Chapter 3

Democracy and Dictatorship Do Not Float Freely:
Structural Sources of Political Regimes in Southeast Asia¹
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“If recent democratization is, indeed, a global process, then the terrain of these studies should better reflect that fact. Moreover, only by expanding the geographical horizons can we know whether our conceit as social scientists – that is, our presumption of generalizability – is well founded.”

Valerie Bunce (2003: 168)

Democratization and Divergence: Regions and Regimes in Political Science

Southeast Asia specialists have paid significant attention to democratization, but leading theorists of democratization have exhibited precious little interest in Southeast Asia.³ This chapter will argue that Southeast Asia provides extraordinarily fertile territory for assessing and improving existing theories in political science about why some authoritarian regimes collapse (while so many others survive), and why some new democracies flourish (while most flounder). I hope to convince the reader that democratization theorists can only continue to ignore Southeast Asia to the detriment of the discipline.

The most compelling methodological rationale to take greater account of Southeast Asia is the region’s astonishing variation in regime outcomes: what Kevin Hewison rightly calls its “remarkable range of political forms” (1999: 224). Whereas Samuel Huntington’s (1991) famed “Third Wave” of democratization has indeed resembled that mighty metaphor in some regions
(i.e. Latin America, Southern Europe, and Eastern Europe), it has represented nothing more than a faint ripple in others (i.e. Central Asia, North Africa, and the Middle East). This makes it nearly impossible to know whether domestic, regional, or global factors are primarily responsible for producing such region-wide patterns of democratic transition or authoritarian durability. In a region exhibiting significant variation in regime outcomes such as Southeast Asia, it is easier (though not entirely unproblematic) to control for confounding regional and global factors, and focus on the divergent domestic factors driving intra-regional divergence in democratization.

By any metric of regime variation, Southeast Asia represents a motley crew. Two countries in the region can be considered electoral democracies (Indonesia and the Philippines), while a third (Thailand) has recently seen its democratic procedures interrupted, almost certainly temporarily, by a military coup aimed at removing the polarizing figure of Thaksin Shinawatra as prime minister. Four other Southeast Asian countries are unambiguously and unabashedly authoritarian (Vietnam, Burma, Laos, and Brunei), and another three present varying mixes of competitive and authoritarian features, while falling short of the minimum procedural definition of electoral democracy (Malaysia, Cambodia, and Singapore). Democratization in Southeast Asia has been more than a mere ripple, but less than a full-fledged wave.

Consolidation is trickier to gauge than contestation, in part because political scientists disagree about what consolidation actually means. But by any reasonable definition, Southeast Asia’s regimes are very diverse on this dimension as well. On the authoritarian side of the contestation continuum, ruling parties in Vietnam, Malaysia, Singapore, and Laos are confronted by weak or non-existent oppositions. Regime change in these cases (as in Brunei’s oil-soaked sultanate) is not unthinkable, but nor is it on anyone’s radar screen. By contrast, Burma’s freakishly durable military regime faces a highly determined opposition which refuses simply to
disappear. Similarly, Cambodia’s electoral authoritarian regime can hardly be said to
countenance its democratic opponents, but nor has it managed to crush them entirely.

This substantial variation in regime outcomes provides enormous opportunities for
improving regime theories, via comparative studies of contemporary divergence within
Southeast Asia itself. By contrast, students of the world’s “wave” and “ripple” regions find
themselves pressed either to devise cross-regional comparisons, or to examine earlier historical
epochs, when regime outcomes in their region of interest still differed. The latter approach
entails an unfortunate silence on the most recent events in global democratization. The former
approach – conducting research across rather than within regions – comes with the danger that,
in a wide-ranging search for cases that vary in outcomes, researchers will compare cases that
vary too greatly in potential causes as well. As Valerie Bunce sensibly puts it in her defense of
intra-regional research designs: “The most illuminating comparisons are those that restrain the
universe of causes while expanding the range of results” (2003: 169). Controlling for all
potential independent variables is tough enough in one world region; doing so in a cross-regional
research design is even more nightmarish.

Yet some regions are more nightmarish for comparative research than others. Herein lies
the biggest obstacle to comparing regime outcomes in Southeast Asia. While the region exhibits
greater variation in outcomes than regions such as Latin America and the Middle East, it also
contains greater variation in factors that might plausibly cause that variation (i.e. economic
development, colonial legacies, and ethnic and religious distributions). Donald Emmerson has
called Southeast Asia the most “recalcitrant” region for students of democratization, in large
measure because its “contiguous states are so diverse, despite their proximity, as to make it
difficult to generalize across them” (1995: 225). The opportunities presented by Southeast Asia’s
diversity in regime outcomes are thereby matched by the obstacles posed by Southeast Asia’s diversity in potential causal factors.

Southeast Asian regime theorists have tended to display more anxiety about the region’s comparative obstacles than anxiousness to take advantage of the region’s comparative opportunities. Multi-country studies remain the exception, not the norm. Yet there are a wealth of single-country studies of Southeast Asian regimes (and at least a handful of comparative works) that have provided valuable empirical insights and surprising theoretical challenges to what we think we know about democratization. By “broadening….the geography of the conversation” to include Southeast Asia, we might build democratization theory in fruitful and surprising new directions (Bunce 2003: 168).

Knowledge Accumulation in the Study of Southeast Asian Democratization

How should we assess the theoretical contributions of Southeast Asian regime studies to political science? A good starting point is James Mahoney’s emphasis on “knowledge accumulation” as a proper benchmark for the progress of any research tradition. In the broadest possible terms, knowledge accumulation occurs whenever researchers “generate useful knowledge about phenomena of interest” (Mahoney 2000a: 1). But how do we determine whether the knowledge produced by a particular research tradition is “useful,” and how can we decide whether that knowledge is “of interest” or not?

Research on Southeast Asian democratization has indeed produced useful findings that should be of interest to more than just Southeast Asianists. As Robert Taylor argues, the finest scholarship on Southeast Asia finds ways “to study its politics on its own terms, but without ignoring the universal features to be found there” (1996: 11). The best work on Southeast Asian
regimes speaks directly to pressing theoretical debates on democratization in political science (and is therefore broadly “of interest”), and expresses its findings in terms that can be translated from context into variables (which makes them broadly “useful” to the study of democratization). On issues ranging from the role of ethnic divisions in hindering democratic transition to the importance of ruling parties in sustaining authoritarian rule, Southeast Asian regime studies provide new thinking, convincing evidence, and compelling arguments with profound implications for democratization theory.

To be sure, Southeast Asianists often neglect to situate their arguments in wider theoretical literatures, treating their cases as sui generis by default, if not necessarily by design. When Southeast Asian regime studies do directly address existing theoretical explanations, moreover, it is rarely if ever with a mind to test these hypotheses in the sense of confirming or falsifying them in any definitive way. Rather, these analyses tend to help accumulate knowledge through “hypothesis elaboration,” in which scholars either (1) “introduce new independent variables that work in conjunction with previously identified ones,” (2) “extend independent variables….to a new set of outcomes,” or (3) “[identify] the scope conditions that govern a hypothesis.” This type of analysis “serves as a springboard for the creation of a new hypothesis that yields additional information about causal patterns” (Mahoney 2003: 135, emphasis in original.).

