
Structure, growth, and power: three rationalist accounts

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Robert Gilpin. *War and Change in International Politics*. Cambridge: Cambridge University Press, 1981.

Douglass C. North. *Structure and Change in Economic History*. New York: W. W. Norton, 1981.

Mancur Olson. *The Rise and Decline of Nations: Economic Growth, Stagflation, and Social Rigidities*. New Haven: Yale University Press, 1982.

These three ambitious works address the same broad and timely questions: why do national economies grow and contract? How is economic growth affected by, and how does it affect, the domestic structures and policies, and the international power, of states? What is the relation between economic health and political tranquillity, within and between states? All three volumes, moreover, claim to proceed from the same perspective, namely that of rational choice, applied by Olson to individual subjects, by Gilpin to state leaderships, and by North to both. Finally, all apply to their chosen subjects an impressive historical erudition, which draws as effortlessly on evidence from ancient empires and primitive tribes as on that from the present day.

To the adherent of rational-choice theory, Olson's volume will be what the Jacobin Terror was to Dickens. It is an "awful" work in both the archaic and the modern senses of the word, stunning in its deductive rigor and power, calamitously wrong in many of its implications. North, while less ineluctable in his logic, less often errs: the slightly less parsimonious and precise model is justified by the greater accuracy of its implications. Gilpin's contribution,

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as he himself emphasizes at the outset, is not intended as an effort in deductive theory but at most as an “analytical framework” (p. 2). Tentative in its rationalism and discursive in its argument, it is likely to be dismissed by more formal theorists as “wisdom literature.” That would be wrong, even if not wholly unjust, for it *is* wise, powerful in its insights, and accurate in its chief conclusions. Its overgrown logic, in which the path of the argument often disappears, can be pruned into order and clarity.

None of the three authors, of course, fully answers all, or indeed any, of the large questions he addresses. (Only Olson aims so high.) Yet I think, and shall try to show, that something very close to a full explanation can be discerned in the agreements and complementarities of these three works. What one or two of them ignore, the third usually addresses; where one errs, another corrects.

I shall therefore proceed in an unconventional and somewhat dialectical way, bringing each work into constructive discourse with the others. In this endeavor I shall take up the volumes not in any order of perceived merit but according to the levels on which their respective analyses are focused: Olson first, as dealing principally with the importance of groups within states; North, who elaborates a powerful theory of the state, second; and last Gilpin, who alone concentrates on the explanation of transactions among states.

Olson and the theory of groups

Because, in contrast to almost all my friends and colleagues, I found much to admire in Olson’s *Rise and Decline of Nations*, I shall concede at once that there is also in it much that alienates: the tone is pompous (what other living author, one wonders, would have had the brass to appropriate for one of his own previous works the short title *The Logic*,¹ or to concede amiably that his own momentous contribution has been possible only because he “stands on the shoulders of giants”²); the name-dropping quickly becomes tiresome (are “helpful conversations and correspondence” with the likes of Senator Daniel Patrick Moynihan really acknowledged because they were helpful, or because they were with Moynihan?); and the evidence deployed is frequently either not to the point or of doubtful accuracy (of which more below). Still, as I have by now frequently rejoined, the *model* is a thing of beauty and power, capable of generating many quite precise, and some wonderfully accurate and counterintuitive, predictions. It is good enough and right just often enough to make us wonder why it is sometimes so wrong and to make us try to correct, rather than to abandon, it.

1. Mancur Olson Jr., *The Logic of Collective Action* (New York: Schocken, 1968).

2. Olson attributes the apothegm in this form to Newton. It seems to have originated with Didacus Stella (as quoted in Lucan, *Civil Wars* 2.10) in the rather less self-flattering form “*pigmei gigantum humeris impositi plusquam ipsi gigantes vident*” (my emphasis). See John Bartlett, *Familiar Quotations*, 14th ed. (Boston: Little, Brown, 1968), p. 134.

The model, to outline it very summarily, elaborates deductively the economic and social effects of what Olson calls “distributional coalitions,” that is, cartels, guilds, and unions that seek to raise not their members’ productivity or expertise but their share of what society already produces. (Following Olson, I shall frequently use “cartel” as a convenient generic equivalent of “distributional coalition.”) Almost without exception, the proliferation of such groups will reduce efficiency, increase social inequality, make politics less stable, curtail economic growth, increase “normal” unemployment, and make depressions more severe and inflation more likely.³ Why?

First, to the extent that a group succeeds in gaining greater compensation than the market would provide, it obviously diverts to itself resources and talents that could find more efficient uses. (This effect could be avoided only if, magically, *all* groups were “symmetrically” organized, so as to be able to block each other’s above-market demands. See immediately below.) Second, as Olson’s *Logic of Collective Action* demonstrated, distributional coalitions are far likelier to succeed among small and wealthy groups than among large and poor ones: lawyers get guilds earlier and tougher than bank clerks do; steel owners cartelize more easily and more powerfully than steelworkers. Hence the net effect of cartelization in any society is ordinarily to make the rich richer and the mighty mightier. (More generally, lawyers and intellectuals, as possessors of superior organizational skills, are rewarded even more disproportionately in cartelized societies.) Third, distributional coalitions, as special-interest groups *par excellence*, are likely to be “passionate” minorities in Anthony Downs’s sense;⁴ and, hence, by Olson’s logic, a society that contains more such groupings is likelier to have unstable majorities and intransitive collective preferences (“Arrow,” or Condorcet, cycles). Fourth, if an existing cartel is threatened by some new technology, it will invest in slowing the innovation or raising its price, as in the familiar cases of “featherbedding” and anti-department store legislation; but even where a new technology would increase demand for a group’s services, the group will often retard its adoption by restricting entry and extracting even higher monopoly wages or rents. Hence a more cartelized society will innovate more slowly and grow less rapidly.

In general, distributional coalitions prefer to fix prices and restrict entry rather than to regulate amounts marketed by their members, simply because the former aspects are easier to police—as OPEC has by now discovered. The net effect of these tendencies is threefold. They divert some available demand from the less to the more cartelized sectors of the economy; the classic parallel might be tariffs or price supports that, by raising the cost of food, depress consumer demand in other sectors. They force some available

3. A cartel can increase social efficiency only if it acts to curtail the power of some existing cartel, for example, if a coalition of shippers achieves deregulation of trucking.

4. In *An Economic Theory of Democracy* (New York: Harper & Row, 1957), pp. 55–69.

labor into those sectors of weaker demand. And they motivate employers of over-market-price labor to hire only the most productive and skilled available workers. In Olson's analysis, which is better supported technically than I am indicating here, these effects lead to a higher "normal" unemployment, whose burden falls, against conventional economic assumptions, disproportionately on the young, the less skilled, the less productive, and the already less advantaged.

At the same time, distributional coalitions usually can decide their affairs only consensually and slowly; hence the prices they set tend to be "sticky" and, of course, especially resistant to decreases. This being so, any unexpected disinflation or slump in aggregate demand especially dislocates a cartelized society. Because prices in the organized sectors resist change, they exceed market levels by even more than before; hence those sectors quickly display unused capacity and high unemployment. At the same time, the whole burden of price-cutting and of attempting to soak up unemployed labor falls on the unorganized "flexprice" sectors (the term is due to Sir John Hicks), so that in the Great Depression agricultural prices dropped far more than industrial ones and unemployed workers indeed turned to the (unorganized) retailing of fruit.

Unanticipated inflation or stimulation of demand has, of course, the opposite effect. While cartel prices are stickier downward, they are still sticky upward (major U.S. unions, for example, typically sign three-year contracts); hence an unanticipated general price rise often *reduces* their premium over market. This change frees up demand in the uncartelized sectors (consider again the parallel effects of a sharp reduction in a food tariff), reduces unemployment, and particularly benefits the younger, less skilled, and less advantaged workers. For all these reasons demand-stimulation is an attractive remedy—not, as Keynes had argued, in *all* depressed economies but in the special case of *highly cartelized* ones. (Mutatis mutandis, monetarism, according to Olson, is a theory that can work only in the special case of relatively *uncartelized* societies.) Where cartelization is far advanced, however, Olson contends, quite drastic inflation will be required to combat a sharp contraction in demand. Thus, after the slump created by the oil price increases of 1973, the moderate inflation to which much of the West resorted had only slight effects on unemployment, leading to a predictable "stagflation" and to "negative Phillips curves." (Obviously, in Olson's analysis, the general shape of any country's Phillips curve must be a function of its level of cartelization.)

