New Institutional Economics

Institutions frame behaviors and exchanges in markets, business networks, communities, and organizations throughout the world. Thanks to the pioneering work of Ronald Coase, Douglass North, and Oliver Williamson, institutions are now recognized as being a key factor in explaining differences in performance between industries, nations, and regions. The fast-growing field of "new institutional economics" (NIE) analyzes the economics of institutions and organizations using methodologies, concepts, and analytical tools from a wide range of disciplines (including political science, anthropology, sociology, management, law, and economics). With contributions from an international team of researchers, this book offers theoreticians, practitioners, and advanced students in economics and social sciences a guide to the recent developments in the field. It explains the underlying methodologies, identifies issues and questions for future research, and shows how results apply to decision-making law, economic policy, managements, regulations, and institutional design.

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New Institutional Economics

A Guidebook

Edited by

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To the more than six hundred fellows who have been teaching, thinking, learning, discussing, sharing meals, and even dancing at the European School for New Institutional Economics (ESNIE) every spring in Corsica since 2002

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Acknowledgements

Every year in the spring, about one hundred scholars of more than forty different nationalities gather at the European School on New Institutional Economics (ESNIE), which is organized at the Scientific Institute of Cargèse, in Corsica (France). New institutional economics (NIE) is being taken in the broad perspective of understanding institutions, organizations, and contracts, through the lenses of economics (and vice versa). The aim of this annual event is to bring together distinguished scholars and PhD students, post-doctoral students, and researchers who are interested in developing the field to provide the latter with intensive training and to develop networking within the NIE community

ESNIE is characterized by multidisciplinary approaches, and by a variety of techniques and tools, and brings to the same place and for a short period of time a number of high-level experts from many different fields and traditions. The event is a unique opportunity to advance the state of the art of this field and, more crucially, to teach and transfer the methods, theories, and research practices of NIE. In return, the young scholars, on the basis of their innovation capabilities and dynamism, actively contribute to the development of the field.

ESNIE is progressively becoming a platform around which many cooperations are organized. Meetings, workshops, publications, research projects, and networks have been launched under the patronage of ESNIE, which is no longer seen as just a spring school nor just an organization, but, rather, as something of more value: a lively and permanently renewed community.

This book is one of the results of this community. It draws from lectures which were initially given at ESNIE and seeks to contribute to the diffusion of the knowledge which was developed and accumulated thanks to this open platform of exchange, discussion, and training.

We are indebted to all the first-class scholars who contributed to ESNIE, and to the development of its reputation, and to the many organizations which sponsored its performance. We are also grateful to those who actively contributed to the organization of the various ESNIE

sessions and related events. These people and organizations are too numerous to be mentioned individually. All their names, and even more importantly, their contributions, are available online at www.esnie.org.

None of this would have been made possible without the dynamic initiated in 1997 by the founders of the International Society for New Institutional Economics (ISNIE). ISNIE and ESNIE are sister organizations and many of the contributors to one are also involved in the other. We wish to extend our thanks to those who funded ISNIE and developed its activities.

Foreword

The New Institutional Economics Guidebook

Oliver E. Williamson

New institutional economics (NIE) has been in existence for thirty years and counting. Periodic reassessments of accomplishments, limitations, and unmet needs are useful for a young field such as this. *The New Institutional Economics: A Guidebook* is such an undertaking.

Having previously written overviews on NIE, I refer interested readers to them.[1] But for a short introductory summary of key ideas and accomplishments of the NIE and some concluding remarks, I have organized the Foreword to this book around provocative passages from each of the chapters. On my reading, the chapters in this book provide corroboration for the proposition that "the new institutional economics is a boiling cauldron of ideas. Not only are there many institutional research programs in progress, but there are competing ideas within many of them" (Williamson 2000, p. 610). That is both the spirit of this guidebook and of my remarks on individual chapters.

The appearance and development of a *new* institutional economics presupposes a predecessor – to which NIE presumably both relates and differs. The common ground is this: unlike the neoclassical resource allocation paradigm (which focussed on prices and output, supply and demand, and was dismissive of institutions [Reder 1999]), both older- and newer-style institutional economics insisted that *institutions matter*. The older institutional economics fell on hard times, however, because it lacked a positive research agenda (Stigler 1983, p. 170) and eventually ran itself into the sand. What NIE does that is different is breathe operational content into the study of institutions. Accordingly, going beyond the proposition that institutions matter, NIE also demonstrates wherein *institutions are susceptible to analysis* (Matthews 1986).

