POLICY RESEARCH WORKING PAPER 1938

What Explains the Success or Failure of Structural Adjustment Programs?

David Dollar
Jakob Svensson

A few political economy variables can successfully predict the outcome of an adjustment loan 75 percent of the time. To select promising candidates for adjustment, the World Bank must do a better job of understanding which environments are promising for reform and which are not. Being more selective may mean smaller volumes of lending.

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Summary findings

In the 1980s development assistance shifted largely from financing investments (such as roads and dams) to promoting policy reform. This change came because of a growing awareness that developing countries were held back more by poor policies than by a lack of finance for investment.

After nearly 20 years' experience with policy-based or conditional lending, there have now been many studies of adjustment lending, most of which take a case-study approach. Many conclude that policy-based lending works if countries have decided on their own to reform.

Dollar and Svensson examine a database of 220 World Bank-supported reform programs to identify why adjustment programs succeed or fail.

They find that a few political economy variables can successfully predict the outcome of an adjustment loan 75 percent of the time. Variables under the World Bank's control — resources devoted to preparation and supervision or number of conditions — have no relationship with an adjustment program's success or failure.

What development agencies must do, then, is select promising candidates for adjustment support. When the candidate is a poor selection, devoting more administrative resources or imposing more conditions will not increase the likelihood of successful reform.

To improve its success rate with adjustment lending, the World Bank must become more selective and do a better job of understanding which environments are promising for reform and which are not. That is likely to lead to fewer adjustment loans, unless there is a significant change in the number of promising reformers. To become more effective at supporting policy reform, the agency must be willing to accept that this may lead to smaller volumes of lending.

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David Dollar and Jakob Svensson

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> ***It seems clear that the lending cum conditionality process works well only when local polities have decided, largely on their own, possibly with outside technical help, to address their reform needs, effect certain policy changes sequentially, and approach the international community for financial help in getting there.***
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> ***-- Gustav Ranis, "On Fast Disbursing Policy-Based Loans"***

1. Introduction

Development assistance shifted to a large extent in the 1980s from financing investment (roads and dams) to promoting policy reform. This reorientation arose from a growing awareness that developing countries were held back more by poor policies than by a lack of finance for investment. The development community has had nearly twenty years of experience now with policy-based or conditional lending. There have been a large number of studies of adjustment lending, almost all of which take a case study approach. Gustav Ranis's conclusion above -- that policy-based lending works if countries have decided on their own to reform -- is echoed by other studies. Our objective in this paper is to look more systematically at the causes of success or failure of adjustment programs, using a database with 220 reform programs supported financially by the World Bank.

We approach this work with two hypotheses, not mutually exclusive. The first hypothesis is the one noted above: that success or failure of reform depends largely on political-economy factors within the country attempting to reform. Our analysis includes several variables that capture elements of the domestic political economy: ethnic fractionalization, whether leaders are democratically elected, length of tenure, and others. It should be pointed out upfront that, even if reform depends primarily on domestic

factors, policy-based lending may still be useful. In this case one should view conditional loans primarily as a commitment technology: they provide an opportunity for reformers to tie their own hands, in the same way that membership in the World Trade Organization commits a government to good policy and insulates it from special-interest politics.

The data that we have cannot be used to discern whether policy-based loans are an effective commitment technology. To determine this would require a study of reform programs supported by adjustment loans compared to reform programs not supported by adjustment loans.

What we can do with our data is look at the important question of whether the World Bank's effort increases the probability of success or failure of a reform program. Thus, a second hypothesis to consider -- not mutually exclusive with the first -- is that factors under the control of the World Bank influence the success of adjustment programs, after controlling for the domestic political-economy factors. The variables under the World Bank's control include the resources devoted to analytical work prior to reform, the resources devoted to preparation and supervision of adjustment loans, the number of conditions, and the sequencing of conditions (prior actions versus first, second, or third tranche conditions). In examining this second hypothesis it is important to recognize that the Bank effort variables are likely to be endogenous. We instrument for these in a two-stage probit regression. The search for good instruments reveals some interesting additional information about how the World Bank allocates resources among activities.

We find considerable support for the first hypothesis, that domestic political-economy factors influence strongly the success or failure of reform programs supported by

adjustment loans. We find no evidence that any of the variables under the World Bank's control affect the probability of success of an adjustment loan. It is possible of course that in exceptional cases the World Bank's effort affects reform. What this kind of econometric work identifies is what is true on average or in general. There are a number of countries -- Kenya or Zambia, for example -- in which the Bank had a series of mostly failed adjustment loans. Our work suggests that these were not fertile grounds for reform, that there are observable indicators that could have predicted this, and that the World Bank working harder was not going to transform Kenya or Zambia into a successful reformer.

These results have clear implications for how to manage policy-based lending. They suggest that the role of adjustment lending is to identify reformers not to create them. Development agencies need to devote resources to understanding the political economy of different countries and to finding promising candidates for support. The key to successful adjustment lending is to find good candidates to support. Adding more conditions to loans or devoting more resources to manage them does not increase the probability of reform. In fact, the World Bank devotes far more resources to the failed programs. Once a bad loan is made there is a tendency to put a lot of resources into salvaging it, and our evidence shows that this is fruitless.

There is a large opportunity cost to managing policy-based lending badly, and it comes in three forms. First, almost all adjustment loans disburse fully, even if policy conditions are not met. Thus, poor choices about which reform programs to support lead to disbursement of large amounts of aid into poor policy environments. Burnside and

Dollar (1997) have shown that aid promotes growth only in a good policy environment, so that the channeling of resources into poor policy environments that accompanies failed adjustment programs has a high cost. Second, the World Bank devotes more administrative resources to failed programs than to successful ones, and we show that these resources have no impact. Deininger, Squire, and Basu (1997) have shown that the Bank's administrative resources have a high return in investment projects, so that using these resources on low-probability reformers has an opportunity cost. Finally, our results support the view that the best justification for policy-based lending is as a commitment technology for sincere reformers. However, the effectiveness of this technology is undermined if adjustment loans are given indiscriminately. In the data set, one-third of adjustment programs supported by the World Bank failed. Such a failure rate may undermine the potential usefulness of the instrument. One reason, for example, that reformers might welcome a commitment technology is to convince private investors that policy change is permanent. However, if one-third of adjustment programs fail (and in most cases money is still disbursed), then this instrument is not a very good signal and not much of a commitment technology. To improve the success rate of adjustment programs, the World Bank needs to be more selective and discerning in providing this kind of assistance.

