

# Political Institutions and Economic Performance: The Effects of Accountability and Obstacles to Policy Change

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*Numerous analyses have been conducted on how political institutions affect economic performance. In recent years the emphasis has been on a causal logic that emphasizes institutional obstacles to policy change, such as those presented by multiple veto points. This has especially been the case when it comes to the important question of how political institutions influence governments' responses to exogenous economic shocks. We make the case for a substantial broadening of focus and show that when it comes to a major type of exogenous shock, a forced exchange-rate devaluation, variations in the breadth of accountability of the chief executive are more robustly associated with the post-shock growth recovery than variations in obstacles to policy change. We first argue that the size of the winning coalition will be positively associated with growth recoveries following forced devaluations. We then use a newly developed measure of the size of the winning coalition to test our claim. Our statistical analysis is based on a study of the responses of 44 countries to forced exchange-rate devaluations in the late 1990s.*

How do political institutions affect economic performance? This is a fundamental and enduring question for political scientists and economists. The empirical literature on this question is rich and diverse but we can roughly divide it into two groups—one that emphasizes the causal role of obstacles to policy change and one that emphasizes the role of leaders' accountability for policy decisions.<sup>1</sup> The former approach focuses on how policy outcomes are influenced by the limits placed by institutions such as legislatures or coalition partners on the chief executive's ability to shift policy from the status quo. The latter approach focuses on the effects of institutionally generated variations in the accountability of the chief executive. Testing for the effects of obstacles to policy change does not preclude testing for the effects of accountability or vice versa; there is no good ex ante theoretical

reason to rule out one or the other channel of causation. Furthermore, a failure to compare explicitly the effects of these channels may result in flawed policy choices; for instance, focusing exclusively on altering the number of obstacles to policy change may be inappropriate if performance outcomes are primarily being driven by the accountability channel. Despite these concerns, there is a gap in the literature when it comes to explicitly comparing the effects of these channels on economic performance. We seek to fill this gap in an important area of economic performance, namely, developing countries' recoveries from forced exchange-rate devaluations. We demonstrate that it is variations in accountability rather than in obstacles to policy change that are robustly associated with variations in economic performance and argue that this has major policy implications.

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<sup>1</sup>Veto player analyses fall in the former category. See Keefer and Stasavage (2003) and Tsebelis (2002). Many studies of the effects of democracy such as Rodrik (1999), discussed later, also fall in this category. Bueno de Mesquita et al.'s (2003) theory of winning coalitions and selectorates falls in the latter category.

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We define a forced devaluation as a devaluation of a fixed/pegged currency which occurs either as a direct consequence of a speculative attack, or as a consequence of contagion from devaluations in proximate countries. Why do we focus on forced exchange-rate devaluations? In an era of global markets, when international capital can be easily withdrawn, such devaluations have become an increasingly common and widespread phenomenon. The recent experiences of Asia and Latin America, the effects of which were felt across the developing world, are cases in point. Such shocks often have devastating consequences. For instance, following Thailand's forced devaluation, Indonesia, hitherto considered to be a star performer, experienced an 80% devaluation of its currency and an economic contraction of nearly 15% of GDP. The impact that forced devaluations can have on countries' economic and social well-being make them eminently worthy of attention.

Forced devaluations are also of exceptional interest from an academic perspective because their consequences for economic growth are not the same across developing countries. Some developing countries quickly and effectively adjust to forced devaluations while others do not, generating a puzzle that is worthy of systematic research.

In this article we study growth recoveries from the wave of forced devaluations across the developing world in 1997–98, which occurred as a consequence of contagion from Thailand's forced devaluation of mid-1997. We find that the extent of the recovery significantly increases in the size of the group to which a chief executive is accountable.

In the next section we briefly discuss the various obstacles-based institutional arguments relating to recoveries from forced devaluations. We then describe our causal argument. In the next section we lay out our methodology for testing our claim, and we then present the results of our empirical tests and discuss the results. In the final section we summarize our findings, discuss the major policy implications, and suggest some directions for further research.

## The Literature

Our article builds on the case literature on post-devaluation responses, to which Stephan Haggard and Andrew MacIntyre are the major contributors. Both authors focus on the influence of obstacles to policy change and base their analyses on veto player theory. Despite the common theoretical base, Haggard and MacIntyre draw different implications for the relationship between obstacles to policy change and recoveries from currency crises.

