The Political Economy of Nominal Macroeconomic Pathologies

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Abstract

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 four-fold. A similar increase in societal divisions increases inflation more than two-fold. 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/or their proximate policy 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/or undemocratic political institutions.

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Why are some countries more prone to inflation and other nominal pathologies than others? It is surprising that while 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, 1998, Acemoglu et. al, 2001, Rodrik et. al., 2004), in growth (Barro and Martin, 1995) and the instability of growth (Rodrik, 1999; Acemoglu et. al. 2003a, Fatás and Mihov, 2003, Quinn and Woolley 2001)—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 3167-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 instruments for political institutions developed by Acemoglu, Johnson, and Robinson (2001), and for trade openness developed by Frankel and Romer (1999), we conduct a causal analysis of the determinants of inflation and other nominal pathologies over the long term.

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

The structure of the paper is as follows. We begin Section I by drawing a distinction between the more conventional approach of attributing nominal outcomes to policy variables and our deep determinants-based approach. We then precisely locate our contributions to the literature on nominal outcomes, and summarize our main findings. In Section II we lay out our hypothesized causal relationships between three deep determinants—societal divisions democracy, and openness—and nominal outcomes, providing some illustrative examples. Section III describes our empirical strategy. In Section IV, we address some issues of measurement and estimation. In Section V, we present our core results and the implied causal relationships. Section VI describes the robustness checks, and Section VII concludes.

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, the former First Deputy Managing Director of the IMF, put 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 ....." (Stan Fischer, 2001). [3]

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[3] Hirschman 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

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."

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 et. al. (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

have been developed that lend themselves to ever further elaboration, some empirical testing, and—most important—the formulation of policy advice." (Hirschman, 1986, 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.

spending, afforded by discretion in fiscal policy, causes instability. Eichengreen et. al. (2003) implicitly make a case for another deep determinant—original sin—some underlying pathology that makes it difficult for developing countries to borrow in their own currency. Reinhart et. al. (2004) 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 and/or experience nominal pathologies and not others. For instance, if monetary or fiscal policy causes prices what in turn causes monetary or fiscal policy and hence instability? Similarly, if the lack of central bank independence causes 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 which have an impact on nominal macroeconomic outcomes such as inflation. We then use the instrumental variables technique to examine the effects of deep determinants on nominal instability in a cross national context.[5] Aside from

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

[5] We 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

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), while Cukierman, Webb, and Neyapti (1992) are concerned with the impact of central bank independence.[6]

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), Almeida and Ferreira (2002), Quinn and Woolley (2002), and Acemoglu et al. (2003a)). Rodrik (1999), for example, examines what happens to growth rates in response to shocks. Acemoglu et. al. (2003a) 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 versus 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 (as captured by income inequality) and democratic political institutions and long-

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.

term inflation. While the result for societal divisions is interesting, the result for democracy

is probably the more surprising given the widespread attention accorded to the effects of

populist democracy on inflation, the poor responses of many democracies to oil shocks, and

the absence of a robust relationship between democracy and growth. From a substantive

perspective a one standard deviation increase in instrumented inequality (roughly the move

from France to the Dominican Republic) leads to a more than two fold increase in inflation.

Similarly, a one standard deviation increase in instrumented democracy (roughly the move

from Uganda to Chile) leads to a 3.6-fold decline in inflation. The coefficients on

instrumented inequality and democracy are significant at the 1 percent level, and our core

results are also robust to several 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.

A. Societal Divisions and Distributive Conflicts

There is a long intellectual tradition going back to Marx and Kalecki, and more

recently to Rowthorn (1979), Lindberg and Maier (1986), Hirschman (1985), Dornbusch and

Edwards (1990) and Sachs (1989), which traces nominal pathologies such as inflation to

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[6] Desai et. al. (2003) study the effects of democracy and income inequality on inflation but, do not address the endogeneity of inequality and institutions to poor macroeconomic performance.

societal divisions and associated distributive conflicts between societal groups.[7]   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 that would have been possible without inflation. This inflationary financing of an investment boom is made possible by implicitly depressing private consumption (consequent upon the real wage decline that accompanies inflation) and thereby increasing savings.

Second, inflation is an instrument par excellence for redistributing wealth: for example, from creditors to debtors and away from those that hold money and other assets (unskilled human capital) that cannot be hedged against inflation. For example, in Chile in the 1870s, land owners were accused of orchestrating inflation to permit them to repay their loans in depreciated paper money.

Third, while Marxian analyses of inflation tend to stress the conflict between wage-earners and capitalists, in many developing countries, particularly in Latin America and Africa, the fissures run as much between sectors as classes. The cleavage is often between urban wage earners employed in non-tradables and those that derive income from resource-

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[7] Even 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) and the "deeper" social causes in a seminar (Seldon, 1975).

intensive export sectors.[8] Any loosening of monetary and fiscal policies has inevitable and intended redistributional effects. Often fiscal expansion takes the form of wage increases granted to public sector employees in the urban sector. The rise in urban real wages consequent upon these policies squeezes profits in the primary sector. Macroeconomic expansion thus has distributional intent and consequences. Occasionally, inflation through selective credit expansion serves to favor some industries over others (in Brazil in the mid-1960s, this was deployed to favor the automobile sector at the expense of basic and consumer goods).[9]

A fourth 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. Sachs' (1989) description of the Latin American experience is on target: "This particular type of overly expansionary macroeconomic policies which lead to high inflation and severe balance of payments crisis, has been repeated so often, and with such common characteristics, that it plainly reveals the

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[8] Bates (1981) provides an insightful analysis of these fissures in Africa.

[9] It should be stressed, however, that the identity of actors engaged in struggle for the size of the economic pie is not time invariant. Hirschman, for example, discusses how the industrial bourgeoisie in Argentina, tend to make common cause with the urban masses during a recession in demanding expansionary economic policies. But when the resulting squeeze on exportables (which is also the wage good) leads to difficulty in importing basic and intermediate inputs, the industrialists distance themselves from the wage demands of the urban masses.

linkages from social conflict to poor economic performance." Thus, borrowing and indebtedness are simply another manifestation of nominal instability and hence related to the same underlying causes.

Two historical examples serve to illustrate some of these effects. In Nigeria, for example, the windfalls from oil prices were used for explicitly redistributional purposes: in the aftermath of the oil shocks in the 1970s, the Nigerian rulers, who were predominantly from the poorer, northern part of the country used the oil revenues to finance a massive expansion of the civil service staffed by northerners (Bevan et. al., 1999). The subsequent decline in revenues led to borrowing by the rulers and to subsequent macroeconomic instability. The parallel market exchange rate which *appreciated* on average by 0.4 percent in the 1970s depreciated on average by nearly 43 percent in the 1980s. Similarly, in Ghana, inflation was part of the arsenal of policy tools deployed by the ruling elite under Nkrumah and later under Rawlings, to redistribute income.

