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Contract Enforcement and Investment: A Systematic Review of the Evidence

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Summary. — This "systematic review" focuses on the empirical research that evaluates the causal link between contract enforcement and investment. The evidence available in a variety of academic media, reviewed with established procedures, provides some but weak support for the existence of such link. During 1990–2010 we only found 19 independent studies that empirically test the relationship, and only one that directly examines the effects of an actual institutional reform. Few of the studies test alternative explanations, perform robustness checks, or critically assess the findings. In sum, the broadly accepted hypothesis of direct causation is still awaiting strong empirical backing.

Key words — investment, institutions, contracts, judiciary, business environment, hold-up

1. BACKGROUND AND RATIONALE

The key role of capital accumulation in economic development has been almost a truism in economics since the classical economists (Smith, Ricardo, Marx). Investment was one of the obvious ways to promote economic growth in their theories as well as in the "modern" growth models (Barro, 1991; Mankiw, Romer, & Weil, 1992; Solow, 1956), and the specific circumstances of underdeveloped economies in this regard have been extensively explored since development economics became a recognizable sub-discipline (see, e.g., Hirschman, 1958; Rostow, 1960; and various contributions in Meier & Seers, 1984).

In recent years, institutions have become more prominent in the scholarly literature, among the factors that could determine growth and investment performance, (North, 1990; Rodrik, 2000; Shirley, 2008). The literature has identified a host of growth and investment-relevant institutions, and their direct or indirect channels of influence. This has led, among others, to the "business environment" and "investment climate" approaches to institutional reforms for growth and poverty reduction (see, e.g., OECD, 2004). Salient among the key institutions are those that protect investors from expropriation and those that determine how contracts are enforced. Though these two classes of institutions may overlap, they are not conceptually identical, and we will argue (as have done; e.g., Acemoglu & Johnson, 2005) that it is analytically desirable to try to disentangle their specific effects.

This article weighs the empirical evidence on one specific link between institutions and economic performance. In particular, we undertake a *Systematic Review* (SR) on the connection between contract enforcement and investment.

(a) *Contract enforcement*

Various economic historians, including North (1990), have argued that the enforcement of contracts became more involved and consequential with increased specialization, larger numbers of trading partners, and geographic dislocation of transactions. With complex contracts it became necessary to devise some form of third-party enforcement. In fact, in modern societies, the three forms of exchanges and enforcement arrangements (tacit, explicit-informal, and formal) coexist, and even archaic and seemingly dysfunctional informal rules can have major impacts. The different behavioral outcomes in countries with similar legislation are a confirmation that informal rules can be powerful (Berkowitz, Pistor, & Richard, 2003).

These historical and theoretical issues are still debated between those that take the view that formal enforcement mechanisms (and contract law) are essential for development and those that believe that informal enforcement mechanisms (and contract arrangements) could be sufficient (North, 1990; Greif, 2006). In the middle, some advance the idea that different enforcement arrangements can work well at different levels of development and/or for different country contexts. The "varieties of capitalism" literature maintains that liberal economies rely more on standard market relationships and enforceable formal contracts than coordinated market economies, where dense business networks and associations disseminate reputations allowing firms to operate on the basis

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of informal contracts (Hall & Soskice, 2001). In the same vein, communalist societies resort more to intra-group sanctions, meanwhile individualist societies rely more on formal contract enforcement (Greif, 2005, chap. 28). Trebilcock and Leng (2006) favor stages of development argument: for low levels of economic development informal contract enforcement may be a good substitute for formal enforcement, but for higher levels of development formal contract law and enforcement are necessary.

The coexistence of formal contracts and informal (frequently tacit) arrangements is also highlighted by Macaulay (1963), who investigates when and why managers decide to formally enforce a contract, and the reasons why third-party enforcement is not invoked more often when the letter of a contract would favor the claimant. He concludes that "businessmen often fail to plan exchange relationships completely, and seldom use legal sanctions to adjust these relationships or to settle disputes" (p. 55). This implies that also individualist, liberal, and developed economies use informal contracts and enforcement arrangements much more extensively than generally assumed in the law and economics and institutionalist literatures. This line of reasoning would conclude that what matters for development is the existence of *some* enforcement capacities rather than specific formal laws and enforcement mechanisms.

Regarding the *actors* of the enforcement process, in contemporary societies, enforcement can come from societal sanctions, from second-party retaliation or from a coercive third party (typically, the state). Given the diversity of contracts, legal traditions, codes, and informal institutions, there is a range of possible innovations that may directly or indirectly impact on the enforcement of contracts. Typically, donor-funded reform programs, for example, tend to tackle simultaneously a number of perceived gaps in the written laws and the functioning of the judiciary (for example, fixing loopholes in commercial or civil legislation, creating non-judicial arbitration mechanisms, facilitating access to the judicial system, reducing costs of litigation, strengthening the capacities of the courts and judges). All of them have some bearing on the speed and effectiveness of contract enforcement, and more broadly on "the rule of law" (see, e.g., World Bank, 2001), but they focus on formal institutions and assume that the rule-enforcement distinction is unproblematic.

(b) *Contract enforcement and investment*

This article focuses on a relatively narrow, yet fundamental "corner" of the broader "institutions and economic performance" literature. Figure 1 is a graphical representation of the field. In the top block, research strands are organized according to the nature of the institutions that might impact on economic performance. Political institutions that set the rules about who governs and how (investigated among others by Alberto Alesina, Thorsten Persson,and Barry Weingast), and legal rules excluding those that allocate political power (i.e., what is most often understood to be "the Law" in "Law and Economics") are represented as the complements of informal economic institutions (investigated, for example, by Sam Bowles, Herbert Gintis, Pranab Bardham, and others). For our purposes it is useful to distinguish within the Law and Economics space a sector that gathers works on the effects and determinants of unilateral state regulations (studied, e.g., by Peter Klein and Pablo Spiller, among others), and which is distinct from the research on institutions that support voluntary dyadic exchanges (exemplified by writings by Benito Arruñada, Katharina Pistor or Holger Spamann, but also by Thorsten Beck and Ross Levine. [1]

Theoretical and empirical works can be thought to cluster around some of the (causal) connections between "institutions" from the Figure's top block and economic outcomes from the bottom area. The similarity between the labels that would result and the sections and chapters of a comprehensive handbook such as Menard and Shirley (2005) suggest the diagram is a reasonable representation of the relevant research fields. In that framework, work on "contract enforcement" and investment occupies a narrow but fundamental space, encompassing formal and informal contracts and enforcement mechanisms (the figure has to be interpreted as covering also studies that would challenge the simpler formal-informal dichotomy, such as Macaulay's, or that highlights the incompleteness of the Law and the ensuing ambiguities of enforcement; Pistor & Xu, 2003). The rationale for the restrictive definition of our focus is discussed below but we can say in advance that it is best suited to undertaking a thorough appraisal of the evidence in support of an influential hypothesis.

The diagram reflects that research on the link between contract enforcement and investment is a cross-cutting sector of the complementary fields of Law and Economics and the study of economic consequences of informal institutions. As defined in this article, the field of unilateral regulations by governments does not intersect with our object of analysis; nor does the field of political institutions and economic outcomes).

Analytically, weak enforcement of contracts has been argued to impact on investment through a number of channels. First, it could most directly influence the uncertainty surrounding a project, and therefore influence investors' decisions by increasing the project's costs, reducing its expected returns, causing both, or generally increasing the value of the "wait" option (Dixit & Pyndick, 1994).

Second, weak enforcement could act indirectly on agents' willingness or ability to invest: it could induce them to choose less-efficient technologies, inhibit them from building relation-specific assets when those relations are dependent on contracts, or amplify the adverse effects of infrastructure or regulatory shortcomings. All these could in turn affect a firm's access to external financing, while capital markets and the banking industry might be more generally crippled by an environment of insecure contracts. These various channels and some others may combine in complex ways. For example, some authors have found analytical support for the idea that weak enforceability increases firms' "sensitivity to the arrival of new technologies and generates greater macroeconomic volatility" (Cooley, Marimon, & Quadrini, 2004). To the extent that aggregate (output) volatility influences investment, there would be a causal chain from enforcement of contracts to capital accumulation. Others have argued that, through financial contracts, imperfect enforcement influences the size distribution and heterogeneity of firms, which could be reflected in aggregate investment levels (Monge-Naranjo, 2009).

