

**Public Enforcement of Securities Laws:
Preliminary Evidence**

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Abstract

The consequence of economic actors ignoring their legal obligations, such as laws that protect outside investors in firms, is a recurring issue. Recent work in finance examines the relative importance for investor protection of private enforcement 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. Much legal scholarship has seen private enforcement of securities laws 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 the securities regulators' staffing levels and budgets. We then examine financial outcomes around the world — such as stock market capitalization, trading volume, number of domestic firms and number of IPOs — in light of these measures of public enforcement and find that more intense public enforcement regularly correlates with strong financial outcomes. Moreover, in horse races between our measures of public enforcement and the usual measures of private enforcement, public enforcement is typically at least as important as private enforcement in explaining important financial market outcomes around the world. Hence, we caution against using the current explanations that rely on the strength of private enforcement to the exclusion of public enforcement in making public policy around the world. We conclude by speculating why public enforcement may well prove to be as, or even more, important than private enforcement in explaining world-wide financial outcomes.

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Introduction	1
I. Public and Private Enforcement?.....	3
A. Private Remedies	4
B. Public Enforcement.....	6
C. Measuring Public Enforcement Inputs: Staffing and Budgets.....	7
II. Results.....	11
A. Public Enforcement and Market Size.....	11
B. Prior Public Enforcement Measures.....	13
C. Limitations to Prior Public Enforcement Indices	14
D. Staffing and Budgets vs. Prior Public Enforcement Measures	15
E. Financial Variables Associated with Dispersed Ownership	16
F. Limits to Both Private and Public Enforcement: Intermediate Financial Variables.....	18
G. Legal Origin and Regulatory Intensity.....	19
H. The Direction of Causality?	21
III. Discussion.....	24
A. Channels from Public Enforcement to Financial Outcomes.....	24
B. The Potential Importance of Public Enforcement	25
C. Developing Better Measures of Public Enforcement	26
D. Public Enforcement and Private Enforcement	26
Conclusion	27
Figure 1: Securities Regulators per Million of Population.....	10
Figure 2: Securities Budgets per Billion of U.S. Dollars of GDP	10
Figure 3: Civil versus Common Law Staffing	19
Figure 4: Civil versus Common Law Budgets.....	20
Table 1: Securities Enforcement Variables.....	30
Table 2: Summary Statistics on Public Enforcement Variables.....	31
Table 3: Pairwise Correlation Matrix for Key Enforcement Variables.....	32
Table 4: New Enforcement Variables and the Size of Capital Markets.....	33
Table 5: New Public Enforcement Variables Versus Prior Indices of Public Enforcement.....	34
Table 6: Regressions with Key Private Enforcement Variables.....	35
Table 7: Regressions with Dependent Variables Associated with Private Control	36
Table 8: Regressions with New World Bank Indices as Dependent Variables	36
Table 9: Public Enforcement and Common Law Origins	37
Table 10: Nested Regressions: Dependent Variables Equals 2004 Market Capitalization to GDP	38
Table 11: Instrumental Variable Regression (via Legal Origin)	39
Table 12: Instrumental Variable Regressions: Size of Economy (via Log GDP)	40

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INTRODUCTION

We evaluate here the relative importance 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 seen public enforcement of securities laws to be unimportant, while promoting private enforcement as central, World Bank (2006: 1), a conclusion we see as reached too hastily. In the academic literature, Djankov et al. (2005) and La Porta, Lopez-de-Silanes & Shleifer (2006) highlighted the importance of evaluating private versus public enforcement of securities obligations that protect investors.

In principle, both enforcement mechanisms could have serious defects and important advantages. On the defect side: A public enforcement system is degraded because public actors typically often have weak incentives to do their jobs well and often have poor information about 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 penalties on wrongdoers. On the positive side, public enforcement could come from public-regarding policymakers who can invoke sharp criminal 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.

Because there is no obvious a priori winner, we evaluate whether public enforcement is in practice valuable by using a new data-set of the actual level of resources devoted around the world on securities market oversight — measured both in terms of staffing levels of the securities regulator and their budgets. Prior studies relied on indices based on the formal powers of regulatory officials to measure public enforcement, a measure that seems less appropriate than money and people.

We first, in Part I, discuss perspectives from the legal academic literature 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

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so to suggest how and why public enforcement could protect investors in ways 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 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 may enhance the efficacy of private enforcement. We then introduce our new measures of public enforcement based on the budgets and staffing levels of securities market regulatory and enforcement agencies.

In Part II we 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. Indeed, our public enforcement variables' relationship to stock market development is robust to private-law-oriented legal factors, such as disclosure requirements and liability rules, which prior work saw to be critical to developing and maintaining strong securities markets. 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, these new measures of public enforcement are more strongly associated with robust financial markets.

The data do not lead us to conclude that public enforcement 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 seems 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 possible — indeed likely — 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 hard and, indeed, 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. And even if it's wide financial markets that call forth public enforcement, we'd like to know whether the better enforcement significantly further improves financial markets. While the finance literature has not yet settled on suitable technique here — the accepted technique is to use instrumental variables but finding a suitable instrument has vexed finance for a decade — and while our findings are only tentative, our

findings on directionality running at least in part from enforcement inputs to finance outcomes are no weaker than those previously used in this area of finance work.

In Part III, we discuss the channels through which public enforcement could improve financial markets and add cautionary notes for policymakers. Public enforcement strength can indicate the degree to which the public authority can (a) conduct market surveillance, by looking for general systemic issues and problems that need rule-making fixes, (b) investigate individual firms for wrong-doing, (c) bring enforcement actions (including those that result in large financial penalties and even prison terms), (d) regulate or even stop individual securities offerings, and (e) 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 enforcement, especially that connected with “tunneling” of value out from the firm into controllers’ hands and similar related party transactions. “Tunneling” and related party transactions are important and deserve the attention they have received. But deep public markets require brokers and others securities-handling institutions that are also sharply regulated, at least in the United States. The “trading” channel has not been the focus of analysis in the law and finance writing of the past decade, yet a reliable trading channel is needed for a strong securities market. This may be a main channel from public enforcement to deep securities markets.

Our concluding note is cautionary: 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). Our findings are 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?

What makes corporate actors play by the rules?

One possibility that surely is important is that wronged investors can sue insiders for their wrongdoing and recover damages. This remedy can be of obvious importance as information on wrong-doing is usually most strongly held by the wronged, who typically have strong incentives to sue the wrong-doer. Another possibility is that public enforcement controls wrong-doing private actors in firms, via good rules, with fines, by enlisting gate-keepers (like accountants) who examine financial transactions, and even by using 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 explored extensively in the legal literature but less deeply explored in the finance literature. There's considerable legal academic support for the view that the efficacy of many, perhaps most, private lawsuits is seriously compromised. They (a) provide meager returns to wronged plaintiffs, (b) do not visit their costs on the wrong-doing actors inside public firms, because the wrong-doers can usually transfer the costs to others, and (c) 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 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 corporations 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." Such results are obviously not conducive to effective deterrence and strong, effective private enforcement.

Moreover, while the total amount of damage done by the insider may be enormous, the portion affecting any single 100-share shareholder is small, of a size insufficient to motivate the shareholder to sue the wrong-doers. That is, in dispersed stock markets dispersed shareholders have limited incentives and information by which they can monitor and control insiders. Those structural limits give insiders more freedom to maneuver, in ways that we might hope private litigation would correct. But those limits on small shareholders' market-based monitoring *also* can disable them from efficaciously suing 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 with limited time and energy spent by the lawyers. Coffee (2006). Shareholders' financial recovery is of less direct interest to the lawyers, and lawyers usually control the litigation. Because the lawyers often control the lawsuit's progress — due to no shareholder typically being large enough to have a strong financial interest — this problem is not small.

Nor is it clear that our system of private litigation in the United States consistently imposes the right level of sanctions. To begin with — and perhaps as a result of the skewed incentives of plaintiffs counsel, see Coffee (2006); Romano (2005) — much private litigation results in meager settlements. Meager settlements suggest low deterrence. Even when larger settlements are reached, they do not often

impose large financial costs on directors or managers, typically the relevant actors.¹ It's often the firm that pays. If it's the firm that pays and not the wrong-doing agents who pay, it's the firm's *shareholders* who ultimately write check for the insiders' wrongdoing. Yet the firm's *shareholders* are often innocent of wrong-doing. They're *not* the wrong-doing insiders. This shift in damages from wrong-doer to innocent shareholder often occurs in American corporate and securities litigation, because indemnification arrangements often have the firm paying off the expenses and lawsuits against wrong-doing managers and because insurance policies (the so-called D&O, directors' and officers', policies) often have insurers picking up the tab and the firm (i.e., the shareholders ultimately) paying the insurance premiums. To be sure, wrong-doing insiders cannot shift all the costs to outside shareholders. And, even when they can shift the costs, 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 view that private corporate and securities litigation *as actually enforced* is well-constructed. Coffee (2006) and Kraakman, Park & Shavell (1994) examine these issues.

There's another basis from the legal literature to be skeptical that private enforcement fully dominates public enforcement. Recent legal scholarship has focused on the fact 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 wrong-doers. *Id.* Third, even when there is no contemporaneous public enforcement action in the specific instance of wrong-doing, private plaintiffs in the United States as well as investors benefit from the well-developed system of disclosure rules and legal requirements that the SEC and other regulators developed over many decades. This ongoing system works in part because public authorities enforce disclosure rules via penalties and ongoing market surveillance.² And the other main mechanism of private enforcement — the market's pricing reaction to the disclosure of information — depends on adequate public enforcement activity. Karpoff et al. (2006). Even private enforcement of federal securities laws is inexorably linked to the public regulatory system.

Moreover, economic theory indicates general differences in private and public enforcement. Polinsky and Shavell (2000: 46) tell us that when the harmed 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: when victims cannot identify the culprit, public enforcement in theory should outperform private enforcement. For insider trading — which can obviously harm securities markets —

¹ To be sure here, it's possible that a system that perfectly deters wrong-doing exhibits no large financial penalties, because the actors internalize the threat and do little wrong. The actual effects of the larger lawsuits — with the penalties being visited upon actors other than the wrong-doers — undermines this possibility.

