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Civil Liberties, Democracy, and the Performance of Government Projects

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This article uses a cross-national data set on the performance of government investment projects financed by the World Bank to examine the link between government efficacy and governance. It demonstrates a strong empirical link between civil liberties and the performance of government projects. Even after controlling for other determinants of performance, countries with the strongest civil liberties have projects with an economic rate of return 8–22 percentage points higher than countries with the weakest civil liberties. The strong effect of civil liberties holds true even when controlling for the level of democracy.

The interrelationship among civil liberties, civil strife, and project performance suggests that the possible mechanism of causation is from more civil liberties to increased citizen voice to better projects. This result adds to the evidence for the view that increasing citizen voice and public accountability—through both participation and better governance—can lead to greater efficacy in government action.

Discussions of governance often generate more rhetorical heat than empirical light. Governance, like religion, is a broad topic that inspires strong beliefs and is difficult to measure reliably. Even a consensus on definitions is elusive: what do we mean by governance? A World Bank policy paper defines governance as "the manner in which power is exercised in the management of a country's economic and social resources for development," which does not easily lend itself to quantification (World Bank 1992, p. 1). We hope to shed some empirical light on one dimension of governance by demonstrating a positive link between a country's civil liberties and the performance of the government's investment projects.

Governance involves actions of publicly vested authorities. We label three interrelated dimensions of government action as *what, how,* and *how well. What* public decisions are taken—including the enactment of laws, policies, and regulations—affects the allocation of public expenditures and investments and de-

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termines incentives for all other actors. *How* public decisions and authority are exercised depends on underlying social structures, political structures, and official and unofficial institutions. *How well* public decisions and authority are exercised determines the efficacy of government in accomplishing its objectives.

Although researchers have written a lot on *what* and *how*, they have written less on *how well*. In this article, we analyze the impact of one element of *how*—the degree of civil liberties—on one element of *how well*—the returns on government investments. Section I reviews recent empirical, cross-national literature linking economic outcomes with government action as well as recent work on the efficacy of government action. Section II discusses data on economic rates of return of public investment projects financed by the World Bank. These data provide a unique quantitative measure of government performance that is comparable across countries. Section III presents evidence of a strong relationship between civil liberties and these rates of return; this relationship is robust to a wide variety of controls, including measures of democracy. Section IV explores the links among civil liberties, citizen voice, and project performance.

I. What, How, And How Well

Economic and social outcomes so depend on governance—for good and for ill—that the *what*, *how*, and *how well* of government action underlie the richest social science traditions. We cannot begin to do justice (even in outline) to this literature. Therefore, in setting the context for our new results, we limit the scope of our review to recent empirical, cross-national research focused principally on economic outcomes (and hence written mostly by economists).

Much recent literature concerns the impact on economic growth of *what* governments do. A small share of these studies examines the effects of directly measurable government actions on growth, such as levels and patterns of public investment expenditures (Easterly and Rebelo 1993 and Devarajan, Swaroop, and Zou 1996). A larger share examines the effects of a specific outcome associated with government actions, including school enrollment rates (Barro 1991), outcome-based measures of outward orientation (Harrison 1995 and Dollar 1992), financial depth (King and Levine 1993), macroeconomic instability (Fischer 1993), and investment in machinery (de Long and Summers 1993). Most such studies, however, offer no explicit link between specific government policies and actions (for example, building more schools) and the growth-promoting outcome (for example, higher enrollment rates).

Another strand of literature analyzes the effects of underlying social structures, political structures, and institutions that determine *how* governments exercise public decisions and authority. Much of this work focuses on the effects of civil and political liberties (Dasgupta 1993). Lipset (1960) demonstrated the association between higher levels of income and higher levels of civil liberties and of popular political participation. However, whether democracy promotes or hinders economic growth remains ambiguous. In the

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1960s and 1970s scholars debated whether democracy was an insuperable obstacle to development. Many argued that a premature move to democracy hindered growth by increasing the influence of special interest groups, fomenting the competition for policy-induced rents, lowering savings rates, reducing the stability of policy (especially macroeconomic policy), and fostering political instability. This position seemed reasonable at the time. The top 20 fastest growing major economies in 1960–74 included only three consistent democracies (and only one of those was a developing country), four decidedly authoritarian Asian economies, and four socialist countries. Current research, although deeply divided, tends to find no causal link at all between democracy and growth. Researchers have revisited the issue as part of the resurgence of empirical work on economic growth (Weede 1983; Kormendi and Meguire 1985; Scully 1988; Grier and Tullock 1989; Helliwell 1992; Barro 1994; and Bhalla 1994). Przeworski and Limongi (1993) and Alesina and Perotti (1994) provide excellent reviews.

The *what* and *how* of government action are, of course, critically linked. Policies and actions matter, and underlying conditions partially determine the choice of good or bad policies. The current studies on growth rarely document this link. However, the literature on central bank independence has established the connection between specific political institutional mechanisms that promote central bank independence and economic outcomes of inflation and growth (Cukierman, Webb, and Neyapti 1992). And recent work by Alesina (1996) shows how institutional arrangements affect budgetary outcomes.

Much of the ambiguity about the impact of democracy on growth revolves around whether more or less popular political participation leads to better or worse policy outcomes. Two contrasting arguments seem to be well documented. One argument is that more democratic arrangements may lead to greater public investments in infrastructure, greater (and more equitable) investments in human capital, more open trade policies (Tavares and Wacziarg 1996), and better provision of a secure legal system and property rights (Clague and others 1997). The other argument is that more democratic arrangements may have negative effects on government policies and actions when vested interests lobby for preferential treatment and against efficiency-enhancing reforms (Olson 1965). Negative effects might occur when local pressures block needed investments because of "not in my backyard" attitudes or when interest groups engage in wars of attrition in order to avoid the costs of stabilization and promote populist macroeconomic policy (Alesina and Drazen 1991). Recent work on economic reform is not entirely sanguine about the ability of democratic politics as usual to bring about economic reforms because the magnitudes of redistribution relative to efficiency gains are often large (Rodrik 1996). According to one view of the success of some East Asian governments in pursuing sensible macroeconomic policies, authoritarian leaders effectively insulated meritocratically selected civil servants from direct popular pressures (World Bank 1993b). This view also recognizes that authoritarian leaders in other contexts have pursued disastrous policies.

