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**“Public and Private Enforcement of Securities Laws: Resource-Based Evidence”**

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\*presenting

# Public and Private Enforcement of Securities Laws: Resource-Based Evidence

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## Abstract

*The legal consequence of economic actors ignoring their legal obligations, such as laws that protect outside investors in firms, is a recurring issue. Recent scholarship examines the relative importance of private enforcement for investor protection on the one hand — via disclosure and lawsuits among contracting parties — and public enforcement on the other — via financial, regulatory, and even criminal rules and penalties. Recent academic work in finance has seen the former to be more important than the latter. Yet much legal scholarship has long seen private enforcement of securities laws in the United States as poorly designed, with firms — and hence wronged shareholders — often bearing the cost of insiders' errors and disclosure failure. To better understand the relative importance of public and private enforcement, we here develop an enforcement variable based on securities regulators' real resources — their staffing levels and budgets. We then examine financial outcomes around the world — such as stock market capitalization, trading volume, the number of domestic firms, and the number of IPOs — in light of these resource-based measures of public enforcement and find that more intense public enforcement regularly correlates with strong financial outcomes. In horse races between our measures of public enforcement and the measures of private enforcement prominent in recent financial scholarship, public enforcement is typically at least as important as private enforcement in explaining important financial market outcomes around the world.*

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# **Public and Private Enforcement of Securities Laws: Resource-Based Evidence**

## **Introduction**

We evaluate here the value of public enforcement of securities law on the development of stock markets around the world. We do so because understanding the relative value of public and private enforcement has major implications both for the academic understanding of what propels strong financial markets and for the content of current development programs. The World Bank, for example, in seeking to promote strong financial markets and economic growth has recently dismissed public enforcement of securities laws as being unimportant, while seeing private enforcement as central, conclusions we consider to have been reached too hastily. World Bank (2006: 1). And in the financial academic literature, Djankov et al. (2005) and La Porta et al. (2006) highlight the importance of evaluating the relative importance of private enforcement versus public enforcement of securities obligations that protect investors.

In principle, both enforcement mechanisms could have serious defects and strong advantages. On the defect side: A public enforcement system is degraded both because public actors have mixed and often weak incentives to do their jobs well and because they often suffer from poor information of both general market and specific firm conditions. But a private enforcement system is subject to free rider effects among wronged investors, to slow and inaccurate judiciaries, to lawyers' rent-seeking, and to the resulting potential inability of private enforcement to visit severe monetary penalties on wrongdoers. On the positive side, public enforcement has the potential to be run by public-regarding policymakers and can invoke sharp criminal, financial, and reputational penalties that deter egregious wrongdoing, while private enforcement actions can be brought by actors who are best informed because they are close to the transactions at hand.

In short, there is no obvious a priori winner. Hence, we seek to evaluate whether public enforcement is in practice valuable by using a new data-set of the resources devoted to securities market oversight around the world — measured in terms of both staffing levels of securities regulators and their budgets. Prior studies relied on indices based on the formal powers of regulatory officials as measuring the strength of public enforcement, a measure that may well be less appropriate than money and people.

In Part I, we discuss legal academic analyses on why private lawsuits — a key component of private enforcement — often do not visit penalties on the relevant actors, can distort incentives, and can be inefficacious. We do so to suggest how and why public enforcement could, at least in theory, have benefits that the real world's prevailing modes of private enforcement lack, since on-the-ground private enforcement is often misdirected. Although public authorities lack ideal incentives and may be more poorly informed than private actors, the common structures of private lawsuits *also* yield poor incentives and weak information for the relevant private actors. If private mechanisms are strongly misdirected, then even suboptimal public enforcement can contribute to good financial outcomes. Moreover, we explain how strong public enforcement enhances the efficacy of private enforcement. We then introduce our own measures of public enforcement based on regulatory budgets and staffing levels of securities market regulatory and enforcement agencies.

In Parts II and III we describe the data and present our findings. First, we explore the relationship between our new measures of public enforcement and the size of a country's capital markets. Generally speaking, allocating more resources is positively associated with measures of robust capital markets, including stock market capitalization, volume of trading, level of IPOs, and number of domestic firms. We then control for measures of private law enforcement. Our resource-based public enforcement variables' relationship to stock market development is robust to adding controls for private-law oriented legal factors, such as disclosure requirements and liability rules, which prior financial work has seen to be critical to developing and maintaining strong securities markets. In several instances, public enforcement dominates private measures, such as liability standards, results consistent with current legal thinking that

liability standards, particularly in the United States, are poorly designed. Next, in horse races between our measures of public enforcement and formal measures of public enforcement powers commonly used in prior academic work on the subject, our new measures of public enforcement are more strongly associated with robust financial markets. Moreover, the two prominently-used formal measures of public enforcement do not correlate with one another, further suggesting that indexing formal regulatory powers may not be the best research methodology to measure real public enforcement intensity.

The data here do not, however, lead us to conclude that public enforcement uniformly trumps private enforcement, but that both play a role in promoting strong capital markets. In some areas, public enforcement may be less effective than private enforcement, in others the opposite may be true. As an example of the former, our public enforcement variables do not predict country-by-country results of variables associated with highly dispersed ownership of public firms. As an example of the latter, our measures of public enforcement are highly correlated with stock market capitalization.

We also discuss the direction of causality. It's possible that more public enforcement produces better outcomes; it's also logically possible that stronger financial outcomes call forth more public enforcement, with higher budgets and deeper staffing levels. In particular, widespread ownership of financial assets in a democracy should make it easier for government officials to spend resources on public enforcement. If influential elements of the polity own financial assets, they will insist on being protected from insiders' machinations. Unpacking causation is complicated, however, and we see the two aspects — market results and budgets — as likely to be simultaneously determined, with causation bidirectional. But we want to ascertain, to the extent one can, that causation does not run solely from markets to budgets. While the finance literature has not yet settled on a suitable method here for this kind of data — the accepted technique is to use instrumental variables, but finding a suitable one has vexed academic finance for a decade — and while our findings are only tentative, we view our findings on directionality as no weaker than those previously used in this area of finance work. (Other techniques to

unpack causation — such as differences-in-differences or country fixed-effects regressions — are not yet viable, due to data limits.)

In Part IV, we discuss the channels through which public enforcement could improve financial markets and offer cautionary notes for policymakers. Public enforcement strength can indicate the degree to which the public authority can (1) conduct market surveillance, by addressing general systemic issues and problems that need rule-making fixes, (2) investigate individual firms for wrongdoing, (3) bring enforcement actions (including those that result in large financial penalties or criminal imprisonment), and (4) write, revise, and enforce better, more sophisticated regulatory rules. We cannot distinguish which among these four channels for public enforcement is the most important.

Moreover, there are multiple targets for public enforcement. Much of the literature has focused on corporate-governance-related enforcement, especially that connected with tunneling value out from the firm into controllers' hands. Tunneling and related party transactions are important and deserve the attention they have received. But deep public markets also require brokers and others securities-handling institutions that are often intensively regulated in the United States and in other countries with deep capital markets. Trading channels have not been the focus of analysis in the law and finance writing of the past decade, yet reliable trading channels might be critically important to building a strong securities market. Perhaps this is an important channel through which public enforcement and deep securities markets interact. Our results indicate that this channel deserves further examination.

Djankov et al. (2005) and La Porta et al. (2006) are the academic articles closest to our work here, although they conclude that public enforcement is of limited value and we do not. On the normative side, our overall conclusion is cautionary: regression results linking private enforcement variables and corporate law quality variables on the one hand with favorable financial outcomes on the other hand could lead policymakers to promote private enforcement rules and institutions to the detriment of public enforcement institutions. The World Bank, for example, has announced that “[i]n banking and securities markets, characteristics related to private monitoring and enforcement drive development more than

public enforcement measures.” World Bank (2006: 1). Researchers now associated with the IMF and the European Central Bank reach a similar conclusion. Bruno and Claessens (2007); Hartmann et al. (2007). Our findings suggest that the data do not warrant so concluding. Policymakers who short-change public enforcement may end up fraying the financial markets they hope to develop.

## **I. Public and Private Enforcement in Legal Analyses**

What makes corporate actors play by the rules?

One possibility that surely is important is that wronged investors can sue wrongdoing insiders and recover damages. This remedy can be of obvious importance as the best information on wrongdoing is often held by those wronged, who also typically have strong incentives to sue the wrongdoer. Another possibility is that public enforcement controls wrongdoing private actors in firms, via good rules, via fines, by enlisting gate-keepers (like accountants) who examine financial transactions, and even via criminal penalties. In principle, each mode — private and public — has strengths and weaknesses.

### **A. Private Remedies**

Consider first the weaknesses of private lawsuits for securities and corporate wrongdoing, weaknesses extensively explored in the legal literature but less deeply explored in the finance literature. The conventional legal academic view is that the efficacy of many, perhaps most, American private lawsuits is seriously compromised. Private securities lawsuits in the United States (1) often provide meager returns to wronged plaintiffs, (2) usually do not visit their costs on the wrongdoing actors inside public firms, because the wrongdoers can usually transfer the costs to others, and (3) often just transfer losses from one innocent group of shareholders to another innocent group, with large fees obtained by the lawyers for both sides. Coffee (2006); Romano (2005); Cox et al. (2003). As Coffee (2006: 1), an authority on securities litigation, concludes: although “the damages recovered through private enforcement” are sometimes high, “the evidence is clear that corporate officers and insiders rarely contribute to securities class action settlements, with the settlement funds coming instead from the corporation and its insurers. As a result, the cost of such actions in the aggregate falls on largely

diversified shareholders. Such a system is akin to punishing the victims of burglary for their negligence in suffering a burglary and does little to deter corporate officials... . The present structure of securities class actions benefits a trio of interest groups — corporate officials, plaintiff’s attorneys, and insurers — but not shareholders.” In Britain, private securities lawsuits are few. Armour, Black, Cheffins, and Nolan (2007). Such results are obviously not conducive to effective deterrence and strong, effective private enforcement. Public regulators are aware of this problem and its seriousness. Grundfest (1994). (Grundfest is a former SEC commissioner.) Presumably, they take these defects into account when designing public enforcement mechanisms.

Moreover, while the total amount of damage done by the insider may be enormous, the portion affecting any single 100-share shareholder in a firm with diffuse ownership is small, of a size insufficient to motivate the shareholder to sue the wrongdoers. That is, in dispersed stock markets diffuse shareholders have limited incentives and poor information for monitoring insiders. Those structural limits give insiders more freedom to maneuver, in ways that we might hope private litigation would correct. But those very limits on small shareholders’ market-based monitoring also afflict their ability to efficaciously sue to enforce insiders’ legal obligations to the small, outsider shareholders.

Collective remedies, such as class actions and derivative lawsuits are available, by which the shareholders can sue en masse. But these modes of private enforcement have their own weaknesses, especially in that the shareholders’ lawyers’ incentives do not match the incentives of the shareholders themselves: The lawyers’ incentives are to achieve a settlement that pays the lawyers well while the lawyer spends limited time and energy on the lawsuit. Coffee (2006). Shareholders’ financial recovery is of less direct interest to the lawyers, and usually the lawyers have historically controlled such securities litigation in the United States.

Nor is it clear that our system of private litigation in the United States consistently imposes the right level of sanctions. To begin with, much private litigation results in meager settlements. Perhaps the meager financial recovery is due to the skewed incentives of plaintiffs’ counsel, see Coffee (2006) and

Romano (2005). Even when larger settlements are reached, they do not often impose large financial costs on directors or managers, typically the relevant actors.<sup>1</sup> It's often the firm that pays. If it's the firm that pays and not the wrongdoing agents who pay, the firm's *shareholders* ultimately write check for the insiders' wrongdoing. Yet the firm's *shareholders* are often innocent of wrongdoing. They're *not* the wrongdoing insiders. This shifting of damage liability from wrongdoer to innocent shareholder often occurs in American corporate and securities litigation, because of indemnification and liability insurance arrangements. Typically the firm pays off the expenses and lawsuits against wrongdoing managers (indemnifying them). Often insurance policies (the so-called D&O, directors' and officers' policies) have insurers picking up the tab and the firm (i.e., the shareholders ultimately) paying the insurance premiums. Directors and managers often insist when agreeing to serve, or as part of their employment, on indemnification and insurance being in place to cover their liability. To be sure, wrongdoing insiders cannot shift all the costs to outside shareholders (and some of these costs might reduce directors' and officers' direct compensation) and some costs are anticipated in compensation contracts. And, even when insiders can shift the costs to outsiders, managers do not view such results as costless — for the typical manager, sitting for a day in a nasty deposition is an uncomfortable experience — but we should not begin with a prior belief that private corporate and securities litigation *as actually enforced* is well constructed. Coffee (2006) and Kraakman, Park & Shavell (1994) review and analyze these issues.

Public and private parties' motivations differ. Private parties typically sue to recover their private losses; they care little for public benefits such as deterrence, example-making, and bringing pressure on outside, potential wrongdoers to desist, as Polinsky (1980: 107), Segal and Whinston (2006: 6), and Shavell (2004) demonstrate. Public authorities use examples for general deterrence, seek publicity to signal potential wrongdoers that the authorities will go after them, and pick the most egregious cases to attack. Public authorities can invoke informal mechanisms, such as private consultations between

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<sup>1</sup> To be sure here, a system that perfectly deters wrongdoing would exhibit no large financial penalties, because the actors internalize the enforcement threat and do little wrong. The actual effects of the larger lawsuits — with the penalties being visited upon actors other than the wrongdoers — makes this result unlikely.

regulator and regulated or administration guidance. This enforcement mode is common in the United Kingdom and Japan. And only public authorities can invoke severe sanctions, such as criminal penalties.

The legal literature suggests a further reason to be skeptical that private enforcement fully dominates public enforcement. Recent legal scholarship has discovered that private litigants in the United States often rely on public enforcement authorities doing the preliminary legal spadework that makes plaintiffs' lawsuits more likely to succeed. First, private lawsuits are much more likely to be brought in the United States when the SEC initiates its own parallel enforcement proceedings. Cox et al. (2003). Second, settlements are higher when there's a public enforcement action against the wrongdoers. Id. Third, even when there is no contemporaneous public enforcement action in the specific instance of wrongdoing, private plaintiffs in the United States as well as investors benefit greatly from the well-developed system of disclosure rules and legal requirements that the SEC and other regulators have developed over many decades. This ongoing system works in part because public authorities enforce disclosure rules via penalties and ongoing market surveillance.<sup>2</sup> And the other main mechanism of private enforcement — the market's pricing reaction to the disclosure of information — also depends on adequate public enforcement activity. Prices react sharply when public authorities announce enforcement measures, at least in the United States. Karpoff et al. (2006). Thus, *private* enforcement of federal securities laws is inexorably linked to the *public* regulatory system.

Economic theory also suggests that the relative value of private and public enforcement may vary by context. Polinsky and Shavell (2000: 46) contend that when the harmed parties know the mechanism of harm and the identity of the perpetrator, private enforcement can be better than public enforcement. But the reverse is also true, they show: when victims cannot identify the culprit, public enforcement in theory should outperform private enforcement. For insider trading — which can obviously harm securities markets — the victim typically cannot identify the secret insider who's trading on nonpublic information. Bhattacharya and Daouk (2006: 4) analyze the enforcement problem here. Similarly, the standard

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<sup>2</sup> Evidence also indicates that, when regulators develop more elaborate legal requirements for insider trading, the technical performance of trading markets improves. Beny (2005).

economics of free-riding and collective action problems tells us that public enforcement could have advantages when the harms are diffuse but important in the aggregate.

Moreover, if one looks closely at the oversight of securities markets in many of the world's most developed financial markets, one does not observe wholly distinct systems of public regulation and private ordering. Self-regulatory organizations — both in the form of exchanges and industry groups such as the NASD in the United States or IDA in Canada or the recently-constructed PCOAB in the United States (Coates (2007)) — play an important role in monitoring securities markets. These self-regulatory organizations are designed to combine the virtues of industry expertise and of publicly-sanctioned enforcement powers. Mahoney (1997). Within the simple public-private dichotomy, it is not even clear where one classifies such key tools of market oversight. They are neither fully private nor fully public.

Lastly and more traditionally, lawyers and lawsuits are not cheap. Some estimates see 40 to 60 percent of the cost of all private litigation in the United States dissipated through litigation costs as opposed to payments to injured parties. Shavell (2004). And there is an economy of scale in private lawsuits: securities litigation focuses on companies with a market capitalization of at least \$50 million. Goshen (2007); cf. Choi (2004); Bohn & Choi (1996). Alexander (1991) estimated fifteen years ago that losses in initial public offerings of less than \$20 million just didn't call forth private lawsuits. Alexander (1991: 511-12). Private enforcement mechanisms may be good for one class of firms — the bigger ones — but weaker for another class.

In short, substantial prior legal scholarship suggests that purely (or even mostly) private enforcement of securities laws is an imperfect tool for policing capital markets. The transaction costs for private litigation can in principle be quite high. Within the American financial regulatory system — where the issue has been most closely studied — private litigation is often misdirected, its useful elements often depend on public enforcement efforts, and often public and private enforcement institutions are tightly intertwined. Even though public enforcement has limitations, and those limitations can be severe,

the two mechanisms of capital market oversight might complement each other, such that neither can work well without the other.

## **B. Public Enforcement**

We do not approach public enforcement with a prior belief that government actors have perfect — or even good — incentives, nor do we begin with the view that government actors usually — or even often — possess enough knowledge to enforce securities laws ideally via public means. But we also do not start with the prior belief that private securities litigation in the nation where it is most highly developed is regularly efficacious in providing a good set of penalties and incentives.

Public enforcement has debilities differing from those afflicting private enforcement. Public agents are not directly rewarded for building better capital markets. Politicians appoint them and those politicians are beholden to an electorate that is itself poorly informed about financial markets. Regulatory authorities may be more interested in achieving favorable media coverage, for its own sake or to enhance their success in budget negotiations or in future campaigns for political office. And regulatory authorities may be subject to agency capture, leading them to favor special interest groups over the public interest.

