The Dynamics of Stagnation

A panel analysis of the onset and continuation of stagnation[*]

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Abstract

This paper analyzes periods of economic stagnation in a panel of countries. We test whether stagnation can be predicted by institutional characteristics and political shocks, and compare the impacts of such variables with those of traditional macroeconomic variables. We examine the determinants of stagnation episodes using dynamic linear and non-linear models. In addition, we analyze whether the effects of the included variables on the onset of stagnation differ from the effects on the continuation of stagnation. We find that inflation, negative regime changes, real exchange rate undervaluation, financial openness, and trade openness have significant effects on both the onset and the continuation of stagnation. Only for trade openness there is robust evidence of a differential impact. Open economies have a significantly lower probability of falling into stagnation, but once in stagnation they do not recover faster.

Keywords: growth episodes, stagnation, institutions, dynamic panel data

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1 Introduction

Since the 1950s, many countries across the globe have experienced substantial increases in GDP per capita brought about by years of economic growth. However, while these gains are mainly the result of steady positive growth rates in the developed world (at least prior to 2008), growth in developing countries has often been erratic and volatile. Most emerging economies have experienced periods of economic stagnation between positive growth spurts and for several countries the absence of sustained growth has proved to be a persistent phenomenon, often lasting for several years or even decades. Explaining why some countries experience more periods of stagnation than others may thus prove essential to the understanding of contemporary differences in levels of development.

Rather than focusing on differences in average growth rates, recent research increasingly aims to unveil the specific characteristics of different growth episodes such as accelerated growth, growth collapses, recoveries, or stagnation. We address two questions within this wider research agenda. First, we ask if institutional characteristics and external or internal political shocks determine the incidence of stagnation, and how these effects compare to standard macroeconomic explanations. Second, we analyze if any of the included variables have a different impact on the onset of stagnation than on its continuation. In other words, we examine if the factors affecting the probability of falling into stagnation are the same as those affecting the probability of continuing to be in stagnation.

Most of the empirical literature on growth episodes uses static models to study factors that are correlated with the onset of a growth spell and, more recently, began to examine factors associated with the duration of growth episodes. Our contribution is to analyze stagnation spells as a dynamic problem, subject to state dependence and interactions between the lagged state and the independent variables. This approach allows the probability of stagnation to depend on whether a country was already in stagnation in the preceding year (state dependence). It lets the data decide whether the included variables have a different effect on the onset of a stagnation episode than its continuation. We estimate the dynamic models using linear probability models, GMM, fixed-effects logit, and a dynamic random effects probit estimator proposed by Wooldridge (2005).

Our results indicate that political regime shifts towards autocracy have strong positive effects on the incidence of stagnation (onset and continuation), but other proxies for institutions and political shocks do not have significant effects. Macroeconomic factors explain the onset of stagnation rather well. Higher inflation positively predicts stagnation, while financial openness, trade openness, and real exchange rate undervaluation are associated with a reduced likelihood of stagnation. We find little evidence that the effects of these variables differ between the onset and continuation of stagnation spells. Only trade openness has robustly different effects. It substantially reduces the chances of falling into stagnation, but the 'protective' effect of openness vanishes once a country is in a stagnation spell. In addition, we find that stagnation spells exhibit a moderate degree of state dependence, which is consistent with other results in the literature on the duration of growth collapses.

The sequel is organized as follows. Section 2 briefly reviews the literature on institutions and growth, and discusses applications of the growth episodes approach. Section 3 defines stagnation episodes and explores their correlations with GDP levels and institutions. Section 4 describes the variables and data. Section 5 outlines the empirical strategy. Section 6 discusses the results. Section 7 concludes.

2 Related literature

An increasingly large body of literature in economics argues that differences in institutional characteristics are the key to understanding the differences in long-run economic performance of nations. While modern institutional theory has many antecedents, it started from the hypothesis that one explanation for the historical rise of the West is well-developed property rights (e.g. North and Thomas, 1973). Since the 1990s, this literature has been extended to view growth-promoting institutions less narrowly. More recent contributions argue, for example, that institutions for growth are multifaceted (Rodrik, 2000), interact with geography and inequality (Engerman and Sokoloff, 1997), develop semi-endogenously (Greif, 2006), and are embedded in informal arrangements (North, Wallis, and Weingast, 2009).[1]

In terms of econometric evidence, several papers have suggested that differences in institutions explain a large part – if not most – of the cross-country variation in *levels* of GDP per capita.[2] However, many of these studies have also been criticized for their underlying assumptions (e.g. Glaeser, La Porta, Lopez-de Silanes, and Shleifer, 2004) and do generally not establish a link between institutions and growth rates (Crombrugghe and Farla, 2012). Potentially bridging this gap in theory, several authors have recently suggested that there is a link between institutional susceptibility to various external or internal shocks and different growth outcomes. North et al. (2009), for example, identify two distinct types of social orders. Open access orders are economically and politically highly developed, experience relatively smooth patterns of economic growth, have active civil societies, many long-lived organizations, heavily formalized rules, and strong rule of law. Large segments of the population have access to political and economic organizations. Limited access orders, on the contrary, are dominated by elites that exclude large parts of the population from access to economic and political organizations. The rents created in this process are then distributed among members of the ruling coalition, in order to achieve a basic degree of social stability and control over violence. Limited access orders typically experience volatile growth patterns and are characterized by polities without broad democratic consent, few organizations, informal rules, weak and unequally enforced rule of law, insecure property rights, and high levels of inequality.

North et al. (2009) suggest that limited access orders are inflexible and less able to cope with shocks, thus causing a higher propensity towards growth collapses and stagnation. Rodrik (1999) links negative growth experiences to terms of trade shocks, latent social conflict, and the ability of institutions to contain conflict and absorb the destructive potential of such shocks. A key question for this paper is to what extent an empirical analysis of stagnation episodes supports these theories. Therefore, we hypothesize that (a) institutional characteristics play an important role in explaining the onset of stagnation and (b) weak institutions prolong the incidence of stagnation spells.

As Pritchett (1998) pointed out, a problem in traditional panel studies of growth rates is that they focus on average trends over a fixed period, while in reality growth is often erratic and may be contingent on very different growth regimes. This conjecture gave birth to a rapidly growing literature, which since has analyzed growth differentials across decades (Rodrik, 1999), growth accelerations (Hausmann, Pritchett, and Rodrik, 2005),

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[1]For a review of the debates see Bluhm and Szirmai (2012) and for an earlier survey see Aron (2000).

[2]The list of empirical studies investigating this issue is long and growing but the seminal papers are Knack and Keefer (1995), Hall and Jones (1999), Acemoglu, Johnson, and Robinson (2001, 2002), and Rodrik, Subramanian, and Trebbi (2004).

switching among multiple growth regimes (Jerzmanowski, 2006), the duration of growth collapses (Hausmann, Rodriguez, and Wagner, 2008), start and stop growth (Jones and Olken, 2008), real income stagnation (Reddy and Minoiu, 2009), and the duration of growth accelerations (Berg, Ostry, and Zettelmeyer, 2012).

This paper relates most to the studies focusing on negative growth experiences. Rodrik (1999) provides first evidence that growth collapses are linked to terms of trade shocks, latent conflict, and the conflict management capacity of institutions. Hausmann et al. (2008) examine the onset and duration of growth collapses. They mainly find that weak export performance and high inflation coincide with the onset of stagnation, but downturns also occur together with wars, sudden stops, and political transitions. However, most of these factors have little influence on the duration of collapses, which is only correlated with a measure of the flexibility of a country's export basket. Last, Reddy and Minoiu (2009) investigate the incidence of stagnation spells (periods of negative growth) and find that these are correlated with weak export performance, low investment, primary commodity exports, and weak institutions.

The study of stagnation spells and other negative growth episodes is also related to the business cycle literature and the literature on economic crises. Although the focus of this paper is primarily on longer-run growth episodes and not on short-run fluctuations, these literatures provide relevant insights and hypotheses (e.g. see Diebold et al. 1993, on duration dependence, Cerra and Saxena 2008, on post-crisis growth, and Bussière and Fratzscher 2006, on recession probabilities).

Most papers in the growth episodes literature use a methodology that can be summarized in two steps. First, a rule-based or statistical filter is applied to the data to identify single or multiple turning points in the GDP series. If the filter is rule-based, then it often includes a criterion implicitly or explicitly defining the length of the spell. If the filter is statistical, then it may find more than one break in the data and thus lead to the identification of distinct growth episodes. In the second step, correlates of these episodes are examined either by testing for differences in means of potentially correlated variables (before and during), or by estimating probit models. The unexamined assumption in these studies is that factors affecting the onset of an episode are the same as those determining if an episode will continue. Further, most studies of growth episodes take very few measures to account for the possible endogeneity of the included regressors.

3 Growth episodes and long-run growth

3.1 Defining the growth episodes

Our classification of growth episodes is a modification of the approach to growth collapses in Hausmann et al. (2008). We define a *stagnation episode* (or stagnation spell) as an event that begins with a contraction of GDP per capita at a time it was higher than ever before, and ends when GDP per capita is again at or above its pre-stagnation level. We denote (for the purpose of this section) the log of GDP per capita in country i in year t by Y_{it} ($i = 1, \ldots, N; t = 1, \ldots, T_i$). Defined formally, a stagnation episode begins when $Y_{it} < Y_{i,t-1}$ and $Y_{i,t-1} \geq \max_{x=1}^{t-1} Y_{i,x}$, and lasts as long as $Y_{i,t+p} < Y_{i,t-1}$ ($p = 1, 2, \ldots$). When $Y_{i,t+p} \geq Y_{i,t-1}$, the stagnation spell is over. Conversely, we define all years when a country is not stagnating as *expansion* years. In other words, an expansion episode begins the first year a country has left (or has not yet experienced) a stagnation spell and lasts until the beginning of the next stagnation spell.

Apart from being very simple, these definitions have several desirable properties. A completed stagnation episode has a net effect of zero on the level of GDP per capita, since it includes both the downturn and the associated recovery. Conversely, the effect of an expansion episode on the level of GDP per capita is always positive. The definition of expansion explicitly excludes growth that is merely restoring what was lost in past crises, as this growth does not account for long-run increases in GDP per capita. Some commonly used filters, such as the Hausmann et al. (2005) growth accelerations filter, do not make this distinction between recoveries and expansions. Thus some of their growth accelerations include recoveries. (See also Bussière and Fratzscher, 2006, on 'post-crisis bias'.)