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

the victim typically cannot identify of the secret insider who's trading on nonpublic information. See Bhattacharya and Daouk (2006: 4) for analysis of the enforcement problem here. Similarly, the standard economics of free-riding and collective action problems tells us the 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 here (Coates (2006)) — play key roles 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 should locate 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 suggest that as much as forty to sixty percent of the cost of all private litigation in the United States is dissipated through litigation costs as opposed to payments to injured parties. Shavell (2004). There seems to be an economy of scale in private lawsuits: the focus for most securities litigation is 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 a private lawsuit. 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, the substantial legal scholarship here suggests to us that purely (or even mostly) private enforcement of securities laws is an imperfect tool for policing capital markets. At least within the American financial regulatory system — where the issue has been most closely studied — private litigation is often misdirected, its useful elements build on public enforcement efforts, and often the two, public and private enforcement, are inexorably intertwined. Even though public enforcement has limitations, and those limitations are 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 begin with a prior belief that government actors here have perfect — or even good — incentives, nor do we begin with the view that government actors usually — or even often — have enough knowledge to enforce effectively securities laws via public means. But we also do not start with the 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 a different set of debilities. Public agents are not directly rewarded for building better capital markets. They are appointed by politicians beholden to an electorate that is itself poorly informed about financial markets. Regulatory authorities may primarily seek favorable media coverage, for its own sake, to promote their careers, or to enhance their success in budget negotiations. And interest groups may capture their regulatory authorities, leading the authorities to favor private interests over the public interest.

Still, existing evidence indicates that public enforcement has a role to play. Bhattacharya and Daouk (2002) 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 we described above, many aspects of effective private enforcement depend on the existence of competent and rigorous public oversight.

* * *

To be sure here, the enforcement problems afflicting developing nations differ from those afflicting developed ones. In developing nations, statist, corrupt regulators who use their public power to extract bribes and to punish political opponents will not 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 a nation is structurally more efficacious than public regulations: one would expect both the judiciary and the regulators to be corrupt and ineffective. The problem there is to improve public administration, not that one governmental arm is intrinsically superior to another. 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.

* * *

Hence, when we compare private to public enforcement, we are dealing with 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 the other.

C. Measuring Public Enforcement Inputs: Staffing and Budgets

As a prelude to exploring in greater detail the relationship between public enforcement and robust capital markets, we develop in this paper several measures of the intensity of public enforcement of securities regulation based on budgetary

resources and staffing levels. Our 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 wrong-doing, to write its rules carefully, to conduct market surveillance and review filings, and to act more often to remedy, fine, and prevent wrong-doing. 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 wrong-doers. Independence and authority could be 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. Simply because budgetary authorities have allocated regulatory authorities ample resources does not mean that the regulator deploys these resources wisely or (in the extreme) 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 utilize those resources to bring enforcement actions, to write good rules, and to hire good people. Deep staffing of sinecures or crony-oriented appointees may show a high budget and high staffing levels, but result in nothing being done to enforce securities regulation. Staffing and budget are enforcement inputs, not outputs. Indeed we have found examples of jurisdictions maintaining relatively similar regulatory staffing levels and budgets, but imposing quite different levels of public sanctions. Moreover, lean staffing and 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, they may still help to give us a 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.

Uncovering reliable budget and staffing data comparable across nations has not been an easy task. See Jackson (2005). Many jurisdictions divide regulatory responsibility among different bodies, with some bodies operating as self-regulatory organizations and others located within sub-national 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.³ Another problem peculiar to budgets is that some jurisdictions include in the budgetary aggregates various fees and taxes some of which may be passed on to other

³ Another complexity, which we have considered but not incorporated into our analysis, is whether some adjustment for economies of scale should not be factored into our measures of budgets and staffing. We simply normalize staffing levels by national populations and budgets by GDP at current exchange rates.

government agencies whereas other jurisdictions report only actual expenditures for supervisory activities.

Notwithstanding these difficulties, we extended Jackson (2005) by assembling three variables measuring actual supervisory resources deployed in a sample of jurisdictions. The underlying data is drawn from the 2006 edition of *How Countries Supervise their Banks, Insurers and Securities Markets*, which the Central Banking Publications of London compiles annually.⁴ Our first variable, covering 30 countries, is limited to jurisdictions that report separate personnel and budget data for securities regulatory bodies and measures staffing levels per million of domestic population. The second variable covers a larger number of jurisdictions (53), adding jurisdictions with consolidated financial supervisory bodies that do not report separate data for securities regulation. To obtain these additional observations, we extrapolated securities staffing by using the ratio of securities staffing to staffs of other financial regulatory functions for those jurisdictions for which we could break out this ratio.⁵

Our third variable is the ratio of regulatory budgets to national GDP, for which we have 41 observations. We again extrapolate the budgets of consolidated supervisory bodies. Because the Central Banking Publications data is less complete for budgets than for staffing, and because some of its budget data was obviously incorrect, we have fewer observations for the budget variable than for the staffing variable. The jurisdictions omitted from our extrapolated budget variable tended to be from less developed countries, where collecting accurate data is more difficult. However, the quality of the variable is probably higher than the extrapolated staffing variable as a result of these omissions. Table 1 reports our three new public enforcement variables along with two formal indices of public enforcement powers developed in La Porta et al. (2006) and Djankov et al. (2005) plus a dummy indicating whether the jurisdiction in question is a common law jurisdiction. Table 2 reports summary statistics for our variables.

The values of our enforcement variables vary considerably, as Figures 1 (reporting direct staffing levels) and 2 (reporting extrapolated budget levels) illustrate.⁶ 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 allocate the highest staffing and budget, and they are jurisdictions where financial activity plays a disproportionate role in the domestic economy. But even if one looks to more traditional economies, there is ample variation. Canada reports nearly 39 regulators staffing their securities agency per million of population whereas Spain with a

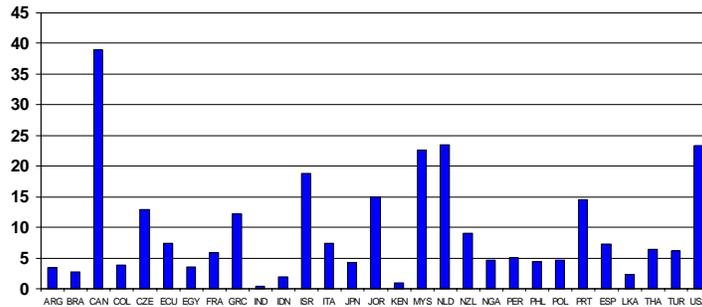
⁴ In prior work, Jackson (2005) combined data from an earlier edition of this publication with other data sources. While this approach allowed him to correct data where more authoritative sources were available, it introduced the possibility of bias, particularly when the more authoritative data is from common law jurisdictions. For this paper, we relied on a single data source.

⁵ Specifically, we extrapolated securities staffing (and also budgets) using the median ratios in jurisdictions where we could observe the actual ratios.

⁶ In these figures (but not the regressions), we excluded Hong Kong and Luxembourg, which as financial centers have unusually high staffing and budget ratios. We did so to more clearly convey the degree of variation in the intensity of regulatory inputs in other jurisdictions in the sample.

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 terms of regulatory budgets, the United Kingdom expends \$65.5 thousand U.S. dollars per billion of GDP while Germany — again a comparably-sized economy — spends less than half that amount, only \$22.2 thousand per billion of GDP.

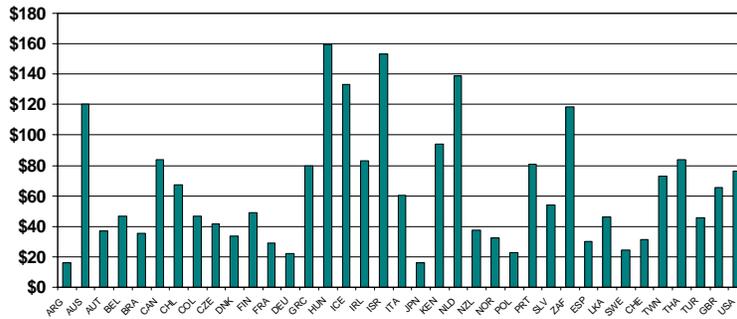
Figure One
Securities Regulators (Pure)
Per Million of Population



Authors’ compilations from How Countries Supervise Their Banks, Insurers and Securities Markets 2006 (Central Banking Publications).

Figure Two
Securities Budgets (Extrapolated)
Per Billion Dollars of GDP

(‘000s of U.S. Dollars based on 12/31/05 Exchange Rates)



Authors’ compilations from How Countries Supervise Their Banks, Insurers and Securities Markets 2006 (Central Banking Publications).

In Table 3, we report a matrix of pair-wise correlation coefficients for both our new enforcement variables and several of the legal indices commonly used in the

finance literature during the past decade. These indices indicate the relative strength 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 — direct staffing is at 0.39, extrapolated staffing at 0.40 and extrapolated budgets at 0.43. Accordingly, some countries must be adopting formal rules that could facilitate enforcement, but then do not allocate enough staffing and budget resources to make the regulator effective. France illustrates this phenomenon. It has a strong formal powers index score of 0.77, more than a standard deviation *above* the mean. But its regulatory budget is only \$22.2 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 would appear to be high on public enforcement, but in our view it should be coded as *low* on public enforcement. In contrast, the Netherlands has a slightly below average formal public enforcement index, but if we use real resources allocations it ranks a standard deviation *above* the mean.

The correlation matrix in Table 3 also reports the relationship between staffing and budget to other commonly used indices of interest, such as a securities disclosure index, a liability index, and a judicial efficiency index. We also include two other indices of public enforcement, based on formal powers.⁷ In general and as would be predicted, our variables are positively correlated with these other indices. This opens up the possibility — which we do not pursue here — that some of the good financial results associated with these three legal variables is due to their correlation with the intensity of regulatory enforcement.

