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THE FAILURE OF JUDGES AND THE RISE OF REGULATORS. By Andrei
  Shleifer. Cambridge, Massachusetts: MIT Press, 2012. 352 pages.
  $40.00.

Reviewed by Jonathan Klick[*]

I.   Introduction

Andrei Shleifer is undoubtedly among the world's most important
economists. By standard citation measures, no one else is anywhere close.
For example, his nearly 19,000 citations in the RePEc rankings[1] as of
October 2012 place him ahead of Nobel Prize[2] winners such as James
Heckman (12,212),[3] Joseph Stiglitz (11,431),[4] and Robert Lucas (9,314).[5]
His work on corporate finance, behavioral finance, and transition economics
earned him the American Economic Association's prestigious John Bates
Clark medal in 1999.[6] Perhaps not even international scandal will keep
Shleifer from taking his place among the Nobelists.[7]

Shleifer's influence in legal scholarship is almost as large. With more
than 1,000 Westlaw citations,[8] Shleifer would compare favorably to most law
and economics specialists in top U.S. law schools.[9] Given all of this, the
publication of Shleifer's book *The Failure of Judges and the Rise of
Regulators*[10] as part of the MIT Press's Walras-Pareto Lecture series is sure
to be of interest to a wide range of legal scholars, students, and policy
makers—and especially to those who do not have access to JSTOR[11] and a

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1. *Top 5% Authors, as of October 2012*, IDEAS, http://ideas.repec.org/top/top
.person.nbcites.html.

2. Formally the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel,
The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, NOBELPRIZE.ORG,
http://www.nobelprize.org/nobel_prizes/economics, but only pedants note this, such as bloggers
who disagree with a given Nobelist's positions.

3. *Top 5% Authors, as of October 2012*, *supra* note 1.

4. *Id.*

5. *Id.*

6. *John Bates Clark Medal*, AM. ECON. ASS'N, http://www.aeaweb.org/honors_awards/
clark_medal.php.

7. For a thorough and exhaustive review of Shleifer's troubles, see David McClintick, *How
Harvard Lost Russia*, INST. INV., Jan. 2006, at 62.

8. Based on a search for "Andrei /2 Shleifer" in Westlaw's Journals & Law Reviews (JLR)
database performed on September 25, 2012.

9. I blame my own paltry 295 on youth and a bias against guys with beards.

10. ANDREI SHLEIFER, THE FAILURE OF JUDGES AND THE RISE OF REGULATORS (2012).

11. JSTOR, http://www.jstor.org.

printer,[12] since all but the introductory chapter previously appeared in academic journals.

In the introductory chapter, Shleifer lays out a connection among these papers that might not have been apparent to people who read them when they first appeared. Although many readers, viewing his papers individually, would have guessed that Shleifer is pessimistic about the ability of courts to resolve disputes in an efficient manner, his optimistic view of regulation as a substitute mechanism is less clear than the claims he makes in this book, such as his statement, "In this book, I argue that the superiority of courts is far from clear cut. And when courts fail, regulation emerges as the more efficient approach."[13]

The sources of court failure, according to Shleifer, are many. As a consequence of judicial discretion, Shleifer suggests that litigation is "expensive and unpredictable, leading [parties] to bear unnecessary risks."[14] This conclusion holds, according to the author, even in the best of circumstances, but Shleifer goes on to list problems endemic to courts such as "weak incentives" due to the job security judges enjoy and the low probability that good performance will be rewarded,[15] the knowledge deficit that arises given the general educations and limited training judges receive in substantive areas,[16] judicial bias,[17] and the asymmetry of resources that often exists between the parties in court.[18]

While I would be the last one to argue that judges have good incentives,[19] it is not all that clear why regulators are preferable along these dimensions. At the end of the introduction, Shleifer appears to hedge somewhat in his language when he presents all this as some kind of possibility theorem,[20] stating, "With all the faults of regulation recognized by

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12. *Printers, Scanners, Inkjet, All in One Printers*, WALMART, http://www.walmart.com/cp/Printers-Ink/37807.

13. SHLEIFER, *supra* note 10, at 6.

14. The explicit context of this statement is one of contract enforcement as between workers and employers, though the implication is more general so as to include most forms of litigation. *Id.*

15. *Id.*

16. *Id.* at 12.

17. *Id.* at 12–14.