On the one hand Haggard, in several contributions, argues that dispersed decision-making authority (multiple veto players) can hinder the ability of governments to undertake necessary economic reforms in response to economic shocks (Haggard 2000; Haggard and Kaufman 1992). The implication is that the presence of very few veto players is most conducive to growth recoveries from forced devaluations. On the other hand MacIntyre (2002) points out that having very few veto players renders commitments to policy reform and good governance incredible and serves as an obstacle to restoring the confidence of investors following a currency shock.<sup>2</sup> Whereas the presence of a very large number of veto players can serve as an obstacle to recovery thanks to legislative gridlock, the presence of a moderate number of veto players may serve to make commitments to good governance credible while avoiding the dangers of gridlock. MacIntyre consequently argues that an intermediate number of veto players is most conducive to restoring the confidence of investors. Thus, while Haggard's analysis implies a linear and negative relationship between obstacles to policy change and growth recoveries from currency devaluations, MacIntyre's argument implies a positive relationship between these variables as one moves from very few to a moderate number of veto players, and a negative relationship only thereafter.

The primacy given to obstacles to policy change can also be seen in some recent quantitative literature in economic growth. Rodrik (1999) examines the responses of a worldwide sample of developing and developed countries to the terms of trade shocks of the 1970s and finds that better institutions for conflict management (most notably democracy) contributed to a superior response and that social conflicts contributed to a weaker response. Rodrik's causal argument for the efficacy of democracy is that democracies embody obstacles to opportunistic grabs for resources in the face of shocks, which in turn generates incentives for cooperation between societal groups over adjustment. While this is not explicit, it does appear that the driving force in this argument is politicians' inability to shift the status quo to their ideal point in democratic environments.

The primacy given to obstacles to policy change in the recent empirical literature is puzzling because, at a theoretical level, the accountability of leaders can play a critical role in influencing responses to shocks. As long as leaders are sufficiently accountable for poor performance, they should find it worthwhile to find ways to circumvent institutional obstacles to policy change. Furthermore, as the diverse arguments described above indicate, obstacles can both hinder and facilitate responses to shocks, and it

<sup>2</sup>This view is consistent with that of North and Weingast (1989).

is hard to make an *ex ante* theoretical claim as to which effect will dominate.<sup>3</sup> The question then is, how does accountability affect recoveries from forced devaluations? We offer a causal explanation in the next section prior to empirically comparing the effects of accountability and obstacles to policy change.

## Accountability and Adjustments to Forced Devaluations

Bueno de Mesquita et al. (2003) have recently developed a rigorous formal framework for evaluating the consequences of variations in the size of the group to which a chief executive is ultimately accountable. We build on their framework below. The critical concept in Bueno de Mesquita et al.'s theory is that of the winning coalition—the section of the populace whose support is essential for the leader to survive in office. Under the assumption that leaders are driven by political survival concerns, this is the section of the population to which the leader is ultimately accountable. Bueno de Mesquita et al.'s theory is based on two critical assumptions. First, as mentioned, the goal of leaders is to survive in office. Second, in service of this goal, leaders choose the degree to which they will focus on policies that benefit society at large (public goods) versus those that exclusively benefit the winning coalition (private goods). The theory identifies the conditions under which policies that exclusively favor the interests of winning coalition members become relatively inefficient as a means of retaining power. In countries with large winning coalitions, private goods are perforce spread relatively thinly across the members of the winning coalition. It follows that if resources are diverted from private goods to public goods in such systems, each individual in the winning coalition is making a relatively small sacrifice in terms of foregone private benefits. As Bueno de Mesquita et al. formally demonstrate, the implication is that political survival is served by a higher diversion of resources to public goods provision in large coalition systems. In other words, the incentives for providing goods that benefit the public at large are increasing in the size of the winning coalition.

This theory yields implications for what we should expect in the face of forced devaluations. In the developing world the high level of foreign debt carried by bank-

<sup>3</sup>Note, also, that multiple veto player environments only produce stable, and hence, credible policy when the status quo falls into the Pareto set of the veto players' ideal points (any policy change would make at least one actor worse off). However, in a severe crisis it is possible that policy change is preferable to the status quo for each veto player.

ing sectors means that the immediate effect of a forced devaluation is generally a decline in economic growth.<sup>4</sup> Restoring growth in the medium term often demands the pursuit of reforms that build the confidence of investors, for instance those imposing limits on wasteful expenditures, increasing the transparency of stock markets, or enhancing the regulation of the banking sector. Given that growth enhancing reforms do not exclusively benefit winning-coalition members, Bueno de Mesquita et al.'s theory implies that leaders of countries with larger winning coalitions have more powerful incentives to pursue reforms that are conducive to growth recoveries following devaluations. (Winning coalition members lose less from funds being diverted from private goods to confidence building reforms where the winning coalition is large.) This logic gives us the following testable hypothesis.<sup>5</sup>

*H1: Increases in the size of the winning coalition (W) are positively associated with growth recoveries from forced exchange-rate devaluations.*

Given the theoretical relationship between accountability and responses to shocks, described above, scholars risk missing an important channel of influence between politics and economic outcomes if they focus on checks to the exclusion of the accountability generating aspects of a polity, such as the size of the chief executive's accountability group/winning coalition. This implies that measures of democracy that take inadequate account of the size of the chief executive's accountability group, which as we will show is the case with the Polity measure, may fail to capture the presence of a powerful effect for political institutions on recoveries. This yields our second hypothesis.