This emphasis is noteworthy, insofar as methodological discussions in political science tend to focus on the role of scholarship in either generating new hypotheses or testing existing ones, with more status typically accruing to the latter task. Hypothesis elaboration is something different, yet equally important. It aims not to have the first word or the final word in a scholarly conversation, but to intervene in that conversation in a way that invites further interventions.
While political scientists often see such an enterprise as properly relegated to a “pre-testing” phase of the research cycle, hypothesis elaboration can be quite useful at any stage. Even after causal hypotheses have been subjected to rigorous quantitative testing (and even when the correlations they uncover are entirely convincing), important questions invariably remain as to the causal mechanisms through which apparent causes produce outcomes of interest. Using deep knowledge of particular cases to assess causal processes serves the valuable purpose of regenerating hypotheses for further consideration and assessment – and in some instances, for a return to the theoretical drawing board.

Southeast Asianists’ emphasis on elaborating (or “regenerating”) hypotheses rather than testing them is not a weakness, I argue. It is a proper response to the democratization literature’s excessive reliance on European and Latin American cases. Before we can test hypotheses in any convincing or remotely definitive way, we must first construct a more complete universe of hypotheses that reflects a more representative sample of cases. It is simply premature to conclude that Southeast Asia’s “refusal to conform” to theoretical expectations exemplifies the “anomalousness” of the region’s democratization experience (Emmerson 1995: 226, 223). Bringing Southeast Asia’s diverse regime experiences into the theory-building process might well show that the region fits broader causal patterns that have not yet been identified, due to the selection bias that has afflicted regime theory in political science to date.

In considering the prospects for research on Southeast Asia to make a bigger theoretical splash, I argue that the region contains the greatest possible lessons in domains where it exhibits the greatest variation. For instance, Southeast Asia can probably tell us little about the historical origins of democracy – a primary focus of studies of Europe and Latin America – because democratic procedures initially arose more or less simultaneously throughout the region, through
global diffusion, as part and parcel of the decolonization process after World War II. For much the same reason, Southeast Asia provides limited guidance in analyzing the theoretically salient topic of democratic breakdown. Having been introduced from the outside during the “second wave” in the 1940s and 1950s, electoral democracy collapsed throughout Southeast Asia during the “second reverse wave” between the mid-1950s and early 1970s.⁹

Where Southeast Asia potentially has the most to teach us is in the core theoretical concerns of the “third wave,” when the region’s contemporary regime variation actually emerged. First, how do we explain the fact that some countries have undergone democratic transitions, while many have not? Second, on the opposite side of the same coin, how can we explain the sources of authoritarian durability in those regimes that have avoided democratization? And third, among those countries where transitions have taken place, why have some had significant success with democratic consolidation, while most new democracies face such serious problems of political performance that regime survival is far from assured?

Southeast Asianists have produced a wide array of works addressing these phenomena of theoretical interest. Yet the shared strength of these diverse studies, I argue, lies in their careful attention to the structural forces that influence regime change, stability, and consolidation. Regional specialists have long recognized that regimes are not simply disembodied rules or practices of governance, or the straightforward product of contingency and elite choice. Like ideas, democracy and dictatorship do not float freely. In trying to understand how regimes work, it is essential to examine – and Southeast Asianists have been exceptionally effective at examining – the supporting institutions that actually shape their performance, cohesion, and prospects for survival: (1) states, (2) political parties, and (3) militaries. At the same time,
regional specialists have climbed outside the state to examine two other key structural forces shaping Southeast Asia’s political regimes: (4) social movements and (5) social classes.

In short, democratization involves not just individual decisions, but organizational collisions. Recognizing this, Southeast Asianists have never fallen victim to a major “mistaken assumption” of the leading “transitions paradigm” in democratization theory: namely, that “the underlying conditions in transitional countries….will not be major factors in either the onset or the outcome of the transition process” (Carothers 2002: 8). Even the strongest proponent of a voluntarist approach to democratization in Southeast Asia, William Case, acknowledges that one can only adequately apprehend elites’ behavior after properly “specifying their institutional or social grounding” (Case 2002: 28). As leading regime theorists seek to “thicken” their analyses with more attention to the political structures that influence processes of regime transition and consolidation, Southeast Asian studies provide a wealth of examples of how this might be achieved.

It is these structures-as-factors and regimes-as-outcomes that guide the discussion from here. To avoid overlap with Meredith Weiss’ chapter on civil society and Kikue Hamayotsu’s chapter on religious politics, I set aside the role of social movement organizations and focus on the other four structural forces mentioned above: (1) social classes, (2) states, (3) militaries, and (4) political parties. In assessing the potential contributions of these studies to regime theory, I focus my attention on democratic transitions and authoritarian durability, rather than democratic consolidation. I do so in part because Allen Hicken covers topics related to democratic consolidation in his contribution on parties and elections, and partly because political scientists are already much more cognizant of the structural sources of democratic consolidation than democratic transition. That the consolidation of democracy depends on capable states and
effective political parties is well understood; that prospects for democratic transition also depend on the character of state and party institutions, much less so.10

**Structural Foundations of Democratic Transitions and Authoritarian Durability**

No aspect of political regimes has attracted more theoretical interest than the puzzle of democratic transitions. Two streams of analysis have dominated the discussion for the past three-plus decades: (1) the Barrington Moore (1966) research tradition, which sees class structure as the primary factor driving democratization; and (2) the Dankwart Rustow (1970) research tradition, which sees regime change as a product of highly contingent elite bargains during moments of political crisis. Whereas social-structural analysis dominated examinations of democratization’s “first wave” in Europe, elite bargains have attracted the most interest since the “third wave” spread to Latin America in the mid-1980s.11

Regime studies have consistently been less diverse than the regime outcomes demanding explanation, however. Scholars’ fascination with uncovering the diverse dynamics of democratization has never been matched by interest in unraveling why authoritarianism so often endures. Only recently, with Barbara Geddes’ path-breaking research on authoritarian institutions (1999a, 1999b), has any sort of research agenda on **authoritarian durability** emerged. This approach has two enormous merits. The first is methodological: it captures much-needed variation in outcomes. The second big benefit is theoretical: by calling attention to authoritarian institutions, it steers a middle path between the apolitical determinism of the “social prerequisites” literature as well as the asocial and ahistorical contingency of the “transitology” literature.12 By looking at political institutions, we can show that social structure is not political
destiny, and that democratization and authoritarian durability are much more than the result of random accidents.

Given Southeast Asia’s longstanding variation in regime outcomes, experts on the region have never ignored the structural foundations of authoritarian durability in their efforts to illuminate the dynamics of democratization. This can be seen not only in sophisticated treatments of Southeast Asian states, parties, and militaries, but also in a number of sensitive studies regarding the effect of class structures on democracy and dictatorship.