B. Democratic Political Institutions and Distributive Conflicts

Independent of the degree of societal division, the presence/absence of democratic political institutions can affect the extent of distributive conflict in a society. The literature has long emphasized how democracies are vulnerable to populist impulses on the part of its leaders. For instance, the literature on Latin America indicates that democracy can give rise to a competitive populism and demand for public goods which, together with coordination problems, lead to a spiral of spending and inflation in the short term (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 et. al. (2003a) is similar and is framed in terms of constraints on the executive helping mitigate appropriation of wealth by political executives toward themselves or their preferred groups. The arguments of both of these papers imply that we should also expect less by way of distributionally motivated efforts to manipulate nominal variables where there are relatively high checks on the power of politicians, i.e. in democracies.

While the above papers focus on the effect of democratic checks and balances, there are also arguments that high accountability, another aspect of democracy, has the effect of checking distributional grabs by those in power. To support such a claim political scientists use the concept of the winning coalition, the group whose support is essential for a chief executive to survive in office. Democracies are known to be associated with having relatively large winning coalitions. Bueno de Mesquita et al. (2003) have formally showed that as the size of the winning coalition increases it becomes increasingly inefficient for the chief executive to focus on diverting resources to the winning coalition to the exclusion of other members of society. The key assumption here is that politicians seek to maximize their

probability of political survival. Politicians allocate their resources between goods that can exclusively be consumed by members of the winning coalition (private goods) and goods that serve the public at large (public goods) with the goal of maximizing this probability. As the winning coalition becomes larger, the amount of private goods received by each member of the winning coalition becomes smaller, rendering private goods provision a less and less efficient way of ensuring political survival. Consequently, as the size of the winning coalition increases, the chief executive focuses more on providing public goods (such as low inflation) while limiting attempts to corner private goods for political insiders.

Likewise Persson et al. (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. The Persson et. al. (1997) argument implies that measures of division of power (on which democracies get high points) should be associated with less distributive grabs for resources by the party in power and thus less inflation.

All these arguments suggest that while democracies may face short term problems on account of populism or legislative gridlock, the mechanisms of accountability and checks and balances will eventually assert themselves resulting in lower inflation over the long term.

As far examples are concerned, Africa has been heavily populated with authoritarian regimes for much of 1960-2000. Inflation has correspondingly been relatively high (62 percent over 1960-2000). In contrast, India has been a democracy for all but a brief period, and the outcome has been a remarkably low inflation rate. The basic time series evidence is also suggestive. Between the 1980s and 1990s, Africa's mean rating on the

democracy index went up from 1.5 to 2.8[10] while the average annual rate of currency

depreciation declined from nearly 50 percent to 17 percent.

C. Openness and Redistributive Conflicts

In light of our definition of deep determinants, it is plausible that openness is another

deep determinant of inflation.[11] Romer (1993), and most recently Rogoff (2003), have argued

that openness affects not just price levels but the rate of inflation via the money supply

channel. In Romer, the impact occurs via unanticipated changes in the exchange rate. An

unanticipated monetary expansion leads to real exchange rate depreciation, which has a

greater impact on prices in more open economies. In the absence of pre-commitment to

monetary targets, openness acts as the disciplining device on the monetary authorities.

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. To quote Rogoff: "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 falls." Thus, openness not only affects the level of prices but the equilibrium

inflation rate.

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[10] The democracy index is measured on a 0–10 scale with higher values indicating greater
democracy.

[11] There 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 et. al., 2003, and the references cited therein).

Openness also renders product markets more competitive. With greater price flexibility, the impact of monetary policy on the real economy becomes less potent. Thus, the lower gains from unanticipated inflation make the monetary authorities' commitment to price stability more credible.

At first blush, the Romer and Rogoff explanations of openness smack of social welfare planners optimizing some objective function that has no distributional elements. But, there is a body of literature (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 and Rogoff explanations is that openness simply raises the costs to the elites that determine monetary policies of attempting to redistribute wealth toward themselves through inflation. In this view, openness is an economic constraint on elites and is part of the broader set of institutions that determines macroeconomic outcomes.

II. 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-run 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 and establish that these 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 Vegh, 2004), central bank independence

(Cukierman, Webb and Neyapti, 1992), original sin (Eichengreen, Hausmann, and Panizza,

2003), and debt intolerance (Reinhart, Rogoff, and Savastano, 2003).

In this paper, since we are concerned with long-run effects, 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:

$$E_i = \phi + \alpha\, C_i + \beta\, I_i + \gamma O_i + \varepsilon_i \tag{1}$$

where E_i is a 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.[12] Throughout the paper, we will be interested in

the size, sign, and significance 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.[13]

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

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

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:

$$M_i = \varpi + \rho\, C_i + \sigma\, I_i + \kappa\, O_i + \mu_i \tag{2}$$

where M captures plausible proximate causes of inflation.

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

III.   MEASUREMENT AND ESTIMATION ISSUES

A number of measurement and estimation issues arise in this study to which we now turn.

A.   Measuring nominal instability, openness, and societal divisions

First, how should nominal macroeconomic pathologies be measured or proxied? The most obvious way, of course, is inflation. While we do conduct regressions using inflation (as conventionally measured) in our robustness checks, we choose to use a less commonly used measure of inflation 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 post-war 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. Prices may hence 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. Figures 1A and 1B present the performance of the different countries (grouped by regions) on our core measure of nominal instability.

Second, any measure of inflation should reflect problems stemming from debt accumulation, rescheduling or accumulation of arrears, and other external pathologies. As argued earlier these are, and also reflect, macroeconomic disequilibria. From this perspective, the market or parallel exchange rate is better suited to capturing these pathologies than conventional measures of prices.[14] Nevertheless, to ensure that our results are not driven by our measure, we show that alternative measures of inflation based on consumer prices and GDP deflators also yield very similar results (see the discussion below).[15] 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.[16]

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

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[14] Of course, if purchasing power parity holds, exchange rate changes should manifest themselves in domestic price changes; but insofar as they do not, the use of the exchange rate measure leads to the more general specification.

[15] In our small sample, the simple correlation between exchange rate and inflation (cpi) measures is 0.94.

[16] The Reinhart and Rogoff (2004) data on parallel market exchange rates does not cover the entire post-war period for all countries. We use countries for which at least 10 years of data are available.

inequality, measured as the Gini coefficient of income inequality, and we show that this variety of fragmentation trumps the others in its effects on the dependent variable.

B.   Estimation Method: OLS, IV, Instrumentation, and Sample

The parameters of interest in equation (1) can most simply be estimated using ordinary least squares. 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. This reverse causation is accurately captured in the statement that Keynes famously (but erroneously as it turns out) attributed to Lenin that there was not better way to revolutionize a society than to debauch its currency. That high levels of inflation have had an impact on political events is illustrated by as the seizure of power by Hitler, and the changes in regimes in Brazil in 1964, Ghana and Indonesia in 1966, Chile in 1973 and Argentina in 1975.

Similarly, if societal division is measured by economic inequality, inflation clearly affects it because of its well-known redistributive effects discussed earlier. High inflation, for example, distributes income away from poor to rich people, who have better access to instruments to hedge against inflation. And in a long cross-section context such as ours, this reverse causality could potentially be important.

Inflation can also affect trade openness through a variety of channels. Most obviously, inflation leads to a real depreciation of the currency and via a number of different channels can reduce the amount of a country's trade.

