0.261)	-0.278* (0.143)
Agriculture Reform Index 2005	0.120*** (0.032)	0.132 (0.088)	0.495** (0.244)	-0.085 (0.222)
Federal funds rate		-0.077 (0.081)	-0.034 (0.114)	-0.015 (0.011)
Volatility of US stock market		-0.016 (0.022)	-0.002 (0.036)	-0.002 (0.004)
Terms of trade		0.007** (0.003)	-0.013 (0.012)	-0.003 (0.008)
Fiscal procyclicality			-0.104** (0.052)	-0.075* (0.041)
Inflation (y/y avg.)			-0.013*** (0.004)	-0.008** (0.003)
Undervaluation Index			0.187 (0.291)	-0.013 (0.162)
Exchange Rate Stability Index			0.010 (0.139)	-0.224* (0.130)
Natural log capital to output ratio				-2.313*** (0.402)
Natural log GDP per capita				-1.016*** (0.178)
Total natural resources rents (% of GDP)				0.000 (0.009)
Percentage of population with at most secondary (Barro-Lee)				0.042*** (0.011)
Dummy for a crisis episode (previous 5 years)				-0.123* (0.065)
Country FE	Y	Y	Y	Y
Year FE	Y	Y	Y	N
Observations	2945	871	209	276
pseudo R-sq	0.110	0.186	0.396	0.556

Table 10: Probit with fixed effects - Sample selection

Dependent variable: Start of episode dummy	(1)	(2)	(3)
Law and order	0.027 (0.017)	0.797*** (0.244)	0.129*** (0.048)
Democratic accountability	0.034*** (0.012)	-0.244** (0.117)	0.039 (0.031)
Federal funds rate		0.279** (0.111)	
Volatility of US stock market		-0.011** (0.005)	
Terms of trade		-0.058 (0.404)	
Trade openness		0.899* (0.531)	
Financial openness		-3.029 (2.848)	
Net FDI (% trend GDP)		0.003 (0.034)	
Net port. inv. (% trend GDP)		-0.088** (0.037)	
Int. reserves (% trend GDP)		-0.027*** (0.010)	
Fiscal procyclicality		-0.963*** (0.361)	
Inflation (y/y avg.)		-0.023 (0.026)	
Undervaluation Index		0.063 (0.051)	
Exchange Rate Stability Index		0.022 (0.023)	
Natural log capital to output ratio		-7.661*** (2.680)	
Natural log GDP per capita		-5.123*** (1.603)	
Total natural resources rents (% of GDP)		0.054** (0.023)	
Percentage of population with at most secondary (Barro-Lee)		0.114*** (0.042)	
Dummy for a crisis episode (previous 5 years)		0.493** (0.207)	
Country FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	999	210	210
pseudo R-sq	0.110	0.728	0.142
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)			

Table 11: Probit with fixed effects - Sample selection - SR 2014

Dependent variable: Start of episode dummy	(1)	(2)	(3)
Trade Reform Index 2014	0.073 (0.209)	-0.820** (0.335)	-0.328 (0.266)
External Finance Reform Index 2014	-0.237* (0.125)	0.187 (0.182)	0.032 (0.138)
Domestic Finance Reform Index 2014	0.106 (0.208)	0.826** (0.324)	0.375 (0.248)
Product Market Reform Index 2014	0.057 (0.145)	-0.020 (0.195)	0.089 (0.145)
Labor Market Reform Index 2014	-0.750*** (0.273)	-1.517 (1.328)	-1.947* (1.007)
Federal funds rate		-0.023** (0.011)	
Volatility of US stock market		-0.007** (0.004)	
Terms of trade		-0.014 (0.010)	
Fiscal procyclicality		-0.040 (0.041)	
Inflation (y/y avg.)		-0.003 (0.003)	
Undervaluation Index		0.216 (0.157)	
Exchange Rate Stability Index		0.001 (0.123)	
Natural log capital to output ratio		-1.785*** (0.374)	
Natural log GDP per capita		-0.786*** (0.164)	
Total natural resources rents (% of GDP)		-0.008 (0.008)	
Percentage of population with at most secondary (Barro-Lee)		0.022** (0.009)	
Dummy for a crisis episode (previous 5 years)		-0.140* (0.084)	
Country FE	Y	Y	Y
Year FE	Y	N	N
Observations	532	280	280
pseudo R-sq	0.157	0.428	0.15

Table 12: Probit with fixed effects - Sample selection - SR 2005

Dependent variable: Start of episode dummy	(1)	(2)	(3)
Trade Reform Index 2005	-0.006 (0.031)	-0.729*** (0.222)	-0.336** (0.166)
Current Account Reform Index 2005	0.127*** (0.040)	-0.023 (0.170)	0.208 (0.154)
Capital Account Reform Index 2005	0.173*** (0.047)	-0.102 (0.227)	-0.372 (0.251)
Finance Reform Index 2005	-0.291*** (0.063)	1.502*** (0.382)	0.412* (0.240)
Product Market Reform Index 2005	-0.014 (0.066)	0.218 (0.230)	0.307 (0.192)
Agriculture Reform Index 2005	0.090*** (0.033)	0.005 (0.227)	0.455** (0.227)
Federal funds rate		-0.025** (0.013)	
Volatility of US stock market		0.004 (0.005)	
Terms of trade		-0.017* (0.010)	
Fiscal procyclicality		-0.151*** (0.055)	
Inflation (y/y avg.)		-0.008** (0.003)	
Undervaluation Index		-0.161 (0.193)	
Exchange Rate Stability Index		-0.077 (0.156)	
Natural log capital to output ratio		-3.170*** (0.589)	
Natural log GDP per capita		-1.021*** (0.215)	
Total natural resources rents (% of GDP)		-0.001 (0.009)	
Percentage of population with at most secondary (Barro-Lee)		0.026** (0.013)	
Dummy for a crisis episode (previous 5 years)		-0.087 (0.068)	
Country FE	Y	Y	Y
Year FE	Y	N	N
Observations	2595	234	234
pseudo R-sq	0.132	0.588	0.200
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)			