Social Classes

Although choice-theoretic approaches to democratization have gained predominance in political science in recent decades, social-structural perspectives have by no means run their course. The main point of consensus in this literature is that economic development fosters democratization by strengthening classes with an interest in avoiding authoritarian rule, while weakening the position of the group generally seen as the major class antagonist of democratization: labor-repressive landed elites.\textsuperscript{13} Disagreements abound, however, over whether it is the bourgeoisie, the middle class, or the working class that tends to serve as the “carrying class” for democracy.\textsuperscript{14} More recently, a debate has arisen as to whether development and democracy are correlated because development presses dictators to introduce democracy (via “endogenous democratization”), or because national wealth helps sustain democracies over time (via “exogenous democratization”).\textsuperscript{15} The big theoretical debates thus surround not whether, but how economic development and related class shifts improve prospects for democracy.

Like these leading theorists, leading Southeast Asianists have consistently highlighted the role of social classes in their studies of political regimes. For instance, successive edited volumes
during the 1990s by Kevin Hewison et. al., Anek Laothamatas, and James Morley similarly attempted to assess the impact of Southeast Asia’s swelling middle class and bourgeoisie on democratization in the region (Hewison, Robison, and Rodan 1993; Anek 1997; and Morley 1999).

The contributors to these volumes were confronted by the same basic puzzle: Considering that Southeast Asia’s capitalist economies had experienced faster growth rates and more dramatic class transformation than most regions in the developing world, why was democratization still so rare? More to the point: Why were the two countries with the fastest growth rates and the largest urban middle classes in the region – Malaysia and Singapore – proving more resistant to democratization than Thailand and the Philippines? And why did the Philippines – the Southeast Asian country with the most intransigent class of landed elites – have less experience with outright authoritarianism than any other country in the region? From a class-analytic perspective, Southeast Asia appeared to be uniquely and intriguingly plagued by both unlikely democracies and unlikely dictatorships.

Indonesia’s transition to electoral democracy in 1998 only seemed to compound the puzzle of Southeast Asia’s glaring mismatch between development and democratization. Harold Crouch and James Morley continued to assert the value of a class-analytic approach, insisting that economic development produced powerful pressures for political change. Yet even by their own coding of socioeconomic development – which considers Indonesia, Thailand, and the Philippines insufficiently developed to sustain democracy – only four of Southeast Asia’s eleven countries support this hypothesis. And all four are poor dictatorships (Burma, Cambodia, Laos, and Vietnam), not rich democracies. “Endogenous” theory is thus questioned by the resilience of
rich dictatorships, while “exogenous” theory seems stymied by the persistence of poor democracies.

Southeast Asia’s striking nonconformity to theoretical expectations pressed these volumes’ editors and contributors to consider additional causal factors. Using Southeast Asian cases alone to reject the class perspective on democratic transitions outright would have been an untenable move, considering this literature’s longstanding value in illuminating democratization in other world regions. Rather than rejecting the importance of class, these authors sought to qualify it by introducing an array of auxiliary hypotheses. The good news for theory-building was that these studies generally stated these new hypotheses in terms of causal variables that could be applied to other cases, rather than attributing Southeast Asia’s apparent exceptionalism to underspecified contextual factors. As argued earlier, this type of “hypothesis elaboration” represents a vital (if underappreciated) step in processes of knowledge accumulation in comparative politics.

To be sure, these analyses are no paragons of parsimony. They adopt more of an additive, “let a hundred variables bloom” approach to explanation, rather than attempting the daunting task of mediating among the multitude of potential explanatory factors they introduce. For instance, Anek’s introductory essay posits at least six variables of importance in breaking the causal chain from development to democracy: (1) statist economic development; (2) political culture, especially collectivist and subservient mass attitudes; (3) communal divisions; (4) performance legitimacy deriving from rapid growth; (5) electoral legitimacy, in those regimes that countenance at least a modicum of political contestation; and (6) the weakness of social capital (Anek 1997: 11-17). Meanwhile, Crouch and Morley bring even more potential “suppressor variables” to the table: (7) country size, (8) external threat, (9) elite cohesion, and,
most interestingly for our discussion, (10) political institutions – specifically the state, party, and
military organizations that provide authoritarian regimes with their necessary institutional spine
(Crouch and Morley 1999: 317).

Many political scientists might dismiss these works as failures to provide parsimonious
causal models. Yet at this stage of theory development, I would argue that this type of work is
absolutely essential. Southeast Asia’s diverse regime experiences are yet to be meaningfully
incorporated into democratization theory. The effort to do so must begin by recognizing the wide
range of factors that might plausibly influence regime outcomes in these theoretically neglected
cases. Causal messiness cannot simply be wished away in a headlong rush to parsimony; it must
be gradually and carefully reduced with informed, controlled comparisons of cases that theorists
have heretofore largely ignored.

A valuable effort to trim this thicket of independent variables comes from Jacques
Bertrand’s (1998) discussion of growth’s ambiguous effect on democratization in Southeast
Asia. His explanation for variation between democratic Thailand and authoritarian Indonesia and
Malaysia is parsimonious in the extreme. Relative ethnic harmony explains why capitalist
economic growth undermined authoritarian rule in Thailand, while greater communal friction
precluded such political transformations in Indonesia and Malaysia.

By arguing that Thailand in fact supports the “endogenous democratization” hypothesis,
Bertrand usefully highlights a nation’s economic growth rather than its total economic wealth as
a possible stimulant for authoritarian collapse. If Bertrand had expanded his purview to the
Philippines, a case of much slower growth, he might have concluded that it is not growth per se,
but the expansion of the educated, urban middle class that provides a necessary social
precondition for democratization – even as Singapore and Malaysia continue to make it
abundantly clear that such a class transformation is no sufficient condition. Including the
democratic Philippines would also have lent powerful additional support to Bertrand’s
hypothesis that muted ethnic and religious tensions represent a necessary condition for
democratic transition to take place.

In making his intriguing argument that communal frictions suppress the impact of
economic growth on democratization, however, Bertrand was not only hindered by suboptimal
case selection. He was also the victim of bad timing: Indonesia experienced a dramatic
democratic transition in the month after his article was published. Since Indonesia is by most
measures the most ethnically diverse country in the region, these events called Bertrand’s
straightforward correlation between communal diversity and authoritarian durability into
question. On the other hand, Malaysia experienced a nasty deepening of authoritarian rule only
months later, and analysts commonly cited the country’s deep communal divides as a barrier to
opposition unity and democratic change.

