

## Do Differences in Legal Protections Explain Differences in Ownership Concentration?

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A key finding of the law and finance literature is that large shareholders are a response to weak legal protections for investors. The empirical support for this rests with analyses of country averages of ownership concentration. The use of averages, however, creates omitted-variable and aggregation biases. When I analyze firm-level observations and control for firm-level determinates, there is no relation between key measures of investors' legal protections and ownership concentration. A re-examination of the theory of block ownership suggests no reason to expect any relation between investors' legal protections and ownership concentration.

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### I. Introduction

One of the most influential research agendas in recent years explores the relation between law and finance. A focus of this research is to understand why the ownership concentration of public corporations varies around the world. This inquiry, for instance, is central to the seminal paper that launched this line of research, La Porta, Lopez-de-Silanes, Shleifer, and Vishny (LLSV) (1998). Understanding the reasons for cross-country differences in ownership concentration is also central to La Porta, Lopez-de-Silanes, and Shleifer (LLS) (1999, 2006, 2008), Beny (2005), Stulz (2005), Djankov, La Porta, Lopez-de-Silanes, and Shleifer (DLLS) (2008), Li, Moshirian, Pham, and Zein (2006), Roe (2006), and Mueller and Philippon (2008).

All of these papers agree on one key point – large-percentage shareholders in public corporations are a response to weak legal protections for investors. Thus, common law countries have less concentrated ownership than civil law countries because they afford stronger legal protections for investors. Similarly, it is reported that ownership is less concentrated in countries with strong investor protection laws.

To the extent there is debate, it centers around two issues. First, why is ownership concentration inversely related to legal protections? Is it because blockholders substitute for lawsuits as a way to monitor managers, or is it because blockholders can appropriate more corporate resources when minority shareholders have few legal rights to stop blockholders' malfeasance? Second, are there country-level factors in addition to the law that influence ownership concentration?

The fundamental relation between legal protections for public market investors and ownership concentration, however, appears settled. Denis and McConnell summarize the empirical research (2003, p. 30): "Strong legal protection for shareholders appears to be a necessary condition for diffuse equity investment. ... In countries with weak

protection, however, it appears that only ownership concentration can overcome the lack of protection." Burkart and Panunzi (2006, p. 1) similarly write that "empirical studies indicate that ownership is on average more concentrated in countries with poor legal shareholder protection." Perotti and von Thadden (2006, p. 158) note that "concentrated ownership will emerge naturally when investor protection is weak."

In spite of the wide acceptance of the relation between investors' legal protections and ownership concentration, there is some anomalous evidence. Holderness (2009) documents that 96% of a representative sample of U.S. public firms have blockholders who on average own 39% of the common stock. He also finds that the ownership of U.S. firms is similar to and by some measures more concentrated than ownership in other countries. Given that the U.S. has strong investor protection laws, this evidence seems at odds with the theory that large shareholders are a response to weak legal protections. The evidence on long term, within-country changes in ownership concentration also seems at odds with the conventional wisdom. Franks, Mayer, and Rossi (forthcoming) document that in 1900 many U.K. firms had dispersed ownership even though at the time shareholders had few rights. They also report that the introduction of a variety of shareholder protection laws after World War II had little effect on ownership concentration. Holderness, Kroszner, and Sheehan (1999) find that U.S. managerial stock ownership increased significantly between 1935 and 1995, even though over this period legal protections for investors strengthened considerably.

This anomalous evidence leads us to re-examine the empirical support for an inverse relation between investors' legal protections and ownership concentration. This support rests exclusively with analyses of country averages of ownership concentration, even though it is companies and not countries that have large shareholders. Because of the country-average approach, existing papers necessarily ignore firm-level determinants of ownership concentration, even though it is well established that larger firms and older firms have less concentrated ownership. Aggregation also eliminates all within-country variation in ownership concentration. Moreover, country averages weight firms differently, sometimes dramatically so, depending on the country and the

database. Given these considerations, it is not surprising that statisticians have long warned against using aggregate data as the unit of analysis.

The aggregate approach of the existing literature is perplexing because the firm-level data are available; indeed, most papers use firm-level data to form the country averages. I illustrate the problems with inferring firm behavior from country averages by analyzing three measures of investors' protection that are central to the law and finance literature: the rights of shareholders to sue corporate directors (Anti-Director Rights Index of LLSV), a common law legal origin, and legal prohibitions on self-dealing by corporate insiders (Anti-Self-Dealing Index of DLLS). There is a statistically significant inverse relation between each of these measures of investors' legal protection and country averages of ownership concentration. When I use the same data on a firm basis and control for firm size and firm age, in all three instances the relation either reverses sign or becomes insignificant. In these exercises, I use the two ownership databases that underlay existing analyses, and I follow regression specifications from the literature.

Given these empirical findings, I re-visit the theoretical literature that predicts a negative relation between investors' legal protections and ownership concentration. There are two branches to this literature, and they have diametrically opposed views on the organizational role of large shareholders. One branch models external blockholders who monitor management to stop the appropriation of corporate resources. The problem is that around the world blockholders typically are the managers. The other branch models internal blockholders who appropriate corporate resources. Although this comports with the reality that most blockholders are insiders, the problem is that in most countries firm value increases with ownership concentration. Both branches of the theoretical literature ignore the effects of large shareholders on management decisions. Given how broadly large shareholders can impact management and given that management decisions are not subject to judicial review, even in countries with strong laws, there is little reason *ex ante* to expect ownership concentration to vary with investors' legal protections.

The findings in this paper are relevant in two broad respects. Most obviously, they raise questions about the widely held belief that large shareholders are a response to weak legal protections for public market investors. This proposition is supported neither by firm-level evidence nor by the existing theory. The findings in this paper also raise questions about other areas in which aggregate data are used to understand individual phenomenon. The conclusions from these investigations are not necessarily wrong, but our confidence in them should be tempered until they are revisited with individual data.

## II. Problems with Inferring Individual Behavior from Aggregate Data

The empirical support for an inverse relation between the law and ownership concentration rests with the ten papers noted in **Table 1**. This has been an influential body of research. For example, the paper that launched the law and finance literature and first proposed a link between the law and ownership concentration, LLSV (1998), has been more cited than any other economics article published since 1998.<sup>1</sup> Another paper, Stulz (2005), is the Presidential Address to the American Finance Association. All of the analyses in Table 1 are based on country averages. All find that the stronger are the legal protections for public market investors, the lower is the average ownership concentration of public corporations in a given country.

On first impression, it seems reasonable to study the impact of national laws with national averages. If national laws do, in fact, impact ownership concentration, it should be reflected in the national average. Furthermore, it is argued that country averages will highlight any differences among countries, including those caused by national laws.

Upon reflection, however, we should become uneasy with using country averages as the unit of analysis. Countries do not have large shareholders – companies have large shareholders. The goal is to understand why some companies have concentrated

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<sup>1</sup> Kim, Morse, and Zingales (2006).

ownership while others have diffuse ownership. The proper unit of analysis is the company, not the country. We are not trying to understand why some countries have certain investor protection laws. If this were our goal, as it is with some of the law and finance research agenda, the proper unit of analysis would be the country.

Country averages eliminate all within-country variation in ownership concentration, but variation in firm ownership concentration is what we are seeking to understand. Country averages also ignore the number of firms used to calculate a country average, and this typically varies by country and by database. Some country averages consist of many observations; others consist of only a handful of observations. It is hard to imagine how we could be better off by ignoring all of this information.

