Chapter Thirteen

Southeast Asia in Political Science: Terms of Enlistment

D O N A L D  K. E M M E R S O N

Rational choice theory, with its claims to axiomatic reasoning, deduction as a fountainhead of truth, and universal principles of politics, is challenging political science. . . . The idea of having a political science specialist for every piece of international real estate may soon seem as arcane as having a specialist for every planet in an astronomy department.

D A V I D  L A I T I N  (1993)

Pluralism without updating is not science. . . . It would be a warping of the scientific frame if we built into the charter of any department of political science that there had to be an expert in “realism,” or in “South Asia,” or in “democracy,” or in “qualitative methods.” . . . [I]nstitutionalizing slots for particular specialties is a threat to scientific progress. . . . A pluralism that shelters defunct practitioners cannot be scientifically justified.


[O]n the eve of the September 11 attacks, half of the top political science departments in the United States did not have a Middle East studies program. It is certainly desirable for a social science to be rigorous, empirical and seek general rules of human behavior. But as Aristotle explained, it should not try to achieve a rigor that goes beyond what is possible given the limitations inherent in the subject matter. In fact, most of what is truly useful for policy is context-specific, culture-bound
Southeast Asian studies and political science are compatible. It will always be possible to define these two fields in exclusionary ways. At the extreme, they can be made to refute one another, methodologically and epistemologically. Yet there is no reason why one cannot study an area and do political science at the same time—and satisfy criteria for quality research on both sides of this polemicized divide, depending on what those criteria are. The italics emphasize my theme.

On the preceding pages, my colleagues have reviewed what scholars at the interface between the area and the discipline have enabled us to know about politically relevant phenomena in Southeast Asia. In their chapters and their careers, these authors have illustrated the compatibility of area studies and political science. In this chapter I want to stand back from the literature and its findings and highlight the interface itself.

To say that Southeast Asian studies and political science are compatible, after all, leaves open the nature of that compatibility. Do the two spheres of endeavor merely coexist? Are they complementary? Do they overlap? And which (if any) of these formats—coexistence, complementarity, overlap—is superior to the others, and why? It is on these questions that I wish to focus here.¹

**Overlapping Spheres**

As a way of describing how area and disciplinary study are related, coexistence is misleading. My feisty opening quotations notwithstanding, this book has not been written by area specialists gazing across a DMZ that separates them from political science, nor by political scientists on the other side of the barbed wire...
gazing back at the area specialists. My colleagues have amply illustrated the value of scholarship that is simultaneously and robustly Southeast Asianist and political scientist in character. In this context, the notion of coexistence essentializes and dichotomizes what amount to contingent differences. It overlooks the fruitful interaction between perspectives associated with these differences. And it risks implying an institutional equality between area studies and the discipline that does not exist.

Compared with mere coexistence, the case for complementarity between area-based and disciplinary scholarship is, on the surface, more accurate and more attractive. Beyond simple juxtaposition, a complementary relationship entails mutuality and interaction. But on whose terms? If the study of an area is institutionally peripheral or subordinate to the discipline, and if the discipline privileges one intellectual perspective, area scholars may face a “complementarity” with the discipline that requires them to adopt that particular approach. This is what I mean in my subtitle by “terms of enlistment.” At its most invidious, such a relationship reduces the area scholar to supplying data—raw material to be sorted and processed into theory in factories downstream that are owned and operated by the discipline in accordance with its convictions.

When David Laitin, as quoted above, insists on his understanding of “science” to the exclusion of “defunct practitioners,” Southeast Asianists prospectively included, he seeks to impose his own terms of enlistment. His is a methodological warning to get with the program—shape up or ship out. Fukuyama, although he may not have had Laitin specifically in mind, regards Laitin’s kind of “science” as intellectually trivial and policy-useless. Fukuyama’s terms of enlistment contradict Laitin’s.

Compared with coexistence and complementarity, the notion of overlap has several advantages. Unlike coexistence, it does not reify the two sides. Unlike complementarity, it does not allocate one function or set of functions to area studies and a different set to political science. The notion of overlap implies what Peter Berger (1974: 123) called “cognitive respect.” It acknowledges that scholars with political science backgrounds and Southeast Asian interests have, or ought to have, freedom of intellectual choice. It recognizes, too, that pre-defunct Southeast Asian political scientists have worked out, in their writings and careers, their own ways of meshing or balancing the area with the discipline.

Some of the scholars working inside this overlap—the zone of greatest area-discipline intimacy and transaction—may willingly conform to an assigned role in an intellectual division of labor. Others, however—emboldened,
one hopes, by some of the arguments in this book—will see no necessarily fatal tension between studying an area and doing political science. They will claim for area studies a full analytic and even a theory-generating role that is at the same time consonant with what they believe political science is, or should be, about.

It is hard for a Southeast Asianist to ignore one of the two spheres. But some may enjoy spending time outside this overlap, either in the rest of the area-study sphere or in the rest of the disciplinary one—extensions where diverse conceptions of scholarship also prevail. In the nonoverlapping space on the area side, for example, some may criticize or even reject the discipline while others settle for coexistence with it, just as scholars in the extension of the disciplinary sphere will hold comparably various views of area studies.