An episode has a minimum duration of one year but can actually last for the entire length of the sampled period (1951–2007). Based on this definition, we can identify long stagnation episodes that may include recurring short-run recessions with incomplete recoveries – incomplete in the sense that the maximum level of GDP per capita prior to the crisis has not been recovered. Stagnation episodes thus deliberately subsume many short-run business cycle fluctuations.

We further differentiate expansions and periods of stagnation into two sub-spells each. For stagnation episodes, we distinguish between *crises*, lasting from the beginning of the stagnation episode to the trough, and *recoveries*, lasting from the year after the trough until the end of the stagnation spell. We define the *trough* to occur at the minimum level of output occurring during a stagnation episode. For expansions, we distinguish between *moderate expansions* with an average growth rate up to 5% per annum, and *rapid expansions* with an average growth rate surpassing 5% per annum.[3] In the – rare – case where growth in the first recovery year is so rapid that pre-crisis output is regained in one year, we consider that year part of an expansion, and exclude it from the stagnation spell.

We apply these definitions to GDP per capita data from the *Penn World Table 6.3* (Heston, Summers, and Aten, 2009). Excluding countries with less than one million inhabitants at the latest recorded year as well as countries with less than 20 years of data, we observe 127 countries for a period of at least 20 years between 1950 and 2007. Within this sample, hence before the beginning of the 2007 financial upheaval, we find a total of 578 stagnation episodes, or 3,276 country-years of stagnation.

[Figure 1 about here]

Figure 1 illustrates how our filter works graphically as applied to Angola and France. These two examples are typical for the different growth experiences of developed and developing countries and show that the filter works reasonably well in identifying episodes of interest. While Angola has had many years of positive growth throughout the sample period, we find only three short expansion spells of which only the last is a rapid expansion. Instead, most of the time, Angola was in one protracted stagnation episode lasting from 1975 until the end of 2004, including significant volatility in between. On the contrary, the French economy grew steadily since 1951 and is characterized by protracted periods of moderate expansion, which are only temporarily interrupted by very short stagnation spells. In the light of these two stylized cases, we propose that the incidence

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[3]More precisely, we measure the growth rate across an expansion as: $\bar{g}_{t,t+q} = q^{-1}[\ln Y_{i,t+q} - \ln Y_{it}]$, where q is the duration of the expansion. We classify an episode as rapid if $\bar{g}_{t,t+q} > 0.05$, and slow to moderate if $\bar{g}_{t,t+q} \leq 0.05$.

of stagnation spells may explain a large part of the difference in long-run levels of GDP per capita.

3.2 Growth profiles

Before focusing on the dynamics of moving in and out of stagnation spells, we first take a more detailed look at the distribution of growth episodes across countries and time. Do developing countries spend more of their time in crisis or stagnation than advanced economies? Are they more prone to experience crisis and stagnation? Using the previously defined growth episodes, Table 1 addresses these issues in more detail.

[Table 1 about here]

Table 1 groups the relative incidence of each type of growth episode from 1951 to 2007 by quartiles of GDP per capita in 2007. The table shows that low-income countries spend most of their time in stagnation, upper middle-income countries almost half the time and high-income countries only about a quarter. In other words, this suggests that the different propensity to experience stagnation spells is closely linked to development outcomes today. Further, using the finer classification of four distinct growth episodes – crisis, recovery, expansion, rapid expansion – we find that a high proportion of time spent in crises at low income and lower-middle income levels is driving this relationship. Once we exclude recoveries from the positive growth experiences, there is little indication that lower income countries experience rapid growth more frequently than high income countries during expansions (as unconditional convergence would imply). In fact, the opposite seems to be the case. While countries in the lowest income group spend relatively more of their expansions growing rapidly ($10.21/22.12 \approx 46.15\%$), upper-middle income and high-income countries spend more time growing rapidly in total. Table 1 confirms that currently poor countries have experienced fewer years of positive growth than rich countries. It contradicts the assertion that once poor countries grow, they do so more rapidly than the rich.[4]

As mentioned earlier, North et al. (2009) suggest lasting institutional differences between limited access orders and open access orders as one possible explanation for the lack of generalized convergence among economies. Developing countries with limited access orders are less adaptive, less able to adjust to various external and internal shocks, and more prone to economic crises and stagnation.

[Table 2 about here]

Table 2 links the conjecture of North et al. (2009) and similar theories to the approach developed in this paper by cross-tabulating the different growth episodes with two indices of institutional characteristics; namely, institutional formalization of regulations and degree of control and intervention by the state. These indices are derived from Crombrugghe and Farla (2012), who aggregate a large number of indicators from the Institutional Profiles Database (IPD) 2009 using principal components analysis.[5]

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[4]We constructed a similar table classifying countries by their GDP per capita in 1960 rather than at the end of the period. Though there were some differences, the basic finding that rich countries spend less of their time in crisis than poor countries is confirmed.

[5]For more details on the construction of the indices see Crombrugghe and Farla (2012). The IPD 2009 is publicly accessible at www.afd.fr/lang/en/home/recherche/bases-ipd.

Similarly to the income classification used before, we group the scores on each component into quartiles ranked from low to high. The upper panel in Table 2 shows the results for the first component and the lower panel the results for the second.

There is a moderate negative correlation (about -0.5 for 2007) between the index of institutional formalization of regulations and the incidence of stagnation episodes. The countries belonging to the highest quartile on this index are in stagnation less than 25% of the sample period, while those ranked in the lowest quartile stagnate almost 70% of the time. In many ways these results resemble those using income groups. For example, fast expansions occur more frequently in the upper middle quartile and crises occur gradually less often at higher quartiles of the index. This suggests that higher institutional formalization of regulations is associated with fewer stagnation spells and increasingly steady growth. However, there is a strong correlation (about 0.8) between GDP per capita and the formalization index, so the direction of causality remains indeterminate.

The bottom panel of Table 2 gives a more differentiated picture. The second principal component, which can be interpreted as the degree of the state's involvement in the private economy but also as its degree of authoritarianism, is associated with more frequent stagnation spells. The lowest incidence of stagnation spells (31.83%) occurs within the group of countries scoring in the lower middle quartile of the index, whereas countries in the highest quartile stagnated during nearly 70% of the sample period. As Crombrugghe and Farla (2012, p. 17) point out, "Western European countries, the USA, Canada, and Australia are at neither extreme of the [index]", which suggests that very low scores represent weak states and very high scores represent mostly authoritarian regimes. This explains why the most stable growth profile is located in the lower middle quartile rather than at either end of the spectrum.

This brief overview of different growth episodes between 1950 and 2007 highlights two points. First, the incidence of stagnation spells is much higher in lower and middle income countries than in high income countries. Second, weak institutions and especially a lack of formalized rules and regulations could be driving less steady growth and more frequent stagnation, but this aspect requires further analysis.

4   Explanatory variables

In the preceding section, we have reported how we defined and obtained our sample of stagnation spells and examined their distribution. In this section, before we start modeling the incidence of stagnation, we briefly outline the sources for and construction of explanatory variables. These broadly belong to two categories: macroeconomic indicators and variables describing political institutions as well as external or internal shocks to these institutions. Table A.1 in the Appendix provides an overview.

Macroeconomic variables: We include a range of variables that are typically associated with sound macroeconomic management. Most of these variables have been found to significantly affect growth performance in traditional panel studies using annual, quinquennial (5-year) or decennial (10-year) growth rates.

In order to control for the level of development, we include the lagged log of GDP per capita ($Log\ GDPC_{(t-1)}$) in nearly all models. Its expected effect is negative, considering that richer countries tend to experience fewer and shorter stagnation spells. Controlling for the level of GDP also serves a practical purpose. As indicated in the previous section,

indices measuring the quality of institutions and GDP are strongly correlated, so that including both will avoid erroneously attributing effects of one to the other.

Maintaining price stability is a core task of central banks and its importance is emphasized in the related literature (e.g. Berg et al., 2012). We expect high inflation to be positively correlated with the onset of stagnation spells. However, for continuation of stagnation the role of inflation is likely to be ambiguous as it could be instrumental – together with the exchange rate – in bringing about devaluation and regaining competitiveness. Our measure of inflation is 100 times the log of 1 plus the annual inflation rate. This measure is close to the actual inflation rate when that rate is small, but also reduces the influence of larger values (e.g. rare periods of hyperinflation). The annual inflation data is from the IMF's *International Financial Statistics* (IFS), complemented with data from the *World Development Indicators* (WDI) whenever the former is missing.

We also measure whether the exchange rate is overvalued or undervalued in real terms. Recent research finds that depreciations are beneficial for growth accelerations (Hausmann et al., 2005) and stimulate growth in general (Rodrik, 2008). This positive effect may operate through many channels, but is most commonly linked to export-led growth and the relative price of manufactured products. On the negative side, abrupt movements of the exchange rate can also be an omen of excessive volatility and an upcoming currency crisis. If the positive consequences are dominant, exchange rate undervaluation may diminish the likelihood of stagnation spells. To capture this effect, we follow Rodrik (2008) in constructing an index of exchange rate undervaluation $(RER\ Value_{(t-1)})$.[6] The index is centered at zero, with higher values indicating exchange rate undervaluation and lower values indicating overvaluation.

We include two measures of trade performance. First, we measure the price of exports relative to imports, the terms of trade $(\Delta\ ToT_{(t-1)})$, as the annual log difference in the net barter terms of trade from the WDI, and supplement this series with data from the IFS when there are gaps in the WDI series. Terms of trade growth, declines and shocks have been linked to growth collapses (Rodrik, 1999; Hausmann et al., 2008), accelerations (Hausmann et al., 2005), and the premature end of fast growth spells (Berg et al., 2012). Second, we also estimate the effects of changes in the value of real merchandise exports $(\Delta\ Real\ Exports_{(t-1)})$, which we measure as the annual log difference in the exports volume index from the WDI, appended with data from the IFS to extend coverage. Growth in real exports has been found to significantly reduce the probability of the onset of a stagnation spell (Hausmann et al., 2008). We examine if this is also the case in the presence of dynamics.

Further, the growth literature has identified *de jure* financial and trade openness as two key policy variables that positively influence growth outcomes. To account for the former, we include the Chinn and Ito (2006) index of financial openness $(Fin.\ Openness_{(t-1)})$. This index is the first principal component of the inverses of four variables measuring restrictions on external accounts, based on the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions* (AREAER). To account for the latter, we use a dummy measure for economic liberalization $(Trade\ Openness_{(t-1)})$ developed by Sachs and Warner (1995) and extended by Wacziarg and Welch (2008). This indicator is coded as one in years a given country is completely open to trade and zero otherwise. While the index's authors have linked their respective measures to average growth rates, the growth episodes literature has found financial openness to precede growth accelerations (Hausmann et al., 2005) and trade liberalization to reduce the risk

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[6]This index is based on the PWT 6.3 and its construction is described in more detail in Table A.1.

that a fast growth spell ends (Berg et al., 2012). Financial liberalization can lead to both increasing capital inflows and financial deepening but also enable capital flight and generally volatile capital flows. The sign of its effect is not clear ex ante. On the contrary, we expect trade openness to unequivocally reduce the probability of stagnation.