Notwithstanding the many successes of institution-free economics in the thirty years after World War II, there is also a downside. As perceived by Ronald Coase (1937, 1960), and others who took a hard look, orthodoxy was beset with conceptual lapses – many of which were traceable, directly or indirectly, to the unacknowledged assumption that transaction costs were zero. Pushing the logic of zero transaction costs to

completion often revealed that the emperor had no clothes. Slowly but surely the need to make provision for positive transaction costs and to ascertain how and why these differed among institutions began to register – with the result that gaps were filled, a deeper understanding of complex economic organization was realized, and public policy was reshaped. With the benefit of hindsight, key features of NIE projects include: (1) eschewing hypothetical ideals by focussing, always and everywhere, on feasible alternatives, all of which are flawed; (2) describing human actors in (more) veridical terms; (3) opening the black box of economic organization and uncovering the purposes served by the mechanisms inside; (4) adopting a main case orientation (of which transaction cost economizing is an obvious candidate); (5) operationalizing the project with reference to the microanalytics of transactions, governance structures, and the rules of the game; thereupon (6) deriving refutable implications and submitting these to empirical testing; and (7) working up the public policy ramifications.

NIE insights and reasoning have since displayed broad reach. Research in economics, the contiguous social sciences (especially political science, sociology, and the law), and applied fields of business (especially strategy, organizational behavior, and marketing) have all been invigorated. Understandably, a new field which displays such vitality has attracted the interest of young scholars – as witnessed by the names of the editors and contributors to this book.

This book, *New Institutional Economics: A Guidebook*, begins with Paul Joskow's introduction, "New Institutional Economics: A Report Card." The quotation that I have chosen from Joskow is this: "One of my colleagues recently suggested that institutional economists had 'won the war' in the sense that it is now widely recognized that understanding how institutions affect economic performance and why different institutional arrangements emerge in different social, cultural, and economic settings is now widely accepted" (pp. 17–18). With one small change, I completely agree. The change is this: rather than "won the war," I would say that we have "won many battles." That small change is consequential for two reasons. First, I am persuaded by Jon Elster's dictum that "explanations in the social sciences should be organized around (partial) *mechanisms* rather than (general) *theories*" (Elster 1994, p. 75; emphasis in original). On my interpretation, the focus on mechanisms directs our attention to the microanalytics of specific phenomena. That is where much of the action resides and where we have won enough skirmishes to persuade others that institutions truly matter and are susceptible to *analysis* (Arrow 1987, p. 734). Second, we are not really at war with anybody. Most students of NIE are pluralists and believe that it

is useful to examine complex phenomena through several perspectives – of which the (more microanalytic) lens of contract is one and the orthodox lens of choice is another. Paul Joskow's own research illustrates the productive use of both of these.

Chapter 1, by Pierre Garrouste and Stéphane Saussier, deals with "The Theories of the Firm." The quotation that I have chosen for this chapter is this: "We believe that Coase's (1937) article is a natural and obliged-to-read paper as it contains all the ingredients for a theory of the firm . . . [Upon] looking at recent developments of the theories of the firm, [however,] it is clear that . . . a unified and unique theory of the firm is a challenge – such a theory [does] not exist yet. That has to be resolved in the next coming years," (Coase 1937 p. 39). My responses are "Yes" and "No." Yes, Ronald Coase (1937) wrote a foundational article that both deserves and occupies a place of great honor in the pantheon of economics. And, yes, the development of a unified and unique theory of the firm is a huge challenge.