A career is not a snapshot but a film. Intellectual preferences and priorities evolve. No scholar is likely to stay in one fixed place from the beginning of a career to its end. Some will look for cases and comparisons beyond Southeast Asia, whereas others will dig deeper into one country in that region. Some will seek greater acquaintance with other disciplines, whereas others are content to explore the perspectives and findings that political science affords. Accordingly, the zone of overlap, like the spheres themselves, will expand or contract. Necessarily, the “terms of enlistment”—the meanings and conditions that locate “the study of Southeast Asia” within “political science” (and vice versa)—will change as well.

An awareness of scholarly flux is not easily reduced to a logo suitable for emblazoning on a banner to be carried by the vanguard of a revolutionary or revisionist movement inside either Southeast Asian studies or political science. But partly for that very reason, a sense of shifting and overlapping spheres not only captures actual complexity and contingency—the dynamics of diversity—better than more schematic alternatives do; it also opens a range of possible scholarly futures for area and disciplinary study that are more appealing, at least to me, than mutual alienation at one extreme or disciplinary hegemony at the other. (Area hegemony is almost impossible to imagine, or so I shall argue.)

The zone of area-discipline overlap is the natural habitat of comparative politics, the subdiscipline of political science that is most hospitable to area studies. No subfield in the discipline is better positioned to mediate or transcend the dichotomy between area-based knowledge and political science. But it is not realistic or desirable to seek in comparative politics a neat—let alone a permanent—resolution of area-discipline tensions. Dissent and disarray in
this context are encouraging signs that the terms of enlistment are still being tested, and contested.

Although arguing for dynamic overlap as the best way of illustrating the compatibility of area studies and political science, this chapter does acknowledge aspects of coexistence and complementarity in matters of professional organization and proposed practice. I begin by highlighting the institutionally hierarchical relationship of area scholarship to political science—their unequal and uneasy coexistence on some campuses. The discussion then differentiates the two spheres while noting the overlap between them and the diversity within them. Next I review in unequal detail two proposed complementarities between the area and the discipline: the “tripartite methodology” of David Laitin and the “analytic narratives” of Robert Bates.

The chapter ends by arguing against an overly monolithic or centripetal view of theory and for a more open methodology of “plural choice.” My intention is not to promote an alternative to “rational choice” but to urge instead that whatever the method a scholar decides to use, the decision should be driven by the nature of the research problem at hand, rather than by professional investment or evangelical faith in a single key that supposedly fits all locks. My wish throughout is to help make the zone of overlap between Southeast Asian studies and political science more productive, intellectually and professionally, for those located there.²

Unequal Organization

Life is ineluctably spatial. People live their lives on location, including even the seat that temporarily locates an airline passenger between locations. Being located somewhere means interacting with the human and physical environment around you. If our shared intellectual goal is to understand these interactions—or to explain, predict, or interpret them—then it follows that we must first be able to situate them.

Indeed, there is no such thing as context-free understanding. The effort to comprehend human thinking, feeling, or behavior in a computer model or a game matrix merely substitutes an artificial environment controlled by the researcher for the more complex and diverse “natural” environments in which these phenomena occur, as the lives of billions of unique individuals elapse and intersect.
At the root of the “area idea” is the proposition that trying to get inside a relevant selection of those life worlds at relevant places and times is a necessary (if insufficient) step toward comprehending what has gone on there, or is going on there, not to mention predicting what will be going on there. Reversing Gertrude Stein’s derisory view of Oakland—“there is no there there”—for area specialists there always is a there there, and a then as well. History is a core discipline in area studies.

In the organization of U.S. universities, nevertheless, Southeast Asia programs, where they even exist, are interstitial between departments that represent the disciplines and, in that capacity, form the main pillars of higher education.

Marginality has some advantages. Not being mono-disciplinary, area-studies programs can help widen intellectual horizons that may have been narrowed in departments. Not being tenure homes, such programs are less subject than departments to rancor and jealousy over hiring, firing, and promotion. In the cooperative activities of area centers—brown-bag talks, conferences, potluck dinners—political scientists may find intellectual and personal relief from the hierarchical ambience, career competition, topical constraint, and authorized methodology that may characterize their disciplinary department. There are, of course, exceptions to these judgments. But successful area programs do often serve as respites of a kind from the quotidian business of impressing one’s departmental colleagues by improving one’s curriculum vitae. Especially keen are the feelings of isolation among area scholars on campuses—by far the majority—that are not large or wealthy enough to sustain a Center for Southeast Asian Studies.

Note the prepositions: a Center for Southeast Asian Studies, but a Department of Political Science. To my knowledge no U.S. university—certainly no major one—houses a Department for Political Science or a Center of Southeast Asian Studies. Compared with the area, the discipline needs no efforts on its behalf. A department “of” the discipline links to something already large and lasting. The notion that every university should maintain a unit devoted to studying Southeast Asia is, in contrast, fanciful. There are far fewer Southeast Asia centers than there are political science departments. When they chose to be “for” rather than “of” Southeast Asian studies, these centers may not have been self-consciously advocating their field of study. Like Marx’s classes “of” and “for” themselves, this is a subtle distinction. Yet the difference in propositional choice between area and discipline remains striking—and is at least
consonant with the observation that in U.S. higher education, compared with political science, Southeast Asian studies are institutionally more precarious.

