In my article entitled “Getting Asia Wrong,” I make two major arguments. First, European-derived theories in general and realist theories in particular frequently have difficulty explaining Asian international relations. Second, international relations scholars need to be as careful about issues of empirical testing and theoretical rigor when studying Asia as they are when studying Europe. In a reply to my article, Amitav Acharya agrees with both of these claims while also critiquing my essay in arguing that shared norms and institutional linkages mitigate rivalry in Asia and that I am a historical determinist. Acharya, however, has misunderstood both international relations theory and the role of history. His response to my article provides me with an opportunity to clarify and briefly expand on the major themes in “Getting Asia Wrong.”

As Acharya’s reply exemplifies, most scholars not only dismiss the notion that the Asian experience might force a rethinking or modification of European-derived theories, but they also pay little attention to the historical Asian international system. Acharya writes, “Contrary to Kang’s argument, Asia’s future will not resemble its past” (p. 150). Acharya, however, has misunderstood my argument: To study the role of history is hardly to predict that it will replicate itself in the future. My main point is that there are good reasons to think that Asian states may not function like European states and that the study of Asia must begin with a discussion of some of Asia’s empirical anomalies and what might explain them. Acharya seems to argue that the only goal to which scholars who study Asia can aspire is to unquestioningly apply existing theory. In contrast, my goal is to expand international relations theory so that scholars can better identify factors that help to explain regional and temporal differences in how states think about and achieve security and how they conduct their international relations.

Hierarchy, Balancing, and Empirical Puzzles in Asian International Relations

David C. Kang

“In Getting Asia Wrong,” I make two major arguments. First, European-derived theories in general and realist theories in particular frequently have difficulty explaining Asian international relations. Second, international relations scholars need to be as careful about issues of empirical testing and theoretical rigor when studying Asia as they are when studying Europe. In a reply to my article, Amitav Acharya agrees with both of these claims while also critiquing my essay in arguing that shared norms and institutional linkages mitigate rivalry in Asia and that I am a historical determinist. Acharya, however, has misunderstood both international relations theory and the role of history. His response to my article provides me with an opportunity to clarify and briefly expand on the major themes in “Getting Asia Wrong.”

As Acharya’s reply exemplifies, most scholars not only dismiss the notion that the Asian experience might force a rethinking or modification of European-derived theories, but they also pay little attention to the historical Asian international system. Acharya writes, “Contrary to Kang’s argument, Asia’s future will not resemble its past” (p. 150). Acharya, however, has misunderstood my argument: To study the role of history is hardly to predict that it will replicate itself in the future. My main point is that there are good reasons to think that Asian states may not function like European states and that the study of Asia must begin with a discussion of some of Asia’s empirical anomalies and what might explain them. Acharya seems to argue that the only goal to which scholars who study Asia can aspire is to unquestioningly apply existing theory. In contrast, my goal is to expand international relations theory so that scholars can better identify factors that help to explain regional and temporal differences in how states think about and achieve security and how they conduct their international relations.
In this reply I make three points. First, I show that Acharya’s assertion that I am claiming an exceptional role for Asia is unfounded; I also show that rigorous social science demands that scholars be open to the possibility that evidence may force theoretical modifications. Second, I demonstrate that the study of hierarchy is a well-developed branch of international relations theory, and I explain why balancing should not be the default hypothesis in international relations. Third, I examine Asia’s empirical record to illustrate the importance of an increased focus on Asian history for the field of international relations; in addition, I enumerate several current empirical anomalies that scholars need to address given the challenge that these pose to conventional theoretical explanations of Asian international relations.

Theory Building and Theory Testing across Regions

International relations scholars must take seriously the possibility that different regions of the world might indeed be different. Relatedly, critiques of this claim as somehow fundamentally exceptionalist misunderstand the nature of scholarly inquiry. It is good social science to be open to the possibility that evidence may not fit a theory, just as it is possible to note difference without resorting to caricature—a trap into which Acharya falls when criticizing some of my assertions as having an “exceptionalist ring” (p. 162). An example of how progressive research might occur is perhaps best demonstrated through a comparison of scholarship on Asian development with scholarship on Asian international relations. According to a number of criteria—theoretical sophistication, attention to the empirical record, and impact on the wider field of social science—the study of Asian international relations lags far behind. A brief review of the intellectual history of Asia confirms this observation.