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A relatively small amount of literature addresses *how well* government accomplishes its objectives. A recent set of papers uses private-service ratings for foreign investors to analyze government efficacy. Mauro (1995) examines the impact of various measures of institutional quality and suggests that corruption is associated with lower economic growth, primarily by reducing investment. Knack and Keefer (1995) find significant negative effects of the overall quality of government on economic outcomes. Chong and Calderón (1996) explore the connection between these same institutional quality indexes and economic inequality.

We focus not on the impact of the how or how well of government policies on aggregate economic outcomes, but on the connection between how and how well. In particular, we examine the link between civil liberties and democracy—critical determinants of how governments exercise public decisions and authority—and the efficacy of public investments.

Why might economists expect such a link? In competitive markets, shareholders and consumers goad managers of private corporate firms—with separate management and ownership—into efficacy. In competitive markets, profit-maximizing shareholders can choose alternative investments, and discriminating buyers can choose alternative suppliers. By contrast, shareholders and consumers do not pressure publicly vested authorities through these channels of choice. Citizens cannot freely choose to own shares of another country. Citizen preferences are not linked to revenues for government services, because taxation is ultimately coercive. Accordingly, other channels induce government performance, including accountability, openness, transparency, predictability, and the rule of law (Brautigam 1992). In Hirschman's evocative phrase, while markets create managerial discipline and induce efficacy through the exercise of choice, governments are principally disciplined through the exercise of voice (Hirschman 1970).

However, very few empirical studies have documented the link between citizen voice—facilitated by openness—and accountability and performance (Paul 1992, 1994, and 1996). Comparing the performance of public irrigation systems in India and Korea, Wade (1994) finds that when irrigation officials face more local connections and accountability, the systems perform better than traditional arrangements that insulate civil servants from performance pressures. Drèze and Sen (1989) argue that no country with a free press has ever had a major famine. They postulate that a free flow of information pressures (even nondemocratic) governments into public action. Literature on the involvement of potential beneficiaries in government-financed investment projects also suggests the importance of citizen voice (World Bank 1995 and Korten and Siy 1988). For instance, Isham, Narayan, and Pritchett (1995) show that aid-financed rural water supply projects performed much better with greater participation of the beneficiaries. Overall, these results suggest that citizen voice is an important determinant of government accountability and efficacy but do not identify the underlying social and political conditions conducive to citizen voice.

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This unexplored chain of reasoning—from social and political conditions to citizen voice to government efficacy—frames the key hypothesis explored here. We hypothesize that basic civil liberties—such as the freedom of individual expression, a pluralistic and free media, the ability of groups to organize, and freedom of dissent and criticism—facilitate greater citizen voice and hence more effective government action. We also consider whether citizen voice requires (or is enhanced by) democracy. For example the country that Wade (1994) argues had less public sector accountability (India) was clearly more democratic.

II. Project Performance as an Indicator of Government Efficacy

Conceptual and practical difficulties explain most of the lack of cross-national research on determinants of the efficacy of government action. Deep conceptual disagreements about *what* governments ought to do, including the objectives that governments ought to pursue and the appropriate means to achieve those objectives, plague the efforts to measure efficacy. These differences imply that efficacy cannot be inferred from the success or failure in achieving measured aggregate outcomes like economic growth. Mistaken beliefs may cause government to pursue policies that are inefficient, or even counterproductive, relative to its ultimate objectives. For instance, many governments have actively and deliberately discouraged many types of foreign investment. Whether that policy has been effectively implemented is a distinct question from whether it has promoted the desired outcomes.

In addition, a practical difficulty hinders the analysis. Nearly all data concerning government actions concern public resources spent on *inputs*, not comparable *outcomes*. The data document finances allocated for roads, but not roads built, and spending on health clinics, but not health outcomes. Nearly every government supports education in a roughly similar way and collects a fair amount of data on education *spending*. But analysts cannot compare cross-country efficacy without comparable measures of student learning that are extremely rare in developing countries. Overall, because governments do not spend money equally effectively, we can learn very little from input data alone, and certainly nothing about government efficacy (Pritchett 1996). For example, Putnam (1993) recognizes this problem and devises his own measures of government efficacy for assessing the performance of regional governments in Italy, where the scope of regional government responsibility is assigned.

Our data provide an opportunity to overcome these conceptual and practical obstacles. The data rate on a comparable quantitative scale the success of investment projects that governments have chosen to undertake. We use the economic rate of return (ERR) as an indicator of outcomes (not just expenditures) calculated similarly for all countries. Moreover, we do not compare the amounts different governments chose to invest, either in total or in distribution across sectors. Rather we compare returns on government investments. The data also have the advantage of being microeconomic and hence much less susceptible to

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argument about reverse causation. Although the level of economic growth could affect the level of civil liberties, we find it unlikely that the returns on individual projects would affect the level of civil liberties.

The Data

The World Bank's Operations Evaluation Department (OED) constructed our data on the performance of government investment projects financed by the World Bank, including both loans from the International Bank for Reconstruction and Development (IBRD) and credits from the International Development Association (IDA). We exclude adjustment (or program) lending from our analysis, because it raises a large set of problems with evaluation, which have been addressed on several occasions both by the World Bank and its staff (Pritchett and Summers 1993) as well as by other less sympathetic analysts. After full disbursement of each World Bank loan—typically five to eight years after the opening of the loan—staff from the World Bank and borrower country jointly write a project completion report assessing project performance. The project completion report, or implementation completion report, is usually written by a staff member in the World Bank division that supervised the loan, but typically not by anyone with major project approval responsibilities. This practice minimizes the incentives to dissemble about project performance. As part of project assessment, OED staff judge each project as satisfactory or unsatisfactory in achieving its development objectives.