Southeast Asia’s democratization experience thus suggests that social-structural variables
such as class and ethnicity should neither be expected to explain outcomes on their own, nor
dismissed out of hand as causally insignificant. This mirrors the consensus of sorts that seems to
be emerging in wider democratization theory. Indeed, it is worth recalling that even Dankwart
Rustow (1970), the godfather of the voluntarist approach to democratization, argued that national
unity and a balance of social power among competing factions were necessary social-structural
precursors for democracy to result from elite negotiations. More recently, path-breaking work by
Eva Bellin (2002) has muscularly reasserted a central role for social classes in shaping Third
Wave democratization. Yet even in Bellin’s work, the impact of business and labor on
democratization depends not on development per se, but on the state’s role in guiding it.
Theorists are thus well-advised to go beyond pure class-based explanatory models, and to consider the causal significance of political institutions in democratic transitions: a theme that has long captured considerable attention among Southeast Asianists, but not among democratization theorists.  

The State

The natural place to start our institutional analysis of democratization is with the state – the institutional apparatus that authoritarian regimes control, and that their opponents hope to seize. Authoritarian regimes are sharply differentiated by the extent to which leaders personalize rule, typically undermining the effectiveness of the state apparatus in the process. In its most extreme form – “sultanism” – personalization tends to produce sharp and violent conflict between regime loyalists, whose future depends on the survival of their patron, and regime opponents, who know that the ruling clique is too desperate to hold onto power to negotiate a peaceful exit (Bratton and van de Walle 1997).

Mark Thompson has applied this model to the Philippines, where Ferdinand Marcos’ personalized rule bore a family resemblance to classic Latin American and African cases of sultanism. Thompson shows how the collapse of the Marcos regime was not simply the result of severe economic crisis and the contingencies of elite calculations, but was deeply influenced by the weakness of the Philippine state. Indeed, the economic crisis of the mid-1980s that helped destroy the Marcos regime cannot be properly understood absent an appreciation of Philippine state incapacity. “Marcos had to increase foreign borrowing because government institutions were too corrupt to be effective revenue collectors,” Thompson argues. “A stagnant tax base could not finance the mounting demands on public resources by his inner circle, whose greed
seemed to know no bounds” (Thompson 1995: 66). If the Marcos regime had enjoyed access to a more effective state, this analysis suggests, it might have avoided financial crisis and subsequent democratic transition altogether.

In Thompson’s framework, personalization of regime power weakens the state, which undermines a regime’s durability by restricting its access to revenue for patronage purposes. But as Richard Tanter and William Liddle have shown, the personalization of power that took place in Indonesia under Suharto had a less pernicious effect on the state than Marcos’ sultanism exhibited in the Philippines. For Tanter, Indonesia’s access to oil revenues and foreign aid produced double-edged rather than purely debilitating effects. On the one hand, these revenues had given rise to “the hypertrophy of the state vis-à-vis other social organizations.” Yet Suharto’s Leviathan also appeared to stand upon clay feet, as Tanter concluded that “this power is highly vulnerable, since the stoppage of external rents can severely damage the government finances” (1990: 71, 70). From this perspective, Suharto’s fall in May 1998 was not simply the contingent product of an unforeseeable economic crisis, but a regime outcome shaped significantly by historical patterns of state formation in Suharto’s New Order.

Whereas Tanter usefully highlights the role of external revenues in shoring up sultanistic patronage networks, Liddle more fundamentally questions the notion that regime personalization and state weakness are opposite sides of the same coin.18 Suharto personally dominated Indonesia as surely as Marcos dominated the Philippines; yet Suharto ruled through what Liddle terms a “presidential-military-bureaucratic complex,” and not simply an ad hoc array of personal alliances. Even after Suharto began cultivating a narrower personal clique in the late 1980s, it remained the case that “the bureaucracy pervades society….Its health centers, agricultural extension services and marketing agencies, religious affairs offices, and requirement of personal
identity cards make it for better and worse a daily reality which most Indonesians cannot escape” (Liddle 1985: 71). It thus appears that the lack of stateness under Marcos cannot be ascribed to regime personalization alone, but must be traced to deeper patterns of state-building in Philippine history.

The causal significance of Philippine state weakness in the fall of Marcos is drawn even more sharply in works that directly compare that country’s democratic transition with the (later) transition in Indonesia. When Philippine specialist Vincent Boudreau compares the country’s “People Power” movement in 1986 with Indonesia’s “reformasi” movement in 1998, he uncovers a series of important contrasts where most analysts have seen similarities. Rather than representing sudden and unpredictable shifts in the sociopolitical terrain, mass anti-regime mobilization in both cases followed patterns that were “entirely in keeping with the larger themes of opposition” throughout the Suharto and Marcos eras (Boudreau 1999: 10).

Boudreau expertly shows how these contrasting patterns of opposition were analytically inseparable from the contrasting “authoritarian architecture of the two regimes” (Ibid.) – especially differences in state power. Whereas the Indonesian regime had successfully crushed its mass opponents at the onset of the New Order, its Philippine counterpart lacked the capacity to do so. This left Marcos with much more limited options than Suharto enjoyed. Boudreau hypothesizes that “expanding guerrilla and protest organizations, in combination, forced Marcos into accelerated and amplified cycles of political liberalization and crackdown that propelled the regime toward its mass-mobilization-caused demise” (Ibid.: 6).

Suharto’s state consistently held a greater power advantage over its potential challengers, who were more often lawyers than guerrillas. Suharto thus faced no pressure to gamble with relatively free national elections, as Marcos fatefully did in February 1986. Boudreau shows that
this divergence was not simply a contingent result of elite miscalculation in the Philippines and elite shrewdness in Indonesia. Rather, it reflected deep structural differences in the two countries’ authoritarian regimes. Similarly, there was far more than contingency at work in the Philippine opposition’s success at seizing political power, whereas Indonesia’s more scattered opposition only managed to “convince those in power to exercise their authority to move Suharto aside” (Ibid.: 13). In sum, “the institutions of state rule and the legacies of that state’s domination over Indonesian society essentially foreclosed the ‘people power’ option to the Indonesian protestors” (Ibid.: 15).

Yet Boudreau’s statist model cannot explain how Indonesian protestors managed to overcome legacies of repression and help stimulate authoritarian collapse at all. At first blush, this seems to recommend closer attention to the manifold contingencies of Suharto’s fall: i.e. Indonesia’s severe economic crisis, the president’s declining health, and the non-revolutionary character of Suharto’s strongest elite opponents. Alternatively, scholars might try to undertake an even deeper and more systematic analysis of the structural factors that shaped Suharto’s fall.

Eva-Lotta Hedman adopts this latter approach in her recent comparison of mass mobilization and democratization in Southeast Asia. She goes beyond Boudreau’s analysis in two important and impressive ways. First, she expands the comparison, adding Thailand and Malaysia to Boudreau’s Indonesian and Philippine cases. By bringing in Malaysia, Hedman gains analytically valuable variation on the dependent variable of democratization. Second, she is more diligent about expressing the contextual factors that differentiate these four cases in terms of clear variables. The result is a staunchly structuralist causal account that provides intriguing comparative implications. Whereas most studies of civil society “privilege voluntarism, agency, and contingency,” Hedman’s approach aims to “underscore the importance of examining the
underlying conditions and mobilizational processes anticipating such ‘euphoric moments’ within a structured comparative analytical framework” (2001: 922).

