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The Strategy of Methodology: The Virtues of Being Reductionist for Comparative Law

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This is a pre-publication draft of a response to three comments on my paper entitled “Levers of Legal Design: Institutional Determinants of the Quality of Law” which appeared together with these comments in the inaugural Focus Feature of the University of Toronto Law Journal, April 2009. The published version is available at <http://utpjournals.metapress.com/content/120886/>

It is hard to say things that are both true and important. Although the economist is often tagged with a type of megalomania, in fact the methodology of economics is fundamentally informed by this notion. Economic analysis is partial. Honestly employed, it disavows a broader claim than it has taken on and it knows that to make any claim that is both potentially true and non-trivial it is necessary to be sometimes brutally incomplete. Economists may focus on analysis of the individual decisionmaker as the basic building block in their models, but in their stance vis-à-vis saying something of value to the world, they are thoroughly communitarian: it takes a village to produce a policy. Few economists are willing to take a single paper, knowingly reductionist, and from there offer a broad unadorned call to action. When they do, they make a mistake.

Most people traveling the interdisciplinary path in law these days are learning that the relationship between the various disciplines on which law can fruitfully call is not, and should not be allowed to devolve into, a battle for dominance between economics, sociology, politics, philosophy, cultural studies and the like. And indeed, the efforts at interdisciplinary thought within legal scholarship and law schools are, in my experience, often farther along than in universities more generally. In my view that is because as legal scholars what holds us together, or at least what can hold us together, is common purpose. One cannot be a legal scholar without sharing the goal of understanding the law.

Some seek to understand the law as a human phenomenon, as a cultural artifact, as a text; to seek the meaning in law in the way one would seek the deeper meaning in a piece of literature or art or social practice. The commitments here are to the non-instrumental. Ernie Weinrib says that (private) law is like love; it's only purpose is to be law. We approach law in this mode in a way deeply respectful of what law is; we argue about what we can ever express about what law is. We count our successes by how well we have interpreted law on its own terms. The scholar's ethical commitment is, in essence, to law itself. In many ways this is essential to what it is to practice law, to be committed to a shared purpose of finding and sharing the inner meaning of a legal act or principle. It is something we consider fundamental to the process of creating a lawyer.

But some also seek to understand law as an instrument. Understanding law as an instrument is goal-oriented, directed not to measurement against hermeneutic standards but against practical

ones. Law is not (just) a text; it has effects in the world, effects on the amount of food, safety, opportunity, agency, and so on that real people experience. Here the scholar's ethical commitment is to the person on whom law operates, for whom it creates a world with mortgages and bank balances; relationships with children, lovers, former spouses and employers; opportunities for education and building a business; clean air and water; security, dignity and privacy. Law is not only the expression of relationships, it is the constitution of them. It lives in a system—a collection of institutions and actors that organize how they wield authority over one another, what is shared and what is withheld, what is allowed and what is not. Lawyers create that system, deliberately by what they choose and organically by how they act and interact.

How law and legal institutions work systemically to produce people's lives – not in its individual instances but in its often obscure aggregative effects—has not traditionally been the affair of legal education and scholarship. Perhaps this is why the scholar who focuses on the instrumental is so often seen as an interloper doing violence to law itself. It may be true that there is an “untranslatable abyss” between the law of one place and the law of another—just as there is between one person's experience of a strawberry and another's—but this does not mean that we have no business seeking to understand why law here produces this effect and law there produces that effect.

To my mind, the goal of the legal scholar is to be competent across these different perspectives. None of us will be an artist at all of them. But neither can we approach the other with ignorance and disdain. We need to strive both to know law better on its own terms and to assume responsibility for the effects of what we create. And we need to be able to learn from one another because interdisciplinarity will never mean that we are all deeply knowledgeable about everything; at best it will mean that we are conversant with and willing to learn from our fellow legal scholars.

What does this mean for the role of economic modeling and analysis in comparative legal scholarship? I am honored by the attention my paper on “Levers of Legal Design” has received from three fine comparative law scholars and keenly aware that I am a Gillian-come-lately to this field. I take on completely Ralf Michaels' tactful observation that there is much more in the comparative literature on the fine-grained detail about the institutions in a variety of legal regimes than I, having only English and middling French to draw upon, was able to muster and it was indeed with considerable trepidation that I ventured what were inevitably overly general or outdated comments on existing systems. Let me however focus more closely on the question of how economic analysis in general, and my paper and these critiques in particular, relate to the point that Michaels expresses as the tension between the need for “thick description” of legal regimes and the abstraction inherent in analytical methods like economics.

