

2006

***The Regulation of Labor* and the Relevance of Legal Origin**

David E. Pozen
Columbia Law School, dpozen@law.columbia.edu

Follow this and additional works at: https://scholarship.law.columbia.edu/faculty_scholarship



Part of the [Comparative and Foreign Law Commons](#), [Labor and Employment Law Commons](#), and the [Law and Economics Commons](#)

Recommended Citation

David E. Pozen, *The Regulation of Labor and the Relevance of Legal Origin*, 28 COMP. LAB. L. & POL'Y. J. 43 (2006).

Available at: https://scholarship.law.columbia.edu/faculty_scholarship/1075

This Article is brought to you for free and open access by the Faculty Publications at Scholarship Archive. It has been accepted for inclusion in Faculty Scholarship by an authorized administrator of Scholarship Archive. For more information, please contact scholarshiparchive@law.columbia.edu.

THE REGULATION OF LABOR AND THE RELEVANCE OF LEGAL ORIGIN

David E. Pozen†

Arguably the most important social science research of the past decade has centered on comparative law and economics. In a celebrated series of articles, the economists Rafael La Porta, Florencio Lopez-de-Silanes, Andrei Shleifer, and intermittent collaborators have explored empirically how a country's legal origin—English common law, French civil law, Germanic code, Scandinavian law, or Soviet socialist law—affects its subsequent institutional and economic development. The common law emerges as the hero of this analysis: Compared with other countries and especially with civil law countries, common law bearers have, *ceteris paribus*, better legal protection of shareholders and creditors;¹ greater judicial independence and economic freedom;² less formalized, shorter, fairer, more consistent dispute resolution procedures;³ less politicized law enforcement;⁴ lower barriers to new business formation;⁵ and more efficient bureaucracies.⁶ Taken together, these studies offer a remarkably broad argument for the common law being a driver of, or at least uniquely consonant with, good government. On the sweep and strength of these findings, La Porta and colleagues have been cited more times since 1997 than any other economists, and policymakers worldwide have been scouring their articles for usable insights.⁷

† J.D., Yale Law School, expected 2007. I thank Patrick Bayer, John Donohue, Joshua Kleinfeld, David Love, Michelle Messer, and two anonymous referees for helpful comments.

1. Rafael La Porta et al., *Law and Finance*, 106 J. POL. ECON. 1113 (1998) [hereinafter La Porta et al., *Law and Finance*]; Rafael La Porta et al., *Legal Determinants of External Finance*, 52 J. FIN. 1131 (1997) [hereinafter La Porta et al., *Legal Determinants*].

2. Rafael La Porta et al., *Judicial Checks and Balances*, 112 J. POL. ECON. 445 (2004).

3. Simeon Djankov et al., *Courts*, 118 Q. J. ECON. 453 (2003).

4. Edward L. Glaeser & Andrei Shleifer, *Legal Origins*, 117 Q. J. ECON. 1193 (2002).

5. Simeon Djankov et al., *The Regulation of Entry*, 117 Q. J. ECON. 1 (2002).

6. Rafael La Porta et al., *The Quality of Government*, 15 J.L. ECON. & ORG. 222 (1999).

7. See Nicholas Thompson, *Common Denominator*, LEGAL AFF., Jan.–Feb. 2005, at 46 (profiling the authors, often referred to collectively as “LLSV,” and describing their impact). LLSV’s law-and-finance scholarship has been particularly influential. See, e.g., John C. Coffee, Jr., *The Rise of Dispersed Ownership: The Roles of Law and the State in the Separation of*

In a new study entitled *The Regulation of Labor*, the authors (with Juan Botero now headlining) push their research in a provocative new direction.⁸ While their previous work had focused on corporate law and on courts, here the authors investigate a novel domain. Using original data compiled on eighty-five countries, Botero and colleagues (Botero et al.) examine the historical determinants of employment, collective relations, and social security laws. They find that legal origin is a stronger predictor of all of these than political or economic variables, with common law associated with the lowest levels of regulation. Legal institutional theory, they conclude, better explains the development of national labor laws than political power or efficiency theories.