But while everyone should read Ronald Coase in the original, I do not know what to make of the claim that the classic article "contains *all* of the ingredients" (emphasis added). Notwithstanding the extraordinary insights of Coase (1937), I would describe this paper as the first stage in a natural progression – from informal to preformal, semiformal, and fully formal stages of analysis. It is, furthermore, noteworthy that the 1937 paper was not self-actualizing – as witnessed by Coase's remark, thirty-five years later, that his 1937 paper was "widely cited and little used" (Coase 1972, p. 63). Progressive operationalization has nevertheless been accomplished in the years since. This has entailed naming a robust "main case" (of which searching for prices is not one), uncovering the relevant mechanisms that distinguish firm and market organization in main case respects (to which law, economics, and organization theory are all pertinent), working up the logic, deriving refutable implications, and submitting these to the data.

Also, it is not at all obvious to me that a "unified and unique theory of the firm" is something that "has to be resolved" soon – although such a theory, if it were not vacuous, would be an auspicious accomplishment. Whatever, those who have general theory ambitions must be urged to develop a general theory which has some teeth. In the meantime many of us will continue to work in the style of Elster (by focussing on specific mechanisms rather than general theories).

Chapter 2, by Éric Brousseau, is titled "Contracts: From Bilateral Sets of Incentives to the Multi-Level Governance of Relations." This is an ambitious survey of the vast economics literature on contracting which has been under development over the past twenty years. Éric

Brousseau's survey reveals that a variety of contractual approaches to economic organization has been developed and is being employed by an ever-growing number of economists.

What are we to make of and what are we to do with this bounty of riches? First of all, multiplicity is a good thing in a new field such as this, which is seeking to understand complex phenomena. In circumstances where "any direction you proceed in has a very high a priori probability of being wrong . . . it is good if other people are exploring in other directions" (Simon 1992, p. 21). Moreover, more than one theory may be instructive, in that different theories could inform different aspects of the complex phenomena in question.

Furthermore, promising theories that "fail" are nonetheless instructive: "science . . . advances primarily by unsuccessful experiments that clear the ground" (Friedman 1997, p. 196). Such ground clearing will be accomplished by examining each theory with respect to the four precepts of pragmatic methodology: keep it simple; get it right; make it plausible; and derive refutable implications to which the data are applied (Georgescu-Roegen 1971; Solow 2001). Sooner or later, all would-be theories need to stand up and be counted.

Chapter 3, by John Nye, examines "Institutions and the Institutional Environment," broadly in the spirit of Douglass North. The passage that I have chosen is this: "While . . . NIE work has begun to revitalize economics . . . the implications for policy are less encouraging. If the relationship between formal and informal institutions is critical to economic performance, if that relationship is poorly understood, and, worst of all, if our basic ability to alter slow-moving institutions is limited, we may not be capable of providing the policy advice that statesmen and bureaucrats regularly seek" (pp 79–80). I agree that NIE has had revitalizing effects and I also agree that giving policy advice is made difficult by our failure to better understand the relationships to which Nye refers. Nye presents us with a research challenge to do better. My advice is that we hold our course and "grow the knowledge" as we have in the past, in a modest, slow, molecular, definitive way. It is not in the least discreditable, moreover, if, often, we are unable to give precise policy advice to statesmen and bureaucrats. Everyone, including statesmen and bureaucrats, needs to come to terms with the limits of our collective knowledge as economists. Too many policy disasters are attributable to "one-handed economists" whose confident pronouncements are in error.

Chapter 4, by Benito Arruñada, deals with "Human Nature and Institutional Analysis," which plainly has a bearing on the provision of policy advice. Rather than deal with this issue, however, I consider instead what he refers to as "two prominent examples of emotional maladaptation with

vast economic consequences: risk aversion and weakness of will" (p. 87). Most treatments of these and related matters operate at the level of the individual, and most of Arruñada's discussion is of this kind. I submit, however, that *organization* is important in both respects.

Not only does organization relieve individual risk aversion – by pooling risks and by hiring specialists to help us deal with risks that "we are programmed to (wrongly) perceive as affecting our survival and reproduction" (p. 87) – but, even more consequentially, organization often permits us to relieve weaknesses of will. Not only can we substitute "rational" economic routines for defective "mental modules" but we can also craft private ordering governance supports that better assure order, the effects of which are to mitigate the conflict that is posed by "maladaptive discounting." To be sure, organizations also pose dysfunctional consequences of their own, so allowance is properly made for these as well. My point is that the current focus of behavioral economics – which is a healthy development of which NIE is a beneficiary – on individual behavior and individual decision making may usefully be extended to make more prominent provision for organization to include both laboratory and field studies. (Some of these issues are raised in the last few pages of Arruñada's essay.)