Still, existing evidence indicates that public enforcement has a role to play. Bhattacharya and Daouk (2006) find significant relationships between whether a nation has brought an insider trading enforcement action and financial outcomes. Cox et al. (2003) show that private lawsuits are more efficacious after regulatory authorities have acted. Karpoff et al. (2006) and Alexander (1999) suggest that public enforcement actions significantly affect firms' capacity to do business smoothly and, hence, should induce corporate actors to want to avoid public sanctions. And, as described above, many aspects of effective private enforcement depend on competent and rigorous public oversight. Moreover, criminal penalties in the United States for financial fraud are substantial and common. While a deep evaluation of their impact on behavior has yet to be done, if criminal penalties are important, only public enforcement can bring them about. Karpoff et al. (2007) report that of the 2,206 individuals whom the SEC targeted

for financial misrepresentation from 1978 to 2006, 93 percent lost their jobs and 28 percent faced criminal charges, with half of those sentenced to an average of five years of jail time.

Public authorities have a wide range of levers to influence market participants. In addition to fines and penalties — which can deter in the same way that private damage awards deter — securities regulators can enjoin private parties actions and obtain cease-and-desist orders to proscribe objectionable behavior. Public authorities can revoke the many licenses and listing privileges that firms, particularly financial firms, must have to stay in business. These levers allow public authorities to pursue informal, less visible means to affect private behavior, including private meetings, informal guidance, and pressure on market actors to come up with, and to comply with, codes of best practice. The well-known British City Code is one such privately-produced but publicly-induced measure. Such informal actions require adequate budgets and high-quality staff to be effective.

The relative value of public and private enforcement may in theory be difficult, or impossible, to assess. One can think of public enforcement as economizing on the transaction costs of misdirected agents (the attorneys who drive such litigation) and severe free-rider effects among shareholders; yet, while it economizes in some dimensions, public enforcement introduces the typical distortions of poorly incentivized and often poorly informed public actors. But one undisputed empirical fact is exceedingly relevant here. A high level of private securities litigation is a uniquely American phenomenon. Yet, in many other jurisdictions with deep and robust capital markets, such as Japan and the United Kingdom, American-style class-action securities litigation is either rare or nonexistent (although consultative regulation and administrative guidance are common), strongly suggesting that private litigation is not a *sine qua non* for robust securities markets.

### **C. Corruption and Enforcement**

To be sure, the enforcement problems afflicting developing nations differ from the problems afflicting developed ones. In developing nations, neither corrupt regulators who use their public power to extract bribes nor regulators in overly statist governments that use regulation to punish political

opponents will build financial markets. Building up public enforcement there may just further empower deleterious elements in that society.

But there's little reason to believe that *private* litigation via the judiciary in such nations is structurally *more* efficacious than *public* regulation: one would expect *both* the judiciary and the regulators to be corrupt and ineffective. For both public and private enforcement, the problem confronting nations with corrupt governments is the need to improve public administration, improving either regulatory bodies in the case of public enforcement or the courts in the case of private enforcement. It is not obvious which institutional structure — the judiciary or the regulators — is easier to reform. Indeed, since courts tend to deal with a broad array of issues (contract, tort, family law, and so on), those seeking to strengthen financial markets in such nations may find it *easier* to build up specialized regulators first. The mechanisms for providing such specialized assistance — through technical assistance from agencies such as the SEC or through financial industry sponsored initiatives — are probably better developed and more effective than more broadly based assistance programs for overall judicial reforms.

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Hence, when we compare private to public enforcement, we see two highly imperfect enforcement channels. It's not clear in principle whether one is central and the other unimportant, whether both play a role, or whether the two work in tandem reinforcing the value of each other.

## **II. Data Description: Measuring Public Enforcement via Regulators' Staffing and Budgetary Resources**

We develop in this article several measures of the intensity of public enforcement of securities regulation based on budgetary resources and staffing levels. Our resource-based concept of public enforcement efficacy measures the level of public resources a nation allocates to its financial regulators normalized to reflect either the economic size of the nation or its population. Higher budget and greater staffing allow the regulator to examine allegations of wrongdoing, to write its rules carefully, to conduct market surveillance and review filings, and to act more often to remedy, prevent, and punish wrongdoing.

We do not focus on the regulatory agencies' formal degrees of independence from other governmental authorities or on formal levels of authority to fine or incarcerate wrongdoers, although this has been where prior work on public enforcement has focused. In our view, regulatory independence and high levels of agency authority are of little value to effective public enforcement if the agency's budget is minuscule and its staffing thin. And conversely, a not-very-independent regulator with a high budget and strong staffing indicates that political and market authorities have given the agency the go-ahead to enforce financial rules. Similarly, even with limited formal sanctions available to it, a well-staffed and well-funded agency can make good use of the sanctions that it has.

Yet, our measures based on budgets and staffing also have their limits, both in concept and construction. Simply because budgetary authorities have allocated ample resources to regulatory authorities does not mean that the regulator deploys these resources wisely or that it even deploys these resources for the purposes for which they were appropriated. Simply because a securities regulator has ample resources does not guarantee that the regulator utilizes those resources to bring enforcement actions, to write good rules, and to hire good people. Deep staffing of sinecures or crony-oriented appointees may lead to a regulator showing a high budget and high staffing levels, but would result in little enforcing of securities regulation. Indeed, we see examples of jurisdictions maintaining relatively similar regulatory staffing levels and budgets, but imposing quite different levels of public sanctions. Moreover, lean staffing and a low budget, if the regulator knows how to pick battles and impose severe penalties, can yield strong public enforcement, such that private actors calculate that the probability of being caught times the penalty imposed exceeds the private benefits sought. Thus, staffing levels and budgets may be a noisy proxy for effective public oversight of capital markets. Jackson (2006). But, while they should not be the last word in measuring public enforcement, these variables still improve our understanding here and help to give us a better preliminary view on the tension between the legal literature's mixed view of private enforcement and the finance literature's negative view of public enforcement.

We considered shifting to enforcement outcomes, such as indicators of aggregate regulatory actions brought, fines levied, and conviction rates. After all, staffing and budget are inputs to enforcement, not ultimate outputs. But we decided not to shift our focus away from staffing and budgets for two reasons, one practical and the other analytic. First, such output data is simply not available for many countries. Second, even if it were available, its interpretation would be ambiguous. A low level of enforcement output could result from the regulators having tremendous budgets and staffing, such that financial actors would not dare break any of the regulator's rules. Third, the mechanisms of enforcement may differ across national boundaries, making variables based on publicly-reported formal sanctions inaccurate measures of regulatory outputs for many jurisdictions. As La Porta et al. (2006: 3) note, public authorities can take a variety of actions — ranging from rulemaking to examinations to formal sanctions — not all of which a metric based solely on direct enforcement actions would reflect.

Constructing a budget and staffing data-set comparable across nations is not an easy task. See Jackson (2007). Many jurisdictions divide regulatory responsibility among different bodies, with some bodies operating as self-regulatory organizations and others located within subnational jurisdictions, such as states or provinces. Other nations consolidate capital market regulation in a single financial supervisory agency or in the nation's central bank. It is often hard to break out the budgets and staffing of securities regulators from those of larger governmental bodies.<sup>3</sup> Another problem is that some jurisdictions include in the budgetary aggregates various fees and taxes some of which may be passed on to other government agencies, whereas other jurisdictions report only actual expenditures for supervisory activities.

Notwithstanding these difficulties, we extended Jackson (2007) by assembling for this paper resource-based data — budget and staffing — for three samples of jurisdictions. We list that data in Table 2. The underlying data is drawn primarily from the 2006 edition of *How Countries Supervise their Banks, Insurers and Securities Markets*, which the Central Banking Publications of London compiles

annually.<sup>4</sup> Each sample of jurisdictions includes two resource-based measures. One is the number of regulatory staff charged with overseeing capital markets divided by country population; the other is the securities regulatory budget divided by GDP. Our first pair of samples — the most basic ones — consist of jurisdictions for which the *How Countries Supervise* source indicated the regulatory staffing level and budget associated with securities enforcement. This sample yielded staffing estimates for 30 countries and budget observations for 26.

*How Countries Supervise* (2006) also indicated the budgets and staffing of several consolidated regulators that are responsible for other areas of the financial services industry as well; data for such regulators are not included in the first, basic sample. But for our second pair of variables, which we denominate our extended samples, we add to the basic sample those jurisdictions where there was objective information with which to allocate staffing and budgets of consolidated agencies to the securities sector, generally through personnel reports included in annual reports or other agency publications. This extended sample includes 36 staffing and 32 budget observations.

Our final sample set, denominated the extrapolated samples, adds to the extended sample the remaining jurisdictions for which the Central Banking Publications data included staffing or budget estimates for consolidated agencies, but for which there was no metric to allocate directly the staffing and budget levels for the securities division. For these jurisdictions, we extrapolated staffing and budget estimates based on the median ratio of securities staff or budgets to other areas of financial regulation where direct data on both levels were available.<sup>5</sup> The extrapolated samples include 53 staffing and 46 budget observations. A description of these and our other variables is in Table 1.

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<sup>3</sup> Another complexity, which we have considered but not incorporated into our analysis, is whether to factor economies of scale into our measures of budgets and staffing. We simply normalize staffing levels by national populations and budgets by GDP at current exchange rates.

<sup>4</sup> Jackson (2007) combined data from an earlier edition of this publication with other data sources. For this paper, we relied on a single data source for our underlying estimates of budget and staffing. However, to the extent that we are aware of improvements such as those of the type used in Jackson (2007) (such as errors in the Central Banking data), the corrections would generally improve the relationship between our new public enforcement variables and the size of domestic capital markets.

<sup>5</sup> As one check on this method of extrapolation, we examined the median ratios used to allocate consolidated staff and budgets in the extrapolated sample to the allocation ratios that would apply to the consolidated regulators in the extended samples, for which we have both an objective allocation and can construct an extrapolated one. In the majority of cases, the objective allocation ratios were quite close to the extrapolation ratios.

We focus our discussion on the extended samples for staffing and budget and report results using the extended samples as our principal resource-based measure of public enforcement. We do so because the extended samples are the largest samples based on objective, not extrapolated, measures of staffing and budgets. But we also run our principal regressions with the basic and extrapolated samples and include the most important results with these alternative samples in appendices. For all of our principal findings, the results we report here are consistent across the three pairs of resource-based samples.

In Table 1 we report our six samples of resource-based public enforcement variables, alongside two indices of public enforcement, based on the regulators' formal powers that La Porta et al. (2006) and Djankov et al. (2005) develop. This resource-based data becomes the primary independent variable analyzed in this paper. At the base of Table 2, we report summary statistics for the resource-based, staffing and budget variables.

The values of the resource-based enforcement variables vary considerably across countries. Countries allocate very different levels of resources to financial oversight. To some degree, these differences reflect the differing roles of financial markets in certain jurisdictions: Financial centers like Hong Kong and Luxembourg have the highest staffing and budgets, and they are jurisdictions where financial activity plays a disproportionately large role in the domestic economy. But even if one looks to more traditional economies, the variation is ample. Canada reports nearly 39 regulators staffing their securities agency per million of population whereas Spain, with a comparable GDP, reports only slightly more than 7 staffers per million of population. By this measure Spain's regulatory intensity is less than one-fifth that of Canada.

In Table 3, we report a matrix of pair-wise correlation coefficients for the resource-based enforcement variables and several other variables that we will run, such as indicators of private enforcement and indices from LLS and Djankov of the regulators' formal public enforcement powers. These latter indices seek to measure public enforcement intensity by constructing indices of law-on-the-books, including indicators of the formal powers of the public enforcers. Looking first to the correlations

between the indicators of regulators' formal powers and all three of our variables, there is some but not overwhelming correlation — generally positive but never greater than 0.5. Accordingly, some countries must be adopting formal rules that could facilitate enforcement, but then fail to allocate the staffing and budget that could make the regulator effective. France illustrates this phenomenon. It has a quite high formal powers index score of 0.77 on the LLS formal enforcement index, more than a standard deviation *above* the mean. But its regulatory budget is only \$29.2 thousand per billion of GDP, less than a third of the mean. If we looked only to the regulator's formal powers and independence, France appears to be *high* on public enforcement, but in our view it should be coded as *low* on public enforcement. This may help explain why our results for public enforcement (as predicting financial strength) differ from prior inquiries. The Netherlands, in contrast, has a slightly below average formal public enforcement authority, but, if we use real resources, it ranks well *above* the mean.

In the correlation matrix in Table 3 we also report the relationship between staffing and budget to other commonly used indices of interest, such as a securities disclosure index, a liability index, an anti-director rights index, and a judicial efficiency index. In general and as would be predicted, our variables are positively correlated with these other indices. This opens up the possibility that some of the good financial results previously associated with these three formal legal indices are due to their correlation with the intensity of regulatory enforcement.

One final point of interest in Table 3: The two leading indices of formal enforcement powers do not correlate with one another. Their pairwise correlation coefficient is negative (-0.01). As we discussed above, there are questions about the theoretical value to enforcement of the components with which these public enforcement indices are constructed. The lack of correlation between the two most commonly used indices of public enforcement in the finance literature does raise questions about their robustness for statistical analysis. One or the other — or perhaps both — would seem to be only weakly measuring the actual intensity of public enforcement.

### **III. Results**

The following is our basic research pattern: We first replicate prior analyses of formal public enforcement indices from La Porta et al. (2006), which concluded that public enforcement was not closely associated with four standard measures of capital markets. Next, we substitute our new resource-based measures of public enforcement for the prior enforcement indices, to determine whether our new variables are more closely associated with the four standard measures of robust capital markets La Porta et al. (2006) analyzed. Enforcement resources strongly and significantly correlate with the size of domestic capital markets. We then examine the relationship between our resource-based measures of public enforcement and more up-to-date 2004 measures of capital market robustness, including controls for a full range of private enforcement indices as well as other standard controls. Throughout, resource-based enforcement strongly and significantly correlates with the size of domestic capital markets. Moreover, our resource-based enforcement variables are consistently as strongly associated with robust capital markets with the best performing index of private enforcement (disclosure) and substantially more strongly associated with robust capital markets than several other indices of private enforcement, including liability rules and anti-director rights.

Those then are our primary results, that real resource measures of enforcement predict market breadth and are robust to, and in some instances dominate, private enforcement measures. On other measures of capital market quality — particularly those associated with dispersed ownership and benefits of private control — resource-based public enforcement is not as strongly associated with the outcomes as are some measures of private enforcement. And, for technical measures of stock market quality, neither public enforcement nor private enforcement are strongly associated with high quality markets. Lastly, after we present our primary results, we explore the relationship between legal origins and our measures of actual resources allocated to public enforcement activity. Here too there are surprises, as the indicators of what works do not nicely map onto legal origin's typical strengths and weaknesses. We end with a discussion and analysis of the difficulty of demonstrating causation in this context.

## **A. Public Enforcement and Market Size**

We begin our analysis by replicating previous work that did not find public enforcement to have much impact on financial market development, by recreating La Porta et al.'s (2006) key regressions on public enforcement. After replicating them, we substitute our resource-based enforcement variables for the variables based on formal regulatory powers. Following La Porta et al. (2006), we first use four dependent variables to measure the robustness of capital markets: stock market capitalization, trading volumes, number of domestic firms, and amount of capital raised through IPOs.

The reports appear in Table 4. In Panel A, we employ the staffing variable from our extended sample; in Panel B, we use our budget variable from the extended sample. Within each panel, we run four sets of regressions for each capital markets measure. In each set, we first replicate the original LLS regression (column 1), then substitute our resource-based variable for the LLS formal enforcement index (column 2), and then present a third regression with both a real resource variable and a formal public enforcement index (column 3). We repeat this set of regressions for each of the four variables measuring the robustness of capital markets. The results of these regressions demonstrate that our resource-based enforcements variables are superior to the LLS formal enforcement index in predicting the four measures of robust capital markets used in this analysis.

Consider the stock market capitalization set in Panel A. In the original LLS formulation, the formal enforcement index has a significant coefficient (at the 10 percent level) in predicting market capitalization. When we added our staffing variable to the regression, the formal public enforcement index is no longer statistically significant but our staffing variable is, at the 10 percent level. The significance of private enforcement indices also declines in several of our reformulated regressions. Look again at Panel A. The real-resource-based staffing variable is significant (at the 10 percent level) and the formal public enforcement index lacks significance.

The results in Table 4 strongly suggest that enforcement variables based on real resources are more closely associated with robust capital markets than are public enforcement indices based on formal

agency characteristics. When we run our resource-based variables head to head with the formal public enforcement index, the resource-based variables are significant in seven of eight cases, whereas the formal public enforcement index is significant only once. When we run our resource-based variables without the formal public enforcement variable, they had statistically significant coefficients in all eight of eight cases, whereas the formal index had statistically significant coefficients in four of eight solo runs.

### **B. Public Enforcement, Controlling For Private Enforcement**

We next see whether resource-based public enforcement is robust to private enforcement. We run regressions similar to those in Table 4, but add the two private enforcement indices that are most strongly associated with robust capital markets: disclosure and liabilities standards. In these regressions we use as dependent variables more current 2004 data from the World Bank databases, because we are no longer seeking to replicate the prior LLS work (which used the older measures).<sup>6</sup> (We similarly use a 2005 wealth control adjusted for purchasing power parity, as well as a control for judicial efficiency, and a control for anti-director rights following a revised index that Djankov et al. (2005) developed.) We report the results in Tables 5 and 6. We also ran extensions with our basic and extrapolated samples of resource-based public enforcement, as well as alternative specifications to check for robustness to influential observations and outliers in the three sample sets. These results, reported in the (unpublished) Appendix, are substantially consistent.