The potential for aggregate data to paint a misleading picture was first identified by Yule (1903) and was addressed by Robinson (1950, p. 357) in a classic paper. Robinson summarizes: “The relation between ecological [aggregate] and individual correlations which is discussed in this paper provides a definite answer as to whether ecological correlations can validly be used as substitutes for individual correlations. They cannot.” More recently, the distinguished statistician David Freedman (2006, p. 4028) has warned, “It is all too easy to draw incorrect conclusions from aggregate data.”

In this section, I explain why averages can produce misleading inferences. These issues do not mean that relations identified with aggregate data will necessarily turn insignificant or (in rarer cases) change sign with individual data. In fact, a relation identified with aggregate data could, in theory, become stronger with individual data. The key is that aggregate data have the potential to produce misleading inferences about individual behavior.

#### A. How to Control for Firm-Level Influences with Country Averages? The Omitted-Variable Bias

**Figure 1** summarizes the ownership data from Worldscope for 1996. (All data and variables used in the paper are defined in **Table 2**.) Worldscope ownership data have been used in several published papers, including two from Table 1 (Stulz 2005 and Li, Moshirian, Pham, and Zein 2006). These data tell us two things about the ownership

concentration of public corporations around the world. First, they tell us that there are firm-level determinants of ownership. If there were only country-level determinates, such as investors' legal protections, all firms in a given country would have the same ownership concentration. Clearly this is not the case. A possible response is that all within-country variation is simply noise. If this were the case, the large body of research studying within-country variation in ownership would be studies of randomness. This seems unlikely.

The Figure 1 data can also be used to assess the importance of firm-level relative to country-level influences. When I regress ownership concentration on country dummies (not reported), the adjusted  $R^2$  is 0.24. This represents the maximum amount of variation in ownership concentration that can be explained by all country-level factors.<sup>2</sup> Thus, although ownership concentration is influenced both by firm-level and by country-level factors, the firm-level factors seem to be the dominant influence.

When averages are the dependent variable, it is no longer possible to control for firm-level determinates. Aggregation eliminates all within-country dispersion. Indeed, aggregation eliminates all firm-level information. Some readers have suggested stratification as a possible solution; analyze firms with the same characteristics across countries. But this approach is based on firm-level data. Moreover, it is difficult from a practical point-of-view to stratify on multiple firm-level variables, especially when there are many countries. Only two papers in Table 1 use stratification, and then only in robustness tests with small samples and stratifying on firm size alone. Otherwise, the existing literature does not control for firm-level determinates of ownership

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<sup>2</sup> An alternative measure is the intra-class correlation from a variance-component model. This decomposes regression residuals between random country-level effects and within-country effects. This analysis reveals that approximately 0.26 of the total variance in ownership concentration results from across-country as opposed to within-country factors. This figure would be zero if all countries had the same average ownership concentration; it would be one if all firms from the same country had the same ownership concentration.

concentration. This is a natural consequence of using country averages as the unit of analysis.

The omission of firm-level determinates from regressions will not bias the country-level coefficients only when the firm-level determinates are uncorrelated with the country-level determinates of ownership concentration. This is unlikely in any observational study. As Greenland (2001, p. 1346) puts it: “the validity of such interpretations depends on assumptions that natural or social circumstances have miraculously randomized.” Furthermore, firm size varies systematically across countries (Kumar, Rajan, and Zingales 2001) and ownership concentration varies systematically with firm size (Demsetz and Lehn 1985). **Table 3** shows that firm size and firm age are significantly correlated with both the rights of shareholders to sue corporate directors, as measure by the Anti-Director Rights Index of LLSV (defined in Table 2), and ownership concentration. Omission of either of these firm-level controls will bias the country-level coefficient which is our primary interest, the Anti-Director Rights Index.

Omitted-variable biases are an issue with any observational study. The problem is that averages exacerbate omitted-variable biases (Aitkin and Longford 1986). With country averages it is not possible to control for differences among firms. If one goes further and (say) collapses the data into common law versus civil law, it is not possible to control either for heterogeneity among firms or for heterogeneity among nations. As Hanushek, Rivkin, and Taylor (1996, p. 625) document, “problems of omitted variables bias tend to increase along with the level of aggregation.”

### B. The Aggregation Biases of Averages

The within-country dispersion is not the only information that averages discard. Averages also discard the number of observations used to form an average. This creates additional potential problems. These are the aggregation biases of averages.

To illustrate these biases, consider two countries. There are 1,000 randomly chosen observations for one country, but only two similarly chosen observations for the other country. When country averages are the unit of analysis, both countries receive equal

weight. At the highest level, this should concern us because we are much more confident about the population average in one of these countries than in the other country. Although an observation from one firm generally provides the same amount of information as an observation from another firm (which is why most analyses are equal weighted by firm), the average from one country does not necessarily provide the same amount of information as the average from another country.

On a more detailed level, aggregation produces several potential difficulties. First, regression coefficients will change as we move from the regression of averages to the regression of the individual observations underlying those averages whenever averages consist of different numbers of observations. How much the coefficients change depends on how unbalanced the data are. For example, in a regression of averages an individual firm from one of our countries, the country we know less about, is given 500 times the weight of an individual firm from the other country. Given that all of the ownership databases used in the literature are unbalanced, this is an issue with all existing analyses.<sup>3</sup> I illustrate this later with an example involving Venezuela.

If an average consists of a small number of observations, as with one of our hypothetical countries and with several countries in the two extant ownership databases, additional problems arise. One problem is that small clusters tend to overstate differences across countries. Assume, for instance, that our hypothetical countries in fact have identical distributions of ownership concentration. The two observations from one of the countries tell us very little about the underlying distribution. The average of these two observations is likely to be quite different than

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<sup>3</sup> In *Worldscope*, for example, there are 33 Indian firms and 1,567 U.K. firms. Therefore, in a regression of country averages a single Indian firm is given approximately 47 times the weight of a single U.K. firm. If one instead uses the LLSV ownership database (defined in Table 2), the ratio changes to 1.4 to one.

the average from the other country, leading us to conclude that the two countries have very different ownership distributions when in reality they are identical.

Small clusters also tend to be unstable. Drop just one firm or a few firms and an average based upon a small number of firms can change substantially. If a regression has a limited number of observations, which is typical with regressions of country averages because there are only so many countries, changing one average can significantly alter the overall results. I illustrate this in the conclusion with an example from another part of the law and finance literature.

### C. Summary

The problem with using country averages as the unit of analysis is not the averages themselves. After all, averages are one of the building blocks of many basic statistical techniques. The problem is that a country-average approach discards all within-country dispersion and ignores the number of observations. Thus, with a country-average approach it is impossible to calculate, much less use, the standard deviation of within-country ownership. A country-average approach also makes it infeasible to control for firm-level influences on firm-level attributes.

During the course of this research, I searched for papers that justify the use of averages as the unit of analysis when the underlying data are available. I could find no such papers. There are papers, however, that mention the use of averages as the unit of analysis as a theoretical possibility but then dismiss them.<sup>4</sup>

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<sup>4</sup> For example, Bowers and Drake (2005, pp. 305-306) write, "this approach implies that [individual units] are identical within [clusters], and thus the mean provides as much information about y and x as the individual observations do. If this decision is not correct, then the analyst has needlessly thrown away a lot of information – and, more important, no longer has a model of an individual-level process. In the end, analysts must worry that the results of such a model reflect the process of aggregation more than, or instead of, an individual-level process." Gelman and Hill (2007, p. 240) similarly write that using averages as the unit of analysis "remove the ability of individual predictors to predict individual outcomes."