Finally, however, an important Olsonian stipulation: the evil effects of cartels will be obviated to the extent that they are "encompassing," that is, that they organize a very large share of any sector's or any society's (or, he somewhat curiously suggests in a reference to Japan [p. 49], any *firm's*) workforce or productive capacity. Cartels of this kind can, for reasons that again flow directly from Olson's *Logic of Collective Action*, find it worth their while not only to claim a larger share for their members but to direct

them toward greater productivity and innovation. Hence the Swedish LO is less malign than the British TUC or the American AFL-CIO.

Now I submit that to have advanced a parsimonious, precise, and logically consistent explanation of societies' different rates of efficiency, growth, political stability, inequality, unemployment, and inflation, while along the way subsuming and seeming to resolve the Keynesian-monetarist controversy, entitles a man to sound a little pompous. (There remain substantial problems of evidence, but defer those for now.) Yet Olson does not stop here but offers a separate account of *why* societies are more or less cartelized. Five factors seem important, principal among them simply *time*: in any society that remains stable in its politics and its boundaries, the number and power of distributional coalitions tend to increase. Second, *consanguinity or shared ethnicity* makes cartelization more likely: thus the more rapid and more effective organization of European nobilities, Indian castes, and white South African workers. What decisively breaks down cartels is, in Olson's view, a third set of factors, namely *foreign occupation, social revolution, and totalitarian rule*. Also tending to weaken cartels is *integration into a larger political unit* (a mere common market will not be nearly so effective). Here the argument could be drawn directly out of (but is not attributed to) *The Federalist No. 10*: factions are weaker in a larger unit and are especially slow to coordinate their efforts in a large unit newly joined. Fifth, Olson allows that various kinds of *state action* can accelerate or retard cartelization. High tariffs, as economists know, favor cartelization by excluding possible competitors; so do restrictive immigration policies. The state may directly organize cartels, as did the National Recovery Administration in the early New Deal, or may stimulate the formation of coalitions by grants of privileged access (the "consultation of affected interests"). Conversely, a state committed to the market may discourage coalitions. In these respects indirect effects of regime form and ideology may be noted: colonial or authoritarian regimes (India and Hong Kong under British rule; France under Napoleon III?) have often resisted or impeded cartels more effectively than indigenous and democratic ones; and Liberal ideology in Britain delayed cartelization in the late 19th and early 20th centuries.

By connecting these two elements of his theory, Olson is able to make some quite specific predictions about which of the world's economies ought to have grown most rapidly at which times and which ought to be most resistant to inflation and recession, which societies ought to be the most nearly equal, and which polities the most stable. Specifically, those societies without substantial racial or ethnic divisions which most recently underwent foreign occupation, social revolution, or totalitarian rule, and which were incorporated into a larger juridical unit, should perform best on all indices. Conversely, the most dismal performances may be expected of societies with long records of political stability, fixed boundaries, and immunity from in-

vasion; deep and multiple divisions of ethnicity or race;⁵ and high tariffs, strict limits on immigration, or other state policies that encourage cartelization.

If these conclusions were correct, they would fully justify the pessimism that pervades Olson's volume: precisely those societies that most consistently escaped totalitarianism, conquest, and revolution would, *ceteris paribus*, perform worst economically, politically, and socially, and eventually would be pushed aside by more vigorous competitors. Every Britain must suffer Britain's fate.

Fortunately, Olson's evidence, and that of his supporters in a balanced and useful volume of discussion and criticism edited by Dennis Mueller, is far from convincing.⁶ In the contemporary world, Olson argues, West Germany, France, and Italy, all with histories of foreign occupation, totalitarian rule (if in the case of France we count Vichy), and recent incorporation into the European Community, ought to be at or near the top in economic performance; so should Japan, with recent totalitarian rule and foreign occupation, and Belgium and the Netherlands, which have undergone foreign occupation and been integrated into a larger unit. At the low end of the scale of anticipated performance, all with stable boundaries and politics over long intervals, stand Australia and New Zealand (also with high tariffs and restrictive immigration), Canada (with high tariffs and ethnic and regional divisions), Britain (with policies that strongly favor interest-group formation, plus substantial restrictions on immigration), and the United States. Indeed, the first group has had the highest rates of growth and the greatest resistance to cyclical recession; the second group has done worst on both scores. Moving to historical evidence, Olson attributes the very rapid growth of Germany and Japan at the end of the 19th century largely to their then-recent consolidation into authentically national states, both having previously been highly decentralized. A seemingly countervailing case like Sweden (stable boundaries and politics, long national integration) is accounted for by its "encompassing" coalitions.

Olson, however, sees as the strongest evidence in support of his claims a regression analysis (pp. 92–117) that relates the economic performance of the fifty United States to their political "age," that is, to the time elapsed since their admission to the Union or (in the case of the former Confederate states) since the end of Reconstruction, which Olson sees as analogous to foreign occupation. The same tack is taken with respect to today's nation-states by Olson's former student Kwang Choi, writing in the Mueller volume (chapter 3), with similarly heartening results.

About all that this highly selective use of evidence shows is a surprising ignorance of recent comparative history. If revolution and foreign conquest

5. The proviso "multiple" is important. In South Africa, for example, one could argue that whites form an "encompassing" coalition; in India, pretty clearly no caste does.

6. Mueller, ed., *The Political Economy of Growth* (New Haven: Yale University Press, 1983).

really conduce to rapid economic growth in ethnically homogeneous societies, why did France's economy grow so slowly both after 1815 and after 1870? If stable states with high tariffs must grow slowly, why did Sweden's great expansion come *after* its adoption of protection in 1887–92 and *before* any significant growth of “encompassing” coalitions?⁷ If incorporation into a larger juridical unit is so economically helpful, why has Austria done better outside the European Community than West Germany has done within it?⁹ If the Civil War and Reconstruction were the boons to the South that Olson claims, why did it take eighty years for their benefit to be felt? (As Frederic Pryor has asked, can Olson seriously contend that his regression of the fifty states' economic growth rates, if run on the data of 1900 or even 1920, would show similar results?)¹⁰ Finally, there are the other variables of the theory, which not surprisingly are neglected in Olson's empirical testing: that recent totalitarian or foreign rule makes societies more equal, or more resistant to inflation, will come as a surprise to the French, the Italians, and the Japanese, who have since World War II suffered some considerable bouts of inflation and whose distributions of wealth and income are often counted among the developed world's more unequal ones.¹¹

Part of the problem is, of course, that element of Olson's theory which tries to predict societies' degrees of cartelization. There is simply no evidence that totalitarian rule or foreign occupation always breaks the grip of social coalitions, and indeed some prominent cases—including ones erroneously cited by Olson as supporting his case—speak for exactly the contrary thesis. The regimes of Fascist Italy and Vichy France, for example, are widely believed to have strengthened interest-group representation and power.¹²

7. See for example Eric J. Hobsbawm, *The Age of Revolution: 1789–1848* (New York: New American Library, 1962), pp. 212–13, and Henry W. Ehrmann, *Politics in France*, 4th ed. (Boston: Little, Brown, 1983), p. 18.

8. M. Donald Hancock, *Sweden: The Politics of Postindustrial Change* (Hinsdale, Ill.: Dryden Press, 1972), p. 23.

9. Between 1960 and 1980, real GDP grew on average 4.2% annually in Austria, 3.7% annually in West Germany. The difference in growth in per capita GDP was even sharper, 3.9 as against 3.2% per year: Organization for Economic Co-operation and Development, *Historical Statistics, 1960–1980* (Paris: OECD, 1982), p. 40.

