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Why Do Many Disinflations Fail?

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Abstract

In this paper we look at 51 stabilizations from high inflation and identify factors that prevented governments from sustaining the initial gains in disinflation. We use three different indicators of failure in sustaining disinflation gains (two dichotomous and one continuous). The most striking result is that we can predict correctly about 85 percent of the dichotomous outcomes, and explain about 60 percent of the variance of the continuous measure, without using the post-stabilization evolution of domestic (policy and non-policy) variables. Initial conditions, particularly a high level of pre-stabilization inflation in accordance with the "crisis hypothesis," and the evolution of external exogenous variables appear to be the most important predictors of success or failure. Exchange-rate-based stabilizations also appear more likely to succeed. The exchange rate anchor retains its significance even after controlling for its potential endogeneity and for fiscal and monetary adjustment during post-stabilization years, a result that suggests that the coordination role of exchange rate anchors may extend to areas such as wage and interest rate determination. We also find that reputational factors and political institutions matter. A long history of high inflation is associated with failures, while countries with a new chief executive and a less constrained executive authority, as well as democracies with a majoritarian electoral rule, are more likely to succeed.

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I. INTRODUCTION

Countries experiencing high inflation typically embark on several disinflation attempts, most of which enjoy only temporary success. In this paper we focus on the question of what makes the initial disinflation gains last over time. Continued adjustment—including tight monetary and fiscal policies and possibly structural reforms—is essential, but is it enough? Can external shocks derail an otherwise successful stabilization? Political institutions and developments may also affect the final outcome not only by impinging on the execution of the required adjustment but also by influencing expectations about the program's sustainability. Similarly, initial conditions—broadly defined to include pre-stabilization features of the economy, the value of policy variables, and even the choice of the nominal anchor—may play a key role both directly and indirectly via expectations. In this paper, we develop an estimation procedure that disentangles empirically the role of each of these factors in a sample of 51 episodes of high-inflation stabilizations that satisfy a criterion of minimum initial success.

This paper differs from the rest of the empirical literature on disinflation from chronically high levels in two important aspects. First, in recent years, most of the literature has concentrated on whether the post-stabilization path of key macroeconomic variables—such as GDP growth, private consumption, the real exchange rate, and the current account of the balance of payments—depends on the nominal anchor used in the disinflation strategy.[2] The focus of attention has been whether exchange-rate-based stabilizations (ERBS), as opposed to money-based stabilizations, generate a distinctive economic cycle. We study, instead, whether the initial inflation reductions were lasting.[3] Several were not: in 30 to 60 percent (depending on the criterion used to measure durability) of the episodes in our sample, the initial gains in disinflation were lost by the third year following stabilization.

Second, this paper is more comprehensive than previous studies in relation to both the number of episodes considered and the questions addressed. Our sample of inflation stabilization episodes, comprising 51 stabilizations (Table 1), is larger than those used in other empirical studies. Previous studies are based on a sample of episodes that are either well-documented in the literature or identified through a numerical rule consisting of an absolute threshold for the level of inflation (i.e., a reduction in inflation—no matter how large—is not considered a "stabilization" if inflation does not breach the threshold).[4] The

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[2] See Calvo and Végh (1999) for a comprehensive review of the literature.

[3] To the best of our knowledge, Veiga (1999) represents the only attempt at identifying empirically the determinants of a successful disinflation strategy. However, his paper takes a view of stabilizations that is heavily influenced by ERBS, and uses a sample comprised mostly of ERBS (29 out of a total of 34 episodes).

[4] Veiga (1999) appears to contain the largest number of episodes of any study based on the compilation of documented cases: 34. The studies by Kiguel and Liviathan (1992), Calvo and

(continued)

sample used here is selected with a numerical rule that requires the crossing of a country-specific threshold—rather than an identical threshold across countries—for a stabilization to be selected. This rule is explained in Section II. For each episode we collect seven years of data: the stabilization year, three years before, and three years after. The choice of this arbitrary 7-year window, particularly the three-year post-stabilization horizon, reflects the need to balance the objective of having a long enough horizon after the stabilization date and that of not having an horizon so long that inflation performance in the final year is largely unrelated to the stabilization program under analysis. The implication is that, in this paper, disinflations that last at least three years are classified as successful, but there is no presumption that they will not fail later on.

When does an inflation stabilization fail? The rule used to separate successes from failures is unavoidably arbitrary and in conducting our empirical investigation we consider three different measures of failure to check the robustness of our results. These measures are described in the next section. At this stage, to provide a preliminary indication of the bivariate relationship between failed stabilizations and some of their potential determinants, we simply divide our sample into two groups. We consider a stabilization failed if, during the first three post-stabilization years, inflation exceeds ¾ of the inflation rate prevailing the year before stabilization. This yields a group of 34 lasting and 17 failed stabilizations (criterion 1 in Table 2).[5] Figure 1 shows that there is a marked difference in inflation rates between the two groups already in year $T+1$ (even though the criterion used for classifying episodes in the two groups does not rely on inflation performance in $T+1$).[6] Following a marked decline (of 50-60 percentage points) in year T in both cases, inflation declines further (by some 15 percentage points) in the successful group, while it remains practically unchanged in the failing group. In years $T+2$ and $T+3$, the differences in inflation levels become more marked and statistically significant, confirming that this simple criterion of success separates the dataset in two groups with very different post-stabilization paths for inflation.

Why do so many initially successful disinflations fail over the following three years?

Végh (1994) and Reinhart and Végh (1994) are based on less than 20 episodes. The second type of studies includes Easterly (1996), which identified 28 episodes, and Fischer, Sahay and Végh (2001), which includes 49 cases.

[5] This is a somewhat loose criterion for success. A stricter criterion—i.e., that inflation remains at or below whatever level inflation reached during the stabilization year (criterion 2 in Table 2)—splits the sample into 20 successful and 31 failed stabilizations.

[6] Figure 1 shows the transformation, $\pi/(1+\pi)$, rather than the actual level of inflation in order to prevent the width of the 95 percent confidence interval from obscuring the changes in the median. We use the same transformation in our regression estimates and for the growth of domestic credit shown in Figure 2 .

Figures 2 and 3 suggest very clearly that insufficient policy adjustment, as captured by the post-stabilization path of monetary and fiscal variables, is partly responsible. Figure 2 shows that the path of credit growth is very similar to that of inflation. In the successful disinflations, there is a marked and durable decline beginning in year *T*. In the failed stabilizations, credit growth decelerates in *T* but increases in *T+1*, and in *T+2* reaches a higher level than in any pre-stabilization year. Figure 3 shows that, in year *T*, the fiscal deficits are nearly identical in both groups but then they differ markedly in the following years. In failed stabilizations, all gains in fiscal adjustment obtained in the previous three years are lost already in the first post-stabilization year. Fiscal adjustment is, instead, sustained in the successful group (the fiscal balance in year *T+3* is almost identical to that in year *T*), while it continues to unravel in the failed-stabilization group (the fiscal balance in year *T+3* is more than 2½ points of GDP worse than in year *T*.)

Should an analysis of the determinants of failed stabilizations end at this point attributing the responsibility of failure entirely to the lack of fiscal and monetary adjustment? We do not believe that this would be appropriate for at least two reasons. First, even if monetary and fiscal policies could account fully for the degrees of success of the stabilization programs in our sample, this explanation could be considered, if not tautological, of very little interest. Most likely, no inflation stabilization could succeed without some degree of fiscal and monetary adjustment. The key question is whether there are some identifiable factors that may be driving policy adjustment itself. Our empirical investigation focuses on these.

Second, it is also possible that monetary and fiscal policies may not be the only determinants of success or failure of inflation stabilizations. Other factors that shape economic agents' inflation expectations are also likely to play an important role. As a result, depending on the evolution of expectations, the same initial fiscal and monetary adjustment may result in success or failure. For this reason, in Section III we investigate other factors that, *in conjunction with* monetary and fiscal adjustment, may contribute to the failure of inflation stabilization.

In order to explore the empirical relevance of various potential determinants of success/failure of a stabilization plan, we address four main questions. First, we consider whether *initial conditions* matter. There is a body of theoretical literature, initiated by Drazen and Grilli (1993), predicting that "bad" initial conditions are conducive to a successful stabilization. Figure 1, indeed, shows that lasting stabilizations reach a higher pre-stabilization level of inflation than those that fail. In Section III, we test this and other dimensions of the "crisis hypothesis." We also focus on two specific initial conditions that have important policy implications: the nominal anchor and the degree of openness of the economy. Interestingly, whereas a simple count of failed stabilizations reveals that ERBS failed as frequently as other stabilizations, no matter what criterion of success we use (Table 2), we will show that a different conclusion can be reached on the basis of *multivariate* econometric analysis. We also test the relationship between initial openness (real and financial) and success. This issue has been studied by Romer (1993) and Lane (1997) who find that greater openness tends to be associated with lower inflation. We also test this hypothesis.

A second question we address in this paper is how important negative *external shocks* are as a determinant of stabilization failures. This question, which points to the importance of luck in the success of economic reforms, has received considerable attention in the empirical literature (Easterly et al., 1993). External shocks are also featured as a key determinant of success in many theoretical models of stabilization (Dornbusch, 1991). Figure 4 shows that, indeed, in our sample, the growth rate of import demand from trading partners of countries with failed stabilizations dips in the first post-stabilization year whereas it increases for countries with successful stabilizations. In Section III, we test whether this and other sources of external shocks, such as the terms of trade and international interest rates, contribute significantly to explain stabilization failures after controlling for other factors.

The third question we address is *whether credibility and reputational factors matter*. A way of answering this question is to study whether inflation stabilization failures are *state dependent*. One dimension of state dependence relates to whether the probability of failure depends on having experienced long periods of high inflation prior to the beginning of the stabilization. Another dimension of state dependence has to do with whether, *ceteris paribus*, stabilizations that succeed (fail) in the initial years of our three-year window are more likely to succeed (fail) in the following years. Evidence in favor of at least one of these two forms of state dependence would indicate that reputational factors play a role in inflation stabilization beyond that of adjustment policies, external shocks, or other initial conditions. The economic literature has paid considerable attention to the role of reputation and credibility in inflation stabilizations, although the emphasis on reputation has declined in recent years.[7] As some of these reputational models of inflation predict that a new government would initially choose a lower level of inflation (Barro, 1986), our tests also control for the number of years the chief executive has been in power.

The fourth question we address is whether prevailing *political conditions*, including the type of *political institutions*, contribute to explain the failure of inflation stabilizations. Many countries in the sample experienced major regime transitions towards or away from democracy prior to stabilization or during the three-year window we examine. Thus, we need to control for the potential effect of instability in the political regime on the likelihood of failure. A substantial body of literature has studied the relationship between inflation and political instability (Cukierman, Edwards, and Tabellini, 1993). More recently, the political economy literature has focused on whether democracies with presidential (as opposed to parliamentarian) regimes and majoritarian (as opposed to proportional) electoral rules tend to be associated with smaller levels of government expenditure. Given the close link between fiscal imbalances and inflation, we investigate whether there is some evidence in our sample of a relationship between these political factors and the likelihood of failure of an inflation stabilization. Finally, we consider whether social conflict and lack of political cohesion are

[7] See Rogoff (1987) for a critical review of this literature through the mid 1980s.

more likely to be associated with stabilization failures, as "war of attrition" theories à la Alesina and Drazen (1991) would suggest.

While the paper focuses on these four broad sets of questions, there are other questions on which it sheds some light, such as whether inflation stabilizations from high levels entail a sacrifice ratio (in terms of foregone GDP growth), as found by Ball (1994) for industrial countries, or whether they are expansionary, as documented by others (Harberger, 1998; Gould, 2001; Hamann, 2001). We also verify whether fiscal and monetary policies retain any capability of explaining stabilization failures once we control for all the potential underlying determinants listed above.

The paper is organized as follows. Section II sets the limits of our investigation and describes our estimation strategy, including the solutions we propose to the several challenges—ranging from sample selection issues to potential endogeneity problems—associated with conducting an econometric analysis on our dataset. Section III presents the estimation results and relates them to the different hypotheses formulated in the theoretical literature. Section IV concludes and outlines some areas for future research.

II. Dataset and Estimation Strategy

In this section, we describe the criteria used to select our sample and our proposed solutions to the problems associated with various econometric issues. They include: *i)* the potential impact of these selection criteria on the estimation results; *ii)* the choice of a specification search strategy that is adequate for a dataset with a small number of observations and a large number of potential explanatory variables; *iii)* the selection of a measure of stabilization failure; *iv)* the appropriate estimation technique; and *v)* how best to control for the potential endogeneity of some regressors.

A. Identification of Episodes

The selection of the stabilization episodes used in this study is based on the application of some rules to a data set of annual inflation rates for the period 1960-1997 for a group of 143 countries.[8] Hamann (2001) contains a detailed explanation of the procedure, as well as the pros and cons of using a rules-based selection criteria. Below, we provide a brief summary of his main findings in this regard.

Building upon earlier work by Bruno and Easterly (1995), Easterly (1996) defines a stabilization episode as a movement from an "inflation crisis" to a "non-crisis" period where the former is defined as a period of at least two consecutive years of inflation above 40 percent and the latter as a period of at least two consecutive years with inflation below 40

[8]This represents a sub-set of countries for which the IMF publishes data which excludes the eastern European countries and the former republics of the Soviet Union.

percent. The two-year minimum is used to eliminate spikes in inflation due purely to one-time price shocks such as changes in key import prices, devaluations or price liberalizations; the 40 percent threshold level is found by Bruno and Easterly (1995) to be useful in discriminating between periods of very high inflation and moderate to low inflation. To establish the timing of stabilizations, Easterly defines the peak year during the crisis period as "year 0", or the stabilization year, and the year after the peak as the first post-stabilization year.

Easterly found 28 stabilization episodes (Table 3), and his rule could be considered a relatively stringent criterion for the selection of stabilization episodes. Notably, the list excludes a number of well documented, albeit mostly failed, stabilization attempts, including Argentina and Chile's *tablitas* of the late 1970s, and the heterodox programs of Argentina, Brazil and Peru in the mid-1980s. Easterly's sample also excludes several programs in Africa.

After experimenting with several numerical rules, Hamann (2001) reports the results obtained with 3 of them, all based on the requirement that, prior to stabilization, inflation remained at or above 40 percent for at least two consecutive years. In that study, the author opted for selecting a sample produced by a rule that did not impose a uniform ceiling on post-stabilization inflation but required a reduction of at least 1/4 in inflation in the first year of stabilization (followed by the requirement that inflation remained below the pre-stabilization level for another year). This exercise produced the 51 stabilization episodes used in this study, including all 28 found by Easterly (although 17 of them are dated a year later).[9] This criterion also picked up the well-known episodes not captured by Easterly's rule.

B. Sample Selection and Scope of our Study

An episode-selection strategy based exclusively on the actual behavior of inflation implies that the selected stabilization episodes are those for which a given reduction in inflation is actually observed. The resulting sample is, thus, comprised of programs that enjoyed some minimum degree of success. Short-lived programs that did not manage to make a significant dent to average annual inflation were not picked up.[10] This incidental

[9]Six additional episodes, denoted by italics in Table 1, were identified when the threshold was lowered to 35 percent in an attempt to capture "near misses".

[10]The rules may also be picking up the delayed effects of programs that pursued more than one objective at a time or, more likely, programs that pursued a sequence of events, such as restoration of external balance first, and only subsequently a reduction in inflation. Thus, either by focusing only on relatively successful episodes, or by pushing forward the timing of stabilization, it is likely that our sample of episodes may be associated with a relatively positive economic outlook around stabilization time (i.e., higher growth, better external accounts, etc.).

truncation would lead to a sample selection bias if we used our sample to draw inferences on the entire set of possible inflation stabilization attempts—because some *bona fide* stabilization episodes may have been left out. Thus, we limit the scope of our study to the class of inflation stabilizations that satisfy a minimum qualifying criteria and our inferences on what makes stabilizations fail are then valid only *within this class*.

For example, if we were to find that larger fiscal surpluses are associated with disinflations that last at least three years, we could make a valid inference on the extent to which post-stabilization fiscal adjustment is conducive to a lasting stabilization *within the set of high-inflation stabilization attempts that are initially successful*. We could not extend this conclusion to the set of all attempts at stabilizing high inflations because of the potential bias associated with the incidental truncation.[11]

We conduct, however, a robustness check on the implications of our sample selection criterion. To have an idea of whether other criteria would have yielded different results, we estimate our benchmark econometric specification also on the smaller sample of 28 episodes identified by Easterly (1996).[12] As shown below, the results obtained using this smaller sample are broadly consistent with the key results obtained with our sample.

C. Specification Search.

The small number of observations and the large number of potential explanatory variables in our dataset makes a general-to-specific specification search impossible. To address this issue, we adopt the following strategy. We organize our dataset of 51 inflation stabilization episodes as a **panel dataset**. The 51 episodes represent the cross-section dimension of the panel; the three post-stabilization years (*T+1, T+2, T+3*) constitute the time-series dimension.[13] This panel structure—with the time-series dimension defined in relation to a cross-section-specific reference date (the stabilization year, in our case)—was first pioneered by Fama et al. (1969) in their classical study of stock splits. Recent studies that have performed econometric estimates on panel datasets with the same format include Bertocchi and Canova (1996) and Milesi-Ferretti and Razin (1998). We thus estimate our empirical

[11] In practice, this bias would arise only if the errors of the equation measuring the determinants of successful inflation stabilizations were correlated with the errors of an equation modeling the determinants of the selection of a stabilization episode. This issue is discussed in greater detail below, where an analogous problem arises in relation to the use of a nominal anchor variable as a regressor.

[12] When stabilization dates do not coincide, we use ours.

[13] We also use T and T-j (j=1,2,3) data to control for initial conditions.

models on a panel dataset with 153 observations.[14] We also use graphical analysis to guide us in the identification of potential regressors.

We perform the specification search in stages by gradually adding blocks of explanatory variables and excluding at each stage those regressors that, as a result of the addition of new variables, become statistically insignificant. We start by using only: *i) initial conditions*, such as the levels—in the years prior to the stabilization—of macro and policy variables, including a variable measuring the number of pre-stabilization years with inflation above 40 percent and dummy variables for the *openness of the current account and the capital account* (based on the IMF's classification) at the time of the stabilization; *ii)* variables controlling for *external conditions* (terms of trade, U.S. interest rates, growth of import demand from trading partners); and *iii)* a dummy variable for the *nominal anchor* taking value 1 in ERBS, controlling for its potential endogeneity with the methodology described below.