(c) *Empirical approaches*

Research on the effects of institutions on economic performance has grown very rapidly since the early 1990s. Theoretical developments such as North's contributions have prompted the search for and elaboration of indicators and proxies for the introduction of institutional variables in empirical (usually econometric) analyses (see, e.g., Knack & Keefer, 1995; Kaufmann, Kraay, & Mastruzzi, 2005). The proliferation of datasets that include such indicators have stimulated

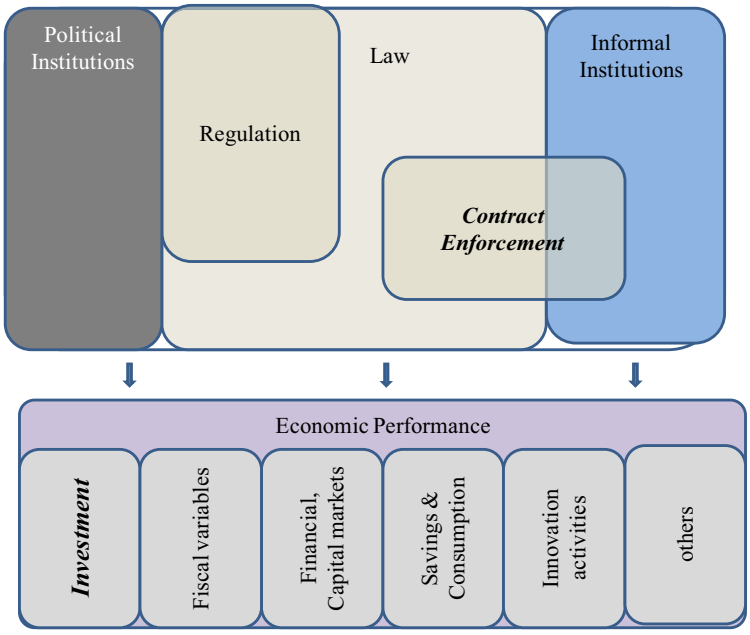

Figure 1. *Contract enforcement and investment in a map of relevant literatures.*

the empirical investigation of a range of research questions, but also some "data-driven" research that has not shed much light on the specific causal pathways that link institutions and economic outcomes (on the latter, see similar observations by Aaron, 2000; Keefer, 2004; Williams & Siddique, 2008).

With some noteworthy exceptions, the empirical literature normally examines recorded variations in certain institutional arrangements that may or may not follow from deliberate reforms. This state of the field results from key features of the object of analysis. Principles of territoriality of the law and equality of all citizens before the Law, in addition to the typical complexity of legal reforms, make this an inhospitable territory for randomized controlled trials or other quasi-experimental strategies requiring control groups. In the best scenarios, diachronic variations are exploited in search of some support for causal hypotheses. In some cases, "recall" indicators in cross-section surveys of key informants are the best data available to attempt to detect a causal link.

The closest to the experimental ideal are exploitations of natural experiments. With observational evidence dominating the research field, instrumental variables methods are the other strategy to deal with potential endogeneity issues and provide some support for causal inference. The broader weaknesses of much of the evidence (including issues of validity of indicators and broader empirical strategy), and the need to explore new research designs, have been noted by several authors (Bazzi & Clemens, 2013; Pande & Udry, 2006, chap. 14; Rodrik, 2005; Rehme, 2011; Shirley, 2008).

(d) *Systematic reviews*

To our knowledge, no systematic assessment of the research on the topic has been conducted until now. To start filling this void, we undertake a SR of the empirical work on contract enforcement and investment. SRs are a meta-research methodology for the identification, treatment, and synthesis of *primary studies* addressing an empirical question.[2] They originate (like *Meta-Analysis* or MA) in the efforts to improve the use of research in policy-making and practice.[3] SRs differ from other reviews in that they attempt to be exhaustive, replicable, and bias-free.[4] They typically cover published and unpublished studies, seeking to avoid some known sources of bias, including *publication bias* (Egger, Jüni, Bartlett, Holenstein, & Sterne, 2003; Petticrew and Roberts, 2006, chap. 1). SRs are especially well suited to getting a sense of the weight (quantitative and qualitative) of the evidence supporting empirical claims. Undertaking one of this kind of assessments on a key empirical question is the main contribution of this article.[5]

As it is the norm for systematic reviews, we developed a protocol that stipulated inclusion and exclusion criteria, methods for identification of primary studies, data extraction, quality assessment, and synthesis, all based on accepted practice in the literature on research synthesis (see Petticrew & Roberts, 2006; the peer-reviewed protocol is Aboal, D., Noya, N., & Rius, 2010, available on request). The protocol was peer reviewed by experts hired by the project funder, the United Kingdom's Department of International Development (DFID).

The studies reviewed here yield estimates of quantitative effects of variation in the quality of contract enforcement on investment, based on observational data of a cross-section, panel, or longitudinal structure. The review does not take it for granted that investment strictly *causes* growth (though it is justified, to some degree, by the assumption that facilitating investment will benefit the growth process).[6] Moreover, the review does not examine all the linkages from quality of institutions to investment (e.g., we do not examine studies on quality of property rights institutions more broadly defined, such as a political system of checks and balances or international investment agreements, etc.).

(e) *Rationale*

Besides shedding light on a substantively important set of theoretical hypotheses, the rationale for the review rests particularly on the fact that donors and governments do allocate financial, human, and political resources to improving the business environment, and in particular to trying to

improve the enforcement of contracts. According to the final statement of the Independent Panel (2013) reviewing the World Bank (WB) Group's *Doing Business* report "[t]he Bank has advised over 80 countries on reforms to regulations measured in the report."[7] The 2012 issue World Bank (2012) mentions that the reports of the period 2005–12 have recorded at least 114 reforms of courts related to commercial dispute resolution, and 23 economies that introduced or expanded specialized commercial courts with the objective of improving the enforcement of contracts. The WB monitoring indicators on contract enforcement are one of the most frequently used in the institutions-performance literature, revealing the close relations between policy and research in this area.

The WB is not the only institution promoting these reforms although it has had a leading role. Other backers include the International Monetary Fund (IMF), the Organization for Economic Co-operation and Development (OECD), the UK's DFID, the Swiss Agency for Development and Cooperation (SDC), and the United States Agency for International Development (USAID). They have supported or encouraged "rule of law" programs in different countries. To illustrate their importance and expected results, consider the case of Rwanda. The country has been reforming its commercial laws and institutions since 2001, supported by the World Bank. It was named the top reformer in Doing Business 2010, for passing a number of legal reforms that largely amount to the importation of American-style code of contract based on common law. In another example, USAID established a three year "Contract Law Enforcement Program" in Kosovo in 2013, with the objective of "assisting Kosovo institutions to improve the enforcement of civil judgments while dramatically reducing the backlog of enforcement cases in the Kosovo court system, and ... assisting Kosovo in developing and strengthening its contract and commercial law framework and systems, including mediation."[8]

While some of these efforts could be justified simply on normative arguments about the development significance of the rule of law, the underlying assumption for many of those reform efforts is that investments will be unleashed by them. Therefore, analyzing systematically the strength of the evidence in favor of the latter assumption may eventually help in deciding whether, how, and how much to invest in supporting reforms of that type.

In the next section we describe the methodology of the SR. The Findings section follows and it has four parts: descriptive synthesis; quality-weighted synthesis; contexts, mechanisms and outcomes, and general issues. The final section concludes.

2. METHODS

We searched for primary studies in the pre-specified sources, identifying those of interest on the basis of predetermined inclusion and exclusion criteria (the issues and solutions discussed in this section are more extensively dealt with in Aboal, D., Noya, N., & Rius, 2012). Those criteria set boundaries to the review regarding eligible types of study designs, definitions of contracts and enforcement, measured outcomes (i.e., definition of "investment"), tested enforcement-investment link, accessibility, historical scope, and publication date.

In general, the methods applied are well-established in the research synthesis field and have been designed to avoid or minimize risks of bias (Petticrew & Roberts, 2006, pp. 232–236 is the source for this paragraph). Mitigation of "publication bias," for example, is one of the purposes of including unpublished work. As understood in the research synthesis field, "publication bias" refers to the tendency of journal editors and researchers to suppress "non-significant" results on the basis of their supposed lack of editorial interest. The inclusion of gray literature can reduce such bias and is normally complemented with other protocol provisions to protect reviews from avoidable bias. For example, SRs strive to assess studies in their own technical merit and do not pay attention to external information about the authors or sponsors that do not strictly reflect technical merit of the study.[9]

We chose to limit the review to studies that empirically address the link or a causal chain from (changes in some indicators of) contract enforcement to (some customary measure of) investment. The demarcation criteria resulted from both conceptual and practical considerations. First, the existence of a relationship between contract enforcement and investment is often assumed and it has important policy implications, but the strength of its empirical backing was unknown. If institutions of contract enforcement are going to have any impact on growth, the "investment channel" is going to have some role in almost any economic explanation. The strength of the evidence on the link would determine the soundness of a lot of policy advice being offered and the pertinence of related research agendas.