### III. Estimation with Hierarchical Data

We are interested in understanding cross-sectional differences in a firm-level characteristic, ownership concentration, which is affected both by firm-level factors and by higher, country-level factors. In other words, we are interested in how macro factors affect micro units. A broad range of research falls within such a hierarchical or mixed-level framework. The law and finance literature is far from unique. For example, researchers may be interested in how teachers' education (higher level) impacts the test scores of their students (lower level). It is reasonable that test scores will be influenced by a combination of individual-level factors, say a student's age, and higher-level factors, including the education of the student's teacher. This hierarchical setting often arises because of geography, as it does with law and finance. For example, researchers may be interested in how a toxic waste dump in a town affects the health of the town's inhabitants.<sup>5</sup> Likewise, political scientists may be interested in how state voter registration laws affect voter turnout.

#### A. Estimation with No Unmeasured Country Effects

If regression residuals are uncorrelated with each other, if there are no unmeasured group or cluster effects including no unmeasured country effects, we can use pooled OLS to analyze hierarchical data, perhaps with Huber-White standard errors. Some readers have questioned this approach in the law and finance context because with unbalanced panels some countries will receive more weight than other countries. This criticism is misplaced, at least when there is no unmeasured country effect.

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<sup>5</sup> Oakes (2003, p. 1930) makes this point by comparing social epidemiological research to educational research: "The analogous problem for social epidemiologists is to estimate the independent effect of toxic dumps, locally promulgated smoking policies, or inducible increases in social networking, on a neighborhood's health. Both problems share two fundamental characteristics. First, they are typically analyzed with non-experimental (i.e., observational) data. Second, people/students are nested within neighborhoods/schools, which yields a hierarchical data structure where measurements are taken on both individuals and the groups in which they act."

Assume we want to test if ownership concentration is influenced by shareholders' rights to sue corporate directors. Consider two countries that have the same value for the Anti-Director Rights Index, say India and the U.K. Under a pooled OLS approach, an individual Indian firm is given the same weight as an individual U.K. firm. Equal weighting makes sense because each firm constitutes one draw from the distribution of firms whose shareholders have the same rights to sue corporate directors, in this instance a value of five. Under the assumption of no unmeasured country effects, countries become irrelevant once we have measured shareholders' rights to sue directors. To be sure, there are more U.K. firms than Indian firms, perhaps in reality and certainly in the existing databases, but the distinction between the U.K. and India becomes irrelevant once we have measured shareholders' rights to sue corporate directors.

Moreover, the law and finance literature assumes the pooling of firm observations. The underlying theory is that a common legal approach will have a similar effect on the ownership concentration of firms in different countries. If each country is totally unique, pooling is inappropriate but a basic premise of the law and finance literature fails.

### B. Estimation with Unmeasured Country Effects

Now assume there is an unmeasured country effect, which appears to be the case with many of the analyses involving law and ownership concentration.<sup>6</sup> As a consequence, the regression residuals of firms from the same country will be correlated with each other. Moulton (1990) shows that with such an unmeasured cluster effect, the OLS assumptions of independence and homoscedasticity are violated. OLS coefficients

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<sup>6</sup> Petersen (2008, p. 476) suggests that when clustered standard errors are two to four times larger than Huber-White standard errors an unmeasured cluster effect likely is present. In many of the regressions reported later in this paper, country-clustered standard errors are ten times the magnitude of Huber-White standard errors.

will be unbiased, but their standard errors will be underestimated, often dramatically so. This is especially true for higher-level variables, in our case country-level variables.

With an unmeasured country effect, we can still use OLS but need to adjust the standard errors to reflect the fact that each residual does not provide as much information as we had originally thought. The typical way this is done, at least in finance, is to cluster standard errors, in our case by country.<sup>7</sup> This technique is appropriate if ownership regression residuals are correlated within a country but uncorrelated across countries. This approach keeps the OLS coefficients unchanged, but adjusts the standard errors.

#### IV. Revisiting Aggregate Findings with Individual Data

Although averages have the potential to produce misleading results, it does not mean they have produced misleading results in the context of law and ownership. I address this issue by revisiting three key findings from literature with individual data. To keep the focus on the implications of using aggregate data to understand individual phenomenon, I hold other factors constant by following the existing literature when possible. I start by estimating the relation between each of the three measures of investors' legal protection and ownership concentration using country averages. I use the two publicly available ownership databases that underlay the Table 1 papers.<sup>8</sup> I control for the level of a country's wealth with the natural log of per capita GDP and for the judicial enforcement of laws with the natural log of how long it takes to litigate a bounced check (DLLS 2008). Both controls are common in the literature. Again, to parallel the existing literature as much as possible, I borrow the data for these controls from previously published papers.

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<sup>7</sup> Petersen (2008).

<sup>8</sup> Two papers from Table 1, LLS (1999) and Roe (2006), use a database of the 20 largest nonfinancial firms from 27 wealthy countries. To my knowledge, this database is not publicly available.

After estimating a relation using country averages, I re-estimate the same relation using the same data but on a firm basis rather than a country-average basis. Because I am using firm observations, I am able to control for firm-level attributes. I consider the two factors that existing research most clearly shows are related to ownership concentration, firm size and firm age. I conduct these analyses within the standard cross-sectional model:

$$y_i = a + \beta'x_i + \delta'c_i + \varepsilon_i$$

where  $y_i$  is firm  $i$ 's ownership concentration,  $x_i$  is a set of firm-level variables for firm  $i$  (such as firm size),  $c_i$  is a set of country-level variables applicable to firm  $i$  (such as the rights shareholders have to sue directors), and  $\varepsilon_i$  is an error term. I estimate this relation with OLS using standard errors clustered by country.

Anti-Director Rights Index. One of the key contributions of the law and finance literature has been to quantify the legal protections for public market investors around the world. This has made possible the impressive body of empirical research into cross-country differences in financial markets over the past decade. Among the various indices of investors' statutory protections, the most widely used is the Anti-Director Rights Index. It was introduced in the paper that launched the law and finance literature (LLSV 1998) and has been used in well over 100 published papers.<sup>9</sup> The index is a composite of six rights shareholders have to bring legal action against corporate directors.

**Table 4** reports regressions of ownership concentration from Worldscope on the Anti-Director Rights Index. This is one of two databases that has been used in published papers (Table 1). When we regress country averages of ownership concentration on the Anti-Director Rights Index (Regressions 1 and 2), the coefficient on the Index is negative and significant. This is what the existing literature finds.

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<sup>9</sup> Spamann (2008).

When we conduct the same regression but with the underlying firm-level data, the coefficient on the Anti-Director Index actually changes sign and is no longer statistically significant. The difference between Regression 1 (or 2) and Regression 3 (or 4) results solely from differences in the weighting of firm observations. If all countries had the same number of observations, these coefficients would not change. These results are particularly sensitive to Venezuela. With country averages, the high ownership concentration and low Anti-Director Rights of Venezuela is one observation out of 45. With firm observations only two of 14,839 observations are Venezuelan. There are, of course, other differences, but this admittedly pronounced difference in weighting illustrates why coefficients change between aggregate and individual data even when there are no firm-level effects.

Venezuela also illustrates the potential unreliability of country averages. There are only two Venezuelan observations from a national population of public corporations which at the time was in excess of one hundred.<sup>10</sup> How confident can we be of a population average with only two observations? If we remove Venezuela from the regression of country averages, the coefficient on the Anti-Director Rights Index in Regression 2 falls from  $-3.65$  ( $p$ -value 0.05) to  $-2.94$  ( $p$ -value 0.11). All of these findings illustrate the aggregation bias with averages.