Table 13: Instrumental variables - Second stage

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)
Law and order	0.025** (0.010)	-0.008 (0.015)	0.001 (0.005)	0.010 (0.011)
Democratic accountability	0.007 (0.005)	0.008 (0.009)	0.036*** (0.012)	0.044 (0.083)
Federal funds rate		-0.010*** (0.003)		-0.011*** (0.003)
Volatility of US stock market		-0.001 (0.001)		-0.001 (0.001)
Terms of trade		-0.002 (0.002)		-0.002 (0.002)
Trade openness		-0.000 (0.001)		-0.000 (0.001)
Financial openness		-0.042 (0.035)		-0.068 (0.059)
Net FDI (% trend GDP)		-0.002 (0.005)		0.002 (0.009)
Net port. inv. (% trend GDP)		0.001 (0.001)		0.002 (0.003)
Int. reserves (% trend GDP)		0.003** (0.001)		0.002 (0.001)
Fiscal procyclicality		0.011 (0.011)		0.018 (0.015)
Inflation (y/y avg.)		0.000 (0.000)		0.000 (0.000)
Undervaluation Index		-0.009 (0.023)		-0.003 (0.024)
Exchange Rate Stability Index		0.010 (0.020)		0.011 (0.020)
Natural log capital to output ratio		-0.346*** (0.084)		-0.375*** (0.096)
Natural log GDP per capita		-0.166** (0.067)		-0.224** (0.110)
Total natural resources rents (% of GDP)		-0.002 (0.003)		-0.003 (0.006)
Percentage of population with at most secondary (Barro-Lee)		0.002* (0.001)		0.002** (0.001)
Dummy for a crisis episode (previous 5 years)		-0.119* (0.069)		-0.107 (0.070)
Country FE	Y	Y	Y	Y
Year FE	N	N	N	N
Observations	3426	1395	3426	1395
First stage F-stat	273.422	271.284	171.978	107.790
p-value of F-test	0.000	0.000	0.000	0.000
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)				

Table 14: Instrumental variables - First stage

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)
Law and Order Index neighbors	0.871*** (0.041)	1.049*** (0.082)		
Democratic accountability	0.105*** (0.014)	0.064** (0.028)		
Democratic Accountability Index neighbors			0.781*** (0.040)	0.392*** (0.139)
Law and order			0.195*** (0.021)	0.090*** (0.033)
Federal funds rate		0.036*** (0.010)		0.019* (0.011)
Volatility of US stock market		-0.009*** (0.003)		-0.001 (0.003)
Terms of trade		0.006 (0.007)		-0.028*** (0.006)
Trade openness		-0.005*** (0.002)		0.002 (0.002)
Financial openness		-0.093 (0.101)		0.328*** (0.116)
Net FDI (% trend GDP)		-0.042*** (0.009)		-0.071*** (0.013)
Net port. inv. (% trend GDP)		-0.008* (0.005)		-0.029*** (0.007)
Int. reserves (% trend GDP)		0.014*** (0.004)		0.010 (0.007)
Fiscal procyclicality		0.000 (0.035)		-0.174*** (0.039)
Inflation (y/y avg.)		-0.000*** (0.000)		-0.000 (0.000)
Undervaluation Index		-0.232** (0.105)		-0.237* (0.122)
Exchange Rate Stability Index		-0.041 (0.094)		-0.053 (0.104)
Natural log capital to output ratio		1.022*** (0.236)		0.292 (0.351)
Natural log GDP per capita		0.702*** (0.168)		0.564* (0.298)
Total natural resources rents (% of GDP)		-0.017 (0.011)		0.066*** (0.010)
Percentage of population with at most secondary (Barro-Lee)		-0.030*** (0.004)		-0.002 (0.005)
Dummy for a crisis episode (previous 5 years)		-0.223 (0.150)		-0.267 (0.189)
Observations	3426	1395	3426	1395
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)				

Table 15: Instrumental variables SR - 2014 - Second stage

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)	(5)
Panel A. No controls					
Trade R Index 2014	-0.338 (0.373)				
External Finance Reform Index 2014		0.738 (2.555)			
Domestic Finance Reform Index 2014			0.076 (0.054)		
Product Market Reform Index 2014				0.000 (0.024)	
Labor Market Reform Index 2014					-15.662 (31.486)
Observations	2485	2485	2485	2485	2485
First stage F-stat	226.069	558.519	352.281	234.020	1660.576
Panel B. With controls					
Trade Reform Index 2014	-0.516** (0.247)				
External Finance Reform Index 2014		-0.790 (0.494)			
Domestic Finance Reform Index 2014			-0.068 (0.072)		
Product Market Reform Index 2014				-0.071* (0.037)	
Labor Market Reform Index 2014					1.316 (1.147)
Observations	1616	1616	1616	1616	1616
First stage F-stat	178.683	331.639	272.785	326.248	513.608
Panel C. No controls - sample selection					
Trade Reform Index 2014	-0.599** (0.295)				
External Finance Reform Index 2014		-1.068 (0.988)			
Domestic Finance Reform Index 2014			-0.019 (0.060)		
Product Market Reform Index 2014				-0.037 (0.025)	
Labor Market Reform Index 2014					0.665 (0.917)
Observations	1616	1616	1616	1616	1616
First stage F-stat	322.851	721.749	329.427	351.735	808.320
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)					

Table 16: Instrumental variables SR - 2014 - First stage

Dependent variable: Structural Reform Index 2014	(1)	(2)	(3)	(4)	(5)
Panel A. No controls					
Trade Reform Index 2014 neighbors	0.290*** (0.053)				
External Finance Reform Index 2014 neighbors		0.061 (0.090)			
Domestic Finance Reform Index 2014 neighbors			1.395*** (0.038)		
Product Market Reform Index 2014 neighbors				1.913*** (0.033)	
Labor Market Reform Index 2014 neighbors					-0.069 (0.137)
Observations	2485	2485	2485	2485	2485
Panel B. With controls					
Trade Reform Index 2014 neighbors	0.466*** (0.084)				
External Finance Reform Index 2014 neighbors		0.392*** (0.137)			
Domestic Finance Reform Index 2014 neighbors			1.297*** (0.068)		
Product Market Reform Index 2014 neighbors				1.917*** (0.056)	
Labor Market Reform Index 2014 neighbors					-0.523*** (0.189)
Observations	1616	1616	1616	1616	1616
Panel C. No controls - sample selection					
Trade Reform Index 2014 neighbors	0.370*** (0.071)				
External Finance Reform Index 2014 neighbors		0.198* (0.116)			
Domestic Finance Reform Index 2014 neighbors			1.272*** (0.053)		
Product Market Reform Index 2014 neighbors				2.076*** (0.038)	
Labor Market Reform Index 2014 neighbors					-0.619*** (0.175)
Observations	1616	1616	1616	1616	1616
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)					