Measurement error afflicts in particular the political variable because available measures only imperfectly capture the functions that political institutions are meant to serve. For instance, an accurate measure of democracy would both capture checks on the power of the executive as well as accountability/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.

To address these issues we resort to a two-stage least squares 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 Engermann and Sokoloff (1998) that economic inequality is more pervasive in countries growing 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/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 et. al. (2001). For trade openness, we use the Frankel and Romer (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 et al. (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. Our strategy to address potential violations of the exclusion restriction is essentially 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 proxying for the disease environment which could be persistent and could thereby affect current health conditions and current income. While 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 arises from the instrumentation strategy that we deploy. Our core results are based on a sample of 70 countries for which we use the FR instrument for openness and the grain cultivation based instrument for inequality. However, since the sample includes countries which are not former colonies and for which colonial settler mortality data is thus not available, we are unable to instrument for democracy in this sample and use the initial period value of the democracy measure to address endogeneity concerns. 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 instrument. In principle, this sub-sample of 48 is large enough to

warrant inference. However, we do not get very good first-stage results for inequality in this sub-sample. Hence for this sub-sample, 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, while in the sub-sample 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. This, of course, is consistent with the presence of measurement error in the data for these variables.

IV. RESULTS

A. 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 Polity (XCONST). As Gleditsch and Ward (1997, p. 380) have found, "this variable virtually determines the democracy and autocracy score values" in Polity's ratings. Later in the paper we report robustness checks with alternative measures of democracy, and the results are unchanged. Likewise, unless specifically mentioned, our measure of inequality is from the WIDER dataset.

In column 1, we present OLS results in which the right hand side variables are average democracy, openness, and inequality over 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.

Since 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 insight that economic inequality is related to the type of agriculture: the greater the reliance on types of agriculture where ownership is widely spread, the less likely will be 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 (wheat, barley, and oats) in 1950.[17] The data are from B.R. Mitchell (1998). This is our core instrument, which is described in greater detail in the Appendix. The first stage results using this instrument are shown in the first two columns of the bottom panel 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 less the inequality) with a t-statistic of 3.57. Weak instrumentation does not appear to be a problem as 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 values for weak instruments in the presence of multiple endogenous regressors.[18] (A Cragg-Donald statistic that falls short of

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[17] Ideally, 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.

[18] It 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, 2004).

these critical values is indicative of 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 (column 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

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 instrumental

variables specification in which we instrument for openness and initial period 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.

All right hand side variables are expressed in normalized form. 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. (Figure 2A

displays the results for the core specifications.) Similarly, a one standard deviation

improvement in democracy (2.4 points in a 7 point scale) reduces inflation by about half.[19]

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

(Note that when we instrument for democracy, as we do Table 1B, the substantive effect of democracy increases.)

In the remaining columns of Table 1A, the combination of instruments and measurement of the right hand side variables is altered. In column 3, we use the predicted value of average inequality over the sample period. Democracy and inequality remain statistically significant. Our results for democracy do not change when the WIDER inequality measure is replaced by that due to 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 really picking up the effects of real instability. In order to address 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 measure of inflation purged of any contamination by real instability. Column 4 shows that the effect 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.)

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 1B we check if our results above are changed if we address the endogeneity of institutions by instrumenting for it using the settler mortality variable. Recall that in this

sub-sample of 48 former colonies, we instrument for openness and democracy and use the initial period value of inequality. In the lower panel of Table 1B, 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 which is reassuringly low (the low correlation signifies that the instruments have explanatory power that is distinct for the two endogenous regressor). In the Stock-Yogo Test, the null hypothesis of weak instrumentation is rejected.

The columns in the top panel of Table 1B 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 sub-sample.

In light of the work of Glaeser et al. (2004), one interesting question is whether we are picking up the effect of institutions or human capital. Glaeser et al. (2004) argue that settlers did not just bring institutions to low mortality environments, they also brought their education. Education, as per Glaeser et al., teaches citizens how to resolve their differences in a non-conflictual manner, which in turn promotes development. They show that when this channel is accounted for, human capital has a significant effect on development while institutions do not. It is plausible that human capital (i.e. 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. 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 by human capital, the results are qualitatively similar in the small sample, while 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.

B. 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 first look at the conventional relationship between many of the proximate determinants—policies and other determinants—and nominal stability outcomes. Columns 1 in Table 2A and 2B display the effects of the many proximate determinants of nominal exchange rate instability. These include monetary policy, lack of central bank independence (CBI), fiscal policy procyclicality (Kaminsky et. al., 2004), fiscal policy volatility (Fatás and Mihov, 2003), original sin (Eichengreen et. al., 2004), and external rating (Reinhart et. al., 2003). As expected, all of these are significantly correlated with nominal outcomes.

We then examine whether these policies/proximate causes are endogenous in the sense of being causally affected by the deep determinants. The results are depicted in the remaining columns of Table 2. It turns out that instrumented democracy and inequality are significant determinants of almost all of these proximate variables, especially in our preferred larger sample. This suggests that several policies and pathologies that affect nominal outcomes might have common origins in authoritarian political institutions and societal divisions.

V. ROBUSTNESS

A. Alternative definitions of instability

We have measured inflation in terms of the changes in the parallel market exchange rate. Tables 3A and 3B check whether our results are robust if our left hand side variable is measured differently. In columns 1 and 2 we take as the dependent variable the log of average annual inflation (CPI and GDP deflator, respectively), while in column 3 we revert to our core exchange rate-based definition of instability and use its standard deviation as the measure of instability. As may be seen the effect of democracy is robust in all the specifications while the effect of inequality only falls short when using the GDP deflator definition.[20]

B. Alternative Measures of Political Institutions

So far we have used Polity's 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 4.

We use two alternative measures to capture constraints on the chief executive; Checks (due to Beck et al. 2001) and Polcon3 (due to 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.

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[20] In results that are available in the working paper version of this paper we show many more of our results for nominal exchange rate changes that carry over to conventional definitions of inflation.

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. Polity is an alternative measure of democracy provided by the Polity IV project 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 Alvarez, Cheibub, Limongi, and Przeworski (2000) which considers democracy to be present when there has been turnover in government. Note that REG is a dummy variable. (While democracy is coded as 0 in REG, we re-label it as 1 in order to facilitate comparability of signs with other measures.)

Finally, Table 4 also includes two variables which capture aspects of democracy that are missed by the Polity measures (which are driven by constraints on the executive). These are "W" (Bueno de Mesquita et. al. 2003), which measures the size of the winning coalition, and "voice" (Kaufman et. al., 2002), which is a perception-based measure of the extent of say that the average person has in a political system. We note that while all these measures are positively correlated, the correlation is not perfect. (The correlations range from 0.8 to 0.9.)

Irrespective of the measure 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 level. 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.

C. Additional Controls

Omitted variables are a common problem in cross-section regressions. So we consider in Table 5 the possible controls that we might have left out of our 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.

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 merely be the consequence of real instability. To address this concern, we introduce two measures of real instability from Acemoglu et. al. (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 proxied by revolutions and coups and in column 6 we add the legal origin of countries as a control. Both inequality and democracy are significant in all specifications in the small sample (Table 5b) and significant in five out of six specifications in the large sample (Table 5a).

