The Rise and Decline of Nations in Historical Perspective

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I

When an historian is called upon to scrutinize the work of an economist (or vice versa), the results are often of questionable value. The intellectual structure of the two disciplines seems to differ in ways that encourage mutual misunderstanding. The historian starts with a knowledge of his necessarily limited terrain: the historical data of some time and place. Here he is at home; he seeks to understand his environment with theory and abstraction (if he is wise), but these are not really his chief interests. The economist starts with a hypothesis which he seeks to test with appropriate data. If, as with Mancur Olson, he combines the rare talents of fruitful theorizing with bold application, the economist will make forays into a wide range of `factual environments' always shielded by a protective layer of abstractions.

By now it should be clear that confrontation between the economist and the historian resembles something like warfare between a conventional army and a band of guerillas: there are no agreed rules of the game nor are there shared criteria for success. But metaphors of war aside, if the chances for fruitful exchange in the academy are better concerning The Rise and Decline of Nations, it is in large part because Olson is no ordinary economist. His book, vastly ambitious as it is, nonetheless takes the historian's perspective seriously. Denying himself the shield of abstraction, Olson holds to high standards in seeking historical verification for his theoretical claims.

Before I begin discussing Olson's interesting and important work, a note of disclaimer is perhaps in order. The self-limitations that every serious historian must impose upon himself also prevent me from commenting on the subject of the rise and decline of nations per se; the subject is simply too vast to be adequately handled in this brief space. I will also pass over any consideration of differential rates of national economic growth; this phenomenon also involves too many variables for a brief discussion. Instead, I will restrict my comments to the more limited problem of understanding the conditions governing the creation and persistence of distributional coalitions. When do they arise? Under what conditions do they flourish? How much economic damage do they cause? Mancur Olson treats all of these relatively manageable questions, often drawing on historical evidence to buttress his claims. It is to these issues that I address myself.

II

Distributional coalitions require two things in order to rise and flourish: stability and
time. These presuppositions, so basic to Olson’s argument, receive scant treatment in his book. And to take for granted that their meaning is obvious would be dangerous indeed.

What is a stable society? Olson answers this question only by default: he tells us that an unstable society is one fundamentally disrupted by revolution or some other source of profound discontinuity in institutions. A stable society, we are left to reason, must be what an unstable society is not. Thus, in recent times Olson can identify Britain as a stable society for it has long been free of revolution, military occupation, or sudden alteration in the constitutional order. France, on the other hand, serves as an example of instability, since it has experienced at least three separate constitutions and military occupation within living memory.

There are good reasons to question whether these characterizations are adequate. They speak only of a narrow conception of stability. For a constitutional lawyer, Britain may well be more ‘stable’ than France. But does this perception also hold for an entrepreneur? I would argue that since World War II the environment for business decisionmaking has been far less stable in Britain, where nationalization and privatization of enterprise follow each other in irregular cycles and the desirability of EEC membership still remains at issue even after 25 years of debate. Conversely, in France certain technocratic and planning ideals have long inspired public policy, making for a more predictable, and hence more stable, business environment.

Olson’s concern seems to be with stability narrowly defined: any environment conducive to the formation and persistence of ‘conspiracies against the public’, i.e., distributional coalitions, he understands to be ‘stable’. One needs to know more about what sort of stability this is, however, for by Olson’s own account several constitutionally stable societies (Britain, Switzerland, and Sweden) exhibit great variety in both the level of development and the influence of their distributional coalitions. I suspect that he is more concerned with particular legal and constitutional arrangements than with stability broadly construed. But lacking expertise in this area, I shall leave further discussion of this conceptual issue to others. As an historian I cannot, however, do the same with another conceptual question: the nature and role of time.

What role does time play in Olson’s theory? Certainly it takes time to form coalitions, and it takes more time for the coalitions to exercise their power effectively. Moreover, coalitions may vary in their gestation periods. Taken together, these factors suggest that time brings about a gradual accumulation of distributional coalitions; from the establishment of a stable society the clock begins to tick, and as time passes, the coalition density rises, and with it the symptoms of impending decay that are the chief concern of The Rise and Decline of Nations.

Much of the evidence that Olson presents about coalition formation strengthens my conviction that the passage of time is less straightforward than is suggested by the metaphor of the ticking clock. In some countries and at some times the clock ticks very slowly; elsewhere and in other periods it runs faster. In this respect, I wonder if it is not more accurate to see distributional coalitions as ubiquitous or, at least, everywhere latent. They can then be said to become particularly active or harmful only in certain contexts. For instance, it may be found that distributional coalitions appear to be more pernicious in their effects in eras of slow growth or decline. Indeed, they may be encouraged to become more active in such situations because questions of distribution seem more important than those of growth. The rapid growth of unionization during the 1930s in the United States and in the era of slow real income
growth in Europe from 1900 to World War I, the proliferation of tariffs in the 1930s, the popularity of mercantilist policies in the seventeenth century 'age of crisis'—all of these varied examples hint that distributional coalitions may often be as much a symptom of decline as a cause of it. If my point is even partially plausible, the interpretation of Olson’s equations regressing the coalition-density in 48 of the United States on several variables related to time and economic growth are open to question: the direction of causation would no longer be obvious.