In a conversation with me years ago a U.S. political scientist, one of the best-known names in comparative politics at the time, only half-humorously dismissed proponents of area scholarship as mere “real estate” agents whose “soak and poke” methods could not withstand scrutiny. Area work, he half-implied, was too parochial and sloppy to qualify as proper political science.4

I have treated—this book treats—the overlap between area concerns and political science as a zone of healthy and productive collaboration, now or in prospect. As the above putdown suggests, however, that cross-hatching has also bred professional disregard, intensified sometimes by the two spheres’ inequality in the organization of academe. Anyone who has moved between these fields in the course of a career has experienced, alongside mutual good will, reciprocal disdain—the area specialist dismissed as unsystematic, the disciplinist disdained as insensitive. Working in such mixtures of affinity and coolness, one may wonder where one’s loyalties should lie.

Dilemmas of allegiance are especially acute among younger scholars seeking tenure. Should they publish in Asian studies journals that share their attraction to the “area idea” or in political science journals that may impress their departmental seniors more? How assiduously should beginning faculty cultivate theory, chase causality, run regressions, build models, and seek patterns in aggregate data across multiple countries? Dare they relinquish quantity for quality and scope for depth in prolonged fieldwork in a small but richly complex place? What might it mean to follow the advice of the influential anthropologist Clifford Geertz and generalize within a case? Or, still referencing Geertz (1973b: 26), how “thick” does description have to be to gain the respect of theoreticians? Once tenure is obtained, if it is, such choices become less acute, and personal preferences enjoy freer rein.

Methodologically, compared with area studies, political science is more presumptive. The New Shorter Oxford English Dictionary, for example, defines political science twice: (1) “the branch of knowledge that deals with the State and systems of government”; and (2) “the scientific analysis of political activity and behavior.”

Definition (1) parallels area studies in featuring a topic, which happens to be “the State and systems of government” rather than “Southeast Asia.” How the discipline deals or should deal with its preferred subject is left open. But the door of methodology is at least partly closed by definition (2), which has
no counterpart in the study of an area. That second account commits the discipline to dealing with “political activity and behavior” not impressionistically or descriptively but through “scientific analysis.” Political science implies an official tool kit; mere Southeast Asian studies does not. Reinforcing the epistemological gap between disciplinary and area knowledge is the difference between centripetally singular “science” and centrifugally plural “studies”—between a do-it-this-way standard and an invitation potentially to do it your own way.5

Crossing Borders

The multidisciplinary scope of Southeast Asian studies necessarily diversifies that set of endeavors compared with the one discipline of political science. Yet the discipline, however firmly ensconced in the academy, is not a solidly consensual monument to science. A version of the intellectual tension between area and discipline appears inside the discipline itself, as the gap between differently set barriers to entry: the low-bar eclecticism of the verb “deals with” in definition (1) and the high-bar certification of “scientific analysis” in definition (2).6

One might expect the transdisciplinary sweep of Southeast Asian studies to encompass topics and viewpoints so multiple and varied as to make political science appear cramped and monochrome by comparison. That might be true if practitioners of “political science” in fact dealt only with narrowly “political” topics using only narrowly construed “science” as an approach. Instead, of course, the writings and teaching of scholars housed in the discipline, including those working on Southeast Asia, span political economy, political history, political anthropology, political sociology, and so on. Nor is it difficult to see politics of some sort at work in almost any situation, entity, activity, or event.

As for the “science” in political science, there is only the loosest of agreements as to what that key qualifier means. Uncertainty and controversy over the appropriateness of this or that approach, and the difficulty faced by any one school that would impose its epistemology on other schools, help to keep a scholar’s choice of method at least somewhat open. Nevertheless, that openness remains greater in Southeast Asian studies than in political science. The discipline is one of the social sciences, and they are diverse, but area studies embrace the humanities more fully and enthusiastically than political science does.
Perspectival controversies inside the discipline matter. A candidacy for tenure may be at stake. In contrast, the institutional subordination of area knowledge and its fragmentation across diverse world regions work to lower the stakes of methodological conflict.

The self-styled “perestroika” backlash in the discipline illustrates this difference. The revolt was meant in part to challenge the perceived dominance of rational-choice assumptions in political science. No such movement developed in Southeast Asian studies, and not just because skepticism toward rational-choice theorizing was already widespread among area scholars at the time. Compared with its political science equivalent, the Southeast Asianist vocation was then, and remains, too weakly organized and professionally too minor for its diverse and dispersed adherents to bother plotting reform. No hegemony, no rebellion.

For if political science combines a hierarchy with a specialty, Southeast Asian studies are a specialty with little to no hierarchy. In the United States, the American Political Science Association (APSA) is for a political scientist what the Association for Asian Studies (AAS) is for an Asianist—a professional “guild” (Emmerson 2003). Elevation to the leadership of APSA means being acknowledged as a leader of the discipline. For younger scholars, publication in APSA’s peer-reviewed journal, The American Political Science Review (APSR), significantly enhances the chance of tenure.7 Appearances on panels at the Association’s annual conventions are lesser stepping-stones toward the same end. The AAS also has its leaders, its Journal of Asian Studies, and its yearly meetings. But the Southeast Asia Council and its members occupy a small box well inside the organizational chart of the AAS. Meeting infrequently and lacking funds and visibility, SEAC is peripheral compared with APSA.