I think the virtues (which come at a cost) of the abstraction necessary to do economic analysis can best be seen by addressing some of the key criticisms Ralf Michaels and John Reitz offer.

The structure and content of the model

Abstraction in economics—making assumptions—is not that different from abstraction in, say, philosophical argument. Economists abstract in order to build models. They build models in order to untangle complex and hard-to-decipher real world interactions and focus attention on the detailed structure of a logic of how processes and systems work. Like the philosopher, the economist is interested in the intricacies of how and whether Y follows from a premise X, and indeed whether steps A, B and C are necessary or sufficient to get from X to Y. Using math—not numbers but mostly algebra—is a stepped-up version of this basic method of honing attention to the inner logic of an argument.

The virtue of building a model is that it allows a clear conversation about what is and is not being claimed. This is what allows me to respond with a fairly crisp answer, for example, to John Reitz’s concern that I am misguided in focusing on the “adaptability” channel, as opposed to the “political” channel, to explain the impact of legal origin on economic variables arguably identified by the empirical literature starting with La Porta and co-authors in 1997.¹ (“LLSV”). As Reitz describes it, the “adaptability” argument claims that legal origin has an impact on economic welfare because “judges in common law systems have greater freedom to change the law.” The political argument is that with greater judicial independence, “common law courts are able to stand up to state power more effectively” and thus promote the efficiency of markets as against government intervention. Reitz has read my model to construct a more nuanced argument for the adaptability channel in this debate, namely that it is not “common law” systems per se that gives judges greater freedom to change the law, but rather specific institutional features such as the organization of the courts and the judiciary, procedure, opinion-writing practices and so on. That is he assumes I make a different move at step one—what produces greater judicial freedom to change the law—and the same move at step two—greater judicial freedom to change the law produces better law.

Understanding why this is not what my model is doing is helpful in illuminating how my project departs from the existing legal origins literature. While it is true that I am focusing on adaptation of the law through adjudication as a key mechanism by which legal institutions have an impact on the quality of law (meaning here the capacity of law to promote social welfare), I am not building on a simple logic of “legal systems produce better economic outcomes when judges have greater freedom to change the law.” I’m rejecting not only step one (common law systems give judges greater freedom) but also step two (greater judicial freedom produces better law.) Indeed, I am rejecting the framework—the set-up—of the legal origins model. The flaws in that framework, I claim, are that it does not explain why judicial freedom would produce better law

¹ Rafael LaPorta, Florencio Lopez-de-Silanes, Andrei Shleifer and Robert Vishny, “Legal Determinants of External Finance” 52 *Journal of Finance* 1131 (1997); Rafael LaPorta, Florencio Lopez-de-Silanes, Andrei Shleifer and Robert Vishny “Law and Finance” 106 *Journal of Political Economy* 1113 (1998).

and it treats the behavior of judges (what decisions they make in individual cases) as if it were itself an institution. That is, the economists in this literature assume that by definition a “common law” system gives judges greater discretion than a “civil code” system.

My starting point is to, first, reject the common law/civil code distinction and focus instead on individual institutional components of legal regimes and to then treat judicial decisions to change the law as the product of an institutional environment and not an inherent characteristic of a legal tradition. In addition, I do not assume that it is “judicial freedom to change the law” that is the relevant variable (either as a dependent variable in step one—something predicted by the institutional environment—or as an independent variable in step two—the cause of changes in the law to produce better economic outcomes.) Instead, I go back to basics and ask: how does information about the welfare effects of an existing rule and a potential new rule get into the legal system? When might that information prompt a change to a new rule, by individual judges across the system as a whole? “Judicial freedom to change the law” is not part of this model, largely because it’s neither clear what “freedom” means here nor is a change in the law simply a matter of what judges decide they want to do with their “freedom.”