Second, we were aware that various institutions (besides those regulating contract enforcement) had been linked in the literature to the growth process, and in particular those that relate to protections against expropriation. Our work's focus on a specific segment of the institutions-development field was not meant to neglect those other factors but to define a tractable object of analysis. We hope that our contribution could then be followed by reviews of other institutional arrangements, seeking integration in a broader framework over time.[10]

On these bases, we did not set an exclusion criterion for studies that seek evidence for involved causal pathways, provided that the whole causal chain that answers our question (i.e., from enforcement mechanisms to investment) was empirically tested. This approach is a response to studies that examine some shorter causal chain (for example, from contract enforcement to financial depth) and draw conclusions on the effects of institutions on investment without providing the evidence of a link (for the same population) from financial depth to investment. Similarly, we excluded studies that focus on other downstream outcomes such as growth, and include measures of contract enforcement among the explanatory variables, but do not test specifically an enforcement-investment channel.

We searched four academic bibliographic databases (Academic Search Complete, Scopus, Jstor, and Econlit), a major working papers repository (RepEc), and an online academic citation tool (CiteULike).We made extensive use of Google Scholar, and its integration with the professional databases available to the local scientific community through the National Innovation and Research Agency.

Searches started with broad parameters derived from our research question and inclusion/exclusion criteria, and were developed with the advice of our information specialist. These searches were gradually narrowed with guidance from three external reviewers contacted by DFID, including one information specialist. All searches were stored to ensure replicability. Two reviewers applied the inclusion and exclusion criteria independently, first to titles and abstracts only, for all items identified by the database searches (initial number: 2,229).

We also followed a "snowballing" strategy to identify relevant references from the bibliography sections of key pieces in the thematic field, and later in those studies' bibliographies, and so on. Overall, the snowballing search led to the identification of an additional 317, non-duplicated, potentially relevant studies (12% of the total studies screened), and 8 (35%) of the final 22 studies included in this systematic review. We scanned titles and checked abstracts for two hundred of the most recent entries (as of 1st of March 2011) in the New Institutional Economics e-library of the Social Sciences Research Network (SSRN; www.ssrn.org), which yielded no new relevant items.

Studies were then either (i) excluded from further consideration, (ii) included in the set of those moving to the next stage, or (iii) marked as "pending" if the reviewer was unsure about their inclusion. The two independent reviews were compared and differences in assessments were discussed and resolved (when no agreement was reached, a third reviewer was asked to enter the discussion until the disagreement was resolved or there was a vote). All the "pending" were resolved and all the "included" were then moved to the full text review phase, where a similar procedure was applied.

Figure 2 summarizes the number of studies dealt with at each stage of the search and screening process.

From the systematic search and screening, 22 primary sources were identified that met the eligibility criteria. These include two sets of items that contained different reports or versions of reports from members of a single team. We count each set as one study, for quantitative description, and in the rest of the analysis we include the best or more recent version, as it is common practice in SRs.[11] This brings the total number of "synthesized items" to 19.

We critically assessed the methodological quality of studies included in our final set before entering them into the synthesis process. We applied two instruments from the literature to capture different aspects of study quality. The "Weight of Evidence" instrument (WoE) was developed by Gough (2007) and is designed to allow rating studies in four dimensions: quality of study execution, research design (with Randomized Controlled Trials as the golden standard and other ratings decreasing from there), focus of the study, and overall quality (a synthetic measure of the previous). The resulting rating is transformed into a five-point scale of quality (low, medium–low, medium, medium–high and high). The second instrument (CI, for "causal inference") seeks to capture the strength of claims of a causal explanation on the basis of the study's design and execution. This allows the reviewers to distinguish stronger and weaker studies among non-experimental ones. A global rating was given to each study, which responds to the question "Can the study deliver inferences about the causal mechanisms at work?"; and the answer was coded at three levels (Yes = 3; Yes, some = 2, or No = 1). The use of this instrument is justified by the intent of the review, which clearly sought to answer an "effect" question in a field where the norm is observational. These quality ratings provide us with synthetic quality indicators, and to use them to qualify specific findings.[12]

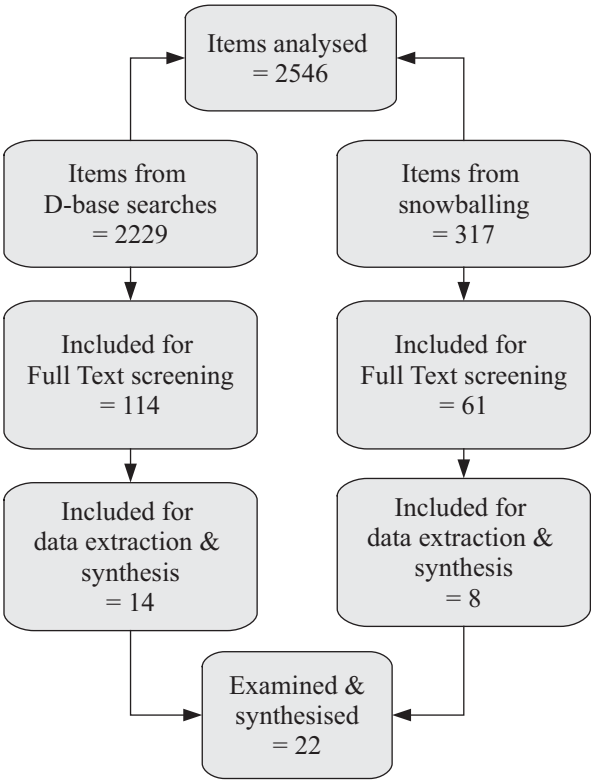

Figure 2. *Search and screening statistics.*

Our review follows standard systematic review procedures and adopts an approach to synthesis that combines narrative synthesis with "quality-adjusted vote counting" (Gough, Oliver, & Thomas, 2012; Petticrew & Roberts, 2006). To some extent, the synthesis follows the "realist" approach, regarding the attention paid to patterns of context-mechanism-outcome (CMOs; Pawson, Greenhalgh, Harvey, & Walshe, 2004). This means that we are not only interested in discerning whether changes in the enforcement of contracts have been followed by increases in investment, but also in the mechanisms that might cause changes in enforceability to influence agents' investment decisions and thus induce changes in aggregate investment levels.

MAs, in turn, require a minimum number of estimations of an empirical parameter, from a "population" of studies sufficiently homogeneous to justify the quantitative synthesis. Typically, MA collects "effect size" estimates from primary studies (or the effect regression parameter when applicable) and computes statistical estimates that summarize the evidence. While MAs can handle diverse samples, model specifications, estimation methods, and even institutional context of the specific studies (i.e., whether sponsored by an interested party or an independent funder), a justification for MA must demonstrate that there is not a mixing of "apples and oranges" (see Petticrew and Roberts, 2006, pp. 203–208). After we completed our SR, we decided not to conduct a MA of the identified studies, considering the advice from the research synthesis literature and the large between-studies heterogeneity in terms of research designs, independent variable constructs, and outcomes, which will be evident in the next section.[13]

3. FINDINGS

(a) Descriptive synthesis of the literature

The analyzed studies have been published mostly in refereed journals (13 studies), but also in working papers series or as conference presentations (6). All but one are available in English. In the studies being synthesized there is a positive association between studies' quality—rated by the review team—and publication in peer-reviewed journals, as well as between study quality and the journals' rank by impact factor.

This correlation cannot be assumed in general as bias from the publication process can affect specific fields differently. [14]

While the majority of studies focus specifically on contract enforcement (15), or on the broader concept of institutions and investment (11), some evidence valuable to answer the research question comes from studies that have slightly different foci. [15] The nature of the "innovations" for which "impact" is sought greatly influences the nature and quality of the evidence available, and therefore the inferences that can be made and the conclusions that can be drawn. The studies that attempt to address the systematic review question are all statistical analyses of non-experimental data, so the group is mainly composed of study designs that cannot be conclusive regarding causality.

Three studies use the same indirect measure of quality of contract enforcement, including the early proponent of the indicator (Clague, Keefer, Knack, & Olson, 1999). These studies measure quality of enforcement through the concept of "contract-intensive money" (that is, the ratio of non-currency money to the total money supply). They argue that "the same governmental deficiencies that require self-enforcement of transactions also lead economic actors to prefer currency" (Clague *et al.*, 1999, p. 188). While many of the studies raise issues of validity of indicators, this is perhaps the measure of contract enforcement that requires stronger "assumptions" to be regarded as a valid construct.