18. *Id.* at 14.

19. See our piece, Eric Helland & Jonathan Klick, *The Effect of Judicial Expedience on Attorney Fees in Class Actions*, 36 J. LEGAL STUD. 171 (2007), where we prove that judges are lazy.

20. In our ambition to be mathematicians, we economists have a long history of developing possibility (or impossibility) theorems, such as Arrow's famous Impossibility Theorem regarding collective choices, Kenneth J. Arrow, *A Difficulty in the Concept of Social Welfare*, 58 J. POL. ECON. 328 (1950), Sen's Liberal Paradox, Amartya Sen, *The Impossibility of a Paretian Liberal*, 78 J. POL. ECON. 152 (1970), and Eric Talley's theorem on the Possibility of an Economist with Good Hair. *But see* Photograph, Professor Eric Helland, Claremont McKenna Department of Economics (and Professor Jonathan Klick) (Sept. 15–16, 2011), http://www.flickr.com/photos/pennlaw/6171946801, for empirical evidence by Helland and Klick regarding the probability of an economist having good hair.

a generation of scholars, it can emerge as the more efficient form of social control. Regulators rise when judges fail."[21] Presumably, Shleifer believes the chapters that follow lay out the case for the superiority of regulators relative to judges. These chapters fall short of this ambition. Instead, the chapters largely focus on problems with courts and some of the correlates of increases in the level of regulation.

Shleifer does little to make the direct case for this argument regarding substitution between litigation and regulation in dispute resolution. The systematic empirical work on this issue in the U.S. context does not support the substitution hypothesis. For example, in work using data on state insurance regulation and class actions involving the same kinds of conduct that fall under the regulations, Eric Helland and I found no evidence of substitution between regulation and litigation on the margin.[22] Perhaps the situation is different in other substantive areas or cross-nationally, but such evidence is not present in the literature.

Intuitively, all of the weaknesses of courts identified by Shleifer seem to be present in regulation as well. Regulators are civil servants with relatively poor incentives, except in the cases where they hope to benefit from the so-called revolving door between the regulators and those they regulate. It is doubtful many would view these as good incentives. The prospect of performance-based termination is also largely absent in many regulatory systems. As for judicial bias exceeding that of regulators, there is no systematic evidence of this. In fact, one could argue that the narrow role of regulators may systematically attract individuals with an ideological bias as the ability to indulge that bias provides psychic income,[23] whereas generalist judges might not expect as many opportunities to indulge their normative preferences given the wide variety of cases they are likely to see and their relatively limited ability to choose what types of cases they will hear. As for asymmetry of resources leading to undue persuasion or outright corruption of judges, presumably such forces are at work for regulators as well, because many regulatory issues involve concentrated benefits and diffuse costs on the

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21. SHLEIFER, *supra* note 10, at 21.

22. *See generally* Eric Helland & Jonathan Klick, *The Tradeoff Between Regulation and Litigation: Evidence from Insurance Class Actions*, 1 J. TORT L. 2 (2007) (finding that, at least in the insurance industry, litigation and regulation go hand in hand); Eric Helland & Jonathan Klick, *Why Aren't Regulation and Litigation Substitutes? An Examination of the Capture Hypothesis*, *in* REGULATORY BREAKDOWN: THE CRISIS OF CONFIDENCE IN U.S. REGULATION ch. 11 (Cary Coglianese ed., 2012) (highlighting the inconsistent outcomes of regulatory action and private litigation in the American insurance industry); Eric Helland & Jonathan Klick, *Regulation and Litigation: Complements or Substitutes*, *in* AMERICAN ILLNESS: ESSAYS ON THE RULE OF LAW (Frank Buckley ed., forthcoming 2013) (manuscript at 19–21), *available at* http://buckleysmix.com/wp-content/uploads/2010/10/Klick-final-edit.pdf (concluding that data and surveys revealed no evidence that supports the substitution hypothesis regarding the relationship between regulation and litigation).