The remainder of the paper is organized as follow: The next section provides the analytical framework and develops the two hypotheses in more detail, relating them to the theoretical literature on policy reform. Section 3 provides the main empirical results. The paper ends with a brief concluding section.

2. Analytical framework

We build our empirical specification on two stands of literature, on the one hand the mainly theoretical literature on political economy of policy reform, on the other hand the mainly case study literature on policy design and World Bank behavior. In this section we briefly discuss the most important variables for the success of structural reforms that have been identified in this literature.

Before proceeding, several caveats are in order. First, the questions asked in the political economics literature is typically "why," or "when" a country chooses to reform or not. The question, why a country initiates a reform but then subsequently chooses not to implement the reform package, has received much less attention. Our data cover this latter type of situation. Hence, it is not possible to apply the predictions from the political economics literature literally.

The standard justification for World Bank structural adjustment lending is that reforms have short-run costs and foreign assistance can help reforms get launched by alleviating these costs. External resources, however, also reduce the cost of doing nothing, that is avoiding reform. The argument from the proponents of foreign assistance is that aid is disbursed conditional on reform. However, if the donor cannot perfectly tell if the recipient government is a genuine reformer, it faces an adverse selection problem. A country not committed to reform may find it in its interest to initiate a reform in order to subsequently receive foreign assistance. Then, once aid is disbursed, the recipient may not exert much effort in making the reform succeed [see Rodrik (1989)]. This argument has empirical support. Collier (1997) argues for example that African governments almost

never declined aid-for-reform packages, even if they had little intention of sustaining the reforms. For our purposes, we want to use the political economy literature to identify governments that are not likely to be committed to reform *ex ante*, but also to identify factors that affect the probability of success once the reform has been launched.

An additional problem concerns the distinction between economic stabilization and structural reform. It has been common practice in much of the literature to conflate these two groups of policies. In this paper we are concerned with the latter. Again, this implies that the predictions from the political economy literature must be judged carefully.

The last caveat has to do with coverage.[1] We have singled out two strands of the literature as the basis for our empirical specification. However, even within these areas we constrain the discussion primarily to those topics for which we are able to collect reliable data.

2.1. Political Economy

Recently, the theory of macroeconomic policy and economic reforms has changed focus. Instead of viewing the making and implementation of economic policy as a control problem in which the issue is to find the optimal policy rule, the core of the analysis has shifted to the actual policy process. Observed economic polices are explained by appealing to specific incentive constraints that may be binding for optimizing policymakers.

The theoretical literature in political economy has identified several factors affecting the likelihood of successful reforms.[2] The first one that we pick up on is political

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[1] There is a large political science literature on policy reforms which we will not cover, see e.g. Haggard & Kaufman (1992), Nelson (1990). There is also a huge literature on political economy of reform based on case-study evidence that we hardly touch upon. A short, but incomplete, biography include Bates & Krueger (1993), Haggard & Webb (1994), Ranis & Mahmood (1991) and Williamson (1994).

[2] For a recent review of the literature, see Tommasi & Velasco (1995).

instability.[3] Generally, political instability shortens the time horizon of a reforming government. The reason for this is that political instability creates an asymmetry with respect to cost and benefits of a reform. Typically, the cost of a reform must be born immediately, while the expected benefits occur in the future. If the incumbent is uncertain he/she will reap the benefits of the reform, this may affect the incentives to exert adjustment effort in the first place.

Implicit in the political instability approach is the view that policy is determined in a unified fashion. A related branch of the literature focuses instead on distributional conflicts across powerful groups in society. Alesina & Drazen (1991) show how *stabilization* can be delayed due to a "war of attrition" between two powerful groups [see also Labán & Sturzenegger (1992) and Velasco (1993)]. In the Alesina & Drazen model, the two groups both bear a cost as long as the stabilization is delayed. A stabilization can only occur if both groups agree to it, but the first to concede bears a larger fraction of the cost of reforms. Since the cost of not stabilizing may differ across groups and is private information, each group has an incentive to postpone concession in the hope that the opponent will be the first to give in. Taken literally the model focuses on delay in initiating reform. In the "ongoing reform" context a natural interpretation is instead an excessive dose of gradualism in continuing with an already initiated reform.

Alesina & Drazen (1991) show that the more uneven the expected costs of stabilization when it occurs (lower political coercion), the later is the expected date of stabilization, or the more excessive the dose of gradualism. Underlying the Alesina &

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[3] See for example Persson & Svensson (1989), Tabellini & Alesina (1991) for models of fiscal policy and political instability, and Svensson (1997a) for a model and empirical evidence on the relationship between structural [legal] reforms and political instability.

Drazen (1991) model is the view that (latent) social conflict is a key factor determining the success of structural reforms. Thus, a second variable that we want to introduce into our empirical analysis is one capturing social cohesion.

Another dimension that has received attention is the identity of the government (free-marketeers, right wing, left-wing, populist). One might conjecture that typical free-marketeers are most likely to carry out market friendly reforms. However, Cukierman & Tommasi (1994) suggest that policies are more likely to be successful if proposed by "unlikely" characters. If voters are not fully informed about the way polices map to outcomes, a "populist" government's ability to implement a reform with short-run costs may be greater than the ability of an "ideological" reformer. The reason is that if a "populist" government proposes a reform, the public has less reason to suspect that the reform is initiated because of ideological tendencies rather than for efficiency-enhancing reasons, and may therefore support it more strongly.

The issue of identity of the reformer is a subset of a much larger issue: credibility.[4] Apart from questions on speed and sequencing of reforms, of interest for empirical work is the argument that a new "reforming" government typically enjoys a "honeymoon" which may extend to months, even years [see Tommasi & Velasco (1995)].[5] At the other spectrum, Cukierman & Liviatan (1992) show that it takes time to build up a reputation.

While there exists a large case-study based literature on democratization (political liberalization) and economic reforms [see for example Haggard & Webb (1994)], there is

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[4] See for example Calvo (1989) for references.

[5] There exists a large literature on sequencing and speed of reforms. We have left this issue out of the discussion due to lack of reliable indicators to measure these variables. For references see e.g. Edwards (1989), Edwards and Van Wijnberger (1986), Calvo (1989), Dewatripoint and Roland (1992, 95). Martinelli and Tommasi (1993), Rodrik (1989).

only limited theoretical work. In fact, the theoretical work on the role of democracy and policy choices does not explicitly deal with economic reforms, but economic policy making in general. Banks & Sundaram (1993), and Besley & Coate (1995), show that the desire to build a reputation may provide a democratically elected government with the incentives to raise (adjustment) effort. Svensson (1997c) studies another mechanism, whereby political liberalization raises the incentives for public agencies to implement polices more efficiently. However, a democratic government also faces constraints that may lower its incentive to exert effort in implementing reforms. For instance, a democratic government may be more shortsighted in that a reform with initial costs but benefits occurring in the future may not be implemented owing to the fear of not getting reelected. Thus, the literature suggests that whether a leader is democratically elected and length of tenure may affect the probability of successful reform.