In addition, for projects in eight economic subsectors with readily quantified and valued project benefits—infrastructure, agriculture, industry, energy, water, urban development, transport, and tourism—project staff, sometimes in collaboration with OED, calculate an ERR. The ERR is the discounted stream of project costs and benefits over the life of the project, evaluated at economic (as opposed to financial) prices and calculated following (roughly) the methodology of Squire and van der Tak (1975). (See Little and Mirrlees 1991 for a discussion of economic pricing in World Bank appraisals and the quality of cost-benefit analysis overall.) The OED staff calculate the ERRs after project completion (ex post) in contrast to the ex ante ERRs computed as part of the internal World Bank procedures for project approval. Ex ante and ex post calculations of the ERRs differ by an enormous gap (6–10 percentage points on average). The gap has a huge variability: regressing ex post on ex ante ERRs results in an R^2 of only about 0.2. Pohl and Mihaljek (1992) study the determinants of this gap. Follow-up studies tend to find that even the ex post ERRs tend to overstate the true economic rate of return because in many cases projects do not sustain the benefit flows as long as anticipated in the ex post ERR calculations. For ex post ERRs evaluators know actual implementation costs and have somewhat more information about actual operating costs and demand, but must still estimate most of the future stream of benefits.

Government Efficacy

Are the rates of return on government investment projects a reasonable proxy for government efficacy? To find the answer, we address two issues. First we

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evaluate the reliability and representativeness of the sample of World Bank–financed projects. Second, we distinguish the impact of civil liberties on government efficacy from other country- and project-level determinants of project performance.

PROJECT ERRS AS AN EFFICACY INDICATOR. Suppose we know the ERR on every government project j undertaken in country i in period t, ERR^{jit}. Then we could calculate the average ERR simply by averaging over all projects. But we do not know the ERRs for all projects in any country, much less for all projects in many countries. We can, however, observe the ERR on the subset of projects financed by the World Bank.

Statistical inference based on this sample is difficult for three reasons. First, although our sample contains an absolutely large number of projects, the median number of projects per country is only 9 (average 13.5). Therefore, the average of these few projects is at best a very noisy indicator of a country average. Second, projects financed by the World Bank represent only a small fraction of most governments' investments. In our sample the average ratio of World Bank disbursements to government investment is just 6 percent. Third, there is a great deal of within-country heterogeneity in project returns (between-country variation in ERRs accounts for only 13 percent of the total ERR variance), while there is very little variance over time in country conditions like civil liberties. This combination implies that the governance variables (many of which are for a single point in time in any case) do not vary sufficiently to allow country fixed-effects estimation. If we have a representative sample, however, these problems merely stack the deck against us; these problems create low explanatory power and large standard errors. Thus, the results will reveal whether we can overcome these problems.

The present empirical exercise does not focus on the representativeness of the sample, because World Bank involvement in the project may raise the ERR (compared with other government projects) through increased attention and resources. Instead, we investigate the potential relationship between ERRs and civil liberties that is specific to World Bank–financed projects. A simple growth accounting relationship allows us to estimate the relationship between overall returns to capital and our sample of ERRs. The regression results suggest that ERRs are representative of economywide (not just government) returns.

If the difference in performance of World Bank–financed projects compared with the government portfolio depends on a country's civil liberties, then a sample selection bias exists. This bias could happen for two reasons. Countries can choose which of their possible projects to finance through the World Bank. Thus the first reason for potential bias is that this choice may involve cream skimming, in which governments seek World Bank financing for projects with very high expected ERRs. Or, second, it may involve laggard dumping, in which governments offer the World Bank the most problematic projects and finance the best projects out of their own budget. In addition to

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these potential causes for bias, the country's civil liberties could affect the World Bank's selection of projects. The World Bank as a development institution invests in a wide variety of investment climates subject to the projects' meeting some minimum criteria. We return to this selection problem below in discussing the empirical results.

OTHER DETERMINANTS OF PROJECT PERFORMANCE. Even if the sample is representative of the returns on the projects in the government's investment portfolio, many factors influence the realized return other than government efficacy. We can think of a schedule of projects as a frontier of potential or achievable project returns from which the government chooses a subset. Economywide and project-specific factors determine the location of this schedule of returns (Isham and Kaufmann 1992, 1995 and Kaufmann and Wang 1995). We identify the possible returns so that we can identify the deviations from this potential as an indicator of government performance.

The lack of a strong correlation between the ERRs and other possible measures of project performance augurs against an interpretation of ERRs as an indicator of government efficacy. For instance, the "Business Environmental Risk Intelligence" and "International Country Risk Guide" rank countries by various characteristics that indicate their attractiveness for foreign investment. These various measures are not significantly correlated with the ERRs in our data set (although they do show a reasonable correlation with the civil liberties variables). In part the lack of correlation might occur because these private sector ratings are flawed indicators of government effectiveness, as they are designed for foreign investors. Governments that are not attractive to foreign investors on these criteria might still be reasonably effective in implementing their own projects.

The basic unit of observation in the data is the project, implemented in a specific country over a specific period. Prior to adding any indicator of civil liberties to our analysis, we specify an equation that relates the ERR^{jit} to three sets of nongovernance variables: sectoral dummies, country characteristics, and regional dummies. We report the results on these control variables in table A-1. An annual publication by OED on evaluation results uses these data to examine project performance by a number of characteristics (see World Bank 1993a). Background papers for the most recent publication also examine the effect of various country aggregate variables, including inflation and World Bank–specific inputs, like supervision activity (Kilby 1995).

Three econometric issues deserve mention. First, the time-varying variables, such as the black market premium, must be matched to the period relevant to project performance. While the arguments can be made in favor of various weights (such as disbursement profile weights), we use a three-year weighted average of the time-varying variable, going back from the year in which the project evaluation was done. Second, although the projects vary tremendously in total cost, from $1.7 million to more than $1 billion, the standard tests do not indicate any

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conditional heteroscedasticity as a function of project size, nor does weighting ordinary least squares (OLS) estimates by project size affect the results. Third, by OED convention the lowest ERR reported is negative 5 percent, which implies that the data are truncated from below; hence, the reported regression results use Tobit estimation unless otherwise noted. However, because only 8.4 percent of the sample is at the truncation point (−5), the Tobit estimates are quite similar to simple OLS estimates (Greene 1981). It is hard to believe that much is gained (or lost) by using Tobit estimates.