23. This is an implication of the model laid out in Jonah Gelbach, Jonathan Klick & Lesley Wexler, *Passive Discrimination: When Does It Make Sense to Pay Too Little?*, 76 U. CHI. L. REV. 797 (2009).

various sides of a proposal, as articulated by Mancur Olson,[24] or because a given side finds it easier to "capture" a regulator due to its repeat player position, as suggested by George Stigler.[25]

I suppose the most intuitive benefit of regulators relative to judges is the expertise regulators are assumed to have given their specialization. But even on this issue, the evidence does not favor Shleifer, mostly because there is no systematic evidence regarding the expertise of regulators. In fact, recent work by Wright and Diveley suggests that generalist judges outperform expert regulators in antitrust disputes.[26] Given the complicated nature of modern antitrust issues, this would have seemed to be a best-case scenario for Shleifer's position. Maybe things are different in other countries or in areas of law that have not been studied empirically, but Shleifer offers no evidence of his own or citations to the work of others.

To be fair, Shleifer may have been stuck, having spent the tens of dollars MIT Press gave him as an advance, and yet finding himself with no idea for a coherent book. As a way out, perhaps he figured he could string together a series of articles he had published on legal-ish topics in fancy economics journals. In that spirit, this Review will largely treat the individual book chapters separately. Since I know little to no theory, I will discuss all of the theory chapters in a relatively brief way in Part II that can be summarized as follows: yep, it's theory, all right. A more extended discussion of the empirical chapters follows in Part III.

II.   Shleifer's Written a Lot of Good Theory, Just Not Here

The best economic theory allows us to make our intuitions about the way the world works more precise and to then test those intuitions, leading us to either have greater confidence in them relative to other plausible intuitions, or to revise them accordingly. Theory also provides a framework for us to identify the tradeoffs we face when making individual or policy decisions. The work through which Shleifer made his reputation fits this ideal nicely. His work on noise trading, for instance, provides the formalization of ideas stated imprecisely by Keynes;[27] it also provides a

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24. *See generally* MANCUR OLSON, THE LOGIC OF COLLECTIVE ACTION: PUBLIC GOODS AND THE THEORY OF GROUPS (1965) (contending that rational self-interest, rather than encouraging group members to act in such a way as to benefit the entire group, will in fact lead individual members to seek personal gain at the expense of the group).

25. *See generally* George Stigler, *The Theory of Economic Regulation*, 2 BELL J. ECON. & MGMT. SCI. 3 (1971) (positing that regulators will not cease bowing to industry interests until the system provides a political support for regulators other than the regulated industry itself).

26. Joshua D. Wright & Angela M. Diveley, Do Expert Agencies Outperform Generalist Judges? Some Preliminary Evidence from the Federal Trade Commission 19–20 (Jan. 23, 2012) (unpublished manuscript), *available at* http://ssrn.com/abstract=1990034.

27. *See generally* J. Bradford De Long, Andrei Shleifer, Lawrence Summers & Robert Waldmann, *The Size and Incidence of the Losses from Noise Trading*, 44 J. FIN. 681 (1989) (finding that stock prices can be greatly depressed by irrational noise trading).

better fit for some empirical regularities that are hard to square with standard finance theory.[28] Importantly, it also highlights the real problems that arise because of noise trading that are absent in the standard model.

The theory chapters in this book, though originally published in top-quality journals, do not fit this description of good theory. Much of the work is either fairly trivial or fails to allow for anything resembling rigorous econometric testing, leaving the reader with the sense that Shleifer was just dressing up his opinions in mathematics as a way to get them into the scientific literature.

The first such chapter deals with judicial fact discretion.[29] The model finds that if judges have a preference for finding damages different than true damages, they will do so if there is a low personal cost involved.[30] That cost is assumed to be lower when judges have more fact discretion.[31]   A subsequent model suggests that if judges are motivated by a fear of reversal on appeal, judges will use fact discretion to fit the current case safely into a settled precedent, again leading to a divergence from a finding that matches true harm.[32] Since setting damages equal to true harm leads to efficient precaution levels, giving judges more discretion with respect to the facts leads to inefficient outcomes.[33] In a statement that will surprise literally no one, Shleifer concludes, "For both models, we have shown that the outcome of a trial is determined at least in part by who the judge is."[34]

Given that the main conclusion of the model is pretty close to "water flows downhill," we need to ask whether there are any subtleties in the model that do provide either interesting testable implications or important policy recommendations. As for testable hypotheses, Shleifer offers some broad claims like "[f]act discretion leads to judicial behavior that is unpredictable from the facts of the case, but predictable from the knowledge of judicial preferences."[35] I suppose this is like a testable hypothesis, except that it requires data that do not generally exist (since predictability implies settlement, and settlements are hardly ever observed), and a metric—knowledge of judicial preferences—that is likely to be correlated with lots of other factors that may lead to predictability. (E.g., a more senior judge's preferences may be better known; a more senior judge may also be less likely to make a legal mistake. If it is harder to predict the outcome of a given case

28. *See generally* J. Bradford De Long, Andrei Shleifer, Lawrence H. Summers & Robert J. Waldmann, *Noise Trader Risk in Financial Markets*, 98 J. POL. ECON. 703 (1990) (setting forth a model for the effects of irrational noise trading on the stock market).