Table 17: Instrumental variables SR - 2005 - Second stage

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. No controls						
Trade Reform Index 2005	-0.002 (0.026)					
Current Account Reform Index 2005		0.132 (0.297)				
Capital Account Reform Index 2005			0.248 (0.494)			
Finance Reform Index 2005				-0.045 (0.112)		
Product Market Reform Index 2005					0.139*** (0.044)	
Agriculture Reform Index 2005						0.220* (0.116)
Observations	5835	5835	5835	5835	5835	5835
First stage F-stat	243.556	305.296	722.794	403.865	42.151	908.685
Panel B. With controls						
Trade Reform Index 2005	-0.785***					
Current Account Reform Index 2005		-1.627 (1.021)				
Capital Account Reform Index 2005			-11.677 (29.871)			
Finance Reform Index 2005				-0.255*** (0.094)		
Product Market Reform Index 2005					-0.132** (0.060)	
Agriculture Reform Index 2005						-1.613* (0.878)
Observations	1430	1430	1430	1430	1430	1430
First stage F-stat	66.139	348.685	1735.865	503.190	94.942	576.388
Panel C. No controls - sample selection						
Trade Reform Index 2005	-0.462*** (0.147)					
Current Account Reform Index 2005		-1.325 (0.963)				
Capital Account Reform Index 2005			-4.900 (9.285)			
Finance Reform Index 2005				-0.037 (0.058)		
Product Market Reform Index 2005					-0.061 (0.037)	
Agriculture Reform Index 2005						11.196 (49.488)
Observations	1430	1430	1430	1430	1430	1430
First stage F-stat	71.689	2353.680	4694.133	615.686	119.941	3297.203
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)						

Table 18: Instrumental variables SR - 2005 - First stage

Dependent variable: Structural Reform Index 2005	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. No controls						
Trade Reform Index 2005 neighbors	0.997*** (0.026)					
Current Account Reform Index 2005 neighbors		0.119*** (0.017)				
Capital Account Reform Index 2005 neighbors			0.166*** (0.036)			
Finance Reform Index 2005 neighbors				0.335*** (0.017)		
Product Market Reform Index 2005 neighbors					1.048*** (0.034)	
Agriculture Reform Index 2005 neighbors						0.573*** (0.054)
Observations	5835	5835	5835	5835	5835	5835
Panel B. With controls						
Trade Reform Index 2005 neighbors	0.466*** (0.066)					
Current Account Reform Index 2005 neighbors		0.224* (0.124)				
Capital Account Reform Index 2005 neighbors			0.048 (0.125)			
Finance Reform Index 2005 neighbors				1.045*** (0.060)		
Product Market Reform Index 2005 neighbors					1.501*** (0.070)	
Agriculture Reform Index 2005 neighbors						0.288** (0.132)
Observations	1430	1430	1430	1430	1430	1430
Panel C. No controls - sample selection						
Trade Reform Index 2005 neighbors	0.511*** (0.054)					
Current Account Reform Index 2005 neighbors		0.166* (0.097)				
Capital Account Reform Index 2005 neighbors			0.054 (0.099)			
Finance Reform Index 2005 neighbors				1.140*** (0.046)		
Product Market Reform Index 2005 neighbors					1.680*** (0.063)	
Agriculture Reform Index 2005 neighbors						-0.025 (0.111)
Observations	1430	1430	1430	1430	1430	1430
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)						

Table 19: Feedback effects

Dependent variable:	Law and order			Democratic accountability		
	(1)	(2)	(3)	(4)	(5)	(6)
Investment boom	0.118*	0.240**	0.147	0.097	0.065	0.046
	(0.071)	(0.120)	(0.122)	(0.090)	(0.137)	(0.141)
Law and order				0.218***	0.104***	0.180***
				(0.022)	(0.032)	(0.032)
Democratic accountability	0.138***	0.080***	0.135***			
	(0.014)	(0.024)	(0.024)			
Federal funds rate (end of period)		0.018			-0.007	
		(0.021)			(0.024)	
Volatility of US Stock Market Index		-0.006			0.007	
		(0.008)			(0.009)	
Terms of Trade Index (2016m1=100)		0.007			-0.028***	
		(0.006)			(0.007)	
Trade Index ([exports + imports]/GDP)		-0.004***			0.003*	
		(0.002)			(0.002)	
Financial Openness Index		0.140			0.259**	
		(0.097)			(0.110)	
5y-ma net FDI (% trend GDP)		-0.045***			-0.073***	
		(0.012)			(0.013)	
5y-ma net port. inv. (% trend GDP)		-0.013*			-0.031***	
		(0.007)			(0.007)	
5y-ma int. reserves (% trend GDP)		0.011**			0.009	
		(0.005)			(0.006)	
5y corr. of gdp and gov. cons. (dev. from trends)		-0.037			-0.189***	
		(0.035)			(0.040)	
Inflation (y/y avg.)		-0.000**			-0.000	
		(0.000)			(0.000)	
Undervaluation Index		0.175			-0.489***	
		(0.120)			(0.136)	
Exchange Rate Stability Index		0.084			-0.085	
		(0.084)			(0.096)	
Natural log capital to output ratio		0.996***			0.322	
		(0.219)			(0.252)	
Natural log GDP per capita		1.652***			0.569***	
		(0.179)			(0.210)	
Total natural resources rents (% of GDP)		-0.005			0.072***	
		(0.010)			(0.011)	
Percentage of population with at most secondary (Barro-Lee)		-0.016***			-0.003	
		(0.004)			(0.005)	
Dummy for a crisis episode (previous 5 years)		-0.065			-0.254*	
		(0.133)			(0.152)	
Country FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Observations	3397	1395	1395	3397	1395	1395
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)						