My model does what an economic analysis does, namely, selects specific elements out to focus on a subset of the decisions that individual actors will make and how those decisions interact to produce effects. I focus specifically on the situations where the law is, in fact, out of step with local or changing conditions and where, in the abstract, a change to a new rule would produce more of what a particular society wants its law to produce. So I am asking, *if* we want law to adapt to better promote desirable ends, what institutional attributes will make adaptation through the judiciary more or less likely? Obviously, the answer to this question cannot be the answer to the broader question the legal origins literature asks: which legal regimes will produce better economic outcomes? But it is, I believe, an answer we need to understand more deeply before we can hope to assemble the answer to the broader question. And what my model reveals is that even this component question is a complex one that cannot be adequately answered with “judicial freedom to change the law.” Taking seriously how judicial institutions work—they are reactive and learn primarily from those before them—reveals that the effect (change in the law when change, by assumption and in the abstract, would be desirable) requires both that judges have an incentive to make welfare-promoting changes in the law *and* that litigants have an incentive to bear the cost of asking and persuading a court to change the law. Moreover, analyzing these incentives on the part of judges and litigants then reveals that they are interdependent and in a subtle, and recursive, way connected to the very reason we are interested in legal change, namely the “error” (from a welfare point of view) that is made when judges persist with the existing rule. The connection is that when judges lack the information they need (the ‘legal human capital’) to recognize the impact of an existing rule on social welfare and the potential value that can be created by changing the rule, seeking change is a risky investment for litigants and a risky action for judges, even if judges anticipate that they will be rewarded (in

their own esteem, their professional standing or some other dimension they care about) for making welfare-improving adjustments in the law.

The point of building a model that starts with the basic building blocks of the incentives of judges and litigants and what decisions they will make about investing in and using information, rather than judicial freedom as a stand-alone variable, is to lay bare how much more complex the claim about judicial adaptation is. Take for example Reitz's view that I have overlooked a key factor in not emphasizing the impact of fee-shifting on the extent of judicial adaptation of rules. The model he (implicitly) is working with is one in which judicial freedom to change the law produces change and the more opportunities for changing the law judges have (the more litigation there is) the more the law will change. I think the volume of litigation will affect rates of legal change, but in order to look at the subtleties of what produces legal change through adjudication, I deliberately controlled for this difference in the volume of litigation across regimes (I also assumed every plaintiff sued and no cases settled in order to take these, obviously relevant and important, considerations off the table for now.) This allows me to show something that is not otherwise obvious, namely that even *if* a system has lots of adjudication it may not have a lot of rule change; and there can be systematic differences between systems even if they do not differ at all in terms of the volume of opportunities that judges have to adapt the law. In particular, I identify a host of institutional factors that are not initially obviously connected to the capacity for judicial adaptation—such as the mechanisms by which judges share information and the audiences who are able to judge a judge's work. Moreover, I am able to emphasize that greater opportunity for judicial adaptation is not necessarily a good thing, because the incremental process of going from “low information/high error” to “higher information/lower error” is a costly one, both in terms of the expenditure on legal resources and the mistakes judges make along the way.

The problem of interdependence and the analysis of systems

I think one of the most challenging aspects of accepting economic analysis as one (among many) tools in the comparative lawyer's toolkit is that economics requires the individual scholar to give up grand claims in favor of more piecemeal contributions, in the hope that when aggregated through a literature individual contributions provide insight into the whole. Comparative lawyers, I take it, generally see legal systems as their unit of analysis. Ralf Michaels suggests, for example, that rather than focusing on individual actors, I would have more to say to comparative law if I focused on “entire legal systems” and the “ability of the legal system as system to evolve.” I certainly agree that the goal is to ultimately be able to say something about legal systems as a whole and indeed it is precisely my goal to make a contribution to understanding how legal systems as systems evolve. But I think it is a mistake to think that we can analyze systems as a whole without analyzing their parts.

This is very different from saying that the parts operate in isolation and that the whole is just the sum of the parts. And here I think Michaels misunderstands my method when he offers that

“where Hadfield focuses on institutional choices in relative isolation, I would want her to focus on their interplay, their mutual dependency.” Perhaps the most important reason for making a lot of assumptions holding various pieces of the whole constant (“*ceteris paribus*”) to focus on a subset of moving parts is precisely to be able to understand “their interplay, their mutual dependency.” My model does this, bringing out some very subtle interplay between the behavior of judges and litigants and how that interplay—not any action in isolation—produces (or not) legal change. I am specifically interested in the systemic accumulation of expertise about the effects of law (shared legal human capital) through the interplay of behavior and institutions such as peer review systems, opinion-writing and publication practices and rules of evidence and procedure. Moreover, by constructing a well-specified model that sharply delineates what does and what does not change—which holds constant some features of the environment and then assess individually how changes in those features will change predicted results in the system (this is the method of comparative statics)—I lay a clear path for future research. For example, I take Michaels’ point about the interplay of institutional choices to mean that making one change in an institutional environment—such as shifting to a capstone judiciary—might require or entail or prompt other institutional features to change—such as legal education or opinion-writing practices. This kind of interrelationship among component parts is indeed what economists hope to identify. The fact that I have not focused this model on predicting how a change in the judiciary might prompt change in legal education or opinion-writing practices, for example, does not mean that I do not fully expect that careful attention might uncover some predictable relationships. It is just that this is too much to do in one paper.