*H2: Increases in the level of democracy as measured by Polity are not significantly associated with recoveries from forced exchange-rate devaluations.*

Given the theoretical ambiguity surrounding the direction of the effect of obstacles to policy change on post shock recoveries described earlier, we do not expect to find conclusive evidence supporting one or the other directional hypothesis for checks on the chief executive's

<sup>4</sup>See the extensive literature on the East Asian crisis. Banking sector stress, caused by a rise in the value of foreign debt following a devaluation, affects growth by limiting the flow of funds within the economy.

<sup>5</sup>For a formal model that describes this mechanism see the working paper version of this article at <http://www.nyu.edu/cgi-bin/cgiwrap/gh375/papers.pl>.

power to shift policy from the status quo. We thus also test the following hypothesis.

*H3: Increases/decreases in checks on the chief executive's power to shift policy from the status quo neither unambiguously hinder nor aid recoveries from forced exchange-rate devaluations.*

## Methodology and Data

### Methodology

**The Sample.** An important methodological question for our analysis is how to identify the appropriate sample. We relied on press reports to identify countries which suffered forced devaluations on account of contagion from Thailand's crisis. Given that press reports emphasize forced devaluations in developing countries following the Thai crisis, we have limited our focus to such countries in our core specifications and have considered other countries only in our robustness checks. When considering countries that were **forced** to devalue it makes sense to focus on countries with fixed or pegged exchange rates. We thus only included countries that qualify as having fixed or pegged exchange rates as per the Reinhart Rogoff classification in our core specifications (Reinhart and Rogoff 2004).<sup>6</sup> In order to alleviate selection concerns we conducted robustness checks with a sample including countries which did not experience devaluations, as well as countries with floating exchange rates and developed countries, and found that this did not change our substantive results. We also included regional dummies in order to control for the possibility that contagion may have been worse in some regions (e.g., Asia) than others. Our final sample of 44 countries is listed in the appendix.

**The Dependent Variable.** Rodrik (1999) is the major recent quantitative attempt to assess the impact of politics on adjustments to economic shocks in a global sample. We use his approach to defining the dependent variable and justify this choice below. Rodrik defines the growth adjustment to a shock as the **difference** in average growth between the post- and preshock periods. (Given that his concern is with terms of trade shocks in the 1970s, his dependent variable is the average annual rate of economic growth in 1975–89 minus the rate in 1960–75.) This way of defining the dependent variable may seem unusual to political scientists who are used to conducting time-

series cross-sectional analysis with a country year as the data point. However, Rodrik's approach is considered best practice in the economic growth literature, thanks to the extreme instability and volatility of GDP.<sup>7</sup> Consistent with Rodrik's definition, our dependent variable is the average annual growth rate over 1997–2002 minus the average annual growth rate in 1990–97.<sup>8</sup> In order to ensure that our definition of the dependent variable does not create a bias in favor of countries that were poor growth performers in the preshock period we control for preshock growth rates in all our specifications.

An associated question relates to the choice of the break year. Our interest is in studying growth recoveries from the wave of forced devaluations that resulted as a consequence of contagion from Thailand's forced devaluation in mid-1997. Our exchange-rate data indicates that this wave by and large petered out by mid-1998, meaning that mid-1997–mid-1998 constitutes an appropriate break point for our analysis. To check for robustness, however, we ran several specifications in which each country was allowed to have a different break year, defined as the year in the 1990s when it experienced its most significant devaluation. This did not affect our substantive results.

**Endogeneity Concerns.** One possible concern is that the winning-coalition score is endogenous to the growth recovery. Our data indicates that only five countries underwent a change of *W* score in the post-shock period. Our results did not change when we dropped these countries, suggesting that our analysis is not seriously afflicted with endogeneity concerns.

**Controls.** We include regional dummies for Latin America, East Asia, and Sub-Saharan Africa, lagged growth for 1990–97, and per-capita GDP in 1997. The lagged growth term is included to capture convergence effects while per capita GDP and regional dummies are included to control for structural characteristics that are correlated with income levels and geographic location. In several specifications we also control for external debt in 1997, since this is the major economic explanation, aside from currency devaluations, for the poor growth performance of several developing countries in the 1997–2002 period. Note that we do not control for exchange rate movements after 1998 because these are likely to be highly endogenous to the growth recovery. (We also leave out other policy variables

<sup>7</sup>See Pritchett (2000, 235) for why this is the case.