Last, we include a measure for income inequality after taxes and transfers ($Inequality_{(t-1)}$). Net income inequality is not only an economic variable but just as much influenced by a country's political institutions. Most of the growth episodes literature does not systematically analyze the role of inequality, with the exception of an early study by Rodrik (1999) and recent evidence of a negative effect on the length of positive growth spells (Berg et al., 2012). In panel studies the effect of inequality on average growth rates remains disputed.[7] However, parts of neo-institutional theory (Engerman and Sokoloff, 1997) and earlier work on the interaction of inequality and growth collapses (Rodrik, 1999) suggest a negative sign for inequality, while earlier theories suggests that inequality rises alongside rapid development and falls again at higher income levels (Kuznets, 1955). Our data for net income inequality is taken from Solt (2009), who appends, benchmarks, and standardizes data from UNU-WIDER's *World Income Inequality Database* (WIID).

Institutional and 'shock' variables: This set of variables aims to capture some observable cross-country heterogeneity that can be attributed to institutions, as well as various shocks which require a response from policy makers and economic actors within the constraints of the political and institutional structure. These shocks may be external or internal but have in common that they pose a challenge to the prevailing regime and/or a country's institutional set-up.

Cross-sectional studies of GDP levels find strong support that institutions explain large parts of long-run growth (Knack and Keefer, 1995; Hall and Jones, 1999; Acemoglu et al., 2001, 2002) and also provide evidence that growth-enhancing institutions (e.g. property rights or executive constraints) contribute to lower growth volatility (Acemoglu et al., 2003). We expect that more open and democratically constrained institutions[8] will reduce the probability of experiencing a stagnation spell. Our measure of political institutions is the revised combined polity score ($Polity2_{(t-1)}$) from the Polity IV project (Marshall and Jaggers, 2010). This measure is the difference between a country's score on the aggregate *institutionalized democracy* index and the score on the *institutionalized autocracy* index coded by the Polity IV project. It has a range from −10 (hereditary monarchy) to +10 (consolidated democracy). For studies requiring time-series, the Polity IV data is uniquely suitable as it provides annual data, starting as early as 1800.

From Polity IV, we also derive two additional measures of political shocks. We code a dummy for positive regime changes ($Regchange +_{(t-1)}$) as major positive changes of the political structure identified by at least a three-point improvement in the polity score. Conversely, we code negative regime changes ($Regchange -_{(t-1)}$) as a negative change of at least three points in the polity score, including interregna and state failure. We expect negative regime changes to increase the probability of stagnation, while positive regime changes may have a stagnation deferring effect. Yet, regime transitions in general may also be a sign of political instability or consolidation of power.

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[7]See Perotti (1996), Deininger and Squire (1998), Barro (2000), and Forbes (2000).

[8]Polity IV scores countries on five indices capturing the openness of the political process and the constraints placed on individual actors. We interpret the data as measuring the degree of open institutions with narrow mandates.

We also include a dummy for the irregular exit of leaders (*Leader Exit*$_{(t-1)}$) based on *Archigos 2.9* (Goemans, Gleditsch, and Chiozza, 2009) as a proxy for internal shocks to a country's political regime. This variable codes an irregular exit whenever a country's major leader (president, chancellor, dictator, and so forth) lost power by means violating established rules and conventions. Such cases include, but are not limited to, the loss of power due to the removal by foreign intervention, assassinations, ill health, and domestic popular protest with foreign support. We focus on leader exit and not entry, as our aim is to link periods of stagnation to unexpected adverse events and not to their possible resolution. Some studies of growth accelerations have accounted for the sudden death of leaders in office but usually not other types of exit (Hausmann et al., 2005; Jones and Olken, 2008).

In order to investigate the impact of large scale violence on stagnation spells, we include a dummy for the occurrence of $War/Conflict_{(t-1)}$ based on the UCDP/PRIO *Armed Conflict Dataset v.4-2011* (Gleditsch et al., 2002). We expect countries that are the location of an interstate war or large civil strife to be especially prone to falling into a stagnation spell. Our measure codes a war if UCDP/PRIO records a conflict intensity of two or higher, corresponding to at least 1,000 battle-related deaths in a country-year, and if the country is recorded as a location of war. In the case of multiple conflicts, we select the conflict with the highest intensity.

5   Empirical strategy

Most extant studies of growth episodes use pooled probit or non-linear panel methods to study the *onset* of specific growth episodes, such as accelerations or collapses. We know of only two papers concerned with modeling the duration of growth spells using observations within the episode (Hausmann et al., 2008; Berg et al., 2012). Other studies exploit only part of the data, either retaining only the first observation of the episodes of interest, or comparing averages of covariates before and after a regime change (e.g. Jones and Olken, 2008). However, we are unaware of any theoretical justification for assuming that determinants of stagnation explain only its onset and not its continuation. Apart from the obvious loss of efficiency, the neglect of information from within episodes also brings with it the risk of rare event bias (King and Zeng, 2001).

In fact, state dependence is likely be a crucial feature of growth episodes in general and of stagnation spells in particular. Yet, apart from Jerzmanowski's (2006) Markov-switching models, the literature does not model the incidence of a growth spell as a dynamic process. The likely explanation is that, in dynamic non-linear models, special efforts are required to address econometric issues such as *spurious* state dependence, endogeneity of the lagged state, unobserved heterogeneity, the initial conditions problem, and non-linear interaction effects.

We propose to phase the issues and first introduce dynamics in a generic linear model of stagnation episodes. This generic model will be used for an initial screening and selection of potential explanatory variables. The linear estimates will also be used to provide initial variance estimates for more complex nonlinear models. Ultimately, linear estimates remain interesting as they are much less demanding in terms of statistical assumptions than the potentially more efficient nonlinear models, and they provide robust benchmark estimates.

For ease of exposition, we write the general model in index form, leaving the functional

form unspecified.

$$y_{it} = \mathbf{1}[\alpha y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta} + \mathbf{x}'_{it}y_{i,t-1}\boldsymbol{\gamma} + \mu_i + \nu_{it} > 0], \qquad i = 1, \ldots, N, \ t = 2, \ldots, T_i \quad (1)$$

where y_{it} indicates whether or not a country i is stagnating in year t, $y_{i,t-1}$ is the lagged state, $\mathbf{x}_{it}$ is a vector of covariates, $\boldsymbol{\alpha}$, $\boldsymbol{\beta}$ and $\boldsymbol{\gamma}$ are parameters to be estimated, μ_i is a time-invariant unobserved country effect, and ν_{it} a residual time-varying error. The interaction term $(\mathbf{x}'_{it}y_{i,t-1}\boldsymbol{\gamma})$ allows the impact of the covariates to be different in (or just after) a stagnation spell than in expansions. For now, the unobserved effects may be of the 'fixed' or 'random' variety: nothing is assumed about their distribution or the absence of correlation with the explanatory variables. In most specifications, we will also include quinquennial dummies among the regressors though not their interactions with $y_{i,t-1}$.

In the linear case, the elements of the parameter vector $\boldsymbol{\gamma}$ can be interpreted as slope shifts in the effects of the variables in $\mathbf{x}_{it}$ if a country is or has just been in a stagnation spell (i.e. $y_{i,t-1} = 1$), while mean shifts are captured by α. This model allows us to test the hypothesis that specific elements or subsets of $\boldsymbol{\gamma}$ are equal to zero. Macroeconomics and political economy theory provide little guidance as to whether and how some effects should differ in the initial and later stages of a stagnation episode. Our modeling strategy is to "let the data decide" which variables in $\mathbf{x}_{it}$ require an interaction term with $y_{i,t-1}$ and which do not. We proceed in four steps. First, we estimate fully interacted linear probability models specified according to equation (1). Second, we test whether the interaction terms that are individually insignificant at the 10% level may also be considered jointly insignificant. Third, based on these tests, we specify a 'parsimonious' reference model retaining only those interactions that pass our inclusion criteria. Fourth, we compare the preceding results with those found using non-linear probability models.

5.1 Linear probability models

Simplest of all is to estimate equation (1) as a linear probability model (LPM) with country fixed effects (FE). This approach is particularly attractive, as FE-OLS requires no distributional assumptions about the unobserved effects and the OLS coefficients are usually a good approximation of the partial effects near the means of the variables. However, the variance of the dependent variable is known to be of the form $\mathbf{x}'_{it}\boldsymbol{\beta}(1-\mathbf{x}'_{it}\boldsymbol{\beta})$, making the LPM inherently heteroskedastic. Furthermore, the LPM can predict probabilities outside the unit interval and hence non-positive variances, unless the predictions are arbitrarily trimmed.

In model (1), the LPM has two further shortcomings. First, it implies awkward restrictions on the unobserved effects. Second, since the same unobserved effects (μ_i) also appear in the lagged state $y_{i,t-1}$, their presence means that the 'Within' OLS estimate of α is downward biased (Nickell, 1981). The 'Nickell bias' is decreasing in T. Our panel has an average length of about 18 years; therefore the bias should be moderate.

For comparison, we also estimate equation (1) using differenced and system Generalized Methods of Moments (GMM) estimators. Differenced GMM uses lagged levels of order two and higher as instruments for the endogenous regressors in a differenced equation (Holtz-Eakin et al., 1988; Arellano and Bond, 1991). To alleviate problems of weak instrumentation, Arellano and Bover (1995) and Blundell and Bond (1998) proposed the 'system GMM' estimator that instruments levels with lagged differences and estimates the equation both in differences and in levels simultaneously. Under the appropriate

conditions system GMM is consistent and more efficient than differenced GMM. The extra moment conditions require that the lagged differences are not correlated with the unobserved effects. In addition, system GMM requires that the initial conditions (y_{i1}) represent a stationary equilibrium, which is arguably an unnatural assumption for the analysis of stagnation spells. We apply two-step system GMM with a small sample correction due to Windmeijer (2005).

System GMM is not a universal panacea. Bun and Windmeijer (2010) recently showed that the level equation in system GMM also suffers from a weak instruments problem. Differenced or system GMM estimates are also often unstable and strongly depend on the instrument matrix used (Roodman, 2009). For this reason, we do not use GMM for model selection but only apply it to the 'parsimonious' specification to check whether the results remain within a reasonable range of the FE-OLS estimates.

