

A Theory of Economic Change

The Rise and Decline of Nations. Economic Growth, Stagflation, and Social Rigidities. MANCUR OLSON. Yale University Press, New Haven, Conn., 1982. xii, 274 pp. \$14.95.

Books with grandiose titles about societal change seldom live up to their promise, and this book is no exception. It is, however, at least partly successful; and even a partial success is an extraordinary achievement. While we still don't know what accounts for secular economic and social change, Mancur Olson has made a solid contribution toward our understanding of those issues.

The Rise and Decline of Nations picks up where Olson's previous book, *The Logic of Collective Action*, leaves off, and one must understand the basic message of that book in order to understand this new study. In the earlier work Olson used the economist's basic behavioral assumption of self-interest-maximizing individuals and asked what kinds of groups will exist in such a society. He found that small groups would exist where the benefits to the individual of group actions would exceed their costs, or where individuals could be coerced into action; and that large groups (for example, the American Medical Association and trade unions) would exist where the members could receive exclusive individual benefits not available to outsiders. He also demonstrated that when large groups were organized to create change but did not possess some exclusive benefits to members they tended to be unstable and disappear. In essence, rational individuals will not incur the costs of participating in large group action when benefits can still be received by being a free rider. Olson's earlier book has had a major impact upon thinking in the social sciences, so much so, indeed, that the very term "free rider" has become a basic part of the vocabulary of social scientists.

In this new study Olson examines the consequences of the formation and development of interest groups for the performance of economies over time. His basic message is straightforward. Olson argues that special interest groups (called distributional coalitions) will tend to emerge and to rigidify economies through time; the result will be stagnation. When these distributional coalitions

are destroyed, economies will grow rapidly. It is worth summarizing the basic implications of this book in Olson's own words:

1. There will be no countries that attain symmetrical organization of all groups with a common interest and thereby attain optimal outcomes through comprehensive bargaining.
2. Stable societies with unchanged boundaries tend to accumulate more collusions and organizations for collective action over time.
3. Members of "small" groups have disproportionate organizational power for collective action, and this disproportion diminishes but does not disappear over time in stable societies.
4. On balance, special-interest organizations and collusions reduce efficiency and aggregate income in societies in which they operate and make political life more divisive.
5. Encompassing organizations have some incentive to make the society in which they operate more prosperous, and an incentive to redistribute income to their members with as little excess burden as possible, and to cease such redistribution unless the amount redistributed is substantial in relation to the social cost of the redistribution.
6. Distributional coalitions make decisions more slowly than the individuals and firms of which they are comprised, tend to have crowded agendas and bargaining tables, and more often fix prices than quantities.
7. Distributional coalitions slow down a society's capacity to adopt new technologies and to reallocate resources in response to changing conditions, and thereby reduce the rate of economic growth.
8. Distributional coalitions, once big enough to succeed, are exclusive, and seek to limit the diversity of incomes and values of their membership.
9. The accumulation of distributional coalitions increases the complexity of regulation, the role of government, and the complexity of understandings, and changes the direction of social evolution.

Olson applies this theoretical framework to the economic history of the Western world. He maintains that it ex-

plains the rapid post-war revival of West Germany and Japan, as well as the growth of Korea, Taiwan, Hong Kong, and Singapore; the slow growth and ungovernability of Britain in recent times; and the relative growth rates of different regions of the United States, as well as the overall rapid growth of the American economy in the 19th century. Further, Olson argues, it helps to account for the emergence of Britain, Holland, and France in the early modern period; to explain the decline of old cities in the midst of expanding countries; and to account for the stagnation of India and China in the 19th century. Finally, in a chapter of sweeping implications, Olson argues that the theory also accounts for the current dilemma of unemployment and stagflation in the Western world today.

The contribution of this study is to show the consequences of interest-group pressures that have evolved in the modern Western world. But a lot that its title promises is left out. The Soviet Union and Eastern Bloc socialist societies have effectively eliminated competing interest groups but nevertheless show many of the characteristics of stagflation of modern Western democracies. Nor can we use Olson's model to account for the persistent instability and slow growth of many Latin American countries; or for large group action where there are not exclusive benefits to the participants, as in the case of what we observed in Iran in 1979 and 1980 in the overthrow of the Shah.

What Olson has done is to develop a part of the story, but the theory is incomplete. Let us see what is missing in Olson's argument. It is the state, with a monopoly of legitimate coercion, that specifies the basic rules of the game—the property rights structure—that, in turn, determine the performance characteristics of economies. Therefore, to the extent that distributional coalitions manage to rigidify a system and to prevent efficiency and productivity growth, it is because they have access to control or influence over the policies of the state. Nowhere in Olson's study is there any analysis of the state and of the way in which the state and distributional coalitions interact to produce the results he describes.

The state is not simply a passive reflection of interest group coalition (which is the implication of Olson's argument). It is built on the distribution of coercive force, which reflects the degree of monopoly power of rulers vis-à-vis constituents. The bargaining power of constituents is one important ingredient,

but so is the bargaining power of rulers, which has depended on the characteristics of military technology. Throughout history stagnation, rather than growth, has been predominant in nation states. From the Egyptian dynasties to the modern Soviet state, the characteristics of the state and the interests of its rulers have produced stagnation even when the distributional coalitions that are the heart of Olson's study have not evolved.

