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Nations and Their Relationships

Comparative Politics

Ronald Rogowski

Comparative politics in the 1980s was characterized chiefly by five interrelated trends.¹ In approximate order of importance, these were:

(1) A far greater attention to the economic aspects of politics, including the ways in which governmental policy affects economic growth or decline (Bates 1981; Zysman 1983; Olson 1982; North 1981; Katzenstein 1985),² the structural constraints on economic policy (Hall 1986), and the effects of economic hardship on political cleavages and coalitions (Gourevitch 1986).

(2) Increased interest in the international context of domestic politics and institutions, encompassing new explanations, not only in the perhaps obvious domain of trade policy (e.g., Milner 1988), but in governmental growth (Cameron 1978), social revolutions (Skocpol 1979), political cleavages (Gourevitch 1986; Rogowski 1989; Frieden 1991a), and forms and styles of governance (Katzenstein 1985).³

(3) An altered and sharpened focus on interest groups, particularly in the context of various kinds of corporatism (Berger 1981; Schmitter 1981; Olson 1982; Katzenstein 1985).

(4) A revival of interest in state structures and their performance (Evans et al. 1985; Powell 1982; Lijphart 1990), treated perhaps most intriguingly as a trade-off between "markets" and "hierarchy" (Williamson 1975, 1985). For obvious reasons, this line of work has assumed new importance with the collapse of the centrally planned economies.

(5) Further work on nationalism and ethnic cleavages (Laitin 1986; Horowitz 1985).

I shall focus on each of these topics in turn, endeavoring at the end to draw the strands together and to speculate about the directions of future work.

Economics and Politics: The "New Political Economy"

The oil price shock of 1973 threw into doubt the comfortable postwar assumption that, at least in the

industrialized West, ever-increasing prosperity would bring an "end of ideology" and convergence on a single liberal model of governance. The ensuing deep recession evoked widely varying governmental responses and highlighted longer-term differences in economic performance. Among the economically advanced nations, the continuing Japanese "miracle" and the quite respectable growth of the continental European economies contrasted sharply with the dismal record of the U.S. and U.K. and the increasingly evident catastrophe of the Soviet Union and Eastern Europe.⁴ For the market economies, the simple statistics of average annual growth in real GDP per capita, both for the whole interval 1960 to 1980 and for the years after the oil price shock, spoke volumes (OECD 1982):

Average Annual Growth in Real GDP Per Capita (%)

	1960-80	1973-80
Japan	6.5	2.7
France	3.8	2.4
Italy	3.7	2.2
Germany	3.2	2.4
U.S.	2.3	1.3
U.K.	2.0	0.9

While official statistics for the USSR, based as they were on "Net Material Product," non-market prices, and propagandistic exaggeration, were not comparable, even they showed an annual total (not per capita) growth rate for the early 1980s of only about 3% (International Monetary Fund 1991, 3); and, by some informed estimates, real per capita Soviet output in this period grew by less than 0.5% annually⁵ -- despite the great benefits that the USSR, as a major exporter of petroleum, drew from the post-1973 price increases.

Among the less developed economies, the differences were even more stark. Africa had experienced declining growth of per capita output since 1960 and, after 1980, actual reductions;⁶ much of the remaining Third World had stagnated economically;⁷ but a few dramatic exceptions -- principally Taiwan, South

Korea, Hong Kong, and Singapore -- had grown so rapidly as to have defined a new category of "newly industrialized countries," or NICs (Haggard 1990).⁸

These disparities in economic growth had obvious implications for national prestige and power and for the survival of individual leaders. Research on voting increasingly generalized to the other Western democracies the U.S. finding that national election outcomes hinged on economic performance;⁹ and prolonged failure in the economic realm destabilized governments of every stripe. Understandably, comparativists began to focus on such questions as: What explained the successes? What tensions and cleavages were unleashed by failure? How, and in what circumstances, had previously damaged economies managed to turn themselves around?

Governmental Policy

In a first subcategory of work, such scholars as Robert Bates, Peter Katzenstein, John Zysman, and Douglass North emphasized the ways in which governmental policy affected economic growth. Bates's (1981) pathbreaking work on what, to the ill-informed, would have seemed a soporific topic, namely post-independence African agricultural policy, demonstrated conclusively, if sometimes obliquely (a) that the African economic disaster was self-inflicted and (b) that, in general, policies either of Soviet-style rapid industrialization at the expense of the countryside or of more moderate import-substituting industrialization (ISI) were doomed.¹⁰ With but two significant exceptions (Côte d'Ivoire and Kenya), the independent governments of land-rich Africa had ruthlessly taxed domestic farmers to gain the investment capital that would supposedly permit rapid industrialization; and they had attempted to stimulate industrial growth through tariffs, quotas, and an overvalued currency. Instead, these policies had yielded -- in ways that Bates showed to have been absolutely predictable from the most rudimentary economics -- black markets, smuggling, declines in agricultural production, flight from the land, monopolistic and uncompetitive industries, massive corruption, and an increasing reliance on imported foodstuffs.

An early side effect of Bates's crucial book was near-total abandonment of two important streams of previous theorizing about development, namely (a) culturalism and (b) the "dependency" and "world-systems" analyses. On the one hand Lipset (1963, chap. 3), McClelland (1961), and others working in the Parsonian tradition had sought to link long-term economic performance to cultures that valued, and role-structures that rewarded, individual achievement. Against the weight of tradition, they contended, governmental policy could achieve little. On the other hand, Cardoso and

Faletto (1979), Gunder Frank (1967), I. Wallerstein (1974), and others had asserted in essence that the causes of particular regions' chronic underdevelopment lay outside themselves: under capitalist exchange, "core" economies could prosper only to the extent that "peripheral" ones yielded up primary goods at starvation wages; the "dependence" of the latter was thus "structural" and could be alleviated only by rapid autarkical industrialization or (as Wallerstein argued most strongly) by a socialist world revolution. Against the culturalists, Bates seemed to show that policy mattered enormously and was not simply an artifact of culture.¹¹ Against the dependency theorists, he demonstrated the utter folly of most efforts at autarkical industrialization.¹²

To James Buchanan (1980), Mancur Olson, Jr. (1982), and other comparative economists, what Bates described was but one manifestation of a far more general phenomenon first identified by Anne O. Krueger (1974), namely governmental policies that awarded (and thus encouraged the pursuit of) what economists call *rents*.¹³ Olson was particularly forceful in contrasting such *redistributive* policies (which sought merely to reallocate slices of a fixed "pie") with *productive* ones (which sought to enlarge it).

The award of rents via discretionary government control of scarce resources, according to these analysts, reduces social output in at least two ways. Most obviously, by distorting incentives it redirects some factors into less productive uses. Krueger argued, however, that the *pursuit* of rents might harm society more: if government can grant rents, private actors will expend resources trying to influence government -- again, to the neglect of actual production. From this standpoint, a mercurial and discretionary government is especially deleterious.¹⁴

Failure, however, proved easier to explain than success, particularly in the advanced industrial societies.¹⁵ Olson attributed virtually all variation in economic performance, in the poorer and the richer countries alike, to the relative prevalence of rent-seeking (Olson 1982, esp. 175-177), but others were less certain. Were the successful advanced economies, in Asia or in Europe, in fact characterized by less governmental intervention and lobbying?

In a highly influential essay at the end of the 1970s, Peter Katzenstein (1978) had looked closely at the quite different ways in which the major industrial states had responded to the oil crises of 1973. Britain and the U.S., in this instance as in others, had relied chiefly on "voluntarist" agreements and on monetary and fiscal policy; Japan had invoked a much broader array of policy instruments, including detailed administrative regulation and allocation of capital; the continental European states

had assumed an intermediate position, with France closer to the Japanese model, Germany and Italy closer to the Anglo-American one. It might have been asserted (although Katzenstein did not do so) that the more interventionist policies had enjoyed greater success.¹⁶

John Zysman (1983), working in part with Laura Tyson (Zysman and Tyson 1983), offered a partial and controversial answer. He contended that the traditional economics of trade (including particularly the Heckscher-Ohlin Theorem) erred in assuming that technology was universally available and that comparative advantage resulted purely from exogenously given factor endowments. Rather, Zysman asserted, technological advantage and imperfect markets could endure, and comparative advantage could be "created":

in advanced industrial economies, comparative advantage...[must] be understood as the cumulative effect of firm capacities and government policy choices and not simply as the effect of resource endowments....(Zysman 1983, 40).

In particular, learning-by-doing, together with economies of scale and scope, implied that favorable governmental policies -- guarantees of stable home demand, provision of human and physical infrastructure, simultaneous encouragement of sectors with shared economies of scope (e.g., industries characterized by "high-volume standardized production") -- could impart lasting competitive advantage and consistently more rapid economic growth. While Zysman readily conceded that such efforts could be perverted into rent-seeking, notably by support of failing industries rather than of genuine "national champions," he insisted that government had a productive, indeed probably an indispensable, role in economic success.

Douglass North (1981) adopted the most general, if in some ways also the most elusive, perspective. Government, he insisted, did not merely award rents or stimulate production. Essentially, its various policies defined and enforced *property rights*, in the sense advanced earlier and less comprehensively by Coase (1960) and by Alchian and Demsetz (1973). The simple analysis of rent-seeking had failed to note that government was prior to, and conditioned, the market. By specifying rights to use, to exclude, to inherit, and to sell, rulers determined who brought what to market. By imposing suboptimal property rights, or by leaving ownership insecure, governments inhibited savings and growth; by approaching more closely to optimal property rights, they increased output.

More originally and provocatively, North asserted that a society's particular endowments of land,

labor, and capital determined a uniquely best set of property rights.¹⁷ Hence a "best" set of policies for Japan might differ radically from the "best" for the U.S., Belgium, or a less developed country. One particularly intriguing implication was that societies would inevitably "outgrow" their property rights as they accumulated capital or people; those that failed to adapt would decline.

Katzenstein's (1985) pathbreaking work on the smaller European democracies was in the spirit of North's theory but moved beyond it in significant ways. States such as Austria, Belgium, and Sweden, he observed, had discovered in policies of economic openness, "flexible adjustment," and democratic corporatism (defined roughly as a property right of each major interest to influence economic policy) the secret of economic success *for their particular situation* of extreme trade-dependence.¹⁸ At the same time, however, their situation was becoming general: rapidly increasing international trade (discussed more fully below) meant that even large countries would have to forgo protectionism and to adapt quickly to changing external markets, exactly as the trade-dependent smaller advanced economies had done long before. "The large states," as Katzenstein put it laconically, "are shrinking." Not Japan's much-vaunted *dirigisme* but the small states' "reactive and flexible policy of industrial adjustment" seemed to him the wave of the future (1985, 21, 27).

Social Sources of Policy

If policy and property rights mattered, so, plainly, did the question of how they were shaped; and work on this issue constituted a second major subcategory in political economy. All students agreed, perhaps none so emphatically as North,¹⁹ that here no system of "natural selection" guaranteed that the fittest would quickly prevail: grossly suboptimal policies and property rights -- in Spain from the seventeenth century, in Britain from the late nineteenth century, in the Communist societies throughout their existence, in post-colonial Africa -- had often continued, with attendant economic decline, for decades if not centuries. How was such evident folly to be explained, particularly in a world characterized (at least in the Realist tradition in international relations) by mortal interstate competition? One class of answers, to be addressed in a subsequent section, emphasizes characteristics of *states*; those to be discussed here focus on aspects of *civil society*.

Bates invoked Mancur Olson's (1965) early work on collective action: agriculture was victimized in most African societies chiefly because farmers were numerous, poor, and ill-organized; where a coterie of wealthy farmers could provide leadership, as in Kenya, policy

was very different (Bates 1981, chap. 5, 126-127).²⁰ Even within countries, large-scale farmers were taxed less onerously.

For Katzenstein (1985, chap. 4), countries' options in economic policy were constrained by their history, indeed almost by their culture. Europe's smaller countries were small because they had lacked the strong feudal aristocracy that made for territorial expansion in the early modern period; they had been dominated by urban mercantile elites. This history had endowed them with a weak political Right, a more self-confident and moderate bourgeoisie, less zeal about the imposition of religious and linguistic uniformity, and less intense class conflict. Those features, in turn, had nurtured patterns of cross-group and cross-class cooperation, norms of proportionality (including early adoption of proportional representation in parliamentary elections), and an ethos of social partnership and "low-voltage" politics. All of this had permitted, perhaps uniquely, the policy procedures and outcomes that by the 1980s characterized these societies.

At the same time, the deviant case of modern Austria suggested how a country might adapt from a much less favorable starting-point (1985, 186-189).²¹ As ruler of the Austro-Hungarian Empire, Austria had been a large state with a strong landed aristocracy, weak urban elites, a powerful Right, and intense class conflict. Civil war, annexation by Hitler, and military defeat and occupation had followed. Influenced profoundly by those disastrous experiences, it emerged after 1945 as

perhaps the most typically "consociational" regime in Europe...exhibit[ing] with particular clarity the coincidence of deep social cleavages and intense elite collaboration (1985, 188).

Its policy making, in short, had come to resemble that of the Netherlands or of Scandinavia. Perhaps evolution was Darwinian (or even Lamarckian) after all, but with an uncomfortably long lag.

John Zysman (1983, chap. 2) saw policy as more constrained, but by economic rather than political history: more specifically, by the financial markets that a country's particular industrial revolution had bequeathed it. As Gerschenkron (1962, chap. 1) had first observed, early developers such as Britain and the U.S. had developed "thick" markets in private credit (stock and bond exchanges) and, consequently, a merchant banking system that provided only short-term credit. Late developers -- notably France, Germany, Italy, and Japan (Gerschenkron had added: pre-revolutionary Russia) -- had required a massive and simultaneous "push" of investment in a situation of initially "thin" credit markets; hence, their entrepreneurs had relied extensively on

industrial or universal banks that extended long-term loans and often took equity positions in specific firms. Frequently the state was also involved, directly or indirectly, in the mobilization and allocation of investment capital.

Out of this gestation, Zysman argued, three kinds of financial markets were born, based respectively on: (1) private exchange in equity and debt (the U.K. and U.S.); (2) bank-dominated lending (Germany; others would add Italy); (3) state-dominated allocation of credit (France, Japan). Only in the last situation was state-orchestrated adjustment politically easy or likely. Where banks held sway, tripartite negotiations among capital, labor, and government were the likely instrument of change. Among the early developers, where capital continued to be allocated privately, all efforts at large-scale restructuring, and particularly attempts at state intervention, were likely to provoke insurmountable political conflict.

Peter Hall's (1986) richly detailed study of recent economic policy making in Britain and France argued with equal force, despite some theoretical confusion,²² that policy was powerfully constrained by social structure and tradition. Not only financial but labor markets tended to remain broadly fixed over generations, to affect economic growth, and to limit policy options.²³ Britain's long economic decline, Hall agreed, was to be blamed chiefly on its inherited financial structure of merchant rather than industrial banks and the consequent absence of leverage to restructure outmoded industry at critical junctures (pp. 38-40). But in labor markets, too, early industrialization had left its mark in the form of a skilled work force, powerful craft unions, and deep resistance to labor-saving innovations; together, these factors encouraged employers to adhere to tradition and avoid new investment (pp. 44-45). Even the radically reforming government of Margaret Thatcher, at least by 1986 when Hall wrote, had been able to achieve surprisingly little with respect to these underlying factors (chap. 5, esp. 132-133).²⁴

North and Olson offered far more sweeping perspectives. For North, suboptimal policies and property rights were normally a consequence of either (a) private-sector monopoly power (e.g., a militarily indispensable aristocracy wins exclusive rights to own certain lands) or (b) high costs of monitoring or enforcement (e.g., a grant of monopoly is more cheaply administered than a tax). For purposes of current research, these categories seemed too general: it was hard to see, for example, how they could explain the differences between British and French, or Japanese and U.S., economic policy or performance.

Olson's theory, although it was widely challenged and frequently rejected,²⁵ was richer and clearer in its empirical implications. As I discuss in greater detail below, it attributed destructive economic

policies to the proliferation of a particular kind of pressure group, namely the "distributional," or rent-seeking, coalition, and asserted that such coalitions ordinarily pervaded a society more the longer it remained free and independent. Japan, France, Italy, and Germany grew more rapidly because, having been occupied, subjected to totalitarian rule, and (except for Japan) integrated into a larger unit (the European Community), they had been "cleansed" of powerful distributional coalitions; Britain, the U.S., New Zealand, and Australia grew slowly precisely because, having avoided those tribulations, they experienced more intense rent-seeking.

Recent events offer fertile new ground for inquiry. In the Soviet systems, policy and property rights were plainly suboptimal, indeed ultimately suicidal; repeated serious attempts at reform failed;²⁶ and efforts to reform policy making (e.g., through democratization) brought down the entire edifice. As stubbornly as in the post-1945 British economy or at General Motors or IBM today, manifestly self-destructive policies resisted all efforts to reverse them. In many Third World countries, however, policy (and, usually, institutions) has changed over the last decade away from what Bates described toward something markedly more favorable to economic growth (Clark 1992, A14).

Economic Death and Political Transfiguration

A third line of work in political economy, associated chiefly with the name of Peter Gourevitch (1986), investigated the political consequences of economic failure. Comparing coalitions and policy choices in five countries (France, Germany, Sweden, the U.K., and the U.S.) in response to three major economic downturns (1873-96, 1929-49, and 1971-present), Gourevitch contended that the policies advocated in each case could be comprehended in five broad categories: (a) classical liberalism (*laissez-faire*); (b) socialization and planning; (c) protectionism; (d) Keynesian demand stimulus; and (e) sector-specific intervention, more recently called "industrial policy" (chap. 2).

Such a perspective offered radical simplification and startling comparison. Despite the striking differences in political base, ideology, and method, for example, the U.S. New Deal, Swedish Socialism, and German National Socialism all endorsed policies of demand stimulus (pp. 128-129). Moreover, it proved possible to specify the enduring preferences, or at least the aversions, of major social groups with respect to these options: agriculture and labor had rejected *laissez-faire* in virtually every crisis; business had similarly spurned socialization. Domestically oriented producers in business, agriculture, and sometimes labor were sympathetic to protection (but often also to demand

stimulus); those who depended on international markets sought non-protectionist remedies (and worried that demand stimulus, by raising wages, might make their products uncompetitive). Actual policy choice was linked to the particular coalition of interests that formed in support of the given option. With respect to demand stimulus in the 1930s, for example, the coalition of urban masses and agriculture, with support from some elements of business, proved crucial.

Gourevitch concluded early in his analysis, however, that one could not simply predict policy from preferences. Indeed, typically in such a crisis, and demonstrably in the 1930s,

policy preferences...evolved rapidly Ideological commitments toward the "proper" role of government disintegrated, and so did political commitments of long standing (p. 159).

To understand a specific outcome required a knowledge not only of "production profile" and pre-existing associational structure, but of state structure, economic ideology, political leadership, and international system. What resulted was, as Gourevitch conceded in his conclusion,

not...a "scientific" testing of alternative explanations...[but] a historical sociology of the trajectories of national responses to external changes (p. 221).

Nonetheless, a clear finding of the study was the lasting impact of the specific choices that had been made in each crisis. The protectionist "deals" of the nineteenth century solidified existing regimes in France, Germany, Sweden, and the United States; the success of demand stimulus in the 1930s in Sweden and the U.S., coupled with memories of the preceding misery, gave the Left in both countries a long-term lease on power. In his concluding focus on the post-1971 crisis and its resultant worldwide wave of "neo-liberalism," Gourevitch suggested that policy choices were again underway whose consequences would extend over decades.

The International Context

Increasingly in the 1980s, students of comparative politics began to grasp how profoundly domestic conflicts and institutions were shaped by states' international surroundings: by what Gourevitch, in a 1978 essay that may fairly be said to have re-opened this whole direction of inquiry, had called simply the influences of "war and trade" (Gourevitch 1978, 883).²⁷

War

In an important historical study that appeared in the previous decade, Perry Anderson (1974) had shown how even the European absolutist states of the seventeenth and eighteenth centuries had been shaped -- partially in the West European cases, predominantly in those of Eastern Europe and Russia -- by the necessities of military competition and survival. The logic was simple: given the military and extractive technology of the age, only absolutist governments could field the armies that survival required. Those states whose rulers imposed absolutist rule (e.g., Austria, Prussia, Russia, Sweden) survived; those where such efforts failed (e.g., Poland), unless they were sheltered by natural defenses (e.g., England), disappeared from the map of Europe.

Douglass North (1981, 30, 135-139) reminded his readers that changes in military technology had often altered both states' optimal geographic extent and their modes of internal governance: the rise of ancient Greek democracy and, subsequently, of the great empires of Macedonia and Rome, was linked to the use of the phalanx in combat; the rise of feudalism and the parcellization of sovereignty, to the predominance of armored knights; the dilution of feudal power and the return of larger states, to the emergence of the pike, the longbow, and gunpowder.

Even the great social revolutions of the modern era in France, Russia, and China, Theda Skocpol (1979) contended in a sweeping comparative study, had all been precipitated by an external cause, namely, the failure of the existing state in its most fundamental task of defense. Defeat or Pyrrhic victory in major external wars had led in each case both to domestic recrimination and to a fatal weakening of internal mechanisms of control.

Trade

In the 1980s as in the previous decade (see Migdal 1983, 324), work on the international-domestic tie centered more on trade than on war. Trade connects directly to the issues of growth and crisis discussed earlier, because trade and consequent specialization are among the most powerful engines of growth; and many of history's most painful economic downturns, from that of declining Rome to the Depression of the 1930s, were compounded (we now believe) by a collapse of interregional trade. Moreover, as Richard Rosecrance (1986) has argued, trading prowess may by now have replaced martial virtue as the principal bestower of influence in international relations.²¹ At the same time, openness to trade evidently exposed a country to more severe economic buffeting from an unpredictable international economy.

The years since 1948 have been marked by an accelerating growth of international trade, and of individual economies' dependence on such trade, that by now eclipses all analogous episodes in recorded history.²² On average over that period, the volume of world trade has doubled about every eleven years, growing 1.4 times as rapidly as output. In the average (industrialized) member state of the Organization for Economic Co-operation and Development (OECD), trade rose from 24.4% of GDP in 1960 to 36.8% in 1989 (OECD 1991, 71-72, Tables 6.12 and 6.13; population-weighted means). From various perspectives, analysts of comparative politics have sought to determine the consequences of this expansion.

David Cameron (1978) established a close connection among the OECD states between trade-dependence and expansion of the public sector: state revenues as a share of GDP grew in direct proportion to the share of GDP that was initially devoted to international trade.³⁰ Presumably the explanatory link was the political need for government to cushion its subjects against international economic shocks. Katzenstein's work buttressed this presumption: the highly trade-dependent small European democracies were indeed characterized by generous public benefits intended to meet, and to hasten adjustment to, the dislocations of ever-changing international markets.³¹

Others sought to link trade-dependence to particular forms of governance and political contestation. By the 1980s, Geoffrey Ingham's (1974, 42) finding, that both labor and business tended to organize more thoroughly and more centrally in those industrial economies that depended heavily on exports, was widely accepted.³² Katzenstein noted, further, that the smaller and more trade-dependent European democracies had better-organized political parties and closer ties between parties and interest groups (1985, 90). Douglas Hibbs established that, since 1945, strikes had declined far more in the small, open European economies than in the larger and more self-sufficient ones, presumably because conflict in the former cases had more self-evidently disastrous consequences (Hibbs 1978, 162; cited in Katzenstein 1985, 88). Several scholars attempted to establish a linkage between trade-dependence and democratic corporatist governance (see below).

Stein Rokkan had called attention to the fact that the smaller democracies had adopted proportional representation (PR) earlier, with less conflict, and in the end with fewer exceptions, than their bigger fellows (1970, 76, 80, 89; cited in Katzenstein 1985, 100, 151). Katzenstein and I suggested, from slightly different directions, a specific symbiosis between PR and reliance on external trade: PR diminished conflict, made policy more predictable and thus investment more secure, and (to the extent that it employed large constituencies)

dissipated regional pressure for protection (Katzenstein 1985, 100-101, 150-156; Rogowski 1987). Empirically, the correlation between trade and PR seemed stronger than that between geographical smallness and PR; and one could venture the prediction that, as even large states' reliance on trade increased, PR would spread (Rogowski 1987).³³

A third line of work sought to link changes in international trade with the country-specific systems of what Rokkan and others had called "cleavage and coalition," i.e., the lines of politically relevant division and alliance. That fluctuations in international trade help some groups and harm others is self-evident; so, presumably, is the tendency of the victims to demand, and of the beneficiaries to oppose, policies that offset those changes of fortune. An example familiar at least since Charles Kindleberger's seminal essay of 1951 is the flood of American wheat onto European markets in the 1870s after the extension to the Great Plains of the U.S. and Canadian railway systems and the introduction of cheaper ocean shipment through iron steamships. Grain farmers throughout Europe demanded tariff protection.³⁴ European workers, as represented in trade unions and parties of the Left, agitated against what they regarded as a food tax.³⁵ In places as diverse as Germany, France, Italy, and Sweden, the farmers (usually in coalition with protectionist industrial interests) prevailed, and tariffs were imposed; in Britain, Belgium, Denmark, and the Netherlands, trade in grain remained substantially free.

Less obvious, but evident on even cursory reflection, are the longer-term effects on wealth, power, and regime of such market shifts and political responses. To take a much-studied contrast from that same period: in Britain, rents and land prices fell precipitously to reflect decreased returns; in Germany, land prices remained high. In Britain, there ensued what Kindleberger (1951) called "the liquidation of agriculture as the most powerful economic group"; in Germany, the landed *Junker* continued their ancient domination. Finally, as Alexander Gerschenkron (1943) argued, tariffs even nurtured German opposition to democracy. Urban consumers, representing a constantly growing share of the populace, wanted free trade but lacked political power. Democratization, it became clear, would mean an end to tariffs. Yet Germany's traditional elites had come to depend so heavily on protection that any liberalization of trade would have meant their ruin. Out of purest self-preservation, they therefore grew increasingly anti-democratic — exactly at the same time as British elites, having moved into non-agricultural investments, were accepting democratic rule.

The lesson seems clear: changes in international markets precipitate domestic conflicts that may reach far beyond the direct arena of trade policy. What proves

extraordinarily difficult is to develop a convincing general theory that can explain and predict the preferences that shape, and the alliances that settle, those conflicts.

For the nineteenth century, for example, there is general agreement that divisions over trade policy followed principally *factoral* lines:³⁶ owners of land, labor, or capital generally held the same preferences in a given country irrespective of their particular line of activity, so that (for example) German farmers and industrialists eventually embraced protectionism almost regardless of their specific products or investments. By about 1900, that solidarity had begun to fray: while heavy industry sought tariff protection in almost every country, light, consumer-oriented industries — chemicals, electrical equipment, home appliances — joined workers in support of free trade. Within industry, in other words, *sectoral* divisions began to appear.

By the 1970s, Stephen Magee (1978) has suggested from U.S. evidence, sectoral divisions had begun to dominate within labor as well: workers in import-threatened industries (steel, automobiles) supported protection, while ones in export-oriented branches (computers, pharmaceuticals) endorsed free trade. Working from broader comparative data, Charles Lipson (1983, 244 and 259) contends that protectionist demands in the industrial countries have been "highly product-specific," limited to "labor-intensive" sectors that are wage-sensitive, produce-standardized, price-competitive goods, use relatively standard technologies, and are under the direction of national firms. At the other extreme, producers of differentiated goods with high research and development, relatively short product lives, and often increasing returns to scale have powerfully supported freer trade. Helen Milner (1988), looking closely at particular sectors in France and the U.S. during two critical periods of controversy, has concluded that trade preferences must often be disaggregated to the level of the firm.

In light of these contradictory data, Peter Gourevitch and Jeffrey Frieden have argued for a general analytic primacy of *sectors*; I have contended that, in general, we do better to focus on *factors* (Gourevitch 1986, 55; Frieden 1991a, 29-35; Rogowski 1989, 18-19). Economic theories of international trade make clear, however, that the argument is essentially over "specificity," or fixedness, of assets. To the extent that factors of production (including human and physical capital) can be moved easily from one employment to another, the Stolper-Samuelson Theorem (1941) applies: locally abundant factors will generally benefit, and locally scarce ones will be harmed, by an expansion of trade. Land was abundant in North America in the nineteenth century and scarce in Europe; labor was abundant in Europe and scarce in North America. Nothing could be

more natural than that farmers were free-trading in America and protectionist in Europe; or that workers were free-trading in Europe and protectionist in America.

To the extent, however, that factors are "trapped" in specific applications -- a worker is trained only for automobile production, a plant is suited exclusively for the output of shoes, a plot of land can grow nothing but rye -- then trade that threatens *that investment*, even if good for the factor as a whole, will arouse at least short-term demands for protection in the affected sectors.³⁷ The U.S., for example, is abundant in capital; hence industrial owners will in general support free trade. Automobile manufacturers, however, have *specific capital*: plants that cannot readily be adapted to other uses, and that Japanese and Korean competition is rapidly making worthless. Hence we are not surprised to learn that firms in this sector demand protection against imports.

The degree to which assets are in fact specific, and whether their specificity changes over time, remains an important, but essentially empirical, research question.

Finally, one must ask whether the growing internationalization of the economy has directly constrained policy. As virtually every account of the early Mitterand government has emphasized (e.g., Hall 1986, chap. 8; Gourevitch 1986, 185-190; Moravcsik 1991, 29-30 and sources there cited), its phase of socialist experimentation -- extensive nationalization, "redistributive Keynesianism," and aggressive expansion of employment -- had to be abandoned chiefly because the international economic ramifications, in a far more trade-dependent economy, could not be borne. A chastened Prime Minister Mauroy declared after two years of socialist rule: "Quite simply, a real left-wing policy can be applied in France only if the other European countries also follow policies of the Left." (*L'Express*, 8 April 1983; quoted in Hall 1986, 201).

Gourevitch has broadened this lesson to assert that growing interdependence pushes all governments toward a revival of classical liberalism and reliance on the market. Citing a "shift rightward" that "no observer would have thought possible in the 1970s," and noting that "governments of quite different partisan colorations have adopted similar [neo-liberal] policies," Gourevitch (1986, 215) offers the explanation that "worldwide competitive pressures have [everywhere] given leverage to investors and managers, who under present conditions particularly want a reduction in costs." Frieden points more specifically to the rapid increase in international mobility of capital as the reason for the rightward shift, and in particular for the dramatic reduction, in every industrialized country, in taxes on the highest incomes (Frieden 1991b). Presumably the shift toward the market in the Third World, and perhaps even in the formerly

communist countries, can be explained in part by the same increase in competitive pressures.

Interest Groups and Corporatism

By the late 1970s, many students of comparative politics saw in democratic corporatism -- defined by one leading author as the "incorpora[ti]on of]...formally designated interest associations...within the process of authoritative decision making and implementation" (Schmitter 1981, 295) -- "the foundation for advanced industrial growth and adaptation" and the "mechanism that could assure survival and even growth in a fast-paced, open international economy" (Migdal 1983, 316-317). In a landmark volume edited by Suzanne Berger (1981),³⁸ Philippe Schmitter (1981) offered perhaps the most cogent and empirically persuasive statement to date of the overall position; and Charles Maier (1981) expanded the perspective developed in his book on post-World War I Europe to trace the antecedents of present-day arrangements back to the plebiscitarian experiments and the tariff struggles of the last half of the nineteenth century -- thus further specifying corporatism as the natural means of governance in a mature, internationally open, industrial democracy.³⁹

Katzenstein's work, as already indicated, proceeded precisely in this vein. A system of powerful, centralized interest groups, accepting an ideology of social partnership and continually bargaining with one another, seemed uniquely well suited to encourage adjustment and growth in an export-oriented industrial economy. Whether in the "social" variant of corporatism, with strong labor movements and weak, decentralized, locally owned businesses; in the "liberal" variant, with weak working-class movements and strong, centralized, multinational business communities; or in the seemingly unique Swedish case, which combined strong multinational business with powerful labor; the recipe, in his view, worked almost equally well (1986, chap. 3).⁴⁰ Seeming to confirm Katzenstein's insight, John Ikenberry (1989) subsequently offered evidence to show that, even in the United States, increasing trade-dependence was engendering a pronounced corporatism in the realm of foreign economic policy, notably in the "fast-track" procedure for ratification of trade agreements.

Schmitter (1981, esp. 297 and 307), it should be recalled, had sounded a more cautionary note. Including in his analysis many of the larger industrial democracies, he had noted that the combination (at least on the working-class side) of high rates of associational membership with disunity or weak centralization led to high levels of instability and conflict rather than to any

form of "social partnership"; Britain and Italy were prominent examples.⁴¹

Mancur Olson, Jr. (1982) dissented far more sharply from the emerging corporatist consensus. While admitting that "encompassing" organizations -- ones that embraced a large share of a given country's work force or industries -- might contribute to social peace and increase productive efficiency, he held that the great majority of pressure groups were "distributional" (i.e., rent-seeking) in the sense outlined earlier. To the extent that such groups pervaded an economy, Olson contended, it was less efficient and more protectionist, innovated less, grew more slowly, had higher "normal" unemployment that particularly burdened the young and the unskilled, experienced greater inequality, inflation, and political instability, and was subject to more intense business cycles.⁴²

A theory more contradictory to Katzenstein's could hardly be imagined. Corporatism, except where (as in Sweden) virtually all groups were encompassing, was predicted to lead to low growth, high conflict, little social compensation, and a substantially closed economy.⁴³ Olson and Schmitter, on the other hand, agreed on crucial points: to Olson, for example, the most detrimental combination would be one of powerful but non-encompassing organizations -- exactly what Schmitter had diagnosed as the problem in Britain and Italy.