5.2   Non-linear probability models

Simple within or first-difference transformations cannot eliminate unobserved heterogeneity in non-linear probability models like logit and probit models, and the assumptions made on the structure of the unobserved effects determine which type of model can be estimated. We apply two techniques: fixed-effects logit and dynamic random-effects probit. On the one hand, the fixed-effects logit estimator is less restrictive in its assumptions about the unobserved heterogeneity but similarly to the LPM with FE, it does not deal with the endogeneity of the lagged state. On the other hand, the dynamic random effects probit estimator requires explicit assumptions about the unobserved heterogeneity, but has been modified to account for the endogeneity of the lagged state, including solutions for the initial conditions problem.

Fixed-effects logit: The dummy variables fixed-effects logit model estimated by unconditional Maximum Likelihood (ML) runs into a statistical problem. Even in a simplified version of equation (1) without the lagged state, we need a consistent estimate over $t = 1, ..., T_i$ for each of the unobserved effects μ_i. Any inconsistency introduced there will contaminate the estimate of $\boldsymbol{\beta}$. This is the well-known *incidental parameters problem* which, for balanced panels, creates a bias in the ML estimator of $\boldsymbol{\beta}$ in the order of $1/T$ (Neyman and Scott, 1948). Given the lengths of our series, we do not expect the bias to be large.

Chamberlain (1980) observed that there is a computational trick that allows consistent estimation of the parameter vector but not the constants by conditioning on the sum of observed outcomes *within* groups $(\sum_{t=1}^{T_i} y_{it})$. In the conditional logit model the incidental parameters (μ_i) drop out. However, conditioning on the sum of the observed outcomes comes at a cost. Since groups in which y_{it} does not change over T_i provide no information for the likelihood, they drop out of the log-likelihood. If there is strong persistence the number of observations used in the estimation may fall a lot. Likewise, time-invariant effects cancel out of the estimation equation. They cannot be estimated, neither can partial effects since these depend on the expected value of the unobserved effects.

Given the expectation that the unconditional ML estimator is not too strongly biased and allows estimation of partial effects, we estimate both models and compare their results.

Dynamic random effects probit: Conditioning on the sum of the observed outcomes does not work for the equivalent probit model. Furthermore, the standard random effects probit model assumes that the unobserved heterogeneity is strictly unrelated to the explanatory variables. The presence of the lagged state ($y_{i,t-1}$) together with μ_i violates this assumption even if α is actually zero (Wooldridge, 2010, p. 626). This is the problem of *true* versus *spurious* state dependence. The estimated effect of $y_{i,t-1}$ depends on three sources: (1) serial correlation in the errors, (2) correlation with the unobserved effects, and (3) true state dependence (Greene, 2011, p. 769). In such a setting, the ordinary fixed or random effects estimation techniques do not provide consistent estimates. In addition, the outcome path may be severely influenced by the initial conditions (y_{i1}), which enter the unconditional likelihood function and prohibit integrating out the unobserved effects (μ_i). We are faced with two related problems: the violation of strict exogeneity in the presence of unobserved heterogeneity, and the treatment of the initial condition in the log-likelihood.

A relaxation of the strict exogeneity assumption known as *correlated random effects* has been developed for static models following the ideas of Mundlak (1978) and Chamberlain (1984). The approach allows for correlation between $\mathbf{x}_{it}$ and μ_i but restricts the unobserved effects to depend on means (or other values) of the explanatory variables according to $\mu_i = \eta_0 + \bar{\mathbf{x}}_i'\boldsymbol{\eta}_2 + \varepsilon_i$, where the ε_i are assumed to be i.i.d. and normally distributed. The vector $\bar{\mathbf{x}}_i$ consists of time-averages (or other values, like initial levels) of the regressors $\mathbf{x}_{it}$.[9] While this approach relaxes the strict exogeneity assumption, it does not address the issue of dynamics and the related initial conditions problem.

Several solutions to the initial conditions problem have been proposed (see Heckman, 1981; Orme, 2001; Wooldridge, 2005). Wooldridge (2005) suggests to condition the density $(y_{i1}, ..., y_{iT_i})$ on the observed history of the covariates *and* the initial values by specifying a distribution of the unobserved effects given the initial conditions. Rabe-Hesketh and Skrondal (2013) show that this simple method performs well in general and Akay (2012) provides Monte Carlo evidence that a constrained model (with time-averages) also works well in unbalanced panels as long the time series lengths are moderately large.

Concretely, Wooldridge (2005) proposes to condition on the entire time series of the strictly exogenous variables less the initial period plus the initial condition (y_{i1}) – an approach that easily extends to interactions. A convenient way to specify the conditional distribution of the unobserved effects is $\mu_i|y_{i1}, \bar{\mathbf{x}}_i \sim \mathcal{N}(\eta_0 + \eta_1 y_{i1} + \bar{\mathbf{x}}_i'\boldsymbol{\eta}_2 + \bar{\mathbf{x}}_i'y_{i1}\boldsymbol{\eta}_3, \sigma_\varepsilon^2)$, where $\bar{\mathbf{x}}_i$ are the time-averages as in Mundlak-Chamberlain. This implies the following parametric specification for the unobserved effects: $\mu_i = \eta_0 + \eta_1 y_{i1} + \bar{\mathbf{x}}_i'\boldsymbol{\eta}_2 + \bar{\mathbf{x}}_i'y_{i1}\boldsymbol{\eta}_3 + \varepsilon_i$, where the ε_i are normal, i.i.d., and independent of $(y_{i1}, \bar{\mathbf{x}}_i)$.

As a final model, we specify the constrained probit version of equation (1):

$$
\begin{aligned}
P(y_{it} = 1 | \mathbf{x}_{i2}, \ldots, \mathbf{x}_{iT_i}, y_{i,t-1}, \ldots, y_{i1}, \varepsilon_i) = \\
\Phi(\alpha y_{i,t-1} + \mathbf{x}_{it}'\boldsymbol{\beta} + \mathbf{x}_{it}'y_{i,t-1}\boldsymbol{\gamma} + \eta_0 + \eta_1 y_{i1} + \bar{\mathbf{x}}_i'\boldsymbol{\eta}_2 + \bar{\mathbf{x}}_i'y_{i1}\boldsymbol{\eta}_3 + \varepsilon_i)
\end{aligned}
\tag{2}
$$

where $\mathbf{x}_{it}$ is the vector of explanatory variables, $y_{i,t-1}$ is the lagged state, $\bar{\mathbf{x}}_i$ is the vector of time-averages of the covariates, the vector $\boldsymbol{\gamma}$ allows for differential effects of the covariates depending on the state ($\mathbf{x}_{it}y_{i,t-1}$), η_1 measures the effect of the initial condition (y_{i1}), and the vector $\boldsymbol{\eta}_3$ measures the effect of the averaged covariates depending on the initial condition ($\bar{\mathbf{x}}_i y_{i1}$). Estimation still proceeds over $i = 1, ..., N$ and $t = 2, ..., T_i$.

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[9]Further, this model implies $\text{Corr}(\varepsilon_i + \nu_{it}, \varepsilon_i + \nu_{is}) = \sigma_\varepsilon^2/(\sigma_\varepsilon^2 + \sigma_\nu^2)$ for any $t \neq s$.

Appendix B outlines why the average partial effects (APEs), which are not identified in the conditional logit model, are in fact identified in this random effects probit model. This model addresses all three fundamental issues involved in our research problem. It specifies a data-coherent functional form, relaxes the strict exogeneity assumption, and consistently estimates the APE of time-varying variables and the lagged state variable in the presence of unobserved effects.

Partial effects of interactions: It is well known that the partial effects in non-linear models are not constant and that the model coefficients only indicate the direction and approximate significance of the effects. A somewhat less well known fact is that non-linearity necessarily implies that the coefficients of the interaction terms do not represent the sign, size or significance of the underlying interaction effect (e.g. see Ai and Norton, 2003; Greene, 2010). Let $F(\mathbf{w}_{it}; \boldsymbol{\theta}) = F(\alpha y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta} + \mathbf{x}'_{it}y_{i,t-1}\boldsymbol{\gamma} + \mu_i)$ denote a generic non-linear version of our interaction model with an implicit error, and let hats indicate estimated values. The change in the partial effect of $x_{k,it} \in \mathbf{x}_{it}$ due to a regime switch into stagnation ($\Delta y_{i,t-1} = 1$) is

$$\text{PE}(\hat{\gamma}_k)_{it} = \Delta F'_k(\mathbf{w}_{it}; \hat{\boldsymbol{\theta}}) = F'_k(\mathbf{w}_{it}; \hat{\boldsymbol{\theta}}|y_{i,t-1} = 1) - F'_k(\mathbf{w}_{it}; \hat{\boldsymbol{\theta}}|y_{i,t-1} = 0) \qquad (3)$$

where $F'_k(\mathbf{w}_{it}; \hat{\boldsymbol{\theta}})$ can denote either a partial derivative with respect to $x_{k,it}$ or a difference, depending on whether $x_{k,it}$ is discrete or continuous, and in either case we write $F'_k(\mathbf{w}_{it}; \hat{\boldsymbol{\theta}}) = \partial F(\mathbf{w}_{it}; \hat{\boldsymbol{\theta}})/\partial x_{k,it}$.

Clearly, $\hat{\gamma}_k$ is not equal to the interaction effect as it would be in a linear model. Now it is straightforward to also compute the APE for either $\Delta y_{i,t-1} = 1$ or $\Delta y_{i,t-1} = -1$ as

$$\text{APE}(\hat{\gamma}_k) = \frac{1}{N(\bar{T}-1)} \sum_{i=1}^{N} \sum_{t=2}^{T_i} \frac{\Delta F'(\mathbf{w}_{it}; \hat{\boldsymbol{\theta}})}{\Delta y_{i,t-1}} \qquad (4)$$

where $\bar{T}$ is the average panel size.

We compute the standard errors of the APEs using the delta method, so that the asymptotic variance of the APE of an interaction with the lagged state is

$$\widehat{\text{AVar}}[APE(\hat{\gamma}_k)] = \frac{\partial \text{APE}(\hat{\gamma}_k)}{\partial \hat{\boldsymbol{\theta}}'} \widehat{\boldsymbol{\Omega}} \frac{\partial \text{APE}(\hat{\gamma}_k)}{\partial \hat{\boldsymbol{\theta}}} \qquad (5)$$

and $\widehat{\boldsymbol{\Omega}}$ is the ML estimate of the asymptotic covariance matrix of $\hat{\boldsymbol{\theta}}$. For the observation-specific partial effects the Jacobian vectors are not averaged.