Results change further when we add firm-level effects in Regressions 5 and 6. (Venezuela remains in all Table 4 regressions.) These changes reflect that ownership concentration is a function of firm-level factors as well as country-level factors. This is an example of the omitted-variable bias of averages. Once we consider firm size and firm age in Regression 6, the coefficient on the Anti-Director Rights Index remains positive and approaches marginal significance ( $p$ -value 0.11). This example should not be taken to imply that coefficients always reverse sign between aggregate and

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<sup>10</sup> Campbell (2005) reports that as of December 31, 1996 there were three stock exchanges in Venezuela. The largest exchange alone had 91 listed firms.

individual data. Typically, they do not. But they do change sign sufficiently often that a term has been coined for it, Simpson's Paradox.

Some readers have raised questions about Table 4 because the countries in these analyses have different numbers of observations; we have an unbalanced panel. This raises the possibility that the results may be unduly influenced by those countries with many observations. To address this possibility, I limit the observations from any country to 300. This affects eight countries. After this change, the coefficient on the Anti-Director Rights Index is 0.28 ( $p$ -value 0.91). I realize this cut-off is somewhat arbitrary, so I experimented with other cutoffs.<sup>11</sup> In all instances we can not reject the hypothesis that ownership concentration is unrelated to the rights of shareholders to sue corporate directors.

Common Law. The difference between common law and civil law countries is central to the law and finance literature. The common law had its origins in the English common law courts. It relies on individual judges to adjudicate disputes. As these precedents accumulate, they begin to have the force of law. The common law spread around the world as the British Empire expanded. It is the legal foundation not only for the United Kingdom, but for countries such as India, Australia, and the United States (with the exception of Louisiana which follows the Napoleonic code). The civil law is older, having its origins in Roman law. In contrast to the common law, it is heavily codified and makes extensive use of legal experts to formulate rules. The civil law is most frequently associated with Napoleon, who imposed it on France in 1803 after the chaos of the French Revolution and then throughout much of continental Europe with his military conquests. Related versions of the civil law developed subsequently in Germany and Scandinavia.

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<sup>11</sup> I alternatively limited the number of (randomly selected) observations from any country to 1,000, 500, 200, 100, and 50.

Common law countries typically offer more legal protections and better enforcement than do civil law countries (LLSV 1998, LLS 2008). Certainly, the simplest and arguably the best test of the theory that large shareholders are a response to poor legal protections for investors is to determine if ownership is less concentrated in firms from common law countries. One advantage of this inquiry is that most countries adopted their legal traditions, or had them imposed through colonization or military conquest, long before the advent of the modern public corporation. Thus, blockholders could have had little, if any, influence over the choice of a country's legal origins, so reverse causation should not be a problem.

To investigate the impact of a common law legal origin, I turn to the other database that underlies the existing literature, the LLSV database of the ten largest non-financial firms from 47 countries. The authors have graciously made the firm-level data available to me. We see in Regression 1 of **Table 5** that with country averages common law countries have less concentrated ownership than civil law countries. This seemingly confirms a basic tenet of the law and finance literature.

By moving from regressions of averages (Regression 1 and 2) to regressions of individual observations (Regression 3 and 4), the estimated coefficient on the common law dummy changes, but not as dramatically as with Anti-Director Rights Index and the Worldscope data. The reason is that the LLSV sample is more balanced. (Most countries have ten observations, but 11 countries have fewer observations.) If the sample were perfectly balanced, the coefficients would not change with the firm observations.

When we add firm-level controls, the relation between legal origins and ownership concentration weakens further. Overall, the coefficient on the common law dummy goes from -6.91 and a  $p$ -value of 0.06 (Regression 1) to -2.99 and a  $p$ -value of 0.36 (Regression 6). We can not reject the hypothesis that common law firms have the same ownership concentration as civil law firms.

Anti-Self-Dealing Index. The reader should be aware that the regressions in Table 4 and Table 5 are not exact replications of specific regressions from the literature. Rather, they illustrate how aggregate data can produce misleading results in the context of key indices of investors' rights and widely used ownership databases. I conclude by replicating the most recently published regression involving investors' legal protections and ownership concentration.

LLS (2008) review the law and finance literature. In so doing they use the Anti-Self-Dealing Index of DLLS (2008) to document an inverse relation between the law and ownership concentration. It is the only legal measure they use in the context of ownership concentration. LLS's ownership regression is exactly replicated in Regression 1 of **Table 6**. The coefficient on the Anti-Self-Dealing Index is negative and marginally significant ( $p$ -value 0.08).

When I reproduce that specification with the underlying firm data, however, the coefficient for the Anti-Self-Dealing Index declines and loses any statistical significance ( $p$ -value 0.15) (Regression 2). After I control for firm size and age, the coefficient on the Anti-Self-Dealing Index declines further (Regressions 3 and 4). The results are more pronounced if we control for the quality of judicial enforcement by using the log of how long it takes to litigate a bounced check. When that variable is added to the regressions (not reported in Table 6), the coefficient on the Anti-Self-Dealing Index in the most complete specification falls to  $-2.92$  ( $p$ -value 0.65). Once again, in all cases with individual data we can not reject the hypothesis that ownership concentration does not vary with the legal prohibitions on self-dealing by corporate insiders.

Summary. These three examples illustrate how aggregate data can produce misleading results. There undoubtedly are some legal measures that are negative and significant with both aggregate and individual ownership data. But the centrality of these three measures of investors' legal protections and the fact that the data in these analyses underlay all published studies to date call into question the existence of a relation between key legal protections for public market investors and ownership

concentration. More broadly, Tables 4–6 caution that the impact of country factors on firm characteristics can not be reliably estimated with country averages.

An earlier version of this paper (Holderness 2007) casts further doubts on a relation between a broad array of investors' legal protections and ownership concentration. Because of issues involving the accuracy and representativeness of the existing databases, I use two different databases of hand-collected ownership data involving 9,491 firms from 32 countries. I also examine all fifteen measures of investors' legal protections that have been proposed in the literature, the common-law civil-law division and fourteen indices of statutory protections. This helps ensure that we will not be data mining by highlighting those indices that happen to be negative while ignoring those indices that happen to be positive. When I use firm data and control for firm-level determinates of ownership, the coefficient on the investor protection measure is negative in 14 cases and positive in 16 cases. Most are statistically insignificant. Those that are statistically significant are as likely to be positive as negative. When a particular index is negative and significant with one database, it is never negative and significant with the other database.

## V. Possible Reasons for Country Averages as the Unit of Analysis

The unreliability of aggregate data has been established at least since Robinson's seminal article in 1950. Firebaugh (2006, p. 4024) writes that "it would be difficult to overstate the impact Robinson's article has had on social science research during the second half of the twentieth century. The use of ecological correlation to study individual-level observations had been commonplace before Robinson's article, and the article sharply curtailed that practice." Nevertheless, some papers continue to use country averages, even though the goal is to understand firm-level behavior and even though the firm-level data are available. This is the universal practice in cross-country studies of ownership concentration. It is also the practice in some, but certainly not all, other topics in law and finance and beyond, as documented in **Table 7**.

Given the potential omitted-variable and aggregation biases of averages, one would expect papers to explain the use of country averages instead of firm observations. But none do so. Readers and seminar participants, including authors of some of the papers listed in Table 1 and Table 7, however, have suggested possible advantages of using averages as the unit of analysis. These explanations seem reasonable at first impression. But upon reflection, they lose force. In fact, averages exacerbate some of the problems they were supposed to control. Here are the three most frequent justifications I have received for using country averages as the unit of analysis.

