

The Political Economy of Nominal Macroeconomic Pathologies

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Recognizing that inflation and the macroeconomic policies that affect it can emanate from distributional conflicts in society, we examine the deep determinants of several nominal pathologies and related policy variables from a distributional perspective. We develop new instruments and use well-established existing instruments for these deep determinants and find that two deep determinants—societal divisions and democratic institutions—have a powerful and robust causal impact on nominal macroeconomic outcomes. Surprisingly, given the widespread attention accorded to the effects of populist democracy on inflation, democracy robustly serves to reduce inflation over the long term. A one standard deviation increase in democracy reduces inflation nearly fourfold. A similar increase in societal divisions increases inflation more than twofold. Our results are robust to alternative measures of democracy, samples, covariates, and definitions of societal division. It is particularly noteworthy that a variety of nominal pathologies and their proximate policy

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causes discussed in the recent macroeconomic literature, such as procyclical policy, absence of central bank independence, original sin, and debt intolerance, have common origins in societal divisions and undemocratic political institutions. [JEL O17, E61, E31]

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Why are some countries more prone to inflation and other nominal pathologies than others?¹ It is surprising that although so much of the recent literature has been devoted to explaining the cross-country variation in *real* variables—for example, in income (Hall and Jones, 1999; Acemoglu, Johnson, and Robinson, 2001; and Rodrik, Subramanian, and Trebbi, 2004), in growth (Barro and Sala-i-Martin, 2003), and the instability of growth (Rodrik, 1999; Quinn and Woolley, 2001; Acemoglu and others, 2003; and Fatás and Mihov, 2003)—much less attention has been paid to systematically analyzing the causal determinants of the cross-country variation in nominal variables.² This difference exists despite the fact that the cross-country variation, for example, in inflation is even more astounding than that in income. In a sample of 70 countries that are covered in this paper, our preferred measure of the core nominal macroeconomic outcome—the annual average rate of change of the nominal parallel market exchange rate—varies 3,167-fold between Nicaragua and Denmark.

In this paper, we attempt to fill this gap in the literature. Constructing a new instrument for societal divisions, and using the widely accepted instrument for political institutions developed by Acemoglu, Johnson, and Robinson (hereafter, AJR, 2001), and for trade openness developed by Frankel and Romer (hereafter, FR, 1999), we conduct a causal analysis of the determinants of inflation and other nominal pathologies over the long term.

I. Deep vs. Proximate Determinants

There are two varieties of explanations for variations in nominal pathologies such as inflation. One of these is that macroeconomic policies “cause” inflation. The relative inattention to the cross-sectional variation in nominal pathologies stems in part from the seeming confidence in the profession of knowing that policies are the causal determinant of such outcomes. For instance, much of the IMF’s work including its macroeconomic programs flows from, and is founded on, this proposition. As Stanley Fischer (2001),

¹This paper is a substantially revised version of Satyanath and Subramanian (2004). A longer, online version of this paper can be found at www.iie.com/publications/author_bio.cfn?author_id=488.

²Exceptions include Cukierman, Webb, and Neyapti (1992); Cukierman, Edwards, and Tabellini (1992); Romer (1993); Campillo and Miron (1996); and Desai, Olofsgård, and Yousef (2003). There is a large and growing literature on financial crises but that is not the concern of this paper.

the former First Deputy Managing Director of the IMF, puts it, “It is not worth arguing very much about those two words (‘Washington Consensus’), but it is worth arguing for the *policies* that we promote—sound money, prudent fiscal policy.”³

The uncomfortable corollary of this view is that variation in nominal outcomes such as inflation across countries arises from “some, perhaps accidental, lapse of attention or virtue on the part of monetary authorities or misguided concentration on the wrong variables such as the rate of interest in lieu of the quantity of money” (Hirschman, 1985, p. 56). If macroeconomic policies were indeed fundamental causes, we would have to believe, as Rogoff (2003) puts it, that the “monetary authorities just got bamboozled by bad Keynesian theories in the 1960s and 1970s. The great inflation of the 1970s and 1980s was the by-product of macroeconomic teaching malpractice. Once the world’s central bankers started coming to their senses in the 1980s, ending inflation was just a matter of communication and technique” (Rogoff, 2003, p. 11).

More recent variants of this argument go beyond monetary and fiscal policies and argue that other aspects of policies or other pathologies determine nominal outcomes. Cukierman, Webb, and Neyapti (1992) suggest that the lack of central bank independence might be a cause of inflation.⁴ Kaminsky, Reinhart, and Végh (2004) have shown that instability arises in part from the procyclicality of capital flows, which is aggravated by a procyclical fiscal policy stance. Fatás and Mihov (2003) make a similar point in arguing that volatility in government spending, afforded by discretion in fiscal policy, causes instability. Eichengreen, Hausmann, and Panizza (2003) implicitly make a case for another deep determinant—original sin—as an underlying pathology that makes it difficult for developing countries to borrow in their own currency. Reinhart, Rogoff, and Savastano (2003) posit implicitly that a history of irresponsible policies (of borrowing) is what makes a country vulnerable to instability.

That the relationship between policies, pathologies, and outcomes might only be a proximate one becomes evident if one poses the question as to why some countries follow distortionary policies or experience nominal pathologies and not others. For instance, if monetary or fiscal policy determines prices, what in turn causes monetary or fiscal policy and hence instability? Similarly, if the lack of central bank independence causes

³Hirschman offers another explanation for a policy-based view of inflation: “Economic theories of inflation dominate not because participants in the discussion are convinced that these theories hold the crucial variables, but rather because intricate analytical structures have been developed that lend themselves to ever further elaboration, some empirical testing, and—most important—the formulation of policy advice” (Hirschman, 1985, p. 53). This explanation is also consistent with the fact that nominal instability has typically been examined in a time-series rather than cross-section context probably because of the availability of high-frequency data and the sophisticated tools of time-series analysis that can be deployed. In a policy-based view, inflation is a technical rather than a political issue.

⁴More precisely, the lack of such independence results in fiscal populism.

instability, why do some countries chose to have such independence and others not? Such questions justify a search for deeper causes for instability.

Our effort falls in this latter category. The starting point for identifying these deeper causes is the recognition that inflation and the policies that affect it are part of the many redistributive tools available to governments. This is the basic political economy of inflation and the other pathologies that we seek to analyze. The demand for and supply of such pressures are thus the deep determinants that have an impact on nominal macroeconomic outcomes such as inflation. We then use the instrumental variables (IV) technique to examine the effects of deep determinants on nominal instability in a cross-national context.⁵ Aside from contributing to the macroeconomics literature, we contribute to the broader political economy literature by developing a new, theoretically grounded instrument for societal divisions.

We differ from other empirical examinations of nominal instability by sorting out the relative causal impacts of all plausible deep determinants. In contrast, Romer (1993) focuses almost exclusively on the role of trade openness in determining inflation. Cukierman, Edwards, and Tabellini (1992) emphasize the role of political instability, as do Campillo and Miron (1996), whereas Cukierman, Webb, and Neyapti (1992) are concerned with the impact of central bank independence. Desai, Olofsgård, and Yousef (2003) study the effects of democracy on inflation, as modified by income inequality. However, their focus is on short-term movements in inflation and their analysis relies on very different identification assumptions.

The strategy that we have adopted for this paper is more closely related to work on the institutional determinants of *real* instability, especially the papers by Sah (1991), Weede (1996), Rodrik (1999 and 2000), Quinn and Woolley (2001), Almeida and Ferreira (2002), and Acemoglu and others (2003). Rodrik (1999), for example, examines what happens to growth rates in response to shocks. Acemoglu and others (2003) analyze the variability of output—normal and large—in the context of a model with explicitly distributional elements. Quinn and Woolley (2001) compare the volatility of growth in democracies vs. authoritarian regimes. We extend this literature by considering the deep determinants of *nominal* outcomes.

Our main conclusion is that there is a strong causal relationship between societal divisions and democratic political institutions and long-term inflation. Surprisingly, given the widespread attention accorded to the effects of populist democracy on inflation, the poor responses of democracies to oil shocks, and the absence of a robust relationship between democracy and growth, democracy robustly serves to reduce inflation over the long term. For example, a one standard deviation increase in inequality (roughly the move from France to the Dominican Republic) leads to a more than twofold

⁵We have undertaken some preliminary work to exploit the time series variation in the data (see Satyanath and Subramanian, 2004) but the difficulties of such an exercise, especially given the persistence of institutions, as well as our interest in the long-run and deep determinants means that the cross-sectional variation remains very much our focus.

increase in inflation. Similarly, a one standard deviation increase in democracy (roughly the move from Uganda to Chile) leads to a 3.6-fold decline in inflation. The *t*-values for the coefficient on inequality and democracy are consistently significant at the 1 percent level and the relationship is robust to alternative measures of democracy, samples, covariates, and definitions of inflation. Furthermore, we find that a wide range of macroeconomic policies and pathologies are themselves causally affected by inequality and democracy.

II. The Deep Determinants of Nominal Pathologies: An Analytical Framework

In this section, we describe the logical causal links between three plausible deep determinants and nominal outcomes.

Societal Divisions and Distributive Conflicts

There is a long intellectual tradition going back to Marx and Kalecki, and more recently to Rowthorn (1977); Lindberg, Barry, and Maier (1985); Hirschman (1985); Sachs (1989); and Dornbusch and Edwards (1991), which traces nominal pathologies such as inflation to societal divisions and associated distributive conflicts between societal groups.⁶ In this view, inflation and the policy instruments affecting it are tools for redistribution.