Chapter 5, by Lee Alston, discusses "The 'Case' for Case Studies in New Institutional Economics." I am persuaded that case studies are both important to and are underutilized by NIE. I organize my remarks around Alston's statement that the benefits of "case studies include: the ability to first understand an issue prior to modeling it; the ability to test theoretical hypotheses; and the ability to shed credible light on the workings of the institutional and economic workings of society" (p. 121).

I concur, but emphasize the need for a *focussed lens* when doing case studies. That is because the phenomena are usually too complex to speak for themselves. Indeed, it has been my experience that a wrong-headed focussed lens is better than no lens at all, since we will then be confronted with contradictions in the data. Such contradictions invite us to rethink the issues by trying to ascertain what factors are responsible for the disparities and, if we are lucky, provide hints as to what *really is going on out there*.

My second remark on Alston is that I regard a case study less as a test of a theory than as a reality check.[2] And, I agree with Alston's third point: that case studies can and do shed light on the inner workings of complex institutions and economic organization. These microanalytics are where much of the action resides.

Chapter 6, by Michael Sykuta, examines the "New Institutional Econometrics: The Case of Research on Contracting and Organization."

Sykuta takes exception with the discriminating alignment hypothesis out of which transaction cost economics works, by observing that "Transactions at one level of the value chain are likely interdependent on the structure and governance at other levels of the supply chain . . . without an eye on the larger system researchers are likely to overlook ways in which organizational structure is influenced by its value chain context" (p. 140). I do not disagree, but would call attention to the first precept of pragmatic methodology: "keep it simple" (Solow 2001, p. 111).

Given the complexity of economic organization, we need to strip things down and identify the main case – which is not to say the only case. However, until such a main case is in place, an emphasis on second-order effects is apt to delay rather than promote the theory development exercise.

The advantages of a main case are these: it is transparent (simple); it is tractable; it invites others to advance rival main-case candidates; and it does not preclude subsequent refinement by making provision for second-order effects – which is good news! NIE is a work in progress for which new challenges are posed and new talents are needed.

Chapter 7, by Stéphane Robin and Carine Staropoli, discusses "Experimental Methodology to Inform New Institutional Economics Issues." Their concluding comment, which I agree with, is this: "We claim that EE [Experimental Economics] and NIE have already research points in common and . . . could mutually gain from common research projects" (p. 157). Earlier, they discuss how EE contributes to a "deeper characterization of the main behavioral hypothesis of NIE (bounded rationality and opportunism)" (p. 156). I offer a reciprocal example of how provision for organization can help to deepen our understanding of "bad games," of which the prisoners' dilemma is the canonical case.

The myopic version of the prisoners' dilemma is that two suspected criminals are apprehended and questioned about a crime. In the hope of extracting a confession, each is presented with payoffs that invite them to confess. Although both would be better off denying guilt, the calculus leads to what (for them) is a bad outcome: defecting is a dominant strategy.

Ways of overcoming this outcome have mainly emphasized spontaneous mechanisms. Camerer and Knez (1996, p. 94) summarize as follows: "[U]nder three conditions, games which are often classified as social dilemmas are [transformed into] games of cooperation. The first condition is that players get utility from [being nice and] cooperating with others who cooperate . . . The second condition is that [if] . . . players can be excluded from benefiting when others cooperate . . . then

players [can be induced to] cooperate. The third condition is . . . [to repeat the game] with sufficiently high probability."

The first condition corresponds to conditional reciprocity, with a predilection to begin with a nice move. The second two conditions entail foresight but leave the basic game intact. None of the three conditions, however, contemplate what I would say is the obvious move: take deliberate action *to alter the payoff matrix by engaging in private ordering* (which also entails foresight).