6 Results and discussion

6.1 Linear models

Table 3 reports the results from variations of the LPM. Since we primarily use the linear models for variable selection, we only briefly discuss the results and defer the economic interpretation until the description of the preferred specification in the next subsection. Column (1) is the standard fixed-effects model with standard errors clustered at the country level. Column (2) is similar but allows for arbitrary heteroskedasticity and autocorrelation within both country and time clusters (Cameron, Gelbach, and Miller,

2011). Column (3) shows the FGLS estimator with clustering at the country level via the weights. Column (4) is the parsimonious FGLS specification discarding those interaction terms that are individually and jointly insignificant. Columns (5) and (6) re-estimate (4) using system GMM.

[Table 3 about here]

Two joint hypothesis tests reported in Table 3 are key to our model-building approach. First, we test if the coefficients of the interactions with the lagged state and the coefficient of the lagged state are jointly zero; taken together these variables comprise Set I. In all models this hypothesis is rejected, indicating the presence of dynamics. Second, we test whether the interactions with the lagged state that are individually insignificant at the 10% level (i.e., Set II) can also be jointly omitted. In columns (1) to (4), the insignificant interactions pass this joint-exclusion test, but not in the GMM estimates.

All fully-interacted specifications provide very similar results. Inflation, financial openness, trade openness, and negative regime changes clearly affect the probability of entering a stagnation spell. Considering the interactions, the coefficients of inflation, trade openness, and negative regime changes are significant in most but not all specifications. Interestingly, the interaction terms often have the *opposite* sign of the non-interacted coefficients, indicating that the respective effects are weaker within a stagnation spell. Column (4) only retains the significant interactions and represents our parsimonious specification which we later re-estimate with non-linear techniques. We still find evidence that inflation, trade openness, and negative regime changes have a different impact within the spell than on the onset probabilities. However, the interaction effect of financial openness is, again, insignificant.

The system GMM specifications in columns (5) and (6) asses the stability of the parameter estimates once we account for the endogeneity of the lagged state and the interactions with the lagged state. While the significance of individual coefficients changes between the two GMM models, the sizes and signs of the previously highlighted coefficients remain broadly similar. In column (5) we use the second and third lag of the predetermined variables as instruments. Nevertheless, the J-test indicates that the number of instruments is too large relative to the group size. We address this concern in column (6) by collapsing the instrument set (see Roodman, 2009). There is no evidence of instrument endogeneity and the estimated parameters come closer to the earlier least-squares estimates. However, applying GMM in our context is not ideal. A moderate $\bar{T}$ quickly leads to instrument proliferation and problems in identifying an instrument set that balances efficiency gains with decreasing relevance. Accordingly, we place less emphasis on the GMM results and rely more on verifying the results from the preceding linear models with non-linear techniques.

The linear models point to several preliminary conclusions. First, we find that inflation, financial openness, trade openness, and negative regime changes have a statistically significant effect on the probability of stagnation, and so do to a lesser extent exchange rate undervaluation and inequality. Second, state dependence plays a large role in determining whether a country experiences a stagnation episode or not. Third, in all specifications there is considerable evidence of unobserved heterogeneity at the country level. Fourth, all significant interaction terms with the lagged state point into the direction opposite of their linear counterparts.

6.2 Non-linear models

Turning to the non-linear models, we first discuss the heterogeneity of the interaction effects using a logit specification and then present the results from the dynamic random effects probit estimator (our preferred specification).

Table 4 shows the estimation results from two types of fixed-effects logit estimators. As a reference, column (1) shows the fully interacted model estimated using conditional maximum likelihood, where the fixed-effects are not estimated but drop out. Column (2) is the conditional logit equivalent of the parsimonious linear model, and column (3a) is the same model using dummy variables for the country fixed effects. The last column (3b) reports the APEs based on column (3a). The APEs of interaction terms are reported similarly to coefficients of a linear model; that is, if $y_{i,t-1} = 0$, the APE is reported in the row of the non-interacted variable and, for $y_{i,t-1} = 1$, the total APE is the sum of the former and the APE of the interaction term.

A complication in interpreting the results of the conditional logit models is that the APEs are not identified, for the simple reason that the individual country effects are not estimated. In order to approximate the APEs, we estimate the equivalent dummy variables fixed-effects logit model and compute the APEs based on its results. Columns (2) and (3a) show that the parameter estimates and the corresponding standard errors remain very similar, justifying this approach.

[Table 4 about here]

We still find evidence that inflation, financial openness, trade liberalization, and negative regime changes significantly affect the probability of stagnation. However, for all but negative regime changes, the APEs of the interaction terms are *statistically insignificant* and in the case of financial openness the interaction effect no longer points in the opposite direction. Applying fixed-effects logit also substantially reduces the estimation sample to 1314 observations in 62 countries, for lack of within-group variation. This loss of observations makes it impossible to identify the effect of the negative regime change interaction term; as a consequence, the interaction is dropped from the earlier parsimonious model.

The APEs are close to the linear approximation in terms of size. Notable changes are that now the effects of changes in the terms of trade, real exchange rate (RER) undervaluation, and inequality are all significant at the 10% level and have increased substantially in absolute size. The degree of state dependence identified by the logit model is also somewhat higher than in the linear approximation (32.1 percentage points higher probability of continuation if a country was stagnating the year before).

Table 4 may seem to provide little evidence that some variables have a different impact on the onset of a stagnation spell than on its continuation. However, in non-linear models, the partial effects of the interaction can be very heterogeneous at the observation level and may be individually significant for a substantial subset of observations. By definition, APEs 'average out' this type of heterogeneity which can be especially troublesome if the partial effects switch signs. In Figure 2 we examine these non-linearities more closely. The figure graphs the distributions of partial effects for the interaction terms of inflation, financial openness, and trade openness over the predicted probability of stagnation, as well as the associated p-values of a Wald-test of the null that the interaction effect at each particular observation is zero.

[Figure 2 about here]

Clearly, the partial effects of all three interaction terms are extremely non-linear and tend to include both positive and negative values. In the case of inflation, we find that for some observations the partial effect is positive, while for most observations it is negative and insignificant. The effect is significant only at negative values for a very small fraction of the distribution (30 observations) and ranges from -0.95% to 0.39%, which is moderately large compared to an APE of 0.6% when $y_{i,t-1} = 0$. The predicted partial effects group into two families of curves with an S-shape. The curves at lower stagnation probabilities are the partial effects for observations where $y_{i,t-1} = 0$ (symbol: o) and the curves going across higher stagnation probabilities are the predicted effects for observations where $y_{i,t-1} = 1$ (symbol: x). The dashed line refers to the APE in the upper panel and the p-value of a Wald test that the APE is zero in the lower panel.

The interaction effect for financial openness is also S-shaped but sloping upwards and ranges from -6.9% to 7.6%. The partial effect is statistically significant for 14% of the predicted outcomes at both negative and positive values. However, similarly to inflation, the large range of insignificant negative and positive values supports the conclusion that the effect is not different from zero on average. In contrast, the evidence of a significant interaction is relatively strong for trade openness. For most observations the partial effect of the interaction term is positive and very large, with an overall range from -3% to 30%. For about 32% of the observations in the sample the partial effect of the interaction term is individually significant at the 5% level.

Overall, the logit specifications confirm the findings of the linear models given a few refinements. Inflation, financial openness, trade openness, and negative regime changes remain significant predictors of being in stagnation. Further, the estimates suggest that exchange rate undervaluation, changes in the terms of trade, and inequality affect the probability of stagnation. However, the evidence in favor of interaction effects with the lagged state for any other variable than trade openness is weak; the estimates suggest that only trade openness has a different impact on the onset of a stagnation spell than its continuation.

We now turn to the last and final set of estimates. Table 5 presents the random effects probit estimates of the dynamic panel model. For comparison purposes, we first report the naïve pooled probit version of the fully-interacted dynamic model and then its random effects counterpart estimated according to the Wooldridge approach – columns (1) and (2), respectively. Column (3a) is parsimonious version of the dynamic random effects model in column (2) and our preferred specification. Column (3b) reports the APEs of the variables of interest and their associated standard errors.

[Table 5 about here]

We report several additional rows in Table 5 to display the regression results more succinctly. The row "$\boldsymbol{\eta}_2$-estimated?" refers to whether the Mundlak-Chamberlain time-averages are included. As these are not interesting by themselves, we do not report their coefficients and partial effects. Similarly, the row "$\boldsymbol{\eta}_3$-estimated?" reports whether the interaction terms are included twice, once as an interaction with the lagged state and once as an interaction with the initial condition. We also do not report the parameter estimates of the latter, as they primarily account for the non-random nature of the unobserved effects.

The random effects probit model gives different (and superior) estimates compared

to the pooled probit version.[10] Several parameters exhibit sign changes and substantial changes in significance levels. More interestingly, even though the Wooldridge estimator of the dynamic model includes several additional terms and requires the assumption that the regressors are only correlated with the unobserved heterogeneity through their averages, the results are remarkably similar to those of the linear models (in Table 3) and logit specifications (in Table 4). We are not able to also compute the partial effects at the observation-level as only the APEs across the entire distribution of the unobserved heterogeneity are identified (see Appendix B and Wooldridge, 2005).

The APE of inflation is similar to that in the previous models. According to the Wooldridge estimator, a unit increase in the inflation index in $t-1$ translates into 0.5 percentage points higher probability of stagnation in year t, if the country was not in stagnation in $t-1$. The APE of the interaction effect is -0.3% and insignificant at all conventional levels, supporting the view that inflation increases the chances of stagnation, no matter if this occurs within or outside of a stagnation spell. We find no evidence in favor of the hypothesis that (higher) inflation helps exiting a stagnation spell.

Next, the effect of financial openness is also similar to that estimated by the linear probability model. A unit increase in the Chinn-Ito index towards more openness reduces the probability of stagnation by 3.8 percentage points. The APE of the interaction term is small and insignificant (-0.6%), suggesting that there is no difference between onset and continuation probabilities. This result is particularly interesting, since capital account openness is sometimes restricted during crises to avert capital flight. Yet we find no evidence that it increases the risk of stagnation when a country is already stagnating, but instead it seems to be associated with a lower probability of crisis throughout.

In the case of trade openness, a discrete change towards openness when $y_{i,t-1} = 0$ has an average partial effect of -21.8%, which is similar to the previous results. The interaction term is highly significant and has a very large effect on the predicted probability of stagnation (18.6%). Adding these two effects, we get the APE for the probability of continued stagnation if $y_{i,t-1} = 1$ and the country is open to trade which is still negative but statistically insignificant (-3.2%). Trade openness has already been shown to affect growth rates (Wacziarg and Welch, 2008), growth accelerations (Hausmann et al., 2005), and the duration of positive growth spells (Berg et al., 2012). Our results add that open economies are significantly less likely to fall into a stagnation spell – it is the single largest effect of a non-political variable in all specifications – but also that openness does not significantly improve the chances of exiting a stagnation spell.