II. RESULTS

We begin by analyzing how our enforcement variables relate to standard measures of capital market size. The two correlate significantly. Our public enforcement variables strongly and significantly predict the size of domestic capital markets. We then examine whether the public enforcement variable is robust to measures of private enforcement. It is. Our measures of public enforcement also have a consistently stronger relationship to capital market size than do formal indices of public enforcement powers utilized in recent prior academic work. We then examine the relationship between our public enforcement variables and several intermediate measures of financial development, such as dispersed ownership and stock market efficiency and stability. Finally, we explore the relationship between legal origins and our measures of actual resources allocated to public enforcement activity.

A. Public Enforcement and Market Size

Table 4 reports regressions exploring the relationship between common

⁷ Curiously, the two indices of formal enforcement powers do not correlate with one another. The lack of consistency further suggests that real resources may measure public enforcement better than formal powers.

indicators of capital market size — ratios of market capitalization to GDP, trading volumes to GDP, the logarithm of the ratio of the number of listed domestic firms to million of population, and the ratio of annual capital raised through IPOs to GDP.⁸ As controls, we include the two indices for securities disclosure standards and securities liability standards, as well as a wealth control, here the log of gross national income per capita in 2005 adjusted on the basis of purchasing power parity. We note statistical significance in the ordinary ways at the one percent, five percent and ten percent levels and report robust standard errors in parentheses. As the main focus of our inquiry is whether our public enforcement variables are associated with robust capital markets, we highlight all instances of statistical significance for these variables.

For at least two of our enforcement variables — the direct, non-extrapolated staffing variable and the extrapolated budget variable — there's a strong relationship between capital market size and the intensity of public enforcement. The estimated coefficients for these variables are statistically significant at the standard levels for all four of the regressions for our direct staffing variable and in three of the four regressions for the extrapolated variable. Moreover, the apparent economic impact is quite large. For example, the average ratio of market capitalization to GDP in the sample is slightly under 84 percent. The coefficients for direct staffing and extrapolated budgets imply that an increase of one standard deviation in the level of an average country's staffing or budget would be associated with an increase of market capitalization to GDP to over 150 percent. By way of comparison, 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 to roughly 100 percent.

While the regressions presented in Table 4 suggest a strong relationship between market size and two of our variables, our third variable — extrapolated staffing to population — does somewhat less well, though even that variable has a statistically significant coefficient in two of four regressions. Several factors could explain why the extrapolated staffing variable performs less robustly. First, as we discussed above, it likely has more measurement error than our other two variables given the relatively large number of developing countries represented. We also might be seeing a sampling effect. The relationship between enforcement intensity and stock market size could be more pronounced in more developed countries.

By way of comparison, the private enforcement variables in Table 4 have a less consistent relationship to the four measures of market size used as dependent variables. The disclosure index has a statistically significant coefficient in only half of the equations, and the liability standard index never has a statistically significant positive coefficient, and surprisingly often has a negative coefficient, which is sometimes statistically significant. In these regressions, public enforcement is much more closely associated with strong capital markets than private enforcement.

⁸ Our data for these variables and come from the World Bank Database. For definitions and other information regarding the World Bank data, see World Bank (2006b).

B. Prior Public Enforcement Measures

The results reported in the preceding section cast some doubt on the results La Porta et al. (2006: 7-9) recently obtained. In that paper, the authors developed a public enforcement index based on formal characteristics of financial supervisory in 49 jurisdictions. Their index assigns values based on the financial supervisor's independence from the executive agency, its investigative powers, its capacity to issue remedial orders, and the range of criminal sanctions available, then adds these values up to reach an index of what they measure as public enforcement.⁹ Their paper then compares the relationship between this public enforcement index and indicia of capital markets development and legal development, such as a disclosure index, an index of liability standards, an index of anti-directors rights and index of judicial efficiency.

The heart of their analysis is a series of regressions, which lead the authors to the following principal findings:

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).

The authors then skeptically conclude that all of the evidence indicates that the overall value of public enforcement particularly in developing countries is low: "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. But much of the evidence they report seems 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." LaPorta et al. (2006: 2). Hence, we cannot concur with them that their data supports their conclusion that "all the evidence" points to the

⁹ 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 wrong-doer]; and (4) [a] jail sentence for" the principal wrong-doer. 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 new index generated different results. It did not.

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.

If their normative intention is simply that nations should not scuttle private litigation, we would agree. But if their intention is to read their evidence that nations should scuttle their mechanisms of public enforcement or that their evidence supports concluding that nations seeking to build financial markets should favor private over public enforcement — or that the evidence shows one thus far to be demonstrably more efficacious than the other — we cannot agree. Neither our evidence nor, we believe, even theirs supports such a conclusion. Yet, their conclusion — without their ambiguous evidence — has found its way into the World Bank's (2006) important policy advice, which we cited above. And, with the spending and staffing variables we introduce here, we must report the contrary: in fact, there's considerable evidence linking public enforcement intensity to securities market breadth.

C. Limitations to Prior Public Enforcement Indices

The relationship between public enforcement and robust capital markets may turn on how the researcher measures public enforcement. While the LLS public enforcement index usefully summarizes various formal aspects of regulatory policy, it falls short of reliably indicating the efficacy of public enforcement. To begin with, whether all of the factors included within their public enforcement index are actually associated with higher quality regulation is subject to question. They give more index points to an independent securities oversight body, like the American Securities Exchange Commission, as opposed to one integrated into a consolidated financial supervisor, like the British Financial Services Authority. While regulatory specialization may be useful, the wider field of regulatory vision afforded by a consolidated regulator could actually improve supervisory oversight. The literature is divided regarding the relative merits of consolidated supervision. Čihák & Podpiera (2006). Hence, we are not fully comfortable with their conclusion that independence trumps consolidation. It may be the contrary, or each may have offsetting costs and benefits.

A more fundamental problem with this 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 make criminal referrals for securities law violations. While this may seem a plausible proxy for the intensity of public law enforcement, recent studies show that many jurisdictions with precisely these powers rarely criminally prosecute securities cases. See, e.g., Jackson (2006). And, although the scope of legal authority is no doubt an important consideration, more important is the *actual* practice of the supervisory staff. As recent research into the efficacy of insider trading regulation has demonstrated (Bhattacharya & Daouk (2002, 2006)), the key issue is not whether a regulatory body has the *formal* power to sanction offending parties, but whether they have *actually* exercised that power. Without evidence linking formal powers to actual practices, many of the elements of LLS's public enforcement index are of dubious

value. Some agencies with formal powers may use them, some may not, perhaps because they lack the budget and staffing resources to make their authority bite. Some agencies lacking a particular power may have an adequate, even if slightly inferior substitute, but have the budget and staffing resources to make good use of its (slightly weaker) formal authority.

D. Staffing and Budgets vs. Prior Public Enforcement Measures

To better understand the relative explanatory power of our new enforcement variables as compared with previously developed indices of public enforcement powers, we ran regressions using both. As before, we used dependent variables reflecting stock market capitalization, trading volumes, number of domestic firms, and amount of capital raised through IPOs. We report these results in Table 5. These regressions are analogous to those originally La Porta (2006) reported.¹⁰ In Panel A, we examined the effect of adding our new public enforcement variables along side a formal public enforcement index; in Panel B, we substituted our variables for the formal index. Within each panel, for each dependent variable, we first reproduce the original LLS regression using a formal enforcement index as an explanatory variable and then add in first our direct staffing variable and next our extrapolated budget variable. While the previously reported regressions are based on a sample including 49 observations, regressions with our direct staffing variable typically include 28 observations and regressions with our extrapolated budget variable include 35 observations.

Consider first the market capitalization columns of Panel A. Previous analysis reported that a statistically significant coefficient for the formal enforcement index (at the ten percent level) when regressed on market capitalization dependent variable; also statistically significant in this regression were indices for judicial efficiency and anti-directors rights and a wealth control. When we added our direct, nonextrapolated, staffing variable to the regression, the formal public enforcement index was no longer statistically significant but our staffing variable was, at the one percent level.

The significance of the private enforcement indices also declined in our reformulated regressions. Look again to Panel A. When we added our extrapolated budget variable to the market-capitalization regression, the extrapolated budget variable was significant (at the five percent level) and the formal public enforcement index again lost significance. As was true in many of the regressions in Table Five, the regressions with our direct staffing variable have somewhat lower mean square errors and somewhat higher adjusted R^2 's than did either the original LLS formulation or regressions using our extrapolated budget variables. This may due to our direct staffing variable — limited primarily to jurisdictions with specialized securities regulations — being a less noisy measure of regulatory intensity than is our

¹⁰ The dependent variables in Table 5 are based on the same definitions used for the dependent variables in Table 4, however, the data is based on averages from 1999 to 2000. Aside from our new public enforcement variables, the other explanatory variables are all from La Porta (2006), and the regressions marked “Original LLS” match results reported in La Porta (2006).

extrapolated budget variable, which estimates how budgetary resources are allocated in consolidated agencies. Or, conceivably, there is some sampling effect related to distinctive characteristics of jurisdictions that maintain separate agencies to oversee capital markets for which direct staffing numbers are available.¹¹ Or we could be observing a result caused by the broader sampling of countries included in the extrapolated budget variable. But whatever the reason, the fit of regressions using our direct staffing variable tended to be better than the fit for regressions with our extrapolated budget variable.

The results we present in Table 5 strongly suggest that our key enforcement variables are more closely associated with robust capital markets than is public enforcement indices based on formal agency characteristics. Consider Panel A of Table 5. When we run our enforcement variables head to head with the formal public enforcement index, our variables were significant in eight of eight cases, whereas the formal public enforcement index was significant only once (at the ten percent level). Moreover, in two cases where the dependent variable is the number of domestic firms, the coefficient for the formal index shifts so as to suggest an odd negative relationship between the older formal regulatory authority index and the number of firms. The oddness of the shift suggests a weakness in whether formal authority actually measures regulatory effectiveness. In Panel B, where we run the public enforcement variables independently, our variables again have statistically significant coefficients in all eight cases (more than half at the one percent level) whereas the formal public enforcement index is significant only half the time and then only at the five or ten percent levels.