<sup>8</sup>For those interested it is hard to find a robust correlation between any variable and growth fluctuations on a year to year basis. Our panel regressions using the country-year as a unit were not productive.

<sup>6</sup>We considered countries which had scores of 1–3 in the gcode for 1996 (the 1997 rating reflects the effects of the Thai crisis).

that would plausibly be expected to be endogenous to our political variables.)

We also include one of two proxies for latent social conflict as controls in all our specifications. The first, the index of ethno linguistic fragmentation (ELF60), measures the likelihood that any two randomly drawn individuals in a country will not be members of the same group. The source for ELF60 is Atlas Narodov Mira, obtained from Easterly and Levine (1997). As for the second, income inequality, we use the WIDER dataset compiled by the United Nations Development Programme (UNDP).<sup>9</sup> We take an average over the period 1980–89 of the Gini values for each country in order to avoid concerns of endogeneity. Since ELF data were available for more countries, we consider our specifications with this variable as constituting our core specifications and treat our specifications with inequality as robustness checks.

## Data

### Data for the Dependent Variable and Exchange Rates.

For our dependent variable we use GDP per capita data from the Global Development Network Growth Database (GDNGD).<sup>10</sup> Real GDP per capita is measured in 1985 US dollars.

Our exchange rate information comes from the IMF's International Financial Statistics (IFS).<sup>11</sup> An exchange rate shock in our tables is the percentage change in the nominal value of a country's currency against the U.S. dollar from June 1997 to June 1998—i.e.,  $(\text{Exchange Rate June 1998} - \text{Exchange Rate June 1997}) / (\text{Exchange Rate June 1997})$ . (Recall that we also conduct robustness checks with other periods.) Note that since we are considering units of foreign currency per dollar, an increase in the exchange rate constitutes a devaluation. The data for our economic controls also comes from GDNGD.

**Data for Political Institutions.** The data for the size of the winning coalition ( $W$ ) comes from Bueno de Mesquita et al.'s "Logic of Political Survival" dataset.<sup>12</sup>  $W$  is a composite index based on data from Polity IV and Banks (1996), which takes values between 0 and 1, with 1 signifying a country which has a large winning coalition.<sup>13</sup>

<sup>9</sup><http://www.wider.unu.edu/wiid/wiid-introduction.htm>.

<sup>10</sup>See the Global Development Network Growth Database Web site, <http://www.nyu.edu/fas/institute/dri/>.

<sup>11</sup><http://ifs.apdi.net/imf/ifsbrowser.aspx?branch=ROOT>.

<sup>12</sup><http://www.nyu.edu/gsas/dept/politics/data/bdm2s2/Logic.htm>.

<sup>13</sup>Note that the  $W$  measure aims to capture the variation in the size of winning coalition both across and *within* regime types. While

Specifically, the index combines  $XRCOMP$  (the competitiveness of executive recruitment),  $XROPEN$  (the openness of executive recruitment), and  $PARCOMP$  (the competitiveness of participation) from Polity IV with  $REGTYPE$  from Banks (civilian character of regime) to arrive at an overall score. Bueno de Mesquita et al. purposely exclude from their calculation of  $W$  the measure  $XCONST$  from Polity IV—a measure of the obstacles to the unilateral decision making power of the executive emanating from legislatures, councils of nobles or advisors, the military, and the judiciary. They also exclude  $DEMOC$  (democracy) and  $AUTO$  (autocracy) because  $XCONST$  is used to calculate each. Bueno de Mesquita et al. (2003, 139) point out that while the correlation between  $W$  and  $XCONST$  is .30, the correlation between Polity's democracy rating and  $XCONST$  is 0.90. The latter finding is consistent with that of Gleditsch and Ward, that in Polity's democracy measures "the recruitment and participation dimensions are . . . empirically extraneous despite their centrality in democratic theory" (1997, 361). Given the above, it makes sense that Bueno de Mesquita et al. (2003, 139) are extremely insistent in pointing out that their measure captures a different aspect of the institutional environment from that captured by Polity's democracy score or by counts of veto players.

In our robustness checks we also consider the size of the selectorate,  $S$ . We do so because one of Bueno de Mesquita et al.'s arguments is that an increase in the size of the winning coalition relative to the size of the selectorate,  $W/S$ , increases the incentives for a government to emphasize public regarding policies. This data also comes from the "Logic of Political Survival Dataset."

Our robustness checks also include the use of a variable termed  $WS:Dem Res$ . This variable aims to capture "the portion or characteristics of a country's degree of democracy or autocracy not endogenous to  $W$  or  $S$ " (Bueno de Mesquita et al. 2003, 137). The variable is created by regressing  $W$  and  $S$  on Polity IV's democracy scale and then saving the residuals.