Using Country Averages to Understand Country Factors. Some readers suggest that country averages are appropriate if one is interested in country-level factors, while individual observations are appropriate if one is interested in firm-level factors. It is fine to focus on country-level determinants, but the problem is that the ownership data reflect the interaction of both country-level and firm-level effects on ownership concentration. As Greenland (2001, p. 1343) explains: “by definition, ecologic [aggregate] data contain only marginal observations on the joint distribution of individually defined confounders and outcomes, and so identify neither contextual [higher level] nor individual-level effects.” Accordingly, even if there are large differences in ownership concentration across countries, one does not know whether these differences are caused by country-level factors (some countries offer stronger legal protections) or by firm-level factors (some countries have larger firms). Given that this is an observational study, the only way to remove the firm-level effects, so the focus can be on the country-level effects, is to control for firm-level influences. But the only reliable way to do this is with firm-level data (Tables 4–6).

Avoid Data Cloning. Another suggested benefit of country averages is that they avoid data cloning. The Oxford English Dictionary defines cloning as “To propagate or reproduce (an identical individual) from a given original.” Recall that the unit of analysis for our purposes is (or should be) the individual firm. When firm observations of ownership concentration are used, there is no cloning because firms have different, sometimes very different, ownership structures (Figure 1). It is true that the assumption

of a country effect means applying the same (scalar) factor equally to all firms within a given country. But this is the inherent claim made by the existing literature with country-level factors.

Addressing Within-Country Correlations of Regression Residuals. The most frequent justification for using country averages is that they eliminate unmeasured country effects. Interdependencies among firms from the same country are possible, even likely, perhaps reflecting an unmeasured influence of social norms. This is a common problem with any observational study involving panel data, and we have panel data with the panels being countries. To deal with this problem it is not necessary to take averages and thus eliminate much of the cross-sectional variation in ownership concentration, the very thing we are attempting to explain. Clustered standard errors were developed to deal specifically with this problem. As Petersen (2008, p. 440) notes, “The correlation of the residuals within a cluster is the problem the clustered standard errors are designed to correct.”

Summary. There is a debate among statisticians over the reliability of inferences drawn from aggregate data. Even a leading proponent of using aggregate data, however, acknowledges that “if one can avoid making inferences about individuals from aggregate data, then one should do so. And of course, valid survey data make ecological inferences superfluous.”<sup>12</sup>

## VI. The Theory of Law and Ownership Reconsidered

Given the empirical findings in this paper, it is appropriate to revisit the “theory [that] shows that ownership concentration is lower in countries with better investor protection.” (DLS 2008, p. 444) There are two branches to this theoretical literature. Both predict a negative relation between investor protection laws and ownership concentration, but they have diametrically opposed views on the role of large

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<sup>12</sup> King (1999) p. 352.

shareholders in public corporations. This fundamental inconsistency is seldom noted and must give one pause for thought, independent of our failure to find empirical support for the claimed relation with key measures of investor protections.

One branch of the theoretical literature views large shareholders as outsiders who monitor managers to halt the appropriation of corporate resources by corporate insiders.<sup>13</sup> Under this view shareholder lawsuits and blockholder monitoring are substitutes. When shareholders have few rights to sue managers, the value of a blockholder who can monitor managers increases and so does ownership concentration. A fundamental problem with this theory is that around the world most large shareholders are managers, not outside monitors.<sup>14</sup> There is little reason to expect these blockholders to monitor themselves. Monitoring implies an external constraint.

The other branch of the theoretical literature views large shareholders as insiders who appropriate corporate resources. Under this very different view of the corporate world, the frequency of blockholders increases as legal constraints decline because it becomes easier for them to appropriate corporate resources.<sup>15</sup> Here blockholders are complements to weak legal protections for (other) shareholders. Although this branch of the literature comports with the reality that most blockholders are insiders and thus in a position to appropriate corporate resources, a fundamental problem is that the evidence in most countries is that firm value typically increases with ownership concentration.<sup>16</sup>

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<sup>13</sup> LLSV(1998), DLLS (2008).

<sup>14</sup> The involvement of blockholders in management has been documented by: LLS (1999) 27 wealthy countries; Faccio and Lang (2002) 13 Western European countries; Claessens, Djankov, and Lang (2000) 9 East Asian countries; Lins (2003) 18 emerging markets countries.

<sup>15</sup> Zingales (1995), Bebchuk (1999), Shleifer and Wolfenzon (2002), Perotti and von Thadden (2006).

<sup>16</sup> Denis and McConnell (2003). The notable exception is the U.S. where there appears to be little relation between ownership concentration and firm value. Demsetz and Villalonga (2001).

Although investor protection laws have the potential to limit the appropriation of corporate resources, they will have little influence on management decisions (Roe 2002). In the United States, for instance, management decisions are not subject to judicial review because of the business judgment rule. Large shareholders, however, will influence management decisions because of their voting power and incentives. These effects are ignored in theoretical analyses that focus exclusively on the appropriation of corporate resources.<sup>17</sup> The sole theoretical paper that considers blockholders' impact on management decisions and not just on the appropriation of corporate resources, Burkart and Panunzi (2006), concludes there is no *ex ante* reason to expect any relation between investors' legal protections and ownership concentration.

## VII. Conclusion

This paper offers two primary contributions. One is methodological, the other substantive. The methodological contribution is that when we seek to understand firm-level attributes, firm-level data are preferable to country averages. This is the more perplexing of the paper's contributions because the omitted-variable and aggregation biases of aggregate data have long been known.<sup>18</sup> Aggregation eliminates all within-group (country) variation, leading to artificial clustering. Nevertheless, all papers seeking to understand cross-country differences in ownership concentration analyze

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<sup>17</sup> Although the role of large shareholders in management decisions is ignored in the theoretical literature on cross-country differences in ownership concentration, it plays a central role in other theoretical papers. Grossman and Hart (1980), for instance, analyze the efforts of an insurgent large-percentage shareholder to gain control of a firm. The insurgent would be a better manager in that he would produce higher profits than the current manager. Shleifer and Vishny (1986) analyze a large shareholder who pushes for a value-enhancing merger or negotiates informally with incumbent management for operational changes that likewise enhance firm value.

<sup>18</sup> A news article, for example, explains the pitfalls of using aggregate data to make predictions about the recidivism rate of individuals. It notes that this problem "is not peculiar to psychology. It has been recognized by statisticians for decades. They call it the ecological fallacy ..." *The Economist*, "Crime prediction: The jailer's dilemma," June 23, 2007, p. 90.

country averages even though they seek to make firm-level inferences and even though the firm-level data are available.

I illustrate the unreliability of aggregate data to infer individual behavior by using three measures of investors' legal protections and the two ownership databases that underlay the existing literature. These three legal measures are central to the law and finance literature – one is the most frequently used of all of the measures of investors' legal protections (Anti-Director Rights Index); another is perhaps the most basic measure of investors' legal protections (the common-law civil-law distinction); the third is the most recent measure of investors' legal protection (Anti-Self-Dealing Index). There is a negative and statistically significant relationship between country averages of ownership and each of these three measures. But when we use firm-level data and control for only two firm-level determinants of ownership concentration, the coefficients on the legal measures either change sign or lose statistical significance. In all three cases we can not reject the hypothesis that there is no relation between the law and ownership concentration.