We then add to the best specification from this first stage a set *of political economy variables*—such as features of the political systems, including their durability, the degree of democracy, and, within the subset of democratic regimes, whether they are parliamentarian or presidential and whether the electoral system is proportional or majoritarian; the numbers of years in office of the chief executive; the degree of political and social fragmentation—and obtain a second specification. In a third stage we add groups of variables dated *T, T+1, and T+2*, starting with *non-policy macro variables* (GDP growth rates, output gaps, international reserves) and ending with *government fiscal balances and domestic credit growth*. The logic of this sequential search is not only to estimate models with a reasonable total number of potential regressors but also to include only exogenous variables at the beginning and add the variables that are more at risk of being endogenous at the end. This allows us to interpret the results based on a smaller set of explanatory variables as a reduced form of a more complicated structural model with simultaneous equations. Note that, to eliminate obvious sources of endogeneity, *we lag all post-stabilization variables one period*. Nevertheless, as discussed below, some variables may still be endogenous because of an omitted variable bias.

D. Measuring Failure.

The dependent variable in our benchmark estimates is a zero-one stabilization failure dummy taking on a value of one when a stabilization fails and zero when it does not. Specifically, this binary variable ($FAIL1_{T+i}$; $i=1,2,3$) takes value zero when inflation in year $T+i$ is lower than 75 percent of inflation in year $T-1$ and one otherwise. The panel structure of the data implies that, if the requirement for failure is only temporarily satisfied, the same stabilization episode may be classified as a failure in year $T+1$ and as a success in year $T+2$ and $T+3$.

[14] The total number of observations used in the econometric estimates is 150 or 153 depending on whether the government fiscal balance—which is not available for Indonesia in the 1960s—is among the regressors.

To verify the robustness of our results, we use two additional measures of failure. The first is another dummy ($FAIL2_{T+i}$) taking on a value of zero if inflation in year $T+i$ is lower than inflation in year T and one otherwise. Since this amounts to a stricter criterion of success, there are more stabilization failures with *FAIL2* than with *FAIL1*. The second alternative measure of durability is a continuous variable equal to the ratio of inflation in year $T+i$ (i=1,2,3) to inflation in year T ($RATIO_{T+i} = \pi_{T+i}/\pi_T$).[15] High values of this ratio will then be associated with stabilization failures.

Both continuous and binary variables have pros and cons. The continuous variable allows a more precise measure of inflation reductions but can take extreme values that could affect estimation results. This problem can be particularly serious in cases of extremely high inflation. The binary variables are more robust to outliers but are more imprecise: two very similar inflation outcomes may have a different binary classification if they are just on opposite sides of the selected threshold level. Estimating models with both continuous and binary dependent variables allows us to check whether the unavoidable shortcomings of each type of dependent variable affect our results in a significant manner.

E. Estimation Techniques.

To be able to draw conclusions on the determinants of stabilization failure within our sample, we need to address the econometric issues associated with the binary nature of *FAIL1* and *FAIL2*, the panel structure of the data, and our interest in assessing in-sample state dependence. We take the binary nature of *FAIL1* and *FAIL2* into account by estimating a *probit* model. The *panel structure of the data* creates the potential problem of episode-specific effects, which—if ignored—might bias the estimated coefficients. The presence of several time-invariant variables and the short time-series dimension of our dataset precludes, however, the estimation of fixed-effects models. Our benchmark regression is then a *pooled regression* with *FAIL1*, in which data from different episodes are pooled together sharing a common intercept term. We take, however, into account the panel structure of the data by allowing for heteroskedastic errors across episodes. Allowing for heteroskedasticity across countries or calendar years yields very similar results.

To allow for in-sample state dependence, we include in our benchmark specification lagged values of the dependent variable in periods $T+2$ and $T+3$. In the probit regressions, the lagged value of the dependent variable in period $T+1$ is zero because stabilization in year T is by definition always successful. This amounts to assuming that the outcome in period $T+1$ is determined only by the other explanatory variables and that state dependence is meaningful only from period $T+2$ onwards. This is not an unusual way of dealing with the problem of

[15] The inflation measure π_{T+i} actually corresponds to the transformation $\pi_{T+i}/(1+\pi_{T+i})$ where π_{T+i} is the inflation rate in year $T+i$. We apply this common transformation to limit the heteroskedasticity problems that hyperinflation relapses otherwise cause and the impact of outliers.

initial conditions typical of dynamic probits (see, for example, Heckman, 1981). Moreover, this is justified because our sample begins at *T+1* not by chance but by construction, as we are interested in the evolution of inflation only in the post-stabilization years. Our treatment of the lagged dependent variable is similar when it is continuous (*RATIO*). In this case, the lagged dependent variable at *T+1* should in principle be one. However, we set it to zero so that the regression will try to explain $RATIO_{T+1}$ only with the other explanatory variables; the lagged dependent variable $RATIO_{T+i-1}$ would then be allowed to explain $RATIO_{T+i}$ only for $i=2,3$.[16]

Since the estimates of our pooled regression might be biased in the presence of unobservable heterogeneity across episodes, we also estimate *random effects models* for *FAIL1*, *FAIL2*, and *RATIO*. Controlling for potential unobservable heterogeneity across episodes is particularly important in view of our interest in identifying in-sample state dependence. By neglecting possible episode-specific effects, we would risk confusing *spurious* state dependence due to temporally persistent unobservables with *true* state dependence, namely a genuinely different behavior of inflation after a stabilization has failed or succeeded in early years. A random effects estimator for the simple form of dynamic probit we consider is *consistent* even if only the number of cross-sections tends to infinity (Hsiao, 1986, p. 170).

However, if the episode-specific effects are correlated with the other regressors, the random effects estimator would be characterized by an omitted variable bias. Since the absence of consistent fixed-effects estimates precludes us from computing the Hausman test to verify the existence of a significant correlation between the regressors and the episode-specific effects, we present the pooled regressions as a norm accompanied by the random effects estimates only when the proportion ρ of the variance contributed by the episode-specific variance component is statistically different from zero.[17] In all probit estimates with *FAIL1* and *FAIL2*, the null hypothesis of ρ equal to zero was accepted. When not rejected, as it happened with the continuous measure *RATIO*, the results of the pooled and random effects regressions were nonetheless similar. This is not a surprising result if one considers that our dependent variable, and many of the regressors, are measured relative to their episode-specific value in period *T*.

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[16] This anomalous feature of our model, together with the fact that several explanatory variables in our specification are time-invariant, prevents us from estimating a dynamic regression for *RATIO* with the Anderson and Hsiao (1981) instrumental variable estimator or Arellano and Bond (1991) GMM estimator. Both methods, in fact, require differencing the estimated equation and use past lags of differenced and non-differenced variables as instruments.

[17] This was done using the likelihood ratio test for *FAIL1* and *FAIL2* and the Breusch-Pagan (1980) lagrange multiplier test for *RATIO*. We report the results of these tests for each specification in our tables.

We also report *static* estimates that ignore the possibility of state dependence for our benchmark regression with *FAIL1* (results were similar in static regressions with *FAIL2* and *RATIO*), to assess whether the inclusion of lagged dependent variables critically affects the other coefficients. As for the dynamic specifications, we report pooled or random effects depending on the results of the tests on the episode-specific variance component. We also estimated a static random effects model with serial correlation of the errors, that we do not present but that—when it converged—yielded similar results (for reasons of space these results are not shown but are available from the authors upon request).

F. Endogeneity Issues.

There is a risk that an omitted variable bias might affect the estimates of the coefficients measuring how the nominal anchor and fiscal and monetary adjustment contribute to stabilization failure. This bias may emerge if two conditions are simultaneously satisfied: *i)* the durability of stabilization depends not only on *observable* government policies but also on the *unobservable* "ability" of the government to stabilize; and *ii)* this unobservable "ability" to stabilize also affects the choice of the anchor or the degree of monetary and fiscal adjustment. For example, a government with a "tough" anti-inflation reputation is likely to be more successful than a government with a "weak" reputation for any degree of monetary and fiscal adjustment, and for any nominal anchor, not only because of its stronger "anti-inflation credentials," but also because of its ability to implement other policies, like incomes policies and structural reform policies (which may contribute to make a stabilization successful but are not explicitly controlled for in our regression). If this "tough" government is also more likely to choose an exchange rate anchor and implement a larger monetary and fiscal adjustment than a "weak" government, estimation techniques that neglect this correlation will tend to overestimate the effect of these policies (i.e., fiscal and monetary adjustment and the anchor.)

This potential omitted variable bias is not only of interest to the econometrician but is also of critical importance for the policymaker. If there is an omitted variable bias, standard estimation techniques (e.g., OLS) may indicate that, to disinflate successfully, a government only needs to choose an exchange rate anchor and adopt moderately restrictive monetary and fiscal policies, when in practice its initial reputation and the adoption of incomes and structural policies are equally essential and could be replaced only by much larger fiscal and monetary adjustments.

The empirical labor economics literature has faced very similar problems in estimating the effects of schooling on wages.[18] To the extent that the unobservable "ability" of an individual affects both his school attendance and his wages, wage equations that neglect this correlation would overestimate the returns to schooling. To address this problem, the labor

[18] Gould (2001) first drew an analogy between the labor literature and that on inflation stabilization programs as a way to address the possible endogeneity of the anchor.

literature has developed specific estimation techniques. When the schooling measure is continuous—as, in our case, the measures of monetary and fiscal adjustment are—standard instrumental variable techniques are appropriate. When the schooling measure is dichotomous—as, in our case, the exchange rate anchor dummy is—a two-step procedure devised by Heckman (1979) or a bivariate probit are the appropriate estimation techniques depending on whether the variable measuring the returns is continuous (*RATIO* in our case) or dichotomous (*FAIL1* and *FAIL2* in our case). In the rest of this section, we briefly describe how we use these procedures to correct for the possible endogeneity of the exchange rate anchor. The problems associated with finding an appropriate instrument for the monetary and fiscal adjustment variables—i.e., a variable that affects the marginal cost of monetary and fiscal adjustment but is uncorrelated with the government's ability to adjust and, at the same time, does not contribute directly to the durability of stabilization—prevent us, for the moment, from performing an instrumental variable estimation to address the endogeneity problem of these variables.[19]

When one of the stabilization failure dummies (*FAIL1* and *FAIL2*) is used to measure durability, we take into account the possible endogeneity of the exchange rate anchor dummy (*ANCHOR*) by estimating a bivariate probit of the following form:

$$ANCHOR = \beta_1 x_1 + u_1 \quad , \quad Var(u_1)=1 \tag{1}$$

$$FAIL1_i = \beta_2 x_2 + \gamma_1 ANCHOR + u_2 \quad , \quad Var(u_2)=1 \quad , \quad i=1,2 \tag{2}$$

where x_1 and x_2 are vectors of exogenous variables.

When appropriately identified, this bivariate probit yields an estimate of the covariance between the errors of the two equations ($Cov(u_1, u_2)=\delta$). If this covariance is significantly different from zero, a univariate probit estimation of equation (1) would yield a biased estimate of the coefficient γ_1 measuring the contribution of the anchor to the success of the stabilization. In fact, this is what would happen, if the *unobservable* ability of the government to stabilize affected both the choice of the anchor and the success of the stabilization, because the errors of the two equations would be correlated.

Before estimating the bivariate probit, it is important to note that, if the errors u_1 and u_2 are *not* independent, the simultaneous equation model (1)-(2) is not fully recursive and at least one of the exogenous variables on the right-hand-side of the exchange rate anchor equation (1) should be excluded from the list of regressors in equation (2) to ensure its identification.[20] In our estimates, we identify the model by including among the set of regressors x_1 a zero-one dummy taking on a value of one in all stabilization episodes involving African or

[19] This problem does not have any straightforward solution in the labor economics literature either (see Card, 2001, for a recent survey.)

[20] Maddala (1983, pp.122-124).

Middle-Eastern countries (which in our sample tended not to choose an exchange rate anchor). This regional dummy appears to be unrelated with the probability of a successful stabilization.[21] For each univariate specification (2), we then always estimate also a bivariate probit model (1)-(2). Because of the complexity of this estimation technique, we estimate a bivariate model only for the pooled regression with heteroskedastic errors and not for the random effects model. We report the coefficient estimates from the bivariate probit (equation (2) only), in addition to those from the univariate probit estimation of equation (2), only when the correlation coefficient between the errors of the two equations is significantly different from zero.

When the continuous variable *RATIO* is used to measure durability, we account for the possible endogeneity of the exchange rate anchor dummy (*ANCHOR*) by estimating a mixed structure:

$$ANCHOR = \beta_1 x_1 + u_1 \quad , \quad Var(u_1)=1 \tag{3}$$

$$RATIO = \beta_2 x_2 + \gamma_1 ANCHOR + u_2 \tag{4}$$

where x_1 and x_2 are vectors of exogenous variables.

The key difference from the system (1)-(2) is that, while *ANCHOR* is still a dichotomous variable, *RATIO* is a continuous variable. As a result, the assumption $Var(u_1)=1$ (typical of probit estimation) is sufficient to ensure the identification of this system and the two sets of regressors x_1 and x_2 may include the same variables.[22] The model is then always identified and we can estimate $Cov(u_1, u_2)=\delta$ to test the possible presence of an omitted variable bias. Another difference from model (1)-(2) is that in this case the estimation method can be either maximum likelihood or an equivalent two-step estimator, analogous to that devised by Heckman (1979) for sample selection problems.[23] As in the previous case, we report the coefficient estimates for equation (4) from this procedure, in addition to those from the univariate probit estimation of the same equation, only when the correlation coefficient between the errors of the two equations is significantly different from zero.

III. RESULTS

In this section we illustrate the results obtained from applying the estimation strategy and techniques described in the previous section to our dataset. When explaining the results, we

[21] The percentage of successful stabilizations in African or Middle-Eastern countries is 60% according to criterion 1 and 40% according to criterion 2 in Table 2, while the corresponding figures for the entire sample are 67% and 39%.

[22] Maddala (1983, pp.120-122).

[23] Greene (2000, pp. 933-934).

draw on the relevant strand of the theoretical literature on inflation stabilization. We start by presenting our benchmark estimates for *FAIL1*. We then check the robustness of the main results to the definition of failure by presenting estimates for *FAIL2* and *RATIO*. Finally, we show the results obtained by estimating a similar set of regressions on Easterly's (1996) sample.

A. Benchmark estimates

Tables 4 and 5 summarize the main results and illustrate the steps of the specification search for *FAIL1*. The estimates in *Table 4* correspond to a dynamic probit specification. As a norm we present the univariate *pooled* estimates. We present the maximum likelihood *bivariate probit* estimates only when the Wald test rejects the null hypothesis of no correlation between the errors of equations (1) and (2). This never happens with the dynamic regressions presented in Table 4. We also estimate a *random effects* model for each specification in the tables but, given that the likelihood ratio test always accepts the null hypothesis of zero contribution of the episode-specific variance component, we also do not present the results of this estimation technique for *FAIL1*. We follow the same approach for all full-sample results presented in this paper. Table 5 presents the results for a *static* regression that excludes lagged values of *FAIL1*, thus neglecting possible in-sample state dependence.[24] In this case, there is one instance in which the Wald test rejects the null hypothesis of no correlation between the errors of equations (1) and (2). The bivariate estimates are, however, very similar to the univariate estimates.[25]

Column (1) in each table presents the coefficient estimates for a model with only *initial conditions*, including the *ANCHOR* dummy, *and exogenous controls*. *Column (2)* shows the results for a model that also includes *political economy* variables. *Column (3)* adds *non-policy macro* variables. *Column (4)* adds *policy macro* variables measuring the extent of post-stabilization fiscal and monetary adjustment. At each step, we add a new set of variables and we drop the variables from the previous model that are no longer statistically significant.

The significance of each block of variables in Table 4 provides some preliminary indications on the determinants of stabilization failures. A first striking feature is the very good fit of the model with only exogenous controls and initial conditions. The pseudo-R^2 of this model is

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[24] As mentioned earlier, we also estimated a static random effects model with AR(1) serial correlation. When the estimated coefficent vector converged the results were always confirmed, but we could not get convergence for some of the larger models in columns *(3)* and *(4)*.

[25] Given that columns (3) and (4) in Table 4 already correspond to a static probit because lagged values of *FAIL1* are no longer significant when we add adjustment variables to the dynamic specification, we do not repeat columns *(3)* and *(4)* of Table 4 in Table 5.

0.492 (column *(1)*).[26] Adding political economy variables increases the fit to 0.564 (column *(2)*). Finally, adding non-policy and policy adjustment variables increases the fit to 0.777 (column *(3)*) and 0.837 (column *(4)*) respectively. As the pseudo-R^2 cannot be interpreted as the share of the variance of the dichotomous variable explained by the regression,[27] we also present other measures of goodness of fit. Efron's (1978) R^2 confirms the results obtained with the pseudo-R^2.[28] All specifications in Table 4 predict correctly at least 85 percent of the stabilization outcomes when we use a probability threshold, p^*, of 0.5 to predict failure. As the threshold of 0.5 may not be appropriate in the case of our unbalanced sample with more successes than failures (Greene, 2000, pp. 833-834), we also present the percentage of stabilization outcomes correctly predicted with a lower threshold equal to the sample frequency of failure (p^*=0.196), which confirms the good performance of the estimated model. Finally, we show the effect of lowering p^* to 0.1 with the objective of minimizing the error in predicting failures. With this low threshold, the models without adjustment variables (columns *(1)* and *(2)*) predict correctly about 97 percent of failures. Interestingly, the cost of this improved predictive ability is low, as more than 75 percent of the successes are still predicted correctly. This evidence suggests that, with an accurate forecast of external conditions and a careful assessment of the situation in which the economy is at the beginning of stabilization, our model might give a pretty good indication about the chances of success of inflation stabilizations.

Another interesting result emerging from the process of specification search is that several exogenous variables, initial conditions, and political economy variables remain statistically significant even after controlling for monetary and fiscal adjustment. This reinforces the previous finding about the explanatory power of initial conditions and exogenous controls, as it shows more clearly that the same degree of policy adjustment in different stabilizations may lead to success or failure depending on the evolution of the external environment, the situation of the economy at the beginning of stabilization, and features of the political system. We now proceed to consider the answers that this econometric exercise can provide to some specific questions on the determinants of failed disinflations.

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[26] Note that this successful fit is only in minimal part due to the inclusion of the lagged dependent variable among the regressors: a static pooled regression with the same set of explanatory variables has a pseudo-R^2 of 0.445 (Table 5).

[27] The pseudo- R^2 is McFadden's (1974) likelihood ratio index equal to $1-\ln L/\ln L_0$, where $\ln L$ is the maximized value of the log-likelihood function of the model and $\ln L_0$ is the log-likelihood computed with only a constant term. This measure is bounded by zero and one.

[28] Efron's (1978) R^2 is equal to $1- \Sigma(FAIL1-p)^2/\Sigma(FAIL1-\underline{p})^2$, where p is the predicted probability of failure and $\underline{p}$ is the sample frequency of failure. This goodness of fit measure compares then the predictive ability of the probit estimator to that of a naïve estimator corresponding to the sample frequency of failure.