There are a number of different ways in which macroeconomic instability can reflect and be a consequence of distributive conflicts between societal groups. First, in early (Marxian) analyses of business cycles, the expansion of bank credit during booms was seen as providing extra purchasing power for business to finance investments beyond the point that would have been possible without inflation. Second, inflation is an instrument par excellence for redistributing wealth: for example, from creditors to debtors, and away from those who hold money and other assets (unskilled human capital) that cannot be hedged against inflation.

A third example relates to borrowing and rising government indebtedness, which often substitutes for inflation as a means of financing unsustainable spending plans and hence promoting the interests of a particular group in society (for example, as in Sachs' (1989) description of the Latin American experiences of the 1970s and 1980s).

Democratic Political Institutions and Distributive Conflicts

Independent of the degree of societal division, the presence or absence of democratic political institutions can affect the extent of distributive conflict in a society. The literature has long emphasized how a democracy is

⁶Even Milton Friedman, who famously described inflation as always and everywhere a monetary phenomenon, is reported to have distinguished the proximate causes (excessive increase in money supply) from the "deeper" social causes in a seminar (Seldon, 1975).

vulnerable to populist impulses on the part of its leaders (Kaufman and Stallings, 1991). Another stream of the literature has focused on how divided government (a condition seldom found in authoritarian regimes) is conducive to delays in adjustments to economic shocks. In this line, Alesina and Drazen (1991) have argued that divided government results in wars of attrition over burden sharing for adjustment. These streams of the literature imply that democracies would be associated with relatively high inflation rates in the short term.

We argue that the propensity for democracy to increase inflation in the short term, as described above, does not necessarily imply that this problem will persist over the long term. The literature gives us ample support for such a theoretical claim. First, the presence of democratic checks on politicians' power can alleviate distributive conflict over the long term. As Rodrik (1999) argues in the context of recoveries from terms of trade shocks, democratic institutions cause contending societal groups to moderate their efforts to pass the burden of adjustment on to other groups. The argument in Acemoglu and others (2003) is similar and is framed in terms of constraints on the executive, helping mitigate the appropriation of wealth toward the executive or their preferred groups. Likewise, Persson, Roland, and Tabellini (1997) show that with appropriate checks and balances, separation of powers between executive and legislative bodies helps prevent the abuse of power by politicians. In effect, under these conditions the two branches discipline each other, and become more accountable to citizens in their choice of policies. From these papers, we draw the following empirical expectation for the effects of democracy. In the long term, the adverse effects of populism or legislative gridlock will be offset by the mechanisms of accountability and checks and balances, resulting in a negative association between democracy and inflation over the long term.

Openness and Redistributive Conflicts

In light of our definition of deep determinants, it is plausible that openness is another deep determinant of inflation.⁷ Most recently, Rogoff (2003) has argued that openness affects not just price levels but the rate of inflation. Rogoff argues, based on modern new open economy models, that monopoly in the product and labor markets creates a wedge between optimal and monopoly levels of employment. This wedge creates a motivation for central banks to inflate in order to drive employment above its "natural" market determined rate: "As the wedge becomes smaller, there is less to gain from unanticipated inflation. Central bank anti-inflation credibility is enhanced, even without any institutional change. As a consequence, average inflation

⁷There is a large and growing body of literature that has examined the impact of financial and trade openness on real instability, including the volatility in income and consumption (see Kose, Prasad, and Terrones (2003) and the references cited therein).

falls” (p. 19). Thus, openness affects not only the level of prices but the equilibrium inflation rate.

At first glance, the Rogoff explanation of openness smacks of social welfare planners optimizing some objective function that has no distributional elements. Yet there is a body of literature (for example, Rajan and Zingales, 2003) that views trade openness, like strong political institutions, as a mechanism for limiting the extent to which the elites can redistribute wealth toward themselves. One way to view the Romer (1993) and Rogoff (2004) explanations is that openness simply raises the costs to the elites who determine monetary policies of attempting to redistribute wealth toward themselves through inflation. All of these views justify including openness as a variable in our specifications, either as a competitor to democracy and inequality, or simply as a control variable.

III. Empirical Strategy

In light of the above discussion, we consider the effects on inflation (and other nominal variables) of three *deep* determinants—democratic political institutions (I), the level of openness of the economy (O), and the level of societal division (C). The aim of the paper is to test whether these determinants are important from a long-term perspective. We also test whether they are indeed deep determinants. Thus, much of the paper is devoted to examining the relationships between the deep determinants (D) and nominal outcomes (E). But we also try to establish that these determinants might be deep determinants by examining the relationships between the deep and the many possible proximate determinants (M), which could include monetary and fiscal policies, fiscal policy volatility (Fatás and Mihov, 2003), fiscal policy procyclicality (Kaminsky, Reinhart, and Végh, 2004), central bank independence (Cukierman, Webb, and Neyapti, 1992), original sin (Eichengreen, Hausmann, and Panizza, 2003), and debt intolerance (Reinhart, Rogoff, and Savastano, 2003).

Because we are concerned with long-term effects, in this paper our approach will rely on exploiting the cross-country variation in the data rather than the time-series variation. Thus, we will rely predominantly on cross-country regressions, with all variables measured as averages over the period 1960–2000. Ignoring nonlinearities, the economic relationship we are most interested in identifying is:

$$E_i = \phi + \alpha C_i + \beta I_i + \gamma O_i + \varepsilon_i, \quad (1)$$

where E_i is the measure of the nominal pathology we are studying in country i ; C_i , I_i , and O_i are, respectively, measures for societal division, democratic political institutions, and trade openness; and ε_i is the random error term.⁸ Throughout the paper, we will be interested in the size, sign, and significance

⁸Unless otherwise noted, all the right-hand-side variables are averages over the period for which instability (the left-hand-side variable) is measured.

of the three coefficients α , β , and γ . We will use normalized measures of C_i , I_i , and O_i in our core regressions, so that the estimated coefficients can be directly compared.⁹

As mentioned, we will also be interested in the relationship between the deep and proximate determinants, and particularly whether we can corroborate the claim that our variables are indeed “deep.” Thus, we will also be interested in the following relationship:

$$M_i = \varpi + \rho C_i + \sigma I_i + \kappa O_i + \mu_i, \quad (2)$$

where M captures plausible proximate causes of inflation.

The data and its sources are described in Appendix Table A.1. Appendix Table A.2 provides the summary statistics for the major variables of interest in this paper, and Appendix Table A.3 lists the countries that are included in the analysis.

IV. Measurement and Estimation Issues

We now turn to a number of measurement and estimation issues that arise in this study.

Measuring Nominal Instability, Openness, and Societal Divisions

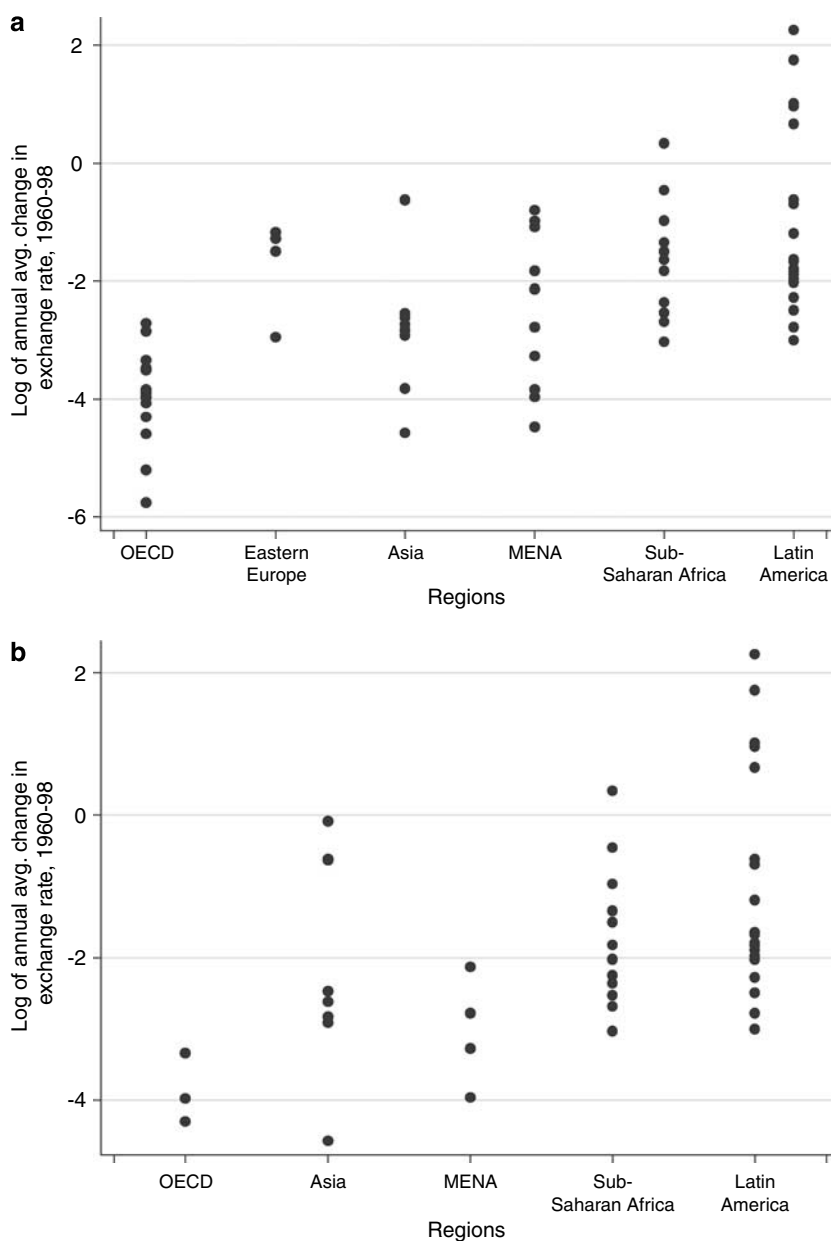
First, how should nominal macroeconomic pathologies be measured or proxied? The most obvious way, of course, is by inflation. Although we do conduct regressions using inflation (as conventionally measured), in our basic specification we choose to use a less commonly used measure of instability in our core specifications. We use the change in the nominal parallel market exchange rate, as compiled by Reinhart and Rogoff (2004). This measure has two advantages. First, it is a clear market-based measure. In many developing countries, for long periods of time in the postwar period, prices have been controlled and/or fixed. Even with a turn toward liberalization since the mid-to-late 1980s, prices of nontradables such as utilities remain regulated in many countries. As a result, prices may not convey all the information about underlying macroeconomic disequilibria. We thus expect parallel exchange rates to respond more clearly to underlying macroeconomic conditions than conventional measures of prices. Figure 1 presents the performance of the different countries (grouped by regions) on our core measure of nominal instability.¹⁰