10. In Mueller, *Political Economy of Growth*, chap. 5, at p. 91.

11. On inflation in the Fourth Republic see Alfred Cobban, *A History of Modern France*, vol. 3: *France of the Republics, 1871–1962* (Harmondsworth: Penguin, 1965), pp. 210 and 213. In 1974, the year after the first great increase in oil prices, inflation in Japan was 21%, in Italy 19%, in France 11%; on average in the European OECD countries, it was 12% (OECD, *Historical Statistics, 1960–1980*, p. 72). According to one widely disseminated estimate, the Gini indices of inequality of personal income in 1962 were France, .52; West Germany, .48; Netherlands, .44; U.K. and Sweden, both .40; and the United States, .35. See Karl W. Deutsch, *Politics and Government: How People Decide Their Fate*, 2d ed. (Boston: Houghton Mifflin, 1974), p. 138.

12. For example, Joseph La Palombara, *Interest Groups in Italian Politics* (Princeton: Princeton University Press, 1964), pp. 132–33; Charles S. Maier, *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I* (Princeton: Princeton University Press, 1975), p. 85; and Philip M. Williams, *Crisis and Compromise: Politics in the Fourth Republic* (Garden City, N.Y.: Anchor, 1966), p. 393.

And few scholars of either postwar Japan or West Germany, so far as I am aware, have been able to share Olson's judgment that the Allied occupations "emasculated or abolished" their pre-existing coalitions (p. 75). On the contrary, it is usually held that the "enterprise groups" and cartels, if affected at all, rapidly came back as strong as before.¹³ In any event, to claim that those societies today have weaker coalitions than does the United States—and this is exactly the implication of Olson's claim about the effects of totalitarianism and conquest—is ludicrous. (On the other hand Olson may well be right about the effects of ethnicity and state action.)

If in fact postwar France, Italy, Japan, and West Germany have some unusually *strong* social coalitions while the United States has unusually weak ones—and that, surely, is much closer to the truth—what becomes of the main part of Olson's theory, which attempts to link superior economic, social, and political performance to *weak* cartelization? Only the possibility, to which Olson occasionally resorts in speaking of Japan and Germany, that the coalitions in the more successful economies have been unusually "encompassing." That answer, however, brings Olson close to the position of such students of modern corporatism as Philippe Schmitter, who has tried to show that in the postwar period powerful interest groups have made societies more governable.¹⁴

Astonishingly, however, Olson never really tries to test directly his best-argued propositions. His evidence about the extent of actual cartelization in particular societies is entirely anecdotal, and he confesses to some uncertainty about how even to operationalize his crucial independent variable (count the number of groups? the size of their memberships? somehow gauge their power?). Failing that, he turns to the indirect tests already mentioned, taking time, stability, and absence of totalitarian episodes as surrogates of his real describing variable; and in this way he comes to grief.¹⁵

Something is cooking here, but its tastes are subtler than Olson appreciates. Few would deny that Britain has been crippled, economically and politically, by powerful and selfish pressure groups. At the other extreme, West Germany, Belgium, the Netherlands, Norway, Austria, and (as Olson admits and explains) Sweden have flourished, so most students agree, largely *because* of their powerful interest associations, which can enforce bargains that guarantee wage and price stability, promote productivity and quality, and accelerate

13. Chitoshi Yanaga, *Big Business in Japanese Politics* (New Haven: Yale University Press, 1968), pp. 35–41; Arnold J. Heidenheimer and Donald P. Kommers, *The Governments of Germany*, 4th ed. (New York: Thomas Y. Crowell, 1975), p. 47.

14. Phillippe C. Schmitter, "Interest Intermediation and Regime Governability in Contemporary Western Europe and North America," in Suzanne Berger, ed., *Organizing Interests in Western Europe: Pluralism, Corporatism, and the Transformation of Politics* (Cambridge: Cambridge University Press, 1981).

15. This pitfall is, fortunately, avoided by most of the contributors to the volume edited by Mueller, *Political Economy of Growth*. Especially valuable are the comparative essays by Moses Abramovitz (chap. 4) and Pryor (chap. 5).

technical adaptation.¹⁶ Japan, Italy, and France appear to have found half the recipe of corporatist success, with strong organizations of capital and management but weak ones of labor; thus their growth has been attended by greater inequality, greater social unrest, and (a little surprisingly, in light of Olson's arguments on this head) a greater propensity to inflation.

We can, however, be a little more precise. In 1970 Bruce Headey proposed and applied to thirteen of the European and North American advanced industrial societies a measure of "organizational centralization" of trade unions, that is, of the power of their central offices. Schmitter later applied Headey's index to two more societies and, for the total of fifteen, combined it into a broader measure of "corporatism." In Table 1 I rank the fifteen societies according to Headey's measure of centralization and Schmitter's index of corporatism. I then compare those rankings with those of average annual growth in per capita gross domestic product (GDP)—the measure of growth that Olson prefers—over the period 1960–1980, as reported by the OECD. Note that in both cases the rank-order correlations are *positive* rather than, as Olson would presumably predict, *negative*; indeed, the estimated correlation between economic growth and Headey's measure of centralization is—for whatever interest it may hold—about 1.38 times its standard error, for a significance level of about .08. Ordinarily, negative findings of this strength are not counted as confirmatory, even in the social sciences.

North on states, firms, and ideologies

For North, here as in his important work with Thomas, the principal determinant of economic growth is the extent to which prevailing property rights relate individual to social benefits and costs.¹⁷ The specification of maximally efficient property rights varies, as before, with population density and capital stocks so that demographic growth remains the great motor of social and political change. Yet North has here substantially refined the previously almost implicit theory of the state, has introduced new theories of ideology and of the firm, and has extended and revised the earlier historical discussion so that he now ranges from the origins of human society to the early part of the present century. He has produced what, despite any flaws, must be acknowledged as a work of brilliance and of overwhelming erudition, one that over the next decade or less will become part of the essential armory of every student of politics or history.

Begin, as North does, with the state: an "organization with a comparative

16. See, for example, Bruce W. Headey, "Trade Unions and National Wages Policies," *Journal of Politics* 32 (1970), pp. 407–39; cf. Franz Lehner, in Mueller, *Political Economy of Growth*, chap. 10.

17. Douglass C. North and Robert Paul Thomas, *The Rise of the Western World: A New Economic History* (Cambridge: Cambridge University Press, 1973).

TABLE 1. *Centralization, corporatism, and growth in fifteen advanced industrial countries, 1960–80*

Country	Centralization ^a	Corporatism ^a	1960–80 Ave. Annual Change in GDP/cap (%)	Growth in GDP/ cap ^a
Austria	1	1	3.9	1
Netherlands	2	6	2.9	10
Belgium	3	7	3.7	(5)
Finland	(5)	(4)	3.8	(2.5)
Norway	(5)	2	3.7	(5)
Sweden	(5)	(4)	2.7	12
Switzerland	7	9	2.1	14
Denmark	8	(4)	2.8	11
West Germany	9	8	3.2	(7.5)
France	10	13	3.8	(2.5)
Canada	(13)	(11)	3.1	9
Ireland	(13)	(11)	3.2	(7.5)
Italy	(13)	15	3.7	(5)
U.K.	(13)	14	2.0	15
United States	(13)	(11)	2.3	13

r₁ of centralization with growth in GDP per capita: .368
r₂ of corporatism with growth in GDP per capita: .284

a. Rank-order.

Sources. Headey, "Trade Unions and National Wages Policies"; Schmitter, "Interest Intermediation and Regime Governability"; OECD, *Historical Statistics, 1960–1980*.

advantage in violence" over some geographic area (p. 21), it trades protection and the enforcement of property rights for taxes. Because, except in the most primitive hunter-gatherer societies, there are economies of scale in the provision of these two services, the existence of a state can increase a society's total income. At the same time, rulers are motivated to maximize their profits, that is, the surplus of their revenues over the costs to them of providing protection and justice. Yet rulers are constrained against simple depredation by the requirements of social efficiency and the availability of substitutes. If, that is, rulers impose grossly inefficient or arbitrary property rights, they may so impoverish the society as to diminish their own long-term rents or to weaken the state against rivals; and external or internal rivals will compete

against the incumbent rulers in part by trying to offer equal or better services at lower cost.