The "crisis hypothesis": do "bad" initial conditions help?

The estimates in column *(1)* of Table 4 show that higher pre-stabilization inflation (π_{T-1}), lower international reserves (RES_{T-1}), and a negative output gap ($YGAP_{T-1}$) significantly increase the likelihood of success. These results are robust to the estimation method (column *(1)* in Tables 4 and 5). Moreover, the pre-stabilization level of inflation remains very significant even after controlling for other sets of variables (columns *(3), (4),* and *(5)* in both tables). This feature is very clear also from Figure 1, which shows that successful stabilizations are associated with higher pre-stabilization inflation levels.

The possibly counterintuitive positive effect of "bad" initial conditions has many theoretical underpinnings. One is the "crisis hypothesis" posited by Drazen and Grilli (1993), where a surge in inflation can prompt the adoption of a stabilization plan which, although beneficial for society as a whole, would not be vote for as a result of conflict between two different social groups.[29] Only when inflation reaches a sufficiently high level, its welfare cost becomes so high that one of the groups in conflict may be willing to accept shouldering all of the tax burden required by inflation stabilization. Our empirical results can be interpreted as indicative that a surge in inflation may not only determine the timing of the initial stabilization but it would also create social and political conditions that might reduce the chances of failure in the following years.[30]

Other models also predict a positive relationship between high levels of pre-stabilization inflation and the probability of success, albeit for different reasons. Orphanides and Wilcox (1996a) describe an opportunistic central bank that chooses to stabilize whenever pre-stabilization inflation exceeds a given (unobservable) threshold, but not if inflation remains below the threshold. Thus, a non-linear relationship between the pre-stabilization level of inflation and the probability of stabilization arises: a surge in inflation may trigger a stabilization attempt. In Orphanides (1996b), a higher pre-stabilization level of inflation is more likely to induce a government to embark on stabilization, because it increases the cost of not acting.[31] In Milesi-Ferretti (1995), a higher pre-stabilization level of inflation tilts the cost-benefit analysis made by the incumbent government in favor of embarking on a disinflation strategy, although the predictions of this model are confined to the case of democracies.

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[29] Drazen and Easterly (2000) have recently found some empirical evidence supporting the crisis hypothesis when inflation reaches extreme levels.

[30] The association between a low initial stock of international reserves and a greater probability of success may be interpreted along the same lines with the depletion of reserves being a proxy for a recent exchange rate crisis.

[31] However, those models also predict that measures aimed at helping a country's citizens to minimize the costs of inflation (such as indexation of nominal contracts) will reduce the likelihood of starting a stabilization for a given inflation rate.

Does the nominal anchor matter?

The estimates in column (1) of Table 4 indicate that ERBS are less likely to fail within the first three post-stabilization years. Table 5 confirms that the effect of *ANCHOR* is also robust to the exclusion of the lagged dependent variable. In this case, there is also one specification (column *(2)*) in which the Wald test of correlation between the errors of the *ANCHOR* and *FAIL1* equations rejects the null hypothesis of no correlation suggesting that the univariate coefficient estimates might be biased. The *ANCHOR* coefficient remains, however, negative and statistically significant also in the bivariate probit estimates (column *(2 BIV.)*). Interestingly, the effect of ERBS can only be identified when other determinants of failure are properly controlled for. In our sample, in fact, the percentage of ERBS that failed by *T+3* is almost identical to that of the entire sample (Table 2).

The role of the nominal anchor in determining the likely success of a stabilization plan is a contentious issue. In the mid-1980s several authors favored the use of the exchange rate (accompanied by other "anchors" such as price and/or wage controls) to act as a coordinating device in order to bring inflation down from chronically high levels at relatively low output costs.[32] Fischer (1986) shows that exchange-rate based stabilizations are likely to be expansionary, in contrast with money-based stabilizations, which are accompanied by an initial recession. To the extent that positive output dynamics help reinforce the initial adjustment (including through a better fiscal position), exchange rate based stabilizations should be more likely to succeed.

Another argument in favor of exchange-rate based stabilizations came from Giavazzi and Pagano (1986), who explained that a strong commitment to a highly visible variable such as the exchange rate (as opposed to a monetary aggregate) had the advantage of imposing the financial discipline needed to support the peg. This argument, initially developed in the context of a multilateral arrangement (the European Union), gained popularity in the early 1990s among developing countries (Aghevli et. Al, 1991), but was subsequently challenged, both theoretically and empirically. On theoretical grounds, Tornell and Velasco (1995) show that the extent to which a peg imposes fiscal discipline is related to the discount rate of the policymaker and, thus, no generalizations could be made about its benefits.

Finally, it must be stressed that the coordinating and/or discipline role of the exchange rate anchor discussed above relates not only to the behavior of fiscal and monetary variables. A credible exchange rate anchor is expected to be a powerful tool for disciplining wages and interest rates.

Our estimates cannot distinguish between the different roles of an exchange rate target proposed in the literature but indicate that the contribution of an exchange rate anchor

[32] See, for example, Cukierman, 1988.

to the success of inflation stabilizations goes beyond the positive effect it might have on post-stabilization output dynamics and its disciplining role on fiscal and monetary policies. It is noteworthy, in fact, that the coefficient of *the ANCHOR dummy remains statistically significant even after controlling for post-stabilization GDP growth (column (3)) and fiscal and monetary adjustment (column (4))*. The statistical significance of the exchange rate anchor can then be attributed to its impact as pre-commitment or coordinating device on inflation expectations.

The determinants of ERBS

To control for the possible endogeneity of the *ANCHOR* dummy we estimated an auxiliary probit equation for the determinants of ERBS. This equation is of interest on its own merits. Why do certain governments choose to use the exchange rate as a nominal anchor but not others? Table 6 provides some answers. The overall fit of the probit is very good with a pseudo R^2=0.804 and over 90 percent of correct predictions at all the probability threshold levels considered, including the sample frequency of ERBS (26 percent). The dummy for African and Middle-Eastern countries (*AFRME*), included to ensure identification of the bivariate probit (see Section II.F), is statistically significant. ERBS tend to be chosen by countries with higher initial GDP per capita ($GDPPC_T$), larger initial stock of international reserves (RES_{T-1}), more favorable fiscal balances (GB_{T-1}), and higher initial GDP growth (GDP_{T-1}). A more depreciated real exchange rate prior to the stabilization ($RERGAP_{T-2}$) also tends to be associated with ERBS. Whereas an open current account ($OPENCA_{T-1}$) increases the chances that a country might choose an exchange-rate-based stabilization, an open capital account ($OPENKA_{T-1}$) tends to reduce them.[33]

We also find some evidence that exchange rate anchors have been used as a precommitment device in countries where the anti-inflation credibility of the government was low. Countries that have a long history of high inflation ($YEARS_{\pi>40}$) tend to choose ERBS. Moreover, we find that an important feature of the political institutions of a country contributes to determine the choice of the nominal anchor. The larger are the institutional constraints of the executive authority—measured by the variable $XCONST_0$—the more likely the country is to choose the exchange rate as a nominal anchor.[34] This result merits further investigation but

[33] Gould (2001) estimates an equation for the choice of ERBS on a smaller sample and with fewer regressors. He also finds the pre-stabilization level of international reserves to be a significant predictor of ERBS.

[34] This variable (obtained from the *POLITY IV* dataset, see Data Appendix) takes values ranging from 1 ("unlimited executive authority") to 7 ("executive parity or subordination") and aims at measuring the extent of the institutional constraints on the decision-making powers of the chief executive, whether an individual or a collective executive. This variable captures the degree of checks and balances between the various parts of the government. In Western democracies the executive branch is typically constrained by the legislative and judicial branches of government. Certain institutional constraints may bind, however, also the

(continued)

suggests that countries where the government is more constrained by the legislature tend to choose an exchange rate anchor to stabilize, consistently with its predicted disciplining and precommitment role.

The Role of Real and Financial Openness

The degree of openness of the economy is another potentially important determinant of the success or failure of disinflation. We measure it with two dummy variables taking on values of one in the absence of restrictions on current account and capital account transactions respectively. The estimates in column (1) of Table 4 indicate that both a high degree of current account ($OPENCA_{T-1}$) and capital account ($OPENKA_{T-1}$) openness prior to stabilization reduces significantly the chances of failure. The effect of the openness of the current account is, however, not robust, as $OPENCA_{T-1}$ drops out in the specification with political economy variables (column *(2)*). This may reflect the fact that the openness of the current account is actually proxying for other political and institutional features of the country. The effect of the openness of the capital account survives, instead, the inclusion of other sets of variables and is also robust to dropping $FAIL1_{T+i-1}$ (Table 5).

Interestingly, however, countries with an open capital account tend to be more vulnerable to international interest rate shocks. The positive coefficient on the variable interacting U.S. interest rates with the lagged value of the capital account openness dummy ($USi_{T+i}*OPENKA_{T+i-1}$ in columns *(3)-(4)*) indicates that a positive shock to U.S. interest rates increases the probability of failure significantly more in a country with an open capital account.

These results are broadly in line with the existing literature. Building on Kydland and Prescott (1977), Romer (1993) explains the link between real openness and low inflation by showing that the magnitude of the inflation bias in the absence of a pre-commitment technology in monetary policy is inversely related to the degree of openness of the economy: greater openness reduces the response of output to an unexpected increase in demand.[35] Romer finds that inflation tends to be consistently higher in less open economies—with the exception of a small group of low inflation, industrialized countries. Romer's results are intended to capture mainly cross-country differences, with the degree of openness considered an exogenous variable. Lane (1997) re-examines the issue within a richer economic structure and finds the same result: an inverse relationship between openness and the time consistent discretionary inflation rate. However, Lane derives a clear policy implication from his results: trade liberalization may lead to a concomitant reduction in inflation.

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chief executive of non-democratic regimes. Examples of accountability groups in this case are a ruling party in a one-party system or the military in coup-prone countries.

[35] This occurs through a direct channel (part of the expansion in demand is spent on foreign goods), and an indirect channel (the expansion in demand leads to a deterioration in the country's terms of trade).

Dornbusch (1991) contains a simple analysis of the role of an open capital account in determining the success of inflation stabilization. By assuming that net private capital inflows will be proportional to the perceived probability of success, he finds a Stackelberg solution to his one-shot stabilization game (the government selects its adjustment effort first, speculators make their decision next) in which, with an open capital account, stabilization benefits from a multiplier effect derived from the supportive speculation. Although the possibility of speculative inflows render the magnitude of the optimal adjustment effort uncertain, Dornbusch finds a general set of conditions under which both the optimal adjustment effort and the probability of success increase. However, he also shows that under other circumstances increased capital mobility may reduce the probability of success. This ambiguous effect of capital account openness is consistent with our estimation results as the increased vulnerability to international interest rates shocks during the adjustment phase might offset the reduction in the likelihood of failure due to an open capital account.

Orphanides (1996a) examines the role of an open capital account and the role of prospective speculative capital flows in a dynamic setting. He also assumes that inflows are proportional to the perceived probability of success of the program and finds that the optimal intertemporal distribution of adjustment (between two stabilization stages) is affected by the prospect of such flows: it is optimal to frontload adjustment to deliberately increase the perceived likelihood of completion, which triggers capital inflows and reduces the magnitude of the needed adjustment in the second stage, thus increasing the probability of completion.

External conditions: is good luck important?

We introduce time-varying *exogenous controls* in all specifications to control for the effect that external developments may have—directly or indirectly—on domestic inflation, thus contributing to success or failure of stabilizations. Column *(1)* in Table 4 shows that higher U.S. interest rates (USi_{T+i}) and a reduction in import demand from trading partners between *T* and *T+1* ($XDEM_{T+1}$-$XDEM_T$) significantly increase the probability of failure.[36] The impact of shocks to the import demand from trading partners on the probability of failure is also very evident from the bivariate correlations shown in Figure 4. As mentioned above, the effect of U.S. interest rates on the probability of failure is much larger in countries with an open capital account. These results are robust to the addition of other explanatory variables and the estimation method. The fact that external shocks remain statistically significant after controlling for output growth, international reserve changes, and fiscal and monetary developments is noteworthy and suggests that these shocks have effects beyond the indirect ones captured by these variables.

[36] We also experimented with other measures of external shocks by including a terms of trade index and U.S. inflation. They were not statistically significant in any specification with the exception of the terms of trade index, which was statistically significant in the regressions with the continuous dependent variable presented in Section III.B.

There is general consensus in the economic literature about the potential disruptive effect of external shocks. This is highlighted in Dornbusch's (1991) model, where successful implementation of a stabilization program requires that the difference between the realization of a random external variable and domestic absorption exceeds the initial level of reserves. Our results are also related to the empirical work of Easterly et al. (1993) and Calvo, Leiderman, and Reinhart (1993), as U.S. interest rates tend to be a measure of supply factors in international capital markets.

State dependence: do past failures reduce the chances of success?

The results in Table 4 provide two different indications on the relevance of state dependence in inflation stabilizations. First, a long history of high inflation, measured by the number of pre-stabilization years in which inflation has remained above 40 percent ($YEARS_{\pi>40}$), tends to increase the probability of failure. This initial condition remains significant when we add other regressors and drop $FAIL1_{T+i-1}$. Second, column *(1)* seems to suggest that there is also in-sample state dependence. The positive coefficient on $FAIL_{T+i-1}$ indicates that failure (success) in periods *T+1* or *T+2* tends to be followed by failure (success) in the following periods. This variable, however, is no longer statistically significant when we add non-policy and policy adjustment variables suggesting that this form of state dependence may be spurious and due to omitted adjustment variables in the specifications of columns *(1) and (2)*. However, in the regressions with *FAIL2* and *RATIO*, presented in Section III.B, the lagged dependent variable never drops out of the regression.

The importance of the length of the pre-stabilization period with high inflation in reducing the chances of a successful stabilization has several possible justifications. First, it may simply reflect reputational and credibility factors. The longer a country has experienced high inflation, the smaller its anti-inflation reputation, and, *ceteris paribus*, the more likely is the stabilization to fail. A credibility argument can explain in-sample state dependence: initially successful stabilizations increase the government's credibility and make the continuation of the stabilization easier, while the opposite happens when stabilizations fail already in period *T+1* or *T+2*. The role of reputation and credibility has been extensively studied in the literature on inflation stabilization (see the critical survey of the literature in Rogoff (1987)).

Second, given that long periods of high inflation also tend to be associated with a high degree of wage and interest rate indexation, $YEARS_{\pi>40}$ can also be proxying for the presence of indexation, confirming the traditional view that indexation tends to make inflation stabilization more difficult.

Third, the positive effect of $YEARS_{\pi>40}$ on the probability of failure may also be interpreted within the framework of Dornbusch (1991) model as a proxy for the political marginal cost of failure. A long history of high inflation may indicate that the government has lost a significant amount of political capital with each failed stabilization. Thus, a higher value of $YEARS_{\pi>40}$ would suggest that the marginal political cost of failure is low and the probability of another failure is higher.

On the other hand, the positive estimated coefficient on $YEARS_{\pi>40}$ does not support Alesina and Drazen's (1991) war of attrition model. In that model, stabilization would be preceded by a protracted period of high inflation. As time goes by, the costs of inflation pile up and the likelihood that one of the two groups involved in the war attrition gives up, allowing the stabilization to succeed, increases. This model can then be loosely interpreted to suggest that the longer the pre-stabilization period of high inflation, the more likely it is that a solution to the underlying social conflict is found, so that stabilization can start and can be successfully completed. This interpretation would imply a negative coefficient on $YEARS_{\pi>40}$, which is not what we find. This result by itself does not necessarily disprove war of attrition stories of the timing of stabilization. Our estimated coefficient simply suggests that this variable is more likely to capture the impact of low anti-inflation credibility, the presence of indexation, or a lower marginal political cost of failing in yet another attempt.

The role of political conditions and institutions

Which political institutions help stabilize inflation?

Column *(2)* in Table 4 shows the effect of including variables measuring political institutions and conditions in the probit regression. The pseudo-R^2 increases from 0.492 to 0.564 and improves the predictive power of the model, especially its ability to predict correctly successful stabilizations (*FAIL1=0*). The inclusion of this set of variables makes statistically insignificant the dummy measuring whether the current account was open prior to the stabilization ($OPENCA_{T-1}$) and the pre-stabilization output gap ($YGAP_{T-1}$), which are dropped from the regression.

Our sample spans a wide range of political institutions and includes several major transitions (pre-stabilization or in-sample) from one political regime to another. As several political regimes are not democratic, it is essential to control for the degree of democracy before considering other features of democratic political institutions such as, for example, the electoral system.

We base our analysis of the degree of democracy of each political regime on the recently updated *POLITY IV* dataset. We first use this dataset to build a zero-one dummy taking value one in democracies, which we then use to study the effect of political economy variables that are relevant only for democracies. To assess the impact of the degree of democracy on the likelihood of a successful inflation stabilization, we also take advantage of the rich classification of the *POLITY IV* dataset and experiment in our regressions with the seven component variables of the democracy and autocracy indices, as well as with a variable measuring the durability of the political regime (defined as the number years since the most recent three-point change in the polity score over a period of three years or less).[37] We find

[37] This dataset classifies political regimes on the basis of six component variables corresponding to three features of the chief executive recruitment process (regulation,

(continued)

four of these variables to be statistically significant ($DURREG_T$, $XCONST_{T+i-1}$, $XROPEN_{T+i-1}$, and $PARCOMP_{T+i-1}$.)

Column *(2)* in Table 4 shows that the durability of the political regime at the start of the stabilization ($DURREG_T$) tends to reduce the likelihood of failure, suggesting that long-standing political regimes are more likely to sustain the stabilization effort. As $DURREG_T$ remains statistically significant when we add adjustment variables (columns *(3)-(4))*, the role of the durability of the political regime goes beyond predicting successful fiscal and monetary adjustment and may reflect the initial positive effect of lasting political regimes on expectations and credibility. This effect is also robust to dropping $FAIL1_{T+i-1}$ (Table 5).

The importance of the durability of the political regime has a theoretical underpinning in previous studies that highlight how political instability can explain high inflation and reliance on seigniorage as a source of revenue (Cukierman, Edwards, and Tabellini (1992)). This prediction reflects a *strategic* behavior on part of the government. If a government is uncertain about being reappointed, it has an incentive to maintain the inefficient tax system associated with high levels of seigniorage in order to constrain the action of future governments and discourage them "from collecting taxes and spending them on goods that are not valued by the incumbent policymaker." Of course, the more polarized is the political system (i.e., the greater the difference in preferences between the incumbent government and its potential successors), the greater is the strategic incentive to use the inflation tax. While this model has been generally tested by constructing a measure of political instability based on transfers of power and government changes, rather than political *regime* changes as in this paper, we think that for the countries in our sample—many of which experienced several transitions from and to the democracy over a relatively short horizon—the durability of the political regime is the appropriate measure of political stability. (We also tried alternative measures of political instability based on the frequency of past government changes but they were not statistically significant.)