Beginning in the late 1970s, scholars of Asian development challenged their colleagues in the fields of economics, political science, and sociology to move beyond the long-standing dichotomy between a neoclassical free market and a centrally planned economy in their study of economic development. Chalmers Johnson, for example, was particularly forceful when arguing that Japan’s economic growth fit into neither category.\(^2\) And as Stephan Haggard has written, “Spearheaded by scholars outside the mainstream of North American economics, this work began by underlining empirical anomalies: the myriad ways in

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which the East Asian cases failed to conform to the neoclassical view.” In the 1980s, as Japan’s economic rise continued, and South Korea and Taiwan became successful developers (i.e., newly industrialized countries, or NICs), the debate over the explanation for their success intensified.

The dependent variable in all three cases was startlingly clear: Each was experiencing economic development that was historically unprecedented by world standards in both its pace and its depth. The issue was how to explain this growth. The debate began by focusing on whether state intervention was central to the NICs’ economic success—the “state versus the market” debate. In surprisingly little time, it became obvious that the common variable was extensive government intervention into the market. This finding made clear the need to recast the standard debate between the virtues of a neoclassical free market versus a centrally planned economy.

Twenty years later, the study of Asian development and the high theoretical standards that this scholarship has established have forced scholars to face a myriad of new ideas and issues. The concept of a “developmental state” has become part of the canon in political economy. New developments in microeconomics, sociology, and anthropology have underscored the role of institutions in East Asia’s economic performance. Scholars no longer view markets as the frictionless intersection of supply and demand curves. Instead markets are being reinterpreted as complexes of principal-agent relationships in which problems of imperfect and asymmetric information, contracting, and credibil-


It is ubiquitous. The smooth functioning of markets requires more than getting policies, incentives, and prices right. Also needed are public and private institutions that facilitate market exchange—from the legal system and a clear delineation of property rights, to the public provision of information, to informal institutions that build trust. Scholars continue to probe the relationship between development and politics, corruption, the international system, and the role of history. As Haggard writes, “In the 1990s, intellectual developments . . . provided earlier insights on government intervention with microfoundations that made them legitimate to the economics profession.”

The continuing debate over Asian development has generated more than its share of controversy. Almost nowhere in this debate, however, do arguments about whether or not Asia is “exceptional” play a role, and most scholars take Asia’s empirical realities on their own terms. They pay close attention to measuring the independent and dependent variables, and they are open to the potential ramifications of their findings for social science theory. Exploring how institutions affect markets, and in particular the impact of Asian governments and the organization of Asian business on economic growth, is an ongoing process that involves careful attention to both theory and evidence. By comparison, the study of Asian international relations is still in its initial stages. But if scholars in the field of political economy can do it, so too can scholars in the field of international relations.


Hierarchy and Balancing in International Relations

In my article, I question whether Asian international relations will be hierarchic or whether a regional balance of power is more likely. In addition, I criticize scholars who automatically assume that balancing will result. Scholars have no reason to think that balancing behavior is homogeneously distributed across regions, and that because it occurred in Europe, it will in Asia as well. The issue for Asian international relations is not a theoretical one about whether hierarchy could exist, but rather an empirical one: Does the evidence show balancing or bandwagoning behavior in Asia?

Acharya writes that “Kang’s notion of hierarchy is not grounded in the available theoretical literature” (p. 154). In making this claim, however, Acharya overlooks at least two well-established schools that examine various forms of hierarchy and bandwagoning. The hegemonic stability school, which emphasizes the beneficial impact that a dominant power can have on less powerful states, is a prime example. Another is the preponderance of power school, which argues that an unequal distribution of power in the international system is more stable than an equal distribution of power. In addition, some scholars have explored ways in which the United States can restrain its own overweening power to “mitigate fears of domination and abandonment” among secondary states in the system. In contrast, Acharya falls into the trap of assuming that balancing is the default hypothesis in international


relations theory. However, as Robert Powell concludes, “Whether states balance, bandwagon, or stand aside while others fight depends in a complicated way on many different factors. . . . when these factors are taken into account, states usually bandwagon.”