On the other hand, even rulers who are inclined to enforce maximally efficient property rights ordinarily cannot—partly for reasons that relate closely to Olson’s argument. First of all rulers act as “discriminating monopolists.” Admitting that some groups in the society are strategically more important—that their cooperation is vital to the functioning of the state or the society—rulers buy their aid with grants of lower taxes or, in part of what North now calls the “constitution” of the state, a greater share of power. These privileges will often themselves be economically inefficient, as was the exemption of the French aristocracy from the *taille*; and, as more and more groups “capture an interest in the state” in this way (p. 43), conflict over property rights will become more open and the rights themselves less certain. By both routes, efficiency and growth decline.

Now it is reasonable to think that, as societies grow more complex, the number and power of such “strategic groups” must rise. More important as sources of injurious pluralism, however, are *mobilization* and the decline of shared *ideology*. In a society, or indeed in any sizable organization, North contends, enforcement costs soon become impossibly high. If constituents operated only on a calculus of evasion, detection, and punishment, free-rider problems would soon sabotage all cooperation. In practice, every large organization has found more cost-effective the inculcation of some coherent set of beliefs, according to which the existing order is fair, just, and natural, and every betrayal of it is base or perverse. (Rivals to the existing leaders must undermine this ideology, ordinarily by supplying one of their own.) North goes so far as to assert that “the stability of any society requires a [shared] ideological superstructure” (p. 182).

Shared ideology, however, can arise and persist only “when the individuals of a universe have similar experiences” (p. 205). The more specialized and differentiated a society’s workforce becomes, the less likely it is to retain a common ideology (p. 51). A second factor is sheer economic dislocation, which can generate “massive ideological alienation” and “activate[] groups to participate in control of the state.” Exactly this combination of specialization and dislocation has characterized the workings of modern markets, North believes, thus accounting for the “diverse ideologies” and the pluralist struggles for control of the state that we now observe (pp. 181–83).

More generally then, and in contrast to Olson, North seemingly would predict powerful and greedy coalitions—and hence lower rates of growth, greater instability, and the rest—not particularly in old and secure states but in highly specialized economies subject to extreme market fluctuations.¹⁸ Growth should, in any event, be slower in societies with deep ideological divisions.

18. North, however, does not really attempt a comparative application of his theory, examining instead secular decline in efficient property rights.

At the same time, North emphasizes that powerful groups are not the only source of inefficiency and economic decline. At least as important are, first, the prevailing, and largely technologically determined, *costs* of protection, agency, enforcement, and collection—the economically optimal tax may be too expensive to collect, economically efficient property rights impossibly costly to enforce—and, second, the “stickiness” of constitutions and property rights, which, no less than Olson’s cartel prices, are hard to alter once established. Again much of the cause, according to North, is ideology, which once inculcated is like one of Thomas Kuhn’s scientific paradigms: while it economizes information and permits concentration on day-to-day problems, it prevents consideration of fundamental alternatives and, indeed, often makes them literally “unthinkable.”

Together these factors of cost and stickiness imply short-term instability and long-term decline for virtually all states. Over the near term, unpredictable innovations, particularly in military technology, may drastically alter the costs of protection, agency, or enforcement, or the strategic importance of particular social groups—as did, for example, the introduction of the phalanx, the stirrup, and the cannon. When that occurs, states that can adapt, even if only by revolutionary change, survive and grow; but, owing to the tenacity of acquired rights, most fail.

Even states that these perils do not befall are doomed over the longer run. If from the beginning they maintain inefficient property rights, they will fail to grow economically and will be eclipsed. If they do grow, they may face political upheaval or economic stagnation even sooner, for their very growth will so increase population and capital stocks as to make their previously efficient property rights a fetter on further growth. Because the now inefficient rights cannot be changed within the existing ideology and constitution, revolution or decline must ensue (pp. 28–29 and 208).

From this part of North’s analysis, it seems to me, we can safely draw three general hypotheses. First, economic growth will generally be more rapid in states strong and autonomous enough to adapt their property rights to changing conditions yet challenged (or wise) enough not to become predatory. Second, *some* social revolutions—in particular, those which install strong states and property rights better suited to the society—will spur economic growth; others will not. Third, extremely rapid growth is its own nemesis; moderate rates are more likely to be sustained.

However, the cases of Japan, Italy, and France raise doubts about all five of the hypotheses (these three and the two stated earlier) that North’s analysis seems to imply. Japan has had to develop a highly specialized economy, and many of the shocks of the international market—most memorably, of course, the oil crisis of 1973—have hit it very hard; yet growth has barely slowed, nor has “massive ideological alienation” become manifest.¹⁹ Neither

19. Japan’s rates of annual growth in real GDP since 1973 (in %) have exceeded the average of the seven major industrial states (including Japan) as follows: in 1974, Japan –1.0 against the average 0.3; 1975, 2.3 and –0.5; 1976, 5.3 and 5.2; 1977, 5.3 and 4.2; 1978, 5.0 and 4.1; 1979, 5.1 and 3.5; and 1980, 4.4 and 1.1 (OECD, *Historical Statistics, 1960–1980*, p. 40).

has Japan's extremely rapid growth seemed to bring it up against limits sooner than, say, West Germany's more moderate rate. Italy, on the other hand, is frequently portrayed as almost the quintessentially weak and penetrated state, in which property rights are always conditioned by political machinations;²⁰ and its ideological divisions, if now mellowed, are still among the deepest in Western Europe. Yet the Italian economy has consistently grown more rapidly than that of the far stabler and more ideologically homogeneous Federal Republic of Germany. Finally, the French Revolution created more rational and efficient property rights, and certainly a more powerful and autonomous state; yet, as I have already noted, the French economy grew very slowly for most of the 19th century.

These cases raise more fundamental issues. First, do differences in property rights really account for recent variations in growth and prosperity? Writing with Thomas, North was plausible in claiming that superior English and Dutch property rights explained those states' 16th and 17th century ascents. But does Germany overtake Britain after 1880 because of a comparative advantage in property rights? Can more efficient property rights explain American or Japanese episodes of rapid growth? By the logic of North's whole argument this must be so—he asserts pretty plainly that U.S. decline since about 1900 is due to abridgments of property rights (pp. 193–98)—but we get no direct argument of the proposition. At one point North actually suggests that efficient property rights had only a brief heyday and that they have by now been substantially diluted everywhere (p. 184), from which it ought of course to follow that economic performance must be everywhere dismal.

Even more doubtful, I think, is the importance North now wants to assign to ideology. Too often it becomes here, as it has for others before North, a euphemism for ignorance: whatever seems at first glance to defy rational explanation, such as the support of middle-class reformers in the United States for measures to alleviate the sufferings of the poor (pp. 197–98), is at once attributed to “ideology.” Nor is there good reason to believe that ideology is so “sticky” or so potent as North now believes. If ardently free-trading Junkers could be converted to protection in a matter of months, if devoutly monarchist English gentlemen could become republicans, or if conservative farmers in North Dakota and Saskatchewan could be converted to socialism—and all these things do indeed seem to have occurred²¹—then one is entitled to a certain skepticism about the persistence and perhaps about the effec-

20. For example, by P. A. Allum, *Italy—Republic without Government?* (New York: W. W. Norton, 1973).

21. Hans-Ulrich Wehler, *Das Deutsche Kaiserreich, 1871–1918* (Göttingen: Vandenhoeck & Ruprecht, 1973), p. 48; Christopher Hill, *The Century of Revolution: 1603–1714* (New York: W. W. Norton, 1966), pp. 173–76; Robert I. Morlan, *Political Prairie Fire: The Nonpartisan League, 1915–1922* (Minneapolis: University of Minnesota Press, 1955); and Seymour Martin Lipset, *Agrarian Socialism: The Co-operative Commonwealth Federation in Saskatchewan* (Berkeley: University of California Press, 1971).

tiveness of such beliefs. Even Kuhnian paradigms, we should recall, do get overthrown. Finally, the claim that political and social stability are impossible without a common “ideological superstructure” is refuted by powerful evidence, as political scientists have been demonstrating now for two decades with evidence from cases like the Netherlands, Norway, and Eastern Europe.²² North offers nothing that would counter those demonstrations.

