

MANCUR OLSON – SOCIAL SCIENTIST

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Isaiah Berlin popularised the saying of the Greek poet Archilochus, ‘The fox knows many things, but the hedgehog knows one big thing.’ For Berlin, the hedgehog stands for the type of thinkers who ‘relate everything to a single central vision, in terms of which they understand, think, and feel’ (Berlin, 1978). In this sense, Mancur Olson was the ultimate hedgehog of the social sciences. Most of his research can be seen as the exploration and application of one idea, but that idea was very big indeed. It was the problem of collective action, namely how individuals acting in their private interest can fail to secure the provision of goods or services that are collectively in the interests of all. In other words, Olson’s focus was on nothing less than what is arguably the most important class of failures of Adam Smith’s invisible hand.

Today’s economics students, who learn about public goods and externalities routinely in their microeconomics courses, and know that a Nash equilibrium of a non-cooperative game does not in general produce a Pareto optimal outcome, may not think this a very startling insight. But in 1965, when Olson’s *Logic of Collective Action* was published, monopoly was almost the only admitted exception to the efficiency of markets. Externalities and public goods were mentioned in the final week of a microeconomics course, more as *curiosa* than as real economic problems to be taken seriously. Classic examples of externalities were ‘bucolic’ – bees pollinating flowers, and sparks from railway engines setting fire to fields. Olson played a substantial part in the line of research that has so drastically changed our views over the last three decades.

His impact in political science was even greater. Many political scientists used to take it as axiomatic that if some outcome would be beneficial to all members of a group, then the group would act in concert to bring it about. They thought in terms of joint rather than individual action, without asking how the joint action would be enforced. In today’s terminology, we can say that they were implicitly thinking of social outcomes as equilibria of cooperative rather than non-cooperative games. Olson focused on enforcement issues; he implicitly thought in terms of non-cooperative games. Therefore his book was an important milestone in the path that led all social scientists to a better appreciation of the full extent of the problem of collective action.

* In preparing this article I have benefitted from the article written by Mancur Olson’s colleague Martin McGuire (1998), the obituary notice compiled by his department chair Mahlon Strasheim, the appreciation by Allan Schmid (1996), and several obituary notices in newspapers and magazines. I have also benefitted from the knowledge and advice of Carol Kaplan, who was Olson’s dedicated and efficient assistant for many years. Finally, I am grateful to Geoff Harcourt, Karla Hoff, Harvey Rosen, and most of all, Richard Zeckhauser, for detailed and perceptive comments and suggestions on an earlier draft.

Although Olson explored and exposed the difficulties of achieving socially desirable outcomes through uncoordinated individual action, he was no naive advocate of central planning or coercion. For him, the operations of governments were to be subjected to the same analytic scrutiny as markets, and government failures had to be exposed just as fearlessly as market failures. Governments had their own private interests too, and unless these interests were best served by a good performance of the whole economy, that is to say, unless the government had an encompassing interest in the economy, its policies would not bring about economic efficiency.

Olson's most recent ideas on alternative economic and political systems and their interactions – capitalism and socialism, democracy and dictatorship – were evolving into a large tapestry that combined economics, politics, sociology, history, and more. In fact an ongoing and evolving theme of his life's work was the enrichment and unification of all social sciences.

In the social sciences, interdisciplinary work often degenerates into undisciplined work. However, Olson's insistence on the highest standards of scholarship and rigour in all intellectual inquiry was evident on reading just a few pages of his writing. In *The Rise and Decline of Nations*, he laid down some very rigorous criteria by which theories should be judged. To give just one example, he insisted that a list of instances supporting a hypothesis, no matter how lengthy, did not clinch the matter; one had to search diligently for counterarguments and counterexamples, however disconcerting. I will discuss these criteria in more detail below. Together they amount to an excellent statement of a truly scientific approach to social analysis. In describing Olson as a *social scientist* in my title, I intend equal emphasis on the two words.

Olson subjected other people's ideas to similar scrutiny. As soon as one mentioned one's current pet idea, he would start a searching *viva voce* examination. But there was nothing personal in Olson's toughest criticisms or interrogations. On the contrary, he was invariably courteous, thoughtful, helpful, and charming. For him, unsurprisingly, research was itself a problem of collective action; it could not be solved by individual action and competition, but required a great deal of cooperation.