D. Samples

In Table 6 we show that our results are robust to changes in the sample. In column 1, we exclude the five highest inflation countries (Argentina, Bolivia, 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 Belsey-Kuh-Welsch test as an influential observation.

Column 3 includes regional dummies,[21] while in columns 4–6, we drop respectively Latin American, sub-Saharan African, and OECD countries from our sample. In the small sample (Table 6b) our core result relating to democracy is significant in all specifications, while inequality is significant in five out of six specifications. In the large sample (Table 6a) 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 precisely estimated is with the inclusion of all the regional dummies.

E.   Which Societal Divisions?

So far we have used income inequality as our measure of societal division. However, other forms of division could also affect our dependent variable. For instance, Alesina and Ferrara show that ethnic fragmentation has adverse effects on the provision of various public goods, and low inflation can be considered to be a public good (Alesina and Ferrara 2005). We consider several measures of fragmentation in our specifications in Table 7.

We use different measures available in the literature for ethnic and/or religious fragmentation (due to Alesina et. al., 2003, and Fearon, 2003) and study their effects on the dependent variable both together with and separately from assessing the effects of inequality. For purposes of comparison we begin by presenting two results for inequality and then proceed to the other measures of fragmentation. In column 1 of Table 7A, 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

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

and 4 in the lower panel of Table 1A). Inequality and democracy are 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 for 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 Alesina et al.'s (2003) measures 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 et al.'s 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 et al.. The effect of income inequality is robust (both statistically and in terms of magnitude) and trumps those of ethnic and religious fragmentation. This result is unchanged when we replace Alesina et al.'s measures with Fearon's ethnic fragmentation measure (column 6).

The small sample (Table 7B) shows similarly strong results for inequality (as well as instrumented democracy), while ethnic fragmentation is not significant even when inequality is excluded as a control.

VI. 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." Societal divisions (especially income inequality) and democratic institutions are the key underlying determinants of inflation and related nominal pathologies.

Our results offer support for concerted long term efforts by international institutions to introduce programs that are aimed at reducing income inequality. However, a recognition of the impact of democracy on long term inflation performance does not necessarily 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, as far as the international financial institutions are concerned, even the IMF cannot do much to increase its influence over stability 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 can fundamentally help secure better economic outcomes elude control. All of this raises the difficult question: should the policy 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?

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Table 1a. Deep Determinants of Nominal Macroeconomic Outcomes: Core Specifications (Large Sample) 1/
(Panel A: Second Stage Results)

	(1)	(2)	(3)	(4)	(5)	(6)
Trade openness	-0.285	-0.104	-0.185	-0.276	-0.100	-0.047
	(1.39)	(0.35)	(0.68)	(0.99)	(0.33)	(0.16)
Democratic political institutions	-0.629	-0.660	-0.384	-0.529	-0.627	-0.474
	(3.60)***	(3.59)***	(2.16)**	(3.08)***	(3.09)***	(2.18)**
Initial Inequality	0.327	1.235	1.111	1.189	1.173	1.282
	(2.04)**	(3.09)***	(3.62)***	(3.56)***	(3.32)***	(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

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. Inequality is measured according to the Gini index for which the data are from WIDER; 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 FR (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 Penn World Tables, 6.1. Instability of political institutions is measured as the standard deviation of the index of constraint on the executive. Throughout the paper, robust t statistics are in parentheses; and *, **, and *** denote significance at the 10, 5, and 1 percent, respectively.

Table 1a. Deep Determinants of Nominal Macroeconomic Outcomes: Core Specifications (Large Sample)
(Panel B. First Stage Regression Results)

Dependent variable	Openness	Initial Inequality	Openness	Initial Inequality
Democratic political institutions	0.191	0.020	0.200	0.034
	(2.70)***	(0.20)	(2.86)***	(0.32)
Openness instrument	0.747	0.029	0.729	-0.071
	(8.66)***	(0.23)	(9.04)***	(0.58)
Inequality instrument (share of arable land devoted to grain production)	-0.4	-1.4		
	(1.32)	(3.57)***		
Inequality instrument (dummy = 1 if share of arable land is above the median value in the sample)			-0.279	-0.695
			(1.99)*	(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

The first two columns correspond to the second stage equation in column 2 in Panel A above. 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 Appendix 2. Robust t statistics in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent, respectively.

Table 1b. Deep Determinants of Nominal Macroeconomic Outcomes: Core Specifications (Small Sample) 1/
(Panel A: Second Stage Results)

	(1)	(2)	(3)	(4)	(5)	(6)
Trade openness	-0.399	-0.607	-0.608	-0.742	-0.564	-0.562
	(1.85)*	(2.12)**	(2.07)**	(2.66)**	(1.38)	(1.90)*
Democratic political institutions	-0.625	-1.354	-1.269	-1.109	-2.490	-1.266
	(2.85)***	(4.42)***	(4.46)***	(3.67)***	(2.81)***	(3.26)***
Initial inequality	0.434	0.676	0.421	0.752	0.712	0.630
	(2.01)*	(2.77)***	(1.77)*	(3.20)***	(2.45)**	(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

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 AJR (2001); and openness is instrumented by fitted openness from FR (1999). Inequality is measured according to the Gini index for which the data are from WIDER; 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 Penn World Tables, 6.1. Instability of political institutions is measured as the standard deviation of the index of constraint on the executive. Robust t statistics in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent, respectively.

Table 1b. Deep Determinants of Nominal Macroeconomic Outcomes: Core Specifications (Small Sample)
(Panel B. First Stage Regression Results)

Dependent variable	(1) Openness	(2) Dem. institutions
Initial Inequality	0.162	0.245
	(1.45)	(2.01)*
Openness instrument (predicted openness)	0.871	0.008
	(6.74)***	(0.06)
Instrument for institutions (settler mortality)	-0.228	-0.497
	(2.07)**	(4.15)***
R-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	

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

Table 2a. How Deep are the Deep Determinants? Large Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	Exchange rate inflation	Log money growth	Original sin	External rating	Fiscal policy cyclicality	Fiscal policy volatility	CBI
Log money growth	1.569 (14.09)***						
Original sin	2.629 (4.21)***						
External rating	-0.236 (6.21)***						
Fiscal policy cyclicality	4.463 (6.43)***						
Fiscal policy volatility	1.845 (7.65)***						
Central bank independence (CBI)	6.928 (7.33)***						
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)
Estimation method	OLS	IV	IV	IV	IV	IV	IV
Observations	n.a.	66	54	42	57	53	49