That economic crises seem either to facilitate or outright cause economic reform is part of the new conventional wisdom [see for example Tommasi & Velasco (1995)]. The argument is straightforward. By making a delay more costly (shock that increases the cost of inflation, for instance) this can actually accelerate the arrival of stabilization.[6] However, the theoretical work on this issue is related to fiscal stabilization [see Drazen & Grilli (1993), Velasco (1993)]. With respect to structural reforms this is much less clear. In fact it has been argued that reforms in a recession may involve a much higher political cost and therefore be more difficult to implement. On the other hand, Rodrik (1994), stressing the agenda-setting role of a reformist government, argues that crisis enables a reformist

[6] Moreover, crises can actually increase welfare if the indirect effect of reducing the delay outweighs the direct cost of the crisis.

government to package fiscal stabilization -- viewed as crucial for the return to price stability -- with structural reform -- viewed as desirable in the longer run but incidental to the immediate crisis. Policy-makers presenting domestic interests with the whole package are more likely to get general support. Even though the interest groups prefer to have only the stabilization component of the package, this is not a choice they face.

Analytically the crises hypothesis is not without problem. As pointed out by Rodrik (1996), there is a strong element of tautology in the association of reform with crises. Adjustment naturally becomes an issue only when current policies are perceived not to be working. A crisis is just the extreme instance of policy failure. In our empirical work we try to identify several measures that capture the extent of pre-reform crisis.

In summary, the theoretical literature does not provide clear guidance as to what kind of government is likely to sustain a reform program. It does, however, suggest some variables that one should include in an empirical analysis of the likelihood of successful reform. Thus, our first hypothesis is that the success or failure of reform is determined to some extent by political economy variables, such as measures of political instability, measures of polarization and social division, the length of tenure of the government, extent of pre-reform crisis, and whether the government is democratically elected. As can be seen in table 1, we were able to collect data for a range of variables that capture these influences.

2.2. World Bank Policies -- Conditionality

There is by now a fairly large literature on World Bank policies and conditionality and their effectiveness, both by the World Bank and by outside observers [see for example

Mosley (1987), Thomas (1991), Mosley et al. (1995) and several World Bank studies]. The weight of this case study evidence is that variables under the World Bank's control do not have a large effect on success or failure of reform, a proposition that we would like to test formally. For example, Mosley et al. (1995) conclude from case study evidence that conditional World Bank aid has affected the polices of the recipients "a little, but not as much as the Bank hoped" [Mosley et al., 1995, p.305]. They argue that the main reason for this is conflicting interests on the World Bank's part. The objective of policy-based lending is not only to change the policy structure viewed to be at the heart of the problem facing the recipient country, and indirectly increase the likelihood of success of World Bank projects, but also to provide quick-disbursing finance so as to hinder potential defaults on its outstanding loans, as well as loans from influential countries and their commercial banks. With respect to conditionality, Mosley's (1995) conclusion is in line with the World Bank's own recent review of policy-based lending [Branson & Jayarajah (1995)] -- conditionality is more effective when it focuses on a small range of quantifiable indicators.

Collier arrives at a more pessimistic conclusion -- "conditionality has failed" [Collier, 1997, p.57]. Collier argues that some governments have chosen to reform, others to regress, but that these choices appear to have been largely independent of the aid relationship. This conclusion is in line with both recent empirical and theoretical findings. Burnside and Dollar (1997) find that aid does not seem to affect macroeconomic policies (trade openness, fiscal surplus and inflation) in any systematic way, neither have the donors allocated aid to countries with "good" polices. Svensson (1997b) provides

theoretical underpinnings for the result, building on the moral hazard problem of foreign assistance and the time inconsistency problem in punishing non-reforming countries. Collier (1997) argues forcefully that government policy is determined by domestic political forces, rather than what the World Bank conditions its aid upon.

3. Explaining Success or Failure of Adjustment Programs

Can the success or failure of adjustment programs be explained by political economy variables? Do variables under control of the World Bank have any effect on the success rate of its adjustment loans? These are the primary questions that we address in this section. We started with 272 World Bank adjustment loans completed during the period [1980-1995]. For 179 of these loans we have been able to assemble data on several political-institutional factors, other exogenous variables (such as initial per capita GDP and population), and variables under the World Bank's control.

The dependent variable in this analysis is a zero-one variable reflecting failure or success of each adjustment loan as determined by the Operations Evaluation Department (OED) of the World Bank. There are several reasons why we think that this is an acceptable measure of success. First, the objective of OED evaluation is not to look narrowly at whether loan conditionalities were met or not; rather, the evaluators make a judgment as to whether or not the larger objective of reform has been met (has trade become more liberal, have enterprises actually been privatized?). Second, while there is clearly a subjective element to such an assessment, OED's independence within the World Bank means that there is no necessary bias in the results. OED is independent of the

Bank's senior management; it has a budget allocated directly by the Board of Directors and reports to them. Third, OED has found that about one-third of adjustment loans fail. In our sample, 36% of the reform programs are judged by OED not to have met their objectives. Finally, the OED measure is meant to capture change in policy. Most previous work on explaining adjustment progress has used different outcome measures (or changes in them) as proxies of reform, but that has obvious shortcomings. For example, outcome is partly driven by exogenous shocks which are difficult to disentangle from policy effects, there is lag between policy change and outcome, and reforms differ in objectives and may therefore not be captured by a single outcome meausure. We avoid these problems by using the OED measure.

Our model can be outlined as follows. Let y_i^* be the probability of success of adjustment program i. This probability is not directly observable. Instead we observe a zero-one indicator of success, y_i. Let p_i be an n×1 vector of political-economy variables reflecting country conditions at the time of approval of adjustment loan i; b_i be a k×1 vector of variables, associated with adjustment loan i, under the World Bank's control; z_i be an m×1 vector of exogenous variables that do not influence success or failure of reform; and ε_{yi} (a scalar) and ε_{bi} (a k×1 vector) mean-zero error terms. Then the model can be expressed as

$$y_i^* = c_y + b_i'\delta_y + p_i'\beta_{yp} + \varepsilon_{yi} \qquad (1)$$

$$b_i = c_b + \lambda_b' z_i + \beta_{bp}' p_i + \varepsilon_{bi} \qquad (2)$$

where c_y is a scalar, δ_y and c_b are k×1 vectors, β_{yp} is a n×1 vector, λ is a m×k matrix, and β_{bp} is a n×k matrix.