Second, any measure of nominal instability should reflect problems stemming from debt accumulation, rescheduling or accumulation of arrears, and other external pathologies. As argued earlier, these are, and also reflect,

⁹That is, all regressors are expressed as deviations from the mean divided by the standard deviation.

¹⁰Of course, one concern with our measure is that parallel rates are based on very thin markets. For this reason, we also report the core results with inflation as the measure of nominal instability.

Figure 1. Nominal Instability by Region¹ (a) Large Samples and (b) Small Samples



Source: Authors' calculations.

¹Measured as log of annual average percent change in the nominal parallel market exchange rate.

OECD = Organization for Economic Cooperation and Development; MENA = Middle East and North Africa.

macroeconomic disequilibria. From this perspective, the market or parallel exchange rate is better suited to capturing these pathologies than conventional measures of prices.¹¹ Nevertheless, to ensure that our results are not driven by our measure, we show that alternative measures of nominal instability such as consumer price inflation also yield very similar results (see the discussion below).¹² Thus we measure inflation as the log of the average annual change (in percent) of the nominal parallel (black) market exchange rate for the period 1960–2000.¹³

We measure openness in the standard way used in the literature, as the ratio of exports and imports to GDP. We measure societal divisions along several dimensions—economic, religious, linguistic, and ethnic. In our core specification, we use economic inequality, measured as the Gini coefficient of income inequality, but we show that alternative measures yield very similar results.

Estimation Method: Ordinary Least Squares, IV, Instrumentation, and Sample

The parameters of interest in Equation (1) can most simply be estimated using ordinary least squares (OLS). Typically, this gives rise to three problems: endogeneity, measurement error, and omitted variables bias. In our basic specification, all three variables—democratic political institutions, openness, and societal division—are potentially endogenous. Clearly, nominal instability can affect political institutions; inflation can influence transitions to democracy or vice versa. Similarly, if societal division is measured by economic inequality, inflation clearly affects it because of its well-known redistributive effects, as discussed earlier. Inflation can also affect trade openness through a variety of channels. Most obviously, inflation leads to a real depreciation of the currency, and through a number of different channels it can reduce the amount of a country's trade.

Measurement error afflicts the political variable in particular because available measures only imperfectly capture the functions that political institutions are meant to serve. For instance, an accurate measure of democracy would capture checks on the power of the executive as well as the accountability and breadth of participation. As Gleditsch and Ward (1997) have pointed out, even the widely used democracy measure developed by Polity takes inadequate consideration of participation.

¹¹If purchasing power parity (PPP) holds, exchange rate changes should manifest themselves in domestic price changes. Insofar as they do not, the use of the exchange rate measure leads to the more general specification.

¹²In our small sample, the simple correlation between exchange rate and inflation (CPI) measures is 0.94.

¹³The Reinhart and Rogoff (2004) data on parallel market exchange rates do not cover the entire postwar period for all countries. We use countries for which at least 10 years of data are available.

To address these issues, we resort to a two-stage least squares (2SLS) methodology. We develop a new instrument for the societal division variable in our core specifications: income inequality. It is well known from the influential work of Engerman and Sokoloff (1994) that economic inequality is more pervasive in countries that grow plantation crops (sugar, tobacco, cocoa) as opposed to small-holding agriculture because the former gives rise to large rents. We construct a number of alternative instruments for income inequality based either on the extent of small-holding agriculture (measured by grain cultivation area as a share of arable land) or on the extent of plantation agriculture (measured by the presence or absence of land under sugar cultivation). We show below that these instruments (and many variants of them) yield very strong first-stage estimates for a wide variety of measures of inequality.

For democracy, we use the settler mortality instrument identified by Acemoglu and others (2003). For trade openness, we use the FR (1999) instrument that is derived from underlying geographic characteristics of countries involved in trade. The FR instrument has been used in a wide variety of empirical applications from growth (Rodrik, Subramanian, and Trebbi, 2004) to financial development (Rajan and Zingales, 2003).

It is true that the identifying assumptions used in these papers for the instrumentation strategy do not strictly carry over because the outcome of interest for us is nominal instability, compared with income in previous work. We maintain, nevertheless, that the instrumentation strategy remains valid for our purposes as well. First, it can be reasonably argued that these essentially historical and geographic instruments are exogenous to current instability. The real difference relates to the exclusion restrictions. In essence, we address potential violations of the exclusion restriction through a variety of robustness checks, which also serve to address the omitted variables bias. For example, we control for human capital, income, and terms of trade shocks in addition to numerous other plausible variables. We also note that some of the exclusion restrictions concerns relating to the settler mortality instrument are probably less significant in the context of studying our dependent variable than in the context of studying development. For example, one concern in AJR (2001) was that settler mortality could be a proxy for the disease environment, which could be persistent and thereby could affect current health conditions and current income. Although this may be a serious concern in the context of studying development, it is harder to make the case that the disease environment would have the same effect on current inflation.

There is one issue relating to the samples that arise from the instrumentation strategy that we deploy. Our core results are based on a sample of 70 countries for which we use the FR (1999) instrument for openness and the Engerman-Sokoloff-based instrument for inequality. However, because the sample includes countries that are not former colonies and for which colonial settler mortality data is thus not available,

for the democracy variable we use the initial value of the democracy measure instead of the average value for the entire time period.¹⁴ As a robustness check, we also conduct regressions for the subset of countries for which settler mortality is available, in which we instrument for democracy using the AJR (2001) instrument. In principle, this subsample of 48 is large enough to warrant inference. However, we do not get very good first-stage results for inequality in this subsample. Therefore, for this subsample, we use alternative strategies for addressing the endogeneity of inequality (principally using initial period values).

To sum up, in our core specification we instrument for openness and inequality and use initial period values of democracy, whereas in the subsample we instrument for openness and democracy and use initial period values of inequality. Our core results for democracy and societal division (as captured by income inequality) broadly hold across both samples, although the magnitudes do change, with the typical pattern being that the coefficients are larger when a particular variable is instrumented compared to when their (uninstrumented) initial values are used.

V. Results

Core Results

In Table 1a, we present our core results relating the three deep determinants to exchange rate instability. Note that unless specifically mentioned, when we refer to democracy below we refer to the measure of constraints on the chief executive developed by the Polity IV Project (Marshall and Jaggers, 2002) (XCONST). As Gleditsch and Ward (1997, p. 380) have found, “this variable virtually determines the democracy and autocracy score values” in Polity’s ratings. We display robustness checks with all the major alternative measures of democracy in the course of the paper. Likewise, unless specifically mentioned, our measure of inequality is from the WIDER data set (UNU, 2000).

In column 1 of Table 1a, we present OLS results in which the right-hand side variables are the average democracy, openness, and inequality over the period 1960–2000. The left-hand side variable is the log of the average change in the parallel exchange rate over the same period. Democracy and inequality display significant coefficients, with greater democracy having a dampening effect on inflation and greater inequality contributing to inflation.

Because we have a new set of instruments for inequality, we now turn to discussing the first-stage results displayed in the bottom panel of Table 1a. Our instrument for inequality attempts to capture the Engerman-Sokoloff

¹⁴As further cross-checks, we try different combinations of variables and instruments with no discernible impact on the results.