A second set of institutional variables in our regressions are those variables that are commonly used

identifying within regime variation is reasonably straightforward for democracies, it is admittedly less so in the case of nondemocratic systems. Still, the measure seems to do a reasonable job of capturing such differences. When assessing the size of the winning coalition, a country with imperfect mass elections must clearly be distinguished from one in which the dictator merely needs to retain the support of a handful of military officials to remain in office, and thus finds it easier to ignore mass dissatisfaction (including riots) and remain in office simply by buying off this key handful. That the  $W$  measure captures this difference is apparent from observing its classification of countries in the appendix.

by scholars to capture the obstacles to policy change in a political environment. There are four widely disseminated variables that have been used to capture such obstacles: the Checks measure from the Database of Political Institutions (Beck et al. 2001), the PolconIII measure from Henisz (2002), the Polarization measure from the Database of Political Institutions (Beck et al. 2001), and the Allhouse measure also from the Database of Political Institutions. Checks and PolconIII are continuous measures aimed at capturing obstacles to policy change. The Polarization measure captures the difference in economic orientation (on a left-right scale) between the pair of veto players who differ most in their orientation. The scale ranges from 0 to 2, with 2 indicating high polarization. Allhouse is a dummy variable which takes a value of 1 when the party of the chief executive controls the legislature, and 0 when it does not.

In order to capture the effects of democracy we use the popular Polity2 measure from the Polity IV dataset, which is the difference between Polity IV's measure of democracy (scale of 0 to 10) minus its measure of autocracy (scale of 0–10). We also conducted regressions using Polity IV's democracy measure. (Since the results were substantively the same, we only present results for the first measure in the tables.)

When we consider these institutional variables we take an average of their values for each country from 1997 till the last date for which data are available. In the case of data from the Database of Political Institutions the

last date is 2000. For PolconIII it is 2001. For the “Logic of Political Survival” database it is 1999. For the Polity measure it is 2000. Since institutions, in general, do not often display dramatic changes over short intervals, we are not overly concerned about the fact that we do not have data for these variables all the way to 2002. We prefer this shortcoming to the alternative of advancing the end year of our dependent variable and leaving little time for recovery or business cycle effects to work themselves out. In order to ensure that our results are not driven by this data limitation, we went through our sample country by country and identified the few countries for which press reports suggest a change in the size of the winning coalition or regime over 2000–02. We then corrected our *W* scores for these countries upwards or downwards in accordance with the direction of change. Our results were substantively unaffected by these changes.

## Results

Our descriptive statistics are presented in Table 1. Recall that our dependent variable is the average annual growth rate over 1997–2002 minus the average annual growth rate over 1990–97, so a higher value on this measure implies a superior recovery. Table 2 shows that devaluations had a significant negative effect on our dependent variable. The result is robust to the inclusion of different measures of latent social conflict. The question that interests

**TABLE 1** Descriptive Statistics

	Mean	Std Dev.	Obs.
<b>Panel A: Growth and Control Variables</b>			
Growth 1990 to 1997	0.01	0.028	44
Growth 1997 to 2002	0.01	0.024	44
Growth Difference	0.00	0.031	44
Log(GDP per capita), 1997	7.59	0.99	44
Exchange Rate Shock	0.28	0.69	44
Ethno-linguistic fractionalization (source: Atlas Marodov Mira) - ELF60	0.52	0.28	44
Lagged Gini - Average 1980's Value (source: Wider)	44.78	8.48	34
<b>Panel B: Institutional Variables</b>			
W (winning coalition size)	0.65	0.23	44
S (selectorate size)	0.98	0.09	44
W over S	0.65	0.23	44
Polity 2 (source: Polity IV)	3.34	5.55	44
Checks (source: DPI)	3.14	2.26	44
PolconIII (source: Henisz)	0.28	0.19	44
Allhouse (source: DPI)	0.63	0.48	44

**TABLE 2 Growth Difference & Exchange Rate Shocks**

Explanatory Variable	Dependent Variable: Growth Difference - Pre & Post 1997		
	(1)	(2)	(3)
Latin America	-.007 (-0.54)	-.011 (-0.86)	.006 (0.24)
East Asia	-.015 (-0.94)	-.015 (-0.94)	-.011 (-0.46)
Sub-Saharan Africa	-.004 (-0.37)	.004 (0.38)	.014 (0.63)
Previous Growth 1990–1997	-.415 (-1.32)	-.413 (-1.29)	-.417 (-1.49)
Log 1997 GDP per capita	-.004 (-0.65)	-.006 (-0.88)	-.005 (-0.66)
Exchange Rate Shock	-.015*** (-2.89)	-.014** (-2.69)	-.015*** (-3.25)
ELF60 – Ethnic Fractionalization		-.018 (-1.21)	
Lagged Gini 1980's Average			-.001 (-1.69)
R <sup>2</sup>	.65	.66	.65
Root MSE	.020	.019	.021
Number of observations	44	44	34

Notes: T-stats produced with robust standard errors in parentheses. Significantly different from zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

us, however, is how did political institutions modify this effect? In other words, our focus is on assessing the interaction of the exchange rate shock with institutional variables capturing accountability and obstacles to policy change.