There are several issues in trying to estimate these equations. If the World Bank control variables were independent of the error term in (1), then we could use probit to estimate the zero-one indicator of success. However, it is likely that the error terms in (2) are correlated with the error term in (1). An exogenous shock that reduces the probability of success is likely to call forth more preparation and supervision resources. Thus, in order to estimate these relationships it will be necessary to find good instruments: that is, exogenous variables that are correlated with Bank effort but that do not influence success or failure of reform. We will argue that there are such instruments and use a two-stage procedure to estimate equation (1). We will also estimate equations for two of the most important Bank-effort variables, the amount of preparation resources and the amount of supervision resources.

Before proceeding it is useful to take an initial look at some of the data. We have almost all of the necessary data for 182 adjustment loans (65 failures, or 36% of the sample; and 117 successes, or 64%). It can be seen in Table 2a that successful adjustment loans are associated with governments that were democratically elected (50% of successes compared to 32% of failures). Also, political instability (measured here by the average number of government crises) is highly correlated with failed adjustment. Two variables that we will use in a non-linear fashion are ethnolinguistic fractionalization and length of time that a government has been in power.

What is striking in Table 2a is that the World Bank related variables are remarkably similar for successful and failed adjustment loans. Number of conditions and loan size are nearly identical. Successful loans get about 10% more preparation resources

(measured in staffweeks) than failed loans. The most striking difference is that failed loans get about 50% more supervision staffweeks. We will show that this relationship is endogenous; once the World Bank has made a bad loan it puts a lot of resources into trying to salvage it. The interesting question will be whether those supervision resources make any difference, after controlling for this endogeneity.

Table 3 reports a series of probit regressions that attempt to explain the probability of success.[7] For comparison, we also report the results of using a linear probability model in Table 6. Regression 1 has only the political-economy variables: success is associated with democratic government and with political stability. Ethnic fractionalization and length of time that the incumbent has been in power enter non-linearly: the basic message is that high degrees of fractionalization are bad for policy reform, and that long-term incumbents are not likely candidates for reform. The turning points for the length of tenure and ethnic fractionalization vary between 15-21 years and 0.44-0.49 respectively in Table 3. These relationships are pretty strong and the basic story is a plausible one. A recently elected government that launches reform has a 95% chance of success, ceteris paribus, compared to only about a 65% probability of success for an authoritarian leader in power already for 13 years (Figure 1). That high probability of success for an elected reformer, however, can be undermined by political instability and ethnic division. An interesting finding is that the marginal impact of a democratically elected government (about 20 percent higher probability of success) is quantitatively independent of the degree of ethnic fractionalization, as shown in Figure 2. Thus, democratically elected governments have a

[7] Using a probit model instead of a linear probability model has a number of well known advantages [see Judge et al (1985)].

higher probability of successfully implementing reforms, irrespective of underlying degree of ethnic fragmentation. Using only the political-economy variables, regression 1 predicts correctly 75 percent of the observations.[8]

In Regression 2 we add some additional exogenous variables: initial per capita GDP, population, and regional dummies. Note that the predicted ability of the model goes down from 75 percent to 73 percent and that none of these variables has much relationship with outcomes. This is important because we are going to use these variables as instruments. It is interesting that adjustment loans do tend to be less successful in low-income countries and in Africa. But Regression 2 indicates that those associations arise from the fact that low-income countries and African countries have characteristics that are not conducive to reform. With political-economy variables in the equation, there is no significance to the African regional dummy or to initial per capita GDP.

In Regression 3 we add Bank-related variables to a probit regression, recognizing that there is an endogeneity issue that has not yet been addressed. Some of these variables we are going to argue are exogenous: whether the adjustment loan focuses on trade reform or sectoral reform depends on the nature of the policy problems in the country and the government's desire to attack particular problems. What is clearly under the Bank's influence is the amount of preparation staffweeks; amount of supervision staffweeks; the staffweeks devoted to analytical work in the four years prior to the loan; the number of conditions; how conditions are allocated between upfront conditionality and first, second, and third tranches; the size of the loan; and the expected length of the reform program.

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[8] The prediction rule is y = 1 if the predicted probability > 0.5, and 0 otherwise; that is, we predict a 1 if the model says a 1 is more likely than a 0.

It is difficult to instrument for all of these endogenous variables at the same time. We use the simple correlations and the partial correlations in the probit regressions to eliminate the variables that seem to have no relationship at all with outcome: number of conditions, loan size, prior analytical work, and expected length of the reform program.[9] Regression 4 shows the probit regression after these are removed. Of the remaining Bank-related variables, the interesting story is that preparation is positively associated with outcomes and supervision, negatively associated. Once we control for these two variables, the number of conditions and the allocation of conditions play no role.

In Regression 5 we instrument for preparation and supervision, using the two-stage generalized least squares estimator derived by Amemiya (1978).[10] The specifications for preparation and supervision are depicted in Table 4 (column 2) and Table 5 (column 2). Once these Bank-effort variables are treated as endogenous, there is no relationship between any of them and the success or failure of adjustment programs. In Regression 6 we drop all of the Bank variables except preparation and supervision -- for which we instrument -- and again find no relationship.

The relationship between the political-economy variables and outcomes is stable throughout all of the regressions. This finding is consistent with the view that there are institutional and political factors that affect the probability of success of a reform program. Given those factors, none of the variables under the World Bank's control affects success or failure of adjustment programs. If endogeneity is ignored, there is a positive

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[9] The measure of analytical work in the four years prior to the adjustment loan comes from Deininger and Squire (1997). They find that that this variable has a strong association with success of investment loans; our regressions show that this is not the case for adjustment loans.

[10] See appendix for a brief description of the two-stage estimator.

relationship between preparation and outcomes, and a negative relationship between supervision and outcomes. That these relationships disappear in the two-stage regressions indicates that the associations reflect how the World Bank allocates resources. In other words, preparation resources favor winners and supervision resources favor losers.