Table 2b. How Deep are the Deep Determinants? Small Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	Exchange rate inflation	Log money growth	Original sin	External rating	Fiscal policy cyclicality	Fiscal policy volatility	CBI
Log money growth	1.384 (13.48)***						
Original sin	3.960 (3.11)***						
External rating	-0.224 (3.16)***						
Fiscal policy cyclicality	3.984 (3.56)***						
Fiscal policy volatility	1.560 (3.94)***						
Central bank independence (CBI)	5.920 (6.26)***						
Openness		-0.401 (1.98)*	0.052 (0.94)	-0.916 (0.52)	0.018 (0.39)	0.079 (0.54)	-0.033 (0.38)
Democratic political institutions		-0.290 (1.68)	-0.206 (3.79)***	4.685 (2.95)***	-0.154 (3.28)***	-0.666 (6.59)***	-0.043 (1.03)
Initial Inequality		0.282 (1.68)*	0.023 (0.74)	-1.568 (1.46)	0.015 (0.44)	0.135 (1.11)	-0.009 (0.21)
Estimation method	OLS	IV	IV	IV	IV	IV	IV
Observations	n.a.	48	31	21	43	39	27

In column 1, the coefficients are based on bivariate regressions of the dependent variable on each of the variables shown. The instruments for the variables in Tables 2a and 2b correspond, respectively, to those in Tables 1A and 1B. Original sin, measured as securities issued in home currency as a share of total securities issued, is from Eichengreen et. al. (2003). External rating by institutional investors is from Reinhart et. al. (2003). The index of procyclicality of fiscal policy, due to Kaminsky et. al. (2004), combines two measures of correlations between real government expenditure and inflation tax on the one hand and real GDP on the other and a measure of the difference between real government expenditure in "good" and "bad times." Fiscal policy volatility is from Fatas and Mihov (2003). Central bank independence, which is measured in terms of the turnover of the head of the institutions, is from Cukierman et. al. (1992).

Table 3a. Robustness to Alternative Nominal Outcomes (Large Sample)

Dependent variable	(1) Inflation (CPI)	(2) Inflation (GDP defl.)	(3) Variability in instability
Openness	-0.118	-0.091	-0.018
	(0.47)	(0.37)	(0.39)
Democratic political institutions	-0.462	-0.355	-0.094
	(3.24)***	(2.61)**	(3.35)***
Initial Inequality	0.495	0.396	0.118
	(1.94)*	(1.52)	(1.99)*
Estimation method	IV	IV	IV
Observations	68	70	70

Table 3b. Robustness to Alternative Nominal Outcomes (Small Sample)

Dependent variable	(1) Inflation (CPI)	(2) Inflation (GDP defl.)	(3) Variability in instability
Openness	-0.635	-0.636	-0.101
	(2.39)**	(2.49)**	(2.13)**
Democratic political institutions	-0.724	-0.603	-0.178
	(2.91)***	(2.49)**	(4.04)***
Initial Inequality	0.589	0.345	0.097
	(2.74)***	(1.46)	(2.48)**
Estimation method	IV	IV	IV
Observations	47	48	48

The definitions of the dependent variables are as follows: columns 1 and 2: the log of the annual average change in inflation in the CPI and GDP deflators, respectively; column 3: the standard deviation of the annual average change in the nominal parallel market exchange rate. The instruments for the variables in Tables 3a and 3b correspond, respectively, to those in Tables 1A and 1B.

Table 4a. 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	-0.252	-0.156	-0.095	-0.194	-0.176	-0.142
	(0.50)	(0.80)	(0.54)	(0.32)	(0.61)	(0.59)	(0.50)
Initial Inequality	1.495	1.115	1.130	1.177	1.433	0.986	1.171
	(3.44)***	(3.09)***	(3.05)***	(2.99)***	(3.43)***	(2.92)***	(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

Table 4b. Robustness to Alternative Definitions of Political Institutions (Small Sample)
(Dependent variable is log of annual average percent change in nominal parallel exchange rate)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Openness	-0.594	-1.182	-0.557	-0.500	-0.778	-0.754	-0.344
	(2.02)**	(2.45)**	(1.80)*	(1.64)	(1.79)*	(1.87)*	(1.18)
Initial Inequality	0.620	0.739	0.663	0.669	0.649	0.656	0.585
	(2.40)**	(2.19)**	(2.57)**	(2.57)**	(2.03)**	(2.13)**	(2.66)**
Polcon3	-1.464						
	(4.11)***						
Checks		-2.003					
		(2.95)***					
Democ			-1.376				
			(4.37)***				
Polity				-1.467			
				(4.31)***			
Reg					-1.867		
					(2.89)***		
Voice						-1.492	
						(3.52)***	
W							-1.135
							(4.20)***
Estimation method	IV	IV	IV	IV	IV	IV	IV
Observations	48	48	48	48	48	48	47

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 Alvarez et. al. (2000) which is 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 survive in office (scale 0 to 1). The instruments for the variables in Tables 4a and 4b correspond, respectively, to those in Tables 1A (columns 1 and 2 of the lower panel) and 1B.

Table 5a. 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	-0.582	-0.220	-0.141	-0.310	-0.146
	(1.43)	(1.65)	(0.73)	(0.51)	(1.05)	(0.45)
Democratic political institutions	-0.250	-0.288	-0.634	-0.430	-0.390	-0.540
	(1.24)	(1.82)*	(3.52)***	(3.11)***	(2.12)**	(2.60)**
Initial Inequality	1.233	1.098	1.193	0.525	1.147	1.441
	(2.09)**	(2.12)**	(2.48)**	(1.28)	(3.25)***	(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 & 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

Table 5b. Robustness to Covariates (Small Sample)

(Dependent variable is log of annual average percent change in nominal parallel exchange rate)

	(1)	(2)	(3)	(4)	(5)	(6)
Openness	-0.650	-0.739	-0.693	-0.793	-0.620	-0.660
	(2.45)**	(2.81)***	(2.05)**	(1.96)*	(2.18)**	(2.29)**
Democratic political institutions	-1.317	-1.035	-1.322	-1.591	-1.229	-1.302
	(2.45)**	(2.29)**	(4.05)***	(3.02)***	(3.19)***	(4.24)***
Initial Inequality	0.742	0.663	0.792	0.821	0.809	0.814
	(2.38)**	(2.65)**	(3.07)***	(3.02)***	(3.26)***	(3.34)***
Standard deviation of real growth	0.035					
	(0.13)					
Worst output drop		0.073				
		(1.32)				
Terms of Trade (TOT) Growth			0.228			
			(0.94)			
Standard Deviation of TOT Growth				-0.315		
				(0.54)		
Revolutions & Coups					0.146	
					(0.52)	
French Legal Origin						0.069
						(0.30)
Socialist Legal Origin						0.640
						(5.37)***
Estimation method	IV	IV	IV	IV	IV	IV
Observations	42	42	46	46	46	48

The worst output drop between any two years over the period 1970-97 is from Acemoglu et. al. (2003). The legal origin variables are dummies. The instruments for the variables in Tables 5a and 5b correspond, respectively, to those in Tables 1A (columns 1 and 2 of the lower panel) and 1B.

