ences. It is easy to demonstrate that the (war) equilibrium of the crisis subgame is extremely sensitive to this asymmetry. (If the challenger's preferences are reversed and brought in line with the defender's, negotiations are implied; and if the defender's preferences are altered, it should capitulate. In neither instance does war remain rational.) Thus, while the empirical findings should still be interpreted as corroborating evidence for the theoretical structure the authors advance, their conclusion about the unlikely possibility of cooperation in prisoners' dilemma games is open to debate. In fact, just the opposite conclusion can be drawn from the discussion of the "self-defense" norm which, like the standard rendition of prisoners' dilemma, is defined in terms of each player's preference for conflict (i.e., "punishment") over capitulation (i.e., the "sucker's" payoff). As the authors show, when information is complete, "if both parties to a conflict of interest are known to be prepared to retaliate if attacked, then cooperation or harmony is guaranteed" (p. 124).

The theory's organizing power is demonstrated in an enlightening explanation of why democracies, as warprone as any other type of state, rarely fight one another. Perhaps even more instructive is the empirical comparison of the game model with balance of power and power preponderance theories. (The hegemonic theories fare rather well; balance of power does not.) At times, however, the interpretation of aspects of these theories is puzzling. For example, Bueno de Mesquita and Lalman claim that preponderance theorists assert that system transforming wars must be large wars. A chapter length discussion of the Seven Weeks War, which is fascinating in its own right, is even devoted to illustrating the proposition that small wars may have major consequences. But the underlying claim seems strained. After all, power transition theory (Organski and Kugler, *The War Ledger*, 1980) admits the possibility of peaceful transitions. Why then should the authors be surprised that a transition war, especially an intracoalition war, be limited? This objection aside, the game model's ability to more fully account for the outbreak of war than either power transition or balance of power mark it as a major theoretical advance.

What place will *War and Reason* hold in the scientific literature of interstate relations? Using Lakatos' criteria, the game model is shown to be superior to the decision-theoretic framework articulated in Bueno de Mesquita's *The War Trap* (1981). It is clearly the better book. Nevertheless, it is unlikely to have the same dramatic impact, simply because *the War Trap* came first. *War and Reason* will probably be thought of as a significant addition to a rapidly growing literature that employs axiomatic theory to understand international conflict and its resolution. It may even come to be seen as *primus inter pares*. But its influence will likely be modulated because the rational choice paradigm, now more than ever, constitutes a progressive research program with many of the characteristics of a maturing science. Under such conditions, revolutionary contributions are most improbable.

Still, *War and Reason* is an innovative and far-ranging work that redefines the state of the art and extends our knowledge about interstate conflict and cooperation. It is the most significant application to date of game-theory to the question of war and peace. In a very real sense, it actualizes the considerable potential of this methodology, not only to highlight fundamental questions about international politics, but to probe deeply into their solution.

State University of New York, Buffalo FRANK C. ZAGARE

Rival Capitalists: International Competitiveness in the United States, Japan, and Western Europe. By Jeffrey A. Hart. Ithaca: Cornell University Press, 1992. 305p. $32.95 cloth, $16.95 paper.

Rival States, Rival Firms: Competition for World Market Shares. By John M. Stopford and Susan Strange, with John S. Henley. New York: Cambridge University Press, 1991. 321p. $59.95 cloth, $18.95 paper.

In these two new books, Jeffrey Hart, John Stopford, and Susan Strange carry the argument about "competitiveness" further—though the Stopford–Strange volume is as concerned with the competitiveness of firms as it is of nations. The two works draw their evidence from varying national contexts: Japan, the United States, Britain, France, and Germany (Hart) and Kenya, Brazil, and Malaysia (Stopford and Strange). Given their different centers of attention, the two books reach proportionately different conclusions. From a developing-country perspective, Stopford and Strange see states as weak, multinationals as strong. The question for them is how a relatively poor country can induce reluctant multinationals to invest within its territorial confines, bringing in new capital and technology. Hart's argument is more traditional: How do strong countries and governments improve the productive efficiency of their societies as a whole? Both, however, have to confront the problem that (with certain key exceptions) the bargaining power of governments as heads of national society has declined in comparison to that of large international corporations.

Hart modifies the preexisting wisdom by contending that the particular content and direction of state–society relationships determine the outcome in international competitiveness. Generally speaking, German success stems from the labor–business partnership; the Japanese miracle is due to the government–business link. Less successful are the pure (unalloyed) types, like the preponderant labor orientation in Britain, state dominance in France, and business precedence in the United States. If this conclusion is correct, the United States should modify its free-market orientation and move toward a mixed relationship of business and government, while France should dilute its statist policies in a new partnership with industry. Britain also needs to shift either toward government or business to improve its competitive performance.

Both books recognize that technology, capital, and a trained labor force are critical to winning the race, at either firm or government levels. Both understand that one has to negotiate with other states, as well as foreign firms, if success is to be achieved. But neither book quite tells one how to do it. For example, should a government seek to create a free-trade area, favoring firms that operate within the bloc? Should it sanction *keiretsu* arrangements for its own firms, so that companies within an industrial group can gain preferred access to capital and technology, strengthening them against foreign competition? How should a government use its administrative and regulatory powers to protect the position of its own industries? In influencing foreign treatment of its exports and multinational corporations, should a government engage in specific, or only in diffuse, reciprocity with foreign nations?