We also tried several other political variables in the outcome regression, including income inequality (as a proxy for distributional conflicts), terms of trade shocks, and the level of inflation and budget surplus prior to the reform (as a rough test of the crisis hypothesis). However, once we control for the variables defined in Table 2a, none of these additional regressors has any explanatory power. As we lose a number of observations by including these additional controls, we leave them out of the base specification. As shown in Table 2b, however, successful adjustment loans are associated with countries with better fiscal balance prior to the reform and larger exogenous shocks during the reform period. One explanation for why the policy variables still do not provide additional information in the outcome regression is that they are driven by the same socio-political variables that affect the likelihood of success [see for example Easterly & Levine (1997)]. In fact, in simple bivariate regressions with prior fiscal stance as dependent variable, initial budget surplus enters with a positive and significant sign.

We turn next to an explicit examination of the allocation of preparation and supervision resources. First, we look at the relationship between preparation and the political-economy variables (Table 4, Regression 1). There is very little relationship, except that more resources are allocated to democratically elected reformers (this is probably what accounts for the correlation between preparation and success). Regression

2 shows a more completely specified equation for preparation resources. Note that preparation is strongly related to a number of variables that in turn have no relationship with outcomes (which is why we have adequate instruments to examine the relationship between preparation and success of reform). First, the World Bank allocates different amounts of resources to different regions, so that preparation resources tend to be low in East Asia and Latin America relative to Africa.[11] (It is interesting that in the outcome equation the political-economy variables are significant while regional dummies are not; whereas in the allocation of preparation resources we have the opposite: regional dummies matter while most of the political-economy factors do not.) Second, there are more resources for large loans and for those with many conditions, though again these characteristics are unrelated to outcomes. Finally, resources go to low-income countries and to countries small in population.

There is a broadly similar story for the allocation of supervision resources (Table 5, Regression 1). These resources favor loans that are large and have lots of conditions. Also, low-income countries and those small in population get more supervision resources. Unlike the preparation equation, regional dummies are no longer important.[12] The regional departments of the World Bank have different amounts to prepare loans, but once these loans are approved the regions devote similar resources to supervising a loan of given characteristics. In the supervision equation we also have to consider that preparation may affect supervision. In studying World Bank-financed investment projects,

[11] F-statistic on the joint hypothesis that the coefficients on the regional dummies are zero is 3.21. Thus, we can reject the hypothesis at the 5 percent significance level.

[12] F-statistic on the joint hypothesis that the coefficients on the regional dummies are zero is 1.55. Thus, we cannot reject the hypothesis at the 5 percent significance level.

researchers have found that more preparation resources lead to a smaller need for supervision resources. However, preparation and supervision are both associated with better outcomes in investment projects. Since there is no relationship between preparation and the success of adjustment programs, it seems unlikely that a large amount of preparatory work would diminish the need for supervision.

In the OLS regression (Table 5, Regression 1) there is a large, positive relationship between preparation and supervision. This reflects the fact that the error terms in the preparation and supervision equations are certainly correlated. Anything unobserved that leads to higher (lower) than predicted preparation will almost certainly lead to higher (lower) than predicted supervision. The fact that the regional dummies seem to belong in the preparation equation but not in the supervision equation means that we can use them as instruments to correct for this simultaneity problem. In the two-stage least squares regression (Regression 2), the relationship between preparation and supervision is no longer significant.

4. Conclusion

In the 1980s the World Bank approved four structural adjustment loans for Zambia, totaling $212 million. All of these loans disbursed almost fully (less than two percent of the committed amount was canceled altogether). After they were completed, the independent Operations Evaluation Department within the World Bank rated three out of four as failures; that is, the reform measures supported by three out of four loans were not satisfactorily implemented. Our results suggest that this outcome was largely

predictable. Zambia at that time did not have conditions conducive to reform. The government had not been democratically elected. It had been in power for a long time in a country that is highly ethnically fragmented. Such a government is not a likely reformer.

More generally, we have shown that a small number of political economy variables can predict the outcome of an adjustment loan successfully 75% of the time. When variables under the World Bank's control -- resources devoted to preparation and supervision or number of conditions -- are added to the analysis, they have no relationship with success or failure of adjustment programs. Our work taken in concert with other research suggests that the key issue for development agencies is to select promising candidates for adjustment support. When a poor selection is made, devoting more administrative resources or imposing more conditions will not increase the likelihood of successful reform.

If the World Bank would like to improve its success rate with adjustment lending, then it must become more selective and do a better job of understanding what are promising environments for reform and what are not. This change is likely to lead to fewer adjustment loans unless there is a significant exogenous change in the number of promising reformers. To become more effective at supporting policy reform the agency would have to be willing to accept that this may lead to smaller volumes of lending.

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Appendix

A.1. Estimation of a simultaneous probit model

The model with preparation and supervision as endogenous variables is

$$r_i = z_{ri}'\lambda_r + p_i'\beta_{rp} + \varepsilon_{ri} \tag{A.1}$$

$$s_i = \delta_{sr} r_i + z_{si}'\lambda_s + p_i'\beta_{sp} + \varepsilon_{si} \tag{A.2}$$

$$y_i^* = \delta_{yr} r_i + \delta_{ys} s_i + p_i'\beta_{yp} + \varepsilon_{yi} \tag{A.3}$$

Where y_i^* is the probability of success of adjustment program *i*. This probability is not directly observable. Instead we observe a zero-one indicator of success, y_i. s_i and r_i are supervision and preparation, respectively, of program *i*. All other variables are defined in section 3, where z_{si}' is a subset of z_{ri}', and where p_i' includes a constant.

In reduced vector form

$$r = X\Pi_1 + u_1 \tag{A.4}$$

$$s = X\Pi_2 + u_2 \tag{A.5}$$

$$y^* = X\Pi_3 + u_3 \tag{A.6}$$

Where X is a t×(n+m) matrix of predetermined variables. Let $\alpha_y' = [\delta_{yr}, \delta_{ys}, \beta_{yp}']$, and $\delta_y' = [\delta_{yr}, \delta_{ys}]$. We assume that $\varepsilon_{ri}, \varepsilon_{si}, \varepsilon_{yi}$ have a joint normal distribution with mean zero and covariance matrix

$$\Omega = \begin{bmatrix} \Sigma_{rs} & \Sigma_{rsy} \\ \Sigma_{yrs} & 1 \end{bmatrix} \tag{A.7}$$

where we have normalized $\sigma_y^2 = 1$.