The implicit assumptions in the classic game are that the police are clever and that thieves are myopic and suffer from "frailty of motive." Suppose, however, that some thieves (or their managers, perhaps the mafia?) have the capacity to look ahead while the robbery is in the planning stage. Suppose that they not only recognize that they might be suspected of committing the robbery, but they also perceive the possibility of being presented by the police with the payoff matrix of the prisoners' dilemma. In anticipation of this dilemma, and so as to better assure that neither defects, they take advance actions that penalize the defection option and make cooperation the dominant strategy. The far-sighted or augmented game thus "defeats," as it were, the myopic game that they would otherwise be confronted with.

Predisposed to work out of spontaneous mechanisms, many economists eschew purposeful efforts to craft credible commitments. If, however, individuals have the capacities to recognize and reconfigure bad games, neglect of intentionality will miss some of the action. A researchable question, to which laboratory experiments could be applied, is "*What are the limits of intentionality*, if players are afforded this reconfiguration option, *in the repeated play of bad games*?" Such work has been taking shape (McCabe, Smith and LePore 2000).

Chapter 8, by Thierry Pénard, examines "Game Theory and Institutions" and advances the argument that ". . . game theory is highly useful in examining the rationale of institutions. Game theory is a rigorous framework for questioning the nature of interpersonal relationships." (p. 179). I agree, but would repeat that the pervasive importance of private ordering is often overlooked in game-theoretic treatments of economic organization. For example, Thierry Pénard tells us that cooperation, or agreement by parties to a contract, is "enforced by reputation and trust rather than by courts" (p. 172). True enough, but if the efficacy of reputation and trust vary with the circumstances then we need to ascertain when they work well and when poorly. Also, if access to the courts for purposes of ultimate appeal serves to delimit threat positions, that should not go unnoticed. More generally, the aforementioned neglect of intentionality, as in designing credible

commitment mechanisms which have the purpose and effect of deterring inefficient breach of contract, bears repeating.

David Kreps's (1999, p. 122) views on transaction-cost economics (TCE) are pertinent: "speaking as a tool-fashioner interested in developing tools that better deal with the *world-as-it-is*, I believe that game theory (the tool) has more to learn from transaction-cost economics than it will have to give back, at least initially" (emphasis added).

Chapter 9, by Jackson Nickerson and Lyda Bigelow, on "New Institutional Economics, Organization, and Strategy" both surveys the empirical literature on TCE and describes awaiting research opportunities, with special emphasis on the latter. The workhorse that they rely on throughout is that of discriminating alignment; transactions (which vary in their attributes) are aligned with governance structures (which vary in their costs and competence) so as to elicit a transaction cost-economizing result. This is truly the big locomotive on which TCE relies.[3]

Note in this connection that TCE, always and everywhere, is an exercise in comparative economic organization. In as much as there is always more than one way to organize economic activity, this requires the student of TCE to come to terms with the defining attributes of each generic mode of governance.

As Jackson Nickerson and Lyda Bigelow show, this strategy for studying economic organization has had broad application, of which more is in prospect as numerous extensions and refinements upon the simple model are worked out.

Chapter 10, by Joanne Oxley and Brian Silverman, examines "Inter-Firm Alliances: A New Institutional Economics Approach." The authors extend the basic TCE logic and include parts of resource-based reasoning to examine how and why alliances vary depending on the attributes of the transaction and the history of the contractual relation between the parties. What appears at the outset to be a wide variety of contractual provisions reduces to three distinct classes: "alliances tend to cluster in discrete forms, within which there is significant variation but between which we can nonetheless identify step function differences in governance attributes" (p. 219). Specifically, technology-related alliances classify as unilateral-, bilateral-, and equity-based, where safeguards progressively build up among them and equity-based alliances have the most hierarchical features. The authors furthermore project that "insights from the existing body of research on vertical relationships may usefully be integrated into future research on alliances" (p. 220). Altogether, their extensions of TCE reasoning into the study of alliances

reveals that many of the puzzles of alliances may be interpreted as variations upon a few key themes.

Chapter 11, by Emmanuel Raynaud, examines "Governance Structure and Contractual Design in Retail Chains." He mainly examines franchising as a mode of governance where, often, the franchisor both makes *and* buys the good or service in question, with benefits to both buy and make options. This is an instructive perspective and has ramifications for both TCE and marketing. Late in the chapter Raynaud examines the ramifications for antitrust, where monopoly (price theoretic) and efficiency (transaction cost) interpretations are contrasted. He observes in this connection that the "antitrust attitude toward the motivations behind vertical restrictions in distribution contracts has evolved considerably over years" and that the inhospitality tradition of ascribing monopoly purpose has been augmented to include a broader understanding of "the benefits of vertical restraints in promoting [efficiency]" (p. 247). I concur and take this to be one of the policy accomplishments of TCE.