This finding suggests that, on top of typically being linked to higher average growth rates, trade openness is 'good for growth' in the sense that it protects countries from stagnation. While trade benefits growth through specialization, technological diffusion, and higher investment levels, it can also stabilize and accelerate growth but it does not necessarily lead to shorter crises (especially when these are caused by international factors). Arguably, the weaker within-stagnation effect could also be driven by the fact that countries often liberalize in response to an ongoing crisis which often yields no immediate benefits and can temporarily increase volatility (Wacziarg and Welch, 2008).

Regarding the political shocks, the confidence interval of the APE of negative regime changes widens a bit (the t-statistic is 1.90), but the absolute size of the effect remains large. If a country experiences a negative regime change, then the probability of stagnation increases by 21.4 percentage points. As in the logit models, we could not

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[10]The coefficients are scaled differently. The comparison should be done in terms of relative sizes, signs, and significance levels – where appropriate.

include the interaction term due to a lack of time variation. Whereas negative regime changes were previously linked to the onset of growth accelerations (Hausmann et al., 2005), our analysis reveals the more plausible result that they strongly predict stagnation episodes. None of the other political variables, that is positive regime changes, irregular leader exits or the Polity score, are robustly related with the incidence of stagnation spells. This finding stands in sharp contrast to the 'institutions trump other factors' perspective (e.g. Acemoglu et al., 2001, 2002; Rodrik et al., 2004) but could be driven by our focus on annual transitions in and out of crises (which are likely to be caused by more proximate factors).

Undervaluation of the real exchange rate has a moderate and marginally significant effect on the probability of stagnation. A one standard deviation change in the undervaluation index reduces the probability of both the onset and continuation of stagnation by about 4%. We cannot corroborate the results from the logit model that changes in the terms of trade and inequality have any meaningful effect on the probability of stagnation. In addition, wars, and civil conflict do not predict stagnation spells, and there is no evidence of an effect of the level of GDP per capita.

Table 5 also reveals that the degree of state dependence is underestimated by both the LPM and FE logit model. If a country experienced stagnation in the previous year, it is 38.9 percentage points more likely to be in stagnation in the current year. Path dependence is thus an important feature of the stagnation process. The models also fit the data reasonably well (Pseudo-$R^2 = 0.255$) and the fit compares favorably to the onset models used elsewhere (e.g. Hausmann et al., 2005, 2008) – although these goodness of fit criteria are not strictly comparable. Hence, we find that dynamic models a) allow us to test refined hypotheses about the difference between onset and continuation of stagnation spells, and b) lead to better predictions of when stagnation occurs.

To summarize, the main results are robust to different specifications, assumptions about the unobserved effects, and correlation between the observed variables and unobserved country-effects. Most of the preferred specifications identify the lags of inflation and negative regime changes as strong predictors of stagnation episodes. Several models also identify trade openness and financial openness as relevant. The non-linear models add three important insights. First, there is less evidence of interaction effects with the lagged state than the LPM suggests. The interactions are highly non-linear and vary significantly across the sample, but only in the case of trade openness we find convincing evidence that its effect differs depending on whether the country was in stagnation before or not. The evidence of interaction effects between the lagged state and inflation, financial openness, or negative regime changes is weak. Second, the non-linear models show that the real exchange rate matters for stagnation. An overvalued real exchange rate raises the chances of stagnation, while undervaluation reduces it. Third, the level of state dependence is still moderate, but larger than estimated by the linear models with fixed-effects.

We also find that lagging all included regressors by one year to assure that they are at least contemporaneously exogenous results in identifying very different significant factors than if the issue of endogeneity is left unaddressed. For example Hausmann et al. (2008), who studied the onset of stagnation, found that exports, inflation, wars, and political transitions matter. We do not find significant effects for exports and wars, although we do confirm their results for inflation and negative regime changes.

Interestingly, we cannot confirm two central results of the previous literature. First, we find little evidence that, apart from negative regime changes, institutions generally

affect the incidence of stagnation. Consistent with the institutionalist literature, we find that poor countries are more prone to stagnation and crisis. However, our analysis contradicts the hypotheses that negative institutional characteristics and internal or external shocks increase the incidence of stagnation. On the contrary, traditional macroeconomic variables predict the incidence of stagnation rather well. Second and contrary to previous research (Rodrik, 1999; Hausmann et al., 2008), our data does not support the finding that changes in real exports or terms of trade affect the probability of stagnation.

7 Conclusion

This paper provides a dynamic analysis of the incidence of stagnation and examines if stagnation spells are determined by institutional factors, various shocks, or macroeconomic factors. Building on a recent contribution by Hausmann et al. (2008), we define stagnation spells as episodes in which GDP per capita is below previously achieved levels. We then use fixed-effects linear models, GMM, fixed-effects logit, and dynamic random effects probit to assess the role of institutions, political shocks, and economic factors. In contrast to the previous literature, we explicitly examine the hypothesis that the effects of variables on the onset of stagnation and on the continuation of stagnation may be different.

We identify several factors that explain the incidence of stagnation spells. Adverse regime changes have the single largest effect on the incidence of stagnation, while higher inflation increases the chances of being in stagnation. More surprisingly, we find that real exchange rate undervaluation, financial openness, and trade openness help reduce the chances of stagnation. Additionally, we evidence that trade openness primarily protects against falling into stagnation, but does not speed up recovery. For all other variables there is no evidence of a differential impact on onset versus continuation.

Though traditional macroeconomic factors have the upper hand in our explanations of stagnation compared to institutional factors, we do not yet know to what extent macroeconomic factors are in turn influenced by underlying institutional characteristics. The use of more differentiated measures and time series for institutions as well as additional techniques for dealing with endogeneity might add further insights. This is a promising avenue for future research.

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Appendix A

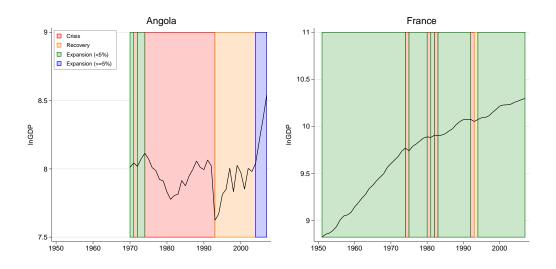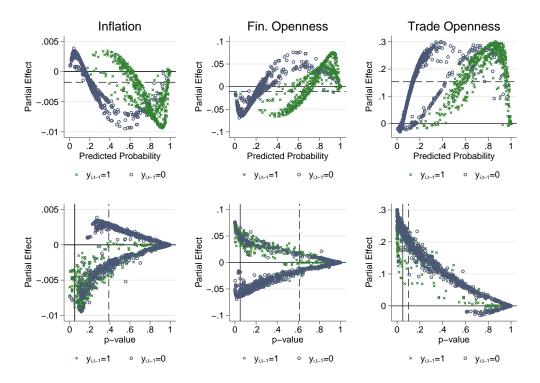**Figure 1** – Examples of growth episodes: Angola and France

Figure 2 – Partial Effects of Interaction Terms in Logit Model

Notes: The dashed line refers to the APE in the upper panel and the p-value of a Wald test that the APE is zero in the lower panel. "Predicted Probability" is the probability of stagnation $P(y_{it} = 1)$.

Table 1 – Growth episodes by income levels in 2007

% Country-years in ...	Low	Low-Mid	Mid-High	High	Total
			Panel A: *Two episode types*		
Expansion	22.12	41.33	54.97	73.14	48.31
Stagnation	77.88	58.67	45.03	26.86	51.69
Total	100.00	100.00	100.00	100.00	100.00
			Panel B: *Four episode types*		
Expansion (above 5%)	10.21	11.52	23.18	22.66	16.99
Expansion (5% or less)	11.91	29.81	31.79	50.49	31.32
Crisis	49.90	30.63	23.81	17.42	30.18
Recovery	27.98	28.04	21.22	9.44	21.51
Total	100.00	100.00	100.00	100.00	100.00

Notes: 127 countries, number of observations 6,338, percentages calculated over 1951 to 2007.

Table 2 – Growth episodes by institutional indicators

% Country-years in ...	Low	Low-Mid	Mid-High	High	Total
			Panel A: *Formalization of regulations*		
Expansion	30.49	40.92	54.61	76.41	51.06
Stagnation	69.51	59.08	45.39	23.59	48.94
Total	100.00	100.00	100.00	100.00	100.00
Expansion (above 5%)	14.49	16.15	20.86	18.38	17.47
Expansion (5% or less)	16.00	24.78	33.75	58.03	33.60
Crisis	40.29	37.13	21.80	14.51	28.29
Recovery	29.22	21.95	23.59	9.08	20.65
Total	100.00	100.00	100.00	100.00	100.00
			Panel B: *Control and intervention*		
Expansion	54.98	68.17	50.56	30.55	51.06
Stagnation	45.02	31.83	49.44	69.45	48.94
Total	100.00	100.00	100.00	100.00	100.00
Expansion (above 5%)	25.36	13.77	20.68	11.67	17.47
Expansion (5% or less)	29.62	54.40	29.87	18.88	33.60
Crisis	26.81	18.13	28.39	39.96	28.29
Recovery	18.21	13.70	21.05	29.49	20.65
Total	100.00	100.00	100.00	100.00	100.00

Notes: 47 countries in 1951, 107 countries in 2007, total number of observations 5,405 (Panel A and Panel B), percentages calculated on the basis of all years between 1951 and 2007.