Hedman locates the source of cross-case divergence in four variables, which closely parallel the factors emphasized by Boudreau. First, she highlights “salient variation in the nature of regimes,” especially the degree to which they are institutionally prone to internal division and vulnerable to electoral challenge. Second, Hedman argues for the significance of “the constellation of classes,” most notably the extent of upper-class cooptation accomplished by the ruling regime. Third, Hedman calls attention to “the legacies of the Left,” as countries experiencing a history of robust socialist movements possessed stronger opposition resources for confronting authoritarian rule from below. And fourth, Hedman highlights “the institutions of religion,” particularly the degree to which these institutions enjoyed some measure of autonomy from state control.

Like Boudreau, Hedman argues that to understand the nature of opposition to authoritarian rule, one must first understand the nature of the state in patterning its emergence and evolution. By stating her complex argument in terms of variables, she presents it in a fashion that speaks directly to broad theoretical concerns and debates in democratization theory, and that can be applied to cases outside Southeast Asia. Particularly resonant is her argument that the state’s relationship with organized religion shapes prospects for anti-regime mass mobilization – a causal pattern that comes into sharper relief when comparing the relative autonomy of Islamic institutions in Indonesia with their more co-opted and controlled counterparts in Malaysia. This serves as a hypothesis that can be widely tested, as well as a useful corrective to Boudreau’s dismissal of Indonesian civil society as weakly organized across the board. In sum, authors such as Thompson, Boudreau, and Hedman provide powerful empirical evidence that the state plays a
central role in processes of democratic transition – a structural pattern that is largely and surprisingly neglected in the “transitions paradigm.”

Theoretical attention to the role of “stateness” in consolidating democracy is yet to be matched by attention to the importance of the state in consolidating dictatorship as well.19 But if Southeast Asia specialists appear to be surpassing democratization theorists in their attention to the state, theorists are setting the pace in examining other regime institutions that Hedman hints at in her discussion of “salient variations in the nature of regimes,” but does not address systematically: militaries and political parties.

Militaries and Political Parties

In an agenda-setting effort to give the bargaining-centered “transitions paradigm” a stronger structural foundation, Barbara Geddes has introduced and tested new hypotheses regarding the role of authoritarian regimes’ political institutions in shaping prospects for regime change. Coding all non-democratic regimes as (1) military, (2) single-party, (3) personal, or some hybrid thereof, Geddes (1999a, 1999b) attempts to show that single-party regimes are more resistant to democratic transitions than personalized regimes, which are more resilient, in turn, than military regimes.20

Her logic is crisply game-theoretic, and her evidence is correlative. Military regimes are especially brittle because of intrinsic divisions between political and professional soldiers, whereas ruling parties more effectively avoid internal splits because politics is the profession of party elites. With no barracks to retreat to, party leaders hang together and hang onto power for dear life. Personal regimes generate similarly hard-core support from loyalists who fear political extinction in any democratic transition.
Geddes finds impressive empirical support for her conclusions in a sweeping quantitative test confirming the greater durability of single-party regimes than their personalized and militarized counterparts. Yet if statistical analysis can indeed help convince us that single-party regimes last longest, such techniques cannot show us why they last longest. In assessing the validity of Geddes’ arguments in Southeast Asia, we must be attentive not only to the presence or absence of a causal correlation between specific regime institutions and regime durability, but also to the accuracy of the causal mechanisms underlying her hypotheses.

On the first question, Southeast Asian evidence clearly supports Geddes’ argument regarding the relative robustness of single-party rule in a correlative sense. From relatively wealthy Malaysia and Singapore to low-income Laos and Vietnam, ruling party apparatuses have helped authoritarian regimes avoid democratic transitions by managing elite relationships and quashing mass dissent (Slater 2003). Meanwhile, the three Southeast Asian countries that have undergone democratic transitions never developed party institutions that superseded either the organized power of the military apparatus, or the political authority of the individual leader. When transitions occurred in Thailand, the Philippines, and Indonesia, the military was a stronger broker than those regime’s respective ruling parties. Why Burma’s military regime has bucked this trend is a puzzle for Geddes’ model, however, as we will discuss shortly.

For now, it is worth considering how some of Geddes’ other hypotheses fit Southeast Asia’s diverse set of regime outcomes. Two of her arguments in particular warrant further scrutiny: that personalized regimes are more resistant to democratization than military regimes, and that the most durable authoritarian regimes of all are so-called “triple hybrid” regimes, or those with an institutional profile combining party, military, and personalistic elements in roughly equal measure. Taken together, these arguments suggest that military regimes can
enhance their durability by building strong party institutions, and by cultivating a clique of hardcore loyalists surrounding the individual ruler.

We have already discussed Mark Thompson’s argument that the personalization of power in the Philippines fostered democratization rather than forestalling it, by undermining the state apparatus Marcos needed for both patronage and repressive purposes. Alfred McCoy (1999) similarly traces the debilitating effects of personalization on the Philippine military. McCoy details Marcos’ venality in military appointments, and convincingly shows how this politicization and personalization destroyed the ethos of apolitical professionalism that had broadly characterized the officer corps during the Philippines’ first few decades of independence.

Such tensions between professional and political soldiers are crucial for our story, because they are precisely the mechanism through which Geddes argues that military regimes derive their extreme brittleness. It is good news for Geddes that McCoy supports this interpretation of military regime breakdown, broadly linking the intra-military friction that produced the fateful 1986 coup d’état to the cleavage between political and professional officers. But it is more problematic for Geddes’ overall framework that personalization clearly worsened military factionalism, especially by producing widespread loathing of Marcos’ top crony general, Fabian Ver. This made democratic transition more likely rather than less. Geddes thus gets partial support from the Philippine case. Her posited causal mechanism of breakdown in military regimes fits the historical material nicely, but her suggestion that personalization should help military rule endure sits very uncomfortably with Philippine evidence.

Long-lived military-led regimes in Burma and Indonesia provide greater challenges to Geddes’ causal framework. The stunning durability of the Burmese military regime is particularly vexing. How has the Burmese military managed to avoid the sort of splits between
political and professional soldiers that Geddes sees as dooming military rule? Mary Callahan provides powerful evidence that militaries are not necessarily riven by the political-professional divisions that Geddes portrays as intrinsic to military institutions. While ruling parties tend to cohere because professional considerations are inseparable from political concerns, Callahan adroitly shows that the Burmese military has cohered because the political is never allowed to distract attention from the professional. In short, Burma’s ruling generals “are war fighters who are not adept at politics. But they are war fighters, first and foremost” (Callahan 2004: 2).

Pathologically fearful of any political opponents who might divide the fragile nation-state, the Burmese military has engaged in a disastrously violent confrontation with democratic activists rather than coming to the sort of compromise that Geddes sees as the natural terminus of military rule.