(1994) insight that economic inequality is related to type of agriculture: the greater the reliance on types of agriculture where ownership is widely spread, the lower the level of inequality; conversely, the more the reliance on plantation-type crops, the more concentrated wealth is likely to be. We proxy small-holder agriculture by the share of total arable area devoted to grains

**Table 1a. Deep Determinants of Nominal Macroeconomic Outcomes:
Core Specifications (Large Sample)**

(A) Second-Stage Results

	(1)	(2)	(3)	(4)	(5)	(6)
Trade openness	-0.285 (1.39)	-0.104 (0.35)	-0.185 (0.68)	-0.276 (0.99)	-0.100 (0.33)	-0.047 (0.16)
Democratic political institutions	-0.629 (3.60)***	-0.660 (3.59)***	-0.384 (2.16)**	-0.529 (3.08)***	-0.627 (3.09)***	-0.474 (2.18)**
Initial inequality	0.327 (2.04)**	1.235 (3.09)***	1.111 (3.62)***	1.189 (3.56)***	1.173 (3.32)***	1.282 (3.17)***
Initial log per capita (PPP) GDP					-0.089 (0.37)	
Instability of political institutions						0.330 (1.46)
Estimation method	OLS	IV	IV	IV	IV	IV
Whether regressor is instrumented						
Openness	No	Yes	Yes	Yes	Yes	Yes
Democratic political institutions	No	No	No	No	No	No
Inequality	No	Yes	Yes	Yes	Yes	Yes
Observations	70	70	70	69	70	70

Note: Except in column 4, the dependent variable is the log of the annual average percentage change in the nominal parallel exchange rate. In column 4, the dependent variable is the component of this variable that is orthogonal to real instability, and is derived as the residual from the regression of the log of the annual average percentage change in the nominal parallel exchange rate on the standard deviation of the real per capita GDP growth. Inequality is measured according to the Gini index (data from UNU, 2000). In column 3, inequality is the average of the contemporaneous values. In columns 2–6, inequality is instrumented by the share of arable land devoted to grain production circa 1950 and is described in the text; openness is instrumented by fitted openness from Frankel and Romer (1999). Initial per capita income (in purchasing power parity (PPP) terms) is for 1960 or for the earliest year for which data are available and is from the Heston, Summers, and Aten (2002). Instability of political institutions is measured as the standard deviation of the index of constraint on the executive. Robust *t*-statistics are in parentheses; and *, **, and *** denote significance at 10, 5, and 1 percent, respectively. In general, we do not present the *R*-squares for the second stage because these are not properly defined.

Table 1a (concluded)

(B) First-Stage Regression Results

Dependent Variable	Openness	Initial Inequality	Openness	Initial Inequality
Democratic political institutions	0.191 (2.70)***	0.020 (0.20)	0.200 (2.86)***	0.034 (0.32)
Openness instrument	0.747 (8.66)***	0.029 (0.23)	0.729 (9.04)***	-0.071 (0.58)
Inequality instrument (share of arable land devoted to grain production)	-0.4 (1.32)	-1.4 (3.57)***		
Inequality instrument (dummy = 1 if share of arable land is above the median value in the sample)			-0.279 (1.99)*	-0.695 (3.29)***
R-squared	0.58	0.17	0.60	0.15
Observations	70	70	70	70
Weak instrumentation diagnostics				
Correlation between fitted values of first-stage regressions		-0.14		-0.09
Cragg-Donald statistic		7.63		6.79
Critical value (5 percent significance, $r = 0.1$)		7.03		7.03
Critical value (5 percent significance, $r = 0.15$)		4.58		4.58

Source: Authors' calculations.

Note: The first two columns correspond to the second-stage equation in column 2 in panel A. The next two columns show the impact of changing the instrument for inequality. The two instruments for inequality are described in the text and in the appendix (web version). Robust t -statistics are in parentheses. *, **, and *** denote significance at 10, 5, and 1 percent, respectively.

(wheat, barley, and oats) in 1950.¹⁵ The data are from Mitchell (1998a and 1998b). We describe our instrument in greater detail in the Appendix of the web version of this paper. The first-stage results using this instrument are shown in the first two columns of panel B of Table 1a. In the first-stage equation for inequality, the instrument has the right (negative) sign (the greater the share of land devoted to grain cultivation, the lower the level of inequality) with a t -statistic of 3.57. Weak instrumentation does not appear to be a problem because the correlation between the fitted values of the two first-stage equations is low and the Cragg-Donald statistic is above the critical Stock-Yogo (2005) values for weak instruments in the presence of multiple endogenous regressors.¹⁶

¹⁵Ideally, we would like to go back farther in time, but doing so reduces the data availability. To the extent that there is persistence in agricultural patterns, not going back need not be a serious concern. We also added maize and rice to the category of small-holder crops and the results were similar.

¹⁶It is worth noting that our first stage equations pass the more demanding Stock-Yogo test for valid inference, which relates to the size of the IV estimate relative to OLS, because the critical values are larger (and rise more sharply with the number of instruments) than for the Stock-Yogo test for unbiased estimation (Stock and Yogo, 2005).

(A Cragg-Donald statistic that falls short of these critical values indicates the presence of a weak instrument.) It is particularly noteworthy that in the first-stage equation for inequality, the democracy variable is not significant. This suggests that we are extracting information about inequality that is not derived from or correlated with institutions.

When we use variants of the instrument—for example, a dummy that takes on a value of 1 for above-median shares of land devoted to grain cultivation and zero otherwise—we obtain similar results (columns 3 and 4). We also get good results when we use a dummy that takes a value of 1 for countries that were sugar producers in 1950 (available upon request). This dummy has a positive sign in the first stage validating the Engerman-Sokoloff (1994) hypothesis that greater sugar cultivation results in greater inequality.

Having established that we have a good first stage for our inequality instrument, we can turn to the second-stage results. Column 2 in Table 1a contains the core IV specification in which we instrument for openness and initial inequality and use the initial period value of democracy. In this specification, democracy and inequality are statistically significant at the 1 percent level. The signs on the coefficients are unchanged relative to the OLS specification in column 1.

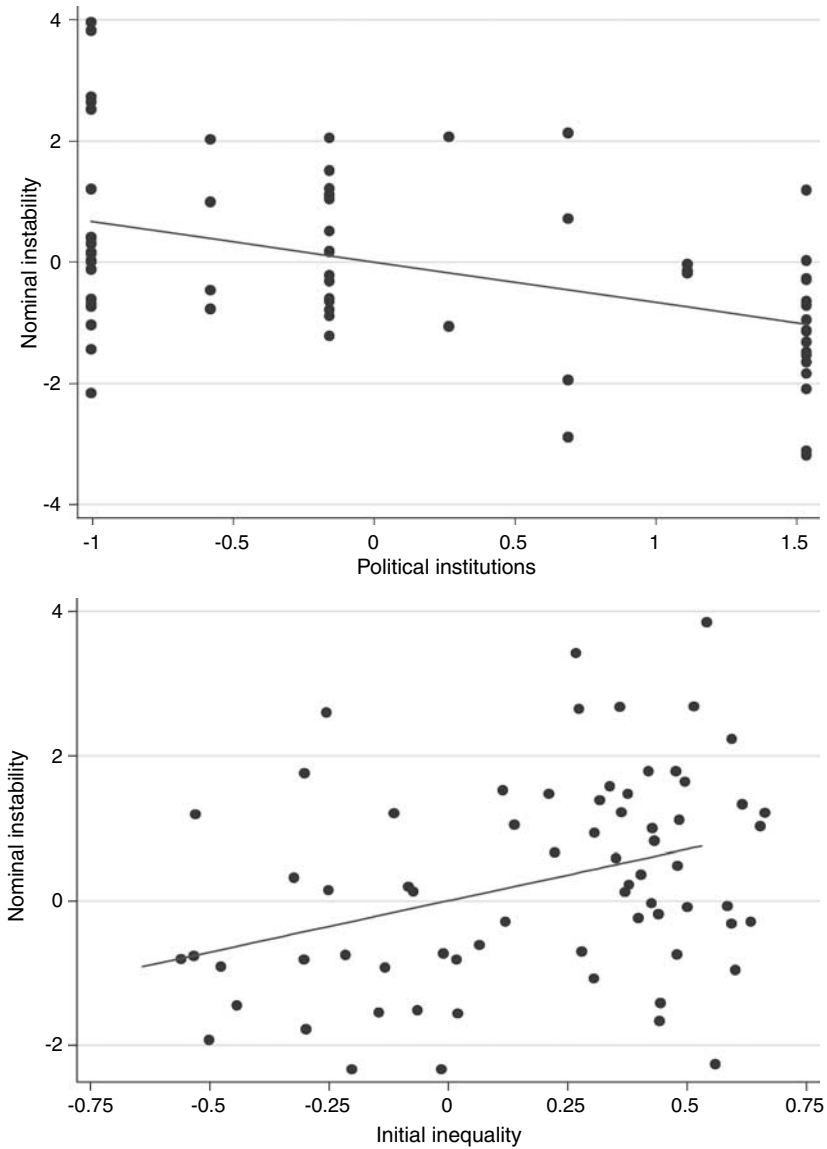
Because all right-hand side variables are expressed in normalized form, the coefficients can be directly compared. The magnitudes of the coefficients indicate that inequality exerts the greatest impact on our measure of inflation, about twice as large as democracy. (Figure 2, panel A displays the results for the core specifications.) The results indicate that a one standard deviation increase in inequality (roughly the move from France to the Dominican Republic) increases our measure of inflation two and a half times. Similarly, a one standard deviation improvement in democracy (2.4 points in a 7-point scale) reduces inflation by about half.¹⁷ (Note that when we instrument for democracy, as we do in Table 1b, the substantive effect of democracy increases.)