The focus on judicial, as opposed to legislative, adaptation of legal rules

Both Michaels and Reitz are somewhat perturbed in particular by my selective focus on courts and the judiciary as opposed to legislatures and worry that this focus amounts to stacking the deck in an evaluative sense in favor of systems that are more heavily judge-driven (i.e., common law systems). This is quite understandable given that the legal origins literature claims that judges do a better job than legislatures in supporting a market economy.

My goal in this work is to focus attention on the role of the systemic accumulation of judicial legal human capital—expertise about the how rules work in practice and the effects they produce—in the process of adaptation of the law to new information due to changes in the environment. Excluding attention to the accumulation of legal human capital in other locations such as legislatures and administrative agencies and focusing on the adaptation of law through courts rather than legislatures, is indeed a deliberate step in the set-up of my model. But this is not because I assume that legal human capital does not or cannot be accumulated in non-judicial bodies or that legislatures do not play a key, perhaps dominant, role in legal adaptation over time. It is because my goal was to hone in on how the structure of distinctively legal (as opposed to political) institutions (the judiciary, procedure, the legal profession, and so on) affects the quality of law. Legislative behavior is one of the “*ceteris*” that is held “*paribus*”.

This approach does not allow a comparison writ large, on the basis of one paper alone, between systems that rely more or less on adaptation of law through the process of adjudication and judicial decisionmaking. I do not make that claim and my paper should not be read to express an opinion on this. As I discuss in the paper, I treat the quality of the law produced by legislatures, regulators and other non-judicial bodies—the stuff that judges interpret and apply—as exogenous, that is to say, outside the model. I note that if these lawmakers are “capable of identifying the need for rule adaptation, obtaining the necessary information to adequately modify the rule, and transmitting that modified rule to courts, then there is less demand from a social welfare point of view for judicial rule adaptation.” (pp. 62-63) I do accept the criticism, however, that my use of the term ‘legal’ to describe only the institutions that enforce or deploy the law can be confusing. My goal is largely corrective in the sense that a great deal of the normative work on what law will help countries better achieve their goals focuses on the enactment of rules and little on the organic process by which rules are implemented and adapted and so I de-emphasize, as I discuss in the paper, formal rule creation. But rather than saying that my model identifies “the” levers of legal design, I should have said it identifies “some of the” levers of legal design.

So why do this? Why not include the capacity for legislative adaptation in the model? The answer is again that when there are too many moving parts, it is difficult to isolate insight into any one of them. What I hope emerges from my effort is greater insight into what I believe is a particularly important, and largely overlooked, mechanism in judicial adaptation. This is the process by which information—the raw material of adaptation—makes its way into the legal (judicial) system. My results highlight some subtle aspects of this process, and some subtle interactions between judges and litigants in this process. The goal of the institutional analysis in the paper is to look systematically at how particular institutional attributes (the structure of the judiciary, the practices of opinion writing and publication, the nature of judicial education and peer review, evidentiary and procedural rules, and so on) affect the interactive decisions that judges and litigants make and how those in turn affect the capacity of the adjudication system as a whole to learn from the environment in order to adapt the law in welfare-improving ways. The only comparison here is between different institutional settings as they could affect *judicial* adaptation of law. This model cannot and is not intended to say whether judicial adaptation of the law will do better or worse than legislative adaptation. But it is intended to inform how that next step in this line of research might proceed by deeper analysis of judicial adaptation: we cannot compare judicial to legislative adaptation if we have not thoroughly understood how judicial adaptation works and how it is affected by the institutional environment.

There is another important ‘reductionist’ move in this paper that intends to eliminate important attributes of the overall problem in order to focus on a narrower question. This is the assumption that when judges do consider changing the law they experience the highest reward for doing so when they change it in ways that are approved of (according to whatever legitimating process we might suppose) by the society in which they live. They are not corrupt. I do not make this

assumption because I do not think some judges are corrupt or that corruption is not a problem, perhaps even a major problem, in some legal regimes. Again, it is a methodological decision, not an ideological decision, intended to see what happens even *if* judges are faithful to their appointed role of implementing the laws.

