

CAN LAWYERS HELP PROMOTE ECONOMIC GROWTH?
EFFECTS OF CONTRACT-ENFORCEMENT EFFICIENCY ON PRIVATE CAPITAL INVESTMENT

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By

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ABSTRACT

Economists are fairly unanimous in the belief that a country's institutional environment plays a significant role in determining its prospects for economic development. This view derives both from economic theory and from an increasingly robust set of empirical findings. The strengthening economic consensus has led to significant investments in institution-building on the part of development donors, including the World Bank. But many who study and practice in this area remain less than fully satisfied with our existing level of understanding. In particular, although the macro-level evidence is very robust, there is relatively less empirical research identifying the specific pathways that link institutions to economic development. In this paper, I add to the empirical foundations of institutional-reform efforts by focusing narrowly on one aspect of institutions—the efficiency of contract-enforcement, in terms of duration and procedural complexity—and estimating its impact on one factor of growth: private capital investment. I hypothesize that relatively more efficient judicial systems will be associated with higher levels of investment, as firms will be able to benefit from contract-enforcement institutions at a lower cost. My regression analyses generally confirm this hypothesis, although I acknowledge that the results are subject to alternative interpretations and, therefore, should be taken with caution. I conclude that developing countries might spur private capital formation by addressing sources of judicial inefficiency, including long delays and unnecessary procedures.

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I. INTRODUCTION

Can lawyers help promote economic growth? Doubtless many businesspeople would say no; lawyers are the people who tell you what you *cannot* do—they constrain free enterprise through regulation, litigation, and other irritations. But, of course, lawyers are also the functionaries who design and administer the system under which business can reliably be conducted. This system goes by many names—the rule of law, or institutions, among others—and has long been recognized as an important enabling ingredient in the existence of complex market economies. Since the 1980s, economists have been developing empirical evidence focused on institutions as a determinant of economic growth (e.g., Scully 1988; Hall & Jones 1999; Acemoglu et al. 2001). This so-called new institutional economics has yielded a consensus that, at some level and in some way, institutions matter. But exactly how do they matter? And, more to the point, how can the world mobilize this knowledge to help developing countries promote economic growth?

So far, the new institutional economics has yielded many informative conclusions that emphasize the importance of institution building, but there is still much room for further elucidation of the specific mechanisms that link institutions to economic development. In this regard, the role of formal legal institutions offers a particularly important field of inquiry, because unlike “social” institutions—which are culturally embedded in a society and therefore difficult to impact—legal institutions can be the object of intervention on the part of reform-minded governments. This paper is a modest attempt to offer analysis on the practical impact of one element of legal institutions—the efficiency of contract enforcement—on an important factor of growth: private capital formation. In pursuing this analysis and drawing out implications for policy, I also take note of the important fact that any institutional reform project

must necessarily consider the tradeoffs, some delicate, which it may entail. Specifically with respect to contract-enforcement efficiency, attention must be paid to the risk of improving courts' efficiency at the cost of harming other values, such as fairness. Given that national court systems handle not only commercial contract cases, but also every other kind of legal dispute, reformers should make sure to consider this paper's findings—which, in general, support the goal of improving efficiency—in conjunction with other values served by judicial systems. With that said, in this paper I argue that developing countries might be able to positively influence private capital investment by reducing difficulties in enforcing contracts. This argument, supported by empirical evidence, provides one more piece of the puzzle in articulating the mechanisms through which institutional reforms can promote economic development.

The focus on contract enforcement is not novel. Classical economic theory has long emphasized the necessity of systems for enforcing promises between transacting agents, and some of the more recent literature has specifically examined how the strength of contract-enforcement institutions affects outcomes including capital investment and economic growth (North 1990; Clague et al. 1999; Acemoglu & Johnson 2005). My focus on contract-enforcement *efficiency*, however, is an attempt to respond to the call for empirical research that can help guide institutional reform efforts in developing countries. It is widely acknowledged that many national court systems are overburdened and unduly bureaucratic (Messick et al. 1999), and it is plausible to assume that the additional time and difficulty involved in adjudicating contract claims could hinder firms' ability to make investments that require contract protection, such as the purchase of expensive new equipment. I investigate this hypothesis by conducting several regression analyses of a worldwide cross-country panel dataset; specifically, I regress the ratio of private capital investment to GDP on two measures of contract-enforcement

efficiency, which capture both the procedural complexity and the duration involved in enforcing a commercial contract through the courts. In doing so, I hope to shed light on how the efficiency of contract institutions affects capital investment in the private sector, and thus how developing countries might be able to increase capital investment by improving the efficiency of their court systems. Given the large amount of aid money flowing into judicial reform projects, both from bilateral aid agencies and from multilateral development banks (World Bank 2012a), this knowledge could prove helpful in guiding future reform efforts.

II. BACKGROUND AND EXISTING RESEARCH

2.0—Introduction

In recent decades, economists have reached a general consensus that institutions matter to economic development (North 1990; Hall & Jones 1999; Williamson 2000; Acemoglu et al. 2001). A substantial number of researchers have even argued that institutions are the main determinant of economic development outcomes, trumping other influential factors such as geography or openness to trade (North 1990; Rodrik et al. 2004). Although not all economists share the opinion that the quality of institutions plays a central causal role in countries' economic development paths (Sachs 2003; Glaeser et al. 2004), it is fair to say that the view that institutions matter to economies—at least to some degree—has become mainstream. Moreover, even on the view of critics who argue that good quality institutions are a result, rather than a cause, of economic development, it is certainly plausible to assume that, all else being equal, extremely poor institutions could hinder the fruition of unrealized economic potential.

2.1—(New) Institutional Economics

What are institutions? Nobel laureate Douglass North defines them as “the humanly devised constraints that structure political, economic and social interaction . . . and therefore [help to] determine transaction and production costs and hence the profitability and feasibility of engaging in economic activity” (1991, p. 97). Contract institutions, in particular, enable complex markets to develop by helping to overcome major transaction costs, such as imperfect information about the trustworthiness of one’s exchange partner. By enabling trade to take place between strangers, and in situations where exchange cannot occur simultaneously, contract institutions allow for the specialization of labor that produces advanced economies. Additionally, when people have the confidence that their property is secure—both from theft or interference by other private actors as well as from government expropriation—then they will be willing to invest their energy and resources into developing it (Hall & Jones 1999). As Adam Smith himself wrote in *The Wealth of Nations*, “[c]ommerce and manufacture can seldom flourish in any state which does not enjoy a regular administration of justice, in which the people do not feel themselves secure in the possession of their property, [and] in which the faith of contracts is not supported by law” (quoted in Rodrik et al. 2004, p. 131).

Although the theoretical justification for institutions’ importance has long been accepted by economists, only recently have researchers begun to build empirical evidence to support the theory and to help explain what underlies cross-country differences in development levels (Williamson 2000). Indeed, the project of explaining the disparity between rich and poor countries—and translating these explanations into prescriptions for economic development that can lift people out of poverty—is perhaps the preeminent challenge of our time (Hall & Jones 1999; Rodrik et al. 2004; cf. Pogge 2008). Several important studies have provided firm support

for the broad proposition that institutions matter to economic growth; but narrower empirical work, focused on producing detailed descriptions of the specific mechanisms through which institutions affect economies, is not as fully developed. Nevertheless, the research to date has laid down a firm foundation on which to build more detailed knowledge.

For example, in a leading study, Hall and Jones (1999) explore their hypothesis that cross-country differences in “social infrastructure”—defined as “the institutions and government policies that determine the economic environment within which individuals accumulate skills, and firms accumulate capital and produce output” (p. 84)—are the main determinants of differences in output per worker, via their effects on capital accumulation and productivity. Examining data from 127 countries, they find strong support for this hypothesis, concluding that a country’s long-run economic performance, measured in terms of output per worker, is primarily driven by its social infrastructure; additionally, the authors determine that this effect occurs through the positive impact of social infrastructure on physical and human capital accumulation, combined with its positive effect on productivity.

Another landmark in the empirical institutional economics literature is Acemoglu, Johnson, and Robinson’s 2001 paper, *The Colonial Origins of Comparative Development*. Utilizing a novel instrumental-variable approach, the authors find that the strength of property-rights institutions—measured by the risk of government expropriation of property—acts as a significant determinant of income per capita. The strength of the paper’s econometric approach (which is discussed further in Part III of this paper, *infra*) has been seen as providing firm support for the basic hypothesis that institutions matter to economic performance.