Area studies are also weaker than the discipline in a more ironic respect. Most students of Southeast Asia are specialists on only one of its ten countries. Other things being equal, the audience at an AAS panel on one of Southeast Asia’s ten states is not likely to include many specialists on the other nine. When the subject of such a panel is politics, the relative homogeneity of the audience may be not only spatial but disciplinary as well. It was partly to remedy such narrowness that the AAS some years ago began urging its members to organize “border-crossing” panels that spanned countries and disciplines alike. The irony is that this was considered an innovation in area scholarship, whose interdisciplinarity already implied broad horizons and ho-
listic perspectives. Self-segmentation by country within Southeast Asia, let alone by region within Asia as a whole, is in its own way no less parochial than mono-disciplinarity.

Generalizing Within Area Studies

In every scholarly genre at any point in time, some ideas are emergent or ascendant while others are in relative stasis or decline. The study of an area is no exception. If today, for their field, Southeast Asianists jotted down these ideas—what’s “in,” what’s “out”—their lists would, of course, vary. But among the more appealing and popular concepts, hybridity, constructedness, and contingency would surely recur. Conversely, among the anathemas on these lists—sins for scholars to avoid—I would expect to find frequent warnings against essentialism, reification, and givenness. Better to reach for a constructivist insight than fall back into the naturalist fallacy.

Southeast Asian studies are intellectually diverse. But they are not random. For various reasons, including globalization and the legacy of postmodernism, those who study Southeast Asia nowadays tend to feature possibility, openness, changeability—and shun imputations of permanency, closure, innateness. If explorations of indeterminacy are “in,” intimations of determinism are definitely “out.” Among the most relativistic Southeast Asianists, paradoxically, primordialism verges on apostasy.

Primordialism is the idea that cultural outlooks and behaviors are foundational—long-standing, slow-changing, and deeply rooted in the hearts and minds of the members of a given culture. Resistance to such an essentialist formulation, including the very notion of “a given culture,” is more intense and widespread in Southeast Asian studies, where anthropology looms large, than it is in political science. Primordialism is not behavioralism, and studies of “political culture” in the discipline are passé, at least under that name. Yet the notion of a fixed or viscous culture could, in principle, satisfy a political scientist’s desire to reach bedrock of some sort, and in that sense may not differ as much as one might think from the quest for universally and enduringly valid generalizations about outlooks and behaviors. Political scientist Samuel Huntington’s controversial but influential study of civilizations (1996) illustrates the point (see Huntington 2004). Certainty may be chimerical, but the appetite and therefore the market for it remain.
Southeast Asia is but a small part of the larger world. Even if they could make law-like statements to account for political phenomena throughout “their” region, how would Southeast Asianists know whether or not those generalizations applied as well to the many peoples, countries, and regions elsewhere? They would not know, at least not without extending the scope of their research to include the rest of the world—that is, not without ceasing to be Southeast Asianists in the sense of specialists who confine themselves to that region. The more research undertaken entirely outside Southeast Asia, the weaker the researcher’s claim to a single-area identity.

An area specialist could avoid losing that identity by staking permanent ground upstream. He or she could supply materials, all drawn from Southeast Asia, to political scientists working downstream: rough insights from “local knowledge” worth reconsidering in other settings, regionally plausible propositions worth wider testing, or mere data and descriptions to be plugged into a model or matched against a generalization that the receiving political scientist might be able to confirm or improve.

To the extent that the processing of area knowledge by disciplinists adding value downstream is aimed at the certainty and closure one associates with law-like generalizations, however, the implications may be repellent for area students who are apt to assume—even to prefer—contingency and openness in human affairs. An area specialist may be happy to generate creative ideas and insights for further consideration by others. But if raw information is all the disciplinist wants from the area-ist, the latter may not be happy at all. Why should responsibility for evidence stop there? Why should it preclude a role in making theories based on that evidence—and assessing them as well? Not all area specialists will want to be hired as hewers of wood and drawers of water on a disciplinarian plantation.

It is from such a biased complementarity that area-based resistance can arise. Instead of helping to inform the discipline, a relativistic Southeast Asianist may choose to question its susceptibility to universalist assumptions, as if any one nontrivial rule could fit all or even most cases. Rejecting a division of scholarly labor seen as invidious—area studies as a mere data bank for the discipline—such a scholar could claim and even celebrate the uniqueness of “local knowledge” (Geertz 1983; see also Emmerson 2004). He or she might use fluency in a local language to assert that local phenomena are fundamentally untranslatable, incommensurate, and thus fortunately beyond one-size-must-fit-all-if-only-we-could-find-it political science.
In this instance as well, the area specialist’s professional identity could be at stake. The more that human beings everywhere think and act similarly in similar situations for similar reasons, the more arbitrarily bounded any one part of the world must be. An area specialist may warrant specializing in one region, country, or part of a country by emphasizing its uniqueness. A universal ontology jeopardizes that justification.