We next see whether our enforcement variables retain statistical significance when run in regressions including the two other principal legal indices of the efficacy of private enforcement: securities law disclosure index and securities liability standards index. The results appear in Table 6. As we did for Panel A of Table 5, we first present previously reported regressions and then reformulate them by including our two key variables. Our enforcement variables again have considerable explanatory power, and the results are robust to these two indices of private enforcement, with our variables maintaining statistically significant coefficients in fifteen out of sixteen regressions. Moreover, introducing our enforcement variables often reduces or eliminates the statistical significance of the private enforcement indices.

E. Financial Variables Associated With Dispersed Ownership

So far, we have limited our analysis to dependent variables associated with the size of a countries 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 staffing and budgets. Other variables have also been used to measure financial depth and we turn to those now, focusing on three dependent

¹¹ To test for sample effects, we reran the original LLS public enforcement regressions using only the sub-sample of 35 countries for which we had extrapolated budget variables. The LLS public enforcement index did not perform any better in this sub-sample than it did for the full LLS dataset of 49 jurisdictions.

variables closely related to the dispersion of ownership: an index measuring ease of access to public markets, median block premia, and concentration of ownership.

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, however, one that is contested in the legal literature and some of the financial literature. Some maintain the opposite perspective, that concentrated ownership is just as likely in principle to be an effective solution to agency problems of corporate managers and when potential agency costs are 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 (1990). A low block premium 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, 1990), especially if blockholders aren't checking managers — then the bases for public policymakers to build institutions protecting outside investors in that nation's firms are less compelling than elsewhere. Public policymakers may desist from such institution-building, because ownership will not separate from control regardless of the institutional quality of shareholder protection. Roe (2002), Gilson (2006). 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 nations, 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.

In Table 7, we regress these three variables associated with dispersed ownership on our two key measures of public enforcement as explanatory variables and several other legal indices and controls. In these regressions, strong public enforcement does not strongly correlate with the variables associated with dispersed ownership. Indeed in three of the equations (one with respect to block premia and two for ownership concentration), the coefficient for our public enforcement variable points away from the direction associated with dispersed ownership and is statistically significant. While these results require further study, at a minimum, one might conclude that to the extent that public enforcement positively influence on the size of a country's capital markets, it does not directly disperse share ownership.¹² Countries that dedicate more resources to securities enforcement do not necessarily have fewer closely held firms or lower premia 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

¹² As always, causation here might run in the other direction, increased concentration of ownership could lead countries to expend more resources on public enforcement in order to safeguard minority interests.

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 in our view to existing public enforcement measures, is still imperfect. For example, it measures resources but not effectiveness. A better public enforcement variable might produce differing results with respect to the impact of public enforcement on dispersed ownership.

F. Limits to Both Private and Public Enforcement: Intermediate Financial Variables

To try to gain additional insight into the channels through which public and private enforcement affects capital markets outcomes, we also ran these regressions using as dependent variables a set of indices that the World Bank is developing as part of a new financial database that includes variables associated with capital market development. The World Bank staff has then employed these variables to develop a series of indices measuring the size of equity markets, ease of access to equity markets, stock market efficiency, and stock market stability. The data set also has one aggregate index that includes all information about a country's capital markets. We report the regression results in Table 8. 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 had positive and statistically significant effects. So, as reported above, it seems that both public enforcement and disclosure are associated with more robust capital markets. With respect to 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 did not. Yet *neither* the public *nor* the private enforcement variables were closely correlated with market efficiency and stability, as we report in Table 8.¹³

Since the regressions overall show public enforcement and disclosure to both be associated with the size of a nation's capital market, one might think that either or both would correlate with improved technical performance of the securities markets. We leave this puzzle for future work. Again, because our variables measure money and

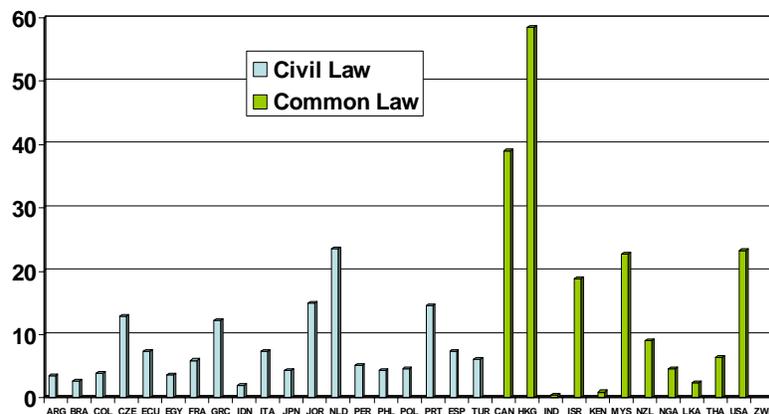
¹³ We sought to see whether strong relationships existed with components of these World Bank efficiency indices, such as trading costs or price synchronicity, and either legal origin or public enforcement. We found no strong relationship between these on the one hand and either legal origin or public 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 has 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 has found a relationship between some of the LLS legal indices and lower trading costs. Eleswarapu (2005).

staff, but not overall effectiveness, perhaps 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. We note again that the private-enforcement variables predict market efficiency and stability no 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.

G. Legal Origin and Regulatory Intensity

As 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 (1997); Roe (2006), we also explored the relationship between common law origins and various measures of public enforcement. As has been observed in prior work, Jackson (2005), the level of amount of public resources devoted to financial regulation appears to be systematically higher in countries from common law origins as compared with countries with civil law origins. This phenomenon is surprising to the extent that the stereotypical view of civil law countries is that they more heavily regulated their economics. For financial markets, the opposite appears to be the case, if one measures oversight by the amount of resources allocated to the regulatory task.

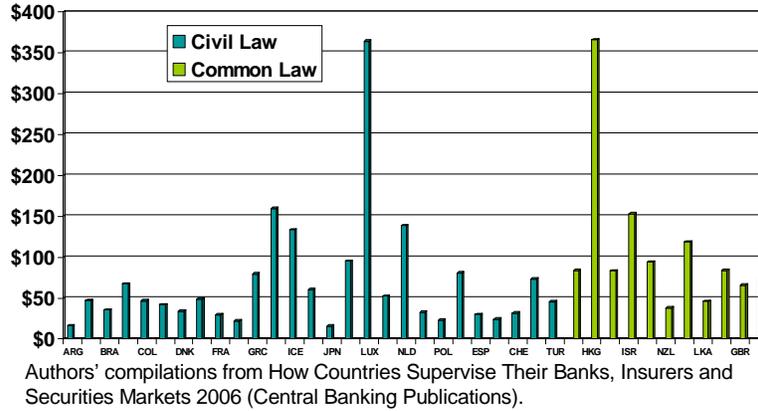
Figure Three
Civil versus Common Law Staffing
(Securities Regulators (Pure) Per Million of Population)



Authors' compilations from *How Countries Supervise Their Banks, Insurers and Securities Markets 2006* (Central Banking Publications).

Figure Four
Civil versus Common Law: Budgets

('000s of U.S. Dollars per Billion of GDP based on 12/31/05 Exchange Rates)



Figures 3 and 4 confirm this point visually, using the actual enforcement level variables we introduce in this paper. Figure 3 displays the distribution of civil law staffing ratios on the left and the distribution of common law staffing ratios on the right. Figure 4 shows the same distributions for our budget variables. The differences between common law and civil law jurisdictions become yet more pronounced when we correct for national wealth and size of the economy. Table 9 shows four simple regressions in which the dependent variables are our three new enforcement variables and then the LLS public enforcement index. 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 capital adjusted for purchasing power parity, and log GDP for 2004, which we include to control for scale effects.

Consider the results in Table 9. First, the common law dummy is statistically significant at the five percent level in all four regressions. 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.

Another potentially interesting result in Table 9 concerns the coefficients for our wealth control. For the three regressions using our public enforcement measures as dependent variables, wealth has a positive coefficient (significant at the one percent). This result is fairly intuitive: wealthier countries would expend more effort on financial regulation (and other supervisory roles). What is puzzling is the *negative* relationship between wealth and the LLS formal index of public enforcement. That

wealthier jurisdictions do not systematically perform better on the LLS index calls into question whether the index is measuring meaningful attributes of public oversight.

In Table 10 we examine the relationship between common law origins and the ratio of stock market capitalization to GDP, the most frequently utilized measure of capital market development, using the more current World Bank figures for 2004. In the first regression, we regress stock market capitalization on common law origins and a wealth control, obtaining the standard result that both explanatory variables are associated with higher stock market capitalization at statistically significant levels. We then run a series of regressions to explore whether common law origins has any residual relationship with stock market capitalization once one controls for the various legal indices and enforcement variables explored in this paper. When we include the legal indices with consistent explanatory power (the securities disclosure and securities liability indices) and one of the other our actual enforcement variables, not only does the common law dummy lose significance, but the sign of its coefficient changes to negative.

The results in Table 10 suggest that whatever effect common law origins may have on the development of robust securities markets, a country can accomplish that result by adopting legal rules *and* enforcement regimes conducive to the emergence of capital markets. Public and private enforcement, strategies which respectively roughly map onto civil-law-style and common-law-style mechanisms, *both* seem to play roles. Once one controls for a limited number of plausible legal mechanisms to contain corporate and financial wrong-doing, 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).

H. The Direction of Causality?

As is usual in these finance inquiries, we cannot reject the possibilities of reverse or bidirectional causality. In fact, our prior 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, see Roe (1994)) and then the players (such as 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 markets to enforcement. 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, the relationship makes legal origins a potentially useful instrumental variable, either formally (La Porta (1998)) or informally (Djankov (2005)). Since the theory was that private enforcement mechanisms associated directly with the common law (such as fiduciary duties) or indirectly with it was driving financial results, there was some plausibility to using legal origin as an instrument.