Another situation that inordinately stimulates coalition formation and activity is inflation and hyperinflation. When societies experience sudden shifts in the distribution of income because of valuation in the distribution of assets and claims fixed in money values, as in Weimar Germany and (less dramatically) the United States very recently, distributional coalitions, whether new or long established, begin to assume a new importance to their members as a means of mitigating inflationary effects.

Finally, a word must be said about the demise of distributional coalitions. Olson implies that old organizations never die, except through a radical restructuring of the society. But surely the calculus that induces people to form and join pressure groups of one sort or another at least occasionally induces them to withdraw their support. The interests of the executive staff may keep the 'society for the Advancement of Buggy Whip Retailers' in existence for a long time, but to count it in the accumulation of distributional coalitions after the membership has long departed would, after some point, become misleading.

In sum, distributional coalition formation may not be the continuous, unidirectional phenomenon that Olson supposes, and this suggests that the conditions that encourage the phenomenon need more careful study.

III

I would do Olson an injustice to leave the subject of the dissolution of distributional coalitions without discussing his view that jurisdictional integration (the merging of autonomous economic units into large polities), free trade, and a competitive environment generally are the best antidotes to the societal 'aging process'. He defends the view with a broad range of historical examples; it is their consistency with Olson’s argument we must now scrutinize.

It does not diminish Olson’s achievement to point out that many others have identified distributional coalitions (but called variously, custom, monopoly, privilege, mercantilism) as a major obstacle to economic advance. In the 1880s the economic historian Arnold Toynbee, who is credited with coining the term 'Industrial Revolution', saw the main task in explaining the rise of modern industrial society as accounting for how the 'plant of progress' broke through 'the cake of custom'—a hard-baked crust of earth, all but impregnable to the tender shoots of enterprise trying to poke through. Finally, of course, the shoots of enterprise succeeded and grew into sturdy plants, in the process crumbling and pulverizing the cake of custom. The key variable in this growth, as Toynbee and many of his successors saw it, was institutional. Britain, without undergoing political revolution, renewed its legislation, withdrew the base of support from the distributional coalitions of the past, and established an economic environment of laissez faire. This process was exemplified in such legislative acts as the repeal of the Corn Laws and Navigation Acts, and the reforms of the Poor Laws.
In recent decades economic historians have depreciated the importance (and questioned the accuracy) of this theme. They have found more compelling explanations for the Industrial Revolution in technological change, the expansion of the market, national character, and capital accumulation, to name but a few.

Olson now directs our attention once again to the themes raised by Arnold Toynbee a century ago. But he sees the cake of custom crumbling much earlier: not in the late eighteenth century, but in the sixteenth and seventeenth centuries. He writes: ‘We know that the creation of effective national jurisdictions in Western Europe was followed by the commercial revolution, and in Britain ultimately by the Industrial Revolution. (Olson, 1982:120) Olson’s judicious wording avoids an explicit assertion of causality, but there is clearly something worth investigating here.

Olson suspects that the creation of large territorial states and efforts to centralize state administration had the effect of destroying the base of support from ‘older’ distributional coalitions and other obstacles to commercial enterprise, particularly those that had long been fostered by petty principalities and the autonomous medieval cities.

The error that must be avoided here is to exaggerate the ‘slate-cleaning’ effects of early modern state building. Bourbon France and Habsburg Spain rarely reduced, let alone abolished, the internal boundaries that separated the constituent elements of their states. Internal tariffs, separate legal systems, etc., long continued to characterize these nations. In France the policies of Richelieu and Colbert tended to drain many local jurisdictions of their autonomy, but the revenue raising possibilities in selling ‘property rights’ in local administration and tax and toll collecting, and in the chartering of guilds caused the ground to be at least as thick with conspiracies and obstacles as ever before.

It may well be true that distributional coalitions could now be larger (and, conceivably less pernicious in their effects), but it remains an open question whether state building replaced small coalitions with larger ones or simply added larger ones to the existing small ones.

In only one large country is it obvious that the state pursued the option of centralization while weakening local conditions. Already in the sixteenth century the English government organized foreign trade in a series of national monopolies, the chartered companies. This collection of privileged merchant guilds was superseded, beginning in 1651, by the decree of the Navigation Acts which created a more all-encompassing national foreign trading monopoly. Olson argues that this sort of change is an improvement over numerous monopolistic organizations since a ‘peak’ organization is less likely to sacrifice national well-being to private gain. Adam Smith, who culminated against the conspiracies against the public made possible by the Navigation Acts in his Wealth of Nations, had a less sanguine opinion. But in any event, the centralized character of English mercantilism was quite exceptional.