Moving labor market regulation from the left-hand to the right-hand side of the equation, Botero et al. then show that heavier regulation corresponds to lower labor force participation and higher unemployment, especially for the young. Coupled with the legal origins evidence, this result implies that the common law leads not only to less intensive worker protection, but also to superior labor market outcomes. *The Regulation of Labor* thus extends La Porta and colleagues' research in at least three significant ways, providing further evidence of: (1) legal origin's lasting and profound impact on present-day outcomes, (2) the common law's uniquely beneficent effects, and (3) "regulatory complementarity," whereby countries' regulatory styles remain consistent across substantive areas of law.⁹ The study's remarkable dataset and its anti-regulation conclusions, meanwhile, may set the agenda for comparative labor law empirical research for years to come.¹⁰

The problem, this paper will argue, is that *The Regulation of Labor's* methodological weaknesses severely undermine its putative contributions. Many scholars have voiced concerns about La Porta

Ownership and Control, 111 YALE L.J. 1, 3 n.2 (2001) (calling LLSV's law-and-finance works "seminal"); Sofie Cools, *The Real Difference in Corporate Law Between the United States and Continental Europe: Distribution of Powers*, 30 DEL. J. CORP. L. 697, 699 (2005) (same); Beth Ahlering & Simon Deakin, *Labour Regulation, Corporate Governance and Legal Origin: A Case of Institutional Complementarity?* 2 (European Corporate Governance Inst., Law Working Paper No. 72/2006, 2005) (noting that LLSV's research has "inform[ed] the policy and working methods of the World Bank and other international financial institutions").

8. Juan C. Botero et al., *The Regulation of Labor*, 119 Q. J. ECON. 1339 (2004).

9. This phrase comes from Shleifer himself. Casey B. Mulligan & Andrei Shleifer, *Conscription as Regulation*, 7 AM. L. & ECON. REV. 85, 87 (2005).

10. For examples of recent works that draw on Botero et al.'s data, see W.S. Siebert, *Labour Market Regulation: Some Comparative Lessons*, 25 ECON. AFF. 3 (Sept. 2005); Ahlering & Deakin, *supra* note 7; Davin Chor & Richard B. Freeman, *The 2004 Global Labor Survey: Workplace Institutions and Practices Around the World* (Nat'l Bureau of Econ. Research, Working Paper No. 11598, 2005).

and colleagues' econometric approach.¹¹ I join this chorus and seek to augment it by providing, in Sections I and II, one of the first systematic methodological critiques of their study¹² and by situating this critique, in Section III, in the broader context of empirical legal scholarship. My critique is able to avoid being highly technical because the methodological problems here are fundamental. Nevertheless, I write this paper not from a place of *ressentiment*, but from a place of admiration; *The Regulation of Labor*, like all of these authors' works, is as impressive as it is important. More, it is a signal example of the intertwined potentialities and problematics of comparative law and economics.

I. MEASUREMENT AND CODING

To be able to write *The Regulation of Labor*, Botero et al. undertook a Herculean effort in creating their dataset. In addition to culling a variety of political, economic, and legal variables from preexisting sources—the legal origins classification scheme is carried over from the authors' previous articles—Botero et al. quantified for the first time the labor laws of eighty-five countries across more than sixty dimensions. The authors group each labor-law variable (e.g., percentage of net salary covered by sickness benefits for a two-month sickness spell) into a sub-index (e.g., sickness and health benefits), and then group each sub-index into one of three indices (employment laws, collective relations laws, or social security laws) meant to summarize the overall stringency of a country's regulations.¹³ All variables are normalized to values between zero and one; the sub-index values are computed as the mean of the sub-index's variables; and the index values are computed as the mean of the index's sub-

11. See Thompson, *supra* note 7, at 46 (summarizing the concerns). The bulk of the methodological criticisms have targeted LLSV's law-and-finance research, see, e.g., Thorsten Beck & Ross Levine, *Legal Institutions and Financial Development* 16–20 (Nat'l Bureau of Econ. Research, Working Paper No. 10126, 2003), and so are only partially apposite here.

12. This appears to be the first commentary devoted wholly to *The Regulation of Labor*. I am aware of two other papers that offer sustained critiques: Beth Ahlering and Simon Deakin examine the validity of Botero et al.'s labor law measures and argue that the study, like the rest of "the legal origin literature[,] relies upon an overly reductive understanding of the common law/civil law divide," Ahlering & Deakin, *supra* note 7, at 43, while Davin Chor and Richard Freeman compare the results obtained from their Global Labor Survey with the Botero et al. indices, Chor & Freeman, *supra* note 10. Both of these critiques are stimulating and valuable, and provide deeper analysis of measurement issues than I do here. My aim in this paper is to point out some additional reasons for skepticism about *The Regulation of Labor's* design and results.

13. The article itself spends nineteen pages explaining the data, Botero et al., *supra* note 8, at 1346–64, and the dataset is downloadable from Andrei Shleifer's Web page, <http://www.andrei-shleifer.com/data.html>.

indices. These variables reflect formal legal rules—labor law statutes—as they apply to a standardized male worker and a standardized employer.¹⁴ They are coded such that higher values correspond to more extensive worker protection, with 1.00 indicating maximal protection. Except for a few civil rights measures derived from U.S. State Department, U.S. Department of Labor, and International Labor Organization reports, the authors themselves created and coded all of their dependent variables.