Table 20: Feedback effects - SRI - 2014

Dependent variable: SRI - 2014	Trade	External finance	Domestic finance	Product market	Labor market
	(1)	(2)	(3)	(4)	(5)
Panel A. No controls					
Investment boom	0.021* (0.012)	-0.021 (0.018)	0.011 (0.011)	-0.011 (0.015)	-0.009 (0.010)
Country FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	2485	2485	2485	2485	2485
Panel B. With controls					
Investment boom	0.001 (0.012)	0.000 (0.028)	0.032** (0.013)	0.016 (0.022)	-0.017 (0.016)
Country FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	1616	1616	1616	1616	1616
Panel C. No controls - sample selection					
Investment boom	0.007 (0.013)	-0.005 (0.028)	0.030** (0.014)	0.029 (0.021)	-0.022 (0.016)
Country FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	1616	1616	1616	1616	1616
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)					

Table 21: Feedback effects - SRI - 2005

Dependent variable: SRI - 2005	Trade	Current acct.	Capital acct.	Finance	Product market	Agriculture
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. No controls						
Investment boom	-0.001 (0.016)	0.032*** (0.012)	0.015 (0.011)	-0.026*** (0.007)	0.005 (0.006)	0.008 (0.012)
Country FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	5075	5075	5075	5075	5075	5075
Panel B. With controls						
Investment boom	-0.003 (0.026)	0.017 (0.041)	-0.006 (0.023)	0.039** (0.016)	0.015 (0.022)	0.040 (0.031)
Country FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	1430	1430	1430	1430	1430	1430
Panel C. No controls - sample selection						
Investment boom	0.004 (0.027)	-0.004 (0.040)	0.015 (0.023)	0.034* (0.018)	0.020 (0.022)	0.011 (0.031)
Country FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	1430	1430	1430	1430	1430	1430
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)						

Table 22: Nonlinearities

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)	(5)	(6)
Law and order	0.131** (0.059)	0.269** (0.130)	0.427*** (0.138)		0.591*** (0.168)	0.038 (0.038)
Law and order squared	-0.017** (0.008)	-0.030 (0.020)	-0.064*** (0.021)			
Democratic accountability	0.016 (0.012)	-0.031 (0.026)	0.007 (0.025)	0.122*** (0.043)	0.047 (0.282)	-0.096 (0.179)
Democratic accountability squared				-0.016** (0.006)	-0.032 (0.041)	0.011 (0.022)
Federal funds rate (end of period)		-0.046*** (0.012)			-2.769 (3.743)	
Volatility of US Stock Market Index		-0.005 (0.003)			-0.040 (0.032)	
Terms of Trade Index (2016m1=100)		0.000 (0.009)			-0.047* (0.025)	
Trade Index ((Exports + Imports)/GDP)		-0.002 (0.002)			-0.018*** (0.007)	
Financial Openness Index		-0.298*** (0.101)			-0.960*** (0.282)	
5y-ma net FDI (% trend GDP)		-0.013* (0.008)			-0.050*** (0.019)	
5y-ma net port. inv. (% trend GDP)		0.006 (0.009)			0.026 (0.026)	
5y-ma int. reserves (% trend GDP)		0.016 (0.010)			0.014 (0.018)	
5y corr. of GDP and gov. cons. (dev. from trends)		0.082** (0.041)			0.214*** (0.070)	
Inflation (y/y avg.)		-0.009*** (0.003)			-0.010*** (0.004)	
Undervaluation Index		0.231 (0.159)			-0.178 (0.348)	
Exchange Rate Stability Index		0.165 (0.131)			0.251 (0.310)	
Natural log capital to output ratio		-2.660*** (0.497)			-8.506*** (2.476)	
Natural log GDP per capita		-0.795*** (0.230)			-5.687*** (1.319)	
Total natural resources rents (% of GDP)		0.012 (0.008)			0.024* (0.014)	
Percentage of population with at most secondary (Barro-Lee)		0.019** (0.008)			0.082*** (0.027)	
Dummy for a crisis episode (previous 5 years)		0.066 (0.067)			0.324** (0.135)	
Country FE	Y	Y	Y	Y	Y	Y
Year FE	Y	N	Y	Y	Y	Y
Observations	1018	306	267	1018	267	267
pseudo R-sq	0.088	0.478	0.142	0.091	0.761	0.096

Table 23: Nonlinearities - SRI - 2014

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	Panel A. No controls					Panel B. With controls					Panel C. No controls - sample selection				
Trade	2.031					0.427					0.470				
	(1.574)					(7.503)					(3.152)				
Trade - squared	-1.877					-6.407					-4.003				
	(1.494)					(5.985)					(2.691)				
External finance		-0.384				18.513***					2.191				
		(1.776)				(5.858)					(2.799)				
External finance - squared		-0.356				-11.310***					-1.632				
		(1.398)				(4.016)					(2.187)				
Domestic finance			0.683				0.478					5.684**			
			(1.445)				7.326					(2.395)			
Domestic finance - squared			1.355				(5.054)					0.558			
			(1.286)				(5.551)					(2.104)			
Product market				0.326			0.060							0.003	
				(1.169)			(2.469)							(1.589)	
Product market - squared				-1.036			0.804							1.783	
				(1.123)			(2.687)							(1.663)	
Labor market					-7.002					-418.156***					-121.192***
					(4.478)					(87.466)					(43.296)
Labor market - squared					3.241					270.096***					75.311***
					(3.645)					(57.922)					(27.566)
Observations	630	630	630	630	630	250	250	250	250	250	250	250	250	250	250

Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)

Table 24: Nonlinearities - SRI - 2005

Dependent variable: Start of episode dummy	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. No controls						Panel B. With controls						Panel C. No controls - sample selection					
Trade	0.234						-11.324**						0.964					
	(0.496)						(5.608)						(1.929)					
Trade - squared	-0.328						4.412						-2.785					
	(0.598)						(5.153)						(1.932)					
Current account		0.369						8.799**						2.437				
		(0.507)						(4.268)						(1.689)				
Current account - squared		0.347						-6.742**						-2.162				
		(0.466)						(3.359)						(1.325)				
Capital account			3.934***						21.106***						1.510			
			(0.617)						(7.927)					(4.906)				
Capital account - squared			-3.989***						-19.067***					-1.872				
			(0.590)						(6.026)					(3.466)				
Finance				-3.190***						24.249***						2.569		
				(0.687)						(6.682)						(2.638)		
Finance - squared				1.786**						-10.656*						0.802		
				(0.741)						(5.856)						(2.327)		
Product market					1.902***						-0.573							-0.740
					(0.718)						(2.885)							(1.464)
Product market - squared					-2.083**						-2.982							1.318
					(0.892)						(3.649)							(1.841)
Agriculture						0.175						9.125*						0.278
						(0.814)						(4.840)						(3.662)
Agriculture - squared						0.629						-14.900**						2.884
						(0.859)						(6.262)						(3.780)
Observations	2945	2945	2945	2945	2945	2945	276	276	276	276	276	276	276	276	276	276	276	276

Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)

Table 25: Principal component

Dependent variable: Start of episode dummy	(1)	(2)	(3)
Principal component of broad institutions	0.022*	0.089**	0.003
	(0.013)	(0.046)	(0.025)
Federal funds rate		-1.860*	
		(1.026)	
Volatility of US stock market		-0.029	
		(0.024)	
Terms of trade		0.016	
		(0.014)	
Trade openness		-0.005*	
		(0.003)	
Financial openness		-0.512***	
		(0.186)	
Net FDI (% trend GDP)		-0.017	
		(0.011)	
Net port. inv. (% trend GDP)		-0.002	
		(0.019)	
Int. reserves (% trend GDP)		0.004	
		(0.016)	
Fiscal procyclicality		0.225***	
		(0.059)	
Inflation (y/y avg.)		-0.011***	
		(0.004)	
Undervaluation Index		0.106	
		(0.280)	
Exchange Rate Stability Index		0.020	
		(0.158)	
Natural log capital to output ratio		-3.491***	
		(0.791)	
Natural log GDP per capita		-3.211***	
		(0.853)	
Total natural resources rents (% of GDP)		-0.007	
		(0.010)	
Percentage of population with at most secondary (Barro-Lee)		0.034***	
		(0.011)	
Dummy for a crisis episode (previous 5 years)		-0.019	
		(0.111)	
Country FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	1018	267	267
pseudo R-sq	0.082	0.591	0.091
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)			

Table 26: Principal component - SRI - 2014

Dependent variable: Start of episode dummy	(1)	(2)	(3)
Principal component SRI - 2014	-0.029 (0.096)	-0.225 (0.220)	0.247 (0.176)
Federal funds rate		-2.094 (1.319)	
Volatility of US stock market		-0.007 (0.026)	
Terms of trade		0.002 (0.010)	
Fiscal procyclicality		0.015 (0.046)	
Inflation (y/y avg.)		-0.011*** (0.004)	
Undervaluation Index		0.725*** (0.208)	
Exchange Rate Stability Index		-0.041 (0.138)	
Natural log capital to output ratio		-2.762*** (0.475)	
Natural log GDP per capita		-0.998*** (0.244)	
Total natural resources rents (% of GDP)		-0.010 (0.011)	
Percentage of population with at most secondary (Barro-Lee)		0.022*** (0.008)	
Dummy for a crisis episode (previous 5 years)		-0.171 (0.106)	
Country FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	630	250	250
pseudo R-sq	0.098	0.375	0.079
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)			

Table 27: Principal component - SRI - 2005

Dependent variable: Start of episode dummy	(1)	(2)	(3)
Principal component SRI - 2005	0.007 (0.019)	0.533*** (0.190)	0.644*** (0.170)
Federal funds rate		0.009 (0.088)	
Volatility of US stock market		0.014 (0.028)	
Terms of trade		-0.015 (0.009)	
Fiscal procyclicality		-0.100** (0.045)	
Inflation (y/y avg.)		-0.010*** (0.003)	
Undervaluation Index		0.661** (0.284)	
Exchange Rate Stability Index		-0.496*** (0.160)	
Natural log capital to output ratio		-2.552*** (0.586)	
Natural log GDP per capita		-0.479* (0.280)	
Total natural resources rents (% of GDP)		-0.012 (0.013)	
Percentage of population with at most secondary (Barro-Lee)		0.059*** (0.015)	
Dummy for a crisis episode (previous 5 years)		-0.133 (0.082)	
Country FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	2945	209	209
pseudo R-sq	0.092	0.552	0.194
Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)			

A Additional Robustness Checks

A.1 Other discrete choice models

We tested the robustness of our results to different functional forms. Tables 28 to 30 at the end of this section summarize the outcome of this exercise. We tested our probit models to three different functional forms from the discrete choice model realm: (a) OLS (e.g., LPM), (b) logit, and (c) relogit. The last is a model that accounts for the presence of rare events such as our investment boom episodes (e.g., when the number of observations associated with an episode is relatively small compared to the number of observations with no episode)²⁰.

As with our baseline models, we present three panels of specifications: (panel A) without controls, (panel B) with controls, and (panel C) without controls but using the same sample as the model with controls, in order to account for the possibility of sample selection bias. We show the results for five of our specifications: (a) “naive” pooled model (column 1), (b) fixed effects model (column 2), (c) fixed effects model for emerging and low income countries (column 3), (d) nonlinearities (columns 4 to 9, depending on the set of institutional variables), and (e) the first principal component of the different institutional indices (last column). We discuss the results briefly for each set of institutional variables below.

A.1.1 Broad measures of institutions

Table 28 shows the results for the law and order and democratic accountability indices. The results from the OLS model (block I of results in the table) are broadly in line with our baseline results, that is, improvements in broad economic and political institutions, especially economic institutions, increase the likelihood of experiencing an investment boom, and this result is consistent to the inclusion of controls, unobserved heterogeneity, different samples, and the presence of nonlinearities. The second block in the table corresponds to the logit form; here the results are even more similar to those of our baseline probits. The last block corresponds to the results of the relogit model. Although this model does not allow the inclusion of fixed effects, its results are broadly the same as all other forms, except in the case of the coefficient of the principal component, which is consistently negative.