There are, however, problems with using legal origin to anchor causality here. True, staffing and budget correlates 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 connection between common law origin and high government spending 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 believed to be strong — could be plausibly tied to legal origin, because common law systems use fiduciary duties and civil law systems are thought not to use anything similar. Similarly, 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 are finding here that it's the *common* law systems that are budgeting much 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 over-arching preferences. But these preferences seem too 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 prominent 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 tests using legal origins as a instrument. As Table 11 shows, common law is as effective an instrument for high public enforcement budgets and staffing as it has been for private enforcement. We include the disclosure index, which is the private law variable most strongly associated with robust capital markets, so that readers can compare how the legal origin instrument does for both enforcement channels. For those who continue to see legal origin as a useful instrument, we just point out that it works as effectively when instrumenting public enforcement, regulatory variables, as it does for private enforcement, judiciary-oriented 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 for private, judicial-oriented enforcement.

We investigated a second instrument: the log of GDP. As has been reported previously (Jackson (2006) and which we confirm in Table 8), there are economies of scale in the regulatory staffing and regulatory budgets: the larger a nation's economy, the fewer regulatory staff members or regulatory expenditures. This phenomenon is reflected in a consistent negative correlation between the logarithm of national GDP and our new 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 with 2004 market capitalization as the dependent variable.¹⁴ In Panel A, we report a series of second-stage regressions along with the original OLS regressions (columns (1) and (2)). 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 data in this table suggests that our new enforcement variables plays some causative role in the development of robust capital markets, and that the causal link is at least as strong as that of disclosure index, which suffers from similar problems of endogeneity. The most straightforward evidence of the relationship is the number of statistically significant coefficients associated with our enforcement variables. The coefficient for the disclosure index is much less frequently significant in the second-stage regressions and sometimes flips over to being negative. The strength of our inferences here are tempered by the fact that a number of these regressions have Cragg-Donald F-statistics beneath the relevant Stock-Yogo critical values, suggestive of a weak instrument. However, the consistency of our estimated coefficients for both the extrapolated budget variable and the direct staffing variable — consistency across somewhat different samples and with the original OLS regressions — increases our confidence in the usefulness of this analysis, as Murray (2006) suggests is appropriate.¹⁵

Still, we do not claim to have decisively resolved the question of causality. The instruments are not consistently strong. The evidence of a causal link between the new enforcement variables and market capitalization is suggestive but not definitive and we are dubious of the connections between the origins instrument and financial outcomes. However, measured at least by the second-stage regressions presented in Table 12, the evidence of a causal connection between the LLS disclosure index and market

¹⁴ 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.

¹⁵ 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.

capitalization is no stronger than the evidence supporting a causal link for our variables. And, 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 supports that claim.

III. DISCUSSION

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.

Public enforcement 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 wrong-doing 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 wrong-doing is detected, higher budget and staffing facilitate regulatory investigations of wrong-doing, 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 as the firm's organizational and reputational capital is diminished. 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 agency being able to write, revise and enforce better, more sophisticated regulatory rules. The completeness of legal rules has been shown to have an impact in the area of insider trading (Beny 2005) and could be important more broadly. We cannot distinguish which among these channels is the most important.¹⁶

Several market channels could also link public enforcement to stronger capital markets. One is the policing of wrong-doing by traditional, insider corporate governance actors who take value from outside investors unless the enforcement machinery prevents them from so doing. More subtly, financial markets depend on relatively low-cost, transparent trading mechanisms. 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

¹⁶ Some of the hypotheses suggested in the text imply that public enforcement interacts with explanatory variables associated with private monitoring, such as the LLS disclosure index or liability index. We experimented with several tests of interaction and found no support for such effects.

this — trading regulation as opposed to just corporate governance — may well be an important and under-appreciated channel for enforcement.

Finally, there may be a relationship between regulatory nuance and our measure of public enforcement intensity. When we see simple, bright lines 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.) A nation with simple, bright line rules, may simply be one 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 in its latest, famous Doing Business Report, World Bank (2006a: 41-43), asks what developing countries should reform vis-à-vis corporate laws. In answering its own question, it focuses primarily on private enforcement via litigation as opposed to public enforcement via rule-making and enforcement: notify investors of directors' interests in deals, require disinterested director approvals, and, where courts are strong, help investors bring lawsuits. For their concept of public enforcement, they rely on research that focuses on agency characteristics, World Bank (2006a: 44 nn.3-5), not budgets and staffing. Similarly, their 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. Public enforcement when measured by budget and staffing *is* associated with strong financial outcomes. While theory could suggest why this should not be so, because public enforcers lack the strong incentives and local information of private agents, we noted above that observers of America's extensive securities litigation regime see the incentives of private enforcement as often being misaligned, see damages as often not being visited on wrong-doers, and see innocents as often having to pay damages to innocents. The problem in matching theory to reality is that it's often the same public agent — the legislature, for example — that designs both the private rules and the public rules. Error and rent-seeking can seep into either.

If it's true that rules on private liability are poorly designed — as many American securities law academics and practitioners have argued — then that poor design could explain why public enforcement could still have a useful effect on financial outcomes. Hence, the evidence does not allow one to ignore public enforcement to focus solely, or even primarily, on private enforcement in recommending how to construct better financial markets.

C. Developing Better Measures of Public Enforcement

Finally, we note the importance of developing better measures of public enforcement effort. The variables that we have introduced in this paper were drawn from surveys undertaken by commercial publishers. While the data has proved quite useful and produced variables with strong statistical power, the source's research methods were not as attuned to issues of consistency and completeness as we 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 extrapolating methods that we used. It would, moreover, be extremely 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. Only with more complete and higher quality data of this sort 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 count our efforts as a success.

D. Public Enforcement and Private Enforcement

Our goal here is not to show that our 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 is substantially and positively associated with the size of a country's capital markets and may well contribute to causation. The results we reported in Tables 4 and 6 show that the relationship between our 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 now well-recognized in the literature. Indeed, the relationship between our public enforcement variables and market size seem to be stronger than that found for liability standards and judicial efficiency.

In sum, 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 one controls for legal indices measuring disclosure, liability standards, and judicial efficiency. While our measures of financial 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. While simple regressions of the sort presented here cannot untangle the question of causation — the presence of more developed capital markets and the concomitant political support for enforcement surely induces some jurisdictions to expend more resources on public oversight — our results

raise the possibility that a well financed public enforcement apparatus has a positive effect on a nation's capital market.

CONCLUSION

Recent work in finance has focused on how securities laws are enforced, usually concluding that private enforcement via lawsuits among contracting parties works best. Public enforcement is seen as working poorly and is generally insignificant in the relevant regressions. Yet the legal literature has persistently pointed out design flaws in the mechanisms of private enforcement prevailing in the United States. Moreover, some structural elements — like dispersed ownership — make collective remedies suboptimal and put authority in the lawyers' hands, an authority that is not used relentlessly in outside investors' interests.

Given this a priori indeterminacy, we constructed two categories of public enforcement intensity, one based on regulatory budgets and another based on 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 emphasize again that we do not see the data as telling us that public enforcement is more important than private enforcement. First, some other 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 mechanisms of enforcement 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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Table 1
Securities Enforcement Variables

Country	World Bank Codes	Public Enforcement (Actual Resources)			Public Enforcement (Formal)		Common Law Jurisdiction
		Direct Staff Per Million of Population	Extrapolated Staff Per Million of Population	Extrapolated Budgets Per Billion US\$ of GDP	Public Enforcement Index (LLS)	Public Enforcement Index (Djankov)	
Argentina	ARG	3.46	3.46	\$15,984	0.58	0.00	0
Australia	AUS		39.35	\$120,162	0.90	0.50	1
Austria	AUT		10.08	\$37,060	0.17	1.00	0
Belgium	BEL		14.39	\$46,926	0.15	0.50	0
Brazil	BRA	2.68	2.68	\$35,260	0.58	0.50	0
Canada	CAN	38.98	38.98	\$83,932	0.80	1.00	1
Chile	CHL		10.39	\$67,137	0.60	1.00	0
Colombia	COL	3.88	3.88	\$46,603	0.58	0.00	0
Czech Republic*	CZE	12.95	12.95	\$41,685		1.00	0
Denmark	DNK		12.34	\$33,745	0.37	0.75	0
Ecuador	ECU	7.41	7.41		0.55	1.00	0
Egypt	EGY	3.59	3.59		0.30	0.00	0
Finland	FIN		10.18	\$48,969	0.32	0.00	0
France	FRA	5.93	5.93	\$29,205	0.77	0.50	0
Germany	DEU		6.81	\$22,196	0.22	1.00	0
Greece	GRC	12.17	12.17	\$79,801	0.32	0.50	0
Hong Kong	HKG	58.47	58.47	\$365,740	0.87	0.00	1
Hungary*	HUN		19.84	\$159,462		0.00	0
Iceland*	ICE		45.07	\$133,571		0.00	0
India	IND	0.43	0.43		0.67	0.50	1
Indonesia	IDN	1.97	1.97		0.62	0.00	0
Ireland	IRL		26.60	\$83,114	0.37	0.00	1
Israel	ISR	18.82	18.82	\$153,118	0.63	1.00	1
Italy	ITA	7.40	7.40	\$60,552	0.48	0.00	0
Japan	JPN	4.31	4.31	\$15,905	0.00	0.00	0
Jordan	JOR	14.90	14.90		0.60	0.00	0
Kenya	KEN	0.96	0.96	\$93,953	0.70	0.00	1
Korea	KOR		13.15	\$95,147	0.25	0.50	0
Luxembourg*	LUX		181.91	\$364,223		1.00	0
Malaysia	MYS	22.65	22.65		0.77	1.00	1
Mexico	MEX		4.71	\$52,494	0.35	0.50	0
Netherlands	NLD	23.52	23.52	\$138,785	0.47	0.00	0
New Zealand	NZL	9.00	9.00	\$37,784	0.33	0.00	1
Nigeria	NGA	4.60	4.60		0.33	0.00	1
Norway	NOR		15.99	\$32,733	0.32	1.00	0
Pakistan	PAK		2.47		0.58	0.75	1
Peru	PER	5.11	5.11		0.78	0.25	0
Philippines	PHL	4.37	4.37		0.83	0.00	0
Poland*	POL	4.64	4.64	\$22,961		1.00	0
Portugal	PRT	14.49	14.49	\$80,842	0.58	1.00	0
Singapore	SGP		88.28		0.87	1.00	1
Slovak Rep*	SLV		13.93	\$53,844		0.00	0
South Africa	ZAF		3.82	\$118,453	0.25	0.00	1
Spain	ESP	7.33	7.33	\$29,931	0.33	0.75	0
Sri Lanka	LKA	2.35	2.35	\$46,126	0.43	0.00	1
Sweden	SWE		8.19	\$24,354	0.50	1.00	0
Switzerland	CHE		8.04	\$31,418	0.33	0.50	0
Taiwan	TWN		14.65	\$73,061	0.52	0.00	0
Thailand	THA	6.40	6.40	\$83,804	0.72	0.00	1
Turkey	TUR	6.18	6.18	\$45,417	0.63	0.00	0
United Kingdom	GBR		14.32	\$65,507	0.68	0.00	1
United States	USA	23.29	23.29	\$76,459	0.90	0.00	1
Uruguay	URY		50.66		0.57	0.50	0
Venezuela	VEN				0.55	0.00	0
Zimbabwe	ZWE				0.42		1