For their independent variables, the authors cull from a variety of data sources. Their preferred political orientation variables, for example, draw on multiple compendia to record the fraction of years during 1928–1995 and 1975–1995 when a country's chief executive and its legislature were both of "leftist" or "centrist" orientation.¹⁵ (What it means to be leftist or centrist, or how this was calculated, is never explained, in the text or the data appendix.) Other independent variables measure for each country: 1997 per capita income; average years of schooling for the population over twenty-five years of age; union density; average levels of autocracy and democracy between 1950 and the 1990s; and the degree to which governments were divided and proportionally representative from 1975 to 1995.

I provide this sketch of Botero et al.'s data because they are at once the major strength and the major weakness of the paper. They are the major strength for obvious reasons: By quantifying so many aspects of so many countries' labor laws, the data enable an empirical analysis of worker protections that is by far the most comprehensive on record.¹⁶ The cost of this great breadth, however, is a loss of confidence that the data actually represent what they are supposed to.

14. Botero et al., *supra* note 8, at 1346, 1353. Among other qualities, the standardized worker is married with two children, lives in the country's most populous city, is not a member of a voluntary labor union, and belongs to the majority race and religion; while the standardized employer is a domestically-owned manufacturing company located in the country's most populous city, has 250 workers, and offers workers no more protections than what is legally mandated. *Id.* at 1353 n.6.

15. Data sources for these two variables include World Bank reports, political encyclopedias, online databases, and unspecified "various regional and country sources." Data Appendix, Variable Definitions, available at <http://www.andrei-shleifer.com/data.html>.

16. Compare Botero et al., *supra* note 8, with Giuseppe Nicoletti & Frederic L. Pryor, *Subjective and Objective Measures of the Extent of Governmental Regulations* (AEI-Brookings Joint Ctr., 2001) (reviewing three recent quantitative studies measuring the extent of regulation in OECD countries). The Global Labor Survey dataset is similarly ambitious in scope, see Chor & Freeman, *supra* note 10, but its variables tend to be more opinionative and impressionistic, they cover only thirty-three countries, and they have not yet, as far as I know, been subjected to the same sort of econometric analysis. The 1994 *OECD Jobs Study* and follow-on reports have analyzed the economic impact of labor market regulations at great depth, but only for a much narrower set of regulations. See, e.g., ORGANIZATION FOR ECONOMIC COOPERATION AND DEVELOPMENT, *THE OECD JOBS STUDY: EVIDENCE AND EXPLANATIONS: PART 1: LABOUR MARKET TRENDS AND UNDERLYING FORCES OF CHANGE* (1994).

Validity slippage could occur on several levels—and mar the study's results twice over, because labor regulation is used as both a dependent and independent variable.¹⁷ First, Botero et al.'s exclusive reliance on formal legal rules might fail to capture functional outcomes. Botero et al. acknowledge this evident concern and take some steps to mitigate it, but they overlook the possibility that enforcement of and compliance with labor laws vary systematically across countries.¹⁸ For example, compliance and enforcement rates may be positively correlated with, and to some extent caused by, the wealth and institutional maturity of a jurisdiction, the longevity of a law's existence, and the degree of a law's compatibility with social norms—all of which are likely to be lower for developing countries as compared to developed countries with respect to labor regulations. Compliance will also likely be weaker in the presence of strict regulation.¹⁹ Validity is further attenuated by Botero et al.'s failure to distinguish between mandatory, default, and optional rules—presumably they coded for default rules—which both magnifies the de jure/de facto cleavage and leaves their variables underspecified.²⁰

The Regulation of Labor's coding scheme exacerbates a second and even more basic concern: the inherent subjectivity in translating statutory provisions (not to mention functional outcomes) into numerical scores. It is easy to dispute, for instance, a “dismissal procedures” sub-index that gives New Zealand a score of 0.14 and

17. Botero et al., *supra* note 8, at tbls.2-5, tbl.8.

18. Botero et al. find this concern minor because they include some rough controls for enforcement quality, because labor law is primarily statute-based (a claim challenged in Ahlring & Deakin, *supra* note 7, at 17), and because several of the sub-indices are constructed to reflect actual economic costs, not just statutory language. Botero et al., *supra* note 8, at 1347. It should be noted that economists, including labor economists, routinely measure enforcement levels. See, e.g., Rita Almeida & Pedro Carneiro, *Enforcement of Labor Regulation, Informal Labor, and Firm Performance* (World Bank, Pol'y Research Working Paper No. 3756, 2005) (measuring labor-law enforcement in Brazil).