29. SHLEIFER, *supra* note 10, ch. 2; *see also* Nicola Gennaioli & Andrei Shleifer, *Judicial Fact Discretion*, 37 J. LEGAL STUD. 1 (2008).

30. SHLEIFER, *supra* note 10, at 29.

31. *Id.* at 29–30.

32. *Id.* at 38–39.

33. *Id.* at 48.

34. *Id.*

35. *Id.*

for a more junior judge, is it because the judge's preferences are not known, or because there are more random errors?)

As for policy implications, the answer would be something along the lines of "don't give judges discretion over facts when the true level of harm is known." If it is not known, all bets are off. Related to the broad theme of the book, Shleifer's model implies that under "extreme" fact discretion, "dispute resolution in court may become socially inefficient. In those instances, adjudication can be replaced by ex ante regulation based on bright-line rules. By relying on few cheap-to-verify facts, these rules are less vulnerable to fact discretion."[36] This last claim—that *ex ante* bright-line regulation is less vulnerable to fact discretion—is simply asserted, but there are plenty of examples where regulators exercise discretion of facts. My favorite example in the literature is Makowsky and Stratmann's finding that traffic cops are more likely to fine out-of-town drivers, and are more likely to do so when budgets are tight (see, water does flow downhill), despite the fact that speed limits are among the brightest of lines.[37]

The next chapter examines evolution in common law.[38] In this model, using the assumptions that judges hold preferences over party types, there is a cost to diverging from legal precedent, and the common law evolves when judges distinguish a current case from existing precedent.[39] Shleifer finds that a wider distribution of judicial preferences will lead to more disagreement with precedent, and that such disagreement leads to more precise legal rules as seemingly similar cases are distinguished on the basis of increasingly specific informational elements.[40] The chapter purports to generate a number of testable predictions,[41] but on inspection, the predictions do not lend themselves to empirical testing. For example, Shleifer states, "But proposition 3 delivers another novel empirical prediction, namely that legal rules are more complex (include more empirical dimensions) when judicial views are more dispersed."[42] Short of an exogenous shock to the dispersion of judicial views (what would that even mean?), it would be impossible to rule out the possibility that inherently more complicated phenomena lead to both more complicated legal rules and a wider dispersion of preferences. The latter is completely plausible, since a more complicated legal area will naturally involve more tradeoffs over which people can have very different views. As for implications, is the common law good or bad? Should the cost of distinguishing be increased, or should judges be allowed

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36. *Id.* at 49.

37. Michael D. Makowsky & Thomas Stratmann, *Political Economy at Any Speed: What Determines Traffic Citations?*, 99 AM. ECON. REV. 509, 526 (2009).

38. SHLEIFER, *supra* note 10, ch. 3; s*ee also* Nicola Gennaioli & Andrei Shleifer, *The Evolution of Common Law*, 115 J. POL. ECON. 43 (2007).

39. SHLEIFER, *supra* note 10, at 55.

40. *Id.* at 55–56.

41. *Id.* at 56.

42. *Id.* at 68.

to simply ignore precedent? It all depends on the unquantifiable parameter values in the model. Somehow this does not seem more helpful than Hayek's hand-wavy attempts to analogize the common law to a market where local knowledge can be leveraged and there is flexibility to adapt to new developments. Nor is it in reality any more precise than Cardozo's optimistic claim that bad decisions tend to balance out over time.