In column 3 of Table 1a, we use the value of average inequality over the sample period (in place of initial inequality). Democracy and inequality remain statistically significant. Our results for democracy do not change when the WIDER (UNU, 2000) inequality measure is replaced by that of Deininger and Squire (1996) (not shown).

One potential concern is that our left-hand side variable, inflation, which is a measure of nominal instability, is actually picking up the effects of real instability. One way to address this is to add a control variable for real instability, which we do in a later table. Here we resort to an alternative means of addressing this concern. We place on the left-hand side the residuals of a regression of our measure of inflation on real instability (standard deviation of per capita GDP growth between 1960 and 2000)—so this is a

¹⁷Our democracy measure XCONST takes a minimum value of 1 and a maximum of 7 in our sample.

Figure 2. Deep Determinants of Nominal Outcomes (Large Sample)
(Conditional correlations)



Source: Authors' calculations.

Note: The slopes of the lines correspond exactly to the coefficient in the specification in column 2 of Table 1a, Panel A.

measure of inflation purged of any contamination by real instability. Column 4 shows that the effects of democracy and inequality are robust to changing the left-hand side variable in this way. (We conduct numerous other robustness checks of the left-hand side variable later in the paper.)

Table 1b. Robustness to Alternative Nominal Outcomes (Large Sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable	Inflation (CPI)	Inflation (GDP defl.)	Inflation (CPI)	Inflation (GDP defl.)	Inflation (CPI)	Inflation (GDP defl.)	Inflation (CPI)	Inflation (GDP defl.)	Inflation (CPI)	Inflation (GDP defl.)
Openness	-0.214 (1.20)	-0.162 (0.86)	-0.118 (0.47)	-0.091 (0.37)	-0.149 (0.61)	-0.117 (0.49)	-0.117 (0.46)	-0.100 (0.42)	-0.081 (0.33)	-0.036 (0.16)
Democratic political institutions	-0.443 (3.05)***	-0.342 (2.45)**	-0.462 (3.24)***	-0.355 (2.61)**	-0.351 (2.52)**	-0.266 (2.04)**	-0.548 (3.18)***	-0.426 (2.66)***	-0.294 (1.85)*	-0.176 (1.24)
Initial inequality	0.067 (0.43)	0.053 (0.33)	0.495 (1.94)*	0.396 (1.52)	0.437 (2.05)**	0.356 (1.62)	0.642 (2.34)**	0.529 (1.83)*	0.530 (2.05)**	0.441 (1.68)*
Initial log per capita (PPP) GDP							0.216 (1.26)	0.192 (1.21)		
Instability of political institution									0.285 (1.75)*	0.316 (1.92)*
Estimation method	OLS	OLS	IV	IV	IV	IV	IV	IV	IV	IV
Observations	68	70	68	70	68	70	68	70	68	70

Source: Authors' calculations.

Note: The dependent variable is the log of the annual average percentage change in consumer prices (odd-numbered columns) or in the GDP deflator (even-numbered columns). Inequality is measured according to the Gini index (data from UNU, 2000). In columns 5 and 6, inequality is the average of the contemporaneous values. In columns 3–10, inequality is instrumented by the share of arable land devoted to grain production circa 1950 and is described in the text; openness is instrumented by fitted openness from Frankel and Romer (1999). Initial per capita income (in PPP terms) is for 1960 or for the earliest year for which data are available and is from the Heston, Summers, and Aten (2002). Instability of political institutions is measured as the standard deviation of the index of constraint on the executive. Robust *t*-statistics are in parentheses; *, **, and *** denote significance at 10, 5, and 1 percent, respectively.

In light of the exclusion and omitted variables considerations mentioned above, in column 5, we introduce the initial period level of per capita GDP (measured in PPP terms) as a control. (We expect that this also proxies for the state of development of the financial system.) Once again, our results for democracy and inequality are robust to this change.

In column 6, we control for political instability as captured by the standard deviation of our democracy score, and our results are unchanged.

In Table 1a, we measured nominal instability in terms of the changes in the parallel market exchange rate. In Table 1b, we check whether our results are robust if our left-hand side variable is measured differently. We take as the dependent variable the log of average annual inflation (CPI in the odd-numbered columns and the GDP deflator in the even-numbered columns). In the remaining columns, we follow the specifications in Table 1a and find that the results are similar. As can be seen, the effect of democracy is robust in seven out of eight specifications in each sample, whereas the effect of inequality only falls short when using the GDP deflator definition.¹⁸

In Table 1c, we check whether our results presented in Table 1a are changed if we address the endogeneity of institutions by instrumenting for it using the settler mortality variable. Recall that in this subsample of 48 former colonies, we instrument for openness and democracy and use the initial period value of inequality. In the lower panel of Table 1c, we report the first-stage regression results for the specification presented in column 2 of the top panel. The instruments are highly significant and have the right sign. The correlation between the fitted values of the first-stage equations is reassuringly low (the low correlation signifies that the instruments have explanatory power that is distinct for the two endogenous regressors). In the Stock-Yogo (2005) test, the null hypothesis of weak instrumentation is rejected.

The columns in the top panel of Table 1c are identical to those for Table 1a. As is apparent, the second-stage results for democracy and inequality are unchanged relative to Table 1a. The magnitudes change to some extent; in particular, instrumenting for institutions doubles the coefficient value from about 0.7 (in the larger sample in column 2 of Table 1a) to 1.35 in the smaller sample (column 2 in Table 1b). The results for openness are somewhat stronger in this subsample. Table 1d shows that the results presented in Table 1b are also substantively unchanged when using the small sample, except that openness also appears to have a significant negative effect on inflation consistent with Rogoff (2003).

In light of the work of Glaeser and others (2004), one interesting question is whether we are picking up the effect of institutions or human capital. Glaeser and others argue that settlers did not just bring institutions to low

¹⁸In results that are available in the working paper version of this paper (Satyanath and Subramanian, 2004), we show many more of our results for nominal exchange rate changes that carry over to conventional definitions of inflation. Note that the first stage for this specification does not change compared to the specification in Table 1a.

mortality environments, they also brought their education. Education, as per Glaeser and others, teaches citizens how to resolve their differences without conflict, which in turn promotes development. They show that when this channel is accounted for, human capital has a significant effect on development whereas institutions do not. It is plausible that human capital (such as higher levels of education in the population) contributes to a less conflictual environment, which in turn results in reduced redistributive struggles and hence lower inflation and greater macroeconomic stability.

**Table 1c. Deep Determinants of Nominal Macroeconomic Outcomes:
Core Specifications (Small Sample)**

(A) Second-Stage Results

	(1)	(2)	(3)	(4)	(5)	(6)
Trade openness	-0.399 (1.85)*	-0.607 (2.12)**	-0.608 (2.07)**	-0.742 (2.66)**	-0.564 (1.38)	-0.562 (1.90)*
Democratic political institutions	-0.625 (2.85)***	-1.354 (4.42)***	-1.269 (4.46)***	-1.109 (3.67)***	-2.490 (2.81)***	-1.266 (3.26)***
Initial inequality	0.434 (2.01)*	0.676 (2.77)***	0.421 (1.77)*	0.752 (3.20)***	0.712 (2.45)**	0.630 (2.38)**
Initial log per capita (PPP) GDP					1.133 (2.15)**	
Instability of political institutions						0.169 (0.60)
Estimation method	OLS	IV	IV	IV	IV	IV
Whether regressor is instrumented						
Openness	No	Yes	Yes	Yes	Yes	Yes
Democratic political institutions	No	Yes	Yes	Yes	Yes	Yes
Inequality	No	No	No	No	No	No
Observations	48	48	48	47	48	48

Note: Except in column 4, dependent variable is the log of the annual average percentage change in the nominal parallel exchange rate. In column 4, the dependent variable is the component of this variable that is orthogonal to real instability, and is derived as the residual from the regression of the log of the annual average percentage change in the nominal parallel exchange rate on the standard deviation of the real per capita GDP growth. In columns 2–6, democratic political institutions, measured as the constraint on the executive, are instrumented by settler mortality from Acemoglu, Johnson, and Robinson (2001). Openness is instrumented by fitted openness from Frankel and Romer (1999). Inequality is measured according to the Gini index (data from UNU, 2000). In column 3, inequality is the average of the contemporaneous values. Initial per capita income (in PPP terms) is for 1960 or for the earliest year for which data are available and is from the Heston, Summers, and Aten (2002). Instability of political institutions is measured as the standard deviation of the index of constraint on the executive. Robust *t*-statistics are in parentheses. *, **, and *** denote significance at 10, 5, and 1 percent, respectively.

Table 1c (concluded)

(B) First-Stage Regression Results ¹		
Dependent Variable	(1) Openness	(2) Democratic Institutions
Initial inequality	0.162 (1.45)	0.245 (2.01)*
Openness instrument (predicted openness)	0.871 (6.74)***	0.008 (0.06)
Instrument for institutions (settler mortality)	-0.228 (2.07)**	-0.497 (4.15)***
<i>R</i> -squared	0.53	0.36
Observations	48	48
Weak instrumentation diagnostics		
Correlation between fitted values of first-stage regressions		0.06
Cragg-Donald statistic		9.41
Critical value (5 percent significance, $r=0.1$)		7.03
Critical value (5 percent significance, $r=0.15$)		4.58

Source: Authors' calculations.