Alvarez, Garrett, and Lange (1991) have recently offered a more nuanced perspective, suggesting that the economy grows most rapidly, and inflation and unemployment are best controlled, in *either* of two cases: where strong and encompassing labor unions co-exist with a leftist government; or where weak unions face a rightist government. Gauging economic performance somewhat more subtly than many earlier efforts (e.g., taking into account vulnerability to international economic shocks), they advance strong empirical evidence in support of their contention. Paradoxically, however, they do not pursue the important lines of inquiry opened by Schmitter and M. Wallerstein.

Following Schmitter (and indirectly Olson), it would be worthwhile to examine the countries in which unions, relative to the share of the work force they organize, are either weakly centralized or disunited. We might expect these to be among the worst economic performers on many dimensions. By one plausible measure that derives from Alvarez, Garrett, and Lange's work, such cases would include (in descending order) Italy, the U.K., Australia, Canada, and France.⁴⁴

The implications of Michael Wallerstein's work (1989) are even more intriguing. If, as Wallerstein argued, union power itself varies inversely with the size of a country's work force, it would seem to follow from

the work of Alvarez, Garrett, and Lange that on average large countries will perform better under rightist, and small countries better under leftist, governments; and that electorates, which presumably seek to reward economic success, will therefore be drawn over time to those respective alternatives. Moreover, governments that seek re-election through economic success will likely endeavor to shape labor markets accordingly: Leftist governments will strengthen unions; rightist ones will weaken them -- a result entirely consonant with Wallerstein's other main finding, that the cumulative time in office of the Left correlates positively with union strength, controlling for the effect of work-force size.⁴⁵

Obviously, much fruitful research remains to be done in this area. Efforts to test Olson's work have perhaps focused too much on his predictions about economic growth, too little on his rich array of hypotheses about inflation, unemployment, instability, inequality, and protectionism.

State Structures

As Skocpol (1985, 3) has observed, comparativists began to rediscover the state sometime in the early 1970s. One powerful influence was Samuel P. Huntington's *Political Order in Changing Societies* (1968),⁴⁶ which took as its leitmotif the fatal weakness of most Third-World states. A second was the growing emphasis among students of international relations on the autonomous position even of "weak" states, such as the U.S., in the making of foreign policy; here Stephen Krasner's 1976 article in *World Politics* and his 1978 book, *Defending the National Interest*, were seminal. A third factor was renewed interest in the historical processes of European state-building, culminating in Charles Tilly's edited volume on *The Formation of National States in Western Europe* (1975).⁴⁷

Fundamentally, this group of scholars asserted that state elites were in significant degree autonomous from society, that state policies did not simply reflect social preferences or pressures, and that differences in state structure and recruitment therefore accounted for much of the observed variation in policy and outcome.⁴⁸ To take a representative example: Margaret Weir and Theda Skocpol (1985) attribute much of the policy variation among Britain, Sweden, and the U.S. in the 1930s (and subsequently) to (a) the centralization of economic policy making in Britain and Sweden, as against its dispersal in the U.S.; and (b) the British Treasury's long-standing ties to the City of London, vs. the Swedish tradition of central administrative boards with deep respect for professional (e.g., economists') judgments. In the U.S., unorthodox ideas about demand

stimulus could readily percolate into parts of the inchoate policy-making apparatus, obtaining early but insecure influence. In Britain, the Treasury remained resolutely orthodox until almost the end of World War II; but, once converted to Keynesianism, became almost equally impervious to such subsequent heterodoxies as monetarism. In Sweden, expert opinion was quickly heard and implemented.

With respect to economic growth, the impressive success of Japan and the NICs led many to believe that growth-stimulating policy could be achieved only by strong, autonomous, "smart" states.⁴⁹ North's (1981) and Olson's (1982) depictions of how powerful pressure groups distort economic policy could also be read as arguments for state autonomy.⁵⁰ Katzenstein (1978, 313-316, 320-321) suggested that the more detailed, supple, and authoritative Japanese (and, to a lesser extent, French) foreign economic policy was a consequence of the greater strength and centralization of those particular states. Students of both developing and advanced countries came to focus on the issue of state "strength" or "capacity."⁵¹ In all of this, there was often an almost Freudian hint of "penetration envy" on the part of Americans accustomed to regarding their own state as weak. Analyses based on state capacity work best if one can assume that relevant state structures change slowly, thus serving as the "bedrock" variable that can explain persistent differences between nations. Weir and Skocpol take for granted that basic patterns of governance from the eighteenth and nineteenth centuries persist into the twentieth; Katzenstein sees the strength and autonomy of the Japanese and French states as a legacy of the absolutism which, in each case, ended feudalism and commenced economic development; Krasner asserts that "the weakness of the American polity is deeply embedded in the country's history."⁵²

Studies both of historical trends and of recent national policy making, however, have tended to rebut historical determinism and to undermine sweeping assessments of state capacity. Anderson (1974) showed how quickly absolutism had sometimes been constructed (e.g., in Prussia under the Great Elector); Skowronek (1982), how even the "weak" U.S. regime had been able to build capacity when necessity demanded it; Zysman (1983, 315), that the vaunted Japanese system was "not a cultural inheritance but an explicit political creation of the postwar years." Both Zysman (1983, 295-299, 299n) and Gourevitch (1986, 190-192, 198-199) concluded that differences in state capacity could not explain the intertemporal or cross-national variations in policy that they observed. Moreover, Zysman suggests, on close inspection assessments of state capacity often blur:

...France is a strong state in terms of energy policy but in the social services the

bureaucracy is trapped in a morass; in welfare policy France is a weak state (p. 297).⁵³

Milner (1987), going further, suggests that the vaunted autonomy of the French state as a whole is greatly overrated in the traditional literature; and, even more problematically, the Communist regimes (presented by Huntington as an enviable example of developing-country institutionalization) turn out to have been sinkholes of influence-peddling and corruption.

The state, it appears, is at best an intervening variable, which itself responds (or fails to respond) to social pressures and needs. Further advance requires a theoretical explanation of the ongoing interaction between the state and its domestic and international environment, along the lines suggested by Migdal's (1988) essay in comparative history.

One possible route to such a theory, only now beginning to be explored, is to extend to the world of government the insights of Oliver Williamson (1975, 1985) and others -- the so-called "new institutional economics" -- on transaction costs and the organization of the firm.⁵⁴ Among the relevant insights of this literature are: (a) that organizational forms are best interpreted as solutions to problems of information and supervision; (b) that competition creates incentives to adopt maximally efficient forms of organization; and (c) that hierarchies replace markets, or vice-versa, to the extent that one or the other form more efficiently resolves problems of information and organization. North (1981, 37ff.) explicitly took this literature as his starting-point for a "neoclassical theory of the state"; Stinchcombe (1990, chap. 9) has applied it creatively to such issues as university governance; and, most importantly for present purposes, Weingast and Marshall (1988) have extended parts of it to the analysis of congressional procedures.

Much more can probably be done. Two brief examples indicate the possibilities. (1) The various forms of claimancy (chiefly debt vs. equity) and attendant voting power in corporations, and the reasons for their use, have been extensively discussed (Alchian and Demsetz 1972, 788-789, 789n; Williamson 1985, chap. 12, esp. 304-309). (The "leveraged buy-outs" of the 1980s, for example, were chiefly a way of increasing managerial autonomy by using non-voting bonds to replace shares of stock that carried voting rights.) Similar considerations should govern the expansion or contraction of governmental participation, more specifically of democratic rule; and Bates and Lien (1985) have argued, in essence, that they do. (2) Chandler, Williamson, and others have persuasively analyzed the emergence of the multidivisional form of corporate organization in the 1920s as an effective answer to problems of informational overload and strategic

planning.⁵⁵ Quite similar logic should apply to: (a) legislatures' centralization of authority in cabinets or "control" committees; (b) the displacement of cabinets by personal staffs of prime ministers or presidents; and (c) the devolution of independent authority to central banks, courts, and regulatory agencies.⁵⁶

A separate, more modest, and quite fruitful approach to the state in this period was simply to test empirically the many assertions about specific institutions' effects -- on policy, on groups, on civil peace or disorder.⁵⁷ In his prizewinning work on *Contemporary Democracies*, Bingham Powell (1982) examined statistically how constitutional variations (parliamentary vs. presidential regimes, proportional vs. majoritarian electoral systems), in the context of country size, level of modernization, and pre-existing cleavages of ethnicity and class, affect such aspects of state performance as participation, stability and effectiveness of government, fractionalization and extremism of political parties, and avoidance of organized protest and revolt. Dovetailing with the work on PR in trade-dependent economies, the proportional parliamentary systems emerged absolved from much of their reputation for poor performance; and, in perhaps his most striking finding, Powell gave a new lease on life to the widely discredited notion of political business cycles⁵⁸ by showing that single-party domination of a country's executive was strongly associated with atypical election-year expansions of real disposable income.⁵⁹

In a similar but narrower vein, Arend Lijphart (1990) re-tested Douglas Rae's (1971) pioneering hypotheses on the effects of electoral laws, using a larger data set and more discriminating measures. Perhaps his most counterintuitive finding was a considerable weakening of "Duverger's Law," the hoary link between electoral system and number of parties. Plurality and majority systems turned out to average just under three "effective" parties normally contesting elections; systems of proportional representation averaged just over four (1990, 490).

A particularly promising avenue of institutional inquiry involves efforts to link economic performance to features of regimes. An early, rather simple hypothesis, now widely accepted, asserted an inverse relationship between central bank independence and inflation.⁶⁰ More recently Grilli, Masciandaro, and Tabellini have claimed that all recent cases of extreme indebtedness and inflation occurred "in countries governed by highly proportional electoral systems" (Grilli et al. 1991, 4). Majority and plurality parliamentary systems, they argue, assure greater price stability; strong presidential regimes (France and the U.S.) do even better. Other economists have pursued similar claims, sometimes with an imperfect understanding of the political institutions.⁶¹ Better

empirical testing, preferably by comparativists, is required.

Nationalism and Ethnic Conflict

The intensity of ethnic cleavage and conflict in the 1980s, not only in the less developed countries (e.g., India) but in Europe (Belgium, the United Kingdom, Spain), North America (Quebec), and most sharply in what had been Yugoslavia and the USSR, remained as a standing rebuke to prevailing theories in comparative politics.⁶² Crawford Young's (1976, esp. 6-11) pioneering discussion seemed increasingly prescient.

According to Parsonian theory, modernization entailed greater universalism and less orientation to ascriptive criteria; yet conflict arose in the most modern nations, led sometimes by their most advanced regions (e.g., Catalonia).⁶³ Marxism held that ethnic and national loyalties were a form of false consciousness that would be superseded under Socialism; but decades of allegedly Socialist rule, and indeed of intense effort to create a "new Soviet man," had evidently succeeded only in intensifying ethnic loyalties.

On a quite different tack, Karl Deutsch (1961; 1969, 73) and Clifford Geertz (1963) both suggested (albeit from contrasting perspectives) that rapid social change might itself re-activate ethnic (for Geertz, "primordial") loyalties;⁶⁴ but the correlation between change and ethnic conflict was imperfect, and the loyalties involved were often as much self-chosen, improvised, or even invented, as they were primordial.⁶⁵ Working from such observations, others contended that most ethnic conflict was entirely tactical, an effort to restrict competition for especially desirable sectors and professions (notably in the civil service) to a limited subgroup.⁶⁶ As David Laitin (1986) trenchantly noted in his study of Nigerian cleavages, such a theory was powerless to explain cases in which tactically tempting ethnic or religious distinctions *failed* to generate political conflict (1986, esp. chaps. 1 and 6; cf. Young 1976, 516-517). Worse, it explicitly assumed that rank-and-file followers of ethnic movements (who could never realistically expect to achieve such jobs) were behaving irrationally;⁶⁷ and it stretched credulity by supposing that visions of slightly better jobs motivated the indescribably cruel conflicts, replete with ritual maimings and slaughter of the innocent, between Tamils and Sinhalese, Sikhs and Hindus, Armenians and Azerbaijani, Serbs and Croats, et cetera ad nauseam.⁶⁸

Several authors attempted in the 1980s to offer more convincing accounts of these important phenomena. Three main lines of argument emerge.

Ranked vs. Unranked Ethnicities

It is useful to distinguish between what Donald Horowitz (1985, 21-36) has called "ranked" and "unranked" systems of ethnicity. In the former (sometimes also called "segmented" or "plural" societies), ethnic groups stand in clear relations of super- and subordination to one another; and their positions are often linked, in ways described earlier in anthropology by Furnivall and others (1948; cf. Smith 1969), to particular occupational positions. In unranked systems (also denominated, particularly in European usage, "pillarized"), each ethnic group spans the whole available range of occupations and statuses.

Ranked systems tend to remain stabler over long periods of time, or to be agitated only by attempts to raise particular groups (e.g., castes) in the given hierarchy (Horowitz 1985, 34). When such systems do break down, however, typically by upward mobility from the subordinate group or a change in technology that "de-skills" the superordinate caste, some of the bloodiest and most revolutionary ethnic conflicts ensue, typically ignited by desperate efforts of the higher-ranked group (or its most threatened elements) to maintain their position: Horowitz (1985, 30, 34-35) advances as examples the revolutions in Rwanda (1959) and Zanzibar (1964) and the repeated pogroms against Hutu by Tutsi in Burundi. Black-white conflict in South Africa and the pre-1965 U.S. South, Catholic-Protestant warfare in Northern Ireland, even Francophone-Anglophone discord in Canada, probably also qualify.⁶⁹

In contrast, the "pillars" of unranked systems, Horowitz notes (citing approvingly Michael Hudson's analysis of pre-civil war Lebanon), are "incipient whole societies" that relate to each other "as if they were states in an international environment" (Horowitz 1985, 31). Exactly as in the classical international balance of power, conflict in such systems is more continual but ordinarily less mortal; only where one "nation" tries to assimilate or subjugate another (as in Lebanon) is major warfare likely (Horowitz 1985, 31, 35; Rogowski 1985, 100). Where that danger is avoided (the Netherlands, Switzerland, by now probably Belgium), ethnic divisions may actually erode peacefully over time.

In the most intense conflicts, Horowitz shows with a wealth of evidence, one or more groups typically express the belief that they are in danger of permanent subjection or actual extinction: with surprising frequency, the parallel of the North American Indians is invoked (Horowitz 1985, 175-181). That threat is perhaps most apparent in the case of a de-skilled or penetrated elite within a *ranked* system: deprived of its former monopoly of privilege (e.g., in landownership, industrial management, the professions), it often lacks

other skills, including (notably among colonial or caste elites) the most rudimentary ones required for self-sufficiency (Rogowski 1985, 94). It may also arise, as Horowitz (179, 186ff) emphasizes, when one previously unranked group progresses much more (or less) rapidly than others, thus moving toward monopoly of (or exclusion from) key positions in the modern economy.

In ranked systems, again, a powerful revolutionary sentiment is unleashed when a formerly servile group comes to understand that all skills are in fact open to them: that French-Canadians, or Indians under the *raj*, or African Americans can manage, administer, judge, or research on an equal basis with their previous overlords.

From this standpoint, Soviet nationalities policy was foolish. While long maintaining a fully ranked system of Russian, or at least Slav, dominance in the military and the all-Union *nomenklatura* (Laitin 1991, 158), it provided full educational opportunities through the university level (and thus a full range of presumably modern skills) to each of the major non-Russian nationalities.

The Colonial Inheritance

Laitin, Horowitz, and others have placed much emphasis on the ethnic groups' colonial or imperial inheritance. In Nigeria, according to Laitin, British colonial rule had been "hegemonic" in the Gramscian sense and had thus indelibly structured perceptions of ethnicity and of "natural" alliance among ethnic groups. Only this legacy, he believed, could explain the continuing absence of religious conflict in Nigerian politics.

Horowitz (esp. chap. 4) has emphasized how colonial or imperial elites allocated occupations among specific groups and labelled them as "backward" or "advanced." Although Gramsci is not prominently cited, Horowitz clearly believes as firmly as Laitin that the colonialists exercised cultural hegemony: the invidious distinctions that they planted, particularly among unranked groups, inflict lasting psychological wounds (images of poor self-worth and weakness) and structure post-colonial conflict as a particularly vicious ethnic rivalry. Only the psychological dimension, involving group legitimacy and self-worth, Horowitz (181-184, chap. 5) believes, can explain the passion and cruelty of the competition.

In his recent work on the politics of language, and even more on ethnic conflict in the former USSR, Laitin has stressed the original ties between local and central elites: Were the former accepted by the latter as full equals -- "most favored lords," in Laitin's phrase (1988, 296-297; 1991, 143-148) -- or were they

patronized as strictly provincial rulers? By a logic too intricate to reproduce here, Laitin predicts that the cases of full incorporation (typical of western European nation-building) will have produced either total assimilation or, where ethnicity does revive (e.g., Catalonia), intense conflicts only within the regional elite. Where acceptance of local elites was halting or absent (as under European colonial rule in India and Africa), ethnicity revives easily under a coalition of old and new elites; but intense conflict often ensues between the locally dominant and locally subordinate ethnic groups, with the latter often preferring continued central rule. Among the territories of the former Soviet Union, Laitin believes, Ukraine exemplifies the former pattern; Central Asia, the latter. Georgia and Estonia assume an intermediate position.

Laitin's argument links readily to Horowitz's, and both gain credence from recent Soviet events. Where colonial rulers see their subjects as most alien, they will be least ready to accept even local elites as equal; and they will be most inclined to draw invidious distinctions about the degree to which their various subject peoples meet the high standard of civilization to which they, the conquerors, have already attained. On the evidence so far, it is precisely the most "colonial" areas of the Soviet empire that have experienced the most vicious interethnic conflict, often on precisely the lines that Horowitz would have led us to expect: e.g., the Armenians are regarded as more "advanced" than the Azerbaijanis, who therefore see themselves as economically threatened.⁷⁰

International Factors

Connecting to an earlier part of this survey, international factors in ethnic revivals have been noted by several authors. In the nineteenth century, international markets and the threat of international conflict both argued for large states. Today, freer trade and a relaxation of tensions encourage the formation of smaller, more ethnically homogeneous units (Rogowski 1985, 380-381). Horowitz (1985, 35) notes that international pressure now virtually prohibits ranked systems of ethnicity — consider only the South African case — and Laitin concedes that international diplomatic recognition of successor republics has played a crucial role in the breakup of the USSR and Yugoslavia (1991, 176).

At a slightly deeper level, one may suggest, the mere availability of wider and more lucrative international markets — for Scottish oil, Quebec timber and hydroelectric power, Uzbek cotton, or Baltic entrepreneurship⁷¹ — may have nourished ethnic grievances and encouraged ethnic separatism.

Conclusion

Twin tendencies run as an unbroken thread through the entire preceding discussion: the growing reliance on economic, and the relegation of sociological, modes of analysis in comparative politics. Those areas in which the most has been accomplished, namely political economy, the study of the international-domestic linkage, and the analysis of pressure groups, are those most characterized by extensive borrowing from, and collaboration with, economists. The area now best poised to make a similar advance, the study of the state, is being prodded by economists to do so. The domain that remains least satisfactorily explained, despite its evident importance, is the one to which the least economic analysis has been applied, namely that of ethnic conflict and nationalism. So far as the study of comparative politics was concerned, Brian Barry (1970) was either prescient or successfully hortatory when, two decades ago, he weighed the previously dominant sociological approaches in the balance and found them wanting.

What of the future? While no one can rule out a sociological revival (see Eckstein 1988; Inglehart 1988) or the emergence of some yet more powerful alternative approach, the likeliest prospect is that the economic perspective will continue to dominate. Relatedly, the most exciting and promising future work is likely to center on the issue of economic growth and performance: what policies and property rights most favor growth in particular circumstances; what institutions best guarantee the maintenance of those policies; and what aspects of domestic civil society or of the international environment favor the adoption and survival of such institutions? Why are anti-growth policies so often chosen? These questions are of particular moment for the post-Communist societies, but they agitate also the Third World (e.g., India, Mexico) and the industrialized West (debates about central bank independence in Italy, New Zealand, and the European Community; about the electoral system in Japan and the United Kingdom; about how to enforce fiscal discipline and whether to adopt a conscious industrial policy in the United States).

A second major area of future inquiry is likely to revolve around economic internationalization. More open trade in goods and services, easier movement of factors, the widening and deepening of such regional regimes as the European Community and (if it comes to pass) the North American Free Trade Area (NAFTA) affect institutions, constrain policies, and re-shape political alignments. Our models for explaining and predicting those effects are in their infancy; but the baby seems lusty and has nourishment close to hand, chiefly from

economics and the comparative histories of U.S., Italian, and nineteenth-century German unification.

Third, we are likely to see much more important work on state structures, from both historical and deductive perspectives. Here the goal will be to explain not the consequences but the causes of state organization and evolution: why power is concentrated or delegated differently; what common forces lead to the broad convergence of regimes that we see among the industrialized states; why, historically, regime-types often ebb or flow in tandem across states (e.g., absolutism in the seventeenth century, democratization in the nineteenth and twentieth).

While I am eager to be proved wrong, I suspect that corporatism's vein of good work is almost played out and that studies of nationalism will continue to disappoint. In the former case, some powerful theory seems to have reached a dead end and to have little new to say; in the latter, it is hard to discern the basic, powerful strands of theory on which alone a better explanatory edifice might arise.

These speculations about the future, however, should be treated with the skepticism that any practitioner of comparative politics deserves. We are, after all, a field whose most signal accomplishment in the past decade has been its utter failure to predict the two most important domestic changes of the last half of the twentieth century, namely, the abandonment of communism as an ideology and the collapse of the centrally planned economies as institutions. In the coming decade, we can only do better.

Notes

This chapter has benefited greatly from critical readings and bibliographic suggestions by Ada Finifter, my colleague Michael Lofchie, and two anonymous readers. For bibliographic and research assistance, I am most grateful to David D'Lugo. As the customary formula has it, the viewpoints and the errors remain wholly my responsibility.

1. Every individual perspective on a field is necessarily idiosyncratic, even parochial, to some degree. To state the main predilections that inform this essay: I focus above all on works that seem to me to have advanced the field theoretically, thus relegating or omitting many contributions that are chiefly descriptive, or whose theoretical ambitions remain unfulfilled; and I am by training and orientation a student of the First World, who runs some risk of overlooking valuable work on regions outside Europe, North America, and Oceania. I have nevertheless endeavored (doubtless without total success) to include research on Africa, Asia, and Latin America that has had a major impact on theoretical discourse.

2. The naming of authors is intended, in each case, to be illustrative rather than exhaustive.

3. As some of the citations make clear, this trend entailed further substantial erosion of the traditional boundary between the subdisciplines of international relations ("IR") and comparative politics.

That trend had already been noted by Joel Migdal (1983) (following Peter Gourevitch) in the previous edition of this book.

4. Only a small part of these disparities can be accounted for by "convergence effects," i.e., the tendency of initially poorer countries to grow more rapidly (see, for a useful summary, Barro and Sala-i-Martin, 1992). Every effort to regress post-1960 economic growth among the OECD states on their 1960 per-capita GDP leaves a large positive residual for Japan and large negative ones for the U.S. and the United Kingdom.

5. Abram Bergson (1991, 33) suggests that Soviet GDP per capita in 1980 was 44% that of the U.S.; in 1985, 42%. Letting 1980 U.S. output = 100 and extrapolating from the 1973-80 U.S. annual growth rate (1.3%), 1985 U.S. output would be $(1.013)^5 \times 100 = 106.7$; 42% of that would amount to 44.8. Dividing that by 44 (the 1980 Soviet base output) yields 1.018; and that to the 1/5 power gives us an annualized growth rate for Soviet per capita GDP of about 0.4%.

6. For sub-Saharan Africa as a whole, per capita GDP grew by 1.3% annually in the 1960s; by 0.7% annually in the 1970s; and then fell in each of the ensuing three years (beginning 1981) by more than 3%. For twelve of thirty-six countries on which data were available, real per capita GDP was lower in 1981 than it had been in 1970 (World Bank 1984, 10 and 23).

7. In every major Latin American country, real per capita GDP was lower in 1985 than it had been in 1980; in Argentina, Peru, and Venezuela, it was lower in 1985 than it had been in 1973. (Balassa et al. 1986, Table 1.2).

8. Taiwan's per capita GDP, for example, grew at an annual rate of 8.1% between 1963 and 1972 and 6.6% between 1973 and 1980, as against OECD (population-weighted) averages of 3.9% and 1.7%, respectively, for those same periods (Amsden 1985, 80, Table 3.1; OECD 1982, Table 3.2).

9. In "at least nine countries...., lower inflation, lower unemployment, and higher rates of growth of disposable income [have been shown to] increase popular support for incumbent elected politicians" (Alt and Chrystal 1983, 150).

10. The first point was no news to Africanists, it having been argued forcefully for some time by Michael Lipton (1976). Bates however made the case conclusively and tied it to larger theoretical concerns; and he reached a much larger audience.

11. That policy matters is now almost universally accepted. For influential treatments, (see World Bank 1984, esp. chaps. 2 and 4; and Balassa et al. 1986).

12. Interestingly, many Soviet scholars were arriving at precisely the same conclusion in this period: see Hough (1986).

13. In economic parlance, we should be clear, a "rent" is any above-market return to a factor of production. Familiar examples of governmentally awarded rents are agricultural subsidies, minimum wages, and restrictions on entry (e.g., in taxis and cable television). Bhagwati (1982) and others prefer the term "directly unproductive profit-seeking."

14. Thus the more discretionary American government that Theodore Lowi (1979) contended had arisen since the New Deal would have particularly damaged U.S. economic growth.

15. Japan, of course, was much studied at both a popular and a scholarly level: examples, respectively, are Ezra Vogel (1979) and Chalmers Johnson (1982). Both the "lessons" and the possibility of their export remained unclear, however. See for example the dissenting view of David Friedman (1988).

16. In the two "horrible years" of 1974 and 1975, U.S. real GDP per capita declined on average by 1.7% annually; British, by 0.9%; Japanese, German, and Italian, by 0.6%. French GDP per capita rose on average by 1.2% annually. Calculated from OECD (1982, Table 3.2).

17. Property rights in land, for example, came to be worth enforcing only after population achieved a certain threshold density (North 1981, 80-82).

18. Trade share has been shown to vary in proportion to the inverse cube root of population (in millions); thus a country that numbers 8 million (cube root = 2) will have roughly twice the trade share of GNP as one that numbers 64 million (cube root = 4) (Taagepera and Hayes 1977).

19. "...efficient" property rights are unusual in history.... As a result, ... technological progress has been slow throughout most of history" (1981, 6).

20. In his current work on coffee-growing in Colombia, Bates (privately circulated ms.) concedes that the Olsonian "latent group" cannot offer a complete explanation: in Colombia, small coffee-growers triumphed politically over both latifundists and an initially hostile state.

21. For reasons not fully clear, however, Katzenstein doubted that the U.S. could adapt similarly: "We cannot apply the 'lessons' of the small European states for the simple reason that we cannot remake our history" (p. 207).

22. Without pausing to think about negative degrees of freedom, Hall suggests (1986, 259) that "five sets of structural variables," each "vary[ing] along several dimensions," will be required to explain his two cases. Indeed, "any further reduction would fail to capture the full range of factors affecting their policies."

23. More recent work raises doubt about the immutability of financial markets. In separate studies of struggles to establish independent central banks, John Goodman (1991) and Lawrence Broz (1992) show that significant changes have occurred quickly.

24. Zymman (1983, 225-227), writing earlier, was even less sanguine about the Thatcher experiment.

25. Among the many critiques are Pryor (1983), Rogowski (1983), Cameron (1988), and Gray and Lowery (1988).

26. See especially Schroeder (1988) and Wolf (1991).

27. Students of international relations began also to explore the link between domestic and international politics, albeit with a predictably different focus on international regimes and the projection of national power. Among the most influential writings were Keohane and Nye (1977) and Ruggie (1983).

28. Rosecrance's views can be usefully contrasted with those of Keohane and Nye (1977, esp. chap. 2), who had portrayed "complex interdependence" as an ideal type, unlikely to be achieved in the real world; cf. Keohane and Nye 1987, 731.

29. For more detailed statistics, and a comparison with earlier expansions, see Rogowski (1989, 88-90).

30. More precisely, Cameron estimated the following regression equation for the OECD states: state revenues/GDP 1975 - state revenues/GDP 1960 = 1.315 + .184(trade/GDP 1960) ($r = .819$, $r^2 = .671$).

31. Nonetheless there were very substantial differences in direction of outlays between Katzenstein's "social" and "liberal" corporatism (1985, 115-121).

32. Michael Wallerstein (1989, 491-492), however, hypothesized persuasively, from a rational-choice perspective, that unionization rates were an artifact of the size of a country's work force, moderated by leftist political dominance; trade had no independent effect. Katzenstein's (1985, 89-91) arguments on the point also emphasized smallness rather than trade-dependence.

33. The United Kingdom and Japan are at present the most interesting test cases for the prediction.

34. U.S. and Canadian grain-growers, unsurprisingly, supported free trade with equal militancy.

35. U.S., Canadian, and Australian workers generally voted in favor of protectionist parties in this period.

36. Most notably, this is clearly implied by the pathbreaking comparative work on this period by Barrington Moore, Jr. (1967).

37. Two fundamental papers on the specific-factors approach are Ronald W. Jones (1971) and Michael Mussa (1974).

38. Several of the contributions are discussed incisively in

Migdal (1983, 317). Also influential were Schmitter and Lehmbruch (1979) and Lehmbruch and Schmitter (1982).

39. Corporatist institutions have of course been observed also in other, often non-democratic contexts, including particularly Latin America. I focus here on the theoretical effort to link a particular kind of corporatism to economic pressures and outcomes.

40. Katzenstein (p. 118) warned, however, that social corporatism's propensity to expand public employment seemed likely to hinder investment and competitiveness.

41. Katzenstein had no occasion to disaggregate these two characteristics, since (as Schmitter's data reveal) among the smaller European democracies they are strongly collinear: almost everywhere that labor organizes a high share of the work force (Sweden, Austria, Norway, Denmark) it is united and centralized; where labor organization is less "dense" (the Netherlands, Switzerland), labor is also more divided, less centralized, or both. The sole (and partial) exception is Belgium, with rather "dense" organization but somewhat low unity Schmitter (1981, 294, 297).

42. Olson's logically admirable argument is too intricate to be recapitulated briefly here; I have attempted to summarize it in Rogowski 1983, 715-717.

43. On the other hand, five of the seven countries that Katzenstein studied (all except Sweden and Switzerland) had suffered foreign occupation or totalitarian rule during World War II; and three (Belgium, Denmark, and the Netherlands) had joined the European Community. Hence Olson would expect them to have a weak interest-group structure in the first place.

44. Alvarez, Garrett, and Lange combine measures of "density" and "centralization" into a single standardized "labor organization index" ("Government and Macroeconomic Performance," p. 353). The two measures are indeed strongly correlated ($r = .79$; centralization = $.055 + .012$ (density)), but of interest are the negative outliers: Italy, for example, is predicted by the regression to have a centralization score of .55 (density = 41), but in reality its centralization is .33. Britain's expected value is .59, its real one .43; and so on.

45. Wallerstein's best-supported OLS estimation (p. 491; $R^2 = .80$) is: unionized % of workforce = $83.4 - 6.35$ (ln work force in thousands) + $.30$ (Wilensky cumulative index of Left party participation since 1919).

46. Cf. Skocpol's (1985, 31) appreciation of this "path-breaking state-centered book."

47. Also highly influential was Perry Anderson's *Lineages of the Absolutist State* (1974). Theoretical debates among Marxist scholars were also influential at the time (see, for example, Krasner's preface to *Defending the National Interest*, xii) but have in recent years receded sharply from attention.

48. The manifesto of the approach, not only in the study of foreign policy, was chapter I of Krasner (1978).

49. On Japan, see Johnson (1982); on the NICs, inter alia Amsden (1985); Evans 1979; and Stephan Haggard 1990.

50. North's detailed treatments of Dutch and English economic success in the early modern period (pp. 152-157), however, supported a more nuanced view.

51. See for example Evans (1985b) and Migdal (1988). The most cogent definition of state strength was offered by Krasner (1978, 55-61).

52. Weir and Skocpol (1985); Katzenstein (1978, 323-330); Krasner (1978, 66).

53. The point had been conceded earlier by Krasner (1978, 58) and Skocpol (1985, 17-18). It is made forcefully, with respect to the developing countries, by Migdal (1988, xviii-xix).

54. A helpful introduction to the political applications of this work is Moe (1984). Other major contributors have been Coase (1960), Alchian and Demsetz (1972), Chandler (1977), and Klein, Crawford, and Alchian (1978).

55. See, for example, Williamson (1985, chap. 8) and sources there cited. These efforts at explanation carry an evident danger of functionalism – because the institution arose, it must have been more efficient – but the danger can be, and I think in the literature cited here has been, successfully resisted.

56. Gary Cox's (1986, 208-211) account of nineteenth-century MPs' delegation of quasi-judicial tasks (enclosures, rights-of-way) seems to me to be entirely in this spirit. For a more direct effort at application, see Weingast and Moran (1983).