We include a set of sectoral dummy variables because the sectors differ substantially in their ability and in their techniques for assessing the ERR. By including the sector dummies, the differing patterns of sectoral investment across countries do not affect the estimates of the other parameters. We also include a dummy variable for project complexity, which accounts for a subset of agricultural projects, including all integrated rural development, irrigation and drainage, and livestock projects, which presented some particular difficulties (World Bank 1988). Our estimates reconfirm that the ERRs for these projects are about 4 percentage points lower on average.

We include a set of time-varying country characteristics that potentially determine returns. We use the economywide capital-labor ratio because a higher capital-labor ratio lowers the potential return on capital. Our estimates confirm this relation: a unit increase in the natural log of the capital-labor ratio reduces the ERR by between 1 and 1.6 percentage points (table A-1). We use the terms of trade because many analysts suspect that terms of trade shocks determine project returns, both in the affected sector and in the economy as a whole. We do not find a particularly large or significant effect. Policy and outcome variables also potentially influence returns. We consider the black market premium to be an omnibus indicator of distorted policies because it is associated with overvalued exchange rates, trade distortions, and macroeconomic instability, all of which have a strong negative impact on ERRs. Even accounting for the black market premium, projects do better in countries with a larger fiscal surplus. We expected that gross domestic product (GDP) growth would also have a large impact on returns, but the effect is modest.

We also include a set of regional dummies based on the World Bank groupings for Latin America and the Caribbean, Sub-Saharan Africa, South Asia, East Asia, and Europe, the Middle East, and North Africa. We find as expected that projects in Sub-Saharan Africa do much worse (10 percentage points), projects in Latin America and the Caribbean and in Europe, the Middle East, and North Africa do about 5 percentage points worse, and countries in East Asia (which includes in addition to the high-performing East Asian countries, the underperforming Southeast Asian and Pacific countries) do about 3 percentage points worse (table A-1). The inclusion of the regional controls does have a significant impact on the estimates of other variables, so in all subsequent tables we report regressions with and without regional controls.

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III. Civil Liberties, Project Performance, and Democracy

Our results here are similar to those reported by Isham and Kaufmann (1995), who argue that many variables, such as policy distortions, affect both public and private sector projects. We ask whether civil liberties have an additional effect on project performance if we control for the set of project and country factors. We describe four measures of basic civil liberties that are relevant to the ability of citizens to exercise voice and present the results of including these measures as determinants of ERRs. We then look at the robustness of the relation between civil liberties and ERRs using a wide variety of controls, including measures of democracy.

Measuring Civil Liberties

Freedom House (1994) publishes a ranking of civil liberties on a scale of 1 to 7 for 165 countries from 1972 to 1994 based on a checklist of 14 civil liberties. The checklist includes media free of censorship, open public discussion, freedom of assembly and demonstration, freedom of political organization, nondiscriminatory rule of law in politically relevant cases, freedom from unjustified political terror, free trade unions and peasant organizations, free businesses and cooperatives, free professional and other private organizations, free religious institutions, personal social rights (for example, the right to own property and to travel internally and externally), socioeconomic rights, freedom from gross socioeconomic inequality, and freedom from gross government indifference or corruption. Humana (1986) ranks human rights achievement in 89 countries for 1985 on a scale of 0 to 100 (the actual range for our sample is 13 to 91) based on the definition of human rights adopted by the General Assembly of the United Nations in 1966 under the International Covenant on Civil and Political Rights. The Humana index includes such items as the right of peaceful assembly, freedom of opinion and expression, the right and opportunity to take part in the conduct of public affairs, the right to freedom of opinion and expression, and the right to form trade unions. Coppedge and Reinicke (1990) rank 170 countries on two dimensions—media pluralism and freedom to organize—on a scale of 1 to 3 for the year 1985.

Creating a reliable empirical cross-country indicator of civil liberties is obviously difficult, and any measure will be subjective and hence debatable. But the actual differences across countries in liberties are so large that, in spite of the complexity and subtleties, any reasonable assessment will produce the same basic pattern across countries. This result is indicated by the high correlations among these measures of civil liberties. The correlation of the Freedom House index (averaged over 1979–86) with the Humana index is 0.83, with freedom to organize, 0.78, and with media pluralism, 0.81. The correlation of the Humana index with freedom to organize is 0.68, and with media pluralism, 0.79. The correlation of freedom to organize with media pluralism is 0.82. (Coppedge and Reinicke's use of the information in the Freedom House and Humana studies in

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their own ranking procedure may account for at least part of the high correlation between the latter two and former two series.)

Civil Liberties and Project Performance

Each of the four measures of civil liberties shows a statistically significant and empirically large association with the return to projects (table 1). The estimates that include regional dummy variables suggest that if the Freedom House civil liberties index improved from that for the worst country (1) to that for the best (7, as in Costa Rica), the ERR would be predicted to increase 8 percentage points, 50 percent of the mean ERR of 16. Similarly, improving from the least civil liberties by the Humana index (13) to one of the best (91, again, Costa Rica) would improve the ERR by an amazing 20 percentage points.

Each of the civil liberties indexes and other determinants of project performance differs in scale. Therefore, to compare the different effects, we calculate the predicted increase in the ERR if each index were improved by 1 standard deviation (column 3 of table 1). A standard deviation improvement in civil liberties would raise the predicted ERR 1.9 points using the Freedom House index, 4.5 points using the Humana index, and 2.6 points using the media pluralism

Table 1. *The Impact of Civil Liberties Indicators on the Economic Rate of Return of Government Projects, Controlling for Economic and Project Variables*

Index	*Without regional variables*	*With regional variables*	*Effect of a 1 standard deviation increase in civil liberties on the economic rate of return*[a] *(percentage points)*
Freedom House civil liberties, 1978–87	1.95 (0.000)*	1.32 (0.047)*	1.9
Humana, 1982–85	0.251 (0.009)*	0.256 (0.025)*	4.5
Media pluralism, 1983–87	2.89 (0.013)*	2.85 (0.062)**	2.6
Freedom to organize, 1983–87	2.45 (0.006)*	−0.057 (0.969)	2.7[b]

* *p*-level less than 0.05.