A re-examination of the theory of law and ownership reveals inconsistencies and a narrow focus. Some theories assume that large shareholders halt the appropriation of corporate resources; other theories assume that large shareholders appropriate corporate resources. All theories focus exclusively on the appropriation of corporate resources to the exclusion of the effects of large shareholders on management decisions. Management decisions are not subject to judicial review, even in countries with strong investor protection laws. Laws may protect shareholders from theft, but they offer no protection from bad management. Thus, there is little *ex ante* reason to expect the incidence of large shareholders to vary with investor protection laws.

The findings in this paper suggest several avenues for future research. I will note two. One would be to understand why ownership concentration varies around the world. After controlling for firm-level determinants of ownership concentration, a substantial country component remains. The empirical evidence and theoretical review in this paper raise doubts about whether cross-country differences in ownership reflect

cross-country differences in investors' legal protections. At the least, the findings in this paper tell us that the theoretical basis and empirical support for a relation between the law and ownership concentration has not yet been reliably established. An alternative avenue would be to investigate if culture affects ownership concentration around the world. Several papers, including Stulz and Williamson (2003) and Guiso, Sapienza, and Zingales (2006), find that other aspects of finance vary with culture.

The findings in this paper also suggest the benefits from re-examining other firm-level topics that have been studied with country averages. The first task is to identify which topics are properly addressed with firm-level data and which with country-level data. One focus of the law and finance literature is to understand why some countries have strong investor protection laws. Here using one observation for each country is appropriate. National governments enact investor protection laws.

At other times the appropriateness of an individual as opposed to a country approach is not as obvious. This can be illustrated with another widely used measure of the development of financial markets, the ratio of the market value of exchange-listed firms to GDP. Countries do not decide to become publicly listed – firms decide to become publicly listed. The unit of analysis is, or should be, the firm. In the United States, for instance, why are companies like Bechtel or Cargill not public in light of the strong investor protection laws? It must have to do with firm-level characteristics. We can think of a probit model of the decision of an individual firm to go public with both firm-level controls, say profitability, and country-level controls, say investor protection laws.<sup>19</sup> A cross-country analysis can focus on the latter, but should include the former for the reasons discussed in this paper.

Several papers, including LLSV (1997) and LLS (2008), document that common law countries have a higher ratio of listed firms to GDP than do civil law countries. Rajan

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<sup>19</sup> There is evidence that firms with certain characteristics are more likely to go public, for example, Pagano and Roell 1998 or Ritter and Welch 2002.

and Zingales (2003) present evidence to the contrary. LLS (2008) criticize Rajan and Zingales by highlighting attributes of some of the firms in the latter's sample. One of the listed Cuban companies, Havana Electric, was headquartered in the United States, and Rajan and Zingales do not control for this heterogeneity. This, of course, is a firm-level characteristic, and it is difficult to control for firm heterogeneity with country averages. This is an example of the omitted-variable bias with aggregate data.

LLS further note that Rajan and Zingales's Cuban sample has few observations. Delete Havana Electric and both the Cuban average and the regression results of the importance of legal origins change considerably. This illustrates the instability of country averages and is reminiscent of the analysis in this paper of the strong influence of just two Venezuelan firms in a regression of country averages. This is an example of the second bias with aggregate data, the aggregation bias.

LLS (2008, p. 318) write: "What is beyond doubt, however, is that strong conclusions reached by Rajan and Zingales on comparative financial development cannot be drawn from their sample." This would not be surprising given the unreliability of aggregate data. The same point can also be made of the law and ownership literature. The basic point of the present paper is that the omitted-variable and aggregation biases of aggregate data make inferences about firm characteristics unreliable. This appears to be the case with the widely held proposition that large shareholders are a response to weak legal protections for public market investors. It is supported neither by firm-level empirical analyses nor by existing theory.

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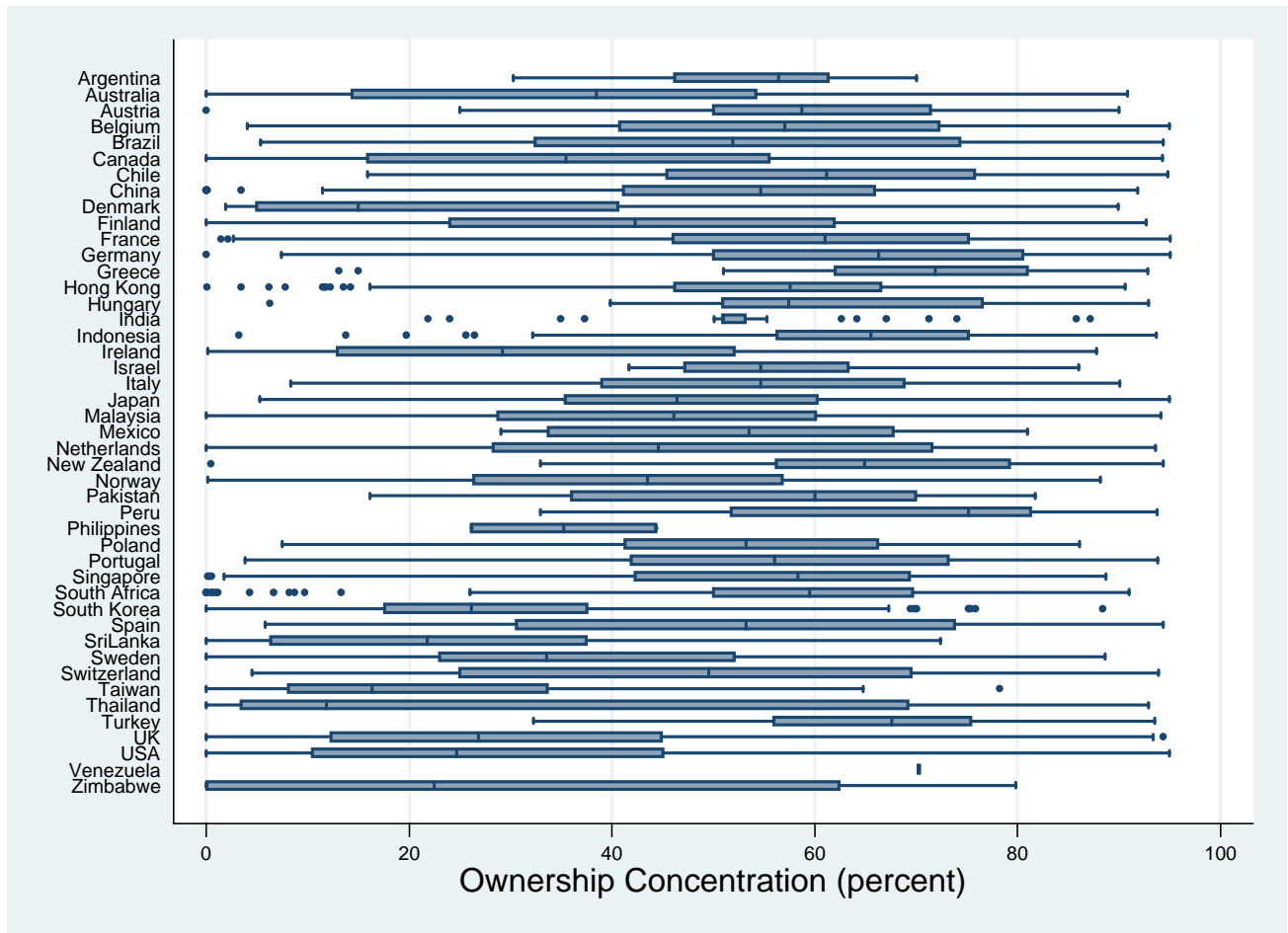
Table 1