¹Corresponds to the second-stage equation in column 2 in panel A. Robust *t*-statistics are in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent, respectively.

Unfortunately, it is not easy to disentangle the relative effects of human capital and institutions because of the lack of good and separate instruments. When we replace institutions with human capital, the results are qualitatively similar in the small sample, whereas in the large sample, the coefficient on institutions is significantly greater (available upon request). This suggests that even if one believes that the democracy measure incorporates elements of human capital, there is probably more relating democracy to macroeconomic stability than human capital.

How Deep are the Deep Determinants?

We have established thus far that the deep determinants matter significantly for inflation. The question then is, how do they do so? We look at the relationship between our deep determinants and money supply, central bank independence, fiscal policy procyclicality (Kaminsky, Reinhart, and Végh, 2004), fiscal policy volatility (Fatás and Mihov, 2003), original sin (Eichengreen, Hausmann, and Panizza, 2003), and external rating (Reinhart, Rogoff, and Savastano, 2003). All of these are significantly correlated with nominal outcomes and may thus be plausibly considered as determinants of nominal stability (results available upon request).

Table 1d. Robustness to Alternative Nominal Outcomes (Small Sample)

Dependent Variable	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
	Inflation (CPI)	Inflation (GDP defl)	Inflation (CPI)	Inflation (GDP defl)	Inflation (CPI)	Inflation (GDP defl)	Inflation (CPI)	Inflation (GDP defl)	Inflation (CPI)	Inflation (GDP defl)	Inflation (CPI)	Inflation (GDP defl)	Inflation (CPI)	Inflation (GDP defl)	Inflation (CPI)	Inflation (GDP defl)
Openness	-0.499 (2.66)**	-0.340 (1.77)*	-0.635 (2.39)**	-0.636 (2.49)**	-0.647 (2.40)**	0.642 (2.51)**	-0.553 (1.49)	0.600 (2.06)**	-0.541 (2.04)**	-0.559 (2.15)**						
Democratic political institutions	-0.382 (2.06)**	-0.346 (2.04)**	-0.724 (2.91)***	-0.603 (2.49)**	-0.637 (2.85)***	-0.558 (2.46)**	-2.092 (2.73)***	-1.540 (2.42)**	-0.508 (1.52)	-0.457 (1.42)						
Initial inequality	0.468 (2.40)**	0.216 (0.97)	0.589 (2.74)***	0.345 (1.46)	0.454 (2.05)**	0.277 (1.29)	0.663 (2.59)**	0.375 (1.56)	0.482 (2.18)**	0.267 (1.14)						
Initial log per capita (PPP) GDP							1.226 (2.68)**	0.935 (2.46)**								
Instability of political institutions													0.370 (1.51)	0.281 (1.02)		
Estimation method	OLS	OLS	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Observations	47	48	47	48	47	48	47	48	47	48	47	48	47	48	47	48

Source: Authors' calculations.

Note: These specifications correspond exactly to those in Table 1b, except that democratic institutions are instrumented by settler mortality (as in Table 1c) and inequality is not instrumented.

The question then is, are these determinants proximate in the sense of being causally affected by the deep determinants? Table 2 depicts the results for the large sample.¹⁹ It turns out that democracy and inequality are significant determinants of many or all of these variables, especially in our preferred larger sample. This suggests that policies and pathologies that affect nominal outcomes might have common origins in authoritarian political institutions and societal divisions.

VI. Robustness

Alternative Measures of Political Institutions

So far we have used Polity's (Marshall and Jaggers, 2002) measure of constraints on the executive (XCONST) as our measure of democracy. (Recall that this is the variable that drives Polity's democracy rating.) We check for the robustness of our core result (in Table 1a, column 2) to alternative measures of democracy in Table 3.

We use two alternative measures to capture constraints on the chief executive; Checks (from Beck and others, 2001) and Polcon3 (from Henisz, 2000). Both are counts of the number of veto players—actors whose approval is necessary for a shift in policy from the status quo. The higher the score, the greater the constraints. In general, authoritarian regimes receive low scores on these variables.

We also display the results for two overall measures of democracy that are driven significantly by the XCONST measure, namely, “democ” and “polity” both from the Polity IV Project (Marshall and Jaggers, 2002). Polity is an alternative measure of democracy and is obtained by subtracting a measure of the extent of authoritarianism in a political system from the democracy measure above. We also report results for the behavioral measure of democracy (REG) developed by Przeworski and others (2000), which considers democracy to be present when there has been turnover in government. Note that REG is a dummy variable. (Whereas democracy is coded as 0 in REG, we relabel it as 1 in order to facilitate comparability of signs with other measures.)

Finally, Table 3 also includes two variables that capture aspects of democracy that are missed by the Polity measures (which are driven by constraints on the executive). These variables are “W” (Buono de Mesquita and others, 2003), which measures the size of the winning coalition, and “voice” (Kaufmann, Kraay, and Mastruzzi, 2003), which is a perception-based measure of the extent of say that the average person has in a political system. We note that although all these measures are positively correlated, the correlation is not perfect. (The correlations range from 0.8 to 0.9.)

¹⁹For readers interested in results for the small sample, please consult the online version of this paper available at http://www.iie.com/publications/author_bio.cfm?author_id=488

Table 2. How Deep Are the Deep Determinants? (Large Sample)

	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	Log money growth	Original sin	External rating	Fiscal policy cyclicality	Fiscal policy volatility	CBI
Openness	-0.115 (0.65)	0.093 (1.62)	-0.704 (0.72)	-0.006 (0.11)	0.054 (0.25)	-0.060 (1.69)*
Democratic political institutions	-0.324 (2.94)***	-0.080 (1.89)*	1.849 (2.26)**	-0.102 (2.50)**	-0.219 (2.09)**	-0.044 (2.34)**
Initial inequality	0.106 (0.62)	0.174 (1.76)*	-4.246 (3.96)***	0.288 (2.51)**	0.920 (2.76)***	0.001 (0.02)
Memorandum item						
This row depicts the bivariate relationship between exchange rate inflation and the variable shown in the column	1.569 (14.09)***	2.629 (4.21)***	-0.236 (6.21)***	4.463 (6.43)***	1.845 (7.65)***	6.928 (7.33)***
Estimation method	IV	IV	IV	IV	IV	IV
Observations	66	54	42	57	53	49

Source: Authors' calculations.

Note: The instruments for the variables in Table 2 and Appendix Table A.2 correspond, respectively, to those in Tables 1a and 1c. Original sin, measured as securities issued in home currency as a share of total securities issued, is from Eichengreen, Hausmann, and Panizza (2003). External rating by institutional investors is from Reinhart, Rogoff, and Savastano, (2003). The index of procyclicality of fiscal policy, from Kaminsky, Reinhart, and Végh (2004), combines two measures of correlations: (1) correlations between real government expenditure and inflation tax on the one hand and real GDP on the other, and (2) a measure of the difference between real government expenditure in "good" and "bad" times. Fiscal policy volatility is from Fatás and Mihov (2003). Central bank independence (CBI), which is measured in terms of the turnover of the head of the institutions, is from Cukierman, Webb, and Neyapti (1992).

Regardless of variable chosen to measure the democratic character of political institutions, democracy displays (in both small and large samples) a strong negative relationship with exchange rate instability, with significance obtained at the 1 or 5 percent levels. The magnitude of the coefficient is similar across most measures of democracy. Note that changing the measure of democracy also leaves unaffected the significant impact of inequality.

Additional Controls

Omitted variables are a common problem in cross-section regressions. So we consider in Table 4 the possible controls that we might have left out of our

Table 3. Robustness to Alternative Definitions of Political Institutions (Large Sample)
(Dependent variable is log of annual average percent change in nominal parallel exchange rate)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Openness	-0.166 (0.50)	-0.252 (0.80)	-0.156 (0.54)	-0.095 (0.32)	-0.194 (0.61)	-0.176 (0.59)	-0.142 (0.50)
Initial inequality	1.495 (3.44)***	1.115 (3.09)***	1.130 (3.05)***	1.177 (2.99)***	1.433 (3.43)***	0.986 (2.92)***	1.171 (3.13)***
Polcon3	-0.516 (2.01)**						
Checks		-0.570 (3.09)***					
Democ			-0.788 (4.62)***				
Polity				-0.720 (3.84)***			
REG					-0.558 (2.34)**		
Voice						-0.487 (2.42)**	
W							-0.721 (3.78)***
Estimation method	IV	IV	IV	IV	IV	IV	IV
Observations	70	70	70	70	69	70	70

Source: Authors' calculations.

Note: "Polcon3" and "Checks" are measures of fragmentation of the political system (scales 1 to 7.3 and 0 to 1, respectively). "Democ" is a general measure of the openness of political institutions (scale 0 to 10). "Polity" is computed by subtracting a measure of the closedness of political institutions from the "Democ" measure (range -10 to 10). "REG" is a measure of democracy from Przeworski and others (2000), a dummy variable that takes on a value of 1 to denote a democracy. "Voice" is a measure of the extent of say that the average person has in a political system. "W" is a measure of the proportion of the population whom the leader must please in order to stay in office (scale 0 to 1). The instruments for the variables in Table 3 correspond to those in Tables 1a (columns 1 and 2 of the lower panel).

core specification. This exercise is also an implicit test of the validity for our 2SLS procedure because we directly control for many of the variables that could plausibly be correlated with our instruments and macroeconomic instability. Note that henceforth all tables relate to our larger sample. Our results are substantively unchanged when we conduct the same regressions on the smaller sample (available upon request).