Chor and Freeman's Global Labor Survey, “an Internet-based survey designed to collect information on the state of workplace practices from labor experts and practitioners around the world,” Chor & Freeman, *supra* note 10, at 3, bypasses formal legal rules altogether in pursuit of functional knowledge on labor regulation. The Global Labor Survey indices are consistent with the rankings of Botero et al., but the correlations are not particularly tight, especially in the area of collective relations. See *id.* at 21 & n.24, 42 tbl.8. Chor and Freeman “attribute part of this to the fact that Botero et al. measure de jure labor regulations, which will inevitably diverge from de facto workplace practices,” and part of it to “differences in the component questions that underlie the two [studies'] measures.” *Id.* at 21-22.

19. See Norman V. Loayza et al., *The Impact of Regulation on Growth and Informality: Cross-Country Evidence* (World Bank, Pol'y Research Working Paper No. 3623, 2005).

20. Holger Spamann and others have critiqued LLSV's failure to distinguish between mandatory, default, and optional rules in their law-and-finance scholarship. See Holger Spamann, *On the Insignificance and/or Endogeneity of La Porta et al.'s “Anti-Director Rights Index” Under Consistent Coding* 6-8 (John M. Olin Ctr. for Law, Econ. & Bus., Harvard Law Sch., Discussion Paper No. 67, 2006); *id.* at 4 n.7 (providing further citations).

Portugal a score of 0.71. Portugal may have more stringent dismissal procedures than New Zealand, but are its procedures really *five times* as stringent? Because Botero et al.'s variables are unique, there is neither an independent check on their accuracy, nor any good way to quantify their measurement error. And what does it even mean to have dismissal procedures that are five times as stringent as those of another country? Because Botero et al. never provide a theory of labor regulation to undergird their coding decisions, this ratio is essentially arbitrary. In multiple senses, then, *The Regulation of Labor's* data may be subject to greater uncertainty than the labor law data generated through the repeated, systematic efforts of groups like the Organization for Economic Co-operation and Development (OECD) and the World Bank.²¹

A third cluster of data limitations involves Botero et al.'s method of indexing their worker-protectiveness scores. This indexing loses potentially interesting information about individual laws. It imposes an awkward commensurability across a wide range of legal protections that influence different workers in different countries in different ways. It unrealistically assumes that these protections operate independently of each other, such that countries do not substitute between, say, employment law and trade unions, and employees do not experience multiplicative returns on the simultaneous presence/absence or generosity/stinginess of different protections.²² And its use of averages masks substantial variability within both the sub-indices and the indices,²³ which would be especially unfortunate if any sub-indices correlate across countries in ways that diverge from their indices.²⁴ Compounding the artificiality of the indexing method,

21. It is thus a promising development that the World Bank has apparently resolved to update and expand on some of Botero et al.'s formal measures in its *Doing Business* database. See World Bank, *Doing Business Database*, <http://www.doingbusiness.org>.

22. Sanford Jacoby contends such substitution is likely to happen. Sanford M. Jacoby, *Economic Ideas and the Labor Market: Origins of the Anglo-American Model and Prospects for Global Diffusion*, 25 COMP. LAB. L. & POL'Y J. 43, 69 n.60 (2003).

23. As an illustration of this concern, Greece and Argentina have aggregate social security index scores—used by Botero et al. as a dependent variable in all regressions—that, at 0.74 and 0.72 respectively, are quite similar. Their sub-index scores, however, betray significant differences in their social security systems: Greece's scores for "old age, disability and death benefits," "sickness and health benefits," and "unemployment benefits" are 0.70, 0.78, and 0.74, while Argentina's comparable scores are 0.37, 0.94, and 0.84. Data Appendix, *supra* note 15. One would never know from the countries' index scores that Greece has a consistently generous social security system across all dimensions, whereas Argentina has a highly generous system with respect to health and unemployment benefits but a rather uncharitable system with respect to old age, disability, and death benefits.

24. Although sub-indices are generally correlated with other sub-indices from the same index, Botero et al., *supra* note 8, at tbl.II, this possibility remains. Botero et al. helpfully include the sub-indices as dependent variables in Table IV, but nowhere else.

Botero et al.'s exclusive focus on standardized workers and employers elides any within-country variability that exists across industry sector, region, age, gender, race, marital status, and the like.