Chapter 12, by Manuel González-Díaz and Luis Vázquez, examines "Make-or-Buy Decisions: A New Institutional Economics Approach." The authors mainly work out of a transaction cost setup in which asset specificity gives rise to hazards of bilateral dependency, and in which hazards are relieved by crafting credible commitments (hybrid contracts) or by unified ownership (vertical integration). They observe, however, that "solving the hold-up problem does not guarantee that other problems can be solved so easily, as, for example, monitoring" (p. 257), and subsequently discuss measurement costs as these bear on vertical integration.

I agree that measurement costs (which have their origins in information asymmetry conditions that are costly to rectify) have many ramifications for economic organization. I contend, however, that transactions between the firm and its customers, workers, and investors have more severe measurement problems than are experienced by firms engaged in intermediate product market exchanges.

The reasons are two, both relating to the proposition that organization matters. The first organizational difference is that it is much more economical for firms to acquire the requisite technical, legal, and managerial expertise to evaluate quality before taking delivery than it is for individual customers, workers, and investors. Second, (and related), intermediate product market transactions are presumed to take place between successive stages of production; each of which possesses the requisite scale and has perfected its internal governance mechanisms, thereby to qualify as a viable economic entity *unless* contractual complications arise at the trading interface.

To be sure, the foregoing applies more to trade between successive stages of production (both laterally and vertically) than it does to trade between a supplier and its distributor, which is often multiple, and hence takes the form of a *network*. As Klein (1980) describes, quality assurance (i.e., measurement) problems can arise between a franchisor and its network of franchisees for which credible contracting supports (in the limit, vertical integration) are needed. When it comes, however, to trade between successive stages of *production*, the trading relation is normally of a bilateral rather than network kind – in which event the main contractual concern is that of bilateral dependency (asset specificity).

Note, moreover, that the quality assurance problems that sometimes arise between the firm and its workers over due care and maintenance of equipment are normally of an *intra*stage rather than an *inter*stage issue. Thus, if it is very costly to monitor tool misuse and maintenance, then it may be better to concentrate the ownership of tools on the workers rather than the employer. Since that problem is posed whether successive viable stages are integrated or not, it should not be regarded as a separate "explanation" for vertical integration.

Chapter 13, by Gary Libecap, deals with "Transaction Costs, Property Rights, and the Tools of the New Institutional Economics: Water Rights and Water Markets." As many of us are vaguely aware, and as Libecap makes it abundantly clear, markets for water pose unusually severe problems, many with path-dependent origins. Once rights have been established, reallocations to elicit a shift from past and current uses to what have become higher-valued uses are deterred by many obstacles. One is that property rights for water in the American West are "incomplete [and] vaguely defined" (p. 271). Also, and less widely appreciated, current users with "established ties to politicians . . . are well placed politically to block reallocation" (p. 270). The early property rights literature (Demsetz 1967) easily relates to the first of these. The second poses political obstacles: with "many constituencies having a stake in existing allocations and a potential veto in any reallocation, a paralysis in present uses emerges" (p. 270).

I have elsewhere discussed the limitations of neoclassical resource allocation reasoning with reference to the "remediableness criterion" (Williamson 1996) – where the latter insists that reform proposals need to come to terms with both feasibility and implementation obstacles. Although no easy solutions emerge, the remediableness criterion has the merit of avoiding the hand wringing that attends "failures" to achieve hypothetical ideals of a zero-transaction cost kind. Although Gary Libecap has never been a hand wringer, I suggest that the short section

of his chapter, "Policy Responses," could usefully be elaborated by the systematic application of remediableness reasoning.