57. Demonstrating such effects, of course, proves nothing about state autonomy. If, for example, PR leads to more stable government but itself is determined by some aspect of civil or international society, institutions still have little independent effect.

58. William Nordhaus (1975) had originally asserted that democratic governments stimulated the economy before an election, and often depressed demand immediately after, resulting in economic perturbations that were at best unhelpful to long-term economic growth. The most influential statement of the hypothesis was in Edward Tufts (1978). Valerie Bunce (1980) subsequently contended that the Soviet and East European governments also stimulated consumer-goods output to win support for new leaders immediately after a change of power. On closer inspection, as Alt and Chrystal (1983, chap. 5) observed in their pioneering and far-ranging textbook on *Political Economics*, evidence for these plausible hypotheses was almost wholly lacking for the democratic states; and Philip Roeder (1985) cast similar doubt on Bunce's assertions.

59. In parliamentary systems where a single party held a majority throughout the period of analysis, real disposable income increased in 78% of the election years but in only 39% of the non-election years; in systems with strong presidencies, the respective figures were 71% for election years, 47% for years without an election. In no other category of systems did the difference between election and non-election years exceed 10% (Powell 1982, 210).

60. See the discussion in Alt and Chrystal (1983, 47-49). An important intervening effort is Richard C. K. Burdekin and Leroy O. Lane (1988).

61. See, for example, Roubini and Sachs (1989); Alesina (1989, esp. 78-86); and Alesina and Summers (1991).

62. Two recent review essays, by Newman (1991) and Laitin (1991), discuss the theoretical issues with particular acuity. Also helpful is the critique by Donald L. Horowitz (1985, chap. 3).

63. Gourevitch (1979) argued that separatist sentiment was likeliest to arise in regions that were economically dynamic but politically subservient.

64. See the discussions in Newman (1991, 454-455), Horowitz (1985, 99-101), and Laitin (1986, 97-98).

65. Notable are David Laitin's (1986, 145-146) analysis of the "instrumental management" of Yoruba identity and Nelson Kasfir's (1979) discussion of the invented "Nubian" ethnicity in Uganda.

66. The clearest, or at least the most provocative, statement of the view came from the pen of Albert Breton (1964). Other prominent representatives of the "conflict" tendency include Fredrik Barth (1969), Howard Melson and Robert Wolpe (1970), and Walker Connor (1973).

67. For explicit statements of the assumption, see Breton (1964) and the sources cited in Horowitz (1985, 101-105). See also Newman (1991, 458).

68. The events that accompanied the partition of India in 1947 are a fair sample: see Larry Collins and Dominique Lapierre (1975, esp. chap. 13). On the other hand, the two World Wars spilled even more blood, for equally little tangible gain to individual participants.

69. One may of course quibble about whether the conflict in Northern Ireland is ethnic. The distinct ancestry of the two groups (the Protestants having originally been Scottish and English settlers) supports an affirmative answer. See the incisive discussion of the larger

definitional issue in Horowitz (1985, chap. 1, esp. 41ff). On the peculiar situation of settler elites, see Ian Lustick (1985).

70. As early as 1968, John Armstrong called attention to the advanced educational and occupational status of the Armenian population of the USSR: 94 of every 10,000 were full-time students in higher education (among Russians, the figure was 90; among Azerbaijanis, 75); 43 of every 10,000 were "scientific workers" (Russians, 33; Azerbaijanis, 24); and 30 in every 1000 were "specialists with higher education" (Russians, 21; Azerbaijanis, 18). Armstrong went on to note that, "like all mobilized diasporas, the Armenians arouse resentment....the Azerbaijanis and some other Turkic groups have a tradition of bitter animosity to Armenians" (Armstrong 1990, 25, 27, 60).

71. Laitin (1991, 162) notes that the Estonian national movement "was propelled...[in part] by the expectation of rapid growth under capitalism in an independent state."

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Global Political Economy

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Introduction

Global political economy is not unified by a shared theoretical approach or encompassing methodology. Rather, it is a collection of orientations, perspectives, theories, and methods addressed to understanding the relations between diverse political and economic phenomena at the global level. The world does not look the same from a bank office in Zurich, a *maquiladora* (border factory) in Mexico, a shantytown in Peru, a rice paddy in Sri Lanka, and a trade office in Washington, D.C. Academics are not above the fray. Neorealists, institutionalists, structuralists, Marxists, deconstructionists, and modern-day mercantilists frame the world differently too.

Without belaboring the obvious, let me briefly describe some of the key axes of disagreement: the actors; the dominant metaphor; and the constitutive principle relating politics and economics. Different theories presuppose the importance of different actors, or even whether actors in the ordinary sense are important at all (Galtung 1971). Thus, individuals, firms, interest groups, epistemic communities, technocrats, bureaucrats, states, and international organizations are variously posited as the central actors. Neoclassical political economy favors individuals and firms; Marxian political economy favors classes and production relations; dependency theory focuses on transnational alliances of capital (financial and industrial); and realist theory places states at the center.

Each approach is defined by a dominant metaphor and associated theoretical principles. Three such metaphors are the market, the polity, and class processes. Neoclassical economics places the market and voluntary exchange at the center. The portals to politics are guarded by the idea of market failure. Realism and neorealism expand on the role of the state and related ideas of power and domination. The connections with economics are controlled by state officials who set parameters to voluntary exchange and who seek to use wealth to enhance relative power. Politics structures the

realm of economics. Class processes are the domain of Marxian political economy. The production, realization, and appropriation of value are thematized. Politics appears either directly in production relations, in the power exercised by owners of capital and bosses (Marglin 1974), or in the ways in which the state advances the systemic interests of capital, i.e., those interests which capitalists cannot advance individually. Crossing the divides that separate these approaches is not an easy task.

Finally, there are different constitutive principles of global political economy, different ways of framing or organizing the connections between politics and economic forces. While they do not correspond in a precise way with theories of international political economy, the differences among them are important. One constitutive principle takes economics as the primary subject matter and looks at associated political activities. What is economic is trade, technology, debt, and what is political are the activities of bargaining, power, and collective problem solving in each arena, in short, the "politics of trade" (investment, debt, etc.) approach. This approach emphasizes the management of international economic problems.

A second constitutive principle conceives economics and politics as analytically coequal subsystems and theorizes about their interactions. Economics is variously conceived as markets, production, and wealth; politics as states, institutions, and power. Global political economy now involves theorizing about the relations between markets and states or power and wealth at the global level.

The third constitutive principle is that of reduction of politics or economics to the other. I do not use the term reduction the way Waltz does (1959, 1979), as the explanation of facts at one level by reference to the facts and relations situated at a different (lower) level. By reduction I refer to the subsumption or assimilation of facts of a political (or economic) nature by concepts of an economic (or political) nature. Reduction here is a conceptual but not a theoretical enterprise. It involves reclassifying and reinterpreting phenomena previously seen as autonomous. The public choice component of

neoclassical economics reinterprets politics as economic insofar as it focuses on political agents rationally pursuing their interests under political constraints. In a similar fashion, some Marxian political economists see politics in the very processes of production and exchange otherwise thought of as economic. "The economy is political" might be a Marxian slogan just as "the political is economic" might serve for public choice theory. This constitutive principle involves a meta-theoretical conception of how politics and economics connect that is very different from approaches seeking to explain economics and politics independently defined.

Any attempt to write about the entire field of global political economy faces a dilemma at the start. Does the author choose to write about as many approaches as possible or to develop a few selected themes in more depth? The former approach risks becoming a catalogue of approaches, sometimes very specific. The latter risks ignoring important work lying outside the controversial main theoretical traditions. Nevertheless, a choice is necessary. This chapter lies more toward the second horn of the dilemma. To convey a sense of the important theoretical controversies, it is necessary to provide detail and depth, even at the expense of horizontal coverage. This means that important literatures are omitted, including world systems theory, public choice, international coordination of economic policies, and post-structural theories. What follows is a critical discussion of four approaches: neoclassical, realist, liberal, and Marxian. In the conclusion, I present a broad assessment and point to areas where future research is needed.

The Neoclassical Approach

The central institution of neoclassical economic theory is the market, a system of voluntary exchange relations among legally independent property owners. The market coordinates wants and capacities of numerous individuals and conveys information about relative scarcities and relative preferences through prices. Within the market, the theoretical status of private agents is that of property owners who may seek to improve their positions through voluntary exchange.

In the neoclassical approach, political economy can be approached in a number of ways. One important connection is via the institution of the market and market failure. A central claim about markets is that they provide for efficient outcomes. This means that if property rights are well-defined, given any initial distribution of endowments and a set of preferences, efficient (Pareto-optimal)¹ outcomes will result. To the extent that markets allow wants to be satisfied (subject to

resource and technological constraints) there is little need for politics. However, markets sometimes fail, i.e., they systematically underproduce and overproduce certain goods.

When markets fail to satisfy the ends of private agents, the political process may provide an alternative. Market failure provides a major theoretical entry point between economics and politics. When the scope of a market failure involves more than one national jurisdiction, we have a stylized example of a "problem" for international political economy (Olson 1980, 10).

Classical and neoclassical economics provide a wide array of theoretical insights relevant for global political economy. Developments in modern neoclassical theory are suggestive of a number of ways to connect politics and economics besides market failure. There is the theory of commercial policy which has both normative and empirical components. Normative commercial policy is concerned with the best (optimal) policy to follow given the objective of maximizing national welfare. The empirical component, concerned with the actual (observed) forces impacting commercial policy, takes economics closer to interest groups and political institutions, phenomena more familiar to political scientists. The literatures on rent-seeking and directly unproductive profit-seeking activities explore distributional struggles rather than aggregate efficiency. Through the concepts of property rights and transaction costs, economists have helped to bridge the gap between the institutional worlds of economics and political science, dominated respectively by the market and the state. Finally, the public choice approach does not rely on markets and market failure for links between economics and politics. Instead, the assumption of rational, self-interested behavior is applied to a variety of non-market settings, including political ones.

All of the above approaches can't be reviewed here. Instead, I will focus on two categories of market failure -- international externalities and public goods -- and strategic trade theory.

Market Failure: Externalities and Public Goods

The concepts of externalities and public goods provide openings for integrating politics and economics at the global level. At the broadest level, an externality occurs when the action of one individual affects others who are not parties to the transaction. Externalities may be positive, e.g., when a breakthrough in pharmaceutical research quickly spreads to countries besides the originating one. Sometimes, arguments for the rapid growth of newly industrializing countries rest on implicit external benefits, e.g., "riding" the product cycle and

benefitting from the technological innovations of the most advanced. Sometimes externalities are negative. International pollution (rivers, acid rain), ozone depletion, global warming (which may help some countries), and overexploiting common property resources are examples of the negative force of externalities.

There are a number of responses to the existence of international externalities. One response, advocated by defenders of the market, is to let the affected parties bargain among themselves about how to distribute costs and benefits. If country B (or a group of countries) suffers from the external effects of country A's actions, B may try to buy out A's harmful behavior. An example of this is supplied by the Japanese suggestion that the Western, industrialized countries buy out Brazilian rights to deplete their own rain forest. This strategy is inspired by the theory which Coase set forth in his classic "The Problem of Social Cost" (1960).

Other responses are possible that require more in the way of active political and institutional change. The dispute could be submitted to an international tribunal, a board of arbitration, or a court such as the International Court of Justice or European Court of Justice. It is also possible to submit conflicts to national courts with the consent of the relevant parties.

Finally, if potential conflicts exist among countries, they may attempt to create economic zones or jurisdictions such that the domain of the zone is coterminous with the scope of the relevant externality. Perhaps more likely is the creation of some institutional arrangement to regulate the activities giving rise to externalities -- a treaty or agreement to control the export of inflation, pollution, unemployment, or a permanent international organization (World Health Organization, International Telecommunication Union, etc.).

Let us now shift the attention to public goods. Public goods have two characteristics: non-excludability and non-rivalness. The former property says that if a good (say defense, law and order, clean air) exists for anyone within a given area or jurisdiction, it exists for all. Within such an area (say a country), it is impossible to exclude certain people from the benefits of protection, even though only part of the population may have made an effort to provide for it. An urban taxpayer benefits no more or less than a mountain dweller who eludes the IRS. The latter characteristic, non-rivalness or jointness of supply, implies that consumption of a good by one will not decrease the amount available for others. Enjoying clean air and secure borders is not at the expense of others, at least up to a point where congestion creates interference. Economic efficiency alone, then, would seem to dictate that, once the good exists, additional members should be permitted to consume it. The

marginal benefit is positive; marginal cost is low or zero. Variations in excludability and rivalness allow us to construct a typology of goods and to locate the position of public goods.

The intersection of goods which are excludable and rival define conventional private goods, the main subject matter of economic theory. Public goods, by contrast, are neither excludable nor rival. Individuals within a given jurisdiction cannot be prevented from consuming the good once it exists regardless of whether they contributed to its production. Cell #3 (non-excludable but rival) identifies common property resources (the mineral seabed, the earth's atmosphere, high-seas fishing resources). Here the goods in question are subtractable (i.e., rival) but it is difficult, costly, or impossible to exclude. Cell #2 refers us to situations where it is possible to exclude but where the goods in question are non-rival.

There are numerous examples of international public goods: nuclear deterrence, as provided by the North Atlantic Treaty Organization (NATO), clean oceans, rules for the disposal of nuclear wastes, and an international program of public health to eliminate infectious diseases. Not all of the above examples exist in fact, and the degree of publicness of a good is a complicated empirical question. But a public good is one which, if it did come into existence, would have certain properties. In a sense, a public good is an extreme case of an externality, where nearly all the benefit is non-capturable, hence there is little individual incentive to provide it.

The supply (or undersupply) of public goods at the international level is a thriving field of research. General treatments are provided by Conybeare (1980, 1984), Frey (1984a, 1984b), Loehr and Sandler (1978), and Sandler (1980). Applied work has been carried out in many fields, including the North Atlantic Treaty Organization (Murdoch and Sandler 1982, 1984; Olson and Zeckhauser 1966; and O'Neal and Elrod 1989), trade (Conybeare 1987; Lipson 1982), and hegemony and stability (Kindleberger 1973; Keohane 1980; Lake 1991; and Snidal 1985).

Political scientists are fond of saying that economic exchange does not take place in a vacuum but have been woefully short of ideas about just how to infuse political content into economic transactions. The provision of international public goods, including the organization and institutions required for provision, should be thought of not as isolated instances of politics and international organization dotting the global landscape but as part of the foundation for that landscape. As Gill and Law remark, these goods are "...effectively the politico-economic conditions for, and aspects of, a liberal international economy..." (1988, 45). Similarly, David

TABLE 1

Types of Goods

		rivalness	
		yes	no
excludability	yes	private goods	spite goods
	no	common property goods	public goods

Lake treats public goods as part of the international economic infrastructure of international politics (Lake 1991).

There are many directions in which externality and public goods theory and research can go. First, one can investigate the circumstances under which international public goods are likely to be supplied or not. In realist and neorealist theory, the international system is represented as anarchic and as lacking in both centralized power and common values. This would not seem to be a promising starting point for public goods provision. Yet there are numerous ways for public goods to be provided short of a central government and sanctions. One facilitating condition concerns the distribution of power. To the extent that power is concentrated -- the limiting case would be a hegemonic power -- public goods can be more easily provided.

This simple proposition, linking the distribution of economic power and public goods provision, is the theoretical core of hegemonic stability theory. Just a little over two decades old, if we consider Gilpin (1971) and Kindleberger (1973) as the founders, this theory has exerted considerable sway in the United States. Part of the reason for its appeal is that it connects a fragment of realism (distribution of power, albeit economic power) with international public goods. Its promise cuts two ways at once: it provides a way of connecting realism and neoclassical economics; and it attempts to show how international governance is possible absent international government.

Research on hegemonic stability theory went through three phases. During the first phase (1971 to 1980), the theory was introduced and secured on the analytical foundation of public goods theory. A hegemon is important, Kindleberger (1973) argued, because a single leader helps to solve collective action problems. This is true whether acting in isolation or as part of a privileged group. The second phase runs from 1980 to the late eighties. It corresponds to the development of the original insights and to their empirical testing (by supporters, skeptics, and agnostics) in various issue areas, such as trade, monetary policy, and petroleum (see Keohane 1980, 1982; Cowhey and Long 1983; Lawson 1983; Evans 1989). The third phase overlaps with the second. It is distinguished in that the primary concerns are theoretical (Snidal 1985; Lake 1991). I will elaborate on these concerns below.

These theoretical developments were, and continue to be, important. Keohane (1984) untied the knot connecting hegemony and public goods (and regimes). International openness, regimes, and public goods were all possible without hegemony. Similarly, Ostrom (1990) made important headway demonstrating that international self-governance structures were possible

in "Global Commons" situations. Both scholars built upon and extended basic economic theory concerned with self-enforcing agreements.

In 1985 Duncan Snidal wrote an important article, "The Limits of Hegemonic Stability Theory." In it he argued that hegemonic stability theory was not so much right or wrong as underspecified. It did not provide enough (theoretical) information to be properly tested, information relating to the importance of size (absolute and relative) and the distributional implications of the theory.

Snidal's first move was to distinguish between two strands of hegemony, benevolent and coercive. The benevolent hegemon pioneers the creation of international public goods and pays for their costs, while others enjoy the benefits while free-riding. The hegemon is exploited at the same time that it also enjoys the public good. The coercive hegemon forces weaker states to make contributions (Snidal 1985, 589), that is, to pay for provision of the good. The two models are distinguished by the role of the hegemon and by different distributional implications. They are united in that both are concerned with collective goods (Snidal 1985, 590). Snidal's superb analysis leaves us with an important unresolved question. Does the coercive hegemon act against the interests of others compared to the *status quo ante* or does it "merely" assure that others pay for their share of the gains?

The most recent attempt to extend hegemonic stability theory is provided by David Lake (1991). In "The Theory of Hegemonic Stability: An Interim Report," Lake correctly points out that Snidal's distinction between coercive and benevolent models has to do with the distribution of gains. All parties gain. This is almost axiomatic since public goods are at issue. Weaker states gain less in the coercive model, more in the benevolent model. But everyone gains. The main issue concerns the ways the dominant state uses (or does not use) its power. From Lake's standpoint, the critical point is that interests (preferences) aren't questioned in both models. Everyone wants the same things. What differs is ability to affect provision and ability to engage in opportunistic behavior.

Lake moves the discussion forward by distinguishing between neoliberal models of hegemonic stability, which are based on common preferences, and neorealist models based on different preferences over outcomes such as international trade and exchange. While the full import of this distinction cannot be discussed here, it clearly broadens the terms of debate. It does so not by diluting concepts but by refining our subject matter in more precise ways. Lake's approach allows us to see divergent behaviour as resulting from pursuit of conflicting interests as well as opportunities.

A second direction in which research could go involves investigating free-riding and burden-sharing. It is a popular concern of security analysts working on NATO, at least this side of the Atlantic. Since Olson and Zeckhauser wrote their pioneering article, "An Economic Theory of Alliances" (1966), free-riding and burden-sharing within alliances have inspired much follow-up research (e.g., Knorr 1985; Murdoch and Sandler 1982, 1984; O'Neal and Elrod 1989; Oppenheimer 1979; and Russett and Sullivan 1971).

Research on burden-sharing within NATO is marred by a weakness sometimes characteristic of the literature on public goods as a whole. This has to do with the difficulty of unambiguously identifying a public good. What is it about NATO, or some outcome associated with it, that is public in nature? Unless a clear answer to this question is forthcoming, the rationale for the burden-sharing cum free-riding perspective breaks down and a model based on the pursuit of separate interests becomes more appropriate.

The generally accepted answer to what is public about NATO is nuclear deterrence. Without doubt, NATO's role in this regard displays attributes of public goods. But the empirical world is more complicated. Under the doctrine of massive retaliation, deterrence was more public (i.e., applied to all NATO members with less discrimination) than after McNamara's Ann Arbor speech in 1962. Here the concepts of flexible response, graduated deterrence, and damage-limitation came into play. The distinctions between "strategic" and "theater" weapons caused further worries among Europeans and led to charges that Europe would become a combat zone for "limited" war while the U.S. and Canada would remain secure. Without trying to resolve the historical quarrel, the conceptual point remains -- the identification of public goods is no easy task. This requires us to do a certain amount of conceptual and empirical spadework before constructing models of burden-sharing and shirking. This work is inevitably of an interpretive nature, requiring us to think not only of force structures but also of plans and strategies, intentions, geopolitical situations, and historical contingencies. In general, the further we move from hardware to software (e.g., from missiles in silos to computer programs for targeting), from overall force structures to strategy and tactics, the greater the ambiguity about what is public and private.

The model of Conybeare and Sandler (1990) is more realistic in that it makes an effort to distinguish and then theorize about pure public goods (deterrence), impure public goods (damage-limitation), and private goods (infrastructure). Building on these distinctions, they construct a joint product model of alliance behavior as resulting from three sets of forces.

The preceding point about public goods has to do with the appropriate conceptual categorization of the

subject matter of global political economy. The same point could be applied to issue areas besides security, for example, trade, finance, monetary relations, the resources of the oceans, and so on. Without a precise understanding of the ways that international trade, investment, and deep seabed mining display public and private characteristics and are rival or excludable or both, we can make little progress theoretically. Before we ask our concepts to do theoretical work (i.e., to explain variation in other concepts), they should be unambiguously connected to empirical referents.

The third and last point concerns theoretical work that needs to be done to advance our understanding of the role of externalities and public goods in global political economy. At times, the relationship between particular issues (e.g., trade), game structures (e.g., prisoners' dilemma), institutional forms (e.g., regimes), and outcomes (e.g., cooperation, allocative results) is very confused. Is free trade a public good (as many hegemonic stability theorists assume), a private good, or a mixed good, offering private benefits as well as important externalities? What are the consequences of conceptualizing trade as a public good or prisoners' dilemma? Does it make a difference for our theories?

Basic theoretical work is needed to clarify the differences among different incentive structures (or games) and to draw out their theoretical implications. Important work has already been done clarifying the differences among games of assurance, prisoners' dilemma, chicken games and others (Stein 1982). Duncan Snidal (1985) has drawn out the consequences of differences in the structure of coordination versus prisoners' dilemma games for cooperation and international organization. Conybeare (1984) has probed prisoners' dilemma and public goods conditions and argued that conceptualizing an issue (such as trade) one way or the other has consequences for theory and policy.

Other scholars are attempting to link particular environmental and incentive configurations to other variables, especially to institutional structure and cooperative-uncooperative outcomes. Elinor Ostrom and her colleagues (1992) have shown that binding cooperation is possible even without central government sanctions. And Lisa Martin (1992a) has theorized the importance of different interest configurations for the form of international institutions.

In particular, Martin has demonstrated how the utility of multilateral norms and institutions varies with the type of problem faced. Robert Keohane (1990b) argues that international institutions are sometimes exclusive by nature (NATO, the EC), sometimes conditionally open (G.A.T.T.), and sometimes unconditionally open (the World Health Organization). He argues a correspondence between degree of openness

and the underlying configuration of interest. Exclusive institutions revolve around distributional issues and are often motivated by rent-seeking. Conditionally open institutions are usually a response to free-riding and unconditionally open structures predominate under coordination incentives (1990, 751-53). There is little reason to exclude countries from membership in the World Health Organization (WHO). A common understanding of disease, medical technology, and treatments benefits all. By contrast, membership in the G.A.T.T. carries a price tag, since non-members could exploit collective benefits while pursuing autonomous commercial policies themselves.

Strategic Trade Theory

Strategic trade theory, or the "new international economics," is a body of thought about how firms and governments think and act with respect to trade in the modern world (Brander and Spencer 1981, 1985; Grossman and Richardson 1986; Krugman 1986, 1987; and Stegemann 1989). Orthodox trade theory, in both its classical (Ricardian) and neoclassical (Heckscher-Ohlin-Samuelson) variants, suggests that governments should do very little beyond providing a healthy domestic and international environment for specialization and exchange. In addition, prescribed governmental policy is non-strategic. There is a best trade policy to follow regardless of what other countries do. With factor endowments, tastes, and technology specified exogenously, the broad lines of comparative advantage are settled. Specialization and exchange take place to exploit comparative cost differences. The assumption of a competitive market means that each firm is a price-taker, that no firm can affect the output and pricing behavior of other firms, and that any "rents" existing in the system will be quickly competed away.

Until recently (10-15 years ago), theories of international trade were organized around the assumption of perfect competition (Stegemann 1989, 73). As Lake points out, neoclassical theories of trade are characterized by "...constant returns to scale which are fully internalized by the firm" (1991, 18). These assumptions discouraged economists from examining the effects of externalities, dynamic phenomena such as learning effects, and scale economies.

How then do we account for the new departures in trade theory? Perhaps empirical observation that some firms behave in a strategic fashion provided part of the impetus. More likely, as Stegemann argues, is that the literature on industrial organization found its way into trade theory. Industrial organization deals with the consequences of different sectoral characteristics, such as size, concentration, capital intensity, mobility, etc. This

literature becomes part of the theoretical foundation of strategic trade theory.

Before proceeding, one point should be clarified. Analysts sometimes attribute different motives (objectives) to governments in strategic trade theory. States are thought of as predatory or motivated by relative rather than absolute gain (wealth-maximization). States may in fact behave in predatory fashion but not necessarily because their goals have fundamentally changed. The basic goal is still national wealth-maximization. Instrumental preferences may change to accommodate different ways of reaching the basic goal. Stegemann is quite correct to point out that national welfare is central to mercantilist, neoclassical, and strategic views of trade. What distinguishes strategic trade theory is the view that the classical harmony between national and global welfare may break down (Stegemann 1989, 80).

A common thread running throughout the literature on strategic trade theory is that market imperfections of several kinds allow welfare-improving (national -- not global) state intervention (Sherman 1991, 1). These imperfections include monopolies, increasing returns to scale, learning-by-doing effects, and spillovers generated by research and development. Brander and Spencer (1981) initiated research in the area of modeling trade policy in a market supplied by a foreign monopolist while Krugman's work (1986, 1987) focuses more on externalities associated with learning, scale economies, and research and development. These developments are important challenges to neoclassical trade theory in that they raise the possibility that profit-shifting (as opposed to wealth-producing) activities may be possible. But it is precisely at this point that the academic sparks begin to fly.

Let us explore in more detail how strategic trade works. In addition to profit-shifting activities, strategic trade has three other characteristics. These have to do with the creation of comparative advantages, the role of governments as actors, and the importance of sectoral policy, as opposed to macroeconomic policy.

First, "the creation of comparative advantage." Just what does this phrase mean? This is best answered by going back to orthodox trade theory as the reference point. According to this theory, firms within countries face constraints established by forces outside themselves. There are a certain amount of natural resources, a labor pool, and a stock of capital inside the country. Although the amount within any category is not unchanging, there is little short-term variation, and this is outside the control of economic and political agents.

This view of fixed and naturally given comparative advantage is increasingly being replaced by a more fluid conception in which relative advantages can

change significantly in the short-run in response to governmental and firm strategies. The idea of human capital (or skilled, knowledge-intensive labor), advanced technology, and research and development all provide cases in point. Labor can be looked at in terms of the physical stock of men and women of working age in a country and can be categorized in terms of different levels of strength, skill, abstract knowledge, and so on. Alternatively, labor can be viewed as a resource subject to transformation by governmental and corporate policies. One way of transforming the work force is through educational policies, either to raise the literacy level in a country or to target specific vocational skills. A variety of work-training programs provided by the public and private sectors might also be relevant. By pursuing such policies, particular countries could acquire a comparative advantage in specific skilled labor categories.

Much the same argument could be made for technology, research, and development. The idea that national technological advantages are temporary and prone to dissipate under pressure of market solvents can be increasingly questioned. In addition, research and development, because they create significant, nonappropriable external benefits, are more and more subject to strategic manipulation both at the level of the firm and governments.

States may play a central role in strategic trade, though this is one of the most controversial aspects of the strategic trade literature (Krugman 1987, 139-40; Stegemann 1989, 89-95). In orthodox trade theory, the role of the state is minimal. Of course the positive theory of trade policy has long recognized protectionist policies, e.g., the demand for protection on the part of failing, uncompetitive industries. The exceptions in normative trade theory allow for protecting "infant industries" and optimal tariffs. However, this permission is carefully qualified, and the burden of proof is placed on those arguing intervention. All in all, governments play only a small part in international trade.

The key actors in strategic trade theory are large, oligopolistic firms and governments. In strategic trade theory, governments are not only interventionist in an empirical sense. It is also argued that sometimes they should be so in the sense that intervention can increase national welfare. Interventionist trade policies may be the rule in those industries where there are large economies of scale, learning effects, and spillover effects from one industry to another. Scale and learning effects are important because they suggest advantages to those firms which get there first, establish their positions, then engage in preemptive market strategies (Gilpin 1987, 217). Spillover effects are important because they suggest the interlocked nature of the benefits of industrial production. Some of these benefits are captured (as income); others escape as externalities. A strategic trade

theory would try to assure that, to the extent possible, these spillovers transpire among national firms and industries.

The role of government intervention to correct for externalities is neither new nor unique to trade. The justification for government intervention in profit-shifting activities is different. In the standard model of duopoly, we have two producers in two different countries with each producer exporting to a third market. In this model, developed by Brander and Spencer (1981, 1985), firms behave like Cournot duopolists, each setting its export level on the basis of what the other does. The Cournot equilibrium therefore corresponds to the Nash equilibrium, i.e., the best one can do given what the other will do (Stegemann 1989, 81).

A sophisticated firm can question treating the behavior of a competitor as given. Why not assume an independent stance, and make the other adjust? In other words, there may be first-mover advantages. But the problem is that this strategy makes sense for both firms, and therefore for neither. Without a way of making its strategy credible, both firms will be nudged back into the Nash equilibrium. Firms by themselves cannot provide credible commitments because the other firm can simply cut prices and ride out the attempt to garner larger profits. The ultimate justification for government intervention is that governments can provide such credible commitments. By paying a subsidy to a national firm to enter a market first, governments can deter entry and shift profits to national firms. Since governments cannot be put out of business, at least not so easily as firms, their commitment to underwrite the firm is more credible (see Stegemann 1989, 81-84; Krugman 1987, 134-37).

Finally, strategic trade implies industrial and sectoral policy as opposed to exclusive reliance on macro policy. Macro policy includes money supply, exchange rate policy, balance of payments adjustments, and demand management. These policies are not intended to affect relative incentives to produce across different sectors of the economy. A focus on strategic trade, however, implies favoring some sectors over others, particularly those producing the highest rents, i.e., returns above those possible if resources are employed in the next best way. Thus, selective subsidies to leading edge industries, export promotion of particular sectors, low-interest loans, and differential tax-breaks for capital depreciation in strategic sectors might all be employed as policies consistent with a strategic trade outlook.

It would still be this side of understatement to say that strategic trade theory has sparked heated controversies within the academy and in policy circles. A survey of world trade conducted by *The Economist* carried a caption that read "Krugman proposes, Bhagwati disposes" (September 22, 1990). Bhagwati has led the

charge, or rather countercharge, advancing the plausible proposition that strategic trade theory is the ideology of protectionist forces.² This leads Bhagwati to worry that resistance to the supply of protection in the executive branch of the U.S. government will be weakened (Bhagwati 1989, 17).

Apart from these opening salvos, criticisms of strategic trade theory have centered on four themes: (1) infrequency with which the assumptions of the new trade theory are satisfied; (2) lack of robustness of the theory; (3) the stringent informational requirements; and (4) encouragement of rent-seeking.

While many economists recognize the widespread existence of learning effects, technological spillovers, and economies of scale, some (perhaps quite a few) are reluctant to admit the pervasiveness of firms with sufficient market power to alter world prices. Yet this is just what the theory requires for firms to pursue strategies that deter market entry, alter the terms of trade, or affect global market shares. Thus it is not clear how widespread the existence conditions of strategic trade theory are (Haberler 1990, 25). Yet the scope of these conditions determines the applicability of strategic trade theory to commercial policy.

A related issue concerns the robustness of the theory. Apart from the issue of how often the assumptions hold, we need to ask how continuous departures affect the performance of the core theoretical propositions. Presumably, classical and neoclassical trade theory are quite robust, in that conclusions hold even under large departures from the conditions contained in the theoretical assumptions. Just the opposite is the case, argue the critics, with strategic trade theory. A number of papers (Eaton and Grossman 1986; Grossman 1986; and Markusen and Venables 1988) attempt to demonstrate lack of robustness.

A third criticism centers on the stringent informational requirements of strategic trade theory. Political agents engaged in commercial policy need to know about market structure, reaction functions of other firms, the nature of scale economies, learning by doing effects, and the short- and long-term consequences of research and development spending. Regarding scale economies and learning-by-doing, it is not enough to know simply that such effects exist, and what the directions are, but what some crucial parameters are, such as the slopes of the curves, how steeply they rise, when they reach their plateaus, etc. On this general point, critics of strategic trade theory join industrial policy critics in arguing that sectoral targeting and interventions are difficult and almost always produce results inferior to the market.

Finally, critics argue that commercial policy based on strategic trade theory will encourage rent-seeking and revenue-seeking (Krueger 1974; Bhagwati

1983). Both activities (the former is a struggle for the windfall premiums associated with import quotas; the latter is the counterpart for revenues) involve socially wasteful expenditures of resources (if only the labor time used up in lobbying) that provide incomes by diverting wealth from other sectors of the economy.