** *p*-level less than 0.10.

Note: The base specification includes capital-labor ratio, black market premium, GDP growth, fiscal surplus, terms of trade changes, sectoral dummies, and a dummy for complex projects (see table A-1). The estimation is based on annual values for 1978–87 for the Freedom House civil liberties index. For the other three indexes, single values were extrapolated to cover the sample period. We report *p*-levels of the test for whether the coefficient is 0 rather than test statistics themselves. The *p*-level is the significance level at which the null hypothesis can be rejected, hence a *p*-level less than 0.05 indicates a rejection of the null hypothesis at (at least) the 5 percent level. The *p*-levels are in parentheses. Sample sizes are 649 for the Freedom House civil liberties index, 236 for the Humana index, 389 for media pluralism, and 389 for freedom to organize.

a. The standard deviations—for the entire sample for which each variable is available—are 1.47 for the Freedom House civil liberties index, 17.8 for the Humana index, 0.91 for media pluralism, and 1.12 for freedom to organize.

b. Using the estimate without regional dummies.

Source: Authors' calculations.

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index. These effects of civil liberties on project returns are empirically large compared with those of macroeconomic policy, an effect that has received a great deal of attention (World Bank 1991). The average of the standardized effect of the four civil liberties indicators on project returns (2.9 percentage points) is much larger than equivalent changes in terms of trade shocks, fiscal deficits, or GDP growth (column 4 of table A-1). Improving civil liberties by a standard deviation would improve project performance by about as much as a standard deviation fall in the black market premium (3.31, table A-1). Although the total effect of good macroeconomic policies is larger (as the effects are additive), clearly civil liberties are as important as any other single determinant of project success.

The relationship between civil liberties and ERRs is the central positive finding of this article. We show that this result is robust to outliers, to the measure of project performance, to possible financing selection effects, and to the inclusion of other variables in the base specification. Of special interest, the inclusion of indicators of political liberties or democracy does not shift the estimates of the importance of civil liberties.

OUTLIERS. A concern with any econometric result is its sensitivity to a few observations. Although the civil liberties indicators we use are bounded, some projects have extreme values for the ERR (the maximum is 155, table A-2). We address the robustness of the estimates to extreme observations and influential data points in two ways. First, we estimate a Tobit specification with lower and upper truncation. Censoring the ERRs above at 50 percent (roughly 2 standard deviations above the mean) does not affect the results. Second, we estimate the same specifications using quantile (median) regression, a technique that is much more robust to extreme observations than Tobit estimates. All the civil liberties variables that are significant in table 1 are also significant using median regressions.

PROJECT PERFORMANCE INDICATOR. The results are not unique to the ERR. If we use the binary "satisfactory or unsatisfactory" rating created by OED, we obtain qualitatively similar results. We have a larger sample of projects using only this rating as the measure of project performance because we include social sector projects that normally do not receive an ERR. (See Kaufmann and Wang 1995 for a discussion of the performance of social sector projects as a function of macroeconomic policies.) Table 2 reports the estimates of a Probit regression. Naturally, because the binary indicator discards a great deal of statistical information, we obtain less precise results: the p-levels are generally higher, and the estimates for the Humana ranking are even insignificant.[1] Nevertheless, the other variables show large increases in the likelihood of a successful project when implemented in countries with higher civil liberties.

1. In the tables we report p-levels of the test whether the coefficient is 0 rather than test statistics themselves. The p-level is the significance level at which the null hypothesis can be rejected, hence a p-level less than 0.05 indicates a rejection of the null hypothesis at (at least) the 5 percent level.

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Table 2. *The Impact of Civil Liberties on the Probability of a Project Being Rated Satisfactory Using a Probit Regression, Controlling for Economic and Project Variables*

Index	*Without regional variables*	*With regional variables*	*Effect of a 1 standard deviation increase on the probability of project success*[a] *(percentage points)*
Freedom House civil liberties, 1978–90	0.018 (0.056)*	0.022 (0.060)*	3.2
Humana, 1982–86	−0.00067 (0.589)	0.0012 (0.388)	2.1
Media pluralism, 1983–90	0.022 (0.296)	0.054 (0.045)*	4.9
Freedom to organize, 1983–90	0.042 (0.009)*	0.040 (0.085)**	4.5

* *p*-level less than 0.05.

** *p*-level less than 0.10.

Note: The value reported is not the coefficient in the Probit regression, but the marginal change in the probability of a successful project as the variable changes, evaluated at the means of all independent variables. See table A-1 for the complete specification. The estimation is based on annual values for 1978–87 for the Freedom House civil liberties index. For the other three indexes, single values are extrapolated to cover the sample period. Sample sizes are 1,155 for the Freedom House civil liberties index, 604 for the Humana index, 740 for media pluralism, and 740 for freedom to organize. The *p*-levels of the test for whether the Probit coefficient is 0 are in parentheses; note that this is not the same as the *p*-level of the statistic reported.

a. The standard deviations—for the entire sample for which each variable is available—are 1.47 for the Freedom House civil liberties index, 17.8 for the Humana index, 0.91 for media pluralism, and 1.12 for freedom to organize.

Source: Authors' calculations.