* Countries not included in original LLS Database

Table 2 Summary Statistics on Public Enforcement Variables						
	<i>Number</i>	<i>Mean</i>	<i>Median</i>	<i>Min.</i>	<i>Max.</i>	<i>Std.</i>
Public Enforcement (Formal)						
Public Enforcement Index (LLS)	49	0.509	0.55	0	0.90	0.22
Public Enforcement Index (Djankov)	54	0.398	0.375	0	1	0.424
Public Enforcement (Actual Resources)						
Direct SEC Staff Per Million of Population	30	11.07	6.29	0.43	58.47	12.44
Extrapolated Staff Per Million of Population	53	17.88	10.08	0.43	181.91	28.18
Extrapolated Budget (US\$000) Per Billion of GDP	41	\$79,205	\$53,844	\$15,905	\$365,740	\$74,659

Table 3
Pairwise Correlation Matrix for Key Enforcement Variables

	<i>Disclosure Index</i>	<i>Liability Index</i>	<i>Anti-Directors Index</i>	<i>Judicial Efficiency Index</i>	<i>Public Enforcement Index (LLS)</i>	<i>Public Enforcement Index (Djankov)</i>	<i>Direct Staff to Population</i>	<i>Extrapolated Staff to Population</i>	<i>Extrapolated Budget to GDP</i>
Disclosure Index	1								
Liability Standards Index	0.5496	1							
Anti-Directors Index	0.5236	0.4999	1						
Judicial Efficiency Index	0.2542	0.2241	0.2113	1					
Public Enforcement Index (LLS)	0.3305	0.3091	0.3691	-0.113	1				
Public Enforcement Index (Djankov)	-0.1518	-0.0778	0.0581	0.2434	-0.0098	1			
Direct Staff to Population	0.3955	0.4417	0.4026	0.555	0.3898	0.1723	1		
Extrapolated Staff to Population	0.2514	0.2755	0.2267	0.4149	0.3979	0.2405	1	1	
Extrapolated Budget to GDP	0.3837	0.3982	0.2839	0.1177	0.43	-0.064	0.8381	0.8005	1

	2004 Market Capitalization			2004 Trading Volumes			Domestic Firms (2004)			IPOs to GDP (2004)		
	Direct Staff Per Million of Population	Extrapolated Staff Per Million of Population	Extrapolated Budget ('000) per US\$ billion of GDP	Direct Staff Per Million of Population	Extrapolated Staff Per Million of Population	Extrapolated Budget ('000) per US\$ billion of GDP	Direct Staff Per Million of Population	Extrapolated Staff Per Million of Population	Extrapolated Budget ('000) per US\$ billion of GDP	Direct Staff Per Million of Population	Extrapolated Staff Per Million of Population	Extrapolated Budget ('000) per US\$ billion of GDP
New Enforcement Variables	6.9679 ^a (1.9587)	1.6265 (1.4088)	1.0478 ^b (0.2657)	2.7391 ^c (1.3666)	0.2354 (0.6885)	0.3271 ^c (0.1803)	0.0457 ^a (0.0133)	0.0188 ^a (0.0065)	0.0032 (0.0020)	0.2441 ^a (0.0834)	0.2089 ^a (0.0298)	.0412 ^a (0.0079)
LLS Disclosure Index	74.2363 ^b (30.0504)	121.6703 ^b (47.9094)	65.4748 (55.9261)	69.8364 (40.1701)	86.3407 ^a (26.1807)	100.6277 ^a (35.6987)	1.1076 (0.6857)	1.4246 ^b (0.6057)	1.0231 (0.9630)	4.6388 ^b (1.8851)	1.8955 (2.9312)	0.9008 (3.9887)
LLS Liability Standards Index	-76.6516 ^c (37.6333)	2.7785 (31.4007)	-27.4254 (39.1547)	-6.8748 (51.7971)	23.2637 (25.3709)	19.0949 (33.1175)	-0.1844 (0.7708)	0.5086 (0.6908)	1.2818 (0.7896)	-5.7377 ^b (1.9943)	-3.3575 ^b (1.5108)	-3.8410 ^c (2.0363)
Log GNI Per Capita (PPP; 2005)	-4.6118 (9.1913)	19.1770 ^b (8.0573)	36.0768 ^a (10.6922)	12.8939 (8.6828)	23.7182 ^a (6.9390)	31.3812 ^a (8.9909)	0.5539 ^a (0.1915)	0.6941 ^a (0.1116)	0.8603 ^a (0.1392)	-0.6609 (0.6472)	-0.1687 (0.3919)	2.1265 ^b (0.9682)
Constant	37.5693 (79.1939)	-193.2945 ^a (69.0369)	-361.7742 ^a 95.8003	-140.7514 ^c (71.0094)	-237.0108 ^a (61.6686)	-338.8298 ^a (88.2739)	-3.9396 ^b (1.6117)	-5.4868 ^a (1.0760)	-7.2551 ^a (1.3906)	6.4442 (6.2138)	2.5451 (2.8792)	-19.6965 ^b (8.0464)
Observations	28	46	34	20	46	34	28	46	34	20	36	34
MSE	48.29	72.164	60.133	42.913	46.893	46.212	0.9287	0.8268	0.804	2.1485	2.4305	2.6572
Adjusted R Squared	76%	32%	59%	59%	37%	47%	56%	61%	58%	68%	67%	50%

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

Table 5

New Public Enforcement Variables Versus Prior Indices of Public Enforcement

(robust standard errors in parentheses)

Panel A: Regressions with New Variables and Old Enforcement Indices

	Market Capitalization			Trading Volumes			Domestic Firms			IPOs		
	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP
New Enforcement Variables	–	0.0137 ^a (0.0044)	0.0017 ^b (0.0007)	–	1.8446 ^b (0.7997)	0.2861 ^b (0.1051)	–	0.0369 ^a (0.0116)	0.0048 ^b (0.0019)	–	.1682 ^a (0.0345)	0.0115 ^c (0.0067)
Public Enforcement Index (LLS)	0.3446 ^c (0.1990)	0.2078 (0.2393)	0.1756 (0.2456)	39.5648 (30.0063)	15.7065 (43.1267)	27.4941 (39.0173)	0.6422 (0.4813)	-0.1282 (0.7525)	-0.1236 (0.4694)	3.7220 ^b (1.5531)	-1.658 (2.1248)	3.7004 ^c (2.0805)
Anti-Director Rights Index	0.0711 ^b (0.0347)	0.0341 (0.0355)	0.0694 ^c (0.0420)	7.8568 (4.7260)	10.2996 (7.7825)	2.7567 (6.1558)	0.1761 ^b (0.0861)	-0.02 (0.1665)	0.2104 ^b (0.0822)	0.3098 (0.2434)	0.1458076 (0.4418)	0.117 (0.3250)
Ln GDP per capita	0.1041 ^a (0.0218)	.0536 ^b (0.0223)	0.1277 ^a (0.0342)	21.4326 ^a (7.0790)	10.1056 ^b (4.8139)	35.0305 ^b (12.9054)	0.2949 ^a (0.1052)	0.1974 (0.1370)	0.3156 ^c (0.1625)	1.2210 ^a (0.2687)	0.5216 ^c (0.2805)	1.7630 ^a (0.5080)
Judicial Efficiency Index	0.0518 ^b (0.0236)	0.0291 (0.0247)	0.0378 (0.0324)	-2.0959 (5.4241)	-1.824 (4.7958)	-9.1709 (8.6927)	0.2551 ^a (0.0750)	0.2263 ^b (0.0922)	0.1837 ^b (0.0846)	0.0355 (0.2168)	-0.3901 (0.2319)	-0.1848 (0.3393)
Constant	-1.2999a (0.2169)	-0.7035b (0.3384)	-1.4360a (0.2960)	-165.9368a (40.4056)	-90.6045 (63.0962)	-234.9664a (73.3187)	-2.847a (0.7578)	-1.2066 (1.3058)	-2.5481b (1.1832)	-10.8554a (2.0799)	-0.523 (2.4375)	-14.3922a (3.5744)
Observations	49	28	35	49	28	35	49	28	35	49	28	35
MSE	0.274	0.18547	0.3041	56.885	34.702	64.559	0.6613	0.70879	0.6195	2.5737	2.0189	2.8468
Adjusted R ²	48%	72%	42%	18%	52%	13%	66%	66%	65%	34%	50%	29%

Panel B: Regressions with New Enforcement New Variables Versus Old Enforcement Indices