The most common claim for Southeast Asia’s uniqueness showcases its diversity. The claim is ironic insofar as diversity implies ranges of variation, which imply variables, which invite cross-case comparisons—comparative political science. The claim is irenic in that it opens opportunities for cooperation—peace—between area and disciplinary specialists. The claim is additionally ironic, however, in a way that hampers such cooperation, epistemological disagreements aside. Although a typical university-based Southeast Asianist teaches much or most of the region, most of her or his research is likely to be limited to one or at most a few countries within it. Such a scholar is not well equipped to accomplish a fraction of the comparisons that the vaunted diversity of Southeast Asia invites, let alone to draw contrasts with other parts of the world.

Many gradations are of course possible between universalism in the service of disciplinary theory and exceptionalism in defense of local knowledge. Like my colleagues who move back and forth between area and discipline, including my co-authors here, I do not wish to essentialize the two genres. They are not necessarily opposed. But they are different—and seen to be so, as the ensuing discussion of “the state of the subdiscipline” of comparative politics will suggest.

**Generalizing Within Political Science**


The tensions between the discipline and the area discussed here were not
Laitin’s topic. In his paper he implied that such disagreements might be the fading legacy of older scholars. He was “encouraged by the orientation and training of the coming generation of comparativists, who are ready to join in on the emergent consensus that I outline here” (2000: 2, including n. 1).

The three components of that would-be-emergent consensus amount to a menu of methodological choices. A comparativist can (1) seek “statistical regularities across a large number of similar units,” notably as reflected in “cross sectional or diachronic data”; (2) endogenize “principal variables” in “formal models”; or (3) “examine real [or virtual] cases to see if the results from the statistical analyses [method (1)] and the theoretical accounts [method (2)] apply to the world” (Laitin 2000: 1–4; see also 2002: 630–631).

Large-\(n\) statistics, mathematical models, reality-checking narratives . . . this division of intellectual labor compartmentalizes and sequences scholarly research. Qualitative work is reserved for “narrative.” But that activity is limited to seeing whether the statistical regularities and the formal models “apply to the world.”

Laitin’s triptych is ecumenical in one sense at least. It reserves one panel for narration. Yet this complementarity is likely, on balance, to discourage a Southeast Asianist political scientist hoping to generate theory using qualitative methods. There is no opportunity for that activity in Laitin’s summary of his scheme. The narrator is called in only after the statistician and the modeler have done their work. And it is their work that becomes the work of this third member of the research team.

Laitin’s complementarity is also remarkable in that it reverses the invidious upstream-downstream specialization that I referred to earlier. The “narrative” function has been redesigned and relocated downstream: from generating raw input—data—at the onset of research to checking the fit between highly processed material—statistics, models—and the actual world. Does this new role empower a narrator to call a halt to the whole process if she or he determines that these statistical patterns and modeled propositions, including draft laws of causation, do not in fact “apply to the world”?

I am not sure. But I strongly suspect that the success of such a whistleblower—the narrator’s ability to persuade the statistician and the modeler to reconsider their quantitative results—would depend on how impressed the latter colleagues were with the methods behind the critique—that is, with the nature and efficacy of the whistle being blown. It may be unfair to note that the narrator-critic would be outnumbered two-to-one in Laitin’s format. But
it seems fair to wonder whether either the statistician or the modeler would accept correction on the basis of purely or mainly qualitative evidence, the sort that goes into the making of a constructed—interpreted—story, which is what a narrative is. Having gone to all the trouble of processing quantitative evidence and deriving mathematical parameters, why would these rigorously “scientific” colleagues have a change of heart and suddenly believe that “mere anecdotes” are “data” after all?

A different response seems more likely: Unless and until you test our products “scientifically,” the way we would, using quantitative methods, we must reject your critique. A few exceptions do not overthrow a rule. The necessarily subjective character of your “narrative” makes its apparent lessons unreliable. Suggestive, perhaps. But definitive? No. Determining what is definitive can only be our own—quantitative, cross-case, model-running—task.

Laitin’s three functions could be performed by one person. But the sheer range of skills and experience required is sure to daunt an individual scholar—the artificial languages of statistics and modeling, the spoken languages of field sites and secondary literatures, and the years of experience, with the computer, in the classroom, on the ground, in the stacks, before credibility across such disparate domains could be acquired.

Ideally, innumeracy should have no more place in the zone of area-discipline overlap than illiteracy does, and rational-choice comparativists should spend long periods seeking “local knowledge” in actual, complex, puzzling field settings where people speak neither math nor English. Realistically, however, collaboration among differently equipped and specialized scholars will remain necessary and desirable.

What is missing in a complementarity that either sequesters the qualitatively minded scholar farthest upstream panning for raw data or farthest downstream narrating reality checks is the possibility that quantitative and qualitative research are best brought to bear throughout the research process, depending on what method or combination of methods is best suited to the problem at hand.10

Laitin illustrated his proposal by reviewing works written from each of his three perspectives on recurring topics in comparative politics: democracy, order, and capitalism. These illustrations acknowledged the creativity of narrators in advancing and debating various answers to large questions. But it is clear from his account that the qualitative mode in which these authors worked necessarily limited their ability to generate real—that is, formal—theory.
Laitin further limited his notion of narrative by identifying it closely with history: “Questions that require sensitivity to change over time lend themselves better to historical rather than statistical analyses” (2000: 12–13). In his paper at least, Laitin admitted a role for “ethnographic, interview, or archival work” in determining whether “changes in parameter values” and “outcomes” that a theory anticipates have actually occurred (2000: 5). However, it remains clear that qualitative political anthropology, for Laitin, is a taker (if also possibly a tester) of theory, not its maker.