Prior analysis showed an association between disclosure and liability standards on the one hand and robust capital markets on the other. But it reported little association between robust capital markets and public enforcement. These findings are replicated in the first four columns of Panel A in Table 5. Here we can see that disclosure does display a consistent and statistically significant relationship to market capitalization even with controls for other key private enforcement indices. Public enforcement in these runs, when proxied via the index of formal powers, does not display such a relationship. In the next

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<sup>6</sup> These dependent variables are analogous to those used in La Porta et al. (2006), whose results we sought to replicate and extend in Table 4. But these more recent dependent variables are based on 2004 data rather than averages for the data from 1996 to 2000. We used contemporary wealth controls whenever possible. Also, our domestic firms measure is based on the ratio of domestic firms to population rather than the log of that ratio.

five columns of Panel A, however, the robustness of those findings is called into question. Once one adds in our real-resource variable for staffing in columns 7 and 9, the coefficient for disclosure remains positive, but loses its significance. Moreover, in columns 8 and 9, the coefficient for the private liability index turns negative in the face of resource-based public enforcement, a result that is inconsistent with prior writings on the subject in the finance literature but quite consistent with the dominant critique of private securities liability rules in the legal literature.<sup>7</sup> Through all five formulations, our resource-based public enforcement variables retain statistically significant, positive coefficients.

The results are similar in the other panels of Table 5, where we present analogous regressions for our three other measures of robust capital markets: trading, domestic firms, and IPO value. In each case, our resource-based staffing variable has statistically significant coefficients. While the disclosure index always stays positive, it sometimes loses statistical significance, and the liability index is rarely statistically significant and turns negative with some regularity. In Table 6, we run analogous regressions with our extended budget sample. The results are quite similar: resource-based public enforcement is generally significant; the results for private enforcement are mixed.

The results for the resource-based variables are significant economically as well as statistically. For example, the average ratio of stock market capitalization to GDP in the extended sample is about .85. The coefficient for our budget variable (.79) in Panel A of Table 6 implies that an increase of one standard deviation in the level of an average country's budget would be associated with a doubling of stock market capitalization. The implied impact of an increase of one standard deviation in the securities disclosure index would be to increase an average country's market capitalization to GDP by less than 10 percent.

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<sup>7</sup> To be clear here, we find significant positive results elsewhere associating disclosure rules and financial outcomes. And our priors are that disclosure rules (whose efficacy depends in part on public enforcement) are an integral and important part of the securities enforcement system. Our results for liability rules are overall not very supportive of its importance and those results are consistent with our own prior, but not with prior finance work, that liability rules may be important for litigation outcomes, but are not important to securities market depth and breadth.

### **C. Robustness Checks and Tests With Alternative Samples**

These results look encouraging for the view that public enforcement intensity, presumably when modeled satisfactorily, plays a role in keeping financial markets strong. We next examined whether these results for the extended resource-based sample held up over the two alternate sample specifications: the basic one (with just the basic, unsupplemented staffing and budget data reported in *How Countries Supervise* (2006)) and the extrapolated one (in which we extrapolate the staffing and budgets for integrated regulators that do not break out their securities regulators' staffing and budget from the ratios of those integrated regulators that do break out such data). The results are robust, with the basic sample's resource-based variable doing better than the extended sample's and the extrapolated sample's doing somewhat worse.

For the basic staffing sample of 30 observations, the data-set typically had sufficient data for the other variables for 28 observations to be present in three panels, but only 20 in the fourth, due to limitations in the number of observations for IPO values to GDP. As before, we ran the public enforcement as an independent variable in 4 panels, each with one of the 4 indicators of financial market strength as the dependent variable. Each panel had 5 separate specifications: resource-based enforcement alone (with just the controls), with formal public enforcement, with disclosure, with liability, and then against both disclosure and liability. The basic staffing sample was significant in all 20 specifications. Disclosure was positive in all 8 of the specifications in which it appeared, significant in 3; liability was always negative, significantly so in 4 of the 8 specifications in which it appeared. These results are presented in the (unpublished) Appendix, in Table 5A. Figures 1 and 2 present residual plots, illustrating the significant relationship between national financial outcomes and resource-based public enforcement, after controlling for private enforcement. Similar graphics are presented in figures in the Appendix for the other enforcement samples.

For the extrapolated staffing sample of 53 observations, 46 were present in three panels, 36 in one. The extrapolated staffing sample was significant in 10 of the 20 models, negative in one. Disclosure was

significant in 4 of the 8 specifications in which it appears, negative in one. These results are in Table 5B of the (unpublished) Appendix.

For the basic budget sample of 26 observations, 24 were preset in three panels, but only 17 in one. Resource-based public enforcement, as measured by the basic budget data was significant in all 20 specifications. Disclosure was significant in 4 out of its 8 appearances. Liability turned negative in 4 out of its 8 appearances, significantly so twice. These results are in Table 6A of the (unpublished) Appendix.

Lastly on this robustness check, we ran these 20 specifications on the extrapolated budget sample of 46 observations. In 3 of the 4 panels for outcomes, 39 observations survived, 31 in the fourth panel. The extrapolated budget variable was significant in all 20 specifications. Disclosure was significant twice in its 8 appearances, turning negative once. Liability was again erratic, turning negative in 4 of its 8 appearance, significantly so twice. These results are in Table 6B of the (unpublished) Appendix.

Overall, the resource-based measures of public enforcement are robust in predicated financial market size and depth across three sample specifications for each of two resource measures.

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With the relevant samples no larger than 53 observations, we sought to be sure that the overall results were not driven by a few influential observations. Hong Kong, Luxembourg, and Singapore have deep and broad financial markets and devote significant resources to supporting their markets. At one level, these observations seem consistent with the hypothesis that resource-based public enforcement plays an important role in financial markets. But it is appropriate to test for the effects of their influence nevertheless, as these small nations and their role as financial centers may in some ways differ from what happens in the rest of the world. To test whether their influence drives the results, we performed two tests. First, we re-ran the extended sample results using STATA's robust regression, which systematically clips or eliminates outliers. Second, we re-ran the regressions without observations for Hong Kong, Luxembourg, and Singapore.

The robust regression results for the extended staffing sample are in Table 5C of the (unpublished)

Appendix. The extended staffing variable is significant in 8 of the 20 specifications and turns negative once. Disclosure is significant in 4 of its 8 appearances. Liability again is erratic, having a negative coefficient in 4 of its 8 appearances. For the extended budget sample, the results (presented in Table 6C of the Appendix) are similar. We also manually removed observations for the three financial centers (Hong Kong, Luxembourg, and Singapore) and again, similar results were obtained. Examination of Figures 1 and 2 present Hong Kong as an influential point, but a visual relationship can be seen between the financial and enforcement residuals.

We also ran the robust regression (and the manual version) with the basic and extrapolated samples. We summarize the effects of robust regression on the coefficients for all three samples in Table 7. Generally, the robust regression reduces the frequency of significant outcomes for resource-based public enforcement, but many remain significant; only occasionally does the coefficient turn negative, and often the loss of significance is at the .15 level for the p-value. However, disclosure suffers from a similar drop in significance. Liability, the other private enforcement variable, performs erratically. We read the evidence here as not supporting a conclusion that private enforcement trumps public enforcement, with both resource-based public enforcement variables and the disclosure-based private enforcement one doing satisfactorily, but with the private enforcement liability variable doing poorly. The results (and our interpretation) of the effect of removing the three financial centers are similar and reported in Table 8A of the Appendix.

#### **D. Financial Variables Associated With Dispersed Ownership**

So far, we have limited our analysis to dependent variables associated with the size of a country's capital markets: stock market capitalization, turnover, number of domestic firms, and IPOs. All of these indicate an important role for public enforcement, as measured by the real resources of staffing and budgets as opposed to formal authority. Other outcome variables measure financial depth and we turn to those now, focusing on three dependent variables closely related to the dispersion of ownership: an index measuring ease of access to public markets, median block premiums, and ownership concentration.

Some analysts have grouped these dependent variables together, implicitly adopting the common views that dispersed ownership is closely linked with strong capital markets. La Porta et al. (2006). This perspective is contested, however, in the legal literature and some of the financial literature. Some maintain the opposite perspective, that concentrated ownership can reduce agency problems of corporate managers and, when potential agency costs are systematically high, one should expect to see more concentrated owners, even if legal protections (whether from public or private enforcement) are strong. See, e.g., Gilson (2006), Roe (2002), cf. Jensen (1989). A low block premium, however, indicates that a nation offers more financial opportunity than a nation with a high block premium, and, hence, is financially valuable. But low and high ownership separation are ambiguous. Indeed, if a country *needs* low ownership separation — because managerial agency costs are especially high, see Jensen (1986, 1989), especially for firms lacking blockholders that could check managers — then the bases for public policymakers to build institutions protecting outside investors in that nation's firms are less compelling than in other nations. That is, even if the institutions supporting diffuse ownership were available, they would not be widely used, because private arrangement to reduce managerial agency costs would be in especially high demand for such firms in such nations. Public policymakers who anticipate this disuse may desist from building those institutions, because ownership will not separate from control regardless of the institutional quality of shareholder protection. Roe (2002), Gilson (2006). Hence, even a legal system with good investor protection may find itself with concentrated ownership if managerial agency costs are best contained via concentrated ownership. Ownership would not diffuse in that nation, but its stock market capitalization as a percentage of GDP might well be high, as the concentrated owners use the stock market to finance their firms. Since the correlation between block premiums and ownership concentration is about .5, there is ample room in the data for nations to have low block premiums but modest ownership separation.

In Table 8, we regress these three variables associated with dispersed ownership on our two key measures of public enforcement and several other legal indices and controls. In these regressions, strong resource-based public enforcement does not strongly correlate with the variables associated with

dispersed ownership. Indeed in three of the equations (one with respect to block premiums and two for ownership concentration), the coefficient for our public enforcement variable points away from the direction associated with dispersed ownership and, for one of those three, is statistically significant. While these results require further study, at a minimum, one might conclude that to the extent that public enforcement positively influences the size of a country's capital markets, it does not directly disperse share ownership. Countries that dedicate more resources to securities enforcement do not necessarily have fewer closely held firms or lower premiums for control, although they have larger stock markets, more IPOs, and more firms overall.

Several possibilities may be in play here: One is that some nations with substantial concentrated ownership need more public enforcement resources to better control insider behavior, thereby generating a negative relationship between regulatory intensity and dispersed ownership. Another is that the dispersion of ownership represents a political choice in certain jurisdictions, and public regulators in such jurisdictions do not expend efforts to thwart that choice. Another is that public enforcement is most efficacious in dealing with the institutions of securities trading — brokers and dealers — and less effective than private remedies in dealing with corporate governance issues. Yet another possibility is that our enforcement measure, while superior to prior public enforcement measures, is still imperfect. It measures resources, not effectiveness. A better public enforcement variable might yield differing results for the impact of public enforcement on dispersed ownership.

#### **E. Limits to Both Private and Public Enforcement: Intermediate Financial Variables**

For additional insight into the channels through which public and private enforcement affect capital markets outcomes, we also ran these regressions using as dependent variables the World Bank measures of capital market development, involving the size of equity markets, ease of access to equity markets, stock market efficiency, and stock market stability. The World Bank data also has an aggregate index that includes all information about a country's capital markets. We report the regression results in Table

9. In each regression, we included key enforcement variables, the private-remedy-oriented disclosure index, and a wealth control. The results are largely consistent with our previous findings.

In terms of the market size index and the World Bank's aggregate equity index, both our public enforcement variables and the disclosure index have positive and statistically significant effects. So, as we report above, it seems that both public enforcement and disclosure are associated with more robust capital markets. For the Equity Market Access Index — reflecting the ease with which companies can raise capital — the disclosure index has a positive and statistically significant coefficient, but our enforcement variables do not. Yet *neither* the public *nor* the private enforcement variables correlate closely with market efficiency and stability, as we report in Table 9.<sup>8</sup>

Since the regressions overall show both public enforcement and disclosure to be associated with the size of a nation's capital market, one might expect that either or both would correlate with improved technical performance of the securities markets. Perhaps, because our variables measure money and staff, but not overall effectiveness, a better-tuned public enforcement variable would yield different results. A better index may shed light on why our public enforcement measures predict stock market size but not results associated with dispersed ownership. Again, the private-enforcement variables fail to predict market efficiency and stability any better. Since neither enforcement variable does well here, we see this non-result as another reason to be cautious in rejecting public for private enforcement in policy-making.

## **F. Legal Origin and Regulatory Intensity**

Because the relationship between legal origins and the development of robust securities markets has been the subject of considerable interest in recent scholarship, see, e.g., La Porta et al. (1998), Roe (2006), we explore the relationship between common law origins and various measures of public enforcement. As Jackson (2007) and Roe (2006) observe, the level of public resources devoted to

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<sup>8</sup> We sought to see whether strong relationships existed with components of these World Bank efficiency indices, such as trading costs or price synchronicity, and either legal origin or public enforcement. We found no strong relationship between these on the one hand and either legal origin or public enforcement on the other. Nor did we find strong effects on national costs of capital for either our public enforcement variables or the private legal indices. Other recent work on this subject found several of the LLS legal indices, including their public enforcement index (of formal powers), to be associated with lower capital costs. See Hail and Leuz (2006). Other research found a relationship between some of the LLS legal indices and lower trading costs. Eleswarapu and Benkataraman (2005).

financial regulation is higher in common law than in civil law countries. This phenomenon is surprising in that the stereotypical view of civil law countries is that they regulate their economies more heavily. As Segal and Whinston (2006: 1) explain:

There are two basic approaches to deterring socially harmful behavior: with the threat of litigation by private parties or with enforcement by public agencies. Both approaches are used in most countries, but in varying degrees. Private litigation is common in the United States and (to a lesser extent) the United Kingdom and other “common law” jurisdictions. In contrast, the “civil law” countries, such as those of continental Europe, have far less private litigation, and rely more on enforcement by public agencies.

For financial market regulation and litigation, the opposite occurs, as measured by resources allocated to the regulatory task.

Table 10 shows a set of simple regressions in which the dependent variables are resource-based public enforcement, the LLS public enforcement index, and private enforcement (measured primarily by disclosure and liability). Explanatory variables are a common law dummy, a control for wealth, here the World Bank 2005 index measuring the log of gross national income per capita adjusted for purchasing power parity, and log GDP for 2004, which we include to control for scale effects.

Consider the results in Table 10. First, the common law dummy is statistically significant in predicting the private enforcement variables of disclosure and liability, the first of which regularly does well in predicting financial outcomes in several of our specifications, the second of which does not. Both of these have been associated with common law institutions of transparency, such as fiduciary duties and private litigation. But the common law dummy is significant as well for both resource-based public enforcement (as well as the LLS formal enforcement index). Common law jurisdictions expend more effort on, and devote more resources to, public enforcement, again challenging traditional assumptions about the primacy of public, state power in civil law jurisdictions. As measured by budgets and staffing, the common law nations are *more* regulatory than civil law nations. Hence, some institutions that seem to work in supporting financial markets are not those traditionally associated with common law strengths.

While Table 10 indicates that there is a measurable relationship between common law origins and the quality of both public enforcement and indices of private law quality, we were interested in exploring

whether common law also positively correlated with robust capital markets once one controlled for real resources expended on public enforcement and the standard private law indices. To explore this question, we re-ran our principal regressions with full controls (column 9 in Table 5 for staffing and column 5 in Table 6 for budget), using our four dependent variables with both staffing and budget variables from our three samples, using standard OLS once and robust regressions once. (This yielded 48 separate regressions.) In Table 11, we summarize the coefficients on common law for these 48 regressions. As this table reveals, common law is rarely significant and is often negative, sometimes significantly so.

These results suggest that whatever effect common law origins may have on the development of robust securities markets, a country seeking securities market development can achieve that effect by adopting legal rules, such as disclosure, *and* real-resource public enforcement regimes conducive to the emergence of capital markets. Public and private enforcement, strategies traditionally associated respectively with civil-law-style mechanisms on the one hand and common-law-style mechanisms on the other, both seem to play roles. Once one controls for a limited number of plausible legal mechanisms to reduce corporate and financial wrongdoing, the relationship between common law origins and capital market development fades away. This resonates with recent important analysis in the legal literature, to the effect that the set of basic corporate problems is small, as is the number of institutional solutions. Kraakman et al. (2004).

### **G. The Direction of Causality?**

As is usual in these finance inquiries, we cannot reject the possibility of reverse or bidirectional causality. In fact, our prior here is that causation is bidirectional, with strong financial markets inducing governments to protect an important constituency and an important market sector. Strong financial markets may emerge for reasons exogenous to the intensity of public enforcement (such as economic conditions, private ordering, or the absence of strong alternative financial channels) and then the relevant players (such as founders, investors, and public servants) call forth strong budgeting and more staffing in financial market regulators. For example, perhaps the underlying strength of property rights varies among

nations, with stronger property rights inducing economic growth, more complex enterprises and, in time, more developed financial markets. Mahoney (2001). Policymakers then find it valuable to protect these developed financial markets with stronger regulators. But we do want to see if we can reject the possibility that causation only runs from market breadth to enforcement strength. Persistent correlation alone does not allow us to do so.

The finance literature in the past decade relied on legal origin to demonstrate causality as running primarily from a legal variable to the financial outcome. The theory is that legal origin, having preceded current financial outcomes by centuries, could not have resulted from current financial markets. With legal origin often correlating with the legal variable being studied, especially variables relating to private litigation, the relationship makes legal origins a potentially useful instrumental variable, either formally (La Porta et al. (1998)) or informally (Djankov et al. (2005)). Since the theory was that private enforcement mechanisms associated directly with the common law (such as fiduciary duties) or indirectly with it were driving financial results, there was some plausibility to using legal origin as an instrument.