The meaning of ‘welfare’ and the ‘quality of law’

A deeper worry about the consequences of a ‘reductionist’ methodology for comparative law—committed as it is to understanding legal systems on their own terms—is that not only are the models partial but the normative framework is also inherently partial, and in particular, partial to the values held by a particular body of researchers. Non-economists worry that when economists have at a problem they are interested only in material welfare and economic efficiency understood in a consumerist sense. This is a particular concern in comparative law because of the risk, which Pierre Legrand suspects is the economist’s covert agenda, that all legal systems will be subjected to a common normative criterion and measured against it: English law is good, French law is bad, French law should be changed to look like English law.

While there is certainly no shortage of economists who understand ‘welfare’ exclusively in terms of material consumption of goods and services and who do seek to compare across systems in terms of a common criterion of welfare defined in this way (LLSV for example), this is not an inherent attribute of economic analysis. Nor is it an attribute of my analysis. The normative criterion I employ is social welfare, meaning the values-informed assessment of a ‘society’ of what are good outcomes. It may be more expensive to produce food on small family farms rather than large corporate farms, but if a society values the social relationships that result from family farming or the continuity of a tradition of family farming, then the social welfare function should reflect this. A social welfare function is fundamentally normative, contested and determined with reference to the particular population of people that it purports to represent. It is not the welfare function that professional economists prefer or are able to measure for purposes of empirical work. Nor is it restricted to material consumption values; it can include the values associated with dignity or equality or autonomy, for example. Although some law and economics scholars have sought to defend this claim² the use of economic methodology does not imply a necessary commitment to efficiency as a normative criterion, exclusively subject assessments as the measure of utility or market-tradeable goods as the only source of human well-being.³

² Louis Kaplow and Steven Shavell “Fairness versus welfare” 114 *Harvard Law Review* 961 (2001).

³ I explore this point in the context of assessing whether feminist commitments to, for example, equality or the non-market provision of caring labor, are compatible with law and economics methodology in Gillian Hadfield “Feminism, Fairness and Welfare: An Invitation to Feminist Law and Economics” 1 *Annual Review of Law and*

In my model, there is no inherent content to the concept of social welfare. Most importantly, the concept of welfare is the concept of welfare that captures the commitments of the particular society we are considering. If French assessments of what produces human well-being are different from English assessments, then the social welfare function differs. It is crucial to recognize that the comparative exercise here is not comparison across actual regimes but theoretical comparison across hypothetical—possible—regimes. The comparative exercise my model engages compares across hypothetical regimes that have the same social welfare function, which is to say that I am effectively asking the question: given this social welfare function, what is the impact of particular changes in the institutional environment in this regime on the capacity of the judicial system to produce welfare-improving adaptation in the law? From a policy perspective—and I think this is clear in the paper—the question is, how can a particular society achieve better results evaluated according to its own assessment of what is socially valuable? The reference point—the ‘measure’ of welfare—is entirely internal to that regime. What will help the Slovak regime achieve the goals the Slovak people have? There really is no interest in my work in making the French law of real estate into the English law of real estate only in analyzing what might make French law better or worse from a French perspective. When judges in my model “get it right” and avoid “judicial error” they adapt the law in ways that through some legitimating process a society has decided they would want the law adapted.

Note that I do not assume that adaptation of the law is always welfare-improving. I examine those cases in which it is. Thus there is no inherent bias towards change rather than stability, as Ralf Michaels worries. Rather, my project is to say: suppose we are concerned about whether a legal system that needs to adapt to improve welfare—say we are looking at a transition state in which judges are faced with a lot of new law and the early interpretations of the law are badly informed and hobbling the capacity of the law to achieve its intended effect. Then, what are the institutional characteristics that might make such a regime more capable of moving past the errors of early decisions and becoming more intelligent in interpreting and applying law? And, even less ambitiously, I do not (as John Reitz worries) intend this paper alone to have crisp policy recommendations about which effects run which way; as Reitz notes, several of my conclusions are of the flavor “this could go either way.” What I do intend is to move the collective effort of developing more concrete policy recommendations further along, and to provide guidance for the next set of papers that might provide input on this question.