A large proportion (15 of the 19 studies) use ratings or reports of the quality of enforcement based in some way on the views of experts and/or businessmen. These include studies exploiting data generated by various World Bank-led initiatives on the business environment, as well as those exploiting secondary data generated since the 1970s by investor advisory companies such as International Country Risk Guide (ICRG) or Business Environment Risk Assessment (BERI), and the Heritage Foundation (on the strengths and limitations of these indicators, see Knack & Keefer, 1995; Williams & Siddique, 2008). A minority of studies chooses to elaborate more replicable constructs. These include the study that actually records legal innovations affecting India's states differently but simultaneously (Chemin, 2006), studies using the resulting ratings from Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2003) based on a cross country battery of indicators of litigation costs, and the studies mentioned that use a proxy based on monetary aggregates.

The studies also vary in terms of the measure of investment used. Twelve studies measure investment as the (net or gross) addition to the stock of capital over GDP (Gross Domestic Product) or over the stock of capital itself. One study measures the level of investment in infrastructure. Three studies focus on the effects of contract enforcement on the investment in R&D. Three studies use other indicators: investment as binomial choices over a range of productive assets; value-added elasticities of investment across industries; and the size of firms' inventories of finished products.

The choice of measures is not unrelated to the choice of unit of analysis and methods. Notably, all included studies considered domestic investment of some kind as their outcome variable, and none focused on foreign investment. This results partly from the design decision to include FDI-related studies if effects on new/fresh/"greenfield" investment could be discerned from those related to mergers or acquisitions that leave the countries' aggregate stock of capital unchanged.

Generally, a majority of the studies—and notably some of those that rate highest in our quality appraisal scales—are based on cross-section analyses with varying degrees of ingenuity and competence in the choice of variables, indicators lag structures. The studies display no straightforward correlation between tools of quantitative investigation and unit of analysis.

Table 1 presents the primary studies organized according to unit of analysis, structure of the datasets, and context of the study. The small number of studies that are able or willing to investigate longitudinal variations in data could anticipate that the literature would be constrained in testing causal hypotheses, given its general difficulty in observing "before–after" variations.

As argued by some authors (e.g., Rodrik, 2005), even the richest data sets available to test "institutions-performance" hypotheses at the aggregate level are plagued with endogeneity issues. Controlled trials, studies of natural experiments, or instrumental variables estimations are the usual strategies for causal inferences. Not surprisingly, our best quality studies (in gray in Table 1) resort to the latter two strategies, but several others fail to consider the endogeneity concerns and proceed (with few or no caveats) to use estimation methods that cannot sustain a causal attribution.

This echoes the concerns of authors like Pande and Udry (2006, chap. 14) that too much emphasis has been placed by the "institutionalist" approach in analyzing cross-country differences, to the detriment of other sources of institutional variation that remain under-studied. Moreover, "insights" on institutions and performance deduced from observed cross-country variations may be vulnerable to the "ecological fallacy," so their translation into policy cannot be mechanical. [16]

(b) *Quality-weighted summary of findings*

The main results and features of each of the nineteen included studies are presented in Table 2. As discussed in the previous section, study quality was rated for all identified studies based on two complementary scales. In general, as shown in (Aboal et al., 2012), the two quality indicators used are strongly correlated but this still leaves room for individual specificities that are worth looking at.

In this section we start from key findings of the six best quality items (summarizing them briefly). [17] Then, we examine whether conclusions change when we include information from the next best four studies. (In section (c) we consider the full set of primary studies, including low-quality ones, to explore patterns in the studies' context, hypothesized causal mechanisms and observed outcomes (i.e., CMOs).

In addition to the treatment of endogeneity and causality, the key ingredients in evaluating the quality of the studies were the performance of robustness analyses of their results, and the validity of the contract enforcement indicators used.

The six top-rated studies have made more or less successful attempts to address in one way or another the issues of endogeneity and causality. Four of them have used instrumental variable approaches (Acemoglu & Johnson, 2005; Levine, 1998; Lin, Lin, & Song, 2010; Pang & Wu, 2009), one of them a difference-in-differences approach (Chemin, 2006) and the final one exploits the time dimension to argue that the causality goes from contract enforcement to investment (Clague *et al.*, 1999). All of them have performed robustness tests, either by introducing new variables in the regressions, by using different methods of estimation or by analyzing different data samples. The strength of the enforcement indicator (or its "validity") is variable across these studies. Chemin (2006) exploits changes in the legislation, Clague *et al.* (1999), on the other hand, use the "Contract Intensive Money" (CIM) whose validity has been questioned. Acemoglu and Johnson (2005), Levine (1998), Pang and Wu (2009) employ indicators

Table 1. *Studies by unit of analysis, structure of data and estimation method*

Structure of data set - as used	Unit of analysis					
	Countries			Firms or industries		
	Study	Context	Effect	Study	Context	Effect
Cross section	Acemoglu & Johnson (2005)	71 countries that are former European colonies. Economic performance measured ca. 1995 and averages over 1960 or 1970 to 1995-98.	NS	Cungu & Swinnen (2003)	Hungary in transition (firms surveyed in 1998). Agricultural enterprises that contract production to processing firms.	+ (ind) NS(dir)
	Brunetti *et al.* (1998)	52 countries, firm managers' views on policies/institutions ca. 1996 and ten years earlier. Economic performance measured 1970-92.	+	Raja & Schaefer (2007)	378 firm-level observations across 14 comparable products and 39 developed and developing countries. Years covered are 1997 to 2003.	+
	Clague *et al.* (1999)	95 developed and developing countries. Economic performance measured between 1970 and 1990.	+	Long (2010)	1,500 Chinese firms from five cities, 2000.	+
	Commander & Tinn (2008)	It is not clear how many countries were included in the regressions. Data on contract enforcement for the year 2003.	(-)/NS	Lin *et al.* (2010)	Over 2,400 firms in 18 Chinese cities, 2003.	+
	Dao (2008)	36 developing countries, 2001-04.	+	Pang & Wu (2009)	923 industry-country observations, for 1963-2002. Developed and developing countries. Data on enforcement ranges from 1998-2007.	+
	Levine (1998)	43 countries that were European colonies, 1975-93.	+ (ind)	Chemin (2006)	Indian states around 2002.	+
	Banerjee *et al.* (2006)	40 developing economies, 1990-2000.	NS			
Time series	Prados de la Escosura & Sanz-Villarroya (2009)	Argentina over 1875-2000.	+			
Panel	Acevedo & Mora-Mora (2009)	20 Latin American countries over 1995-2003.	+			
	Alfranca & Huffman (2003)	Seven EU countries between 1984 and 1995.	+ (I)			
	Clarke (2001)	52 developed, middle- and low-income countries, 1983-1994.	+			
	Le (2004)	25 developing countries, 1975-1995.	+			
	Poirson (1998)	53 developing countries, 1980-1995.	NS			

Notes: "Grayed" items are highest quality (see Section 2); (−)/NS: two regressions yield NS coefficients, one yields a significant negative; +(ind): estimated relationship is positive but indirect (through another intermediate outcome); +(I) positive effect shows up only when contract enforcement is allowed to interact with variable for efficiency of bureaucracy; (ind) = indirect effect; (dir) = direct effect.

Table 2. *Summary of papers' findings*

Authors	Contract enforcement (CE) indicator(s)	Investment indicator(s)	CE variables' sign (signs shown only if significant)[a]			Other results (for at least one of the CE variables in other regressions)	Main conclusion (according to authors when available)[c]	Quality assessment	
			Var. 1	Var. 2	Var. 3			WoE	CI
Acemoglu and Johnson (2005)	Legal formalism; Overall procedural complexity; and number of distinct procedures to collect debt	Investment ratio to GDP	NS	NS	NS	-	Contracting institutions affect the form of financial intermediation, not investment (they are superseded by property right institutions)	med	3
Acevedo and Mora Mora (2009)	Judiciary ability to provide legal support and protect private property	Investment ratio to GDP	+			NS	The variable judiciary is positively correlated with Investment/GDP	low	1
Alfranca and Huffman (2003)	Relative degree to which contractual agreements are honored and complications presented by language and mentality difference	Aggregate private investment in agricultural R&D	+				Better contract enforcement increases aggregate private agricultural R&D investment	med/low	2
Banerjee *et al.* (2006)	Rule of law indicator. ICRG	Greenfield infrastructure investment	NS			–	Better contract enforcement is not correlated with greenfield investment in infrastructure (reviewers' conclusion)	low	1
Brunetti *et al.* (1998)	Predictability of judiciary enforcement	Annual average of investment in percent of GDP	+				Predictability of judiciary enforcement is positively correlated with investment	med/low	1
Chemin (2006)[b]	Count of amendments that increase judiciary efficiency	Net addition to plant and machinery assets. Net addition to tools and other fixed assets. Net addition to transport and equipment assets	+ (for the 3 inv. var.)				A speedier judiciary decreases the probability of experiencing a breach of contract and increases the incentives to invest	high/med	3
Clague *et al.* (1999)	Contract-intensive money	Investment as a percentage of GDP	+				CIM is positively related to investment	med/low	3
Clarke (2001)	Rule of law indicator	Research and Develop. Expenditure as% of GDP	+			NS	Countries with weaker rule of law tend to have lower R&D expenditures	low	1
Commander and Tinn (2008)	Procedures/time/cost	Gross fixed capital formation to GDP	–	NS	NS		Investment is unrelated to most Doing Business indicators, while there is a weak association with procedures to deal with licenses and contract enforcement	med/low	1