A second result regarding the role of political institutions is that the larger are the institutional constraints of the chief executive ($XCONST_{T+i-1}$) the more likely is stabilization to fail.[38] We lag *XCONST* one period to avoid simultaneity problems. The statistical

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competitiveness, and openness), the independence of the executive authority, and two features of political competition (regulation and competitiveness of participation). These indices are then combined to obtain two summary indices of democracy and autocracy, each ranging from zero to 10. The difference between the democracy and autocracy index yields a polity index ranging from –10 to + 10 with political systems being usually considered democracies only when the polity index is positive. We follow the same convention in building our zero-one democracy dummy.

[38] This variable takes values ranging from 1 ("unlimited executive authority") to 7 ("executive parity or subordination".)

significance of this variable is robust to the exclusion of $FAIL1_{T+i-1}$ and to the estimation method. An obvious question raised by this result is whether the statistical significance of $XCONST_{T+i-1}$ is entirely attributable to the effect of some successful inflation stabilizations performed in our sample by non-democratic regimes (for example, Chile 1975 or Brazil 1966.) The answer is no. If we estimate two separate coefficients of $XCONST_{T+i-1}$ for democratic and non-democratic regimes, we find that they are both positive and statistically significant and not significantly different. This indicates that the institutional constraints of the chief executive have an impact on the probability of stabilization failure in both democracies and non-democracies.

To the extent that democratic presidential systems are associated with fewer institutional constraints on the chief executive, the statistical significance of $XCONST_{T+i-1}$ may suggest that presidential systems are less likely to fail. We can check this hypothesis using Persson and Tabellini's (2001a) classification of presidential and parliamentary systems based on whether the executive is accountable to the legislature through a vote of confidence. We find that the two measures are indeed related. In the subsample of observations corresponding to democracies, almost all parliamentarian regimes (93 percent) has an *XCONST* value of 7 corresponding to the maximum level of constraints of the chief executive. While some 46 percent of the presidential systems are equally constrained, the rest has lower *XCONST* scores. The finer distinction provided by the *XCONST* variable is, however, important because using the Persson and Tabellini's dummy for presidential systems, we could not find a statistically significant effect of presidential regimes.

Column *(2)* in Table 4 shows that also greater openness in the chief executive recruitment process ($XROPEN_{T+i-1}$) and greater political competition ($PARCOMP_{T+i-1}$) tend to be associated with a smaller likelihood of failure. Only $XROPEN_{T+i-1}$, however, remains statistically significant when we add the adjustment variables. Different estimation methods confirm these findings. As high values of $XROPEN_{T+i-1}$ and $PARCOMP_{T+i-1}$ tend to be associated with democratic regimes, these results suggest that, while constraints on the chief executive tend to increase the likelihood of stabilization failure, selecting the chief executive through an open process and allowing competition in the political arena contribute to the success of inflation stabilization.

The last political institution we consider is the electoral system. As majoritarian systems have been found to be associated with smaller levels of government expenditure,[39] and given the fiscal origin of many inflationary problems, we check whether majoritarian electoral systems are less likely to be associated with stabilization failures by creating a zero-one

[39] Persson (2001) and Persson and Tabellini (2001a,b). See also Milesi-Ferretti, Perotti, and Rostagno (2001) on the effects of the size of electoral districts—as a measure of majoritarian versus proportional electoral systems—on government expenditure composition. Persson, Tabellini, and Trebbi (2000) have extended this analysis to the study of the impact of political institutions on corruption.

dummy that takes value one when the electoral system is majoritarian and the political system is democratic. We use the definition of majoritarian system of Persson and Tabellini's (2001a), which is based on a pluralty electoral rule (only the winners of the highest vote shares get seats in a given district) and neglects the district size, which for many countries in the sample would be unavailable. We lag this dummy one period to avoid simultaneity problems (MAJ_{T+i-1}). Unfortunately, we cannot estimate its effect on *FAIL1* because MAJ_{T+i-1} is too successful: whenever an electoral system is majoritarian, *FAIL1* takes value zero indicating that the stabilization does not fail. As a result, probit estimates cannot be performed. When we use, however, the two alternative measures of failure discussed in section III.B (*FAIL2* and *RATIO*), we do not have this perfect prediction problem and we find that MAJ_{T+i-1} significantly reduces the chances of stabilization failure.

How does social and political cohesion matter?

Social tensions, a divided and polarized government, or a split between the executive authority and the legislature could be symptoms of an ongoing "war of attrition"—as described in Alesina and Drazen (1991) and Drazen and Grilli (1993)—that might postpone the stabilization of inflation or cause the failure of an initially successful attempt.

We first consider whether an indicator of social tensions based on the number of strikes, anti-government demonstrations, and riots, tends to increase the probability of stabilization failure. We obtain these three variables from Banks' Cross-National Time-Series Data Archive and then, to limit the number of regressors, we construct an index corresponding to their first principal component, which accounts for about 60 percent of the overall variance. As the impact of social tensions is likely to be time and country specific, we use as a regressor the *lagged* deviation of this index from its moving average over the previous three years ($SOCCONF_{T+i-1}$). An increase in this variable would then measure an increase in social tensions in relation to the previous three years. We lag this measure to avoid simultaneity problems. Table 4 shows that an increase in $SOCCONF_{T+i-1}$ tends to augment the probability of stabilization failure as predicted by war of attrition models. This result is confirmed when we add adjustment variables or we estimate a static regression. Other proxies of social fragmentation, such as, for example, a measure of ethnolinguistic fractionalization, were not statistically significant.

We considered three measures of political cohesion. The first two indices are the Roubini and Sachs (1989) index and the government polarization index from the Database of Political Institutions of the World Bank. The Roubini and Sachs index ranges from zero to three with 0 corresponding to the maximum of political cohesion (a one-party majority parliamentary government or a presidential government, with the same party in the majority in the executive and legislative branch) and three to the minimum (minority parliamentary government). The polarization index ranges from zero to two and measures the maximum difference of orientation among government parties. The third is an index of party coalitions from Banks' Cross-National Time-Series Data Archive that ranges from zero to three. As the three indices are very correlated and tend to perform similarly in the regressions, we construct an index corresponding to their first principal component, which accounts for about

70 percent of the overall variance ($POLINCOH_{T+i-1}$). We lag the index one period to avoid simultaneity problems.

Contrary to the theoretical predictions, the estimated coefficient of $POLINCOH_{T+i-1}$ is negative suggesting that less cohesive governments are *less* likely to fail. This result is robust to the addition of the adjustment variables and to the estimation technique. As shown in Section III.B, these estimates are also robust to the variable used to measure stabilization failure. While this result is puzzling at first sight, it reflects the fact that some successful disinflations were implemented by "national unity" coalitions among the major parties in parliament (for example, Israel 1986) or by minority governments that reached an agreement with opposition parties. Once these broad-based stabilization efforts begin, they may be more likely to be sustained over time because all the key political players have a stake in the success of the stabilization and few can benefit from its failure. This evidence casts some doubts on the mechanism through which war of attrition models foresee the end of an high inflation episode. According to these models, the end of high inflations should be associated with one of the two groups involved in the war attrition giving up and suffering the redistributive costs of the stabilization. By contrast, our estimates suggest that some stabilizations are associated with a cooperative agreement among the major parties. Further research could investigate whether this result might depend on our focus on the durability of stabilization attempts and on high—as opposed to moderate—inflation stabilizations.

The contribution of non-policy adjustment variables

Column *(3)* in Table 4 presents the results of a specification search that takes into account a set of non-policy macro variables. These variables improve considerably the fit of the regression with the pseudo-R^2 increasing from 0.564 to 0.777 and the precision with which especially successful stabilizations are predicted. We find four statistically significant variables: a measure of real exchange rate appreciation ($RERNOFAIL_{T+i-1}$), GDP growth (GDP_{T+i-1}), the increase in international reserves as a ratio to imports from year T-1 ($RES_{T+i-1} - RES_{T-1}$),[40] and a variable interacting the level of U.S. interest rates at time $T+i$ with a dummy indicating whether the capital account was open in time $T+i-1$ ($USi_{T+i}*OPENKA_{T+i-1}$.) All variables are lagged one period to avoid simultaneity problems. The inclusion of this set of adjustment variables makes statistically insignificant the pre-stabilization stock of international reserves (RES_{T-1}) and the variable measuring the degree of political competition ($PARCOMP_{T+i-1}$), which are dropped from the regression together with $FAIL1_{T+i-1}$ $(i=2,3)$. We have already commented on the significance of $USi_{T+i}*OPENKA_{T+i-1}$ and on the implications of the non-significance of the lagged dependent variable. We shall then discuss only the other results.

[40] We also tried to measure reserves as a ratio to M2 but the fit was better with the ratio to imports.

To find a statistically significant effect of real exchange rate appreciation on the probability of failure, we need to control for whether the stabilization has already failed or not. Specifically, the variable $RERNOFAIL_{T+i-1}$ is derived by multiplying a real exchange rate index equal to 100 in period T (RER_{T+i-1}) by a zero-one dummy taking value one when the stabilization has not yet failed in period $T+i-1$ $(1-FAIL1_{T+i-1})$.[41] Conditioning the effect of the real exchange rate on the continuation of a successful stabilization is important because stabilization failures tend to be associated with large real depreciations. As a result, introducing the real exchange rate among the regressors without controlling for whether the stabilization has yet failed would result in appreciated real exchange rates being associated with successful stabilizations and depreciated real exchange rates being associated with failures. The interesting question is, instead, whether successful stabilizations that have experienced a real appreciation are more likely to fail in the following period. The answer to this question is positive as indicated by the statistically significant positive coefficient on $RERNOFAIL_{T+i-1}$. It is important to note that, as the effect of the real exchange rate appreciation is conditioned on the past success of stabilization, in-sample state dependence continues to play a role also in the estimates of columns *(3)* and *(4)*.

Lagged GDP growth (GDP_{T+i-1}) tends to reduce the chances of failure. Positive growth may contribute to the success of inflation stabilization both directly by improving the fiscal position of the government and indirectly by creating the necessary consensus to sustain the adjustment effort. This positive association between growth and successful stabilizations emerges very clearly also from the bivariate correlations shown in Figure 5. This result indicates that, in our sample of high inflation stabilizations, there is no evidence of the "sacrifice ratio" (in terms of foregone GDP growth) found by Ball (1994) for industrial countries. This evidence is consistent with the findings of the literature on ERBS but our results suggest that the positive association with growth extends to the wider category of successful stabilizations.[42] This result needs, however, to be qualified further. The estimates with the other two measures of stabilization failure discussed in Section III.B show that, during the adjustment phase, not only GDP growth but also the output gap matters, with more positive output gaps being associated with failures. (In the regressions with *FAIL1*, the output gap had the same sign we found in the estimates with *FAIL2* and *RATIO* but it was not statistically significant.) This suggests that growth is beneficial as long as it does not cause the economy to grow above potential. The bivariate correlations shown in Figure 6 are consistent with this interpretation.

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[41] We set $RERNOFAIL_{T+i-1}$ to zero in period $T+1$ because the lagged value of *RER* in $T+1$ is equal to 100 for all countries and it would be meaningless to try to use this constant value to explain the stabilization outcome in period $T+1$.

[42] Gould (2001) also points out that the positive correlation between inflation stabilization and growth is not limited to ERBS.

The estimates in column *(3)* indicate that an increase in international reserves from year T-1 (RES_{T+i-1} - RES_{T-1}) is associated with successful stabilizations. This is evident also from the bivariate analysis in Figure 7. An increase in international reserves may be important because it reduces the vulnerability of the stabilization program to external shocks or because it reflects confidence effects, especially in the case of ERBS. This interpretation has solid foundations in the theoretical literature.

In Dornbusch (1991), the implementation of a stabilization program is associated with a net flow of foreign exchange which, if negative, must exceed the level of reserves. Thus, a higher level of reserves increases the likelihood of success. A similar condition is required for successful completion of stabilization in Orphanides (1996a, 1996b). However, in those models the stock of reserves plays a more complicated role. First, as in Dornbusch (1991), it directly increases the chances of success of a program for a given adjustment effort, and thus, the likelihood that it will be implemented. Second, since (i) the necessary degree of adjustment is a decreasing function of the stock of reserves, and (ii) the current level of reserves is subject to a stochastic component, a positive shock to available reserves reduces the magnitude (and its associated welfare cost) of adjustment and, thus, makes it more likely that a stabilization will be implemented. Orphanides (1996a) shows that, having decided to start a program, a government facing a positive shock on reserves during the first stage of stabilization will be more likely to complete its adjustment in the second stage, as the needed second dosage of adjustment will also be lower. Werner (1999) also shows that a high initial stock of reserves increases the likelihood that the first stage of stabilization will be undertaken since a loss in reserves is needed during the first stage (when inflation is lowered by pegging the exchange rate but not adjusting domestic absorption).[43]

The impact of policy adjustment variables

Column *(4)* in Table 4 presents the results of a specification search that takes into account *policy* macro variables. These variables improve the fit of the regression further with the pseudo-R^2 increasing from 0.777 to 0.837. Consistently with the bivariate association shown in Figures 2 and 3, the change in fiscal balances (GB_{T+i-1} - GB_T) and bank credit growth (DC_{T+i-1}) are statistically significant with the expected signs. These results are robust to the estimation method and confirmed in the static regression. All the variables included in the specification of column *(3)* remain statistically significant. This fact is noteworthy because it indicates that the same fiscal and monetary adjustment may result in a stabilization success or in a failure depending crucially on the realization of the many other factors we identified in the previous regressions.

[43] The author also shows that a government without enough reserves can undertake the first stage of stabilization if it can borrow abroad. In this case, the higher the interest rate paid on public debt, the stronger the needed adjustment will be in stage two and, thus, the less likely that stage two will be successfully implemented.

It is also remarkable that both fiscal and monetary adjustment are statistically significant. The fact that the fiscal adjustment variable remains statistically significant even after controlling for domestic credit growth suggests that smaller fiscal deficits have an effect on the disinflation process over and above their mechanical direct effect on monetary aggregates via a smaller monetization. It is also interesting that fiscal balances continue to play a role after controlling for GDP growth, whose statistical significance may in part reflect the positive effect that growth is bound to have on fiscal adjustment. Conversely, the fact that domestic credit growth remains statistically significant even after controlling for fiscal developments suggests that—at least in this sample of countries and with this measure of stabilization failure—fiscal developments do not fully account for disinflation and that domestic credit conditions are also important. This is consistent with stabilizations in which the initial fiscal tightening is accompanied by capital inflows that lead to a domestic credit boom and ultimately to the failure of the stabilization.[44]

There is no lack of reference to the importance of monetary and fiscal adjustment in the theoretical (and policy) literature on inflation stabilization. In the Dornbusch (1991) model, for example, as well as in the extensions by Orphanides (1996a, 1996b), adjustment plays a key role. Macroeconomic adjustment is typically modeled as a reduction in domestic absorption, which can be easily associated with fiscal retrenchment or a tightening of monetary conditions.[45]

Alfaro (1999) and Werner (1999) provide another interesting perspective on the role of adjustment by studying why it may be optimal to undertake stabilizations that are doomed to fail, where failure is associated with the lack of adoption of meaningful macroeconomic adjustment. Alfaro (1999) models temporary exchange rate based stabilizations that are not sustainable in the long-run, because they are not accompanied by the necessary supporting fiscal policies. She focuses on the distributive consequences of temporary policies in a setting where economic agents differ in their initial endowments (tradable vs. non tradable goods). An unsustainable exchange-rate-based stabilization is shown to be a way to engineer a large real exchange rate appreciation and a transfer of resources in favor of the non-tradable sector and, thus, a rational political decision aimed at favoring a given group in society. In Werner's (1999) two-stage model, the first step of stabilization is also modeled as an eventually unsustainable exchange-rate-based stabilization. However, its implementation is aimed at building a consensus (economic agents learn gradually about the benefits of lower inflation) to implement the fiscal adjustment needed for the ultimate success of the stabilization plan.

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[44] We also experimented with a lending boom measure equal to the lagged difference of domestic credit growth and nominal GDP growth, which was not statistically significant.

[45] In terms of outcomes, it can be proxied by the evolution of the current account balance, although this variable will also be affected by developments in external demand, international interest rates and the terms of trade.

In the absence of a good understanding of the benefits of low inflation, fiscal adjustment would be rejected by the population.

B. Robustness to different measures of failure

In Tables 7 and 8 we present the dynamic regressions for the dichotomous measure of failure *FAIL2* and the continuous measure *RATIO*. In the case of *FAIL2*, we present the pooled regression for all specification as the likelihood ratio and Wald tests always reject the null hypotheses of an episode-specific variance component and of correlation between the errors of the *FAIL2* and *ANCHOR* equations. In the case of *RATIO*, instead, we also present the random effects estimates for all specifications and those with the Heckman correction for the model with only initial condition and external variables. All estimation techniques yield very similar results. *Column (1)* in both tables presents the coefficient estimates for a model with only *initial conditions and exogenous controls*. *Column (2)* shows the results for a model that also includes *political economy* variables. *Column (3)* adds *non-policy and policy macro* variables. (For reasons of space, we skip presenting the intermediate step with non-policy adjustment variables). We conducted the specification search by adding progressively blocks of variables as we did with *FAIL1*.

The overall fit of the model for *FAIL2* is not as good as the one for *FAIL1* but the regression with only exogenous controls and initial conditions still has a pseudo-R^2 of 0.315. The addition of political economy variables increases substantially the fit of this regression raising the pseudo-R^2 to 0.501. Also the share of outcomes correctly predicted increases baove 85 percent with both a conventional 0.5 threshold and a threshold equal to the sample frequency of failure (0.413) . With a threshold equal to 0.2, the model would correctly predict 90 percent of failures and 70 percent of successes. Taking adjustment variables into account further increases the pseudo-R^2 but only to 0.549 and leaves the predictive ability of the model almost unchanged. The fit of the model for *RATIO* with only exogenous controls and initial conditions is extremely good with an R^2 of 0.581. The addition of political economy and adjustment variables increases the R^2 only marginally to 0.618 and 0.632 respectively.