Table 3 considers our core specifications where, in addition to the economic controls, we have added ethnic fractionalization as a control proxy for latent social conflict. In each column we show the results for a regression which includes a different single political institutional variable. (In the following table we introduce multiple institutional variables in the same specification.) Note that the numbers in parentheses are *t*-statistics, and the stars are for two tailed tests. A strong case can be made for the use of one tailed tests for the measure that captures accountability (*W*), but not for the measures relating to obstacles to policy change since the theory relating to the former offers a unique directional prediction while theories relating to the latter do not, as described earlier. However, since it is obvious that the use of a one-tailed test for *W* only serves to strengthen the results for this variable, which are strong enough as it is, we stick to displaying stars for two tailed tests for all the institutional variables. Our findings with respect to each hypothesis are as follows.

H1: Row 1 in Table 3 shows the effect of an exchange-rate devaluation when the value of each institutional variable is 0. Column 1, row 1 shows that an exchange-rate devaluation has a significant negative effect when  $W = 0$  ( $t = -2.30$ ). We now draw the reader's attention to the row that displays the interaction of the institutional variable with the exchange-rate devaluation (row 3). Column 1 shows that *W* significantly modifies the effect of an exchange-rate devaluation in a positive direction ( $t = 2.09$ ). A shift from a low level of *W* to a moderately high level (slightly above .50) is sufficient to wipe out the significant negative effect of an exchange-rate devaluation on the dependent variable, i.e., results in a complete recovery.<sup>14</sup>

H2: Using the same approach we can see from row 1, column 2 that at a Polity score of 0 an exchange-rate devaluation has a significant negative effect ( $t = -2.50$ ). However, unlike for *W*

<sup>14</sup>We used the procedure recommended by Brambor, Clark, and Golder (2004) to capture the significance of the effect of the devaluation at different *W* values.

TABLE 3 Growth Difference and Institutions

Explanatory Variable	Dependent Variable: Growth Difference - Pre & Post 1997							
	W (1)	Polity (2)	Checks (3)	Log Chks (4)	Polcon3 (5)	Chks Sq (6)	Pol3 Sq (7)	Allhouse (8)
Exchange Rate Shock	-.131** (-2.30)	-.014** (-2.50)	.016 (1.21)	-.003 (-0.72)	-.032 (-1.37)	.084 (1.16)	-.098*** (-3.57)	-.079** (-3.01)
Institutional Variable	-.007 (-0.40)	-.000 (-0.13)	.004* (1.90)	.001 (0.18)	-.010 (-0.35)	.009 (0.66)	-.047 (-0.91)	-.007 (-0.89)
Institutional * Exch Rate Shock	.209** (2.09)	.000 (0.14)	-.023* (-2.01)	-.045* (-1.99)	.099 (0.90)	-.098 (-1.26)	.656*** (3.37)	.093** (2.65)
Institutional Variable Squared						-.002 (-0.81)	.102 (1.27)	
Institutional Sqd * Exch Rate Shock						.016 (0.99)	-1.09*** (-3.61)	
Latin America	-.015 (-1.10)	-.010 (-0.74)	-.014 (-0.95)	-.013 (-0.96)	-.013 (-0.89)	-.007 (-0.54)	-.017 (-1.19)	-.016 (-1.21)
East Asia	-.028 (-1.65)	-.015 (-0.98)	-.004 (-0.21)	-.007 (-0.42)	-.022 (-1.40)	.002 (0.12)	.009 (0.55)	-.030* (-1.80)
Sub-Saharan Africa	.003 (0.26)	.005 (0.38)	.005 (0.40)	.003 (0.22)	.006 (0.50)	.009 (0.78)	.006 (0.51)	.002 (0.13)
Previous Growth 1990 – 1997	-.730*** (-4.08)	-.411 (-1.20)	-.480** (-2.05)	-.452** (-2.07)	-.498 (-1.68)	-.551*** (-2.94)	-.755*** (-4.80)	-.643*** (-3.86)
Log 1997 GDP per capita	-.002 (-0.24)	-.006 (-0.85)	-.008 (-1.68)	-.007 (-1.16)	-.003 (-0.47)	-.005 (-0.77)	-.001 (-0.26)	-.001 (-0.21)
ELF60 – Ethnic Fractionalization	-.016 (-1.16)	-.018 (-1.18)	-.026** (-2.08)	-.020* (-1.73)	-.019 (-1.22)	-.025** (-2.12)	-.019 (-1.54)	-.016 (-1.29)
R <sup>2</sup>	.73	.67	.73	.74	.68	.77	.77	.76
Root MSE	.018	.020	.018	.018	.020	.017	.017	.017
Number of observations	44	44	44	44	44	44	44	44