The above data concerns are substantial, yet *The Regulation of Labor*—and arguably all of the authors' legal origins work—is beset by two other data concerns still more fundamental: the possibility of miscoding and a lack of transparency. La Porta and colleagues have already been charged with misclassifying certain countries' legal typology.²⁵ Others have argued that “transplant” countries, which received their legal systems through conquest or colonization, follow systematically different paths of legal evolution than “origin” countries, irrespective of legal family.²⁶ Still others have found regional patterns in legal evolution not captured by the authors' framework.²⁷ *The Regulation of Labor* is vulnerable to each of these criticisms, and also to criticisms of its worker-protection characterizations. “[A] labor scholar would immediately notice that there might be some problem in the construction of [the study's] indices,” Sanford Jacoby points out, “because the industrial relations regime in Canada is coded as less ‘pro-worker’ than that of the United States.”²⁸ Indeed, in direct conflict with this result, the OECD recently gave Canada a significantly higher rating than the United States on overall “employment protection legislation” for each of the past three decades.²⁹

The potential for such miscoding here, it is important to stress, is not just a function of the inevitable messiness of quantifying complex real-world phenomena; it is also a function of process and subject-matter competence. Because Botero et al. did not use survey data or, for the most part, objective measures, nearly all of *The Regulation of Labor's* data values reflect the authors' judgment calls. It would take

25. Mark West, for instance, has ridiculed the coding of Japan as a Germanic-code country. See Thompson, *supra* note 7, at 46.

26. See Daniel Berkowitz et al., *Economic Development, Legality, and the Transplant Effect*, 47 EUR. ECON. REV. 165 (2003) [hereinafter Berkowitz et al., *Economic Development*]; Daniel Berkowitz et al., *The Transplant Effect*, 51 AM. J. COMP. L. 163 (2003) [hereinafter Berkowitz et al., *Transplant Effect*].

27. See Katharina Pistor et al., *The Evolution of Corporate Law: A Cross-Country Comparison*, 23 U. PA. J. INT'L ECON. L. 791 (2002).

28. Jacoby, *supra* note 22, at 69 n.60. In a similar vein, Spamann and others have argued that there is “strong and systematic measurement error” in LLSV's coding of shareholder protection rules. Spamann, *supra* note 20, at 1; accord Cools, *supra* note 7. When Spamann recoded LLSV's Antidirector Rights Index, this time with the help of local lawyers, nearly all of the results from LLSV's famous *Law and Finance* paper, La Porta et al., *Law and Finance*, *supra* note 1, ceased to be statistically significant.

29. ORGANISATION FOR ECONOMIC CO-OPERATION AND DEVELOPMENT, OECD EMPLOYMENT OUTLOOK 2004, at 117 tbl.2.A2.4 (2004).

an astonishing facility with international labor law (and foreign languages) to accurately discern the worker protectiveness of sixty-plus regulatory fields across eighty-plus countries. Yet no one of the authors is a labor law scholar, much less a global authority.³⁰ It is therefore particularly jarring that Botero et al. do not give readers a reasonable explanation of how they constructed their data or how to replicate their process.³¹ The dataset may ultimately prove as unreliable as it may prove invalid.

II. OMITTED VARIABLES, AHISTORICITY, AND ENDOGENEITY

Even assuming, *arguendo*, that Botero et al.'s data represent with perfect fidelity the state of each country's labor regulations, functionally as well as formally, *The Regulation of Labor* would still not be able to generate meaningful causal conclusions on legal origins. As in the authors' related work, this study's legal origins inquiry proceeds from a clever insight: Because most countries in the world received their legal typology by transplant and never changed it thereafter, their legal origins are exogenous to their economies. This offers the researcher a natural experiment.

Yet that same colonial genesis also means that a country's legal typology is part of a larger socio-historical package, which creates a tangle of econometric problems. First, because countries within a legal family tend to share so many extralegal commonalities—historical, linguistic, geographical, cultural—all of which might plausibly influence regulation-setting and none of which are controlled for (apart from a few cultural variables used in unreported regressions), Botero et al. cannot identify the effects attributable specifically to legal origins.³² For all the authors' codificatory ambition, the dataset remains ironically impoverished. This omitted variables problem is compounded by the problem of time. Botero et

30. The extent to which Botero et al. collaborated with foreign lawyers, labor law scholars, or international research bodies is not made clear in the article or the data appendix. Whatever form this collaboration might have taken, one assumes that Botero et al. made all the coding decisions themselves. Enlisting the help of local experts can be an effective, if resource-intensive, way to help comparative researchers understand foreign legal systems; but it is no panacea for measurement error. See Spamann, *supra* note 20, at 21 (discussing the benefits and drawbacks of cross-country collaboration).

31. Cf. Lee Epstein & Gary King, *The Rules of Inference*, 69 U. CHI. L. REV. 1, 86, 103 (2002) (emphasizing the importance of clear coding and data-generation instructions). Botero et al. promise "a full description of all variables and the data" in the online appendix, Botero et al., *supra* note 8, at tbl.1, but the appendix says little on coding and nothing on the data-generation process.