These two studies built on substantial earlier foundations. For example, Scully (1988) provided evidence that institutional factors such as political, civil, and economic liberty all had

positive and significant effects on both economic growth and economic efficiency in national economies. Knack and Keefer (1995) contributed to the literature by employing more accurate measures of property-rights and contract institutions; specifically, they used data collected by international investment risk analysts to build indices of institutional quality. Their regression analyses showed that institutional quality exerted a significant effect on both growth and investment. Similarly, Mauro (1995) utilized nine indicators of institutional quality drawn from another private firm, Business International, which had surveyed market participants in 68 countries and asked about perceptions of political stability, the efficiency and integrity of the legal system, and levels of bureaucracy, red tape, and corruption. Employing both ordinary least squares and instrumental-variables approaches, Mauro found that corruption and bureaucratic inefficiency negatively impacted investment and growth.

Recent additions to the institutional economics literature bolster the central conclusion that institutions matter. For example, Rodrik, Subramanian, and Trebbi (2004) argue that institutional quality “trumps” both geography and openness to international trade as a “deep determinant” of current income disparities between countries. Their empirical work accords with North’s theoretical argument that institutional quality not only matters to economic development, but is in fact the central explanatory factor underlying cross-country differences in development levels. Gwartney, Holcombe and Lawson (2006) focus specifically on the impact of institutions on investment, as a mechanism for their influencing growth. Additionally, the authors divide their investment data into public and private components, because high total investment rates can result from government activity rather than strong private capital formation, and it is the latter type of investment that we presume to be affected by better protection of property rights and more effective contract institutions. Utilizing the Economic Freedom of the

World index as their measure of institutional quality, they find that institutions exert a substantial impact on private investment and growth.

2.2—Contract Institutions: Existing Research and Scholarship

The question then arises as to how one goes about building strong institutions that will help promote economic development. The empirical research on this narrower, more policy-relevant issue is fairly scant, albeit growing (e.g., Pande & Udry 2006). One major factor underlying the state of the literature is the frequent use of very broad measures of institutional quality, such as indices that combine information on numerous types of institutions, as well as on closely related factors such as the prevalence of corruption (e.g., Mauro 1995). The theoretical literature, however, has consistently emphasized contract enforcement as one of the institutional factors that is most crucial to economic growth. For example, Douglass North has written that, “the inability of societies to develop effective, low-cost enforcement of contracts is *the most important source* of both historical stagnation and contemporary underdevelopment in the Third World” (1990, p. 54, emphasis added). Accordingly, some researchers have narrowed their focus to specifically examine contract-enforcement institutions.

An early example comes from Clague, Keefer, Knack, and Olson (1999), who employed an original measure of the strength of contract institutions, which they called “Contract-Intensive Money” (CIM). This variable was defined as “the ratio of non-currency money to the total money supply, or $(M_2 - C) / M_2$, where M_2 is a broad definition of the money supply and C is currency held outside banks” (p. 188). The authors’ rationale for this measure was that in countries where there is reliable government enforcement of contracts, market participants will be more willing to place their money into non-currency forms. Their empirical results showed

strong and significant positive associations between CIM and the total investment-to-GDP and the private investment-to-GDO ratios, as well an association with growth. Additionally, the authors found positive associations between CIM and the size of a country's capital stock, its income level, and its productivity.

Acemoglu and Johnson (2005) disaggregate property-rights institutions—defined as protections from expropriation by governments and “powerful elites”—from contract institutions, and find that the two differ in their effects on investment and growth. Their empirical analyses show that while property-rights institutions have “first-order effects” on both outcome variables, as well as on a country's level of financial development, contract institutions only affect “the form of financial intermediation” (p. 953). The authors speculate that market participants in countries where government contract-enforcement institutions are weak are nonetheless able to reduce transaction costs via private contracting and other informal mechanisms, such as reliance on business reputation, which leads to the relative unimportance of formal contract institutions with respect to growth, investment, and financial market development.

The question whether informal means of contract enforcement are sufficient substitutes for a reliable and efficient official judicial system has received attention from other scholars within the economic, legal, and sociological literatures. In earlier work, Johnson and colleagues (2002) examined the role of courts in post-Communist Eastern Europe and former Soviet republics, and found that although informal “relational contracting” constituted the main form of contractual relations between firms in those economies, formal judicial contract institutions also played a role in fostering productive business relationships. In particular, formal courts allowed firms to begin relationships with new trading partners, because relational contracting depends on

firms' having had previous interactions through which mutual trust was developed, or otherwise being able to obtain information about unknown firms through existing business and social networks. Therefore, informal contract institutions, while important, are not a perfect substitute for formal courts.

Trebilcock and Leng (2006), writing in the *Virginia Law Review*, canvass the various arguments and evidence in favor of and opposing the value of formal versus informal contract-enforcement mechanisms. They label the two camps “contract formalists” and “contract informalists.” After a survey of the economic, historical, and legal scholarship on the subject, they conclude that informal contracting may serve reasonably well in under-developed countries, but that more advanced economies will be unable to function in the absence of formal contract institutions, given firms' needs to trade in long-lived and expensive machinery, in complex services, as well as with a variety of unknown trading partners. The authors argue, therefore, for a nuanced approach to the question whether formal contract institutions are a prerequisite to economic development.

2.3—Institutional Economics: Critiques from Economists and Lawyers

As already noted, the empirical institutional economics literature has been able to provide macro-level results to support the proposition that institutions matter, but has had less success in producing information that could actually help guide policy makers who would like to improve institutional quality as a means to develop economies. Although some researchers have moved in the direction of policy relevance by narrowing their focus to examine contract-enforcement institutions in particular, their measures of the quality of contract institutions still tend to be fairly broad, and detailed prescriptions for judicial reform are lacking.

This frustrating lack of actionable information has been acknowledged by many institutional economists. For example, Acemoglu, Johnson, and Robinson admit that in their analysis, “[i]nstitutions are treated largely as a ‘black box,’” and therefore their results “do not point out what concrete steps would lead to an improvement in [property-rights] institutions” (2001, p. 1395). Rodrik (2004), another leading voice in favor of the crucial importance of institutions, has conceded that the broad measures of institutional quality commonly used in the literature produce results that, even if causally robust, are unable to distinguish between various specific sources of institutional quality, and therefore cannot provide guidance for improving institutions. Writing with co-authors, Rodrik has also emphasized that the institutional economics research project has only, at least thus far, clarified the *general objective* of improving institutions, but has had little to say about *how* to achieve this goal. (Rodrik et al. 2004). Pande and Udry (2006) argue that the existing empirical institutional economics literature—having relied heavily on instrumental variables—had “served its purpose and [was] essentially complete,” and that therefore the research focus should turn to more micro-level data that might help elucidate “how specific institutions influence economic outcomes.”

These self-critiques on the part of economists have been echoed by legal scholars. Interestingly, for a brief moment during the 1960s and early 1970s, there was intense excitement in the legal academy about the prospect of using law as a means of encouraging economic development (Trubek & Galanter 1974; Davis 2004; Tamanaha 2011). But this intellectual trend went out of style, and it seems that only once economists began focusing intently on legal institutions in the late 1980s and 1990s did lawyers wake back up to join the discussion. Since then, they have begun to voice both interest in and concerns with the economic approach, particularly with respect to the usefulness of the empirical research as a guide for legal

institutional reform. For example, Davis (2004) has wondered whether the choices of independent variables in most cross-country analyses result in an inability to convey useful information about countries' legal systems; specifically, he points out that indices of "institutional quality" may lack the requisite precision to guide reformers, and, concomitantly, may include aspects of institutional quality that are simply not viable candidates for reform. Michaels (2009) makes another important point from the lawyer's perspective, namely that empirical studies that examine "the law on the books" miss the fact that what is most important is how law is actually *applied* in the real world, which also entails people's attitudes toward the law. Michaels concedes that empirical work must be limited to those factors that can be measured, but argues that this necessary reductionism implies that empirical institutional economics should be supplemented by other methods of analysis, such as traditional qualitative comparative law work.