Initial conditions, external shocks, and state dependence

The estimates with *FAIL2* and *RATIO* confirm the indications obtained from the *FAIL1* regressions on the most relevant initial and external conditions. Higher pre-stabilization inflation, lower international reserves, and a negative output gap significantly increase the likelihood of success, confirming the evidence in support of the "crisis hypothesis".[46] We also find confirmation of the increased chances of success associated with an exchange-rate-

[46] In addition, there is some indication that also a negative pre-stabilization fiscal balance (GB_{T-1}) contributes to the likelihood of success, suggesting that a fiscal crisis can also play a role.

based stabilization. In the case of *FAIL2*, however, the *ANCHOR* dummy becomes statistically significant only after we control for political economy variables. An open capital account prior to stabilization continues to reduce the probability of failure, but we no longer find evidence that it makes the stabilization more vulnerable to interest rate shocks, nor we find any indication that an open current account matters. Among external conditions, a reduction in import demand from trading partners significantly increases the probability of failure for both *FAIL2* and *RATIO*. The level of U.S. interest rates, instead, matters only with *FAIL2*, while terms of trade shocks ($TT2_{T+i}$) are a good predictor of the evolution of *RATIO*. Pre-stabilization and in-sample state dependence are important with both *FAIL2* and *RATIO*. The number of pre-stabilization years with inflation above 40 percent increases both measures of failure. The statistical significance of $FAIL2_{T+i-1}$ and $RATIO_{T+i-1}$ is an indication that failure (success) in any of the post-stabilization years tends to be followed by another failure (success). Contrary to the estimates with *FAIL1*, the in-sample state dependence remains statistically significant even after controlling for the adjustment variables.

Political economy variables

Column *(2)* in Tables 7 and 8 shows that the same set of political economy variables contributes to explain *FAIL2* and *RATIO*. Only two, however, remain statistically significant in the *RATIO* regression when we add non-policy and policy adjustment variables. As in the *FAIL1* estimates, the more durable is the political regime and the fewer are the constraints on the chief executive the less likely is the stabilization to fail. Democracies with majoritarian electoral rules (MAJ_{T+i-1}) are also less likely to fail (as already mentioned, this coefficient could not be estimated for *FAIL1* because all majoritarian regimes were successful under that measure of failure). The regressions with *FAIL2* and *RATIO* also confirm the surprising result that less politically cohesive regimes are less likely to fail, which, as discussed in Section III.A, can be attributed to the successful stabilizations implemented by some "national unity" governments.

This new set of regressions also highlights the relevance of a zero-one dummy $CE_{years<=3}$, that takes value one when the chief executive of the country has been in power for less than three years. This variable was estimated with a similar sign but it was just below the significance threshold in the *FAIL1* regressions. The estimated coefficient indicates that new chief executives are less likely to fail. Reputational models of monetary policy in which the public is unsure about the policymaker's ability to precommit to a low level of inflation (Barro, 1986) or about its inflation preferences (Vickers, 1986) provide a possible explanation of this result. Barro's model, in particular, predicts that, for long enough terms in office, there will be no inflation in the early periods of a term regardless of which type the policymaker actually is. Similarly, as long as pooling equilibria prevail at the beginning of a term, inflation will be lower when a new chief executive is in power also in Vickers' model regardless of its inflation preferences.

The greater success of new chief executives can, however, also be explained otherwise. Persson and Tabellini (1990), for example, propose a model in which unexpected policy actions disrupt the system of expectations of private agents but a new government has the

potential for stabilizing these expectations as the economy focuses on a new set of policy proposals. In this case, new chief executives would be more likely to succeed because they could benefit from more favorable expectations. Finally, new chief executives may stand to loose more political capital in case of failure and, according to Dornbusch's (1991) stylized model, may choose a greater adjustment. Our estimation results allow us to partially discriminate among these alternative explanations. The fact that $CE_{years<=3}$ remains statistically significant in both the *FAIL2* and *RATIO* regressions after we control for the adjustment variables suggests that reputational and expectational factors are important and that this variable is not only a proxy of adjustment.

Non-policy and policy adjustment variables

The estimates in Tables 7 and 8 confirm the relevance of most non-policy adjustment variables that were found statistically significant in the *FAIL1* regressions. An increase in international reserves from year T-1 (RES_{T+i-1} - RES_{T-1}) and lagged GDP growth (GDP_{T+i-1}) tend to reduce the chances of failure. In addition, the more positive is the output gap ($YGAP_{T+i-1}$) in the stabilization year and the following years the more likely is a stabilization to fail. This result appears consistent with Orphanides' "opportunistic" theory of disinflation according to which a large negative output gap could create a favorable condition for a successful disinflation by permitting higher non-inflationary growth in the post-stabilization years.[47]

Among the policy variables, higher domestic credit growth tends to increase the likelihood of failure in both the *FAIL2* and *RATIO* regressions. Fiscal adjustment is, instead, significant only in the *RATIO* regression. The lack of significance of the fiscal adjustment in the *FAIL2* regression can be interpreted as indicating that some other regressors—most likely, political economy variables and adjustment variables such as GDP growth and domestic credit growth—fully account for what would otherwise be the contribution of fiscal policy adjustment to the success of inflation stabilization.

C. Robustness to different sample selection criteria

In Table 9 we present dynamic *pooled* regression estimates obtained by running more parsimonious specifications for *FAIL1*, *FAIL2*, and *RATIO* on the smaller dataset corresponding to Easterly (1996). The statistical tests accept the null hypothesis of a zero share of variance attributable to episode-specific effects, thus indicating that pooled estimates are adequate. The smaller size of the sample prevents us, instead, from reliably estimating the models with the endogeneity correction for the anchor. The small sample size also suggests that results should be interpreted with caution. In particular, we had to drop some variables in some

[47] Our measure of output gap is based on a Hodrick-Prescott filter. Similar results were obtained extrapolating a linear trend estimated with observations up to the beginning of the stabilization.

probit specifications because they perfectly predicted success or failure. The main purpose of this estimation exercise is then only to have an approximate indication of whether a different selection criterion for the stabilization episodes would have led us to very different conclusions on the determinants of stabilization failures.

The estimates based on Easterly's sample broadly confirm our previous results. The fit of the three models is surprisingly good with pseudo-R^2 equal to 0.806 for *FAIL1* and 0.584 for *FAIL2*, and an R^2 of 0.765 for *RATIO*. Higher pre-stabilization inflation continues to increase the likelihood of success yielding further support to the "crisis hypothesis." The association between ERBS and successful stabilizations is also confirmed, while a long history of high inflation remains a predictor of failure. In this subsample, we do not find, instead, evidence that openness matters.

Among the exogenous controls, higher U.S. interest rates (USi_{T+i}), and a reduction in import demand from trading partners between period *T* and *T+1* ($XDEM_{T+1}$-$XDEM_T$) significantly increase the probability of failure with *FAIL1*, while terms of trade shocks are more important with *FAIL2* and *RATIO*.

In the estimates with the dichotomous variables, we find little evidence of in-sample state dependence as $FAIL1_{T+i-1}$ and $FAIL2_{T+i-1}$ are dropped from the set of regressors. $RATIO_{T+i-1}$ remains, instead, statistically significant.

Some political economy variables play a role also in this smaller dataset. Our measure of increasing social tensions contributes to explain stabilization failures as measured by *FAIL1*. The constraints on the executive authority, political cohesion, and the number of years the chief executive has been in power are statistically significant determinants of both *FAIL2* and *RATIO*. In the case of *FAIL2*, the duration of the political regime at the start of stabilization also plays some role.

Among the adjustment variables, higher international reserves and improved fiscal balances reduce the chances of failure with all three measures, whereas domestic credit growth is relevant only for *FAIL1*.

IV. Summary and Conclusions

This paper highlights four sets of factors that can help predict the failure of high inflation stabilizations over a three year horizon. Initial conditions—including the choice of the nominal anchor and the degree of openness of the economy, external shocks, credibility factors, and political institutions predict correctly about 85 percent of the stabilization outcomes, measured by the two dichotomous variables, and explain about 60 percent of the variance of our continuous measure. We also find that many of these factors retain their significance even after controlling in the regressions for the evolution of post-stabilization macro variables such as GDP growth, real exchange rate developments, changes in international reserves, and monetary and fiscal adjustment.

Among the *initial conditions*, our findings strongly support the "crisis hypothesis" indicating that higher pre-stabilization inflation tends to be associated with a smaller probability of failure. We also find that exchange-rate based stabilizations are less likely to fail over the following three years. As this effect remains statistically significant even after controlling for monetary and fiscal adjustment, we conclude that exchange rate anchors may not only have a disciplining effect on monetary and fiscal policies but may also play a role in coordinating expectations as suggested by some theoretical contributions. An important *caveat* of this result is, however, that a significant effect of exchange rate anchors can only be identified after controlling for other initial conditions, external shocks, and, in the case of our second dichotomous measure of failure *FAIL2*, political institutions and conditions. A simple bivariate analysis indicates, in fact, that the frequency of failure among ERBS is similar to that of stabilizations with other nominal anchors. The important policy implication is that an exchange rate anchor alone cannot guarantee success and that other initial and political conditions should be in place. "Good luck" is also important. We find, in fact, that *external shocks* such as changes in U.S. interest rates, imports demand from trading partners, and terms of trade, significantly contribute to stabilization failures. The openness of the capital account at the beginning of the stabilization tends, instead, to reduce the chances of failure but we also find some indication that it may significantly amplify the effect of international interest rate shocks. We only find weak evidence that an open current account contributes to a successful disinflation.

Credibility and reputational factors contribute to stabilization failures. We find strong evidence in support of initial *state dependence* with countries having a longer history of high inflation at the start of the stabilization being more likely to fail. This evidence is also consistent with the presence of institutions, such as indexation, in chronically high inflation countries that make stabilization more difficult. We also find some support for in-sample state dependence indicating that stabilization that succeed (fail) at the beginning of our three-year window are *ceteris paribus* more likely to continue to succeed (fail) in the following year. In our benchmark estimates, however, past *in-sample* outcomes are no longer statistically significant predictors of success when we control for the post-stabilization path of non-policy and policy variables, suggesting that a successful adjustment can in some cases overcome the effects of state dependence. We also find evidence that chief executives that have been in power for less than three years are more likely to succeed, consistently with reputation models that predict new governments would choose lower levels of inflation independently from their ability to pre-commit or preferences.

Political institutions and conditions are important determinants of stabilization failures. We find that the more constrained the authority of the chief executive is the higher is the likelihood of failure. Importantly, we show that this result is not driven by some successful stabilizations implemented by non-democracies in our sample but also holds for the subsample of democracies, yielding indirect support to recent political economy theories that point to the greater ability of presidential regimes to limit government expenditure. We show, however, that, to identify this effect, it is important to use the finer classification of executive authority constraints from the *POLITY IV* dataset, while a simple zero-one dummy for presidential regimes was not statistically significant. We also find that majoritarian electoral

rules within the subset of democratic countries tend to reduce the likelihood of failure. As our sample is characterized by several political regime transitions from and to democracy, as well as changes in the degree of democracy, we also check whether "younger" political regimes are more likely to fail finding support for this hypothesis, which is related to the prediction that political instability and inflation are associated. Finally, we find some confirmation of "war of attrition" models of inflation stabilization as increases in an indicator of social tensions significantly augment the likelihood of failure in our benchmark regressions. Contrary to the predictions of this class of models, we do not find any evidence that more polarized governments are more likely to fail. Our data actually provide support to the opposite prediction, indicating that more polarized governments are more likely to succeed. This surprising result reflects the success in stabilizing inflation of some "national unity" governments, such as Israel in 1986, characterized by high polarization scores, and suggests that the end of war of attritions may sometimes be associated with a cooperative agreement among the major parties rather than with one of the groups involved in the war of attrition imposing the redistributive costs of stabilization on the other.

Finally, the econometric analysis necessary to eliminate the potential bias associated with the endogeneity of the anchor (Section II.F) is of separate interest as it sheds some light on the *determinants of the choice of the anchor*. Countries appear to choose ERBS when they have a higher initial GDP per capita, larger initial stock of international reserves, more positive fiscal balances, and a higher initial GDP growth. A more depreciated real exchange rate prior to the stabilization also tends to be associated with exchange-rate based stabilizations. An open current account increases the chances that a country might choose an exchange-rate-based stabilization, while an open capital account tends to reduce them. We also find some evidence that exchange rate anchors have been used as a precommitment device. Countries with a long pre-stabilization history of high inflation and a more constrained executive authority tend, in fact, to choose the exchange rate as a nominal anchor.

While providing a number of interesting insights, this paper has limits that future research could try to overcome. These are associated mainly with the sample selection issue, the small number of observations, and the estimation technique.[48] As discussed in Section II.B, our results on the determinants of inflation stabilization failures are valid within the set of

[48] Future research may also try to overcome some data limitations. In spite of the large set of potential determinants of stabilization failure that we consider, there are a few important ones that we neglect. Measures of income inequality could not be used to test "war of attrition" models because they were missing for some countries in the sample. Similarly, we could not find a complete set of indicators of central bank independence. Finally, the role of financial development, whose association with inflation has been recently studied by Albanesi (2000), cannot be properly assessed because the values taken by standard measures of financial development—such as ratios of M2 or bank credit to GDP—tend to be closely related with inflation developments before and after inflation stabilizations with a causal relationship trivially running from the latter to the former.

stabilizations that are sufficiently successful at the outset to be selected by our numerical rule. Our conclusions cannot apply, instead, to the larger set of all attempts at stabilizing inflation nor to inflation stabilization episodes selected with alternative rules. As a robustness check, we re-estimate our model on the subsample of inflation stabilizations selected by Easterly (1986) and we find confirmation of our main results (Section III.C). Future research should consider verifying our findings on datasets with inflation stabilization episodes selected with different criteria such as, for example, that of Fischer, Sahay, and Végh (2001). We experimented with the part of this dataset (29 episodes) overlapping with ours and obtained broadly similar results, but a proper test—which is beyond the scope of this paper—would require using their full dataset.

Even though our dataset is larger than those used in previous studies, it remains a small dataset. One issue is the direct effect of the sample size on the statistical significance of the estimated coefficients. Extending the post-stabilization horizon by one or two years may look like a sensible solution to this problem but this solution has some shortcomings. As already mentioned, there is a need not to have a post-stabilization horizon so long that inflation performance in the final years is largely unrelated to the stabilization. Moreover, for countries with more than one stabilization episode, lengthening the horizon would make observations corresponding to different stabilization episodes overlap as they would enter the dataset twice, first as the final years of an earlier episode and then as the initial years of a new one. In sum, a three year horizon may look like a sensible middle ground and is broadly consistent with the view that the final outcome of most stabilizations can be observed within a four- to five-year period (Reinhart and Végh, 1995.)

A more drastic alternative solution to the sample size problem would have been to use a large panel with long time-series of data for each country and apply dynamic panel data methods to identify short-term dynamics and long-term relationships between inflation and other variables. While this approach would have significantly reduced the degrees of freedom problem and simplified the specification search, we believe that the structural breaks associated with inflation stabilization attempts would have strongly affected the results, as statistical relationships between variables are likely to be significantly different around the time of inflation stabilization when announcements and policy signals play a key role.

A second issue related to the sample size is that many potential determinants of the stabilization outcome, especially variables measuring political institutions, are relatively time-invariant. In a dataset that forces us to separate democracies from non-democracies, this leaves us with very few observations determining the outcome (for example, only six democracies in our sample have majoritarian electoral systems). This problem has no easy solution and is common to many political economy studies, which have usually been based on cross-country datasets with few observations. However, the panel structure of our dataset and the relatively large time variation of some political institutions—resulting from the frequent political regime changes—provides us with a sample in which political economy variables may actually show more variation than in other studies.

This paper uses a variety of estimation techniques described in Section II to take into account the dichotomous or continuous nature of our stabilization failure measures, possible unobservable heterogeneity across episodes, and the endogenous choice of the nominal anchor. Recently, Persson and Tabellini (2001b) have used nonparametric matching estimates techniques to overcome some of the problems associated with empirical work on small political economy datasets, such as the biases due to systematic selection and possible non-linearities. They conducted this exercise, however, under the assumption of conditional independence, which is exactly the one we try to test by using bivariate probit and Heckman's procedure when we control for the possible endogeneity of the anchor. As we found, however, little evidence of endogeneity, it might be interesting in future research to apply matching techniques also to our dataset. To estimate a dynamic probit on our dataset, future research may also consider using simulated-based inference methods—such as the maximum smoothly simulated likelihood method recently applied by Falcetti and Tudela (2001) to the study of currency and banking crises or the Markov chain Monte Carlo method applied to the issue of state failures by Beck et al. (2001). The small sample size and the large number of potential regressors might, however, make it difficult to use these advanced techniques on our dataset.

Appendix I: Episode Selection

Since Hamann (2001) selected stabilization episodes exclusively on the basis of inflation performance, he sought to rule out the possibility that they represented positive supply shocks, rather than bona-fide stabilization episodes. He used two checks for this purpose. The first was to look for the existence of IMF programs during a period of up to two years prior to the stabilization date identified in Table 1. Of the 51 episodes 38 were preceded by, or coincided with, an IMF Fund program. For the remaining 13 episodes, a second check consisted on looking for independent evidence in the literature that a stabilization plan had indeed been put in place at around the time identified in Table 1. For eight of these cases, there is plenty of supporting evidence: Argentina 1980 actually represents its *tablita* experiment, which ran from December 1978 to February 1981; Brazil 1990 is president Collor's stabilization plan of 1990-91; Indonesia's "stabilization and rehabilitation" program started in 1966 is documented by Azis (1994); the Israeli stabilization plan has been analyzed extensively—Bruno and Piterman (1988) is the first of several studies dealing with the Israeli stabilization; Iceland's two stabilization plans are reviewed by Andersen and Guðmundsson (1998); and Ghana's strategy to mop up excess liquidity in 1978 is documented by Sowa (1993). Other episodes are not well documented in the literature but clearly constitute bona-fide stabilization programs: Lebanon's 1993 stabilization is part of the economic reconstruction plan started in 1991 following the Taif peace treaty, and Ecuador 1994 reflects the efforts of that country's authorities to bring down inflation through the active use of the exchange rate as a tool to anchor expectations about inflation. Thus, for only four of the 51 episodes there is no independent evidence of the adoption of a stabilization plan: Lebanon 1988, Sierra Leone 1992, Syria 1988 and Guinea Bissau 1993.

An alternative to a rules-based method for selecting episodes would have been a comprehensive review of the history of those countries, aimed at identifying periods in which governments put in place anti-inflation economic programs. This alternative route would necessarily entail some controversial judgments regarding, for instance, whether to exclude programs that could not be considered serious attempts at disinflation, or programs that were abandoned soon after they were implemented. Furthermore, in most cases, identifying the precise timing of stabilization episodes could be equally arbitrary. It is not always the case that inflation stabilization programs have a clearly identifiable start date, with the exceptions being perhaps those in which the exchange rate played a central role and its future path was announced along with other measures.[49] Moreover, it is often the case that inflation is successfully brought down after more than one attempt and, even in those cases, it is not entirely clear when a given stabilization plan ended and the next one began.