For instance, from table 2 using the mean of the Freedom House variable, an increase of 1 standard deviation in civil liberties lowers the probability of a failed project 3.2 percentage points, which reduces the predicted failure rate 16 percent (from the mean failure of 20 percent). Similarly, an increase of 1 standard deviation in media pluralism reduces the failure rate almost 5 percentage points, or 25 percent (table 2).[2]

SELECTION EFFECTS. Do selection effects create the relationship between civil liberties and the performance of World Bank–financed projects? We consider two perspectives on World Bank project selection decisions. One interpretation says that the World Bank's Articles of Agreement preclude explicit consideration of noneconomic factors, particularly civil liberties or political factors, in the selection of World Bank projects. This view suggests that World Bank project selection should be uncorrelated with civil liberties. We create an indicator to measure World Bank involvement in a country's investment as the ratio of World Bank loan or credit disbursements to total government investment. We find a *negative* bivariate correlation between World Bank involvement and civil liberties, principally because the World Bank has greater involvement in poorer countries,

2. It takes some calculations to compare, but the magnitudes of the effects are roughly similar to those with ERRs.

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which on average have fewer civil liberties. Controlling for per capita income and population, we find no correlation between World Bank involvement and civil liberties using the Freedom House index or the media pluralism index. We find only mildly positive correlation between World Bank involvement and civil liberties using the Humana index (p-level, 0.07). If we introduce World Bank involvement as an independent explanatory variable, we obtain reasonably robust results, in that the sign and magnitude of the effects are roughly similar in all regressions and the statistical significance is maintained except in some of the regressions with regional dummies.

The second perspective says that decisions concerning World Bank project selection are based on the ex ante ERRs, not the ex post ERRs (which are obviously available only after project completion). If World Bank project selection causes the partial association of civil liberties and ERRs, the association should appear in the ex ante ERRs. However, when we use the ex ante ERR as the dependent variable, we find no relationship with the Humana index (p-level, 0.98), no relationship with the media pluralism index (p-level, 0.59), and a modest negative relationship with the Freedom House index (p-level, 0.10). These results suggest that differences in the implementation of the projects, not differences in project selection for financing, cause the relation between project performance and civil liberties.

OMITTED VARIABLES. The partial association between civil liberties and ERRs is robust. We explore the possibility, however, that some other variable is associated with both project performance and civil liberties and hence that the partial association of civil liberties is an artifact of bias from an omitted variable. This possibility has two versions: an incidental association between the omitted variable and civil liberties, or, much worse, civil liberties as a proxy for the true omitted variable. We address these concerns in turn, with a separate section devoted to the impact of civil liberties and democracy on project performance.

The specification of the variables included in the general specification for project performance is not tightly theoretically constrained. Our analysis concerns the robustness of the project performance result. Therefore, we experiment with "data undermining" by searching for variables whose inclusion changes the civil liberties results. Besides those variables reported in our base specification in table A-1, we experimented with the inclusion of other variables. We tried the stock of education because greater human capital perhaps led to higher returns. We tried an indicator for trade policy because results by López (1995) suggest an interaction between trade and returns to capital. We tried ethnolinguistic fractionalization, which Easterly and Levine (1996) show is associated with good economic outcomes and good government policies. And we tried dummy variables for whether the country gained independence from France, Spain, or the United Kingdom and for the year the country gained independence; Chong and Calderón (1996) argue that these factors have a lasting effect on government institutional arrangements. We also added a dummy variable for

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IDA credits (as separate from IBRD loans) and found no difference. Although each of these variables is plausibly correlated with both civil liberties and government efficacy, their inclusion in the project performance equation did not substantially alter the magnitude or significance of the civil liberties coefficient.[3]

Civil Liberties and Democracy

By far the most important question on robustness is whether the results reflect civil liberties or capture some more directly political element. Civil and political liberties and more democratic political regimes are closely associated with each other, both of necessity (a certain degree of civil liberties is a precondition for democracy) and in practice. Yet there are clear analytical and practical distinctions between civil liberties and more strictly political rights and practices. In particular, the degree of civil and political liberties varies widely among nondemocracies. At the extremes, totalitarian regimes clearly differ from authoritarian regimes in the degree to which the regime attempts to control nonpolitical dimensions of society and in the degree to which it tolerates opposition, criticism, and dissent. Therefore, finding an association between more civil liberties and better ERRs does not imply an association between different types of political regimes and better performance. Here we explore the association between ERRs and political liberties and type of political regime.

As with civil liberties, measuring and classifying political regimes raises substantial difficulties. The most widely used measure of democracy in the economic literature is the Freedom House index of political liberties, a subjective ranking from 1 to 7 based on 11 indicators of political rights: chief authority recently elected by a meaningful process; legislature recently elected by a meaningful process; fair election laws; fair reflection of voter preference in distribution of power; multiple political parties; recent shifts in power through elections; significant opposition vote; freedom from domination by the military, foreign powers, and other powerful groups; no major group or groups denied reasonable self-determination; decentralized political power; and informal consensus (de facto opposition power). Alesina and others (1992) construct another index of type of political regime that provides an annual ranking for 1982–94 for 43 countries by democratic status on a three-point scale. The complexity of the classification of political systems does not impede a reasonably reliable cross-national ranking of countries. The correlation of the Freedom House political liberties index with Alesina's democracy index is 0.69.

When we include the indicators of civil liberties in the equation for project performance together with indicators of democracy, the civil liberties indicators retain all of their importance, while the democracy indicators do not have any additional explanatory power (see table 3). The Freedom House political liberties variable shows a weak association alone, and when combined with a civil liberties variable it is consistently *negative* (sometimes significantly so). Simi-

3. Although the results on some of these variables might be of independent interest, we do not report the results because we do not want to appear to be mining the data.