The upshot of these criticisms is that there is a large gap between strategic trade theory and normative commercial policy. Even those who recognize the merit of strategic trade ideas, such as Krugman, are very cautious about the proper policy conclusions. The consensus seems to be that the conditions supporting a successful strategic trade policy are fragile. The costs of organizing are high and, if interest groups are part of the political process behind the formulation of policy, firm- or industry-specific rents rather than national welfare are likely results. Information requirements are high, governments are not likely to possess the information, and, if they do, they are not likely to act purely on informational considerations. In the hurley-burley world of politics, interest groups will be motivated by a variety of short- and long-term goals rather than by national welfare. Krugman perhaps understates when he says: "Governments do not act in the national interest, especially when making detailed microeconomic interventions" (Krugman 1987, 142).

Numerous empirical studies of strategic trade theory have been carried out but will not be reviewed here. For examples and surveys, see Cline (1986), Gasiorek, Smith, and Venables (1989), and Baldwin (1988). The general finding is that trade behavior in particular sectors does not reflect strategic behavior and when it does, it does not result in net gains. But these studies are equivocal and do not allow disciplined inferences about many propositions in strategic trade theory. Some of these studies virtually equate strategic trade with protectionism, an equation that is not at all forced by the theory (Krugman 1987). Other studies rely on weak designs. Demonstrating that automobiles or computers do not reflect the principles of strategic trade theory is not conclusive, unless one can argue that these sectors are "crucial cases." Instead, designs that employ multiple cross-sectoral variation in properties such as scale economies, externalities, and learning-by-doing effects are required. A more promising approach is provided by Milner and Yoffie (1989) who use strategic trade theory to account for patterns of conditional protection.

To the extent strategic trade theory becomes recognized as playing a central role, it erodes some obstacles to integrating politics and economics. An economic theory of trade built on the premises of imperfect competition, oligopolistic market structure, externalities, and the need for non-economic actors to back up (make

credible) strategies of firms, provides obvious entry points for politics. Political scientists have long questioned neoclassical assumptions about market competition, low (or nonexistent) barriers to entry, the assumption of no long-term rents, and, in general, the entire worldview that decisions about the international division of labor are best left to the market.

While the primary benefits of strategic trade theory are likely to lie in trade theory itself, there is considerable potential for cross-fertilization between development theory and trade theory. At a minimum, innovations in strategic trade theory offer a fresh look at infant industry arguments and specialization in accord with naturally bestowed comparative advantage.

Finally, strategic trade theory is very suggestive of aspects of dependency theory, an association which is likely to be strenuously resisted. Although the methodological tools of strategic theory are rejected by dependentistas, in favor of historical-structural analysis, similarities at the level of perceived structure and operation of the international system are considerable. Cardoso and Faletto (1971, 1978) have long viewed the world system as oligopolistic rivalry among firms and states, have never believed that politics could be separated from economics, and have accepted the long-term, self-reproducing superiority of industries in dominant countries as a fact of life. This last point is dependency theory's way of saying that long-term rents exist (or continuously reproducible rents, within and across sectors). Furthermore, Brewster (1973) and Sunkel (1973) firmly believe in the importance of intersectoral linkages (spillovers) while Mytelka (1978) and Gereffi (1978) emphasize the continuing importance of economies of scale, learning by doing, and the role of the knowledge industry in maintaining technological superiority.

I have argued that market failure is the master concept linking politics with economics within the neoclassical tradition. The reach of this concept is broader than is sometimes supposed and could be extended to include not only externalities, public goods and strategic trade, but also international property rights and international organization. Insofar as markets do not spontaneously produce their own existence conditions — secure property rights, communications and infrastructure, and minimal global institutions — a failure of sorts exists. Nevertheless, the market failure approach is limited as I hope to show by remarking on externalities and strategic trade.

One limitation is that the externalities paradigm cuts a narrower path to international political economy than one might think. Since the world is very interdependent, one might think that externalities are correspondingly high. While there is some truth in this, the political relevance of externalities is diminished in

two ways. First, not all effects transmitted from one country to another are externalities. International externalities define a subset of international interdependencies. If a firm in country A produces a product that puts a firm in country B out of business, and this result was effected through the market, there is no externality. Similarly, if adverse terms of trade or volatile commodity earnings are the result of movements in supply and demand, they are not externalities either. To avoid misunderstanding, the fact that externalities have a clearly limited domain is not a conceptual defect. My point is simply that the externalities paradigm does not include much that may otherwise be considered political. Much work in international political economy concerns how political phenomena (e.g., power bargaining, autonomous state actions) affect market processes or how the market creates outcomes that are politically contested by market and non-market players. In *Structural Conflict: The Third World Against Global Liberalism* (1985), Stephen Krasner takes as an important theme the political contestation over markets and market outcomes themselves. Both Gruber (1992) and Krasner (1991) remind us that much of the subject matter of international relations has to do with relative capabilities and winning and losing — not with overcoming Pareto-suboptimality.

Realist Approaches

According to many accounts, realism, and more recently, neorealism³ is the dominant approach in international relations theory, though not necessarily in global political economy. In "Thucydidean Realism," Michael Doyle identifies a number of surveys in which evidence is given for this proposition (Doyle 1990, 223). In *The Power of Power Politics*, John Vasquez reports that over 90% of the hypotheses tested by behaviorists are realist in origin (Vasquez 1983, 162-170). However, since its central research agenda is guided by power, anarchy, and security, realism's reach into the realm of international economics is uncertain. In this section I will show some of the ways politics and economics are connected within realism.

I define realism in terms of its theorization or assumptions about (1) relevant actors (states), (2) environment (anarchy), (3) capacities ("powers") of the units, and (4) motivations. It seems that something must be said (or assumed) about all four points before the realist system can be set into motion. In brief, the central actors are nation-states. Non-state actors are downplayed or theorized as dependent entities; for example, multinational corporations are seen as operating in a space defined by state policies. Power, however defined, identifies the capabilities of states. Because

relative power is so crucial to the success of many other ends, power is also a goal. The environment within which states interact is anarchic, i.e., without centralized rule and power. And finally, with regard to motives, states are variously seen as pursuing security, power, and wealth. Security, especially survival, is the most important motivation and is treated as a scarce good, the improvement of the security of some states coming at the expense of others. When conflicts are acute, security can be thought of as dominating other goals, but when security concerns decline, a variety of other goals may push toward the foreground.⁴

Realist theory is structural theory, i.e., theory about the importance of third-image factors. For Kenneth Waltz (1979), there are three central components of international structure. First, there is the ordering principle, anarchy, which defines how units relate to one another. Second, there is the distribution of capabilities, i.e., how power is spread across states. Third, there is the differentiation principle. Within states, differentiation of functions is high; across states, it is low. Classical realist theory tends to take anarchy and states themselves as givens and looks at consequences of exogenous variation in the distribution of capabilities.⁵ How does the change in the power of one country, either through domestic growth processes or alliances, affect the preferences, opportunities, and policies of others? Faced with such changes, will a state choose to ally with the weaker grouping (balance), join the stronger alliance (bandwagon), stay neutral, pursue a divide-and-rule strategy, or increase its arms (Walt 1987)?

Part of the difference between realists and nonrealists lies not in what is excluded or included, but in what each takes as exogenous versus problematic. With respect to what is fixed and what is allowed to vary, world order and regional integration theorists are outside the realist mainstream, since it is precisely the idea of anarchy that they make problematic. Integration theorists often take distribution of capabilities as a given and look at the way in which anarchic structures and the nature of states can be transformed. Successful regional integration constitutes a mitigation (or perhaps transformation) of anarchy. The work of Karl Deutsch on national and international community building focuses on "core areas," as reflecting the distribution of capabilities (including administrative and social capabilities -- not just military ones). Deutsch's dependent variables are pluralistic and amalgamated security communities, concepts defining different degrees of centralized decision making within a regional system where the expectation that violence will be used to address conflicts has been reduced. States within Deutschian security communities are not asocial, perpetually insecure, and necessarily threatened by

relative power changes (Deutsch 1953; Deutsch et al. 1957).

Realism and Economics

Security questions lie at the center of the realist research program. The central issue concerns the relationship between the international distribution of power and state behavior. How do states, within anarchy, respond to power changes? Thus diplomacy, alliances, divide-and-rule strategies, and arms races are important components of realist research. With the exception of the focus on arms races, where national military capabilities are expressed as a reaction function to the capabilities of others, national power is fixed by forces outside realist theory. The explanatory task is to account for how power combines across countries and the implications for perceptions of insecurity, conflict, war, and peace.

Every theory must take some things as exogenous. This allows us not to have to explain everything at once. Given the interest in explaining war and peace, realism does not have to take on the additional burden of explaining variations in national power. Yet, to connect to economics, realism must move beyond security issues narrowly defined. In the remainder of this section, I explore four theoretical links: (1) the use of state power to structure international economic exchange; (2) the effect of international economic exchange (especially asymmetries in trade patterns) on the power of states; (3) the effect of the systemic distribution of military power on economic exchange; and (4) finally, the connection between realism and institutions.

State Capacity to Structure Economic Exchange

Realists stress the capacity of state agents to structure exchange relations at the international level. In this they differ from neoclassical economists who see exchange dictated by relative factor proportions and cross-national preference patterns -- and interdependence theorists, who see technology as the basic determinant of exchange. In either case, states are faced with international exchange patterns that are given, outside their control. Realists attempt to turn the tables, arguing that what others treat as independent is really dependent. Disagreements center on which ways to draw the causal arrows -- not on which variables are salient.

There are numerous specific ways in which this argument is made. The most general claim involves the rejection of the twin proposition that technology and economics are autonomous and that their growth erodes state sovereignty. As Thomson and Krasner (1989)

assert, "Realists argued first that growing interdependence has been a function of political power and political choice, not of exogenous technological change" (1989, 196). Further, states are the necessary, if not sufficient, condition for international exchange, since it is states that establish secure property rights across countries. Without a medium of exchange, convertibility, and secure rights for foreign investments and goods in transit, international trade could hardly have taken root (Thomson and Krasner 1989, 197-98). States have "consolidated sovereignty," to use Thomson and Krasner's phrase. States thus exert control over their economies, including economic activities in foreign countries, in ways they didn't previously. Vernon and Spar point out that U.S. courts did not consider foreign activity of U.S. firms as subject to U.S. law before 1909 (1989, 114).

A corollary point is that the mix of autonomy-interdependence is not a technological given, but is influenced by state preferences. Realists refer not just to extreme cases (Albania, Burma, and the People's Republic of China) but to any country concerned with minimizing vulnerabilities and risks at the same time that it strives to enjoy the benefits of international specialization. While autonomy is one of the traditional concerns of dependency theory, scholars demonstrate that the tradeoffs also face advanced countries. Vernon and Kapstein worry that over 20% of the ingredients of U.S. weapons come from outside the country (1991, 5). In the same issue of *Daedalus*, Moravcsik argues that military planners have traditionally supported free trade, perhaps surprisingly, but the rationale is that trade helps to secure financing from abroad and it allows economies of scale not afforded by the domestic market (1991, 23-25).

Finally, apart from the ratio of trade to gross national product, states also play a role in affecting trade partners and the composition of trade. The issue of trade partners is taken up in the discussion of security externalities. Here I just mention the case of the former Soviet Union, Eastern Europe, and the Council on Mutual Economic Assistance (CMEA). If technical forces were adequate to explain patterns of exchange, CMEA countries would not have formed a separate world system largely cut off from the advanced capitalist world. While the extent of regional autarky is controversial (see Chase-Dunn 1982), Szymanski argues strongly that Soviet participation in the capitalist world economy was severely limited so as to reduce vulnerability. Trade was designed to cover shortfalls or to provide non-essential items. Economic exchange was thus similar to the long-distance trade in luxury items that Wallerstein discusses under pre-capitalist systems (Szymanski 1982, 58, 66).

Finally, states are sensitive to the product lines in which countries specialize. Strategic trade predicts that states will foster industries associated with large

externalities. Different products vary in associated wages, profits, demand-elasticities, spinoffs, and value added. Also, most states have a food and raw materials policy. After World War I and up to the common agricultural policy of the European Community (EC), most Western European countries had independent agricultural policies, despite the large gains to be had by cross-national specialization in food and manufactures. One suspects that comparative cost differences between food and industry were much larger than those across different manufacturing sectors.

International Exchange, Power, and Vulnerability

The preceding section dealt with the ways in which states structure the realm of international economic exchange. Exchange patterns were the dependent variables. The second realist theme turns the tables and asks how international exchanges affect state power and vulnerability. National power can be thought of as constituted by two kinds of capabilities: attribute and exchange capabilities. Attribute capabilities refer to properties of states, or the national economy or society, e.g., size of army, air force, navy, gross national product, etc. Exchange capabilities refer to resources that flow out of the connections among countries. These connections are varied and might include trade, aid, technology flows, foreign investment, financial movements, debt, migration, and knowledge transfer. The general idea is that economic exchanges confer advantages (or liabilities) on countries and to the extent that such ties are asymmetric and not easily replaced, they provide a source of leverage, hence power.

Albert Hirschman (1945) was one of the first to recognize fully the political potential implicit in classical arguments about the gains from trade. The "gains from trade" describe the incremental consumption that occurs through trade compared to the baseline of domestic production. Formulated in opportunity cost terms, it becomes clear that the classical concept "gains from trade," when inverted, approximates the vulnerability of a country to another (Hirschman 1945, 18).⁶

Hirschman opened a conceptual door left slightly ajar by Ricardo. The opportunity costs idea, concretized in the form of trade among countries, points the way to a connection between power and exchange theory. If power implies the capacity to reward and punish, and if exchanges define flows of valued objects, numerous policy implications result. If A is dependent on B, if the costs of shifting, substituting, or doing without are also high, B will have power over A.

Hirschman's insights were suggestive of refinements in thinking about power (Baldwin 1979; Caporaso 1978). Bargaining power may be defined as the ability to

control the outcomes of specific events. A different order of control is suggested by "structural power," the power to manipulate the choices, capabilities, and payoffs that other actors possess. As Hirschman put it, the purpose of these strategies is not immediately to exert one's will in a specific contest, but rather to shape the matrix of incentives in such a way as to "...make the pursuit of power a relatively easy task" (Hirschman 1978, 46).

The economic strategy of Germany, during the Third Reich, of creating "exclusive complementarities" with Eastern and Central European countries, attempted to put these countries in a dependent position. Similarly, when the Soviet Union sold oil and other natural resources to Eastern Europe below world market prices, it did so in part to increase the opportunity cost of changing suppliers (see Marrese 1986). And when twelve countries form an economic union with internally free trade and common external tariffs, they deprive nonmembers of the power to exploit divisions among them that are implicit in all social dilemmas.

The insights pioneered by Hirschman have not been vigorously pursued by realism. Yet, a diverse body of scholars is working on the connections between international exchange and power. They include dependency theorists (Cardoso and Faletto 1978; Dos Santos 1970; Sunkel 1973), national security strategists (Knorr and Trager 1977; Knorr 1975), peace researchers (Galtung 1971), and international political economy analysts (Strange 1988, 1989, 1991). In addition, there is a substantial amount of research and theory development on economic sanctions, which in many ways is a logical extension of international influence structures (Doxey 1972; Galtung 1967). The most comprehensive and insightful books in this area are Baldwin's *Economic Statecraft* (1985) and Martin's *Coercive Cooperation* (1992b).

While this literature is impressive in scope, much of it is not consistently motivated by what I take to be Hirschman's central project -- the development of a theory of structural power. In *National Power and the Structure of Foreign Trade* (1945), Hirschman sets out to construct a theory of international influence networks that is importantly different from power in realist theory. Considered as a capability, realism treats power as an attribute of a country or distribution of countries. Bipolarity and multipolarity are distributional parameters.⁷ Considered as a behavior (the activity of power), realism treats power as a relation. Hirschman wants to extend the capability aspect of power by pursuing the implications of the ways (mostly economic) in which countries are connected (by trade, investments, technology, and so on). To the extent that these relations identify gains which, if lost, could not easily be recouped

through partner or product substitution, they identify vulnerabilities. Vulnerabilities can be exploited; they are sources of power.

Power for Hirschman is not an attribute or distributional parameter. It is more tied to network and exchange theory than to attribute theory. The threat of using power is a threat to withdraw something rather than to deliver something. Understanding international influence networks is an important project that will help to round out the realist conception of power.

Systemic Distribution of Power and Economic Exchange

The first realist theme attempted to connect state power and exchange by referring to state actions aimed at limiting or expanding the realm of exchange. The state acted as a sort of architect creating barriers and opportunities to exchange. But what motivates states? What inclines leaders to seek liberalization or protection, autonomy or interdependence, overall efficiency or minimum vulnerability? One answer lies within domestic politics. States are agents of society and pursue goals in line with internal domestic pressures. A second answer lies at the structural (third-image) level. Since realism is a theory about the importance of international structure, I pursue the structural vein.

Two elements of structure are anarchy and the distribution of power. Both Waltz (1979) and Grieco (1988, 1990) have written extensively about the dampening effect of anarchy on international exchange. Within anarchy, states must be sensitive to relative as well as absolute gains. The distribution of gains -- not just total gains from trade -- must be brought into the calculation of whether and how to specialize and cooperate.

While anarchy doubtlessly produces levels of exchange below the prediction based on world government, by itself it is incapable of explaining important variations in levels of exchange and exchange partners. It explains why the average level of exchange is low and why profitable opportunities for exchange go unrealized. Anarchy is too global a property to explain temporal and partner variations. Without appealing to attribute similarities (e.g., sharing democratic principles, common ethnic or religious membership), structural theory can point to the influence of changes in international economic or political structure as determinants of exchange.

Two such international structural theories are hegemonic stability theory and the theory of security externalities developed by Joanne Gowa (1989). Hegemonic stability theory was discussed in a previous section and won't be elaborated here. Its major thesis is that the systemic distribution of power (economic power)

affects the provision of international public goods. One such good is a free trade regime. Hegemonic stability theory attempts to explain trade liberalization – hence the level of exchange – by the degree of concentration of power.

For hegemonic stability theorists, the *systemic* distribution of power is confined to those who share purposes regarding public goods. After World War II, hegemony applied to countries within the advanced capitalist world. Shares of resources were calculated as fractions of the total resources within the Organization for Economic Cooperation and Development (OECD). The worldwide distribution of political-military power was bipolar at the same time that the U.S. was hegemonic within the OECD.

Joanne Gowa, in "Bipolarity, Multipolarity, and Free Trade" (1989), addresses the important issue of the global distribution of power and its relationship to trade. She criticizes the view that the prisoners' dilemma structure is responsible for the scarcity of open international markets. Instead she focuses on the security externalities (roughly the consequences of trade for defense and war-making) that result from open trade arrangements (Gowa 1989, 1246).

Gowa's theory joins a fragment of Hirschman's argument (1945) about the supply effects of trade to the military benefits of trade on trade partners. Because of these external benefits, countries cannot be indifferent to their trade partners. Because exchange among countries not only makes more goods available for consumption, but also allows countries to shuffle (substitute) domestic resources in more efficient combinations, it inevitably affects military capability.

Because trade takes place in an anarchic international system, countries must pay attention to relative as well as absolute gain. This directs attention to distributional themes rather than just growth and allocative efficiency. A number of specific predictions flow from Gowa's theory. First, countries are more likely to trade with allies than with neutrals or enemies. This preference is due to more than affective ties, common values, and communitarian considerations. Indeed, the prediction would also apply to cases where countries ally purely out of convenience, even in the face of great differences in domestic structure and national values. Second, trade is more likely to be open (free trade) under bipolarity than under multipolarity. The argument for this prediction is not obvious. Security externalities characterize bloc settings of various kinds. The critical factor is the degree of certainty with which one knows (military) friend and foe. Gowa presents two arguments for the greater openness (within poles) of bipolar systems, one based on the lesser exit costs of alliances in a multipolar world and the second based on

incentives for great powers to forgo their market power (Gowa 1989, 1249-252).

Structural international relations theory predicts that multipolar systems are more fluid than bipolar systems, fluidity defined in terms of the rate of change in membership from one alliance to another (Snyder 1984). This proposition has been confirmed empirically by Duncan and Siverson (1982). Perhaps the lower rate of exit from bipolar alliances is due to the greater cost of exit – a country is less likely to find a compatible ally just on statistical grounds in a bipolar world. The relative stability of bipolarity leads countries to attach greater importance to the future (a low discount rate) than under multipolarity.

A third prediction, which Gowa does not make but which seems perfectly consistent with her analysis, is that regional integration is more likely to occur if it takes place within a more comprehensive military alliance. This point was made very early by Stanley Hoffmann (1963) and later by Kenneth Waltz (1979). The European Coal and Steel Community and the European Economic Community were and are both nested within a more comprehensive military organization, the North Atlantic Treaty Organization. This fact may help to explain why the EEC countries have integrated further than the member states of the European Free Trade Association, which has a sizeable number of neutrals (e.g., Sweden, Switzerland, Austria).

Both hegemonic stability theory and Gowa's theory of security externalities attempt to relate the international distribution of power to economic exchange. There are important differences in how this is done. First, hegemonic stability theory focuses on economic power while Gowa focuses on military power. The former approach looks at shares of trade, investment, and industrial output while the latter examines the ingredients of military power. Second, the two approaches sometimes differ in what they take to be the systemic level. Hegemonic stability theory sometimes refers to the distribution of economic power within a bloc (e.g., within the OECD world) and sometimes within the system as a whole (nineteenth century hegemony). Gowa, by contrast, is interested in the global distribution of power. Finally, hegemonic stability theory yields predictions relevant to liberalization-protectionism while the theory of security externalities predicts trade partners.

Realism and Institutions

In this section I briefly discuss realism's account of institutions. I do not elaborate the theory, leaving that for a later section. Instead, I use this opportunity as a way of anticipating and connecting with the subsequent treatment of liberalism, within which a more complete theory of institutions is taking shape.

In one sense, realism's theory of institutions is straightforward. International institutions are expressions of power. The Security Council of the United Nations, for example, recognizes World War II's victors by giving permanent membership and veto power to the U.S., England, France, the Soviet Union, and China. The Bretton Woods system, along with the International Monetary Fund and World Bank, recognizes the liberal international purposes of the United States.

This approach, however appealing, leaves some unanswered questions. Two stand out. The first question concerns the incremental explanatory power of the view that institutions reflect power (Krasner 1983). If institutions *only* reflect power, aren't they theoretically redundant and therefore dispensable in an explanatory sense? The second question asks how such an institutional account can explain the apparently voluntary nature of many international organizations and the great diversity of institutional forms. Can the World Health Organization, the Council of Mutual Economic Assistance, and the North Atlantic Treaty Organization be understood as reflections of power in the same way?

These questions are raised not because realism is incapable in principle of answering them, but because realism will have to break new ground to do so. To respond to the charge of redundancy, realists can point to the role of institutions in adding to the strengths of the powerful, increasing the efficiency with which they pursue goals. Institutions can reduce transaction costs and make it easier to coordinate policies among allies.

The second question, concerning the voluntary nature of many international organizations, raises the issue of how to characterize the interests among states. Classical realism thought in terms of conflictual and harmonious or zero-sum and non-zero-sum relations. Before modern game theory filtered into international relations, the range of interest structures (prisoners' dilemmas, coordination, chicken) was very limited. Without understanding the significance of mixed-motive games for international relations, it was impossible to develop a theory of cooperation. Realism had to first break loose from its zero-sum moorings. In a realist world where power is primary, and interests are zero-sum, institutions would either be irrelevant or limited to enhancing the efficiency of domination. In a harmonious world where agents costlessly transact, institutions would be unnecessary for the most part. What is locally rational (i.e., in a state's interest without coordination) would be beneficial for others.

In the subsequent discussion on liberalism, the significance of different interest structures is pursued.

Liberalism and Global Political Economy

The strongest contemporary challenge to realism comes from an unexpected quarter -- not Marxism or world systems theory -- but rather from liberalism, an approach whose roots lie more in domestic than international politics. Liberalism has "no canonical description" (Doyle 1986, 1152) and no generally accepted text compared to Waltz's *Theory of International Politics* (1979). It picks up on the importance of international society among functionalists and international pluralists, the role of common understandings salient to world order and epistemic community theorists, and the importance of institutions and regulations for resolving collective action problems among institutionalists.

What precisely is liberalism's challenge? At one level the challenge presents itself as a dispute about the goals (or implications of different goals) of state leaders (Grieco 1988, 1990; Snidal 1991; Powell 1991). Realists such as Grieco have argued that relative gains are central while institutionalists (Keohane 1984, 1990a) and some game theorists (Powell 1991; Snidal 1991) have argued in favor of contextually defined absolute gains. This is directly related to the debate about the scope of cooperative exchange. Realists argue that the realm of cooperative exchange is limited; liberals that it is much larger and capable of expanding.

While the relative-absolute gains debate has been prominent, there are other differences. Liberalism predicts outcomes (war, peace, cooperation) that are not structurally derived,⁸ i.e., not determined by the existing or changing distribution of power. Structural (third-image) explanation is central to realism. Liberalism thus points to the importance of unit-level factors (e.g., form of government), emphasizes the private (non-governmental) and sociological aspects of international relations, along with state power, and sees possibilities for cooperation and progress in contrast to the endemic conflict and eternal cycles of realism (Keohane 1990a, 174).

What makes the challenge so acute is that liberalism accepts many (not all) of realism's ground rules -- the importance of states, power politics, and anarchy -- and comes to different conclusions. The differences are not so much a product of fundamentally different worldviews as of contrasting ways of reasoning. It is not true, as both Keohane (1990a) and Moravcsik (1992) point out, that liberalism is a normative and idealistic theory. It has a positive content that has been useful in critiquing (and forcing revisions in) realism.

Nevertheless, there are important differences in assumptions and analytical starting points. Liberalism

exploits a two-way tension between state and society. The state is recognized as an actor but one that is embedded within a domestic and international society. Liberals insist on the importance of civil society (Moravcsik 1992, 6-9; Zacher and Matthew 1992) and on the connections between society, state, and individual. Domestic and transnational social forces can create constraints on states and can lead them to change policies. The European Community's White Paper outlining moves toward further integration and the collapse of communism in Eastern Europe owe a debt (probably smaller in the second case) to transnational social and economic forces.

Another important difference between liberalism and realism concerns the nature of interests. Realism takes state interests, at least the primary interest of security and relative power, as fixed, relative, and dominating other interests such as wealth. States desire power, in particular, relative power. This is not a matter of psychological taste so much as physical survival. Preferences are structurally determined. Anarchy, insecurity, and relative power are part of one coherent ensemble. It is this ensemble which liberalism tries to detach and analyze. Realists see a causal chain running from anarchy to the security dilemma, to the concern for relative power to limited cooperation. Liberals accept anarchy as a descriptive characterization of the global system but do not draw the same conclusions as realists.

In the remainder of this section I briefly describe different strands in liberal global political economy, discuss neoliberal institutionalism in more detail, and, finally, try to identify some research directions ahead.

Liberalism's varied intellectual heritage (Smith, Kant, Locke, Madison, Hume) suggests diverse theoretical strands:⁹ Republican, commercial, sociological, and institutional.

Republican liberalism suggests the importance of limited, constitutional government, of democratic rights, elections, freedom from state intrusion into the "private" sphere, and control of the rulers by the ruled. Michael Doyle's starting point is that "...neither the logic of the balance of power nor the logic of hegemony explains the separate peace maintained for more than 150 years among states sharing one particular form of governance -- liberal principles and institutions." (Doyle 1986, 1157). While there are numerous empirical and theoretical issues to be resolved (see Schweller 1992; Lake 1992; Russett 1990), pointing to the association between liberal democracy and peace is a provocative starting point for future research.

A second variant is commercial liberalism. The starting point is civil society, a world of self-interested economic agents engaged in specialized production and exchange. Barring political obstacles (a big condition from our standpoint), Smith and Ricardo secured the foundation of gains from trade. Countries, by

specializing in areas where they had a relative advantage, can gain by exchanging with other countries similarly motivated. This familiar economic story has a political implication. Specialized production and exchange, even according to purely technical criteria, lead to interdependence, vulnerability (often uneven) and political leverage. As Nye points out (Nye 1988, 238), sophisticated versions of liberalism provide a process-level theory linking international interactions to domestic society and politics. Commercial liberalism not only creates webs of interdependence that constrain states and create opportunities, it may also lead to redefinitions of self-interest. As Nye (1988) argues, realism recognizes learning as a response to changing payoffs. No change of identity, of the unit of account, or of connection between means and ends is involved. But regional integration theory is precisely a change of all these.

The third strand of liberalism is sociological (Nye 1988; Zacher and Matthew 1992). Sociological liberalism stresses the nature of group life in pluralistic industrial societies. Individuals, operating through a variety of groups -- occupational, professional, scientific, public-interest-oriented -- spawn contacts across national lines that may lead to redefinitions of interest, new knowledge, and even new identities.

Sociological liberalism adds a new dimension to international political economy. State to state interaction is important, but sociological liberals see these interactions taking place within a milieu in which domestic and transnational contacts and coalitions of interests and identities are prominent too. These coalitions may affect perceptions of state interests (as arguably cross-border coalitions of business interests do in the European Community) and provide vehicles (or obstacles) and resources for pursuing state goals. Indeed, state goals and social contacts may go hand in hand, as when the Federal Republic of Germany's *Ostpolitik* included social contacts among its goals, e.g., twinning cities, tourism, and cultural exchange.

Joseph Nye (1988) is quite right to point to the transformative potential of sociological liberalism (indeed, of liberalism in general). Nye identifies non-power incentives and the capacity to communicate as strengths of liberal theory, and correspondingly, as areas of weakness in realism (Nye 1988, 250).

The final version of liberalism is liberal institutionalism, or neoliberal institutionalism, as Robert Keohane has labelled it (1989). The starting point of the program is a dissatisfaction with the ability of extant theories to explain important variations in international outcomes. Neither realism, nor Marxism, nor process-level theories provide adequate explanations. Hegemonic stability theory provides a partial explanation but offers few refinements useful for explaining regional and cross-

issue variations. As Keohane notes, if Marxism and realism were true, "we would not observe variations in cooperation from one time period to another, or issue by issue, that were unexplained by the dynamics of capitalism or by changes in international structure." (Keohane 1990a, 171).

The basic theoretical claim of neoliberal institutionalism is simple – international institutions make a difference in explaining cooperative and noncooperative outcomes. The approach accepts states, anarchy, and power distribution as important. It does not see all of international politics as either purely conflictual or perfectly harmonious. Instead, liberal institutionalists start from mixed-motive games, where elements of conflict and cooperation exist. In these circumstances, institutions may make behavior more transparent (e.g., making defection visible), reduce uncertainty and transaction costs, lengthen the frame of interaction (thus enhancing the value of reputation), link issues, and provide monitoring and limited enforcement mechanisms. In short, institutions can foster cooperative gains in circumstances where they might not have been realized. There is some theoretical value added here since institutional claims are not based on changing the interests, the actors, the anarchical environment, or the power distribution.

The liberal's world is one of absolute gain, i.e., gains are judged against the pre-exchange baseline of utility separately for each actor. Agents are better off if they improve their pre-exchange conditions, independently of what others do. Realist skepticism about this proposition centers on the pervasiveness of relative gains. If states are in a security dilemma, and if economic gains are relevant for power, states must judge improvement by a relative yardstick. They must ask not only "am I better off?" but "how does my gain compare with that of others?" As a result, large absolute gains may be forgone because of adverse distributional implications.

As broad propositions, few would doubt either the relativity of power or the link between unequal economic gain and power. The controversial question is how much scope to give to relative and absolute gains. To what extent are states motivated by one or the other? What are the conditions determining which goals states pursue? Are preferences exogenous, given by complex domestic politics, or structurally determined?

Realists such as Grieco (1988, 1990) and Mearsheimer (1990) see power and anarchy as pervasive facts of international life. For Mearsheimer (1990) shifts in the distribution of capabilities have consequences for insecurity and conflict. While he does not concentrate on the implications for economic exchange (except for a brief analysis of the EC), this connection seems obvious.

Grieco has tackled the problem more directly arguing that liberals have exaggerated the scope of cooperative exchange. While the issue can't be discussed in detail here, the battle lines have been clearly drawn between those who feel that relative gains are salient and pervasive, making cooperation difficult, fragile, or impossible, and those who believe joint gains are extensive, robust, and growing.

In the face of the realist critique, three responses are possible. The first is to question seriously the descriptive accuracy of relative gains as a motive for states. It asks how widespread relative gains-seeking is. The second accepts relative gains as an assumption and explores the consequences for cooperation. The third accepts both relative and absolute gains as analytical categories and theorizes the conditions under which each holds. The first project is mainly empirical and descriptive; the second is formal and deductive; the third is synthetic and theoretical.

The first approach, though disarmingly simple, is actually fraught with ambiguities. How does one observe motives? They cannot be directly observed, and they are resistant to the standard attitudinal measures (questionnaires, content analysis). While motives can often be inferred from behavior, there are special difficulties in this case. Snidal is correct to point out that intense bargaining over distribution of gains is not evidence that states pursue relative gains (1991, 703). Any agent interested in absolute gain will also be interested in bargaining over the way gains are distributed. Bargaining behavior is therefore theoretically equivocal. It does not distinguish relative from absolute gains.