32. Note in this regard the euphemistic quality of "transplant," and how the term serves to dull and obscure the underlying historical narrative.

al.'s cross-sectional approach examines different countries' regulations at a single point in time, 1997, and tells us nothing about how labor laws evolve. This ahistoricity fails to capture any intertemporal patterns or fluctuations in the data, and therefore further erodes the ability to draw causal inferences.³³ Panel data analysis may not have been a viable alternative because the fixed effects would have wiped out the legal origins results. But if Botero et al. had run their cross-sectional regressions for more than just 1997, it would have at least shed some light on the results' sensitivity to the choice of a reference year.

There is, moreover, a problem at the level of theory. Although many hypotheses have been proffered on how different legal traditions might influence regulation³⁴—most notably accounts of how common law yields more contractarian, less regulated societies—Botero et al. never actually specify the mechanisms through which legal origins determine regulatory outcomes.³⁵ Even more so than the typical cross-sectional study, *The Regulation of Labor's* results are unlikely to be robust to alternative specifications, time periods, or explanatory variables. Unadjusted R² values in the 0.19 to 0.64 range, trumpeted by Botero et al. in the text, do nothing to allay these problems.³⁶

All of the previous two paragraphs' criticisms can and have been leveled, in scattered form, against these authors' other legal origins research;³⁷ especially acute in *The Regulation of Labor*, however, is

33. Imagine, as a hypothetical, that leftist governments worldwide always enact highly worker-friendly labor laws, whereas centrist and rightist governments always enact highly worker-unfriendly laws. Now imagine two countries, *A* and *B*, where *A* had a leftist government from 1928 to 1990 and then a centrist government afterwards, and *B* alternated between leftist and rightist governments by decade, with rightists regaining power in the 1990s. Looking at 1997 labor laws, both *A* and *B* appear highly worker-unfriendly. Not only would Botero et al.'s static analysis not capture the evolutionary discrepancies between *A*'s and *B*'s labor law systems; it would also fail to identify leftist/centrist/rightist government orientation as the causal factor driving these systems' development.

34. Botero et al. summarize this literature in the article. Botero et al., *supra* note 8, at 1344–46.

35. Nor do Botero et al. account for the substantial variability in worker protectiveness that exists within legal families, as evident from the summary data in Table III. Italy and Burkina Faso, for instance, both have French legal origins, but the former has an aggregate index score $((0.6499 + 0.6310 + 0.7572) / 3 = 0.68)$ almost twice as large as that of the latter $((0.4396 + 0.5258 + 0.1447) / 3 = 0.37)$. Legal origins clearly are not too tight a straightjacket, or the whole story.

36. Botero et al., *supra* note 8, at 1365 (describing Table IV, the basic specification).

37. See, e.g., Beck & Levine, *supra* note 11, at 16–20 (summarizing and providing citations); Jacoby, *supra* note 22, at 68–69 (critiquing *The Regulation of Labor* specifically); Daniel M. Klerman & Paul G. Mahoney, *The Value of Judicial Independence: Evidence from Eighteenth Century England*, 7 AM. L. & ECON. REV. 1, 2 (2005) (highlighting problems of theory and under-parameterization). The most dramatic rebuttal belongs to Mark West, who regressed countries' World Cup soccer success against LLSV's legal origins variables, and found French legal origin and antidirector shareholder rights to have statistically significant positive

the additional problem of endogeneity.³⁸ Botero et al.'s main findings appear in Tables IV and VI, which show that relative to common law origins, all other legal traditions have a statistically significant positive relationship to worker protection levels on at least one index after controlling for GNP per capita and "left power" variables, and in Table VIII, which shows a negative, moderately significant relationship between worker protection levels and desirable labor market outcomes. There is potential endogeneity here because levels of worker protection may influence levels of GNP, left power, and labor market outcomes, rather than the other way around. (Indeed, Table VIII broadcasts this possibility by implying that higher levels of regulation lead to lower employment and therefore, presumably, to lower GNP.)³⁹ Or the variables may jointly determine one another. Most of Botero et al.'s legal classifications go back hundreds of years to the date of transplant, leaving a vast temporal expense in which reciprocal causal iterations could have occurred. The key parameters,

coefficients, with an adjusted R² value of 0.41. Mark D. West, *Legal Determinants of World Cup Success* tbl.1 (unpublished manuscript) (2002), http://papers.ssrn.com/abstract_id=318940. West's point, left implicit, was that these are spurious correlations; no one would seriously argue that a country's legal status or shareholder-rights regime determines soccer success. The causal story linking legal origin to current worker-protection levels is admittedly more plausible, but it too is severely attenuated.

38. Spamann also finds acute endogeneity problems in LLSV's analysis of shareholder protection rules. Spamann, *supra* note 20, at 28-34, 67.