But problems afflict using legal origin to anchor causality here. True, staffing and budget correlate with legal origin: Common law nations spend more on financial regulation and throw more people into the financial regulatory fray than do civil law nations. But the causal connection between common law origin and high government spending and deeply-staffed regulators is more obscure than for other legal-financial connections based on, say, fiduciary duties. The causality of corporate law's effects on finance through fiduciary duties — if fiduciary duties were believed to be central to financial development — *could* be plausibly tied to legal origin, because common law systems traditionally used fiduciary duties and civil law systems are thought not to use anything similar. Similarly, causality could be associated with the disclosure variable, since disclosure could be seen as arising out of common law fiduciary concepts. And the causality of *heavy*-regulation regimes and poor financial outcomes could be tied to legal origin, because civil law systems are frequently seen to regulate their economies more extensively than do common law systems. But we find here that it's the *common* law systems that are budgeting

more for regulation and hiring more regulators, making the legal origin nexus fainter than in the other areas. While it's possible that the underlying idea is that common law systems just prefer financial markets, this seems too attenuated to serve as an instrumental variable anchor. After all, we are no longer talking of institutions — like fiduciary duties, judicial enforcement, and a common law aversion to regulatory intensity — but overarching preferences. But these preferences are only weakly tied to legal origin, since the legal origins originated long *before* financial markets were well developed, and many intervening events could have affected, and surely did affect, preferences. Perhaps because of these kinds of concerns, even proponents of using legal origin as an instrument seem to have withdrawn their support here. Djankov, Hart, McLeish, and Shleifer (2006: 35) (“Since legal origin influences many of the independent variables in these specifications, we cannot properly run instrumental variable regressions, and hence cannot claim that these effects are causal.”).

Notwithstanding these concerns, because of legal origins' prominence in the finance literature and prior readers' comments, we ran two-stage least squares regressions using legal origins as an instrument. In simple regressions, without controls, the resource-based public enforcement variables, when instrumented with legal origin, typically has a coefficient in the second stage that is significant in predicting basic financial market outcomes. The second stage coefficients on the instrumented variable are similarly significant when origin is used to instrument disclosure in predicting financial outcomes. For those who continue to see legal origin as a useful instrument, we thus note that we obtain qualitatively similar results when instrumenting public enforcement, regulatory variables, in simple models, as we do when using it to instrument private enforcement variables. Hence, those who still consider it a good instrument would need to consider public enforcement to be as strongly shown to cause strong financial market outcomes as private enforcement mechanisms. To us, though, the results cast further doubt on the value of legal origin as an instrument.<sup>9</sup>

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<sup>9</sup> Moreover, *neither* public nor private enforcement indices such as disclosure are effectively instrumented by common law in models that contain the full model with all controls, of the form in column 9 of Table 5. The critical coefficients in the second-stage regression in the models of that form were rarely significant and often negative. This lack of significance, and the frequency of the unexpected sign, occurs for both of the resource-based public, and the disclosure- and liability-based private, enforcement measures.

Because of the mounting concerns over the use of common law as an instrument, we investigated a second instrument: the log of GDP. As Jackson (2006) reports and as we confirm in Table 8, there are economies of scale in the regulatory staffing and regulatory budgets: The larger a nation's economy, fewer regulatory staff members or less regulatory expenditures could do the job as well. This phenomenon is reflected in a consistent negative correlation between the logarithm of national GDP and our resource-based public enforcement variables and makes it a potential instrument to explore causality. Scale effects also appear to exist for some private enforcement indices, such as the LLS disclosure index utilized in many of our previously reported regressions: the larger a country, the more extensive its disclosure requirements. Accordingly, log of GDP could be a plausible instrument of private law enforcement as well.

Table 12 reports the results of our instrumental variable regressions using two-stage least squared regressions for the four dependent variables used above to measure robust capital markets: market capitalization, trading, domestic firms, and IPOs.<sup>10</sup> In Panel A, we report a series of second-stage regressions along with the original OLS regressions (columns 1 and 3) and then the coefficients from second-stage regressions using first extended staff (column 2) and then extended budget (column 4). In Panel B, we report the corresponding first stage regressions.

While one must be careful about making strong inferences from analyses of the sort presented in Table 12 (see Murray (2006)), the results suggest that our new real resource enforcement variables play a causative role in the development of robust capital markets, and that the causal link is at least as strong as for the disclosure index, which suffers from similar potential endogeneity problems. The most straightforward evidence of the relationship is the number of statistically significant coefficients associated with our enforcement variables. The strength of our inferences here is tempered by the number of regressions that have Cragg-Donald F-statistics beneath the relevant Stock-Yogo critical values,

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Hence, in our full models, the common law instrument failed to produce evidence of causality for either public or private enforcement. This result may be seen as consistent with recent doubts about the usefulness of legal origin as an instrument.

<sup>10</sup> Comparable results are generally obtained if domestic firms or IPOs are used as the dependent variable. The results in Table 8 are not replicated when trading volume is the dependent variable.

suggesting that the instrument is weak. However, both the statistical significance of the coefficient on our real resources variables and their consistency of our estimated coefficients with the coefficients in the original OLS regressions increase our confidence in the usefulness of this analysis, as Murray (2006) suggests such results should.<sup>11</sup>

We do not claim to decisively resolve the question of causality. The log of GDP instrument is not ideal, and so the evidence of a causal link between the new enforcement variables and robust capital markets is suggestive but not definitive. However, measured at least by the second-stage regressions presented in Table 12, the evidence of a causal connection is as strong as the similar instrumental variable analysis that has been offered to support the causal link between private enforcement and robust capital markets. More anecdotally, regulatory budgets rise after market problems — scandals and crashes — and shrink during market highs. The American experience during the 1990s illustrates: When the stock market did well, Congress cut the SEC’s funding. After the Enron and related scandals indicated weaknesses, Congress raised the budget and expanded the SEC’s staffing. Increased public enforcement resources did not follow strong market results, but followed financial market weaknesses. We understand that this is a familiar pattern around the world. Moreover, the markets that have been most successful in attracting new listings in recent decades — the United Kingdom, Hong Kong, Luxembourg, and, until recently, the United States — have relatively high levels of real resources dedicated to securities enforcement. To the extent that the burden of our argument here is that academic writers and policymakers should not yet dismiss public enforcement as a potentially central source of capital market development, our instrumental variable analysis buttresses that claim.

#### **IV. Discussion**

Here we discuss the potential importance of public enforcement and why prior research led to contrasting conclusions.

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<sup>11</sup> We examined, but do not report, analysis using both instruments. The results were analogous, with public enforcement doing as well as, and often better than, private enforcement.

### **A. Channels from Public Enforcement to Financial Outcomes**

While our results show that public enforcement is clearly associated with important financial market outcomes, we are less sure of the channel through which public enforcement would work, because it can affect financial markets through several channels. First, high budgets and staffing make it easier for the public authorities to conduct market surveillance, by looking for wrongdoing and problems that need rule-making fixes. Market actors would presumably understand that authorities would be more likely to detect any deviant and punishable transgressions in nations where budgets and regulatory staffing are higher. Second, once wrongdoing is detected, higher budget and staffing facilitate regulatory investigations of wrongdoing, making it easier for the agency to bring enforcement actions, including those that result in large financial penalties and even jail terms. The penalties for these enforcement actions may not just be the direct ones imposed by the regulator, but indirect ones when public enforcement damages the firm's organizational and reputational capital. Recent studies have shown that the reputational damage, as trading partners impose market penalties, is quite high, often exceeding the direct financial penalties. Alexander (1999); Karpoff et al. (2006). Third, a higher budget and more staffing facilitate the regulatory agency being able to write, revise, and enforce better, more sophisticated regulatory rules. The completeness of legal rules has been shown to affect the efficacy of regulation of insider trading (Beny (2005)) and could be important more broadly. Fourth, much public enforcement is done informally, say, through administrative guidance in Japan or a regulator's raised eyebrow in England. Britain, with an important securities market, relies little on private litigation, Armour, Black, Cheffins, and Nolan (2007), and much on informal public enforcement. Even self-enforcement, such as Britain's well-known City Code on takeovers, comes from a public-private panel that includes representatives from the Bank of England and the stock exchange, as well as private players, and was for years convened by, and physically met in, the Bank of England. Armour and Skeel (2007). Such informal public enforcement requires highly trained staffers. However, we cannot distinguish which among these four major public enforcement channels is the most important.

Several market channels could also link public enforcement to stronger capital markets. One is the policing of the wrongdoing-potential of traditional, insider corporate governance actors who take value from outside investors unless the enforcement machinery prevents them from so doing. Financial markets also depend on relatively low-cost, transparent trading mechanisms. Regulatory budgets and staffing can make trading less dangerous for the typical shareholder, thereby deepening financial markets. Again, the aggregate budget numbers and total staffing numbers do not let us distinguish which market channel is most sensitive to budgets and staffing. The fact that public enforcement is closely associated with the size of capital markets but not the core corporate governance features that support capital markets is suggestive that this — trading regulation as opposed to just corporate governance — may well be an important and under-appreciated enforcement channel.

Finally, the precision of regulation may relate to our measure of public enforcement intensity. When we see simple, bright-line rules in a nation, we could attribute this result to several causes. (A popular one recently has been legal origin, with civil law nations using bright-line rules and common law nations using more subtle, fiduciary-based ex post decisionmaking. But it is not the only or, when one keeps the consequences of budgetary and staffing constraints in mind, even the most likely one.) A nation whose financial regulations are simple, bright-line rules may simply be a nation whose financial regulators have a weak budget and low levels of staffing. If there are only a few trained people inside the regulatory agency, and where the budget for rule-making is low, well-intentioned regulators may well have to choose bright-line rules with sporadic enforcement. Where the budgets are stronger and the staffing deeper, the agency can write more nuanced, tailored rules.

## **B. The Potential Importance of Public Enforcement**

The World Bank (2006a: 41-43) in its latest, famous *Doing Business* report asks how developing countries should reform their corporate laws. In answering its own question, the Bank focuses primarily on private enforcement via litigation as opposed to public enforcement via rule-making and regulatory enforcement: notify investors of directors' interests in deals, require disinterested director approvals, and,

where courts are strong, help investors bring private lawsuits. For its concept of public enforcement, it rely on research measuring agencies' formal characteristics, World Bank (2006a: 44 nn.3-5), not the real resource measures of budgets and staffing that we use in our analysis here. Similarly, its indices of investor protection rely on private-oriented remedies: the extent of disclosure, the extent of director private liability, and the ease of shareholder suits. World Bank (2006a: 93, 102-03). Our results here suggest caution. The same public agent — the legislature, for example — designs both the private rules and the public rules. Error and rent-seeking can degrade either.

### **C. Prior Public Enforcement Measures**

Although our method is in the spirit of the cross-country legal methodology that La Porta et al. (1998) pioneered in finance, the public enforcement results we report above differ from La Porta et al.'s (2006: 7-9) public enforcement results and interpretation. (They interpreted their results as indicating that public enforcement does not have a positive effect on the development of securities markets.) The contrasting results may be due to those authors using an index of public enforcement differing from that which we use here. La Porta et al. (2006) analyze a public enforcement index based on formal characteristics of financial supervision in 49 jurisdictions. The formal characteristics in their index are the financial supervisor's independence from the executive, the supervisor's level of investigative powers, its capacity to issue remedial orders, and the range of criminal sanctions available. They add these values up to reach an index of what they measure as public enforcement.<sup>12</sup> They then see how well this public enforcement index predicts indicia of capital markets development, compared to how well capital markets development is predicted by private law measures, such as a disclosure index, an index of liability standards, an index of anti-director rights, and an index of judicial efficiency.

From those regressions, the authors report the following principal findings:

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<sup>12</sup> A similarly motivated measure of public enforcement appears in Djankov et al. (2005), which proposes a second index of public enforcement, which ranges from 0 to 1, with a quarter point given when the regulator can sanction a specified insider transaction via "(1) [a] fine for the approving body; (2) jail sentences for the approving body; (3) fines for [principal wrongdoer]; and (4) [a] jail sentence for" the principal wrongdoer. Our analysis in this paper focuses primarily on the LLS index and empirical results presented in La Porta et al. (2006). We reran several regressions to see if using the Djankov index generated different results. It did not.

Perhaps most interestingly, both disclosure requirements and liability standards are positively correlated with larger stock markets. ...

The results for public enforcement ... are less consistent. Public enforcement only matters for the external-market-capitalization-to-GDP ratio and IPOs, although it has a large economic effect on both variables. ... In contrast, anti-director rights, but not public enforcement, matter for the number of firms, block premium, and ownership concentration.

These results suggest a preliminary view of what works, and what does not, in securities laws. *Public enforcement plays a modest role at best in the development of stock markets. In contrast, the development of stock markets is strongly associated with extensive disclosure requirements and a relatively low burden of proof on investors seeking to recover damages resulting from omissions of material information from the prospectus.*

La Porta et al. (2006: 19-20) (emphasis supplied).

The authors then skeptically conclude: “*All the evidence*,” they state, “suggests that relying on public enforcement is unlikely to be a useful strategy for jump-starting the development of securities markets in poor countries.” Id. at 25 (emphasis supplied). But in fact key elements of the evidence they report on public enforcement are rather positive: a strong correlation between their measure of public enforcement and both external-market-capitalization-to-GDP ratio and IPOs, with public enforcement having “a large economic effect on both variables.” La Porta et al. (2006: 2).<sup>13</sup> Hence, their own results are not fully consistent with a conclusion that “all the evidence” points to the weakness of public enforcement, as stock market capitalization and the number of IPOs in our view are important financial outcomes and are predicted by their public enforcement variable. Their paper also included a robustness check in which they analyzed the impact of supervisory staffing on capital market development and reported that “the (log) of the number of employees is insignificant in our regression.”<sup>14</sup> Our staffing regressions have different outcomes. Regardless of how one might interpret prior results, the results we report above do not support such a conclusion about the unimportance of public enforcement.

If their normative intention is simply that nations should not scuttle private litigation, we would fully agree. But to the extent that their results have been interpreted to mean that nations should scale

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<sup>13</sup> The La Porta et al. (2006) conclusions about public enforcement seem to have been influenced by the fact that in less developed countries their formal public enforcement variable was less strongly associated with robust capital markets than in their sample as a whole. This distinction did not exist for our public enforcement variables; indeed, the relationship between our public enforcement variables and capital market size is even *stronger* when we limit the analysis to non-OECD countries, although this sub-sample is small for conventional statistical analysis.

back their mechanisms of public enforcement and build financial markets principally around mechanisms of private enforcement — or that their evidence shows private enforcement to be demonstrably more efficacious than the other, as their conclusion seems to indicate — we cannot agree. Neither our evidence nor, we believe, even theirs supports such a conclusion. Yet, their conclusion — without even the ambiguities they note — has found its way into the World Bank’s (2006) important policy advice, cited above. Moreover, with the variables we introduce here, based on actual spending and staffing, we report the contrary: considerable evidence links public enforcement intensity to securities market breadth.

#### **D. Limitations to Prior Public Enforcement Indices**

How *should* public enforcement be measured? While the LLS public enforcement index usefully summarizes various formal aspects of regulatory policy, we suspect that it falls short of reliably indicating the efficacy of public enforcement. To begin, the direction of some factors included in their public enforcement index seems to us, perhaps as lawyers, to be ambiguous. They give more points to an independent securities oversight body, like the American Securities and Exchange Commission, as opposed to one integrated into a consolidated financial supervisor, like the British Financial Services Authority. While regulatory specialization, such as that at the SEC, may be useful, the wider field of regulatory vision afforded a consolidated regulatory operation could actually improve supervisory oversight. The literature is divided regarding the relative merits of consolidated supervision. Čihák & Podpiera (2006). Hence, without more evidence, we are uncomfortable with concluding that independence trumps consolidation and wonder whether coding based on such a conclusion accurately reflects the strength of public enforcement. The correct coding could be to the contrary of that used, or each regulatory structure may have offsetting costs and benefits.

Several coefficients for our wealth control in Table 10 are also interesting and bear on how best to measure public enforcement. For the two regressions using our real-resource-based public enforcement measures as dependent variables, wealth has a positive coefficient. This result is intuitive: wealthier

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<sup>14</sup> The functional form of our staffing and budget variables – which are normalized for population and the size of economy – may explain why we obtained different results. Also, our data on staffing and budgets may be more complete and accurate.

countries would expend more effort, even as a proportion of their GDP, on financial regulation (and other supervisory roles). But the *negative* relationship between wealth and the LLS formal index of public enforcement is puzzling. That wealthier jurisdictions do *not* systematically do better in the LLS index calls into question whether the index captures strongly meaningful measures of public oversight and suggests that alternative measures of the efficacy of public enforcement, such as those based on real resources, should be sought in order to measure public enforcement's efficacy.

A more fundamental problem with the authority-based enforcement index is that it relies heavily — too heavily, in our view — on the formal legal powers with respect to investigative authority and sanctioning. Consider, for example, the power of a securities regulator to refer securities law violations to criminal justice authorities for prosecution. While this seems a plausible proxy for the intensity of public law enforcement, recent studies show that many jurisdictions with these powers have little or no criminal prosecution of securities cases. See, e.g., Jackson (2006). And, although the scope of legal authority is no doubt important, more important are the *actual* practices of supervisory staff. As recent research into the efficacy of insider trading regulation has demonstrated (Bhattacharya & Daouk (2006)), the key issue is not whether a regulatory body has the *formal* power to sanction offending parties, but whether that power has *actually* been exercised. Without evidence linking formal powers to actual practices, the value of many key elements of LLS's public enforcement index is suppositional. Some agencies with formal powers may use them, while some may not, perhaps because they lack the budget and staffing resources to make their authority bite. An agency lacking a particular power may have a slightly inferior substitute, but have the budget and staffing resources to make good use of its (slightly weaker) formal authority.