Despite these seeming failures, the core of North’s work—his analysis of the state and, as I shall indicate below, of the firm—remains as a vast and essential contribution, which cannot possibly be discarded. Because, as North has elsewhere observed, the state is so singularly lacking in Olson’s book, we have reason to try North as its corrective; and Olson’s analysis of group influence may similarly help to complete North.²³

The counterpoint: North and Olson

The central mystery remains that of the relations among the state, organized groups, and economic performance. On this North’s analysis of the firm sheds much light. The firm after all is a group, if a highly specialized one, and it resembles the state in its reliance on supervision, command, and ideology. To economic historians the central question is why hierarchy of this kind should so generally have supplanted the market. Why, for example, does the simple textile broker, purchasing from many competing producers and paying by quality and quantity of output, give way to the textile firm, which controls every phase of production, pays by input (hours worked), and must bear heavy costs of supervision and agency? North suggests three main motivations: new *economies of scale*, for example in the vertical integration of production; a greater need for *uniform quality*, coupled with high costs of postproduction inspection; and the need to *avoid bottlenecks* and postcontract cheating by highly specialized and essential classes of producers.

All of this is highly plausible, indeed too plausible to confine to the firm. If workers and managers can be motivated in these ways to accept hierarchy, why not other classes of producers or indeed other groups? If the state happens to be the form of hierarchy that can best meet these three problems, why should not the state, rather than firms, assume the functions? If firms can develop ways of avoiding the bottlenecks that greedy groups of producers might create, why can states or coalitions not do the same?

22. Arend Lijphart, *The Politics of Accommodation: Pluralism and Democracy in the Netherlands*, 2d ed. (Berkeley: University of California Press, 1975), chap. 2; Harry Eckstein, *Division and Cohesion in Democracy: A Study of Norway* (Princeton: Princeton University Press, 1966), esp. chap. 3; cf. Brian Barry, *Sociologists, Economists and Democracy*, 2d ed. (Chicago: University of Chicago Press, 1978), pp. 86–87.

23. Douglass C. North, “A Theory of Economic Change” (review of Olson, *Rise and Decline of Nations*), *Science* 219 (14 January 1983), pp. 163–64.

I am indeed suggesting that we regard states, firms, and social coalitions merely as aspects of a single phenomenon, namely that of rationally accepted subordination. (North himself implies in his concluding chapter that all three are really parts of the “structure,” or institutional framework, of his title.) In practice, certainly, no rigid separation is possible: the Renaissance guilds acted as pressure groups within their cities and appropriated parts of state power; they also closely resembled firms in their reliance on wage labor and in their concern with product quality. More recently the Irish state has guaranteed the quality of exported butter and the Mexican state the quality of handicrafts; pressure groups, notably the bar and local medical associations, have governed important questions of property rights; and in not a few cases, particularly in Southern Italy, Detroit, and Las Vegas, organizations not exactly coterminous with the state have enjoyed a “comparative advantage in violence.”

On this point Olson and North correct each other: groups can gain control of property rights without, or even against, state action (Olson’s example of the Indian castes will persuade at least those who have read Bailey or Dumont), but—and here North’s analysis of the firm must be generalized to correct Olson—even small groups need not pursue merely distributional goals. As just noted, the guilds, despite their narrowness, had as a major function the certification of quality, and so do present-day professional associations.²⁴ Farmers’ cooperatives in late 19th century Denmark sought not protection from the market but greater quality and productivity.²⁵ Not only in Sweden but in West Germany and Japan, trade unions have frequently held back wage demands and helped to boost quality in the interest of export promotion, not only for the economy as a whole but for their particular firm or sector.²⁶

Since even narrow coalitions can act to achieve greater efficiency—*productively* rather than *distributionally*—Olson’s theory must be modified. The evil consequences he predicts indeed ensue when distributional coalitions proliferate; but, since Olson is wrong in relating distributional orientation only to size and proliferation only to age, we must try anew to analyze why coalitions will grow and why some will pursue productivity, others only a bigger share.

North offers no definite answer, but the beginnings of one are not far to seek in his framework. As North and Thomas had observed, and as Olson admits (pp. 132ff), cartels cannot long flourish without insulation from a larger market; and protection, whether by tariffs or by more covert methods,

24. On the guilds see North and Thomas, *Rise of the Western World*, p. 57.

25. Alexander Gerschenkron, *Bread and Democracy in Germany* (Berkeley: University of California Press, 1943), pp. 37–40.

26. Guido S. Goldman, “The German Economic Challenge,” in Andrei S. Markovits, ed., *The Political Economy of West Germany: Modell Deutschland* (New York: Praeger, 1982), p. 18; John Holusha, “Japan’s Productive Car Unions,” *New York Times*, 30 March 1983, p. 27.

cannot succeed where autarky is impossible.²⁷ Hence, to the extent that a country's factor endowments require it to trade, we might expect, its coalitions will seek efficiency and quality rather than a bigger share.²⁸ The sole exception would logically be those economies whose exports, usually in the simplest primary products, are subject to little variation in quality and hence to little improvement.

If these arguments are correct, Olson's conclusions are all but reversed. Postwar Germany, Italy, and Japan succeed economically not because of totalitarian episodes, or foreign occupation, or integration into larger units but because *losses* of territory, of empire, or of the hope of empire left them with factor endowments that indisputably forced them to trade. Britain and the United States fall behind precisely because they remain so long in the contrary position: possessed of dominions that made autarky seem achievable—in 1953 over half and in 1965 still one-third of Britain's trade was with the Empire and Commonwealth, and until 1973 trade was less than 10 percent of U.S. gross national product (GNP)—their coalitions, even if weaker, could concern themselves more with distribution and less with production and quality.²⁹ Perhaps at the greatest advantage were such small states as Belgium and the Netherlands, whose situation had long condemned them to trade and whose coalitions were therefore old hands at the enhancement of productivity.³⁰

In this light, the European Community also assumes a different aspect. While the separate-state coalitions were indeed initially weakened, exactly as Madison would have foretold, any advantage thus conferred (and in already free-trading cases like Belgium's it was probably nil) will have been quickly outweighed by an opposite effect, namely the real possibility of European self-sufficiency.³¹ That half of the Community's trade in 1980 was internal

27. That is, tariffs in trade-dependent countries cannot serve distributional goals. They can of course be used initially by such economies as part of a larger strategy to achieve competitiveness, as the experience of Sweden (see above) and Japan has conclusively demonstrated.

28. Cf. Peter J. Katzenstein, "The Virtues of Necessity: Small States in the International Economy" (tentative title of a forthcoming book).

29. A useful contrast is this: in 1954, trade outside the Commonwealth and Empire was equivalent to 18% of British GDP; by contrast in 1957, the last year before the creation of the European Economic Community, trade amounted to 30% of West Germany's GDP. See, for these and figures in the text, Organization for European Economic Co-operation, *Economic Conditions in the U.K.* (Paris: OEEC, 1955), pp. 9 and 15; OECD, *Historical Statistics of Foreign Trade* (Paris: OECD, 1982), pp. 84–85; and OEEC, *Economic Conditions in the FRG* (Paris: OEEC, 1960), pp. 30–35. More generally on the Empire as a source of British decline see Brian Lapping, *The Labour Government 1964–70* (Harmondsworth: Penguin, 1970), pp. 36 and 102.

30. Even since 1973, these two countries' increases in productivity, measured as growth in real value-added per person employed, have been among the five highest in the OECD in both the manufacturing and the industrial sectors: OECD, *Historical Statistics of Foreign Trade*, p. 44.