(*continued on next page*)

Cungu and Swinnen (2003)	Ineffective court enforcement of contracts	Flow of gross capital investment as a% of gross capital stock	– indirect effect, NS direct effect				Ineffective contract enforcement positively affects delayed payments. Contract breaches, in the form of delayed payments, have a significant negative effect on investment level	med/low	2
Dao (2008)	Courts constraint (are courts a major or very severe constraint to CE)	Share of gross capital formation to GDP.	–				Courts constraints linearly affect the share of gross capital formation to GDP of developing countries	low	1
Le (2004)	Contract-intensive money. Variability of contract-intensive money	Private investment as a percentage of GDP	+	+			Contract enforcement and its variability are positively correlated with investment	low	1
Levine (1998)	ENFORCE (efficiency of legal system)	Per capita capital stock growth	+				The data indicate a close relationship between the legal system and banking development, and between banking development and capital stock growth	med/low	3
Lin *et al.* (2010)	Contract (whether a firm usually signs written contracts with its clients) Contracts upheld? (managers opinion)	R&D spending as a percentage of sales)	+	+			Contract enforcement plays an essential role in promoting corporate R&D	med/low	3
Long (2010)	Quality of local courts. Quality of non-local courts	Investment rate	+	+			Higher proportions of business disputes settled through the courts system is correlated with a higher investment rate	med/low	2
Pang and Wu (2009)[c]	Interaction of Contract intensity with an index of the quality of contract enforcement and courts, and two additional indicators of legal quality	Industrial investment elasticity to value added	+	+	+	NS	Countries with better contract enforcement tend to have more efficient capital allocation in industries which are more contract-intensive	med	3
Poirson (1998)	Rule of law indicator	Nominal private fixed investment in percentage of nominal GDP	NS				Once civil liberties, the quality of the bureaucracy, and the risk of expropriation are controlled for in the regression, rule of law became NOT significant	med/low	1

(*continued on next page*)

Table 2 (*continued*)

Authors	Contract enforcement (CE) indicator(s)	Investment indicator(s)	CE variables' sign (signs shown only if significant)[a]			Other results (for at least one of the CE variables in other regressions)	Main conclusion (according to authors when available)[c]	Quality assessment	
			Var. 1	Var. 2	Var. 3			WoE	CI
Prados de la Escosura and Sanz-Villarroya (2009)	Contract-intensive money	Investment ratio to	+				Poor compliance with contracts hindered investment in broad capital	low	1
Raja and Schaefer (2007)	Protection of property rights	Ratio of total inventories to net sales	+				The level of inventory holdings tends to rise with the probability of breach or lack of enforcement	med/low	2

Notes: NS = Not significant.
Source of the coefficients (in the original papers): (1): Table 4, Panel B, cols 1, 3, 4; (2): Table 2, col. 2; (4): Table 4; (5): Table 7, col. 8; (6): Table 7, col. 4; (7): Table 3, cols 1 to 3; (8): Table 3, col. 1; (9): Table 3, col. 1; (10): Table 3, col. 1; (12): Table I, col. 1 and Table II, col. 5; (13): Table 1; (15): Table 3, col. 7; (16): Table 3, col. 1 and Table 4, col. 2; (17) Table 6, col. 4; (18) Table 4 col. 1; (19): Table 2; (20): In text; (21): Table 2, col. 2; (22): Table 1.
[a] Sign of coefficients associated with the different contract enforcement (CE) indicators used in regressions (col. 2 of the table, in the same order of appearance). Authors' preferred specification (reviewers' interpretation).
[b] The authors also use three additional CE indicators, all of which have positive coefficients.
[c] They also carried out regressions with three additional variables, all of which have negative coefficients.

that depend on expert opinions (legal formalism), and Lin *et al.* (2010) make use of business survey data. None of them discusses in much detail the adequacy of the chosen contract enforcement indicator in the specific development and institutional context of the studied regions.

Regarding the main results, Chemin (2006) examined India's experience around the 2002 enactment of the Code of Civil Procedure Amendment Act, to assess the net impact of the Act taking into account the fact that some states had already enacted in previous years some of the amendments adopted nationally in 2002. This is the only study to examine effects of an actual reform episode; firm-level data, combined with sub national variation in legislation, and a nationwide reform allow the author to draw strong conclusions (based on a difference-in-differences approach) that we found persuasive.

The study shows that the legal reform (the degree to which it changed legal processes at state and national levels) impacted positively on small firms' decisions to invest in a variety of fixed assets. The evidence shown in the paper supports the view that the decision to invest is favored by fewer breaches of contracts, greater access to credit, and thicker rental markets, that result from a speedier judiciary. These results would count as a "positive" for the effect of contract enforcement on investment, besides the additional light on causal chains.

Acemoglu and Johnson (2005) used a cross section of countries that were once European colonies, to unpack the concept of growth and investment-friendly institutions, distinguishing between property rights and contracting institutions. They find evidence that variation in contracting institutions has an impact on the structure of financing, but does not seem to have a direct effect on investment rates. Their instrumental variables approach addresses endogeneity issues: they proxy some of the contemporary institutional variables by correlated but unequivocally preceding variables (e.g., English legal origin, settler mortality, population density circa 1,500), controlling for the possible reverse-causality bias. Their samples are sometimes very small (for example, in looking at the stability of findings for sub-samples). Their results would count as a "negative" for the review question.

Pang and Wu (2009) looked at the effect on the allocation of capital to more or less efficient industries. The article sheds light on the direct effect of contract enforcement on the rate of capital accumulation by industries. As the previous study discussed, it is generous in its testing for alternative specifications, discussing indicators and endogeneity, and using instrumental variables techniques to sidestep that trap. Like Acemoglu and Johnson (2005) it also takes advantage of the Djankov *et al.*'s data on procedural complexity, and has a large sample of industries-countries-years.

The study argues persuasively that better enforcement of contracts is associated positively with more efficient allocation of capital, understood as investment going to industries with greater growth potential (measured by the elasticity of investment to value added). The authors also show that the observed effect is more pronounced in industries which are more contract-intensive. This study also counts as a "positive" since the outcome (the investment distribution by industry) implies changes in investment levels by industry linked to their exposure to contract uncertainty. As a by-product, investment's responsiveness to growth would reinforce the well-established "accelerator" effect (that is, output growth's positive effect on investment).

Clague *et al.* (1999) are much more direct. Their approach is to create the above-mentioned indirect proxy for effective-