Laitin also implicitly dismissed, or at least demoted, other disciplines, such as sociology, anthropology, linguistics, philosophy, and literature, as makers of theory about politics—except perhaps insofar as researchers in these fields could be either explicitly statistical or historical, or could build formal models. This surely must come as a disappointment to a political-science-schooled area specialist accustomed to learning, and learning theory, from diversely insightful work by scholars in some or all of these disciplines—work that is neither exactly historical nor quantitative but does illuminate, or generalize from, aspects of the world.

Viewed against the academic organization of knowledge, such narrowness makes a certain sense. Political science would lose its identity if it became transdisciplinary—the equivalent of area studies going global. One could argue that proponents of multidisciplinary research on the area side of the divide should be pleased enough at history’s incorporation into Laitin’s political science scheme not to ask for more diversity than that. Yet even with history inside it, such an amputated world seems designed to induce at least mild claustrophobia in a Southeast Asianist accustomed to multidisciplinary horizons.

The difficulty is that these other disciplines come with their own preferred concepts, approaches, and methods. One can sympathize with Laitin’s dilemma: (a) to organize a hierarchy that will enforce methodological tranquility within political science, and to keep the alternative universes of other disciplines outside the building, where they cannot disrupt what goes on inside it; or (b) to let them inside the building, where they can enrich political science but at the risk of importing an anarchy of assumptions and techniques.

Between these alternatives, Laitin proposed a less strict version of (a) that may still be too strict to attract many area scholars, namely: (c) a limited partnership with history and at least the possibility of openness to diachronic nar-
ratives from other parts of academe, provided that these liaisons are restricted to helping to improve, through case-specific evidence, political science theory as defined in quantified terms. In practice, the success of this middle option would depend, for example, on how many area specialists would willingly accept such a division of labor, knowing that they would have to check their own epistemologies at the door. By the light of this third option, area studies and comparative politics are compatible, but only on the latter’s terms as interpreted by Laitin.

Compatibility on Whose Terms?

Two years before the APSA panel on which he reviewed the state of comparative politics, a book appeared by five other authors working from rational-choice perspectives—three political scientists and two economists. They called it Analytic Narratives (Bates et al. 1998), henceforth abbreviated \( AN \).

In one sense, at the time, this was a breakthrough. Rational-choice theorists were admitting that “narratives” really could be analytic. In that recognition—and, by extension, the reverse admission, that analysis could be narrative—lay a possible synthesis at the end of a long dialectic in the literature. The arguments in that dialectic (Friedman 1996) could be traced back, through the antithesis to rational-choice theorizing articulated most notably by Green and Shapiro (1994), to the thesis propounded by the school itself in a literature commonly dated from the work of William Riker (1990, 1962). But the synthesis was not to be. Or if \( AN \) was a synthesis, it did not last long. Among other scholars, Jon Elster (2000), in a devastating review of the book in the \( APSR \), kept the dialectic going.

Area scholarship was relevant but not central to these debates. One looks in vain through \( AN \), or through Laitin (2002) or (2000), for a discussion of area studies as an enterprise in its own right, with its own findings and arguments, let alone its own theories or pre-theories. The omission is all the more striking for that fact that both Bates and Laitin began their careers as area specialists—in African studies.\(^{11}\)

Analytic Narratives paid major attention to history. It could have been subtitled “Modeling European and American History.” Of the five core chapters apparently written to illustrate what analytic narratives were, four took up,
respectively, the political economy of late-medieval Genoa; absolutism in seventeenth- and eighteenth-century France and England; conscription in nineteenth-century France, Prussia, and the United States; and political stability in the United States prior to the Civil War. The fifth, by Bates, concerned the workings of the International Coffee Organization from 1962 to 1989. Here is how Bates and his collaborators summarized what they were up to:

We call our approach analytic narrative because it combines analytic tools that are commonly employed in economics and political science [read: rational choice] with narrative form, which is more commonly employed in history. Our approach is narrative; it plays close attention to stories, accounts, and context. It is analytic in that it extracts explicit and formal lines of reasoning, which facilitate both exposition and explanation. (AN: 10)

What this paragraph seems to mean is that economics and political science, or more accurately the partisans of rational-choice theorizing within these disciplines, are analytic. In contrast to these fields, which have tools, history offers only a narrative form. Historians describe. They tell stories. They compose accounts of what happened. They provide context. They offer their narratives for consumption by those who add real value: namely, the authors, all of them working in genuinely analytic disciplines where supposedly genuine theory—formal theory—is made. That downstream activity “extracts explicit and formal lines of reasoning” from upstream historical narration.

At least Laitin, as I have noted, would later turn this division of labor upside down by placing “narrative” farthest downstream. The narrators in his scheme were not merely adherents of a form. To run reality checks on formal theory, they would have to wield tools as well. However, the nature and provenance of those tools remained unclear. Would they be the methods of large-n, quantifying “science”? If so, in what sense would these narrators be allowed to narrate?

I began this essay by acknowledging the compatibility of area studies and political science. But that compatibility is not, and should not become, the unequal congruity of inferior and superior within a hierarchy in which the area student knows his or her place and the disciplinarian knows what counts. A less skewed and more respectful arrangement would reflect a sharing of interests and a shared sense of one’s own limitations.