One concern is whether we are actually picking up the effects of real rather than nominal instability. For example, if there are real shocks, and macroeconomic policies are not countercyclical, nominal instability could be

Table 4. Robustness to Covariates (Large Sample)
(Dependent variable is log of annual average percent change in nominal parallel exchange rate)

	(1)	(2)	(3)	(4)	(5)	(6)
Openness	-0.474 (1.43)	-0.582 (1.65)	-0.220 (0.73)	-0.141 (0.51)	-0.310 (1.05)	-0.146 (0.45)
Democratic political institutions	-0.250 (1.24)	-0.288 (1.82)*	-0.634 (3.52)***	-0.430 (3.11)***	-0.390 (2.12)**	-0.540 (2.60)**
Initial inequality	1.233 (2.09)**	1.098 (2.12)**	1.193 (2.48)**	0.525 (1.28)	1.147 (3.25)***	1.441 (3.01)***
Standard deviation of real growth	0.409 (1.85)*					
Worst output drop		0.127 (2.90)***				
Terms of trade (TOT) growth			0.079 (0.32)			
Standard deviation of TOT growth				0.713 (2.93)***		
Revolutions and coups					0.433 (2.33)**	
French legal origin						0.072 (0.31)
Socialist legal origin						0.769 (3.21)***
Estimation method	IV	IV	IV	IV	IV	IV
Observations	58	58	67	67	66	69

Source: Authors' calculations.

Note: The worst output drop between any two years over the period 1970–97 is from Acemoglu and others (2003). The legal origin variables are dummies. The instruments for the variables in Table 4 correspond, respectively, to those in Tables 1a (columns 1 and 2 of the lower panel) and 1c.

merely the consequence of real instability. To address this concern, we introduce two measures of real instability from Acemoglu and others (2003). The first is the standard deviation of real growth rates and the second is the worst output drop between any two years (columns 1 and 2). Columns 3 and 4 add terms of trade changes or their variability as controls. In column 5, we control for extreme political instability as a proxy of revolutions and coups, and in column 6, we add the legal origin of countries as a control. Either inequality or democracy is significant in every specification and each of these is significant in five out of six specifications.

Samples

In Table 5, we show that our results are robust to changes in the sample. In column 1, we exclude the five highest inflation countries (Argentina, Bolivia,

Table 5. Robustness to Regional Dummies, Influential and Extreme Observations (Large Sample)*(Dependent variable is log of annual average percent change in nominal parallel exchange rate)*

Omitted observations	(1) BRA, ARG, NIC, BOL, PER	(2) NGA	(3) None	(4) Latin America	(5) Sub- Saharan Africa	(6) OECD
Openness	0.042 (0.14)	-0.107 (0.36)	-0.126 (0.41)	0.163 (0.40)	-0.174 (0.55)	-0.170 (0.57)
Democratic political institutions	-0.511 (3.03)***	-0.664 (3.53)***	-0.573 (3.04)***	-0.630 (2.96)***	-0.655 (3.10)***	-0.361 (1.66)
Initial inequality	1.123 (3.04)***	1.241 (3.07)***	0.689 (1.21)	1.494 (2.40)**	1.163 (2.71)***	0.946 (2.14)**
Latin America dummy			1.122 (1.46)			
Sub-Saharan Africa dummy			0.657 (1.03)			
North Africa/Middle East dummy			0.050 (0.08)			
Estimation method	IV	IV	IV	IV	IV	IV
Observations	65	69	70	51	58	51

Source: Authors' calculations.

Note: In column 1, five of the highest instability observations (Argentina [ARG], Bolivia [BOL], Brazil [BRA], Nicaragua [NIC], and Peru [PER]) are omitted. In column 2, the Belsley-Kuh-Welsch (1980) test for influential observations is applied, which leads to the omission of Nigeria (NGA) from the sample. Columns 4, 5, and 6, omit, respectively, observations relating to Latin America, sub-Saharan Africa, and the OECD countries. The instruments for the variables in Table 5 correspond to those in Table 1a (columns 1 and 2 of the lower panel).

Brazil, Nicaragua, and Peru) and find that the effects of democracy and inequality are robust. In column 2, we drop Nigeria because it is identified by the Belsley-Kuh-Welsch (1980) test as an influential observation. Column 3 includes regional dummies,²⁰ whereas in columns 4 to 6, we drop, respectively, Latin American, sub-Saharan African, and OECD countries from our sample. Democracy is significant in all cases except in the last column where it falls narrowly short of significance. The only specification where the inequality variable is not significant is with the inclusion of all the regional dummies.

²⁰Our result is also robust to the inclusion of a dummy for East Asian countries (not shown).

Which Societal Divisions?

Nothing in our approach identifies which particular type of societal division—economic, ethnic, religious, or linguistic—should matter. Any of these could potentially lead to the distributive pressures described earlier. For instance, Alesina and La Ferrara (2005) show that ethnic fragmentation has adverse effects on the provision of various public goods. Because low inflation can be perceived as a public good, we explicitly check to see if this form of societal fragmentation differs in its effects from other forms of societal fragmentation.

We use different measures available in the literature for ethnic and/or religious fragmentation (from Alesina and others, 2003; and Fearon, 2003) and assess the robustness of democracy and inequality to controlling for these different measures of societal division. In column 1 of Table 6, we use an instrument for inequality in the first stage that is a dummy that takes on a value of 1 for countries that are above the median in terms of the share of cultivated area devoted to small-holder agriculture (the first stage corresponding to this specification is reported in columns 3 and 4 in the lower panel of Table 1a). Inequality and democracy remain significant. In column 2, we use as the instrument a dummy if a country was a sugar producer in 1950. The first stage is slightly weaker (albeit still significant) than the specifications based on arable land devoted to grain cultivation, but inequality and democracy are significant in the second stage. In column 3 we use the measures of Alesina and others of ethnic and religious fractionalization without instrumenting for them (on the grounds that they can be treated as exogenous) and exclude income inequality. In column 4, we replace Alesina and others' measures with Fearon's measure of ethnic fragmentation. Ethnic fragmentation is significant in both these specifications. In column 5, we simultaneously include income inequality (instrumented using our continuous measure of small-holder agriculture presented in Table 1a) and the measures of ethnic and religious fragmentation developed by Alesina and others. The effect of income inequality is robust and trumps those of ethnic and religious fragmentation. This result is unchanged when we replace Alesina and others' measures with Fearon's ethnic fragmentation measure (column 6).

VII. Concluding Remarks

This paper has provided strong evidence validating Hirschman's claim that "It has long been obvious that the roots of inflation lie deep in the social and political structure in general, and in social and political conflict and conflict management in particular" (1985, p. 53). Societal divisions (especially income inequality) and democratic institutions are the key underlying determinants of inflation and related nominal pathologies.

Efforts by international institutions to introduce programs that are aimed at reducing income inequality are probably useful. However, recognizing the impact of democracy on long-term inflation performance does not necessarily

Table 6. Robustness to Alternative Sources of Conflict (Large Sample)
(Dependent variable is log of annual average percent change in nominal parallel exchange rate)

	(1)	(2)	(3)	(4)	(5)	(6)
Openness	-0.125 (0.47)	-0.061 (0.20)	-0.151 (0.50)	-0.185 (0.64)	-0.060 (0.21)	-0.075 (0.26)
Democratic political institutions	-0.657 (3.66)***	-0.667 (3.29)***	-0.604 (3.33)***	-0.598 (3.46)***	-0.715 (3.97)***	-0.642 (3.65)***
Initial inequality	1.113 (2.10)**	1.489 (2.45)**			1.012 (2.40)**	1.051 (2.64)**
Ethnic fractionalization (Alesina and others, 2003)			0.375 (2.40)**		0.089 (0.42)	
Religious fractionalization (Alesina and others, 2003)			0.137 (0.84)		0.239 (1.35)	
Ethnic fractionalization (Fearon, 2003)				0.433 (3.51)***		0.203 (1.17)
Estimation method	IV	IV	IV	IV	IV	IV
Observations	70	70	70	70	70	70

Source: Authors' calculations.

Note: Column 1 uses the first-stage equations from columns 3 and 4 of the lower panel in Table 1a, where the instrument for inequality is a dummy that takes on a value of 1 if a country is above the median in terms of the share of arable land devoted to agriculture. In column 2, the instrument for inequality is a dummy that takes a value of 1 if the country was a sugar producer circa 1950. In columns 3 and 4, only openness is instrumented using the Frankel-Romer (1999) instrument. In columns 5 and 6, the instrument is the share of grain acreage as a share of arable land.

indicate a precise and implementable short-term policy agenda. For one, institutions tend to change very slowly and the determinants of shifts to democracy are complex and still not thoroughly understood. Furthermore, even the IMF cannot do much to influence stability-related outcomes. IMF conditionality on policies is considered intrusive enough. It would be difficult to imagine any appetite for extending conditionality to a country's fundamental political institutions.

The dilemma with respect to democracy is the same as the one that arises out of the broader view about the role of institutions in economic development. Some reforms that could fundamentally help secure better economic outcomes elude control. This discussion raises a difficult question: should the policymaking community accept that it has perhaps only a modest role in trying to alleviate nominal pathologies? To paraphrase Yeats, is it better to moderate the conviction that significant change from the outside is possible than to act with the passionate intensity that it is?