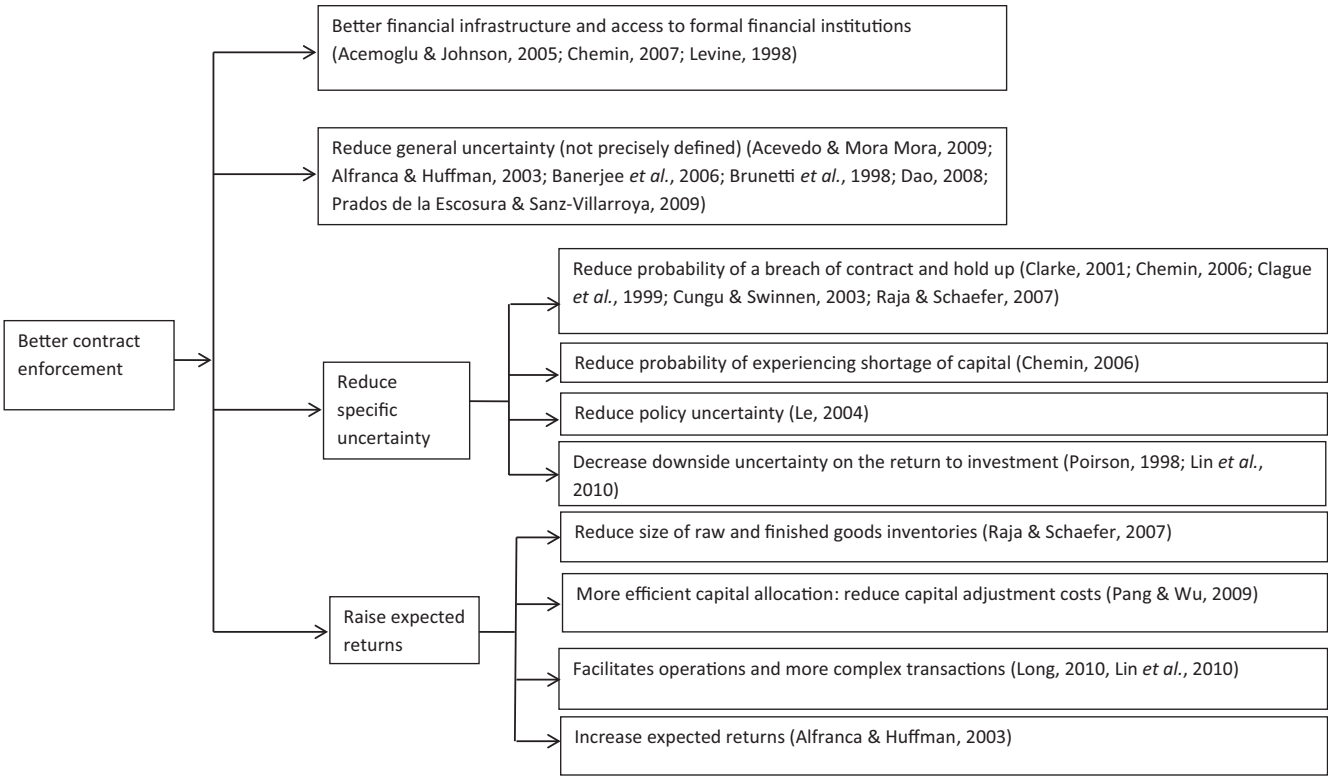

Figure 3. *Mechanisms through which contract enforcement would affect investment, according to the included primary studies.*

ness of contract enforcement. If the proxy is considered valid, then it is possible to look at the relations of interest from a long-term, dynamic perspective (see, e.g., Prados de la Escosura & Sanz-Villarroya, 2009). However, they use time variations only for a few specific countries, and just to justify the choice of indicator by inspecting its behavior during salient macroeconomic events. When it comes to the statistical analysis, they work with averages to run cross-section regressions. Their discussion of the indicator's correlation with other measures, of whether the indicator captures what it is asked to, and the inclusion of some sensitivity analyses, adds to the credibility of the story. Regarding the review question, they claim to find statistically significant evidence of a positive effect of contract enforceability on investment ratios, so their results count as a "positive." They are among the few that run separate estimations for developed and developing countries, suggesting that they may recognize contexts as conditioning the notion and nature of enforcement.

Levine (1998) is mostly concerned with the effect of institutions on financial development and growth. A carefully crafted study, it digs deeper into the financial development-growth link than into the contract enforcement-investment one. Regarding the former, the study can be read as demonstrating that financial development matters for growth of the capital stock, and the latter is partly explained by enforcement of contracts (proxied by a composite index of rule of law and risk of confiscation). The study uses instrumental variables and a cross-section sample. His results would count as "positive" for an indirect effect of contract enforcement on investment through financial development.

Finally, the last top-rated study (Lin *et al.*, 2010) examines the effects of contract enforcement on corporate R&D in a sample of 2,400 firms of 18 Chinese cities. Given the potential endogeneity of the quality of enforcement variables (measure of managers' faith that their rights will be protected by the contract and upheld by the legal system) the authors use the industry-location average of these variables as instruments. The evidence presented in the study shows that contract enforcement plays an important (positive) explanatory role both in the decision to invest in R&D and in the amount invested in R&D by firms; therefore this study counts as a "positive."

In brief, the six best-rated studies suggest that (i) contract enforcement affects investment directly, (ii) it probably matters also indirectly through development of financial markets and facilitating the access to resources of more investment-efficient industries, and (iii) it may augment the accelerator effect (i.e., growth's entrance among the factors explaining investment). More specifically, at least in one major developing country (India), there seems to be evidence that relevant enforcement deficits have to do with the length of processes, which might create incentives to breach contracts and to prevent available funds from entering the circuit that leads to investment. That said, the heterogeneity of methods and findings, the ambiguities intrinsic in cross-country evidence, and the limited feasible treatment of endogeneity, would prevent an impartial reader from drawing strong conclusions, even from the best primary studies in the review.

When we add into the picture the four studies rated "2" in the CI scale (the causal inference) and "med/low" in WoE (the overall strength of the study) the between-studies heterogeneity is greater than before. A "vote count" shows that three of the additional four studies find that better contract enforcement is positively associated with higher levels of investment, with one of them focused only on R&D investments. The remaining study finds a positive indirect effect of enforcement on investment, through less breaches of contract. The contract enforcement and breach of contract indicators in this study are

all based on firm managers' perceptions recorded through surveys.

In general, these studies cannot go beyond detecting correlations between the enforcement proxy and the investment indicator. In fact, in some cases there is the very credible possibility of reverse causality.[18] Additionally, most of these studies do not perform any robustness checks (Long, 2010, is an exception). Like the top rated, they barely discuss the validity of the contract enforcement construct.

(c) *Synthesis of context-mechanisms-outcomes*

Leaving aside their weaknesses as attempts to substantiate causal claims, the studies in the second- and third-quality categories—like those in the first—can still be informative about the mechanisms thought to bring about the enforcement-investment empirical relationship. The map of "mechanisms" resulting from the whole set of studies does not exhaust the pool of hypotheses about how the effect is caused, since we excluded from the selection those research outputs that were only theoretical and those that were on related topics but did not contain an explicit test of the enforcement-investment link.

Among the included studies, almost all implicitly or explicitly adhere to a basic story stating that effective third-party enforcement enables more complex contracting, and that contract uncertainty will tend to depress investment by affecting expected returns, increasing investment costs, restricting access to key resources, or making some complex transactions plainly unfeasible. Unfortunately, several studies are not explicit about the causal pathways presumed to exist behind their specific empirical work. At least five studies (Acevedo & Mora Mora, 2009; Banerjee, Oetzel, & Ranganathan, 2006; Brunetti *et al.*, 1998; Commander & Tinn, 2008; Dao, 2008) jump quickly from cursory remarks about a link between institutional uncertainty and investment to the analysis of the data (more on this below).

Among the rest, there are those studies that elaborate slightly on the assumed mechanisms. In various cases the elaboration is meant to justify the focus on some specific component of overall capital accumulation (e.g., investments in R&D in Alfranca & Huffman, 2003; Clarke, 2001; Lin *et al.*, 2010; or accumulation of inventories in Raja & Schaefer, 2007). In other cases, conceptual elaboration seems to justify a specific form of causal pathway for which the authors want to provide empirical support. In Acemoglu and Johnson (2005), (better) contracting institutions are shown to have an effect on (a more developed) financial structure, which implies (greater) availability of funds for investments. In Cungu and Swinnen (2003), better contract enforcement seems to reduce delayed payments, which in turn have negative effect on investment. Lastly, the results in Alfranca and Huffman (2003) suggest that a worse contracting environment does not always inhibit R&D investments, but it does when it coincides with a relatively inefficient bureaucracy; which makes it the only study to propose a *conditional* cause–effect association. In general, all the studies so far lack an explicit analytical model.

Some of the CMO schemes that seem free from ad-hoc theorizing include, first, those building on the analytics of the "hold-up problem" (originating in work by Oliver Williamson, and Benjamin Klein, Robert Crawford and Almen Alchian, in the 1970s and 1980s, as discussed in Cungu & Swinnen, 2003). In that framework weak enforcement encourages hold-up strategies (such as delayed payments to suppliers or delayed delivery to buyers), that affect investments through their impact on cash flows and indirectly through greater downward uncertainty of returns. The mechanism at work results from the existence of commitments, on which parties to a contract rely for their business operations, and the power bestowed on another party to renege. Contract breaches would be facilitated by greater cost of capital (that makes it costlier to shift to other business partnerships in the short and medium term) or lower cost of non-compliance (due to enforcement imperfections). The study that highlights hold-up-related mechanisms comes from a transition economy, and the authors base the relevance of the problem on that context, but there is no claim that it might not be relevant in other societies.[19]

Another non-trivial mechanism operates through better enforcement reducing capital adjustment costs, facilitating the process by which funding reaches more efficient industries. Citing a similar origin in the institutional economics of Williamson and others (acknowledging more recently Grossman, Hart, Moore, Acemoglu, Antras, and Helpman), the study by Pang and Wu (2009) explores a channel from incomplete contracts to capital misallocation that operates through imperfect contract enforcement rather than through financial system efficiency. It is argued that the sectoral distribution of investment will reflect a bias against more efficient industries when contract enforcement is weak(er), particularly if efficient industries are highly "contract dependent."