### **E. Developing Better Measures of Public Enforcement**

Finally, we note the importance of developing better measures of public enforcement effort. The variables that we introduce in this paper are drawn from surveys undertaken by commercial publishers. While the data produces variables with quite strong predictive power, the source's research methods were not as attuned to issues of consistency and completeness as would have been ideal. Budgets and staffing

levels for a number of jurisdictions were not available and it would be preferable to gather direct information about the allocation of staffing and budgetary resources in countries with consolidated regulatory operations rather than relying on the extrapolating methods that we use. It would, moreover, be useful to collect information on the actual enforcement activities undertaken in each jurisdiction: how many cases prosecuted per year; how many sanctions imposed and with what level of monetary penalty; how many criminal convictions obtained, as the World Bank has successfully accomplished in related areas. It would also be useful to collect information on regulatory resources dedicated to other public functions, such as promulgating new regulations or conducting onsite examinations.<sup>15</sup> Only with more complete and higher quality data will analysts be able to make more authoritative conclusions regarding the impact of public enforcement activities on financial markets. If this paper does nothing more than to stimulate the World Bank and other agencies to develop such data, we will see our efforts as successful. Until then, we do not see the data as supporting the proposition that public enforcement is less valuable than private enforcement in developing financial markets.

## **F. Public Enforcement and Private Enforcement**

Our goal here is not to show that our real resource, public enforcement variables are more closely associated with strong capital market than other factors. Rather we seek to show that the intensity of public enforcement substantially and positively predicts the size of a country's capital markets and seems to contribute to stronger, deeper financial markets. The results we reported in Tables 6 and 7 show that the relationship between our real resource, public enforcement variables and the size of a country's capital market is as persistent as the relationship between market size and other legal factors that are well recognized in the literature. Indeed, the relationship between our public enforcement variables and market size seems to be at least as strong as that found for previously-prominent private litigation basics, such as liability standards and judicial efficiency.

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<sup>15</sup> Although not reflected in their conclusions (nor noted by the World Bank), La Porta et al. (2006: 21) found that the formal power to make regulation had a statistically significant relationship with robust capital markets.

While public and private enforcement interact in important ways, they do not interact simply via higher public enforcement resources going into designing better private liability systems. (Even if they did primarily interact in that way, the results here then suggest that the intensity of continuing *public* design work is what would make private enforcement effective.) A basic familiarity with the American Code of Federal Regulations as it relates to securities laws reveals detailed, step-by-step instructions on what firms must do when selling securities. Regulators examine financial firms, review issuers' filings with the regulator, and bring enforcement actions against wrong-doers. Banking and insurance industry regulatory intensity is high in nations where securities regulation is intense. Jackson (2007: 271). For both banking and insurance, even in the United States, private liability is trivial.

Outside of the United States, the securities regulators are rarely involved in judicial, private enforcement because there is very little of that, as Armour, Black, Cheffins, and Nolan (2007) show, so budget and staffing are likely to map onto the intensity of public enforcement. Bhattacharya and Daouk's (2006) result on the relationship between insider trading enforcement actions and securities markets' strength is consistent.

Our results reveal a fairly robust relationship between the intensity of public enforcement and the size of a country's capital markets. This relationship persists even when we control for legal indices measuring disclosure, liability standards, and judicial efficiency. While our measures of resource-based regulatory intensity do not predict every financial outcome equally well, they predict enough of them well to support the view that public regulation plays an important role in setting the institutional foundations for securities markets.

## **Conclusion**

The relative value of public and private enforcement is a key issue for understanding how financial markets deepen and broaden. Recent work in finance has focused on how securities laws work concludes that private enforcement via private lawsuits among contracting parties works best. Public enforcement is seen as working poorly. Yet the legal literature has persistently found deep flaws when analyzing private

enforcement mechanisms prevailing in the United States. Structural elements — like dispersed ownership — make collective private remedies potentially quite suboptimal and put authority in the lawyers' hands, an authority that we know is not relentlessly used in outside investors' interests.

Given this a priori indeterminacy, we constructed two categories of resource-based public enforcement intensity, one using regulatory budgets and another using regulatory staffing. We found significant correlation between measures of public enforcement and key financial outcomes, such as stock market capitalization, trading volumes, the number of domestic firms, and the number of IPOs. Moreover, in horse races between our measure of public enforcement and the usual measures of private enforcement, public enforcement typically dominates private enforcement in explaining these financial outcomes around the world.

The apparent importance of regulatory intensity seems to go against the basic characteristics of legal origin type. Intensity's importance thus casts some doubt on the literature assessing the importance of legal origin to financial outcomes, since the tool of public enforcement (as opposed to fiduciary-oriented private litigation) has not usually been strongly associated with the common law.

Still, there is more to do. Neither the public enforcement measures nor the private enforcement variables predict several intermediate outcomes associated with the development of strong capital markets, such as stock market efficiency or other more technical measures of stock market performance.

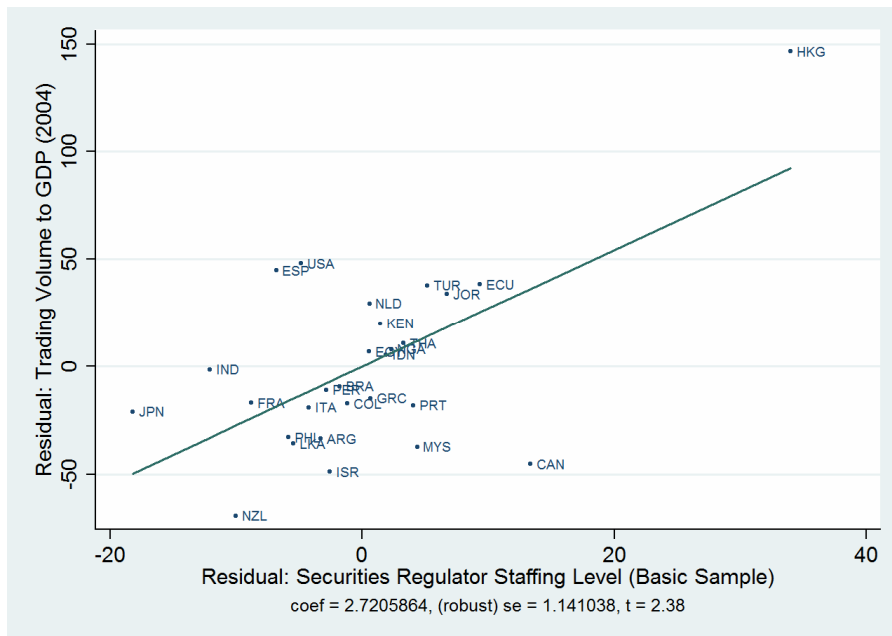
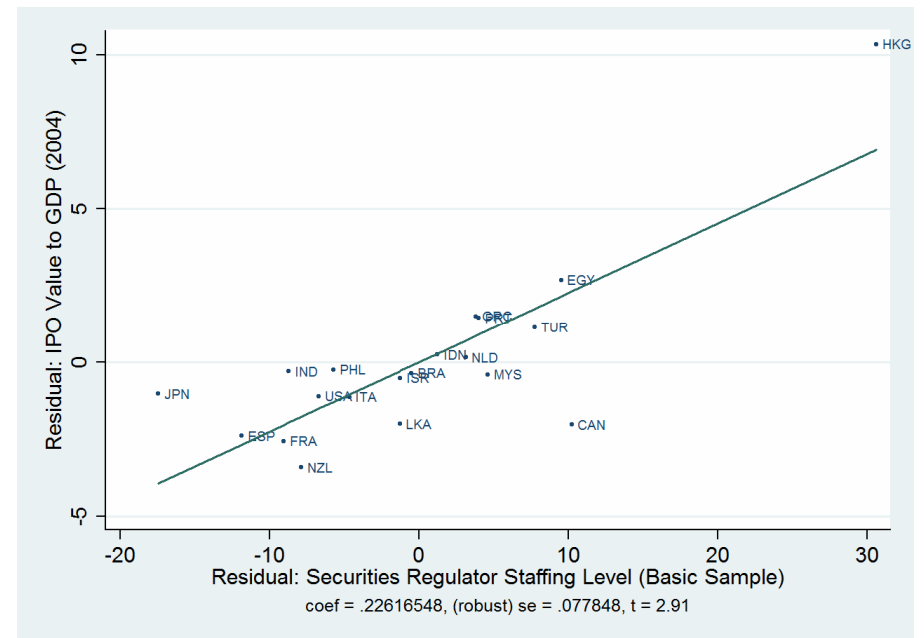
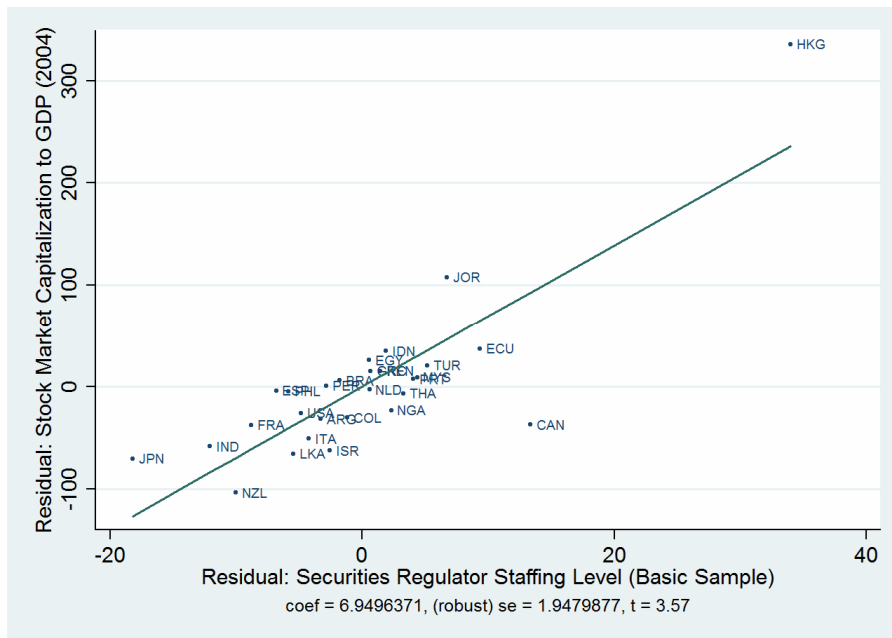
We re-emphasize that we do not see the data as telling us that public enforcement is more important than private enforcement. First, some financial indicators — in particular, those associated with dispersed ownership — seem to tie more strongly to private enforcement indices than to our public enforcement measures. Second, even where public enforcement is statistically significant, private indicators are sometimes significant as well. Third, the measures of both public and private enforcement — and of their effectiveness — are highly imperfect. Further improving how we measure these is possible for both and may yield better understanding of which outcomes public enforcement most affects, which ones private enforcement influences, and how the two main enforcement mechanisms interact.

Overall, and most importantly, we caution against using the current explanations for the relative power of private and public enforcement in making public policy around the world. Public enforcement as we measure it does rather well in the regressions. Since public actors typically design both the private and public systems, the debilities of public actors can creep into both designs. The evidence does not warrant rejecting public enforcement in favor of private enforcement for building good securities markets.

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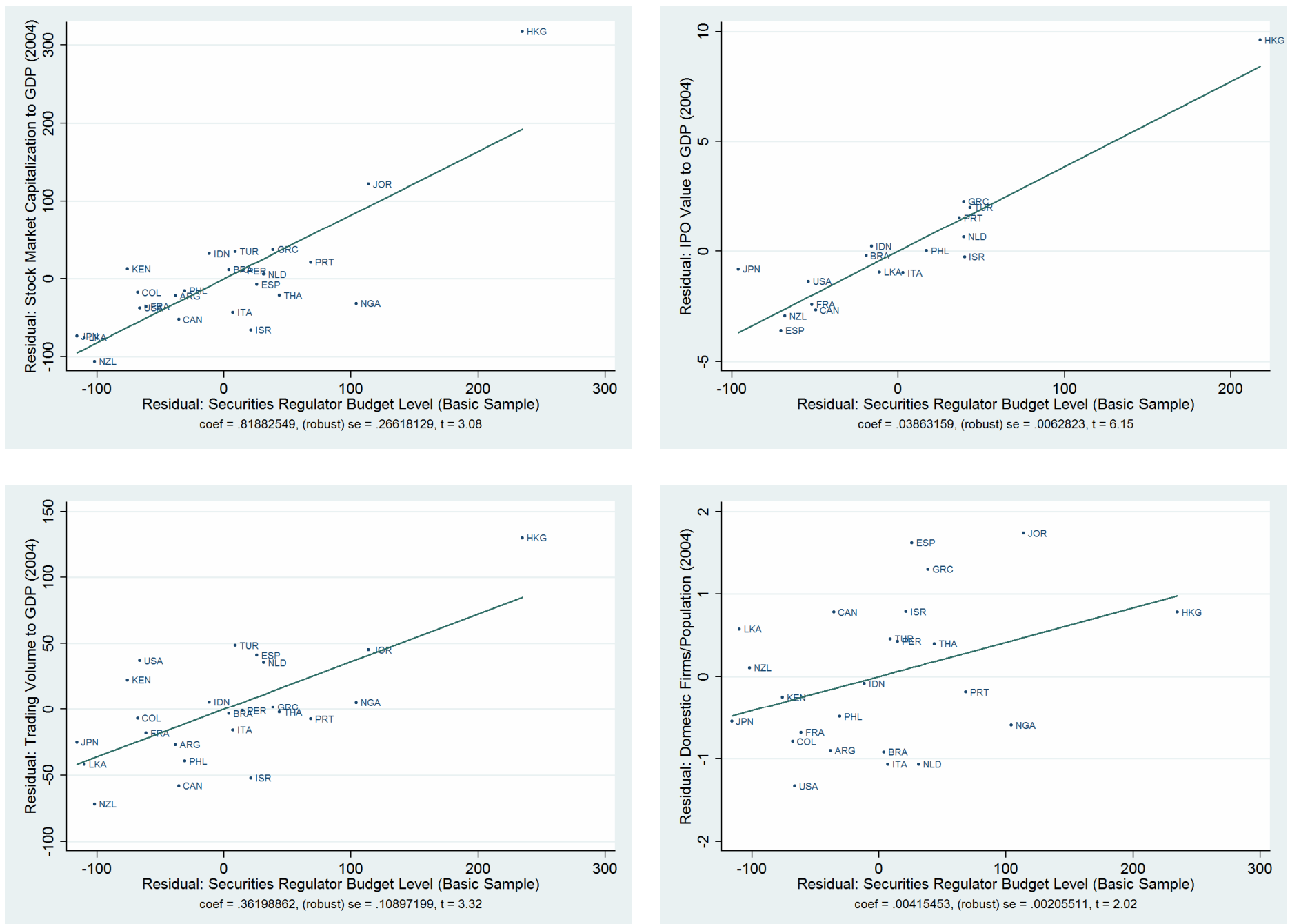
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**Figure 1. Residual plot of financial market size on securities regulator staffing**

These figures plot the residuals of four financial market outcomes on the residual for the regulators' staffing levels (basic sample). The model specification is of the form in Table 5, column 9, but using the basic budget sample. Independent variables are disclosure, liability standards, anti-director rights, a wealth control and judicial efficiency.



**Figure 2. Residual plot of financial market size on securities regulator budget**

These figures plot the residuals of four financial market outcomes on the residual for the regulators' budget levels (basic sample). The model specification is of the form in Table 6, column 5, but using the basic budget sample. Independent variables are disclosure, liability standards, anti-director rights, a wealth control and judicial efficiency.

Table 1  
Description of variables

This table lists our independent, dependent, and control variables. We measure public enforcement first with resources and second via the regulator's formal powers. Resources are measured by staffing levels (per million of population) and budgets (per billion US of GDP). For each resource measure, we begin with a basic sample, as reported by the How Countries Supervise (2006) databook. We extend each sample with staffing and budget data from other sources. We also extrapolate securities regulators' budgets and staffing for integrated regulators. We use public enforcement measures that look to the enforcement authorities' formal powers. Private enforcement variables measure the intensity of disclosure rules and measure the nature of liability in private litigation. The control variables seek to measure the countries' legal environment, their per capita wealth and income, and in some regressions nations' legal origin. Outcomes are standard measures of the size of securities markets, ownership considerations, and market efficiency.