Chapter 14, by Michael Cook, Peter Klein, and Constantine Iliopoulos, discusses "Contracting and Organization in Food and Agriculture." These authors conclude that despite its "unique institutional environment, the food sector faces the basic problems of economic organization: the need to reduce transactions costs, the need to protect relationship-specific investments, the need to design structures that adapt to change. For this reason, many NIE theories translate easily and naturally to an agricultural setting." I agree, and especially want to emphasize the impor-tance of the uniqueness of agriculture to which the authors refer.

What distinguishes the NIE is the combination of a focussed lens that operates at a microanalytic level with deep knowledge of the phenomena. Thus, the lens of contract calls attention to and interprets contractual regularities which hitherto had been slighted. In as much as the specifics vary, however, deep knowledge of the particulars leads to qualifications and refinements. The combination of deep knowledge of agriculture with the concepts and apparatus of the NIE is what Michael Cook, Peter Klein and Constantine Iliopoulos bring to bear.

Chapter 15, by Pablo Spiller and Sammy Liao, examines "Buy, Lobby, or Sue: Interest Groups' Participation in Policy-making: A Selective Survey." Although most of the chapters in this volume work from a TCE setup, Spiller and Liao work out of the "positive political theory" (PPT) branch of the NIE, which focusses on the rules of the game. As they point out, however, "The distinguishing feature of NIE . . . is its emphasis in opening up the black box of decision making [whether] understanding the rules . . . of the game [PPT, or the] play of the game [TCE]" (p. 303). Specifically, their objective is to "explicate the *micro-analytic* features of the way interest groups [*actually*] interact with policy makers" (p. 303).

This is very much in the spirit of Kenneth Arrow's (1987) remark that the "New Institutional Economics movement does not consist primarily of giving new answers to the traditional questions of economics – resource allocation and the degree of utilization. Rather, it consists of answering new questions, why economic institutions emerged as they did and not otherwise; it merges into economic history, but brings sharper nano-economic . . . reasoning to bear" (Arrow 1987, p. 734). Pablo Spiller and Sammy Liao not only take institutions seriously (by answering new questions) but examine the microanalytic mechanisms of buying, lobbying, and suing with reference to the institutional environment (polities, judiciaries) within which they are embedded and furthermore review empirical evidence that relates thereto. As compared

to TCE (which combines economics with law and organization theory), PPT principally combines economics and political theory. NIE is greatly enriched in the process.

Chapter 16, by Jean-Michel Glachant and Yannick Perez, addresses "Regulation and Deregulation in Network Industry." The quotation that I have selected for this is: "The efficiency of [each . . . type] of arrangement for network industries should thus not be seen [in absolute terms], but rather . . . depends on [the comparative analysis of each feasible alternative]," to which they add that "there is no single 'best solution' applicable to all [network] . . . industries" (p. 325). The move from examining actual modes in relation to hypothetical ideals by insistence upon examining feasible alternatives, all of which are flawed, is the first of the seven key features that I ascribed to NIE in my introductory remarks. Glachant and Perez's discussion of why regulation and deregulation need to be examined comparatively, and of the compli-cations posed by differences in the institutional environment across nation states, (with special reference to Germany and Great Britain), illustrates why there are no all-purpose solutions to network industries. Rather, the logic of comparative economic organization needs to be worked out with respect to the attributes of different network industry transactions in relation to the applicable nation-state rules of the game. This may be tedious, but global prescriptions are naïve and invite public policy error.

Chapter 17, by Stefan Voigt, examines "Constitutional Political Economy: Analyzing Formal Institutions at the Most Elementary Level." Stefan Voigt distinguishes between normative and positive branches of the economic analysis of constitutions, and associates the normative branch with James Buchanan, whereas Voigt's chapter deals mainly with positive constitutional economics.

Stefan Voigt makes note of many similarities between constitutional political economy (CPE) and NIE, and he avers that "CPE could greatly profit from positioning itself within the broader NIE" by making express allowance for the complications posed by pre-existing informal institutions when designing new constitutional rules and giving more prominence to problems of credible commitment (p. 366). More generally, the lens of contract approach (which Buchanan contrasts with the more conventional lens of choice) is instructive for studying both political and economic institutions, hence to both the CPE and NIE research agendas. These two have much in common and may be expected to "flourish together."

Chapter 18, by Sonja Opper, discusses "New Institutional Economics and Its Application on Transition and Developing Economies." This