Table 6a. 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	-0.107	-0.126	0.163	-0.174	-0.170
	(0.14)	(0.36)	(0.41)	(0.40)	(0.55)	(0.57)
Democratic political institutions	-0.511	-0.664	-0.573	-0.630	-0.655	-0.361
	(3.03)***	(3.53)***	(3.04)***	(2.96)***	(3.10)***	(1.66)
Initial Inequality	1.123	1.241	0.689	1.494	1.163	0.946
	(3.04)***	(3.07)***	(1.21)	(2.40)**	(2.71)***	(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

Table 6b. Robustness to Regional Dummies, Influential and Extreme Observations (Small 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	-0.378	-0.660	-0.430	-0.598	-0.839	-0.659
	(1.21)	(2.28)**	(1.57)	(1.04)	(2.12)**	(2.32)**
Democratic political institutions	-1.339	-1.479	-1.620	-1.426	-2.372	-1.287
	(4.94)***	(4.35)***	(2.69)**	(4.11)***	(3.82)***	(2.50)**
Initial Inequality	0.485	0.725	0.470	0.331	0.860	0.704
	(1.90)*	(2.78)***	(1.48)	(0.83)	(2.29)**	(2.29)**
Latin America dummy			0.683			
			(0.99)			
Sub-Saharan Africa dummy			-0.638			
			(0.67)			
North Africa/Middle East dummy			-2.801			
			(2.37)**			
Estimation method	IV	IV	IV	IV	IV	IV
Observations	43	47	48	29	34	44

In column 1, 5 of the highest instability observations (Argentina, Bolivia, Brazil, Nicaragua, and Peru) are omitted. In column 2, the Belsey-Kuh test for influential observations is applied which leads to the omission of Nigeria 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 Tables 6a and 6b correspond, respectively, to those in Tables 1A (columns 1 and 2 of the lower panel) and 1B.

Table 7a. 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	-0.061	-0.151	-0.185	-0.060	-0.075
	(0.47)	(0.20)	(0.50)	(0.64)	(0.21)	(0.26)
Democratic political institutions	-0.657	-0.667	-0.604	-0.598	-0.715	-0.642
	(3.66)***	(3.29)***	(3.33)***	(3.46)***	(3.97)***	(3.65)***
Initial Inequality	1.113	1.489			1.012	1.051
	(2.10)**	(2.45)**			(2.40)**	(2.64)**
Ethnic Fractionalization (Alesina et. al.)			0.375		0.089	
			(2.40)**		(0.42)	
Religion Fractionalization (Alesina et. al.)			0.137		0.239	
			(0.84)		(1.35)	
Ethnic Fractionalization (Fearon)				0.433		0.203
				(3.51)***		(1.17)
Estimation method	IV	IV	IV	IV	IV	IV
Observations	70	70	70	70	70	70

Table 7b. Robustness to Alternative Sources of Conflict (Small Sample)
(Dependent variable is log of annual average percent change in nominal parallel exchange rate)

	(1)	(2)	(3)	(4)
Openness	-0.604	-0.600	-0.531	-0.539
	(2.15)**	(2.11)**	(1.95)*	(1.96)*
Democratic political institutions	-1.464	-1.342	-1.260	-1.235
	(2.80)***	(3.70)***	(2.52)**	(3.37)***
Initial Inequality	0.712	0.670		
	(2.57)**	(2.70)***		
Ethnic Fractionalization (Alesina et. al.)	-0.032		0.099	
	(0.11)		(0.33)	
Religion Fractionalization (Alesina et. al.)	0.241		0.154	
	(0.80)		(0.54)	
Ethnic Fractionalization (Fearon)		0.017		0.067
		(0.07)		(0.26)
Estimation method	IV	IV	IV	IV
Observations	48	48	48	48

Column 1 in Table 7a uses the first stage equations in 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 of Table 7a 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 FR instrument. In columns 5 and 6 the instrument is the share of grain acreage as a share of arable land. In Table 7b, openness and democratic institutions are instrumented as described in the notes to Panel A of Table 1b.

Figure 1. Nominal Instability by Region[1/]
A. Large Sample
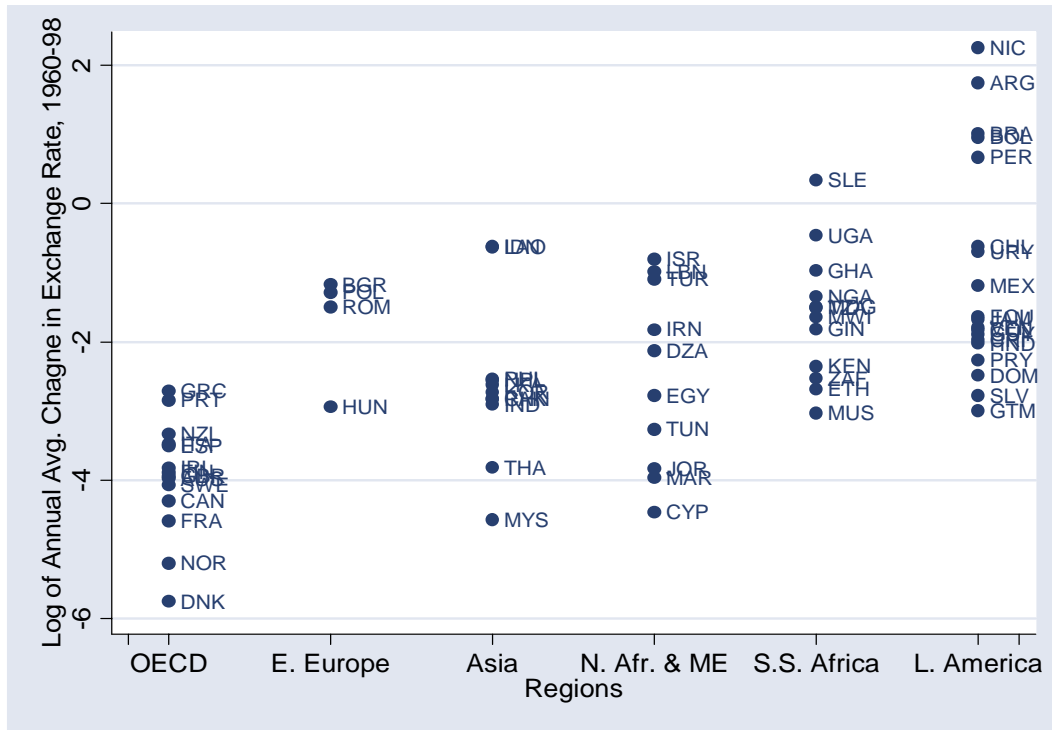
B. Small Sample
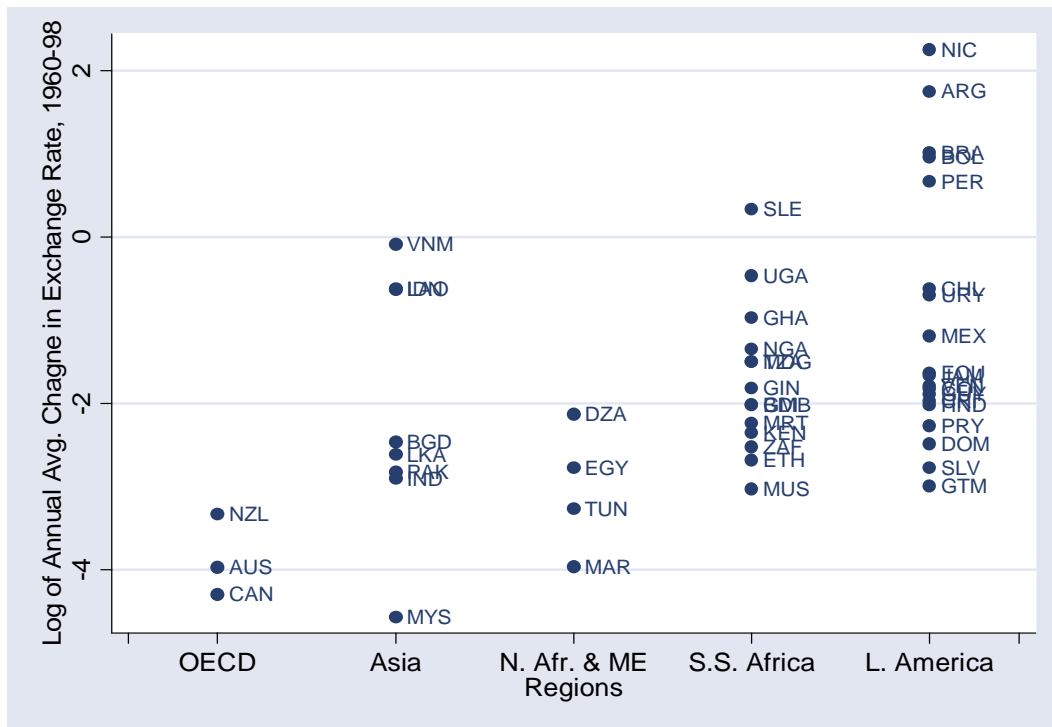
[1/] Measured as log of annual average percent change in the nominal parallel market exchange rate.