	Market Capitalization			Trading Volumes			Domestic Firms			IPOs		
	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP
New Enforcement Variables	–	.0159 ^a (0.0348)	.0019 ^a (0.0006)	–	2.0129 ^a (0.6307)	0.3113 ^a (0.0920)	–	0.0356 ^a (0.0083)	0.0046 ^b (0.0018)	–	0.1504 ^a (0.2288)	0.0162 ^b (0.0060)
Public Enforcement Index (LLS)	0.3446 ^c (0.1990)	–	–	39.5648 (30.0063)	–	–	0.6422 (0.4813)	–	–	3.7220 ^b (1.5531)	–	–
Anti-Director Rights Index	0.0711 ^b (0.0347)	0.0434 (0.0361)	0.0800 ^b (0.0365)	7.8568 (4.7260)	11.0006 (7.7863)	4.4134 (6.4520)	0.1761 ^b (0.0861)	-0.0257 (0.1572)	0.2030b (0.0805)	0.3098 (0.2434)	0.0718 (0.4508)	0.34 (0.3793)
Ln GDP per capita	0.1041 ^a (0.0218)	0.0464 ^b (0.0207)	0.1246 ^a (0.0333)	21.4326 ^a (7.0790)	9.5572 ^b (4.5017)	34.5556 ^a (12.5637)	0.2949 ^a (0.1052)	0.2019 (0.1195)	0.3177 ^c (0.1621)	1.2210 ^a (0.2687)	0.5794 ^c (0.2856)	1.6990 ^a (0.5058)
Judicial Efficiency Index	0.0518 ^b (0.0236)	0.0206 (0.0208)	0.0347 (0.0312)	-2.0959 (5.4241)	-2.4698 (3.6860)	-9.6429 (8.4209)	0.2551 ^a (0.0750)	0.2315 ^b (0.0877)	0.1858 ^c (0.0834)	0.0355 (0.2168)	-0.3219 (0.1947)	-0.2483 (0.3261)
Constant	-1.2999a (0.2169)	-0.5150b (0.2284)	-1.3465a (0.2369)	-165.9368a (40.4056)	-76.3556c (37.8691)	-220.9429a (66.0907)	-2.847a (0.7578)	-1.3229 (0.8238)	-2.6111a (1.1566)	-10.8554a (2.0799)	-2.0272 (1.5305)	-12.5048a (3.2500)
Observations	49	28	35	49	28	35	49	28	35	49	28	35
MSE	0.274	0.18518	0.301	56.885	34.056	63.705	0.6613	0.69359	0.6096	2.5737	1.9968	2.8924
Adjusted R ²	48%	72%	43%	18%	54%	15%	66%	68%	66%	34%	51%	27%

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

Part A: Disclosure Requirements												
	Market Capitalization			Trading Volumes			Domestic Firms			IPOs		
	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP
New Enforcement Variables	–	0.0145 ^a (0.0036)	0.0014 ^c (.0007)	–	1.8826 ^a -0.6551	0.2365 ^b (0.1110)	–	0.0326 ^a (0.0083)	0.0038 ^b (0.0018)	–	0.1419 ^a (0.0218)	0.0126 ^c (0.0068)
Disclosure Index	0.5813 ^a (0.1377)	0.3296 ^a (0.1085)	0.6333 ^b (0.2457)	97.2049 ^a (34.0413)	29.98349 (21.1182)	101.8699 (61.8797)	1.1103 ^b (0.4127)	0.676225 (0.5625)	0.9698 (0.6791)	4.6983a (1.4395)	1.971 (1.9321)	4.3143 (2.9190)
Anti-Director Rights Index	0.042 (0.0308)	0.0178 (0.0342)	0.0406 (0.0389)	1.7897 (5.5914)	8.6737 (7.4384)	-1.927 (7.6848)	0.1195 (0.0946)	-0.0782 (0.1755)	0.1426 (0.0953)	0.1371 (0.2772)	-0.0811 (0.4561)	0.0714 (0.3322)
Ln GDP per capita	0.0957 ^a (0.0229)	0.05153 ^b (0.0193)	0.1038 ^a (0.0362)	20.2746 ^a (6.4414)	10.0259 ^b (4.4362)	31.1993 ^a (11.2509)	0.2789 ^b (0.1075)	0.2124594 (0.1253)	0.2857 ^c (0.1481)	1.1393 ^a (0.2439)	0.6103 ^b (0.2699)	1.5569 ^a (0.4758)
Judicial Efficiency Index	0.0386 ^c (0.0204)	0.0161 (0.0200)	0.042 (0.0304)	-4.044 (5.3761)	-2.8758 (3.7794)	-8.4722 (8.1296)	0.2302 ^a (0.0664)	0.2224 ^b (0.0830)	0.1970 ^b (0.0768)	-0.0843 (0.2106)	-0.34857 (0.2195)	-0.1987 (0.3145)
Constant	-1.2056a -0.2037	-0.6343a -0.2190	-1.4444a -0.2665	-160.15a -37.7904	-87.2097b -40.0854	-236.6869a -69.3842	-2.6758a -0.6693	-1.5677c -0.8194	-2.7610b -1.0174	-9.5765a -1.8551	-2.7407c -1.4549	-13.1716a -3.3147
Observations	49	28	35	49	28	35	49	28	35	49	28	35
MSE	0.2563	0.17451	0.2869	53.838	34.174	62.483	0.6341	0.69297	0.5982	2.5034	1.9939	2.8508
Adjusted R ²	54%	75%	48%	27%	54%	19%	69%	68%	72%	38%	51%	29%
Panel B: Liability Standards												
	Market Capitalization			Trading Volumes			Domestic Firms			IPOs		
	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP	Original LLS	Pure Staff to Population	Extrapolated Budget to GDP
New Enforcement Variables	–	0.0146 ^a (0.0041)	0.0015 ^b (0.0007)	–	1.8106 ^b (0.7010)	0.2062 (0.1502)	–	0.0352 ^a (0.0083)	0.0036 ^b (0.0014)	–	0.1420 ^a (0.0187)	0.0131 ^b (0.0055)
Liability Standards Index	0.4481 ^a (0.1289)	0.2252 (0.1395)	0.3330 ^c (0.1958)	90.3188 ^a (31.4726)	34.682 (26.1675)	101.5841 ^b (48.5968)	0.7522 ^c (0.4245)	0.0564 (0.5911)	0.8853 (0.5461)	3.7150 ^a (1.3750)	1.4505 (1.1841)	2.7134 (2.0177)
Anti-Director Rights Index	0.0515 (0.0330)	0.0203 (0.0330)	0.0553 (0.0373)	1.914 (5.3484)	7.4472 (7.1985)	-3.1201 (6.3950)	0.1474 (0.0883)	-0.0315 (0.1925)	0.1373 (0.0993)	0.2049 (0.3216)	-0.0768 (0.5197)	0.1387 (0.4334)
Ln GDP per capita	0.0878 ^a (0.0240)	0.0432 ^b (0.0203)	0.1050 ^a (0.0317)	18.5645 ^a (6.0737)	9.0682 ^b (4.2945)	28.5508 ^a (9.5808)	0.2665 ^b (0.1089)	0.2011 (0.1206)	0.2654 (0.1717)	1.0733 ^a (0.2370)	0.5590 ^c (0.2848)	1.5386 ^a (0.4578)
Judicial Efficiency Index	0.0457 ^b (0.0226)	0.0222 (0.0199)	0.0333 (0.0319)	-2.9061 (5.0634)	-2.2158 (3.6767)	-10.0934 (8.2806)	0.2439 ^a (0.0768)	0.2319 ^b (0.0921)	0.1819 ^b (0.0795)	-0.0275 (0.2031)	-0.3113 (0.1997)	-0.2603 (0.3311)
Constant	-1.0818a (0.2026)	-0.5278b (0.2114)	-1.2061a (0.2271)	-138.501a (35.2721)	-78.3322b (37.2591)	-178.1072a (47.5834)	-2.4459a (0.7360)	-1.3261 (0.8557)	-2.2378c (1.2240)	-8.5704a (1.7468)	-2.1098 (1.4534)	-11.3606a (2.9150)
Observations	49	28	35	49	28	35	49	28	35	49	28	35
MSE	0.2644	0.18118	0.2979	53.748	33.775	61.135	0.6528	0.70904	0.5911	2.5516	2.0107	2.8855
Adjusted R ²	51%	73%	44%	27%	55%	22%	67%	66%	68%	36%	51%	27%

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

	LLS Dependent Variables Related to Private Control					
	Block Premia		Ownership Concentration		Access to Equity	
	Pure Staff to Population	Extrapolated Budget to GDP	Pure Staff to Population	Extrapolated Budget to GDP	Pure Staff to Population	Extrapolated Budget to GDP
New Enforcement Variables	0.0011 (0.0022)	0.0004 ^c (.0002)	0.0043 ^b (0.0016)	0.0009 ^a (.0002)	0.002 (0.0133)	-0.0014 (0.0015)
Disclosure Index	-0.3095b (0.1245)	-0.3718b (0.1410)	-0.1885c (0.1062)	-0.3749a (0.0801)	1.3223 ^b (0.5321)	0.5283 ^c (0.4751)
Liabilities Standards Index	-0.0312 (0.0902)	-0.0887 (0.1029)	-0.0923 (0.0662)	-0.1515c (0.0780)	1.0189 (0.6322)	1.2025 ^b (0.5097)
Log GDP per Capita	-0.0039 (0.0279)	-0.0106 (0.0326)	-0.029 (0.0178)	-0.0512a (0.0175)	0.1403 (0.1370)	0.2705 (0.1208)
Judicial Efficiency Index	-0.0037 (0.0154)	-0.012 (0.0130)	-0.0207c (0.0101)	-0.0021 (0.0121)	0.0987 (0.0837)	0.1388 (0.1208)
Constant	0.3598 ^c (0.1936)	0.5512 ^c (0.2715)	1.0098 ^a (0.1570)	1.1868 ^a (0.1035)	1.6131 (1.0820)	0.6076 (2.4967)
Observations	21	30	28	35	25	33
MSE	0.10105	0.1083	0.09181	0.0921	0.73669	0.6365
Adjusted R ²	18%	35%	43%	60%	48%	53%