The other two approaches bold promise. Snidal (1991) accepts as an assumption the relative-gains motive and asks to what extent its existence hinders cooperation. The simple answer is that the conditions under which cooperation is hindered are limited to a world of just two states (or bipolarity) and pure relative gains (no mixed interests). Snidal remarks that "Only in the very special case of the two-state interaction, with high concern for relative gain and near disregard for absolute gains, is the realist case compelling" (Snidal 1991, 701).

Robert Powell's article (1991) reflects the third approach. Part of Powell's motivation is with the overall theoretical coherence of global political economy. If structural realism and neoliberal institutionalism make fundamentally different assumptions about state preferences, political economy and security studies are not easily unified within a single theory. Security studies are likely to be preoccupied by "us-them" and "win-lose" situations, economic exchange with joint gains and cooperation. Powell reasons that if differences in preferences are fundamental, i.e., are not simply local

expressions of a basic theoretical principle, we may have to look to domestic politics for their causes. This undermines the structural project of realism and (partly) liberalism.

Powell takes a cue from neoclassical economics which has succeeded in unifying theories of market shares and profit-maximization (1991, 1304). He develops a model where the pursuit of relative and absolute gains is tied to changing external constraints. The units do not change. The underlying goals of states are invariant. Instead, derived preferences change in response to changes in the structure of the environment. Powell's approach is unifying in the sense that both motives (preferences) are particular contextual expressions of one underlying model.

Space limitations prevent a fuller discussion of liberalism. Let me simply mention a few points of broad significance. The movements toward democracy and markets in many parts of the world, the collapse of state communism in Eastern Europe and the Soviet Union, and the reunification of East and West Germany are important, challenging developments. Without depreciating state power and systemic factors, liberalism's focus on domestic and transnational social forces and coalitions provides an insightful vantage point. Risse-Kappen's (1991) critique of "peace through strength" points to the importance of societal forces as does Kaldor's (1991, 1) notion of "detente from below." Along the same lines Timothy Ash (1990) reminds us that the whole reintroduction of "Mitteleuropa" into our modern cold war and post-cold-war vocabulary was animated by a Czech émigré in Paris (Kundera) and a playwright in Prague (Havel). While these phenomena may or may not have been determinative, they were and are a part of the story. Liberalism's domestic focus does not make it less relevant for understanding international politics and economics. Some argue that liberalism's focus on rights and free exchange is appropriate within sovereign political communities but not among them; in short, that liberalism's organizing principles do not travel well. This argument overlooks the similarities between domestic and international politics and denies the international system a capacity to transform itself (Modelski 1990). Judith Shklar (1989) reminds us that liberalism within domestic societies was born in fear (and bloodshed, intolerance, insecurity). The analogy is appropriate to the changing international system.

Marxian International Political Economy

With Marxism, it would seem that there are many possible vantage points from which to view political

economy. Marxists have argued the existence of politics in the silent process by which surplus value is appropriated from direct producers, the expansion of capital and the historical manner in which the modern unequal division of labor came about, unequal exchange, the role of the state in managing the interests of capital, the mobilization of class bias, and bargaining between labor and capital for respective shares of the surplus (Caporaso and Levine 1992, chap. 3).

The analytical core of Marxism has, perhaps confusingly, been represented as both class process and power-bargaining (Wolff and Resnick 1987, 145). Classical Marxism (the Marxism of *Capital* and *Theories of Surplus Value*) is generally understood as a theory about capital accumulation, classes, and class processes. The production of value by workers and the class processes by which surplus value is appropriated and distributed, are central themes. As Marxian theory has evolved, power concepts have played a greater role. Sometimes, power is treated as central, as the critical variable that does the explanatory work. Classes are important to the extent that they are vehicles for exerting power. According to this approach, large, wealthy firms and states influence smaller firms and states. Class processes give way to the market power of firms, union strength, and state power exercised in the interest of particular classes. While this approach may be useful, and is prominent in dependency theory, it offers little that is distinctively Marxian. Thus the linking of politics and economics at the international level is not without obstacles for Marxism.

In this section, I will discuss three Marxian or Marxian-inspired approaches to international political economy: the internationalization of capital, or imperialism; unequal exchange theory; and Gramscian conceptions of hegemony.

Internationalization of Capitalism and Imperialism

The core of Marx's *Capital* ([1867] 1967) is an endogenous theory of capitalist development. Unlike the neorealist system, where states are in equilibrium until upset by exogenous power shifts, capitalism is in constant motion, propelled by its own inner dynamic. While my intent here is to focus on current developments in Marxian theory and research, it may be helpful to sketch the basic model of capital accumulation.

Starting with a simple model of a closed capitalist economy, one based on private ownership of productive capital, free wage labor, and market competition, Marx tried to demonstrate capital's laws of motion, that is, the time path that capitalism would follow. In this model, C stands for constant capital, V for variable

capital (labor), and S for surplus. The accounting scheme employed is:

$$C + V + S = \text{total output (or value)}.$$

This accounting expression is an algebraic identity – not a prediction equation. Marx broke down a society's total output into three categories: (1) how much it takes to replace C (depreciation of capital costs); (2) how much it takes to reproduce V (wage costs); and (3) a residual category, the surplus. Society goes through three stages. In the first stage, C and V are just barely replaced. This is simple reproduction. In the second, C and V are augmented, and S increases proportionately. This is equivalent to the quantitative expansion of factor inputs (longer working day, more machines) but no change of technology. Both C/V (organic composition of capital) and S/C+V (rate of profit) stay the same. During phase three, there are changes in technology (capital expenditures increase relative to labor, so C/V goes up). Here the rate of profit changes also.

Marx's argument is that increases of C/V ultimately lead to a lowering of the rate of profit. Marx argued that long-term decline in profits would lead to the search for foreign markets and cheaper foreign inputs, particularly labor. Imperialism, then, could be seen as the extension of capitalist exchange and production relations into the foreign realm.

Current Marxian theory and research seem less interested in deducing imperialism from capitalism's laws of motion and more interested in exploring a variety of motives for capital expansion. The internationalization of capital is an empirical fact. Firms may go abroad to increase profits, to hold on to market shares, or to fend off economic decline at home. Modern Marxian scholarship recognizes diverse motives and seeks to explore the consequences for the expansion of capital (Hymer 1979; Murray 1971; Picciotto 1990; Poulantzas 1974; Radice 1984; Warren 1971).

Under the conceptual umbrella of "the internationalization of capital," various research emphases are possible: (a) the political infrastructure of global capital; (b) the expansion of capital and the less developed world; and (c) capital's global cooperation problem.

The Political Infrastructure of Global Capital

The formation of the European Economic Community (EEC) in 1958 and its expansion to a membership of twelve countries provides one focus for Marxist scholarship. Not only does the EEC facilitate the coordination of economic policies, it also provides a framework for the international expansion of capital.

Scholars in the Soviet Union and East Europe were quick to see the internationalization of capital aspects of European integration. Western Marxist writers have been equally sensitive to the pressures of global competition and have interpreted integration as a response to these pressures. In addition, some writers (Cocks 1991; Lambert 1990) have highlighted the political and legal framework of European integration. Peter Cocks sees European integration as a method of state-building, an effort to create the political infrastructure and means of legitimation needed for capital to move freely at the regional level (Cocks 1991, 35-36). The market, i.e., an expanded regional market, is not seen as the natural result of breaking down national-political obstacles to the movement of capital. Positive measures are required, including "... a correspondingly enlarged legal system that protects private property and enforces contracts and second, an adequate supply of labor" (Cocks 1991, 36).

While Cocks's concern with the political-infrastructure requirements of capital is important, others have taken this as a given and have criticized the weak political institutions and political practices of the European Community. To understand this debate, Marxists distinguish two senses of politics. Politics can refer to the basic political conditions of exchange, e.g., private property rights, a judicial system to enforce contracts, a framework of legal liability, and so on. Politics can also mean organized contests for public office, coalition-building, and authoritative rule. The idea of democratic politics implies institutionalized party competition and electoral accountability. John Lambert, a critic of the European Community, accepts economic integration as a fact (1990). Lambert sees capital as moving freely within a European space, responding to constantly shifting opportunities. Yet, this economic activity is for the most part unaccountable to organized democratic forces. Some scholars (Picciotto 1990) see this imbalance as reflective of the enormous structural advantages of capital over labor (and consumers), starting with its superior mobility. Picciotto is pessimistic about recreating the political shell of parliamentarism at the international level. Others (Lambert 1990; S. Williams 1991) clearly recognize the advantage of capital but see the possibilities of correcting the imbalance through organized European political action.

The Internationalization of Capital and the Less Developed World

The relationship between the advanced capitalist countries (ACCs) and the less developed countries (LDCs) identifies an important topic of research. The primary research question relates to development, defined *inter alia* as the expansion of material output, structural

diversification of the economy, improvement of well-being, eradication of poverty, increasing equity, and the evolution of responsive democratic institutions. Neoclassical economics approaches development from the standpoint of instituting market incentives. Thus, putting in place private property rights and a structure of relative prices that reflect real scarcities is critical. Realism, despite its great-power focus, has attempted to extend its theory to cover relations with LDCs (see Krasner 1985; Lake 1987). Marxian theories have approached the less developed world from the standpoint of the expansion of global capitalism. The focus is more on classes and economic processes (commodity production, exchange, realization of surplus value) than on national economies.

A necessary caveat to this section is that what is or is not Marxian is controversial. If we adopt a narrow stance, an understanding of the expansion of capital to the LDCs is Marxian only when it can be shown to result from the logic of competitive capital accumulation, particularly changes in the composition of capital and the rate of profit. With a broader stance, numerous approaches emphasizing the contact of advanced capital with the less developed world might qualify as Marxian. In addition, the literature is voluminous and difficult to summarize. It includes theories of imperialism, world systems theory, and dependency theory, each with Marxian and non-Marxian strands.

Instead of focusing on particular schools, I will briefly describe three ideas relevant to capital expansion into the less developed world: foreign expansion and incorporation lead to underdevelopment; expansion and incorporation lead to peripheral capitalist development; and expansion leads to associated dependent development.

Did the expansion of capitalism, including private ownership of productive assets, markets, and wage labor, have a positive or negative effect on what came to be called the Third World? A. G. Frank (1962) conceptualizes the world as one integrated system, composed of juridically separate states, but functioning as one coherent whole. For him development and underdevelopment are two sides of the same coin, the advancement of the First World coming at the expense of the Third World. Countries are not simply "undeveloped." Instead, they are "underdeveloped."

Frank's argument is a variant of Paul Baran's (1973) surplus drainage mechanism. In their contact with ACCs, surplus from LDCs gets channeled into the rich zones of the world economy. A variety of mechanisms may be responsible for this: unequal exchange (trade), profit repatriation, interest payments, and royalty and licensing fees (see Warren 1980, 140-43). Data on these processes are difficult to come by and are often subject to many interpretations. For example, Bill Warren (1980, 141) is very critical of the practice of many surplus-drainage theorists of subtracting the amount of capital

leaving a country from the amount entering and concluding that the difference represents an absolute loss.

The second view is that the foreign expansion of capital creates local accumulation and peripheral capitalist development. The contact between advanced capital and LDCs destroys many aspects of traditional ways of life — customs, kinship units, handicraft forms of production, etc. Pre-capitalist forms of production are replaced with capitalist forms. Both the technical forces of production and capitalist production relations come to dominate. Notwithstanding the negative aspects of these changes, capitalist transformation is seen as progressive. For a full treatment of this approach, see Bill Warren, *Imperialism: Pioneer of Capitalism* (1980).

The third approach argues that foreign capital neither retarded the periphery nor propelled the Third World along a path of economic growth similar to that presumably experienced by Europe. The associated dependent development school, identified most closely with Fernando H. Cardoso (1973), argues that foreign capital stimulates economic growth in the periphery but that this growth is propelled by the rhythms of core capital, is nonautonomous, and produces a variety of distortions: internal inequality, marginalization of key sectors, and structural disintegration of the economy. This last (disintegration) implies not that the economy is falling apart but that it is both sectorally and functionally disconnected. This in turn undermines efforts to control the economy by manipulating the key instruments of macroeconomic policy, e.g., interest rates, tax policy, money supply (see Brewster 1973; Sunkel 1973).

Dependency theory has had many critics (Packenham 1992; Almond 1990). However, in this writer's opinion, dependency theory died out more from neglect than frontal criticism. By the middle of the 1980s, little work was being done in the United States on dependency theory.

There is no space to evaluate these views of the effects of foreign capital expansion here. Yet, is it interesting to ask how each approach might respond to the appearance of the newly industrializing countries (or semi-periphery) in the global economy. The first approach (Frank's development of underdevelopment) simply cannot explain the NICs. The NICs pursued policies based on a strong export-orientation and invited foreign capital on favorable terms. Their key policy was to integrate into the international capitalist system and to pursue absolute gains rather than to spend resources bargaining over distributional issues. The success of the NICs is a refutation of the first theory.

The second approach accepts, even anticipated, the spread of capitalist development throughout the periphery. Peripheral capitalism is seen as a necessary and progressive phase of a larger historical process.

Moralizing as to whether this is good or bad is beside the point according to proponents. The third approach, that of Cardoso and Wallerstein (who are very different in other ways), recognizes the growth of the semi-periphery but sees it as both derivative and limited. Neoclassical economists and classical Marxists such as Warren have no difficulty accepting (even expecting) many more members of the semi-periphery and the passage from there to the core. The space at various levels of the global hierarchy is not limited. Contrast this expansive view to the view of Wallerstein, who argues that only a fraction of poor countries can fit into a growing world market (1979, 76) and that, when the world economy experiences a downturn, the weakest (poorest) suffer first (1979, 88). Wallerstein treats economic growth as a scarce good (with rival properties) for which bargaining power is relevant. This contrasts with the view that economic growth is essentially non-rival, that contact with those doing well improves one's own chances, and that voluntary exchange rather than power is the appropriate instrument of improvement.

Global Cooperation Problems for Capital

A third application within the internationalization of capital umbrella concerns the coordination of separate capitals internationally. The relation of owners of capital to one another is of a dual nature. On the one hand, capitalists are competitors. They strive to outperform one another in global markets, and, if they are successful enough, they will drive competitors out of business. On the other hand, they share interests with respect to labor and international infrastructural goods (currency stability, tax policies of governments, predictable political environment, labor laws, regulations regarding capital mobility etc.). Since these goods are public in substantial ways, they generate collective action problems. While the literature on international regimes has studied the cooperation problem in many issue areas, few have researched the capital cooperation problem (Duvall and Wendt 1987).

Along these lines, Picciotto's article (1990) raises interesting research possibilities. Picciotto does not approach the problem of cooperation from the standpoint of noncooperative game theory. Neither does he see the formation of international organizations or regimes as crucial. Instead, he tries to formulate a logic of international corporate culture. This culture is a collection of understandings, codes, and informal agreements. While it doubtless owes something to international institutions such as the World Bank and the International Monetary Fund, it works primarily from the ground up, relying on conventions stitched together by

central bankers, key leaders of business interest groups, and members of national finance ministries. The solution to the problem of cooperation is "...a nationally based but internationally coordinated corporate capitalism" (Picciotto 1990, 33).

Theories of Unequal Exchange

Given the tremendous inequality in incomes and life circumstances among countries in the world today, it is not surprising that liberal and Marxian economists have addressed themselves to understanding the process of development and underdevelopment. According to Immanuel Wallerstein (1974), the period from 1450 to 1800 allowed for the eradication of feudalism and fostered primitive accumulation of capital in the core. It is only with the nineteenth century that what is today called "the widening gap" emerged. From the fifteenth to the eighteenth century differences in wealth between Europe and the other centers of civilization -- China, India, and the Middle East -- were not great (Bairoch 1979). By the late 1970s, the gap between richest and poorest was thirty to one (McGowan and Kordan 1981, 56). One theory which attempts to explain the gap relies on conditions relating to exchange among rich and poor. Terms of trade theories, at least some of them, provide arguments about the transfer of surplus from less to more developed countries.

The terms of trade theory refer to a class of arguments about why certain countries or classes gain systematically through trade while others lose. Strictly speaking, terms of trade theory is about relative -- not absolute -- gains and losses. While some theories can speak to absolute immiseration (see Dutt 1986), the distribution of benefits is generally at issue rather than the entire Ricardian theory of comparative advantage. Given this broad definition of the concept, Frank, Prebisch, Emmanuel, Singer, and Lewis can all be interpreted as terms of trade theorists.

Operationally, the terms of trade are defined as a ratio of export to import prices,¹⁰ using a standard definition of goods and employing the assumption of constant technology. The basic descriptive claim is that poor countries (or those that produce primary goods), will suffer a long-term decline in their terms of trade, giving up more than they receive in the long haul.

Trevor Bell (n.d.) is correct to argue that neoclassical trade theory has no comprehensive hypothesis about long-term trends in the terms of trade. Instead it deals with the issue in terms of changing relative prices of imports and exports due to exogenous changes in supply and demand. Neoclassical theory is therefore noncommittal about long-term movements in prices.

Whether trade at one point in time embodies equal value for both parties is a metaphysical question under market conditions.¹¹

Raul Prebisch, an Argentine economist, advanced a terms of trade argument based on assumptions of differential elasticities of demand and factual claims about the position of less developed countries (LDCs) in the international division of labor (Prebisch 1950, 1969). Prebisch also made an argument about the effect of surplus labor in LDCs, but he is most remembered for the elasticity theory. The argument is simple. Poor countries are poor not because of the absence of productive inputs, or because rising population pressures periodically consume investible surplus, but because of the goods they produce and export in the international division of labor.

LDCs produce raw materials and food goods (non-processed food goods). Advanced countries produce manufactured goods. Prebisch combined these facts¹² with an argument about elasticities, maintaining that as income increases a declining share will be spent on primary goods, a rising share on manufactured goods. This assertion provides a hypothesis about the secular trend in relative demand, missing from neoclassical theory. As demand for manufactured goods outstrips demand for primary goods, prices for the former rise more rapidly. When these respective goods trade in international markets, the secular terms of exchange favor the advanced countries.

In contrast to Prebisch, A. Emmanuel (1972) tries to develop a theory of unequal exchange independently of the exchange system (trade) itself. Technically, trade is treated as the carrier of inequalities whose source lies in domestic production relations. Grounding inequality of exchange in production relations is seen as perhaps the primary task for classically oriented Marxists and neo-Ricardians. As a result, Emmanuel eschews theories which appeal to market power, monopolistic advantage, rents from control over technology, or differential elasticities in product markets. Instead, he posits the fundamental cause of unequal exchange to be the differential rewards to labor in different countries.

The assumptions of *Unequal Exchange* include the mobility of capital globally but the immobility of labor, an assumption which is largely true. While international movements of people (hence labor power) are not unimportant, these movements are overwhelmed by movements of physical and financial capital. Since capital is mobile, rates of profit equalize. Labor, however, is protected by state boundaries. Advanced countries are labor-scarce and have strong, well-organized labor movements. This was more true in 1972 when Emmanuel wrote *Unequal Exchange* than during the

eighties. Other countries (LDCs) are labor-abundant and have organizationally weak labor forces. As a result, the wage bill is higher in advanced countries than in LDCs, higher than would be predicted by productivity differences. The residual implies that part of production costs are due to class struggle.

When countries with strong and weak labor movements trade with one another, the former reap a premium reflecting these institutional differences between countries. The terms of trade reflect these wage-level differences. They do not cause them.

Whereas most terms of trade theorists attribute importance to market imperfections (monopoly power, spillover benefits of processed goods, learning by doing effects), Emmanuel conceives adverse terms of trade and underdevelopment as the inevitable outcome of free trade among more and less developed countries, even under the most competitive conditions. The book is subtitled "A Study of the Imperialism of Trade."

While Emmanuel's effort is ambitious, it contains internal inconsistencies and troubling assumptions. He sees wages as independent and autonomous, rather than as reflecting productivity differences. It is not plausible, on neoclassical or Marxian grounds, that wages cause productivity differences. Second, by his own assumptions, rates of profit (Marx's $S/C+V$) equalize across countries. This logically implies that LDCs cannot do worse than advanced countries in terms of relative surplus value (Weeks and Dore 1979, 71). Given any arbitrary starting point, if the initial cross-country rates of profit are not in equilibrium, the natural flow of capital would be from advanced to less developed countries. As Weeks and Dore point out, this mechanism predicts higher rates of accumulation in LDCs (1979, 71), an implication not congenial to unequal development favoring the core.

All in all, *Unequal Exchange* does not wear well with time. As Barratt-Brown pungently put it, the theory of unequal exchange is contrary both to the neoclassical doctrine of comparative costs and to the Marxian argument that the rate of capital accumulation and socially necessary labor time determine prices (Barratt-Brown 1974, 230).

Gramscian Approaches

Classical Marxian theory focuses on the economy, particularly the productive forces and class processes through which value and surplus value are produced. The engines of change in society (and in politics and culture) are located in economic forces, particularly in the pace of capital accumulation. Some have abandoned the notion of economic primacy and have replaced it with a non-reductionist focus on politics and

economics (Bowles and Gintis 1986). Other scholars see culture and ideas as important and not simply derivative of economics. It is this strand of thought that I want to discuss briefly here, using the work of Robert Cox (1983, 1986, 1987), Stephen Gill (1990, 1991), and Augelli and Murphy (1988).¹³

The basic idea of Cox's work is that world politics and global political economy cannot be reduced to either a political or economic essence. The neorealist approach is truncated since it omits the social forces (most notably economic) underlying and traversing states and the milieu of beliefs and ideologies in which states are embedded. Neorealism relegates these forces to the second-image level and argues that they can be safely ignored because of the overriding force of security concerns. Cox argues that such a view may have made sense in the eighteenth and early nineteenth centuries when the separation between civil society and the state was cleaner. In today's world, state and society are interpenetrated. We should therefore talk about state-society complexes, or "forms of state" with different underlying social coalitions rather than the state as an abstract category (1986, 5).

Cox's approach is brought out nicely in his treatment of hegemony. Hegemony is not just a power concept. It cannot be accessed simply with reference to states and the distribution of capabilities. Instead, hegemony "...is based on a coherent conjunction or fit between a configuration of material power, the prevalent collective image of world order (including certain norms) and a set of institutions which administer the order with a certain semblance of universality (that is, not just as the overt instruments of a particular state's dominance)" (Cox 1986, 223).

Cox's historical method relies on an analysis of three sets of important factors, none of which can be reduced to the others: social forces, state forms, and ideas. Social forces include, but are not limited to, economic (material) factors. State forms include the many different institutional expressions that authority and organized power can take. And ideas refer to beliefs, common understandings, and the shared codes which allow for predictability and coordinated actions. The importance of the tripartite focus is that it allows one to pose questions and make problematic some things that would otherwise be unquestioned and nonproblematic. Certain leads and lags (Cox 1986, 224) are possible in the cycles of hegemonic rise and decline. Britain had imperial pretensions and beliefs well after her material base had declined. Similarly, there are some who argue that the U.S. is not declining in strict power terms. Rather it is U.S. moral leadership and legitimacy that are questioned. Johnson's failure to raise taxes to finance the Vietnam War and the subsequent shifting of part of the

costs to Europe, Nixon's abrupt suspension of the gold standard, and Reagan's insensitive unilateralism combined to undermine confidence in U.S. leadership.

A second thing that Cox's framework allows is the exploration and possibly explanation of the content of world order or regimes. In a neorealist world, states are modeled as having interests and power but not beliefs. It must be presumed that states have the necessary beliefs about the causal structure of the world, at a minimum, to see the connections among means and ends. But beliefs in a large sense, including purposes, principles, and existential propositions, are not at work. Thus, when it comes to world order, cooperative institutions, or regimes, the best that neorealism can do is provide expectations about when order is more or less likely and something about its form (Martin 1992a).

While I do not have the space to develop related contributions, the work of Augelli and Murphy (1988), Gill (1990, 1991), and Gill and Law (1989) is important. Augelli and Murphy examine the reconstitution of "American" hegemony during the Reagan era, after significant reversals during the late sixties and seventies. Gill and Law (1989) deal with the structural power of capital in both its national and international dimensions. While state structures are important for all these authors, hegemony is not confined to the distribution of power among states. International capital is also important. Capital, especially in the form of foreign direct investment, is not just an abstract factor of production. It is also a social and political presence providing or restricting jobs, income, technology, and knowledge.

The Gramscian approach is less a theory than a conceptual framework within which certain questions can be explored. The three main components of the framework — material capabilities, ideas, and institutions — are touchstones to be used in historical analysis, not independent and dependent variables. Nevertheless, the framework is suggestive of different problems and hypotheses. For one thing, the prevailing image differs from the neorealist theory of states and state actions. Cox's view of the world is not state-centered. Hegemony cannot be represented as a distributional parameter, as a share of total system-wide resources controlled by one country. Cox is more interested in which fractions of international capital occupy important locations in the world economy.

Following from the above, evidence for hegemony or hegemonic decline is not provided by the percentage of economic assets controlled by one (or several) powers but in the privileged position of key social forces (particularly international capital) of major capitalist states. Better indicators of hegemony are provided by the increasing importance of the economic summits among the advanced capitalist countries, the

anticipated success of the European Community's deregulatory 1992 project, privatization schemes carried out by governments in Western Europe and North America, the collapse of socialism in East Europe, and the continuing influence of international financial institutions in the Third World. By these measures, capitalism, particularly advanced fractions of international capital, are not in decline.

Two final comments relate to the treatment of culture and governance in the Gramscian approaches. The liberal conception of international governance is driven by the provision of international public goods. International politics has to do with those institutions, rules, and collective action procedures associated with the provision of these goods. Realists see a decentralized, state-centric world but recognize the embryo of international governance in international organizations. Inis Claude (1962) arrays international organizations along a continuum from least to most centralized. Politics in the international system is represented as the reconstitution of capacities at another, more global, level.

The focus of Cox and Gill is different. Cox sees key elements of the internationalization of the state in the ascending importance of key national ministries (trade, finance) and in their increasing utilization as points of contact among "national" ministers. In addition, there is an emerging world or transnational class culture, with bankers, capitalists, civil servants and perhaps academics as part of it. Brussels, Paris, London, Geneva and New York and Tokyo may be the centers of this emerging culture, less in the sense of what is headquartered here and more in terms of serving as nodes of contact for key elites, providing the international watering holes reminiscent of Bohemian Grove (Domhoff 1974). Gramscian conceptions of international governance, then, are based neither on collections of states in international organizations nor on "...hierarchical power structures with lines of force running exclusively from the top down..." (Cox 1986, 230).

Finally, while the Gramscian approach is clearly distinct, it does resonate with the works of others. Susan Strange's work on the U.S. empire (1988, 1989) emphasizes strongly U.S. structural resources in capital, technology, and knowledge and is also sensitive to the moral dimension of U.S. foreign policy. William Appleman Williams speaks of "Empire as a Way of Life" (1991), arguing that the privileged position of the U.S. in the world has deep roots in American culture and national experience. Finally, John Ruggie has consistently advocated the importance of ideas and social purpose along with power and interest as conventionally defined (Ruggie 1982, 1992). Using counterfactual thinking, he argues that the systemic ordering principle of the postwar order, liberal multilateralism, is not the one that would have been instituted had Germany or Japan won the war.

There was nothing inevitable about a liberal multilateral world order. It was the product of a distinctive set of social and political forces in the United States.

Conclusion

The preceding review of the global political economy literature is far from comprehensive. Entire fields of work have been omitted, among them world systems theory, dependency theory, long cycle theory, regulation theory, and theories based on foreign economic policy making. I have not omitted these theories because they lack importance. My justification is simply that I place greater value on developing the arguments of a few theories than saying something about all of them.

Taking the neoclassical, realist, liberal, and Marxian approaches as points of departure, a number of gaps and blind spots emerge. These gaps, I argue, characterize scholarly work in mainstream international relations journals in the United States. Most striking are the paucity of articles and books on north-south relations, poverty, and development (or underdevelopment). A research program concerning south-south relations scarcely exists, north-south relations figure a bit more prominently, and north-north relations dominate the agenda, more or less in conformity with Johan Galtung's feudal interaction structure (Galtung 1971). I recognize that research agendas are motivated in part by theoretical concerns, in part by questions that reflect important values. Development and poverty certainly fall into the latter category and often into the first. We should ask ourselves why there is not more work on the global political economy of development.¹⁴

A second gap, Marxian theories notwithstanding, is the analysis of classes, class conflict, and the consequences of changes in the global political economy for different classes. There is surprisingly little research as opposed to essays and broad speculative work, even within Marxian political economy. Some of the best work examining the connections between the global political economy and domestic classes is done by scholars who cannot be neatly categorized within one of the dominant approaches (see Bergquist 1984; Pahl 1984; and Piore and Sabel 1984). Rupert's (1990a) research on the "production of hegemony" involves an understanding of state-society relations that builds on classes.

A third gap involves underemphasis on the political aspects of international economic involvement in two senses: first, the construction of favorable international economic structures, a project limited to major powers; second, the effects of international economic position on conflict and war. This gap may seem odd in light of the large literature on

interdependence. However, interdependence is taken as a datum, i.e., a given, and not as something that is purposefully constructed. Yet the present international economy is not the one expected on the basis of an economic model of comparative advantage and factor endowments. In other words, a large political element is implied in the exchange and production structures of the global (political) economy. The purposeful structuring of the conditions of international economic exchange has not received adequate attention. This makes it seem as if the capitalist world economy is an accident, more or less the result of the external projections of separate national capitalisms.

Apart from these broad gaps, each approach examined here has strengths and weaknesses. Neoclassical political economy has excelled as a policy science, applying its tools and theory to issues of protection, property rights, and foreign investment. It has contributed to our understanding by elaborating the idea of market failure at the international level. Finally, blended with realism, it has supplied a basis for thinking about international institutions.

When I began this paper, there were two areas where I thought neoclassical political economy was weak and not likely to make a contribution. These two areas were unequal exchange and the formation of preferences. The reason for pessimism about unequal exchange rested on competitive markets. The formation of preferences, on the other hand, was thought to be a social matter and that economists were correct to take preferences as given. This assumption proved partly wrong. I have reviewed how Lake interprets strategic trade theory as molding preferences and how Gowa treats international power structures as affecting economic incentives. Ronald Rogowski has shown how aspects of international trade theory (Stopler-Samuelson theorem) can be used to make predictions about the preferences of different domestic groups (1989).

The chief limitation of neoclassical economics is that it molds political economy along lines guided by market failure and efficiency. Yet politics and political economy are not limited to market failure. Politics sometimes involves structuring market activities, determining the scope of markets, regulating their impacts (not just externalities), and engaging in redistributive politics that violates ordinary efficiency criteria.

Neoclassical economics has had difficulty coming to grips with power (see Caporaso and Levine 1992; Heilbroner 1988; Gruber 1992). Partly, this is due to conceptualizing economics as voluntary exchange. Economists have attempted to identify the core of an economy by isolating the set of exchanges that are Pareto-superior (where at least one person improves but

no one is worse off). The omission of a theory of power in neoclassical economics is not simply an oversight or something that can be "filled in" with additional work. The gap results from the focus on voluntary exchange and mutually improving transactions. Implicitly, the relevant power idea is "power to" (to organize collectively, to achieve common goals) rather than "power over" (to achieve goals at the expense of others). Politics involves exchange also, though here welfare loss is typical, some would say central. Conflict, antagonisms, domination, and winning and losing characterize politics. The work of Krasner (1985, 1991), Spiro (1993), and Gruber (1992) helps to understand these differences and to retain our focus on some traditional political concerns.

Realism has attempted to theorize global political economy on the basis of the state operating within an anarchic environment pursuing economic goals. It has contributed to our understanding of the political conditions guiding economic exchange and international institutions. Given the anarchy assumption, realism has availed itself of the theory of non-cooperative games, an approach developed largely in economics. Realism has (in part) become economic in its approach as well as its substantive concerns.

On the other hand, some realists, particularly Krasner (1991), have kept traditional power-oriented concerns on the agenda and have sustained the view that these concerns are distinct from economic ones. These distinctions are perhaps most easily seen when it comes to institutional and bargaining phenomena. Economic approaches typically deal with situations marked by Pareto sub-optimality and ask how states can forge arrangements (institutions) to arrive at the Pareto frontier. In "Global Communications and National Power," (1991), Krasner argues that disagreements are more often over which point on the frontier should be chosen, rather than over how to arrive at the frontier. The primary focus is (or should be) distributional conflict rather than market failure (p. 336). The point is general and is not limited to communications. Relative power capabilities are important in situations regarding relative power, the ability to eliminate other actors, and in cases where some states affect the assignment of property rights (not at all rare in international relations) (Krasner 1991, 365).

The chief limitation of realism is its truncated view of social and political life. It largely ignores domestic politics and the social forces (as Cox and Gill term it) that undergird state to state politics. Every theory must omit many things. This is not a criticism that not everything is included. The omission of domestic politics makes it seem as if international politics is simply the collision of predetermined state interests or interests determined by international structure. Civil society disappears. This cuts realists off from

understanding the role of business groups, professional groups (say life scientists working on the greenhouse effect), the Catholic Church (in Poland), and the influence of social groups working on their own *Ostpolitik* in the former Federal Republic of Germany -- except to the extent that these groups operate through the state.

Realism's fear seems to be that, if examined, domestic politics will prove to be a Pandora's box of country-specific parameters. This fear seems exaggerated. Domestic politics, and its implications for global political economy, can be theorized, as the work of Rogowski (1989), Putnam (1988), Katzenstein (1978, 1985), Goldstein (1988), Frieden (1991), and Lake (1991) shows.