2.4—Summary

Over the past twenty-five years, economists have built an impressive body of theory and evidence to support the proposition that institutions matter to economic development. Their arguments have become mainstream wisdom in development thought, and consequently have influenced the practice of development-focused work. The World Bank, for example, has firmly embraced the view that institution building must play a core role in any serious development strategy, and accordingly has invested significant resources in judicial reform, anti-corruption, and other types of governance-building projects (World Bank 1997; Grindle 2010; Laver 2012). But there is a concern among many—including lawyers and institutional economists themselves—that the empirical research to date has not provided the level of detail necessary to

guide efforts to build and reform developing-country institutions. Consequently, despite near unanimity on the idea that institutions matter, this economic conclusion may be too broad to be useful to the project of development. The task, then, is to build on the formidable foundations laid by economists such as North, Acemoglu, and Rodrik by developing more refined empirical approaches that might offer the possibility of providing practical guidance in putting institutional economics into practice. In the following section, I describe how this paper will modestly attempt to add to the literature in this respect.

III. CONCEPTUAL APPROACH

The conceptual approach pursued in this paper follows from the preceding discussion. In an effort to provide more targeted empirical results that might serve usefully to guide actual institutional-reform projects, I narrow my focus to one element of contract institutions—specifically, their *efficiency*—and study this element’s effect on a particular factor of economic growth, namely private capital investment. This approach offers several advantages compared to existing research. First, although several studies in the institutional economics literature have looked specifically at contract institutions, few have looked more narrowly on the impact that contract-enforcement efficiency has on economic outcomes (e.g., Djankov et al. 2008). To the extent that efficiency measures have been utilized, they have usually been folded into more general indices of contract-institution quality; additionally, these indices themselves have often been instrumented for. In sum, the literature lacks findings on contract-enforcement efficiency, and this study endeavors to help fill that gap. Moreover, as Davis (2004) warns, useful empirical research must focus on those institutional factors that are actually amenable to reform. I believe that contract-enforcement efficiency—as measured by procedural complexity and litigation

duration—meets this requirement, because clear conclusions will enable reformers to gauge the effect of reducing the number of procedures required to enforce a contract (which might be accomplished directly through legislation), and the effect of reducing the number of days required to enforce a contract in the courts (which might be accomplished by, for example, hiring more judges and administrators, or implementing electronic docket-management systems).

This paper further refines its focus by utilizing private capital investment as a dependent variable, rather than broader measures such as total investment or per capita income. It is hoped that such a focus will facilitate a clearer understanding of the *mechanisms* through which contract-enforcement efficiency affects economic outcomes. I assume, based on an ample theoretical and empirical foundation, that the cost of enforcing contracts will affect economies primarily through its impact on firms' investment decisions. Acemoglu et al., for example, summarize the theoretical underpinnings of institutional economics as holding that “[c]ountries with better ‘institutions,’ more secure property rights, and less distortionary policies *will invest more in physical and human capital*, and will use these factors more efficiently to achieve a greater level of income” (2001, p. 1369, emphasis added). Similarly, Mauro remarks that, “it seems that a considerable portion of the effects of corruption on growth works through its effects on the total amount of investment” (1995, pp. 704–05). Haggard et al. (2008) note that good property-rights institutions set conditions in which firms have strong incentives to invest. Gwartney et al. (2006) also emphasize the importance of a country's institutional framework to its level of investment, both through the security that property-rights protections offer to firms that build their capital stock, as well as via the contribution that institutional quality makes to the development and operation of the capital market. I follow the example of Gwartney and his colleagues by honing in on private capital investment as a dependent variable; however, as

already noted, I further refine the focus by using a narrower and more policy-relevant independent variable, namely the efficiency of formal contract-enforcement institutions.

The most formidable econometric challenge that has bedeviled researchers in this field is the endogeneity of institutions. Obviously, good-quality institutions are a likely consequence of economic development, just as they are a likely cause of it (Mauro 1995; Hall & Jones 1999; Haggard et al. 2008). This thicket of simultaneity is precisely what has led to the proliferation of instrumental-variables approaches in the literature, including Acemoglu et al.'s trailblazing use of colonial-settler mortality as an instrument for property-rights institutions. Despite the great strength of these instrumental approaches—which have enabled the economic community to reach a strong consensus on the macro-level conclusion that institutions matter—they have contributed to the inability of the field to offer much in the way of guidance for policy makers (Gwartney et al. 2006). This weakness of instrumental-variables econometrics, combined with the fact that measurement of institutional quality has been notoriously broad and imprecise (as noted *supra*, section 2.3), is what drives my own empirical approach. Specifically, I endeavor to use narrower measurements of institutions, to derive reliable conclusions without the use of instruments, and to employ country-level fixed effects to produce more policy-relevant results. The empirical model in its most basic form is a pooled OLS equation, specified as follows:

$$y_{i,t} = \alpha + \beta_1 X_{i,t} + \beta_2 Z_{i,t} + \varepsilon_{i,t} \quad (1)$$

where $y_{i,t}$ is private capital investment, $X_{i,t}$ is contract-enforcement efficiency, and $Z_{i,t}$ is a vector of control variables. The set of control variables that I employ in this paper derives from the guidance of both the institutional economics literature, as well as more general macroeconomic work on investment. Accordingly, the standard macroeconomic controls that I use include inflation, the real interest rate, and total trade (Wai & Wong 1982; Greene &

Villanueva 1991; Erden & Holcombe 2005). In some specifications I also include the one-year lag of my dependent variable (private capital investment), as well as GNI per capita. Finally, in most specifications I include a control for the quality of property-rights institutions, following the suggestion of Acemoglu and Johnson, who note that, “in regressions that do not control for property rights institutions, the importance of contracting institutions is exaggerated because they capture some of the cross-country differences in the protection of property rights” (2005, pp. 971–74). Accordingly, in order to ensure that my estimates of contract-enforcement efficiency are not biased by their correlation with property-rights institutions, I am careful to include a control for property-rights protection.

One of the main contributions of this paper is my estimation of a fixed-effects model, which gives a new perspective on institutions that escapes the focus of the cross-country comparisons used in much of the literature. Country-level fixed effects should be particularly useful to the project of producing policy-relevant results because reforms to legal institutions will necessarily take place *within* individual countries, and therefore our goal should be to estimate what the effect of a change in contract-enforcement efficiency would be within these countries. In other words, although purely cross-country analyses have been useful to draw out macro-level conclusions about the relationship between institutions and economic outcomes, as part of the effort to explain cross-country differences, a fixed-effects analysis will give more useful information about what we might expect from country-level policy reforms. Equation 2 provides the basic fixed-effects model:

$$y_{i,t} = \alpha + \beta_1 X_{i,t} + \beta_2 Z_{i,t} + \delta_i + \varepsilon_{i,t} \quad (2)$$

where δ_i is a country-level fixed effect. In further specifications I also include a fixed-effect for year, ξ_t . Finally, in section 5.3 I estimate a two-stage least squares model to ensure the robustness of my results.

IV. DATA

I use the World Bank's World Development Indicators (WDI) panel dataset to empirically test my hypothesis. The WDI dataset includes every country in the world, although data relevant to this study was not available for each country. Additionally, because the World Bank only began administering its *Doing Business* surveys—from which I draw data on contract-enforcement efficiency—in 2003, I use a panel beginning in that year and extending to 2009. Thus, for any one country, the panel provides at most seven years of data. For many countries, however, data is missing for one or several of the years in the panel.

The dependent variable is *private sector, gross fixed capital formation*, measured as % of gross domestic product (GDP). The World Bank calculates this variable drawing from its own national accounts data, as well as from those of the Organization for Economic Cooperation and Development (OECD). According to the World Bank, the variable “covers gross outlays by the private sector (including private nonprofit agencies) on additions to its fixed domestic assets” (2012b, online). In this paper I will refer to this variable as *Private Capital Investment*.

The two key independent variables measure the number of procedures and number of days required to enforce a commercial contract in a country's courts; in this paper, they are labeled *Contract Procedures* and *Contract Time*, respectively. These data are drawn from the *Doing Business Project*, a joint endeavor of the World Bank and International Finance Corporation, which involves collecting survey data on a number of topics to gauge the ease of

doing business in a given country. Thus, for example, the project collects data on issues including: the ease of starting a business; the availability of electricity; requirements for registering property; and investor protections, among many others. In this paper, I use *Doing Business* data on contract enforcement, which was collected via a survey given to law firms in each country. The survey presents a hypothetical breach-of-contract scenario, involving two firms and a contract worth 200% of the income per capita of the country in which the survey is being administered. The survey asks responding lawyers to imagine they are bringing the case on behalf of an entrepreneur who has sold goods to a buyer; but the buyer, after receiving delivery of the goods, has refused to pay for them, claiming that the goods' quality is inadequate. The scenario assumes that the contract dispute proceeds through the courts and goes to trial, where expert testimony is heard as to the quality of the goods at issue. After winning at trial, the entrepreneur successfully enforces the judgment (World Bank & IFC 2012).