The balancing proposition that grows out of this literature—Kenneth Waltz’s confident assertion that “hegemony leads to balance” and has done so “through all of the centuries we can contemplate”—is alive and well. And as Stuart Kaufman and William Wohlforth note, “Criticism of Waltz concerns mainly his theoretical explanation for recurrent balances, not the phenomenon itself.” This essay is too short for a full theoretical elaboration of hierarchy and how it existed in the historical Asian context. Instead, the remainder of this section aims to show that the theoretical edifice constructed by Waltz is badly in need of repair.

In *Theory of International Politics*, Waltz claims that anarchic and hierarchic orders are two ends of a spectrum and that international relations is anarchic. Hence, Waltz defines hierarchy as the opposite of anarchy; in this world, hierarchy and anarchy cannot coexist. The dominant prediction that arises from this formulation is that balances occur. Further, Waltz restricts his locus of inquiry to the great powers, defining away the tremendous diversity that exists within the international system and thus making his theory fit his evidence.

In recent years, however, some international relations scholars have increasingly begun to challenge the balancing hypothesis. If twenty years ago the conventional wisdom was that balancing was a universal law of international relations, there is now considerable evidence from outside the European context—including ancient Assyria, medieval Asia, India, and Latin America—that in systems consisting of one major power, the secondary states often do not balance against it. In one recent project, for example, scholars found that stable hierarchies are at least as common as balancing or empire. And as Wohlforth and Kaufman point out, “Core propositions from many theories concerning balance and hierarchy fall flat when confronted with evidence from

systems other than those comprised of the European states and their contemporary descendents.”

19 Scholars are beginning to explore the theoretical underpinnings of international systems much more broadly than before.

In developing the balance of power thesis, Waltz understandably focused on “a few big things,” and in particular on the bipolar confrontation between the United States and the Soviet Union and the potential for nuclear holocaust. At the time, Waltz argued, “The theory of international politics is written in terms of the great powers of an era. It would be . . . ridiculous to construct a theory of international politics based on Malaysia and Costa Rica . . . A general theory of international politics is necessarily based on the great powers.”21 Even within a Waltzian world, however, small and medium powers do exist. This is not to argue for inclusion into his theory of variables such as ideology, nonstate actors, or international institutions, but rather to acknowledge that in accepting the nation-state as the unit of analysis, one must allow for the role of small nation-states. For Waltz, the great powers are all that matter, so again he has chosen a set of cases that fit his theory.

In explaining his dependent variable—Cold War stability between two nuclear superpowers—Waltz was correct to restrict his focus to the great powers. Small powers did not matter in the global struggle between the United States and the Soviet Union. A theory designed to explain the Cold War, however, may not explain why Asian states are not necessarily balancing China in the same way that the United States balanced the Soviet Union. Because the world is no longer made up of two superpowers and all the rest, scholars who want to explore other international systems or alternative reasons for state behavior need to move away from Waltz’s truncated definition of which countries matter and how anarchy interacts with hierarchy. If Thailand can start a global eco-

nomic crisis, and if war in Afghanistan or Taiwan could have a direct impact on the United States, perhaps we should consider incorporating such countries and situations into our theories.

Indeed, even Waltz allows for the possibility that balancing may not occur, although he merely asserts this, rather than providing a theory to help explain it. In *Theory of International Politics*, Waltz’s escape clause was to argue that “secondary states, if they are free to choose, flock to the weaker side.”22 But this admission is hardly sufficient. Nor is his implicit dismissal of hierarchy acceptable. Is Waltz suggesting that in a system with one dominant state, secondary states without a balancing option have no choice but to acquiesce and accommodate? Is this not hierarchy?