A.1.1 Estimation of α_y'

The two-stage procedure proposed Heckman (1977), Nelson and Olson (1977) and others [see Lee (1981)], would be to estimate Π_1 and Π_2 by OLS and Π_3 by probit, then estimate (A.3) by probit after substituting $X\hat{\Pi}_1$ for *r* and $X\hat{\Pi}_2$ for *s*. That is

$$y^* = \delta_{yr} X\hat{\Pi}_1 + \delta_{ys} X\hat{\Pi}_2 + p\beta_{yp} + \eta \tag{A.8}$$

Instead of estimating (A.8), Amemiya (1978) suggests one should solve by regression methods the structural parameters from the estimated reduced form parameters. Based on this principle, one can derive asymptotically more efficient estimators. As shown by Amemiya, the key to this result is to note that the structural parameters are related to the reduced form parameters according to

$$\Pi_3 = \Pi_1\delta_{yr} + \Pi_2\delta_{ys} + J_y\beta_{yp} \tag{A.9}$$

where $XJ_y = p$. Amemiya shows that by exploiting equation (A.6) and (A.8), equation (A.9) can be written as

$$\hat{\Pi}_3 = \hat{G}\alpha_y + \upsilon \tag{A.10}$$

where $\upsilon = \left[\hat{\Pi}_3 - \Pi_3\right] - \delta_{yr}\left[\hat{\Pi}_1 - \Pi_1\right] - \delta_{ys}\left[\hat{\Pi}_2 - \Pi_2\right]$, and $\hat{G} = \left[\hat{\Pi}_1 \hat{\Pi}_2 J_y\right]$.

The estimates suggested by Amemiya are generalized least squares, GLS, estimates given by

$$\hat{\alpha}_y^G = (\hat{G}'\hat{V}^{-1}\hat{G})^{-1}\hat{G}'\hat{V}^{-1}\hat{\Pi}_3 \tag{A.11}$$

where $\hat{V}$ is a consistent estimator of the asymptotic variance-covariance matrix of υ. Thus, to be able to estimate (A.11), we need a consistent estimator of $\hat{V}$.

Rivers and Vuong (1988), using the results derived in Amemiya (1978), show that

$$\hat{V} = d(X'X)^{-1} + V_0 \tag{A.12}$$

where $d = \delta_y' \Sigma_{rs} \delta_y - 2\delta_y' \Sigma_{rsy}$ and V_0 is the variance-covariance matrix of $\hat{\Pi}_3$. Initial consistent estimate δ_y can be obtained from $\hat{\Pi}_1, \hat{\Pi}_2$, and $\hat{\Pi}_3$ [Amemiya (1979)]. As for $\sigma_{jy}, j = \{r, s\}$, it may be consistently estimated by

$$\sigma_{jy} = T^{-1}\sum_{i=1}^{T}(y_i \hat{v}_{ji} \hat{f}_i^{-1}) \tag{A.13}$$

where $\hat{v}_{ri}[\hat{v}_{si}]$ is the least squares residual from (A.4) [(A.5)], and

$$\hat{f}_i = \frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}(\hat{\Pi}_3' x_i)^2} \tag{A.14}$$

The asymptotic variance-covariance matrix of α_y^G is

$$V(\hat{\alpha}_y^G) = (\hat{G}'\hat{V}^{-1}\hat{G})^{-1} \tag{A.15}$$

Figure 1. Elections, Tenure, and Probability of Successful Reform

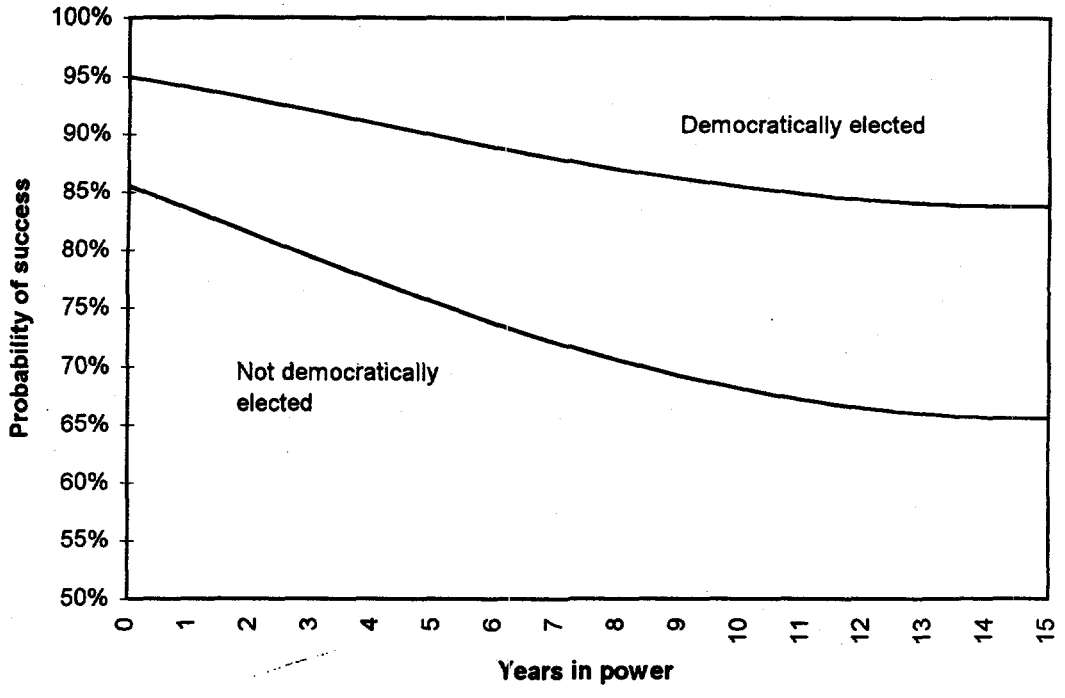
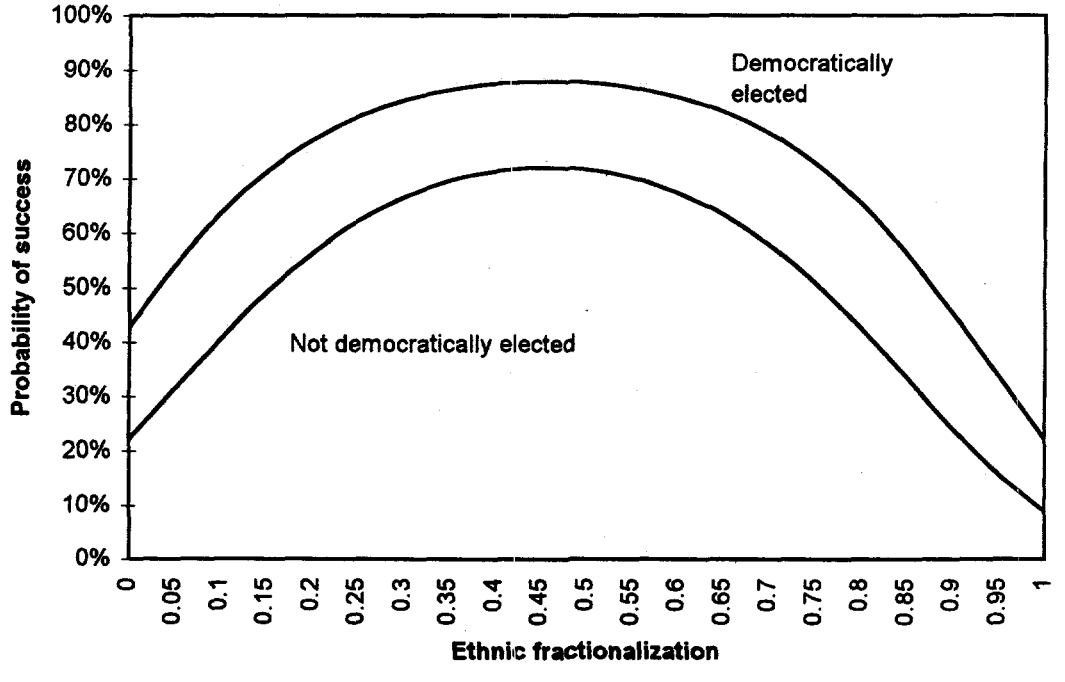