31. In Madison's day the construction of effective distributional coalitions over large areas doubtless took much longer. Modern means of communication have made organization far easier and more rapid. (I owe this point to my colleague Marvin Hoffenberg.)

suggests a potential for cartelization and protection that, at least in agriculture and steel, has perhaps already begun to be exploited.³²

Olson thus revised can also lend substance and precision to North's account of the rise of state intervention and the supposed decline of efficient property rights in most advanced economies since about 1890. Olson contends that, wherever strong coalitions have managed *within* a country to insulate one sector's prices from market fluctuations, those caught in the now more violently fluctuating "flexprice" sectors have quickly turned to the state for relief; for example, U.S. farmers' support for Populism. The same logic ought to apply *internationally*: those societies most dependent on the international market, and particularly those whose products are subject to the most violent price fluctuations, ought also to incline most, not necessarily as North contended to "massive ideological alienation," but to state intervention. David Cameron has presented evidence to show that the state has indeed grown most rapidly in recent decades in those countries most exposed to world trade; and in American (and, until World War I, British) "exceptional" resistance to socialism, there is supporting historical evidence.³³

Taken together, these results imply that highly trade-dependent economies, at least to the extent that they export more than undifferentiated primary products, will have both interventionist states and dense networks of powerful productive coalitions,³⁴ often closely entwined and interpenetrated—a picture very different from Olson's but one to be encountered in the Low and Scandinavian countries, in postwar West Germany, and, with the reservations already suggested, in Japan and Italy.

Less trade-dependent countries may be expected to have powerful distributional coalitions, but it does not follow that in such cases the state will be "weak" in all senses or the mere plaything of competing interests. States may gain and hold wide powers, both North's analysis and the history of European absolutism teach us, as much (or more) because of *foreign threats* as because of the vicissitudes of markets. When states are thus strengthened in societies with powerful distributional coalitions, the conflict between state and society is likely to be sharp and continual. Theda Skocpol's work, which deals precisely with relatively self-sufficient empires under conditions of military threat, can be read as a chronicle of the most intense moments of such conflicts; and the whole history of modern France can be understood—at least Charles de Gaulle so understood it—as an unceasing struggle between

32. On internal trade see OECD, *Historical Statistics of Foreign Trade*, pp. 38–39. A modified version of Olson's second chief proposition may eventually prove to be true, namely that distributional coalitions tend to gain strength at a constant (if secularly increasing) rate in *self-sufficient* societies.

33. David R. Cameron, "The Expansion of the Public Economy: A Comparative Analysis," *American Political Science Review* 72 (December 1978), pp. 1243–61.

34. In fact unions in open economies seem to be more powerful and centralized—a curious finding, if they are as distributional as Olson would imply. See Geoffrey K. Ingham, *Strikes and Industrial Conflict* (London: Macmillan, 1974), p. 42.

the needs of national survival and the greed of domestic interests.³⁵ In societies of this kind, as both North and Olson would expect, the economy performs best when the state is powerful, autonomous, and threatened by readily available “substitutes”: France under Napoleon III and de Gaulle, Germany under the Empire.³⁶ Paradoxically, however, the extreme powers that states of this kind achieve in wartime often prove disastrous for them over the longer run: for reasons that Olson’s earlier work can also make clear, distributional coalitions will often behave as productive ones under conditions of total war. When they do so, embattled governments are tempted to relate to them accordingly: Germany in World War I, Britain in World War II.³⁷ When, once the emergency is past, the coalitions revert to type, the state has often lost all power to resist and can regain it, if at all, only by a breach of the existing constitution.

More ominously, the view taken here implies also that in countries of “borderline autarky”—ones that approach but do not achieve self-sufficiency—distributional coalitions may arise but will necessarily advocate conquest of the lacking resources as part of their program: Japan, and perhaps Germany (as I discuss below), in the 1920s and 1930s, may serve to illustrate the point.

Olson and North thus combined and amended can, I think, also account for the frequently tyrannical governments and insecure property rights of today’s Third World countries. Their exposure to trade keeps government strong and distributional coalitions weak (the sole exceptions, predictably, have been countries with some potential for self-sufficiency, like Brazil and Argentina); and the almost invariant quality of their exports provides little incentive for the development of productive coalitions. As Tocqueville might have put it, here the state is strong and society is weak.

Finally, we should observe that the theory so amended no longer entails either Olson’s or North’s pessimistic predictions. To the extent that states become and remain dependent on trade, exporting goods differentiable in quality, nothing in the theory so far excludes continued growth. Yet there may be other reasons for pessimism, as we shall see.

Gilpin on hegemony and systemic change

Olson and North both seem implicitly to assume that a state’s international power depends almost wholly on its economic health and growth. This Gilpin

35. Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge: Cambridge University Press, 1979), esp. pp. 40–41 and 47–48; and Charles de Gaulle, *Memoirs of Hope: Renewal and Endeavor*, trans. by Terence Kilmartin (New York: Simon & Schuster, 1971), pp. 147–48 and 308.

36. On Napoleon III’s France see for example J. C. Asselain and Christian Morrisson in Mueller, *Political Economy of Growth*, chap. 8, at p. 167.

37. Gerald D. Feldman, *Army, Industry, and Labor in Germany, 1914–1918* (Princeton: Princeton University Press, 1966); Samuel H. Beer, *British Politics in the Collectivist Age* (New York: Vintage, 1969), pp. 212–16.

denies. Moreover, he focuses on two problems largely neglected by the other two theorists: first, the governance, and the impact on individual states' economic choices, of the *international system*; and second, the likely incidence and effects of *war*.

Following McNeill, Gilpin first observes that military and economic power can actually be inversely related, since for long centuries less advanced economies routinely plundered wealthier ones.³⁸ The extent to which wealth translates into power in fact varies, Gilpin contends, with three main factors: economic development and growth; structures of social organization and governance; and the available technologies of warfare, transportation, and communication. The rise of the nation-state, a new and more effective form of governance, mobilized greater power from the same resources. The introduction of new weaponry and new infantry techniques in the 17th century gave the advantage to more expensive standing armies and hence made wealth matter more, a tendency that was accelerated by the even more expensive technologies of the 19th and, until recently, the 20th century—improved artillery and armor, steam and steel battleships, aircraft. (Nuclear weapons on the other hand, Gilpin speculates, may have substantially divorced military from economic power by giving even quite modestly endowed states enormous destructive potential.) In transportation, the advent of the railroad, by permitting more rapid deployment of soldiers and weapons, strengthened such continental powers as Germany, Russia, and the United States independent of any increase in their wealth and thus positioned them better to challenge Britain's sea-based hegemony.

Such changes matter, according to Gilpin, because they profoundly affect the stability of the international system and the likelihood of armed conflict. Gilpin takes it as axiomatic—and this is his fundamental break with most previous analyses of these issues—that states strive to change the international system only to the extent that the benefits of doing so outweigh the costs. State leaderships, in other words, behave *rationally*. Only when every power estimates the costs of change as equal to or greater than the benefits will the system be stable or, what Gilpin defines as the same thing, “legitimate” (pp. 10–12 and 50–51). When any power anticipates that benefits will exceed costs, the system will be unstable. And what principally lowers the costs of a challenge is some shift in power that favors the challenger.

What is sought is change that raises the prestige or the position in the international division of labor of the dissatisfied power, or that modifies the rules of the system in its favor. *How* change is sought varies with the type of international system. While other systems have been known historically and are still theoretically possible (empires, true balances of power), the international arena since at least 1815 has been hegemonic, that is, its boundaries, basic rules, hierarchy, and international division of labor have been

38. William H. McNeill, *Past and Future* (Chicago: University of Chicago Press, 1954).

primarily determined by a single leading power. Barring what Gilpin calls systems change—transformation into a nonhegemonic system—alterations either will be at the relatively trivial level of interactions change, a category to which Gilpin relegates not only shifts of alliance but most so-called changes of “regime,” including those analyzed by Robert Keohane and Joseph Nye, or will be systemic change, displacement of the previous hegemonic power and fundamental revision of boundaries, rules, and all the rest (pp. 39–44). While such a displacement might in theory be attempted or achieved peacefully, it has historically occurred only as a consequence of what Gilpin, following Raymond Aron, calls hegemonic war—a general and unlimited conflict for domination of the system (chap. 5, esp. pp. 207–8).