Sometimes states bandwagon. The issue is not nation-states themselves, but rather the international distribution of power and capabilities. Already scholars know that small powers do not necessarily balance. If there is one dominant power, even other great powers may not balance, seeing it in their interests to accommodate the status quo.23 Bandwagoning—or at least acquiescence to the status quo—by secondary states is a central feature of hierarchy.24 In contrast to realist predictions that secondary states will be fearful of and balance against the dominant state, in hierarchy the secondary states flock to its side with a view toward gaining benefits.25 This behavior is consistent with Randall Schweller’s distinction between balancing for security and bandwagoning for profit.26

One response from realists is that differential power does not constitute a hierarchy and that the existence of balancing or bandwagoning behavior proves nothing.27 They argue that just because some states are weaker does not mean that they will not strive to maintain their independence, which the great pow-

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22. Ibid., p. 127.
24. A challenge for international relations scholars is to define more clearly our basic theoretical concepts, such as bandwagoning, balancing, engagement, containment, hegemony, accommodation, hiding, and hedging.
27. According to Waltz, “State actions are not determined by structure. . . . Because states coexist in a self-help system, they are free to do any fool thing they care to, but they are likely to be rewarded for behavior that is responsive to structural pressures and punished for behavior that is not.” Waltz, “Evaluating Theories,” *American Political Science Review*, Vol. 91, No. 4 (December 1997), p. 915.
ers are largely able to achieve. If realism cannot predict state behavior, then realists ought to admit as much. Instead, they continue to predict overwhelmingly that states will balance in the face of predominant power. On the other hand, if balancing and bandwagoning are not predictions that derive from a Waltzian approach, then that only buttresses my point that scholars need to be more careful in explaining Asian state behavior.

Scholarship that ignores Asian states’ history and the role of preferences in favor of a purely structural formulation of international relations also ignores many of the theoretical advances of the past decade by individuals such as Robert Powell and James Fearon, among others. The most sophisticated theoretical treatments of deterrence, spiral models, and power transitions contend that understanding preferences is vital for drawing any conclusions about state behavior. As Haggard notes, “In the absence of information on actors’ preferences or a clear sense of the nature of the strategic interaction in question, we are unlikely to generate defensible expectations about state behavior or the propensity for conflict.”

In sum, the notion of hierarchy is well established in the international relations literature, and balancing should not be the default hypothesis in international relations theory. Balancing is the expected outcome under certain conditions (i.e., when there is a small number of great powers). Hierarchy and bandwagoning are the expected outcomes when one state is dominant in the system. The question then is, What is happening in Asia?

**Empirical Anomalies and Historical Asia**

In addition to opening theoretical space for consideration of alternative explanations of Asian international relations, scholars should consider more carefully the empirical record of Asian states, including both the historical origins of the Asian international system and present-day empirical anomalies. One can list dozens of books in the political science mainstream literature that deal

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30. This section is drawn from David C. Kang, “Hierarchy in Asian International Relations: 1300–1900,” paper prepared for the conference “Hierarchy and Balancing in Ancient Systems.”
with pre-1945 Europe.\textsuperscript{31} In contrast, there are only two widely read works of political science that deal with pre-1945 Asia.\textsuperscript{32}

The field of international relations tends to treat the contemporary Asian system as if it emerged fully formed from nothingness in the post–World War II, postcolonial era. But many Asian countries have been geographically defined, centrally administered states for far longer than those in Europe. To ignore the evolution of these states is at best an oversight; at worst, it reveals an unwillingness to engage Asia directly. This is especially puzzling given the huge amount of attention that international relations scholars have paid to the historical roots of the European system. If such scholars were dismissive of all history, at least that would be consistent. But ignoring Asian history while studying European history in essence biases their conclusions in favor of the European experience.