Variable	Description	Sources
<i><u>Public enforcement: resource-based and formal powers</u></i>		
Staff per million population (basic sample)	The 2005 size of the securities regulators' staff, divided by the country's population in millions. The data is limited to those countries who report their staffing levels in the semi-official Guide to Financial Regulators, which reports 30 countries' staffing levels.	How Countries Supervise (2006); including only those countries where actual staffing levels were reported. Population data downloaded from World Bank Data and Statistics website.
Staff per million population (extended sample)	The 2005 size of the securities regulators' staff, divided by the country's population in millions. This data adds to the basic sample of Staff per million population observations for countries that report staffing data for a consolidated financial services regulator and for which the regulators' annual reports or similar official documents report the relevant staffing data. This adds observations for Belgium, Germany, Iceland, Ireland, Luxembourg, and the United Kingdom, yielding 36 observations in total.	How Countries Supervise (2006); regulators' annual reports and similar for several nations. Population data downloaded from World Bank Data and Statistics website. Individual country online sources are listed in the appendix.
Staff per million population (extended and extrapolated sample)	The 2005 size of the securities regulators' staff, divided by the country's population in millions. For this variable we added to Extended Staff observations for countries with consolidated financial supervisory bodies that do not report separate staffing levels for securities regulators. We extrapolated securities staffing estimates by using the median ratios of securities regulator staffing to the consolidated regulator's staffing for the jurisdictions that reported enough data to construct such a ratio. This yields 53 observations.	How Countries Supervise (2006); regulators' annual reports and similar official documents for the additional nations. Population data downloaded from World Bank Data and Statistics website.
Budget per billion US\$ of GDP (basic sample)	As with the basic Staff per million population, this variable is the securities' regulators' 2005 budget divided by the country's GDP, limited to those countries that report their staffing levels in the semi-official Guide to Financial Regulators, which reports 26 observations here. The budget reports were adjusted to US dollars at the December 31, 2005 exchange rates.	How Countries Supervise (2006); GDP data downloaded from World Bank Data and Statistics website.
Budget per billion US\$ of GDP (extended sample)	Analogous to extended Staff sample, yielding 32 observations, by adding budgets for the securities regulator in a consolidated financial authority for which objective allocation of the securities regulatory budget was possible. Budget in local currency were adjusted to US dollars at the December 31, 2005 exchange rate.	How Countries Supervise (2006); regulators' annual reports and similar official documents. GDP data downloaded from World Bank Data and Statistics website.
Budget per billion US\$ of GDP (extended and extrapolated sample)	Analogous to extrapolated Staff sample, yielding 46 observations. We added 14 observation to the extended budget sample for nations where overall staffing of the consolidated financial agency was available. We used the median ratio of securities staff to consolidated staff for countries in which the actual staffing levels of each were available, and the overall staffing level for each of these 14 observations by that ratio. Budget in local currency were adjusted to US dollars at the December 31, 2005 exchange rate.	How Countries Supervise (2006); regulators' annual reports and similar official documents. GDP data downloaded from World Bank Data and Statistics website.
Formal public enforcement index (LLS)	Formal public enforcement equals the arithmetic mean of: (1) supervisor characteristics index; (2) its rule-making power index; (3) its investigative powers index; (4) orders authority index; and (5) criminal authority index, as described in La Porta et al. (2006).	La Porta et al. (2006)
Formal public enforcement index (Djankov)	Public enforcement here is an index aggregating whether certain suspect corporate transactions can lead to a fine or jail sentences for the approving body, or fine or jail sentence for the the principal wrongdoer.	Djankov et al. (2005)
<i><u>Private enforcement indices</u></i>		
Disclosure index	The index of disclosure equals the arithmetic mean of: (1) nature of liability on a prospectus; (2) extend compensation must be disclosed; (3) shareholders disclosure; (4) extent inside ownership must be disclosed; (5) extent irregular contracts must be disclosed; (6) and the extent that related party and irregular transactions must be disclosed, as described in La Porta et al. (2006).	La Porta et al. (2006)
Liability standards index	The index of liability standards equals the arithmetic mean of: (1) liability standard for the issuer and its directors; (2) liability standard for the distributor; and (3) liability standard for the accountant, as described in La Porta et al. (2006).	La Porta et al. (2006)

Table 1 (con'd)  
Description of variables

<u>Control variables</u>		
Anti-director rights index (original and revised)	An index formed by adding one when: (1) shareholders can mail in their proxy vote; (2) shareholders are not required to deposit their shares prior to the shareholders' meeting; (3) cumulative voting is allowed; (4) an oppressed minorities mechanism is in place; (5) shareholders have preemptive rights by default; and (6) shareholders owning or voting 10% of the share capital can call a shareholders' meeting.	La Porta et al. (1998) (original), revised in Djankov et al. (2005)
Judicial efficiency index	Assessment of the efficiency and integrity of the legal environment as it affects business, particularly foreign firms produced by the country risk rating agency International Country Risk (ICR). It may be taken to represent investors assessment of conditions in the country in question. Average between 1980 and 1983. Scale from 0 to 10, with lower scores representing lower efficiency levels.	Political Risk Services (1996); International Country Risk Guide
Common law jurisdiction	Coding of countries by legal origin, as provided by LLSV (1996). Any data missing from La Porta et al. is coded using the CIA Factbook.	LLSV (1996: 1130-31), CIA Factbook (2006)
Ln GNI per capita	The log of gross national income per capita in 2005 adjusted on the basis of purchasing power parity.	Derived from data downloaded from World Bank Data and Statistics website.
Log of GDP	The log of 2005 gross domestic product in current US dollars.	Derived from GDP variables downloaded from World Bank Data and Statistics website.
<u>Outcome variables</u>		
Market capitalization	Ratio of a country's 2004 stock market capitalization to its GDP	World Bank (2006b)
Trading volume	Ratio of a country's 2004 stock market trading volumes to GDP	World Bank (2006b)
Domestic firms	The ratio of the number of a country's listed domestic firms in 2004 to millions of population	World Bank (2006b)
IPOs to GDP	The ratio of annual capital raised in a country through IPOs in 2004 to its GDP	World Bank (2006b)
Block premia	The block premia is computed taking the difference between the price per share paid for the control block and the exchange price two days after the announcement of the control transaction, dividing by the exchange price and multiplying by the ratio of the the proportion of cash flow rights represented in the controlling block, as described in Dyck and Zingales (2004).	Dyck and Zingales (2004)
Ownership concentration	Average percentage of common shares not owned by the top three shareholders in the ten largest non-financial, privately-owned domestic firms in a given country.	La Porta et al. (2006).
Access to equity	Index of the extent to which business executives in a country agree with the statement "Stock markets are open to new firms and medium-sized firms". Scale from 1 (strongly agree) though 7 (strongly disagree)	Schwab et al. (1999)
Equity index	Index averaging other indices measure market size, market access, market efficiency, and market stability in 2004.	World Bank (2006b)
Equity Market Size Index	Index of factors measuring the size of capital markets in 2004	World Bank (2006b)
Equity Market Access Index	Index of factors measuring the ease of accessing capital markets in 2004	World Bank (2006b)
Equity Market Efficiency Index	Index of factors measuring the efficiency of capital markets in 2004	World Bank (2006b)
Equity Market Stability Index	Index of factors measuring the stability of capital markets in 2004.	World Bank (2006b)

Table 2  
Resource-based securities law enforcement data: staffing and budget

This table provides resource-based data to measure public enforcement intensity, based on securities regulators' staffing and budget levels. The basic sample, in column 1, shows the staffing levels per million of a country's population, from *How Countries Supervise* (2006). Column 2 extends the sample to countries that provide their securities' regulators budget through other sources (indicated in Table 1). Column 3 adds in extrapolated staffing levels for integrated regulators: Some nations, such as the United Kingdom, have a single financial markets regulator, one that regulates securities markets, banking, and sometimes insurance. For integrated regulators that do not break out their securities regulators' budgets from the integrated budget, we extrapolated the securities regulatory budget using the median proportion of securities regulator budget/integrated regulatory budget for those nations for which we could determine both. Columns 4, 5, and 6 show resource-based securities regulator data for the regulators' budgets, measured analogously. Columns (7) and (8) show previously-assembled measures of public enforcement based on the regulator's formal authority, such as independence from the executive, rule-making authority, authority to bring criminal actions, and so on.

Country	Resource-based measures of public enforcement						Formal measures of public enforcement	
	Staff per million of population			Budget per billion US\$ of GDP			(7) Public Enforcement Index (LLS)	(8) Public Enforcement Index (Djankov)
	(1) Basic observations	(2) Basic sample, with extended observations	(3) Extended sample, with extrapolated observations	(4) Basic observations	(5) Basic sample, with extended observations	(6) Extended sample, with extrapolated observations		
Argentina	3.46	3.46	3.46	\$15,984	\$15,984	\$15,984	0.58	0.00
Australia			39.35			120,162	0.90	0.50
Austria			9.24			34,497	0.17	1.00
Belgium		12.93	12.93		67,274	67,274	0.15	0.50
Brazil	2.68	2.68	2.68	35,260	35,260	35,260	0.58	0.50
Canada	38.98	38.98	38.98	83,932	83,932	83,932	0.80	1.00
Chile			10.39			67,137	0.60	1.00
Colombia	3.88	3.88	3.88	46,603	46,603	46,603	0.58	0.00
Czech Republic	12.95	12.95	12.95	41,685	41,685	41,685		1.00
Denmark			12.34			33,745	0.37	0.75
Ecuador	7.41	7.41	7.41				0.55	1.00
Egypt	3.59	3.59	3.59				0.30	0.00
Finland			10.18			48,969	0.32	0.00
France	5.93	5.93	5.93	29,205	29,205	29,205	0.77	0.50
Germany		4.43	4.43		13,527	13,527	0.22	1.00
Greece	12.17	12.17	12.17	79,801	79,801	79,801	0.32	0.50
Hong Kong	58.47	58.47	58.47	365,740	365,740	365,740	0.87	0.00
Hungary			10.90			82,120		0.00
Iceland		15.86	15.86		44,094	44,094		0.00
India	0.43	0.43	0.43				0.67	0.50
Indonesia	1.97	1.97	1.97	5,571	5,571	5,571	0.62	0.00
Ireland		28.02	28.02		88,871	88,871	0.37	0.00
Israel	18.82	18.82	18.82	153,118	153,118	153,118	0.63	1.00
Italy	7.40	7.40	7.40	62,889	62,889	62,889	0.48	0.00
Japan	4.31	4.61	4.61	17,000	17,000	17,000	0.00	0.00
Jordan	14.90	14.90	14.90	326,569	326,569	326,569	0.60	0.00
Kenya	0.96	0.96	0.96	93,953	93,953	93,953	0.70	0.00
Korea			13.15			95,147	0.25	0.50
Luxembourg		314.43	314.43		512,897	512,897		1.00
Malaysia	22.65	22.65	22.65				0.77	1.00
Mexico			4.71			52,494	0.35	0.50
Netherlands	23.52	23.52	23.52	138,785	138,785	138,785	0.47	0.00
New Zealand	9.00	9.00	9.00	37,784	37,784	37,784	0.33	0.00
Nigeria	4.60	4.60	4.60	338,349	338,349	338,349	0.33	0.00
Norway			15.99			34,106	0.32	1.00
Pakistan			2.47				0.58	0.75
Peru	5.11	5.11	5.11	109,651	109,651	109,651	0.78	0.25
Philippines	4.37	4.37	4.37	66,120	66,120	66,120	0.83	0.00
Poland	4.64	4.64	4.64	22,961	22,961	22,961		1.00
Portugal	14.49	14.49	14.49	80,842	80,842	80,842	0.58	1.00
Singapore			88.28				0.87	1.00
Slovak Republic			13.93			53,844		0.00
South Africa			3.82			118,453	0.25	0.00
Spain	7.33	7.33	7.33	29,931	29,931	29,931	0.33	0.75
Sri Lanka	2.35	2.35	2.35	46,126	46,126	46,126	0.43	0.00
Sweden			10.23			27,234	0.50	1.00
Switzerland			8.04			31,418	0.33	0.50
Taiwan			14.65			73,061	0.52	0.00
Thailand	6.40	6.40	6.40	83,804	83,804	83,804	0.72	0.00
Turkey	6.18	6.18	6.18	45,417	45,417	45,417	0.63	0.00
United Kingdom		19.05	19.05		81,709	81,709	0.68	0.00
United States	23.64	23.29	23.29	76,459	76,459	76,459	0.90	0.00
Uruguay			50.66				0.57	0.50
Venezuela							0.55	0.00
Zimbabwe							0.42	
Number of observations	30	36	53	26	32	46	49	54
Mean	11.09	20.20	19.73	\$93,598	\$101,310	\$89,441	0.52	0.40
Median	6.29	7.36	10.18	\$64,504	\$66,697	\$64,504	0.55	0.38
Minimum	0.43	0.43	0.43	\$5,571	\$5,571	\$5,571	0.00	0.00
Maximum	58.47	314.43	314.43	\$365,740	\$512,897	\$512,897	0.90	0.90
Standard deviation	12.48	51.81	44.19	\$99,084	\$117,648	\$100,764	0.22	0.22

Table 3

## Pairwise Correlation Matrix for Key Enforcement Variables

In this table we report pairwise correlations between the key enforcement variables. The six resource-based measures of enforcement correlate at levels of .54 and higher. The resource-based measures of public enforcement do not correlate as strongly with enforcement measures derived from the regulators' formal powers, with the correlations at levels ranging from -.05 to .41. Of note is that the two public enforcement measures based on formal powers of the regulator correlate negatively.

	<i>Disclosure</i>	<i>Liability</i>	<i>Anti-director rights (original)</i>	<i>Judicial efficiency</i>	<i>Public enforcement (LLS)</i>	<i>Public enforcement (Djankov)</i>	<i>Regulatory staff (basic)</i>	<i>Regulatory staff (extended)</i>	<i>Regulatory staff (extrapolated)</i>	<i>Regulatory budget (basic)</i>	<i>Regulatory budget (extended)</i>	<i>Regulatory budget (extrapolated)</i>
Disclosure	1.00											
Liability standards	0.55	1.00										
Anti-director rights	0.52	0.50	1.00									
Judicial efficiency	0.25	0.22	0.21	1.00								
Public enforcement (LLS)	0.33	0.31	0.37	-0.11	1.00							
Public enforcement (Djankov)	-0.15	-0.08	0.06	0.24	-0.01	1.00						
Regulatory staff (basic)	0.40	0.45	0.40	0.56	0.39	0.17	1.00					
Regulatory staff (extended)	0.41	0.45	0.40	0.54	0.32	0.27	1.00	1.00				
Regulatory staff (extrapolated)	0.26	0.28	0.25	0.42	0.41	0.25	1.00	1.00	1.00			
Regulatory budget (basic)	0.25	0.02	-0.03	0.35	0.15	-0.19	0.54	0.54	0.54	1.00		
Regulatory budget (extended)	0.27	0.08	0.04	0.28	0.20	0.03	0.54	0.71	0.71	1.00	1.00	
Regulatory budget (extrapolated)	0.29	0.14	0.09	0.14	0.25	-0.05	0.54	0.71	0.71	1.00	1.00	1.00

Table 4  
Resource-based enforcement and formal powers-based enforcement as predicting financial market size

In this table, we test whether measures of the level of a nation's resource-based public enforcement predict the level of a nation's financial market strength. We use two measures here of resource-based public enforcement: the securities regulator's staffing level per million of population and the securities regulator's budget/GDP. Financial outcomes are the country's stock market capitalization/GDP, stock trading volume/GDP, number of domestic firms, and number of IPO's, all standard measures of financial development. We control for judicial efficiency, corporate law (via the anti-director rights index), and GDP/capita. We run three models for each pairing of enforcement and outcomes. In the first model, in column 1 of each the eight pairings, we see whether preexisting formal measure of regulatory powers predict the financial outcomes. In the second model, in column 2, we substitute the resource-based measure of public enforcement. In these eight pairings, resource-based enforcement is always positive and significant. In the third model, in column 3, we run both the formal measure of public enforcement and resource-based measure. Public enforcement is significant in these models once and turns negative once. The resource-based measure is significant in a majority of the cases. Robust standard errors are in parentheses; the constant is not reported.

	<u>Market Capitalization</u>			<u>Trading Volumes</u>			<u>Domestic Firms</u>			<u>IPOs</u>		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	Formal public enforcement	Staffing resources	Staffing and formal	Formal public enforcement	Staffing resources	Staffing and formal	Formal public enforcement	Staffing resources	Staffing and formal	Formal public enforcement	Staffing resources	Staffing and formal
Staffing resources (extended)	–	0.014 <sup>a</sup> (0.00)	0.014 <sup>b</sup> (0.00)	–	1.77 <sup>b</sup> (0.65)	1.42 <sup>c</sup> (0.83)	–	0.036 <sup>a</sup> (0.01)	0.034 <sup>a</sup> (0.01)	–	0.13 <sup>a</sup> (0.03)	0.14 <sup>a</sup> (0.04)
Formal public enforcement (LLS)	0.34 <sup>c</sup> (0.20)	–	0.36 (0.22)	39.56 (30.01)	–	35.82 (43.89)	0.64 (0.48)	–	0.15 (0.71)	3.72 <sup>b</sup> (1.55)	–	-0.60 (2.07)
Anti-director rights	0.07 <sup>b</sup> (0.03)	0.06 <sup>b</sup> (0.03)	0.04 (0.03)	7.86 (4.73)	9.69 <sup>c</sup> (5.63)	7.57 (5.62)	0.18 <sup>b</sup> (0.09)	0.04 (0.09)	0.03 (0.11)	0.31 (0.24)	0.32 (0.35)	0.36 (0.34)
Judicial efficiency	0.052 <sup>b</sup> (0.02)	0.03 (0.02)	0.044 <sup>c</sup> (0.02)	-2.10 (5.42)	-1.74 (8.13)	-0.23 (4.52)	0.26 <sup>a</sup> (0.08)	0.21 <sup>b</sup> (0.08)	0.224 <sup>b</sup> (0.08)	0.04 (0.22)	-0.23 (0.20)	-0.26 (0.25)
Ln GDP per capita	0.10 <sup>a</sup> (0.02)	0.049 <sup>b</sup> (0.02)	0.064 <sup>a</sup> (0.02)	21.43 <sup>a</sup> (7.08)	8.86 <sup>c</sup> (4.36)	10.32 <sup>b</sup> (4.61)	0.29 <sup>a</sup> (0.11)	0.17 (0.12)	0.18 (0.13)	1.22 <sup>a</sup> (0.27)	0.72 <sup>a</sup> (0.26)	0.70 <sup>b</sup> (0.28)
Observations	49	32	32	49	32	32	49	32	32	49	32	32
MSE	0.27	0.21	0.20	56.89	34.06	34.09	0.66	0.66	0.67	2.57	2.28	2.32
Adjusted R <sup>2</sup>	0.48	0.67	0.69	0.18	0.49	0.49	0.66	0.68	0.67	0.34	0.48	0.46

Panel B: Regulator's budget (extended sample) as predicting financial market size

	<u>Market Capitalization</u>			<u>Trading Volumes</u>			<u>Domestic Firms</u>			<u>IPOs</u>		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	Formal public enforcement	Staffing resources	Staffing and formal	Formal public enforcement	Staffing resources	Staffing and formal	Formal public enforcement	Staffing resources	Staffing and formal	Formal public enforcement	Staffing resources	Staffing and formal
Budget resources (extended)	–	0.0015 <sup>a</sup> (0.00)	0.0011 <sup>b</sup> (0.00)	–	0.23 <sup>a</sup> (0.07)	0.19 <sup>b</sup> (0.08)	–	0.0034 <sup>c</sup> (0.00)	0.00 (0.00)	–	0.010 <sup>b</sup> (0.00)	0.0091 <sup>c</sup> (0.00)
Formal public enforcement (LLS)	0.34 <sup>c</sup> (0.20)	–	0.46 <sup>b</sup> (0.20)	39.56 (30.01)	–	44.55 (41.97)	0.64 (0.48)	–	0.80 (0.78)	3.72 <sup>b</sup> (1.55)	–	1.59 -2.33
Anti-director rights Index	0.071 <sup>b</sup> (0.03)	0.097 <sup>a</sup> (0.03)	0.060 <sup>c</sup> (0.03)	7.86 (4.73)	12.12 <sup>b</sup> (5.28)	8.56 (5.55)	0.18 <sup>b</sup> (0.09)	0.14 (0.09)	0.07 (0.12)	0.31 (0.24)	0.64 (0.38)	0.51 (0.37)
Judicial efficiency	0.052 <sup>b</sup> (0.02)	0.00 (0.03)	0.03 (0.03)	-2.10 (5.42)	-7.07 <sup>c</sup> (3.98)	-5.27 (5.51)	0.26 <sup>a</sup> (0.08)	0.16 (0.09)	0.20 <sup>b</sup> (0.10)	0.04 (0.22)	-0.40 (0.30)	-0.31 (0.36)
Ln GDP per capita	0.10 <sup>a</sup> (0.02)	0.14 <sup>a</sup> (0.03)	0.14 <sup>a</sup> (0.03)	21.43 <sup>a</sup> (7.08)	26.06 <sup>a</sup> (5.29)	25.41 <sup>a</sup> (5.40)	0.29 <sup>a</sup> (0.11)	0.41 <sup>b</sup> (0.16)	0.40 <sup>b</sup> (0.16)	1.22 <sup>a</sup> (0.27)	1.57 <sup>a</sup> (0.39)	1.55 <sup>a</sup> (0.40)
Observations	49	28	28	49	28	28	49	28	28	49	28	28
MSE	0.27	0.23	0.21	56.89	35.19	34.80	0.66	0.72	0.72	2.57	2.63	2.67
Adjusted R <sup>2</sup>	0.48	0.61	0.66	0.18	0.48	0.49	0.66	0.64	0.64	0.34	0.34	0.32

a = significant at 1%; b = significant at 5%; and c = significant at 10 %.