Marxian global political economy starts on a promising note. It is conceptually comprehensive, attempting to bring together political institutions, social forces, and ideas. At present, however, little systematic theory exists in testable form. Instead, the theory is applied illustratively to various global situations. While Marxism has historical roots, the modern Gramscian approach is very new and the group of scholars working in the tradition very limited.

Modern Marxian political economy has departed from the classical project in two quite distinct directions. The Gramscian turn, exemplified by Cox, Gill, Rupert, and Augelli and Murphy, abandons the development of the forces of production as the (single) engine of change and places ideas on an analytically coequal plane with material forces and state institutions. Rational choice Marxism abandons the project of deducing capital expansion from the laws of motion of capitalism in favor of collective action problems inherent in class conditions. While these foci are valuable (both capitalists and workers confront social dilemmas), analytical Marxism is limited by not having a theory of capitalist development. What was attractive about Marxism is that it said something about the time path of capitalism and the transformation of interests along this path. One does not need to believe in the iron laws of increasing organic composition of capital and falling rate of profit to argue for attention to the historical dimension of capitalism.

In my opinion, Gramscian approaches could be refreshed and sharpened by contact with the more classical strains of Marxism. Forces of production have broadened into a less definable "material interests," and production relations have either disappeared or have been absorbed by "social forces." Perhaps regulation theory has furthered the older notion of relations of production (see Noel 1987). In any case, the Gramscian approach would do well to rethink the role of this important concept.

The Gramscian approaches reviewed here have been organized around the interplay of material interests, state forms, and ideas. This is not the only way to go. Mark Rupert's project (1990b) is to understand (historically) the construction of international civil society. His focus is not directly on states-markets and power-wealth. Instead, he first wants to understand how an international space for "private" capital was created, how labor and capital came to confront one another as juridically free agents within international civil society. Domination has to do not so much with states over states or states over non-state actors, as with global capital over labor. For Rupert, global political economy involves tasks that are radically different from realists and liberals.

There are other directions in which Marxian global political economy might go. If Marxism is concerned with competitive capital accumulation, we can ask about the consequences of accumulation processes at the global level. Few countries have anything remotely resembling closed economies. Almost all "national" economies must insert themselves into the global economy to complete their cycles of accumulation. Some "tie-ins" are for markets, some for physical capital, some for financial capital, and others for labor (skilled or unskilled), and knowledge. Most of these global links do not involve arms-length transactions, although some do more than others. Taking these economic connections seriously will involve understanding better the political forces associated with economic integration, a project central to dependency theory.

Ten years ago it might have been debated whether global political economy was fad or field. The concerns that have dominated research have proved to be durable. Significant research accomplishments have been made in numerous areas, among them institutions, cooperation, public goods theory, strategic trade, and hegemony. Still, significant gaps remain which are not just due to finite resources. Hopefully, this essay has conveyed a sense of progress as well as pointed to some areas where future work is sorely needed.

Notes

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1. A Pareto-optimal outcome is one that cannot be improved on in the sense that there is no alternative that makes someone better off without making someone else worse off.

2. Many political scientists would welcome the introduction of the concept of ideology into economic theory but as yet it is rare to see it used in either an explanatory or descriptive way. Agents are typically characterized by preferences and by beliefs about the causal structure of the environment (i.e., information) but not "belief systems" that disguise and yet advance the interests of particular groups. In addition, there seems to be no good *a priori* reason why the term ideology should apply only to non-market configurations.

3. The exact difference between realism and neorealism is unclear and bound to be controversial. On one interpretation, neorealism is "merely" a more rigorous version of realism, one grounded on microeconomic foundations. On another not entirely different interpretation, neorealism provides a theoretically unified systemic account of interstate politics. Some claim that neorealism attempts to correct for realism's inability to deal with economic issues (Hollis and Smith 1990, 36-37).

4. This last point concerning the goals of states requires some comment. While much effort has been spent trying to justify the content of utility functions on *a priori* grounds — witness the debate over absolute vs. relative gains — there seems little to be gained and much to lose by unduly restricting the range of state motives *a priori*. Why not allow the goals of states to change depending on situational properties? In neoclassical economic theory private agents pursue absolute gains or market shares, cooperative or uncooperative strategies, narrowly egoistic, predatory, or altruistic actions — all depending on the structure of the environment. Not only would this enhance flexibility of analysis, it would also open the way toward incorporating institutions into realist theory and facilitate a reconciliation with those whose work has been relegated to "mitigation of anarchy" — the work of those in international law, morality, norms, learning, etc.

5. There are some scholars who are trying to endogenize capabilities in ways that are consonant with realist theory. See Kugler and Domke (1986), Organski and Kugler (1980), and Snider (1988).

6. I stress that this is an approximation rather than an exact relation. The gains from trade do not convert directly to the losses of trade disruption because this simple equation ignores substitution possibilities.

7. Let me try to clarify this point. It might appear that bipolarity and multipolarity are relational concepts since they refer to the way something (power) is distributed across countries. But notice that power itself is a property attached to countries — not to pairs or larger sets. Attribute power is therefore different from network power, where capabilities are expressed in terms of positions (and roles) in exchange structures. I refer to bipolarity and multipolarity as distributional parameters since they summarize how national attributes are distributed, in much the same way that one could summarize the height of individuals in a group.

8. Not everyone would agree with this assertion. In "Liberalism and International Relations Theory," (1992) Moravcsik argues that liberalism is structural theory (at least partly), but it is the pattern of preferences rather than the distribution of capabilities that counts.

9. See Keohane "International Liberalism Reconsidered" (1990a) for one classification; also see Zacher and Matthew "Liberal International Theory: Common Threads, Divergent Strands" (1992) for a comprehensive view of liberalism which distinguishes different approaches.

10. Technically speaking, this is a definition of the price, or commodity terms of trade. Terms of trade may also be defined with reference to quantities of goods exchanged (gross barter terms of trade) and in terms of quantities of labor and capital contained in the goods exchanged (factorial terms of trade) (see Barratt-Brown 1974, 231).

11. In other words, it makes little sense to ask, in any particular exchange, which party acquired more value. Terms of trade theories logically imply a time dimension, i.e., they have to do with changes in ratios of exchanges over time.

12. At least they were facts during a rather long historical era. Beginning with the 1960s, a rising share of LDC production was accounted for by manufactures. While a large proportion of LDC manufactures was accounted for by a few countries (newly industrializing countries), there is some evidence that manufacturing production is spreading to a sizable number of third-world countries (see Caporaso 1981; Haggard 1990).

13. The question arises as to whether these authors should be classified as Marxian. None focuses exclusively or mainly on the process of competitive capital accumulation or production and distribution of the economic surplus. Much of Robert Cox's work attempts to understand labor as an economic category and social relation. All of these authors attempt to understand ideas, institutions, politics, and economic process as a coherent ensemble, i.e., as organically connected. While the lines of causality are not one-way, the authors attach substantial weight to economic forces.

14. I recognize that certain global political economy approaches are focused (some exclusively) on development issues. Dependency theory and world systems theory come to mind. The limitation applies in particular to neoclassical, liberal, and realist approaches. Important exceptions within the realist tradition include Krasner (1985) and Lake (1987).

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Political Conflict, War, and Peace

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World politics can be divided into two main strands. One strand is the broad study of war and peace that encompasses the analysis of competition, conflict and warfare; the second equally broad strand deals with the field of political economy and includes political development, integration, trade, negotiations, and political demography. This chapter focuses on the study of conflict (for political economy see Caporaso in this volume).

War is aptly and generally defined by Malinowski (1968, 28) as an "armed contest between two independent political units, by means of organized military force, in the pursuit of a tribal or national policy." Levy (1983, 51) introduces the more restricted concept of "international war" as "a substantial armed conflict between the organized military forces of independent political units." International war so defined excludes many domestic conflicts that produced enormous casualties such as the American Civil War (1861-1865), the Boxer Rebellion (1899), or the Russian Revolution and Civil War (1917-1921). It also does not include terrorist campaigns such as the protracted conflict in Northern Ireland (1969-1991), or guerrilla activities such as those leading to the fall of Chiang Kai-shek in China (1949) or to the fall of Batista in Cuba (1958). Also excluded are border incursions such as the Sino-Soviet Split (1959-1969). Limited punitive strikes such as Israeli actions against Lebanon (1967-1991) do not fall under this definition of war, or even protracted confrontations such as the Cold War between the United States and the USSR (1946-1989); because they do not qualify as substantial international conflict. The fighting in Yugoslavia illustrates the difficulty of defining war. In the early stages, this is not an international war because competitors are not independent states. Later this conflict also does not qualify as a war because it does not involve organized military directly attached to nations. Thus, using Levy's definition, the Yugoslav conflict has not yet turned into an international war.

Much of the work I report on here deals with Levy's restricted concept of international war, but many generalizations and inferences may apply to conflict in

general.¹ Given this conception of war, peace defines all interactions among nations that are a complement to international war. Nations are at peace when no serious contests arise, when disputes are resolved by accommodation or continue to simmer, and, as is frequently the case, when one side yields to the demands of another without resort to force.

The concern with war and peace is so central to world politics that it defined this field for many years. For that reason it is simply impossible to do justice to the whole literature. Let me start this review, therefore, with two important disclaimers. First, in this short space it is not possible to provide a fair summary of all developments in the vast field of international war. Second, even if length were not a constraint, I am a captive of my own interest and research experience. Thus, like a myopic observer looking at an open field through a long funnel, I am aware of minute distinctions among works whose results directly relate to my own research agenda, and still see through this lens, albeit imprecisely, the contributions of distant but directly related predecessors. This intellectual funnel is also a blinder, however, and much of the research on international war that has evolved in directions different from those I chose will not attract recognition and is, unfairly, discarded. Like most reviews, therefore, this is a personal journey through the vast literature on international war that says perhaps as much about my own perspective on war and peace, as about the development of this vibrant branch of world politics.

To provide a guide, let me first outline my general perspective on the field. The study of war and peace is still chaotic because an agreement on a general paradigm to study this subject has not been reached.² My review emphasizes the war literature that clearly specifies general propositions, and places such propositions under empirical scrutiny. Such systematic study of war and peace is in the early stages of development, and today's findings will no doubt be seriously revised and perhaps reversed in the next decade. Yet, a body of theory supported by evidence is now available that provides hope for large theoretical gains in

the near future. This review centers on a select number of general propositions that have undergone empirical scrutiny.

Paradigms on War

A consensus on a paradigm for the study of war has not emerged, and students of war and peace have not achieved closure on testing procedures. In this sense, the field of war and peace is a "developing" discipline that lacks two key characteristics of a "mature" discipline: agreement on a common paradigm to ground research and consensus on a common empirical basis for the evaluation of competing theories.³ Because a common paradigm is lacking and because the criteria for falsification are hotly contested, theories of war and peace continue to be judged by the novel insights they suggest, whether or not such insights conform with existing empirical evidence. This review will argue that over the last two decades the most important contribution of the empirical literature on war and peace has been to show that strongly held, popular and academic constructs are usually tenuously consistent and frequently totally inconsistent with observed reality. Indeed, empirical evaluations that support general structures are rare (Midlarsky 1988).

Given this empirical record, the corpus of consistent, cumulative theories about war is thin. At this state of theoretical development it is difficult to determine whether new propositions advanced by the modern scholars supersede those advanced by earlier scholars, and it is hard to decide which theoretical strands should be discarded and which should be kept and expanded. It is not surprising, therefore, that in this vast field of competing theories, support by ardent advocates counts for as much or more than support by evidence. Moreover, perhaps because empirical tests do not conform closely with theoretical expectations, much of the debate is aimed at the philosophy of scientific research rather than at the findings from such research (Gurr 1980; Midlarsky 1989; Kugler and Lemke 1993). One wonders whether this exercise will ultimately simplify or complicate the enterprise of knowledge creation. However, there is little doubt that the debate regarding rules of falsification complicates any attempt to define and review this field.⁴

Given the current development of the study of war and peace, a reviewer is forced to make unpalatable choices. If vague falsification guidelines are accepted, the literature to be reviewed is immense and clouded by internal and external inconsistency. On the other hand, if stringent criteria for rejection are adopted, the surviving literature is tiny and there is little left worth reporting. There is no simple escape from this dilemma. My

personal solution is predicated by my interest in general, cumulative, empirical knowledge. In the study of war and peace many propositions are advanced to explain a single event. Indeed, major wars such as World Wars I and II have generated such an enormous body of scholarly work that a listing of alternative explanations alone would require space far larger than this paper. For this reason, I emphasize the necessity of an agreement on empirical criteria for rejection that permits researchers to choose among the already vast array of competing propositions. Thus, I consider works that approach the simple scientific research principles proposed by Lakatos:⁵

A scientific theory *T* is falsified if and only if another theory *T'* has been proposed with the following characteristics: (1) *T'* has excess empirical content over *T*; that is, it predicts novel facts, that is, facts improbable in the light of, or even forbidden by *T*; (2) *T'* explains the previous success of *T*, that is, all the unrefuted content of *T* is included (within limits of observable error) in the contents of *T'*, and (3) some of the excess content of *T'* is corroborated (Lakatos 1978, 32).

In Lakatos' view, a superior scientific theory is simple and makes definite predictions that can be tested by observation. In the absence of a contending theory, systematic, parsimonious statements that account for a given phenomenon are superior to no statements at all. Indeed, like paleontologists who had to wait until the discovery of bone dating techniques to reconstruct a time pattern and develop connections to understand the evolution of a species,⁶ students of war and peace could use Lakatos' rules of falsification to evaluate alternative explanations and, based on comprehensiveness, parsimony, and predictive capacity, choose those alternatives that deserve further scrutiny while discarding those options that do not conform to empirical reality. Analysts of world affairs frequently talk about taking such steps but have, thus far, failed to accept them when new propositions are introduced.

Using Lakatos' perspective, I will discuss the progress and potential of contending paradigms that dominate the current study of war and peace.

The Realist Tradition

The classic account of international war comes from the realist tradition in world politics. Realists propose that changing distributions of power among nations are directly related to the likelihood of war.

Despite differences, realists argue that nation states are effective units of analysis, and that through their growth and alliance behavior they set the broad pre-conditions for war and peace. This approach to the study of war has a very long tradition that can be traced from Thucydides (400 B.C.) to Machiavelli (1513), to Hobbes (1651), to Hume (1741), to von Clausewitz (1832), to Morgenthau (1948), to Organski (1958), to Waltz (1979), and to Gilpin (1981).

Two contradictory propositions dominate. The first, and predominant, proposes that a power parity among contenders leads to peace while an imbalance in the distribution of power leads to war. The alternative proposition contends that power preponderance or hegemony leads to peace while parity creates the conditions for war. Much of the war literature has developed along these two competing lines of inquiry. Let me consider each perspective in turn.

The Balance of Power Perspective

Balance of power is the dominant theory that adopts the power parity perspective. As the term implies, balance of power contends that an equal distribution of power leads to peace, whereas a power imbalance brings about the necessary conditions for war. Underlying this bold thesis is the assumption that nations exist in an environment of anarchy where power distributions other than parity, as Waltz (1959, 232) aptly argues, lead to wars "because there is nothing to prevent them." In this anarchic international order, decision makers can seldom manipulate their own domestic growth enough to affect relative power distributions. They can, however, alter relative power distributions through alliances or through expansion into regions where potential opponents threaten to gain power. Hence, war is expected when one side or alliance achieves a power advantage over another. The function of alliances is to preserve parity among contending coalitions and provide the weaker nations with security. Under power parity, a great power and its coalition cannot attack another great power and its allies because the costs of war would be prohibitive. Peace is preserved by the fear of war, and not by an intrinsic desire of contending parties to maintain the status quo.

Balance of power advocates concentrate on severe international war of global proportions. Levy's definition of international war is transformed into total global war by Gilpin's (1981) addition of stringent and complex criteria. He claims that only three global wars — the Napoleonic Wars, 1803-1815; World War I, 1914-1918; and World War II, 1939-1945 — have been waged in the last two centuries. Less stringent definitions of major power wars add the Crimean War, 1853-1856; the

Austro-Prussian War, 1866; the Franco-Prussian War, 1870-1871; the Russo-Japanese War, 1905; and the Korean War, 1950-1953 (Levy 1983; Organski and Kugler 1980). Thus, by any count only a handful of global wars have been fought during this period and no more than a score have been waged in all of recorded history (Gilpin 1981; Thompson 1988; Midlarsky 1988).

Attention centers on global wars because the resulting massive destruction of industry and population leaves lasting scars on the productive capacity and population profile of the international community. Global wars also may mark the beginning of many major transformations in the international system (Modelski 1978). Moreover, since the seminal work of Brodie (1946) brought the specter of nuclear conflict into clear focus, the threat of a global nuclear war has been the dominant concern of the peace analysts.

The focus of balance of power is on global war, but implications about lesser encounters are not excluded. Following the dictum of von Clausewitz, most balance of power advocates would argue that "war is the continuation of policy by other means," conceding that asymmetric wars of limited severity cannot be prevented. Thus, minor adjustments among contending coalitions resulting in limited war (i.e., the USSR's 1956 intervention in Hungary, or the United States' 1989 intervention in Panama) that disturb rather than profoundly affect the international order continue to be waged. In contrast, major wars ensue when the equality among large contending coalitions is fundamentally disturbed. After such wars, however, states attempt to avoid dismemberment of key members of contending coalitions to assure that a balance can once more be reimposed.

Despite its wide acceptance and influence, balance of power theory has been exposed to a very limited number of empirical tests, and, with few exceptions, most of these tests have produced negative or contradictory results. The pioneering empirical analysis by Singer, Bremer, and Stuckey (1972) associated power equality with peace in the nineteenth century and with war in the twentieth century. Using Lakatos' criteria such results are spurious or at best suggest a major specification error, however, a flurry of similar investigations preserved the century distinctions and continued to draw contradictory results (Singer 1979, 1980). An overview presented by Siverson and Sullivan (1983) suggested that while Ferris (1973) found systematic support a linkage between power equality and peace, other evaluations reject parity as a condition for peace, or found no systematic relation between power distribution and war. The empirical rejection of balance of power propositions is reinforced by more current findings (Organski and Kugler 1980; Bueno de Mesquita

1981a; Moul 1988; Bueno de Mesquita and Lalman 1988; Houweling and Sicamma 1991; Mansfield 1992). Moreover, explorations of the balance of power theory using formal logic suggest internal inconsistencies in the overall structure. Niou, Ordeshook, and Rose (1989) show that while peace is possible under power equality conditions, very stringent and seldom met conditions -- i.e., full knowledge of the war outcome, or multiple power poles -- must prevail to ensure stability in world politics. Kugler and Zagare (1990) demonstrate that to maintain stability under a power equality, leaders must be unwilling to take risks. Moreover, Bueno de Mesquita and Lalman (1992) reveal that under balance of power assumptions actors must concurrently be risk-acceptant and risk-averse to avoid instability -- a logical impossibility. Thus, despite a long ingrained tradition of accepting power equality as a condition for peace, the lack of empirical support reinforced by logical inconsistency poses grave questions about the validity of this perspective in world politics.

An overview of current events likewise leads us to seriously question the empirical validity of generalizations based on a balance of power model. The collapse of the Warsaw Pact, and the ensuing dismemberment of the USSR, are disturbing events from a balance of power perspective because after the power equality of competing forces was destroyed, and a key actor was dismembered, no war followed. One would anticipate that such events would set in motion the preconditions for major war. Yet most policy observers and practitioners contend that peace has broken out (Mueller 1989). Simply stated, power changes of the magnitude recorded between 1989-1992 are rare and must qualify as systemic changes. Their scope is rivaled only by transformations following World Wars I and II. Yet the destruction of the Soviet Empire and the dismemberment of the Soviet Union itself led to a rapprochement between East and West, have not increased tensions, and have diminished the potential for global war. Such results are inconsistent with the well established expectations of the balance of power theory where war is anticipated when the equilibrium breaks down, and when major actors are dismembered.

The lack of empirical consistency, however, has not affected the influence of the balance of power perspective on national policy, since nuclear deterrence theory retains the same general structure.

The Power Preponderance Perspective

Within the realist tradition, power preponderance theories pose a direct challenge to the conception of balance of power. Power transition, and more recently hegemonic theory, challenge the central inference of

balance of power that power parity leads to peace. Power transition, proposed by Organski (1958) and later modified as hegemonic theory by Gilpin (1981), contends that the international order is not anarchical but hierarchical. Nations recognize their power position in the international hierarchy and acknowledge that their influence will be roughly based on differences in the distribution of power. At the top of the hierarchy is the dominant nation or hegemon, that for most of its tenure as leader of a regime is the single most powerful actor in the international order and controls the status quo.⁷ Below the dominant nation are the great powers, with the potential to overtake the dominant country at some future point in time. In the last 200 years this group has included Great Britain, France, Austria-Hungary, Germany, Russia/USSR, the United States, Japan, China and in the future perhaps India (Waltz 1979, Table 8.1; Organski and Kugler 1980). At any point in time, very few nations can hope to become the dominant world power. Below the great powers are the large powers who are influential but who cannot individually contend for regime leadership. This group includes, among others, Italy, the Netherlands, Spain, Canada, Argentina, Indonesia, Brazil, Australia, South Africa and their like. At the bottom of the international hierarchy are the remaining 100-plus nations that exist within or outside the regime established by the dominant power, and cannot directly challenge for control of the global order.

Organski (1958) implied that within hierarchical regimes nations attempt to maximize net gains and do not disregard the stability resulting from the existing status quo. Relations between nations where the choice to wage war is prompted by expectations of marginal gains from war, and offset by disturbances in the regime status quo resulting from such action, are relations of conditional, not total anarchy (Grieco 1988; Kugler and Hussein 1989; Kugler and Organski 1989; Kugler and Zagare 1990; and Snidal 1991). Here the conditional anarchy assumptions of power transition and hegemony differ substantively from the total anarchy postulated by balance of power.

Power preponderance theories reject the notion that nations maximize absolute gains in power or security and exist in total anarchy. If this were true, a dominant power that became a hegemon would seek to become a universal empire. The empirical record of national interaction refutes such an assertion. Consider. After World War II, the United States emerged as the undisputed hegemon and maintained this condition until the early 1950s, yet it did not seek to create a universal empire. Moreover, preponderance has been present in the international system over the last 200 years. Yet, unlike the predictions of balance of power but congruent with power preponderance, dominant nations did not expand to the limit of their capabilities, rather they sought to

maintain the status quo by preserving the international order (Gilpin 1981; Kugler and Organski 1987). To ensure the stability of a regime and the maintenance of the status quo, nations seek alliances that are stable and reliable instruments not easily altered.

The power preponderance perspective conceives of the organization of world politics in a manner similar to that of domestic politics. National actors accept their position in the political order and recognize influence based on differences in the power distributions among competing political groups. Competition in the domestic and international systems differs only in degree. Despite the absence of enforceable codes of international law, nations, like domestic political groups, are in constant competition over scarce resources and driven by the search for net gains to be accrued from conflict or cooperation. In sum, the objective of nations is not to maximize power or security; rather the objective is to maximize net gains. Peaceful competition ensues when parties agree that the net gains from war are inferior to the net benefits of the status quo; war emerges when the opposite is true. Power preponderance claims that in the international hierarchical system one or more among the major powers may become a challenger to the dominant power. Thus, a nation dissatisfied with the existing status quo will hide its time until it becomes powerful enough to challenge the status quo.

The conditions for global war emerge when the challenger reaches parity with the dominant nation and seeks to establish a new place for itself in the international order. In sum, power preponderance suggests that power equality creates the necessary but not sufficient conditions for waging a global war by giving contending sides an equal opportunity for success; while gross power inequality assures peace. Less dramatic but still asymmetric power relations lead to limited war (Organski 1958; Claude 1962; Organski and Kugler 1980; Bueno de Mesquita 1981a; Gilpin 1981; Houweling and Sicamma 1988; Kugler and Organski 1989; Kugler and Zagare 1990).

Choucri and North (1975) advanced the proposition that lateral pressure theory is an important complement to power preponderance. Lateral pressure theory contends that the competition for power, influence, and global dominance is the source of war in the international system. The reasons for war are both domestic and international. Nations are driven to war by their desire to expand beyond existing borders. When such expansion tendencies clash with those of other nations of equal size, the probability of global war increases as leaders of influential sectors of competing great powers perceive the economic, political, or security dominance of their country being threatened by the

increasing capabilities of challengers. The addition of expansionist tendencies driven by domestic requirements extends the notions of power preponderance. Lateral pressure explains, for example, that the collision of Japan's expansion tendencies with American interests in the Pacific Basin was the reason for the United States intervention in World War II. In sum, lateral pressure theory uses domestic and international events to account for the initiation of major wars.⁸

Empirical evaluations of the proposals advanced by power preponderance have fared relatively well. Organski and Kugler (1980) report that for the last two centuries, where the empirical record is robust, global wars occurred only when a contender achieved parity with the dominant power, and immediately after the transition had occurred. Alliances are shown to be fixed and to determine the outcome in favor of the dominant power, even though that nation is no longer preponderant.⁹ The results linking war with a transition prior to the conflict, and the proposition that a speedy overtaking will trigger war, remain unclear. The early studies, associating the timing of major war with the period immediately after the transition (Organski and Kugler 1980), were challenged by evaluations that link such initiations to the period just before a transition (Thompson 1988). Recent empirical work suggests that the transition itself may or may not add specificity to the timing of war. For example, Kim and Morrow (1990) question the need for a transition to precede war, and show that the speed of closure may also be irrelevant. However, new empirical work consistently associates power parity among major nations with war. Lemke (1991) shows that power transition generalizes to regional wars in Latin America. Werner and Kugler (1993) demonstrates that arms buildups combined with a power overtaking account for 80% of major power wars where territory is at stake, and Vasquez (1992) suggests that territorial contiguity combined with a power overtaking enhance dramatically the prospects for major war.

Indirect evidence also suggests that when power parity is present, adherence to the status quo can moderate conflict. Bueno de Mesquita (1990) shows that the 1866 war between Prussia and Austria-Hungary was waged when these nations reached parity. But because these nations had a history of cooperation (against their main foe, France) and valued the status quo, the war did not escalate. However, Prussia in the subsequent Franco-Prussian War of 1870 escalated the war, since regime dominance was at stake. Therefore, while we still lack effective measures of the status quo, this work suggests that parity leads to restricted war when both nations agree on the international status quo, and to major, total war among great nations that have dramatically different

visions of the world order. Such was the case between Germany and the United Kingdom prior to both World War I and World War II.

A reason why power preponderance theories have gained ground is that they are consistent with empirical reality and provide general results. Recall that Singer, Bremer, and Stuckey (1972) reported that for the nineteenth century an imbalance of power was associated with global war and for the twentieth century power parity was the precondition for such conflicts. The evolving empirical record seems to have shifted from this position. Organski and Kugler (1980), Bueno de Mesquita (1981a), and Houweling and Sicamma (1988) find no such differences. Formal derivations are also supportive of power preponderance arguments. Kugler and Zagare (1987, 1990) show that under parity when challengers are willing to take risks, war can occur, while peace is preserved when the actors are risk neutral or risk-averse. War and peace, then, are tenuously in balance when parity is achieved and national elites play a critical role in determining the choice between conflict or stability. Indeed, parity conditions imply that elites play a critical role in the preservation of peace. Perhaps because power parity is only the necessary but not sufficient condition for major war, that balance of power advocates such as Morgenthau (1948) attributed war to failures of diplomatic balancing efforts and Jervis (1976) ascribes war to misperception. Indeed, Bueno de Mesquita and Lalman's (1992) formal explorations of the internal logic of realism show that power preponderance is consistent with rational expectations for war initiation both under certainty and uncertainty, while balance of power is not, and Kugler and Zagare (1990) demonstrate that balance of power can maintain peace only when risk takers are not present. However, power preponderance theories only identify the necessary conditions for the initiation of major war and cannot account with any precision for the specific reasons that lead to the initiation or avoidance of war within relatively long periods of parity. Decision-making theories, to be considered later, take a stab at this problem.

The Collective Security Perspective

The reemergence of the collective security perspective is motivated by the decline and dismemberment of the Soviet Union following the collapse of the communist block starting in 1989. The debate is vibrant but not new. Following World War I, President Wilson argued that power hegemony rather than equality preserved peace in world politics (Claude 1962). Frustrated with the dire consequences of anarchical competition among nations, Wilson proposed the creation of a League of Nations to prevent the repetition of global

war. The League of Nations would institutionalize a collective security alliance binding major powers to jointly punish, with overwhelming force, all "aggressors" that departed from the legal conventions agreed upon by the League. Because international organizations rather than nations were to decide how to exercise power, President Wilson was incorrectly branded an "idealist" not aware of realist propositions. Wilson was no idealist. The League of Nations he proposed would institutionalize the power hierarchy then in place, and would force all nations to play by the rules established by the major powers. Once national self-determination established the legitimate distribution of contested territories, overwhelming force would be used against any violator of existing agreements. Failure to comply would lead to collective punishment.

Wilson's collective security agreement was specifically designed to ensure peace where balance had failed. In practice, collective security would most likely increase the incidence of asymmetric wars, but such conflicts were deemed a necessary evil required to avoid the much larger and dangerous global war. Even though vestiges of collective security are used to justify asymmetric interventions (Korean War 1950-1953, Kuwait 1991), collective security has not been used to prevent symmetric confrontations among major powers. Claude (1962) argues in support of institutionalized preponderance that empirical tests failed to do justice to the theory. He argues that the League of Nations died because key actors among the major powers did not participate and not because the principle of collective security was untenable. Moreover, the United Nations is an ineffective instrument of collective action not because collective action is impossible but because major powers have granted themselves a veto power in the Security Council, and have denied such privilege to the other members of the international community. The United Nations arrangements are therefore a far cry from Wilson's conception of a hegemonic preponderance that would impose established rules whenever aggression by major or minor powers was detected.

Collective security is frequently confounded with interdependence. Indeed, Keohane and Nye (1977) seem to be applying Wilson's principles to conceptions of the functioning of regimes, when they claim that a hegemon is responsible for distributing collective security within a regime. However, there is a critical difference between Collective Security and Interdependence. Collective Security assumes that nations are interested in preventing all aggression in order to ensure peace in the long run. Thus, if one nation violates the frontiers of another becoming an "aggressor," collective action snaps into place. The alliance of powerful nations would not be deterred from military action against any aggressor, small

or large. Interdependence, on the other hand, focuses on aggression by countries outside of a given regime, assuming that the hegemon would protect regime members by punishing any outside action against them. The hegemon, however, would not punish "aggression" by members of the regime against outsiders.

The preconditions for collective security have not been institutionalized. Recall that after 1945 the United States emerged as the world hegemon and consolidated this position by establishing a preponderant alliance under NATO in the west and SEATO in the east. The Soviet Union and China were the only great powers excluded; India remained neutral. The Soviet Union countered with the Warsaw Pact, and initially obtained support from China. Bipolarity was born. After the Sino-Soviet Split where China detached from the Soviet bloc, the Communist world was fractured but remained distinct from the American led alliance. Perceptions of bipolarity remained. Only after 1989, when the Soviet Union withdrew from Eastern Europe and shed parts of the old Russian empire, has the opportunity to test collective security emerged. Will institutions like the European Community expand to incorporate the new states, thus creating a regional collective regime? Will NATO incorporate Eastern Europe, Russia, Ukraine, Byelorussia and many of the new republics, becoming a collective agreement preponderant over all others? Will a broader Atlantic and Pacific alliance anchored by the United States survive or will it splinter with the demise of bipolarity? Will the United Nations evolve into a world collective security regime and become the focal point for a power preponderance that averts major confrontations? Evidence to support or reject such claims is lacking. Yet, the jury is at last in the courtroom and the evidence is starting to unfold.

It is perhaps useful to contrast the implications of the various realist perspectives on nuclear deterrence and proliferation to assess the likelihood of nuclear war in our lifetime.

The Nuclear Perspective: Nuclear Deterrence

Theory is important not only when it is confirmed by evidence but also when it is acted upon. Recall that ancient navigators feared crossing oceans long after their equipment was adequate for this task, because they assumed a flat world and concluded that upon reaching the ocean's edge their crafts would spill into an abyss. The preservation of peace in the nuclear era is not much different from the problem faced by ancient navigators. The fear of the "unthinkable" consequences associated with nuclear war have forced practitioners to adopt untested postures that hopefully can prevent a nuclear holocaust. Nuclear policies are advanced and

adopted not because there is experience with the outcomes, but because the theory suggests what choices could reduce the danger of war. Thus, despite very tenuous support for parity assumptions during the pre-nuclear period, deterrence policies from the logic of Balance of Terror to Mutual Assured Destruction are derived from the balance of power perspective. Unlike the ancient navigators, however, we cannot, and do not wish to, experiment with nuclear weapons to ascertain the validity of our assumptions about deterrence. Such tests would potentially decide not simply the life of a crew, but the life of large portions of humanity and even the human species.

Before 1945, few realists disputed von Clausewitz's conception of war as "the continuation of policy by other means." Yet, following the bombing of Hiroshima and Nagasaki in 1945, Bernard Brodie contradicted this widely accepted axiom and built the cornerstone of deterrence: "Thus far the chief purpose of our military establishment has been to win wars. From now on its chief purpose must be to avert them. It can have almost no other useful purpose" (Brodie 1946, 76). Power politics moved from understanding how to wage wars to illuminating how to prevent them. For this reason, the theory of deterrence plays a critical role in world politics.