What, then, can make it rational for a state’s leadership to risk hegemonic conflict? By the logic of Gilpin’s assumptions, the answer can obviously be only some antecedent change that lowers the costs or raises the benefits of doing so. We have already seen what usually lowers the cost: as the hegemon weakens or a rival gains strength, the rival’s estimated costs of a challenge decrease. In contrast, what chiefly raises the benefit of a challenge to the existing system (or, what is the same thing, raises the opportunity costs of continued acceptance) and thus also decreases stability in the system is four factors: first, innovations that radically increase *economies of scale*, especially in the provision of defense and other public goods (for example, the military revolution of the 17th century, which made not only wealth but size more important); second, the rise of *costly externalities* that are beyond the reach of the existing state (the Athenians’ desire to make their allies pay a larger share of the common defense); third, *domestic political upheavals*, often a consequence of economic growth, which bring to power persons or groups who see larger opportunities or who simply have different memories (as perhaps in the French and Soviet revolutions); and fourth, the grounded perception that some “*window of opportunity*” is about to close forever (observable, Gilpin thinks, in the “race for empire” of the late 19th century and, frequently, in a hegemonic power’s contemplation of pre-emptive war against a rising potential contender).

Independent of these factors, Gilpin contends—and here he shares the neo-Malthusian pessimism of North—all powers tend over time to weaken, and hegemonic powers tend to do so particularly. Thus, cyclical hegemonic conflict is all but inevitable. In part this weakening flows from a universal tendency to economic decline, which in turn Gilpin sees as due to the exhaustion of cheaply exploited factors of production and of existing property structures *à la* North (a fate that, however, in Gilpin’s view can be postponed by societies with open frontiers, such as the United States, Russia, and China for much of their history); and the randomness and clustering of crucial innovations, which therefore tend rarely to occur successively in the same society. Specific to hegemonic powers, however, are two further factors: the tendency of military and administrative expenditures to increase more rapidly

than income; and the displacement of investment by both private and public consumption. Increasing military expenditure is caused by the diffusion of military technology, by increasing costs of agency, by tendencies toward suboptimization (secession or autonomy), and not least by decreasing martial virtue among the citizens of the (secure) hegemon. Decreasing investment stems both from military consumption and from Veblenesque effects of affluence in the society at large.

From all of this it must follow—although I am here inferring from Gilpin rather than summarizing him—that the most dangerous situation of all will be one in which a hegemonic power is declining economically while its costs of defense are rising and while a rapidly rising rival is benefiting not only from economic growth but from changes in transportation and in economies of scale in defense; and in which negative externalities, for example in trade, are growing: in short, exactly the situation that characterized the rivalry of Britain and Germany at the turn of the present century.

Gilpin's is obviously a rich and insightful work, but it is also a sprawling and eclectic one, whose logic has more leaks than the *Mary Rose*. Exactly because—as with Olson, albeit for quite different reasons—I want to defend the book and emphasize its worth, I shall at once concede its faults. First, Gilpin's grasp, not only of economics but of comparative history, seems less sure than North's. One never feels quite certain that Gilpin understands indifference curves or the law of diminishing returns, and uncertainty turns to impatience when to his hesitant recapitulations of the familiar is added the zeal of the convert. In his historiography Gilpin falls into such solecisms as claiming that trade was insignificant in the “command economies” of all previous empires, presumably including the fiercely trading one of Rome (pp. 127 and 133);³⁹ describing secure property rights across national boundaries—which, if one accepts Keynes's description, almost certainly reached their zenith in the 19th century⁴⁰—as a “revolutionary feature of the modern world,” exercised in the 1980s on an unparalleled scale (p. 25); or taking for granted that the present-day United States suffers from comparatively “high taxation” (p. 232) when in fact its rate is the second lowest (roughly tied with Japan's) among the major industrial powers.⁴¹

Far more serious than such slips as these is the work's lack of deductive rigor and parsimony. To accept Gilpin's central conclusions, we are required to grant at least a half-dozen weakly supported premises: for example, that crucial innovations arise randomly and tend to cluster, that the effects of innovations on military power can rarely be foreseen, that the hegemon's citizens always lose their martial virtue, that Veblen was right about con-

39. See for example Frank Burr Marsh, *A History of the Roman World from 146 to 30 B.C.*, 3d ed. (London: Methuen, 1963), pp. 3–5.

40. John Maynard Keynes, *The Economic Consequences of the Peace* (New York: Harcourt, Brace and Howe, 1920), pp. 11–16.

41. OECD, *Historical Statistics, 1960–1980*, p. 59.

spicuous consumption, that costs of agency must always rise in large states. One objects to these claims not so much because they are doubtful (although I think most are) but because they are necessary to the argument. Unless Einstein was wrong, and God is malicious as well as cunning, reality cannot be *this* complex—so, at least, the scientist in us protests. Gilpin's conclusions may well be right and, indeed, I shall argue presently that most of them probably are, but his argument of them is too cluttered, too remote from deductive clarity, to be convincing.

The fugue of the art: Gilpin, North, and Olson

Gilpin's central contention requires only two assumptions: if most states value hegemony more than peace and if every hegemon eventually weakens, then recurrent hegemonic wars are indeed likely. Gilpin, however, wants to go beyond this, as should we, to identify factors that accelerate or retard the inevitable cycle.

North and Olson offer ways of tidying up Gilpin's argument. Most of its sprawl arises from his efforts to account for innovation and military power as independent variables. With respect to innovation, North observes that many technological advances, from agriculture to the three-field system to steam power, appear to have lain unused for decades or centuries until shifts in relative prices made their employment profitable. From this he suggests the radical simplification that technology is ordinarily a *dependent* variable in human affairs rather than the basic cause that its historians at their most zealous have made it out to be.⁴² Innovation, as use and not merely as invention, is almost entirely determined by the incentives to individual innovators—the extent to which they are enabled to appropriate a share of the social benefits of their work—and by relative prices, which determine whether there is a social benefit.

Military technology, it must be stressed, is for North the significant exception. Given states' obvious interest in coercive power, innovations in this field tend to go unused only where a regime supposes itself to be invulnerably protected by the older technology or where, as Gilpin observes (p. 63), introduction of the new weaponry seems as likely to ruin existing elites as would military defeat.

If North's view is the correct one, Britain's hegemonic decline has little to do with German railroads—and the railroad itself, as Hobsbawm has shown, resulted less from any technical breakthrough than from a growth of markets and a surfeit of capital—and almost everything to do with the British failure to innovate, either militarily or economically, in, for example,

42. For example, Lynn White Jr., *Medieval Technology and Social Change* (New York: Oxford University Press, 1966), esp. pp. 69–76.

large-scale steel production and the new chemical and electrical industries.⁴³ (In fact in this case power waned quite directly with wealth: British politicians debated repeatedly, beginning with Gladstone's final fall from office in 1894, whether they could afford to keep pace with the German navy, while Germany comfortably supported that navy along with an army that dwarfed Britain's.)⁴⁴

British military failure, to the extent that it was independent of economics, is explained readily enough from North's perspective: military innovation is less likely to be valued by states that are, or think themselves, secure. British economic failure was due not to any random "clustering" of innovations in other lands but to long British neglect of technical education and of investment in basic research:⁴⁵ in short, from a social failure to lower the costs or to increase the incentives of innovators. The more basic question is, then, why did Britain fail to offer the requisite facilities and incentives? Gilpin, who does not deny the significance of failures to innovate, attributes them in general to ill-defined "institutional rigidities" (p. 189). Olson's logic, we can now recall, is clearer and more informative: innovation and growth (and, one suspects, investment) are less likely in societies dominated by distributional coalitions: that is, as has been argued above, in societies that are or can easily be economically self-sufficient and whose military security denies the state any claim to countervailing strength.⁴⁶

Taken together these propositions imply, as I think Charles Maier first phrased it, that "hegemony corrupts." The secure state, with guaranteed access to raw materials and markets, grows less rapidly, rewards innovation less, and neglects the arts of war. This is Gilpin's result without Gilpin's tenuous logic, and it implies a stricter corollary: that decline will set in almost immediately after hegemony, or at least secure self-sufficiency, is achieved. This seems to hold for both Britain and the United States, which Gilpin takes as the only two historical cases of hegemony. Probably it also applies to the recent experience of the Soviet bloc.⁴⁷ This much of the shared pessimism thus seems warranted, but we have still no reason to think, as Olson and North do, that *all* highly developed economies must eventually decline.