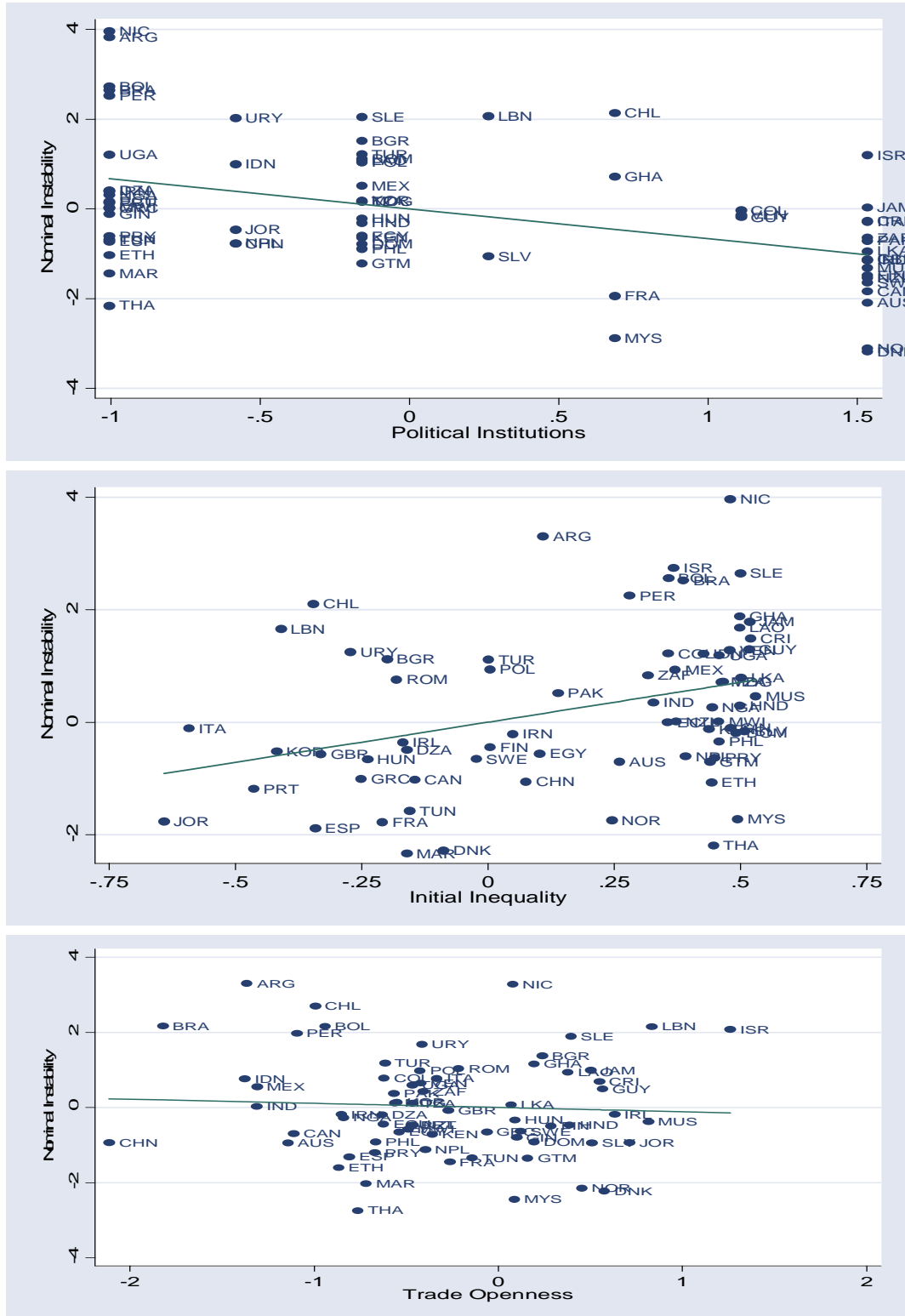Figure 2a. Deep Determinants of Nominal Outcomes (Large Sample)
(Conditional Correlations)

Appendix Table 1: Variables Description with Data Source

Variable Name	Variable Description	Source
logexchpar	Log of annual average change in nominal parallel market exchange rate, 1960-98	Reinhart and Rogoff, 2003
lcopen	Trade to GDP ratio. Average 1960-2000	Penn World Tables, 6.1
xconst	Constraint on the executive. Average 1960 (or indep) to 2000	Polity IV
xconst70	xconst for 1970 (or first non-missing observation)	Polity IV
logfrankrom	Fitted openness (instrument for openness)	Rodrik et. al., 2004
logem4	Settler mortality (instrument for institutions)	AJR, 2001
gini_WIDER	Economic inequality. Average 1960 (or indep) to 1999	WIID, 2000
gini_WIDER_in	Economic inequality. Initial Value (1960, indep, or earliest avail.)	WIID, 2000
logmgrowth	Log (money_gwt)	IFS
sin33_a	Original sin	Eichengreen et. al.,2003
rating1	External rating	Reinhart et. al.,2003
cyclical_index	Fiscal policy cyclicality	Kaminsky et. al., 2004
volatility	Fiscal policy volatility	Fatas and Mihov, 2003
cbto	Central bank independence	Cukierman et. al., 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, 2001
checks	Measure of fragmentation of political system. Average 1975 (or indep) to 2000	Beck et. al. 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 et. al., 2002
polariz	Measure of fragmentation of political system. Average 1975 (or indep) to 2000	Beck et. al. 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	de Mesquita et. al., 2003
totgav60_99	Terms of Trade (goods and services) Growth	World Development Indicators
totgstdev60_9	Standard Deviation of TOT Growth	World Development Indicators
revcoup	Revolutions & Coups	Barro and Lee, 1994
cath	Dummy for Catholic country	Rodrik et. al., 2004
prot	Dummy for Protestant country	Rodrik et. al., 2004
musl	Dummy for Muslim country	Rodrik et. al., 2004
lfr	Dummy for country with French legal origin	Rodrik et. al., 2004
lso	Dummy for country with Socialist legal origin	Rodrik et. al., 2004
lnrgdpch60	Initial (1960) level of per capita PPP GDP	Penn World Tables, 6.1
ethnic	Fractionalization - Ethnic	Alesina et. al., 2003
religion	Fractionalization - Religion	Alesina et. al., 2004
ethfrac	ethno-linguistic fractionalization (Soviet Atlas plus est's for missing in 1964)	Easterly and Levine, 1997
relfrac	religious fractionalization	CIA factbook
ef	Ethnic fractionalization	Fearon, 2002
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, 1960-00	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, 2003
infl_cpi_log_	Log (Annual Inflation, CPI) - Standard Deviation 1960 (or indep) to 2001	IFS
gcode5	Percent of time (1960-1998) that exchange rate regime is classified as "freely falling"	Reinhart and Rogoff, 2003