"The Rise of the Regulatory State"[43] is the next theory chapter in the book. Simply put, the theory shows that if the bad guys can subvert the courts more cheaply than they can subvert the regulators, it is more efficient to rely on regulation, and vice versa.[44] Shleifer indicates that progressive regulation at the turn of the last century is consistent with this story, since industrial interests got rich during this period and so could dominate the courts.[45] Why they could not dominate the regulators as much is not clear, but it must be true—otherwise, the story wouldn't fit the theory. The following chapter, "Coase Versus the Coasians,"[46] has much the same flavor when it suggests that one should rely on judges to enforce contractual agreements and other background rules when, on net, they're relatively better at doing so than regulators are and vice versa. Because regulators are more easily incentivized, Shleifer asserts that this balance will often favor regulators.[47] The last of the theory chapters, "Legal Origins,"[48] is perhaps the best known of Shleifer's work to a legal audience, including the subsequent empirical literature it spawned.[49] The central idea that common law and civil law systems developed as reactions to different legal realities between England and France[50] is both important and interesting, as is the further implication that these historically dependent decisions can end up having important and predictable consequences even after those conditions have since passed.[51] Unfortunately, this work is bundled up in a wave of bad

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43. *Id.* ch. 6; *see also* Edward L. Glaeser & Andrei Shleifer, *The Rise of the Regulatory State*, 41 J. ECON. LIT. 401 (2003).

44. SHLEIFER, *supra* note 10, at 147.

45. *Id.* at 143.

46. *Id.* ch. 7; *see also* Edward Glaeser, Simon Johnson & Andrei Shleifer, *Coase Versus the Coasians*, 116 Q.J. ECON. 853 (2001).

47. SHLEIFER, *supra* note 10, at 178.

48. *Id.* ch. 8; *see also* Edward L. Glaeser & Andrei Shleifer, *Legal Origins*, 117 Q.J. ECON. 1193 (2002).

49. For examples of such literature, see Carsten Hefeker & Michael Neugart, *Labor Market Regulation and the Legal System*, 30 INT'L REV. L. & ECON. 218 (2010), Mark J. Roe, *Legal Origins, Politics, and Modern Stock Markets*, 120 HARV. L. REV. 460 (2006), and Mathias M. Siems, *Shareholder Protection Around the World (Leximetric II)*, 33 DEL. J. CORP. L. 111 (2008).

50. Briefly, because England was relatively peaceful internally, it could rely on decentralized dispute resolution, whereas internal conflict in France made this unworkable as local nobles would have subverted a decentralized dispute-resolution process. France thus required enforcement from the central government, but this centralized control had to rely on a more rigid system of bright-line rules due to the information costs involved in a nonlocalized system. SHLEIFER, *supra* note 10, at 210–11.

51. *Id.* at 209–10.

empirical analyses attempting to relate current legal rules and metrics of financial and macroeconomic development to a country's legal origins.[52] In a nutshell, the empirical literature on this topic suffers from simultaneity[53] problems of epic proportions. Legal institutions, political institutions, and cultural institutions are all bound up in unknowable ways leaving us with no possible hope of untangling causality.[54] Shleifer and company's claims that the underlying empirical work is robust and the suggestion that such stability improves confidence in causality are flat out false.[55] But other than that, Legal Origins is probably the high point of the book.

In sum, although Shleifer is a creative, insightful, and technically proficient theorist, this book provides no evidence of that.

III. As an Empiricist, Shleifer's a Good Theorist

As suggested above, Shleifer's theoretical undertakings in this area do not focus on developing feasible empirical predictions. Instead, most of his claims of providing empirical predictions suggest no workable econometric test. The remaining chapters do, however, examine data. Unfortunately, they do so in a way that suggests Shleifer has ignored all developments in empirical microeconomics over the past two decades.[56]

Modern empirical work in economics focuses on solving the omitted variable bias problem. Because various variables are often correlated with each other, examining the effect of x on y is problematic unless one controls for all other variables that happen to be correlated with x and y. Intuitively, failure to do so means that some of the effect of z on y will be captured in an estimate of x's effect on y. The estimated correlation will include the "true" effect of x on y, but it will be biased because of the unaccounted-for effect of z. A naive response would be to simply control for all of the other variables that matter, but this is often technically difficult if, for example, the data on z have not been collected or involve some measurement error. Sometimes which z variable should be included is unknown. Even the best economic or legal theories do not completely lay out all of the determinants of y and how

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52. For a review (and an example) of this bad literature, see Rafael La Porta, Florencio Lopez-de-Silanes & Andrei Shleifer, *The Economic Consequences of Legal Origins*, 46 J. ECON. LIT. 285 (2008).