I supplement the WDI data with select variables drawn from the University of Gothenburg's Quality of Government dataset, which compiles data from a number of sources. Most importantly, I include a measure of property-rights protection, created by the Heritage Foundation; the variable ranges from 0 to 100, with higher values indicating stronger protection of property rights. Additionally, I employ measures of *Legal Origin* and *Colonial Origin*, which come from, respectively, La Porta et al. (1999) and Hadenius and Teorell (2005). These variables are used as instruments for Contract Procedures in a two-stage least squares estimation, which I include as a robustness check.

Preliminary scatterplot analyses of the panel revealed the presence of several outliers, which were subsequently removed from the dataset. These outlying countries were: Azerbaijan, Bhutan, Cape Verde, Ecuador, Grenada, Mongolia, and Slovenia. In varying degrees,

observations for each of these countries lay sufficiently far from the main cluster of observations, collected around the correlation line, that it was evident the countries did not adhere to whatever general trends might be generating the data. I judged that the risk of these outliers' biasing the results, or adding additional error to the data, outweighed any loss from their exclusion. Thus, the decision was made to remove these countries from the dataset. This removal is less problematic given that the number of countries is small, and the countries themselves are relatively diverse in terms of geographic location.

Another preliminary analysis revealed a marked lack of variation over time in the two contract-enforcement variables. Two explanations come to mind with respect to this observation. First, it could certainly be the case that over the short panel, there was relatively little change in the judicial systems of most countries. Second, it is also possible that the amount of actual variation was too difficult for lawyers to perceive, and so, given that the data come from surveys of lawyers' subjective perceptions of countries' legal systems, the true amount of variation was underreported. Nevertheless, the amount of within-country variation was ample enough to support robust results in a fixed-effects specification, as reported later in this paper.

Table 1: Summary Statistics

Variable	N	Minimum	Maximum	Median	Mean	S.D.
Private Capital Investment (%)	629	0	37.27	16.00	15.87	6.58
Contract Procedures (# of procedures)	629	21	55	38.00	38.61	5.80
Contract Time (# of days)	629	120	1715	581.00	630.61	280.68

Source: World Bank 2012b

Table 1 presents summary descriptive statistics for the study's three main variables. Data for the dependent variable, Private Capital Investment, was missing for a significant number of

countries, at least for certain years of the panel. Thus, only 629 observations include investment data, and data for the two key independent variables was dropped so as to include only those observations with investment measures. Also noteworthy is the average length of time required to enforce a contract; the mean value for Contract Time is nearly two years (630.61 days). The variable's standard deviation, meanwhile, is close to one year (280.68 days). Obviously, then, enforcing a commercial contract can be a time-consuming endeavor in many countries across the world. Given that for many small firms, such a lengthy period of uncertainty could be a substantial hindrance to the conduct of business, it seems plausible to assume at the outset that the inefficiency of contract enforcement in terms of litigation time could be a significant constraint on those firms' ability to make capital investments. The other measured component of contract-enforcement efficiency—Contract Procedures—displays substantially less variability across the panel, with a mean of nearly 39 procedures and a standard deviation of about 6.

Preliminary analysis of the dependent variable yields a couple of observations. First, it is apparent that the average level of capital investment in the private sector is about 16% of GDP across the worldwide panel. Given that average level, the standard deviation of nearly 7% seems substantial. Accordingly, one can anticipate that there will be a fair amount of variation in the dependent variable, which should hopefully contribute to my ability to estimate the impact of contract-enforcement efficiency on this variation. Second, it is worth noting that the maximum level of private capital investment is extremely high, reaching nearly 40% of GDP. That some countries can achieve such high levels of private sector investment likely means that conditions for investment are appropriate; if my hypotheses are correct, these high levels of investment should be at least partly determined by the efficiency of contract-enforcement institutions.

Table 2: Summary Statistics by Income Group

Variable	N	Minimum	Maximum	Median	Mean	S.D.
Low-Income Countries						
Private Capital Investment (%)	264	0	37.27	13.50	13.12	6.74
Contract Procedures (# of procedures)	337	24	54	40.00	39.99	5.02
Contract Time (# of days)	337	195	1800	552	648.47	317.67
Lower-Middle-Income Countries						
Private Capital Investment (%)	230	1.85	35.20	16.63	16.52	6.18
Contract Procedures (# of procedures)	320	28	55	39	38.93	6.03
Contract Time (# of days)	320	195	1800	590.50	672.63	309.80
Upper-Middle-Income Countries						
Private Capital Investment (%)	171	0	41.87	18.17	18.34	6.35
Contract Procedures (# of procedures)	227	27	51	37	36.97	5.81
Contract Time (# of days)	227	210	1715	590	610.89	257.24
High-Income Countries						
Private Capital Investment (%)	14	1.22	31.04	15.87	15.62	7.67
Contract Procedures (# of procedures)	269	20	58	33	34.52	8.11
Contract Time (# of days)	269	120	1642	505	500.07	246.60

Source: World Bank 2012b

Legend: Low Income: GNI per cap. \leq \$1025; Lower-Middle Income: $\$1025 < \text{GNI per cap.} \leq \4035 ; Upper-Middle Income: $\$4035 < \text{GNI per cap.} \leq \$12,475$; High Income: GNI per cap. $> \$12,475$

In Table 2, I present summary statistics by income group, following the World Bank's four-way categorization. First, one can observe an evident pattern of capital formation in the private sector, wherein low-income countries have the lowest rates of investment (around 13% of

GDP), the two categories of middle-income countries have the highest rates (between 16% and 19%), and high-income countries, at about 16%, have somewhat lower rates than those of middle-income economies, but still exceed low-income countries' rates. This pattern is not surprising: low-income countries suffer from a lack of private-sector capital investment, perhaps due to factors including institutional weakness, economic uncertainty, and a lack of access to investment capital; middle-income countries are in robust growth phases, with strong private sector activity and entrepreneurship—likely complemented by government investment—as well as increasingly available capital; and high-income countries, perhaps facing a combination of decreasing returns to physical capital formation, the movement of manufacturing to the developing world, and increasing returns to human capital and technological investment, see investment rates lower than those in middle-income countries, yet still much stronger than those in the poorest parts of the world.

Turning to the measures of contract-enforcement efficiency, a clear pattern emerges with respect to Contract Procedures: their number decreases as income rises. Additionally, the relative differences between income levels with respect to Contract Procedures appears to vary as well; the differences in both means and medians increase as income level rises. Specifically, the differences in means and medians between low-income and lower-middle-income countries are both about one procedure, while those between lower-middle and upper-middle income countries are about two procedures, and those between upper-middle and high-income countries are about two-and-a-half for the means, and four procedures for the median country. Looking at Contract Time, most apparent is the much lower measures for high-income countries, at around 500 days for both mean and median, compared to the other three categories. But also evident is the distinction between medians and means for the three lower-income categories: in each case,

the mean exceeds the median, and this difference is relatively large for the two poorest groups. We can infer, therefore, that certain countries within these samples have exceptionally slow-moving court systems, and their high numbers of days required for contract enforcement is weighing the mean higher. Finally, it is also worth noting that the standard deviation of Contract Time decreases steadily as income-level rises. This may indicate that richer countries have more stable and predictable judicial systems. Such stability and predictability could, in turn, make it easier for firms to rely on the judicial system for contract enforcement. If this is true, then this dimension of variance should also be taken into account when evaluating the efficiency of a country's courts with respect to enforcing contracts.

Table 3: Correlation Matrix—Dependent & Key Independent Variables

	Private Capital Investment	Contract Procedures	Contract Time
Private Capital Investment	1.000		
Contract Procedures	-0.0840	1.000	
Contract Time	-0.1054	0.2178	1.000

Source: World Bank 2012b

Note: N = 629

Table 3 presents simple correlations between the dependent and key independent variables across the entire panel. The negative correlations between the dependent variable and each independent variable conform to my hypothesis that more inefficient contract-enforcement regimes will be associated with lower rates of private capital investment, given the increased cost of enjoying contract-enforcement protection. Additionally, and unsurprisingly, the two measures of contract-enforcement efficiency are positively correlated.