Table 5  
Resource-based staffing variable in horse race with private enforcement (panels A and B)

In this table we present horse races between resource-based public enforcement and private enforcement variables. Panel A uses market capitalization as the outcome variable, Panel B trading volume/GDP as the outcome variable, Panel C the number of domestic firms, and Panel D IPOs. We run 9 models with each of the four outcome variables. Column 1 tests public enforcement alone, then we add disclosure in column 2, liability standards in column 3, and both disclosure and liability standards in column 4. Next we add a resource-based measure of public enforcement, the extended sample of the securities regulators' staffing levels. Columns 5 and 6 run the public enforcement variables, with resource-based enforcement alone first in column 5, and then with formal public enforcement, measured by the securities regulators' powers in column 6. Columns 7, 8, and 9 check for the robustness of the resource-based enforcement measure in horse races between public and private enforcement: i.e., the staff-based resources variable runs against disclosure in column 7, against liability in column 8, then against both private enforcement measures in column 9. We add three conditioning variables to each model: wealth, judicial efficiency and anti-directors rights. The resource-based measure is significant in 19 of the 20 models in which it appears; disclosure is significant without resource-based enforcement in the majority of runs, but insignificant in 8 out of the 12 models in which it runs against resource-based-enforcement. Liability standards often turn negative in the face of the resource-based measure of public enforcement. Robust standard errors are in parentheses; the constant is not reported.

*Panel A: Dependent Variable = Market Capitalization to GDP (2004)*

	<u>Without Extended Staffing Variable</u>				<u>With Extended Staffing Variable</u>				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Securities regulator staffing level (extended sample)					6.00 <sup>b</sup> (2.30)	6.17 <sup>b</sup> (2.57)	5.88 <sup>b</sup> (2.39)	6.39 <sup>b</sup> (2.33)	6.25 <sup>b</sup> (2.36)
Formal Public Enforcement	83.17 (69.10)	56.84 (71.62)	76.99 (77.82)	57.30 (77.97)		-15.15 (61.31)			
Disclosure		94.15 <sup>b</sup> (36.86)		94.80 <sup>b</sup> (37.14)			26.26 (38.77)		44.67 (35.65)
Liability Standards			19.54 (34.64)	-2.03 (37.03)				-52.12 (47.16)	-61.69 (45.62)
Anti-Directors Rights (rev.)	16.44 (10.98)	6.48 (11.27)	14.77 (12.16)	6.59 (12.17)	2.31 (9.10)	2.81 (9.34)	0.37 (9.62)	6.35 (10.44)	3.78 (10.52)
Judicial Efficiency	11.21 <sup>c</sup> (5.58)	9.65 <sup>c</sup> (5.17)	11.01 <sup>c</sup> (5.81)	9.66 <sup>c</sup> (5.32)	3.16 (4.80)	2.55 (5.44)	2.71 (4.75)	3.24 (4.79)	2.48 (4.84)
Wealth Control	22.68 <sup>b</sup> (9.83)	20.01 <sup>b</sup> (9.79)	21.68 <sup>b</sup> (10.60)	20.10 <sup>c</sup> (10.40)	-3.99 (10.37)	-4.73 (10.77)	-3.88 (10.30)	-3.21 (10.33)	-2.88 (10.15)
Observations	47	47	47	47	32	32	32	32	32
MSE	75.09	73.92	75.88	74.84	59.89	60.96	60.77	59.60	60.05
R Squared	0.33	0.36	0.33	0.36	0.66	0.66	0.66	0.67	0.68

*Panel B: Dependent Variable = Trading Volume to GDP (2004)*

	<u>Without Extended Staffing Variable</u>				<u>With Extended Staffing Variable</u>				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Securities regulator staffing level (extended sample)					2.41 <sup>b</sup> (1.14)	1.92 (1.35)	2.14 <sup>c</sup> (1.22)	2.41 <sup>c</sup> (1.25)	2.22 <sup>c</sup> (1.27)
Formal Public Enforcement	51.91 (35.49)	33.03 (33.83)	43.07 (36.58)	29.96 (35.63)		43.64 (45.71)			
Disclosure		67.52 <sup>a</sup> (24.78)		63.17 <sup>b</sup> (25.46)			56.16 <sup>c</sup> (29.06)		60.18 <sup>b</sup> (26.58)
Liability Standards			27.93 (25.94)	13.56 (26.72)				-0.59 (34.86)	-13.48 (32.19)
Anti-Directors Rights (rev.)	13.21 <sup>b</sup> (5.12)	6.07 (6.07)	10.82 <sup>c</sup> (6.17)	5.37 (6.60)	13.06 <sup>c</sup> (6.83)	11.64 (7.24)	8.90 (7.28)	13.10 (8.00)	9.64 (8.11)
Judicial Efficiency	4.55 (3.54)	3.43 (3.22)	4.26 (3.67)	3.36 (3.31)	0.21 (3.83)	1.99 (4.83)	-0.77 (3.35)	0.21 (3.89)	-0.81 (3.33)
Wealth Control	25.46 <sup>a</sup> (6.43)	23.55 <sup>a</sup> (6.09)	24.02 <sup>a</sup> (6.58)	22.97 <sup>a</sup> (6.22)	15.96 <sup>b</sup> (6.94)	18.12 <sup>b</sup> (7.17)	16.20 <sup>b</sup> (6.95)	15.97 <sup>b</sup> (7.01)	16.41 <sup>b</sup> (6.99)
Observations	47	47	47	47	32	32	32	32	32
MSE	47.33	46.20	47.50	46.68	42.00	41.93	41.09	42.80	41.77
R Squared	0.41	46.20	0.42	0.46	0.58	0.60	0.61	0.58	0.61

a = significant at 1%; b = significant at 5 %; and c = significant at 10 %.

Table 5  
Resource-based staffing variable in horse race with private enforcement (panels C and D)

*Panel C: Number of domestic firms/population (2004)*

	<u>Without Extended Staffing Variable</u>				<u>With Extended Staffing Variable</u>				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Securities regulator staffing level (extended sample)					0.04 <sup>a</sup> (0.01)	0.04 <sup>a</sup> (0.01)	0.03 <sup>a</sup> (0.01)	0.04 <sup>a</sup> (0.01)	0.03 <sup>a</sup> (0.01)
Formal Public Enforcement	0.61 (0.53)	0.26 (0.53)	0.37 (0.56)	0.14 (0.54)		-0.53 (0.83)			
Disclosure		1.24 <sup>c</sup> (0.74)		1.08 (0.80)			0.85 (0.75)		0.85 (0.81)
Liability Standards			0.77 (0.61)	0.53 (0.64)				0.20 (0.58)	0.01 (0.64)
Anti-Directors Rights (rev.)	0.26 <sup>c</sup> (0.13)	0.12 (0.18)	0.19 (0.15)	0.10 (0.18)	0.11 (0.20)	0.13 (0.21)	0.05 (0.23)	0.10 (0.21)	0.05 (0.23)
Judicial Efficiency	0.26 <sup>a</sup> (0.09)	0.24 <sup>a</sup> (0.08)	0.25 <sup>a</sup> (0.09)	0.24 <sup>a</sup> (0.08)	0.19 <sup>b</sup> (0.08)	0.17 (0.10)	0.17 <sup>b</sup> (0.08)	0.19 <sup>b</sup> (0.09)	0.17 <sup>b</sup> (0.08)
Wealth Control	0.56 <sup>a</sup> (0.17)	0.51 <sup>a</sup> (0.18)	0.52 <sup>a</sup> (0.17)	0.49 <sup>a</sup> (0.17)	0.39 <sup>c</sup> (0.23)	0.37 (0.22)	0.40 <sup>c</sup> (0.23)	0.39 <sup>c</sup> (0.23)	0.40 <sup>c</sup> (0.23)
Observations	48	48	48	48	32	32	32	32	32
MSE	0.81	0.79	0.80	0.79	0.85	0.86	0.85	0.87	0.87
R Squared	0.65	0.68	0.67	0.68	0.65	0.65	0.66	0.65	0.66

*Panel D: Dependent Variable = IPO value/GDP (2004)*

	<u>Without Extended Staffing Variable</u>				<u>With Extended Staffing Variable</u>				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Securities regulator staffing level (extended sample)					0.20 <sup>b</sup> (0.09)	0.25 <sup>b</sup> (0.10)	0.211 <sup>b</sup> (0.09)	0.24 <sup>a</sup> (0.08)	0.24 <sup>a</sup> (0.08)
Formal Public Enforcement	4.92 (3.15)	4.11 (2.80)	5.62 (3.58)	4.64 (3.12)		-4.51 (3.00)			
Disclosure		2.38 (2.85)		3.76 (3.38)			-2.26 (4.04)		0.33 (4.22)
Liability Standards			-1.97 (2.14)	-2.79 (2.55)				-4.77 <sup>b</sup> (2.15)	-4.85 <sup>c</sup> (2.51)
Anti-Directors Rights (rev.)	0.85 (0.79)	0.73 (0.75)	0.97 (0.86)	0.83 (0.80)	0.03 (0.68)	0.22 (0.56)	0.11 (0.57)	0.25 (0.71)	0.24 (0.65)
Judicial Efficiency	0.49 (0.30)	0.42 (0.34)	0.48 (0.31)	0.36 (0.37)	0.33 (0.33)	0.18 (0.29)	0.44 (0.50)	0.33 (0.35)	0.31 (0.53)
Wealth Control	1.01 (0.70)	1.04 (0.72)	1.15 (0.76)	1.26 (0.83)	0.61 (0.57)	0.94 (0.80)	0.86 (0.85)	0.80 (0.69)	0.77 (0.83)
Observations	36	36	36	36	24	24	24	24	24
MSE	3.72	3.76	3.75	3.77	3.12	3.06	3.18	2.94	3.03
R Squared	0.31	0.32	0.32	0.34	0.51	0.55	0.52	0.59	0.59

a = significant at 1%; b = significant at 5 %; and c = significant at 10 %.

Table 6

## Budget-based public enforcement in horse race with private enforcement

In this table, we again present horse races between resource-based public enforcement and private enforcement variables, in this table using the budget-based (extended sample) to measure the intensity of public resource-based enforcement. Panel A uses market capitalization as the outcome variable, Panel B trading volume/GDP as the outcome variable, Panel C the number of domestic firms, and Panel D IPOs. We run 5 models with each of the four outcome variables. Columns 1 and 2 run the public enforcement variables, with resource-based enforcement alone first in column 1, and then with formal public enforcement, as measured by the securities regulators' powers in column 2. Columns 1, 2, and 3 check for robustness of the resource-based enforcement in horse races between public and private enforcement: i.e., the budget-based resource variable runs against disclosure in column 3, then against liability in column 4, and then against both private enforcement measures in column 5. We add three conditioning variables to each model: wealth, judicial efficiency and anti-directors rights. The resource-based measure is significant in all twenty models, notably including those in column 5 with all of the private-oriented enforcement variables. Disclosure is often significant when run without resource-based enforcement, as seen in Table 5, but not always significant when run against the budget-based indicator of public enforcement intensity, as seen in column 3 and 5 of the four panels in this table. Liability standards turn negative in the face of the resource-based measure of public enforcement in 3 of the 8 models. Robust standard errors are in parentheses; the constant is not reported.

	Panel A					Panel B				
	<i>Dependent variable = Stock market capitalization/GDP</i>					<i>Dependent variable = Trading volume/GDP</i>				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Regulatory budget (extended sample)	0.80 <sup>a</sup> (0.28)	0.76 <sup>b</sup> (0.28)	0.79 <sup>b</sup> (0.29)	0.80 <sup>b</sup> (0.29)	0.79 <sup>b</sup> (0.30)	0.38 <sup>a</sup> (0.10)	0.32 <sup>a</sup> (0.10)	0.33 <sup>a</sup> (0.11)	0.37 <sup>a</sup> (0.11)	0.33 <sup>a</sup> (0.1115)
Formal public enforcement		47.45 (57.92)					62.19 (42.65)			
Disclosure			20.44 (39.04)		21.41 (44.96)			76.07 <sup>b</sup> (36.36)		73.70 <sup>b</sup> (32.70)
Liability Standards				1.09 (26.71)	-2.73 (30.08)				19.90 (30.46)	6.73 (27.46)
Anti-director rights (rev.)	17.72 (13.77)	15.79 (13.08)	16.71 (13.57)	17.63 (14.99)	16.87 (14.72)	21.85 <sup>a</sup> (7.26)	19.33 <sup>b</sup> (7.26)	18.09 <sup>b</sup> (7.18)	20.32 <sup>b</sup> (8.37)	17.69 <sup>b</sup> (7.92)
Judicial efficiency	-7.22 (7.64)	-4.97 (6.96)	-7.29 (7.89)	-7.23 (7.78)	-7.28 (8.04)	-5.21 (4.55)	-2.26 (5.23)	-5.48 (4.51)	-5.34 (4.65)	-5.52 (4.57)
Wealth control	64.97 <sup>b</sup> (24.47)	62.64 <sup>b</sup> (22.71)	63.93 <sup>b</sup> (25.19)	64.90 <sup>b</sup> (25.48)	64.04 <sup>b</sup> (26.07)	47.79 <sup>a</sup> (10.59)	44.74 <sup>a</sup> (9.46)	43.94 <sup>a</sup> (11.09)	46.64 <sup>a</sup> (11.03)	43.67 <sup>a</sup> (11.53)
Observations	28	28	28	28	28	28	28	28	28	28
MSE	62.41	62.90	63.68	63.82	65.18	41.06	39.52	39.11	41.61	39.99
R Squared	0.66	0.67	0.67	0.66	0.67	0.64	0.68	0.69	0.65	0.69
	Panel C					Panel D				
	<i>Dependent variable = Domestic firms/population</i>					<i>Dependent variable = IPOs/GDP</i>				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Regulatory budget (extended sample)	0.0049 <sup>b</sup> (0.002)	0.0049 <sup>b</sup> (0.002)	0.0043 <sup>b</sup> (0.002)	0.0047 <sup>b</sup> (0.002)	0.0043 <sup>b</sup> (0.002)	0.038 <sup>a</sup> (0.010)	0.045 <sup>a</sup> (0.011)	0.038 <sup>a</sup> (0.011)	0.041 <sup>a</sup> (0.008)	0.040 <sup>a</sup> (0.008)
Formal public enforcement		0.03 (0.82)					-4.21 (3.07)			
Disclosure			0.92 (0.96)		0.78 (1.03)			-0.70 (4.21)		1.54 (5.03)
Liability Standards				0.53 (0.60)	0.39 (0.63)				-3.2109 <sup>c</sup> (1.59)	-3.65 (2.29)
Anti-director rights (rev.)	0.24 (0.19)	0.24 (0.21)	0.19 (0.21)	0.20 (0.20)	0.17 (0.22)	0.17 (0.89)	0.32 (0.78)	0.19 (0.80)	0.31 (0.89)	0.28 (0.85)
Judicial efficiency	0.12 (0.09)	0.12 (0.11)	0.12 (0.08)	0.12 (0.09)	0.12 (0.09)	-0.01 (0.43)	-0.17 (0.39)	0.02 (0.61)	0.03 (0.44)	-0.03 (0.63)
Wealth control	0.83 <sup>a</sup> (0.22)	0.83 <sup>a</sup> (0.23)	0.78 <sup>a</sup> (0.24)	0.80 <sup>a</sup> (0.22)	0.77 <sup>a</sup> (0.24)	1.20 (0.80)	1.18 (0.82)	1.15 (0.99)	1.09 (0.74)	1.17 (0.98)
Observations	28	28	28	28	28	21	21	21	21	21
MSE	0.87	0.88	0.87	0.87	0.88	3.04	2.98	3.14	2.99	3.08
R Squared	0.66	0.66	0.68	0.67	0.68	0.60	0.64	0.60	0.64	0.64

a = significant at 1%; b = significant at 5 %; and c = significant at 10 %.