Equally fundamental is the dilemma that people don't always "free-ride." People frequently act through conviction about the legitimacy or fairness of the set of rules of the game that surrounds them. That is, if people are convinced the rules are fair, they may obey them even when at times they could be better off not obeying them. Conversely, when people are convinced that the rules of the game are unfair, they sometimes form large groups to attempt to overthrow the system and again are not free riders. In Olson's world, in which everybody is narrowly self-interested, stability of any kind would be impossible, because people would cheat, steal, murder, and so on, whenever they could get away with it. The costs of enforcement of any set of rules in such a society would be prohibitive. We do observe theft, shirking, murder, and the like, but we also observe that people do not undertake such actions when they believe the rules to be fair.

The enormous investment that governments and voluntary organizations make to convince people of the fairness and justice (or conversely of the unfairness or injustice) of the rules of the game is evidence enough that ideology matters. People do indeed behave the way Olson argues a good deal of the time—hence the strength of his argument. But many of Olson's distributional coalitions achieved their power and coherence because of strong ideological conviction. This is true equally of the labor movement and of today's environmental groups, such as the Sierra Club and the antinuclear movement.

Ideology also plays a key role in the relative stability of Western democracies in contrast to Latin American countries. Without incorporating the significance of ideology in modifying group behavior, we are missing an essential ingredient in the makeup of institutions.

As a result of a failure to take into account the existence of non-free-riding in the world, Olson gets caught up in a basic contradiction to his argument. The implications of his book are clear—the future of the world is dismal because self-interest groups will tend to form

coalitions and throttle economies. In the absence of revolutions, which Olson deplores and argues are not an answer to his problem, there appears no way out of secular stagnation. But wait: at the end Olson says we do have a way out. People can understand the message of his book and act differently. That is, they can act to eliminate distributional coalitions and to force societies to be competitive and, therefore, more productive. But here he has made his contradiction, because that would not be in the interests of the individuals themselves.

The essence of Olson's message is that these distributional coalitions are a natural outcome of individuals' acting in their own self-interest; and indeed it would take some strong conviction for them to act differently—that is, not free-ride. Olson's study does pose a major issue for the Western world: he is quite correct that the growth of special interest groups has led to many of the modern dilemmas. His failure to take into account both the role of the state and the importance of ideological conviction, however, results in his being caught in the trap of his solution's contradicting the basic premise of his book.

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A Wartime Effort

Radiation. One Story of the M.I.T. Radiation Laboratory. ERNEST C. POLLARD. Woodburn Press: Science and People, Durham, N.C., 1982. xvi, 198 pp. Paper, \$10.

In recent years, many have reflected on the conception, creation, and use of the atomic bomb during World War II, its subsequent effects on the United States, and the relations it created between the military services and the American scientific community. In contrast, few retrospective analyses have been made of the numerous other major wartime interactions between science and the military, even though their lingering effects have also been extremely important in shaping relations between the federal government and science over the last four decades. In this brief volume, Ernest Pollard reflects on one such interaction, the work of the Radiation Laboratory of the Massachusetts Institute of Technology, where he served as a staff member from 1941 to 1945.

By any measure, the Radiation Laboratory was a remarkable scientific insti-

tution. Formed in November 1940, it had a clear, but hardly simple, mission during its five-year life: to develop, from the newly invented multicavity magnetron, microwave radar equipment that could be used during World War II. Many, especially in the military, believed this to be impossible, for the technology was immature and the several attempts to develop microwave radar in service laboratories during the 1930's before the magnetron was available had all failed miserably. Better, they thought, to concentrate on improving the long-wave sets already in operation. The enthusiastic, dedicated employees of the Radiation Laboratory soon proved how wrong such judgments were. By 1942, microwave radars were regularly being delivered to the services, and the laboratory, with a staff that grew to 3900, eventually designed two-thirds of all the U.S. radar equipment built during the war. Then, when the conflict ended, the staff produced a series of authoritative textbooks on radar and its electronic components that are classics in their field. Looking back, Pollard tries to determine how all this happened. What were the qualities of leadership, cooperation, commitment, and intellectual stimulation that made the Radiation Laboratory so successful? And what had it been like to be part of such an institution?

There is much to fault in his answer. The most serious flaw is that the book, despite being aimed at a general audience—the cover, for example, boasts that there is *no* mathematics here and only *three* figures—provides too little general information about the formation, growth, organization, scope and extent of the Radiation Laboratory, and hence fails to give uninformed readers the overview they need to put Pollard's reflections in context. Additionally the chapters are a series of essays written at various times, and they are not well integrated. The text wanders too freely between personal reminiscence, general narrative description, and analysis. Significant conclusions are often poorly explained and supported. The volume is thus less insightful and well crafted than it should be, far less so than its obvious model, A. P. Rowe's *One Story of Radar*.

Yet despite its rough state, this book is a welcome publication. It should prove useful and interesting to all concerned with understanding the varied forms of interaction between science and the military during the war that have influenced American science ever since. Moreover, Pollard's comrades in arms will find his views on them and their joint enterprise