Table 4: Correlation Matrix—Dependent, Key Independent, & Main Controls

	Private Capital Investment	Contract Procedures	Contract Time	Property Rights	Inflation	Real Interest Rate	Trade	GNI per Capita
Private Capital Investment	1.000							
Contract Procedures	-0.0770	1.000						
Contract Time	-0.1099	0.2901	1.000					
Property Rights	0.1809	-0.2229	0.0611	1.000				
Inflation	0.0067	-0.0369	-0.1124	-0.0821	1.000			
Real Interest Rate	-0.2676	0.0444	-0.0650	-0.1849	-0.1770	1.000		
Trade	0.2082	-0.3267	-0.2075	0.3347	-0.0547	-0.0427	1.000	
GNI per Capita	0.2738	-0.4003	-0.1890	0.5015	-0.1251	-0.1247	0.5463	1.000

Sources: Heritage Foundation 2002 (Property Rights) & World Bank 2012b (all other variables)

Note: N=281

Table 4 presents correlations including the main control variables that will be employed in my regression analyses. First, it should be noted that the slight changes in correlations between the dependent and two key independent variables, compared to those displayed in Table 3, derive from the reduction in sample size that occurred due to the addition of the controls into the correlation matrix, for which observations are often missing. Still, these correlations are quite close to those appearing in the previous table. Proceeding to an analysis of the additional correlations presented here, we can observe a strong positive correlation between investment and the protection of property rights, which accords with the central proposition of institutional economics. The correlations between investment and the real interest rate, investment and

openness to trade, and investment and GNI per capita all exhibit the expected signs. The correlation between investment and the inflation rate is positive, which is unexpected, but its magnitude is negligible. One explanation might be that higher inflation rates encourage firms to use their capital, rather than to hold it in banks or portfolio investments where it will quickly lose value.

The variable capturing the number of contract procedures exhibits expected correlations between most of the control variables. Countries having more procedurally complex court systems also have worse property rights protections, higher interest rates, less openness to trade, and lower per capita income. Inflation again exhibits a sign consistent with the theory that higher rates of inflation encourage firms to use their capital now, rather than wait for it to lose its value. Contract Time exhibits characteristics that are somewhat different from those of Contract Procedures. Specifically, Contract Time is positively correlated with property rights protection, albeit at a small magnitude, and negatively correlated with the real interest rate. One explanation for these correlations might be that in countries with stronger property-rights protections and lower interest rates, firms are more willing to make capital investments and are also more willing to use the court systems. Thus, with more firms filing lawsuits in the courts, the average time to process a case increases, but the added wait is a function of the trustworthiness and effectiveness of the courts—a good thing. Finally, it is worth noting that the quality of property-rights protection exhibits expected correlations with inflation, the real interest rate, trade openness, and per capita income.

V. EMPIRICAL RESULTS

5.0—Introduction

In this Part, I present results from several regression analyses. First, I build on the basic information offered by the descriptive statistics and develop a deeper understanding of the data via a pooled ordinary least squares (OLS) model. Second, I proceed to the study's main empirical model, which includes country-level fixed effects. I ensure robustness by estimating an instrumental-variables model, employing legal origin and colonial origin as instruments for contract-enforcement efficiency.

The regression results provide some support for my hypothesis that more efficient systems of contract enforcement will positively influence firms' willingness and ability to make capital investments. In the pooled OLS model, the measure of litigation duration—the number of days required to enforce a contract in the courts—exhibits a negative and significant association with the level of private capital investment. This suggests that countries with less delay in their court systems will benefit from increased investment on the part of firms. Once country-level fixed effects are included in the model, however, it is the measure of procedural complexity that appears to have a strong and significant impact on investment levels. Both results support the basic hypothesis that contract-enforcement efficiency is an element of a country's institutional environment that affects the incentives and constraints that firms face in making important decisions about investment. These investment decisions, in turn, are relevant to a country's ability to promote economic growth. In sum, therefore, I conclude that judicial reform efforts might be worthwhile means of increasing investment and thus promoting growth.

5.1—Pooled OLS Model

As a first step, I estimate a pooled OLS model, which neglects to recognize the panel nature of the data. Table 5 displays results that include various combinations of the two contract-enforcement variables—Contract Procedures and Contract Time—with basic controls; any given specification includes just one control, at most.

Table 5: Pooled OLS—Single Controls

<i>Dependent Variable: Private Capital Investment (% of GDP)</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Contract Procedures	-0.0953** (0.0480)		-0.0727 (0.0478)	-0.0165 (0.0626)	-0.0707 (0.0479)	-.00632 (0.0622)	-0.0304 (0.0491)
Contract Time		-0.0025*** (0.0009)	-0.0021** (0.0009)	-0.0027** (0.0011)	-0.0023** (0.0009)	-0.0027*** (0.0011)	-0.0018* (0.0010)
Property Rights				0.0668*** (0.0230)			
Inflation					-0.0353 (0.0275)		
Real Interest						-0.0356*** (0.0073)	
Trade							0.0279*** (0.0068)
Constant	19.5518*** (1.8513)	17.4328*** (0.6532)	20.0319*** (1.8292)	15.4789*** (2.6907)	20.3327*** (1.8278)	20.7674*** (2.2087)	15.7589*** (2.1024)
Prob > F	0.0474	0.0068	0.0097	0.0014	0.0114	0.0000	0.0000
R ²	0.0071	0.0111	0.0150	0.0449	0.0174	0.0582	0.0454
N	629	629	629	343	629	500	625

Note: Robust standard errors in parentheses.

Significance Levels: * <= 0.10; ** <= 0.05; *** <= 0.01

Sources: Heritage Foundation 2002 (Property Rights) & World Bank 2012b (all other variables)

These initial results conform to my basic hypothesis, given the negative estimated coefficients for both contract-enforcement variables, and the consistent statistical significance of Contract Time in particular. But these results are not very illuminating, given what we have

already observed of the raw correlations in Tables 3 and 4. The next step, therefore, is to determine whether these results are robust to the inclusion of multiple control variables.

Table 6: Pooled OLS—Combined Controls

<i>Dependent Variable: Private Capital Investment (% of GDP)</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Contract Procedures	-0.0727 (0.0478)	-0.0165 (0.0626)	-0.0168 (0.0627)	0.0060 (0.0781)	0.0416 (0.0805)	0.0802 (0.0808)
Contract Time	-0.0021** (0.0009)	-0.0027** (0.0011)	-0.0027** (0.0011)	-0.0030** (0.0012)	-0.0025* (0.0013)	-0.0020 (0.0013)
Property Rights		0.0668*** (0.0230)	0.0669*** (0.0228)	0.0558*** (0.0223)	0.0389* (0.0220)	0.0131 (0.0227)
Inflation			0.0044 (0.0367)	-0.0493 (0.0517)	-0.0357 (0.0685)	-0.0143 (0.0689)
Real Interest				-0.0436*** (0.0104)	-0.0434*** (0.0102)	-0.0414*** (0.0098)
Trade					0.0197** (0.0095)	0.0114 (0.0102)
GNI per capita						0.0004** (0.0002)
Constant	20.0319*** (1.8292)	15.4789*** (2.6907)	15.4382*** (2.7313)	16.2844*** (3.0886)	13.3770*** (3.5507)	12.1447*** (3.6108)
Prob > F	0.0097	0.0014	0.0036	0.0000	0.0000	0.0000
R ²	0.0150	0.0449	0.0449	0.1107	0.1286	0.1464
N	629	343	343	284	283	281

Note: Robust standard errors in parentheses.

Significance Levels: * <= 0.10; ** <= 0.05; *** <= 0.01

Sources: Heritage Foundation 2002 (Property Rights) & World Bank 2012b (all other variables)

The results in Table 6 demonstrate that the estimated coefficient on Contract Time was fairly robust to the inclusion of several macroeconomic control variables. Its magnitude is very consistent among the specifications included in this table, as well as in comparison to those included in Table 5. In general, the model predicts that a one-hundred day increase in the time

required to enforce a contract is associated with a 0.2% decrease in the investment-to-GDP ratio. Given that the standard deviation of Contract Time for the panel is about 300 days (see Table 1), this means that a one standard deviation increase in Contract Time is associated with a 0.6% decrease in the investment-to-GDP ratio. Although the magnitude of this predicted change in private capital formation is not very large, neither is it insubstantial.