Figure 2. Elections, Ethnic fractionalization, and Probability of Successful Reform



Note: The probabilities are evaluated at the mean values of the explanatory variables. The marginal effect of a democratically elected government is the difference between the two functions.

Table 1. Variables defined in the political economy literature and empirical proxies

Variable defined in the literature	*Empirical proxy*
Political instability	Average number of governmental crises during the implementation of the program [*Source:Banks (1994)*]
Social division	Ethic fragmentation [*Source: Easterly & Levine (1997)*], Income inequality [*Source: Deininger & Squire (1996)*]
Length of tenure	Years the incumbent that signed the reform has been in power [*Source: Europa Yearbook (various years)*]
Democratically elected	Dummy variable taking the value 1 if the incumbent that signed the reform was put in power by a democratic election prior to the reform, 0 otherwise [*Source: Europa Yearbook (various years*]
Crisis	Terms-of-trade shock: prior reform, during implementation of reform [*Source: WDI 1997*], Inflation prior to reform [*Source: WDI 1997*], Budget surplus prior to reform [*Source: WDI 1997*]

Table 2a. Features of Successful and Failed Adjustment Programs

	Successful	*Failed*
Country Characteristics		
Democratically Elected	50.4%	32.3%
Government Crisis During Reform Period	8.0%	22.8%
Ethnoliguistic Fractionalization	0.48	0.51
Length of Time the Incumbent has been in Power Prior to the Reform	7.5	7.8
World Bank Related Variables		
Preparation Staff Weeks	141	128
Supervision Staff Weeks	69	101
Number of Conditions	45	44
Loan Size (million $)	160	153
Sample Information		
Number of Loans	117	65

Table 2b. Features of Successful and Failed Adjustment Programs (small sample)

	Successful	*Failed*
Country Characteristics		
Budget surplus prior to the reform	-0.043	-0.059
Inflation prior to the reform	27 %	34 %
Income inequality	44.0	43.5
Terms of trade shock	-1.92	-1.54

Table 3. Probit Outcome Regressions

Dependent variable: OED evaluation on adjustment operations

Regression No.	(1)	(2)	(3)	(4)	(5)	(6)
Observations	220	215	163	182	179	179
Countries	67	67	58	60	60	60
Constant	-0.098	-0.753	-0.735	-0.895	-0.366	1.175
	(-0.32)	(-0.34)	(-0.46)	(-0.83)	(-0.25)	(0.93)
Ethnic Fractionalization	5.930	6.218	6.590	8.584	7.763	6.861
	(4.16)	(4.00)	(3.00)	(4.52)	(4.04)	(3.74)
Ethnic Fractionalization^2	-6.513	-7.00	-6.940	-8.804	-8.046	-7.212
	(-4.27)	(-3.89)	(-3.01)	(-4.40)	(-3.79)	(-3.57)
Government Crisis	-1.301	-1.494	-2.950	-2.433	-2.285	-1.942
	(-3.94)	(-4.10)	(-4.60)	(-4.47)	(-4.29)	(-3.92)
Democratically Elected	0.585	0.658	0.857	0.792	0.912	0.812
	(2.61)	(2.71)	(2.704)	(2.72)	(3.09)	(2.80)
Time in Power	-0.089	-0.10	-0.175	-0.133	-0.113	-0.107
	(-2.07)	(-2.16)	(-2.79)	(-2.45)	(-2.09)	(-2.00)
Time in Power^2	0.003	0.003	0.006	0.005	0.004	0.004
	(2.15)	(2.21)	(2.56)	(2.34)	(2.02)	(1.88)
Preparation Staff Weeks			0.682	0.903	0.323	0.091
			(1.39)	(2.16)	(0.24)	(0.08)
Supervision Staff Weeks			-1.554	-1.428	-0.869	-0.934
			(-2.73)	(-2.98)	(-0.67)	(-0.84)
Finance Conditions (%)			1.274	1.252	1.423	
			(1.78)	(1.86)	(2.02)	
Macro & Fiscal Conditions (%)			0.448	0.927	0.766	
			(0.44)	(1.06)	(0.89)	
Sectoral Conditions (%)			2.087	1.536	1.161	
			(2.82)	(2.46)	(1.83)	
Trade Conditions (%)			1.965	1.181	0.961	
			(2.42)	(1.85)	(1.46)	
2nd and 3rd Tranch Conditions			1.849	0.915		
			(2.28)	(1.45)		
Number of Conditions (%)			0.368			
			(1.39)			
Loan Size (log)			-0.144			
			(-0.82)			
Expected Reform Period			-1.4E-3			
			(-0.31)			
Prior Analytical Work (log)			0.051			
			(0.35)			
Sub-Saharan Africa		-0.175				
		(-0.44)				
Latin America & Caribbean		0.009				
		(0.02)				
East Asia		0.056				
		(0.12)				
Initial GDP per capita (log)		-0.213				
		(-0.98)				
Initial Population (log)		0.144				
		(1.56)				
Predicted ability	0.75	0.73	0.83	0.80		

Note: Probit regressions. Regressions (5)-(6) are estimated by a two-stage procedure [Amemiya (1978)], described in appendix, with preparation and supervision specifications given in column 2, Table 4, and column 2, Table 5.