Another alternative would have been to rely on existing work documenting stabilization programs in high inflation countries. But those studies are not likely to provide a

[49]Even in those cases, other stabilization policies may have been put in place before the announcement of a path for the exchange rate, thus obscuring the timing of stabilization.

comprehensive account of stabilization episodes. Furthermore, doubts about the precise timing of the stabilization programs and consistency across countries of the methodology for selecting relevant episodes are not likely to be resolved by relying on a survey of existing studies of stabilization experiences.

Thus, in order to test the robustness of our results to the chosen selection rule, in section ? we show the results arising from re-estimating our main specifications using Easterly's sample of countries. A comparison of the results generated by each of these samples will hopefully shed some light about whether the identified stylized facts are robust to changes in the precise statistical definition of a stabilization episode.

APPENDIX 2: SOURCES OF DATA

The IMF's International Financial Statistics was the main source of data for all variables, except for the current account and the real exchange rate. Current account data from the IMF's WEO database was used, except for one case (Brazil 1963-69) where, due to the lack of data on the current account, trade balance data from IFS had to be used. In several instances, data from national sources were needed to fill some gaps (especially in the oldest episodes), or when IFS data contained breaks (typically in the case of the population variable, which was needed to compute GDP per capita). The fiscal data refers to the central government. For the real exchange rate, a multilateral real effective exchange rate variable was available from an IMF's internal database from 1980 onwards. For countries for which this variable was not available, or in cases where data prior to 1980 was needed, a bilateral real exchange rate vis-à-vis the US dollar was constructed. Political economy variables are from the World Bank Dataset on Political Institutions (DPI), the POLITY IV dataset, and Banks' Cross-National Time-Series Data Archive. Information in various issues of Banks' "Political Handbook of the World" was used to eliminate missing values in these datasets and in the series of political and electoral regimes provided by Guido Tabellini. In most cases, we added Sao Tome and Principe to the existing datasets by following as closely as possible the methodology used for other countries. In the POLITY IV dataset, we also interpolated the "transition" codes (–88), as suggested in the users manual; furthermore, we recoded ourselves, using the methodology spelled out in the users manual, some observations corresponding to Lebanon that the POLITY IV dataset had coded as "interruption" (-66) or "interregnum" (-77), in order to avoid missing values.

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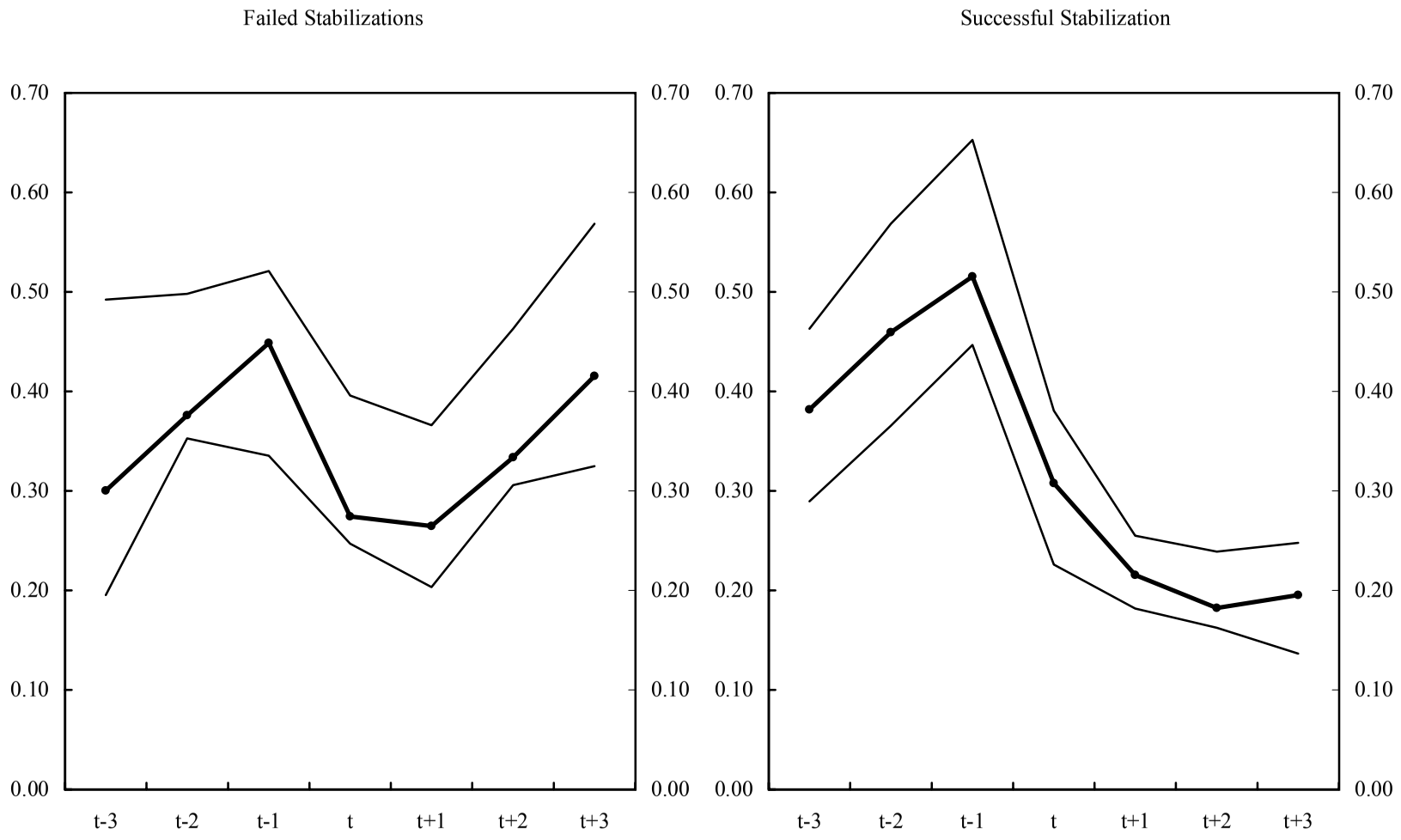
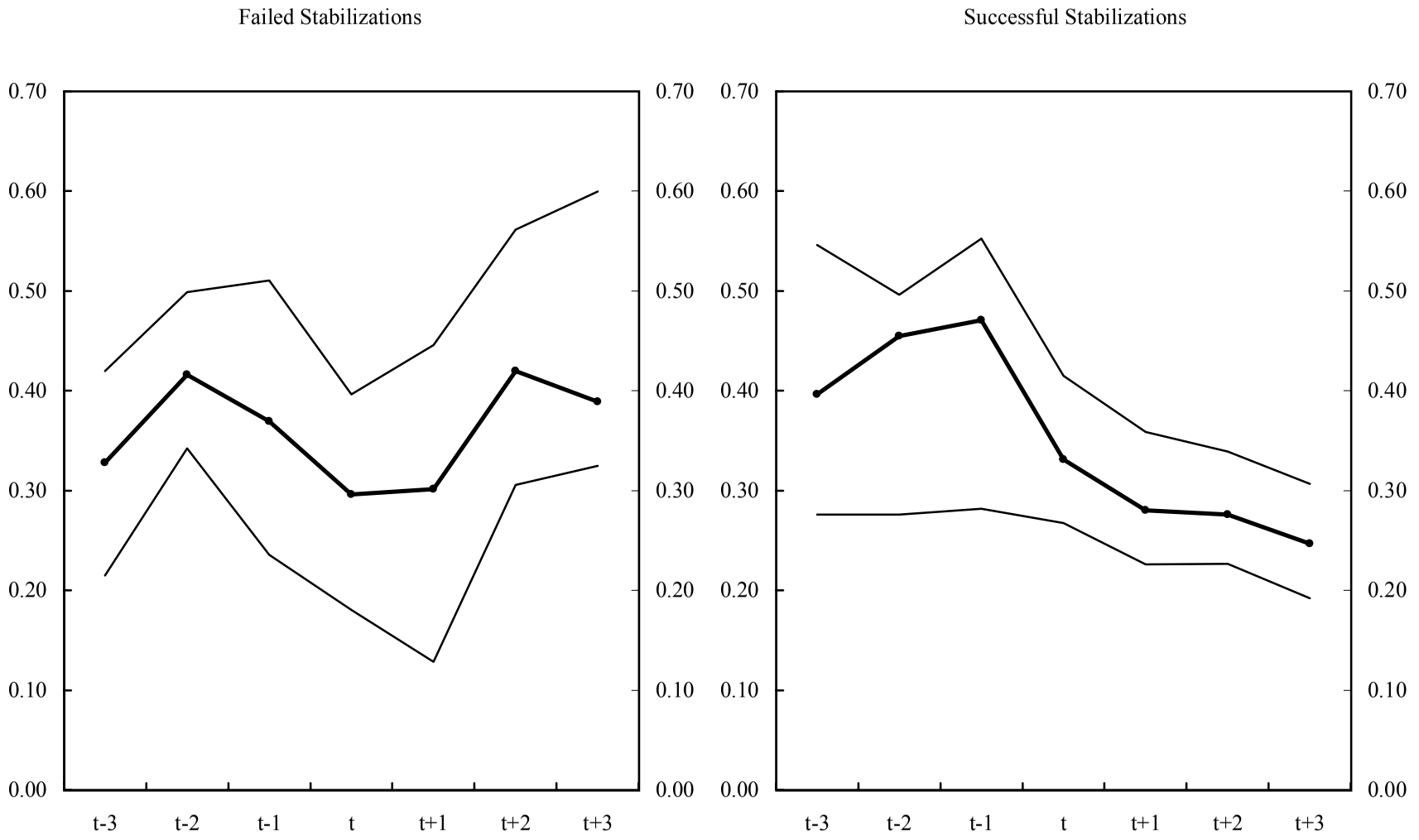
Figure 1. Inflation 1/



1/ Inflation re-scaled as $\pi = \pi/(1+\pi)$.

Source: IMF, *International Financial Statistics* ; national sources and author's own estimates.

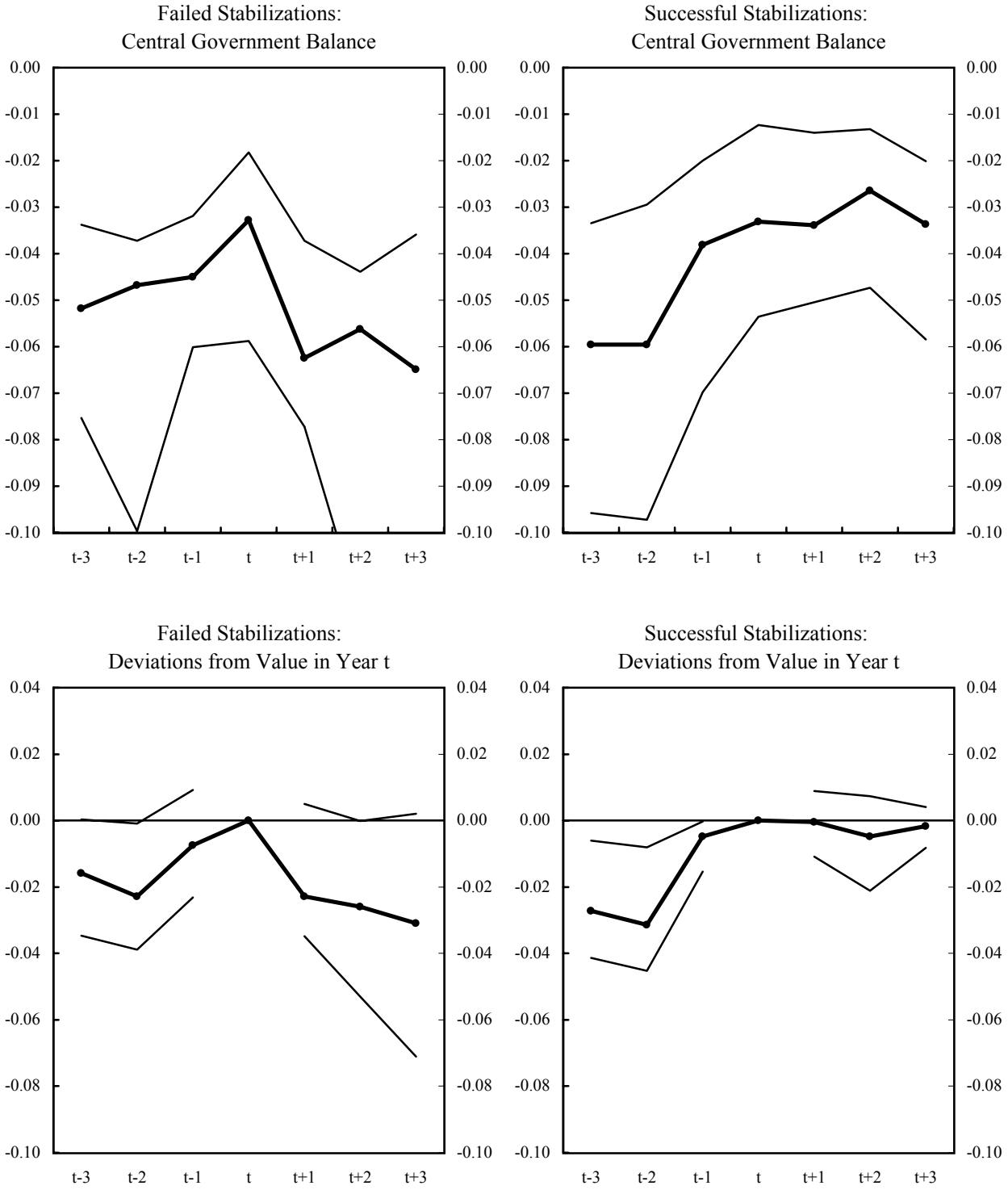
Figure 2. Credit Growth



1/ Credit growth (x) re-scaled as z = x/(1+x).

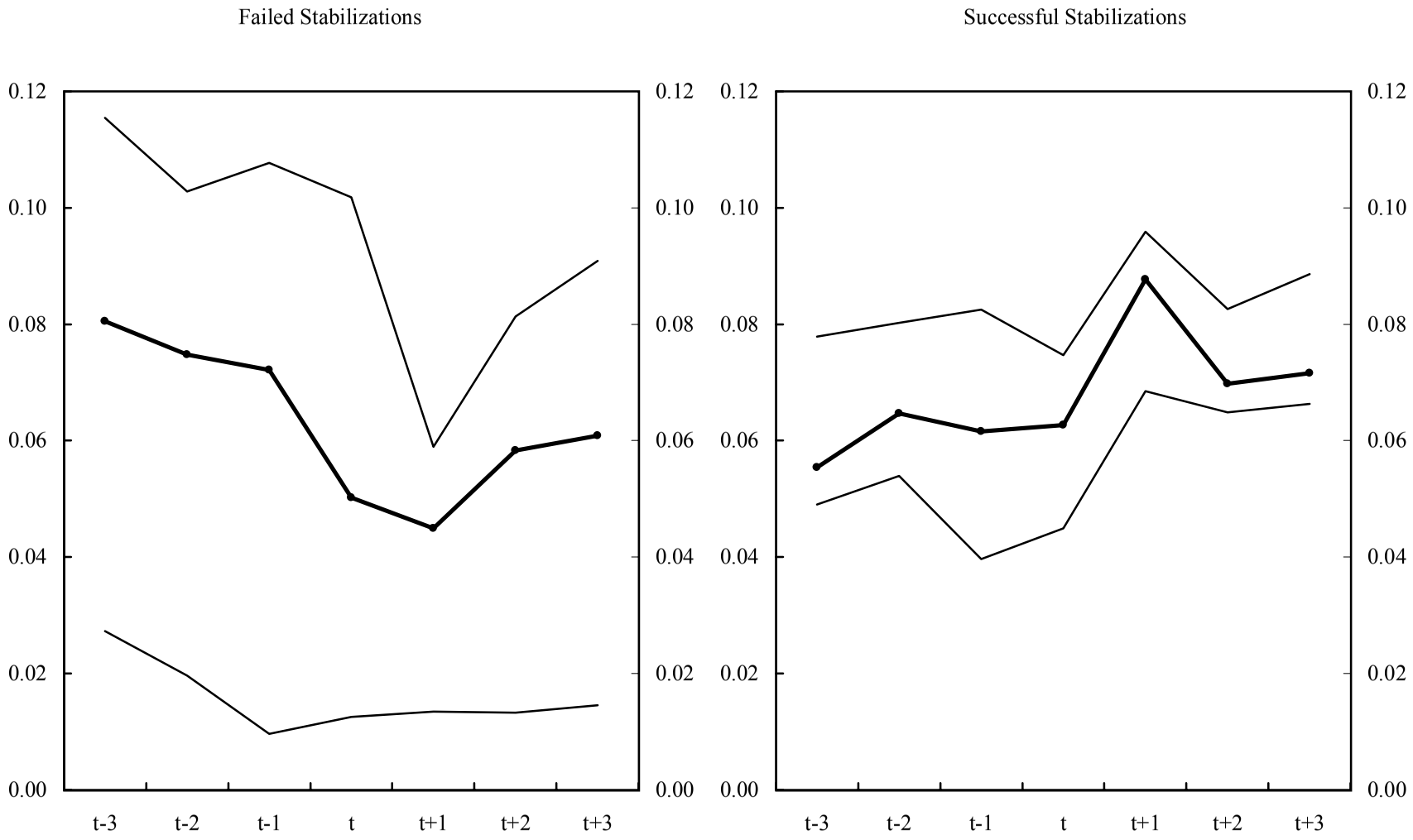
Source: IMF, *International Financial Statistics* ; national sources and author's own estimates.