## Summary of the Literature on Ownership Concentration around the World

	Analyses of Country Averages Only	Firm-Level Controls in Regressions	Conclusion
La Porta, Lopez-de-Silanes, Shleifer, and Vishny 1998	Yes	None	Ownership concentration is lower in common law countries and when shareholders have strong legal rights to sue corporate directors (Anti-Director Rights Index, Original).
La Porta, Lopez-de-Silanes, and Shleifer 1999	Yes	No Regressions*	Same as above.
Beny 2005	Yes	None	Concentration is lower in countries with strong legal prohibitions on insider trading.
Stulz 2005	Yes	None	Concentration is lower when the threat of expropriation by the government is lower and when shareholders have strong legal rights to sue corporate directors (Anti-Director Rights Index, Original).
La Porta, Lopez-de-Silanes, and Shleifer 2006	Yes	None	Concentration is marginally lower with disclosure requirements of securities laws; after controlling for disclosure, the Anti-Director Rights Index, Original is found to be unrelated to ownership concentration.
Li, Moshirian, Pham, and Zein 2006	Yes	None	Financial institutions own more stock as Anti-Director Rights Index, Original increases.
Roe 2006	Yes	None*	Ownership concentration is lower when shareholders have strong rights to sue corporate directors (Anti-Director Rights Index, Original) and when the country was at peace during the 20 <sup>th</sup> century.
Mueller and Philippon 2008	Yes	None	Families own less stock when national labor relations are good and when the Anti-Director Rights Index, Original increases.
Djankov, La Porta, Lopez-de-Silanes, and Shleifer 2008.	Yes	None	Concentration is lower with ex-post private controls on self-dealing by corporate insiders; this Anti-Self-Dealing Index is seen as superior to the Anti-Director Rights Index, Revised.
La Porta, Lopez-de-Silanes, and Shleifer 2008	Yes	None	Ownership concentration is lower with the Anti-Self-Dealing Index.

\* Some analyses are conducted only with medium-sized firms.

Figure 1



Ownership concentration at public corporations from 45 countries. Data come from Worldscope for 1996 and are defined in Table 2. Countries are included if data on the rights of shareholders to sue corporate directors are available (Anti-Director Rights Index). The box represents the 25<sup>th</sup> and 75<sup>th</sup> percentiles. The ends of the whiskers extend to the minimum and maximum values or to 1.5 times the inter-quartile range, whichever is closer to the box. Values outside of this range are represented by dots.

Table 2  
Description of Data and Variables Used in Paper  
Firm-Level Variables

Variable	Description	Source
Ownership Concentration (LLSV)	<p>“Average percentage of common shares owned by the top three shareholders in the ten largest non-financial, privately-owned domestic firms in a given country. A firm is considered privately-owned if the State is not a known shareholder in it. Source: La Porta et al. (2006)” Shareholders are not required to own any minimum percentage stock to be included in this measure.</p>	<p>LLS (2006). The country averages come from Andrei Shleifer’s website accessed on March 4, 2009. The underlying firm-level data come directly from the authors. The data come from the period 1995-1996, with the exception of Ecuador (2000), Uruguay (1998-2000) and Jordan (1999). (Private communications with the authors.) The only change to the firm-level data involves the Austrian firm Jenbacher Werke. That firm was included twice with an ownership concentration of 51%. I deleted one of these duplicate observations.</p>
Ownership Concentration (Worldscope)	<p>Worldscope item WC05475. “It represents shares held by insiders. For Japanese companies closely held represents the holdings of the ten largest shareholders. For companies with more than one class of common stock, closely held shares for each class are added together. It includes but is not restricted to: shares held by officers, directors, and their immediate families; shares held in trust; shares of the company held by any other corporation; shares held by pension/benefit plans; shares held by individuals who hold 5% or more of the outstanding shares. It excludes: shares under option exercisable within 60 days; shares held in a fiduciary capacity; preferred stock or debentures that are convertible into common shares.” To correct obvious data errors and to exclude firms that are effectively privately held, I drop firms for which this data item is greater than 95%.</p>	<p>Thomson Financial Datastream. Data is from December 31, 1996.</p>

Table 2 (continued)  
Firm-Level Variables (continued)

Firm Size	The natural log of the market value of the firm's equity. Datastream item WC07210. "The total market value of the company based on year end price and number of shares outstanding converted to U.S. dollars using the year end exchange rate. For companies with more than one type of common/ordinary share, market capitalization represents the total market value of the company." If data item WC07210 is unavailable, I use the alternative data items MVC and MVCU, in that order.	Thomson Financial Datastream. The data are as close to December 31, 1996 as possible. If the data do not come from 1996, they are converted to 1996 dollars with the GNP Deflator. If Firm Size is missing (see below), a value of zero is assigned.
Missing Firm Size	Takes a value of one if Firm Size is missing and zero otherwise.	If Firm Size is unavailable for all years between 1992 and 2000, it is recorded as missing.
Firm Age	The natural log of the number of years since incorporation as of 1996. Datastream item WC18273. "Date of Incorporation represents the date the company was incorporated."	Thomson Financial Datastream. The data are as close to December 31, 1996 as possible. For LLSV firms with missing Datastream data, I searched company websites for information on the date of incorporation. If Firm Age is missing (see below), a value of zero is assigned.
Missing Firm Age	Takes a value of one if Firm Age is missing and zero otherwise.	If Firm Age is unavailable for all years between 1992 and 2000 or if it is unavailable after Internet research, it is recorded as missing.

Table 2 (continued)

Indices of Country-Level Legal Protections for Investors

Variable	Description	Source
Anti-Director Rights Index	"Aggregate index of shareholder rights. The index is formed by summing: (1) vote by mail; (2) shares not blocked or deposited; (3) cumulative voting; (4) oppressed minority; (5) pre-emptive rights; and (6) capital." The Anti-Director Rights Index was first proposed in LLSV 1998. The revised and most current version of the Index DLLS (2008) is used.	Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008). These data come from Andrei Shleifer's website accessed on March 4, 2009.
Common Law Legal Origin	Takes a value of one if a country has a common law legal tradition and zero otherwise.	La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998). These data come from Andrei Shleifer's website accessed on March 4, 2009.
Anti-Self-Dealing Index	"Average of ex-ante and ex-post private control of self-dealing."	Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008). These data come from Andrei Shleifer's website accessed on March 4, 2009.
Ex-Ante Private Enforcement	"Index of ex-ante control of self-dealing transactions. Average of approval by disinterested shareholders and ex-ante disclosure."	
Ex-Post Private Enforcement	"Index of ex-post control over self-dealing transactions. Average of disclosure in periodic filings and ease of proving wrongdoing."	

Ancillary Country-Level Control Variables

Variable	Description	Source
Per Capita GDP	The natural logarithm of "GDP per capita in Purchasing Power terms – in 1994 – World Development Indicators."	La Porta, Lopez-de-Silanes, and Shleifer (2008). These data come from Andrei Shleifer's website accessed on March 4, 2009. I choose this particular measure of per capita GDP so I could replicate the regression of ownership concentration on the Anti-Self-Dealing Index reported in LLS (2008, p. 295, Panel B).
Judicial Enforcement	"Logarithm of the length (in calendar days) of the judicial procedure to collect on a bounced check."	Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008). These data come from Andrei Shleifer's website accessed on March 4, 2009.