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

	Equity Index 2004		Equity Market Size Index 2004		Equity Market Access Index 2004		Equity Market Efficiency Index I 2004		Equity Market Efficiency Index II 2004		Equity Market Stability Index 2004	
	Staff to Population	Budget to GDP	Staff to Population	Budget to GDP	Staff to Population	Budget to GDP	Staff to Population	Budget to GDP	Staff to Population	Budget to GDP	Staff to Population	Budget to GDP
New Staffing Variables	0.0325 ^a (0.0110)	0.0047 ^a (.0015)	0.1249 ^b (0.0528)	0.0187 ^a (0.0066)	0.01 (0.0181)	-0.0007 (0.0019)	0.0019 (0.0156)	-0.0006 (0.0021)	-0.0002 (0.0125)	-0.0015 (0.0017)	0.0032 (0.0076)	0.0016 ^c (0.0009)
Disclosure Index	1.2155 ^b (0.4739)	1.7421 ^a (0.4687)	2.6074 ^b (0.9886)	3.8152 ^a (1.2671)	2.5446 ^a (0.7847)	3.0396 ^a (0.7381)	-1.1437 (0.9677)	-0.2812 (1.0284)	-0.2787 (0.7579)	0.0029 (1.0782)	0.9127 (0.7618)	0.1146 (0.4235)
Log GNI per capita (PPP; 2005)	0.4055 ^a (0.1341)	0.6048 ^a (0.1327)	0.5470 ^c (0.2906)	1.4528 ^a (0.3094)	-0.0117 (0.2224)	0.1873 (0.1444)	0.8834 ^b (0.3457)	0.5087 (0.3059)	0.4826 (0.3117)	0.29 (0.3072)	0.2395 ^b (0.0895)	0.2423 ^b (0.0948)
Constant	0.5259 (1.1107)	-1.5455 (1.3431)	-1.0100 (2.1886)	10.1638 ^a (3.0846)	3.5655 ^c (1.9134)	1.4778 (1.7389)	-3.0251 (3.2645)	0.3551 (3.0164)	-0.3238 (2.9308)	1.6711 (2.9000)	2.1442 ^c (1.1006)	2.5083 ^b (0.9721)
Observations	28	34	28	34	28	35	27	35	25	35	28	35
MSE	0.63524	0.59513	1.6069	1.7681	1.0997	0.9747	1.1858	0.972	0.96346	0.8972	0.56607	0.4864
Adjusted R ²	66%	60%	66%	56%	21%	23%	32%	6%	8%	2%	27%	11%

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

	New Public Enforcement Variables			LLS Public Enforcement Index
	Direct Securities Staff Per Million of Population	Extrapolated Staff Per Million of Population	Extrapolated Budgets Per Billion US\$ of GDP	
Common Law Dummy	10.8522 ^b (4.5484)	12.7553 ^b (5.6317)	54.8851 ^b (22.2256)	0.1589 ^b (0.0658)
Log GNI Per Capita (PPP; 2005)	9.5419 ^a (3.0073)	19.5349 ^a (6.9349)	48.1154 ^a (15.1182)	-0.0212 (0.0389)
Log of GDP(2004)	-2.4459 (1.4998)	-9.8044 ^b (4.2131)	-25.2245 ^b (7.5808)	0.0057 (0.0273)
Constant	-51.2449 ^a (15.8735)	-53.0941 ^b (22.7062)	-96.5469 (144.8871)	0.5908 ^c (0.3308)
N	30	53	41	49
MSE	0.3632	22.491	63.643	0.208
Adjusted R Squared	46%	36%	29%	8%

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

Table 10

Nested Regressions: Dependent Variable Equals 2004 Market Capitalization to GDP

(robust standard errors in parentheses)

	Regressions with Single Explanatory Variable Plus Common Law Dummy and Wealth Control				Regressions with Multiple Explanatory Variables and Controls					
	Only Controls	Primary LLS Variables with Controls			Securities Staff (direct) Per Million of Population	Securities Budgets (Extrapolated) Per Billion US\$ of GDP	LLS Public Enforcement Index	Securities Staff (direct) Per Million of Population	Securities Budgets (Extrapolated) Per Billion US\$ of GDP	LLS Public Enforcement Index
LLS Variables										
Disclosure Index		117.2183 ^a (43.5116)						131.6899 ^a (34.7524)	121.5864 (81.6733)	104.9410 ^b (46.4390)
Liability Standards Index			46.0273 ^b (0.2349)					-78.2616 ^b (36.0114)	-15.6773 (41.7368)	6.9024 (36.2708)
Judicial Efficiency Index				7.4007 (7.5713)						
Enforcement Measures										
Pure Staff to Population					7.0658 ^a (2.1451)			7.6083 ^a (1.9207)		
Extrapolated Budget to GDP						0.5995 ^c (0.3486)		1.1138 ^a (0.2505)		
LLS Public Enforcement Index							61.4709 (55.6835)			43.8805 (71.1391)
Common Law Dummy	62.3708 ^b (29.9276)	20.0157 (30.9040)	48.8097 (30.3984)	49.9404 ^c (25.0268)	-7.2772 (17.0228)	36.6727 (23.4710)	47.9384 ^c (25.2569)	-45.8073 ^a (13.4738)	-39.2255 (33.3407)	15.0503 (29.3961)
Log GNI Per Capita (PPP; 2005)	41.2358 ^a (11.0449)	31.7950 ^a (11.6822)	36.4821 ^a (12.0296)	29.1039 ^c (15.9649)	-9.5124 (11.1770)	34.8177 ^a (9.1936)	41.4534 ^a (11.6465)	-14.037 (8.7515)	27.9555 ^b (12.0417)	33.2878 ^b (13.6704)
Constant	-331.5977 ^a (106.9776)	-293.9367 ^a (109.3703)	-300.5416 ^b (113.9301)	-266.2012 ^b (122.7867)	89.7605 (92.6291)	-311.9594 ^a (88.5502)	-358.2558 ^a (118.7717)	101.0442 (73.4405)	-312.1843 ^a (99.5023)	-325.1259 ^b (146.9373)
N	53	47	47	47	30	40	47	28	34	47
MSE	73.514	74.536	76.677	76.602	51.22	68.566	76.373	46.397	59.773	75.716
Adjusted R Squared	25%	28%	23%	23%	71%	43%	24%	77%	60%	25%

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

Table 11					
Instrumental Variable Regression (via legal origin)					
<i>Panel A: Second-stage Regressions</i>					
Dependent Variable is Market Capitalization to GDP in 2004					
	OLS Regressions		Common Law Instrument		
	Extrapolated Budget to GDP	Direct Staff to Population	IV = Extrapolated Budget to GDP	IV = Direct Staff to Population	IV = LLS Disclosure Index
	(1)	(2)	(3)	(4)	(5)
New Enforcement Variables	1.0189 ^a (0.2689)	6.5111 ^a (2.1120)	1.29368 ^b (0.5382)	6.3981 ^a (1.6819)	
Disclosure Index	53.7035 (47.9492)	47.9951 ^c (27.8719)			178.1757 ^a (67.1598)
Log GNI Per Capita (PPP; 2005)	33.7381 ^a (9.4002)	-6.8462 (9.7350)	15.0081 (21.3276)	-4.7417 (14.1153)	27.6126 ^b (11.2850)
Observations	34	28	40	30	47
Uncentered R Squared	n.a.	n.a.	0.6881	0.8428	0.6515
Cragg-Donald F Statistic	n.a.	n.a.	4.315	9.11	36.576****
<i>Panel B: First-stage Regressions</i>					
Dependent Variables					
	Extrapolated Budget to GDP	Direct Staff to Population	Disclosure Index		
Common Law	52.8283b (25.4311)	10.8988 ^a (3.6109)	0.3284 ^a (0.0543)		
Log GNI Per Capita (PPP; 2005)	28.53631 ^c (15.8914)	7.1451 ^a (1.7573)	0.06861 ^b (0.0268)		
Observations	40	30	47		
Uncentered R Squared	0.5947	0.7038	0.9283		
F-Test for Excluded Instruments	4.32	9.11	36.58		

a = significant at 1%; b = significant at 5 %; and c = significant at 10 %. * = Exceeds Stock Yogo Weak ID Test critical values at 25% maximal IV size; ** = at 20 % maximal IV size; *** = at 15% maximal IV size; and **** = at 10 % maximal IV size.

Table 12					
Instrumental Variable Regressions: Size of Economy (via log GDP)					
<i>Panel A: Second-stage Regressions</i>					
Dependent Variable is Market Capitalization to GDP in 2004					
	OLS Regressions		Log GDP Instrument		
	Extrapolated Budget to GDP	Direct Staff to Population	IV = Extrapolated Budget to GDP	IV = Direct Staff to Population	IV = LLS Disclosure Index
	(1)	(2)	(6)	(7)	-8
New Enforcement Variables	1.0189 ^a (0.2689)	6.5111 ^a (2.1120)	0.2446 (0.3561)	5.1531 ^c (3.1101)	
Disclosure Index	53.7035 (47.9492)	47.9951 ^c (27.8719)			-175.1999 (243.9278)
Log GNI Per Capita (PPP; 2005)	33.7381 ^a (9.4002)	-6.8462 (9.7350)	38.8140 ^b (17.8753)	3.3917 (22.3956)	44.5964 ^b (19.4300)
Observations	34	28	40	30	47
Uncentered R Squared	n.a.	n.a.	0.6687	0.8222	0.2845
Cragg-Donald F Statistic	n.a.	n.a.	9.157***	2.457	3.345
<i>Panel B: First-stage Regressions</i>					
Dependent Variables					
	Extrapolated Budget to GDP	Direct Staff to Population	Disclosure Index		
Log GDP in 2005	-24.7332a (8.1733)	-2.4783 (1.5811)	0.0528 ^c (0.0288)		
Log GNI Per Capita (PPP; 2005)	41.6988 ^b (16.0752)	8.9639 ^a (2.4781)	0.0036 (0.0424)		
Observations	40	30	47		
Uncentered R Squared	0.6372	0.637	0.878		
F-Test for Excluded Instruments	9.16	2.46	3.34		

a = significant at 1%; b = significant at 5%; and c = significant at 10%. * = Exceeds Stock Yogo Weak ID Test critical values at 25% maximal IV size; ** = at 20% maximal IV size; *** = at 15% maximal IV size; and **** = at 10% maximal IV size.