Figure 3. Fiscal Adjustment
(In percent of GDP)



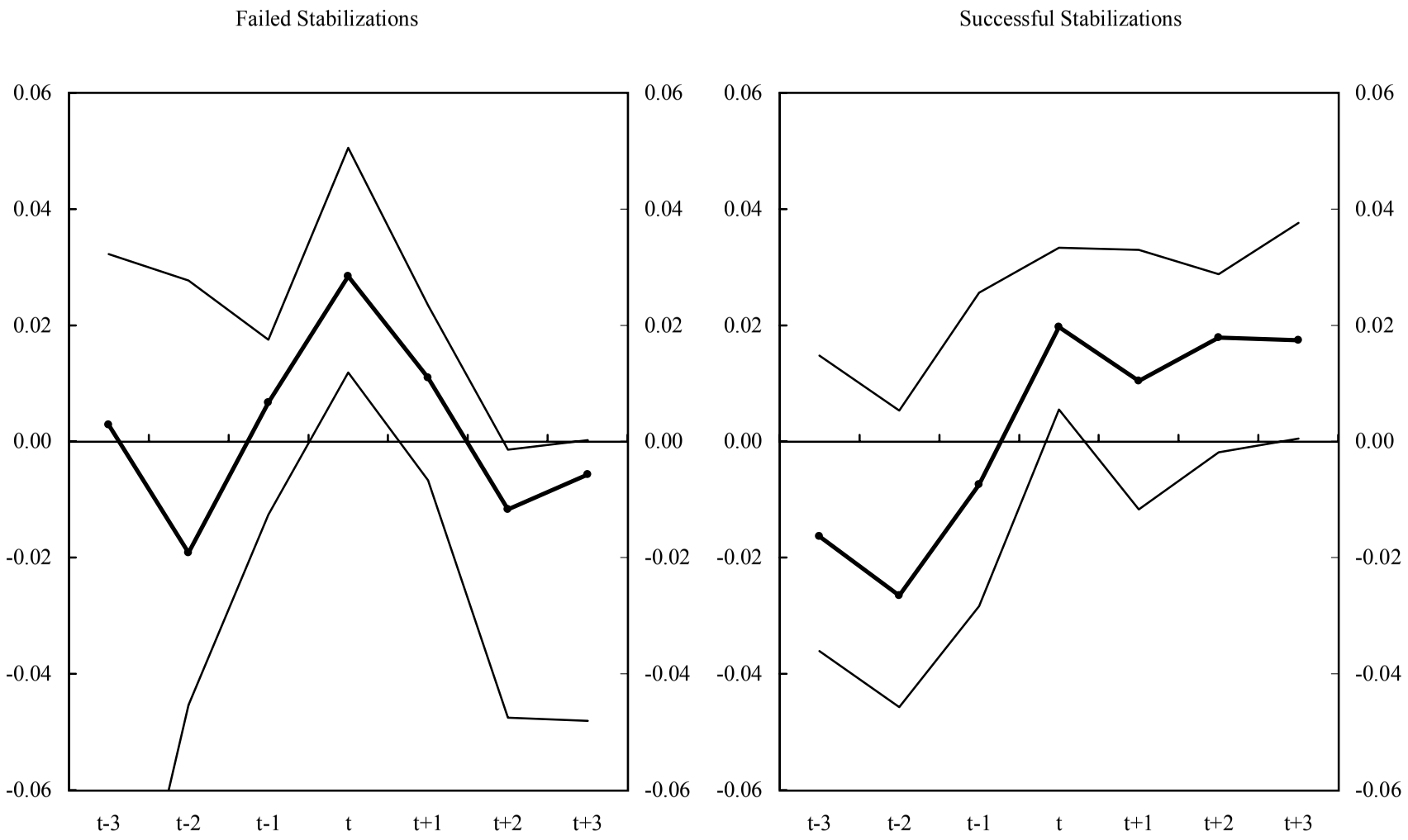
Source: IMF, *International Financial Statistics* ; national sources and author's own estimates.

Figure 4. Growth in Demand from Trade Partners



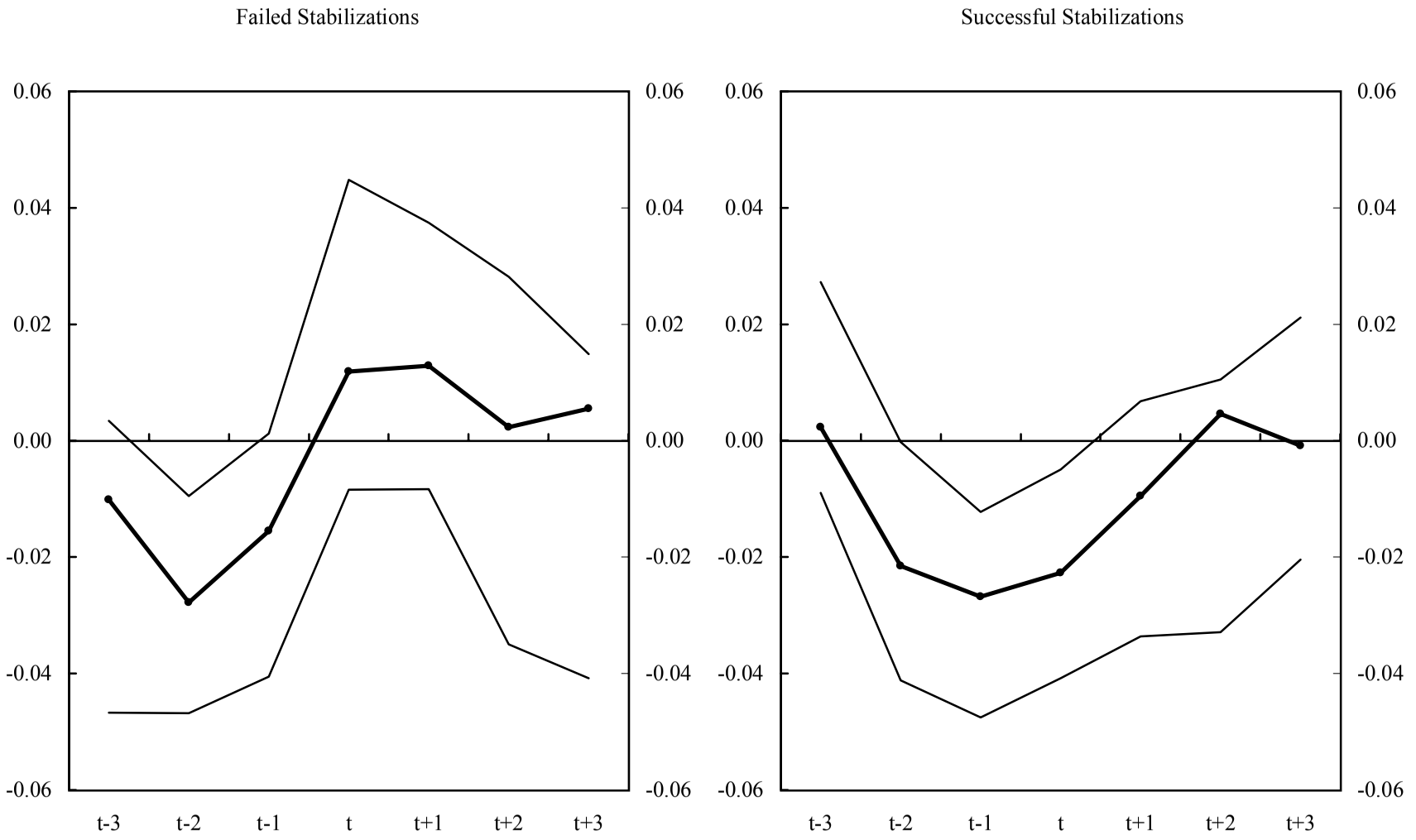
Source: IMF, *World Economic Outlook* ; national sources and author's own estimates.

Figure 5. Growth in Per-Capita GDP



Source: IMF, *International Financial Statistics* ; national sources and author's own estimates.

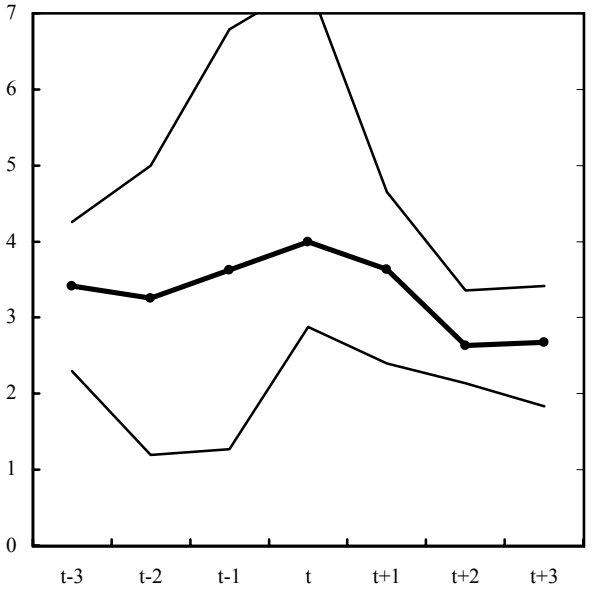
Figure 6. Output Gaps
(In percent of Potential GDP)



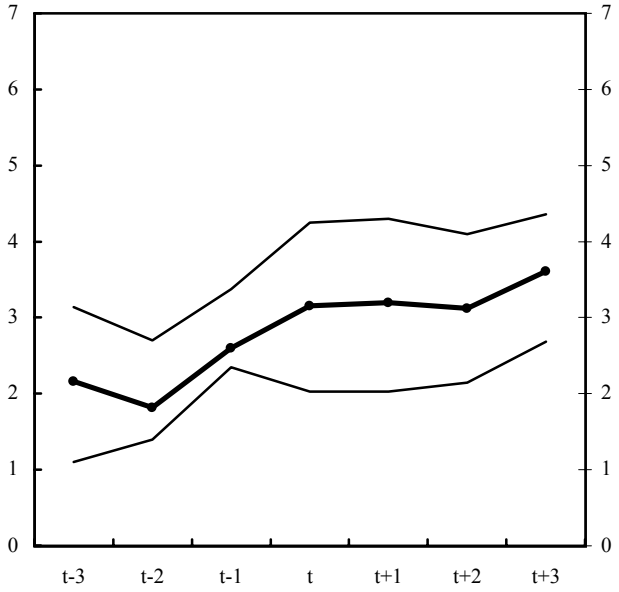
Source: IMF, *International Financial Statistics* ; national sources and author's own estimates.

Figure 7. Gross International Reserves
(In months of Imports)

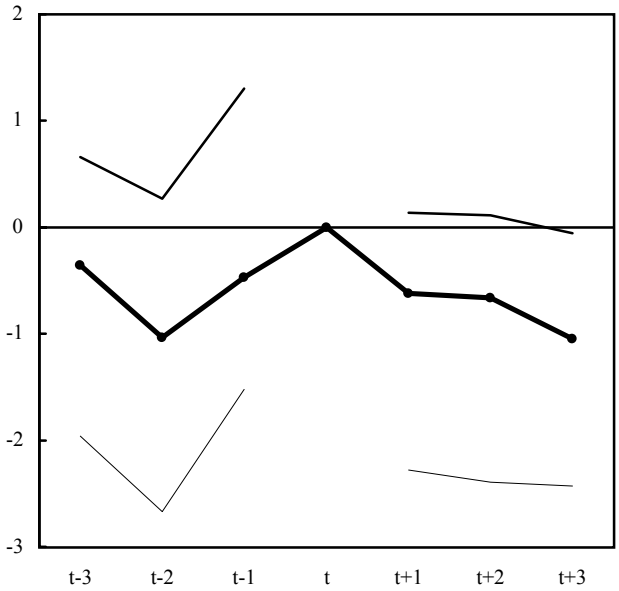
Failed Stabilizations:
International Reserves



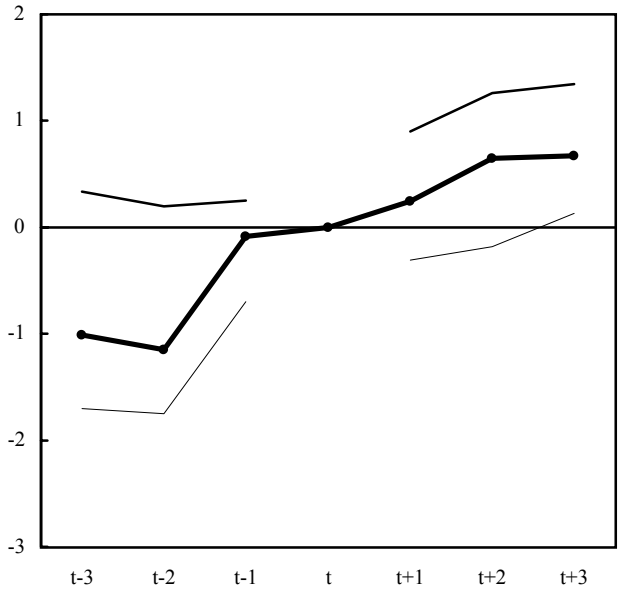
Successful Stabilizations:
International Reserves



Failed Stabilizations:
Deviations from Value in Year t



Successful Stabilizations:
Deviations from Value in Year t



Source: IMF, *International Financial Statistics* ; national sources and author's own estimates.

Table 1. Stabilization Episodes

	STABILIZATION DATE		Date of Fund arrangement 1/
	Easterly (1996)	Hamann (1999)	
Argentina 1		1977	August 1976
Argentina 2		1980	
Argentina 3		1986	March 1986
Argentina 4	1990	1991	May 1990
Bangladesh	1975	1975	June 1974
Bolivia	1986	1986	June 1986
Brazil 1	1965	1966	January 1965
Brazil 2		1991	
Chile 1		1965	January 1965
Chile 2	1974	1975	January 1974
Congo, Democratic Republic of 1		*1969*	July 1967
Congo, Democratic Republic of 2		1980	August 1979
Congo, Democratic Republic of 3	1984	1985	December 1983
Costa Rica	1983	*1983*	December 1982
Dominican Republic	1991	1992	August 1991
Ecuador 1		1990	September 1989
Ecuador 2	1993	1994	
Ghana 1		1978	
Ghana 2	1984	*1985*	August 1983
Guinea		*1988*	July 1987
Guinea Bissau 1		1990	March 1989
Guinea Bissau 2	1993	1993	
Guyana		1992	July 1990
Iceland 1	1975	1976	
Iceland 2	1984	1984	
Indonesia	1967	1967	
Israel	1985	1986	
Jamaica	1992	1993	June 1991
Lebanon 1		1988	
Lebanon 2		1993	
Mexico 1		1984	January 1983
Mexico 2	1988	1989	November 1986
Mozambique		1988	June 1987
Nicaragua	1991	1991	September 1991
Nigeria	1989	1990	February 1989
Peru 1		1986	April 1984
Peru 2	1991	1991	September 1991
Sao Thome & Principe		1992	June 1989
Sierra Leone 1		1988	November 1986
Sierra Leone 2	1992	1992	
Somalia 1	1981	1982	February 1980
Somalia 2	1985	*1985*	February 1985
Syrian Arab Republic		*1988*	
Turkey	1981	1981	June 1980
Uganda 1	1981	1982	January 1980
Uganda 2	1988	1989	June 1987
Uruguay 1	1968	1969	June 1968
Uruguay 2		1976	May 1975
Uruguay 3	1980	1981	March 1979
Uruguay 4		1992	December 1990
Zambia		1994	July 1992
Total number of episodes	28	51	38
Number of Easterly (1996) episodes	28	28	
Preceded by Fund arrangements	21	38	

Sources: Easterly (1996), IMF's *International Financial Statistics* , and national sources.
1/ Date of nearest Fund arrangement prior to stabilization.

Table 2. Successful Stabilization Episodes

	Stabilization Date	Exchange rate anchor	Successful	
			Criterion 1	Criterion 2
Argentina 1	1977		Yes	Yes
Argentina 2	1980	Yes		
Argentina 3	1986	Yes		
Argentina 4	1991	Yes	Yes	Yes
Bangladesh	1975		Yes	Yes
Bolivia	1986		Yes	Yes
Brazil 1	1966	Yes	Yes	Yes
Brazil 2	1991		Yes	
Chile 1	1965		Yes	Yes
Chile 2	1975		Yes	Yes
Congo, Democratic Republic of 1	1969		Yes	
Congo, Democratic Republic of 2	1980			
Congo, Democratic Republic of 3	1985			
Costa Rica	1983		Yes	Yes
Dominican Republic	1992		Yes	
Ecuador 1	1990		Yes	
Ecuador 2	1994	Yes	Yes	
Ghana 1	1978			
Ghana 2	1985			
Guinea	1988		Yes	
Guinea Bissau 2	1990			
Guinea Bissau 3	1993		Yes	
Guyana	1992		Yes	Yes
Iceland 1	1976			
Iceland 2	1984	Yes	Yes	
Indonesia	1967		Yes	
Israel	1986	Yes	Yes	Yes
Jamaica	1993		Yes	
Lebanon 1	1988		Yes	Yes
Lebanon 2	1993		Yes	Yes
Mexico 1	1984			
Mexico 2	1989	Yes	Yes	
Mozambique	1988		Yes	Yes
Nicaragua	1991	Yes	Yes	Yes
Nigeria	1990			
Peru 1	1986	Yes		
Peru 2	1991		Yes	Yes
Sao Thome & Principe	1992			
Sierra Leone 1	1988		Yes	
Sierra Leone 2	1992		Yes	Yes
Somalia 1	1982			
Somalia 2	1985			
Syrian Arab Republic	1988		Yes	Yes
Turkey	1981		Yes	
Uganda 1	1982			
Uganda 2	1989		Yes	Yes
Uruguay 1	1969	Yes	Yes	
Uruguay 2	1976			
Uruguay 3	1981	Yes		
Uruguay 4	1992	Yes	Yes	Yes
Zambia	1994		Yes	Yes
Total number of episodes	51		34	20
Successful programs in percent of total programs			(67%)	(39%)
Exchange rate-based stabilizations		13	9	5
Successful ERBS programs in percent of ERBS programs			(69%)	(38%)

Sources: Easterly (1996), IFS, and national sources.

Table 3. Stabilization Episodes: Anchors

	Easterly (1996)		Hamann (1999)		ERBS historical dates 1/
	Date	Exchange rate anchor	Date	Exchange rate anchor	
Argentina 1			1977		
Argentina 2			1980	Yes	1979
Argentina 3			1986	Yes	1985
Argentina 4	1990	Yes	1991	Yes	1991
Bangladesh	1975		1975		
Bolivia	1986		1986		
Brazil 1	1965	Yes	1966	Yes	1964
Brazil 2			1991		
Chile 1			1965		
Chile 2	1974		1975		
Congo, Democratic Republic of 1			*1969*		
Congo, Democratic Republic of 2			1980		
Congo, Democratic Republic of 3	1984		1985		
Costa Rica	1983		*1983*		
Dominican Republic	1991		1992		
Ecuador 1			1990		
Ecuador 2	1993	Yes	1994	Yes	1993
Ghana 1			1978		
Ghana 2	1984		*1985*		
Guinea			*1988*		
Guinea Bissau 1			1990		
Guinea Bissau 2	1993		1993		
Guyana			1992		
Iceland 1	1975		1976		
Iceland 2	1984	Yes	1984	Yes	1983
Indonesia	1967		1967		
Israel	1985	Yes	1986	Yes	1986
Jamaica	1992		1993		
Lebanon 1			1988		
Lebanon 2			1993		
Mexico 1			1984		
Mexico 2	1988	Yes	1989	Yes	1988
Mozambique			1988		
Nicaragua	1991	Yes	1991	Yes	1991
Nigeria	1989		1990		
Peru 1			1986	Yes	1986
Peru 2	1991		1991		
Sao Thome & Principe			1992		
Sierra Leone 1			1988		
Sierra Leone 2	1992		1992		
Somalia 1	1981		1982		
Somalia 2	1985		*1985*		
Syrian Arab Republic			*1988*		
Turkey	1981		1981		
Uganda 1	1981		1982		
Uganda 2	1988		1989		
Uruguay 1	1968	Yes	1969	Yes	1968
Uruguay 2			1976		
Uruguay 3	1980	Yes	1981	Yes	1979
Uruguay 4			1992	Yes	1991
Zambia			1994		
Total number of episodes	28		51		
Exchange rate-based stabilizations		9		13	

Sources: Easterly (1996), Hamann (1999).