His rigorous yet gentlemanly scholarship was augmented by his amazing intellectual energy and enthusiasm. He found something of interest, worth careful thought and analysis, in seemingly the most trivial incidents and observations, and brought his hedgehog's world-view to shed new light on these. He described his own perspective with typical insight and humor: "When you're a kid with a hammer, the whole world looks like a nail." This combination of a child-like delight in exploration and a thoroughly mature vision, persistence and analytic power was what made the briefest encounter with him such a memorable experience, and deeper acquaintance such a rewarding one.

1. Olson's Life

Mancur Lloyd Olson, Jr. was born on January 22, 1932 in Grand Forks, North Dakota. He came from a farming family of immigrants from Norway. His first

name, pronounced 'man-sir', was common among people of Nordic origin in North America, but unknown in Norway. He claimed, only half in jest, that it derived from the Arabic 'mansoor', meaning victorious. Presumably an adventurous group of vikings picked it up during a raid on Arabia unrecorded in history, and then took it with them to North America.

He was educated in a small local school and then at the North Dakota State University, where he received his B.S. degree in 1954. For the next two years he was a Rhodes Scholar at Oxford. Then he commenced graduate studies at Harvard, interrupted by military service in the U.S. Air Force – as a faculty member in the Economics Department at the Air Force Academy from 1961 to 1963.

He worked on his doctoral dissertation during this time. He was not only a self-starter but a self-finisher, and submitted a complete draft of his thesis. The Harvard professor who read it promptly rejected it. But this is not the kind of story, so beloved by journalists, of an original young genius misunderstood by hidebound old fuddy-duddies. The reader was Thomas Schelling, himself a brilliant and unorthodox economist, who sent 'superb, severe, and detailed criticisms' (Olson, 1990 p. 72). Olson worked hard to take them into account in two rounds of revisions. Then the thesis was not merely accepted, but also recommended for publication by the Harvard University Press. Olson received his Ph.D. in 1963. The thesis later became the legendary *Logic of Collective Action*.

Olson joined the Princeton economics faculty as Assistant Professor in 1963. He briefly moved away from academia from 1967 to 1969, to serve as Deputy Assistant Secretary in the U.S. Department of Health. Princeton did not give him tenure – one of the many mistakes this department has made in its tenure and promotion decisions – and in 1969 he joined the faculty of the University of Maryland, where he remained, ascending to Distinguished University Professor in 1979.

In 1990 he founded the Center for Institutional Reform and the Informal Sector (IRIS). Based at the University of Maryland but with operations that spanned the world, it quickly grew into a leading institution with dozens of researchers around the world, conducting research and training in developing countries and former socialist countries.

He was President of the Public Choice Society from 1972 to 1974, President of the Southern Economic Association for 1981–2 and of the Eastern Economic Association for 1993–4, and Vice-President of the American Economic Association in 1986. His numerous honors include Fellowships of the American Academy of Arts and Sciences and the American Association for the Advancement of Sciences, and an Honorary Fellowship of University College, Oxford. His books *The Logic of Collective Action* and *The Rise and Decline of Nations* won awards and acclaim, and were translated into several languages; I will discuss these, and a small selection from the huge list of his publications, in more detail later in this article.

When he died suddenly on February 19, 1998 of a heart attack, he was in the midst of an extremely active period of research. He left behind a nearly-

complete book manuscript and several research articles, and some other ongoing projects. He is survived by his wife Alison, daughter Ellika, sons Severin and Sander, and innumerable intellectual admirers, heirs and followers throughout the social sciences.

2. The Logic Of Collective Action¹

The analysis and applications in this book are so rich and varied that it is impossible to summarise them adequately. I can only offer a personal and selective statement of some of its themes.

Olson insisted that the question of whether a group will succeed in providing a collective good that benefits all its members should be analysed from the bottom up, namely by examining the incentives of individuals in the group to contribute to the proposed collective action. In modern language, he asked whether the mutually beneficial actions can be a Nash equilibrium of a non-cooperative game.