Nuclear deterrence theory shifts the focus from power as conventional capabilities to power as nuclear capacity. It is defined as the ability to prevent attack by a credible threat of unacceptable retaliation. The writings of Brodie (1946, 1959), Wohlstetter (1959), Kaufmann (1956), Kissinger (1957) and most recently Intriligator and Brito (1987) and Buzan (1987) among others, helped define deterrence as a separate field (for an excellent historical review, see Freedman 1981).

Massive Retaliation was the first deterrence policy explicitly adopted by the United States. Under Massive Retaliation, a preponderant nuclear defender credibly threatens to devastate non-nuclear opponents when provoked. The expectation is that nuclear terror can credibly compel potential opponents to avoid challenges they would otherwise initiate.

With the advent of nuclear parity, deterrence reverts in its core to the balance of power model proposed in a multilateral context by Hume (1741) and Morgenthau (1948), adapted to a bilateral environment by Waltz (1979) after World War II, and formalized using the tools of social choice by Niou, Ordeshook, and Rose (1989). Balance of Terror and Mutual Assured Destruction, the two dominant nuclear deterrence strategies after Massive Retaliation, explicitly adopt equilibrium principles from balance of power.

The Balance of Terror is achieved when both sides have nuclear capabilities but their arsenals are not

large enough to assure retaliation if attacked by the other side first. Wohlstetter (1959) was the first to note that a nation with a limited nuclear arsenal could not avert a preemptive strike by threatening to retaliate, if most of the weapons available would be lost while enduring the first strike. To compensate for this uncertainty, a nuclear nation would have to retaliate when the first warning of an impending attack was detected. Thus, Balance of Terror is tenuously unstable until a credible second strike capability is developed (Bottome 1971). In practice, the specter of unstable deterrence remains through the nuclear period and is brought to life by Eisenhower's concern about the bomber gap, Kennedy's apprehension regarding the missile gap, and Reagan's worries about the "window of vulnerability" to a preemptive first strike.

The basic contention of Mutual Assured Destruction under balance of power assumptions is that equality of nuclear capabilities and secure second strike capabilities on both sides minimize the likelihood of war, because the costs of war become "unacceptable." The logic of Mutual Assured Destruction is devastatingly simple. Well informed, closely matched contenders, capable of destroying an opponent with a retaliatory strike, are aware that war among them will result in enormous damage to themselves and by extension to many other parties. As the capacity to wage a nuclear war increases, deterrence becomes increasingly stable.

William Riker (1962) provided strong impetus to deterrence by deducing that in a zero-sum, multi-actor, interactive environment minimal winning coalitions should form among contending parties. Riker shows that the costs of increasing a coalition beyond the number necessary to win by a small margin are prohibitive. These results provide a strict, logical justification for the automatic creation of equilibriums among contending nuclear poles. Riker's argument reinforces Claude's (1962) suggestion that in the nuclear era there seems to be little need to require purposeful intervention by competing actors to establish equality among competing poles. Thus, following the logic of balance of power, Mutual Assured Destruction assumes anarchy, allows for flexibility of alliances, and ensures stability due to the enormous absolute costs of nuclear war (for a discussion of automatic and purposeful balances see Morgenthau 1948; and Claude 1962).

Intriligator and Brito (1987) are to my knowledge the first scholars who provide a very clear graphic and internally consistent outline that transforms verbal statements derived from the balance of power perspective into structured equations. Their summary provides a temporal connection between the successive deterrence policies.

This structure directly links the period of Balance of Power, when conventional weapons were the only means of destruction, to the period of Massive

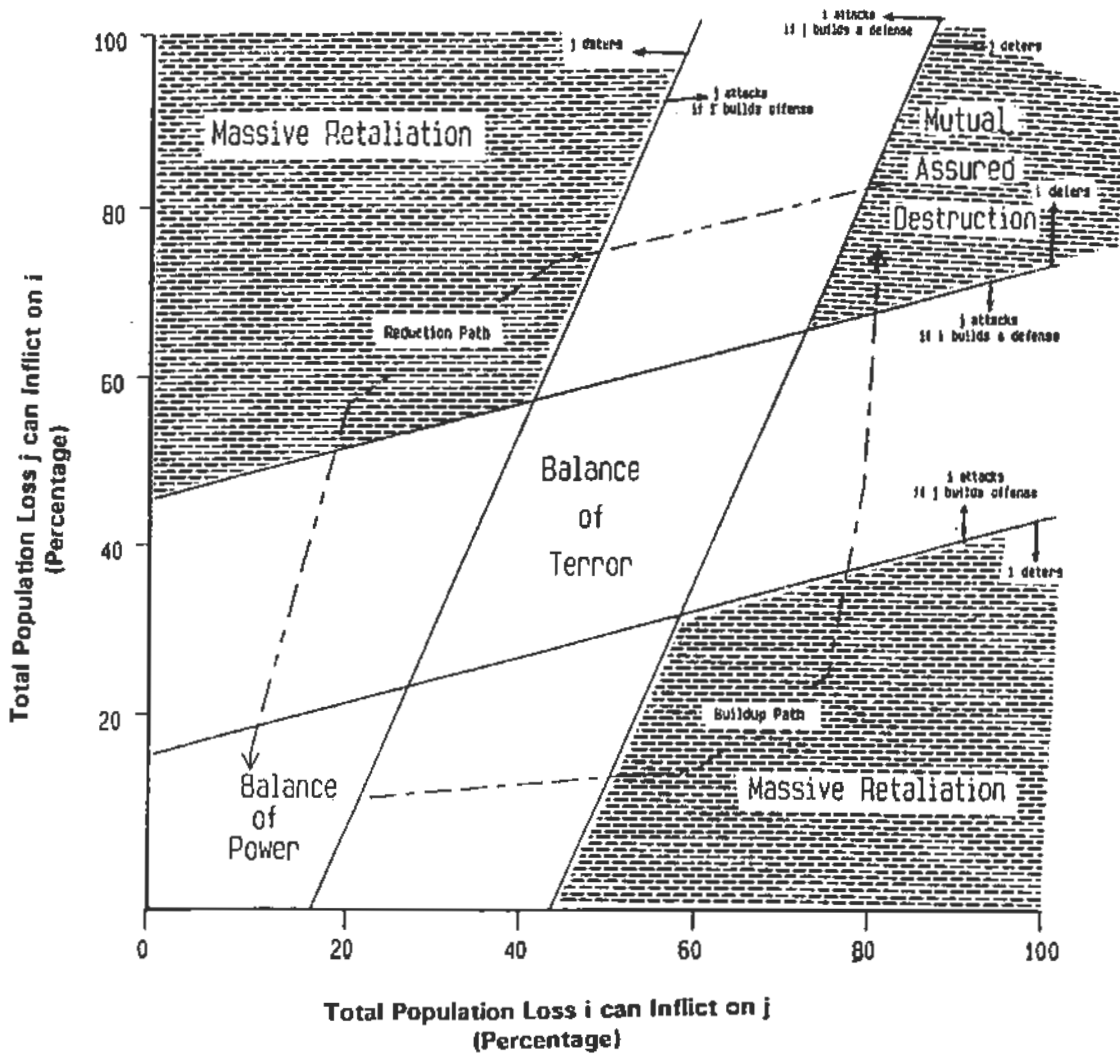
Retaliation, when the nuclear monopoly held by the United States could compel opponents, to the period of Balance of Terror when limited nuclear arsenals were available to both sides and finally to the period of Mutual Assured Destruction, when nuclear nations could rely on a secure second strike capability.

Like Brodie (1946, 1959), Intriligator and Brito assume that when nations anticipate that the costs of war exceed a threshold above which they are no longer willing to initiate war, they will fight only in self defense. When a second threshold is exceeded, a nation is no longer willing to confront the opponent and it will be deterred from war or yield to demands. Therefore, the possibility of war exists only when costs are "acceptable." Unstable conditions are found first when contending actors only have conventional capabilities and cannot impose sufficient costs to deter opponents. Conventional parity and preponderance are both conditions for war, but the first leads to severe wars. The possibility of war is also given in the areas of transition from the region of Balance of Power to Massive Retaliation and Balance of Terror, from Massive Retaliation and Balance of Terror to Mutual Assured Destruction. During such transitions at least temporarily one actor cannot fully deter the other and war is possible. In the stable area of Massive Retaliation there is still a threat of war. If the preponderant nation fails to adhere to the principle of retaliation only when attacked, a preemptive war would follow. The ultra-stable condition of Mutual Assured Destruction is attained only under redundant nuclear parity. Here all sides will choose to yield rather than fight because the costs of war initiation are "unacceptable." Finally, under the Balance of Terror, nations that fear preemption and do not have enough weapons to assure a retaliatory strike might preempt first. Thus, a Balance of Terror is tenuously unstable.

The critical difference between unstable power parity under a Balance of Power, or Balance of Terror, and stable parity under Mutual Assured Destruction, is the absolute cost of war. Under a Balance of Power, war can be waged because the cost is relatively low. Under the Balance of Terror, war can be waged for fear of preemption. But under Mutual Assured Destruction, costs increase beyond acceptable thresholds and ensure stable deterrence. Paradoxically, further "overkill" provides a cushion against technological breakthroughs or an uneven deployment of new generations of strategic weapons. Nuclear arms races ensure peace. The costs of war are the key difference between the nuclear and pre-nuclear era.

Short of a nuclear confrontation, however, it is impossible to empirically test nuclear deterrence and determine if nuclear weapons have prevented the recurrence of war. By itself, the absence of a nuclear

Figure 1



LEGEND

-  Stable Deterrence
-  Unstable Deterrence

war after the bombing of Hiroshima and Nagasaki provides little evidence for or against nuclear deterrence (Morgan 1977; Modelski and Morgan 1985; Kugler 1984). Systematic work has established that the necessary but not sufficient conditions for global war are rare indeed (Organski and Kugler 1980; Bueno de Mesquita 1981a; Houweling and Sicamma 1988). It is possible that the addition of nuclear weapons to the arsenals of major powers has prevented a global war, as classical nuclear deterrence suggests. Yet, it is equally plausible that the peace enjoyed by major powers after 1945 is simply due to the lack of convergence among the rare factors that lead to massive confrontations. It is not enough to assert, for example, that nuclear deterrence worked when the United States enjoyed superiority simply because the Soviet Union did not attack Western Europe after 1945. How do we know that the Soviet Union wished to attack? Furthermore, implications following from classical nuclear deterrence assumptions would suggest that the United States should have attacked the USSR after 1945 before the USSR achieved nuclear capabilities, or that it should do so now because of the opportunity created by the dismemberment of the Soviet Empire. Yet, such assumptions are clearly not consistent with national behavior.

Most damaging to the claim of classical nuclear deterrence are studies of war which suggest that conventional weapons can be used to account for the absence of global war in the international system. Bueno de Mesquita (1980) shows that conventional weapons account for the outcome of almost all crises and wars since 1945. Organski and Kugler (1980) find that nuclear deterrence has not affected the way crises have been resolved since 1945. Indeed, nuclear nations won as frequently as they lost in crises or wars that called for nuclear confrontations. Huth and Russett (1984) and later Huth (1988) show that factors other than nuclear weapons account for the relatively low level of conflict registered after 1945. All such tests are, however, inconclusive. Achen and Snidal (1989) argue persuasively that many potential confrontations may have been avoided before they started, because contending nuclear parties already understood that nuclear weapons would be called upon to settle such issues. Indeed, short of a nuclear war one cannot directly dismiss Brodie's (1946) claim that the only acceptable role of nuclear weapons is to prevent nuclear war and not to wage or win such a war. However, one can also make the equally valid claim that the introduction of nuclear weapons raised the stakes in a global war, but did not fundamentally change the behavior of nations. The jury on this issue is still out (for alternative evaluations of the same evidence see Huntington 1982; and Kugler 1984).

There is no doubt that classical nuclear deterrence theory based on the concept of nuclear

retaliation must be identified as a major contribution of international relations theory to peace in the post-war era (for a recent review see Harvey and James 1992). Yet, following the collapse of the Soviet Union one must ask: is deterrence congruent with reality or is deterrence a well established but untested myth? Like the ancient mariners who navigated further and further without encountering an abyss, following the collapse of nuclear bipolarity many practitioners will no doubt ask, what ensures peace under preponderance?

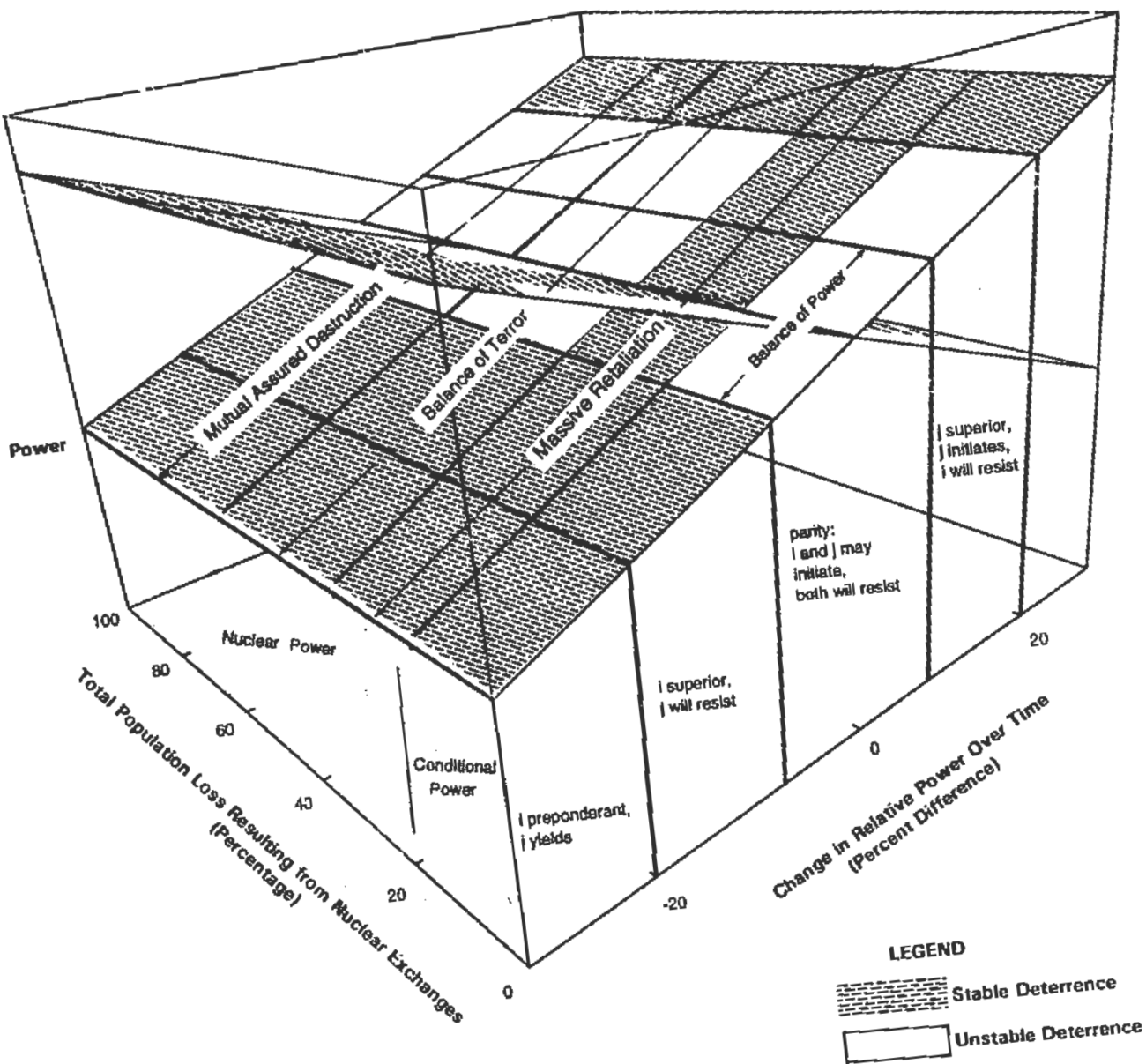
Certainly, many alternative propositions will emerge. Yet, one existing alternative to classical nuclear deterrence is provided by the perspective of power preponderance. If the international system is hierarchically ordered, if actors are constrained by the status quo, and if power asymmetry rather than equality ensures stability, then what can be deduced about stable deterrence?

Kugler and Zagare (1990), using power transition as a point of departure, suggest that nuclear deterrence has been stable since 1945 because power asymmetry was the rule, and because in the contest among the two superpowers, the contender did not achieve marginal superiority over the dominant nation. Their version of deterrence can be graphically presented as shown in Figure 2.

This figure summarizes dynamics and levels of conflict anticipated by the power preponderance perspective in the nuclear era. Like Intriligator's and Brito's perspective, the preponderance model anticipates that large disparities in power would lead to peace. Thus, under Massive Retaliation, characteristic of the early part of the nuclear era, the interaction between non-nuclear and nuclear nations is expected to be stable. As the capabilities of a non-nuclear power increase, so does its ability to resist demands by the preponderant nuclear actor. The conditions for a limited nuclear war increase as parity is approached, if the rising nation is not a satisfied status quo power. Limited conflicts waged at this stage are frequently won by the dominant power. The few decided in favor of the weaker side occur when the costs endured by the challenger are much higher than those suffered by the dominant nation (i.e., Vietnam, Afghanistan). The most serious conflict is anticipated when the challenger overtakes the dominant nation in power terms and matches its ability to destroy (Organski and Kugler 1980; Houweling and Sicamma 1988; for alternate timing see Thompson 1988).

Deterrence under power parity is therefore tenuous.¹⁰ Parity provides the necessary conditions for peace and serious conflict. The challenger can make demands and wage a war with a probability of success that is equivalent to that confronted by the deterrer. The risk of taking such an action is high, but marginal probing by a risk taker should result in some gains that

Figure 2



may eventually lead to escalation if repeated. Deterrence is determined by the joint decision of both nuclear actors not to act -- if one chooses to break the compact, the terror of nuclear weapons cannot deter the initiation and eventual escalation of such conflict (Kugler and Zagare 1987; Powell 1990; Zagare 1990). Parity is the necessary but not the sufficient condition for war. Choices are open to decision makers whether to initiate or to avoid war under specific circumstances. From the perspective of the power preponderance, therefore, the Balance of Power, Balance of Terror, and Mutual Assured Destruction are all policies that share in common one condition: tenuous stability.

In the nuclear era, no country has thus far traversed the path to power parity and nuclear parity. In the period between 1945 and approximately 1960, the preponderance of power and nuclear capabilities held by the United States ensured stability despite the Cold War. Once parity in nuclear weapons was achieved in the mid-1960s, stability was maintained by the continuing disparity in capabilities between the United States and the USSR. The convergence in nuclear capabilities was never accomplished by power parity. Thus, the USSR would have had to initiate a conflict against an opponent whose power was far superior. As new information about the Soviet economy has become available, it is clear that the power of NATO was overwhelmingly superior to that of the Warsaw Pact. Since 1945, the preconditions for a major war have not been met.

The implications for today's world are telling. A nuclear overtaking may be very difficult to achieve by any superpower. Yet, the potential for a nuclear war has not disappeared from the world arena and may be enhanced by nuclear proliferation.¹¹ Continuing disputes in the Middle East, or confrontations among the emerging splinter republics from the old Soviet Union, may still provide gruesome testimony to the failure of deterrence. In a period of nuclear parity, then, effective negotiations about the status quo rather than nuclear terror are the tools that can prevent war.

Nuclear Proliferation

A second aspect of the nuclear era is the Nth nation dilemma. This process is attracting increased attention as the Soviet Union splinters into its component republics and as nations like Iraq, Iran, or Korea seek to achieve nuclear status. Like the ancient navigators who did not venture far from the coastline, it is impossible for political analysts to ascertain empirically whether nuclear deterrence is stable or tenuous short of a nuclear war. Yet, as more nations achieve nuclear parity with chosen opponents, this question becomes more and more relevant. One may, therefore, assess the validity of the

competing perspectives by looking at implications and contrasting them with actual national behavior.

Consider first the balance of power perspective. Logically if one argues that Mutual Assured Destruction is stable, one must conclude that extending the nuclear balance will secure stability for a larger number of nations. Many scholars advocate the carefully managed proliferation of nuclear weapons to allies to reduce the risk of accidents, while enhancing regional peace. Rosen (1977), perhaps the most explicit and eloquent advocate of the introduction of nuclear weapons into the Middle East, argues that such capabilities should have the same salutary effects on the Arab and Israeli contenders, as they have had on the United States and the Soviet Union at the height of the Cold War. Waltz (1981) likewise argues that if Mutual Assured Destruction is stable, expanding the scope of nuclear deterrence should dramatically reduce the likelihood of war whether contenders are large or small. Intriligator and Brito (1981), and Bueno de Mesquita and Riker (1982) are somewhat more reserved, advocating restricted proliferation to allies or nations in danger of being attacked. Such managed nuclear proliferation would reduce the possibility that a regional nuclear contest could transform itself into a world confrontation by linking the defense of a weak nation to that of a superpower.

Nuclear proliferation has not gained acceptance. Nuclear nations have restricted access to nuclear technology and have not seen fit to proliferate nuclear weapons strategically in the hopes of enhancing regional stability. Active prevention seems to be the rule. The coalition of nations led by the United States that frustrated Iraq's attempt to incorporate Kuwait, for instance, was to some degree motivated by the fear that Mr. Hussein would acquire nuclear weapons and threaten his neighbors. Indeed, a major priority of UN inspectors in Iraq has been to remove every nuclear device from that country. However, Israel has not been pressured to remove its nuclear devices to balance efforts in Iraq. Iran's attempts to acquire nuclear weapons are monitored but not yet threatened. Why?

Leaders of nuclear nations that practice MAD as a deterrence strategy do not follow the nuclear proliferation tactic consistent with MAD. This policy inconsistency fails to meet Lakatos' demand that the inferences from a theory should consistently reflect empirical reality. A second alternative is to accept inconsistency. Indeed, many policy analysts who argue that mutual deterrence is viable, reject the logical links to proliferation on common sense grounds (Epstein 1976; Nye 1990). Again, Lakatos' criteria of consistency force us to reject this option, because it allows support for a desirable aspect of a theory while rejecting its undesirable implications.¹² A third alternative is that the

behavior does not conform with the theory. Jervis (1984) suggests this line of inquiry, but does not abandon Mutual Assured Destruction as the superior option.

Following the dictates of Lakatos, if the theory itself is flawed, so are the deductions. One reason we act contrary to the fear of falling from the edge of the Earth, is we fear the consequences of a tenuous nuclear deterrence. Consider the implications.

From the perspective of power preponderance, avoiding nuclear proliferation to smaller countries ensures stability. Proliferation is, as practitioners have recognized, a very dangerous practice unlikely to produce lasting peace. As the number of actors who achieve nuclear parity increases, the probability of a regional or a global nuclear war likewise increases. Thus, nuclear deterrence is stable under parity conditions provided that competing actors do not want to challenge the status quo and maximize long-term stability, but may lead to war if such actors, either by accident or design, opt for confrontation. Nuclear weapons are, from this perspective, as dangerous as their conventional counterparts. Maybe nuclear weapons have not been used, thus far, simply because unilateral destruction by supporters of the status quo is unacceptable under conditions of preponderance and the tenuous nuclear parity among the superpowers was short lived? If such deductions are accurate and proliferation leads to regional parity, nuclear war may well be in our future.¹³

I have thus far reviewed the realist perspectives that accept the basic premise that nations, power distributions, and national security are the key elements required to understand world politics. Let me now explore important contributions to the understanding of war that depart from this perspective.

The Global Perspective: The Cycle of War

Basic challenges come from analysts who dispute the core assumption of realism that war emerges from an interaction between nations. Based on extensive observation of historical trends, the English historian Arnold Toynbee (1934-1954) proposed a cycle of war and peace that coincided roughly with a period of a little more than one hundred years. Each cycle started with a global war, was followed by a peaceful period, then a smaller war, a pause, and finally another global war. Toynbee attributed this repetitive pattern to the war weariness of the combatants. He reasoned that the generation directly involved in major war would be reluctant to re-enter a similar war. However, the generation once removed from a global war, enticed by writings that glorified war and encouraged by victories in

smaller wars, would once more view global war as a viable alternative to advance national goals. Toynbee's vision is congruent with the post World War II period, where a major victory by the United States resulted in a long peace punctured only by a draw in the Korean war, then a defeat in Vietnam, and consolidated by the success in Kuwait. Americans and Europeans, Toynbee would argue, have overcome the weariness of global war and might seek to once more create or enter a global war. Toynbee's argument is persuasive; however, the anticipated cycle fails to materialize under systematic scrutiny of the historical record (Blainey 1973; Thompson 1988).

Exploring how a global cycle interacts with war is not without promise. Recent interest centers on whether major war is related to a long cycle of war leadership, or to a Kondratieff economic long-term cycle. In both, a strong relationship is anticipated between the initiation of major wars and the performance of the global economy (Modelski 1978; Goldstein 1985; Thompson 1988). In cycle theory, global war is associated with systemic world fluctuations over which national decision makers have no direct control, and nations are pawns in a larger game. Nations, like surfers, can maneuver in and out of war during the buildup of a wave, but are dragged under by forces beyond their control when the wave breaks, dragging nations into major confrontations.

Goldstein (1985) and Thompson (1988) show that the evidence for and against Kondratieff economic cycles and war is very tentative. In part, this uncertainty is due to a lack of clear specification of economic and political cycles and in part to data limitations. Cycle theory requires long time series where almost no reliable evidence exists, forcing compromises that foster further controversy about the structure of detected cycles. A variant of the long cycle theory was developed by Modelski (1978), then extended and tested by Thompson (1988). These scholars postulate that a regular cycle of war is associated with changes in world leadership over the past five centuries. They center on the global implications of a system dominated by a single state, and focus on the control of sea lines that insure trade dominance. The global power seeks naval dominance to maintain an economic system that strengthens trade. The costs of such leadership are high, allowing new rivals to emerge, leading to succession in world leadership. From this perspective, the two world wars are the result of declining British leadership and the failed attempt by Germany to compete in world markets. Similar patterns are detected when the Netherlands and Portugal lost their superiority as major naval powers, after having colonized much of Africa, Asia, and Latin America. The anticipated American decline should lead to similar instability.

The cycle of world leadership is plausible, but the evidence at hand seems insufficient to accept or reject. First, the focus on naval strength as an indicator of overall power produces further distortions. Major historical powers like Sweden, Russia, Austria-Hungary, Prussia, Poland-Lithuania, and France had no major sea profile and are undervalued despite their historical importance. Further, victory in global wars was decided by land fighting and not naval warfare. Recall that Europe was altered over time by major wars fought on land: the collapse of the Polish-Lithuanian confederation led to the rise of Russia in the East and Prussia in the West; Napoleon's largest battles were fought against Prussia and Russia; the destruction of the Austro-Hungarian Empire was the major outcome of World War I. Sea power had little to do with such transformations. Significantly, except for the period of Pax Britannica, in Midlarsky's (1988) and Kennedy's (1987) historical evaluation of the rise and fall of great powers and the evolution of world wars share little in common with the picture derived from the cycle of world leadership. The actors differ and so do many of the identified confrontations. The main difference can be traced to the definition of power. Thus far, analysis of alternate time series produces sometimes promising and sometimes contradictory results for the cycle theories (Thompson 1988 and 1992).

Many of these criticisms are now being confronted. Rasler and Thompson (1992) have expanded the temporal domain to allow more effective testing. They have redefined power to include naval and conventional military capabilities. They have introduced both domestic fiscal constants and cycle effects to account for long-term transformations that may be associated with major war. The new approach is vibrant and intriguing. If nations are merely pawns in a larger world structure, can war be averted? What is gained by direct national intervention?

The question posed by cycle theory is central in a nuclear world. What are the prospects of averting nuclear war if or when the next wave crashes? Modelski and Morgan (1985) suggest that nuclear deterrence may well be unstable. They find that the ability to manage cycles is not part of nuclear deterrence, that credibility has not been demonstrated to be a significant factor in a historical context, and that the absence of major war thus far coincides with the most benign phase of the power cycle. Modelski and Morgan, like advocates of power preponderance, conclude that nuclear deterrence is at best tenuous.

A different approach to cycle theory considers the relative position of a nation in respect to all others in the international system. Taagepera (1986) shows that all nations and empires have a characteristic relative pattern of rise and decline over time. Kennedy (1987)

documents the rise and fall of great empires and associates such changes with military overspending. No nation or empire has persisted forever. This well documented phenomenon of rise and decline characterized the expansion and contraction of Rome, the Mongol Empire, Venice, the Ottoman Empire, Spain, France, the United Kingdom, Russia, and in the distant future perhaps the United States.

Doran links relative power decline to account for cycles of war (Doran and Parsons 1980; Doran 1991). Great nations emerge from the pack of smaller nations and expand to their maximum strength to become a dominant nation when their power is relatively larger than that of others. However, this dominance is followed by a leveling off period, and terminates after a period of decline when a new competitor catches up and overtakes the dominant nation. Using the cycle of relative power, Doran tests the link between stages in a nation's relative cycle and the decisions to initiate war. Relative cycle theory departs fundamentally from other power distribution models because it rejects interaction among states as the main cause of war, and instead purports that domestic factors drive the external actions. The specification of the theory remains somewhat vague regarding the specific phase in the relative power cycle when war is waged. There is also some lack of clarity regarding the severity of war.

Empirical evidence regarding the relative cycle of war is, thus far, mixed. In their early work, Doran and Parsons (1980) suggested a strong relationship between the relative cycle of power and war. Thompson's (1988) follow-up study tends to discount or diffuse the earlier findings. However, Doran (1991) has recently revised and restructured his argument and finds strong empirical congruence. Moreover, Houweling and Sicamma (1991), combining power transitions with the relative power cycle, improve dramatically previous accounts for the waging of major power wars. When these two conditions coincide — (1) two major nations reach a power balance; and (2) the dominant nation is in relative decline — major war is waged. Cycle theory recasts the relationship between power and war in ways congruent with power preponderance models and seemingly adds to them. The results are encouraging but preliminary.

The empirical jury on cycles and relative power is still out, but the direction seems promising and new extensions may produce further fruitful results.

The Arms Race Perspective

Just prior to World War II, Richardson (1960a) argued that the result of an unstable arms race is war.

National elites react to the buildup of armaments by opposing nations with buildups of their own. Richardson's logic is disarmingly simple. When competing parties reach an equilibrium in their arms expenditures, a tenuous stability is attained between arms expansion and reduction. An unexpected increase leads to an arms race and war, while an unexpected decrease leads to disarmament and peace. Smoker (1964) asserted that the fear inspired by an arms race would lead to war unless a restraining effect kicked in and forced the two countries to back down. Smoker explicitly expanded the connection between war and arms races, suggesting that the weaker of the competing sides would initiate the war because, unable to compete, the weaker nation is forced to preempt or be left behind.¹⁴ Huntington (1958) suggested a distinction between qualitative and quantitative arms races, indicating that naval arms competition did not lead to war while mobilization did so prior to the two world wars. These intuitively appealing insights have been explored in some depth.

Richardson was unusual among early formal scholars because he compiled the data required to test his propositions. His empirical analysis shows an explosive arms race between Germany and the United Kingdom before 1939, and forecasts the initiation of World War II. Other empirical tests produced less supportive results. Wallace (1979) originally found a very strong relationship between arms races and the escalation of disputes to war. His results seemingly resolved the long-standing conflict regarding the relationship between arms races and war. However, Wallace's study was severely criticized on methodological grounds for breaking down multilateral wars into separate dyads and thus inflating the results (Weede 1980). Furthermore, Wallace's arms race index not only placed inordinate weight on the last two years of the arms race but also failed to adequately capture the interactive component that Wallace believed to be integral to arms races. Follow-up studies on Wallace that explore the relationship between arms races and escalation to war have suggested either weak or very confounded and complex results (Smith 1980; Horn 1987). Siverson and Diehl (1989, 212) conclude after a thorough survey of the literature that "if there is any consensus among arms race studies, it is that some arms races lead to war and some do not." Like Siverson and Diehl, most researchers in the field now concur that the relationship between arms races and war is very weak (Diehl 1983; Diehl and Kingston 1987; Horn 1987). Supporting this perception, Morrow (1989, 502) states that "rather than asking whether arms races lead to war, we should ask which arms races lead to war."

Recently, arm buildups have emerged as an important focus of research on wars. Studies in this field indicate a significant distinction between arms races,

which are based upon the interaction and acceleration effect across countries; and arms buildups, which reflect unusually large defense expenditures by our nation. As Richardson's original equations specified, an arms race required that the level of military expenditures of nation A be determined at least in large part by the level of military expenditures of nation B and vice versa. For every action, there would be a reaction. The model of an unstable arms race suggested that the rate of acquisition would become increasingly faster. Military buildups do not incorporate interactive concepts. They assume, instead, that if nation A begins to increase its military expenditures, nation B can match that threat in many different ways -- only one of which is to match its expenditures to those of its opponent. Thus, one nation's military buildup, or "abnormally" high level of growth, refers to the "normal" buildup of that nation.