Figure 3 identifies other mechanisms discussed in the synthesized literature, including better known ones such as those involving costs and speed of dispute settlement, access to funding (operating through formal financial institutions and other sources of funds), and profitability or corresponding risks.

From confronting the enumeration of presumed causal mechanisms (Figure 3) with the map of related literatures sketched above (Figure 1) it appears that empirical work on the contract enforcement–investment link has been mostly informed by basic analytics of capital accumulation and is disconnected or weakly linked to other areas of the "institutions-performance" field. Only those studies that refer to the effects of enforcement on investment through financial depth (top box in Figure 3), which reflect the influence of the Law and Finance literature; or those highlighting the hold-up problem, which are applications of the economic theory of incomplete contracts, seem rooted in theories beyond basic economics.

Perhaps the limited attention to theory in the reviewed literature helps explain the dearth of conceptual discussion on the applicability of frameworks, definitions, and measures to heterogeneous samples of countries and times. In fact, the evidence analyzed largely comes from developed and developing countries over the last third of the 20th century, though a few examine 21st century cases and data. Many studies work with heterogeneous samples of countries (presumably to buy power for the statistical tests), and generally tend to offer limited discussion of uneven levels of development and institutional sophistication. The distribution of studies by sample coverage (8 focused on sets of developing countries, 7 mixing developing and developed, 3 studying transition economies, and one on European countries) would not be an obstacle *per se* for exploring context-contingent theories. The limited attention to varieties of contexts and validity of indicators across classes of countries would rather seem a manifestation of weak linkages of empirics with theory in the sub-field.

As part of the context of almost all the studies (by design as well as by data availability), countries are portrayed as experiencing more or less gradual changes in their institutional landscape, and these in turn are expected to have some effect over aggregate economic outcomes (with various confounding factors that are partially controlled for). As discussed below,

this contains an implicit view of law and institutions that some authors in the field see as problematic.

(d) *General remarks*

Before moving to the conclusions and implications, there are a few additional observations worth making, on the review's results and the relationships between theories, explanation, and empirics.

First, the work reviewed tends to take a bird's eye perspective on the analyzed phenomena, often missing detailed characterizations of specific institutions, their composite architecture, their mechanics in interaction with their contexts, and their outcomes. The bird's eye perspective was predictable in studies of countries as units of analysis, but it is also present in the literature based on firms' or firm managers' reports, and could be one reason why several heterogeneous constructs for contract enforcement are allowed to co-exist.[20] The scarce conceptualization complicates theory testing and undermines theory selection, eroding the usefulness of empirical findings for policy design.

Several simplifications, and claimed political neutrality, are the bases of what is known as the "endowment perspective (on the law and development)," which has a major influence in the institutional economics literature despite demonstrations of its faults (Milhaupt & Pistor, 2008).[21] The endowment view would see norms as building blocks, legal regimes as stocks of rules that can be easily reorganized, and both as essentially exogenous to the economic process. Such view has been related to multilateral reform agendas for developing and transition countries, and it does not seem inappropriate to assert that it provides the implicit conceptual background for most of the papers in our review.

Legal (sometimes called *institutional*) "reform" has specific meaning in that perspective: assuming a dominant influence of written norms and formal contracts over behaviors, reforms would consist of adding, removing, or modifying statutory provisions, or designing better contracts. This could account for some naiveté regarding measurement of quality of enforcement (which would just result from more stringent norms and/or more intense policing) and the unfounded preference for cross-country studies.

Based on the findings so far, one might be tempted to conclude that institutional change that fosters reliability of contracts (in some sense) will be rewarded with greater additions to a society's stock of capital. Fifteen of the 19 synthesized studies find a positive association between the construct for institutional variation and the capital accumulation variables, and the proportion is about the same when only the strongest studies are considered. Such general conclusion without qualifications is probably what policy makers (some being sponsors, in one way or another, of a good part of this literature) take away from casual observation of research from the last couple of decades.

The conclusion might have been reinforced by researchers' tendency to conflate contracting institutions with institutions more specifically related to the protection of property rights, or to place them all under the larger umbrella of the "rule of law." Two of the 19 studies in the SR (Acevedo & Mora Mora, 2009; Raja & Schaefer, 2007) actually use as proxies of "contract enforcement" some indicators that are explicitly about the *protection of property rights*; three more studies use ratings of countries with respect to *the rule of law* (Banerjee *et al.*, 2006; Clarke, 2001; Poirson, 1998), and one study (Levine, 1998) uses a combined index of *both*. These studies' approach may be quite misleading, particularly when proxies start to be confused with the thing they are supposed to represent.

The suspicion of publication bias in this literature is justifiable and nurtured by the observation that those few studies that do not find a "positive" are presented in a way that makes the null the expected result (e.g., Acemoglu & Johnson, 2005), or have a broader focus beyond effects of contract enforcement (Banerjee *et al.*, 2006; Poirson, 1998). Moreover, in these cases the ambiguous or negative result are largely inferred by us, the reviewers, and not recognized by the authors. With this in mind, the above-mentioned results of "vote counting" (quality-weighted or not) have to be taken with a grain of salt.

4. CONCLUSIONS AND IMPLICATIONS

The systematic review reveals that the evidence on the impact of improvements in contract enforcement on investment is spotty, comes from a rather disjoint body of literature and generally does not meet accepted standards for causal inference; only in roughly one third of the studies does it meet weaker though defensible standards for non-experimental data. The literature is thin, there are important ambiguities associated with the most widely used indicators of institutional change, there is a paucity of studies designed to address a "cause-and-effect" question, and there are symptoms of publication bias. In many studies, the analytical approach is generally sound but the available indicator of quality of enforcement or breach of contracts has questionable validity. A significant number of studies are also of low quality due to their publication of a few "statistically significant" results without robustness analyses. For policy makers and donor agencies, it seems that too much confidence has been put on a plausible but unproven causal hypothesis.

Some of the hypothesized mechanisms through which enforcement of contracts might affect investment seem consistent with available evidence (for example, the causal channel through breaches of contract and their effect on the value of investment projects). However, much remains to be done for research to be able to justify specific policy interventions. It is unfortunate that only a few studies rely on indicators of variation in enforcement that minimize the influence on measurement of subjective judgments, since these could be biased in ways that obscure the studies' policy implications.

The empirical literature reviewed largely embraces the "endowment perspective" in law and development, and is strikingly disconnected from more elaborate conceptions of contracts and enforcement (see, e.g., Berkowitz *et al.*, 2003; Spamann, 2009; recall Macaulay, 1963). Such disconnect suggests that there are enduring barriers between disciplines—particularly between law, sociology, and economics—that may be hindering the literature's growth. Inter-disciplinary dialog could result in improved conceptual frameworks, definitions, and measures of relevant variables, and this could motivate original empirical work.

Improving contract enforcement could be a valuable objective in itself, to the extent that it is associated with fairness and the equality of citizens before the law. Quite another thing is to advocate institutional reforms that will demand investment of political capital and other resources, to improve the expediency and predictability of judicial rulings in the name of broad economic benefits that are not yet proven to exist. Moreover, even if the institutional constructs and inference strategies were sound, there may be other conceptual failings in the literature to be considered before pushing a reform agenda for all or specific groups of countries. In fact, it may occur that the

law and its effects on behaviors cannot, for example, be conveniently manipulated by altering specific legal statutes, or without considering the process of their adoption, or ignoring the political context.

As pointed out by Berkowitz *et al.* (2003, p. 164) in the last three decades

> "many countries borrowed from different legal systems, not infrequently in an attempt to signal to foreign investors from different countries that they comply with their domestic legal standards. ... Yet, the results of these efforts have been mixed".

The entanglement of norm and enforcement, the neglect of transplantation problems, and insufficient attention to politics-law interactions may explain a good part of the outcome, which has led the Independent Panel (2013, p. 1–2) reviewing the *Doing Business* report to warn that it

> "should not be viewed as providing a one-size-fits-all template for development. Empirical evidence on the results of business-regulation reforms captured by the report is mixed and suggestive at best. ... The evidence in favor of specific country reforms is contingent on many auxiliary factors not captured by Doing Business report topics".