Notes: T-stats produced with robust standard errors in parentheses. Significantly different from zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

*the modifying effect of Polity is not significant ( $t = 0.14$ ). All this is in line with Hypothesis 2.*

H3: Columns 3–5 show that the two measures of obstacles to policy change, Checks from DPI and PolconIII from Henisz, yield conflicting results. Checks (linear and logged) have a significant negative modifying effect. However, PolconIII has a positive, albeit insignificant, modifying effect. In brief, we are unable to conclusively determine if obstacles are helpful or hurtful to adjustments from exchange-rate devaluations. When the two measures of obstacles are entered quadratically, as per MacIntyre's hypothesis, they continue to yield opposite predictions. Whereas PolconIII strongly confirms MacIntyre's

*hypothesis that environments with an intermediate number of constraints on the chief executive are most conducive to adjustments from devaluations, the Checks measure suggests that they are the least conducive (see columns 6 and 7). We also find that polarization does not have a significant modifying effect on the relationship between exchange-rate devaluations and the dependent variable (not shown).*

*The most unambiguous result supporting the view that obstacles affect post-shock recoveries is presented in column 8, row 3. Allhouse, a dummy variable which captures the presence of unified government, reveals that executive control of the*



TABLE 4 Double Interactions

Explanatory Variable	Dependent Variable: Growth Difference - Pre & Post 1997				
	W with Allhouse (1)	W with Checks (2)	W with PolconIII (3)	W with S & Dem Res as a Control (4)	W over S with Dem Res as a Control (5)
Latin America	-.021 (-1.60)	-.014 (-1.01)	-.024 (-1.54)	-.014 (-1.07)	-.016 (-1.19)
East Asia	-.014 (-0.87)	-.004 (-0.21)	-.010 (-0.64)	-.027 (-1.67)	-.028* (-1.75)
Sub-Saharan Africa	.004 (0.32)	.003 (0.23)	.003 (0.19)	.002 (0.13)	.001 (0.10)
Previous Growth 1990 – 1997	-.838*** (-5.15)	-.746*** (-4.93)	-.844*** (-4.96)	-.767*** (-4.19)	-.773*** (-4.26)
Log 1997 GDP per capita	-.001 (-1.49)	-.007 (-1.07)	-.004 (-0.64)	-.002 (-0.37)	-.002 (-0.33)
ELF60 – Ethnic Fractionalization	-.019** (-1.76)	-.022** (-2.09)	-.024** (-1.72)	-.018 (-1.18)	-.019 (-1.29)
Exchange Rate Shock	-.152*** (-3.76)	.161 (0.69)	-.187*** (-5.08)	-.136** (-2.49)	-.138** (-2.47)
W	.019 (0.86)	-.031 (-1.02)	-.015 (-0.72)	-.013 (-0.66)	
W * ER Shock	.176* (1.72)	-.240 (-0.61)	.260** (2.41)	.218** (2.26)	
Obstacles Measure	.005 (0.23)	-.012 (-1.55)	-.110* (-1.76)		
Obstacles Measure * ER Shock	.225*** (4.20)	-.098 (-1.32)	.937** (2.73)		
Obstacles Measure * W	-.010 (-0.37)	.019 (1.61)	.188*** (2.83)		
Obstacles Measure * W * ER Shock	-.294*** (-3.60)	.125 (1.04)	-1.38*** (-3.96)		
WS: DemRes				.025 (1.04)	.018 (0.76)
S				-.020 (-0.31)	
W over S					-.014 (-0.70)
W over S * ER Shock					.222** (2.25)
R <sup>2</sup>	.81	.85	.82	.74	.74
Root MSE	.016	.014	.016	.018	.018
Number of observations	44	44	44	44	44

Notes: T-stats produced with robust standard errors in parentheses. Significantly different from zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

legislature has a significant positive modifying effect ( $t = 2.65$ ). Given that Allhouse is defined as a dummy variable the interpretation would be that major changes in the direction of greater central-

ization, of the nature of the chief executive gaining control over the legislature, contribute significantly to growth adjustments to exchange-rate devaluations. (This would be in line with Haggard's view.)