Table 7  
Distribution of coefficients on public enforcement and private enforcement variables, using robust regressions, in predicting four financial market size outcomes

In this table, we summarize the coefficients on the resource-based public and the private enforcement variables, with a view to testing for their robustness when influential observations are eliminated or dampened via STATA's *rreg* command. Columns 1-3 summarizes the nature of the coefficients in 24 full horse-race regressions that contain both resource-based and private enforcement variables. We use six resource-based samples, as outlined in Table 1 and whose data is listed in Table 2: We use three samples for each of a staff-based resource measure and a budget-based resource measure. For each of these two we have 3 sub-samples—basic, extended, and extrapolated—as described in Table 1. We test whether these resource-based variables predict four basic outcomes: stock market capitalization, trading volume, number of firms, and IPOs, yielding 24 regressions for the 6 samples and 4 outcomes. Columns 1-3 report the overall results for the 3 types of samples and column 4 summarizes those results: The resource-based measures of public enforcement are significant 92% of the time. The private enforcement variable for disclosure is largely significant (in 42% of the regressions) and always positive. The liability index does less well: never significant and negative more often than positive. Columns 5-8 summarize the effect that running STATA's *rreg* command (to clip or eliminate influential observations) has on the results. The resource-based and the disclosure variables hold up fairly well; the liability index does not. The resource-based measures of public enforcement persist as significant in two-thirds of the regressions and nearly always are positive. The disclosure index also persists as significant in nearly half of the regressions. The liability index continues to do less well.

	<u>Standard OLS regressions</u>				<u>Rreg regressions</u>				<u>Both OLS and rreg</u>
	Columns (1)-(4) report the nature of the coefficients in 24 OLS regressions, with 4 financial market size variables as the outcomes and the six resource-based public enforcement variables as the main independent variables				Columns (5)-(8) report the nature of the coefficients in 24 rreg regressions, with 4 financial market size variables as the outcomes and the six resource-based public enforcement variables as the main independent variables				Column (9) summarizes the nature of the coefficients in the 24 OLS and 24 rreg regressions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Coefficients for budget and staffing (basic)</i>	<i>Coefficients for budget and staffing (extended)</i>	<i>Coefficients for budget and staffing (extrapolated)</i>	<i>Subtotal of coefficients in all standard OLS regressions</i>	<i>Coefficients for budget and staffing (basic)</i>	<i>Coefficients for budget and staffing (extended)</i>	<i>Coefficients for budget and staffing (extrapolated)</i>	<i>Subtotal of coefficients in all rreg regressions</i>	<i>Summary of coefficients in all regressions, both standard OLS and rreg</i>
<b>Resource-based public enforcement</b>									
Coefficients that are both positive and significant	100%	100%	75%	92%	25%	25%	75%	42%	67%
Positive coefficients	100%	100%	100%	100%	100%	100%	88%	96%	98%
Negative coefficients	0%	0%	0%	0%	0%	0%	13%	4%	2%
Coefficients that are both negative and significant	0%	0%	0%	0%	0%	0%	0%	0%	0%
<b>Disclosure index</b>									
Coefficients that are both positive and significant	63%	25%	38%	42%	38%	38%	63%	46%	44%
Positive coefficients	100%	100%	100%	100%	100%	100%	100%	100%	100%
Negative coefficients	0%	0%	0%	0%	0%	0%	0%	0%	0%
Coefficients that are both negative and significant	0%	0%	0%	0%	0%	0%	0%	0%	0%
<b>Liability index</b>									
Coefficients that are both positive and significant	0%	0%	0%	0%	0%	0%	0%	0%	0%
Positive coefficients	25%	38%	50%	38%	50%	50%	75%	58%	48%
Negative coefficients	75%	63%	50%	63%	50%	50%	25%	42%	52%
Coefficients that are both negative and significant	38%	13%	13%	21%	13%	13%	25%	17%	19%
<b>Anti-director rights index (revised)</b>									
Coefficients that are both positive and significant	50%	13%	13%	25%	38%	50%	50%	46%	35%
Positive coefficients	100%	100%	100%	100%	100%	75%	100%	92%	96%
Negative coefficients	0%	0%	0%	0%	0%	25%	0%	8%	4%
Coefficients that are both negative and significant	0%	0%	0%	0%	0%	0%	0%	0%	0%
<b>Judicial efficiency index</b>									
Coefficients that are both positive and significant	13%	13%	25%	17%	25%	13%	25%	21%	19%
Positive coefficients	25%	50%	75%	50%	50%	50%	63%	54%	52%
Negative coefficients	75%	50%	25%	50%	50%	50%	38%	46%	48%
Coefficients that are both negative and significant	13%	0%	0%	4%	0%	0%	25%	8%	6%

Table 8  
Regressions with dependent variables associated with private control

In this table, we examine resource-based public enforcement and private enforcement measures' association with variables related to private control. (These variables were outlined and highlighted in LLS (2006). Columns 1 and 2 look at block premiums, a variable that seeks to measure the degree to which insiders can transfer value to themselves. Columns 3 and 4 measure access to equity. The outcome measured in 5 and 6 is ownership concentration. Columns 1, 3, and 5 use staffing (extended sample) as the resource-based measure of public enforcement. Columns 2, 4, and 6 use budget (extended sample.) Disclosure is significant across all six and in the expected direction. Public enforcement is erratic in this set of regressions. Robust standard errors are in parentheses; the constant and the wealth control are not reported.

	<u>Block Premia</u>		<u>Access to Equity</u>		<u>Ownership Concentration</u>	
	(1)	(2)	(3)	(4)	(5)	(6)
	Staff (extended) as resource- based measure	Budget (extended) as resource-based measure	Staff (extended) as resource- based measure	Budget (extended) as resource-based measure	Staff (extended) as resource-based measure	Budget (extended) as resource-based measure
Resource-based public enforcement	0.0007 (0.0013)	0.0002 (0.0002)	-0.0022 (0.0125)	0.0003 (0.0020)	0.004 <sup>b</sup> (0.0017)	0.0003 (0.0004)
Disclosure	-0.31 <sup>b</sup> (0.14)	-0.31 <sup>b</sup> (0.14)	1.49 <sup>a</sup> (0.52)	1.03 <sup>c</sup> (0.52)	-0.21 <sup>c</sup> (0.10)	-0.28 <sup>a</sup> (0.09)
Liability standards	-0.03 (0.06)	-0.04 (0.06)	0.91 (0.58)	0.95 (0.58)	-0.08 (0.06)	-0.03 (0.06)
Anti-director rights (rev.)	0.03 (0.04)	0.02 (0.04)	-0.19 <sup>c</sup> (0.09)	-0.19 (0.11)	-0.01 (0.01)	-0.01 (0.02)
Judicial efficiency	-0.01 (0.01)	0.00 (0.01)	0.13 (0.08)	0.10 (0.13)	-0.02 <sup>b</sup> (0.01)	-0.02 (0.01)
Observations	23	21	29	25	32	28
MSE	0.09	0.10	0.72	0.74	0.10	0.10
Adjusted R <sup>2</sup>	0.24	0.25	0.49	0.44	0.44	0.43

a = significant at 1%; b = significant at 5 %; and c = significant at 10 %.

Table 9  
Regressions with new World Bank indices as dependent variables

In this table we use the World Bank's indices on equity market quality and efficiency as the outcome variables. We regress the outcomes on resource-based public enforcement (via the extended staffing sample in columns 1, 3, 5, 7, and 9, and via the extended budget sample in columns 2, 4, 6, 8, and 10) and on the disclosure and liability private enforcement measures. The resource-based samples are significant in the expected direction in 4 of the 10 runs; disclosure is significant in the expected direction in 6 of 10. But neither has the expected sign (in one instance for each, significantly) in most remaining runs. Liability does less well. Robust standard errors are in parentheses; the constant and the wealth control are not reported.

	<u>Equity Index</u>		<u>Equity Market Size</u>		<u>Market Access</u>		<u>Market Efficiency</u>		<u>Market Stability</u>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Staff (extended) as resource- based measure	Budget (extended) as resource- based measure	Staff (extended) as resource- based measure	Budget (extended) as resource- based measure	Staff (extended) as resource- based measure	Budget (extended) as resource- based measure	Staff (extended) as resource- based measure	Budget (extended) as resource- based measure	Staff (extended) as resource- based measure	Budget (extended) as resource- based measure
Resource-based public enforcement	0.026 <sup>c</sup> (0.014)	0.003 <sup>c</sup> (0.002)	0.12 <sup>c</sup> (0.060)	0.02 <sup>a</sup> (0.006)	-0.0040 (0.016)	-0.0008 (0.002)	-0.0080 (0.018)	-0.004 <sup>c</sup> (0.002)	-0.0020 (0.008)	0.0008 (0.001)
Disclosure	1.01 <sup>b</sup> (0.42)	1.06 <sup>b</sup> (0.48)	3.07 <sup>a</sup> (0.94)	3.17 <sup>b</sup> (1.15)	2.05 <sup>b</sup> (0.82)	2.44 <sup>b</sup> (0.87)	-1.84 <sup>c</sup> (1.05)	-1.23 (1.12)	0.68 (0.75)	-0.15 (0.52)
Liability standards	0.14 (0.51)	0.44 (0.46)	-1.22 (1.33)	-0.12 (1.01)	0.32 (0.80)	0.38 (0.84)	0.75 (0.72)	0.71 (0.66)	0.82 <sup>c</sup> (0.41)	0.77 <sup>b</sup> (0.36)
Anti-directors rights (rev.)	0.11 (0.02)	0.16 (0.11)	0.26 (0.33)	0.47 (0.35)	0.18 (0.11)	0.08 (0.18)	0.25 (0.21)	0.18 (0.16)	-0.05 (0.10)	-0.08 (0.09)
Judicial efficiency	0.02 (0.06)	-0.07 (0.07)	-0.08 (0.17)	-0.43 <sup>c</sup> (0.23)	0.10 (0.11)	0.07 (0.12)	0.00 (0.11)	0.02 (0.12)	0.04 (0.05)	0.04 (0.06)
Observations	32	28	32	28	32	28	31	28	32	28
MSE	0.65	0.66	1.77	1.73	1.12	1.15	1.08	1.01	0.52	0.44
Adjusted R <sup>2</sup>	0.62	0.63	0.56	0.61	0.09	0.07	0.42	0.54	0.31	0.25

a = significant at 1%; b = significant at 5%; and c = significant at 10%.

Table 10  
Public enforcement and common law origins

This table regresses the resource-based enforcement variables, the formal public enforcement variable, and the private enforcement variables on a common law dummy. The disclosure and liability variables are significant, as prior work has indicated. But the common law dummy is significant as well for both the resource-based public enforcement and the formal powers enforcement variables, although these are not measures traditionally associated with common law institutions. The wealth control here is the World Bank 2005 index measuring the log of gross national income per capita adjusted for purchasing power parity, and log GDP for 2004, which we include to control for scale effects. Robust standard errors are in parentheses; the constant is not reported.

	<i>Resource-based public enforcement's association with common law</i>		<i>Private enforcement measures' association with common law</i>			<i>Public institutional measures' association with common law</i>	
	Securities regulator's staff/population (extrapolated)	Securities regulator's budget/GDP (extrapolated)	Disclosure	Liability	Anti-director rights (revised)	Judicial efficiency	Formal public enforcement powers (LLS)
Common law dummy	12.80 <sup>c</sup> (6.40)	60.80 <sup>c</sup> (30.62)	0.32 <sup>a</sup> (0.05)	0.21 <sup>a</sup> (0.06)	1.68 <sup>a</sup> (0.27)	1.27 <sup>a</sup> (0.44)	0.16 <sup>b</sup> (0.07)
Log GNI per capita (purchasing power parity, 2005)	25.10 <sup>c</sup> (13.19)	17.67 (32.56)	0.03 (0.03)	0.05 (0.03)	0.10 (0.15)	1.62 <sup>a</sup> (0.31)	-0.02 (0.04)
Log GDP (2004)	-12.81 (8.17)	-28.90 (17.34)	0.05 <sup>b</sup> (0.02)	0.03 (0.02)	0.15 (0.10)	-0.28 (0.17)	0.01 (0.03)
Observations	53	46	49	49	49	49	49
MSE	38.96	92.85	0.17	0.23	1.05	1.51	0.21
R-squared	0.27	0.21	0.52	0.23	0.40	0.49	0.14

a = significant at 1%; b = significant at 5%; and c = significant at 10%.

Table 11

Distribution of coefficients on common law dummy, after controlling for public and private enforcement measures

This table reports the residual value of the common law dummy, after one controls for public enforcement, as measured with the values reported in Table 2, and private enforcement, using the previously-used measures, indicated in Table 1. The outcomes are again the same market size measures: stock market capitalization, trading volume, number of domestic firms, and IPOs. This set of specifications yields 24 models for Panel A, and another 24 for Panel B. Panel A reports the standard OLS regressions, with the four financial market breadth variables as the outcomes. Column 1 reports the distribution of coefficients for the common law dummy, using the basic samples for staffing and for budget as the public enforcement measures. Column 2 does the same, but used the extended resource-based measures. Column 3 does the same for the extrapolated samples. Panel B repeats the process, using robust regressions. The last column summarizes the overall results. Overall, the residual effect of common law is rarely positive and significant, is often negative, and is occasionally negative and significant.

	<u>Panel A: Standard OLS Regressions</u>				<u>Panel B: RREG Regressions</u>				<u>Significance of common law dummy overall</u>
	<i>Basic samples</i> (1)	<i>Extended samples</i> (2)	<i>Extrapolated samples</i> (3)	<i>Subtotal</i> (4)	<i>Basic samples</i> (1)	<i>Extended samples</i> (2)	<i>Extrapolated samples</i> (3)	<i>Subtotal</i> (4)	
Common law dummy									
Coefficients that are both positive and significant	0%	0%	0%	0%	13%	0%	0%	4%	2%
Positive coefficients	25%	25%	25%	25%	63%	38%	38%	46%	35%
Negative coefficients	75%	75%	75%	75%	38%	63%	63%	54%	65%
Coefficients that are both negative and significant	25%	25%	0%	17%	13%	0%	13%	8%	13%

Table 12

Instrumental variable regressions: size of economy (via log GDP)

In this table we use the size of a nation's economy as an instrument for resource-based enforcement in predicting four basic financial outcomes. Panel B reports the first-stage regressions; Panel A reports the second-stage regressions. Panel A-1 examines the 2-stage results with stock market capitalization/GDP as the outcome; Panel A-2 examines trading volume/GDP; Panel A-3 examines the number of domestic firms; and Panel A-4 examines IPOs. Each reports OLS and 2-stage results, with the first set of columns reporting the OLS and 2-stage results when using regulatory staff/population (extended sample). The second set reports the OLS and 2-stage results when using regulatory budget/GDP (extended sample). Robust standard errors are in parentheses; constants are not reported.

<i>Panel A: Second-stage Regressions</i>																
	Panel A-1: Dependent variable = Stock market capitalization/GDP				Panel A-2: Dependent variable = Trading volume/GDP				Panel A-3: Dependent variable = Domestic firms/population				Panel A-4: Dependent variable = IPOs/GDP			
	Instrumented variable is regulatory staff/population (extended sample)		Instrumented variable is regulatory budget/GDP (extended sample)		Instrumented variable is regulatory staff/population (extended sample)		Instrumented variable is regulatory budget/GDP (extended sample)		Instrumented variable is regulatory staff/population (extended sample)		Instrumented variable is regulatory budget/GDP (extended sample)		Instrumented variable is regulatory staff/population (extended sample)		Instrumented variable is regulatory budget/GDP (extended sample)	
	OLS Regression	Ivreg2	OLS Regression	Ivreg2	OLS Regression	Ivreg2	OLS Regression	Ivreg2	OLS Regression	Ivreg2	OLS Regression	Ivreg2	OLS Regression	Ivreg2	OLS Regression	Ivreg2
Resource-based enforcement	6.25 <sup>b</sup> (2.36)	5.49 <sup>b</sup> (2.19)	0.79 <sup>b</sup> (0.03)	0.92 <sup>b</sup> (0.43)	2.22 <sup>c</sup> (1.27)	-1.29 (1.98)	0.33 <sup>b</sup> (0.11)	-0.2436 (0.41)	0.03 <sup>a</sup> (0.01)	0.10 <sup>b</sup> (0.04)	0.0043 <sup>b</sup> (0.00)	0.017 <sup>c</sup> (0.01)	0.24 <sup>a</sup> (0.08)	0.23 <sup>b</sup> (0.11)	0.04 <sup>a</sup> (0.01)	0.040 <sup>a</sup> (0.02)
Observations	32	32	28	28	32	32	28	28	32	32	28	28	24	24	21	21
Cragg-Donald Wald F Statistic	n.a.	7.613	n.a.	22.743	n.a.	7.613	n.a.	2.743	n.a.	7.613	n.a.	2.743	n.a.	6.592	n.a.	6.372
<i>Panel B: First-stage Regressions</i>																
	Extended Staff	Extended Budget	Extended Staff	Extended Budget	Extended Staff	Extended Budget	Extended Staff	Extended Budget	Extended Staff	Extended Budget	Extended Staff	Extended Budget	Extended Staff	Extended Budget		
Log GDP in 2005	-3.69 <sup>b</sup> (1.34)	-21.49 (12.98)	-3.69 <sup>b</sup> (1.34)	-21.49 (12.98)	-3.69 <sup>b</sup> (1.34)	-21.49 (12.98)	-3.69 <sup>b</sup> (1.34)	-21.49 (12.98)	-3.69 <sup>b</sup> (1.34)	-21.49 (12.98)	-4.04 <sup>b</sup> (1.59)	-30.48 <sup>b</sup> (12.07)	-4.04 <sup>b</sup> (1.59)	-30.48 <sup>b</sup> (12.07)		
Uncentered R Squared	0.79	0.72	0.79	0.72	0.79	0.72	0.79	0.72	0.79	0.72	0.83	0.75	0.83	0.75		
F-Test for Excluded Instruments	7.61	2.74	7.61	2.74	7.61	2.74	7.61	2.74	7.61	2.74	6.44	6.37	6.44	6.37		
a = significant at 1%; b = significant at 5%; and c = significant at 10 %. * = Exceeds Stock Yogo Weak ID Test critical values at 15% maximal IV size.																