53. Simultaneity problems occur when two variables simultaneously cause each other. John Antonakis et al., *On Making Causal Claims: A Review and Recommendations*, 21 LEADERSHIP Q. 1086, 1094–95 (2010).

54. On this issue, see Jonathan Klick, *The Perils of Empirical Work on Institutions*, 166 J. INST. & THEORETICAL ECON. 166, 166 (2010).

55. *See generally* Eric Helland & Jonathan Klick, *Legal Origins and Empirical Credibility*, *in* DOES LAW MATTER? ON LAW AND ECONOMIC GROWTH 99 (Michael Faure & Jan Smits eds., 2011) (showing that the results are actually not robust at all and arguing that, even if they were, it would provide no confidence that the relationships are causal).

56. For a nice discussion of the improvements that have been made in the field, see Joshua D. Angrist & Jörn-Steffen Pischke, *The Credibility Revolution in Empirical Economics: How Better Research Design Is Taking the Con out of Econometrics*, 24 J. ECON. PERSP. 3 (2010).

they are related to x. Other times, while z is known, it is impossible to code it in a principled way.

This omitted variable problem is ubiquitous when dealing with observational data.[57] The sources of this omitted variable problem in legal analyses are varied. When examining the outcomes of cases as a function of time or jurisdiction or substantive area, one form of the omitted variable bias that arises is a selection effect whereby cases may settle differentially across time or jurisdiction or substantive area. Since those settled cases do not have observed judicial outcomes, there can be no confidence in inferences based on observed cases. Similarly, when trying to examine the effect of a legal rule on behavioral outcomes, if the legal rule is adopted by some jurisdictions because of unobserved characteristics or changes in background trends that affect both the likelihood a jurisdiction adopts a rule and the underlying behavior, there can be no confidence in estimated correlations.

Modern empirical microeconomic work focuses on what are referred to as natural or quasi-experiments, where the researcher attempts to exploit seemingly random variation that affects the policy of interest. Work in the area of criminal law and policy provides some of the best illustrations of this approach. For example, along with Tabarrok, I have some work[58] showing that when the level of police protection rose in Washington, D.C., during periods of concern about terrorism, crime fell dramatically, and when the police protection went back to normal levels, crime reverted to its baseline.[59] Usually the study of police and crime levels is hampered by the fact that places that have (or expect to have) high crime levels are also the ones that hire more police, but it is not possible to adequately control for these expectations when calculating the correlation between police and crime. Because the terror concerns we relied on were unrelated to issues having to do with crime, we could have some confidence that our estimated effect of police on crime did not suffer from an omitted variable bias.[60]

Another good example is provided by Helland and Tabarrok where they examine the effect of three-strikes laws on criminal activity.[61] Again, in the standard case, it is not possible to simply look at crime levels between places that do and do not have such laws since chances are that the places adopting three-strikes laws are doing so because of their beliefs about the trajectory of

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57. Experimental analyses avoid the problem by relying on explicit randomization of the x variable of interest (the so-called treatment). If there is randomization, even if a z variable that matters for y is not accounted for, there is no bias in the estimated correlation between x and y since there is no correlation between z and x.

58. Jonathan Klick & Alexander Tabarrok, *Using Terror Alert Levels to Estimate the Effect of Police on Crime*, 48 J.L. & ECON. 267 (2005).

59. *Id.* at 271.

60. We verified that other changes, such as a reduction in the number of tourists, were likely not occurring simultaneously. *Id.* at 271–72.

61. Eric Helland & Alexander Tabarrok, *Does Three Strikes Deter? A Nonparametric Estimation*, 42 J. HUM. RESOURCES 309 (2007).

crime or because public opinion is becoming more receptive to all sorts of anticrime measures, not all of which can be quantified. Helland and Tabarrok solve this problem by examining individuals arrested for the same crimes before the three-strikes laws were even considered. For seemingly random reasons, one of them pleads to a crime that falls outside of the three-strikes law as adopted later, while the other one agrees to a plea involving a crime that is eventually covered by the three-strikes law.[62] This shows that the individual who is randomly hit with a strikable offense (after the fact) appears to be deterred from engaging in criminal activity relative to his otherwise similar counterpart.[63] Because of studies like this, our knowledge of the causal effects of criminal law and policy has grown enormously in the past decade or so.