Acharya’s article critiques my brief overview of Asian history in “Getting Asia Wrong,” yet his discussion of Asia’s historical record relies almost exclusively on a 1968 volume edited by John Fairbank.\textsuperscript{33} There has been a tremendous amount of historical scholarship in the intervening thirty-five years.\textsuperscript{34} Below I briefly expand my argument to consider (1) whether other states accommodated to China in the past, and (2) the existence of hierarchy in Asian international relations historically.

First, accommodation of China was the norm in East Asia during the Ming (1368–1644) and Qing (1644–1911) eras. This did not, however, involve a significant loss of national independence, as nearby states were largely free to conduct their domestic and foreign policy independent of China. Regarding the Vietnamese Le dynasty (1427–1787), for example, David Marr writes, “This


\textsuperscript{33} Fairbank, The Chinese World Order.

\textsuperscript{34} Much of this literature is cited in my book-length manuscript. David C. Kang, “Hierarchy, Alliances, and Stability in Asia,” Dartmouth College, 2003.
reality [China’s overwhelming size], together with sincere cultural admiration, led Vietnam’s rulers to accept the tributary system. Providing China did not meddle in Vietnam’s internal affairs. . . . Vietnamese monarchs were quite willing to declare themselves vassals of the Celestial Emperor. The subtlety of this relationship was evident from the way in which Vietnamese monarchs styled themselves ‘king’ (vương) when communicating with China’s rulers, but ‘emperor’ (hoàng de) when addressing their own subjects or sending messages to other Southeast Asian rulers.”

Japan also worked within the Chinese-dominated international system. To eliminate the insecurity caused by fear of a Chinese invasion, the Ashikaga Shogunate (1333–1573) sought investiture by the Ming emperor. Kawazoe Shoji writes, “Japan had to become part of the Ming tribute system and thus cease to be the ‘orphan’ of East Asia. For centuries the Japanese had feared attack by the Silla (Korea), and the Mongol invasions had provided real grounds for fearing a Ming attack.” Even the Tokugawa shogunate (1600–1868) recognized China’s centrality and Japanese-Korean relations as equal. According to Key-hiuk Kim, “The Tokugawa rulers tacitly acknowledged Chinese supremacy and cultural leadership in the East Asian world. . . . though Tokugawa Japan maintained no formal ties with China. . . . for all intents and purposes it was as much a part of the Chinese world as Ashikaga Japan had been.”

Thus, Asian states of varying size and technological capability existed in an international system based on rules and norms that revolved around China. From Japan to Siam, and for more than six centuries, this system functioned in essentially the same manner.

Second, although economic relations in historical Asia were as vibrant as those in Europe, conflict was notably rare. Centuries separated major interstate conflict in Asia, which tended to occur when order within the central power had begun to break down. As Chinese dynasties began to decay, conflict along and among the peripheral states would flare up, as the central power turned its attention inward. Thus in 1274 and 1281, as the Sung and Chin dynasties were crumbling, the Mongols under Kublai Khan tried unsuccessfully to conquer Korea and Japan. Centuries later, as the Ming dynasty began to weaken,
the Japanese general Hideyoshi twice attempted to invade China through Korea (in 1592 and 1598). With the restoration of order in China, however, conflict among the peripheral states ceased, and intraregional relations remained relatively peaceful for several hundred years. The dominant power had no need to fight, and the secondary powers had no desire to fight.

This is not to say that conflict in Asia was totally absent, but rather that interstate war was much less common than it was in Europe. Pirates, nomads, and other nonstate actors existed in the historical Asian system, just as Barbary Coast pirates and similarly powerful actors could be found in Europe. The Chinese, for instance, engaged in long-running border battles with the Mongols to the north, at times employing as many as 500,000 troops in an effort to secure this front.39

That Asian international relations do not conform to the classical European balancing model has been empirically validated by research that examines the origins of war over the past 150 years. Scott Bennett and Allan Stam subjected the European model to empirical testing across regions and time and found that although it works well in Europe, “significant differences in preferences for conflict exist across regions.” They also found “no support for the argument that [Asian] behavior will converge on that of Europe. In fact, all of the regions outside of Europe appear to diverge from the European pattern [of classical balance of power].”40