On both sides of the interface between area work and political science,
scholars have a mutual interest in theory broadly understood. The problem is that for many on the disciplinary side, area students are not truly theoretical, but when viewed from the area-study side, the discipline looks too narrowly theoretical.

In this regard, the time is past due for area scholars to retire a canard about the other side. Few political scientists believe they are building a single, permanent, universal “Scientific Theory of Politics” that will put a STOP to all lesser efforts, including the qualitative fumblings of area specialists. The latter should not fear a straw man. Even Laitin, in his Darwinian vision of area studies possibly becoming “defunct” (2003: 108), did not go so far as to claim that “science” would definitively prevail. Here he is on the same subject, but in a still less cumulative vein:

Comparativists will drop old questions, not because they are solved but because new questions have pushed their way onto the political agenda. . . . Choice of the dependent variable cannot be separated from the goals, interests, and generational perspectives of researchers. . . . Also, questions comparativists ask about outcomes continually get specified anew, as the way we ask our questions about political outcomes changes over time. . . . [Such] questions never get satisfactorily solved, as on the brink of discovery they get specified in a new way, opening up new lines of inquiry. (Laitin 2002: 632, italics added; see also 2000: 5–6)

As for Bates and his co-authors, in AN they quoted and rejected the accusation by Green and Shapiro (1994: x) that the failures of rational-choice theory were “‘rooted in the aspiration’” of its partisans “‘to come up with universal theories of politics.’” AN’s authors argued that notwithstanding their book’s debt to “deductive reasoning”—each chapter, they acknowledged, “constructs, employs, or appeals to a formal model”—the essays “seek no universal laws of human behavior.” They noted that the pioneer of rational-choice thinking in political science, William Riker (1962), had believed that rational choice held “the promise of a universal approach to the social sciences, capable of yielding general laws of political behavior.” But the authors of AN were unwilling to go along. They dismissed Riker’s claim as having been “based on an ‘overconfident’ and naïve vision of the sciences” (AN: 11, including n. 11).

Opponents of STOP who work wholly or partly on the area side of the interface with political science ought to relax a bit and treat Riker’s imperial vision for rational choice as comparable to the withering away of the state for
classical Marxists, or to the resurrection of a worldwide caliphate for committed Islamists—attractive to militants in this or that school but too fanciful to be taken seriously by others as a scenario for the future. More worthy of worry are the skewed terms on which rational-choice theorists would establish and institutionalize the compatibility of area studies with political science.

Theory and Choice

The most important question, in the end, is not whether area students feel welcome in a disciplinary club, or whether rational-choicers are imperialists. If diplomacy aims to prevent conflict, scholarship does not. Scholarship is about who is right. And scholarship is therefore necessarily also about what “being right” means. Discord is not the bane of scholarship but its lifeblood.

Is David Laitin’s conception of theory right? Theory, for Laitin, is formal theory, and formal theory “(a) postulates relationships among abstract variables, (b) has rules of correspondence such that one can map values for a large number of real world cases on each of the variables, and (c) provides an internally consistent logic that accounts for the stipulated relationships” (2000: 3).

One can—I will—briefly question five aspects of this understanding of theory: closure, scale, quantification, abstraction, and consistency.

By closure I mean the idea that formal theory is theory period—ruling out any other kind. Is it not possible that Laitin’s formula, however attractive, is only one way of specifying what theory can be and how it could be made?

By scale I refer to his insistence on large ns. Could there not also be theory on a small scale? Consider a theory of regime change that has been stretched to fit the nearly two hundred member states of the United Nations. Will that necessarily explain regime change in Southeast Asia better—more convincingly—than a theory that has been developed intensively in that region’s ten countries and is meant to apply only to them?

Quantification is crucial to statistical analysis and formal modeling and therefore to Laitin’s notion of theory. If quantifying a phenomenon entails losing some information about it, and if quantification thereby omits some phenomena, or distorts some more than others, how can the omissions and distortions be handled except by recourse to qualitative methods? Arguably, quantitative methods have made the need for qualitative ones even greater,
notwithstanding Laitin’s having marked only the latter for possible extinction (2003: 180).

As for postulations, relationships, variables, rules, internally consistent logic, and what they say about the “real world,” these notions still must be expressed to some considerable extent in words—as part of a qualitative methodology. And if qualitative methods are essential, how can qualitative theory be overlooked? Is it not altogether possible that some phenomena are better explained by qualitative theory than by its quantitative counterpart, or at least that the balance of qualitative and quantitative methods involved should not be determined in advance? Is “informal theory” inferior to “formal theory” if, for a given empirical relationship to be explained or an analytic problem to be solved, the cost of quantification measured in omission and distortion exceeds the benefit measured in specification?

As for abstraction, decades ago Giovanni Sartori, writing in the *APSR*, famously distinguished three kinds of theory on the “ladder of abstraction”: narrow-gauge, middle-range, and global. He went on to advocate “develop[ing] the discipline [of political science] along a medium level of abstraction” using “better intermediate categories” to improve theories of the middle range (1970: 1,044, 1,053).