For all the advances made through these empirical tools, however, they fundamentally can only identify marginal effects. That is, my work with Tabarrok tells us zero about why the baseline level of crime in Washington is higher than that in New York City. The Helland and Tabarrok work is not useful for determining why any given individual commits a crime to begin with. The tools we use only help us to understand what changes occur when a policy is implemented (or, more generally, when a particular x variable changes) relative to some unexplained preexisting baseline.

Shleifer's empirical work does not fit this model. Rather than focusing on well-identified marginal effects, Shleifer purports to explain baselines, largely ignoring the hopelessness involved in any such attempt. Three empirical chapters in this book rely purely on comparing outcome measures across jurisdictions that have different legal institutions, drawing conclusions based on those correlations. So, for example, in the chapter entitled "Courts,"[64] which attempts to analyze the relationship between legal formalism and the ability of parties to quickly settle disputes and finds that greater formalism is associated with delay with no apparent offsetting accuracy benefit,[65] Shleifer concludes "our results suggest a practical strategy of judicial reform, at least with respect to simple disputes, namely the reduction of procedural formalism."[66] At no point does the analysis rule out the possibility that, for example, the formalistic French are not simply different than the less formal Americans in other ways that are likely to impact delay. This kind of cross-sectional comparison has no chance of sorting out these issues, and conclusions based on this analysis are close to worthless in terms of having confidence in causality.

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62. *Id.* at 312–13.

63. *See id.* at 312–14, 326–27 (showing deterrence rates drawn from the study's data).

64. SHLEIFER, *supra* note 10, ch. 5; *see also* Simeon Djankov et al., *Courts*, Q.J. ECON. 453 (2003) (chapter published as article).

65. *Id.* at 106, 141.

66. *Id.* at 142.

The chapter "The Extent of the Market and the Supply of Regulation"[67] is similar in this respect. Shleifer presents evidence that jurisdictions with larger populations adopt more regulations, and concludes that this evidence supports the view that there are fixed costs in implementing regulations, and therefore that large scale is necessary to justify undertaking those costs.[68] While the evidence is consistent with that hypothesis, it is also consistent with a hypothesis that people like regulations and so there is more movement into places that are expected to increase their regulation. It is also consistent with the hypothesis that policy makers believe more people require more regulation since individualized litigation will be more difficult with a large population. There are probably a dozen more plausible stories that are also consistent with the findings.

In "The Regulation of Entry,"[69] Shleifer presents evidence suggesting that larger barriers to setting up a new business are "associated with greater corruption and a larger unofficial economy, but not with better quality of private or public goods."[70] Again, not having actually observed a plausibly random change to the regulation of entry in a sample of countries, Shleifer is left making inferences about baselines, and that kind of analysis is about as reliable as if Shleifer had simply written an article called *I Think Barriers to Entry Are Bad* with text saying, "See the title."

The remaining empirical chapter, "The Evolution of a Legal Rule,"[71] is effectively a case-counting exercise meant to see if states converge to the presumably efficient economic loss rule.[72] Finding a nontrivial number of instances where courts diverge from the rule and no steady trend toward it, Shleifer concludes that "the hypothesis that, in commercial fields, the common law is predictable and efficient, or at least is moving there, is not supported by our study."[73] Putting aside the question as to whether the economic loss rule is efficient or whether by "efficiency" we mean making tradeoffs across many dimensions at the lowest cost, as an empirical matter, it is very difficult to draw strong conclusions from a reading of appellate cases due to sample selection problems and other kinds of omitted variable biases.

The funny thing is, in many ways, I agree with Shleifer's conclusions, but the empirics add nothing to my confidence in them. Much like the theory chapters, the methodological machinery does little to move the ball forward.

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67. *Id.* ch. 9; *see also* Casey Mulligan & Andrei Shleifer, *The Extent of the Market and the Supply of Regulation*, 120 Q.J. ECON. 1445 (2005).

68. SHLEIFER, *supra* note 10, at 262.

69. *Id.* ch. 2; *see also* Simeon Djankov et al., *The Regulation of Entry*, 117 Q.J. ECON. 1 (2002).

70. SHLEIFER, *supra* note 10, at 298.

71. *Id.* ch. 10; *see also* Anthony Niblett et al., *The Evolution of a Legal Rule*, 39 J. LEGAL STUD. 325 (2010).

72. SHLEIFER, *supra* note 10, at 78–79.

73. *Id.* at 104.