39. In Table VIII, that is, Botero et al. take their article in a different direction, from examining the determinants of labor regulations to examining the consequences of labor regulations: "Outcomes," this section is titled. Botero et al., *supra* note 8, at 1375. Against measures of unofficial economy size, labor force participation, and unemployment (all of which Botero et al. concede "have measurement problems," *id.*), the authors run OLS regressions with only average years of schooling and one of three labor law indices as independent variables. The results vary somewhat across specifications, but in general they show a negative, moderately significant relationship between worker protection levels and desirable labor market outcomes, with stronger collective relations laws uniquely associated with a larger unofficial economy. Unadjusted R² values range from 0.01 to 0.42. The omitted variables problem is especially acute here, amounting almost to misspecification—does anyone believe that average years of schooling and a single index of labor laws capture even a fraction of the forces that determine labor market outcomes?—and there is a substantial endogeneity concern in that bad labor market conditions might stimulate increased labor regulation. The problem of ahistoricity also remains. See *supra* note 33 and accompanying text. Imagine if Botero et al. had run their cross-section analysis at a much earlier point in time, say 1900, before any of the relevant labor laws and social security programs were in place. They would still find the common law countries to be richer and to have lower unemployment, but the relevance of labor regulation to these outcomes would obviously be nil.

Overall, Table VIII is too under-parameterized, too half-hearted an effort to offer much, and only serves to deflect attention from Botero et al.'s primary arguments on the determinants of labor laws. Its inclusion appears especially odd in light of the great body of work that exists on the economic consequences of labor regulation. See, e.g., Botero et al., *supra* note 8, at 1341 n.2 (citing representative examples); Richard B. Freeman, *Labour Market Institutions Without Blinders: The Debate over Flexibility and Labour Market Performance* (Nat'l Bureau of Econ. Research, Working Paper No. 11286, 2005) (summarizing the recent literature).

and the model itself, are left unidentified. Along with the problems of omitted variables, sequencing, and specification, endogeneity makes it impossible to know whether legal origin is responsible for, not just correlated with, present-day labor law and labor market outcomes.

The correlations are interesting, however, and in fact represent the second major contribution of *The Regulation of Labor* after the dataset itself (assuming it is at least moderately valid).⁴⁰ In labor law as in other areas, the common law tradition again emerges as associated with significantly lower levels of regulation. At the end of the article (Table VII), Botero et al. present pairwise correlations between their three labor law indices and variables from their prior studies reflecting the difficulty of starting a new business and the formality of court proceedings. The correlations are not overwhelming, but they are generally positive and statistically significant, leading Botero et al. to conclude that “[r]egulatory style is pervasive across activities—consistent with the legal theory,” rather than a political, economic, or cultural theory of regulatory development.⁴¹ Although the data concerns outlined in Section I demand caution, *The Regulation of Labor*’s broad results do deepen Botero et al.’s descriptive case for regulatory complementarity.⁴² Common law origins may not in and of themselves determine today’s outcomes, but with each new paper by these authors it becomes harder to deny that the common law has a special affinity with more informal and efficient regulation.

III. ON BEING PRODUCTIVELY REDUCTIVE

The Regulation of Labor’s strengths and weaknesses would be notable in their own right, but they are especially notable because they illuminate broader issues in these authors’ project and in comparative law and economics more generally. Attempting to compare legal provisions and identify causality across numerous countries, over numerous years, is extremely difficult to do. Problems with measurement and coding, omitted variables, time sequencing, model specification, and endogeneity are endemic to the enterprise. That these problems arise so dramatically in *The Regulation of Labor* and its precursors reflects the ambition of the authors. To investigate

40. Cf. Ahlring & Deakin, *supra* note 7, at 25 (“[I]t is . . . possible that the index of Botero *et al.*, even based, as it is, on ‘law in the books,’ is a good, working proxy for the social effects of laws. Perhaps more to the point, it is almost certainly the most rigorous and comprehensive one that we currently have.”).

41. Botero et al., *supra* note 8, at 1375.

42. See *supra* note 9 and accompanying text.

cross-country structural questions as they do, analytic precision must, to an extent, be sacrificed. By contrast, economists investigating labor laws have been able to generate much more robust results when they confine themselves to within-country, cross-region variation, and add a time-series element.⁴³

So what usable insights can be gleaned from *The Regulation of Labor*? For the policymaker, precious few. The article contains an important normative element in its claim that heavier labor regulation is associated with lower labor force participation and higher unemployment; Table VIII's prescriptive implications could be summed up, it seems, by adding the letters *De* before *regulation* in the title.⁴⁴ But Table VIII is the least reliable and least original part of the entire study.⁴⁵ Botero et al. are more suggestive in their examination of legal origins. But even if one believes their causal story and their data, it would be enormously costly—in most cases, prohibitively costly—to switch legal systems now from, say, civil to common law, and nothing in their analysis suggests that such a switchover would work.