Calculations of the total effect of a currency shock drawn from Table 4 show that when Allhouse is included as a control, the growth shock becomes insignificant when  $W$  exceeds 0.5 if the obstacles environment is not conducive to recoveries (Allhouse = 0), and is insignificant at all levels of  $W$  if Allhouse = 1. The effect of  $W$  is also robust to the inclusion of PolconIII as a control, but is not robust to the inclusion of the Checks measure as a control. However, in the latter case the modifying effect of Checks also drops to insignificance. This result is not entirely surprising since the Checks measure incorporates an assessment of the competitiveness of elections, causing multicollinearity between  $W$  and the Checks measure. To address the multicollinearity question we present in Table 4, column 4 a specification in which we control for WS:Dem Res, the measure which captures those aspects of Polity's democracy rating that are not endogenous to the size of the winning coalition or the selectorate. Since Polity's rating is driven by constraints on the executive (see previous section), this is an alternative way of controlling for obstacles. The modifying effect of  $W$  remains significant ( $t = 2.26$ ). The result is unchanged when we replace  $W$  with the ratio of the winning coalition to the selectorate ( $W/S$ ).

The strong effect of  $W$  is also robust to all of the additional control specifications we devised. The preeminent economic explanation for the poor performance of several countries following 1997, aside from exchange-rate devaluations, is that they had incurred a large quantity of external debt by 1997. We thus conducted a robustness check adding External Debt/GDP in 1997 as a control to the specifications of Table 3. The modifying effect of  $W$  remained significant ( $t = 1.83$ ), while the modifying effect for the Polity measure remained insignificant ( $t = -0.36$ ). The results for the two measures of obstacles, DPI's Checks and Henisz's PolconIII continued to contradict each other, while the modifying effect of Allhouse remained significant (tables available on request).

We also ran regressions which differed from those presented in Table 3 only in that the ethnolinguistic fragmentation measure was replaced by income inequality as a proxy for latent social conflict.  $W$  continued to display a significant modifying effect ( $t = 2.20$ ), while the picture for democracy remained the same. The major change is that the modifying effect of Checks (linear and logged) was not robust ( $t = -1.44$ ). The remaining results are substantively identical to Table 3 (tables available on request).

## Conclusion

We have chosen possibly the major variety of economic shock of the last decade (currency devaluation) and have applied recent developments in our ability to parse out the

effects of accountability from those of obstacles to policy change. We have shown that much may be missed about the relationship between political institutions and economic outcomes if the modifying effect of accountability is not taken into account. These findings have significant policy implications. For one, they imply that international financial institutions may be justified in focusing on encouraging reforms in electoral institutions, rather than focusing on insulating policy makers from electoral pressures, when it comes to helping countries to respond to economic shocks. (We refer here to the World Bank's and IMF's focus on insulating central banks and other branches of the financial bureaucracy.<sup>15</sup>) In other words, rather than pushing to insulate the executive branch from the electorate, a major priority in recent years, the international financial institutions may be justified in focusing on supporting reforms which ensure that political leaders' electoral success is contingent on the support of as large a section of the population as possible.

Our future research agenda includes the systematic consideration of other major economic shocks in the past as a means of further assessing the role of accountability and checks in crisis recovery. However, in the short term it is our hope that our findings in one important and policy relevant area will result in a more comprehensive consideration of the effects of institutions on economic outcomes.

## Data Appendix

Country	<i>W</i> Score 1997–1999 Average
Rwanda	0
Burkina Faso	0.25
Gambia, The	0.25
Morocco	0.25
Sierra Leone	0.25
Togo	0.25
Cote d'Ivoire	0.333
Angola	0.5
Cameroon	0.5
Kenya	0.5
Mauritania	0.5
Pakistan	0.5
Senegal	0.5
Tunisia	0.5
Zimbabwe	0.5
Indonesia	0.583

(continued on next page)

<sup>15</sup>See Estache and Martimort (1999), Keefer and Stasavage (2000), Khemani (2003), and World Bank (1998).

## Data Appendix (continued)

Country	W Score 1997–1999 Average
Bolivia	0.75
Botswana	0.75
Brazil	0.75
Central African Republic	0.75
Chile	0.75
Colombia	0.75
Dominican Republic	0.75
Ecuador	0.75
Guatemala	0.75
India	0.75
Jamaica	0.75
Korea, Rep.	0.75
Malawi	0.75
Malaysia	0.75
Mali	0.75
Mexico	0.75
Mozambique	0.75
Nepal	0.75
Paraguay	0.75
Peru	0.75
Philippines	0.75
Singapore	0.75
Tanzania	0.75
Costa Rica	1
Mauritius	1
Taiwan	1
Trinidad and Tobago	1
Uruguay	1

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