He recognised *group size* as the most important consideration: small groups are more likely to solve the collective action problem than are groups with many members. Olson argued this point in a very simple way. In a group of n similar people, each derives only $(1/n)$ -th of the total benefit that the collective good generates; therefore his incentive to contribute toward the good is only $(1/n)$ -th of what is optimal for the group. This effect becomes more pronounced as n increases, and in the limit the incentive to contribute becomes vanishingly small. Perhaps the most prominent application of this occurs in conflicts over trade policy, where small groups of producers are able to organise and lobby to obtain protectionist policies that hurt large numbers of unorganised consumers, even though the aggregate losses of the consumers far exceed the gains of the producers.

Next comes *asymmetry* among members. If one member gets a much larger share of the benefit of the collective good than another, then the first has a much larger incentive to contribute. Thus we should see large members doing more to provide collective goods, and small ones free-riding. Olson found many applications of this principle of 'the exploitation of the great by the small', most prominently in military alliances.

Third, among the various devices a group might use to induce individuals to join and contribute to the collective good, Olson emphasised the role of *selective incentives*. These are private benefits, offered only to those who join and pay the membership fee, part of which then goes toward the provision of the collective good. This associated private benefit should be sufficiently large, and unavailable outside the group, so that people find it in their own interest to join and get this package of public and private benefits, rather than stay outside and get the free-riding benefit of the public good alone. Examples include the discounts that are negotiated for their members by groups like the

¹ From now on, I will abbreviate the title as *Logic*.

Association of Retired Persons, which then uses membership fees for lobbying activities.

The book is most remarkable for its broad and comprehensive vision of collective action problems. Olson considers many dimensions of the problem at once, weaving his verbal arguments with skill and illustrating them with convincing examples. The contrast with the large and still flourishing subsequent literature on collective action problems could not be starker. The latter is mathematical, consisting of a sequence of formal models each of which examines one issue or one aspect of the problem in isolation. Formal modelling has merits that are now well understood: it forces the analyst to make his assumptions explicit, it leaves no room for logical slips, and it lays bare all the implications of the assumptions. Indeed, many formal models of collective action have been built by others, and have yielded findings which escaped Olson. One is the importance of income effects. Warr (1983), and Bergstrom *et al.* (1986), found that in a group whose members have identical preferences for the public good but different incomes, there will be a threshold level of income such that members below this level will contribute nothing to the public good, while those with incomes in excess of this level will contribute the entire excess. This also has the corollary that a redistribution of income among those making positive contributions will have no effect on the quantity of the public good. But such a sharp result is not what we observe in reality; the formal model ignores other features like taste differences and effects of ongoing relationships.

Olson occasionally engaged in collaborative research involving formal models. The best known is Olson and Zeckhauser (1965), a beautiful model showing the exploitation of the great by the small. For the last two years, he and I were collaborating on a model of a two-stage game. The first stage of joining a group was non-cooperative, followed by a second cooperative stage of Coaseian bargaining for contributions to the public good, played only among those who joined in the first stage (Dixit and Olson, 1999).

Formal modelling has come to dominate economic theory in the thirty years since *Logic* was published. But that book has not been supplanted by the formalism; it remains an outstanding example of how informal verbal analysis based on a broad vision can stand as a valuable complement to formal modeling. The lasting, and still growing, impact of the book was recognised by the American Academy of Management by giving it an 'enduring contribution' award in 1993, and by the American Political Science Association's Leon D. Epstein award in 1995.

3. Rise And Decline Of Nations²

In this book Olson applied his theory of collective action to an issue of the utmost importance in economic and political sciences, in history, and in

² From now on I shall refer to this by Olson's own acronym, *RADON*.

practical international affairs: Why do some countries at some times grow rapidly, while others stagnate? His sweeping, original, and provocative analysis of this question took him to a new level of fame in academia and beyond. The book won the American Political Science Association's Gladys M. Klammerer award for 1983.

The argument builds upon the concept of a *distributional coalition*. This is a group whose collective action can secure a larger share of the society's fruits of economic activity for its members, at the expense of those outside the group. Various tax and subsidy policies, entry and mobility barriers, and tariffs and quotas on imports, are important but not the only examples of policies that redistribute income and wealth to special interest groups. Most such policies reduce the total income or wealth of the society; they create dead-weight losses. Therefore when a *distributional coalition* successfully solves its collective action problem, the result is economically damaging to the society as a whole.