APPENDIX I

See Tables A.1–A.3.

Table A.1. Variables Description with Data Sources

Variable Name	Variable Description	Source
logexchpar	Log of annual average change in nominal parallel market exchange rate, 1960–98	Reinhart and Rogoff, 2004
lcoopen	Trade to GDP ratio, average 1960–2000	Penn World Tables 6.1 (Heston, Summers, Aten, 2002)
xconst	Constraint on the executive, average 1960 (or indep.) to 2000	Polity IV Project (Marshall and Jaggers, 2002)
xconst70	xconst for 1970 (or first nonmissing observation)	Polity IV
logfrankrom	Fitted openness (instrument for openness)	Rodrik, Subramanian, and Trebbi (2004)
logem4	Settler mortality (instrument for institutions)	Acemoglu, Johnson, and Robinson (2001)
gini_WIDER	Economic inequality, average 1960 (or indep.) to 1999	WIDER Database (UNU, 2000)
gini_WIDER_in	Economic inequality, initial value (1960, indep., or earliest available)	WIDER Database
logmgrowth	Log (money_gwt)	IFS
sin33_a	Original sin	Eichengreen, Hausmann, and Panizza (2003)
rating1	External rating	Reinhart, Rogoff, and Savastano (2003)
cyclical_index	Fiscal policy cyclicality	Kaminsky, Reinhart, and Végh (2004)
volatility	Fiscal policy volatility	Fatás and Mihov (2003)
cbto	Central bank independence	Cukierman, Webb, and Neyapti (1992)
polcon3	Measure of fragmentation of political system, average 1960 (or indep.) to 2001	Henisz (2000)
legral	Henisz: Legral_2002, average 1960 (or indep.) to 2001	Henisz (2000)
checks	Measure of fragmentation of political system, average 1975 (or indep.) to 2000	Beck and others (2001)
democ	Measure of openness of political system, average 1960 (or indep.) to 2000	Polity IV
voice	Measure of say in political system, value in 2000	Kaufman Kraay, and Mastruzzi, 2003
polariz	Measure of fragmentation of political system. Average 1975 (or indep.) to 2000	Beck and others (2001)
polity	Openness of political system, average 1960 (or indep.) to 2000	Polity IV
WoverS	Loyalty norm (measure of say in electing leader), average 1960 (or indep.) to 1999	Bueno de Mesquita and others (2003)
totgav60_99	Terms of trade (TOT, goods and services) growth	World Bank, World Development Indicators

Table A.1 (concluded)

Variable Name	Variable Description	Source
totgstdev60_9	Standard deviation of TOT growth	World Development Indicators
revcoup	Revolutions and coups	Barro and Lee (1994)
cath	Dummy for Catholic country	Rodrik, Subramanian, and Trebbi (2004)
prot	Dummy for Protestant country	Rodrik, Subramanian, and Trebbi (2004)
musl	Dummy for Muslim country	Rodrik, Subramanian, and Trebbi (2004)
lfr	Dummy for country with French legal origin	Rodrik, Subramanian, and Trebbi (2004)
lso	Dummy for country with Socialist legal origin	Rodrik, Subramanian, and Trebbi (2004)
lnrgdpch60	Initial (1960) level of per capita PPP GDP	Penn World Tables, 6.1
ethnic	Fractionalization—Ethnic	Alesina and others (2003)
religion	Fractionalization—Religion	Alesina and others (2003)
ethfrac	Ethnolinguistic fractionalization (Soviet Atlas, plus est. for missing in 1964)	Easterly and Levine (1997)
relfrac	Religious fractionalization	CIA factbook
ef	Ethnic fractionalization	Fearon (2003)
lamerica	Dummy for Latin America	n.a.
ssafrica	Dummy for sub-Saharan Africa	n.a.
nafrme	Dummy for North Africa/Middle East	n.a.
infl_cpi_log	Log of annual average inflation, 1960–2000	IFS
infl_defl_log	Log (annual inflation, GDP deflator)—average 1960 (or indep.) to 2001	IFS
exch_par_log_	Standard deviation of annual growth in nominal parallel market exchange rate	Reinhart and Rogoff (2004)
infl_cpi_log_	Log (annual inflation, CPI)—standard deviation 1960 (or indep.) to 2001	IFS
gcode5	Percentage of time (1960–98) that exchange rate regime is classified as “freely falling”	Reinhart and Rogoff (2004)

Table A.2. Summary Statistics (Large Sample)

Variable	Description	Obs.	Mean	Std. Dev.	Min	Max
logexchpar	Log of exchange rate change	70	-2.21	1.59	-5.75	2.25
lcopen_par	Openness	70	3.85	0.51	2.56	5.20
logfrankrom	Fitted openness	70	2.65	0.71	0.83	4.22
gini_WIDER_in	Initial inequality	70	44.31	10.49	23.00	67.83
xconst70	Political institutions	70	3.70	2.39	1.00	7.00
grainsarable	Instrument for inequality	70	0.20	0.27	0.00	1.72
graindummy	Instrument for inequality	70	0.50	0.50	0.00	1.00
sdummy	Instrument for inequality	70	0.31	0.47	0.00	1.00
orthex	Orthogonal to real instability	69	0.01	1.52	-3.33	4.35
lnrgdpch_i	Per capita GDP	70	7.85	0.88	5.94	9.35
xconstsd_par	Political instability	70	1.08	0.94	0.00	3.00

Table A.2 (concluded)

Variable	Description	Obs.	Mean	Std. Dev.	Min	Max
logmgrowth	Money growth	66	-1.41	0.88	-2.54	1.81
sin33_a	Original sin	54	0.89	0.23	0.00	1.00
rating1	External rating	42	10.57	4.70	4.33	19.00
cyclical_index	Fiscal policy cyclicality	57	0.13	0.23	-0.51	0.54
volatility	Fiscal policy volatility	53	1.95	0.67	0.48	3.53
cbto	Central bank independence	49	0.24	0.17	0.05	0.92
exch_par_log_sd	Variability in instability	70	0.27	0.24	0.04	1.22
gcode5	Extreme instability	68	0.11	0.16	0.00	0.62
polcon3_par	Political institutions	70	0.23	0.16	0.00	0.54
checks_par	Political institutions	70	2.68	1.33	1.00	6.79
democ_par	Political institutions	70	4.68	3.60	0.00	10.00
polity_par	Political institutions	70	1.60	6.13	-7.62	10.00
reg1	Political institutions	69	-0.51	0.39	-1.00	0.00
voice	Political institutions	70	0.32	0.87	-1.43	1.70
WoverS_par	Political institutions	70	0.64	0.24	0.13	1.00

Source: Authors' calculations.

Table A.3. List of Countries (Large Sample)

S. No.	IFS code	IBRD code	Name	S. No.	IFS code	IBRD code	Name
1	612	DZA	Algeria	36	664	KEN	Kenya
2	213	ARG	Argentina	37	544	LAO	Lao P.D.R.
3	193	AUS	Australia	38	446	LBN	Lebanon
4	218	BOL	Bolivia	39	674	MDG	Madagascar
5	223	BRA	Brazil	40	676	MWI	Malawi
6	918	BGR	Bulgaria	41	548	MYS	Malaysia
7	156	CAN	Canada	42	684	MUS	Mauritius
8	228	CHL	Chile	43	273	MEX	Mexico
9	924	CHN	China	44	686	MAR	Morocco
10	233	COL	Colombia	45	558	NPL	Nepal
11	238	CRI	Costa Rica	46	196	NZL	New Zealand
12	423	CYP	Cyprus	47	278	NIC	Nicaragua
13	128	DNK	Denmark	48	694	NGA	Nigeria
14	243	DOM	Dominican Rep.	49	142	NOR	Norway
15	248	ECU	Ecuador	50	564	PAK	Pakistan
16	469	EGY	Egypt	51	288	PRY	Paraguay
17	253	SLV	El Salvador	52	293	PER	Peru
18	644	ETH	Ethiopia	53	566	PHL	Philippines
19	172	FIN	Finland	54	964	POL	Poland
20	132	FRA	France	55	182	PRT	Portugal
21	652	GHA	Ghana	56	968	ROM	Romania
22	174	GRC	Greece	57	724	SLE	Sierra Leone
23	258	GTM	Guatemala	58	199	ZAF	South Africa
24	656	GIN	Guinea	59	542	KOR	Korea, Rep. of

Table A.3 (concluded)

IFS				IFS			
S. No.	code	IBRD code	Name	S. No.	code	IBRD code	Name
25	336	GUY	Guyana	60	184	ESP	Spain
26	268	HND	Honduras	61	524	LKA	Sri Lanka
27	944	HUN	Hungary	62	144	SWE	Sweden
28	534	IND	India	63	738	TZA	Tanzania
29	536	IDN	Indonesia	64	578	THA	Thailand
30	429	IRN	Iran, I. R. of	65	744	TUN	Tunisia
31	178	IRL	Ireland	66	186	TUR	Turkey
32	436	ISR	Israel	67	746	UGA	Uganda
33	136	ITA	Italy	68	112	GBR	United Kingdom
34	343	JAM	Jamaica	69	298	URY	Uruguay
35	439	JOR	Jordan	70	299	VEN	Venezuela

Source: Authors calculations.

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