Table 3  
Correlation Matrix

Pearson correlation coefficients among country-level and firm-level variables associated with the ownership concentration of public corporations. The data come from Worldscope and are defined in Table 2. There are 4,595 firms. (*p*-values are in parentheses.)

	Ownership Concentration	Anti-Director Rights Index	Firm Size	Firm Age
Ownership Concentration	1.00			
Anti-Director Rights Index	0.14 (0.00)	1.00		
Firm Size	-0.24 (0.00)	-0.09 (0.00)	1.00	
Firm Age	-0.10 (0.00)	0.27 (0.00)	0.09 (0.00)	1.00

Table 4

Anti-Director Rights Index Regressions with Country Averages and Firm Observations

Regressions of ownership concentration on the rights of shareholders to sue corporate directors as measured by the Anti-Director Rights Index. The ownership data are the same in all regressions and come from Worldscope. Regressions 1–2 use country averages of ownership concentration. These Regressions are estimated with OLS using Huber-White robust standard errors. Regressions 3–6 use the underlying, firm observations. These Regressions are estimated with OLS using standard errors that are clustered by country. The data and variables are defined in Table 2. (*p*-values are in parentheses.)

	Country Averages		Firm Observations			
	1	2	3	4	5	6
<b>Anti-Director Rights Index</b>	<b>-3.70</b> <b>(0.01)</b>	<b>-3.65</b> <b>(0.05)</b>	<b>2.13</b> <b>(0.53)</b>	<b>2.32</b> <b>(0.51)</b>	<b>2.71</b> <b>(0.49)</b>	<b>4.54</b> <b>(0.11)</b>
Per Capita GDP	-1.65 (0.50)	-1.62 (0.49)	-10.11 (0.00)	-6.64 (0.06)	-6.57 (0.08)	-6.75 (0.03)
Judicial Enforcement		0.14 (0.96)		5.91 (0.14)	5.76 (0.16)	6.00 (0.08)
Firm Size					-2.42 (0.00)	-2.42 (0.00)
Firm Age						-3.56 (0.00)
Missing Firm Age						-0.09 (0.99)
Constant	77.21 (0.00)	76.01 (0.01)	131.08 (0.00)	70.13 (0.19)	81.44 (0.15)	78.28 (0.08)
Adjusted <i>R</i> <sup>2</sup>	0.06	0.10	0.05	0.07	0.10	0.14
Observations	45	45	14,839	14,839	14,839	14,839

Table 5

## Common Law Legal Origin Regressions with Country Averages and Firm Observations

Regressions of ownership concentration on a dummy variable that takes the value of one if a country has a common law legal origin and zero otherwise. The ownership data are the same in all regressions and come from LLSV. Regressions 1–2 use country averages of ownership concentration. These Regressions are estimated with OLS using Huber-White robust standard errors. Regressions 3–6 use the underlying, firm observations. These Regressions are estimated with OLS using standard errors that are clustered by country. The data and variables are defined in Table 2. (*p*-values are in parentheses.)

	Country Averages		Firm Observations			
	1	2	3	4	5	6
<b>Common Law Legal Origin</b>	<b>-6.91</b> <b>(0.06)</b>	<b>-4.90</b> <b>(0.19)</b>	<b>-5.31</b> <b>(0.15)</b>	<b>-3.35</b> <b>(0.37)</b>	<b>-2.98</b> <b>(0.37)</b>	<b>-2.99</b> <b>(0.36)</b>
Per Capita GDP	-6.30 (0.00)	-4.93 (0.01)	-6.64 (0.00)	-5.21 (0.01)	-2.54 (0.17)	-2.00 (0.29)
Judicial Enforcement		5.38 (0.04)		5.04 (0.05)	3.00 (0.21)	3.05 (0.20)
Firm Size					-2.41 (0.00)	-2.21 (0.00)
Missing Firm Size					-11.51 (0.06)	-10.32 (0.08)
Firm Age						-2.22 (0.21)
Missing Firm Age						-3.39 (0.72)
Constant	107.07 (0.00)	65.88 (0.01)	109.24 (0.00)	69.32 (0.00)	70.39 (0.00)	72.15 (0.00)
Adjusted $R^2$	0.17	0.22	0.07	0.09	0.12	0.13
Observations	49	49	446	446	446	446

Table 6

Anti-Self-Dealing Index Regressions with Country Averages and Firm Observations

Regressions of ownership concentration on legal prohibitions on self-dealing by corporate insiders as measured by the Anti-Self-Dealing Index. The ownership data are the same in all regressions and come from LLSV. Regression 1 uses country averages of ownership concentration. This Regression is estimated with OLS using Huber-White robust standard errors. This replicates the ownership regression reported in LLS (2008, p. 295, Panel B). Regressions 2–4 use the underlying, firm observations. These Regressions are estimated with OLS using standard errors that are clustered by country. The data and variables are defined in Table 2. (*p*-values are in parentheses.)

	Country Averages	Firm Observations		
	1	2	3	4
<b>Anti-Self-Dealing Index</b>	<b>-12.77</b> <b>(0.08)</b>	<b>-10.02</b> <b>(0.15)</b>	<b>-5.82</b> <b>(0.35)</b>	<b>-5.84</b> <b>(0.33)</b>
Per Capita GDP	-4.95 (0.02)	-5.61 (0.00)	-2.38 (0.21)	-1.84 (0.34)
Firm Size			-2.70 (0.00)	-2.51 (0.00)
Missing Firm Size			-13.58 (0.03)	-12.40 (0.04)
Firm Age				-2.26 (0.20)
Missing Firm Age				-3.96 (0.67)
Constant	98.44 (0.00)	102.94 (0.00)	88.28 (0.00)	90.54 (0.00)
Adjusted <i>R</i> <sup>2</sup>	0.17	0.07	0.11	0.12
Observations	49	446	446	446

Table 7

The Choice of Dependent Variable when Individual Data are Available

Summary of the type of dependent variables used in a broad but non-exhaustive array of papers. When averages are used as the dependent variable, individual data are used to calculate the averages.

<u>Averages</u>	<u>Both Averages and Individual Observations</u>	<u>Individual Observations</u>
All Ten Papers from Table 1 (ownership concentration)	Nenova (2003) (dual class stock)	LLSV (2000) (dividend policy)
LLS (2002) (government ownership of banks)	Dyck and Zingales (2004) (block premiums)	LLSV (2002) (market-to-book ratio)
Demirguc-Kunt and Maksimovic (1998) (internal versus external financing)	Dittmar, Mahrt-Smith, and Servaes (2003) (corporate cash holdings)	Kalcheva and Lins (2007) (corporate cash holdings)
Demirguc-Kunt and Maksimovic (1999) (debt maturity)		Fan, Titman, and Twite (2006) (capital structure)
Leuva, Nanda, and Wysocki (2003) (earnings management)		Rajan and Zinglaes (1995) (capital structure)
Morck, Yeung, and Yu (2000) (stock price movement)		Esty and Megginson (2003) (loan syndicates)
Griffin, Nardari, and Stulz (2007) (stock trading)		Durnev and Kim (2005) (corporate governance and disclosure practices)
Kumar, Rajan, and Zingales (2001) (firm size)		Qian and Strahan (2007) (bank loans)
Jin and Myers (2006) (stock price movement)		Doidge, Karolyi, and Stulz (2007) (corporate governance)
Rajan and Zingales (2003) (stock market development)		Dahya, Dimitrov, and McConnell (2008) (corporate governance and firm value)
		Khorana, Servaes, and Tufano (2009) (mutual fund fees)