1/ Year in which exchange rates were actually pegged, if peg ocurred within the first six months of the year; the following year if peg ocurred during the last six months of the year.

Table 4. Dynamic *FAIL1* Estimates

(heteroskedastic corrected standard errors in parenthesis)[1]

	(1)	(2)	(3)	(4)
Initial conditions:				
π_{T-1}	-6.717 ***	-10.776 ***	-26.828 ***	-43.979 ***
	(1.204)	(2.212)	(6.059)	(12.390)
RES_{T-1}	0.141 ***	0.169 ***		
	(0.038)	(0.035)		
$YGAP_{T-1}$	7.599 ***			
	(2.728)			
ANCHOR	-1.115 **	-2.086 ***	-6.213 ***	-10.651 ***
	(0.455)	(0.482)	(1.926)	(3.922)
$OPENCA_{T-1}$	-1.603 ***			
	(0.590)			
$OPENKA_{T-1}$	-0.953 **	-3.015 **	-13.018 ***	-24.866 ***
	(0.477)	(1.120)	(2.893)	(7.621)
External controls:				
USi_{T+t}	30.001 ***	35.959 **	79.681 **	143.648 ***
	(7.437)	(9.886)	(20.153)	(43.554)
USi_{T+t} *$OPENKA_{T+t-1}$			45.574 ***	105.309 ***
			(16.882)	(43.965)
$XDEM_{T+1}$ - $XDEM_{T}$	-11.829 ***	-17.024 ***	-39.541 ***	-62.978 ***
	(2.520)	(3.682)	(9.635)	(19.816)
Credibility/Reputation:				
$YEARS_{\pi>40}$	0.291 ***	0.496 ***	1.414 ***	2.312 ***
	(0.064)	(0.100)	(0.305)	(0.667)
$FAIL1_{T+t-1}$ $(t=2,3)$	0.291 ***	1.045 **		
	(0.064)	(0.485)		
Political economy variables:				
$DURREG_{T}$		-0.017 *	-0.039 **	-0.047 **
		(0.009)	(0.016)	(0.023)
$XCONST_{T+i-1}$		0.582 ***	1.269 ***	2.359 ***
		(0.167)	(0.388)	(0.878)
$XROPEN_{T+i-1}$		-0.235 ***	-1.027 ***	-2.295 **
		(0.082)	(0.324)	(0.932)
$PARCOMP_{T+i-1}$		-0.419		
		(0.273)		
$SOCCONF_{T+i-1}$		0.452 **	1.167 ***	2.054 ***
		(0.200)	(0.267)	(0.693)
$POLINCOH_{T+i-1}$		-0.648 ***	-1.749 ***	-2.560 ***
		(0.183)	(0.447)	(0.780)
Non-policy macro variables:				
GDP_{T+t-1}			-15.114 **	-32.660 **
			(5.957)	(14.247)
$RES_{T+t-1} - RES_{T-1}$			-0.645 ***	-1.033 **
			(0.219)	(0.407)
$RER_{NOFAIL\ T+t-1}$			0.019 ***	0.024 ***
			(0.006)	(0.008)
Policy macro variables:				
$GB_{T+t-1} - GB_{T}$				-29.588 **
				(12.264)
DC_{T+t-1}				0.402 ***
				(0.155)
Constant:	-1.838 ***	-1.800 **	-3.170 ***	-6.119 ***
	(0.582)	(0.743)	(1.187)	(2.313)
χ^2 of LR test of: $\rho=0$	0.000	0.000	0.000	0.000
χ^2 of Wald test of: $\delta=0$	0.000	0.010	0.000	0.200
Pseudo R^2	0.492	0.564	0.778	0.837
Efron's R^2	0.470	0.530	0.781	0.848
% hits (p*=0.5)	85.0%	87.6%	86.9%	88.0%
% hits (p*=0.196)	85.0%	86.9%	93.5%	93.3%
% hits (p*=0.1)	79.7%	86.9%	94.1%	93.3%
% $_{FAIL1=1}$ hits (p*=0.1)	96.7%	96.7%	73.3%	70.0%
% $_{FAIL1=0}$ hits (p*=0.1)	75.6%	84.6%	99.2%	100.0%
Number of observations	153	153	153	150

[1] Data are annual. Three (***), two (**), and one (*) stars mark statistical significance respectively at one, five, and ten percent levels.

Table 5. Static *FAIL1* Estimates

(heteroskedastic corrected standard errors in parenthesis)[1]

	(1)	*(2)*	*(2 BIV.)*
Initial conditions:			
π_{T-1}	-8.015 ***	-12.129 ***	-11.793 ***
	(1.222)	(2.000)	(2.101)
RES_{T-1}	0.170 ***	0.191 ***	0.200 **
	(0.041)	(0.037)	(0.039)
$YGAP_{T-1}$	9.281 ***		
	(2.763)		
ANCHOR	-1.299 **	-2.267 ***	-2.357 ***
	(0.496)	(0.508)	(0.510)
$OPENCA_{T-1}$	-1.746 ***		
	(0.668)		
$OPENKA_{T-1}$	-1.127 **	-3.364 ***	-2.800 **
	(0.529)	(1.079)	(1.040)
External controls:			
USi_{T+1}	30.900 ***	36.618 ***	32.843 ***
	(7.870)	(9.611)	(9.453)
$XDEM_{T+1} - XDEM_{T}$	-13.329 ***	-18.785 ***	-16.795 **
	(2.520)	(3.457)	(3.341)
Credibility/Reputation:			
$YEARS_{\pi>40}$	0.336 ***	0.559 ***	0.512 ***
	(0.065)	(0.091)	(0.091)
Political economy variables:			
$DURREG_{T}$		-0.016 *	-0.015 *
		(0.009)	(0.008)
$XCONST_{T+i-1}$		0.609 ***	0.650 ***
		(0.170)	(0.204)
$XROPEN_{T+i-1}$		-0.272 ***	-0.281 ***
		(0.086)	(0.090)
$PARCOMP_{T+i-1}$		-0.419 *	-0.335
		(0.259)	(0.276)
$SOCCONF_{T+i-1}$		0.519 **	0.535 **
		(0.216)	(0.222)
$POLINCOH_{T+i-1}$		-0.720 ***	-0.803 ***
		(0.185)	(0.226)
Constant:	-1.409 **	-1.455 *	-1.366 *
	(0.623)	(0.803)	(0.820)
χ^2 of LR test of: $\rho=0$	0.000	0.000	
χ^2 of Wald test of: $\delta=0$	0.063	43.512 ***	
Pseudo R^2	0.445	0.537	
Efron's R^2	0.427	0.504	
% hits (p*=0.5)	86.3%	85.6%	
% hits (p*=0.196)	84.3%	88.2%	
% hits (p*=0.1)	75.2%	72.6%	
% $FAIL1=1$ hits (p*=0.1)	96.7%	100.0%	
% $FAIL1=0$ hits (p*=0.1)	69.9%	65.9%	
Number of observations	153	153	153

[1] Data are annual. Three (***), two (**), and one (*) stars mark statistical significance respectively at one, five, and ten percent levels.

Table 6. *ANCHOR* Estimates

(heteroskedastic corrected standard errors in parenthesis)[1]

AFRME	-3.400 **
	(1.411)
$GDPPC_T$	0.0007 ***
	(0.0002)
RES_{T-1}	0.231 **
	(0.117)
GB_{T-1}	32.664 ***
	(11.189)
GDP_{T-1}	41.709 ***
	(14.279)
$RERGAP_{T-2}$	-13.319 ***
	(3.690)
$OPENCA_{T-1}$	6.798 ***
	(1.887)
$OPENKA_{T-1}$	-5.451 ***
	(1.903)
$YEARS_{\pi>40}$	0.326 **
	(0.139)
$XCONST_0$	0.606 **
	(0.248)
Constant	-10.041 ***
	(2.218)
Pseudo R^2	0.804
Efron's R^2	0.801
% hits (p*=0.5)	94.0%
% hits (p*=0.26)	92.0%
% hits (p*=0.1)	92.0%
% $_{ANCHOR=1}$ hits (p*=0.1)	92.3%
% $_{ANCHOR=0}$ hits (p*=0.1)	91.9%
Number of observations	50

[1] Data are annual.
Three (***), two (**), and one (*) stars mark statistical significance respectively at one, five, and ten percent levels.

Table 7. Dynamic *FAIL2* Estimates

(heteroskedastic corrected standard errors in parenthesis)[1]

	(1)	(2)	(3)
Initial conditions:			
π_{T-1}	-3.793 ***	-6.950 ***	-9.447 ***
	(0.877)	(1.465)	(1.443)
RES_{T-1}	0.081 **	0.123 ***	
	(0.037)	(0.041)	
GB_{T-1}	1.479	4.933 **	
	(1.282)	(1.996)	
GDP_{T-1}	-4.328		
	(2.948)		
$YGAP_{T-1}$	4.301 **	7.770 **	
	(1.853)	(2.575)	
ANCHOR	-0.633	-2.194 ***	-2.510 ***
	(0.419)	(0.489)	(0.503)
$OPENKA_{T-1}$	-1.017 **	-2.116 ***	-1.636 **
	(0.473)	(0.771)	(0.694)
External controls:			
USi_{T+t}	9.721 *	26.227 ***	24.887 ***
	(5.566)	(6.199)	(7.199)
$XDEM_{T+1} - XDEM_{T}$	-6.432 ***	-5.903 **	-4.180 *
	(2.301)	(2.614)	(2.515)
Credibility/Reputation:			
$YEARS_{\pi>40}$	0.146 ***	0.241 ***	0.270 ***
	(0.056)	(0.063)	(0.069)
$FAIL2_{T+t-1}$ $(t=2,3)$	1.111 ***	0.926 **	0.619 *
	(0.317)	(0.371)	(0.374)
$CE_{YEARS<=3}$		-1.414 ***	-1.279 ***
		(0.295)	(0.349)
Political economy variables:			
$DURREG_{T}$		-0.039 ***	-0.045 ***
		(0.011)	(0.011)
$XCONST_{T+i-1}$		0.755 ***	0.968 ***
		(0.151)	(0.143)
MAJ_{T+i-1}		-3.062 ***	-3.175 ***
		(0.686)	(0.538)
$POLINCOH_{T+i-1}$		-0.616 ***	-0.898 ***
		(0.187)	(0.204)
Non-policy macro variables:			
GDP_{T+t-1}			-7.034 ***
			(3.148)
$YGAP_{T+t-1}$			6.841
			(4.607)
$RES_{T+t-1} - RES_{T-1}$			-0.273 ***
			(0.060)
Policy macro variables:			
$GB_{T+t-1} - GB_{T}$			-3.431
			(6.678)
DC_{T+t-1}			0.346 ***
			(0.073)
Constant:	0.130	-1.022	-0.492
	(0.702)	(0.725)	(0.908)
χ^2 of LR test of: $\rho=0$	0.000	0.000	0.000
χ^2 of Wald test of: $\delta=0$	0.093	0.082	0.000
Pseudo R^2	0.315	0.501	0.562
Efron's R^2	0.358	0.555	0.601
% hits (p*=0.5)	75.0%	85.3%	85.3%
% hits (p*=0.413)	78.0%	86.0%	84.7%
% hits (p*=0.2)	69.3%	78.7%	78.0%
% $FAIL1=1$ hits (p*=0.2)	93.6%	90.3%	93.6%
% $FAIL1=0$ hits (p*=0.2)	52.3%	70.5%	67.1%
Number of observations	150	150	150

[1] Data are annual. Three (***), two (**), and one (*) stars mark statistical significance respectively at one, five, and ten percent levels.

Table 8. Dynamic *RATIO* Estimates

(heteroskedastic corrected standard errors in parenthesis)[1]

	(1)	*(1 RE)*	*(1 HECK.)*	*(2)*	*(2 RE)*	*(3)*	*(3 RE)*
Initial conditions:							
π_{T-1}	-1.056 ***	-1.056 ***	-1.179 ***	-1.262 ***	-1.263 ***	-0.969 ***	-0.969 ***
	(0.208)	(0.255)	(0.255)	(0.225)	(0.270)	(0.211)	(0.255)
RES_{T-1}	0.027 **	0.027 **	0.033 **	0.029 ***	0.029 **		
	(0.011)	(0.012)	(0.015)	(0.010)	(0.013)		
GB_{T-1}	1.250 ***	1.250 ***	1.410 ***	1.698 ***	1.698 ***		
	(0.414)	(0.606)	(0.342)	(0.471)	(0.639)		
$YGAP_{T-1}$	0.887 ***	0.887 *	1.082 ***	1.054 ***	1.054 *		
	(0.317)	(0.520)	(0.384)	(0.351)	(0.545)		
ANCHOR	-0.220 **	-0.220 *	-0.363 *	-0.366 ***	-0.366 ***	-0.219 **	-0.219 **
	(0.109)	(0.116)	(0.194)	(0.114)	(0.126)	(0.092)	(0.107)
$OPENKA_{T-1}$	-0.325 ***	-0.325 **	-0.339 ***	-0.345 ***	-0.345 **	-0.264 ***	-0.265 **
	(0.106)	(0.154)	(0.122)	(0.109)	(0.160)	(0.073)	(0.119)
External controls:							
ToT_{T+t}	-0.011 **	-0.011 ***	-0.010 *	-0.009 *	-0.009 **	-0.011 ***	-0.011 ***
	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
$XDEM_{T+1} - XDEM_{T}$	-1.497 **	-1.497 **	-1.767 **	-1.278 **	-1.278 *		
	(0.591)	(0.720)	(0.873)	(0.607)	(0.710)		
Credibility/Reputation:							
$YEARS_{\pi>40}$	0.023 *	0.023	0.033 **	0.026 **	0.026	0.030 **	0.030 **
	(0.013)	(0.016)	(0.016)	(0.012)	(0.016)	(0.015)	(0.015)
$RATIO_{T+t-1}$ (t=2,3)	0.584 ***	0.584 ***	0.580 ***	0.533 ***	0.533 ***	0.473 ***	0.473 ***
	(0.106)	(0.062)	(0.094)	(0.109)	(0.063)	(0.103)	(0.065)
$CE_{YEARS<=3}$				-0.170 **	-0.170 *	-0.164 **	-0.164 *
				(0.076)	(0.092)	(0.075)	(0.086)
Political economy variables:							
$DURREG_{T}$				-0.007 ***	-0.007 **		
				(0.002)	(0.003)		
$XCONST_{T+i-1}$				0.070 ***	0.070 **		
				(0.024)	(0.031)		
MAJ_{T+i-1}				-0.412 ***	-0.412 ***	-0.296 ***	-0.296 **
				(0.110)	(0.153)	(0.101)	(0.133)
$POLINCOH_{T+i-1}$				-0.062 **	-0.062		
				(0.032)	(0.042)		
Non-policy macro variables:							
GDP_{T+t-1}						-1.568 ***	-1.568 **
						(0.515)	(0.690)
$YGAP_{T+t-1}$						1.996 ***	1.996 ***
						(0.536)	(0.739)
$RES_{T+t-1} - RES_{T-1}$						-0.078 ***	-0.078 ***
						(0.026)	(0.022)
Policy macro variables:							
$GB_{T+t-1} - GB_{T}$						-1.531 *	-1.531
						(0.917)	(1.065)
DC_{T+t-1}						0.034 **	0.035 **
						(0.015)	(0.017)
Constant:	2.330 ***	2.330 ***	2.249 ***	2.274 ***	2.274 ***	2.420 ***	2.420 ***
	(0.533)	(0.477)	(0.636)	(0.562)	(0.487)	(0.503)	(0.467)
χ^2 of LM test of: $\rho=0$	9.690 ***			8.340 ***		9.090 ***	
χ^2 of Wald test of: $\delta=0$	2.860 *			1.570		1.330	
R^2	0.581			0.618		0.632	
Number of observations	150	150	150	150	150	150	150

[1] Data are annual. Three (***), two (**), and one (*) stars mark statistical significance respectively at one, five, and ten percent levels.

Table 9. Dynamic pool estimates on Easterly's (1996) sample

(heteroskedastic corrected standard errors in parenthesis)[1]

	FAIL1	*FAIL2*	*RATIO*
Initial conditions:			
π_{T-1}	-35.470 **	-12.913 ***	-1.456 ***
	(17.660)	(2.528)	(0.222)
GDP_{T-1}		-18.747 ***	
		(5.418)	
ANCHOR	-9.935 **	-1.249 ***	-0.311 ***
	(4.218)	(0.456)	(0.119)
External controls:			
USi_{T+t}	109.048 *		
	(59.797)		
ToT_{T+t}		-0.103 **	-0.036 ***
		(0.046)	(0.011)
$XDEM_{T+1} - XDEM_{T}$	-48.034 **		
	(23.950)		
Credibility/Reputation:			
$YEARS_{\pi>40}$	0.620 **	0.235 ***	0.021
	(0.287)	(0.062)	(0.014)
$RATIO_{T+t-1}$ $(t=2,3)$			0.425 ***
			(0.105)
$CE_{YEARS<=3}$		-1.475 ***	-0.370 ***
		(0.342)	(0.108)
Political economy variables:			
$DURREG_{T}$		-0.021	
		(0.013)	
$XCONST_{T+i-1}$		0.482 ***	0.063 *
		(0.137)	(0.032)
$SOCCONF_{T+i-1}$	1.065 ***		
	(0.289)		
$POLINCOH_{T+i-1}$		-0.819 ***	-0.050
		(0.227)	(0.038)
Non-policy macro variables:			
GDP_{T+t-1}			-2.015 *
			(1.132)
$RES_{T+t-1} - RES_{T-1}$	-0.589 ***	-0.393 ***	-0.131 **
	(0.140)	(0.127)	(0.057)
Policy macro variables:			
$GB_{T+t-1} - GB_{T}$	-30.776 ***	-8.589 *	-2.700 **
	(10.820)	(4.818)	(1.210)
DC_{T+t-1}	0.625 *		
	(0.334)		
Constant:	0.473	15.016 ***	5.120 ***
	(2.406)	(5.419)	(1.174)
χ^2 of LR / LM test of: $\rho = 0$	0.840	0.000	0.630
Pseudo R^2 / R^2	0.806	0.584	0.765
Number of observations	81	81	81

[1] Data are annual. Three (***), two (**), and one (*) stars mark statistical significance respectively at one, five, and ten percent levels.