The role of “culture”

Social Science 285 (2005). As I note there, Kaplow and Shavell’s effort to defend an exclusive focus on efficiency and radically subjective utility assessments—with no objective judgments about what constitutes value or the good—largely fails and for reasons that the welfare economics literature has understood well since Kenneth Arrow *Social Choice and Individual Values* (1951). For a discussion of these points in welfare economics see Amartya Sen “Social Choice and Justice: a review article.” 23 *Journal of Economic Literature* 1764 (1985) and Amartya Sen “The possibility of social choice” 89 *American Economic Review* 349 (1999).

The biggest challenge for the greater use of ‘reductionist’ methods like economic analysis in comparative law is, I believe, cultural. I mean this in two senses. Economic scholars and traditional comparative law scholars live in different intellectual cultures. They speak different languages, value different things and have a different shared terrain of what has gone before and where the discipline is headed. And—this is the second sense in which the divide is ‘cultural’—they conceptualize the role of legal or judicial culture differently in the task of understanding the legal communities. To put it in the reductionist terms of the economic modeler, traditional comparative law (as I understand it) has treated culture as a fundamental independent variable—something that explains differences. Economic analysis treats culture as a dependent variable—something to be explained. It is not wrongheaded to devote scholarly energy to rich thick descriptions of existing culture. But nor is it an insult to the role of culture to seek to explain it as a response (at least in part) to other factors. The ultimate aim, I think, has to be to mutual enrichment of both scholarly efforts.

Particularly where our goal is fundamentally dynamic—to understand how law evolves—it is important, I believe, not to treat judicial or legal culture (ideology or attitudes) as a fixed feature of a given legal community. The strategy of my approach is expressly to assume that there is nothing inherently different between the way judges in a capstone judiciary view the world and their role and the way those in a career judiciary do. John Reitz worries that this is doomed to fail because culture, particularly attitudes about the proper role of the state versus markets, is a fundamental (independent, exogenous) characteristic of a legal regime. My goal, however, is to see whether we can find in different judicial institutional structures a basis for predicting, rather than assuming, that judges in one will be more inclined to value adaptation to information about the environment while judges in the other will be more inclined to value stability of existing legal rules. The model suggests that there are factors that could explain such differences: the nature of the judicial audience (peers versus the general public) and the potential for promotion within the system, for example. I do not intend anything so crass as to dismiss out of hand the German tradition (as Michaels describes it) of emphasizing intrinsic coherence and adapting law only in relation to the internal premises of law rather than external societal requests for change. But I do intend to put on the research agenda the question of what institutional attributes might build and/or stabilize such a tradition, and whether such a tradition is mutable. This does not presuppose an answer to the question of whether an emphasis on societal adaptation is preferable for law to an emphasis on internal coherence. But it does say, *if* we have a goal of increasing the capacity of law to adapt organically through adjudication to local and changing circumstances more effectively, what institutional features might promote the achievement of that goal? Put differently, how might a particular ‘culture’ of judicial decisionmaking be developed (in a new legal regime, for example) or changed (in the context of increasing heterogeneity and more rapid change in the environment in which law operates, for example.)

Again, it is important to emphasize that the model is partial, that this method of analysis does not purport to be a complete account of what creates, sustains and changes a judicial philosophy or

culture. Changes in the institutional structure might not, or might not quickly, change culture, even though the reductionist model appears to predict that this would be instantaneous. One of the features of culture is precisely that it is a social phenomenon that is not fully accounted for by an account of individual incentives and behaviors. But if we are in agreement that legal and judicial culture is an important part of how law works in fact, and that law and legal actors—and legal culture—do evolve and change over time, then clearly one piece of the analysis needs to work through how institutional and incentive structures may provide part of the ground on which culture is erected and stabilized. It can only move this goal forward to have other pieces of the work focusing on thick descriptions of legal culture.

So let me conclude by echoing Ralf Michael's hopeful view that we are seeing the development of a more truly interdisciplinary collaboration between law and economics scholars and comparative law scholars. One of the scholarly values of comparative law that I see in the thoughtful comments on my paper is a strong commitment to respect for the internal coherence of other systems and an effort to understand and evaluate them on their own terms. This is also wise advice for those of us who seek to improve understanding through interdisciplinary conversation and convergence: we will travel further if we understand what each has to offer and look for the places where we can accomplish more of the goal we all share—a better understanding of law and how it operates in and produces the world—than if we see in the other's methodology covert agendas, narrow-mindedness or sloppy thinking. Reductionist methodology has value, even for a community of scholars that ultimately cares deeply about the richness of the actual. And model-building is better if it is informed by that richness.