In addition to the factors conducive to the success of a distributional coalition that were studied in *Logic* – numbers, asymmetry, and ability to provide selective incentives – Olson now stresses the time dimension. In a stable society and economy, distributional coalitions gradually evolve methods for solving their collective action problems. Moreover, the coalitions that have formed are more likely to be the ones that have adapted to the status quo and benefit from its continuation; therefore they are likely to oppose innovations that would increase the economy's growth rate. In other words, distributional coalitions can trap a society into a stagnant economic state.

Conversely, anything that shakes up a society's institutions and destroys the cohesion of distributional coalitions and the sources of their special benefits can be conducive to faster economic growth. Olson offers this as the primary reason why Germany and Japan were able to grow so fast after the second world war, more important than the fact that the destruction of much of their physical plant and infrastructure enabled them to start from scratch and install newer capital incorporating the latest technologies.

As with the other book, this very brief account cannot begin to do justice to the richness of the argument in *RADON*. In the space available, I want to mention only one other matter, namely the concept of *encompassing interests*. A group that represents a large proportion of people in a society will behave very differently from a group with a narrower membership. It will internalise more of the cost inflicted on society by its actions in pursuit of distributional benefits for its members. Therefore it will carry such damaging activities to less extreme levels. Thus a company-wide labour union will not push wage demands as far as would a craft-based union, and a nationwide union will recognise to a greater extent the inflation risk inherent in excessive general wage increases.

I want to spend some time and space discussing Olson's general attitude toward theory and its testing as revealed by his opening discussion in this book. It is worth quoting him extensively on this point (Olson, 1982, pp. 13–4):

The reader should ... not accept the argument in this book simply because he or she finds it plausible and consistent with known facts. Many

plausible stories have been told before and often also widely believed, yet they failed to stand up. The reader should not place even a small degree of confidence in the argument here unless he or she finds that it explains a large number of facts in different categories and with such a limited set of 'causes' or postulates that it is clear I could not consciously or unconsciously have been adding as many as were needed to cover every outcome I could have been aware of at the time I worked out the theory. ... Finally, the reader should also be wary about whether the cases I have set out to explain are simply those that happen to fit the theory.

In other words, there are internal tests for judging theories as well as empirical tests. In the former category fall the criteria of *power* (explaining many diverse phenomena) and *parsimony* (using a small set of assumptions). To put it somewhat differently, when someone offers a theory as an explanation of one set of facts, you should ask 'What can this theory explain that it was not tailor-made to explain?' In empirical tests, it is very important to *avoid selective evidence* in one's own work, and look out for this when judging other people's work. In this large and varied world, it is easy to find several instances that will conform to almost any theory. Often the same facts are consistent with several alternative theories. One must always look for a large data set, where facts differ sufficiently to enable one to discriminate between the explanations.

Olson also stresses the importance of recognising the multiplicity of forces at work in most situations (Olson, 1982, p. 14):

[W]e need not ... expect any one theory to explain everything. ... Absolutely nothing in all of epistemology suggests that valid explanations should be monocausal. ... It is often said in methodological discussions that every meaningful scientific theory must specify one or more possible events or observations, or experimental results, which would, if they occurred, refute the theory. This rule has no applicability to multicausal conceptions unless a perfect experiment is performed [to test all the causes simultaneously].

In other words, observable phenomena result from a combination of underlying conditions, only one of which is the subject of one's attention; therefore empirical tests are almost always tests of a *joint hypothesis*. Rejection of a joint hypothesis amounts to rejection of one or more of its component assumptions; more refined tests must be performed to find out which part.

Such discipline on the process of research should be the norm in all sciences. Regrettably, social scientists often fall short of that; they build *ad hoc* theories, and offer selected items of evidence in support of their theories. Just as Olson's theorising in *Logic* was largely without formal mathematical models, his empirical scrutiny of his theories in *RADON* was without formal statistical or econometric procedures. Although he operated in his unique informal style, he strove to adhere to the strict scientific discipline that he advocated.

4. Roving And Stationary Bandits

When Olson was a graduate student, he was intrigued to read a statement by a peasant in Southern Italy: 'Monarchy is the best kind of government because the King is then the owner of the country. Like the owner of a house, when the wiring is wrong, he fixes it.' Although a firm believer in democracy, Olson had to admit the kernel of truth in this. He thought more about the matter, and many years later offered an answer (Olson, 1991; 1993).