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Table 3. *The Impact of Civil Liberties and Democracy Variables on the Economic Rate of Return of Government Projects*

		Civil liberties indicator		
Variable	*No civil liberties indicator*	*Freedom House civil liberties index*	*Humana index*	*Media pluralism*
Freedom House political liberties index	0.138 (0.805)	−2.08 (0.025)*	−0.798 (0.526)	−0.594 (0.175)
Civil liberties indicator		3.39 (0.003)*	0.297 (0.024)*	3.41 (0.062)**
Alesina democracy index = 2 (medium democratic)	3.61 (0.163)	3.96 (0.135)	5.77 (0.159)	6.03 (0.088)**
Alesina democracy index = 1 (most democratic)	0.651 (0.757)	0.989 (0.638)	−0.376 (0.921)	2.51 (0.434)
Civil liberties indicator		1.25 (0.081)**	0.271 (0.023)*	2.67 (0.082)**
Sample size				
Using the Freedom House political liberties index	649	649	236	448
Using the Alesina democracy index	372	372	236	448

* *p*-level less than 0.05.
** *p*-level less than 0.10.
Note: The base case regression is as in table 1 including the regional dummies (see table A-1). We report *p*-levels of the test whether the coefficient is 0 rather than test statistics themselves. The *p*-level is the significance level at which the null hypothesis can be rejected, hence a *p*-level less than 0.05 indicates a rejection of the null hypothesis at (at least) the 5 percent level. The *p*-levels are in parentheses.
Source: Authors' calculations.

larly, including Alesina's democracy index has no impact on the estimates of the impact of civil liberties.[4] We do not place much importance on these negative results on democracy because the two variables, civil liberties and political regime, move closely together (the correlation of the civil and political Freedom House variables is 0.89). Their closeness creates both statistical and interpretational problems, but the civil liberties variable is not a proxy for democracy.

More important than the statistical concerns is the problem of practical interpretation. Because the civil and political liberties variables typically move in tandem, the question of the impact of changing civil liberties without changing democracy may not be practically relevant. Nearly every policy change that changes civil liberties is likely to have as its natural counterpart a political change as well. Hence the usual ceteris paribus assumption—that all else (particularly political liberties) remains the same—in assessing shifts in civil liberties is inappropriate and should be replaced with an assumption that the two variables move together. The results from column 2 of table 3 (in which the civil and

4. This finding is robust to the use of other indicators of democracy. We also used an indicator of type of political regime created by the IRIS center at the University of Maryland and an indicator of fair elections from Coppedge and Reinicke (1990). Using these indicators gave similar results of no partial impact of democracy and unchanged estimates on civil liberties.

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political Freedom House variables have the same scale) show that if we increase both civil and political variables by 1 (on the common scale of 1 to 7), the ERR would increase about 1.31 percentage points. The regression suggests that this net effect is due to a large positive effect of civil liberties (3.39) offset by a large negative political effect (−2.08). Most important, the joint shift (1.31) is of the same magnitude of the shift in civil liberties alone (1.32) estimated from table 1. This result suggests that the total effect of an improvement in civil liberties is positive, even accounting for the induced political changes.

IV. Civil Liberties, Civil Strife, and Project Performance

In the data, an interesting interrelationship among civil liberties, civil strife, and project performance suggests that the possible mechanism of causation is from more civil liberties to increased citizen voice to better projects. After controlling for population, higher indicators of some types of civil strife, such as an increased number of riots, protest demonstrations, and strikes, are strongly *positively* correlated with project performance (table 4). High ERR countries have average rates of return twice as high (22.2) as low ERR countries (11.2). High ERR countries have many more riots, demonstrations, and political strikes per capita (adjusted for population) than countries with poor project performance.

The civil unrest variables (riots, protest demonstrations, and strikes) come as the number of incidents per country per year (Banks 1979, updates). This means that countries with larger populations have a greater absolute number of incidents. However, it does not seem right simply to normalize to per capita, as there are plausibly some increasing returns to scale in civil unrest. Consequently, for each of the three variables we regress the absolute number of incidents on population*ln(population), which is equivalent to adjusting the per capita level for the total population in semilog form. We report the residual of this regression as excess civil unrest over the amount expected for a given level of population. The population adjustment is also very significant, and the R^2 varies from 0.02 (strikes) to 0.18 (riots). The results reported below were unchanged by using other concave functional forms in place of this semilog form.

That greater civil tension is associated with *better* projects might appear puzzling. Typically, analysts associate all forms of political and social instability with *worse* investment climate. They base this reasoning on associating civil strife with risks to private projects and with political instability. In our analysis, governments finance all the projects. We tried including as separate regressors indicators of political instability, such as the Taylor and Jodice (1983 and supplements) series on irregular government transfers and an index by Alesina and Perotti (1993) on sociopolitical instability, but neither had any impact on project success or the civil liberties variables.

Some degree of civil tension reflects a citizen's ability to agitate and influence government's behavior without negative repercussions, a mechanism that plausibly leads to greater accountability and hence better choice and implementation

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Table 4. *The Impact of Civil Strife Variables on the Economic Rate of Return of Government Projects*

	Civil strife indicator[a]		
Impact measure	*Riots*	*Political strikes*	*Protest demonstrations*
Deviation of civil strife from population-adjusted level[b]			
High-ERR countries[c]	2.48	3.19	0.30
Medium-ERR countries[d]	0	−0.02	0.16
Low-ERR countries[e]	−0.19	−0.23	−0.04
Correlation of population-adjusted level of civil strife with Freedom House civil liberties index	0.27 (0.000)*	0.34 (0.000)*	0.17 (0.000)*
Estimates of the project performance regression[f]			
Without the Freedom House civil liberties index	0.42 (0.040)*	1.67 (0.097)**	0.81 (0.003)*
With the Freedom House civil liberties index	0.21 (0.34)	0.45 (0.683)	0.68 (0.013)*

* *p*-level less than 0.05.

** *p*-level less than 0.10.

Note: We report *p*-levels of the test whether the coefficient is 0 rather than test statistics themselves. The *p*-level is the significance level at which the null hypothesis can be rejected, hence a *p*-level less than 0.05 indicates a rejection of the null hypothesis at (at least) the 5 percent level. The *p*-levels are in parentheses. The sample size is 649.

a. Values for the civil strife indicators are per capita, adjusted for total population size. We regress the absolute number of incidents on population*ln(population), which is equivalent to adjusting the per capita level for the total population in semilog form, and report the residual of this regression as excess civil unrest over the amount expected for a given level of population. The population adjustment is also very significant, and the *R*-squared varies from 0.02 (strikes) to 0.18 (riots).

b. ERR categories are determined by average rates of return classified by country for all countries with at least 10 projects for 1974–87.

c. There are six high-ERR countries, three in South Asia and three in East Asia. The average ERR for the high-ERR countries is 22.2.

d. There are 11 medium-ERR countries, five in Latin America and the Caribbean; two in Sub-Saharan Africa; three in Europe, the Middle East, and North Africa; and one in South Asia. The average ERR for the medium-ERR countries is 17.

e. There are 12 low-ERR countries, nine in Sub-Saharan Africa, two in Latin America and the Caribbean, and one in South Asia. The average ERR for the low-ERR countries is 11.2

f. This is the base regression (see table A-1) without sectoral or regional dummies.