Table 4. Preparation regressions

Dependent variable: Preparation Staff Weeks

Regression No.	(1)	(2)
Observations	219	179
Countries	67	60
Constant	1.813	3.311
	(21.58)	(4.38)
Ethnic Fractionalization	0.376	0.018
	(1.00)	(0.04)
Ethnic Fractionalization^2	-0.327	0.043
	(-0.82)	(0.10)
Government Crisis	-0.132	-0.223
	(-1.51)	(-2.48)
Democratically Elected	0.098	0.124
	(1.67)	(1.98)
Time in Power	0.013	0.004
	(1.24)	(0.36)
Time in Power^2	-3.4E-3	-3.7E-3
	(-0.95)	(-0.99)
Finance Conditions (%)		-0.149
		(-1.07)
Macro & Fiscal Conditions (%)		-0.260
		(-1.33)
Sectoral Conditions (%)		0.002
		(0.02)
Trade Conditions (%)		-0.021
		(-0.15)
Number of Conditions (%)		0.153
		(3.29)
Loan Size (log)		0.281
		(5.29)
Structural Adjustment Loan		-0.145
		(-2.16)
Sub-Saharan Africa		-0.080
		(-0.78)
Latin America & Caribbean		-0.284
		(-3.06)
East Asia		-0.148
		(-1.39)
Initial GDP per capita (log)		-0.064
		(1.04)
Initial Population (log)		-0.147
		(-3.90)
R^2	0.04	0.34
Adjusted R^2	0.01	0.26

Note: Estimation by OLS

Table 5. Supervision regressions

Dependent variable: Supervision Staff Weeks

Regression No.	(1)	(2)
Observations	179	179
Countries	60	60
Constant	2.685	3.272
	(4.02)	(3.11)
Ethnic Fractionalization	-0.134	-0.144
	(-0.42)	(-0.46)
Ethnic Fractionalization^2	0.213	0.254
	(0.59)	(0.73)
Government Crisis	-0.029	-0.017
	(-0.39)	(-0.18)
Democratically Elected	-6.1E-3	-0.009
	(-0.01)	(-0.18)
Time in Power	0.003	0.004
	(0.29)	(0.48)
Time in Power^2	-3.6E-3	-4.7E-3
	(-1.14)	(-1.47)
Preparation Staff Weeks	0.339	0.364
	(5.14)	(1.34)
Finance Conditions (%)	-0.078	-0.120
	(-0.67)	(-0.99)
Macro & Fiscal Conditions (%)	-0.323	-0.256
	(-1.97)	(-1.41)
Sectoral Conditions (%)	0.180	0.175
	(1.65)	(1.59)
Trade Conditions (%)	-0.141	-0.141
	(-1.25)	(-1.23)
Number of Conditions (%)	0.074	0.077
	(1.85)	(1.28)
Loan Size (log)	0.210	0.220
	(4.37)	(2.50)
Structural Adjustment Loan	-0.062	-0.105
	(-1.10)	(-1.58)
Sub-Saharan Africa	0.093	
	(1.09)	
Latin America & Caribbean	0.020	
	(0.25)	
East Asia	-0.118	
	(-1.33)	
Initial GDP per capita (log)	-0.153	-0.184
	(-2.96)	(-3.39)
Initial Population (log)	-0.099	-0.124
	(-3.00)	(-2.66)
R^2	0.50	
Adjusted R^2	0.45	

Note: Estimation by OLS [col. (1)], and 2SLS estimation [col. (2)] with instruments given in regression 2, Table 4.

Table 6. Linear Probability Regressions

Dependent variable: OED evaluation on adjustment operations

Regression No.	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Observations	220	215	163	182	179	179
Constant	0.472	0.674	0.505	0.306	0.513	0.852
	(4.77)	(1.01)	(1.40)	(1.13)	(1.30)	(2.54)
Ethnic Fractionalization	1.888	1.939	1.513	2.199	2.187	2.122
	(4.29)	(4.09)	(2.97)	(4.62)	(4.23)	(4.20)
Ethnic Fractionalization^2	-2.073	-2.196	-1.589	-2.244	-2.275	-2.235
	(-4.42)	(-4.00)	(-2.95)	(-4.49)	(-3.99)	(-4.05)
Government Crisis	-0.423	-0.452	-0.635	-0.575	-0.617	-0.594
	(-4.08)	(-4.20)	(-4.77)	(-4.85)	(-4.84)	(-4.82)
Democratically Elected	0.184	0.204	0.232	0.218	0.260	0.253
	(2.66)	(2.68)	(2.96)	(2.86)	(3.25)	(3.15)
Time in Power	-0.026	-0.028	-0.041	-0.033	-0.030	-0.029
	(-2.08)	(-2.15)	(-2.80)	(-2.34)	(-2.09)	(-2.04)
Time in Power^2	9.7E-3	0.001	0.001	0.001	0.001	0.001
	(2.30)	(2.28)	(2.83)	(2.43)	(2.18)	(2.08)
Preparation Staff Weeks			0.142	0.196	-0.009	-0.019
			(1.18)	(1.81)	(0.02)	(-0.06)
Supervision Staff Weeks			-0.316	-0.344	-0.207	-0.236
			(-2.47)	(-3.00)	(-0.55)	(-0.74)
Finance Conditions (%)			0.316	0.298	0.359	
			(1.76)	(1.66)	(1.92)	
Macro & Fiscal Conditions (%)			0.119	0.207	0.191	
			(0.47)	(0.89)	(0.79)	
Sectoral Conditions (%)			0.450	0.366	0.299	
			(2.44)	(2.14)	(1.66)	
Trade Conditions (%)			0.449	0.270	0.247	
			(2.30)	(1.59)	(1.36)	
2nd and 3rd Tranch Conditions			0.413	0.266		
			(2.09)	(1.51)		
Number of Conditions (%)			0.076			
			(1.21)			
Loan Size (log)			-0.063			
			(-1.45)			
Expected Reform Period			-7.84E-05			
			(-0.69)			
Prior Analytical Work (log)			0.008			
			(0.22)			
Sub-Saharan Africa		-0.080				
		(-0.66)				
Latin America & Caribbean		-0.020				
		(-0.18)				
East Asia		0.025				
		(0.19)				
Initial GDP per capita (log)		-0.086				
		(-1.24)				
Initial Population (log)		0.030				
		(1.12)				
R^2	0.17	0.20	0.34	0.32		
Adjusted R^2	0.15	0.15	0.28	0.26		

Note: Estimation by OLS [cols. (1)-(4)], and 2SLS [cols. (5)-(6)] with preparation and supervision specifications given in column 2, Table 4, and column 2, Table 5.

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