For legal scholars, on the other hand, *The Regulation of Labor* holds vast potential. The study's sweeping characterization of international labor regulations and its demonstration of their legal-typology correlations can frame almost any comparative labor law inquiry. It is an extreme case of productively reductive research. More nuanced comparativists, steeped in the histories and cultures of their subjects, have in these authors' work a reference point that is becoming increasingly irresponsible to ignore. The possibilities for fruitful syntheses between this research and more traditional, finer-grained legal research are endless.

43. See, e.g., Timothy Besley & Robin Burgess, *Can Labor Regulation Hinder Economic Performance? Evidence from India*, 119 Q. J. ECON. 91 (2004) (exploring variation across Indian states to find that labor regulations have negative effects on employment, investment, and productivity in manufacturing); David Card & Alan B. Krueger, *Minimum Wages and Employment: A Case Study of the Fast Food Industry in New Jersey and Pennsylvania*, 84 AMER. ECON. REV. 772 (1994) (using a comparison with Pennsylvania to find no employment decline caused by New Jersey's minimum wage increase).

44. As readers of the *Comparative Labor Law & Policy Journal* are no doubt already aware, the issue of labor market deregulation is currently of intense interest in Western Europe. Deregulation proposals in France and Germany have sparked heated debates—and, in France, general strikes and mass protests. See Richard Bernstein, *Political Paralysis: Europe Stalls on Road to Economic Change*, N.Y. TIMES, Apr. 14, 2006, at A8; David Pozen, *Blame It on Globalization: West European Countries Need To Reform Their Welfare Policies for Domestic Reasons*, YALEGLOBAL, May 21, 2003, available at <http://yaleglobal.yale.edu/display.article?id=1663>. In this sense, Botero et al.'s study appeared on the scene at an auspicious time for influencing the policymaking process.

45. See *supra* note 39 and accompanying text.

The limits of *The Regulation of Labor*, however, suggest two preconditions for this potential to be realized in full. First, empirical legal scholars who create new, non-survey datasets might be encouraged to submit them to peer review before publishing analyses based on the data. At present, no such first-order peer review exists. To be sure, Botero et al. published their article in a top peer-reviewed journal, the *Quarterly Journal of Economics*, and they published an earlier version as a National Bureau of Economic Research working paper.⁴⁶ Yet these institutions are set up to vet authors' manuscripts, not necessarily their data collection and coding methods. (And the typical student-run law review is not even set up to do the former where sophisticated empirical scholarship is concerned.) A separate body tasked specifically with reviewing datasets in its field could go a long way toward rationalizing methodological debates, and would be especially valuable when the data span multiple countries, as here.

A second suggestion is aimed at the level of the author, not the academic infrastructure; it concerns presentation. Deirdre McCloskey has, with brilliance and panache, been hammering the point for years that despite its trappings of scientific precision, empirical economics cannot provide certain answers.⁴⁷ No one seriously disputes this, but rhetorically, McCloskey also highlights, some imply otherwise.⁴⁸ *The Regulation of Labor*, like other works in La Porta and colleagues' oeuvre, is selectively candid about its vulnerabilities. The article would not leave an untrained reader with the understanding that it demonstrates only correlation, never causation, and that even its correlations are deeply disputable. To the contrary, its tone conveys authoritativeness—from the conclusion: “There is, finally, strong evidence that the origin of a country’s laws is an important determinant of its regulatory approach, in labor as well as in other markets. . . . This evidence is broadly consistent with the legal theory”⁴⁹—while its title, like all the authors’ titles—*Law and Finance*, *Legal Origins*, *The Quality of Government*, *The Regulation of Labor*—conveys definitiveness, if not closure. La Porta and colleagues have started much, as this paper explains, and they have good reason to preen. But the regulation of labor is a vast subject, and their study raises more questions than it answers.

46. Juan Botero et al., *The Regulation of Labor* (Nat'l Bureau of Econ. Research, Working Paper No. 9756, 2003).

47. See generally DEIRDRE MCCLOSKEY, *THE SECRET SINS OF ECONOMICS* (2002); DONALD N. MCCLOSKEY, *KNOWLEDGE AND PERSUASION IN ECONOMICS* (1994).

48. See DONALD N. MCCLOSKEY, *THE RHETORIC OF ECONOMICS* (1985); Donald N. McCloskey, *The Rhetoric of Law and Economics*, 86 MICH. L. REV. 752 (1988).

49. Botero et al., *supra* note 8, at 1379.