There are at least six empirical anomalies in contemporary Asian international relations that realist interpretations cannot explain. First, the main empirical anomaly, and the main problem with a theoretical view based on realism, is the focus of attention on the most powerful countries. For Asia, the biggest threats arise not from the most powerful country (the United States) or even the second most powerful country (Japan), but rather from the region’s smallest and weakest states (Taiwan and North Korea, respectively).41 This anomaly cannot be explained without first understanding these states’ interests and the nature of their interactions with other countries.42 Writing about different behavior across regions, Bennett and Stam note “It is not that the

actors are not rational, even though a universal model may fail. Rather, they
simply are not playing the same game with the same preferences.”

A second empirical anomaly concerns the thorny issue of Taiwanese sovereign-
ty.

Taiwan is not recognized as a sovereign state, yet many international
relations scholars treat it like one because it acts like one. This not only does
the field of international relations a disservice, but it is also logically inconsis-
tent with the Westphalian view that formal recognition is paramount. Al-
though Acharya argues that China uses Westphalian concepts, Chinese
scholars point out that when discussing Taiwan, Chinese know exactly when
they want to use English words and meanings and when they want to use Chi-
inese words and meanings, and so do the Taiwanese. Scholars need to con-
front such realities, especially because of their such ramifications for both
China and Taiwan.

A third anomaly is the remarkable staying power of Asia’s three Leninist
states: China, Vietnam, and North Korea have survived despite the collapse of
the European communist bloc more than a decade ago. Although China and
Vietnam (and, to a lesser extent, North Korea) have engaged in some economic
reforms, they remain authoritarian political regimes. It also bears mention
that all three are products of anti-Western, anticolonial movements. North Ko-
rea, in particular, has survived much longer than almost anyone predicted.
Although minuscule compared with any of its neighbors, North Korea is the
country most likely to be at the center of conflict in Northeast Asia.

44. Aihwa Ong, “The Chinese Axis: Zoning Technologies and the Logic of Exception in Variegated
Sovereignty,” paper prepared for the conference “Peace, Development, and Regionalization in East
Asia”; and Andreas Osiander, “Sovereignty, International Relations, and the Westphalian Myth,”
45. Personal communication from Peter Katzenstein, September 29, 2003. See also Yun-han Chu,
“Taiwan’s Security Dilemma: Military Rivalry, Economic Dependence, and the Struggle over Na-
tional Identity,” paper prepared for the conference “Peace, Development, and Regionalization in East Asia.”
46. Shelley Rigger, “Competing Conceptions of Taiwan’s Identity,” in Suisheng Zhao, ed., Across
the Taiwan Strait: Mainland China, Taiwan, and the 1995–1996 Crisis (New York: Routledge, 1997);
Christopher Hughes, Taiwan and Chinese Nationalism: National Identity and Status in International So-
ciety (New York: Routledge, 1997); and Gary Klintworth, New Taiwan, New China: Taiwan’s Changing
Role in the Asia-Pacific Region (New York: St. Martin’s, 1995).
47. On North Korean economic reforms, see David C. Kang, “The Avoidable Crisis in North Ko-
48. Several years ago, I predicted that North Korea would survive into the foreseeable future. Da-
vid C. Kang, “Rolling with the Punches: North Korea and Cuba during the 1980s,” Journal of East
49. Victor D. Cha and David C. Kang, Nuclear North Korea: A Debate on Engagement Strategies (New
A fourth anomaly concerns the attitude of South Korea and Japan to the Taiwan-China conflict. A realist would argue that both countries should have much to fear from an aggressive China, and hence they should be eager to help the United States and Taiwan contain it, either through more active measures today or through promises to come to Taiwan’s aid in the event of a Chinese attack. A liberal would assert that, as democracies, South Korea and Japan should be eager to defend democratic Taiwan against authoritarian China. Yet because of their perception of the Taiwan-China issue as more of an internal than an international matter, both countries have shown a reluctance to get involved.