A.1.2 Narrow measures of institutions - SRI - 2014

Table 29 shows the results for the SRI - 2014 variables. Results from the OLS model are broadly in line with our baseline results, that is, domestic finance reforms increase the likelihood of experiencing an investment boom, whereas external finance reforms decrease it. These results are consistent to the inclusion of controls and fixed effects and in different samples. Contrary to our baseline model, trade reform effects are not statistically significant under OLS. Regarding the logit form (block II), the negative effect of trade reform is

²⁰For details on the methodology, see: King & Zeng (1999).

picked up again and the effects of domestic finance and labor reform remain consistent with our baseline. Lastly, relogit model results, although mostly insignificant, maintain the pattern with regard to domestic finance reforms (positive effect) and external finance reforms (negative effects).

A.1.3 Narrow measures of institutions - SRI - 2005

Table 30 repeats the process for the SRI - 2005 variables. Results from the OLS model show a consistent positive effect for finance and agriculture reforms and a negative effect on capital account reform, as in our baseline model. The logit form (block II) shows negative effects for trade and capital account reforms and a positive effect on current account and finance reforms, all of which is in line with our baseline. The relogit results (block 3) repeat the pattern associated with the logit model. Lastly, the principal component is positive and statistically significant for the OLS and logit models.

Table 28: Robustness to functional form

	Pooled	FE	EMDE	NL	NL	PCA	Pooled	FE	EMDE	NL	NL	PCA	Pooled	FE	EMDE	NL	NL	PCA
I. OLS																		
	Panel A. Without controls						Panel B. With controls						Panel C. Without controls - Sample selection					
Law and order	0.003	0.008	0.009	0.036**			0.001	0.022**	0.033**	0.091***			-0.009***	0.011	0.023**	0.087***		
	(0.003)	(0.005)	(0.006)	(0.013)			(0.004)	(0.008)	(0.010)	(0.023)			(0.003)	(0.008)	(0.009)	(0.024)		
Law and order - squared				-0.004*						-0.011***						-0.012***		
				(0.002)						(0.003)						(0.003)		
Democratic accountability	-0.006*	0.006	0.009*		0.031*		0.009	0.006	0.019*		-0.022		0.003	0.002	0.013			-0.001
	(0.003)	(0.005)	(0.004)		(0.012)		(0.006)	(0.008)	(0.009)		(0.030)		(0.004)	(0.009)	(0.008)			(0.026)
Democratic accountability - squared					-0.004**						0.004							0.000
					(0.002)						(0.004)							(0.003)
Scores for component 1						0.009*						0.015*						0.004
						(0.004)						(0.007)						(0.009)
Country FE	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y
Year FE	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y
Observations	3449	3449	2749	3449	3449	3449	1395	1395	864	1395	1395	1395	1395	1395	864	1395	1395	1395
II. Logit																		
Law and order	0.003	0.017	0.027	0.155***			0.001	0.545***	0.817***	0.253**			-0.908***	0.347	0.134***	0.424***		
	-0.002	-0.019	-0.018	-0.044			-0.004	-0.139	-0.213	-0.092			-0.003	-0.343	-0.046	-0.117		
Law and order - squared				-0.018**						-0.028						-0.063***		
				-0.006						-0.015						-0.02		
Democratic accountability	-0.005*	0.02	0.031**		0.116**		0.004	-0.177**	-0.253**		0.079		0.003	-0.01	0.034			-0.068
	-0.002	-0.014	-0.012		-0.04		-0.006	-0.069	-0.096		-0.201		-0.005	-0.182	-0.027			-0.146
Democratic accountability - squared					-0.015*						-0.04							0.008
					-0.006						-0.035							-0.018
Scores for component 1						0.024						0.094*						0.006
						-0.016						-0.037						-0.031
Country FE	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y
Year FE	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y
Observations	3449	1018	999	1018	1018	1018	1395	267	210	306	267	267	1395	267	210	267	267	267
III. Relogit																		
Law and order	0.069		0.382***	2.369***			0.023		0.366**	2.907***			-0.242***		0.253**	2.967***		
	(0.058)		(0.066)	(0.362)			(0.121)		(0.118)	(0.866)			(0.078)		(0.102)	(0.799)		
Law and order - squared				-0.315***						-0.436**						-0.449***		
				(0.048)						(0.137)						(0.117)		
Democratic accountability	-0.123*		0.092		0.554*		0.102		0.393*		-0.134		0.069		0.382***			-0.088
	(0.058)		(0.064)		(0.256)		(0.177)		(0.173)		(0.897)		(0.137)		(0.146)			(0.746)
Democratic accountability - squared					-0.100**						0.029							0.019
					(0.035)						(0.111)							(0.094)
Scores for component 1						-0.089***						-0.067						-0.188***
						(0.025)						(0.124)						(0.044)
Country FE	N		N	N	N	N	N		N	N	N	N	N		N	N	N	N
Year FE	N		N	N	N	N	N		N	N	N	N	N		N	N	N	N
Observations	3449		2749	3449	3449	3449	1395		864	1395	1395	1395	1395		864	1395	1395	1395

Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)

A.2 Varying episode identification parameters

We also test the sensitivity of our results to variation in our episode identification parameters. Specifically, we modify the minimum average annual growth rate of capital stock per capita from 3.5 percent to 5 percent and the minimum within-window acceleration threshold from 2 percent to 3 percent. A stricter filter for identification has two related consequences that act on opposite directions for the purposes of inference: (a) while it will pick up stronger or clearer cases of investment growth and acceleration, it will (b) reduce the number of events in our sample and thus reduce the explanatory power of coefficients. A lenient filter on the other hand will pick up weaker episodes, with the benefit of increasing the number of sample events but at the cost of having biased coefficients.

Since we are more interested in stronger rather than weaker episodes, we chose the 3.5 percent growth and 2 percent acceleration threshold as our baseline and the 5 percent growth and 3 percent acceleration benchmark as our robustness scenario. This stricter filter picks up 92 episodes, a notable reduction of our baseline number of 159. Table 31 shows the results of our different specification for our 3 sets of institutional variables, again testing the results to the inclusion of controls and sample size.

A.2.1 Broad measures of institutions

The first block shows results for our broad measures of institutions. As expected, the coefficients of the variables lose statistical relevance. Nonetheless, some of the models show positive effects for democratic accountability and a positive, although diminishing, effect for law and order. Last, although the principal component is not significant, its coefficient is consistently positive.