Table 3 – Linear Models – Probability of Stagnation

Independent Variable	(1) LPM	S.E.	(2) LPM	S.E.	(3) FGLS	S.E.	(4) FGLS	S.E.	(5) SYS-GMM	S.E.	(6) SYS-GMM	S.E.
Log GDPC$_{(t-1)}$	0.052	0.073	0.052	0.094	0.061	0.073	0.040	0.065	-0.018	0.035	-0.029	0.020
Inflation$_{(t-1)}$	0.005*	0.003	0.005	0.004	0.005**	0.002	0.005**	0.002	0.003	0.004	0.013	0.010
Δ ToT$_{(t-1)}$	-0.080	0.081	-0.080	0.082	-0.092	0.109	-0.036	0.054	-0.035	0.043	-0.056	0.049
Δ Real Exports$_{(t-1)}$	-0.136	0.169	-0.136	0.177	-0.080	0.143	-0.014	0.047	0.011	0.067	0.055	0.062
RER Value$_{(t-1)}$	-0.150*	0.088	-0.150	0.113	-0.083	0.075	-0.016	0.039	-0.146***	0.046	-0.155***	0.042
Fin. Openness$_{(t-1)}$	-0.043***	0.016	-0.043**	0.020	-0.042**	0.017	-0.034**	0.016	-0.043*	0.023	-0.020	0.026
Trade Openness$_{(t-1)}$	-0.236***	0.081	-0.236***	0.073	-0.233***	0.062	-0.231***	0.058	-0.185	0.148	-0.286**	0.125
Inequality$_{(t-1)}$	-0.007	0.004	-0.007	0.005	-0.003	0.004	-0.002	0.003	0.007**	0.003	0.005**	0.002
Polity2$_{(t-1)}$	-0.011	0.007	-0.011	0.007	-0.008*	0.005	-0.003	0.003	-0.003	0.004	-0.004	0.004
Regchange +$_{(t-1)}$	0.033	0.068	0.033	0.077	0.006	0.103	0.017	0.032	0.036	0.036	0.031	0.043
Regchange −$_{(t-1)}$	0.338*	0.187	0.338	0.202	0.277*	0.149	0.274*	0.143	0.450	1.050	0.348*	0.210
Leader Exit$_{(t-1)}$	-0.071	0.053	-0.071	0.055	-0.045	0.054	0.004	0.028	-0.037	0.043	-0.033	0.043
War/Conflict$_{(t-1)}$	0.083	0.065	0.083	0.091	0.097	0.087	-0.005	0.042	-0.022	0.068	0.016	0.080
Log GDPC$_{(t-1)} \times y_{(t-1)}$	0.002	0.048	0.002	0.050	-0.039	0.042						
Inflation$_{(t-1)} \times y_{(t-1)}$	-0.004*	0.003	-0.004	0.004	-0.005**	0.002	-0.005**	0.002	-0.002	0.004	-0.012	0.010
Δ ToT$_{(t-1)} \times y_{(t-1)}$	0.031	0.093	0.031	0.084	0.067	0.125						
Δ Real Exports$_{(t-1)} \times y_{(t-1)}$	0.117	0.166	0.117	0.169	0.073	0.151						
RER Value$_{(t-1)} \times y_{(t-1)}$	0.146	0.103	0.146	0.105	0.081	0.075						
Fin. Openness$_{(t-1)} \times y_{(t-1)}$	0.021	0.019	0.021	0.019	0.034*	0.018	0.023	0.016	0.064**	0.029	0.038	0.039
Trade Openness$_{(t-1)} \times y_{(t-1)}$	0.186**	0.084	0.186**	0.088	0.207***	0.066	0.208***	0.059	0.132	0.169	0.292*	0.148
Inequality$_{(t-1)} \times y_{(t-1)}$	0.001	0.003	0.001	0.004	0.001	0.003						
Polity2$_{(t-1)} \times y_{(t-1)}$	0.007	0.007	0.007	0.007	0.007	0.005						
Regchange +$_{(t-1)} \times y_{(t-1)}$	-0.017	0.068	-0.017	0.076	0.011	0.108						
Regchange −$_{(t-1)} \times y_{(t-1)}$	-0.354*	0.184	-0.354*	0.193	-0.280*	0.166	-0.277*	0.160	-0.399	1.040	-0.363*	0.211
Leader Exit$_{(t-1)} \times y_{(t-1)}$	0.101*	0.060	0.101	0.068	0.059	0.060						
War/Conflict$_{(t-1)} \times y_{(t-1)}$	-0.171**	0.081	-0.171*	0.090	-0.126	0.096						
$y_{(t-1)}$	0.158	0.473	0.158	0.505	0.486	0.423	0.222***	0.059	0.363**	0.160	0.317**	0.141
Constant	0.758	0.746	0.373	0.846	0.463	0.647	0.617	0.560	0.368	0.392	0.508*	0.261
Observations	1691		1691		1691		1691		1691		1691	
Country FE	YES		YES		YES		YES		–		–	
Clusters	country		country-year		country		country		country		country	
Joint: Country FE [p-values]	0.000		0.000		0.000		0.000					
Joint: 5-Year FE [p-values]	0.101		0.451		0.430		0.364		0.053		0.024	
Joint: Set I [p-values]	0.000		0.000		0.000		0.000		0.000		0.000	
Joint: Set II [p-values]	0.796		0.226		0.708		0.167		0.759		0.038	
Instruments									390		35	
AR1 [p-value]									0.000		0.000	
AR2 [p-value]									0.304		0.120	
Hansen's J [p-value]									1.000		0.327	

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4 – Logit Models – Probability of Stagnation

Independent Variable	(1) Logit CML	S.E.	(2) Logit CML	S.E.	(3a) Logit ML	S.E.	(3b) APEs	S.E.
Log GDPC$_{(t-1)}$	0.417	0.707	0.430	0.695	0.430	0.738	0.058	0.099
Inflation$_{(t-1)}$	0.037	0.025	0.043	0.026	0.046	0.028	0.006*	0.004
Δ ToT$_{(t-1)}$	-0.556	0.571	-0.828*	0.479	-0.885*	0.516	-0.119*	0.070
Δ Real Exports$_{(t-1)}$	-0.784	1.163	-0.621	0.735	-0.681	0.795	-0.091	0.107
RER Value$_{(t-1)}$	-1.375**	0.567	-0.957*	0.503	-1.020*	0.533	-0.137*	0.071
Fin. Openness$_{(t-1)}$	-0.453***	0.121	-0.396***	0.110	-0.421***	0.118	-0.058***	0.016
Trade Openness$_{(t-1)}$	-1.441***	0.486	-1.424***	0.456	-1.527***	0.489	-0.259***	0.091
Inequality$_{(t-1)}$	-0.050	0.031	-0.052*	0.029	-0.054*	0.030	-0.007*	0.004
Polity2$_{(t-1)}$	-0.066	0.044	-0.051	0.033	-0.054	0.036	-0.007	0.005
Regchange $+_{(t-1)}$	0.193	0.561	0.129	0.376	0.136	0.397	0.018	0.054
Regchange $-_{(t-1)}$	2.102**	0.900	1.945**	0.938	2.038**	0.991	0.288**	0.135
Leader Exit$_{(t-1)}$	-0.501	0.426	-0.060	0.317	-0.058	0.334	-0.007	0.044
War/Conflict$_{(t-1)}$	0.115	0.382	-0.428	0.342	-0.454	0.357	-0.059	0.045
Log GDPC$_{(t-1)} \times y_{(t-1)}$	0.082	0.284						
Inflation$_{(t-1)} \times y_{(t-1)}$	-0.012	0.012	-0.019	0.015	-0.021	0.016	-0.002	0.002
Δ ToT$_{(t-1)} \times y_{(t-1)}$	-1.128	1.025						
Δ Real Exports$_{(t-1)} \times y_{(t-1)}$	0.290	1.244						
RER Value$_{(t-1)} \times y_{(t-1)}$	0.859	0.624						
Fin. Openness$_{(t-1)} \times y_{(t-1)}$	0.144	0.117	0.045	0.107	0.045	0.116	-0.011	0.022
Trade Openness$_{(t-1)} \times y_{(t-1)}$	0.947*	0.528	0.885*	0.487	0.939*	0.521	0.153	0.094
Inequality$_{(t-1)} \times y_{(t-1)}$	0.003	0.019						
Polity2$_{(t-1)} \times y_{(t-1)}$	0.013	0.042						
Regchange $+_{(t-1)} \times y_{(t-1)}$	-0.164	0.620						
Leader Exit$_{(t-1)} \times y_{(t-1)}$	0.833*	0.506						
War/Conflict$_{(t-1)} \times y_{(t-1)}$	-1.051*	0.537						
$y_{(t-1)}$	-0.004	2.813	1.189***	0.456	1.282***	0.487	0.321***	0.029
Observations	1314		1314		1314			
Country FE	YES		YES		YES			
5 Year FE	YES		YES		YES			
Clustered Errors [Country]	YES		YES		YES			
Countries	62		62		62			
Log-pseudolikelihood	-465.864		-469.311		-555.547			

Notes: In column (1) Regchange $-_{(t-1)} \times y_{(t-1)}$ had been dropped due to a lack of within-group variance. The asymptotic standard errors of the APEs are computed via the delta method. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Table 5 – Probit Models – Probability of Stagnation

Independent Variable	(1) Probit	S.E.	(2) Woold	S.E.	(3a) Woold	S.E.	(3b) APEs	S.E.
Log GDPC$_{(t-1)}$	0.027	0.103	0.449	0.377	0.398	0.367	0.083	0.076
Inflation$_{(t-1)}$	0.017***	0.006	0.012	0.008	0.020***	0.008	0.005***	0.002
Δ ToT$_{(t-1)}$	-0.328	0.300	-0.435	0.476	-0.321	0.349	-0.067	0.072
Δ Real Exports$_{(t-1)}$	-0.226	0.540	-0.570	0.565	-0.362	0.358	-0.075	0.074
RER Value$_{(t-1)}$	-0.719***	0.202	-0.786**	0.331	-0.513*	0.278	-0.106*	0.058
Fin. Openness$_{(t-1)}$	-0.056	0.039	-0.205***	0.074	-0.153**	0.073	-0.038**	0.018
Trade Openness$_{(t-1)}$	-0.567***	0.217	-0.926***	0.240	-0.774***	0.228	-0.218***	0.068
Inequality$_{(t-1)}$	0.014**	0.007	-0.026	0.016	-0.022	0.015	-0.004	0.003
Polity2$_{(t-1)}$	-0.014	0.013	-0.031	0.020	-0.027*	0.016	-0.006*	0.003
Regchange +$_{(t-1)}$	0.039	0.269	0.138	0.405	0.066	0.220	0.014	0.046
Regchange −$_{(t-1)}$	1.074*	0.557	1.238**	0.617	1.047*	0.584	0.214*	0.112
Leader Exit$_{(t-1)}$	-0.196	0.188	-0.118	0.235	-0.006	0.171	-0.001	0.035
War/Conflict$_{(t-1)}$	0.110	0.257	0.280	0.374	-0.199	0.246	-0.041	0.051
Log GDPC$_{(t-1)} \times y_{(t-1)}$	-0.259	0.182	-0.129	0.154				
Inflation$_{(t-1)} \times y_{(t-1)}$	-0.008	0.006	-0.004	0.008	-0.012	0.008	-0.003	0.002
Δ ToT$_{(t-1)} \times y_{(t-1)}$	-0.091	0.393	-0.097	0.750				
Δ Real Exports$_{(t-1)} \times y_{(t-1)}$	-0.435	0.574	0.286	0.745				
RER Value$_{(t-1)} \times y_{(t-1)}$	0.635	0.390	0.532*	0.308				
Fin. Openness$_{(t-1)} \times y_{(t-1)}$	0.096	0.060	0.086	0.078	-0.019	0.068	-0.006	0.017
Trade Openness$_{(t-1)} \times y_{(t-1)}$	0.474	0.321	0.816***	0.262	0.646***	0.238	0.186***	0.067
Inequality$_{(t-1)} \times y_{(t-1)}$	0.005	0.012	0.008	0.011				
Polity2$_{(t-1)} \times y_{(t-1)}$	0.008	0.022	0.005	0.020				
Regchange +$_{(t-1)} \times y_{(t-1)}$	0.166	0.284	-0.137	0.482				
Leader Exit$_{(t-1)} \times y_{(t-1)}$	0.290	0.259	0.201	0.290				
War/Conflict$_{(t-1)} \times y_{(t-1)}$	-0.267	0.404	-0.803*	0.458				
y_0			0.969	2.621	1.244***	0.477		
$y_{(t-1)}$	3.407*	1.787	1.554	1.569	1.020***	0.229	0.389***	0.035
Constant	-1.287	1.105	-0.733	2.482	-2.958	1.871		
Observations	1586		1471		1596			
$\boldsymbol{\eta}_2$-estimated?	NO		YES		YES			
$\boldsymbol{\eta}_3$-estimated?	NO		YES		YES			
5-Year FE	YES		YES		YES			
Countries	90		81		90			
Pseudo-R^2	0.423		0.269		0.255			
Log-likelihood	-632.218		-574.211		-596.272			