If evidence is to guide policy, more research needs to be supported on the causal links, and more of it should follow a *falsificationist* program; that is, should expose the extant hypotheses to stringent empirical tests, seeking to weed-out those more frequently refuted and expanding knowledge by building on the more resilient ones (Popper, 2004; Hands, 2001).[22] Yet, the epistemological considerations should not stop there. Two observed patterns raise important issues in that regard: the "estimation without (or with implicit) theory," and the "proliferating mechanisms" process. The first refers to the observation that several studies omit an explicit analytical framework and, after superficial remarks stating expected results, jump to the empirical tests of a set of hypotheses. This could be cataloged as naïf empiricism. Indeed, the statistical testing without a theoretical framework implicitly and mistakenly assumes that data will speak for itself, and deprives the analyst of *explanations* that are essential to deriving policy advice.

The second pattern—mechanism proliferation—is logically consistent with what could be called "inconsistent instrumentalism" or "unacknowledged realism" in economics (see Fleetwood, 2001). Such position results from the tensions between an intended empiricist/instrumentalist rhetoric and a practice that implicitly recognizes the existence of unobservables and the "capacities" of inanimate things, relying on "mechanisms" as the way of explaining (Lawson, 2001; Hands, 2001). From the latter's angle, developing interpretive mechanisms that accommodate empirical regularities is the way to explaining, and therefore mechanism-development results in a proliferation of observationally equivalent explanations. One could note that our own approach (that is, that of SRs and "realist synthesis") reflects a similar epistemology (the attention to CMOs is typical "realist synthesis" and in line with realism in economics). This shows how observations from SRs can provide the motivation and starting point for greater methodological self-consciousness, with interesting prospects for applied research.

Looking into the future, the research agenda will require a rebalancing. While the literature reviewed seems to support the conventional assumption, it is not yet clear if observed regularities are robust across various samples (for time spans, country categories, data structures, etc.). In particular, testing for the stability of econometric estimates across meaningful groups of countries, such as cultural or legal communities, would go beyond testing for robustness and could shed light on the contingent relevance of the institutions-performance hypotheses. As we saw, primary studies manipulating cross-country samples were largely shy about that possibility, despite its economic and social significance.

While there may be ways of improving the overall robustness of cross-country panel analyses (and the development of new indicators of countries' institutional attributes will continue stimulating that "industry"), panels of *sub national* jurisdictions and/or firms would seem to carry more promise in terms of uncovering causal mechanisms, as they make it easier to control for key unobserved heterogeneities. They will comprise those studies that can exploit a natural experiment, and those taking advantage of spatial or temporal variation in agents' exposure to institutional arrangements (Pande & Udry, 2006, chap. 14). The use of theory-based approaches, measuring the impact on intermediate outcomes along the causal chain would also shed light on the channels of transmission. A focus on actual reform episodes and creative research designs will take the literature several steps farther. In general, robust evidence will come from studies based on an explicit theory and with credible identification strategies (good examples are Lilienfeld-Toal, Mookherjee, & Visaria, 2012; Visaria, 2009).

More research is needed on the response to institutional innovations of firms, bureaucrats, and other "micro" agents (e.g., banks) that might be involved in the link through some direct or indirect channel. Research will add light by exploring untapped datasets (e.g., records from, and statistics on judicial systems or their components, etc.), and by reporting more of the actual action in the offices (e.g., results of robustness tests and/or other omitted analyses that might reveal something about robustness). For studies based on macro data and instrumental variables, credibility could be strengthened by heeding the advice of Bazzi and Clemens (2013) about basing decisions on theory and serious scrutiny of construct validity. Research could also use qualitative methods and draw information from suitably designed consultation with business people and government officials. Being close to the institutional "action" may allow researchers to seize scarce opportunities to advance knowledge through suitably *designed* programs.

NOTES

1. Some regulatory norms are policies rather than institutions, though still "rules of the game;" we acknowledge this limitation of the diagram but leave aside finer distinctions in the interest of space.

2. Our research question was "What is the evidence of the impact on investment rates of reforms to improve the enforcement of contracts?"

3. On the history and factors driving the development of research synthesis as a field, see Chalmers, Hedges, and Cooper (2002).

4. The typical literature review in the Journal of Economic Literature or the Journal of Economic Surveys, for example, fail to describe how the included references were identified and chosen (i.e., they are normally not

replicable and it is hard for readers to tell if the conclusions are a balanced representation of the whole field or could be biased in some known or unknown way).

5. This journal recently published a SR on another relevant impact question (Van Rooyen, Stewart, & De Wet, 2012, on impacts of microfinance).

6. Other key determinants of investment include access to funds and the cost of borrowing, taxes, and public expenditures (public and private investment have been found to be complementary or substitutes, depending on various circumstances), and the size of the market as determined, for example, by trade policies.

7. http://www.dbrpanel.org/ Accessed January 29, 2014.

8. http://www.usaid.gov/news-information/fact-sheets/contract-law-enforcement-cle-program Accessed: January 29, 2014.

9. Our SR provides a telling example of the problems of focusing only on published work: the best quality-rated study (Chemin, 2006) was only accessible to us in working paper format. While an exclusionary reviewer might have left it outside the relevant sample of studies, covering the gray literature allowed us to catch what would be published some years later (Chemin, 2012).

10. SRs are notoriously labor-intensive. A study that explores two or three levels in an ordinarily complex causal framework, for relatively focused review questions, may easily involve half a dozen authors or more (see e.g., Tripney *et al.*, 2009).

11. Alfranca and Huffman (2001, 2003) are the first set of related studies and Gow and Swinnen (2002), Cungu and Swinnen (2003), and Cungu, Gow, Swinnen, and Vranken (2008) are the second. We "represent" the first pair with the most recent (Alfranca & Huffman, 2003), and the second group with what we considered to be the best quality report (Cungu & Swinnen, 2003).

12. Further details are explained in (Aboal et al., 2012).

13. Given the number of primary *studies*, the use of statistical methods to synthesize quantitative findings would require using more than one *estimate* per study so as to increase the sample size, a procedure that is not peacefully accepted in the synthesis literature (Becker, Hedges, & Pigott, 2004). Also, good portions of the relevant professional community advocate the assessment of the studies' "quality of execution" before deciding to include them in a statistical synthesis (Gough, 2007), a recommendation that would aggravate the sample size issues in our case.

14. Four out of six studies in the top-quality class (five in six, if Chemin, 2012, post-cut-off date publication is counted) were available in journals between ranks 52 and 5, and all four in the next quality class were available in journals ranked 44 to 401 in RePEc's 10-year impact ranking, at the same time that only four in nine studies in the third and last class had been published in professional journals ranked 32 to 434. In brief, publication is more frequent and outlets' impacts greater for better quality studies. Neither publication source nor its rank was considered relevant when we rated study quality. (RePEc's journal ranking is available at http://ideas.repec.org/top/top.journals.recurse.html; consulted on January 19, 2014.)

15. The numbers do not add up to 19 because more than one focal theme was recorded for some studies.

16. We refer to the "ecological fallacy" as the logical flaw of generalizing about individuals what has actually been observed at some higher level of aggregation (e.g., geographic units).

17. The initial focus of the synthesis is on six studies that attained at least a "med/low" rating in the summary dimension of the WoE tool, and a rating of 3 in the CI scale. That is followed by observation of change and similarity of the conclusion when studies that also have a "med/low" rating in WoE but a rating of 2 in the CI scale are added.

18. Brunetti, Kisunko, and Weder (1998) run a regression of the average investment rate in the period 1970–92 (alternatively 1980–92) on a measure of contract enforcement in the year 1996.

19. In a study that was eventually removed from the final set due to exclusion criteria on the outcome variable, the transition or hybrid economy context drives an opposing hypothesis consistent with some evidence: Feng Lu and Yao (2009) examine the possibility that enhanced legal enforcement in a transition economy with financial repression may limit capital available to the private sector (which comes from "leakage" from the state-owned enterprises), and thus depress private investment.

20. For example, Acemoglu and Johnson (2005) and, say, Brunetti *et al.* (1998), share the same "bird's eye" approach, even when the former use the Djankov *et al.* (2003) indices of legal formalism constructed from lawyers' assessments of how the law in a country deals with some business cases and the latter is based on firm managers' responses on dimensions of credibility and predictability of the global legal and law-making systems.

21. The "*endowment perspective on the law and development*" must not be confused with the theories of development that recognize a key role to *natural resource endowments*. Since major strands within the latter recognize institutions as the variable(s) mediating the effects of resource endowments on economic performance, confusion is not only possible but likely.

22. Hands (2001), especially Section 7.1, provides thorough discussion and extensive references to the literature on strengths/weaknesses of varieties of falsificationism, as well as their successive transformation and alternatives.

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