IV

The fastest growing economy of the seventeenth century was that of the Dutch Republic. On the face of it, the extraordinary achievements of the Dutch fit Olson’s theory nicely: a revolt succeeded in establishing a new state which quickly rose to unprecedented heights of prosperity and economic power. But the Dutch Revolt did not clear the boards of distributional coalitions, with the single—and partial—exception of those supported by the Church. On the contrary, a major motive for the
revolt against Spanish control was to prevent the introduction of the centralizing and rationalizing policies of Philip II.

While the Republic’s central government was new, it was nonetheless endowed with very limited powers. Power resided in the provinces (which were old institutions), and still more in the cities (likewise, very old). Throughout the seventeenth century most economic policy making continued to be made at the municipal level, and it is at this level that most distributional coalitions found nourishment. For example, when the new state formed a navy, it established five rather autonomous admiralties scattered in cities throughout the Republic; when the central government sought to form a large monopoly firm to trade with the East Indies, the organization was divided into six separate chambers, each located in a different city.

It would be difficult to imagine a more favorable environment for the creation and preservation of local cabals and conspiracies. But, despite all of this, it must be admitted that natural tendencies to exploit these opportunities for monopoly creation were often held in check by competition among the towns, and by the presence of numerous economic opportunities that eroded barriers to market entry necessary for an effective distributional coalition. Hence, Dutch foreign trade (everywhere except to the East and West Indies where interests of state prevailed) was free and competitive and guilds had little power in those industries that exported beyond their own towns.

With the passage of time, the Dutch economy became more and more sclerotic. By the late seventeenth century, and certainly in the following century, there is abundant evidence that monopolistic arrangements are more effective and more widespread. Symbolic of this process is the oligarchization of political life. Foreign trade and the international financial sectors continued to function in a competitive framework, but Dutch society was increasingly burdened by vested interests than had been the case a century earlier. Was this a consequence of the passage of time in a stable society, or was it a reaction to changes in the international environment that reduced the number of economic opportunities, thus redirecting the attention of the Dutch from expansion to distribution and preservation? The evidence, it seems to me, suggests that the second explanation is more compelling.

V

In the struggle over the distribution of international trade in a period of little or no economic growth the fragmented energies of the Dutch were no match against the well-organized, national-level organizations they competed with. And when we turn to the late nineteenth and twentieth centuries, a story can be told that bears some comparison to that of Anglo-Dutch competition in the pre-industrial era.

This time it is Britain that is conspicuous as a land of free trade or low tariffs, as an economy open to the activities of multinational firms, and as a recipient of a flow of Irish and Commonwealth immigrants that was very large relative to the modest increase in the demand for labor. Britain, in short, had long been a land in which market forces were remarkably pervasive. Yet despite these characteristics, which, by Olson’s reasoning, should have retarded the formation of distributional coalitions, the country was riddled with them. Moreover, most were small by modern standards. This, Olson theorizes, encourages pressure groups to pursue short-sighted self-serving policies, which goes far to explain the slow growth and relative decline of the
British economy during the last century. Germany, on the other hand, is a success story. Its rapid economic growth Olson ascribes in part to the effects of the unification of numerous petty jurisdictions into Bismark's German Empire. Tariff barriers fell and jurisdictional integration withdrew support from the legacy of local distributional coalitions. This breath of fresh air invigorated the German economy.

What Olson does not emphasize sufficiently is the fact that nineteenth-century Germany shed its petty jurisdictions and guilds not for free trade, but for a comprehensive cartelization of heavy industry, vigorous unionization, protective tariffs, and an extensive bureaucracy; it is no accident that the term 'organized capitalism' finds its first application in Wilhelminian Germany. But despite all of this the German economy grew rapidly and surpassed Britain in many areas by the turn of the century—or was it because of all this? I do not wish to deny that the suppression of market forces fosters an inefficient, sub-optimal allocation of resources. Presumably, the German economy could, at any given time, have derived greater benefit from the resources at its disposal. But could it have grown faster? The point I wish to make here reiterates that which Joseph Schumpeter made so clearly in his Capitalism, Socialism and Democracy: economic development in the long run does not flow automatically from the achievement of short-run optimalization of resource allocation. Or the other way around, static efficiency does not lead directly to dynamic growth.

In comparing Germany and Britain, one might conclude that Britain enjoyed the benefits of pervasive market forces, but that these were subject to sabotage by short-sighted distributional coalitions. Meanwhile, Germany suffered from systematic distortions in resource allocation, but possessed comprehensive distributional coalitions that were capable of setting and pursuing long term goals.

These observations speak directly to the question: How much damage do distributional coalitions cause? Could it be that the extra losses inflicted by small coalitions exceed the benefits derived from the more competitive economy in which these uncoordinated pressure groups seem to be found? I am not altogether happy with this conclusion; personally, I prefer to believe that the disappearance of all conspiracies against the public will redound to the benefit of all. Indeed, I found Olson's critique of a society riddled with 'complex understandings' (Olson, 1982:69–73) to be an eloquent statement of a too-long ignored phenomenon. But, in the final analysis, the historical evidence cited here seems to suggest the view that distributional coalitions are here to stay. In the light of this, we should be concerned with identifying those that do the least harm, and trying to foster the environment in which they can be realized.

Reference