Do Sartori’s three types of theory differ only in degree? Does climbing the ladder imply only a larger $n$? Since Sartori wrote, has political science attained the highest rung, with no need to back down? Or are there intellectual issues involved that have not gone away, including what Sartori called “conceptual stretching”—a pitfall to which large-$n$ categorizers may be especially blind? And if that is so, are there circumstances in which one would want to resist abstraction, avoiding a higher rung for the sake of greater analytic clarity and empirical certainty on a lower one? Is “local knowledge” something that a political scientist should want to leave behind?

It is conventional to assess theories by consistency and correspondence—the seamlessness of their internal logic on the one hand, and of their external fit with reality on the other, insofar as that reality can be independently apprehended. Between two theories about the same subject, one may appear to be modestly less consistent than the other yet seem to correspond to reality better. In such an instance, should consistency overrule correspondence? And what if the slightly less well-knit but apparently more accurate theory is narrow-gauge or even micro-gauge in the sense of being limited to a single country, province, village, or even household?
Could the consistency of the analyst’s mathematical logic be failing fully to capture the sometimes inconsistent thinking and behavior to be found among the people under study? If so, is the solution to climb the ladder of abstraction, enlarging the $n$ in the hopes of ensuring actor rationality at least on average? Or to pursue the apparent anomaly more deeply in that local situation? Consistency may not be the hobgoblin of small minds, but it could be less evident in some smaller populations, whose outlooks and actions might therefore require, first and foremost, a qualitative understanding of what is going on.

None of these questions is against “science.” Rather, they ask how narrowly the term should be construed.

Perhaps in the busy overlap between area studies and political science one ought to consider, alongside rational-choice methods, what might be termed a methodology of “plural choice.” By “plural choice” I mean to acknowledge (a) the hypothetical multiplicity of motives and behaviors in a population under study, (b) the intellectual diversity of approaches and methods available to those who wish to study that population, and (c) the need to make the choice of a research method fit the research problem, not the other way around.

By pluralism I do not imply indecision. One must choose, and as path dependence sets in, future choices will become less plural. Nor do I imply hostility toward a “science” of rational choice. Depending on the research problem, a highly statistical, mathematically modeled, and large-$n$-scaled approach may indeed work best. I do, however, imply a commitment not to put theory permanently first, above all other forms of understanding, or to commit oneself first and foremost to a method, as if it fit all problems, like the proverbial man whose possession of a hammer made everything he saw resemble a nail.

A plural-choice approach need not compete with its rational-choice alternative, but could improve it by addressing objections to it: for example, that it makes insufficient allowance for nonrational choice, or that it privileges the rationality of material interest.

Within economics, there is already a literature on “bounded rationality” and a subfield, behavioral economics, that explores “quasi-rational choice.” Plural-choice theorists would go beyond these modifications, however, to explore awkward questions that rational-choice theorists, with their emphasis on voluntary decisions to make or break coalitions, have tended to treat as exogenous to their models or simply to ignore.

One of these questions would require the researcher to pay close and sus-
tained attention to first-order beliefs, emotions, and intentions. By “first-order” I mean what actors actually think they are doing, and not—or not just—what a formal modeler deduces they must up be to.\textsuperscript{14} Apparent in this context is the self-impoverishment that rational-choice theory has achieved through its lack of interest in anthropology.

Discovering and charting first-order consciousness is, of course, not all a scholar has to do. When anthropologists deploy Kenneth Pike’s classic distinction between “emic” and “etic” discourse, they do not rule out the latter, second-order, analyst-driven understandings. Anthropology is far more than ethnography, and even ethnography is more than purely descriptive. The “narratives” that rational choicers consider merely atheoretical, or at best pre-theoretical, involve decisions of selection and presentation that owe at least something to the analyst’s own “etic” concerns, including theoretical ones—matters that traditionally may have been, and may still be, external to the individuals and communities under study.

For imaginative ethnographers and sober modelers alike, however, it stands to reason that the greater the gap between what an analyst thinks is going on and what the objects of her or his analysis think is happening—and why—the greater the danger that explanations and predictions deduced from formal models will fail. For the sake of the abstraction, coverage, consistency, and specification that formal modeling requires, the modeler risks imputing to distant, diverse, and variously informed actors a motivation that plays no role, or a minor one, in the first-order mixture of motive and reason—and arguably, yes, a kind of theory—that actually leads them to do what they do.

The implications of mixed rationality as a basis for inter-specialist conciliation and synergy are unclear. But such an approach at least leaves open the chance of success. Each party to the dispute would have to be willing to give up something, but perhaps not too much. On the disciplinary side, the modelers would have to qualify their assumptions, possibly in both meanings of that verb. But a modified—diversified—conception of rationality would remain central to the interface, and thus to cooperation across it. On the area side, scholars would be nudged toward becoming more numerate, and toward a greater tolerance for comparison. Die-hard exceptionalists among them might even be persuaded to try comparing “apples and oranges” after all. At the same time, the new credibility of inductive research, and the new migration of ex-“field workers” downstream toward theories of their own making, would
be welcomed by area specialists, if not also their informants, as enhancing cognitive respect and, more important, scholarly potential.

Meanwhile, pending such bargains, in the dynamic, creative zone of area-discipline overlap where this book was conceived, Southeast Asianists who are simultaneously political scientists, and vice versa, will seek their own terms of enlistment.