Source: Authors' calculations.

of projects. Indeed, table 4 shows that higher civil liberties are strongly associated with higher levels of riots, demonstrations, and political strikes (although regional dummy variables sharply attenuate this effect). Table 4 also shows, even controlling for our set of exogenous and policy variables, a positive and significant relation between the ERR and the number of riots, protest demonstrations, and political strikes. However, adding the degree of civil liberties sharply reduces the estimated impact of political manifestations: the coefficient on riots falls from 0.42 to 0.21, and the coefficient on strikes falls from 1.67 to 0.45.

For a given level of civil liberties, neither riots nor political strikes are associated with better performance (although the protest demonstrations variable does retain some effect). The results support a chain of causation that runs from greater civil liberties to higher levels of citizen involvement and political partici-

pation—including as one dimension civil manifestations—to better projects. Environments that allow civil strife or unrest to occur also allow other mechanisms for expression of popular (dis)content with government performance. The availability and effectiveness of those mechanisms improve government efficacy.

V. Conclusions

The extent of a country's civil liberties has a substantial impact on the successful implementation of government investment projects financed by the World Bank. This impact of civil liberties is as empirically large as the more celebrated impact of economic distortions on project returns. Given that citizen voice is an important precondition for government accountability and, not coincidentally, that voice is suppressed in the absence of civil rights, this result is perhaps not surprising. This result adds to the evidence for the view that increasing citizen voice and public accountability—through both participation and better governance—can lead to greater efficacy in government action. Some analysts argue that there is a trade-off between liberties and development. We find the opposite evidence, that suppressing liberties is likely to be inimical to government performance. This has obvious implications not just for governments but also for development assistance (Picciotto 1995 and OECD 1995).

The most important aspects of civil liberties and political regimes go beyond whether they promote or discourage economic outcomes. Here we have examined the instrumental value of civil liberties and political structure in producing greater efficacy of government. Although we have focused on the instrumental value, we want to emphasize that we believe government respect for civil liberties is valuable regardless of its instrumental economic value.

(*Appendix tables begin on the following page.*)

Table A-1. *Base Specification for the Nongovernance Determinants of the Economic Rate of Return of Government Projects, 1974–87*

				With regional dummies	
Variable	*Mean*[a]	*Number of dummy variables*	*Estimate without regional dummies*	*Estimate*	*Effect of a 1 standard deviation increase on the economic rate of return*
Exogenous variable					
ln(capital/labor)	8.22		−1.09	−1.66	−1.67
	[1.01]		(0.067)**	(0.060)**	
Dummy for project complexity		319	−4.29	−4.23	
			(0.017)*	(0.016)*	
Terms of trade shock	−3.29		0.0015	0.001	0.0035
	[3.35]		(0.889)	(0.922)	
Policy variable					
Black market premia	46.6		−0.046	−0.037	−3.31
	[89.5]		(0.000)*	(0.000)*	
Fiscal surplus	−5.21		0.197	0.266	0.925
	[3.48]		(0.149)	(0.063)**	
GDP growth	3.71		0.193	0.013	0.646
	[3.35]		(0.357)	(0.949)	
Regional dummy variable[b]					
East Asia		278		−3.33	
				(0.154)	
Latin America and the Caribbean		314		−4.74	
				(0.072)*	
Europe, the Middle East, and North Africa		283		−4.93	
				(0.100*)	
Sub-Saharan Africa		430		−10.8	
				(0.000)*	
Sectoral dummy variable[c]					
Agriculture		604	0.027	1.39	
			(0.992)	(0.602)	
Energy and public utilities		339	−3.92	−3.18	
			(0.136)	(0.220)	
Transport and tourism		413	3.85	6.24	
			(0.137)	(0.016)*	
Urban		48	10.1	11.9	
			(0.011)*	(0.003)*	

* *p*-level less than 0.05.

** *p*-level less than 0.10.

Note: We report *p*-levels of the test whether the coefficient is 0 rather than test statistics themselves. The *p*-level is the significance level at which the null hypothesis can be rejected, hence a *p*-level less than 0.05 indicates a rejection of the null hypothesis at (at least) the 5 percent level. The *p*-levels are in parentheses. The sample size is 761.

a. Standard deviations are in square brackets. Standard deviations are calculated for the entire sample.

b. South Asia (184 observations) is excluded. Regions are based on World Bank regional classifications.

c. Industry (84 observations) is excluded.

Source: Authors' calculations. For exogenous and policy variables, World Bank data.

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Table A-2. *Summary Statistics*

Variable	*Mean*	*Standard deviation*	*Range*	*Number of countries*	*Years*
Economic rate of return	16.01	15.16	−5–155	56[a]	1974–90
Civil liberties index					
Freedom House	4.68	1.47	1–7	56	1974–90
Humana	55.13	17.80	13–91	38	1986
Media pluralism	2.50	0.91	1–4	56	1985
Freedom to organize	2.45	1.12	1–4	56	1985
Political liberties index					
Freedom House	4.73	1.85	1–7	55	1974–90
Alesina	2.52	0.79	1–3	55	1974–82
Civil unrest indicator					
Riots	0.14	1.61	−3.83–17.50	56	1974–89
Protest demonstrations	0.29	1.63	−0.79–14.54	56	1974–89
Strikes	0.07	0.50	−0.43–3.50	56	1974–89

a. 1,488 projects.

Source: Authors' calculations; Freedom House (1994); Humana (1986); Alesina and others (1992); Banks (1979 and updates).

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