An examination of the estimates for Contract Procedures yields less stable conclusions. First, in no specification did this variable achieve statistical significance. Second, and perhaps more interesting, is the fact that its sign flips to being positive—that is, out of line with my hypothesis—once the control for real interest rate is added into the model. One interpretation of this result is that the interest rate basically proxies for the level of risk in an economy, and, concomitantly, the cost of financing for investments (see, e.g., Hubbard 1998). Higher general risk, as well as an elevated cost of capital, both could negatively impact the willingness and ability of firms to make capital improvements. Thus, it is unsurprising that we see a negative and significant estimated coefficient for the interest rate. On this risk interpretation of the interest rate, we might infer that the Contract Procedures variable also captures some similar element of risk in an economy; for example, it might plausibly proxy for government bureaucracy or government intrusiveness into the economy, both of which would negatively affect growth prospects, as well as the willingness of firms to expand. Thus, if both Contract Procedures and the real interest rate are capturing a similar economic risk factor, then it is unsurprising that the inclusion of the interest rate—likely a much better measure of risk than the number of procedures required to enforce a contract—would extinguish the negative coefficient on Contract Procedures. If this interpretation is correct, however, then it still bears noting the

stability and significance of the negative coefficient on Contract Time, even after the inclusion of the interest rate control.

Table 7: Pooled OLS—Subregressions by Income Level

<i>Dependent Variable: Private Capital Investment (% of GDP)</i>					
<i>Subregressions: (1) Full Sample (2) Low Income Only; (3) Lower-Middle Income Only; (4) Upper-Middle Income Only; (5) Upper-Middle & High Income Combined</i>					
	(1)	(2)	(3)	(4)	(5)
Contract Procedures	0.0416 (0.0805)	0.2783* (0.1475)	-0.1271 (0.0864)	0.1477 (0.1880)	0.1620 (0.1967)
Contract Time	-0.0025* (0.0013)	0.0022 (0.0020)	-0.0044** (0.0019)	-0.0065*** (0.0023)	-0.0058*** (0.0022)
Property Rights	0.0389* (0.0220)	0.0003 (0.0534)	0.0422 (0.0469)	0.0158 (0.0324)	0.0128 (0.0317)
Inflation	-0.0357 (0.0685)	-0.0610 (0.1360)	0.0019 (0.1293)	-0.0098 (0.0845)	-0.0257 (0.0926)
Real Interest	-0.0434*** (0.0102)	-0.0382*** (0.0093)	0.0361 (0.1230)	-0.1927*** (0.0665)	-0.2122*** (0.0726)
Trade	0.0197** (0.0095)	0.0577*** (0.0209)	0.0035 (0.0174)	0.0166 (0.0204)	0.0045 (0.0118)
Constant	13.3770*** (3.5507)	-1.236 (6.079)	22.0454*** (04.9264)	15.2938* (7.9095)	15.9023** (8.0054)
Prob > F	0.0000	0.000	0.0551	0.0059	0.0099
R ²	0.1286	0.2173	0.1125	0.2179	0.1935
N	283	108	104	67	71

Note: Robust standard errors in parentheses.

Significance Levels: * <= 0.10; ** <= 0.05; *** <= 0.01

Sources: Heritage Foundation 2002 (Property Rights) & World Bank 2012b (all other variables)

Legend: Low Income: GNI per cap. <= \$1025; Lower-Middle Income: \$1025 < GNI per cap. <= \$4035; Upper-Middle Income: \$4035 < GNI per cap. <= \$12,475; High Income: GNI per cap. > \$12,475

Table 7 presents results from pooled OLS regressions on subsamples of the full panel, divided by income-level group. Column 1 provides results from the full sample for comparison, and it should also be noted that Column 5 displays results from a regression on a combined

subsample of the two highest income-level groups, which was performed due to there being an insufficient number of observations in the high-income group alone. There are several noteworthy features of these results. First, and most strikingly, the estimated coefficients on both contract-enforcement efficiency variables are positive for the subsample of low-income countries; in fact, the coefficient on Contract Procedures is statistically significant at the 10% level, and of a relatively high magnitude. One explanation for this result could be that for low-income countries, measures of contract-enforcement efficiency proxy for some other determinant of investment, such as general government effectiveness. Or it might be the case that for low-income countries, a higher number of procedures indicates that a judicial system actually functions and is utilized by market participants, whereas other low-income countries' judicial systems are so dysfunctional that firms prefer to substitute informal means of contracting. Alternatively, the result could be caused by the positive correlation between Contract Procedures and the real interest rate, as already discussed. Another noteworthy aspect of the low-income regression results is the extremely low value of the constant. Given that only two variables—Contract Procedures and trade—have positive coefficients of reasonably large magnitudes, this result seems to support the notion that Contract Procedures is proxying for some other more fundamental determinant of investment.

Turning to examine the other three subregressions, the results are more in line with what was seen when regressing the full panel. Specifically, the estimated coefficient on Contract Time is negative and statistically significant; interestingly, we can observe that the variable's magnitude is greatest for upper-middle-income countries, where a one standard deviation increase in the number of days required to enforce a contract (about 250 days) is associated with a roughly 1.5% decrease in the investment-to-GDP ratio. A plausible interpretation of this

differential impact is that upper-middle-income countries are at a stage in their economic development where the growth outlook appears strong, and economic activities that entail physical capital investment—such as manufacturing—form the backbone of the economy. Thus, slow-moving contract-enforcement systems have a relatively large impact on firms’ ability to buy new equipment and make other capital investments. In general, however, the results show a strong negative impact of judicial delay on investment, in line with my core hypothesis.

Less information can be gleaned from the estimated coefficients on Contract Procedures, but, as explained in the analyses of preceding results, this could be due to an overlap between this variable and the real interest rate, both of which likely capture a risk factor in the economy.

5.2—Fixed-Effects Model

Building on the pooled OLS results, in this section I estimate my core model, which includes country-level fixed effects. This model is more closely aligned with the central goal of this paper, which is to offer operational guidance for institutional reform. The model’s benefits derive from its within-country focus; because policy reforms will necessarily take place at the country level, it is necessary to produce estimated effects of national judicial-efficiency reforms. Consequently, the results presented below are useful in that they offer a sense of what effects a country’s government might be able produce on private investment behavior, if it were to implement reforms to the court system to reduce litigation delay and procedural complexity.

Table 8: Country-Level Fixed Effects

<i>Dependent Variable: Private Capital Investment (% of GDP)</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Contract Procedures	-1.0918*** (0.4053)		-1.2059*** (0.4717)	-0.5382 (0.5959)	-1.2105*** (0.4066)	-1.2964*** (0.3922)	-1.8746*** (0.3734)
Contract Time		-0.0060 (0.0062)	0.0028 (0.0064)	0.0057 (0.0082)	0.0075 (0.0162)	0.0080 (0.0163)	0.0152 (0.0169)
Lagged Investment				0.3693*** (0.0604)	0.2086*** (0.0811)	0.2326*** (0.0825)	0.2145** (0.1065)
Property Rights					-0.1702* (0.0945)	-0.1756* (0.0932)	-0.2085** (0.1047)
Inflation						-0.0399 (0.0308)	0.0337 (0.0833)
Real Interest							0.0014 (0.0040)
Trade							0.0733* (0.0392)
Constant	58.0236*** (15.6452)	19.6365*** (3.9408)	60.6871*** (16.5233)	27.4857 (20.3460)	60.6473*** (16.5977)	63.8110*** (16.1024)	74.8040*** (18.2210)
Prob > F	0.0082	0.3418	0.0260	0.0000	0.0004	0.0001	0.0000
R ²	0.7613	0.7569	0.7614	0.8481	0.9070	0.9081	0.9095
N	629	629	629	535	259	259	217

Note: Robust standard errors in parentheses.

Significance Levels: * <= 0.10; ** <= 0.05; *** <= 0.01

Sources: Heritage Foundation 2002 (Property Rights) & World Bank 2012b (all other variables)

The fixed-effects model produces strikingly different results than were seen under the pooled model. Most evident is the estimated coefficient on Contract Procedures: whereas in the pooled model it was generally unstable (that is, it flipped signs depending on the specification), statistically insignificant, and of relatively small magnitude, these characteristics are all very different in the fixed-effects model. As the results in Table 8 show, the estimated coefficient on Contract Procedures is consistently negative, strongly significant, and of a very high magnitude.

Looking at the specification in column 7, which includes all controls, the results show that a one-unit increase in Contract Procedures is associated with a decrease in the Private Capital Investment ratio of nearly 2%. Given that the standard deviation of Contract Procedures is 5.8, this means that a one standard-deviation increase in this variable is associated with a roughly 10–12% decrease in the investment share of GDP. And to put that number in perspective, consider that the standard deviation of Private Capital Investment itself is about 7%; therefore, a one standard-deviation increase in Contract Procedures predicts a greater-than-one standard-deviation decrease in the private investment-to-GDP ratio. Thus, these results appear to strongly support the hypothesis that contract-enforcement efficiency affects firms' capital investment decisions; specifically, this fixed-effects model demonstrates that even slight increases in the number of procedures required to pursue a breach-of-contract claim in court can have a powerful deterrent effect on investment.