The point is not that the solitary Burmese example definitively falsifies Geddes’ arguments about military regimes. Rather, the point is that the divisiveness or cohesion of military and party institutions is a research question demanding intense empirical scrutiny, not a matter to be determined deductively. By climbing inside the belly of the Burmese military beast, Callahan exemplifies how statistical “outliers” can serve to keep fascinating research questions on the agenda in comparative politics. Most intriguingly, Callahan argues that the Burmese military’s cohesion – and hence the military regime’s remarkable survival – derives from Burma’s abnormally simultaneous struggle against both domestic insurgents and foreign incursions during its decolonization process.

Such historical considerations weigh heavily in the finest studies of the Indonesian military as well. Like Callahan’s, Harold Crouch’s analysis calls into question Geddes’ fundamental assertion that military regimes “carry within themselves the seeds of their own
disintegration” (Geddes 1999a: 5). If divisions between political and professional soldiers in Burma were dampened by shared and simultaneous operational experience against domestic and foreign enemies, Indonesian soldiers developed significant professional solidarity through shared traumas during the national revolution of the 1940s, the separatist rebellions of the 1950s, and the communist upsurge of the 1960s. With its ideology of *dwifungsi*, or dual function, the Indonesian military defined politics as inherent to its professional mission. Seizing power in the mid-1960s did not suddenly turn the military into a political animal, since “it had never previously regarded itself as an apolitical organization” (Crouch 1978: 344). The Suharto regime thus rested on a highly unified and politically ambitious military apparatus at its onset. The military’s cohesion and shared will to power more closely resembled Geddes’ portrayal of ruling parties than ruling militaries.

Like Burma’s, Indonesia’s military-led government lasted for decades – but unlike Burma’s, it ultimately collapsed in the face of popular upheaval and elite fragmentation. Geddes attributes the Suharto regime’s impressive durability to its “triple hybrid” institutional character, as it rested on a combination of military, party, and personal power rather than military power alone. But this raises two big questions. First, why did Indonesia’s “triple hybrid” regime prove less durable than Burma’s pure military regime? And second, can we really explain the Suharto regime’s considerable resilience in terms of its relative reliance on civilian party elites and the President’s personal clique?

Close attention to the dynamics of Indonesian democratization suggests otherwise. Suharto’s personalization of the military had the same sort of debilitating effects on military cohesion as Marcos’ personalization of the military in the Philippines. Jun Honna’s (2003) analysis of Indonesian military politics during the 1990s shows how seriously *esprit de corps* had
eroded since the era when Crouch conducted his research – a historical slide paralleling the one
detailed in McCoy’s longitudinal study of two classes of the Philippine Military Academy.

As in the Philippines, a dictator obsessed with personal power made loyalty rather than
seniority or merit the primary basis for appointments and promotions, as best witnessed in
Suharto’s blatant cultivation of his son-in-law, Prabowo Subianto. The division between military
“hardliners” and “softliners” that facilitated Suharto’s collapse was not a matter of differences in
ideology or democratic proclivity, Honna shows, but of differences in how particular factions
fared in Suharto’s reshuffles. The prevailing military faction was no more democratically
inclined than its opponents, but rather more desperate “to preserve the integrity of the institution
and not leave it vulnerable to the type of political machinations that wreaked such havoc under
Soeharto” (Honna 2003: 200).

When combined with the Burmese example, the Indonesian experience with democratic
transition suggests further grounds for questioning the hypothesis that personalization bolsters
military rule. Even more problematically for Geddes’ framework, the Burma-Indonesia
comparison suggests that the military might have been the primary institutional source of
authoritarian cohesion in both cases, rather than any party apparatus or personal clique of the
leadership. Perhaps both regimes proved so durable because they were constructed upon the
shoulders of military elites who had been unified by shared operational experiences. And perhaps
military-backed rule collapsed in Indonesia before Burma because Suharto tried harder than
Burma’s Ne Win to lessen his dependence on the military through personalizing political power.

Not only did Suharto build a more personalized regime than Ne Win; he also devoted far
more resources to party-building, as captured in Geddes’ coding of the Suharto regime as a
“triple hybrid” regime. Geddes argues that these multiple institutional foundations should serve
as a source of strength – especially relative regime dependence on a supposedly loyal and cohesive ruling party, as opposed to a presumably more fractious and apolitical military.

John Sidel (1998) calls this argument into question with his detailed examination of the collapse of the Suharto regime. Sidel clearly agrees with Geddes that the regime had taken on “triple hybrid” qualities. While the military was obviously the major institutional support for the regime upon its establishment in the mid-1960s, Suharto ultimately elaborated a national political party (Golkar) to mobilize civilian support, and nurtured a coterie of personal loyalists to assume top positions in all of Indonesia’s leading political and business organizations. But Sidel argues that Suharto’s construction of multiple institutional sources of support proved to be his undoing rather than his salvation:

“….the most important tensions within the regime, it may be argued, stemmed from the peculiar mix of institutional bases and personal networks through which Suharto entrenched himself in power and exerted authority over the more than thirty years of his rule. In this regard, one key structural tension within the regime developed between the pattern of circulation within the Armed Forces and the process of personal accumulation by the President, between the military circuitries of his regime and the more civilian networks for his electoral and ideological legitimation and his (and his family’s) economic enrichment” (Sidel 1998: 162, emphasis in original).

For our purposes here, the most interesting aspect of this argument relates to the tensions that arose between the New Order’s military and civilian wings – in institutional terms, between the military and Golkar. Suharto accelerated his party-building efforts in the late 1980s in a concerted effort to gain a civilian counterweight to military power; but this did not necessarily have beneficial consequences for his regime. Instead, the “elevation of civilians to new heights
of influence in the regime inspired disaffected elements in the military to promote oppositional activities against the President” (Ibid.: 174).

As the regime began to look like less of a military regime, and developed more of a “triple hybrid” institutional bricolage, it seemingly produced worsening elite conflict rather than heavier institutional ballast. It is worth conjecturing, in fact, that Indonesia might never have undergone a democratic transition at all if Suharto had allowed his regime to sustain the purely military character of its early years. As the Burmese example shows, military regimes can exhibit astounding resilience, even in the absence – or perhaps because of the absence – of countervailing powers in a ruling party or loyalist clique.

To be sure, the Indonesian case does not falsify Geddes’ arguments regarding either the general durability of “triple hybrid” regimes or the fortifying effect of ruling parties, any more than the Burmese case falsifies her argument that pure military regimes tend to be exceptionally brittle. Suharto’s regime collapsed, but it enjoyed a long run in power before doing so. Nevertheless, Sidel’s analysis suggests that far more attention needs to be paid to the causal mechanisms through which authoritarian regimes endure or implode. Statistical tests of correlative hypotheses are valuable, but they should not be seen as the end of the road. They should be treated as one important stage in the process of knowledge accumulation, which can be fruitfully followed by critical examination of such hypotheses’ empirical implications at close range.