A.2.2 Narrow measures of institutions - SRI - 2014

Block II shows results for SRI - 2014 indicators. We observe a positive—although diminishing—effect of both domestic and external finance reforms at the start of stronger investment boom episodes. We also observe that the opposite occurs regarding product and labor market reforms, that is, the effect is initially negative but after some threshold it becomes positive. Lastly, despite the coefficient of the principal's not being consistently significant, it is positive in the majority of our models.

A.2.3 Narrow measures of institutions - SRI - 2005

For the SRI - 2005 indices, we observe that trade reforms have a negative but subsequently a positive in starting stronger investment booms, and the same happens with finance reforms, although linearly. We also observe a positive effect for current and capital account reforms, particularly in the models without controls. The principal component of these reforms also shows a positive and statistically significant effect in the model with controls.

Table 31: Robustness to varying episode identification parameters

II. Broad measures	Panel A. Without controls						Panel B. With controls						Panel C. Without controls - sample selection																			
	Pooled	FE	EMDE	NL	NL	PCA	Pooled	FE	EMDE	NL	NL	PCA	Pooled	FE	EMDE	NL	NL	PCA														
Law and order	-0.002 (0.002)	-0.028 (0.016)	-0.013 (0.016)	0.071 (0.057)			-0.003 (0.003)	-0.051 (0.039)	-0.016 (0.047)	0.229 (0.139)			-0.004* (0.002)	-0.184 (0.130)	0.028 (0.026)	0.327*** (0.117)																
Law and order - squared																																
Democratic accountability	-0.001 (0.002)	0.037* (0.013)	0.046*** (0.012)		0.048 (0.046)		0.000 (0.003)	-0.010 (0.039)	-0.059 (0.046)	0.217 (0.195)			0.003 (0.003)	0.125 (0.101)	0.070*** (0.025)	-0.061 (0.101)																
Democratic accountability - squared																																
Scores for component 1					0.011 (0.014)					0.008 (0.020)									0.011 (0.023)													
Country FE	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	N	Y	Y	Y	Y	Y	Y	Y													
Year FE	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N													
Observations	3601	551	546	551	551	551	1445	173	142	173	173	1445	173	142	173	173	173	173	173													
III. SRI - 2014	Panel A. Without controls										Panel B. With controls										Panel C. Without controls - sample selection											
	Pooled	FE	EMDE	NL	NL	NL	NL	NL	NL	PCA	Pooled	FE	EMDE	NL	NL	NL	NL	NL	NL	PCA	Pooled	FE	EMDE	NL	NL	NL	NL	NL	NL	NL	PCA	
Trade reform	0.019 (0.024)	-0.213 (0.175)	0.006 (0.035)	-0.909* (2.74)							0.004 (0.023)	-0.040 (0.254)	0.024 (0.035)	-14.565 (14.300)							0.002 (0.020)	1.130 (1.72)	0.028 (0.042)	-4.303 (6.910)								
Trade reform - squared																																
External finance reform	-0.029 (0.020)	0.136 (0.100)	-0.018 (0.025)	1.147 (2.22)							-0.004 (0.013)	1.075*** (0.242)	0.018 (0.017)	29.392*** (11.280)							-0.015 (0.013)	2.212** (1.098)	0.004 (0.019)	17.401*** (5.71)								
External finance reform - squared																																
Domestic finance reform	-0.017 (0.015)	-0.228 (0.147)	-0.038 (0.021)	-1.471* (1.425)							0.010 (0.013)	0.029 (0.210)	0.007 (0.020)	19.368** (3.965)							0.010 (0.013)	-0.261 (1.098)	0.024 (0.025)	1.687 (4.257)								
Domestic finance reform - Squared																																
Product market reform	-0.005 (0.012)	0.184 (0.110)	-0.021 (0.019)	0.826 (1.400)			0.189 (1.556)				-0.009 (0.011)	-0.330** (0.125)	-0.021 (0.021)	9.119* (3.821)							0.006 (0.009)	0.409 (0.747)	-0.012 (0.017)	1.947 (2.125)								
Product market reform - squared																																
Labor market reform	-0.017 (0.016)	0.471 (0.290)	-0.030 (0.023)	2.983 (4.129)			-1.175 (5.709)				0.041** (0.016)	1.256** (0.466)	0.007 (0.019)	-217.840* (92.794)							0.026 (0.016)	9.570*** (2.445)	0.008 (0.020)	-110.682** (46.370)								
Labor market reform - Squared																																
Scores for component 1							-0.056 (0.047)																									
Country FE	N	Y	N	Y	Y	Y	Y	Y	Y	N	Y	N	Y	Y	Y	Y	Y	Y	Y	N	Y	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Year FE	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	
Observations	2564	451	1602	421	431	421	421	421	421	1065	239	940	239	239	239	239	239	239	239	1065	239	940	239	239	239	239	239	239	239	239	239	
III. SRI - 2005	Panel A. Without controls										Panel B. With controls										Panel C. Without controls - sample selection											
	Pooled	FE	EMDE	NL	NL	NL	NL	NL	NL	PCA	Pooled	FE	EMDE	NL	NL	NL	NL	NL	NL	PCA	Pooled	FE	EMDE	NL	NL	NL	NL	NL	NL	NL	PCA	
Trade reform	-0.005 (0.007)	-0.043 (0.027)	0.008 (0.009)	0.138 (0.399)							-0.059** (0.022)	-0.080** (0.029)	9.468** (3.62)								-0.049*** (0.016)	-0.061*** (0.025)	6.259*** (2.127)									
Trade reform - squared																																
Current account reform	0.038** (0.013)	0.118** (0.042)	0.024*** (0.014)	0.497* (0.463)			0.335 (0.245)				0.022 (0.017)	0.058* (0.024)		2.174 (1.514)							0.015 (0.015)	0.046** (0.019)	1.486 (1.166)									
Current account reform - squared																																
Capital account reform	0.018** (0.008)	0.157*** (0.043)	0.028** (0.011)	1.519*** (0.362)							-0.003 (0.010)	0.011 (0.010)		-1.373 (1.267)							-0.001 (0.006)	-0.000 (0.014)	0.263 (1.214)									
Capital account reform - squared																																
Finance reform	-0.078*** (0.018)	-0.359*** (0.096)	-0.109*** (0.020)	-1.631** (0.407)			-1.631** (1.255)				-0.017 (0.749)	-0.059* (0.014)		0.950 (1.470)							0.002 (0.014)	-0.006 (0.022)	1.259 (1.368)									
Finance reform - squared																																
Product market reform	-0.015 (0.021)	0.060 (0.055)	0.003 (0.026)	1.625 (0.645)			-1.361 (0.749)				-0.010 (0.015)	-0.002 (0.023)		4.628*** (1.941)							0.005 (0.014)	0.020 (0.026)	2.002 (1.586)									
Product market reform - squared																																
Agriculture reform	-0.001 (0.006)	0.151*** (0.033)	-0.004 (0.006)	1.625 (0.407)			1.625 (0.893)				0.011 (0.007)	-0.004 (0.011)		1.966** (0.966)							0.006 (0.007)	-0.004 (0.011)	1.244 (0.845)									
Agriculture reform - squared																																
Scores for component 1							-0.020 (0.016)																									
Country FE	N	Y	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	
Year FE	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	
Observations	6069	2124	4290	6069	6069	6069	6069	6069	6069	2124	1473	845	1473	1473	1473	1473	1473	1473	1473	4290	845	1473	1473	1473	1473	1473	1473	1473	1473	1473	1473	

Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)

A.3 Varying time window

Finally, we test the sensitivity of our results to variation in our episode time window. Specifically, we modify the time window from 8 to 6 years. A shorter time window for identification has similar consequences to those of varying growth and acceleration parameters. While a shorter time window will pick up weaker cases of investment growth and acceleration, it will increase the number of events in our sample and thus reduce the explanatory power of coefficients. A larger time window on the other hand will pick up stronger episodes, at the cost of decreasing the number of events but with the benefit of having less-biased coefficients.

In this case, since in the previous exercise the fact of having fewer events complicated to some extent the main interest of our exercise, which is assessing the robustness of our baseline model results, we chose to show here results for the shorter time window with our baseline growth and acceleration parameter. This exercise picks up 192 episodes. Table 32 shows the results of our different specifications for our 3 sets of institutional variables, testing the results to the inclusion of controls and sample selection.

A.3.1 Broad measures of institutions

The first block shows results for our broad measures of institutions. As expected, we observe patterns similar to those of our baseline model, though there is an interesting pattern for shorter investment booms: the law and order index has positive but diminishing effects, while the democratic accountability index has negative but subsequently positive effects.

A.3.2 Narrow measures of institutions - SRI 2014

Block II shows results for the SRI 2014 - indicators. We observe a positive—although diminishing—effect of domestic finance, external finance, and product market reforms at the start of shorter investment boom episodes. We also observe that the opposite occurs regarding trade and labor market reforms, that is, the effect is negative but after some threshold it becomes positive. Last, despite the coefficient of the principal component's not being consistently significant, it is positive in the majority of our models.

A.3.3 Narrow measures of institutions - SRI - 2005

For the SRI - 2005 indices, we observe a positive effect for current account and agricultural reforms, particularly in the models without controls and a positive effect for finance reforms in the models with controls. The principal component of these reforms also shows a consistent—although statistically insignificant—positive effect.

Table 32: Robustness to varying episode time window

II. Broad measures															
	Panel	FE	EMDE	NL	NL	NL	NL	NL	NL	NL	NL	NL	PCA		
	Panel A. Without controls														
Law and order	0.003	0.025	0.031*	0.097*											
Law and order - squared	(0.003)	(0.017)	(0.015)	(0.046)											
Democratic accountability	-0.007**	0.003	0.016	0.098**											
Democratic accountability - squared	(0.003)	(0.012)	(0.010)	(0.041)											
Scores for component 1				0.014									-0.028		
				(0.013)									(0.028)		
Country FE	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y		
Year FE	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y		
Observations	3285	1087	1180	1087	1087	1087	1087	1087	1087	1087	1087	1087	1087		
III. SRI - 2014															
	Panel	FE	EMDE	NL	NL	NL	NL	NL	NL	NL	NL	NL	NL	PCA	
	Panel A. Without controls														
Trade reform	0.013	-0.067	-0.02	1.965											
Trade reform - squared	-0.034	-0.151	-0.065	-1.483											
External finance reform	-0.075**	-0.074	-0.195	-1.488											
External finance reform - squared	-0.028	-0.109	-0.129	-1.752											
Domestic finance reform	0.026	0.452**	-0.059	1.825											
Domestic finance reform - squared	-0.024	-0.153	-0.153	-1.505											
Product market reform	0.01	0.025	0.109	1.825											
Product market reform - squared	-0.017	-0.111	-0.12	-1.3											
Labour market reform	-0.042	-0.530**	-0.615**	4.658**											
Labour market reform - squared	-0.024	-0.192	-0.206	-1.12											
Scores for component 1				0.088										0.284**	
				-0.082										-0.181	
Country FE	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Year FE	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Observations	2435	702	568	702	702	702	702	702	702	702	702	702	702	702	
III. SRI - 2005															
	Panel	FE	EMDE	NL	NL	NL	NL	NL	NL	NL	NL	NL	NL	NL	PCA
	Panel A. Without controls														
Trade reform	-0.020	-0.011	-0.017	0.482											
Trade reform - squared	(0.011)	(0.026)	(0.028)	(0.402)											
Current account reform	0.009**	0.124**	0.138**	0.505											
Current account reform - squared	(0.020)	(0.035)	(0.037)	(0.491)											
Capital account reform	0.003	0.050	0.167**	1.888**											
Capital account reform - squared	(0.013)	(0.036)	(0.038)	(0.548)											
Finance reform	-0.001**	-0.249**	-0.339**	3.333**											
Finance reform - squared	(0.026)	(0.054)	(0.057)	(0.625)											
Product market reform	-0.011	-0.031	-0.068	0.829											
Product market reform - squared	(0.023)	(0.050)	(0.050)	(0.685)											
Agriculture reform	0.006	0.101**	0.075**	1.720**											
Agriculture reform - squared	(0.009)	(0.029)	(0.030)	(0.510)											
Scores for component 1				0.064											0.129
				(0.060)											(0.129)
Country FE	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2781	820	295	820	820	820	820	820	820	820	820	820	820	820	820

Standard errors in parentheses (* p<0.10 ** p<0.05 *** p<0.01)