Zinnes, Gillespie, and Rubinson (1976) formally show that with minor changes in the structures of Richardson's equations, arms buildups could result from competition among domestic groups. After every over-expenditure or under-expenditure, national elites respond to domestic constraints and return to the equilibrium point following a self-regulating process. Empirical evaluations of arms expenditures show that arms buildups, suggestive of a military industrial complex, seem to dominate; while external factors, suggestive of an interactive arms race, are far less influential (Ostrom and Aldrich 1978; Kugler, Organski, and Fox 1980; Ward 1984; Ostrom and Marra 1986; and McGinnis 1991). Contrary to Richardson's expectations, therefore, empirical analysis strongly suggests that domestic pressures fuel arms buildups, while the external competition has a relatively small impact.

From the perspective of war studies this is unfortunate. The domestic arms buildup model accounts for budgetary allocations, but does not provide direct assistance in understanding the initiation of war. Indeed, war may occur during an arms buildup, an arms reduction, or when arms acquisition policies among competitors are stable. The absence of a direct theoretical link between arms buildups and war encourages ad hoc arguments that posit positive as well as negative relationships between military increases and the escalation of violence. A positive relationship is generally defended on the basis of an "armaments-tensions" circle to which arms buildups allegedly contribute (Singer 1958). Conversely, the peace through strength argument suggests that a military buildup may actually diminish the likelihood of war (Morgenthau 1948).

Recently, Werner and Kugler (1993) persuasively argues that it is necessary to focus on the context of power distribution within which the buildup is

taking place in order to understand the theoretical connection to war. Linking the structural conditions of power configurations with the choice by national leaders to initiate an abnormal period of arms acquisitions, Werner finds a very strong and consistent association with war.

The arms races-arms buildup literature is an excellent example of a successful interaction between empirical investigations and effectively specified theories. While the cumulative evidence indicates that the original connection between war and arms races may not be robust, it leads to the specification of arms buildup propositions that are robust. This research led to the discovery that is critical to assess the links between war and arms buildups.

The appeal of arms races is still quite strong in the world of practitioners, and there is delay in transmitting the notion that arms races and war are not directly connected, but that arms buildups seem to be. However, practitioners seem to have dismissed unwarranted implications. President Reagan, for example, advocated the buildup of arms in order to force the Soviet Union to agree to American peace overtures, not to generate war. Mr. Gorbachev initiated many unilateral arms reductions without waiting for an American response or obtaining one. President Bush seems to have adopted a similar strategy. Arms negotiators may perhaps look at these actions and reconsider the implication of Richardson's model that mutual, linked moves are the most effective way to achieve arms reductions.

The Decision Makers' Perspective: Group Preference and War

Students of decision making provide a very different reaction to the realist controversy about the initiation of war. Contributors to this branch of war studies argue that for far too long it was assumed that world politics was played in a field where all nations were like billiard balls of different sizes or that these nations allied according to neutral principles of power maximization, and otherwise conformed to similar rules of behavior. Decision making models stress differences among nations and emphasize, in particular, the vast disparity in the preferences that elites wish to maximize.

Allison's (1969) classic treatment of the Cuban Missile Crisis provided a major impetus for this work. Allison posited that elites and pressure groups had far more to say about the initiation of war than relative power structures. Moreover, he argued that assumptions of a common, self-evident national interest were grossly violated in practice. Finally, Allison offered a set of

general criteria and rules that could help account for war initiation. He suggested that to understand a confrontation and its resolution or escalation to war, one needs to know the goals of elites, the options that are available to manipulate outcomes, and the probability of success the elites attach to each option. With this summary information, decision makers could "rationally" minimize costs by choosing the option that left them least exposed and provided the maximum probability of success. If the first policy failed, the decision makers could choose the next option along the ladder that minimized costs and provided an increased probability of success. If none was available, the crisis ended. Differences in decision patterns emerged. Some elites might choose options that maximized the probability of success while minimizing cost, while others would first minimize costs and then maximize the probability of success. Indeed, a decision maker like Hitler, unlike Kennedy, for example, might have chosen to invade rather than blockade Cuba. Neither, however, would have chosen nuclear war as the first option to settle that crisis. Such rhetoric was simply a bluff.

As an alternative to this "rational actor" model, Allison then developed the more complex "organizational model" that introduced the possibility that different groups within one nation would hold competing goals thus advocating alternative policies. Elites could no longer simply strive for a set goal by minimizing their costs while attempting to maximize the probability of success, but had to struggle with each other. Allison did not provide the formal structures necessary to solve the problem posed by the increased complexity and failed to note that the "organizational model" was simply an extension of his simpler "rational actor" model. However, he opened the door to the introduction of positive political theory to study war (for a critical review of Allison's work see Bendor and Hammond 1992).

Bruce Bueno de Mesquita (1981a) was the first to make systematic use of Allison's insight. He made a major contribution to the field of international politics by introducing to international relations a deductive perspective.

Bueno de Mesquita's (1981a) seminal *The War Trap* forced students of war to consider a formal, deductive specification of the necessary conditions for war and peace. He provided clearly specified variables and empirically tested propositions deduced from explicitly stated assumptions. Bueno de Mesquita assumed that nations are led by single strong leaders, that such actors maximize net gains in confrontations, that risk is a variable connected to individual decision makers, and that divergent preferences for competing goals held with varying degrees of commitment are at the root of war. Unlike realists who considered power as the key

variable, Bueno de Mesquita argued that to account for war one must also consider policy goals, commitments, and risk propensity. Power provides elites with the ability to execute policies that seek to maximize their net gains. Power is not the end itself.

This decision making perspective, which has roots in micro-economic theory, generated a very dynamic and still growing literature that concentrates on war initiation. Lalman's (1988) extension allows differentiation between war initiation and escalation. Morrow (1985) extends this analysis to competitive interaction among dimensions such as trade, human rights, and diplomatic recognition. This approach allows scholars to concurrently assess policies while varying the goals of competing elites. The promise of cumulative work is starting to be a reality.

Empirical results are general and supportive. Bueno de Mesquita (1981a) shows that expected utility provides the necessary but not sufficient conditions for war. The incidence of war is over-predicted by a wide margin because the model fails to effectively discriminate between war and non-war before a dispute does occur. But once a crisis develops, calculations of net gain accurately account for the escalation and termination of disputes. Results are very specific. Once a crisis starts, net gains analysis distinguishes between asymmetric and symmetric wars, anticipates when wars will be limited and when they will escalate, determines when confrontations will remain bilateral and when they will become multilateral, and indicates how a war will terminate (Bueno de Mesquita 1981a, 1985; Bueno de Mesquita and Lalman 1986). This approach is powerful because all wars can be treated without regard to the number of actors, or to questions of major or minor power participation. Moreover, expected utility applies to wars regardless of their intensity, and the logic can be extended from international to domestic confrontations (Muller et al. 1991). The main limitation is that a static picture of evolving confrontations is presented that depends on short-term updates to account for war dynamics. Some of these limitations are now being overcome with game theory, which I consider next.

Game theory provides a separate but related perspective on the problem of war (von Neumann and Morgenstern 1953). Unlike expected utility theory, which assumes independent decisions by each actor, game theory directly considers the interdependence of strategic choice among the participants.

Building on the early contributions of Schelling (1960, 1966), Ellsberg (1959, 1961), Kaplan (1957), Kahn (1960, 1962, 1965), Rapaport (1964), and Snyder (1961, 1972), there has been a flurry of attempts in the past few years to use game theory to understand interstate war. Unlike the early, static modeling efforts,

contemporary applications have used recent refinements of equilibrium solution concepts (Brams and Wittman 1981; Fraser and Hipel 1979; Kreps and Wilson 1982; Selten 1975) and breakthroughs in games of incomplete information (Rasmusen 1989) to develop dynamic models that more closely capture the essence of strategic choice. Snidal (1985c), Brams (1985), Zagare (1987), Langlois (1989), Powell (1990), Kilgour (1991), Wagner (1991) and Bueno de Mesquita and Lalman (1992), among others, have developed rich game-theoretic models mapping a variety of conflictual relationships. O'Neill (1989) and Zagare (1990) offer insightful reviews of this extensive literature.

The essential contribution of war is the clarification of the dichotomy between cooperation and conflict. Political transactions are intrinsically conflictual since political demands are imposed by one state on another when a disagreement occurs (Bueno de Mesquita et al. 1985). The weak party cannot walk out, but must accept the offer or resist and pay the consequences of dissent. Thus, like a convicted murderer who cannot escape the sentence for his transgression by refusing to cooperate, a nation cannot avoid the repercussions generated by a challenge by choosing not to yield. Opponents must choose between war, deterrence, or concessions to resolve the impasse.

Cooperation, however, is possible. Recent work on decision making shows that cooperation is the outcome of sophisticated calculations of the future costs of confrontations. Pioneering work by Axelrod (1984) demonstrates that a simple tit-for-tat strategy, within constraints, can be adopted to advance interactive diplomacy and generate cooperation. Applying this conception to arms races, for example, suggests that weapons reductions unilaterally initiated by one nation can reinforce similar behavior in opponents. Powell (1989) further explores the notion of strategic bargaining in a nuclear context and shows that in sequential interactions, the ability to preempt specific choices, particularly under uncertainty, can result in stable outcomes even among risk takers. Likewise, Kilgour and Zagare (1991) and Wagner (1991) extend such arguments and generalize their application, suggesting the beginning of a general strategy of conflict resolution.

Much of this work remains formal, however, and its power rests on the force of the argument rather than empirical evidence. The empirical analysis of game theoretical solutions is in its infancy. Many propositions have been generated, but few have been exposed to empirical tests. Two important exceptions deserve mentioning. Niou, Ordeshook, and Rose (1989) explore the implications of balance of power and show formally and empirically that the conditions for stability under balance are difficult to reconcile with international reality.

Bueno de Mesquita and Lalman (1992) apply game theory to extend their previous expected utility analysis of war and show that the addition of interactive structures greatly aids in the understanding of conflict development and its resolution. They persuasively argue that the analysis of interactive preferences is essential to understand the escalation of crisis to war.

The early results from formal analysis augur well for the future understanding of war. However, current decision-making approaches provide a limited time perspective and the detail required for effective evaluations is taxing. Few systematic data sets do justice to these requirements, which in turn leads to much debate about the validity of the reported findings. Yet, the promise is that ultimately formal models will be able to provide a general theory of conflict initiation, escalation, termination, and particularly management.

Other Perspectives on War

Much of the work on war providing important angles of vision on this phenomenon cannot be directly connected to traditional over-arching explications of war. Such work should not be overlooked, because it points to new directions that need serious investigation. I will, somewhat capriciously, select arguments that have attracted my attention from this vast literature.

Democracy and War

A growing number of studies deals with the connection between democracy and war. The puzzle to be explained is posed by Rummel (1963), who points out that while wars are waged among autocratic or totalitarian regimes with the same frequency as between these regimes and democracies, democracies do not wage war on other democracies. The debate was unearthed by Manz and Abdolali (1989), who show that the phenomenon of war avoidance by democracies withstands statistical scrutiny after normal controls for factors related to war propensity. The results are robust challengers who have considered without altering the relation of democracy and peace, levels of national wealth, rates of economic growth, territorial contiguity, alliance structures, opportunity, and historical precedent (Morgan and Campbell 1991; Siverson and Emmons 1991). Ember, Ember, and Russett (1992) have even extended the scrutiny for an explanation of this unexpected stability to domestic confrontations among primitive societies, showing that political participation deters war. Recently, Maoz and Russett (1991) propose that the absence of war among democracies is robust and probably due to political constraints imposed on democracies that attempt

to mobilize the population as well as the general norms that distinguish electoral system from authoritarian regimes.

If such results hold, increasing the level of participation would deter war. Thus, establishing functioning democracies may provide a new means of controlling war. Perhaps the growing trend toward democratic rule will contribute more to peace than either nuclear deterrence or arms control. These results challenge assumptions of theories focusing on power distribution or cycles, which assume that countries act in a general manner regardless of the form of government.¹⁵

Many are skeptical of such findings. Weede and Waltz question the selection of cases. If democracy is defined somewhat more liberally, World War II and the American Civil War were initiated by elected leaders. The collapse of Yugoslavia resulted from domestic and then international confrontations among elected elites. The question is, are they democratic? And if not, what is a democracy? With the emergence of new democratic systems, this line of research will no doubt be expanded.

Consequences of War

Evaluation of the consequences of war has also carved out an independent empirical niche. Organski and Kugler (1980) demonstrate the existence of a "phoenix factor," whereby within 20 years nations recover economic losses and more than overcome the aggregate populations losses incurred by the waging of war. Thus, following conventional wars, actors have reentered the international system and competed at a level similar to that prior to the war, after a single generation.

A political explanation for the phoenix factor flows from Olson's (1982) seminal work on collective goods. Olson argues that devastated, defeated nations, like Germany or Japan after World War II, have a substantial advantage over devastated victors, such as England or France after World War II, and even over relatively untouched contenders, such as the United States after World War II. He claims that the vanquished nations reinvigorate their economies because invaders destroy the political structures and coalitions created to protect specific interests, and allow the pure market place to reemerge. Victors, on the other hand, preserve such coalitions and the inefficiencies associated with their presence.

Empirical evaluations of these propositions produce inconclusive results. Chan (1987), using data from the Far East, shows that the vanquished accrue the beneficial economic effects anticipated by Olson. On the other hand, Kugler and Arbetman (1989b) find no evidence linking the decline of political structures to

economic recovery in Western societies. Indeed the contrary seems to be the case. Further exploration of this puzzling pattern is warranted.

War Termination

A related literature deals with the termination of war. Rosen (1972) explored the relationship between the severity of war and the willingness to suffer and shows that all wars end, but some end very differently than others. Uralis (1971) places an upper boundary on casualties and shows that contenders will terminate a total war if casualties exceed 20% of the population. However, Bueno de Mesquita demonstrates that different calculations based on the importance the competitors attach to the outcome are required in order to understand the termination of limited wars. Work in this vein suggests that there is a calculus of war: contenders do not blindly pursue a lost cause, but withdraw when losses exceed expectations. In Vietnam 50,000 Americans died before the United States withdrew, in Afghanistan 10,000 Russians were killed before the Soviet Union withdrew. These superpowers could have secured victory by escalating either war, but they chose not to. Cioffi-Revilla (1991a) points out that United States' success in Kuwait was assured by effective anticipation of low American casualties. Iusi-Scarborough and Bueno de Mesquita (1988) show that suffering and endurance in war are directly related to the willingness of elites to accept casualties and their calculations are in turn conditioned by expectations of success. The end of war is seemingly predictable.

The Diffusion of War

The analysis of diffusion of war concentrates on the economic, human, and political changes caused by war. Siverson and Starr (1991) show that the diffusion of international wars is directly related to territorial contiguity and alliance structures. Looking at war in its geopolitical context, they employ the concept of diffusion as a tool to understand the systematic effect of war on the international environment and its entities. Siverson's and Starr's empirical analysis demonstrates that opportunity and willingness have independent and important effects on the likelihood of nations entering ongoing wars. According to their study, contiguity provides the opportunity for war expansion and has a significant effect on the timing of war entry of the individual nation. In contrast, alliance commitments, expressing the willingness of actors to expand war, have a greater influence as a spatial factor on the diffusion of war. Thus, the involvement of allies or bordering nations in wars increases the probability that nations at peace will

join the war. Furthermore, Siverson and Starr reveal that the interaction of both factors has an even greater impact on the question of when and where war takes place than either factor alone. Demonstrating that the geopolitical situation provides environmental opportunities and constraints for nations, Siverson and Starr claim that people consider the factors of opportunity and willingness in their utility calculations on initiating or joining wars.

Other Perspectives on War

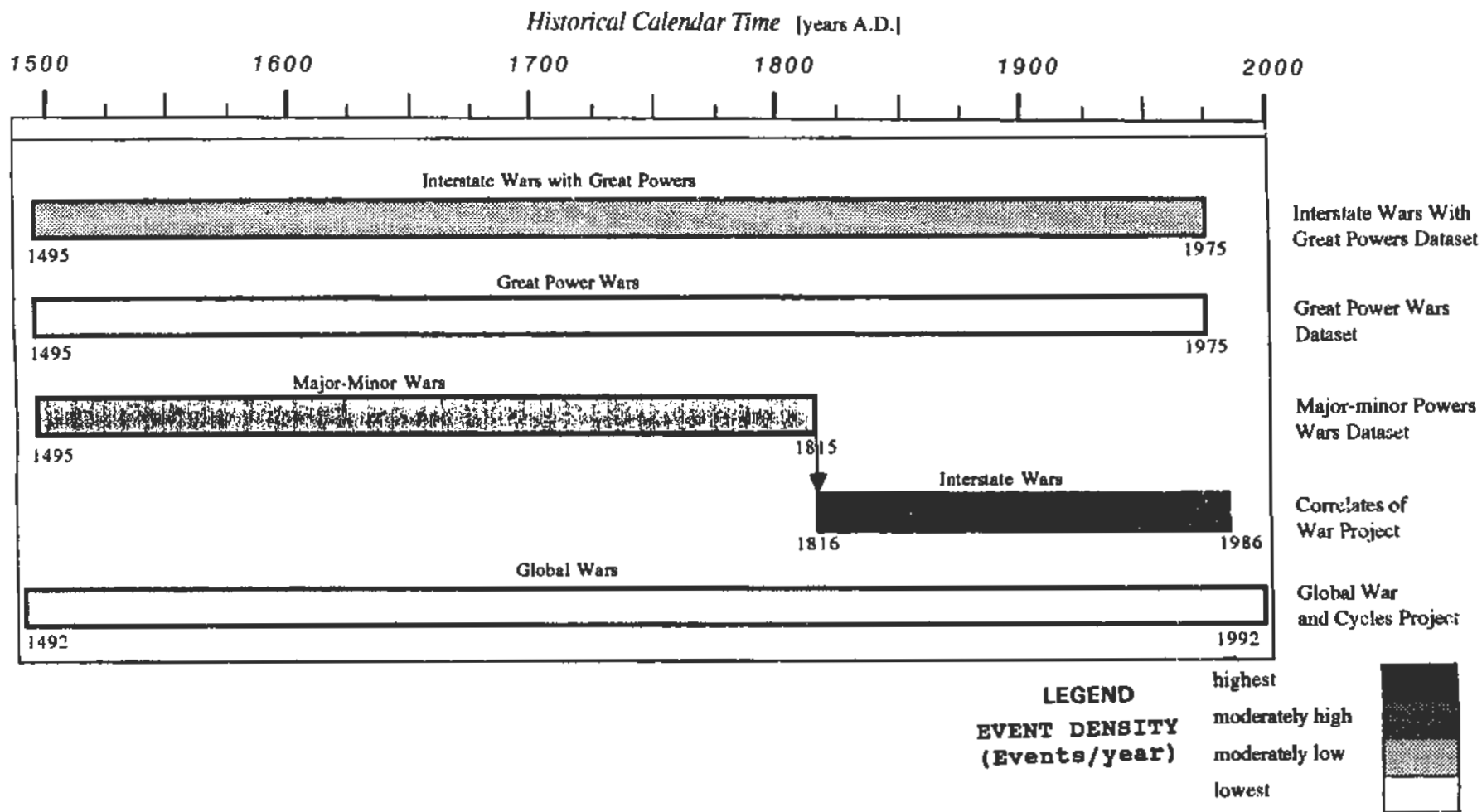
Numerous findings could be added. Empirical work shows that nations tend to bandwagon after war (Walt 1987). Allies tend to fulfill their commitments, demonstrating that wars among allies are possible but less intense than wars among unattached nations (Bueno de Mesquita 1981b; Maoz 1983). Enduring rivalries should be considered in anticipating war, suggesting that national elites may choose to initiate international war in order to avoid domestic instability (Rummel 1963; Tanter 1966; Wilkenfeld 1968, 1969). Studies of geopolitical factors long excluded should once more be considered in the study of war (Ward 1992). Important insights about the evolution of wars have also recently emerged. Maoz (1990) shows that war produces important non-linear structures that prevent logical, linear solutions. Since we are working with a paradoxical behavior, our answers may be far too simple. Our methods may be too simple, as well. Much of this new work is loosely connected to the broader frameworks, yet if some of these well-documented insights are incorporated into existing frameworks, they may force us to restructure our thinking. The agenda for future research is very broad.

Data Sources

Like early paleontologists who faced an enormous and bewildering legacy of bones, and developed crude but accurate characteristics that defined species, political scientists are overburdened by multiple, detailed accounts of seemingly distinct events that can be categorized as international wars. The challenge is to create a coherent pattern from this record that accounts for this vast diversity. Fortunately, data collection is one area where research efforts have made major strides in the last three decades. Figure 3 gives a good overview on the achievements of major data-collecting projects.¹⁶

The systematic study of war owes much to J. David Singer, Melvin Small, and their colleagues at the Correlates of War Project (COW). Following the tradition set by the seminal collections of Sorokin (1937), Wright (1942), and Richardson (1960b), participants in the COW project have collected data on Major/Minor

Figure 3



Power wars, and Interstate wars that capture many characteristics of the international war phenomenon (Singer and Small 1972; Small and Singer 1982; Singer and Diehl 1990). The COW project provides the first truly comprehensive cross-temporal report on the initiation, frequency, duration, participation in, and severity of wars. In addition, the COW project pioneered in the collection of data on non-militarized disputes, helped in the compilation of behavioral precursors to war, and amassed many elements thought to be potential correlates of international war. A non-exhaustive list includes alliances, military size, arms expenditures, industrial output, national capabilities, systemic polarity, geographic proximity, or indices of diplomatic status inconsistency.¹⁷ Jack Levy has added an extended data set on Great Power Wars, focusing on the most severe conflicts. William Thompson and Karen Rasler have developed a comprehensive collection of socio-economic data surrounding Global Wars. The ambitious Long Range Analysis of War Project headed by Cioffi-Revilla (1991b) is gathering all available information on all wars that have left a historical mark starting in 3000 BC.

Many of the above data collection efforts are being supported or expanded by the Data Development for International Research Program (DDIR) headed by Dina Zinnes and Richard Merritt. Under DDIR's auspices the collection of wars involving great powers was extended back to 1495, a large data set of wars between major and minor powers was compiled, a summary of international military interventions in the nuclear era was completed, and much more. Due to these and other efforts, the world politics community now has access to standard definitions and measures of territorial dispersion of wars, enduring rivalries, numerous alliance indices, event data summaries, and various ways to document international crises.¹⁸

Despite shortcomings, the growing data collections on war attributes and corollaries finally allow modern scholars to test propositions about war and replicate previous results. Indeed, for the first time since before the birth of Christ, when Thucydides (400 B.C.) and Kautilya (300 B.C.) proposed contending explanations of the causes of war, competing propositions accounting for war are under empirical scrutiny. This empirical scrutiny, once started, will no doubt continue expanding the base of systematic information about war.

Conclusions

My reading of the literature on war and peace suggests that key elements that still permeate much of the modern work on war have been inherited from ancient thinkers. The importance of Thucydides' and

Kautilya's¹⁹ work for the analysis of war and peace is not due to the completeness and accuracy of their insights, but rather because this work provides the initial statement from which the systematic study of war evolved.

Lakatos suggests that a theory is superior to another if it can encompass a wider portion of the phenomenon than its alternative. The reason one wishes to identify a common ancestor is that it is easier to summarize a field by tracing its evolution from the origin and concentrating on the most fruitful branches. Like Galileo, whose contributions were critical to the early development of physics, Thucydides provides the study of war and peace with a loose framework upon which many builders can construct vastly different structures. Just as Aristotle's insights about our physical environment were surpassed by Galileo's, extended by Newton and Einstein, and will assuredly be superseded by an as yet unknown physicist;²⁰ the work of Thucydides has been surpassed by modern scholars without diminishing its impact. Without the foundation laid out long ago by these giants, it is difficult to conceive the evolution of insights on war and peace proposed by Machiavelli, Hobbes, Hume, Clausewitz, Carr, Wright, Morgenthau, von Neumann and Morgenstern, and the modern extension by Organski, Waltz, Allison, Keohane, Gilpin, or Bueno de Mesquita. The study of war is still developing; ancient writers receive as much attention today as do propositions advanced by modern analysis. Theories about war are still not exposed to Lakatos' criteria for theoretical specification or empirical tests. Validity is still ascertained by authority and frequently by consistency alone. Unless we alter this approach, the study of war and peace will remain in its infancy. Refinement requires that we stop relying on important but vague insights that may already have been rejected by evidence, and that we drop arguments that are internally inconsistent even when they can be traced to ancient authority, and that we reject formal deductions whose only claim is consistency. New and plausible insights should be required to survive tests of consistency and basic empirical evaluations.

The scientific understanding of war must tell as much about war as about peace. Indeed, a successful understanding of the puzzle of war initiation, escalation, or diffusion should simultaneously show how peace can be attained, extended, and diffused. To study war, we must study peace. Thus far, however, we have concentrated on specifying the necessary conditions for war. To gain a fuller understanding of war, more attention will have to be paid to conditions that lead to cooperation and allow peace to break out.

I hope to have persuaded the reader that current theories of war are, at best, imprecise. Our best perspectives specify the necessary but not sufficient

conditions leading to the initiation, escalation, diffusion, and termination of war. A search to approach sufficiency is afoot in different directions.

Can we say that there is progress in the study of war and peace? The answer is a tentative yes. As part of this exercise, I randomly read articles on international war in leading journals, starting with the end of World War II.²¹ In journals from the 1940s and 1950s, I found much precise description but only limited attempts to go beyond the case under scrutiny. It is not surprising that few academics refer to that work today. In the 1960s and 1970s, journal articles remain largely descriptive, but a change is noticeable.²² Published empirical results seldom supported widely held propositions. Instead, they legitimized radically different theories from the accepted norm. In the 1980s and 1990s the development of alternative perspectives on the causes of war exploded. The legacy of the last two decades shows a growing commitment to theoretical specification and an increasing link to empirical evaluations. The study of war and peace is poised at the verge of generating a consistent paradigm that may guide work in the next decade.

It is my belief that the next generation of scholars will be far more dependent on their own cohorts than on their ancient ancestors. The massive improvements in specification and extensive empirical developments permit rejection of some plausible propositions, while others are preserved because they survive initial tests. As we move into the future, and larger, well-documented data sets become increasingly available to students of politics, one expects that current work will be dramatically revised and superseded. Improvements in formal structures and statistical developments that have started to appear in the last decade will undoubtedly expand and integrate larger sections of the field. This generation can take credit for being the first to face the scientific challenge and explore, admittedly very incompletely, the propositions generated by generations of students of war and peace. The next generation faces the urgent challenge of controlling war that now can escalate to unthinkable levels. That task is challenging and urgent.

Notes

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1. I use this particular definition of war as a means to narrow my subject area and not because it has more validity than other definitions of war. A most comprehensive, all-inclusive definition is provided by Cioffi-Revilla (1991b), who wishes to catalog all possible conflicts. "War is an occurrence of purpose, collective violence among two or more social groups pursuing conflicting political goals that result in fatalities, with at least one belligerent group being organized under the command of authoritative leadership."

2. Thomas Kuhn (1970) proposed the notion of a paradigm but conceded that it has been used in various ways. In later work he argues that a paradigm "...stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science" (Kuhn 1970, 175) Here I employ paradigm in the first sense defined by Kuhn. For an enlightening discussion with world politics applications see Vazquez (1983, 1-12).

3. For a particularly heated discussion of data and paradigm testing in the context of deterrence, see Richard Ned Lebow and Janice Gross Stein (1990) and Paul Huth and Bruce Russett (1990).

4. For a practical review of epistemological problems in political science, see Tilly 1985 and Collier 1991.

5. The more advanced the field, the stronger is the consensus on rules of falsification. Note that Stephen Hawking, despite the enormous complexity of his ideas on time and space, accepts with little discussion Lakatos' criteria and through their application is able to reject complex theoretical alternatives (Stephen W. Hawking, 1988. *A Brief History of Time*. Bantam, 9-13 and 47-49).

6. In no area is the difficulty of establishing a paradigm more apparent than in the study of human evolution. Paleontologists organized their discipline around Darwin's insight that the current expressions of a species, despite differences in size and appearance, evolved from a common extinct ancestor that was less adapted to survival than those still among us. Darwin's notions were and still are in sharp contrast to the theory of human creation held by the Western church. The debate on the evidence is being fought to this day. For example, "scientific" creationists challenge bone dating techniques and attempt to show that species were concurrently created. The modern study of genetics, however, has independently confirmed the validity of species transformation, lending further credence to Darwin's insight.

7. The difference between "hegemon," that is omnipotent over all its allies and foes and "dominant," which is simply the largest among the major powers is important. A dominant power is large, indeed the largest among large, but it is not large enough to impose its preferences on the whole coalition or the rest of the world. Thus, a dominant nation at the top of a hierarchy needs allies to preserve a regime. A "hegemon" is autarchic, requiring no allies to exercise influence over the regime or the world. Empirically, dominance is frequent but hegemony is very rare in the international system, appearing for less than 20 years over the last 200, and usually after devastating wars (Keohane 1984; Strange 1985; Russett 1985; Kugler and Organski 1989).

8. Recent extensive work on Japan and the elaborate evaluation of the interactions among European nations during World War I suggest that this avenue is promising (Choucri and North 1975, 1989).

9. This is a critical difference between power transition and hegemonic theory. Power transition contends that a dominant nation is the largest among major powers but not preponderant over all. Hegemonic theory, on the other hand, suggests that stability is maintained only when the largest power is preponderant, a condition not found in the international system in the last 200 years, with the exception of the brief interlude after World War II when the United States was preponderant mainly because the competition was exhausted by war. This unfortunate specification has detracted from the central

finding that preponderance and not necessarily hegemony is associated with the absence of major war (Gilpin 1981; Russett 1985; Kugler and Organski 1989; Nye 1990).

10. Modelski and Morgan (1985) argue along similar lines, but center their criticism of the stability of deterrence on the global cycle (for details, see pp. 49-50).

11. John Mueller (1989) brings vividly to attention an oversight present in most war theories: the conditions for war are effectively and fully elaborated but those for peace are simply noted. Organski and Kugler (1980), for example, carefully define conditions for war under parity and transition, but fail to elaborate with equal precision the reasons for peace under preponderance. The reverse tendency, however, is present in the treatment of nuclear deterrence. Intriligator and Brito (1989) elaborate extensively the conditions for peace under deterrence, but fail to label the conditions their model suggests will lead to nuclear war. Indeed, their model suggests that since 1945, during transitions from the cone of war to compellence or from compellence to mutual deterrence, a nuclear war would be waged. The lack of a nuclear war under such conditions is -- after the fact -- dismissed or attributed to luck. This emphasis on the partial outcome one wishes to explore is prevalent.

12. Other reasons for nations to oppose nuclear proliferation might exist. Bueno de Mesquita and Riker (1982) and Wu (1990) point out that proliferation would reduce the bargaining power of a nation possessing nuclear weapons versus a prospective nuclear power.

13. For an alternate view of deterrence not based on power distributions, see John Mueller (1989).

14. This argument is used by Intriligator and Brito (1987) to oppose the development of defensive missile systems that could force nuclear nations out of stable Mutual Assured Destruction and into regions of uncertain deterrence (Figure 1).

15. Decision-making approaches would account for such phenomena if it can be proven that dramatically different preferences are held by authoritarian, totalitarian, and democratic populations and elites.

16. For specific information, consult Claudio Cioffi-Revilla's Long Range Analysis of War (LORANOW) Project, University of Colorado at Boulder.

17. Paul Diehl's (1991) bibliographic review of the COW project provides an excellent overview of data expansions.

18. Recently, many of these data collections have been extended under the auspices of the Merriam Laboratory for Analytical Political Research as part of the Data Development for International Relations project. Important extensions include Ted Robert Gurr, *National Capabilities*; Jack Levy, *Great Power Wars*; Manus Midlaraky, *Major-Minor Powers Wars*; John Wilkenfeld, *International Crisis Behavior*; Frederick Parson, *Interventions*; Philip Schaefer, Gary Goertz, and Paul Diehl, *Territorial Change Coding Manual*; and Randolph Siverson with Harvey Starr, *Data on International Borders, 1816-1965*. The quantitative analysis of international events now offers a number of perspectives: Rudolph Rummel's *Dimensionality of Nations* (DON); Charles McClelland's *World Events Interaction Survey* (WEIS); Edward Azar's *Conflict and Peace Database* (COPDAT); Charles Herman's *Comparative Research on the Events of Nations* (CREON); Charles Taylor and David Jodice's *World Handbook of Political and Social Indicators*; and Ernest Haas, Joseph Nye, and Robert Butterworth's *SHERFACTS* on conflict management. These now provide a very extensive cross-temporal and cross-national basis for empirical research. The data sets are available to the community through the ICPSR at the University of Michigan. In addition, interested readers can obtain current issues of the *DDIR-Update* that reports on data developments for international relations.

19. Perhaps Kautilya, who wrote his classic *Arthashastra* sometime between 321-296 B.C., deserves equal credit with Thucydidea. This work parallels in many respects the critical elements of Thucydides' postulates on war. However, I know of no evidence

that the Western thinkers who laid the foundations of contemporary war theory, such as Machiavelli, Hobbes, or Morgenthau, discovered the writings of Kautilya.

20. The development of physics is outlined in a clear and brilliant manner by Hawking (1988).

21. *The American Political Science Review*, *World Politics*, *Foreign Affairs*, and more recently *The Journal of Conflict Resolution*.

22. This change coincides with the appearance of *The Journal of Conflict Resolution*.

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