Political Parties and the State

We might best advance this “hypothesis elaboration” process by turning our attention back to the political institution that was discussed at length earlier in this chapter, but which is
glaringly absent from Geddes’ institutional model: the state. As we have seen, the relative strength of the Indonesian state vis-à-vis the Philippine state provides an important clue in understanding Suharto’s greater ability than Marcos to withstand pressures for democratization. This suggests that strong parties might effectively forestall democratization when they are combined with strong states, but foster authoritarian collapse when they are forced to co-exist with strong militaries.

Further evidence in support of the notion that robust party-state combinations provide the strongest institutional bulwark against democratization is presented in studies of two of Southeast Asia’s highly consolidated single-party regimes: Singapore and Vietnam. Quite tellingly, Chan Heng Chee’s (1976) classic study of Singapore’s ruling party finds it impossible to ignore the role of the state, while Stein Tønnesson’s (2000) highly original study of the Vietnamese state consistently addresses the role played by the ruling party.

Chan examined the grassroots organization of Singapore’s People’s Action Party (PAP) in the mid-1970s, seeking sources of institutional robustness that would explain its stranglehold on power. What she found, to her evident surprise, was that the PAP’s grassroots presence had atrophied since the heady days of decolonization in the late 1950s and early 1960s. “The party no longer stresses the importance of socializing the new recruit into party life nor organizing activities to mobilize its members,” Chan reported. “It is striking that practically no political education exists at branch level; and party-building by the PAP, in terms of recruiting members and inducting them into party thinking and party life so that they may perpetuate the party commitment, is not a priority” (Chan 1976: 131-132). The conventional wisdom that the PAP served as a quasi-Leninist political machine appeared to be greatly exaggerated.
The key to Singapore’s exceptional political stability, Chan concluded, was not the PAP’s internal organization and practices, but its firm command over a highly effective state apparatus. “With the assistance of the bureaucracy,” she argued, “the PAP has built an image of effectiveness.” Credit for Singapore’s rapid growth and quasi-welfare state accrued directly to the PAP, as “the long years of partnership between the PAP and the civil servants have undoubtedly accelerated the fusion of the party and Government identity.” Simply put, Singaporeans had come to view “the party as synonymous with the State” (Ibid.: 224).

Tønnesson provides a vivid metaphor for such party-state fusion in Vietnam, where economic reforms have seemingly forestalled the need for political reforms. Rather than seeing states and markets as oppositional forces, Tønnesson views “the state as a system of bones, muscles, lungs, nerves, and veins, and the party as the head, employing market forces to take care of feeding and digestion” (2000: 250).

To keep vital revenues flowing to the party-state’s coffers after the catastrophic collapse in Soviet aid, Vietnamese Communist Party (VCP) leaders had little choice but to welcome foreign direct investment. “Despite certain declarations to the contrary,” Tønnesson argues, “the Vietnamese communist leaders endeavored to keep the new ‘business management layer’ inside the state” (Ibid.: 247, emphasis in original). Joint ventures between foreign corporations and state-owned enterprises have seized the commanding heights of the Vietnamese economy, often with the help of sectoral monopolies granted by – who else? – party leaders. This keeps the VCP’s revenue lifeblood flowing, strengthening regime institutions amid the challenges for regime maintenance presented by rapid socioeconomic change in a society lacking easily manipulated communal divisions. While social-structural conditions seemingly make Vietnam a
prime candidate for democratization, institutional factors appear to be pushing in the opposite direction.

Perhaps the most intriguing evidence in support of the causal linkage from party-state power to authoritarian durability comes from Kate Frieson’s (1996) study of Cambodia’s aborted democratic transition in 1993. This book chapter is extraordinarily valuable in methodological terms, because it examines the only instance when a Southeast Asian single-party regime was defeated at the ballot box – thus providing much-needed variation in regime outcomes, via a fascinating snapshot of a ruling party actually losing its uncontested grip. In elections bankrolled and monitored by the United Nations, the long-ruling Cambodian People’s Party (CPP) was outpolled by the royalist Funcinpec, and forced, for a four-year period, to share power with its rivals.

Why did the CPP lose in the 1993 vote, when no other ruling party in Southeast Asia’s post-colonial era has done the same? At one level, this surprising result was due to the pure contingency of events. The UN had intervened in Cambodia’s civil war, and was committed to securing a fair electoral result, devoting $2 billion to the task of reconstructing Cambodia’s war-torn economy and polity. Should this rare defeat of a single-party regime thus be seen as the result of contingent rather than structural factors?

When one reads Frieson’s analysis against the backdrop of the studies we have just examined, one sees that external intervention was not simply a contingent, unpredictable factor in the regime-change equation. Rather, the intervention of the UN authority, known as UNTAC, helped to loosen both the coercive and financial stronghold of the Cambodian state – dominated by CPP apparatchiks – over Cambodian voters. With its $2 billion in aid funds, largely distributed through non-governmental organizations rather than the Cambodian bureaucracy,
UNTAC was perceived by voters upon its arrival in March 1992 as “the richest patron in centuries” (Frieson 1996: 232). It was only a mild exaggeration to say that by the time of the vote, “UNTAC was the state” (Ibid.: 233). To be sure, the CPP still attempted to link party and state power to its electoral benefit, as it “rallied its administrative structures and civil servants, including teachers, soldiers, and police, to work for the CPP. Those who refused were told they would lose their jobs” (Ibid.: 234). But with state power temporarily fragmented, rather than monopolized by the CPP, Cambodian voters enjoyed a moment of relative immunity from the ruling party’s electoral intimidations and inducements.

This victory for Cambodian democracy did not long outlast the UN-mandated disruption of party-state power, however. The 1993 elections forced CPP elites to share power in the legislature, but the state apparatus remained overwhelmingly controlled by pro-CPP elements. The withdrawal of UN forces left CPP leader Hun Sen with a free hand to overturn his power-sharing arrangement with Funcinpec – which he did, unilaterally and violently, in 1997. Cambodia’s return to authoritarianism cannot therefore be understood simply as the contingent result of one man’s desire for absolute power. Hun Sen was as power-hungry in 1993 as in 1997; but only when he had regained his previous structural advantage, with unencumbered access to combined party-state power, did he enjoy the institutional capacity to destroy democratic procedures to his own political benefit.

In sum, Southeast Asian regime studies have tended to be strongest where democratization theory has tended to be weakest. They have consistently contributed thought-provoking analyses of the structural underpinnings of democracy and dictatorship. As democratization theorists ponder “the end of transitology,” and increasingly examine the social
and institutional forces that shape regime outcomes, they are well-advised to take a closer look at studies of democratic transition and authoritarian durability in Southeast Asia.  

Accumulating Knowledge by Marrying Big Questions with Deep Answers

One of the most distinguished figures in modern American political science, Adam Przeworski, was recently asked to comment upon the successes and failures of knowledge accumulation in comparative politics over the past three decades. His commentary on what he sees as the field’s biggest failings is rather remarkable, considering the studies of Southeast Asian democratization and authoritarianism just discussed in this chapter:

“We still do not know why and when people with guns obey people without them: the determinants of civilian control over the military. We still don’t understand political parties very well. This is truly an important topic, which we have neglected….Though we have learned a lot in general about authoritarianism, I also think we know disastrously little about the structure of dictatorships” (quoted in Munck and Snyder 2004: 30).