It can be fairly asked, no less of Gilpin's account than of the one I am offering, why such a seemingly poisoned apple as hegemony should be so avidly sought. Gilpin's answer, while predictable, is only partly persuasive:

43. Hobsbawm, *Age of Revolution*, pp. 63–67; on military innovation see Philip Magnus, *King Edward the Seventh* (Harmondsworth: Penguin, 1964), chap. 17; on economic innovation see David S. Landes, *The Unbound Prometheus: Technological Change and Industrial Development in Western European Economies from 1750 to the Present* (Cambridge: Cambridge University Press, 1972), pp. 262–76 and 281–90.

44. Philip Magnus, *Gladstone: A Biography* (New York: E. P. Dutton, 1954), pp. 414–20.

45. Landes, *Unbound Prometheus*, pp. 339–58.

46. For an enlightening contrast between the British Empire's relative self-sufficiency and Germany's growing dependence on insecure trade before World War I see David Calleo, *The German Problem Reconsidered: Germany and the World Order, 1870 to the Present* (Cambridge: Cambridge University Press, 1978), pp. 35 and 83.

47. Cf. Pryor in Mueller, *Political Economy of Growth*, pp. 96–97

leading powers seek hegemony to keep international markets open, which it is in their interest to do (pp. 138–39). This can account for Britain's rise, but only imperfectly for Germany's subsequent challenge or for the long hesitancy of some plausible challengers, including notably the United States.⁴⁸ I suspect, rather, that states do not really seek hegemony except, as Gilpin describes empire-seeking in another context, "preclusively": to prevent an economically faltering hegemon from excluding its more efficient competitors from crucial markets or resources. This, at any rate, is David Calleo's account of the 19th century German challenge, which was advocated (as Kehr showed with respect to naval expansion) more by the free-trading than by the protectionist forces within the Reich.⁴⁹ If challenges are generally so motivated, the struggle for hegemony becomes a classic example of the Prisoner's Dilemma. All advanced states would be better off and would continue to grow more rapidly if free trade were preserved without hegemony; yet each, fearing that some other will restrict it adversely, seeks hegemony.

On one other significant point, however, Gilpin's analysis sets limits to those of Olson and North. Because changes in international hegemony can alter the conditions and the stability of trade and the range of "permitted" activities in the international division of labor, hegemonic instability or displacement can also profoundly affect the prosperity of economies, the policy choices open to governments, the strength of contending domestic groups and coalitions, and, indeed, states' particular forms of property rights. Osvaldo Sunkel and Pedro Paz have argued that Latin America's industrialization was first accelerated by world depression and war and then retarded and reversed by the slump in export earnings that resulted from the final shift from British to U.S. hegemony after World War II.⁵⁰ Russia's economic policy and progress before World War I depended no less on other powers' trade policies and on whether British hegemony would survive the growing German challenge. And David Abraham has recently claimed that even so profound an alteration of domestic power, property rights, and the international system as the rise to power of National Socialism in Germany may find its explanation in shifting international governance: the dynamic and export-oriented industrial sectors in Weimar Germany, which for reasons of self-interest had allied with workers to support the democratic regime, were fatally weakened by the rise of international protectionism after 1929, and their collapse may have so vitiated the "Weimar Coalition" of interests as to make possible Hitler's seizure of power.⁵¹

48. David Lake, "The International Economic Structure and American Foreign Economic Policy, 1887–1934," *World Politics* 35 (July 1983).

49. Calleo, *German Problem Reconsidered*, chap. 4.

50. Sunkel with Paz, *El subdesarrollo latinoamericano y la teoría del desarrollo*, 4th ed. (Madrid: Siglo veintiuno de España, 1973), pp. 71 and 436ff.

51. Abraham, *Collapse of the Weimar Republic: Political Economy and Crisis* (Princeton: Princeton University Press, 1981).

While general observations on these points are still difficult, we can perhaps advance three claims. First, stable hegemony helps, and instability harms, nearly all economies that export quality-differentiable products.⁵² Second, within borderline autarkical economies as defined earlier, stable hegemony strengthens trade- and productivity-oriented sectors, while instability increases the weight of protectionist forces and distributional coalitions. Third, where an exporter of primary products enjoys complementarity with the hegemonic power—as did the United States before the Civil War and Argentina before 1914—it will be less subject to market fluctuation and hence, by our earlier reasoning, likely to have a weaker state than its situation would otherwise imply.

If we grant that different hegemonies can affect the international division of labor differently, we must also qualify substantially Gilpin's general assertion of the likelihood of war in situations of "eroding hegemony."⁵³ The threat that a rising power presents to a hegemon, and hence the likelihood that the hegemon will strike pre-emptively or the challenger preclusively, must vary with the presumptive role of the old hegemon in its successor's "new order." That such British statesmen as Cecil Rhodes and Stanley Baldwin looked far more equably on an American than on a German succession is to be explained not only by cultural ties (which had failed notably to prevent conflict when Prussia supplanted Austrian regional domination) but, surely, by relative American uninterest in many of those regions and sectors that most concerned the British.⁵⁴

What, finally, do these works tell us about the value of the rational-choice approach? The answer depends of course on the goal, on what one regards as good theory. The traditional standard demands simultaneously high marks on at least five criteria: parsimony, power, rigor, precision, and empirical accuracy. A useful second-best criterion has recently been proposed by Brian Barry, namely that theory "should help us to interpret and understand some complex social phenomenon by seizing on some crucial aspect of it and enabling us to think more clearly and systematically about it than we would have been able to do without the theory."⁵⁵ By this less demanding measure, North's volume succeeds: its model of the rent-seeking state, of the firm,

52. The principal exception is of course the hegemon itself, which for reasons already advanced may be expected to decline under stable hegemony and may actually be stimulated by instability—as was the U.K. after 1931. (I am grateful to Peter Gourevitch and Calum MacDonald for having called this instance to my attention.)

53. Gilpin himself, interestingly, draws back from the implications of his arguments for the present day, claiming that hegemonic war between a declining United States and a rising USSR is unlikely because of a series of fortunate coincidences (pp. 234–40).

54. On British attitudes see Edward Hallett Carr, *The Twenty Years' Crisis, 1919–1939: An Introduction to the Study of International Relations* (New York: Harper Torchbooks, 1964), pp. 232–33.

55. Brian Barry, "Review Article: *Crisis, Choice, and Change*," *British Journal of Political Science*, 7 (1977), pp. 99–113 and 217–53, at p. 237.

and even of ideology does “enable us to think more clearly and systematically” about wide ranges of social phenomena and thus lends credence to the rational-choice perspective that underlies it.

By the more rigorous traditional demands, paradoxically, Olson’s theory may deserve higher praise, for it meets four of the five standards, failing only on the (alas, essential) one of accuracy. Yet even it, and a fortiori the efforts of North and Gilpin, fails to make a convincing case for the rational-choice approach as applied to questions of international politics and economic growth. Only when some rational-choice theory as parsimonious, powerful, rigorous, and precise as Olson’s also demonstrates great and surprising accuracy will—or should—a skeptical world be convinced. These efforts bring us palpably near, but not to, the goal.