The impressive magnitude of these estimates calls for more careful analysis. Can it be possible that just one additional procedure could deter between 1–2% of investment as a share of GDP? It is certainly possible, but one might also ask whether the estimate captures a more nuanced relationship between procedural complexity and firms' willingness to invest. Specifically, it could be the case that only *certain* procedures have a truly strong deterrent effect on investment, due to those specific procedures' being especially burdensome, or perhaps sources of rent-seeking behavior on the part of government officials. For example, perhaps certain procedures entail getting the signature of government officials, which could be both burdensome, time consuming, and a locus for corruption (see Buscaglia & Dakolias 1999). Thus it might be that only these particular procedures—rather than all procedures—have a real impact on judicial efficiency, and therefore on investment. Accordingly, it would be useful if future

Doing Business reports dug deeper into the data on procedural complexity to code types of procedures by factors that might plausibly lead to significant deterrent effects on the use of the judicial system. Likely chief among these factors is the opportunity for rent-seeking.

There are three other variables of note in Table 8. First, it bears mentioning that the estimated coefficient on Contract Time is negligible in magnitude and never statistically significant. One reason for this could be a lack of variation in this variable within countries over the seven years of the panel. Another explanation could lie in the possibility that the relationship of duration to investment decisions is nonlinear; perhaps Contract Time only really matters when the duration is below a certain threshold. Thus, it is possible that once litigation duration reaches a certain length—say, one year—then additional waiting time simply does not matter, because market participants who are willing to use the courts are already prepared to wait a long time for adjudication. In other words, as the length of time required to litigate increases, the marginal impact of an additional day might decrease. Next, notice that the inclusion of the one-year lagged value of Private Capital Investment produces a positive and statistically significant estimated coefficient on that variable, which is unsurprising. It is somewhat interesting, though, that its inclusion in column 4 seems to strongly affect the estimate on Contract Procedures, which loses significance and half of its magnitude. After the inclusion of additional controls, however, Contract Procedures returns to statistical significance and regains more than all of its lost magnitude. Finally, it is curious that the property-rights protection variable enters in negatively in each of the three specifications in which it is included; one would expect exactly the opposite effect, as better property-rights protections presumably encourage private capital formation.

Table 9: Fixed Effects—Additional Specifications

<i>Dependent Variable: Private Capital Investment (% of GDP)</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Contract Procedures	-0.5789 (0.4704)	-0.6962 (0.5441)	-0.9670** (0.4001)	-0.6780 (0.9788)	-0.5146 (0.9287)	-1.8746*** (0.3734)	-1.5798*** (0.3515)
Contract Time	0.0055 (0.0058)	-0.0004 (0.0102)	0.0182 (0.0171)	-0.0068 (0.0106)	-0.0029 (0.0115)	0.0152 (0.0169)	0.0251 (0.0183)
Lagged Investment			0.1645 (0.1047)			0.2145** (0.1065)	0.1752 (0.1287)
Property Rights		0.0177 (0.0590)	-0.1225 (0.0888)	-0.0787 (0.0910)	-0.0280 (0.0809)	-0.2085** (0.1047)	-0.1512 (0.0974)
Inflation				-0.0105 (0.0696)	-0.0075 (0.0721)	0.0337 (0.0833)	0.0470 (0.0888)
Real Interest				-0.0270*** (0.0028)	-0.0278*** (0.0028)	0.0014 (0.0040)	0.0002 (0.0046)
Trade				0.1077* (0.0579)	0.0743 (0.0611)	0.0733* (0.0392)	0.0402 (0.0369)
Time Fixed Effects	Yes	Yes	Yes	No	Yes	No	Yes
Constant	33.1787* (17.1358)	40.9877** (18.9685)	42.7340*** (16.1190)	39.5020 (34.3672)	31.4346 (32.7406)	74.8040*** (18.2210)	57.8995*** (16.1972)
Prob > F	0.0000	0.0004	0.0000	0.0000	0.0000	0.0000	0.0000
R ²	0.7845	0.7993	0.9137	0.7905	0.7981	0.9095	0.9155
N	629	343	259	283	283	217	217

Note: Robust standard errors in parentheses.

Significance Levels: * <= 0.10; ** <= 0.05; *** <= 0.01

Sources: Heritage Foundation 2002 (Property Rights) & World Bank 2012b (all other variables)

Table 9 presents results from several additional fixed-effects specifications, in order to gauge the effect of including time fixed effects, as well as to investigate the role that lagged investment plays in the model. It appears that the inclusion of time fixed effects has little to no impact on the estimation, but that the lagged investment variable is indeed playing a crucial part in driving the results. Specifically, comparisons between columns 4 through 7 demonstrate that

the inclusion of time fixed effects has effectively no impact on either specification (that is, with and without lagged investment), but that the differences in estimates in the presence versus absence of the lagged investment variable are extremely significant, both statistically and substantively. It is also noteworthy that the inclusion of lagged investment zaps the statistical significance, as well as the magnitude, of the real interest rate.

To summarize, I have estimated several fixed-effects specifications in order to develop a sense of how country-level policy reforms—specifically, efforts to make judicial systems more efficient by reducing delays and procedural complexity—could impact private capital investment. The results are generally consistent with my hypothesis that more efficient systems of contract enforcement will positively impact firms’ willingness and ability to make capital investments. In this respect, the results are also in line with the work of Clague and his colleagues (1999), who found that their measure of the quality of contract enforcement—Contract-Intensive Money—was positively associated with the ratio of private investment to GDP. What my results add to the literature is a focus on efficiency, which is one specific factor of quality, as well as the estimation of a fixed-effects model that can help to predict what country-level policy reforms might be able to accomplish. In other words, although my results are certainly consistent with the literature’s broad findings that institutional quality—and, in particular, the quality of contract institutions—help to explain cross-country variation in income and investment levels, this paper also supplements this conclusion with more detailed information about the country-level effects of judicial-efficiency reforms. This addition to the institutional economics literature could be valuable as a guide to reformers, who will need this kind of detailed information in order to make good decisions about whether and how to intervene to reform developing-country institutions, for the purpose of promoting economic growth.

5.3—Robustness

Given the prevalence of instrumental-variables approaches in the institutional economics literature, it seemed wise to follow this standard method in testing the robustness of my results. Specifically, I adhere to the guidance of Acemoglu and Johnson (2005), who counsel that measures of legal origin are a strong instrument for the quality of contract institutions. I supplement the legal origin variable with another, related measure—colonial origin—and use these two variables together to instrument for Contract Procedures in a two-stage least squares (2SLS) estimation. Although I also estimated specifications that instrumented for Contract Time, these estimations produced undesirable results, as judging from the consistently significant Hansen J statistics. Accordingly, I only present 2SLS results for Contract Procedures, although I do include Contract Time as a control in one specification. This limitation is not very concerning, given that the fixed-effects model seemed to demonstrate that Contract Procedures matter much more for firms' investment decisions than does Contract Time. Additionally, the 2SLS model employs the log of the dependent variable, Private Capital Investment, which differs from previous estimations. This log transformation was necessary due to the Hansen J statistic's significance when using the level form of the dependent variable.

Table 10: Two-Stage Least Squares—Instrumenting for Contract Procedures

<i>Dependent Variable: Logged Private Capital Investment (% of GDP)</i>							
<i>Instruments: Legal Origin and Colonial Origin, instrumenting for Contract Procedures</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Contract Procedures	-0.1039*** (0.0254)	-0.1114*** (0.0332)	-0.0215** (0.0105)	-0.0992*** (0.0369)	-0.0991*** (0.0370)	-0.1201* (0.0642)	-0.1200*** (0.0406)
Contract Time		0.0003 (0.0002)					
Lagged Investment			0.0657*** (0.0042)				
Property Rights				-0.0040 (0.0038)	-0.0040 (0.0038)	-0.0056 (0.0056)	
Inflation					0.0001 (0.0043)	-0.0064 (0.0067)	-0.0064 (.0047)
Real Interest						0.0038 (0.0094)	-0.0016* (0.0010)
Trade							-0.0026 (0.0017)
Constant	6.6633*** (0.9634)	6.7320*** (1.1544)	2.4648*** (0.4200)	6.6295*** (1.4886)	6.6229*** (1.4982)	7.4696*** (2.5185)	7.5419*** (1.6432)
Prob > F	0.0000	0.0009	0.0000	0.0194	0.0450	0.0790	0.0001
R ² (uncentered)	0.9243	0.9190	0.9853	0.9391	0.9392	0.9231	0.9212
Hansen J	1.186	3.948	1.855	0.698	0.690	0.049	0.518
N	612	612	521	340	340	281	479

Note: Standard errors in parentheses, robust to both heteroskedasticity and autocorrelation.