A fifth anomaly involves the ejection of U.S. bases from the Philippines after the Cold War. Given the tremendous security benefits that the Philippines enjoys as a member of the U.S. alliance system in Asia, why would it take such a seemingly self-defeating action? The standard realist explanation is that it reflected a surge in Filipino nationalism—an explanation that seems rather exceptionalist. As Yuen Foong Khong writes, “By 1989 it became obvious that the negotiations had become entangled with a fierce domestic political debate within the Philippines. The surge in Filipino nationalism derailed the negotiations.” Realists, however, cannot so easily attribute the ouster of the U.S. bases to domestic politics. A more likely explanation is that the Philippines does not view China as the threat that realists believe it should.

Sixth, despite seemingly every reason to be fully incorporated into the U.S. alliance system, South Korea clearly has a different perspective on the role of the United States in Northeast Asia. The idea that Seoul might not want to continue its close alliance with Washington was unthinkable even two years ago. But a resurgent Left in South Korea, combined with worries that the United States—not North Korea—is the destabilizing force in the the region has led many in South Korea to view the U.S. presence with some alarm. This

50. Daniel Okimoto writes, “[The U.S. alliances in Asia have] withstood the test of time, lasting for longer than a half-century; they have also functioned effectively to deter blackmail, coercion, conflict, and war.” Okimoto, “KASA and JASA: Twin Pillars of Asia’s Security Architecture,” paper presented at the conference “Peace, Development, and Regionalization in East Asia.”


has caused much consternation in Washington, which is beginning to take the threat to the alliance more seriously. Chung-min Lee writes, "For the first time since the bilateral alliance [with the United States] was forged more than a half-century ago, more Koreans are at least entertaining the specter of closer political, security, and economic ties with China." There are deep divisions in South Korea concerning the utility of a continued alliance with the United States, U.S. policy toward North Korea, and South Korea’s relations with the other powers in the region. Although differences over how to deal with North Korea are nothing new, in the past these differences were often tactical, resolved in large part because of the common perception that North Korea represented a serious security threat. In recent years, however, South Korean and U.S. security perceptions have begun to significantly diverge.

Instead of addressing such anomalies, Acharya chose to emphasize two other arguments: first, that India is a significant actor in Asian international relations and, second, that norms matter in explaining these relations. Neither argument is sustainable. Although India certainly is an important actor in South Asia, the fact that China extends into two regions does not mean that the two regions are the same. This would be similar to arguing that because the United States is involved in Asia and Europe, both of those regions are the same. India may matter to South Asia, but it does not figure in East Asian security issues such as North Korea and Taiwan, or even in Philippine security decisionmaking.

In addition, the norms and institutions argument has little empirical validity. One recurrent finding is the disparity in attitudes and beliefs on these subjects within South Korea, Japan, Thailand, the Philippines, and Indonesia. These countries are certainly modern, but a common desire for a security community is far from a reality. Norms that might influence Asian state behavior are startlingly absent, and here Aaron Friedberg’s original argument is true: Given their vast disparities in wealth, political development, ethnicity, religion, and language, states in East Asia do not share a common bond, beyond the desire to be wealthy and secure. Institutions such as the Association of Southeast


Asian Nations and the ASEAN Regional Forum remain peripheral to the conduct of Asian international relations. That these Asian states may have modern aspirations is one thing; to argue that they share deeply held norms and a desire for more regional institutions is another.

Conclusion

Amitav Acharya has made a number of important points regarding the study of Asian international relations. In addition to focusing on standard realist concerns such as material capabilities, international relations scholars need to give greater consideration to the role of institutions, ideas, and history in Asia, as well as to definitions of the Asian region and its subsystems. The key question is whether balancing or bandwagoning best characterizes contemporary Asian relations. The evidence, although mixed, shows that Asian nations do not feel particularly threatened by any country and hence are not balancing China. Close examination of mainstream realist theoretical approaches to Asian international relations reveals a number of puzzles, and scholars of Asian international relations need to pay more attention to the empirical record, both historical and modern. My goal is not to replace one set of unquestioned assertions with another, but rather to open up the field for continued progress.