If political scientists indeed know “disastrously little” about these vital subjects, it might be in part because of a disciplinary predilection to doubt whether area studies can produce knowledge of theoretical value. Przeworski implies as much when he comments on where he currently sees the biggest theoretical contributions being generated. “I think some of the best research in comparative politics is done these days by economists,” Przeworski says. “They don’t know enough about politics, particularly about institutions, but they address central questions and get provocative answers” (Ibid.). We seem to have reached a peculiar juncture in the development of our discipline, when one of our finest comparativists suggests that “some of the best research in comparative politics” is being conducted by scholars who “don’t know enough about politics.”
Knowledge accumulation in the study of democratization is most likely to occur if we can marry the deep knowledge of politics and institutions displayed by the authors discussed in this chapter, with those askers of big and provocative questions praised by Przeworski. Big questions demand deep answers; but deep answers will only attract much attention from theorists if they address the biggest political questions imaginable. This will require unprecedented levels of mutual engagement between deductive theorists and area specialists. If our understanding of democratization and authoritarianism is to grow, theorists and area experts alike need to consider spending some quality time in unfamiliar sections of their university libraries.
Notes

1 The title borrows from an article by Thomas Risse-Kappen entitled “Ideas Do Not Float Freely” (1994). While he argues that structural forces largely determine the fate of particular political ideas, I make a similar argument regarding the fate of political regimes (democracy vs. dictatorship).

2 This manuscript has benefited from the comments of Jason Brownlee, Allen Hicken, Erik Kuhonta, Bill Liddle, Gabriella Montinola, Jan Teorell, Danny Unger, Tuong Vu, and two anonymous reviewers at Stanford University Press. Thanks also go to Allison Youatt for her meticulous editorial assistance. A shorter version of this chapter focused entirely on authoritarian institutions has been published in the Taiwan Journal of Democracy (Slater 2006).

3 To my knowledge, Haggard and Kaufman (1995) and Huntington (1991) are the only major theory-building exercises in the study of democratization that give serious consideration to Southeast Asian cases. Even in these two instances, other world regions receive the lion’s share of attention.

4 It is obviously troubling that all three of these countries have seen military forces acting as decisive arbiters in recent succession crises. Such problems of post-transition politics are beyond the scope of this essay; but the lingering significance of the Thai, Indonesian, and Philippine militaries surely underscores the importance of examining the institutions that make authoritarianism work and, quite often, keep democracy from working well.

5 Steven Levitsky and Lucan Way (2002) argue that regimes fail to meet this minimum democratic standard when “incumbents routinely abuse state resources, deny the opposition adequate media coverage, harass opposition candidates and their supporters, and in some cases manipulate electoral results.” While ruling parties in Malaysia, Singapore, and Cambodia may
not be major manipulators of election tallies, they all harass oppositionists in ways that make elections intrinsically unfair.

6 It is symptomatic that perhaps the four most impressive recent contributions to regime theory in Latin America examine the First or Second Wave origins of regimes (when outcomes still varied) rather than Third Wave transitions (when authoritarianism collapsed everywhere but Cuba). See Mahoney (2001), Lopez-Alves (2000), Yashar (1997), and Collier and Collier (1991).

7 This is a somewhat broader definition than the one Mahoney adopts in the final version of this manuscript, but one that I still find quite appropriate.

8 On Europe, see, e.g., Downing (1992) and Moore (1966); on Latin America, see the cites in note 6.

9 For an explanation of region-wide democratic breakdown that parallels O'Donnell’s (1973) famous structural economic explanation for authoritarian resurgence in Latin America, see Rocamora (1978).

10 Suffice it to say that Southeast Asianists have produced as much if not more useful theoretical knowledge on the role of classes, parties, states, militaries, and social movements in democratic consolidation than in democratic transition. Southeast Asian political studies pioneered the study of clientilism as a class phenomenon, showing how severe inequality undermined democratic quality. Classic studies include Scott (1972b) and Anderson (1988), with recent work by Robison and Hadiz (2004) serving as a worthy descendent of this research tradition. For recent work on the role of parties in democratic consolidation, see Ockey (2003) on Thailand, Hutchcroft and Rocamora (2003) on the Philippines, and Slater (2004) on Indonesia. Sidel (1999) and the essays in Trocki (1998) are great contributions to our understanding of the role of the state (including
weak states) in improving or reducing democratic quality. Excellent studies of why militaries in Southeast Asia do or do not obey civilian authorities include Chandra and Kammen (2002) and McCoy (1999). Finally, Hefner (2000) and Mujani and Liddle (2004) provide theoretically insightful analyses of social movement organizations in consolidating democratic politics.  

11 Examples are legion, but no single volume signaled the shift in terrain more than O’Donnell and Schmitter (1986).

12 I am grateful to Bill Liddle for reminding me of the stifling effects of theorists’ early obsession with the social prerequisites of democracy. For the classic statement, see Lipset (1959).

13 Mahoney (2003) provides an excellent overview of this literature and its progress.


15 Endogenous democratization has been most recently and rigorously championed by Boix and Stokes (2003); exogenous democratization has been most vigorously defended by Przeworski and Limongi (1997).

16 Theorists have been more concerned with examining how institutional weakness undermines democracies than dictatorships. On the role of political parties in preventing *democratic* collapse, see Bermeo (2003), Ertman (1998), and Luebbert (1991). Although Huntington (1968) ostensibly trained his sights on political instability regardless of regime type, his concern clearly focused on the many falling democracies of that era.

17 See the essays, including Thompson’s, in Chehabi and Linz (1998).

18 I make a similar argument in Malaysia (Slater 2003), where the personalization of power under Mahathir Mohamad in the 1990s clearly failed to undermine either state or regime. To the
contrary, Mahathir ironically made use of a highly developed and loyal state apparatus to crush his opponents and personalize power in the first place.

19 See Linz and Stepan 1996.

20 For a critique and recommended amending of this typology, see Slater (2003).

21 I exclude East Timor because electoral democracy was introduced there amid a struggle for national independence, not a struggle for popular sovereignty against local authoritarian rulers. Like the former communist countries’ simultaneous transitions from foreign domination and authoritarian rule, East Timor’s democratic transition might best be seen as a belated “second wave” transition – driven by decolonization – rather than fitting a “third wave” logic.

22 For a fascinating analysis of revenue-mobilization strategies in Burma, see Steinberg (2001).

23 Thanks to Allen Hicken and Erik Kuhonta for alerting me to the limitations of UNTAC’s influence, which were indeed considerable.

24 Beyond the existing works cited here, works in progress on the role of parties and states in sustaining Southeast Asian authoritarianism include Brownlee’s (2007) comparison of Malaysia and the Philippines with Iran and Egypt; Smith’s (2007) comparison of Indonesia and Iran; and Slater’s (2005) comparison of party, state, and regime outcomes in seven Southeast Asian cases.