Notes: In column (1) Regchange −$_{(t-1)} \times y_{(t-1)}$ had been dropped due to a lack of within-group variance. The asymptotic standard errors of the APEs are computed via the delta method. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Table A.1 – Independent Variables, Description, Construction and Sources

Name	Description	Construction	Data Source
Log GDPC$_{(t-1)}$	Logarithm of GDP per capita	$\ln(RGDPCH_{i,t-1})$	PWT 6.3
Inflation$_{(t-1)}$	Change in consumer prices	$100 \times \ln(1 + [gCPI_{i,t-1}])$	IFS & WDI[a]
Δ ToT$_{(t-1)}$	Change in terms of trade	$\ln(TOT_{i,t-1}) - \ln(TOT_{i,t-2})$	WDI & IFS[b]
Δ Real Exports$_{(t-1)}$	Change in exports volumes	$\ln(EXP_{i,t-1}) - \ln(EXP_{i,t-2})$	WDI & IFS[c]
RER Value$_{(t-1)}$	Real exchange rate valuation	see note[d]	PWT 6.3
Fin. Openness$_{(t-1)}$	Capital account openness	$KAOPEN_{i,t-1}$	Chinn-Ito '09
Trade Openness$_{(t-1)}$	Trade liberalization measure	1 if open in $t-1$	W-W '08
Inequality$_{(t-1)}$	Gini coefficient for income	$GINI_{i,t-1}$	Solt '09
Polity2$_{(t-1)}$	Revised combined polity score	$DEMOC_{i,t-1} - AUTOC_{i,t-1}$	Polity IV
Regchange $+_{(t-1)}$	Positive regime change	based on $REGTRANS_{i,t-1}$[e]	Polity IV
Regchange $-_{(t-1)}$	Negative regime change	based on $REGTRANS_{i,t-1}$[e]	Polity IV
Leader Exit$_{(t-1)}$	Irregular exit of leader	1 if $EXIT_{i,t-1} \neq 1$[g]	Archigos 2.9
War/Conflict$_{(t-1)}$	Conflicts (≥ 1000 deaths)	see note[f]	UCDP/PRIO

[a] We use the IFS series (CPI y-o-y %-change based on line 64) as a benchmark and append it with the WDI series in 59 cases where the former has missing data.

[b] We use the WDI series as a benchmark (which comprises of UNCTAD and IFS data) and append it with the export volume index from the IFS for missing years/countries.

[c] From the WDI 2011, we use the series 'net barter terms of trade', and from the IFS, we derive the equivalent net barter terms of trade by dividing the unit value of exports (line 74) by the unit value of imports (line 75) and multiplying the result by 100. We then append the WDI series of growth rates with the growth rates from the IFS series whenever the former has missing information.

[d] Rodrik (2008) proposes a simple way to calculate an index of "real" exchange rate (RER) overvaluation based only on the Penn World Tables. The method involves three steps. (1) compute the PPP-adjusted exchange rate: $\ln(RER_{it}) = \ln(XRAT_{it}/PPP_{it})$. (2) Estimate the Balassa-Samuelson effect: $\ln(RER_{it}) = \alpha + \beta \ln(RGDPCH_{it}) + \gamma_t + u_{it}$. (3) Take the difference between the actual RER and the predicted RER from (2), hence: RER Value$_{(i,t-1)} = \ln(RER_{(i,t-1)}) - \ln(\widehat{RER_{i,t-1}})$.

[e] We use the Polity IV variable REGTRANS to identify regime changes in either direction based on a minimum 3-point change in a country's democracy or autocracy score. We exclude the code 0 for "minor changes", which denotes *any* change in the democracy or autocracy scores. Further, we do not code -77 for "interregna", -66 for (foreign) "interruptions" and -88 for regime "transitions" as negative regime changes to avoid collinearity with the leader exit and war/conflict dummies.

[f] This dummy is constructed based on the UCDP/PRIO Armed Conflict Dataset v.4-2011, 1946–2010. We first converted the conflict-year database into country-year format and then coded the intensity levels for the highest intensity conflict in any given country-year. The dummy is unity if the intensity level of the conflict was coded as 2 in $t-1$ and the country was listed as a location of the conflict.

[g] The Archigos 2.9 time-series database records entries, tenure and exits of country leaders and the conditions on which they entered and exited. In some instances there are multiple observations per country-year, in such an event we code an irregular exit if any one observation within that year is identified as "irregular". Irregular exit refers to leaders that died in office, committed suicide, or left office due to ill health, other irregular means or the deposition by another state.

Appendix B

Average Partial Effects: Dynamic Random Effects Probit

Wooldridge (2005) shows that in the dynamic random effects probit model, a consistent and $\sqrt{N}$-asymptotically normal estimator of the APEs of time-varying variables is available. However, the APEs of time-invariant covariates are not identified. Using the same assumptions as in Section 5.2, we can write the conditional expectation over the distribution of μ_i as:

$$\mathrm{E}[\Phi(\alpha y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta} + \mu_i)] = \mathrm{E}[\Phi(\alpha y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta} + \eta_0 + \eta_1 y_{i1} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_2 + \varepsilon_i)] \tag{1}$$

where $\mathbf{x}_{it}$ denotes time-varying regressors, $\bar{\mathbf{x}}_i$ their time-averages, and $\varepsilon_i = \mu_i - \eta_0 - \eta_1 y_{i1} - \bar{\mathbf{x}}'_i\boldsymbol{\eta}_2$. The expectation runs over the distribution of $(y_{i1}, \bar{\mathbf{x}}_i, \varepsilon_i)$.

Following Wooldridge (2005), we can get rid of the unobserved effects by applying the law of iterated expectations and defining the average structural function (ASF):

$$
\begin{aligned}
\mathrm{ASF}(\mathbf{x}_{it}, y_{i,t-1}) &= \mathrm{E}[\mathrm{E}[\Phi(\alpha y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta} + \eta_0 + \eta_1 y_{i1} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_2 + \varepsilon_i)|y_{i1}, \bar{\mathbf{x}}_i]] & (2) \\
&= \mathrm{E}[\Phi[(\alpha y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta} + \eta_0 + \eta_1 y_{i1} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_2) \times (1+\sigma_\varepsilon^2)^{-1/2}]] & (3) \\
&= \mathrm{E}[\Phi(\alpha_\varepsilon y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta}_\varepsilon + \eta_{\varepsilon,0} + \eta_{\varepsilon,1} y_{i1} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_{\varepsilon,2})] & (4)
\end{aligned}
$$

Assuming that $\varepsilon_i \sim$ i.i.d. $N(0, \sigma_\varepsilon^2)$ and $\varepsilon_i \perp (y_{i1}, \bar{\mathbf{x}}_i)$, the scaled parameters in (4) are what standard random effects probit estimates. The panel-level likelihood based on the density of $(y_{i1}, ..., y_{iT_i})$ given $(y_{i1}, \bar{\mathbf{x}}_i)$ can be written as

$$\mathcal{L}_i = \int_{\mathbb{R}} \left[\prod_{t=2}^{T_i} \Phi\left(q_{it}(\alpha y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta} + \eta_0 + \eta_1 y_{i1} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_2 + \varepsilon_i)\right) \right] \sigma_\varepsilon^{-1}\phi(\varepsilon_i/\sigma_\varepsilon)\mathrm{d}\varepsilon_i \tag{5}$$

where $q_{it} = (2y_{it} - 1)$ and $\phi(\cdot)$ is the standard normal pdf. The model log-likelihood is $\ln\mathcal{L} = \sum_{i=1}^N \ln\mathcal{L}_i$.

The scaled parameters also show up directly in the average structural function (ASF). A consistent estimator of the ASF is the simple average across all observations. The derivative of the ASF with respect to a continuous time-varying regressor, the finite difference for a binary regressor, or a mix thereof, is equivalent to the APE of that variable. This approach can be easily extended to include interactions with the lagged state and other non-linearities. Wooldridge's device to get to the APEs is to always average across the distribution of $(y_{i1}, \bar{\mathbf{x}}_i)$ first and then to specify the derivatives/differences. E.g., the APE of a continuous time-varying variable (without an interaction) in $\mathbf{x}_{it}$ is:

$$\mathrm{APE}(\beta_k) = \beta_{\varepsilon,k} \times \left[\frac{1}{N(\bar{T}-1)} \sum_{i=1}^N \sum_{t=2}^{T_i} \phi\left(\alpha_\varepsilon y_{i,t-1} + \mathbf{x}'_{it}\boldsymbol{\beta}_\varepsilon + \eta_{\varepsilon,0} + \eta_{\varepsilon,1} y_{i1} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_{\varepsilon,2}\right) \right] \tag{6}$$

where we also average over time to obtain a single scale factor. It is straightforward to apply the results of Section 5.2 to the APEs of the interaction terms.