Very small primitive tribes with a few families often solve problems of collective action by consensus, and their agreements are self-enforcing because of the ongoing relationships in the stable groups. But larger societies need enforcement mechanisms, and must delegate a monopoly of coercion to an entity called the government. The problem is that the government may then use its power in its private interest rather than that of society. This problem is more or less acute depending on how much of the interest of the society is reflected in that of the government, or using the concept Olson developed in *RADON*, on how encompassing is the government's interest.

Olson explains the idea using his memorable distinction between roving and stationary bandits. Make the extreme assumption that governments are like bandits, interested only in what they can extract for themselves out of an economy. A roving bandit will act like a slash-and-burn cultivator, taking everything from one group, and moving on to another. He will levy a 100% tax rate, and will not provide any public goods. But this will destroy all incentive for the society to produce anything, so the roving bandit will not get very much either. A stationary bandit will cultivate the economy over which he has control for the longer run – not from any benevolence or ethics, but to maximize his own take. He will levy a revenue-maximising tax rate, corresponding to the top of the Laffer curve. He will even provide public goods, to the extent that they raise total income and thereby increase his tax take by enough. If the tax rate is t , his optimum spending on public goods will be where its marginal product is $(1/t)$. At the social optimum the marginal product is 1. Thus the stationary bandit will provide less public goods than is socially optimal, but he will provide much more than a roving bandit.

This leads to the interesting conclusion that 'Long live the King' may be a prayer in the subjects' own interests. A king with a longer time horizon has a more encompassing interest; therefore he levies a lower tax rate and provides more public goods. A dynastic succession makes the horizon even longer and the interest even more encompassing.

But an ideal democracy does even better than a stationary bandit. Under majority rule, the majority is concerned with its own market income as well as the tax revenues it can extract from the rest of society. Therefore its interest is automatically more encompassing. Therefore it chooses a tax rate lower than that of the stationary bandit, and it provides more of the public goods. Of course in practice democratic governments may be influenced by distributional coalitions, and the comparison may become less clear.

The fact that almost three decades passed between Olson's becoming aware

of the question and his publishing an answer speaks to his tenacity in research. At a more minor level, this tenacity led him to engage in lengthy discussions with coauthors to get the precise formulation and wording of every argument, and even every footnote, just right.

In recent years Olson built upon the idea of the degree of encompassing interests of different forms of governments, and used it in a massive study of the transition from former communist and other dictatorships to democratic governance. He had completed a manuscript titled *Capitalism, Socialism, and Dictatorship*, and was revising it for publication at the time of his death.

5. Olson as an Economist and Social Scientist

Mancur Olson's contributions have had a major impact on all the social sciences. He is even more renowned in political science than in economics. Nevertheless, his thinking was always firmly rooted in economics. In particular, he held to two of the main tenets of our discipline – methodological individualism, and rational behaviour. For him the individual was 'the primitive entity of economic and social life' (Olson, 1990, p. 77), and the idea that social outcomes should be analysed starting with individuals' decisions to participate is the very basis of *Logic*. That these decisions are generally made rationally (which does not always mean selfishly) is also important for the theory. Most of Olson's colleagues and friends have heard him react to some assertion about a social phenomenon by asking 'But would that behaviour be rational?' He recognised, and used in his analysis, rationality of even extreme and repugnant behaviour: 'If violence were never rational, there would not be so much of it' (Olson, 1991, p. 132). But not 'If violence were not rational, there would never be any of it'; his view of rationality was less dogmatic, more complex and more subtle than that of many economists (Olson, 1990, p. 77).

How did such a thoroughgoing economist get such respect and even adulation from other social scientists, instead of the resentment one might expect? Part of the explanation can be found in the rigour and integrity of his scholarship, his tenacity to struggle with a problem till he understood it fully, and his ability to write about it clearly. In part it must have been his quirky charm. But in large measure, I think, it was his knowledge of, and respect for, the arrays of facts and ideas of the other disciplines. The combination of an economist's analytical approach, the political scientist's regard for institutions, and the historian's knowledge of the subtleties and complexities of the world, was what made Olson the social scientist he was.

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