Significance Levels: * <= 0.10; ** <= 0.05; *** <= 0.01

Sources: La Porta et al. 1999 (Legal Origin); Hadenius & Teorell 2005 (Colonial Origin); Heritage Foundation 2002 (Property Rights); World Bank 2012b (all other variables)

Table 10 presents the results of various 2SLS specifications. Most importantly, the estimated coefficients on Contract Procedures confirm the main conclusion from the fixed-effects regressions, which is that the procedural complexity of contract enforcement is associated with a lowering of firms' willingness and ability to make capital investments. As is evident from

a comparison of columns 1–7, the results are robust to various specifications, displaying a remarkable consistency in both statistical significance and magnitude. In general, the results show that a one-unit increase in Contract Procedures is associated with a 0.1% decrease in the ratio of private investment to GDP (remembering, of course, that this ratio is now logged). The only substantial deviation from this relationship appears in column 3, where the inclusion of a control for the one-year lag of Private Capital Investment leads to a decrease in the magnitude of the estimated coefficient on Contract Procedures. This result accords with the pattern seen in the fixed-effects model.

This instrumental-variables estimation generally accords with the results from similar approaches in the literature. Consistent with the work of Acemoglu and Johnson (2005), I found that using legal origins was a viable instrument for contract institutions; in this paper, however, I supplemented legal origins with another, similar variable—colonial origins—which made for a better instrument overall. This could be somehow due to the fact that, unlike Acemoglu and Johnson, the independent variable for which I was instrumenting was a narrower measure of contract-enforcement quality: specifically, procedural complexity. In any event, the combination of legal and colonial origins made for a strong instrument for Contract Procedures, and its exogeneity is supported by the many economists who have used similar historical measures in their own work (Rodrik et al. 2004; Acemoglu & Johnson 2005).

With respect to the estimation results, the 2SLS model produced results that were, as already noted, strongly consistent with the fixed-effects estimations, which in turn were quite consistent with the general findings in the literature. Specifically, the 2SLS model provides further support for both the proposition that contract institutions affect investment (Clague et al. 1999), as well as the broader proposition that institutional quality matters to economic outcomes

(Hall & Jones 1999; Acemoglu et al. 2001; Rodrik et al. 2004). The confirmation provided by the 2SLS approach is important due to the likely endogeneity of contract-enforcement institutions to economic development; in other words, the 2SLS model gives us a better indication that there may be a causal relationship between judicial efficiency and firms' investment decisions, and if this is true it would provide key support for the idea that judicial reform efforts might be able to spur more investment. Of course, one must be careful not to assume causality from these data, but nevertheless we can say with at least some confidence that there appears to be a robust association between contract-enforcement efficiency and private capital investment. And because economic theory articulates causal mechanisms through which such efficiency could affect firms' decision-making, the consistency of these empirical findings with the theory is another encouraging sign that developing countries might indeed be able to promote development through improvements to judicial efficiency.

VI. IMPLICATIONS FOR POLICY

This paper began with a question: Can lawyers help promote economic growth? My answer is: most definitely. The foregoing empirical analyses, focused on an important characteristic of national legal infrastructure, have provided further evidence to support the proposition that institutions matter to economic development. Specifically, this paper has shown that the efficiency of a country's formal system of contract enforcement—that is, the efficiency of its courts—likely exerts an impact on firms' willingness and ability to make capital investments. This conclusion supports the predictions of economic theory, which holds that contract institutions are an important factor in enabling firms to transact with each other, and

therefore the cost of enforcing contracts—in terms of time, difficulty, and consequent legal expenses—should affect firms’ behavior in the marketplace (North 1990).

The question arises, therefore, as to the relevance of this evidence for policy makers who would like to promote economic development. An examination of the parameter estimates from this paper’s fixed-effects regression analyses yields the suggestion that reductions in the number of procedures required to enforce a contract could yield an increase in a country’s level of private capital investment. The length of time required to enforce a contract, on the other hand, appears to explain some of the cross-country variation in investment levels, but loses significance in fixed-effects estimations, which suggests that within-country reductions in litigation processing time might not be an effective lever for promoting capital investment. Nevertheless, the significant parameter estimates on the number-of-days variable in the pooled OLS models is at least suggestive of the relevance of duration to firms’ decision-making; it certainly stands to reason that, all else being equal, firms would prefer a court system that processed claims quickly rather than slowly. Further, more micro-level research could usefully bridge the gap between this paper’s pooled OLS and fixed-effects findings, with respect to whether and how the duration of contract enforcement might affect firms’ behavior.

The paper’s main findings, though, rest on the interpretation of the parameter estimates associated with the procedural complexity of contract enforcement, as they were estimated in the fixed-effects model and confirmed in the 2SLS robustness check. Although these findings, as already noted, seem to confirm my central hypothesis—and, moreover, are consistent with empirical work on judicial reform (see Botero et al. 2003)—there are numerous reasons why the results should be interpreted cautiously. First, it is quite plausible that the measure of procedural complexity is proxying for the existence of bureaucracy and corruption in national economies. If

this is true, then the effect of reducing the number of procedures required to enforce contracts might be negligible, if a country's governance system still suffers from more general flaws. Another related point of caution is that although most procedural requirements might not be particularly problematic for firms to navigate, *certain* of these procedures could provide opportunities for corrupt officials to seek rents. Thus, it might not be the procedures themselves that are the problem, but rather that they provide a convenient avenue for corruption to work its way into the justice system. If this is true, then a more nuanced approach would be required to meaningfully improve contract-enforcement efficiency; specifically, more micro-level data would need to be collected to determine whether certain types of procedures—for example, those requiring signatures from government officials—provide ripe opportunities for rent-seeking. Such a determination could lead to a policy prescription involving reforms of judicial systems targeted at those specific procedures through which corruption infects the courts. Of course, if the corruption is endogenous to the government, then such reform may face serious challenges.

It must also be acknowledged that judicial efficiency cannot be examined in isolation from other aspects of the court system. Indeed a single-minded focus on efficiency could lead to policy prescriptions that produce adverse consequences on other valuable elements of judicial institutions, which may affect not only economic development, but also justice concerns. To be more specific, consider this paper's findings with respect to the procedural complexity of contract-enforcement: a simple policy implication might be that countries should simply reduce the number of procedures required to enforce a contract. But jumping to this conclusion without considering other factors would be premature. Court procedures are often in place in order to protect litigants, and thus to ensure that justice is done. For example, the requirement that a firm bringing a lawsuit notify the other party is a crucial mechanism of fairness (see Solum 2004).

Thus, there is a danger that a focus on reducing the procedural complexity of litigation, without taking into consideration other important concerns, could lead to unintended negative consequences. In fact, the balance between the procedural protections of litigation and its consequent inconvenience has been fought out in the United States in debates over private arbitration. Firms often prefer to arbitrate, rather than litigate, commercial disputes precisely because arbitration offers reduced procedural complexity and the promise of a quick decision (Carbonneau 2012). But many have argued that the loss of procedural protections that accompanies arbitration's simplicity leads to an unfair advantage for large firms that arbitrate frequently (e.g., Feingold 2002). Drawing on these types of considerations, policy makers should be wary of pursuing judicial reforms that are focused on efficiency alone, as the benefits of efficiency might come at the expense of other values that the justice system serves. Ultimately, a court system that is fast and simple, but which cannot ensure fairness, could be just as harmful to potential litigants as inefficiency currently is.

United States Supreme Court Justice Felix Frankfurter once wrote, "There can be no free society without law administered through an independent judiciary" (1947, p. 312). This paper firmly relies on that proposition, and adds to it the qualification that even an independent judiciary, if unable to efficiently adjudicate contract disputes, will negatively affect the market conditions in which free exchange should occur. Admittedly, improving slow and complex court systems is challenging work (see World Bank 2012a). Nevertheless, this paper has shown that countries might be able to unlock unrealized capital investment activity by making contract enforcement more efficient. Although these efforts must be balanced against other values, such as procedural justice, it appears clear that well-designed judicial reforms should be a part of any country's plan to promote economic development.

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