Appendix Table 2a: Summary Statistics (Large Sample)

Variable	Description	Observations	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
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

Appendix Table 2b: Summary Statistics (Small Sample)

Variable	Description	Observations	Mean	Std. Dev.	Min	Max
logexchpar	Log of Exchange Rate Change	48	-1.75	1.50	-4.58	2.25
lcopen_par	Openness	48	3.83	0.54	2.58	5.20
xconst_par	Political Institutions	48	3.98	1.80	1.36	7.00
logfrankrom	Fitted Openness	48	2.53	0.66	1.11	3.96
logem4	Settler Mortality	48	4.54	1.16	2.15	7.60
gini_WIDER_in	Initial Inequality	48	46.29	10.01	29.96	67.83
orthex	Orthogonal to real instability	47	0.45	1.46	-2.16	4.35
lnrgdpch_i	Per Capita GDP	48	7.58	0.83	5.94	9.35
xconstsd_par	Political Instability	48	1.03	0.93	0.00	2.76
logmgrowth	Money growth	48	-1.37	0.95	-2.29	1.81
sin33_a	Original sin	31	0.94	0.16	0.44	1.00
rating1	External rating	21	8.57	3.53	5.00	18.00
cyclical_index	Fiscal policy cyclicality	43	0.20	0.17	-0.17	0.54
volatility	Fiscal policy volatility	39	2.24	0.56	0.88	3.53
cbto	Central bank independence	27	0.29	0.20	0.09	0.92
exch_par_log_sd	Variability in Instability	48	0.32	0.26	0.04	1.22
gcode5	Extreme Instability	44	0.12	0.16	0.00	0.62
polcon3_par	Political Institutions	48	0.19	0.15	0.00	0.47
checks_par	Political Institutions	48	2.35	1.08	1.00	5.67
democ_par	Political Institutions	48	3.85	3.42	0.00	10.00
polity_par	Political Institutions	48	0.49	5.85	-7.56	10.00
reg1	Political Institutions	48	-0.60	0.38	-1.00	0.00
voice	Political Institutions	48	-0.05	0.83	-1.43	1.70
WoverS_par	Political Institutions	47	0.57	0.24	0.13	1.00

Appendix Table 3a: List of Countries (Large Sample)

S. No.	IFS Code	IBRD Code	Country Name	S. No.	IFS Code	IBRD Code	Country Name
1	612	DZA	Algeria	36	664	KEN	Kenya
2	213	ARG	Argentina	37	544	LAO	Laos
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 Republic	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	South Korea
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	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

Appendix Table 3b: List of Countries (Small Sample)

S. No.	IFS Country Code	World Bank Country Code	Country Name
1	612	DZA	Algeria
2	213	ARG	Argentina
3	193	AUS	Australia
4	513	BGD	Bangladesh
5	218	BOL	Bolivia
6	223	BRA	Brazil
7	618	BDI	Burundi
8	156	CAN	Canada
9	228	CHL	Chile
10	233	COL	Colombia
11	238	CRI	Costa Rica
12	243	DOM	Dominican Republic
13	248	ECU	Ecuador
14	469	EGY	Egypt
15	253	SLV	El Salvador
16	644	ETH	Ethiopia
17	648	GMB	Gambia
18	652	GHA	Ghana
19	258	GTM	Guatemala
20	656	GIN	Guinea
21	336	GUY	Guyana
22	268	HND	Honduras
23	534	IND	India
24	536	IDN	Indonesia
25	343	JAM	Jamaica
26	664	KEN	Kenya
27	544	LAO	Laos
28	674	MDG	Madagascar
29	548	MYS	Malaysia
30	682	MRT	Mauritania
31	684	MUS	Mauritius
32	273	MEX	Mexico
33	686	MAR	Morocco
34	196	NZL	New Zealand
35	278	NIC	Nicaragua
36	694	NGA	Nigeria
37	564	PAK	Pakistan
38	288	PRY	Paraguay
39	293	PER	Peru
40	724	SLE	Sierra Leone
41	199	ZAF	South Africa
42	524	LKA	Sri Lanka
43	738	TZA	Tanzania
44	744	TUN	Tunisia
45	746	UGA	Uganda
46	298	URY	Uruguay
47	299	VEN	Venezuela
48	582	VNM	Vietnam

Appendix. Description of Inequality Instruments

The crops data for our inequality instruments are drawn from International Historical Statistics: The Americas 1750-1993 4th edition (1998), London: MacMillan, and International Historical Statistics: Africa, Asia, and Oceania 1750-1988 3rd edition (1998), also from the same publisher. Both volumes are by B.R. Mitchell. We provide a description of the variables that we use in this paper, and the method of data construction below.

1) grainsarable- The International Historical Statistics (IHS) volumes provide annual data by country for the amount of land that is being used for wheat, barley, and oats cultivation. The grainsarable variable sums the land devoted to each of these uses in each country in 1950 (or if 1950 data is missing, data for the nearest succeeding year) and divides this sum by the amount of arable land in the country as reported by the CIA Factbook. (IHS coverage drops prior to 1950.) The countries for which we had to use data after 1950, with the year we used in parentheses, are: Bolivia (1954), China (1952), Ethiopia (1953), Ghana (1951), Guatemala (1951), Indonesia (1951), Nepal (1961), Paraguay (1951), Peru (1951), South Korea (1951), Uruguay (1951), and Tanzania (1958). Wherever cultivation data is reported we use the data unchanged. If a country does not report data for any crop from the IHS list (which includes an extensive list of crops) we consider it a missing observation. If a country reports data for some crops, but does not report any wheat, barley, or oats cultivation, we code it as a 0.

2) graindummy- We code countries at and below the median in our sample for grainsarable as 0, and countries above the median as 1.

3) sdummy- If a country reports land under sugar cultivation we code it as a 1. If a country does not report sugar cultivation, but does report data for other crops, we code it as a 0. If a country does not report cultivation of any crop we code it as a missing observation.