Stopford and Strange rightly conclude that unfettered markets do not necessarily produce the results one wants, as witness the comparative decline of both Britain and the United States, the most liberal and open of modern economies. Hart agrees and observes that "corporatism," "culture," "statism," and "coalitional" explanations also fail the test, because, as independent variables, they do not vary with change in the dependent variable (competitive success). But Hart's own state–society explanation is vulnerable on the same grounds. Underneath the general social formulas, there must be other specific causes that are being neglected in both analyses.

In fact, the world economy today is still largely politically dominated and controlled, not by a hegemonic power but by an oligopoly of states. Each member needs to maintain its market share, but none wants to risk a game of ruin with others—depressing the world price and threatening its own productive future. Thus, each is forced to respond to others when trade or financial flows become too unbalanced. In this sense, losers are occasionally compensated by winners and helped to regain their footing. Russia is, or soon will be, the beneficiary of others' indulgence. And before long, the American turn will come; for the United States boasts the largest trade imbalance and the highest degree of indebtedness of any country. The United States will demand, and probably receive, special treatment, contingent upon its own domestic economic reform. As Thomas Schelling says, in bargaining, weakness is strength. Both books unfortunately neglect such bargaining perspectives, though such strategies (hegemonic, free-rider, or reciprocator) appear to vary with the outcomes in national prosperity and competitiveness that they both depict.

This, of course, does not entirely solve the problem, because governments (with the possible exception of the government of Japan) cannot exert a decisive influence upon their own firms. And even in Japan, *keiretsu* independence seems to be growing. Why is this? And is there anything that can be done about it? The present weakness of governmental policy is probably the effect of a technology that puts more and more emphasis upon factors of production that governments cannot fully control. The most firmly fixed factors of production (which are amenable to national fiat) are no longer the most economically important. *Land*, for example, is no longer a decisive ingredient in the most productive value-added technology; and the possession of more land is no great advantage. Thus, governments' monopoly of territory serves them ill. Governments can influence, to a degree, *specific capital*—the industrial assets that are placed within national confines. But these, as Stopford and Strange point out, can be shifted elsewhere if financial incentives so dictate. Even more significant *mobile capital* can shift like quicksilver from country to country. When Saddam Hussein occupied Kuwait City, he found the cupboard bare and the capital flown. *Labor*, of course, is likely to be more stationary and therefore more amenable to governmental control. But in recent years, it has become more mobile, though still not as fluid or fungible as capital. Still, after World War II, the Soviets commanded their labor force with rigorous political controls. Vietnam could not do so, however; and the most economically relevant population, the ethnic Chinese–Vietnamese, went elsewhere after the war ended. In fact, though Japan has not yet fully understood the phenomenon, a country that can induce the most highly trained and industrious labor force to visit its shores has a mighty advantage in world economic politics. Countries today, however, face brain drains, as well as inflows.

The insight that unites the two books is that states cannot guarantee their own economic future. They must negotiate to ensure themselves economically and deal with foreign corporations, as well as foreign governments. As the important factors of production that one wishes to acquire become more *mobile*, this task becomes more difficult. And in the twenty-first century, it will become more complicated still, as *information* becomes the needed desideratum. Service industries that provide information are the most mobile of all, yet they are probably the most critical to future success. That is why the issue is still up in the air and why neither book can hazard a guess on the evolution of competitive development. States have become much more like firms, and the contours of the international political market have yet to be drawn. A good guide to success, however, is to develop the most highly trained and industrious labor force, ease the entry of foreign capital, and assist the development of new technology through government-assisted research and development. The states that succeed will do so because of the success of their firms in international competition. Rival states depend in part on allied firms.

University of California, Los Angeles RICHARD ROSECRANCE

Analogies at War: Korea, Munich, Dien Bien Phu, and the Vietnam Decisions of 1965. By Yuen Foong Khong. Princeton: Princeton University Press, 1992. 286p. $39.50 cloth, $17.95 paper.

Diffidence and Ambition: The Intellectual Sources of U.S. Foreign Policy. By Carlo Maria Santoro. Boulder: Westview, 1992. 316p. $57.50.

These two otherwise dissimilar books are part of a growing literature exploring the role of "ideas" in shaping government policy. Santoro posits that national security and interdependence—the principles that guided and legitimized U.S. Cold War policy—were developed in 1940–41 before the Japanese attack on Pearl Harbor, when the United States was pursuing a policy of neutrality. The purpose of his book is to trace the evolution of conceptual precursors through the Council on Foreign Relations' (CFR's) War and Peace Studies Project, which was begun in 1939 as a service to the State Department. Khong contends that the Johnson administration's perception that the Vietnam conflict was analogous to the Korean War led them to intervene in Vietnam in 1964–65 and to do so with less force than officials believed was necessary to accomplish American objectives. His vivid memory of MacArthur's disastrous march to the Yalu caused President Lyndon B. Johnson to reject such options as bombing petroleum and lubricants in North Vietnam or mining Haiphong Harbor. In short, Khong maintains that the inferences they drew from the Korean analogy explain not only *why* the Johnson administration escalated the Vietnam War but *how*.

A central problem with previous studies in this genre is that they fail to uncover the mechanisms by which ideas are translated into action. It is one thing to show